Title
Essays on Globalization and Economic Development

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Publication Date
2020

Peer reviewed|Thesis/dissertation

Essays on Globalization and Economic Development

by

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A dissertation submitted in partial satisfaction of the
requirements for the degree of
Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

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Spring 2020

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Abstract

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In this dissertation, I study the effects of policies introduced by developing countries to draw on the benefits of globalization. Chapters 2 and 3 focus on the impacts of foreign multinational companies (MNCs) that open affiliates in Costa Rica on the performance of local firms and wages of local workers. These two chapters bring novel evidence to the continued debate on the generous policies governments use to compete over multinationals. Chapter 4 studies an industrial policy implemented in Romania to spur the development of the information technology (IT) sector. The policy was justified by both an underdeveloped potential in the IT industry in Romania and a predicted explosion in global demand for IT services. Though industrial policies are central to debates about the role of the state in economic development, the empirical literature on industrial policies is scant. This last chapter aims to close the gap with evidence on the effectiveness of industrial policy.

In Chapter 2 (co-authored with Alonso Alfaro-Ureña and José Vasquez), we investigate the effects of becoming a supplier to multinational corporations (MNCs) using administrative data tracking all firm-to-firm transactions in Costa Rica. Event-study estimates reveal that after starting to supply to MNCs, domestic firms experience strong and persistent improvements in performance, including the expansion of their workforce by 26% and gains in standard measures of total factor productivity (TFP) of 6-9% four years after. Moreover, the sales of domestic firms to buyers other than the first MNC buyer grow by 20%, both through a larger number of buyers and larger sales per buyer. We propose a simple model by which TFP and reputation affect the number of buyers, but TFP alone affects sales conditional on buying. We find a model-based increase in TFP of 3% four years after. Finally, we collect survey data from managers in both domestic firms and MNCs for further insights on mechanisms. Our surveys suggest that becoming suppliers to MNCs is transformative for domestic firms, with changes ranging from new managerial practices to better reputation.

In Chapter 3 (co-authored with Alonso Alfaro-Ureña and José Vasquez), we estimate the effects of foreign multinational corporations (MNCs) on workers. To that end, we combine microdata on all worker-firm and firm-firm relationships in Costa Rica with an instrumental variable strategy that exploits shocks to the size of MNCs in the country. First, using a

within-worker event-study design, we find a direct MNC wage premium of nine percent. This premium reflects above market wages rather than compensation for disamenities.

Next, we study the indirect effects of MNCs on workers in domestic firms. As MNCs bring jobs that pay a premium, they can improve the outside options of workers by altering both the level and composition of labor demand. MNCs can also enhance the performance of domestic employers through firm-level input-output linkages. Shocks to firm performance may then pass through to wages. We show that the growth rate of annual earnings of a worker experiencing a one standard deviation increase in either her labor market or firm-level exposure to MNCs is one percentage point higher than that of an identical worker with no change in either MNC exposure.

Finally, we develop a model to rationalize the reduced-form evidence and estimate structural parameters that govern wage setting in domestic firms. We model MNCs as paying a wage premium and buying inputs from domestic firms. To hire new workers, domestic firms need to incur recruitment and training costs. Model-based estimates reveal that workers in domestic firms are sensitive to improvements in outside options. Moreover, the marginal recruitment and training cost of the average domestic firm is estimated at 90% of the annual earnings of a worker earning the competitive market wage. This high cost allows incumbent workers to extract part of the increase in firm rents coming from intensified linkages with MNCs.

In Chapter 4 (co-authored with Smaranda Pantea), we study the firm and sector-level effects of an industrial policy designed to support the development of the IT sector in Romania. In 2001, Romania introduced an unexpected personal income tax break to programmers with eligible bachelor's degrees and who work on software development for firms in eligible IT sector codes. In 2013, policy-makers suddenly expanded the scope of the original tax break to cover more bachelor's degrees and sector codes in IT. We first use firm-level data and difference-in-difference designs around each policy episode to show that treated firms experience strong and long-lasting growth. We then employ sector-level data and a synthetic control design to show that after the introduction of this policy in 2001, the IT sector grew faster in Romania than in otherwise similar countries. Finally, downstream sectors relying more on IT services also grew faster in Romania after 2001. Our results suggest that this policy has been effective in promoting the development of the IT sector, a sector typically seen as key to the transition to a knowledge economy.

To my parents, Tinca and Doru. Pentru părinţii mei, Tinca şi Doru

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Acknowledgments

First and foremost, I am incredibly grateful to my advisor Andrés Rodríguez-Clare, for his unabated support and guidance. Andrés has struck the perfect balance between encouraging me to be independent and to express my own taste and creativity in research, and carefully accompanying and advising my journey through the Ph.D. I can only hope ever to become such an advisor myself. I also want to express my gratitude to Alan Auerbach, Ben Faber, and Cecile Gaubert, who have each been exceptionally generous with their time and invaluable feedback. In addition, this work has significantly benefited from the comments of Thibault Fally, Fred Finan, Pat Kline, Ted Miguel, Enrico Moretti, Emmanuel Saez, Reed Walker, Danny Yagan and, in general, the wider Berkeley community. I also thank the staff of the Department of Economics – especially Patrick Allen and Camille Fernandez – who have always gone the extra mile to help.

In addition to José Vasquez, who earns a special mention at the end of my acknowledgments, I owe a debt of gratitude to my co-authors Alonso Alfaro-Ureña and Smaranda Pantea. Having had Alonso as a co-author has been one of my luckiest draws in the Ph.D. Alonso has enthusiastically joined José and I in the most ambitious of research plans and has helped us turn them into reality. Smaranda Pantea deserves praise for her team spirit and for enabling me to conduct research on my home country, Romania.

I want to thank the personnel of the Central Bank of Costa Rica for their hospitality during my visits. I am especially grateful to Rodrigo Cubero, Eduardo Prado, Róger Madrigal, Evelyn Muñoz, Ilse Sobrado, Henry Vargas, and Pablo Villalobos at the BCCR for their outstanding role in making the Costa Rican administrative data available for research. Marco Badilla, Santiago Campos, Bryan Carmona, Alfredo Mendoza, and Stephanie Rodriguez have been instrumental in helping us construct and clean the data.

This dissertation has benefited from the generous financial support of several sponsors. I thank the Weiss Fund for Research in Development Economics, the research initiative “Private Enterprise Development in Low-Income Countries", the Robert D. Burch Center for Tax Policy and Public Finance, the Center for Equitable Growth, the Clausen Center for International Business and Policy, the Center for Effective Global Action, the Institute for Research on Labor and Employment, and the Opportunity Lab.

I also want to acknowledge the friends and colleagues that I met during my Ph.D. for their important intellectual and personal spillovers. In particular, I am grateful to David Birke, Alessandra Fenizia, Ingrid Haegele, Sandile Hlatshwayo, Matthias Hoelzlein, Jonathan Holmes, Carla Johnston, Ezequiel Lembergman, Juliana Londoño-Vélez, Sergii Meleshchuk, Ilya Morozov, Murilo Ramos, Evan Rose, Mathieu Pedemonte, Nick Sander, Dario Tortarolo, Mauricio Ulate, Francis Wong, and Román Zárate. All of them helped create cherished memories. I would also like to thank Ashish Aggarwal, Evan Friedman, Juan Herreño, and Xavier Moncasi, who made a fantastic first-year study group.

Furthermore, I want to acknowledge my non-Ph.D-related friends who have been steadfastly in my corner during the past six years. In particular, I thank Mary Arghir, Georgiana FitzPatrick, Julio Galvez, Alin Radu, Mine Senses, Chloe Tucker, and Paolo Zacchia, who

- while at a distance - have never been distant. While more than 10,000km away from Romania, the “Berkeley Contingent" of Romanians has always helped me create heartwarming Romanian moments in Berkeley. I am particularly indebted to Cristina Blaj, Anna Carson, Alina Constantinescu, Ciprian Domnişoru, Mihai Maruseac, Andra Munteanu, and Raluca Ada Popa, for their kindness. Moreover, in addition to Alonso Alfaro-Ureña, I also thank Humberto Grant, Evelyn Navarro, César Rodríguez, Irene Alvarado, and Gaby Villalobos. Their warm welcome makes me remember my time in San José with immense fondness.

Last but not least, I thank my family. My parents, Tinca and Doru Manelici, are my rock. Despite my having inflicted a painful physical distance between us, they have wholeheartedly supported my plan to pursue a Ph.D. I am grateful for all they have sacrificed for me to be in the position to even dream of a professional milestone such as a Ph.D. at UC Berkeley. I am also grateful to my parents-in-law, Ligia and Domingo, for always rooting for my success and wellbeing. Finally, and most importantly, I want to thank José. The lives of José and I are so deeply and beautifully intertwined that I cannot begin to do justice to the many ways in which José has made this dissertation possible. We are the dream team in all parts of life.

Chapter 1

Dissertation Introduction

In this dissertation, I study the effects of policies introduced by developing countries to tap into the benefits of globalization. The first two chapters focus on the impacts of foreign multinational corporations (MNCs) that open affiliates in Costa Rica on the performance of local firms and wages of local workers. These two chapters provide new evidence to the unresolved debate on the generous policies governments use to compete over MNCs. The last chapter studies an industrial policy implemented in Romania to foster the development of the information technology (IT) sector. The policy was justified by both an underdeveloped potential in the IT industry in Romania and an expected explosion in global demand for IT services. Though industrial policies are pivotal to debates about the role of the state in economic development, the empirical literature on industrial policies is scarce. This last chapter aims to close the gap with evidence on the effectiveness of industrial policy.

The first chapter of my dissertation is named *"The Effects of Joining Multinational Supply Chains: New Evidence from Firm-to-Firm Linkages"*. In this first chapter, I study the effects of foreign MNC affiliates in Costa Rica on domestic firms engaged in supplying relationships with these affiliates. The purpose of this chapter is to bring novel evidence – that leverages uniquely rich administrative data – on the existence, size, and mechanisms of these effects. This paper contributes to an extensive literature that has singled out the effects on domestic suppliers as the most likely spillover effects of MNCs on domestic firms.

The analysis of administrative data tracking all firm-to-firm transactions in Costa Rica shows first-time suppliers to MNCs experience strong and persistent improvements in firm performance. For instance, four years after they start supplying an MNC, these firms hire 26% more workers, attain 9% higher profits per worker, and exhibit gains of 6-9% in standard measures of TFP. Their business with other buyers improves as well, in terms of both average sales and number of buyers. We propose a simple model by which TFP and reputation affect the number of buyers, but TFP alone affects sales conditional on buying. We find a model-based increase in TFP of 3% four years after. Survey evidence suggests that becoming suppliers to MNCs is transformative for domestic firms, with changes ranging from new managerial practices to better reputation. These changes arise from interactions during which MNCs communicate their expectations and advice on how to meet them, and the efforts of new suppliers to rise to the challenge.

The second chapter is called *"The Effects of Multinationals on Workers: Evidence from Costa Rica"*. In this second chapter, I turn my attention from the effects of foreign MNCs on domestic firms to those on workers. Workers are likely to be affected by the arrival and expansion of MNCs through both the product and labor markets. Yet, there is very little worker-level evidence on these effects. The Costa Rican context is again useful for this research interest thanks to both its unique merge of administrative datasets and its small domestic market (which allows us to propose credibly exogenous variation in the size of MNC in Costa Rica).

This chapter studies the effects of foreign MNC affiliates in Costa Rica on the wages of workers – both those who become directly employed by the MNC affiliates and those working for domestic firms (who are indirectly affected through the labor and product markets). The *direct effect* is a 9% wage premium upon joining an MNC. Evidence from survey and administrate data indicates that MNCs pay a wage premium to workers to avoid worker turnover, motivate the worker, and ensure cross-country pay fairness within the MNC. We then study the *indirect effects* of MNCs on workers in domestic firms. We allow MNCs to affect both the outside options of workers in the labor market and the performance of domestic employers. To provide causal estimates of the effects of exposure to MNCs through these two channels, we use an IV strategy that exploits the plausibly exogenous variation in MNC employment outside of Costa Rica. Our IV estimates imply that the growth rate of annual earnings of a worker experiencing a one standard deviation increase in either the labor market or the firm-level exposure to MNCs is 1.1 percentage points higher than that of an identical worker with no change in either MNC exposure. In the third and final part of this chapter, we develop a model to rationalize our reduced-form evidence and estimate the parameters that govern wage setting.

Improving the outcomes of workers through the attraction of foreign MNCs is one of the most popular policies pursued by governments in developing countries. The rationale behind this policy is that these countries cannot create *good jobs* from within, and that foreign direct investment is necessary to increase the prevalence of such jobs. An alternative is to design industrial policies that provide incentives for industries that are believed to create good jobs and to trigger positive externalities on other firms and workers in the economy. Designing and implementing industrial policies that are actually effective in spurring the development of the targeted industry might be difficult in developing countries.

The third and last chapter of my dissertation is called *"Industrial Policy at Work: Evidence from Romania's Income Tax Break for Workers in IT"*. In this last chapter, I study a unique industrial policy introduced in Romania in 2001. Since 2001, this policy provides a full personal income tax break to employees with an eligible bachelor's degree specialization who work directly on software development and generate revenues from this activity for a firm in the IT sector. In the first part of the paper, we use firm-level data and difference-in-differences designs to study the effects of the introduction of the tax break in 2001 and its 2013 reform. The DiD estimates suggest that firms treated by the 2013 reform experience large and long-lasting increases in size. In the second part of the paper, we switch to a sector-level cross-country study of the impacts of the 2001 introduction of the tax break. We show that both the treated IT industry and industries that rely more heavily on IT services have thrived relatively more relative to control industries in Romania and relative to the same industries in *"synthetic Romania"*. This evidence is reassuring as to the ability of governments in developing countries to design effective industrial policies.

Chapter 2

The Effects of Joining Multinational Supply Chains: New Evidence from Firm-to-Firm Linkages[1]

2.1 Introduction

Governments around the world compete to attract foreign direct investment – typically in the form of affiliates of multinational corporations (MNCs) – through costly public programs such as tax holidays or subsidized industrial infrastructure.[2] The expectation of these governments is that MNCs are not only high-performers themselves, but that they also help improve the performance of domestic firms. This latter prospect is particularly appealing for developing countries, where most firms are small and low-performing.[3] While there are other channels by which MNCs may affect domestic firms, both scholars and policy-makers view direct supply chain linkages as one of the most promising channels for performance gains.[4]

In this paper, we ask what are the effects of becoming a supplier to MNCs on domestic firms. A complete answer to this question has so far proven elusive for three related reasons.

[1] This paper is joint work of Alonso Alfaro-Urena, Isabela Manelici, and Jose Vasquez. All permissions to reprint this material as a chapter of the present dissertation have been obtained.

[2] The competition in investment incentives (fiscal, financial, and other) for MNCs is so high that governments are adopting ever more sophisticated approaches such as special tax incentives focused on intangible assets (UNCTAD, 2018a). Moreover, the number of Special Economic Zones – the mainstay of investment promotion and facilitation policies – rose from 76 in 1986 (spread across 47 countries) to over 4,500 in 2018 (spread widely across the world) (UNCTAD, 2018b).

[3] See Tybout (2000); Bloom, Mahajan, McKenzie, and Roberts (2010); Hsieh and Klenow (2014).

[4] See the reviews of Harrison and Rodríguez-Clare (2010); Havránek and Iršová (2011); Alfaro (2017). For instance, Alfaro (2017) concludes that "FDI can play an important role in economic growth, most likely via suppliers." The World Bank 2020 World Development Report on "Global Value Chains: Trading for Development" announces that it will assess the typical tools used by policy-makers to "form [...] linkages and networks in GVCs": incentive packages offered to foreign investors, and other policies meant to encourage investors to create "backward in-country linkages" post-investment.

First, it has been exceedingly difficult to observe direct business linkages between domestic suppliers and MNCs in conventional data, especially for the entire economy. Past research has thus relied on sector (or sector-by-region) level variation in the degree of foreign ownership in downstream sectors. Second, firm supply linkages may be endogenous. Without observing actual linkages, it is difficult to tease out the direction of causality between supplying to MNCs and changes in firm performance. Third, the same inability to directly observe suppliers has limited previous research from painting a complete picture of the effects of becoming a supplier to MNCs.

To make progress on these three challenges, we bring together a rich collection of microdata from Costa Rica that includes the universe of firm-to-firm transactions in the country. This makes it possible to observe the actual linkages between MNCs and their domestic suppliers.[5] Second, we adopt an event-study strategy to estimate the effects of starting to supply to MNCs. Third, we provide a detailed account of the changes faced by first-time suppliers to MNCs. We begin with standard measures of firm performance using typical balance sheet data, such as firm size or total factor productivity (TFP) from production function estimations. We then leverage the firm-to-firm transaction data and a simple model to infer changes in TFP from changes in sales to buyers other than the first MNC buyer. Finally, we conduct a new survey of managers in a representative sample of 164 domestic firms and MNCs. These surveys reveal key mechanisms by which first-time suppliers to MNCs improve firm performance.

The analysis proceeds in four steps. In the first step, we introduce the new database that we assemble for this research and the empirical context. Most of our progress relies on the firm-to-firm transaction data collected by the Ministry of Finance since 2008. We match this data with corporate income tax data and foreign ownership data. We can then identify MNCs and domestic firms in buyer-supplier relationships and characterize these firms and relationships. Our event of interest is the first time a domestic firm sells to an MNC in Costa Rica. We focus on events occurring between 2010 and 2015, for which we observe the transition of domestic firms into their new role as suppliers of MNCs. During this period, there are 3,697 domestic firms who start supplying to one of 444 MNCs. These relationships constitute a significant fraction of each domestic firm's output, where the average (median) amount first sold to an MNC is 62,400 (18,590) U.S. dollars and represents 19% (6%) of all sales that year.

In addition to this rich data environment, Costa Rica offers a number of additional advantages to study the effects of MNCs. Ever since the entry of Intel in 1997, the country has attracted a large and diverse set of MNCs.[6] This feature of our setting allows us to characterize the linkages that most benefit domestic suppliers. Second, a Costa Rican public agency (Procomer) implements "Productive Linkages," a program aimed at mediating link-

[5]The data cover the universe of all firm-to-firm relationships whose transactions in a year amount to more than 4,200 U.S. dollars. See Section 2.2 for additional details.

[6]In 2017, the Costa Rican foreign direct investment (FDI) stock per capita was the second largest in Latin America.

ages between MNCs and domestic suppliers.[7] We use the variation granted by the rules of this program for a robustness check to our main event-study results.

In the second step, we describe and implement our main event-study design to estimate the effects of starting to supply to MNCs. Our baseline results use the sample that includes both domestic firms who supply for the first time to an MNC in Costa Rica sometime between 2010 and 2015, and domestic firms who never supply to an MNC between 2008 and 2017. Credible estimates hinge on the assumption that firms yet to supply to MNCs form a credible counterfactual for first-time suppliers to MNCs, after accounting for time-invariant differences between firms (through firm fixed effects) and common shocks (through fixed effects at the four-digit sector by province by calendar year level). As we can estimate event-study coefficients for the four years before a first supplying experience, this method allows us to transparently show that first-time suppliers do not exhibit pre-trends in observables.

The main concern for identification is that firms experience unobservable firm-specific shocks that affect both the timing of their first supplying transaction with an MNC and their subsequent performance. We provide several pieces of evidence to alleviate this concern, including evidence against the effects being driven by a change in managers just before the event. Moreover, we conduct a battery of additional robustness checks that demonstrate that our results are robust to only keeping the first-time suppliers in the analysis, varying the set of fixed effects, and balancing the sample of first-time suppliers around the event year.

Our baseline results show that first-time suppliers experience large and long-lasting improvements in firm size. Four years after their first sale to an MNC buyer, firms have 33% higher sales, 26% more employees, 22% more net assets, and 23% higher total input costs. We find no evidence of selection into supplying to MNCs based on past firm growth. As these firms were provided with a positive demand shock, one natural concern is that this expansion is purely mechanical. We exploit the firm-to-firm transaction data to show that four years after starting to supply to MNCs, sales to buyers other than the first MNC buyer increase by 20%, sales to other corporate buyers grow by 45%, the number of corporate buyers rises by 36%, and the average sales to other corporate buyers increase by 14%.[8]

We then examine standard measures of TFP, ranging from the residual of ordinary least squares (OLS) estimates of a Cobb-Douglas production function to those from standard methods that account for the potential endogeneity of firm-level input choices. We continue to find no evidence of selection into supplying to MNCs, this time based on past TFP growth.

[7] Programs similar to "Productive Linkages" have become increasingly popular among governments looking to improve the local integration of (multinational or large) corporations (see the American Supplier Initiative in the U.S. or the Local Content Unit in Rwanda, Steenbergen and Sutton, 2017). Typically, the aim of these programs is not to replace unmediated market-based linkages between MNCs and domestic suppliers with linkages mediated by the program, but to create additional opportunities for linkages (e.g., by lowering informational barriers on the capabilities of domestic suppliers). Only about 1% of the number (value) of linkages between MNCs and domestic suppliers occurring economy-wide in Costa Rica are mediated by the "Productive Linkages" program.

[8] The *corporate buyers* of a firm are those whose purchases in a year amount to more than $4,200 U.S. dollars (the reporting threshold of the form behind the firm-to-firm transaction data).

In contrast, after their first MNC sale, domestic firms experience sizable and lasting gains in TFP, such that their TFP is between 6 and 9% higher than in the year before the event. While we do not observe prices directly, we provide evidence that mark-up effects are unlikely to explain this observed TFP growth. Under certain assumptions, such as that no output or input price variation is correlated with the event, these results capture the behavior of true TFP.

We also implement an alternative event-study design that leverages the rules of the “Productive Linkages" program. The program evaluates the ability of domestic firms to supply to MNCs and assigns them scores. Scores assess a firm's readiness to supply to MNCs on aspects unobserved in conventional administrative data (such as whether the firm is ISO 9001 certified or not). Based on these scores, Procomer proposes shortlists to MNCs. A small subset of deals lends itself to the implementation of a “winner vs. losers" research design in the spirit of Greenstone, Hornbeck, and Moretti (2010). We find that winners and losers are not statistically different before the event, both in scores and other observables. Also, by their very participation in the program, all contenders are interested in supplying to MNCs and deem themselves ready to do so. This design yields results that are qualitatively similar to those from the main event-study design. While the main economy-wide design and this design have different advantages and disadvantages, they paint a very consistent picture.

In the third step, we propose alternative measures of firm performance that leverage our findings from firm-to-firm transaction data. Specifically, we develop a simple framework that allows us to interpret the behavior of sales to buyers other than the first MNC buyer (hereafter, *sales to others*). Under fairly general demand and total cost curves, changes in sales to others are informative regarding changes in supply-side parameters (here, TFP and reputation). These sales can grow both through sales conditional on buying (the intensive margin) and the number of buyers (the extensive margin). We assume that TFP affects both margins: higher-TFP firms sell more because they have a cost advantage and are better at finding buyers. We use the term *reputation* as an umbrella term over a set of firm-level features other than TFP that only affect the number of buyers. Some of these features are not about reputation *per se* but refer instead to the marketing technology or search costs, among others.

In our model, increases in a measure we call *adjusted sales to others* reflect increases in *composite TFP* (TFP, reputation, and the interaction between the two). The adjustment is done via a parameter δ that controls for both potential returns to scale and the effects of the MNC demand shock on prices. To estimate the increase in TFP alone, our model leads us to a measure of *average* adjusted sales to others. We bring our theoretical results to the data in two steps. First, we estimate δ using an instrumental variable strategy based on government demand shocks. Second, we use the main event-study design to estimate the effect of becoming a supplier to MNCs on (average) adjusted sales to others. We conclude that four years after, composite TFP increases by 6%, while TFP alone increases by 3%. This highlights the potential of the extensive margin to magnify differences in TFP. We obtain similar results across reasonable ranges of the main parameters of the model (δ and the elasticity of demand, σ).

In the fourth and final step, we document additional evidence on the mechanisms behind performance gains to suppliers to MNCs. First, we explore treatment effect heterogeneity using our administrative data. For instance, we find that suppliers in manufacturing see their performance improve twice as much as suppliers in retail and services. Conversely, MNCs in manufacturing and MNCs in high-tech sectors trigger the highest performance gains for their suppliers. We conjecture that MNCs are likely to devote more attention to relationships where the supplied input has a direct bearing on their core activity. Also, suppliers might receive more support from MNCs whose product is of high quality (or complex), as imperfections in inputs can be particularly costly.

We then rely on surveys conducted on a representative sample of MNCs and domestic suppliers. Both MNCs and domestic firms recognize how consequential it is for a domestic firm to start supplying to MNCs. After becoming suppliers to MNCs, most firms undergo a series of interrelated changes, which include expansions in product scope with higher-quality products, better managerial and organizational practices, and improved reputation. These changes arise from interactions during which MNCs communicate expectations and advice, and from the significant efforts exerted by new suppliers to deliver on their contracts.

Our work is related to several literatures. At its core, this article contributes to an extensive literature studying interventions aimed at improving firm performance in developing countries. In a recent review, Woodruff (2018) notes that most of this literature focuses on interventions that alleviate supply-side constraints (e.g., programs granting access to credit or training). Despite the popularity of supply-side interventions, literature reviews suggest that the evidence is mixed as to whether they can actually alter the long-term growth of firms.[9]

While notably scarcer, there is increasing evidence that demand is an important determinant of (small) firm dynamics. In particular, improving access to foreign buyers – through trade[10] or foreign direct investment (FDI) – is believed to hold great promise for firms in developing countries.[11] The expectation is that foreign buyers do not only provide demand shocks but also provide valuable learning opportunities.

By studying the effects of supplying to foreign buyers, this paper relates to a voluminous

[9]For examples of papers in this strand of the literature, see De Mel, McKenzie, and Woodruff (2008); Bloom, Eifert, Mahajan, McKenzie, and Roberts (2013); Fafchamps, McKenzie, Quinn, and Woodruff (2014); Banerjee, Duflo, Goldberg, Karlan, Osei, Parienté, Shapiro, Thuysbaert, and Udry (2015). For reviews, see Banerjee (2013); McKenzie and Woodruff (2013).

[10]There is a long literature linking the exposure to trade to the performance of firms (see review in De Loecker and Goldberg, 2014). On developing countries in particular, see Clerides, Lach, and Tybout (1998); Pavcnik (2002); Verhoogen (2008); Goldberg, Khandelwal, Pavcnik, and Topalova (2010); Topalova and Khandelwal (2011); Bustos (2011); Atkin and Donaldson (2018); Atkin, Faber, and Gonzalez-Navarro (2018); Fieler, Eslava, and Xu (2018).

[11]Other ways in which governments can improve demand conditions include building infrastructure (see Faber, 2014; Ghani, Goswami, and Kerr, 2016; Asher and Novosad, 2018; Donaldson, 2018) and expanding public procurement (see Ferraz, Finan, and Szerman, 2016; Lee, 2017; Carrillo, Donaldson, Pomeranz, and Singhal, 2018).

literature on learning-from-exporting.[12] There are three key differences between exporting and supplying to MNCs locally. First, exporting is only possible for firms selling tradable goods and services, and even further, only possible for firms competitive enough to overcome trade costs.[13] Second, the proximity between buyers and suppliers is likely to facilitate learning. Finally, MNCs are exceptional firms - globally and even more so in a developing country.[14] Hence, MNCs are likely to be more sophisticated buyers than the usual importer.[15]

By studying the effects of supplying to MNCs in one's country, this paper is also closely related to a vast literature on the effects of FDI on firm performance. Papers on this topic generally combine firm-level panel data with sector-level input-output (I-O) tables and find that an increase in FDI at the sector (or sector-by-region) level is associated with increases in standard measures of TFP of (nearby) domestic firms in upstream sectors (commonly referred to as spillovers from backward linkages).[16] Moving from variation in sector-level proxies of exposure to FDI to variation in the actual linkage status of a firm presents new opportunities for precision and insight on the process of joining MNC supply chains.[17]

Finally, this paper also relates to empirical work made possible by the recent availability of domestic firm-to-firm transaction data.[18] This paper studies in detail the effects of

[12] Recent papers find strong positive causal effects of exporting on firm performance (De Loecker, 2007, 2013; Atkin, Khandelwal, and Osman, 2017).

[13] Only 7% of the domestic firms studied here have ever exported before starting to supply to MNCs in Costa Rica. Our surveys suggest that supplying to MNCs locally is seen as a stepping stone to exporting in the future.

[14] MNCs disproportionately populate the right tail of the TFP distribution in Costa Rica (see Figure A.1, A.1). For papers on the exceptional nature and practices of MNCs, see Helpman, Melitz, and Yeaple (2004); Harrison and Scorse (2010); Ramondo and Rodríguez-Clare (2013); Antràs and Yeaple (2014). On global value chains, see Gereffi, Humphrey, and Sturgeon (2005); Alfaro, Antràs, Chor, and Conconi (2015); Taglioni and Winkler (2016); Antràs and de Gortari (2017).

[15] In addition – while not a difference *per se* between exporting and supplying to MNCs – our data also allows us to explore treatment effect heterogeneity based on buyer characteristics (other than its country and purchases, the typical information present in customs data).

[16] For classic papers in the FDI literature, see Haddad and Harrison (1993); Aitken and Harrison (1999); Blomström and Sjöholm (1999); Djankov and Hoekman (2000); Javorcik (2004); Alfaro, Chanda, Kalemli-Özcan, and Sayek (2004); Haskel, Pereira, and Slaughter (2007); Blalock and Gertler (2009); Keller and Yeaple (2009). In their meta-analysis of the literature, Havránek and Iršová (2011) find robust evidence for increases in the performance of domestic firms in supplier sectors (backward spillovers), small increases for firms in customer sectors (forward spillovers), and no effect for firms in the same sector (horizontal spillovers).

[17] Using our firm-to-firm transaction data, we find that sector-level backward linkages predict less than 1% of the actual firm-level linkages (see Figure A.2, A.1). This may explain why estimates of spillovers from backward linkages vary broadly across studies, from strongly positive to negative (Havránek and Iršová, 2011).

[18] Alfaro-Ureña, Fuentes, Manelici, and Vasquez (2018a) show that the main stylized facts established for the production networks of Belgium and Japan (the countries most studied thus far) also hold for the Costa Rican network. Dhyne, Kikkawa, Mogstad, and Tintelnot (2018b) and Dhyne, Kikkawa, and Magerman (2018a) are examples of papers studying the production network of Belgium. For Japan, see for example Bernard, Moxnes, and Saito (2019); Furusawa, Inui, Ito, and Tang (2017); Miyauchi (2018). Contemporaneous papers studying the production networks of Ecuador, Chile, and Turkey are Carrillo, Donaldson, Pomeranz, and Singhal (2018); Huneeus (2018); Demir, Javorcik, Michalski, and Örs (2018).

establishing a specific type of firm-to-firm linkage: the one with the first MNC buyer. After this new linkage, domestic firms improve their performance in two equally important ways: through the number of buyers (the extensive margin) and the sales per buyer (the intensive margin).[19]

This paper proceeds as follows. Section 2.2 describes the data and context. Section 2.3 introduces our event-study strategy and Section 2.4 presents its results. Section 2.5 introduces a theoretical framework that allows us to interpret our event-study findings, in particular those on sales to buyers other than the first MNC buyer. Section 2.6 draws on heterogeneity analyses and surveys for more insights on mechanisms. Section 2.7 concludes.

2.2 Data and Description of Supplying Linkages

2.2.1 Data

Economy-wide administrative data. The main dataset tracks the universe of firm-to-firm relationships in Costa Rica between 2008 and 2017. This information is collected by the Ministry of Finance of Costa Rica through the D-151 tax form. Firms must report the tax identifier (ID) of all their suppliers and buyers with whom they generate at least 2.5 million Costa Rican colones (around 4,200 U.S. dollars) in transactions that year, in addition to the total amount transacted. Given the third-party reporting nature of the D-151, it is used by the Ministry of Finance to enforce corporate income tax compliance.[20] We keep for our analysis approximately 92% of all transactions and 88% of the value of all transactions, which were either filled in correctly or with minor mistakes that could be fixed (e.g., misreporting of decimal points).

We merge this dataset with two other administrative datasets that track the universe of formal firms in Costa Rica over the same time period. The first of these is built from yearly corporate income tax returns and contains typical balance sheet variables. The second dataset comes from the Social Security Fund and includes firms' wage bill and number of workers.

Additionally, we construct a comprehensive dataset on the foreign ownership of firms. In Costa Rica there is no source which provides centralized and exhaustive reporting of the country of origin of firms' capital. To overcome this data limitation, we combine information from five different sources. The first three are annual surveys conducted by BCCR and inquiring on the foreign ownership of firms. These surveys tend to oversample large firms. The fourth source is the organization responsible for drawing FDI to Costa Rica (CINDE),

[19] Our findings on the importance of the extensive margin of sales in firm growth are in line with the findings of Bernard, Dhyne, Magerman, Manova, and Moxnes (2018). The authors use firm-to-firm transaction data from Belgium to show that firms can be large due to their higher productivity (or product quality) or their selling to more and/or larger buyers (among other factors). Cross-sectionally, 81% of the variation in firm sales within narrowly-defined sectors is explained by firms' ability to attract many and/or large buyers.

[20] In the D-151 one can identify firms who reduce their taxes by over-reporting purchases or under-reporting sales.

which provides information on the foreign ownership of firms they attracted. Finally, we bring in Orbis data, which has a high coverage of firms in Costa Rica and allows us to identify firms in the country that are affiliates of MNCs.

A last challenge in building the final administrative dataset is to assign tax IDs to firm groups and properly turn tax ID-level information into group-level information.[21] In A.6.1 we discuss how we overcome this challenge, in addition to providing more details on data construction and summary statistics.

"Productive Linkages" program data. Since 2001, Costa Rica's trade promotion agency (Procomer) has implemented a matchmaking program called "Productive Linkages." Its main objective has been to insert local firms into export supply chains, where the exporter is usually an MNC affiliate in Costa Rica. Procomer has built a comprehensive database of local firms that are suitable and willing to supply to MNCs. Procomer staff visit firms and evaluate them on criteria that are typically unobservable in tax records but are nonetheless relevant to MNCs. Each firm is then assigned an aggregate score. When MNCs approach Procomer with an input need, Procomer identifies which suppliers can produce that input, ranks them based on their score, and shares with the MNC a shortlist of the highest ranked suppliers.[22]

A.6.2 describes the historical records shared by Procomer with BCCR, the steps undertaken to digitize them, the interviews we carried out with former and current Procomer staff to uncover missing institutional details, and the sample construction. We learned that, while the program was not designed as an experiment, by applying sensible restrictions to the universe of deals mediated by Procomer, one can retrieve a set of deals with a quasi-experimental setup. Specifically, we focus on deals between domestic suppliers and MNCs that are first-time deals with an MNC for the domestic firm, occur in our sample period, and where the shortlisted contenders had not yet supplied to an MNC either.

Survey data. In the summer of 2018, we conducted surveys of both MNCs and their domestic suppliers. Our main objective was to shed light on typically unobservable aspects of relationships between the two types of firms. We targeted both firms involved in deals mediated by the "Productive Linkages" program and deals that happened unmediated, in the broader economy. This allowed us to also inquire about the potential benefits of mediation.

The surveys were administered in two versions: a longer field survey conducted at the main location of the firm and a shorter web-based one. Core questions were mirrored between surveys to both domestic firms and MNCs. Given the retrospective nature of some of the topics covered, the ideal respondent was the founder or general manager of the domestic firm

[21] A firm can split its reporting across several tax IDs (e.g., by assigning all workers to one tax ID and all sales to another). If they share ownership and make decisions as a unit, tax IDs should not be treated as independent firms but should be aggregated into firm groups. Throughout the paper we use *firms* to refer to *firm groups*.

[22] Procomer has a strong reputation both in Costa Rica and abroad. In several years, the International Trade Centre granted Procomer the title of "Best Trade Promotion Organization from a Developing Country." The World Bank frequently mentions the "Productive Linkages" program as a role model for its ability to improve the local integration of MNC affiliates (see for example Akhlaque, Lopez, Chua, and Coste, 2017).

and the supply chain manager of the MNC. The need to reach specific employees compounded the already difficult task of establishing a first contact with these firms.

We gathered responses from a total of 164 firms, of which 38 were surveyed in person and 126 online. 106 respondents are domestic suppliers to MNCs and 58 are MNCs based in Costa Rica. When pooling survey answers from both buyers and sellers, these 164 responses cover at least one side of the buyer-seller pair for about 20% of the pairs of interest. Comparisons of the firms that did and did not respond suggest that a response bias is unlikely. D.1 describes the surveys in detail.

2.2.2 Description of MNCs, Domestic Suppliers, and Their First Linkage

MNCs in Costa Rica. We start from the 2,171 firms in Costa Rica that belong to corporate groups where at least one firm is partially foreign-owned.[23] From this set of firms, we create three mutually exclusive subsets: firms that are fully domestically-owned (despite being part of a corporate group where another firm is partially foreign-owned), firms that are themselves at least partially foreign-owned but whose median number of workers is under 100 (across all years of activity in the country), and firms that are themselves at least partially foreign-owned and whose median number of workers is over 100.[24]

In this paper we focus on the effects of starting to supply to the 622 firms in the third category.[25] All 622 firms are MNC affiliates, with known global ultimate ownership and a substantial presence in Costa Rica.[26] From the universe of firm-to-firm transactions in Costa Rica we learn that between 2010 and 2015, 444 of these 622 MNCs became the first MNC buyer from one of 3,697 domestic firms. 47% of these MNCs are from the United States, with the other 53% coming from either Latin America and the Caribbean or Western Europe.

[23] A corporate group is a set of firms that share ownership, but do not necessarily behave as one business.

[24] This size threshold is less restrictive than other choices in the literature. The average annual sales of the plants from Greenstone, Hornbeck, and Moretti (2010) are 11 times larger than the average sales of our 622 MNCs. Abebe, McMillan, and Serafinelli (2019) consider only openings of FDI plants in manufacturing where, in the year of the plant opening or in the year that follows, the plant hires at least 100 workers or at least 1% of the workers in local manufacturing.

[25] Firms in the first category (fully domestically-owned firms) operate in different sectors than those of firms that are partially foreign-owned and part of their same corporate group. Given the loose connection between firms part of the same corporate group, particularly when in different sectors, we exclude them from the analysis. The typical firm in the second category is not an MNC affiliate (but a single location firm with partial foreign ownership) and serves local demand, either in service sectors (e.g., hotels) or in sectors with low domestic input requirements (e.g., import/export retail or real estate agencies). We focus on firms in the third category to also circumvent issues related to FDI statistics, such as the rising use of shell companies. These firms hire 75% of the workers and export 90% of the totals across firms in the three categories combined. See A.6.1.

[26] As customary (Antràs and Yeaple, 2014; Caves, 2007), we define an MNC as “an enterprise that controls and manages production establishments/plants located in at least two countries." We focus on MNCs with their parent in a foreign country and affiliates in Costa Rica (as opposed to MNCs whose parent is Costa Rican).

These 444 MNCs differ from one another in ways that are potentially relevant to the outcomes of first-time suppliers. While manufacturing is the most frequent sector among these MNCs (covering 40% of these MNCs), the remaining 60% of MNCs fall into sectors as diverse as retail, agriculture, and information and communication. Alternatively, 66% of these MNCs are in low-tech or medium low-tech sectors (as classified by the OECD), with the other 34% split between medium high-tech and high-tech sectors. Moreover, while Costa Rica's Free Trade Zone (FTZ) regime is the mainstay of its export and investment promotion strategy, 61% of these 444 MNCs operate outside FTZs. In Section 2.6 we ask whether differences in these characteristics of the first MNC buyer may affect subsequent supplier outcomes.

Domestic suppliers to MNCs. We start from the universe of domestic firms in Costa Rica and restrict our attention to those that have at least a median of three workers and median yearly revenues of 50,000 U.S. dollars (CPI-deflated to 2013 dollars) across all years of activity. We remove firms that are state-owned, registered as households, NGOs, or part of the financial, construction, and education sectors. This leaves us with 24,370 firms. Of these firms, we use the universe of firm-to-firm transactions between 2008 and 2017 to identify and keep only two types of firms: the 3,697 firms that become first-time suppliers to an MNC sometime between 2010 and 2015,[27] and the 14,338 firms never supplying to an MNC between 2008 and 2017. Our interest lies in the firms in the first category, but we also use firms in the second category to construct counterfactuals.

Across the 3,697 first-time suppliers to an MNC, the average (median) firm is small or medium-sized, hiring 19.5 (7.8) workers in 2009.[28] 72% of firms operate in low-tech or medium low-tech sectors, such as retail (including repair and maintenance) or accommodation and food services. The remaining 28% are split between medium high-tech and high-tech sectors, such as the manufacturing of machinery and equipment, or professional, scientific, and technical services. In Section 2.6, we check whether the sector of first-time suppliers may help or hinder their ability to benefit from supplying to MNCs.

Figure 2.1 contains photographs of four domestic firms that belong to and are representative of our sample of first-time suppliers to MNCs. These photographs are meant to provide an illustration of their size, activity, and organization. The first two firms supply automotive mechanic services and retail and maintenance of cutting tools. They hire less than five full-time workers, their facilities are modest and space-constrained, and their processes seem artisanal. The other two firms specialize in tailored precision machining and industrial supplies. They hire between 10 and 20 full-time workers, the layout of their plants is more spacious and organized, and exhibit more capital stock and standardization in processes.

Relationships between MNCs and their domestic suppliers. In Costa Rica, MNCs and domestic firms can establish a buyer-seller relationship either independently, unmediated by

[27] We start in 2010 to ensure we measure correctly the first year when a firm supplies an MNC. After 2015, we are no longer able to observe at least two years after each first-time linkage. See A.6.1 for details.

[28] In 2009 the average (median) never-supplier hires 11.6 (6.0) workers. These statistics for first-time and never-suppliers do not yet account for different sectoral and provincial compositions of the two samples.

any government institution or mediated by Procomer through the “Productive Linkages" program.

Because more than 99% of relationships between MNCs and domestic firms (both in number and value) are formed without mediation, we prioritize the analysis of unmediated relationships. As mentioned above, we find 3,697 domestic firms who supply to an MNC for the first time sometime between 2010 and 2015, and do so in an unmediated fashion. We refer to these first-time supplying instances as (unmediated economy-wide) events. Across these events, the average (median) first sale to an MNC is of 62,400 (18,590) U.S. dollars and represents an average (median) share of 19 (6) % of that year's total sales. The relationship with the first MNC buyer lasts on average (median) 2.76 (2) years. These values and durations suggest that the relationship with the first MNC buyer is plausibly consequential for the supplier.

We contrast these statistics with those for the sample of events mediated by the “Productive Linkages" program and find them to be comparable.[29] In our field surveys, we asked domestic suppliers with deals through Procomer about why they sought such deals in addition to their unmediated deals. For 60% of these firms, Procomer granted better access to MNCs, for 53%, Procomer deals were no different from their other deals but provided another source of business, and for 40%, Procomer lent them credibility in front of MNCs. Hence, it seems that whether first deals with MNCs are mediated or not is not a first-order feature of these deals. On the grounds of these similarities, we use the “Productive Linkages" analysis as a robustness check to our main economy-wide analysis.

Our surveys provide context on the expectations of both MNCs and domestic suppliers ahead of a first linkage. When evaluating a supplier in Costa Rica, MNCs pay particular attention to four aspects: the quality of the inputs delivered, the willingness or ability of the supplier to adapt to the needs of the MNC, the price, and organizational traits such as reliability or the traceability of inputs. MNCs cannot afford a slow learning curve of the domestic supplier; their expectations need to be met soon after establishing the contract (or else the contract is discontinued). Before their first MNC buyer, all domestic firms expected MNCs to differ from domestic buyers. The largest expected differences involved MNCs placing larger orders, being more reliable payers, offering longer contracts, and helping suppliers to adopt better management practices. Despite expecting differences, domestic firms were still taken by surprise by the quick pace, breadth and depth of the changes necessary to supply to MNCs. For many of them, what followed after their first MNC deal was *“as if being thrown into the water without knowing how to swim and having to learn fast"* (direct quote from one business owner).

[29] For descriptive statistics on the events mediated by “Productive Linkages", see A.6.2.

2.3 Event-Study Designs

2.3.1 Economy-Wide Event-Study Design

In our main empirical analysis, we study the effects of becoming a first-time supplier to an MNC in Costa Rica. Between 2010 and 2015, 3,697 such events occur across the Costa Rican economy.[30] More specifically, we estimate the following event-study specification:

$$y_{it} = \alpha_i + X_{it}^{\top}\beta + \lambda_{spt} + \sum_{k=\underline{C}}^{\overline{C}} \theta_k D_{it}^k + \varepsilon_{it}, \tag{2.1}$$

where y_{it} is an outcome variable for firm i in calendar year t, α_i is a firm fixed effect, and X_{it} is a vector with firm-level time-varying characteristics. λ_{spt} are four-digit sector $\times$ province $\times$ calendar year fixed effects. We define the event-time dummies as $D_{it}^k := \mathbb{1}[t = \tau_i + k]$ $\forall k \in (\underline{C}, \overline{C})$, $D_{it}^{\overline{C}} = \mathbb{1}[t \geq \tau_i + \overline{C}]$, and $D_{it}^{\underline{C}} = \mathbb{1}[t \leq \tau_i + \underline{C}]$, where $\mathbb{1}[.]$ is the indicator function and τ_i is the first year when firm i sells to an MNC. ε_{it} is an error term. We normalize $\theta_{\text{-}1} = 0$ and set $\underline{C} = -5$ and $\overline{C} = +5$.

The interpretation of the θ_k sequence depends on the sample over which we run the event-study regression. In all our economy-wide regressions, we use two samples: the *full sample* includes both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC in the firm-to-firm transaction data, whereas the *restricted sample* contains only the firms that eventually become first-time suppliers to MNCs. With the full sample, we compare the outcomes of first-time suppliers in event year k to the outcomes in event year *-1* of firms that are yet to supply to an MNC (future first-time suppliers and never-suppliers alike) and that are in the same narrowly-defined sector and province.[31] With the restricted sample, we compare the outcomes of suppliers in event year k to the outcomes of future first-time suppliers in the same narrowly-defined sector and province in the year before their event (in excess of fixed effects).[32]

Identification of the event-study coefficients hinges on the assumption that firms yet to supply to MNCs form a credible counterfactual for firms that start supplying to MNCs, after accounting for time-invariant (observed and unobserved) differences between firms and

[30] There are 3,813 domestic firms that became first-time suppliers to 471 MNCs. However, in the main event-study regression (2.1) studying the impact on total sales, only 3,697 of these domestic firms are used in the estimation, with the rest being dropped due to the fine set of fixed effects used. For consistency, in Section 2.2.2 we present summary statistics only for those 3,697 firms and their associated 444 first MNC buyers.

[31] For never-suppliers, $D_{it}^k := 0$, $\forall t$ and $\forall k$. The outcomes of never-suppliers are thus part of the set of outcomes assigned to event year *-1*, together with the outcomes of first-time suppliers in event year *-1*. We cluster standard errors at the two-digit sector $\times$ province level to account for possible correlations in outcomes among firms in these cells. We cannot add event-year clustering as never-suppliers do not have an event year.

[32] With this sample, we cluster standard errors at the province $\times$ event year level. Event year clustering is recommended whenever event dates are concentrated on a few values, as in our case from 2010 to 2015.

common sector-by-province-by-year shocks.[33] One might be concerned that – even when chosen from the same four-digit sector and province – never-suppliers do not provide a suitable counterfactual for first-time suppliers. With the restricted sample we can directly test if our estimates are explained by the contrast to never-suppliers or by the staggered timing of a first transaction with MNCs. To preview the results, we find similar estimates across samples, which points to the event as the primary driver of our estimated effects.

Implicit in attributing these effects to becoming a supplier to MNCs is the assumption that there is no selection of firms into supplying to MNCs based on transitory firm-specific shocks that can determine outcomes (Blundell and Dias, 2009).[34] More specifically, shocks with the following three characteristics can pose a threat to identification: (i) they affect the timing of the event, (ii) they affect firm performance after the event, but (iii) they do not affect firm performance before the event. The last condition is important, as we do not find any evidence of pre-existing differential trends for first-time suppliers to MNCs.

Without exhaustive information on first-time suppliers beyond what is available in tax data, it is hard to dismiss this threat definitively. To make progress on this, Section 2.4.2 conducts a battery of checks on its plausibility, such as whether results are driven by changes in firm management contemporaneous with the event. We ultimately conclude that there is limited scope for results to be driven by firm-specific time-varying unobservables satisfying the three conditions above. That is, the event-study design appears suitable for our context and intention to identify the treatment effects of joining MNC supply chains.

2.3.2 Robustness Check: "Winner vs. Losers" Event-Study Design

We use Procomer's "Productive Linkages" program as a robustness check. Its rules generate quasi-experimental variation in opportunities to supply to MNCs among firms shortlisted for a given deal with an MNC. Procomer undertakes thorough evaluations of domestic firms willing to supply to MNCs and assigns them an overall score of readiness to do so. Based on scores, Procomer proposes shortlists of candidate suppliers to MNCs. As most of the information behind scores is typically not available in tax data, these shortlists are likely to provide stronger control groups than those based on tax data alone.[35]

[33] This design is not challenged by selection on levels, observable or not. For instance, even before starting to supply to MNCs, first-time suppliers hire on average 19% more workers than never-suppliers in the same four-digit sector and province. In addition, a consistent estimate of the average treatment effect requires that treated and control firms experience the same macro shocks (Blundell and Dias, 2009). Differential trends might arise if treated and controls operate in different markets. We limit comparison firms to nearby firms in the same four-digit sector to control for common shocks, such as those to factor markets or transportation networks.

[34] In other words, "the availability of panel data allows us to consistently estimate treatment effects without assuming ignorability of treatment and without an instrumental variable, provided the treatment varies over time and is uncorrelated with time-varying unobservables that affect the response" (Wooldridge, 2002).

[35] For instance, Procomer asks whether the firm uses an enterprise resource planning software or whether it carries out financial feasibility studies for its projects. See Figure A.6 (A.6.2) for more examples.

The shortlists of Procomer are similar in spirit to the location rankings for "million dollar plants" (MDP) from Greenstone, Hornbeck, and Moretti (2010). Our argument parallels theirs: shortlisted firms (counties) missing a deal with an MNC (MDP) offer a valid counterfactual to what would have happened with the winners' performance had they not won the deal. In contrast to Greenstone, Hornbeck, and Moretti (2010), we observe the Procomer scores behind the ranking shared with MNCs. In Section 2.4.2, we show the similarity between winners and losers in scores, in addition to other observable characteristics.

The "winner vs. losers" event-study design is a generalized triple-difference design where firms experience a first deal with an MNC in different years. We modify equation (2.1) to allow for an extra interaction between event dummies D^k_{idt} and an indicator dummy of winning deal d, $\mathbb{1}\{Winner\}_{id}$. We label the winner and losers of the same deal with the same d subscript. We investigate the effect of being considered for deal d on both the winner and losers of that deal by running the following regression:

$$y_{idt} = \alpha_i + X_{it}^\top \beta + \gamma_d + \lambda_t + \sum_{k=\underline{C}}^{\overline{C}} \theta_k^L D^k_{idt} + \sum_{k=\underline{C}}^{\overline{C}} \theta_k^{Diff} \mathbb{1}\{Winner\}_{id} D^k_{idt} + \varepsilon_{idt}, \tag{2.2}$$

where y_{idt} is the outcome of firm i part of deal d in year t, λ_t is the calendar year fixed effect, and $\mathbb{1}\{Winner\}_{id}$ is an indicator function that equals 1 if firm i is the winner of deal d. γ_d are deal fixed effects that force the effects on the winner to be measured with respect to those on the actual contenders to the same deal. Our coefficients of interest are θ_k^L and θ_k^{Diff}, which are interpreted as the effect of the event on the losers and on the difference in outcomes between winners and losers, respectively. All other variables are defined the same as for equation (2.1).

2.4 Event-Study Results on Improvements in Firm Performance

2.4.1 Baseline Results

We implement the event-study specification (2.1) to estimate the effects of starting to supply to an MNC on firm scale and standard measures of TFP. We also bring in the firm-to-firm transactions to study the effects on the sales made to buyers other than the first MNC buyer. These results characterize the 3,697 domestic firms who become first-time suppliers to an MNC in Costa Rica between 2010 and 2015. Hereafter, we mention the results from the full sample that includes both first-time suppliers and firms never supplying to an MNC. For completeness, all tables also report the results for the restricted sample that excludes never-suppliers.

Firm scale. Figure 2.2 plots the event-study coefficients for total sales, the number of workers, net assets, and input costs. Reassuringly, we find no evidence of selection into supplying based on past firm growth. It is only after firms start supplying to MNCs that they experience strong and lasting growth. These effects already manifest themselves in the year of their first transaction with an MNC, when the average growth relative to the previous year is of 16% in sales, 6% in the number of workers, and 9% in input costs. Firms continue expanding over the next two years to plateau thereafter at 33% higher sales, 26% more workers, 22% more assets, and 23% higher input costs. Table 2.1 provides additional details. In particular, it shows that the full sample estimates hold up to dropping the never-suppliers. This suggests that the driver of our baseline results is the event, and not the comparison to never-suppliers.

The magnitude and long-run nature of these effects are noteworthy. The average (median) first sale to an MNC is of 62,400 (18,590) U.S. dollars and represents an average (median) share of 19% (6%) of that year's total sales. In other settings where firms receive demand shocks that are comparable (or even bigger), firms do not grow as much. For instance, Atkin, Khandelwal, and Osman (2017) find that Egyptian firms who receive large export orders for rugs (with cumulative payments of 155,682 U.S. dollars for 11 weeks of work) did not increase their number of employees and capital usage. Similarly, supply-side interventions such as business training can also fail to boost firm scale (Karlan and Valdivia, 2011).

Business with other buyers. The natural concern with these findings of firm growth is that they are largely explained by the addition of a new (MNC) buyer. We now leverage the firm-to-firm transaction data to investigate this possibility. In addition to the pattern of total sales, Figure 2.3 shows the patterns of sales to all buyers *except* the first MNC buyer (*sales to others*), all corporate buyers (*total corporate sales*), and all corporate buyers *except* the first MNC buyer (*corporate sales to others*). The *corporate buyers* of a firm in a given year are those reported in the firm-to-firm transaction data, i.e., firms in Costa Rica whose purchases of goods or services exceed 4,200 U.S. dollars that year. Sales to others are equal to total sales minus the sales to the first MNC buyer. Total corporate sales are those made to all corporate buyers. Corporate sales to others exclude the sales to the first MNC buyer.[36]

Across these four sets of buyers, we find no evidence of differential trends in sales before the event of a first sale to an MNC. However, we find large and lasting increases in the four types of sales after the event. Most importantly, these increases are maintained even after we exclude the sales to the first MNC buyer. In the year of the event, sales to others decrease by 19%. This suggests that firms may be capacity-constrained in the short-run. Four years after the event, sales to others increase by 20%, while corporate sales to others increase by 45%.[37]

[36] Aside from total corporate sales, total sales contain exports and sales to end consumers (general public) and firms in Costa Rica whose purchases that year sum up to less than the reporting threshold. We call this difference *non-corporate sales*. Total sales come from corporate income tax returns. Corporate sales and corporate sales to others come from the firm-to-firm transaction data.

[37] Sales to others increase less than corporate sales to others due to a slower increase of 16% in non-corporate sales (see column (1) in Table A.3, A.1). Figure A.3 (A.1) shows how the composition of the sales of first-time suppliers to MNCs changes with the event time. Sales are assigned to five types of buyers: the government, domestic buyers, partially foreign-owned buyers (but not MNC affiliates), MNCs, and exports.

Next, we ask whether these changes in sales to others work through the change in the number of buyers (extensive margin) or average sales (intensive margin). Figure 2.3 (Panel 2.3e) plots the event-study coefficients from a regression where the dependent variable is the log number of corporate buyers (except the MNC triggering the event). We find no differential trends in the number of corporate buyers in the years preceding a first contract with an MNC. There is clear evidence, however, of a gradual increase in the number of other corporate buyers after the event, such that, four years later these firms have about 36% more corporate buyers.[38]

To study responses along the intensive margin, we study the average value of transactions across corporate buyers in each event year. The year when firms make their first sale to an MNC, they see a large decline in their average transaction with other corporate buyers. However, in the next four years, the average transaction becomes 14% higher than in the year before the event. Table A.4 (A.1) shifts to an event-study where each observation is the transaction value associated to a supplier-buyer-year triad. With supplier×buyer fixed effects, we show that four years after the event of the supplier, sales within supplier-buyer pairs are 5% higher. Tables 2.2 and 2.3 provide more details and robustness checks to our results in Figure 2.3 (e.g., we show that results are not driven by demand from buyers who themselves started supplying to MNCs).[39]

Standard measures of TFP. We first estimate TFP using OLS, assuming either a Cobb-Douglas or a translog production function. To this end, in specification (2.1), we use log sales as the outcome variable and the logs of the number of workers, net assets, and input costs as the time-varying controls. We also construct a TFP index for the Cobb-Douglas production function. Instead of estimating input coefficients, we "residualize" sales by subtracting firm-level inputs used, weighted by their respective two-digit-level cost shares.[40] As OLS does not account for the potential endogeneity of firm-level input choices, we also use the methods proposed by Levinsohn and Petrin (2003) and Ackerberg, Caves, and Frazer (2015).

Figure 2.4 summarizes these results and Table 2.4 provides details. Reassuringly, firms that start supplying to MNCs do not display a history of TFP growth. After their events however, suppliers exhibit large increases in TFP, such that four years later, TFP is 6 to 9%

[38] Figure A.4 (A.1) reveals that part of these new buyers are MNCs other than the first MNC buyer. While the lack of pre-trends is mechanical, the continued increase in the number of new MNC buyers is not.

[39] Our findings of increased sales to others suggest that suppliers may not be the only ones who benefited from their new supplying relationship, but that these other buyers benefited as well. Kee (2015) uses a representative sample of Bangladeshi garment firms to show that domestic firms who share suppliers with foreign-owned firms experience both expansions in product scope and productivity. Kee's paper provides empirical support for the theory of Rodríguez-Clare (1996a) and Carluccio and Fally (2013a). While these potential gains to domestic buyers are certainly relevant to any estimation of the aggregate effects of MNCs, they are beyond the scope of this paper.

[40] The dependent variable for the Cobb-Douglas TFP index is $Y_{ist} - \alpha_{k,s2D} \times K_{ist} - \alpha_{l,s2D} \times WB_{ist} - \alpha_{m,s2D} \times M_{ist}$, where $\alpha_{l,s2D}$=(two-digit sectoral wage bill)/(two-digit sectoral revenues), $\alpha_{m,s2D}$=(two-digit sectoral input costs)/(two-digit sectoral revenues), and $\alpha_{k,s2D} = 1 - \alpha_{l,s2D} - \alpha_{m,s2D}$ (to avoid the need to measure capital costs).

higher than in the year before the event.[41] Under certain assumptions, we can interpret these estimates as capturing the behavior of true TFP. In particular, if we assume away input and output price variations correlated with the event, then the methods of Levinsohn and Petrin (2003) and Ackerberg, Caves, and Frazer (2015) already address the other main concern of TFP estimation (input choice endogeneity) and provide credible estimates of true TFP.

We now address the likelihood of one specific type of price variation that could be triggered by the event and lead to an overestimation of true TFP: higher mark-ups charged by the domestic firm after becoming a first-time supplier to an MNC. While we cannot directly rule out this possibility – as we do not observe prices and quantities separately – we provide several pieces evidence against it.

We first use the empirical model of De Loecker and Warzynski (2012), that allows for the estimation of mark-ups by relying on standard cost minimization conditions for variable inputs free of adjustment costs. Table A.2 (A.1) points to a decline in the mark-up of domestic firms, after they become suppliers to MNCs. Hence, if anything, mark-up effects would lead to an underestimation of the true TFP gain.

Further, the answers from our surveys to domestic suppliers and MNCs are compatible with these mark-up estimates. Out of 106 domestic firms, 43 firms found that it was particularly challenging to find a first MNC buyer. Among the three biggest challenges was the fact that MNCs expected lower prices than these firms could offer. Of the 49 domestic firms who assessed that they were explicitly helped by their first MNC buyer to adjust, 34 firms said that MNCs expected in return either unchanged prices (for improving quality) or lower prices (for unchanged quality or even for improving quality).

We then asked domestic firms about their pricing practices for the same order (defined as same product, quality, and quantity) coming from either MNC or domestic buyers. 58% replied that they usually charge the same price to both types of buyers, with the other 42% split in half between whether they charge MNCs more or less. During the in-person surveys, we asked domestic firms if they had ever incurred losses from deals with MNCs. 11 of 15 firms stated that they have made deals at a loss, particularly among the first MNC deals.[42]

From surveys of 58 MNCs, we learn that prices are among the top three criteria in choosing a local supplier. Of the 40 MNCs that claimed to provide explicit help to their new domestic suppliers, 27 expect, in return, prices that either remain unchanged or fall (for an improving quality). MNCs have a privileged access to imports (particularly those in FTZs, which are exempted from custom duties) and, through their corporate commodity manager, are well-informed on suitable suppliers abroad. This suggests that there is little room for domestic suppliers to obtain higher mark-ups from MNCs. Overall, irrespective of the angle of the questions and whether they were addressed to MNCs or domestic firms,

[41] Table A.1 (A.1) shows results for more measures of performance, e.g., profits or sales per worker.

[42] The typical domestic supplier seems to bear most of the risk. For one supplier: "when the MNC develops a prototype for an input, they send us a blueprint. They have a budget for that input, which we agree with. During the process of development (more meetings, R&D processes and follow-ups), there are a lot of changes and improvements that increase the initial cost. We sometimes have to absorb this extra cost to keep the deal and the buyer, and to be taken into account in the future."

we find no indication that suppliers extract higher mark-ups from MNCs. To the contrary, MNCs expect lower mark-ups. Our survey evidence (see D.1.3) is in line with previous evidence.[43]

Finally, we have just seen that starting to supply to MNCs improves the business performance of domestic firms with other buyers, both on the extensive and intensive margins. While this can occur despite price hikes, it suggests that the appeal of the products offered by these suppliers must have increased more than their prices. We conclude that it is unlikely that mark-ups explain the strong and persistent gains in standard measures of TFP.

2.4.2 Robustness Checks to the Baseline Results

Main Economy-Wide Event-Study Design

There is one remaining threat to identification that is not entirely addressed by our findings thus far: the selection of firms into supplying to MNCs based on transitory firm-specific shocks that can determine outcomes. We now investigate the plausibility of this threat.

To start, we asked in our surveys whether domestic firms took special measures to get ready for or attract their first MNC buyer. 44% of domestic firms replied that they did not. Of the other 56%, the most common measures taken ahead of a first sale to MNCs involved efforts to contact MNCs (in-person, online, at business fairs etc.). These efforts are likely to increase the probability of a first deal with an MNC, but unlikely to directly affect TFP. Our surveys also asked domestic firms whether there was any notable change that happened in the firm just before the first contract with the MNC. To the extent that this change can explain the wide-ranging effects just documented, then we would be misattributing these effects to the first deal with the MNC. 100 of the 106 domestic firms denied that such a change took place. None of the six positive answers challenges the interpretation of our estimates as measuring the treatment effect of becoming a supplier to MNCs. See D.1.3 for details.

Moreover, we use administrative data from the Costa Rican Social Security Fund to rule out what we believe to be the most plausible confounding factor: a change in management preceding the first contract with an MNC buyer. A well-connected and talented manager can bring in both this contract and improvements in firm performance. Of the 3,697 first-time suppliers, we identify those having replaced one of their top two earners (plausibly the top tier of managers) in either the year of the first transaction with an MNC or the year before. For this replacement to qualify as a threat, we focus on workers that are new-hires (as opposed to internal promotions). Reassuringly, our estimates are robust to excluding those domestic firms having hired new managers just before their event (see Table A.10 in A.2.2).

[43] Javorcik, Keller, and Tybout (2008) interview suppliers to Wal-Mart in Mexico who describe the bargaining style of Wal-Mart as "take-or-leave-it." To sell to Wal-Mart, firms must accept lower profit margins. Surveys from the Czech Republic find that 40% of suppliers to MNCs had to lower prices 1-30% (Javorcik, 2008).

We also probe the robustness of our baseline event-study results to other common concerns about the event-study methodology. Results are qualitatively similar when we vary the set of fixed-effects used in our baseline regressions (see Tables A.7 to A.9 in A.2 and the discussion that precedes them). Results are also similar when we estimate the regressions on a balanced sample in event time (see Table A.11 in A.2.3). Finally, to accommodate the possibility that the treatment onset is the first contact with an MNC and such contacts occur a year before the first transaction, we redefine the event-year as the year before the first transaction. Results only change in their almost mechanical delay by a year (see A.2.4). These alternate specifications corroborate the suitability of our event-study specification to estimate the effects of interest.

"Winner vs. Losers" Event-Study Design

As argued in Section 2.3.2, the "Productive Linkages" program delivers plausible quasi-experimental variation in opportunities to supply to MNCs. Moreover, as described in Section 2.2.2, deals with MNCs mediated by this program appear to be similar along several key characteristics to economy-wide deals. We now examine whether our findings from the economy-wide event-study design are similar to those obtained from the "Productive Linkages" design.

We first compare winners and losers before the relevant deal (i.e., the deal won by the winner and the deal to which the loser was a contender). Figure 2.5a shows the histograms of winners' and losers' scores (based on which Procomer established the short-lists), while Figure 2.5b plots the histogram of within-deal differences between winners' score and the average of losers' scores. In both figures there is no systematic tendency for the winners' scores to be larger than the losers'. One might interpret this finding as the scores being uninformative. Various pieces of evidence contradict this interpretation, however. First, Procomer scores are positively correlated with firm performance, measured with administrative data.[44] Second, Procomer aims to establish a good reputation for both domestic suppliers and its ability to identify them; assigning uninformative scores would undermine the confidence of MNCs. Table A.35 (A.6.2) compares winners and losers in the year before the deal and fails to find statistically significant differences between winners and losers. Last, all firms that were losers in some deal ultimately became suppliers to MNCs. We conclude that the only meaningful difference between winners and losers is the timing of a first deal with an MNC.

We then proceed to estimating the "winner vs. losers" event-study specification from equation (2.2). Figure 2.6 plots the estimates of the θ_k^L and θ_k^{Diff} coefficients, where the θ_k^L estimates depict the average behavior of losers to a deal and the θ_k^{Diff} estimates depict the average behavior of winners relative to that of losers to their same deal. We look into five measures of firm performance: total sales, the number of workers, the TFP index, the sales to others, and the number of other corporate buyers. Reassuringly, winners do not exhibit pre-existing trends with respect to the losers. In contrast, after winning their first deal,

[44] Figure A.7 (A.6.2) plots Procomer scores against firm value-added per worker. We find similar positive correlations for other measures of firm performance.

winners improve their performance. While estimates are noisy due to the small sample size, they are comparable to those obtained from the main economy-wide analysis. As estimates of θ_k^L for $k>0$ suggest, the gains in winner performance do not come at the expense of the losers' performance, whose performance is left unscathed by the loss of the deal. Table 2.5 provides more details.

While the main economy-wide design and the "winner vs. losers" design have different advantages and disadvantages, it is comforting to see that their results are qualitatively similar.

Robustness Check on Interpretation: Improvements in Third-Party Reporting

One might worry that domestic firms starting to supply to MNCs improve their tax compliance in ways that cast doubt on the interpretation of our baseline results. The third-party reporting structure of the firm-to-firm transaction data offers a unique opportunity to evaluate this concern. In theory, third-party reporting has self-enforcing properties. However, when tax authorities lack resources to pursue inconsistencies between the reports of the buyer and supplier of a transaction, the odds of being audited are not equally distributed across transactions and firms. This weakens the incentives of compliance for transactions or firms under lower scrutiny. If domestic suppliers believe that MNCs are more prone to audits than domestic buyers, these suppliers may pay additional attention to their D-151 reporting.[45]

Firms can improve their D-151 reporting by reducing gaps in reported values for transactions declared by both firms in a buyer-seller pair and/or by lowering the share of transactions only reported by one party. We construct three proxies of reporting quality. The first is a weighted average of the within-pair percentage difference between the larger and the smaller of the two values reported, across all pairs where a given firm is the seller. If buyers consistently report larger amounts than sellers (as tax evasion incentives would suggest), then this measure captures the extent of under-reporting of one's sales compared to the reports of one's buyers. The second measure keeps only pairs where a firm is the buyer and is meant to quantify the extent of over-reporting of its purchases. Finally, we construct a measure of the frequency of transactions found only in the D-151 forms of one firm in the pair.

In A.2.5 we show that becoming a supplier to MNCs is unlikely to have a bearing on either measure of third-party reporting quality, and if it does, the effect is the opposite to that predicted by a reduction of tax-evasive behaviors. Hence, we do not ascribe our results to changes in third-party reporting behavior.

[45] Pomeranz (2015) finds that randomly-assigned audit announcements lead to an increase in value-added tax payments by both treated firms and their suppliers. The increase is higher for treated firms than for their suppliers.

2.5 Alternative Model-Based Measures of Firm Performance

In Section 2.4.1 we studied standard measures of TFP recovered from production function estimations that use sales and expenditure data. These measures already address key challenges of TFP estimation, such as the potential endogeneity of input choices. Nonetheless, an important concern that is not addressed by these measures is that of unobserved variation in prices across firms (De Loecker and Goldberg, 2014). While we find evidence against increases in mark-ups, input and output prices can still change with the event. Not accounting for such changes in prices can bias the estimation of input elasticities in the production function and confound changes in prices or returns to scale with changes in true TFP.

In the absence of disaggregated firm-level data on prices and quantities, we make progress via a simple model that exploits the richness of our transaction data to deliver model-consistent estimates of TFP. The model allows for firm-level changes in prices and scale effects by assuming a fairly general structure for demand and cost functions. The intuition is analogous to that of revealed preferences approaches used to infer TFP and/or quality adjustments from demand estimation.[46] We first infer changes in a *composite TFP* (TFP and other factors, such as reputation, that improve the appeal of the firm) from changes in a measure of adjusted sales to buyers other than the first MNC buyer (hereafter, *adjusted sales to others*). The adjustment controls for potential returns to scale and effects of the MNC demand shock on prices. We then decompose the sales to others into the intensive (average sales, conditional on buying) and extensive (number of buyers) margins. Increases in *average adjusted sales to others* are informative on changes in TFP alone. Among others, this approach has the advantage that it does not require the estimation of production function elasticities.[47]

We summarize the model and its results here, and present more details on derivations and robustness checks in A.3 and A.4, respectively.

2.5.1 Model Environment

Let us consider a domestic supplier firm (henceforth, the supplier) selling a variety of a good to a number of buyers indexed by i. The supplier produces a total quantity of the variety $Q = \sum_i q_i$ with a total cost $TC(Q) = \kappa \left(\frac{Q}{\phi}\right)^{\frac{1}{\gamma}}$, where κ is a constant, ϕ is a productivity

[46] See Broda and Weinstein (2006, 2010); Khandelwal (2010); Hallak and Schott (2011); Feenstra and Romalis (2014); Hottman, Redding, and Weinstein (2016); Bartelme, Costinot, Donaldson, and Rodriguez-Clare (2018).

[47] We circumvent the need to estimate production function elasticities by using transaction data to indirectly infer TFP changes. This is one way in which our approach differs from that of De Loecker (2011). To control for price variation, De Loecker (2011) combines a CES demand system with production function estimation.

shifter (TFP), and $\gamma > 0$ is the returns to scale parameter of the production function.[48]

We assume that the supplier uses a market penetration technology such that in equilibrium, a higher TFP supplier has a higher probability to sell to any buyer i (therefore selling to more buyers in equilibrium). This can be microfounded with either marketing (Arkolakis, 2010) or search costs (Bernard, Moxnes, and Saito, 2019). Additionally, there can be other factors such as the reputation or visibility of the supplier that, while potentially related to TFP, can also improve the probability of selling to a buyer. We will generically call all these factors *reputation* and denote them by r. We define the probability of selling to buyer i as $n_i \equiv n_i(\phi, r) \in [0, 1]$. We refer to ϕ and r as the *supply-side parameters.*

Each buyer combines a continuum of differentiated varieties according to a CES aggregator with elasticity $\sigma > 1$. At price $p(\phi)$, the effective demand for the variety of the supplier is given by $q_i(\phi, r) = n_i(\phi, r) b_i p(\phi)^{-\sigma}$. Here, $b_i = \frac{y_i}{P_i^{1-\sigma}}$, where y_i is the budget and P_i is the price index faced by buyer i. Implicitly, the supplier is free to supply to buyers other than the first MNC buyer (we rule out exclusivity clauses) and does not price discriminate among buyers. Both assumptions are motivated by our surveys. We also abstract from interactions between the market for this good and other markets, acting through general equilibrium effects.[49]

2.5.2 The Effect of the Event on Model-Based Measures of Firm Performance

As in our empirical analysis, consider the event where the supplier starts selling to its first MNC buyer (MNC_0). The event *may* lead to changes in one or both of the supply-side parameters (ϕ and r). Our model aims to help us estimate the change in ϕ (TFP).

We define $\widetilde{Q} = \sum_{i \neq MNC_0} q_i$ and $\widetilde{B} = \sum_{i \neq MNC_0} n_i b_i$ as the quantity sold to and the aggregate demand shifter of all other buyers (i.e., all buyers other than MNC_0). Using the structure of our model, we show in A.3 that sales to other buyers can be written as:

$$\ln(p\widetilde{Q}) = \kappa' + \delta \ln(pQ) + \ln(\widetilde{B}) + (\sigma - 1)\ln(\phi), \tag{2.3}$$

where κ' is a constant and $\delta \equiv \delta(\gamma, \sigma) = (\gamma - 1)(\sigma - 1) \in (1 - \sigma, 1)$.

This δ parameter captures the effect of returns to scale interacted with the demand curve parameter. δ plays a key role in defining what we call the adjusted sales to others. When $\delta \neq 0$ ($\gamma \neq 1$), sales to other buyers depend on firm scale (i.e., total sales), as a change in firm scale affects the optimal price even when TFP remains constant. This parameter is similar to a parameter defined in Bartelme, Costinot, Donaldson, and Rodriguez-Clare (2018), which is used to estimate external economies of scale at the sector level.

[48] In the case of perfectly competitive input markets, our expression for the total cost function encompasses both Cobb-Douglas and general returns to scale CES production functions.

[49] Under these assumptions, the profit-maximizing price is equal to the familiar mark-up over marginal cost, $p = \frac{\sigma}{\sigma-1} MC(Q)$. The second order condition for profit maximization asks for the returns to scale to not be "too large," i.e. $1 - \frac{1}{\gamma} < \frac{1}{\sigma} < 1$.

We then take the total derivative of both sides of equation (2.3) and rearrange terms such that the left-hand side depends only on information observable in firm-to-firm transaction data and δ. We then assume that the demand shifters of buyers i other than MNC_0 ($b_i = y_i/P_i^{1-\sigma}$) do not change systematically due to the event.[50] Finally, we take expectations over all domestic firms that become first-time suppliers to an MNC and find that:

$$\mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\widetilde{Q}}{(pQ)^{\delta}}\right)\right] = (\sigma-1)\varepsilon_{\phi} + \varepsilon_{\widetilde{n}}, \tag{2.4}$$

where $\varepsilon_{\phi} = \mathbb{E}\left[\mathsf{d}\ln(\phi)\right]$ and $\varepsilon_{\widetilde{n}}$ is the expectation of a weighted average of $\mathsf{d}\ln(n_i)$ $\forall i \neq MNC_0$.[51] The left-hand side of equation (2.4) is the expectation of the change in adjusted sales to others.

Let us now define $\varepsilon_{\phi'} = \varepsilon_{\phi} + \frac{1}{(\sigma-1)}\varepsilon_{\widetilde{n}}$ and call it *composite TFP*. The following result emphasizes what needs to be known to estimate changes in composite TFP via equation (2.4).[52]

Proposition 1. *With values for δ (the parameter capturing the effect of returns to scale interacted with the demand curve parameter), σ (the elasticity of demand), pQ (total sales), and $p\widetilde{Q}$ (sales to others, before and after the event of interest), one can estimate $\varepsilon_{\phi'}$ (the change in composite TFP) after an event. Specifically, $\varepsilon_{\phi'} = \frac{1}{(\sigma-1)}\mathbb{E}\left[\mathsf{d}ln\left(\frac{p\widetilde{Q}}{(pQ)^{\delta}}\right)\right]$.*

Proof. *See A.3.*

We can think of changes in composite TFP as measuring changes in supply-side features that affect suppliers' growth both through their number of buyers (extensive margin) and through their average sales made to actual buyers (intensive margin). Composite TFP is thus akin to a multi-dimensional productivity which includes TFP to reputation.

There are (at least) three ways to relate $\varepsilon_{\phi'}$ with ε_{ϕ}. First, note that $\varepsilon_{\phi'} = \varepsilon_{\phi}$ only if $\varepsilon_{\widetilde{n}} = 0$. That is, increases in composite TFP and TFP would be equal only when the increase in adjusted sales to others occurs uniquely through the intensive margin. Given that we find an increase of 36% in the number of buyers, we expect the increase in composite TFP to be larger than the increase in TFP alone. Second, whenever $\varepsilon_{\widetilde{n}}$ depends only on firm-level features other than ϕ (say, reputation), then changes in composite TFP not only capture changes in TFP but also changes in these other features that affect the appeal of the firm. This case motivates the interpretation of $\varepsilon_{\phi'}$ as multi-dimensional productivity. Finally, it is

[50] More precisely, we assume $\varepsilon_{\widetilde{b}} = 0$, where $\varepsilon_{\widetilde{b}}$ is the expectation of a weighted average of $\mathsf{d}\ln(b_i)$, $\forall i \neq MNC_0$. This does not rule out changes in the composition of buyers (thus changes in the average b_i of the actual buyers). It only rules out systematic changes in the b_is of all other potential buyers due to the event of the supplier.

[51] The weight for buyer i is equal to $n_i/(\sum_{k\neq MNC_0}^{N} n_k b_k)$.

[52] Note that if one is only interested in whether the event leads to an overall improvement in supply-side parameters (ϕ and/or r), one does not need to take a stand on the value of σ. Formally, $\mathbb{E}\left[\mathsf{d}\ln\left(p\widetilde{Q}/(pQ)^{\delta}\right)\right] > 0$ if and only if there are overall improvements in supply-side parameters (ϕ and/or r).

very plausible that $\varepsilon_{\widetilde{n}}$ does depend on ϕ as well. In the likely case that ε_{ϕ} positively affects $\varepsilon_{\widetilde{n}}$, then an increase in composite TFP is likely to "double-count" the increase in TFP.[53]

To estimate the increase in TFP alone (ε_{ϕ}), we make two additional assumptions. First, we assume that there is a large number of potential buyers in the country. Second, we assume that for any changes in ϕ and/or r, all buyers i equally adjust their probability to buy from the supplier, i.e., $\mathsf{d}\ln(n_i) = \mathsf{d}\ln(n)$, $\forall i \neq MNC_0$. Under these conditions, $\varepsilon_{\widetilde{n}} = \mathbb{E}\left[\mathsf{d}\ln(\widetilde{N})\right]$, where $\widetilde{N}$ is the number of buyers other than MNC_0.[54] This leads us to Result 2.[55]

Proposition 2. *With values for δ (the parameter capturing the effect of returns to scale interacted with the demand curve parameter), σ (the elasticity of demand), pQ (total sales), $p\widetilde{Q}$ (sales to others), and $\widetilde{N}$ (the number of other buyers, before and after the event of interest), one can estimate ε_{ϕ} (the change in TFP) after an event. Specifically, $\varepsilon_{\phi} = \frac{1}{(\sigma-1)}\mathbb{E}\left[\mathsf{d}ln\left(\frac{p\widetilde{Q}/(pQ)^{\delta}}{\widetilde{N}}\right)\right]$.*

Proof. *See A.3.*

Given that our administrative data allows us to track total sales, sales to others, and the number of other buyers, the remaining step before bringing these results to the data is to settle on credible estimates of δ and σ. In the following section we describe our IV approach to estimating δ. With its estimate in hand, we use the event-study specification in equation (2.1) with adjusted sales and average adjusted sales as dependent variables. Last, we follow Broda and Weinstein (2006) and set σ equal to 6, which is a standard value in the trade literature.

2.5.3 IV Estimation of the δ Parameter

Our preferred estimate of δ comes from an IV strategy. Consider a buyer j and the same assumptions of our model. Denote by an overline all variables that aggregate across all buyers other than j. We can write the expectation of the total differential of log sales to buyers different from j divided by the number of buyers different from j as:

$$\mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\overline{Q}}{\overline{N}}\right)\right] = \delta\mathbb{E}\left[\mathsf{d}\ln\left(pQ\right)\right] + (\sigma-1)\varepsilon_{\phi} + \varepsilon_{\overline{b}},$$

[53] For example, in the *ad hoc* case where $\varepsilon_{\widetilde{n}} = (\sigma-1)\varepsilon_{\phi}$, then $\varepsilon_{\phi'} = 2\varepsilon_{\phi}$ (i.e. the increase in composite TFP overestimates the increase in actual TFP by 100%).

[54] The first assumption implies that with a large number of potential buyers, the total number of other buyers of the supplier ($\widetilde{N}$) is given by the sum of their probabilities of buying from the supplier (n_i): $\widetilde{N} = \sum_{i\neq MNC_0}^{N} n_i$. A weaker version of the second assumption would suffice, but for the sake of exposition we proceed with this stronger version. We provide a discussion of this assumption and its implications in A.4.5.

[55] Similar to the case for Result 1, if one is only interested in testing whether the event leads to an increase in TFP, then one does not need to take a stand on σ. $\mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\widetilde{Q}/(pQ)^{\delta}}{\widetilde{N}}\right)\right] > 0$ if and only if $\varepsilon_{\phi} > 0$.

The empirical counterpart of this equation is given by the following linear regression:

$$\Delta\ln\left(\frac{\overline{pQ}}{\overline{N}}\right)_{it} = \alpha_i + \lambda_{spt} + \delta\Delta\ln(pQ)_{it} + \nu_{it}, \tag{2.5}$$

where the structural error ν_{it} contains both a multiple of the change in firm TFP and changes in the aggregate demand shifter of other buyers (net of firm and four-digit sector $\times$ province $\times$ year fixed effects, α_i and λ_{spt} respectively).

The OLS estimate of δ is likely to be inconsistent, as the error term (e.g., its component coming from a potential change in TFP) may not only affect average sales to other buyers directly through prices, but may also be correlated to total sales. We can overcome this endogeneity via an IV approach. We require the instrument (a) to shift the total sales of firm i, and (b) to affect the average sales to buyers different from j only through a potential scale effect. The ideal instrument would not be correlated with either changes in the TFP of firm i, or changes in the demand parameters from buyers other than buyer j. We propose a special case where buyer j is the government. Our instrument exploits the moment in which a supplier gets a first procurement contract from the government. More precisely, our proposed instrument for the change in log total sales of supplier i at time t is a dummy variable indicating whether supplier i is awarded a procurement contract at time $t-1$ or not.

The exclusion restriction is plausible because (i) the government is a buyer which is unlikely to provide learning opportunities to suppliers (so that supplying to the government at $t-1$ is uncorrelated with changes in firm TFP at t), and (ii) it is unlikely that supplying to the government at $t-1$ is systematically correlated with changes in average demand shifters of other buyers at time t.[56] Moreover, our instrument is relevant, as procurement contracts with the government in year $t-1$ affect the change in total sales from $t-1$ to t. See A.4.1 for additional details.

Table A.15 (A.4.2) reports the results from this IV strategy. Our preferred estimate of δ is of -0.22 and stems from the full sample including both firms that experience the event of starting to supply to the government and firms that never supply to the government. That said, if we use $\delta = -0.08$, the estimate from the restricted sample, results do not change significantly. The first-stage F-statistic is 50 (110 for the restricted sample).

[56] Note that the structural error ν_{it} does not depend on r. Equation (2.5) already takes into account the extensive margin, hence any supply-side parameter other than ϕ affecting the probability of selling to new buyers. Even if starting to sell to the government induces an improvement in one's reputation, this does not invalidate our instrument. One concern is that changes in TFP might drive procurement contracts with the government in the first place. This is partially alleviated by using the instrument with a lag, as future changes in TFP are less likely to predict past contracts. In addition, Table A.14 (A.4.1) shows event-study regressions where the event is defined as the first time a domestic firm gets a procurement contract with the government. We do not find evidence of selection based on pre-trends in TFP. We only find small and short-lived changes in TFP after the event, lending support to our exclusion restriction. See A.4.1 for more details.

2.5.4 Model-Based Results

Result 1. In columns (1) and (2) of Table 2.6, we study the behavior of composite TFP before and after domestic firms become first-time suppliers to an MNC. The dependent variable of these event-studies is $1/(\sigma - 1)$ times the log of adjusted sales to others. We construct adjusted sales to others in two ways: one combines corporate income tax returns data with the firm-to-firm transaction data, the other uses only the firm-to-firm transaction data.[57] In both cases, we find no evidence of differential trends before the event and a strong and positive growth afterwards. Four years later, composite TFP is 6% higher than in the year before the event.[58]

Figure 2.7 compares this model-based measure of composite TFP to those from three standard measures of TFP: a Cobb-Douglas TFP index, and Cobb-Douglas and translog production function estimation residuals. For direct comparability, all estimates use total sales (to others) from corporate income tax returns data. The message from this figure is clear: estimates from all four measures of TFP are statistically similar.[59]

Result 2. Column (3) of Table 2.6 shows the effect of becoming a supplier to MNCs on TFP alone (as opposed to composite TFP). The dependent variable is now $1/(\sigma - 1)$ times the log of average adjusted sales to others. We construct average adjusted sales to others only from firm-to-firm transaction data, as this allows us to track changes in the intensive and extensive margin for the same set of buyers. Again, we find no evidence of differential trends in TFP before the event and strong and positive growth after.

Contrasting these results with those from Result 1 informs us on the importance of the extensive margin (recall that composite TFP and TFP are only equal when $\varepsilon_{\widetilde{n}} = 0$). To this end, we compute (one minus) the ratio of the TFP gain according to Result 2 (0.047 from column (3) from Table 2.6) over the gain in composite TFP according to Result 1 (0.109 from column (2)). This exercise indicates that the increased ability to get new buyers (the extensive margin) accounts for 57% of the change in composite TFP. One limitation of the TFP estimates from column (3) is that they describe the behavior of transactions with corporate buyers alone.

To make statements that describe TFP based on the average sales to *all* other buyers (not just those recorded by the firm-to-firm transaction data) one requires additional assumptions on the pattern of the number of buyers whose transactions are under the reporting threshold. Under the proportionality assumption that the extensive margin matters as much for the

[57] The total sales from firm-to-firm transaction data are the total corporate sales defined in Section 2.4.1, whereas the sales to others from firm-to-firm transaction data are the corporate sales to others defined in the same section.

[58] We prefer the estimate in column (1) because it captures the behavior of sales to *all* other buyers, not only those recorded in the firm-to-firm transaction data.

[59] The only difference that is statistically significant pertains to the year of the event. During that year, suppliers experience a net increase in total sales and a concomitant fall in sales to others. While standard measures of TFP only take into account the net increase in total sales, our model rationalizes the decrease in sales to others as a decrease in composite TFP. This fall in sales to others is likely to be driven by adjustment frictions upon starting to supply to MNCs, outside the scope of this model.

sales to corporate buyers above the threshold as to those below, the TFP estimate from Result 2 would become 43% of the 6% estimate from Result 1 (column (1) from Table 2.6), or around 3%.

Estimating the share of these extensive margin effects uniquely due to changes in TFP (ϕ) or reputation (r) is outside the scope of this paper. We therefore remain agnostic on how ϕ and r relate to each other and to the probability of selling to a new buyer (n_i). We only assume that both ϕ and r have a positive effect on this probability. That said, ϕ is likely to be positively correlated with r; a firm that reveals itself as able to learn and adapt fast is likely to improve its reputation, and vice versa. Section 2.6 provides intuition on this relationship from our surveys.

An exhaustive anatomy of the changes undergone by first-time suppliers to MNCs requires significantly more data than what is commonly recorded for an entire economy (e.g., data on prices, product quality, product scope, reputation). Nonetheless, the findings in this section represent a step forward in terms of understanding these changes, relative to what can be known from corporate income tax returns data alone. In particular, we have shown that by combining firm-to-firm transaction data with a simple model, we can learn about the potential role of the extensive margin. While part of the improved ability to sell to more buyers may be a consequence of gains in TFP, the extensive margin seems able to compound these gains.

2.5.5 Robustness Checks for the Model-Based Results

Our baseline model-based results use $\delta = -0.22$ and $\sigma = 6$, which imply returns to scale $\gamma = 0.96$. A.4 explores their sensitivity to both parameters. We first vary δ between -1.2 and 0.3, keeping σ at 6. For this σ and range of δ, the returns to scale of the production function lie between 0.76 and 1.06. Tables A.16 and A.17 implement Result 1 using balance sheet and firm-to-firm transaction data to construct the adjusted sales to others, whereas Table A.18 implements Result 2 using firm-to-firm transaction data to construct the average adjusted sales to others. As expected, the more negative (positive) the δ – i.e., the more decreasing (increasing) the returns to scale, γ – the larger (smaller) are the implied TFP gains from the event. For values of δ close to -0.22, results remain largely unchanged.

Figure A.5 shows how results vary not only with γ (or δ) but also with σ. As one would expect, the more elastic the demand curve (the larger the σ), the more sensitive are the sales to others to changes in prices. This means that a larger σ requires a smaller TFP gain to rationalize a given increase in sales to others. At the same time, the more decreasing the returns to scale (the smaller the γ), the higher prices will get after a given increase in the scale of the supplier. For this reason, the smaller the γ, the larger is the increase in TFP that generates a given increase in sales to others. That said, our baseline results are robust to values of γ and σ around our preferred values of 0.96 and 6, respectively.

Finally, we also infer σ and γ from estimates of mark-ups and input elasticities of the production function of first-time suppliers to MNCs (following De Loecker and Warzynski, 2012). This can be done since our model implies a one-to-one relationship between the mark-up μ and the demand elasticity σ ($\mu = \sigma/(\sigma - 1)$). Moreover, the returns to scale γ can be computed as the sum of the input elasticities of the production function. This approach gives us $\sigma = 5.03$ and $\gamma = 0.92$ (hence $\delta = -0.33$). Results for these values are similar to our baseline results. See A.4.3 for details.

2.6 Additional Evidence on Mechanisms

In this section, we present additional evidence on the ways in which domestic firms interact with MNCs and how they adjust in response to their new status as suppliers to MNCs.

Evidence from administrative data on heterogeneous effects. We use the administrative data and the economy-wide event-study to characterize the heterogeneity of effects by sector. We split domestic firms based on either their sector or that of their first MNC buyer and run separate regressions on each sector-specific sample. Sectors fall into one of four categories: manufacturing, retail (including repair and maintenance), services, or agriculture. Table 2.8 looks into the Cobb-Douglas TFP index. Suppliers in manufacturing benefit most from starting to supply to MNCs, with an 11% higher TFP four years later, while suppliers in retail and services attain only half of this gain. Suppliers in agriculture see no effect. When we split firms by the sector of the MNC buyer, only those starting to supply to an MNC in manufacturing see their TFP grow. Our overall estimate of a 6% higher TFP index four years later is therefore driven by suppliers whose first MNC buyer was in manufacturing, or by suppliers in manufacturing and – to a lesser extent – in retail and services.

Table 2.7 divides firms based on the technological (knowledge) intensity of the sector of either the supplier or the first MNC buyer. We categorize sectors as high- or low-tech according to OECD classifications. The high- (low-)tech category also includes high (low) knowledge-intensive services.[60] Suppliers in low-tech sectors are those who benefit the most from starting to supply to MNCs. Conversely, suppliers whose first MNC buyer is in a high-tech sector are those whose performance improves the most. We also split suppliers depending on whether their first MNC buyer is under the Free Trade Zone (FTZ) regime or not. First-time suppliers to an MNC in FTZs experience stronger performance gains. The findings on the high-tech or FTZ nature of the MNC are compatible with each other and with those from Table 2.8, given the sizable overlap between MNCs in FTZs, high-tech MNCs, and MNCs in manufacturing. The findings on suppliers' sectoral splits are reconciled by the fact that 87% of suppliers in high-tech sectors operate in knowledge-intensive services (e.g., professional, scientific and technical services), while 58% of suppliers in low-tech sectors are in manufacturing and retail.

This heterogeneity analysis suggests that the nature of inputs supplied can affect the extent to which suppliers can learn from MNCs and improve their performance. MNCs are

[60] The OECD classifies manufacturing sectors as high-tech, medium high-tech, medium low-tech or low-tech, and service sectors as high- or low-knowledge intensive. We label as *high-tech* the high-tech or medium high-tech manufacturing sectors and high knowledge-intensive service sectors, all others are referred to as *low-tech*.

more likely to be invested in the success of supplying relationships where the input has a direct bearing on their core output. Also, suppliers might receive more support from MNCs whose product is high-quality (or complex), as imperfections in inputs can be particularly costly. This might explain why high-tech (or manufacturing) MNCs trigger the highest performance gains and particularly so for domestic firms in manufacturing.[61]

Evidence from surveys to managers in domestic suppliers and MNCs. We now summarize the key takeaways from our surveys, inviting readers to D.1 for details.

To set the stage, our surveys first asked MNCs about the factors that were important to their decision to open an affiliate and later stay and/or expand in Costa Rica. To both questions, the local availability of suitable suppliers ranked only sixth among the eight options.[62] We then asked MNCs about the corporate hierarchy of sourcing decisions. The headquarters (HQ) is involved in all sourcing decisions and particularly so in those involving core inputs. In theory, local affiliates show interest in having more domestic suppliers. In practice, they seem reluctant to trust domestic firms with critical inputs and prefer, instead, the global suppliers recommended by the HQ. Domestic firms are more likely to be considered for secondary inputs. Domestic firms echoed a difficulty to establish a first contract with MNCs. For the 43 of the 106 domestic firms for whom it was particularly difficult to start supplying to MNCs, the three most frequent reasons were that MNCs did not know or trust them, that MNCs were difficult to contact, and that MNCs expected lower prices than they could offer.

Against a backdrop of relatively low integration in Costa Rica, we asked MNCs whether, once they agree to be supplied by a domestic firm, they offer the firm any explicit support to boost its ability to supply to them successfully. A total of 40 out of 58 MNCs (69%) replied positively. The three most frequent ways in which MNCs claimed to help domestic firms were the sharing of *blueprints* or clear details about the expected product or services, visits of the supplier to the MNC to learn about the processes where its input is used, and visits of the MNC to the supplier to carry out audits and offer guidance on improvements. We also asked the mirror questions to domestic firms. In terms of explicit help, 47 of 106 domestic firms (44%) acknowledged receiving such help. The three most important forms of help coincided with those mentioned by MNCs. What follows is a quote where the general manager of a domestic supplier describes the usefulness of the help offered by their first MNC buyers:

> We felt that, while working with a multinational, we could tap into a "global catalog" of best practices. On the spot, we were learning a lot, not having to go

[61] This intuition is supported by survey responses of MNCs on the explicit or direct help extended to domestic suppliers. Of the 31% of MNCs who denied providing any explicit help, 78% are in low-tech sectors, whereas of the 69% of MNCs who claimed providing help, 58% are in high-tech sectors. MNCs in manufacturing are more likely to grant several types of support at once (e.g., reciprocated visits, sharing of blueprints and best practices, putting the domestic firm in contact with suppliers to other affiliates).

[62] The five factors weighting more heavily in the decision of MNCs to invest in Costa Rica were the education of workers, the tax incentives, the distance to target markets, the Costa Rican market, and wages.

> through the same struggles as suppliers to other affiliates in the past, skipping hardships, and having a steeper learning curve.

MNCs are more likely to perceive these interactions as direct help than domestic suppliers for two reasons. First, MNCs are particularly demanding with their suppliers and new suppliers have a short period of time to adapt. Second, domestic suppliers declared that most of the efforts to adapt to the expectations of MNCs are born by the domestic firm alone. When we asked MNCs what they assess to be the biggest disadvantage or risk for domestic firms that become their suppliers, the pressure to adapt fast was among the most frequent answers. In the words of the supply chain manager of one MNC:

> The biggest disadvantage of starting to work with us has to do with our "zero tolerance" policy. There is no forgiving of mistakes in the "major league." [...] New suppliers can have some failures at the beginning, but very fast they need to succeed in delivering whatever they committed to deliver. We cannot afford to be the sponsor of a supplier that does not rise to the occasion. We are willing to help them, and we do help them, but cannot be a charitable benefactor forever and ever. Suppliers are under a lot of pressure to adapt fast, to change all their paradigms of how to do business.

We then surveyed domestic firms about the changes that they experienced after their first supplying relationship with an MNC. 62% of the 106 domestic respondents mentioned having expanded their product scope, in particular with higher-quality goods and services demanded by MNCs.[63] These higher-quality products required firm-wide changes; for instance, introducing a quality management system. Also, higher-quality products require better inputs. This explains why 39% of suppliers had to change their sourcing strategy, 44% hired more high-skilled workers, and 27% had existing workers work harder. 50% of firms improved their managerial and organizational practices, in part advised by MNCs, in part prompted by pressure from MNCs to meet the agreed standards and to do so consistently.[64]

Overall, domestic firms implemented several interrelated changes as a consequence of becoming suppliers to MNCs. When asked about the most important of them, respondents typically struggled to isolate one change as being distinctively more important than the rest.

[63] It is plausible that if domestic firms expand their offer of goods or services, they become attractive to buyers in more areas of activity. Table A.5 (A.1) uses the main economy-wide samples (based on administrative data) to show that, four years after having a first MNC buyer, domestic firms sell to buyers in 25% more two-digit sectors and 29% more four-digit sectors. These increases are beyond those mechanically granted by increases in firm size, as we already control for the total sales of the domestic firm. We also find weaker evidence of an increase in the number of sectors from which domestic firms purchase their own inputs.

[64] According to the supply chain manager of one MNC: "A big risk for domestic firms that start supplying to MNCs comes from failing to deliver consistently their product or service at the expected parameters. The product or service supplied is continuously assessed. Suppliers cannot miss the mark, not even once. If they supplied everything correctly one time, then in theory they have the technical ability to do that again. But this consistency has to do, more than anything, with a managerial vision of excellence."

The testimonial of the general manager of one domestic supplier emphasizes the interrelated nature of these changes:

> The biggest change came with the expansion of the portfolio of goods and services we offered. This part has been the most challenging and the riskiest. That said, this change implied many others. One must be very agile in the organization of production, have inventories for very different inputs, improve financing etc. It can be a wild experience, far from one's comfort zone.

Did starting to supply to MNCs also help the reputation of these domestic firms? Our surveys suggest that it did. When asked whether it was easier to find more MNC buyers after the first such buyer, 83 domestic firms (78%) responded positively. Of these, 86% stated that it became easier to gain the trust of new MNCs. Similarly, their improved visibility in the domestic market also helped with domestic buyers. That said, earning a reputation does not automatically imply that this reputation is positive and thus helpful in selling to new buyers. Domestic firms were motivated to learn and adapt quickly to the expectations of their first MNC buyers, in order to avoid being characterized as bad suppliers. In fact, MNCs believed that one of the biggest risks for suppliers was to be revealed as incapable of coping with the standards of MNCs and for this information to be shared with other potential clients, particularly other MNCs. This points to an important relationship between a firm's reputation and TFP. While investigating this relationship is outside the scope of this paper, it suggests that reputation can magnify the importance of differences in TFP on overall firm performance.

2.7 Conclusion

In this paper, we show that upon becoming suppliers to MNCs, domestic firms in Costa Rica experience strong and persistent gains in firm performance. For instance, four years after, domestic firms hire 26% more workers and experience gains of 6 to 9% in standard measures of TFP. We then exploit the fact that we can observe all firm-to-firm sales of first-time suppliers to explore additional measures of firm performance. Sales to buyers other than the first MNC buyer increase by 20%, with this growth occurring both on the extensive (number of buyers) and intensive (sales conditional on buying) margins. We propose a simple model wherein TFP and reputation affect the extensive margin, but TFP alone affects the intensive margin. We find a model-based increase in TFP of 3% four years after. Finally, we collect survey data from managers in both domestic firms and MNCs, from which we learn that first-time suppliers experience wide-ranging improvements such as those to their managerial practices and reputation. These insights from surveys corroborate our model-based findings.

We highlight four avenues for future research. To start, our surveys underscore the interdependence of the upgrades made by domestic firms upon becoming suppliers to MNCs. For example, successful expansions in product scope (typically with higher-quality products) need to go hand in hand with a higher efficiency, so that firms can switch seamlessly between products requiring different inputs and processes. Separately estimating the contributions of changes in efficiency, product scope, and quality to changes in TFP requires information not available in tax data. An environment closer to a randomized control trial would make it possible to disentangle these interrelated effects of supplying to MNCs.

Given the importance of finding new buyers for firm performance, new work could also investigate the factors that affect the number of buyers. One challenge is to separately identify the role of TFP (or reputation, broadly construed) on the probability of selling to new buyers. The plausible correlation between TFP and reputation (on top of well-known difficulties to measure them both) compounds any such attempt. In addition, increases in TFP (or reputation) are likely to increase the probability of selling to specific buyers, adding another layer of complexity to the role of new buyers in explaining firm performance.

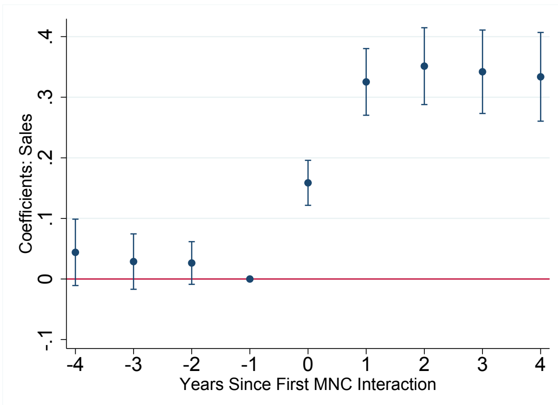
Another question that arises is to what extent our results come from the multinational nature of buyers, as opposed to their managerial expertise or technological level. For instance, we find that firms who start supplying to MNCs in high-technology sectors experience the strongest TFP gains. The main obstacle faced here is that in the developing world, there are rarely any comparable domestic buyers. In countries with a sufficient number of comparable domestic buyers, one could ask whether MNC buyers trigger larger TFP boosts than otherwise similar domestic buyers. This also relates to the question of why only supplying to certain types of MNCs leads to TFP gains. Although these questions are beyond the scope of this paper, they are fruitful avenues for future work.

Finally, a natural next step is to study the general equilibrium effects of forming relationships with MNCs. A comprehensive evaluation of the benefits of MNC entry requires not only credible estimates of their effects on domestic suppliers but also estimates of their actual integration in the domestic economy. Firm-to-firm transaction data allow one to circumvent the use of I-O tables and provide such credible measures of integration.

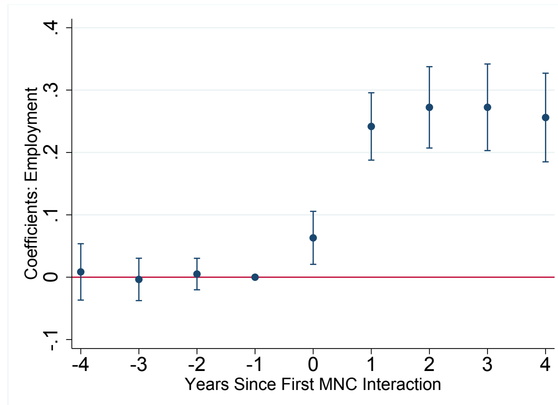
Figures

Figure 2.1: Four Examples of Domestic Suppliers to MNCs

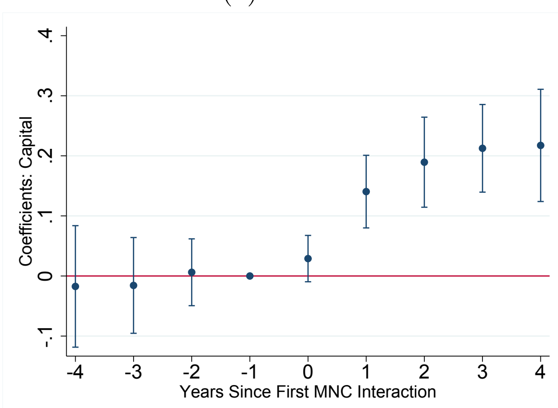
Notes: Figure 2.1 is a collage of four photographs taken by the authors during visits to four domestic suppliers to MNCs. All four firms have responded to the in-person long survey. Firms in the top row supply automotive mechanic services (left-hand side firm), and retail and maintenance of precision cutting tools (right-hand side firm). These firms have under five full-time employees, their facilities are modest and space-constrained. Their deals with MNC buyers are discontinuous, occurring mostly when MNCs have an emergency. Firms in the bottom row specialize in tailored precision machining (left-hand side firm), and tailored industrial supplies (right-hand side firm). These firms hire between 10 and 20 full-time employees, the layout of their plant is more spacious and organized, and they display more capital and standardization in processes. Their relationships with MNCs are longer-lasting and involve products or services that relate to the core activity of the MNC.



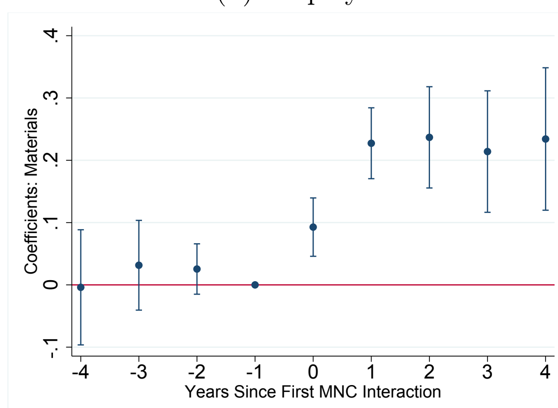
(a) Total Sales



(b) Employment



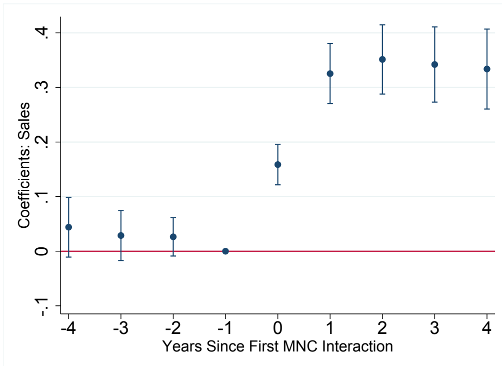
(c) Net Assets



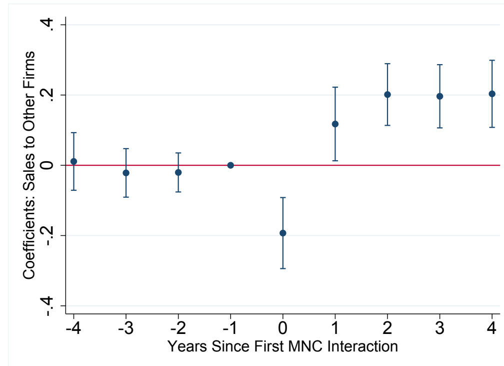
(d) Input Costs

Figure 2.2: Domestic Firms Increase Their Scale after Starting to Supply to MNCs

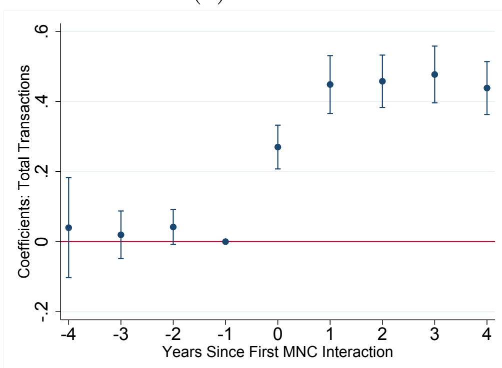
Notes: Figure 2.2 plots the estimated θ_k event-study coefficients from a regression of the form given in equation (2.1), where the dependent variable is, in turn, log total sales (Panel 2.2a), log employment (Panel 2.2b), log net assets (Panel 2.2c), and log input costs (Panel 2.2d). The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. These regressions do not include the vector of firm-level time-varying characteristics, X_{it}, but include firm and four-digit sector $\times$ province $\times$ calendar year fixed effects. The vertical lines reflect the 95% confidence intervals. The coefficients plotted correspond to columns (1)-(4) in Table 2.1, obtained from the full sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017.



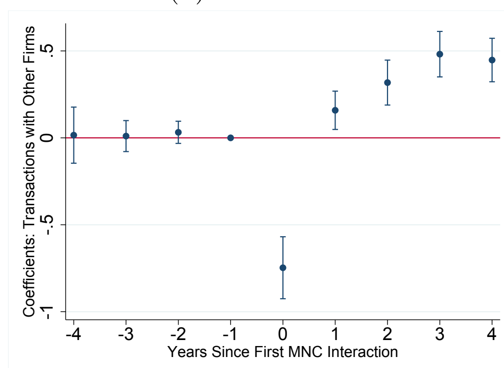
(a) Total Sales



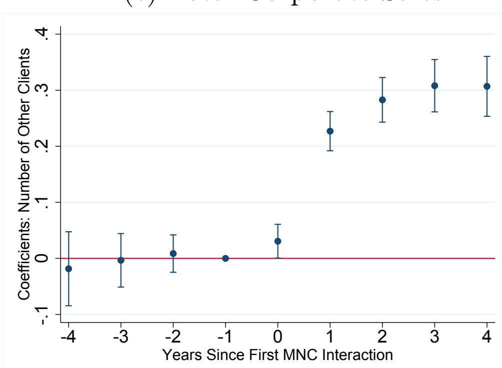
(b) Sales to Others



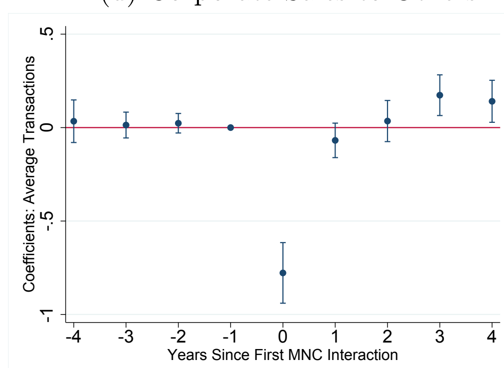
(c) Total Corporate Sales



(d) Corporate Sales to Others



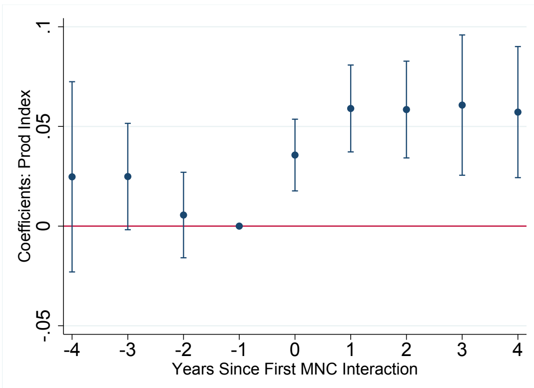
(e) Number of Other Corporate Buyers



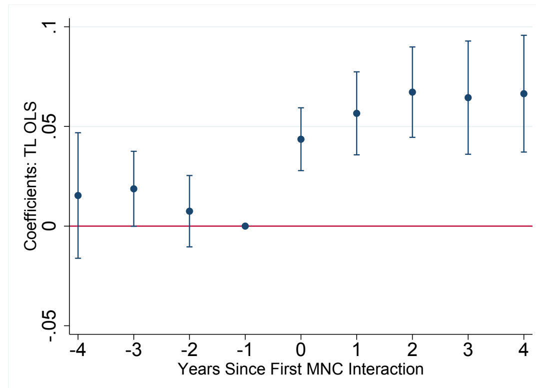
(f) Average Sales to Other Corp. Buyers

Figure 2.3: Domestic Firms Improve Their Sales to Others after Starting to Supply to MNCs

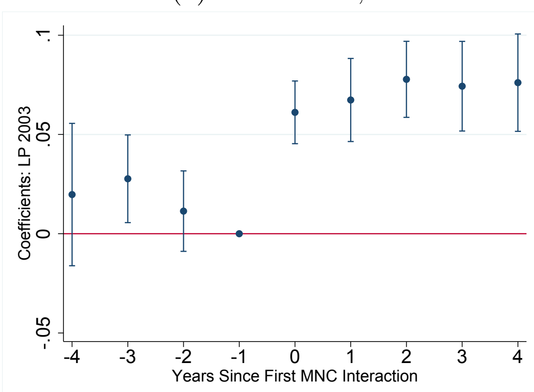
Notes: Figure 2.3 plots the estimated θ_k event-study coefficients from a regression of the form given in equation (2.1), where the dependent variable is, in turn, log total sales (Panel 2.3a), log sales to buyers other than the first MNC buyer (Panel 2.3b), log total sales to corporate buyers (Panel 2.3c), log sales to corporate buyers other than the first MNC buyer (Panel 2.3d), log number of other corporate buyers (Panel 2.3e), and log average value of sales to other corporate buyers (Panel 2.3f). The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. These regressions do not include the vector of firm-level time-varying characteristics, X_{it}, but include firm and four-digit sector $\times$ province $\times$ calendar year fixed effects. The vertical lines reflect the 95% confidence intervals. The coefficients plotted correspond to columns (1)-(2) in Table 2.2 and columns (1)-(4) in Table 2.3, obtained from the sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017.



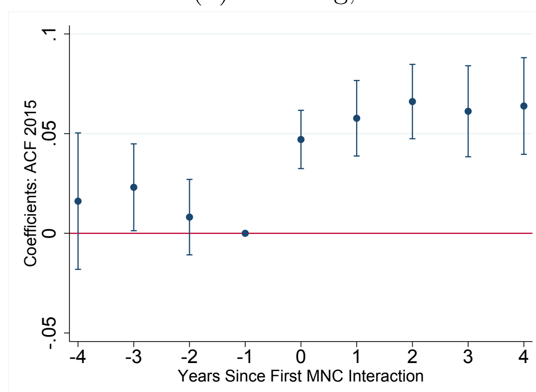
(a) TFP Index, OLS



(b) Translog, OLS



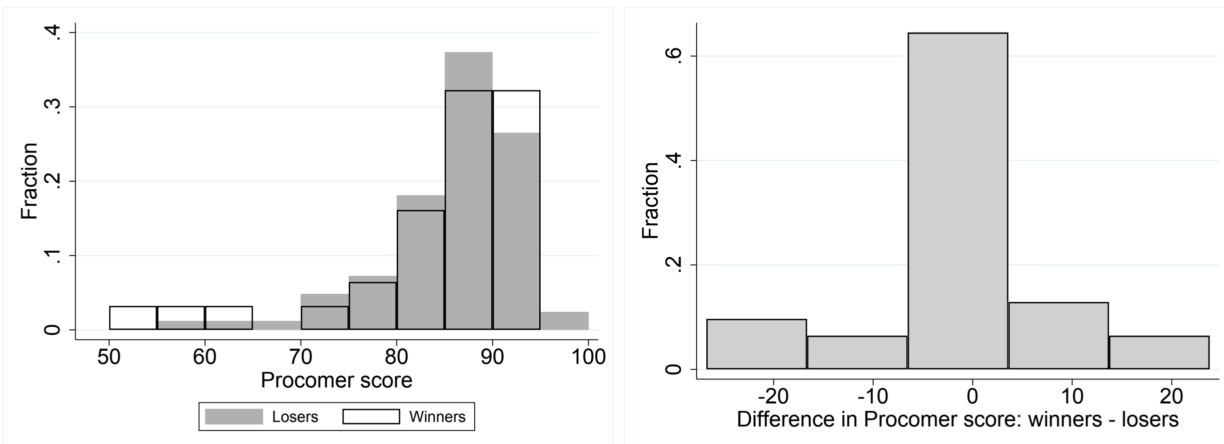
(c) Levinsohn and Petrin (2003)



(d) Ackerberg et al. (2015)

Figure 2.4: Domestic Firms Improve Their TFP after Starting to Supply to MNCs

Notes: Figure 2.4 plots the estimated θ_k event-study coefficients from specification (2.1) adapted to four measures of TFP. In Panel 2.4a we use as dependent variable a TFP index constructed assuming a Cobb-Douglas production function. This method “residualizes" sales by subtracting firm-level inputs used, weighted by the respective two-digit-level cost shares. Panels 2.4b use measures of TFP resulting from OLS production function estimation, under the translog functional form assumption. Panels 2.4c and 2.4d estimate TFP using the methods proposed by Levinsohn and Petrin (2003) and Ackerberg, Caves, and Frazer (2015). The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. The vertical lines reflect the 95% confidence intervals. The coefficients plotted correspond to columns (1), (3), (4), and (5) in Table 2.4 obtained from the sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017.

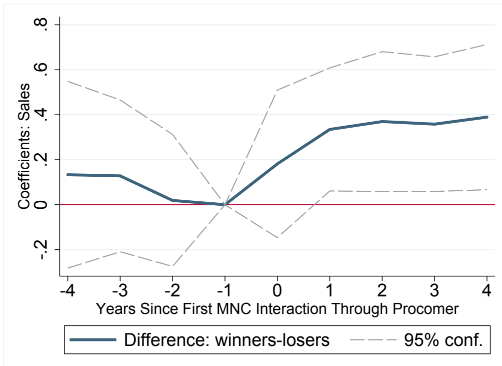


(a) Distributions of Scores

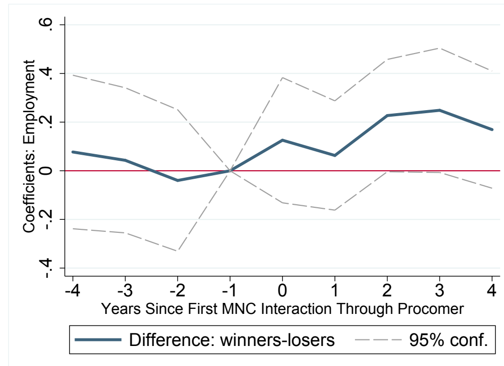
(b) Within-Deal Differences in Scores

Figure 2.5: Robustness Check: Scores of Firms in the "Productive Linkages" Program

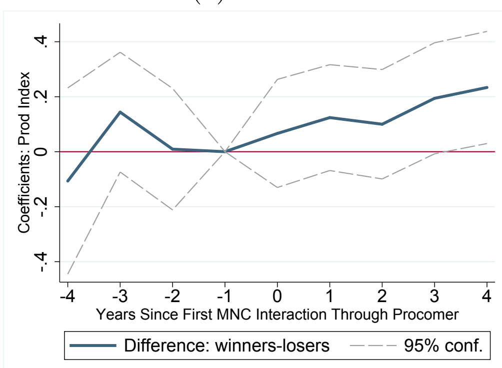
Notes: Figure 2.5 compares the Procomer scores of winning and losing firms in our sample of first-time deals with MNCs mediated through the "Productive Linkages" program of Procomer. Panel 2.5a shows the histogram of Procomer scores for winners (white bars) and losers (grey bars). Panel 2.5 presents a histogram of differences between winner and loser scores. This difference is constructed by subtracting from the score of the winner the average score of the losing contenders to the same deal. These histograms characterize the sample of 31 "Productive Linkages" deals, involving 31 winners and 84 losers. This exercise is part of a robustness check to the baseline event-study results plotted in Figures 2.2, 2.3, 2.4 and 2.7.



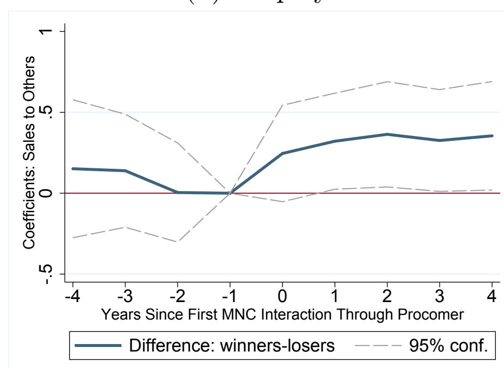
(a) Total Sales



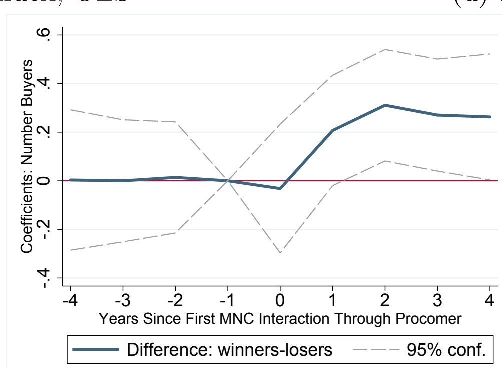
(b) Employment



(c) TFP Index, OLS



(d) Sales to Others



(e) Number of Other Corporate Buyers

Figure 2.6: Robustness Check: Domestic Firms Improve their Performance after First "Productive Linkages" Deal

Notes: Figure 2.6 plots the estimated θ_k^{Diff} event-study coefficients from a regression of the form given in equation (2.2), where the dependent variable is, in turn, log total sales (Panel 2.6a), log employment (Panel 2.6b), log TFP index (Panel 2.6c), log sales to others (Panel 2.6d), and log number of other corporate buyers (Panel 2.6e). The event is defined as the first time a domestic firm experiences a deal with an MNC buyer, mediated by the "Productive Linkages" program. θ_{-1}^{Diff}, the coefficient of the year prior to the event, is normalized to zero. The dashed lines delimit the 95% confidence intervals. The coefficients plotted correspond to columns (1)-(5) in Table 2.5. These regressions are run on the sample of 31 "Productive Linkages" deals, involving 31 winners and 84 losers. This exercise is part of a robustness check to the baseline event-study results plotted in Figures 2.2, 2.3, 2.4 and 2.7.

