Uncertainty, Real Options, and Firm Inaction: Evidence from Monthly Plant-Level Data*

Daniel Carvalho†

Using monthly plant-level data on Brazilian manufacturing, I examine how firms open, close and adjust the size of their existing plants around political events expected to significantly reduce political uncertainty. The analysis uses the fact that this resolution of political uncertainty leads to a drop in exchange rate volatility. I combine within-industry variation in the firm-level importance of both exports and imports to estimate how uncertainty affects firms' decisions while controlling for first-moment effects. I find that uncertainty leads firms to significantly reduce both plant expansions and contractions. These effects are mostly important for large discrete decisions which are plausibly harder to reverse. The results also suggest that firms' decisions to expand and contract their plants are less responsive to changes in business conditions in the presence of higher uncertainty. Overall, the evidence broadly supports the predictions of real-options effects where higher uncertainty creates an incentive for firms to wait and become inactive.

* I would like to thank Nick Bloom, Harry DeAngelo, Jerry Hoberg, Nir Jaimovich, Hynseob Kim, Oguzhan Ozbas, Gordon Phillips, Toni Whited, an anonymous referee, and seminar participants at USC Marshall for helpful comments.
† USC Marshall School of Business. E-mail: daniel.carvalho@marshall.usc.edu.

Understanding how uncertainty affects decisions has long been a topic of interest in finance and economics. A large body of research builds on the idea that uncertainty induces firms to delay important decisions that are costly to reverse such as investment through real-options effects. When facing such decisions under uncertainty, firms might prefer to become inactive and wait for fundamentals to more clearly determine their optimal choice. These wait-and-see incentives are a central idea in applications of real options to a range of issues from the optimal choice of investment projects and their valuation to the analysis of aggregate and firm investment dynamics.[1] Moreover, these inaction incentives can have important economic consequences in periods of high uncertainty. Intuitively and in models formalizing this wait-and-see idea, greater inaction makes firms' decisions less responsive to differences in fundamentals such as a drop in the cost of capital. As a consequence, it can lead the economy to become significantly less responsive to policies such as monetary policy during recessions and uncertain times. It also limits the reallocation of resources such as capital across firms and can lead higher uncertainty to directly translate into lower aggregate productivity and lower aggregate output over short time horizons.[2]

This paper studies how uncertainty affects firms' decisions to open, close, and adjust the size of existing plants using monthly plant-level data and provides broad support for several key predictions of these wait-and-see effects. While previous research has emphasized how real-option considerations should affect firm decisions under uncertainty, we still have limited evidence that these incentives do play a first-order role in determining firms' decisions in practice. Previous empirical evidence on the effect of uncertainty at the firm level has typically focused on the investment-uncertainty relationship. This research has documented that higher uncertainty is usually associated with lower investment.[3] However, this effect can be driven by multiple alternative channels. For example, higher uncertainty might lead managers to apply

[1] See Dixit and Pindyck (1994) and Bloom (2014) and the references therein for detailed overviews of this literature. See Trigeorgis (1996) for applications of real options to the optimal choice and valuation of investment projects which date back at least to Brennan and Schwartz (1985) and McDonald and Siegel (1986). This approach is commonly covered in corporate finance textbooks (e.g., Brealey, Myers, and Allen (2008)). See Caballero and Engel (1999); Grenardier (2002); and Aguerrevere (2003) for applications of inaction incentives and real options to the analysis of investment dynamics.

[2] The idea that such inaction effects lead to limited firm responsiveness to fundamentals has been emphasized at least since Bernanke (1983). See Bloom (2009); Vavra (2013); and Bloom et al. (2014) for analyses of these economic implications of firm inaction due to uncertainty. These economic implications of inaction incentives have also been emphasized by policymakers (e.g., Blanchard (2009)).

[3] For example, see the references in Bloom (2014) and the text below.

higher discount rates to riskier future cash flows when valuing incremental investments (e.g., Panousi and Papanikolaou (2012)). Alternatively, higher uncertainty can increase firms' risk of financial distress and their cost of external financing (Gilchrist, Sim, and Zakrasjek (2011), and Christiano, Motto, and Rostagno (2014)). Therefore, a negative sign for the investment-uncertainty relationship does not necessarily capture inaction incentives. Moreover, to the extent that uncertainty matters through discount rates or financing frictions, it has different economic implications. For example, if uncertainty matters through financing frictions, economic conditions might become more sensitive to policies addressing such frictions in periods of high uncertainty. Isolating the empirical significance of wait-and-see effects is important because, under broad conditions, theory makes unclear predictions for the importance of such effects (e.g., Abel et al. (1996), and Bar-Ilan and Strange (1996)).

To study the above issue, I examine detailed predictions of wait-and-see effects for how uncertainty should affect firms' decisions to adjust their plants across multiple margins. These adjustments include both decisions to expand and contract plants as well as actions which are plausibly more and less expensive to reverse. A key prediction from inaction incentives is that uncertainty about operating conditions should not only affect firms' decisions to expand their business but induce firms to freeze adjustments in both directions (expansions and contractions). Intuitively, firms facing better (worse) operating conditions can increase their short-term profits by expanding (contracting). However, because of uncertainty, these conditions might change and firms might need to reverse their initial decisions. If both decisions to expand and contract are costly to reverse, higher uncertainty increases the value of waiting before making each of these decisions.

A related central prediction from inaction incentives is that the previous effects should be mostly important for decisions which are harder to reverse because of adjustment costs. This should be the case for costly adjustments which are largely discrete in nature or lumpy because of fixed adjustment costs. I examine firms' decisions to open and close plants, which are important discrete choices by firms and are plausibly exposed to sizeable fixed adjustment costs. In the context of these decisions, a key prediction from inaction incentives is that uncertainty should lead firms to freeze both decisions to open and close plants. Moreover, these effects should be especially important for firms with fewer plants, where the discreteness and fixed costs associated with these actions is more prominent. I contrast these predictions with the effect of uncertainty on intensive-margin adjustments to the size of plants. These continuous adjustments are plausibly easier to reverse and should be less affected by inaction incentives.

As previously discussed, another key prediction from inaction incentives is that uncertainty should make firms less responsive to changes in business conditions. These effects should also be mostly important for decisions which are harder to reverse and affect both expansions and contractions. These multiple predictions from inaction incentives are not easily replicated by alternative channels through which uncertainty might matter. For example, an increase in discount rates will induce firms to open fewer new plants but also will increase incentives to close existing plants today.[4]

These predictions from inaction incentives are studied by analyzing firms' decisions with monthly plant-level data around significant and predictable drops in political uncertainty. I use political events determined far in the past to predict the precise timing of these reductions in political uncertainty. The use of high-frequency data allows me to examine sharp changes in firm decisions around the expected resolution of uncertainty. As firms expect uncertainty to be resolved, inaction should sharply reduce after the resolution of uncertainty. Given that the timing of these events is not affected by economic shocks, one can arguably isolate the effect of these events by studying firms' decisions in a short window of time around them. This approach also allows one to separate changes in expected uncertainty, which will shift around such short windows, from changes in realized volatility. For example, in model simulations, Bloom (2009) shows that such realized volatility effects will confound the effects of changes in uncertainty after three to four months.

This analysis is implemented in the context of Brazilian manufacturing, where there is significant within-industry variation in firm exposure to political uncertainty. Important economic and political news is often associated with significant changes in exchange rates due to capital inflows and outflows in Brazil. This leads to high levels of political uncertainty being an important source of exchange rate volatility. In order to estimate the effect of changes in

[4] See Dixit (1989) and Dixit and Pyndick (1994) for models of wait-and-see effects in the context of discrete choices such as entry and exit decisions. See Bloom et al. (2007) and Bloom (2009) for models of these effects in the context of continuous decisions exposed to nonconvex adjustment costs such as fixed costs.

uncertainty around these events, I use within-industry variation in the firm-level importance of international trade to predict differences in firms' exposure to exchange rate volatility.

A fundamental identification challenge in this analysis is controlling for the potential effects of changes in firms' expectations about first moments around the previous events. For example, exporting firms which are exposed to exchange rate volatility are also exposed to changes in the expected level of exchange rates. I first document that drops in exchange rate volatility are significantly more important than changes in the level of exchange rates in narrow time windows around these events. I then propose an approach to control for such first-moment effects by combining variation in the firm-level importance of both exports and imports. This approach builds on the fact that exporters and importers are affected in the opposite way by shocks to the level of exchange rates, but are both affected in the same direction by exchange rate volatility. I first estimate the effect greater export dependence on changes in the decisions of exporters around the events and the analogous effect of greater import dependence for importers. I then analyze how fluctuations in uncertainty affect firms' decisions by computing an average between these two estimates. Intuitively, the effects of first-moment shocks to the exchange rate on exporters and importers offset each other when this average is computed. I provide different sources of evidence to support this last hypothesis including direct evidence from a large unexpected devaluation of the exchange rate.

Both the availability of detailed high-frequency plant-level data and the previous within-industry variation in firm exposure to political uncertainty represent important advantages from focusing on the empirical setting used in this paper. An additional advantage of studying this issue in the context of Brazil is the existence of important political events associated with large changes in political uncertainty. I focus the analysis on events associated with the first election of President Lula in 2002. Both the time-series of exchange rate volatility and anecdotal evidence suggest that this election stands out as a major source of political uncertainty in Brazil during the sample period. Because of the historical policy prescriptions of his party, there was a first-order concern that the election of Lula could undermine macroeconomic stability and there was significant uncertainty about his economic policy choices.

Following the previously outlined approach, I find evidence that higher uncertainty significantly reduces the likelihood that firms both open and close plants around these political events. For the average firm exposed to international trade, these effects represent an increase of 50.4% and 17.0% in plant openings and closures, respectively, during a four-month period immediately after the resolution of uncertainty. I also find that these effects are mostly important for large discrete decisions which are plausibly harder to reverse. Both effects are only important for firms with fewer plants, where the opening or closure of a given plant is plausibly associated with greater discrete costs. These results are matched with economically and statistically more limited effects on continuous intensive-margin adjustments to plant size. Additionally, I find direct support to the idea that plant openings and closures are less sensitive to changes in business conditions in the presence of higher uncertainty. More specifically, I find that the previous negative effects of uncertainty on plant openings and closures are only important for industries experiencing positive and negative revenue shocks, respectively. These last patterns are also more limited for intensive-margin adjustments to plant size.

When measuring both the timing of plant openings and closures and intensive-margin adjustments to plant size, I focus on adjustments to plant employment. This ensures that the previous analysis captures adjustments to the same production factor (labor) across different margins. The estimated limited effects for intensive-margin adjustments to plant employment are consistent with the facts that labor turnover is very high in Brazil relative to other countries and that Brazilian manufacturing firms intensely readjust their labor through these margins.

After presenting the main results, I implement several robustness checks on the basic approach used to estimate the effect of uncertainty in this paper. As previously described, I provide direct evidence suggesting that this approach controls for the effects of changes in the expected level of exchange rates. An additional concern is that greater firm export and import dependence are both correlated with firm characteristics that predict firms' exposure to other sources of first-moment effects. For example, these events might lead firms to update their expectation about future economic growth and both firm export and import dependence might be associated with greater human capital intensity. I also address this possibility in multiple ways. I show that the main results in the paper are not significantly affected by the inclusion of many important firm- and plant-level controls. I also implement falsifications tests, including an analysis of firms which both export and import. Intuitively, when estimated over a fixed set of firms exposed to these two margins of trade, the marginal effects of greater export and import dependence should offset each other if they are driven by exchange-rate considerations. In contrast, if greater export and import dependence drive the results through other channels, one should not expect the last two marginal effects to offset each other. Instead, the average between these two effects should be associated with important changes on plant adjustments. I find that this is not the case around the same set of events analyzed in the main results. While completely ruling out concerns about first-moment effects is challenging, the analysis in this paper suggests that the results capture the effect of fluctuations on uncertainty.

Overall, this paper contributes to previous research analyzing how uncertainty, including political uncertainty, and real-option considerations affect firms' decisions. The first contribution of this paper is to provide plant-level evidence that the effect of uncertainty on firms' decisions is consistent with several detailed predictions of wait-and-see effects. These predictions capture key features from inaction incentives and consider how firms adjust their plants across multiple margins, including both decisions to expand and contract, and adjustments which are plausibly more and less expensive to reverse. As previously discussed, existing research on the firm-level effects of uncertainty has typically focused on analyzing the link between uncertainty and investment or related measures of firm growth.[5] A related line of research has examined the importance of real-option considerations for understanding firm decisions in natural resource industries (e.g., Moel and Tufano (2002) and Kellogg (2014)). This research typically uses time-series variation to identify the relationship between the volatility of commodity prices or other conditions and firm decisions. To the best of my knowledge, this literature has also not considered firms' adjustments across several different margins along the lines of this paper.[6] The second contribution of this paper is to provide evidence that uncertainty can have a first-order effect on firms' extensive-margin decisions to operate a plant. These results also contribute to previous research analyzing firms' decisions to restructure their operations at the plant level through plant closures (Kovenock and Phillips (1997)) or related actions such as plant sales (Maksimovic and Phillips (2001), and Yang (2008)) and within-plant adjustments (Giroud and

[5] For example, see Leahy and Whited (1996); Guiso and Parigi (1999); Bloom, Bond, and Van Reenen (2007); Julio and Yook (2012); Stein and Stone (2012); Kim and Kung (2014); Gulen and Ion (2015); and Jens (2016). When analyzing the determinants of the investment-uncertainty relationship, this research typically examines broad cross-sectional patterns in the significance of this link (e.g., Kim and Kung (2014) and Gulen and Ion (2015)).

[6] A few previous studies (Moel and Tufano (2002) and Jens (2016)) examine the link between uncertainty and firm decisions to both expand and contract their operations in one margin but all find mixed evidence on the hypothesis that uncertainty freezes adjustments in both directions. One issue that limits the ability of these studies to examine this hypothesis is reduced data availability on both expansions and contractions.

Mueller (2015)). Finally, this paper also complements previous work linking politics to the real behavior of firms (Bertrand et al. (2007), and Carvalho (2014)) and connecting political uncertainty to aggregate-level outcomes (e.g., Alesina and Perroti (1996), and Baker, Bloom, and Davis (2013)).

1. Political Uncertainty and Exchange Rate Volatility in Brazil

The analysis in this paper examines the differential behavior of firms with greater exposure to exchange rate volatility around political events predicted to significantly reduce political uncertainty. An important component of this analysis is finding national political events that significantly mattered for foreign investors and exchange rates. Brazil has a democratic regime with a federal structure and a presidential multi-party political system since the early 1990s. Anecdotal evidence suggests a major source of political uncertainty affecting foreign investors during this period was the first election of President Lula in 2002. The data used in this paper is available between 1995 and 2006. Between 1995 and 1998, Brazil had a fixed exchange rate regime and there were no fluctuations in exchange rate volatility. This limits the search for political events to the period between 1999 and 2006. As I discuss below, the uncertainty associated with the election of President Lula also stands out in the time series of exchange rate volatility as a major source of increase in this volatility between 1999 and 2006.

During the period leading to this election and the start of President Lula's first term, there were two major sources of political uncertainty. First, there was some uncertainty about the winner of the election: Lula or the incumbent party's candidate. Second, there was significant uncertainty about the type of economic policies to be chosen by Lula if elected. After three failed attempts in presidential elections, Lula was seen in the second semester of 2002 as finally having a high chance of winning the presidential election taking place in October 2002. A key component of this election, relative to his previous campaigns, had been Lula's explicit emphasis on keeping macroeconomic stability. Brazil had very high inflation until 1994 when a macroeconomic program successfully stabilized the economy. After eight years of macroeconomic stability, the election of Lula was seen by many at the time, especially foreign investors, as a major threat to the achieved macroeconomic stability. Major sources of concern were the previous economic policy positions of his party, the Brazilian Labor Party. While Lula had shifted toward more conservative fiscal and macroeconomic policies, his party had explicitly rejected such policies in the recent past.

Anecdotal evidence suggests that both of these sources of uncertainty were important and mattered for exchange rates. For example, in June of 2002, Goldman Sachs (2002) presented a new tool to follow the currency market perception of a Lula victory in the upcoming election. This tool was labeled the "Lulameter" and described as estimating "a market-implied probability of a Lula victory and where the BRL [Brazilian currency] trade would be following the election". Goldman Sachs (2002) emphasizes the point that "investors should protect against BRL exposure into the elections period".

This tool illustrates how investors associated different electoral outcomes with different implications for the Brazilian exchange rate. Even when the outcome of the election was significantly less uncertain, investors still faced significant uncertainty about which policies Lula would choose. For example, in early October 2002, *The Economist* (2002) explained that the Brazilian currency "has fallen sharply against the American dollar" because of the prospect of a Lula victory. *The Economist* (2002) states that "if he wins, Mr. Da Silva [Lula] is likely to come under immediate pressure to show he intends to live up to his pre-election promises". *The Economist* (2002) also argues that Lula "may not have much time to do this, and will probably have to do something to reassure the markets before he formally takes power next January".

