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New Perspectives on Policy Uncertainty

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New Perspectives on Policy Uncertainty

by

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Committee in charge:

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Professor Benjamin Faber
Professor Thibault Fally

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Abstract

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In recent years, the ubiquitous and intensifying nature of economic policy uncertainty has made it a popular explanation for weak economic performance in developed and developing markets alike. The primary channel for this effect is decreased and delayed investment as firms adopt a "wait and see" approach to irreversible investments (Bernanke, 1983; Dixit and Pindyck, 1994). Deep empirical examination of policy uncertainty's impact is rare because of the difficulty associated in measuring its magnitude and changes over time. In this dissertation, I leverage the recent advent of global news aggregators to directly identify and measure policy uncertainty shocks based on "news chatter" in the press. Unlike previously used measures of economic uncertainty (e.g., strike days or exchange rate volatility), "news chatter" uncertainty indices pick up economic volatility as well as the threat or anticipation of volatility stemming from policy uncertainty, whether or not it comes to fruition. The more holistic character of such measures allows for a more nuanced examination of uncertainty's impact on firm decisions and outcomes. After constructing novel measures of policy uncertainty, I then explore how they translate into economic outcomes that extend beyond the traditional investment channel.

In Chapter 1, I offer new insights into the channels policy uncertainty operates through by constructing a novel and rich dataset of type-specific policy uncertainty indices and leveraging previously unexamined variation in firm-level exposure to external markets to create firm-specific measures of policy uncertainty. Specifically, I exploit variation in firms' exposure to external markets to construct a firm-level measure of policy uncertainty. The approach both highlights a new channel for policy uncertainty and allows for stronger causal identification of the effects of policy uncertainty on economic performance. As part of this effort, I refine prior approaches to measuring policy uncertainty and distinguish between generic, fiscal, monetary, and trade policy uncertainty. I find that firms with greater exposure to external markets tend to experience larger declines in investment, sales, profits, and employment when fiscal and monetary policy uncertainty increase. Unexpectedly, increases in trade policy uncertainty appear to have a positive impact on exports for exposed firms. Both sets of findings can be rationalized in a standard model

of firm investment under uncertainty. In particular, I present evidence that exposed firms may perceive increased uncertainty around trade agreement negotiations as a signal that negative outcomes are less likely in the near-term, incentivizing immediate investments.

Historically, exchange rate depreciation makes a country's exports more competitive and cheaper, increasing its exports. Since the end of the Great Recession, many countries have seen this relationship weaken. In Chapter 2, I advance policy uncertainty as a new explanation for such dilutions in the relationship between exchange rates and export performance. Using South Africa as a case study, I find that increased policy uncertainty diminishes the responsiveness of exports to exchange rate fluctuations.

In Chapter 3, I examine a more extreme version of policy uncertainty–regime uncertainty. In 2010, the International Criminal Court (ICC) issued an indictment against six of Kenya's foremost leaders for crimes against humanity related to 2008 post-election violence. I find strong evidence that firms connected to the accused experienced declines in valuations during ICC shocks, with particularly negative revaluations for firms with highly public links to the accused. The results suggest that the negative effects of regime uncertainty outweighed any positive "rule of law" shock that the ICC's intervention might have provided to firms.

Together the studies provide new insights on the connections between policy uncertainty and weak aggregate economic performance. In addition to offering more nuance for policy directives, the results will help discipline future theoretical efforts to more accurately model complex dynamics in modern open economies.

This is dedicated to the bright memory of Mosupatsela Karabo Vika Moleah.

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Chapter 1

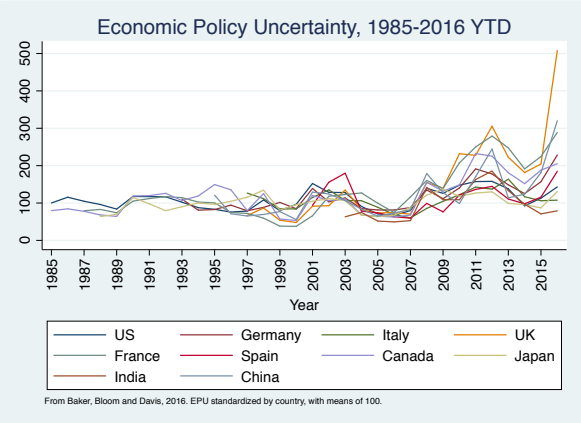
Unpacking Policy Uncertainty: Evidence from European Firms

"Business leaders from kitchen-table start-ups to vast multi-nationals are already telling me that the uncertainty over the [British EU] referendum result is causing them to delay investment decisions, to think twice about creating new jobs." - *The Telegraph*, 14 May 2016

1.1 Introduction

In the wake of the Great Recession, economic policy uncertainty has increased across the globe (see Figure 1.1). The intensifying nature of policy uncertainty makes it a popular explanation for recent economic puzzles: sluggish recoveries; the outsized trade collapse during the crisis; and disconnects between the real effective exchange rate (REER) and exports.[1] At the micro-level, firms have long cited policy uncertainty as a primary impediment to doing business (Smith & Hallward-Driemeier, 2005).

Figure 1.1: Global Economic Policy Uncertainty Index, January 1997-August 2016



Notes: The figure shows Davis's (2016) real GDP-weighted news chatter policy uncertainty measure for 16 countries, representing two-thirds of global output. The period average for the index is 62 percent higher in the period since December 2007, the start of the Great Recession according to the NBER's Business Cycle Dating Committee. The index for each country is constructed by counting articles that match search algorithms that pick up triple mentions of uncertainty, the economy, and policy and are normalized to mean=100. More detail on the construction of such measures features in Section 4.

Despite its ubiquity as an explanation for negative outcomes, there are limitations in our understanding of the consequences of policy uncertainty. First, firm-level analyses are rare in the literature. Most empirical investigations of policy uncertainty employ vector auto-regression or cointegration approaches at the macro-level, limiting the ability to control for confounding factors. The difficulty in measuring vulnerability to policy uncertainty at the firm-level is a primary reason for the popularity of macro analyses. Second,

[1]The United States, Canada, Germany, Italy, the United Kingdom, France, India, and China all experienced historically high policy uncertainty in the period since 2009 (Baker, Bloom, & Davis, 2016). Some attribute the muted U.S. recovery to domestic policy uncertainty (e.g., Baker, Bloom, & Davis, 2012; Taylor, 2014; and Bordo, Duca, & Koch, 2016), while Hlatshwayo & Saxegaard (2016) find that policy uncertainty contributes to the REER-export disconnect.

policy uncertainty is largely treated as a homogenous, rather indeterminate concept.[2] Little is known about what types of policy uncertainty are particularly detrimental and how the impact of policy uncertainty varies across firms (e.g., how fiscal policy uncertainty impacts a Ford factory relative to a mom-and-pop grocer).[3]

This paper seeks to address these limitations by analyzing the impact of different types of policy uncertainty at the firm-level. To do so, I construct a novel and rich database of disaggregated measures of policy uncertainty that outperform prior measures based on several metrics. Second, I exploit variation in export shares to create firm-specific measures of exposure to external vs. domestic policy uncertainty, allowing for a stronger causal identification of policy uncertainty's effect on economic outcomes. Finally, I explore whether there is variation in firm responses to fiscal, monetary, and trade policy uncertainty. Policy uncertainty is both an impulse and propagation mechanism for broader economic uncertainty. However, relative to economic uncertainty, policy uncertainty is of particular interest because it can be moderated, if not altogether avoided, by governments.

The choice to employ firms' trade exposure as the central margin of variation reflects recent findings in the literature. For example, Colombo (2013) finds that shocks to U.S. economic policy uncertainty generate drops in European industrial production, suggesting an important role for external policy uncertainty in driving domestic outcomes. Intuitively, the macroeconomic developments of trading partner countries are likely to be important for a firm's outcomes if its revenues are largely export sales. While I use export shares in an effort to get at the cross-country channel for policy uncertainty, export exposure also reflects other linkages. Large exporters also tend to be large importers (Amiti, Itskhoki, & Konings, 2014) and bilateral trade flows are a key determinant of how financial market shocks are transmitted across countries (Forbes & Chinn, 2004).

There is a growing literature on how to measure policy uncertainty (e.g., Alexopoulos & Cohen, 2009; Gunnemann, 2014; Redl, 2015; and Baker et al., 2016). Traditionally, policy uncertainty was proxied for by measures of economic uncertainty (e.g., stock market volatility, strikes, and mentions of uncertainty in central banks' statements) due to lack of better alternatives. The advent of news aggregators allows for more nuanced measurement of policy uncertainty, capturing the nature and magnitude of policy uncertainty in a way that is most salient for businesses.[4] While useful, these measures can also fall prey to bias and noise stemming from incorrectly specified and overly broad search algorithms. This paper builds and improves on prior approaches by adopting a multi-stage refinement process for constructing the search algorithms, leveraging the expertise of journalists

[2]For instance, Stock & Watson (2012), Colombo (2013), Sum (2013), and Bordo, Duca, & Koch (2016) all use generic measures of economic policy uncertainty without examining the time-varying sources of such policy uncertainty.

[3]Where policy uncertainty is disentangled into particular types, analysis focuses on the most straightforward of relationships—trade policy uncertainty on trade outcomes (Handley & Limao, 2013, 2015; Pierce & Schott, 2016) or healthcare and defense policy uncertainty on healthcare and defense firms (Baker et al., 2016).

[4]The use of the narrative approach in capturing policy shocks was championed in earlier work (e.g., Romer & Romer, 1989, 2004).

and professors of journalism to discipline the algorithm. Altogether, I construct 308 novel "news chatter" type-specific, time-varying measures of policy uncertainty across 44 countries.[5]

The new measures perform well with respect to multiple benchmarks: accuracy; variation; and differentiation from measures of economic uncertainty. Across the sample period and with a high level of accuracy, the measures pick up increases in type-specific policy uncertainty with little overlap across measures. Comparing respective human audits conducted on Baker et al.'s (2016) measure and the generic measure of this paper, my measure shows a 35 percent improvement in accuracy relative to their measure.[6] The new measures also show a large degree of variation across time and across countries.[7] Finally, the measures are not highly correlated with traditional measures of economic uncertainty (e.g., the VIX, deviations in professional growth forecasts, or stock market volatility), indicating that these measures of policy uncertainty, while related, are not merely proxies for economic uncertainty or negative economic outcomes.

To examine the effects of the various types of policy uncertainty, I use Bureau Van Dijk's Amadeus database of firms across four European countries—the United Kingdom, Greece, Turkey, and France—over the period from 2003 to 2015. At the firm-level, "effective" policy uncertainty is the ratio of external policy uncertainty relative to domestic policy uncertainty interacted with firm export shares to measure exposure. I find that firms with greater exposure to external markets tend to experience larger declines in investment, sales, profit, and employment when effective fiscal and monetary policy uncertainty increase. This finding fits with the predictions of real options theory. A model of firm choice under uncertainty suggests that rising policy uncertainty associated with increases in potentially negative outcomes should lower current investment as firms increasingly opt to "wait and see" with respect to investments that feature sunk or partially irreversible costs.[8] Increases in external policy uncertainty relative to domestic policy uncertainty signal an increased likelihood of bad outcomes for firms that are highly exposed to external markets, causing such firms to delay investments.

Unexpectedly, increases in effective trade policy uncertainty appear to have the opposite effect. Firms with greater exposure to external markets see increases in sales—specifically exports—in response to increases in effective trade policy uncertainty. A series

[5]The types include: generic, trade, fiscal, monetary, a measure for the resolution of uncertainty, and two additional trade policy uncertainty measures.

[6]"Accuracy" is defined as the percent of true positives of the total number of audited algorithm-selected articles. The comparison is based on the results from human audits associated with both measures conducted by my research assistants, who were trained using Baker et al.'s (2016) training materials.

[7]The exception is trade policy uncertainty, which varies greatly across time but far less so across countries. Given that three of the four countries are in a common market together (and the fourth has a free trade agreement and was attempting accession over the sample period), this result is in line with expectations.

[8]Investments are not limited to the choice to participate in a given market or launch an additional product; they can include the choice to expand facilities, scale up employment, conduct research & development, and adjust production lines to meet changing consumer preferences.

of checks confirm that this result is robust across sectors and across firm characteristics (e.g., tenure and size). One might argue that this result could reflect the relative importance of domestic policy uncertainty (and non-importance of external policy uncertainty) for exporters. In this case, the ratio of external to domestic policy uncertainty would not be the right measure to examine exporters' sensitivity to trade policy uncertainty; one would want to look solely at responsiveness to domestic trade policy uncertainty. However, disentangling external from domestic trade policy uncertainty reveals that firms with greater export exposure "lean into the wind" when faced with increases in external trade policy uncertainty while there is no significant evidence of differential impacts in response to increases in domestic trade policy uncertainty.

At first glance, this result does not seem to fit with the predictions of real options theory; however, a closer look at the timing of trade policy uncertainty reveals that spikes often occur near expected conclusion dates for negotiations. This suggests that exposed firms may associate increased news chatter with the resolution of a trade agreement—associated with greater market access or a decline in trade costs—or as a signal of the protracted continuation of the status quo. In either case, decreases in the probability or potential magnitude of bad news should increase the incentive to immediately invest under the real options framework. I test this assertion using two new trade policy uncertainty measures—one that measures protectionist uncertainty and another that measures trade negotiation uncertainty. I find evidence that exposed firms respond negatively to uncertainty around protectionism and positively to trade negotiation uncertainty. These results indicate that exposed firms may interpret increased uncertainty around trade negotiations as a signal that negative outcomes are less likely or smaller in the near-term, incentivizing immediate investments.

The paper is structured into seven parts. The next section discusses the connections the research has with the policy uncertainty literature; Section 3 presents a stylized model of firm choice under uncertainty to motivate my empirical approach; Section 4 details construction and performance of the policy uncertainty measures; Section 5 presents the empirical results; Section 6 explores the result on trade policy uncertainty in more depth; and Section 7 concludes, suggesting directions for future research and relating the findings back to the aggregate "puzzles" mentioned above.

1.2 Connections and Contributions to the Literature

The study of the relationship between policy uncertainty and negative economic outcomes rests in the real options literature, which also informs a broader economic uncertainty literature. Under a highly uncertain policy environment and in the presence of fixed, irreversible costs, the value of delay rises, hindering firm performance (e.g., Bernanke, 1983; Baldwin & Krugman, 1989; Rodrik, 1991; and Dixit & Pindyck, 1994). Bloom (2009) revived the uncertainty literature by constructing a quantitative real business cycle model of firms facing uncertainty and non-convex labor and capital adjustment

costs. The model features a zone of inaction for investment and hiring, which increases in size as uncertainty increases. This results in pro-cyclical growth in productivity, a stylized fact of business cycles. Several studies relate firm uncertainty, often proxied by stock price volatility (e.g., Leahy & Whited, 1996 and Bloom, Bond, and Van Reenen, 2007) or business survey results (e.g., Guiso & Parigi, 1997 and Bachmann, Elstner, & Sims, 2013) to negative outcomes in investment and production.[9]

Within the literature directed particularly at *policy* uncertainty, the research relates to four broad areas: the measurement of policy uncertainty; the impact of policy uncertainty using firm-level variation; analyses of type-specific policy uncertainty; and the cross-country effects of economic policy uncertainty.

Examples of modern measures of policy uncertainty include the use of political proxies. Both Durnev (2010) and Julio & Yook (2012) use election year dummies. While election years can be associated with increases in policy uncertainty, this relationship is not deterministic. For instance, if a strong incumbent has a clear and stable lead throughout an election season there is no reason to think that economic agents would anticipate a change in policy regime. On a related point, if one party (or one coalition) has long held power, election seasons would be less reflective of policy uncertainty and more reflective of within party/coalition power dynamics. Finally, focusing on election years misses important variation in non-election year policy uncertainty—especially in response to shocks that do not have a domestic origin.[10] Baker et al. (2016) greatly improved on this literature by employing "news chatter" measures of policy uncertainty.[11] The approach allows for far more nuance in approximating time-varying policy uncertainty. Moreover, as mentioned above, "news chatter" picks up policy uncertainty that is most salient to economic agents. Others have followed suit in creating news-based measures (e.g., Shoag & Veuger (2013) at the U.S. state level and Redl (2015) for the South African case).

While Baker et al. (2016) take great care in constructing their measures, a human audit reveals that there is still considerable noise in their baseline algorithm.[12] One shortcoming of their approach is related to the limited number of newspaper sources they run their search algorithm on—two sources in most instances. To the extent that the selected newspapers have particular political slants, the constructed measures will be politically biased. A more problematic shortcoming is the non-restrictive nature of the search algorithms. The "triple" of mentions related to uncertainty, the economy, and policy can appear anywhere in an article, generating many false positives. Methodologically, my

[9] Additional connections with the empirical literature on economic uncertainty: Romer, 1990; Ramey & Ramey, 1995; and Alexopolous & Cohen, 2009.

[10] Shelton & Falk (2016) address many of these concerns by using term-limits as instruments for electorally-related policy uncertainty in the context of U.S. gubernatorial elections.

[11] In earlier versions of the paper, they paired their news index with in concert with the present value federal tax codes set to expire and deviations in professional forecasting. However, to extend the measure to multiple countries and across time, they restrict their analysis to the "news chatter" approach.

[12] By choosing a policy word set that minimizes false negatives and positives, they likely reduce much of this noise when it comes to their chosen policy uncertainty measure. However, without an additional audit on the final algorithm, it is unclear how much noise is reduced.

construction of policy uncertainty most closely connects with Gunnemann (2014). We both employ more restrictive algorithms, with far more sources (>36,000 in my case), and across a number of countries. My work differs from his in that I rely on journalistic standards and input to select the phrasing and number of words that can separate the "key word" mentions. I also exclude mentions of decreases in policy uncertainty, words related to equity markets, and impose a minimum word count. The latter two are to reduce the counts of "news ticker" articles that relate to summaries of a day's events and typical equity market fluctuations. Finally, I also create type-specific policy uncertainty measurements.[13]

Firm-level analyses of policy uncertainty offer better causal identification relative to aggregate studies, but were rare until recently. The papers that most closely connect with my approach are Gulen & Ion (2016) and Baker et al. (2016). Both papers use variation in exposure to domestic government purchases as a proxy for firms' vulnerability to domestic policy uncertainty. Baker et al. (2016) find that firms in sectors with more exposure to government purchases see larger declines in investment and employment growth and larger increases in stock price volatility in response to increases in domestic policy uncertainty. They also find evidence that sector-specific uncertainty measures—healthcare, national security, and defense—outperform the generic policy uncertainty measure in predicting the outcomes of firms in those industries.

Gulen & Ion (2016) also examine cross-sectional variation in firms' need to make investments that are irreversible. In line with theoretical predictions, policy uncertainty has larger impacts on firms that face higher investment irreversibility. Julio & Yook's (2012) use their election year proxy in a firm analysis, finding that increases in policy uncertainty are associated with declines in firm investment, but, again, this approach misses non-election variation in policy uncertainty. Stein & Stone (2012), who examine economic uncertainty more broadly, also leverage cross-firm variation in sensitivity—drawing identification from industries' relative sensitivity to changes in energy and exchange rates. They use this variation to identify firm-specific uncertainty, which they find is associated with drops in capital investment.

Historically, explicit research on policy uncertainty was infrequent and often directed at particular types of policy (Friedman, 1968; Higgs, 1997; Hasset & Metcalf, 1999; Gorodnichenko & Shapiro, 2007).[14] As policy uncertainty spiked in the wake of the Great Recession, there has been renewed interest in understanding its impacts. Analysis has largely focused on generic policy uncertainty (e.g., Stock & Watson, 2012, and Bordo, Duca, & Koch, 2016). However, there is also a nascent literature that examines specific types of policy uncertainty. Handley and Limao (2013) consider the specific role of trade policy uncertainty in trade outcomes and build a general equilibrium model allowing for export entry and upgrading with impact on importer price indices. They show that a re-

[13] More detail on the construction of these measures in Section 4.

[14] Unlike other papers in this area, Gorodnichenko & Shapiro (2007) examine the beneficial implications of monetary policy certainty. See Gunnemann (2014) or Bloom (2014) for more discussion of this earlier body of work.

duction in trade policy uncertainty following China's World Trade Organization (WTO) accession generated larger export growth in industries that faced higher potential profit losses should most-favored nation status have been lost. Their work is empirically supported by Pierce and Schott (2016). Handley & Limao (2015) build a dynamic trade model with heterogenous firms, uncertainty, and fixed costs where firms make entry and trade decisions. The model predicts that firms will limit investment and entry into new export markets under conditions of high trade policy uncertainty. They test this using Portugal's accession to the European Community in the late 1980s, finding that the reduction of trade policy uncertainty led to increases in exports.[15] My research also examines the impact of trade policy uncertainty on trade outcomes, but differs in that I focus my analysis at the firm-level rather than the sector-level and I allow exporting firms to face both domestic and foreign trade-related policy uncertainty.

Finally, my analysis relates to research on the cross-country interactions between policy uncertainty and outcomes. Sum (2013) examines the relationship between U.S. and European economic policy uncertainty, finding strong evidence of co-integration. Colombo (2013) employs a structural VAR approach to show that U.S. economic policy uncertainty reduces European industrial production more than European policy uncertainty reduces it. Arguably, a cross-country channel could operate through either trade or financial linkages. However, recent fallout from the U.K.'s European Union (EU) referendum points to the importance of the trade channel. Eichengreen, Gupta, & Ospino (2016) find early evidence that the U.K.'s vote to leave the EU affected emerging markets through a trade channel, where countries with higher export shares to the EU—not merely the U.K.—experienced more negative effects. The work of Wolfers & Zitzewitz (2016) supports this finding; they show that countries with U.S. free trade agreements see large exchange rate movements in response to news about the 2016 presidential election. The inclusion of external policy uncertainty in this research is motivated by this body of work and I build on this literature by conducting a firm-level analysis of how differences in exposure to exporting translate into differences in the impact of policy uncertainty across firms.

1.3 Motivating Theory

In an environment with rising policy uncertainty and sunk costs, there can be benefits to delaying costly decisions under real options theory. To illustrate this in a straightforward manner and motivate my empirical approach, I present a firm choice model with uncertainty over payoffs from a firm's action—for example, this "action" could be building a factory, expanding an existing facility, making a capital investment, or hiring employees.[16]

There are two goals of the model: (1) to provide intuition for why firms might delay

[15] While this paper uses firm-level data, it is not a panel. Therefore extensive margin effects are examined at the sector-level.

[16] This model is an extension of Dixit & Pindyck's (1994) "investment under uncertainty" model.

costly choices in the face of uncertainty and (2) to expose the asymmetry between good and bad news in driving such choices.

1.3.1 A Two-Period Firm Choice Model

A price-taking manager is deciding whether or not to take action on a project that will produce one additional unit of a good each period, with zero per-period operating costs. Let $F > 0$ be a sunk cost of such an action and $r > 0$ be the interest rate.

The additional value received from action in period $t = 0$ is P_0. From period $t = 1$ it will be:

$$P_1 = \begin{cases} (1+\mu)\, P_0 \textit{ with probability } A \\ (1-\delta)\, P_0 \textit{ with probability } B \\ P_0 \textit{ with probability } C = 1 - A - B, \end{cases}$$

where $A,\ B,\ C,\ \delta,\ \mu \in (0,1)$ and $A + B + C = 1$.

As a preliminary step, assume that the opportunity to act can only occur in period 0. Let V_0 be the expected present value from taking action. Then the net payoff of acting in $t = 0$, Ω_0, solves $max\left[V_0 - F, 0\right]$. Now allow for action to remain an option in the second period. Then in period $t = 1$, for each of the potential P_1 outcomes, the firm would invest if $V_1 > F$, with a net payoff of $\Gamma_1 = max\left[V_1 - F, 0\right]$. At period $t = 0$, P_1 is not known, making V_1 and Γ_1 random variables. Let E_0 be the expectation at $t = 0$. Then $E_0\left[\Gamma_1\right]$ is the continuation value associated with waiting until period $t = 1$ to act. Returning to the $t = 0$ decision, the firm can take action immediately and get $V_0 - F$. If it decides to postpone, it gets one-period discounted $E_0\left[\Gamma_1\right]$. So the net payoff of the action becomes $\mathit{\Gamma}_0 = max\left\{V_0 - F, \frac{1}{1+r} E_0\left[\Gamma_1\right]\right\}$. For full details, see Appendix A.1.1.

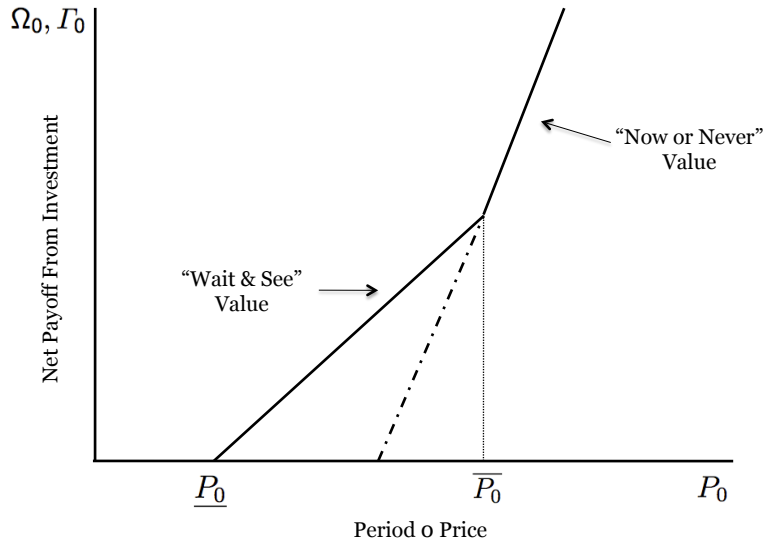
The difference between the two cases—the now-or-never option or the option to act in the second period ($\mathit{\Gamma}_0 - \Omega_0$) is the value of the option to postpone action. The ability to wait allows for the ability to base firm action on different contingencies, offering extra value from "waiting and seeing." To illustrate the trade-off, see Figure 1.2.

For some initial $P_0 < \underline{P_0}$, firms will never act (i.e., both V_0 and V_1 are less than F). Intuitively, $\underline{P_0}$ is lower for the "wait and see" value since you are able to reject action should there be a price decrease in $t = 1$ (i.e., with the option to delay you are less vulnerable).[17] The slope for the "wait and see" option is lower since, in delaying, you forego $t = 0$ P_0.[18] Let $\overline{P_0}$ be the critical price such that for $P_0 > \overline{P_0}$ firms are better off acting immediately rather than waiting (i.e., the cost of waiting exceeds the gains from waiting). For $P_0 > \overline{P_0}$, the cost of waiting outweighs the gain from waiting; the net present value is always positive. For $P_0 \in \left(\underline{P_0}, \overline{P_0}\right)$, it is worthwhile to act only if the price increases or stays the same in period $t = 1$. Within this price range, the net present value of the project becomes negative should a price decrease occur.

[17] To see this formally, see Appendix A.1.2. For the "wait and see" option, $\underline{P_0} = \frac{r(CF+AF)}{C(1+r)+A(1+\mu)(1+r)}$.

[18] To see this formally, see Appendix A.1.2.

Figure 1.2: Net Payoff from Firm Action under Uncertainty



Notes: The figure shows a graphical illustration of the model of firm choice under uncertainty.

By comparing the net present value to acting now versus waiting one period to act, one can solve for $\overline{P_0}$ (see Appendix A.1.3):

$$\overline{P_0} = \left(\frac{r}{1+r}\right) F \frac{(r+B)}{r+B(1-\delta)}$$

In Equation (2), increases in the magnitude of potential bad news, δ, or the probability of bad news, B, increase $\overline{P_0}$, increasing the incentive to delay firm actions (see Appendix A.1.3). In the context of this work, an increase in the probability of a downward price movement is associated with an increase in policy uncertainty.[19]

This core implication—that firms "wait and see" as a way to avoid later regretting a choice—is an insight built into other models of policy uncertainty (e.g., Bernanke, 1983; Bloom, Bond, Van Reenen, 2007; Bloom, 2009; Bloom, Floetotto, Jaimovich, Saporta-Eksten, & Terry, 2012; and Handley & Limao, 2015). Equation (2) also illuminates an important asymmetry—while bad news increases the likelihood of delaying action, $\overline{P_0}$ does not depend directly on μ or A, the magnitude of good news *or* the probability of good news.

[19] $\overline{P_0}$ is also increasing in F, so larger sunk costs will be associated with higher likelihood of adopting "wait and see" behavior; however, I do not find evidence in the literature that sunk costs associated with specific investment actions are time-varying in real terms.

1.3.2 From the Model to the Empirics

To examine whether the theory is borne out by the data, I use annual firm-level data across four European countries—the United Kingdom, France, Greece, and Turkey.[20] The theory suggests that both the magnitude of potential bad news, δ, and the probability of bad news, B, increase the likelihood that firms will delay action. However, both parameters also impact the first moment of policy, $E_0(P_1)$, which will have an impact on firm outcomes (e.g., sales).[21] In order to get at the isolated impact of δ and B on outcomes through their impact on changes in $\overline{P_0}$, the first moment of policy must be controlled for in the empirical specifications. The constructed measures of policy uncertainty proxy the both the magnitude of potentially bad news and the probability of bad news—the more news chatter about potentially negative news there is, the worse the impact of that potentially negative news and the more likely that outcome is. In the context of this research, an increase in the probability of bad outcomes is also dependent on a firm's relative exposure to different markets (domestic vs. external). Thus, increases in external policy uncertainty *relative to* domestic policy uncertainty signal an increased likelihood of bad outcomes, B, for firms that are more exposed to external markets, causing such firms to delay investments.

The model assumes that the probabilities and magnitudes of outcomes are known by the manager when she makes choices.[22] This mandates that timing be an important consideration in taking the model to the data. I will assume that managers have an information set at the beginning of t that includes its lagged firm-time characteristics (e.g., its amount of fixed assets and prior success) and knowledge of lagged policy uncertainty when choices must be made. The manager uses $t-1$ policy uncertainty to make assumptions about the probability of bad news, B, occurring in t in deciding to delay or undertake costly actions in t.[23]

Finally, I choose to focus on a number of intensive margin outcomes in my empirical

[20] The countries were chosen because their firm-level data include reporting of both domestic and export sales.

[21] $E_0(P_1) = A(1+\mu)P_0 + B(1-\delta)P_0 + CP_0$. An increase in either δ or B decrease the first moment. The latter effect is because $A+B+C=1$; an increase in B is offset by either a decrease in A and/or C. In all cases, $E_0(P_1)$ falls when B increases.

[22] Policy uncertainty is often distinguished from more traditional notions of risk. This research conceives of policy uncertainty as closer to Knightian uncertainty—"a fundamental lack of knowledge about the future." As Bernanke (1983) points out, Knightian uncertainty is reducible if one simply waits. This differs from parametric uncertainty, which is irreducible and typically associated with risk around expected returns. In the model presented, the probabilities and magnitudes are assumed to be known. However, one can relax the assumption that B is perfectly calibrated. Even if B is "fuzzy," in what follows, I assume that firms know whether it is generally increasing or decreasing based on the news they read. Examples of fuzzy, but useful information around probabilities of outcomes are election polls. While they are often noisy, with time-varying error bands, they still prove useful in gauging the probability of an outcome.

[23] An alternative view is that investments that feature sunk costs also feature "time to build" so that policy uncertainty impacts investment choices concurrently, but they only show up in outcomes with a lag. In Section 5.5 I test the robustness of this assumption using alternative timing specifications.

analysis (e.g., sales, employment, and investment) since firm actions are not limited to entry choices or purely capital investments. Any actions that feature sunk costs will be delayed in context of the model—the choice to expand facilities, scale up employment, reach new market segments, conduct market research, or adjust production lines to meet changing consumer preferences.[24] Firms are also linked to one another via supply chains. Firms that provide inputs to other firms' activities are vulnerable to downstream firms' "waiting-and-seeing." Lastly, for the dataset I employ, I do not observe export destinations.[25] It may be that destination market choices are being made (i.e., the decision to stop serving a particular market), but they only show up on the intensive margin. Indeed, most firm actions will impact firm outcomes along the intensive margin, but will not show up as a binary choice (e.g., the choice to export or sell domestically).

1.4 Data

1.4.1 Measuring Policy Uncertainty

Policy uncertainty captures more than uncertainty about the specifics of what economic policies will be adopted. The concept also reflects uncertainty about the economic impacts of policy actions (e.g., uncertainty about the impact of an agreed upon trade deal); uncertainty over who will make policy choices; uncertainty created by policy inaction (e.g., the choice to delay decisions on the federal budget); and uncertainty about policy responses to non-economic shocks (e.g., a natural disaster). Unlike traditionally-used measures of economic uncertainty (e.g., strike days or exchange rate volatility), "news chatter" policy uncertainty indices pick up economic volatility as well as the threat of volatility related to policy uncertainty, whether or not it comes to fruition. Business confidence surveys are similar, however they tend to be ambiguous with respect to the underlying source of uncertainty (e.g., a common question is "are you worried about the direction of the economy over the medium-term?").

To construct policy uncertainty indices for fiscal, monetary, and trade policy uncertainty, as well as a more general "generic" policy uncertainty index, I designed and ran search algorithms on Dow Jones's Factiva news aggregator. Factiva covers over 36,000 sources in 28 languages. These sources include almost 700 newswires (e.g., the Associated Press and Reuters) and all major newspapers. In addition to digitized newspaper inclu-

[24] Any non-convex costs—sunk or partially irreversible—will generate an incentive to delay investment in the face of high policy uncertainty (Bloom, 2014). Cooper and Haltiwanger (2006) show that investment adjustment costs, even when small, matter for firm investment choices. Bloom (2009) estimates a number of labor and capital adjustment costs: in his preferred specification, capital has an estimated resale loss of 34 percent, while fixed investment, partially irreversible hiring and firing costs, and fixed costs of hiring and firing are all estimated to be roughly 2 percent. As he notes, there is a large degree of variation (depending on approach and/or data) in the literature in the estimation of such costs. For instance, Nickell (1986) finds labor adjustment costs in the range of 8 to 25 percent of annual wages.

[25] More detail on how I link firms' exports to destinations can be found in Section 4.1.2.

sion dating back to the 1980s, newspapers' online websites (e.g., The Guardian Online, The New York Times Online) are also included. The news aggregator allows for filtering of results by language, source location, geographic coverage, company/industry, and a select group of subjects.

1.4.1.1 Developing Type-Specific Policy Uncertainty Search Algorithms

The search algorithm counts articles that meet a quadruple metric: mention of a policy type within the same paragraph as uncertainty, which must be within eight words of a country reference, and contain no mention of excluded terms.[26] To develop the algorithms, I first compiled word banks related to policy types—trade, monetary, and fiscal—using Baker et al. (2016) as a base, reading articles on economic policy via Google News, and collecting word suggestions from graduate students with expertise in each area.[27] Next, I consulted with two journalists that write and report on economics-related news to ensure that the search algorithms pick up policy uncertainty in a way that journalists write about policy uncertainty, as opposed to the way economists might write about policy uncertainty.[28] Particular attention was placed on generating the set of uncertainty-related terms that are used in association with negative increases in policy uncertainty (e.g., concern, doubt, worry, anxiety, etc.). Finally, to ascertain journalistic standards around sentence length and construction, I spoke with two professors of journalism to understand how "leads" are covered and to select the number of words that might fall between core search terms.[29] Factiva also has some foreign language sources. To include these sources in my searches, I relied on translations by native-speaking economic graduate students, who also offered adjustments based on language and country context for certain terms.[30]

The algorithms are both more flexible and more restrictive than those used in the prior literature. One one hand, I allow many terms to vary in how they are presented (e.g., import tariffs can also by picked up as tariffs on imports). On the other hand, to ensure that type-specific uncertainty measures are not merely proxies for other types of policy uncertainty, the search algorithms reject articles that mention other types of uncertainty (e.g, fiscal policy uncertainty's algorithm picks up articles that meet the fiscal search terms, but excludes articles that meet trade or monetary policy search terms). In an additional effort to ensure that the algorithms pick up news related to the country in question, I applied Factiva's country-specific filters. Articles are also required to be longer than 99 words so that tickers and news summaries are less likely to be included in the

[26]See Appendix A.2 for more detailed examples of the search algorithms.

[27]For terms related to trade policy, common words from WTO trade arbitrations for the four countries were also included in the word bank.

[28]I thank Sam Fleming—US Economics Editor at the Financial Times—and Juliana Goldman—CBS News Correspondent, formerly at Bloomberg News—for their time and contributions.

[29]I thank Douglas Foster—Northwestern University, Medill School of Journalism—and Kenichi Serino—University of Witswatersrand School of Journalism—for their time and contributions.

[30]I thank Yusuf Mercan (Turkish), Caroline Le Pennec (French), and Eric Avis (French) for their time and contributions. Factiva does not allow for searches in Greek.

count. Finally, the algorithms also exclude references to an absence of uncertainty (e.g., "without doubt" or "no uncertainty") and references to equity market volatility, which occurs rather frequently and is often unrelated to policy uncertainty.

1.4.1.2 Constructing Policy Uncertainty Indices

Country-Level Domestic Policy Uncertainty By type-year, the policy uncertainty measure is the count of the number of articles that match the criteria of the search algorithm divided by the count of articles that match a normalization search algorithm. The normalization algorithm is meant to pick up changes in source coverage over time and counts articles that include the term "today" within eight words of the country name. When policy uncertainty spikes, general coverage of the country spikes (e.g., the attempted Turkish coup of 2016), diluting the measure of policy uncertainty in periods of high policy uncertainty. In order to alleviate this, I define the normalizer as the average of general mentions in the current year and one year ahead. In general, source coverage is increasing over time, so this is a conservative approach for the normalizer (relative to an average that includes any prior data). Finally, each country's uncertainty measures are standardized to have unit standard deviations and normalized to have a mean of 100, with larger values reflecting higher policy uncertainty. Figure 1.3 shows the domestic policy uncertainty measures for the U.K., Greece, France, and Turkey.[31]

Firm-Specific Policy Uncertainty Firms that participate in exporting are also vulnerable to external policy uncertainty within their exporting markets. To account for this:

1. I identify the top export destinations for both goods and services at one-digit SITC and EBOPS levels over the full period from 2003 to 2015 for each of the four countries.[32]

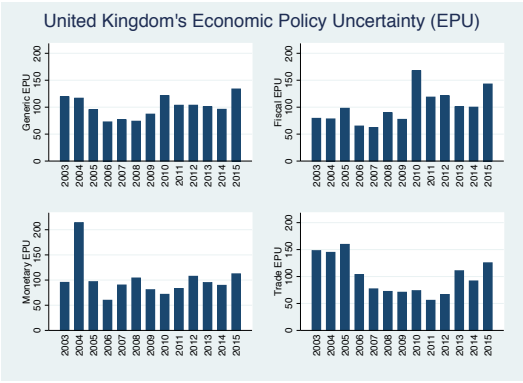
2. For 44 unique top-export countries, I constructed type-specific policy uncertainty measures in the same manner as the domestic policy uncertainty measures—176 additional indices.[33]

[31]For detail on the events that lead to spikes in policy uncertainty, see the online appendix at www.sandile.com.

