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Topics in Productivity, Exporting and Outsourced Services

DISSERTATION

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by

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To all of you: I hope that I can one day deliver a message that is worthy of what you have given to me.

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ABSTRACT OF THE DISSERTATION

Topics in Productivity, Exporting and Outsourced Services

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This dissertation is a collection of three empirical studies using the a survey of Chilean Manufactures. The first two chapters of this dissertation evaluates the productivity impact of exporting and service outsourcing. The last chapter evaluates selection mechanisms into exporting.

The first chapter examines productivity effect of exporting. I find that asymmetric responses, between exporters and non-exporters, to changes in the domestic lending rate can cause propensity-score-matching-difference-in-differences to underestimate the effect of export entry if these asymmetries are not accounted for in propensity-score-matching. Further, this chapter shows show a significant productivity effect of export entry after allowing unobserved time effects to interact with unobserved plant fixed effects. I find that export entry can cause firms to be as much as 20 percent more productive on average than they would have been if they did not export after I allow these macroeconomic time effects to have different impacts based on treatment status using a newly developed method by Hsiao, Ching, and Wan (2012). This paper highlights the use of general models, such as HCW, as a tool to help researchers test possible misspecification that can arise when unobserved heterogeneity interacts with the macro-environment and the unobserved effects are not netted out with propensity-score-matching.

In the second chapter, I measure the productivity impact of outsourcing business services. Firm level studies on the impact of service adoption have remained sparse and when they do exist, the identification issue of selection is not addressed in a rigorous way. The work in this dissertation identifies the productivity effect of service adoption, while controlling for selection. I find that freight, accounting and advertising service outsourcing can boost productivity but I do not find any productivity effects for foreign technical support.

The last Chapter evaluates the selection effect into exporting. I cannot find any evidence showing that productivity alone causes export entry. However, I do identify entrepreneurial type and business service outsourcing as two factors that increase productivity as well as increase the likelihood of entering foreign markets.

CHAPTER 1: The Productivity Effect of Export Entry

Introduction

This paper uses a Chilean panel of manufactures and shows a positive treatment effect of exporting on productivity when latent macroeconomic factors are allowed to interact with plant heterogeneity using a new method proposed by Hsiao, Ching and Wan (2012). Secondly, this paper shows that different access to liquidity can bias PSM-DID estimates if these differences are not accounted for in the estimation of the propensity score. My findings stand in sharp contrast to a large literature that finds little or no productivity effect of export entry, Wagner (2012). My findings also stand in contrast to previous work on the Chilean economy that use revenue based productivity and account for unobserved firm heterogeneity[1].

The unique findings in this paper were made possible by a newly developed methodology by Hsiao, Ching, and Wan (2012), HCW hereafter. To understand the novelty of this methodology, one must notice that we cannot simply compare productivity before and after export entry due to the existence of unobserved macroeconomic factors or time fixed effects. Domestic demand shocks, credit market reforms, institutional changes, infrastructure development can all cause exporters' productivity to change, independent of the treatment of exporting. At the same time, plant level heterogeneity, or plant fixed effects, which might be unobserved, can also drive productivity differences. For example, plants can have different quality management, access to credit markets or know-how. One way to control for these unobservables is to assume that plant specific unobservables are independent of latent macroeconomic factors. In other words, it is common to assume that latent macroeconomic factors have quantitatively the same influence on exporters and non-exporters, or that these two types of fixed effects are additively separable. Then taking simple differences across plants cancels out the macroeconomic factors while taking differences within each plant cancels out unobserved plant level heterogeneity. Thus Difference-in-Differences, DID hereafter, nets out two types of unobservables.

[1]Garcia and Voightlander (2013) show a positive effect by showing that marginal costs drop with prices while quantities rise after entry. The first paper on the Chilean economy by Alverez and Lopez (2005) do not account for unobserved firm heterogeneity, or biases that result from selection - both of which present a major challenge in identification

However, there are reasons to believe that additive separability may not appropriately characterize many differences between exporters and non-exporters. Better managers might be able to better manage the business cycle and at the same time are more likely to export. Differences in access to capital will effect how plants respond to changes in financial markets. In general, different exposures to the domestic market can have a different effect on plants who operate exclusively in the domestic market compared to those who also operate in international markets. The HCW method can allow for *any* unobserved plant fixed effect to interact with *any* unobserved time fixed effect. Alternatively it can also allow *any* group level fixed effect to interact with *any* unobserved time fixed effect.

For example, consider that the commercial lending rates greatly improved during the post-treatment periods and fell from 49 percent to 22 percent over the course of 6 years. At the same time, there is evidence that Chilean exporters are less credit constrained than non-exporters. Alverez and Lopez (2014) show evidence that Chilean plants who have access to banking finance are more likely to be selected into exporting. This finding is supported in other empirical studies as well as by theory proposed by Muuls (2015). It is less clear how these asymmetries in credit access might effect the productivity of exporters versus non-exporters. On one hand, exporters might have a higher need for credit in conducting international trade and therefor might be more sensitive to interest rate changes. On the other hand non-exporters might have less access to credit than exporters and thus be less able to fund productivity augmenting investment projects. It seems that the more constrained groups should be more effected by interest rate changes. I find evidence of the later; that productivity and investment of non-exporters are more sensitive than that of exporters to changes in interests rates.

When exporters are less credit constrained than domestic firms and domestic capital markets improve, then the productivity of domestic firms who are more constrained can rise relative to firms who have access to international financial markets, all else constant. In such a case, the productivity impact of exporting, can be biased downward if we assume that unobserved factors are additively separable from plant level heterogeneity and propensity-score-matching is imperfect.

If the differences in relative response to interest rate fluctuations are not included in propensity-score-matching, then DID estimates might underestimate the treatment effect of export entry but to what extent? To answer this, I fit two PSM-DID models. In the first, I estimate a standard model that matches on lagged levels of various plant level observable characteristics, but that does not include a measure for liquidity. In the second, I include two new dimensions in the matching algorithm. First, is an identifier of lender status. Some firms are able to lend while others are not. The intuition is that lenders are more likely to have higher liquidity than non-lenders. Second, I create a simple metric to capture plant specific responses to interest rate changes and include this metric in the propensity score. Under the new specification, the DID estimate of the treatment effect of export entry is 7.6 percent and significant. Interpreted as a growth rate, then heterogeneous responses to interest rate changes can explain 96 percent of the HCW estimates at the 2 year horizon.

My findings add to the growing list of explanations why previous studies have not found a treatment effect of export entry. Garcia and Voightlander (2013) show that exporting reduces marginal costs but firms can respond by lowering prices. If productivity gains are passed onto consumers in the form of lower prices then a revenue based measure of productivity might be a negatively biased estimate of true productivity. Atkin, Khandelwal, and Osman (2014), show us that productivity gains can be embodied in a quality measure, whereby exporting can cause firms to produce a better quality good. My paper shows that allowing plants to heterogeneously respond to latent factors can also produce new results and guide further research. In the case of Chilean manufactures, accounting for heterogeneous access to liquidity can produce new results. Taken as a whole, we should be cautious when weighting previous studies that show exporting to have little or no impact on productivity, especially in light of new research that suggests mechanisms of selection that were unaccounted for in previous studies.

The rest of the paper is organized as follows Section 2 briefly surveys the literature, Section 3 describes the data, Section 4 defines the problem of selection and provides a comparative methodological overview of the HCW method as well as PSM-DID, Section 5 delivers the results and Section 6 concludes.

Literature Review

The trade literature has long noted that exporters are systematically different than non-exporters, Bernard and Jensen (1999). Many of these differences have been attributed to selection. Surprisingly, most existent empirical studies fail to find supporting evidence for the treatment effect of exporting on productivity, Wagner (2007). Many authors such as Clerides, Lach, and Tybout (1998) and Bernard and Jensen (1999) find productivity gains after exporting to be negligible. Ranjan and Raychaudhuri (2011) show evidence of a positive treatment effect in India using propensity-score-matching. De-

Loecker (2007) finds some supporting evidence of a positive effect with a Slovanian panel as do Garcia and Voightlander (2013). However the consensus in the literature shows little or no effect.

Methodologically, selection is a formidable obstacle in identifying the treatment effect of export entry. Combining propensity-score-matching and difference-in-differences is one strategy to deal with selection on unobservables. However, these estimates are generally biased if unobserved plant fixed effects and time effects interact, Gobillon and Magnac (2013). This leaves open the possibility of selection on unobservables as a continuing problem under a PSM-DID strategy.

At the same time, the trade literature has expanded on the topic of selection. One of these branches, expands the Melitz (2003) model and asserts that financial constraints are an important driver of firm selection into exporting Muuls (2015). This follows the theoretical work of Manova (2013) and Chaney (2005). Several plant level studies have explored various country panels including Chile, Alverez and Lopez (2014), who provide supporting evidence that financial constraints are an important driver of selection into exporting.

New research on selection into exporting should motivate a re-examination of previous studies on the treatment effect of export entry that leaned on propensity-score-matching. Omitting these mechanisms can violate the identifying assumptions of matching methodologies that have been popular in this literature. For example, this paper shows that omitting access to liquidity in the calculation of the propensity score can negatively bias PSM-DID estimates for this Chilean panel.

Finally, the evidence in this paper, that export entry augments a revenue based measure of productivity is also unique. Previous Chilean research by Alverez and Lopez (2005), did not directly control for selection even though they noted its existence in the Chilean panel. Garcia-Voightlander (2013) controlled for selection and find no evidence of a treatment effect on revenue measured productivity using a Chilean panel. Although they do find evidence that costs fall with prices, leaving markups relatively constant while outputs rise. This brings evidence why previous studies found little or no effect as well as offers new evidence of a positive treatment effect. This paper does not offer an explanation that competes with Garcia and Voightlander's explaination. It is quite possible that my methodological approach combined with their productivity measure might yield an even larger effect than is reported in both studies although such effort will be saved for later work.

Data

A panel of plants is compiled from the Encusta Nacional Industrial Annal (ENIA) survey conducted by the National Census Bureau of Chile. The survey covers all manufacturing establishments that have at least 10 employees. It provides detailed information on sales, employment and related measures, various input spendings, capital, inventory, and the revenue from exporting. The panel spans the years from 1984 to 1996, and contains about 5,000 plants on average per year with a total of 86,186 plant-year observations.[2] Industry classification is available at the four-digit level of International Standard Industrial Code (ISIC). The data set contains entry/exit dummies that equals one for the year that a plant enters/exits.

As the first step, I identify exporters v.s. non exporters. An important feature of the data is that information on exporting is not available until 1990. This raises the question of how to deal with missing information on exporting before 1990. One could delete all data before 1990, which would severely shorten the series. Instead, I drop off only plants that ceased production before 1990, since they have an equal chance to be exporters or non-exporters. In other words, I keep in the data set observations of plants that are in operation beyond 1990, either they entered before or after 1990, and identify exporters as those that report positive exporting revenue post 1990; the rest are taken as non exporters. Note plants that report zero exporting revenue after 1990 are identified as non-exporters even if some of them may have exported for a while but quited exporting later prior to 1990: the number of such plants is fairly small and should not influence the results according to Bernard and Jensen (1999).

The starting year of exporting is identified as follows. For plants whose first year of exporting is shown as 1990, their starting year of exporting is counted as missing, because one cannot determine whether they started exporting before or on 1990. For plants whose first year of exporting is shown as after 1990, it is possible that they started exporting before 1990 without being observed, stopped exporting for a while, and resumed exporting later. This possibility is evaluated by comparing the length between 1990 and their observed export-starting year with the average length of non-exporting years after their first observation on exporting. If the former is longer than the latter, then this plant's starting year of exporting is identified as the first year that reports positive revenue from exporting;

[2]The original data set starts from 1979. However, a large recession in 1982 causes potential problems with forecasting. Therefore, observations prior to 1984 were dropped.

it is recorded as missing otherwise. I estimate plant-specific productivity applying the estimators of Levinsohn-Petrin (LP) (2003) following the mass in the literature that is reviewed by Wagner (2007). See Appendix for details.

Selection - Motivation and Theory

Motivating Evidence

Estimating the treatment effect of export entry is confounded by a large problem of selection on unobservables. The selection of certain types of plants into exporting is well established in the literature; exporters are more productive, skill intensive, pay higher wages and are more capital intensive on average than non-exporters. Further, these differences arise well before exporting occurs. This raises problems with identification because the counterfactual outcome, the productivity of exporters had they not began to export, would likely be different than that of non-exporters, see Angrist and Pischke (2008) for a discussion. Differences between exporters and non-exporters as well as the selection effects can be seen by running the following simple regression under different specifications of D_i^x:

$$C_{it} = \gamma_0 + \gamma_1 D_i^x + \Gamma_x X_{it} + \epsilon_{it} \tag{1}$$

where C_{it} represents a single element of the set; LP measure of productivity-tfp; relative employment of white collar vs. blue collar workers; relative wage; total wage bill; value added per worker (man/year) and capital per worker[3]. X_{it} is a set of controls including a set of industry dummies and year dummies.

D_i^x will be defined in three ways. To show simple differences by export status, define $D_i^x = D_i$ as an ever-export dummy that equals one if a firm ever exports for at least a year (either in the past, or at the present, or in the future) and equals zero otherwise. The corresponding results of the OLS estimate of γ_1 is reported in table 1 for each plant characteristic. In other words, γ_1 is estimated individually for each characteristic, resulting in six regressions, one fore each characteristic. This shows that exporters have different characteristics on average than non-exporters.

[3]See Appendix for a discussion on the LP productivity measure

These differences could arise from either selection or because of entry into the export market. The contribution of selection can be obtained by re-defining $D_i^x = D_{it}^B$, a selection dummy that is equal to one if plant i exports at a future point in time, defined t_e and $t < t_e$. It is undefined if $t \geq t_e$. If plant i never exports then $D_{it}^B = 0$. The point estimates of γ_1 reveal that differences between exporters and non-exporters arise before export entry.

Now let us consider the standard Melitz assumption, that productivity differences are endowed at the time of birth. To do this, redefine $D_{it} = D_{i,t^b}$. Let $D_{i,t^b} = 1$ if plant i was born in year t^b, and $t = t^b$ and $t < t_e$. $D_{i,t^b} = 0$ if plant i was born in year t^b and $t = t^b$. It is undefined if $t \neq t^b$. The patterns in the first year of plant operation are similar to the pre-exporting selection effects, with one exception that future exporters do not appear to be born more skill intensive than non-exporters. If selection is a characteristic of birth then the factors that drive the selection process are very likely to be unobservable because the causal process that drives selection is likely to occur in a time period before data is likely to be available. The fact that differences arise during the first year should motivate researchers to employ methods that control for selection on unobservables. It is not clear that conditioning on these selection effects will capture the underlying mechanism of selection.

Table 1: Firm Characteristics by Activity

	(1)	(2)	(3)	(4)	(5)	(6)
$C_{it} =$	tfp	white / blue	relative wage	wages	VA/L	K/L
$D_{it} = D_i$	0.812***	0.0880***	0.565***	0.615***	0.817***	1.230***
	(0.00954)	(0.00921)	(0.00706)	(0.00604)	(0.0112)	(0.0147)
$D_{it} = D_{it}^B$	0.723***	0.127***	0.477***	0.517***	0.776***	0.907***
	(0.0187)	(0.0168)	(0.0145)	(0.0122)	(0.0219)	(0.0280)
$D_{it} = D_{i,t^b}$	0.449***	0.0398	0.519***	0.277***	0.448***	0.809***
	(0.0823)	(0.0860)	(0.0728)	(0.0581)	(0.103)	(0.132)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: this table reports the OLS estimates on γ_1 of $C_{it} = \gamma_0 + \gamma_1 D_{it} + \Gamma_x X_{it} + \epsilon_{it}$. The regression is done individually for each C_{it}, which is a firm level characteristic described by each of the column headings. All characteristics are in logs. In the top section: D_{it} equals 1 if plant i ever engages in exporting and equals zero if plant i will never export. In the second section: $D_{it} = D_{it}^B$ equals 1 if plant i will export in the future and equals zero if plant i will never export. In the last section: $D_{it} = D_{i,t^b}$ equals 1 if plant i will be an exporter and the year is equal to the first year any data is observed for this firm. It equals zero if plant i will never export and the year is equal to the first year any data is observed. X_{it} is a set of controls including industry dummies and year dummies. Robust standard errors are in parentheses. * indicates $p < 0.05$, ** indicates $p < 0.01$, *** indicates $p < 0.001$. See text for more details.

In light of these systematic differences, I would like to measure the productivity effect of entering the export market, hereafter called the treatment effect. It should be clear, from Table 1, that accounting for selection is a formidable challenge. One popular approach is to match a treated plant with a non-exporting plant that is very similar, this is called a matching strategy. If it were possible to find a set of control plants that are identical to export entrants, with the sole exception that they never began exporting, then measuring the effect of export entry would be straight forward. Unfortunately, there are two challenges in implementing this matching strategy. First is the curse of dimensionality as it becomes increasingly difficult to match on a high dimension. This problem is mostly resolved with propensity score matching (PSM) proposed by Rosenbaum and Rubin (1983).