Both these sources of uncertainty were predicted to be significantly reduced by political events in the period between the end of October 2002 and the end of December 2002. The first political event predicted to reduce the previous sources of uncertainty during this period is the election taking place in October. While elected at the end of October, President Lula would start his term in the beginning of January 2003. Newly elected presidents typically nominate the main members for their cabinet during this period between national elections and the start of their term. In the case of Lula, these nominations would clearly indicate his broad economic policy choices. Indeed, during this period, Lula attempted to further signal his commitment to macroeconomic stability by nominating the former president of FleetBoston's Corporate and Global Bank as the Brazilian central-bank governor, for example. In his confirmation hearing in the Brazilian senate in December 2002, the new central-bank governor emphasized the importance of macroeconomic stability.

Motivated by the previous timeline determined far in the past, I use the period between October and December 2002 as a period expected to significantly reduce political uncertainty. Panel A of Figure 1 plots the time series of the volatility of the Brazilian exchange rate between 1999 and 2006. For each month, the exchange rate volatility is computed as the annualized standard deviation of daily log changes in the exchange rate. The figure plots the monthly exchange rate volatility divided by the average value of this variable between July 2002 and June 2003. The six-month periods prior to the resolution of uncertainty (July 2002 to December 2002) and after uncertainty is resolved (January 2003 to July 2003) are shown by the vertical lines. The patterns in this figure suggest the economic importance of political uncertainty near the election of President Lula. As the figure illustrates, there is a fivefold increase in exchange rate volatility in the period leading to the beginning of Lula's first term. The figure also illustrates how this increase in exchange rate volatility stands out as the main shock to exchange rate volatility during the sample period. Indeed, the exchange rate volatility of the Brazilian currency reached its second highest monthly value between 1995 and 2014 in August 2002. This pattern of uncertainty increasing prior to pivotal political events is consistent with the evidence of Kelly, Pastor, and Veronesi (2015) across a broader range of pre-determined pivotal political events.

The patterns documented in Panel A also confirm the view that this high level of political uncertainty is significantly reduced after the beginning of Lula's first term (January 2003 to July 2003). The volatility of the Brazilian exchange rate is significantly lower in this period and remains lower in a persistent way afterward. This supports the view that political uncertainty is largely reduced in response to the political events in the end of 2002. Panel B shows this same pattern in greater detail by focusing on the 12-month period shown by the vertical lines in Panel A. Panel C shows these patterns in a narrower 6-month window around the expected resolution of uncertainty. In these last two panels, the month representing the end of the predicted resolution of uncertainty (December 2002) is shown by the vertical line. These graphs suggest a quantitative important and sharp drop in exchange rate volatility over a short period of time.

Panel A of Table 1 quantifies the magnitude of this drop in exchange rate volatility. *Log(ExVol)* is the log of the monthly exchange rate volatility. *Post* - *Pre* reports the difference between the average value of this variable in the subsamples *Post*=1 and *Pre*=1. *Pre* and *Pos*t are indicators that equal one in the period prior to uncertainty resolution (the last months of 2002) and the period after uncertainty is resolved (the first months of 2003), respectively. Across different time windows, the resolution of political uncertainty is associated with an economically large drop in exchange rate volatility. For example, exchange rate volatility in the three months after uncertainty resolution is 43.2% lower relative to the same volatility in the previous three months (6-month window).

This evidence motivates the focus of this paper on the political events surrounding the first election of President Lula. This focus on economically large shifts in uncertainty helps ensure that, to the extent that wait-and-see effects are important, they should be detected in the data.[7] These fluctuations in uncertainty take place over narrow periods of time with a pre-determined timing that is not affected by economic shocks. These facts are important for identification purposes. In the analysis that follows, I examine changes in firms' decisions around such narrow periods of time. As previously discussed, a fundamental issue in this analysis is controlling for the potential effects of changes in firms' expectations about first moments caused by these events. Before proposing and implementing an approach to explicitly control for such first-moment effects, I examine changes in the level of the Brazilian exchange rate around the previously documented drops in exchange rate volatility.

Figure 2 plots the level of the Brazilian exchange rate over the same periods analyzed in Panels B and C of Figure 1. For each month, the exchange rate is computed as the average value of the daily nominal exchange rate (R$/U.S. Dollar). Across all panels, the figure plots the monthly exchange rate divided by the average value of this variable between July 2002 and June 2003. This normalization is also implemented in Figure 1 and makes the magnitudes of the changes in Figure 2 directly comparable in percentage terms to the ones in Figure 1. Figure 2 suggests that the previous sharp changes in exchange rate volatility are matched with limited and economically much smaller changes in the level of exchange rates. Panel B of Table 1 quantifies the magnitude of the changes in the level of the exchange rate over this period in an analogous way to the analysis in Panel A. Across different windows of time, there is an appreciation of the Brazilian exchange rate which is approximately nine times smaller in magnitude than the previous drop in volatility. The absolute magnitude of this appreciation is approximately 5%

[7] This focus on economically important events is similar to the one in the literature on credit supply shocks (e.g., Kwhaja and Mian (2008)). While Figure 1 suggests two other spikes in exchange rate volatility, they are economically much smaller. These other spikes are also not associated with pre-determined events which create incentives for sharp changes in firm inaction over time.

across different windows, in contrast with drops in volatility between 36% and 52% over the same periods. Measuring changes in expectations is challenging. However, these patterns suggest that, at least in the narrow windows of time here considered, the major effect of the previous political events was to reduce uncertainty about the exchange rate. These events are associated with limited surprises on the level of the exchange rate. One simple explanation for this pattern is the following. The expectation that Lula would be elected was already largely reflected in the level of the exchange rate in the beginning of these periods. Therefore, most relevant news during these periods was about his choice of economic policies. While there was considerable uncertainty about these choices right before the events, there were no significant surprises on these choices during the events.

2. Data, Sample, and Summary Statistics

2.1. Data Sources and Variables

The information on plants and firms used in this paper comes from two main data sources. The main source is the labor force record RAIS (*Relação Anual de Informações Sociais*) from the Brazilian labor ministry. Every employer in Brazil is required by law to annually report detailed information on workers and establishments to RAIS. The primary role of this record is to provide information for a federal wage supplement program. But this record is also used as a main source of information by the labor ministry and other government agencies to track the Brazilian formal labor market. For each year, the unit of observation in this report is a job, which is uniquely identified by worker- and plant-level identifiers combined with start and end dates. The start and end dates are the months in which a job starts and ends, respectively. As a worker transitions across different plants during a year, even if across plants from the same firm, different job observations are reported. This allows one to construct a list of all workers in an establishment in a given month. The set of all jobs in plant p in month t can be constructed as all jobs in p that have a start date until month t and an end date after month t. Monthly plant employment (*Plant Employment*) can then be measured as the total number of jobs in a given plant-month. For each job, the data provides information on the average wage during the job as well as on worker characteristics such as education. Using this information, one can then measure the average value of wages and workforce characteristics in a given plant-month. Additionally, this data includes firm-level identifiers and basic plant and firm characteristics such as the location of the establishment. All identifiers can be linked across years and this data is available between 1995 and 2006.[8]

Using these records, I track the existence of plant openings and closures and determine the month in which they take place in the following way. A plant is defined as being active or inactive in a given month if it has positive employment or zero employment in that month, respectively. *Plant Opening* is an indicator that equals one in the first month that a plant is active. *Plant Reopening* is an indicator that equals one in the first month that a plant switches into being active after a period of temporary inactivity. A plant experiences a period of temporary inactivity if it is inactive for at least three consecutive months between two periods of activity. *Plant Closure* is an indicator that equals one in the last month that a plant is active prior to a period of inactivity. A plant experiences a period of inactivity if it is inactive for at least three consecutive months. *Permanent Plant Closure* is an indicator that equals one in the last month that a plant is active.

These definitions are motivated by both economic and data considerations. Intuitively, these variables measure firms' decisions to start and stop operating a plant by tracking when they alternate between zero and positive employment. Note that these transitions capture the month in which workers start and stop being paid. Firms' decisions to open and close plants involve both important labor and capital adjustments. It is natural to imagine that the months in which the first workers are paid and the last workers stop being paid are closer to the start and end of operations than the analogous capital adjustments. Firms might start building a new plant significantly before they operate it and might only liquidate all of the plant's assets months after the end of the operations. When constructing indicator variables to measure the timing of exit and entry decisions for a broad set of firms and plants, the U.S. Census Bureau also uses these transitions between positive and zero paid employees (Jarmin and Miranda (2002)). Additionally, firms often decide not to use their existing capital, keeping it as unused, and they might close and reopen their operations without completely liquidating their capital and buying new capital.

[8] The ministry of labor estimates that the RAIS records cover well above 90 percent of formal workers in Brazil near the sample period. The analysis in this paper focuses on the manufacturing sector which intensely relies on formal workers and anecdotal evidence suggests that this is particularly true for largest firms. Carvalho (2014) finds evidence that firm level employment constructed using this database for manufacturing firms is strongly related to independent measures of firm employment from surveys of manufacturing firms by the Brazilian statistical agency (IBGE). Menezes-Filho, Muendler, and Ramey (2008) also show that this data exhibits many of the same properties found in employee-employer matched datasets for France and the United States.

These decisions to open and close a plant's operations are important and are likely to be associated with significant sunk costs as well as short-term investments. Therefore, they capture firm actions which are naturally exposed to real options considerations. Indeed, building on the idea that the previous costs are relevant, many previous applications of real options examine firms' decisions to start and stop operating a given production unit.[9]

One important issue with the measurement of the previous variables is that the unique plant identifier is based on the firm's tax identifier. Even in the absence of a plant closure, a plant identifier might become permanently inactive because of the sale of the plant or its division, a spin-off, a merger, or a change in the firm's tax identifier. These same events can lead a plant identifier to become active for the first time in the absence of a new plant opening. I detect these changes in the plant identifiers of active plants by tracking individual workers over time. Using this approach, I find that these identifier changes represent approximately 10 percent of initial closures and openings. I describe this approach in greater detail in Appendix C, where I also discuss an analysis suggesting that this and related sources of measurement error such as labor hoarding are unlikely to drive the sign and significance of the results.

Using this same data, I also measure intensive-margin monthly adjustments to plant employment. I separately examine firms' decisions to hire and fire workers. The timing of decisions can be measured at a monthly frequency by using the start and end dates for each job. For each job that starts or terminates during a year, the data provides information on the event. The data describes whether a firing fine was paid or not, what allows one to disentangle separations caused by a firing decision by the firm. *Layoff Rate* is the ratio of the number of workers fired by the firm in the plant in the current month to the average employment of the plant in the previous and current months. *Hiring Rate* and *Separation Rate* are defined analogously to *Layoff Rate* using the number of workers joining the plant in the current month and the total number of separations in the plant in the current month, respectively. *Plant Employment Growth* is the ratio of the change in plant employment between the current and

[9] For example, Brennan and Schwartz (1985) model how real options shape the optimal decisions by a gold mining firm to open and shut down a mine as output prices fluctuate. Dixit (1989) models how output price fluctuations affect firms' optimal decisions to start and stop operating a production technology in the presence of real-option incentives.

previous months to the plant's average employment between these two months. Notice that all these variables only include intensive-margin changes in plant employment as they have missing values when the plant is not active in both the previous and current months.[10]

I also use the RAIS data to construct monthly measures of plant age, plant wages, plant wage growth, and plant labor skill. *Plant Age* is the number of months since the plant first appeared in the data (divided by twelve). *Plant Wage* is the average wage across all jobs in a plant-month. I measure plant labor skill using two measures of average worker education: the share of workers in the plant-month with complete high school education (*LaborSkill1*) and the share of workers with complete college education (*LaborSkill2*). I also construct monthly measures of firm employment, employment growth, age, wages, wage growth, and worker skill by following analogous approaches at the firm level. The RAIS data does not contain information on firm or plant revenues, capital, and investment.

This data is combined with administrative record on every legally recorded export and import transaction by Brazilian firms. This customs data (*SECEX*) is also available between 1995 and 2006. For each transaction, the information available includes the value of the transaction in U.S. dollars, the nature of the transaction (export or import), the month-year in which the transaction takes place, and the firm's tax code which can be uniquely matched to the previous data. Using this customs data, I construct monthly measures of firms' total exports and imports. I normalize firms' total exports and imports by firms' total revenues. Information on firm revenue is not available from the previous data sources. While normalizing these variables, I measure firms' total revenues by combining their total employment with information on the ratio of the total value of shipments to the total employment for their industry. This last information is available only at an annual frequency from the annual survey of manufacturers (PIA) from the Brazilian statistical office (IBGE). This data is available at an industry level similar to a 3-digit SIC code.

2.2. Main Sample and Summary Statistics

The main sample in the paper is constructed around the previously described political events that take place at the end of 2002. Bloom (2009) suggests that the effects of changes in realized

[10] I follow Davis and Haltiwanger (1999) in computing these rates as the ratio of flows to the average employment between the current and the previous periods. I have found very similar results when I computed rates as the ratio of flows to the plant's employment in the previous month.

volatility confound the impact of changes in uncertainty after four months and completely offset uncertainty effects after six months. Motivated by these horizons, I define the overall sample period as a twelve-month period including the second semester of 2002 (before uncertainty resolution) and the first semester of 2003 (after uncertainty resolution).

When constructing the main sample used in the paper, I start with the universe of manufacturing firms with at least 50 employees on average during 2001. Manufacturing firms are firms with most of their employment in manufacturing. Using labor data from 2001 (average values), I construct variables capturing different firm initial characteristics and exclude firms with missing values for these variables. These characteristics are used as controls in the analysis and include firm's total employment, age, wages, and the two previous measures of labor skill. The initial database then consists of monthly plant-level data for these firms between July 2002 and June 2003. This database is combined with information from both customs data and the annual survey of manufacturers (see Section 2.1) to measure firms' initial exposure to trade. Firms' initial exposure to exports and imports are measured as the ratio of total exports to total sales in 2001 (*IFExpRatio*) and the ratio of total imports to sales in 2001 (*IFImpRatio*), respectively. Finally, I exclude firms with positive values for both *IFExpRatio* and *IFImpRatio* from the sample used in the main results. I discuss the motivation for this restriction and the use of alternative related restrictions in Section 3.1.

Table 2 reports summary statistics for the previously described sample. I report these statistics for both the overall sample and for the subsamples of exporters and importers, which include firms with positive values for *IFExpRatio* and *IFImpRatio*, respectively. The unit of observation is a plant-month. On average, each month covers 13,119 plants from 7,810 firms, including 2,048 plants from 1,317 exporters and 3,524 plants from 1,656 importers. All variables are defined in the Appendix B.

3. Results

3.1. Empirical Methodology

I examine changes in how firms adjust their plants around the predictable drops in exchange rate volatility described in Section 1. I relate changes in these decisions to within-industry differences in the importance of exports and imports for firms, which predict firms' exposure to exchange rate volatility. I propose an approach to control for the effect of changes in the expected level of exchange rates, while estimating the effect of exchange rate volatility. The intuition for this approach is the following. Both increases in the importance of net exports (exports minus imports) and net imports (imports minus exports) increase firms' exposure to exchange rate volatility. What matters for this exposure is firms' absolute net exposure to trade. In contrast, increases in the importance of net exports and net imports affect firms' exposure to the level of exchange rates in opposite ways. Therefore, one can analyze the effect of greater exposure to exchange rate volatility by estimating an average between two effects. Namely, the effect of greater export dependence for exporters and the effect of greater import dependence for importers.

One challenge in implementing this intuition is that firms which are currently net exporters might become net importers in some future scenarios. While an increase in the importance of exports increases the exposure of net exporters to volatility, it reduces the exposure of net importers to the same volatility. Therefore, as uncertainty about firms' future net exposure to trade becomes relevant, increases in the importance of exports today have offsetting effects on their exposure to exchange rate volatility. Similarly, when this uncertainty is important, increases in the importance of imports today also have offsetting effects on their exposure to exchange rate volatility. I address this issue by focusing on samples of firms which are mostly exposed to one margin of trade. This is intended to mitigate the uncertainty about the direction of firms' future trade exposure. In the analysis that follows, I exclude firms which initially both export and import (in 2001) and firms are only initially exposed to one margin of trade. In the Internet Appendix, I show that the main results in the paper are robust to different approaches that include firms which initially both export and import but are significantly biased in one direction.