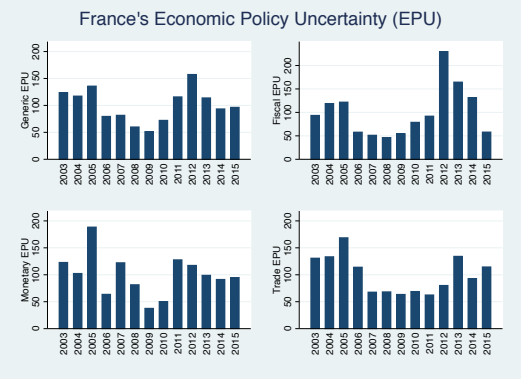
[32]Comtrade's service data only extends to 2014 and does not include services export data by destination for Turkey.

[33]Only English language search algorithms are used for the construction of the external uncertainty indices. The countries are: Albania; Australia; Azerbaijan; Belgium; Brazil; Bulgaria; Canada; China; Cyprus; Egypt; Finland; France; Germany; Gibraltar; Greece; Hungary; India; Iran; Iraq; Ireland; Israel; Italy; Japan; Luxembourg; Macedonia; Malta; Morocco; Netherlands; Nigeria; Norway; Oman; Poland; Romania; Russia; Saudi Arabia; Singapore; South Africa; Spain; Switzerland; Syria; Turkey; U.K.; U.S.; and the United Arab Emirates.

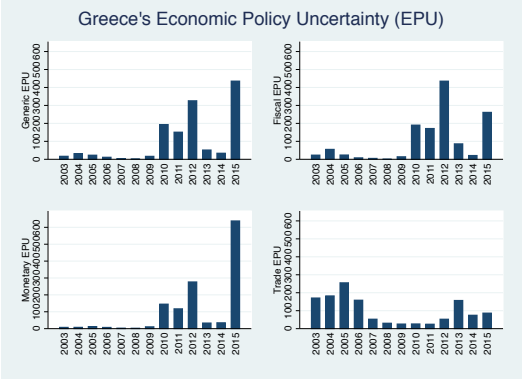
Figure 1.3: Domestic Policy Uncertainty Indices, 2003-2015



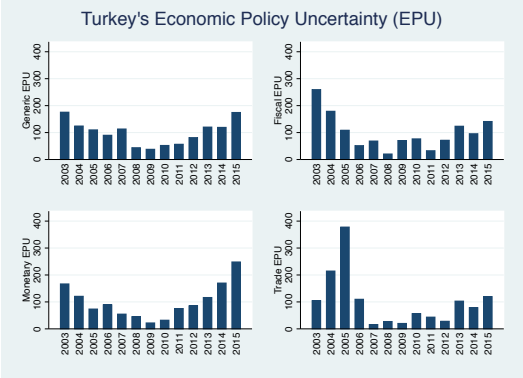
(a) U.K.



(b) France



(c) Greece



(d) Turkey

Notes: The figures show domestic policy uncertainty, by country. Each country's uncertainty measures are standardized to have unit standard deviations and normalized to have a mean of 100, with larger values reflecting higher policy uncertainty.

3. I then calculate the mean country-sector export shares θ_{mcs} at the 4-digit NAICS level for goods and 3-digit NAICS level for services over the sample period for each of the 44 markets, m.[34] Despite only constructing policy uncertainty measures for the 44 top-export countries, across sectors the average coverage for services is 91 percent to service exports, with standard deviation of .05. For goods, average coverage of total exports is 84 percent, with a standard deviation of .08. For each sector, period, and uncertainty-type, the measures are constructed as follows:

   $External\ Policy\ Uncertainty_{cst} = \sum_{m=44} \theta_{mcs} Uncertainty\ Type_{mt}$.

4. Finally, at the firm-level, initial export share is used to create a weighted average of domestic and external policy uncertainty for a firm in a country and sector at time t:

$$Firm\ Policy\ Uncertainty_{icst} = \alpha_i External\ Policy\ Uncertainty_{cst} + (1-\alpha_i) Domestic\ Policy\ Uncertainty_{ct}$$

   where α_i is a firm's export share in its initial period in the sample. For instance, if a firm exports 60 percent of sales in its initial period of entry into the sample, it faces 60 percent external uncertainty and 40 percent domestic policy uncertainty; if it exports zero percent of sales, it only faces domestic policy uncertainty.[35]

1.4.2 Performance of the Search Algorithms: Human Audit

To assess the performance and content of the policy uncertainty measures, I conducted a partial human audit of the measures using research assistants. To train the research assistants, I relied on Baker et al.'s (2016) extensive "coding guide" manual on how to conduct human audits of news chatter measures. I also offered supplemental training on policy implementation (e.g., how modern central banks conduct monetary policy).[36] A random sample of 10 percent of articles for fiscal, monetary, and trade policy uncertainty and two percent of the generic policy uncertainty articles were audited.[37] The audit was conducted on the search algorithms for the United Kingdom. To ensure accuracy in the auditing process, the research assistants had a portion of overlapping audit assignments;

[34] For insight on why mean shares are preferable to median or initial sample period exports, see Appendix A.4.

[35] 12 percent of firms in the sample export. A histogram of α_i conditional on exporting can be found in Appendix A.6.

[36] The 66-page training manual can be found here: http://www.policyuncertainty.com/research.html. The audit template and raw results are available from the author upon request.

[37] Two percent amounts to 500 articles. As I refined the algorithms, additional rounds of partial audits were conducted. Altogether, we audited almost 3,000 articles across the four policy uncertainty types and 1,300 in the final round of audits.

conducted re-audits of each other's results; and any non-agreements were discussed and addressed.

1.4.2.1 Accuracy, by Type

Several metrics are employed to assess accuracy: the percent of articles that reference the correct type of policy uncertainty; whether correct reference to a type of policy uncertainty varies over time in a way that systemically biases the measures; whether articles are about increases or decreases in policy uncertainty; if policy uncertainty is related to domestic issues or foreign issues; and how much overlap there is between the various measures of type-specific policy uncertainty.

Amongst the generic policy uncertainty articles, 73.3 percent of articles referenced generic economic policy uncertainty and 3.9 percent of economic policy uncertainty articles were mainly about foreign countries (see Table 1.1).[38]I can compare these results to audit results on Baker et al.'s (2016) U.K. generic economic policy uncertainty algorithm: 54.2 percent of audited articles referenced economic policy uncertainty and 51.8 percent of economic policy uncertainty articles were mainly about foreign countries.[39]

Amongst the type-specific policy uncertainty audits, over 70 percent of the audited articles reference the correct type-specific policy uncertainty and over 87 percent of audited articles relate to domestic policy uncertainty; and there is little overlap across article types (see Table 2).

Table 1.1: Generic Policy Uncertainty Accuracy Comparison

	BBD (2016)	**Hlatshwayo (2016)**
% Economic Policy Uncertainty (EPU)?	54.2	73.3
% of EPU Mainly About a Foreign Country?	51.8	3.9

Notes: The table compares results for Baker et al.'s (2016) preferred algorithm for the United Kingdom and mine. For the results associated with this paper's algorithm, I excluded my audits for the calculations; however, the difference when including my audit results is minimal.

One might worry that the accuracy of the measures vary over time in a way that could bias their interpretation (namely, if periods of high policy uncertainty were associated with low accuracy then changes in the measure would reflect changes in its noise). Figure 1.4 displays the percent of audited articles that relate to type-specific policy uncertainty

[38]I excluded counts of audits I conducted in these calculations. Also, to be clear, this gives the true positive rate, but does not address true negatives that may be excluded based on my algorithm. Based on the sample size, the confidence interval percentages are +- 4.7.

[39]For this audit, I used their algorithm and selected U.K. sources (the Times of London and the Financial Times). The term sets, as laid out in their paper, are (E) economic OR economy; (P) spending OR policy OR deficit OR budget OR tax OR regulation OR "Bank of England"; and (U) uncertain OR uncertainty. Based on the current sample size, the confidence interval for the Baker, et al. percentages are +- 6.8.

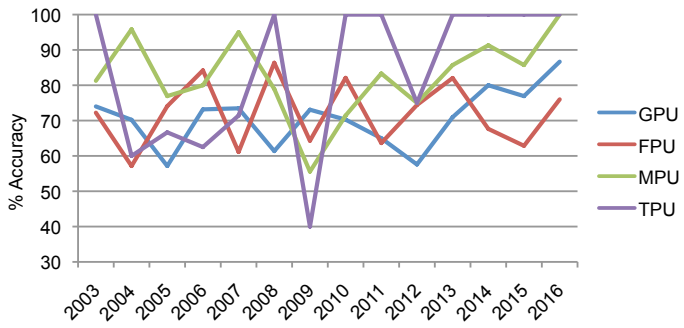
Table 1.2: Type-Specific Accuracy (Percent)

Type Match?	**Fiscal**	**Monetary**	**Trade**
% Policy Type	72.6	78.5	89.0
Of Policy Match Articles			
% Domestic Focus?	96.2	91.4	87.6
% Overlap with Other Types?	1.7	0.4	0.0

Notes: The table shows several accuracy metrics for the type-specific search results used in constructing type-specific policy uncertainty indices.

over the sample period. While there is fluctuation in accuracy for the measures, accuracy is generally high across the measures. For fiscal policy uncertainty, the lowest accuracy (57 percent) occurs in 2004, a low uncertainty year for fiscal policy; in 2010—a high fiscal policy uncertainty period—accuracy is at 82 percent.

Figure 1.4: Accuracy of Policy Uncertainty Algorithms, 2003-2016



Notes: This figure displays the accuracy of the policy uncertainty algorithms over time, where accuracy is the percent of audited articles that are the correct policy uncertainty type divided by the total articles audited for each year for each type-specific algorithm. GPU is generic policy uncertainty; FPU is fiscal policy uncertainty; MPU is monetary policy uncertainty; and TPU is trade policy uncertainty.

The lowest period of accuracy for monetary policy uncertainty falls in 2015 (64 percent), but on the whole, accuracy is high throughout the sample period. Finally, 2009 is a low accuracy year for trade policy uncertainty, however it is also a low year with respect to the trade policy uncertainty indices.

1.4.2.2 How similar are the measures to measures of economic uncertainty?

Some argue that measures for economic policy uncertainty are simply proxies for economic uncertainty. Table 1.3 displays the average coefficients of non-determination $(1 - R^2)$ across the four sample countries for country-specific regressions of the policy

uncertainty measures on traditional measures of economic uncertainty—CBOE Volatility Index (VIX); stock market volatility (SMV); the interquartile of average probability distributions for EU real GDP growth from professional forecasters (ECB I); and the standard deviations of professional forecasts for real GDP growth (ECB II).

Table 1.3: Mean Coefficients of Non-Determination

	GPU	FPU	TPU	MPU	VIX	SMV	ECB I	ECB II
GPU	0.00							
FPU	0.37	0.00						
TPU	0.84	0.91	0.00					
MPU	0.41	0.73	0.86	0.00				
VIX	0.85	0.93	0.67	0.89	0.00			
SMV	0.79	0.86	0.75	0.81	0.45	0.00		
ECB I	0.88	0.77	0.91	0.98	0.99	0.91	0.00	
ECB II	0.76	0.71	0.63	0.76	0.92	0.82	0.68	0.00

Notes: The table shows the coefficient of non-determination (that is, the variation in policy uncertainty not explained by various economic uncertainty measures) for the types of policy uncertainty. The measures of economic uncertainty include the the CBOE Volatility Index (VIX); stock market volatility (SMV); the interquartile of average probability distributions for EU real GDP growth from professional forecasters (ECB I); and the standard deviations of professional forecasts for real GDP growth (ECB II).

The coefficients of non-determination reveal a large degree of variation in the policy uncertainty measures that is not explained by economic uncertainty measures. For the country-specific coefficients of non-determination, see Appendix A.3.

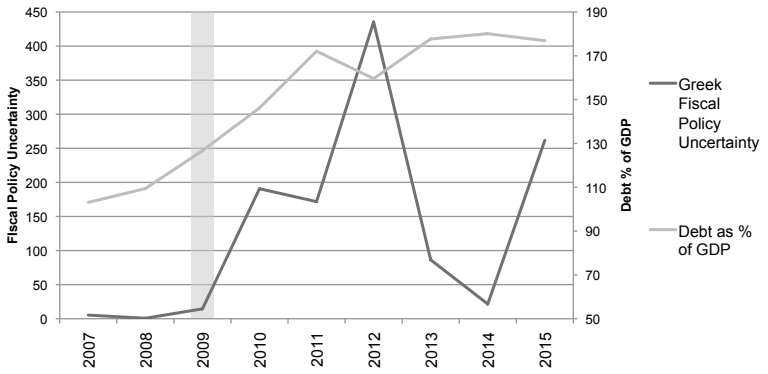
1.4.2.3 On the temporal nature of policy uncertainty

Real options theory relies on the assumption that certainty can be attained by waiting. This suggests that policy uncertainty must be temporary in nature in order for firms to have incentive to respond to it (Bernanke, 1983). A benefit of "news chatter" measures is that newspapers tend to cover "new" news, suggesting that the indices are more likely to capture temporary uncertainty (e.g., policy uncertainty associated with an upcoming election or a budget approval process) as opposed to long-standing, structural uncertainty. Moreover, examination of the above indices reveals spikes in the policy uncertainty measures followed by low policy uncertainty periods, pointing to resolution of prior uncertainty.

Greece's recent debt crisis offers a useful example. Figure 1.5 plots Greece's fiscal policy uncertainty index and debt as a percent of GDP. The debt crisis began in 2009, but policy uncertainty around leadership's response to it only amplified in 2010. Between 2010 and 2012, Greece cycled through multiple heads of state, the threat of referendums on bailout deals, and the rise of fringe anti-austerity political parties. However, in late 2012, ECB President, Mario Draghi, made his now infamous comment that the Bank would "do

whatever it takes to preserve the euro," followed with the introduction of bond-buying programs targeted at Greece and a less restrictive bailout deal.

Figure 1.5: Greece's Debt Crisis-Related Policy Uncertainty and Debt/GDP



Notes: This figure shows Greece's fiscal policy uncertainty index and debt as a percent of GDP (from Eurostat) from 2007 to 2015. The grey bar denotes the start of the Greek debt crisis. The left hand axis is for the policy uncertainty index and the right hand axis is for debt as a percent of GDP.

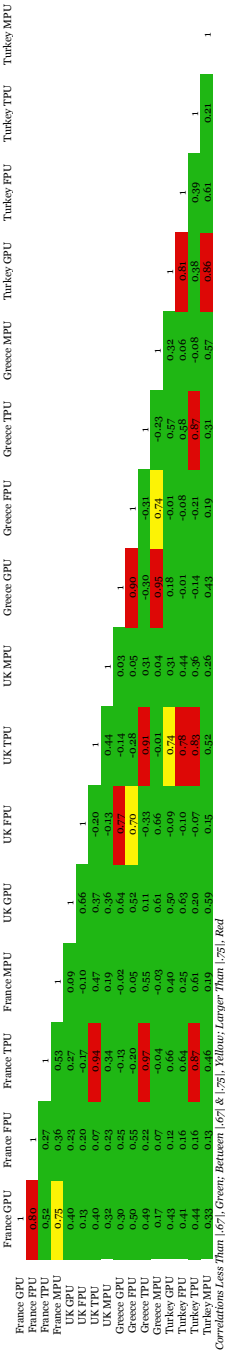
Despite still rising debt and an ongoing economic crisis, policy uncertainty surrounding the debt crisis was all but resolved during 2013 and 2014. This period of relative policy stability ended in 2015 when snap elections brought the anti-austerity Syriza party to power; government defaulted on a payment to the IMF; discussions were held about Greece's potential exit from the eurozone; and Greek citizens voted against new austerity measures in a referendum. This example not only illustrates that policy uncertainty can be resolved, but also that such resolution can take place in the face of ongoing economic uncertainty.

1.4.2.4 How much overlap is there across the measures?

Given that three of the four countries are in the European Union and the fourth is in the accession process, it was not clear ex-ante that fluctuations in policy uncertainty would vary dramatically across the same countries. However, the measures show substantial variation. The average cross-country correlation for generic policy uncertainty is .41. For the fiscal, monetary, and trade policy uncertainty, the average cross-country correlations are .24, .21, and .90, respectively. Figure 1.6 is a heat-map of the country-measure specific correlations. Red cells represent correlations higher than .75; yellow cells are correlations between .67 and .75; and green cells represent correlations lower than .67.

There is sizable variation both across time and countries across the measures, with the exception of trade policy uncertainty. This is not unexpected given that the European Commission of the EU is responsible for trade agreements for member countries and Turkey has a customs union agreement with the EU.

Figure 1.6: Correlations Across Policy Uncertainty Measures



Notes: This figure is a heat-map of the country-measure specific correlations. Red cells represent correlations higher than .75; yellow cells are correlations between .67 and .75; and green cells represent correlations lower than .67.

1.4.3 Firm-Level Data

The Bureau Van Dijk's Amadeus database includes firms across European countries covering the 13-year period from 2003 to 2015. While the dataset covers 43 economies, I utilize firm data from the U.K., Greece, Turkey, and France. Selection was based on the provision of export data by entities in these countries. The dataset is based on local records compiled from regulatory filings. The database contains the most recent ten years of data for each firm. In order to avoid reverse causality between firms' outcomes and policy uncertainty, I exclude the top percentile of firms, based on sales, from the sample. The total number of firms in the sample is 1.5 million with a total of 8.9 million observations. Annual exporting participation varies between 10-15 percent of firms over the sample period.

The firm-level time-varying variables include: sales; number of employees; staff costs (for the U.K. and France); materials costs (for the U.K. and France); total cost of goods (for Greece and Turkey); fixed assets; depreciation; and firm age. I also construct a measure that proxies within-market externalities arising from the presence of other firms within an industry in a given country and year. As discussed by Bernard & Jensen (2004), the presence of other firms may reduce production costs or costs of accessing new markets. All value-based measures are deflated using country consumer price indices.

Table 1.4 shows the descriptive statistics. In line with expectations, exporters tend to be larger, older, and more productive. However, non-exporters have more fixed assets in this database. With the exception of the investment specifications (where fixed assets is used to measure investment rates), controls for fixed assets and age are used throughout as firm-time controls.

Table 1.4: Sample Descriptive Statistics

	Exporters	**Non-Exporters**
Sales ($)	5,375,332 (15,705,167)	2,095,050 (10,352,291)
Employees	26 (93)	20 (113)
Fixed Assets ($)	1,972,723 (55,508,345)	3,362,264(134,562,785)
Age	16 (13)	12 (12)
Labor Productivity	480,944 (1,576,829)	303,243 (1,318,535)

Notes: The table contains means with standard deviations in parentheses. Sales and fixed assets are in real terms; number of employees is in units; and labor productivity is total sales divided by number of employees.

1.5 Empirical Analysis

1.5.1 Specification and Interpretation

A central benefit of constructing a firm-based policy uncertainty measure is that it allows for the control of country-sector-time effects, capturing a multitude of confounding variables—such as, country-sector business cycles (e.g., Afonso & Furceri, 2009) as well as relative prices, sector productivity trends, and various other shocks. Since the firm policy uncertainty measure accounts for exposure to external markets, a first moment control for policy must also account for external markets. To do so, I follow a similar approach to that used to construct external policy uncertainty but use external real $t-1$ GDP forecasts for t from the IMF's Spring World Economic Outlook historical databases: $External\ Forecasts_{cst-1} = \sum_{m=1}^{44} \theta_{mcs} Forecast_{mt-1}$, where θ_{mcs} are mean country-sector export shares, as discussed above. The first moment control is: $Firm\ Specific\ Forecasts_{icst-1} = \alpha_i External\ Forecasts_{cst-1} + (1-\alpha_i) Domestic\ Forecast_{ct-1}$. α_i is the initial export share of firm i. My baseline specification is:

$$Outcome_{icst} =$$
$$\gamma_i + \mu_{cst} + \beta \times Firm\ Policy\ Uncertainty\ Type_{icst-1} + \mathbf{Z}_{it-1} + \ Forecasts_{icst-1} + \varepsilon_{isct},$$

where γ_i are firm fixed effects; μ_{cst} are country-sector-time fixed effects; $\mathbf{Z}_{it-1}$ is a vector of lagged firm-time characteristics; and $Forecasts_{icst-1}$ is the firm-level first moment control for policy. The variables are log-transformed.

With the inclusion of country-sector-time fixed effects, μ_{cst}, β is a coefficient that measures sensitivity across firms with different levels of exposure. Re-arranging the firm policy uncertainty (PU) measure yields: $Firm\ PU_{icst} = \alpha_i\,(External\ PU_{cst} - Domestic\ PU_{ct}) + Domestic\ PU_{ct}$.[40] The final term is absorbed by the country-sector-time fixed effects. Thus, β estimates the sensitivity across firms to changes in relative policy uncertainty (i.e., the ratio of external to domestic policy uncertainty), where sensitivity varies based on initial export shares, α_i. I use the term "effective policy uncertainty" to refer to the interaction between α_i and the ratio of policy uncertainties.

Finally, as discussed in Section 4, the measures of monetary, fiscal, and trade policy uncertainty have minimal overlap with respect to content (i.e., based on the audits, fiscal, monetary, and trade articles have overlap with other types of 1.7, .4, and 0 percent, respectively). However, in reality, periods of high fiscal policy uncertainty tend to overlap with periods of high monetary policy uncertainty for firms—an additional finding of this research (see Table 1.5).

While horse-race styled specifications are instructive with respect to the relative importance of different policy uncertainty types, such specifications will also suffer from

[40]Recall, external policy uncertainty is the sector-specific weighted average of each country's (i.e., U.K., France, Greece, and Turkey) trading partners' uncertainty and domestic policy uncertainty is a country-time varying measure.

Table 1.5: Firm Uncertainty Correlations

	Firm GPU	Firm FPU	Firm MPU	Firm TPU
Firm GPU	1.00			
Firm FPU	0.89***	1.00		
Firm MPU	0.85***	0.64***	1.00	
Firm TPU	0.45***	0.40***	0.46***	1.00

Notes: The table shows the correlations between the firm-specific measures of policy uncertainty across types, with significance levels of * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

multicollinearity when including both fiscal and monetary policy uncertainty. Given the high degree of correlation between fiscal and monetary policy uncertainty, I create a composite "macro" policy uncertainty which is the simple mean of fiscal and monetary policy uncertainty.

1.5.2 Baseline Results

Firms' sales, profits, capital investment, and employment fall in response to increases in effective policy uncertainty (see Table 1.6). The impact of effective policy uncertainty varies widely across firms with different export exposure.

Table 1.6: Firm-Specific Baseline Results

	(1) Sales	(2) Profits	(3) Investment	(4) Employment	(5) Avg Wage
Firm GPU_{t-1}	-0.082***	-0.089***	-0.049**	-0.053***	-0.002
	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)
Observations	6280569	4375354	4661420	2309533	2162715
F	1148.302	556.209	44.731	768.479	151.908
R-squared	0.942	0.933	0.435	0.959	0.821
R-squared within	0.042	0.027	0.001	0.039	0.004
Firm FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm GDP Forecasts?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes

Notes: The sample period is 2003-2015. The firm-level dependent variable in: column (1) is the log of real sales; column (2) is the log of firm profits, which is sales minus costs; column (3) is the log of capital investment, which is the change in fixed assets adjusted for depreciation; column (4) is the log of employment; column (5) is the log of average wage, which is total staff costs divided by number of employees. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. In column (3), log of fixed assets is excluded since fixed assets are used in calculating capital investment. Standard errors are clustered at the country-time level between brackets, *$* p < 0.10$, **$ p < 0.05$, ***$ p < 0.01$.*

In response to a standard deviation percent increase in generic effective policy uncertainty, a firm with an initial export share at the 75th percentile sees sales decline by 1.4 percent while a firm with the median export share sees sales decline by .3 percent (see Table 1.7). The coefficients on capital investment and employment are quite similar. In response to a standard deviation increase in effective policy uncertainty, a firm at the 75th percentile of exposure sees capital investment and employment fall by .9 percent while a firm at the median sees capital investment and employment fall by .2 percent.

Table 1.7: Percent Response to a One Standard Deviation Percent Increase in Effective Policy Uncertainty

	Sales	Profits	Invest	Emp.
75th %tile of Exporters	-1.4	-1.5	-0.9	-0.9
50th %tile of Exporters	-0.28	-0.31	-0.2	-0.2
Full Sample Mean	-0.11	-0.12	-0.1	-0.1

Notes: Exposure Share at the 75th percentile (.37); 50th percentile (.08); and the Full Sample mean (.03).

1.5.3 Type-Specific Policy Uncertainty Results

Turning to the type-specific indices, effective fiscal and monetary policy underlie the negative relationship between exposure and sales outcomes, with significant coefficients of

-.06 (see columns (2) - (3) of Table 1.8). Unexpectedly, effective trade policy uncertainty has the opposite sign and is significant, with a coefficient of .09.

Table 1.8: Type-Specific Policy Uncertainty

	(1) Sales	(2) Sales	(3) Sales	(4) Sales	(5) Sales
Firm GPU_{t-1}	-0.082***				
	(0.03)				
Firm FPU_{t-1}		-0.055**			
		(0.03)			
Firm MPU_{t-1}			-0.064***		
			(0.02)		
Firm Macro PU_{t-1}					-0.078***
					(0.02)
Firm TPU_{t-1}				0.091**	0.074*
				(0.04)	(0.04)
Observations	6280569	6280569	6280569	6280569	6280569
F	1148.302	1265.329	1190.700	1229.619	1042.269
R-squared	0.942	0.942	0.942	0.942	0.942
R-squared within	0.042	0.042	0.042	0.042	0.042
Firm FE?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm-Time GDP Forecasts Control?	Yes	Yes	Yes	Yes	Yes

Notes: The sample period is 2003-2015. The firm-level dependent variable is the log of real sales. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. Standard errors are clustered at the country-time level between brackets, ** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

In the horserace specification with both effective macro and trade policy uncertainty, the negative magnitude of the average of fiscal and monetary policy uncertainty increases to -.08 while the coefficient on effective trade policy decreases to .07; the coefficients remain significant. Effective fiscal and monetary policy uncertainty similarly lower profits, investment, and employment for more highly exposed firms (see Table 1.9). For profits and employment, effective trade policy uncertainty is no longer statistically significant despite still having a positive coefficient.

Table 1.9: Profits, Investment, and Employment Results

	(1) Profits	(2) Invest	(3) Employment	(4) Avg Wage
Firm Macro PU_{t-1}	-0.078***	-0.034*	-0.050***	-0.005
	(0.02)	(0.02)	(0.01)	(0.01)
Firm TPU_{t-1}	0.040	0.110**	0.011	0.029**
	(0.05)	(0.05)	(0.03)	(0.01)
Observations	4375354	4728337	2309533	2162715
F	479.083	8.656	588.064	108.917
R-squared	0.933	0.743	0.959	0.821
R-squared within	0.027	0.000	0.039	0.004
Firm FE?	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes
+ Firm GDP Forecasts Control?	Yes	Yes	Yes	Yes

Notes: The sample period is 2003-2015. The firm-level dependent variable in: column (1) is the log of firm profits, which is sales minus costs; column (2) is the log of capital investment, which is the change in fixed assets adjusted for depreciation; column (3) is the log of employment; column (4) is the log of average wage, which is total staff costs divided by number of employees. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. In column (2), log of fixed assets is excluded since fixed assets are used in calculating capital investment. Standard errors are clustered at the country-time level between brackets, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

However, for capital investment and average wages, increases in effective trade policy uncertainty tend to increase investment and average wages for firms with higher export shares. If increases in trade policy uncertainty induce rather than delay investment, the result on average wages could reflect compositional shifts, where more exposed firms skill-upgrade by hiring more expensive and better quality workers, while firing low quality workers.[41] For individual type-specific results, see Appendix A.7.

1.5.4 Decomposition

There are three sources of variation in the firm-based policy uncertainty measure—the treatment of exporters relative to non-exporters; the variation in across country-sectors in the construction of shares for external policy uncertainty; and the continuous variation across α_i initial shares. To assess the relative importance of these sources of variation, I relax my baseline approach by perturbing the firm-based measure to uncover the source of identifying variation in the results for sales.

[41] Average wage results are based on firms in the U.K. and France; labor bill data was not provided by Greek or Turkish firms.

1.5.4.1 Binary Export Exposure

To examine how reliant the results are on the continuity of the alpha measure along initially exporting firms, I replace the continuous alpha with binary initial export status (i.e. so that now $\alpha_i = 1$ if a firm exports in the initial period; 0 otherwise). The magnitude of the coefficients on policy uncertainty fall across the different outcomes, particularly for effective macro policy uncertainty, and the coefficient on capital investment becomes statistically insignificant (see Table 1.10).

Table 1.10: Binary Results

	(1) Sales	(2) Profits	(3) Investment	(4) Employment	(5) Avg Wage
Firm Macro PU_{t-1}	-0.027*	-0.019***	-0.013	-0.028***	0.001
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Firm TPU_{t-1}	0.052**	0.031*	0.023	0.024	0.008
	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Observations	6280569	4375354	4728337	2309533	2162715
F	985.226	504.088	3.211	594.976	117.171
R-squared	0.942	0.933	0.743	0.959	0.821
R-squared within	0.042	0.027	0.000	0.040	0.004
Firm FE?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm GDP Forecasts Control?	Yes	Yes	Yes	Yes	Yes

Notes: The sample period is 2003-2015. In these specifications, a binary measure of initial exporting status replaces the continuous initial share measure of exposure for firm-specific policy uncertainty. The firm-level dependent variable in: column (1) is the log of real sales; column (2) is the log of firm profits, which is sales minus costs; column (3) is the log of capital investment, which is the change in fixed assets adjusted for depreciation; column (4) is the log of employment; column (5) is the log of average wage, which is total staff costs divided by number of employees. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. In column (3), log of fixed assets is excluded since fixed assets are used in calculating capital investment. Standard errors are clustered at the country-time level between brackets, *$* p < 0.10$, **$p < 0.05$, ***$p < 0.01$.*

The positive result on effective trade policy uncertainty is again significant and robust in the horse-race specification for sales, but loses its significance for capital investment and average wages.[42] The large decrease in the magnitude of the coefficients and the loss of significance on capital investment points to the importance of continuous variation in initial share in driving the baseline results.

1.5.4.2 Perturbing External Policy Uncertainty

Equal Shares

The first perturbation of external policy uncertainty is to allow shares to be equal across

[42]The results by type-specific policy uncertainty for sales can be found in the online appendix at www.sandile.com.

trade partners, over time, and across sectors. In the baseline, external policy uncertainty (EPU) was $EPU_{cst} = \sum_{m=1}^{44} \theta_{mcs} Uncertainty_{mt}$, where θ_{mcs} was the mean market-county-sector share. This becomes $EPU_t^{Perturb\ I} = \sum_{m=1}^{44} \bar{\theta} \times Uncertainty_{mt}$, where $\bar{\theta} = \frac{100}{44}$. The coefficients on effective macro policy uncertainty do not change dramatically; however, the adjustment strengthens the statistical significance and the magnitude of the coefficients for effective trade policy uncertainty across outcomes, with the exception of average wages (see Table 1.11).

Table 1.11: Equal Trade Share Results

	(1) Sales	(2) Profits	(3) Investment	(4) Employment	(5) Avg Wage
Firm Macro PU_{t-1}	-0.079***	-0.076***	-0.035	-0.041***	-0.016
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Firm TPU_{t-1}	0.129***	0.112**	0.097*	0.067**	0.012
	(0.04)	(0.05)	(0.05)	(0.03)	(0.01)
Observations	6280569	4375354	4728337	2309533	2162715
F	1014.966	536.773	4.716	599.802	123.285
R-squared	0.942	0.933	0.743	0.959	0.821
R-squared within	0.042	0.028	0.000	0.039	0.004
Firm FE?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm GDP Forecasts Control?	Yes	Yes	Yes	Yes	Yes

Notes: The sample period is 2003-2015. In these specifications, instead of using mean Comtrade market shares to construct the external policy uncertainty measure, equal shares across all markets and sectors are used. The firm-level dependent variable in: column (1) is the log of real sales; column (2) is the log of firm profits, which is sales minus costs; column (3) is the log of capital investment, which is the change in fixed assets adjusted for depreciation; column (4) is the log of employment; column (5) is the log of average wage, which is total staff costs divided by number of employees. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. In column (3), log of fixed assets is excluded since fixed assets are used in calculating capital investment. Standard errors are clustered at the country-time level between brackets, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

One possible explanation for the stronger results on trade is that most of the 44 countries are "potential" trading partner countries for firms in the four sample countries, regardless of their particular mean share over the period. In this case, the external policy uncertainty across both realized and "potential" partners would have bearing on firm choice. To test this, it would be ideal to have information about destinations at the firm-level, which is missing from the Amadeus dataset. With such data, I could construct a measure of the likelihood that a market is a potential trading partner based on other firms' export destinations at the country-sector level and weight trading countries based on potential.

Random Re-assignment of Shares Across Markets

The second perturbation of external policy uncertainty is to random re-assign the shares across the 44 export market destinations. External policy uncertainty now becomes

$EPU_{cst}^{Perturb\ II} = \sum_{m=1}^{44} \theta_{ncs \neq mcs} Uncertainty_{mt}$, where θ_{ncs} is a random other country's mean market-county-sector share. The result on effective macro policy uncertainty strengthens for capital investment and falls for sales, profits, and employment (see Table 1.12).

Table 1.12: Randomized Trade Share Results

	(1) Sales	(2) Profits	(3) Investment	(4) Employment	(5) Avg Wage
Firm Macro PU_{t-1}	-0.073***	-0.073***	-0.044**	-0.036***	0.002
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
Firm TPU_{t-1}	0.054***	0.058***	0.005	0.032**	-0.006
	(0.01)	(0.02)	(0.02)	(0.01)	(0.00)
Observations	6280569	4375354	4728337	2309533	2162715
F	1109.123	485.498	3.269	773.224	147.158
R-squared	0.942	0.933	0.743	0.959	0.821
R-squared within	0.042	0.028	0.000	0.039	0.004
Firm FE?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm GDP Forecasts Control?	Yes	Yes	Yes	Yes	Yes

Notes: The sample period is 2003-2015. In these specifications, instead of using mean Comtrade market shares to construct the external policy uncertainty measure, random shares across markets are used. The firm-level dependent variable in: column (1) is the log of real sales; column (2) is the log of firm profits, which is sales minus costs; column (3) is the log of capital investment, which is the change in fixed assets adjusted for depreciation; column (4) is the log of employment; column (5) is the log of average wage, which is total staff costs divided by number of employees. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. In column (3), log of fixed assets is excluded since fixed assets are used in calculating capital investment. Standard errors are clustered at the country-time level between brackets, *$* p < 0.10$, **$p < 0.05$, ***$p < 0.01$.*

For effective trade policy uncertainty, the coefficients fall in magnitude for sales, investment, and average wages, where statistically significance falls away for the latter two outcomes. At the same time, the coefficients for profits and employment rise and become significant relative to the baseline horse-race specifications.

Based on the perturbations of external policy uncertainty's construction, one might conclude that use of specific country-sector trade weights are not central to firms' responses (i.e., it is not the construction of the Comtrade trade-shares that is driving the results). However, it would be erroneous to conclude that the specific set of trading partners included in the weighted average of external policy uncertainty does not matter. More than half of the 44 external markets are European countries. It may be that exposure to a particular European country is proxied by exposure to other European countries.[43]

[43]To test this assertion, one could allow total Europe or EU as its own "market" and construct amorphous "European" policy uncertainty measures, followed by re-weighting the external policy uncertainty measure based on the new European share relative to non-Europe markets. Alternatively, one could add random countries' policy uncertainty measures (i.e., construct measures for countries that the sectors do not trade with ever over the sample and substitute these uncertainty measures for trading partner's policy uncertainty measures).

Together, the switch to a binary alpha and the perturbations of external policy uncertainty do not change the qualitative reactiveness to effective policy uncertainty—the takeaway is still that increases in effective macro policy uncertainty harms more exposed firms, while increases in effective trade policy uncertainty help more exposed firms. The most important margin of variation appears to be the continuity of alpha, the initial share, based on the large falls in magnitude of the coefficients on both macro and trade policy uncertainty across several outcomes.

1.5.5 Robustness

There are a number of remaining potential concerns about the identification strategy. First, one might worry that the first moment control used in the baseline is not accurately capturing the first moment of policy. In this case, the coefficients on policy uncertainty would be capturing the impact of both the first moment and changes in $\overline{P_0}$ on outcomes. A second issue could be that the estimated coefficients on policy uncertainty reflect differences in shocks across firm-types that are not attributable to policy uncertainty. Third, the selection of initial export share as the exposure measure helps avoid endogeneity issues, but one might wonder how much the results change if I allow for time-varying export shares in constructing effective policy uncertainty. Fourth, the outcome variables are likely to be serially correlated, suggesting that I should control for pre-existing trends by adding lagged dependent variables. Finally, I make a timing assumption that a one-period lag on policy uncertainty is the most appropriate choice to reflect firm choices in period t. It could be that alternative lags or concurrent policy uncertainty also influence firm choices.

To address these concerns and assess the robustness of the baseline results, I examine the use of other first moment policy controls; add controls for group-time trends; allow the exposure measure to vary over time; add lagged dependent variables to the baseline specification; and allow for different timing effects of policy uncertainty on firm outcomes.

The use of IMF's WEO forecasts in the baseline follows the rest of the literature in using forecasts as a control for the first moment of policy (see column (1) of Appendix A.8.1 for the baseline result across different outcomes). However, the baseline results are also robust to the use of two alternative firm-specific first moment controls—realized real GDP and WEO $t-1$ real GDP growth revisions (i.e., the revisions in t growth forecasts from the spring to the fall releases), where both are constructed in a similar manner as the firm-specific forecast control (see columns (2)-(3) in Appendix A.8.1). In particular, the robustness of the coefficients to the use of the growth revisions is encouraging as $t-1$ revisions could occur in response to a rise in policy uncertainty in related countries muting the impact of the policy uncertainty measures.[44]

The estimated coefficients on effective policy uncertainty could be picking up some

[44]For robustness of the sales results by type-specific policy uncertainty, see the online appendix available at www.sandile.com.

difference in firm-type (i.e., exporter vs. non-exporter) responses that is not attributable to fluctuations in policy uncertainty. For instance, relative to non-exporters, exporting firms also face "additional upfront sunk and fixed outlays specific to international trade": meeting regulatory requirements in both source and destination countries; setting up distribution networks abroad; conducting destination-specific research; and meeting destination-specific preference and capacity requirements (Chor & Manova, 2012). In the context of the model, higher fixed costs also lead to a higher incentive to delay investment. Thus, a firm's higher exposure to external markets (as proxied by export share) makes it more vulnerable to increases in external policy uncertainty, while its ex-ante participation in exporting activities could also be associated with a greater incentive to delay investments. Columns (4) - (7) add controls for firm types interacted with time variables: initial status with country-sector-time; initial share interacted with country-time and sector-time, separately; initial shares interacted with time; and per-period export shares. Adding such controls allows for the time trend of exporters to be different, while still allowing effective policy uncertainty to retain some identifying variation (e.g., for initial status interacted with country-sector-time fixed effects, the identifying variation on effective policy uncertainty comes from the continuity of alpha).