The second, larger problem is that researchers can never be certain if there are dimensions that are left unaccounted for by matching. This problem can be stated as selection on unobservables. Systematic differences between exporters exist from the first year of plant operation. However, it is not clear that simply conditioning on these differences captures the underlying mechanism. Thus unobservables can pose a large identification problem. Fortunately, there are ways to address this problem. A common approach is to use Difference-in-Differences which assumes that unobservables are plant fixed effects and time fixed effects that do not interact. However, if these two types of effects interact then DID estimates are generally biased, Gobillon and Magnac (2013).

Selection on Observables

To formalize ideas, let us define two sets of factors, a vector of idiosyncratic factors $\boldsymbol{g_{it}}$, either observable or unobservable and a vector of common unobservable latent factors $\boldsymbol{f_t}$, that together drive plants' productivities to grow over time. $\boldsymbol{f_t}$ represents common factors and can be national economic growth, capital or labor market conditions, judicial reforms, weather, environmental improvements, or exchange rate fluctuations; $\boldsymbol{g_{it}}$ can be plant age, manager's ability, plant's capacity to adopt new technologies, location, or input usages. Idiosyncratic factors, $\boldsymbol{g_{it}}$, will influence how plant i is influenced by changes in $\boldsymbol{f_t}$; let the vector $\boldsymbol{b_i}$ capture this relationship. In other words, let $\boldsymbol{b_i} = h\left(\boldsymbol{g_{it}}\right)$, where $h(\bullet)$ is an function that links idiosyncratic factors $\boldsymbol{g_{it}}$ to responses $\boldsymbol{b_i}$. Consider this general model and suppose exporting treatment occurs only starting from T_i^B so that $t \leq T_i^B$ corresponds to the ex-ante exporting period.

$$z_{it} = \boldsymbol{x_{it}\beta} + \boldsymbol{b_i f_t} + u_{it}, \qquad t = 1, ..., T_i^B - 1 \tag{2}$$

$\boldsymbol{x_{it}}$ is a set of industry and year controls. u_{it} is an error term for the i^{th} plant. Firm specific fixed effect, can easily be specified by making one element in $\boldsymbol{f_t}$ to be equal to one. For example, define $\boldsymbol{f_t} = [1, f_1, f_2 ... f_F]$. Starting from T^B, a plant may or may not export. Define $d_{it} = 1$ if the plant exports and $d_{it} = 0$ otherwise, so that:

$$z_{it} = \boldsymbol{x_{it}\beta} + \boldsymbol{b_i f_t} + d_{it}\tau_{it} + u_{1t}, \qquad t = T_i^B, ..., T \tag{3}$$

Apparently, τ_{it} denotes the causal (treatment) effect of exporting on plant productivity. If d_{it} is exogenous – in other words – the assignment of exporting treatment is random across all plants, one can estimate $\tau = E(\tau_{it})$ by comparing productivities of exporting plants with those of non-exporters controlling for other factors. However, the assignment of d_{it} is never random. Hereby, d_{it} and u_{it} can be correlated: for example, plants with a smart manager is more productive and at the same time more likely to export. Moreover, plants with a smart manager can be more active in adopting new technologies while exporting, which intensifies the treatment; therefore, τ_{it} and u_{it} can be positively related. The endogeneity of d_{it} and τ_{it} would likely cause upper bias in the estimate of τ_{it}. However, I will show later that there is far more empirical support for a negative bias in this Chilean data set.

Propensity Score Matching is one way to control for the endogeneity that is caused by selection on observables. The goal is to identify control plants with characteristics close enough to the exporting plants and use their post-treatment outcome as counterfactuals to control for the causality of firm characteristics, g_{it}, on export status d_{it}. This method maintains that all firm characteristic, $\boldsymbol{g_{it}}$, that drive selection are observable. Rosenbaum and Rubin (1983) show that, instead of matching each dimension of plant-specific characteristics $\boldsymbol{g_{it}}$, one can simply match a one-dimensional propensity score $p(x_i)$ as a plant's propensity to be exposed to the treatment. The propensity score is estimated by fitting a simple logistic regression model.

$$d_{it}^* = F(\boldsymbol{g_{i,t-1}}, z_{i,t-1}) \qquad t = 1, ..., T_i^B \tag{4}$$

where $\boldsymbol{g_{i,t-1}}$ are lagged plant characteristics, and $z_{i,t-1}$ is lagged productivity[4]. Plant characteristics that are included in this estimation are, capital, materials, labor and of course we also include productivity, which is motivated by the Melitz idea that there is selection on productivity. $F()$ is a logistic cumulative distribution function. d^*_{it} is not the standard export dummy d_{it}. d^*_{it} is defined in the following way:

$$d^*_{it} = \begin{cases} 1 & \text{if } \ t = T_i^B \\ 0 & \text{if } \ t < T_i^B \ \text{ or } \ d_{it} = 0 \\ (.) & \text{if } \ t > T_i^B \end{cases} \tag{5}$$

The purpose of defining it this way is to capture the likelihood of starting to export. If we simply used d_{it}, which equals 1 for any year that the plant is exporting then we would be measuring the likelihood of continuing to export as well.

Next, I estimate $\hat{d^*_{it}}$ from Equation 4, which I will define as the p-score, denoted $\hat{p_{it}}$. A number of different matching options are available. Here, I match a treatment plant with a single control plant that has the closest p-score. The matching is done with replacement and defines a matched pair, whose relationship will be stable over the sample period. The purpose here is not to use propensity-score-matching alone but to pair it with difference-in-differences as well as into the Hsiao Ching and Wan estimator. One-to-one nearest neighbor matching makes the integration straight forward. All matches were on common support so there was not a need to drop any plants from the sample.

Matching on the p-score has the effect of equalizing average differences in characteristics, that are included in the estimation of the p-score, $\boldsymbol{g_{it}}$ and z_{it} in this case. The unmatched pairs have clearly different group averages that mirror those reported in Table 1. The matched, treatment and control groups have means whose differences are statistically insignificant from zero.

It is common to test if there is equalization of mean characteristics between treatment and control groups as a way to show a "quality" in matching. However, this test has serious limitations. First, it only shows that there is an equalizing effect on the characteristics that are included in matching but it can't say anything about dimensions that are omitted. An identifying assumption of this method is

[4] Alternatively, I include average pre-exporting values in place of lagged values. Including lagged values causes the entire plant to be dropped if the lagged year is missing, even if there is plenty of information in other years. Taking averages of past values thus allows for a larger sample. Further, lagged values can suffer from endogeneity if managers anticipate entering the export market in previous years. Such an effect would be reduced but not eliminated by averaging.

that selection into treatment is equivalent to random assignment after controlling for $\boldsymbol{g_{it}}$. This would only be true in theory if the propensity score is properly and fully specified[5]. Secondly, there might only be a small proportion of plants who are driving the average effect of treatment yet equalizing group average characteristics gives each plant an equal weight. We can't say much about the quality of the match for the plants who might matter the most.

Selection on Unobservables

Difference-in-Differences Estimator

Propensity score matching is unlikely to resolve the endogeneity created by omitted variables in Equation 3. Thus performing Difference-in-Differences on the matched pairs gains ground under two assumptions. First DID assumes that these omitted variables are fixed effects of two varieties: one that represents firm heterogeneity, defined $\tilde{b_i}$ and another that represents effects that are invariant across firms for a given time, denoted $\tilde{\boldsymbol{f_t}}$. A further assumption is that these effects are separably additive. These assumption amount to assuming that $\boldsymbol{f_t} = [1, \tilde{\boldsymbol{f_t}}]'$ and $\boldsymbol{b_i} = [\tilde{b_i}, 1]$. Under these assumption we can re-write our general model in Equations 2 and 3 as follows:

$$z_{it} = \boldsymbol{x_{it}\beta} + \tilde{b_i} + \tilde{\boldsymbol{f_t}} + u_{it}, \qquad t = 1, ..., T_i^B - 1 \tag{6}$$

$$z_{it} = \boldsymbol{x_{it}\beta} + \tilde{b_i} + \tilde{\boldsymbol{f_t}} + d_{it}\tau_{it} + u_{1t}, \qquad t = T_i^B, ..., T \tag{7}$$

Under this data generating process, the DID estimator yields the average treatment effect of the treated, ATT hereafter. DID can be combined with PSM by using the control-treatment group pairs as defined by propensity score matching. DID-PSM thus controls for selection as well as for unobservable common factors.

The DID estimate of the ATT of export entry is obtained by running the following simple OLS regression:

$$z_{it} = \gamma_1 D_t + \gamma_2 D_i^m + \gamma_3 D_i^m D_{it} + \Gamma_x X_{it} + \epsilon_{it} \qquad t = 1, ..., T \tag{8}$$

[5] The validity of HCW estimates does not depend on selection. The HCW results will be robust even when PSM is not used as I will show later in the paper. See HCW (2012) for details.

z_{it} is the LP productivity; X_{it} is a set of other exogeneous industry and year controls. D_i^m equals 1 if plant i is an export starter and zero if plant i is a non-exporter; it is undefined when export entry is not observable. D_{it} equals 1 if $t \geq T_i^B$ and zero otherwise. In this setup, D_{it} controls for any common time variations caused by latent factors indicated as $\boldsymbol{f_t}$ in Equations (2) and (3). D_i^{psm} controls for any unobservable initial difference between treatment and controls that have not been captured by PSM. Combining one-to-one nearest-neighbor-matching, with DID amounts to specifying D_i^{psm} to be equal to one if plant i is an export starter and equal to zero if plant i is a non-exporter that has been matched. It is undefined for plants who were not matched. Hence, γ_3 is the parameter of interest – the DID estimator for the treatment effect of exporting.

Equation (8) is estimated over the matched sample for each export-entry cohort – 1991, 1992, 1993 and 1994 as well as for a pooled sample[6]. When $m = PSM$ then controls are defined by PSM, if empty then PSM is not used. Matching is done with replacement so many control plants are matched multiple times. Weights must be used to reflect this in DID estimation or else the control group will be under-represented.

Finally, notice that Difference-in-Differences on the original specification, Equation 2 and 3, that includes interactive effects, $b_i f_t$, yields the following (ignoring $x_{it}\beta$ without loss of generality)[7]

$$ATT^{DID} = \left(\frac{1}{N_{T^a}} \sum_{t>T^B} f_t - \frac{1}{N_{T^b}} \sum_{t<T^B} f_t \right) \left(\frac{1}{N^T} \sum_{i=1}^{N^T} b_i - \frac{1}{N^C} \sum_{i=1}^{N^C} b_i \right) + \bar{\tau} \tag{9}$$

Where N_{T^b} denotes the number of time periods before treatment, N_{T^a} is the number of periods after treatment,N^T is the number of treated plants and N^C represents the number of controls. Equation 9 tells us that DID is consistent if exporters are on average influenced the same as non-exporters by common macro-economic factors. This interpretation of DID estimates will be helpful in motivating the Hsiao Ching and Wan estimator that allows for exporters and non-exporters to respond differently on average to changes in $\boldsymbol{f_t}$.

[6]The treatment and control group pairs were forced to be strongly balanced as a precaution against biases that might arise because of systematic differences in reporting. Balancing the panel did not have a qualitative effect on the estimates

[7]This was derived with the simple DID expression $\left(\bar{z_a}^T - \bar{z_a}^C\right) - \left(\bar{z_b}^T - \bar{z_b}^C\right)$. Where averages are over number of plants as well as periods.

Allowing Interactive Effects $\boldsymbol{b_e f_t}$ using HCW

It may be unreasonable to assume that exporters and non-exporters respond similarly to macroeconomic factors. To capture these differences I allow $\boldsymbol{b_i}$ to be different by export status, $\boldsymbol{b_i} = \boldsymbol{b_e} = [\boldsymbol{b_1}, \boldsymbol{b_0}]$. Define e as an exporter identifier that is equal to one if plant i is an export starter and zero if plant i never exports. This application will be appropriate for measuring average treatment effects of export entry, since we are concerned about differences in $\boldsymbol{b_i}$ that vary systematically between exporters and non-exporters[8]. Consider $\boldsymbol{f_t}$ as the arrival of a new technology that is not directly related to exporting, plants with smart managers should be more effective in adopting technological improvements and thus bear a bigger influence of such improvement while, at the same time, they are more likely to export. In this case, certain magnitudes of the productivity differential between exporting plants and their controls are driven by the technological improvements ,$\boldsymbol{f_t}$, as well as the difference in their ability to adopt such improvements, $\boldsymbol{b_i}$. The selection of better managers into exporting can cause $\boldsymbol{b_i}$ to vary systematically between exporters and non-exporters thus causing differences in $\boldsymbol{b_e}$. DID estimates will be generally biased if $\boldsymbol{f_t} \neq [1, \tilde{\boldsymbol{f_t}}]'$ and $\boldsymbol{b_i} \neq [\tilde{b_i}, 1]$ Gobillon and Magnac (2013).

An alternative approach to DID is to allow interactions between $\boldsymbol{f_t}$ and $\boldsymbol{b_i}$ and estimate Equations 2 and 3. One way is to fit a factor model as proposed by Bai and Ng (2002) as long as both N and T are sufficiently large. Another way is offered by Hsiao Ching and Wan (2012), who propose an approach that requires less information. Their approach abstracts away from the structure on the right-hand-side of Equations 2 and 3 with the purpose to estimate counterfactual outcomes; in this case, the productivity of exporters had they not started to export. Their method takes advantage of the fact that $\boldsymbol{f_t}$ is common between both treatment and control group even if the influence $\boldsymbol{b_i}$ is not. Thus common factors across plants cause the outcomes of treatment and control plants to be correlated while $\boldsymbol{b_i}$ establishes the degree of correlation. The HCW approach estimates the *correlations* between treated and control groups using the pre-treatment sample, and then construct the post-treatment counterfactuals using the estimated correlations. Put differently, the HCW approach predicts the post-treatment counterfactuals of the treated based on the post-treatment observables of the controls

[8]In another paper, I allow $\boldsymbol{b_i}$ to vary by plant. The point estimates of the treatment effect of export entry $\hat{\tau}$ are very close to those reported in this paper. Estimation of $\boldsymbol{b_e}$ instead of $\boldsymbol{b_i}$ is used in this paper for two reasons, 1) I wanted to take advantage of the additional power in the cross-section given the relatively short time dimension of the data and 2) The object of interest is an average effect, where we are concerned about average differences between exporters and non-exporters.

and the pre-treatment correlations between the treated and control groups. The treatment effect is estimated by comparing the observed outcome of the treated with the predicted outcome of the treated in the absence of the treatment.

The HCW approach is implemented by first running the following regression over the pre-exporting sample:

$$z_{it^*}^T = \gamma_0 + \Gamma_z z_{it^*}^c + \boldsymbol{\Gamma_c C_{it^*}^c} + \Gamma_x X_{t^*} + \epsilon_{it^*} \qquad t^* = 1, ..., (T_i^* - 1) \tag{10}$$

where $z_{it^*}^T$ is the productivity of future exporters, $z_{it^*}^c$ is the productivity of control plants, $\boldsymbol{C_{it^*}^c}$ is a vector of control plants' characteristics[9]. The covariates in $\boldsymbol{C_{it^*}^c}$ will be chosen from the set of $\mathbb{C}$. In other words, $\boldsymbol{C_{it^*}^c} \subseteq \mathbb{C}$. The selection will be such that a goodness-of-fit criteria is maximized. The full set of $\mathbb{C}$ will include the control plant's productivity, real investment, real capital stock, real material usage, and industry interaction terms. X_{t^*} is a vector of industry and year controls.

Estimation of Equation (10) establishes the correlation pattern of exporters and non-exporters before exporting begins. Then a forecast for the ex-post counterfactual productivity of exporting plants in the absence of exporting, denoted $z_{it^*}^0$, is estimated by combining the pre-treatment point estimates together with the post-treatment observed outcomes of the control plants.

$$\hat{z}_{it^*}^0 = \hat{\gamma}_0 + \boldsymbol{\hat{\Gamma}_c C_{it^*}^c} + \hat{\Gamma}_z z_{it^*}^c + \hat{\Gamma}_x X_{t^*} \qquad t^* = T_i^*, ..., t^* \tag{11}$$

The Average Treatment Effect of the Treated is then estimated simply by subtracting the counterfactual outcomes from the observed outcomes and then averaging across plant:

$$ATT_{t^*}^{HCW} = \frac{1}{N} \sum_i (z_{it^*}^T - \hat{z}_{it^*}^0) \tag{12}$$

HCW Procedure: The procedure in which I implement HCW in this paper is as follows:

1. Calculate tfp using Levinsohn-Petrin (2003)
2. Perform propensity-score-matching, as defined in the previous section and define a set of matched treatment-control pairs.

[9] The timing is adjusted so that all cohorts can be pooled together by defining $t^* = t - t_s$, where t_s is the year of export entry for entering cohorts 1991-1994. Thus t^* can be thought of as a distance from the event horizon.

3. Select a pooled model that maximizes a goodness-of-fit criteria. Here I will maximize adjusted R^2. The procedure is described in detail in the next paragraph.

4. Estimate the HCW model in step (3) for the pre-exporting period, Equation 10.

5. Forecast the productivity of exporters using the point estimates that were obtained in the pre-exporting estimation combined with observed data in the post-exporting period, Equation 11.