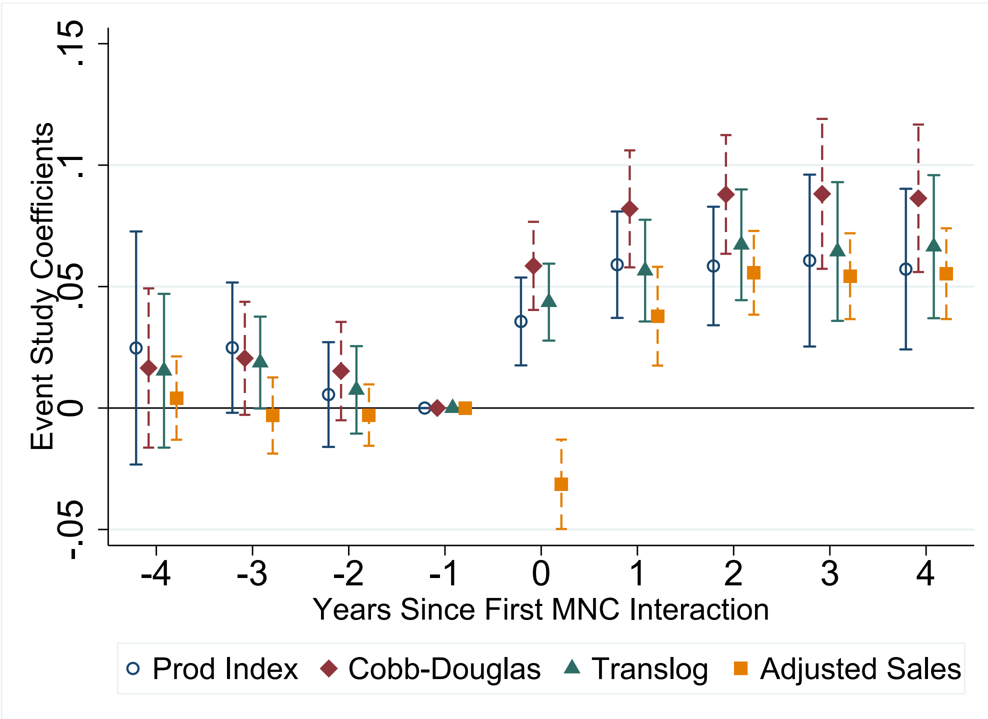


Figure 2.7: Standard Measures of TFP vs. Model-Based Measure of Composite TFP

Notes: Figure 2.7 plots the estimated θ_k event-study coefficients from specification (2.1) adapted to four different measures of TFP. The circular, rhomboid, and triangular sequences pertain to standard measures of TFP. "Prod Index" is the TFP index that uses as dependent variable a residualized version of sales. "Cobb-Douglas" and "Translog" come from OLS production function estimations assuming a Cobb-Douglas and translog specification for the production function. These three sets of coefficients can be found (in order) in columns (1), (2), and (3) of Table 2.4. The rectangular markers ("Adjusted Sales") depict the evolution of our model-based estimates of changes in *composite TFP* (which, in our model, encompasses true TFP, reputation, and their interaction). These estimates are the empirical application of Result 1, which states that changes in adjusted sales to others are informative on changes in composite TFP. The adjustment controls for both potential returns to scale and effects of the MNC demand shock on prices (via a parameter, δ). These model-based estimates pertain to our preferred values for $\delta - 0.22$ and the elasticity of demand $\sigma = 6$. These estimates can be found in Column (1) of Table 2.6. The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. The vertical lines reflect the 95% confidence intervals. For direct comparability all the four sequences of event-study coefficients use total sales (to others) from corporate income tax returns data. Also, all estimates are obtained from the sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017.

Tables

Table 2.1: Domestic Firms Increase Their Scale after Starting to Supply to MNCs

	Sales (1)	Employment (2)	Capital (3)	Materials (4)	Sales (5)	Employment (6)	Capital (7)	Materials (8)
4 years before event	0.044	0.009	-0.017	-0.004	-0.022	-0.054	-0.067	0.003
	(0.028)	(0.023)	(0.052)	(0.047)	(0.053)	(0.049)	(0.053)	(0.069)
3 years before event	0.029	-0.004	-0.016	0.032	0.001	-0.027	-0.049	0.057
	(0.023)	(0.017)	(0.041)	(0.037)	(0.041)	(0.035)	(0.044)	(0.049)
2 years before event	0.026	0.005	0.006	0.025	0.007	-0.010	-0.005	0.036
	(0.018)	(0.013)	(0.028)	(0.021)	(0.023)	(0.019)	(0.025)	(0.030)
Year of event	0.159***	0.063***	0.029	0.093***	0.191***	0.088***	0.092***	0.110***
	(0.019)	(0.022)	(0.020)	(0.024)	(0.021)	(0.019)	(0.027)	(0.026)
1 year after event	0.325***	0.242***	0.140***	0.227***	0.377***	0.286***	0.212***	0.252***
	(0.028)	(0.028)	(0.031)	(0.029)	(0.035)	(0.031)	(0.045)	(0.044)
2 years after event	0.351***	0.272***	0.189***	0.237***	0.408***	0.317***	0.281***	0.255***
	(0.032)	(0.033)	(0.038)	(0.041)	(0.054)	(0.046)	(0.063)	(0.072)
3 years after event	0.342***	0.272***	0.213***	0.214***	0.389***	0.313***	0.321***	0.241**
	(0.035)	(0.035)	(0.037)	(0.050)	(0.072)	(0.061)	(0.076)	(0.095)
4 years after event	0.334***	0.256***	0.217***	0.234***	0.382***	0.295***	0.336***	0.252**
	(0.037)	(0.036)	(0.048)	(0.058)	(0.089)	(0.074)	(0.095)	(0.115)
Mean Dep. Var. (level)	0.85	13.2	2.93	0.78	1.45	18.9	0.96	1.40
SD Dep. Var. (level)	2.54	32.6	712.8	2.68	4.50	45.1	3.91	4.74
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	Yes	No	No	No	No
Adjusted R^2	0.77	0.74	0.81	0.83	0.80	0.77	0.82	0.86
# Observations	116,683	116,683	94,038	67,194	23,961	23,961	21,792	14,199
# Fixed Effects	25,174	25,174	21,480	15,894	7,366	7,366	7,019	4,870
# Firms	18,035	18,035	14,804	10,834	3,482	3,482	3,287	2,195

Notes: Table 2.1 shows the results of running the event-study specification (2.1) adapted to four dependent variables capturing firm size: log total sales, log total number of workers, log net assets, and log input costs. The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. These regressions do not include the vector of firm-level time-varying characteristics, X_{it}, but include firm and four-digit sector $\times$ province $\times$ calendar year fixed effects. Columns (1)-(4) pertain to the full sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017. Clustering of standard errors is at the two-digit sector by province level. Columns (5)-(8) focus only on the restricted sample of domestic firms becoming first-time suppliers to an MNC between 2010 and 2015 and use standard error clustering at event by province level. For sales, net assets, and input costs, means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.2: Domestic Firms Improve Their Sales to Others

	Total Sales (1)	Sales to Others (2)	Sales to Others Untreated (3)	Total Sales (4)	Sales to Others (5)	Sales to Others Untreated (6)
4 years before event	0.044	0.011	0.014	-0.022	-0.047	-0.034
	(0.028)	(0.042)	(0.042)	(0.053)	(0.119)	(0.124)
3 years before event	0.029	-0.022	-0.021	0.001	-0.041	-0.037
	(0.023)	(0.035)	(0.036)	(0.041)	(0.076)	(0.078)
2 years before event	0.026	-0.020	-0.021	0.007	-0.028	-0.026
	(0.018)	(0.028)	(0.029)	(0.023)	(0.036)	(0.037)
Year of event	0.159***	-0.193***	-0.189***	0.191***	-0.122*	-0.125*
	(0.019)	(0.052)	(0.051)	(0.021)	(0.062)	(0.063)
1 year after event	0.325***	0.118**	0.122**	0.377***	0.205**	0.201**
	(0.028)	(0.053)	(0.052)	(0.035)	(0.090)	(0.092)
2 years after event	0.351***	0.201***	0.199***	0.408***	0.320***	0.308**
	(0.032)	(0.045)	(0.049)	(0.054)	(0.115)	(0.119)
3 years after event	0.342***	0.196***	0.203***	0.389***	0.333**	0.326**
	(0.035)	(0.046)	(0.044)	(0.072)	(0.147)	(0.154)
4 years after event	0.334***	0.203***	0.204***	0.382***	0.380**	0.358*
	(0.037)	(0.049)	(0.048)	(0.089)	(0.171)	(0.181)
Mean Dep. Var. (level)	0.85	0.84	0.84	1.45	1.42	1.40
SD Dep. Var. (level)	2.54	2.54	2.52	4.50	4.51	4.47
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	No	No	No
Adjusted R^2	0.77	0.70	0.69	0.80	0.64	0.63
# Observations	116,683	116,683	116,683	23,961	23,961	23,961
# Fixed Effects	25,174	25,174	25,174	7,366	7,366	7,366
# Firms	18,035	18,035	18,035	3,482	3,482	3,482

Notes: Table 2.2 shows the results of running the event-study specification (2.1) adapted to three dependent variables: log total sales (across all buyers, including the first MNC buyer), log sales to others (all buyers with the exception of the first MNC buyer), and log sales to others untreated (across all buyers with the exception of the first MNC buyer and other buyers that started supplying to MNCs themselves). The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. These regressions do not include the vector of firm-level time-varying characteristics, X_{it}, but include firm and four-digit sector $\times$ province $\times$ calendar year fixed effects. Columns (1)-(3) pertain to the full sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017. Clustering of standard errors is at the two-digit sector by province level. Columns (4)-(6) focus only on the restricted sample of domestic firms becoming first-time suppliers to an MNC between 2010 and 2015 and use standard error clustering at event by province level. Means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.3: Domestic Firms Increase Their Corporate Sales to Others

	Total Corp Sales (1)	Corp Sales Others (2)	Number Other Buyers (3)	Av. Sales Other Buyers (4)	Total Corp Sales (5)	Corp Sales Others (6)	Number Other Buyers (7)	Av. Sales Other Buyers (8)
4 years before event	0.040	0.016	-0.034	0.034	-0.051	-0.139	-0.037	-0.096
	(0.073)	(0.082)	(0.024)	(0.058)	(0.072)	(0.148)	(0.039)	(0.137)
3 years before event	0.020	0.010	-0.007	0.014	-0.029	-0.103	-0.007	-0.088
	(0.035)	(0.045)	(0.018)	(0.035)	(0.053)	(0.100)	(0.024)	(0.094)
2 years before event	0.042	0.032	-0.009	0.023	-0.001	-0.029	-0.012	-0.031
	(0.025)	(0.033)	(0.015)	(0.027)	(0.036)	(0.045)	(0.016)	(0.048)
Year of event	0.270***	-0.747***	0.015	-0.778***	0.290***	-0.636***	0.013	-0.667***
	(0.032)	(0.091)	(0.019)	(0.083)	(0.028)	(0.074)	(0.019)	(0.071)
1 year after event	0.448***	0.159***	0.251***	-0.068	0.491***	0.295***	0.241***	0.069
	(0.042)	(0.056)	(0.023)	(0.047)	(0.047)	(0.095)	(0.030)	(0.089)
2 years after event	0.458***	0.318***	0.319***	0.035	0.520***	0.484***	0.300***	0.202*
	(0.038)	(0.066)	(0.025)	(0.056)	(0.061)	(0.121)	(0.041)	(0.112)
3 years after event	0.477***	0.481***	0.349***	0.173***	0.552***	0.683***	0.324***	0.370**
	(0.041)	(0.067)	(0.025)	(0.056)	(0.072)	(0.164)	(0.051)	(0.161)
4 years after event	0.438***	0.448***	0.356***	0.141**	0.534***	0.704***	0.327***	0.383*
	(0.039)	(0.064)	(0.029)	(0.057)	(0.089)	(0.201)	(0.062)	(0.191)
Mean Dep. Var. (level)	0.39	0.37	7.94	0.038	0.59	0.56	16.8	0.033
SD Dep. Var. (level)	1.20	1.21	29.1	0.056	1.79	1.81	53.8	0.045
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	Yes	No	No	No	No
Adjusted R^2	0.75	0.63	0.86	0.57	0.74	0.59	0.84	0.51
# Observations	63,793	63,793	63,793	63,793	21,200	21,200	21,200	21,200
# Fixed Effects	16,833	16,833	16,833	16,833	6,925	6,925	6,925	6,925
# Firms	10,985	10,985	10,985	10,985	3,379	3,379	3,379	3,379

Notes: Table 2.3 uses only firm-to-firm transaction data and shows the results of running the event-study specification (2.1) adapted to four dependent variables: log total sales to corporate buyers (including the first MNC buyer), log sales to corporate buyers other than the first MNC buyer, log number of other corporate buyers + 1 (number of corporate buyers tracked by the firm-to-firm transaction data, excluding the first MNC buyer, + 1), and log average sales to other corporate buyers (total sales to other corporate buyers, divided by the number of other corporate buyers +1). The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. These regressions do not include the vector of firm-level time-varying characteristics, X_{it}, but include firm and four-digit sector × province × calendar year fixed effects. Columns (1)-(4) correspond to the full economy-wide sample (including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017), columns (5)-(8) correspond to the restricted economy-wide sample (including only first-time suppliers to MNCs). Except for the number of buyers, means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.4: Domestic Firms Improve in Standard Measures of TFP

	Prod Index (1)	CD OLS (2)	TL OLS (3)	LP (4)	ACF (5)	Prod Index (6)	CD OLS (7)	TL OLS (8)	LP (9)	ACF (10)
4 years before event	0.025	0.016	0.015	0.020	0.016	-0.009	-0.012	0.017	0.028	0.027
	(0.024)	(0.017)	(0.016)	(0.018)	(0.017)	(0.021)	(0.022)	(0.018)	(0.023)	(0.020)
3 years before event	0.025*	0.020*	0.019*	0.028**	0.023**	-0.002	-0.004	0.020	0.034*	0.032*
	(0.014)	(0.012)	(0.010)	(0.011)	(0.011)	(0.015)	(0.015)	(0.015)	(0.017)	(0.016)
2 years before event	0.006	0.015	0.007	0.011	0.008	-0.002	0.010	0.010	0.015	0.013
	(0.011)	(0.010)	(0.009)	(0.010)	(0.010)	(0.012)	(0.013)	(0.011)	(0.013)	(0.011)
Year of event	0.036***	0.059***	0.044***	0.061***	0.047***	0.040***	0.061***	0.041***	0.060***	0.043***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)	(0.011)	(0.009)	(0.007)	(0.008)	(0.008)
1 year after event	0.059***	0.082***	0.057***	0.067***	0.058***	0.072***	0.090***	0.051***	0.068***	0.053***
	(0.011)	(0.012)	(0.011)	(0.011)	(0.010)	(0.015)	(0.013)	(0.012)	(0.013)	(0.013)
2 years after event	0.058***	0.088***	0.067***	0.078***	0.066***	0.076***	0.097***	0.054***	0.064***	0.050***
	(0.012)	(0.012)	(0.012)	(0.010)	(0.010)	(0.020)	(0.017)	(0.017)	(0.017)	(0.017)
3 years after event	0.061***	0.088***	0.064***	0.074***	0.061***	0.080***	0.101***	0.049**	0.056**	0.041*
	(0.018)	(0.016)	(0.014)	(0.012)	(0.012)	(0.026)	(0.021)	(0.020)	(0.021)	(0.021)
4 years after event	0.057***	0.086***	0.066***	0.076***	0.064***	0.083**	0.099***	0.043*	0.047*	0.036
	(0.017)	(0.015)	(0.015)	(0.013)	(0.012)	(0.031)	(0.027)	(0.025)	(0.026)	(0.027)
Mean Dep. Var. (level)	0.93	1.12	1.12	1.12	1.12	0.90	2.00	2.00	2.00	2.00
SD Dep. Var. (level)	0.56	3.17	3.17	3.17	3.17	0.52	5.74	5.74	5.74	5.74
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Adjusted R^2	0.72	0.95	0.97	0.63	0.62	0.74	0.96	0.97	0.64	0.64
# Observations	64,419	64,419	64,419	64,419	64,419	13,706	13,706	13,706	13,706	13,706
# Fixed Effects	15,464	15,464	15,464	15,464	15,464	4,774	4,774	4,774	4,774	4,774
# Firms	10,492	10,492	10,492	10,492	10,492	2,144	2,144	2,144	2,144	2,144

Notes: Table 2.4 shows the results of running the event-study specification (2.1) adapted to five measures of TFP. The event is defined as a first time sale to an MNC. Columns (1) and (6) use as dependent variable a TFP index constructed under the assumption a Cobb-Douglas production function. This method “residualizes" sales by subtracting firm-level inputs used, weighted by the respective two-digit-level cost shares. Columns (2) and (7) use a measure of TFP resulting from OLS production function estimation. These columns assume a Cobb-Douglas technology, with revenues (CPI-deflated to 2013 U.S. dollars) as the output measure and total net assets, number of workers, and input costs as input measures for K, L, and M respectively. Columns (3) and (8) differ from columns (2) and (7) in their assumption of a translog functional form. For both Cobb-Douglas and translog, we estimate the coefficients on factors of production over the entire sample of domestic firms, controlling for narrowly defined fixed effects. Columns (4) and (9) show results of production function estimation following Levinsohn and Petrin (2003). Columns (5) and (10) show results of production function estimation following Ackerberg, Caves, and Frazer (2015). θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. Columns (1)-(5) report-event study estimates for the sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017. Clustering of standard errors is at the two-digit sector by province level. Columns (6)-(10) focus only on the sample of domestic firms becoming first-time suppliers to an MNC and use standard error clustering at event by province level. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.5: Robustness Check: Domestic Firms Improve their Performance after First "Productive Linkages" Deal

	Employment (1)	Total Sales (2)	Productivity Index (3)	Sales to Others (4)	Number of Other Buyers (5)
Losers (θ_k^L)					
4 years before event	-0.145	-0.199	-0.038	-0.216	-0.135
	(0.204)	(0.277)	(0.195)	(0.281)	(0.160)
3 years before event	-0.100	-0.119	-0.037	-0.126	-0.071
	(0.151)	(0.205)	(0.124)	(0.209)	(0.117)
2 years before event	-0.074	-0.048	0.057	-0.057	-0.019
	(0.102)	(0.133)	(0.085)	(0.135)	(0.085)
Years of event	-0.040	-0.010	0.018	-0.005	-0.007
	(0.103)	(0.123)	(0.066)	(0.124)	(0.080)
1 year after event	-0.038	-0.038	-0.010	-0.039	0.017
	(0.127)	(0.179)	(0.114)	(0.181)	(0.103)
2 years after event	-0.116	-0.101	0.025	-0.097	-0.011
	(0.183)	(0.250)	(0.168)	(0.254)	(0.144)
3 years after event	-0.137	0.018	-0.017	0.020	0.020
	(0.238)	(0.323)	(0.224)	(0.329)	(0.185)
4 years after event	-0.074	0.041	0.005	0.041	0.043
	(0.286)	(0.386)	(0.273)	(0.393)	(0.219)
Winners-Losers (θ_k^{Diff})					
4 years before event	0.077	0.133	-0.107	0.151	0.004
	(0.161)	(0.212)	(0.173)	(0.218)	(0.147)
3 years before event	0.043	0.128	0.144	0.139	-0.012
	(0.152)	(0.172)	(0.111)	(0.178)	(0.128)
2 years before event	-0.040	0.019	0.009	0.004	0.011
	(0.148)	(0.150)	(0.113)	(0.156)	(0.117)
Years of event	0.126	0.182	0.066	0.246	-0.001
	(0.131)	(0.167)	(0.100)	(0.152)	(0.136)
1 year after event	0.063	0.335**	0.124	0.322**	0.215*
	(0.115)	(0.140)	(0.098)	(0.151)	(0.117)
2 years after event	0.227*	0.370**	0.100	0.364**	0.312***
	(0.118)	(0.159)	(0.102)	(0.166)	(0.117)
3 years after event	0.249*	0.358**	0.194*	0.326**	0.280**
	(0.130)	(0.153)	(0.103)	(0.161)	(0.118)
4 years after event	0.169	0.389**	0.234**	0.355**	0.265**
	(0.123)	(0.165)	(0.104)	(0.171)	(0.132)
Firm FE	YES	YES	YES	YES	YES
Deal FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Adjusted R^2	0.88	0.83	0.37	0.83	0.90
# Observations	1,097	1,111	1,087	1,100	1,101
# Winners	31	31	31	31	31
# Losers	84	84	83	83	83

Notes: Table 2.5 shows the results of running the event-study specification (2.2) adapted to five dependent variables: log total sales, log employment, log TFP index, log sales to others, and log number of other corporate buyers. We report the estimates for both the θ_k^L and θ_k^{Diff} coefficients, which measure the effects of the event on the outcomes of losers and on the difference between the outcomes of the winner and losers' to a deal, respectively. The event is defined as the first time a domestic firm experiences a deal with an MNC buyer, mediated by the "Productive Linkages" program. These regressions are run on the sample of 31 "Productive Linkages" deals, involving 31 winners and 84 losers. θ_{-1}, the coefficients of the year prior to a first sale to an MNC, are normalized to zero. All regressions include firm, deal, and year fixed effects. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively. This exercise is part of a robustness check to the baseline event-study results presented in Tables 2.1 to 2.4 and 2.6 to 2.8.

Table 2.6: Model-Based Estimates of Gains in Composite TFP and TFP Alone after Domestic Firms Start Supplying to MNCs

Result	1	1	2	1	1	2
Source of Sales to Others	Bal. Sh.	Trans.	Trans.	Bal. Sh.	Trans.	Trans.
	(1)	(2)	(3)	(4)	(5)	(6)
4 years before event	0.004	0.005	0.008	-0.010	-0.029	-0.021
	(0.009)	(0.019)	(0.014)	(0.025)	(0.031)	(0.028)
3 years before event	-0.003	0.003	0.004	-0.008	-0.021	-0.018
	(0.008)	(0.010)	(0.008)	(0.016)	(0.021)	(0.020)
2 years before event	-0.003	0.008	0.007	-0.005	-0.005	-0.006
	(0.006)	(0.007)	(0.006)	(0.008)	(0.010)	(0.010)
Year of event	-0.031***	-0.136***	-0.142***	-0.016	-0.113***	-0.119***
	(0.010)	(0.019)	(0.017)	(0.013)	(0.015)	(0.014)
1 year after event	0.038***	0.052***	0.006	0.058***	0.080***	0.035*
	(0.011)	(0.013)	(0.011)	(0.019)	(0.020)	(0.018)
2 years after event	0.056***	0.084***	0.027**	0.082***	0.119***	0.063***
	(0.010)	(0.015)	(0.013)	(0.024)	(0.026)	(0.023)
3 years after event	0.054***	0.117***	0.056***	0.084**	0.160***	0.098***
	(0.010)	(0.015)	(0.012)	(0.031)	(0.034)	(0.033)
4 years after event	0.055***	0.109***	0.047***	0.093**	0.163***	0.099**
	(0.011)	(0.014)	(0.012)	(0.036)	(0.042)	(0.039)
Mean Dep. Var.	1.27	0.98	0.63	1.32	1.03	0.60
SD Dep. Var.	0.31	0.50	0.40	0.44	0.68	0.56
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	No	No	No
Adjusted R^2	0.72	0.65	0.59	0.67	0.61	0.53
# Observations	116,536	63,078	63,078	23,801	20,491	20,491
# Fixed Effects	7,132	5,794	5,794	3,860	3,451	3,451
# Firms	18,024	10,895	10,895	3,468	3,291	3,291

Notes: Table 2.6 implements Results 1 and 2 for our preferred values of $\delta = -0.22$ and $\sigma = 6$. Results 1 and 2 propose model-based formulas for changes in composite TFP and TFP. The first line in the column title specifies the result whose empirical application we report in that column. The second line in the column title indicates the main data source used to construct the dependent variable. "Bal. Sh." stands for balance sheet and refers to the construction of sales to others as the total sales from balance sheet data (specifically, corporate income tax returns data), from which we subtract the amounts sold to the first MNC buyer. "Trans" refers to the firm-to-firm transaction data, which is used to construct the total sales to other corporate buyers. Note that Result 2 calls for the use of firm-to-firm transaction data, where we can observe the number of corporate buyers. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. These regressions do not include the vector of firm-level time-varying characteristics, X_{it}, but include firm and four-digit sector × province × calendar year fixed effects. Columns (1)-(3) correspond to the full economy-wide sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017. Columns (4)-(6) focus only on the restricted sample of domestic firms becoming first-time suppliers to an MNC between 2010 and 2015. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.7: Heterogeneity in Performance Gains Based on Domestic Firm (MNC) Sector and FTZ Status (MNCs only)

	DOM Low-Tech (1)	DOM High-Tech (2)	DOM Low-Tech (3)	DOM High-Tech (4)	MNC Low-Tech (5)	MNC High-Tech (6)	MNC Not in FTZ (7)	MNC In FTZ (8)
4 years before event	0.03*	-0.07	-0.00	-0.08	-0.00	-0.02	0.02	-0.06
	(0.02)	(0.07)	(0.03)	(0.11)	(0.04)	(0.06)	(0.04)	(0.05)
3 years before event	0.02	-0.05	-0.01	-0.03	-0.01	0.00	0.01	-0.03
	(0.01)	(0.06)	(0.02)	(0.09)	(0.03)	(0.04)	(0.03)	(0.04)
2 years before event	0.01	-0.02	0.00	0.03	0.00	0.01	0.02	-0.01
	(0.01)	(0.05)	(0.01)	(0.06)	(0.02)	(0.03)	(0.02)	(0.03)
Year of event	0.03***	0.03	0.04***	0.02	0.03*	0.09***	0.02	0.09***
	(0.01)	(0.04)	(0.01)	(0.05)	(0.02)	(0.03)	(0.02)	(0.03)
1 year after event	0.06***	0.05	0.08***	0.02	0.06**	0.12***	0.04*	0.11***
	(0.01)	(0.04)	(0.02)	(0.08)	(0.02)	(0.04)	(0.03)	(0.04)
2 years after event	0.06***	0.04	0.08***	0.01	0.07**	0.11**	0.05	0.11**
	(0.01)	(0.04)	(0.03)	(0.11)	(0.03)	(0.05)	(0.04)	(0.05)
3 years after event	0.05***	0.07*	0.09**	0.03	0.08*	0.14*	0.03	0.16**
	(0.01)	(0.04)	(0.04)	(0.14)	(0.04)	(0.07)	(0.05)	(0.07)
4 years after event	0.05***	0.07	0.10**	0.02	0.07	0.15*	0.03	0.18**
	(0.01)	(0.04)	(0.05)	(0.17)	(0.05)	(0.09)	(0.06)	(0.08)
Mean Dep. Var. (level)	0.90	1.28	0.87	1.23	0.90	0.96	0.88	0.97
SD Dep. Var. (level)	0.53	0.69	0.51	0.65	0.55	0.50	0.53	0.54
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-2DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	No	No	No	No	No	No
Adjusted R^2	0.69	0.73	0.71	0.72	0.73	0.77	0.74	0.70
# Observations	60,497	5,762	13,376	2,111	11,933	2,925	10,476	4,340
# Fixed Effects	11,024	1,813	3,009	792	3,020	993	2,678	1,408
# Firms	9,673	1,088	1,982	395	1,819	479	1,579	704

Notes: Table 2.7 shows the results of running the event-study specification (2.1) adapted to the TFP index (constructed under the assumption of a Cobb-Douglas production function) as the dependent variable. All regressions have the same dependent variable, but differ in the sample over which the regression is run. Columns (1) and (4) separate domestic firms (DOM) based on the sector of the domestic firm and whether the OECD classifies this sector as high- or low-tech. The OECD classifies manufacturing sectors as high-tech, medium high-tech, medium low-tech or low-tech, and service sectors as high- or low-knowledge intensive. Manufacturing sectors that are high-tech or medium high-tech, and service sectors that are high-knowledge intensive are labeled as *high-tech*, all others as *low-tech*. Columns (5)-(8) separate domestic firms based on characteristics of the first MNC buyer. This second separation can only be done in the restricted sample (as never-suppliers do not have a first MNC buyer). Columns (5) and (6) separate domestic firms based on whether the sector of their first MNC buyer is high- or low-tech, whereas columns (7) and (8) separate domestic firms based on whether their first MNC buyer was part of a Free Trade Zone (FTZ) or not. The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. These regressions do not include the vector of firm-level time-varying characteristics, X_{it}, but include firm and two-digit sector × province × calendar year fixed effects. Columns (1) and (2) pertain to the full sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017. Columns (3)-(8) use the restricted sample, including only first-time suppliers. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.8: Heterogeneity in Performance Gains Based on Domestic Firm (MNC) Sector

	DOM MFG (1)	DOM RET (2)	DOM SER (3)	DOM AGR (4)	DOM MFG (5)	DOM RET (6)	DOM SER (7)	DOM AGR (8)	MNC MFG (9)	MNC RET (10)	MNC SER (11)	MNC AGR (12)
4 years before event	-0.03	0.02	0.06	0.06	-0.04	-0.02	0.08	-0.11	-0.05	-0.07	-0.00	0.04
	(0.04)	(0.02)	(0.04)	(0.09)	(0.08)	(0.03)	(0.09)	(0.15)	(0.05)	(0.08)	(0.06)	(0.10)
3 years before event	-0.03	0.01	0.04	0.04	-0.02	-0.01	0.04	-0.11	-0.08**	-0.01	-0.00	0.05
	(0.03)	(0.01)	(0.04)	(0.08)	(0.05)	(0.02)	(0.07)	(0.11)	(0.04)	(0.05)	(0.04)	(0.08)
2 years before event	-0.00	0.01	-0.00	0.01	0.00	0.00	0.03	-0.06	-0.00	-0.00	-0.01	0.04
	(0.03)	(0.01)	(0.03)	(0.06)	(0.04)	(0.01)	(0.04)	(0.08)	(0.02)	(0.03)	(0.03)	(0.05)
Year of event	0.04**	0.04***	0.04*	-0.06	0.06*	0.05***	0.02	0.00	0.08***	0.01	0.03	0.02
	(0.02)	(0.01)	(0.03)	(0.05)	(0.03)	(0.01)	(0.04)	(0.08)	(0.02)	(0.03)	(0.03)	(0.05)
1 year after event	0.10***	0.05***	0.06**	-0.04	0.13**	0.07***	0.02	0.08	0.12***	0.03	0.07*	-0.02
	(0.02)	(0.01)	(0.02)	(0.06)	(0.05)	(0.02)	(0.06)	(0.11)	(0.03)	(0.05)	(0.04)	(0.08)
2 years after event	0.09***	0.05***	0.06**	-0.03	0.13*	0.08**	0.01	0.15	0.13***	0.04	0.08	0.01
	(0.02)	(0.01)	(0.03)	(0.06)	(0.07)	(0.03)	(0.08)	(0.15)	(0.05)	(0.07)	(0.05)	(0.10)
3 years after event	0.07**	0.05***	0.08***	0.03	0.12	0.08**	0.01	0.23	0.16***	0.05	0.07	-0.06
	(0.03)	(0.01)	(0.03)	(0.07)	(0.09)	(0.04)	(0.11)	(0.20)	(0.06)	(0.09)	(0.07)	(0.12)
4 years after event	0.11***	0.04***	0.06*	0.02	0.18*	0.08	-0.02	0.24	0.17**	0.04	0.07	-0.03
	(0.03)	(0.01)	(0.03)	(0.09)	(0.11)	(0.05)	(0.13)	(0.25)	(0.07)	(0.11)	(0.08)	(0.16)
Mean Dep. Var. (level)	0.96	0.73	1.22	1.16	0.96	0.74	1.22	1.00	0.92	0.86	0.94	0.82
SD Dep. Var. (level)	0.41	0.34	0.67	0.91	0.44	0.33	0.72	0.73	0.54	0.44	0.54	0.64
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-2DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	Yes	No	No	No	No	No	No	No	No
Adjusted R^2	0.60	0.57	0.71	0.67	0.60	0.57	0.75	0.68	0.73	0.69	0.77	0.79
# Observations	9,806	33,550	17,998	4,929	2,792	7,836	3,822	1,039	5,904	2,920	4,489	837
# Fixed Effects	2,076	5,374	4,498	894	910	1,306	1,340	246	1,797	957	1,407	314
# Firms	1,424	5,164	3,389	788	396	1,099	722	161	923	451	716	120

Notes: Table 2.8 shows the results of running the event-study specification (2.1) adapted to the TFP index (constructed under the assumption of a Cobb-Douglas production function) as the dependent variable. All regressions have the same dependent variable, but differ in the sample over which the regression is run. Columns (1)-(8) separate firms based on the sector of the domestic firm (DOM). The four largest sectoral groups are manufacturing (MFG), retail (including repair and maintenance, RET), services (SER), and agriculture (AGR). Columns (9)-(12) separate firms based on the sector of the first MNC buyer. The event is defined as a first time sale to an MNC. θ_{-1}, the coefficient of the year prior to a first sale to an MNC, is normalized to zero. These regressions do not include the vector of firm-level time-varying characteristics, X_{it}, but include firm and two-digit sector × province × calendar year fixed effects. Columns (1)-(4) pertain to the full sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC between 2008 and 2017. Columns (5)-(12) focus only on the restricted sample of first-time suppliers. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

2.8 Transitional Section

In this chapter of my dissertation, I have studied the effects of foreign MNC affiliates in Costa Rica on domestic firms engaged in supplying relationships with these affiliates. The purpose of this chapter was to bring novel evidence – that leverages uniquely rich administrative data – on the existence, size and mechanisms of these effects. This paper contributes to an extensive literature that has singled out the effects on domestic suppliers as the most likely spillover effects of MNCs on domestic firms.

In the following chapter, I switch my attention from the effects of foreign MNCs on domestic firms to those on workers. Workers are plausibly affected by the arrival and expansion of MNCs through both the product and labor markets. Yet, there is scant worker-level evidence on these effects. The Costa Rican context is again appropriate for this endeavor thanks to both its unique merge of administrative datasets and due to its small domestic market (which allows us to propose credibly exogenous variation in the size of MNCs).

Chapter 3

The Effects of Multinationals on Workers: Evidence from Costa Rica[1]

3.1 Introduction

Developed and developing countries alike make considerable efforts to attract foreign multinational corporations (MNCs). These efforts are particularly pronounced in developing countries, where high-wage, high-performance firms are scarce. In 72% of developing countries, MNCs are offered tax incentives, which have become only more generous over the past decade (World Bank, 2018). Most work on MNCs has examined their impact at the firm, industry, or macroeconomic level. In this paper, we study the effects of MNCs at the *worker* level. We consider both the effects on workers directly employed by MNCs and those indirectly exposed to MNCs in the domestic economy. Both effects are central to a complete assessment of the effectiveness and distributional implications of policies to attract MNCs.

Our study of the effects of MNCs on workers requires an empirical setting with two characteristics. First, to understand the incidence of MNCs on workers, one needs to identify which workers are affected by MNCs and through which channels. As MNCs bring jobs that pay a premium to their direct hires, they can also improve the outside options of workers in domestic firms by altering both the level and composition of labor demand. Moreover, MNCs can enhance the performance of domestic employers through firm-level input-output linkages. Shocks to firm performance may then pass through to wages. Therefore, to disentangle these effects, we would ideally like to observe both worker-firm and firm-firm matches at the level of an economy. Second, the decision of MNCs to expand or contract within an economy may be endogenous to labor and product market conditions that can directly influence worker outcomes. Hence, we need a context with credibly exogenous variation in the size of MNCs.

Costa Rica provides an empirical setting that meets both requirements. First, it allows us to assemble a unique data set combining matched employer-employee panel data with

[1] This paper is joint work of Alonso Alfaro-Urena, Isabela Manelici, and Jose Vasquez. All permissions to reprint this material as a chapter of the present dissertation have been obtained.

tax records on firm-to-firm transactions, annual corporate income tax returns, and firm-level foreign ownership data. The resulting dataset covers all formal workers and firms in the country. We complement these data with a nationally representative household survey with information on non-wage job attributes, and a survey we conducted with human resources executives at MNCs on their wage setting practices. These data enable us to shed light on the channels by which MNCs affect workers. Second, Costa Rica is a small developing country that has placed the attraction of MNCs at the top of its policy agenda. While MNC subsidiaries now employ a substantial share of workers in Costa Rica, they account for a negligible share of the global employment of their parent groups. Hence, we exploit variation in the growth of employment in MNC subsidiaries outside Costa Rica – this growth is correlated with the local growth in employment of MNC subsidiaries and is plausibly exogenous to labor and product market conditions in the country.

In the first part of the paper, we estimate the *direct effect* of being hired by an MNC on wages. To overcome potential selection effects, we compare the within-worker earnings changes during moves from domestic firms to MNCs to the changes during moves between domestic firms. The wages of workers who move from a domestic firm to an MNC increase 9% more on average than those of workers who move from one domestic firm to another. This MNC premium varies greatly across industries and is higher for workers with a college education than for those without (12% vs. 8%). Identification in this design requires movers not to select into firms based on shocks to their productivity. We corroborate this assumption by showing that workers who are about to experience a major wage gain by moving to an MNC show no pretrend in wages at their origin firm. Lastly, we instrument for the likelihood of a move from a domestic firm to an MNC by the contemporaneous expansion in employment *outside* of Costa Rica of MNCs with subsidiaries in the worker's labor market in Costa Rica. The IV estimate of the MNC premium is equal to 15%, with its 95% confidence interval including the 9% estimate from the movers design. Thus, both approaches yield comparable results.

Why would MNCs pay a wage premium? One possibility is that the premium compensates workers for undesirable job attributes. We find that MNC workers enjoy better in-kind and monetary benefits than workers in domestic firms while working a similar number of hours. We also show that MNCs have higher worker retention rates. Last, we find that while both MNCs and domestic firms face an upward-sloping labor supply, MNCs face a higher elasticity than domestic firms. Hence, if anything, MNCs appear to offer better amenities than domestic firms. Alternatively, MNCs may have to pay greater hiring and training costs than domestic firms or abide by MNC-wide wage setting policies (as in Hjort et al., 2019). Both possibilities are consistent with above-market wages. One way to investigate the plausibility of larger hiring and training costs is to control for firm characteristics that have been found to correlate with these costs, such as size and industry (Manning, 2011). These two controls explain about half of the MNC premium, with the remaining half being consistent with MNC-specific policies. Our survey results indicate that MNCs pay a higher wage to the same worker compared to domestic firms to avoid worker turnover, motivate the worker, and ensure cross-country pay fairness within the MNC.

In the second part of the paper, we study the *indirect effects* of MNCs on workers in domestic firms. There is growing evidence that workers' wages are affected not only by their productivity but also by their outside options in the labor market and by the performance of their employer (Beaudry et al., 2012; Card et al., 2018; Caldwell and Harmon, 2019). For this reason, we allow MNCs to affect both the outside options of workers in the labor market (by changing the level and composition of labor demand) and the performance of domestic employers (through firm-level input-output linkages).

We define two measures of exposure to MNCs: a labor market exposure and a firm-level exposure. We consider a labor market to be a two-digit industry within a given region. The labor market exposure measure is a weighted average of changes in MNC employment across all labor markets in the economy, where the weights reflect worker mobility flows between markets during the pre-period (2006 to 2008). We then scale each market-specific component in the labor market exposure sum by one plus the MNC wage premium. This last interaction is guided by the intuition that MNC expansions in industries with high MNC premia are likely to improve the outside options of workers in domestic firms more than similarly sized MNC expansions in industries with lower or no premia.

The firm-level exposure measure is based on firm-to-firm input-output linkages to MNCs. More precisely, it is a weighted sum of the growth rate of each MNC in the economy, weighted by the share of sales of the domestic firm going to that MNC (either directly or indirectly). We focus only on the buyer role of MNCs as both meta-analyses and Alfaro-Urena et al. (2019b) find that, by and large, MNCs affect the performance of their domestic suppliers only (as opposed to the performance of clients or competitors). Shocks to the size of MNC buyers are likely to result in shocks to both the demand and productivity of domestic firms. In the presence of frictions such as those driven by hiring and training costs, incumbent workers at domestic firms could extract part of the increase in rents generated by these shocks. To our knowledge, this is the first paper to explore the implications of shocks in the domestic production network on workers. Thus far, the empirical literature on domestic production networks has shown how shocks propagating through the network can impact firm-level and aggregate outcomes.[2]

We are interested in the causal effects of changes in the labor market and firm-level exposure to MNCs on workers' wages. OLS estimates, however, may be biased due to simultaneity and omitted variables. For instance, workers in a labor market may receive unobserved positive productivity shocks, which would lead to both expansions of MNCs and higher wages for workers in that market (independently of the MNC expansions). In such a case, OLS would overestimate the effect of increases in labor market exposure to MNCs on wages. Finally, OLS estimation of the firm-level exposure coefficient may also be biased if shocks to the productivity of workers in a given firm affect the growth of the direct or indirect MNC buyers from that firm. To address these concerns, we exploit the same variation in MNC employment in Costa Rica as that used for the IV estimation of the MNC premium,

[2]See Dhyne et al. (2018a); Bernard et al. (2019); Furusawa et al. (2017); Miyauchi (2018); Huneeus (2018); Demir et al. (2018).

namely the variation in MNC employment *outside* of Costa Rica for MNCs with subsidiaries in Costa Rica.

We find that MNC expansions have a positive and significant impact on the wages of workers in domestic firms. This impact manifests through both the labor market and firm-level exposure of the worker to MNCs. Our IV estimates imply that the growth rate of annual earnings of a worker experiencing a one standard deviation increase in either the labor market or the firm-level exposure to MNCs is 1.1 percentage points higher than that of an identical worker with no change in either MNC exposure. This increase is half of the average annual increase in real earnings during our period of study.

To explore the implications of our results for rent-sharing, we re-estimate our main empirical specification by replacing the firm-level change in exposure to MNCs with the change in value added per worker. We exploit our source of variation in firm performance – exogenous shocks to the size of a firm's direct and indirect MNC buyers – to estimate the pass-through of changes in value added per worker to wages (referred to as rent-sharing elasticity). Our estimate of 0.09 implies that for each extra dollar of value added per worker, incumbent employees see their salaries increase by 9 cents. Existing studies report estimates of the pass-through rates between 0.05 and 0.20 (Card et al., 2018). We contribute to this work by providing the first estimate that characterizes a broad set of firms in a developing country.

We conclude the reduced-form part of the paper with a back-of-the-envelope calculation. The aggregate gains in labor earnings attributable to MNCs are approximately 169 million U.S. dollars per year (or 735 U.S. dollars per MNC job per year).[3] Of these gains, 60% are paid in the form of wage premia to workers directly hired by MNCs, with the remaining 40% coming from domestic-wage increases caused by the entry and expansion of MNCs. This amount is a likely lower bound since we abstract from the likely positive effect of MNCs on transitions from unemployment and informal employment to formal employment. During the same period, the yearly average foregone taxes due to tax exemptions offered to MNCs through Special Economic Zones amount to 467 million U.S. dollars (or 2,030 U.S. dollars per MNC job per year). As the aggregate gains in labor earnings match around 36% of these foregone taxes, gains in labor earnings alone do not justify the tax incentives extended to MNCs.

In the third and final part of the paper, we develop a model to rationalize our reduced-form evidence and estimate the parameters that govern wage setting. In our model, domestic firms incur hiring and training costs, which make them willing to confer rents on incumbent workers. The model also features two labor market imperfections. First, domestic firms have labor market power. Incumbent workers have idiosyncratic taste shocks for potential employers, which are private information for the worker but drawn from a distribution that is known to employers. Firms set wages taking into account that incumbent workers have an upward-sloping labor supply to the firm. Second, domestic firms demand new workers at the domestic market wage, but new workers supply labor according to the expected wage.

[3] As a reference, during the period of study, the average monthly earnings of a Costa Rican worker are 640 real U.S. dollars.

This expected wage is increasing in the random probability of being hired by an MNC and, thus, entails a premium over the domestic market wage. The resulting excess labor supply to industries with higher MNC presence affects the equilibrium marginal revenue product of labor of domestic firms.

The expansion of an MNC can affect wages paid to incumbent workers at domestic firms in three ways. First, the increase in labor demand puts pressure on the domestic market wage paid both in the industry of the MNC and in all other industries (weighted by the probability of transitioning to these other industries). Second, the MNC shifts the composition of labor demand toward jobs with a wage premium. This further improves the outside options of incumbent workers by making it more attractive for them to leave their current domestic employer. Finally, the expansion of the MNC also increases the demand for domestic inputs. In the presence of hiring and training costs, the domestic suppliers of the MNC have higher incentives to retain their incumbent workers and, thus, post a higher wage.

Wages depend on three structural parameters: the marginal cost of hiring and training the first new worker, the elasticity of the marginal cost of hiring and training with respect to the number of new hires, and the retention-wage elasticity that dictates the degree of attachment of incumbent workers to their current employer. Our model-based estimates show a high average marginal hiring and training cost equal to 90% of one year of earnings paid at the market wage, which is comparable to the estimated replacement cost faced by U.S. firms after a patent allowance shock (Kline et al., 2019). We then estimate a retention-wage elasticity of 9, which implies that incumbent workers see their employer and other firms as relatively close substitutes. Nonetheless, we reject that the inverse of the retention-wage elasticity is equal to zero. Workers earn a large – but not full – share of the value of their marginal product of labor.

Our findings suggest three avenues for future research. First, while we focus on the effects of MNCs on wages, MNCs are also likely to affect the extensive margin of employment. Moreover, in developing countries, labor reallocation across the formal and informal sectors provides another potentially important margin of adjustment to MNCs. A complete assessment of the effects of MNCs on workers would need to incorporate these additional margins. Second, MNCs seem to pay above-market wages, which suggests that MNCs create "good jobs" in the host economy (Acemoglu, 2001; Green, 2015). More research is needed to understand how MNCs sustain above-market wages in equilibrium. Finally, our model-based estimates draw attention to the high costs of hiring and training at domestic firms. While these costs allow incumbent workers to extract rents from employers, they also act as an obstacle to firm growth. Direct evidence on a potential link between the small size of firms in developing countries (Tybout, 2000; Bloom et al., 2010) and their hiring and training costs would be welcome.

Related Literature. Our paper contributes primarily to two literatures. First and foremost, we contribute to the vast literature on the effects of foreign direct investment (FDI) on the host economy. Most papers study the effects of FDI at the firm, industry, or macroeconomic level. Firm-level regressions that estimate the effects of changes in MNC presence in either the industry (by region) of the firm or vertically-related industries sometimes add the firm-level number of workers and wage bill as outcome variables.[4] However, firm-level data sheds little light on which workers are affected by FDI and through which channels, both of which are important for understanding the incidence of MNC shocks.

In the few papers with individual-level data, the emphasis is typically on measuring the wage gain for workers who either join a foreign firm or whose firm becomes foreign-owned. This wage gain has been estimated in several developed countries and in one developing country (Brazil), with estimates ranging from 5% to 10%.[5] To our knowledge, all estimates use a variant of the movers design, relying on the assumption of no selection into firms based on idiosyncratic shocks to workers' productivity. We strengthen the causal claim over the MNC premium by using variation in the propensity of workers to move to MNCs due to plausibly exogenous changes in the presence of MNCs in their labor market.[6] We then bring evidence from administrative data and surveys that the MNC premium is consistent with above-market wages, rather than a compensation for inferior amenities at these firms.

Significantly less is known about the channels by which MNCs may affect workers in domestic firms.[7] In this regard, the contemporaneous paper by Setzler and Tintelnot (2019) on MNCs in the U.S. is the closest to ours. In their framework, wage gains for workers in domestic firms derive from either demand effects in the labor market or productivity spillovers to domestic firms. The authors find that an increase in the share of MNC employment within a commuting zone has a statistically insignificant effect on the average worker. We study the effects of MNCs on workers in Costa Rica, a typical developing country for which attracting MNCs is a top policy priority. As a result of Costa Rica's concerted efforts, foreign MNCs now employ 28% of all formal private-sector workers (relative to 6% in the U.S). We allow MNCs to affect the outside options of workers in the labor market and the potential rents of domestic firms that can be shared with workers. Increases in rents are not contingent on productivity increases; they can also arise from standard product demand effects. Moreover, workers in the same labor market can be differentially exposed to MNCs based on the firm-

[4]See the reviews of Javorcik (2014); Hale and Xu (2019). Hale and Xu (2019) point to firm-level and industry-level studies that suggest that increased FDI in a given industry correlates with higher wages. The same authors then mention that the "spillover effect of FDI on other industries' labor markets is yet to be fully researched."

[5]The estimates of the MNC (foreign-owned firm) wage premium are 5% for Sweden (Heyman et al., 2007), 6% for Norway (Balsvik, 2011), 10% for Portugal (Martins, 2011), 6% for Brazil (Hijzen et al., 2013), 8% for Japan (Tanaka, 2015), 7% for Germany (Schröder, 2018), and 7% for the U.S. (Setzler and Tintelnot, 2019).

[6]In an exercise that is conceptually close to ours, Frías et al. (2019) provide a causal estimate of the exporter wage premium by using an IV strategy to estimate the effect of within-plant changes in wage premia on changes in the export share.

[7]Poole (2013) is a notable exception. The paucity of papers studying the (indirect) effects of FDI with individual-level data stands in contrast to the literature that uses individual-level data to study the effects of trade on workers (Autor et al., 2014; Krishna et al., 2014; Pavcnik, 2017; Dix-Carneiro and Kovak, 2017; Helpman et al., 2017; Helm, 2019). The effects of FDI on workers are likely to differ from the effects of trade, given that MNCs are exceptional employers and buyers that directly insert themselves into the labor and product markets of the host economy. Moreover, MNCs increasingly operate in services, whereas most of the research on the effects of trade pertains to manufacturing industries.

to-firm linkages of their employer to MNCs.

The second literature to which we contribute is the one that studies how changes in firms' performance and outside options in the labor market affect wages. First and foremost, we complement this work by studying both of these wage determinants within the same empirical framework. Moreover, we add to a small set of papers that estimate the pass-through of changes in value added per worker to wages by using plausibly exogenous firm-specific shocks to instrument for changes in value added per worker (Garin and Silvério, 2018; Kline et al., 2019; Howell and Brown, 2019).[8] We exploit a new source of variation in firm performance – exogenous shocks to the size of a firm's direct and indirect buyers – to estimate the rent-sharing coefficient and the retention-wage elasticity in a developing country.

By emphasizing that MNC expansions can change both the level and composition of demand in a two-digit industry and region, our paper is related to Beaudry et al. (2012).[9] Their paper finds that switching the composition of jobs between low-paying and high-paying industries has important effects on wages in other industries in the same city. There are two key differences between the analysis in Beaudry et al. (2012) and ours. First, as the same industry can experience different MNC presence shocks across regions, we obtain region-specific shocks to the average premium of an industry. In Beaudry et al. (2012), the more aggregated nature of the data allows for changes in the premia of an industry to occur only at the national level. Second, because our analysis is at the individual level, we can explore the importance of pay differences between MNCs and domestic employers within the same industry.

The remainder of the paper is structured as follows. Section 3.2 describes the data and context. Section 3.3 presents the direct effects on workers who join MNCs. Section 3.4 explains the reduced-form empirical strategy used to study the indirect effects of MNCs on workers in domestic firms and the associated findings. Section 3.5 lays out a stylized model of an economy that formalizes the mechanisms documented in the reduced-form sections. We also leverage the model to estimate structural parameters that govern the labor market. Section 3.6 concludes.

3.2 Data and Context on MNCs in Costa Rica

3.2.1 Data

We bring together a new collection of microdata to assess the effects of MNCs on workers. We combine three types of data: (i) administrative (matched employer-employee data, firm-

[8] Guiso et al. (2005); Card et al. (2015); Lamadon et al. (2019); Friedrich et al. (2019) assume that worker-specific innovations to earnings neither co-vary across coworkers nor with shocks to firm value added.

[9] More generally, we relate to work that examines the role of workers' outside options in wage setting (Krueger and Summers, 1988; Katz et al., 1989; Gibbons and Katz, 1992; Acemoglu, 2001; Fortin and Lemieux, 2015; Jäger et al., 2018; Green et al., 2019; Caldwell and Harmon, 2019; Caldwell and Danieli, 2018; Schubert et al., 2019). Of these, only the handful of recent papers use individual-level data.

to-firm transaction data, corporate tax returns, foreign ownership data), (ii) commercial (Orbis and Compustat), and (iii) survey-based (our own survey data collection and a nationally representative household survey). For details on these datasets and the procedures undertaken to clean them, see B.3.

Administrative Datasets

Matched employer-employee panel data. We construct a matched employer-employee panel covering the universe of formal workers in Costa Rica from January 2006 to December 2017. This project represents the first time that this data is used for research and even more, combined with the three administrative datasets described below. This panel is built on data collected by the *Caja Costarricense de Seguro Social* (Costa Rica's Social Security Administration). We observe (at least once) 1.9 million unique person identifiers (PIDs). For each PID, this data records, on a monthly basis, information on demographic characteristics (date of birth, nationality, sex, district of residence), and the labor earnings and occupation at each employer. We trace employers by their unique corporate tax ID (CID). Monthly labor earnings are not censored. The occupation is recorded as a standardized four-digit code.

We restrict the sample to full-time male and female employees aged 20 to 60, who are not self-employed. We aggregate the data to the quarterly or yearly level, depending on the analysis. We sum the earnings received by a given individual from each job in each quarter (year) and designate the employer that paid the highest total amount as the main employer for that quarter (year). Most full-time workers are employed by only one firm in any quarter (the average is 1.18 per quarter). While throughout the paper, we use the terms "wages" and "labor earnings" interchangeably, in practice, we only observe labor earnings and whether the employee works part-time or full-time. We only keep individuals who are employed full-time. Table B.19 (B.3.1) provides the summary statistics of this resulting dataset.

Like most matched employer-employee datasets, Costa Rica's dataset does not contain the number of hours worked. While this data also does not include the education of the worker, following the Costa Rican law, employers assign occupational codes that are one-to-one mapped to the educational attainment of the worker. Therefore, we infer education from the occupational code and group workers in two categories: with or without a college education. Finally, this data does not track informal employment.[10]

Firm-to-firm transaction data. All firms in Costa Rica are required by the Ministry of Finance to report, using the D-151 tax form, the CID of all their suppliers and buyers with whom they generate at least 2.5 million Costa Rican colones (around 4,200 U.S. dollars) in transactions during a given year, in addition to the total amount transacted. We combine all D-151 tax forms between 2008 and 2017 into a dataset that allows us to track the universe of

[10]In Costa Rica, the rate of informality for employed individuals aged 15 to 64 is 30% – smaller than in other Latin American countries (e.g., Mexico 55%, or Argentina 47%) but higher than the OECD average (17%) (OECD, 2017c).

firm-to-firm relationships in Costa Rica for that period. From this dataset, we keep only those CIDs that appear in the other administrative datasets (i.e., firms that submit corporate tax returns and report their employees to the Social Security Administration). This data allows us to identify domestic firms whose performance is affected by MNCs through supply-chain linkages.

Corporate tax returns data. We then use the universe of corporate tax returns from 2005 to 2017 to construct a firm-level dataset with balance sheet variables (such as total revenue and value added) and other characteristics (such as the firm's region and two-digit industry). We link the corporate tax returns data to the employer-employee data via firms' unique CIDs. We exclude state-owned enterprises, nonprofit organizations, and observations with zero reported total sales or just one employee in a given year. In our analysis of the effects of MNCs on workers in domestic firms, we also exclude MNC firms. Moreover, we restrict our sample to firms with non-missing information on value-added, and that are successfully merged to the matched employer-employee data. To avoid outliers, we exclude firms at the top and bottom 1% of annual percentage changes in value added per worker. Table B.4 (B.1.1) summarizes the steps taken in the construction of the final dataset of analysis from Section 3.4.

Foreign ownership data. To construct a comprehensive account of foreign-owned firms in Costa Rica, we combine information from: (i) three annual surveys conducted by BCCR, (ii) the records of the investment promotion agency of Costa Rica (CINDE), and (iii) Orbis.

Data on the Worldwide Size of MNCs with Subsidiaries in Costa Rica

To construct the instrumental variables (IVs) for the change in MNC presence in Costa Rica, we rely on Orbis and Compustat. We first use Orbis to gather data on the consolidated accounts of MNCs with a subsidiary in Costa Rica. As the largest of these MNCs are publicly traded, we complement the Orbis data with data from Compustat. The final dataset contains data on 239 MNCs and has an unbalanced panel structure from 2006 to 2017. The two variables that are key to the construction of our leading set of IVs are the main industry code of the MNC and its worldwide number of workers. We also use Orbis to construct a second set of IVs for robustness checks. The latter IVs use employment changes in MNCs with at least one subsidiary in one of twenty Latin American and Caribbean countries.

Survey Data

Surveys we conduct in partnership with CINDE. In March 2019, we collaborated with CINDE (the Costa Rican investment promotion agency) on the design of a survey containing eleven questions on the hiring and wage setting practices of MNC subsidiaries in Costa Rica. The survey was administered the same month online and received 46 responses from the human resources (HR) executives of a representative set of MNCs (out of 246 contacted MNCs).

National Survey of Household Income and Expenditures (*Encuesta Nacional de Ingresos y Gastos de los Hogares* or ENIGH). Through ENIGH, the National Institute of Statistics and Censuses of Costa Rica collects data on the sources of income and expenditures on goods and services of a set of representative households. We use data from the 2018 round, which we merge with the 2017 matched employer-employee data based on PIDs. For 1,316 individuals, ENIGH contains information on the number of hours worked and monetary and in-kind benefits from employment. Of these workers, we study the 723 who have positive earnings in 2017, and who are not retirees, self-employed, public sector employees, or with special contracts (*convenios*).

3.2.2 MNCs in Costa Rica

We define "MNC subsidiaries" as those firms in Costa Rica that are subsidiaries of foreign-owned MNCs. We focus on MNCs whose median number of workers in Costa Rica is over 100. These MNCs, with a substantial economic presence in Costa Rica, are less likely to be shell companies. After applying these restrictions, we find 622 unique MNC subsidiaries that operate in Costa Rica at some point between 2005 and 2017.[11] We use 2006 to 2008 (the first three years of the matched employer-employee data) as the pre-period and study the effects of changes in the presence of MNCs in the country occurring between 2009 and 2017. This choice allows us to compute pre-period values for variables (such as the number of workers transitioning from one two-digit industry×region to another) whose post-2009 values might be equilibrium reactions to contemporaneous changes in the presence of MNCs.

Starting the treatment period in 2009 has an additional benefit. That year, Costa Rica ratified a new trade agreement with the U.S., called CAFTA-DR. The debate in Costa Rica on whether to sign the agreement or not was polarized and settled only by a referendum in which the decision to join CAFTA-DR won by a small margin. This makes its occurrence and timing plausibly exogenous to labor and product market conditions in Costa Rica. Two components of CAFTA-DR were foreseen to affect the composition of U.S. FDI flows to Costa Rica (World Bank, 2017). First, FDI in IT-enabled business services was expected to boom after the liberalization of the telecommunications sector. Second, with the strengthening of intellectual property rights and the legal framework protecting foreign investors, CAFTA-DR was predicted to increase FDI in technology-intensive industries. Besides, Costa Rica became attractive to MNCs in the medical device industry after the U.S. Food and Drug Administration opened its first office and regional hub in Latin America and the Caribbean in Costa Rica in 2009.

The industries for which MNC employment has grown the most (in % terms) between 2009 and 2017 are business support services, medical devices, HR services, computer programming, and scientific and technical activities. Conversely, the industries with the sharpest

[11] Larger MNCs are also more likely to be found in Orbis and Compustat, which is necessary for the construction of the IVs. These 622 MNCs employ 75% of the workers employed by all of the firms in Costa Rica with some degree of foreign ownership. For detailed descriptive statistics on these 622 MNCs, see Appendix F from Alfaro-Urena et al. (2019b).

contraction in MNC presence have been those manufacturing apparel, metallic products, food products, motor vehicles, and electronic components. It is reassuring that the industries that most expanded after 2009 were those predicted by the rules introduced by CAFTA-DR.

Let us denote by $\Delta\mathcal{M}_{st}$ the percentage increase between years $(t-1)$ and t in the number of MNC workers in the labor market s in Costa Rica, i.e.,

$$\Delta\mathcal{M}_{st} \equiv \frac{M_{s,t}^{CR} - M_{s,t-1}^{CR}}{M_{s,t-1}^{CR}} \times 100, \tag{3.1}$$

where M is the number of MNC workers in market s in a given year and the CR superscript emphasizes that these are workers employed *in Costa Rica*. Tautologically, $M_{s,t}^{CR}$ is the sum of $M_{m,t}^{CR}$ across all MNCs m in market s in Costa Rica $\left(M_{s,t}^{CR} \equiv \sum_{m\in s} M_{m,t}^{CR}\right)$.

Throughout the paper, a labor market s is a two-digit industry $\times$ region. While there could be up to 480 markets (given the 80 two-digit industries and six regions in Costa Rica), in practice, we have 412 such markets (as not all two-digit industries exist in all regions). The average (median) number of workers in each market is 1,944 (140) in 2009 and 2,209 (141) in 2017. The manufacturing of motor vehicles, or food and beverage services are examples of two-digit industries. In Costa Rica, regions are defined based on commuting patterns. The average (median) region covers 8,515 (9,528) square miles (similar to commuting zones in the U.S.).

Table B.1 (B.1.1) presents summary statistics for the market-level growth in MNC employment ($\Delta\mathcal{M}_{st}$). On average, between 2009 and 2017, markets experience an increase of 13% in MNC employment. While the median market is relatively unaffected (1%), some markets experience extreme contractions (*p1*=-100%) or extreme expansions (*p99*=240%) in MNC employment. On a yearly basis, on average, markets experience an increase of about 4%. Even at this higher frequency, some labor markets can be dramatically affected (*p1*=-83% and *p99*=141%).

One might worry that MNCs have expanded into markets that were systematically more high-skill intensive, which may obfuscate any attempt to disentangle the effect of MNCs on wages and broader trends in the high-skill wage premium. Figure 3.1 relates the percentage growth in the period of analysis (2009 to 2017) in MNC employment in each of the 412 two-digit industry $\times$ region markets in Costa Rica ($\Delta\mathcal{M}_{st}$) and the share of college graduates in those markets during the pre-period (2006 to 2008). On average, labor markets with a higher share of college-educated workers have experienced a higher growth rate in MNC employment. This reflects the fact that CAFTA-DR has made FDI inflows into high-tech and knowledge-intensive industries significantly more attractive. Notwithstanding, there is still considerable variation in the share of college graduates across markets with similar growth rates and the growth rate of MNC employment across markets with similar shares of college graduates.

In 2017, there are 538 MNCs subsidiaries in Costa Rica. These subsidiaries employ 28% of all (formal) private-sector workers. Their workers' wage bill represents 38% of the private sector wage bill. The average MNC (domestic firm) employs 492 (16) workers. The MNC (domestic firm) at the 99 percentile of the size distribution employs close to 6,000 (200) workers. Contrary to the common perception that the majority of MNC workers are college-educated, we find that 82% of MNC workers in 2017 have less than a college degree (relative to 92% in the domestic private sector). Thus, it is not a priori evident that MNC expansions would disproportionately benefit college-educated workers.

3.3 Direct Effects of MNCs on Wages: The MNC Wage Premium

We define the “MNC wage premium" as the additional average percentage gain in labor earnings experienced upon moving from a domestic firm to an MNC relative to the gain in labor earnings experienced upon moving from one domestic firm to another. The MNC wage premium is interesting in its own right. Moreover, as we discuss in Sections 3.4 and 3.5, if MNCs pay above-market wages, their expansions or contractions affect the wages of workers in domestic firms not only through the neoclassical demand channel but also by altering the composition of jobs that serve as potential outside options.

3.3.1 Movers Design Estimates of the MNC Premium

We first estimate the average MNC premium using a within-worker event-study – also called a movers design (as in Card et al., 2013). The within-worker comparison is necessary, as wage differentials may reflect differences in unmeasured labor quality. The sample is restricted to workers who switch employers and have at least eight quarters of tenure at both the origin and destination firm. Hence, a move (event) is an across-quarter change in employers. We exclude movers to or from public sector employment. We study not only moves from a domestic firm to an MNC (DOM-MNC), but also the reverse moves from an MNC to a domestic firm (MNC-DOM), between domestic firms (DOM-DOM), and between MNCs (MNC-MNC). Our movers design specification is the following:

$$w_{it} = \sum_{k=\underline{C}}^{\overline{C}} \psi_k^{DD} D_{it}^k + \sum_{k=\underline{C}}^{\overline{C}} \psi_k^{DM} D_{it}^k I_i^{DM} + \sum_{k=\underline{C}}^{\overline{C}} \psi_k^{MD} D_{it}^k I_i^{MD} + \sum_{k=\underline{C}}^{\overline{C}} \psi_k^{MM} D_{it}^k I_i^{MM} + \alpha_i + \gamma_t + \epsilon_{it}, \tag{3.2}$$

where w_{it} is the log quarterly-average labor earnings of worker i in quarter-year t, α_i and γ_t are worker i and quarter-year t fixed effects. D_{it}^k are event-time dummies defined as $D_{it}^k := \mathbb{1}[t = \tau_i + k]\ \forall k$ s.t. $\underline{C} < k < \overline{C}$, $D_{it}^{\overline{C}} = \mathbb{1}[t \geq \tau_i + \overline{C}]$, $D_{it}^{\underline{C}} = \mathbb{1}[t \leq \tau_i + \underline{C}]$ (where $\mathbb{1}[.]$ is the indicator function and τ_i is the quarter-year when worker i moves employer). We set $\underline{C} = -8$ and $\overline{C} = +8$. I_i^{XX} with $XX \in \{DD, DM, MD, MM\}$ is an indicator for the type of move of worker i. DD stands for DOM-DOM, DM stands for DOM-MNC, MD for MNC-DOM, and MM for MNC-MNC. Our coefficients of interest are the ψ_k for all four types of moves. A causal estimate of these coefficients requires workers not to select into firms based on their idiosyncratic time-varying error term, ϵ_{it}. We normalize $\psi_{-2} = 0$ for each type of move. We use robust standard errors clustered at the individual-level.

Table B.3 (B.1.1) presents summary statistics on the sample of workers used to estimate the regression in equation (3.2). In total, there are 84,756 unique workers in this sample, i.e., workers who we observe as changing employer in event quarter 0, and with the same old employer in the previous eight quarters and with the same new employer in the following eight quarters. Of these, 13,754 individuals move from a domestic firm to an MNC. Columns (4), (5), and (6) show that workers who move from one domestic firm to another tend to not only earn less, on average, than workers who move from a domestic firm to an MNC, but, in addition, come from smaller domestic firms at which co-workers earn less. This confirms the intuition that movers to MNCs are selected on levels.

Figure 3.2 presents two versions of the movers design side-by-side. Panel 3.2a presents raw means of the log wages of workers before and after their move (without α_i and γ_t). Panel 3.2b plots the results from the specification in equation (3.2). Both figures point to the same four takeaways: (i) irrespective of the type of move, workers do not display differential pre-trends, (ii) both DOM-DOM and MNC-MNC moves lead to a small increase in labor earnings (about 4% and 6%, respectively), (iii) DOM-MNC moves result in large boosts in labor earnings (about 13%), and (iv) MNC-DOM moves bring large declines in labor earnings (about 9%, symmetric to the gains from DOM-MNC moves, with respect to DOM-DOM moves). Thus, the MNC wage premium (the difference between the DOM-MNC increase and the DOM-DOM increase) is about 9%. In addition, Panel 3.2a echoes the finding from Table B.3 that workers engaged in DOM-MNC moves already had higher labor earnings that those engaged in DOM-DOM moves.

We also perform an AKM decomposition (Abowd et al., 1999) and regress the firm fixed effects on an MNC dummy. While both the movers design and the AKM design rely on the same identification assumption, they differ in how they weigh each firm when comparing the average firm effects of MNCs to those of domestic firms. The movers design uses frequency weights based on how many workers move between one type of firm to another. In the AKM-based exercise, the coefficient on the MNC dummy compares the firm-size weighted average of the firm fixed effects of MNCs to that of domestic firms. This AKM-based exercise delivers an estimate of the MNC premium of around 10%. Hence, the movers-weighted and employment-weighted estimates are similar.

The main threat to identification is that the move of a worker and, in particular, a move from a domestic firm to an MNC (or the reverse) is driven by unobserved shocks to her productivity, which would be subsumed in the error term. In B.1.2, we present three robustness checks. First, we estimate the MNC premium using only moves occurring within the first twelve months after the entry of a new MNC. Second, we estimate the premium using only workers coming from unemployment, whose earnings we benchmark to those of workers with similar observable characteristics but with continuous employment in a domestic firm. Finally, we estimate the premium only using the moves of workers who come from exiting domestic firms. As the estimates from these alternative specifications are similar to our main estimate of 9% (though noisier due to the smaller sample sizes), we assess that this main estimate is unlikely to be driven by contemporaneous shocks to workers' productivity.

3.3.2 IV Estimate of the MNC Premium

We also use an IV strategy that takes advantage of exogenous variation in the size of MNCs in Costa Rica. To our knowledge, this is the first time that the MNC premium is estimated without relying on the assumption of movers designs. Consider worker i who in year $(t-1)$ is part of two-digit industry × region market $s(i, t-1)$. To relate the change in wages of worker i upon moving to (from) an MNC employer from (to) a domestic employer, we adopt the following specification:

$$\begin{aligned}\Delta w_{it} =&\psi \Delta \mathbb{1}[j(i) = MNC]_t + \boldsymbol{X_i}'\boldsymbol{\beta}_{char} + \alpha_{j(i,t)} + \\ &+ \gamma_{ind(s(i,t-1))} + \mu_{reg(s(i,t-1))} + \underline{\gamma}_{ind(s(i,t))} + \underline{\mu}_{reg(s(i,t))} + \epsilon_{it}\end{aligned} \tag{3.3}$$

where Δw_{it} is the percentage change in the monthly average labor earnings of worker i between year $(t-1)$ and year t, $\Delta \mathbb{1}[j(i) = MNC]_t$ is the difference between two indicator functions which take value 1 if the employer of i, $j(i)$ is an MNC, and $\boldsymbol{X_i}$ is a vector of dummies for worker i characteristics. $\alpha_{j(i,t)}$ are firm $j(i,t)$ fixed effects, $\gamma_{ind(s(i,t-1))}$ and $\underline{\gamma}_{ind(s(i,t))}$ are two-digit industry fixed effects for the industry of the market s of i in $(t-1)$ and t respectively, $\mu_{reg(s(i,t-1))}$ and $\underline{\mu}_{reg(s(i,t))}$ are region fixed effects for the region of the market s of i in $(t-1)$ and t respectively, and ϵ_{it} is an idiosyncratic error term. Notice that we assume that the effect of moving from a domestic firm to an MNC is symmetric to the effect of the reverse move. Moreover, we use movers between firms of the same ownership type as the reference. These choices are consistent with our findings from the movers design.[12]

The most plausible concern with the OLS estimate of ψ in equation (3.3) is that workers switch to MNCs upon receiving a positive productivity shock. If that were the case, then the OLS estimate would be upward biased. To alleviate potential endogeneity concerns, we instrument the move from a domestic firm to an MNC between years $(t-1)$ and t by the contemporaneous change in MNC employment *outside* of Costa Rica of MNCs with subsidiaries in the labor market of the worker in $(t-1)$. Precisely we define the instrument, $\Delta\mathcal{O}_{s(i,t-1),t}$, as:

$$\Delta\mathcal{O}_{s(i,t-1),t} \equiv \frac{M^{Out}_{s(i,t-1),t} - M^{Out}_{s(i,t-1),t-1}}{M^{Out}_{s(i,t-1),t-1}} \times 100, \tag{3.4}$$

where $M^{Out}_{s,t}$ is defined as the year-t number of workers *outside* of Costa Rica for MNCs whose subsidiaries operate in the two-digit industry × region market s in Costa Rica. Hence, $M^{Out}_{s,t}$ is the sum of the MNC-specific outside of Costa Rica number of workers across all MNCs in s.

[12] For comparability with the movers design, we only use the sample of workers who experience an employer change between $(t-1)$ and t (i.e., we exclude stayers – individuals with the same main employer $j(i)$ in both $(t-1)$ and t).

Expansions in the global employment of MNCs with subsidiaries in Costa Rica are likely to predict the expansion of their subsidiaries in Costa Rica. Workers who move from a labor market $s(i, t-1)$ that experiences an increase in MNC employment are more likely to move to an MNC than workers who move from a market whose MNC employment has increased less. The exclusion restriction requires the expansion of MNCs outside of Costa Rica to affect the earnings of worker i only through its effect on the probability of i moving to the MNC subsidiaries in Costa Rica.

While the relationship between MNC expansions in Costa Rica and MNC expansions outside of Costa Rica is not the first stage of the IV, it is closely linked. Therefore, it is important to understand how $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$ relate. Figure B.1 and Table B.2 (B.1.1) show that $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$ (residualized of year and industry fixed effects) have a robust correlation of 0.86. One plausible explanation for this positive correlation is that MNC subsidiaries in Costa Rica carry out different tasks than those in their other locations.[13] We find that the four-digit (two-digit) industry code of the MNC subsidiary in Costa Rica is different from that of the MNC group in 82% (72%) of the cases. This is consistent with MNCs in Costa Rica having expanded mostly through "vertical" investment, by which the parent and subsidiaries exchange inputs and outputs through intrafirm trade.[14] Thus, $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$ are plausible complements (as also shown in Harrison and McMillan, 2011).

What is the nature of the MNC-wide shock that affects the size of the subsidiary in Costa Rica? One such shock could affect the global demand of the final good of the MNC, which triggers a shock to the demand of the input provided by the Costa Rican subsidiary. In the model in Section 3.5, we assume that MNCs in Costa Rica are exposed to exogenous shifts in their international demand. Another shock could affect the productivity of the MNC (e.g., coming from the unexpected allowance of a new patent, such as in Kline et al., 2019), which could then lead to expansions or contractions across all locations. The last scenario is one that involves financial shocks at the HQ, which are also known to affect location decisions abroad (Desai et al., 2004; Baker et al., 2008; Erel et al., 2012; Alfaro and Chen, 2018).[15]

Table 3.1 presents the OLS and IV estimates. The OLS estimate of the MNC premium

[13] The traditional theory of the expansion of multinationals emphasizes two types of expansion. "Horizontal" foreign investment is understood to mean situating production facilities to avoid trade costs (Markusen, 1984), whereas "vertical" investment represents firms' attempts to take advantage of cross-border factor cost differences (Helpman, 1984). Most past research found the bulk of FDI to be horizontal. However, newer research suggests that data limitations have led the literature to systematically underestimate vertical FDI, which is far more prevalent than previously thought (Alfaro and Charlton, 2009).

[14] Among the 82% of cases in which the subsidiary and the HQ of the MNC operate in different industries, the most frequent combination of industries features a subsidiary operating in business support services (such as "activities of head offices", or "activities of call centres") and the MNC group operating in various industries (such as the "manufacture of underwear" or the "operation of dairies and cheese making"). Most of the remaining combinations also point to obvious input-output relationships, such as the "growing of tropical fruits" (subsidiary industry) – "processing and preserving of fruit and vegetables" (MNC group industry) or the "manufacture of cordage, rope, twine and netting" (subsidiary) – "manufacture of irradiation, electromedical and electrotherapeutic equipment" (group).

[15] In the model presented in Section 3.5, all these shocks have isomorphic effects on domestic firms. Hence, we do not distinguish between them in our study of the effects of MNCs on workers in domestic firms.

is 7.6%. This estimate is in line with the 9% estimate from the movers design. The only difference is definitional; in this exercise, we benchmark DOM-MNC moves to both DOM-DOM and MNC-MNC moves, whereas in the movers design we benchmark DOM-MNC moves only to DOM-DOM moves. The IV estimate is 15% (with an F-statistic of 677). While the IV estimate is larger than the OLS estimate, we cannot reject that the two estimates coincide. This finding assuages the concern that workers move to MNCs after receiving a positive and contemporaneous productivity shock. There is still the possibility that there are heterogeneous treatment effects. Compliers in this IV exercise may be workers who come from lower-paying domestic firms and who need a considerable expansion of MNCs in their labor market to move to an MNC. Notwithstanding, it is reassuring that our IV and movers design deliver similar estimates.

3.3.3 Interpretation of the MNC Premium

Finding an MNC wage premium is not per se incompatible with a competitive labor market. In particular, the MNC premium might serve as compensation for differences in undesirable job attributes. Put differently, an MNC wage premium might not be a utility premium. If that were the case, then an increase in the presence of MNCs in a labor market could no longer be interpreted as an improvement in the composition of outside options of workers in that market (but only in demand). It is, therefore, important for the study of the indirect effects of MNCs on workers to establish whether the premium is compensating for disamenities or consistent with above-market wages.

Compensating Differentials

Better monetary and in-kind benefits at MNCs. For a sample of 723 workers surveyed in 2018 for the National Survey of Household Income and Expenditures (with ENIGH as its acronym in Spanish), we observe the number of hours worked for their employer in the previous week and whether this employer provides them with a series of monetary and in-kind employment benefits, that is, whether the employer pays for extra hours of work, a bonus salary at the end of the year, sick leave or vacation days, social security contributions, and occupational hazard insurance.[16] Table 3.2 presents OLS regressions on the cross-section of workers surveyed in 2018, for which the main explanatory variable is whether the individual worked for an MNC in 2017.[17] Working for an MNC in 2017 is not correlated with working

[16] Another piece of qualitative evidence comes from the Great Place to Work Institute for Central America and the Caribbean. In 2019, this institute assessed and ranked 39 employers in Costa Rica. Of these 39 employers deemed as "great places to work," 29 were subsidiaries of MNCs such as Cisco Systems, Bridgestone, or 3M. See here.

[17] 2017 is the last year from the matched employer-employee data that is available as of now. We need the matched employer-employee data to be able to track the identity of the employer. An obvious caveat is that the employer of 2017 might not be the same employer described in the 2018 survey. As soon as the 2018 matched employer-employee data becomes available, we will match each individual to the actual employer from the month when she was surveyed for ENIGH in 2018. That said, we assume that the qualitative

extra hours in the employment held in 2018. Workers who worked for an MNC in 2017 are also 7% to 20% more likely to benefit from all of the above mentioned monetary and in-kind benefits.[18]

Higher retention probabilities at MNCs. We use the matched employer-employee data to provide evidence of the revealed desirability of MNC jobs. Figure B.12 (B.1.2) plots the retention probability (i.e., the probability that a worker who started employment in quarter 1 at firm j is still working for firm j in quarter $t \geq 1$) for two groups of workers: those who start employment in quarter 1 in a domestic firm, and those who start employment in quarter 1 in an MNC. In both groups, we only include workers whom we observe to be employed by a different firm in the quarter after the separation from employer j. For these workers, the separation is more likely to result from the worker quitting than from being fired. While this graph showcases an overall high job churn, workers who start an employment spell at an MNC are more likely to be retained by the MNC than those starting an employment spell at a domestic firm.

Lower wage increases necessary for MNCs to expand. In B.1.2, we investigate how the ratio of wages for new vs. incumbent workers in a given occupation and firm changes with the size of the expansion of that firm. We then contrast how this ratio relates to the size of the expansion for domestic firms vs. MNCs. If MNCs are more attractive as employers than domestic firms, then MNCs should not find it as difficult to expand as domestic firms. We find that both MNCs and domestic firms pay larger relative wages (for new workers vs. incumbents) the larger the expansion of the firm.[19] However, the increase in the relative wage is twice as substantial for domestic firms than it is for MNCs. Thus, both types of firms face an upward-sloping labor supply, but the elasticity faced by MNCs is much higher than the one domestic firms face. This evidence combined suggests that, if anything, MNCs provide better work conditions than domestic firms.

Explanations Consistent with Above-Market Wages

Labor recruitment and training costs (Oi, 1962; Manning, 2011), or efficiency wages (Shapiro and Stiglitz, 1984). The evidence so far suggests that MNCs pay wages that are above the competitive levels of the domestic economy. Why would MNCs find it profitable to do so? An older literature on industry wage differentials and the firm size premium proposes two main plausible answers.

conclusion from this exercise will not be altered, as 70% of workers in the economy are "stayers" (i.e., they have the same employer for any two consecutive years, see Table B.19 in B.3.1).

[18] This is also consistent with older evidence on inter-industry wage differentials. Katz and Summers (1989) show that the consideration of fringe benefits reinforces, rather than reduces, industry compensation differences.

[19] The average (median) ratio of the wages of new workers in a given occupation relative to incumbent workers in that same occupation and firm is 0.88 (0.86). Our analysis emphasizes how the ratio of wages of new workers to incumbent workers changes with the size of an expansion of the firm in the given occupation, but *does not imply* that the ratio is larger than 1.

One strand of literature (dating back to Oi, 1962) stresses that worker turnover is undesirable to firms due to hiring and training costs. If working for MNCs requires building more firm-specific human capital (e.g., due to their more complex processes), this would imply that worker turnover is more costly for MNCs and would rationalize their premium. Another candidate explanation is the need for firms to deter workers from shirking. Conferring rents on them, which are forfeited if caught shirking, may be an efficient alternative to more extensive monitoring costs (Katz and Summers, 1989). Previous research points to MNCs as firms with high monitoring costs (due to the physical distance between the parent its subsidiaries; see Head and Ries, 2008) and for whom worker shirking can be more costly (e.g., due to their higher-capital intensity, as in the hold-up problem of Acemoglu, 2001).

Based on our surveys completed by HR executives at MNCs in Costa Rica, both factors seem to be at play. The two most common justifications for paying the same worker a higher wage than that of a domestic firm are that "workers [...] must be motivated to work hard" (33% of responses) and that MNCs want "to retain talent, to avoid turnover of workers whose training [they] invest in" (27%).[20]

While we cannot provide direct evidence on the turnover or monitoring costs of MNCs relative to domestic firms,[21] the literature suggests that observable firm characteristics, such as the size or industry of the firm, correlate with these costs (Brown and Medoff, 1989; Oi and Idson, 1999). Larger firms provide more training than smaller firms, with the gap growing at higher education levels. As MNCs tend to be larger and hire relatively more college-educated workers, it is, therefore, plausible that accounting for these firm and worker characteristics would account for at least part of the MNC premium.

Next, we examine whether the MNC premium depends on the education of the worker. Finding differences in MNC premia for workers with or without college education would point to the distributional effects of MNCs. Moreover, finding such differences would also be in line with the idea that turnover and monitoring costs vary by worker characteristics. To that end, we divide workers into two categories: those with a college degree and those without. Figure B.6 (B.1.2) presents the event-study estimates for each educational group and for two types of moves (DOM-MNC and DOM-DOM). We find that college graduates who make DOM-

[20] 11% of responses also suggest that the "company will employ the worker in projects that will generate higher income and where her competence will be better utilized." However, differences in productivity are, by themselves, *not* enough to explain why more productive firms would pay higher wages. In a competitive labor market model, more productive firms would be larger but would not pay higher wages than lower productivity firms. Recent work assumes that individuals have non-pecuniary idiosyncratic preferences for working at different firms (Card et al., 2018; Berger et al., 2019; Setzler and Tintelnot, 2019). As higher productivity firms want to be larger, they need to pay both marginal and inframarginal workers at a higher rate.

[21] While we do not observe the hiring and training costs of MNCs in Costa Rica, anecdotes suggest that MNCs spend considerable resources on both. The training of workers in MNCs can either be offered by the MNC subsidiary directly (e.g., the HQ sends specialists to the subsidiary to deliver standardized training) or at third-party institutes which provide the training on behalf of the MNC. Of these institutes, the National Institute of Learning of Costa Rica (with its acronym in Spanish, INA) is the most likely partner, in particular for the lower-skilled workers. In 2015, MNCs from Special Economic Zones (SEZs) contributed with 22 million U.S. dollars to INA, which represented 11% of the budget of this institution that year (Procomer, 2016).

MNC moves experience the highest premium (about 24%). College graduates transitioning from one domestic firm to another experience a premium as large as non-college graduates transitioning to an MNC (about 11%). Non-college graduates moving from one domestic firm to another see their quarterly-average earnings increase by around 3%.[22]

Figure B.7 examines the role of firm size and industry in explaining the differential educational premium. One may be concerned that college-graduates move, on average, to larger employers than non-college graduates, which could drive part of their larger premium. Panel B.7a shows that conditional on their type of move (either DOM-DOM or DOM-MNC), both college and non-college graduates move to similarly sized employers. Panel B.7b re-estimates equation (3.2), this time controlling for the firm size and industry. The new premia are smaller in magnitude than those in Figure B.6, becoming 18% for college graduates in DOM-MNC moves, 12% for college graduates in DOM-DOM moves, 5% for non-college graduates in DOM-MNC moves, and 2% for non-college graduates in DOM-DOM moves. Thus, while both MNCs and domestic firms pay higher raises to college-educated workers than to workers who have not attended college, the difference is larger for MNCs (13%) than for domestic firms (10%).

Figure 3.3 explores the heterogeneity of the MNC premium across two-digit industries. We estimate the average industry-specific MNC premia by restricting only to within-industry moves between domestic firms and MNCs. Panel 3.3a illustrates the heterogeneity in MNC premia, with some industries having premia as high as 50% or as low as a 10% discount. Among the industries with the highest MNC premia are the manufacturing of non-metal mineral products, professional and scientific services, engineering activities, and telecommunications. Industries such as forestry, manufacturing of apparel, land transportation, and cleaning services are among those with the lowest MNC premia.

One might ask whether these differences in industry-specific MNC premia reflect fundamental differences between industries or differences in the extent to which they employ college-educated workers. Panel 3.3b in Figure 3.3 shows a strong positive correlation between the MNC premium of college-educated workers in an industry and the MNC premium of non-college-educated workers in the same industry. Industries that tend to pay high premia to their college-educated workers also tend to pay high premia to their non-college-educated workers, and vice versa. Given the salience of the industry for the MNC premium, in Sections 3.4 and 3.5, when we study the indirect effects of changes in MNC presence on workers, we weigh the changes in the MNC presence of each industry with its industry-specific MNC premium.

These findings imply that about half of the MNC premium can be explained by firm characteristics that have been shown to correlate with various types of labor market imperfections (see Manning, 2011, for a review). That said, MNCs tend to be considerably larger

[22] One might worry that moves to MNCs are more frequent in the second half of our sample period and that the college premium in Costa Rica has increased with time (e.g., due to the higher demand for college-educated workers by MNCs). Figures B.8 and B.10 (B.1.2) run the same analysis separately for each half of our sample period. We compare MNCs to domestic firms and college-educated to non-college-educated workers in each sub-period and find no distinction in patterns across time.

than local firms in developing countries and operate in more sophisticated industries. MNC workers enjoy the full premium paid by MNCs, even if the MNC status per se explains only half of the premium.

Other factors, such as MNC-wide wage setting policies. Recent research shows that multi-establishment firms do not decide on employment and wages for each establishment as an independent unit, but also use information about the conditions in all other establishments in the group (see Giroud and Mueller, 2019). This interdependence between the outcomes of establishments in a group is likely to be even stronger for settings in which establishments are in a vertical (input-output) relationship – which seems to be the case for MNCs in Costa Rica. Concerns around wage equity are also likely to arise more frequently the more dissimilar the living standards are between the countries where MNCs operate. Hjort et al. (2019) find that MNCs – particularly those from inequality-averse countries – anchor their wages to HQ levels.[23] We conjecture that – at least to some extent – the practice of within-MNC wage compression is motivated by increasing consumer scrutiny over the practices of MNCs abroad (for example, see Harrison and Scorse, 2010).

Our survey to HR executives from MNCs lends support to the equity consideration as one of the explanations for the MNC premium. In particular, 27% of respondents stated that “for reasons of equity, the wages [they] pay to [their] workers in Costa Rica should be closer to the wages of similar workers in the HQ or other subsidiaries of [their] group."

Another plausible driver of above-market wages is the higher scrutiny of MNCs who benefit from preferential tax regimes (such as those offered by Special Economic Zones). To the extent that these tax regimes are justified (at least in part) by the high-quality employment that they are to create, the wages and work conditions offered by beneficiary firms are topics of polemic and broad interest in the host economy. In B.1.2 we divide MNC subsidiaries into two groups: those that are part of the Costa Rican Special Economic Zone regime (called *Zona Franca* or ZF) and those that are not. Workers who move from a domestic firm to an MNC in ZF experience a 10% higher premium than those who move from a domestic firm to an MNC outside of the ZF. After controlling for the size and industry of firms, the ZF MNC premium remains 7% higher than the non-ZF premium. While not definitive, this finding is consistent with MNCs in ZFs sharing part of their tax savings with workers.

3.3.4 Takeaways on the MNC Premium

Of our findings on the MNC premium, six directly inform our analysis of the indirect effects of MNCs on workers in domestic firms. First, we find that when hired by an MNC, workers

[23] One might expect that workers in college-educated occupations are those who work in international teams and who are more likely to benefit from within-MNC wage compression. Hjort et al. (2019) find that the correlation between the average wage MNCs pay local workers at foreign establishments and the average wage they pay workers in the same position at the HQ are strongly correlated, and particularly so for low-skilled staff. This is in line with our finding of a sizable MNC premium for workers without a college degree (8%).

receive, on average, a 9% higher wage than the counterfactual average wage of a move to a domestic firm. Second, our evidence suggests that the MNC premium has a causal interpretation. Third, the MNC premium does not seem to compensate for inferior amenities at MNCs. Fourth, part of the MNC premium is explained by observable characteristics such as the size and industry of the firm. Controlling for the size and industry of the old and new employer explains around half of the premium. The remaining half is consistent with other MNC-specific considerations, such as MNC-wide wage setting policies. Because the MNC premium varies greatly across industries, in the rest of the paper, we explicitly incorporate this heterogeneity. Fifth, while the MNC premium for college-educated workers is larger than the MNC premium for non-college-educated workers (12% vs. 8%), because most workers (in both MNCs and domestic firms) do not have a college degree, in our main specification we treat workers as homogeneous. Sixth and last, we do not find evidence that MNC premia change over time, which explains why we treat the MNC premium as constant for each industry.

3.4 Indirect Effects of MNCs on Wages

This section presents the main reduced-form findings on the effects of changes in MNC presence in Costa Rica on workers in domestic firms. We conjecture that expansions (contractions) of MNCs can affect the wages of workers in domestic firms through three channels: (i) changes in demand in the labor market, (ii) changes in the composition of demand in the labor market towards (or away from) MNC employers that pay a premium, and last, (iii) changes in the performance of domestic employers through supply-chain linkages to MNCs. We will group the first two effects as resulting from the "labor market exposure" to MNCs. The last effect results from the "firm-level exposure" to MNCs.

We then replace the change in firm-level exposure to MNCs by the change in the value added per worker of the firm. We use the same instrument proposed for the change in firm-level exposure to instrument for the change in value added per worker. The aim of this analysis is twofold. First, we contribute to the growing literature that uses matched employer-employee data and plausibly exogenous firm-level shocks to estimate the "rent-sharing" coefficient, i.e., the pass-through of firm-level changes in value added per worker to worker wages. Our estimate uses a novel source of variation coming from shocks to the set of direct and indirect (MNC) buyers of a firm. Second, this exercise allows us to build intuition on the magnitude of the effects of the firm-level exposure to MNCs on wages.

We conclude this section with a discussion about the distributional implications of the indirect effects and a back-of-the-envelope calculation of the wage gains attributable to MNCs from both the direct effects estimated in Section 3.3 and the indirect effects estimated in this section.

3.4.1 Main Empirical Specification for the Indirect Effects

We estimate the effects of changes between two consecutive years in exposure to MNCs on the contemporaneous changes in yearly labor earnings of workers in domestic firms. Our primary sample includes only stayers (or incumbent workers), defined as workers who remain at the same domestic employer for two consecutive years. The focus on stayers enables us to link changes in the performance of a domestic employer to changes in wages paid by the same employer. We use yearly changes (as opposed to longer differences) due to the relatively high turnover of workers across firms.

Consider worker i who is employed by the same domestic firm $j(i)$ in both years $(t-1)$ and t. Denote by $s(i)$ the two-digit industry $\times$ region market of i's employer. As worker i does not change employer and market between $(t-1)$ and t, we do not index $s(i)$ by time. To study the effects of exposure to MNCs on workers i, we use the following empirical specification:

$$\begin{aligned}\Delta w_{it} =& \beta_{LME}\Delta LME_{s(i),t} + \beta_{FLE}\Delta FLE_{j(i),t} + \\ & \boldsymbol{X'_{ij,t-1}}\boldsymbol{\beta}_c + \alpha_{j(i)} + \gamma_{ind(s(i))\times t} + \mu_{reg(s(i))\times t} + \rho_{ind(s(i))\times reg(s(i))} + \epsilon_{it},\end{aligned} \tag{3.5}$$

where the outcome Δw_{it} is the percentage change in the monthly average labor earnings of worker i between year $(t-1)$ and year t. The main explanatory variables of interest are $\Delta LME_{s(i),t}$ and $\Delta FLE_{j(i),t}$, which refer to the labor market and firm-level exposure measures. We define these measures in detail in Section 3.4.2. The remaining elements are other relevant controls. $\alpha_{j(i)}$ are firm $j(i)$ fixed effects, $\boldsymbol{X_{ij,t-1}}$ is a vector of worker and firm characteristics, e.g, the sex, year-of-birth, college education status, Costa Rican national status of the worker, and the share of total sales of the worker's employer to MNCs in year $(t-1)$.[24] $\gamma_{ind(s(i))\times t}$ controls for potential shocks to the two-digit industry of the two-digit industry $\times$ region market of i and $\mu_{reg(s(i))\times t}$ controls for potential shocks to the region of the same market. $\rho_{ind(s(i))\times reg(s(i))}$ controls for differences in levels between markets. As the labor market exposure varies at the two-digit industry $\times$ region level, regressions with two-digit industry $\times$ region $\times$ year fixed effects absorb this measure (but do not absorb the firm-level exposure measure). All changes are defined over two consecutive years, $(t-1)$ to t. In all regressions using this specification, we use robust standard errors clustered at the firm level.

[24] In the measure of firm-level exposure that we define in Section 3.4.2, the exposure weights do not sum to one at the level of firm j (given that firms also tend to have a large share of domestic clients). Thus, it is important to control for the total share sold to MNCs. This ensures that our IV estimate is only driven by the variation in the share of sales sold to MNCs and not by unobserved shocks that systematically differ between MNC and domestic clients (Borusyak et al., 2018).

3.4.2 Margins of Indirect Exposure to the MNC Shock

Labor Market Exposure to MNCs

We now propose a measure called "Labor Market Exposure" to MNCs (LME, henceforth). We define a labor market as the two-digit industry × region s in which an individual works in year t. The assignment of the labor market $s(i)$ to an individual i is based on the two-digit industry and region of the firm employing i in year t. We assume that all workers in market $s(i)$ face the same change in $LME_{s(i)}$, which is brought about by the changes in MNC presence in their own market but also by the changes in MNC presence in other markets s'. Now, let us introduce and motivate our definition of $\Delta LME_{s(i),t}$:

$$\Delta LME_{s(i),t} \equiv \sum_{s'} \pi_{s(i)s',t_0} \psi_{s'} \nu_{s',t-1} \Delta \mathcal{M}_{s',t}. \tag{3.6}$$

$\Delta LME_{s(i)t}$ is a sum across all two-digit industry × region markets s' in Costa Rica, in which market s' is weighted by its "closeness" to the market s of the worker. $\pi_{s(i)s',t}$ – the "closeness" measure – is the number of workers who start year t in market $s(i)$ and end t in s', divided by the total number of workers who start t in market $s(i)$. On the one hand, weighing the importance of changes in other markets by $\pi_{s(i)s',t}$ is consistent with a long line of research that finds sizable mobility costs, across both regions and industries. In other words, $\pi_{s(i)s',t}$ acknowledges that not all jobs in the economy are equally accessible to workers in s. On the other hand, a worker is not only exposed to the shocks occurring in one's labor market, but also to shocks in connected labor markets. Defining $\Delta LME_{s(i),t}$ as not only based on one's labor market $s(i)$ allows for the boundaries of labor markets to be porous. Empirical worker transitions across markets capture factors that are relevant to workers upon deciding to switch industries and regions, which would not otherwise be captured by alternative approaches (such as those that build upon the occupational similarity between industries).

We compute these shares for each of the pre-period years (2006 to 2008, or t_0) and then average them across these years. By construction, $\sum_{s'} \pi_{s(i)s',t_0} = 1$. As the average π_{ss,t_0} is 0.82, most of the change in labor market exposure to MNCs experienced by a worker comes from the change in her own market. The average probability of staying in the same region but changing the two-digit industry during the year is 0.13, while the average probability of staying in the same industry but moving to another region is 0.02. The remaining 0.03 pertains to moves outside of one's region and two-digit industry.

$\nu_{s',t-1}$ is the share of workers employed by MNC subsidiaries in market s' in Costa Rica in year $(t-1)$. In 2009, the share of MNC employment in the average (median) market ($\nu_{s',2009}$) was 0.08 (0). In 2017, the average (median) share of MNC employment ($\nu_{s',2017}$) was 0.09 (0). We therefore weigh percentage changes in MNC employment in market s' ($\Delta\mathcal{M}_{s',t}$) between year $(t-1)$ and t by the share of MNC employment in market s' in $(t-1)$ ($\nu_{s',t-1}$). For descriptive statistics on $\Delta\mathcal{M}_{s',t}$, see Section 3.2.2.

$\psi_{s'}$ is defined as one plus the average MNC wage premium in the two-digit industry of s', $ind(s')$.[25] We have discussed the industry-specific MNC wage premia and their large heterogeneity in Section 3.3. This heterogeneity motivates the interaction of changes in MNC presence in market s' ($\nu_{s',t-1}\Delta\mathcal{M}_{s',t}$) with the MNC premium in the industry of that market ($\psi_{s'}$).

The interaction with $\psi_{s'}$ reflects the fact that two similarly sized MNC expansions would have different effects on the average market wage depending on the magnitude of the premium they pay. Consider the following two hypothetical cases. In both cases, assume there is only one industry with MNCs in year $(t-1)$, which,- in addition, experiences growth in MNC employment between years $(t-1)$ and t. Denote by s' the industry of the first case and by s'' the industry of the second case. In the first case, assume MNCs in s' pay the same wage as domestic firms, i.e., $\psi_{s'} = 1$. In the second case, MNCs in s'' pay a 20% premium with respect to domestic firms, i.e., $\psi_{s''} = 1.2$. Moreover, assume that both s' and s'' were equally close to s in t_0 (that is, $\pi_{ss',t_0} = \pi_{ss'',t_0}$). Had we not acknowledged the actual MNC premia in s' and s'', we would have expected both cases to lead to the same effect on the wages of workers in domestic firms in s. However, in the first case, the expansion of MNCs in s' would lead to higher wages only through demand effects. In contrast, in the second case, the expansion of MNCs in s'' is likely to lead to larger increases in wages than those found in the first case, due to a combination of demand and composition effects (with the premium $\psi_{s''}$ further improving the outside options in s'').

Our interaction with $\psi_{s'}$ echoes one of the central insights of Beaudry et al. (2012).[26] There are two key differences between their analysis and ours. First, as the same industry can experience different MNC presence shocks across regions ($\nu_{s',t-1}\Delta\mathcal{M}_{s',t}$), we obtain region-specific shocks to the average premium of an industry.[27] Second, because our analysis is at the worker-level (as opposed to the industry-level), we can also study the effects of changes in MNC presence on the wages of workers in the same industry of the shock. Put differently, the more disaggregated data on which our analysis rests allows us to acknowledge pay heterogeneities across employers in the same industry.

[25] The s' subscript on $\psi_{s'}$ is an abuse of notation as we cannot compute market-specific premia due to the reduced number of moves between domestic firms and MNCs for which both firms belong to the same market.

[26] In that paper, the authors study whether changing the composition of jobs between low-paying and high-paying industries has important effects on wages in other industries. In their index of industrial composition, the authors interact each industry share with the national-level wage premium of that industry relative to an arbitrarily chosen industry. In contrast to Beaudry et al. (2012) (who allow for industries to have time-varying premia), our estimates of MNC premia from Section 3.3 suggest that, at least for the time period that we study, MNC premia are time-invariant. This explains why $\psi_{s'}$ does not have a time subscript.

[27] One can define the average premium of an industry as $(1-\nu_{s't})\,1 + \nu_{s't}\psi_{s'}$, where the premium of MNCs in s' ($\psi_{s'}$) is defined relative the domestic wage (normalized to 1). In contrast, in Beaudry et al. (2012), the more aggregate nature of the data implies that changes in the premia of an industry can only be observed at the national level.

Firm-Level Exposure to MNCs

MNCs can affect workers in domestic firms not only through the labor market but also through the product market. These product market interactions may affect the performance of these domestic firms, which, in turn, may affect the outcomes of their workers. In this subsection, we define the "Firm-Level Exposure" to MNCs (abbreviated FLE, henceforth) as the exposure of domestic firms j to the expansion or contraction of MNCs.

Define $\theta_{jm,t}$ as the share of total (direct and indirect) sales of firm j to the subsidiary of MNC m in Costa Rica in year t. We consider not only the direct sales to MNCs, but also the indirect sales made through one's buyers at different supply-chain distances. For details on how we construct θ_{jm}, see B.3.3.[28] Then,

$$\Delta FLE_{j(i),t} \equiv \sum_m \theta_{j(i)m,t-1} \Delta \mathcal{M}_{mt}, \tag{3.7}$$

where $\theta_{jm,t-1}$ is the share of total (direct and indirect) sales to the subsidiary of MNC m in Costa Rica in year $(t-1)$ and $\Delta\mathcal{M}_{mt}$ is the percentage increase in the employment of MNC buyer m in Costa Rica between $(t-1)$ and t. Note that we are weighting the importance of changes in employment of each MNC buyer m by its $(t-1)$ share of total sales $\big(\theta_{j(i)m,t-1}\big)$, as opposed to the same share in year t. Note also that $\theta_{j,t-1} \equiv \sum_m \theta_{j(i)m,t-1} \leq 1$.[29]

What type of shocks to firm j does $\Delta FLE_{j(i),t}$ capture? First, it captures likely demand shocks to firm j from its expanding (or contracting) MNC buyers. Second, intensifying (or weakening) the linkage to MNC buyers may also affect the productivity of the firm. Alfaro-Urena, Manelici, and Vasquez (2019b) show that domestic firms that become first-time suppliers to MNCs do not only grow in size, but also improve their productivity. In the model in Section 3.5.1, demand and productivity effects have an isomorphic effect on the wages of incumbent workers. In practice – as we discuss in Section 3.4.5 – shocks to demand and productivity may have different implications for workers.

Two arguments motivate why our measure of firm-level exposure to MNCs only considers the exposure through firm-level supplying linkages. First, meta-analysis studies find that the "average forward spillovers [of FDI (the effects of MNCs on the productivity of their buyers)] are negligible" (Havránek and Iršová, 2011). In contrast, the same meta-analysis studies find that the "average backward spillover [of FDI (the effect of MNCs on the productivity of their suppliers)] is large."

Second, because most MNCs in Costa Rica are export-oriented, MNCs and domestic firms do not compete directly in the product market. In 2018, we conducted a survey with executives from MNCs in Costa Rica (see Alfaro-Urena, Manelici, and Vasquez, 2019b). In

[28] Across all domestic firms, the average (median) share of total sales to MNCs in 2017 is 0.24 (0.11). Meanwhile, the average (median) share of *direct* sales to MNCs for the same firms and year is 0.07 (0.00). Thus, most of the variation in total sales to MNCs is actually driven by indirect sales, for which MNCs are buyers-of-buyers.

[29] While the formula of $\Delta FLE_{j(i),t}$ has an intuitive empirical structure, it also has the advantage that (under some theoretical assumptions) it is proportional to changes in the value added per worker of firm j. We develop this intuition formally in the model in Section 3.5.

particular, we investigated the reasons why these MNCs chose to open a subsidiary in Costa Rica. The four most important attractions of Costa Rica were the quality of education, the relatively competitive wages, the tax incentives available in Special Economic Zones, and the distance to target markets. The domestic market was one of the least important reasons for coming to or staying in Costa Rica. Moreover, most of these MNCs have the parent located in a nearby country (such as the U.S., Canada, or another Latin American and Caribbean country) and tend to export a large share of the production to their parent. Finally, meta-analyses on the "horizontal spillovers of FDI" conclude that the effects of FDI on the productivity of domestic firms in the same industry are, on average, zero (Havránek and Iršová, 2013).

3.4.3 Instrumental Variables Strategy

We are interested in the causal estimates of the effects of changes in the labor market and firm-level exposures to MNCs on worker wages. OLS estimates of β_{LME} and β_{FLE} from equation (3.5), however, may be biased due to simultaneity and omitted variables. For instance, workers in a labor market may receive unobserved positive productivity shocks, which would lead to both expansions of MNCs and higher wages for workers in that market (independently of the MNC expansions). In such a case, OLS would overestimate the effect of increases in labor market exposure to MNCs on wages. Finally, OLS estimation of the firm-level exposure coefficient may also be biased if shocks to the productivity of workers in a given firm affect the growth of the direct or indirect MNC buyers from that firm.

To address these concerns, we exploit the same variation in MNC employment in Costa Rica as that used for the IV estimation of the MNC premium, namely the variation in MNC employment *outside* of Costa Rica for MNCs with subsidiaries in Costa Rica. Specifically, we construct the instruments for $\Delta LME_{s(i),t}$ and $\Delta FLE_{j(i),t}$ by using $\Delta\mathcal{O}_{st}$ and $\Delta\mathcal{O}_{mt}$ as the IV analogues of $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{M}_{mt}$.[30] $\Delta\mathcal{O}_{st}$ and $\Delta\mathcal{O}_{mt}$ are the percentage changes in employment of MNC groups in market s in Costa Rica and of the specific MNC m, respectively, both *outside* of Costa Rica.

The exclusion restriction for the IV of $\Delta LME_{s(i),t}$ is that changes between $(t-1)$ and t in the employment outside of Costa Rica of MNCs whose subsidiary is in labor market s in Costa Rica are not correlated with contemporaneous shocks to the productivity of workers in labor market s in Costa Rica. Two pieces of evidence suggest that this assumption is likely to hold. First, the average (median) share of the worldwide number of workers of each MNC group who work in the Costa Rican subsidiary of that MNC group is 0.8% (0.2%). This makes it unlikely that shocks to the productivity of workers in market s in Costa Rica would drive the worldwide growth of these MNCs. Second, and more importantly, as MNC subsidiaries in Costa Rica tend to be in a different (upstream) industry than that of the MNC

[30] This means that we instrument $\Delta LME_{s(i),t} \equiv \sum_{s'} \pi_{s(i)s',t_0}\psi_{s'}\nu_{s',t-1}\Delta\mathcal{M}_{s',t}$ by $IV\left(\Delta LME_{s(i),t}\right) \equiv \sum_{s'} \pi_{s(i)s',t_0}\psi_{s'}\nu_{s',t-1}\Delta\mathcal{O}_{s',t}$ and $\Delta FLE_{j(i),t} \equiv \sum_m \theta_{j(i)m,t-1}\Delta\mathcal{M}_{mt}$ by $IV\left(\Delta FLE_{j(i),t}\right) \equiv \sum_m \theta_{j(i)m,t-1}\Delta\mathcal{O}_{mt}$. We weight the importance of shifters by $(t-1)$ and t_0 values to avoid that our measures of exposure reflect endogenous responses of labor markets and firms to the MNC shocks.

group, it is less likely that shocks to the productivity of workers in the upstream industry in Costa Rica are correlated with shocks to a different industry outside of Costa Rica.

The exclusion restriction behind the IV of $\Delta FLE_{j(i),t}$ is that changes between $(t-1)$ and t in the size of MNC m outside of Costa Rica are not correlated with contemporaneous shocks to the performance of domestic firm j, a direct or indirect supplier to the subsidiary of MNC m in Costa Rica. Similar to the discussion of the exclusion restriction for $\Delta LME_{s(i),t}$, the assumption is plausibly valid for two reasons. First, the average share of the input costs of MNC subsidiaries that are costs with inputs from a given domestic firm is less than 1%. Hence, it is unlikely that shocks to specific domestic firms would affect the performance of the Costa Rican subsidiary of m, and, even more unlikely to affect the performance of the MNC outside of Costa Rica. Given that most domestic firms are exposed to MNCs mostly indirectly (as suppliers of suppliers of MNCs), this assumption is even more plausible. Second, because subsidiaries of MNCs in Costa Rica tend to be in a different industry than that of the MNC group, this further weakens the link between a supplier to the subsidiary in Costa Rica and the MNC outside of Costa Rica.

3.4.4 Estimates of the Indirect Effects of MNCs on Wages

Table 3.4 reports OLS estimates of equation (3.5). In Column (1) we only use the change in the labor market exposure (LME) to MNCs as an explanatory variable, in Column (2) we use only the change in the firm-level exposure (FLE), and in Column (3) we use both changes at the same time. Both changes in LME and FLE are strongly and positively associated with changes in worker wages. Reassuringly, the magnitudes of both the LME and FLE coefficients are largely unaffected by whether the two measures are included together or separately. This indicates that the market-level variation in exposure to MNCs is mostly unrelated to the firm-level exposure.

To interpret the magnitude of the OLS estimate of the coefficient on the labor market exposure, consider a hypothetical two-digit industry $\times$ region labor market s with the following characteristics: π_{ss,t_0} is 0.82 (the average share of stayers in the same market across all markets), $\psi_{s(i)}$ is 1.2 (a typical 20% MNC wage premium), $\nu_{s,t-1}$ is 0.25 (a higher than average share of MNC employment in $(t-1)$ in s). In the first scenario, this market experiences growth in MNC employment of 4% between $(t-1)$ and t (the average value for $\Delta\mathcal{M}_{st}$ across all markets and years). In the second scenario, MNC employment remains constant. In both scenarios, assume that all other markets do not experience any change in MNC employment between $(t-1)$ and t. The OLS coefficient of 0.05 on the labor market exposure measure indicates that the wages of stayers in domestic firms would grow 0.05 percentage points more in the first scenario relative to the second. Alternatively, one can compare the growth in the wages of two otherwise identical workers who happen to be in labor markets that differ by one standard deviation (7.04) in their labor market exposure to MNCs. Therefore, the wage of the worker in the more exposed market would grow 1.02 percentage points more than the wage of the worker in the less exposed market.

To interpret the magnitude of the OLS estimate of the importance of the firm-level exposure, consider two domestic firms: one whose share of total sales to this MNC in $(t-1)$ is 0.24 (the average share of total sales to MNCs in 2017) and a second whose share is 0. Assume that this MNC grows by 4% between $(t-1)$ and t. Given these numbers, the OLS coefficient of 0.74 on the firm-level exposure implies that the wages of stayers in the first firm would grow 0.71 percentage points more than those of the stayers in the second firm. Alternatively, one can compare the growth in wages of two otherwise identical workers who happen to work for firms that differ by one standard deviation (0.38) in their firm-level exposure to MNCs. Then, the wage of the worker in the more exposed firm would grow 1.25 percentage points more than the wage of the worker in the less exposed firm.

Table 3.3 reports the first stage and reduced form estimates for the leading IV Set 1. From Columns (1) and (2) we learn that both instruments are strongly correlated with the endogenous variable they are meant to instrument for. In Columns (3) and (4) we regress each measure of the change in exposure on the IVs of both measures. As expected, each measure of exposure is only correlated with its IV. Columns (5) to (7) contain the reduced form coefficients, which show a strong relationship between changes in wages in Costa Rica and the instruments (based on changes in the size of MNCs outside of Costa Rica).

In Columns (8) to (11) of Table 3.3, we perform a falsification test to verify that future values of the instrument (based on future changes in the size of MNCs outside of Costa Rica) do not predict current changes in worker outcomes in Costa Rica. We find that the year $(t+1)$ values of the instruments are not correlated with year t changes in worker wages. Hence, our identification strategy isolates market-level and firm-level shocks caused by shocks to MNCs rather than other temporal confounds.

Columns (4) to (6) of Table 3.4 report the leading IV estimates. The F-statistic is 26.3 when we only use the LME measure as an explanatory variable, 83.4 when we only use the FLE measure, and 41.2 when we use both variables together, all of which are above the commonly used threshold of 10. The IV estimates are 2.9 times larger than the OLS estimates for the labor market exposure measure and 4.5 times larger for the firm-level measure. A plausible candidate explanation for the larger IV estimate of β_{LME} is related to the simultaneous determination of changes in wages and MNC presence. The fact that the OLS estimate of β_{FLE} is also attenuated is in line with other empirical work that uses firm-level shocks to firm performance to measure rent-sharing. The typical explanation for this pattern is that wages respond more strongly to lower frequency fluctuations in surplus, or, put differently, short-run fluctuations in firm performance are poor measures of underlying changes in product market conditions (Guiso et al., 2005; Garin and Silvério, 2018; Card et al., 2018; Kline et al., 2019).

Robustness checks. First, we used Orbis data to construct an alternative set of IVs for the two measures of exposure to MNCs. We redefine $\Delta\mathcal{O}_{st}$ as the percentage change in MNC employment outside of Costa Rica for MNCs with subsidiaries in at least one of twenty Latin American and Caribbean countries. Over 90% of these 4,595 MNC groups do not have a subsidiary in Costa Rica. To assign changes in MNC employment outside of Costa Rica to two-digit industry × region markets in Costa Rica we rely on the main two-digit industry code of these MNCs. Namely, we attribute the change in employment of each MNC to a two-digit industry × region market in Costa Rica based on the common two-digit industry and based on the year $(t-1)$ share of total employment in that two-digit industry in the given region. Given that with this new source of variation we only have market-level variation, $\Delta FLE_{j(i)t}$ is redefined as $\sum_{s'} \theta_{j(i)s',t-1} \Delta \mathcal{M}_{s't}$. Fortunately, there is still considerable firm-level variation in the extent to which domestic firms $j(i)$ supply MNCs in market s'.

In contrast to IV Set 1, IV Set 2 now assumes that shocks to MNCs in a given industry outside of Costa Rica are correlated to shocks to MNC subsidiaries *in the same industry* in Costa Rica (as opposed to the upstream industries of the MNC subsidiaries in Costa Rica). Table B.2 (B.1.1) (Columns (3) and (4)) confirms a strong positive correlation between $\Delta \mathcal{M}_{st}$ and $\Delta \mathcal{O}_{st}$ (0.53 with year and two-digit industry fixed effects). At the same time, one can also notice that this correlation is lower than for the $\Delta \mathcal{O}_{st}$ from IV1, which uses the more direct variation in the behavior of the actual MNCs in Costa Rica.

Table B.10 (B.1.3) is the counterpart of Table 3.3 for IV Set 2. Both the first stage and reduced form relationships are weaker for IV Set 2 than for IV Set 1. Nonetheless, the patterns are qualitatively similar. Moreover, IV Set 2 also passes the falsification test by failing to predict changes in wages with leads of the IVs. Table 3.4 presents alongside the IV estimates based on IV Set 1 alone, IV Set 2 alone, and the two sets of IVs together. Reassuringly, despite using a different source of variation, the two IV sets deliver almost identical results. Formally, we perform a standard Hansen-J overidentification test, which fails to reject that the estimates are statistically the same (our Hansen-J statistic has a p-value of 1).

Second, Table B.11 (B.1.3) reports the OLS and IV estimates from the main equation (3.5) for two samples: the main sample of stayers, and a sample with year $(t-1)$ firm-level cohorts. In addition to stayers at firm j and workers who move from j directly into new employment in year t, the firm-level cohort sample also includes individuals who move from j into unemployment (as long as they find employment by the end of t). The estimates from the firm-level cohort sample tend to be slightly smaller than those from the main sample of stayers. This is driven by the fact that the sample is not selected on worker outcomes in t. We prefer the sample with stayers because it enables us to link the change in wages of a worker to the change in firm-level exposure to MNCs of the same employer.

Third and last, in Table B.12 (B.1.3), we compare our main OLS and IV estimates with OLS and IV estimates from regressions with fewer fixed effects than those from the main specification in equation (3.5). Results remain largely unchanged.

3.4.5 The Effect of Changes in Value Added per Worker

We now replace the change in firm-level exposure to MNCs by the change in value added per worker of the firm – which no longer intends to capture changes in exposure to MNCs alone. Precisely, we estimate the following regression, where $\Delta\left(VA/L\right)_t$ is the percentage change increase in value added per worker between year $(t-1)$ and t:

$$\begin{aligned}\Delta w_{it} =& \beta_{LME}\Delta LME_{s(i),t} + \beta_{VA/L}\Delta\left(VA/L\right)_t + \\ &+ \boldsymbol{X'_{ij,t-1}}\boldsymbol{\beta}_c + \alpha_{j(i)} + \gamma_{ind(s(i))\times t} + \mu_{reg(s(i))\times t} + \rho_{ind(s(i))\times reg(s(i))} + \epsilon_{it}.\end{aligned} \tag{3.8}$$

Our objective is to estimate the “rent-sharing" coefficient – the pass-through of changes in the value added per worker on worker wages. The intuition of the first stage is that shocks to the size of the MNC buyers m of a firm j turn into shocks to the sales of j to buyers m. This is likely to affect the firm's value added per worker. Changes in sales to MNC buyers may bring two types of changes for the firm – to its scale and productivity. The exclusion restriction requires that changes in sales to MNCs only affect worker wages through the extent of rent-sharing of their firm. One scenario that stands out as problematic is one in which workers in firms that start selling more to MNCs increase their productivity in ways that are directly valued by competing employers. In such a case, the IV would overestimate the degree of rent-sharing, as it would attribute the part of the increase in wages coming from improved outside options to rent-sharing. While we cannot directly rule out this threat, two aspects make it less likely. First, we study the effects of yearly changes in value added per worker on annual changes in wages. Alfaro-Urena et al. (2019b) find that firms gradually improve their total factor productivity. Moreover, if working for a firm that intensifies its relationship with MNCs leads only to context-specific learning, then workers should not see their outside options improve. Last, we assume that nonpecuniary firm amenities do not react to the shock to firm value added.

Table 3.5 contains the estimates from four OLS regressions. The first three introduce $\Delta LME_{s(i),t}$ alone (Column (1)), the change in value added per worker alone (Column (2)), or both explanatory variables at once (Column (3)). All three columns contain the main set of fixed effects from equation (3.5), namely region × year, two-digit industry × year, and two-digit industry × region (in addition to firm fixed effects). Column (4) is an OLS regression with only the change in value added as the explanatory variable, and in which we replace the three sets of fixed effects just mentioned by two-digit industry × region × year (and continuing to keep the fixed effects). As the measure of labor market exposure to MNCs varies at the two-digit industry × region × year level, it is absorbed by this last set of fixed effects.

The OLS estimate of the coefficient on $\Delta LME_{s(i),t}$ from Column (3) is almost the same as that from Column (3) in Table 3.4. This suggests that the market level variation in the exposure to MNCs is largely unrelated to the variation in firm-specific outcomes. As for the coefficient on changes in value added per worker, its OLS estimate is identical and equal to 0.008 across all three specifications. Table 3.5 presents the IV results. The IV estimate of the coefficient on $\Delta LME_{s(i),t}$ from Column (3) is only a bit smaller than that in Column (6) in Table 3.4. The IV estimate on the change in value added per worker is unaffected by whether we include $\Delta LME_{s(i),t}$ or not, and by the set of fixed effects we use.

The IV estimate of the pass-through of changes in value added per worker on wages is 0.09, which is about 11 times larger than the OLS estimate. Finding an OLS estimate that is biased towards zero is in line with the existing literature. The most likely culprits for this bias are either the noisy nature of the measure of surplus (here, value added per worker) or the fact that wages may be less responsive to transitory fluctuations in rents. Our IV estimate of 0.09 is lower than the existing IV estimates: 0.14 for exporters in Portugal (Garin and Silvério, 2018) and 0.35 for patent-winning firms in the U.S. (Kline et al., 2019). This is consistent with the intuition that direct or indirect suppliers to MNCs in developing countries may have lower hiring and training costs than exporters or patent winners in developed countries.[31] Alternatively, consider a model that explains rent-sharing through Nash bargaining. Through the lens of that model, we can rationalize our lower rent-sharing coefficient by a lower Nash bargaining weight for workers in developing countries, where unemployment and informality are more prevalent.