I estimate the following specification:

$$Y_{pijt} = \alpha_{jt} + \gamma_1 \times IFExpRatio_i + \beta_1 \times Post_t \times IFExpRatio_i + \gamma_2 \times IFImpRatio_i + \beta_2 \times Post_t \times IFImpRatio_i + \delta' X_{it} + \varepsilon_{pit}, \quad (1)$$

where Y_{pijt} is the outcome variable for plant p in firm i, industry j and month t, α_{jt} is an industry-month fixed effect, industry is a three-digit code (similar to a three-digit SIC) for the main industry of the firm, *IFExpRatio* is the initial ratio of firm exports to sales (measured in 2001), *IFImpRatio* is the initial ratio of firm imports to sales (measured in 2001), $Post$ is an indicator that equals one after the predicted resolution of uncertainty (after December 2002), and X denotes a vector of controls. These controls include different firm and plant initial characteristics, which are measured prior to the sample period. I control for firm and plant initial employment, age, wages, and labor skill (*LaborSkill1* and *LaborSkill2*). For each variable, I sort the sample into three groups based on the variable and include both indicators for these three subgroups and their interactions with *Post* as controls. I also include indicators for the (state) location of plants and their interactions with *Post* as controls.[11] When estimating Equation (1), I weight the observations by the inverse of the initial number of plants in the firm (*IFNPlants*). The unit of observation is a plant-month and, while the median number of plants is two, some firms initially have many plants. This step ensures that the results can be interpreted as average firm-level effects in the sample. Additionally, one should expect the wait-and-see effects analyzed in the paper to become less relevant as firms have many plants and decisions over individual plants are less important. This intuition is confirmed in the analysis that follows.

The coefficients of interest are β_1 and β_2 and estimate how the initial importance of exports and imports for firms predict changes in plant outcomes around the drop in exchange rate volatility. Recall that the sample excludes firms which both export and import so these coefficients capture variation in the net importance of exports for exporters and the net importance of imports for importers. I formalize the previous intuition by estimating the effect of drops in exchange rate volatility as $\frac{1}{2}(\beta_1 + \beta_2)$. Intuitively, both coefficients capture the effect of the expected drop in exchange rate volatility combined with the effect of updates in firms' expectations about the level of the exchange rate. Independently of the direction of these first-moment effects, they should offset each other in the average between the coefficients. I provide different sources of evidence to support this hypothesis, including direct evidence on the effect of first-moment shocks to the exchange rate. One important issue is that both the importance of

[11] While some of the outcome variables are binary variables, I estimate linear regressions because of the importance of including industry-time fixed effects in the analysis. The inclusion of these fixed effects creates an incidental parameters problem that one can address in the linear model by using the demeaned data. While there are approaches to estimate the model parameters of non-linear models in the presence of such incidental parameters, these methods do not allow one to estimate marginal effects. For example, see Wooldridge (2002, Ch. 15) for a discussion of these issues.

exports and imports might be correlated with firm characteristics that predict their exposure to other potential sources of first-moment effects. I address this issue in the analysis that follows.

3.2. Plant Openings and Closures Results

I start by examining firms' decisions to open and close plants around the resolution of uncertainty. This analysis is based on the estimation of Equation (1) with the following outcome variables: *Plant Opening*, *Plant Reopening*, *Plant Closure*, and *Permanent Plant Closure*. Note that the summary statistics in Table 2 suggest that these decisions are important and capture large discrete actions by firms. The median number of plants in the firm is two in the sample and approximately 11% of plants are opened or closed in a given year. The fact that approximately 81% percent of plant closures are permanent suggests that firms find it costly to reverse a plant closure.

Table 3 reports the results examining plant openings and closures. Across all specifications, I report the previous average coefficient between *Post* × *IFExpRatio* and *Post* × *IFImpRatio*. In order to better capture the magnitude of the effects, this average coefficient is multiplied by the mean of *IFVolExp* in the subsample of firms with some exposure to trade. *IFVolExp* equals the absolute value of *IFExpRatio* minus *IFImpRatio*. This average coefficient is also divided by the mean of the outcome variable in the sample. This scaled estimate can be interpreted as the average estimated effect across firms exposed to trade, measured in percentage terms of the outcome variable. I report the results for different time periods around the resolution of uncertainty. As motivated in Section 2.2, I consider windows of time with at most six months after the resolution of uncertainty. Finally, I include all the controls discussed in Section 3.1 when examining plant closures, but exclude plant-level controls in the result examining plant openings. Plant-level controls are defined using plant initial characteristics and can only be constructed for pre-existing plants.

Across different specifications, the results show that firms are significantly more likely to both open new plants and close existing plants in the period after the resolution of uncertainty. For example, the effects represent a 50.4 and 17.0 percent increase in the likelihood of a plant opening and closure in the four-month period after the resolution of uncertainty relative to the four previous months, respectively. While both plant openings and closures increase with the resolution of uncertainty, the effects are economically stronger for plant openings. One possible explanation is that opening a new plant is associated more important sunk costs and investments that will be lost in case the firm decides to reverse the decision. The results are also economically more important for the opening of a new plant relative to the reopening of a previously active plant, especially when estimated using longer windows of time. This would be expected if the previous sunk costs are more important for new plants. Finally, the documented increase in plant closures after the resolution of uncertainty is matched with a similar important increase in permanent plant closures.

An important point while analyzing permanent plant closures is that this variable is constructed based on information about future decisions of firms. After a given firm closes a plant in month *t*, it has an option to reopen it after *t* and permanent plant closures condition on the fact that the firm decided not to exercise this option in the future. Ideally, one would want to only measure decisions made in month *t*. Uncertainty might affect the likelihood that an initial action is subsequently reversed conditional on the same initial decision. Motivated by this issue, I focus on plant closures (permanent or not) in the analysis but show in the Internet Appendix that the main results in the paper remain similar if *Plant Closure* is replaced with *Permanent Plant Closure*.

These findings support the prediction from inaction incentives that uncertainty leads firms to freeze both expansions and contractions. Moreover, the magnitude implied by the effects is economically important. To the extent that the results isolate the effect of exchange rate volatility, they capture only one channel through which the resolution of political uncertainty affects firms.

3.3. Uncertainty Resolution and Within-Plant Employment Adjustments

I next examine how firms change continuous within-plant labor adjustments around the resolution of uncertainty. I estimate Equation (1) using the following outcome variables: *Hiring Rate*, *Layoff Rate*, and *Plant Employment Growth*. It is plausible to expect these decisions to be less costly to reverse when compared to the previous actions to adjust plants on the extensive margin. Note that all these decisions are defined based on adjustments to the same production factor (labor). A plant opening and closure involves hiring and layoff decisions. However, there are two important additional aspects of these extensive-margin decisions that plausibly expose them to stronger inaction incentives. First, these decisions are discrete in nature. Second, the decisions to start and stop operating a plant are likely to be associated with significant fixed adjustment costs, in addition to the costs involved in hiring and firing workers. When firms can continuously adapt the size of an adjustment, the existence of significant adjustment costs is not enough to create wait-and-see incentives. There must be nonconvex adjustment costs such as fixed costs that lead to an option value of doing nothing. This option can also become valuable when firms make discrete adjustments associated with significant costs (e.g., see Dixit and Pyndick (1994)). As previously discussed, the median number of plants by firms in the sample is two, what makes the discreteness and fixed costs associated with opening and closing a given plant especially relevant in the setting here examined.

Panel A of Table 4 reports the results for plant hiring and firing adjustments. The results are scaled in the same way as in Table 3, based on different outcome variables, to better capture their economic magnitude. Across all specifications, I find that the resolution of uncertainty is associated with some increase in the hiring and layoff of workers. However, these effects are economically small relative to the extensive-margin results and mostly statistically insignificant. Note that the results are comparable across these different outcomes, as they are all scaled to capture percentage changes in outcomes. For example, the effects represent a 1.2 and 1.3 percent increase in plant hiring and firing in the four-month period after the resolution of uncertainty relative to the four previous months, respectively.

Panel B of Table 4 reports results analyzing changes in the intensive-margin employment growth of plants. These results are scaled in a similar way to the previous ones but not divided by the mean of the outcome variable in the sample. The previous average coefficient is only multiplied by the mean of *IFVolExp* in the subsample of firms with some exposure to trade. The results show that the resolution of uncertainty is not associated with economically or statistically significant changes in within-plant employment growth.

Together with the previous evidence on plant closures and plant openings, these results support the prediction from inaction incentives that uncertainty should matter more for decisions which are harder to reverse. The evidence that the resolution of uncertainty is associated with limited effects on labor intensive-margin adjustments in this setting can be rationalized by the following considerations. The results are designed to isolate one channel through which political uncertainty can matter for firms. The inaction effects associated with this specific channel might only be detectable in the data when decisions are exposed to sufficiently strong wait-and-see incentives. The view that inaction incentives in hiring and firing decisions are not very strong in this setting is consistent with important facts on Brazilian labor markets. Labor turnover is very high in Brazil relative to other countries and this has often been linked to limited incentives for training and human capital investments in Brazilian firms. This has also been commonly pointed as evidence that hiring and firing costs are limited in Brazil. Table 2 illustrates this high turnover during the sample period. On average, 46.2% of workers leave plants during the sample year and 65 percent of these separations represent a firing decision by the firm. Over this same period, firms hire the equivalent of 42.1% of their plants' workers. This high turnover leads to low job tenures. Approximately 50% of currently employed workers in the sample had been with their employer for less than two years. These low job tenures in turn limit the importance of the firing fines imposed on firms. The median firing fine across all layoffs in the sample is equivalent to 2.2 times the value of workers' monthly wage.[12]

3.4. Plant Discrete Adjustments and Uncertainty Resolution

I refine the previous evidence by connecting the results to the initial number of plants in the firm. The discreteness and fixed costs associated with plant openings and closures are more prominent when firms have fewer plants. Therefore, one should expect the decisions to open or close a given plant to become especially costly to reverse when firms have few initial plants. Inaction incentives should then lead firms to mostly freeze plant openings and closures under these conditions. In contrast, there is no clear reason to expect inaction incentives to create an important link between intensive-margin plant adjustments and the initial number of plants.

Table 5 reports the results analyzing these predictions from inaction incentives. The results use the same specifications as in Table 3 (Column (2) of Panels A and B) and Table 4 (Columns (2) and (5) of Panel A) estimated in different subsamples, based on firms' initial number of plants (*IFNPlants*). *IFNPlants* is the firm's average number of plants between January 2002 and June 2002. Table 2 shows that the median value for this variable in the sample is 2.0. I classify firms as having few plants if they have at most two plants. Alternatively, I also construct this

[12] For example, see World Bank (2002) and Gonzaga (2003) and the references therein for a discussion of these issues.

subsample using one or three plants as the cutoff. The share of plants in the subsample of firms with at most one, two, and three plants is 43%, 61%, and 70%, respectively.

As in the previous analysis, the results capture the average coefficient between *Post* × *IFExpRatio* and *Post* × *IFImpRatio* and are scaled to better reflect the magnitude of the effects. The previous average coefficient is multiplied by the mean of *IFVolExp* in the sample of firms with some exposure to trade. In order to make the results comparable to the previous analysis, this mean is based on the overall sample and is the same used in Table 3. In each result, the previous average coefficient is also divided by the mean of the outcome variable in the subsample being analyzed. This last step ensures that the results are comparable across different samples and outcomes. Motivated by the discussion in Section 2.2, I use a sample period with eight months in the main specification. This leads to a four-month period after the resolution of uncertainty. Recall that the analysis in Bloom (2009) suggests that changes in realized volatility confound the impact of changes in uncertainty after four months.

The results show that the previous plant opening and closure results are driven by firms with few plants. The results are only statistically and economically important when estimated with firms that initially have at most two or three plants. In contrast, there is no connection between the hiring and layoff results and firms' initial number of plants. These intensive-margin results are limited across all subsamples. This illustrates that the contrast between firms with different numbers of plants is only relevant for the specific discrete actions that change in importance across these firms.

These results provide additional evidence that the resolution of uncertainty mostly matters for decisions which are harder to reverse. More specifically, these findings support the interpretation for the previous results that uncertainty is especially significant for plant openings and closures because these adjustments involve important discrete actions by firms.

3.5. Plant Adjustments, Uncertainty, and Changes in Business Conditions

Another central implication from inaction incentives is that uncertainty should make firms less responsive to changes in business conditions. These effects should also be mostly important for decisions which are harder to reverse and affect both expansions and contractions. I examine these predictions by estimating the previous results in different subsamples, based on changes to industry revenue growth. Using industry-level data, I first construct measures of shocks to industry revenue growth. I then classify industries as experiencing positive or negative revenue shocks and separately estimate the results for each of these industries. Intuitively, firms facing improved (worsened) operating conditions can increase their short-term profits by expanding (contracting) in response to these industry shocks. In the presence of higher uncertainty, these innovations to industry conditions are more likely to be undone in the future. This increases the option value of waiting before reacting to changes in industry conditions with adjustments which are costly to reverse. Consequently, wait-and-see incentives should lead the resolution of uncertainty to mostly increase plant openings (closures) in industries experiencing positive (negative) shocks. Additionally, these patterns should be less important for intensive-margin expansions and contractions which are plausibly less exposed to inaction incentives.

This analysis is formalized in the following way. For each *i*, *Positive Industry Revenue Shock_i* and *Negative Industry Revenue Shock_i* are industries where *IndRevGrowthShock_i* > 0 and *IndRevGrowthShock_i* < 0, respectively. *IndRevGrowthShock_1* is the residual of a cross-sectional regression predicting industry revenue growth in 2002 using industry revenue growth in the previous three years and a constant. *IndRevGrowthShock_2* is defined analogously but extends the previous regression to include data from the previous five years as well as industry employment growth and wage growth as predictors. *IndRevGrowthShock_3* is the difference between industry revenue growth in 2002 and the average industry revenue growth in the previous three years. These different measures of industry shocks capture alternative simple approaches to measure innovations to industry revenue growth.

While constructing these industry shocks with annual data, I measure shocks taking place in 2002. The main results measure changes in decisions in early 2003 and this is intended to capture recent industry conditions, which should ultimately determine inaction incentives in the results. While the analysis measures changes in decisions relative to the last months of 2002, the identification of the effects of interest uses within-industry variation in firm exposure to uncertainty. This mitigates the concern that changes in inaction during the first part of the sample would both influence the choice of subsamples and predict the significance of the results. This concern is further mitigated by the fact that, in addition to data from previous years, only industry revenue data for the entire year of 2002 is used to construct these subsamples.

Table 6 reports the results following the previously outlined approaches. As before, the results capture the average coefficient between *Post* × *IFExpRatio* and *Post* × *IFImpRatio* and are scaled to better represent the magnitude of the effects. This average coefficient is scaled following the same approach in Table 5. Panel A of Table 6 reports the results for plant openings and closures. Across different measures of industry revenue shocks, the results show that increases in plant openings after the resolution of uncertainty are only important for industries experiencing positive shocks. There are no significant changes in plant openings for other industries. On the other hand, the increase in plant closures associated with uncertainty resolution is only important for industries experiencing negative shocks. There also no significant plant closure effects for other industries. Notice that these patterns cannot be explained by simple differences in the average importance of plant openings and closures between these samples. Recall that all effects are normalized by the mean of the outcome variable in the subsample being analyzed and capture percentage changes in this outcome. Panel B of Table 6 shows the analogous results for within-plant adjustments. These results are also directly comparable to the previous ones examining plant openings and closures. The results show that changes in within-plant hiring around the resolution of uncertainty are not related to industry shocks. In the context of layoff decisions, there is some connection between the previous effects and industry conditions. Increases in layoffs after the resolution of uncertainty are more important for industries facing negative shocks. However, this pattern is economically and statistically limited when compared to the one in the extensive-margin results.

Overall, these findings support additional predictions from wait-and-see incentives. Namely, they support the predictions that higher uncertainty makes plant adjustments less sensitive to business conditions and that this effect is mostly important for decisions which are harder to reverse. The results also support the prediction that these effects should hold both for expansions and contractions.

4. Robustness

An important concern with the interpretation of the previous results is that the political events analyzed in this paper might be associated with changes in firms' expectations about first moments. The empirical approach used in the previous analysis is designed to explicitly control for changes in firms' expectations about the level of exchange rates around these events.

However, there are still two potential reasons to be concerned about first-moment effects. First, the effects of changes in the expected level of exchange rates on exporters and importers might not offset each other as assumed in the previous approach. The analysis in Section 1 suggests that changes in the level of exchange rates are limited when compared to the observed decline in exchange rate volatility during the sample period. But it is challenging to measure changes in expectations which could still have significantly changed over this period. Second, firms' initial dependence on exports and imports could be both correlated with firm characteristics that predict their exposure to other sources of first-moment effects, different from exchange-rate shocks. For example, firms might update their expectation about future economic growth around the events and both exporters and importers might be more responsive to such news. In the analysis that follows, I address both these concerns using several additional results.[13]

4.1. Robustness to the Choice of Controls

I start by examining the robustness of the previous results to the inclusion and exclusion of important controls. In the main results, I only include initial firm and plant characteristics and their interactions with *Post* as controls. This is motivated by the possibility that firm and plant outcomes might be affected by the resolution of uncertainty. The inclusion of controls for changes in these same outcomes could then bias results estimating the effect of uncertainty. I now examine how the results change as one controls for outcomes that are likely to be correlated with first-moment effects. I first include additional controls for first-moment effects driven by exchange rates. These controls include Δ*Net Exports* and the two- and three-month lags of *Net Exports*. *Net Exports* is the ratio of monthly net exports (exports minus imports) to the initial value of firm revenues (measured in 2001). I annualize monthly net exports to make it comparable to annual revenues. These variables directly control for changes in firms' current operating conditions in international trade. These additional controls also include the monthly level of the exchange rate and its one-month lag as well as interactions of both these variables with *IFExpRatio* and *IFImpRatio*. These last controls are limited by the short time series used around the events and the fact that the resolution of uncertainty might be associated with some change in the level of exchange rates.