6. Calculate the ATT, Equation 12.

7. Bootstrap the entire procedure

Goodness-of-fit One of the requirements of HCW is that the model is optimized on a goodness-of-fit criteria. This means that researchers should pick control plants and characteristics that maximize a metric such as R^2, adjusted-R^2 or a bayesian information criteria (BIC)[10]. Note that I am not concerned about recovering consistent point estimates in the fitted model. Rather, I seek a model that is good at predicting the productivity of our treated plants, without saying anything about the right-hand-side of my model.

The search for an optimal model creates a curse of dimensionality, with exception only in cases with a very small number of plants. I overcome this curse of dimensionality by limiting my search to characteristics of the matched control plant that is defined in propensity-score-matching. The idea is that plants that have similar characteristics would also have similar co-movements to the macroeconomy. Figure 1 shows that matching does improve the in-sample fit compared to when the control group is randomly assigned. However, the control group does not need to be similar to the treatment group. It is entirely feasible that plants that are very different can have a high degree of co-movement. The process is still computationally burdensome, even when limiting the search space. A need for automated model selection becomes immediately clear once we consider that the entire process must be bootstrapped, which requires that model selection must be done for every bootstrapped draw.

Automated model selection is provided by code that is made available by Lindsey and Sheather (2010). The model selection process is conducted as follows:

[10]The criteria makes little difference. However, in general, the BIC penalizes over-fitting the most, followed by adjusted-R^2 and then R^2.

Model Selection Procedure

1. Take one element, defined as C_e from the set of control plant characteristics, $\mathbb{C}$. Define N_c to be the number of elements in $\mathbb{C}$. Now estimate a one element version of Equation 10 N_c times individually for each element in $\mathbb{C}$.

$$z_{it^*}^T = \gamma_0 + \Gamma_z z_{it^*}^c + \Gamma_c C_e + \Gamma_x X_{t^*} + \epsilon_{it^*} \qquad e = 1, ..., N_c$$

2. Pick the element that produced the highest adjusted-R^2, defined as C_e^*. Now estimate a two element model $(N_c - 1)$ times.

$$z_{it^*}^T = \gamma_0 + \Gamma_z z_{it^*}^c + \Gamma_c^* C_e^* + \Gamma_c C_e + \Gamma_x X_{t^*} + \epsilon_{it^*} \qquad e = 1, ..., N_c$$

3. Repeat iterative process until a model with the entire set of covariates is included.

4. Pick the model that has the highest adjusted R^2. Define the set of covariates in this model to be $\boldsymbol{C_{it^*}^c}$, which is reflected in Equation 10.

The optimal model is then used to recover the counterfactual outcome: the productivity of export starters had they never exported. The benefit of the HCW method is that we have effectively avoided identification issues that are caused by selection into exporting. However, a new set of identifying assumptions is needed for HCW to be consistent.

Identification Assumptions: Besides goodness-of-fit, the following assumptions must also be maintained within this HCW framework:

1. The pre-treatment period must dominate the forecast period.

2. The idiosyncratic component, b_i, which describes the influence of common factors, is assumed to be time consistent.

3. Export entry does not cause non-exporters to become more or less productive.

I impose the following restrictions to address (1), given that there is not a formal definition of domination. First, I limit the forecast of the counterfactual outcome to a maximum of two years. Two years also is the maximum forecast period for the 1994 cohort since the panel ends in 1996. Secondly, I drop plants who have less than 3 years of data pre-exporting. These restrictions effectively make the pre-treatment period greater than the post-treatment period for every plant.

For purposes of mitigating Assumption (2), I pick a sample period that is free of recessions. For Chile there was a recession in 1982 and a recovery by 1984. This was followed by long period of stable economic growth that concluded in 1997 when the Asian financial crisis found it's way to Chile. This makes the years 1985-1996 the most attractive to ensure time stability of the parameter space[11].

The robustness of Assumption (3) could be checked by selecting control plants that are in a remote geographic location or in a different industry. The forecast period and rate of technological dissemination will also influence the reasonableness of Assumption (3). The longer the forecast period and the faster the rate of technological diffusion, the less likely that Assumption (3) will hold. In general, there could be either positive or negative spill-overs from export entry. Positive spillovers might occur if technology or know-how is transferred from the foreign market to exporters who then transfer it to domestic firms. This would likely cause a negative bias for the treatment effect of export entry. On the other hand, the competitive impact of exporting might negatively effect domestic plants productivity. There certainly are channels by which export entry can spill-over into factor markets but it is less clear if these changes in factor markets have productivity implications. Productivity should be the part of production that cannot be explained by factors of production. However, the treatment effect of export entry will likely be overestimated to the extent that export entry has negative spillovers on domestic firms. This is because negative spillovers cause the productivity of domestic firms to be lower than in the counterfactual state of no exporting. A lower level of productivity for control firms should cause a lower predicted counterfactual, $\hat{z}^0_{it^*}$ since the co-movement of productivities is usually positive in this economy. This causes an overestimate of the treatment effect τ.

[11] The military dictator Pinochet lost an election in 1990 and capital market reforms followed. This might have caused structural reforms in the credit market but probably not in the openness to international trade. Pinochet was installed by a CIA backed coup in 1973 and had already made many progressive reforms to open up the country; reducing the tariff from 105 percent in 1974 to only 12 percent in 1979. Other reforms including laissez-faire microeconomic policies, dismantling labor unions and privatization of SOE's was rapidly occurring before the 1982 recession. This reduces the likelihood that the 1990 election represents a structural break in terms of exporters and non-exporters. However, the possibility that structural changes in 1990 cause an identification issue cannot be eliminated.

Results

This section implements four different approaches in estimating the ATT of export entry. First, simple DID is implemented without propensity-score-matching by estimating Equation 8 . Comparing the point estimates of unmatched DID with PSM-DID will give us our first evidence that selection-bias is negative for these Chilean exporters. Here, the propensity score is estimated using a standard specification that does not include access to liquidity (See Section for details). Next, I estimate a HCW model. This brings new evidence on the treatment effect of export entry, showing a significantly positive effect, that is in stark contrast to PSM-DID estimates under a standard specification of the propensity score. The HCW result also stands in contrast to the general findings in the trade literature, as surveyed by Wagner (2012).

If the HCW estimates are correct then this suggests that there is a negative selection bias that is even more severe than is implied by the comparison of standard DID with PSM-DID. All models should agree if they are properly specified.

Finally, I show that differences in liquidity constraints between exporters and non-exporters can interact with a decreasing lending rate to negatively bias PSM-DID estimates if these differences are not modeled into the propensity score. Theory shows liquidity constraints to be an important mechanism for selection into exporting Chaney (2005). The identification assumption in propensity-score-matching is that all significant mechanisms for selection are included in the estimation of the propensity score. The final section incorporates a measure of liquidity constraints into PSM. Now PSM-DID estimates, accounting for differences in liquidity constraints, agree with HCW estimates that entering the export market is productivity augmenting.

DID and PSM-DID Estimates

The point estimates of Equation 8 using DID without matching with propensity-score-matching are reported in Table 2. Since the LP productivity is in log levels, the DID estimator $\hat{\gamma}_3$ reflects the difference in growth between new exporters and their counterfactuals, relative to the pre-entry period ($t < T_i^B$). Without matching, the ATT of exporting, γ_3, is estimated to be negative for all cohorts as well as for the pooled model. These negative point estimates are significant for two of the cohorts, both statistically as well as economically, with point estimates of -0.144 and -0.200 being significant at the 1 percent level. If we do not control for selection into exporting then we would erroneously conclude that entering the export market has a negative impact on productivity for these Chilean manufactures.

Once we control for selection by propensity-score-matching, then difference-in-difference point estimates of the average treatment effect of export entry tell a qualitatively different story. All of the point estimates of γ_3 under PSM-DID are now insignificantly different from zero, with exception of the 1992 cohort of export entrants. The point estimates under PSM-DID are also less negative in magnitude compared to DID without matching for all but one cohort.

Difference-in-differences using the matched pairs suggests that the treatment effect is underestimated in DID without PSM. In words, this says that the selection effect of export entry can be negative which is contrary to intuition. We might suspect that exporters have better managers who are able to better respond to macroeconomic conditions thus leading to an overestimate of the treatment effect without PSM. In later sections, I will show that there is a very intuitive explanation for a negative selection effect. Finally, note that the DID-PSM finding of an insignificant effect echoes the literature that is documented by Wagner (2007).

HCW Estimates

Next, I estimate the average treatment effect of the treated (ATT), using an HCW model as described in Section . The point estimates, with 95 percent confidence intervals for the $ATT_{t^*}^{HCW}$ are plotted in Figure 1. The HCW estimates tell us that exporters experience an immediate boost in productivity as a result of export entry. Furthermore, gains seem to grow steadily over time. This result stands in sharp contrast to the PSM-DID estimates that are documented in Table 2 as well as many others who used DID in other studies, Wagner (2012).

Finally, I wanted to check if it is reasonable to assume that the HCW method is invariant to selection on unobservables. To test this assumption I randomly match control and treated plants with replacement. Figure 1 reports the point estimates, which are quantitatively similar with and without PSM. Also we can see that the in-sample fit using PSM is better than with random sampling.

Table 2: The Treatment Effect of Exporting: DID with Matching

			DID without Matching		
	(1)	(2)	(3)	(4)	(5)
	$T_i^B = 1991$	$T_i^B = 1992$	$T_i^B = 1993$	$T_i^B = 1994$	Pooled
γ_1	0.741***	0.725***	0.516***	0.462***	0.574***
	(0.0816)	(0.0907)	(0.0958)	(0.0699)	(0.0477)
γ_2	0.481***	0.492***	0.490***	0.489***	0.0160***
	(0.0248)	(0.0253)	(0.0253)	(0.0251)	(0.00565)
γ_3	-0.144***	-0.200***	-0.0590	-0.0838	-0.0103*
	(0.0440)	(0.0560)	(0.0603)	(0.0654)	(0.00550)
Observations	16037	15630	15562	15526	72508
			DID with PSM		
γ_1	0.340***	0.481***	0.178***	0.0963**	0.228***
	(0.0513)	(0.0539)	(0.0546)	(0.0459)	(0.0268)
γ_2	0.430***	0.535***	0.535***	0.435***	0.252***
	(0.0480)	(0.122)	(0.134)	(0.129)	(0.0469)
γ_3	-0.0301	-0.133*	-0.084	0.001	-0.029
	(0.0671)	(0.0786)	(0.0907)	(0.0887)	(0.042)
Observations	2266	1526	1348	1325	6464

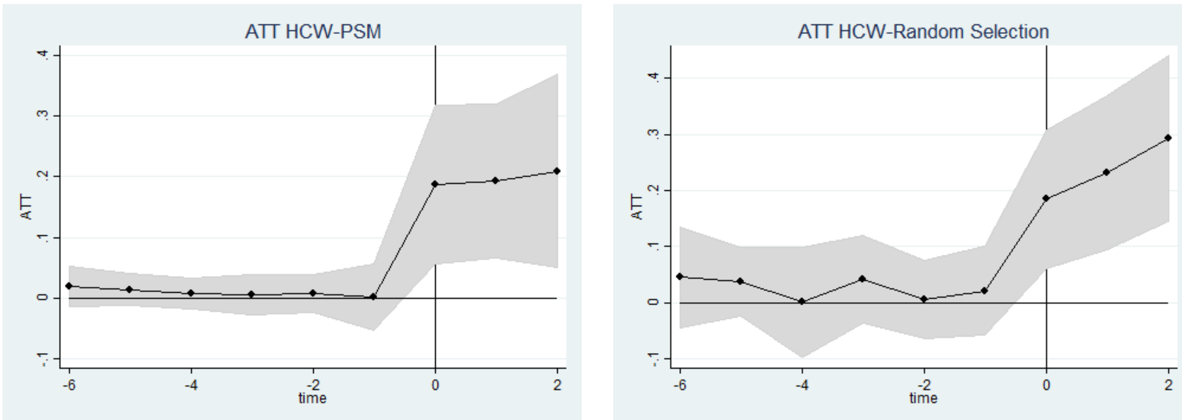
Note: this table reports the OLS estimated coefficients on γ_1, γ_2, and γ_3 of Equation (8): $z_{it} = \gamma_1 D_i^{psm} + \gamma_2 D_{it} + \gamma_3 D_i D_{it} + \Gamma_x X_{it} + \epsilon_{it}$. z_{it} is the LP plant productivity measure in log levels. D_{it} equals 1 if $t \geq T_i^B$ and zero otherwise, D_i^{psm} equals 1 if plant i is defined as treated by the Propensity Score Matching and zero otherwise. Exogenous controls X_{it} is a set of industry dummies at the 2-digit International Standard Industry Classification level. Regression is conducted separately for $T_i^B = 1991, 1992, 1993, 1994$ as well as pooled for all cohorts. See text for more details. Robust standard errors in parentheses do not account for added variation of LP productivity estimates and thus are under-reported. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Accounting for Liquidity Constraints

It could have been the case that DID and HCW produced the same point estimates. Indeed, if both models were properly specified then they should produce similar estimates. If these models produce qualitatively different estimates, as they did in this paper, then a researcher is confronted with a challenge to correct the models so that they agree.

If the HCW estimates are correct then this suggests the presence of an interactive effect that is not netted out with propensity-score-matching. In other words, exporters and non-exporters can be effected differently by unobservable macroeconomic factor. However, the HCW method does not

Figure 1: ATT^{HCW} with 95 percent confidence intervals



Note: this figure plots the Average Treatment Effect for the Treated where the x-axis is time, denoted $t^* = t - t_s$ where t is the year reported in the data set and t_s is the year the firm starts exporting. Thus $t^* = 0$ denotes the time when firms begin exporting. Bootstrapped standard errors show the 95 percent confidence interval for each period. Dots represent the discrete nature of the estimates. See text for more details.

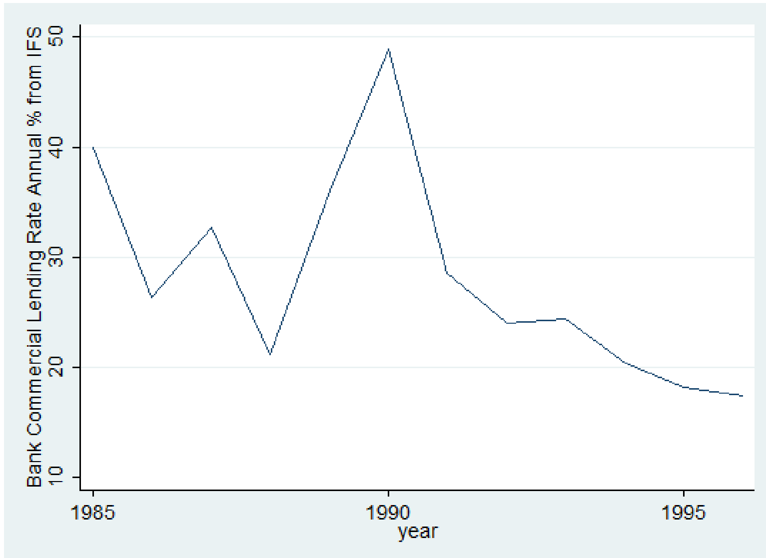
inform us which interactions might be important in driving these differences. For specific insight, we need to examine the economic environment as I will do in the following section.

I find that exporters respond less to changes in the domestic lending rate, the domestic lending rate improved, which can cause investment to rise more for domestic firms than for exporters. If these differences in investment behaviors translate into changes in productivity then this can cause difference-in-differences to underestimate the effect of export entry.

Motivating Evidence for Modeling Liquidity Constraints

There are likely many interactive effects that can significantly drive differences in productivity between exporters and non-exporters. This paper considers only one of the many, the commercial lending rate that began to decline in the early 1990's. Figure 2 plots the commercial lending rate over time. During pre-treatment years, the rate was 35 percent on average between 1985 and 1990. It climbed to a high of 49 percent in 1990 and then began a steady and rapid decline to a low of 17 percent by 1996; averaging only 22 percent between 1991 and 1996. Thus, for the 1991 cohort of export entrants; the average interest rate is 13 percent higher during the pre-treatment period than in the post-treatment period.

Figure 2: Commercial Lending Rate 1979-1996



Note: this figure plots the commercial lending rate over the sample period. Four cohorts of export entrants, 1991-1994, each experience a declining interest rate during entry, which makes the average interest rate before export entry higher than the average interest rate after entry. This shows that capital markets improved for each of these cohorts after they began to export.