The relationship between the estimates of for β_{FLE} and $\beta_{VA/L}$ coefficients. Consider our IV estimate of 3.3 for $\hat{\beta}_{FLE}$ and its 95% confidence interval of [1.5,5.1]. We ask whether this range is reasonable in the light of our estimate for the rent sharing coefficient $\hat{\beta}_{VA/L}$ of 0.09. To that end, let us return to our hypothetical example with only one MNC in the economy that grows 4% between $(t-1)$ and t. An incumbent worker at a domestic firm selling 24% of its sales to the MNC sees her wages grow between 1.4 and 4.9 percentage points more than a worker working at a firm selling 0% to the MNC. Assume that no other factors are impacting the value added per worker of these firms besides their different exposure to the expanding MNC. The IV estimate of the rent-sharing coefficient is 0.09 and its 95% confidence interval is [0.04, 0.15]. Using the highest estimate of the rent-sharing that our data cannot reject (0.15) and the lowest prediction of the percentage points growth of the wages in the first firm (1.4), yields that the value added per worker of the worker would need to increase by 9.3 percentage points from year $(t-1)$ and t. Evidence from Alfaro-Urena et al. (2019b) suggests that this magnitude is plausible.[32]

3.4.6 Who Gains from Increases in Exposure to MNCs?

College vs. non-college-educated workers. In Table B.13 (B.1.3) we present the OLS and IV estimates from equation (3.5) on two groups of stayers in domestic firms: only those with a college degree (Panel B) and only those without a college degree (Panel C). There are two main messages that emerge from this comparison. First, both the OLS and IV estimates from the full sample are the most similar to those from the sample of workers without college (with the latter estimates being a bit higher than those from the full sample). This similarity

[31] In Costa Rica, exporters and patent-holders are, on average larger and more productive than suppliers to MNCs.

[32] In Alfaro-Urena et al. (2019b), we find that during the year when domestic firms become a first-time supplier to an MNC, on average, their value added per worker increases by 6%. While we have not yet explored how the value added per worker increases with subsequent increases in the amounts sold to MNCs, one might speculate that there are non-linearities in learning from MNCs. While the magnitude of the IV estimate of the firm-level exposure to MNCs appears large, a high elasticity of purchases from local suppliers to MNC employment and non-linearities in learning would make this magnitude plausible.

is somewhat unsurprising once we realize that almost 90% of the observations from the full sample come from this subgroup. In general, workers who did not attend college represent the majority of the workforce in both domestic firms *and* MNCs. Second, the results for college-educated workers are less conclusive. Their analysis is hampered by the fact that the reduced form estimates are not significant. Because college-educated workers are in the minority, one may need to define more targeted shocks for them within a market or firm.

We repeat the analysis by education level for the specification in equation (3.8), which replaces the change in firm-level exposure to MNCs by the change in value-added per worker. Table B.15 (B.1.3) presents the OLS estimates, which are mainly identical for the two types of workers. However, as Table B.16 (B.1.3) shows, the IV estimates diverge again. The rent-sharing coefficient of college-educated workers is not significantly different from zero, whereas the rent-sharing coefficient of workers without college is slightly larger than the pooled estimate. The IV estimation of the coefficients for college-educated workers is, again, hindered by the reduced form estimates being non-significant. The rent-sharing coefficient of workers without a college degree might also be higher because their wages tend to be more affected by firm-specific temporary productivity shocks, whereas those of high-skilled workers tend to be more affected by firm-specific permanent shocks (Friedrich et al., 2019).

Male vs. female workers. In Table B.14 (B.1.3) we split the sample of stayers into two groups: women only (Panel B) and men only (Panel C). The OLS estimates for the importance of changes in labor market exposure are similar for women and men. However, the IV estimates for the same coefficients diverge; while, for women, we no longer find a statistically significant effect, for men, the effect becomes larger than that for the full sample. The OLS estimates for the importance of changes in firm-level exposure suggest larger effects for women than for men. The IV estimates revert this pattern, with women experiencing only about 70% of the effects on men. Overall, women seem to not be in as good of a position as men to benefit from improvements in the labor market and firm-level exposure to MNCs.

We repeat this heterogeneity analysis also for the rent-sharing coefficient in equation (3.8). While the OLS estimates are identical for women and men (see Table B.17, B.1.3), the IV estimate of the rent-sharing coefficient for women is 0.07 and for men is 0.10 (see Table B.18, B.1.3). Thus, women's wages are only 70% as responsive to observable measures of the surplus per worker as men. This is lower than the 90% found by Card et al. (2015) for Portugal. As in Kline et al. (2019), a potential explanation for the gender difference in earnings pass-through is that the marginal replacement costs of men could – on average – exceed those of women. If women work in occupations requiring lower hiring and training costs, this could explain their relatively lower pass-through rate.[33]

The characteristics of workers with different levels of labor market exposure to MNCs. To assess the distributional implications of expansions or contractions in MNC

[33] Costa Rican women have relatively low labor force participation rates (43% in 2018, relative to 58% in Portugal in 2010). Costa Rican working women also tend to concentrate in more traditional service-oriented occupations.

employment across labor markets, one needs to understand the characteristics of workers in those labor markets. Do they tend to be more college-educated, are they more likely to be male, already earn high wages? Table B.5 (B.1.1) presents descriptive statistics on workers in a given labor market in the pre-period (2006 to 2008). Workers in 2006 to 2008 are separated in terciles by the percentage change in MNC employment between 2009 and 2017 ($\Delta\mathcal{M}_{s,2009-2017}$) in their labor market s in a given year between 2006 and 2008.

Labor markets in the top tercile of MNC employment growth after 2009 already had a 20% higher share of MNC employment between 2006 and 2008 than those in the bottom tercile and a 1.2% higher MNC premium. On average, workers in the top tercile labor markets had 9% higher labor earnings that those in the bottom tercile, were 3% more likely to be college-educated, and were 4% *less* likely to be male. These workers earned higher labor earnings across both domestic firms and MNCs, and across levels of education. Thus, the expansion of MNCs after 2009 is likely to have benefited workers with relatively more favorable initial labor market conditions. The higher prevalence of women in these labor markets, however, had a counterbalancing distributional benefit.

The labor markets in the bottom tercile – which experienced, on average, contractions of about 22% in MNC employment – tended to have higher shares of MNC employment that the middle tercile. Thus, the growth of MNC employment post-2009 was not monotonically related to the initial share of MNC employment. Workers in the bottom tercile were 5% more likely to be male than in the mid tercile and 3% less likely to have a college degree. Thus, the contractions of MNCs are likely to have hurt relatively more workers without a college education and men.

The characteristics of workers with different levels of firm-level exposure to MNCs. Table B.6 (B.1.1) provides descriptive statistics on the sample of domestic firms and their incumbent workers between 2009 and 2017, by the tercile of subsequent yearly growth in the firm-level exposure to MNCs. Firms in the top tercile of future changes in firm-level exposure to MNCs tend to employ 32 more workers on average, pay 26% higher wages to their incumbent workers, have 3% more male workers, and 4% more college-educated workers than firms in the bottom tercile. However, similar to the case of the labor market exposure, there is a non-monotonous relationship between changes in firm-level exposure to MNCs and initial conditions. Firms who experience contractions in firm-level exposure tend to employ nine workers more on average, pay 7% higher wages to incumbent workers, have 8% more male workers, and 1% more college-educated workers than firms in the mid tercile.

The correlation between the labor market and firm-level exposure to MNCs. To understand the distributional implications of exposure to MNCs, one also needs to know whether the workers whose labor markets and firms experience increases in exposure to MNCs are the same or not. Figure 3.4 is a binned scatter plot of the worker-year labor market exposure to MNCs with respect to the worker-year firm-level exposure to MNCs. Both measures have been residualized by the same fixed effects and controls used in equation (3.5). The plot displays a clear negative relationship between the labor market and the firm-level exposures of workers. Workers who are hurt by the contraction of MNCs in an industry may benefit from working in a firm that supplies MNCs in another industry, and the reverse. Given these counterbalancing forces, increases in the presence of MNCs have an ambiguous effect on inequality.

3.4.7 Back-of-the-Envelope Aggregation of the Wage Gains from MNCs

A comprehensive evaluation of the costs and benefits of attracting MNCs on the labor market requires knowing (i) the effects on the wages of employed workers, and (ii) the effects on employment generation (or destruction) both in the formal and informal sectors. In this paper, we have focused on the first point. We can use our reduced-form evidence from Sections 3.3 and 3.4 to approximate the benefits of attracting MNCs based on the wage gains that we measure. Since the extensive margin of employment is outside the scope of our empirical analysis, we assume full employment in our calculations and abstract from potential effects due to changes in unemployment. Also, for the sake of simplicity, we focus on our average treatment effect estimates.

We first consider the wage gains of workers directly hired by MNCs. Between 2007 and 2017, 500,492 individuals started working for an MNC in Costa Rica, with an average employment duration at those MNC jobs of 2.4 years (28.8 months). We assume that those individuals were drawn from similar domestic firms to those in our movers analysis. Thus, we can rely on our estimate of the MNC premium of 9%. In the year before moving to the MNC, the average monthly earnings of employed workers were approximately 840 real U.S. dollars of 2013. Given the 9% premium, the average worker increases her monthly earnings by 76 U.S. dollars. Let us take a conservative approach and assume that, in the absence of those MNC jobs, workers would earn the same 840 U.S. dollars per month. The estimate of aggregate wage gains owed to the wage premium is 1,095 million U.S. dollars (76 U.S. dollars $\times$ 28.8 months $\times$ 500,492 workers) for the entire period or about 100 million U.S. dollars per year.[34]

Let us now consider the wage gains for workers in domestic firms. Assume that the wages of public sector employees are unaffected. Also assume that all domestic firms sell (directly or indirectly) around 24% of their sales to MNCs (the average in the economy). Between 2009 and 2017, the average market experienced an increase in MNC presence of 12.9%. Using these averages together with our IV estimates from Table 3.4, we find a change in earnings for the average worker at a domestic firm equal to $3.3 \times 0.24 \times 12.9 + 0.14 \times 12.9 \approx 12\%$ between 2009 and 2017. Hence, MNCs lead to indirect increases in labor earnings of about 1.5% per year. Taking the average monthly earnings of 640 real U.S. dollars in the economy and applying these gains to approximately 600,000 incumbent workers at domestic firms, we find aggregate gains of around 69 million U.S. dollars per year ($0.015\times$ 640 U.S. dollars $\times$ 12 months $\times 600,000$ workers).

[34] This amount could increase to 166 million U.S. dollars per year if, instead, we assume that workers were drawn from the set of compliers identified by our IV estimate of the MNC premium.

Adding the wage gains of workers directly hired by MNCs to those of workers employed at domestic firms, we reach an estimate for the average wage gains of around 169 million U.S. dollars per year. Given an average number of MNC workers of about 230,000 per year over the period of study, these 169 million U.S. dollars per year amount to 735 U.S. dollars per MNC job per year. While this gain may seem small, it is larger than the average monthly earnings of a worker in Costa Rica (640 U.S. dollars). Moreover, these numbers are a lower bound, as they rely on the assumption that, in the absence of MNCs, the domestic economy would be able to generate the number of jobs created by MNCs.

How do these numbers compare to the value of the tax exemptions that MNCs receive? The official estimate by the Costa Rican government for the 2011 to 2015 period shows that the foregone tax collection due to tax exemptions offered to MNCs in *Zona Francas* (the Costa Rican Special Economic Zones) amounts to 467 million U.S. dollars on average per year.[35]. This estimate of the costs of attracting MNCs is an upper bound since it assumes that MNCs would remain in the country in the absence of tax benefits. Comparing the wage benefits with the costs, we notice that the increase in labor earnings is equivalent to around 36% of the costs.

Does this mean that there is a net loss from attracting MNCs? Not necessarily. As stated before, in this simple calculation, we are abstracting from other potentially important effects that manifest on the extensive margin of employment and informality. Moreover, this calculation also does not account for the effects of MNCs on domestic firm profits, in particular, the part of those profits that is not shared with workers in the form of wage increases. These caveats notwithstanding, we find that the gains in labor earnings are unable to justify the generous tax incentives extended to MNCs by themselves.

3.5 A Stylized Model of an Economy with MNCs

Motivation. The first objective of the model is to formalize the channels by which MNCs affect workers in domestic firms, i.e., through changes in the level and the composition of labor demand (given the MNC wage premium) and changes in domestic firm outcomes (given supply-chain linkages with MNCs). The second objective of the model is to combine the structure of the model with the plausibly exogenous MNC shocks to infer the degree of labor market imperfections in the economy. On the one hand, if incumbents are stuck at their firm, there would not be any gains in earnings. On the other hand, if incumbents see domestic employers as perfect substitutes, they would be able to take full advantage of the potential gains in earnings. Thus, policies to attract MNCs can be more or less successful in improving worker earnings depending on the magnitude of the labor market imperfections. Moreover, what we learn about the ways in which these imperfections affect worker outcomes is not specific to the MNC shocks, but it also applies more broadly to other shocks affecting workers.

[35]See https://procomer.com/downloads/zonas-francas/balance_zf_2011_2015.pdf.

We model wage setting using a wage posting model, as opposed to a bargaining model. This choice is motivated by the specificity of Costa Rican labor market institutions, which are closer to those of the U.S. than to those of Europe or other Latin American countries. The unionization rate is notably low, even slightly lower than that in the U.S. Collective bargaining agreements over wages and working conditions are also limited (OECD, 2017c). Moreover, the employment protection legislation for workers with regular contracts is one of the least stringent in the OECD and Latin American countries (OECD, 2017c).[36] These features suggest that workers have a low bargaining power over their wages (particularly workers without college, who are in the majority in both domestic firms and MNCs).

In line with previous work on rent-sharing with workers (Becker, 1962; Stevens, 1994; Manning, 2006; Garin and Silvério, 2018; Kline et al., 2019), we model firms as having to bear a cost of recruitment and/or training when hiring a worker. Put differently, firms need to engage in costly searches for workers. Alternatively, workers and firms need to invest in the acquisition by workers of industry (or firm) specific capital, whose costs and returns are later shared by the worker and employer (Hashimoto, 1981; Neal, 1995; Lazear, 2009). This imperfect substitutability of incumbent workers with new hires is what allows the former to benefit from positive shocks to the performance of their employer.

Three pieces of evidence suggest that this modeling choice is also appropriate for Costa Rica. First, PricewaterhouseCoopers conducts annual studies on labor turnover across MNCs in Costa Rica (PwC, 2018). Labor turnover costs are perceived as high, and both MNCs and policy-makers seek ways to reduce these costs. Second, in our survey of HR executives from MNCs in Costa Rica, the costs of hiring and training stand out as one of the main reasons why MNCs choose to pay higher wages than domestic firms. These pieces of evidence point to the hiring and training costs of MNCs being larger than those of domestic firms. That said, incumbent workers at domestic firms also tend to receive higher wages than new workers in the same occupation.[37]

Summary. We propose a static model featuring the labor and product markets of a small open economy. There are three types of agents in this economy: workers, MNC subsidiaries, and domestic firms. The economy is formed by a finite but large number of industries indexed by s. We assume that in each industry, there is one MNC and a large number of domestic firms producing differentiated varieties.

MNCs produce according to an international demand shifter and export all their production. Each MNC produces using labor and a composite of varieties purchased from domestic firms. When hiring workers, MNCs need to incur a hiring and training cost and pay the workers an exogenous premium with respect to the market wage. The assumption of an exogenous premium is not problematic given that this model aims to formalize the effects of

[36] For details, see B.4.1.

[37] We calculate the ratio of the average wages paid to new workers hired in year t in four-digit occupation o by a given firm j to the average wages paid in the same year t by the same firm j to its incumbent workers in the same occupation o, $\left(\overline{w}_{oj,t}^{NEW}/\overline{w}_{oj,t}^{INC}\right)$. Incumbent workers are workers who are employed by firm j in both $(t-1)$ and t. In Table B.8 (B.1.2), we show that the average (median) of this ratio is 0.88 (0.86).

MNCs on wages set *by domestic firms*.[38] We take the set of domestic suppliers of each MNC as exogenous, i.e., we do not model the decision of MNCs regarding which domestic firms to purchase from. There is one final consumption good, which is a composite of domestic varieties (whose prices are determined in equilibrium), and an imported good (whose price is set internationally). Domestic firms produce the domestic variety using labor only. Their variety serves as an input to either the production of the final good or the production of MNCs.

The modeling of the labor market for domestic firms builds most directly on Kline et al. (2019). As in their model, there are two types of workers of homogeneous ability: new workers and incumbent workers. Domestic firms post a wage for their incumbent workers, who decide, based on this wage, competitive wages in all industries and their taste draws whether to remain or not with the firm. Domestic firms also choose how many new workers to hire. To hire new workers, firms need to pay them a wage equal to the market wage and cover hiring and training costs.

The three innovations of our model (relative to Kline et al., 2019) are the following: (i) it models industries explicitly (allowing workers to have idiosyncratic preferences over industries and featuring a switching cost to be incurred by incumbent workers whenever they choose to become new workers in a different industry), (ii) it incorporates a richer structure for the product market, (iii) it introduces MNCs – exceptional firms whose wage setting is affected by factors exogenous to the local labor and product markets. The wages paid to incumbent workers by domestic firms are affected by the expansions of MNCs in three ways. First, since MNCs and domestic firms compete for workers in the labor market, the expansion of MNCs affects wages through a standard increase in labor demand. Second, since MNCs pay a premium beyond the market wage, the expansion of MNCs shifts the composition of jobs in those same industries towards jobs with a wage premium. Third, since domestic firms are directly and indirectly exposed to MNCs through supply linkages, the expansion of MNCs can potentially affect domestic wages through rent-sharing between the worker and her domestic employer.

Three structural parameters govern the magnitude of the wage gains of incumbents from MNCs. The first is the cost of hiring and training the first hire (as a proportion of the domestic market wage). The second is the elasticity of hiring and training costs with respect to the number of new hires. These two parameters inform us about the marginal cost of replacement of an incumbent worker and, thus, of the potential wage gains of an incumbent worker when her employer improves its performance. The last parameter is the elasticity of worker retention with respect to the posted wage. This parameter informs us how much an incumbent worker at a domestic firm can benefit from increases in her outside options and replacement costs. We estimate these parameters in Section 3.5.5.

[38] Proposing a microfoundation for the MNC premium is outside the scope of this paper. That said, Section 3.3 discusses a set of plausible explanations, such as the existence of MNC-specific policies over wage equity within the MNC group. Note that the MNC premium (which results from comparing the wages paid to new hires by MNCs vs. the wages paid to new hires by domestic firms) is conceptually different from the within-firm difference between the wages paid to incumbent workers vs. those paid to new hires.

3.5.1 The Product Market

The production of MNCs and their demand for domestic intermediates. For notational simplicity, we assume that there is only one MNC per industry s, which we denote by $MNC(s)$. We assume that the MNC in s faces a demand given by $Q_{MNC(s)} = B_{MNC(s)} p_{MNC(s)}^{-\sigma}$, where σ is the elasticity of demand and $B_{MNC(s)}$ is a demand shifter. We assume that $B_{MNC(s)}$ is set in the rest of the world and is exogenous to labor and product market conditions in the domestic economy. For brevity, we suppress the subscript s for now. All MNCs have a constant elasticity of substitution (CES) production function given by:[39]

$$Q_{MNC} = T_{MNC}\Big(L_{MNC}^{\frac{\sigma-1}{\sigma}} + J_{MNC}^{\frac{\sigma-1}{\sigma}}\Big)^{\frac{\sigma}{\sigma-1}},$$

where T_{MNC} is the factor-neutral productivity of the MNC, L_{MNC} is the labor employed by the MNC, and J_{MNC} is a composite of domestic varieties purchased from an MNC-specific exogenous set of domestic suppliers $\mathcal{S}_{MNC}$. The composite of intermediates is given by $J_{MNC}^{(\sigma-1)/\sigma} = \sum_{j\in\mathcal{S}_{MNC}} q_{j,MNC}^{(\sigma-1)/\sigma}$, where $q_{j,MNC}$ is the quantity of the domestic variety sold by firm $j \in \mathcal{S}_{MNC}$ to the MNC. The profit-maximizing MNC for the choice of the bundle of intermediates leads to $J_{MNC} = \left(\frac{\sigma-1}{\sigma}\right)^{\sigma}\left(\frac{A_{MNC}}{P_{MNC}}\right)^{\sigma}$ where $A_{MNC} \equiv B_{MNC}^{\frac{1}{\sigma}} T_{MNC}^{\frac{\sigma-1}{\sigma}}$ is a revenue shifter for the MNC. The demand from the MNC for the variety of supplier j is equal to:

$$q_{j,MNC} = \left(\frac{\sigma-1}{\sigma}\right)^{\sigma} \frac{A_{MNC(s)}^{\sigma} Q_{MNC(s)}}{J_{MNC(s)}} \equiv b_{MNC(s)} p_j^{-\sigma}, \tag{3.9}$$

where $b_{j,MNC)} \equiv P_{MNC}^{\sigma} Q_{MNC}$. A_{MNC} is determined only by forces outside of the domestic economy, i.e., by the exogenous worldwide demand for the product of the MNC subsidiary, B_{MNC}, and its exogenous productivity, T_{MNC}. An increase in either B_{MNC} or T_{MNC} triggers increases in the demand for inputs from domestic firms $j \in \mathcal{S}_{MNC}$.

Final demand for the products of domestic firms. We assume that domestic firms sell their output to either a domestic final good producer f or MNC subsidiaries.[40] Workers in this economy consume a final good Y, which is produced by final good producer f. Firm f does not hire workers but uses a technology that combines a domestic composite Y_{DOM} and an imported variety Y_{IMP} (purchased at an internationally set price). The domestic composite aggregates the production across all industries, which is, itself, an aggregate of the production of all domestic firms in each industry. The production of the final good is

[39] We assume the same elasticity of substitution σ in the demand and production functions. While this assumption is made for simplicity, it does not impact the insights of the model.

[40] This has the disadvantage of not allowing domestic firms to be exposed to MNCs indirectly through their domestic buyers. Nonetheless, by not allowing domestic firms to sell inputs to other domestic firms, we abstract from the choice of intermediate goods and simplify the domestic firm problem.

given by $Y = \left(Y_{DOM}^{(\sigma-1)/\sigma} + Y_{IMP}^{(\sigma-1)\sigma}\right)^{\sigma(\sigma-1)}$, where

$$Y_{DOM} = \left(\sum_s \left[\sum_{j\in s} x_j^{\frac{\sigma-1}{\sigma}}\right]\right)^{\frac{\sigma}{\sigma-1}},$$

and where x_j is the demand for the variety produced by firm j in industry s. This demand is given by $x_j = P^\sigma Y p_j^{-\sigma} \equiv b_{DOM} p_j^{-\sigma}$, where $b_{DOM} \equiv P^\sigma Y$ and P is the overall price index in the economy (over domestic and imported varieties).

The total demand for the variety of each domestic firm j is given by the demand coming from the domestic market and the demand coming from all of its MNC buyers:

$$Q_j = x_j + \sum_{s'\in\mathcal{B}_j} q_{j,MNC(s')} = b_{DOM} p_j^{-\sigma} + \sum_{s'\in\mathcal{B}_j} b_{j,MNC(s')} p_j^{-\sigma} = (b_{DOM} + B_{j,MNC}) p_j^{-\sigma} = B_j p_j^{-\sigma},$$

where $\mathcal{B}_j$ is the set of MNC buyers of firm j (if firm j does not supply any MNC subsidiary, then $\mathcal{B}_j$ is empty), $B_{MNC} \equiv \sum_{s'\in\mathcal{B}_j} b_{MNC(s')}$ and $B \equiv b_{DOM} + B_{MNC}$.

3.5.2 The Labor Market

There are two types of workers in our model: new workers and incumbent workers. New workers can be thought of as inexperienced workers, without previous attachment to a firm or industry. Incumbent workers start the period employed by firm j in industry s. Incumbent workers decide whether to remain with their current employer or join the pool of new workers to change employer.

Wage posting firms start with a number of incumbents I_j^0. They then need to make two decisions: the wage to post for incumbent workers (W_j) and the number of new workers to hire (N_j) at the competitive wage in s. When hiring N_j new workers, firms need to pay a recruitment and training cost $c(N_j)$. We assume $c(.)$ is twice differentiable and convex, which is consistent with the empirical evidence in favor of increasing marginal costs of recruitment (Manning, 2011). At the end of the period, the firm has a total of $L_j = I_j(W_j) + N_j$ workers available for production.

The hiring and wage setting of MNCs. Since our main interest, at this point, is on the effects of MNCs on employees working in domestic firms, we make two simplifying assumptions. First, we assume that MNCs start the period without incumbent workers of their own. This is equivalent to assuming that MNCs enter the economy at the beginning of the period. MNCs hire both on the entry market of new workers and on the market of former incumbent workers who break ties with their domestic employer. Therefore, for MNCs, $L_{MNC(s)} = N_{MNC(s)}$ (where L denotes the total number of workers in firm $MNC(s)$ and N denotes the total number of new workers hired by $MNC(s)$).

Our second simplifying assumption is that the MNC in s pays a wage $\psi_s\omega_s$, where ω_s is the domestic market wage of industry s, and $\psi_s \geq 1$ is a wage premium set exogenously by the HQ. We allow for heterogeneities in wage setting across industries.[41] ψ_s could be microfounded with MNC-wide fairness policies that bring the wage of new hires by MNCs in s from the domestic entry wage ω_s to a wage closer to that of HQ workers ($\psi_s\omega_s$).

When MNC subsidiaries hire new workers, they need to pay a hiring and training cost $C_{MNC}(N_{MNC(s)})$. To simplify derivations, we assume $C'_{MNC}(N_{MNC}) = c_0 N_{MNC}^{\alpha_m}$ (where $\alpha_m \geq \alpha > 1$ and where α is the corresponding exponent for the marginal cost of hiring and training of domestic firms). With α_m and α above 1, there are increasing marginal costs of hiring and training. The possibility that MNCs incur higher costs of hiring and training is consistent with MNCs employing workers in tasks with a more specialized and proprietary nature.

The labor market for new workers. At the beginning of the period, new workers receive taste shock draws for all industries. Their preferences over industries are distributed i.i.d. type 1 extreme value with dispersion parameter $1/\eta_N$. The draws of taste shocks are private information for new workers. Within an industry, new workers can be hired by either domestic firms or the MNC in that industry. All new workers hired by domestic firms j in industry s receive the domestic market wage in that industry, denoted ω_s. All new workers hired by MNCs receive an industry-specific premium over the domestic market wage in that industry. Specifically, a new worker hired by the MNC in s is paid $\psi_s\omega_s$ ($\psi_s \geq 1$). We assume that when hiring new workers, all firms in industry s take ω_s as given.

We assume that after choosing an industry s, new workers are assigned randomly to firms in that industry, such that the probability to join a given (domestic or MNC) firm is equal to the share of its demand for new workers over the total demand for new workers in industry s ($\frac{N_j}{N_s}$ for domestic firms or $\frac{N_{MNC)(s)}}{N_s}$ for the MNC). This random search feature implies that new workers cannot choose whether to join a domestic firm or the MNC in s. Their choice of industry is based on the expected or average wage for new workers (denoted by $\tilde{\omega}_s$) and not on the realized wage (either ω_s or $\psi_s\omega_s$). We do not allow new workers to revisit their choice of an industry once the random allocation of an employer in that industry has materialized.[42] This assumption is in line with the "good jobs" literature, which argues that above-market wages in "good jobs" can be sustained as an equilibrium outcome when they are rationed and assigned based on "luck", i.e., there is no feature of the worker that makes her more deserving of the job in terms of productivity or preferences (see Green, 2015, for a discussion).

Given these assumptions, the overall supply of new workers to industry s is given by

$$l_{Ns} = \frac{\tilde{\omega}_s^{\eta_N}(\psi_s)}{\sum_{s'} \tilde{\omega}_{s'}^{\eta_N}(\psi_{s'})} L_N^0, \tag{3.10}$$

[41] In our model, when an incumbent worker leaves her firm to join a firm in industry s, she is paid ω_s if hired by a domestic firm j in s or paid $\psi_s\omega_s$ if hired by the MNC in s.

[42] In a dynamic version of the model, new workers would have to wait one period for new taste draws across industries and employer draws within an industry. Moreover, we would need to assume workers are myopic, as they do not acknowledge that they become incumbents during the next period and that each firm would have a firm-specific rent-sharing.

where L_N^0 is the start-of-period economy-wide number of new workers, and

$$\tilde{\omega}_s(\psi_s) \equiv \omega_s \left(1 - \frac{N_{MNC(s)}}{N_s}\right) + \psi_s \omega_s \frac{N_{MNC(s)}}{N_s}.$$

This way to model the expected wage has a precedent in Beaudry et al. (2012). Note that whenever $\psi_s = 1$ (MNCs do not pay a wage premium) then $\tilde{\omega}_s(\psi_s) = \tilde{\omega}_s(1) = \omega_s$. In such case, the composition of employment (domestic vs. MNC employers) is irrelevant and all new workers in s are paid the same market wage ω_s. Otherwise, $\tilde{\omega}_s$ is increasing in the MNC wage premium ψ_s and in the share of new workers hired by the MNC in industry s.

The labor market for incumbent workers. Incumbent workers start the period employed by a domestic firm. They choose whether to stay or leave their beginning-of-period employer for a new employment opportunity depending on the wage posted by their initial employer, the expected competitive market wages in all industries, and their draws. In contrast to new workers, who choose to join industry s' (without prior firm or industry attachment), incumbent workers from firm j in industry $s(j)$ who become new workers in industry s' pay a cost to change industries that depends on their starting and ending industry $\left(\tau_{s(j)s'} \leq 1\right)$. We model this as an iceberg cost on the competitive market wage in industry s'.[43]

The initial number of incumbent workers of each domestic firm j in industry s is denoted by I_j^0 and is taken as given. The domestic firm has to decide and post a wage W_j for incumbent workers at the beginning of the period. After the wage is posted, incumbent workers decide whether to remain with firm j and earn W_j, or to switch to a different employer.

Incumbents draw a taste shock for their current employer and for all industries, which leads to upward-sloping supply curves to their domestic employer and all industries. The draws of taste shocks are private information for incumbent workers. While these taste shocks are not verifiable to the firm, the firm knows they are distributed i.i.d. type 1 extreme value with dispersion parameter $1/\eta_I$. Firms take this into account when posting a wage for their incumbents.

Similar to the new workers, incumbent workers draw taste shocks for industries but cannot choose their employer in an industry (which is assigned randomly after the incumbent worker chooses an industry). If they decide to leave their employer but remain in industry s, the former incumbent worker has a probability $\left(N_{MNC(s)}/N_s\right)$ to be hired by an MNC and receive a wage equal to $\psi_s \omega_s$ and a probability $\left(1 - N_{MNC(s)}/N_s\right)$ to be hired by a domestic firm and receive a wage ω_s. Our way to think about outside options is similar in spirit to that of Beaudry et al. (2012). The expected wage of incumbent workers who break ties with their initial employer but stay in s is the same as the expected wage of new workers in s

[43] Because our model is a one-period model, this iceberg cost is equivalent to incumbent workers experiencing a permanent tax on their wages. A dynamic version of the model is one in which workers forfeit part of their wage only during the period when they switch industries, as at the beginning of the next period these workers become incumbents again. This assumption is consistent with industry-specific human capital (Neal, 1995).

$(\tilde{\omega}_s(\psi_{s'}))$. Incumbent workers from industry s who move to s' experience an expected wage of $\tau_{ss'}\tilde{\omega}_{s'}(\psi_{s'})$.

The decision of an incumbent worker in firm j is based on the wage posted by the current employer (W_j), the vector of expected wages in all industries $(\tilde{\boldsymbol{\omega}})$, the vector of moving costs $\left(\boldsymbol{\tau_{s(j)}}\right)$, and the individual-specific taste shocks drawn at the beginning of the period. Given all these assumptions, when the employer posts a wage W_j, the labor supply of incumbent workers to their employer j is:

$$I_j(W_j;\boldsymbol{\psi}) = \frac{W_j(\boldsymbol{\psi})^{\eta_I}}{W_j(\boldsymbol{\psi})^{\eta_I} + \sum_{s'}\left(\tau_{s(j)s'}\tilde{\omega}_{s'}(\psi_{s'})\right)^{\eta_I}} I_j^0 \equiv \frac{W_j(\boldsymbol{\psi})^{\eta_I}}{\Omega_{js}(W_j,\tilde{\boldsymbol{\omega}};\boldsymbol{\psi})^{\eta_I}} I_j^0 \equiv \pi_j(W_j,\tilde{\boldsymbol{\omega}};\boldsymbol{\psi})I_j^0, \tag{3.11}$$

where $\Omega_{js}(W_j,\tilde{\boldsymbol{\omega}};\boldsymbol{\psi}) = \left(W_j(\boldsymbol{\psi})^{\eta_I} + \sum_{s'}\left(\tau_{s(j)s'}\tilde{\omega}_{s'}(\psi_{s'})\right)^{\eta_I}\right)^{1/\eta_I}$. We allow the taste dispersion parameter of incumbent workers to possibly differ from the taste dispersion parameter of new workers $(\eta_I \neq \eta_N)$.[44] $\pi_j(W_j,\tilde{\boldsymbol{\omega}};\boldsymbol{\psi}) \equiv \frac{W_j(\boldsymbol{\psi})^{\eta_I}}{\Omega_{js}(W_j,\tilde{\boldsymbol{\omega}};\boldsymbol{\psi})^{\eta_I}}$ is the share of the initial number of incumbent workers of firm j $\left(I_j^0\right)$ that remain with the firm, which is a function of the wage set by the firm for incumbents W_j and the vector of industry-specific expected entry wages $\tilde{\boldsymbol{\omega}}(\boldsymbol{\psi})$.

3.5.3 The Problem of the Domestic Firm

Domestic firms produce using only labor and sell their output to either the final good producer f or to MNCs. The production function of firm j is given by $Q_j = T_j L_j$, where T_j is its physical productivity and L_j is the total number of workers. The total demand for the variety of firm j is given by $Q_j = B_j p_j^{-\sigma}$, where $B_j \equiv b_{DOM} + \sum_{s'\in\mathcal{B}_j} b_{j,MNC(s')} \equiv b_{DOM} + B_{j,MNC}$. The revenue of firm j is given by

$$p_j Q_j = B_j^{\frac{1}{\sigma}} Q_j^{\frac{\sigma-1}{\sigma}} = B_j^{\frac{1}{\sigma}} T_j^{\frac{\sigma-1}{\sigma}} L_j^{\frac{\sigma-1}{\sigma}} = A_j L_j^{\frac{\sigma-1}{\sigma}}, \tag{3.12}$$

where $A_j \equiv B_j^{\frac{1}{\sigma}} T_j^{\frac{\sigma-1}{\sigma}}$ is the revenue shifter of firm j. Note that there is an isomorphism between the demand shifter of the firm and physical productivity. To simplify notation, we focus on the revenue shifter A_j as the heterogeneous feature of firm j.

Firm j in industry s chooses the number of new hires N_j and the wage of its incumbent workers W_j that maximize its profits:

$$\max_{N_j,W_j} \quad A_j\left(I_j(W_j)+N_j\right)^{\frac{\sigma-1}{\sigma}} - \left(\omega_{s(j)}N_j + W_j I_j(W_j)\right) - c\left(N_j\right),$$

[44] We assume that incumbent workers receive new draws of their taste shocks (new relative to those received in a pre-period – which we do not model – when those workers were "new workers"). Put differently, we assume that the taste shocks received by a worker when she was a new worker are uncorrelated to the taste shocks received when she is an incumbent.

where the first term represents the total revenue of firm j, the second term represents its wage bill, and the third and last term represents its recruitment and training cost.

From the first-order condition (FOC) with respect to the number of new hires N_j, we obtain that, the firm equates the marginal revenue product MRP_j with the marginal cost of a new hire $\omega_{s(j)} + c'(N_j(\boldsymbol{\psi}))$:

$$MRP_j(\boldsymbol{\psi}) = \omega_{s(j)} + c'(N_j(\boldsymbol{\psi})), \tag{3.13}$$

where $MRP_j(\boldsymbol{\psi}) = \frac{\sigma-1}{\sigma} A_j L_j(\boldsymbol{\psi})^{-\frac{1}{\sigma}} = \frac{\sigma-1}{\sigma} A_j \left(I_j(W_j;\boldsymbol{\psi}) + N_j(\boldsymbol{\psi})\right)^{-\frac{1}{\sigma}}$.

From the FOC with respect to the wage of incumbent workers, and by assuming that firm j disregards its effect on $\Omega(W_j, \tilde{\boldsymbol{\omega}})$, we obtain that:

$$W_j(\boldsymbol{\psi}) = \frac{\eta_I}{\eta_I + 1} MRP_j(\boldsymbol{\psi}). \tag{3.14}$$

Equation (3.14) is useful to discuss the two types of labor market imperfections in our model and the conditions under which the model collapses to the competitive benchmark. The first labor market imperfection comes from domestic firms internalizing that incumbents have an upwards sloping supply curve to the firm. As in standard monopsony models, the firm equates the marginal revenue product of an incumbent worker to her marginal factor cost. This results in a posted wage equal to an exploitation rate $\frac{\eta_I}{\eta_I+1}$ times MRP_j. Thus, employers exert market power over their workers.

The second labor market imperfection stems from the existence of exogenous MNC premia $\psi_s \geq 1$. Workers supply labor to industries according to the expected wage ($\tilde{\omega}_s$), which is higher than the wage paid by domestic firms (ω_s). The possibility of receiving the premium makes workers over-supply labor to industries with higher MNC presence. Since workers are randomly allocated to firms according to the share of MNC employment in the industry, too many workers end up working for domestic employers with lower MRP_j than that of alternative domestic employers in other industries (with fewer MNCs).

We can rearrange the terms of equation (3.14) to provide an intuitive expression for the two labor market imperfections. Define $MRP_j(1)$ as the optimal marginal revenue product in the absence of MNCs (or whenever MNCs do not pay a premium). $\psi = 1$ leads to an efficient MRP_j because domestic firms hire new workers according to the domestic market wage and new workers supply labor to each industry according to the same market wage. We can write equation (3.14) as:

$$W_j(\boldsymbol{\psi}) = \frac{\eta_I}{\eta_I + 1} MRP_j(1) + \frac{\eta_I}{\eta_I + 1}(MRP_j(\boldsymbol{\psi}) - MRP_j(1)).$$

Note that the first term incorporates a market power distortion whenever η_I is finite. However, the second term incorporates a distortion even when domestic firms face an infinitely elastic labor supply ($\eta_I \to +\infty$) because the MNC premium ψ creates a wedge in the optimal allocation at domestic firms. It is also apparent from the previous equation that our model collapses to the fully competitive benchmark only when $\eta \to +\infty$ and $\psi = 1$ at the same time.

3.5.4 First-Order Approximation of the Equilibrium Conditions

The equilibrium in this economy is characterized by the set of posted wages and new hire decisions such that equations (3.13) and (3.14) are satisfied $\forall j$. It also has to satisfy the market clearing condition for new workers presented in equation (B.19).

Given the non-linearity of the equilibrium conditions described above, it is not possible to find a closed-form solution of the wage setting equation for incumbent workers at domestic firms. We consider a log-linearized version of the equilibrium conditions of this economy. We denote $\hat{X}$ as log-deviation of variable X from its equilibrium and interpret it as percentage deviations and $\overline{X}$ as the equilibrium value of variable X around which the first order approximation is taken. We focus on the main equations and their intuition here and refer the reader to B.2 for all the derivations.

We are interested in the determinants of changes in wages set by domestic firms for their incumbent workers. Using the first order conditions of the profit-maximization problem of a domestic firm j, we show in equation (B.14) of B.2 that one can write the equilibrium log-linear approximation for the change in the wage set by j for its incumbent workers as:

$$\widehat{W}_j = \beta_{1j}\widehat{A}_j + \beta_{2j}\widehat{\omega}_{s(j)} + \beta_{3j}\sum_{s' \neq s(j)} \pi_{js'}\widehat{\omega}_{s'} + \beta_{4j}\sum_{s'} \pi_{js'} \frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\left(\widehat{N}_{MNC(s')} - \widehat{N}_{s'}\right), \tag{3.15}$$

where $\beta_{1j}, \beta_{2j}, \beta_{3j}, \beta_{4j}$ are elasticities and $\pi_{js'} \equiv \frac{\left(\tau_{ss'}\bar{\tilde{\omega}}_{s'}\right)^{\eta_I}}{\overline{\Omega}_{js}^{\eta_I}}$ is the equilibrium probability that a worker from firm j moves to a market s'.[45] The first term on the right hand side represents the effect of changes in revenue shifters of firm j on incumbent wages. It is through this term that the wage setting of firm j is exposed to the MNC shock. The second term refers to changes in the competitive wages of new workers in the same market as firm j. The third term refers to changes in the competitive wages in other markets. These latter changes influence the wages of firm j depending on the ability of its workers to move to each market s', which is reflected in the equilibrium probabilities $\pi_{js'}$. The last term is related to changes in the composition of employment towards MNC jobs that pay a premium $\psi_{s'}$. As before, the changes in employment composition in market s' depend on how "easy" it is for workers from firm j to transition into market s'.

Our model also allows us to link the changes in wages and employment to the fundamental revenue shifters (both demand from clients and productivity of firms) in general equilibrium. This is useful for two main reasons: (i) it motivates the construction of our measures of labor market exposure and firm-level exposure to the expansion of MNC employment, and (ii) it also lays out the model-consistent conditions for the exclusion restriction that our IV strategy had to satisfy. Using the dependence between the competitive entry wages and the revenue shifters of firms in general equilibrium we show in B.2.2 that we can write equation (3.15)

[45] In our model, the β_j elasticities are firm-specific, since they depend on the initial characteristics of each firm before the "MNC shock." We lay out the identification assumptions for the estimation of the average of each elasticity and its relation to the primitives of the model in the next subsection.

as:

$$\begin{aligned}\widehat{W}_j =& \Gamma_{1j}\sum_{s'}\pi_{js'}\left(\sum_{s''}\sum_{k=MNC\in s''}\lambda_{s'ks''}\widehat{A}_k\right)\\ &+\Gamma_{2j}\sum_{s'}\pi_{js'}\tfrac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\left(\widehat{N}_{MNC(s')}-\widehat{N}_{s'}\right)\\ &+\Gamma_{3j}\sum_{s'\in\mathcal{B}_j}\theta_{MNC(s')j}(1+\varphi_{s'})\widehat{A}_{MNC(s')}+\Gamma_{4j}\sum_{s'}\pi_{js'}\left(\sum_{s''}\sum_{k=DOM\in s''}\lambda_{s'ks''}\widehat{A}_k\right)\\ &+\Gamma_{5j}\widehat{T}_j+\Gamma_{6j}\theta_{DOMj}\widehat{b}_{DOM},\end{aligned}\tag{3.16}$$

where $\{\Gamma_{1j},\Gamma_{2j},\Gamma_{3j},\Gamma_{4j},\Gamma_{5j},\Gamma_{6j}\}$ are equilibrium elasticities mediating effects. The right-hand side of this equation has six terms that affect the wage of a worker in firm j in industry s.

The first two terms capture the spirit of our measure of labor market exposure described in Section 3.4 (although we use changes in employment to proxy for the changes in revenue shifters in the model). The first term captures how changes in revenue shifters affect the labor demand in each market. It is a weighted average of the weighted changes in revenue shifters of MNCs in different industries. Then the weighted sum is weighted by the transition probabilities $\pi_{js'}$. The second term captures how changes in the composition of employment affect wages beyond changes in labor demand. This term is a weighted average of the relative employment of MNCs multiplied by a term that reflects the differential wage premium. The third term is akin to our measure of firm-level exposure described in equation (3.7). This term captures how changes in the revenue shifters of MNCs, multiplied by the elasticity of size to sourcing $(1+\phi_s)$, affect wages in firm j depending on $\theta_{MNC(s')j}$, where $\theta_{MNC(s')j} = \overline{b}_{,MNC(s')}/\overline{B}_j = \overline{q}_{j,MNC}/\overline{Q}_j$ is the equilibrium share of sales that firm j sells to each MNC in s'.

The last three terms relate to changes in domestic shifters that affect wages of firm j, and that could occur for other reasons unrelated to changes in MNC shifters. Thus, these three terms are part of our regression error in equation (3.5). The fourth term is identical to the first term of equation (3.16), with the exception that it refers to revenue shifters of domestic firms. It captures changes in labor demand in local firms. These changes could happen, for example, if domestic workers become more productive in a given industry. The fifth term $\widehat{T}_j$ is the change in firm-level physical productivity that is unrelated to any level of exposure to MNCs (e.g., a change in management or organization of the firm). Finally, the last term $\theta_{DOMj}\widehat{b}_{DOM}$ is the product of the change in the demand shifter of the domestic consumer $\widehat{b}_{DOM}$ times the degree of exposure of firm j to the domestic client producing the final good θ_{DOMj}.

This model-based decomposition is helpful to clarify the potential endogeneity concerns of an OLS estimation of equation (3.5). Any shock that affects both the revenue shifters of MNCs and domestic firms in the same market would violate the exclusion restriction of the

OLS estimator. A valid instrument should, thus, affect the revenue shifters of MNCs without having a systematic correlation with the revenue shifters of domestic firms. We believe that our instrumental variables, exploiting variation in the global employment of MNCs with subsidiaries in Costa Rica, represent plausibly valid candidates.

3.5.5 Estimation of Key Model Parameters and Discussion

In this subsection we use equation (3.15) to obtain estimates of the average elasticities β_j and to link these estimates to the structural parameters of the model. In particular, we are interested in obtaining estimates for the retention-wage elasticity (η_I), the cost of hiring and training of a worker as a proportion of the market wage (c_0/ω_s), and the elasticity of the marginal cost of hiring and training with respect to the number of hires (α). To do this we proceed in five steps. First, we write each of the elements of $\{\beta_{1j}, \beta_{2j}, \beta_{3j}, \beta_{4j}\}$ explicitly in a model-consistent way. Second, we estimate the reduced-form average elasticities. Third, we present estimation and results for the retention-wage elasticity η_I. Fourth, we calibrate the relevant equilibrium shares from the data. Fifth, we use the estimated elasticities together with the calibrated parameters to infer estimates of $\{c_0/\omega_s, \alpha\}$. We do this last step by minimizing the norm of the distances between the estimated elasticities and the structural elasticities. We also compute standard errors of $\{c_0/\omega_s, \alpha\}$ using a bootstrap procedure.[46]

Step 1. Model-consistent elasticities. We can write $\{\beta_{1j}, \beta_{2j}, \beta_{3j}, \beta_{4j}\}$ as:

$$\begin{aligned}
\beta_{1j} \equiv & \frac{\xi_j^C \alpha\sigma}{\xi_j^C \alpha\sigma + (1-\xi_j^I) + \xi_j^C \xi_j^I \alpha\eta_I (1-\pi_{jj})} \\
\beta_{2j} \equiv & \frac{(1-\xi_j^C)(1-\xi_j^I) + \xi_j^C \xi_j^I \alpha\eta_I (1+\pi_{js})}{\xi_j^C \alpha\sigma + (1-\xi_j^I) + \xi_j^C \xi_j^I \alpha\eta_I (1-\pi_{jj})} \\
\beta_{3j} = & \beta_{4j} \equiv \frac{\xi_j^C \xi_j^I \alpha\eta_I}{\xi_j^C \alpha\sigma + (1-\xi_j^I) + \xi_j^C \xi_j^I \alpha\eta_I (1-\pi_{jj})},
\end{aligned} \tag{3.17}$$

where $\xi_j^I \equiv \frac{\overline{I}_j}{\overline{L}_j}$ (equilibrium share of incumbents in the total number of workers), $\xi_j^C \equiv \frac{c_0 \overline{N}_j^\alpha}{c_0 \overline{N}_j^\alpha + \overline{\omega}_s}$ (equilibrium share of the hiring and training marginal cost in the total labor cost per worker). $\beta_{3j} = \beta_{4j}$ because from the point of view of firm j, it does not matter whether incumbent workers could find more attractive options in other markets due to higher competitive wages or a shift in composition towards MNCs paying a premium. This equivalence is similar to the one discussed by Beaudry et al. (2012) in the context of a search-bargaining

[46] The elasticities could also be estimated using a simulated method of moments in which we simulate the economy of the model and infer the elasticities that would produce the closest regression coefficients to the ones obtained with the true data. We leave this alternative for future work.

model. We use this equality to regroup the terms in equation (3.15) and write it as:

$$\widehat{W}_j = \frac{\xi_j^C \alpha\sigma}{\xi_j^C \alpha\sigma + (1-\xi_j^I) + \xi_j^C \xi_j^I \alpha\eta_I (1-\pi_{jj})} \widehat{A}_j + \frac{(1-\xi_j^C)(1-\xi_j^I) + \xi_j^C \xi_j^I \alpha\eta_I (1+\pi_{js})}{\xi_j^C \alpha\sigma + (1-\xi_j^I) + \xi_j^C \xi_j^I \alpha\eta_I (1-\pi_{jj})} \widehat{\omega}_{s(j)}$$
$$+ \tfrac{\xi_j^C \xi_j^I \alpha\eta_I}{\xi_j^C \alpha\sigma + (1-\xi_j^I) + \xi_j^C \xi_j^I \alpha\eta_I (1-\pi_{jj})} \underbrace{\left[\sum_{s'\neq s} \pi_{js'} \widehat{\omega}_{s'} + \sum_{s'} \pi_{js'} \tfrac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})} \left(\widehat{N}_{MNC(s')} - \widehat{N}_{s'} \right) \right]}_{\widehat{\mathcal{C}}_s}$$
$$= \beta_{1j} \widehat{A}_j + \beta_{2j} \widehat{\omega}_{s(j)} + \beta_{3j} \widehat{\mathcal{C}}_s. \tag{3.18}$$

The new element $\widehat{\mathcal{C}}_s$ combines the third and fourth terms from equation (3.15). Thus, it includes both the incumbents' wage effects coming from changes in the competitive wages in other labor markets and from changes in the composition of employment towards MNC jobs that pay a premium.

Step 2. From model to estimation. Our goal in the second step is to bring the previous equation to the data. To remain as close to the equation from the model as possible, there are four points to make. First, we need to address the construction of the explanatory variables. We compute the growth in the competitive wage paid to new workers in domestic firms in market s (ω_s) as the growth in the average residualized earnings paid to all new workers in domestic firms in market s. We compute residual earnings using the residuals of a earnings regression after controlling for individual fixed effects, year of birth dummies, a college dummy, a sex dummy and a Costa Rican national dummy. Then, we compute $\widehat{N}_{MNC(s)}$ and $\widehat{N}_s$ as the growth of new employment of MNCs and domestic firms in market s. Finally, we compute $\widehat{A}_j = \widehat{\frac{VA_j}{L_j}} - \frac{\sigma-1}{\sigma}\widehat{L}_j$, as suggested by the model.

Second, in the model, the β_j elasticities are heterogeneous. To obtain the average elasticities, we write the empirical counterpart of equation (3.18) as follows:

$$\widehat{W}_{it} = \overline{\beta}_1 \cdot \widehat{A}_{j(i),t} + \overline{\beta}_2 \cdot \widehat{\omega}_{s(i),t} + \overline{\beta}_3 \cdot \widehat{\mathcal{C}}_{s(i),t} + \alpha_{j(i)}$$
$$+ \gamma_{ind(s(i))\times t} + \mu_{reg(s(i))\times t} + \rho_{ind(s(i))\times reg(s(i))} + \varepsilon_{it}, \tag{3.19}$$

where ε_{it} is equal to $(\beta_{1j} - \overline{\beta}_1)\widehat{A}_{j(i),t} + (\beta_{2j} - \overline{\beta}_2)\widehat{\omega}_{s(i),t} + (\beta_{3j} - \overline{\beta}_3)\widehat{\mathcal{C}}_{s(i),t}$ net of the fixed effects. Equation (3.19) is the specification we take to the data.

Third, we aim to provide a consistent estimation of the average elasticities $\{\overline{\beta}_1, \overline{\beta}_2, \overline{\beta}_3\}$. These coefficients capture the average effect of the firm-level revenue shifters, the market wage, and the composition term on wages of incumbent workers in domestic firms. We also use these coefficients to infer our parameters of interest through equation (3.17). We rely on an IV strategy similar to the one used in Section 3.4. We construct the instruments for $\widehat{A}_{j(i),t}$, $\widehat{\omega}_{s(i),t}$, and $\widehat{\mathcal{C}}_{s(i)t}$ by leveraging the changes in global employment of MNCs with subsidiaries in Costa Rica.[47]

[47] Concretely, $IV\left(\widehat{A}_{j(i),t}\right) \equiv IV\left(\Delta FLE_{j(i),t}\right) = \sum_m \theta_{j(i)m,t-1}\Delta\mathcal{O}_{mt}$, $IV\left(\widehat{\omega}_{s(i),t}\right) \equiv \Delta\mathcal{O}_{s(i),t}$ and

Fourth and finally, under certain conditions, the heterogeneity of the β_j elasticities might pose a threat to identification. A consistent estimation of the average elasticities in equation (3.19) requires stronger assumptions for the IV strategy. This happens because the residual ε_{it} may be correlated with the instruments even if the instruments are uncorrelated with the heterogeneous coefficients. A sufficient condition discussed in Card (2001) and Heckman and Vytlacil (1998) in the context of heterogeneous returns to education, and adapted to our context, would have two parts. First, the instruments need to be uncorrelated with the heterogeneous coefficients. Second, the first stage regression should provide consistent estimates of the effect of the instrument of the endogenous variables (i.e., the instruments should be uncorrelated with the error term in the structural version of the first stage regression). The first condition would be violated, for example, if more able workers chose to work for domestic firms that supply MNCs that experience larger future global growth. The second condition would be violated if there are other factors that affect domestic firms' or sectoral wage growth in Costa Rica, which also affect the global growth of MNCs with subsidiaries in Costa Rica. However, as seen in Section 3.4.3, this concern is less likely to be warranted whenever the parent and its subsidiaries are in different industries.

Step 3. Estimates of the retention-wage elasticity. Before discussing the calibration of the parameters $\{\sigma, \xi_j^I, \pi_{jj}, \pi_{js}, \overline{N}_j\}$, we estimate one of our parameters of interest: the retention-wage elasticity η_I. To do this in a model-consistent way, we rearrange equation (3.11) and take logs on both sides to write:

$$\ln\left(\frac{I_j(W_j)/I_j^0}{1 - I_j(W_j)/I_j^0}\right) = \eta_I \ln(W_j) + \ln\left(\sum_{s'} \left(\tau_{s(j)s'}\omega_{s'}\right)^{\eta_I}\right),$$

where the right hand side represents the log retention rate on the population of incumbents at each firm j. We then estimate the empirical counterpart of the previous equation. The second term on the right-hand side is a function of changes in the wages of all industries. We proxy for this function using a fine set firm and industry-region-year fixed effects. Our estimating equation is:

$$\ln\left(\frac{I_{jt}/I_{jt}^0}{1 - I_{jt}/I_{jt}^0}\right) = \eta_I \ln(W_{jt}) + \alpha_j + \gamma_{ind(s(j))\times reg(s(j))\times t} + \varepsilon_{jt}, \tag{3.20}$$

where I_{jt}^0 is the number of workers of firm j who are observed working for j in both $(t-2)$ and $(t-1)$, i.e., the incumbents of firm j at the beginning of year t. I_{jt} is the number of workers of firm j who are observed working for j in $(t-2)$, $(t-1)$, and t, i.e., the workers who were incumbents at the beginning of year t and continue with firm t throughout t. $log(W_{jt})$ is the log of the yearly average labor earnings of incumbent workers who remain at firm j in year t (i.e., those I_{jt} workers who are observed employed by firm j in $(t-2)$, $(t-1)$, and

$IV\left(\widehat{\mathcal{C}}_{s(i),t}\right) \equiv \sum_{s'\neq s} \pi_{js'}\Delta\mathcal{O}_{s',t} + \sum_{s'} \pi_{js'} \frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\Delta\mathcal{O}_{s',t}$. For the last term, both $\pi_{js'}$ and $\overline{N}_{MNC(s')}/\overline{N}_{s'}$ are calculated using 2006 to 2008 data.

t). An observation in equation (3.20) is a firm-year. We instrument $\ln(W_j)$ with the same instrument that we use for our measure of firm-level exposure to MNCs.

Table 3.6 reports the first stage, reduced form, OLS and IV regressions based on this equation. Our IV specification finds an estimate for the retention-wage elasticity η_I of 9.28. From the first order condition of the domestic firm problem (equation (3.14)) our estimate of η_I implies a value of the exploitation index $\frac{\eta_I}{1+\eta_I}$ of 0.90. This value is relatively high compared to other estimates in the literature (e.g., Manning, 2011; Berger et al., 2019; Kline et al., 2019). However, it is difficult to find an appropriate benchmark for our result since most of the evidence on monopsony or rent-sharing comes from developed countries. Ours is the first paper that uses plausibly exogenous firm-level shocks to estimate their pass-through to workers' wages in a developing country setting. In addition, we focus our estimation on private domestic firms only, which are numerous (close to 30,000) and tend to be small (with a mean and median number of employees of 16 and 5, respectively). Thus, it may not be surprising to find that domestic firms have low labor market power.

As shown in Table 3.5, we find that a 10% increase in value added per worker leads to a 0.9% increase in incumbent wages, a result in the range suggested by previous research using matched employer-employee data (Card et al., 2018). Our different value of $\eta_I/(\eta_I+1)$ is consistent with the fact that, in our model, η_I governs the pass-through of both improvements in outside options and employer-level shocks. Besides, an infinitely elastic retention-wage elasticity ($\eta_I \to +\infty$) is not a sufficient condition for our model to collapse to the perfectly competitive labor market benchmark in the presence of MNC wage premia $\psi_s > 1$. Hence, the large value of η_I does not mean that labor markets are close to perfectly competitive. However, it does suggest that incumbent workers see firms as close substitutes and that markdowns under the marginal product of labor are small. Therefore, it appears that the monopsony power of domestic firms does not play an important role in our context. Most of the labor market imperfections are due to wedges that the MNC premia create in the labor market.

Step 4. Calibration of equilibrium moments. To estimate the structural parameters $\{c_0/\omega_s, \alpha\}$, we need to take a stand on six equilibrium moments. First and foremost, we set $\eta_I = 9.28$, as estimated using our IV strategy in the previous step. We set a value of $\sigma = 5.03$ (estimated in Alfaro-Urena et al. (2019b) for other purposes but in the same context). This value is close to the standard values of around six which are common in the literature (Broda and Weinstein, 2006). The other four moments are computed using averages across firms in our data. We set ξ_j^I (the share of incumbents among total workers) equal to 0.67. We set π_{jj} and π_{js} (the probabilities that an incumbent stays at her firm and that she moves to another firm in the same market, respectively) equal to 0.70 and 0.12 respectively. Finally, we set $\overline{N}_j$ (the average number of new hires) equal to 5.08.

Steps 5. Estimation of the marginal hiring and training cost. Tables 3.7 and 3.8 contain the results from the estimation of equation (3.19). Table 3.7 presents the first stage and reduced form, while Table 3.8 reports the OLS and IV results. Panel A of Table 3.8

refers to the estimated coefficients of equation (3.19). Panel B refers to the inferred structural parameters from our model. As in our estimation of indirect effects if Section 3.4, we focus on incumbent workers at domestic firms.

The IV results imply that, for each dollar increase in the revenue shifter of firm j ($\hat{A}_j$), incumbent workers get around nine cents. The results also show that an exogenous increase of 1% in the competitive market wage of a given market s implies that incumbents in that market see their wage increase by 1.81%. The more than proportional wage increase is rationalized in our model by a high replacement cost.

The value of $c_0/\overline{\omega} = 0.60$ implies that the cost of hiring and training the first new worker is 60% of the competitive market wage. Moreover, the positive value of the marginal cost elasticity $\alpha = 0.25$ implies that the cost of hiring and training increases are convex. However, we are unable to reject that α is statistically different from zero, which suggests that hiring and training costs could be linear in the number of hires.[48]

The value of $c_0/\overline{\omega}$ together with a marginal hiring and training cost elasticity α imply that the average marginal cost of hiring and training is 0.9 times the competitive wage ($C'(\overline{N}_j)/\overline{\omega}_{s(j)} = c_0/\overline{\omega} \times \overline{N}_j^{\alpha} \approx 0.9$). This magnitude is smaller but comparable to the estimated replacement cost of around 1.1, faced by U.S. firms after a patent allowance shock (Kline et al., 2019). These features make firms responsive to improvements in the outside options of their workers.

To provide a quantification of the importance of the marginal replacement cost on changes in wages one can go back to the partial-equilibrium wage setting equation of firm j. Consider the log-linearized version of equation (3.14) after replacing MRP_j by the elements in equation (3.13):

$$\begin{aligned}\widehat{W}_j =& \frac{\overline{\omega}_{s(j)}}{\overline{\omega}_{s(j)}+C'(\overline{N}_j)}\widehat{\omega}_{s(j)} + \frac{C'(\overline{N}_j)}{\overline{\omega}_{s(j)}+C'(\overline{N}_j)}\widehat{c'(N_j)} = \frac{1}{1+C'(\overline{N}_j)/\overline{\omega}_{s(j)}}\widehat{\omega}_{s(j)} + \frac{C'(\overline{N}_j)/\overline{\omega}_{s(j)}}{1+C'(\overline{N}_j)/\overline{\omega}_{s(j)}}\widehat{c'(N_j)} \\ \approx& \frac{1}{1+0.9}\widehat{\omega}_{s(j)} + \frac{0.9}{1+0.9}\widehat{c'(N_j)} \approx 0.53\widehat{\omega}_{s(j)} + 0.47\widehat{c'(N_j)}. \end{aligned} \tag{3.21}$$

Therefore, our model implies that the wage growth of employees at firm j would come in roughly similar proportions from changes in the competitive market level outside option and from changes in the replacement cost. This result is consistent with our evidence in Section 3.4. If we take a proportional change of one standard deviation increase in labor market exposure (SD=7.04) and firm level exposure (SD=0.38), our main IV specification in Table 3.4 predicts that the earnings of incumbent workers at domestic firms would grow 1.02% (7.04×0.145) due to their increased labor market exposure to MNCs and 1.25% (0.38×3.3) due to their increased firm-level exposure to MNCs. This means that around 45% of the total increase comes from increases in labor market exposure (which reflects improvements in the outside options) and the remaining 55% comes from increases in firm-level exposure

[48] The literature has found both convexity and linearity of the hiring and training cost, e.g., Dix-Carneiro et al. (2019) develops a structural model to study informality and finds that hiring costs are very convex for firms in the tradable sector (equivalent to $\alpha = 2.28$ in our notation), whereas Bloom (2009) cannot reject linearity in a linear-quadratic model of employment adjustment.

(which impact incumbent wages through the increase in the replacement cost of incumbent workers).

Overall, our estimates suggest that a social planner who cares about domestic workers' wages has little room for increasing the ability of workers to earn the full value of their marginal product of labor. There is more potential room for improvement from the side of the cost of hiring and training. While higher replacement costs result in higher wages for incumbent workers, these costs could also constrain firm growth. This can lead to unemployment or informality, margins that fall outside the scope of this paper. Besides, our findings suggest that the planner has scope to help local residents by boosting labor demand both directly through the hiring of MNCs and indirectly through supplying linkages.

3.6 Conclusion

This paper estimates the effects of MNCs on workers by combining administrative data on all worker-firm and firm-firm matches in Costa Rica with an instrumental variable strategy that exploits variation in the size of MNCs in the country. First, we find a direct MNC wage premium of 9%, which is consistent with MNCs paying above-market wages rather than compensating workers for disamenities. The wage premium is not explained away by firm characteristics such as size or technological sophistication and is larger for workers with a college education (12%) than for those without one (8%).

Second, we study the indirect effects of MNCs on the wages of incumbent workers at domestic firms. We separately estimate the effects of MNCs on outside options in the labor market and those mediated by changes in the performance of domestic employers from input-output linkages to MNCs. We show that the growth rate of annual earnings of a worker experiencing a one standard deviation increase in either the labor market or the firm-level exposure to MNCs is one percentage point higher than that of an identical worker with no change in either MNC exposure.

Third, and lastly, we present a stylized model of an economy that allows for both types of exposure to MNCs. Our model-based estimates imply that workers have a low attachment to their employer and are, therefore, sensitive to changes in their outside options. We also find that the average domestic firm faces high marginal hiring and training costs, equivalent to one year of worker earnings paid at the domestic market wage. These high costs allow incumbent workers to extract part of the increase in employer rents resulting from higher sales to MNCs.

We highlight three avenues for future research. First, this paper focuses on those effects of MNCs on workers that are measurable with administrative data. While such data cover all formal workers and firms, they exclude the informal sector. This sector accounts for a large share of total employment in developing countries. Recent work has shown how international trade can have significant reallocation effects between the formal and informal sectors (McCaig and Pavcnik, 2018; Dix-Carneiro et al., 2019). MNCs are likely to have an even stronger impact on reallocation than trade, as they embed themselves directly into the labor and product markets of the host country. Understanding the effects of MNCs on informality is vital for a comprehensive assessment of policies to attract MNCs to developing countries.

Second, our results on the direct effects of MNCs suggest that MNCs create "good jobs" in the host economy (Acemoglu, 2001; Green, 2015). While we provide evidence that indicates that MNCs pay above market-clearing wages, more research is required to understand the mechanisms that sustain these wages in equilibrium. The recent paper by Hjort et al. (2019) takes a step in this direction by studying the fairness concerns of the HQs of MNCs as a potential mechanism. More work is necessary to understand this and other types of company-wide policies that could lead to wage premia.

Finally, our model-based estimates suggest that domestic firms in Costa Rica face higher marginal costs of hiring and training than firms in developed countries (as a multiple of the domestic market wage, see the review in Manning, 2011).[49] Such high costs might be one potential explanation for the well-established facts that firms in developing countries tend to be small and low-performing (Tybout, 2000; Bloom et al., 2010) and that they grow relatively little over their life cycle (Hsieh and Klenow, 2014). Future work should provide more direct evidence on the quantitative importance of hiring and training costs in explaining these facts.

[49] In a contemporaneous paper, Dix-Carneiro et al. (2019) find comparably large hiring costs in Brazil.

Figures

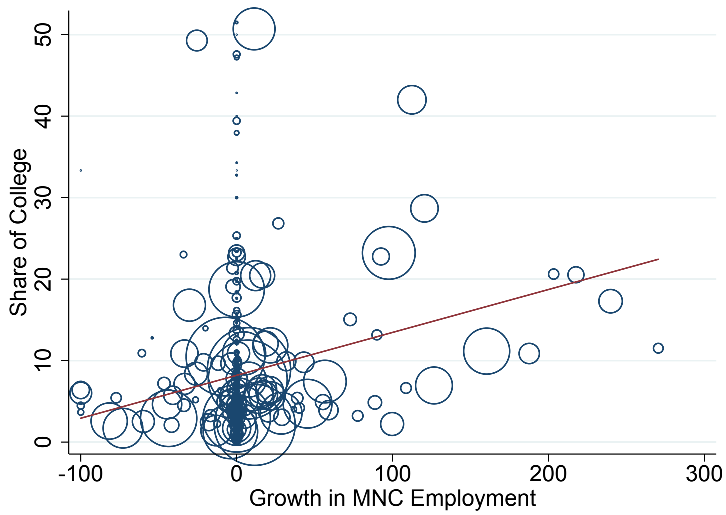
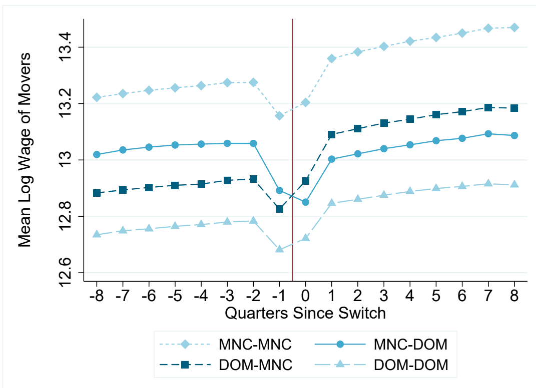
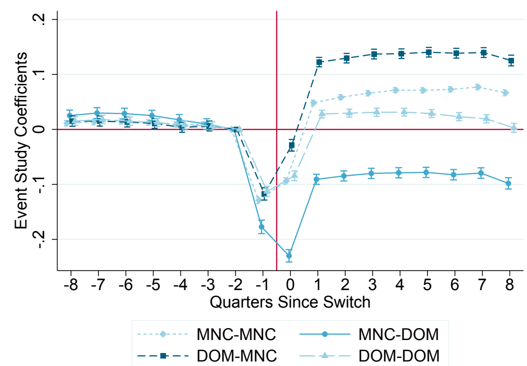


Figure 3.1: % Growth of MNC Employment between 2009-2017 vs. % Workers with College

Notes: Figure 3.1 relates the percentage growth in the period of analysis (2009 to 2017) in MNC employment in each of the 412 two-digit industry × region markets in Costa Rica and the share of workers with a college degree in those markets in the pre-period (2006 to 2008). The size of the circle reflects the number of workers in each market in the pre-period (2006 to 2008).



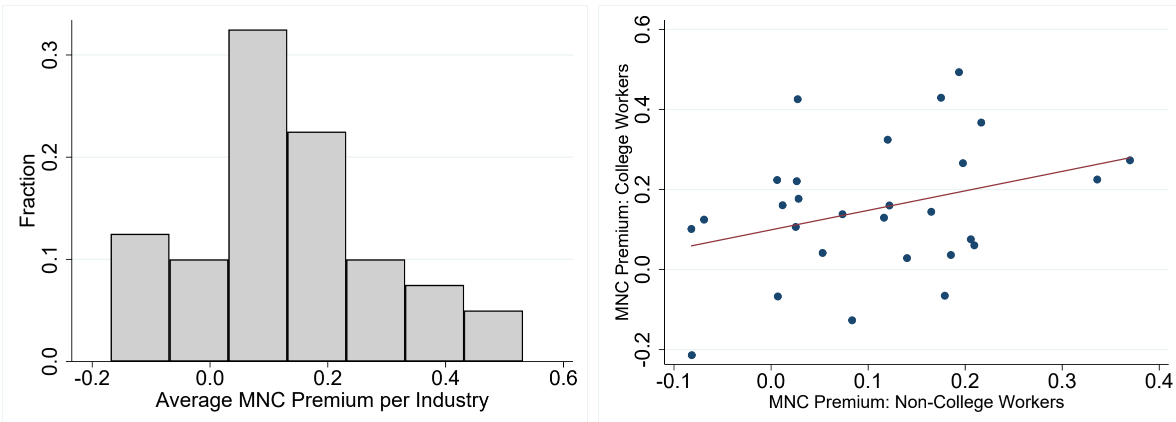
(a) Raw Means



(b) Movers Design

Figure 3.2: The Wage Effects of Moves To/From Domestic Firms/MNCs

Notes: Panel 3.2a plots the raw means of log worker quarterly-average labor earnings in each quarter before and after a change in employer. Panel 3.2b plots the event-study coefficients from the specification in equation (3.2), where the event is defined as an across-quarter change in employer. The sample is restricted to workers with the same main employer continuously between quarter *-8* and *-1* and the same new main employer between quarters *0* and *+8*. The dependent variable is the log worker quarterly-average labor earnings. In Panel 3.2b, we use robust standard errors clustered at the individual level.



(a) Histogram of Industry-Specific MNC Premia

(b) Within-Industry Coll vs. Non-Coll Premia

Figure 3.3: The MNC Premium Differs by Industry

Notes: Figure 3.3a plots the estimates of industry-specific MNC premia based on the movers design and using only moves from domestic firms to MNCs for which both the domestic firm and the MNC are in the same industry. Figure 3.3b plots the correlation between the MNC premium of college-educated workers in a given industry and the MNC premium of non-college-educated workers in the same industry. The differential premium of college vs. non-college-educated workers is estimated via adding a set of interaction terms between the event dummies and a college-educated dummy in the main movers design specification from equation (3.2). In both figures, the industry refers to the two-digit industry of each firm.

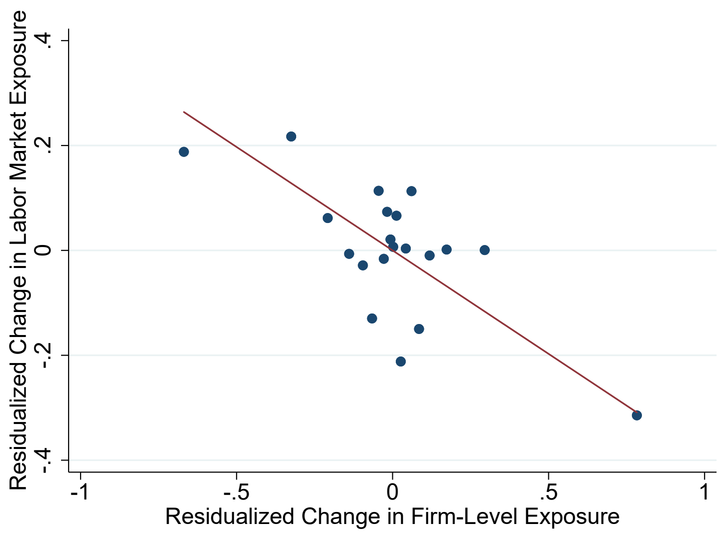


Figure 3.4: Correlation between Workers' Labor Market and Firm-Level Exposure to MNCs

Notes: Figure 3.4 displays a binned scatter plot of the worker-year labor market exposure to MNCs with respect to the worker-year firm-level exposure to MNCs. Both measures of exposure have been residualized by the same fixed effects and controls used in our main equation (3.5). We use twenty equal-sized bins.

Tables

	OLS	First Stage	Reduced Form	IV
Dependent Variable	Δw_{it}	$\Delta \mathbb{1}[j(i) = MNC]_t$	Δw_{it}	Δw_{it}
	(1)	(2)	(3)	(4)
$\Delta \mathbb{1}[j(i) = MNC]_t$	0.076***			0.154**
	(0.003)			(0.064)
$IV\ (\Delta \mathbb{1}[j(i) = MNC]_t)$		0.025***	0.004**	
		(0.001)	(0.002)	
Observations	1,529,265	1,529,265	1,529,265	1,529,265
F-Statistic				677.7

Table 3.1: The Wage Effect of Moving to/from an MNC Employer: OLS and IV Estimates

Notes: Table 3.1 presents the OLS and IV estimates for the specification described in equation (3.3). The goal of this exercise is to estimate the effects of a move to (from) an MNC from (to) a domestic firm on the labor earnings of the moving worker. Column (1) contains the OLS estimate, Column (2) the first stage of the IV exercise, Column (3) the reduced form of the IV exercise, and Column (4) the IV estimate. The dependent variable in Columns (1), (3), and (4) is the change in log yearly labor earnings for worker i between year $(t-1)$ and t. The dependent variable in Column (2) is $\Delta \mathbb{1}[j(i) = MNC]_t$, where $\Delta \mathbb{1}[j(i) = MNC]_t \equiv \mathbb{1}[j(i,t) = MNC] - \mathbb{1}[j(i,t-1) = MNC]$. The IV of $\Delta \mathbb{1}[j(i) = MNC]_t$ is $\Delta \mathcal{O}_{s(i,t-1),t}$, the expansion between $(t-1)$ and t of MNC employment *outside* of Costa Rica for MNCs with subsidiaries in the market of the worker in $(t-1)$ (the year before the move). Each regression controls for $\boldsymbol{X_i}$ (a vector of dummies for worker i characteristics: the college education status, Costa Rican national status, year-of-birth and sex), firm $j(i,t)$ fixed effects (where $j(i,t)$ is the employer of i in t), fixed effects for the industry of the market s of i in $(t-1)$ and t respectively, and fixed effects for the region of the market s of i in $(t-1)$ and t respectively. Each regression uses robust standard errors clustered at the individual level.

Dependent Variable	Extra Hours (1)	Paid Extra (2)	Paid Bonus (3)	Paid Sick Leave (4)	Paid Vacations (5)	Hazard Insurance (6)	Soc. Sec. Contrib. (7)
MNC	0.693	0.137**	0.067**	0.162***	0.132***	0.171***	0.192***
	(0.467)	(0.055)	(0.029)	(0.043)	(0.037)	(0.039)	(0.032)
Other Controls							
Wage	0.272	0.070**	0.066***	0.069**	0.078***	0.082***	0.058**
	(0.170)	(0.030)	(0.023)	(0.028)	(0.026)	(0.026)	(0.025)
W/ College	-0.127	-0.026	0.022	0.132**	0.113**	0.115**	0.077
	(0.305)	(0.092)	(0.039)	(0.063)	(0.045)	(0.057)	(0.056)
Male	-0.065	-0.028	0.008	0.053	0.041	0.058	0.084**
	(0.243)	(0.045)	(0.029)	(0.042)	(0.038)	(0.040)	(0.038)
Age	0.083	-0.023	-0.017	-0.009	-0.023*	-0.033**	-0.039***
	(0.069)	(0.017)	(0.012)	(0.016)	(0.014)	(0.015)	(0.014)
Age^2	-0.001	0.000	0.000	0.000	0.000	0.000**	0.000***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	723	469	469	469	469	469	469
Adjusted R^2	0.014	0.035	0.048	0.072	0.080	0.094	0.11

Table 3.2: MNCs Have Better Amenities than Domestic Firms

Notes: Table 3.2 presents OLS regressions on a cross-section of workers surveyed in 2018 as participants in the National Survey of Household Income and Expenditures (abbreviated ENIGH). Column (1) uses as dependent variable the answer to the question: "In the last week, how many hours more than the usual were you required to work for your employer?". Columns (2) to (7) have as dependent variable dummies which take value one if the person answered that her employer is providing her with a given benefit: is paid for extra hours of work (Column (2)), receives a bonus salary at the end of the year (Column (3)), is paid for sick leave (Column (4)), has paid vacation days (Column (5)), has occupational hazard insurance (Column (6)), the employer pays Social Security contributions for the worker (Column (7)). The MNC dummy takes value one if the main employer of the worker was an MNC in 2017. In addition, we control for the log of the average monthly labor earnings of the worker in 2017, whether the worker has a college degree or not (1 if yes), if the worker is male or not (1 if yes), and the age and the square of the age of the worker. As soon as the 2018 matched employer-employee data becomes available, we will match each worker to the employer she had in the actual month when she was surveyed for ENIGH in 2018. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	First Stage: Main IV Set 1				Reduced Form: Main IV Set 1			Placebo Reduced Form: Main IV Set 1		
Dep. Var.	$\Delta LME_{s(i),t}$	$\Delta FLE_{j(i),t}$	$\Delta LME_{s(i),t}$	$\Delta FLE_{j(i),t}$	Δw_{it}	Δw_{it}	Δw_{it}	Δw_{it}	Δw_{it}	Δw_{it}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$IV\left(\Delta LME_{s(i),t}\right)$	0.615***		0.616***	-0.007	0.068**		0.065**			
	(0.120)		(0.120)	(0.007)	(0.031)		(0.030)			
$IV\left(\Delta FLE_{j(i),t}\right)$		0.093***	-0.044	0.093***		0.304***	0.300***			
		(0.010)	(0.044)	(0.010)		(0.077)	(0.077)			
$IV\left(\Delta LME_{s(i),t+1}\right)$								-0.024		-0.024
								(0.022)		(0.022)
$IV\left(\Delta FLE_{j(i),t+1}\right)$									-0.031	-0.030
									(0.073)	(0.073)
Observations	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	2,721,231	2,721,231	2,721,231
Adjusted R^2	0.91	0.48	0.91	0.48	0.045	0.045	0.045	0.047	0.047	0.047

Table 3.3: The Effects of Changes in Exposure to MNCs on Workers in Domestic Firms. First Stage, Reduced Form, and Placebo IV for Leading IV Set 1. Stayers Only

Notes: Table 3.3 reports the first stage and reduced form estimates associated to the IV strategy described in Section 3.4 for the estimation of the regression in equation (3.5). This exercise uses the leading IV Set 1 (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). Δw_{it} is the percentage change in the monthly average labor earnings of worker i between years $(t-1)$ and t. The difference between the reduced form estimates in Columns (5) to (7) and those in Columns (8) to (10) is that in the latter columns we use the value of the instrument from the next period $(t+1)$ (instead of the contemporaneous value of the instrument). These regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). All regressions include firm fixed effects, region $\times$ year, two-digit industry $\times$ year, and two-digit industry $\times$ region fixed effects, and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, college education, Costa Rican nationality). Robust standard errors clustered at the level of the firm in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

				Main: IV Set 1			Rob. Check: IV Set 2			Rob. Check: Both IV Sets		
Dep. Var. : Δw_{it}	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)	IV (7)	IV (8)	IV (9)	IV (10)	IV (11)	IV (12)
$\Delta LME_{s(i),t}$	0.047***		0.050***	0.111**		0.143**	0.111*		0.147**	0.111**		0.145***
	(0.015)		(0.016)	(0.053)		(0.066)	(0.061)		(0.072)	(0.050)		(0.055)
$\Delta FLE_{j(i),t}$		0.718***	0.735***		3.269***	3.291***		3.293*	3.365*		3.274***	3.306***
		(0.137)	(0.134)		(0.909)	(0.910)		(1.826)	(1.834)		(0.868)	(0.866)
Observations	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017
F-Statistic				26.3	83.4	41.2	35.2	17.9	8.74	40.0	53.2	27.3
Hansen Overid p-val										1.00	0.99	1.00

Table 3.4: The Effects of Changes in Exposure to MNCs on Workers in Domestic Firms. OLS and IV Estimates for Leading IV Set 1, Robustness Check Set IV2 and Both Sets Together. Stayers Only

Notes: Table 3.4 reports the OLS and IV estimates for the regression in equation (3.5). Δw_{it} is the percentage change in the monthly average labor earnings of worker i between year $(t-1)$ and year t. This exercise uses first the leading IV Set 1 (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica) in Columns (4)-(6), then the robustness check IV Set 2 (the instrument using changes in MNC employment outside of Costa Rica for MNCs with subsidiaries in at least one of twenty Latin American and Caribbean countries) in Columns (7)-(9), and last, both sets of IVs together in Columns (10)-(12). These regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). All regressions include firm fixed effects, region $\times$ year, two-digit industry $\times$ year, and two-digit industry $\times$ region fixed effects, and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, college education status, Costa Rican nationality). Robust standard errors clustered at the level of the firm in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	First Stage: Main IV Set 1			Reduced Form: Main IV Set 1		OLS		Main: IV Set 1	
Dep. Var.	$\Delta LME_{s(i),t}$	$\Delta\left(value\text{-}added/worker\right)_t$		Δw_{it}	Δw_{it}	Δw_{it}	Δw_{it}	Δw_{it}	Δw_{it}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\Delta LME_{s(i),t}$						0.047***		0.129**	
						(0.015)		(0.065)	
$\Delta\left(value\text{-}added/worker\right)_t$						0.008***	0.008***	0.092***	0.092***
						(0.001)	(0.000)	(0.029)	(0.029)
$IV\left(\Delta LME_{s(i),t}\right)$	0.616***	-0.152		0.065**					
	(0.120)	(0.227)		(0.030)					
$IV\left(\Delta FLE_{j(i),t}\right)$	-0.044	3.327***	3.242***	0.300***	0.298***				
	(0.044)	(0.648)	(0.653)	(0.077)	(0.077)				
Fixed Effects									
Region × Year	Yes	Yes	No	Yes	No	Yes	No	Yes	No
Two-Digit Industry × Year	Yes	Yes	No	Yes	No	Yes	No	Yes	No
Two-Digit Industry × Region × Year	No	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3,080,017	3,080,017	3,079,984	3,080,017	3,079,984	3,080,017	3,079,984	3,080,017	3,079,984
Adjusted R^2	0.91	0.25	0.27	0.045	0.047	0.046	0.048		
F-Statistic								13.1	24.7

Table 3.5: The Effects of Changes in Labor Market Exposure to MNCs and in Firm Value Added Per Worker on Workers in Domestic Firms. Stayers Only. Leading IV.

Notes: Table 3.5 reports the first stage, reduced form, OLS and IV estimates for the modified version of the main regression (equation (3.8)). The first stage, reduced form, and IV regressions use the leading IV Set 1 (the instruments using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). The modification, which drives the difference between the exercise in this table and that in Table 3.4, is that instead of the change in firm-level exposure to MNCs, we use the change in the value added per worker of the firm (see equation (3.8)). Δw_{it} is the percentage change in the monthly average labor earnings of worker i between year $(t-1)$ and year t. Columns (1) to (3) contain the first stage, Columns (4) and (5) contain the reduced form regressions, Columns (6) and (7) the OLS regressions, and (8) and (9) the IV regressions. With the exception of the regression in Column (1), all other regressions have two versions, one with $\Delta LME_{s(i),t}$, and one without. Whenever $\Delta LME_{s(i),t}$ is included, the fixed effects used vary at the region × year and two-digit industry × year levels. Whenever $\Delta LME_{s(i),t}$ is excluded, the fixed effects vary at the region × two-digit industry × year level. All these regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). All regressions include firm fixed effects and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, college education, Costa Rican nationality). Robust standard errors clustered at the level of the firm in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Regression	First Stage	Reduced Form	OLS	IV
Dependent Variable	$log(W_{jt})$	log(Retention rate$_{jt}$)		
	(1)	(2)	(3)	(4)
$log(W_{jt})$			0.269***	9.283***
			(0.054)	(3.197)
$IV\ (log(W_{jt}))$	0.007***	0.061***		
	(0.001)	(0.019)		
Fixed Effects				
Two-Digit Industry × Region × Year	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Observations	181,298	181,298	181,298	181,298
Adjusted R^2	0.90	0.45	0.45	
F-Statistic				40.2

Table 3.6: Model-Based Estimation of the Retention-Wage Elasticity for Incumbent Workers

Notes: Table 3.6 reports the first stage, reduced form, OLS and IV regressions based on equation (3.20). The first stage, reduced form, and IV regressions use the leading IV Set 1 (the instruments using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). W_{jt} is the average wage of incumbents at firm j in year t. For $IV\ (log(W_{jt}))$ we use $IV\left(FLE_{j(i),t}\right)$. The retention rate is defined as $\frac{I_{jt}/I_{jt}^0}{1-I_{jt}/I_{jt}^0}$. I_{jt}^0 is the number of workers of firm j who are observed working for j in both $(t-2)$ and $(t-1)$, i.e., the incumbents of firm j at the beginning of year t. I_{jt} is the number of workers of firm j who are observed working for j in $(t-2)$, $(t-1)$, and t, i.e., the workers who were incumbents at the beginning of year t and continue with firm t throughout t. By construction, $I_{jt}/I_{jt}^0 \leq 1$. $log(W_{jt})$ is the log of the yearly average labor earnings of incumbent workers who remain at firm j in year t (i.e., those I_{jt} workers who are observed employed by firm j in $(t-2)$, $(t-1)$, and t). An observation is a firm-year. Given these definitions, the first year t is 2011 (as incumbents of firms j at the beginning of 2011 need to be observed working for j in 2009 and 2010). ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	First Stage: Main IV Set 1			Reduced Form: Main IV Set 1			
	$\hat{A}_{j(i),t}$ (1)	$\hat{\omega}_{s(i),t}$ (2)	$\hat{C}_{s(i),t}$ (3)	Δw_{it} (4)	Δw_{it} (5)	Δw_{it} (6)	Δw_{it} (7)
$IV\left(\hat{A}_{j(i),t}\right)$	0.031***	-0.000	0.001	0.003***			0.003***
	(0.007)	(0.000)	(0.001)	(0.001)			(0.001)
$IV\left(\hat{\omega}_{s(i),t}\right)$	-19.028	2.966***	-4.556**		2.355**		2.515**
	(13.692)	(0.904)	(2.145)		(1.112)		(1.214)
$IV\left(\hat{C}_{s(i),t}\right)$	-1.555	-0.907***	5.256***			-0.019	-0.396
	(3.850)	(0.166)	(0.829)			(0.360)	(0.406)
Observations	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017
Adjusted R^2	0.22	0.57	0.70	0.045	0.045	0.045	0.045

Table 3.7: Model-Based Wage Equation (Reduced Form and First Stage). Stayers Only. Leading IV.

Notes: Table 3.7 reports the first stage and reduced form estimates for the model equation (3.19) and for the leading IV Set 1 (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). Δw_{it} is the percentage change in the monthly average labor earnings of worker i between year $(t-1)$ and year t. $\hat{A}_{j(i),t}$ is the change in the firm revenue shifter, $\hat{\omega}_{s(i),t}$ is the change in the competitive market wage, and $\hat{\mathcal{C}}_{s(i),t}$ is the change in the composition term. $IV\left(\widehat{A}_{j(i),t}\right) \equiv IV\left(\Delta FLE_{j(i),t}\right) = \sum_m \theta_{j(i)m,t-1}\Delta\mathcal{O}_{mt}$, $IV\left(\widehat{\omega}_{s(i),t}\right) \equiv \Delta\mathcal{O}_{s(i),t}$ and $IV\left(\widehat{\mathcal{C}}_{s(i),t}\right) \equiv \sum_{s'\neq s}\pi_{js'}\Delta\mathcal{O}_{s',t} + \sum_{s'}\pi_{js'}\frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\Delta\mathcal{O}_{s',t}$. For the last term, both $\pi_{js'}$ and $\overline{N}_{MNC(s')}/\overline{N}_{s'}$ are calculated using 2006 to 2008 data. Columns (1) to (3) report the estimates from the first stage regressions for each of the three explanatory variables regressed on all three instruments. Columns (4) to (7) report the estimates from the reduced form regressions in which we either introduce one instrument at a time (Columns (4) to (6)) or all instruments at the same time (Column (7)). All regressions include only stayers, i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t. All regressions include firm fixed effects and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, college education, Costa Rican nationality). Standard errors for the regression coefficients are clustered at the level of the firm. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var. : Δw_{it}	OLS (1)	IV (2)
Panel A: Regression Coefficients		
Change in the Firm Revenue Shifter $\left(\hat{A}_{j(i),t}\right)$	0.008***	0.088***
	(0.001)	(0.030)
Change in the Competitive Market Wage $\left(\hat{\omega}_{s(i),t}\right)$	0.447***	1.817***
	(0.030)	(0.679)
Change in the Composition Term $\left(\hat{\mathcal{C}}_{s(i),t}\right)$	-0.003	0.264**
	(0.004)	(0.134)
Panel B: Inferred Parameters		
Marginal Cost of Hiring and Training of First Hire $\left(\frac{c_0}{\bar{\omega}}\right)$	0.393***	0.602***
	(0.104)	(0.189)
Elasticity of Marginal Cost of Hiring and Training	0.172***	0.255
With Respect to the Number of Hires (α)	(0.035)	(0.216)
Observations	3,080,017	3,080,017
Adjusted R^2	0.047	
F-Statistic		8.02

Table 3.8: Model-Based Wage Equation (OLS and IV Estimates) and Estimation of the Structural Parameters. Stayers Only. Leading IV Set 1

Notes: Table 3.8 reports the OLS and IV estimates for the model equation (3.19) using the leading IV Set 1 (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). Δw_{it} is the percentage change in the monthly average labor earnings of worker i between year $(t-1)$ and year t. Both regressions include only stayers, i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t. Both regressions include firm fixed effects and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, college education, Costa Rican nationality). Standard errors for the regression coefficients are clustered at the level of the firm. Standard errors for the inferred model parameters are calculated using bootstrap. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

3.7 Transitional Section

In this chapter of my dissertation, I have studied the effects of foreign MNC affiliates in Costa Rica on the wages of workers – both those who become directly employed by the MNC affiliates and those working for domestic firms (who are indirectly affected through both the labor and product markets). The direct effect was estimated to be a 9% wage premium upon joining an MNC. Evidence from survey and administrate data suggested that MNCs pay a wage premium to workers to avoid worker turnover, motivate the worker, and ensure cross-country pay fairness within the MNC.

In the second part of the chapter, we studied the indirect effects of MNCs on workers in domestic firms. We allowed MNCs to affect both the outside options of workers in the labor market and the performance of domestic employers. To provide causal estimates of the effects of exposure to MNCs through these two channels, we used an IV strategy that exploits the plausibly exogenous variation in MNC employment outside of Costa Rica. Our IV estimates imply that the growth rate of annual earnings of a worker experiencing a one standard deviation increase in either the labor market or the firm-level exposure to MNCs is 1.1 percentage points higher than that of an identical worker with no change in either MNC exposure. In the third and final part of this chapter, we developed a model to rationalize our reduced-form evidence and estimate the parameters that govern wage setting.

Improving the outcomes of workers through the attraction of foreign MNCs is one of the most prevalent policies pursued by governments in developing countries. The rationale behind this policy is that these countries cannot create *good jobs* from within, and that foreign investment is necessary. An alternative is to design industrial policies that provide incentives for industries that are believed to create good jobs and to trigger positive externalities on other firms and workers in the economy. Designing and implementing industrial policies that are actually effective in spurring the development of the targeted industry might be difficult in developing countries.

In the last chapter of my dissertation, which follows, I study a unique industrial policy introduced in Romania in 2001. Since 2001, this policy provides a full personal income tax break to employees with an eligible bachelor's degree specialization who work directly on software development and generate revenues from this activity for a firm in the IT sector. In the first part of the paper, we use firm-level data and difference-in-differences designs to study the effects of the introduction of the tax break in 2001 and its 2013 reform. Our DiD estimates suggest that firms treated by the 2013 reform experience large and long-lasting increases in size. In the second part of the paper, we switch to a sector-level cross-country study of the impacts of the 2001 introduction of the tax break. We show that both the treated industry and industries that rely more heavily on IT services have thrived relatively more relative to control industries.

Chapter 4

Industrial Policy at Work: Evidence from Romania's Income Tax Break for Workers in IT[1]

4.1 Introduction

> *"When listing the world's most promising places for tech and start-ups, you could be forgiven for overlooking Romania. During the almost three decades since a revolution lifted the nation out of communism, the country has maintained a low profile internationally. Nonetheless, a quieter type of revolution has been percolating behind the scenes." – 2016 article in Quartz, a New York-based business news portal.*

According to both business and technology journalists, Romania has emerged over the past two decades as the "unlikely Silicon Valley of Europe."[2] The comparison with Silicon Valley is noteworthy given that few topics have been researched as extensively as the rise of Silicon Valley and the types of policies that countries pursue in the hope of developing their own high-technology sectors.[3] In this article, we provide firm and sector-level evidence that an industrial policy introduced by Romania in 2001 has been key for the impressive development of its information technology (IT) sector.

Since 2001, this policy provides a full personal income tax break to employees with an eligible bachelor's degree specialization who work directly on software development and generate revenues from this activity for a firm in the IT sector (specifically, in the "Software

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[1] This paper is joint work of Isabela Manelici and Smaranda Pantea. All permissions to reprint this material as a chapter of the present dissertation have been obtained.

[2] For example, see 2019 HeadHuntingIT blog post, December 2018 *France 24* article, September 2018 *Accace* article, March 2018 *Outsourcing Portal* article, September 2017 *Financial Times* article, May 2017 *IDG Connect* article, 2017 *Teamfound* blog post, May 2016 *Forbes* article, or April 2016 *TechCrunch* article.

[3] A Google Scholar search on the keywords "Silicon Valley" returns 898,000 papers.

consultancy and supply" sector, with the NACE Rev 1 code 722). In 2013, an amendment to the 2001 law greatly expanded the pool of eligible firms and workers by adding several newly eligible sector codes for the firms and bachelor's degree specializations for the workers.[4] This intricate set of rules – on the worker, the firm, and the activity performed by the worker in the firm – implies that the tax break rewards very specific matches between workers and firms. The expectation was that lowering the tax burden on these matches would lead to an increase in their prevalence. Moreover, this restrictive set of conditions ensures that the tax break actually subsidizes software development (as opposed to other misreported activities). Finally, for workers to benefit from this tax break, their employer has to prepare the necessary paperwork and apply for their income tax break. This requirement of an explicit "buy-in" from the employer is another reason to expect a shared economic incidence of this tax incentive.

In the first part of the paper, we use firm-level data and difference-in-differences (DiD) designs to study the effects of the introduction of the tax break in 2001 and its 2013 reform. The unexpected nature of both policy events enables us to credibly estimate their impact. The analysis of the 2001 event allows us to study the behavior of firms in a set of comparable sectors (including the IT sector) just before and after the introduction of a differential tax treatment of the IT sector. These DiD estimates measure the intent-to-treat effects of the policy on firms in the eligible IT sector. The analysis of the 2013 reform is based on more comprehensive (administrative) data, that includes information on the number of income tax exempt employees. This allows us to estimate the effects of this reform on firms whose programmers actually benefit from the now more widely available income tax break.

To study the impact of the 2001 introduction of an income tax break for programmers, we use firm-level data from Amadeus. In the 2001 DiD strategy, a firm is considered treated if it belongs to the sector that became targeted by the law (i.e., if in the "Software consultancy and supply" sector, with the NACE Rev 1 code 722).[5] The outcomes of firms in eligible sectors are compared to the outcomes of firms in comparable non-eligible sectors, such as NACE Rev 1 sector 731 (Research and experimental development on natural sciences and engineering). For identification, we rely on the unexpected nature of the passing of the law and the lack of preexisting differential trends in outcomes between treated and comparison firms. However, after 2001, firms in the eligible IT sector embark on a differential upward trend relative to firms in comparable non-eligible sectors. By 2005, firms in the eligible sector have, on average, 31% higher operating revenues than in 2000, hire 17% more workers, and have 17% more assets than firms in comparable non-eligible sectors. We also find a relative improvement of 12 points in the solvency ratio of firms in the eligible sector. We use six other combinations of comparison sectors to show that results are not an artifact of the baseline

[4] We will use the terms "IT" and "software development" to refer to the sector initially targeted by the 2001 tax break law ("Software consultancy and supply" or NACE Rev 1 722) and to the activity typically performed in that sector. We will also use the term "IT" to refer to the enlarged set of eligible sectors, as per the 2013 amendment. We therefore use the term IT loosely. In a stricter sense, not all IT sectors are eligible for the income tax break of their workers (particularly in 2001).

[5] In Amadeus one cannot observe whether a firm has workers who benefit from the income tax break or not.

choice of comparison sectors.

Next, we examine the 2013 amendment to the income tax break law, which greatly expanded the set of eligible firms and workers. Given that the administrative data available for this exercise includes the number of employees that are actually exempt from income tax each year, we refine the definition of treatment from one based on the sector of the firm to one based on the extent of workforce exemption. We classify a firm as treated if it jumps from under 5% of workforce exemption before 2013 to over 20% of workforce exemption after 2013. The reference group contains other firms in ICT service sectors (eligible and non-eligible) that remain at 5% of workforce exemption throughout the entire sample period. The identification relies on the unexpected and generous expansion in firm and worker eligibility that occurred in 2013 – the most plausible driver of the sudden jump in the firm-level share of tax exempt workers – and the lack of differential preexisting trends between treated and comparison firms.

Our DiD estimates suggest that firms treated by the 2013 reform experience large and long-lasting increases in size. In 2015, these firms have 37% more revenues than in 2012, 38% more sales, 21% more employees, and 26% more assets (all relative to the comparison firms). These baseline estimates are robust to (i) running the baseline regression on a dataset from Amadeus that starts in 2008 and allows us to observe a longer pre-reform period; (ii) using an alternative comparison group of firms in other high-tech knowledge-intensive service sectors; (iii) restricting the sample only to firms in the eligible IT sector; (iv) using an alternative event-study research design; (v) varying the threshold choice (e.g., from 20 to 15%); and (vi) defining treatment based only on the sector of the firm, as in the 2001 DiD exercise.

These firm-level results corroborate the hypothesis of a shared economic incidence of the tax break between workers and firms. Lacking worker-level data, we do not attempt to estimate how the tax incentive is split between workers and firms.[6] To interpret the magnitudes of the estimates from the two analyses, we first assume a 25% take-up between 2001 and 2015 for firms in the eligible sector. We can then translate the 17% 2001 pooled DiD (intent-to-treat) estimate on employment into a 68% treatment-on-the-treated estimate. As the median firm in the eligible sector has four workers in 2000, a 68% increase in employment between 2001 and 2005 is sensible. This 68% estimate is larger than the corresponding 21% estimate from the 2013 analysis. This is most likely due to the fact that the “early adopters" (i.e., firms whose workers become exempt from the income tax just after 2001) are positively selected relative to the “late adopters." That said, the very purpose of the 2013 reform was to expand the scope of the tax break to include firms and workers unable to benefit from the tax break beforehand.

In the second part of the paper, we switch to a sector-level cross-country study of the impacts of the 2001 introduction of the tax break. The research design is based on the

[6] It is not surprising to find at least a partial incidence on firms. Using linked employer-employee data from Sweden, Saez et al. (2017) find that a payroll tax reduction for young workers (from 31.4% to 15.5%) has had a full incidence on firms. The authors find no effect on net-of-tax wages of young treated workers relative to older untreated workers. Firms employing many young workers receive a larger tax windfall and increase their employment, capital, and sales after the reform.

synthetic control method (SCM). We first use data from Eurostat and the World Bank to evaluate the effects of this policy on the relative growth of the IT sector. This analysis is complementary to the firm-level analysis in two ways. First, it captures not only the intensive margin of growth of the IT sector but also its extensive margin (i.e., the entry of new firms into the IT sector). Second, because we benchmark the relative growth of the IT sector in Romania to that in a synthetic Romania based on comparable Central and Eastern European (CEE) countries, we control for potentially confounding sector-specific productivity or demand shocks.

The SCM estimates indicate that, in 2015, the gross revenue (employment) in the IT sector of Romania is 6.52 (1.83) times larger than the gross revenue (employment) in 2000. This value reflects the exceptional growth of the IT sector in Romania – plausibly owed to the 2001 policy – as it is relative to the growth of gross revenue (employment) in all other sectors in Romania and relative to the same difference in growth rates in synthetic Romania. Given this extra double-differencing – which controls for broader trends in the rest of the Romanian economy and in similar economies – the SCM estimates of the expansion of the IT sector in Romania are smaller than its actual expansion.[7] Placebo tests suggest that a similar growth cannot be replicated in countries that did not implement this policy.

Next, we provide evidence on the inter-industry effects of the tax break, again using SCM on Eurostat, Comtrade, and World Bank data. The improvements in the prices, quality, and variety of IT services – which are likely to have occurred alongside the expansion of the IT sector – are expected to benefit more those downstream sectors which have a stronger reliance on IT service inputs. We, therefore, ask whether sectors that relied more on IT services before the tax break expanded more than sectors with less of such a reliance (in Romania, relative to synthetic Romania). We group sectors into high- and low-intensity of use of IT services, based on the share of the IT sector in their total input expenditures (according to the input-output table of Romania for the year 2000).

These SCM results suggest that, after 2001, the high-intensity sectors of Romania grew more than its low-intensity sectors and more than in synthetic Romania (for instance, 0.75 times more in terms of gross revenue and 0.61 times more in terms of employment). Moreover, high-intensity sectors also improved their export performance more, which suggests a shift in Romania's comparative advantage. These results serve two purposes. First, they represent an additional indirect check on the effectiveness of the policy of interest, particularly due to the delayed onset of the downstream SCM effects and their smaller magnitude (both relative to the direct SCM effects). Second, while not a definitive test, the faster growth of downstream sectors relying more on IT service inputs is a necessary condition for the IT sector to have generated inter-industry externalities (one of the theoretical conditions that justify industrial policies favoring a certain sector). All the SCM results survive a battery of robustness checks.

[7] The actual gross revenue in IT in Romania grew 14 times (from 282 million euros in 2000 to 4,031 million euros in 2015) and the actual number of workers grew six times (from 13,691 workers in 2000 to 81,780 workers in 2015).

Finally, we propose a set of back-of-the-envelope cost estimates of this policy to the government. The corresponding amount of foregone fiscal revenues is equivalent to 4.7 to 6.4% of the total gross wage bill in the IT sector. In terms of value added of the sector, these foregone tax revenues range between 2.8 and 3.8%. These numbers suggest that the income tax break did not only signal a commitment of the government to the development of the IT sector, but was also a sizable incentive.

Most directly, this article contributes to a long-standing academic debate on industrial policy (IP). To begin with, academics are skeptical that IPs can be effective, particularly in less developed countries, where public resources are often captured by "sunset industries" or lobbying firms. Moreover, even when governments identify the high-potential sectors and genuinely intend to support them, they may still lack the capacity to design and implement IPs that meet that goal. However, despite this skepticism, the set of papers that provide well-identified reduced-form evidence on the effectiveness of IP is rather small and recent.[8] We contribute to this debate by bringing evidence on the effectiveness of a Romanian IP with a unique design[9] and targeting a sector of general interest. The natural follow-up question is whether an effective IP is also efficient. While answering this question lies outside the scope of this paper, the tax break we study appears to meet the theoretical criteria for welfare-improving IP put forward by the literature. First, it encourages software development, a "new" activity for the domestic economy in 2001 (Rodrik, 2004), and one that is knowledge-intensive (Aghion et al., 2011; European Commission, 2017; Cherif and Hasanov, 2019). Second, given its skill endowment, Romania most likely had a latent comparative advantage in this activity (Rodrik, 1996; Harrison and Rodríguez-Clare, 2010) and only lacked a policy signal to tilt resources towards it.[10] Third, we find that the growth of the IT sector has

[8] For such examples, see Görg et al. (2008); Aghion et al. (2015); Juhász (2018); Criscuolo et al. (2018); Cai and Harrison (2019). Rodrik (2008) justifies the difficulty to conduct statistical inference on the effects of IPs and notes the scarcity of credible evidence, particularly from less developed countries. Until recently, the empirical evidence on IP came in two forms: (i) detailed country studies (Amsden, 1989; Evans, 1995), or (ii) cross-industry (country) econometric studies, which regress a measure of economic performance on indicators of government support (Krueger and Tuncer, 1982; Lee, 1996; Beason and Weinstein, 1996). The main drawback of the former type of evidence is that causality is typically defended using a narrative approach, whereas the main shortcoming of the latter is one of misspecification (Rodrik, 2012).

[9] The most commonly studied IPs involve tax breaks or credits for capital and R&D (Fowkes et al., 2015; Boeing, 2016; Cai and Harrison, 2019), grants (Görg et al., 2008; Criscuolo et al., 2018), or trade tariffs (Aghion et al., 2015)

[10] Although in practice, it is challenging to identify latent comparative advantage sectors, in theory, social planners can identify these sectors and design policies which tilt resource allocation towards them (Itskhoki and Moll, 2018). This IP was meant to boost a sector that, while in its infancy, was perceived to have an unrealized potential. IT specialists and policy-makers of the time believed that Romania had the appropriate conditions to develop a strong IT sector, consistent with having a latent comparative advantage in this sector. Romania's rigorous education in STEM, inherited from the country's communist past, was a particularly valuable asset. However, short of a clear policy push to this sector, a large share of Romanian engineers were either emigrating or working in lower value-added branches of engineering. This insight is formally developed by Rodrik (1996): Eastern Europe countries – while relatively well-endowed with skills and human capital – can be stuck in a low-income, low-tech equilibrium (even when a high-tech equilibrium is viable) due to coordination failures.

supported the growth of IT-using sectors – a necessary condition for the sector to generate inter-industry externalities.[11] Last, this policy benefits a sector that is competitive, and within that sector, all firms and workers meeting the eligibility criteria (Aghion et al., 2015).

By studying a policy that targets the IT sector, we naturally relate to research on this specific sector. One strand of this research establishes the wide-ranging effects of the IT sector, of which the effects on productivity have garnered the most attention.[12] In particular, Van Ark et al. (2008) makes the case that the later emergence and smaller size of the IT sector in the European Union (compared to the United States) explains its slower productivity growth. This makes the policy under study especially relevant to countries that grapple with the drawbacks of an underdeveloped IT sector. Another strand of this literature studies the determinants of firm growth in the IT sector.[13] We add to this literature by estimating the plausibly-causal effects of tax incentives on the growth of IT firms. While we are not the first to point to tax incentives as options for countries to spur the growth of their IT sector,[14] we are unaware of other studies that bring econometric evidence on their effectiveness.

Finally, given the specifics of the policy, we also relate to research on policies aimed at reducing non-wage labor costs. Most papers in this literature evaluate the effects of reductions in non-wage labor costs for hard-to-employ workers (such as unemployed individuals, youth, parents returning to work, or people with disabilities) and find positive effects on firm size.[15] For instance, Kangasharju (2007) studies the effects of wage subsidies for hard-to-place job

[11] An important theoretical motive to deviate from policy neutrality requires that the targeted sector would later generate externalities (Succar, 1987; Greenwald and Stiglitz, 2006; Harrison and Rodríguez-Clare, 2010). One such externality occurs between sectors, through the supply of specialized inputs used by (many or high-technology) downstream sectors. It is common for policy-makers to label upstream sectors that provide such inputs as "strategic" and grant them special policy treatment. The premise behind such special treatment is that, in its absence, "strategic" sectors would under-develop, at the detriment of their downstream sectors. This type of argument was frequently used in favor of protecting the semiconductor industry in the U.S., as discussed in research on this industry (Borrus et al., 1986; Irwin and Klenow, 1994, 1996). By studying not only the targeted sector, but also those affected through I-O linkages, we relate to both seminal (Hirschman, 1958; Pack and Westphal, 1986; Rodríguez-Clare, 1996b) and recent work on the I-O implications of IP (Forslid and Midelfart, 2005; Du et al., 2014; Blonigen, 2016; Huremović and Vega-Redondo, 2016; Lane, 2016; Liu, 2017; Joya and Rougier, 2019).

[12] IT – or ICT, more broadly – has been found to improve productivity in IT-using firms (Wilson, 2009; Jorgenson et al., 2008; Syverson, 2011; Bloom et al., 2012), increase wages in high-skill locations (Forman et al., 2012), lead to the fragmentation of production (Fort, 2017), reduce information frictions (Steinwender, 2018), foster service exports (Kneller and Timmis, 2016), connect rural markets (Couture et al., 2018), and improve educational attainment (Beaudry et al., 2010) etc.

[13] The following factors have a strong explanatory power for firm growth in the IT sector: the founders' human capital (Colombo and Grilli, 2005; Ganotakis, 2012), firm internationalization (Ganesan and Samii, 2014; Falk and Hagsten, 2018), the initial size, age, and productivity of the firm (Johansson, 2004; Falk and Hagsten, 2018).

[14] See, for instance, Tigre and Botelho (2001); Biswas (2004); Tan and Leewongcharoen (2005); Chen et al. (2018a).

[15] For examples on positive effects on employment, see Crépon and Desplatz (2002); Rotger and Arendt (2010); Kangasharju (2007); Moczall (2013); Cahuc et al. (2014); Saez et al. (2017); on positive effects on profits, revenue, and long-term investment, see Månsson and Quoreshi (2015); Saez et al. (2017). For a review, see Eurofound (2017).

seekers (mostly long-term unemployed) in Finland and finds a pooled DiD estimate of a 9% increase in employment. Our larger estimates for IT firms (the pooled DiD estimate from the 2013 exercise is 15%) are most likely explained by the higher value of the incentive for the IT sector; the labor costs associated with programmers are likely to be more significant to IT firms than those associated with hard-to-employ workers (typically hired in low-skilled support positions, such as cleaners or secretaries, by firms in all sectors). By studying a personal income tax break for programmers, a high-skill/high-wage occupation, we also relate to a smaller set of papers that study reductions in taxes on the wages of R&D workers. As most papers study the effects over a short-term horizon – during which the supply of researchers is most likely inelastic – their most common finding is an increase in researcher wages.[16] Our findings of positive effects on employment most likely stem from a more elastic supply of programmers (relative to researchers), the fact that this policy reduced the incentives of Romanian programmers to emigrate (emigration which decreased the number of programmers available to work for firms in the country), and the fact that the 2013 amendment acted like a shock to the supply of programmers eligible for the income tax break.

The remainder of the article is organized as follows. Section 4.2 describes the two policy episodes of interest: the 2001 introduction of the income tax break to workers in IT and its 2013 reform. Section 4.3 presents our firm-level empirical strategy and findings. In Section 4.4, we bring sector-level cross-country evidence on both the direct and downstream effects of the 2001 introduction of the tax break. Section 4.5 provides back-of-the-envelope cost estimates of this tax break. Section 4.6 concludes.

4.2 Romania's Income Tax Break for Workers in IT

Before 2001, high labor taxes were seen as a major constraint to the development of the IT sector in Romania. These taxes lowered net wages for programmers and were seen as a root cause of the high emigration rates of programmers. These same taxes also led to relatively high labor costs for firms, limiting their growth.[17] For these reasons, Romania's IT sector did not stand out in Europe at the time.

Since 2001 however, Romania's IT sector has experienced dramatic shifts. The sector has greatly expanded, both in absolute terms and as a share of GDP. It has also become more integrated into the global economy through flows of foreign direct investment (FDI) into the sector and the growing importance of foreign revenue. IT-related bachelor's degrees remain among the most popular degrees to this day. A tax break, effective since 2001, is widely perceived as having triggered these shifts.

[16] See Hægeland and Møen (2007); Dumont (2013); Lokshin and Mohnen (2013); Lelarge et al. (2015).

[17] This article (in Romanian) from January 2001 summarizes how the news on the introduction of this tax break was received by firms and workers in the IT sector. For instance: "All stakeholders in the IT industry are looking forward to [...] this measure. Why? Because of the generally high levels of migration of the workforce from company to company or from Romania abroad [due to low wages] and the substantial burden of labor taxation on firms."

In 2001, Romania introduced a personal income tax break for programmers at the proposal of Mr. Varujan Pambuccian, a member of the Chamber of Deputies (one of the two legislative bodies of the bicameral legislature of Romania). Mr. Pambuccian expected that this tax break would address both the concerns of workers and firms in the IT sector. He first proposed to reduce the top marginal income tax rate on wages of IT professionals from 40 to 8%.[18] Mr. Pambuccian was and still is an independent legislator, meaning that his proposal was not automatically backed up by a majority in the Chamber of Deputies.[19] In fact, his first proposal was deemed unrealistic and was rejected. Eventually, however, Mr. Pambuccian managed to rally support for his initiative. Surprisingly, in the final version of the law, the tax break became a full tax break (i.e., no income tax to be paid overall). Given the rejection of the first version of the law and the uncertainty around the approval of the final version, its adoption and its more generous nature came as unexpected and positive shocks to the IT sector.

To benefit from the tax break, workers had to fulfill all of the following criteria: (i) they had to have an eligible bachelor's degree (in either automation, computers, informatics, cybernetics, mathematics, or electronics), (ii) they had to work for a firm whose sector code is "Software consultancy and supply" (7220 in NACE Rev 1), (iii) they had to work for the unit in charge of software development, (iv) they had to have an eligible occupation title (such as "programmer" or "computer systems designer"), and finally (v) they had to work for a firm that kept separate balance sheets recording revenues from software development and that generated a gross revenue of at least 10,000 U.S. dollars from this activity in the previous fiscal year (per exempted employee). An important feature of the policy is that, while the exemption applies to the income tax owed by workers, the firm is responsible for preparing the justifying documents, applying for the tax break, and archiving the documents for potential future audits.

Four features of this tax break deserve emphasis. First, this break was particularly generous at the time of its introduction. In 2001 the wages of programmers were among the highest in the country. Before the full income tax break, programmers faced a top marginal tax rate of 40%. In 2004, all workers in the country saw a generalized reduction in their income tax rate due to the switch from progressive income taxation to a flat rate of 16%. Despite this change, the trend of growing wages in the IT sector meant that this tax break remained a sizable incentive. Second, the rules for benefiting from the tax break were meant to ensure that the economic incidence of the tax break was shared between workers and firms. The policy rewarded specific types of matches between workers and firms engaged in software development activities. We expect that lowering the tax burden on this activity led to an increase in its prevalence. We document the equilibrium effects of this policy using firm- and sector-level data. Third, its strict accountability rules (explained above) ensured

[18] In 2001, personal income taxes were progressive, with rates between 18 and 40%. Payroll taxes (social security contributions and insurance) were paid both by employers (up to 40%) and employees (17%) on gross salary.

[19] Mr. Pambuccian is part of the Armenian minority in Romania, which he represents in the Chamber of Deputies.

that exempted workers were actually developing software. Hence, the effects we estimate are plausibly real responses to the incentive and not a mere relabeling of activities.[20] Finally, this policy was designed to benefit all eligible workers and firms in the IT sector. Sectoral policy works better when benefits are less concentrated (Aghion et al., 2011).

The first major amendment to the tax break law occurred in 2013.[21] As for the initial introduction of the policy back in 2001, the passing of this amendment and its final eligibility criteria were also unexpected and uncertain. Negotiations were initiated in 2012 and were completed in 2013, years during which Romania had three different prime-ministers.[22] The amendment expanded the lists of eligible sector codes (for the firms) and of eligible majors for the bachelor's degree (for the workers). The new list of eligible sector codes consisted of the following NACE Rev 2 codes: 5821, 5829, 6201, 6202, and 6209. This turned previously ineligible activities in sectors such as "Database activities" or "Other computer related activities" into newly eligible activities.[23] The number of eligible bachelor's degree majors increased from six to 14 (adding majors such as "Cybernetics and economics"). Combined, these changes significantly increased the number of eligible firms and workers.

We study the effects of the initial introduction in 2001 of the income tax break for workers in IT and of its 2013 reform. Two additional amendments were introduced in the second half of 2015 and 2016 respectively, which we do not study due to data availability constraints and their more limited scope. C.5.1 presents more details on both the 2001 tax break law and its subsequent amendments. C.5.2 summarizes other policies relevant to the Romanian IT sector and argues why these other policies do not jeopardize our ability to isolate the effects of the income tax break.

4.3 Firm-Level Analysis

Our analysis of the effects of the income tax break to workers in IT proceeds in two steps: a firm-level analysis and a sector-level cross-country analysis. In this section, we conduct a firm-level analysis centered around each of the two main events in the history of this policy:

[20] Chen et al. (2018b) study a Chinese policy that awards substantial corporate tax cuts to firms that increase R&D investment and find that 30% of the increase in R&D comes from the relabeling of administrative expenses.

[21] In 2004, the law had a minor revision without any economic effects.

[22] Mr. Pambuccian had initiated the negotiations toward the introduction of this amendment with Prime Minister Emil Boc in 2012, continued them with Prime Minister Mihai Ungureanu in 2012, and finally completed them with Prime Minister Victor Ponta in 2013. Hence, these disruptions made the success of these negotiations unpredictable.

[23] Romania had to officially transition from the NACE Rev 1 classification of sector codes to the NACE Rev 2 classification. Because the crosswalks between classifications are not bijective, and because policy-makers also intended to expand the scope of the tax break, some sectoral codes became newly eligible due to the transition from the NACE Rev 1 classification to the NACE Rev 2 classification. For instance, NACE Rev 1 code 726 (Other computer related activities) was not eligible pre-2013 but became eligible post-2013 as NACE Rev 2 code 6209 (which contains, in addition to NACE Rev 1 code 726, NACE Rev 1 code 7222, Other software consultancy and supply, a code eligible since 2001). See Table C.16 in C.5.1.

its initial introduction in 2001 and its only major reform in 2013. At the end of the section, we provide a discussion of the findings.

4.3.1 2001 Income Tax Break

We begin by studying the impact of the 2001 income tax break on firms already active in 2001. The main advantage of this analysis is that it studies the IT sector over a period when, still in its infancy, it received the unexpected positive news of the introduction of a dedicated tax break to programmers.

Data. For this analysis, we use Amadeus data on firms in Romania.[24] We construct a panel of firms between 1999 and 2005, with four outcome variables: operating revenues, number of workers, total assets, and solvency ratio. These variables are both likely to react to the new incentive and are among the few variables whose values are less frequently missing. As reporting to Bureau Van Dijk is not consistent across years – with many firms not complying with continuous annual reporting – we focus on firms who report these four outcomes at least in 2000, 2001, and 2002. In order to trim outliers, the sample is winsorized at the 1st and 99th percentiles of the distribution of operating revenues per worker.

Empirical Strategy. We estimate the firm-level effects of the introduction of the tax break on firms via a difference-in-differences (DiD) design. The first difference is taken between firm outcomes in a given year between 1999 and 2005 and the same firm outcomes in the year 2000 (the reference year). The second difference is taken between the contemporaneous outcomes of firms in the sector with NACE Rev 1 code 722 (the eligible sector) and the outcomes of firms in comparable sectors. Formally, we use the following model:

$$y_{ist} = \alpha_i + \sum_{k=1999}^{2005} \delta_k \mathbb{1}[t = k] + \sum_{k=1999}^{2005} \beta_{DiD,k} \mathbb{1}[t = k]\, Target\_sector_{is} + \varepsilon_{ist}, \tag{4.1}$$

where i stands for firm, s for the sector of firm i, t for the calendar year. α_i is the firm fixed effect.[25] $\mathbb{1}[t = k]$ is an indicator function that takes value 1 whenever an observation is in calendar year k. It is meant to capture common shocks across all firms in a given calendar year. We set 2000 as the reference year for the DiD coefficient estimates. We use as outcome variables, y_{ist}, the firm i, year t, log(operating revenue), log(number of workers), log(total assets), and the solvency ratio.

The treated firms are those whose NACE Rev 1 sector is 722 (Software consultancy and supply); hence, for firms whose sector s is 722, $Target\_sector_{is} = 1$. The comparison group for our baseline results has $Target\_sector_{is} = 0$ and includes firms from NACE Rev

[24] Amadeus – a commercial dataset provided by Bureau Van Dijk – contains balance sheet information on firms in Europe. Amadeus data comes from official business registers, annual reports, newswires, and webpages. The Amadeus coverage of firms in Romania is good relative to the census of firms in Romania, particularly for larger firms.

[25] Given that in this dataset firms cannot be observed switching sectors (as we only observe their 2005 sector), adding or not adding sector fixed effects is inconsequential.

1 sectors 721 (Hardware consultancy), 723 (Data processing), 724 (Database activities), 725 (Maintenance and repair of office, accounting and computing machinery), 726 (Other computer related activities), 731 (Research and experimental development on natural sciences and engineering), and 732 (Research and experimental development on social sciences and humanities). These comparison sectors share several features in common with 722, including their focus on high value-added services and their reliance on high-skilled workers and technology. We later show that our results are robust to six alternative comparison groups.

