How does Finance Affect Growth? Evidence from a Natural Experiment in Venezuela

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Abstract

This paper examines the relation between financial development and economic growth by looking at plant-level evidence. I argue that by focusing on a largely exogenous event, the implementation of exchange controls in Venezuela in 1983, this approach avoids many of the challenges facing the literature. Most importantly this quasi-experimental setting allows me to deal directly with the issue of causality and makes it possible to avoid the significant challenge of finding an accurate proxy for financial development. To identify the effect of the event on plant performance I employ a difference-in-differences methodology and a variety of treatment-control comparisons. I find convincing evidence that a negative shock to financial development reduces plant output and productivity. Overall, my findings highlight that causality can run from finance to growth.

1. Introduction

Does developing a country's finance system increase economic growth or is financial development mostly a side-effect of economic growth? The direction of this causality between financial development and economic growth is important because it has implications for development policy. Starting with Goldsmith (1969), this has motivated a large theoretical and empirical literature. There is a strong consensus in the empirical literature that there is a positive correlation between financial development and economic growth, however, we still lack a convincing answer to which way the causality goes. On one side of the argument, Miller (1988) claims "[that the fact that] financial markets contribute to economic growth is a proposition too obvious for serious discussion." According to this line of thought, better financial systems are more efficient at allocating scarce capital throughout the economy boosting productivity and capital accumulation rates.[1] On the other hand Lucas (1988) dismisses finance's role as a determinant of economic growth. He argues that finance does not cause growth, but instead mostly responds to changes in demand for it from the real sector of economy. On the same note, Dehejia and Lleras-Muney (2007) suggest that not all financial development may be beneficial for economic growth.[2] It is probable that the truth lies somewhere between these two stories. Supporting this view Jung (1986) finds bidirectional causality between financial and real variables. Moreover, it may be that the interaction between economic growth and financial development may be dynamic as suggested in Greenwood and Jovanovic (1990). Theoretical research suggests that countries with more sophisticated financial systems will have fewer financial frictions, more investment in technology, more innovation and consequently higher growth rates. These higher growth rates are also likely to boost the demand for financial services which makes the financial system increasingly more sophisticated. This creates a virtuous circle. However, the crucial question is what starts this circle. Does the causality go mostly from finance to growth or from growth to finance? This paper exploits a quasi-experimental

[1] Many recent studies support this view that there is a persuasive link between financial development and economic growth. Some prominent examples are Beck, Levine, and Loyza (2000), Guiso, Sapienza, and Zingales (2004), Jayaratne and Strahan (1996), King and Levine (1993), Levine and Zervos (1998) and Rajan and Zingales (1998)

[2] This view is supported by the economic history literature. Examples are Goldsmith (1969), McKinnon (1973), Rousseau (2002) and Sylla (2002). Contemporaneous events in late 2008 also seem to underscore this point.

econometric design to gain new insight into this issue.

The traditonal approach to studying the relationship between finance and growth relies on cross-country studies. Two influential studies by King and Levine (1993a,b) using cross-country evidence showed that financial development affects growth by promoting increases in productivity. Subsequently a large and burgeoning literature looks at the link between financial market development and economic outcomes, using cross-country datasets and methodologies established by La Porta, Lopez-de-Silanes, Schleifer and Vishny (hereafter LLSV: 1997, 1998). These methodologies use legal origin as an instrument for financial development. However, cross-country studies suffer from some serious shortcomings. First, as pointed out in Levine (2005), "one problem plaguing the entire study of finance and growth pertains to proxies for financial development." Although a multitude of indicators of financial development exist, they frequently fail to accurately capture the concepts of financial development emerging from theoretical models. The presence of measurement error in these proxies may significantly bias coefficient estimates of the effect of finance on economic outcomes. Second, cross-country studies are unable to control for all the unobservable factors that affect growth. This unobserved heterogeneity in the data raises concerns about the reliability of inference. For instance, in some models in macroeconomics the propensity of households to save affects long-term growth rates. Since saving rates may be correlated with financial development omitting this variable may induce a spurious correlation between financial development and economic growth. To make matters worse, there is no widely accepted model of economic growth and little consensus on which variables should be included in growth regressions.[3] Another significant challenge faced by cross-country studies is addressing the issue of causality. Recent papers such as Levine (1998, 1999) and Beck, Levine and Loyaza (2000) build on the innovations in LLSV and use improved econometric techniques such as dynamic panel regressions to better address simultaneity concerns. Rousseau and Wachtel (1998) analyze a set of industrializing countries using time series techniques to better characterize the direction, timing and relative strength of causal links

[3] As discussed in Barro and Sala-i-Martin (2003) there exist numerous growth studies that suggest different determinants of growth. These theories tend not to be mutually exclusive resulting in a set of potential variables that is larger than the number of countries in cross-country data sets.

between the financial and real sectors. Bekaert, Harvey and Lundblad (2004) use sharp changes in financial development to help identify the effect of finance on growth. Nevertheless, despite these improvements these studies still suffer from problems such as weak instruments, poor proxies and/or omitted variables that may impede accurate inference.