If exporters are less credit constrained than non-exporters then they have better access to liquidity, as Muuls (2015), Alverez and Lopez (2014) and Nagaraj (2014) suggest. Better access to liquidity should mean that their investment decisions should not be as influenced by the decline in the lending rate. A simple test is to consider how the growth rate of investment, denoted $\Delta log(I_{it})$, varies with the growth rate of the lending rate, denoted $\Delta log(r_t)$, by export status. The following simple regression is done individually for exporters and non-exporters:

$$\Delta log(I_{it}) = \beta_0 + \beta_1 \Delta log(r_t) + \epsilon_{it} \tag{13}$$

Table 3 reports the OLS results. Growth in the commercial lending rate is significant and negatively correlated with the growth rate of investment for non-exporters but not for exporters. We can see that both, the lending rate decreased, Figure 2, and exporters respond less than non-exporters to this decrease, Table 3. Taken together, we have found a candidate that has the potential to negatively bias DID estimates of the treatment effect of export entry. But, DID estimates of the treatment effect of export entry will only be downward biased if differences in investment plans transmit to differences in productivity. To test this, I propose the following simple model:

$$z_{it} = \beta_0 + \beta_r r_t + \beta_x D_i^x + \beta_n r_t D_i^x + \gamma_x X_{it} + \epsilon_{it} \tag{14}$$

Define D_i^x as a dummy that is equal to one for exporters and zero for non-exporting plants. The lending rate, r_t is included as is the interaction with treatment. Thus β_r is the correlation of the lending rate and the non-exporting plant productivity, while the correlation for exporters is captured by $\beta_t = \beta_r + \beta_n$. Industry and year dummies are included in $X_i t$ in order to net out macroeconomic movements across industries and time. The point estimates of Equation (14) are reported in Table 3.

The lending rate is negatively correlated with productivity as reflected in the point estimate β_r, which is negative and significant as might be expected. However, the interaction coefficient, β_n, is positive and significant. This reduces the strength of the correlation between productivity and interest rates for exporters relative to non-exporters. In other words, the productivity of exporters can respond less on average to changes in domestic interest rates than non-exporters. If this is true, then the decline in the commercial lending rate, reported in Figure 2, has the potential to downward bias DID estimates. In summary, the asymmetric response to interest rate changes are a good candidate that might explain why DID estimates are insignificant for this Chilean panel. Next I will show how significant these differences are.

Accounting for Liquidity PSM-DID

The previous section shows evidence that asymmetric responses to changes in lending rates have the potential to cause DID to underestimate the treatment effect of export entry. Next, I will show how big this impact can be. To do so, I make a proxy for liquidity and incorporate this into my estimation of the propensity score.

The data does not directly measure liquidity. But I propose that the interest that the plant collects is a proxy. Collected interest is a byproduct of external investment and external investment occurs when a plant has an excess of cash flows or an excess of liquidity. See the Appendix for further discussion on the proxy measure. It quickly becomes clear that some plants are lenders, who collect interest, while others are not. I will define a lender, L_i, as equal to one if plant i ever collects

Table 3: Credit Constraints by Export Status

	(1)	(2)	(3)
	z_{it}	$\Delta log(I_{it})_{D^x=0}$	$\Delta log(I_{it})_{D^x=1}$
D_i^x	0.578***		
	(0.0279)		
r_t	-0.0177***		
	(0.000953)		
$r_t D_{it}^x$	0.00611***		
	(0.000964)		
$\Delta log(r_t)$		-0.0563*	-0.0175
		(0.0258)	(0.0183)
Observations	44180	10265	8627

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Column (1) reports the OLS estimates on γ_1 of $z_{it} = \gamma_0 + \gamma_1 D_{it}^x + \gamma_2 r_t + \gamma_3 r_t D_{it}^x + \Gamma_x X_{it} + \epsilon_{it}$. z_{it} is the LP productivity in log levels. D_{it}^x is a dummy indicating whether a firm ever exports. r_t is the Annual Bank Commercial Lending Rate. X_{it} is a set of controls including industry dummies and year dummies. Columns (2) and (3) reports the OLS estimates of $\Delta log(Iit) = \alpha_0 + \alpha_1 \Delta log(r_t) + \zeta_{it}$ separately for exporters, $D^x = 1$ and non-exporters, $D^x = 0$. Robust standard errors are in parentheses. * indicates $p < 0.05$, ** indicates $p < 0.01$, *** indicates $p < 0.001$. See text for more details.

interest and equal to zero otherwise. Then I force matched pairs to have the same lending status, when conducting propensity-score-matching. Also I include lagged levels of interest collected in the calculation of the propensity score.

As a final step, I create a metric, denoted D_i^c, for the plant specific investment response to the commercial lending rate and incorporate this metric in the calculation of the propensity score. Including this metric in matching allows plants whose investment patters are highly correlated with changes in domestic lending rates to be paired. First, calculate the plant specific correlation coefficient of investment and the commercial lending rate, denoted, $\sigma_i^{I,r}$. Next, define a dummy variable, D_i^c. Let $D_i^c = 1$ if the investments of plant i are highly correlated with the commercial lending rate, $\mid \sigma_i^{I,r} \mid \geq 0.75$, and zero otherwise. Thus D_i^c is defined as:

$$D_i^c = \begin{cases} 1 & \text{if } \mid \sigma_i^{I,r} \mid \geq 0.75 \\ 0 & \text{if } \mid \sigma_i^{I,r} \mid < 0.75 \end{cases}$$

Including D_i^c in the calculation of the propensity score and exact matching based on lending status have a dramatic effect on the PSM-DID point estimates of the treatment effect of export entry. Point estimates, in Table 4, for the pooled model are 0.076 and significant at the 10 percent level.[12]. Previously, the point estimates were negative and insignificant.

Table 4: The Treatment Effect of Exporting: PSM-DID with Credit Constraints

	DID-PSM: controlling for $\sigma_i^{I,r}$ in PSM				
D_i^{psm}	0.306***	0.270***	0.0767	0.103**	0.203***
	(0.0472)	(0.0533)	(0.0543)	(0.0456)	(0.0267)
D_{it}	0.330***	0.353***	-0.107	0.321***	0.135***
	(0.0910)	(0.123)	(0.139)	(0.121)	(0.0296)
$D_{it}D_i^{psm}$	-0.00912	-0.0221	0.253***	0.00645	0.0760*
	(0.0669)	(0.0784)	(0.0974)	(0.0916)	(0.0425)
Observations	2077	1283	1145	1134	5625

Note: this table reports the OLS estimated coefficients on γ_1, γ_2, and γ_3 of Equation (8): $z_{it} = \gamma_1 D_{it} + \gamma_2 D_i^{psm} + \gamma_3 D_i D_{it} + \Gamma_x X_{it} + \epsilon_{it}$. z_{it} is the LP plant productivity measure in log levels. D_{it} equals 1 if $t \geq T_i^B$ and zero otherwise, D_i^{psm} equals 1 if plant i is defined as treated by the Propensity Score Matching and zero otherwise. Exogenous controls X_{it} is a set of industry dummies at the 2-digit International Standard Industry Classification level. The firm specific correlation of investment growth and interest rate growth, denoted $\sigma_i^{I,r}$, is controlled included as a dummy in the calculation of the propensity score. See text for details. Regression is conducted separately for $T_i^B = 1991, 1992, 1993, 1994$ as well as pooled for all cohorts. See text for more details. Bootstrapped estimates of the pooled model, that account for all variability, were very similar. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[12]These estimates do not account for all variability in the estimation procedure. For robustness, the entire process was bootstrapped. The mean 0.053 has a 95 percent confidence band of 0.049 and 0.058

Conclusion

The average treatment effect on the treated of export entry using a standard specification of PSM-DID, that does not account for access to liquidity, Table 2 are starkly different than the HCW results in Figure 1. HCW shows evidence of an effect while PSM-DID does not. However, PSM-DID estimates agree with HCW estimates after plant level liquidity is accounted for in propensity-score-matching.

To some extent, selection into treatment and the common slope assumption of DID are addressing a related, if not principally the same, issue. Note that if assignment into treatment is random after conditioning on the propensity score then both treatment and control groups would have the same average response to unobserved factors, thus insuring that the common slope assumption in DID is satisfied. However, if PSM is improperly specified and thus unable to purge the selection effect then applying DID is an improvement. But only insofar as the endogeneity that is not resolved with PSM can be decomposed into additively separable time and plant fixed effects. DID-PSM can still fail if the matched control group responds differently to common macro-economic factors than the treated group. This is where we find gains in applying a general model that allows unobserved time and plant fixed effects to interact such as HCW. As we saw, HCW shows a positive treatment effect that stands in contrast with a majority of the trade literature that was built on a PSM-DID framework, Wagner (2012).

Furthermore, I show that PSM-HCW is complementary to PSM-DID. A qualitative comparison between the two estimates can give guidance on how PSM-DID might need to be re-specified, where simple pre-trend analysis is less informative. Thus PSM-HCW can be used for more than a simple robustness check for the common slope assumption in DID.

Also, this paper only explores asymmetric responses to lending rates but there are many potential other factors that can also be a source of selection bias. Extensions of the Melitz model have lead to new research agendas that describe new mechanisms of selection. Access to credit was one of these new branches. This paper is a reminder that new findings on selection should also re-motivate us to look backwards on previous topics where we were confronted with selection as an obstacle for identification. It is a reminder that new evidence on selection might show that the assumptions that were made in these previous studies are likely false. More importantly, incorporating new mechanisms of selection might give new answers, as they did here for the treatment effect of export entry.

CHAPTER 2: The Productivity Effect of Business Service Outsourcing

Introduction

The study of technology inherently is divided into a study about the creation of technology and the study of the dissemination of technology. Creation and dissemination of technology can both be active mechanisms when a manufacturing plant decides to outsource a business service. Business services can be a form of technology, when these services give clients valued know-how. For example, knowledge about optimal organizational design or production design, such as Ford's assembly line are a clear form of technology. On the other hand business services can disseminate physical technologies by pairing downstream clients with physical technologies. Yet another way that business services might impact productivity is by giving smaller clients access to technologies that are only justified when there are economies of scale. Database and cloud computing services to SME's are good examples.

The intuition in the first paragraph is supported by a significant number of empirical studies at the industry level that find positive correlations between service adoption and productivity. For example, The Eurpoean Competitiveness and Sustainable Industrial Policy Consortium conducted a industry study for several European economies and found that industries who use services more intensely are also more productive. The targeted policy implication is that countries should foster the domestic service environment to boost productivity. However, we should be cautious about these industry level studies as it is not clear if productivity is being driven by self selection. The extent that selection is present, weakens the the policy implications. If more productive firms are more likely to use services, then giving firms access to services might do little to shift the existing distribution of firm productivity. Indeed, Antras and Helpman (2004) suggest that there is a selection effect into service usage. Falk and Jarocinska (2010) note that studies that address this endogeneity are extremely sparce. Thus, a policy recommendation to promote business service development, hinges on the ability of a service to enhance productivity at the firm level.

Measuring this object is complicated by the identification problem of selection. Most papers completely avoid endogeneity beyond controlling for fixed effects but this is risky. The current firm level empirical evidence on specific types of service adoption is sparse as well as conflicting. Some firm level studies find a positive effect, Gorzig and Stephan (2002), Arnold, Mattoo and Narciso (2008), while others find a no effect of service outsourcing, Gorg, Hanley and Strobl (2008) as well as Gorg and Hanley (2004). Furthermore, these studies have not examined the productivity effects of different types of service outsourcing even though we would expect different types of services to potentially have different impacts at the firm level.

There are empirical methods that address the identification problem of selection. If we work with firm or plant level data then it is possible to model the selection process and use Difference-in-Differences or use a Hsiao Ching and Wan (2012) estimator that is invariant to selection. This paper, measures the productivity effect of four types of business services, while addressing the endogeneity of simulteneity and omitted variables.

To implement this identification strategy, I focus on the extensive margin and ignore the intensive margin. These margins represent outcomes from different problems that the firm faces. On the extensive margin, the plant is faced with a make-versus-buy decision. Indeed, an entire literature of make-versus-buy, views service outsourcing as a binary outcome, see Merino and Rodriguez (2007). Plants who choose to buy or outsource a service are employing a new type of organizational structure within their plant. Perhaps they are adopting a completely new type of business function or perhaps they are moving an internal process outside of the firm boundary. Either way, it is clear that a new type of technology is being employed on the extensive margin that was not previously employed. This allows me to view this extensive margin as a treatment effect, just as the trade literature views exporting as a treatment effect. From a practical standpoint, working on the extensive margin also allows me to employ several econometric techniques that mitigate problems of selection. The intensive margin is not a clean as the extensive margin because there is an added burden of identifying the portion of intensity that is explained by new service adoption. Intensity can change for numerous reasons and yet I am only interested in finding the portion that is driven by technological adoption. Also, identification on the intensive margin requires an instrument, which is more difficult to find. More details can be found in the following section.

In addition to these main points, I make two other minor contributions with this work. First, this paper adds to the literature on outsourcing international services. There are industry studies that show a positive effect of service offshoring within the US, Amiti and Sang-Jin (2005). Alternatively, this paper is set within the context of a developing country. The expectation would be to find a positive effect. Surprisingly, I do not find evidence that outsourcing foreign technical services has any impact on plant level productivity. Further research should be done to follow up with this non-result.

As a final contribution, this paper shows how the Hsiao, Ching and Wan (2012) estimator can be used to show robustness against the identification assumptions that are made in propensity-score-matching difference-in-differences. The central idea is that the HCW method is invariant to selection. The problem of selection is oftentimes tested for within the PSM-DID context, using pre-trends and quality-of-matching tests. Hopefully readers will agree after reading this dissertation, that these common tests can rely on fairly strong assumptions. Alternatively, researchers can show robustness by simply fitting an HCW model and check the results against PSM-DID estimates. This type of testing shows that results are robust under a completely different set of identifying assumptions.

Data, Measure and Methodology

Data and Measure

Data A panel of plants is compiled from the Encusta Nacional Industrial Annal (ENIA) survey conducted by the National Census Bureau of Chile. The survey covers all manufacturing establishments that have at least 10 employees. It provides detailed information on sales, employment and related measures, various input spendings, capital, inventory, and the revenue from exporting. The panel spans the years from 1984 to 1996, and contains about 5,000 plants on average per year with a total of 86,186 plant-year observations.[13] Industry classification is available at the four-digit level of International Standard Industrial Code (ISIC). The data set contains entry/exit dummies that equals one for the year that a plant enters/exits.

Measures The data reveals that some plants use services for all years, some never use a service, some start outsourcing after periods of not outsourcing. Still others outsource services sporadically. As a first step, I identify plants who start freight, accounting, advertising and foreign technical support services. For discussion, let an element from this set be defined as a service, denoted s, which will be interchangeably described as an activity. An important feature of the data is that the survey begins in

[13]The original data set starts from 1979. However, a large recession in 1982 causes potential problems with forecasting. Therefore, observations prior to 1984 were dropped.

1979. This raises the question of how to deal with missing information before 1979. The starting year of each activity is identified as follows. For plants whose first year of service s is shown as 1979, their starting year of service s is counted as missing, because one cannot determine whether they started before or on 1979. For plants whose first year of exporting is shown as after 1979, it is possible that they started before 1979 without being observed, stopped activity s for a while, and resumed later. This possibility is evaluated by comparing the length between 1979 and their observed starting year with the average length of non-participation years after their first observation on activity s. If the former is longer than the latter, then this plant's starting year of activity s is identified as the first year that reports positive expenditure on service s; it is recorded as missing otherwise.

Table 5: Summary of Service Outsourcing Types

Service	**User**	**Starter**	**Always**	**Quitter**	**Has Gaps**
Exporting	1054	33.2%	29.4%	23.2%	2.3%
Freight	3041	24.5%	61.5%	20.4%	2.7%
Advertising	2569	37.5%	42.2%	32.6%	3.0%
Accounting	3847	19.8%	66.9%	16.6%	3.7%
Foreign	472	66.1%	13.3%	64.4 %	1.3%

This table reports the type of participation for each activity of the set s=(Exporting, Freight, Advertising, Accounting). A "user" is defined as a plant that ever participates in activity s. A starter is defined in the text. An "Always" user is a plant who always participates. A "Quitter" starts and then quits. Plants who sporadically participate are categorized as "Has Gaps". These categories are expressed as percentages of users. These are not mutually exclusive sets. Some starters are also quitters and some starters have gaps.

Table 5 reports the participation pattern for each service as well as for exporting. It is clear that, as a percentage of total users, there are relatively less starters in accounting services. This is clearly because plants are more likely to always use accounting services. Freight outsourcers share a similar pattern. 61.5% of freight users have always used freight services. 41% of advertisers are consistent participants. Only 29% of exporters participate in every period. Starting and quitting rates are fairly similar across activity types with exception of Foreign Technical Support users who have relatively high starting and quitting rates. Finally, the number of plants who sporadically engage in these activities is fairly small for all activity types. The ability to clearly define starters allows me to view the act of starting as a treatment that the plant receives.

Finally, I estimate plant-specific productivity applying the estimators of Levinsohn-Petrin (LP) (2003) following the mass in the literature that is reviewed by Wagner (2007). See Appendix for details.

Methodology

This paper employs propensity-score-matching difference-in-differences, PSM-DID hereafter to perform a quasi-experiment and test the productivity treatment effect of service outsourcing. I also use the method of Hsiao Ching and Wan (2012) to show robustness. Please see Sections , and for a technical description of the methodologies.

Using a Chilean panel of manufacturing plants, I examine the productivity effect of four types of business service adoption, freight, accounting, advertising and foreign technical support services. In this sense I will consider the adoption of business services as a treatment that the plant receives and thus the identification goal is to uncover the treatment effect of service outsourcing. However, there are other factors in the economy that can cause plant level productivity to change over time and these will need to be controlled. Identification is confounded by non-random assignment into treatment. This means that we cannot simply compare service participants with non-participants to control for unobserved factors that might also be driving productivity to change over time. Such a strategy is at the heart of simple Difference-in-Differences. One way to address the problem of non-random assignment is to combine DID with propensity score matching, PSM hereafter, which pairs treated plants who begin a business service with non-service using control plant based on their propensity to start the service under evaluation. A combined PSM-Difference-in-Differences estimate increases the chance that both groups will respond in similar ways to unobserved factors, thus PSM increases the consistency of DID.