For this strategy to deliver credible estimates of the treatment effect of this policy, we first rely on its unexpected introduction. As discussed in Section 4.2, this policy was introduced at the initiative of one independent policy-maker alone. The success of his initiative and the ultimate generosity of the tax break came as a surprise to the IT industry. Second, identification hinges on the assumption that firms in the comparison group form a suitable counterfactual for firms in the IT sector, after accounting for fixed differences between firms and common year-specific shocks.[26] The lack of differential pre-trends between treated and comparison firms is an important test for the validity of both assumptions.

As Amadeus does not include information on the extent to which the workers of a given firm have actually benefited from the income tax break, these DiD estimates do not measure the impact of the introduction of the exemption on firms that start having exempted workers, but instead measure the average impact on firms that are part of a sector with a newly available exemption for their workers involved in software development.

Baseline Results. Figure 4.1 plots the DiD estimates from the model in Equation (4.1). These estimates pertain to our baseline choice of the comparison group (see definition above). Reassuringly, across all outcome variables, we observe a lack of preexisting differential trends between treated and control firms, and between 1999 and 2000. After 2001, however, firms in the eligible sector experience significant improvements in all three measures of firm size (operating revenue, number of workers, and total assets) and in their financial health (solvency ratio). These improvements are mostly gradual, with part of the improvement already taking place in 2001. By 2005, firms in the IT sector have a 31% higher operating revenue, hire 17% more workers, have 17% more assets, and a 12-points higher solvency ratio than firms in comparable sectors (relative to 2000). The upper panel of Table 4.1 provides more details.[27]

Robustness Checks. This finding of strong and lasting boosts in firm size and financial performance is not driven by the choice of the baseline comparison group. We propose six

[26] Table C.17 (C.3.1) shows that the median firm in the eligible sector is comparable to the median firm in the comparison sectors, while the average firm is significantly different (smaller or larger, depending on the variable). Given our use of firm fixed effects, time-invariant differences are not a threat to the identification of the effects of interest. We also control in some specifications for the initial size of the firm in order to account for potentially-heterogeneous effects based on initial firm size.

[27] The lower panel of Table 4.1 shows the estimate of a pooled DiD coefficient that measures the average increase in an outcome from the 1999-2000 period to the 2001-2005 period. These estimates are 31% for operating revenue, 11% for the number of workers, 22% for total assets, and nine points for the solvency ratio.

other comparison groups, which are combinations of sectors akin to the IT sector. The new sectors in the pool of candidates (in addition to those forming the baseline set of comparison sectors) were selected on similar criteria as the baseline set: their focus on high value-added services and the type of inputs employed (workers in particular).[28] For instance, one of our proposed sets of comparison sectors is exactly the set of sectors whose firms became newly eligible for the income tax break of their programmers after the 2013 reform.[29] Policy-makers considered that these sectors were similar to the IT sector and were therefore deserving of the same tax treatment. Another set of comparison sectors excludes all IT-relevant sectors, in order to avoid concerns of spillovers from the eligible software development sector to other non-eligible sectors such as the hardware consultancy sector. Table C.1 (C.1.1) shows that the main takeaways from our baseline estimates survive across these six other comparison groups. While the magnitude of the coefficients varies, the sign and significance of the DiD coefficients remains largely unchanged.

4.3.2 2013 Reform to the Income Tax Break Law

We now move on to study the impact of the 2013 reform to the conditions of eligibility for the income tax break for workers in IT. The main advantage of this analysis is that it is performed on administrative data recording the actual firm-level exemption rate from the income tax of its workers. This allows us to estimate the effect of the actual exemption as opposed to the effect on firms in the eligible sector, irrespective of the actual exemption status of their workers.

Data. The firm-level analysis of the impact of the 2013 reform is based on administrative datasets collected by the National Agency of Fiscal Administration of Romania (*Agenţia Naţională de Administrare Fiscală*). The first dataset contains company balance sheets, which give us information on yearly revenue, sales, and total assets. We add firm-level information coming from two compulsory fiscal forms that record the income taxes paid by

[28] In order to construct these six sets of comparison sectors we draw from the following list of NACE Rev 1 codes: 721 (Hardware consultancy), 722 (Software consultancy and supply), 723 (Data processing), 724 (Database activities), 725 (Maintenance and repair of office, accounting and computing machinery), 726 (Other computer related activities), 731 (Research and experimental development on natural sciences and engineering), 732 (Research and experimental development on social sciences and humanities), 741 (Legal, accounting, book-keeping and auditing activities, tax consultancy, market research and public opinion polling, business and management consultancy, and holdings), 742 (Architectural and engineering activities and related technical consultancy), 743 (Technical testing and analysis), 744 (Advertising), 748 (Miscellaneous business activities), and 3002 (Manufacture of computers and other information processing equipment).

[29] We refer to sectors 721 (Hardware consultancy), 724 (Database activities), 726 (Other computer related activities), and 3002 (Manufacture of computers and other information processing equipment). See Table C.16 in C.5.1 for details. We do not prefer this set of sectors over the baseline set because it contains 3002: this is not only a manufacturing sector, but also one that provides inputs to the treated sector (therefore indirectly treated itself).

workers.[30] In particular, these forms track the firm-level number of workers exempted from paying any income tax. The resulting dataset starts in 2011 and ends in 2015.[31]

We remove from the analysis sample any firms with negative or missing values for the main variables of interest: revenue, sales, number of workers, and assets. Observations in the top and bottom 1% of the distribution of labor productivity (sales per worker) are also excluded. Moreover, we remove firms that benefited from major State Aid programs (797/2012 and 332/2014) during the period studied. We also remove from the analysis sample firms that we do not observe at least in 2012, 2013, and 2014.

Next, we keep in the analysis sample only firms who, in 2011 and 2012, had less than 5% of workers exempted from the income tax. In our baseline specification below, we will turn our attention to firms who experience a jump in their share of worker exemption from under 5 to over 20% after 2013. In order to improve the interpretation of the estimates, we exclude firms who never reach the 20% threshold after 2013, while at the same time surpassing the 5% threshold at least once between 2011 and 2015.

C.3.2 contains descriptive statistics on our main analysis samples. Table C.18 compares the firm size and productivity in 2011 for the three groups of firms in our baseline sample: firms in non-eligible sectors, firms in eligible sectors with less than 5% of employees exempted from the income tax throughout the entire sample period, and firms with less than 5% of exempted employees in 2011 and 2012, and which jumped to over 20% of exempted employees after 2013. Firms in non-eligible sectors are, on average, the largest; firms in eligible sectors for which a large share of workers became exempted after 2013 have the second-highest average size; while firms in eligible sectors that remain under 5% exemption rate have the smallest average size. The three types of firms do not differ in their average relative productivity. In our baseline specification, we control for time-invariant differences in size using firm fixed effects. In a variant of our baseline specification, we also control for initial differences in the size category, relative productivity, and age.

Table C.19 shows that the percentage of firms in the eligible sectors with at least one worker exempted from the income tax has increased from 36% in 2011 to 41% in 2015. If we require firms to have more than 20% of their workers exempted from the income tax, the share of such firms increases from 29% in 2011 to 35% in 2015.[32] Table C.20 documents the

[30] Firms fill in and submit these D112 and D205 forms, retain the owed income taxes from the wages of their workers, and transfer these taxes to the tax authority (all on behalf of their workers).

[31] The baseline version of this analysis relies heavily on the forms recording the number of workers exempted from the income tax. As these forms were first introduced in 2011, this dataset can only start that year. The original dataset includes 2016 as well, but we are excluding 2016 from the analysis because the outcomes in that year are likely to be affected by the 2015 amendment to the income tax break law (see Section 4.2 and C.5.1 for details).

[32] This relatively low level of take-up of the tax exemption can be explained by the restrictive set of conditions that must be jointly met by firms and programmers in order for programmers to qualify for the tax break; by difficulties in hiring eligible programmers in a tight labor market; by a lack of knowledge of the administrative procedures to apply for the break; or by the high perceived cost of preparing the required documentation. While we cannot distinguish between these scenarios, the first two are the most plausible. The restrictive nature of the criteria to qualify for the break and the need to implement the NACE Rev 2 classification are

predictors of firm-level share of workers who are exempt from the income tax. The reference category contains firms that operate in non-eligible sectors, are domestically-owned, have a micro size, and have been in business for more than five years. The reference year is 2011. Firms that are foreign-owned and/or larger are more likely to have a higher share of workforce exemptions from the income tax. This may reflect either their ability to meet the restrictive conditions of the exemption (for instance, by attracting eligible workers) or to assemble the documentation necessary to solicit the tax break.

Table C.20 also shows that in 2013 and after, firm-level shares of workforce exemption experience a significant jump with respect to their 2011 level, whereas no such jump occurs in 2012. Table C.2 further emphasizes these first-stage effects of the 2013 reform on the firm-level share of workforce exemption. The main takeaway is that in 2013 and after, firms in eligible sectors (according to the new definition of eligibility post-2013) experience an increase in their share of workers exempted from the income tax relative to their 2012 share. In 2015, the average firm in eligible sectors has a share of exempted workers that is 3% higher than in 2012. Also, there are 6% more firms in eligible sectors whose share of workforce exemption is larger than 20% in 2015 relative to 2011. Again, we find no evidence of trends in these measures of workforce exemption between 2011 and 2012. These findings suggest that the 2013 reform was effective in its goal to broaden the access of firms and workers to the tax break.

Empirical Strategy. In order to estimate the firm-level effect of the 2013 extension of the tax break to new firm activities and bachelor's degree majors, we estimate the following DiD specification:

$$y_{ist} = \alpha_i + \boldsymbol{X'}_{ist}\beta_c + \lambda_{st} + \sum_{k=2011}^{2015} \delta_k \mathbb{1}[t=k] + \sum_{k=2011}^{2015} \beta_{DiD,k}\mathbb{1}[t=k] Exempted_{isk} + \varepsilon_{ist}, \quad (4.2)$$

where i stands for firm i, s for the sector of firm i, and t for the calendar year. α_i is the firm fixed effect (FE), and λ_{st} is the sector-by-year FE. $\mathbb{1}[t=k]$ is an indicator function that takes value 1 whenever an observation is in calendar year k. It is meant to capture common shocks across all firms in a given calendar year. We set 2012 as the reference year for the DiD coefficient estimates. We use as the outcome variables, y_{ist}, the firm i, year t, log(revenue), log(sales), log(number of workers), and log(total assets).

$Exempted_{isk}$ is a dummy variable that takes value 1 whenever in year k the firm i in sector s has more than 20% of its workforce exempted from the income tax. As mentioned above, we only keep firms with under 5% of workers exempted from the income tax in 2011 and 2012. Hence, by construction, all firms in the analysis sample have $Exempted_{isk} = 0$ for $k < 2013$. We choose 5% – instead of 0% – because there are other categories of workers who can benefit from this tax exemption, particularly those with disabilities.[33] We keep only firms with at most 5% workforce exemption before 2013 – as opposed to higher percentages

the main motivations of the 2013 amendment.

[33] For firms with more than 50 workers, at least 4% of their workers must be workers with a disability. In our data, we can only observe the number of employees exempted from the income tax and not the basis of the

– to mimic the tax conditions of IT firms before the initial introduction of the policy in 2001. That said, this threshold is ad-hoc and we run robustness checks that vary this threshold. Conversely, the 20% threshold after 2013 is chosen to avoid inadvertently measuring the effect of other income tax exemptions that are unrelated to the policy under study, and to ensure that the exemption from the income tax has a non-trivial effect on firm labor costs. This choice is meant to capture both firms whose sectoral code became suddenly eligible in 2013 and firms (with an eligible NACE Rev 1.1 sectoral code before 2013) for whom a significantly larger share of workers became eligible in 2013.[34] 20% is a typical percentage of exemption in the eligible sector. This threshold is also ad-hoc and we run robustness checks around this threshold as well.

In addition to the sample restrictions introduced in the data section above, in the baseline exercise, we only keep firms in ICT service sectors.[35] These firms have similar economic activities to the IT sector and face similar technology and demand shocks. We compare the average change in outcomes for firms for which more than 20% of workers become exempted from the income tax for the first time after 2013 (after having less than 5% exempted workers pre-2013) to the average change in outcomes for firms in ICT service sectors with less than 5% of workers exempted throughout 2011 to 2015.

There are two important differences between this DiD specification and the one in Equation (4.1). First, while in the previous specification firms were deemed treated after 2001 when they were part of the eligible sector (NACE Rev 1 code 722), firms are now deemed treated if they are part of the eligible sectors (NACE Rev 2 codes 5821, 5829, 6201, 6202, and 6209) *and* if they start with less than 5% of workers exempted from the income tax pre-2013 and suddenly exceed the 20% threshold of workforce exemption after 2013. Second, we now provide estimates that characterize a subset of the firms in the IT sector, which were most likely ineligible for the tax exemption before the 2013 amendment (either due to their sector code or due to their workers' bachelor's degrees).

This new definition of treatment has several advantages. While in the Amadeus data used for the 2001 exercise we did not observe how many of a firm's employees actually

exemption. That said, when a firm has more than 5% of its workers exempted from the income tax, it is very likely that at least part of these exempted workers owe their exemption specifically to the income tax break for workers in IT. Indeed, in October 2017, when information on the justification for the tax exemption became available, 96% of the exempted employees in eligible sectors were exempted due to the tax break for programmers. See C.5.2 for details.

[34] Our data does not allow us to separate between these two possibilities for two reasons. First, we do not have worker-level information. Second, we only observe firms' sector codes in 2016, already translated to the NACE Rev 2 classification. Put differently, we do not observe the sector codes that firms had before Romania transitioned to the NACE Rev 2 classification. As shown in Table C.16 (C.5.1), the NACE Rev 2 codes that became eligible in 2013 contain NACE Rev 1.1 codes that were both eligible and ineligible before 2013.

[35] We use the OECD definition of ICT service sectors, which includes the following NACE Rev 2 codes: 582 (Software publishing), 61 (Telecommunications), 62 (Computer programming, consultancy and related activities), 631 (Data processing, hosting and related activities, and web portals), and 951 (Repair of computers and communication equipment). Of these ICT sectors, only sectors 582 and 62 benefited from an income tax exemption for employees working on software development.

benefited from the tax break (if any), the administrative data we use in this exercise tracks this number and allows us to focus on firms whose workforce became treated to a sizable degree. In addition, defining treatment as firm-specific allows us to control for sector-by-year FEs, in addition to firm FEs. This new set of FEs control for potential sector-specific demand and/or technology shocks contemporaneous to the 2013 reform.

This new definition also raises concerns over the extent to which a jump after 2013 in the share of tax exempted employees is endogenous to firm characteristics. Lacking worker-level data and data on the NACE Rev 1 code of the sector of firms pre-2013, we cannot unequivocally address these concerns. Notwithstanding, we rely on the unexpected timing and generous nature of the 2013 reform (see Section 4.2). We also build on general information about the state of the IT sector around 2013. At that time, the sector faced a notably scarce labor supply relative to demand, which forced IT firms to hire programmers who were not eligible for the tax break. It is therefore very plausible that if a firm suddenly jumped from under 5 to over 20% of workforce exemptions after 2013, this jump was caused by the expansion of the lists of eligible sector codes and bachelor's degree majors. Conversely, firms who stayed under the 5% threshold throughout 2001 to 2015 either had a sector code that did not become eligible for the tax break or hired programmers whose bachelor's degree majors remained ineligible after 2013.

Moreover, all our specifications include firm fixed effects to control for time-invariant firm-specific unobservables. What we cannot rule out directly are firm-specific shocks contemporaneous with the 2013 policy shock, which may be the actual driver of both the jump in the firm-level workforce exemption share and the estimated firm growth effects. A change in management is a shock that may potentially explain both behaviors. That said, the jumps we study occurred after 2013, implying that they were most likely driven by the policy shock and not by large-scale coordinated changes in management. Last, the lack of preexisting differential trends with respect to firms in the comparison group also suggests that firms that experienced a sudden increase in the share of exempted workers were unlikely to be undergoing notable productive or organizational changes.

Following the literature on firm growth, in part of our specifications we also control for lagged size, age, and relative productivity (found in the firm-specific vector of time-varying characteristics, $\boldsymbol{X}_{ist}$) (Doms et al., 1995; Lotti et al., 2003; Coad, 2009; Barba Navaretti et al., 2014). Categories for firm size follow the Eurostat definition: micro (1-9 workers), small (10-49 workers), medium (50-249 workers), and large (250 or more workers). Young is an age dummy variable that takes the value 1 if the firm is five years or younger (as in Lotti et al., 2003; Falk and Hagsten, 2018). Relative productivity is defined as a firm's labor productivity relative to the most productive firm in the same sector (as in Falk and Hagsten, 2018).

Last, we will show that our baseline estimates are robust to (i) running the regression in Equation (4.2) on a dataset from Amadeus that starts in 2008 and allows us to observe a longer pre-reform period, (ii) using an alternative comparison group of firms in other high-tech knowledge-intensive service sectors, (iii) restricting the sample only to firms in the eligible IT sector, (iv) using an alternative event-study research design, (v) varying the threshold choice (e.g., from 20 to 15%), and (vi) defining treatment only based on the sector of the firm, as in the 2001 DiD specification.

Baseline Results. Figure 4.2 plots the DiD estimates from the model in Equation (4.2). These estimates pertain to our baseline choice of the comparison group (see definition above). We find that across all outcome variables the estimate of $\beta_{DiD,2011}$ is not statistically different from zero. This lack of anticipation effects is in line with the unexpected nature of the expansion. Moreover, firms whose workforce became significantly exempted from the income tax after 2013 did not embark on differential growth trends relative to firms whose exemption rate was left unaffected by the reform. After 2013 however, the treated firms experience a gradual growth along all four measures of firm size, such that in 2015, they have a 37% higher revenue, 38% higher sales, 21% more workers, and 26% more assets relative to 2012.[36]

Table 4.2 provides more details on these estimations. Columns (1)-(4) show the results of the estimations without firm controls (other than firm FEs) and plotted in Figure 4.2, whereas columns (5)-(8) show the results from adding firm-specific controls (initial size, relative productivity, and age). As expected, controlling for year $t-1$ productivity, size, and age decreases the magnitude of the coefficients, but they all remain statistically and economically significant. As in Doms et al. (1995); Falk and Hagsten (2018), firms that were initially larger or more productive grew faster. Age is found to have an insignificant effect on firm growth.

Heterogeneity Analysis. The DiD estimates so far refer to the average effect on treated firms. However, an important policy concern is that the tax break expansion benefited only specific groups of firms, for instance large foreign firms. We estimate the baseline specification on four groups of firms, defined based on their size and age in 2011: (i) hiring strictly less than ten workers (micro firms), (ii) hiring at least ten workers (small, medium, or large firms), (iii) strictly less than five years old (young firms), (iv) at least five years old (old firms). Because we do not observe foreign ownership in 2011, we cannot directly alleviate the concern that foreign firms benefited more from this policy. That said, most foreign-owned firms in the sector are likely to be large and most micro firms are likely to be domestically-owned.

Table 4.3 reports the results of this heterogeneity analysis. The main takeaway is that the tax exemption had a positive effect on all four types of firms that are considered. The effects are as large for micro-sized firms (typically start-ups) as they are for larger firms. Younger firms experience higher growth than older firms. This last finding is likely to reflect the fact that the older firms treated by the 2013 reform may be negatively selected: most older firms already had more than 5% exempted employees in 2011 and, hence, were not included in

[36] The lower panel of Table 4.2 shows the estimate of a pooled DiD coefficient that measures the average increase in an outcome from the 2011-2012 period to the 2013-2015 period. These estimates are 25% for revenue, 27% for sales, 15% for number of workers, and 18% for total assets. Controlling for other firm characteristics reduces the estimated effects to 13% for revenue, 16% for sales, 11% for number of workers, and 8% for total assets.

this analysis. To conclude, the 2013 expansion of the tax break has been beneficial for firms of all sizes and for young firms in particular.

Robustness Checks. All robustness checks for our 2013 baseline results are in C.1.1. In our first exercise, we run our baseline regression from Equation (4.2) on an Amadeus dataset that starts in 2008. The firms in the Amadeus sample are those firms from the baseline sample based on administrative data (5,177 firms) that were found in Amadeus (3,889 of these 5,177 firms). Table C.4 presents the results from this Amadeus sample using data from two sources: the Amadeus data and the administrative data. The similarity between the estimates in columns (5)-(8) and those in columns (1)-(4) from Table 4.2 suggest that the sample matched with Amadeus is representative for the baseline sample. The similarity between the estimates in columns (1)-(4) and those in columns (5)-(8) suggest that the Amadeus data is of comparable quality to the administrative data. The main advantage of the results from columns (1)-(4) (relative to those in columns (1)-(4) from Table 4.2) is that they allow us to observe firms for up to five years before the reform (relative to 2 years). Treated and comparison firms continue to exhibit parallel trends before 2013, which is reassuring in regard to the suitability of the comparison group.

Next, we show that our baseline estimates are robust to reasonable alternative comparison groups. In Table C.5, the sample of analysis uses firms in high-tech knowledge-intensive service sectors (as classified by Eurostat). In addition to other similar sectors, this category includes the eligible sectors. These new results corroborate with the baseline results. To the extent that one might be concerned that results are driven by the inclusion of firms in non-eligible sectors, Table C.6 repeats the analysis using only firms in eligible sectors. Despite losing half of the baseline sample, results are strikingly similar.

Another concern is that our baseline results are driven by the contrast to firms who always remain under the 5% threshold of worker exemption. To overcome this concern, we use an event-study design where we exploit the staggered timing of the moment when firms jump over the 20% threshold. Even in the restricted sample, which only keeps the firms experiencing the jump, the event-study estimates from Columns (5)-(8) in Table C.7 continue to display a lack of pre-trends and a clear pattern of growth after the expansion in the tax exemption. These findings suggest that our baseline results are not an artifact of the choice of the comparison group. C.1.1 provides more details on this event-study analysis.

Estimates of the DiD coefficients are also robust to the choice of the share of exempted workers above which we consider a firm to become treated. Table C.8 shows the results of estimations where treatment arises when firms jump after 2013 to at least 15% of workers exempted from income tax. These results are only slightly smaller in magnitude to those obtained for the 20% threshold, which is consistent with the fact that we are including firms with a lower intensity of treatment in the sample. Results from other values of thresholds are available upon request.

In a last robustness check, we revert to the definition of treatment based on a firm's sector, that we use to study the initial introduction of the policy in 2001 (see Equation (4.1)). This definition mitigates concerns over the potentially-endogenous firm-level jump in the share of exempted employees. Because this definition does not depend on a firm's share of exempted employees, we estimate this model on two samples: the full sample of firms in ICT service sectors, unrestricted based on firms' exemption shares (columns (1)-(4) in Table C.9), and the baseline sample, for direct comparability (columns (5)-(8)). Before 2013, we continue to find no evidence of differential trends between firms in eligible sectors and firms in non-eligible ICT service sectors. After 2013 however, we find evidence of growth for firms in eligible sectors. Although statistically significant, these effects are smaller than our baseline effects in Table 4.2. This is intuitive: in 2015, the average firm in eligible sectors has a share of exempted workers that is 3% higher than that in 2012 (see Table C.2). This jump is to be compared with the jump from under 5% pre-2013 to over 20% post-2013 that characterizes the treated firms in our baseline specification. The smaller effect is therefore in line with the smaller jump. Notwithstanding, the results from this exercise also lend support to the suitability of the baseline DiD design used for the 2013 analysis.

4.3.3 Discussion of the Firm-Level Findings

Overall, we find that both policy episodes – the introduction of the tax break in 2001 and its 2013 reform – have led to strong and long-lasting growth for firms in the eligible IT sectors. A natural first question is whether these firm-level effects are consistent with the statutory incidence of the tax break on the personal income of workers. We provide three arguments in favor of an affirmative answer.

First, the tax break only applies to the income of workers with an eligible bachelor's degree specialization who are matched with firms operating in an eligible sector. To benefit from the tax break, these workers are then supposed to develop software for these firms that generates at least 10,000 U.S. dollars in business revenue per year and per exempted worker. These restrictive requirements imposed on worker-firm matches are likely to turn the qualifying firms into desirable employers and to improve their bargaining power over a shared economic incidence of the tax break.

Second, the tax break law stipulates that it is the responsibility of the firm to prepare the necessary paperwork and apply for the income tax break of its workers. This requirement of an explicit "buy-in" from the employer is another reason to expect a shared economic incidence of this tax incentive. To the extent that access to financing is scarce in Romania (as in other emerging economies) and firms are cash-constrained, firms can use their savings in labor costs from the tax break towards expansion.

Third and last, both policy episodes have led to improvements in the labor productivity of treated firms (measured as revenues or sales per worker). For instance, Table C.3 (C.1.1) shows that in 2015, firms treated by the 2013 reform became 16% more productive than comparison firms. There are three plausible drivers of these improvements in labor productivity. First, this policy allows firms to pay workers higher net wages and, by doing so, to improve their level of motivation and efficiency. Second, the increase in the number of workers in the

IT sector (see Figure 4.4) is likely to have led to sector-level economies of scale.[37] Finally, it is also likely that both policy episodes have attracted higher-ability workers to software development.

While all these factors suggest a shared economic incidence of the tax break, the estimation of how the tax incentive is split between firms and workers is outside the scope of this paper. Most important, we lack worker-level data, meaning that any estimate of this split would lean too heavily on the structure of a model. Moreover, the split is likely to change with time, depending on factors such as the entry of new firms into the IT sector and the long-run elasticity of the supply of programmers. That said, we conclude that firm-level effects are in line with the incentive structure of this policy.

Before comparing the magnitude of the estimates derived from the two firm-level exercises, it is important to first highlight the distinguishing features of each exercise. When this policy was introduced in 2001, the IT sector of Romania was in its infancy. Before 2001, Romanian programmers were emigrating at high rates, lacking confidence that the Romanian IT sector was poised for growth. According to media articles from 2001, both programmers and incumbent IT firms perceived the policy as a signal that the development of the sector had become a priority for policy-makers. Whereas firms and workers who became eligible in 2001 for the tax break are still eligible to this day, in the 2001 exercise, we only focus on the 1999 to 2005 period. This aims to isolate the effects of this tax break from either those of other policies that may interact with it (such as the switch in 2004 from progressive income taxation to a flat income tax) or other global shocks that may differentially affect the IT sector.

Moreover, in this 2001 exercise, we use Amadeus data that does not contain the actual exemption rate of the workers of a firm but only the sector of the firm. Hence, our 2001 exercise estimates the effects of the introduction of the policy on firms whose sector is eligible (NACE Rev 1 722), which are “intent-to-treat" (ITT) effects. Table 4.1 indicates that the typical firm in the eligible sector hired 17% more workers in 2005 relative to 2000 and relative to the typical firm in non-eligible comparable sectors. Under the assumption of 25% take-up among firms in the eligible sector, we can convert our ITT estimates to “treatment-on-the-treated" (TOT) estimates. This back-of-the-envelope calculation suggests that firms whose workers actually benefited from the income tax break grew their workforce by 68%. Given that in 2000, the median firm in the eligible sector hired four workers, such an increase in the number of workers is not unreasonable. Furthermore, the firms who took advantage of this new tax break during those initial years were likely to be positively selected from all firms in the IT sector, lending further credibility to these magnitudes.

The key advantage of the exercise studying the 2013 reform is that it estimates the effects on firms who actually benefit from the tax break expansion by increasing their share of workforce exemption from under 5% pre-2013 to over 20% post-2013. The benefit of this definition is that it allows us to isolate firms experiencing a sizable (relative) decrease in labor

[37] While measuring external economies of scale for manufacturing sectors alone, Bartelme et al. (2018) find large sector-level economies of scale, particularly in high-technology sectors such as “Computers and Electronics."

costs. That said, this definition also has the disadvantage of focusing on a sample of firms that in 2012 – 11 years after the initial introduction of the policy – have under 5% exempted employees (most likely either due to their sector code not being eligible or their not being able to hire eligible workers, both according to pre-2013 eligibility rules). This suggests that this sample of firms is likely to be negatively selected relative to the sample of firms with higher exemption rates (e.g., be younger and less experienced, or smaller). This is likely to explain the smaller 21% estimate for the increase in workforce after the 2013 reform (see Table 4.2), relative to the 68% back-of-the-envelope TOT estimate for 2001. Nevertheless, the purpose of the 2013 reform was exactly that of improving the reach of the tax break to relevant but not yet eligible firms and workers. In addition, the size of the tax incentive (as a share of the wage) is larger between 2001 and 2004 than after 2004 (hence after 2013). This is because – while programmers are fully exempt from the income tax in all years after 2001 – between 2001 and 2004 Romania had a progressive income tax (with a 40% marginal top tax rate), whereas in 2004, it switched to a flat income tax rate of 16%. This is another plausible reason why the treatment effects are smaller in the 2013 exercise than in the 2001 exercise.

4.4 Sector-level Cross-Country Analysis: 2001 Income Tax Break

The firm-level analysis in the previous section focuses on the behavior of incumbent firms in the IT sector in Romania, either at the time of the introduction of the 2001 income tax break or of its expansion in 2013. We now present two sector-level cross-country analyses: one that estimates the overall growth after 2001 of the eligible IT sector relative to the rest of the economy, the other that estimates the growth after 2001 of sectors using IT services intensively relative to those using IT services less.

4.4.1 Direct Effects on the Expansion of the IT Sector

This sector-level study of the overall growth of the IT sector relative to the rest of the economy complements the firm-level analysis from Section 4.3 in two ways. First, this analysis captures not only the intensive margin of growth (as the firm-level analysis does) but also the extensive margin (through the entry of new firms into the IT sector). While the available data does not allow us to conduct a complete analysis of the patterns of entry (exit) into (out of) the IT sector, C.2 presents suggestive evidence that entry rates into the IT sector have increased after 2001 (both the entry of entirely new firms and the entry of firms previously in other sectors), whereas the exit rates have not.

Second, this synthetic control method (SCM) analysis allows us to alleviate concerns about potential confounding factors that may affect the IT sector globally and that may be the true cause of the effects measured with firm-level data. One such confounding factor could be the dot-com crash of 2001. In its aftermath, U.S.-based companies may have chosen to mitigate some of the losses incurred during the crash by offshoring part of their operations in CEE countries.[38] Sector-specific global technological or demand shocks could be another confounding factor for the growth of the Romanian IT sector. We therefore benchmark the growth of the IT sector in Romania to that in similar neighboring countries, that are likely to have been similarly affected by such confounding third factors.

Empirical Strategy. We use SCM to measure the effect of the income tax break on the growth of the IT sector in Romania. SCM is a data-driven approach to small-sample studies proposed by Abadie and Gardeazabal (2003) and used to estimate treatment effects. The intuition of SCM is that a weighted combination of countries provides a better comparison for Romania than any single country alone. SCM makes explicit the relative contribution of each control country to the counterfactual of interest, and the similarities (or lack thereof) between Romania and synthetic Romania, in terms of pre-intervention outcomes and other predictors of post-intervention outcomes (Abadie et al., 2010). The choice of weights is such that the resulting unit closely matches the treated unit over the pre-treatment period. Outcomes for the synthetic control are then projected into the post-treatment period based on these weights. Inference is conducted using placebo tests. The same model is estimated on each untreated country, assuming that it was treated in 2001. The result is a distribution of placebo effects. If this procedure does not yield effects on untreated countries as large as the effects for Romania, then it is unlikely that the estimated effect for Romania is a result of chance.

Formally, SCM entails the following. Let J be the number of available control countries (“the donor pool"), where J equals 13 in our case. Let $\boldsymbol{W} = (w_2, ..., w_{J+1})'$ be a $J \times 1$ vector of weights w_j, such that $w_j \geq 0$ and $\sum_{j=2}^{J+1} w_j = 1$. w_j is the weight of country j in synthetic Romania. SCM chooses W such that synthetic Romania most closely matches the real Romania before 2001 (more specifically, in 1999 and 2000). Let $\boldsymbol{X_1}$ be a $(K \times 1)$ vector of pre-2001 values of K predictors for the relative growth of the Romanian IT sector. Similar to $\boldsymbol{X_1}$, we define $\boldsymbol{X_0}$ as the $(K \times J)$ matrix containing the values for the same predictors for the J candidate control countries. We include in $\boldsymbol{X_1}$ and $\boldsymbol{X_0}$ the three predictor variables just mentioned.

Let V be a diagonal matrix with non-negative components, whose diagonal elements represent the relative importance of these three predictor variables in the construction of synthetic Romania. The vector of weights $\boldsymbol{W}$ is chosen to minimize the objective function $(\boldsymbol{X_1} - \boldsymbol{X_0 W})'\boldsymbol{V}(\boldsymbol{X_1} - \boldsymbol{X_0 W})$, such that $w_j \geq 0$. We follow Abadie and Gardeazabal (2003) and Abadie et al. (2010) and allow for the choice of the weighting matrix, $\boldsymbol{V}$, to be data-driven. $\boldsymbol{V}$ is the matrix that allows for the pre-2001 outcome of Romania to be closest to

[38] Given that the typical firm in the IT sector is high skill-intensive and low capital-intensive, researchers have argued that IT sectors in CEE had suitable conditions for development (Radosevic, 2006; Grigoraş et al., 2017): CEE countries tend to have a highly-skilled labor force in relevant technical and scientific fields, while lacking modern physical capital. This explains why it is important to benchmark the growth of the Romanian IT sector to that in other CEE countries.

the outcome for the synthetic control obtained from $\boldsymbol{W}^*(\boldsymbol{V})$.[39]

Treatment effects, α_{1t}, for Romania in post-treatment years t (2002 to 2015) are estimated as the difference between the year t outcomes for Romania and those for synthetic Romania:

$$\hat{\alpha}_{1t} \quad = \quad Y_{1t} \ - \ \sum_{j=2}^{J+1} w_j^* Y_{jt},$$

where Y_{1t} is the year t outcome for Romania and Y_{jt} is the same outcome in year t for control country j.

Concretely, we first contrast the within-country growth of the IT sector to the growth of all other sectors in the economy. Through SCM, we then compare this relative growth of the IT sector in Romania to the relative growth of the IT sector in synthetic Romania. An advantage of SCM is that it delivers the optimal set of weights to construct synthetic Romania and limits the researchers' degrees of freedom in the choice of the comparison group. Last, we ask whether the relative growth of the IT sector in Romania is exceptional compared to the relative growth experienced by the sector in untreated countries.

Data. The data source for the dependent variables is Eurostat, Structural Business Statistics. We require from these variables to be available in 1999 and 2000 for both Romania and all other countries in the donor pool. Also, we require these variables to appear consistently in the following years. Finally, we want these variables to have at least a minimum relevance for this study. These conditions are met by the following five variables: number of employees, gross revenue (turnover or gross premiums written, in million euro), production value (in million euro), gross investment in tangible goods (in million euro), and the number of enterprises. Data for the predictors (pre-treatment characteristics used to generate the weights) comes from the World Bank, World Development Indicators. We use as predictors the GDP per capita (constant LCU), the share of manufacturing value-added coming from the medium and high-tech industry, and the share of GDP coming from services.

For data availability reasons, the IT sector is defined broadly as K72 (Computer and related activities).[40] We use as comparison sectors all other sectors in the economy. To obtain normalized values for the outcome variables each year and country, the yearly absolute value of the variable in the treated sector is divided by its value in 2000, the year prior to

[39] We implement SCM with the help of the **synth** and **synth_runner** packages in Stata (Quistorff and Galiani, 2017). We depart from the default option of these packages by selecting the *nested* option. Hence **synth** embarks on a fully nested optimization procedure that searches among all (diagonal) positive semidefinite $\boldsymbol{V}$ matrices and sets of $\boldsymbol{W}^*$-weights for the best fitting convex combination of the control units. The fully nested optimization contains the regression based $\boldsymbol{V}$ as a starting point, but produces convex combinations that achieve even lower mean squared prediction error.

[40] K72 includes K721 (Hardware consultancy), K722 (Software consultancy and supply, including K7221 – Publishing of software – and K722 –Other software consultancy and supply), K723 (Data processing), K724 (Database activities), K725 (Maintenance and repair of office, accounting and computing machinery), and K726 (Other computer related activities). We use the two-digit aggregation of the data due to frequent missing values at the three-digit level.

the introduction of the policy in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors.[41]

The donor pool of countries from which to create the synthetic control for Romania contains Bulgaria, the Czech Republic, Estonia, Hungary, Ireland, Latvia, Lithuania, Poland, Portugal, Slovenia, and the Slovak Republic. These countries were chosen based on their geographic proximity, similarity in development, performance in the IT sector pre-2001, and data availability. See C.4 for details on data construction.

Baseline Results. Figures 4.3 and 4.4 present the output of our SCM analysis for two dependent variables: gross revenue and employment (both normalized). The upper left panels show the evolution of these outcomes in Romania and in synthetic Romania. Before 2001, the growth of the IT sector of synthetic Romania closely mimics that of Romania. From 2001 onward, both the gross revenue and employment of the Romanian IT sector experience a marked relative growth. The upper right panels show the difference between the outcomes of Romania and those of synthetic Romania. Fourteen years after, the gross revenue (employment) in the IT industry in Romania had expanded 6.52 (1.83) times more relative to the gross revenue (employment) in all other sectors, relative to the year 2000, and relative to the corresponding relative growth in synthetic Romania.

The lower left panels plot the raw paths of these normalized outcomes of the IT sector in Romania and the 11 donor countries. We notice how exceptional the growth was in Romania, compared to that in all donor countries. Last, we implement a battery of placebo tests that considers all other donor countries as potentially treated and proposes synthetic controls for each. Reassuringly, the lower right panels show that the relative growth for Romania is starker than the relative growth for all other donor countries. For at least until 2008, the actual treatment differences of gross revenue and employment growth for Romania lie outside the range of placebo differences. Formally, these results are confirmed by the almost-zero p-values until 2008 (see Table C.10, C.1.2).

One might be concerned that synthetic Romania is an unreasonable proposition of SCM. Synthetic Romania is a combination of Bulgaria, the Czech Republic, and Slovakia (with weights varying with the outcome variable). Table C.11 shows that synthetic Romania is reasonably similar in terms of the share of services in GDP and the share of high-tech manufacturing in total manufacturing value added. While synthetic Romania is different to Romania in terms of its GDP, our SCM analysis is relative to each country's level in 2000. Hence, this proposal of synthetic Romania seems appropriate.

Robustness Checks. All robustness checks figures and tables can be found in C.1.2. We first show that these findings are not unique to gross revenue and employment. Figure C.1 (and its associated Tables C.10 and C.11) shows similar patterns of outstanding growth

[41] For example, a value of 2 in year t for a given country means that the multiplication factor of the value of the dependent variable in the treated sector in year t, relative to year 2000, is larger by 2 units than the counterfactual multiplication factor in comparison sectors.

in the IT sector of Romania, this time in terms of production value.[42] Again, we employ permutation methods to assess the statistical likelihood of our results. In the first seven years after the introduction of the policy, almost-zero p-values allow us to rule out a treatment effect of zero. Visually, the lower right panel of Figure C.1 shows that the actual difference for Romania is consistently above the upper limit of placebo differences.

One concern with deriving results from the entire 1999 to 2015 time series of Eurostat data may come from the need to rely on a crosswalk between NACE Rev 1.1 and Rev 2 sector codes. Because pre-2007 data is reported for NACE Rev 1.1 sectors and post-2007 data for NACE Rev 2 sectors, one needs a crosswalk to stitch together the time series. As the relationship between classifications is not bijective, there is no widely-used crosswalk. While we envisaged several options, we find that results are not driven by the crosswalk choice.[43]

We test whether our results are driven by the use of the full time series, by truncating the time series in 2007. This way we can study the effect of the 2001 policy using data that is consistently reported within one classification. Fortunately, we find that results for pre-2007 years are not affected by the addition of post-2007 years (see Figure C.2). However, this finding does not imply that post-2007 results are not affected by the stitching of the sector-level time series. We cannot distinguish whether our weaker post-2007 results are driven by the differential effects of the crisis on the IT sector (compared to the rest of the economy), by a later introduction by donor countries of other policies that also favor the IT sector, or by an imprecise stitching of the sector-level time series.

Last, an alternative exercise is one in which we would contrast the growth of the IT sector to that of the same three-digit comparison sectors used in the firm-level analysis (see Sections 4.3.1 and 4.3.2). A first constraint comes from the fact that Eurostat data is at the two-digit level. Second, the two-digit sectors 64 and 92 cannot be used, as their data is frequently missing across years and countries. Third, sector 73 is small and with noisy data, and sector 74 contains several three-digit sectors other than those we use as control to sector 722 (sectors we believe are dissimilar to 722). Despite these caveats, sector 72 still exhibits a faster growth than that of comparison sectors 73 and 74. Results are available upon request.

4.4.2 Downstream Effects of the Expansion of the IT Sector

Given that, in 2017, the IT sector accounted for less than 2% of Romania's total employment and that only specific workers in this sector are eligible for the income tax break, one might question the wider effects of the policy. The IT sector is a sector whose inputs are broadly used, across sectors and households. While the development of the IT sector most likely led to level effects – given its large base of users – we propose a research design that allows us to credibly estimate the differential effects of the policy on sectors that relied more heavily on IT services relative to sectors that relied less.

[42] While the underlying data is of lower quality, we obtain qualitatively similar results for the number of firms and gross investment in tangible goods (both normalized). Results are available upon request.

[43] See C.4 for details on our crosswalk construction and choice. SCM results using different crosswalk choices are available upon request.

The development of the IT sector after 2001 is likely to have boosted the development of sectors relying more heavily on IT services in two ways.[44] First, after the tax break, labor productivity (measured as sales per worker) increased (see Section 4.3.3). As labor is the main input in the production of IT services, this is also likely to have improved the quality of IT services. Hence, the tax break is likely to have lowered the quality-adjusted price of IT services. Under a plausible market structure and well-behaved cost and demand conditions, this should lead to output increases in sectors purchasing more inputs from the IT sector (Lane, 2016). To the extent that the increase in the quality of IT inputs was not fully priced, then IT-using sectors not only experienced increases in output but also in productivity.

Second, the IT sector has also expanded through the entry of new firms (see Section 4.4.1 and C.2). Figure C.12 (C.2.3) suggests that part of this entry occurred through foreign direct investment in the IT sector of Romania, which has intensified since the early 2000s. This implies that the IT sector has considerably expanded the set of varieties proposed to downstream sectors. Whenever downstream sectors have a love of variety for intermediate inputs, this expansion in varieties leads to productivity gains.[45] In addition, varieties proposed by foreign-owned firms are likely to have been of higher quality than those proposed by domestic firms.

All in all, improvements in the prices, quality, and variety of IT inputs are likely to have provided a boost to downstream sectors relying more on IT. We study not only the evolution of the size of these sectors, but also their export performance. To the extent that a stronger IT sector generates productivity gains for IT-using downstream sectors, this is likely to shift trade patterns.

Empirical Strategy. To study the effect of the 2001 tax break on the expansion of downstream sectors, we employ a similar SCM to the one described in Section 4.4.1. The main difference between these exercises is in the definition of treated and comparison sectors. Hereafter, we define treated sectors as those downstream sectors for which the IT sector is most important as the supplier of inputs. Conversely, comparison sectors are those relying relatively less on the IT sector as an input supplier.

Data. We start from the input-output table (I-O table, henceforth) of Romania for 2000. We use the harmonized I-O table provided by the OECD, which tracks the flows of goods and services between all two-digit NACE Rev 1 sectors. Given that 2000 is the year before the unexpected introduction of the income tax break for workers in IT, inter-industry linkages are not yet affected by this break. We then compute the share of the total input expenditures of a given sector purchased from the IT sector (NACE Rev 1 sector 72, "Computer and related

[44] One scenario in which the introduction of this policy might have *hurt* the development of sectors relying more heavily on IT services, is one in which firms in these sectors used to produce programming services in-house. As programmers only benefit from the tax break if hired by a firm in the eligible IT sector, this may have made them less likely to join ineligible sectors. We believe this scenario is unlikely, as it was uncommon for firms in non-IT sectors to develop software in-house. Note that employees who work in maintenance are not eligible for this tax break, irrespective of their employer.

[45] See Rodríguez-Clare (1996a); Goldberg et al. (2010); Carluccio and Fally (2013b); Kee (2015)

activities"). Based on these shares we identify the sectors for which IT services are the most important inputs in 2000. Based on their position in this sector-level distribution of shares, we assign sectors to either a high- or low-intensity category of use of IT services. The treated high-intensity category contains sectors that are among the top 25% users of IT services. All other sectors lying in the bottom 75% constitute the control category.[46]

Similar to the SCM in Section 4.4.1, we rely on Eurostat data to construct the same normalized dependent variables and World Bank data for the same predictor variables. In addition, we use UN Comtrade data to study the export performance of sectors relying more on IT services, relative to those relying less. Given data availability, we use SCM to study the exports of goods alone. One notable advantage of UN Comtrade data is that it starts in 1996, offering three more years than the Eurostat data of pre-treatment years.[47] See C.4 for details on data construction.

Baseline Results. We first ask whether sectors with stronger upstream linkages to the IT sector experienced a more pronounced growth than sectors with weaker linkages. Figures 4.5 and 4.6 provide a visual answer for gross revenue and employment. Fourteen years after the introduction of the policy, gross revenue (employment) in sectors with high-intensity use of IT services has grown 0.75 (0.61) times more than gross revenue (employment) in low-intensity sectors (compared to year 2000 and compared to the equivalent difference in synthetic Romania). When implementing the permutation method suggested by Abadie et al. (2010), we find that our SCM estimates lie at the upper limit of the distribution of placebo estimates. The low p-values in Table C.13 (C.1.2) rule out null effects.

Next, we study the export performance of sectors relying more on IT services, compared to those relying less. Because high-intensity sectors (defined until now as those over the third quartile of the IT-usage intensity distribution) are all service sectors, we now define high-intensity sectors as those manufacturing sectors between the second and third quartile (see Table C.12 in C.1.2). Figure 4.7 depicts a striking relative growth in the export trade value of goods from high-intensity sectors in Romania (relative to those from low-intensity sectors and relative to synthetic Romania).[48] Placebo tests show that this relative growth in Romania is exceptional compared to that predicted for all other countries in the donor pool.[49] Table C.15 (C.1.2) makes the same argument, formally. This evidence suggests that

[46] We also calculate the share of the total sales of the IT sector purchased by each sector. Again, we assign sectors to quarters based on these new shares. While these two classifications are conceptually different, given the I-O table of Romania for 2000, we find a full overlap in the sets of sectors that belong to the top 25% of the two distributions. Hence, there is no practical difference in the final split of sectors between the top and bottom three quartiles.

[47] We cannot study service exports, as the EBOPS data on service exports only starts in 2000. Studying the exports of the treated IT sector (72 NACE Rev 1 code) is not possible either, as the output of 72 is mostly in the form of services. Moreover, Romania only starts reporting exports for 72 in 2005.

[48] Most of this growth is explained by the SITC Rev 1 commodity codes 54 (Medicinal and pharmaceutical products), 62 (Rubber manufactures), 73 (Road vehicles, other than motor vehicles), 86 (Watches and clocks), and a combination of Chemical Industry codes such as 59, 55, 53.

[49] Figure C.7 (C.1.2) shows that after 2000 the exports of service sectors relying more on IT services also grew

the development of the IT sector in Romania not only increased the output of IT-using sectors, but also improved their comparative advantage.[50]

Robustness Checks All robustness checks figures and tables can be found in C.1.2. We first show that our baseline findings on the relative growth of downstream sectors with heavier usage of IT services are not specific to gross revenue, employment, or export value. For instance, we find that the production value of IT-using sectors has also grown significantly more in Romania (see Figure C.3).[51]

Second, it is important to note that the SCM findings presented above are robust to different choices of the weighting matrix $\boldsymbol{V}$ and matrix of weights $\boldsymbol{W}$ matrices (defined in Section 4.4.1).[52] Another potential concern with SCM relates to its sensitivity to the number of pre-intervention periods used in the computation of the weights. This concern cannot be ruled out with the Eurostat data, as the panel only starts in 1999. However, a benefit of Comtrade data is that it allows us to observe export patterns consistently since 1996. We run the Comtrade SCM exercise varying the number of pre-treatment years used in the estimation. Reassuringly, treatment effects remain unaltered.

Third, one might be concerned that our results are driven by specific sectors from the treated category. From the beginning, in the analysis using Eurostat data, we exclude NACE Rev 1 sector 72, as this two-digit sector contains the three-digit sector 722 eligible by the income tax break. This avoids the risk of a mechanical result. In addition, as a robustness check, we also exclude NACE Rev 1 sectors 73 and 74. These two sectors belong to the treated category of sectors (see Table C.12), but also contain three-digit NACE Rev 1 sectors that we use as comparison sectors in the firm-level analysis of Section 4.3.1. To the extent that sectors 73 and 74 – or their subset of three-digit sectors comparable to sector 722 – were experiencing correlated shocks with those of sector 722, our findings could be affected by such shocks. Figure C.4 shows that when we exclude sectors 73 and 74 the treatment effect is actually larger than the one found in the baseline Figure 4.6.[53]

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noticeably faster in Romania than in comparable countries. Among these sectors, those under NACE Rev 1 sector 74 (e.g., call centers, advertising, business and management consultancy, secretarial and translation activities etc.) experienced the most impressive growth. Romania's trend is compared to that of the five countries that constitute the typical synthetic Romania in all SCM exercises thus far, i.e. Bulgaria, the Czech Republic, Hungary, Lithuania, and Slovakia.

[50] The behavior of FDI flows to Romania also supports this claim. Figure C.12 (C.2.3) shows that FDI in high-intensity sectors grew faster than FDI in low-intensity sectors. While not the only driver behind this relative growth in FDI, Romania's IT sector is frequently mentioned among those that are most significant. For instance, Renault decided in 2007 to build its Technocentre in Romania, which took over a large share of the R&D previously done in France. In various articles motivating this decision, Romania's IT sector seems to have played an important role. As an example, this article describes how Romania's accession to the EU benefits France, e.g., through its IT skills and their importance to firms such as Renault.

[51] We find similar, yet noisier, patterns for gross investment and the number of firms. Results are available upon request.

[52] We implement these different choices by selecting different options of the **synth** and **synth_runner** packages in Stata. Our main results are obtained using the *nested* option, which maximizes the fit during pre-intervention periods.

[53] We decide to keep 73 and 74 in the main specification for two reasons. First, results obtained when including

Fourth, we also show that results have qualitatively similar patterns when we change the threshold of the grouping of sectors into the high- and low-intensity categories. Figure C.5 presents results from the grouping of sectors *under the median* of usage of inputs from the IT sector into the low-intensity category (as opposed to *under the third quartile*) and sectors *over the median* into the high-intensity category. As expected, while the difference in the development of these two new categories becomes less stark, the general pattern is maintained.

Fifth, one might also worry that relying on Romania's I-O table from 2000 to construct the high- and low-intensity treatment categories is a concern in itself. As an alternative, we use the classification of sectors proposed by van Ark et al. (2003). Sectors are assigned one of the following six categories based on U.S. measures of pre-2000 ICT (information and communication technology) intensity from Stiroh (2002): ICT-producing manufacturing, ICT-producing services, ICT-using manufacturing, ICT-using services, non-ICT manufacturing, and non-ICT services. We exclude sectors in ICT-producing manufacturing and ICT-producing services, as they might be directly affected by the tax break. We group sectors in ICT-using manufacturing and ICT-using services into the high-intensity category, and sectors in non-ICT manufacturing and non-ICT services into the low-intensity category. While the patterns obtained with this grouping are noisier than those obtained with our preferred grouping, we still find a stronger relative growth in Romania in ICT-using sectors compared to non-ICT using, and compared to synthetic Romania.[54] We assess our initial grouping to be superior, as it is more narrowly defined around the treated sector (NACE Rev 1 sector 722) than the one proposed by Stiroh (2002).[55]

Finally, as for the SCM exercise in Section 4.4.1, we verify whether our results are sensitive to the exclusion of the second half of the Eurostat time series. As explained in Section 4.4.1 the lengthening of sector-level time series to include 2007 to 2015 relies on an inherently-imprecise crosswalk between the NACE Rev 2 industry classification and NACE Rev 1. Figure C.6 shows that results for the years under the NACE Rev 1 classification (1999 to 2006) are identical to those for the same years obtained using the full time series (1999 to 2015). This concern does not apply to the Comtrade SCM exercise as the data is reported within the same classification for all years.

In addition to these robustness checks, the timing of the relative growth of IT-using sectors speaks against concerns of reverse causality, i.e., it is the development of downstream sectors using IT intensively that actually boosted the development of the IT sector.[56]

these two two-digit sectors are more conservative. Second, both sectors contain many other three-digit sectors that are not comparable to 722, and, hence, are less likely to experience correlated shocks with 722. The main SCM exercise using Comtrade data excludes, by construction, all service sectors, hence sectors 72, 73, and 74 are not a concern.

[54] Results are available upon request.

[55] ICT contains several other (significantly larger) sectors than 722, unrelated to the policy we study.

[56] This does not exclude the possibility of a feedback loop between the development of IT-using sectors and the IT sector itself. In a 2016 *Reuters* article, Florin Talpeş (a pioneer in Romania's IT sector) advised new entrants in the IT industry to focus on developing technology for the now-mature automotive industry (e.g., driver-less technology or car connectivity).

4.4.3 Discussion of the Sector-Level Cross-Country Findings

In Section 4.4.1, we show that, since 2001, the IT sector in Romania has grown significantly faster compared to the rest of the sectors in Romania and compared to the same relative growth in similar countries. This finding gives us confidence that the effects we measure are plausibly caused by the tax break to programmers introduced in Romania, and not by other global supply- or demand-side shocks that benefit the IT sector in Romania (and in all other similar countries). This (relative) growth of the IT sector has occurred both on the intensive (through the growth of incumbent firms, see Section 4.3) and on the extensive margin (through the entry of new firms in the sector, both domestic and foreign). In Section 4.4.2 we find that since 2001, IT-using sectors in Romania have grown significantly faster compared to non-IT using sectors in Romania and compared to the same relative growth in similar countries. This pattern provides support to the conjecture that improvements in the quality-adjusted prices and variety of IT services benefit more sectors relying more heavily on these services.

As to the magnitude of these relative growth rates, several pieces of evidence lend them credibility. First, while the estimated magnitude of the relative growth rate of the IT sector in Romania might seem impressive, this magnitude is lower than the actual growth of the IT sector in Romania. For example, our SCM estimate for 2015 for the number of workers is 1.83, i.e., the number of workers in IT grew 1.83 times faster between 2000 and 2015 compared to the number of workers in the rest of the Romanian economy and compared to the same relative growth in synthetic Romania. In the raw data, the employment in IT in Romania grew six times, from 13,691 workers in 2000 to 81,780 workers in 2015.[57] Hence, our estimates attenuate the actual growth of the IT sector in Romania after 2001, as they control for broader trends in the rest of the Romanian economy and in similar economies. Second, as in the case of the firm-level results, we find stronger increases in revenue in the IT sector than in employment. This points to a consistency between our sector-level and firm-level evidence. Third, the relative growth of downstream sectors with stronger links to the IT sector is not as large as the relative growth of the IT sector itself. As one would expect, the sector receiving the tax incentive directly grows faster than downstream sectors benefiting from the incentive indirectly, through cheaper or more diverse inputs.[58]

It is important to emphasize that there are features of either the available data or of the empirical strategies that do not recommend a direct comparison of the sector-level and the firm-level evidence. First, due to data constraints, the sector-level and firm-level growth rates in IT are measured relative to different comparison groups.[59] The choice of the reference

[57] The gross revenue grew 14 times (from 282 million euros in 2000 to 4,031 million euros in 2015), the number of firms grew four times (from 3,174 firms in 2000 to 12,432 firms in 2015).

[58] Figure C.12 (C.2.3) confirms the same intuition: the IT sector itself is the one becoming more attractive for FDI, followed by downstream sectors using IT services intensively.

[59] The sector-level cross-country data is at the two-digit level, with frequent missing values in the two-digits containing the three-digit codes used for comparison in the firm-level analysis. Hence, our baseline sector-level estimates are with respect to the rest of the economy, whereas the firm-level estimates are with respect to firms in certain three-digit sector codes.

group can obviously affect the exact magnitude of the estimates. Second, the sector-level evidence also allows for growth through firm entry, whereas the firm-level evidence characterizes incumbent firms alone.

Finally, the time frame of analysis is also important for the magnitudes and their interpretation. We conduct each firm-level exercise in a relatively narrow time window around the two policy of interest: 1999 to 2005 for the 2001 introduction of the income tax break, and 2011 to 2015 for the 2013 amendment to the tax break. To improve identification, the firm-level analysis, therefore, uses short-term variation in the tax conditions of firms in the IT sector in Romania. In the long term, there are general equilibrium effects (such as those on the supply of programmers) or unrelated shocks (such as the global financial crisis) that would have hindered the interpretation of long-term firm-level growth estimates.

By virtue of their long time frame (1999 to 2015), the sector-level cross-country findings are likely to capture not only the direct effects of the initial introduction of the policy in 2001, but also those of other developments in Romania and abroad that differentially affect the IT sector of Romania since 2001. For instance, to the extent that in the early 2000s the development of the IT sector in Central and Eastern Europe was at the cusp of multiple potential equilibria, this policy is likely to have acted as a timely signal to both local and foreign firms. In a world with first-mover advantage and path dependence, this policy is likely to have tilted the balance towards an equilibrium favorable to the IT sector of Romania. In this light, it is likely that a sizable part of the subsequent growth of the IT sector is due to a snowballing effect of the signal of the policy, as opposed to the size of the actual incentive it provides.

In a similar vein, the exact magnitude of the effects on IT-using sectors captures more than the indirect incentive granted by this tax break. It also reflects the idea that in an economy with coordination failures – due to economies of scale and imperfect tradability of services (such as IT) associated to skill-intensive manufacturing – government policy can move the economy towards the "high-wage, high-tech equilibrium" (Rodrik, 1996). Moreover, it suggests the possibility of strong complementarities between this IP and the FDI attraction and trade opportunities that followed Romania's joining of the EU in 2007.[60] While Romania's comparison countries in the SCM also joined the EU in 2004 or 2007, only Romania saw such a distinctive growth in the exports of IT-intensive downstream sectors.

To conclude, while it is outside the scope of this paper to disentangle the direct effects of this income tax break and those circumstances that may have amplified or dampened its effects in the aggregate and in the long-run, it is reassuring that the sector-level cross-country evidence and the firm-level evidence paint an overall consistent picture by which the income tax break has been effective in its objective to boost the development of the IT sector in Romania.

[60] Topalova and Khandelwal (2011) also provides evidence "that there may be strong complementarity among different industrial policies." In their case, "the impact of trade reforms appears to be magnified as FDI was allowed or restrictive licensing requirements were removed."

4.5 Back-of-the-Envelope Cost Estimates of the Policy

One important policy concern is the cost of this policy to the government, given that the IT workers who benefit from this income tax exemption are among the highest-paid workers in Romania. Another concern might be that our findings of large and persistent firm- and sector-level growth after the introduction and extension of the income tax break, may be disproportionate compared to the incentive offered by this tax break. To alleviate these concerns, we estimate the cost of this policy to the government, or put differently, the implicit incentive to the IT sector. Under the assumption that the administrative costs of this policy are negligible (both to firms and to the government), we define this cost as the amount foregone in tax revenues due to the tax exemption. We estimate this amount for 2015, the last year of our firm-level analysis. In 2015, these foregone tax receipts refer to the flat personal income tax of 16% owed on the taxable income of all exempted employees.

Assumptions on the wages and employment of the exempted employees are necessary. If the tax exemption were to be unexpectedly removed at the beginning of 2015, we assume that exempted employees would retain full-time employment. Then, in our first scenario we assume that in the short term, gross wages would remain unchanged. Therefore, our first estimate is based on the actual average wages of the exempted employees. In the absence of the policy, the currently exempted employees may also move to other ICT services sectors or other high-tech knowledge-intensive sectors. Our second and third estimates are based on the average wages in those two types of sectors, respectively. Table 4.4 shows our estimates for these three scenarios.

Overall, the estimates show that the tax receipts foregone in 2015 due to the policy vary between 62 and 80 million euros. To put these numbers into perspective, they represent between 4.7 and 6.4% of the total gross wage bill in the IT sector, which suggests that the income tax break is likely to represent an important incentive to the sector. Note that these shares are computed out from the total wage bill of the sector (which includes the wage bill of firms with little to no exemption of their employees). For firms with a large share of employees exempt from the income tax, the share between the “forgiven" tax bill and their total wage bill is significantly larger. In terms of the value added of the sector, the foregone tax revenues are in the range of 2.8 and 3.8% of the value added. These estimates suggest that the income tax exemption does not only signal a commitment of the Romanian government to the development of the IT sector but is also a sizable incentive. This incentive is also likely to act as an indirect incentive to other sectors, particularly those sectors that rely heavily on IT services.

4.6 Conclusion

This paper examines the effects of a unique industrial policy (IP) that was introduced by Romania in 2001: a personal income tax break for workers with specific IT-relevant bachelor's degrees and who work directly on software development for a firm with an eligible IT sector code. In 2013, the law of the tax break was amended to allow for a significantly larger list of eligible sector codes for firms and eligible bachelor's degrees for workers. We exploit both policy episodes to bring plausibly-causal estimates of the firm and sector-level effects of this policy. Across various empirical strategies and measures of firm size, we bring evidence of a strong and lasting growth of IT firms in Romania. This is in line with a shared economic incidence of the tax incentive between firms and workers. Sector-level cross-country evidence makes the additional point that the growth after 2001 of the IT sector in Romania is unlikely to be driven by factors unrelated to this IP, as similar neighboring countries fail to show comparable growth. Moreover, in Romania, sectors relying relatively more on IT inputs also grew faster.

Our results suggest that this policy has been effective in its goal to support the development of the IT sector and to reallocate resources (such as high-skill labor) towards this sector. This is an important achievement, because many IPs only allow specific groups to extract rents, without actually affecting resource allocations. Moreover, this shift was made towards a high-skill/high-wage sector, a policy priority in both developed and developing countries.

Establishing whether this policy was also efficient is beyond the scope of this project, but a fruitful area for future research. A number of special features of this policy motivate such additional research. First, this policy was not designed to rescue a "sunset industry" (as has been the case in several East Asian IPs). On the contrary, it targeted an industry under-developed in Romania at the time, but generally seen as crucial for growth. Second, the effects on IT-using downstream sectors suggest that this policy may have also mitigated (inter-industry) coordination failures coming from scale economies and imperfectly-tradable services (here, IT services) useful for skill-intensive downstream sectors (as in Rodrik, 2004). Last, this policy involved reductions in labor taxes, as opposed to the vastly more common reductions in corporate taxes or state aid packages. With worker-level data, one could study the distributional implications of this policy design relative to the common designs.

Figures

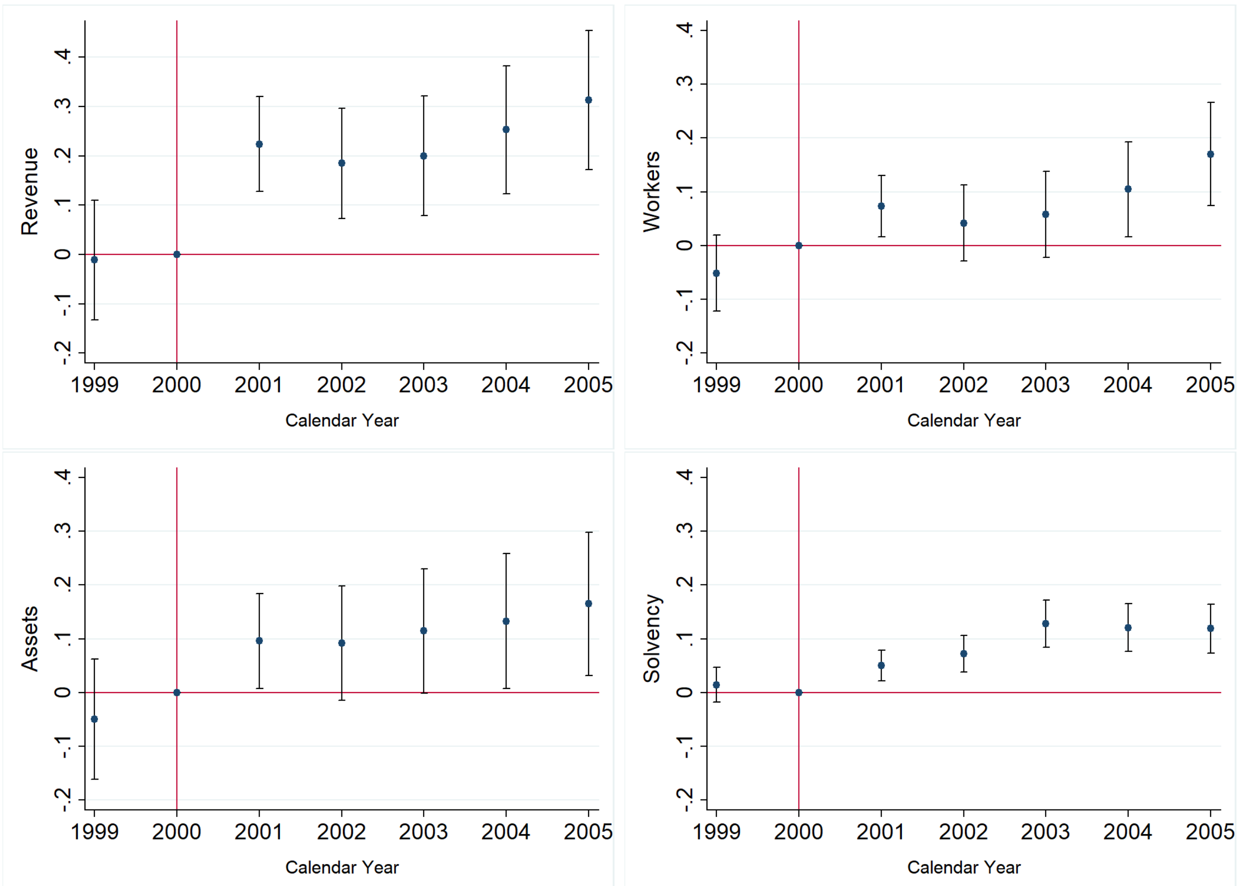

Figure 4.1: DiD Estimates of the Effects of the 2001 Income Tax Break

Notes: In this exercise, we study the firm-level impact of the introduction of the 2001 income tax break to workers in IT. Figure 4.1 plots the baseline estimates of the yearly DiD coefficients from Equation (4.1), $\beta_{DiD,t}$, together with their 95% confidence intervals. We consider four firm-level outcome variables: log(operating revenue), log(number of workers), log(total assets), and the solvency ratio. The coefficients for the year 2000, the year prior to the introduction of the tax break, are normalized to zero. Treated firms are those in the NACE Rev 1 sector 722 (Software consultancy and supply). Firms join the baseline comparison group if their NACE Rev 1 sector code is either 721 (Hardware consultancy), 723 (Data processing), 724 (Database activities), 725 (Maintenance and repair of office, accounting and computing machinery), 726 (Other computer related activities), 731 (Research and experimental development on natural sciences and engineering), or 732 (Research and experimental development on social sciences and humanities). The data source is Amadeus, a commercial database from Bureau Van Dijk. The regression model includes firm and calendar year fixed effects. See Table 4.1 for more details.

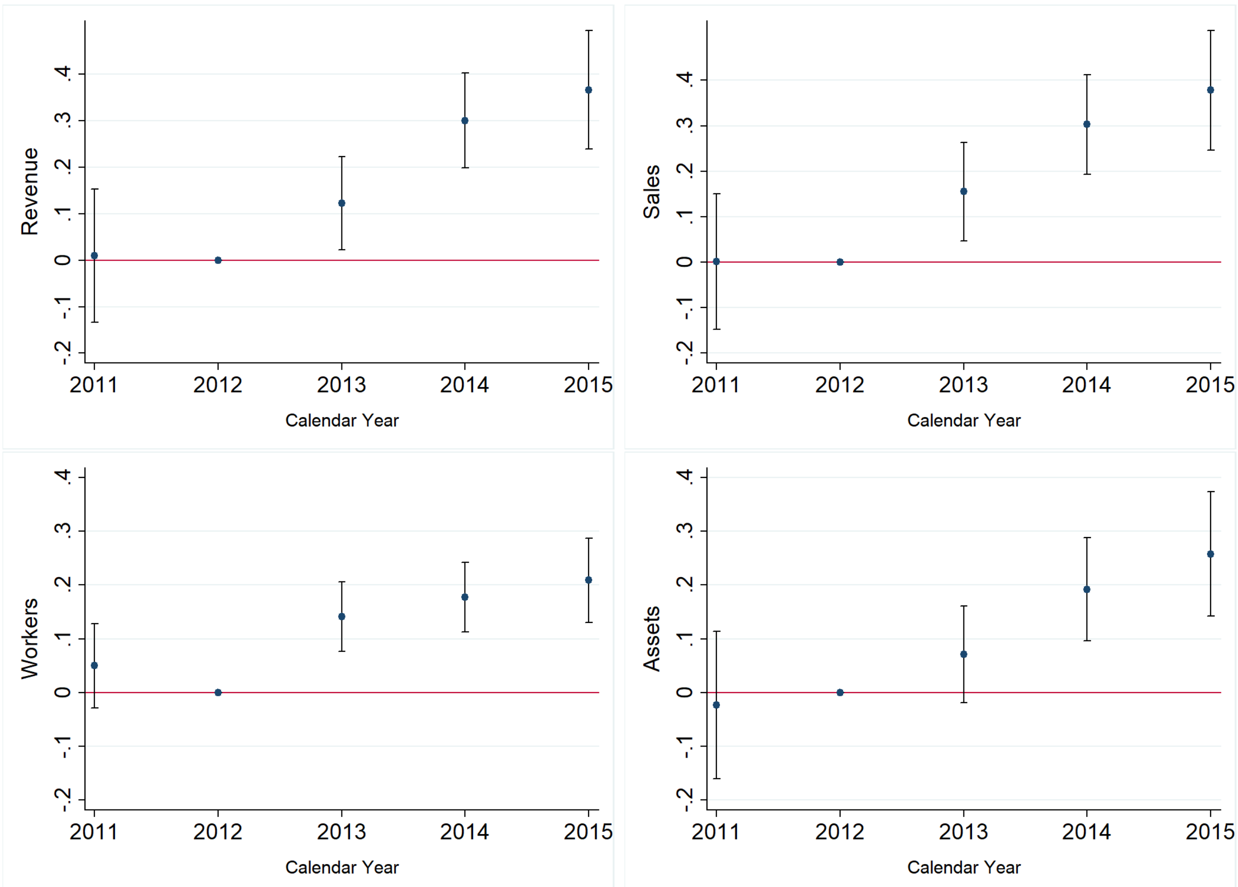

Figure 4.2: DiD Estimates of the Effects of the 2013 Reform

Notes: In this exercise, we study the firm-level impact of the introduction of the 2013 reform to the 2001 tax break. Figure 4.2 plots the baseline estimates of the yearly DiD coefficients from Equation (4.2), $\beta_{DiD,t}$, together with their 95% confidence intervals. We consider four firm-level outcome variables: log(revenue), log(sales), log(number of workers), and log(total assets). The coefficients for the year 2012, the year prior to the 2013 reform, are normalized to zero. Treated firms are those whose share of income tax exempted workers jumps from under 5% to over 20% after 2013. This exercise builds on administrative tax data collected by the Ministry of Finance. The regression model includes firm and sector-by-year fixed effects. See columns (1)-(4) from Table 4.2 for more details.

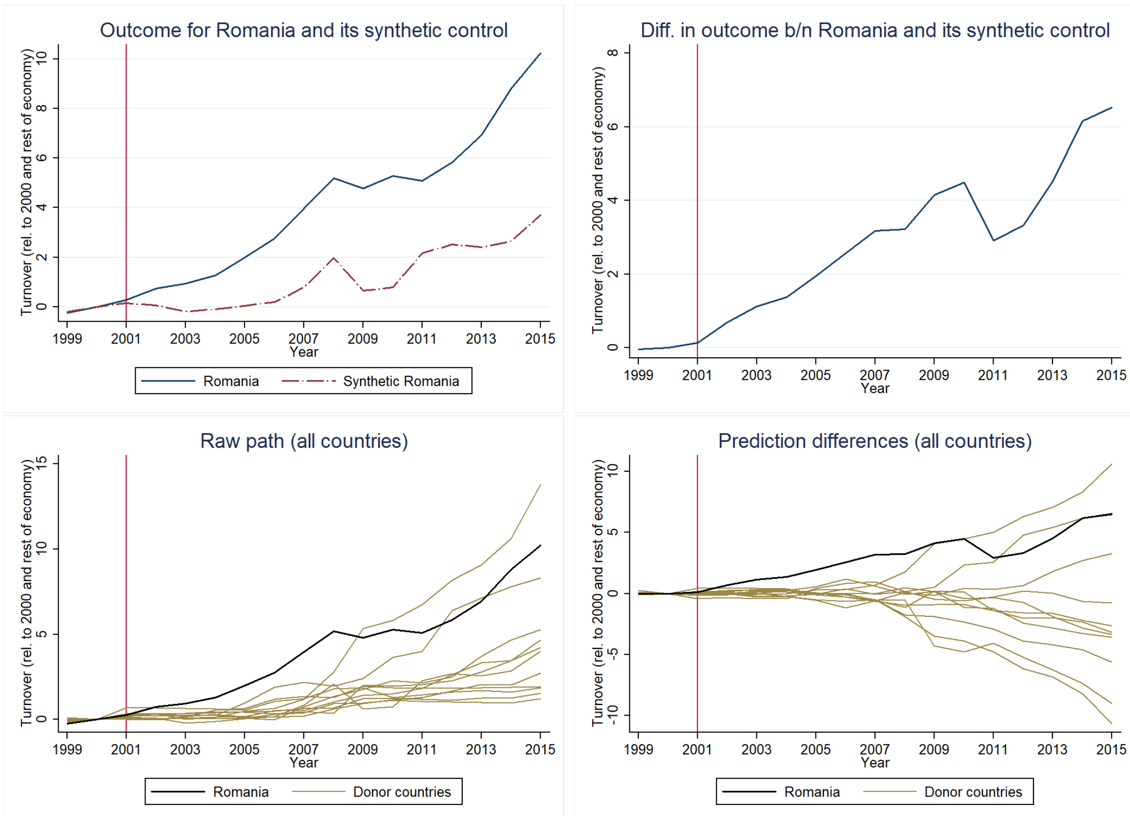

Figure 4.3: IT Sector Vs. Rest of the Economy. SCM with Outcome Variable: Gross Revenue (“Turnover or Gross Premiums Written") - Million Euro (Normalized)

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.1 to study the sector-level direct effects of the introduction of the 2001 law granting an income tax break to workers in IT. All figures have as dependent variable the country-level (normalized) gross revenue (“Turnover or gross premiums written - million euro"). The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sector is K72 (“Computer and related activities," including “Software consultancy and supply " and “Publishing of software"). We use as comparison sectors all other sectors in the economy (all except K72). The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the “GDP per capita (constant LCU)," “Medium and high-tech industry (% manufacturing value added)" and “Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

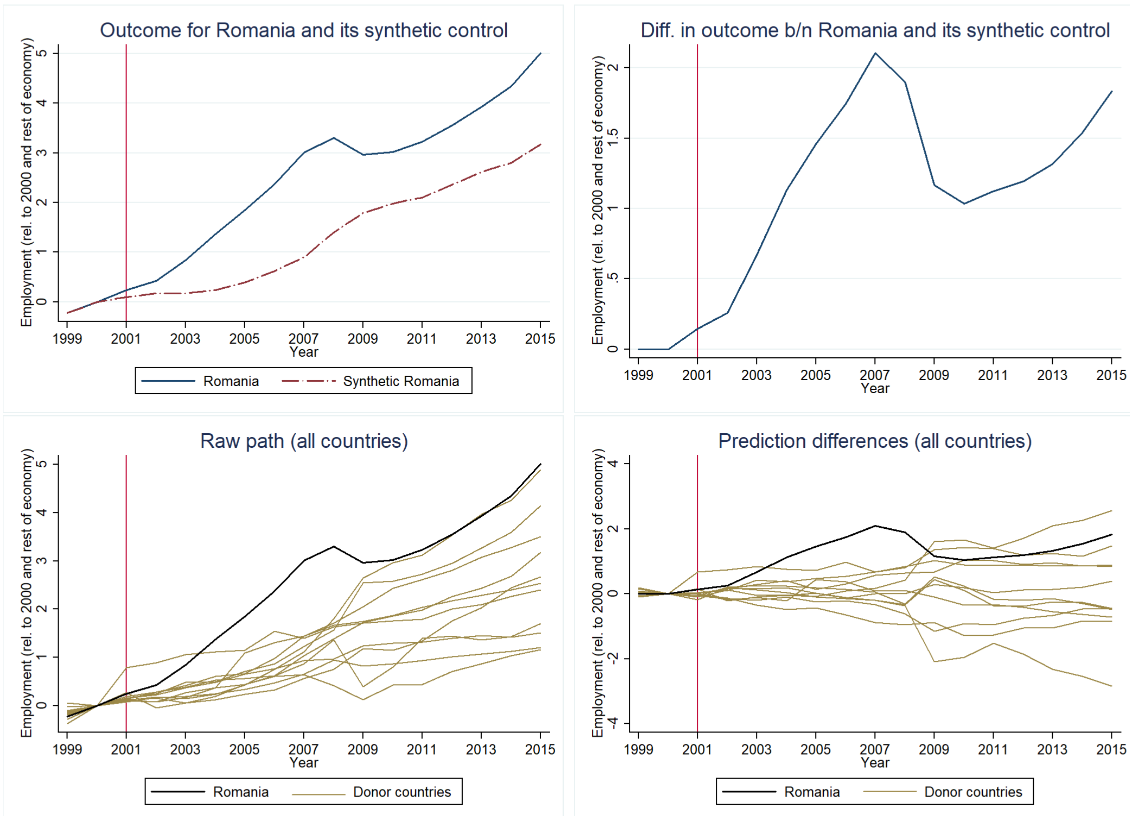

Figure 4.4: IT Sector Vs. Rest of the Economy. SCM with Outcome Variable: "Employees - Number" (Normalized)

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.1 to study the sector-level direct effects of the introduction of the 2001 law granting an income tax break to workers in IT. All figures have as dependent variable the country-level (normalized) "Employees - number" The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sector is K72 ("Computer and related activities," including "Software consultancy and supply " and "Publishing of software"). We use as comparison sectors all other sectors in the economy (all except K72). The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the "GDP per capita (constant LCU)," "Medium and high-tech industry (% manufacturing value added)" and "Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

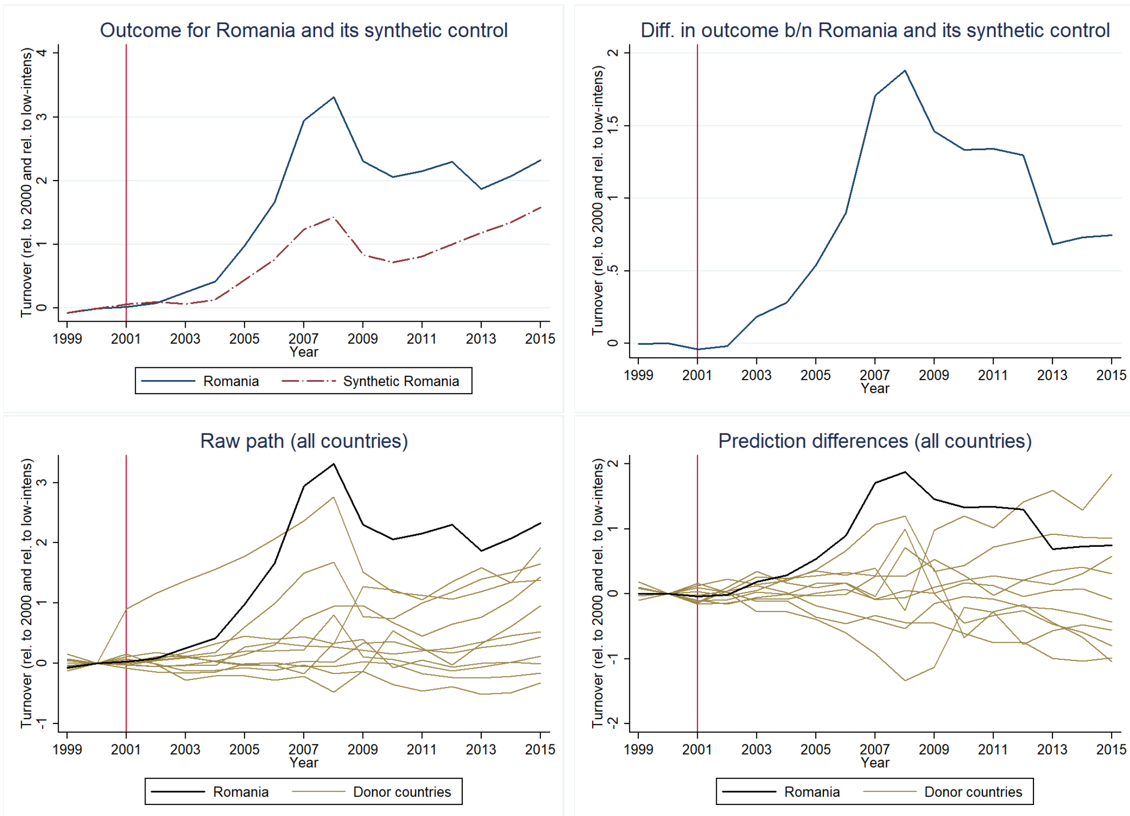

Figure 4.5: IT-Using Sectors Vs. Non-IT Using Sectors. SCM with Outcome Variable: Gross Revenue ("Turnover or Gross Premiums Written") - Million Euro (Normalized)

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.2 to study the sector-level downstream effects of the introduction of the 2001 law granting an income tax break to workers in IT. All figures have as dependent variable the country-level (normalized) gross revenue ("Turnover or gross premiums written - million euro"). The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sectors are those that use K72 ("Computer and related activities," including "Software consultancy and supply" and "Publishing of software") services at high-intensity. We exclude K72 itself from this category. Sectors that have a low-intensity of use of K72 services serve as comparison sectors. The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the "GDP per capita (constant LCU)," "Medium and high-tech industry (% manufacturing value added)" and "Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

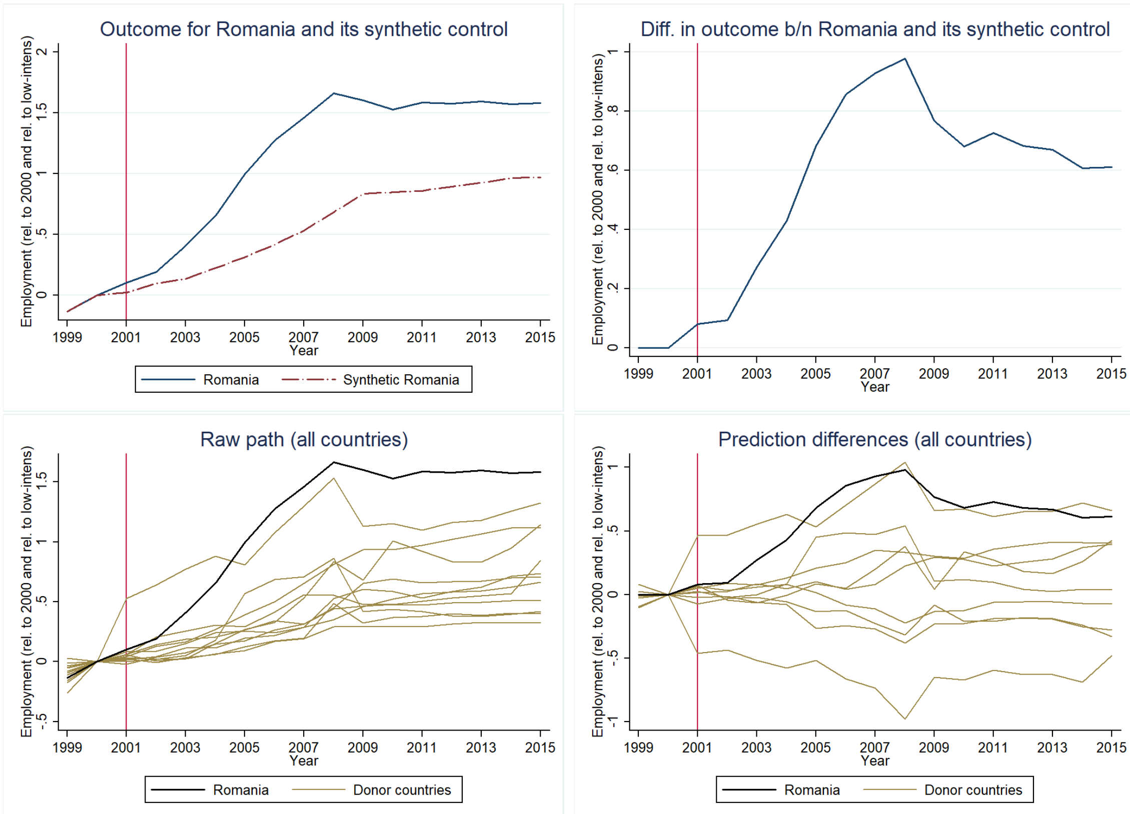

Figure 4.6: IT-Using Sectors Vs. Non-IT Using Sectors. SCM with Outcome Variable: “Employees - Number" (Normalized)

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.2 to study the sector-level downstream effects of the introduction of the 2001 law granting an income tax break to workers in IT. All figures have as dependent variable the country-level (normalized) “Employees - number." The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sectors are those that use K72 (“Computer and related activities," including “Software consultancy and supply" and “Publishing of software") services at high-intensity. We exclude K72 itself from this category. Sectors that have a low-intensity of use of K72 services serve as comparison sectors. The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the “GDP per capita (constant LCU)," “Medium and high-tech industry (% manufacturing value added)" and “Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

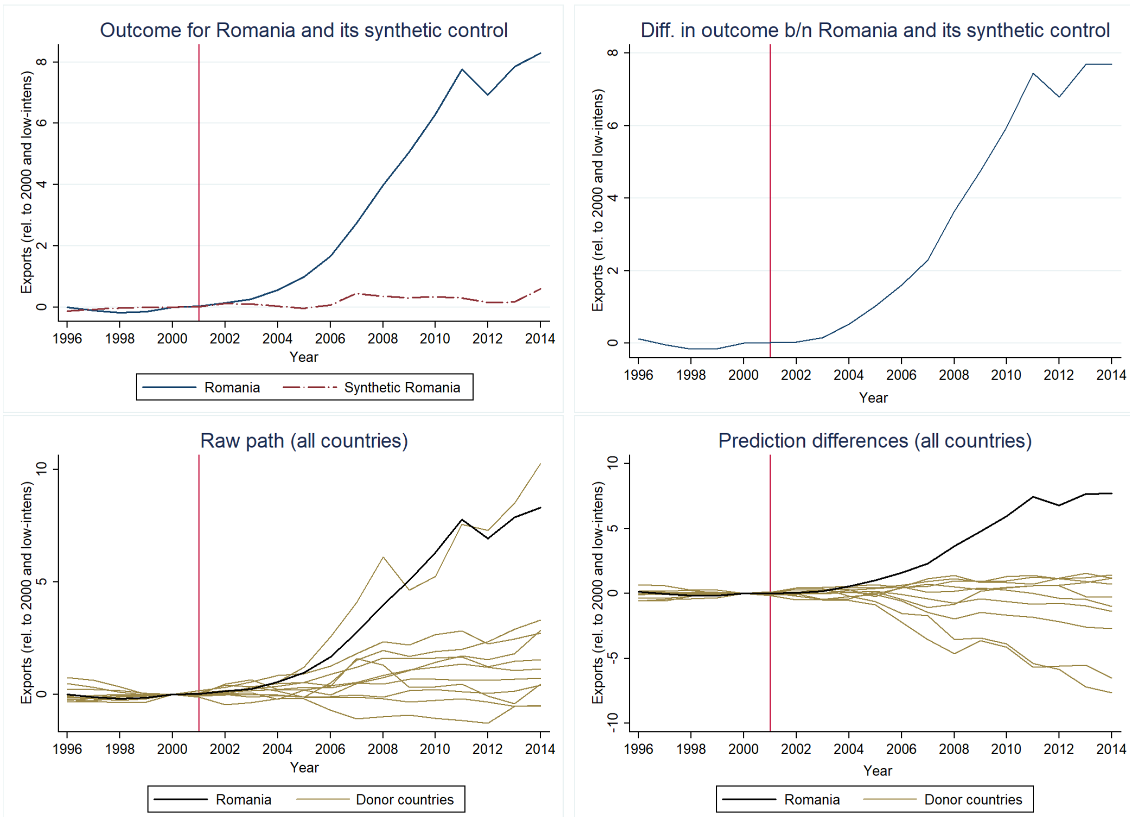

Figure 4.7: IT-Using Sectors Vs. Non-IT Using Sectors. SCM with Outcome Variable: “Goods Export Value" (Normalized)

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.2 to study the sector-level downstream effects of the introduction of the 2001 law granting an income tax break to workers in IT. All figures have as dependent variable the country-level (normalized) “(Goods Export) Trade Value (US$)." The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sectors are those that use K72 (“Computer and related activities," including “Software consultancy and supply" and “Publishing of software") services at high-intensity. K72 itself is excluded from this category. Sectors that have a low-intensity of use of K72 services serve as comparison sectors. The data source for the dependent variable is UN Comtrade, Goods Exports. Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the “GDP per capita (constant LCU)," “Medium and high-tech industry (% manufacturing value added)" and “Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

Tables

	Revenue (1)	Workers (2)	Assets (3)	Solvency (4)
Yearly Regression				
$\beta_{DiD,1999}$	-0.011	-0.051	-0.049	0.014
	(0.062)	(0.036)	(0.057)	(0.017)
$\beta_{DiD,2001}$	0.224***	0.073**	0.096**	0.050***
	(0.049)	(0.029)	(0.045)	(0.015)
$\beta_{DiD,2002}$	0.185***	0.042	0.092*	0.072***
	(0.057)	(0.036)	(0.054)	(0.018)
$\beta_{DiD,2003}$	0.200***	0.058	0.115*	0.128***
	(0.062)	(0.041)	(0.059)	(0.022)
$\beta_{DiD,2004}$	0.253***	0.105**	0.133**	0.121***
	(0.066)	(0.045)	(0.064)	(0.023)
$\beta_{DiD,2005}$	0.313***	0.170***	0.165**	0.119***
	(0.072)	(0.049)	(0.068)	(0.023)
R^2	0.868	0.878	0.882	0.582
Pooled Regression				
β_{DiD}	0.312***	0.106***	0.221***	0.093***
	(0.054)	(0.034)	(0.052)	(0.016)
R^2	0.792	0.877	0.796	0.561
# Observations	10,534	10,576	10,401	10,101
# Firms	1,622	1,622	1,614	1,604

Table 4.1: Difference-in-Difference Around 2001 Income Tax Break: Baseline Results

Notes: In this exercise we study the firm-level impact of the introduction of the 2001 law granting an income tax break to workers in IT. The data source is Amadeus, a commercial database from Bureau Van Dijk. In this table we report the baseline point estimates of the difference-in-difference coefficients of interest from the Equation (4.1) (upper panel) and from the pooled version of Equation (4.1) (lower panel), i.e., $\beta_{DiD,t}$ and β_{DiD} respectively. The outcome variables used are log(operating revenue), log(number of workers), log(total assets), and the solvency ratio. The year prior to the introduction of the law (2000) is the reference year. Treated firms are those in the NACE Rev 1 sector 722. Firms in the baseline comparison group are those whose NACE Rev 1 sector code is either 721, 723 , 724, 725, 726, 731, or 732. All specifications include firm and calendar year fixed effects. The number of observations and firms is the same for the yearly and pooled versions of the same regression. Robust standard errors, clustered at the firm-level, in parenthesis. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Revenue (1)	Sales (2)	Workers (3)	Assets (4)	Revenue (5)	Sales (6)	Workers (7)	Assets (8)
Yearly Regression								
$\beta_{DiD,2011}$	0.010	0.001	0.050	-0.023	0.000	0.000	0.000	0.000
	(0.073)	(0.076)	(0.040)	(0.070)	(.)	(.)	(.)	(.)
$\beta_{DiD,2013}$	0.122**	0.155***	0.141***	0.071	0.021	0.063	0.084**	0.016
	(0.051)	(0.055)	(0.033)	(0.046)	(0.052)	(0.057)	(0.037)	(0.050)
$\beta_{DiD,2014}$	0.300***	0.303***	0.177***	0.192***	0.164***	0.183***	0.107***	0.093*
	(0.052)	(0.056)	(0.033)	(0.049)	(0.050)	(0.055)	(0.036)	(0.051)
$\beta_{DiD,2015}$	0.366***	0.378***	0.209***	0.258***	0.221***	0.231***	0.127***	0.133**
	(0.065)	(0.067)	(0.040)	(0.059)	(0.058)	(0.062)	(0.041)	(0.057)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Rel. prod.					1.593***	1.278***	0.218**	1.765***
					(0.225)	(0.197)	(0.085)	(0.132)
Small					0.346***	0.344***	0.373***	0.223***
					(0.048)	(0.047)	(0.037)	(0.038)
Medium					0.631***	0.662***	0.752***	0.634***
					(0.118)	(0.106)	(0.116)	(0.117)
Large					0.969**	0.993**	1.089**	1.034**
					(0.394)	(0.386)	(0.466)	(0.406)
Young					-0.037	-0.037	-0.001	-0.041**
					(0.023)	(0.023)	(0.014)	(0.020)
Adjusted R^2	0.869	0.874	0.907	0.901	0.906	0.909	0.924	0.931
Pooled Regression								
β_{DiD}	0.253***	0.273***	0.153***	0.179***	0.131***	0.155***	0.105***	0.081*
	(0.044)	(0.046)	(0.025)	(0.040)	(0.046)	(0.051)	(0.034)	(0.047)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.868	0.874	0.907	0.900	0.906	0.909	0.924	0.930
# Observations	22,592	22,212	22,598	22,587	16,820	16,536	16,824	16,813
# Firms	5,177	5,128	5,177	5,177	4,864	4,804	4,865	4,863

Table 4.2: Difference-in-Differences Design Around 2013 Reform: Baseline Results

Notes: In this exercise we study the firm-level impact of the 2013 expansion to the income tax break law of 2001. Here, we report the baseline point estimates of the difference-in-difference coefficients of interest from the Equation (4.2) (upper panel) and from the pooled version of Equation (4.2) (lower panel), i.e., $\beta_{DiD,t}$ and β_{DiD} respectively. The outcome variables used are log(revenue), log(sales), log(number of workers), and log(total assets). The year prior to the amendment of 2013 (2012) is the reference year. The only difference between columns (1)-(4) and columns (5)-(8) is that the latter include firm-specific time-variant controls in addition to firm fixed effects, whereas the former include firm fixed effects alone. All specifications include firm and sector-by-year fixed effects. Heteroskedasticity robust errors in parentheses. The reference category for the firm size is “Micro" and for “Young" firms the reference category are firms “Older than five years." ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Revenue (1)	Sales (2)	Workers (3)	Assets (4)	Revenue (5)	Sales (6)	Workers (7)	Assets (8)
Panel A: Size	*Micro: < 10 workers*				*Small, medium, large: ≥ 10 workers*			
β_{DiD}	0.248***	0.276***	0.161***	0.173***	0.283***	0.264***	0.144***	0.235***
	(0.051)	(0.053)	(0.028)	(0.046)	(0.078)	(0.080)	(0.055)	(0.083)
Adjusted R^2	0.790	0.795	0.790	0.853	0.944	0.942	0.945	0.960
# Observations	19,664	19,310	19,670	19,659	2,928	2,902	2,928	2,928
# Firms	4,451	4,410	4,451	4,451	726	718	726	726
Panel B: Age	*Young: < 5 years old*				*Old: ≥ 5 years old*			
β_{DiD}	0.341***	0.389***	0.229***	0.198***	0.119**	0.113**	0.055*	0.104**
	(0.072)	(0.074)	(0.038)	(0.064)	(0.047)	(0.050)	(0.031)	(0.045)
Adjusted R^2	0.807	0.824	0.862	0.843	0.911	0.910	0.934	0.939
# Observations	10,148	9,972	10,152	10,144	12,444	12,240	12,446	12,443
# Firms	2,410	2,391	2,410	2,410	2,767	2,737	2,767	2,767

Table 4.3: Difference-in-Differences Design Around 2013 Reform: Heterogeneity of the Baseline Results

Notes: In this exercise we explore the heterogeneity of the baseline effects of the 2013 expansion to the income tax break law of 2001. The outcome variables used are log(revenue), log(sales), log(number of workers), and log(total assets). For brevity, we implement the pooled version of the DiD Equation (4.2) on the baseline sample of 5,177 firms (see Table 4.2). The sample is split in two parts based on the number of workers or age of the firm (both in 2011). The DiD regressions are run separately on each part of the baseline sample. All specifications include firm and sector-by-year fixed effects. Heteroskedasticity robust errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Scenario	(1)	(2)	(3)
Equivalent Subsidy (Mil. RON)	355.55	262.50	273.62
Equivalent Subsidy (Mil. Euro)	79.98	59.05	61.55
% Value Added	3.84%	2.84%	2.96%
% Revenue	1.92%	1.64%	1.48%
% Production	2.02%	1.82%	1.55%
% Wage Bill	6.39%	4.72%	4.92%

Table 4.4: Back-of-the-Envelope Calculation of Foregone Tax Revenues in 2015

Notes: Data sources: Administrative data from the National Agency of Fiscal Administration of Romania (*Agenţia Naţională de Administrare Fiscală*) and Structural Business Statistics from Eurostat. In all three scenarios the employees exempted in 2015 are assumed to maintain full employment were the exemption to be removed at the beginning of that year. Scenario (1) for the back-of-the-envelope calculation of foregone tax revenues from the income tax break is one in which the wages of exempted employees remain unchanged. Scenario (2) is one in which exempted employees are paid the average wage in non-eligible ICT services sectors. Scenario (3) is one in which exempted employees are paid the average wage in non-eligible high-tech knowledge-intensive sectors.

Chapter 5

Dissertation Conclusion

This dissertation is comprised of three chapters entitled “The Effects of Joining Multinational Supply Chains: New Evidence from Firm-to-Firm Linkages", “The Effects of Multinationals on Workers: Evidence from Costa Rica" and “Industrial Policy at Work: Evidence from Romania’s Income Tax Break for Workers in IT." These chapters studied policies taken by governments in developing countries to take advantage of the opportunities of globalization.

In the first two chapters of my dissertation, I quantified two types of effects of policies to attract affiliates of multinational corporations (MNCs) to one’s country: those on firms who become suppliers to these MNCs and those on workers (both directly employed by MNC affiliates and employed by domestic firms). In the third and last chapter of my dissertation, I estimated the effects of an industrial policy from Romania that is meant to boost the IT industry – an industry with an unexplored potential in Romania and with a forecasted increase in global demand.

Each chapter of the dissertation took stock of the particular takeaways from that chapter’s work and the future work that is welcomed. To avoid redundancy, I invite the interested chapter to the conclusion of each chapter. In this dissertation conclusion, I take the opportunity to comment on the broader takeaways from the three chapters combined.

The first two chapters of my dissertation have shown strong and long-lasting positive effects of MNC affiliates on domestic supplying firms and the domestic labor market. Nonetheless, one might wonder whether a similarly-performing domestic firm could trigger the same effects. In the context of Costa Rica (and of most developing economies), one cannot answer this question as there are too few (if any) similarly-performing domestic firms. However, if the answer to the question were positive, then governments might find it more cost-effective to incentivize the development of high-performing domestic firms.

The third chapter of my dissertation studies an industrial policy introduced by the Romanian government to encourage the development of the information technology (IT) industry in the country. IT is an industry that was both perceived to create *“good jobs"* for high-skilled workers and to trigger positive externalities on industries relying more heavily on IT services. The evidence on the positive effects of this policy is reassuring as to the ability of governments in developing countries to design effective industrial policies. Future work would need to compare the overall welfare effects of similar industrial policies to those of policies that target the attraction of foreign MNC affiliates.

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Appendix A

Appendix for “The Effects of Joining MNC Supply Chains"

A.1 Additional Evidence

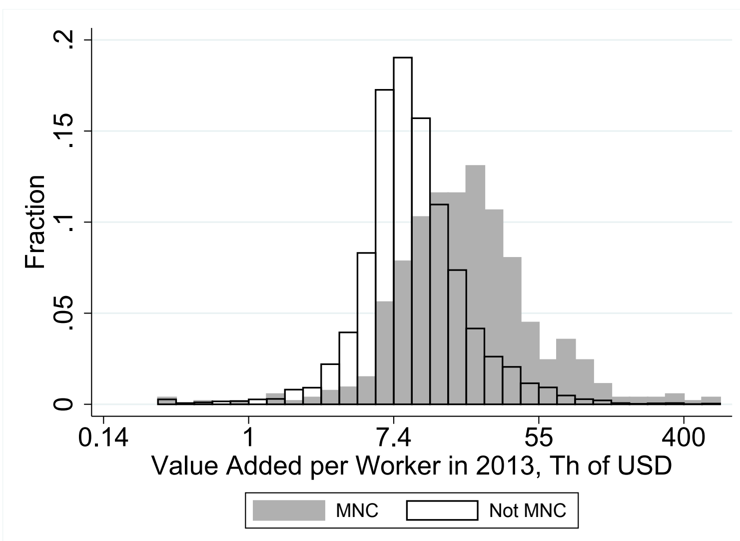


Figure A.1: Distributions of Value Adder Per Worker for MNCs vs Non-MNCs in Costa Rica

Notes: Figure A.1 plots two histograms of the value added per worker (in 2013, in thousands of U.S. dollars) for two types of firms in Costa Rica: all MNC affiliates and all firms that are not MNC affiliates. Both histograms contain only firms that hire more than ten workers that year.

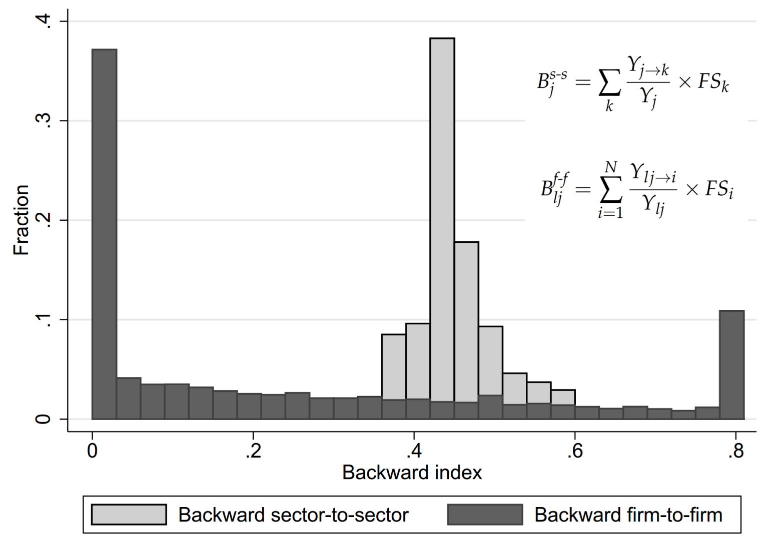


Figure A.2: Histograms of Two Firm-level Measures of Backward Linkages

Notes: Figure A.2 plots two measures of firm-level backward linkages. Firms are not weighted by their size; histograms are based on firm counts. The “Backward sector-to-sector" measure is the typical one used in the FDI spillovers literature; all firms in a given sector j are assigned the same value of the backward linkage measure, depending on the extent to which the sector j of the firm sells to a given sector k (from I-O table coefficients) and the share of foreign ownership in those sectors, FS_k (overall foreign share of sector k). “Backward firm-to-firm" uses the actual firm-to-firm transaction data, and in particular the exact amounts sold by firm l to buyer firm i and the actual share of foreign ownership of buyer i (FS_i). All linkage values above 0.8 are binned up at 0.8. When we run a regression over the entire sample of firms in Costa Rica of the firm-level “Backward firm-to-firm" measure on their sector-level “Backward sector-to-sector" measure, we obtain an R^2 of less than 1%.

Table A.1: Domestic Firms Improve Their Performance after Starting to Supply to MNCs

	VA (1)	Profits (2)	VA/L (3)	Profits/L (4)	Sales/L (5)	VA (6)	Profits (7)	VA/L (8)	Profits/L (9)	Sales/L (10)
4 years before event	0.010	-0.088*	0.022	-0.025	0.036*	-0.097	-0.205***	-0.016	0.000	0.033
	(0.038)	(0.052)	(0.024)	(0.042)	(0.020)	(0.066)	(0.071)	(0.040)	(0.062)	(0.027)
3 years before event	-0.001	0.001	0.004	0.037	0.032*	-0.060	-0.070	-0.029	0.054	0.028
	(0.031)	(0.037)	(0.023)	(0.031)	(0.018)	(0.042)	(0.047)	(0.030)	(0.039)	(0.022)
2 years before event	0.021	-0.029	0.016	-0.001	0.021	-0.021	-0.065**	-0.006	0.012	0.016
	(0.022)	(0.021)	(0.020)	(0.022)	(0.015)	(0.031)	(0.027)	(0.026)	(0.029)	(0.015)
Year of event	0.058***	0.058**	0.037**	0.084***	0.096***	0.109***	0.095***	0.061***	0.056**	0.103***
	(0.020)	(0.026)	(0.017)	(0.024)	(0.012)	(0.022)	(0.028)	(0.021)	(0.027)	(0.016)
1 year after event	0.215***	0.216***	0.011	0.096***	0.084***	0.307***	0.299***	0.056**	0.061*	0.091***
	(0.031)	(0.029)	(0.017)	(0.025)	(0.012)	(0.034)	(0.041)	(0.026)	(0.034)	(0.019)
2 years after event	0.261***	0.241***	0.020	0.108***	0.079***	0.365***	0.337***	0.071**	0.047	0.091***
	(0.035)	(0.030)	(0.019)	(0.032)	(0.012)	(0.050)	(0.054)	(0.031)	(0.042)	(0.026)
3 years after event	0.260***	0.230***	0.017	0.105***	0.070***	0.383***	0.355***	0.088**	0.026	0.076**
	(0.045)	(0.038)	(0.025)	(0.030)	(0.013)	(0.064)	(0.073)	(0.038)	(0.048)	(0.033)
4 years after event	0.254***	0.220***	0.025	0.090***	0.078***	0.393***	0.371***	0.110**	-0.011	0.087**
	(0.044)	(0.045)	(0.024)	(0.032)	(0.014)	(0.083)	(0.084)	(0.054)	(0.056)	(0.041)
Mean Dep. Var. (level)	0.18	0.26	0.017	0.024	0.081	0.22	0.45	0.015	0.031	0.10
SD Dep. Var. (level)	0.55	0.77	0.040	0.042	0.18	0.63	1.27	0.043	0.062	0.31
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Adjusted R^2	0.71	0.71	0.67	0.60	0.78	0.71	0.74	0.52	0.61	0.80
# Observations	110,857	110,857	110,857	110,857	116,683	23,130	23,130	23,130	23,130	23,961
# Fixed Effects	24,591	24,591	24,591	24,591	25,174	7,252	7,252	7,252	7,252	7,366
# Firms	17,552	17,552	17,552	17,552	18,035	3,447	3,447	3,447	3,447	3,482

Notes: Table A.1 shows the results of running the event-study specification (2.1) adapted to five dependent variables: log value added, log profits, log value added per worker, log profits per worker, and log sales per worker. The event is defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. These regressions do not include firm-level time-varying controls, x_{it}, but only firm and four-digit sector $\times$ province $\times$ calendar year fixed effects. Columns (1)-(5) pertain to the full sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC during our entire firm-to-firm transaction dataset. Clustering of standard errors is at the two-digit sector by province level. Columns (6)-(10) focus only on the restricted sample of domestic firms becoming first-time suppliers to an MNC between 2010 and 2015 and use standard error clustering at event by province level. Means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.2: Domestic Firms (Weakly) Reduce their Mark-ups after Starting to Supply to MNCs

Outcome: Mark-up	(1)	(2)
4 years before event	0.007	0.063*
	(0.032)	(0.036)
3 years before event	-0.007	0.027
	(0.017)	(0.026)
2 years before event	0.002	0.022
	(0.009)	(0.015)
Year of event	-0.008	-0.031*
	(0.015)	(0.017)
1 year after event	-0.018	-0.062**
	(0.012)	(0.024)
2 years after event	-0.022	-0.087***
	(0.015)	(0.029)
3 years after event	-0.029	-0.118***
	(0.020)	(0.034)
4 years after event	-0.034*	-0.143***
	(0.017)	(0.043)
Mean Dep. Var. (level)	1.25	1.26
SD Dep. Var. (level)	0.52	0.52
Firm FE	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes
Never Suppliers	Yes	No
Adjusted R^2	0.80	0.78
# Observations	50,062	10,803
# Fixed Effects	12,796	4,020
# Firms	8,658	1,868

Notes: Table A.2 shows the results of running the event-study specification (2.1) using firm-level mark-ups as the dependent variable. Mark-ups are estimated using the methodology of De Loecker and Warzynski (2012) for a value-added Cobb-Douglas production function. The event is defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. Column (1) reports event-study estimates for the sample including both domestic firms that become first-time suppliers to an MNC after 2010 and domestic firms never observed as supplying to an MNC during our entire firm-to-firm transaction data. Clustering of standard errors is at the two-digit sector by province level. Column (2) focuses only on the sample of domestic firms becoming first-time suppliers to an MNC after 2010 and use standard error clustering at event by province level. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

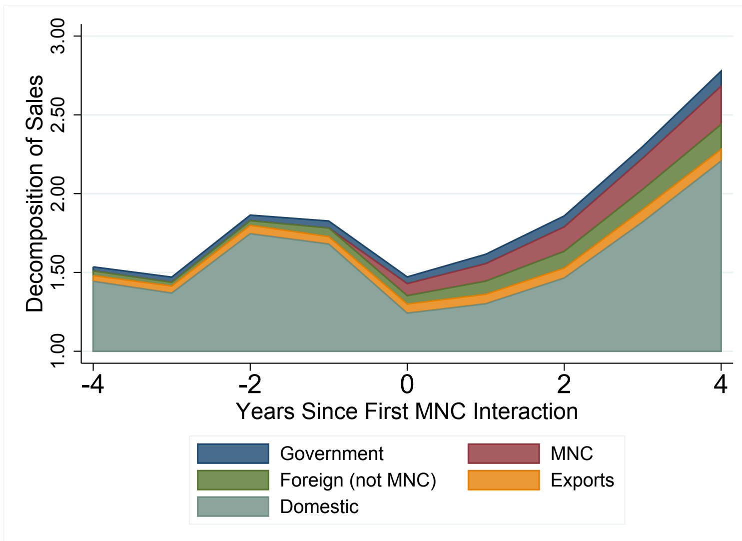


Figure A.3: Decomposition of Sales for First-time Suppliers to MNCs

Notes: Figure A.3 plots a decomposition of the sales of first-time suppliers to MNCs. The horizontal axis refers to event years and the vertical axis to total sales in millions of U.S. dollars (CPI-deflated to 2013 dollars). For each event-year, we calculate the average amount in each category of buyers across all suppliers. We exclude the top 1% largest transactions to avoid outliers driving these averages. We split transactions into five categories: sales to MNCs, sales to partially foreign-owned firms that are not MNCs, exports, sales to the government, and sales to domestically-owned firms. These averages are not demeaned through any fixed effect.

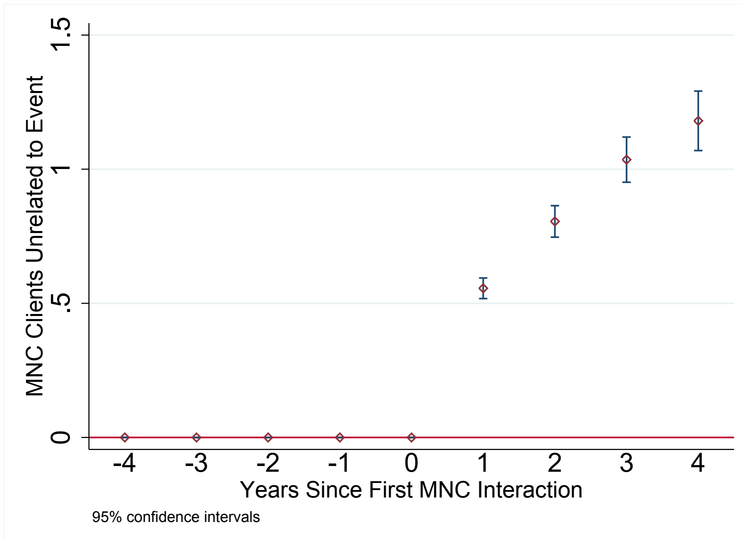


Figure A.4: Average Number of MNC Buyers, Other Than First MNC Buyer

Notes: Figure A.4 plots the average (across first-time suppliers to MNCs) number of MNC buyers in a given event year that are *different* from the initial MNC buyer triggering the event for each supplier. The horizontal axis refers to event years and the vertical axis to the average number of (other) MNC buyers. The vertical lines reflect the 95% confidence intervals. By construction, all averages for event years *-4* to *0* are zero.

Table A.3: After Starting to Supply to MNCs, Sales to Non-Corporate Buyers Increase, but Their Share in Overall Sales to Others Falls

	Sales to Others Non-Corp (1)	Sh. of Other Sales Non-Corp (2)	Sales to Others Non-Corp (3)	Sh. of Other Sales Non-Corp (4)
4 years before event	-0.005	-0.015	-0.146	-0.019
	(0.049)	(0.009)	(0.094)	(0.023)
3 years before event	-0.034	-0.020**	-0.065	-0.017
	(0.047)	(0.008)	(0.067)	(0.012)
2 years before event	-0.022	-0.013	-0.035	-0.006
	(0.037)	(0.009)	(0.039)	(0.010)
Year of event	-0.086**	-0.050***	-0.031	-0.051***
	(0.041)	(0.009)	(0.042)	(0.010)
1 year after event	0.129***	-0.073***	0.203***	-0.069***
	(0.039)	(0.012)	(0.056)	(0.013)
2 years after event	0.144***	-0.072***	0.254***	-0.062***
	(0.047)	(0.012)	(0.079)	(0.018)
3 years after event	0.101*	-0.075***	0.211*	-0.061**
	(0.060)	(0.012)	(0.106)	(0.023)
4 years after event	0.164***	-0.071***	0.317**	-0.051*
	(0.045)	(0.011)	(0.130)	(0.028)
Mean Dep. Var. (level)	0.68	0.74	1.01	0.55
SD Dep. Var. (level)	2.17	0.36	3.72	0.36
Firm FE	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	No	No
Adjusted R^2	0.70	0.74	0.71	0.63
# Observations	108,844	116,683	21,448	23,961
# Fixed Effects	24,420	25,174	6,991	7,366
# Firms	17,565	18,035	3,364	3,482

Notes: Table A.3 shows the results of running the event-study specification (2.1) adapted to two dependent variables: log total sales to all non-corporate buyers and the share of sales to non-corporate buyers out of all sales to others. The total sales to all non-corporate buyers are constructed starting from total sales in a given year (from corporate income tax returns data), from which we subtract all sales to (corporate) buyers (including the MNC triggering the event, from firm-to-firm transaction data). Total sales to all non-corporate buyers include all those sales to end consumers (general public) and firms that do not amount to 4,200 U.S. dollars in a given year. The share of non-corporate sales out of all sales to others is meant to capture potential reallocations of sales to others (sales excluding the MNC triggering the event) among buyers of different types. The event is defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. These regressions do not include firm-level time-varying controls, x_{it}, only the fixed effects reported in each column. Robust standard errors in parentheses. Means (in levels) for columns (1) and (3) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.4: Domestic Firms See Their Transactions Increase

Outcome: (log) Value of Transaction	(1)	(2)	(3)	(4)
4 years before event	0.007	0.007	0.003	-0.039*
	(0.018)	(0.015)	(0.015)	(0.020)
3 years before event	-0.003	0.011	0.011	-0.014
	(0.013)	(0.011)	(0.011)	(0.014)
2 years before event	0.002	0.009	0.009	0.004
	(0.009)	(0.007)	(0.007)	(0.010)
Year of event	-0.002	0.016**	0.018***	0.017*
	(0.009)	(0.007)	(0.007)	(0.009)
1 year after event	0.018	0.038***	0.040***	0.051***
	(0.012)	(0.010)	(0.010)	(0.013)
2 years after event	0.022	0.039***	0.039***	0.055***
	(0.015)	(0.013)	(0.013)	(0.017)
3 years after event	0.027	0.044***	0.047***	0.085***
	(0.020)	(0.016)	(0.017)	(0.022)
4 years after event	0.043*	0.046**	0.047**	0.089***
	(0.024)	(0.020)	(0.020)	(0.027)
Mean Dep. Var. (level)	0.031	0.036	0.036	0.035
SD Dep. Var. (level)	0.071	0.078	0.078	0.078
Supplier FE	Yes	No	No	No
Supplier-Buyer FE	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No
Year-Prov FE	No	No	Yes	No
Year-4DSect-Prov FE	No	No	No	Yes
Adjusted R^2	0.20	0.71	0.71	0.72
# Observations	412,420	305,005	305,005	304,400
# Fixed Effects	3,537	83,338	83,398	88,708
# Suppliers	3,527	3,382	3,382	3,341
# Buyers	99,111	44,951	44,951	44,917

Notes: Table A.4 shows the results of running the event-study specification (2.1) adapted to one dependent variable: log value of the transaction made by a given supplier - buyer pair, in a given year. The unit of observation is at the seller-buyer-year level. The event is defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. All four regressions have the same dependent variable, but differ in which fixed effects we activate (hence the variation that we exploit). To construct the dependent variable we use the firm-to-firm transaction data (from D-151 tax forms). These regressions do not include firm-level time-varying controls, x_{it}, only the fixed effects reported in each column. In years when there is no transaction between a given supplier-buyer pair, that triad is dropped. For brevity, the table only contains domestic firms that become first-time suppliers to an MNC (the restricted economy-wide sample). All means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.5: Domestic Firms Start Selling to (Buying from) More Sectors After Event

	# 2D-Sect Buyers (1)	# 4D-Sect Buyers (2)	# 2D-Sect Suppliers (3)	# 4D-Sect Suppliers (4)	# 2D-Sect Buyers (5)	# 4D-Sect Buyers (6)	# 2D-Sect Suppliers (7)	# 4D-Sect Suppliers (8)
log total sales	0.169***	0.191***	0.265***	0.308***	0.308***	0.352***	0.322***	0.366***
	(0.012)	(0.015)	(0.004)	(0.005)	(0.008)	(0.008)	(0.007)	(0.009)
4 years before event	-0.018	-0.019	-0.006	-0.010	-0.024	-0.033	0.023	0.010
	(0.014)	(0.016)	(0.020)	(0.022)	(0.038)	(0.037)	(0.026)	(0.027)
3 years before event	-0.007	-0.002	-0.015	-0.015	-0.011	-0.009	0.009	0.003
	(0.011)	(0.013)	(0.013)	(0.013)	(0.022)	(0.022)	(0.018)	(0.019)
2 years before event	-0.018	-0.014	-0.003	-0.007	-0.020	-0.017	0.011	0.003
	(0.014)	(0.014)	(0.010)	(0.011)	(0.016)	(0.016)	(0.013)	(0.013)
Year of event	-0.197***	-0.155***	0.023**	0.024**	-0.229***	-0.187***	0.005	0.004
	(0.014)	(0.013)	(0.010)	(0.010)	(0.017)	(0.019)	(0.009)	(0.009)
1 year after event	0.190***	0.218***	0.037***	0.040***	0.128***	0.157***	0.009	0.010
	(0.016)	(0.017)	(0.011)	(0.012)	(0.026)	(0.026)	(0.013)	(0.015)
2 years after event	0.226***	0.260***	0.052***	0.055***	0.146***	0.183***	0.011	0.015
	(0.018)	(0.019)	(0.014)	(0.014)	(0.032)	(0.032)	(0.017)	(0.020)
3 years after event	0.250***	0.285***	0.051***	0.055***	0.173***	0.213***	0.010	0.015
	(0.017)	(0.020)	(0.012)	(0.013)	(0.046)	(0.043)	(0.022)	(0.026)
4 years after event	0.250***	0.288***	0.063***	0.066***	0.174***	0.220***	0.023	0.030
	(0.020)	(0.023)	(0.015)	(0.016)	(0.052)	(0.052)	(0.027)	(0.032)
Mean Dep. Var. (level)	2.57	3.04	4.26	5.48	4.60	6.00	5.44	5.44
SD Dep. Var. (level)	2.66	3.94	3.43	5.50	4.06	6.45	4.62	4.62
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	Yes	No	No	No	No
Adjusted R^2	0.82	0.84	0.81	0.84	0.79	0.82	0.82	0.85
# Observations	115,800	115,800	115,800	115,800	23,092	23,092	23,092	23,092
# Fixed Effects	25,101	25,101	25,101	25,101	7,234	7,234	7,234	7,234
# Firms	17,996	17,996	17,996	17,996	3,442	3,442	3,442	3,442

Notes: Table A.5 shows the results of running the event-study specification (2.1) adapted to four dependent variables: the number of two-digit sectors of buyers in a given year, the number of four-digit sectors of buyers in a given year, the number of two-digit sectors of suppliers (of the supplier) in a given year, and the number of four-digit sectors of suppliers (of the supplier) in a given year. For a given domestic supplier and regression, there is only one observation per year that is an unweighted count of the number of sectors of its buyers (or suppliers) that event year. To avoid mechanical results, the MNC buyer triggering the event is excluded from the set of buyers described in this table. The event is defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. These regressions control for the contemporaneous log total sales of the domestic firm, in addition to firm and four-digit sector $\times$ province $\times$ calendar year fixed effects. Columns (1)-(4) pertain to the full sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC during our entire firm-to-firm transaction dataset. Clustering of standard errors is at the two-digit sector by province level. Columns (5)-(8) focus only on the restricted sample of domestic firms becoming first-time suppliers to an MNC between 2010 and 2015 and use standard error clustering at event by province level. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.6: Buyer Characteristics Change After Domestic Firms Start Supplying to MNCs

	Sh Buyers in HT-sect (1)	Ave Empl of Buyers (2)	Ave Sales of Buyers (3)	Ave Exp Sh of Buyers (4)	Sh Buyers in HT-sect (5)	Ave Empl of Buyers (6)	Ave Sales of Buyers (7)	Ave Exp Sh of Buyers (8)
log total sales	-0.001	0.132***	0.159***	0.002	0.003	0.224***	0.245***	0.005**
	(0.001)	(0.021)	(0.024)	(0.001)	(0.002)	(0.044)	(0.046)	(0.002)
4 years before event	-0.004	-0.003	-0.004	-0.008	-0.002	0.190	0.068	-0.007
	(0.004)	(0.073)	(0.077)	(0.005)	(0.008)	(0.125)	(0.123)	(0.008)
3 years before event	-0.006**	-0.077	-0.122**	-0.005	-0.004	0.023	-0.091	-0.004
	(0.003)	(0.055)	(0.050)	(0.004)	(0.006)	(0.097)	(0.099)	(0.005)
2 years before event	-0.003	-0.034	-0.049	0.001	-0.005	-0.011	-0.053	-0.000
	(0.002)	(0.032)	(0.038)	(0.002)	(0.003)	(0.056)	(0.053)	(0.003)
Year of event	-0.007***	-0.210***	-0.213***	-0.010***	-0.007**	-0.302***	-0.253***	-0.011***
	(0.002)	(0.052)	(0.052)	(0.002)	(0.003)	(0.055)	(0.063)	(0.003)
1 year after event	-0.004**	0.184***	0.224***	0.002	-0.006	0.018	0.161**	0.001
	(0.002)	(0.052)	(0.056)	(0.003)	(0.004)	(0.073)	(0.078)	(0.005)
2 years after event	-0.002	0.328***	0.382***	0.008***	-0.004	0.079	0.275***	0.007
	(0.003)	(0.042)	(0.044)	(0.003)	(0.006)	(0.095)	(0.090)	(0.006)
3 years after event	-0.001	0.374***	0.441***	0.012***	-0.003	0.045	0.294**	0.011
	(0.003)	(0.050)	(0.052)	(0.003)	(0.008)	(0.123)	(0.125)	(0.008)
4 years after event	0.000	0.411***	0.459***	0.011***	-0.001	0.027	0.301*	0.009
	(0.003)	(0.052)	(0.055)	(0.004)	(0.010)	(0.164)	(0.159)	(0.010)
Mean Dep. Var. (level)	0.014	431.5	70.8	0.045	0.018	409.6	61.2	0.048
SD Dep. Var. (level)	0.088	1386.4	263.1	0.14	0.078	1168.0	210.7	0.13
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	Yes	No	No	No	No
Adjusted R^2	0.64	0.72	0.73	0.77	0.46	0.62	0.64	0.67
# Observations	54,363	54,363	54,363	54,363	18,830	18,830	18,830	18,830
# Fixed Effects	14,998	14,998	14,998	14,998	6,315	6,315	6,315	6,315
# Firms	9,652	9,652	9,652	9,652	3,086	3,086	3,086	3,086

Notes: Table A.6 shows the results of running the event-study specification (2.1) adapted to three dependent variables: the share of buyers in high-tech sectors, the average number of workers of buyers, the average total sales of buyers, and the share of exports in the total sales of the buyers (averaged across all years for a given buyer). For a given domestic firm and regression, there is only one observation per year that is a weighted average of the characteristics of its buyers that year (weighted by their importance to that supplier). To avoid mechanical results, the MNC buyer triggering the event is excluded from the set of buyers described in this table. The event is defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. These regressions do not include firm-level time-varying controls, x_{it}, but only firm and four-digit sector × province × calendar year fixed effects. Columns (1)-(4) pertain to the full sample including both domestic firms that become first-time suppliers to an MNC between 2010 and 2015 and domestic firms never observed as supplying to an MNC during our entire firm-to-firm transaction dataset. Clustering of standard errors is at the two-digit sector by province level. Columns (5)-(8) focus only on the restricted sample of domestic firms becoming first-time suppliers to an MNC between 2010 and 2015 and use standard error clustering at event by province level. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

A.2 Robustness of Event-Study Results

A.2.1 Robustness to Different Sets of Fixed Effects

We investigate the stability of our economy-wide event-study coefficients to four combinations of fixed-effects (FEs). We start with only ten calendar year FEs to control for year-specific shocks. We then add firm FEs, to also control for firm-specific time-invariant characteristics. Next, we replace the calendar year FEs with four-digit sector × calendar year FEs to control for industry-specific time-varying shocks. Our preferred combination of FEs (firm and four-digit sector × province × calendar year FEs) allows for a spatial dimension to shocks. We report the event-study coefficients for three outcome variables: log total sales (Table A.7), translog TFP (Table A.8), and log sales to others (Table A.9).