[13] An additional important concern is the potential existence of measurement error in plant opening and closure variables, which could affect the results. As discussed in Section 2.1, a specific source of concern is that measured events might not capture an actual closure or opening of a plant. I address this issue in Appendix C.

Table 7 reports the main previous results on plant openings and closures with these additional controls, labeled as international trade controls. Across different specifications, the results remain very similar to the ones without international trade controls. For example, the estimated effects for plant openings and closures in Table 3 (Column (2) of Panels A and B) change from 0.504 to 0.489 and from 0.170 to 0.160, respectively. I then include additional controls for first-moment effects that capture effects potentially unrelated to international trade. These controls capture firm- and plant-level changes in growth over time and are labelled growth controls. Firm-level controls include the one-month lag of firm employment and wage growth, and the two- and three-month lags of firm employment and wages. Plant-level controls include analogous variables defined at the plant level. As in Table 3, plant opening results do not include these plant-level controls which can only be defined for pre-existing plants. Relative to the previous international trade controls, the main disadvantage of these growth controls is that they are arguably more affected by fluctuations in uncertainty. This disadvantage might be mitigated by the fact that the resolution of uncertainty is not associated with significant changes in within-plant employment growth in this setting. Table 7 also reports results with these growth controls. Both plant openings and closures effects also remain very similar after the inclusion of these growth controls.

This evidence suggests that controlling for changes in outcomes that are likely to be correlated with first-moment effects does not significantly matters for the results. I also consider the robustness of the results to the choice of firm and plant characteristics used as controls. This analysis is especially useful in addressing the previously discussed concern on alternative channels for first-moment effects. To the extent that this concern is important in explaining the results, controlling for observable firm characteristics that predict firms' export and import dependence should significantly matter. Including such important controls should significantly reduce the economic magnitude of the results. I examine the robustness of the main results to the exclusion of controls for firm and plant initial human capital characteristics. These human capital controls capture differences in firm- and plant-level wages and shares of skilled workers (*LaborSkill1* and *LaborSkill2*). While this study does not use firm- and plant-level data on productivity or profitability, previous research has found a strong link at the firm level between these characteristics and wages (e.g., Abowd, Kramarz, and Margolis (1999)). These controls should arguably play an important role in predicting firms' exposure to first-moment effects, different from exchange-rate effects, such as changes in expectations about economic growth.

Table 7 reports results excluding these initial human capital controls which are included in all previous specifications. Together with the previous results, this evidence suggests that these human capital controls play a limited role in affecting the estimated effects. Moreover, depending on the outcome, the inclusion of these controls is associated with an increase or decrease in the economic magnitude of the results. For example, the estimated effects for plant openings and closures in Table 3 (Column (2) of Panels A and B) change from 0.571 to 0.504 and from 0.153 to 0.170 with the inclusion of these human capital controls, respectively. The absolute magnitude of these changes represents between 11 and 12 percent of the initial effects.

This evidence suggests that the inclusion of controls for outcomes and observable characteristics associated with first-moment effects at the firm- and plant-level does not significantly matters for the results.

4.2. Results Controlling for Differences in Firms' Sensitivity to Shocks

The empirical approach used in this paper builds on the idea that changes in the level of the exchange rate have effects on exporters and importers that offset each other. One potential issue with this approach is that exporters and importers have different characteristics (see Table 2). Consequently, the same shock to firms' operating profits might lead to responses with significantly different magnitudes when it affects exporters and importers. I now propose an approach to refine the previous analysis and address this issue. In order to see the logic for this approach, it is useful to interpret the previous analysis in an alternative way. The previous analysis estimates an average between the effects of greater net export dependence for exporters and greater net import dependence for importers. Estimating the sum of these two effects is equivalent to estimating the differential effect of greater net export dependence for exporters relative to importers. This comes from the fact that increases in net import dependence are equivalent to decreases in net export dependence. Therefore, the previous analysis can be interpreted as implementing the following two stages. In the first stage, one estimates how greater net export dependence by firms predicts changes in their decisions around the resolution of uncertainty. This effect is separately estimated for different types of firms. In the second stage, one then examines the differential importance of the first-stage effect for exporters relative to importers. The previous results include important firm- and plant-level controls while estimating the first stage of this analysis. However, these results do not include controls for the determinants of this first-stage effect in the analysis in the second stage. I address this issue by implementing the previous results in a way that better captures this last interpretation. While analyzing the determinants of the first-stage effect across firms, I control for important firm characteristics that might predict their sensitivity to changes in operating profits.

I formalize this analysis by estimating the following specification:

$$Y_{pijt} = \alpha_{jt} + \gamma_0 \times IFNetExpRatio_i + \gamma_1 \times Post_t \times IFNetExpRatio_i$$
$$+ \gamma_2 \times IFNetExpRatio_i \times Exporter_i$$
$$+ \beta \times Post_t \times IFNetExpRatio_i \times Exporter_i + \delta' X_{it} + \varepsilon_{pit}, \quad (2)$$

where *IFNetExpRatio* equals *IFExpRatio* minus *IFImpRatio*, *Exporter* is an indicator that equals one if *IFNetExpRatio* is positive, and all other variables are defined in the same way as in Equation (1). The specification includes the following controls: *Exporter*, *Importer*, *Post* × *Exporter*, and *Post* × *Importer*. *Importer* is an indicator that equals one if *IFNetExpRatio* is negative. Additionally, the results include the following control variables: *IFirmEmp*, *IFirmAge*, *IFirmWage*, *IFirmLaborSkill1*, and *IFirmLaborSkill2*. For each control variable in this last list, I sort the sample into three groups based on the variable and include both indicators for these three subgroups and their interactions with *Post*, *IFNetExpRatio*, and *Post* × *IFNetExpRatio* as controls in the results. I also include indicators for the (state) location of plants and their interactions with *Post* as controls.[14]

The coefficient of interest β measures the differential effect of firm net export exposure on plant adjustments around the resolution of uncertainty for exporters. Recall that $\frac{1}{2}\beta$ is equivalent to the average effect $\frac{1}{2}(\beta_1 + \beta_2)$ in Equation (1). In order to make the results comparable to the ones reported in Tables 3 and 4, and better capture the economic magnitude of the effects, the estimated value for $\frac{1}{2}\beta$ is scaled in an analogous way to $\frac{1}{2}(\beta_1 + \beta_2)$ in the previous results.

[14] In this analysis, the effect of firms' net export exposure is allowed to depend on each characteristic and this limits the variation in the data that allows one to estimate the effect of interest. Since the issue being addressed is a concern about firm-level differences in the data, I include only controls for firm initial characteristics.

Table 8 reports the results from the estimation of Equation (2). Firms are significantly more likely to both open and close plants after the resolution of uncertainty. As in the previous results, these extensive-margin effects are associated with limited changes in within-plant adjustments. The economic magnitude of the effects with this approach is important and similar to the one in the previous results. Recall that these results are directly comparable to the ones in Tables 3 and 4 but use different sources of variation in the data to estimate the effects of interest. For example, the estimated effects for plant openings and closures in Table 3 (Column (2) of Panels A and B) change from 0.504 to 0.422 and from 0.170 to 0.173 with this new approach, respectively. These results suggest that the main findings in the paper are robust to controlling for important potential differences between exporters and importers in their sensitivity to shocks.

4.3. Results with Firms that both Export and Import

As a final check, I implement two falsification tests intended to further address the key identification concerns previously discussed. In the first test, I estimate the main results in a sample of firms which initially both export and import. Intuitively, when analyzed over the same set of firms, increases in firm export dependence and reductions in firm import dependence should affect firms' exposure to the exchange rate in a similar way. These two sources of variation should affect firms' exposure to both the level of the exchange rate and exchange rate volatility in a comparable way. Therefore, if the results are driven by exchange-rate considerations, they should not be significant when β_1 and β_2 in Equation (1) estimate marginal effects for the same set of firms. In contrast, one should not expect this to be the case if greater export and import dependence drive the results through alternative channels, different from exchange-rate considerations. If the results are driven by a firm-level correlation between trade variables and alternative sources of first-moment effects, then the results should remain significant when the effects of both these trade variables are estimated over the same set of firms. Recall that β_1 and β_2 in Equation (1) are estimated over different sets of firms in the main results. Increases in the importance of exports among (net) exporters and reductions in the importance of imports among (net) importers are not equivalent. While both these changes affect firms' exposure to the level of the exchange rate in a similar way, they affect firms' exposure to exchange rate volatility in opposite ways.

A first approach to implement the previous idea is to simply estimate Equation (1) in the sample of firms which initially both export and import. One issue with this approach is that the coefficients β_1 and β_2 could capture marginal effects for different subsets of firms. While both effects are estimated using the same sample, in principle, differences in the distributions of *IFExpRatio* and *IFImpRatio* in this sample could lead this to be the case. In order to mitigate this issue, I refine this first approach by estimating the previous coefficients separately across subsamples, based on *IFNetExpRatio*. I then estimate an average effect using the results for each of the subsamples. This is intended to limit differences in the net trade exposure of the firms driving each of the two previous marginal effects.

I formalize this second approach by estimating the following specification:

$$Y_{pijt} = \alpha_{jt} + \sum_{\tau=1}^{4} \beta_{1\tau} \times Post_t \times IFImpRatio_i \times NTrade(\tau)_i$$
$$+ \sum_{\tau=1}^{4} \beta_{2\tau} \times Post_t \times IFExpRatio_i \times NTrade(\tau)_i + \delta' X_{it} + \varepsilon_{pit}, \quad (3)$$

where $NTrade(1)$ to $NTrade(4)$ are indicators that equal one in the different quartiles of *IFNetExpRatio* in the sample, and all other variables are defined in the same way as in Equation (1). The specification includes the following controls: $IFImpRatio \times NTrade(1)$ to $IFImpRatio \times NTrade(4)$ and $IFExpRatio \times NTrade(1)$ to $IFExpRatio \times NTrade(4)$. Additionally, the results include the same controls used in Tables 3 and 4. The coefficients of interest $\beta_{1\tau}$ and $\beta_{2\tau}$ estimate how firms' export dependence and import dependence predict changes in plant outcomes around the resolution of uncertainty in each subsample. The effect of interest is the equally-weighted average of these eight coefficients.

Table 9 reports the results using the two previously discussed approaches. Panels A and B report results using the first approach, which estimates Equation (1) in the sample of firms that initially both export and import (measured in 2001). This sample is constructed in the same way as the main sample used in the previous results with this different restriction on firm-level trade variables (see Section 2.2). Panels C and D report results using the second approach which estimates Equation (3) in the same sample. Across all specifications, the effects of interest are scaled in an analogous way to Tables 3 and 4. In order to make the results comparable across tables, the effects are scaled using the same mean of *IFVolExp* used in Table 3.

Across different specifications and approaches, the results suggest that the effects of greater export and import dependence offset each other in this sample. The results for plant openings and closures are always economically small when compared to the analogous results in Table 3. These effects are also statistically insignificant across specifications. Similarly, the estimated effects for within-plant adjustments are economically and statistically insignificant. This evidence supports the view that the main results in the paper capture exchange-rate effects, as opposed to a correlation between firms' exposure to trade and first-moment effects from alternative channels.

4.4. Exchange Rate Devaluation and Plant Adjustments

In the second falsification test, I examine firms' responses to a large change in the expected value of the Brazilian exchange rate. This provides a direct test for a key assumption used in the empirical approach of this paper. Namely, that the potential effects of such first-moment shocks to the exchange rate on exporters and importers offset each other. I study the large devaluation of the Brazilian currency in January of 1999 as a clear and significant shock to the Brazilian exchange rate. This devaluation was the outcome of an abrupt change in the exchange rate regime from a fixed exchange rate regime into a floating exchange rate regime. An important point is that the timing of this devaluation was arguably unexpected.[15] Therefore, an analysis of firms' decisions in a narrow window of time around this event is likely to a capture major shift in their expectation about the level of the exchange rate. Figure 3 plots the monthly value of the Brazilian exchange rate over time around this event. As the figure illustrates, this event was associated with a sharp depreciation of the Brazilian currency within a two-month period.

I estimate Equation (1) around this event in an analogous way to the analysis in the main results. I define *Post* as an indicator which equals one in the months after the devaluation is announced (January 1999) and zero in the months prior to this announcement. As in the main results, I consider short windows of time around the event to isolate changes in expectations. The exchange rate depreciated approximately 40 percent in the four-month period after the event relative to the fourth-month period prior to the event. The magnitude of this devaluation is similar in magnitude for the different windows of time considered in the analysis. It is plausible

[15] The Brazilian central-bank governor at the time discusses this devaluation episode in Franco (2000). He explains that "No indication of these changes was ever mentioned to the IMF or to the official sector before they were implemented…".

to assume that the absolute magnitude of this devaluation is significantly larger than the absolute magnitude of potential changes to the expected level of the exchange rate in the main analysis. The exchange rate changes by approximately 4-5 percent around these previous events (see Table 1). The Brazilian exchange rate appreciates in the subsequent months outside the sample period used in the main results. However, even conservative approaches to measure possible absolute changes in the expected value of the exchange rate around these events lead to magnitudes smaller than 40 percent. For example, one can assume that firms' updated value for the expected exchange rate after the events equals the lowest realized value of the exchange rate (R$/U.S. Dollar) in 2003. This still leads to an absolute change in the expected value of the exchange rate that equals approximately 30 percent.

Table 10 reports the results of the estimation of Equation (1) around this event. In addition to the estimated value for $\frac{1}{2}(\beta_1 + \beta_2)$, I also separately report the estimated values for β_1 and β_2. I scale these three effects in the same way as the estimated value for $\frac{1}{2}(\beta_1 + \beta_2)$ in Tables 3 and 4. As in the previous results, the effects are scaled using the same mean of *IFVolExp* used in Table 3. This ensures that their magnitude is comparable across different tables.

The results provide direct evidence that the main approach used in this paper controls for the effects of changes in the expected level of the exchange rate. The estimated effects for plant openings and closures around this major exchange rate devaluation are economically small when compared to the results in Table 3. Moreover, these effects are statistically insignificant in all specifications. The estimated effects for within-plant adjustments are also not economically or statistically important. These findings do not simply capture the possibility that changes in the level of the exchange rate do not affect firms' decisions. Several estimated effects for exporters or importers are both statistically and economically important. These significant effects are all consistent with the intuition that the devaluation of the exchange rate affects exporters and importers by increasing and reducing their incentives to grow their business, respectively. However, in all these cases, both the economic and statistical significance of the effects is significantly limited after one estimates an average between the effects for exporters and importers.

4.5. Addressing Alternative Interpretations

While completely addressing the previous identification concerns about first-moment effects is challenging, the collective evidence in this paper suggests that the main results capture the effects of fluctuations in uncertainty. As a last step in the analysis, I briefly summarize how the robustness checks and overall evidence in the paper address the previous concerns. First, the estimated effects are not sensitive to the inclusion or exclusion of several controls which should affect the importance of first-moment shocks in driving the results. Second, the results are robust to important falsification tests which directly test the possibilities raised in these concerns. One possibility is that the approach used in this paper does not properly controls for the effects of changes in the expected level of the exchange rate. I examine firms' responses to a major devaluation of the exchange rate and provide direct evidence against this possibility. Another possibility is that the results are driven by a correlation between firms' exposure to trade and first-moment effects from alternative channels, different from exchange-rate effects. I address this possibility by estimating the main results in the paper over a fixed set of firms which both export and import. As discussed in Section 4.3 in greater detail, the previous possibility predicts that one should still find important effects using this approach. I provide evidence that such effects are not important. Finally, I find in Section 3.3 that the resolution of uncertainty is not associated with significant changes in the intensive-margin employment growth of plants. While uncertainty could in principle affect this growth, this lack of result is consistent with important features of the setting being studied (see Section 3.3). On the other hand, it is plausible to imagine that first-moment effects would lead to significant changes in this growth rate. Indeed, the evidence in Section 4.4 directly supports this idea in the same setting of the main results.