However, the common slope assumption of DID sill might not be satisfied, which is to say that our PSM strategy failed on some dimension. The common slope assumption can be tested in traditional ways, which look for well established pre-trends. However, a big problem arises with this testing method. Pre-trend analysis assumes that the past will repeat itself. However, it could very well be the case that treatment and control groups respond differently to common unobserved factors, these factors have very little movement in the pre-treatment period but then jump at around the same time that treatment occurs. Such an unfortunate, and possible, state of events can cause pre-treatment analysis to incorrectly confirm the existence of a pre-trend and as a result, falsely ascribe movements in the dependent variable as being caused by treatment, when in fact they are being driven by changes in unobserved common factors. A much more robust way to show robustness is to compare PSM-DID point estimates against a model that relaxes the common slope assumption. Accordingly, I fit a more general model of Hsiao, Ching, and Wan (2012), HCW hereafter, which is a more general model and does not require a common slope assumption and is invariant to the problems of selection. A detailed presentation of this model can be found in Section , although it is omitted here.

Both methods, PSM-DID and PSM-HCW provide similar evidence, that starting a accounting, freight or advertising service has a causal positive impact on plant level productivity but there is no evidence that starting a foreign technical support service has productivity benefits.

Results

To begin, let us first note that service expenditure is positively correlated to higher productivity for every service type. In order to see this, let us run the following simple regression:

$$z_{it} = \gamma_) + \gamma_1 D_i^s + \Gamma_x X_{it} + \epsilon_{it} \tag{15}$$

Where z_{it} is the Levihnson-Petrin productivity for firm i in year t, D_i^s is a service dummy for service s that equals 1 if plant i ever shows a positive expenditure on service s and is zero otherwise. X_{it} is a simple set of industry and year controls. The superscript s will be an element of the set (freight services, advertising, foreign technical support, accounting). I estimate 15 individually for each service s.

The coefficient on D_i^s captures differences in productivity that can can arise both before and after a plant begins service s. Thus γ_1 captures both selection and learning effects. A more detailed discussion about the selection effect of entering service each service can be found in Section . For now, the goal is to simply mitigate the biases that can be caused by self selection. The average treatment effect for each service type is the statistical object that this section attempts to recover.

The point estimates of Equation 15 are listed in Table 6. On average, freight participants are 57 percent more productive than freight non-participants; advertisers are 47 percent more productive than advertising non-participants; foreign support users are 50 percent more productive than non-users and plants who outsource accounting services are 25 percent more productive than those who do not.

Table 6: Productivity Differences by Service Outsourcing Participation

	(1) s = Tech Supp	(2) s = Advertising	(3) s = Freight	(4) s = Accounting
D_i^s	0.495***	0.474***	0.569***	0.253***
	(0.010)	(0.010)	(0.011)	(0.031)
Industry	Y	Y	Y	Y
Year	Y	Y	Y	Y
Observations	44,180	44,180	44,180	44,180
Adj. R^2	0.45	0.44	0.45	0.42

Note: this table reports the OLS estimated coefficients on γ_1 of Equation (15: $\mathrm{z}_{it} = \gamma_) + \gamma_1 D_i^s + \Gamma_x X_{it} + \epsilon_{it}$. z_{it} is the LP plant productivity measure in log levels. D_i^s equals 1 if plant i ever has positive expenditure on service s. Exogenous controls X_{it} is a set of industry dummies at the 2-digit International Standard Industry Classification level and year dummies. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

To separate the ex-ante selection effect from the ex-post learning effect for each service s, the propensity score matching (PSM) proposed by Rosenbaum and Rubin (1983) can be applied to identify control plants that had a-priori similar likelihood of starting service s but that had kept the business function in-house. The intuition of the propensity score is to create a control group that is similar to the treatment group. Rosenbaum and Rubin (1983) show that matching on a propensity score, conditional on plant characteristics, can have the effect of normalizing these characteristics across groups. Thus matching treatment and control plants who have a similar propensity score effectively creates groups that are more similar in terms of characteristics. A more detailed discussion is given in Section and will not be repeated here.

Intuitively, I would like to control for unobserved factors that can drive productivity to change. For example, interest rates and other macroeconomic, political or social factors can cause productivity to change, which means that we cannot simply examine how productivity changes after the time of treatment. If we have a control group that is also subject to these unobserved factors then we can employ difference-in-differences to net these factors out. Choosing control groups who have a similar propensity score as the treatment group has the effect of creating similar groups, which can be assumed to respond to unobservable factors in similar ways. However, Angrist and Pischke (2008) point out that we are still left conditioning on observable characteristics. Clearly, factors that are not conditioned on in PSM may still cause a potential bias even if both groups are statistically identical in matched characteristics. Thus it seems largely uninformative to show that treatment and control groups are similar after matching, as is commonly practiced.

The propensity score is estimated by fitting a simple logistic regression model.

$$logit\left(\mathrm{E}[d_{it}^{s}=1]\right)=\beta_0+\beta_z z_{i,t-1}+\boldsymbol{\beta_x x_{i,t-1}} \tag{16}$$

where d_{it}^{s} is a dummy that is equal to one if plant i starts service s in time t. It equals zero if plant i has a zero expenditure on service s in all time periods in t and before and it is undefined if service entry is not observable or if entry has already commenced. Following convention, Equation 16 is fit using the ex-ante sample ($t \leq T_i^B$). $\boldsymbol{x_{it}}$ includes lagged values of capital, materials and productivity. The nearest neighbor method is used and exact matches are forced on 2-digit industry. As in De Loecker (2007), treatment is defined as service entry (rather than plants that continue to use services); the control plants are those with the closest propensity score to each treated observation, that belong to the same 2-digit industry, and that are non-users of service s.

The requirement of common support is satisfied and treatment groups are statistically similar to control groups in terms of $\boldsymbol{x}$ characteristics. Table 7 reports the effects of this balancing in for exporters to illustrate the concept. However, such tests are almost non-informative given the extremely large dimension of potential selection mechanisms, all of which must be specified for PSM assumptions to be valid. In Section I showed that modeling behavioral reactions to interests rates had a profound influence on PSM-DID estimates of the treatment effect of export entry. In Section I show that levels can be a misleading dimension for identifying a selection mechanism: I find ample support that productivity in levels is associated with increased likelihood of export entry but the time path of productivity is not. Thus it seems highly likely that modeling propensity scores on simple levels is not capturing the main process for selection. It is unrealistic to consider that the search space is ever adequately explored. An alternative approach to show robustness is to fit a HCW model that is invariant to selection on unobservables. If the HCW results agree with PSM-DID then this shows that the results are robust under a completely different set of identifying assumptions.

To control for unobserved effects that can drive productivity, I estimate the pooled difference-in-differences regression is run on the propensity matched sample.

Table 7: Balancing Covariates Using PSM

	Unmatched Sample			Matched Sample		
$x_{it} =$	materials	capital	productivity	materials	capital	productivity
$d_{it} = 1$	12.043	11.120	7.595	12.081	11.126	7.629
$d_{it} = 0$	10.514	9.263	6.722	12.844	11.843	7.834

Note: Matching on the propensity score has the effect of normalizing the covariates used in the calculation of the propensity score across treatment and control groups. However, there is no guarantee that similarities will exist on dimensions that are not included in matching. These confounding dimensions can seriously bias estimates and so tables such at this one do not provide strong evidence that selection is un-confounded after matching.

$$z_{it^*} = \gamma_1 D_{t^*} + \gamma_2 D_i^{psm} + \gamma_3 D_i D_{it^*} + \Gamma_x X_{it^*} + \epsilon_{it^*} \qquad t^* = 1, ..., T^* \tag{17}$$

z_{it^*} is the LP productivity; D_i^{psm} equals 1 if treated and zero if selected as controls, both defined by PSM; D_{it^*} equals 1 if $t^* \geq T_i^s$ and zero otherwise. Cohorts of service entrants for the years 1988-1993 are time centered relative to their starting year so that $t^* = t - T_i^s$, where T_i^s is the starting year of plant i in service s and t denotes the year. X_{it^*} is a set of exogeneous industry and year controls. In this setup, D_{it^*} controls for any common time variations caused by unobserved factors. D_i^{psm} controls for any unobservable initial difference between treatment and controls that have not been captured by PSM, Blundell and Dias (2009). Hence, γ_3 is the parameter of interest; the DID estimator for the treatment effect of exporting. Equation (17) is estimated over the matched sample.

The results are reported in Table 8. Since the LP productivity is in log levels, the DID estimator $\hat{\gamma}_3$ reflects the difference in growth between new exporters and their counterfactuals, relative to the pre-entry period ($t^* < T_i^B$). In other words, it estimates the average treatment effect for the treated (ATT). The ATT of each service is estimated to be significantly positive with exception of foreign technical services.

Finally, I estimate the treatment effect using the HCW method that is outlined in Section for a robustness check the adequacy of the PSM assumption, that selection is unconfounded after propensity score matching. Note that this assumption is synonymous to the DID assumption of a common trend since randomly assigned groups should be expected to respond the same to unobserved factors. Bootstrapped estimates are plotted in a time centered plot in Figure 3 along with 95 percent confidence intervals. Note that the DID estimates qualitatively and quantitatively agree with the HCW estimates, which are invariant to selection on unobservables. This evidence is stronger than doing a traditional trend analysis or checking for the quality of matches in propensity score matching.

Discussion

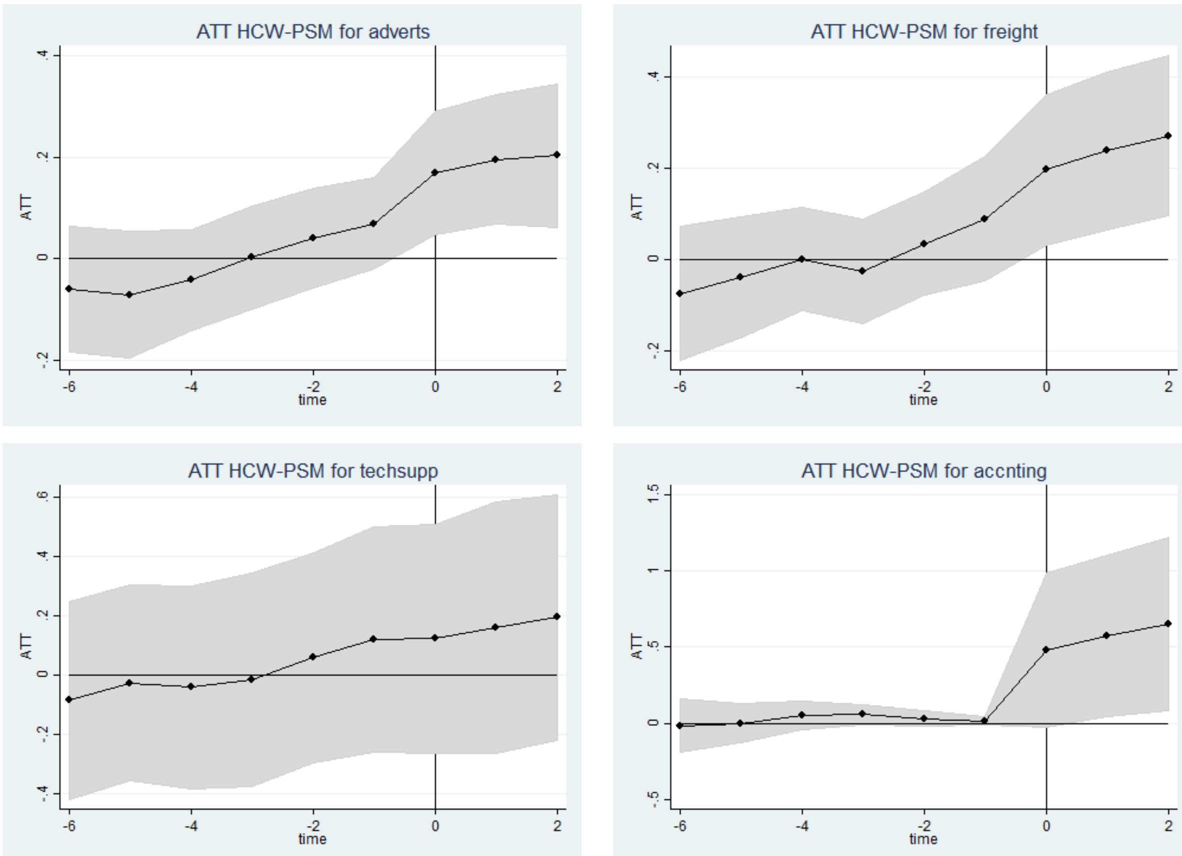
This paper shows that outsourcing a service has a positive effect on productivity. Other firm studies have been inconclusive and have not addressed the identification issues of selection. Further work should expand on who benefits the most from these types of services. This chapter is non-informative about why these gains happen or who benefits the most from these services. The HCW method can give a much richer analysis by recovering firm specific treatment effects. With these firm specific treatment effects, it is possible to perform a much deeper study about the firm heterogeneity in experiencing service outsourcing. However, firm specific effects are infeasible to estimate for this panel because of the limited time dimension. Future work with a different data set might offer a richer description about the distribution of effects.

Table 8: DID Estimates for Service Starters

	(1) Tech Supp	(2) Advertising	(3) Freight	(4) Accounting
treat (γ_2)	0.553***	0.0446	0.208***	-0.286***
	(0.0529)	(0.0344)	(0.0298)	(0.0567)
time (γ_1)	-0.0204	-0.0969**	-0.00827	-0.0923
	(0.0583)	(0.0396)	(0.0338)	(0.0653)
interact (γ_3)	0.0337	0.189***	0.0862**	0.234***
	(0.0726)	(0.0426)	(0.0404)	(0.0701)
Industry Dummies	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES
Observations	1700	6457	4128	2811

Note: this table reports the OLS estimated coefficients on γ_1, γ_2, and γ_3 of Equation (17). γ_3 is the treatment effect. z_{it^*} is the LP plant productivity. See text for more details. Robust standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure 3: ATT_s^{HCW} By Service Type: with 95 percent confidence intervals



Note: this figure plots the Average Treatment Effect for the Treated where the x-axis is time, denoted $t^* = t - t_s$ where t is the year reported in the data set and t_s is the year the firm starts exporting. Thus $t^* = 0$ denotes the time when firms begin exporting. Bootstrapped standard errors show the 95 percent confidence interval for each period. See text for more details.

CHAPTER 3: Selection into Exporting and Outsourcing

Introduction

Bernard and Jensen (1995) observed that exporters have systematically different characteristics than non-exporters. Productivity is one of these characteristics, with exporters being more productive on average than non-exporters. Melitz (2003) provides a mechanism for sorting based on productivity endowments in the presence of fixed costs of export entry. Since then the role of productivity in the mechanism of selection has largely been taken as given[14]. However, after 13 years of research, it is still not clear if simply becoming more productive will have any influence on a plants exporting behavior. I examine a Chilean panel of manufacturers and I cannot find any evidence that changes in productivity alone predicts exporting entry. However, I do find significant evidence that exporters are more productive than non-exporters starting from the first year of plant operation. This is not inconsistent with the Melitz model since productivity in the Melitz model is an endowment. Thus the interpretation of the mechanism in the model should be one of endowment and not productivity.

If productivity alone does not drive export entry then there might be confounding variables that causes productivity to increase as well as increases the likelihood of export participation. The productivity differences between exporters and non-exporters, evaluated over the life-cycle of the plant, gives insight into what types of choices might confound selection. Firms are confronted with different choices over the life-cycle. When a business first forms, owners must choose, whether to take a business partner, what technologies to hire, what industry to join and what type of preparation to make before entry. Choices that are unique to subsequent periods include, whether or not to upgrade technologies, the choice to outsource business functions, the choice to pursue organizational reorganization, and the expansion into other markets. When I evaluate differences in characteristics between exporters and non-exporters over the life-cycle, then I find that a significant amount of these differences occurs in the first period. Subsequently, the wedge between exporters and non-exporters continues to grow over time.

[14]Some researchers have found some evidence of gearing-up, Iacovone and Javorcik (2012) as well as Lopez (2009). Yet gearing-up is a different concept than enabling entry.

To decompose the selection effect into exporting, this paper examines one choice from the first period and one choice from subsequent periods in the life-cycle. From the first period, I examine the choice to take a business partner. For the set of choices in subsequent periods, I examine the choice of outsourcing a business service. Formally, this paper seeks to identify how much of the productivity selection effect into exporting can be explained by these two choices. I find that the choice of a partner can account for as much as 42 percent of the selection effect into exporting while various types of services account for roughly 10 percent.

One of the first choices that a business owner makes is whether to take a partner or whether to start alone. This choice separates two types of entrepreneurs. For discussion purposes let us call this entrepreneurial endowment. Schoar (2010), also distinguishes between two types of entrepreneurs, those who are transformative and those who are a subsistence type. Subsistence entrepreneurs maintain a standard of living, while transformative entrepreneurs are the Elon Musks, who want to grow an empire and deeply penetrate a market.