There are three main patterns that come out of these results. First, the largest jump in R^2 occurs upon including firm FEs, especially when the outcome is a measure of firm size and when we do not include firm-specific time-varying controls.[1] Second, adding firm FEs is most consequential for the full sample, in particular for resolving the differential trends before the event. This highlights the differences in levels between first-time suppliers and never-suppliers. Even without firm FEs, in the restricted sample (including only firms that become first-time suppliers to MNCs) there is clear evidence of the lack of trends before the event and the sharp upward trend after. Third, for any combination of FEs (from the parsimonious ten FEs in regressions (1) and (5), to tens of thousands of FEs in all other regressions) all outcomes take off the year of the event.[2] All in all, we conclude that firm FEs are important to control for differences in levels, but do not drive our results.

[1] In Table A.8, we already control for second-order Taylor polynomial terms in K_{it}, L_{it}, and M_{it}. Even without firm FEs, the R^2 of the regressions in columns (1) and (5) are already above 0.90.

[2] Also, notice that allowing for potential spatial disparities in four-digit sector shocks barely affects the results. We keep the additional interaction with the province of the supplier to (modestly) raise the explanatory power.

Table A.7: Robustness of Baseline Event-Study Results for Total Sales to Different Sets of Fixed Effects

Outcome: (log) Total Sales	(1)	(2)	(3)	(4) Baseline	(5)	(6)	(7)	(8) Baseline
4 years before event	0.414***	0.072***	0.043	0.044	0.067	-0.021	-0.022	-0.022
	(0.069)	(0.027)	(0.027)	(0.028)	(0.077)	(0.059)	(0.043)	(0.053)
3 years before event	0.406***	0.067***	0.038*	0.029	0.104**	0.011	-0.000	0.001
	(0.058)	(0.020)	(0.021)	(0.023)	(0.048)	(0.041)	(0.035)	(0.041)
2 years before event	0.348***	0.045**	0.031*	0.026	0.071**	0.023	0.014	0.007
	(0.060)	(0.019)	(0.018)	(0.018)	(0.035)	(0.030)	(0.024)	(0.023)
Year of event	0.281***	0.158***	0.167***	0.159***	0.132***	0.200***	0.190***	0.191***
	(0.049)	(0.021)	(0.019)	(0.019)	(0.025)	(0.020)	(0.019)	(0.021)
1 year after event	0.476***	0.338***	0.337***	0.325***	0.384***	0.406***	0.375***	0.377***
	(0.040)	(0.029)	(0.027)	(0.028)	(0.041)	(0.033)	(0.027)	(0.035)
2 years after event	0.537***	0.370***	0.361***	0.351***	0.496***	0.457***	0.404***	0.408***
	(0.039)	(0.035)	(0.031)	(0.032)	(0.051)	(0.045)	(0.041)	(0.054)
3 years after event	0.586***	0.365***	0.351***	0.342***	0.561***	0.462***	0.390***	0.389***
	(0.042)	(0.039)	(0.033)	(0.035)	(0.062)	(0.056)	(0.054)	(0.072)
4 years after event	0.648***	0.358***	0.345***	0.334***	0.639***	0.462***	0.382***	0.382***
	(0.043)	(0.040)	(0.035)	(0.037)	(0.075)	(0.066)	(0.064)	(0.089)
Mean Dep. Var. (level)	0.85	0.85	0.85	0.85	1.45	1.45	1.45	1.45
SD Dep. Var. (level)	2.54	2.54	2.54	2.54	4.50	4.50	4.50	4.50
Firm FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Year-4DSect FE	No	No	Yes	No	No	No	Yes	No
Year-4DSect-Prov FE	No	No	No	Yes	No	No	No	Yes
Never Suppliers	Yes	Yes	Yes	Yes	No	No	No	No
Adjusted R^2	0.037	0.76	0.77	0.77	0.024	0.79	0.80	0.80
# Observations	116,683	116,683	116,683	116,683	23,961	23,961	23,961	23,961
# Fixed Effects	10	18,045	19,942	25,174	10	3,492	4,919	7,366
# Firms	18,035	18,035	18,035	18,035	3,482	3,482	3,482	3,482

Notes: Table A.7 shows the results of running four variants of the event-study specification (2.1) for one dependent variable: log total sales. The event is still defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. Columns (1)-(4) correspond to the full economy-wide sample (including first-time suppliers to MNCs and never-suppliers), columns (5)-(8) correspond to the restricted economy-wide sample (including only first-time suppliers to MNCs). These regressions do not include firm-level time-varying controls, x_{it}. The only difference between columns (1)-(4) and between columns (5)-(8) comes from the combination of fixed effects used in each column. Columns (4) and (8) use our preferred combination of fixed effects. Means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.8: Robustness of Baseline Event-Study Results for Translog TFP to Different Sets of Fixed Effects

Outcome: TL TFPR	(1)	(2)	(3)	(4) Baseline	(5)	(6)	(7)	(8) Baseline
4 years before event	0.051***	0.019	0.017	0.015	0.023	0.026	0.023	0.017
	(0.018)	(0.013)	(0.013)	(0.013)	(0.021)	(0.025)	(0.024)	(0.018)
3 years before event	0.044***	0.020**	0.018*	0.019**	0.021	0.026	0.019	0.020
	(0.014)	(0.009)	(0.010)	(0.009)	(0.019)	(0.016)	(0.016)	(0.015)
2 years before event	0.033***	0.005	0.004	0.007	0.011	0.013	0.010	0.010
	(0.012)	(0.009)	(0.009)	(0.008)	(0.010)	(0.012)	(0.011)	(0.011)
Year of event	0.091***	0.038***	0.042***	0.044***	0.069***	0.042***	0.043***	0.041***
	(0.013)	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.007)
1 year after event	0.096***	0.050***	0.055***	0.057***	0.075***	0.051***	0.051***	0.051***
	(0.012)	(0.010)	(0.011)	(0.012)	(0.011)	(0.014)	(0.013)	(0.012)
2 years after event	0.100***	0.057***	0.064***	0.067***	0.081***	0.056***	0.059***	0.054***
	(0.013)	(0.011)	(0.012)	(0.012)	(0.016)	(0.018)	(0.017)	(0.017)
3 years after event	0.091***	0.051***	0.062***	0.064***	0.074***	0.048**	0.053**	0.049**
	(0.012)	(0.012)	(0.013)	(0.013)	(0.023)	(0.023)	(0.020)	(0.020)
4 years after event	0.089***	0.050***	0.064***	0.066***	0.072***	0.041	0.049*	0.043*
	(0.011)	(0.013)	(0.015)	(0.015)	(0.025)	(0.030)	(0.026)	(0.025)
Mean Dep. Var. (level)	1.12	1.12	1.12	1.12	2.00	2.00	2.00	2.00
SD Dep. Var. (level)	3.17	3.17	3.17	3.17	5.74	5.74	5.74	5.74
Firm FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Year-4DSect FE	No	No	Yes	No	No	No	Yes	No
Year-4DSect-Prov FE	No	No	No	Yes	No	No	No	Yes
Never Suppliers	Yes	Yes	Yes	Yes	No	No	No	No
Adjusted R^2	0.90	0.96	0.96	0.97	0.93	0.97	0.97	0.97
# Observations	64,419	64,419	64,419	64,419	13,706	13,706	13,706	13,706
# Fixed Effects	10	10,502	12,079	15,464	10	2,154	3,238	4,774
# Firms	10,492	10,492	10,492	10,492	2,144	2,144	2,144	2,144

Notes: Table A.8 shows the results of running four variants of the event-study specification (2.1) for one dependent variable: a measure of TFP based on a translog production function (OLS regression). The event is still defined as a first time sale to an MNC. Reported are the coefficients for event-time −4 to +4, where the coefficients for the year prior to the event are normalized to zero. Columns (1)-(4) correspond to the full economy-wide sample (including first-time suppliers to MNCs and never-suppliers), columns (5)-(8) correspond to the restricted economy-wide sample (including only first-time suppliers to MNCs). The only difference between columns (1)-(4) and between columns (5)-(8) comes from the combination of fixed effects used in each column. Columns (4) and (8) use our preferred combination of fixed effects. Means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars).

Table A.9: Robustness of Baseline Event-Study Results for Sales to Others to Different Sets of Fixed Effects

Outcome: (log) Sales to Others	(1)	(2)	(3)	(4) Baseline	(5)	(6)	(7)	(8) Baseline
4 years before event	0.411***	0.018	-0.014	0.011	0.050	-0.113	-0.033	-0.047
	(0.070)	(0.042)	(0.044)	(0.042)	(0.075)	(0.096)	(0.103)	(0.119)
3 years before event	0.401***	-0.004	-0.029	-0.022	0.082*	-0.087	-0.042	-0.041
	(0.059)	(0.034)	(0.036)	(0.035)	(0.048)	(0.069)	(0.072)	(0.076)
2 years before event	0.343***	-0.021	-0.030	-0.020	0.058	-0.053	-0.026	-0.028
	(0.060)	(0.030)	(0.029)	(0.028)	(0.035)	(0.039)	(0.039)	(0.036)
Year of event	-0.242**	-0.218***	-0.201***	-0.193***	-0.356***	-0.140***	-0.151***	-0.122*
	(0.106)	(0.053)	(0.053)	(0.052)	(0.060)	(0.049)	(0.056)	(0.062)
1 year after event	0.108	0.114**	0.124**	0.118**	0.055	0.217***	0.169**	0.205**
	(0.095)	(0.053)	(0.052)	(0.053)	(0.072)	(0.070)	(0.078)	(0.090)
2 years after event	0.227***	0.203***	0.200***	0.201***	0.251***	0.343***	0.257**	0.320***
	(0.080)	(0.047)	(0.044)	(0.045)	(0.083)	(0.092)	(0.099)	(0.115)
3 years after event	0.292***	0.204***	0.202***	0.196***	0.358***	0.388***	0.268**	0.333**
	(0.082)	(0.051)	(0.045)	(0.046)	(0.108)	(0.111)	(0.126)	(0.147)
4 years after event	0.406***	0.193***	0.196***	0.203***	0.507***	0.401***	0.275*	0.380**
	(0.082)	(0.058)	(0.050)	(0.049)	(0.111)	(0.139)	(0.146)	(0.171)
Mean Dep. Var. (level)	0.84	0.84	0.84	0.84	1.42	1.42	1.42	1.42
SD Dep. Var. (level)	2.54	2.54	2.54	2.54	4.51	4.51	4.51	4.51
Firm FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Year-4DSect FE	No	No	Yes	No	No	No	Yes	No
Year-4DSect-Prov FE	No	No	No	Yes	No	No	No	Yes
Never Suppliers	Yes	Yes	Yes	Yes	No	No	No	No
Adjusted R^2	0.012	0.69	0.69	0.70	0.016	0.64	0.64	0.64
# Observations	116,536	116,536	116,536	116,536	23,801	23,801	23,801	23,801
# Fixed Effects	10	18,034	19,931	25,156	10	3,478	4,903	7,328
# Firms	18,024	18,024	18,024	18,024	3,468	3,468	3,468	3,468

Notes: Table A.9 shows the results of running four variants of the event-study specification (2.1) for one dependent variable: log total sales except those to first MNC buyer. The event is still defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. Columns (1)-(4) correspond to the full economy-wide sample (including first-time suppliers to MNCs and never-suppliers), columns (5)-(8) correspond to the restricted economy-wide sample (including only first-time suppliers to MNCs). These regressions do not include firm-level time-varying controls, x_{it}. The only difference between columns (1)-(4) and between columns (5)-(8) comes from the combination of fixed effects used in each column. Columns (4) and (8) use our preferred combination of fixed effects. Means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

A.2.2 Robustness to Excluding First-time Suppliers Hiring New Managers

Table A.10: Robustness of Baseline Event-Study Results for Total Sales to Excluding First-time Suppliers Hiring New Managers

Outcome: (log) Total Sales	Baseline (1)	No ΔT1 Event (2)	No ΔT2 Event (3)	No ΔT1 Event-1 (4)	No ΔT2 Event-1 (5)	Baseline (6)	No ΔT1 Event (7)	No ΔT2 Event (8)	No ΔT1 Event-1 (9)	No ΔT2 Event-1 (10)
4 years before event	0.044	0.032	0.018	0.060**	0.047	-0.022	-0.053	-0.023	-0.008	-0.009
	(0.028)	(0.029)	(0.031)	(0.027)	(0.031)	(0.053)	(0.056)	(0.057)	(0.055)	(0.052)
3 years before event	0.029	0.021	0.014	0.053**	0.043*	0.001	-0.014	0.017	0.029	0.015
	(0.023)	(0.025)	(0.026)	(0.025)	(0.022)	(0.041)	(0.044)	(0.041)	(0.045)	(0.043)
2 years before event	0.026	0.012	0.014	0.057***	0.044**	0.007	-0.007	0.008	0.042*	0.029
	(0.018)	(0.019)	(0.019)	(0.017)	(0.017)	(0.023)	(0.022)	(0.023)	(0.023)	(0.024)
Year of event	0.159***	0.118***	0.123***	0.143***	0.130***	0.191***	0.158***	0.142***	0.181***	0.160***
	(0.019)	(0.016)	(0.021)	(0.019)	(0.018)	(0.021)	(0.025)	(0.024)	(0.022)	(0.020)
1 year after event	0.325***	0.278***	0.274***	0.312***	0.303***	0.377***	0.345***	0.301***	0.374***	0.356***
	(0.028)	(0.023)	(0.025)	(0.027)	(0.028)	(0.035)	(0.040)	(0.042)	(0.036)	(0.035)
2 years after event	0.351***	0.300***	0.283***	0.339***	0.325***	0.408***	0.374***	0.303***	0.411***	0.385***
	(0.032)	(0.027)	(0.028)	(0.033)	(0.032)	(0.054)	(0.063)	(0.061)	(0.055)	(0.053)
3 years after event	0.342***	0.284***	0.279***	0.331***	0.320***	0.389***	0.360***	0.281***	0.402***	0.373***
	(0.035)	(0.029)	(0.031)	(0.035)	(0.033)	(0.072)	(0.086)	(0.083)	(0.075)	(0.070)
4 years after event	0.334***	0.280***	0.272***	0.325***	0.315***	0.382***	0.362***	0.267**	0.402***	0.371***
	(0.037)	(0.034)	(0.034)	(0.037)	(0.036)	(0.089)	(0.108)	(0.100)	(0.094)	(0.087)
Mean Dep. Var. (level)	0.85	0.84	0.84	0.85	0.85	1.45	1.44	1.46	1.45	1.46
SD Dep. Var. (level)	2.54	2.47	2.35	2.50	2.44	4.50	4.28	4.01	4.33	4.20
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Adjusted R^2	0.77	0.77	0.77	0.77	0.77	0.80	0.81	0.81	0.81	0.80
# Observations	116,683	114,541	113,172	115,045	114,381	23,961	21,793	20,482	22,305	21,698
# Fixed Effects	25,174	24,769	24,488	24,895	24,761	7,366	6,816	6,507	6,948	6,832
# Firms	18,035	17,681	17,443	17,807	17,699	3,482	3,118	2,902	3,253	3,154

Notes: Table A.10 shows the results of running the event-study specification (2.1) for one dependent variable: log total sales. The event is still defined as a first time sale to an MNC. Reported are the coefficients for event-time −4 to +4, where the coefficients for the year prior to the event are normalized to zero. Columns (1)-(5) correspond to the full economy-wide sample (including first-time suppliers to MNCs and never-suppliers), columns (6)-(10) correspond to the restricted economy-wide sample (including only first-time suppliers to MNCs). Columns (1) and (6) report our baseline results from Columns (1) and (5) in Table 2.1. Columns (2)-(5) differ from Column (1) (columns (7)-(10) differ from Column (6)) in their excluding first-time suppliers who have hired new managers either in the event year (“Event") or in the year prior to the event (“Event-1"). In this exercise, we identify managers as the top earners that year. In columns (2), (4), (7), and (9) we only drop first-time suppliers that hire a new worker that becomes the top earner in the firm (presumably the top manager or “T1"), whereas in columns (3), (5), (8), and (10) we also drop first-time suppliers that hire a new worker that becomes the top two earner in the firm (presumably the top two manager or “T2"). Means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

A.2.3 Robustness to Balancing the Sample in Event Time

In Table A.11, we replicate the main economy-wide event-study analysis on a version of the restricted sample balanced in event time from *-1* to *+1*. This new sample allow us to rule out compositional confounds around the event year. However, it also carries the obvious drawbacks of omitting young firms and of imposing survival after the event. Adding this requirement of balancing delivers qualitatively similar results.

Table A.11: Robustness Using a Balanced Sample in Event Time

	CD *K,L,M* (1)	TL *K,L,M* (2)	CD Index (3)	Y (4)	L (5)	K (6)	VA (7)	Sales to Others (8)	Total Trans (9)	Trans w/ Others (10)	Number Buyers (11)
4 years before event	0.01	0.03	-0.01	0.05	0.00	-0.08	-0.02	-0.01	0.04	0.03	0.02
	(0.03)	(0.03)	(0.04)	(0.07)	(0.06)	(0.10)	(0.08)	(0.10)	(0.11)	(0.18)	(0.05)
3 years before event	0.01	0.03	-0.01	0.05	0.01	-0.05	-0.02	-0.02	0.04	0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.05)	(0.04)	(0.07)	(0.06)	(0.07)	(0.06)	(0.12)	(0.04)
2 years before event	0.02	0.01	-0.00	0.03	0.01	-0.00	0.00	-0.03	0.04	0.06	0.01
	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.07)	(0.02)
Year of event	0.05***	0.03***	0.04**	0.29***	0.24***	0.20***	0.21***	0.05	0.38***	-0.42***	0.08***
	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)	(0.05)	(0.04)	(0.14)	(0.03)
1 year after event	0.07***	0.04**	0.07***	0.30***	0.24***	0.21***	0.22***	0.14*	0.40***	0.18	0.19***
	(0.02)	(0.02)	(0.02)	(0.05)	(0.04)	(0.06)	(0.04)	(0.08)	(0.07)	(0.14)	(0.04)
2 years after event	0.07**	0.04*	0.08***	0.29***	0.23***	0.28***	0.24***	0.18*	0.39***	0.31*	0.22***
	(0.03)	(0.02)	(0.03)	(0.07)	(0.05)	(0.07)	(0.06)	(0.10)	(0.10)	(0.18)	(0.05)
3 years after event	0.08**	0.04	0.10**	0.24***	0.21***	0.30***	0.25***	0.16	0.39***	0.45**	0.21***
	(0.04)	(0.03)	(0.04)	(0.09)	(0.07)	(0.10)	(0.08)	(0.12)	(0.12)	(0.23)	(0.06)
4 years after event	0.07	0.03	0.09*	0.22**	0.16*	0.31**	0.23**	0.18	0.36**	0.41	0.20**
	(0.05)	(0.04)	(0.05)	(0.11)	(0.09)	(0.12)	(0.09)	(0.14)	(0.15)	(0.28)	(0.08)
Mean Dep. Var. (level)	2.20	2.20	0.86	1.64	21.7	1.07	0.25	1.61	0.62	0.60	17.3
SD Dep. Var. (level)	5.99	5.99	0.49	4.84	50.4	3.27	0.66	4.85	1.92	1.93	50.8
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	No	No	No	No	No	No	No	No	No	No	No
Balanced Only	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.97	0.98	0.75	0.81	0.80	0.82	0.74	0.67	0.75	0.57	0.84
# Observations	10,295	10,295	10,295	17,203	17,203	17,203	17,203	17,203	17,203	17,203	17,203
# Fixed Effects	3,655	3,655	3,655	5,437	5,437	5,437	5,437	5,437	5,437	5,437	5,437
# Firms	1,416	1,416	1,416	2,145	2,145	2,145	2,145	2,145	2,145	2,145	2,145

Notes: Table A.11 shows the results of running the event-study specification (2.1) adapted to eleven dependent variables. All columns correspond to a balanced version of the restricted economy-wide sample (including only first-time suppliers to MNCs), where the imposed balancing is between event years *-1* and *+1*. The event is still defined as a first time sale to an MNC. Reported are the coefficients for event-time -4 to $+4$, where the coefficients for the year prior to the event are normalized to zero. Except for employment and the number of buyers, means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

A.2.4 Robustness to the Definition of the Event Year

One pattern that is potentially puzzling is the onset of treatment effects from event year *0*. While increases in firm size might to some degree be mechanical (if firms do not shed domestic buyers when becoming suppliers to an MNC), increases in performance may be expected with delay. To shed light on this pattern, one would ideally observe both the moment when the domestic firm starts its collaboration with its first MNC buyer and the moment when the first payment is made. Unfortunately, in the firm-to-firm transaction dataset, we cannot observe the starting date for the collaboration. What this dataset can offer is the *year of the first transaction* of a domestic firm with an MNC, which we label as event year *0*. This dataset also does not record when during a year transactions occur, only the cumulative value transacted in a year between two firms.

To make progress, we use the data from Procomer described in A.6.2. We first find that in the full sample of 1,985 deals mediated by Procomer between 2001 and 2016, the dates when deals are agreed upon are evenly distributed across months. While the dates recorded by Procomer as the dates of the agreement are not necessarily those when the transaction is made, we assume there is no reason for transactions to be more concentrated in certain months of the year. Second, from the email archive shared with us, we found that around 65% of deals go from first contact to agreeing on the deal in the same calendar year. Another 27% of deals have the date of the first contact and the sealing of the deal one calendar year apart. In our surveys to domestic firms we asked a slightly different question: “How quickly did your firm find a first MNC buyer after deciding that it wanted to have such buyers?" 55% of firms responded that it took less than a year, 9% between one and two years, and 8.5% over two years (see D.1). Jointly, these findings suggest that most transactions are likely to occur within a year of the first contact.

Given the information available in firm-to-firm transaction data, one cannot disentangle the following two scenarios (or combinations thereof). In one scenario, effects in event year *0* reflect adjustment and learning in the new role as a supplier to an MNC. These processes may be onset as soon as the collaboration starts, most likely in the preceding months to the transaction. In the other scenario, the smaller year *0* effects are simply “partial year effects" (Bernard, Boler, Massari, Reyes, and Taglioni, 2017). If the lag between the first contact and the first transaction is short, this would suggest fast learning in the new role of supplier to MNCs. As we cannot distinguish between these scenarios, we recommend caution on the interpretation of year *0* effects. That said, a potentially-imprecise measure of the exact year *0* does not affect the causal interpretation of our results or their general pattern of growth.

As a robustness check, instead of defining the event year as the first year when we observe domestic firm i having a transaction with an MNC buyer, we define it as the year *prior* to that of the first transaction. With this definition of the event year, we are focusing on what is likely to be the year of the first contact with an MNC (for contacts that materialize in a transaction a year later). Table A.12 shows that, with this new definition of the event year, results are almost mechanically delayed by a year, with the first gains in TFP manifesting themselves a year after the presumable first contact. While our preferred definition of the event year is the year when they first transact with an MNC, we are reassured that results are only changed in their timing as we shift the event year one year backwards.

Table A.12: Robustness of Baseline Event-Study Results to Different Definition of Event Year

	Prod Index (1)	CD *K,L,M* (2)	TL *K,L,M* (3)	Prod Index (4)	CD *K,L,M* (5)	TL *K,L,M* (6)
4 years before "event"	-0.008	0.001	-0.002	-0.064**	-0.057*	-0.051*
	(0.019)	(0.017)	(0.014)	(0.029)	(0.030)	(0.025)
3 years before "event"	0.019	0.001	0.007	-0.023	-0.039*	-0.025
	(0.024)	(0.017)	(0.016)	(0.023)	(0.021)	(0.016)
2 years before "event"	0.019	0.005	0.011	-0.012	-0.025	-0.009
	(0.013)	(0.012)	(0.010)	(0.014)	(0.015)	(0.012)
Year of "event"	-0.006	-0.015	-0.008	0.013	-0.001	0.007
	(0.011)	(0.010)	(0.009)	(0.011)	(0.012)	(0.010)
1 year after "event"	0.030**	0.043***	0.036***	0.060***	0.062***	0.062***
	(0.014)	(0.012)	(0.010)	(0.020)	(0.022)	(0.014)
2 years after "event"	0.053***	0.067***	0.049***	0.103***	0.099***	0.088***
	(0.014)	(0.014)	(0.012)	(0.021)	(0.025)	(0.016)
3 years after "event"	0.053***	0.073***	0.060***	0.125***	0.118***	0.112***
	(0.016)	(0.014)	(0.013)	(0.024)	(0.028)	(0.020)
4 years after "event"	0.055**	0.073***	0.057***	0.148***	0.140***	0.130***
	(0.022)	(0.017)	(0.016)	(0.031)	(0.034)	(0.025)
Mean Dep. Var. (level)	0.93	559.5	559.5	0.86	1100.8	1100.8
SD Dep. Var. (level)	0.56	1584.7	1584.7	0.49	2994.4	2994.4
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	No	No	No
Adjusted R^2	0.72	0.95	0.97	0.75	0.97	0.98
# Observations	64,419	64,419	64,419	10,295	10,295	10,295
# Fixed Effects	15,464	15,464	15,464	3,655	3,655	3,655
# Firms	10,492	10,492	10,492	1,416	1,416	1,416

Notes: Table A.12 shows the results of running specification (2.1) adapted to the same three measures of TFP defined for Table 2.4. There is only one difference with respect to specification (2.1): in this table, instead of defining the event year as the first year when we observe domestic firm i having a transaction with an MNC buyer, we define the event year as the year *prior* to that of the first transaction. With this definition of the event year, we are focusing on what may be the year of the first contact with an MNC (for contacts that materialize in a transaction a year later). Results for *4 years before "event"* are particularly noisy as they use data only for firms we observe four years before their first year transacting with an MNC. Columns (1)-(3) report event study estimates for the sample including both domestic firms that become first-time suppliers to an MNC after 2010 and domestic firms never observed as supplying to an MNC during our entire firm-to-firm transaction data. Clustering of standard errors is at the two-digit sector by province level. Columns (4)-(6) focus only on the sample of domestic firms becoming first-time suppliers to an MNC after 2010 and use standard error clustering at event by province level. Means (in levels) are reported in millions of U.S. dollars (CPI-deflated to 2013 dollars). Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

A.2.5 No Evidence of Changes in Third-Party Reporting

Table A.13: Similar Compliance in Third Party Reporting After Supplying to an MNC

	Seller-Diff (1)	Buyer-Diff (2)	Mis-Seller (3)	Seller-Diff (4)	Buyer-Diff (5)	Mis-Seller (6)
4 years before event	0.002	0.003	0.002	0.012	0.008	-0.002
	(0.006)	(0.008)	(0.003)	(0.017)	(0.013)	(0.005)
3 years before event	0.002	0.001	0.001	0.010	0.007	-0.004
	(0.007)	(0.007)	(0.002)	(0.013)	(0.010)	(0.004)
2 years before event	-0.002	-0.004	-0.002	0.005	-0.003	-0.000
	(0.004)	(0.005)	(0.003)	(0.009)	(0.007)	(0.003)
Year of event	0.000	0.001	0.000	-0.003	-0.003	0.002
	(0.005)	(0.005)	(0.002)	(0.007)	(0.006)	(0.002)
1 year after event	0.007^{*}	0.006	-0.001	-0.002	-0.004	0.005
	(0.004)	(0.007)	(0.002)	(0.011)	(0.010)	(0.004)
2 years after event	0.008^{*}	0.005	-0.001	-0.006	-0.010	0.006
	(0.005)	(0.006)	(0.002)	(0.015)	(0.015)	(0.006)
3 years after event	0.004	0.000	-0.002	-0.015	-0.018	0.006
	(0.005)	(0.006)	(0.002)	(0.020)	(0.018)	(0.007)
4 years after event	0.014^{**}	0.009	-0.003	-0.012	-0.014	0.008
	(0.006)	(0.006)	(0.003)	(0.024)	(0.023)	(0.009)
Mean Dep. Var. (level)	0.038	0.048	0.012	0.074	0.061	0.013
SD Dep. Var. (level)	0.15	0.15	0.073	0.20	0.17	0.058
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	No	No	No
Adjusted R^2	0.19	0.12	0.15	0.15	0.10	0.045
# Observations	109,438	109,438	109,438	23,677	23,677	23,677
# Fixed Effects	24,115	24,115	24,115	7,323	7,323	7,323
# Firms	17,129	17,129	17,129	3,472	3,472	3,472

Notes: Table A.13 shows the results of running specification (2.1) adapted to three measures of quality in third-party reporting. For this exercise, we use the raw version of D-151, as opposed to the clean version used in the main analysis (see A.6.1). "Seller-diff" is a weighted average of the percentage difference in values reported, across all transactions in a year for which a firm is the seller. The percentage difference is computed as the (maximum value reported-minimum value reported)/(minimum value reported). "Seller-diff" uses as weights the importance of the transaction in that year for the seller. "Buyer-diff" is analogously constructed, this time keeping only transactions for which a firm is the buyer. "Mis-Seller" is defined as (the total number of buyers that reported a given firm as a seller and that are not reported back by the seller)/(the total number of buyers of the seller that are reported by either side). The event is defined as a first time sale to an MNC. Columns (1)-(3) report event study estimates for the sample including both domestic firms that become first-time suppliers to an MNC after 2010 and domestic firms never observed as supplying to an MNC during our entire firm-to-firm transaction data. Clustering of standard errors is at the two-digit sector by province level. Columns (4)-(6) focus only on the sample of domestic firms becoming first-time suppliers to an MNC between 2010 and 2015 and use standard error clustering at event by province level. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

In addition to the discussion in Section 2.4.2 (based on the results in Table A.13), note that we find marked increases in measures of firm performance that either do not have a direct link to firms' tax liability or imply an opposite behavior to that predicted by a mere reduction in tax-evasive behaviors (see Section 2.4.1). For instance, had costs been artificially high prior to a first deal with an MNC, a higher scrutiny on firms dealing with MNCs would imply a lowering of their previously-inflated costs. The marked boost in input costs (see columns (4) and (8) of Table 2.1) suggests a legitimate expansion in operations. Moreover, persistent boosts in proxies of TFP (e.g., sales/worker, OLS production function estimation) are implausible behavioral responses to what may be a heightened scrutiny on one's tax compliance. It is therefore unlikely that tax compliance effects can reproduce our baseline results.

A.3 Additional Model Derivations

In this section we present the derivations of the main results of the model. In the environment introduced in Section 2.5, we have that (i) $Q = Bp^{-\sigma}$ (demand equation) and (ii) $p = c_0 Q^{\frac{1}{\gamma}-1}\phi^{\frac{-1}{\gamma}}$ (price equal constant mark-up times marginal cost), where $Q = \sum_i q_i$ and $B \equiv \sum_i n_i b_i$.

A.3.1 Derivation of Equation (2.3)

Combining (i) and (ii) from above we have that $Q = B\left(c_0 Q^{\frac{1}{\gamma}-1}\phi^{\frac{-1}{\gamma}}\right)^{-\sigma}$, which implies:

$$Q = \left(c_0^{-\sigma\gamma} B^{\gamma}\phi^{\sigma}\right)^{\frac{1}{\gamma+\sigma-\sigma\gamma}}. \tag{A.1}$$

$$\Rightarrow pQ = c_0 Q^{\frac{1}{\gamma}-1}\phi^{\frac{-1}{\gamma}}\left(Bc_0^{-\sigma\gamma}\phi^{\sigma}\right)^{\frac{1}{\gamma+\sigma-\sigma\gamma}}$$

$$= c_1 B^{\frac{1}{\gamma+\sigma-\sigma\gamma}}\phi^{\frac{\sigma-1}{\gamma+\sigma-\sigma\gamma}}, \tag{A.2}$$

where $c_1 = c_0^{\frac{\gamma+\sigma-2\sigma\gamma}{\gamma+\sigma-\sigma\gamma}}$. Equation (A.2) is useful because it allows us to write B (which is not observable) as a function of total sales (which we observe), the constant c_1, and ϕ (the most relevant variable in our context). Let us invert equation (A.2) as follows (the usefulness of this will become clear soon):

$$B^{\frac{1}{\gamma+\sigma-\sigma\gamma}} = c_1^{-1}(pQ)\phi^{-\frac{(\sigma-1)}{\gamma+\sigma-\sigma\gamma}}. \tag{A.3}$$

Define the quantity sold to others as $\widetilde{Q} = \sum_{i \neq MNC_0} q_i = \tilde{B} p^{-\sigma}$. We can write total sales to others as:

$$
\begin{aligned}
p\tilde{Q} = p\tilde{B}p^{-\sigma} = \tilde{B}\left(c_0 Q^{\frac{1}{\gamma}-1}\phi^{\frac{-1}{\gamma}}\right)^{1-\sigma} &= \tilde{B}c_0^{1-\sigma}\phi^{\frac{\sigma-1}{\gamma}}Q^{\frac{(\gamma-1)(\sigma-1)}{\gamma}} \\
&= \tilde{B}c_0^{1-\sigma}\phi^{\frac{\sigma-1}{\gamma}}\left(c_0^{-\frac{\sigma\gamma}{\gamma+\sigma-\sigma\gamma}}B^{\frac{\gamma}{\gamma+\sigma-\sigma\gamma}}\phi^{\frac{\sigma}{\gamma+\sigma-\sigma\gamma}}\right)^{\frac{(\gamma-1)(\sigma-1)}{\gamma}} \\
&= c_2\tilde{B}\phi^{\frac{\sigma-1}{\gamma+\sigma-\sigma\gamma}}\left[B^{\frac{1}{\gamma+\sigma-\sigma\gamma}}\right]^{(\gamma-1)(\sigma-1)},
\end{aligned}
\tag{A.4}
$$

where we use equation (A.1) to go from the second to the third line and $c_2 = c_0^{\frac{\gamma(1-\sigma)}{\gamma+\sigma-\sigma\gamma}}$.

When $\gamma \neq 1$ (the supplier does not have constant returns to scale), the equilibrium sales to others depend not only on the demand shifter of those other buyers ($\tilde{B}$), but also on the aggregate demand shifter (B) that includes the first MNC buyer, MNC_0. This happens because the demand from MNC_0 may affect the scale of the firm and thus its price, even if $\tilde{B}$ and ϕ remain constant. When $\gamma = 1$, equation (A.4) collapses to an analogous of equation (A.2). We now make use of equation (A.3). Substituting equation (A.3) into (A.4) gives us:

$$
\begin{aligned}
p\tilde{Q} &= c_2\tilde{B}\phi^{\frac{\sigma-1}{\gamma+\sigma-\sigma\gamma}}\left[c_1^{-1}(pQ)\phi^{-\frac{(\sigma-1)}{\gamma+\sigma-\sigma\gamma}}\right]^{(\gamma-1)(\sigma-1)} \\
&= c_2 c_1^{-(\gamma-1)(\sigma-1)}\tilde{B}\phi^{\sigma-1}(pQ)^{(\gamma-1)(\sigma-1)}.
\end{aligned}
$$

Defining $\delta \equiv \delta(\gamma, \sigma) = (\gamma-1)(\sigma-1)$ and substituting in the previous equation we find:

$$
p\tilde{Q} = c_3\tilde{B}\phi^{(\sigma-1)}(pQ)^{\delta}, \tag{A.5}
$$

where $c_3 = c_2 c_1^{-(\gamma-1)(\sigma-1)}$. Using $1 - \frac{1}{\gamma} < \frac{1}{\sigma}$ from the second order condition for profit maximization we get $0 < \gamma + \sigma - \sigma\gamma = -(\gamma-1)(\sigma-1) + 1 = -\delta + 1$, and conclude that $\delta < 1$. Since $\gamma > 0$ then $\gamma - 1 > -1$. Finally, $\delta > -(\sigma - 1)$. Thus $\delta \in (1-\sigma, 1)$. Taking logs of both sides of equation (A.5) and defining $\kappa' = \ln(c_3)$ we arrive to equation (2.3) in the paper.

A.3.2 Derivation of Result 1

We start from the equilibrium relation in equation (2.3) and take the total derivative of both sides of the equation. We then rearrange terms such that the left-hand side depends only on variables that are observable in firm-to-firm transaction data and δ. Last, we take expectations over all domestic firms that experience an event and find:

$$
\begin{aligned}
\mathbb{E}\left[\mathsf{d}\ln(p\widetilde{Q})\right] - \delta\mathbb{E}\left[\mathsf{d}\ln(pQ)\right] =& \mathbb{E}\left[\mathsf{d}\ln(\widetilde{B})\right] + (\sigma-1)\varepsilon_{\phi} \\
\Rightarrow \mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\widetilde{Q}}{(pQ)^{\delta}}\right)\right] =& \mathbb{E}\left[\mathsf{d}\ln(\widetilde{B})\right] + (\sigma-1)\varepsilon_{\phi},
\end{aligned}
\tag{A.6}
$$

where $\varepsilon_\phi = \mathbb{E}\left[\mathsf{d}\ln(\phi)\right]$.

Let us focus on the term $\mathbb{E}\left[\mathsf{d}\ln(\widetilde{B})\right]$. Recall that $\widetilde{B}$ depends on $n_i(\phi, r)$, $\forall i \neq MNC_0$.

A change in firm TFP (ϕ) or reputation (r) triggered by the event induces a new demand shock (a change in $\widetilde{B}$) coming from an increase in the probability to sell to new buyers. The total derivative of $\ln(\widetilde{B})$ can thus be split into a part that accounts for changes in probabilities (n_i) for a constant demand shifter (b_i) and one that accounts for changes in b_i for a constant n_i.

$$
\begin{aligned}
\mathbb{E}\left[\mathsf{d}\ln(\widetilde{B})\right] =& \mathbb{E}\left[\frac{1}{\widetilde{B}}\mathsf{d}\widetilde{B}\right] = \mathbb{E}\left[\frac{1}{\widetilde{B}}\sum_{i\neq MNC_0}^{N} b_i \mathsf{d}(n_i) + n_i \mathsf{d}(b_i)\right] = \mathbb{E}\left[\frac{1}{\widetilde{B}}\sum_{i\neq MNC_0}^{N} n_i b_i \mathsf{d}\ln(n_i) + n_i b_i \mathsf{d}\ln(b_i)\right] \\
=& \mathbb{E}\left[\sum_{i\neq MNC_0}^{N} \mathsf{d}\ln(n_i)\frac{n_i b_i}{\sum_{k\neq MNC_0}^{N} n_k b_k} + \mathsf{d}\ln(b_i)\frac{n_i b_i}{\sum_{k\neq MNC_0}^{N} n_k b_k}\right] \\
=& \mathbb{E}\left[\sum_{i\neq MNC_0}^{N} \mathsf{d}\ln(n_i)\frac{n_i b_i}{\sum_{k\neq MNC_0}^{N} n_k b_k}\right] + \mathbb{E}\left[\sum_{i\neq MNC_0}^{N} \mathsf{d}\ln(b_i)\frac{n_i b_i}{\sum_{k\neq MNC_0}^{N} n_k b_k}\right] \\
=& \varepsilon_{\widetilde{n}} + \varepsilon_{\widetilde{b}},
\end{aligned} \tag{A.7}
$$

where $\varepsilon_{\widetilde{n}}$ and $\varepsilon_{\widetilde{b}}$ are expectations of weighted averages of $\mathsf{d}\ln(n_i)$ and $\mathsf{d}\ln(b_i)$, respectively, $\forall i \neq MNC_0$. The weights are equal to $\frac{n_i b_i}{\sum_{k\neq MNC_0}^{N} n_k b_k}$. Note that these weights sum up to one.

We assume that the demand shifters ($b_i = y_i / P_i^{1-\sigma}$) of other buyers do not change systematically as a consequence of the event. This assumption (which implies $\varepsilon_{\widetilde{b}} = 0$) in combination with our result in equation (A.7) allow us to simplify equation (A.6) to:

$$
\mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\widetilde{Q}}{(pQ)^\delta}\right)\right] = (\sigma - 1)\varepsilon_\phi + \varepsilon_{\widetilde{n}}. \tag{A.8}
$$

Note that through the lens of our model, the left-hand side of equation (A.8) informs us about changes in either ϕ or n_i (owed to changes in either ϕ, r, or both). Equation (A.8) is the same as equation (2.4) in the paper. The interpretation of this equation leads to Result 1.

A.3.3 Derivation of Result 2

To estimate the change in TFP alone (ε_ϕ), we rely on two additional assumptions: (a-i) there is a large number of potential buyers in the country and (a-ii) for any changes in ϕ and/or r, all buyers i equally adjust their probability to source from the supplier, i.e., $\mathsf{d}\ln(n_i) = \mathsf{d}\ln(n)$, $\forall i \neq MNC_0$. We discuss assumption (a-ii) in detail in A.4.5.

Under assumption (a-i), the total number of other buyers of the supplier ($\widetilde{N}$) is given by the sum of the probabilities of buying from the supplier: $\widetilde{N} = \sum_{i\neq MNC_0}^{N} n_i$. This allows us to exploit the change in the number of buyers after the event. Assumption (a-ii) in combination with our definition of $\varepsilon_{\widetilde{n}}$ (see equation (A.7)) implies that $\varepsilon_{\widetilde{n}} = \mathbb{E}\left[\mathsf{d}\ln(n)\right]$.

We can then write the expected derivative of the log number of other buyers as:

$$
\begin{aligned}
\mathbb{E}\left[\mathsf{d}\ln(\widetilde{N})\right] =& \mathbb{E}\left[\frac{1}{\widetilde{N}}\mathsf{d}\widetilde{N}\right] = \mathbb{E}\left[\frac{1}{\widetilde{N}}\sum_{i\neq MNC_0}^{N}\mathsf{d}(n_i)\right] = \mathbb{E}\left[\sum_{i\neq MNC_0}^{N}\frac{\mathsf{d}(n_i)}{n_i}\frac{n_i}{\widetilde{N}}\right] \\
=& \mathbb{E}\left[\sum_{i\neq MNC_0}^{N}\mathsf{d}\ln(n_i)\frac{n_i}{\sum_{k\neq MNC_0}^{N}n_k}\right] = \mathbb{E}\left[\mathsf{d}\ln(n)\sum_{i\neq MNC_0}^{N}\frac{n_i}{\sum_{k\neq MNC_0}^{N}n_k}\right] \\
=& \mathbb{E}\left[\mathsf{d}\ln(n)\right] = \varepsilon_{\widetilde{n}}.
\end{aligned}
$$

We can then write equation (A.8) as:

$$
\begin{aligned}
\mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\widetilde{Q}}{(pQ)^{\delta}}\right)\right] =& (\sigma-1)\varepsilon_{\phi} + \mathbb{E}\left[\mathsf{d}\ln(\widetilde{N})\right] \\
\Rightarrow \mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\widetilde{Q}/(pQ)^{\delta}}{\widetilde{N}}\right)\right] =& (\sigma-1)\varepsilon_{\phi},
\end{aligned}
\tag{A.9}
$$

where we refer to the left-hand side of the equation as the *average adjusted sales to others*. Finally, dividing both sides of this equation by $(\sigma-1)$ leads to Result 2.

A.4 Additional Model-Relevant Evidence

A.4.1 Motivating the Use of Public Demand Shocks

MNC buyers may differ from domestic buyers not only in their potential for knowledge transfers (that may help improve the efficiency, quality, or product mix of suppliers), but also in features of their contracts that are themselves attractive to domestic suppliers. According to our survey answers (see Question 2 in D.1.3), reliable payment, the potential for future scaling of the collaboration, transparent decision-making are attractive features of supplying to MNC. An indirect way to check whether these features are the main drivers of our results is to study the effects of other types of demand shocks that share these relevant features with demand shocks from MNCs. For this reason, we study the effects of starting to procure the government on the performance of domestic firms.

Table A.14: TFP Estimation After Starting to Supply to the Government

	Prod Index (1)	CD K,L,M (2)	TL K,L,M (3)	Prod Index (4)	CD K,L,M (5)	TL K,L,M (6)
4 years before event	-0.011	0.002	-0.016	-0.084*	-0.061*	-0.063*
	(0.024)	(0.020)	(0.019)	(0.049)	(0.035)	(0.032)
3 years before event	0.012	0.022	0.013	-0.018	-0.018	-0.023
	(0.022)	(0.016)	(0.014)	(0.038)	(0.023)	(0.025)
2 years before event	0.004	0.030***	0.012	-0.019	-0.007	-0.012
	(0.013)	(0.010)	(0.009)	(0.024)	(0.015)	(0.015)
Year of event	-0.020	0.024**	0.013	-0.002	-0.001	0.009
	(0.013)	(0.012)	(0.009)	(0.024)	(0.012)	(0.012)
1 year after event	-0.021	0.028**	0.021**	0.017	0.006	0.027
	(0.015)	(0.013)	(0.009)	(0.042)	(0.019)	(0.018)
2 years after event	-0.018	0.026**	0.015	0.038	0.021	0.045*
	(0.017)	(0.013)	(0.011)	(0.049)	(0.026)	(0.024)
3 years after event	-0.026	0.011	0.002	0.045	0.018	0.043
	(0.018)	(0.012)	(0.010)	(0.059)	(0.031)	(0.029)
4 years after event	-0.017	0.007	0.012	0.065	0.015	0.065
	(0.022)	(0.018)	(0.014)	(0.076)	(0.037)	(0.041)
Mean Dep. Var. (level)	0.92	1.66	1.66	0.86	4.26	4.26
SD Dep. Var. (level)	0.56	6.47	6.47	0.52	17.7	17.7
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	Yes	Yes	No	No	No
Adjusted R^2	0.70	0.96	0.97	0.62	0.97	0.98
# Observations	86,232	86,232	86,232	7,122	7,122	7,122
# Fixed Effects	19,377	19,377	19,377	2,353	2,353	2,353
# Firms	13,304	13,304	13,304	895	895	895

Notes: Table A.14 shows the results of running specification of equation (2.1) adapted to the same three measures of TFP defined for Table 2.4. The event is defined as a first time sale to the government. Columns (1)-(4) report event study estimates for the sample including both domestic firms that become first-time suppliers to the government after 2010 and domestic firms never observed as supplying to the government during our entire firm-to-firm transaction data. Clustering of standard errors is at the two-digit sector by province level. Columns (5)-(8) focus only on the sample of domestic firms becoming first-time suppliers to an MNC after 2010 and use standard error clustering at event by province level. Means (in levels) of sales (residualized in columns (1) and (4)) are reported in millions of U.S. dollars (CPI-deflated to 2013 U.S. dollars). Clustered standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Government procurement accounted for approximately 15% of the 2014 Costa Rican GDP (excluding oil revenues) (OECD, 2015). Typically, over 90% of government purchases are carried out by five autonomous institutions: the Costa Rican Electricity Institute (*Instituto*

Costarricense de Electricidad), the National Road Council, the Costa Rican Department of Social Security (*Caja Costarricense de Seguro Social*), the Costa Rican Oil Refinery (*Refinadora Costarricense de Petróleo*) and the National Bank of Costa Rica (OECD, 2015). Hence, government purchases share with MNC purchases features of reliability and scale.[3] Once a firm is already pre-registered and pre-qualified, future contracts with the government are also more likely to occur. Surprisingly, when we go to the data and study the features of first-time sales to the government, we find to be very similar to those of first-time sales to an MNC. The average (median) first transaction with the government is of 59,8K U.S. dollars (17.7K), whereas the average (median) first transaction with an MNC is of 56,7K U.S. dollars (11.9K). The lengths of these relationships are also very similar.

In terms of process, government entities generally acquire their goods and services through public tenders, which are advertised in the official legal bulletin, *La Gaceta*, and other major newspapers. In 2010, the Costa Rican government created an electronic platform for public procurement called Mer-Link.[4] Mer-Link allows for a transparent search of both open and closed public tenders, with a detailed description of the product or service procured. All firms are evaluated in their ability to fulfill a given contract, with the details of the evaluation available for public consultation. This evaluation process has similar learning benefits to the evaluations carried out by Procomer in its "Productive Linkages" program and to audits carried out independently by MNCs prior to contracting a new supplier.

We propose here a new event-study, with the event defined as a first sale to the government. As before, data constraints require such a sale to occur between 2010 and 2015. To avoid overlapping treatments, we only preserve domestic firms that never supply to an MNC. We continue to use the event-study design described in Section 2.3, altered only in the event of interest. We repeat for the restricted set of first-time suppliers to the government all regressions conducted for the restricted sample of first-time suppliers to an MNC. Those exercises using the full sample of first-time suppliers and never-suppliers to an MNC are replicated with the full sample of first-time suppliers and never-suppliers to the government.

Table A.14 is analogous to Table 2.4, with the event and samples adapted to the current exercise. The new table exhibits significantly smaller and shorter-lived improvements in measures of TFP, which are not robust across samples and definitions of the dependent variable. These event-study findings motivate our exclusion restriction in the IV exercise described in Section 2.5.3, useful to estimate δ. See Section 2.5.2 for more details.

[3] The same argument is made in Ferraz, Finan, and Szerman (2016): the government is a more reliable payer than most private parties. This reliability gives vendors security that the terms of the contract will be respected, which encourages them to make the investments necessary to fulfill the contract.

[4] To access the Mer-Link website, see here. Mer-Link coexists with another purchasing system, called CompraRed, but Mer-Link has grown into the dominating platform.

A.4.2 Instrumental Variable Strategy to Estimate δ

Table A.15: Instrumental Variable Strategy for Estimation of δ

	(1) δ / (SE)	(2) First-Stage F	(3) # Observations
Full Sample	-0.217*	49.52	78,603
	(0.126)	–	–
Restricted Sample	-0.080	109.60	10,483
	(0.087)	–	–

Notes: Table A.15 shows the results of the instrumental variable strategy described in Section 2.5.3. We estimate equation (2.5) by instrumenting the change in log total sales of supplier i at time t with a dummy variable indicating whether supplier i is awarded a procurement contract at time $t-1$ or not. We estimate this equation over two samples that both exclude suppliers to MNCs, in order to isolate the effect of starting to sell to the government. The “Restricted Sample” focuses on firms that start supplying to the government in the period of our sample. The “Full Sample" also includes firms that never sell to the government over this period. Both regressions include firm fixed effects, as well as four-digit sector×province×year fixed effects. Robust standard errors are clustered at the two-digit sector×province level. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.16: Robustness of the Empirical Application of Result 1 to Different Values of δ

	$\delta = -0.22$ (1)	$\delta = -0.22$ (2)	$\delta = -1.2$ (3)	$\delta = -0.9$ (4)	$\delta = -0.6$ (5)	$\delta = -0.3$ (6)	$\delta = 0$ (7)	$\delta = 0.3$ (8)	$\delta = -1.2$ (9)	$\delta = -0.9$ (10)	$\delta = -0.6$ (11)	$\delta = -0.3$ (12)	$\delta = 0$ (13)	$\delta = 0.3$ (14)
4 years before event	0.004	-0.010	0.013	0.010	0.007	0.005	0.002	-0.000	-0.015	-0.014	-0.012	-0.011	-0.009	-0.008
	(0.009)	(0.025)	(0.014)	(0.013)	(0.011)	(0.010)	(0.008)	(0.007)	(0.032)	(0.029)	(0.027)	(0.025)	(0.024)	(0.023)
3 years before event	-0.003	-0.008	0.003	0.001	-0.001	-0.003	-0.004	-0.006	-0.008	-0.008	-0.008	-0.008	-0.008	-0.008
	(0.008)	(0.016)	(0.012)	(0.011)	(0.009)	(0.008)	(0.007)	(0.006)	(0.022)	(0.020)	(0.018)	(0.016)	(0.015)	(0.014)
2 years before event	-0.003	-0.005	0.002	0.001	-0.001	-0.002	-0.004	-0.006	-0.004	-0.004	-0.005	-0.005	-0.006	-0.006
	(0.006)	(0.008)	(0.009)	(0.008)	(0.007)	(0.006)	(0.006)	(0.005)	(0.010)	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)
Year of event	-0.031***	-0.016	0.001	-0.009	-0.019*	-0.029***	-0.039***	-0.048***	0.022	0.011	-0.001	-0.013	-0.024*	-0.036***
	(0.010)	(0.013)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.016)	(0.015)	(0.014)	(0.013)	(0.012)	(0.012)
1 year after event	0.038***	0.058***	0.101***	0.082***	0.062***	0.043***	0.024**	0.004	0.131***	0.108***	0.086***	0.063***	0.041**	0.019
	(0.011)	(0.019)	(0.014)	(0.013)	(0.012)	(0.011)	(0.011)	(0.010)	(0.024)	(0.022)	(0.021)	(0.019)	(0.018)	(0.017)
2 years after event	0.056***	0.082***	0.124***	0.103***	0.082***	0.061***	0.040***	0.019**	0.161***	0.137***	0.113***	0.088***	0.064***	0.040*
	(0.010)	(0.024)	(0.015)	(0.013)	(0.011)	(0.010)	(0.009)	(0.008)	(0.032)	(0.029)	(0.027)	(0.025)	(0.023)	(0.022)
3 years after event	0.054***	0.084**	0.121***	0.101***	0.080***	0.060***	0.039***	0.019**	0.159***	0.136***	0.113***	0.090***	0.067**	0.044
	(0.010)	(0.031)	(0.016)	(0.014)	(0.012)	(0.011)	(0.009)	(0.008)	(0.041)	(0.038)	(0.035)	(0.032)	(0.029)	(0.027)
4 years after event	0.055***	0.093**	0.120***	0.101***	0.081***	0.061***	0.041***	0.021**	0.167***	0.144***	0.122***	0.099**	0.076**	0.053
	(0.011)	(0.036)	(0.017)	(0.015)	(0.013)	(0.011)	(0.010)	(0.009)	(0.047)	(0.043)	(0.040)	(0.037)	(0.034)	(0.032)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No
# Observations	116,536	23,801	116,536	116,536	116,536	116,536	116,536	116,536	23,801	23,801	23,801	23,801	23,801	23,801

Notes: Table A.16 shows the results of running specification (2.1) with the measure of *adjusted sales to others* as the dependent variable. For this table, total sales and sales to others use total sales values from corporate income tax returns data. For sales to others, we subtract from total sales those sales made to the first MNC buyer. Each column implements Result 1 for a different value of δ, as indicated above the column number. Columns (1) and (2) show our baseline findings for $\delta = -0.217$ and for the two samples (full and restricted) - this estimate of δ comes from the instrumental variable strategy described in Section 2.5.3 and implemented in Table A.15. The rest of the columns show the robustness of our baseline findings to values of $\delta \in [-1.2, 0.3]$. For $\sigma = 6$, the corresponding values of $\gamma \in [0.76, 1.06]$. Columns (3)-(8) report event-study estimates for the full sample including both domestic firms that become first-time suppliers to an MNC and never-suppliers. Clustering of standard errors is at the two-digit sector by province level. Columns (9)-(14) focus only on the restricted sample of domestic firms that become first-time suppliers to an MNC and use standard error clustering at event by province level. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.17: Robustness of the Empirical Application of Result 1 to Different Values of δ - Transaction Data Only

	$\delta=-0.22$ (1)	$\delta=-0.22$ (2)	$\delta=-1.2$ (3)	$\delta=-0.9$ (4)	$\delta=-0.6$ (5)	$\delta=-0.3$ (6)	$\delta=0$ (7)	$\delta=0.3$ (8)	$\delta=-1.2$ (9)	$\delta=-0.9$ (10)	$\delta=-0.6$ (11)	$\delta=-0.3$ (12)	$\delta=0$ (13)	$\delta=0.3$ (14)
4 years before event	0.005	-0.029	0.012	0.010	0.008	0.005	0.003	0.001	-0.037	-0.035	-0.032	-0.030	-0.028	-0.025
	(0.019)	(0.031)	(0.033)	(0.029)	(0.025)	(0.020)	(0.016)	(0.013)	(0.041)	(0.037)	(0.034)	(0.032)	(0.030)	(0.028)
3 years before event	0.003	-0.021	0.008	0.006	0.005	0.003	0.002	0.001	-0.023	-0.023	-0.022	-0.021	-0.021	-0.020
	(0.010)	(0.021)	(0.016)	(0.014)	(0.012)	(0.011)	(0.009)	(0.008)	(0.029)	(0.026)	(0.024)	(0.022)	(0.020)	(0.019)
2 years before event	0.008	-0.005	0.017	0.014	0.012	0.009	0.006	0.004	-0.004	-0.004	-0.005	-0.005	-0.006	-0.006
	(0.007)	(0.010)	(0.011)	(0.010)	(0.009)	(0.008)	(0.007)	(0.006)	(0.015)	(0.013)	(0.012)	(0.010)	(0.009)	(0.008)
Year of event	-0.136***	-0.113***	-0.075***	-0.093***	-0.112***	-0.131***	-0.149***	-0.168***	-0.048***	-0.068***	-0.088***	-0.107***	-0.127***	-0.147***
	(0.019)	(0.015)	(0.022)	(0.021)	(0.020)	(0.019)	(0.018)	(0.018)	(0.018)	(0.017)	(0.016)	(0.015)	(0.015)	(0.015)
1 year after event	0.052***	0.080***	0.140***	0.113***	0.086***	0.059***	0.032***	0.005	0.176***	0.146***	0.117***	0.088***	0.059***	0.030
	(0.013)	(0.020)	(0.020)	(0.018)	(0.015)	(0.013)	(0.011)	(0.009)	(0.026)	(0.024)	(0.022)	(0.020)	(0.019)	(0.018)
2 years after event	0.084***	0.119***	0.174***	0.147***	0.119***	0.091***	0.064***	0.036***	0.219***	0.189***	0.158***	0.128***	0.097***	0.066***
	(0.015)	(0.026)	(0.021)	(0.019)	(0.017)	(0.015)	(0.013)	(0.011)	(0.034)	(0.031)	(0.028)	(0.026)	(0.024)	(0.023)
3 years after event	0.117***	0.160***	0.211***	0.182***	0.153***	0.125***	0.096***	0.068***	0.264***	0.232***	0.201***	0.169***	0.137***	0.105***
	(0.015)	(0.034)	(0.021)	(0.019)	(0.017)	(0.015)	(0.013)	(0.012)	(0.043)	(0.040)	(0.037)	(0.035)	(0.033)	(0.031)
4 years after event	0.109***	0.163***	0.195***	0.168***	0.142***	0.116***	0.090***	0.063***	0.263***	0.232***	0.202***	0.171***	0.141***	0.110***
	(0.014)	(0.042)	(0.019)	(0.017)	(0.016)	(0.014)	(0.013)	(0.012)	(0.053)	(0.049)	(0.046)	(0.043)	(0.040)	(0.038)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No
# Observations	63,078	20,491	63,078	63,078	63,078	63,078	63,078	63,078	20,491	20,491	20,491	20,491	20,491	20,491

Notes: Table A.17 shows the results of running specification (2.1) with the measure of *adjusted sales to others* as the dependent variable. For this table, total sales and sales to others are based only on the firm-to-firm transaction data. Here, total sales are replaced by total corporate sales, i.e., the sum of all sales recorded by the firm-to-firm transaction data in a year. Also, sales to others are replaced by the sum of all sales recorded by the firm-to-firm transaction data, made to buyers other than the first MNC buyer. Each column implements Result 1 for a different value of δ, as indicated above the column number. Columns (1) and (2) show our baseline findings for $\delta = -0.217$ and for the two samples (full and restricted) - this estimate of δ comes from the instrumental variable strategy described in Section 2.5.3 and implemented in Table A.15. The rest of the columns show the robustness of our baseline findings to values of $\delta \in [-1.2, 0.3]$. For $\sigma = 6$, the corresponding values of $\gamma \in [0.76, 1.06]$. Columns (3)-(8) report event-study estimates for the full sample including both domestic firms that become first-time suppliers to an MNC and never-suppliers. Clustering of standard errors is at the two-digit sector by province level. Columns (9)-(14) focus only on the restricted sample of domestic firms that become first-time suppliers to an MNC and use standard error clustering at event by province level. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.18: Robustness of the Empirical Application of Result 2 to Different Values of δ - Transaction Data Only

	$\delta = -0.22$ (1)	$\delta = -0.22$ (2)	$\delta = -1.2$ (3)	$\delta = -0.9$ (4)	$\delta = -0.6$ (5)	$\delta = -0.3$ (6)	$\delta = 0$ (7)	$\delta = 0.3$ (8)	$\delta = -1.2$ (9)	$\delta = -0.9$ (10)	$\delta = -0.6$ (11)	$\delta = -0.3$ (12)	$\delta = 0$ (13)	$\delta = 0.3$ (14)
4 years before event	0.008	-0.021	0.016	0.014	0.011	0.009	0.007	0.005	-0.028	-0.026	-0.024	-0.022	-0.019	-0.017
	(0.014)	(0.028)	(0.027)	(0.023)	(0.019)	(0.015)	(0.012)	(0.009)	(0.036)	(0.033)	(0.031)	(0.029)	(0.027)	(0.027)
3 years before event	0.004	-0.018	0.009	0.007	0.006	0.004	0.003	0.001	-0.020	-0.020	-0.019	-0.018	-0.018	-0.017
	(0.008)	(0.020)	(0.013)	(0.011)	(0.009)	(0.008)	(0.007)	(0.007)	(0.026)	(0.024)	(0.022)	(0.020)	(0.019)	(0.018)
2 years before event	0.007	-0.006	0.015	0.013	0.010	0.007	0.005	0.002	-0.004	-0.005	-0.005	-0.006	-0.006	-0.007
	(0.006)	(0.010)	(0.009)	(0.008)	(0.007)	(0.006)	(0.005)	(0.005)	(0.013)	(0.012)	(0.011)	(0.010)	(0.010)	(0.010)
Year of event	-0.142***	-0.119***	-0.081***	-0.100***	-0.118***	-0.137***	-0.156***	-0.174***	-0.055***	-0.074***	-0.094***	-0.114***	-0.133***	-0.153***
	(0.017)	(0.014)	(0.020)	(0.019)	(0.018)	(0.017)	(0.017)	(0.016)	(0.016)	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)
1 year after event	0.006	0.035*	0.095***	0.068***	0.040***	0.013	-0.014	-0.041***	0.130***	0.101***	0.072***	0.043**	0.014	-0.015
	(0.011)	(0.018)	(0.018)	(0.016)	(0.013)	(0.011)	(0.009)	(0.008)	(0.022)	(0.021)	(0.019)	(0.018)	(0.018)	(0.018)
2 years after event	0.027**	0.063***	0.118***	0.090***	0.062***	0.035***	0.007	-0.021**	0.163***	0.132***	0.102***	0.071***	0.040*	0.010
	(0.013)	(0.023)	(0.019)	(0.017)	(0.015)	(0.013)	(0.011)	(0.010)	(0.030)	(0.027)	(0.025)	(0.024)	(0.022)	(0.022)
3 years after event	0.056***	0.098***	0.149***	0.120***	0.092***	0.063***	0.035***	0.006	0.202***	0.170***	0.138***	0.106***	0.074**	0.042
	(0.012)	(0.033)	(0.019)	(0.017)	(0.015)	(0.013)	(0.011)	(0.010)	(0.039)	(0.037)	(0.035)	(0.033)	(0.032)	(0.032)
4 years after event	0.047***	0.099**	0.133***	0.107***	0.081***	0.054***	0.028**	0.002	0.199***	0.168***	0.138***	0.107***	0.077*	0.046
	(0.012)	(0.039)	(0.016)	(0.015)	(0.013)	(0.012)	(0.011)	(0.011)	(0.047)	(0.044)	(0.042)	(0.040)	(0.038)	(0.038)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-4DSect-Prov FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Never Suppliers	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No	No
# Observations	63,078	20,491	63,078	63,078	63,078	63,078	63,078	63,078	20,491	20,491	20,491	20,491	20,491	20,491

Notes: Table A.18 shows the results of running specification (2.1) with the measure of *average adjusted sales to others* as the dependent variable. For this table, total sales and sales to others are based only on the firm-to-firm transaction data. Here, total sales are replaced by total corporate sales, i.e., the sum of all sales recorded by the firm-to-firm transaction data in a year. Also, sales to others are replaced by the sum of all sales recorded by the firm-to-firm transaction data, made to buyers other than the first MNC buyer. The number of other buyers is the number of buyers recorded by the firm-to-firm transaction data, other than the first MNC buyer. Each column implements Result 2 for a different value of δ, as indicated above the column number. Columns (1) and (2) show our baseline findings for $\delta = -0.217$ and for the two samples (full and restricted) - this estimate of δ comes from the instrumental variable strategy described in Section 2.5.3 and implemented in Table A.15. The rest of the columns show the robustness of our baseline findings to values of $\delta \in [-1.2, 0.3]$. For $\sigma = 6$, the corresponding values of $\gamma \in [0.76, 1.06]$. Columns (3)-(8) report event-study estimates for the full sample including both domestic firms that become first-time suppliers to an MNC and never-suppliers. Clustering of standard errors is at the two-digit sector by province level. Columns (9)-(14) focus only on the restricted sample of domestic firms that become first-time suppliers to an MNC and use standard error clustering at event by province level. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

A.4.3 Inferring γ and σ from DLW (2012)

Table A.19: Inferred γ and σ from the Method of De Loecker and Warzynski (2012)

	Labor (1)	Capital (2)	μ (3)	γ (4)	σ (5)	δ (6)	Obs (7)
All sectors (pooled)	0.84	0.08	1.25	0.92	5.03	-0.32	82,094
	(0.00)	(0.00)	(0.00)	(0.00)	(0.08)	(0.02)	
Agriculture, forestry and fishing	0.68	0.09	1.12	0.77	9.20	-1.87	5,229
	(0.01)	(0.01)	(0.02)	(0.01)	(1.38)	(0.41)	
Manufacturing	0.88	0.08	1.19	0.96	6.21	-0.21	14,922
	(0.02)	(0.00)	(0.03)	(0.02)	(0.83)	(0.15)	
Wholesale and Retail Trade	0.81	0.08	1.25	0.88	4.98	-0.46	42,033
	(0.00)	(0.01)	(0.01)	(0.01)	(0.11)	(0.04)	
Transportation and Storage	1.00	0.04	1.57	1.03	2.74	0.06	1,375
	(0.11)	(0.06)	(0.18)	(0.12)	(1.98)	(0.43)	
Accommodation and Food Services	0.77	0.07	1.05	0.84	20.88	-3.23	9,280
	(0.04)	(0.01)	(0.05)	(0.03)	(8.46)	(1.53)	
Information and Communication	0.82	0.08	1.21	0.90	5.87	-0.48	896
	(0.16)	(0.06)	(0.25)	(0.14)	(24.62)	(5.24)	
Professional, Scientific and Technical	0.88	0.09	1.29	0.98	4.44	-0.08	3,432
	(0.01)	(0.01)	(0.02)	(0.02)	(0.30)	(0.06)	
Administrative and Support Service	0.88	0.05	1.21	0.93	5.80	-0.32	1,998
	(0.03)	(0.02)	(0.04)	(0.03)	(1.51)	(0.31)	
Human Health and Social Work	0.86	0.09	1.36	0.95	3.81	-0.14	861
	(0.18)	(0.06)	(0.29)	(0.16)	(7.61)	(1.79)	
Other Services	0.85	0.02	1.26	0.83	4.92	-0.68	1,275
	(0.18)	(0.08)	(0.31)	(0.17)	(13.84)	(4.17)	

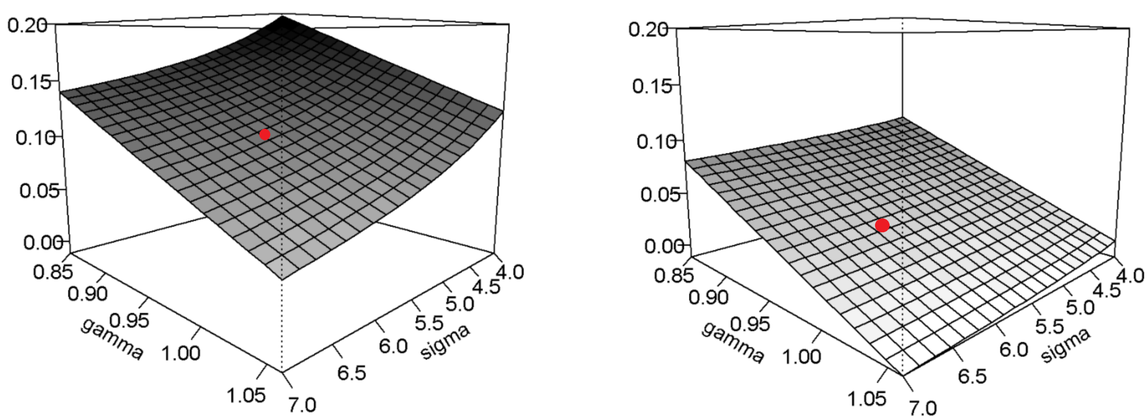
Notes: Table A.19 shows results from the De Loecker and Warzynski (2012) methodology for the economy-wide sample, pooled across all sectors and separately by sector. Column (1) and (2) show the estimated input elasticities for labor and capital in a Cobb-Douglas value-added production function. Column (3) shows the mark-up (μ). Column (4) corresponds to the returns to scale parameter (γ), which is calculated as the sum of columns (1) and (2). Column (5) corresponds to the inferred elasticity of demand (σ). Our assumption of CES demand for buyers implies a constant mark-up over marginal cost given by $\mu = \frac{\sigma}{\sigma-1}$, which allows us to infer σ from our estimated μ. Column (6) computes the resulting value for $\delta = (\gamma-1)(\sigma-1)$. Finally, column (7) reports the number of observations. This methodology implies values of $\delta \in [-1.87, 0.06]$ across sectors. The estimation based on all sectors implies $\delta = -0.33$, which is close to $\delta = -0.22$ estimated with our IV strategy. Bootstrap standard errors are shown in parenthesis.

One concern with our preferred values of σ and γ is that σ is taken from the literature, whereas γ is inferred from the sample of first-time suppliers to the government (and not to

MNCs). Using the method of De Loecker and Warzynski (2012), we can infer both σ and γ from the same baseline sample of first-time suppliers to MNCs. By assuming a Cobb-Douglas production function specification, we estimate the returns to scale parameter (γ) and the mark-up of firms (μ). Under our CES assumption for the demand system, we then infer the elasticity of demand (σ) from the mark-up, since the mark-up is given by $\mu = \frac{\sigma}{\sigma-1}$.

Using this approach we estimate an average mark-up across sectors of 1.25 (25% over marginal cost). This implies $\sigma = 5$, which is close to the value of 6 from Broda and Weinstein (2006) that we use in our baseline findings. We also find returns to scale of the production function $\gamma = 0.92$. With these estimates in hand, we obtain $\delta = (\gamma - 1)(\sigma - 1) = -0.33$. This estimate of δ is close to the one obtained from the full sample using the IV methodology (see Table A.15, A.4.2). Reassuringly, the findings from this approach are similar to our baseline findings. That said, this approach is not our first choice, since we aim to provide an alternative to the standard approach of production function estimation.

A.4.4 Robustness of Model-Based Results to γ and σ



(a) Result 1, Composite TFP

(b) Result 2, TFP

Figure A.5: Estimates of Composite TFP and TFP Alone for Different Values of σ and γ

Notes: Figure A.5 presents the estimated changes in two measures of TFP (vertical axis): composite TFP (Panel A.5a) and true TFP (Panel A.5b), for different calibrations of the relevant parameters γ (returns to scale) and σ (elasticity of demand). These graphs are the empirical applications of Results 1 and 2. For comparability, they are both constructed using only sales to corporate buyers, from the firm-to-firm transaction data. The axis on the left considers values of γ between 0.85 and 1.05. The axis on the right considers values of σ between 4 and 7. The red dots correspond to our baselines estimates obtained from $\gamma = 0.96$ and $\sigma = 6$ (associated with $\delta = -0.22$).

Instead of estimating γ and σ ourselves (or taking a value of σ from the literature), we investigate here the sensitivity of our baseline model-based results to reasonable ranges of values for these parameters. Figure A.5 presents the estimates of the model-based composite TFP and TFP alone according to either Result 1 or Result 2 for different calibrations of γ and σ. The two ranges considered include both of our preferred values of γ and σ (0.96 and 6, respectively, for $\delta = -0.22$) that deliver our baseline results in Table 2.6.

A.4.5 Discussion of Assumption (a-ii)

In Section 2.5.2, we assume that for any changes in ϕ and/or r, all buyers i equally adjust their probability to source from the supplier, i.e., $\mathsf{d}\ln(n_i) = \mathsf{d}\ln(n)$, $\forall i \neq MNC_0$. This assumption (labeled as assumption (a-ii) in A.3.3) is instrumental to reaching Result 2.

Let us now relax this assumption. Define $\omega_i = \frac{n_i b_i}{\sum_{k\neq MNC_0}^{N} n_k b_k}$ and $\omega_i' = \frac{n_i}{\sum_{k\neq MNC_0}^{N} n_k}$. Using the definition of $\varepsilon_{\widetilde{n}}$ (see equation (A.7)) and taking the total derivative of $\ln(\widetilde{N})$, we obtain:

$$
\begin{aligned}
\varepsilon_{\widetilde{n}} =& \mathbb{E}\left[\sum_{i\neq MNC_0}^{N} \mathsf{d}\ln(n_i)\omega_i\right] \\
\mathbb{E}\left[\mathsf{d}\ln(\widetilde{N})\right] =& \mathbb{E}\left[\sum_{i\neq MNC_0}^{N} \mathsf{d}\ln(n_i)\omega_i'\right] \\
\Rightarrow \varepsilon_{\widetilde{n}} =& \mathbb{E}\left[\mathsf{d}\ln(\widetilde{N})\right] + \mathbb{E}\left[\sum_{i\neq MNC_0}^{N} \mathsf{d}\ln(n_i)\times(\omega_i - \omega_i')\right]. \qquad \text{(A.10)}
\end{aligned}
$$

Equation (A.10) tells us that, in the general case where $\mathsf{d}\ln(n_i)$ depends on the buyer i, $\varepsilon_{\widetilde{n}}$ and $\mathbb{E}\left[\mathsf{d}\ln(\widetilde{N})\right]$ need not be equal. Without assumption (a-ii) equation (A.9) can be written as:

$$
\mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\widetilde{Q}/\left(pQ\right)^{\delta}}{\widetilde{N}}\right)\right] = (\sigma-1)\varepsilon_{\phi} + \mathbb{E}\left[\sum_{i\neq MNC_0}^{N} \mathsf{d}\ln(n_i)\times(\omega_i - \omega_i')\right] = (\sigma-1)\varepsilon_{\phi} + \tilde{\varepsilon}.
$$

Whenever assumption (a-ii) does not hold, $\tilde{\varepsilon}$ is likely to add a bias to Result 2. The sign of $\tilde{\varepsilon}$ depends on the covariance between $\mathsf{d}\ln(n_i)$ and $(\omega_i - \omega_i')$. Given the definitions of ω_i and ω_i', we have that $(\omega_i - \omega_i') > 0$ if and only if $\sum_k (b_i - b_k)n_k > 0$. Thus, the sign of $\tilde{\varepsilon}$ would ultimately depend on the covariance between $\mathsf{d}\ln(n_i)$ and $\sum_k (b_i - b_k)n_k$. This covariance would be positive (negative) if the change in the probability of matching with a given buyer ($\mathsf{d}\ln(n_i)$) would be higher for buyers with bigger (smaller) demand shifters (b_i) than that of the average buyer. In summary:

$$
\mathbb{E}\left[\mathsf{d}\ln\left(\frac{p\widetilde{Q}/\left(pQ\right)^{\delta}}{\widetilde{N}}\right)\right] \quad \begin{cases} > (\sigma-1)\varepsilon_{\phi} & \text{if Cov}\left[\mathsf{d}\ln(n_i)\ ,\ \sum_k (b_i - b_k)n_k\right] > 0 \\ < (\sigma-1)\varepsilon_{\phi} & \text{if Cov}\left[\mathsf{d}\ln(n_i)\ ,\ \sum_k (b_i - b_k)n_k\right] < 0 \end{cases} \qquad \text{(A.11)}
$$

Result 2 would provide an upper (lower) bound of the importance of ε_{ϕ} in $\varepsilon_{\phi'}$ if the first (second) case of equation (A.11) were the relevant one to our context. We are now interested in investigating whether indeed all buyers i equally adjust their probability to source from the supplier, i.e., $\mathsf{d}\ln(n_i) = \mathsf{d}\ln(n)$, $\forall i \neq MNC_0$. In our model, the only characteristic of buyers that differentiates them is their demand shifter b_i. We now ask whether $\mathsf{d}\ln(n_i)$ may be correlated with b_i. As we do not observe b_i directly, we use firm size as a proxy. Table A.6 (A.1) shows that the average size of buyers increases after the event (column (2) for average employment and column (3) for average sales). This suggests that the probability of selling to buyers with higher than average demand shifters increased relatively more than the one of selling to buyers with lower than average demand shifters. The first case of equation (A.11) is therefore more likely to apply to our setup. Hence, the importance of ε_ϕ in $\varepsilon_{\phi'}$ might be smaller than our baseline model-based estimates suggest.

A.5 Summary Statistics for Main Sample

	N	Mean	S.D.	Median
Never Suppliers in 2009				
Total Sales	8,389	676.7	1,740.0	292.2
Number of Workers	8,389	11.6	28.7	6.0
Wage bill	8,389	79.0	299.7	31.8
Exports	201	891.1	1,430.5	246.4
Imports	1,268	207.2	619.9	48.4
Value Added	7,940	154.9	462.9	58.7
Input Costs	4,938	601.2	1,477.8	232.2
Total Net Assets	6,641	448.2	1,673.6	134.1
First-Time Suppliers in 2009 (Unbalanced)				
Total Sales	1,555	1,495.8	4,321.4	477.5
Number of Workers	1,555	19.5	45.1	7.8
Wage bill	1,555	131.5	311.6	47.3
Exports	111	742.8	2,131.0	57.0
Imports	454	567.9	1,863.2	111.3
Value Added	1,475	203.1	471.3	69.4
Input Costs	1,040	1,431.7	4,259.9	379.3
Total Net Assets	1,442	926.9	2,519.6	254.1
First-Time Suppliers in 2009 (Balanced)				
Total Sales	1,520	1,516.5	4,367.4	483.6
Number of Workers	1,520	19.6	45.3	7.9
Wage bill	1,520	132.7	314.4	47.5
Exports	110	749.5	2,139.5	57.8
Imports	446	574.4	1,878.5	113.7
Value Added	1,443	205.0	475.3	70.8
Input Costs	1,016	1,456.4	4,306.0	396.9
Total Net Assets	1,411	938.5	2,542.9	257.4

Table A.20: Summary Statistics for the Firms in the Main Economy-Wide Sample

Notes: Except for the number of employees, all means, standard deviations, and medians are in thousands of CPI-deflated 2013 U.S. dollars. Statistics for each variable are calculated only across the firms with non-missing values for that variable that year. All values correspond to 2009, a year that is by construction prior to all events in the main economy-wide sample. Part of the firms in the overall main sample were not yet active in 2009, which explains the difference in the number of firms described in this table and the overall number of firms in the main economy-wide sample. The upper panel presents raw summary statistics for the sample of firms active in 2009 and never observed as supplying to an MNC in our 2008 to 2017 firm-to-firm transaction data. The middle panel presents raw summary statistics for the sample of firms active in 2009 and observed as supplying for the first time to an MNC in Costa Rica sometime between 2010 and 2015. In 2009, there were 15,788 firms that satisfy our minimal size restrictions and that are split in three disjoint sets: 8,389 are never-suppliers (upper panel), 1,555 will become first-time suppliers sometime between 2010 and 2015, 5,844 are observed as already supplying to an MNC in either 2008 or 2009. Firms observed as supplying for the first time to an MNC after 2016 are dropped altogether from this calculation. The lower panel presents raw summary statistics for the sample of firms active in 2009, observed as supplying for the first time to an MNC in Costa Rica sometime between 2010 and 2015, and observed at least one year before and after their event.

	Suppliers (Events)	MNCs (New, unique)	MNCs (Total, unique)
2010	761	263	263
2011	665	71	332
2012	646	43	372
2013	539	31	400
2014	517	19	421
2015	569	17	436
Total	3,697	444	

Table A.21: Number of Events (First-Time Suppliers to MNCs) and MNCs Triggering Them

Notes: Table A.21 describes the main economy-wide sample of firms observed as supplying for the first time to an MNC in Costa Rica sometime between 2010 and 2015. The second column reports the number of events that occur in each calendar year, or alternatively, the number of domestic firms that become first-time suppliers to an MNC that year. The third column reports the total number of new and unique MNCs that trigger an event in each calendar year, with the total showing the number of unique MNCs that we observe in the baseline sample. The fourth column shows the number of unique MNCs that trigger an event in each calendar year. Since MNCs may trigger events in multiple years, a total is not presented for this column. By definition, the values in the first row of the third and fourth columns are identical. The interpretation of the number 71 in the third column is the following: of the 332 unique MNCs that trigger the 665 events of 2011, 71 MNCs are new with respect to the 263 MNCs triggering events in 2010.

Country of GUO	Frequency	Percentage
United States	209	47.1
Panama	28	6.3
Great Britain	18	4.1
Spain	17	3.8
Mexico	17	3.8
Switzerland	13	2.9
Colombia	13	2.9
Germany	11	2.5
France	11	2.5
Canada	10	2.3
...	...	...
Total	444	100

Table A.22: Country of Global Ultimate Ownership for the MNCs Triggering the Event

Notes: Table A.22 documents the ten most frequent countries of global ultimate ownership (GUO) for the MNCs triggering the events in our main economy-wide sample. Other origin countries are as follows: Japan (9 MNCs), Guatemala (8), Netherlands (8), El Salvador (8), Ireland (6), Venezuela (5), Belgium (4), China (4), and Nicaragua (4). Together they cover 403 of the 444 distinct MNCs. Each observation is a unique MNC. Since one MNC can trigger multiple events, the frequency of each country in the sample of unique MNCs is likely to differ from the frequency of each country in the sample of events (triggered by these MNCs).

	Suppliers	MNCs
Agriculture, Forestry and Fishing	7.91	7.82
Manufacturing	9.47	39.92
Wholesale and Retail Trade	35.11	19.31
Transportation and Storage	5.91	3.49
Accommodation and Food Services	15.93	6.22
Information and Communication	2.63	3.76
Professional, Scientific and Technical	8.56	3.52
Administrative and Support Service	6.85	7.68
Human Health and Social Work	2.93	0.73
Art, Entertainment and Recreation	1.50	0.46
Other Services	3.06	0.05
Mining and Quarrying	0.15	0.03
Water Supply, Sewerage and Waste Management	-	0.24
Construction	-	0.87
Real Estate	-	4.00
Education	-	1.89

Table A.23: Sectoral Composition of the Sample of First-Time Suppliers and MNCs

Notes: Table A.23 presents the share of firms in a given sector of the 3,697 first-time suppliers to an MNC in the first column, and of their first 444 MNC buyers in the second column. Both types of firms pertain to the main economy-wide sample.

Variable	N	Mean	Median	S.D.
First transaction with MNC ($\times$ 1,000 U.S. dollars)	3,697	62.40	18.59	110.31
Length of relationship with first MNC buyer	3,697	2.76	2.00	1.91
Length of relationship with all MNC buyers	3,697	3.69	3.00	2.11

Table A.24: Characteristics of Amount and Length of Relationship with First MNC Buyer

Notes: Table A.24 refers to all economy-wide domestic firms observed as supplying for the first time to an MNC in Costa Rica sometime between 2010 and 2015. The first line presents descriptive statistics of the first transaction with an MNC buyer. The second line describe the length of that relationship with the first MNC buyer, while the third line describes the length of relationships with all MNC buyers (including both the first MNC buyer and subsequent ones). Note that both of the duration variables are top censored, hence underestimated. For instance, for firms first supplying to an MNC in 2015 we can observe only two years more of their firm-to-firm transactions.

Calendar Year / Event Year	*0*	*+1*	*+2*	*+3*	*+4*	*+5*	*+6*	*+7*
2010	761	636	563	480	414	332	266	197
2011	665	549	453	383	335	273	211	
2012	646	525	430	353	290	223		
2013	539	446	360	304	235			
2014	517	397	327	252				
2015	569	407	316					
Total	3,697	2,960	2,449	1,772	1,274	828	477	197

Table A.25: Number of Firms Still Supplying to at Least One MNC Buyer in a Given Event Year

Notes: Table A.25 refers to all economy-wide domestic firms observed as supplying for the first time to an MNC in Costa Rica sometime between 2010 and 2015. The second column reports the distribution, by calendar year, of our events. By construction, in event year *0*, all firms that become a first-time supplier to an MNC have to appear in the calendar year row of their event year. Thus, by construction, the total number of firms in the column of event year *0* has to be 3,697. In the column of event year *+1*, we can trace how many of the firms who experience the event in a given calendar year are still selling to at least one MNC buyer one year after their event. The last column describes the number of firms that still supply to MNCs seven years after their first sale to an MNC. As one can note, by construction, some cells are empty. For instance, we cannot observe firms that are first supplying to an MNC in 2013 (hence have event year *0* as 2013) in event year *+5*, as our firm-to-firm does not allow us to observe those firms in 2018 (as our firm-to-firm dataset spans 2008 to 2017).

Event Year	Number of Suppliers (1)	Number of MNC Buyers Mean (2)	Median (3)	S.D. (4)
0	3,697	1.44	1.00	1.34
+1	2,960	1.92	1.00	2.02
+2	2,449	2.25	1.00	2.66
+3	1,772	2.62	1.00	3.32
+4	1,274	2.89	2.00	3.90
+5	828	3.15	2.00	4.38
+6	477	3.64	2.00	5.73
+7	197	4.02	2.00	7.02

Table A.26: Number of MNC Buyers in a Given Event Year

Notes: Table A.26 refers to all economy-wide domestic firms observed as supplying for the first time to an MNC in Costa Rica sometime between 2010 and 2015. For each event year $+k$, we show summary statistics of the number of MNC buyers (columns (2)-(4)) for domestic firms still supplying to an MNC $+k$ years later, as opposed to all firms still active $+k$ years later (column (1)).

Event Year	N	Mean	Median	S.D.
0	3,697	0.19	0.06	0.27
+1	2,960	0.22	0.08	0.29
+2	2,449	0.23	0.10	0.29
+3	1,772	0.25	0.11	0.29
+4	1,274	0.25	0.11	0.29
+5	828	0.25	0.13	0.29
+6	477	0.26	0.14	0.29
+7	197	0.26	0.12	0.30

Table A.27: Share of Total Sales Going to MNC Buyers in a Given Event Year

Notes: Table A.27 refers to all economy-wide domestic firms observed as supplying for the first time to an MNC in Costa Rica sometime between 2010 and 2015. For each event year $+k$, we show summary statistics of the share of total sales directed to MNC buyers for domestic firms still supplying to an MNC $+k$ years later (as opposed to all firms still active $+k$ years later).

A.6 Data Construction and Statistics

A.6.1 Administrative Data

All the administrative data described hereafter is confidential and could only be stored and accessed in person in a fully-secured location at the Central Bank of Costa Rica (BCCR).

Corporate Income Tax Returns and Social Security Data

Our first administrative dataset contains the universe of corporate income tax returns of active firms over the 2008 to 2017 period. Firms are corporations or individuals conducting business in Costa Rica. Every firm must file yearly tax declarations called D-101 (*"Declaracion Jurada del Impuesto Sobre la Renta"* or the "Affidavit of Income Tax") to the Ministry of Finance (*Ministerio de Hacienda*). This form contains information on profits, revenues, costs, assets, among others. Costs are broken down into several components such as administrative costs, material inputs, capital depreciation, interest payments, and other costs. Not filing the D-101 leads to payments of fines of up to 385 U.S. dollars, plus 11 to 12% annual interest on the firm's income tax liability. At this point, we refer to firms and tax identifiers (IDs) interchangeably.

We use the tax ID to merge the corporate income tax returns data with data from the Costa Rican Social Security Fund (*"Caja Costarricense del Seguro Social"*). This adds two new variables: the number of employees and the total wage bill. Tax IDs that report data to the Social Security at some point between 2008 and 2017 are considered active and kept for analysis.

The challenge going forward is that a given firm may have several tax IDs, most frequently due to accounting or tax reasons. Given that our paper is centered on trade between firms, we need to aggregate all data up to the firm level.

Hence, we add to the information from the two datasets above information on firm ownership and management from BCCR and other sources. BCCR identifies groups of tax identifiers (IDs) that have common owners using data from the National Registry of firms, domestic and foreign surveys, and other public and private information. These groups of tax IDs are called "grupo corporativo" or *corporate group*. A "grupo empresarial" or *firm group* is a set of tax IDs who not only share ownership, but also behave as one firm, meaning that one cannot consider them as separate business ventures.[5] This information is complemented with information on corporate groups from Orbis, a product of Bureau Van Dijk.[6]

We add to the same firm group those tax IDs that belong to the same corporate group and also operate in the same sector as the tax IDs in the firm group. We expand our dataset with the tax returns of tax IDs that lack social security data, if we learn that these tax IDs are part of a corporate group.[7]

For the purpose of our empirical analysis, we collapse the data and treat firm groups as one individual firm. We keep track of business relationships of all tax IDs in the group with all other tax IDs in the economy, but keep only one identifier for the group. We keep the fixed characteristics (identifier, sector, location) of the most relevant tax IDs in terms of sales within the group. For all other variables, values are summed across all tax identifiers under the same firm group identifier.

We want to keep the universe of domestic private firms that are part of the non-financial market economy. Therefore, we drop non-governmental organizations (NGOs), public entities (including utilities), and those observations that are registered as households. We drop data from the education sector and the construction / real estate sector,[8] as well as firms related to the financial sector. We drop firms for which we do not know either the sector or the province, as both are necessary in our event-study design. We do not keep firms for which there is less than one worker reported during all years of activity. These criteria leave us with 78,137 firms.

[5] In a hypothetical example, tax IDs A, B, and C belong to the same "grupo empresarial" or "firm group". While these tax IDs are distinct, they operate as a single business unit whose objective is to sell product z in Costa Rica. Assume that all sales are reported by tax ID A, all workers are hired by tax ID B, and tax ID C owns all the assets. Not aggregating the information of these three tax IDs up to the firm group level but treating tax IDs as distinct firms would lead to an overestimation of the number of firms in the economy and misleading conclusions on the behavior of each tax ID.

[6] This dataset is discussed in more detail in A.6.1.

[7] For instance, this can include firms that report large revenues, but do not report any employees.

[8] Most of these firms are active for one construction project only, disappearing immediately after.

Table A.28: Descriptive Statistics, All Domestic Firms Vs. Domestic Firms Kept After Minimal Size Restrictions

	# Firms	Mean	S.D.	Median
Domestic non-financial market economy				
Total Sales	78,137	495.1	3,114.9	118.3
Number of Workers	76,372	7.2	32.2	2.4
Wage Bill	76,650	53.4	300.7	12.6
Exports	4,487	451.7	2,804.2	23.6
Imports	21,521	224.1	1,579.7	13.8
Value Added	74,985	113.8	590.2	34.9
Input Costs	67,739	320.8	2,542.3	24.6
Total Net Assets	69,098	407.1	5,825.3	55.7
Domestic firms kept after min. size restr.				
Total Sales	24,370	1,242.1	5,345.5	380.1
Number of Workers	24,370	17.1	53.0	6.7
Wage Bill	24,370	135.6	497.3	42.3
Exports	2,846	546.5	3,361.0	13.7
Imports	9,195	439.3	2,333.3	22.0
Value Added	24,233	243.8	962.4	86.2
Input Costs	16,881	1,091.3	4,930.1	264.4
Total Net Assets	21,654	952.2	7,940.9	193.1

Notes: Table A.28 reports summary statistics across 2008 to 2017 across eight variables for all firms in the non-financial market economy (upper panel) and for all firms kept in our sample of analysis (lower panel). With the exception of employment, the mean, standard deviation, and median are in thousands of U.S. dollars (CPI-deflated to 2013 dollars).

We impose minimal size restrictions for the sample considered in our empirical exercise. Firms have to report both workers and sales with no gaps in the data, and we consider only firms that, over the years, have a median of at least three workers. Finally, we drop firms with median sales of less than 50,000 U.S. dollars (CPI-deflated to 2013 dollars). These restrictions leave us with 24,370 firms. Note that these 24,370 domestic firms include four types of firms: the never-suppliers (never supplying to an MNC between 2008 and 2017), the first-time suppliers to an MNC sometime between 2010 and 2015, the always-suppliers (already supplying to an MNC in either 2008 or 2009), and the first-time suppliers in either 2016 or 2017. Of these 24,370 firms, in the main economy-wide event-study, we only use the firms in the first two categories. In Table A.28 we present descriptive statistics of the same eight variables from Table A.20 for all firms in the non-financial market economy (upper panel) and for all 24,370 firms surpassing our minimal size restrictions (lower panel).

Table A.29: Coverage of Data After Minimal Size Restrictions

Total Sales	78.6%
Number of Workers	81.7%
Wage Bill	84.2%
Exports	83.1%
Imports	89.3%
Value Added	76.2%
Input Costs	82.0%
Total Net Assets	73.5%

Notes: Table A.29 presents the average coverage between 2008 to 2017 of the values for all firms kept after implementing our minimal size restrictions out the values for all firms in the non-financial market economy (across eight variables).

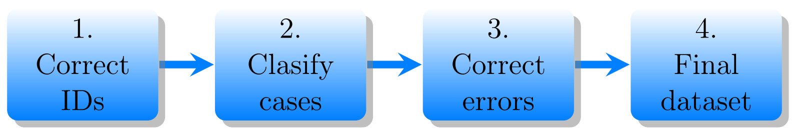
Despite losing more than two thirds of the firms, Table A.29 shows that we keep those that employ most of the labor force and represent the largest share of sales, exports, income, costs and assets. For most variables, the firms we keep cover over 80% of the value across all firms in the non-financial market economy.

Firm-to-Firm Transaction Data

Our most important dataset allows us tracks all firm-to-firm relationships in Costa Rica between 2008 and 2017. This data is collected by the Ministry of Finance through the tax form D-151, the *"Declaración anual resumen de clientes, proveedores y gastos específicos"* (Declaration of the yearly summary of buyers, suppliers and specific expenses). This declaration is compulsory not only to private businesses, but to all actors in the economy (e.g. individuals providing professional services, public entities, NGOs, embassies etc.), irrespective of being subject to the corporate income tax or not. A late filing of this fee is heavily penalized, e.g. in 2016 the late filing fee could go from 7,040 to 70,400 U.S. dollars.

To help enforce taxes, each firm has to report all of its corporate suppliers and buyers with a yearly accumulated amount of transactions above 2.5 million Costa Rican colones (approximately 4,200 U.S. dollars).[9] Third-party reporting, of the type D-151 ensures, is used by the tax authority to identify firms that have not complied with their filing obligations, e.g. firms that have over-reported their costs or under-reported their revenues to reduce their profit tax liability. The tax authority uses different communication interventions, namely emails, phone calls, or personal visits, to follow up with non-filers (Brockmeyer, Hernandez, Kettle, and Smith, 2016). As D-151 forms contain the yearly amount sold to or bought from each partner, this dataset allows us not just to track buyer-supplier relationships in a given year, but also to measure the intensity of those relationships.

A sequence of steps was followed to ensure that several coding or reporting errors were corrected in the raw D-151 database, and that the IDs of firms identified as buyers and sellers are coherent with the rest of our data. The steps can be summarized as follows:



The first step relates to the fact that the Ministry of Finance usually assigns extra characters to the IDs of corporations or individuals, which need to be removed before the data can be linked to the tax returns and social security microdata. The presence of foreign IDs require additional steps to ensure data quality: it is not unusual that the initial transactions of a foreign firm are recorded using passport or foreign ID numbers, whereas, later on, those transactions are recorded using a Costa Rican tax ID. BCCR tracks those changes to ensure that the transactions are imputed to the correct tax ID when building the dataset.

The second step involves identifying different reporting inconsistencies. The ideal case is one in which the transaction between two firms is reported by both firms, given the same description, and has the exact same reported amount in both filings. In such case, the duplication is taken into consideration to keep it as one observation, and there is no need to perform any additional corrections. However, inconsistencies arise when transactions appear only once, the amount shown is different within a pair, submissions that were rejected by the Ministry of Finance cause duplicates of correct lines, or there is a lack of data. Also, whenever individuals buy from firms, individuals are not required to report that purchase, so around one fifth of the reports by firms have no counterpart but cannot be classified as an error or misreporting.

The corrections that were done to the dataset are summarized herafter:

1. Whenever the transaction was reported by both parts but with amounts appearing to differ because of an error in the position of the decimal point, historical data was used to identify the correct amount among the two options.

[9]For the sale of professional services by individuals, the threshold is less than 100 U.S. dollars.

2. Whenever a pair of transactions had one of the partners reporting a transaction with an amount of zero, the amount from the partner reporting a positive value was assumed to be correct. The same solution was used whenever one partner filled in either its own tax ID or the tax ID of its partner, instead of the value of their transaction.

3. Whenever the difference in the amount of a pair of transactions was more than 20% or more than 50 million colones (close to 100 thousand U.S. dollars), and one of the partners of the transaction reported a value of more than 500 million colones (close to 1 million U.S. dollars) careful manual checks were completed (using historical data to identify the correct value).[10]

4. Whenever a transaction appeared more than once because of a resubmission (usually for corrections), we only kept the most recent observation.

Tables A.30 and A.31 summarize the number of transactions and the corresponding value of the transactions that were analyzed, for three different years (as examples, the same analysis was carried out for all years between 2008 and 2017). For the empirical exercise we can use two sets of transactions: first, those showing up in pairs that were either matched perfectly in the raw data or with inconsistencies that were solved by the corrections explained beforehand. The second set of transactions that we can use are the cases where transactions had no partner, either because there was a reason for not having it as explained above, or because there is missing information.

Unsolved cases include those that could eventually be corrected but for which the value of the transaction is below our chosen threshold for manual checks. The second category of data that we cannot use are cases where transactions had no duplicate, but they are classified as rejected by the Ministry of Finance in the revision of the tax declaration submissions. There is a small set of transactions that we were able to identify as duplicates of others that are already considered in the data. Finally, the smallest set of transactions includes those that were excluded due to being mistakenly reported.[11]

At the end of all these efforts of data-checking and cleaning, we manage to use more than 80% of the transactions and value of the transactions coming from the raw D-151 forms. For the second half of the sample period, we manage to use over 90% of the data, which is consistent with firms learning how to file the D-151 form without mistakes. Moreover, the transactions that we lose are either rejected, duplicated, or excluded (especially during the first years of our sample). Hence, the dropped transactions relate to reporting errors, not real transactions. Additionally, the transactions that are not used because they are categorized as "unsolved" are usually less than 10% of the total. It should be noted that their value represents a slightly larger percentage; that is because some of their mistakes involve ignoring the decimal point, which can overestimate the values of the transaction by several orders of magnitude.

[10] This last criterion was added to prioritize which transactions would be manually checked.

[11] For example, the Ministry of Finance is aware that accounting firms sometimes mix up the forms of different buyer firms when submitting them to the tax authority, which are later rectified.

Table A.30: Number of Cases, Firm-to-firm Transaction Data

Type of case	2008 Count	2008 %	2012 Count	2012 %	2015 Count	2015 %
Data in pairs	535,863	41.9%	998,355	40.5%	1,383,820	42.2%
No partner and accepted	493,769	38.7%	1,256,978	51.0%	1,626,907	49.6%
Subtotal of used data	**1,029,632**	**80.6%**	**2,255,333**	**91.5%**	**3,010,727**	**91.9%**
Unsolved	128,599	10.1%	202,710	8.2%	251,499	7.7%
No partner and rejected	108,969	8.5%	-	0.0%	-	0.0%
Duplicate	4,904	0.4%	5,936	0.2%	14,652	0.4%
Excluded	5,414	0.4%	34	0.0%	32	0.0%
Total	**1,277,518**	**100.0%**	**2,464,013**	**100.0%**	**3,276,910**	**100.0%**

Table A.31: Value of Transactions, Firm-to-firm Transaction Data

Type of case	2008 Value	2008 %	2012 Value	2012 %	2015 Value	2015 %
Data in pairs	45,812	63.6%	55,489	67.5%	69,450	69.1%
No partner and accepted	11,808	16.4%	16,637	20.2%	18,496	18.4%
Subtotal of used data	**57,620**	**80.0%**	**72,126**	**87.7%**	**87,946**	**87.6%**
Unsolved	7,766	10.8%	10,002	12.2%	12,324	12.3%
No partner and rejected	6,145	8.5%	-	0.0%	-	0.0%
Duplicate	170	0.2%	71	0.1%	172	0.2%
Excluded	359	0.5%	1	0.0%	2	0.0%
Total	**72,060**	**100.0%**	**82,200**	**100.0%**	**100,444**	**100.0%**

Notes: Values in millions of CPI-deflated 2013 U.S. dollars.

Moreover, in a related paper, we show that the behavior of the Costa Rican production network is similar to that of the production networks of Japan and Belgium (see Alfaro-Ureña, Fuentes, Manelici, and Vasquez, 2018b). This is reassuring as to quality of the firm-to-firm transaction data from Costa Rica.

As mentioned at the beginning of Section 2.3.1, we only consider “first-time supplying to an MNC" events occurring between 2010 and 2015. We choose 2010 as the starting year because we aim for a reliable measure of the year when a domestic firm sells to its first MNC buyer. 2008 was the first year when the D-151 tax form (the base for the firm-to-firm transaction dataset) could be filed electronically. However, as 2008 was the year of transition to the digitized form, firms were still allowed to file the form on paper. We therefore suspect that the 2008 dataset is incomplete.[12] Even if a firm is observed as selling to an MNC in 2009 but not in 2008, we cannot rule out that this firm was selling to MNCs in 2008 as well (filing the form on paper in 2008). To improve the measurement of the first year of supplying to an MNC, we treat as first matches only those occurring after 2010 for domestic firms that had not sold to an MNC in both 2008 (the year of transition to electronic filing) and 2009 (the first year mandatory electronic filing). We stop with 2015 to be able to observe each firm at least two years after its event.

Foreign Ownership Data

In Costa Rica, there is no centralized and complete reporting of the country of origin of firms' capital. To overcome this data limitation, we combine information from various sources.

Our first source is the reporting of firms that are active under the Free Trade Zone (FTZ) regime. Costa Rica has followed a strategy of pursuing FDI investment by offering benefits to firms established in FTZ regimes. As summarized in OECD (2017a), the FTZ regime exempts beneficiary firm from custom duties on imports and exports, the withholding tax (on royalties, fees, dividends), interest income, the sales tax on local purchases of goods and services and the stamp duty. In addition, the FTZ regime exempts profits from corporate income tax for eight years and provides a 50% corporate income tax reduction during the following four years, but differences exist depending on the types of activities and the location of the FTZ. Profits from sales to the domestic market are taxed under separate tax rules. Firms that may apply for the FTZ regime must be either (i) export service firms (at least 50% of services must be exported), (ii) scientific research firms (firms or organizations), (iii) "strategic firms" or part of "strategic sectors" or (iv) "significant suppliers" (at least 40% of their sales are made to FTZ firms). Due to those benefits, firms have to comply with full reporting of their sources of capital. This information is collected by Procomer and made available to BCCR for statistical purposes.

A complementary source of information is the Costa Rican Investment Promotion Agency (CINDE), which is a a private, non-profit organization that started its operations in 1982. CINDE has mediated the entry of more than 300 foreign-owned firms in Costa Rica, such as Intel, Procter&Gamble, Hewlett Packard, or St. Jude Medical.[13] CINDE shared with us information on the foreign ownership of firms they attracted. This set of foreign-owned firms contains both firms in the FTZ regime and firms that did not qualify for this regime.

Beyond the foreign-owned firms in FTZs and foreign-owned firms attracted by CINDE, there are limitations to the knowledge of foreign ownership of the remaining firms in the econ-

[12] This is likely to explain the lower data coverage for 2008 that we report in Tables A.30 and A.31.

[13] CINDE was awarded in 2018 for the fourth consecutive year as the "Best Investment Promotion Agency" of Latin America and the Caribbean in a ranking compiled by the *Site Selection* magazine.

omy. BCCR carries out three surveys that serve as sources of complementary information on flows and sources of capital for foreign-owned firms.

1. *Encuesta Trimestral de Balanza de Pagos*, or the "Quarterly Balance of Payments Survey": collects information on a sample of large firms (currently 250 to 300 firms) about their country of origin and percentage of foreign ownership.

2. *Encuesta Anual*, or the "Annual Survey": similar to the quarterly survey, but administered on a yearly basis. It contains a sample of 50 to 100 firms.

3. *Estudio Economico*, or the "Economic Study": when Costa Rica updated the system of national accounts, BCCR surveyed thousands of firms. Out of those, it identified and started tracking close to 944 firms having received foreign capital. For those firms, the "Economy Study" tracks the percentage of foreign ownership.

Our last source of information is Orbis, a commercial product belonging to Bureau Van Dijk.[14] We queried Orbis for all MNCs (*Global Ultimate Owners* in Orbis nomenclature) that have a presence (affiliate or branch) in Costa Rica, identifying the names and IDs of firms in Costa Rica and abroad, including intermediate ownership. As mentioned in A.6.1, Orbis allowed us to expand our knowledge of firm and corporate groups in Costa Rica. Orbis was also used to identify which of the foreign-owned firms in Costa Rica are actually part of an MNC group and which ones are single location firms. For foreign firms for which this information was not available in Orbis, we carried out extensive manual searches.

After cross-checking all sources, we have identified 3,855 tax IDs that are part of a corporate group in which there are tax IDs with partial or full foreign ownership. To obtain a sample comparable to that of our domestic firms, we apply the same criteria used in A.6.1. We exclude NGOs, governmental entities (e.g., embassies) and households, so as to focus on private firms alone. After adding the information on the different layers of shared ownership, we arrive to 2,171 firm groups that are part of a corporate group with at least partial foreign ownership (see A.6.1 for details on the difference between firm groups and corporate groups).

As motivated in Section 2.2.2, not all of these 2,171 firm groups are suitable for our analysis. Out of these 2,171 firm groups we create three mutually exclusive sets: (i) firm groups that are entirely domestically-owned (despite being part of corporate groups where another firm group is partially foreign-owned), (ii) firm groups that are themselves at least partially foreign-owned but whose median of workers is under 100 workers (across all years of activity in the country), and (iii) firm groups that are themselves at least partially foreign-owned and whose median of workers is over 100 workers.

Given our interest in measuring the performance gains of joining MNC supply chains, we focus on the 622 firm groups in category (iii), that are actual MNC affiliates and that have a

[14]The financial and balance sheet information in ORBIS comes from business registers collected by the local Chambers of Commerce to fulfill legal and administrative requirements (Kalemli-Ozcan, Sorensen, Villegas-Sanchez, Volosovych, and Yeşiltaş, 2015).

substantial economic presence in the country. The fully domestically-owned firm groups in category (i) operate in different sectors than those of firm groups that are partially-owned and part of their same corporate group. Given the loose connection between firm groups part of the same corporate group, particularly when not in the same sector, we do not consider them for analysis. The typical firm in category (ii) is not an MNC affiliate (but a single-location firm with partial foreign-ownership) and serves local demand, either in service sectors (e.g., hotels) or in sectors with low domestic input requirements (e.g., import/export retail or real estate agencies). For these reasons, we also do not consider firms in the category (ii) for analysis. Another important advantage of focusing only on firms in category (iii) is that it allows us to circumvent issues related to FDI statistics, such as the rising use of shell companies. Shell companies, or “special purpose entities (SPEs) are companies that do not have substantial economic activity in a country but are used by companies as devices to raise capital or to hold assets and liabilities. SPEs can lead to the inflation of FDI statistics" and obscure the ultimate purpose of FDI (OECD, 2017b).

In Table A.32 we present descriptive statistics for three types of firms (firm groups): (a) the sample of domestic private firms that are part of the non-financial market economy (if part of a corporate group, this group is fully domestically-owned), (b) firms that are part of a corporate group with partial foreign ownership that are not large MNC affiliates and not considered for analysis (puts together categories (i) and (ii) defined in the previous paragraph), or (c) the sample of MNC affiliates considered for analysis (category (iii) above). Category (a) is the same one described in Table A.28. The firms that are part of corporate groups with partial foreign ownership and that are excluded from the analysis are significantly larger than domestic firms, while (large) MNCs are themselves an order of magnitude larger than the excluded firms part of corporate groups with partial foreign ownership.

While restrictions on the MNC status and median number of workers might seem costly for the number of firms kept – out to the respective totals for the full sample of 2,171 firms part of a corporate group with partial foreign ownership – these 622 MNCs are actually responsible for most of the foreign activity in Costa Rica. Table A.33 presents totals adding up values for all firms part of the non-financial market economy, domestic- and foreign-owned alike. Columns (B) and (C) present the percentage of those values that are accounted for by firms part of a corporate group with partial foreign ownership and (large) MNCs, respectively. The last column shows that for most of the variables, the MNCs that we use for our empirical exercises account for over 75% of the totals across all firms part of a corporate group with partial foreign ownership. Hence, the criteria leading to the sample of 622 MNCs are not restrictive in terms of their coverage of the full sample of firms associated with foreign ownership.

Table A.32: Descriptive Statistics by Firm Ownership

	# Firms	Mean	S.D.	Median
Fully domestic firms				
Total Sales	78,137	495.1	3,114.9	118.3
Employment	76,372	7.2	32.2	2.4
Wage bill	76,650	53.4	300.7	12.6
Exports	4,487	451.7	2,804.2	23.6
Imports	21,521	224.1	1,579.7	13.8
Value Added	74,985	113.8	590.2	34.9
Input Costs	67,739	320.8	2,542.3	24.6
Total Net Assets	69,098	407.1	5,825.3	55.7
Firms part of corporate groups with partial foreign ownership *Excluding (Large) MNCs*				
Total Sales	1,549	7,863.3	65,002.5	1,042.5
Employment	1,538	51.6	353.5	13.2
Wage bill	1,539	634.2	3,905.0	158.8
Exports	544	1,933.1	9,343.1	73.8
Imports	1,037	1,936.1	7,151.8	117.1
Value Added	1,527	1,778.3	12,939.6	298.8
Input Costs	1,453	5,477.5	52,538.1	236.1
Total Net Assets	1,533	8,222.8	45,932.0	969.1
(Large) MNCs				
Total Sales	622	42,746.4	10,3204.9	12,205.1
Employment	622	380.7	882.3	170.0
Wage bill	622	5,093.2	10,282.1	2,228.8
Exports	473	19,458.7	88,196.7	1,689.2
Imports	606	14,738.3	70,525.4	1,522.7
Value Added	621	12,561.7	52,734.4	3,956.0
Input Costs	601	24,510.0	59,848.6	4,084.2
Total Net Assets	619	40,518.1	81,037.5	10,450.4

Notes: With the exception of the number of workers, the mean, standard deviation, and median are in thousands of CPI-deflated 2013 U.S. dollars. These statistics are averages across 2008 to 2017.

Table A.33: MNC Sample Coverage

	(A) Total	(B) All firms part of corporate groups w/ partial foreign owner.	(C) (Large) MNCs	(C)/(B)
Total Sales	77,450.5	50.1%	34.3%	68.6%
Number of Workers	868.5	36.4%	27.3%	74.9%
Wage Bill	8,236.4	50.3%	38.5%	76.4%
Exports	12,282.4	83.5%	74.9%	89.7%
Imports	15,762.3	69.4%	56.7%	81.6%
Value Added	19,050.5	55.2%	40.9%	74.2%
Input Costs	44,417.2	51.1%	33.2%	64.9%
Total Net Assets	65,819.0	57.3%	38.1%	66.6%

Notes: Number of workers in thousands. All other variables are in millions of CPI-deflated 2013 U.S. dollars. These statistics are averages across 2008 to 2017.

Table A.34: Country of Global Ultimate Ownership

Country of GUO	Frequency	Percent	Cumulative
United States	328	52.73	52.73
Panama	35	5.63	58.36
Great Britain	23	3.70	62.06
Mexico	21	3.38	65.43
Spain	20	3.22	68.65
Colombia	16	2.57	71.22
Chile	15	2.41	73.63
Netherlands	15	2.41	76.05
Germany	14	2.25	78.30
France	14	2.25	80.55
Canada	13	2.09	82.64
Japan	10	1.61	84.24
Guatemala	9	1.45	85.69
El Salvador	9	1.45	87.14
Ireland	7	1.13	88.26
...	...	...	
Total	622	100	

Notes: Table A.34 reports the countries of global ultimate ownership (GUO) that correspond to at least seven of the 622 MNCs in the final sample. 53% of MNCs have the United States as their country of GUO.

A.6.2 Procomer "Productive Linkages" Data

Data Cleaning and Sample Construction

We were granted access to the records of Procomer (the Trade Promotion Agency of Costa Rica) that track its implementation of "Productive Linkages:" a matchmaking program between MNCs and domestic firms. At its origins in 1999, the program was supported by the Inter-American Development Bank and was known as the "Supplier Development Project for High-Technology MNCs." The program has since undergone several changes to its name (*Costa Rica Provee* or *"Costa Rica Supplies"* was its longest-lasting name) and, to a lesser extent, to its organizational structure. That said, on its key aspects, the program has not been significantly altered since 2001.[15] This allows us to consider matches mediated by Procomer since 2001 as receiving a similar treatment.

This confidential data could only be stored and accessed in a fully-secured location at the Central Bank of Costa Rica. Before making use of the Procomer records, we first had to complete three main tasks:

1. Carefully assign tax IDs to firms, as in most Procomer data sources firms were identified through a (non-standardized) version of their name. Without assigning a unique tax ID to each firm, one could not combine the various Procomer data sources and merge the result with administrative data sources.

2. Digitize those parts of the data shared as PDFs (mostly summaries of firm evaluations, approximately 650 PDFs) or archived emails (approximately 8,000 emails).

3. Check both the internal consistency of Procomer's records and their accuracy (e.g., the occurrence and amount of a certain transaction) in the firm-to-firm transaction data. We found reassuring overlaps between Procomer records and administrative records.

After concluding these tasks, we learned that Procomer had successfully mediated 1,985 deals between 2001 and 2016. For all deals, we observe the buyer and winning supplier, the year the deal was made, its amount, and a description of the good or service traded. These 1,985 deals correspond to 560 unique suppliers and 324 unique buyers.[16] Commonly purchased goods include machinery, plastic accessories, and chemical products. Among services, metalworking, software development, and plant and equipment maintenance are the most frequent.

The archived emails allowed us to reconstruct the shortlists for which there was no centralized record. Whenever there was no systematic archiving of the shortlists shared by

[15] For more details, see Monge-González and Rodriguez-Álvarez (2013).

[16] Despite an exhaustive search, we were not able to find the tax ID of two of these firms. For obvious reasons, these firms and the deals they participated in cannot be used in the analysis.

Procomer with MNCs, we re-constructed them with the help of Procomer staff, by applying the rules originally used to generate them.[17]

We add 1,149 evaluations undertaken by Procomer between 2004 and 2015. Each evaluation involves a firm visit from a Procomer assessor and a detailed survey. Recent surveys are organized around five modules: productive capacity, market capacity, cooperation, R&D capacity, and quality.[18] For example, the quality module asks whether the firm has both general quality management certificates (e.g., ISO-9001) and sector-specific certificates (e.g., ISO-13485, quality management requirements for organizations producing medical devices and related services). The cooperation module asks whether the firm has employees able to negotiate in the language relevant to the market it targets.

Each evaluation is concluded with an absolute score, a letter grade category based on this absolute score, and recommendations on which Procomer program the firm is fit to benefit from. The program we study here ("Productive Linkages" or *Linkage,* as abbreviated by Procomer) is one option of follow-up. Figure A.6 provides an anonymized example of the actual summary of an evaluation of a firm manufacturing plastic bags.

These 1,149 evaluations refer to 921 distinct firms. Firms with multiple attempted deals are more likely to have multiple evaluations, as Procomer aimed to keep scores updated for active candidates. To compare winning and losing candidates for a deal, we use the absolute score of their most recent evaluation carried out prior to that deal.

Before setting the final set of rules that define the sample for the "winner vs. losers" research design, more context on the motivations and implementation of the "Productive Linkages" program was needed. To that end, we carried out extensive interviews with both contemporary and past Procomer staff, as well as with MNCs and domestic firms participating in the "Productive Linkages" program (see description of firm surveys in D.1). The main takeaway from these interviews was that in order to implement a clean "winner vs. losers" design, one had to study only deals meeting several strict criteria.