The coefficients on effective policy uncertainty for sales, profits, and capital investment are robust to these checks and the magnitudes on the coefficients increase (see Appendix A.8.1). For employment, the coefficient falls in magnitude but remains significant for the control that interacts initial status with country-sector-time, but loses significance with the additional group-time controls. For average wages, the inclusion of the group-time controls increase the magnitude of the coefficients on effective policy uncertainty, but only the result on the initial status control remains insignificant (in line with the baseline).

As firms increasingly export more or less of their sales, their exposure/vulnerability to effective policy uncertainty should fluctuate. Moreover, it could be that the initial sample entry period for firms is a systemically biased year with respect to exporting shares (e.g., if exporters tend enter the sample small and then scale up their exporting). To check for this, I re-construct the firm policy uncertainty measures using export share, α_{it}, instead of initial entry export share, α_i. However, this introduces endogeneity as changes in firm export share over the sample could also be determined by the relationship between policy uncertainty and the outcome variables. To address this, I instrument the time-varying export share uncertainty with the initial share firm uncertainty. Across the outcomes, the core results hold (see column (8) across outcomes in Appendix A.8.1).

To examine the robustness of the result to the inclusions of controls for pre-existing trends, I run dynamic specifications that include lagged dependent variables. The inclusion of lagged dependent variables lead to inconsistent estimates that are also biased. However, as Bernard & Jensen (2004) note, a specification in levels that includes the fixed effects provides a lower bound on the coefficient for the lagged dependent variables. To address the inconsistency, I also use an Arellano-Bond difference GMM approach using lagged levels as instruments as an additional check. The results are robust to both

approaches for controlling for lagged dependent variables (see Appendix A.8.2).

Finally, in the baseline I assume that lagged policy uncertainty impacts current firm sales, but do not allow for more persistent impacts in the specification. I also do not allow for concurrent impacts of policy uncertainty on outcomes. To examine both possibilities, I follow Jordà (2005) by using local projections of lagged policy uncertainty on sales (see Appendix A.8.3). Concurrent effective policy uncertainty has a negative impact on sales, however the coefficient is smaller in magnitude than the baseline. This likely reflects issues related to timing aggregation. If a rise in policy uncertainty occurs early in the year, managers may delay firm actions within that same year. Additionally, not all firms use calendar years as fiscal years. Both possibilities would explain concurrent effects of policy uncertainty. However, in support of my approach, the magnitude of the coefficient increases for sales in t. It remains at a similar magnitude for $t+1$, and $t+2$ sales before losing significance. This exercise is supported by the findings of Gulen and Ion (2016) and points to persistent effects of policy uncertainty on firm outcomes.

In summary, the baseline results are robust and the qualitative results continue to hold when substituting other first moment policy controls; including group-time trends; substituting alpha for time-varying export shares; including lagged dependent variables to control for pre-existing trends in outcomes; and allowing for different timing assumptions.

1.6 A New Puzzle for Trade Policy Uncertainty?

The results of Section 5.3 suggest that rising effective trade policy uncertainty tends to be associated with higher sales, capital investment, and average wages for firms with higher export shares. At first glance, this finding appears to contradict the literature that shows positive impacts on outcomes from the reduction of trade policy uncertainty (e.g., Handley & Limao, 2015). In what follows, I run a series of tests in an effort to explore this result.

1.6.1 A deeper look at the trade policy uncertainty results

To uncover whether a particular component of sales is driving the positive result, I separate total firm sales into domestic sales and exports. In addition, to examine the importance of the extensive margin (i.e., entry), I use the inverse hyperbolic sine (IHS) transformation to allow for zeros in these separate specifications. The IHS transformation is defined as $ln(x+(x^2+1)^{\frac{1}{2}})$. It both allows for zeros in the estimation and behaves like the traditional log transformation for reasonable values of x (Zhang, Fortney, Tilford, & Rost, 2000).[45] Large differences between the non-IHS results and the IHS results indicate an important role for the extensive margin. Table 1.13 shows that the positive result

[45]The extensive effects of trade policy uncertainty can also be estimated directly using a nonlinear model (see Appendix A.9.1).

is driven by exposed firms' exports increasing in response to increases in effective trade policy uncertainty, not domestic sales (columns (2) and (3)).

Table 1.13: Trade Policy Uncertainty Sales Breakout Results

	(1) Sales	(2) Domestic Sales	(3) IHS Domestic Sales	(4) Exports	(5) IHS Exports
Firm TPU_{t-1}	0.091**	-0.151*	-0.151*	0.308*	1.997*
	(0.04)	(0.08)	(0.08)	(0.18)	(1.08)
Observations	6280569	6242080	6361912	840670	6521693
F	1203.134	1364.719	1199	445.357	63.879
R-squared	0.942	0.932	0.937	0.884	0.763
R-squared within	0.042	0.036	0.036	0.007	0.004
Firm FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm-Time GDP Forecasts Control?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes

Notes: The sample period is 2003-2015. The firm-level dependent variable in: column (1) is the log of real total sales; column (2) is the log of firm domestic sales, which are total sales minus export sales; column (3) is the inverse hyperbolic sine transformation (IHS) of domestic sales; column (4) is the log of exports; column (5) is the IHS of exports. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. Standard errors are clustered at the country-time level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

For a firm with initial exporting share of 25 percent (roughly, the mean share amongst exporters), a one standard deviation percent increase in lagged firm trade policy uncertainty increases export growth by an average of 2.8 percent in column (4) and by 18.4 percent with the IHS specification in column (5).

The difference between the IHS results and standard results for exports show that increases in sales in response to effective trade policy uncertainty are being driven, in part, by export participation (i.e, a binary choice to participate or not participate at all in a period), not just investment related to increases along the intensive margin. This is confirmed by running an export participation linear probability model—à la Bernard & Jensen (2004)—with the addition of policy uncertainty (see Appendix A.9.1).

1.6.2 What does not explain the positive coefficient on trade policy uncertainty for exports?

There are a number of possible explanations for the positive coefficients on trade policy uncertainty with respect to exports. For example, it could be that particular sectors are driving the outcome or that exporters are concerned about domestic trade policy uncertainty, not external trade policy uncertainty. However, in what follows, I debunk many of these hypotheses.

Explanation 1: Sector, firm, or crisis period characteristics?

To assess whether particular sector, firm, or crisis period characteristics are driving the result on exports, I estimate coefficients across different sectors separately before

turning to specifications that include interactions for firm and period characteristics. Both goods and service sectors have a positive coefficient on effective trade policy uncertainty, although the magnitude is larger for the services sector (see Appendix A.9.2). At the one-digit level, there is not a particular sector responsible for the positive coefficient. Moreover, traditionally non-tradable industries like education, health-care, and other services do not have statistically significant results (which is in line with expectations since firms in non-tradable industries should not be impacted by trade policy uncertainty). To examine if there are specific firm characteristics or crisis observations driving the outcome, I construct dummies for the European debt crisis year (2009-2012); above median firm size and tenure; or if firm is in the goods sectors. The result is not explained by firm size, tenure, or whether the firm is in a goods or services sector (although larger and older firms seem to have slightly less positive coefficients) (see Appendix A.9.2).

Explanation 2: Is trade policy uncertainty asymmetric to other types of policy uncertainty when it comes to exporters?

Recall that the coefficients on effective policy uncertainty reflect the effect of interactions between the initial export share and the ratio of external to domestic policy uncertainty. An alternative way of interpreting the result is to take the inverse of the ratio of external policy uncertainty relative to domestic policy uncertainty: exports fall as the domestic policy uncertainty increases relative to external policy uncertainty. It could be that domestic trade policy uncertainty is more important than external trade policy uncertainty for exporter choices. This would suggest that trade policy uncertainty should be treated asymmetrically to fiscal and monetary policy uncertainty with respect to exposure/vulnerability to policy uncertainty. If I construct interactions between alpha—the initial export share—on domestic trade policy uncertainty and foreign trade policy uncertainty, separately, I find that the coefficients on both the external and domestic trade policy uncertainty interactions are positive, but only the coefficient on the external trade policy uncertainty is significant (Table 1.14).[46] This suggests that the positive responses of largely exposed firms to increasing excess policy uncertainty are reflective of such firms "leaning into the wind" with respect to foreign trade policy uncertainty.

[46]For the non-IHS results, see Appendix A.9.3.

Table 1.14: IHS Trade Policy Results Across Domestic and External Policy Uncertainty

	(1) Sales	(2) IHS Exports	(3) IHS Domestic Sales	(4) Sales	(5) IHS Exports	(6) IHS Domestic Sales
Alpha x Domestic TPU_{t-1}	0.033	1.614	-0.141			
	(0.03)	(1.07)	(0.09)			
Alpha x External TPU_{t-1}				0.060**	2.225**	-0.185**
				(0.03)	(0.87)	(0.07)
Observations	6280569	6521693	6242080	6280569	6521693	6242080
F	1265.462	91.533	1041.646	1277.875	105.008	1094.107
R-squared	0.942	0.763	0.932	0.942	0.763	0.932
R-squared within	0.042	0.004	0.036	0.042	0.005	0.036
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes	Yes
+Firm-Time GDP Forecast Control?	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The sample period is 2003-2015. The firm-level dependent variables are the log of real total sales (columns (1) and (4)); the IHS of exports (columns (2) and (5)); and the IHS of domestic sales, which are total sales minus export sales (columns (3) and (6)). For the interaction terms, Alpha refers to firms' initial export shares; Domestic TPU is the domestic, country-time trade policy uncertainty measure; and External TPU is the external, sector-country-time foreign trade policy uncertainty measure. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. Standard errors are clustered at the country-time level between brackets, ** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

In summary, the positive result on effective trade policy uncertainty is robust across traded sectors, firm characteristics, and cannot be explained by focusing solely on domestic trade policy uncertainty.

1.6.3 Does trade policy uncertainty reflect a decline in the probability of bad news?

In the context of the model, the empirical results on effective trade policy uncertainty are indicative of a decrease in the magnitude and/or likelihood of negative outcomes. When the probability of bad news, B, or the magnitude of potentially bad news, δ, decrease, the incentive to wait falls and firms take immediate action to expand, hire new employees, invest in new capital, etc. For a moment, assume that δ is fixed.[47] A decline in B could be offset by either an increase in the probability of good news, A, or an increase in the probability that the status quo will prevail, C.[48] Increases in A and/or C could result from firms' perception that increases in effective trade policy uncertainty are signals

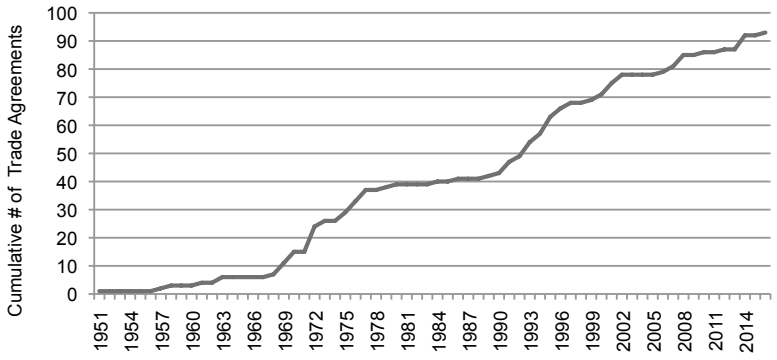
[47]This assumption is not unreasonable in the context of trade agreement negotiations where counterfactuals to payoffs associated with a new agreement are known and fixed (see Handley & Limao, 2015). In the context of trade policy uncertainty associated with protectionism, both δ and B are likely to increase, which I will discuss below.

[48]A decline in B implies that B was some strictly positive value. One might argue that there is no positive probability of a negative outcome in the context of trade agreements (i.e., that B is zero). However, recall that effective policy uncertainty compares external to domestic policy uncertainty; exporting firms have reason to worry about the potential for trade diversion if its trading partners enter agreements with other countries (e.g., European firms and the U.S.-Asia TPP negotiations). Theoretically, recall that $\overline{P_0} = \left(\frac{r}{1+r}\right) F \frac{(r+B)}{r+B(1-\delta)}$. If B is zero, the bad news principle falls away completely, as the impact of bad news through δ also falls away. In this case, changes in A or C would impact firms through the

that resolutions of trade agreements are nearing (whether or not such resolutions actually occur in the near-term) or signals that trade negotiations will stall indefinitely, preserving the status quo. In the context of this research, I am unable to disentangle A from C; even a preservation of the status quo could be a positive signal to a European firm worried about trade diversion due to its trading partners' new trade agreements.

It may seem counterintuitive to conceptualize trade policy uncertainty as ever reflecting a strictly positive signal for an exporter (i.e., that it reflects improved market access or fewer barriers to trade). However, two arguments support this.[49] First, European firms' priors for resolution are likely to be positive with respect to contentious trade agreement negotiation outcomes since the overarching narrative for Western countries in the past 30 years has been towards greater, not lesser integration, especially in Europe. Figure 1.7 displays the cumulative number of trade agreements reached by the European Commission over the period from 1951 to 2014. With the exception of a slowdown in the number of new agreements in the 1980s, there have been consistent increases over time.[50]

Figure 1.7: European Commission's Trade Agreements, 1951-2014



Notes: Figure 7 displays the cumulative number of trade agreements reached by the European Commission over the period from 1951 to 2014. The data were sourced from Dür, Baccini, and Elsig (2014).

Moreover, before major trade agreements often come "dramatic" moments of uncertainty. This relates to the trade negotiation process—while the parameters of negotiations (e.g., agriculture, intellectual property, etc.) are typically made public during the process, the details of deals are only released once agreement between the negotiating parties has been reached.[51] Once details are known, this can generate intense backlash as par-

first moment channel. Empirically, I would expect to see insignificant coefficients on effective trade policy uncertainty after controlling for the first moment of policy uncertainty if B was zero–which is not the case.

[49] Although, I anticipate Brexit to be an exception to this rule.

[50] Based on data from Dür, A., Baccini L., and Elsig M. (2014). The Design of International Trade Agreements: Introducing a New Dataset. The Review of International Organizations, 9(3): 353-375.

[51] From the EU's rules: "The draft texts of the negotiations are not made public during the negotiations.

ticular firms and sectors that believe they will be negatively affected lobby to block the deal's approval. "Losing" sectors and factions can generate considerable press attention during these periods. In an October 2014 *Financial Times* piece, Pascal Lamy, former Director-General of the WTO, lamented that "[a] loud minority is managing to convince consumers they will have to eat chlorinated chicken and genetically modified food." This implies that spikes in trade policy uncertainty, which represent increases in potentially negative news based on the human audit results of Section 4.2, overlap with a timing signal that resolution of trade negotiations is near. That is, just as news chatter about the negative potential implications of trade deals is rising, exporters—the majority of which are likely to benefit from such a deal—may be given signals that resolution of agreements are near.[52] The Uruguay Round of the World Trade Organization began in 1986 and provides a useful example. Figure 1.8 shows the evolution of a constructed Uruguay Round uncertainty index over the period from 1985 to 2000 (the Doha Round began in 2001). Chatter about uncertainty increased right before the round's conclusion in 1994.

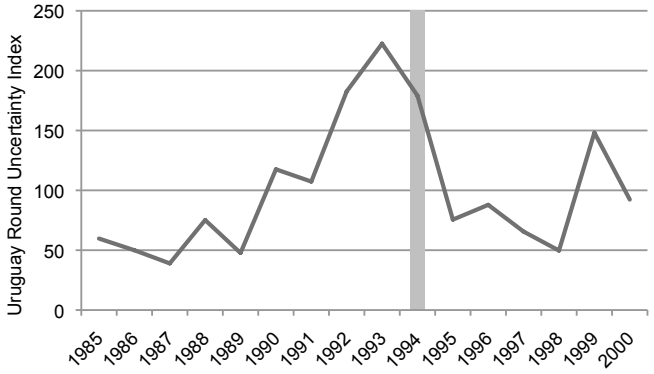
The constructed trade policy uncertainty measure includes news chatter about uncertainty around trade agreement negotiations as well as protectionism. Increases in policy uncertainty about protectionist sentiments should signal increases in both the potential for bad news and the magnitude of impact for a bad news shock. To separate out the two types of trade policy uncertainty, I construct two new policy uncertainty measures: one that references uncertainty around protectionism (Firm TPU I) and another that references trade agreement uncertainty (Firm TPU II). As before, to isolate the separate impacts of each type of policy uncertainty, I exclude the other policy term sets in the search algorithm (i.e., if the search algorithm picks up terms related to protectionism uncertainty, it excludes counts of articles related to monetary, fiscal, and trade agreement uncertainty).[53]

Even when certain chapters (or topics) are 'closed', the negotiation is not over until everything is agreed. When negotiations reach the stage of technical finalisation, the European Parliament and the Council are informed immediately. Finalised texts are sent to the Member States and to the Parliament." Retrieved on 9/17/2016 from http://trade.ec.europa.eu/doclib/docs/2012/june/tradoc_149616.pdf.

[52]One could also interpret this result as evidence of measurement error for trade policy uncertainty. Ideally, one would want a measure that linked the details of trade agreements with the sector a firm is in. For instance, if an agreement is set to improve market access for auto manufacturers, but introduce burdensome regulation around intellectual property for software design firms, a trade policy uncertainty measure should account for the difference in "potential" impact across such sectors. However, such a refinement would require fine detail on trade agreements—some of which are still being negotiated—and a sense of the differential impact of various parameters across sectors.

[53]Protectionism search terms: safeguard measure* or domestic content or anti-dumping or sanitary measure* or rules of origin or countervailing measure* or banana war* or dumping or quota or voluntary export restraint or local content requirement or protectionism or (trade near2 (war or controversy or dispute or polic* or restriction* or quota or sanction or content or embargo or anti or barrier* or red tape or subsid*)) or (import near2 (license or fees or duty or barrier* or tariff* or competit* or tax*)) or (export near2 (license or tax* or subsid*). Trade agreement search terms: GATT or free trade zone or customs union or WTO or World Trade Organization or Doha Round or (trade near2 (deal or delegation or bilateral or free or preferential or commission or negotiation* or agreement* or TRIPS or multilateral)).

Figure 1.8: WTO Uruguay Round Policy Uncertainty Index



Notes: This figure shows an uncertainty measure associated with the Uruguay Round of the WTO and a grey bar denoting the conclusion of the round. The Uruguay Round Uncertainty Index is based on a search for (WTO or World Trade Organization or GATT or "General Agreement on Tariffs and Trade" or Uruguay Round) within the same paragraph as (uncert* or ambiguous or dubious or precarious or unpredictable or undecided or undetermined or unresolved or unsettled or concern or worr* or anxiet* or doubt* or unclear). A search for today is used as a normalizer. The resulting index is normalized to a mean of 100 over a period from 1985 to 2000.

The results for the separate measures are shown in Table 1.15. Increases in effective trade protectionism uncertainty are negatively associated with exports for more exposed firms, while increases in effective trade agreement uncertainty are positively associated with exports.[54] This exercise provides evidence that the positive result on the standard trade policy uncertainty measure is due to trade agreements, not protectionism, supporting the above discussion. As an additional check, I interacted the trade policy uncertainty measures with a time dummy for the preceding year of the deadline for large-scale trade negotiations over the period–the Doha Round and the TTIP negotiations (2004 and 2014, respectively). As expected, these years account for the positive coefficient on trade protectionism (see Table 1.16).

[54]For this exercise, I cluster at the treatment level—the firm level. The results are not significant when clustering at the country-time level; a substantial number of articles are excluded when moving from the original to the separate measures. 10-25 percent of the original trade policy articles are excluded across the four sample countries. It is likely that important variation in both measures is lost due to the strict constraints imposed (e.g., if a protectionism article mentions a trade war, it cannot also mention the WTO or a trade agreement), contributing to the large standard errors on the coefficients.

Table 1.15: Trade Policy Uncertainty Breakout: Protectionism vs. Trade Agreements

	(1) Exports	(2) Exports
Firm TPU I: Protectionism$_{t-1}$	-0.415***	
	(0.02)	
Firm TPU II: Trade Agreements$_{t-1}$		0.060**
		(0.03)
Observations	6308764	5694957
F	1276.897	1111.388
R-squared	0.751	0.761
R-squared within	0.004	0.004
Firm FE?	Yes	Yes
Firm-Time Controls?	Yes	Yes
+ Firm-Time GDP Forecasts Control?	Yes	Yes
Country-Sector-Time FE?	Yes	Yes

Notes: The sample period is 2003-2015. The firm-level dependent variable is the IHS of exports. For the independent variables, Firm TPU I is a firm-level measure of protectionist policy uncertainty and Firm TPU II is a measure of trade agreement policy uncertainty. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. Standard errors are clustered at the firm-level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 1.16: Timing and Trade Agreement Uncertainty

	(1)
Firm TPU II$_{t-1}$	-0.193***
	(0.03)
Timing x Firm TPU II$_{t-1}$	2.207***
	(0.07)
Observations	5694957
F	911.322
R-squared	0.762
R-squared within	0.004
Firm FE?	Yes
Firm-Time Controls?	Yes
+Firm-Time GDP Forecast Control?	Yes
Country-Sector-Time FE?	Yes

Notes: The sample period is 2003-2015. The firm-level dependent variable is the IHS of exports. For the interaction term, timing refers to a dummy for 2004 and 2014. Firm-time controls include the log of age, which is a minimum of one, and the log of fixed assets. Standard errors are clustered at the firm level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

In particular, this paper's results reveal that exporting firms may view increases in effective trade policy uncertainty as a signal of a decrease in the probability of bad outcomes, B, associated with trade agreement negotiations.[55] Future work should explore if this results falls away once a measure for exporters' first moment expectations with respect to trade deals is included (e.g., based on detailed surveys of exporters' opinion of trade deals across both source countries and export destinations).

1.7 Conclusion

Policy uncertainty is one of the most frequently cited impediments to doing business across the globe (Smith & Hallward-Driemeier, 2005). This research offers new insights into its consequences and the channels it operates through. Using a novel dataset of measures of generic, fiscal, monetary, and trade policy uncertainty, I exploit variation in firm-level exposure to external policy uncertainty to construct measures of firm-specific effective policy uncertainty. I find that firms with greater vulnerability to external policy tend to experience larger declines in investment, sales, profit, and employment when effective fiscal and monetary policy uncertainty increase. Increases in effective trade policy uncertainty have the opposite effect. If spikes in trade policy uncertainty are seen as signals that trade agreement negotiations are nearing or that the status quo will be maintained, then the prediction of standard real options theory applies: a decrease in the potential for bad news will raise the incentive to immediately take action.

Returning to the aggregate puzzles mentioned above, the results of this paper can help explain the exceptional decline in trade during the Great Recession and sluggish recoveries. The research suggests that the Great Trade Collapse largely operated through macro policy uncertainty channels, supporting the work of Chor & Manova (2012) and Bordo et al. (2016) on credit channels. When it comes to the sluggish recoveries in France and the United Kingdom, the results provide evidence that the impact of external policy uncertainty on exposed firms is muting growth. Both France and the United Kingdom have seen increases in effective macro policy uncertainty accompanied by sluggish growth since 2011 and 2009, respectively (see Appendix A.11). For France, the increase in effective macro policy uncertainty was driven by a rise in external macro policy uncertainty and

[55] While this work cannot distinguish between an increase in A vs. C relative to a decrease in B, concluding that exporters see increases in effective trade policy uncertainty as a positive signal is not in contradiction with the rest of the literature (see Appendix A.10). There are also alternative theories of investment under uncertainty that could explain this counterintuitive outcome. The growth options literature posits that investments with bounded worst case outcomes paired with a large (or unconstrained) size of the prize will induce firms to invest in the face of increasing uncertainty. This is likely the context for trade agreements. In the worst case, current relationships are maintained (which are subject to existing WTO agreements, FTAs, bilateral agreements, regional agreements, and distribution networks). At the same time, the size of the prize is often large. The Centre for Economic Policy Research report estimates that TTIP will increase EU to U.S. exports by 28 percent with increased sales of 187 billion euros per year (Francois et al., 2013).

a fall in domestic macro policy uncertainty; for the U.K., both external and domestic macro policy uncertainty have increased, but external policy uncertainty has increased by more. In both instances, more heavily exposed firms saw larger declines in sales, profits, and investment (see Appendix A.11).[56] Heavily exposed firms also tend to have higher exports indicating that exposure, in the presence of higher macro policy uncertainty, can help explain muted trade growth and, in part, the muted recovery in both France and the U.K. An implication of this finding is that governments are limited in their ability to unilaterally counter low growth by decreasing domestic policy uncertainty.

Several avenues for future research follow from this work. An initial course of action is to directly explore additional margins of variation with respect to external policy uncertainty, including import exposure and exposure to cross-country financial linkages. The alternatives would suggest a meaningful role for external policy uncertainty in both exporting firms and firms that sell solely to a domestic market. Moreover, cross-country financial linkages might prove more important for monetary policy uncertainty's impact across firms. Making an explicit distinction on the effect of policy uncertainty across firms and conglomerates is another promising path for future work. In this paper, I implicitly assumed that investment choices are primarily reflected at the firm-level. However, firm choices could be made at the conglomerate-level for multi-firm conglomerates. If so, multi-firm conglomerates might engage in strategic portfolio choices in response to increases in policy uncertainty (e.g., strategic pricing) to offset the impact at the conglomerate-level.[57] Multi-firm conglomerates also vary in whether they are domestically or foreign-owned. If a domestic firm is part of a foreign-owned multi-firm conglomerate, it could be buffered from domestic policy uncertainty shocks effective to external (or headquarter) policy uncertainty shocks.[58]

This research provides new insights on the connections between policy uncertainty and firm performance. In addition to offering more nuance for policy directives, the findings of this work can help discipline future theoretical efforts to more accurately model complex dynamics in open economies by accounting for the impact of external policy uncertainty in explaining domestic outcomes via the trade channel.

[56] In France, effective trade policy uncertainty also increased during this period (with a positive offsetting effect), but not by as much as effective macro policy uncertainty.

[57] I find some preliminary evidence of this (see online appendix).

[58] This connects to the literature on the performance of multinationals relative to domestic firms during economically uncertain periods. For example, Desai, Foley & Forbes (2008) find that multinational affiliates tend to expand in sales and investment relative to domestic firms in response to large depreciations, leveraging access to parent firms. Garicano & Steinwender (2015) find supporting evidence that domestic firms experience larger drops in access to finance during crises relative to affiliates with foreign-located parent companies.

Chapter 2

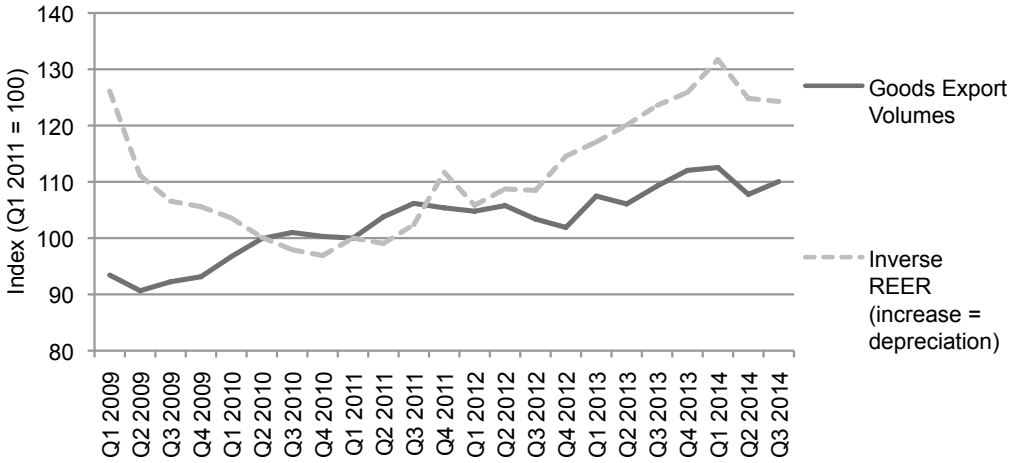
The Consequences of Policy Uncertainty: Disconnects and Dilutions in the South African Real Effective Exchange Rate-Export Relationship

2.1 Introduction

In the post-crisis period, several countries have experienced large depreciation episodes coupled with falling export volumes, marking a disconnect in the traditional relationship between the real effective exchange rate (REER) and export performance.[1] This puzzle is most notably present in Japan.[2] From 2011 to 2014, the Japanese REER depreciated by almost 30 percent; over the same period its export volumes fell by 0.6 percent.[3]

Far more common than REER-export disconnects are dilutions of the REER-export relationship (IMF, 2015). Dilutions in the REER-export relationship occur when the relationship is still in line with theoretical expectations (i.e., rising exports when the REER depreciates), but exports are far less responsive than expected based on historical estimates. One prominent example, and the focus of this paper, is South Africa. From the end of 2011 to early 2014, South Africa's real effective exchange rate (REER) depreciated by 20.6 percent. Over the same period, export volumes rose by only 6.8 percent, suggesting a simple price elasticity of -.3 (see Figure 2.1).

Figure 2.1: South Africa's Real Effective Exchange Rate-Export Dilution



Sources: IMF IFS; Haver Analytics

While a proportion of this sluggish responsiveness can be attributed to weak foreign demand, a demand side explanation does not provide a complete explanation. When we control for foreign demand in an autoregressive distributed lag framework, the price

[1]This chapter is co-authored with Magnus Saxegaard and was previously published as an IMF Working Paper (WP/16/113) in 2016 under the same title.

[2]Other countries with sizable depreciations and falling export volumes include: Cameroon; Cote d'Ivoire; Croatia; Dominica; Gambia; Ireland; Israel; Lesotho; Malawi; Romania; Togo; and Tunisia (based on authors' own calculations).

[3]Amiti, M., Itskhoki, O., & Konings, J. (2014).

elasticity for this period falls to -.4, but is insignificant.[4] This stands in contrast to historical estimates that place South Africa's aggregate export elasticity within a range of -.6 to -6.0.[5] In 2015, export performance improved, however the low elasticity over the 2011-2014 period remains a puzzle.

Recent papers have promoted a number of alternative explanations for these more recent disconnects and dilutions–from increasingly multi-national supply-chains to muted exchange rate pass-through to export prices. This research puts forward an alternative explanation: high domestic policy uncertainty reduces the responsiveness of exports to relative price changes by increasing the real option value of firms adopting a "wait and see" approach when it comes to making large, exporting-related fixed cost investments. For example, uncertainty over how a firm's assets will be taxed might lead that firm to halt expansion or capital investment until that uncertainty is resolved, even in the face of high and rising demand for its products. As opposed to other forms of uncertainty shocks (e.g., natural disasters, terrorist attacks, and external shocks), policy uncertainty is unique in that it can be proactively managed, if not altogether avoided. Existing research on uncertainty makes the link between high uncertainty and poor investment, employment, and output; this research (to our knowledge) is the first to link uncertainty to declining export relative price responsiveness.

After constructing a novel "news-chatter" measure of South African economic policy uncertainty, we find that increased policy uncertainty has diminished the responsiveness of exports to relative price changes. Moreover, increases in policy uncertainty are associated with both direct short and long-run effects on export performance. To date, the uncertainty explanation is robust to alternative explanations that center on credit constraints, supply-chains, and threshold/ boundary effects. Finally, we show that a measure of competitiveness that adjusts the REER for uncertainty outperforms the unadjusted REER in tracking exports in South Africa.

The chapter proceeds as follows: section 2.2 discusses the recent alternative explanations for disconnects and dilutions; section 2.3 describes the channels between uncertainty and poor export performance/responsiveness; section 2.4 details the construction of the measurement for South Africa's domestic policy uncertainty; section 2.5 presents the empirical approach and results; section 2.6 introduces an alternative measure of competitiveness and shows that it outperforms the REER in tracking exports over a 25 year period (including the more recent "puzzle" period); section 2.7 concludes.

[4]This approach was introduced by Peseran, Shin, and Smith (2001) and is described in Section 2.6.2. The sample period covers 2011 to 2014 and uses data at the quarterly frequency.

[5]See Edwards and Garlick (2008) for a summary of historical estimates and Anand, Perelli, and Zhang (2014) for a more recent summary. Also, there was a disconnect in the period prior to 2010; this earlier disconnect can be attributed to the commodity boom.

2.2 Alternative Explanations for REER-Export Disconnects & Dilutions

The preponderance of dilutions and disconnects between REER movements and exports in the post-crisis period has generated several competing explanations. Using a rich firm-level French dataset, Berman, Martin, & Mayer (2012) find evidence of muted exchange rate pass-through to export prices. In response to depreciations, highly productive exporting firms opt to buffer their profits by increasing their mark-ups and engaging in pricing to market behavior, with the result of muting the response of export volumes to changes in relative prices.

Amiti, Itskhoki, & Konings (2014) and Ahmed, Appendino, & Ruta (2015) advance supply-chain explanations, noting that large exporters also tend to be large importers. While large depreciations mean that these firms' exports are becoming more price competitive, their imports are also getting more expensive, partially offsetting the impact of the depreciation. The former authors decompose the importance of this effect relative to the pricing to market effect and find that the two explanations contribute equally in explaining the incomplete pass-through and muted export volume response to depreciations.

The disconnect (i.e., depreciation and falling exports) tends to arise under crisis conditions. Chor & Manova (2012) point to the importance of credit constraints and tight lending conditions in explaining the decrease in export flows during crises. Using a sample largely comprised of European countries, economies with tighter credit conditions saw larger drops in exports, driven by sectors that are heavily reliant on external financing. Alessandria, Pratap and Yue (2013) find evidence to support this finding in emerging markets.

Shirakawa & Shiono (2013) explore a threshold effect, or boundaries, explanation in the Japanese context, where exported goods are highly differentiated. At a lower bound, the market for a highly differentiated good is saturated so additional falls in relative price will not engender increased sales. At a higher bound, the non-substitutability of an intermediate product suggests that downstream customers will purchase it despite the increase in cost stemming from further appreciations. The authors argue that Japan was already below its lower bound in 2011, explaining why exports are were not responsive to the large depreciation.

The International Monetary Fund (2015) conducted a large cross-country study of dilutions, finding that boosts to exports from depreciations tend to be largest when countries have economic slack, normally functioning financial systems, and lower intermediate input trade–confirming many of the above results. Focusing on South Africa in particular, Anand, Perrelli, and Zhang (2014) use a firm-level dataset to examine what characteristics tend to dilute the relationship between the REER and exports. Their results reveal the central role of supply-side bottlenecks in reducing export price elasticity. They find that electricity constraints, low diversification, labor market rigidities, and sectoral concentration all contribute to lower relative price responsiveness for exporting firms.

While the above explanations are presented as alternatives, an uncertainty explanation can be framed as being embedded in several of these explanations. Higher borrowing costs in the credit constraint channel could result from increased uncertainty.[6] Out of bounds depreciations or appreciations could be considered by firms to be proxy evidence of uncertainty, as large fluctuations in the exchange rate might be caused by domestic policy uncertainty. Finally, the decision to engage in pricing to market behavior could be endogenously driven by uncertainty as firms try to shield their foreign customers (and market shares) from domestic policy uncertainty and its effects on costs and the exchange rate.

2.3 From Uncertainty to Exports: Channels

Under a highly uncertain policy environment and in the presence of fixed, irreversible costs, the real option value of delay rises (Dixit & Pindyck, 1994; Bernanke, 1983), hindering export responsiveness and performance. The primary channel for this effect is decreased and delayed investment, as firms adopt a "wait and see" approach. A large literature points to the presence of particularly large fixed costs for exporters relative to producers that sell to a purely domestic market. Such exporting-related fixed costs include export market entry costs (Roberts and Tybout, 1997); costs associated with hiring and firing (Bernanke, 1983; Bloom, 2009; Schaal, 2013); the cost of technology adoption (Bessen, 2002); traditional investment adjustment costs (Ramey & Shapiro, 2001); and the cost of expansion/upgrading of existing facilities to meet importers' demands and preferences. In this context, uncertainty has both direct and indirect effects on exports. The direct effect is a level effect—exports falling under high uncertainty. The indirect effect, and the focus of this paper, is a sensitivity effect—firms are less responsive to changes in demand, prices, and productivity under high uncertainty (Bloom, 2013).

Bloom (2009) constructs a model of firms facing uncertainty, non-convex labor adjustment costs, and non-convex capital adjustment costs. The model features a zone of inaction for investment and hiring, which increases in size as uncertainty increases. "Firms only hire and invest when business conditions are sufficiently good, and only fire and disinvest when they are sufficiently bad."[7] Once the uncertainty is resolved, correction should take place (with firms far from optimum undertaking large corrections and firms close to optimum undertaking small corrections). Since policy uncertainty is correlated with economic uncertainty, this results in pro-cyclical growth in productivity, a stylized fact of business cycles. Baker, Bloom, and Davis (2016) find that increased policy uncertainty leads to diminished investment, employment, and output across 12 major economies.

Handley & Limao (2015) consider the role of trade policy uncertainty in export out-

[6] In particular, there is a large risk premia literature that makes the connection between uncertainty and increased borrowing costs (e.g., Arellano, Bai and Kehoe, 2010; Christiano, Motto and Rostagno, 2009; and Gilchrist, Sims and Zakrasjek, 2010).

[7] Bloom, 2009.

comes. They build a dynamic trade model with heterogenous firms, uncertainty, and fixed costs where firms make entry and trade decisions. The model predicts that firms will limit investment and entry into new export markets under conditions of high policy uncertainty. They test this using Portugal's accession to the European Community in the late 1980s, finding that the reduction of trade policy uncertainty led to improvements in export performance along both the intensive and extensive margins.

Altogether, the theoretical and empirical results of the uncertainty literature suggest both a direct impact of uncertainty on export performance and an indirect effect via decreased relative price responsiveness under periods of high uncertainty due to firms' being in a "zone of inaction" state space. These effects should materialize along both the intensive and extensive margins. Finally, policy uncertainty is both an impulse and a propagation mechanism; that is, while uncertainty can certainly drive a decline in exports directly, it also can arise as a result of economic volatility where it takes on a propagation and amplification role (e.g., if, in response to an external shock, a policymakers disagree on how to best counter it).