I find that entrepreneurial endowment can account for as much as 42% of the productivity differences between exporters and non-exporters. Also, I find that entrepreneurial endowment is a strong predictor of future export participation, even at long horizons. This confounds a naive regression of export status on productivity, since entrepreneurs who take a partner are more productive and more likely to start exporting than single business owners.

A second objective of this paper is to bring attention to the apparent link between exporting and service outsourcing. Exporters are more likely to outsource freight, advertising and foreign technical services. The choice of outsourcing business services does not seem to be related to entrepreneurial type with the exception of accounting services. This finding should raise caution in evaluating the impact of service adoption on exporting behavior. It can clearly be the case that more productive plants can self select into both exporting and service outsourcing. This endogeneity makes it difficult to evaluate if service outsourcing impacts exporting behavior. However, this endogeneity issue can be addressed in examining the impact of service adoption on the productivity selection effect of exporting. I evaluate the ability of service adoption in explaining the productivity selection effect of exporting by using a modified identification strategy of Hsiao Ching and Wan (2012). This method can create a measure of the systematic differences of future exporters and non-exporters in a counterfactual state of the world; one in which no plants outsource a service. The method is invariant to selection and thus I can ignore the issues of selection that I have highlighted in this paper.

With the counterfactual selection effect of exporting in hand, I can compare it to the actual selection effect and recover a measure for the percentage of the selection effect into exporting that can be accounted for by service outsourcing. I find that starting advertising can account for 9.1 percent of the selection effect into exporting, accounting service adoption can produce 8.7 percent and freight services can account for 11.8 percent of the selection effect. This gives some evidence that access to services might be particularly beneficial to future exporters. Service adoption can at least partially explain the systematic productivity differences between these two groups.

This paper does not answer the larger question, what separates the entrepreneurial space? The separation of this entrepreneurial space can be driven by heterogeneous preferences or by differences in ability or by both. Schoar (2010), suggests that entrepreneurs can have different preferences. Lucas (1978) argued that the managers ability is the determinant of plant size. The distinction between preferences and ability may be complementary. I find that single owner plants are less likely to outsource advertising than partnered plants. This weakly suggests that these plants may have a preference for staying small. Of course, it could be the case that these firms realize that they lack the ability to manage an expansion in demand. More work should be done to disentangle ability versus preferences. If the space is driven by differences in preferences then there is likely little that can be done from a policy standpoint to draw these small market owners into an international market.

Literature Review

A first wave of empirical trade research began with an observation that exporters are systematically different from non-exporters, Bernard and Jensen (1995). It has since been well documented that exporters are more productive, capital intensive, skill intensive, and pay a higher skill premium than their non-exporting counterparts even before exporting begins, see Wagner (2012) for a review of the literature. The theory of Melitz (2003) developed a mechanism to explain this sorting of productivity by export types but it did not make productivity endogenous. Recent models of Melitz and Redding (2012), following Bustos (2011), have added an upgrading feature to the Melitz (2003) model. This makes productivity endogenous after a plant begins formal operation but it leaves productivity endowments as exogenous. The model is motivated by empirical evidence, which finds that exporters are more innovative and they spend more money on new technologies, Bustos (2011). This paper adds to this literature by showing that the productivity differences between exporters and non-exporters largely begins in the first year of operation. There is a thinner margin in this Chilean panel for exporters to grow relatively more productive over time.

Measure and Data:

In her study, Schoar identifies business owners as transformative types and self employed as subsistence entrepreneurs. In the context of this paper, I want to conceptualize manufactures who have one owner as subsistence entrepreneurs and those who have partners as transformative types. This classification of entrepreneurial type rests on the idea that transformative entrepreneurs realize that greatness is not achievable alone. Those entrepreneurs who have grand dreams need partners to realize their vision.

This Chilean data set is not ideal for this question because it is at the establishment level and yet the question is at the firm level. Thus the existence of subsidiaries creates a problem. Subsidiaries are problematic in this establishment level data set because a single entrepreneur can own many plants. If the number of owners is observed to be zero then I interpret this plant as being a subsidiary. Thus I eliminate plants that I identify as subsidiaries as well as plants who appear in 1979 since their entry date is not observed. This eliminates 1,374 plants from the sample leaving 2,967 plants who have more than one owner.

Selection Effects by Activity Type Over Time

It is important to distinguish a selection effect from a selection mechanism. Selection effects are defined as differences in characteristics between future participants and non-participants. They tell us that certain types of plants self select into a particular activity. They are often observable and yet these objects might not play a role in the mechanism of selection. However, the timing of when these selection effects become observable contains information about the timing of the mechanism. Identifying the timing is critical since the plant has a clear evolutionary time path of plant development, with distinct periods in which agents face an unique set of challenges. The temporal location of the mechanism along the life cycle of the plant is thus informative about what types of mechanisms can be reasonable.

Figure 4 plots the average productivity, capital intensity, value added and skill intensity over the plant life-cycle by export status. It is immediately clear that large systematic differences exist between these two groups. Many of these differences begin from the first year of operation. These systematic differences between exporters and non-exporters also have a different evolutionary path over the plant life-cycle. Exporters start off more productive than non-exporters and this gap widens over the first 5 years before contracting and then diverging again. The capital intensity of exporters slowly grows over the plant life-cycle while it slowly declines for non-exporters. Value added differences exist between groups from the first year and slowly diverge as a plant ages. The oldest non-exporters have a decline in value added while the oldest exporters show an acceleration in growth. Skill intensity differences between exporters and non-exporters are less obvious in the early years of the life cycle but then diverge in later years as older exporters gain skill intensity.

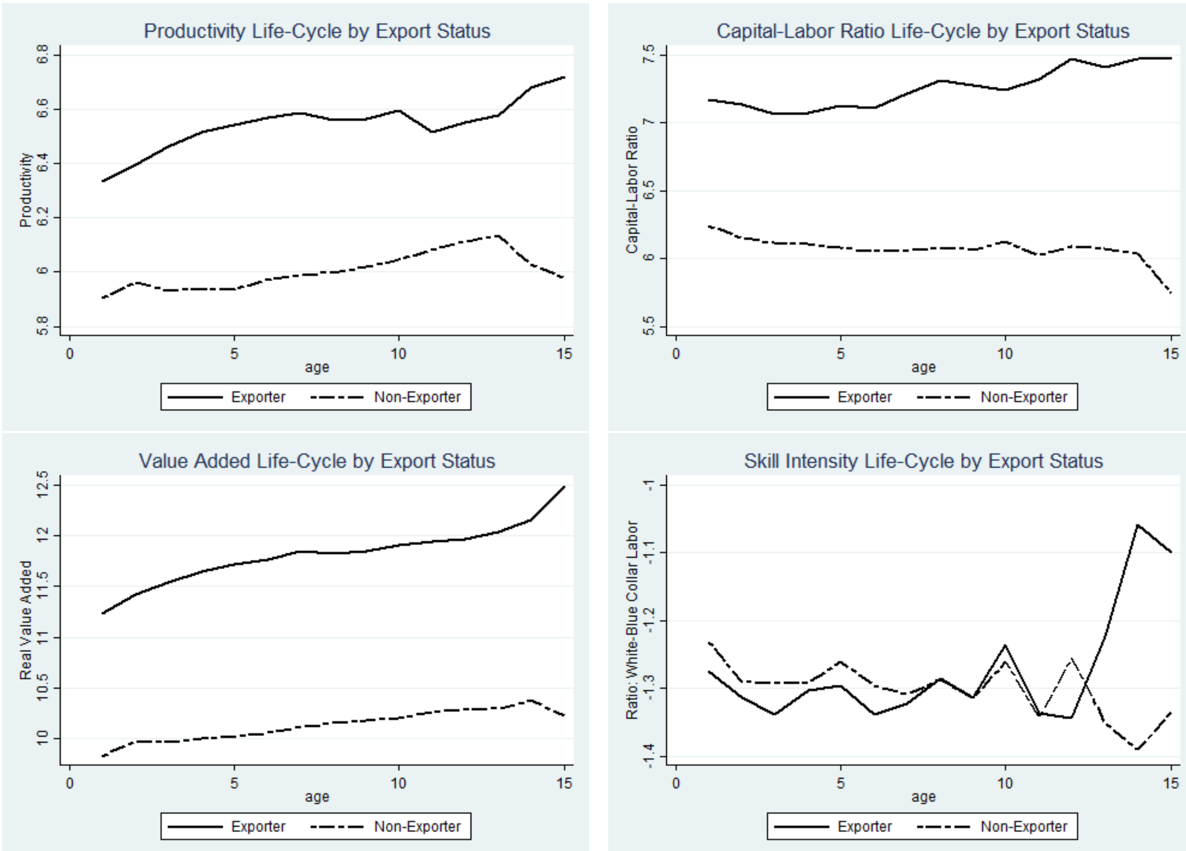
A common finding is that exporters are more productive, pay higher wages, have higher capital intensity and are more skill intensive than non-exporters even before exporting begins. Interestingly these selection effects are not unique to export participation alone but a similar pattern is shared by plants who outsource advertising relative to plants who do not spend on advertising. This is also true for freight, foreign technical support, and accounting users relative to their respective non-user counterparts. This suggests that certain types of plants are more likely to engage in a menu of activities than their less productive counterparts. Another observation, is that similarly to exporting, many of these selection effects for services also exist from the first year that the plant is observed.

To see these patterns, let us begin by first defining our variables. This section evaluates the pre-activity characteristic differences of participants and non-participants in five activities; 1) exporting is denoted as e; 2) advertising is denoted as a; 3) hiring foreign technical support is denoted as p; 4) accounting is denoted as c and 5) freight is denoted as f. Collectively they belong to a set of activities, denoted $s \in (a, p, c, f, e)$. The set of plant characteristics under consideration will be; productivity, denoted as z_{it}; skill intensity, denoted as $\frac{L_w}{L_b}$[15]; skill premium, denoted $\frac{W_w}{W_b}$[16]; total wage bill; and capital per worker. Let each of these characteristics be an element of the set $\mathbb{C}$.

[15]measured as the relative employment of white collar vs. blue collar workers in man-years

[16]measured as the relative wage of white collar vs. blue collar workers

Figure 4: Characteristics over the Life-Cycle: by Export Status



To see the sorting of characteristics by type, let us define a participation dummy, D_i^s. Notice that D_i^s is time invariant, which is to say that plant i is counted as a participant if it ever participates in activity s, then $D_i^s = 1$ and it equals zero otherwise. Define:

$$D_i^s = \begin{cases} 1, & \text{if } \; s_{it} > 0 \; \text{ any } \; t \in T \\ 0, & \text{if } \; s_{it} = 0 \;\; \forall t \end{cases} \tag{18}$$

Then the following simple regression can be run individually for each characteristic in the set $\mathbb{C}$ and is descriptive of average differences in characteristics by activity status:

$$C_{it} = \gamma_0 + \gamma_1 D_i^s + \mathbf{\Gamma}_x \boldsymbol{X}_{it} + \epsilon_{it} \tag{19}$$

Where $\boldsymbol{X}_{it}$ is a set of industry and year dummies. The point estimates for γ_1 are reported in the top section of Table 9 that is labeled "Participation". The cells in Table 9 report point estimates for each activity s and characteristic C_{it} pair. In other words, each cell is the result from an individual regression, which should be interpreted as descriptive. We can see that exporters are on average 74.6 percent more productive, 8.4 percent more skill intensive, pay a 52.5 percent higher wage premium, have a 73 percent higher wage bill and are 112.7 percent more capital intensive on average than their non-exporting counterparts.

Characteristic differences between participants and non-participants can be driven by a selection effect or a learning effect, whereby participation can cause plant characteristics to change. I define a selection dummy, D_{it}^{s}, to explore the degree by which these differences in characteristics exist even before participation begins. This dummy is defined for each activity type and $D_{it}^{s} = 1$ if plant i participates in activity s in the future but has not yet started to participate. It is equal to zero for non-participants. Define D_{it}^{s} as follows:

$$D_{it}^{s} = \begin{cases} 1, & \text{if } \; s_{it} > 0 \;\; \& \;\; t < T_s^B \\ 0, & \text{if } \; s_{it} = 0 \;\; \forall t \\ \text{"."} & \text{if } \; s_{it} > 0 \;\; \& \;\; t \geq T_s^B \;\; \textbf{or} \;\; T_s^B \;\; \text{is unobserved} \end{cases} \tag{20}$$

Where T_s^B is the year that the firm starts activity s[17]. With D_{it}^{s} defined, I run the following simple regression where $\boldsymbol{X}_{it}$ is a set of industry and year dummies:

$$C_{it} = \gamma_0 + \gamma_1 D_{it}^{s} + \boldsymbol{\Gamma}_x \boldsymbol{X}_{it} + \epsilon_{it} \tag{21}$$

The point estimates represent differences between future starters and non-users that exist prior to starting. Each of these differences is formally called a selection effect. The estimates are reported in the second section of Table 9 that is titled, "Selection". We can see that the patterns of selection parallel those of participation for exporters. This suggests, that the differences in characteristics between participants and non-participants exist even before participation begins for exporters. Again, a striking observation is that selection effects across activity type and characteristics with exception

[17]To be defined as a starter, the plant must have a zero entry for activity s in the first year that the plant is observed in the data. Further, I require that the number of zero entries before the first non-zero entry, dominates the number of gaps in the data. A gap is defined as a non-zero entry, followed by one or more zero entries and then followed by a non-zero entry. This requirement increases assurance that the plant did not use a service prior to being observed in the data set

of skill intensity. Future service participants are not any more skill intensive, on average, than non-participants.

A second observation is that the selection effects are smaller in magnitude than the participation effects. For example, future advertisers are 21.6 percent more productive than non-advertisers before advertising begins. However, advertisers are 47.4 percent more productive than non-advertisers when we examine the entire life of the plant. This difference in magnitude leaves open the possibility of a treatment effect, whereby participation changes plant characteristics. In fact, I find that a treatment effect for advertising, accounting and freight services in Section .

Finally, it is possible to determine the first time that these selection effects present themselves in the data. Define a first year of operation dummy, $D^s_{i,t^b} = 1$ if plant i will participate in activity s and the year is equal to the first year of operation, denoted $t = t^b$. The first year of operation dummy will equal zero if the plant is a non-participant in activity s and $t = t^b$. This dummy will be undefined for all time periods other than the first year of operation. Formally, I will define D^s_{i,t^b} as follows:

$$D^s_{i,t^b} = \begin{cases} 1 & \text{if } \; s_{it} > 0 \;\; \& \;\; t = t^b \\ 0 & \text{if } \; s_{it} = 0 \;\; \forall t \;\; \& \;\; t = t^b \\ \text{"."} & \text{if } \; t \neq t^b \end{cases}$$

Now I run the following simple OLS regression individually for each characteristic-activity pair:

$$C_{it} = \gamma_0 + \gamma_1 D^s_{i,t^b} + \mathbf{\Gamma}_x \boldsymbol{X}_{it} + \epsilon_{it} \tag{22}$$

Where $\boldsymbol{X}_{it}$ is a set of industry and year dummies. The point estimates for γ_1 are reported in the bottom section of Table 9 that is labeled "First Operation". It is clear that there are many types of selection effects are observable in the first year that the plant opens its door for operation. For example, future exporters are 37.8 percent more productive on average than non-exporters in the first year of production. The selection dummy from the previous section informs us that future exporters are 62.8 percent more productive than non-exporters over the entire pre-exporting period. Thus a little more than half of the productivity selection effect for exporting can be accounted for by differences within the first year that the plant is born. The rest is due to an evolution of productivity that differs based upon future activity status.