First, while the objective of "Productive Linkages" was to link domestic suppliers to MNC buyers, Procomer sometimes fostered linkages for suppliers that were foreign and/or for domestic buyers. Having been already had deals through Procomer in the past also did not disqualify a firm from joining future shortlists. The objective of Procomer was to share with each MNC a shortlist that contained the most competent firms to supply the demanded input.

Our interest lies in the impact of the first "Productive Linkages" deal of a domestic firm

[17] For each deal, Procomer considered only firms that were either in the same four-digit ISIC sector or in the same sector category of the "suppliers database" of CINDE. All candidates needed to have been evaluated by Procomer prior to the deal and, hence, have a *Procomer score.* "Productive Linkages" only considered shortlists of up to five candidates. Shortlists could contain less than five candidates in cases in which (i) the scores of the last ranked firms were much worse than those of the highest scored candidate, or (ii) there were fewer than five firms in the needed supplying sector. In sum, for each deal, we use up to five of the highest-scoring firms satisfying the sectoral condition, as long as the difference between each firm's score and the highest score in that shortlist is less than 20 points.

[18] While the structure of the survey evolved across time, there is considerable continuity in the themes covered.

with an MNC. For this reason, we only consider the first such deals. To be precise, for firms that are only matched in one year by Procomer we keep all deals occurring that year. For firms with deals in several years, we only keep the deals occurring in the first year.

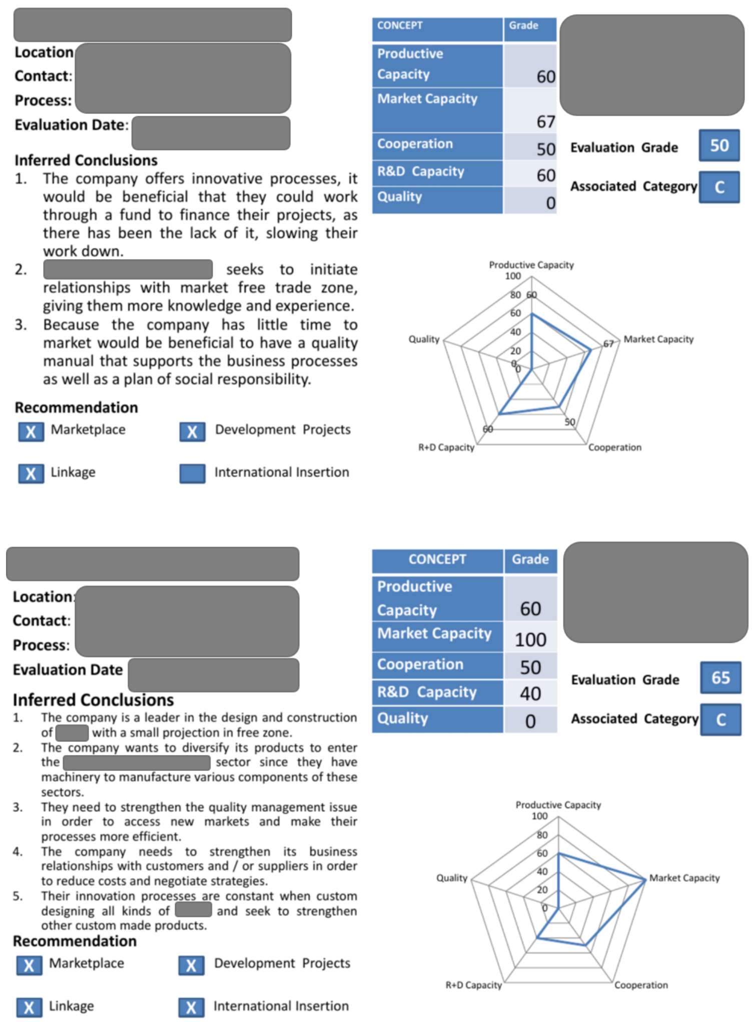


Figure A.6: Anonymized Summary Sheets of the Evaluations of Two Domestic Firms

Notes: The two figures above are anonymized summary sheets of two actual Procomer evaluations. Each summary sheet is based on a survey asking detailed questions on each of the five modules appraised by Procomer: productive capacity, market capacity, cooperation, R&D capacity and quality. Each evaluation is concluded with an absolute score, a letter grade category based on the range of the absolute score and recommendations on which Procomer program the firm is fit to benefit from. The “Productive Linkages" program is one option of follow-up. The top summary sheet belongs to a firm that seeks to initiate business relationships with MNCs in a Free Trade Zone (FTZ), with the hope of acquiring knowledge and experience. The bottom summary sheet pertains to a firm diagnosed as having to make its processes more efficient; Procomer assesses that this boost in efficiency can be obtained through stronger buying and selling relationships [..with MNCs part of the FTZ].

Whenever the event was triggered by more than one MNC buyer, the amount associated to the event is the sum of all amounts sold to MNCs that year. We dismiss events for which this sum is less than 5,000 U.S. dollars, as to maintain a comparable "observability" threshold as in the firm-to-firm transaction data.

Moreover, we also drop first deals where (i) losers had already experienced deals with MNCs prior to the relevant deal (the deal where they are losers), or where (ii) losers start supplying to MNCs in the two years after the relevant deal. Otherwise, losers do not provide a valid counterfactual for the winner, as they have already experienced an event or are experiencing one contemporaneously. Allow them in the sample would obscure the interpretation of the behavior of winner outcomes relative to losers' outcomes.

Last, we only study first deals brokered by Procomer between 2009 and 2015 because (i) the corporate income tax returns and firm-to-firm transaction datasets only start in 2008 and we want to be able to cross-check Procomer records with these administrative datasets, and (ii) we need at least two years' worth of administrative data after the deal to study its effects. Applying all these restrictive conditions leaves us with 31 events that involve 31 distinct domestic winners, 84 domestic losers (of which 51 distinct),[19] and 53 distinct MNCs triggering these 31 events.

Descriptive Statistics of Final Sample

In this section we present descriptive statistics on the Procomer sample of analysis. Table A.35 compares winners and losers in the year before the relevant deal (the deal won by the winner or the deal to which the loser was a contender). This table fails to find statistically significant differences between winners and losers across several measures of firm performance built on data coming from different sources: corporate income tax returns data, firm-to-firm transaction data, and records of Procomer scores. Nevertheless, one can note that losers tend to be larger than winners. This aligns with anecdotal evidence from Procomer staff: sometimes deals did not materialize with the losers because losers were attending to other business at the exact moment at which the potential MNC buyer required their full attention. Such situations granted opportunities to smaller firms to win those MNC deals.

One may be concerned that Procomer scores are not informative about firm performance. For instance, one may fear that government officials are unable to correctly assess firm capabilities or that they may have ulterior motives to provide a too high or too low score to specific firms (to draw the attention of MNCs to their preferred candidates). Figure A.7 plots the relationship between the Procomer score of firms and their value added per worker

[19] One might be concerned that the fact that some firms may belong to several shortlists is driven by Procomer staff trying to promote those firms against their merit. From interviews with Procomer staff, domestic firms, and MNCs we concluded this concern is most likely not justified for two reasons. First, MNCs were not obliged to purchase from any given supplier proposed by Procomer or to even purchase through Procomer to begin with. If a supplier did not meet the needs of the MNC, that supplier would not be chosen. Moreover, a recurrent theme during our interviews with Procomer staff was that of a need to build a strong positive reputation for domestic suppliers. Had firms undeserving of their score been added to shortlists, this would have jeopardized Procomer's attempt to create this positive reputation.

(in thousands of U.S. dollars) in the year before the relevant "Productive Linkages" deal (i.e., the deal for which a given firm is either a winner or loser). The value added per worker is computed using administrative data alone. We make the distinction between losers and winners, to check whether there is any systematic difference in the assessment of losers vs. winners.

Table A.35: Comparison Between Winners and Losers in Year Before Deal

	Winners (1)	Losers (2)	Difference (3)
Employment	43.79	69.06	-25.27
	(61.12)	(83.79)	(16.48)
Value-added per worker	13.30	19.48	-6.18
	(8.01)	(17.22)	(3.22)
Total transactions per worker	52.15	64.82	-12.67
	(42.60)	(76.89)	(14.60)
Number of buyers per worker	1.69	2.06	-0.37
	(1.51)	(2.91)	(0.55)
Procomer score	84.16	86.03	-1.88
	(10.48)	(7.33)	(1.74)
# Winners	31	-	-
# Losers	-	84	-

Notes: Table A.35 presents summary statistics describing winners and losers in the year prior to the relevant deal (deal won by the winner or deal to which the loser was a contender). Column (3) reports the difference between winners' and losers' values. Value-added per worker and total transactions per worker are measured in CPI-deflated 2013 U.S. dollars. Robust standard errors in parentheses.

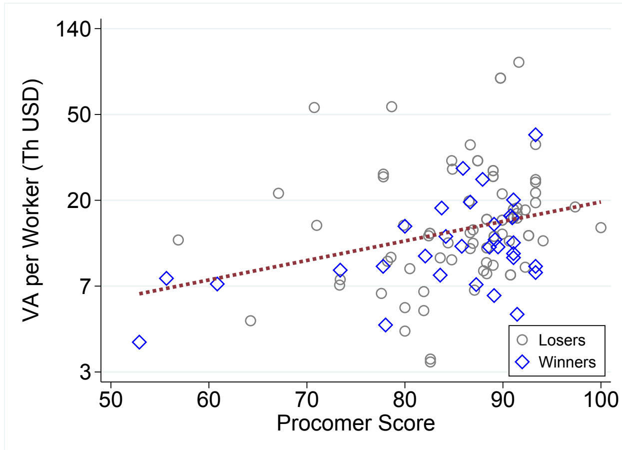


Figure A.7: Relationship between Procomer Score and Value Added Per Worker

Notes: Figure A.7 plots the relationship between the score assigned to firms by Procomer and their value added per worker (in thousands of CPI-deflated U.S. dollars) in the year before the relevant "Productive Linkages" deal (i.e., the deal for which a given firm is either a winner or loser). The figure makes the distinction between losers and winners, to investigate whether there is any systematic difference in the scoring of losers vs. winners. This figure only focuses on the sample of "Productive Linkages" deals used in the analysis.

We note that there is no systematic pattern assigning high scores to low value-added firms or vice versa. There is a clear positive correlation between the Procomer score and the value-added per worker, which means scores are informative on firm performance. That said, this correlation is far from 1. Rather than posing a problem, we interpret this to be evidence in favor of the usefulness of the Procomer score: its main advantage is that Procomer evaluates firms on features that are unobserved in our administrative data and that, while not reflected in the value-added per worker of the firm, are relevant to MNCs.

Table A.36 reports summary statistics on the first relationship with an MNC buyer mediated by the "Productive Linkages" program. We notice that these mediated relationships are comparable to those in our baseline sample of unmediated economy-wide first-time supplying relationships (see Table A.24 in A.5).

Table A.36: Descriptive Statistics of Relationship with First MNC Buyer For Winners in Sample of Deals Mediated by ‘Productive Linkages" Program

	N	Mean	Median	S.D.
First transaction with MNC (thous. of U.S. dollars)	31	53.45	29.53	81.16
Length of relationship with first MNC buyer (years)	31	3.87	3.00	2.66

Notes: Table A.36 provides descriptive statistics of the first relationship with an MNC mediated by the “Productive Linkages" program. The first row reports summary statistics of the amount sold to this MNC buyer in the first year of the relationship. The second row describes the overall length of this relationship (in years). These statistics characterize the sample of 31 “Productive Linkages" deals.

Figure A.8 plots the frequency of shortlists containing two, three, four, and five candidates in the sample of “Productive Linkages" deals used in the analysis. Most shortlists proposed to MNCs contained four candidates.

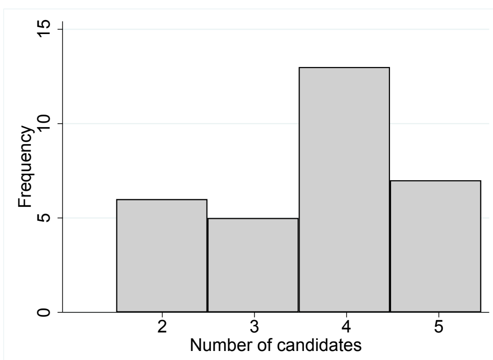


Figure A.8: Distribution of Shortlist Length for Sample of Deals

Notes: Figure A.8 plots the frequency of shortlists containing two, three, four, and five candidates in the sample of “Productive Linkages" deals used in the analysis.

Appendix B

Appendix for "The Effects of MNCs on Workers"

B.1 Additional Evidence and Robustness Checks

B.1.1 Descriptive Statistics

	Number of s	Mean	*p1*	*p10*	*p50*	*p90*	*p99*	SD
Panel A: 2009-2017								
$\Delta\mathcal{M}_{st}$	412	12.9	-100.0	-60.8	1.2	99.7	239.9	68.4
$\Delta\mathcal{O}_{st}$ from IV Set 1	412	21.2	-99.6	-38.7	5.4	96.8	237.5	61.0
$\Delta\mathcal{O}_{st}$ from IV Set 2	412	60.1	-99.5	-32.7	12.4	171.4	1068.2	175.8
Panel B: Yearly								
$\Delta\mathcal{M}_{st}$	3,699	3.7	-83.3	-17.4	0.2	18.1	141.3	111.1
$\Delta\mathcal{O}_{st}$ from IV Set 1	3,699	3.0	-68.4	-12.0	0.7	18.0	127.2	28.4
$\Delta\mathcal{O}_{st}$ from IV Set 2	3,699	6.1	-53.0	-12.7	2.3	22.9	151.1	32.6

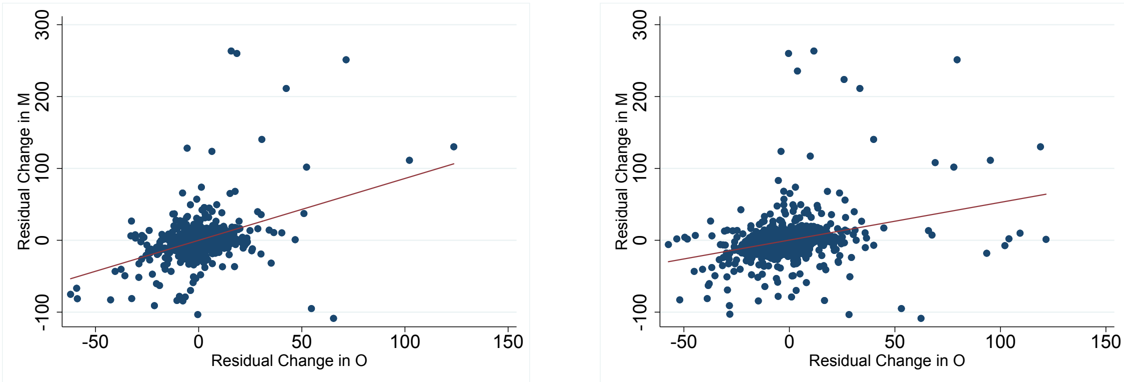
Table B.1: $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$

Notes: Table presents summary statistics for the market-level growth in MNC employment inside and outside of Costa Rica, $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$, respectively. $\Delta\mathcal{O}_{st}$ can either come from the Leading IV (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica) or the robustness check IV Set 2 (the instrument using changes in MNC employment outside of Costa Rica for MNCs with subsidiaries in at least one of twenty Latin American and Caribbean countries). Markets are defined at the two-digit industry $\times$ region level. Panel A calculates growth rates over the entire 2009 to 2017 period, whereas Panel B calculates yearly growth rates.

	$\Delta\mathcal{M}_{st}$ (1)	$\Delta\mathcal{M}_{st}$ (2)	$\Delta\mathcal{M}_{st}$ (3)	$\Delta\mathcal{M}_{st}$ (4)	$\Delta\mathcal{M}_{st}$ (5)	$\Delta\mathcal{M}_{st}$ (6)
$\Delta\mathcal{O}_{st}$ from IV Set 1	0.814***	0.862***			0.601***	0.608***
	(0.171)	(0.186)			(0.155)	(0.172)
$\Delta\mathcal{O}_{st}$ from IV Set 2			0.525***	0.532***	0.309***	0.315***
			(0.131)	(0.131)	(0.114)	(0.105)
Year FE	No	Yes	No	Yes	No	Yes
Two-Digit Industry FE	No	Yes	No	Yes	No	Yes
Observations	644	644	806	805	629	629
Adjusted R^2	0.15	0.15	0.096	0.075	0.17	0.17

Table B.2: Correlation between $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$

Notes: Table B.2 presents the regressions of $\Delta\mathcal{M}_{st}$ on the $\Delta\mathcal{O}_{st}$ from either the leading instrument IV Set 1 (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica) or the robustness check instrument IV Set 2 (the instrument using changes in MNC employment outside of Costa Rica for MNCs with subsidiaries in at least one of twenty Latin American and Caribbean countries). Columns (1) and (2) use the $\Delta\mathcal{O}_{st}$ from IV Set 1, Columns (3) and (4) use $\Delta\mathcal{O}_{st}$ from IV Set 2, and Columns (5) and (6) use both. Odd and even-numbered columns differ in the inclusion or exclusion of year and two-digit industry fixed effects. This regression only contains the markets s with non-zero values of MNC employment.



(a) Corr. b/n $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$ from IV Set 1 (b) Corr. b/n $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$ from IV Set 2

Figure B.1: Growth Rates of MNC Employment Inside and Outside of Costa Rica

Notes: Figure B.1 plots the relationship between $\Delta\mathcal{M}_{st}$ and $\Delta\mathcal{O}_{st}$, the growth rates of MNC employment inside and outside of Costa Rica (residualized of year and two-digit industry fixed effects) associated to two-digit industry $\times$ region markets s in year t. Panel B.1a uses the outside Costa Rica employment in the same MNC groups as those with subsidiaries in Costa Rica. Panel B.1b uses the growth in MNC employment of groups with a subsidiary in one out of twenty Latin American and Caribbean countries. This figure only contains the observations with non-zero values of MNC employment.

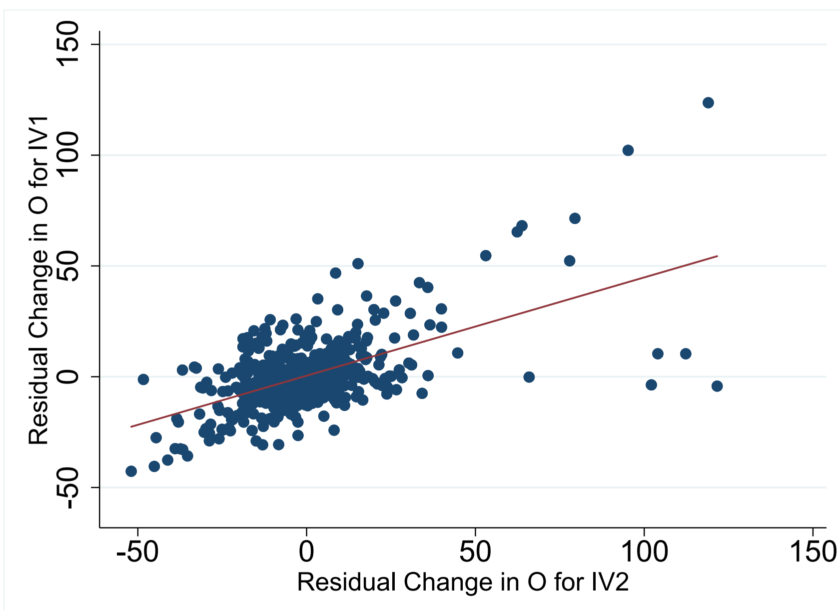


Figure B.2: Correlation between $\Delta\mathcal{O}_{st}$ from IV Set 1 and $\Delta\mathcal{O}_{st}$ from IV Set 2

Notes: Figure B.2 plots the relationship between $\Delta\mathcal{O}_{st}$ from IV Set 1 and $\Delta\mathcal{O}_{st}$ from IV Set 2 (residualized of year and two-digit industry fixed effects). This plot only contains the markets s with non-zero values of MNC employment.

Table B.3: Summary Statistics for the Sample Used in the Movers Design

Group	Number of Observations	Number of Individuals	Number of Firms	Log Wage Before	Log Wage Coworkers Before	Size Before	Log Wage After	Log Wage Coworkers After	Size After
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All Movers	1,559,512	84,756	26,093	12.99	12.86	37.66	13.06	12.96	36.96
				0.64	0.48	191.47	0.65	0.50	197.71
MNC-MNC Movers	281,384	15,544	579	13.37	13.18	402.11	13.46	13.28	353.65
				0.65	0.43	868.68	0.69	0.47	860.51
DOM-MNC Movers	234,005	13,754	4,843	12.99	12.87	67.87	13.19	13.14	369.35
				0.60	0.48	205.11	0.58	0.40	864.60
MNC-DOM Movers	190,757	11,217	4,198	13.15	13.02	415.58	13.08	12.97	73.19
				0.62	0.38	876.56	0.64	0.50	222.53
DOM-DOM Movers	853,366	47,114	23,845	12.82	12.71	28.49	12.89	12.80	27.26
				0.59	0.46	114.99	0.58	0.47	114.70

Notes: Table B.3 presents summary statistics for the sample of workers to which we apply the movers design described in Section 3.3. An observation in this table is a worker $\times$ quarter $\times$ year. The data over which we run the movers regression is balanced, in the sense that each worker is observed for exactly 17 quarters: eight quarters before the move, the quarter of the move, and eight quarters after the move. The only exception applies to the minority of workers who have more than one event. The relevant quarter $\times$ year observations of those workers are repeated. This explains why the numbers in Column (1) are slightly larger than the multiplication of Column (2) by 17 (the number of quarter $\times$ year of each worker in the balanced panel). Columns (4) to (6) refer to the employer before the move, Columns (7) to (9) refer to the employer after the move. Columns (4) and (7) refer to the labor earnings of the mover, Columns (5) and (8) refer to the average labor earnings of the coworkers of the mover. Labor earnings are in 2013 CPI-deflated Costa Rican Colones (in 2013, 1 U.S. dollar $\approx$ 500 Colones). Columns (6) and (9) refer to the number of workers at the employer of the mover, before and after. The statistic under each titled average is the standard deviation of the variable above.

Table B.4: Summary Statistics for the Steps of the Construction of the Final Sample of Workers in Domestic Firms

Year Sample	Number of Individuals I (1)	Number of Firms I (2)	Number of Individuals II (3)	Number of Firms II (4)	Number of Individuals III (5)	Number of Firms III (6)	Number of Individuals IV (7)	Number of Firms IV (8)
2009	1,054,362	74,519	572,105	40,445	538,048	33,754	388,713	26,186
2010	1,076,511	77,603	574,260	41,089	551,397	35,314	392,635	27,025
2011	1,098,885	79,234	577,738	41,492	553,769	35,715	397,598	28,245
2012	1,137,004	79,783	606,488	41,569	582,969	35,777	426,271	28,667
2013	1,131,449	77,817	594,839	40,252	569,920	34,472	427,442	28,246
2014	1,131,358	76,977	591,820	39,310	566,897	33,938	426,208	27,586
2015	1,130,973	76,634	588,807	39,003	569,029	34,422	420,378	27,454
2016	1,157,860	75,773	597,972	38,708	578,335	34,261	425,066	27,698
2017	1,186,333	75,821	614,469	38,887	578,148	32,026	437,638	27,855

Notes: Table B.4 presents the number of unique individuals and firms in four samples. Sample I – the sample in Columns (1) and (2) – includes all workers and firms in the formal economy of Costa Rica each year (without self-employed individuals). Sample II – the sample in Columns (3) and (4) – excludes from Sample I those firms that are MNCs and the public sector, in addition to firms with only one worker. This sample drops all workers whose main employer in a given year is dropped according to these rules. Sample III – the sample in Columns (5) and (6) – keeps only those firms (and their associated workers) from Sample II that are matched to the corporate income tax records and that have the information necessary to compute value added. Sample IV – the sample in Columns (7) and (8) starts from sample III and drops the firms with extreme values for the change in value added per worker (top and bottom 1%). Moreover, it drops the workers with extreme changes in yearly labor earnings (the top and bottom 1%). We also drop observations for which we have missing changes in annual labor earnings (for the worker) or missing changes in value added per worker (for the firm). Sample IV is the final sample used in the analysis of the effects of MNCs workers in domestic firms.

	Bottom Tercile Mean (SD) (1)	Mid Tercile Mean (SD) (2)	Top Tercile Mean (SD) (3)	T2-T1 Diff(SE) (4)	T3-T2 Diff(SE) (5)	T3-T1 Diff(SE) (6)
Labor Market Characteristics						
Growth MNC Empl 2009-2017	-22.30	4.34	58.84	26.64***	54.51***	81.14***
	(27.00)	(3.88)	(56.84)	(0.03)	(0.06)	(0.07)
Share MNC Empl 2006-2008	0.21	0.14	0.40	-0.07***	0.26***	0.20***
	(0.23)	(0.15)	(0.29)	(0.00)	(0.00)	(0.00)
MNC Wage Premium	1.088	1.101	1.099	0.013***	-0.002***	0.012***
	(0.10)	(0.08)	(0.10)	(0.00)	(0.00)	(0.00)
Worker Characteristics						
log(labor earnings): All	12.09	12.09	12.18	0.01***	0.08***	0.09***
	(0.68)	(0.67)	(0.70)	(0.00)	(0.00)	(0.00)
log(labor earnings): NC DOM	11.97	11.99	12.00	0.03***	0.01***	0.03***
	(0.21)	(0.20)	(0.18)	(0.00)	(0.00)	(0.00)
log(labor earnings): C DOM	12.71	12.75	12.80	0.05***	0.05***	0.09***
	(0.32)	(0.31)	(0.22)	(0.00)	(0.00)	(0.00)
log(labor earnings): NC MNC	12.24	12.25	12.29	0.01***	0.04***	0.05***
	(0.34)	(0.14)	(0.31)	(0.00)	(0.00)	(0.00)
log(labor earnings): C MNC	13.25	13.21	13.26	-0.04***	0.05***	0.01***
	(0.40)	(0.31)	(0.29)	(0.00)	(0.00)	(0.00)
Male	0.73	0.68	0.69	-0.05***	0.01***	-0.04***
	(0.44)	(0.46)	(0.46)	(0.00)	(0.00)	(0.00)
College-Educated	0.07	0.10	0.10	0.03***	0.00	0.03***
	(0.26)	(0.30)	(0.30)	(0.00)	(0.00)	(0.00)
Observations: Worker-Year	823,193	823,194	823,197	1,646,387	1,646,391	1,646,390

Table B.5: Descriptive Statistics of Labor Markets and Workers in the Pre-Period By Tercile of Subsequent Growth in MNC Employment in the Labor Market

Notes: Table B.5 presents descriptive statistics over the sample of workers in the pre-period (2006 to 2008). Note that these are not necessarily the same workers as those in the sample of workers for 2009 and 2017 (over which we run the analysis of the indirect effects). Each observation is a worker-year. Workers from 2006 to 2008 are separated in terciles by the value of the percentage change in MNC employment between 2009 and 2017 ($\Delta\mathcal{M}_{s,2009-2017}$) in their labor market s in a given year (2006, 2007, or 2008). Columns (1), (2), and (3) present descriptive statistics over the workers in the bottom, mid, and top tercile of MNC employment growth from 2009 to 2017. Columns (4), (5), and (6) present the differences between the means of the mid tercile and the bottom tercile, top tercile and mid tercile, and top and bottom terciles, respectively. The average labor earnings are provided for all workers, for those without a college degree and who work for domestic firms (NC DOM), for those with a college degree and who work for domestic firms (C DOM), for those without a college degree and who work for MNCs (NC MNC), and for those with a college degree and who work for MNCs (C MNC). ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Bottom Tercile Mean (SD) (1)	Mid Tercile Mean (SD) (2)	Top Tercile Mean (SD) (3)	T2-T1 Diff(SE) (4)	T3-T2 Diff(SE) (5)	T3-T1 Diff(SE) (6)
Firm Characteristics						
$\Delta FLE_{j(i),t}$	-0.07	0.001	0.27	0.07***	0.26***	0.34***
	(0.19)	(0.00)	(0.40)	(0.00)	(0.00)	(0.00)
Nr. Employees	20.96	12.01	52.48	-8.96***	40.47***	31.52***
	(93.86)	(88.72)	(162.65)	(0.44)	(0.97)	(1.02)
Worker Characteristics						
log(labor earnings): All DOM	12.48	12.41	12.74	-0.07***	0.33***	0.26***
	(0.47)	(0.47)	(0.48)	(0.00)	(0.00)	(0.00)
Male	0.68	0.60	0.71	-0.08***	0.11***	0.03***
	(0.33)	(0.37)	(0.26)	(0.00)	(0.00)	(0.00)
College-Educated	0.09	0.08	0.12	-0.01***	0.04***	0.04***
	(0.19)	(0.19)	(0.20)	(0.00)	(0.00)	(0.00)
Observations: Firm-Year	61,499	155,398	29,544	216,897	184,942	91,043
Observations: Worker-Year	1,027,639	1,027,639	1,027,639	2,055,278	2,055,278	2,055,278

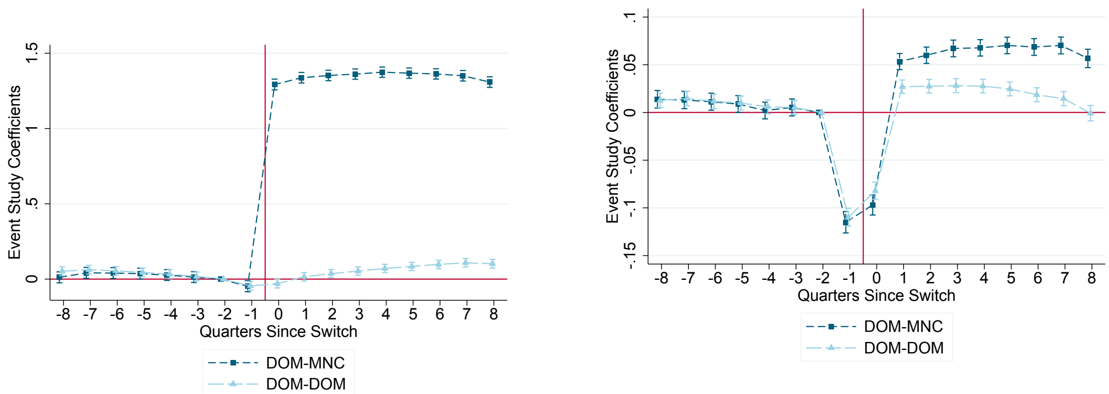
Table B.6: Descriptive Statistics of Domestic Firms and Their Incumbent Workers By Tercile of Subsequent Yearly Growth in Firm-Level Exposure to MNCs

Notes: Table B.6 presents descriptive statistics over the sample of domestic firms and their incumbent workers for 2009-2017. We first split the sample of worker-years into terciles according to the annual change in their firm-level exposure to MNCs $\left(\Delta FLE_{j(i),t}\right)$. We collapse the data such that each observation is a firm-year and present the descriptive statistics at the firm-year level (which is the unit of variation of the $\Delta FLE_{j(i),t}$). Columns (1), (2), and (3) present descriptive statistics for the workers in the bottom, mid, and top tercile of changes in $\Delta FLE_{j(i),t}$. Columns (4), (5), and (6) present the differences between the means of the mid tercile and the bottom tercile, top tercile and mid tercile, and top and bottom terciles, respectively. The number of observations in each column corresponds to firm-year observations. Each tercile has 1,027,639 worker-year observations. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

B.1.2 Magnitude and Interpretation of the MNC Wage Premium

Robustness Checks on the MNC Wage Premium Estimate from the Movers Design

Additional Evidence on the MNC Wage Premium Estimate from the Movers Design



(a) Log Employer Number of Workers

(b) Log Worker Quarterly-Average Labor Earnings. Controls for Employer Number of Workers and Industry

Figure B.3: Employer Size and Worker Quarterly-Average Labor Earnings

Notes: Figure B.3 explores the importance of employer size in explaining the change in earnings upon changing employers. Panel B.3a uses as dependent variable the log number of workers of the employer that quarter. Panel B.3b uses as dependent variable the log quarterly-average worker labor earnings. The difference between the estimates in Panel B.3b and those in Figure 3.2 comes from the additional controls in Panel B.3b for the logs of the number of workers of the employer that quarter and the square of this number, and the two-digit industry code of the employer. This exercise pools together MNCs inside and outside of the *Zona Franca* (Free Zone) regime and workers of different educational attainment. We use robust standard errors clustered at the individual level.

The following specification estimates non-parametrically the contribution of firm size and industry characteristics to the size of the MNC wage premium. It also uses a more generous definition of the sample than that used in the main sample employed in the movers design. Workers used in the regression described in equation (B.1) are only required to have worked for the same employer in the twelve months before a move.

$$w_{it} = \alpha_i + \lambda_t + \mu_r + \rho_o + \sum_{a\in\{\text{DOM,MNC}\}} \sum_{b\in\{\text{SMALL,BIG}\}} \sum_{c\in\{\text{HT,LT}\}} \psi_{(a+b+c)} D_{j(i,t)\in\{a+b+c\}} \tag{B.1}$$

where w_{it} is the log of the labor earnings of individual i in month-year t, α_i is an individual fixed effect, λ_t is a month-year t fixed effect, μ_r is a region fixed effect, and ρ_o is an occupation fixed effect. $D_{j(i,t)\in\{a+b+c\}}$ is a dummy that takes value 1 if the employer $j(i,t)$ of i in t has characteristics a, b, and c, where DOM indicates that employer $j(i,t)$ is a domestic firm, MNC indicates that employer $j(i,t)$ is an MNC, SMALL means that the sales of employer $j(i,t) < 5$ million USD, BIG means that the sales of employer $j(i,t)$ $\geq$5 million USD, HT indicates that the industry of employer $j(i,t)$ is high-tech (according to the OECD classification), and LT indicates that the industry of employer $j(i,t)$ is low-tech. Estimates of the regression described in equation (B.1) can be found in Table B.7.

Table B.7: Does the MNC Size or Industry Explain its Premium? Not Entirely

Dependent Variable: w_{it}	(1)	(2)	(3)
DOM + SMALL + HT	0.031**	0.028**	0.019**
	(0.001)	(0.001)	(0.001)
MNC + SMALL + LT	0.196**	0.198**	0.204**
	(0.003)	(0.003)	(0.003)
MNC + SMALL + HT	0.247**	0.247**	0.229**
	(0.004)	(0.004)	(0.004)
DOM + BIG + LT	0.198**	0.191**	0.179**
	(0.001)	(0.001)	(0.001)
DOM + BIG + HT	0.218**	0.208**	0.193**
	(0.001)	(0.001)	(0.001)
MNC + BIG + LT	0.260**	0.258**	0.248**
	(0.001)	(0.001)	(0.001)
MNC + BIG + HT	0.280**	0.276**	0.252**
	(0.001)	(0.001)	(0.001)
Individual FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Region FE	No	Yes	Yes
Occupation FE	No	No	Yes
Observations	6,096,274	6,096,274	6,096,272

Notes: Table B.7 presents the estimates of the $\psi_{(a+b+c)}$ coefficients on the dummies of employer characteristics from equation (B.1). The reference category is that of an employer which is domestic, small, and in a low-tech industry (DOM+SMALL+LT). Columns (1), (2), (3) differ in the fixed effects used. We use robust standard errors clustered at the individual level.

Zona Franca **(Free Zone) status.** Like most countries around the world, Costa Rica has a Special Economic Zone regime called *Zona Franca* (Free Zone) under which authorized businesses (the majority of which MNCs) are exempt from the normal regime applicable in Costa Rica, in particular concerning customs and taxation. Entities established in *Zona Francas* may enjoy tax exemption on the exports of their goods, income tax (ranging from 0% to 100% depending on the activity, location in Costa Rica and the number of years the entity has already enjoyed this benefit), sales tax, selective consumption tax, real estate transfer tax, and withholding tax on remittances abroad, as well as the free possession and use of currencies related to their local operations.

We now investigate whether MNCs attracted in the ZF regime pay different premia to their workers relative to MNCs outside of the ZF regime and subject to the same obligations as domestic firms. The answer to this question is central to policy-making, as one of the most frequent arguments in favor of the ZF regime is that it creates “good jobs" for locals that would have presumably not been created without such a regime. We divide MNC subsidiaries in Costa Rica into two groups: those that are part of the ZF regime and those that are not.

Figure B.4 plots the event-study coefficients for three types of moves: from a domestic firm to an MNC in the ZF regime (DOM-MNC(ZF)), from a domestic firm to a non-ZF MNC (DOM-MNC(NOT ZF)), and from one domestic firm to another. The magnitude of a gain upon changing employer is the highest for DOM-MNC(ZF) moves, followed by DOM-MNC(NOT ZF) moves, and then finally, by DOM-DOM moves.

Figure B.5 investigates the role of firm size and industry in explaining the difference between the average premium of moves to an MNC in the ZF relative to the average premium of moves to an MNC outside of the ZF. First, in Panel B.5a we use the log number of workers of the firm as the dependent variable and find that moves to an MNC in a ZF are moves to larger firms on average than moves to an MNC not in a ZF. This is in line with ZFs targeting firms that can make more substantial investments. Panel B.5b plots again the event-study coefficients for the labor earnings as the dependent variable, this time after controlling for the firm size and industry. These controls make moves to a non-ZF MNC and a domestic firm significantly more similar among themselves, particularly in the short-term. However, there remains a significant difference between moving to a ZF MNC versus non-ZF MNC that is not explained away by the size and industry of the MNC.

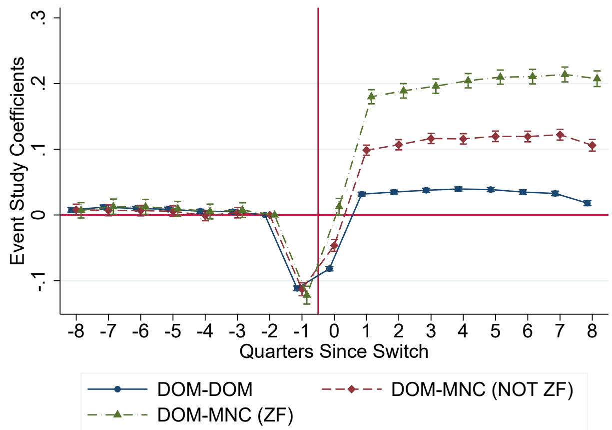
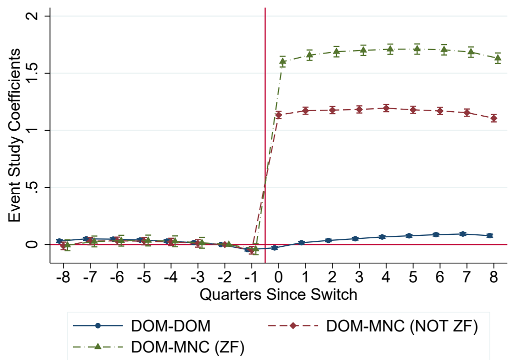
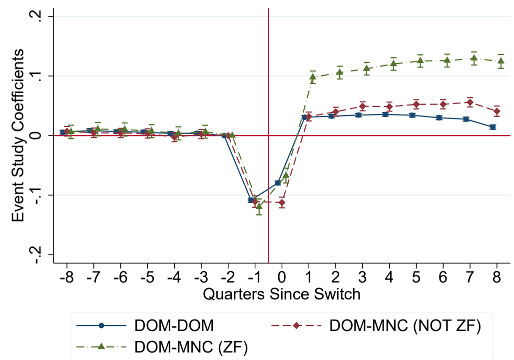


Figure B.4: Log Worker Quarterly-Average Labor Earnings. Three Types of Worker Moves (DOM-DOM, DOM-MNC (in FZ), DOM-MNC (not in FZ)

Notes: Figure B.4 plots the event-study coefficients from a specification where the event is defined as an across-quarter switch in employment. The sample is restricted to workers with the same main employer continuously between quarter *-8* and *-1* and the same new main employer continuously between quarters *0* and *+8*. The dependent variable is the log worker quarterly-average labor earnings. In this exercise, MNCs in Costa Rica are split into two mutually exclusive categories based on whether they belong to the *Zona Franca* (Free Zone) regime or not. We use robust standard errors clustered at the individual level.



(a) Log Employer Number of Workers



(b) Log Quarterly-Average Labor Earnings. Controls for Employer Number of Workers and Industry

Figure B.5: Employer Size, FZ Status and Worker Quarterly-Average Labor Earnings

Notes: Figure B.5 explores the importance of employer size in explaining the change in earnings upon changing employers. In this exercise, MNCs in Costa Rica are split into two mutually exclusive categories based on whether they belong to the *Zona Franca* (Free Zone) regime or not. Panel B.5a uses as dependent variable the log number of workers of the employer that quarter. Panel B.5b uses as dependent variable the log quarterly-average worker labor earnings. The difference between the estimates in Panel B.5b and those in Figure B.4 comes from the additional controls in Panel B.5b for the logs of the number of workers of the employer that quarter and the square of this number, and the two-digit industry code of the employer. We use robust standard errors clustered at the individual level.

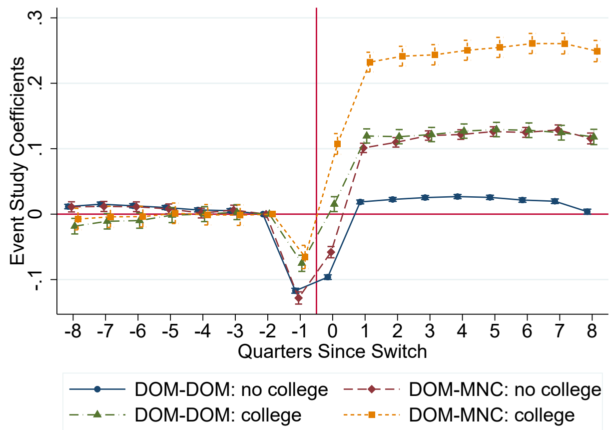
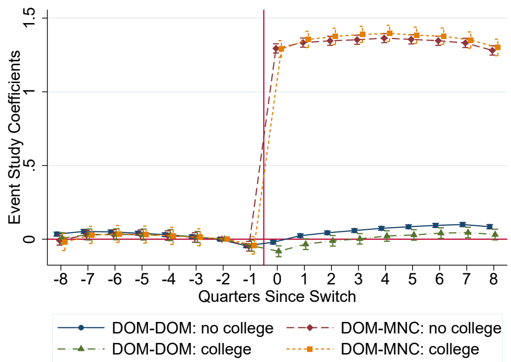
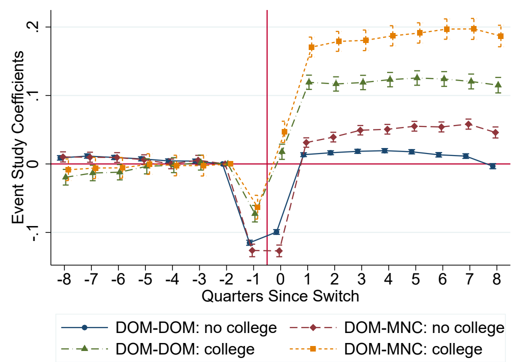


Figure B.6: Log Worker Quarterly-Average Labor Earnings. Two Types of Worker Moves (DOM-DOM and DOM-MNC), by Educational Attainment

Notes: Figure B.6 plots the event-study coefficients from a specification where the event is defined as an across-quarter switch in employment. Workers are split into two categories of educational attainment: college or more (“college") and less than college (“no college"). The sample is restricted to workers with the same main employer continuously between quarter *-8* and *-1* and the same new main employer continuously between quarters *0* and *+8*. The dependent variable is the log worker quarterly-average labor earnings. This exercise pools together MNCs inside and outside of the *Zona Franca* (Free Zone) regime. We use robust standard errors clustered at the individual level.



(a) Log Employer Number of Workers



(b) Log Worker Quarterly-Average Labor Earnings. Controls for Employer Number of Workers and Industry

Figure B.7: Employer Size and Worker Quarterly-Average Labor Earnings, by Educational Attainment

Notes: Figure B.7 explores the importance of employer size in explaining the change in earnings upon changing employers. In this exercise, workers are split into two categories of educational attainment: college or more ("college") and less than college ("no college"). Panel B.7a uses as dependent variable the log number of workers of the employer that quarter. Panel B.7b uses as dependent variable the log quarterly-average worker labor earnings. The difference between the estimates in Panel B.7b and those in Figure B.6 comes from the additional controls in Panel B.7b for the logs of the number of workers of the employer that quarter and the square of this number, and the two-digit industry code of the employer. We use robust standard errors clustered at the individual level.

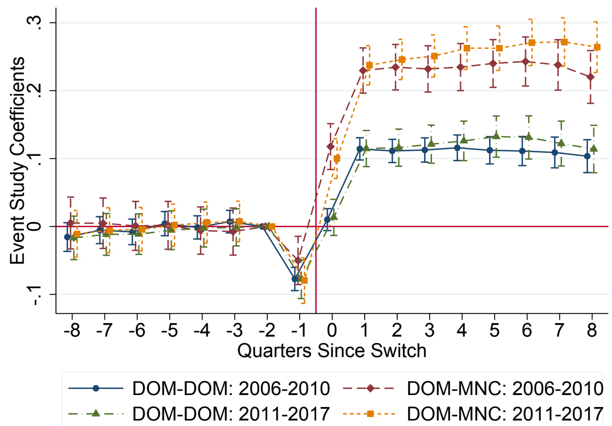
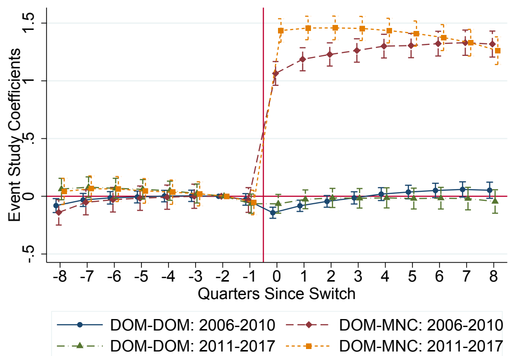
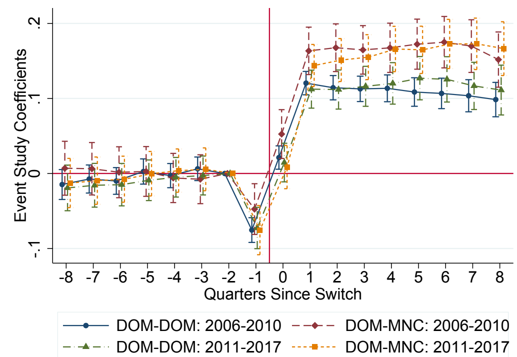


Figure B.8: Log Worker Quarterly-Average Labor Earnings. Two Types of Worker Moves: DOM-MNC and DOM-DOM, and Two Periods: 2006-2010 and 2011-2017. College Graduates Only

Notes: Figure B.8 plots the event-study coefficients from a specification where the event is defined as an across-quarter switch in employment. This exercise only studies workers with college or more. The sample is restricted to workers with the same main employer continuously between quarter *-8* and *-1* and the same new main employer continuously between quarters *0* and *+8*. The dependent variable is the log worker quarterly-average labor earnings. This exercise pools together MNCs inside and outside of the *Zona Franca* (Free Zone) regime. We use robust standard errors clustered at the individual level.



(a) Log Employer Number of Workers



(b) Log Worker Quarterly-Average Labor Earnings. Controls for Employer Number of Workers and Industry

Figure B.9: Employer Size and Worker Quarterly-Average Labor Earnings. Two Periods: 2006-2010 and 2011-2017. College Graduates Only

Notes: Figure B.9 explores the importance of employer size in explaining the change in earnings upon changing employers. This exercise only studies workers with college or more. Panel B.9a uses as dependent variable the log number of workers of the employer that quarter. Panel B.9b uses as dependent variable the log quarterly-average worker labor earnings. The difference between the estimates in Panel B.9b and those in Figure B.8 comes from the additional controls in Panel B.9b for the logs of the number of workers of the employer that quarter and the square of this number, and the two-digit industry code of the employer. This exercise pools together MNCs inside and outside of the *Zona Franca* (Free Zone) regime. We use robust standard errors clustered at the individual level.

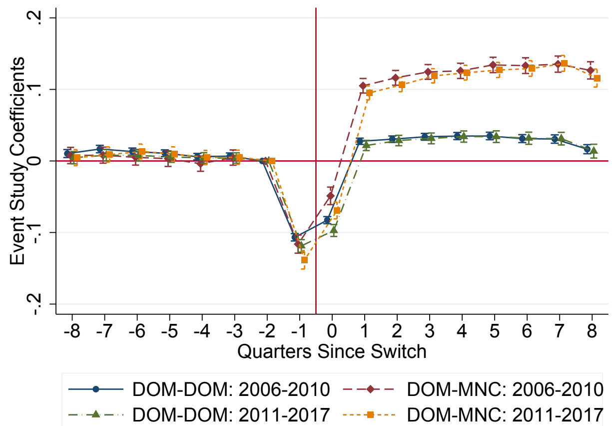
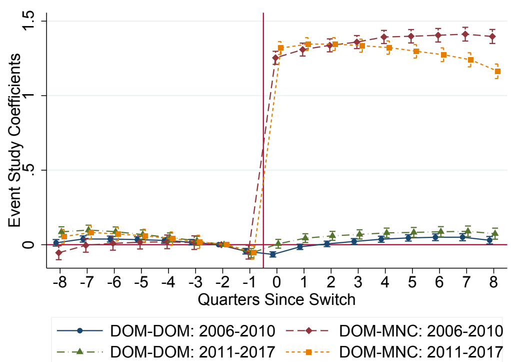
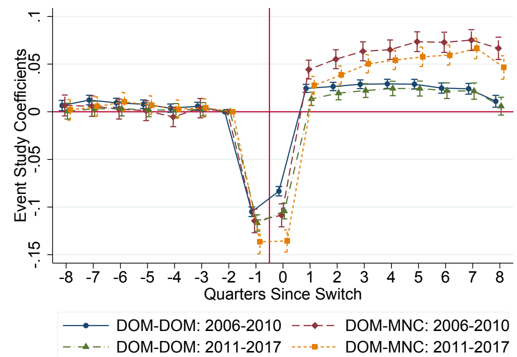


Figure B.10: Log Worker Quarterly-Average Labor Earnings. Two Types of Worker Moves: DOM-MNC and DOM-DOM, and Two Periods: 2006-2010 and 2011-2017. Less Than College Only

Notes: Figure B.10 plots the event-study coefficients from a specification where the event is defined as an across-quarter switch in employment. This exercise only studies workers with less than a college degree. The sample is restricted to workers with the same main employer continuously between quarter *-8* and *-1* and the same new main employer continuously between quarters *0* and *+8*. The dependent variable is the log worker quarterly-average labor earnings. This exercise pools together MNCs inside and outside of the *Zona Franca* (Free Zone) regime. We use robust standard errors clustered at the individual level.



(a) Log Employer Number of Workers



(b) Log Worker Quarterly-Average Labor Earnings. Controls for Employer Number of Workers and Industry

Figure B.11: Employer Size and Worker Quarterly-Average Labor Earnings. Two Periods: 2006-2010 and 2011-2017. Less Than College Only

Notes: Figure B.11 explores the importance of employer size in explaining the change in earnings upon changing employers. This exercise only studies workers with less than a college degree. Panel B.11a uses as dependent variable the log number of workers of the employer that quarter. Panel B.11b uses as dependent variable the log quarterly-average worker labor earnings. The difference between the estimates in Panel B.11b and those in Figure B.10 comes from the additional controls in Panel B.11b for the logs of the number of workers of the employer that quarter and the square of this number, and the two-digit industry code of the employer. This exercise pools together MNCs inside and outside of the *Zona Franca* (Free Zone) regime. We use robust standard errors clustered at the individual level.

MNC Wage Premium Unlikely To Be Driven by Inferior Amenities at MNCs

(a) Evidence using matched employer-employee data

(a1). Retention probabilities for domestic firms vs. MNCs. Figure B.12 shows that workers who start employment at a domestic firm are more likely to change employment in the coming quarters than workers who start employment at an MNC.

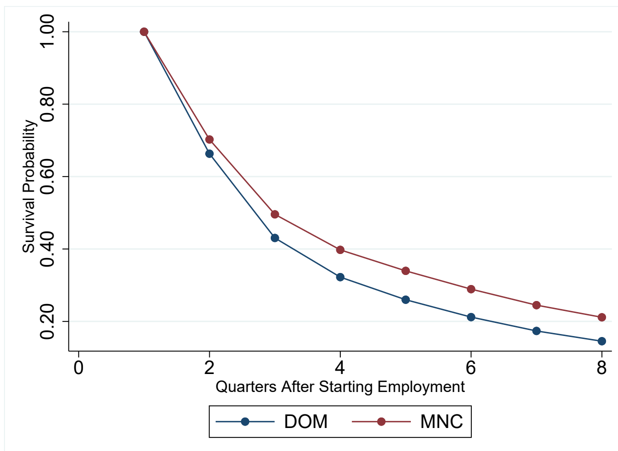


Figure B.12: Higher Retention Probabilities at MNCs than at Domestic Firms

Notes: Figure B.12 plots the retention probability (i.e., the probability that a worker who started employment in quarter 1 at firm j is still working for firm j in quarter $t \geq 1$) for two groups of workers: (i) workers who start employment in quarter 1 in a domestic firm (in blue, labelled by “DOM") and (ii) workers who start employment in quarter 1 in an MNC (in red, labelled by “MNC"). In both groups, we only include workers who are observed employed by a different firm in the quarter after the separation. For these workers, the separation can be interpreted as a quit (as opposed to a firing). By construction, all workers are working for the given firm in quarter 1, which explains the survival probability of 1 in quarter 1.

(a2). Ease of expanding for domestic firms vs. MNCs. The purpose of this exercise is to inquire how the ratio of wages for new vs. incumbent workers changes with the size of an expansion. We compare how this ratio relates to the size of the expansion for domestic firms vs. MNCs. If MNCs are more attractive employers than domestic firms, as firms aim for a larger expansion, this ratio should get relatively smaller for MNCs than for domestic firms.

Let $\ell_{oj,t}$ be the year-t number of employees of firm j (in industry $k(j)$) who work in four-digit occupation o. Let $\overline{w}_{oj,t}^{INC}$ be the year-t average wage of incumbent workers of firm j in occupation o. Incumbent workers are those who worked for firm j in both years $(t-1)$ and t. Finally, let $\overline{w}_{oj,t}^{NEW}$ be the average wage for workers newly-hired by firm j in the same occupation o in year t. The outcome variable is the ratio of the year t average wages for new workers relative to incumbent workers. Specifically,

$$\left(\text{Rel Wages}\right)_{oj,t} = \frac{\overline{w}^{NEW}_{oj,t}}{\overline{w}^{INC}_{oj,t}}.$$

Now let us define the explanatory variable, which measures the change between years $(t-1)$ and t in the number of workers employed in occupation o by firm j, $\Delta log(\ell_{oj,t}) = log(\ell_{oj,t}) - log(\ell_{oj,t-1})$. Going forward, we only consider the cases of expansion $(\Delta log(\ell_{oj,t}) > 0)$.

Table B.8 presents summary statistics for the outcome variable $\left(\left(\text{Rel Wages}\right)_{oj,t}\right)$, the main explanatory variable $(\Delta log(\ell_{oj,t}))$, and the number of workers in each occupation-firm in years $(t-1)$. It is important to emphasize that the average (median) ratio of $\left(\left(\text{Rel Wages}\right)_{oj,t}\right)$ is 0.88 (0.86). This means that new workers hired in a given occupation o typically earn less than the incumbent workers in the same occupation. Our analysis emphasizes how the ratio of wages of new workers to incumbent workers changes with the size of an expansion of the firm in the given occupation, but *does not imply* that the ratio is larger than 1 to begin with.

Table B.8: Summary Statistics for the Sample Used to Study the Differential Ease of Expanding of Domestic Firms vs. MNCs

	N	Mean	Median	SD	Min	Max
$\left(\text{Rel Wages}\right)_{oj,t}$	260,371	0.88	0.86	0.32	0.15	3.40
$\Delta log(\ell_{oj,t})$	260,371	0.52	0.41	0.33	0.001	1.95
$\ell_{oj,t-1}$	260,371	17.60	3	175.79	1	23,913

Notes: Table B.8 presents summary statistics for the sample used to study the differential ease of expanding of domestic firms vs. MNCs. An observation in this analysis is an *occupation* $\times$ *firm* $\times$ *year* $(o \times j \times t)$. The first row reports summary statistics for the outcome variable, $\left(\text{Rel Wages}\right)_{oj,t}$ (the ratio of the year t average wages for new workers relative to incumbent workers). The second row reports summary statistics for the main explanatory variable, $\Delta log(\ell_{oj,t})$ (the increase in the number of workers from year $(t-1)$ to year t). The last row contains descriptive statistics on the number of workers in each occupation-firm in years $(t-1)$. The regression weighs observations according to $\ell_{oj,t-1}$. All wages are inflation-adjusted.

The version of the empirical specification with all interactions is the following:

$$\begin{aligned}\left(\text{Rel Wages}\right)_{oj,t} =& \alpha_j + \lambda_{o \times ind(j) \times t} + \beta_1 \Delta log(\ell_{oj,t}) + \beta_2 \Delta log(\ell_{oj,t}) \mathbb{1}[o = college] + \\ & \beta_3 \Delta log(\ell_{oj,t}) \mathbb{1}[j = MNC] + \beta_4 \Delta log(\ell_{oj,t}) \times \mathbb{1}[o = college] \mathbb{1}[j = MNC] + \\ & \varepsilon_{oj,t}\end{aligned} \tag{B.2}$$

where $\mathbb{1}[o = college]$ is an indicator equal to one if occupation o requires having a college degree (e.g. electronic engineer), and $\mathbb{1}[j = MNC]$ is an indicator equal to one if firm j is an MNC.

Results from regression (B.2) (and its variants) are presented in Table B.9. Columns (1) to (3) do not include the interactions of $\Delta log(\ell_{oj,t})$ with neither $\mathbb{1}[o = college]$ nor $\mathbb{1}[j = MNC]$. Columns (1) to (3) differ among themselves in the fixed effects used. Columns (4) to (6) include the interactions with $\mathbb{1}[o = college]$ and $\mathbb{1}[j = MNC]$. These last columns differ in the fixed effects used. We take column (6) as our baseline specification since it includes the most disaggregated set of controls: firm fixed effects plus the interaction of four-digit occupation × four-digit industry × year fixed effects. However, results are qualitatively similar across specifications.

Table B.9: Findings on the Differential Ease of Expanding of Domestic Firms vs. MNCs

Outcome variable: $(\text{Rel Wages})_{oj,t}$	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta log(\ell_{oj,t})$	0.043***	0.035***	0.030***	0.036***	0.032***	0.029***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$\Delta log(\ell_{oj,t})\mathbb{1}[o = college]$				0.102***	0.069***	0.064***
				(0.006)	(0.006)	(0.008)
$\Delta log(\ell_{oj,t})\mathbb{1}[j = MNC]$				-0.013***	-0.011***	-0.018***
				(0.004)	(0.004)	(0.005)
$\Delta log(\ell_{oj,t})\mathbb{1}[o = college]\mathbb{1}[j = MNC]$				-0.012	-0.036***	-0.030**
				(0.009)	(0.010)	(0.014)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-4D Occup FE	Yes	No	No	Yes	No	No
Year-4D Occup-2D Ind FE	No	Yes	No	No	Yes	No
Year-4D Occup-4D Ind FE	No	No	Yes	No	No	Yes
Observations	260,371	249,352	203,300	260,371	249,352	203,300
Adjusted R^2	0.46	0.53	0.59	0.46	0.53	0.59

Notes: Table B.9 presents the results of the variants of the regression described in equation (B.2). Columns (1) to (3) differ among themselves in the set of fixed effects used. Columns (4) to (6) add three interaction terms to the regressions run in Columns (1) to (3). All wages are inflation-adjusted. The regression weighs observations according to $\ell_{oj,t-1}$. Robust standard errors in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

There are three main takeaways. First, firms, on average, pay higher wages to new employees (relative to incumbent ones), the larger the expansion of a four-digit occupation within the firm. In particular, firms increase the pay of the new workers relative to incumbents by 1.7% more if they double their number of employees in a given occupation as opposed to expanding 50% (the mean). This is consistent with the firm facing an upward-sloping labor supply.

Second, the increase in the relative wage is significantly higher for college-educated occupations. This is consistent with low-skilled workers having a higher labor supply elasticity.

Third and last, both types of firms (MNC and domestic) in both types of college categories face larger relative wages (new workers vs. incumbents), the larger the expansion in the occupation at the firm. However, the increase in the relative wage is around twice as large for domestic firms than for MNCs (both for college and non-college occupations). Thus, MNC firms also face an upward-sloping labor supply, but the elasticity is much higher than the one domestic firms face.

B.1.3 The Indirect Effects of Exposure to MNCs on Workers in Domestic Firms

	First Stage: IV Set 2				Reduced Form: IV Set 2			Placebo Reduced Form: IV Set 2		
Dep. Var.	$\Delta LME_{s(i),t}$ (1)	$\Delta FLE_{j(i),t}$ (2)	$\Delta LME_{s(i),t}$ (3)	$\Delta FLE_{j(i),t}$ (4)	Δw_{it} (5)	Δw_{it} (6)	Δw_{it} (7)	Δw_{it} (8)	Δw_{it} (9)	Δw_{it} (10)
$IV\left(\Delta LME_{s(i),t}\right)$	0.284***		0.284***	-0.003	0.032*		0.032*			
	(0.048)		(0.048)	(0.002)	(0.017)		(0.017)			
$IV\left(\Delta FLE_{j(i),t}\right)$		0.021***	-0.007	0.021***		0.069*	0.070*			
		(0.005)	(0.018)	(0.005)		(0.036)	(0.036)			
$IV\left(\Delta LME_{s(i),t+1}\right)$								0.009		0.009
								(0.025)		(0.025)
$IV\left(\Delta FLE_{j(i),t+1}\right)$									-0.003	-0.003
									(0.036)	(0.036)
Observations	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	2,721,231	2,721,231	2,721,231
Adjusted R^2	0.91	0.46	0.91	0.46	0.045	0.045	0.045	0.047	0.047	0.047

Table B.10: The Effects of Changes in Exposure to MNCs on Workers in Domestic Firms. First Stage, Reduced Form, and Placebo IV for Robustness Check IV Set 2. Stayers Only

Notes: Table B.10 reports the first stage and reduced form estimates for the IV strategy described in Section 3.4. This exercise uses the robustness check IV Set 2 (the instrument using changes in MNC employment outside of Costa Rica for MNCs with subsidiaries in at least one of twenty Latin American and Caribbean countries). The difference between the reduced form estimates in Columns (5) to (7) and those in Columns (8) to (10) is that in the latter columns we use the value of the instrument from the next period $(t+1)$ (instead of the contemporaneous value of the instrument). These regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). All regressions include firm fixed effects, region $\times$ year, two-digit industry $\times$ year, and two-digit industry $\times$ region fixed effects, and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, college education, Costa Rican nationality). Robust standard errors clustered at the level of the firm in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Sample	Main: Stayers Only			Stayers and Movers		
Dep. Var. : Δw_{it}	OLS (1)	IV Set 1 (2)	IV Set 2 (3)	OLS (4)	IV Set 1 (5)	IV Set 2 (6)
$\Delta LME_{s(i),t}$	0.050***	0.143**	0.147**	0.044***	0.114*	0.136*
	(0.016)	(0.066)	(0.072)	(0.017)	(0.065)	(0.073)
$\Delta FLE_{j(i),t}$	0.735***	3.291***	3.365*	0.685***	2.911***	4.135**
	(0.134)	(0.910)	(1.834)	(0.128)	(0.887)	(2.005)
Observations	3,080,017	3,080,017	3,080,017	3,740,151	3,740,151	3,740,151
Adjusted R^2	0.045	0.044	0.044	0.036	0.035	0.034
F-Statistic	-	41.2	8.74	-	44.4	8.89

Table B.11: The Effects of Changes in Exposure to MNCs on Workers in Domestic Firms. OLS and IV. Stayers and Movers

Notes: Table B.11 (B.1.3) reports the OLS and IV estimates for the main specification in equation (3.5) and for two samples: the main sample of stayers, and a sample that includes both the stayers and the movers. Stayers work for firm j in both year $(t-1)$ and t. Movers work for firm j in $(t-1)$, but are no longer observed in j in t. We allow in the sample of movers individuals who move from firm j into unemployment, as long as they find employment by the end of year t. To movers, we assign the firm-level exposure measure of their employer in year $(t-1)$. Δw_{it} is the percentage change in the monthly average labor earnings of worker i between year $(t-1)$ and year t. This exercise uses first the Leading IV (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica) in Columns (2) and (5), then the robustness check IV Set 2 (the instrument using changes in MNC employment outside of Costa Rica for MNCs with subsidiaries in at least one of twenty Latin American and Caribbean countries) in Columns (3) and (6). All regressions include firm fixed effects, region $\times$ year, two-digit industry $\times$ year, and two-digit industry$\times$region fixed effects, and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, college education status, Costa Rican nationality). Robust standard errors clustered at the level of the firm in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rob. Check		Main	Rob. Check: IV Set 1		Main IV1
Dep. Var. : Δw_{it}	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
$\Delta LME_{s(i),t}$	0.051***	0.050***	0.050***	0.130*	0.143**	0.143**
	(0.015)	(0.016)	(0.016)	(0.073)	(0.066)	(0.066)
$\Delta FLE_{j(i),t}$	0.749***	0.735***	0.735***	3.217***	3.291***	3.291***
	(0.138)	(0.134)	(0.134)	(0.914)	(0.910)	(0.910)
Fixed Effects						
Region × Year	No	Yes	Yes	No	Yes	Yes
Two-Digit Industry × Year	Yes	Yes	Yes	Yes	Yes	Yes
Two-Digit Industry × Region	No	No	Yes	No	No	Yes
Observations	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017
F-Statistic				39.7	41.2	41.2

Table B.12: The Effects of Changes in Exposure to MNCs on Workers in Domestic Firms. OLS and IV Estimates for Leading IV and Variations in Fixed Effects. Stayers Only

Notes: Table B.12 reports the OLS and IV estimates for the IV strategy described in Section 3.4. This exercise uses the Leading IV (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). Columns (1), (2), and (3) differ among themselves in the set of fixed effects used. Our preferred set of fixed effects is that in Column (3). Similarly, Columns (4), (5), and (6) differ among themselves in the set of fixed effects used. Our preferred set of fixed effects is that in Column (6). These regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). All regressions include firm fixed effects, and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, college education, Costa Rican nationality). Robust standard errors clustered at the level of the firm in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var. : Δw_{it}	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Panel A: Both With or Without College						
$\Delta LME_{s(i),t}$	0.047***		0.050***	0.111**		0.143**
	(0.015)		(0.016)	(0.053)		(0.066)
$\Delta FLE_{j(i),t}$		0.718***	0.735***		3.269***	3.291***
		(0.137)	(0.134)		(0.909)	(0.910)
Observations	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017
F-Statistic				26.3	83.4	41.2
Panel B: College Educated Only						
$\Delta LME_{s(i),t}$	0.079***		0.085***	0.071		0.070
	(0.030)		(0.030)	(0.078)		(0.079)
$\Delta FLE_{j(i),t}$		1.090***	1.099***		0.909	0.927
		(0.351)	(0.351)		(1.352)	(1.351)
Observations	341,312	341,312	341,312	341,312	341,312	341,312
F-Statistic				27.4	22.6	11.3
Panel C: Without College Only						
$\Delta LME_{s(i),t}$	0.046***		0.049***	0.115**		0.150**
	(0.016)		(0.016)	(0.057)		(0.070)
$\Delta FLE_{j(i),t}$		0.647***	0.664***		3.508***	3.528***
		(0.139)	(0.136)		(0.956)	(0.956)
Observations	2,734,629	2,734,629	2,734,629	2,734,629	2,734,629	2,734,629
F-Statistic				26.2	97.0	47.8

Table B.13: The Effects of Changes in Exposure to MNCs on Workers in Domestic Firms. OLS and IV Estimates for Leading IV. Stayers Only. All, College-Educated Only, Without College Only

Notes: Table B.13 reports the OLS and IV estimates for the IV strategy described in Section 3.4. This exercise uses the Leading IV (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). These regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). Panel A includes all stayers in domestic firms, Panel B includes only those stayers who are college-educated, and Panel C includes only those stayers who are not college-educated. All regressions include firm fixed effects, region × year, two-digit industry × year, and two-digit industry × region fixed effects, and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, sex, and Costa Rican nationality). Robust standard errors clustered at the level of the firm in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var. : Δw_{it}	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Panel A: Both Women and Men						
$\Delta LME_{s(i),t}$	0.047***		0.050***	0.111**		0.143**
	(0.015)		(0.016)	(0.053)		(0.066)
$\Delta FLE_{j(i),t}$		0.718***	0.735***		3.269***	3.291***
		(0.137)	(0.134)		(0.909)	(0.910)
Observations	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017	3,080,017
F-Statistic				26.3	83.4	41.2
Panel B: Women Only						
$\Delta LME_{s(i),t}$	0.046***		0.046***	0.039		0.050
	(0.015)		(0.016)	(0.055)		(0.059)
$\Delta FLE_{j(i),t}$		0.843***	0.845***		2.444**	2.456**
		(0.190)	(0.190)		(1.211)	(1.214)
Observations	974,286	974,286	974,286	974,286	974,286	974,286
F-Statistic				32.2	66.4	32.7
Panel C: Men Only						
$\Delta LME_{s(i),t}$	0.046**		0.050***	0.138**		0.177**
	(0.018)		(0.018)	(0.063)		(0.073)
$\Delta FLE_{j(i),t}$		0.674***	0.695***		3.476***	3.497***
		(0.151)	(0.148)		(0.972)	(0.968)
Observations	2,097,458	2,097,458	2,097,458	2,097,458	2,097,458	2,097,458
F-Statistic				20.3	80.2	40.0

Table B.14: The Effects of Changes in Exposure to MNCs on Workers in Domestic Firms. OLS and IV Estimates for Leading IV. Stayers Only. All, Women Only, Men Only

Notes: Table B.14 reports the OLS and IV estimates for the IV strategy described in Section 3.4. This exercise uses the Leading IV (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). These regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). Panel A includes all stayers in domestic firms, Panel B includes only those stayers who are women, and Panel C includes only those stayers who are men. All regressions include firm fixed effects, region $\times$ year, two-digit industry $\times$ year, and two-digit industry $\times$ region fixed effects, and control for the $(t-1)$ share of total sales to MNCs and a vector of worker characteristics (age, education status, and Costa Rican nationality). Robust standard errors clustered at the level of the firm in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Dep. Var. : Δw_{it}	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Panel A: Both With or Without College				
$\Delta LME_{s(i),t}$	0.047***		0.047***	
	(0.015)		(0.015)	
$\Delta\,(value\text{-}added/worker)_t$		0.008***	0.008***	0.008***
		(0.001)	(0.001)	(0.000)
Observations	3,080,017	3,080,017	3,080,017	3,079,984
Adjusted R^2	0.045	0.046	0.046	0.048
Panel B: College Educated Only				
$\Delta LME_{s(i),t}$	0.079***		0.078***	
	(0.030)		(0.030)	
$\Delta\,(value\text{-}added/worker)_t$		0.009***	0.009***	0.009***
		(0.001)	(0.001)	(0.001)
Observations	341,312	341,312	341,312	340,937
Adjusted R^2	0.067	0.068	0.068	0.070
Panel C: Without College Only				
$\Delta LME_{s(i),t}$	0.046***		0.046***	
	(0.016)		(0.016)	
$\Delta\,(value\text{-}added/worker)_t$		0.008***	0.008***	0.008***
		(0.001)	(0.001)	(0.000)
Observations	2,734,629	2,734,629	2,734,629	2,734,576
Adj. R2	0.045	0.045	0.045	0.047
Fixed Effects				
Region × Year	Yes	Yes	Yes	No
Two-Digit Industry × Year	Yes	Yes	Yes	No
Two-Digit Industry × Region	Yes	Yes	Yes	No
Two-Digit Industry × Region × Year	No	No	No	Yes

Table B.15: The Effects of Changes in Labor Market Exposure to MNCs and in Firm Value Added Per Worker on Workers in Domestic Firms. Stayers Only. All, College-Educated Only, Without College Only

Notes: Table B.15 reports the OLS estimates for the modified main regression described in Section 3.4. The modification, which drives the difference between the exercise in this table and that in Table 3.4, is that instead of the change in firm-level exposure to MNCs we use the change in the value added per worker of the firm (see equation (3.8)). Columns (1) to (4) differ in the explanatory variables used and in the set of fixed effects. When one includes two-digit industry × region × year, one absorbs all variation occurring at the two-digit industry × region level (namely $\Delta LME_{s(i),t}$). All these regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). Panel A includes all workers, both with or without college. Panel B includes only workers with a college education. Panel C includes only workers without a college education.

Dep. Var. : Δw_{it}	IV (1)	IV (2)	IV (3)	IV (4)
Panel A: Both With or Without College				
$\Delta LME_{s(i),t}$	0.111**		0.129**	
	(0.053)		(0.065)	
$\Delta\left(value\text{-}added/worker\right)_t$		0.091***	0.092***	0.092***
		(0.029)	(0.029)	(0.029)
Observations	3,080,017	3,080,017	3,080,017	3,079,984
F-Statistic	26.3	26.3	13.1	24.7
Panel B: College Educated Only				
$\Delta LME_{s(i),t}$	0.071		0.060	
	(0.078)		(0.080)	
$\Delta\left(value\text{-}added/worker\right)_t$		0.024	0.024	0.027
		(0.032)	(0.031)	(0.035)
Observations	341,312	341,312	341,312	340,937
F-Statistic	27.4	4.26	2.14	3.50
Panel C: Without College Only				
$\Delta LME_{s(i),t}$	0.115**		0.139**	
	(0.057)		(0.070)	
$\Delta\left(value\text{-}added/worker\right)_t$		0.099***	0.099***	0.099***
		(0.031)	(0.031)	(0.031)
Observations	2,734,629	2,734,629	2,734,629	2,734,576
F-Statistic	26.2	33.2	16.6	31.5
Fixed Effects				
Region × Year	Yes	Yes	Yes	No
Two-Digit Industry × Year	Yes	Yes	Yes	No
Two-Digit Industry × Region	Yes	Yes	Yes	No
Two-Digit Industry × Region × Year	No	No	No	Yes

Table B.16: The Effects of Changes in Labor Market Exposure to MNCs and in Firm Value Added Per Worker on Workers in Domestic Firms. Stayers Only. All, College-Educated Only, Without College Only. Leading IV

Notes: Table B.16 reports the IV estimates for the modified main regression described in Section 3.4 and for the Leading IV (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). The modification, which drives the difference between the exercise in this table and that in Table 3.4, is that instead of the change in firm-level exposure to MNCs we use the change in the value added per worker of the firm (see equation (3.8)). Columns (1) to (4) differ in the explanatory variables used and in the set of fixed effects. When one includes two-digit industry × region × year, one absorbs all variation occurring at the two-digit industry × region level (namely $\Delta LME_{s(i),t}$). All these regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). Panel A includes all workers, both with or without college. Panel B includes only workers with a college education. Panel C includes only workers without a college education.

Dep. Var. : Δw_{it}	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Panel A: Both Women and Men				
$\Delta LME_{s(i),t}$	0.047***		0.047***	
	(0.015)		(0.015)	
$\Delta\,(\textit{value-added/worker})_t$		0.008***	0.008***	0.008***
		(0.001)	(0.001)	(0.000)
Observations	3,080,017	3,080,017	3,080,017	3,079,984
Adjusted R^2	0.045	0.046	0.046	0.048
Panel B: Women Only				
$\Delta LME_{s(i),t}$	0.046***		0.046***	
	(0.015)		(0.015)	
$\Delta\,(\textit{value-added/worker})_t$		0.008***	0.008***	0.008***
		(0.001)	(0.001)	(0.001)
Observations	974,286	974,286	974,286	974,010
Adjusted R^2	0.039	0.040	0.040	0.041
Panel C: Men Only				
$\Delta LME_{s(i),t}$	0.046**		0.046**	
	(0.018)		(0.018)	
$\Delta\,(\textit{value-added/worker})_t$		0.008***	0.008***	0.008***
		(0.001)	(0.001)	(0.001)
Observations	2,097,458	2,097,458	2,097,458	2,097,375
Adjusted R^2	0.049	0.049	0.049	0.052
Fixed Effects				
Region × Year	Yes	Yes	Yes	No
Two-Digit Industry × Year	Yes	Yes	Yes	No
Two-Digit Industry × Region	Yes	Yes	Yes	No
Two-Digit Industry × Region × Year	No	No	No	Yes

Table B.17: The Effects of Changes in Labor Market Exposure to MNCs and in Firm Value Added Per Worker on Workers in Domestic Firms. Stayers Only. By sex. OLS.

Notes: Table B.17 reports the OLS estimates for the modified main regression described in Section 3.4. The modification, which drives the difference between the exercise in this table and that in Table 3.4, is that instead of the change in firm-level exposure to MNCs we use the change in the value added per worker of the firm (see equation (3.8)). Columns (1) to (4) differ in the explanatory variables used and in the set of fixed effects. When one includes two-digit industry × region × year, one absorbs all variation occurring at the two-digit industry × region level (namely $\Delta LME_{s(i),t}$). All these regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). Panel A includes all workers, both female and male. Panel B includes only the women. Panel C includes only the men.

Dep. Var. : Δw_{it}	IV (1)	IV (2)	IV (3)	IV (4)
Panel A: Both Women and Men				
$\Delta LME_{s(i),t}$	0.111**		0.129**	
	(0.053)		(0.065)	
$\Delta\left(value\text{-}added/worker\right)_t$		0.091***	0.092***	0.092***
		(0.029)	(0.029)	(0.029)
Observations	3,080,017	3,080,017	3,080,017	3,079,984
F-Statistic	26.3	26.3	13.1	24.7
Panel B: Women Only				
$\Delta LME_{s(i),t}$	0.039		0.052	
	(0.055)		(0.062)	
$\Delta\left(value\text{-}added/worker\right)_t$		0.067*	0.068*	0.065*
		(0.036)	(0.036)	(0.036)
Observations	974,286	974,286	974,286	974,010
F-Statistic	32.2	13.4	6.70	13.0
Panel C: Men Only				
$\Delta LME_{s(i),t}$	0.138**		0.158**	
	(0.063)		(0.076)	
$\Delta\left(value\text{-}added/worker\right)_t$		0.098***	0.099***	0.100***
		(0.031)	(0.031)	(0.032)
Observations	2,097,458	2,097,458	2,097,458	2,097,375
F-Statistic	20.3	28.7	14.4	27.0
Fixed Effects				
Region × Year	Yes	Yes	Yes	No
Two-Digit Industry × Year	Yes	Yes	Yes	No
Two-Digit Industry × Region	Yes	Yes	Yes	No
Two-Digit Industry × Region × Year	No	No	No	Yes

Table B.18: The Effects of Changes in Labor Market Exposure to MNCs and in Firm Value Added Per Worker on Workers in Domestic Firms. Stayers Only. By sex. IV.

Notes: Table B.18 reports the IV estimates for the modified main regression described in Section 3.4 and for the Leading IV (the instrument using changes in MNC employment outside Costa Rica for the same MNCs with subsidiaries in Costa Rica). The modification, which drives the difference between the exercise in this table and that in Table 3.4, is that instead of the change in firm-level exposure to MNCs, we use the change in the value added per worker of the firm (see equation (3.8)). Columns (1) to (4) differ in the explanatory variables used and in the set of fixed effects. When one includes two-digit industry × region × year, one absorbs all variation occurring at the two-digit industry × region level (namely $\Delta LME_{s(i),t}$). All these regressions include only stayers (i.e., workers in domestic firms who stay in the same domestic firm in both year $(t-1)$ and t). Panel A includes all workers, both female and male. Panel B includes only women. Panel C includes only men.

B.2 Additional Model Derivations

B.2.1 Log-Linearization of the FOCs

Log-Linearization of the FOCs of the Domestic Firms' Problem

The equilibrium of the profit maximization of a domestic firm j is described by the following sets of equations:

$$W_j = \frac{\eta_I}{\eta_I+1}\frac{\sigma-1}{\sigma}A_j\left(\frac{W_j^{\eta_I}}{\Omega_{js}^{\eta_I}}I_j^0 + N_j\right)^{-\frac{1}{\sigma}} \forall j, \tag{B.3}$$

$$\frac{\sigma-1}{\sigma}A_j\left(\frac{W_j^{\eta_I}}{\Omega_{js}^{\eta_I}}I_j^0 + N_j\right)^{-\frac{1}{\sigma}} - c'(N_j) = \omega_{s(j)}\forall j, \tag{B.4}$$

where $\Omega_{js} \equiv \Omega_{js}(W_j, \tilde{\boldsymbol{\omega}}) = \left(W_j^{\eta_I} + \sum_{s'}(\tau_{s(j)s'}\tilde{\omega}_{s'})^{\eta_I}\right)^{1/\eta_I}$. Equations (B.3) and (B.4) are the FOCs of the domestic firm problem.

Then, we add the FOCs of the MNC problem:

$$\frac{\sigma-1}{\sigma}A_{MNC(s)}N_{MNC(s)}^{-\frac{1}{\sigma}} = \psi_s\omega_s + C'_{MNC(s)}\left(N_{MNC(s)}\right) = \psi_s\omega_s + c_0 N_{MNC}^{\alpha_m}. \tag{B.5}$$

$$J_{MNC(s)} = \left(\frac{\sigma-1}{\sigma}\right)^{\sigma}\left(\frac{A_{MNC(s)}}{P_{MNC(s)}}\right)^{\sigma}. \tag{B.6}$$

Let us first log-linearize equations (B.3) and (B.4) with respect to W_j, N_j, A_j, ω_s and Ω_{js}. Variables with hats denote log differences ($\hat{x} \equiv \log(x) - \log(\overline{x})$) and variables with an overline denote initial equilibrium values.

$$\widehat{W}_j = \widehat{A}_j - \frac{1}{\sigma}\widehat{L}_j = \widehat{A}_j - \frac{1}{\sigma}\left[\frac{\overline{I}_j}{\overline{L}_j}\eta_I(\widehat{W}_j - \widehat{\Omega}_{js}) + \frac{\overline{N}_j}{\overline{L}_j}\widehat{N}_j\right]$$

$$\widehat{A}_j - \frac{1}{\sigma}\left[\frac{\overline{I}_j}{\overline{L}_j}\eta_I(\widehat{W}_j - \widehat{\Omega}_{js}) + \frac{\overline{N}_j}{\overline{L}_j}\widehat{N}_j\right] = \frac{c_0\overline{N_j^{\alpha}}}{c_0\overline{N_j^{\alpha}}+\overline{\omega_s}}\alpha\widehat{N}_j + \frac{\overline{\omega_s}}{c_0\overline{N_j^{\alpha}}+\overline{\omega_s}}\widehat{\omega}_s.$$

Define $\xi_j^I \equiv \frac{\overline{I}_j}{\overline{L}_j}$, $\xi_j^N \equiv \frac{\overline{N}_j}{\overline{L}_j}$, $\xi_j^C \equiv \frac{C'(\overline{N}_j)}{C'(\overline{N}_j)+\overline{\omega_s}} = \frac{C'(\overline{N}_j)}{MRP_j} = \frac{c_0\overline{N_j^{\alpha}}}{MRP_j}$, and $\xi_j^O \equiv \frac{\overline{\omega_s}}{c_0\overline{N_j^{\alpha}}+\overline{\omega_s}}$ (where $\xi_j^I + \xi_j^N = 1$ and $\xi_j^C + \xi_j^O = 1$). Then:

$$\widehat{W}_j = \widehat{A}_j - \frac{1}{\sigma}\left[\xi_j^I\eta_I(\widehat{W}_j - \widehat{\Omega}_{js}) + (1-\xi_j^I)\widehat{N}_j\right]$$

$$\widehat{A}_j - \frac{1}{\sigma}\left[\xi_j^I\eta_I(\widehat{W}_j - \widehat{\Omega}_{js}) + (1-\xi_j^I)\widehat{N}_j\right] = \xi_j^C\alpha\widehat{N}_j + (1-\xi_j^C)\widehat{\omega}_s.$$

Rearranging:

$$\begin{aligned}\widehat{W}_j\left(\sigma + \xi_j^I\eta_I\right) =& \sigma\widehat{A}_j - (1-\xi_j^I)\widehat{N}_j + \xi_j^I\eta_I\widehat{\Omega}_{js}\\ \widehat{W}_j =& \frac{\sigma}{\sigma+\xi_j^I\eta_I}\widehat{A}_j - \frac{(1-\xi_j^I)}{\sigma+\xi_j^I\eta_I}\widehat{N}_j + \frac{\xi_j^I\eta_I}{\sigma+\xi_j^I\eta_I}\widehat{\Omega}_{js}\end{aligned} \tag{B.7}$$

Then

$$\begin{aligned}
\tfrac{\sigma}{\sigma+\xi_j^I\eta_I}\widehat{A}_j - \tfrac{(1-\xi_j^I)}{\sigma+\xi_j^I\eta_I}\widehat{N}_j + \tfrac{\xi_j^I\eta_I}{\sigma+\xi_j^I\eta_I}\widehat{\Omega}_{js} =& \xi_j^C\alpha\widehat{N}_j + (1-\xi_j^C)\widehat{\omega}_s \\
\left(\xi_j^C\alpha + \tfrac{(1-\xi_j^I)}{\sigma+\xi_j^I\eta_I}\right)\widehat{N}_j =& \tfrac{\sigma}{\sigma+\xi_j^I\eta_I}\widehat{A}_j - (1-\xi_j^C)\widehat{\omega}_s + \tfrac{\xi_j^I\eta_I}{\sigma+\xi_j^I\eta_I}\widehat{\Omega}_{js} \\
\tfrac{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}{\sigma+\xi_j^I\eta_I}\widehat{N}_j =& \tfrac{\sigma}{\sigma+\xi_j^I\eta_I}\widehat{A}_j - \tfrac{(1-\xi_j^C)(\sigma+\xi_j^I\eta_I)}{\sigma+\xi_j^I\eta_I}\widehat{\omega}_s + \tfrac{\xi_j^I\eta_I}{\sigma+\xi_j^I\eta_I}\widehat{\Omega}_{js}
\end{aligned}$$

$$\widehat{N}_j = \tfrac{\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j - \tfrac{(1-\xi_j^C)(\sigma+\xi_j^I\eta_I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \tfrac{\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\Omega}_{js}. \tag{B.8}$$

Now replace $\widehat{N}_j$ from equation (B.8) into equation (B.7) to obtain:

$$\begin{aligned}
\widehat{W}_j =& \tfrac{\sigma}{\sigma+\xi_j^I\eta_I}\widehat{A}_j - \\
&\tfrac{(1-\xi_j^I)}{\sigma+\xi_j^I\eta_I}\left(\tfrac{\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j - \tfrac{(1-\xi_j^C)(\sigma+\xi_j^I\eta_I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \tfrac{\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\Omega}_{js}\right) \\
&+ \tfrac{\xi_j^I\eta_I}{\sigma+\xi_j^I\eta_I}\widehat{\Omega}_{js} \\
=& \tfrac{\sigma}{\sigma+\xi_j^I\eta_I}\left(1 - \tfrac{(1-\xi_j^I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\right)\widehat{A}_j + \tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \\
&+ \tfrac{\xi_j^I\eta_I}{\sigma+\xi_j^I\eta_I}\left(1 - \tfrac{\xi_j^N}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\right)\widehat{\Omega}_{js} \\
=& \tfrac{\sigma}{\sigma+\xi_j^I\eta_I}\tfrac{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j + \tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \\
&+ \tfrac{\xi_j^I\eta_I}{\sigma+\xi_j^I\eta_I}\tfrac{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\Omega}_{js}
\end{aligned}$$

$$\widehat{W}_j = \tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j + \tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\Omega}_{js}. \tag{B.9}$$

Hereafter, we write together the versions of equations (B.8) and (B.9), where we do not yet replace the log-deviation of $\Omega_{js} = \left(W_j^{\eta_I} + \sum_{s'}(\tau_{s(j)s'}\tilde{\omega}_{s'})^{\eta_I}\right)^{\frac{1}{\eta_I}}$:

$$\begin{aligned}
\widehat{N}_j =& \tfrac{\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j - \tfrac{(1-\xi_j^C)(\sigma+\xi_j^I\eta_I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \tfrac{\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\Omega}_{js} \\
\widehat{W}_j =& \tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j + \tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\Omega}_{js}.
\end{aligned} \tag{B.10}$$

Recall that $\Omega_{js}^{\eta_I} = W_j^{\eta_I} + \sum_{s'}(\tau_{s(j)s'}\tilde{\omega}_{s'})^{\eta_I}$. Define $\pi_{js'} \equiv \frac{\left(\tau_{ss'}\bar{\omega}_{s'}\right)^{\eta_I}}{\overline{\Omega}_{js}^{\eta_I}}$ and $\pi_{jj} \equiv \frac{\overline{W}_j^{\eta_I}}{\overline{\Omega}_{js}^{\eta_I}}$.

$$\widehat{\Omega}_{js} = \frac{\overline{W}_j^{\eta_I}}{\overline{\Omega}_{js}^{\eta_I}}\widehat{W}_j + \sum_{s'}\frac{\left(\tau_{ss'}\bar{\omega}_{s'}\right)^{\eta_I}}{\overline{\Omega}_{js}^{\eta_I}}\widehat{\tilde{\omega}}_{s'} = \pi_{jj}\widehat{W}_j + \sum_{s'}\pi_{js'}\widehat{\tilde{\omega}}_{s'} \tag{B.11}$$

We now replace the expression for $\widehat{\Omega}_{js}$ into equations (B.8) and (B.9)

$$\begin{aligned}\widehat{N}_j =& \tfrac{\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j - \tfrac{(1-\xi_j^C)(\sigma+\xi_j^I\eta_I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s \\ &+ \tfrac{\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(\pi_{jj}\widehat{W}_j + \sum_{s'}\pi_{js'}\widehat{\tilde{\omega}}_{s'}\right). \end{aligned} \tag{B.12}$$

$$\begin{aligned}\widehat{W}_j =& \tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j + \tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s \\ &+ \tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(\pi_{jj}\widehat{W}_j + \sum_{s'}\pi_{js'}\widehat{\tilde{\omega}}_{s'}\right) \\ &\left(1 - \tfrac{\xi_j^C\xi_j^I\alpha\eta_I\pi_{jj}}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\right)\widehat{W}_j = \tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j + \tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s \\ &+ \tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\sum_{s'}\pi_{js'}\widehat{\tilde{\omega}}_{s'} \\ &\tfrac{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{W}_j = \tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j + \\ &\tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\sum_{s'}\pi_{js'}\widehat{\tilde{\omega}}_{s'}\end{aligned}$$

$$\begin{aligned}\widehat{W}_j =& \tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\widehat{A}_j + \tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\widehat{\omega}_s + \\ &+ \tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\widehat{\tilde{\omega}}_{s'}.\end{aligned} \tag{B.13}$$

Now, recall that $\tilde{\omega}_{s'} \equiv \omega_{s'}\left[1 + (\psi_{s'} - 1)\frac{N_{MNC(s')}}{N_{s'}}\right]$. Let us assume that the MNC premia ($\psi_{s'}$) do not change with time, which is consistent with our empirical evidence. Then, we can replace $\widehat{\tilde{\omega}}_{s'}$ with $\widehat{\omega}_{s'} + \frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\left(\widehat{N}_{MNC(s')} - \widehat{N}_{s'}\right)$ into equation (B.13). This leads to:

$$\begin{aligned}\widehat{W}_j =& \tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\widehat{A}_j + \tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\widehat{\omega}_s \\ &+ \tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\widehat{\omega}_{s'} \\ &+ \tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\tfrac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\left(\widehat{N}_{MNC(s')} - \widehat{N}_{s'}\right),\end{aligned} \tag{B.14}$$

Note that to the extent that MNCs do not pay a premium with respect to domestic employers (i.e., $\psi_{s'} = 1$) or that the growth rate of the number of MNC workers is not different from the growth rate of new workers in the entire industry, then the economy would collapse to one where the wages of incumbent workers in domestic firms are not explicitly responding to changes in the composition of employment towards or away MNCs. Equation (B.14) is the one we estimate in Section 3.5.5 to recover the structural parameters of interest.

Log-Linearization of the FOCs of the MNC Problem

Let us first log-linearize equation (B.5) with respect to $N_{MNC(s)}$, $A_{MNC(s)}$, and ω_s:

$$\tfrac{\sigma-1}{\sigma} A_{MNC(s)} N_{MNC(s)}^{-\frac{1}{\sigma}} = \psi_s \omega_s + C'_{MNC(s)}\left(N_{MNC(s)}\right) = \psi_s\omega_s + c_0 N_{MNC}^{\alpha_m}. \tag{B.15}$$

$$\widehat{A}_{MNC(s)} - \tfrac{1}{\sigma}\widehat{N}_{MNC(s)} = \tfrac{\psi_s\overline{\omega}_s}{\psi_s\overline{\omega}_s + c_0\overline{N}_{MNC(s)}^{\alpha_m}}\widehat{\omega}_s + \tfrac{c_0\overline{N}_{MNC(s)}^{\alpha_m}}{\psi_s\overline{\omega}_s + c_0\overline{N}_{MNC(s)}^{\alpha_m}}\alpha_m\widehat{N}_{MNC(s)}. \tag{B.16}$$

Define $\xi_{MNC(s)}^C = \frac{c_0\overline{N}_{MNC(s)}^{\alpha_m}}{\psi_s\overline{\omega}_s + c_0\overline{N}_{MNC(s)}^{\alpha_m}}$. Then

$$\widehat{A}_{MNC(s)} - \tfrac{1}{\sigma}\widehat{N}_{MNC(s)} = \left(1 - \xi_{MNC(s)}^C\right)\widehat{\omega}_s + \xi_{MNC(s)}^C\alpha_m\widehat{N}_{MNC(s)}. \tag{B.17}$$

Therefore

$$\widehat{N}_{MNC(s)} = \tfrac{\sigma}{1+\xi_{MNC(s)}^C\alpha_m\sigma}\widehat{A}_{MNC(s)} - \tfrac{\sigma\left(1-\xi_{MNC(s)}^C\right)}{1+\xi_{MNC(s)}^C\alpha_m\sigma}\widehat{\omega}_s. \tag{B.18}$$

B.2.2 Log-Linearization of the Labor Market Clearing Condition

The labor market clearing condition for new workers in industry s is given by:

$$N_s \equiv N_{MNC(s)} + \sum_{j\in\mathcal{D}_s} N_j = \tfrac{\tilde{\omega}_s^{\eta_N}}{\Omega_N^{\eta_N}} L_N^0 + \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \tfrac{\left(\tau_{s'(j')s}\tilde{\omega}_s\right)^{\eta_I}}{\Omega_{j's'}^{\eta_I}} I_{j'}^0 \forall s, \tag{B.19}$$

where $\Omega_N = \left(\sum_{s'}\tilde{\omega}_{s'}^{\eta_N}\right)^{1/\eta_N}$, $\Omega_{js} \equiv \Omega_{js}(W_j, \tilde{\boldsymbol{\omega}}) = \left(W_j^{\eta_I} + \sum_{s'}(\tau_{s(j)s'}\tilde{\omega}_{s'})^{\eta_I}\right)^{1/\eta_I}$, and $\Omega_{j's'} \equiv \Omega_{j's'}(W_{j'}, \tilde{\boldsymbol{\omega}}) = \left(W_{j'}^{\eta_I} + \sum_{s''}(\tau_{s'(j')s''}\tilde{\omega}_{s''})^{\eta_I}\right)^{1/\eta_I}$. The RHS term is the overall demand for new workers by the MNC in s and all domestic firms j in s (set denoted by $\mathcal{D}_s$). The LHS term is the overall supply of new workers, who are either newly-entered in the labor market at the beginning of the period (the first term) or incumbents who break ties with their beginning-of-period employer j' in industry s' to join industry s.

Last, the product markets clear when the total production of the variety of each domestic firm j is equal to the total demand (coming from the demand of the domestic market and the demand coming from all its MNC buyers, if any). We have already incorporated this condition in the definition of firm revenues in equation (3.12).

Let us now log-linearize the labor market clearing condition introduced in equation (B.19) with respect to $N_{MNC(s)}$, N_j, ω_s, Ω_N, and $\Omega_{j's'}$.

$$N_{MNC(s)} + \sum_{j\in\mathcal{D}_s} N_j = \tfrac{\tilde{\omega}_s^{\eta_N}}{\Omega_N^{\eta_N}} L_N^0 + \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \tfrac{\left(\tau_{s'(j')s}\tilde{\omega}_s\right)^{\eta_I}}{\Omega_{j's'}^{\eta_I}} I_{j'}^0 \forall s, \tag{B.20}$$

where $\Omega_N = \left(\sum_{s'} \tilde{\omega}_{s'}^{\eta_N}\right)^{1/\eta_N}$, $\Omega_{js} \equiv \Omega_{js}(W_j, \tilde{\boldsymbol{\omega}}) = \left(W_j^{\eta_I} + \sum_{s'}(\tau_{s(j)s'}\tilde{\omega}_{s'})^{\eta_I}\right)^{1/\eta_I}$, and $\Omega_{j's'} \equiv \Omega_{j's'}(W_{j'}, \tilde{\boldsymbol{\omega}}) = \left(W_{j'}^{\eta_I} + \sum_{s''}(\tau_{s'(j')s''}\tilde{\omega}_{s''})^{\eta_I}\right)^{1/\eta_I}$.

To that end, define

$$N_s \equiv N_{MNC(s)} + \sum_{j \in \mathcal{D}_s} N_j,$$

$$L_{Ns} \equiv \tfrac{\tilde{\omega}_s^{\eta_N}}{\Omega_N^{\eta_N}} L_N^0,$$

$$I_s \equiv \sum_{s'} \sum_{j' \in \mathcal{D}_{s'}} \tfrac{\left(\tau_{s'(j')s}\tilde{\omega}_s\right)^{\eta_I}}{\Omega_{j's'}^{\eta_I}} I_{j'}^0.$$

Equation (B.19) can be rewritten as $N_s = L_{Ns} + I_s$. Then

$$\widehat{N}_s = \tfrac{\overline{L}_{Ns}}{\overline{N}_s}\widehat{L}_{Ns} + \tfrac{\overline{I}_s}{\overline{N}_s}\widehat{I}_s = \Psi_s^N \widehat{L}_{Ns} + \Psi_s^I \widehat{I}_s, \tag{B.21}$$

where $\Psi_s^N \equiv \frac{\overline{L}_{Ns}}{\overline{N}_s}$ and $\Psi_s^I \equiv \frac{\overline{I}_s}{\overline{N}_s} = 1 - \Psi_s^N$. $\widehat{N}_s = \frac{\overline{N}_{MNC(s)}}{\overline{N}_s}\widehat{N}_{MNC(s)} + \sum_{j \in \mathcal{D}_s} \frac{\overline{N}_j}{\overline{N}_s}\widehat{N}_j$. Define $\chi_{MNC(s)}^N \equiv \frac{\overline{N}_{MNC(s)}}{\overline{N}_s}$ and $\chi_j^N \equiv \frac{\overline{N}_j}{\overline{N}_s}$. Hence, the left-hand side of equation (B.21) is equal to

$$\widehat{N}_s = \chi_{MNC(s)}^N \widehat{N}_{MNC(s)} + \sum_{j \in \mathcal{D}_s} \chi_j^N \widehat{N}_j. \tag{B.22}$$

Then:

$$\widehat{L}_{Ns} = \eta_N \left(\widehat{\tilde{\omega}}_s - \widehat{\Omega}_N\right). \tag{B.23}$$

Now, we are left with deriving $\widehat{I}_s$. To that end, define $Z_{j'}^s = \frac{\left(\tau_{s'(j')s}\tilde{\omega}_s\right)^{\eta_I}}{\Omega_{j's'}^{\eta_I}} I_{j'}^0$. Then

$$\widehat{I}_s = \sum_{s'} \sum_{j' \in \mathcal{D}_{s'}} \tfrac{\overline{Z}_{j'}^s}{\overline{I}_s} \widehat{Z_{j'}^s} = \sum_{s'} \sum_{j' \in \mathcal{D}_{s'}} \zeta_{j'}^s \eta_I \left(\widehat{\tilde{\omega}}_s - \widehat{\Omega}_{j's'}\right), \tag{B.24}$$

where $\zeta_{j'}^s \equiv \dfrac{\frac{\left(\tau_{s'(j')s}\overline{\tilde{\omega}}_s\right)^{\eta_I}}{\overline{\Omega}_{j's'}{}^{\eta_I}} I_{j'}^0}{\sum_{s''}\sum_{j'' \in \mathcal{D}_{s''}} \frac{\left(\tau_{s''(j'')s}\overline{\tilde{\omega}}_s\right)^{\eta_I}}{\overline{\Omega}_{j''s''}^{\eta_I}} I_{j''}^0} = \frac{\overline{Z}_{j'}^s}{\overline{I}_s}$. We now replace $\widehat{N}_s, \widehat{L}_{Ns}, \widehat{I}_s$ from equations (B.22), (B.23), and (B.24) into equation (B.21):

$$\chi_{MNC(s)}^N \widehat{N}_{MNC(s)} + \sum_{j \in \mathcal{D}_s} \chi_j^N \widehat{N}_j = \Psi_s^N \eta_N \left(\widehat{\tilde{\omega}}_s - \widehat{\Omega}_N\right) + \Psi_s^I \sum_{s'} \sum_{j' \in \mathcal{D}_{s'}} \zeta_{j'}^s \eta_I \left(\widehat{\tilde{\omega}}_s - \widehat{\Omega}_{j's'}\right). \tag{B.25}$$

To make progress, we need to find expressions for the change in the number of workers hired. To do that, replace $\widehat{W}_j$ from equation (B.13) into equation (B.12):

$$\begin{aligned}
\widehat{N}_j = {} & \tfrac{\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{A}_j - \tfrac{(1-\xi_j^C)(\sigma+\xi_j^I\eta_I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\widehat{\omega}_s + \tfrac{\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\sum_{s'}\pi_{js'}\widehat{\widetilde{\omega}}_{s'} \\
& + \tfrac{\xi_j^I\eta_I\pi_{jj}}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\widehat{A}_j + \\
& + \tfrac{\xi_j^I\eta_I\pi_{jj}}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\widehat{\omega}_s \\
& + \tfrac{\xi_j^I\eta_I\pi_{jj}}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\widehat{\widetilde{\omega}}_{s'}
\end{aligned}$$

$$\begin{aligned}
\widehat{N}_j = {} & \left(\tfrac{\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)} + \tfrac{\xi_j^I\eta_I\pi_{jj}}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\tfrac{\xi_j^C\alpha\sigma}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\widehat{A}_j \\
& - \left(\tfrac{(1-\xi_j^C)(\sigma+\xi_j^I\eta_I)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)} - \tfrac{\xi_j^I\eta_I\pi_{jj}}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\tfrac{(1-\xi_j^C)(1-\xi_j^I)}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\widehat{\omega}_s \\
& + \left(\tfrac{\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)} + \tfrac{\xi_j^I\eta_I\pi_{jj}}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\tfrac{\xi_j^C\xi_j^I\alpha\eta_I}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\sum_{s'}\pi_{js'}\widehat{\widetilde{\omega}}_{s'}
\end{aligned}$$

$$\begin{aligned}
\widehat{N}_j = {} & \left[\tfrac{\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(1+\tfrac{\xi_j^I\xi_j^C\eta_I\alpha\pi_{jj}}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\right]\widehat{A}_j \\
& - \left[\tfrac{(1-\xi_j^C)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(\sigma+\tfrac{\xi_j^I\eta_I[\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})]-\xi_j^I(1-\xi_j^I)\eta_I\pi_{jj}}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\right]\times \\
& \times\widehat{\omega}_s + \left[\tfrac{\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(1+\tfrac{\xi_j^I\xi_j^C\eta_I\alpha\pi_{jj}}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\right]\sum_{s'}\pi_{js'}\widehat{\widetilde{\omega}}_{s'} \qquad \text{(B.26)}
\end{aligned}$$

Next, we replace the $\widehat{N}_{MNC(s)}$ and $\widehat{N}_j$ in the left-hand side (LHS) of equation (B.25) with the expressions found in equations (B.18) and (B.26):

$$\begin{aligned}
LHS = \widehat{N}_s = {} & \chi_{MNC(s)}^N\widehat{N}_{MNC(s)} \\
& + \sum_{j\in\mathcal{D}_s}\left[\tfrac{\chi_j^N\sigma}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(1+\tfrac{\xi_j^I\xi_j^C\eta_I\alpha\pi_{jj}}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\right]\widehat{A}_j \\
& - \sum_{j\in\mathcal{D}_s}\left[\tfrac{\chi_j^N(1-\xi_j^C)}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(\sigma+\tfrac{\xi_j^I\eta_I[\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})]-\xi_j^I(1-\xi_j^I)\eta_I\pi_{jj}}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\right]\times \\
& \times\widehat{\omega}_s + \sum_{j\in\mathcal{D}_s}\left[\tfrac{\chi_j^N\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(1+\tfrac{\xi_j^I\xi_j^C\eta_I\alpha\pi_{jj}}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\right]\sum_{s'}\pi_{js'}\widehat{\omega}_{s'} \\
& + \sum_{j\in\mathcal{D}_s}\left[\tfrac{\chi_j^N\xi_j^I\eta_I}{\xi_j^C\alpha(\sigma+\xi_j^I\eta_I)+(1-\xi_j^I)}\left(1+\tfrac{\xi_j^I\xi_j^C\eta_I\alpha\pi_{jj}}{\xi_j^C\alpha\sigma+(1-\xi_j^I)+\xi_j^C\xi_j^I\alpha\eta_I(1-\pi_{jj})}\right)\right]\times \\
& \times\sum_{s'}\pi_{js'}\tfrac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\left(\widehat{N}_{MNC(s')}-\widehat{N}_{s'}\right). \qquad \text{(B.27)}
\end{aligned}$$

where $\widehat{N}_{MNC(s)} = \frac{\sigma}{1+\xi^C_{MNC(s)}\alpha_m\sigma}\widehat{A}_{MNC(s)} - \frac{\sigma\left(1-\xi^C_{MNC(s)}\right)}{1+\xi^C_{MNC(s)}\alpha_m\sigma}\widehat{\omega}_s$, $\widehat{N}_{MNC(s')} = \frac{\sigma}{1+\xi^C_{MNC(s')}\alpha_m\sigma}\widehat{A}_{MNC(s')} - \frac{\sigma\left(1-\xi^C_{MNC(s')}\right)}{1+\xi^C_{MNC(s')}\alpha_m\sigma}\widehat{\omega}_{s'}$.

$$\begin{aligned}
LHS = \widehat{N}_s = \chi^N_{MNC(s)} & \frac{\sigma}{1+\xi^C_{MNC(s)}\alpha_m\sigma}\widehat{A}_{MNC(s)} \\
& + \sum_{j\in\mathcal{D}_s}\left[\frac{\chi^N_j\xi^I_j\eta_I}{\xi^C_j\alpha(\sigma+\xi^I_j\eta_I)+(1-\xi^I_j)}\left(1+\frac{\xi^I_j\xi^C_j\eta_I\alpha\pi_{jj}}{\xi^C_j\alpha\sigma+(1-\xi^I_j)+\xi^C_j\xi^I_j\alpha\eta_I(1-\pi_{jj})}\right)\right]\times \\
& \quad \times\sum_{s'}\pi_{js'}\frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\frac{\sigma}{1+\xi^C_{MNC(s')}\alpha_m\sigma}\widehat{A}_{MNC(s')} \\
& + \sum_{j\in\mathcal{D}_s}\left[\frac{\chi^N_j\sigma}{\xi^C_j\alpha(\sigma+\xi^I_j\eta_I)+(1-\xi^I_j)}\left(1+\frac{\xi^I_j\xi^C_j\eta_I\alpha\pi_{jj}}{\xi^C_j\alpha\sigma+(1-\xi^I_j)+\xi^C_j\xi^I_j\alpha\eta_I(1-\pi_{jj})}\right)\right]\widehat{A}_j \\
& \qquad - \chi^N_{MNC(s)}\frac{\sigma\left(1-\xi^C_{MNC(s)}\right)}{1+\xi^C_{MNC(s)}\alpha_m\sigma}\widehat{\omega}_s \\
- \sum_{j\in\mathcal{D}_s} & \left[\frac{\chi^N_j(1-\xi^C_j)}{\xi^C_j\alpha(\sigma+\xi^I_j\eta_I)+(1-\xi^I_j)}\left(\sigma+\frac{\xi^I_j\eta_I[\xi^C_j\alpha\sigma+(1-\xi^I_j)+\xi^C_j\xi^I_j\alpha\eta_I(1-\pi_{jj})]-\xi^I_j(1-\xi^I_j)\eta_I\pi_{jj}}{\xi^C_j\alpha\sigma+(1-\xi^I_j)+\xi^C_j\xi^I_j\alpha\eta_I(1-\pi_{jj})}\right)\right]\times \\
\times\widehat{\omega}_s & + \sum_{j\in\mathcal{D}_s}\left[\frac{\chi^N_j\xi^I_j\eta_I}{\xi^C_j\alpha(\sigma+\xi^I_j\eta_I)+(1-\xi^I_j)}\left(1+\frac{\xi^I_j\xi^C_j\eta_I\alpha\pi_{jj}}{\xi^C_j\alpha\sigma+(1-\xi^I_j)+\xi^C_j\xi^I_j\alpha\eta_I(1-\pi_{jj})}\right)\right]\sum_{s'}\pi_{js'}\widehat{\omega}_{s'} \\
& - \sum_{j\in\mathcal{D}_s}\left[\frac{\chi^N_j\xi^I_j\eta_I}{\xi^C_j\alpha(\sigma+\xi^I_j\eta_I)+(1-\xi^I_j)}\left(1+\frac{\xi^I_j\xi^C_j\eta_I\alpha\pi_{jj}}{\xi^C_j\alpha\sigma+(1-\xi^I_j)+\xi^C_j\xi^I_j\alpha\eta_I(1-\pi_{jj})}\right)\right]\times \\
& \quad \times\sum_{s'}\pi_{js'}\frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\frac{\sigma\left(1-\xi^C_{MNC(s')}\right)}{1+\xi^C_{MNC(s')}\alpha_m\sigma}\widehat{\omega}_{s'} \\
& - \sum_{j\in\mathcal{D}_s}\left[\frac{\chi^N_j\xi^I_j\eta_I}{\xi^C_j\alpha(\sigma+\xi^I_j\eta_I)+(1-\xi^I_j)}\left(1+\frac{\xi^I_j\xi^C_j\eta_I\alpha\pi_{jj}}{\xi^C_j\alpha\sigma+(1-\xi^I_j)+\xi^C_j\xi^I_j\alpha\eta_I(1-\pi_{jj})}\right)\right]\times \\
& \qquad \times\sum_{s'}\pi_{js'}\frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\widehat{N}_{s'}.
\end{aligned} \tag{B.28}$$

Therefore, the LHS terms for industry s equalizes $\widehat{N}_s$ to three large weighted sums: i) one weighted sum over the $\widehat{A}$s of all domestic firms and MNCs in the economy (denote the total number of firms in the economy by $|k|$), (ii) another weighted sum over the $\widehat{\omega}$ of all the industries in the economy (denote the number of industries in the economy by $|s|$), and (iii) last, a weighted sum over the $\widehat{N}$ of all industries in the economy.

By using all the labor market clearing conditions across all industries, one can write the following system:

$$\widehat{\boldsymbol{N}}_{|s|\times 1} = \boldsymbol{P}_{|s|\times|k|}\widehat{\boldsymbol{A}}_{|k|\times 1} + \boldsymbol{R}_{|s|\times|s|}\widehat{\boldsymbol{\omega}}_{|s|\times 1} + \boldsymbol{Q}_{|s|\times|s|}\widehat{\boldsymbol{N}}_{|s|\times 1},$$

where the subscripts denote the dimensions of each matrix. We can rewrite the system by isolating $\widehat{\boldsymbol{N}}_{|s|\times 1}$:

$$\widehat{\boldsymbol{N}}_{|s|\times 1} = \left(\boldsymbol{I}_{|s|\times|s|} - \boldsymbol{Q}_{|s|\times|s|}\right)^{-1} \boldsymbol{P}_{|s|\times|k|}\widehat{\boldsymbol{A}}_{|k|\times 1} + \left(\boldsymbol{I}_{|s|\times|s|} - \boldsymbol{Q}_{|s|\times|s|}\right)^{-1} \boldsymbol{R}_{|s|\times|s|}\widehat{\boldsymbol{\omega}}_{|s|\times 1}. \quad \text{(B.29)}$$

In words, the log-deviation in the number of new workers in each industry s $\left(\widehat{N}_s\right)$ can be written as the sum of two weighted sums: (i) one of all the log-deviations in the revenue shifters $\left(\widehat{A}\right)$ of all the firms in the economy (both domestic firms and MNCs) and (ii) another weighted sum of all the log-deviations of the entry wages in the domestic markets $(\widehat{\omega})$ of all the industries in the economy.

Next, we deal with the right-hand side (RHS) of equation (B.25). Recall that $\Omega_N = \left(\sum_{s'} \tilde{\omega}_{s'}^{\eta_N}\right)^{1/\eta_N}$, $\Omega_{js} \equiv \Omega_{js}(W_j, \tilde{\boldsymbol{\omega}}) = \left(W_j^{\eta_I} + \sum_{s'}(\tau_{s(j)s'}\tilde{\omega}_{s'})^{\eta_I}\right)^{1/\eta_I}$, and $\Omega_{j's'} \equiv \Omega_{j's'}(W_{j'}, \tilde{\boldsymbol{\omega}}) = \left(W_{j'}^{\eta_I} + \sum_{s''}(\tau_{s'(j')s''}\tilde{\omega}_{s''})^{\eta_I}\right)^{1/\eta_I}$. Define $\kappa_{s'} \equiv \frac{\overline{\tilde{\omega}}_{s'}^{\eta_N}}{\overline{\Omega}_N^{\eta_N}}$. Define $\pi_{js'} \equiv \frac{\left(\tau_{ss'}\overline{\tilde{\omega}}_{s'}\right)^{\eta_I}}{\overline{\Omega}_{js}^{\eta_I}}$ and $\pi_{jj} \equiv \frac{\overline{W}_j^{\eta_I}}{\overline{\Omega}_{js}^{\eta_I}}$.

$$\begin{aligned}
\widehat{\Omega}_N =& \sum_{s'} \frac{\overline{\tilde{\omega}}_{s'}^{\eta_N}}{\overline{\Omega}_N^{\eta_N}} \widehat{\omega}_{s'} = \sum_{s'} \kappa_{s'}\widehat{\tilde{\omega}}_{s'}, \\
\widehat{\Omega}_{j's'} =& \frac{\overline{W}_{j'}^{\eta_I}}{\overline{\Omega}_{j's'}^{\eta_I}}\widehat{W}_{j'} + \sum_{s''} \frac{\left(\tau_{s's''}\overline{\tilde{\omega}}_{s''}\right)^{\eta_I}}{\overline{\Omega}_{j's'}^{\eta_I}}\widehat{\tilde{\omega}}_{s''} = \pi_{j'j'}\widehat{W}_{j'} + \sum_{s''}\pi_{j's''}\widehat{\tilde{\omega}}_{s''}.
\end{aligned} \quad \text{(B.30)}$$

We replace the expressions for $\widehat{\Omega}_N$ and $\widehat{\Omega}_{j's'}$ from equation (B.30) and the expression of $\widehat{W}_{j'}$ from equation (B.14) into the RHS to reach:

$$\begin{aligned}
RHS =& \left(\Psi_s^N \eta_N\right)\widehat{\tilde{\omega}}_s - \Psi_s^N \eta_N \widehat{\Omega}_N + \left(\Psi_s^I \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \zeta_{j'}^s \eta_I\right)\widehat{\tilde{\omega}}_s - \Psi_s^I \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \widehat{\Omega}_{j's'} \\
=& \left[\Psi_s^N \eta_N + \left(\Psi_s^I \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \zeta_{j'}^s \eta_I\right)\right]\widehat{\tilde{\omega}}_s - \Psi_s^N \eta_N \sum_{s'} \kappa_{s'}\widehat{\tilde{\omega}}_{s'} - \Psi_s^I \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \zeta_{j'}^s \eta_I \sum_{s''} \pi_{j's''}\widehat{\tilde{\omega}}_{s''} \\
& - \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \left(\frac{\pi_{j'j'}\Psi_s^I \zeta_{j'}^s \xi_{j'}^C \eta_I \alpha\sigma}{\xi_{j'}^C\alpha\sigma + (1-\xi_{j'}^I) + \xi_{j'}^C\xi_{j'}^I\alpha\eta_I(1-\pi_{j'j'})}\right)\widehat{A}_{j'} - \\
& - \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \left(\frac{\pi_{j'j'}\Psi_s^I \zeta_{j'}^s \xi_{j'}^O \xi_{j'}^N \eta_I}{\xi_{j'}^C\alpha\sigma + (1-\xi_{j'}^I) + \xi_{j'}^C\xi_{j'}^I\alpha\eta_I(1-\pi_{j'j'})}\right)\widehat{\omega}_{s'} - \\
& - \sum_{s'}\sum_{j'\in\mathcal{D}_{s'}} \Psi_s^I \zeta_{j'}^s \eta_I \pi_{j'j'} \left(\frac{\xi_{j'}^C \xi_{j'}^I \alpha\eta_I}{\xi_{j'}^C\alpha\sigma + (1-\xi_{j'}^I) + \xi_{j'}^C\xi_{j'}^I\alpha\eta_I(1-\pi_{j'j'})} \sum_{s''} \pi_{j's''}\widehat{\tilde{\omega}}_{s''}\right)
\end{aligned}$$

$$
\begin{aligned}
RHS = &-\sum_{s'}\sum_{j'\in\mathcal{D}_{s'}}\left(\tfrac{\pi_{j'j'}\Psi_s^I\zeta_{j'}^s\xi_{j'}^C\eta_I\alpha\sigma}{\xi_{j'}^C\alpha\sigma+(1-\xi_{j'}^I)+\xi_{j'}^C\xi_{j'}^I\alpha\eta_I(1-\pi_{j'j'})}\right)\widehat{A}_{j'}- \\
&-\sum_{s'}\sum_{j'\in\mathcal{D}_{s'}}\left(\tfrac{\pi_{j'j'}\Psi_s^I\zeta_{j'}^s\xi_{j'}^O\xi_{j'}^N\eta_I}{\xi_{j'}^C\alpha\sigma+(1-\xi_{j'}^I)+\xi_{j'}^C\xi_{j'}^I\alpha\eta_I(1-\pi_{j'j'})}\right)\widehat{\omega}_{s'}- \\
&+\left[\Psi_s^N\eta_N+\left(\Psi_s^I\sum_{s'}\sum_{j'\in\mathcal{D}_{s'}}\zeta_{j'}^s\eta_I\right)\right]\widehat{\omega}_s-\Psi_s^N\eta_N\sum_{s'}\kappa_{s'}\widehat{\omega}_{s'}-\Psi_s^I\sum_{s'}\sum_{j'\in\mathcal{D}_{s'}}\zeta_{j'}^s\eta_I\sum_{s''}\pi_{j's''}\widehat{\omega}_{s''} \\
&+\left[\Psi_s^N\eta_N\left(\Psi_s^I\sum_{s'}\sum_{j'\in\mathcal{D}_{s'}}\zeta_{j'}^s\eta_I\right)\right]\tfrac{(\psi_s-1)(\overline{N}_{MNC(s)}/\overline{N}_s)}{1+(\psi_s-1)(\overline{N}_{MNC(s)}/\overline{N}_s)}\left(\widehat{N}_{MNC(s)}-\widehat{N}_s\right) \\
&-\Psi_s^N\eta_N\sum_{s'}\kappa_{s'}\tfrac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\left(\widehat{N}_{MNC(s')}-\widehat{N}_{s'}\right) \\
&-\Psi_s^I\sum_{s'}\sum_{j'\in\mathcal{D}_{s'}}\zeta_{j'}^s\eta_I\sum_{s''}\pi_{j's''}\tfrac{(\psi_{s''}-1)(\overline{N}_{MNC(s'')}/\overline{N}_{s''})}{1+(\psi_{s''}-1)(\overline{N}_{MNC(s'')}/\overline{N}_{s''})}\left(\widehat{N}_{MNC(s'')}-\widehat{N}_{s''}\right) \\
&-\sum_{s'}\sum_{j'\in\mathcal{D}_{s'}}\Psi_s^I\zeta_{j'}^s\eta_I\pi_{j'j'}\left(\tfrac{\xi_{j'}^C\xi_{j'}^I\alpha\eta_I}{\xi_{j'}^C\alpha\sigma+(1-\xi_{j'}^I)+\xi_{j'}^C\xi_{j'}^I\alpha\eta_I(1-\pi_{j'j'})}\sum_{s''}\pi_{j's''}\widehat{\omega}_{s''}\right) \\
&-\sum_{s'}\sum_{j'\in\mathcal{D}_{s'}}\Psi_s^I\zeta_{j'}^s\eta_I\pi_{j'j'}\left(\tfrac{\xi_{j'}^C\xi_{j'}^I\alpha\eta_I}{\xi_{j'}^C\alpha\sigma+(1-\xi_{j'}^I)+\xi_{j'}^C\xi_{j'}^I\alpha\eta_I(1-\pi_{j'j'})}\right)\sum_{s''}\pi_{j's''}\times \\
&\times\tfrac{(\psi_{s''}-1)(\overline{N}_{MNC(s'')}/\overline{N}_{s''})}{1+(\psi_{s''}-1)(\overline{N}_{MNC(s'')}/\overline{N}_{s''})}\left(\widehat{N}_{MNC(s'')}-\widehat{N}_{s''}\right) \qquad \text{(B.31)}
\end{aligned}
$$

where $\widehat{N}_{MNC(s)}=\frac{\sigma}{1+\xi_{MNC(s)}^C\alpha_m\sigma}\widehat{A}_{MNC(s)}-\frac{\sigma\left(1-\xi_{MNC(s)}^C\right)}{1+\xi_{MNC(s)}^C\alpha_m\sigma}\widehat{\omega}_s$, $\widehat{N}_{MNC(s')}=\frac{\sigma}{1+\xi_{MNC(s')}^C\alpha_m\sigma}\widehat{A}_{MNC(s')}-\frac{\sigma\left(1-\xi_{MNC(s')}^C\right)}{1+\xi_{MNC(s')}^C\alpha_m\sigma}\widehat{\omega}_{s'}$ and $\widehat{N}_{MNC(s'')}=\frac{\sigma}{1+\xi_{MNC(s'')}^C\alpha_m\sigma}\widehat{A}_{MNC(s'')}-\frac{\sigma\left(1-\xi_{MNC(s'')}^C\right)}{1+\xi_{MNC(s'')}^C\alpha_m\sigma}\widehat{\omega}_{s''}$.