2.4 Measuring South African Economic Policy Uncertainty

"Policy uncertainty has been advanced as one of the reasons for poor business confidence levels and the slow pace of private sector investment spending." - *Business Day*, 2 April 2013

"Energy constraints, inadequate transport capacity and uncertainty in the regulatory environment have held back progress." - South African Medium Term Budget Policy Statement, 2011

"The [mining] industry continued to be negatively affected by infrastructural constraints, policy uncertainty, ongoing labour tension, lower commodity prices alongside rising operational costs, and the fragile global recovery." - South African Reserve Bank *Quarterly Bulletin*, March 2015

Policy uncertainty is often mentioned as a key impediment to South African economic performance, above and beyond traditional supply-side barriers like electricity provision, quality of ports, high concentration in several sectors, skill mismatches, and labor market rigidities. However, examination of its impact is rare because of the difficulty associated with accurately measuring it. Baker, et al. (2016) championed a recent approach to measuring uncertainty via the use of "news chatter" in the press. Unlike more traditional outcome measures of uncertainty in an economy (e.g., strike days or currency volatility), "news chatter" uncertainty indices also pick up the threat or anticipation of volatility, whether or not it comes to fruition. At the aggregate level, we constructed both economic

policy and political uncertainty indices. At the sector-level, we constructed industry-related indices. To construct the indices, we count the number of articles that match a certain search algorithms (see below).

Since coverage of different sources varies overtime, the absolute counts are normalized using the number of articles that include the term "today" within 10 words of "South Afric*", where * denotes a wildcard. All indices are standardized to a range of [0,100].[8]

2.4.1 Search Algorithms & Construction

The following search algorithms were employed via the Dow Jones Factiva news aggregator:[9]

- **Aggregate Economic Uncertainty:** Articles that contain three mentions of words related to policy, economics, and uncertainty (i.e., one mention of each area) within 10 words of "South Afric*".
  - Policy: "Department of Energy", "Department of Labour", "Department of Trade and Industry", "Economic Development Department", "government", "interest rate", "National Treasury", "policy", "South African Reserve Bank", "spending", "tax", and "yield"
  - Economic: "econ*"
  - Uncertainty: "uncert*"
- **Aggregate Political Uncertainty:** Articles that contain three mentions of words related to government, politics, and uncertainty (i.e., one mention of each area) within 10 words of "South Afric*".
  - Government: "ANC", "government", "National Assembly", "National Council of Provinces", "Parliament", "President", "protes*", [10]
  - Political: "political", "power", "shake up"
  - Uncertainty: "uncert*"
- **Sector-Specific Uncertainty:** Articles that contain four mentions of words related to the sector, policy, economy, and uncertainty (i.e., one mention of each area) within 10 words of "South Afric*".

[8] A similar method was employed by Redl (2015).

[9] Factiva covers 32,000 sources in 28 languages; over 760 African-based sources; over 450 continuously updated newswires / 700 wires (e.g., Associated Press and Reuters); and press websites (e.g., allAfrica.com; Mail & Guardian Online; etc.). Results can be filtered by language, source type/ source location, subject classification (inclusions & exclusions), and company / industry classification.

[10] The ANC is included as it has been the party in power since the end of Apartheid.

- Each of the following sectors: electricity/energy/load shedding, mining, manufacturing (manufactur*), agriculture (agricultur*)/farming, telecommunications, retail, finance
- Policy: "government", "policy", "Parliament", "regulation", "spending", and "tax"
- Economic: "econ*"
- Uncertainty: "uncert*"

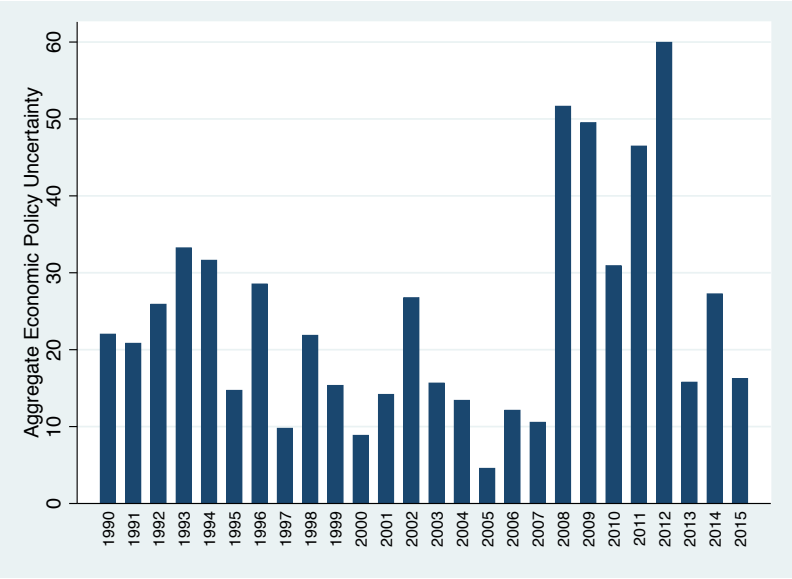
The measures for aggregate political uncertainty and selected sector-specific uncertainty are shown in Appendix B.1.

2.4.2 The Constructed Measure

Aggregate Economic Policy Uncertainty

Economic policy uncertainty worsened during the transition to democracy, however economic policy uncertainty reached maximal levels in the late 2000s in response to external shocks, an energy crisis, and investment regulation uncertainty. While external pressures loomed large in 2008, domestic pressures were also present.

Figure 2.2: South African Economic Policy Uncertainty



The figure displays annual averages of the quarterly measure of uncertainty.

The ruling national party, the African National Congress, split into various factions in 2008 leading many to question whether South Africa would trend towards more populist policies (e.g., nationalization of the mines). Additionally, the national electricity grid nearly collapsed in early 2008 and the government was widely criticized for the electricity

shortages. Uncertainty spiked in 2012 behind news that new power stations were far behind schedule, over budget, and double-digit rate hikes were likely to become the norm going forward. 2012 was also one of the most strike-heavy years in the post-Apartheid era—marked by the Marikana mining tragedy when police and 3,000 striking platinum miners violently clashed at the Lonmin Marikana mine, resulting in the death of 34 miners. The violence was coupled with increasing uncertainty as to how the government would regulate mining companies going forward with respect to ownership and investments.

2.4.3 What Does the Measure of Aggregate Economic Policy Uncertainty Pick Up?

All "news chatter" search algorithms produce imperfect proxies. However, the policy uncertainty measure appears to capture domestic policy disagreements and outages. Below are a few quotes from articles that are included in the count:

- "Revlon, the New York-based beauty products concern, said it was leaving because of 'the **uncertainty** in the **economic** and political situation in **South Africa** created by the **government**'s lack of progress in dismantling its system of apartheid and its failure to achieve racial equality."' - Chicago Sun Times, 5 December 1986

- "**South Africa**'s recovery from the 2008-09 recession has been lackluster, as it suffers the twin blows of low business confidence and policy **uncertainty** at home and the effects of Europe's woes."- Financial Times, 20 July 2012

- "The **South African government** is creating **uncertainties** in the country's mining industry by failing to take a definitive stance on **regulation**." - Wall Street Journal, 15 May 2015

All point to domestic economic policy uncertainty. However, the search algorithm also picks up articles that do not relate to domestic policy uncertainty:

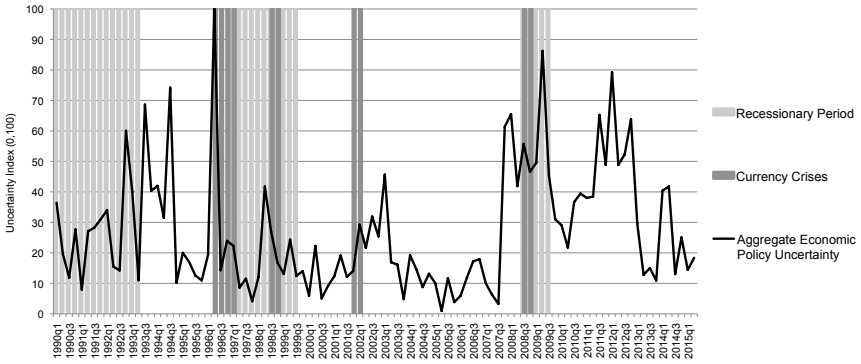
- "South Africa's **economic** growth rate is higher than many predictions. Inflation and interest rates are within government targets. [...] There is one dark cloud, however: the oil price. [...] Back in January 2002 the oil price was below $20 a barrel. **Then US President George Bush started creating all sorts of uncertainty**. Result: the oil price started to run. Fortunately for **South Africa**, the rand started to strengthen at the same time." - AllAfrica.com, 8 November 2004

While imperfect, on the whole, the search algorithm does seem to pick more articles related to domestic economic policy uncertainty than not.[11] Still, given the broad scope of the policy uncertainty search algorithm, one might wonder whether the variable is just another measure of economic volatility. To examine this, we ran several checks.

[11]This conclusion is currently based on an eye-ball audit of the articles. A human audit will be undertaken to get a more definitive answer on how many articles are relevant/non-relevant.

First, we examine the classification of the articles by subject matter.[12] Of the top three subject classifications, two are related to political news, with the top subject being "Domestic Politics."[13] The second check is to explore the correlation between the uncertainty measure and professional forecasting standard deviations, as variation in professional forecasts is often viewed as a strong proxy for economic volatility.[14] While one would expect some correlation, given that policy uncertainty can arise in reaction to economic uncertainty and vice versa, a correlation near one would suggest that the measure is just picking up economic uncertainty. The correlation between uncertainty and forecasting variation is .28. The below figure shows South Africa's recessions, currency crises, and the policy uncertainty variable. As illustrated, spikes in policy uncertainty do not *always* coincide with economic turmoil—particularly in the more recent period.

Figure 2.3: South African Economic Policy Uncertainty, Crises, and Recessions



While these checks suggest that the measure picks up aspects of uncertainty beyond economic volatility, to accurately assess what the measure proxies, a human audit is the first-best approach (and will feature in forthcoming research). A human audit would also answer other open questions. For example, the set of search terms could be further refined to capture the way domestic policy uncertainty is covered in the local media. Additionally, it is possible that some of these articles reference declines in uncertainty, not rises in uncertainty. In Baker, et al.'s (2016) audit of the U.S. uncertainty measure, roughly 5 percent of audited stories were related to a resolution or decrease in policy uncertainty. However, South Africa went through a positive, uncertainty-reducing transition during the sample period suggesting that the number of "positive" uncertainty stories could be higher. For example, there was a considerable reduction in policy uncertainty during the second half of 1996 when then Minister of Finance, Trevor Manuel, released details on

[12]Factiva, not the news organizations, classifies the articles by type (where more than one classification can be assigned to any given article).

[13]The top three subjects, in order of classifications, are "Domestic Politics," "Corporate/Industrial News," and "Political/General News."

[14]The forecasting measure comes from Redl (2015) and is the simple average of a normalized index of the standard deviation of forecasts for growth, inflation, and the domestic interest rate.

the government's new economic program, the Growth, Employment and Redistribution (GEAR) strategy.

2.5 The Role of Policy Uncertainty in Explaining the Dilution in the South African Real Effective Exchange Rate-Export Relationship

2.5.1 Methodology

Traditionally, the literature has specified aggregate export demand functions of the following form:

$ln(export\ volumes)_t = \alpha + \beta_1 ln(relative\ prices)_t\ + \beta_2 ln(foreign\ demand)_t + \varepsilon_t$

to estimate the long-run elasticities via OLS. However, this approach fails to account for short-term dynamics following shocks and the non-stationary nature of such data. Moreover, by using aggregate trade data, such analyses suffer from aggregation bias. To address these concerns, this research employs a dynamic, pooled mean group (PMG) panel approach (Peseran, Shin, and Smith, 1999) over the period from 1995 to 2014 on ten sectors. This approach allows for the estimation of both short-run and long-run parameters (restricting the long-run parameters to be equal across sectors), while also being applicable to both stationary and non-stationary covariates. Such an approach also combats aggregation bias. Typically, the literature has addressed aggregation bias by disaggregating to the bilateral level. As Bahmani-Oskooee and Ardalani (2006) note, "[w]hen aggregate trade data are employed in import and export demand functions, significant price elasticity with one trading partner could be more than offset by an insignificant price elasticity with another trading partner, yielding an insignificant price elasticity." Moreover, Dekle, Jeong, and Ryoo (2006) show that ignoring heterogeneity biases export price elasticity estimates towards zero. For example, Narayan & Narayan (2010) and Anand, et al. (2014) find that the South Africa's export relative price elasticity is not statistically distinguishable from zero when using aggregate trade data. This paper combats this bias by allowing parameters to vary across sectors in the short-run.

The PMG estimator is more flexible than dynamic fixed effects models where all parameters are assumed to be the same in the cross-section, with the exception of intercepts. However, it is more restrictive than the mean group estimator which allows for all parameters to differ across sectors. Intuitively, the use of the PMG estimator in this context assumes that, in the long-run, all sectors will eventually respond to relative price and foreign demand changes, despite exhibiting different short-run dynamics and different speeds of returning to equilibrium relationships following shocks (where such differences are driven by fundamental differences across sectors). In order for the PMG estimator to be consistent, poolability must hold for the long-run estimates. When it does, PMG offers efficiency gains over a mean-group approach. To test for this, we run Hausman

tests comparing the long-run coefficients from mean group estimations to the PMG results; poolability holds and therefore PMG is preferred to the mean group estimator. To implement PMG, we employ the following error correction specification:

$$\triangle ln\left(exports\right)_{it} = \alpha_i + \sum_{j=1}^{q} \beta_{ij} \triangle ln\left(exports\right)_{it-j} + \sum_{j=0}^{p} \eta_{\mathbf{ij}}^{'} \triangle \mathbf{ln}\left(\mathbf{short-run\ determinants}\right)_{\mathbf{it-j}}$$

$$+\zeta_i \left(ln(exports)_{it-1} - \lambda_{\mathbf{i}}^{'} \mathbf{ln}(\mathbf{long-run\ determinants})_{it-1} \right) + \varepsilon_{it}$$

Where i denotes sectors and t denotes years, with coverage of ten sectors over a time period of 1995 to 2013 or 2014 (depending on the determinants included), α_i are sector fixed effects, and ε_{it} is assumed to be independently distributed across time and sectors.[15] The LR relationship can be expressed as:

$$ln\left(exports\right)_{it} = \theta_{\mathbf{i}}^{'} \mathbf{ln}(\mathbf{long-run\ determinants})_{it} + \vartheta_{it}$$

Where $\theta_{\mathbf{i}} = -\frac{\lambda_{\mathbf{i}}^{'}}{\zeta_{\mathbf{i}}}$ are the long-run coefficients and ϑ_{it} are stationary. ζ_i is the error correction term that picks up the speed of adjustment. As above-mentioned, under the PMG estimator, the long-run coefficients are assumed to be constant across sectors, so that $\theta_i = \theta \ \forall \ i$. The long-run parameters are computed via pooled maximum likelihood estimation. In the aggregated results below, the short-run estimates are the unweighted averages of the estimated parameters.

2.5.2 Data

The variables are constructed as follows:

- *Export volumes*: sector-specific South African export volumes from Quantec's Standardized Industry database; derived from Customs & Excise and SARB data and deflated using appropriate Stats SA price indices.
- *REER*: The IMF's International Financial Statistics real effective exchange rate, where increases are appreciations/ decreases in competitiveness.
- *Demand volumes*: sector-specific world import data from Comtrade, deflated using sector-specific U.S. import price indices where available and the overall U.S. import price index where unavailable.
- *High Exchange Rate Volatility:* a dummy for South African periods of high exchange rate volatility in 1996, 1998, 2001, and 2008; identified via a GARCH model by Duncan and Liu (2009).

[15] The number of lags of the export first differences and the determinant first-differences is limited to 1. A number of standard panel unit root tests (e.g., Harris-Tzavalis and Im-Pesaran-Shin) were run to check that the first differences are stationary, all with rejections of non-stationarity.

- *Electricity Bind*: mean ratio of electricity consumption to electricity production interacted with sector-specific electricity intensity of usage. Production and consumption data sourced from Statistics South Africa via Haver Analytics. Electricity intensity data from Inglesi-Lotz and Blignaut (2011).

- *Aggregate Economic Policy Uncertainty*: authors' own calculations; see section 2.4. While sector-specific indices were constructed, we opt to use the aggregate measure since outlier articles can bias industry uncertainty measures in sectors that the media rarely cover.

- *Sector Labor Rigidities*: working days lost by sector by year; sourced from the ILO for years prior to 2008 and from various South African Department of Labour's Annual Industrial Action Reports thereafter. Unionization rates are from Statistics South Africa.

With the exception of demand, which is expected to have a positive relationship with exports, all other determinants are expected to lower exports.

2.5.3 Long-run Results

Column (1) provides the baseline, long-run results.[16] If no controls are included for policy uncertainty or supply-side factors, the REER elasticity is insignificant despite having the expected sign. Increases in foreign demand are a strong, positive indicator of increases in export volumes, which is true across all specifications. In contrast, periods with high exchange rate volatility are associated with falls in exports across all specifications. Adding policy uncertainty in column (2) leads to a slight decrease in the REER elasticity, but it remains insignificant. Still, policy uncertainty enters significantly, pointing to a negative direct effect of uncertainty on exports. Moreover, the uncertainty coefficient is significant in the presence of the dummy for periods of high currency volatility (a proxy for currency crises). This suggests that uncertainty impacts exports, even when the economy is not in a economically volatile period.

[16]The Hausman test statistic is 1.99 and insignificant, indicating that the pooled-mean group estimator is favored over the mean group estimator for this sample.

Table 2.1: Long-Run Results

Dependent Variable: Change in ln(Export Volumes)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Real Effective Exchange Rate	-0.157	-0.206	-0.426**	-0.548**	-0.506**	-2.683***	-3.061**
	(0.161)	(0.166)	(0.203)	(0.257)	(0.227)	(1.103)	(1.429)
Foreign Demand	0.840***	0.854***	0.837***	0.827***	0.921***	0.858***	1.019***
	(0.0478)	(0.0543)	(0.0507)	(0.0678)	(0.0470)	(0.0613)	(0.0882)
High Exchange Rate Volatility	-0.128***	-0.137***	-0.142***	-0.204***	-0.0833*	-0.157**	-0.172**
	(0.0483)	(0.0523)	(0.0507)	(0.0702)	(0.0484)	(0.0631)	(0.0698)
Aggregate Economic Policy Uncertainty		-0.0577*		-0.0794*	-0.0662**	-3.361**	-4.873***
		(0.0336)		(0.0438)	(0.0265)	(1.643)	(2.232)
Electricity Bind			-2.290	-1.388	-3.612***	-2.634	
			(1.579)	(1.964)	(1.230)	(1.728)	
Labor Disputes					0.0156		
					(0.0114)		
Real Effective Exchange Rate x Policy Uncertainty						0.768**	1.047**
						(0.360)	(0.488)
Observations	190	190	190	190	190	190	190

Notes: Standard errors are in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

If electricity constraints are added, as in column (3), the REER elasticity now falls to -.43 and is significant at the 5 percent level, suggesting that omitted variable bias was resulting in its underestimation. While large in magnitude, the electricity constraint coefficient is noisy. This is likely due to measurement error; the measure proxies binds as periods when consumption approaches production levels. However, this measure does not pick up actual blackouts and also does not account for the fact that South Africa has imported electricity during high demand periods in recent years.[17]

When both uncertainty and electricity constraints are included (column (4)), the REER elasticity falls to -.55, in line with historical estimates of South Africa's export relative price elasticity. The inclusion of labor rigidities increases the REER elasticity slightly and it also enters insignificantly. This was unexpected given that South Africa ranks 5th in the world for the average rate of days not worked per 1000 workers.[18] Moreover, the transition to democracy featured a rapid increase in the number of unionized workers (all government and state-owned enterprises were unionized). It could be that, prior to the 2012 Marikana mine violence, strikes typically did not hinder exports as they were relatively non-violent and resolved in an expedient manner due to the close relationship between government and labor. Another reason for its insignificance is that the uncertainty measure picks up some of the impacts of labor strife (this is also the case for electricity constraints).

With confirmation of the direct effect of uncertainty on exports, column (6) explores the presence of an indirect, sensitivity effect of uncertainty on price responsiveness. The interaction term shows that with higher uncertainty, REER responsiveness falls. When

[17]The volume of imported electricity increased by almost 20 percent from 2013 to 2014. Source: StatsSA/ http://www.statssa.gov.za/?p=4045

[18]Bhorat, H., & Tseng, D. (2014).

uncertainty is one standard deviation below the mean, the implied REER elasticity is -.92; at the mean, the REER elasticity is -.41. When uncertainty is one standard deviation above the mean, the REER elasticity is .10, with a reversal in its expected sign and a small magnitude. We obtain similar results when dropping the electricity constraint (see column (7)).

Finally, to test the stability of the results, we interacted a period dummy (for subsequent periods from 2004–the midpoint of the sample–onwards) with economic policy uncertainty to assess whether uncertainty has had a larger impact due to the increases in its magnitude in the period after 2007. There is scant evidence of a period effect, with the exception of the 2010 to 2014 dilution "puzzle" period.[19] During this period, economic uncertainty had a larger negative role on exports relative to the 1995-2010 period. This suggests evidence of a threshold effect; that is, fluctuations in policy uncertainty are less impactful when uncertainty is at a lower magnitude.

Together, the results presented above support the theoretical literature that points to dual roles for policy uncertainty–a direct effect and a sensitivity effect as firms become less sensitive to relative price changes during high uncertainty periods.

2.5.4 Short-run Results

While not of central interest, there is important heterogeneity in parameters across sectors in the short-run. The below results, which correspond to column (4) in the long-run results above, show the average short-run parameters across the 10 sectors and a few of the sectors' results shown as examples.

Table 2.2: Selected Short-Run Results

Selected Short-run Coefficients	**Average**	**Mining and Supporting Firms**	**Chemicals, Rubber, Plastics, and Glass**	**Motor Vehicles and Other Transport**
Speed of Adjustment	-0.268***	0.036	-0.348***	-0.207***
Real Effective Exchange Rate	-0.428***	0.114	-1.023***	-0.363*
Foreign Demand	0.326**	0.027	0.455*	0.928***
Aggregate Economic Policy Uncertainty	-0.040***	-0.023	-0.048*	-0.065**

*Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

The average speed of adjustment term indicates that sectors take ~3.7 years to adjust back to long-run equilibrium relationships following disequilibrating shocks (or a 27 percent adjustment per annum). Manufacturing sectors tend to adjust faster than commodity-producing sectors; mining's speed of adjustment term is near zero. In fact, the mining sector shows very little responsiveness to relative prices, foreign demand, or policy uncertainty in the short-run. This may reflect a number of factors including the industry's non-competitive market structure and long lead times on fulfilling orders. The lack of mining relative price responsiveness can also be explained by the high incidence of dollar pricing in commodities. In the short-run, the chemicals/rubber/plastics/glass industry is far more responsive to relative price changes than the transport vehicles in-

[19] Results available upon request from the the authors.

dustry; the opposite is true of demand. The reasons for these disparities may relate to differences in the composition of importers across industries or myriad other fundamental factors. Regardless of the reasons for their differences, the variability makes it clear how important allowing for short-run heterogeneity is. The short-run elasticities for electricity constraints and labor disputes (when included) are insignificant and not shown.

2.5.5 Uncertainty vs. Competing Explanations

This section examines whether the importance of uncertainty, both directly and via the sensitivity effect, is is robust to the inclusion of controls for alternative channels.

2.5.5.1 Credit Frictions

The credit friction channel argues that increases in borrowing costs and a tight lending environment during times of crisis tends to lower exports, particularly in sectors that are heavily reliant on external financing. To examine this channel, we use Chor & Manova's (2012) sector external finance dependence measure interacted with South African interbank rates as a sector-specific external financing constraint measure. The external financing dependence measure, which proxies sectors' total requirement for external financing, is based on US firm data. The authors used it in a cross-country context, arguing that the financing choices of US firms reflect "an optimal choice over external financing and asset structure."[20] To examine whether these effects are amplified during economically vulnerable periods, we include an interaction term for high exchange rate volatility periods in the specifications.

Below are the long-run results. While the signs of the coefficients on the external financing constraint are negative as expected, they are insignificant. The direct and indirect relationships between the REER and uncertainty hold.

As discussed, the credit channel could embed an uncertainty explanation (i.e. uncertainty leads to increases in borrowing costs, which then leads to a reduction in responsiveness). Thus, policy uncertainty may be absorbing the relevant variation associated with a credit channel. However, even when policy uncertainty is not included in the specification, the coefficients on the credit channel variables are still insignificant. It may be the case that credit access is not binding constraint for South African exporters; however this would make the experience of South African exporters an outlier relative to the experience of exporters in emerging and developed markets alike. This channel deserves more attention in future research.

[20] Chor & Manova, 2012.

Table 2.3: Credit Friction Results

Dependent Variable: Change in ln(Export Volumes)	(1)	(2)
Real Effective Exchange Rate	-0.487**	-1.521***
	(0.228)	(0.565)
Aggregate Economic Policy Uncertainty	-0.0584*	-2.040**
	(0.0353)	(0.845)
REER x Policy Uncertainty		0.440**
		(0.186)
External Financing Constraint (EFC)	-0.0317	-0.0466
	(0.0321)	(0.0363)
EFC x High Exchange Rate Volatility	-0.000380	0.0120
	(0.0113)	(0.0156)
Observations	190	190

Notes: Full model (not shown) includes electricity bind, high exchange rate volatility, and demand parameters. Standard errors are in parentheses, *$* p < 0.10$, **$ p < 0.05$, ***$ p < 0.01$.

2.5.5.2 The increasing importance of global supply/value-chains

Increasingly multinational supply-chains moderate the zero-sum nature of depreciations. In South Africa, the domestic value added share of gross exports fell from 86.8 to 80.5 percent from 1995 to 2011, suggesting an increasing role for imported intermediates trade.[21] To examine this channel, we use the OECD's Trade in Value Added measure of foreign value added share of gross exports by sector. It is expected that the relative price responsiveness of exports will fall as import content of exports rises. [22]

Below are the selected long-run results. Column (1) offers support for the support for supply-chain/ value-added literature. For import content one standard deviation below the mean, the REER elasticity is -1.39; at the mean level of import content, the REER elasticity is -.24. For import content one standard deviation above the mean, the REER elasticity is positive and .92. For highly integrated sectors, a REER appreciation's positive effects (from lower imported intermediate input costs) appear to offset the negative effects of the appreciation.

[21] OECD Trade in Value Added (TiVA) dataset.

[22] For missing data within 1995 to 2011 period, we linearly interpolated the missing values; after 2011, we linearly extrapolated using average growth over the period from 2007 to 2011.

Table 2.4: Supply Chain Results

Dependent Variable: Change in ln(Export Volumes)	(1)	(2)
Real Effective Exchange Rate	-10.82***	-3.362***
	(2.727)	(1.256)
Aggregate Economic Policy Uncertainty	-0.0969**	-4.512**
	(0.0494)	(1.788)
Import Content in Exports	-15.68***	0.204
	(4.170)	(0.159)
REER x Import Content in Exports	3.514***	
	(0.924)	
REER x Policy Uncertainty		0.977**
		(0.392)
Observations	190	190

Notes: Full model (not shown) includes electricity bind, high exchange rate volatility, and demand parameters. Standard errors are in parentheses, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

The coefficient on policy uncertainty in column (1) remains significant and falls relative to other specifications. Finally, in column (2) the sensitivity effect of uncertainty on exports survives inclusion of the supply-chain control (which is insignificant).

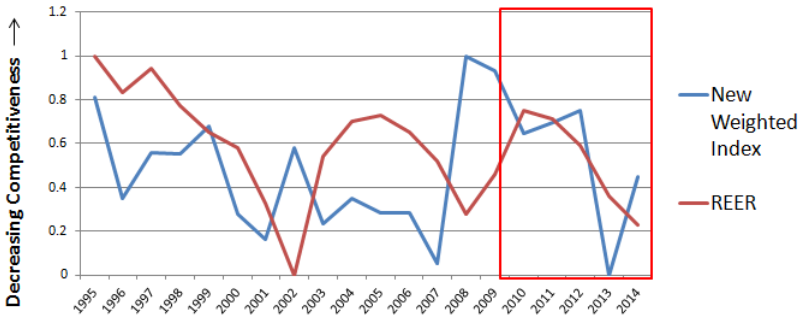
2.5.5.3 Threshold Effects

There is no evidence of threshold effects in exports' REER elasticity. To check for this, we used both 90/10 and 75/25 percentiles and examined whether there were different coefficients for high bounds, low bounds, and dual bounds. The lack of boundary effects can be attributed to several explanations. For example, given the large proportion of commodity exports in South Africa, its exports are less differentiated than Japanese exports. Also, it may be that there is less expectation of a consistent range for the REER in developing market contexts.

2.6 Alternative Measure of Export Competitiveness

The results of the previous section reveal the importance of electricity constraints and policy uncertainty (directly and indirectly via decreased price responsiveness) in tracking exports. In this section, we use these findings to construct alternatives to the REER as measure of export competitiveness that adjust for uncertainty and electricity constraints.

Figure 2.4: Alternative Measures of Export Competitiveness



2.6.1 Measures of Export Competitiveness

Four new measures are created. The first is a weighted index, where the weights, θ_i, are constructed from the coefficients from column (6), under section 2.5.3, normalized by the REER coefficient θ_0.

$$New\ Index\ of\ Competitiveness =$$

$$ln\left[REER^{\theta_0} \times Uncertainty^{\theta_1} \times (REER \times Uncertainty)^{\theta_2} \times Electricity\ constraint^{\theta_3}\right]$$

The second index is an unweighted aggregate where each component is given equal weight (all $\theta_i = 1$). A third index drops the electricity constraint component, where the weights are based on a PMG specification without electricity constraints. The fourth index drops policy uncertainty; again, the weights for this index are based on a PMG specification that excludes policy uncertainty. A comparison of the weighted index and the standard REER show the large impact of the adjustments (see below figure).

While the two measures sometimes track each other (especially in the period prior to 2001), the correlation between the two measures is only .23 over the period from Q1 1990 to Q4 2014. During the recent puzzle period (marked by the red box), the REER shows a consistent and persistent improvement in export competitiveness in the past five years. However, the new weighted index shows that, while there was an increase in competitiveness in 2013, both 2012 and 2014 saw decreases in competitiveness.

2.6.2 Methodology

To compare the relative indices' ability to track exports, we employ an autoregressive distributed lag (ARDL) cointegration approach paired with error correction bounds testing developed by Banerjee, et al. (1998) and extended by Peseran, Shin, and Smith's

(2001) (hereafter, PSS).[23] The approach uses aggregate time-series data, leaving it open to aggregation bias. This makes the likelihood of getting significant results less likely than in the panel setting since the elasticities will be biased towards zero. Additionally, the choice to use aggregate trade data (unlike in Section 2.5, where sector-level data are used) is motivated by policymakers – in computing export price elasticities, which are then used to assess the extent of currency misalignment, aggregate trade volumes are used by international financial institutions and governments alike. The following ARDL specification is used:

$$\triangle ln\left(exports\right)_t = \alpha + \sum_{i=1}^{m} \beta_i \triangle ln(exports)_{t-i} + \sum_{j=0}^{n} \gamma_j \triangle ln(external\ demand)_{t-j}$$

$$+ \sum_{k=0}^{o} \iota_k \triangle ln(Competitiveness\ Measure)_{t-k}$$

$$+\lambda_1 ln(exports)_{t-1} + \lambda_2 ln(external\ demand)_{t-1} + \lambda_3 ln(Competitiveness\ Measure)_{t-1} + \varepsilon_t$$

If no long-run relationship exists, then $\lambda_1 = \lambda_2 = \lambda_3 = 0$ in the above unrestricted error correction model. The distribution of the F-test for this hypothesis is nonstandard; bounds are provided by Peseran, et al. (2001) for large samples and by Narayan and Narayan (2004) for smaller samples. If the F-stat falls outside two bounds, one can determine if the variables are cointegrated (below a lower bound indicates no cointegration; above an upper bound indicates definite cointegration; within the bounds is an inconclusive result). Once cointegration is confirmed, the correct ECM is chosen based on a BIC test and the short and long-run elasticities can be computed. Higher frequency, quarterly data is available at the aggregate level for a longer period (1990 onwards). Data for the REER come from the IMF's IFS; foreign demand is proxied by OECD real GDP[24]; and aggregate export volumes are from the South African Reserve Bank.

2.6.3 Results

All the models show evidence of cointegration; the F-stats are above the 1 percent upper bound. There is stronger evidence of cointegration for the new measures of competitiveness, particularly for the weighted and uncertainty-adjusted measures. The weighted index also outperforms the other measures in tracking quarterly export volumes over the

[23] This approach outperforms the more widely used JJ maximum likelihood test for cointegration, which is not robust with small sample sizes. Moreover, PSS is applicable irrespective of whether the included time-series are of varying degrees and types of stationarity.

[24] Level data available up to 2013Q4. We used OECD quarterly real GDP growth rates from OECD to complete the series.

sample period; a 10 percent decrease in competitiveness is associated with a 5.7 percent increase in export volumes. The elasticities for the other measures are not statistically distinguishable from zero, although the index with no electricity bind (i.e. the uncertainty-adjusted REER) comes close to being marginally significant.

Table 2.5: Alternative Measures of Competitiveness Cointegration Results

Measures of Competitiveness	**Estimated Long-Run Elasticity**	**PSS Bounds Test F Statistic**
Real Effective Exchange Rate	-0.07	6.42***
Weighted New Index	-0.57**	19.10***
Unweighted New Index	-0.00	17.70***
No Electricity Bind	-0.39	18.10***
No Uncertainty	-0.14	6.92***

Granger causality tests of the same models (i.e., exports, a competitiveness measure, and foreign demand) support these findings. The weighted index granger causes exports, while the REER does not. Exports do not granger cause either measure. The results of this section indicate that the REER is an insufficient measure of competitiveness for tracking exports in South Africa. Policy uncertainty and electricity constraints greatly improve a competitiveness measure's ability to track exports.

2.7 Conclusion

This research bridges the economic policy uncertainty and REER-exports disconnect-dilution literature by examining the impact of policy uncertainty on exports' relative price responsiveness. We find that aggregate policy uncertainty is associated with lower export performance in both the short and long-run, directly and indirectly (via decreased relative price responsiveness). In the absence of policy uncertainty and other supply-side constraints, South African exports would be far more responsive to relative price changes. The policy uncertainty channel is robust to several alternative explanations for the REER-export relationship's dilution. Finally, the real effective exchange rate (REER) is an insufficient measure for tracking overall export performance. A measure that appropriately adjusts for uncertainty and electricity constraints greatly outperforms the REER in tracking exports.

There are several future directions for research. Conducting a human audit of the uncertainty variable will help uncover what types of policy uncertainty are driving the results. Second, because uncertainty is both an impulse and propagation mechanism, it is difficult to establish causality. Examination of large uncertainty shocks in an event-study framework may help advance a causal explanation (e.g., use of the December 2016 changes in the South African Minister of Finance). Bernanke, et al. (1997) also suggest structural VAR methods that adjust for feedback mechanisms. In terms of alternative explanations, the exchange rate pass-through explanation still needs to be explored. Extension of the work to other countries, especially other dilution/disconnect countries, would help assert the generalizability of the work. There are also further refinements that can be made to the competitiveness measure. For example, instead of just accounting for South African policy uncertainty, the policy uncertainty of its trading partners can also be adjusted for.

Chapter 3

Valuing Justice: the International Criminal Court's Indictment of the "Ocampo 6"

3.1 Introduction

On December 30th, 2007, Kenya's Electoral Commission Chairman announced the official results of the country's 2007 presidential election—Mwai Kibaki, the incumbent candidate, had won with an edge over Raila Odinga of 231,728 votes out of 10 million votes cast. Almost immediately, widespread allegations of election fraud surfaced.[1] Supporters of both Kibaki and Odinga took to the streets to defend their respective candidates; what started as peaceful protests quickly devolved into violence amplified by long-standing ethnic rivalries. Thirty days later more than 1,220 people were dead, approximately 100,000 properties were destroyed, and some 1.2 million had been displaced in what was the worst violence seen in Kenya's post-independence period.[2] As the dust settled, it became clear that the violence had been too well organized and targeted to be the result of "random" clashes. Both Kenyans and the international community demanded that the perpetrators—later revealed to be six of Kenya's foremost public figures—be brought to justice. After several false starts at the local level, the International Criminal Court (ICC) stepped in to accomplish this task.[3]

This research examines how investors and Kenyan firms have responded to the actions of the ICC, exploiting variation in stock market responses between firms connected to the accused and those not connected to the accused in the context of an event study framework. In line with the recent political connection literature (e.g., Faccio, 2006; Acemoglu, et al., 2013), several definitions of connectedness are employed. In addition to shedding light on the value of different types of political connections in Kenya, this research also offers perspective on valuations of "imported" or supplementary institutions—the ICC is a voluntary organization and a complement to national courts. The supranational court only intervenes when courts are unwilling or unable to do so and only for the most egregious of crimes.

Responses to ICC actions can be interpreted as reactions to "judicial" shocks; the market is responding to the imposition of rule of law. Historically, these types of shocks have been difficult to examine; country-level judicial institutions are endogenous to countries' histories, culture, elites-power dynamics, and other factors that make the actions of such institutions endogenous as well. The actions of the ICC differ in that they can be considered as exogenous to local conditions as the ICC's timing has been largely unrelated to domestic forces.[4]

[1] For example, several districts posted voter turnouts exceeding 100 percent. When challenged, election commissioners made their own subjective adjustments to the tallies. "Results were announced even when documents were missing, incomplete, unsigned by officers or party representatives, incorrectly tabulated, photocopied or forged." (Bengali, 2008)

[2] One Luo woman described her experience during the post-election violence in Mombasa, stating that "it was as if we ceased to be human for a moment." (Bjork & Goebertus, 2014)

[3] For a timeline of the events post-election, see Appendix C.1.

[4] One element of endogeneity in the ICC shocks was the ICC's adoption of the deadline for establishment of a local tribunal created by the Waki Commission (a group tasked with investigating the post-election violence). However, once this deadline passed, the ICC acted independently in its investi-

ICC-led judicial shocks also differ from other politically-destabilizing shocks (e.g. coups, assassinations, or a dictator's illness) in that judicial shocks can be potentially stabilizing under the assumption that these shocks can be a deterrent of new atrocities and unrest. That is, the accused positively alter their behavior upon indictment in an attempt to disprove the claims of the ICC. Perceivable declines in the use of violence-encouraging hate speech from the 2007 to 2013 election offer anecdotal support of this claim.[5] Moreover, following the indictment, two of the accused—who were on opposite sides of the 2007 post-election violence—announced a joint bid for executive. When Kenya's recent 2013 presidential campaign and election transpired without violence or allegations of fraud, many pointed to the ongoing ICC proceedings as the key driver of the positive outcome (ICG, 2013; Hansen, 2013).[6]

While arguments supporting the stabilizing-effect of ICC intervention are well-received in the policy and legal world (with convincing anecdotal evidence to validate them), this research does not find any evidence of investors levying any aggregative opinion—good or bad—on the ICC's intervention for the economy as a whole. While overall abnormal market returns tended to be negative during ICC news shocks, difference-in-differences estimations uncover that unconnected firms did not experience significantly negative revaluations surrounding adverse ICC news announcements. This finding suggests that investors in Kenyan firms have been discerning when it comes to the fallout associated with the ICC case by focusing their revaluation efforts largely on connected firms.