Table 9: Timing of Selection Effects by Activity Type

	$C_{it} =$				
	tfp	$\frac{L_w}{L_b}$	$\frac{W_w}{W_b}$	Wage	$\frac{K}{L}$
Participation	$C_{it} = \gamma_0 + \gamma_1 D_i^s + \Gamma_x X_{it} + \epsilon_{it}$				
s = Exporting	0.746***	0.0837***	0.525***	0.730***	1.127***
	(0.00893)	(0.00849)	(0.00665)	(0.00637)	(0.0138)
s = Advertising	0.474***	0.284***	0.356***	0.455***	0.679***
	(0.0102)	(0.00930)	(0.00826)	(0.00763)	(0.0172)
s = Freight	0.569***	0.154***	0.445***	0.574***	0.865***
	(0.0108)	(0.0107)	(0.00933)	(0.00811)	(0.0184)
s = Accounting	0.253***	0.198***	0.0315	0.0873***	0.128**
	(0.0305)	(0.0241)	(0.0188)	(0.0182)	(0.0414)
s = Foreign Tech Support	0.495***	0.138***	0.269***	0.404***	0.633***
	(0.0105)	(0.00912)	(0.00774)	(0.00791)	(0.0161)
Selection	$C_{it} = \gamma_0 + \gamma_1 D_{it}^s + \Gamma_x X_{it} + \epsilon_{it}$ if $t < T^B$				
s = Exporting	0.628***	0.109***	0.415***	0.565***	0.790***
	(0.0148)	(0.0134)	(0.0119)	(0.0113)	(0.0229)
s = Advertising	0.216***	0.0284	0.112***	0.139***	0.260***
	(0.0171)	(0.0148)	(0.0144)	(0.0134)	(0.0277)
s = Freight	0.117***	-0.0188	0.0893***	0.0757***	0.112***
	(0.0193)	(0.0178)	(0.0162)	(0.0145)	(0.0317)
s = Accounting	0.135***	0.0293	0.0657**	0.0940***	0.0879
	(0.0310)	(0.0252)	(0.0217)	(0.0223)	(0.0482)
s = Foreign Tech Support	0.290***	0.0188	0.160***	0.220***	0.256***
	(0.0177)	(0.0153)	(0.0141)	(0.0135)	(0.0271)
First Operation Year	$C_{it} = \gamma_0 + \gamma_1 D_{it^b}^s + \Gamma_x X_{it} + \epsilon_{it}$ if $t = t^b$				
s = Exporting	0.378***	0.0780	0.334***	0.403***	0.670***
	(0.0479)	(0.0522)	(0.0470)	(0.0442)	(0.0928)
s = Advertising	0.187***	0.0902**	0.0727*	0.134***	0.252***
	(0.0348)	(0.0337)	(0.0340)	(0.0306)	(0.0642)
s =Freight	0.243***	0.00423	0.118**	0.170***	0.185*
	(0.0394)	(0.0389)	(0.0386)	(0.0333)	(0.0728)
s =Accounting	0.171***	0.0369	0.0461	0.0953*	0.141
	(0.0501)	(0.0449)	(0.0425)	(0.0398)	(0.0883)
s =Foreign Tech Support	0.446***	-0.0509	0.209***	0.290***	0.310**
	(0.0572)	(0.0514)	(0.0510)	(0.0444)	(0.102)

Notes: this table reports the OLS estimates on γ_1 for the equation in the section header. Each cell represents an individual regression with C_{it} defined by the column headers and s defined by the row headers. The top section defines D_i^s as a participation dummy equal to 1 if firm i ever engages in activity s. The middle section defines a D_{it}^s if $t < T^B$ to be a Pre-activity dummy thus capturing the selection effect. The bottom section captures differences in characteristics between participants and non-participants that arise from the first year of plant operation. X_{it} is a set of controls including industry dummies and year dummies. Robust standard errors are in parentheses. * indicates $p < 0.05$, ** indicates $p < 0.01$, *** indicates $p < 0.001$. See text for more details.

Predicting Future Export Behavior

Productivity and the Likelihood of Export Participation

It is not clear if the correlation between productivity and export participation is causal or being driven by confounding factor that drives both productivity gains as well as increases the probability of export entry. Certainly, productivity has explanatory power over the cross section since we observe sorting. However, if the relationship is causal then we would expect that increasing productivity, within a firm, would also increase the likelihood of export entry. In other words, a causal relationship should be observable in the time dimension of the panel as well. This is because the time path of productivity should effect the probability of export entry if these two have a causal link. Intuitively, if there is a causal link then some plants should be able to become more productive and thus enter the export market.

This section examines several metrics including productivity growth, positive productivity growth shocks, as well as a metric of consistent positive productivity growth. I cannot find any evidence that the time path of productivity has any statistical explanatory power for export entry. Thus the correlation between productivity and export entry seems to be spuriously driven by an unidentified confounding factor.

To test the predictive power of productivity, I examine three individual cohorts of export entrants in this Chilean panel of manufacturers, 1991, 1992 and 1993 export entrants. Their probability of entering the export market can be modeled with a simple logistic model:

$$logit\left(\mathrm{E}[d^*_{it} = 1]\right) = \beta_0 + \beta_g g(z_{it}) + \boldsymbol{\beta}_x \boldsymbol{X}_{it} \tag{23}$$

where d^*_{it} is the export entry dummy as defined in Equation 24, $g(z_{it})$ is a function of plant productivity that will take several different specifications. $\boldsymbol{X}_{it}$ is a set of 2-digit industry and year dummies. The export entry dummy, d^*_{it} is intended to capture export entry and not the probability of continuing to export. Thus it is equal to 1 if the firm enters exporting in time t, zero if plant i does not export in time t and never exported prior to t. It is undefined for previous exporters or whenever export entry is not observable.

Fitting the model in Equation 23 with $g(z_{it}) = z_{it}$ yields a positive and highly significant point estimate of 0.877, which can be found in Table 10. This is simply evidence of sorting. A lagged value of z_{it} is also significant but omitted for brevity. This tells us that more productive plants are more likely to start exporting. But this does not necessarily tell us that productivity in itself can lead to export entry. For example it could be the case that other actions boost productivity and at the same time lead to export entry.

The robustness of productivity as a predictor of export entry breaks down after we start looking at functions of productivity instead of simply levels. To test the time path of productivity let us first see if contemporaneous growth rates, defined as $\dot{z}_{it}$, or lagged growth rates of productivity are predictive of export entry. This is done by defining $g(z_{it}) = \dot{z}_{it}$ and then estimating Equation 23. From Table 10, we can see that neither, $\dot{z}_{it}$, nor $\dot{z}_{i,t-1}$ has any significance in increasing the likelihood of export entry. Not only are the point estimates statistically insignificant but they are also low in magnitude at 0.0435 and 0.00614 respectively. This might not be surprising since productivity is measured as a residual.

Next, I loosen the specification and ask if a large productivity shock is predictive of export participation. To capture a large shock, I calculate the standard deviation of productivity growth, denoted σ_z and then I create a dummy that is equal to 1 if plant i has productivity growth that is greater than or equal to two positive standard deviations from the mean[18]. If productivity is a mechanism for selection into exporting then it seems likely that such a large positive shock might increase the likelihood of export entry. The point estimate is -0.109 and statistically insignificant.

Contemporaneous productivity shocks might be endogenous to export entry or perhaps there is some time needed to gear-up for exporting. To test lagged effects, whereby a plant receives a large productivity shock in time t and then enters exporting in $t = t + s$, I create a dummy that is equal to one if the plant ever has a shock to productivity growth that is greater than 2 standard deviations from the mean. Once again, the point estimates are statistically not different from zero. Changing the time horizon did not produce different results so they are not reported here.

One possible explanation for this lack of evidence is that the plant views these shocks as transitory. However, there are some plants who always experience positive productivity growth. These plants

[18]The results are invariant if I use a 1 standard deviation cutoff. Also, the mean is statistically zero, although it is not exactly zero.

would be less likely to view these productivity gains as transitory. Thus I define a dummy that is equal to one if the plant always has positive productivity growth. Table 10 reports the point estimates as statistically insignificant. I also test to see if consistent plant contraction, or consistent negative growth decreases the probability of export entry. The point estimate is -0.00929 and insignificant, which suggests that plants who experience consistent negative productivity growth do not have less of a likelihood of entering the export market.

As a final attempt to find some evidence, I try to cook a result with an extremely generous specification. Define a dummy that is equal to one if the average plant level productivity growth is positive and zero if the average plant level productivity growth is negative. This pegs the winners directly against the losers. Even still, the point estimate is negative -0.0227 and insignificantly different from zero. In conclusion, there seems to be a lack of *any* evidence that the time path of productivity has *any* bearing on a firms likelihood to start exporting. It seems that domestic plants cannot be enticed to enter exporting simply because of changes in productivity. Perhaps there is little that can be done to pull a locally minded business man into a global economy.

Entrepreneurial Status and Export Participation

This section begins the search for confounding variables that can drive productivity to increase as well as increase the likelihood of export participation. As mentioned in the introduction, I examine two choices that the firm makes over its life-cycle 1) whether or not to take a business partner and 2) whether or not to outsource a service. This section begins with the first choice, which determines, what I define as, entrepreneurial status or type. The type of the firm is either a one owner firm or a firm in which there are partners. This section asks if entrepreneurial type can increase the likelihood of participation in the following activities; exporting, outsourcing of accounting, advertising, foreign technical support and freight services. The primary activity of focus is exporting. In summary, I find that plants who start with at least one partner are more likely to export and outsource advertising services. There is no relationship for other activities.

Table 10: Productivity as a Predictor of Export Entry

$logit\,(\mathrm{E}[d^*_{it}=1])$								
$g(z_{it})$								
z_{it}	0.877***							
	(0.101)							
$\dot{z}_{it}$		0.0435						
		(0.185)						
$\dot{z}_{i,t-1}$			0.00614					
			(0.190)					
Large Contemporaneous Shock				-0.109				
				(0.731)				
Large Shock					0.0437			
					(0.216)			
Always Positive Growth						-0.473		
						(0.515)		
Always Negative Growth							-0.009	
							(0.733)	
Cooked								-0.023
								(0.163)
Observations	3138	2759	2578	3934	3934	3437	3934	3934

Reported are the point estimates for β_g from the model: $logit\,(\mathrm{E}[d^*_{it}=1]) = \beta_g g(z_{it}) + \boldsymbol{\beta}_x \boldsymbol{X}_{it}$. The function $g(z_{it})$ represents various functions of productivity, which are listed in the left column. Note that $g(z_{it})$ takes on a binary indicator for the following variables.
Large Contemporaneous Shock is defined by a dummy that is equal to one if $\dot{z}_{it} \geq \mu_{\dot{z}} + 2 * \sigma_{\dot{z}}$.
Large Shock is defined by a dummy that is equal to one if $\dot{z}_{it} \geq \mu_{\dot{z}} + 2 * \sigma_{\dot{z}}$ any t.
Always Positive Growth is defined by a dummy that is equal to one if $\dot{z}_{it} > 0 \forall t$.
Always Negative Growth is defined by a dummy that is equal to one if $\dot{z}_{it} < 0 \forall t$.
Cooked is defined by a dummy that is equal to one if $\left(\frac{1}{N}\sum \dot{z}_{it}\right) > 0$ and $d_{it} = 0$ if $\left(\frac{1}{N}\sum \dot{z}_{it}\right) < 0$.
These regressions were run for each cohort of export entrants. This table is estimated using the 1991 cohort of export entrants which is qualitatively representative of the others. Robust standard errors are reported in parenthesis. Please see text for further details. * indicates $p < 0.10$, ** indicates $p < 0.05$, *** indicates $p < 0.01$

Entrepreneurial Type and the Probability of Entry

Consider the following logistic model, which models export entry:

$$logit\,(\mathrm{E}[d^s_{it}=1]) = \beta_0 + \beta_1 D^o_i + \beta_{zb} z_{it^b} + \beta_3 D^L_i + \beta_K \frac{K_{i,t-1}}{L_{i,t-1}} + \beta_z z_{i,t-1} + \beta_W \frac{W^w_{i,t-1}}{W^b_{i,t-1}} + \boldsymbol{\beta}_x \boldsymbol{X}_{it} \tag{24}$$

Where d^s_{it} is defined the same as d^*_{it} in Equation 5. The explanatory variable of interest is the single owner dummy, $D^o_i = 1$ if plant i has one owner in the first year of business and equal to zero otherwise. The productivity in the first year, denoted z_{it^b}, is included as a proxy variable to control

Table 11: Entrepreneurial Class and Export Entry

logit(E[$d^s_{it} = 1$])			Robust to Additional Controls	
	(1)	(2)	(3)	(4)
Single Owner	-0.386**		-0.489***	
	(0.160)		(0.165)	
z_{i,t^b}	0.404***	0.496***	0.315**	0.421**
	(0.125)	(0.169)	(0.128)	(0.174)
z_{t-1}	0.535***	0.529***	0.461***	0.370**
	(0.128)	(0.164)	(0.136)	(0.178)
# Partners		0.0754		0.0166
		(0.0893)		(0.0939)
$\frac{K_{t-1}}{L_{t-1}}$			0.002***	0.001***
			(0.001)	(0.000)
$\frac{W^w_{t-1}}{W^b_{t-1}}$			0.462***	0.464***
			(0.130)	(0.171)
borrower dummy			0.978***	0.449
			(0.351)	(0.386)
Industry	Y	Y	Y	Y
Year	Y	Y	Y	Y
Industry x Year	Y	Y	Y	Y
Observations	5931	2753	5352	2452

This table reports the point estimates from the logistic regression in Equation 24, where d^s_{it} identifies export entry, z_{i,t^b} is the productivity of plant i in the first year of operation, z_{t-1} is lagged productivity, $\frac{K_{t-1}}{L_{t-1}}$ is lagged capital intensity, $\frac{W^w_{t-1}}{W^b_{t-1}}$ is lagged skill premium and the borrowing dummy is equal to one if plant i ever paid interest. The number of partners is only counted when the plant has more than one owner. If single ownership is included in the definition of the number of partners then the point estimate becomes positive and statistically significant. This result does not give additional information and thus it is omitted. Robust standard errors are reported in parenthesis. Please see text for further details. * indicates $p < 0.10$, ** indicates $p < 0.05$, *** indicates $p < 0.01$

for unobserved characteristics such as differences in ability. Previous findings, that the time path of productivity is independent of the dependent variable d^s_{it}, provides justification for z_{it^b} as a valid proxy for ability. $\boldsymbol{X}_{it}$ includes industry, year, industry-year and plant entry fixed effects. Plant entry is the first year of plant operation and including this controls for persistent yearly effects that might be caused by economic conditions during this year that persist over time. For example, it is cheaper to buy capital during economic down turns and thus starting a new plant in this period might make better equipment affordable. This advantage can persist and would not be captured by yearly fixed effects. Equation 24 is estimated with and without lagged capital intensity $\frac{K_{i,t-1}}{L_{i,t-1}}$, lagged productivity $z_{i,t-1}$, and lagged skill premium $\frac{W^w_{i,t-1}}{W^b_{i,t-1}}$. The inclusion of these variables is to show robustness.

Point estimates for Equation 24 are reported in Table 11. The key observation is that starting a plant as a single owner is significantly negatively correlated with the likelihood of becoming an exporter, while having more partners conditional on a partnership is not significant. Another way to interpret this is to say that partnership is relevant on the extensive margin but not on the intensive margin. One striking feature is that this early business decision has prediction power on exporting decisions that are separated by relatively long periods of time. Another striking observation is the magnitude on the point estimate for starting as a single owner.

Finally, I would like to know if entrepreneurial type can predict service adoption. The natural extension is to re-estimate the model for each of the outsourcing activities, freight, advertising, foreign technical support and accounting. Point estimates are reported in Table 12. I do not find evidence that starting a manufacturing plant as a single owner statistically effects the probability of starting these services, with the exception of advertising, which has a negative correlation. One interpretation is that subsistence entrepreneurs seem to be content to remain small in size and thus they do not advertise. The non-result for the other types of outsourced services hints that entrepreneurial type does not effect most types of service adoption.

Service Outsourcing and the Likelihood of Export Entry

This section provides some evidence that outsourcing a business service is correlated with an increase in export participation. The conclusions in this section rest on Table 13. The table can be replicated in regression form. However, most of the coefficients are obvious from Table 13. Also, presenting this in table format is more honest since I am not going to address any identification problems.

Table 13 shows that a higher percentage of export entrants outsource services compared to their non-exporting counterparts. Service participation predicts export participation. Also, note that service participation precedes export participation in terms of timing. The year when a plant starts exporting is denoted as T_e and the year that the plant start a service s, is denoted as T_s. When $T_s < T_e$, then the plant starts service s before starting to export. The majority of export entrants fall into this category.

Table 12: Entrepreneurial Class and Service Outsourcing

logit(E[$d_{it}^s = 1$])	s = Advertising	s = Freight	s = Accounting	s = Foreign
Single Owner	-0.207**	-0.0696	-0.199	0.0894
	(0.0996)	(0.114)	(0.134)	(0.172)
z_{i,t^b}	0.0476	0.0363	0.136	0.187
	(0.0825)	(0.0965)	(0.117)	(0.131)
z_{t-1}	0.148*	0.209**	0.101	0.454***
	(0.0806)	(0.0955)	(0.117)	(0.138)
$\frac{K_{t-1}}{L_{t-1}}$	0.002	0.001**	-0.009	-0.003
	(0.007)	(0.002)	(0.000)	(0.008)
$\frac{W_{t-1}^w}{W_{t-1}^b}$	0.241***	0.330***	0.0333	0.196
	(0.0728)	(0.0877)	(0.101)	(0.129)
borrower	0.343**	0.747***	0.495***	0.0933
	(0.135)	(0.154)	(0.173)	(0.276)
Industry	Y	Y	Y	Y
Year	Y	Y	Y	Y
Industry x Year	Y	Y	Y	Y
Observations	3860	3066	1127	9134

This table reports the point estimates from the logistic regression in Equation 24, where d_{it}^s identifies service entry, of type s, z_{i,t^b} is the productivity of plant i in the first year of operation, z_{t-1} is lagged productivity, $\frac{K_{t-1}}{L_{t-1}}$ is lagged capital intensity, $\frac{W_{t-1}^w}{W_{t-1}^b}$ is lagged skill premium and the borrowing dummy is equal to one if plant i ever paid interest. Each column reports a different s as is labeled on the column header. Robust standard errors are reported in parenthesis. Please see text for further details. * indicates $p < 0.10$, ** indicates $p < 0.05$, *** indicates $p < 0.01$

This pattern of relative timing is almost like an argument for Granger causality. There is a strong relationship showing that service entry comes before exporting entry. However, I want to temperate these results because service entry might be driven by the same latent factors that drive export entry. Note that Table 9 shows us that selection effects are similar across all activity types. This increases the likelihood that all of these activities might be driven by the same mechanism. Studies that try to link services and trade should also take caution with interpretation in light of the evidence that I have presented thus far.