Overall, the RHS contains three large weighted sums: (i) one weighted sum over the revenue shifters $\left(\widehat{A}\right)$ of all firms in the economy (both domestic firms and MNCs), (ii) another weighted sum over all the log-deviations of the entry wages in the domestic markets $(\widehat{\omega})$ of all the industries in the economy, and (iii) last, a weighted sum over the log-deviations of the number of new workers in all industries of all industries in the economy $\left(\widehat{N}\right)$.

Now, recall that $\widehat{N}_s=RHS$ for each industry s. We can replace all $\widehat{N}_s$, $\widehat{N}_{s'}$, and $\widehat{N}_{s''}$ with their respective formulas obtained from the system in equation (B.29) and arrive to a system (one equation per industry s) that relates the log-deviations in the revenue shifters $\left(\widehat{A}\right)$ of all the firms in the economy (both domestic firms and MNCs) to the log-deviations of the entry wages in the domestic markets $(\widehat{\omega})$ of all the industries in the economy. Ultimately, one can describe this relationship in a matrix form:

$$\widehat{\boldsymbol{\omega}}_{|s|\times 1} = \boldsymbol{\Lambda}_{|s|\times|k|}\widehat{\boldsymbol{A}}_{|k|\times 1} \tag{B.32}$$

In words, each log-deviation in the entry wage in the domestic market s $(\widehat{\omega}_s)$ can be written as a weighted sum of all the log-deviations in the revenue shifters $\left(\widehat{A}\right)$ of all the firms in the economy (both domestic firms and MNCs). Unfortunately, one cannot obtain an explicit expression for these weights, but they combine characteristics of firms (e.g., the equilibrium share of the total cost of a new hire that goes to the hiring and training cost, ξ_j^C), of industries (e.g., the equilibrium share of MNCs in the employment of the industry or their premium ψ_s), and of the relationships between industries (e.g., the equilibrium transition probabilities $\pi_{ss'}$).

One can therefore write

$$\widehat{\omega}_s = \sum_{s'}\sum_{k'\in s'}\lambda_{sk's'}\widehat{A}_{k'} \tag{B.33}$$

where $\lambda_{sk's'}$ is the element of matrix $\boldsymbol{\Lambda}_{|s|\times|k|}$ in row s and column $k's'$ (s' being the industry of firm k').

Determinants of the Wage Setting Equation in General Equilibrium

Let us go back to equation (B.14). We want to write the change in wages of incumbent workers at domestic firms as a function of the revenue shifters of all firms in the economy. Let us first focus on $\widehat{A}_j$. Recall that $A_j \equiv B_j^{\frac{1}{\sigma}}T_j^{\frac{\sigma-1}{\sigma}}$ is the revenue shifter of firm j, where T_j is the physical productivity of firm j and $B_j \equiv b_{DOM} + \sum_{s'\in\mathcal{B}_j} b_{j,MNC(s')} \equiv b_{DOM} + B_{j,MNC}$ ($\mathcal{B}_j$ is the set of MNC buyers of firm j and $b_{j,MNC(s')}$ is the demand shifter of the MNC in industry s'). Define $\theta_{DOMj} = \frac{\overline{q}_{DOM}}{\overline{Q}_j}$ and $\theta_{MNC(s')j} \equiv \frac{\overline{q}_{MNC(s')j}}{\overline{Q}_j}$.

$$\begin{aligned}
\widehat{B}_j =& \frac{\overline{b}_{DOM}}{\overline{B}_j}\widehat{b}_{DOM} + \frac{\overline{B}_{j,MNC}}{\overline{B}_j}\widehat{B}_{j,MNC} = \frac{\overline{b}_{DOM}}{\overline{B}_j}\widehat{b}_{DOM} + \frac{\overline{B}_{j,MNC}}{\overline{B}_j}\sum_{s'\in\mathcal{B}_j}\frac{\overline{b}_{j,MNC(s')}}{\overline{B}_{j,MNC}}\widehat{b}_{j,MNC(s')} \\
\widehat{B}_j =& \frac{\overline{b}_{DOM}}{\overline{B}_j}\widehat{b}_{DOM} + \sum_{s'\in\mathcal{B}_j}\frac{\overline{b}_{j,MNC(s')}}{\overline{B}_j}\widehat{b}_{j,MNC(s')} = \frac{\overline{b}_{DOM}\overline{p}_j^{1-\sigma}}{\overline{B}_j\overline{p}_j^{1-\sigma}}\widehat{b}_{DOM} + \sum_{s'\in\mathcal{B}_j}\frac{\overline{b}_{j,MNC(s')}\overline{p}_j^{1-\sigma}}{\overline{B}_j\overline{p}_j^{1-\sigma}}\widehat{b}_{j,MNC(s')} \\
\widehat{B}_j =& \frac{\overline{q}_{DOM}}{\overline{Q}_j}\widehat{b}_{DOM} + \sum_{s'\in\mathcal{B}_j}\frac{\overline{q}_{MNC(s')j}}{\overline{Q}_j}\widehat{b}_{j,MNC(s')} = \theta_{DOMj}\widehat{b}_{DOM} + \sum_{s'\in\mathcal{B}_j}\theta_{MNC(s')j}\widehat{b}_{j,MNC(s')}.
\end{aligned} \tag{B.34}$$

Then, in the MNC problem we defined $b_{j,MNC(s')} \equiv \left(\frac{\sigma-1}{\sigma}\right)^{\sigma} A^{\sigma}_{MNC(s')}\frac{Q_{MNC(s')}}{J_{MNC(s')}}a^{\sigma}_{j,MNC(s')}$. Similar as for domestic firms, $A_{MNC(s')} = B^{\frac{1}{\sigma}}_{MNC(s')}T^{\frac{\sigma-1}{\sigma}}_{MNC(s')}$, where $B_{MNC(s')}$ is the demand shifter of $MNC(s')$ (which is determined only by market forces in the rest of the world) and $T_{MNC(s')}$ is the physical productivity of $MNC(s')$. Assume that $a_{j,MNC(s')}$ stays constant (where $a_{j,MNC(s')}$ can be thought of as the variable that governs whether $MNC(s')$

buys or not from domestic firm j). Last, note that $\widehat{Q}_{MNC(s')} - \widehat{J}_{MNC(s')} \propto \widehat{A}_{MNC(s')}$ or $\left(\widehat{Q}_{MNC(s')} - \widehat{J}_{MNC(s')}\right) \equiv \varphi_{s'}\widehat{A}_{MNC(s')}$. Hence,

$$\widehat{b}_{j,MNC(s')} = \sigma\widehat{A}_{MNC(s')} + \sigma\left(\widehat{Q}_{MNC(s')} - \widehat{J}_{MNC(s')}\right) = \sigma(1+\varphi_{s'})\widehat{A}_{MNC(s')}.$$

We now replace the expression for $\widehat{b}_{j,MNC(s')}$ into the expression for $\widehat{A}_j$.

$$\begin{aligned}\widehat{A}_j =& \tfrac{\sigma-1}{\sigma}\widehat{T}_j + \tfrac{1}{\sigma}\widehat{B}_j = \tfrac{\sigma-1}{\sigma}\widehat{T}_j + \tfrac{1}{\sigma}\left(\theta_{DOMj}\widehat{b}_{DOM} + \sum_{s'\in\mathcal{B}_j}\theta_{MNC(s')j}\widehat{b}_{j,MNC(s')}\right)\\ =& \tfrac{\sigma-1}{\sigma}\widehat{T}_j + \tfrac{1}{\sigma}\theta_{DOMj}\widehat{b}_{DOM} + \tfrac{1}{\sigma}\sum_{s'\in\mathcal{B}_j}\theta_{MNC(s')j}\sigma(1+\varphi_{s'})\widehat{A}_{MNC(s')}\\ =& \tfrac{\sigma-1}{\sigma}\widehat{T}_j + \tfrac{1}{\sigma}\theta_{DOMj}\widehat{b}_{DOM} + \sum_{s'\in\mathcal{B}_j}(1+\varphi_{s'})\theta_{MNC(s')j}\widehat{A}_{MNC(s')}. \end{aligned} \tag{B.35}$$

equation (B.35) allows us to decompose the revenue shifter of firm j into three components: (i) one that is related to its shocks to physical productivity $\left(\widehat{T}_j\right)$, (ii) another related to shocks to the domestic demand shifter $\left(\widehat{b}_{DOM}\right)$, and (iii) last, one related to shocks to the revenue shifters of its MNC buyers $\left(\widehat{A}_{MNC(s')}\right)$.

Note that there is an isomorphism between modeling the effects of buyers as only working through the demand shifters of the firm (b_{DOM} or $b_{MNC(s')j}$) – which is the avenue we take – and modeling them as working through both the productivity and the demand shifters. For instance, one can imagine that shocks to either the productivity $\left(T_{MNC(s')}\right)$ or the demand $\left(B_{MNC(s')}\right)$ of $MNC(s')$ can affect the productivity of its supplier (T_j). That said, in our model, an incumbent worker in j is indifferent to the source of a given improvement in A_j.

Next, let us rewrite the ratio of the last term in equation (B.14)

$$\frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})} = \frac{\overline{\tilde{\omega}}_{s'}-\overline{\omega}_{s'}}{\overline{\tilde{\omega}}_{s'}},$$

where $\tilde{\omega}_{s'} \equiv \omega_{s'}\left[1+(\psi_{s'}-1)\frac{N_{MNC(s')}}{N_{s'}}\right]$ was the expected entry market wage in industry s'. Therefore, the ratio captures the importance of the MNC premium in increasing the expected entry market wage above the entry market wage in a world without MNCs. To the extent that MNCs do not pay a large enough premium or that the share of MNCs in the industry was small in the reference equilibrium, then MNCs do not have a large effect on the expected entry market wage.

We now replace the formula of $\widehat{A}_j$ into equation (B.13):

$$\begin{aligned}
\widehat{W}_j =& \frac{\xi_j^C \alpha(\sigma-1)}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\widehat{T}_j \\
&+\frac{\xi_j^C \alpha}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\theta_{DOMj}\widehat{b}_{DOM} \\
&+\frac{\xi_j^C \alpha\sigma}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\sum_{s'\in\mathcal{B}_j}(1+\varphi_{s'})\theta_{MNC(s')j}\widehat{A}_{MNC(s')} \\
&+\frac{\xi_j^C \xi_j^I \alpha\eta_I}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\left(\sum_{s''}\sum_{k\in s''}\lambda_{s'ks''}\widehat{A}_k\right) \\
&+\frac{\xi_j^C \xi_j^I \alpha\eta_I}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\left(\widehat{N}_{MNC(s')}-\widehat{N}_{s'}\right).
\end{aligned} \tag{B.36}$$

We can separate the term in the fourth line between the weighted sum of demand shifters for domestic firms $k = DOM$ and the ones for MNC firms $k = MNC$. The first two terms capture the spirit of our measure of labor market exposure. The third is the model equivalent of our measure of firm-level exposure. The remaining three terms are subsumed in the error term of our main empirical specification for the indirect effects described in equation (3.5).

$$\begin{aligned}
\widehat{W}_j =& \frac{\xi_j^C \xi_j^I \alpha\eta_I}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\left(\sum_{s''}\sum_{k=MNC\in s''}\lambda_{s'ks''}\widehat{A}_k\right)+ \\
&\frac{\xi_j^C \xi_j^I \alpha\eta_I}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\frac{(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}{1+(\psi_{s'}-1)(\overline{N}_{MNC(s')}/\overline{N}_{s'})}\left(\widehat{N}_{MNC(s')}-\widehat{N}_{s'}\right)+ \\
&\frac{\xi_j^C \alpha\sigma}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\sum_{s'\in\mathcal{B}_j}\theta_{MNC(s')j}(1+\varphi_{s'})\widehat{A}_{MNC(s')}+ \\
&\frac{\xi_j^C \alpha(\sigma-1)}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\widehat{T}_j+ \\
&\frac{\xi_j^C \xi_j^I \alpha\eta_I}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\sum_{s'}\pi_{js'}\left(\sum_{s''}\sum_{k=DOM\in s''}\lambda_{s'ks''}\widehat{A}_k\right)+ \\
&\frac{\xi_j^C \alpha}{\xi_j^C \alpha\sigma+(1-\xi_j^I)+\xi_j^C \xi_j^I \alpha\eta_I(1-\pi_{jj})}\theta_{DOMj}\widehat{b}_{DOM}.
\end{aligned} \tag{B.37}$$

This last equation is the same as equation (3.16) discussed in Section 3.5.4.

B.3 Data

B.3.1 Administrative Data

A. Matched employer-employee data. The matched employer-employee data is built on data from the Costa Rican Social Security Fund (*Caja Costarricense de Seguro Social* or CCSS). Given the nature of CCSS records, this data includes only individuals with social security contributions. This excludes the informal sector.

In CCSS, the total contributions to the social security of individuals are split by two types of insurance. The specific regulations of each type of insurance directly affect how an insured person is classified and, therefore, the criteria that must be used for the correct identification of workers. The raw data files were separated by the type of insurance.

The first type of insurance is for "Disability, Old Age and Death" (*Invalidez, Vejez y Muerte* or IVM). Two considerations motivated our choice of the sample of workers. First, not all workers contribute to the IVM (due to exceptions, such as those to workers in the judicial system). Second, non-working individuals can choose to contribute to IVM voluntarily. The second type of insurance is for "Security, Sickness and Maternity" (*Seguridad, Enfermedad y Maternidad* or SEM)," which is mandatory for all salaried workers, independent workers and the retirees of the national pension regimes. Given that the set of SEM contributors is a supraset of the IVM contributors, the analysis will be mainly carried out relying on the SEM records.

The SEM data has a monthly frequency, starts in January 2006, and ends in December 2017. Each line in the SEM data characterizes a given personal tax ID (PID), the type of insurance held, an indicator of whether the insurance is voluntary, the type of disability (if any), gender, age and date of birth, country of birth, monthly labor earnings, code of the occupation, type of work day, location codes for both the individual and the employer, corporate tax ID (CID) of the employer, type of firm, and economic activity of the firm.

The files that resulted from appending the raw monthly files contained a total of 13,804,333 entries in 2006 and ended with a total of 20,948,279 entries in 2017. The next step revolved around the cleaning of the unique tax IDs for both employees and employers. In particular, some tax IDs have changed over time (for instance, because the firm has changed its organizational structure). We have used correspondences to ensure that the same employee or employer can be tracked over time with a unique identifier. Because each employee can have more than one employer per month, each employee has as many monthly entries as employers that month. The minority of cases with duplicates in the employee-employer-month triad were due to either repetition of the entry with different vintages of a tax ID or due to typos that could be addressed manually.

Table B.19: Summary Statistics for the Matched Employer-Employee Data

Year	Number of Observations (1)	Number of Individuals (2)	Number of Firms (3)	Mean Log Wage (4)	SD Log Wage (5)	College Educated (6)	Public Sector (7)	MNC Employer (8)	Male (9)	Stayer (10)	Costa Rican National (11)
2006	9,995,988	1,081,025	98,572	12.740	0.786	0.181	0.272	0.180	0.656		0.918
2007	10,429,890	1,155,168	96,398	12.758	0.791	0.176	0.257	0.188	0.656	0.662	0.912
2008	10,157,020	1,084,760	78,441	12.753	0.786	0.160	0.239	0.198	0.662	0.652	0.898
2009	9,946,083	1,058,652	79,433	12.810	0.821	0.168	0.263	0.199	0.653	0.692	0.897
2010	10,265,800	1,079,953	82,353	12.843	0.805	0.167	0.263	0.205	0.651	0.689	0.895
2011	10,572,580	1,103,652	84,186	12.890	0.821	0.171	0.270	0.208	0.647	0.692	0.895
2012	10,994,210	1,139,384	84,637	12.915	0.822	0.172	0.265	0.207	0.648	0.705	0.894
2013	11,076,160	1,134,648	82,053	12.932	0.832	0.175	0.267	0.208	0.644	0.725	0.898
2014	11,100,330	1,133,506	81,011	12.958	0.831	0.176	0.269	0.209	0.643	0.731	0.899
2015	11,100,750	1,135,353	80,526	12.991	0.815	0.179	0.267	0.214	0.641	0.731	0.899
2016	11,351,410	1,163,327	79,630	13.038	0.831	0.179	0.266	0.221	0.637	0.717	0.901
2017	11,635,540	1,191,060	79,892	13.054	0.831	0.180	0.262	0.224	0.632	0.713	0.903

Notes: Table B.19 presents summary statistics of the matched employer-employee data with minimal restrictions, i.e., the raw data from which we dropped (i) non-working individuals (those voluntarily insured) and independent workers, and (ii) individuals under age 20 and over age 60. Observations are at the worker $\times$ employer $\times$ month level. If a worker has more than one employer in a given month, she appears as many times that month as the number of employers. Column (2) contains the number of unique personal tax IDs each year. Column (3) includes the number of unique corporate tax IDs each year. Labor earnings are in 2013 CPI-deflated Costa Rican Colones (in 2013, 1 U.S. dollar $\approx$ 500 Colones). Column (6) reports the share of college-educated workers in each year. Column (7) reports the share of workers employed in the public sector in each year. Column (8) reports the share of workers employed by an MNC in the entire formal economy in each year. Column (9) reports the share of male workers in each year. Column (10) reports the share of workers who in a given year have the same main employer as the one they had in the previous year. Column (11) contains the share of workers who are Costa Rican nationals in each year.

In the next stage, we produced descriptive statistics of the data to identify concerning patterns. The variable of most interest to our study is the labor earnings variable. Given the purpose of our research, we dropped non-working individuals (voluntarily insured) and independent workers (for whom there are no alternative means of cross-checking the labor earnings). In addition, we drop individuals under age 20 and over age 60. Table B.19 presents summary statistics for this sample.

On data quality and the Costa Rican labor market: Alfaro-Urena et al. (2019a) is a report on the evolution of labor earnings and inequality in Costa Rica. Because this report benchmarks the patterns in Costa Rica to those in other countries whose matched employer-employee datasets have been extensively used for research, Alfaro-Urena et al. (2019a) is informative on the quality of the data and on the extent to which the Costa Rican labor market is atypical.

B. Other administrative data. The remaining three administrative datasets (firm-to-firm transaction data, corporate income tax data, and foreign ownership data) have been introduced in detail in the Online Appendix F on "Data Construction and Statistics" of Alfaro-Urena et al. (2019b). Please refer to that project for more details than those already presented in Section 3.2.1.

B.3.2 Orbis and Compustat Data

To construct instruments for the expansion of MNC subsidiaries in Costa Rica, we have integrated data from two commercial databases: Compustat and Orbis.

A. Compustat. Compustat is a product of Standard&Poor's Global Market Intelligence. Compustat covers publicly-traded companies in the United States and other major markets. Compustat compiles the financial reports filed by public companies, which include variables such as income, expenses, assets, and liabilities.

We used the *Code Lookup* function of Compustat to search manually for the unique Compustat ID (called *gvkey*) of MNC groups with subsidiaries in Costa Rica. Our starting point was the list of 622 MNC subsidiaries in Costa Rica, for which we know the MNC group name and country of ultimate ownership. We undertook this search both in the "North America Fundamentals Annual" and "Global Fundamentals Annual" databases, to cover both U.S. owned and non-U.S. owned MNCs. The search was based on the name of the MNC group. Whenever the search yielded various results, we chose the *gvkey* of the firm whose industry SIC code and reporting period (used as a proxy for the period of activity) coincide with those of the correct MNC. For example, the search for "Intel" (*contains "Intel"*) yields 42 results. Of these, we keep the *gvkey* of "INTEL CORP" whose industry SIC code is 3674 (Semiconductors and Related Devices) and whose reporting period is 1971 to 2018 (which has the highest overlap among all options with Intel's existence since 1968).

This manual search led to finding 173 distinct MNC *gvkeys*. For these 173 *gvkeys*, we exported the following six variables: *ACT - Current Assets - Total, EMP – Employees, LOC*

– Current ISO Country Code – Headquarters, MKVALT – Market Value - Total - Fiscal, REVT – Revenue - Total, SALE – Sales/Turnover (Net). We chose the level of consolidation of the accounts as "CONSOL== C," which means that the values exported correspond to the combined reports of the parent and subsidiaries' accounts. The dataset has annual frequency and is an unbalanced panel between 2006 and 2017.

B. Orbis. Orbis is Bureau van Dijk's flagship company database. The financial and balance sheet information in Orbis comes from business registers collected by the local Chambers of Commerce to fulfill legal and administrative requirements (Kalemli-Ozcan, Sorensen, Villegas-Sanchez, Volosovych, and Yeşiltaş, 2015). The construction of our two proposed instrumental variable sets for the MNC presence in Costa Rica relies heavily on Orbis. We have carried out two major sets of queries, each associated with each IV set (the leading IV set and the IV set used in the robustness check).

The query for the data to construct IV Set 1 (the leading IV set): First, we have queried Orbis for information on all *bvdidnumbers* (unique identifiers of companies in Orbis) with a subsidiary in Costa Rica. These *bvdidnumbers* correspond to the *global ultimate owners* (abbreviated GUOs) of MNC subsidiaries in Costa Rica. Then, for each GUO *bvdidnumber*, we exported information on the NACE Rev 2 four-digit and ISIC three-digit industry codes of the GUO and "key financials and employees." Specifically, in addition to the industry codes, we exported the following variables: the number of employees, operating revenue (turnover), cash flow, total assets, P/L before tax, P/L for period [=Net income], shareholders funds, current ratio (x), profit margin (%), ROE using P/L before tax (%), ROCE using P/L before tax (%), and the solvency ratio (asset-based) (%).

We drop GUOs whose country was the same as the country of the subsidiary, and for which we only observe values for the number of employees and operating revenues in at most one year. The level of consolidation of the data is either "C1" or "C2."[1] For firms with more than one report per consolidation code × year, we keep the values reported at the latest date. For firms with both C1 and C2 reports in a given year, we take the average between the C1 and C2 values. Last, for firms in both this dataset and our Compustat dataset, we use the information on employment from Compustat to improve the quality of the information on employment from Orbis. Namely, in years when we only observe employment in one of the two datasets, we keep as the final value that unique value. In years when we observe employment both in Compustat and in Orbis, the final value is the average between the Compustat and the Orbis values.

We use this combined (Orbis and Compustat) dataset to construct our leading IV set. This dataset has an unbalanced panel structure between 2006 and 2017. Unfortunately, this dataset does not contain all of the 622 MNCs with subsidiaries in Costa Rica, but only 239 of

[1] C1 refers to the account of a company-headquarter of a group, aggregating all companies belonging to the group (affiliates, subsidiaries, etc.), where the company headquarter has no unconsolidated account. C2 refers to the account of a company-headquarter of a group, aggregating all companies belonging to the group (affiliates, subsidiaries, etc.) where the company headquarter also presents an unconsolidated account.

them. As large firms are overrepresented in both datasets, we, therefore, have MNC-specific information for the largest 239 of the 622 MNCs in Costa Rica.[2]

How do we proceed in the cases where we lack information to construct $\mathcal{O}_{st}$ and $\mathcal{O}_{mt}$ (see Section 3.4.3 for definitions)? Whenever we have information on the outside employment of a given MNC, we assign that information to the two-digit industry $\times$ region market s of its MNC subsidiary. To the markets with MNCs in Costa Rica but without data on the outside-of-Costa Rica employment of those MNCs, we assign a value based on the value for that same two-digit industry of that market aggregated at the level of Costa Rica, then apportioned to the region of the market according to the initial share of total employment in that market. In addition, whenever we lack MNC-specific information on its outside of Costa Rica employment, we replace $\mathcal{O}_{mt}$ by $\mathcal{O}_{st}$ (where s is the market that the subsidiary of m is part of in Costa Rica).

The query for the data to construct IV Set 2 (the IV set in the robustness check): Second, we have queried Orbis for information on all *bvdidnumbers* (unique identifiers of companies in Orbis) with a subsidiary in a list of twenty Latin American and Caribbean countries. This list of countries contains Argentina, Belize, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, Guatemala, Guyana, Honduras, Mexico, Nicaragua, Panama, Peru, Paraguay, El Salvador, Suriname, and Uruguay.

This list yielded a total of 4,595 unique *bvdidnumbers* of GUOs with a subsidiary in at least one of the twenty countries mentioned above. This list includes the list of MNCs identified in the previous step (i.e., the MNCs with a subsidiary in Costa Rica). For each of these *bvdidnumbers*, we exported the same variables as in the step above, that is, their industry codes, and "key financials and employees." Cleaning the raw data involved the same steps as those described in the paragraph above. The structure of the cleaned dataset is an unbalanced panel between 2006 and 2017. We use this dataset to construct the second IV set, which we use in our robustness checks to the leading IV set (described above). As Table B.20 shows, over half of these 4,595 MNCs are from one of the following five countries: the United States, Japan, Spain, Canada, and Italy.

B.3.3 Details on Variable Construction

Definition of the share of total (direct and indirect) sales to MNCs, $\theta^H_{jm,t}$. We want to measure the total (direct plus indirect) sales of each firm in the economy to MNCs in Costa Rica. That is, we consider not only direct sales to MNCs but also indirect sales made through one's clients at different supply-chain distances. This accounts for the fact that while few domestic firms are direct suppliers to MNCs, there are considerably more indirect suppliers. Accounting for indirect sales is in line with recent work on production networks, showing that the network structure of an economy can amplify shocks to specific nodes.

[2] The subsidiaries of the MNCs whose consolidated accounts we have found employ 58% of all the workers in MNCs subsidiaries in the country.

Table B.20: Countries of the Global Ultimate Owners for MNCs with Subsidiaries in Latin America and the Caribbean

Country	Number	%	Cumulative
US	1,023	22.3	22.3
JP	479	10.4	32.7
ES	382	8.3	41.0
CA	367	8.0	49.0
IT	269	5.9	54.8
GB	264	5.8	60.6
DE	214	4.7	65.2
FR	149	3.2	68.5
IN	110	2.4	70.9
AU	102	2.2	73.1
NL	98	2.1	75.2
SE	93	2.0	77.3
CN	78	1.7	79.0
TW	77	1.7	80.6
CH	62	1.4	82.0
BR	61	1.3	83.3
DK	59	1.3	84.6
KR	57	1.2	85.8
BE	52	1.1	87.0
BM	51	1.1	88.1
CL	38	0.8	88.9
FI	38	0.8	89.7
NO	36	0.8	90.5
Total	4,595	100	

Notes: Table B.20 summarizes the country of the global ultimate owner of MNCs with subsidiaries in at least one of twenty Latin American and Caribbean countries. For brevity, we do not report the countries that cover the remaining 10% of MNCs.

Let N_t be the total number of firms operating in Costa Rica in year t. Denote by N_t^M the number of MNC subsidiaries in the country that same year. While we omit subscript t from now on, note that N_t can differ across years. Define s_{jl} as the share of sales of firm j that go to firm l. Since no firm sells to itself, $s_{jj} = 0$. Now define the $N \times N$ matrix Σ as the matrix containing all the shares s_{jl} of sales between all firms in the economy:

$$\Sigma = \begin{pmatrix} 0 & s_{12} & \dots & s_{1N} \\ s_{21} & \ddots & & s_{2N} \\ \vdots & & \ddots & \vdots \\ s_{N1} & s_{N2} & \dots & 0 \end{pmatrix}.$$

Since the elements of matrix Σ are shares, note that $\sum_l s_{jl} = 1 \ \forall j$. Without loss of generality, we order firms such that the first N^M columns of matrix Σ correspond to the shares firms sell to the N^M MNCs in Costa Rica that year, indexed by m. The column m (containing the sales sold by each firm in the economy to MNC m) is denoted as $\Sigma_m = (s_{1m}, ..., s_{Nm})'$.

Define $\Sigma_m(h) \equiv \Sigma^h \cdot \Sigma_m$, where Σ^h is the h-power multiplication of matrix Σ (for instance, Σ^0 yields the $N \times N$ identity matrix). Intuitively, $\Sigma_m(0) = \Sigma_m$ contains the shares that firms sell directly to MNC m. $\Sigma_m(h)$ contains the shares sold indirectly to MNCs through clients who are at supply-chain distance $(h-1)$.[3]

We can now define our object of interest. We denote as $\Theta_m(H)$ the vector of total (direct and indirect) shares of sales to MNC m (up to supply chain distance H):

$$\Theta_m(H) = \Big(\sum_h^H S^h\Big) \cdot \Sigma_m.$$

The total share of sales of domestic firm j to MNC m (of degree H) can be found in the entry corresponding to firm j in vector $\Theta_m(H)$. Denote this share by θ_{jm}^H. Going forward, we omit the H superscript.

B.4 Additional Context on Costa Rica

B.4.1 Details on Labor Market Institutions in Costa Rica

Less than 1% of private-sector workers are members of a union in 2015. When including the public sector as well, the trade union density is at 7%, which is slightly lower than that in the United States. Collective bargaining agreements over wages and working conditions are also limited (OECD, 2017c).

According to OECD (2017c), employment protection legislation for workers with regular contracts in Costa Rica is one of the least stringent in the OECD and Latin American

[3] In this case, the MNC m is at supply chain distance h. To fix ideas, a firm's *direct* clients are at supply chain distance 0, the clients of one's direct clients are at supply chain distance 1, and so forth. For example, $\Sigma_m(1)$ contains the indirect sales to MNC m, made through one's direct clients (at supply chain distance 0).

countries. First, regulations on advance notification and severance pay are milder than in most OECD countries. Second, employers can dismiss an employee without cause, provided prior notice is offered. Third, Costa Rica does not operate any special regulations against collective dismissals other than those applying to individual dismissals. Given these lax regulations, employers do not prefer temporary contracts over open-ended contracts. This explains why temporary employment is less common in Costa Rica than in other OECD countries.

There are two main policies that address the personal cost of unemployment. First, employers have to pay severance to dismissed workers that is equal to one month for each year of service (with a maximum of eight months of pay). Second, Labor Capitalization Funds are accounts funded through employer contributions and meant to support the employees in case they leave the labor force. In practice, employees can and tend to withdraw the funds after five years of contributions, limiting the support that remains available to them in the event of unemployment. Overall, the unemployment insurance scheme of Costa Rica is seen as not adequately covering the costs of unemployment. Moreover, job-placement and labor market intermediation services are under-developed.

Costa Rica uses a multi-tiered system of legal wage floors, with 24 minima that differ by occupation and skill level. Minimum wages are revised twice a year, mainly based on expected inflation and growth in GDP per capita. The minimum wage for unskilled workers amounts to 70% of median wages. This fraction is relatively high relative to that in OECD countries. That said, these minimum wages are perceived as a reference by the private sector, and non-compliance is high (particularly in industries such as agriculture, construction, or domestic service).

Appendix C

Appendix for “Industrial Policy at Work"

C.1 Additional Evidence

C.1.1 Firm-Level Evidence

2001 Income Tax Break: Amadeus Data

	Revenue *C1*	Workers *C1*	Assets *C1*	Solvency *C1*	Revenue *C2*	Workers *C2*	Assets *C2*	Solvency *C2*
β_{DiD}	0.304***	0.040	0.202***	0.105***	0.255***	0.062*	0.145***	0.073***
	(0.069)	(0.043)	(0.064)	(0.019)	(0.056)	(0.035)	(0.053)	(0.016)
R^2	0.804	0.856	0.800	0.555	0.777	0.845	0.769	0.555
# Observations	6,947	6,977	6,866	6,659	9,480	9,503	9,350	9,090
	Revenue *C3*	Workers *C3*	Assets *C3*	Solvency *C3*	Revenue *C4*	Workers *C4*	Assets *C4*	Solvency *C4*
β_{DiD}	0.569***	0.319***	0.563***	0.180***	0.131***	0.065**	0.063	0.050***
	(0.091)	(0.057)	(0.094)	(0.030)	(0.046)	(0.029)	(0.043)	(0.013)
R^2	0.800	0.890	0.811	0.550	0.759	0.846	0.763	0.519
# Observations	5,365	5,394	5,326	5,169	36,457	36,649	35,778	34,486
	Revenue *C5*	Workers *C5*	Assets *C5*	Solvency *C5*	Revenue *C6*	Workers *C6*	Assets *C6*	Solvency *C6*
β_{DiD}	0.145***	0.073**	0.080*	0.054***	0.149***	0.062**	0.075*	0.053***
	(0.046)	(0.029)	(0.043)	(0.013)	(0.045)	(0.029)	(0.042)	(0.013)
R^2	0.766	0.857	0.771	0.521	0.761	0.844	0.762	0.524
# Observations	37,493	37,691	36,815	35,482	41,619	41,829	40,850	39,415

Table C.1: Robustness of the Baseline Results to the Choice of the Comparison Group

Notes: In this exercise we study the firm-level impact of the introduction of the 2001 law granting an income tax break to workers in IT. The data source is Amadeus, a commercial database from Bureau Van Dijk. The outcome variables used are log(operating revenue), log(number of workers), log(total assets), and the solvency ratio. Treated firms are those in the NACE Rev 1 sector 722. The difference between the six sets of results comes from the set of firms allowed in the comparison group. Comparison firm i joins a sample based on its NACE Rev 1 sector. All specifications include firm fixed effects. Robust standard errors, clustered at the firm-level, in parenthesis. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

2013 Reform to the Income Tax Break

Dependent variable	1 if more than 20% exempted employees			Share of exempted employees		
Sectors of Firms	ICT services (1)	HKTI services (2)	Eligible (3)	ICT services (4)	HKTI services (5)	Eligible (6)
Eligible sector × d_{2011}	0.005	0.005		0.004	0.004	
	(0.007)	(0.007)		(0.004)	(0.004)	
Eligible sector × d_{2013}	0.020***	0.020***		0.011***	0.011***	
	(0.006)	(0.006)		(0.004)	(0.004)	
Eligible sector × d_{2014}	0.035***	0.035***		0.023***	0.023***	
	(0.006)	(0.006)		(0.004)	(0.004)	
Eligible sector × d_{2015}	0.055***	0.056***		0.026***	0.026***	
	(0.007)	(0.007)		(0.004)	(0.004)	
d_{2011}			0.004			0.004
			(0.007)			(0.004)
d_{2013}			0.020***			0.011***
			(0.006)			(0.004)
d_{2014}			0.034***			0.023***
			(0.006)			(0.004)
d_{2015}			0.055***			0.026***
			(0.007)			(0.004)
Adjusted R^2	0.727	0.729	0.682	0.772	0.774	0.740
# Observations	26,507	27,407	16,207	26,507	27,407	16,207
# Firms	5,721	5,932	3,499	5,721	5,932	3,499

Table C.2: 2013 Reform: First Stage Effects on the Share of Income Tax Exempted Employees

Notes: In this exercise, we focus on the 2013 tax reform and study its first stage effects on the firm-level share of workforce exemption from the income tax. Columns (1)-(3) use as the dependent variable a dummy variable that takes value 1 for firm i in year t if firm i has more than 20% of its workers exempted from the income tax in year t. Columns (4)-(6) use as the dependent variable the share of workers of firm i who are exempted from the income tax in year t. The samples used in this table are different from the baseline sample in Table 4.2 and the samples in Tables C.5 and C.6 in that the former keep *all* firms in ICT service sectors, in HTKI (high-tech knowledge-intensive) service sectors, and in eligible sectors, whereas the latter bring additional restrictions on the firm-level share of workforce exemption before and after 2013. The dependent variables and samples used in this table are the same as those used in Table C.20. In addition to interaction terms between a dummy taking value 1 if the sector of firm i is eligible for the income tax break of its eligible workers (Eligible sector) and a year dummy (d_t), columns (1), (2), (4), (5) include firm fixed effects and year fixed effects (d_t, whose estimates are omitted from the table). In addition to the year fixed effects (d_t), columns (3) and (6) also include firm fixed effects. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sales per Worker	Sales per Worker
$\beta_{DiD,2011}$	-0.056	0.000
	(0.068)	(.)
$\beta_{DiD,2013}$	0.001	-0.026
	(0.053)	(0.058)
$\beta_{DiD,2014}$	0.142***	0.090
	(0.052)	(0.055)
$\beta_{DiD,2015}$	0.161***	0.119*
	(0.062)	(0.063)
Controls	No	Yes
Rel. prod.		1.199***
		(0.228)
Small		-0.012
		(0.043)
Medium		-0.070
		(0.089)
Large		-0.070
		(0.133)
Young		-0.024
		(0.026)
Adjusted R^2	0.732	0.781
# Observations	22,356	16,652
# Firms	5,142	4,830

Table C.3: Difference-in-Differences Design Around 2013 Reform: Sales per Worker

Notes: In this exercise, we focus on the 2013 tax reform and study the evolution of labor productivity (defined as sales per worker) for the baseline sample defined in Section 4.3.2. We report point estimates of the DiD coefficient from Equation (4.2). The only difference between Column (1) and Column (2) is that the latter includes firm-specific time-variant controls in addition to firm fixed effects, whereas the former includes firm fixed effects alone. Heteroskedasticity robust errors in parentheses. All specifications include firm and sector-by-year fixed effects. The reference category for the firm size is "Micro" and for "Young" firms the reference category are firms "Older than five years." ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Amadeus Data				**Administrative Data**			
	Revenue (1)	Sales (2)	Workers (3)	Assets (4)	Revenue (5)	Sales (6)	Workers (7)	Assets (8)
$\beta_{DiD,2008}$	-0.070	-0.077	0.027	-0.020				
	(0.087)	(0.086)	(0.059)	(0.086)				
$\beta_{DiD,2009}$	0.029	0.005	0.048	0.005				
	(0.072)	(0.071)	(0.043)	(0.074)				
$\beta_{DiD,2010}$	0.026	0.018	0.037	0.063				
	(0.072)	(0.072)	(0.040)	(0.065)				
$\beta_{DiD,2011}$	0.012	0.006	0.055	-0.009	0.018	0.015	0.068	-0.015
	(0.076)	(0.076)	(0.040)	(0.072)	(0.077)	(0.081)	(0.042)	(0.074)
$\beta_{DiD,2013}$	0.107*	0.101	0.122***	0.113*	0.078	0.107*	0.143***	0.062
	(0.062)	(0.062)	(0.038)	(0.058)	(0.055)	(0.061)	(0.035)	(0.049)
$\beta_{DiD,2014}$	0.264***	0.258***	0.165***	0.192***	0.240***	0.257***	0.181***	0.142***
	(0.063)	(0.063)	(0.039)	(0.062)	(0.056)	(0.061)	(0.035)	(0.052)
$\beta_{DiD,2015}$	0.324***	0.328***	0.210***	0.306***	0.302***	0.315***	0.209***	0.233***
	(0.077)	(0.076)	(0.046)	(0.069)	(0.070)	(0.074)	(0.043)	(0.062)
Adjusted R^2	0.828	0.831	0.878	0.866	0.870	0.860	0.912	0.900
# Observations	25,693	25,684	25,775	25,452	17,471	17,302	17,471	17,471
# Firms	3,889	3,889	3,889	3,874	3,888	3,868	3,888	3,888

Table C.4: Robustness of the Baseline Results to Adding More Pre-Reform Years Using Amadeus Data

Notes: In this exercise we check the robustness of the baseline results from Table 4.2 on the firm-level impact of the 2013 amendment to the income tax break law of 2001. The outcome variables used are log(revenue), log(sales), log(number of workers), and log(total assets). The only difference between columns (1)-(4), columns (5)-(8), and columns (1)-(4) in Table 4.2 stems from the source of the data over which we estimate the model in Equation (4.2). Columns (1)-(4) use data from Amadeus, a Bureau van Dijk product. Columns (5)-(8) use the administrative data described in Section 4.3.2. The only difference between the sample used in Columns (5)-(8) and the baseline sample (used in columns (1)-(4) from Table 4.2) is that the former is the subset of the latter that we were able to match with Amadeus. The similarity between the estimates in columns (5)-(8) and those in columns (1)-(4) from Table 4.2 suggest that the sample matched with Amadeus is representative for the baseline sample. The similarity between the estimates in columns (1)-(4) and those in columns (5)-(8) suggest that the Amadeus data is of comparable quality to the administrative data. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Revenue (1)	Sales (2)	Workers (3)	Assets (4)	Revenue (5)	Sales (6)	Workers (7)	Assets (8)
$\beta_{DiD,2011}$	0.008	0.001	0.052	-0.024	0.000	0.000	0.000	0.000
	(0.073)	(0.076)	(0.040)	(0.069)	(.)	(.)	(.)	(.)
$\beta_{DiD,2013}$	0.123**	0.156***	0.141***	0.071	0.024	0.066	0.083**	0.017
	(0.051)	(0.055)	(0.033)	(0.046)	(0.052)	(0.057)	(0.037)	(0.050)
$\beta_{DiD,2014}$	0.300***	0.304***	0.176***	0.191***	0.165***	0.185***	0.106***	0.094*
	(0.052)	(0.056)	(0.033)	(0.049)	(0.050)	(0.055)	(0.036)	(0.051)
$\beta_{DiD,2015}$	0.365***	0.379***	0.207***	0.258***	0.225***	0.236***	0.125***	0.136**
	(0.065)	(0.066)	(0.040)	(0.058)	(0.058)	(0.062)	(0.041)	(0.057)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Rel. prod.					1.393***	1.064***	0.204**	1.688***
					(0.244)	(0.210)	(0.086)	(0.133)
Small					0.331***	0.331***	0.393***	0.219***
					(0.045)	(0.044)	(0.036)	(0.036)
Medium					0.630***	0.635***	0.703***	0.512***
					(0.105)	(0.098)	(0.104)	(0.093)
Large					0.870***	0.856***	0.960***	0.753***
					(0.241)	(0.237)	(0.279)	(0.251)
Young					-0.011	-0.024	-0.011	-0.022
					(0.024)	(0.024)	(0.014)	(0.020)
Adjusted R^2	0.869	0.872	0.918	0.911	0.902	0.904	0.932	0.937
# Observations	23,517	23,164	23,526	23,516	17,528	17,263	17,535	17,525
# Firms	5,388	5,339	5,388	5,388	5,082	5,024	5,084	5,082

Table C.5: Robustness of the Baseline Results to the Comparison Group Choice. Here Comparison Group in High-Tech, Knowledge-Intensive Sectors

Notes: In this exercise we check the robustness of the baseline results from Table 4.2 on the firm-level impact of the 2013 amendment to the income tax break law of 2001. The outcome variables used are log(revenue), log(sales), log(number of workers), and log(total assets). The only difference between this Table and Table 4.2 stems from the choice of the comparison group. In Table 4.2 we allowed firms whose sector was classified as part of ICT services, according to the OECD definition. All other specification features are the same as those in Equation (4.2). The only difference between columns (1)-(4) and columns (5)-(8) is that the latter include firm-specific time-variant controls in addition to firm fixed effects, whereas the former include firm fixed effects alone. Heteroskedasticity robust errors in parentheses. All specifications include firm and sector-by-year fixed effects. The reference category for the firm size is "Micro" and for "Young" firms the reference category are firms "Older than five years." ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Revenue (1)	Sales (2)	Workers (3)	Assets (4)	Revenue (5)	Sales (6)	Workers (7)	Assets (8)
$\beta_{DiD,2011}$	-0.001	-0.009	0.048	-0.041	0.000	0.000	0.000	0.000
	(0.075)	(0.078)	(0.040)	(0.071)	(.)	(.)	(.)	(.)
$\beta_{DiD,2013}$	0.123**	0.155***	0.144***	0.069	0.019	0.060	0.085**	0.012
	(0.052)	(0.056)	(0.033)	(0.047)	(0.052)	(0.058)	(0.038)	(0.051)
$\beta_{DiD,2014}$	0.302***	0.304***	0.179***	0.192***	0.162***	0.180***	0.108***	0.092*
	(0.053)	(0.057)	(0.034)	(0.050)	(0.051)	(0.056)	(0.036)	(0.052)
$\beta_{DiD,2015}$	0.364***	0.376***	0.211***	0.256***	0.217***	0.228***	0.126***	0.131**
	(0.066)	(0.068)	(0.040)	(0.060)	(0.059)	(0.063)	(0.041)	(0.059)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Rel. prod.					1.618***	1.032***	0.253**	1.787***
					(0.342)	(0.281)	(0.103)	(0.179)
Small					0.303***	0.319***	0.364***	0.168***
					(0.059)	(0.059)	(0.048)	(0.054)
Medium					0.690***	0.654***	0.729***	0.559***
					(0.150)	(0.123)	(0.100)	(0.179)
Large					0.000	0.000	0.000	0.000
					(.)	(.)	(.)	(.)
Young					-0.065*	-0.057*	-0.020	-0.040
					(0.034)	(0.033)	(0.018)	(0.029)
Adjusted R^2	0.840	0.847	0.904	0.879	0.883	0.891	0.925	0.916
# Observations	11,251	11,093	11,255	11,248	8,343	8,222	8,346	8,340
# Firms	2,600	2,579	2,600	2,600	2,429	2,404	2,430	2,429

Table C.6: Robustness of the Baseline Results to the Comparison Group Choice. Here Comparison Group Only in Eligible Sectors

Notes: In this exercise we check the robustness of the baseline results from Table 4.2 on the firm-level impact of the 2013 amendment to the income tax break law of 2001. The outcome variables used are log(revenue), log(sales), log(number of workers), and log(total assets). The only difference between this Table and Table 4.2 stems from the choice of the comparison group. In Table 4.2 we allowed firms whose sector was classified as part of ICT services, according to the OECD definition. Thus the comparison is between firms under the 5% threshold of exempted employees with firms that jump over the 20% threshold after 2013 (all in eligible sectors). All other specification features are the same as those in Equation (4.2). The only difference between columns (1)-(4) and columns (5)-(8) is that the latter include firm-specific time-variant controls in addition to firm fixed effects, whereas the former include firm fixed effects alone. Heteroskedasticity robust errors in parentheses. All specifications include firm and sector-by-year fixed effects. The reference category for the firm size is "Micro" and for "Young" firms the reference category are firms "Older than five years." ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Event-Study Design. The 2013 reform to the income tax break law substantially expanded the scope of the income tax exemption. However, the treated firms in the main DiD exercise increased the share of exempted employees at different times after the amendment in 2013. We apply a more general version of the DiD design, i.e., an event-study design that allows us to exploit the differential timing of the first large-scale exemption across firms. Specifically,

$$y_{ist} = \alpha_i + \lambda_{st} + \sum_{k=-4}^{+2} \theta_k D_{ist}^k + \varepsilon_{ist}, \tag{C.1}$$

where D_{ist}^k is an event year dummy variable. We map calendar years t to their event year analog. τ_i is the event year of firm i, i.e., the year when i jumped from less than 5 to more than 20% of its workers exempted from the income tax. Then $D_{ist}^k := \mathbb{1}[t = \tau_i + k]\ \forall k$, where $\mathbb{1}[.]$ is an indicator function for the expression in the brackets being true.[1] All other variables are defined as in Equation (4.2).

A key advantage of this design is that it allows us to lengthen the period over which we observe the firms-to-be-treated. While the main DiD design can only exploit 2011 as a pre-period year, in the event-study design firms that become treated in 2015 are observed for three years before their event.[2]

The sequence of θ_k is our object of interest. The interpretation of the θ_k depends on the sample of estimation.[3] We use two samples that enable complementary findings. In the "baseline sample" we allow firms that are in ICT service sectors but do not experience an event. This sample has the benefit of a significantly larger size. This is also the same sample as the one used for our baseline results in Table 4.2. The "restricted sample" only uses firms that eventually experience the event. With the second sample, θ_k for $k \geq 0$ captures the average increase in the outcome of interest in event year k across eventually treated firms, compared to the average level of that outcome for the same firms in the year before their event. The baseline sample adds to the reference outcomes those of firms that have less than 5% of workers exempted from the income tax, while still in ICT service sectors.[4]

One concern relative to this design is related to the extent to which a delay in the onset of a large-scale exemption can be plausibly exogenous to the firm. We claim that the most plausible scenarios do not pose threats to the identification of the causal effects of the event. It is very likely for the 2013 amendment to not have received as much media coverage as the initial introduction of the policy. Hence, firms are likely to have learned about the 2013 amendment at different points in time, which may explain the differential timing of the onset

[1] As we use the same baseline sample as for the DiD design, by construction $\tau_i \geq 2013\ \forall i$.

[2] Given the length of our panel in calendar years and the fact that events are constrained to occur between 2013 to 2015, we observe a maximum of two years after the event (for firms whose events occur in 2013) and a maximum of four years prior to the event (for firms whose events occur in 2015).

[3] The sample on which we estimate Equation (C.1) implicitly assumes a counterfactual path for the eventually-exempted firms.

[4] As not all parameters are identified as written, we normalize θ_{-1} to zero to simplify the test for an effect on impact.

of treatment. As long as the reason for the delay of the event is unrelated to future trends of expansion after the event, our findings have a causal interpretation.

Table C.7 reports the event-study estimates from the model in Equation (C.1). Columns (1)-(4) report the results for the full baseline sample, whereas columns (5)-(8) report the results for the restricted sample. We first notice that overall, prior to their event, firms are not embarked on a trend of growth. In columns (1)-(4), in event year *-3* we find evidence of some statistically-significant differences between eventually exempted firms and firms never exempted. For all other years however, we fail to find distinctive growth trends. Moreover, when we restrict in columns (5)-(8) to the restricted sample, we find that firms were not embarked on growth trends prior to their event.

Second, all models show that firms experiencing an event expand along the four measures of size: revenue, sales, employment, and total assets. This expansion already occurs in the first year of treatment and intensifies in the next two years. Reassuringly, the magnitudes from the event-study design are comparable to those from the historical DiD design applied on Amadeus data and from the DiD design applied to the same administrative data from 2011 to 2015.

	Baseline Sample				Restricted Sample			
	Revenue (1)	Sales (2)	Workers (3)	Assets (4)	Revenue (5)	Sales (6)	Workers (7)	Assets (8)
θ_{-4}	0.025	-0.007	0.021	0.011				
	(0.121)	(0.112)	(0.067)	(0.118)				
θ_{-3}	-0.198**	-0.175**	-0.068	-0.081	-0.205**	-0.159	-0.066	-0.084
	(0.079)	(0.085)	(0.044)	(0.073)	(0.100)	(0.100)	(0.056)	(0.099)
θ_{-2}	-0.087	-0.043	-0.015	-0.050	-0.082	-0.027	-0.018	-0.045
	(0.055)	(0.058)	(0.033)	(0.051)	(0.062)	(0.063)	(0.036)	(0.059)
θ_{0}	0.109**	0.152***	0.100***	0.099**	0.113	0.151**	0.086*	0.111
	(0.051)	(0.053)	(0.031)	(0.043)	(0.074)	(0.077)	(0.045)	(0.071)
θ_{+1}	0.286***	0.308***	0.161***	0.255***	0.287**	0.314**	0.171**	0.278**
	(0.057)	(0.060)	(0.036)	(0.052)	(0.126)	(0.126)	(0.072)	(0.118)
θ_{+2}	0.422***	0.458***	0.208***	0.312***	0.470**	0.510***	0.248**	0.367**
	(0.085)	(0.086)	(0.056)	(0.088)	(0.200)	(0.197)	(0.114)	(0.186)
Adjusted R^2	0.869	0.874	0.907	0.901	0.868	0.871	0.908	0.872
# Observations	22,592	22,212	22,598	22,587	1,393	1,383	1,393	1,393
# Firms	5,177	5,128	5,177	5,177	317	316	317	317

Table C.7: Robustness of the Baseline Results to an Event-Study Design

Notes: We use an event-study design to study the impact of the 2013 amendment to the income tax break law of 2001. A firm experiences an "event" when it jumps after 2013 from under 5 to over 20% of workers exempted from the income tax. This table reports the point estimates of the event-study coefficients from Equation (C.1). The outcome variables used are log(revenue), log(sales), log(number of workers), and log(total assets). We propose two samples: the baseline sample includes firms in ICT services not experiencing the event, in addition to those experiencing the event (columns (1)-(4)), while the restricted sample is constrained to only include the firms experiencing the event (columns (5)-(8)). The reference year for each firm is the one prior to the year of its event. All specifications include firm and sector-by-year fixed effects. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Last, reporting alongside the point estimates of θ_k from the two samples allows us to check whether results are driven by the contrast with the "never-exempted" (i.e., firms with under 5% of their workforce exempted in all years, or firms in non-eligible sectors) or by the staggered timing of the event across treated firms. As point estimates do not seem to vary much with the sample, we conclude that our results are driven by the differential firm-level timing of this large-scale exemption.[5]

[5] Only the standard errors are affected by the choice of the sample, as the restricted sample contains only 6% of the firms in the baseline sample.

	Revenue (1)	Sales (2)	Workers (3)	Assets (4)	Revenue (5)	Sales (6)	Workers (7)	Assets (8)
$\beta_{DiD,2011}$	-0.004	-0.012	0.043	-0.026	0.000	0.000	0.000	0.000
	(0.070)	(0.073)	(0.039)	(0.066)	(.)	(.)	(.)	(.)
$\beta_{DiD,2013}$	0.115**	0.131**	0.136***	0.046	0.035	0.055	0.081**	-0.002
	(0.048)	(0.053)	(0.032)	(0.044)	(0.048)	(0.054)	(0.036)	(0.047)
$\beta_{DiD,2014}$	0.281***	0.302***	0.184***	0.161***	0.161***	0.186***	0.115***	0.063
	(0.050)	(0.054)	(0.032)	(0.046)	(0.047)	(0.053)	(0.035)	(0.048)
$\beta_{DiD,2015}$	0.336***	0.359***	0.218***	0.212***	0.209***	0.240***	0.139***	0.091*
	(0.061)	(0.065)	(0.038)	(0.056)	(0.054)	(0.060)	(0.039)	(0.054)
Controls	No	No	No	No	Yes	Yes	Yes	Yes
Rel. prod.					1.595***	1.414***	0.218***	1.792***
					(0.222)	(0.229)	(0.085)	(0.133)
Small					0.358***	0.357***	0.373***	0.232***
					(0.046)	(0.046)	(0.037)	(0.037)
Medium					0.639***	0.672***	0.752***	0.633***
					(0.114)	(0.103)	(0.112)	(0.114)
Large					0.925***	0.937***	1.042**	0.898**
					(0.344)	(0.341)	(0.407)	(0.363)
Young					-0.035	-0.022	-0.001	-0.044**
					(0.022)	(0.026)	(0.014)	(0.021)
Adjusted R^2	0.868	0.859	0.908	0.901	0.907	0.898	0.924	0.931
# Observations	22,678	22,434	22,678	22,677	16,888	16,715	16,888	16,887
# Firms	5,194	5,159	5,194	5,194	4,881	4,846	4,881	4,881

Table C.8: Robustness of the Baseline Results to the Threshold Choice

Notes: In this exercise we check the robustness of the baseline results from Table 4.2 on the firm-level impact of the 2013 amendment to the income tax break law of 2001. The outcome variables used are log(revenue), log(sales), log(number of workers), and log(total assets). The only difference between this Table and Table 4.2 stems from the choice of the threshold above which we consider a firm to be treated by the 2013 amendment, i.e., instead of allowing $Exempted_{isk}$ to become 1 only when the firm passes above the 20% threshold of exempted employees, we lower this threshold to 15%. All other specification features are the same as those in Equation (4.2). Similar results are obtained when we lower this threshold further to 10%. The only difference between columns (1)-(4) and columns (5)-(8) is that the latter include firm-specific time-variant controls in addition to firm fixed effects, whereas the former include firm fixed effects alone. Heteroskedasticity robust errors in parentheses. All specifications include firm and sector-by-year fixed effects. The reference category for the firm size is "Micro" and for "Young" firms the reference category are firms "Older than five years." ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

	Full Sample				**Baseline Sample**			
	Revenue (1)	Sales (2)	Workers (3)	Assets (4)	Revenue (5)	Sales (6)	Workers (7)	Assets (8)
$\beta_{DiD,2011}$	-0.004	-0.007	-0.006	-0.021	0.016	-0.008	0.012	-0.011
	(0.026)	(0.028)	(0.015)	(0.025)	(0.029)	(0.029)	(0.015)	(0.027)
$\beta_{DiD,2013}$	0.038*	0.038*	0.023*	-0.000	0.007	0.014	0.012	-0.021
	(0.020)	(0.022)	(0.012)	(0.018)	(0.023)	(0.023)	(0.013)	(0.020)
$\beta_{DiD,2014}$	0.084***	0.091***	0.073***	0.027	0.050**	0.054**	0.050***	0.000
	(0.022)	(0.024)	(0.013)	(0.020)	(0.024)	(0.024)	(0.014)	(0.022)
$\beta_{DiD,2015}$	0.094***	0.109***	0.100***	0.067***	0.028	0.065**	0.064***	0.020
	(0.027)	(0.029)	(0.016)	(0.025)	(0.029)	(0.029)	(0.016)	(0.027)
Adjusted R^2	0.890	0.883	0.926	0.911	0.868	0.873	0.907	0.901
# Observations	26,507	26,281	26,507	26,506	22,592	22,212	22,598	22,587
# Firms	5,721	5,696	5,721	5,721	5,177	5,128	5,177	5,177

Table C.9: Robustness of the Baseline Results to the Definition of Treatment. Here Treatment Based on Firm Sector, Full and Baseline Sample

Notes: In this exercise we study the effects of the 2013 expansion to the income tax break law of 2001 using a DiD design similar to the DiD design used to study the initial introduction of the law in 2001. Specifically, treatment is assigned to firms based on whether their sector was targeted by the income tax break law or not, interacted with a dummy activated when $t \geq 2013$ (as in Equation (4.1), for year 2001), as opposed to their workforce exemption status. Note that this design is oblivious to a firm's workforce exemption status both before and after the 2013 reform. The outcome variables used are log(revenue), log(sales), log(number of workers), and log(total assets). Columns (5)-(8) use the exact same baseline sample used in Table 4.2. Columns (1)-(4) use an expanded version of the baseline sample. Because the definition of treatment used here does not impose any restrictions on the firm-level share of exempted workers, neither before nor after 2013, the full sample used in columns (1)-(4) contains *all* firms in the eligible sectors (NACE codes 5821, 5829, 6201, 6202, or 6209) and *all* firms in the control group (i.e. all firms in other ICT service sectors, based on the OECD definition). This explains why the sample sizes in columns (1)-(4) are larger than the sample sizes in columns (5)-(8). Heteroskedasticity robust errors in parentheses. All specifications include firm, year, and sector fixed effects. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

C.1.2 Sector-Level Cross-Country Effects of 2001 Tax Break

IT Sector Vs. Rest of the Economy

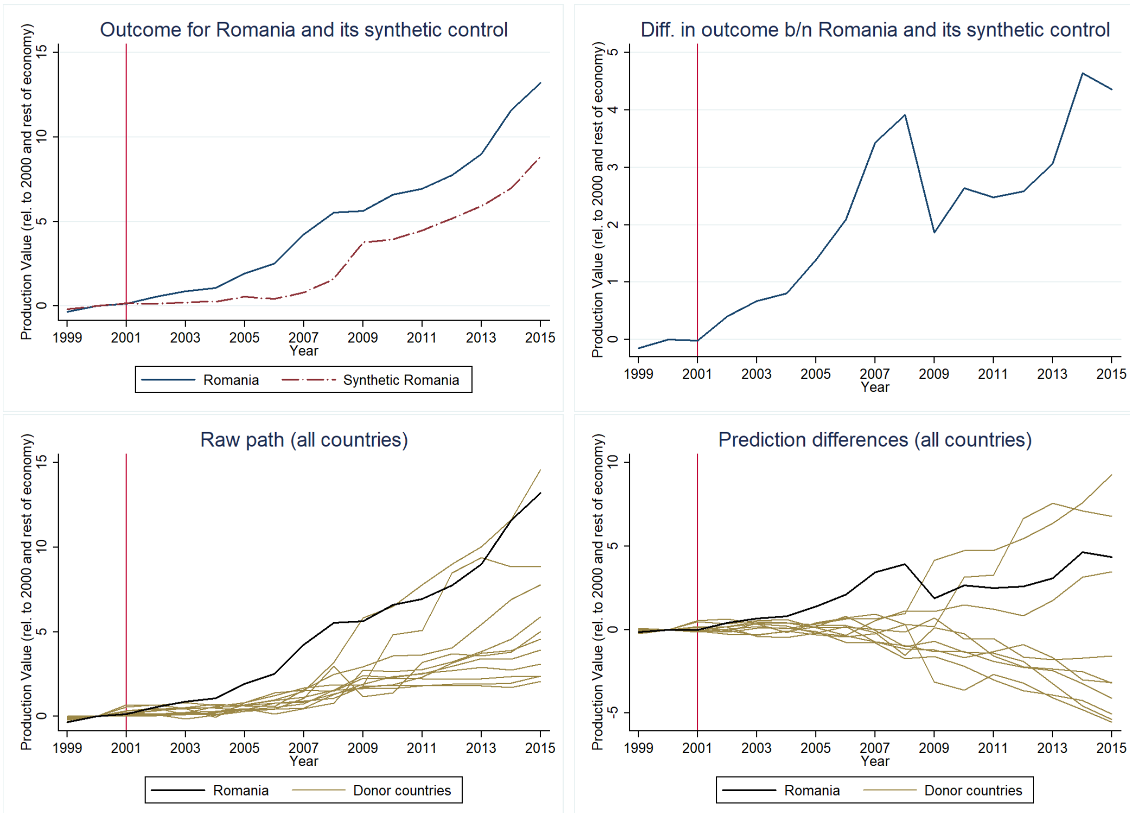


Figure C.1: IT Sector Vs. Rest of Economy. SCM with Outcome Variable: "Production Value" - Million Euro (Normalized)

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.1 to study the sector-level direct effects of the introduction of the 2001 law granting an income tax break to workers in IT. All figures have as dependent variable the country-level (normalized) " Production value - million euro" The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sector is K72 ("Computer and related activities," including "Software consultancy and supply " and "Publishing of software"). We use as comparison sectors all other sectors in the economy (all except K72). The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the "GDP per capita (constant LCU)," "Medium and high-tech industry (% manufacturing value added)" and "Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

	Gross Revenue			**Employment**			**Production Value**		
Post-treatment year	$\hat{\alpha}_{1t}$	p-values	Standardized p-values	$\hat{\alpha}_{1t}$	p-values	Standardized p-values	$\hat{\alpha}_{1t}$	p-values	Standardized
2001	0.13	0.27	0.55	0.14	0.27	0.00	-0.01	1.00	1.00
2002	0.69	0.00	0.27	0.26	0.09	0.00	0.41	0.10	0.50
2003	1.13	0.00	0.27	0.67	0.09	0.00	0.67	0.00	0.80
2004	1.37	0.00	0.27	1.13	0.00	0.00	0.80	0.00	0.30
2005	1.95	0.00	0.36	1.46	0.00	0.00	1.39	0.00	0.40
2006	2.57	0.00	0.36	1.75	0.00	0.00	2.10	0.00	0.50
2007	3.17	0.00	0.36	2.10	0.00	0.00	3.43	0.00	0.30
2008	3.21	0.00	0.27	1.90	0.00	0.00	3.92	0.00	0.40
2009	4.14	0.09	0.27	1.17	0.27	0.00	1.87	0.20	0.70
2010	4.49	0.09	0.27	1.03	0.45	0.00	2.64	0.30	0.70
2011	2.92	0.36	0.27	1.12	0.36	0.00	2.48	0.40	0.70
2012	3.32	0.45	0.36	1.19	0.18	0.00	2.59	0.40	0.90
2013	4.52	0.36	0.36	1.32	0.18	0.00	3.06	0.50	0.90
2014	6.15	0.36	0.36	1.54	0.18	0.00	4.64	0.30	0.70
2015	6.52	0.27	0.36	1.83	0.18	0.00	4.36	0.50	0.80

Table C.10: Post-treatment Results: Effects, p-values, Standardized p-values. Outcomes: Gross Revenue, Employment, and Production Value

	Romania	Synthetic Romania		
		Gross Revenue	Employment	Prod. Value
GDP per capita (constant LCU, \$)	9,663	9,796	15,972	5,341
Services (% of GDP)	53.23	60.32	60.07	62.59
High-tech manufacturing (% mfg. value added)	23.85	35.69	34.05	23.85

Table C.11: Predictor balance, averaged between 1999 and 2000. Synthetic Romania = 0.104 Czech Republic and 0.896 Slovakia (Gross Revenue); = 0.534 Bulgaria and 0.466 Czech Republic (Employment); = 0.339 Bulgaria and 0.661 Lithuania (Production Value)

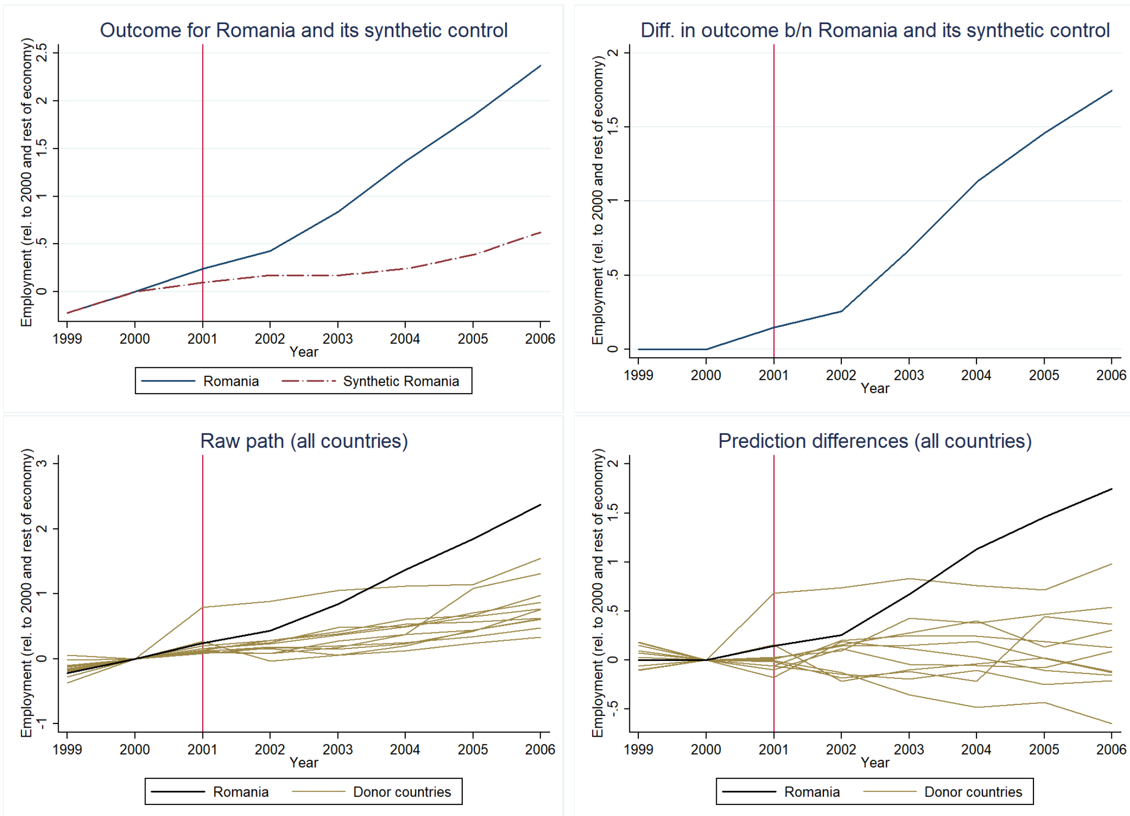


Figure C.2: IT Sector Vs. Rest of Economy. SCM with Outcome Variable: "Employees - Number" (Normalized). Robustness to the Correspondence between NACE Industry Classifications

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.1 to study the sector-level direct effects of the introduction of the 2001 law granting an income tax break to workers in IT. This exercise differs from the one presented in Figure 4.4 in the period of analysis. Instead of considering the entire 1999 to 2015 period, we cut the analysis in 2006 (the last year where the data is available in the NACE Rev 1 classification). This tests the robustness of our results to the correspondence we develop between the NACE Rev 1 and NACE Rev 2 classifications. All figures have as dependent variable the country-level (normalized) "Employees - number." The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sector is K72 ("Computer and related activities," including "Software consultancy and supply " and "Publishing of software"). We use as comparison sectors all other sectors in the economy (all except K72). The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the "GDP per capita (constant LCU)," "Medium and high-tech industry (% manufacturing value added)" and "Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

IT-Using Sectors Vs. Non-IT Using Sectors

Under Quartile 1	**Between Quartiles 1-2**
Wood and products of wood and cork (20) Basic metals (27) Fabricated metal products (28) Electricity, gas and water supply (40-41)	Manufacture of textiles and textile products (17) Manufacture of wearing apparel; dressing, dyeing of fur (18) Manufacture of pulp, paper and paper products (21) Publishing, printing and reproduction of recorded media (22) Manufacture of machinery and equipment n.e.c. (29) Manufacture of furniture; manufacturing n.e.c. (36) Recycling (37) Construction (45)
Between Quartiles 2-3	**Over Quartile 3**
Manufacture of chemicals and chemical products (24) Manufacture of rubber and plastic products (25) Manufacture of other non-metallic mineral products (26) Manufacture of office machinery and computers (30) Manufacture of electrical machinery and apparatus (31) Manufacture of radio, television and communication (32) Manufacture of medical, precision, optical instruments (33) Manufacture of motor vehicles, trailers, semi-trailers (34) Manufacture of other transport equipment (35) Hotels and restaurants (55)	Wholesale and retail trade (50-52) Transport and storage (60-63) Real estate activities (70) Renting of machinery and equipment (71) Computer and related activities (72) R&D and other business activities (73-74)

Table C.12: Sectors with Consistently-Available Data in Eurostat, by Quarter of IT-use Intensity

Notes: This table groups sectors with consistently-available data in Eurostat (across variables, years, and countries - see C.4 for details) based on their dependence on the IT sector for inputs. To establish this dependence, we start from the input-output table (I-O table, henceforth) of Romania for the year 2000. We use the harmonized I-O table provided by the *OECD*, that tracks the flows of goods and services between all two-digit NACE Rev 1 sectors in Romania. We then compute the share of the total input expenditures of a given sector purchased from the IT sector (NACE Rev 1 sector 72, "Computer and related activities"). It is based on this distribution of shares that we compute the quartiles mentioned in this table. In the main specification of the inter-industry SCM analysis we exclude sector 72 altogether from the analysis, as this is the treated sector itself. We also assign sectors over quartile three into the high-intensity category and all other sectors into the low-intensity. Note that the sectors actually used in the analysis and presented in this table are a subset of all sectors in the economy, as not all sectors had consistently-available data. Nevertheless, the grouping of these sectors by quarters was based on the full I-O matrix, which includes all sectors in the economy.

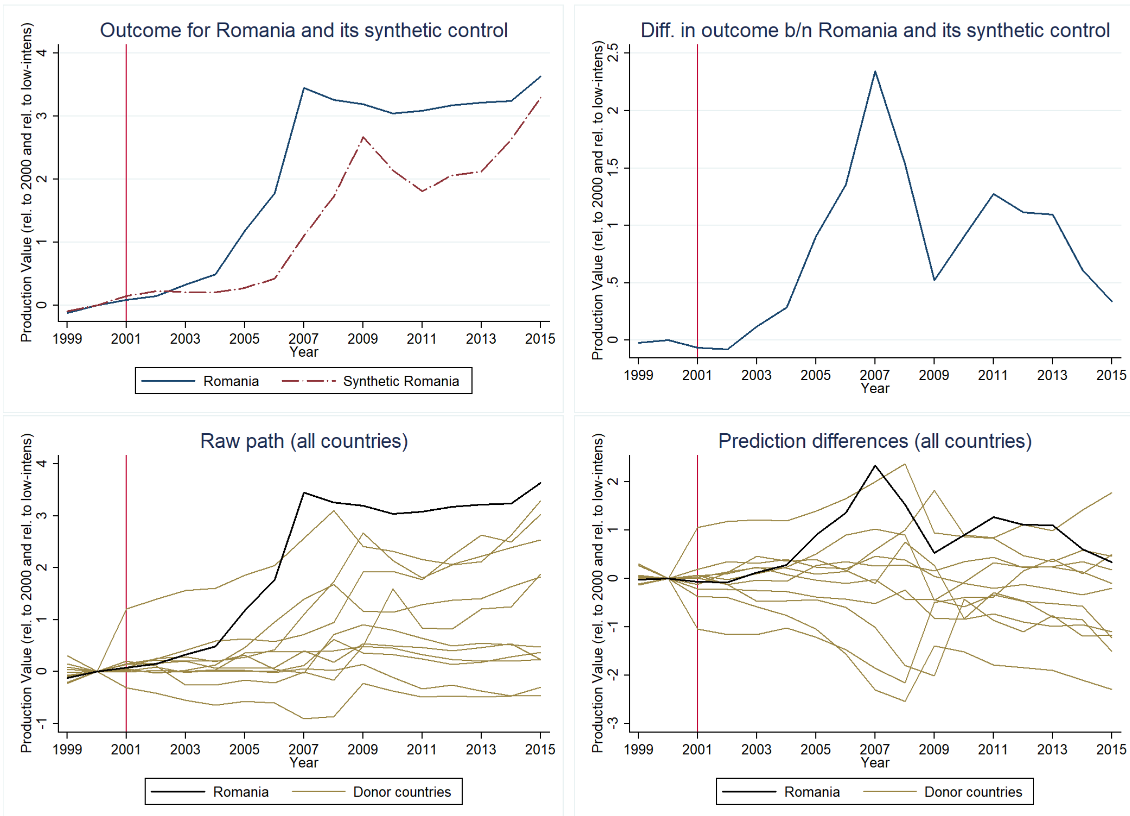


Figure C.3: IT-Using Sectors Vs. Non-IT Using Sectors. SCM with Outcome Variable: "Production Value" - Million Euro (Normalized)

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.2 to study the sector-level downstream effects of the introduction of the 2001 law granting an income tax break to workers in IT. All figures have as dependent variable the country-level (normalized) " Production value - million euro." The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sectors are those that use K72 ("Computer and related activities," including "Software consultancy and supply" and "Publishing of software") services at high-intensity. We exclude K72 itself from this category. Sectors that have a low-intensity of use of K72 services serve as comparison sectors. The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the "GDP per capita (constant LCU)," "Medium and high-tech industry (% manufacturing value added)" and "Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

	Gross Revenue			**Employment**			**Production Value**		
Post-treatment year	$\hat{\alpha}_{1t}$	p-values	Standardized p-values	$\hat{\alpha}_{1t}$	p-values	Standardized p-values	$\hat{\alpha}_{1t}$	p-values	Standardized
2001	-0.04	0.80	0.50	0.08	0.22	0.22	-0.07	0.64	0.64
2002	-0.02	1.00	0.50	0.09	0.22	0.11	-0.08	0.91	0.64
2003	0.18	0.30	0.50	0.27	0.22	0.11	0.12	0.82	0.55
2004	0.28	0.00	0.40	0.43	0.22	0.11	0.29	0.55	0.45
2005	0.54	0.00	0.40	0.68	0.00	0.11	0.90	0.27	0.27
2006	0.90	0.00	0.40	0.86	0.00	0.11	1.36	0.27	0.18
2007	1.71	0.00	0.40	0.93	0.00	0.11	2.34	0.00	0.18
2008	1.88	0.00	0.40	0.98	0.11	0.11	1.53	0.36	0.36
2009	1.46	0.00	0.40	0.77	0.00	0.11	0.52	0.45	0.27
2010	1.33	0.00	0.40	0.68	0.00	0.11	0.90	0.09	0.27
2011	1.34	0.00	0.40	0.73	0.00	0.11	1.27	0.09	0.27
2012	1.30	0.10	0.40	0.68	0.00	0.11	1.11	0.18	0.27
2013	0.68	0.30	0.40	0.67	0.00	0.11	1.10	0.09	0.27
2014	0.73	0.30	0.50	0.61	0.22	0.11	0.61	0.45	0.36
2015	0.75	0.50	0.50	0.61	0.11	0.11	0.34	0.73	0.55

Table C.13: Post-treatment Results: Effects, p-values, Standardized p-values. Outcomes: Gross Revenue, Employment, and Production Value

	Romania	Synthetic Romania		
		Gross Revenue	Employment	Prod. Value
GDP per capita (constant LCU, $)	9,663	5,342	9,616	5,771
Services (% of GDP)	53.23	62.59	62.53	60.85
High-tech manufacturing (% mfg. value added)	23.85	23.87	24.15	30.53

Table C.14: Predictor balance, averaged between 1999 and 2000. Synthetic Romania = 0.341 Bulgaria and 0.659 Lithuania (Gross Revenue); = 0.363 Bulgaria + 0.002 Hungary + 0.625 Lithuania (Employment) ; = Bulgaria (Production Value)

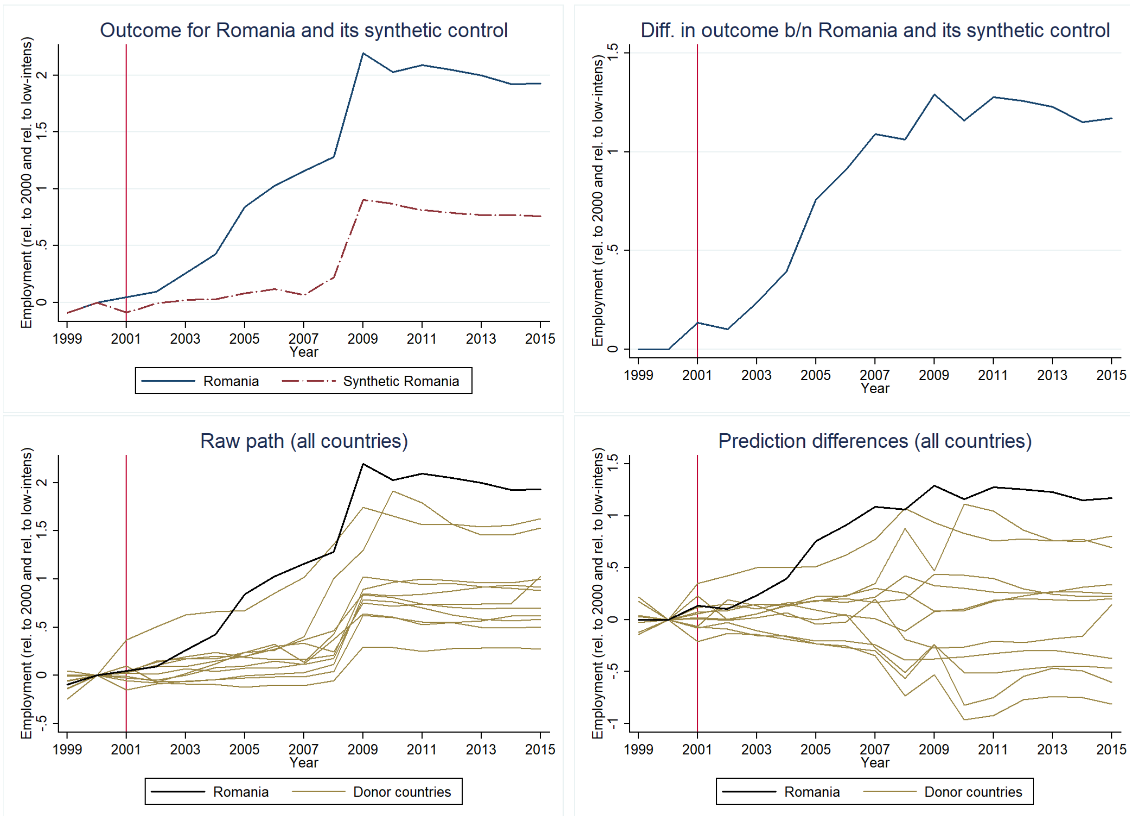


Figure C.4: IT-Using Sectors Vs. Non-IT Using Sectors. SCM with Outcome Variable: "Employees - Number" (Normalized). Robustness to Exclusion of Sectors Comparable to IT from High-Intensity Sectors Category

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.2 to study the sector-level downstream effects of the introduction of the 2001 law granting an income tax break to workers in IT. This exercise differs from the one presented in Figure 4.6 in its exclusion of sectors K73 and K74 from the analysis. Both sectors are otherwise part of the list of sectors that rely heavily on IT services (top 25% users of IT services). We used these sectors as comparison sectors in the firm-level analysis. All figures have as dependent variable the country-level (normalized) "Employees - number." The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sectors are those that use K72 ("Computer and related activities," including "Software consultancy and supply" and "Publishing of software") services at high-intensity. We exclude K72 itself from this category. Sectors that have a low-intensity of use of K72 services serve as comparison sectors. The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the "GDP per capita (constant LCU)," "Medium and high-tech industry (% manufacturing value added)" and "Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

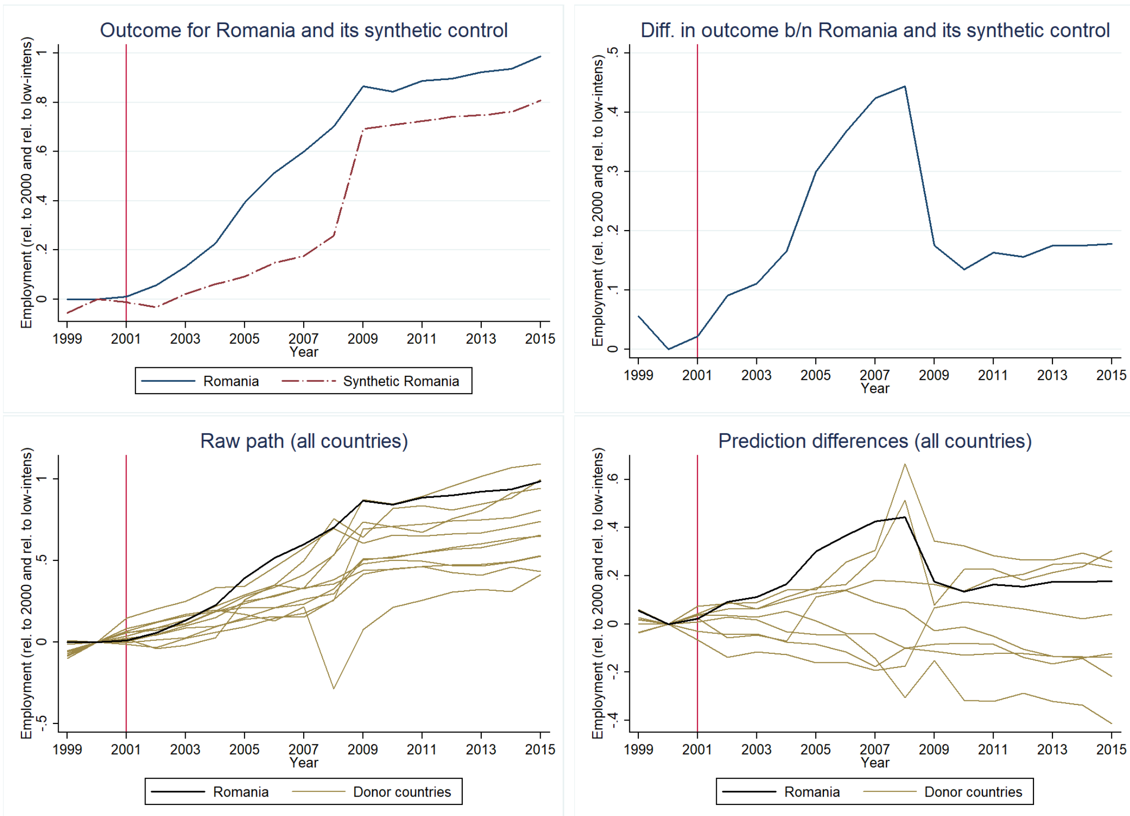


Figure C.5: IT-Using Sectors Vs. Non-IT Using Sectors. SCM with Outcome Variable: “Employees - Number" (Normalized). Robustness to Grouping of Quarters into High- and Low-Intensity

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.2 to study the sector-level downstream effects of the introduction of the 2001 law granting an income tax break to workers in IT. This exercise differs from the one presented in Figure 4.6 in the grouping of quarters into high- and low-intensity IT users. The main results compare the top 25% sectors in terms of IT services use (high-intensity category) with the bottom 75% (low-intensity category). In this exercise we compare the top 50% (high-intensity category) to the bottom 50% (low-intensity category). All figures have as dependent variable the country-level (normalized) “Employees - number." The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sectors are those that use K72 (“Computer and related activities," including “Software consultancy and supply" and “Publishing of software") services at high-intensity. We exclude K72 itself from this category. Sectors that have a low-intensity of use of K72 services serve as comparison sectors. The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the “GDP per capita (constant LCU)," “Medium and high-tech industry (% manufacturing value added)" and “Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

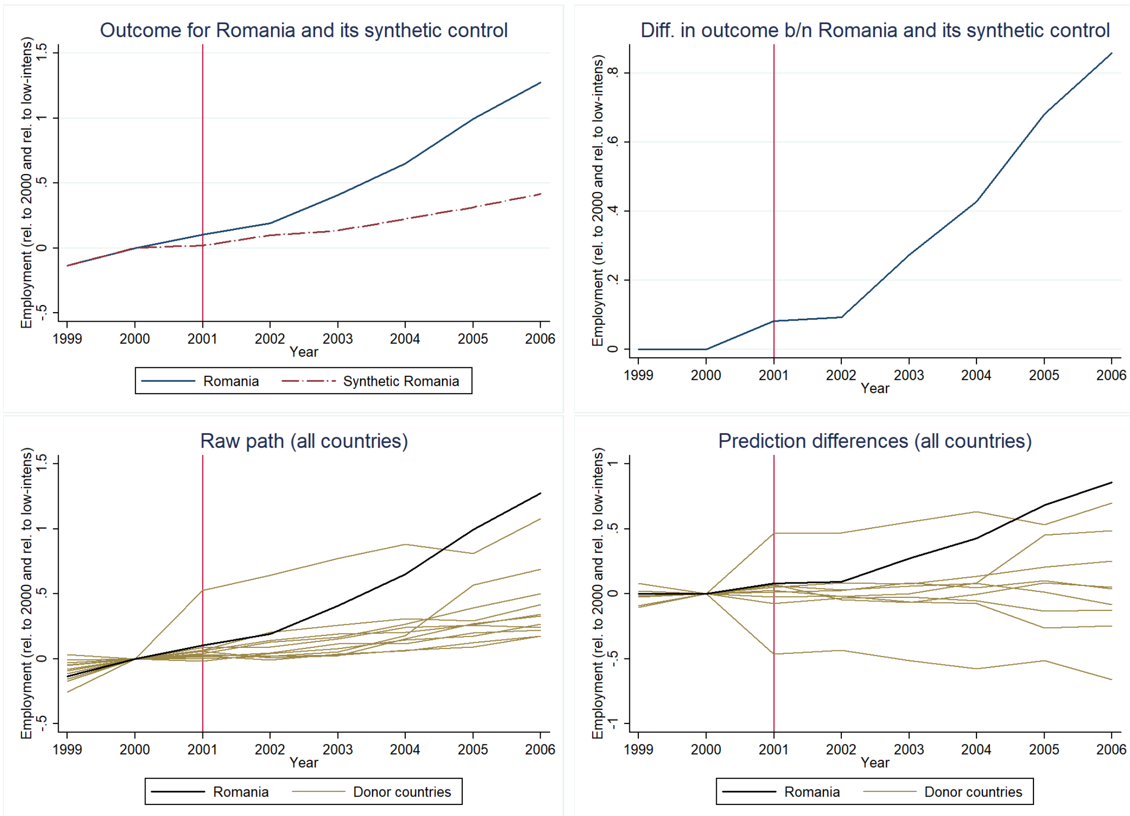


Figure C.6: IT-Using Sectors Vs. Non-IT Using Sectors. SCM with Outcome Variable: "Employees - Number" (Normalized). Robustness to the Correspondence between NACE Industry Classifications

Notes: In this exercise we use the synthetic control method introduced in Section 4.4.2 to study the sector-level downstream effects of the introduction of the 2001 law granting an income tax break to workers in IT. This exercise differs from the one presented in Figure 4.6 in the period of analysis. Instead of considering the entire 1999 to 2015 period, we cut the analysis in 2006 (the last year where the data is available in the NACE Rev 1 classification). This tests the robustness of our results to the correspondence we develop between the NACE Rev 1 and NACE Rev 2 classifications. All figures have as dependent variable the country-level (normalized) "Employees - number." The yearly absolute value of the dependent variable in the treated sector is divided by its value in 2000, the year prior to the introduction of the income tax break in Romania. From these resulting yearly ratios we subtract the corresponding ratios for the comparison sectors. The treated sectors are those that use K72 ("Computer and related activities," including "Software consultancy and supply" and "Publishing of software") services at high-intensity. We exclude K72 itself from this category. Sectors that have a low-intensity of use of K72 services serve as comparison sectors. The data source for the dependent variable is Eurostat, Structural Business Statistics, Annual detailed enterprise statistics on services (NACE Rev 1.1). Data for the predictors comes from the World Bank, World Development Indicators. We use as predictors the "GDP per capita (constant LCU)," "Medium and high-tech industry (% manufacturing value added)" and "Services, etc., value added (% of GDP)." All figures are an output of the **synth_runner** package for Stata (Quistorff and Galiani, 2017), with the *nested* option specified.

Post-treatment year	$\hat{\alpha}_{1t}$	p-values	Standardized p-values
2001	0.02	0.90	0.90
2002	0.03	0.80	0.70
2003	0.16	0.80	0.40
2004	0.53	0.10	0.10
2005	1.03	0.00	0.20
2006	1.60	0.10	0.10
2007	2.30	0.10	0.10
2008	3.64	0.10	0.10
2009	4.76	0.00	0.10
2010	5.96	0.00	0.10
2011	7.46	0.00	0.10
2012	6.79	0.00	0.10
2013	7.69	0.00	0.10
2014	7.70	0.00	0.10

Table C.15: Post-treatment Results: Effects, p-values, Standardized p-values. Outcome: (Goods Export) Trade Values

	Romania	
	Actual	Synthetic
GDP per capita (constant LCU, \$)	9,771	20,689
Services (% of GDP)	47.89	58.41
High-tech manufacturing (% mfg. value added)	24.33	32.60

Table C.16: Predictor balance, averaged between 1996 and 2000. Synthetic Romania = 0.706 Czech Republic, 0.240 Lithuania, and 0.053 Slovakia

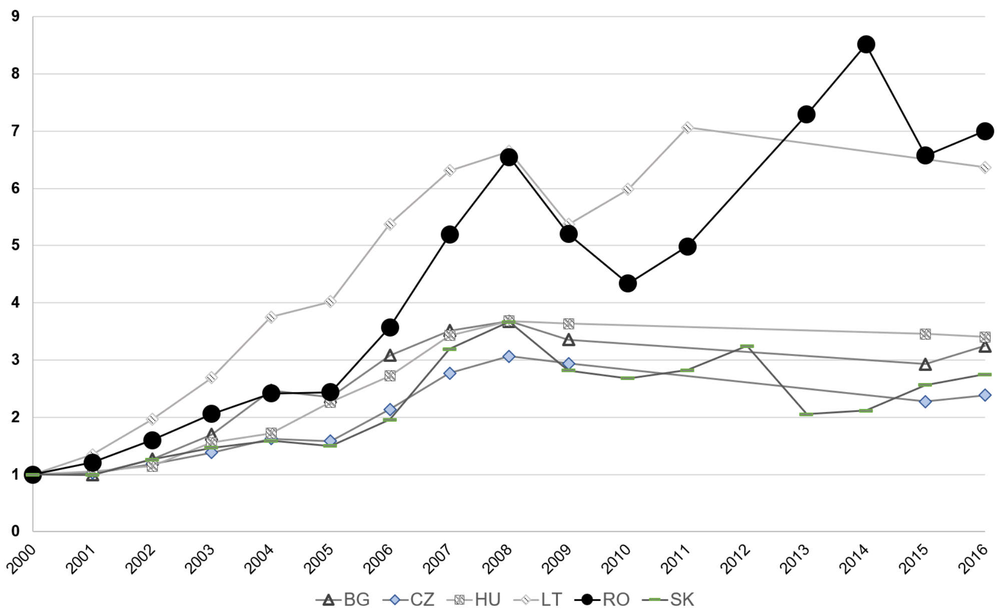


Figure C.7: Trends in Export Value of IT-using Service Sectors

Notes: This figure plots the evolution of export trade values for service sectors relying more on IT services. These sectors are NACE Rev 1 sectors 51 (Wholesale trade and commission trade), 60 (Land transport), 62 (Air transport), 63 (Cargo handling and storage), 74 (Other business activities), and 75 (Public administration and defence). In Romania, most of the impressive growth experienced between 2001 and 2016 is explained by sectors under NACE Rev 1 sector 74 (e.g., call centers, advertising, business and management consultancy, secretarial and translation activities etc.). The figure includes Romania and five other countries that appear in SCM exercises as part of synthetic Romania, i.e. Bulgaria, the Czech Republic, Hungary, Lithuania, and Slovakia. This evolution is with respect to the export value in year 2000. This figure uses UN Comtrade data, EBOPS 2002. A similar graph can be made comparing the growth of IT-using service sectors to the growth of less IT-using service sectors. Romania continues to stand out in a relative comparison as well.

C.2 Growth of the Sector on the Extensive Margin

C.2.1 Sectoral Switches of Existing Firms to the IT Sector

One channel through which the income tax break law could have boosted the growth of the Romanian IT sector is by changing incentives of firms (workers) to conduct their activity in the eligible sector. Using a sector-level transition matrix shared by the *National Bank of Romania*, we study transitions into the eligible sector (NACE Rev 1 code 722), around the introduction of the 2001 income tax break. While this evidence is descriptive, it highlights abnormal trends in transitions concomitant to the introduction of the income tax break.

Figure C.8 plots the absolute number of firms alive in year $t-1$ (with an economic activity different from 722) that switched in year t to the NACE Code 722 (7221 and 7222, Software consultancy and supply). Figure C.9 plots the year t share of firms in NACE Code 722 alive in year $t-1$ coming from firms with a different economic activity in $t-1$ that have switched their year t economic activity to 722. We notice that while switching one's sector towards 722 was a trend occurring both before and long after the passing of the 2001 and 2004 laws, immediately after the initial passing of the law, switching to NACE Rev 1 sector 722 became a visibly more popular practice. From 2004 onward, the momentum of this switching practice is lost, which suggests that most justifiable switches occurred immediately after the passage of the law.

One caveat to this exercise is that we cannot pin down the exact reasons behind these switches in firms' main sectoral code. Hence, this type of growth of the sector may be to some extent artificial, if firms switching their main sectoral activity were already conducting most of their activity in the eligible sector, but had a misassigned main sector. As the law made the income tax break eligibility dependent on a firm's main sectoral code, the law may have incentivized corrections in firms' sectoral codes.

Most switches in firms' main sectoral code occurred from sectoral codes complementary to the eligible sector (e.g., 721 - “Consulting in the field of computing equipment / hardware" and 726 - “Other activities related to computer science"). We conjecture that these switching firms were multi-activity firms that decided to focus on software development once the income tax break for programmers became available. One might be concerned that these sectoral switches were meant to deceit tax authorities. We do not believe that this concern is justified, because the sector of the firm was only one of several strict requirements for a worker to qualify for the exemption. In particular, the firm had to provide thorough evidence that a revenue of at least 10,000 U.S. dollars per exempted employee came from software development and that employees involved in this software development had eligible bachelor degrees.

For these reasons, the evidence in Figures C.8 and C.9 suggests that switches were likely to be motivated by an actual focus of the firm on software development. Even if part of these switches may not have resulted in a stronger focus on software development (had some of these firms had a previously misassigned main sector), it is plausible to expect that the income tax break law has strengthened incentives to focus on software development. Moreover, the stark jump in switches around 2001 is a convincing piece of evidence on the salience of the law.

C.2.2 Firm Entries and Exits in the IT Sector

The income tax break may have also affected the growth of the software sector by increasing the birth rate of new firms in the sector and/or decreasing firm death rates. In this section, we provide descriptive evidence on the evolution of firm birth and death rates in the sector (722, in NACE Rev 1.1 classification) in the years before and after the introduction of the policy. The birth/entry (death/exit) rate at time t is defined as the number of firm births/entries (deaths/exits) relative to the population of active firms at the beginning of time t. Our analysis is based on sector-level data from the Eurostat Business Demography

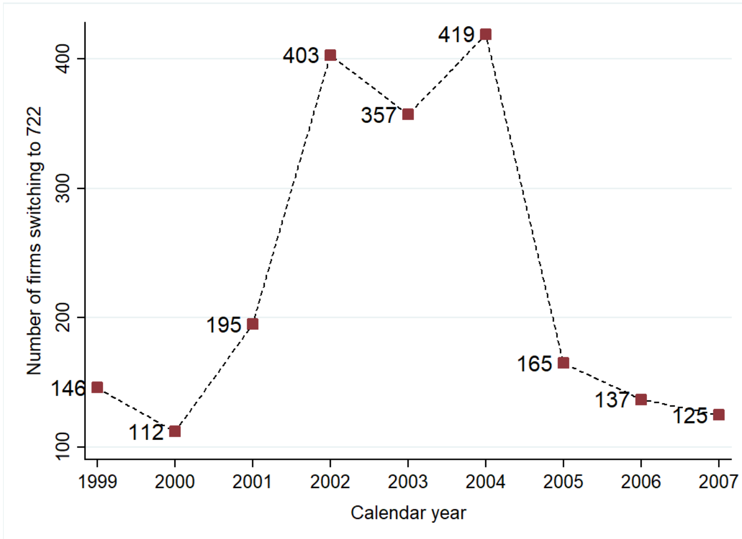


Figure C.8: Romania: Number of firms alive in year $t-1$ and switching their economic activity in t towards the beneficiary sector, 722

Notes: Data source: *National Bank of Romania*. This graph plots the number of firms alive in year $t-1$ (with an economic activity different from 722) that switched in year t to the NACE Code 722 (7221 and 7222, Software consultancy and supply). We notice two peaks in this practice in 2002 and 2004, the years after the first and second laws of income tax exemption for workers in sector 722 were passed in Romania.

database. Figures C.10 and C.11 show the evolution of birth and death rates in the software sector between 2000 and 2007 for Romania and four other comparable Central and Eastern European (CEE) countries. These other countries were chosen based on data availability for the entire 2000 to 2007 period and on their similarity to Romania (i.e., they are transition economies, at similar stages of development, and with similar economic structures).

The main takeaway from Figure C.10 is that the birth rate of firms in the IT sector in Romania experienced a notable increase in 2003 and 2004 relative to its pre-tax break levels.[6] No similar increase can be observed for any of the other four CEE countries. There is a two-year lag in the peak of the firm birth rate in Romania, which is consistent with a need for entrepreneurs to be reassured that the tax break was not to be reverted in the short-term. Figure C.11 shows the evolution of firm death rates in the IT sector in Romania and in comparable CEE countries. While overall the death rate in Romania does not seem to be affected by the introduction of the tax break, throughout the period of analysis Romanian firm death rates remain less volatile than those in comparable countries. The combined trends in firm birth and death rates lead to an increasing trend in the stock of firms in the IT sector in Romania.

[6] As shown in C.2.3, from 2003 to 2016, the stock of FDI in the IT sector of Romania has been multiplied by twenty. A sizable share of the firm entry into the IT sector is likely to be due to foreign firm entry.

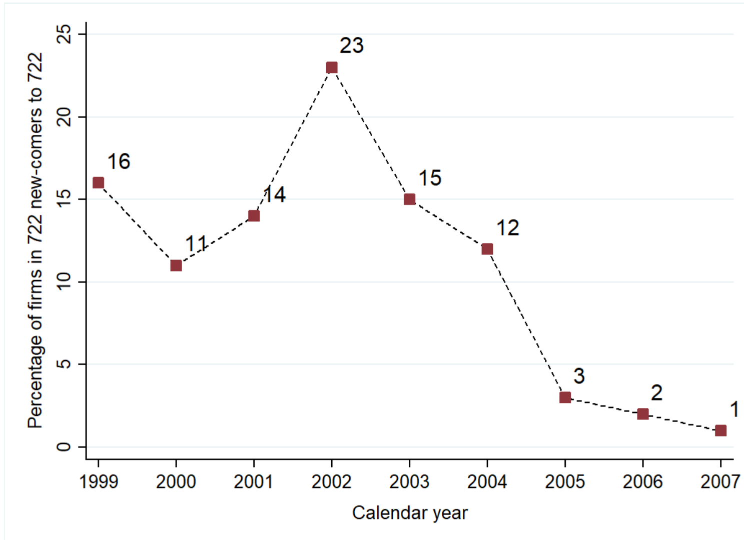


Figure C.9: Romania: Among firms in NACE code 722 in year t and alive in year $t - 1$ (with any economic activity), the percentage of new-comers in t to 722

Notes: Data source: *National Bank of Romania.* This graph plots the year t share of firms in NACE Code 722 (7221 and 7222, Software consultancy and supply) alive in year $t-1$ coming from firms with a different economic activity in $t-1$ that have switched their year t economic activity to 722. We notice a clear peak in this practice in 2002, the year after the first law of income tax exemption for workers in sector 722 was passed in Romania.

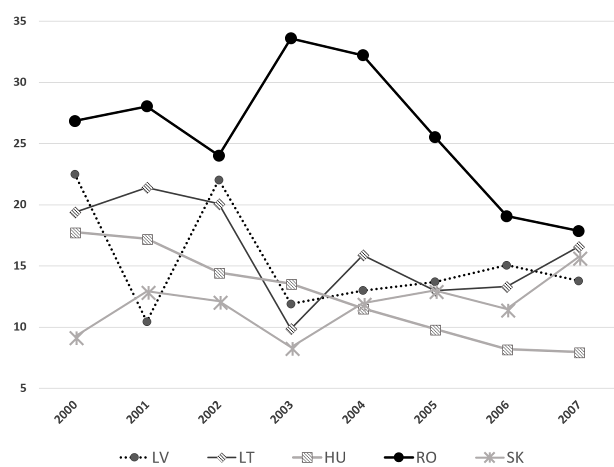


Figure C.10: Entry Rates of Firms in the “Software Consultancy and Supply" Sector in Romania and Comparable Countries from Central and Eastern Europe

Notes: Data source: Sector-level data from the Eurostat Business Demography database. The entry rate at time t in a given country is defined as the number of firm entries (births) relative to the population of active firms in that country at the beginning of time t.

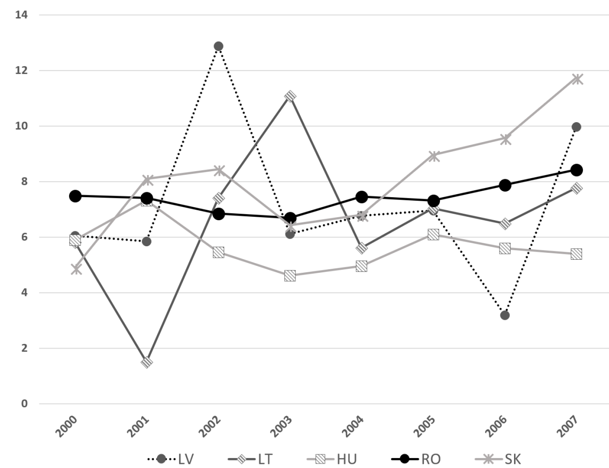


Figure C.11: Death Rates of Firms in the "Software Consultancy and Supply" Sector in Romania and Comparable Countries from Central and Eastern Europe

Notes: Data source: Sector-level data from the Eurostat Business Demography database. The exit rate at time t in a given country is defined as the number of firm exits (deaths) relative to the population of active firms in that country at the beginning of time t.

C.2.3 Firm Entry through Foreign Direct Investment in IT

In terms of foreign ownership in the IT sector of Romania, the early 2000s were the turning point. Between 2000 and 2004, the first multinational firms decided to offshore part of their operations to Romania (Pruna and Soleanicov, 2012). Moreover, during the same period, there were also smaller foreign companies that acquired Romanian firms, e.g., Adobe Systems Inc. who acquired InterAKT (Pruna and Soleanicov, 2012). Ever since, Romania has continued attracting steady FDI inflows into the IT sector, to the point that in 2017 the biggest players in the IT sector of Romania were multinationals (Oracle, IBM, Ericsson, and Endava). Bitdefender is the only Romanian firm with comparable operating revenue from its operations in Romania.[7] In 2017 foreign-owned companies produced 73% of the gross revenues and hired 59% of total employees in the Software and IT Services of Romania.

[7]See December 2017 article from the *Romanian Journal*.

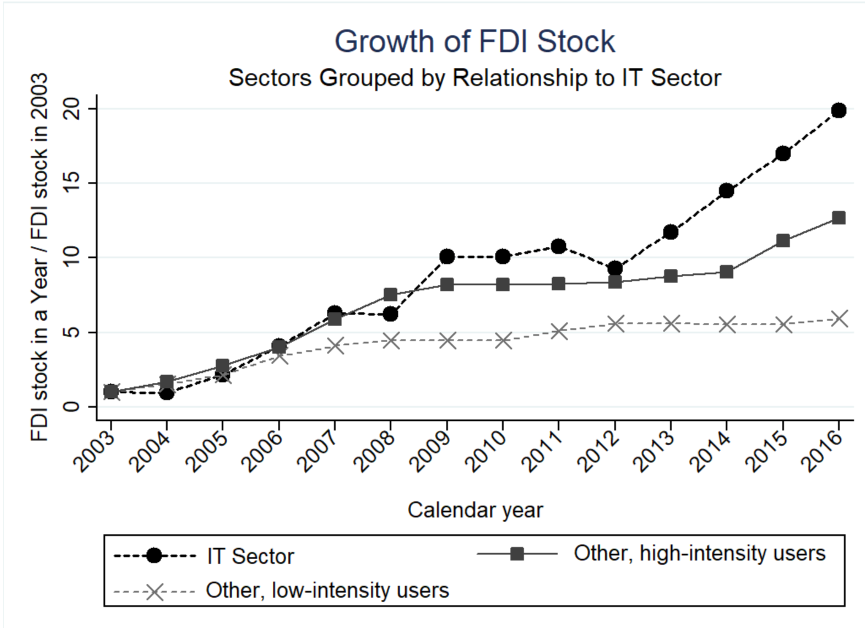


Figure C.12: Growth of the Stock of FDI in Romania, by Relationship to the IT Sector

Notes: Data sources: *National Bank of Romania* (for historical sector-level FDI stocks) and *OECD* (for the Romanian input-output table for the year 2000, at the two-digit NACE Rev 1 level). This figure plots the growth of the FDI stock in Romania between 2003 and 2016 (defined as the yearly value of the FDI stock divided by the relevant FDI stock value in 2003) for three categories of sectors. The first category is the sector 72 ("Computer and related activities") itself. The second category called "Other, high-intensity users" contains all two-digit NACE Rev 1 sectors that are in the top quarter of the distribution of sector-level shares of input purchases from sector 72 in a sector's total input expenditure (except sector 72). These shares are computed based on Romania's input-output table for the year 2000. The last category of sectors called "Other, low-intensity users" contains all other sectors for which IT inputs are less important. The following are the NACE Rev 1 codes and names of the sectors in the "Other, high-intensity users" category: 50-52 (Wholesale and retail trade; repairs), 60-63 (Transport and storage), 70 (Real estate activities), 71 (Renting of machinery and equipment), 73-74 (R&D and other business activities), 75 (Public administration and defence; compulsory social security), 80 (Education), and 85 (Health and social work).

An ideal dataset to study the causal effects of the income tax break for workers in IT on Foreign Direct Investment (FDI, henceforth) into the IT sector of Romania would have had to start before 2001, include not only Romania but also comparable countries, and be disaggregated at the sector-level. Unfortunately, such a dataset was not found. For Romania, the longest sector-level FDI time series (from the National Bank of Romania) starts in 2003. With this dataset, we study the growth of the FDI stock for three categories of sectors: the IT sector itself, the group of sectors that are "high-intensity users" of IT services, and the "low-intensity users".[8] Figure C.12 plots the growth of the FDI stock in Romania between 2003 and 2016 (defined as the yearly value of the FDI stock divided by the relevant FDI

[8]See Section 4.4.2 for details on how we assign sectors into either the high- or low-usage groups.

stock value in 2003) for these three categories of sectors. The figure shows that sectors with the highest share of IT services in their input expenditure are those experiencing the highest growth of their FDI stock since 2003. The IT sector itself is experiencing an even starker growth in its FDI stock.

The importance of foreign revenue has also been growing steadily over the years, to the point that in 2017 total foreign revenues were three times larger than domestic revenues.[9]

C.3 Descriptive Statistics of Firm-Level Data

C.3.1 Amadeus Data: 1999 - 2005

Variable	# Obs.	Mean	Median	SD
Firms in eligible sector				
Operating Revenue	669	175.46	37.00	588.81
Number of Workers	669	34.38	4.00	646.07
Total Assets	669	92.44	16.00	300.20
Solvency Ratio	652	0.20	0.16	0.33
Firms in baseline set of comparison sectors				
Operating Revenue	953	321.92	39.00	1,631.15
Number of Workers	953	16.78	3.00	58.92
Total Assets	953	248.32	12.00	2,361.41
Solvency Ratio	926	0.20	0.13	0.33

Table C.17: Descriptive Statistics of Firms in Treated and Comparison Sectors in Year 2000

Notes: Table C.17 reports descriptive statistics for the baseline sample used to study the firm-level impact of the introduction in 2001 of the income tax break for workers in IT. The data source is Amadeus. These statistics pertain to year 2000, the year prior to the introduction of the policy of interest. The upper panel reports summary statistics for the firms in the eligible sector (722, NACE Rev 1), for the four main outcome variables of interest. The lower panel reports summary statistics for the firms that belong to the baseline set of comparison sectors: 721, 723, 724, 725, 726, 731, 732, all NACE Rev 1. The unit of measure for the operating revenue and assets variables is thousands of euros.

[9] Source of last two sets of statistics: the Association of Employers in the Software and Services Industry or ANIS.

C.3.2 Administrative Data: 2011 - 2015

Sector eligible Exempted after 2013	Not eligible Not exempted		Eligible Not exempted		Eligible Exempted	
	Mean	SD	Mean	SD	Mean	SD
Employees	21.9	253.1	4.1	13.4	10.4	34.4
Revenue (× 1,000 RON)	8,718.9	143,729.8	890.0	4,464.1	2,100.6	11,125.0
Sales (× 1,000 RON)	8,179.2	137,021.7	653.0	3,006.2	2,015.6	11,091.8
Assets (× 1,000 RON)	11,230.0	177,337.7	1,001.0	8,221.4	1,365.6	8,433.1
Rel. Prod	0.60	0.11	0.58	0.11	0.58	0.11
Micro	0.84	0.36	0.91	0.29	0.81	0.39
Small	0.11	0.31	0.06	0.24	0.13	0.34
Medium	0.02	0.14	0.01	0.09	0.03	0.18
Large	0.03	0.17	0.02	0.15	0.02	0.15
Young	0.39	0.49	0.42	0.49	0.47	0.50

Table C.18: Descriptive Statistics of Firms in Year 2011, by Firm Category

Notes: This table reports descriptive statistics for 2011 (two years before the expansion of the income tax break law) and for the three types of firms that we include in our baseline sample. The first sample includes firms in sectors comparable to the IT sector. These comparable sectors were, however, not targeted by the income tax break law. The second and third samples contain firms in the eligible sectors. The second sample contains firms with less than 5% of employees exempted from the income tax in each year of our 2011 to 2015 sample. The third sample contains firms that had less than 5% of exempted employees in 2011 and 2012, but jumped to over 20% of exempted employees after 2013. 1 RON ≈ 0.2 euros.

Table C.18 presents descriptive statistics on firm size (measured in terms of employment, revenue, production, or assets) and relative productivity in 2011 for the three groups of firms in our baseline sample: firms in non-eligible sectors, firms in eligible sectors with less than 5% of employees exempted from the income tax throughout the entire sample period, and firms that had less than 5% of exempted employees in 2011 and 2012, but jumped to over 20% of exempted employees after 2013. Firms in non-eligible sectors are on average the largest. Firms in eligible sectors for which a large share of workers become exempted after 2013 have the second-highest average size. Last, firms in eligible sectors whose workers never became large-scale exempted have the smallest average size. In this last “eligible not exempted" category there is a larger share of micro firms (firms with less than ten employees): 91%, compared to 84% and 81%. The three types of firms do not differ in their average relative productivity. In the econometric analysis, we control for time-invariant differences in size using firm fixed effects and time-variant controls for size category, relative productivity, and age.

Share of firms with at least 1 exempted employee, raw dataset

Year	% of firms in ICT services	% of firms in HTKI services	% of firms in eligible sectors
2011	20%	18%	35%
2012	19%	18%	34%
2013	21%	20%	37%
2014	23%	22%	41%
2015	24%	23%	42%

Share of firms with at least 1 exempted employee, clean dataset used as main sample

Year	% of firms in ICT services	% of firms in HTKI services	% of firms in eligible sectors
2011	22%	21%	36%
2012	21%	20%	35%
2013	23%	22%	38%
2014	23%	22%	38%
2015	25%	25%	41%

Share of firms with 20% or more exempted employees, clean dataset used as main sample

Year	% of firms in ICT services	% of firms in HTKI services	% of firms in eligible sectors
2011	18%	17%	29%
2012	17%	17%	28%
2013	18%	18%	30%
2014	19%	19%	32%
2015	21%	21%	35%

Table C.19: Share of Firms with Exempted Employees in Different Samples

Notes: Table C.19 reports the share of firms in a given sample that have at least 1 exempted employee or that have that more than 20% of their employees exempted from the income tax for each year between 2011 and 2015. By construction, all firms in non-eligible sectors have less than 5% of their employees exempted from the income tax (employees who are likely to have a disability, another criterion on which this tax break can be granted). The sample of "firms in ICT services" used in the two lower panels is the baseline sample whose results are reported in Table 4.2. The sample of "firms in HTKI services" (HTKI stands for "high-tech knowledge-intensive") used in the two lower panels is the robustness check sample whose results are reported in Table C.5, (C.1.1). The sample of "firms in eligible sectors" used in the two lower panels contains only firms in sectors whose IT workers can be eligible to the income tax break and is the robustness check sample whose results are reported in Table C.6 (C.1.1).

Dependent variable	1 if more than 20% exempted employees			Share of exempted employees		
Sectors of Firms	ICT services (1)	HTKI services (2)	Eligible (3)	ICT services (4)	HTKI services (5)	Eligible (6)
In an eligible sector	0.273***	0.285***		0.170***	0.178***	
	(0.007)	(0.007)		(0.005)	(0.005)	
Foreign-owned	0.153***	0.143***	0.182***	0.096***	0.089***	0.115***
	(0.022)	(0.020)	(0.027)	(0.014)	(0.013)	(0.018)
Small	0.049***	0.044***	0.077***	0.021***	0.019***	0.032***
	(0.010)	(0.009)	(0.016)	(0.005)	(0.005)	(0.009)
Medium	0.126***	0.107***	0.178***	0.054***	0.047***	0.074***
	(0.024)	(0.020)	(0.033)	(0.013)	(0.011)	(0.017)
Large	0.136***	0.113***	0.223***	0.081***	0.069***	0.127***
	(0.043)	(0.034)	(0.061)	(0.023)	(0.019)	(0.033)
Young	0.004	0.004	0.005	0.006	0.006	0.010
	(0.006)	(0.006)	(0.010)	(0.004)	(0.004)	(0.007)
Rel. prod.	0.028	0.022	-0.001	0.001	-0.001	-0.032
	(0.034)	(0.032)	(0.058)	(0.026)	(0.024)	(0.045)
County-level % exemption	0.270***	0.273***	0.444***	0.182***	0.183***	0.299***
	(0.032)	(0.032)	(0.053)	(0.022)	(0.022)	(0.037)
d_{2012}	0.001	0.003	0.003	0.001	0.002	0.003
	(0.004)	(0.004)	(0.007)	(0.002)	(0.002)	(0.004)
d_{2013}	0.011**	0.012***	0.018**	0.007**	0.007**	0.012**
	(0.004)	(0.004)	(0.008)	(0.003)	(0.003)	(0.005)
d_{2014}	0.016***	0.016***	0.026***	0.011***	0.011***	0.019***
	(0.005)	(0.005)	(0.008)	(0.003)	(0.003)	(0.005)
d_{2015}	0.022***	0.022***	0.035***	0.009***	0.009***	0.014**
	(0.005)	(0.005)	(0.009)	(0.003)	(0.003)	(0.006)
Adjusted R^2	0.186	0.189	0.067	0.158	0.162	0.050
# Observations	26,514	27,414	16,234	26,514	27,414	16,234
# Firms	5,728	5,939	3,526	5,728	5,939	3,526

Table C.20: Predictors of Firm-level Exemption Performance

Notes: Table C.20 reports the results of OLS regressions run on three samples used in the analysis in Section 4.3.2 for 2011 to 2015. Columns (1)-(3) use as the dependent variable a dummy variable that takes value 1 for firm i in year t if firm i has more than 20% of its workers exempted from the income tax in year t. Columns (4)-(6) use as the dependent variable the share of workers of firm i who are exempted from the income tax in year t. The samples used in this table are different from the baseline sample in Table 4.2 and the samples in Tables C.5 and C.6 in that the former keep *all* firms in ICT service sectors, in HTKI (high-tech knowledge-intensive) service sectors, and in eligible sectors, whereas the latter bring additional restrictions on the firm-level share of workforce exemption before and after 2013. The explanatory variables of productivity, size, and age are contemporaneous. Data on foreign ownership in 2016 comes from Amadeus. Columns (3) and (6) include only firms whose sector is targeted by the income tax break. The reference category contains firms that operate in non-eligible sectors, are domestically-owned, have a micro size, and are older than five years. The reference year in 2011.

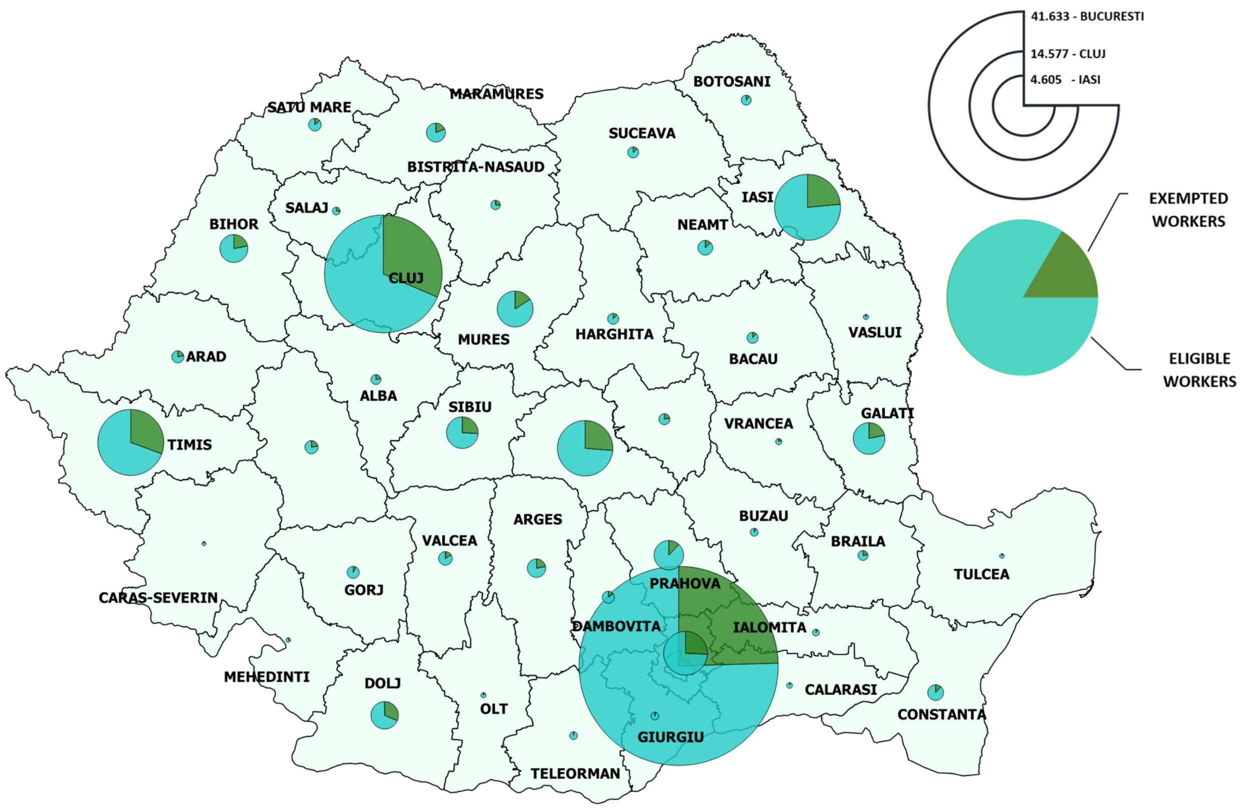


Figure C.13: County-Level Number of Employees in the Eligible IT Sector in 2015 and the Share of these Employees Actually Exempted from the Income Tax

Notes: The legend reports the total number of employees in the eligible IT sectors in 2015 in the three counties with the largest number of such employees, i.e., București, Cluj, and Iași. This map uses the sample from the 2011 to 2015 administrative dataset kept for the econometric analysis, as described in Section 4.3.2.