To this end, there is strong evidence that ICC-connected firms experienced declines in abnormal returns during ICC shocks, with particularly negative revaluations for ICC-board linked firms. One explanation for the disproportionate impact on ICC-board linked firms is that these firms had more transparent links to the current President, and ICC accused, Uhuru Kenyatta, relative to firms with board members that serve as advisors to the ICC accused. This research not only confirms that close connections with political leadership have value for connected firms, but also that a lack of transparency about such connections can potentially shield connected firms from large negative shocks related to their connections.

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gations and court proceedings.

[5]Since the early 1990s, Kenya's political candidates have used hate speech to rally their respective bases and intimidate other ethnic groups into not voting. In the 2007 election, hate speech played a catalyzing role in the genocide-like violence against the two candidates' ethnic groups. In recognition of this, the ICC brought charges against, Joshua Sang, a Kalenjin radio disc jockey who during the post election period hosted shows where it was encouraged that "the 'people of the milk' (the Kalenjin) to 'cut the grass' and get rid of the 'weeds' (the Kikuyu). (Bruce-Lockhart, 2013)." During opening proceedings for the cases, the ICC also warned the remaining accused that ongoing use of hate speech (and the politicization of ethnicity) would result in further charges (and the possibility that the accused would be jailed at the Hague during the course of the trial). Several scholars have documented a noted decline in hate speech used by the accused after the warning was issued (e.g. Ezeibe, 2013; Bruce-Lockhart, 2013). Sang was the only non-politician in the Kenyan ICC cases.

[6]According to a Brooking Institution report, "there were no serious incidences of violence and the electoral process was deemed by many international observers as free, fair and credible."

The next section discusses this chapter's connections to existing literature, followed by more context and detail on the ICC and its Kenyan case. Section 3.4 discusses channels found in the literature that might link the actions of the ICC to investor opinions on aggregate outcomes and the firm-level outcomes of connected and unconnected firms. Section 3.5 gives an overview of the data and empirical methodology of the paper; Section 3.6 discusses preliminary empirical results. Section 3.7 presents some robustness checks and falsification efforts, while Section 3.8 briefly discusses some suggested extensions to the current work before concluding.

3.2 Connections with Existing Literature

To date, research on the efficacy of the ICC's interventions has been limited to legal scholars, historians, and policymakers; therefore, the present research adds economic perspective to this ongoing conversation.[7] One of the most contentious areas of the debate is whether the ICC tends to have a stabilizing or destabilizing impact in countries where its prosecutions focus. Typically, references to stabilization in this literature are directed at political stability; however the implications of political stability (or a lack thereof) easily spillover to economic outcomes. Destabilization can occur if the ICC tends to impede the ability of violent actors to reach a peaceful solution. For instance, some argue that Joseph Kony, leader of the notorious Lord's Resistance Army and an ICC fugitive, cannot negotiate a ceasefire or surrender to state authorities because he would be immediately arrested by the ICC (Lomo, 2006). Destabilization can also occur through the creation of a power vacuum (Ku & Nzelibe, 2006), a point I discuss in more detail below.

On the other hand, international tribunals like the ICC can also alter the behavior of the accused in positive ways, promoting stability. Akhavan (2009) argues that "tribunals alter the cost-benefit calculus of using atrocities. [...] In Cote d'Ivoire, the mere threat of an ICC investigation contributed to preventing escalation of an inter-ethnic war by putting an end to state-sponsored incitement to hatred. In Uganda, ICC arrest warrants against rebel leaders responsible for mass atrocities helped pressure neighboring Sudan to eliminate a long-standing safe haven for the rebels, bringing to an end a devastating civil war."[8] Kenya's peaceful 2013 election reinforces this viewpoint.

Within the economic discipline, this research complements the large literature on political connections. In his seminal study, Fisman (2001) showed that Indonesian firms connected to the Suharto regime, infamous for its poor governance, lost more market value than unconnected firms during episodes where information was leaked that Suharto's health was in jeopardy. Johnson & Mitton's (2003) research on Malaysia showed that politically-connected firms' value rose and fell with the prospects of their political connections.

[7] In fact, I was unable to find any papers on the International Criminal Court written by economists.

[8] Akhavan, P. (2009). Are international criminal tribunals a disincentive to peace?: Reconciling judicial romanticism with political realism. Human Rights Quarterly,31(3), 624-654.

Faccio's (2006) cross-country examination of different types of firm political connections motivated my choice to examine heterogeneity between connected and unconnected firms. Not only did she find that different types of political connections matter, she also found that the frequency of political connections to firms tends to be highest in countries with high levels of corruption. Transparency International ranks Kenya 136th out of 177 countries when it comes to perceived levels of public sector corruption and Kenyans cite the police, political parties, and the judiciary as the country's most corrupt institutions.[9] These indicators of poor governance suggest that Kenya's firms are likely to have political connections. Two studies on subsets of firms in Kenya confirm this.

Looking at financial institutions in Kenya during the 1990s, Brownbridge (1998) found that politically connected banks tended to be vulnerable due to insider lending to politicians and an overreliance on public sector deposits. The author relied on interviews with banking officials and industry reports to get a sense for banks' connectedness with politicians. Patel, et al. (2007) conduct a case study on a single Kenyan firm, British American Tobacco (BAT), finding that its political connections allowed it to influence policy-making in its favor. For instance, when a competing firm entered the Kenyan market, BAT orchestrated the passing of legislation that forced farmers to sell their tobacco exclusively to BAT. While limited in scope, these papers suggest that political connections have significant value for Kenyan firms—a finding this research confirms.

The work also relates to the literature on institutional development. Within the New Institutional Economics literature, there is broad agreement on proximate causes on institutional development: low income inequality and strong political competition (North, 1993; Weingast, 1995). Shirley (2008) notes, "where ruling elites had to bargain with one another or seek support from ordinary citizens, they created institutions to secure those bargains that curbed their power to expropriate. If the payoff to the ruler from abiding by these constraints was larger than the payoff from reneging, the institutions became self-enforcing and endured." Some suggest that the ICC encouraged an alliance between Kenyatta and Ruto (i.e., forced bargaining with one another), catalyzing a domestically-driven process of institution-building, in part due to distaste of the ICC intervention. In April 2011, Ephraim Maina, a member of parliament, explicitly made this argument: "We must now concentrate on enacting laws that will lead to creation of a tenable judicial mechanism and ensure it is in place by September when the Six return to The Hague. With this, the country will be able to argue for a deferral and transfer of the case home."[10]

Finally, the research complements literature on the efficacy of "importing" or adopting outside institutions. To date, this literature has examined questions related to the adoption of federalism, parliamentary systems, and legal regimes (e.g., Berkowitz, 2003; Xanthaki, 2008).[11] For example, Murg (2012) explores why adopted legal systems to enforce contracts in China, Russia, Kazakhstan, and the Ukraine failed during the 1980s

[9] Transparency International, Corruption Perceptions Index (2013).

[10] As cited in Hansen, T. O. (2011). Transitional Justice in Kenya-An Assessment of the Accountability Process in Light of Domestic Politics and Security Concerns. Cal. W. Int'l LJ, 42, 1.

[11] Bermeo, N. G. (2002). The import of institutions. Journal of Democracy, 13(2), 96-110.

and 1990s. He finds that many bypassed the new legal structure in favor of pre-reform era institutions, suggesting that path dependency is hard to break. The "import" of the ICC differs in two ways from the types of institutional adoptions studied in the literature. First, the ICC does not need to be absorbed into domestic law in order to operate; the ICC is autonomous. Second, the ICC requires that member states pre-commit to cooperating with future prosecutions, which Simmons & Danner (2010) point to as its key institutional innovation. Therefore, this research sheds light on a new form of institutional adoption.

3.3 The International Criminal Court and its Kenyan Case

3.3.1 The ICC

As a recent addition to the set of permanent supranational institutions, the ICC's primary stated role is to end impunity for perpetrators of genocide, crimes against humanity, and war crimes or aggression.[12] With 122 member-countries, the ICC has jurisdiction over a large swath of the developing world, including 34 African countries.[13] Prior to the ICC's creation, temporary tribunals such as the International Tribunal Court for Rwanda and the International Tribunal Court for Yugoslavia prosecuted such crimes. These tribunals were imposed in the aftermath of military conflict by victorious powers, with limited jurisdiction and resources. The ICC eclipsed past efforts in that it is a permanent institution with the ability to independently choose which cases come before it—in the words of legal scholars, the ICC is a much more "credible commitment" to fighting impunity.[14] Simmons & Danner (2010) note the importance of this shift in international law:

> The development of the ICC represents a stunning change of course. Not only does the ICC promise more stringent enforcement of international crimes, [the ICC] also takes away from sovereign states the discretion to decide when to initiate prosecutions—a right they have heretofore jealously guarded. Indeed, the decision by some national leaders to join the Court seems potentially to run against their own self-interest, since it is widely assumed the ICC will

[12]"War crimes are violations of the international Geneva Convention to protect prisoners of war, as well as other laws that apply to international armed conflict. Crimes against humanity include those crimes that systematically exterminate, enslave, torture, rape, and persecute victims based on political, gender, religious, ethnic, national, or cultural differences. Crimes of aggression consist of the use of armed force by a state against the territorial integrity, sovereignty, or political independence of another state, or violations of the Charter of the United Nations." (Council on Foreign Relations, 2013) Note: the court does not yet have jurisdiction over crimes of aggression; it will be able to try these cases starting 2017.

[13]Of the 122 member countries, 26 are Latin American, 34 are African, 18 are from the Asia-Pacific region, 18 are from Eastern Europe, and 25 are from Western European and North America.

[14]Cases can also be referred to it by the UN Security Council, as was done in the case of Bashir (Sudan) and Kaddafi (Libya).

> focus on prosecutions of high-level figures in countries where mass atrocities occur.[15]

The ICC is funded by member states, voluntary donations, and the United Nations (in the event that a case is referred to it by the UN Security Council).

3.3.2 Kenya & the ICC

More than a year after the post-election violence—and after Kenya's parliament twice stymied efforts to establish a local tribunal—the ICC's Chief Prosecutor, Luis Moreno-Ocampo indicated that he would bring two cases against six Kenyan leaders—coined the "Ocampo 6." Then President Kibaki and Prime Minister Odinga were cleared of any wrong-doing, but their fierce supporters, Uhuru Kenyatta and William Ruto, respectively, were implicated. Case I, which charged the accused with murder, deportation or forcible transfer of a population, rape, and persecution, was brought against:

1. Uhuru Kenyatta: named by Fortune as the richest man in Kenya, son of Kenya's founding father, and then Deputy Prime Minister and Minister of Finance;[16]
2. Francis Muthaura: a long-time civil servant, then head of the National Security Advisory committee; and
3. Mohamed Hussein Ali: then commissioner of the Kenyan police.

Case II charged the accused with murder, torture, and persecution and was brought against:

1. William Ruto: then Minister of Higher Education, Science, and Technology and chair of Odinga's political party;
2. Joshua Sang: a popular radio disc jockey with the same ethnicity of Odinga and the only non-politician/ civil servant in the ICC Kenyan cases; and
3. Henry Kosgey: then Minister of Industrialization.

Charges have subsequently been dropped or remain unconfirmed against the six accused.[17]

In response to the ICC's indictment and in an effort to avoid prosecution, Kenyatta and Ruto (with five others) joined forces in a bid for the executive office—ironically naming themselves the "G-7". Amongst others, Hansen cited the alliance as a way to obviate the

[15] Simmons, B. A., & Danner, A. (2010). Credible Commitments and the International Criminal Court. International Organization, 64(2).

[16] Following the indictment, Kenyatta stepped down as Minister of Finance, but refused to relinquish his position as Deputy Prime Minister.

[17] Charges against Muthaura were dropped the ICC because of witness non-cooperation; the prosecutor cited death, intimidation, and bribery as reasons for the failed case.

ICC's indictment: "the main objective of the G-7 coalition is to avoid accountability for the post-election violence."[18] In line with that objective, the two men hired PR firms to politicize the ICC process, making claims that their opponent, Odinga, had encouraged the ICC to target Kenyatta and Ruto to eliminate his competition. In this way, the 2013 presidential elections became a "referendum on the ICC" for many in Kenya.[19]

While Kenyatta and Ruto may have won the battle to gain control of the executive branch, the war still continues. The Bensouda 3 remain entangled in the day to day requirements of the trial, with many citing it as reason for the G-7's poor policy-making thus far. Alex Awiti, Director of the East African Institute at the Aga Khan University, attests that Kenya has "not seen real ground work to support robust growth and income inequality is still a huge challenge. The ICC has been a huge distraction."[20] Despite this, the Bensouda 3 have continued to cooperate with the proceedings. In his post-election acceptance speech Kenyatta said that he intends to continue abiding by his "international obligations."[21] Although membership in the ICC is voluntary, withdrawal takes at least a year and has no bearing on ongoing cases. To date, Kenya has not formally begun such a process despite some political posturing from the Kenyan parliament.[22]

For a full timeline of ICC-related events, see Appendix C.1.

3.4 From the ICC to the Market: Potential Channels and Mechanisms

This research employs an event study approach, exploiting variation in listed firms' abnormal returns before and after ICC "judicial" shocks. Thus, it is necessary to establish some a priori hypotheses about what mechanisms might translate ICC news into changes in listed firms' valuations. In what follows, I distinguish between the impact of the ICC at an aggregate level (i.e., an impact on Kenya's macroeconomic prospects, reflected by overall stock market performance) and at the firm-level, based on whether a firm is connected or not connected. The latter follows directly from the literature on political connections. The former is supported by Blanchard (2000), who notes that, "institutions

[18]Hansen, T. (2013). Kenya's Power-Sharing Arrangement and Its Implications for Transitional Justice. The International Journal of Human Rights. 17, 307-327.

[19]Lynch, G., & Zgonec-Rožej, M. (2013). The ICC Intervention in Kenya. Chatham House Programme Paper AFP/ILP, 1, 2013.

[20]Doya, D. (2014, March 4). Kenyan President Struggles With Growth as He Faces ICC Trial. Bloomberg. Retrieved June 2, 2014, from http://www.bloomberg.com/news/2014-03-04/kenya-president-struggles-with-growth-as-he-faces-violence-trial.html

[21]Kenyatta's acceptance speech: "To the nations of the world I give you my assurances that I and my team understand that Kenya is part of the community of nations and while as leaders we are, first and foremost, servants of the Kenyan people, we recognize and accept our international obligations and we will continue to co-operate with all nations and international institutions– in line with those obligations."

[22]In September 2013, the Kenyan parliament voted to cut ties with the ICC; however, this action had no legal bearing on Kenya's ICC membership.

also matter for short-run fluctuations." If the ICC is viewed as a supplementary institution, its shocks should have macroeconomic implications that show up in changes in the overall market index.

3.4.1 Destabilizing, Especially For Connected Firms

In the aggregate, ICC shocks can be destabilizing if they are simply a new manifestation of political uncertainty and instability shocks, which are known to have negative growth implications and depress market returns (e.g., Fosu, 2002; Brada, et al. 2006; Bernhard & Leblang, 2006; Scholtens & Steensma, 2012; Abdelbaki, 2013; Kelly, et al., 2014). An oft-heard argument is that ICC proceedings contribute to instability by creating a power vacuum within countries. Ku & Nzelibe (2006), examining international criminal tribunals (ICT) more generally, posit that when politicians are politically indispensable (as Kenyatta, richest man in the country and son of the founding President, is), "the ex ante benefits of deterrence from ICT prosecution will likely be outweighed by the ex post harms of prosecuting a spoiler—an individual whose prosecution is likely to generate local political instability. In other words, the prospect of prosecution by an ICT may sometimes exacerbate the risks of humanitarian atrocities."[23] The two also highlight a free-riding element of international courts' interventions. Countries may not undertake necessary institutional reforms if international courts like the ICC are present to deliver justice whenever atrocities occur. However, as pointed to earlier, there has been the opposite reaction in Kenya as leadership has sought to overhaul the domestic judiciary so that the ICC will drop the cases.[24]

At the firm-level, the literature on political connections suggests that connected firms should fare worse than unconnected firms given that they are at risk of losing implicit subsidies and/or preferential treatment if their political leader is removed from power (e.g. Mitton & Johnson, 2003; Acemoglu, et al., 2013).

3.4.2 Stabilizing, with an Ambiguous Impact on Connected Firms

The case for a development-enhancing effect of the ICC shocks is more tenuous, but also multi-dimensional. As referenced in the introduction, if ICC shocks positively change the behavior of the accused, then the shock can be politically stabilizing and thus have positive implications for the overall stock market's performance. Simmons & Danner (2010) argue that the ICC's pre-commitment, or "hand-tying," requirement can deter crimes against humanity and find some empirical evidence to support this claim—"for states unable to make credible promises to ramp down violence, the ICC [. . .] is associated with tentative steps toward peacemaking."[25]

[23]Ku, J., & Nzelibe, J. (2007). Do International Criminal Tribunals Deter or Exacerbate Humanitarian Atrocities. Washington University Law Quarterly, 84, 06-27.

[24]For an overview of recent Kenyan judicial reforms, see Ndungu, 2012.

[25]Simmons and Danner (2010), pg 32.

Based on the ICC prosecution's role in diminishing the "politicization of ethnicity" in Kenya—both via a reduction in the use of hate speech in media (Hirsch, 2013) and through the "forced" alliance of Kenyatta and Ruto—there is some evidence of such a mechanism. The G-7 government has also signaled a stronger anti-corruption stance relative to previous regimes. In October 2013, Kenyatta launched a site where people are encouraged to submit names of corrupt officials as well as load videos and photos of officials requesting bribes directly to the Office of the Presidency.[26] Kenyatta has also moved to "make examples" out of corrupt officials in his own offices, firing four top civil servants citing evidence of corrupt practices (Namunane, 2014).

In a February 2014 statement, Kenyatta affirmed that these actions are only the beginning: "I wish to state that the government will not tolerate corrupt public officers. The time for transformation has come and those who are not ready to change should leave and give a chance to others willing to serve. For those who are not ready to change, we shall not plead with you any more."[27] Confirming his no-tolerance policy, the Ethics and Anti-Corruption Commission is currently bringing a case against the current Central Bank Governor, who is accused of fraudulent activity associated with a $13 million USD tender. Still, many may not see the government's recent actions as benevolent; it may be that this anti-corruption movement is actually a political witch-hunt, targeted at political enemies or those who supported the ICC prosecutions.

At a more abstract level, the ICC could have a stabilizing impact if it is perceived by investors as supplementing Kenya's weak local judicial system. In this way, the proceedings (regardless of their effect on Kenyatta and Ruto's behavior) could be viewed as a "performance" of enhanced rule of law, a well-studied determinant of growth. North (1990) showed that strong judicial systems are a determinant of strong economic performance. Similarly, Barro (1996) found that rule-of-law has "substantial explanatory power for economic growth."[28] A number of scholars have since confirmed Barro and North's findings (e.g. Knack & Keefer, 1997; Acemoglu & Robinson, 2001; Mauro, 2002). Under this logic, the end of impunity in Kenya, signaled by the ICC's pursuit of justice, should have a positive impact on the economy's prospects, reflected in a positive overall effect on firm valuations. Legal scholars too have pointed out the performative benefits of international tribunals. Discussing the Nuremberg trial, Douglas (2005) notes,

> The trial was understood as an exercise in the reconstitution of the law, an act staged not simply to punish extreme cases but to demonstrate visibly the power of the law to submit the most horrific outrages to its sober ministrations. In this regard, the trial was to serve as a spectacle of legality, making visible

[26] See http://www.president.go.ke/en/category/corruption.php

[27] Namunane, B. (2014, February 11). President Uhuru Kenyatta plans crackdown on corrupt officials. Daily Nation. Retrieved June 2, 2014, from http://www.nation.co.ke/news/politics/State-House-intrigue-over-corruption-ring/-/1064/2202784/-/1241c9m/-/index.html

[28] Barro,R. 1996. "Determinants of Economic Growth: A Cross-Country Empirical Study." NBER Working Papers 5698. National Bureau of Economic Research, Inc.

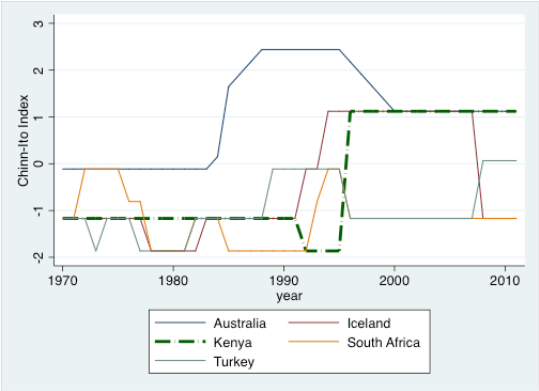

Figure 3.1: Chinn-Ito Index of Financial Openness

> both the crimes of the Germans and the sweeping neutral authority of the rule of law.

Even if the ICC has a stabilizing impact at an aggregate level, politically-connected firms may still fare worse than non-politically connected firms. Suppose the ICC prosecution leads to a lower incidence of corruption and a resulting loss in implicit subsidies/political favors for connected firms. Even if a politically-connected firm's politician retains power, the market environment will have shifted in favor of unconnected firms who now face a more equitable playing field. However, this outcome is not guaranteed. ICC prosecution could lead to "peace," but no lasting reform efforts as the accused may do the minimum required to keep order without upsetting the status quo. That is, Kenyatta and Ruto may no longer engage in hate speech, but they may still offer their firm connections political favors. Thus, the relative impact on politically-connected and unconnected firms is ambiguous when the ICC is expected to have a stabilizing effect at the aggregate level.

3.5 Data & Constructed Measures

To examine what mechanisms seem to be operating in the Kenyan case, the research employs a standard event study methodology, exploiting stock market variation in the pre and post-shock periods in the aggregate and across connected and unconnected firms. A liberalized financial market helps relevant news translate into large stock market fluctuations. Following a liberalization campaign in the mid-1990s, Kenya rose from being amongst the 10 percent most closed economies to among the most open developing economies (O'Connell, et al., 2010). The openness of Kenya's capital account is comparable to Australia and Turkey based on the Chinn-Ito index (see Figure 3.1), a de jure measure of financial openness where higher values indicate higher financial openness.

Stock market event studies also require that Fama's semi-strong efficient market hypothesis (EMH) hold—i.e., that stock prices will respond to newly public and relevant

Table 3.1: NSE and JSE Comparative Descriptive Statistics, 2013

	Market Capitalization (billions of USD)	Equity Turnover (billions of USD)	Listed Firms	Foreign Ownership
NSE	23	1.78	49	∼50%
JSE	1,007	369.89	386	37%

information (and with the appropriate magnitude). In the case of the Nairobi Stock Exchange (NSE), weak form efficiency holds (e.g., Magnusson & Wydick, 2002; Jefferis & Smith, 2005). However, semi-strong efficiency has not yet been confirmed in a formal analysis (Kakiya, et al., 2013). Still, according to Stephen, et al. (2013) the market is improving in efficiency overtime with the introduction of automated trading in the mid-2000s (as well as a number of high profile IPOs that more than doubled the number of investors trading on the exchange). Thus, the NSE is likely approaching semi-strong efficiency as it continues reforming and listing new firms (also, the results of this research lend support to this assertion in that we see significant effects from "new" ICC news). Table 3.1 presents some comparative descriptive statistics for the Nairobi and Johannesburg stock exchanges (NSE and JSE, respectively).

Daily data for NSE firm stock prices, as well as market indices, were sourced from DataStream. The NSE is the 4th largest exchange in Africa and over most of the sample period the NSE had a transaction time of +4 days.[29]

3.5.1 Abnormal Returns

At the firm-level, the market model was used to estimate firm abnormal returns, using 2004 data for the estimation window. 2004 was chosen as the estimation window because the period between 2005 and 2008 featured a number of major shocks to the Kenyan economy including ongoing tensions with Somalia, a major drought that affected 10 million people, a Kenyan military plane crash that killed several high-ranking politicians, and the violent rejection of a draft constitution. Comparatively, 2004 was a stable year. The sample period covers 2008 through March of 2014.

Equation 3.1 was estimated to calculate the normal returns parameters over the estimation window, where $R_{it}$is the period t return for firm i and $R_{mt}$is the period t return for the market portfolio. ε_{it} is a mean zero disturbance term and α_i, β_i, $\sigma^2_{\varepsilon_i}$ are estimated parameters.

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it} \tag{3.1}$$

$$E\left[\varepsilon_{it}\right] = 0 \tag{3.2}$$

[29] At the time it was +3 days, with the expectation that it would fall to +2 days by the end of 2015.

$$Var\left[\varepsilon_{it}\right] = \sigma^2_{\varepsilon_i} \tag{3.3}$$

Abnormal returns, equation 3.4 for firm i on date t for the sample period were then calculated as the difference between actual returns, R_{it}, on date t for firm i and the normal returns, using the parameter estimates from 3.1 and R_{mt}, the period t return of the market portfolio.

$$AR_{it} = \varepsilon^*_{it} = R_{it} - \hat{\alpha}_i - \hat{\beta}_i R_{mt} \tag{3.4}$$

At the market level, for the NSE20 Index and the NSE All Share Index where noted, a constant-mean-return model was employed to calculate market abnormal returns, equation 3.8. Here a constant mean from the estimation window for the market returns is subtracted from the actual market returns during the sample period.

$$R_{mt} = \mu + \epsilon_t \tag{3.5}$$

$$E\left[\epsilon_t\right] = 0 \tag{3.6}$$

$$Var\left[\epsilon_t\right] = \sigma^2_{\epsilon_i} \tag{3.7}$$

$$Market\ AR_t = \epsilon^*_t = R_{mt} - \hat{\mu} \tag{3.8}$$

From a possible sample of 49 firms, 7 were dropped due to no data availability during the 2004 estimation window or due to low within-month variability in firm returns of less than 1 percent over the sample period.

3.5.2 Events and Event Windows

In consultation with a team of four international human rights legal experts, including one that worked directly with former ICC chief prosecutor Ocampo in 2010, twenty-one dates were selected as "strong" adverse ICC shocks and eleven dates were identified as "weak" ICC shocks, where strength refers to the strength of the institution and process.[30] Weak shocks are those that put the ICC's ability to successfully try the Kenyan cases into question (e.g., in December 2014, Kenyatta's case was finally dismissed by the ICC due to a lack of evidence). For a list of each group, see Appendix C.2. For the core empirical analysis, I will focus on the "strong" adverse ICC dates.

Given the +4 day NSE transaction period over most of the sample, t=[0,3] will be considered a lead-in/leaked news period, where t=0 is the announcement date. Three event windows are examined: one day shocks (t=[0,4]), seven day shocks (t=[0,10]), and ten day shocks (t=[0,13]). The event windows are capped at ten days to avoid

[30]For biographies on the consultants, see Appendix C.5.

too much overlap between the shocks. Moreover, given that abnormal returns are zero in expectation, a thirty or sixty day window should produce statistically insignificant results. This was found to be the case in almost all of the specifications discussed below.[31]

3.5.3 Political Connections

I constructed three measures of connectedness using hand-coded board membership from published annual reports of listed firms for the fiscal years of 2006/7 and 2012/13—the beginning and end of the period covered. There are over 500 unique board members spread across 49 firms. Board memberships are often used to pick up political connections in the literature (e.g. Hillman, 2005; Goldman, et al., 2009; Chen, et al., 2011). They tend to be sticky across the Kenyan sample, especially amongst smaller firms. Using biographies within the reports, for each board member I coded high-ranking (e.g. permanent secretary or minister) positions in government and other board affiliations.

The first and most generic measure of connection is government connections. Government connections, whether current or past, of a firm's board members may give that firm a direct line to high-ranking officials currently in power. This is the weakest type of connectedness to the ICC accused.

The second measure is ICC-linked board member connections.[32] With the exception of Kenyatta, none of the Ocampo 6 owned substantial businesses, as most were career civil servants and politicians. Kenyatta's family, on the other hand, owns no less than 10 companies including a commercial bank; the country's largest dairy farm; a media company with 4 radio stations, a TV station, and a newspaper; a hotel chain; and farms on thousands of acres of land. Unfortunately, the Kenyatta-family companies (largely managed by his brother Muhoho Kenyatta) are not listed on the NSE. However, some of them have corporate boards. The ICC-linked board member connection picks up connections between Kenyatta-family company boards and listed firm boards. Listed firms with board members that also serve on boards with Muhuho have an indirect, but close link to the President's office.

The third, and closest, measure of connection is ICC-linked advisor connections. Kenyatta's close advisors include his formal appointments in addition to his personal lawyer, the former head of the Treasury, and other close confidants that were repeatedly referenced as part of his "inner circle" the literature and media. As Kenyatta's key influencers, companies with these individuals as board members have a direct line to the President.

Table 3.2 provides a summary of connections by type.

One question is whether investors know about these relationships. Given the small size of the Kenyan market, the availability of the annual reports online, and economic

[31] Thirty and sixty day results available by the author upon request.

[32] 2012/13 ICC Board connections feature a number of new additions due to the recent expansion on Kenyatta-family board of the Commercial Bank of Africa. These individuals were included as their appointment is likely related to long-standing relationships between the Kenyatta-family and these individuals that were not formalized by board membership, but were nevertheless there.

Table 3.2: Number of Firms by Connectivity of Type

	Not Connected	Connected
Any Link	19	23
Government	20	22
Any ICC Link	30	12
ICC Board	38	4
ICC Advisor	34	8

activity's geographic concentration in the capital city of Nairobi, it is likely that investors are aware of who is and is not linked to the accused. For instance, when Joseph Kinyua was formally appointed as Kenyatta's advisor in 2013, one MP posed the following question in conversation with a reporter: "Don't you think the President was looking for a way to reward a trusted personal friend?"[33]

Moreover, since the ICC proceedings began, there have been several media investigations attempting to map out Kenyatta's financial network. However, this has proved particularly difficult given the secrecy of the family as an article from Business Daily Africa noted in November 2013: "The full extent of the [Kenyatta] business dynasty... is still a closely guarded secret known only to the family, top lawyers and the elite investors with whom they do business."[34]

Given the presence of "known" unknowns in my connections sample, the coded number of ICC board member and advisor connections is likely to be too low. Ideally one would want to supplement such measures with interviews in Kenya with local business leaders.

Table 3.3 presents the correlations among the relations.

[33]Shiundu, A. (2013, October 23). Joseph Kinyua's appointment to State House unprocedural, MPs say. The Standard. Retrieved June 2, 2014, from http://www.standardmedia.co.ke/mobile/?articleID=2000096080&story_title=mps-kinyua-s-appointment-to-state-house-unprocedural

[34]Juma, V. (2013, November 11). Kenyatta business empire goes into expansion drive. Business Daily Africa. Retrieved March 5, 2014, from http://mobile.businessdailyafrica.com/Corporate-News/Kenyatta-business-empire-goes-into-expansion-drive/-/1144450/2069704/-/format/xhtml/item/2/-/j31djp/-/index.html

Table 3.3: Correlations Between Political Connection Measures

	Any Link	Government	Any ICC	ICC Board	ICC Advisor
Any Link	1.00				
Government	0.95	1.00			
Any ICC	0.57	0.50	1.00		
ICC Board	0.29	0.15	0.51	1.00	
ICC Advisor	0.44	0.46	0.77	-0.16	1.00

Notably, amongst ICC board and advisor connections, firms tend to have either a formal board member link to a Kenyatta firm or an advisor link, but not both. This will prove useful in the difference-in-differences specifications discussed below.

3.5.4 Firm Data & Additional Controls

I also hand-coded a dataset for annual key firm data from 2006 to 2012 using the annual reports of listed firms. Table 3.4 presents the summary statistics, by political connection, for the year 2012. The statistics shaded in light grey are for unconnected firms; those in the darker grey are for connected firms. Statistics for assets, profit, market capitalization, and investment are in thousands of Kenyan shillings. The final column gives the p-values of two-sided t-tests for whether the means between the two groups are significantly different from zero, allowing for unequal variances between the groups.

Table 3.4: Summary Statistics, by Connection Type

Any Link	**Obs**	**Means, 0**	**Std Dev, 0**	**Obs**	**Means, 1**	**Std Dev, 1**	**P-value for hypothesis that diff not equal to zero**
Assets	20	29090584.15	50345670.99	22	60575404.27	88268756.78	0.160231867
Return on Equity	6	0.191666667	0.079351539	14	0.854285714	2.290709377	0.299316373
Profit	20	880130.95	2719311.83	22	2708510.045	3596890.257	0.069302896
Market Capitalization	13	6145012.077	10330959.21	7	23588722.71	49376200.93	0.389648554
Current Ratio	11	3.128181818	5.25224108	8	1.56625	1.142815289	0.35926727
Return on Assets	20	0.052935628	0.177563947	22	0.081263937	0.113581553	0.546769823
Investment	12	405536	327858.5948	14	3519851.429	6962873.96	0.11840712

Government	**Obs**	**Means, 0**	**Std Dev, 0**	**Obs**	**Means, 1**	**Std Dev, 1**	**P-value for hypothesis that diff not equal to zero**
Assets	22	37653076.45	65719520.4	24	62373109.29	87871718.27	0.283583036
Return on Equity	7	0.185714286	0.074129873	15	0.810666667	2.213837866	0.293179826
Profit	22	1383392.182	3521228.01	24	2603280.625	3479159.919	0.24423095
Market Capitalization	15	11378111.27	19440203.69	8	23003371	45743450.68	0.510947368
Current Ratio	11	3.128181818	5.25224108	9	1.557777778	1.069307928	0.354098703
Return on Assets	22	0.0545165	0.169082833	24	0.077708126	0.109377865	0.587848423
Investment	13	574264	684567.343	15	3861177.067	6838580.937	0.084921705

Any ICC	**Obs**	**Means, 0**	**Std Dev, 0**	**Obs**	**Means, 1**	**Std Dev, 1**	**P-value for hypothesis that diff not equal to zero**
Assets	30	27735340.33	46680965.56	12	90200863.92	106823300.5	0.073253093
Return on Equity	14	0.877142857	2.281414637	6	0.138333333	0.164610652	0.249411965
Profit	30	1660532.933	2703907.253	12	2281154.333	4591358.461	0.667427732
Market Capitalization	15	13390851.2	34896805.58	5	8828689.6	6446302.402	0.636001397
Current Ratio	13	2.904615385	4.825456136	6	1.53	1.350881194	0.35700165
Return on Assets	30	0.087087137	0.156924507	12	0.019492089	0.106632765	0.118502067
Investment	17	831572.2941	1572005.009	9	4445291.444	8497995.36	0.240494804

ICC Advisors	**Obs**	**Means, 0**	**Std Dev, 0**	**Obs**	**Means, 1**	**Std Dev, 1**	**P-value for hypothesis that diff not equal to zero**
Assets	34	31339231.29	52737634.61	8	106117089.1	115985379.3	0.114314839
Return on Equity	16	0.7975	2.135114361	4	0.0875	0.180069431	0.208389131
Profit	34	1564107	3103000.687	8	3001275.25	4072373.464	0.373557315
Market Capitalization	18	12985371.11	31803381.25	2	5634768	1138113.82	0.342963286
Current Ratio	16	2.729375	4.392855516	3	1.09	0.165227116	0.157392739
Return on Assets	34	0.079236935	0.15987272	8	0.019057922	0.040467453	0.058794513
Investment	19	795082.5263	1491220.179	7	5576826.286	9461524.437	0.230682409

ICC Board Members	**Obs**	**Means, 0**	**Std Dev, 0**	**Obs**	**Means, 1**	**Std Dev, 1**	**P-value for hypothesis that diff not equal to zero**
Assets	38	44236761.13	72813131.42	4	58368413.5	91788354.26	0.782818509
Return on Equity	18	0.701666667	2.024849304	2	0.24	0.084852814	0.350269201
Profit	38	1942794.474	3028982.069	4	840912.5	5869438.902	0.734519629
Market Capitalization	17	12478370.82	32745680.03	3	10957970.67	8090740.745	0.871165908
Current Ratio	16	2.564375	4.377984268	3	1.97	1.988491891	0.719677492
Return on Assets	38	0.072765197	0.142831061	4	0.020360423	0.194600605	0.633043457
Investment	24	2215604.708	5470517.084	2	484919.5	522535.6478	0.155218075

Investment is higher for government-connected firms; assets are higher for ICC-linked firms; and return on assets is higher for non-ICC advisor firms. In all other cases, the differences between key measures are not statistically different from zero.

Additional control data for the Kenyan schilling/USD exchange rate, the S&P 500 Index, and the FTSE Index were sourced from Global Financial Database. In the specifications below, I opt to use the S&P 500 Index as it has a higher correlation with the Kenyan NSE20 Index and since inclusion of both the FTSE and S&P 500 could bias results due to their high correlation with one another. Finally, I also coded a variable for key Kenyan news events (with a ten day event window). For the list of Kenyan news events, see Appendix C.3.

3.6 Empirical Results

3.6.1 Overall Market

Table 3.5 reports the results from a time-series OLS estimation of the All Share Index abnormal returns on the three event windows and controls:

$$Market\ AR_t = \alpha + \beta_1 Shock_t + \phi' x_t + \varepsilon_t \tag{3.9}$$

where α is a constant; $Shock_t$ is either a one, seven, or ten day shock indicator variable; x_t are a set of time-varying controls; and ε_t is a mean zero disturbance term.

Columns (1) – (3) control for S&P 500 market returns and the Kenyan news dummy, while (4) – (6) add the percentage change in the Kenyan schilling/USD exchange rate as a control.[35] Robust standard errors are used to correct for heteroskedasticity and serial correlation.

Table 3.5: All Share Abnormal Market Return Results

	(1) Market AR	(2) Market AR	(3) Market AR	(4) Market AR	(5) Market AR	(6) Market AR
One Day Shock	-0.00145			-0.00114		
	(0.00107)			(0.00101)		
Seven Day Shock		-0.00239***			-0.00228***	
		(0.000701)			(0.000678)	
Ten Day Shock			-0.00150**			-0.00151**
			(0.000742)			(0.000726)
Exchange Rate % Change				-0.291***	-0.290***	-0.293***
				(0.0597)	(0.0595)	(0.0595)
S&P 500 Return	0.00270	0.00323	0.00394	-0.00263	-0.00221	-0.00155
	(0.0264)	(0.0261)	(0.0263)	(0.0251)	(0.0247)	(0.0249)
Kenyan News	-0.000103	0.000107	-0.0000050	-0.000366	-0.000148	-0.000247
	(0.00129)	(0.00128)	(0.00129)	(0.00124)	(0.00123)	(0.00124)
Constant	-0.000217	-0.0000074	-0.000072	-0.00019	0.0000254	-0.0000249
	(0.000244)	(0.000255)	(0.000255)	(0.000241)	(0.000250	(0.000244)
Observations	1500	1500	1500	1500	1500	1500
Adj. R-squared	0.000	0.006	0.002	0.038	0.044	0.041

Notes: Robust standard errors in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

[35] While some may consider including exchange rate fluctuations to be a "bad control" since the shocks could presumably have some impact on the exchange rate, there is little evidence of this. When regressing the exchange rate change on the shock variables, there were no significant coefficients.