6. Conclusion

Using monthly plant-level data on Brazilian manufacturing, this paper studies detailed predictions for how wait-and-see incentives should affect firm decisions to adjust their plants across multiple margins. A first key prediction from inaction incentives is that uncertainty should not only affect firms' decisions to expand their business but induce firms to freeze adjustments in both directions (expansions and contractions). Another key prediction from wait-and-see incentives is that both these previous effects should be mostly important for decisions that are harder to reverse such as large discrete and costly adjustments. I examine these predictions by studying how uncertainty affects firms' decisions to both open and close plants, which are important discrete choices by firms and plausibly exposed to sizeable adjustment costs. In the presence of high uncertainty, inaction incentives should lead firms to freeze both decisions to open and close plants. Additionally, these effects should be mostly important when firms have fewer plants and decisions over individual plants are closer to large discrete adjustments. I also examine how uncertainty affects continuous intensive-margin adjustments to the size of plants. Firms can plausibly reverse these adjustments at a lower cost, which should be less affected by inaction incentives. Finally, another key prediction from wait-and-see effects is that uncertainty should make firms less responsive to changes in business conditions. I examine this important prediction by connecting the previous effects to changes in industry conditions.

I test these predictions by studying firms' decisions at a high frequency around political events expected to significantly reduce political uncertainty in Brazil. The timing of these events is determined far in the past and the use of high-frequency data allows me to examine sharp changes in firm decisions around the expected resolution of uncertainty. The analysis uses the fact that this resolution of political uncertainty leads to a predictable drop in exchange rate volatility. Incentives for inaction should sharply reduce around these events, and this effect should be differentially important for firms more exposed to exchange rate volatility. I combine within-industry variation in the firm-level importance of both exports and imports to estimate how fluctuations in uncertainty affect firms' decisions while controlling for the effects of first-moment shocks.

Overall, I find broad support for all the previous detailed predictions from wait-and-see incentives for the effect of uncertainty on firms' decisions. Although the empirical approach in this paper explicitly controls for first-moment effects, I discuss and address the possibility that these effects could still drive the results. After presenting several robustness checks, I argue that the collective evidence in this paper suggests that that this is not the case.

Taken together, the results in this paper provide new evidence that real-option considerations can have first-order effects on firms by inducing them to wait and become inactive in the presence of high uncertainty. While the analysis in this paper does not provide direct evidence on the broad implications of these inaction effects, previous research has extensively analyzed these implications. The results in this paper complement this research by providing direct micro evidence on a key effect driving these analyses.

In addition to the important set of plant adjustments studied in this paper, wait-and-see effects could affect firm incentives in a broader range of related decisions. For example, these incentives could shape decisions such as the sale of individual assets, plants, divisions, and mergers. These transactions involve important large decisions by multiple firms and inaction considerations could matter for all firms involved in the transactions. Further understanding the role of inaction incentives in driving fluctuations in these transactions is a promising direction for future research. Another interesting direction for future research is to examine the detailed predictions from wait-and-see effects considered in this paper in a broader range of settings.

References

Abel, A., 1983, Optimal Investment Under Uncertainty, *American Economic Review* 73, 228 – 33.

Abel, A., A. Dixit, J. Eberly, and R. Pidynck, 1996, Options, the Value of Capital, and Investment, *Quarterly Journal of Economics*, 111, 753-777.

Abowd, J., F. Kramarz, and D. Margolis, 1999, High Wage Workers and High Wage Firms, *Econometrica* 67, 251-333.

Aguerrevere, F., 2003, Equilibrium Investment Strategies and Output Price Behavior: A Real-Options Approach, *Review of Financial Studies* 16, 1239–72.

Alesina, A., and R. Perroti, 1996, Income Distribution, Political Instability, and Investment, *European Economic Review* 40, 1203-1228.

Baker, S., N. Bloom, and S. Davis, 2013, Measuring Economic Policy Uncertainty, Working Paper.

Bar-Ilan, A., and W. Strange, 1996, Investment Lags, *American Economic Review* 86, 610 –22.

Bernanke, B., 1983, Irreversibility, Uncertainty, and Cyclical Investment, *Quarterly Journal of Economics* 98, 85 –106.

Bertrand, M., F. Kramarz, A. Schoar, and D. Thesmar, 2007, Politicians, Firms and the Political Business Cycle: Evidence from France, Working Paper, Chicago Booth.

Blanchard, O., 2009, (Nearly) Nothing to Fear but Fear Itself, *The Economist,* January 29.

Bloom, N., S. Bond, and J. Van Reenen, 2007, Uncertainty and Investment Dynamics, *Review of Economic Studies* 74, 391– 415.

Bloom, N., 2009, The Impact of Uncertainty Shocks, *Econometrica* 77, 623– 85.

Bloom, N., M. Floetotto, N. Jaimovich, I. Saporta-Eksten, and S. Terry. 2014, Really Uncertain Business Cycles, Working Paper, Stanford University.

Bloom, N., 2014, Fluctuations in Uncertainty, *Journal of Economic Perspectives* 28, 153-176.

Brealey, R., S. Myers, and F. Allen, 2008, *Principles of Corporate Finance*, (MacGraw-Hill, New York, NY)

Brennan, M., and E. Schwartz, 1985, Evaluating Natural Resource Investments, *Journal of Business* 58, 135 –57.

Caballero, R. and E. Engel, 1999, Explaining Investment Dynamics in U.S. Manufacturing: a Generalized Ss Approach, *Econometrica* 67, 783-826.

Carvalho, D., 2014, The Real Effects of Government-Owned Banks, *Journal of Finance* 69, 577-609.

Christiano, L., R. Motto, and M. Rostagno, 2014, Risk Shocks, *American Economic Review* 104, 27–65.

Davis, S., and J. Haltiwanger, 1999, "Gross Job Flows." In *Handbook of Labor Economics*. Vol. 3, ed. Orley Ashenfelter and David Card, 2711-2805. New York: Elsevier Science.

Dixit, A., 1989, Entry and Exit Decisions Under Uncertainty, *Journal of Political Economy* 97, 620-638.

Dixit, A., and R. Pindyck, 1994, *Investment under Uncertainty* (Princeton University Press, Princeton, NJ)

Franco, G., 2000, The Real Plan and the Exchange Rate, *Essays in International Finance*, Department of Economics, Princeton University.

Gilchrist, S., J. Sim, and E. Zakrajsek, 2011, Uncertainty, Financial Frictions and Investment Dynamics, Unpublished paper.

Giroud, X., and H. Mueller, 2015, Capital and Labor Reallocation Within Firms, *Journal of Finance* 70, 1767-1804.

Goldman Sachs, 2002, The Lulameter, *Emerging Markets Strategy* Issue No 02/10, June 6.

Gonzaga, G., 2003, Labor Turnover and Labor Legislation in Brazil, *Economía: Journal of the Latin American and Caribbean Economic Association* 4, 165-222.

Grenardier, S., 2002, Option Exercise Games: An Application to the Equilibrium Investment Strategies of Firms, *Review of Financial Studies* 15, 691-721.

Guiso, L., and G. Parigi, 1999, Investment and Demand Uncertainty, *Quarterly Journal of Economics* 114, 185 –227.

Gulen, H., and M. Ion, 2015, Policy Uncertainty and Corporate Investment, *Review of Financial Studies*, forthcoming.

Hartman, R., 1972, The Effects of Price and Cost Uncertainty on Investment, *Journal of Economic Theory* 5, 258– 66.

Jarmin, R., and J. Miranda, 2002, The Longitudinal Business Database, Working Paper, Center for Economic Studies, Washington, DC.

Jens, C., 2016, Political Uncertainty and Investment: Causal Evidence from U.S. Gubernatorial Elections, *Journal of Financial Economics*, forthcoming.

Julio, B. and Y. Yook, 2012, Political Uncertainty and Corporate Investment Cycles, *Journal of Finance* 67, 45-82.

Kellogg, R., 2014, The Effect of Uncertainty on Investment: Evidence from Texas Oil Drilling, *American Economic Review* 104, 1698-1734.

Kelly, B., L. Pastor, and P. Veronesi, 2015,The Price of Political Uncertainty: Theory and Evidence from the Option Market, *Journal of Finance*, forthcoming.

Kim, H., and H. Kung, 2014, The Asset Redeployability Channel: How Uncertainty Affects Corporate Investment?, Working Paper.

Kovenock, D., and G. Phillips, 1997, Capital Structure and Product Market Behavior: An Examination of Plant Exit and Investment Decisions, *Review of Financial Studies* 97, 767-803.

Kwhaja, A., and A. Mian, 2008, Tracing the Impact of Bank Liquidity Shocks: Evidence from an Emerging Market, *American Economic Review* 98, 555-599.

Leahy, J., and T. Whited, 1996, The Effect of Uncertainty on Investment: Some Stylized Facts, *Journal of Money, Credit and Banking* 28, 64– 83.

McDonald, R., and D. Siegel, 1986, The Value of Waiting to Invest, *Quarterly Journal of Economics* 101, 707–728.

Maksomovic, V. and G. Phillips, 2001, The Market for Corporate Assets: Who Engages in Mergers and Asset Sales and Are There Efficiency Gains?, *Journal of Finance* 56, 2019-2065.

Menezes-Filho, N., M. Muendler, and G. Ramey, 2008, The Structure of Worker Compensation in Brazil, with a Comparison to France and the United States, *Review of Economics and Statistics* 90, 324-346.

Moel, A., and P. Tufano, 2002, When Are Real Options Exercised? An Empirical Study of Mine Closings, *Review of Financial Studies* 15, 35-64.

Oi, W., 1961, The Desirability of Price Instability under Perfect Competition, *Econometrica* 29, 58–64.

Panousi, V., and D. Papanikolaou, 2012, Investment, Idiosyncratic Risk, and Ownership, *Journal of Finance* 67, 1113– 48.

Stein, L., and E. Stone, 2012, The Effect of Uncertainty on Investment, Hiring, and R&D: Causal Evidence from Equity Options, Working Paper.

Trigeorgis, L., 1996, *Real Options*, (MIT Press, Cambridge, MA).

The Economist, 2002, Running Out of Time, October 3.

Vavra, J., 2013, Inflation Dynamics and Time-Varying Volatility: New Evidence and an Ss Interpretation **,** *Quarterly Journal of Economics* 129, 215–258.

World Bank, 2002, *Brazil Jobs Report* (Washington, DC).

Wooldridge, J., 2002, *Econometric Analysis of Cross-Section and Panel Data*, (MIT Press, Cambridge, MA)

Yang, L., 2008, The Real Determinants of Asset Sales, *Journal of Finance* 63, 2231 – 2262.

Appendix A: The Role of Uncertainty in Firm Decisions

Different theories have suggested multiple channels for the effect of uncertainty on firms' decisions to adjust their production factors. A first reason uncertainty might affect decisions is because of Oi-Hartman-Abel effects. In the models of Oi (1961), Hartman (1972), and Abel (1993) uncertainty matters because one production factor is flexible relative to the other (labor is flexible relative to capital). As output price (or wage) shocks take place, firms are able to flexibly adjust their labor conditional on their capital stock. This leads the value of an additional unit of capital to be convex on the output price. Consequently, uncertainty increases investment.

A second channel through which uncertainty can matter is by affecting discount rates. As future cash flows become riskier, risk-averse investors want to expand less. Even in the context of firm-specific uncertainty, incentive contracts limit diversification and can lead uncertainty to increase the discount rate used by risk-averse managers to value incremental expansions (Panousi and Papanikolaou (2012)). Risk aversion now leads to the concavity of the value function. Additionally, higher uncertainty can lead firms to expand less production factors because of financing frictions, which increase the cost of financial distress or the costs of raising external funds (e.g., Gilchrist, Sim, and Zakrasjek (2011), and Christiano, Motto, and Rostagno (2014)).

Finally, a large body of theoretical research on the effect of uncertainty focuses on the importance of real options (see Dixit and Pyndick (1994) and Bloom (2014) for surveys). This research emphasizes that uncertainty increases the option value of doing nothing. As uncertainty increases, the conditions leading firms to make an initial adjustment are more likely to change and adjustment costs make it expensive for firms to reverse their initial decisions. There is a limited upside from adjusting as the firm has the option to adjust in the future. On the other hand, there is a significant downside because of limited reversibility. This leads to the concavity of the value function. These wait-and-see incentives have several predictions which are analyzed in the paper. Although these effects are intuitive, real options will not always lead uncertainty to induce inaction. For example, if there is a significant lag in investment decisions, uncertainty can increase firms' incentives to expand (Bar-Ilan and Strange (1996)). This expansion allows firms to better capture future potential upsides and firms can use the additional capacity as a call option.

Appendix B: Variable Definitions

A plant is defined as being active or inactive in a given month if it has positive employment or zero employment in that month, respectively.

Plant Opening is an indicator that equals one in the first month that a plant is active.

Plant Reopening is an indicator that equals one in the first month that a plant switches into being active after a period of temporary inactivity. A plant experiences a period of temporary inactivity if it is inactive for at least three consecutive months between two periods of activity.

Plant Closure is an indicator that equals one in the last month a plant is active prior to a period inactivity. A plant experiences a period of inactivity if it is inactive for at least three consecutive months.

Permanent Plant Closure is an indicator that equals one in the last month a plant is active.

Plant Employment is the total number of workers in the plant in month *t*. This is measured as the total number of jobs in the plant with a start date (month) until month *t* and an end date (month) after month *t*.

Layoff Rate is the ratio of the number of plant workers fired by the firm in month *t* to the average employment of the plant during months *t* and *t-1*. By construction, this variable does not include workers fired in a plant closure (permanent or not).

Hiring Rate is the ratio of the number of workers joining the plant in month *t* to the average employment of the plant during months *t* and *t-1*. By construction, this variable does not include workers hired in a plant opening or reopening.

Separation Rate is the ratio of the number of worker separations in the plant in month *t* to the average employment of the plant during months *t* and *t-1*. By construction, this variable does not include worker separations that take place during plant closures (permanent or not).

Plant Employment Growth is the ratio of the change in plant employment between months *t* and *t-1* to the average employment of the plant during months *t* and *t-1*.

Plant Wage is the average wage (R$) across all workers in the plant in month *t*. For each worker, the wage represents the average monthly value of the worker's total compensation. This average monthly compensation is computed using only information from the current job and includes only months in the current calendar year.

Plant Wage Growth is the change in the log of *Plant Wage* between months *t* and *t-1*.

Firm Employment, *Firm Employment Growth*, *Firm Wage,* and *Firm Wage Growth* are variables defined at the firm-month level in an analogous way to *Plant Employment*, *Plant Employment Growth*, *Plant Wage,* and *Plant Wage Growth*.

IFNPlants is the average number of plants in the firm between January 2002 and June 2002.

IFExpRatio (=ExpRatio) is the ratio of firm annual total export sales to firm annual total revenues in 2001.

IFImpRatio (=ImpRatio) is the ratio of firm annual total import costs to firm annual total revenues in 2001.

IFVolExp is the absolute value of *IFExpRatio* minus *IFImpRatio.*

IFEmp is the average monthly value of *Firm Employment* in 2001.

IFAge is the average monthly age of the firm in 2001. In each month, firm age is the number of months (annualized) since the first month in which the firm has positive employment in the data.

IFWage is the average monthly value of *Firm Wage* in 2001.

IFLaborSkill1 is the average monthly value of *Firm LaborSkill1* in 2001. *Firm LaborSkill1* is the share of workers in the firm in month *t* with complete high-school education.

IFLaborSkill2 is the average monthly value of *Firm LaborSkill2* in 2001. *Firm LaborSkill2* is the share of workers in the firm in month *t* with complete college education.

IPEmp, *IPAge*, *IPWage*, *IPLaborSkill1,* and *IPLaborSkill2* are plant-level variables defined in an analogous way to *IFEmp*, *IFAge*, *IFWage*, *IFLaborSkill1,* and *IFLaborSkill2*. In contrast with firm-level variables, plant-level variables are computed using average values between January 2002 and June 2002.

In the context of Tables 2 to 9, $Post$ is an indicator that equals one in 2003 (after the resolution of political uncertainty) and equals zero in 2002. In Table 10, $Post$ is an indicator that equals one after January 1999 (after the announcement of the exchange-rate devaluation) and equals zero prior to January 1999.

ExVol is the monthly exchange rate volatility. For each month, the exchange rate volatility is computed as the annualized standard deviation of daily log changes in the exchange rate (R$/U.S. Dollar).

Ex is the monthly level of the exchange rate (R$/U.S. Dollar). For each month, the exchange rate level is the average value of the daily exchange rate.

Net Exports equals *Exports* minus *Imports. Exports* is the ratio of total firm exports in month *t* (annualized) to firm annual total revenues in 2001. *Imports* is the ratio of total firm imports in month *t* (annualized) to firm annual total revenues in 2001.

Δ*Net Exports* is the change in *Net Exports* between months *t* and *t-1*.

IFNetExpRatio equals *IFExpRatio* minus *IFImpRatio*.

Exporter and *Importer* are indicators that equal one if *IFNetExpRatio* is positive and negative, respectively.