Table 13: Relative Timing of Export and Service Participation

	Any Service	**Advertising**	**Accounting**	**Tech-Supp**	**Freight**
			Export Entrants - $N = 408$		
$T_s < T_e$	216	118	84	62	72
$T_s = T_e$	33	15	7	10	11
$T_s > T_e$	50	13	11	22	10
Non-User	1	34	9	266	16
			Non-Exporters - $N = 5,334$		
Starter	2,750	1,482	1,082	221	1,140
Non-User	221	2,021	476	4,671	1,544

Notes: This table reports the number plants that start a service before, during and after entering the export market by service type. The year of starting a service is denoted T_s and the year of entering the export market is denoted T_e. The number of export starters is denoted as N.

Accounting for Selection Effects

The previous section showed some evidence that productivity does not predict export entry and that entrepreneurial type as well as service outsourcing are predictors of future export participation. A larger set of questions now remains: can entrepreneurial type and/or service entry explain any of the systematic differences between exporters and non-exporters. In other words, can entrepreneurial type or service participation explain the productivity selection effect of export entry? I have developed a separate accounting exercise for each question. A different econometric approach is needed because entrepreneurial type is a choice from the beginning of the life-cycle while the choice of service adoption is from the middle of the life cycle. These are presented in the following sections.

Entrepreneurial Type and Selection Effects

This section evaluates the selection effect by entrepreneurial type. The exercise is a more formal way of showing average differences but it is not intended to identify a causal link. The following simple regression captures these differences.

$$C_{it} = \gamma_0 + \gamma_1 Export_{it} + \gamma_2 Single_{it} + \gamma_3 Export_i * Single_i + \mathbf{\Gamma}_x \boldsymbol{X}_{it} + \epsilon_{it} \tag{25}$$

The dummy for being a future exporter is interacted with a dummy for starting as a single owner. Each is also included individually. On the left hand side, I have plant characteristics that will be one element from the set of productivity, skill intensity, skill premium, wage, and capital intensity. The point estimates are reported in Table 14. The coefficient on the interaction term γ_3 is significant but only at the 10 percent level for productivity and at the 5 percent level for the skill premium. Taken literally, these point estimates say that the systematic differences between exporters and non-exporters is less on average for single owner businesses than it is for multi-owner businesses; roughly 42 percent less in terms of productivity differences and 54 percent less in terms of differences in skill premium.

Table 14: Entrepreneurial Type and the Selection Effect of Exporting

$C_{it} = \gamma_0 + \gamma_1 Export_{it} + \gamma_2 Single_{it} + \gamma_3 Export_i * Single_i + \mathbf{\Gamma}_x \boldsymbol{X}_{it} + \epsilon_{it}$					
	$C_{it} =$				
	tfp	$\frac{L_w}{L_b}$	$\frac{W_w}{W_b}$	Wage	$\frac{K}{L}$
γ_1	0.456***	-0.0837	0.422***	0.364***	0.478***
	(0.0713)	(0.0877)	(0.0778)	(0.0693)	(0.150)
γ_2	-0.0228	-0.293***	0.246***	0.106***	-0.0294
	(0.0305)	(0.0373)	(0.0310)	(0.0260)	(0.0583)
γ_3	-0.193*	0.114	-0.229**	-0.0590	0.170
	(0.110)	(0.126)	(0.103)	(0.0978)	(0.226)
Observations	2523	2652	2326	2652	2585
Industry	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y
Industry x Year	Y	Y	Y	Y	Y

The regression is estimated individually for tfp, skill intensity, skill premium, wage and capital intensity. Robust standard errors are reported in parenthesis. * indicates $p < 0.10$, ** indicates $p < 0.05$, *** indicates $p < 0.01$

Business Service Adoption and Selection Effects

This section measures the part of the selection effect into exporting that can be explained by service adoption. If service outsourcing is more prevalent in plants who export versus domestic plants then it is reasonable that exporters might receive some added benefits from these services relative to non-exporters. Measuring this relative advantage is confounded by many factors of selection that makes a simple regression approach largely unfeasible. A further complication is that both selection into outsourcing and selection into exporting might be driven by the same mechanism. To address this complicated issue of selection, I will use a model by Hsiao Ching and Wan (2012) that is invariant to selection. This model can recover a useful counterfactual; the state of the world had no plants started service outsourcing.

Secondly, this approach will leverage a helpful feature of this data; starting to outsource each service precedes export entry in terms of timing. This is a very robust characteristic of this data set. I also showed evidence that plants who start outsourcing freight, advertising or accounting services all experience gains in productivity as a result, see Section for details. These two things make my econometric approach feasible in terms of potentially finding an economically significant effect.

The econometric goal is to individually consider the adoption of freight, accounting, foreign-technical-support and advertising services to see if these activities can account for pre-participation productivity differences between exporters and non-exporters that are summarized in Table 9.

It would be very useful to recover a counterfactual; the productivity of plants had they not outsourced various services. Then I compare the selection effect of exporting using factual data, denoted z^1_{its} and counter-factual data, denoted z^0_{its}. To solidify ideas, the factual data is the productivity of plant i in time t after adoption of service s and the counterfactual data is the productivity of plant i in time t after adoption of service s has service s never been adopted. The percentage difference between the observed productivity selection effect into exporting and the counterfactual selection effect is what I want to estimate.

To see the strategy, let me begin with an ideal world where counterfactual data is directly observable. After the strategy is clear then I will show another strategy to obtain this fictitious data. If we had counterfactual data then we would estimate the following equation, where D^B_{it} is a selection dummy for export entry that is defined in Equation 20:

$$z^0_{its} = \gamma^0_0 + \gamma^0_1 D^e_{it} + \gamma^0_x X_{it} + \epsilon^0_{it} \tag{26}$$

The point estimate $\hat{\gamma}^0_0$ is the estimated selection effect if plant i had not used service s. Notice that only z^0_{its} is not observed, which is the productivity of future exporters had they not adopted service s. If we had z^0_{its} then we can compare point estimates of $\hat{\gamma}^0_0$ with the point estimates of the following equation:

$$z^1_{it} = \gamma^1_0 + \gamma^1_1 D^B_{it} + \gamma^1_x X_{it} + \epsilon^1_{it} \tag{27}$$

Then the statistic of interest would be:

$$\hat{P}_s = \frac{\left(\hat{\gamma}^1_x - \hat{\gamma}^0_x\right)}{\hat{\gamma}^1_x} * 100$$

Another way to think of $\hat{P}_s$ is the contribution of service s to pre-exporting productivity growth differences between future exporters and non-exporters. This statistic of interest can then be bootstrapped to obtain standard errors.

Possibly the larger challenge is to estimate the counterfactual data. I adopt the method of Hsiao Ching and Wan (2012) for this task. This model assumes the data generating process is driven by the following general factor model.

$$z_{it} = ax_{it} + b_i f_t + u_{it} \tag{28}$$

This says that productivity of firm i is driven by observable characteristics, denoted x_{it} as well as by a matrix of un-observable factors that are common across individuals, denoted f_t, and a matrix of unobserved individual characteristics that are constant across time, denoted b_i. We can generally think of f_t as being macroeconomic factors and b_i as unobserved firm heterogeneity. Firm specific characteristics are allowed to interact with time fixed effects although they are also allowed to act independently on z_{it}. See Section for a detailed description of this estimator.

Below is a brief overview of the method. Please see for a detailed description of the method. I first capture the correlation pattern of control plant characteristics and treated plant outcomes by running the following regressions over the ex-ante sample:

$$z^T_{its} = \gamma_0 + \Gamma_z \tilde{z}^c_{its} + \Gamma_x X_t + \epsilon_{it} \qquad t = 1, ..., (T_s - 1) \tag{29}$$

where z^T_{its} is the productivity of future starters of service s. $\tilde{z}^c_{its}$ is a vector of characteristics of control plants, who never use service s. The characteristics include the control plant's productivity, real investment,real capital stock, real material usage, and industry interaction terms. The year that the firm adopts service s is denoted by T_s.

Estimation of Equation (29) establishes the correlation pattern of service adopters and non-service users before service usage begins. Then a forecast for the ex-post counter-factual productivity of service adopters in the absence of service usage, denoted $z^0_{it^s}$, is estimated by combining the pre-treatment point estimates from Equation 29, together with the post-treatment observed outcomes of the control plants.

$$\hat{z}^0_{it} = \hat{\gamma}_0 + \hat{\Gamma}_z \tilde{z}^c_{it} + \hat{\Gamma}_x X_t \qquad t = t_s, ..., T \tag{30}$$

With the counter-factual in hand, we can now re-estimate the pre-exporting productivity differences between future exporters and non-exporters had neither group started using service s. To do this, I define a productivity vector that is equal to observed productivity before entry into service s and equal to the estimated counter-factual productivity afterwards.

Estimates are reported in Table 15. The selection effect using counterfactual data for service outsourcing for service s is reported row $\hat{\gamma}^1_{1,s}$ for each type of service. The selection effect for the observed data is reported in the row $\hat{\gamma}^0_{1,s}$. The percentage difference between the two, $\hat{P}$ can be interpreted as the percentage of the selection effect of exporting that can be explained by entry into service s for this sub-sample of s type service entrants.

The estimation strategy as described thus far is restricted in the sense that I am looking at a sub-sample of service starters of type s. This leaves out a large portion of the sample and might not reflect the effect for the entire population. To address this I add the rest of the sample to the group and re-estimate the same statistics. This is reported in the bottom half of Table 15.

These results only apply to a small subset of plants; those who start a service. One striking feature, is that these relatively small number of plants still have significant explanatory power in explaining differences in larger groups. When the sample is broadened to include all plants where entry is observed in either service s or exporting, then the magnitude of $\hat{P}$ is reduced to roughly 10 percent for all services except foreign-technical-support[19]. A 27 percent average effect in only 118 Advertising Entrants, having 1398 observations, is capable of explaining 9 percent of the selection effect over 16,782 observations.

Conclusions

The selection into exporting seems to be largely driven by a mechanism that occurs before the first year of plant operation, which supports the Melitz idea of endowment. There is evidence that the productivity of exporters diverges even more during the pre-exporting period. However, productivity

[19] See Appendix for definition of entry. This sample includes plants that always use service s as well as those who never use service s whereas the first sample only includes service starters.

Table 15: Service Outsourcing Contriubtion to the Selection Effect of Export Entry

Sample - Only Service Entrants of type s				
	$s =$ Advertising	$s =$ Accounting	$s =$ Tech-Supp	$s =$ Freight
$\hat{\gamma}^1_{1,s}$	0.538***	0.491***	0.249	0.572***
	(0.084)	(0.095)	(0.220)	(0.084)
$\hat{\gamma}^0_{1,s}$	0.391***	0.315***	0.309***	0.429***
	(0.088)	(0.088)	(0.120)	(0.088)
$\hat{P}_s$	0.274**	0.360***	-0.436	0.252**
	(0.110)	(0.1261)	(7.488)	(0.109)
Number of Observations	1398	991	793	824
Sample - Export and Service Entry is Observable				
	Advertising	Accounting	Tech-Supp	Freight
$\hat{\gamma}^1_1$	0.571***	0.675***	0.559***	0.494***
	(0.051)	(0.068)	(0.050)	(0.063)
$\hat{\gamma}^0_1$	0.519***	0.617***	0.563***	0.436***
	(0.051)	(0.073)	(0.050)	(0.065)
$\hat{P}$	0.091***	0.087**	-0.007	0.118**
	(0.036)	(0.041)	(0.009)	(0.052)
Number of Observations	16782	10050	20270	12497

$\hat{P}_s$ is the percent of the selection effect into exporting that can be explained by starting service s. The sample is restricted to plants who start service s. $\hat{P}$ is the same statistic except applied to a larger sample that includes all plants where both export entry and service entry is observable. Bootstrapped standard errors are reported in parenthesis. Observations are mean observations from the bootstrapped samples. Please see text for details. * indicates $p < 0.10$, ** indicates $p < 0.05$, *** indicates $p < 0.01$

itself seems to have little influence on a firms likelihood of entering the export market. Thus it seems that the correlation between productivity and export entry is spuriously driven by confounding variables.

This paper identifies two potential confounders; entrepreneurial type and the adoption of different types of services. Both can account for part of the selection effect into exporting and both are associated with an increased likelihood of export participation.

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APPENDIX

Data and Defining Entry

This paper will examine entry into four individual services; freight, advertising, foreign technical support, and accounting services. An entrant into service "s" will be plants who have a zero entry for the first period that data is observed followed by a non-zero entry at some point afterwards. For consistency, the plant must also not have any gaps in the data that are equal to or greater than the number of unbroken zero entries at the begining of the sample. For example, if a firm enters the market in 1985 and reports zero services until 1987 then there cannot be more than one gap in the rest of the sample where the firm reports a zero entry followed by a non-zero entry. Also, firms who only use services for one period are not counted as service entrants.

HCW: Model Selection

The covariates $\tilde{z}_t^c$ are selected to maximize the fitness of the in-sample prediction. This task is simple when the number of potential covariates is small. However, there is a curse of dimensionality when the covariate list is not small. A systematic search for model selection has been aided by the rapid advancement of computer technology as well as by algorithms that reduce computational intensity. Lindsey and Sheather provide code that utilizes a leaps-and-bounds algorithm that is developed by Furnival and Wilson. This leaps-and-bounds algorithm confines the search space and thus further reduces the computational intensity by eliminating unlikely models from the search universe. This makes a systematic search computationally feasible. The criteria that I chose for model selection is to minimize is the Bayesian Information Criteria (BIC) although one could also maximize adusted R^2. The BIC criteria generally picks smaller models which leads to a more parsimonious model.

The search universe includes control plant's productivity, real investment,real capital stock and real material usage. Finally, I also include year dummies as well as industry dummies. The search algorithm of Lindsey and Sheather provides a vector of $\tilde{z}_t^c$ consisting of the control plant's productivity and other controls, which then are used to estimate Equation 10.

Implementation of LP Productivity Measure

Also, take note that Chilean data was used by Levinsohn-Petrin (2003) who show that the problem of zero values is far less severe than Olley-Pakes estimation, which relies on investment data that frequently is zero. Plant-specific, time-varying productivity are extracted by estimating a production function that features Hicks-neutral technology for each industry:

$$Y_{it} = A_{it} W_{it}^{\beta^w} B_{it}^{\beta^b} K_{it}^{\beta^k}$$

. Y_{it} represents output of plant i, in year t, B_{it} stands for blue-collar labor, W_{it} stands for white-collar labor, K_{it} for capital, M_{it} for material usage; A_{it} is the parameter capturing the Hicks neutral technology. Here labor is measured as person-years. Taking logs of both sides of the production function gives a reduced form regression where lower case letters indicate log-level values of the variables.

$$y_{it} = \beta_0 + \beta^k k_{it} + \beta^w l_{it}^w + \beta^b l_{it}^b + z_{it} + \epsilon_{it} \tag{31}$$

This specification decomposes technology, after controlling for labor and capital, into an average technology component β_0, a plant-specific technology z_{it}, and an *iid* component ϵ_{it} so that defining $a_{i,t}$

as technology of plant i in year t: $a_{i,t} = \beta_0 + z_{it} + \epsilon_{it}$. The plant-specific productivity z_{it} is measured following Levinsohn-Petrin (LP) (2003), correcting the simultaneity bias using plants' material usage. Each regression is conducted at the two-digit ISIC level to control for the cross-sector permanent productivity heterogeneity. Industry-by-industry regression also allows industries to have different production technologies. Real values of output, capital, and materials are calculated using series on deflators provided by the Chilean National Statistics Bureau which include, at the three-digit ISIC level, deflators for output, raw materials, capital buildings, capital machinery, capital vehicles, capital furnitures, and miscellaneous capital. The base year is set as 1985.[20] Vogel and Wagner (2011) argue productivity outliers can bias the results severely. Accordingly, estimators more than 7 standard deviations from the mean are dropped. This gives us total sample size of 40,877 observations for 5,631 plants.

Propensity Score Matching

Discussion on Liquidity Proxy

Presumably, plants who are able to make loans or engage in external investment projects have a liquidity that is in excess of that which is needed for internal investment. Reducing the effectiveness of the proxy is the possibility that some of these investments have longer terms, which decreases the liquidity of these investments. However, longer termed investments are not likely to be a significant problem for this proxy for two reasons. First, lenders are likely to incorporate liquidity needs into their objective function. Second, external investment is created by the existence of excess cash flows. It is precisely the excess in liquidity that motivates financial managers to invest externally. One could assume that the excess liquidity arrive at a relatively constant rate but this rate differs by plant. Thus some plants have excess in every period and can lend while others have a deficit, which prevents them from lending. In this case, the lending plants have higher access to liquidity, relative to non-lenders, even if they cannot liquidate their current portfolio.

[20]Unfortunately, this data set does not have data on plant-level output price that is necessary for calculating cost-based productivity measures as proposed by Garcia and Voigtlander (2013). An alternative is proposed by Hsieh and Klenow (2009) to proxy quantity-based productivity assuming a model of monopolistic competition. Such exercises are left for future research.