Over the one day shock period (column (1)) the average market abnormal daily returns fall by .15 percentage points, however this result is not significant.[36] For seven day event windows under column (2), adverse ICC shocks generate a drop of .24 percentage points in daily abnormal returns, significant at the 1% level. The effect of the shocks moderates as the event period lengthens to a ten day window, but is still significant at the 5% level.[37]

The results are robust to inclusion of exchange rate changes as a control in columns (4)-(6).[38] The small magnitudes of the coefficients are not uncommon in the literature (Campbell, Lo, & MacKinlay, 1997; Wong, 2002) and are quite large if annualized. Note that, in line with expectations, the constant terms are not significantly different from zero—that is, on average abnormal returns are zero over the sample period.

To check that there are no large outliers in certain event dates driving this result, Appendix C.4 plots the ten day weighted average cumulative abnormal returns by event date.[39] For 2/3 of the 21 ICC shocks, cumulative abnormal returns were negative ten days after the transaction period. On average, the overall market results suggest the ICC is destabilizing for the stock market as a whole.

3.6.2 Across Connected and Unconnected Firms

To examine heterogeneity between connected and unconnected firms, I employ a difference-in-differences specification.

$$AR_{it} = \alpha + \gamma_i + \beta_1 Shock_t + \beta_2 \left(Shock_t \times Connection_i\right) + \varepsilon_{it} \tag{3.10}$$

where α is a constant; γ_i are firm fixed effects; $Shock_t$ is either a one, seven, or ten day shock indicator variable; $Connection_i$ is an indicator for whether the firm is connected or non-connected to any link, government, any ICC links, an ICC-Board Member link, or and ICC-Advisor link; and ε_{it} is a mean zero disturbance term.[40]

[36] The lack of significance for the one day shocks also supports literature examining whether the EFM holds for Kenya—i.e., that news is not immediately reflected in stock prices.

[37] Given that the market averages for 2004 were so small, these results are nearly identical those of the market returns for the NSE20 and All Share Index.

[38] I also ran specification including month and week fixed effects in place of exchange rate changes, S&P 500, and Kenyan news. The coefficients on the shocks are robust to the inclusion of month fixed effects, but not week fixed effects.

[39] Assets were used as weights to construct the average abnormal return for the market, which are then summed over the ten day windows.

[40] One could include a vector of time-varying firm-observables, however I exclude them here for three reasons. Primarily, the firm observables are at an annual frequency, making them quite coarse. On a related point, high investment, high profit, and high return on asset firms are consistently high, suggesting that the firm dummies should absorb these largely time-invariant effects. Third, the mentioned observables could be classed as "bad controls," in that one would expect the shocks to have some impact on them (i.e. they could be outcome variables) (Svensson, 1998). In addition, the more traditional diff-in-diff approach with multiple groups and shocks is to include both date and firm fixed effects. Implementing this specification has no influence on the diff-in-diff coefficients and standard errors, as seen in Appendix C.7.

Of primary interest will be β_1, which estimates the average impact of ICC shocks on abnormal returns for non-connected firms; $\beta_1 + \beta_2$, which tells us the average impact of ICC shocks for connected firms; and β_2, the difference-in-difference estimator, which tells us the average impact of ICC shocks for connected firms minus the average impact of ICC shocks on non-connected firms.

With respect to inference, as Cameron and Trivedi (2005) note, "NT correlated observations have less information than NT independent observations."[41] To ensure the inference is robust to serial correlation in residuals within firms as well as cross-sectional correlation by date, the standard errors are clustered at both the firm and date level in line with recommendations from Cameron & Trivedi (2005), Petersen (2009), and Bertrand, et al. (2004).[42]

Table 3.6 presents the estimation results for the one (Columns (1)-(5)), seven (Columns (6)-(10), and ten day (Columns (11)-(15)) event windows across different types of connections. Across the specifications, the coefficients on the shock indicators are largely insignificant, suggesting that the ICC shocks do not affect unconnected firms' abnormal returns. This indicates that the destabilizing outcome in the overall market results are driven by connected firms, rather than unconnected firms. Firms with "any link" tend not to experience a differential impact on average abnormal return outcomes, illustrating that differences in types of connections will matter. For the one day shock estimations, only the coefficient on the diff-in-diff estimator for government connections is significant. Here, the average treatment effect of one day ICC shocks on government-connected vs. unconnected firms is a fall in abnormal returns of .14 percentage points. While the other diff-in-diff estimators are insignificant, they are all negative.

For the seven day shock estimations, the average treatment effect for government-connected firms becomes insignificant, but retains its sign while decreasing substantially in magnitude. The average treatment effect for firms with any ICC link is now significant at the 1% level. The ICC Board and ICC Advisor interaction terms reveal that that the ICC Board connections are driving this outcome, with average treatment effects of -.35 and -.19 percentage points, respectively. The ten day shock estimations confirm the disproportionate impact on ICC Board firms, with the average treatment effect coefficient for ICC Advisor firms becoming insignificant.[43]

[41] Cameron and Trivedi (2005).

[42] Improved inference based on clustered standard errors requires that the number of clusters trends towards infinity. However, there is no set number of clusters considered to be too small. Based on Patrick Kline's advice, clusters over 20 seem to be sufficient. Others (e.g. Angrist & Pischke, 2008) suggest 50. In these estimations, I assume 42 is close enough to 50 to trust the standard errors.

[43] Notably, when running similar specifications using the "weak" ICC dates (i.e. signals that the ICC's intervention was unlikely to be successful or, eventually, had failed), there is an asymmetry in the response of investors. Almost without exception, none of the shock or shock-connection interaction terms are significant. This suggests that investors only respond to "strong" ICC news, not "weak" news. Finally, I can run similar specification on firm returns, rather than abnormal returns. The results are similar, but slightly weaker.

Table 3.6: Abnormal Returns, Difference-in-Difference Results

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR	(7) AR	(8) AR	(9) AR	(10) AR	(11) AR	(12) AR	(13) AR	(14) AR	(15) AR
One Day Shock	-0.000376	-0.000338	-0.000672	-0.000878	-0.000938										
	(0.000718)	(0.000703)	(0.000627)	(0.000587)	(0.000622)										
One Day Shock x Any Link	-0.00127														
	(0.000817)														
One Day Shock x Gov't		-0.00140*													
		(0.000826)													
One Day Shock x Any ICC			-0.00141												
			(0.000884)												
One Day Shock x ICC Board				-0.00206											
				(0.00160)											
One Day Shock x ICC Advisor					-0.000712										
					(0.000803)										
Seven Day Shock						-0.000844	-0.000921*	-0.000191	-0.000699	-0.000663					
						(0.000541)	(0.000526)	(0.000859)	(0.000738)	(0.000815)					
Seven Day Shock x Any Link						-0.000345									
						(0.00118)									
Seven Day Shock x Gov't							-0.000215								
							(0.00122)								
Seven Day Shock x Any ICC								-0.00295***							
								(0.000986)							
Seven Day Shock x ICC Board									-0.00351***						
									(0.00132)						
Seven Day Shock x ICC Advisor										-0.00194**					
										(0.000926)					
Ten Day Shock											-0.000979	-0.00104*	-0.000260	-0.000596	-0.000643
											(0.000618)	(0.000591)	(0.000791)	(0.000680)	(0.000750)
Ten Day Shock x Any Link											0.000191				
											(0.00108)				
Ten Day Shock x Gov't												0.000312			
												(0.00111)			
Ten Day Shock x Any ICC													-0.00215**		
													(0.000911)		
Ten Day Shock x ICC Board														-0.00292**	
														(0.00137)	
Ten Day Shock x ICC Advisor															-0.00121
															(0.000819)
Observations	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830
adj. R^2	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001

Notes: All estimations include firm fixed effects and constants (not shown). Standard errors clustered by date and firm are in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Since the correlation in firm connections for ICC advisor and ICC board firms is so low, one can go a step further and control for the respective average treatment effects for different types of ICC connected firms. The results, shown in Table 3.7, confirm the above findings: the negative average treatment effect for ICC board connected firms is larger in magnitude than the ICC advisor connected firms. This complements Faccio's (2006) finding that there are differential impacts on the value of connections between different types of connections.

Table 3.7: Abnormal Return, Difference-in-Difference Results, Comparing ICC Board vs. ICC Advisor Firms

	(1) AR	(2) AR	(3) AR
One Day Shock	-0.000672		
	(0.000627)		
One Day Shock x ICC Board	-0.00226		
	(0.00164)		
One Day Shock x ICC Advisor	-0.000978		
	(0.000821)		
Seven Day Shock		-0.000191	
		(0.000859)	
Seven Day Shock x ICC Board		-0.00402***	
		(0.00139)	
Seven Day Shock x ICC Advisor		-0.00241**	
		(0.000968)	
Ten Day Shock			-0.000260
			(0.000791)
Ten Day Shock x ICC Board			-0.00326**
			(0.00143)
Ten Day Shock x ICC Advisor			-0.00160*
			(0.000854)
Observations	67830	67830	67830
adj. R^2	-0.001	-0.001	-0.001

Notes: All estimations include firm fixed effects and constants (not shown). Standard errors clustered by date and firm are in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

The overall insignificance of government connections relative to unconnected firms is intuitive. Recall that government connections for firms indicate whether a firm has past or current high-ranking government officials on its board. Many of these officials are career civil servants. Not only do such civil servants tend to outlast political regime changes, which is particularly true in developing markets where human capital associated with civil service is scarce, the high-rank of these board members indicates that these individuals have outlasted many regime changes. This suggests that they do not favor particular politicians and, thus, are unlikely to get favors in return. Another explanation is given by Faccio (2006). She finds that the appointment of politicians to boards is of insignificant value for those firms, stating that the "benefits do not outweigh the costs" of these types of connections. In the Kenyan setting of ICC shocks, she might argue that government-connected firms are at risk of losing subsidies, but may also gain due to a decline in

extractive behavior on the part of such individuals—resulting in a neutral/insignificant value for this type of connection.

The fact that ICC shocks generate relatively negative abnormal returns for ICC-linked firms fits with the hypothesis that firms with close connections to the accused will fare worse than unconnected firms due to the heightened risk that they will lose implicit subsidies and preferential treatment. This result is also in line with Fisman's (2001) research.

The larger effects of an ICC board connection relative to an ICC advisor connection contrast with the expectation that ICC advisors should have particularly negative outcomes because of their more direct linkage with the accused. This contradictory finding could be associated with investors' knowledge of these associations. It was particularly difficult to identify ICC-linked advisors, as most were not "formally" associated with Kenyatta until his appointment of advisors after the 2013 election.[44] Any business linkages he may have had with them prior to their appointments were covert. In December 2013, the current ICC chief prosecutor remarked that all attempts to access Kenyatta's financial records had been met with "obfuscation and intransigence."[45]

If investors (˜50 percent international) were relying on easily accessible information for connections to the accused over the course of the 2008 to 2014 sample, they were more likely to pick up the ICC board connections and divest accordingly from them. In line with this narrative, one would then expect to see increasingly negative outcomes for ICC advisor firms associated with adverse ICC shocks in the post-election period.[46] Another implication of these results is that government's refusal to share the records may have lessened the negative impact of the ICC process for the Kenyan economy.

Thus far, the estimations have only examined daily average abnormal returns and, while promising, there is considerable volatility in abnormal returns following the start of the event dates. Over a period of 4 to 10 days the connected firms' abnormal returns, while on average being negative, could sum to rather small cumulative magnitudes as investors learn more about the implications of a particular ICC announcement.[47] To get a sense for the cumulative effects of the shocks on connected and unconnected firms, I employ the Acemoglu, et al. (2013) cross-firm specification (adjusted to allow for multiple events):

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[44]Many of the advisors were identified over the course of an initial botched attempt at picking up connections via news articles. Over the course weeks reading through Kenyan newspaper articles I continued to come across individuals that seemed to be constantly linked to the Ocampo 6—particularly for Kenyatta and Ruto. I included these individuals as part of Kenyatta's informal advisors.

[45]Associated Press. (2013, December 2). Kenya 'refuses to give up Kenyatta records'. New Vision, Uganda. Retrieved June 3, 2014, from http://www.newvision.co.ug/news/650106-kenya-refuses-to-give-up-kenyatta-records.html%20target=

[46]

[47]Part of the reason that I enlisted the help of legal experts is because it is hard to distinguish what legal announcements are meaningful. For instance, how does one distinguish between "a request to issue a summons" versus "a vote on issuing summons"?

$$CAR_{it} = \alpha + \beta Connection_i + \phi' x_{it} + \varepsilon_{it} \tag{3.11}$$

where CAR_{it} is the cumulative abnormal returns for a one, seven, or ten day shock for firm i on event date t; $Connection_i$ is a time-invariant indicator for whether a firm is connected to government, an ICC Board, or an ICC Advisor; $x_{it}$are a set of firm, time-varying controls; and ε_{it} is a mean zero disturbance term.

Based on the summary statistics, return on assets (a profitability indicator) and the log of assets (a size indicator) were used as time-varying controls for these specifications.[48]

As referenced before, there may be within-firm and within-date residual correlation. However, here there is less expectation of within-firm serial correlation given that the CARs are calculated for 21 events across a six year period. I confirm this using Petersen's (2008) algorithm to assess whether date or time correlation drive bias in standard errors.[49] The result of this exercise indicates that date clustering is most appropriate. Still, a set of 21 clusters is far below the 50-cluster standard suggested by Angrist & Pischke (2008).

There are several fixes for too few clusters suggested by the literature; three are employed in this paper. The first is to use a t-distribution with 20 degrees of freedom (clusters-1) to assess significance, as suggested by Bell and McCraffy (2002). Simulations by Cameron, Gelbach, and Miller (2008) indicate that this inference adjustment works reasonably well. Second, I use the 2-step Fama-McBeth approach suggested by Cochrane, 2001, which corrects for cross-sectional correlation. The procedure estimates T cross-sectional regressions and averages over them to produce the coefficient estimates and corrected standard errors.[50] Finally, I estimate the standard errors using block bootstrapping clustered on event date. All three approaches yield similar inference outcomes and the estimations for the Fama-McBeth and bootstrapped standard errors can be found in Appendix C.6.

[48] As in the above analyses, the issue of bad controls could be present, biasing the coefficient estimates. An estimation without the firm controls produced higher estimates, but no major changes in significance. The correlation between return on assets and the log of assets is -.27.

[49] Specifically, Petersen suggests comparing White-robust standard error regressions with regressions based on firm and date clusters to establish whether a firm or date effect tends to dominate. Relative to the White-robust specifications, clustering on date had an influence on significance, whereas clustering on firm had no influence on significance of coefficients, suggesting that there is a larger time effect in this date (i.e. larger standard errors and that $Corr(\varepsilon_{it}, \varepsilon_{kt}) \neq 0$).

[50] Skoulakis (2006) notes, that "while the same idea is used in the analysis of both factor models and panel data sets, there are important differences between the two cases. In the context of a factor model, the regressors are the betas (or factor loadings) which are unknown, and thus have to be estimated, and are typically assumed to be time-invariant. The fact that the betas have to be estimated, using time series regressions, gives rise to the well-known error-in-variables problem. On the contrary, in the context of panel data, the regressors are time-varying but directly observable and, thus, there is no error-in-variables problem. The main consideration in the analysis of panel data is to properly take into account the cross-sectional and serial correlations. [U]nder the assumption that the explanatory variables do not vary with time, the FM procedure is essentially equivalent to using OLS. However, the assumption of time-invariant regressors is extremely restrictive and rather unrealistic from an application point of view."

Table 3.8 presents the estimates for the cumulative abnormal return panels. The results are shaded by connection type—columns (1)-(3) for government; (4)-(6) for ICC board links; and (7)-(9) for ICC advisor links. The positive coefficients on government show that government-connected firms experience larger cumulative abnormal returns relative to non-government connected firms—almost 4 percentage points higher over a ten day window (Column(3)). At first glance, this is surprising given the abnormal return results above. However, the coefficient estimates on the ICC board connections uncover the source of this outcome. ICC board firms—whose correlation with government connections is .15—experienced large and growing declines in cumulative abnormal returns following the ICC shocks. At the one day window (Column(4)), board connected firms fare 4 percentage points worse than non-board connected firms; by the ten day window (Column (6)) the gap is 12 percentage points. The coefficients on the ICC advisor estimates are significant at the seven and ten day window, but far smaller than the ICC board estimates. In short, the negative revaluations of ICC board connected firms are the key drivers of the empirical results. These CAR estimation results support the "news" hypothesis shared above; investors seemed far more aware of ICC board member connections and responded accordingly to negative news of the ICC shocks.

Table 3.8: Cumulative Abnormal Return Panel Results, Clustering by Date

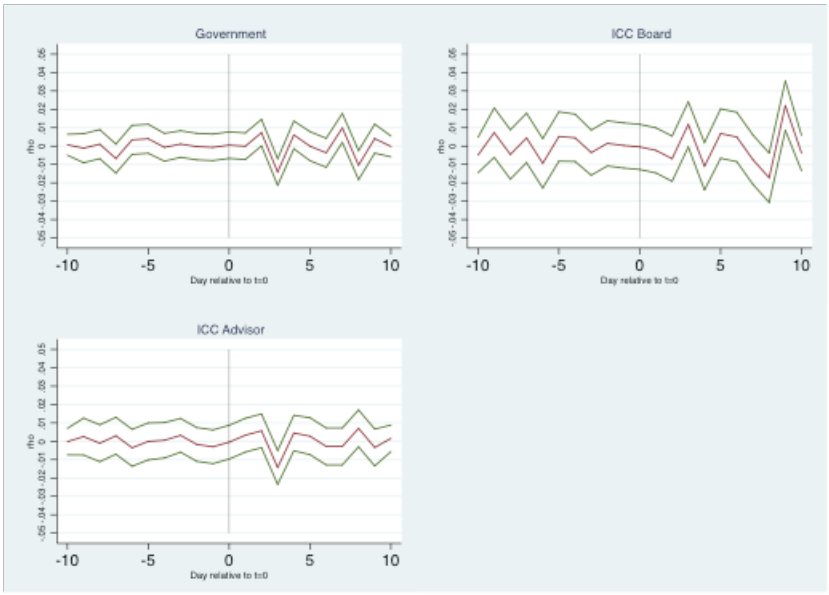
	(1) CAR One	(2) CAR Seven	(3) CAR Ten	(4) CAR One	(5) CAR Seven	(6) CAR Ten	(7) CAR One	(8) CAR Seven	(9) CAR Ten
Govt	0.00515	0.0319*	0.0394**						
	(0.00343)	(0.0171)	(0.0184)						
ICC Board				-0.0406***	-0.0979***	-0.120***			
				(0.00575)	(0.00968)	(0.00993)			
ICC Advisor							-0.00438	-0.0169*	-0.0197***
							(0.00415)	(0.00962)	(0.00643)
ln(Assets)	-0.00255**	-0.00963**	-0.00912*	-0.00171*	-0.00559**	-0.00413	-0.00155	-0.00453*	-0.00294
	(0.00101)	(0.00415)	(0.00448)	(0.000989)	(0.00266)	(0.00281)	(0.00115)	(0.00242)	(0.00277)
Return on Assets	0.00208	-0.00614	-0.00516	0.00221	-0.00544	-0.00430	0.00232	-0.00500	-0.00378
	(0.00233)	(0.00776)	(0.00857)	(0.00235)	(0.00774)	(0.00848)	(0.00232)	(0.00748)	(0.00836)
Constant	0.0296	0.122*	0.107	0.0229	0.0833*	0.0592	0.0172	0.0599	0.0320
	(0.0179)	(0.0642)	(0.0697)	(0.0177)	(0.0479)	(0.0520)	(0.0203)	(0.0453)	(0.0513)
Observations	856	856	856	856	856	856	856	856	856
adj. R^2	0.003	0.006	0.007	0.039	0.023	0.031	0.002	0.001	-0.000

Notes: Significance of coefficients based on Bell and McCraffy (2002) adjustment. Standard errors, clustered by date, in parentheses, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

3.7 Robustness Checks and Falsification Exercises

A key identifying assumption in difference-in-differences estimation is that of parallel or common trends; that is, connected and unconnected firms must follow the same time trend prior to the event windows to support the internal validity of the estimation approach. As a partial test for the common trends assumption, I employ an event-time specification

Figure 3.2: Event-Time Coefficients with Abnormal Returns as Dependent Variable



(equation 11) to estimate the dynamic effects of the shock-connection interaction terms relative to t=0 as seen in Reber (2005), amongst others.

$$AR_{it} = \gamma_i + \theta_t + \sum_{\tau=-10}^{-1} \rho_\tau D_{i\tau} + \sum_{\xi=0}^{10} \rho_\xi D_{i\xi} + \eta_{it} \tag{3.12}$$

where γ_i are firm fixed effects as before; θ_t are date fixed effects; $D'_{it}s$ are the shock-connection interaction terms at leads and lags to capture anticipatory and lagged treatment effects; and η_{it} is a mean zero disturbance term.

The pattern of the rho's overtime illustrate the change in trends for abnormal returns leading up to and after the start date of the ICC shocks. If the timing of the shocks are unrelated to trends and firms do not show responses to the shocks prior to the start of the event window, then there should be no trend in the rhos for t $\leq$ -1; in this way the approach also doubles as a placebo test. The specification is also a partial test of market efficiency, as it examines if adverse news about the ICC proceedings is reflected in firms' returns, and whether these adjustments filter through quickly or over a longer horizon.

Figure 3.2 plots the rho's, with 95 percent confidence intervals, across days relative to the start of ten day event shocks. As anticipated, the impact of the shock tends to hit roughly 3 to 4 days after announcement, in line with the NSE's transaction window. There also is no significant trend prior to the announcement date, lending validity to the difference-in-differences estimations conducted above. The deviations in trend seen around "-7" are due to some event dates being quite close to one another, an issue I will address as a robustness check for my preliminary results.

In terms of efficiency, the abnormal returns do seem to respond to "new" ICC news, supporting my earlier claim that the NSE is likely trending towards semi-strong efficiency. However, as exemplified by the ICC Board event-time figure, the shocks do seem to take some time to filter in completely.

As a robustness check, I used 2006 daily data for the estimation window instead of 2004 data. Recall that estimation windows are selected with the intention of estimating "normal" return parameters. In 2006 Kenya experienced a famine that hit an estimated 4 million people; a corruption scandal that resulted in the minister of finance resigning; and regional flooding that led to the displacements of thousands and the loss of crops in crucial sectors of the economy. I expect that these shocks bias the parameter estimates, however using estimation windows close to the sample period is standard practice in event studies.[51] Another benefit of using 2006 as the estimation window is that I gain 3 previously excluded firms.

Overall, signs do not tend to change for the baseline specifications shared above. However, the coefficient estimates do fall by 10 to 20 percent on the abnormal return specifications, with some losing significance. Still, the general conclusions hold: non-connected firms do not experience significant abnormal returns during shocks and ICC board connected firms experience relatively larger falls in abnormal returns relative to government and ICC advisor connected firms. For the cumulative abnormal return panels, there is a loss of significance on many of the coefficients for government and the ICC advisor firms. ICC board firms still fare significantly worse than non-ICC board firms, but the relative impact on cumulative abnormal returns for the ten day shock falls from -12 to -4 percentage points.

To establish the causal relationship between the ICC-connected firms' outcomes and the shocks, it is necessary to disqualify some competing explanations for the results. First, there may be concern about whether the estimated effects reflect the value of political connections or an evaluation of expected policy outcomes for an alternative regime. It could be that ICC-connected firms especially benefitted from the G-7's particular policy stance relative to that of Odinga, the opponent in the 2013 race for the presidency. This explanation does not seem to hold any weight given the similarity in policy regimes across the top political parties in Kenya. Kenyatta is the son of the first president and Odinga is the son of the first vice-president. Historical splits in party affiliation occurred in response to different factions or individuals attempting to gain the presidency, not because of ideological differences over how to run the country.[52] As journalist and owner of the Ugandan newspaper, *The Independent*, argues,

> The political class in Kenya possesses wealth and money and therefore a shared interest in the existing regime of property rights and the political institutions and public policies that undergird this structure. Since most of this wealth is

[51] Typically a pre-event window of 120 to 250 days is used, however the campaign and election violence in 2007 to early 2008 disqualifies that year.

[52] Several newspaper articles surrounding the campaign confirmed this point (e.g. Bloomberg, 2013).

> derived from controlling the state, the political class in Kenya may be united around money but is divided around power. [...] The contest for power, therefore, tends to obscure the economic unity of this class by highlighting their ethnic differences. Electoral competition in Kenya therefore tends to get politically charged around the issue of identity.[53]

Therefore we can disqualify an explanation that places policy differences as key driver of the results.

A second argument might be that ICC-connected firms also happen to be the most sensitive to all shocks, be they economic, political, or social in nature. If this were true, then it could be that outsized effects of ICC shocks on ICC-connected firms is just picking up the outsized sensitivity of a small subset of firms. Firm fixed effects in the difference-in-differences specification likely addressed this concern, so here I focus on the cumulative abnormal returns specifications. As a sensitivity control, I constructed an index based on within-firm standard deviation in abnormal returns for the violent 2007 post-election period (which ran from December 30, 2007 to April 17, 2008 when the power-sharing agreement was implemented). The index is normalized to be between 0 and 1.[54] Table 3.9 presents the results, where shading indicates different connection types as above.

Table 3.9: Cumulative Abnormal Return Panel Results with Post-Election Vulnerability Control, Clustering by Date

	(1) CAR One	(2) CAR Seven	(3) CAR Ten	(4) CAR One	(5) CAR Seven	(6) CAR Ten	(7) CAR One	(8) CAR Seven	(9) CAR Ten
Government	0.00690*	0.0329*	0.0400*						
	(0.00352)	(0.0184)	(0.0200)						
ICC Board				-0.0421***	-0.109***	-0.136***			
				(0.00483)	(0.00928)	(0.00882)			
ICC Advisor							-0.00454	-0.0167*	-0.0194***
							(0.00411)	(0.00953)	(0.00633)
ln(Assets)	-0.00253**	-0.00962**	-0.00912*	-0.00181*	-0.00630**	-0.00509*	-0.00140	-0.00471*	-0.00325
	(0.00101)	(0.00414)	(0.00447)	(0.000957)	(0.00270)	(0.00281)	(0.00110)	(0.00240)	(0.00270)
Return on Assets	0.00183	-0.00630	-0.00524	0.00233	-0.00454	-0.00309	0.00217	-0.00483	-0.00348
	(0.00243)	(0.00777)	(0.00871)	(0.00242)	(0.00769)	(0.00849)	(0.00241)	(0.00747)	(0.00841)
Post-Election Instability Index	0.0158	0.00945	0.00496	-0.00847	-0.0641***	-0.0857***	0.0112	-0.0128	-0.0220
	(0.0125)	(0.0218)	(0.0248)	(0.0112)	(0.0168)	(0.0183)	(0.0120)	(0.0171)	(0.0195)
Constant	0.0201	0.116*	0.104	0.0291*	0.130**	0.121**	0.00875	0.0694	0.0485
	(0.0163)	(0.0581)	(0.0609)	(0.0158)	(0.0506)	(0.0519)	(0.0181)	(0.0445)	(0.0481)
Observations	856	856	856	856	856	856	856	856	856
adj. R^2	0.003	0.005	0.006	0.038	0.025	0.036	0.002	-0.000	-0.001

Notes: Significance of coefficients based on Bell and McCraffy (2002) adjustment. Standard errors, clustered by date, in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

Interestingly, controlling for post-election sensitivity across firms raises the coefficients on government and ICC board connections relative to the baseline and slightly drops

[53]Mwenda, A. (2013, March 18). The aristocracisation of Kenya politics. The Independent. Retrieved June 3, 2014, from http://independent.co.ug/andrewmwenda/?p=490

[54]The index's correlation with return on assets and log of assets is .08 and -.13, respectively.

coefficients on ICC advisor connections. But with only minor changes in magnitude and significance (and no changes in signs), it does not seem that particularly sensitive firms are responsible for the variation we see in abnormal returns surrounding ICC shocks across connected and unconnected firms. Where significant, the coefficients on the index are negative implying that more sensitive firms tend to see lower cumulative abnormal returns—in line with expectations.

A second control for "sensitivity" is the mean frequency of all types of connections across firms at NAICS two-digit sector level.[55] This control (which largely picks up government connections) is included in order to ascertain whether "connection-required" sectors are driving the results, with the assumption that such firms' returns are quite vulnerable to ICC shocks because they are mere extensions of the government. Boutchkova, et al. (2012) show that industries particularly vulnerable to the public sector exhibit more volatility. For instance, Kenya Airways and Kenya Power are still predominantly owned by government and are thus required to have a number of government officials on their boards. Again, I normalize the index to be between 0 and 1.

Table 3.10: Sector Sensitivity Index Descriptive Statistics

Sector	**NAICS Code**	**Num of Firms in Sector**	**Sector Sensitivity Index**
Couriers & Delivery (i.e. Express Kenya)	49	1	0
Information (i.e. Media)	51	2	0
Accommodation and Food	72	1	0
Agriculture	11	6	0.02
Retail	44	2	0.05
Wholesale	42	4	0.07
Construction	23	6	0.2
Finance & Insurance	52	12	0.2
Manufacturing II	32	2	0.23
Manufacturing I	31	4	0.34
Utilities (i.e. Kenya Power)	22	1	1
Transportation (i.e. Kenya Airways)	48	1	1

Table 3.11 presents the results with the sector sensitivity index. As above, the coefficients on the connection indicators maintain their significance and do not switch signs.

[55]For holding companies, I coded the sector based on the sector of the majority of the firm's subsidiaries.

Table 3.11: Cumulative Abnormal Return Panel Results withSector Sensitivity Index, Clustering by Date

	(1) CAR One	(2) CAR Seven	(3) CAR Ten	(4) CAR One	(5) CAR Seven	(6) CAR Ten	(7) CAR One	(8) CAR Seven	(9) CAR Ten
Government	0.00477*	0.0318*	0.0410**						
	(0.00272)	(0.0175)	(0.0187)						
ICC Board				-0.0405***	-0.0972***	-0.120***			
				(0.00601)	(0.00958)	(0.00989)			
ICC Advisor							-0.00717	-0.0284**	-0.0311***
							(0.00523)	(0.0127)	(0.00974)
ln(Assets)	-0.00264**	-0.00965**	-0.00875*	-0.00189	-0.00667**	-0.00497	-0.00190	-0.00599**	-0.00438
	(0.00115)	(0.00403)	(0.00443)	(0.00113)	(0.00309)	(0.00343)	(0.00118)	(0.00275)	(0.00317)
Return on Assets	0.00206	-0.00615	-0.00505	0.00217	-0.00574	-0.00453	0.00223	-0.00537	-0.00416
	(0.00235)	(0.00773)	(0.00854)	(0.00237)	(0.00773)	(0.00851)	(0.00233)	(0.00752)	(0.00840)
Sector Sensitivity Index	0.00253	0.000516	-0.0103	0.00327	0.0205	0.0159	0.0123	0.0509**	0.0504**
	(0.0102)	(0.0106)	(0.0121)	(0.0113)	(0.0160)	(0.0187)	(0.0130)	(0.0220)	(0.0240)
Constant	0.0309	0.122*	0.102	0.0251	0.0968*	0.0697	0.0211	0.0761	0.0481
	(0.0194)	(0.0625)	(0.0689)	(0.0192)	(0.0537)	(0.0597)	(0.0205)	(0.0489)	(0.0556)
Observations	856	856	856	856	856	856	856	856	856
adj. R^2	0.001	0.005	0.006	0.038	0.022	0.030	0.002	0.002	0.000

Notes: Significance of coefficients based on Bell and McCraffy (2002) adjustment. Standard errors, clustered by date, in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

The index does not have much significance across the estimations. However, it almost always has a positive sign, indicating that the parastatal firms (where the index = 1) tended to fare better than wholly unconnected firms in response to ICC shocks. In fact, based on the estimates from Column (9), parastatals (whether connected to the ICC via an G-7 advisor or not) tended to have higher cumulative abnormal returns than other firms, which is surprising in light of the other results. One might argue that investors' valuations of parastatals is a more direct measure relative to total market returns for whether investors viewed the ICC as stabilizing or destabilizing. If that is a credible argument, these results offer weak, but suggestive evidence of a stabilizing role for the ICC.

Some of the shock windows overlap, resulting in the double-counting of some of the shocks' effects. To ensure the results are not being driven by this overlap I excluded event dates that fell within the ten day shock window of another event date. While the coefficients fall slightly (as expected), there is no change in the major conclusions of the paper. The conclusions of the paper are also robust to the exclusion of the 10/15/2008 Waki Commission shock, which produced exceptionally large and negative cumulative abnormal returns relative to other event dates (see Appendix C.4).

Finally, I also check for robustness of the results to the exclusion of thinly traded firms. As Fisman (2001) points out, thinly traded firms may not experience changes in stock price around shocks, but their underlying value could nevertheless be lower. In this case, thinly traded firms will tend to bias the coefficient estimates towards zero. On the other hand, if unconnected firms in particular are thinly traded then the diff-in-diff estimates could be picking up overall market declines that do not show up in the aggregate specifications because unconnected firms are not experiencing fluctuations in price. To

examine this, I removed all firm-event observations for which firm returns were zero (as in Fisman, 2001). Appendix C.8 presents the results. For ICC board and ICC advisor firms the coefficient estimates rise, indicating that thinly traded firms bias the baseline estimates downwards. The overall conclusions of the paper remain intact.

3.8 Conclusion

This paper examined investor responses to the International Criminal Court's case against the Ocampo 6 using a standard event study framework. Overall market returns suggest that adverse news related to the ICC case had a destabilizing impact on listed firms. However, the difference-in-differences estimation uncovered that unconnected firms did not experience significantly negative revaluations surrounding adverse ICC news announcements. There is some evidence that government and ICC-advisor linked firms experienced small declines in abnormal returns, but these results pale in comparison to the large losses of ICC-board linked firms.

The absence of discernible abnormal returns for non-connected firms surrounding ICC shocks indicates that investors do not appear to be forming aggregative opinions about the ICC as a stabilizing or de-stabilizing force for Kenya's economy. That is, from the standpoint of investors the ICC—as an "imported" institution—only has value in how its actions relate to firms connected to the accused. Moreover, ICC-connected firms make up a small portion of the overall economy (less than 1 percent), so there is little indication that the ICC has aggregate implications on the Kenyan economy.

The results of this paper not only confirm that close connections with political leadership have value for connected firms, but also that a lack of transparency can potentially shield connected firms from large negative shocks related to their connections.

There are a number of next steps and extensions for the research. Two weaknesses of this paper's approach to measuring political connections are that it does not exploit the intensity of connections and that connections are constant across time. To allow for dynamism, I can code the additional 2,000+ board member biographies for 2008, 2009, 2010, 2011, and 2013. Another oft-used connection measure is based on shareholder data (Faccio, 2006; Faccio, et al., 2006; Boubakri, et al., 2012). Listed firms in Kenya report their top 10 shareholders in annual reports. However, there are limitations to using shareholder data since wealthy individuals and families often use shell companies or nominee accounts for their investments making it difficult to uncover connections (Faccio, 2006).

With respect to the external validity of the work, there are two prospective extensions. One option is to examine investors' responses on the Khartoum Stock Exchange to the Sudanese ICC case against President al-Bashir.[56] Another option is to use cases brought

[56]Other ICC cases include the Democratic Republic of the Congo, Uganda, the Central African Republic, Libya, and Côte d'Ivoire. In addition, the ICC is currently conducting preliminary analysis in eight countries: Afghanistan, Colombia, the Republic of Korea, Georgia, Guinea, Honduras, Nigeria and

under the U.S. Alien Tort Statute (ATS). The ATS "gives the federal courts jurisdiction to hear lawsuits filed by non-U.S. citizens for torts committed in violation of international law."[57] Although, many of these cases are brought against low-level torturers, and génocidaires, making their connections with firms unlikely.

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Appendix A

Appendix Figures, Tables, and Materials for Chapter 1

A.1 Theoretical Appendix

A.1.1 Appendix I.I

Suppose a price-taking manager is deciding whether or not to take action on a project that will produce one additional unit of a good each period, with zero per-period operating costs. This "action" could be towards building a factory, expanding an existing facility, making a capital investment, hiring employees, sourcing additional resources to meet changing consumer preferences, or otherwise.

Let $F > 0$ be a sunk cost of such an action and $r > 0$ be the interest rate. The additional value received from action in period $t = 0$ is P_0. From period $t = 1$ onward it will be

$$P_1 = \begin{cases} (1+\mu)\,P_0 \text{ with probability } A, \\ (1-\delta)\,P_0 \text{ with probability } B \\ P_0 \text{ with probability } C = 1 - A - B \end{cases}$$

where $A,\ B,\ C,\ \delta,\ \mu \in (0,1)$, $A + B + C = 1$, and $r,\ F > 0$.

Let V_0 be the expected present value from taking action.

$$V_0 = \underbrace{P_0}_{Current\ Revenue} + \underbrace{\left[A\,(1+\mu)\,P_0 + B(1-\delta)P_0 + CP_0\right]\left[\frac{1}{1+r} + \frac{1}{(1+r)^2} + \cdots\right]}_{Weighted\ Average\ of\ Future\ Revenues}$$

$$V_0 = \frac{rP_0}{r} + \frac{A\,(1+\mu)\,P_0 + B(1-\delta)P_0 + CP_0}{r}$$

$$V_0 = \frac{P_0\,(r + A\,(1+\mu) + B(1-\delta) + C)}{r}$$

$$Net\ Payoff\ for\ Immediate\ Action = V_0 - F = \frac{P_0\,(r + A\,(1+\mu) + B(1-\delta) + C)}{r} - F$$

Now allow for action to remain an option in the second period. The firm decision becomes: act in $t = 0$ or wait to see what happens in $t = 1$ and decide then. Suppose the firm decides not to act in period $t = 0$.

The present value of revenue streams, discounted back to period $t = 1$ is

$$V_1 = P_1 + P_1/\,(1+r) + P_1/\,(1+r)^2 + \cdots = P_1\,(1+r)\,/r.$$

Then in period $t = 1$, for each of the potential P_1 outcomes, the firm would invest if $V_1 > F$, with a net payoff of

$$\Gamma_1 = max\,[V_1 - F, 0]\,.$$

At period $t = 0$, P_1 is not known, making V_1 and Γ_1 random variables. Let E_0 be the expectation at $t = 0$. Then

$$E_0\left[\Gamma_1\right] = A \times max\left[\left(1+\mu\right)P_0\left(1+r\right)/r - F, 0\right] + B \times max\left[\left(1-\delta\right)P_0\left(1+r\right)/r - F, 0\right]$$

$$+C \times max\left[P_0\left(1+r\right)/r - F, 0\right]$$

This is the continuation value associated with waiting until period $t = 1$ to act.