Appendix C: Measurement Error in Plant Openings and Closures

As discussed in Section 2.1, an important issue with the measurement of plant openings and closures is that the unique plant identifier in the data is based on the firm's tax identifier. Even in the absence of plant closures and openings, a plant identifier might permanently become inactive or a new plant identifier might emerge. These changes in identifiers can take place because of partial-firm sales, spin-offs, mergers, or changes in firm tax identifiers. While constructing the outcome variables used in the analysis, I propose and implement an approach to identify these events and exclude them while defining openings and closures. For each permanent closure of a plant identifier, I track all new openings of plant identifiers with plants in the same region and broad industry (similar to 2-digit SIC code) as the closure. I restrict this search to openings that take place close in time to the original closure. More specifically, for each plant identifier closure taking place in month *t*, I search for plant identifier openings that take place between months *t-1* and *t+2*. The previous plant-level industry condition is motivated by the idea that a plant will not immediately experience a broad change to its industry around these events. I then identify if one of these openings includes most of the workers in the original closure. I classify the original closure as a plant id change if this last condition is satisfied. I use the unique worker identifier in the data to implement this last step, which generates a unique plant opening match for each closure classified as a plant id change. The worker-level identifier in the data can be used to track workers across different firms because it is based on a number assigned by the federal government (PIS-PASEP). This number is used by the federal government to determine social program and social security benefits. I then identify new plant id openings that represent an id change by an active plant in the following way. For each new opening of a plant identifier, I construct a list of all workers in the opening. I then search for all permanent plant id closures with any of these workers and apply the previous steps to find matched plant id openings for each of these closures. I classify the original opening as a plant id change if the opening is classified as a match for some closure in this last step.

Following this approach, I find that approximately 10 percent of permanent closures in plant identifiers represent an identifier change by an active plant. I also find that a similar fraction of new plant identifier openings capture an identifier change by an active plant. I argue that the previous approach is unlikely to significantly underestimate the importance of such plant identifier changes. In principle, there are two main reasons why this could happen. First, worker identifiers could change around these events. This issue is mitigated by the previously discussed fact that worker identifiers are assigned by the federal government and are not affected by these firm events. Second, the previous approach restricts the search for matched openings to events that take place close in time to the original plant closure and involve plants in the same broad industry as the original closure. These restrictions are imposed for computational reasons. I find evidence suggesting that these restrictions do not lead one to significantly underestimate the importance of plant identifier changes. Recall that I restrict openings to be only in the same broad industry as the original closures (similar to 2-digit SIC code). I find that 89 percent of matches are in the same narrow industry (similar to 4-digit SIC code) as the original closure. Also recall that for each plant identifier closure taking place in month t, I search for plant identifier openings that take place between months $t-1$ and $t+2$. I find that 70 and 25 percent of matched openings take place one and two months after the original closure, respectively.

In the Internet Appendix, I examine how this source of measurement error affects the results. I argue that, under plausible conditions, this source of measurement error will have a limited effect on the results and will not change their sign or significance. Intuitively, one can think of the results as a weighted average of the effect of interest and the effect of uncertainty resolution on plant identifier changes (measurement error effect). In the context of plant openings and closures, the weight on the measurement error effect will be given by the share of measured events that are not actual events of interest. When this last share is low, measurement error is likely to affect the results in a limited way, unless the previous measurement error effect is significantly larger in absolute magnitude than the effects of interest. I argue that this last condition is unlikely to hold in the context of this paper. More importantly, I provide direct evidence supporting this view. I estimate the main results using all plant identifier openings and closures, without excluding the episodes classified as changes in plant identifiers. I find that these results have the same signs as the main results with economically similar magnitudes. Moreover, consistent with the previous discussion, I find that these effects are typically smaller than the effects in the main results and that this difference is often close to 10 percent. Note that the share of plant openings and closures that represent detected changes in plant identifiers is approximately 10 percent.

Finally, an additional source of measurement error is the possibility that firms open and close their plants without labor extensive-margin adjustments. I argue that this must capture labor hoarding, where firms with no production keep their workers. The other possibility is that firms produce output with no paid workers. More specifically, plant openings and closures might capture a firm decision to hire paid workers prior to any production and a decision to keep workers after shutting down production, respectively. In the Internet Appendix, I analyze the potential influence of labor hoarding on the results. The analysis suggests that this source of measurement error is also unlikely to drive the results. I first illustrate that, in order to drive the results, labor hoarding must be both frequent and significantly affected by changes in uncertainty. I then discuss how it is challenging to reconcile these hypotheses with the intensive-margin results in the paper. However, even if labor hoarding drives the results, this would not significantly matter for the main implications of the analysis in the paper. The results could still be interpreted as analyzing the effect of uncertainty on firms' decisions to adjust plant employment on the extensive margin. These effects would still be consistent with detailed implications from inaction incentives.

Table 1

Exchange Rate Volatility and Level Around the Resolution of Political Uncertainty

This table reports the monthly exchange rate volatility and level of the Brazilian currency (R$/U.S. Dollar) around the political events examined in the paper. These events take place in last quarter of 2002. *Log(ExVol)* is the log of the monthly exchange rate volatility. For each month, the exchange rate volatility is computed as the annualized standard deviation of daily log changes in the exchange rate. *Log(Ex)* is the log of the average value of the daily exchange rate in the month. *Post - Pre* is the difference between the average value of these variables in the subsamples *Post*=1 and *Pre*=1. When the sample period is defined using a 6-month window, the sample covers the period between October 2002 and March 2003. *Pre* and *Post* are indicators that equal one in the last three months of 2002 and the first three months of 2003, respectively. When the sample period is defined using 8-, 10-, and 12-month windows, the sample period and indicators (*Pre* and *Post*) are defined analogously.

Panel A: Changes in Exchange Rate Volatility

	Log(ExVol)			
	6-Month Window	8-Month Window	10-Month Window	12-Month Window
Post - Pre	-0.432	-0.364	-0.469	-0.521

Panel B: Changes in Exchange Rate Level

	Log(Ex)			
	6-Month Window	8-Month Window	10-Month Window	12-Month Window
Post - Pre	-0.049	-0.054	-0.054	-0.048

Table 2
Summary Statistics

Panel A reports summary statistics on the main sample used in the analysis. The unit of observation is a plant-month. The sample period covers the 12-month window between July 2002 and June 2003. The sample includes all manufacturing firms with at least 50 employees in 2001. Firms that both export and import during 2001 are excluded from the main sample (see text for more details). Exporters and Importers denote subsamples of firms with positive exports and imports during 2001, respectively. Panel B reports firm initial characteristics. These variables are measured using annual values from 2001. Panel C reports plant initial characteristics. These variables are defined only in the subsample of plants that exist in the first semester of 2002 and are measured using monthly averages for this period. See Appendix B for all variable definitions.

Panel A: Main Sample

	Overall Sample				Exporters				Importers			
	Nobs	Mean	Standard Deviation	Median	Nobs	Mean	Standard Deviation	Median	Nobs	Mean	Standard Deviation	Median
Plant Opening	157,428	0.0031	0.0556	0.0000	24,571	0.0038	0.0614	0.0000	42,288	0.0042	0.0644	0.0000
Plant Reopening	157,428	0.0005	0.0232	0.0000	24,571	0.0006	0.0239	0.0000	42,288	0.0003	0.0175	0.0000
Plant Closure	157,428	0.0057	0.0756	0.0000	24,571	0.0050	0.0706	0.0000	42,288	0.0059	0.0764	0.0000
Permanent Plant Closure	157,428	0.0047	0.0682	0.0000	24,571	0.0039	0.0624	0.0000	42,288	0.0051	0.0711	0.0000
Hiring Rate	156,737	0.0351	0.0933	0.0000	24,461	0.0381	0.1026	0.0089	42,046	0.0315	0.0938	0.0000
Layoff Rate	156,751	0.0246	0.0791	0.0000	24,463	0.0242	0.0823	0.0059	42,051	0.0223	0.0741	0.0000
Separation Rate	156,751	0.0385	0.1048	0.0129	24,463	0.0394	0.1106	0.0155	42,051	0.0335	0.0976	0.0080
Net Exports	157,419	0.0163	0.2810	0.0000	24,571	0.2049	0.4812	0.0003	42,285	-0.0660	0.3101	0.0000
Δ Net Exports	157,409	0.0009	0.2448	0.0000	24,571	0.0049	0.4283	0.0000	42,281	-0.0007	0.2957	0.0000
Firm Emp	157,428	277.1	471.2	116.0	24,571	291.1	533.0	132.0	42,288	439.0	620.9	191.0
Firm EmpGrowth	157,428	-0.0041	0.1222	0.0000	24,571	-0.0025	0.1392	0.0000	42,288	0.0003	0.1056	0.0000
Firm Wage (R$)	157,371	735.8	504.1	591.8	24,567	666.3	332.9	595.1	42,276	1088.2	677.7	894.9
Firm WageGrowth	157,363	0.0102	0.0688	-0.0003	24,566	0.0120	0.0711	0.0000	42,276	0.0094	0.0681	-0.0022
Plant Emp	157,428	96.4	185.6	64.0	24,571	133.0	267.9	77.0	42,288	101.2	164.4	59.0
Plant EmpGrowth	156,865	-0.0041	0.1811	0.0000	24,470	-0.0016	0.2018	0.0000	42,069	-0.0020	0.1807	0.0000
Plant Wage (R$)	156,873	764.9	731.6	583.1	24,547	718.3	509.3	596.1	42,242	1136.5	1090.0	859.8
Plant WageGrowth	156,276	0.0105	0.0909	0.0000	24,436	0.0118	0.0905	0.0000	42,022	0.0091	0.1015	0.0000

Panel B: Initial Firm Characteristics												
	Overall Sample				Exporters				Importers			
	Nobs	Mean	Standard Deviation	Median	Nobs	Mean	Standard Deviation	Median	Nobs	Mean	Standard Deviation	Median
IFExpRatio	157,428	0.0166	0.0810	0.0000	24,571	0.1064	0.1802	0.0225	42,288	0.0000	0.0000	0.0000
IFImpRatio	157,428	0.0177	0.0748	0.0000	24,571	0.0000	0.0000	0.0000	42,288	0.0657	0.1329	0.0159
IFVolExp	157,428	0.0343	0.1076	0.0000	24,571	0.1064	0.1802	0.0225	42,288	0.0657	0.1329	0.0159
IFNPlants	157,428	6.5	24.5	2.0	24,571	2.8	3.7	2.0	42,288	6.7	12.6	3.0
IFEmp	157,428	264.3	433.4	114.8	24,571	253.0	430.1	135.8	42,288	430.2	602.9	190.1
IFAge	157,428	4.2	2.3	6.0	24,571	4.7	2.1	6.0	42,288	4.8	2.0	6.0
IFWage (R$)	157,428	653.0	453.1	522.3	24,571	587.9	294.0	522.9	42,288	979.3	607.6	798.8
IFLaborSkill1	157,428	0.3560	0.2320	0.3235	24,571	0.2921	0.2005	0.2561	42,288	0.4829	0.2315	0.4708
IFLaborSkill2	157,428	0.0707	0.1059	0.0370	24,571	0.0576	0.1050	0.0344	42,288	0.1273	0.1342	0.0802

Panel C: Initial Plant Characteristics												
	Overall Sample				Exporters				Importers			
	Nobs	Mean	Standard Deviation	Median	Nobs	Mean	Standard Deviation	Median	Nobs	Mean	Standard Deviation	Median
IPEmp	153,976	94.1	158.8	64.8	23,988	124.0	213.0	78.7	41,103	102.0	160.8	60.8
IPAge	153,976	5.4	2.3	7.0	23,988	5.8	2.2	7.0	41,103	5.3	2.4	7.0
IPWage (R$)	153,555	728.8	690.4	555.4	23,934	677.5	452.3	570.1	41,079	1090.2	1000.5	832.3
IPLaborSkill1	153,976	0.4160	0.3027	0.3578	23,988	0.3616	0.2823	0.3014	41,103	0.5517	0.2925	0.5300
IPLaborSkill2	153,976	0.0874	0.1674	0.0256	23,988	0.0784	0.1568	0.0278	41,103	0.1599	0.2236	0.0680

Table 3
Plant Openings and Closures Around the Resolution of Uncertainty

This table presents results analyzing firms' decisions to open and close plants around political events predicted to significantly reduce uncertainty. The results are based on the estimation of Equation (1) using the sample described in Table 2. The unit of observation is a plant-month. The outcome variables are *Plant Opening*, *Plant Reopening*, *Plant Closure* and *Permanent Plant Closure*. *Plant Opening* and *Plant Reopening* indicate whether a plant is opened for the first time and reopened during a given month, respectively. *Plant Closure* and *Permanent Plant Closure* indicate whether a plant is closed and permanently closed during a given month, respectively. The overall sample covers the 12-month period between the last six months of 2002 and the first six months of 2003. The 6- and 8-month window samples are defined analogously based on two three- and four-month periods in the end of 2002 and the start of 2003, respectively. All results are weighted by the inverse of *IFNPlants*, the initial number of firm plants. *Post* is an indicator that equals one in 2003. *IFExpRatio* denotes firms' initial ratio of total exports to total revenues. *IFImpRatio* denotes firms' initial ratio of total imports to total revenues. The table reports the average coefficient between *Post* × *IFExpRatio* and *Post* × *IFImpRatio.* In order to better capture the magnitude of effects, this average coefficient is multiplied by the mean of *IFVolExp* in the subsample of firms with some exposure to trade. *IFVolExp* equals the absolute value of *IFExpRatio* minus *IFImpRatio*. This average coefficient is also divided by the mean of the outcome variable in the overall sample. Reported standard errors capture standard errors for this scaled average coefficient. The results in Panel A and B include the following control variables: *IFirmEmp*, *IFirmAge*, *IFirmWage*, *IFirmLaborSkill1*, and *IFirmLaborSkill2*. Additionally, the results in Panel B also include the following control variables: *IPlantEmp*, *IPlantAge*, *IPlantWage*, *IPlantLaborSkill1*, and *IPlantLaborSkill2*. For each control variable, I sort the sample into three groups based on the variable and include both indicators for these three subgroups and their interactions with *Post* as controls in the results. I also include indicators for the (state) location of plants and their interactions with *Post* as controls. Finally, all results include firm industry × month fixed effects. See Appendix B for all variable definitions. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Plant Openings

	Plant Opening			Plant Reopening		
	6-Month Window	8-Month Window	12-Month Window	6-Month Window	8-Month Window	12-Month Window
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.663***	0.504***	0.352***	0.542***	0.288	0.198
	(0.179)	(0.179)	(0.101)	(0.187)	(0.180)	(0.123)
Nobs	78,558	104,814	157,428	78,558	104,814	157,428
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Plant Closures						
	Plant Closure			Permanent Plant Closure		
	6-Month Window	8-Month Window	12-Month Window	6-Month Window	8-Month Window	12-Month Window
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.246***	0.170***	0.122***	0.303***	0.209***	0.150***
	(0.067)	(0.054)	(0.042)	(0.083)	(0.067)	(0.052)
Nobs	76,611	102,232	153,555	76,611	102,232	153,555
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 4

Uncertainty Resolution and Plant Intensive-Margin Adjustments

This table presents results from the analysis in Table 3 (Columns (1) to (3) of Panel B) using different outcome variables. The outcome variables are *Hiring Rate*, *Layoff Rate*, and *Employment Growth*. *Hiring Rate* and *Layoff Rate* are the percentage of plant workers joining a plant and fired in a given month, respectively. *Employment Growth* is the monthly change in the log of plant employment. These variables only include intensive-margin flows to plant employment. As in Table 3, the results capture the average coefficient between *Post × IFExpRatio* and *Post × IFImpRatio* and are scaled to better capture the magnitude of the effects. The results in Panel A are scaled in an analogous way to Table 3, based on different outcome variables. The results in Panel B are scaled in a similar way but not divided by the mean of the outcome variable in the sample. The previous average coefficient is only multiplied by the mean of *IFVolExp* in the subsample of firms with some exposure to trade. Reported standard errors capture standard errors for this scaled average coefficient. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Hiring and Layoff Rate

	Hiring Rate			Layoff Rate		
	6-Month Window	8-Month Window	12-Month Window	6-Month Window	8-Month Window	12-Month Window
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.020*	0.012	0.005	0.007	0.013	0.026*
	(0.011)	(0.011)	(0.009)	(0.018)	(0.016)	(0.015)
Nobs	76,581	102,189	153,489	76,581	102,189	153,489
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Intensive-Margin Employment Growth

	Employment Growth		
	6-Month Window	8-Month Window	12-Month Window
	(1)	(2)	(3)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.0005	-0.0001	-0.0008
	(0.0013)	(0.0011)	(0.0009)
Nobs	76,581	102,189	153,489
R-square	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes

Table 5

Plant Discrete Adjustments and Uncertainty Resolution

This table presents results from the analysis in Table 3 (Column (2) of Panels A and B) and Table 4 (Columns (2) and (5) of Panel A) estimated in different subsamples, based on firms' initial number of plants (*IFNPlants*). *IFNPlants* is the firm's average number of plants between January 2002 and June 2002. As in Table 3, the results capture the average coefficient between *Post* × *IFExpRatio* and *Post* × *IFImpRatio* and are scaled to better capture the magnitude of the effects. The previous average coefficient is multiplied by the mean of *IFVolExp* in the sample of firms with some exposure to trade. In order to make the results comparable to the previous analysis, this mean is based on the overall sample and is the same used in Table 3. In each result, this average coefficient is also divided by the mean of the outcome variable in the subsample being analyzed. Reported standard errors capture standard errors for this scaled average coefficient. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Plant Opening Results by Initial Number of Plants