Figure C.13 is informative on the spatial distribution in 2015 of the number of workers in the sectors targeted by the income tax exemption and the share of these workers who are actually benefiting from this exemption. The unit of analysis is a county (*judeţ*). This map highlights the striking spatial concentration of the IT industry in Romania's leading counties, București (the capital), Cluj, Iași, and Timiș. While Bucureşti stands out as a giant in terms of its agglomeration of workers in IT, the second-placed county (Cluj) clusters only a third of the same type of employees. The third- and fourth-placed counties (Iaşi and Timiş) house a tenth of the number of workers in Bucureşti.

This map also draws attention to heterogeneity in the share of employees in the IT sector who actually benefit from the tax exemption. This share reaches a peak in Timiș (around 46%) and a low in Teleorman (under 5%). This heterogeneity is likely to reflect differences across workers and firms in their ability to meet the requirements of the tax break (firms in less developed counties are more likely to hire workers with a profile that does not meet the educational criteria, such firms are also less likely to earn more than 10,000 U.S. dollars per eligible employee etc.). Discrepancies are also likely to reflect differential abilities across firms to fill in the necessary paperwork for a worker to be granted the tax break.

From Table C.20 we learn that IT firms located in counties with a higher share of exemption of the total workforce in IT are more likely to have a higher share of workforce exemption themselves. This finding points to potentially localized knowledge spillovers on the necessary procedures to benefit from the tax break and the importance of a strong local pool of skilled programmers.

Another map can be drawn for the number of IT firms in each county and the share of these firms actually employing exempted employees. Such a map (available upon request) shows a slightly more equal distribution of the number of firms, which captures the tendency of the median firm in leading counties to hire significantly more workers than the median firm in lagging counties.

The absolute number of firms in the eligible sectors in a given county is to a certain extent capturing the size of the population of that county. For comparability across counties, we compute the county-level number of workers in the eligible sectors per 100,000 inhabitants. While Bucureşti remains the leader in the country with 2,257 workers in the eligible sectors per 100,000 inhabitants, Cluj continues to stand out in a comparable position with 2,078 such workers per 100,000 inhabitants. The following three performers are Timiş, Braşov, and Iaşi, each between 580 to 660 workers in IT per 100,000 inhabitants. The lowest-ranked 30 counties are home to less than 100 workers in the eligible sectors per 100,000 inhabitants.

C.4 Data Construction for Synthetic Control

C.4.1 Eurostat Data

Downloading the Data. The first step in this process was to download data from Eurostat, using the Structural Business Statistics: Annual Detailed Enterprise Statistics tables. The data were available to download in batches, after having selected the sectors/industries, countries, years, and variables of interest. These selections are detailed below:

- **Sectors**: Data were available for download at the one-, two-, three-, and four-digit sector/industry level. For the main analysis, we selected and downloaded data at the two-digit sector level due to high levels of missing data at the three- and four-digit level. According to the Rev 1.1 sector classification, there were 59 possible two-digit sectors for which we might like to have data. Our process enabled us to find corresponding data as early as 1999 for 48 of these sectors. Missing sectors include A01 ("Agriculture, hunting and related service activities"), A02 ("Forestry, logging and related service activities"), B05 ("Fishing, fish farming and related service activities"), L75 ("Public administration and defence; compulsory social security"), M80 ("Education"), N85 ("Health and social work"), O90 ("Sewage and refuse disposal, sanitation and similar activities"), O91 ("Activities of membership organizations n.e.c."), O92 ("Recreational, cultural and sporting activities"), O93 ("Other service activities"), and P95 ("Activities of households as employers of domestic staff"). For each two-digit sector, data were downloaded in two pieces: firstly as per the Rev 1.1 sector classification and secondly as per the Rev 2 sector classification. Since the transition from Rev 1.1 to Rev 2 in the year 2007 was not one-to-one, fractions of two-digit sectors were carefully linked across the two classifications according to the degree of mutually overlapping three-digit sectors between any pair of two-digit sectors. More detail on the construction of this crosswalk follows in the "Creation of Crosswalk between Rev 1.1 and Rev 2 Sector Codes" section.

- **Countries**: Data were downloaded for the following fourteen countries of interest: Bulgaria, Cyprus, the Czech Republic, Estonia, Hungary, Ireland, Latvia, Lithuania, Malta, Poland, Portugal, Romania, the Slovak Republic, and Slovenia. Data were widely available for most countries, with frequent missing values for Cyprus and Malta across the sectors, variables, and years of interest.

- **Variables**: Data were downloaded for the following five variables: Gross revenue (Turnover or gross premiums written) - million euro (V12110), Production value - million euro (V12120), Employees - number (V16130), Gross investment in tangible goods - million euro (V15110), and Enterprises - number (V11110).

- **Years**: For each sector, country, and year, relevant data were downloaded for as many years as possible, ranging from 1995 to 2016. There was low data availability prior to 1999 and after 2015 across the set of countries, industries, and variables of interest.

After having downloaded all available data for five variables from 1995 to 2016 across 14 countries and 48 sectors, there was a further selection of years, countries, and industries to include in the final analysis based on having a sufficiently high level of non-missing data to complete the analysis. These selections are detailed in the "Selection of Years, Countries, and Industries for Final Analysis" section.

Creation of Crosswalk between Rev 1.1 and Rev 2 Sector Codes. The SCM analysis compares pre-trends from before and after 2001, between Romania and synthetic Romania, for sectors with high- versus low-intensity of IT use. Since the Rev 1.1 sector classification was in effect for the treatment period of interest, our analysis seeks to classify sectors as either high- or low-intensity of IT use according to their Rev 1.1 sector classification. For this reason, the data downloaded as per Rev 2 sector classifications (for years 2007 onwards) needed to be stitched together with the data downloaded as per Rev 1.1 sector classifications (for years prior to 2007) according to a correspondence or crosswalk between the two sector classifications.

An extensive correspondence detailing equivalence between two-, three-, or four-digit sectors (as applicable and as dictated by the transition) between Rev 1.1 and Rev 2 was obtained from Eurostat and used for constructing a crosswalk that could link sectors across sector classifications at the two-digit level. The revision between Rev 1.1 and Rev 2 was in most cases not a one-to-one transition. For six of the 59 Rev 1.1 sectors, one two-digit Rev 2 sector corresponded to a single two-digit Rev 1.1 sector. Six of the 59 two-digit Rev 1.1 sectors were either split into two or more corresponding two-digit Rev 2 sectors or were part of a merge into one corresponding two-digit Rev 2 sector. The remaining 47 of 59 sectors were combinations of splits and merges, that is, some portion of the two-digit sectors as per Rev 1.1 were split into two or more sectors as per Rev 2, where they were merged with other portions of two-digit Rev 1.1 sectors that were similarly split among two or more Rev 2 sectors.

Two primary methods were used to create a crosswalk to link some fraction of each two-digit Rev 2 sector to a corresponding two-digit Rev 1.1 sector. In the first method ("gross revenue and count"), average gross revenue across all 14 countries of interest was used as a proxy for the size of each Rev 1.1 sector at the three-digit level. Data for this portion of the task was similarly downloaded from Eurostat, using the Structural Business Statistics: Annual Detailed Enterprise Statistics tables. This information on sector size was used to calculate what portion of a two-digit Rev 2 sector stemmed from distinct three-digit Rev 1.1 sectors, and in turn, to which two-digit Rev 1.1 sectors these three-digit sectors corresponded. Mechanically, this process involved calculating the average gross revenue in 2007 across all 14 countries of interest for each three-digit Rev 1.1 sector included in the correspondence table (in cases where the correspondence table indicated linkages at the four-digit level, these were aggregated up to the three-digit level so that all comparisons at this stage were made at the three-digit level). The size of corresponding three-digit sectors common to a pair of two-digit Rev 1.1 and Rev 2 sectors relative to the total size of the Rev 2 sector were used to assign fractions of that two-digit Rev 2 sector to corresponding Rev 1.1 sectors. To illustrate, suppose a particular two-digit Rev 2 sector was composed of five three-digit Rev 2 sectors that corresponded to five three-digit Rev 1.1 sectors, three of which corresponded to one two-digit Rev 1.1 sector (totaling average gross revenue of 200) and 2 of which corresponded to a second two-digit Rev 1.1 sector (totaling average gross revenue of 300). In this case, 200/500=40% of each variable value from the Rev 2 sector would be allocated to the first Rev 1.1 sector, and 300/500=60% of each variable value from the Rev 2 sector would be allocated to the second Rev 1.1 sector.

The second method, ("count only") used a similar approach. However, instead of calculating the fraction of each two-digit Rev 2 code to assign to each two-digit Rev 1.1 code using an estimate of the size of corresponding three-digit sectors, this method used the count of three-digit sectors common to a pair of two-digit Rev 1.1 and Rev 2 sectors relative to the total number of three-digit sectors that make up the two-digit Rev 2 sector to assign the fractions. In the same example as above, 3/5=60% of each variable value from the two-digit Rev 2 sector would be allocated to the first two-digit Rev 1.1 sector, and 2/5=40% of each variable value would be allocated to the second Rev 1.1 sector.

For the first method ("gross revenue and count"), in cases where gross revenue was not available in 2007 in order to make the necessary estimation of sector size, fractions were allocated based on the "count only" method.

The resulting versions of the crosswalk between the two sector classifications are similar, and there is no clear test that can be used to assess which of the two methods provides a more accurate representation of each Rev 1.1-equivalent sector from 1995 to 2016. Visual inspection of trend lines (focusing in particular on the gross revenue variable for Romania over time) seems to suggest that the "count only" method leads to a smoother correspondence across Rev 1.1 and Rev 2. While there remain some jumps in the times series according to both correspondences (which could reflect the similarity in timing of the transition from Rev 1.1 to Rev 2 along with the global financial crisis), the "count only" method appears to minimize these jumps. For this reason, the "gross revenue and count" method was disregarded in favor of moving forward with the "count only" method.

Data Cleaning. After downloading data for as many countries, variables, years, and sectors as possible and constructing a crosswalk to link the data across Rev 1.1 and Rev 2 sector classifications, the next step was to combine the data, clean the data, and select years, countries, and industries to include in the final analysis.

Data that was downloaded according to the Rev 2 sector classification (for 2007 onward) was split fractionally to corresponding Rev 1.1 sectors as per the crosswalk and combined with the data that was downloaded according to the Rev 1.1 sector classification (for prior to 2007). In cases where data was available under both classifications for overlapping years (as was often the case for 2005, 2006, and 2007), the data from the Rev 1.1 classification was prioritized, then filled in with data from the Rev 2 classification when the Rev 1.1 data was missing.

Selection of Years, Countries, and Industries for Final Analysis. Years, countries, and industries to include in the final analysis were selected to maximize availability of data and minimize the incidence of missing observations in the resulting panel.

Less than 70% of the data across all countries and all industries were available for years prior to 1999 and after 2015, so only the years from 1999 to 2015 inclusive were retained for the analysis sample. (Note that the 70% cutoff applies to all countries and all sectors; this percentage would be higher once restricting to the final set of countries and sectors retained for analysis.)

Similarly, Malta and Cyprus were dropped from the analysis sample due to low data availability. The remaining 12 countries retained featured relatively high data availability: at least 75% of the desired data were non-missing for the 12 countries retained across all years, and at least 85% of the desired data were non-missing when restricting attention to the years 1999-2015. (Again, these percentages are for data across all sectors and would be larger once additionally restricting to the final sectors chosen for their sufficient level of data availability.)

Finally, because of the importance of estimating pre-trends for Romania, sectors were dropped if data were missing for any of the seven variables in either 1999 or 2000 for Romania. This requirement eliminated 14 sectors from the analysis sample (C10, C11, C12, C13, C14, G51, G52, I60, I61, I62, I64, J65, J66, and K67). An additional four sectors were dropped for reasons of low data availability: considering only 1999-2015 for all seven variables and the 12 retained countries, these were four industries for which more than 10% of the desired data was missing (D15, D16, D19, and D23).

After restricting the analysis sample to the years 1999-2015, dropping Cyprus and Malta, and restricting to the 48 sectors where data was available for all seven variables for Romania in 1999 and 2000 and where there was a sufficiently high level of non-missing data, remaining missing values were imputed as follows. In cases where there was a missing observation for a particular variable within a time series, the missing observation's value was estimated by a simple average of the value for the most recently available and next available year, including for cases where the gaps were greater than one year. For cases where 1999 and potentially several consecutive years were missing, all of those observations were set to match the value for the earliest available year. Similarly, in the rare situation where there were potentially several consecutive years missing data ending in 2015, all of those variable's values were set to match that variable's value for the latest available year.

The ultimate level of imputation of missing values required was minimal, ranging from 0.8% to 1.9% of desired year by country by sector observations across the seven variables.

C.4.2 UN Comtrade Data

Downloading the Data. For for the exports of goods and services, the data was downloaded from the UN Comtrade official website. We downloaded the data for the same set of twelve countries that constitute the pool of donor countries for the SCM exercise using Eurostat data: Bulgaria, the Czech Republic, Estonia, Hungary, Ireland, Latvia, Lithuania, Poland, Portugal, Romania, the Slovak Republic, and Slovenia.

For goods exports, we downloaded the data disaggregated by two-digit SITC Rev 1 commodity codes. For service exports, we downloaded the data disaggregated by two-digit (X.X) EBOPS2002 codes (except Financial Services which is just classified as 6).[10] The variable of interest was Trade Value (US$) for Trade Flow=Exports.

Data Cleaning and Selection of Years and Codes for Final Analysis. The dataset for goods exports had the following features:

- There was a high rate of missing data for BG in 1995 and HU, PT in 2015
- We had to remove some codes to make sure that the codes entering the aggregates (high- versus low-intensity of IT inputs use) are consistent across countries:
  - 34. Missing: BG (1995, 1998-2000, 2003), LV (1998), RO (1996-1997), SK (2005)
  - 35. Missing: BG (1995-2006), HU (1995-1996, 2001-2002), IE (2010-2011), PT (1997-1998, 2015), RO (1996), SK (1995-1996, 2005)
  - 41. Missing: RO (2003)

[10]EBOPS2002 includes the standard items, memorandum items, and supplementary items. Only standard items were used. For codebook, visit link.

- 52. Missing: HU (1998-1999, 2006, 2009, 2015), IE (2001, 2003, 2005), LV (2009), SK (2007)
- 93. Missing: CZ (1996-2001, 2003) , EE (1995-1998, 2002), HU (1995-1996, 2015), IE (1996), PL (1996-1997), RO (2006), SK (1995-2003)
- 95. Missing: BG (1995, 1997-2001, 2003, 2007-2013), HU (1995, 2003, 2009, 2015), IE (1997, 1998, 2000), RO (1998-2005), SI (1996, 1999 -)

Removing the above sectors results in a (close to) consistent panel for 1996-2014. Note that in 2008, SK does not have sector 12.

The download of the service export data allowed us to notice that the data is available only from 2000 to 2016. Even during these years, most countries present a lot of missing values across codes. In particular, for Romania the treated NACE Rev 1 sector (72) starts reporting data only in 2005, not allowing us to study the service exports of this sector. The resulting (unbalanced) panel does not allow for an SCM, as we only observe one year before the event in 2001. Moreover, the large extent of missing values delivers noisy estimates of treatment effects.

Creation of Crosswalk between EBOPS2002 Codes and NACE Rev 1.1 Sector Codes. Our categories of high- and low-intensity (of IT usage) sectors were constructed based on Romania's input-output Table for 2000, which is at the two-digit NACE Rev 1 sector level. Hence, we had to construct a mapping between two-digit SITC Rev. 1 commodity codes and two-digit (X.X) EBOPS2002 service codes and the two-digit NACE Rev 1 codes. After commodity or service codes were mapped to NACE Rev 1 codes, we aggregated export values at the NACE Rev 1 level.

C.5 Context on Policies Relevant to the IT Sector in Romania

C.5.1 Details on Romania's Income Tax Break for Workers in IT

Order Number	4079/ 268/ 1480/ 2001	661/ 444/ 2196/ 2001	250/ 189/ 748/ 2004	539/ 225/ 1479/ 2013	217/ 4172/ 1348/ 835/ 2015	872/ 5932/ 2284/ 2903/ 2016
Effective date	08/05/2001	11/22/2001	06/29/2004	09/10/2013	07/30/2015	01/09/2017
CAEN Code	7220 (Rev 1)	7220 (Rev 1)	**7221, 7222 (Rev 1.1)**	**5821, 5829, 6201, 6202, 6209 (Rev 2)**	5821, 5829, 6201, 6202, 6209 (Rev 2)	5821, 5829, 6201, 6202, 6209 (Rev 2)
Occupa-tions	Analyst,	Analyst,	Analyst,	Analyst,	Analyst,	Analyst,
	Programmer,	Programmer,	Programmer,	Programmer,	Programmer,	Programmer,
	Computer Systems Designer, Engineer or Programmer of IT Systems, Database Manager,	Computer Systems Designer, Engineer or Programmer of IT Systems, Database Manager,	Computer Systems Designer, Engineer or Programmer of IT Systems, Database Manager,	Computer Systems Designer, Engineer or Programmer of IT Systems, Database Manager,	Computer Systems Designer, Engineer or Programmer of IT Systems, Database Manager,	Computer Systems Designer, Engineer or Programmer of IT Systems, Database Manager,
	Software Engineer and Manager of IT Projects	Software Engineer and Manager of IT Projects	Software Engineer and Manager of IT Projects	Software Engineer and Manager of IT Projects	Software Engineer and Manager of IT Projects	Software Engineer and Manager of IT Projects
					Computer System Programmer	**Computer System Programmer**
Unit within the Firm	Unit Specialized in IT	Unit Specialized in IT	Unit Specialized in IT	Unit Specialized in IT	Unit Specialized in IT	Unit Specialized in IT

Figure C.14: 2001 Income Tax Break and Its Subsequent Amendments: Eligibility Criteria for the Income Tax Break (1/2)

Extra Eligibility Criteria for the Income Tax Break, in Addition to Those from Table C.14 (2/2)

Order Number: 4079/ 268/ 1480/ 2001
Eligible Major during Higher Education for Exempted Worker: Automation, Computers, Computer Science, Cybernetics, Mathematics, Electronics.
Minimum Annual Revenue from Software Development: Annual income of at least 10,000 U.S. dollars per employee benefiting from the income tax break.
Balance for Software Development Income: A balance is required, in which the income from software development needs to be explicitly reported.

Order Number: 661/444/2196/2001
Eligible Major during Higher Education for Exempted Worker: Automation and Industrial Computer Science; Computers, Electrical Engineering and Computers; Electronics; Applied Electronics, Electronics and Telecommunications, Communications; Mathematics, Mathematical Computer Science; Computer Science, Computer Science and Economics, Applied Computer Science, Cybernetics and Computer Science and Economics, Cybernetics and Economic Prediction, Accounting and Computer Science and Management.
Minimum Annual Revenue from Software Development: Annual income, in preceding year, of at least 10,000 U.S. dollars per employee benefiting in a given year from the income tax break.
Balance for Software Development Income: A balance is required, in which the income from software development needs to be explicitly reported.

Order Number: 250/189/748/2004
Eligible Major during Higher Education for Exempted Worker: Automation and Industrial Computer Science; Computers, Electrical Engineering and Computers; Electronics; Applied Electronics, Electronics and Telecommunications, Communications; Mathematics, Mathematical Computer Science; Computer Science, Computer Science and Economics, Applied Computer Science, Cybernetics and Computer Science and Economics, Cybernetics and Economic Prediction, Accounting and Computer Science and Management.
Minimum Annual Revenue from Software Development: Annual income, in preceding year, of at least 10,000 U.S. dollars per employee benefiting in a given year from the income tax break.
Balance for Software Development Income: A balance is required, in which the income from software development needs to be explicitly reported.

Order Number: 539/225/1479/2013
Eligible Major during Higher Education for Exempted Worker: Automation and Industrial Computer Science; Computers, Electrical Engineering and Computers; Electronics; Applied Electronics, Electronics and Telecommunications, Communications; Mathematics,

Mathematical Computer Science; Computer Science, Computer Science and Economics, Applied Computer Science, Cybernetics and Computer Science and Economics, Cybernetics and Economic Prediction, Accounting and Computer Science and Management. **Newly eligible majors:** Industrial Computer Science, Applied Computer Science in Electrical Engineering, Applied Computer Science in Material Engineering, Mathematics and Applied Computer Science in Engineering; Cybernetics and Economics; Physics and Computer Science; Chemistry and Computer Science; Automation and Applied Computer Science, Equipment for Modeling, Simulation and Computerized Warfare, Engineering of Multimedia Systems; Technologies and Telecommunication Systems, Remote Controls and Electronics in Transportation; Transmissions and Military Electronic Equipment.
Minimum Annual Revenue from Software Development: Annual income, in preceding year, of at least 10,000 U.S. dollars per employee benefiting in a given year from the income tax break.
Balance for Software Development Income: A balance is required, in which the income from software development needs to be explicitly reported.

Order Number: 217/4172/1348/835/2015 and 872/5932/2284/2903/2016
Eligible Major during Higher Education for Exempted Worker: A diploma issued after a form of higher education, irrespective of major.
Minimum Annual Revenue from Software Development: Annual income, in preceding year, of at least 10,000 U.S. dollars per employee benefiting in a given year from the income tax break.
Balance for Software Development Income: A balance is required, in which the income from software development needs to be explicitly reported. **New:** New firms or firms undergoing a restructuring during that fiscal year are exempted from this requirement.

NACE Rev 1.1	Description Rev 1.1	NACE Rev 2	Description Rev. 2	Comments
7221	Publishing of software	5821	Publishing of computer games	Publishing of computer games
7221	Publishing of software	5829	Other software publishing	All software publishing, except computer games publishing
7221	Publishing of software	6201	Computer programming activities	Software programming
7222	Other software consultancy and supply	6201	Computer programming activities	Includes:
				Analysis, design and programming of systems ready to use: development, production, supply and documentation of made-to-order software based on orders from specific users writing of programs following directives of the user web page design
7222	Other software consultancy and supply	6202	Computer consultancy activities	Analysis, design and programming of systems ready to use: - analysis of the user's needs and problems, consultancy on the best solution
7222	Other software consultancy and supply	6209	Other information technology and computer service activities	Software installation services

Figure C.15: Correspondence Table between NACE Sector Codes Rev 1.1 and Rev 2

Notes: Source Eurostat.

NACE Rev 2	Description Rev 2	NACE Rev 1.1	Description Rev 1.1	Comments
5821	Publishing of computer games	7221	Publishing of software	Publishing of computer games
5821	Publishing of computer games	724	Database activities	On-line computer games publishing
5829	Other software publishing	7221	Publishing of software	All software publishing, except computer games publishing
5829	Other software publishing	724	Database activities	All on-line software publishing, except computer games on-line publishing
6201	Computer programming activities	7221	Publishing of software	Software programming
6201	Computer programming activities	7222	Other software consultancy and supply	Includes: Analysis, design and programming of systems ready to use: - development, production, supply and documentation of made-to-order software based on orders from specific users - writing of programs following directives of the user - web page design
6201	Computer programming activities	724	Database activities	Designing of structure and content of database
6202	Computer consultancy activities	721	Hardware consultancy	All
6202	Computer consultancy activities	7222	Other software consultancy and supply	Analysis, design and programming of systems ready to use: - analysis of the user's needs and problems, consultancy on the best solution
6209	Other information technology and computer service activities	3002	Manufacture of computers and other information processing equipment	Installation of personal computers and peripheral equipment
6209	Other information technology and computer service activities	7222	Other software consultancy and supply	Software installation services
6209	Other information technology and computer service activities	726	Other computer related activities	NACE 1.1 class 72.60 was an ”empty class”.

Figure C.16: Correspondence Table between NACE Sector Codes Rev 2 and Rev 1.1

Notes: Source Eurostat.

C.5.2 Other Policies Relevant for the IT Sector

State Aid Program

Between 2011 and 2016, several state aid programs that supported job creation and investment were implemented.[11] The most relevant for the sectors studied was the program created by Government Decision 797/2012. It supported large investments in new technologies with an IT component and job creation of at least of 200 new jobs. While firms in most manufacturing, energy and service sectors were eligible, mainly firms in high-tech knowledge intensive sectors benefited from it. We drop from our sample of analysis firms that have benefited from such State Aid. The program created through Government decision 332/2014, which aimed to support large investments, job creation, and regional development, also benefited several firms in high-tech knowledge-intensive services. Firms in high-tech knowledge-intensive sectors were also eligible for several smaller programs supporting SMEs and start-ups, such as "Start-up Nation," but these programs were smaller and less likely to affect major investments or job creation (e.g., "Start-up Nation" had an upper limit of approximately 44,000 euros).

Other Tax Exemptions

Programmers are not the only category of workers exempted from the personal income tax in Romania. Two other categories of workers exempted from the income tax could be employed by IT firms, without being programmers: workers with serious disabilities[12] and since 2016 workers in research and development (defined broadly, with no requirement to work in software development).[13] For companies with at least 50 employees, it was compulsory that at least 4% of their workers have disabilities. When firms could not comply, they had to pay a given amount to support the inclusion of people with disabilities.

Despite these other exemptions, the vast majority of exempted employees in eligible sectors (5821, 5829, 6201, 6202 and 6209) were exempted due to the tax break for workers in IT. In October 2017,[14] workers benefiting from the law under study in this article represented 96% of the exempted employees in eligible sectors.

[11] Government decisions 797/2012, 322/2014 and 807/2014.
[12] See Law 448/2006 for details.
[13] See Order 4947/899/2018/1840/906/2016 published in September 2016 for details.
[14] In 2017 we can observe the reason of the income tax exemption, which we cannot observe in previous years.

Appendix D

Survey Data Collection

D.1 Surveys for “The Effects of Joining Multinational Supply Chains"

D.1.1 Survey Design and Implementation

We targeted with surveys the domestic firms in three groups. First, we targeted a 20% random sample of the 3,813 domestic firms experiencing an event in the economy-wide sample (3,813 firms that experienced a first-time supplying event with an MNC between 2010 to 2015), that is, 762 domestic firms. Second, we targeted *all* the winning firms in the “winner vs. losers" Procomer sample (31 firms). Last, we targeted *all other* domestic firms that started supplying to MNCs through Procomer (385-31=354 firms). It was essential to include the first sample, as it is the one generating our baseline results. The second sample is the basis of one of our main robustness checks. Most of the firms in the last sample are experienced suppliers and can bring a long-term perspective on their relationships to MNCs. In addition to the domestic firms in these three groups, we also targeted *all* the MNCs that served as first MNC buyers to these domestic firms (471, 53, and 163 respectively).[1]

Surveys had two core objectives: inquire on specific threats to identification and shed light on features of linkages between MNCs and their new suppliers that are unobservable in administrative data. We designed four surveys: two for domestic firms and two for MNCs. For each type of firm (domestic or MNC), we wrote a short and a long version of the survey. The *short version* of the survey focuses only on the core topics. The *long version* requests more details on the core topics, in addition to more information useful for context.

[1]These three sets of MNCs are overlapping as the same MNC can trigger events of the three types: economy-wide (unmediated), mediated by Procomer after 2009 and in our sample of analysis, or mediated by Procomer in any year and not part of our sample of analysis. Note also that some MNCs trigger events for more than one supplier; that explains why the number of MNCs triggering events can be smaller than the number of domestic firms experiencing the events. That said, it can also be that some suppliers sell to more than one MNC in the first year in which they sell to at least one MNC (the year of the event); that explains why the number of MNCs triggering events can also be larger than the number of domestic firms experiencing the events.

The co-authors of this project designed the survey instruments. BCCR, Procomer, and CINDE provided feedback that improved the initial drafts.[2] We first wrote the questionnaires in English. Once we refined the order, structure, and wording of questions, a native Spanish speaker translated the questionnaires. We only conducted one round of surveys, all of which took place between June and September of 2018.

Long version. Long surveys were conducted in person and lasted 45 minutes to an hour. Procomer or CINDE established the first contact with firms by email. The email contained an official letter from BCCR describing the study and guaranteeing a fully-secured treatment of the data collected. Once a firm agreed to participate, our team would be granted permission to contact the firm directly in order to set up the survey meeting.[3]

We decided to apply the long version of the survey to the firms involved in the “winner vs. losers" design, that is to the 31 domestic suppliers experiencing the eligible Procomer events and the MNC buyers that triggered those events. This choice has two advantages. First, these are firms for which we had more reliable contacts (from either Procomer or CINDE); this improved the chance of a positive response to our request. Second, all of these firms had other deals (with domestic suppliers/MNCs) that were not mediated by Procomer. Applying the long version of the survey to these firms allowed us to inquire whether deals mediated by Procomer were different or not from unmediated deals.

The first in-person surveys served as the pilot, allowing the team to test not only the questionnaire, but also the survey protocols and logistics. For this reason, at least one of the co-authors joined these first meetings. Once this piloting phase ended, a team of two enumerators split the remaining in-person surveys among themselves. In the summer of 2018, both enumerators were in their final year of undergraduate studies in economics at the main national university. Enumerators went unaccompanied to their meetings, to avoid any risk of answers being influenced by either a Government official or our team.

The team agreed with BCCR, CINDE, and Procomer to share only the aggregated findings of the surveys. Enumerators made sure that firms knew that their specific answers were not to be shared with these public entities. This measure was meant to create an environment of trust and elicit truthful responses. Also, as almost all questions did not refer to the “Productive Linkages" program but focused on MNC-supplier relationships more broadly, enumerators clarified that surveys were not meant for program evaluation.

Short version. Short surveys were designed to be filled in online through a Google Form and take 15 to 20 minutes. The person filling in the survey would do so in the absence of any Government official or team member. In the invitation email, we included an official phone number and email address, in case the firm had any inquiries. We received few inquiries - of those, most were concerned whether the survey was legitimate or an imposture.

The invitation to participate in the online survey was sent to the firms that we targeted

[2] All three entities frequently survey firms in Costa Rica.

[3] Procomer contacted domestic suppliers and MNCs part of their “Productive Linkages" database. CINDE contacted MNCs under the Free Trade Zone regime. Unless a firm agreed to participate in the survey, the email address of their contact was not revealed to our team.

from the economy-wide sample of events (762 domestic firms and 471 MNCs) and to the firms involved in Procomer events that are not part of our sample of analysis (354 domestic firms and 163 MNCs).[4]

Depending on the firm, the invitation was sent by Procomer, CINDE, or BCCR. Procomer and CINDE had readily-available email addresses of specific key employees in each firm. As Procomer and CINDE contacted firms in their portfolio, this also reassured firms on the intention of the survey. Both factors significantly increased the likelihood of an answer.

BCCR contacted firms in the economy-wide restricted sample. Our team had to search for appropriate contacts from scratch. This step was the most challenging in the implementation of the surveys. Whenever firms could be found online with more than a phone number and a physical address, the most direct contact available was either a general email address (e.g., info@firm.cr) or a contact form on the website. To increase the likelihood of an answer, the two enumerators made calls to all firms with a phone number, describing the survey and requesting a direct email address of the person most qualified to answer the survey. Despite calls being made from an official BCCR number, many firms distrusted the calls and refused to share a personal email address.

We made up to six attempts to contact each firm. Depending on the available/preferred mode of contact, these attempts were either callbacks or email reminders. An unexpected challenge for the short survey came from the fact that certain corporate anti-virus software directed our email to the spam folder of the recipient, as it contained the link to the survey. Recipients were also advised against clicking on the link, to avoid phishing or malware downloads. Receiving the email from an official email address was not sufficient reassurance for some firms. One goal behind our persistent attempts was to bring reassurance on the safety of participating in the survey. It is important to emphasize that surveys to both MNCs and domestic suppliers required specific knowledge about relationships between MNCs and domestic suppliers. Our ideal respondent was the employee whose job attributes and tenure with the firm allowed him/her to provide the most accurate answers. Questions to MNCs did not require the respondent to witness the first linkage to a specific domestic supplier. However the respondent had to be well-informed on the local procurement practices of the MNC. For this reason, we aimed to survey the supply chain (procurement, operations) manager of each MNC.

For domestic suppliers, part of the questions was retrospective. This required from the respondent to have worked at the firm before and during the first deals with MNCs. Given this constraint and the fact that most firms are small family-owned businesses, the ideal respondent was the founder of the firm (who is typically the general manager as well). The retrospective nature of the survey to domestic suppliers is unlikely to have jeopardized answer quality for two reasons. First, most questions did not ask for specific details on the first deal with an MNC, details which might otherwise be affected by the time lag. Second, survey

[4] Again, note that while the sets of domestic firms in these different samples are disjoint, the sets of MNCs triggering the events are not.

answers show that the first deals with MNCs were transformative for the domestic firm. Thus, it is unlikely for the firm founder to misremember the circumstances of those deals.

We went to great lengths to identify the most suitable respondent inside each firm and make sure this person actually answered the survey. The supply chain manager of the MNC and the owner of the domestic firm are typically busy and inaccessible. Most firms do not even publicize the names of people in these positions, as to avoid their being pursued with unsolicited business proposals. It took considerable effort to ensure that our survey was known to and answered by the right person within each firm.

D.1.2 Survey Response Rate and Representatives

In Table D.1 we report the number of firm responses to our four surveys: the two versions of the survey to domestic firms (the long and the short) and the two versions of the survey to MNCs (again, the long and the short).

Number of responses	Long survey	Short survey	Total
Domestic	15	91	106
MNCs	23	35	58
Total	38	126	**164**

Table D.1: Number of Firm Responses

Notes: This table summarizes the number of survey responses by survey version (long or short) and target (domestic supplier or MNC). Out of a total of 164 completed surveys, 38 were completed in person and 126 online. Out of the same total of 164 completed surveys, domestic suppliers filled in 106 and MNCs filled in 58.

Response rate for MNCs. These 58 MNCs have triggered a total of 645 (distinct) events out of our economy-wide sample of 3,813 events (or 17%). These 58 MNCs include 51 of the 471 MNCs triggering these 3,813 events (or 11%). For the Procomer sample of analysis, these 58 MNCs cover 21 of the 31 events of interest (or 68%) and include 21 of the 53 MNCs triggering these 31 events (or 40%). When we focus on Procomer events other than those in the sample of analysis, 32 of these 58 MNCs trigger 122 events of a total of 354 (other) Procomer events (or 34%). As a percentage of the number of MNCs having (other) deals mediated by Procomer, these 32 MNCs represent 20% (of a total of 163 MNCs).

Recall that the same MNC can trigger events in all three samples. Overall these 58 responses from MNCs trigger 788 (788=645+21+122) events or 19% of the 4,198 events targeted (4,198=3,813+31+354) and 11% of the 527 distinct MNCs targeted (the union of 471, 53, and 163 MNCs).

Response rate for domestic firms. Of the 106 domestic firms answering the survey, 34 are part of the economy-wide sample, 12 are part of the Procomer sample of analysis, and the remaining 60 are part of the Procomer sample of suppliers not keep for analysis.

Out of the 762 targeted domestic firms and their associated economy-wide events, we have a response rate of 4%.[5] If we refer to the overall sample of 3,813 domestic firms and their associated economy-wide events, we have a response rate of 1%. Note, however that only 762 of these 3,813 firms were actually contacted. Of the targeted 31 domestic firms and their associated winning events in the Procomer sample of analysis, our 12 responses cover 39%. When we focus on Procomer suppliers other than those in the sample of analysis, the 60 surveyed suppliers represent 17% of the total of 354 targeted suppliers (or events).

Overall, the 106 responses from domestic firms cover 9% of the total of 1,147 domestic firms (events) targeted (1,147=762+31+354).

Combined response rate. The combined response rate is defined as the percentage of events on which we have a survey response from either the domestic firm experiencing the event or the MNC triggering that event.

Of the 3,813 events that create our economy-wide sample, we have information on 650 events, or 17%. Of the 31 events in the Procomer sample of analysis, we have responses from either the supplier or the MNC buyer for 24 events, that is, 77% of events. Of the 354 events mediated by Procomer but not in the sample of analysis, we have responses from either the supplier or the MNC buyer for 160 events, that is, for 45% of events.

Of the total 4,198 the events (4,198=3,813+31+354) of interest, we have information from either the supplier or the MNC buyer for 834 (834=650+24+160) events, that is, for 20% of events.

Table D.2 summarizes the statistics just discussed. Three patterns stand out. First, comparing column (1) to columns (2) and (3) one notices the higher response rates achieved for firms in the Procomer database, relative to the firms in the economy-wide sample whose contacts we searched for ourselves online. This is due to the higher quality of the contacts in the Procomer database. Second, we have achieved significantly higher response rates for face-to-face surveys than for online surveys. This is due to a certain distrust of survey invitations sent by email and to be filled in by clicking on a link (that the receiver fears to be a virus). Third, when one allows for an event to be described by either the domestic supplier experiencing the event or by the MNC triggering it, we reach a higher overall coverage of events.

While the response rate might appear low (particularly for the online surveys to domestic firms in the economy-wide sample), one should consider the following factors. Business surveys are often challenged with low response rates. Whenever businesses are not mandated to take part in a survey, they often refuse to disclose proprietary information. The type of firms targeted by our surveys are either MNCs (hence firms with strict confidentiality rules) or domestic firms (of which, many preoccupied about revealing their trade secrets or suspicious over being contacted by email). Our survey was also not incentivized. Given the type of firms we targeted, it was unfeasible to provide a financially-meaningful incentive. Last, it was essential to the success of our survey for it to be filled in by the appropriate

[5] When it comes to domestic firms, percentages out of number of domestic firms or events are identical as each domestic firm is mapped one-to-one to an event.

Sample	(1) Economy-Wide	(2) Procomer Sample	(3) Procomer Other	(4) All Samples
Version	Online	Face-to-face	Online	
Domestic (% targeted firms)	4%	39%	17%	9%
Domestic (% targeted events)	4%	39%	17%	9%
MNCs (% all firms)	11%	40%	20%	11%
MNCs (% all events)	17%	68%	34%	19%
Combined (% all events)	17%	77%	45%	20%

Table D.2: Summary of Firm Response Rates

Notes: This table summarizes the survey response rates by firm type (domestic supplier or MNC), as a percentage of either the relevant number of firms or events, and with respect to three firms/events samples (firms/events targeted and contacted of all the economy-wide sample, all firms/events in the economy-wide sample – targeted or not –, all firms/events in the Procomer sample of analysis, all other firms/events in the Procomer set of deals, not part of the sample of analysis). Note that all MNCs from the economy-wide sample and all firms/events in the Procomer set of deals were targeted and contacted. The only firms for which only a 20% sample was targeted and contacted were the domestic firms experiencing economy-wide events.

person within each firm. This factor was an important constraint to us, as it was generally difficult to reach these firms and particularly so, to reach key employees.

Representativeness of domestic firm respondents. In Table D.3 we compare the 106 domestic firms that have participated in our survey to the 4,092 domestic firms of interest who have not participated. Recall that most of these 4,092 non-respondents have not been actually contacted, as we have only contacted a 20% random sample of the 3,813 domestic firms experiencing economy-wide events. We pool across firms coming from the three samples (economy-wide events, Procomer events in the sample of analysis, and Procomer events not in the sample), but the same patterns apply to comparisons of surveyed vs. not surveyed firms in the same sample. It is only for brevity that we show the pooled comparison alone.

From Table D.3 we learn that the differences in firm size and firm performance between surveyed and non-surveyed domestic firms are not statistically significant. It is reasonable to expect that the answers of the responding domestic firms are representative for the overall samples of interest.

Table D.3: Comparison Between Surveyed and Not Surveyed Domestic Firms in Terms of Firm Size and Firm Performance

	Surveyed	Not surveyed	Difference
Number of Workers	23.28	23.58	-0.304
	(26.48)	(54.75)	(6.67)
Total Sales	2.241	1.773	0.467
	(3.86)	(4.57)	(0.56)
Value Added Per Worker	13.08	13.28	-0.200
	(11.11)	(62.36)	(7.57)

Notes: Table D.3 compares the domestic firms who have participated in our survey to the domestic firms who have not in terms of their number of workers and total sales in 2009. The total sales are in millions of CPI-deflated 2013 U.S. dollars. The value added per worker is in thousands of CPI-deflated 2013 U.S. dollars. Standard deviations in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

Representativeness of MNC respondents. In Table D.4 we compare the 58 responding MNCs (who have accepted our survey invitation) to the remaining 469 MNCs who we have invited to participate in our survey, but who have either declined or have not replied to our request (typically because the email address was incorrect or because it was a generic email address). We pool surveyed vs. not surveyed MNCs across the three samples (economy-wide events, Procomer events in the sample of analysis, and Procomer events not in the sample), but the same patterns apply to comparisons of surveyed vs. non-surveyed MNCs in the same sample. It is for brevity that we report the pooled comparison alone. Pooling is particularly inconsequential for MNCs as the same MNC can be part of all three samples (i.e., triggering events for domestic firms in the three samples).

Table D.4 shows that surveyed MNCs have, on average, higher total sales than non-surveyed MNCs and are more likely to be part of Free Trade Zones. While they also seem to hire more workers and have a higher value added per worker, these two differences are not statistically significant. These findings reflect the fact that our most reliable contacts of MNCs came from CINDE and Procomer, who work closely with MNCs in Free Trade Zones. MNCs in Free Trade Zones tend to be larger and more sophisticated. Given our topics of interest, it is unclear how this affects the representativeness of their answers. Last, by comparing Tables A.22 and D.13 we notice that the countries of global ultimate ownership of the MNCs are similar between those of all the MNCs triggering events economy-wide and the surveyed MNCs.

Table D.4: Comparison Between Surveyed and Not Surveyed MNCs in Terms of Size, Performance, and Free Trade Zone Status

	Surveyed	Not surveyed	Difference
Number of Workers	561.4	408.2	153.2
	(874.28)	(923.49)	(131.26)
Total Sales	108.4	43.35	65.01***
	(280.76)	(76.15)	(16.75)
Value Added Per Worker	74.75	47.83	26.93
	(131.98)	(166.10)	(23.26)
Free Trade Zone	0.564	0.408	0.156*
	(0.50)	(0.49)	(0.07)

Notes: Table D.4 compares the MNCs who have participated in our survey to the MNCs who have not in terms of their number of workers, total sales, value added per worker, and Free Trade Zone status (1 if the MNC is part of the Free Trade Zone regime), all averaged across all years of activity in Costa Rica. The total sales are in millions of CPI-deflated 2013 U.S. dollars. The value added per worker is in thousands of CPI-deflated 2013 U.S. dollars. Standard deviations in parentheses. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively.

D.1.3 Survey Questions and Answers

Two features of our survey structure deserve mentioning. First, for a given type of survey (to domestic suppliers or to MNCs), questions in the long version are a strict superset of questions in the short version. The overlapped questions are identical between the two versions (no change in wording, no change in the order of proposed answers). This allows us to pool answers from the long and short versions. Second, across the two survey types, some key questions are mirrored. For instance, both domestic suppliers and MNC are asked about the potential help provided by MNCs to first time suppliers. This allows to learn about the same topic from both perspectives.

Before analyzing the answers, we had to standardize the responses to open ended questions and perform some minimal quality checks on answers provided. One example of a quality check relates to the compatibility between a given question asked and the answer provided. E.g., one question asks MNCs about what they believe to be the most important benefit to domestic firms upon becoming their suppliers. Two MNCs provided answers that refer to the most important benefit *to the MNC* when having more domestic suppliers and had to be discarded. Another quality check makes sure that answer provided in the "Other: _____" option was not actually already covered by existing options that were not selected.

In what follows, we pool answers across sample sources. We do so because answers did not differ substantively among domestic firms/MNCs coming from different samples.

Survey Answers from Domestic Firms

Position	Frequency	Percent
CEO/President/Founder	58	54.7
Sales/Marketing/Client Outreach Manager	15	14.2
Other Unit Manager	11	10.4
Operations/Supply Chain Manager	9	8.5
Professional/Analyst	5	4.7
Assistant to CEO/President/Founder	4	3.8
Senior Partner	4	3.8
Total	N=106	100.0

Table D.5: Summary of Job Titles for Respondents to the Survey to Domestic Firms

Notes: This table summarizes the job titles (positions) of respondents to the survey to domestic firms. We have grouped job titles under seven categories. Under “CEO/President/Founder," one can find job titles such as Owner (“Dueño"), President (“Presidente"), or General Manager (“Gerente General"). Under “Sales/Marketing/Client Outreach Manager," one can find job titles such as Commercial Director/Manager (“Gerente/Director Comercial") or (“Gerente Mercadeo y Ventas"). Under “Other Unit Manager," one can find job titles such as Finance Director (“Directora Financiera"), R&D Manager (“Gerente de Investigación y Desarrollo"), or Accounting Supervisor (“Supervisor de Contabilidad"). Under “Operations/Supply Chain Manager," one can find job titles such as Operations Director (‘Directora de Operaciones‘") or Logistics Manager (“Jefe de Logistica"). Under “Professional/Analyst," one can find job titles such as Technical Advisor (“Asesor Técnico") or Business and Operations Analyst (“Analista de Negocios y Operaciones"). Under “Assistant to CEO/President/Founder," one can find job titles such as Assistant to General Manager (“Asistente de Gerencia/Asistente de Gerencia General"). Under “Senior Partner," one can find job titles such as Partner (“Socio") or Managing Partner (“Socio Director").

Question 1: “Your position in the firm." Question type: open-ended. Survey version: both long and short (N=106). Responses are summarized in Table D.5.

Question 2: “Did your firm expect multinational buyers to be different from domestic buyers?" Question type: Dichotomous. Survey version: only long (N=15).

100% of answers were positive (“Yes, our firm expected the contracts with multinational buyers to be markedly different from those with domestic buyers.") Please note that we emphasized that the question referred to expectations of the firm *before* the first contract with an MNC.

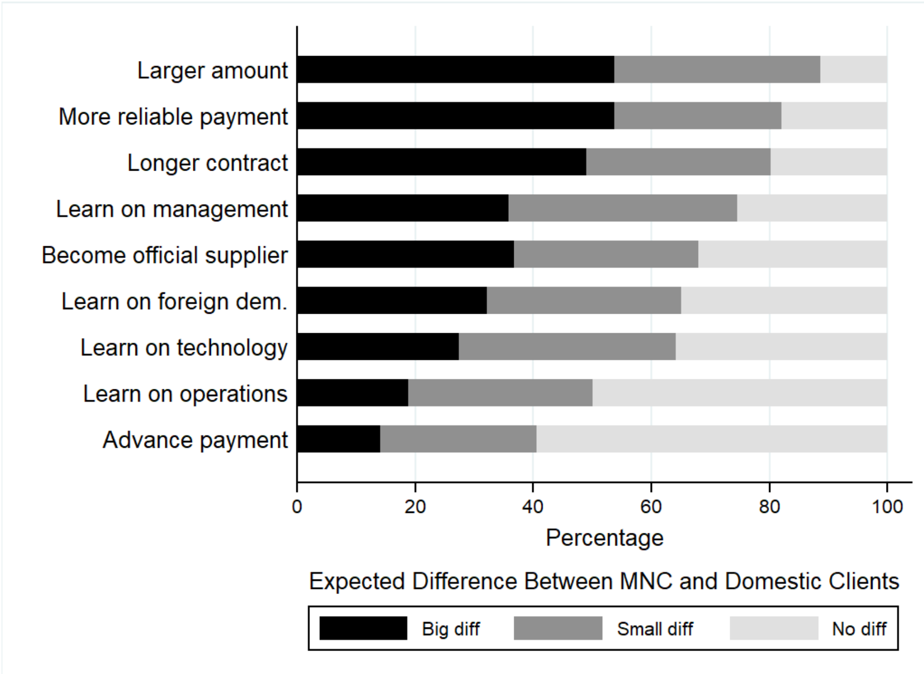


Figure D.1: Question 3: Before the first contract with an MNC, how did your firm expect MNCs buyers to be different from domestic buyers?

Notes: This graph summarizes the answers of 106 domestic firms to the survey question ""Before the first contract with an MNC, how did your firm expect MNCs buyers to be different from domestic buyers?" Percentages do not need to sum up to 100 across options, as each firm had to rate the extent to which each proposed option applied to the firm. Percentages only need to sum up to 100 for each option.

Question 3: "Before the first contract with a multinational firm, how did your firm expect multinational buyers to be different from domestic buyers? Complete all the options, selecting whether you agree with the proposed difference. "Our firm expected contracts with multinationals...". Question type: Likert-type scale. Survey version: both long and short (N=106).

For each proposed difference, the respondent had to choose one of three options of answer: "No, this difference was not expected," "Yes, this was a **small** expected difference," "Yes, this was a **large** expected difference." We proposed nine potential differences (in order):"...would be more reliable in terms of payment," "... would help us with financing in advance," "... would order larger amounts," "... would have longer-term contracts," "... would help us improve management practices," "...would help us improve our technological knowledge," "...would help us improve our logistics and inventories," "... would help us learn about foreign demand, which would help improve our export performance," "... would allow us to become an official supplier not only for the affiliate in Costa Rica, but also for affiliates in other countries."

Figure D.1 summarizes the answers to Question 3.

Question 4: “Before the first contact with a multinational in Costa Rica: Did the firm plan and make special arrangements to establish a relationship with this type of firm? Please, choose a SINGLE answer." Question type: Dichotomous. Options (in order): “Yes, our firm planned and adopted special measures in advance to start supplying to the multinationals" or “No, our firm did not take special measures to start supplying to the multinationals." Survey section: “On special preparations before establishing a relationship with multinationals in Costa Rica." Survey version: both long and short (N=106).

47 domestic firms chose the negative answer (44%) and 59 domestic firms chose the positive answer (56%).

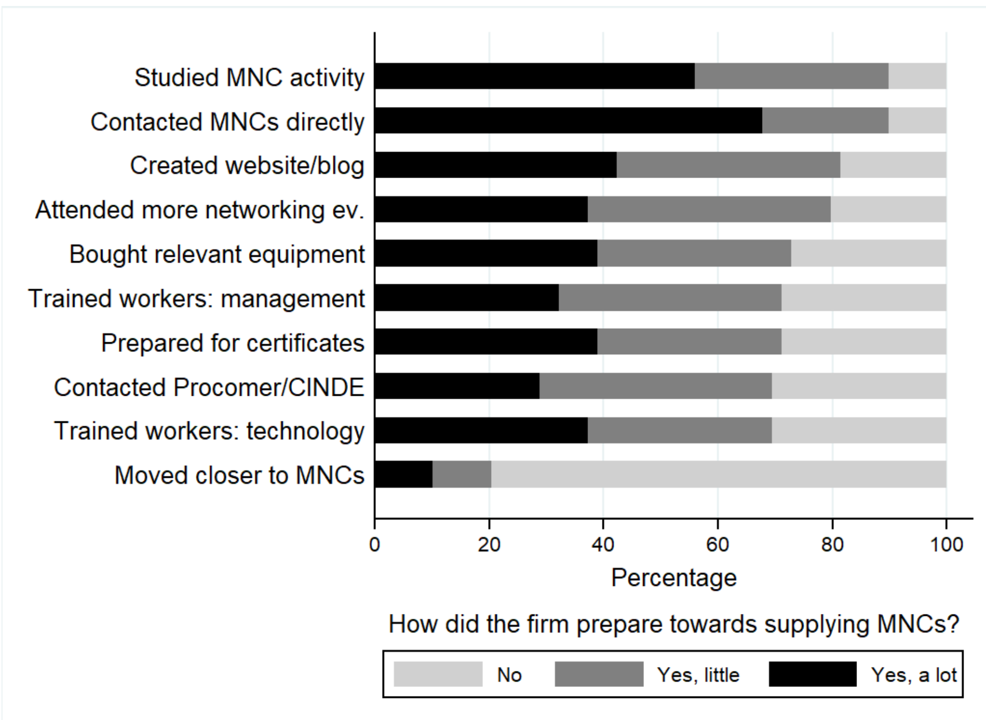


Figure D.2: Question 5: How did your firm prepare to supply to multinationals?

Notes: This graph summarizes the answers of 59 domestic firms to the survey question “How did your firm prepare to supply to multinationals (before establishing the first contact)?" The other 47 domestic firms had answered that they had not taken any special measures towards starting to supply to an MNC. Percentages do not need to sum up to 100 across options, as each firm had to rate the extent to which each proposed option applied to the firm. Percentages only need to sum up to 100 for each option.

Question 5: Question: “How did your firm prepare to supply to multinationals? (before establishing the first contact). Complete all the options, choosing an answer that best describes whether a given measure was taken by your firms "Before the first contact with a multinational, our firm ..." This question was a follow-up to Question 4. If a firm answered negatively to Question 4, this question would be automatically skipped.

For each proposed measure, the respondent had to choose one of three options of answer: “No, our firm did not do this," “Yes, our firm did this but very little," or “Yes, our firm was very involved in this change." We proposed ten measures that the firm might have undertaken in preparation of approaching MNC buyers (in order): “... studied the activity of the multinational to adapt and offer its product to them," “... trained its workers on technologies relevant to supplying to multinationals," “... trained its workers on administrative or management practices relevant to supplying to multinationals," “...began preparing for certifications that were relevant to supplying to multinationals," “... bought machinery that potentially necessary to supplying to multinationals," “... changed its location to be closer to multinationals," “... started participating in more business events to try to find multinational buyers," “... started contacting multinationals directly, trying to present its products / services," “... created a website / blog / social networking page to be easier to find by multinationals," “... approached Procomer / CINDE / MEIC to request assistance in the search for multinational buyers."

Figure D.2 summarizes the answers to Question 5.

Question 6: “Was there any notable change within your firm just before the first contract with a multinational that resulted in your firm starting to supply to that multinational? If the answer is YES, provide details about the unexpected event. If the answer is NO, skip to the next question." Question type: open-ended. Survey version: both long and short (N=106)

100 domestic firms (94%) answered negatively (variations of "N/A", "No", "No change"). Six domestic firms (6%) answered positively, offering details on the said change. Here is an example of one of these positive answers: “Yes, we started advertising our products on a new website and placed ads of the firm in the main search engines." The described changes do not challenge the interpretation of our estimates as capturing the treatment effect of becoming a supplier to MNCs.

Question 7: “To your knowledge, did your firm face difficulties in establishing the first contracts with multinational buyers? Please choose ONE option only." Question type: Dichotomous. Options (in order): “NO, it was relatively easy to start supplying to multinational buyers" or “YES, we faced difficulties in trying to start supplying to multinational buyers." Survey section: “Possible difficulties when trying to establish the first contracts with multinationals." Survey version: both long and short (N=106).

63 domestic firms (59%) provided a negative answer, 43 domestic firms (41%) provided a positive answer.

Question 8: This question was a follow-up to Question 7. If a firm answered negatively to Question 7, this question would be automatically skipped. Question: “Why was it difficult to get a first contract with a multinational? Consider all the potential answers, indicating how important a given explanation was for this difficulty." Question type: Likert-type scale. Survey version: both long and short (N=106 surveys, but 43 answers in practice).

For each proposed measure, the respondent had to choose one of four options: "Very important/Crucial," "Important," "Perhaps a bit important, not central," or "Irrelevant." We proposed eight potential reasons (in order): Multinationals "were difficult to contact," "were not interested in sourcing locally," "did not know the firm and did not trust the product / service offered," "expected types of products or services that the firm did not offer," "expected a quality of products or services that the firm could not offer at that time," "required products or services produced faster than the firm could commit," "expected lower prices than those that this firm could offer," "required products or services for which the firm had to make large investments (for example, buy a machine, expand the scale of production)."

Figure D.3 summarizes the findings from Question 8.

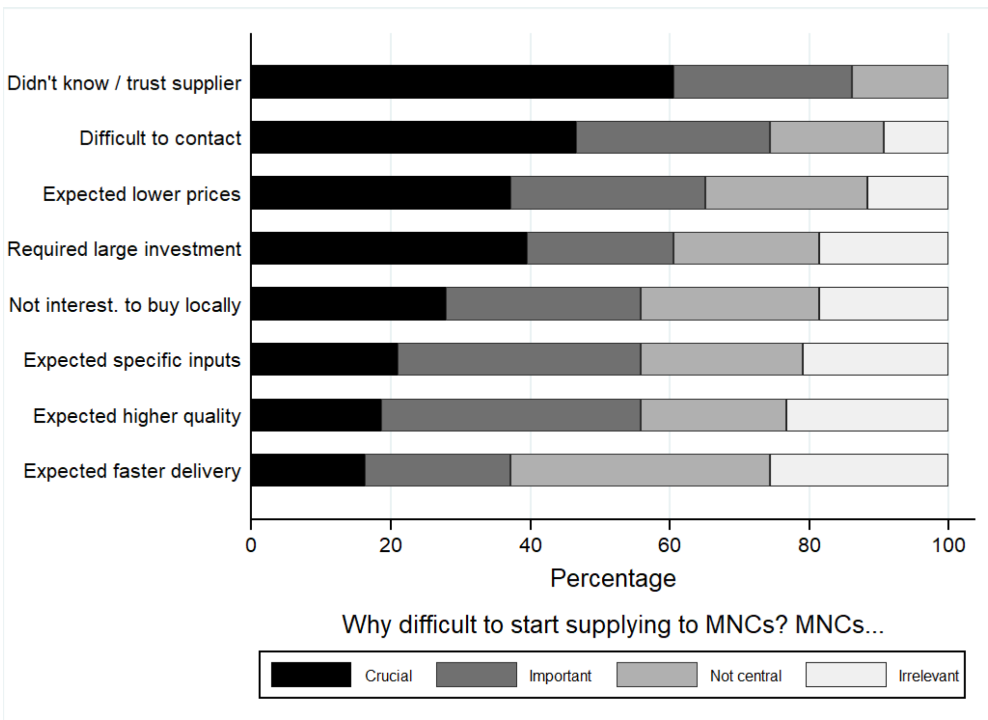


Figure D.3: Question 8: Why was it difficult to get a first contract with a multinational?

Notes: This graph summarizes the answers of 43 domestic firms to the survey question "Why was it difficult to get a first contract with a multinational?" The other 63 domestic firms had answered that it was not particularly difficult to establish a contract with a multinational. Percentages do not need to sum up to 100 across options, as each firm had to rate the extent to which each proposed option applied to the firm. Percentages only need to sum up to 100 for each option.

Question 9: "What were the changes that the firm experienced when becoming a supplier to its first multinational buyers? Select all the answers that are TRUE." Question type: Multiple-choice. Survey section: "During and immediately after the first contracts with multinational buyers." Survey version: both long and short (N=106).

The question allowed for multiple answers among ten options (in order): “The multinational firm required specific products or services, so we expanded our portfolio of products or services that we offered," “We completely replaced the products or services that we previously offered, with those demanded by multinationals," “We continued to offer the same products or services, but the quality and / or the price changed," “We decided to expand our productive capacity in order to meet the larger orders from multinationals," “We hired more highly qualified workers to help us better serve multinational buyers," “Our workers had to work harder and longer hours, because the expectations of the multinational were higher than they were used to," “We changed our sourcing strategy (for example, we sourced differently locally, imported more)," “We learned from the multinational about management practices or organization," “We learned from the multinational about technology relevant for our products or services."

Figure D.4 summarizes the answers to Question 9.

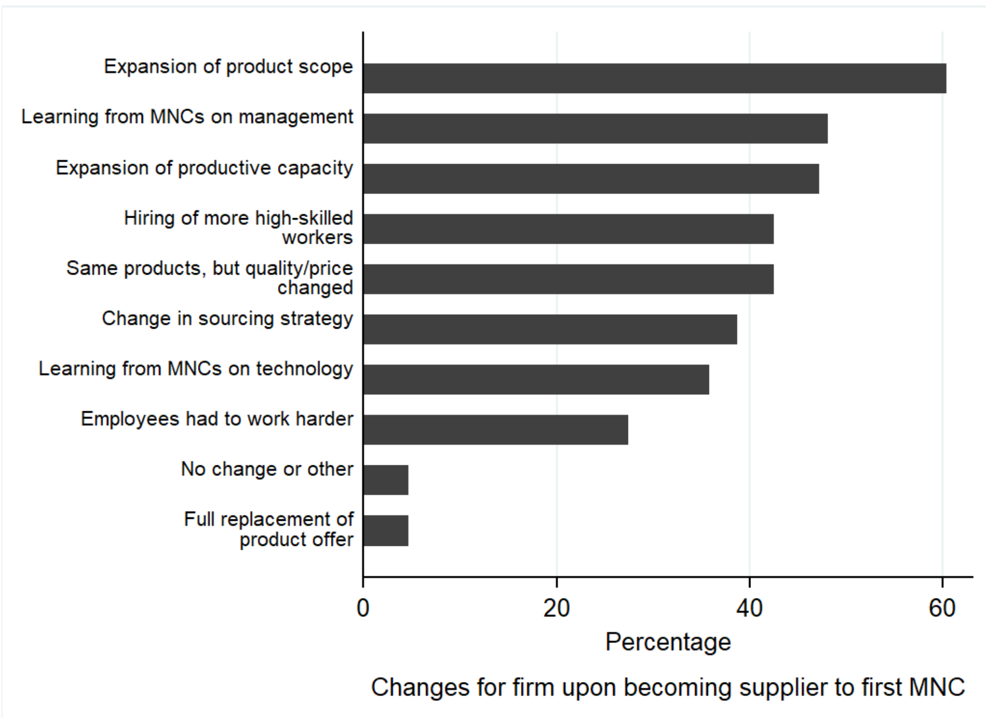


Figure D.4: Question 9: What were the changes that the firm experienced when becoming a supplier to its first MNC buyers? Select all the answers that are TRUE.

Notes: This graph summarizes the answers of 106 domestic firms to the survey question: “What were the changes that the firm experienced when becoming a supplier to its first multinational buyers? Select all the answers that are TRUE." Note that percentages do not need to sum up to 100 across options, as each firm could select all options that applied.

Question 10: “Please provide more details about the most important change that the firm experienced upon becoming a supplier to multinationals." Question type: Open-ended.

Survey section: “During and immediately after the first contracts with multinational buyers." Survey version: both long and short (N=106).

Answers to this question were unguided, hence in order to be summarized had to analyzed and grouped by main topic. Table D.6 summarizes the most frequent changes.

Most Important Change	Frequency	Percent	Cum.
Improved management/organizational practices	24	22.64	22.64
Improved product/service quality, established quality management system	16	15.09	37.74
Increased productive capacity / expansion abroad	13	12.26	50.00
No important change	9	8.49	58.49
Other	9	8.49	66.98
Improved efficiency / delivery times	8	7.55	74.53
Improved sourcing / supply chain strategy	8	7.55	82.08
Expanded product / service scope	7	6.60	88.68
Had to improve firm financing ability	4	3.77	92.45
Acquired new machinery / equipment	3	2.83	95.28
Improved job security / worker safety	3	2.83	98.11
Worked longer hours	2	1.89	100.00
Total	N=106	100	

Table D.6: Question 10: What was the most important change experienced upon becoming a supplier to MNCs?

Notes: This table summarizes the answers of 106 domestic firms to the survey question: “Please provide more details about the most important change that the firm experienced upon becoming a supplier to multinationals." As this question was open, the team had to organize answers by topic.

Question 11: “How did the first multinational buyers help the firm to undergo these changes? Mark all the answers that are TRUE." Question type: Multiple-choice. Survey section: “Possible help from the multinational." Survey version: both long and short (N=106).

The question allowed for multiple answers among nine options (in order): “The multinational did not participate directly, did not provide any explicit help, we dealt with the changes on our own," “The multinational provided a model ("blueprint") of the desired product or service or some other relevant documentation," “Employees of the multinational visited our firm and helped us with advice in the adjustment process (for example, the multinational conducted audits of the firm and guided it on ways to improve)," “Our employees made visits to the multinational to observe parts of their production that were relevant to the input we were supplying to the multinational," “The multinational had standardized training programs that they offered to our employees," "The multinational put us in contact with another firm that supplies similar products or services to the multinational in other locations, to advise us on best practices," "The multinational has lent us money or paid us in advance so that we can make the necessary investments," "The multinational is the one that bought the specific machinery necessary to supply the good / service and they have lent / rented the machinery to us," "Other: _____."

Figure D.5 summarizes the answers to this question.

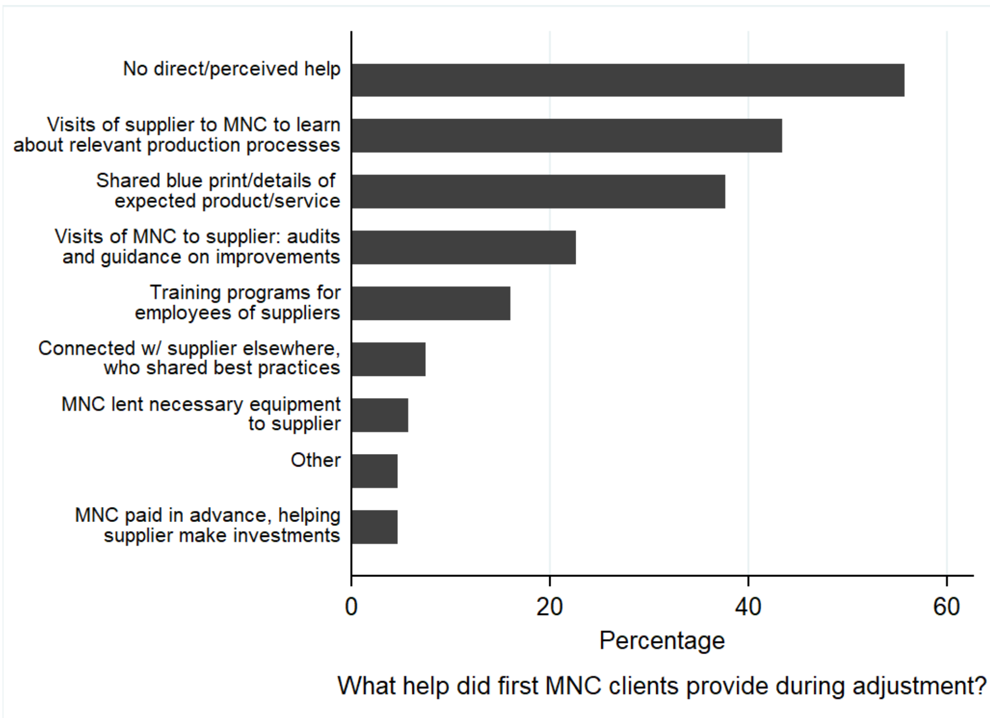


Figure D.5: Question 11: How did the first MNC buyers help the firm to undergo these changes?

Notes: This graph summarizes the answers of 106 domestic firms to the survey question "How did the first multinational buyers help the firm to undergo these changes? Mark all the answers that are TRUE." Note that percentages do not need to sum up to 100 across options.

Question 12: "From the previous answers, please provide more details about the most important assistance provided by the first multinational buyers." Question type: Open-ended. Survey section: "Possible help from the multinational." Survey version: both long and short (N=106).

In the open-ended field, suppliers explained the nature of their interactions with their first MNC buyers and the extent to which these interactions are perceived as help or as integral to their deal. The main takeaway from these answers is that the adjustment period was exacting for most local suppliers. While interactions with MNCs were instrumental in understanding MNCs' expectations from both the supplier overall and the product/service provided in particular, these interactions were not always perceived as supportive/helpful. Our interpretation is that during these interactions MNCs placed high demands on their new suppliers and, while the MNC was constructive in proposing ways to improve, implementing those suggestions was still in the responsibility of the supplier. For example, the answer of one domestic form captures the subtle distinction between direct and indirect help:

> The most important help received from MNCs came in the form of audits to our plant. Another important and related support from MNCs was to give us time to address the [quality] complaints they made during these audits so that we could develop a business model incorporating their quality standards.

Question 13: "If the multinational provided direct/explicit help, how was your firm supposed to reward the multinational for this help? Please choose ONE option only." The question allowed for a single answer among seven options (in order): "The multinational did not offer any (direct/explicit) help in our adjustment to supply it, so this question does not apply," "The help offered was not NOT to be rewarded, it was part of the Corporate Social Responsibility strategy of the multinational, there were no specific expectations from the multinational in exchange of that help," "The help provided was to be rewarded through lower prices than those we could offer before the collaboration with the multinational, for the same product or service (same quality)," "The help provided was to be rewarded through higher quality products / services, at prices that did not change much," "The help provided was to be rewarded through higher quality products / services AND ALSO through prices falling," "The help provided was to be rewarded through an exclusive contract between our firm and the multinational, we had to become its exclusive suppliers," and "Other: ____." Survey version: both long and short (N=106)

Table D.7 summarizes the answers to Question 13.

Question 14: "If your firm has incurred losses from deals with MNC buyers, why does your firm have such deals with MNCs, despite this risk of losses? If your firm has never incurred losses with MNCs, you can skip the question." Question type: Open-ended. Survey section: "Possible help from the multinational." Survey version: long only (N=15).

11 of 15 respondents have provided examples of situations when they have incurred losses from deals with MNCs and their reasons behind tolerating such losses. In general, the answers reflect the stronger bargaining power of MNCs and the longer-term vision of the supplier, who is willing to accept short-term losses with the expectation that the MNC would be satisfied with its service and continue purchasing its service in the future. The supplier would learn from its initial mistakes and reduce the probability of future losses.

We have already provided an example of one such situation in Section 2.4. Hereafter, we present two other examples.

> When we started supplying to MNCs, at the very beginning, there was a certain margin of loss. We were expected to be very fast. In the workshop we had

Most Important Change	Frequency	Percent
No direct/explicit help	57	53.77
Better quality of product/service, same prices	18	16.98
Better quality of product/service, falling prices	12	11.32
No need for compensation, part of MNC CSR	11	10.38
Lower prices for same product/service quality	4	3.77
Other	4	3.77
Total	N=106	100

Table D.7: Question 13: If the multinational provided direct/explicit help, how was your firm supposed to reward the multinational for this help? Please choose ONE option only
Notes: This table summarizes the answers of 106 domestic firms to the survey question: "If the multinational provided direct/explicit help, how was your firm supposed to reward the multinational for this help? Please choose ONE option only"

> to make a lot of efforts. We decided to produce more than what was initially ordered by the MNC, to have a margin in case the MNC ordered more. The extra quantities produced and not ordered became losses.

An example from another supplier:

> There is uncertainty not in the costs of a given product, but in whether the product will correspond to the expectations [of the MNC buyer]. Given the business of our firm, there is no standardized product. Hence some products might end up costing us more if more iterations are needed. The final product might look very different from what we initially thought. If we make mistakes and do not design the right product from the beginning, this can lead us to a loss. However, we see this as a learning opportunity. Sometimes one has to incur losses to learn.

Question 15: "For a purchase order of the same product, quantity and quality, is there a difference in the price charged to a national buyer with respect to a multinational buyer? Please choose ONE option from the following." The question allowed for a single answer among five options (in order): "Almost always a higher price for the multinational buyer," "More often a higher price for the multinational buyer," "In most cases, the same price for both types of buyers," "More frequently, a lower price for the multinational buyer," and "Almost always a lower price for the multinational buyer."

Survey version: only short (N=91.) There was an almost identical question in the long survey as well. However, that question was amended to specify that the order was for the same *quantity*. Suppliers explained during the interviews that for the same product and quality, MNCs are more likely to be offered lower prices as they typically place larger orders.

Table D.8 summarizes the choices made by the 91 domestic firms to Question 15.

Answer	Frequency	Percent
Usually same price	53	58.24
More frequently a lower price for MNC	14	15.38
More frequently a higher price for MNC	10	10.99
Almost always a higher price for MNC	9	9.89
Almost always a lower price for MNC	5	5.49
Total	N=91	100

Table D.8: Question 15: For a purchase order of the same product, quantity and quality, is there a difference in the price charged to a national buyer with respect to a multinational buyer?

Notes: This table summarizes the answers of 91 domestic firms to the survey question: "For a purchase order of the same product, quantity and quality, is there a difference in the price charged to a national buyer with respect to a multinational buyer? Please choose ONE option from the following."

Choices	Freq.	Percent	Details on main reason	Freq.	Percent
No. No Impact	59	55.66			
Yes. Sold More	31	29.25			
			Better quality, same prices	15	48.39
			Higher visibility	9	29.03
			Same quality, lower prices	4	12.90
			Attractive new offer	2	6.45
			Better quality, lower prices	1	3.23
			Total	**N=31**	100
Yes. Sold Less	16	15.09			
			Own decision to focus on MNCs	9	56.25
			Attractive new offer, higher prices	4	25.00
			Not attractive, similar prices	3	18.75
			Total	**N=16**	100
Total	**N=106**	100			

Table D.9: Question 16: Has becoming a supplier of MNCs changed your firm's business with domestic buyers?

Notes: This table summarizes the answers of 106 domestic firms to the survey question: "Has becoming a supplier of a multinational changed your firm's business with domestic buyers? Please choose ONE option only from the options below that best describes this impact."

Question 16: "Has becoming a supplier of a multinational changed your firm's business with domestic buyers? Please choose ONE option only from the options below that best describes this impact." The question allowed for a single answer among ten options (in order): "No. There was no impact on our domestic business, we continued to sell the same products, at the same prices, without changes in the demand of domestic buyers," "Yes, in general we DECIDED to sell LESS to domestic buyers, since we decided to focus only on multinational buyers," "Yes, in general we started selling LESS to domestic buyers, because we started producing goods or services that were not attractive to domestic buyers, despite similar prices," "Yes, in general we started selling LESS to domestic firms because, despite producing attractive goods or services, these goods or services were too expensive for domestic buyers," "Yes, in general we started selling MORE to domestic buyers, because we were selling better quality products / services, at the same price as before," "Yes, in general, we started selling MORE to domestic buyers, because we were selling products / services of the same quality, but at lower prices than before," "Yes, in general we started selling MORE to domestic buyers, because we were selling better quality products / services EVEN IF at higher prices than before," "Yes, in general we started selling MORE to domestic buyers, because we were selling new products or services than those we offered before," "Yes, in general we started selling MORE to domestic buyers, because selling to multinationals made us more visible in the market. However, the products and prices had not really changed," and "Other: _____." Survey version: both long and short (N=106). Section: "Relationships with other types of buyers."

Table D.9 reports the findings from this question. First, we group choices in three broad categories: "No. No Impact" (option 1), "Yes. Sold Less" (options two to four), and "Yes. Sold More" (options five to nine). While five firms had originally chosen the "Other: _____" option, their answers fell into an already existing option among the previous nine. These broad groups are reported in decreasing order of frequency. We then provide details on the actual choices of firms falling into either the "Yes. Sold More" or "Yes. Sold Less" categories.

Question 17: "Did becoming a supplier to a first multinational improve the ability of your firm to obtain more multinational buyers? Please choose ONE option only." Question type: Dichotomous. Options in order: "NO. Finding each new multinational buyer is as difficult as finding the first multinational buyer" or "YES. Becoming a supplier to a first multinational improved the capacity of our firm to obtain more multinational buyers." Survey version: both long and short (N=106). Section: "Relationships with other types of buyers."

83 domestic firms chose the "YES" answer (78%) and 23 domestic firms chose the "NO" answer (22%).

Answer	Frequency	Percentage
Easier to gain MNCs' trust	71	85.5
Learned about MNCs' needs	60	72.3
Improved managerial practices	52	62.7
Expanded product/service offer	43	51.8
Improved quality without price rise	37	44.6
Improved quality with price rise	25	30.1
Lowered prices on prior products/services	5	6
Other	2	2.4

Table D.10: Question 18: Why was it easier to find more multinational buyers after having your first (multinational) buyer? Please choose all the options that are TRUE.
Notes: This table summarizes the answers of 83 domestic firms to the survey question: "Why was it easier to find more multinational buyers after having your first (multinational) buyer? Please choose all the options that are TRUE." Note that the frequency of answers does not need to sum up to 83 or the percentage to 100, as each firm could select all options that applied.

Question 18: "Why was it easier to find more multinational buyers after having your first (multinational) buyer? Please choose all the options that are TRUE." Question type: Multiple-choice. Survey section: "About the multinational buyers that followed." Survey version: both long and short (N=106 surveys, but 83 answers in practice).

This question was a follow-up to Question 17. If a firm selected the negative answer in Question 17, it would automatically skip this question. Hence, the following findings pertain to the 83 domestic firms choosing "YES" in Question 17.

Table D.10 summarizes the answers to Question 18.

Question 19: "How many of the deals of your firm with multinational buyers in Costa Rica occur through Procomer? Please choose ONE option only." The question allowed for a single answer among five options (in order): "(Almost) all deals are mediated through Procomer," "More than half of the deals are mediated by Procomer, but not all," "Less than half of the deals are mediated through Procomer, but there are still many," "Very few (or almost none) of these deals are mediated through Procomer." Survey version: long only (N=15). Survey section: "On the intermediation of deals with multinationals by Procomer."

Table D.11 summarizes the answers to Question 19.

Question 20: "What are the main reasons why your firm wants to make such deals through Procomer? Please, choose (at most) the two most relevant options." The question allowed for at most two answers out of six options (in order): "Procomer deals are not different from the deals we get for ourselves, but allow us to have multiple sources of deals," "Procomer

Answer	Frequency	Percentage
Very few to almost none	12	80.00
Less than half, but some	2	13.33
(Almost) all	1	6.67
Total	N=15	100

Table D.11: Question 19: How many of the deals of your firm with multinational buyers in Costa Rica occur through Procomer? Please choose ONE option only.
Notes: This table summarizes the answers of 15 domestic firms to the survey question: "How many of the deals of your firm with multinational buyers in Costa Rica occur through Procomer? Please choose ONE option only."

has better access to multinational buyers or the specific type of deals our firm wishes to have (for example, larger amounts, longer contracts, more high-tech buyers, etc.)," "Procomer gives us credibility in front of multinational buyers," "Procomer prepares us before each specific deal with a multinational buyer, so we feel better prepared to start deals mediated by Procomer," "Procomer accompanies our deals with multinational buyers, provides us with services even after the deal was made and is in progress," and "Other: ____." Survey version: long only (N=15). Survey section: "On the intermediation of deals with multinationals by Procomer."

Table D.12 summarizes the answers to Question 20.

Question 21: "Please share with us the most negative surprise or the biggest disappointment for your firm after becoming a supplier to MNCs." Question type: Open-ended. Survey section: "Questions to wrap up." Survey version: only long (N=15).

The general message is that domestic suppliers often find themselves in asymmetric relationships with MNCs, where they feel that their efforts to make the relationship successful are not reciprocated. There is also a significant imbalance of power, size, and financial robustness between MNCs and domestic suppliers to which MNCs do not seem to be sensitive. Hereafter, we include the answers of two different suppliers that are representative of the other answers.

> One negative surprise is that MNCs do not seem to understand how impactful some of their mistakes are for their small suppliers. For instance, MNCs do not seem to be aware of how costly it is for us, as a small firm, to prepare a bid. Therefore they invite us to bid, despite having already chosen the winner. Or, sometimes, bills are misplaced, and our payment is made with delay. Even officially, MNCs have gone from 15 days of trade credit to up to 120 days. MNCs

Answer	Frequency	Percentage
Procomer has better access to MNCs	9	60.0
Deals not different, just another source of deals	8	53.3
Procomer offers credibility in front of MNCs	6	40.0
Procomer helps prepare the firm before the deals	0	0.0
Procomer accompanies the firm during the deals	0	0.0
Other	2	13.3

Table D.12: Question 20: What are the main reasons why your firm wants to make such deals through Procomer? Please, choose (at most) the two most relevant options.
Notes: This table summarizes the answers of 15 domestic firms to the survey question: "What are the main reasons why your firm wants to make such deals through Procomer? Please, choose (at most) the two most relevant options."

> use the entire trade credit length agreed upon initially (say 120 days). Once a bill gets to accounting, it will be paid automatically 120 days after. It is true that the payment is most of the time reliable. But small suppliers like us are bearing a lot of the risks and providing financing to MNCs, as opposed to the other way around. This is surprising given how small our bills are compared to the overall turnover of these MNCs.
>
> We were very hopeful of positive outcomes before the first contracts. However, we had to lower prices massively to be granted those contracts. MNCs were aggressive in negotiating the reduction of prices. We still have to offer very low rates to maintain these contracts. Also, we started the deals with MNCs with one month of trade credit. Now, MNCs expect 3.5 months of credit on average. Last, we feel that MNCs are not very interested in developing local suppliers, that they act as if they are entitled to receive high-quality goods or services at meager prices.

Question 22: "Please share with us the most positive surprise or the biggest unexpected benefit for your firm after becoming a supplier to MNCs." Question type: Open-ended. Survey section: "Questions to wrap up." Survey version: only long (N=15).

The main takeaway from these answers is that these domestic firms are now enjoying the fruits of their initial hardships experienced upon becoming suppliers to MNCs. The following is a representative quote from one of the respondents.

The beginnings [of relationships with MNCs] were very tough because we had to lower prices a lot. Once we adapted to the new ways of doing business, we started growing. We started buying new machines or renovating older machines, having more employees. The hardship at the beginning allowed us to rise afterward. Year after year, the contracts get renewed, so we need to continue learning and maintaining competitive prices. Whenever the costs of inputs increase, we have to improve on some other dimension to keep our prices low [better-trained machine operators, faster machines, etc.]. Also, now the MNCs have become more involved. Sometimes staff from MNCs ask: "What is slowing you down? Let us help you with that."

Survey Answers from Multinational Firms (MNCs)

Question 1: "Country where the headquarters of the multinational is." Question type: open-ended. Survey version: both long and short (N=58). Responses are summarized in Table D.13.

HQ country	Frequency	Percentage
United States	24	41.38
Great Britain	4	6.90
Costa Rica	3	5.17
Germany	3	5.17
Netherlands	3	5.17
Panama	3	5.17
Spain	2	3.45
France	2	3.45
Japan	2	3.45
Venezuela	2	3.45
Belgium	1	1.72
Canada	1	1.72
Switzerland	1	1.72
Colombia	1	1.72
Guatemala	1	1.72
Ireland	1	1.72
Cayman Islands	1	1.72
Mexico	1	1.72
Peru	1	1.72
El Salvador	1	1.72
Total	N=58	100

Table D.13: Question 1: MNC's Headquarters Country

Notes: This table summarizes the answers of 58 multinationals to the survey question: 'Country where the headquarters of the multinational is."

Question 2: "Your position (job title) in the multinational." Question type: open-ended. Survey version: both long and short (N=58). Responses are summarized in Table D.14.

Position (Standardized)	Frequency	Percentage
Supply Chain/Procurement/Operations Manager	22	37.93
General Manager CR Operation / Country Manager	18	31.03
Other Unit Manager	14	24.14
Supply Chain/Procurement Specialist	4	6.90
Total	N=58	100

Table D.14: Summary of Job Titles for Respondents to the Survey to Multinationals
Notes: This table summarizes the answers of 58 respondents (to the survey to multinationals) to the survey question: "Your position (job title) in the multinational." We have grouped job titles under four categories. Under "Supply Chain/Procurement/Operations Manager," one can find job titles such as Purchasing Manager ("Gerente de Compras"), Global Operations Manager ("Gerente Global de Operaciones"), or Purchasing and Logistics Manager ("Gerente de Compras y Logistica"). Under "General Manager CR Operation / Country Manager," one can find job titles such as Plant Manager ("Gerente de Planta"), Manager of XX Costa Rica ("Gerente de XX Costa Rica") or Site Supervisor. Under "Other Unit Manager", one can find job titles such as Manager of Public Relations ("Gerente Asuntos Públicos"), Manager of Government Affairs ("Gerente de Asuntos Gubernamentales"), or Finance Manager ("Gerente Financiero"). Under "Supply Chain/Procurement Specialist," one can find job titles such as Buyer ("Encargado de Compras") or Import/Export Analyst ("Analista Import / Export").

Question 3: "To your knowledge, how important were the following factors in the decision of the multinational to locate itself in Costa Rica? Complete all the options, choosing how important you think each criterion was. Note: There is a separate question about the decision to stay and / or expand in Costa Rica." Question type: Likert-type scale. Survey version: both long and short (N=58). Section: "General questions about the multinational's incentives to invest in Costa Rica."

For each proposed factor, the respondent had to choose one of four options: "Very important/Crucial," "Important," "An advantage, but not that important," or "Not important, does not apply." We proposed eight potential reasons (in order): "The distance between Costa Rica and the HQ country," "The distance between Costa Rica and your target markets," "The Costa Rican market itself," "The level of education of the labor force," "Relatively low wages for the type of employees needed by the multinational," "Tax conditions such as the Free Zone regime," "The availability of suppliers at the prices and / or quality that the multinational needs," "The natural resources (for example, minerals) of Costa Rica, necessary for the production of the multinational."

Figure D.6 summarizes the findings from Question 3.

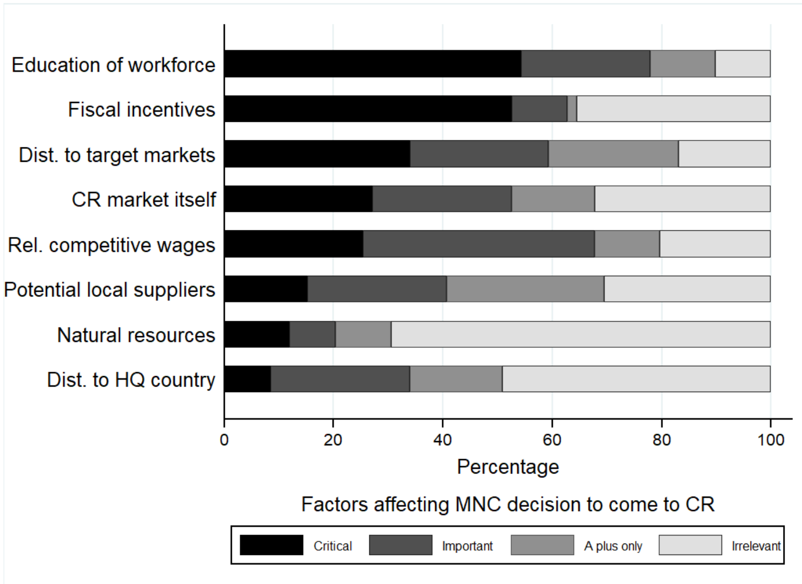


Figure D.6: Question 3: How Important Were the Following Factors in the Decision of the Multinational to Locate Itself in Costa Rica?

Notes: This graph summarizes the answers of 58 multinationals to the survey question "To your knowledge, how important were the following factors in the decision of the multinational to locate itself in Costa Rica? Complete all the options, choosing how important you think each criterion was." Percentages do not need to sum up to 100 across options, as each respondent had to rate the extent to which each criterion had been relevant to the MNC. Percentages only need to sum up to 100 for each criterion.

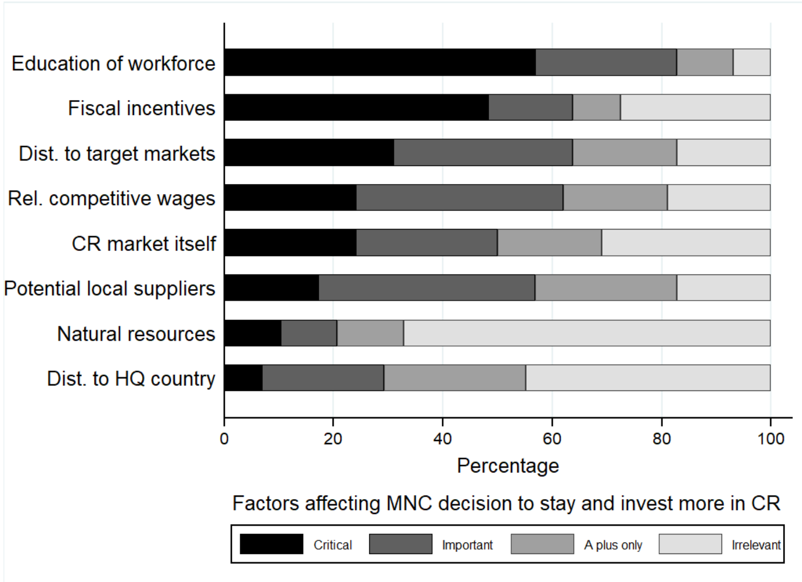


Figure D.7: Question 4: To your knowledge, how important were the following factors in the decision of the multinational to STAY or EXPAND in Costa Rica?

Notes: This graph summarizes the answers of 58 multinationals to the survey question “To your knowledge, how important were the following factors in the decision of the multinational to STAY or EXPAND in Costa Rica? Complete all the options and choose how important you think each criterion was." Percentages do not need to sum up to 100 across options, as each respondent had to rate the extent to which each criterion had been relevant to the MNC. Percentages only need to sum up to 100 for each criterion.

Question 4: “To your knowledge, how important were the following factors in the decision of the multinational to STAY or EXPAND in Costa Rica? Complete all the options and choose how important you think each criterion was." Question type: Likert-type scale. Survey version: both long and short (N=58). Section: “General questions about the multinational's incentives to invest in Costa Rica." The scale and the options were the same as those proposed for Question 3.

Figure D.7 summarizes the findings from Question 4.

Criterion	Critical	V. Important	Important	Only useful	Irrelevant
Quality of products/services	75.9	15.5	6.9	0.0	1.7
Ability to adapt to MNCs	60.3	25.9	10.3	1.7	1.7
Price of products/services	43.1	32.8	15.5	6.9	1.7
Reliability, traceability etc.	31.0	37.9	19.0	6.9	5.2
ISO certificates	20.7	50.0	15.5	5.2	8.6
Productive capacity	12.1	29.3	36.2	10.3	12.1
Will or ability to invest	8.6	32.8	25.9	15.5	17.2
Distance supplier-MNC	6.9	24.1	20.7	27.6	20.7
Prior experience exporting	5.2	19.0	15.5	25.9	34.5
Foreign language	5.2	19.0	17.2	20.7	37.9
Same HQ country	3.4	0.0	5.2	19.0	72.4
Be part of a FTZ	3.4	1.7	13.8	22.4	58.6
Will to move closer	1.7	17.2	19.0	37.9	24.1
Prior experience w/ MNCs	1.7	36.2	25.9	20.7	15.5
Being foreign-owned	0.0	0.0	1.7	13.8	84.5

Table D.15: Question 5: In general, how important are the following criteria when choosing a new supplier in Costa Rica (Costa Rican or not)?

Notes: This table summarizes the answers of 58 multinationals to the survey question "In general, how important are the following criteria when choosing a new supplier in Costa Rica (Costa Rican or not)? Complete all the options, selecting the importance that you think each criterion has." Percentages do not need to sum up to 100 across criteria, as each respondent had to rate the extent to which each criterion is relevant to the MNC. Percentages only need to sum up to 100 for each criterion.

Question 5: "In general, how important are the following criteria when choosing a new supplier in Costa Rica (Costa Rican or not)? Complete all the options, selecting the importance that you think each criterion has." Section: "Relations with local suppliers (located in Costa Rica). From this moment, our questions will focus on the relationship between the multinational and its local suppliers."

For each proposed factor, the respondent had to choose one of five options: "Of critical importance," "Very important," "Important," "Useful, but not a decisive factor," or "Without importance, irrelevant, does not apply." We proposed fifteen potential reasons (in order): "The physical distance between the supplier and the multinational," "The willingness of the supplier to move closer to the multinational," "Having previous experience with multinationals," "Having previous experience exporting," "Being from the same country as the multinational," "Being foreign-owned, even if not from the same country as the multinational," "Being under the Free Trade Zone regime," "The price of goods or services already on offer," “The quality of goods or services already on offer," “Willingness or ability to adapt and supply the exact product or service needed by the multinational," “Having a manager (or employee) who speaks the main language of the multinational," “Reliability / inventory management / input traceability / other characteristics of the organization," “Having standardized quality certificates, relevant to the business (for example, ISO 13485 in the medical device sector)," “The size of the supplier, that is, that already has sufficient productive capacity," “The willingness or ability to make large investments to supply to the multinational."

Table D.15 summarizes the answers to Question 5.

Question 6: “Does the multinational provide any particular support or guidance to a new supplier to improve its ability to supply to the multinational?" Question type: Dichotomous. Survey version: both short and long (N=15). Question type: Dichotomous. The two options available were “NO, the multinational does not provide any explicit support" and “YES, the multinational carries out specific actions to help the new supplier adapt to their relationship."

40 multinationals answered “YES" (69%) and 18 multinationals answered “NO" (31%).

Question 7: “Which of the following options describe the way(s) in which the multinational provides support to the new supplier to adapt to their new relationship? Mark all the answers that are TRUE." Question type: Multiple-choice. Survey section: “More details on the support provided by the multinational to suppliers." Survey version: both long and short (N=40).

We proposed eight potential options (in order): “The multinational provides an instruction manual ("blueprint") of the desired product or service or other relevant documentation," “Employees of the multinational visit the supplier and help it with advice in the adjustment process (for example, the multinational performs supplier audits and guides the supplier on ways to improve)," “Employees of the supplier are invited to visit the multinational to observe parts of its production that are relevant to the inputs they will supply to the multinational," “The multinational has standardized training programs that the multinational offers to employees of local suppliers," “The multinational puts the supplier in contact with another supplier that sells similar products or services to the multinational in other places, to advise the new supplier on best practices," “The multinational lends money or pays the firm in advance so that the firm can make the necessary investments," “The multinational is the one that buys the specific machinery necessary to provide the good / service and lends / rents it to the local supplier," or “Other: _____."

Table D.16 summarizes the answers of 40 multinationals to Question 7.

Support	Frequency	Percentage
Share blueprint/details of expected product/service	33	82.5
Visits of supplier to MNC, learn about relevant production process	33	82.5
Visits of MNC to supplier, audits and guidance on improvements	32	80.0
Training programs for suppliers' workers	13	32.5
Connect w/ supplier elsewhere, who shares best practices	9	22.5
MNC pays in advance, helping supplier make investments	6	15.0
MNC lends necessary equipment to supplier	2	5.0
Other	5	12.5

Table D.16: Question 7: Which of the following options describe the way(s) in which the multinational provides support to the new supplier to adapt to their new relationship?
Notes: This table summarizes the answers of 40 respondents (to the survey to multinationals) to the survey question: "Which of the following options describe the way(s) in which the multinational provides support to the new supplier to adapt to their new relationship? Mark all the answers that are TRUE." Note that the 18 multinationals that responded "NO" to Question 6 skipped this question.

Question 8: "If possible, please provide more details on the most important way in which the multinational assists the supplier to adjust to its new relationship with the multinational. For example, the duration of the assistance provided, the frequency of the assistance, the number of trained employees, the size of the loan offered and the conditions, etc." Question type: Open-ended. Survey section: "More details on the support provided by the multinational to suppliers." Survey version: long and short (N=40). This question was a follow-up to Question 7, for those having chosen "YES" in Question 6.

Each MNC responding positively to Question 6 provided details on its most important form of support extended to its new suppliers. The main takeaway is that there is great variety in the breadth and depth of the support provided by MNCs to their new suppliers. The lighter forms of assistance include sharing of detailed descriptions of the good or service expected (without additional guidance on how to actually produce it) or sharing of an instruction manual on the general practices that MNCs expect their suppliers to follow. The following quote pertains to one of the MNCs whose support seemed more substantial.

> The most important help that we offer comes in the form of standardized training programs. Given that our industry has very high standards of quality, we need to make sure that our suppliers can live up to the same standards as we do. For that reason, our local experts provide tailored training to suppliers, share corporate best practices with them. This leads to a win-win: it benefits us as it turns the supplier into an ally, it benefits the supplier as it is improving its [business and technical] practices. Whether the training is offered only to the manager of the

supplier or whether it includes other employees as well depends on the nature of the training, how deep it goes into the processes of the supplier, how large is the gap between where the supplier is and where it needs to get.

Compensation	Frequency	Percentage
Increasing quality, prices not changing much	15	37.5
Increasing quality, falling prices	12	30.0
Not to be compensated, part of CSR	8	20.0
Other	3	7.5
Exclusivity contract b/n MNC and supplier	1	2.5
Quickly falling prices, same product/service	1	2.5
Total	N=40	100

Table D.17: Question 9: How is the supplier expected to compensate the multinational for the support received? Please choose ONE option only.
Notes: This table summarizes the answers of 40 respondents (to the survey to multinationals) to the survey question:"How is the supplier expected to compensate the multinational for the support received? Please choose ONE option only." Note that the 18 multinationals that responded "NO" to Question 6 skipped this question.

Question 9: "How is the supplier expected to compensate the multinational for the support received? Please choose ONE option only." Survey section: "More details on the support provided by the multinational to suppliers." Survey version: long and short (N=40).

The question allowed for a single answer among seven options (in order): "The support provided is NOT intended to be reciprocated. For example, this support is part of the Corporate Social Responsibility strategy of the multinational," "The support must be corresponded through lower prices in the SHORT-TERM than the prices that the firm could offer before the collaboration with the multinational, for the same product or service," "The support must be corresponded through a trend of GRADUALLY decreasing prices compared to the prices that the firm could offer before the collaboration with the multinational, but for the same product or service," "The support must be corresponded through ensuring a higher quality of the product / service, BUT with prices that do not change much," "The support must be corresponded through ensuring a greater quality of the product / service AND with prices also falling," "The support must be reciprocated through an exclusivity contract between the firm and the multinational, the firm must become an exclusive supplier," or "Other: _____."

Table D.17 summarizes the answers of 40 multinationals to Question 9.

Question 10: "Please, if possible, provide more details about the previous answer." This question is a follow-up to the question above. Survey section: "More details on the support provided by the multinational to suppliers." Survey version: long only (N=23).

By and large, MNC staff describe the support provided to the suppliers of the MNC as meant to establish a win-win collaboration. The following answer from the Supply Chain Manager of one MNC is representative for all other 22 answers.

> While there is no formal commitment during the period of support, we expect that the supplier is willing to educate itself, to learn how to improve the quality and service offered. Moreover, we help the supplier improve its processes, its management practices. Hence there is the expectation that cost reductions would be shared between the supplier and us, that the help we provided led to a win-win situation. For instance, we excel in lean manufacturing and invite suppliers to see how we manage our operation, so that they can apply the same principles to their operation. Suppliers are under constant control of their quality and service. If we put suppliers under probation and if their quality/service does not improve within a couple of months, they lose the contract with us.

Questions 11, 12, and 13: We summarize here the answers to three consecutive and related questions: "From your point of view, what are the three most probable profits/benefits/advantages that Costa Rican firms experience when they become suppliers of MNCs? Provide details to your answers." All three answers were open-ended. Survey version: long only (N=23).

In Table D.18 we categorized the answers provided by the 23 respondents into four categories, which we created based on the common themes emerging across answers.

	Most important benefit		Second most important benefit		Third most important benefit
8	Stability and predictability	11	Learning opportunities	12	Learning opportunities
7	Learning opportunities	7	Stability and predictability	5	Scale and global opportunities
7	Scale and global opportunities	4	Scale and global opportunities	2	Stability and predictability
1	Reputation	1	Reputation	1	Reputation
0	None	0	None	3	None
N=23		N=23		N=20	

Table D.18: Questions 11, 12, and 13: Top three most important benefits to becoming a supplier to MNCs, according to MNCs

Hereafter, we provide an example of an answer for each of the four categories. Each answer comes from a different respondent.

Example for “stability and predictability":

> The first most important gain/benefit /advantage for Costa Rican firms is the contract length. The type of business they establish is a win-win relationship, where it is possible for suppliers to project themselves into the future and begin to be part of a stable supply chain.

Example for “learning opportunities":

> The third largest gain/benefit/advantage derived from becoming a supplier to MNCs has to do with the improvements and the strengthening of the management model of the supplier, both concerning production and service provision. The modus operandi a supplier learns during the collaboration with MNCs is helpful in several ways. If the supplier manages to standardize processes and apply the same principles for other clients, the supplier will always win because it is better prepared. This gain is particularly significant for SMEs.

Example for “scale and global opportunities":

> Once a firm joins our list of approved suppliers for a given commodity, opportunities are global for that supplier within the organization. [They] are in the system and visible globally. That supplier becomes available to anyone at any site. As long as the pricing is correct and the business proposition is the right one, then they can supply elsewhere as well.

Example for “reputation":

> The second largest gain goes to the reputation of the supplier. Once one MNC uses a supplier, given the high expectations of MNCs, if that initial deal goes well, the news spreads to other MNCs that have similar requirements.

Questions 14, 15, and 16: We summarize here the answers to three consecutive and related questions: “From your point of view, what are the three losses/risks/disadvantages that Costa Rican companies experience when they become suppliers of MNCs? Provide details to your answers." All three answers were open-ended. Survey version: long only (N=23).

In Table D.19 we categorized the answers provided by the 23 respondents into six categories, which we created based on the common themes emerging across answers.

Table D.19: Questions 14, 15, and 16: Top three most important risks to becoming a supplier to MNCs, according to MNCs

Most important risk		Second most important risk		Third most important risk	
11	Financial or legal risk	7	None	18	None
7	Demanding changes	5	Demanding changes	2	Financial or legal risk
3	None	4	Financial or legal risk	1	Bad reputation
1	Bad reputation	4	Bad reputation	1	Demanding changes
1	Specificity	2	Other	1	Other
0	Other	1	Specificity	0	Specificity
N=23		N=23		N=23	

Hereafter, we provide an example of an answer for the categories “financial or legal risk," “demanding changes,"" “bad reputation," and “specificity." Each answer comes from a different respondent.

Example for “financial or legal risk":

> A first considerable risk comes from the volumes ordered by MNCs. The supplier might need to invest a lot to live up to its large orders. However, if the supplier is unable to deliver the expected level of quality and service, it might lose the contract and get in trouble because of the investment made. It is not the policy of the multinational to sign long-term contracts with a supplier because they cannot commit to continuing a contract with a supplier that does not deliver what it is supposed to deliver time and again.

Example for “demanding changes":

> The most significant disadvantage/risk has to do with the level of pressure that a firm is put under when becoming a supplier to an MNC. Supplying to an MNC comes with many requirements, many specifications, high standards. MNCs are very demanding. This can be very stressful for a small Costa Rican firm. Sometimes some misunderstandings come up due to misaligned expectations.

Example for “bad reputation":

> The second most important risk is reputational. MNCs participate at seminars, at fora. They exchange on their experience with local suppliers. If a given relationship with an MNC goes sour, then this will become quickly known to other MNCs as well. For this reason, every commercial relationship matters for

> the reputation of a supplier, not to gain a reputation of being a bad supplier, from which it is hard to recover.

Example for “specificity":

> Given the market in which the MNC is, suppliers of direct inputs might feel too narrowly specialized.

Questions 17 and 18: We bundle together these two questions. Question 17 asked about the procurement decision process on key inputs, Question 18 about the decision process on secondary inputs. “WHICH AFFILIATE decides on the procurement of KEY (or SECONDARY) INPUTS for the affiliate in Costa Rica and HOW? Please choose ONE option only. Note: Key inputs are those inputs that affect the quality and final characteristics of the core product. An example of a good / service that may **not** be key (may be secondary) is packaging or spare parts for the machinery used in production."

The question allowed for a single answer among six options (in order): “Most decisions about key (secondary) inputs are made by the headquarters (or another affiliate other than the affiliate in Costa Rica), with little to no feedback on Costa Rican suppliers from the Costa Rican affiliate," “Most of the decisions on key (secondary) inputs are made by the headquarters (or another affiliate other than the affiliate in Costa Rica), but with comments on Costa Rican suppliers from the Costa Rican affiliate," “Decisions on key (secondary) inputs are made jointly between the headquarters (or another affiliate other than the affiliate in Costa Rica) and the Costa Rican subsidiary," “Most decisions on key (secondary) inputs are made by the Costa Rican affiliate, but with comments from the headquarters (or another affiliate other than the affiliate in Costa Rica)," “Most decisions on key (secondary) inputs are made by the Costa Rican affiliate, with little to no feedback from the headquarters (or any affiliate other than the affiliate in Costa Rica)," or “Other: _____."

Table D.20 summarizes the answers from both Questions 17 and 18.

	Core inputs		Secondary inputs	
	Frequency	Percent	Frequency	Percent
HQ, little local feedback	7	12.1	0	0.0
HQ, with local feedback	12	20.7	2	3.5
Joint decision	15	25.9	10	17.2
Local, with HQ feedback	8	13.8	15	25.9
Local, little HQ feedback	12	20.7	28	48.3
Other	4	6.9	3	5.2
Total	N=58	100	N=58	100

Table D.20: Questions 17 and 18: WHICH AFFILIATE decides on the procurement of KEY (SECONDARY) INPUTS for the affiliate in Costa Rica and HOW? Please choose ONE option only.

Notes: This table summarizes the answers of 58 respondents (to the survey to multinationals) to the survey questions: "WHICH AFFILIATE decides on the procurement of KEY (SECONDARY) INPUTS for the affiliate in Costa Rica and HOW? Please choose ONE option only."

D.2 Surveys for "The Effects of Multinationals on Workers"

D.2.1 Survey Conducted in Collaboration with CINDE

The survey instrument was designed in collaboration between CINDE (the Costa Rican investment promotion agency) and our team. The focus of the survey is on the hiring practices of MNCs with a subsidiary in Costa Rica.

A. Survey response rate and representativeness. The survey was sent on March 18, 2019, to the contacts of CINDE in the Human Resources (HR) departments of 246 MNCs. Responses were recorded until March 29, 2019. During this window, 46 MNCs responded to the survey. CINDE regularly conducts surveys over the same set of MNCs. A response rate of 19% is typical. The relatively low response rate is, to some extent, explained by the turnover of employees in MNCs, which imposes regular efforts to update the list of contacts.

As Table D.21 shows, of the 46 surveyed MNCs, 54% operate in services, 33% in life sciences, and 13% in advanced manufacturing. In the set of 246 contacted MNCs, 55% work in services, 23% in life sciences, and 22% in advanced manufacturing.

Table D.21: Industry of Surveyed MNCs and All MNCs

Industry Group	MNCs in survey sample	All MNCs contacted
Services	54%	55%
Life Sciences	33%	23%
Advanced Manufacturing	13%	22%
Number of MNCs	46	246

Notes: Table D.21 summarizes the industry group to which the 46 respondents of the survey belong.

B. Survey questions and answers. **Questions 1 and 2:** “When the company decided to settle in the country, which of the following steps were carried out to form the main team? Select all that apply" (Question 1). “Please order the steps of the first hiring process, with 1 denoting the first step performed" (Question 2). These two questions presented the same seven options: (i) hire recruitment agencies to hire the main team or “heads," (ii) expatriate an executive in charge of operations, (iii) form a team with expatriates of the company, (iv) form a team with locals, (v) run a media campaign (social media, press, other) to receive applications, then use overseas offices to evaluate profiles, (vi) advertise available positions on the company website, (vii) other.

Question 3: “Once the main team was formed, which of the following processes were carried out to hire the remaining staff?" Select all that apply. This question presented eight options: (i) hire a recruitment agency, (ii) run a media campaign (social media, press, other) to receive applications, then use the local team to evaluate profiles, (iii) advertise available positions on the company website, (iv) run a campaign with municipalities, (v) run a campaign during employment fairs of Free Zones, (vi) establish partnerships with educational institutions, (vii) attend (other) employment fairs, (viii) other.

Question 4: “What are the most important qualities when pre-selecting the most skilled workers (managers, engineers, administrative staff, etc.). Select the three most important options." This question presented six options: (i) previous experience in multinational corporations, (ii) experience working abroad, (iii) academic studies and the institution where the worker graduated, (iv) previous experience in the same industry in which the company operates, (v) previous experience in the same job position, (vi) experience in personnel management.

Question 5: “Which are the most important qualities when pre-selecting the less-skilled

Table D.22: Answers to Question 1

Answer	N
Form a team with locals	36
Hire recruitment agencies to hire the main team or “heads"	27
Expatriate an executive in charge of operations	18
Advertise available positions on the company website	17
Form a team with expatriates of the company	11
Run a media campaign (social media, press, other) to receive applications, then use overseas offices to evaluate profiles	11
Other	3

Notes: Table D.22 summarizes the answers to Question 1.

Table D.23: Answers to Question 2

Answer	O=1	O=2	O=3	O=4	O=5
Hire recruitment agencies to hire the main team or “heads"	14	6	3	2	1
Expatriate an executive in charge of operations	13	3	1	0	1
Form a team with expatriates of the company	2	5	2	1	0
Form a team with locals	11	11	8	4	1
Run a media campaign (social media, press, other) to receive applications, then use overseas offices to evaluate profiles	0	3	6	2	0
Advertise available positions on the company website	3	5	3	4	2
Other	3	2	2	0	0

Notes: Table D.23 summarizes the answers to Question 2. "O=1" means that a given step was done first in order.

Table D.24: Answers to Question 3

Answer	N
Run a media campaign (social media, press, other) to receive applications, then use the local team to evaluate profiles	26
Hire a recruitment agency	22
Advertise available positions on the company website	18
Attend (other) employment fairs	18
Establish partnerships with educational institutions	11
Run a campaign during employment fairs of Free Zones	7
Run a campaign with municipalities	6
Other	3

Notes: Table D.24 summarizes the answers to Question 3.

Table D.25: Answers to Question 4

Answer	N
Previous experience in the same job position	33
Previous experience in the same industry in which the company operates	29
Academic studies and the institution where the worker graduated	23
Experience in personnel management	23
Previous experience in multinational corporations	20
Experience working abroad	1

Notes: Table D.25 summarizes the answers to Question 4.

workers (operators, packers, mechanics, cleaning staff, etc.). Select the three most important options." This question presented six options: (i) previous experience in multinational corporations, (ii) experience working abroad, (iii) academic studies and the institution where the worker graduated, (iv) previous experience in the same industry in which the company operates, (v) previous experience in the same job position, (vi) experience in personnel management.

Table D.26: Answers to Question 5

Answer	N
Previous experience in the same job position	41
Previous experience in the same industry in which the company operates	38
Academic studies and the institution where the worker graduated	26
Previous experience in multinational corporations	23
Experience working abroad	1
Experience in personnel management	0

Notes: Table D.26 summarizes the answers to Question 5.

Question 6: “What are the most important sources of information your company uses when it comes to choosing the most skilled workers (managers, engineers, administrative staff, etc.) to hire? Select the three most important options." This question presented eight options: (i) Curriculum Vitae, (ii) letters of recommendation or references of former employers / teachers, (iii) immediate availability, (iv) test of cognitive, psychometric and / or psychological skills (IQ, emotional intelligence, ability to work in a team), (v) test of knowledge or professional skills related to the job, (vi) evaluation of the work done during a trial period, (vii) test / interview about the use of English or other languages, (viii) criminal records.

Question 7: “What are the most important sources of information your company uses when it comes to choosing the less-skilled workers (operators, packers, mechanics, cleaning staff, etc.) to hire? Select the three most important options." This question presented eight options: (i) Curriculum Vitae, (ii) letters of recommendation or references of former employers / teachers, (iii) immediate availability, (iv) test of cognitive, psychometric and / or psychological skills (IQ, emotional intelligence, ability to work in a team), (v) test of knowledge or professional skills related to the job, (vi) evaluation of the work done during a trial period, (vii) test / interview about the use of English or other languages, (viii) criminal records.

Table D.27: Answers to Question 6

Answer	N
Curriculum Vitae	38
Test / interview about the use of English or other languages	29
Test of knowledge or professional skills related to the job	25
Letters of recommendation or references of former employers / teachers	15
Test of cognitive, psychometric and / or psychological skills (IQ, emotional intelligence, ability to work in a team)	14
Evaluation of the work done during a trial period	4
Criminal records	3
Immediate availability	1

Notes: Table D.27 summarizes the answers to Question 6.

Table D.28: Answers to Question 7

Answer	N
Curriculum Vitae	30
Test of knowledge or professional skills related to the job	25
Letters of recommendation or references of former employers / teachers	18
Immediate availability	14
Evaluation of the work done during a trial period	14
Test / interview about the use of English or other languages	11
Test of cognitive, psychometric and / or psychological skills (IQ, emotional intelligence, ability to work in a team)	9
Criminal records	8

Notes: Table D.28 summarizes the answers to Question 7.

Question 8: "Indicate which of the following resources you use to set wages. Select all that apply." This question presented seven options: (i) information provided by CINDE, (ii) information provided by recruitment agencies, (iii) information about wages from the headquarters, (iv) information from surveys about wages, (v) information about wages from the Ministry of Labor and Social Security, (vi) benchmarking with wages in the industry, (vii) other.

Table D.29: Answers to Question 8

Answer	N
Information from surveys about wages	33
Benchmarking with wages in the industry	33
Information about wages from the headquarters	14
Information about wages from the Ministry of Labor and Social Security	12
Information provided by CINDE	10
Information provided by recruitment agencies	5
Other	0

Notes: Table D.29 summarizes the answers to Question 8.

Question 9: "With what frequency do you update the wage brackets?" This question presented four options: (i) quarterly, (ii) semiannually, (iii) every year, (iv) other.

Table D.30: Answers to Question 9

Answer	N
Every year	25
Every semester	12
Other	3
Every trimester	1

Notes: Table D.30 summarizes the answers to Question 9.

Question 10: "In general, for a worker of the same education, with the same number of years of experience and hired in the same occupation, your company pays the same salary or a salary higher than what a domestic company would pay? Please, choose only one option that applies best." This question presented three options: (i) we pay the same wage, (ii) we pay a higher wage, (iii) other.

Table D.31: Answers to Question 10

Answer	N
We pay a higher wage	31
We pay the same wage	9
Other	3

Notes: Table D.31 summarizes the answers to Question 10.

Question 11: "If you chose the option that your company pays a higher salary than a domestic company, please rate the following options from 1 (the most important reason to pay more) to 5 (the least important reason to pay more). If you answered in the previous question that your company pays the same as a national company, you can skip this question." This question presented five options: (i) even if we hire a worker with the same education, experience, and occupation as a domestic company, our company has a better selection filter of workers, and the worker we hire tends to be more competent. Therefore, the worker needs to be paid more, (ii) even if the worker hired by us and by the national company is equally competent, for reasons of equity, the wages we pay to our workers in Costa Rica should be closer to the wages of similar workers in the headquarters or in other subsidiaries of our group, (iii) even if the worker hired by us and by the domestic company is equally competent, the workers of our company must be motivated to work hard. Then, the worker needs to be compensated for that, (iv) even if the worker hired by us and by the national company is equally competent, our company will employ the worker in projects that will generate higher income and where its competence will be better utilized. Therefore, the worker needs to be paid more, (iv) other (please fill in the blank). Please rate this option as number 5 if nothing is filled.

Table D.32: Answers to Question 11

Answer	R=1	R=2	R=3	R=4	R=5
Our company has a better selection filter of workers and the worker we hire tends to be more competent.	11	1	11	6	0
The workers of our company must be motivated to work hard. Then, the worker needs to be compensated for that.	6	11	6	5	1
Our company will employ the worker in projects that will generate higher income and where its competence will be better utilized.	2	12	4	9	2
For reasons of equity, the wages we pay to our workers in Costa Rica should be closer to the wages of similar workers in the headquarters or in other subsidiaries of our group.	5	4	7	9	4
Other: We pay higher wages to motivate and retain talent, to avoid turnover of workers whose training we invest in.	5	1	1	0	22

Notes: Table D.32 summarizes the answers to Question 11. “R=1" means that a given answer has been ranked first out of five options.

C. Summary and discussion of survey answers. **Questions 1 and 2.** Answers to these questions suggest that the main team is mostly formed by locals. These local employees are typically found with the help of recruitment agencies and expatriates who come to Costa Rica to support the first rounds of hiring.

Question 3. To hire the rest of the staff, MNCs use various communication media (e.g., social media, company website, printed press, etc.). MNCs also continue to rely heavily on recruitment agencies. To a lesser extent, MNCs use employment fairs, partnerships with educational institutions, campaigns with municipalities, etc.

Question 4. The most important criteria upon pre-selecting the most skilled workers (e.g., managers, engineers, administrative staff) are: (i) having previous experience in the same job position (33 respondents chose this answer), (ii) having experience in the same industry in which the company operates (29), (iii) the academic studies and the institution where the worker graduated (23), (iv) having experience in personnel management (23), (v) having previous experience in MNCs (20), and last, (vi) having experience working abroad (1).

Question 5. The most important criteria upon pre-selecting the less-skilled workers (e.g., operators, packers, mechanics, cleaning staff) are: (i) having previous experience in the same job position (41 respondents chose this answer), (ii) having experience in the same industry in which the company operates (38), (iii) the academic studies and the institution where the worker graduated (26), (iv) having previous experience in MNCs (23), (v) having experience working abroad (1), (vi) having experience in personnel management (0). Questions 4 and 5 bring a series of insights. First, experience in the same job position and the same industry are the most valuable pre-selection criteria for both high- and low-skilled workers. Second, academic studies and previous experience with MNCs are also important criteria. Last, experience with personnel management is only necessary for high-skilled workers.

Question 6. The most important sources of information used by MNCs when it comes to choosing the most skilled workers (e.g., managers, engineers, administrative staff, etc.) to hire are: (i) the Curriculum Vitae (38 respondents chose this answer), (ii) a test / interview about the use of English or other languages (29), (iii) a test of knowledge or professional skills related to the job (25), (iv) letters of recommendation or references of former employers / teachers (15), (v) a test of cognitive, psychometric and / or psychological skills (IQ, emotional intelligence, ability to work in a team) (14), (vi) an evaluation of the work done during a trial period (4), (vii) criminal records (3), (viii) the immediate availability (1).

Question 7. The most important sources of information used by MNCs when it comes to choosing the less-skilled workers (operators, packers, mechanics, cleaning staff, etc) to hire: (i) the Curriculum Vitae (30 respondents chose this answer), (ii) a test of knowledge or professional skills related to the job (25), (iii) letters of recommendation or references of former employers / teachers (18), (iv) the immediate availability (14), (v) an evaluation of the work done during a trial period (15), (vi) the test / interview about the use of English or other languages (11), (vii) a test of cognitive, psychometric and / or psychological skills (IQ, emotional intelligence, ability to work in a team) (9), (viii) criminal records (8). Questions 6 and 7 bring a series of insights. First, language skills are more important for high- than low-skilled workers. Second, both types of workers receive tests of the knowledge or professional qualifications relevant to the job. Third, letters of recommendation from former employers are useful to the evaluation of both types of workers. Fourth, trial periods are slightly more frequent for low- than high-skilled workers. Last, the immediate availability of low-skilled workers is seen as an advantage.

Question 8. The most frequently used resources to set wages are: (i) surveys about wages (33 respondents chose this answer), (ii) benchmarking with wages in the industry (33), (iii) information about wages from the headquarters (14), (iv) information about wages from the Ministry of Labor and Social Security (12), (v) information provided by CINDE (10), (vi) information provided by recruitment agencies (5). Local wages seem to anchor wage setting for MNCs. That said, HQ's also influence wage setting.

Question 9. Most MNCs update wage brackets (at least) once a year.

Question 10. Most MNCs pay higher wages than domestic firms for a worker of the same education, with the same number of years of experience and hired in the same occupation.

Question 11. MNCs claim to have a better selection filter than domestic firms, meaning that workers hired by MNCs tend to be more competent than workers hired by domestic firms (even if of the same education, with the same number of years of experience and hired in the same occupation). That said, the (unobserved) ability of a worker is not the only explanation for the wage differential (particularly for the within-worker wage differential, which keeps the unobserved ability constant). MNCs pay higher wages also to motivate workers, to retain workers in whom they have invested, because these workers are employed in higher income-generating projects, and last but not least, for reasons of within-MNC wage equity.

D.2.2 Representative Household Survey Data

We use information from a survey conducted by INEC – *"Instituto Nacional de Estadistica y Censos"* or the National Institute of Statistics and Censuses of Costa Rica. This survey – called the *Encuesta Nacional de Ingresos y Gastos de los Hogares* or the National Survey of Household Income and Expenditures (abbreviated as ENIGH) – collects data on the household sources of income and expenditures on goods and services. We use data from the 2018 round, which surveyed a nationally representative sample of 9,828 households. Across these 9,828 households, the survey recorded 3,411 individual tax IDs (the tax ID information not being compulsory). Of these 3,411 tax IDs, 3,034 had the correct number of digits to qualify as a possibly valid tax ID.

We merge the 3,034 ENIGH tax IDs with the tax IDs in the 2017 matched employer-employee data. Of the 3,034 potentially valid tax IDs, 1,316 are found in the 2017 matched employer-employee data. For these 1,316 individuals, ENIGH adds (to the labor earnings information from social security records) information on the number of hours worked, and monetary and in-kind benefits from employment.

We remove the individuals with zero earnings throughout 2017 (one tax ID), retirees (18 tax IDs), self-employed or individuals working for the public sector (525 tax IDs), individuals with special contracts or *convenios* (13 tax IDs), individuals working part-time (37 tax IDs). We are left with 723 individuals. Among the 2,688 individuals dropped from the initial sample (3,411-723), 1,294 declared to be unemployed.