Returning to the $t = 0$ decision, the firm can take action immediately and get $V_0 - F$. If it decides to postpone, it gets one-period discounted $E_0\left[\Gamma_1\right]$. So the net payoff of the action becomes

$$\mathit{\Gamma}_0 = max\left\{V_0 - F, \frac{1}{1+r}E_0\left[\Gamma_1\right]\right\}.$$

The difference between the two cases—the now-or-never option or the option to act in the second period ($\mathit{\Gamma}_0 - \Omega_0$) is the value of the option to postpone action. The ability to wait allows for the ability to base action on different contingencies, offering extra value from "waiting and seeing." More formally, the net payoff is convex in initial price; by Jensen's inequality, the expectation of the separate maximization problem is larger than the maximum of the average expectation.

A.1.2 Appendix I.II

The point of indifference between taking action now versus waiting occurs in the range of P_0 where one takes action if P_0 increases or stays the same, but not if P_0 decreases. The now or never option yields a net payoff of $V_0 - F = \frac{P_0(r+A(1+\mu)+B(1-\delta)+C)}{r} - F$. If the manager delays and the price goes up or stays the same, she receives $\frac{A}{1+r}\left[P_0\frac{(1+\mu)(1+r)}{r} - F\right] + \frac{C}{1+r}\left[P_0\frac{1+r}{r} - F\right]$.

The now or never function has a lower intercept than the delay payoff since: $-F < -\frac{FA}{1+r} - \frac{FC}{1+r} = -\frac{F}{1+r}\left(A+C\right)$ for all A, $C \in (0,1)$ and r, $F > 0$.

It also has a steeper slope since $\frac{r+A(1+\mu)+B(1-\delta)+C}{r} > \frac{A(1+\mu)+C}{r}$ for $A,\ B,\ C,\ \delta,\ \mu \in (0,1)$ and $r > 0$.

Solving for $\underline{P_0}$ for now or never yields $\underline{P_0} = \frac{rF}{r+A(1+\mu)+B(1-\delta)+C}$. $\underline{P_0}$ associated with delay and no price decrease is $\underline{P_0} = \frac{r(CF+AF)}{C(1+r)+A(1+\mu)(1+r)}$.

A.1.3 Appendix I.III

Setting the net payoff from the now or never option equal to the net payoff to delay, I solve for $\overline{P_0}$, the cutoff between taking action immediately and delaying:

$$\frac{P_0\left(r + A\left(1+\mu\right) + B(1-\delta) + C\right)}{r} - F = \frac{A}{1+r}\left[-F + P_0\frac{\left(1+\mu\right)\left(1+r\right)}{r}\right] + \frac{C}{1+r}\left[P_0\frac{1+r}{r} - F\right]$$

$$P_0\left[r + B(1-\delta)\right] = -\frac{rA}{1+r}F + rF - F\frac{rC}{1+r}$$

Substituting $C = 1 - A - B$,

$$P_0 \left[r + B(1-\delta) \right] = rF \left(1 - \frac{(1-A-B)}{1+r} - \frac{A}{1+r} \right)$$

$$\overline{P_0} = \left(\frac{r}{1+r} \right) F \frac{(r+B)}{r+B(1-\delta)}$$

$\overline{P_0}$ does not depend on μ or A, the magnitude of good news or the probability of good news. As δ, the magnitude of potential bad news increases, $\overline{P_0}$ increases. An increase in B, the probability of bad news, also leads to an increase in $\overline{P_0}$ as $\frac{\partial \overline{P_0}}{\partial B} = \frac{\delta r^2 F}{(1+r)(B(\delta-1)-r)^2}$ which is positive for $B,\ \delta \in (0,1)$ and $r,\ F > 0$. This is the bad news principle: as the probability of bad news increases, the value of delay increases and firms wait to take action.

A.2 Example Search Algorithms

Below are the search algorithms used for the United Kingdom. The remaining algorithms are available in the online appendix.[1]

Generic: (safeguard measure* or domestic content or anti-dumping or sanitary measure* or TTIP or GATT or free trade zone or rules of origin or EFTA or customs union or countervailing measure* or banana war* or GATT* or dumping or quota or voluntary export restraint or local content requirement or WTO or World Trade Organization or protectionism or (trade near2 (war or deal or delegation or controversy or bilateral or free or preferential or dispute or polic* or restriction* or quota or commission or sanction or content or embargo or negotiation* or agreement* or anti or deal or barrier* or red tape or subsid* or TRIPS)) or (import near2 (license or fees or duty or barrier* or tariff* or competit* or tax*)) or (export near2 (license or tax* or subsid* or competit*)) or government or (spending near2 (government or public or fiscal)) or austerity or tax* or (fiscal near2 (plan or crisis or emergency or measure* or gap or discipline or consolidation or stimulus)) or (budget near2 (surplus or deficit or plan or revenue or balanced or gap)) or (debt near2 (public or national or sovereign or government)) or government revenue* or budget or deficit reduction or public revenue or entitlements or automatic stabili?er* or monetary policy or yield or interest rate or policy or regulat* or Bank of England or central bank or monetary or quantitative easing or money supply or bond purchases or overnight rate or tight money or loose money or discount rate or loose* policy or tight* policy or accomm* policy or monetary accomm* or asset purchases or open market operations) same (uncert* or ambiguous or dubious or precarious or unpredictable or undecided or undetermined or unresolved or unsettled or concern or worr* or anxiet* or doubt* or unclear) near8 (United Kingdom or UK or Brit*) not ("without doubt" or "no uncertainty" or "no doubt" or shares or equit* or stock market) and wc>99 and re=UK and date from 01/01/2003 to 06/30/2016

Fiscal: ((spending near2 (government or public or fiscal)) or austerity or tax* or (fiscal near2 (plan or crisis or emergency or measure* or gap or discipline or consolidation or stimulus)) or (budget near2 (surplus or deficit or plan or revenue or balanced or gap)) or (debt near2 (public or national or sovereign or government)) or government revenue* or budget or deficit reduction or

[1]The online appendix can be accessed on www.sandile.com.

public revenue or entitlements or automatic stabili?er*) same (uncert* or ambiguous or dubious or precarious or unpredictable or undecided or undetermined or unresolved or unsettled or concern or worr* or anxiet* or doubt* or unclear) near8 (United Kingdom or UK or Brit*) not (safeguard measure* or domestic content or anti-dumping or sanitary measure* or TTIP or GATT or free trade zone or rules of origin or EFTA or customs union or countervailing measure* or banana war* or GATT* or dumping or quota or voluntary export restraint or local content requirement or WTO or World Trade Organization or protectionism or (trade near2 (war or deal or delegation or controversy or bilateral or free or preferential or dispute or polic* or restriction* or quota or commission or sanction or content or embargo or negotiation* or agreement* or anti or deal or barrier* or red tape or subsid* or TRIPS)) or (import near2 (license or fees or duty or barrier* or tariff* or competit* or tax*)) or (export near2 (license or tax* or subsid* or competit*)) or Bank of England or central bank or monetary or monetary policy or yield or interest rate or quantitative easing or money supply or bond purchases or overnight rate or tight money or loose money or discount rate or loose* policy or tight* policy or accomm* policy or monetary accomm* or asset purchases or open market operations or "without doubt" or "no uncertainty" or "no doubt" or shares or equit* or stock market) and wc>99 and re=UK and date from 01/01/2003 to 06/30/2016

Trade: (safeguard measure* or domestic content or anti-dumping or sanitary measure* or TTIP or GATT or free trade zone or rules of origin or EFTA or customs union or countervailing measure* or banana war* or GATT* or dumping or quota or voluntary export restraint or local content requirement or WTO or World Trade Organization or protectionism or (trade near2 (war or deal or delegation or controversy or bilateral or free or preferential or dispute or polic* or restriction* or quota or commission or sanction or content or embargo or negotiation* or agreement* or anti or deal or barrier* or red tape or subsid* or TRIPS)) or (import near2 (license or fees or duty or barrier* or tariff* or competit* or tax*)) or (export near2 (license or tax* or subsid* or competit*))) same (uncert* or ambiguous or dubious or precarious or unpredictable or undecided or undetermined or unresolved or unsettled or concern or worr* or anxiet* or doubt* or unclear) near8 (United Kingdom or UK or Brit* or EU or European Union or European Commission or EC) not ((spending near2 (government or public or fiscal)) or austerity or tax* or (fiscal near2 (plan or crisis or emergency or measure* or gap or discipline or consolidation or stimulus)) or (budget near2 (surplus or deficit or plan or revenue or balanced or gap)) or (debt near2 (public or national or sovereign or government)) or government revenue* or budget or deficit reduction or public revenue or entitlements or automatic stabili?er* or Bank of England or central bank or monetary or monetary policy or yield or interest rate or quantitative easing or money supply or bond purchases or overnight rate or tight money or loose money or discount rate or loose* policy or tight* policy or accomm* policy or monetary accomm* or asset purchases or open market operations or "without doubt" or "no uncertainty" or "no doubt" or shares or equit* or stock market) and wc>99 and re=UK and date from 01/01/2003 to 06/30/2016[2]

Monetary: (monetary policy or yield or interest rate or Bank of England or central bank or monetary or quantitative easing or money supply or bond purchases or overnight rate or tight money or loose money or discount rate or loose* policy or tight* policy or accomm* policy or monetary accomm* or asset purchases or open market operations) same (uncert* or ambiguous or

[2]Since trade policy is negotiated by the EU, EU search terms are included in the trade policy algorithms.

dubious or precarious or unpredictable or undecided or undetermined or unresolved or unsettled or concern or worr* or anxiet* or doubt* or unclear) near8 (United Kingdom or UK or Brit*) not ("without doubt" or "no uncertainty" or "no doubt" or shares or equit* or stock market or safeguard measure* or domestic content or anti-dumping or sanitary measure* or TTIP or GATT or free trade zone or rules of origin or EFTA or customs union or countervailing measure* or banana war* or GATT* or dumping or quota or voluntary export restraint or local content requirement or WTO or World Trade Organization or protectionism or (trade near2 (war or deal or delegation or controversy or bilateral or free or preferential or dispute or polic* or restriction* or quota or commission or sanction or content or embargo or negotiation* or agreement* or anti or deal or barrier* or red tape or subsid* or TRIPS)) or (import near2 (license or fees or duty or barrier* or tariff* or competit* or tax*)) or (export near2 (license or tax* or subsid* or competit*)) or (spending near2 (government or public or fiscal)) or austerity or tax* or (fiscal near2 (plan or crisis or emergency or measure* or gap or discipline or consolidation or stimulus)) or (budget near2 (surplus or deficit or plan or revenue or balanced or gap)) or (debt near2 (public or national or sovereign or government)) or government revenue* or budget or deficit reduction or public revenue or entitlements or automatic stabili?er*) and wc>99 and re=UK and date from 01/01/2003 to 06/30/2016

Normalizer: today and (United Kingdom or UK or Brit*) and wc>99 and re=UK and date from 01/01/2003 to 06/30/2016

A.3 Coefficients of Non-Determination

The below tables display the average coefficients of non-determination ($1 - R^2$) across the four sample countries for regressions of the policy uncertainty measures on traditional measures of economic uncertainty:

- VIX: CBOE Volatility Index
- SMV: Stock market volatility
- ECB I: the interquartile of average probability distributions for EU real GDP growth from professional forecasters
- ECB II: the standard deviations of professional forecasts for real GDP growth[3]

[3]Stock market volatility is sourced from Bloomberg and measures the 360-day standard deviation of the return on the national stock market index; VIX is sourced from the Chicago Board Options Exchange, CBOE Volatility Index (retrieved from FRED, Federal Reserve Bank of St. Louis; https://fred.stlouisfed.org/series/VIXCLS, May 5, 2016); and the standard deviations and probability distributions of professional forecasts are sourced from the ECB's Survey of Professional Forecasters database which can be found here: https://www.ecb.europa.eu/stats/prices/indic/forecast/html/index.en.html.

Table A.1: U.K.'s Coefficients of Non-Determination

	GPU	FPU	TPU	MPU	VIX	SMV	ECB I	ECB II
GPU	0.00							
FPU	0.57	0.00						
TPU	0.86	0.96	0.00					
MPU	0.87	0.98	0.81	0.00				
VIX	0.99	0.99	0.77	0.98	0.00			
SMV	0.92	1.00	0.76	0.97	0.17	0.00		
ECB I	0.77	0.56	0.91	1.00	0.99	0.97	0.00	
ECB II	0.92	0.56	0.60	1.00	0.92	0.99	0.68	0.00

Table A.2: France's Coefficients of Non-Determination

	GPU	FPU	TPU	MPU	VIX	SMV	ECB I	ECB II
GPU	0.00							
FPU	0.36	0.00						
TPU	0.73	0.93	0.00					
MPU	0.44	0.87	0.72	0.00				
VIX	0.73	0.77	0.61	0.82	0.00			
SMV	0.82	0.86	0.62	0.89	0.15	0.00		
ECB I	0.90	0.77	0.92	0.98	0.99	1.00	0.00	
ECB II	0.99	0.90	0.63	0.80	0.92	0.95	0.68	0.00

Table A.3: Greece's Coefficients of Non-Determination

	GPU	FPU	TPU	MPU	VIX	SMV	ECB I	ECB II
GPU	0.00							
FPU	0.19	0.00						
TPU	0.91	0.90	0.00					
MPU	0.09	0.45	0.95	0.00				
VIX	1.00	1.00	0.60	1.00	0.00			
SMV	0.42	0.64	0.63	0.39	0.73	0.00		
ECB I	0.85	0.75	0.86	0.95	0.99	0.90	0.00	
ECB II	0.43	0.48	0.55	0.43	0.92	0.59	0.68	0.00

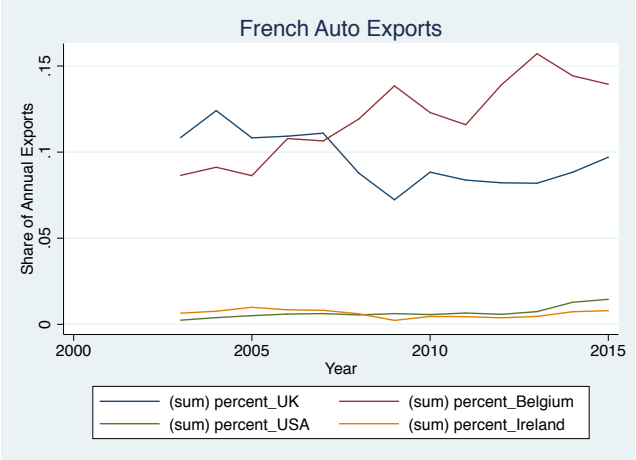
A.4 Choice of Mean Share for External Policy Uncertainty Construction

There is much more variability in trade relationships for services, relative to goods trade. By taking the mean shares—rather than the median or the initial period—of the sample I account for the fact that destinations remain relevant, even if a sector is not currently exporting there. This is not very consequential for sectors with relative stability in trade partners (e.g., Autos in

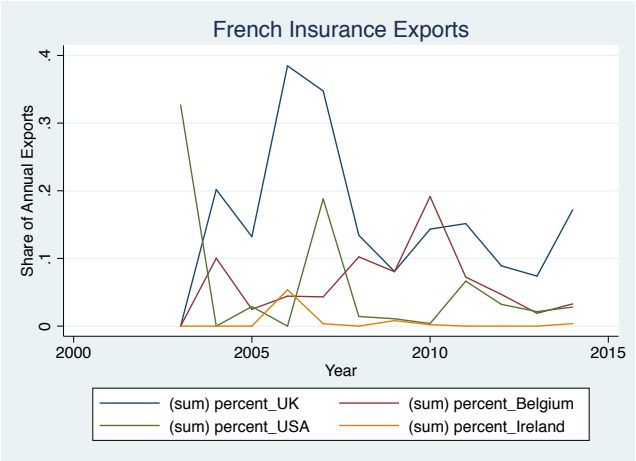
Table A.4: Turkey's Coefficients of Non-Determination

	GPU	FPU	TPU	MPU	VIX	SMV	ECB I	ECB II
GPU	0.00							
FPU	0.35	0.00						
TPU	0.85	0.85	0.00					
MPU	0.26	0.63	0.96	0.00				
VIX	0.68	0.95	0.71	0.76	0.00			
SMV	1.00	0.97	0.99	0.98	0.72	0.00		
ECB I	1.00	1.00	0.95	0.99	0.99	0.75	0.00	
ECB II	0.69	0.89	0.74	0.79	0.92	0.75	0.68	0.00

France).



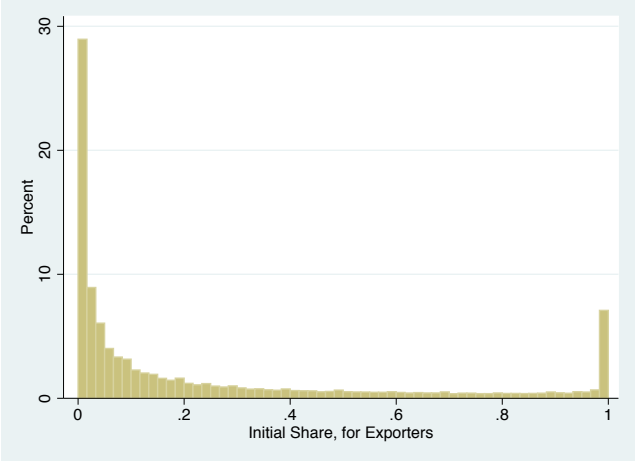
However, examining the Insurance sector in France illuminates the importance of the mean share. If I were to only take the median, a country like the U.S. would seem altogether trivial for French insurance exports, despite accounting for a large share of such exports at the beginning of the sample. If I took the initial share it would overweight the U.S.'s overall importance and ignore the U.K.



A.5 Correspondences

For goods, Pierce & Schott's (2012) HS to NAICS correspondence was used. For services, Erik van der Marel's 3-digit correspondence between EBOPS and NAICS was used. Much of the United Kingdom's 2003 services data by country-service is still classed as confidential/non-publishable, so 2002 data were used. Turkey does not have services data available by sector in the early portion of the sample period.

A.6 Export Exposure, for Exporters



A.7 Type-Specific Policy Uncertainty Results

Table A.5: Type-Specific Policy Uncertainty, Profits

	(1) Profits	(2) Profits	(3) Profits	(4) Profits	(5) Profits
Firm GPU_{t-1}	-0.089***				
	(0.02)				
Firm FPU_{t-1}		-0.067***			
		(0.02)			
Firm MPU_{t-1}			-0.057**		
			(0.02)		
Firm Macro PU_{t-1}					-0.078***
					(0.02)
Firm TPU_{t-1}				0.058	0.040
				(0.05)	(0.05)
Observations	4375354	4375354	4375354	4375354	4375354
F	556.209	532.234	595.658	552.509	479.083
R-squared	0.933	0.933	0.933	0.933	0.933
R-squared within	0.027	0.027	0.027	0.027	0.027
Firm FE?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm-Time GDP Forecast Control?	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the country-time level between brackets, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

Table A.6: Type-Specific Policy Uncertainty, Investment

	(1) Invest	(2) Invest	(3) Invest	(4) Invest	(5) Invest
Firm GPU_{t-1}	-0.049**				
	(0.02)				
Firm FPU_{t-1}		-0.063***			
		(0.02)			
Firm MPU_{t-1}			-0.008		
			(0.02)		
Firm Macro PU_{t-1}					-0.037*
					(0.02)
Firm TPU_{t-1}				0.082*	0.074
				(0.04)	(0.05)
Observations	4661420	4661420	4661420	4661420	4661420
F	44.731	47.102	43.668	56.340	43.752
R-squared	0.435	0.435	0.435	0.435	0.435
r2_within	0.001	0.001	0.001	0.001	0.001
Firm FE?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm-Time GDP Forecast Control?	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the country-time level between brackets, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

Table A.7: Type-Specific Policy Uncertainty, Employment

	(1) Employment	(2) Employment	(3) Employment	(4) Employment	(5) Employment
Firm GPU_{t-1}	-0.053***				
	(0.02)				
Firm FPU_{t-1}		-0.045***			
		(0.01)			
Firm MPU_{t-1}			-0.037**		
			(0.02)		
Firm Macro PU_{t-1}					-0.050***
					(0.01)
Firm TPU_{t-1}				0.024	0.011
				(0.03)	(0.03)
Observations	2309533	2309533	2309533	2309533	2309533
F	768.479	757.591	778.139	760.611	588.064
R-squared	0.959	0.959	0.959	0.959	0.959
R-squared within	0.039	0.039	0.039	0.039	0.039
Firm FE?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm-Time GDP Forecast Control?	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$* p < 0.10$, **$ p < 0.05$, ***$ p < 0.01$.

Table A.8: Type-Specific Policy Uncertainty, Average Wage

	(1) Avg Wage	(2) Avg Wage	(3) Avg Wage	(4) Avg Wage	(5) Avg Wage
Firm GPU_{t-1}	-0.002				
	(0.01)				
Firm FPU_{t-1}		0.006			
		(0.01)			
Firm MPU_{t-1}			-0.007		
			(0.01)		
Firm Macro PU_{t-1}					-0.005
					(0.01)
Firm TPU_{t-1}				0.028**	0.029**
				(0.01)	(0.01)
Observations	2162715	2162715	2162715	2162715	2162715
F	151.908	164.016	150.057	128.176	108.917
R-squared	0.821	0.821	0.821	0.821	0.821
R-squared within	0.004	0.004	0.004	0.004	0.004
Firm FE?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm-Time GDP Forecasts Control?	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$* p < 0.10$, **$ p < 0.05$, ***$ p < 0.01$.

A.8 Robustness Checks

A.8.1 Robustness, By Outcome

Below are robustness checks by outcome: sales, profits, investment, employment, and average wages. Column (1) is the baseline approach and includes a first moment of policy control (FMC) for WEO forecasts; Column (2) uses realized GDP as the first moment control; Column (3) uses within year WEO revisions; Column (4) - (7) add various group-time controls; and Column (8) uses time-varying export share to construct the firm-specific uncertainty measure and then instruments for it using initial export share policy uncertainty.

Table A.9: Robustness, Sales

	(1) FMC: Forecasts	(2) FMC: GDP	(3) FMC: Revisions	(4) Status x CST	(5) Share x CT, Share x ST	(6) Share x T	(7) Time-Varying Share	(8) Exposure Instrument
Firm GPU_{t-1}	-0.082***	-0.081***	-0.067**	-0.064***	-0.128***	-0.127***	-0.125***	-0.096***
	(0.03)	(0.03)	(0.02)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	6280569	6280569	6280569	6279614	6281097	6281146	5421083	6280569
F	1148.302	1146.236	1177.180	1137.684	108.930	108.448	119.231	1134.329
R-squared	0.942	0.942	0.942	0.942	0.999	0.999	0.934	0.942
R-squared within	0.042	0.042	0.042	0.042	0.077	0.077	0.078	0.043
First Stage F Stat?	–	–	–	–	–	–	–	221.8
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	–	–	–	–	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A.10: Robustness, Profits

	(1) FMC: Forecasts	(2) FMC: GDP	(3) FMC: Revisions	(4) Status x CST	(5) Share x CT, Share x ST	(6) Share x T	(7) Time-Varying Share	(8) Exposure Instrument
Firm GPU_{t-1}	-0.089***	-0.091***	-0.105***	-0.082***	-0.121***	-0.120***	-0.118***	-0.099***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	4375354	4375354	4375354	4374418	4375902	4375950	3682046	4375354
F	556.209	554.244	558.635	522.714	73.523	73.456	80.212	556.262
R-squared	0.933	0.933	0.933	0.933	0.999	0.999	0.924	0.933
R-squared within	0.027	0.027	0.027	0.027	0.079	0.079	0.083	0.028
First Stage F Stat?	–	–	–	–	–	–	–	440.4
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	–	–	–	–	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A.11: Robustness, Investment

	(1) FMC: Forecasts	(2) FMC: GDP	(3) FMC: Revisions	(4) Status x CST	(5) Share x CT, Share x ST	(6) Share x T	(7) Time-Varying Share	(8) Exposure Instrument
Firm GPU_{t-1}	-0.053**	-0.054**	-0.065**	-0.083***	-0.178**	-0.178**	-0.175**	-0.062**
	(0.02)	(0.02)	(0.03)	(0.03)	(0.07)	(0.07)	(0.07)	(0.03)
Observations	4728337	4728337	4728337	4727689	4728592	4728608	4019990	4728337
F	4.853	4.882	6.344	2.790	27.671	27.734	28.028	5.176
R-squared	0.743	0.743	0.743	0.743	0.984	0.984	0.733	0.743
R-squared within	0.000	0.000	0.000	0.000	0.022	0.022	0.025	0.000
First Stage F Stat?	–	–	–	–	–	–	–	248.9
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	–	–	–	–	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A.12: Robustness, Employment

	(1) FMC: Forecasts	(2) FMC: GDP	(3) FMC: Revisions	(4) Status x CST	(5) Share x CT, Share x ST	(6) Share x T	(7) Time-Varying Share	(8) Exposure Instrument
Firm GPU_{t-1}	-0.053***	-0.052***	-0.043***	-0.019**	-0.010	-0.010	-0.010	-0.056***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Observations	2309533	2309533	2309533	2308877	2309806	2309842	1910251	2309533
F	768.479	770.567	605.996	610.492	157.354	155.849	144.170	763.576
R-squared	0.959	0.959	0.959	0.959	0.984	0.984	0.955	0.959
R-squared within	0.039	0.039	0.039	0.039	0.036	0.037	0.030	0.039
First Stage F Stat?	–	–	–	–	–	–	–	999.8
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	–	–	–	–	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A.13: Robustness, Average Wages

	(1) FMC: Forecasts	(2) FMC: GDP	(3) FMC: Revisions	(4) Status x CST	(5) Share x CT, Share x ST	(6) Share x T	(7) Time-Varying Share	(8) Exposure Instrument
Firm GPU_{t-1}	-0.002	-0.002	-0.012	-0.016	-0.174***	-0.174***	-0.174***	-0.003
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.02)
Observations	2162715	2162715	2162715	2162405	2162811	2162840	1766464	2162715
F	151.908	151.218	104.059	139.822	28.697	29.111	30.997	151.929
R-squared	0.821	0.821	0.821	0.821	0.999	0.999	0.814	0.821
R-squared within	0.004	0.004	0.004	0.004	0.077	0.077	0.077	0.004
First Stage F Stat?	–	–	–	–	–	–	–	394.4
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	–	–	–	–	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

A.8.2 Adding lagged dependent variables to control for pre-existing trends.

Table A.14: Lagged Dependent Levels with Fixed Effects Results

	(1) Sales	(2) Profits	(3) Investment	(4) Employment	(5) Avg Wage
Firm GPU_{t-1}	-0.058***	-0.062***	-0.057***	-0.033***	0.001
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
$Sales_{t-1}$	0.384***				
	(0.03)				
$Profits_{t-1}$		0.236***			
		(0.03)			
$Investment_{t-1}$			-0.021		
			(0.03)		
$Employment_{t-1}$				0.440***	
				(0.04)	
Avg $Wage_{t-1}$					0.145***
					(0.03)
Observations	6233953	4232138	3616432	1794206	1654249
F	788.085	738.169	8.863	645.394	105.054
R-squared	0.954	0.942	0.775	0.971	0.842
R-squared within	0.197	0.083	0.001	0.226	0.025
Firm FE?	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm GDP Forecasts Control?	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time Controls?	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

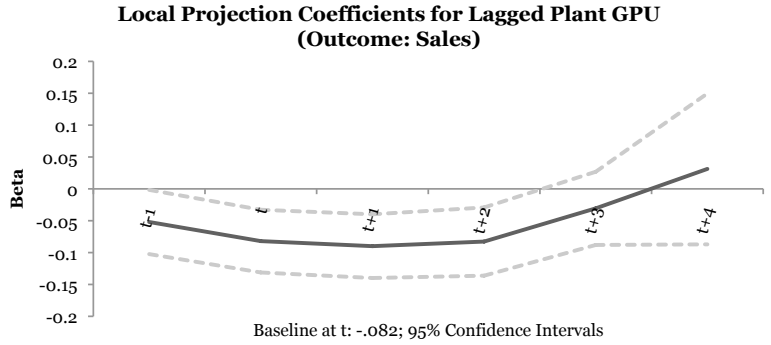
Table A.15: Lagged Dependent Difference GMM Results

	(1) Sales	(2) Profits	(3) Investment	(4) Employment	(5) Avg Wage
Firm GPU_{t-1}	-0.041***	-0.057***	-0.119***	-0.011***	-0.113***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$Sales_{t-1}$	0.488***				
	(0.00)				
$Profits_{t-1}$		0.391***			
		(0.00)			
$Investment_{t-1}$			0.010***		
			(0.00)		
Employment $_{t-1}$				0.617***	
				(0.01)	
Avg $Wage_{t-1}$					0.199***
					(0.00)
Observations	5087553	3445642	2613402	1282994	1166809
F	62687.192	43160.403	3870.841	1879.420	11354.748
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes
+ Firm GDP Forecasts Control?	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

A.8.3 Timing

Figure A.1: Jorda (2005) Local Projection Results



A.9 Additional Trade Policy Uncertainty Results

A.9.1 Export Participation Linear Probability Model Specification & Results

To examine the extensive margin effects of policy uncertainty, I extend Roberts & Tybout's (1997) export participation specification, which was also employed by Bernard and Jensen (2004), to include policy uncertainty. This specification has the benefit of testing for the importance/presence of sunk costs via the inclusion of lagged export participation.

$$S_{icst} = \begin{cases} 1 \; if \; \gamma_i + \tau_{cst} + \beta \times Uncertainty \; Type_{icst-1} + \mathbf{Z}_{it-1} + \mathbf{A}_{ct-1} + \psi S_{ics,t-1} + \varepsilon_{icst} \geq 0 \\ 0, \; otherwise \end{cases}$$

where S_{icst} is a binary variable for firm participation in exporting in period t; $\mathbf{Z_{it}}$ is a vector of time-varying firm characteristics; $S_{ics,t-1}$ is lagged participation, which should only be significant in the face of sunk costs.

There are several identification issues associated with such a specification. Bernard, Jensen, and Lawrence (1995) and Bernard and Jensen (1999) uncover large contemporaneous changes in firm fundamentals when firms opt-in to exporting. To moderate simultaneity bias, all variables are lagged. Following Bernard & Jensen (2004), I estimate this as a linear probability model (LPM) with fixed effects since time-invariant firm characteristics are unlikely to be uncorrelated with time-varying firm characteristics (as required by random effects nonlinear models). I can also control for several other dimensions of fixed effects in this context. Since my interest is primarily in β (i.e., policy uncertainty's impact), I also preference the LPM approach since it allows for the estimation of constant marginal effects. Identification of ψ, the coefficient on the lagged dependent variable has received considerable attention from the literature; it is likely to be downwardly biased and inconsistent if estimated via an LPM (Heckman, 1981). Lagged participation is likely to be correlated with lagged policy uncertainty and other country-time variables. I instrument for lagged participation using a higher-order lagged firm attribute—fixed assets. Fixed assets are highly serially correlated, so I also include lagged fixed assets within $\mathbf{Z_{it}}$ to help address this concern. This specification also ignores the initial conditions problem. However, given the length of the sample period and the number of firms, the initial condition problem should not be too egregious (Skrondal & Rabe-Hesketh, 2014).[4]

Effective monetary policy uncertainty decreases extensive market participation for more exposed firms, but increases in effective trade policy uncertainty increase the probability that a more exposed firm will participate in exporting in a particular period.[5]

[4]For a detailed discussion on this approach and possible sources of bias see Skrondal & Rabe-Hesketh (2014).

[5]The coefficients on the lagged dependent variable reflect the presence of sunk costs and are are in line with those estimates found in Bernard and Jensen (2004).

Table A.16: Extensive Margin Linear Probabilty Model Results

	(1) Exporting	(2) Exporting	(3) Exporting	(4) Exporting
Exporting_{t-1}	0.339***	0.338***	0.339***	0.338***
	(0.06)	(0.06)	(0.06)	(0.06)
Firm GPU_{t-1}	-0.032			
	(0.02)			
Firm FPU_{t-1}		-0.015		
		(0.02)		
Firm MPU_{t-1}			-0.041***	
			(0.01)	
Firm TPU_{t-1}				0.088**
				(0.04)
Observations	5198590	5198590	5198590	5198590
F	67.348	69.310	73.754	67.533
R-squared	0.722	0.722	0.722	0.722
Firm FE?	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the country-time level between brackets, *$* p < 0.10$, **$ p < 0.05$, ***$ p < 0.01$.

A.9.2 Sector, Firm, and Crisis Period Results

Table A.17: Impact of Trade Policy Uncertainty on Firm Exports, by Sector

	(1) Firm TPU_{t-1}
Goods	1.561*
	(0.83)
Services	3.661**
	(1.69)
Agriculture, Forestry, Fishing and Hunting	2.020**
	(0.99)
Mining, Utilities, Contruction	3.646**
	(1.44)
Manufacturing	1.417*
	(0.83)
Wholesale, Retail, and Transportation	3.992**
	(1.60)
Professional Services	3.297*
	(1.87)
Education and Healthcare	2.995
	(3.11)
Arts and Entertainment	3.417**
	(1.49)
Other Services	3.731
	(2.59)
Firm FE?	Yes
Firm-Time Controls?	Yes
Country-Sector-Time FE?	Yes
Horse-Race Specifications?	Yes
IHS Transformation?	Yes

Notes: Standard errors are clustered at the country-time level between brackets, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18: Impact of Trade Policy Uncertainty, Across Firm Characteristics and Timing

	(1) Exports	(2) Exports	(3) Exports	(4) Exports	(5) IHS Exports	(6) IHS Exports	(7) IHS Exports	(8) IHS Exports
Firm TPU_{t-1}	0.260	0.351**	0.304*	0.411	1.732	2.029*	2.023*	2.536*
	(0.28)	(0.17)	(0.18)	(0.25)	(1.70)	(1.08)	(1.08)	(1.47)
Crisis x Firm TPU_{t-1}	0.072				0.378			
	(0.21)				(1.25)			
Size x Firm TPU_{t-1}		-0.060*				-0.053***		
		(0.03)				(0.01)		
Tenure x Firm TPU_{t-1}			0.005				-0.037*	
			(0.01)				(0.02)	
Goods x Firm TPU_{t-1}				-0.250				-1.484
				(0.24)				(1.47)
Observations	840670	840670	840670	840670	6521693	6521693	6521693	6521693
F	347.094	316.397	359.851	360.855	44.450	54.553	73.725	53.220
R-squared	0.884	0.884	0.884	0.884	0.763	0.763	0.763	0.763
R-squared within	0.007	0.007	0.007	0.007	0.004	0.004	0.004	0.004
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the country-time level between brackets, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

A.9.3 Separate Policy Uncertainty non-IHS Export Results

Table A.19: Trade Policy Results Across Domestic and External Policy Uncertainty

	(1) Sales	(2) Exports	(3) Domestic Sales	(4) Sales	(5) Exports	(6) Domestic Sales
Alpha x Domestic TPU_{t-1}	0.033	0.233	-0.141			
	(0.03)	(0.18)	(0.09)			
Alpha x External TPU_{t-1}				0.060**	0.340**	-0.185**
				(0.03)	(0.15)	(0.07)
Observations	6280569	840670	6242080	6280569	840670	6242080
F	1265.462	389.706	1041.519	1277.875	379.309	1093.988
R-squared	0.942	0.884	0.932	0.942	0.884	0.932
R-squared within	0.042	0.008	0.036	0.042	0.008	0.036
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time Controls?	Yes	Yes	Yes	Yes	Yes	Yes
+Firm-Time GDP Forecast Control?	Yes	Yes	Yes	Yes	Yes	Yes
Country-Sector-Time FE?	Yes	Yes	Yes	Yes	Yes	Yes

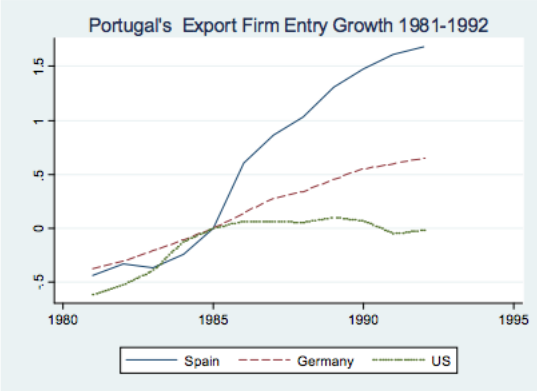
Notes: Standard errors are clustered at the country-time level between brackets, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

A.10 Handley & Limao Pre-Event Trends

The work of Handley & Limao (2013, 2015) and others suggest that a reduction in policy uncertainty (via trade agreements) induces more export participation and the introduction of new product lines. The results in this paper seem to contradict that. However, a figure from an early version of Handley & Limao's (2015) work shows evidence

of a pre-event positive uptick in growth in firm entry prior to the 1985 accession event (see below).[6] Thus, a positive response to anticipation of trade agreement resolution is not in contradiction to the empirical finding elsewhere that the relative slopes before and after agreements are reached do indeed significantly increase.

Figure A.2: Evidence of a Pre-trend in Handley and Limao's Sample



A.11 Connections to Sluggish Recoveries

Both France and the United Kingdom have seen sluggish growth since 2011 and 2009, respectively.[7]

Table A.20: Real GDP Growth Averages

	Sluggish Recovery	1998-2007
France	.6	2.4
U.K.	2.0	3.0

Table A.21: Change in French Policy Uncertainty, 2011-2015

Effective Macro PU	.69
External Macro PU	.31
Domestic Macro PU	-.38
Effective TPU	.49
External TPU	1.1
Domestic TPU	.6

[6]Figure 3 from the NBER 2013 version of Handley & Limao (2015).
[7]Based on IMF's October 2016 WEO database.