	Plant Opening					
	8-Month Window					
	NPlants ≤ 1	NPlants > 1	NPlants ≤ 2	NPlants > 2	NPlants ≤ 3	NPlants > 3
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	1.000***	0.241**	0.800***	0.085	0.717***	0.061
	(0.295)	(0.115)	(0.213)	(0.108)	(0.192)	(0.121)
Nobs	46,022	58,792	64,695	40,119	73,898	30,916
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Plant Closure Results by Initial Number of Plants

	Plant Closure					
	8-Month Window					
	NPlants ≤ 1	NPlants > 1	NPlants ≤ 2	NPlants > 2	NPlants ≤ 3	NPlants > 3
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.314**	0.149**	0.332***	-0.021	0.247***	0.076
	(0.145)	(0.074)	(0.097)	(0.087)	(0.084)	(0.081)
Nobs	45,299	56,933	63,408	38,824	72,381	29,851
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Hiring Results by Initial Number of Plants						
	Hiring Rate					
	8-Month Window					
	NPlants ≤ 1	NPlants > 1	NPlants ≤ 2	NPlants > 2	NPlants ≤ 3	NPlants > 3
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.010	0.021	0.013	0.023	0.010	0.018
	(0.014)	(0.021)	(0.013)	(0.026)	(0.013)	(0.024)
Nobs	45,298	56,891	63,398	38,791	72,370	29,819
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel D: Layoff Results by Initial Number of Plants						
	Layoff Rate					
	8-Month Window					
	NPlants ≤ 1	NPlants > 1	NPlants ≤ 2	NPlants > 2	NPlants ≤ 3	NPlants > 3
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	-0.005	0.031	0.003	0.034	0.007	0.035
	(0.021)	(0.022)	(0.019)	(0.031)	(0.018)	(0.032)
Nobs	45,298	56,891	63,398	38,791	72,370	29,819
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6
Plant Adjustments, Uncertainty, and Changes in Industry Conditions

This table presents results from the analysis in Table 3 (Column (2) of Panels A and B) and Table 4 (Columns (2) and (5) of Panel A) estimated in different subsamples, based on changes to industry revenue growth (*IndRevGrowthShock*). For each *i, Positive Industry Revenue Shock_i* and *Negative Industry Revenue Shock_i* are industries where *IndRevGrowthShock_i* > 0 and *IndRevGrowthShock_i* < 0, respectively. *IndRevGrowthShock_1* is the residual of a cross-sectional regression predicting industry revenue growth in 2002 using industry revenue growth in the previous three years and a constant. *IndRevGrowthShock_2* is defined analogously but extends the previous regression to include data from the previous five years as well as industry employment growth and wage growth as predictors. *IndRevGrowthShock_3* is the difference between industry revenue growth in 2002 and the average industry revenue growth in the previous three years. As in Table 3, the results capture the average coefficient between *Post* × *IFExpRatio* and *Post* × *IFImpRatio* and are scaled to better capture the magnitude of the effects. The previous average coefficient is multiplied by the mean of *IFVolExp* in the sample of firms with some exposure to trade. In order to make the results comparable, this average is based on the overall sample and is the same used in Table 3. In each result, this average coefficient is also divided by the mean of the outcome variable in the subsample being analyzed. Reported standard errors capture standard errors for this scaled average coefficient. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Plant Openings Results by Changes in Industry Conditions

	Plant Opening					
	8-Month Window					
	Positive Industry Revenue Shock_1	Negative Industry Revenue Shock_1	Positive Industry Revenue Shock_2	Negative Industry Revenue Shock_2	Positive Industry Revenue Shock_3	Negative Industry Revenue Shock_3
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.833***	0.147	0.842***	0.204	0.867***	0.156
	(0.242)	(0.136)	(0.260)	(0.131)	(0.233)	(0.102)
Nobs	48,645	56,094	44,719	60,020	40,645	64,094
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Plant Closures Results by Changes in Industry Conditions

	Plant Closure					
	8-Month Window					
	Positive Industry Revenue Shock_1	Negative Industry Revenue Shock_1	Positive Industry Revenue Shock_2	Negative Industry Revenue Shock_2	Positive Industry Revenue Shock_3	Negative Industry Revenue Shock_3
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.044	0.362***	0.068	0.294***	-0.003	0.339***
	(0.066)	(0.104)	(0.075)	(0.084)	(0.087)	(0.071)
Nobs	47,353	54,812	43,469	58,696	39,700	62,465
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Hiring Results by Changes in Industry Conditions

	Hiring Rate					
	8-Month Window					
	Positive Industry Revenue Shock_1	Negative Industry Revenue Shock_1	Positive Industry Revenue Shock_2	Negative Industry Revenue Shock_2	Positive Industry Revenue Shock_3	Negative Industry Revenue Shock_3
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	-0.011	0.023	-0.005	0.020	0.018	0.002
	(0.018)	(0.018)	(0.020)	(0.017)	(0.017)	(0.019)
Nobs	47,353	54,812	43,469	58,696	39,700	62,465
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel D: Layoffs Results by Changes in Industry Conditions

	Layoff Rate					
	8-Month Window					
	Positive Industry Revenue Shock_1	Negative Industry Revenue Shock_1	Positive Industry Revenue Shock_2	Negative Industry Revenue Shock_2	Positive Industry Revenue Shock_3	Negative Industry Revenue Shock_3
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	-0.013	0.053*	-0.027	0.054*	-0.004	0.035
	(0.021)	(0.030)	(0.021)	(0.028)	(0.020)	(0.029)
Nobs	47,327	54,795	43,445	58,677	39,692	62,430
R-square	0.01	0.01	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7
Robustness to the Choice of Controls

This table presents results from the analysis in previous tables with different sets of controls. The results are based on the specifications in Table 3 (Column (2) of Panels A and B), Table 4 (Columns (2) and (5) of Panel A), Table 5 (Column (3) of Panels A and B) and Table 6 (Column (1) of Panel A and Column (2) of Panel B). The results with international trade controls include additional controls for exchange rate first-moment effects. These controls include the monthly level of the exchange rate and its one-month lag, as well as interactions of both these variables with *IFExpRatio* and *IFImpRatio*. These controls also include *ΔNet Exports* and the two- and three-month lags of *Net Exports*. The results with growth controls include controls for firm- and plant-level changes in growth over time. Firm-level controls include the one-month lag of *Firm EmpGrowth* and *Firm WageGrowth*, and the two- and three-month lags of *Firm Emp* and *FirmWage*. Plant-level controls include analogous variables defined at the plant level. As in Table 3, plant opening results do not include these plant-level controls which are included only on plant closure results. Initial human capital controls are included in the previous results and consist of all control variables based on firm- and plant-level wages and shares of skilled workers discussed in Table 3. The results without these controls include all other controls discussed in Table 3. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Plant Openings

	Plant Opening					
	8-Month Window					
	Overall Sample			NPlants ≤ 2		
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.489***	0.499***	0.571***	0.786***	0.807***	0.886***
	(0.142)	(0.143)	(0.134)	(0.220)	(0.225)	(0.209)
Nobs	104,783	104,728	104,814	64,684	64,629	64,695
R-square	0.01	0.01	0.01	0.01	0.01	0.01
International Trade Controls	Yes	Yes		Yes	Yes	
Growth Controls		Yes			Yes	
Initial Human Capital Controls	Yes	Yes		Yes	Yes	
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Plant Openings - Continuation

	Plant Opening		
	8-Month Window		
	Positive Industry Revenue Shock_1		
	(1)	(2)	(3)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.719***	0.724***	0.816***
	(0.225)	(0.230)	(0.212)
Nobs	48,625	48,599	48,645
R-square	0.01	0.01	0.01
International Trade Controls	Yes	Yes	
Growth Controls		Yes	
Initial Human Capital Controls	Yes	Yes	
Industry-Month FE	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes

Panel C: Plant Closures

	Plant Closure					
	8-Month Window					
	Overall Sample			NPlants ≤ 2		
	(1)	(2)	(3)	(4)	(5)	(6)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.160***	0.151***	0.153***	0.332***	0.322***	0.289***
	(0.060)	(0.057)	(0.050)	(0.107)	(0.100)	(0.090)
Nobs	102,204	102,169	104,814	63,400	63,365	64,695
R-square	0.01	0.01	0.01	0.01	0.01	0.01
International Trade Controls	Yes	Yes		Yes	Yes	
Growth Controls		Yes			Yes	
Initial Human Capital Controls	Yes	Yes		Yes	Yes	
Industry-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel D: Plant Closures - Continuation

	Plant Closure		
	8-Month Window		
	Negative Industry Revenue Shock_1		
	(1)	(2)	(3)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.395***	0.358***	0.296***
	(0.116)	(0.113)	(0.092)
Nobs	54,804	54,781	56,094
R-square	0.01	0.01	0.01
International Trade Controls	Yes	Yes	
Growth Controls		Yes	
Initial Human Capital Controls	Yes	Yes	
Industry-Month FE	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes

Panel E: Plant Intensive-Margin Adjustments

	Overall Sample			
	8-Month Window			
	Hiring Rate		Layoff Rate	
	(1)	(2)	(3)	(4)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.007	0.016	-0.003	0.004
	(0.012)	(0.012)	(0.018)	(0.015)
Nobs	102,153	104,336	102,161	104,347
R-square	0.01	0.01	0.01	0.01
International Trade Controls	Yes		Yes	
Growth Controls				
Initial Human Capital Controls	Yes		Yes	
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Table 8
Results Controlling for Differences in Firms' Sensitivity to Shocks

This table presents results from the estimation of Equation (2) using the sample described in Table 2. The unit of observation is a plant-month. The outcome variables are *Plant Opening*, *Plant Closure*, *Layoff Rate*, and *Hiring Rate* (see Tables 3 and 4). The 6-month window sample covers the period between the last three months of 2002 and the first three months of 2003. The 8-month window sample is defined analogously based on two four-month periods in the end of 2002 and the start of 2003. All results are weighted by the inverse of *IFNPlants*, the initial number of firm plants. *Post* is an indicator that equals one in 2003. *IFNetExpRatio* equals *IFExpRatio* minus *IFImpRatio*. *IFExpRatio* denotes firms' initial ratio of total exports to total revenues. *IFImpRatio* denotes firms' initial ratio of total imports to total revenues. *Exporter* is an indicator that equals one if *IFNetExpRatio* is positive. The table reports the coefficient on *Post* × *IFNetExpRatio* × *Exporter*. In order to make the results comparable to ones reported in Tables 3 and 4, and better capture the economic magnitude of the effects, this coefficient is scaled in the following way. First, this coefficient is divided by two. This coefficient is then multiplied by the mean of *IFVolExp* used to scale the effects in Table 3 (mean in the subsample of firms with some exposure to trade). Finally, this coefficient is divided by the mean of the outcome variable in the sample being analyzed. Reported standard errors capture standard errors for this scaled coefficient. The specification includes the following controls: *Exporter* *Importer*, *IFNetExpRatio*, *IFNetExpRatio* × *Exporter*, *Post* × *Exporter*, *Post* × *Importer*, and *Post* × *IFNetExpRatio*. Additionally, the results include the following control variables: *IFirmEmp*, *IFirmAge*, *IFirmWage*, *IFirmLaborSkill1*, and *IFirmLaborSkill2*. For each control variable in this last list, I sort the sample into three groups based on the variable and include both indicators for these three subgroups and their interactions with *Post*, *IFNetExpRatio*, and *Post* × *IFNetExpRatio* as controls in the results. I also include indicators for the (state) location of plants and their interactions with *Post* as controls. Finally, all results include firm industry × month fixed effects. See Appendix B for all variable definitions. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Plant Openings and Closures

	Plant Opening		Plant Closure	
	6-Month Window	8-Month Window	6-Month Window	8-Month Window
	(1)	(2)	(3)	(4)
Post × IFNetExpRatio × Exporter	0.609***	0.422***	0.241***	0.173***
	(0.223)	(0.170)	(0.063)	(0.052)
Nobs	78,558	104,814	78,558	104,814
R-square	0.01	0.01	0.01	0.01
Post × NetExpRatio × Controls	Yes	Yes	Yes	Yes
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Panel B: Plant Intensive-Margin Adjustments

	Hiring Rate		Layoff Rate	
	6-Month Window	8-Month Window	6-Month Window	8-Month Window
	(1)	(2)	(3)	(4)
Post × IFNetExpRatio × Exporter	-0.011	-0.022	-0.027	-0.022
	(0.018)	(0.016)	(0.018)	(0.015)
Nobs	78,183	104,347	78,183	104,347
R-square	0.01	0.01	0.01	0.01
Post × NetExpRatio × Controls	Yes	Yes	Yes	Yes
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Table 9
Results with Firms that Both Export and Import

Panels A and B present results from the analysis in Table 3 (Columns (1) and (2) of Panels A and B) and Table 4 (Columns (1), (2), (4), and (5) of Panel A) estimated in a different sample. This sample includes firms which both export and import in 2001 (see text for details). In order to make the results comparable across tables, the effects are scaled using the same mean of *IFVolExp* used in Table 3. The analysis is otherwise implemented in an analogous way to Tables 3 and 4. Panels C and D present results from the estimation of Equation (3) using the same sample as Panels A and B. The outcome variables are *Plant Opening*, *Plant Closure*, *Layoff Rate*, and *Hiring Rate* (see Tables 3 and 4). The 6-month and 8-month sample periods cover the end of 2002 and start of 2003 and are defined in the same way as in Table 3. All results are weighted by the inverse of *IFNPlants*, the initial number of firm plants. *Post* is an indicator that equals one in 2003. *IFExpRatio* denotes firms' initial ratio of total exports to total revenues. *IFImpRatio* denotes firms' initial ratio of total imports to total revenues. *NTrade_1* to *NTrade_4* are indicators for the four quartiles of *IFNetExpRatio* = *IFExpRatio* - *IFImpRatio* in the sample. The table reports the average coefficient across *Post* × *IFExpRatio* × *NTrade_i* and *Post* × *IFImpRatio* × *NTrade_i* for $i = 1,2,3,4$. In order to make the results comparable to the ones reported in Tables 3 and 4, and better capture the economic magnitude of the effects, this average coefficient is scaled in the following way. First, this average coefficient is multiplied by the same mean of *IFVolExp* used to scale the effects in Table 3. This average coefficient is then divided by the mean of the outcome variable in the sample being analyzed. Reported standard errors capture standard errors for this scaled average coefficient. Columns (1) and (2) of Panel C include the same set of controls as in Panel A of Table 3. All other specifications in Panels C and D include the same set of controls as in Panel B of Table 3. See Appendix B for all variable definitions. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Plant Openings and Closures - Simple Approach

	Plant Opening		Plant Closure	
	6-Month Window	8-Month Window	6-Month Window	8-Month Window
	(1)	(2)	(3)	(4)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	-0.072	-0.059	0.032	0.013
	(0.067)	(0.051)	(0.035)	(0.028)
Nobs	65,134	86,870	62,848	83,837
R-square	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Panel B: Plant Intensive-Margin Adjustments - Simple Approach

	Hiring Rate		Layoff Rate	
	6-Month Window	8-Month Window	6-Month Window	8-Month Window
	(1)	(2)	(3)	(4)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.001	-0.002	0.002	-0.005
	(0.011)	(0.009)	(0.008)	(0.008)
Nobs	62,820	83,803	62,820	83,803
R-square	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Panel C: Plant Openings and Closures - Refined Approach				
	Plant Opening		Plant Closure	
	6-Month Window	8-Month Window	6-Month Window	8-Month Window
	(1)	(2)	(3)	(4)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	-0.100	-0.063	-0.046	-0.058
	(0.095)	(0.073)	(0.067)	(0.054)
Nobs	65,134	86,870	62,848	83,837
R-square	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Panel D: Plant Intensive-Margin Adjustments - Refined Approach				
	Hiring Rate		Layoff Rate	
	6-Month Window	8-Month Window	6-Month Window	8-Month Window
	(1)	(2)	(3)	(4)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.008	-0.005	0.008	-0.001
	(0.010)	(0.011)	(0.020)	(0.017)
Nobs	62,820	83,803	62,820	83,803
R-square	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Table 10
Exchange Rate Devaluation and Plant Adjustments

This table presents results analyzing firms' decisions to adjust their plants around a large devaluation of the Brazilian currency in January 1999 (see text for details). The results are based on the estimation of Equation (1) around this event and the unit of observation is a plant-month. The main sample and all variables (including controls) are constructed in an analogous way to Tables 3 and 4 using this different event. The 6- and 8-month window samples are defined based on two three- and four-month periods prior to January 1999 and after January 1999, respectively. All results are weighted by the inverse of *IFNPlants* , the initial number of firm plants. *Post* is an indicator that equals one in 1999. *IFExpRatio* denotes firms' initial ratio of total exports to total revenues. *IFImpRatio* denotes firms' initial ratio of total imports to total revenues.The table reports the average coefficient between *Post* × *IFExpRatio* and *Post* × *IFImpRatio* . In order to make the results comparable to the ones reported in Tables 3 and 4, and better capture the economic magnitude of the effects, this average coefficient is scaled in the following way. First, this average coefficient is multiplied by the mean of *IFVolExp* used to scale the effects in Table 3. This average coefficient is then divided by the mean of the outcome variable in the sample being analyzed. Reported standard errors capture standard errors for this scaled average coefficient. See Appendix B for all variable definitions. Standard errors are heteroskedasticity robust and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Plant Openings and Closures

	Plant Opening		Plant Closure	
	6-Month Window	8-Month Window	6-Month Window	8-Month Window
	(1)	(2)	(3)	(4)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	-0.036	-0.076	0.029	0.038
	(0.073)	(0.070)	(0.070)	(0.055)
Post × IFExpRatio	0.023	-0.102	-0.159	-0.112
	(0.077)	(0.095)	(0.108)	(0.084)
Post × IFImpRatio	-0.095	-0.050	0.217**	0.187**
	(0.139)	(0.118)	(0.098)	(0.077)
Nobs	75,026	100,073	72,799	97,077
R-square	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Panel B: Plant Intensive-Margin Adjustments

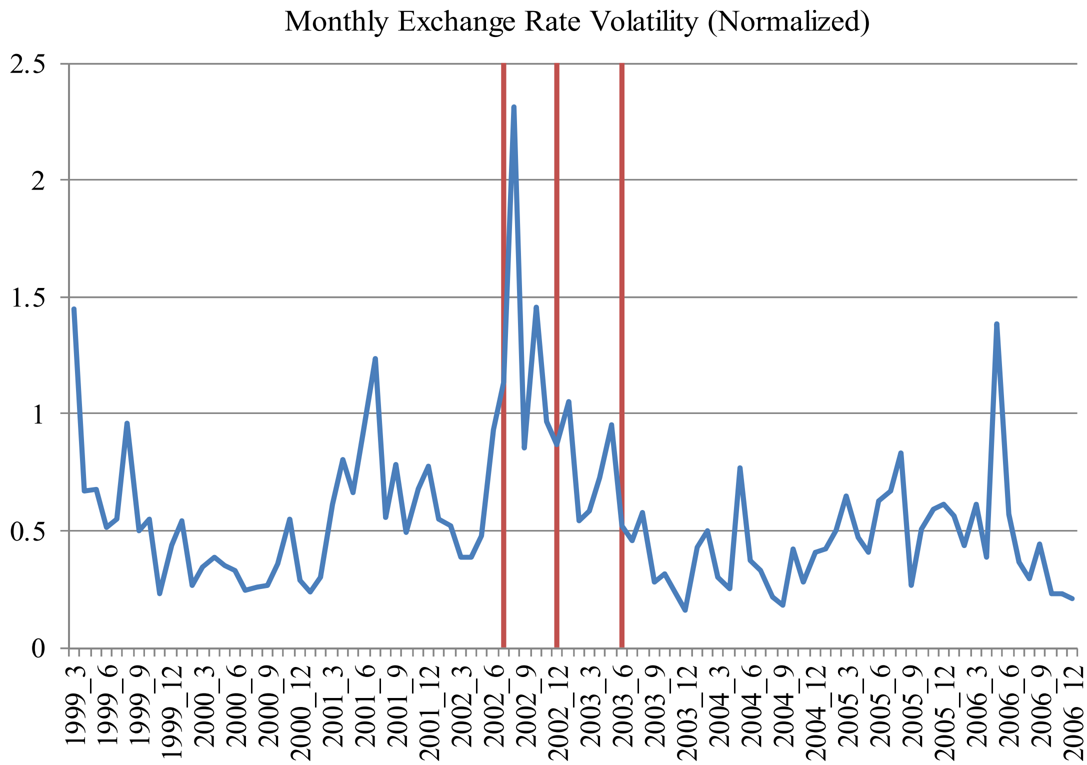
	Hiring Rate		Layoff Rate	
	6-Month Window	8-Month Window	6-Month Window	8-Month Window
	(1)	(2)	(3)	(4)
*1/2 * (Post × IFExpRatio + Post × IFImpRatio)*	0.002	-0.012	0.012	0.006
	(0.016)	(0.015)	(0.021)	(0.017)
Post × IFExpRatio	0.082***	0.058***	-0.005	-0.007
	(0.024)	(0.021)	(0.036)	(0.029)
Post × IFImpRatio	-0.078***	-0.081***	0.029	0.018
	(0.025)	(0.027)	(0.026)	(0.022)
Nobs	72,766	97,038	72,766	97,038
R-square	0.01	0.01	0.01	0.01
Industry-Month FE	Yes	Yes	Yes	Yes
Post × Plant Location FE	Yes	Yes	Yes	Yes

Figure 1

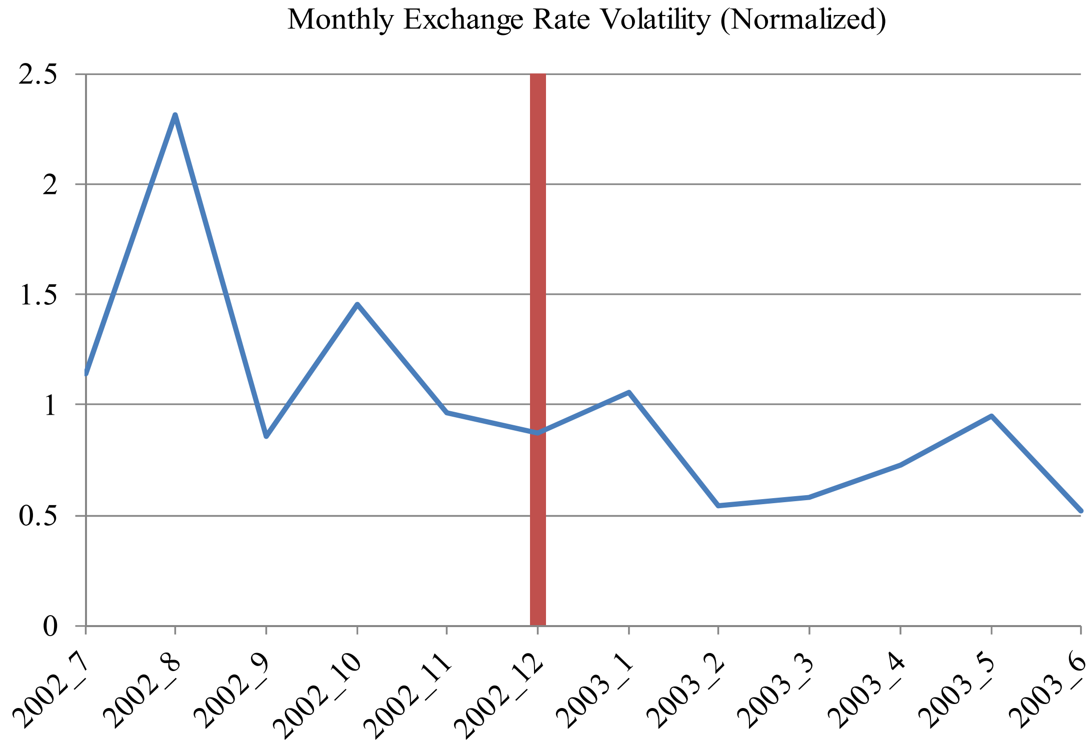
Exchange Rate Volatility Around the Resolution of Political Uncertainty

This figure shows the monthly exchange rate volatility of the Brazilian currency (Real) around the major resolution of political uncertainty due to political events that take place in the end of 2002. These events are national elections and the appointment of new members for the presidential cabinet (see text for discussion). For each month, the exchange rate volatility is computed as the annualized standard deviation of daily log changes in the exchange rate (R$/U.S. Dollar). Across all panels, the figure plots the monthly exchange rate volatility divided by the average value of this variable between July 2002 and June 2003. Panel A shows the volatility of the Brazilian exchange rate between 1999 and 2006. The six-month periods prior to the resolution of uncertainty (July 2002 to December 2002) and after uncertainty is resolved (January 2003 to July 2003) are shown by the vertical lines. Panel B shows the exchange rate volatility only during these last two periods. The month separating these periods (December 2002) is shown by the vertical line. Panel C shows the previous patterns using three-month periods prior to the resolution of uncertainty (October 2002 to December 2002) and after uncertainty is resolved (January 2003 to March 2003).

Panel A: Broader Time-Series Patterns



Panel B: 12-Month Window Around the Resolution of Uncertainty



Panel C: 6-Month Window Around the Resolution of Uncertainty

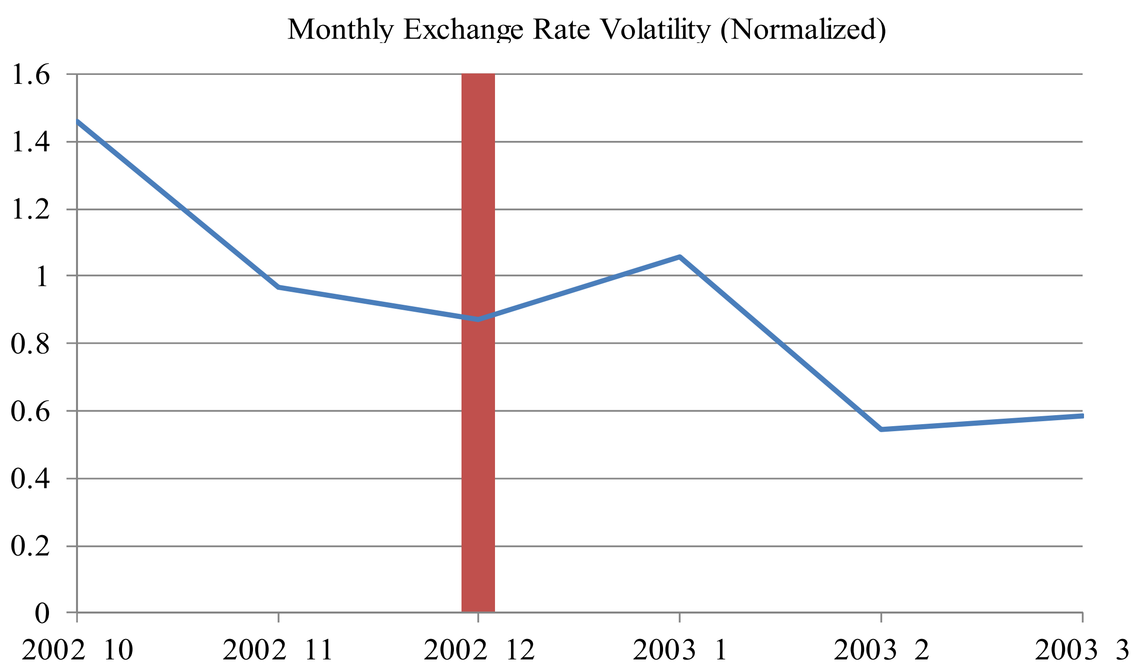
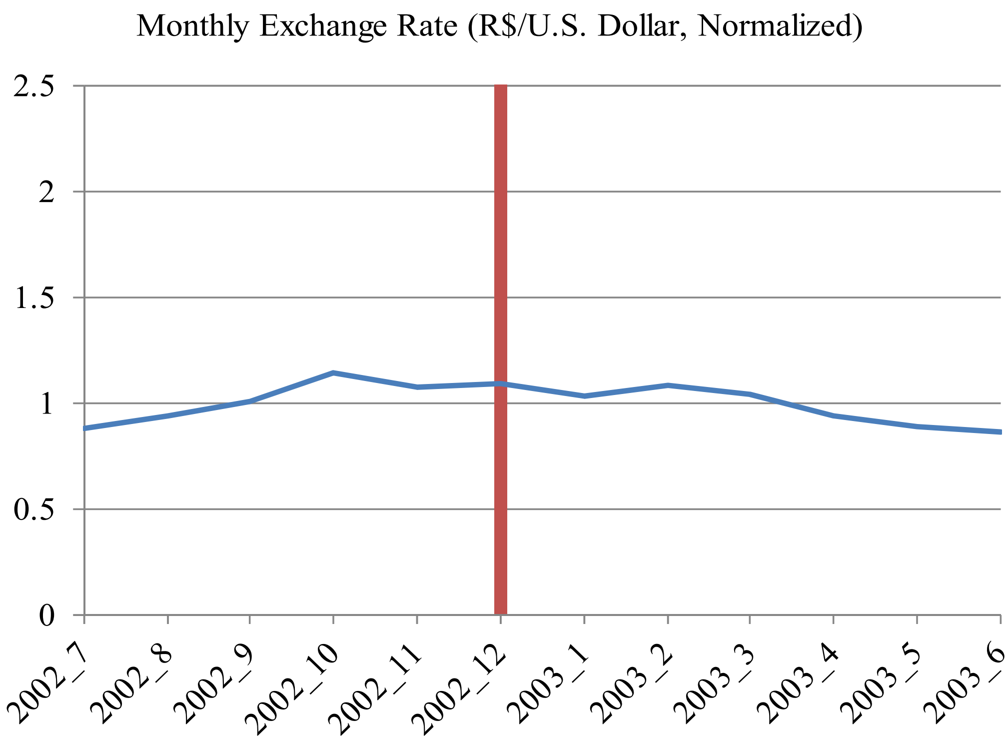


Figure 2

Exchange Rate Level Around the Resolution of Political Uncertainty

This figure shows the Brazilian monthly exchange rate around the major resolution of political uncertainty due to political events that take place in the end of 2002. These events are national elections and the appointment of new members for the presidential cabinet (see text for discussion). For each month, the exchange rate is computed as the average value of the daily nominal exchange rate (R$/U.S. Dollar). Across all panels, the figure plots the monthly exchange rate divided by the average value of this variable between July 2002 and June 2003. Panels A and B report the monthly exchange rate over the same periods analyzed in Panels B and C of Figure 1, respectively. As in Figure 1, the periods prior to the resolution of uncertainty and after uncertainty is resolved are separated by a vertical line.

Panel A: 12-Month Window Around the Resolution of Uncertainty



Panel B: 6-Month Window Around the Resolution of Uncertainty

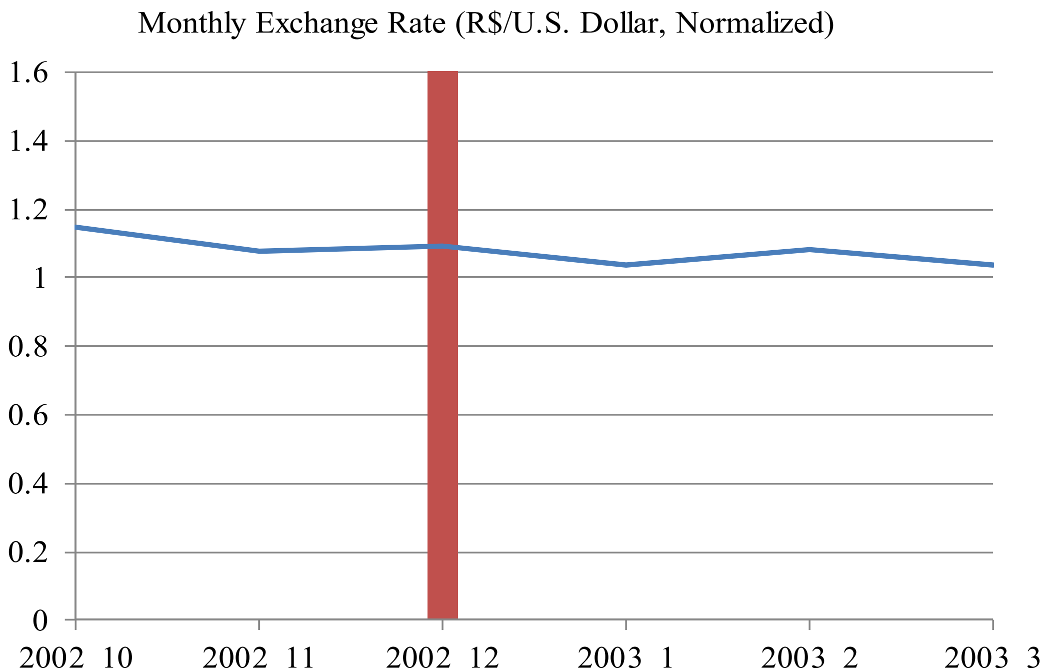


Figure 3
Exchange Rate Level Around Devaluation

This figure shows the Brazilian monthly exchange rate (R$/U.S. Dollar) around a major exchange rate devaluation in January 1999 (see text for more details). The date representing the devaluation is indicated in the figure. For each month, the exchange rate is computed as the average value of the daily nominal exchange rate.