Table A.22: Change in U.K. Policy Uncertainty, 2010-2015

Effective Macro PU	.35
External Macro PU	.82
Domestic Macro PU	.47
Effective TPU	-.06
External TPU	.51
Domestic TPU	.57

Table A.23: Magnitude of Sluggish Growth Period Impacts on Sales Across Initial Shares

	Macro PU		TPU	
	France	U.K.	France	U.K.
75th Percentile	-2.00	-1.03	1.35	-.18
50th Percentile	-.41	-.21	.28	-.04

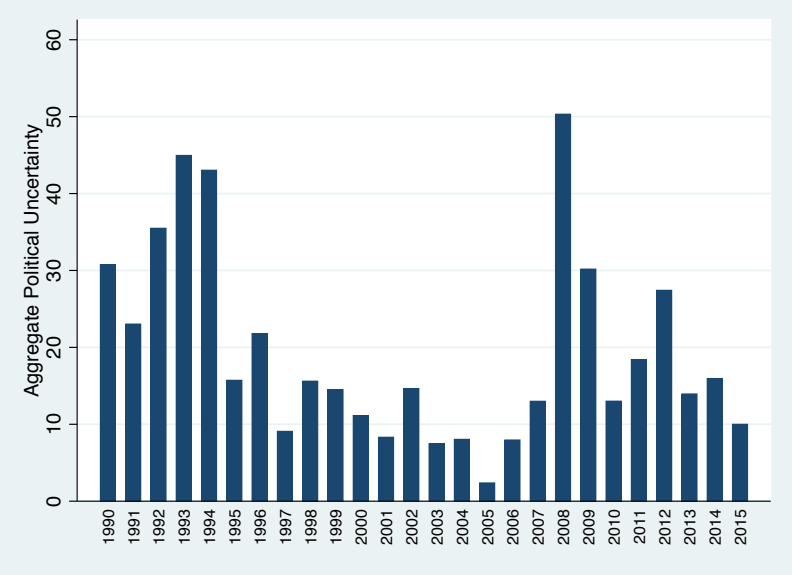
Table A.24: Initial Share & Mean Firm Correlations

	Mean Exports	Mean Sales
France	.34	.10
U.K.	.37	.12

Appendix B

Appendix Figures, Tables, and Materials for Chapter 2

Figure B.1: South African Political Uncertainty



Note: Figure displays annual averages of the quarterly measure of uncertainty.

B.1 Political Uncertainty and Sector-Level Uncertainty Examples

Aggregate Political Uncertainty

Aggregate political uncertainty was high around the transition period and, more recently, during the 2008 ANC Mbeki/Zuma factional split.

The correlation between this and the policy uncertainty measure is .75. When using the political uncertainty variable in the export empirical analysis, we find it to be insignificant.

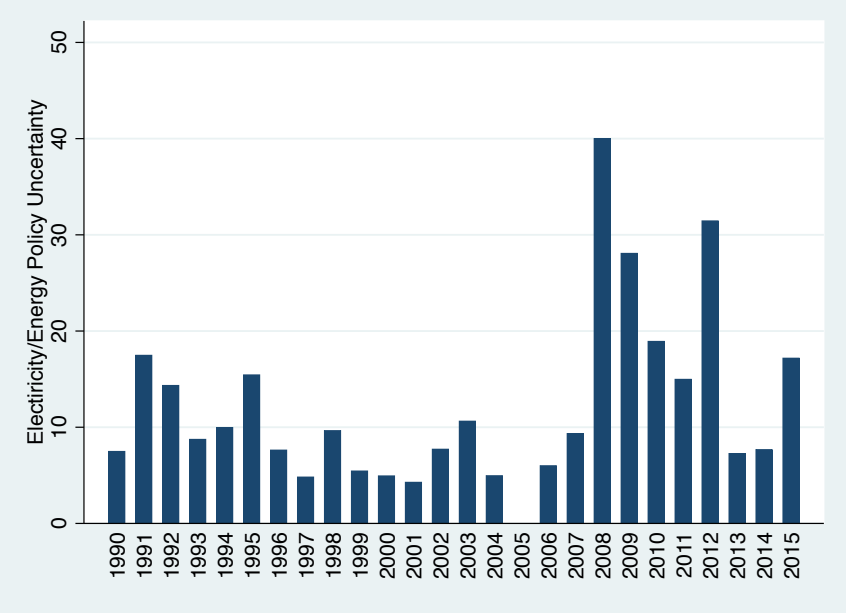
Sector Example I: Electricity/ Energy Sector

Electricity/energy policy uncertainty spikes at exactly the time that the South African national grid all but collapsed (late January 2008). Uncertainty has remained high since then. It spiked again in 2012 behind news that the new power stations were far behind schedule, over budget, and double-digit rate hikes were to become the norm going forward.

Sector Example II: Mining Sector

The mining sector's uncertainty index spikes during 2012, when South African police shot at 3,000 striking platinum miners at the Lonmin Marikana mine; 34 miners were killed. The strike accompanied several other large labor disputes in the mining sector

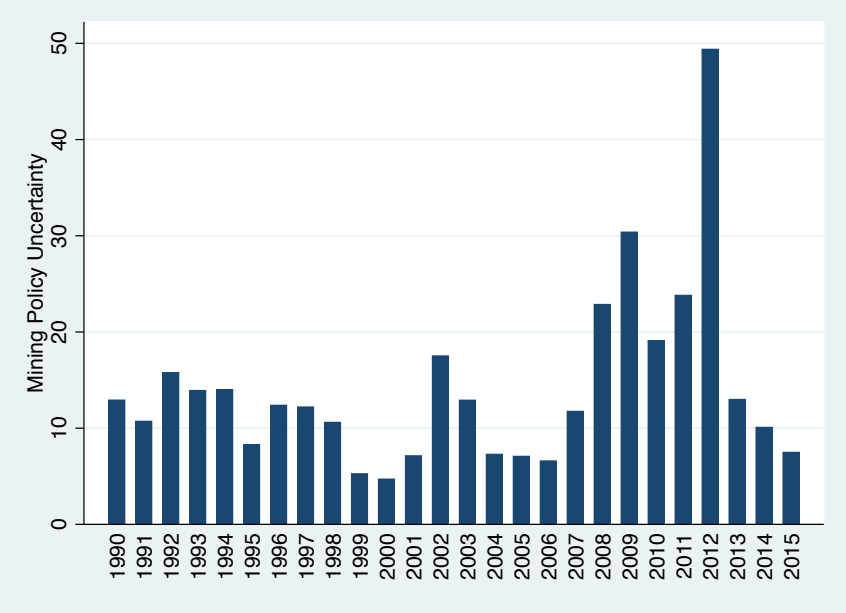
Figure B.2: Electricity Policy Uncertainty



Note: Figure displays annual averages of the quarterly measure of uncertainty.

that same year, in what was the amongst the most protest-heavy years since the end of apartheid.

Figure B.3: Mining Policy Uncertainty



Note: Figure displays annual averages of the quarterly measure of uncertainty.

Appendix C

Appendix Figures, Tables, and Materials for Chapter 3

C.1 Timeline of Election-Violence Related Events

Below is a timeline of key post-election events following the December 2007 Kenyan General Election.[1]

- December 27, 2007: General elections take place in Kenya.
- December 30, 2007: Incumbent President Mwai Kibaki of the Party of National Unity (PNU) is declared the winner of the general elections, though his 'victory' over opposition candidate Raila Odinga of the Orange Democratic Movement (ODM) amidst allegations of election fraud on both sides triggering outbreaks of violence.
- February 5, 2008: The International Criminal Court Prosecutor says his office has begun a preliminary examination of the post-election violence in Kenya.
- February 28, 2008: A mediation team, led by former UN Secretary-General Kofi Annan, oversees the signing of a power-sharing agreement called the National Accord and Reconciliation Act, which establishes a coalition government with Kibaki as president and Odinga as prime minister. It also set up the Commission of Inquiry on Post-Election Violence (CIPEV), which later became known as the Waki Commission after its chair, Judge Philip Waki.
- October 15, 2008: The Waki Commission submits its report and recommendations to the government of Kenya; recommendations include the establishment of a special tribunal of national and international judges to investigate and prosecute perpetrators of the post-election violence. The report also states that if the tribunal is not set up within six months, information collected by the Waki Commission will be passed to the ICC, including a sealed envelope of names of those suspected to be most responsible for the violence.
- November 11, 2008: ICC Prosecutor Luis Moreno Ocampo warns that he will take over the Kenyan case if the Waki Commission's deadlines for local tribunal are not met.
- February 12, 2009: The Kenyan parliament votes against the establishment of the proposed tribunal made up of Kenyan and international judges to address the post-election violence.
- July 3, 2009: Three Kenyan Cabinet ministers sign an agreement with the ICC committing Kenya to establish a credible and independent tribunal to try perpetrators of post-election violence by August.

[1]This timeline was largely constructed by the International Justice Monitor, a project of the Open Society Justice Initiative. However, it has been supplemented by additional key events from a team of international law consultants that I enlisted to help decipher some of the legal maneuvers of the ICC.

- July 8, 2009: Former UN Secretary-General Kofi Annan gives an envelope with the names of suspects to the ICC.
- July 16, 2009: The Prosecutor is sent six boxes containing documents and supporting materials compiled by the Waki Commission during its investigations. The documentation includes a sealed envelope that contains a list of suspects identified by the Waki Commission as those most responsible for the violence.
- September 30, 2009: Ocampo announces that the ICC will be prosecuting those responsible for 2007 post-election violence in Kenya because of the country's failure to establish its own tribunal.
- November 9, 2009: Parliament begins debate on another constitutional amendment to form a local tribunal.
- November 26, 2009: ICC Prosecutor Luis Moreno-Ocampo files a request seeking authorization from Pre-Trial Chamber II to open an investigation in relation to the crimes allegedly committed during the 2007-2008 post-election violence in Kenya.
- March 31, 2010: Pre-Trial Chamber II issues its majority decision (2-1) that there is a reasonable basis to proceed with an investigation into the situation in Kenya in relation to crimes against humanity within the jurisdiction of the Court committed between June 1, 2005 and November 26, 2009.
- September 21, 2010: International Criminal Court chief prosecutor Luis Moreno Ocampo said that he will present two separate cases to judges charging between four and six people.
- December 15, 2010: The ICC Prosecutor requests the issuance of 'summonses to appear' for six people in the court's Kenya investigation – William Samoei Ruto, Henry Kiprono Kosgey, Joshua arap Sang (case one) and Francis Kirimi Muthaura, Uhuru Muigai Kenyatta, and Mohamed Hussein Ali (case two) – for their alleged responsibility in the commission of crimes against humanity.
- March 8, 2011: Pre-Trial Chamber II issues the summonses to appear for the aforementioned six individuals, as it finds reasonable grounds to believe that they committed the crimes alleged by the Prosecutor.
- March 31, 2011: Kenyan government files an application challenging the ICC's jurisdiction over the cases.
- April 7, 2011: The first three defendants (Ruto, Kosgey, and Sang) make their initial appearance before the Court in The Hague.
- April 8, 2011: The second three defendants (Muthaura, Kenyatta, and Ali) make their initial appearance before the Court in The Hague.

- September 1, 2011: Confirmation of charges hearing begins for the first three defendants (Ruto, Kosgey, and Sang).
- September 8, 2011: Confirmation of charges hearing concludes for the first three defendants.
- September 21, 2011: Confirmation of charges hearing begins for the second three defendants (Muthaura, Kenyatta, and Ali).
- October 5, 2011: Confirmation of charges hearing concludes for the second three defendants.
- January 23, 2012: Pre-Trial Chamber II confirms charges against Ruto, Sang, Muthaura, and Kenyatta. Charges against Ali and Kosgey are rejected.
- January 26, 2012: Uhuru Kenyatta resigns as Finance Minister, and Francis Muthaura resigns as Head of Civil Service. Kenyatta keeps his post as Deputy Prime Minister.
- December 4, 2012: Kenyatta and William Ruto, who formerly belonged to a competing political party, form an alliance in advance of the March 2013 presidential election. Kenyatta runs as the presidential candidate with Ruto as his running mate.
- March 4, 2013: The presidential election is held in Kenya.
- March 8, 2013: The case involving Ruto is postponed.
- March 11, 2013: The Office of the Prosecutor drops all charges against Francis Muthaura after a key witness recanted his statements linking Muthaura to planning the 2007-2008 post-election violence.
- March 30, 2013: After receiving legal challenges to the poll results, the Supreme Court of Kenya validates the election of Kenyatta and Ruto as president and deputy president, respectively.
- May 6, 2013: The Ruto case is again postponed.
- April 9, 2013: Kenyatta and Ruto officially take office.
- June 18,2013: Trial Chamber V(a) rules, in a majority decision, that Ruto does not have to be continuously present at his trial in The Hague due to the exceptional nature of his position as a sitting deputy president. The prosecution appeals the decision, and Ruto is required to attend trial in person until the Appeals Chamber issued a judgment.
- September 10, 2013: The trial for Ruto and Sang begins. The trial was initially scheduled to start

- April 10, 2013. Then it was postponed to May 28. Judges ordered the postponements following defense requests for more time to prepare their case.
- October 18, 2013: Trial Chamber V(b) rules, in a majority decision, that Kenyatta does not have to be continuously present at his trial The Hague due to the exceptional nature of his position as a sitting head of state.
- December 19, 2013: The prosecution requests a three-month postponement in the case against Kenyatta. The trial was scheduled to begin February 5, 2014.
- February 5, 2014: Trial Chamber V(b) holds a status conference to discuss the prosecution request to postpone the trial. No new start date is scheduled at this time.
- September 5, 2014: The prosecution requests Trial Chamber V(b) to indefinitely adjourn the Kenyatta trial saying it is not in a position to proceed to trial due to lack of cooperation by the Kenyan government.
- October 8, 2014: Trial Chamber V(b) holds a status conference to discuss the prosecution's request for adjournment. The defense for Kenyatta asked the judges to terminate his case and enter a verdict of not guilty.
- December 5, 2014: The ICC prosecutor withdraws the charges against Kenyatta.

C.2 List of Strong and Weak ICC Shocks

For details on the ICC actions associated with the dates see Appendix 1.

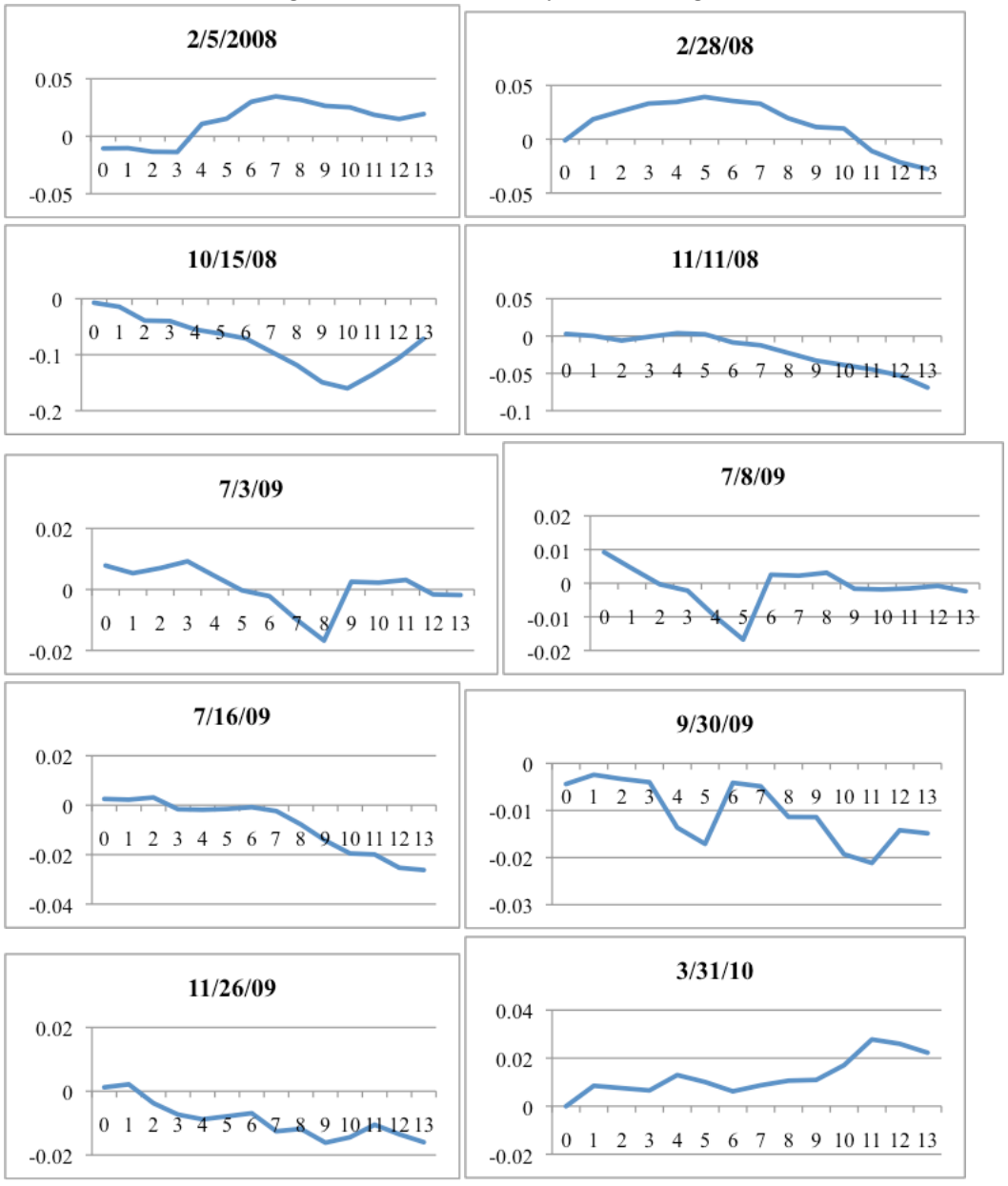
Strong ICC Shocks	**Weak ICC Shocks**
2/5/08	2/12/09
2/28/08	3/31/11
10/15/08	12/4/12
11/11/08	3/11/13
7/3/09	3/30/13
7/8/09	6/18/13 (wobbler)
7/16/09	3/8/13
9/30/09	5/6/13
1/26/09	10/18/13
3/31/10	12/19/13
9/21/10	2/5/14
12/15/10	9/5/14
3/8/11	10/8/14
4/7/11	12/5/14
9/1/11	
9/21/11	
10/5/11	
1/23/12	
1/26/12	
6/18/13 (wobbler)	
9/10/13	

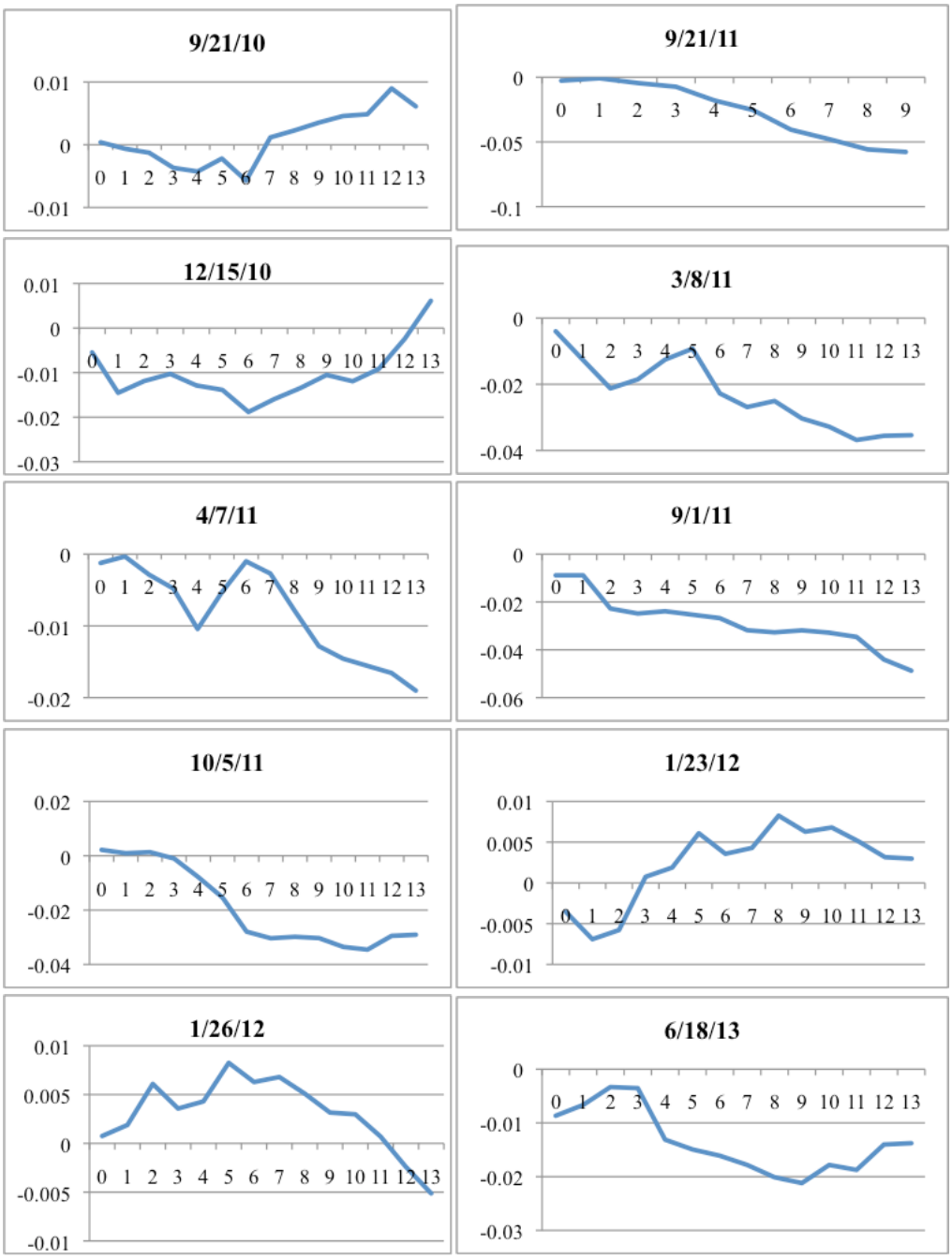
C.3 Kenyan News Shocks

- 5/5/2007 - Kenya Air Crash
- 12/30/2007- Election Violence
- 2/28/2008- Power-sharing deal
- 12/2/2008- Kibaki-Odinga MP Dismissal Debacle
- 1/26/2010- US aid suspended over corruption allegations
- 2/15/2010- Govt scandal
- 4/1/2010- New Constitution approved by Parliament
- 8/27/2010-New Constitution signed into law by President
- 10/19/2011- Kenya invades Somalia 3/26/2012- Oil discovered
- 12/2/2012- Kenyatta-Odinga alliance announced
- 3/9/2013- Kenyatta and Ruto win the executive office
- 9/21/2013- Westgate mall attack
- 11/30/2013-Monetary union agreement signed

C.4 10-Day Weighted Sample Average Cumulative Abnormal Returns, By Event Date

Abnormal returns weighted across firms by date using assets.




9/21/10
0.01
0
-0.01
0 1 2 3 4 5 6 7 8 9 10 11 12 13
9/21/11
0
-0.05
-0.1
0 1 2 3 4 5 6 7 8 9
12/15/10
0.01
0
-0.01
-0.02
-0.03
0 1 2 3 4 5 6 7 8 9 10 11 12 13
3/8/11
0
-0.02
-0.04
0 1 2 3 4 5 6 7 8 9 10 11 12 13
4/7/11
0
-0.01
-0.02
0 1 2 3 4 5 6 7 8 9 10 11 12 13
9/1/11
0
-0.02
-0.04
-0.06
0 1 2 3 4 5 6 7 8 9 10 11 12 13
10/5/11
0.02
0
-0.02
-0.04
0 1 2 3 4 5 6 7 8 9 10 11 12 13
1/23/12
0.01
0.005
0
-0.005
-0.01
0 1 2 3 4 5 6 7 8 9 10 11 12 13
1/26/12
0.01
0.005
0
-0.005
-0.01
0 1 2 3 4 5 6 7 8 9 10 11 12 13
6/18/13
0
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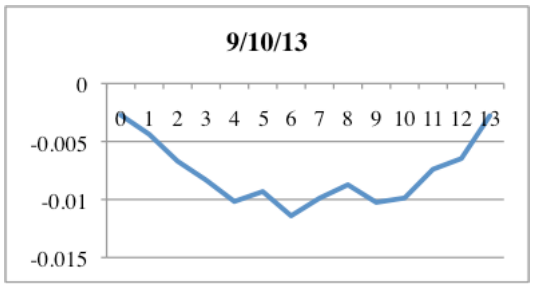
-0.02
-0.03
0 1 2 3 4 5 6 7 8 9 10 11 12 13


9/10/13
0
-0.005
-0.01
-0.015
0 1 2 3 4 5 6 7 8 9 10 11 12 13

C.5 Biographies of International Human Rights Legal Consultants

Tendayi Achiume earned her B.A. with distinction at Yale University in 2005 and received her J.D. from Yale Law School in 2008. While at law school, she also earned a Graduate Certificate in Development Studies from Yale, and was a member of the Lowenstein International Human Rights Clinic. Achiume served as Managing Editor of Submissions for the Yale Journal of International Law and was awarded the Howard M. Holtzmann Fellowship in International Arbitration and Dispute Resolution for her research on the role of transnational public policy in international arbitration. As a Kirby Simon summer fellow, Achiume worked at Human Rights Watch with the Hissène Habré prosecution team and then worked for a Senegalese human rights NGO. She worked as a law clerk to Deputy Chief Justice Moseneke and Justice Mokgoro on the Constitutional Court of South Africa. Following her clerkship, she received the 2009-2010 Bernstein International Human Rights Fellowship to spend a year working with Lawyers for Human Rights in its Johannesburg Refugee and Migrant Rights Project unit. Achiume also designed and taught a semester-long seminar in the International Human Rights Exchange Programme, which is based at the University of the Witswatersrand in Johannesburg and administered jointly with Bard College. Her publications include "Beyond Prejudice: Structural Xenophobic Discrimination Against Refugees," in 45(2) Georgetown Journal of International Law 323 (2014); and a co-authored piece, "Prison Conditions in South Africa and the Role of Public Interest Litigation Since 1994" in 27(1) South African Journal of Human Rights 183 (2011).

Ting Ting Chen is originally from Shanghai, China. She received her J.D. from the City University of New York School of Law where she was the Public Interest Practice Editor of the New York City Law Review and the Frank Durkan Fellow in Human Rights. Ting Ting was a 2009 Fulbright Scholar to South Africa, for which she received the Amy Biehl award. Ting Ting clerked at the Constitutional Court of South Africa for Justice Albie Sachs and Justice Edwin Cameron. During law school Ting Ting interned with the United Nations International Criminal Tribunal for Rwanda, the Center for Constitutional Rights, the Civil Rights Bureau of the Attorney General's Office, and the Innocence Project New Orleans. In addition, she worked for the U.N. Special Rapporteur on China for hearings in front of the Committee Against Torture and was a legal researcher for the U.N. Human Rights Committee. As a clinical extern she worked on Wiwa et al v. Royal Dutch Petroleum, a case against Shell Oil seeking corporate accountability under the Alien Tort Statute for its complicity in the hanging of Ken Saro-Wiwa and the "Ogoni Nine" and for human rights abuses in the Niger Delta.

Jacob Foster attended UCLA School of Law and represents both plaintiffs and defendants in complex litigation that includes contract disputes, consumer class actions, insurance recovery, and white collar criminal defense. Jake served in the Immediate Office of the Chief Prosecutor of the International Criminal Court on secondment in 2011,

where he worked on the investigation and prosecution of crimes against humanity, war crimes, and genocide around the world. He was profiled by the San Francisco Chronicle in the front page story "Seeking to Right Wrongs Against Humanity" and by the Daily Journal in the article "From Associate to War Crimes Prosecutor." He has spoken about the investigation and prosecution of atrocity crimes at Stanford Law School, Yale University, the American Bar Association, the World Affairs Council, and the Commonwealth Club of San Francisco.

Tyler Nims attended law school at Northwestern University School of Law. He currently clerks in the Chambers of the Honorable Raymond J. Dearie, Eastern District of New York. He has previously worked at the Constitutional Court of South Africa with Chief Justice S. Sandile Ngcobo and the United Nations International Criminal Tribunal for Rwanda.

C.6 Fama-McBeth and Cluster- Bootstrapped CAR Estimations

Table C.1: Cumulative Abnormal Return Fama-McBeth Results

	(1) CAR One	(2) CAR Seven	(3) CAR Ten	(4) CAR One	(5) CAR Seven	(6) CAR Ten	(7) CAR One	(8) CAR Seven	(9) CAR Ten
Government	0.00441	0.0318*	0.0393**						
	(0.00308)	(0.0166)	(0.0181)						
ICC Board				-0.0407***	-0.0976***	-0.120***			
				(0.00574)	(0.00986)	(0.0101)			
ICC Advisor							-0.00462	-0.0147*	-0.0177***
							(0.00403)	(0.00845)	(0.00613)
ln(Assets)	-0.00213*	-0.0100**	-0.00945**	-0.00135	-0.00589*	-0.00428	-0.00113	-0.00496*	-0.00323
	(0.00115)	(0.00434)	(0.00447)	(0.00118)	(0.00292)	(0.00275)	(0.00131)	(0.00251)	(0.00262)
Return on Assets	0.0102	0.0110	0.0152	0.0141*	0.0203	0.0258	0.0115	0.0135	0.0170
	(0.00787)	(0.0158)	(0.0183)	(0.00790)	(0.0161)	(0.0180)	(0.00849)	(0.0170)	(0.0192)
Constant	0.0229	0.126*	0.110	0.0164	0.0854	0.0588	0.01000	0.0646	0.0345
	(0.0191)	(0.0661)	(0.0688)	(0.0196)	(0.0503)	(0.0507)	(0.0217)	(0.0448)	(0.0484)
Observations	856	856	856	856	856	856	856	856	856
adj. R^2									

Notes: Fama McBeth standard errors in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

Table C.2: Cumulative Abnormal Return Panel Results, Bootstrapped Check

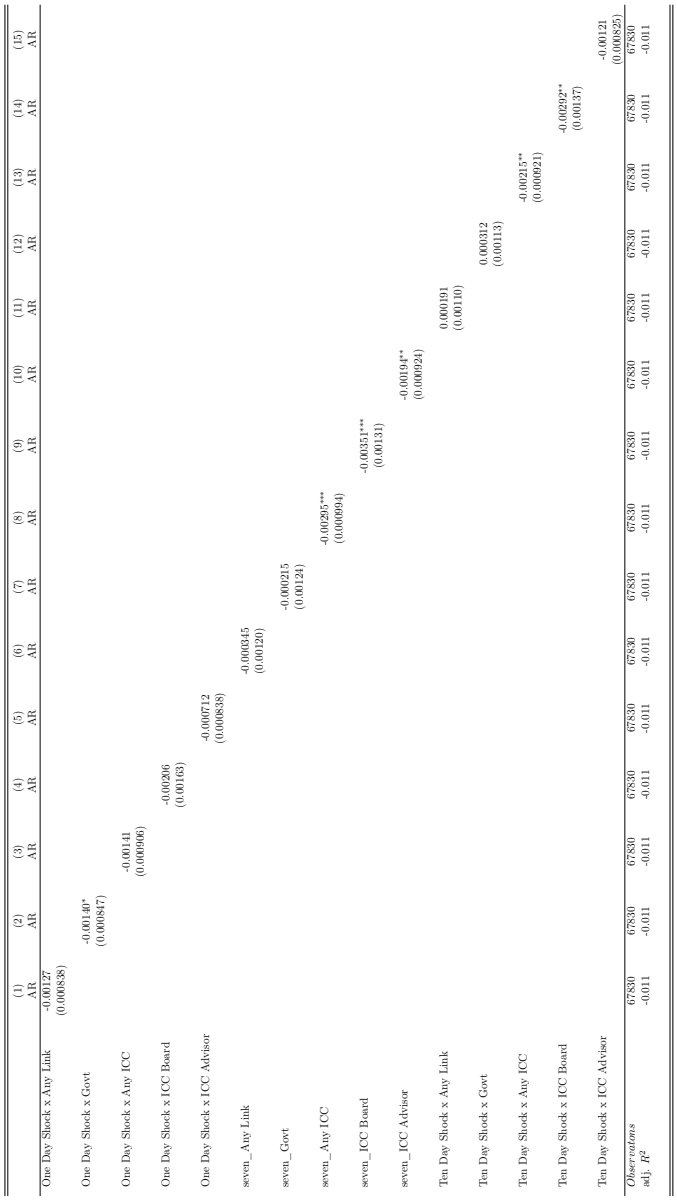
	(1) CAR One	(2) CAR Seven	(3) CAR Ten	(4) CAR One	(5) CAR Seven	(6) CAR Ten	(7) CAR One	(8) CAR Seven	(9) CAR Ten
Government	0.00515*	0.0319*	0.0394**						
	(0.00312)	(0.0176)	(0.0184)						
ICC Board				-0.0406***	-0.0979***	-0.120***			
				(0.00578)	(0.00974)	(0.00950)			
ICC Advisor							-0.00438	-0.0169*	-0.0197***
							(0.00396)	(0.00952)	(0.00643)
ln(Assets)	-0.00255**	-0.00963**	-0.00912**	-0.00171*	-0.00559**	-0.00413	-0.00155	-0.00453**	-0.00294
	(0.00103)	(0.00429)	(0.00442)	(0.000945)	(0.00272)	(0.00265)	(0.00117)	(0.00226)	(0.00256)
Return on Assets	0.00208	-0.00614	-0.00516	0.00221	-0.00544	-0.00430	0.00232	-0.00500	-0.00378
	(0.00265)	(0.00805)	(0.00953)	(0.00237)	(0.00902)	(0.00898)	(0.00282)	(0.00776)	(0.00933)
Constant	0.0296*	0.122*	0.107	0.0229	0.0833*	0.0592	0.0172	0.0599	0.0320
	(0.0179)	(0.0666)	(0.0687)	(0.0167)	(0.0486)	(0.0490)	(0.0208)	(0.0419)	(0.0468)
N	856	856	856	856	856	856	856	856	856
adj. R^2	0.003	0.006	0.007	0.039	0.023	0.031	0.002	0.001	-0.000

Notes: Bootstrapped clustered standard errors in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

C.7 Traditional Multi-Shock, Multi-Group Difference-in-Difference Results

Below are estimation results for the more traditional diff-in-diff estimate for multiple groups and shocks, with time and firm fixed effects and double clustered standard errors.

Table C.3: Traditional Multi-Shock, Multi-Group Difference-in-Difference Results



	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR	(7) AR	(8) AR	(9) AR	(10) AR	(11) AR	(12) AR	(13) AR	(14) AR	(15) AR
One Day Shock x Any Link	-0.00127														
	(0.000838)														
One Day Shock x Govt		-0.00140*													
		(0.000847)													
One Day Shock x Any ICC			-0.00141												
			(0.000906)												
One Day Shock x ICC Board				-0.00206											
				(0.00165)											
One Day Shock x ICC Advisor					-0.000712										
					(0.000838)										
seven_Any Link						-0.000345									
						(0.00120)									
seven_Govt							-0.000215								
							(0.00124)								
seven_Any ICC								-0.00295***							
								(0.000994)							
seven_ICC Board									-0.00351***						
									(0.00131)						
seven_ICC Advisor										-0.00194**					
										(0.000924)					
Ten Day Shock x Any Link											0.00191				
											(0.00110)				
Ten Day Shock x Govt												0.000312			
												(0.00113)			
Ten Day Shock x Any ICC													-0.00215**		
													(0.000921)		
Ten Day Shock x ICC Board														-0.00292**	
														(0.00137)	
Ten Day Shock x ICC Advisor															-0.00121
															(0.000825)
Observations	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830	67830
adj. R^2	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011	-0.011

Notes: All estimations include firm fixed effects and constants (not shown). Standard errors clustered by date and firm are in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*

C.8 Thinly Traded Firms Robustness Check Results

Table C.4: Thinly Traded Difference-in-Difference Results

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR	(7) AR	(8) AR	(9) AR	(10) AR	(11) AR	(12) AR	(13) AR	(14) AR	(15) AR
onedayshock	-0.000390	-0.000340	-0.000902	-0.00127	-0.00121										
	(0.00137)	(0.00132)	(0.00111)	(0.00101)	(0.00104)										
One Day Shock x Any Link	-0.00179														
	(0.00133)														
One Day Shock x Govt		-0.00196													
		(0.00133)													
One Day Shock x Any ICC			-0.00178												
			(0.00122)												
One Day Shock x ICC Board				-0.00205											
				(0.00188)											
One Day Shock x ICC Advisor					-0.00119										
					(0.00115)										
sevendayshock						-0.00129	-0.00144	-0.000117	-0.000990	-0.000752					
						(0.000950)	(0.000919)	(0.00145)	(0.00120)	(0.00133)					
seven_Any Link						-0.000179									
						(0.00171)									
seven_Govt							0.0000706								
							(0.00176)								
seven_Any ICC								-0.00398***							
								(0.00152)							
seven_ICC Board									-0.00414**						
									(0.00169)						
seven_ICC Advisor										-0.00288**					
										(0.00144)					
tendayshock											-0.00139	-0.00150	-0.000109	-0.000719	-0.000587
											(0.00110)	(0.00104)	(0.00134)	(0.00111)	(0.00121)
Ten Day Shock x Any Link											0.000594				
											(0.00159)				
Ten Day Shock x Govt												0.000812			
												(0.00161)			
Ten Day Shock x Any ICC													-0.00285**		
													(0.00135)		
Ten Day Shock x ICC Board														-0.00316**	
														(0.00155)	
Ten Day Shock x ICC Advisor															-0.00198
															(0.00125)
N	63435	63435	63435	63435	63435	63435	63435	63435	63435	63435	63435	63435	63435	63435	63435
adj. R^2	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001

Notes: All estimations include firm fixed effects and constants (not shown). Standard errors clustered by date and firm are in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Thinly Traded Cumulative Abnormal Return Panel Results, Clustering by Date

	(1) CAR One	(2) CAR Seven	(3) CAR Ten	(4) CAR One	(5) CAR Seven	(6) CAR Ten	(7) CAR One	(8) CAR Seven	(9) CAR Ten
Government	0.00473	0.0393	0.0487						
	(0.00425)	(0.0282)	(0.0300)						
ICC Board				-0.0410***	-0.111***	-0.129***			
				(0.00922)	(0.0193)	(0.0221)			
ICC Advisor							-0.00933	-0.0204*	-0.0262***
							(0.00551)	(0.0107)	(0.00918)
ln(Assets)	-0.00323*	-0.0145	-0.0123	-0.00219	-0.00892	-0.00551	-0.00159	-0.00788	-0.00399
	(0.00181)	(0.00864)	(0.00906)	(0.00172)	(0.00587)	(0.00610)	(0.00189)	(0.00589)	(0.00619)
Return on Assets	0.00648*	-0.00968	0.00219	0.00703**	-0.00764	0.00463	0.00683*	-0.00830	0.00392
	(0.00334)	(0.00909)	(0.00986)	(0.00304)	(0.00802)	(0.00863)	(0.00354)	(0.00867)	(0.00952)
Constant	0.0393	0.200	0.154	0.0295	0.144	0.0852	0.0172	0.119	0.0520
	(0.0319)	(0.137)	(0.143)	(0.0309)	(0.108)	(0.112)	(0.0338)	(0.108)	(0.113)
Observations	502	502	502	502	502	502	502	502	502
adj. R^2	0.005	0.005	0.005	0.048	0.020	0.024	0.007	-0.000	-0.002

Notes: Significance of coefficients based on Bell and McCraffy (2002) adjustment. Standard errors, clustered by date, in parentheses, ** p < 0.10, ** p < 0.05, *** p < 0.01.*