This paper takes a novel approach to overcome some of these challenges. I focus on a policy reform that represents a sharp shock to the level of financial development in one country. This policy reform [hereafter 'the event'] is the introduction of complex foreign exchange controls in Venezuela. Since the event made it difficult for firms to raise capital, obtain trade credit and contract with foreign suppliers it arguably worsened the level of financial development in Venezuela. Moreover, I am able to isolate a set of plants that are less likely to be affected by the event. Foreign subsidaries still had access to capital and trade credit from their parents abroad and are therefore less likely to face higher financial constraints after the event. Consequently, plants that are owned by foreign firms ('the treated group') are less likely to experience the negative shock to financial development compared to locally owned plants ('the control group'). Comparing the differential impact of the event on these two groups lets me examine the effect of a shock to financial development on real variables such as capital, output, productivity and inputs in a quasi-experimental setting. Splitting the sample into treatment and control groups is crucial for identification. To illustrate this point consider that the event coincided with a sharp recession that likely caused growth opportunities to worsen. [4] In this case differential outcomes in plant-level output may reflect changes in growth opportunities rather than shifts in financial development.

To control for these possibilities I use a difference-in-differences technique. As emphasized in the survey by Angrist and Krueger (1999), this methodology is ideal for analyzing the effect of sharp changes in government policy. By comparing the behavior of the treated and control groups in a short window around the event we can control for changes in unobservables that affect both groups. Intuitively, the comparison with the locally owned plants allows us to observe how the foreign owned plants would have performed without the treatment. However, a critical assumption

[4] As can be observed in Figure 1a Venezuela's GDP fell dramatically in the early 80s contracting 3.5% in 1982 and 6.6% in 1983.

is that in the abscence of treatment the two groups would show similar trends in the outcome variables. For this 'parallel trends' assumption to hold there must be no other variable or event that affects the treatment and control groups differentially. For instance, if foreign owned plants are more apt at dealing with a deteriorating economic environment then this could contaminate my results. In this case, the fact that foreign owned plants outperform could be caused by factors not related to the shift in financial devlopment. To guard against this I carefully check that the parallel assumption is not likely to be violated. I implement matching estimators and recommendations in Smith and Todd (2003), Dehejia and Wahba (1999) and Abadie and Imbens (2006) to ensure that the treatment and control groups are similar and likely to experience similar trends around the event window, but for the effect of the event itself.

This approach has several advantages compared to cross-country studies. First, by restricting attention to an event that represents a significant negative shock to the financial system, I circumvent the challenge of finding an accurate proxy for financial development. As will be discussed in far more detail in the next section, the implementation of exchange controls was a massive intervention in the market and the result was to make it significantly harder for Venezuelan firms to contract for all types of financing.[5] Second, by analyzing a rich set of plant-level data from one country my analysis steers clear of the problem of controlling for unobservable country-level variables. Third, by studying a sharp policy change I am able to assess the causal link between a shock to financial development and plant-level variables. I am also able to test which plant outcomes respond to changes in the financial environment. This sheds light on the channels through which financial development can affect economic growth. The plant-level dataset from Venezuela also offers some additional benefits.[6] As suggested in McKinnon (1973) the influence of financial systems on growth may be the most emphatic when countries are developing. However, the data on emerging economies tends to be poor so most firm or plant-level studies rely for the most part on data from developed countries. Rousseau and Wachtel (1998) take an innovative approach to

[5] It is also comforting to note that this event coincided with the deterioration of other proxies for financial openness such as the IMF capital account indicator in 1983.

[6] I am very grateful to Prof. Ann Harrison for providing the data. The data is also available on her website at *http://are.berkeley.edu/~harrison/#recentwork*

overcoming this problem by looking at the earlier stages of industrialization in developed countries. Since the dataset I use contains data on both private and publicly owned plants, I be able to get a large sample in a country that, like most developing economies, has a small and illiquid stockmarket. For these reasons this study therefore provides a fresh perspective on the link between finance and growth using evidence from an emerging market economy. On the downside, Venezuela may not be representive of a typical developing country. In the early 80s its economy is characterized by Naim (1984) as being heavily oil dependent with a relatively small and state dependent private sector.

Many facets of my study are similar to other recent studies that look at micro-level data and/or sharp policy reforms to tease out the direction of causality. Jayaratne, and Strahan (1996) and Dehejia and Lleras-Muney (2007) both look at state-level banking reform in United States. They use these reforms and difference-in-differences techniques to determine if these improvements to the financial system affected real outcomes. In the same spirit, Guiso, Sapienza, and Zingales (2004) look at how local differences in financial development in Italy affect the real economy. My study is also related to papers that use micro-level data. An innovative study by Rajan and Zingales (1998) uses firm-level data to investigate if financially dependent firms perform worse in economies with higher levels of financial development. Building on this effort Love (2003) estimates a structural model to study the link between financial development and a firm-level proxy for finanical constraints.

My study is also close to studies that use data on emerging markets. Allen, Qian, and Qian (2005) looks at the links between law, finance and growth using Chinese data. Gatti and Love (2008) look at the relationship between access to credit and productivity using Bulgarian data. Two recent contributions that are closely related to this paper are Vig (2008) and Butler and Cornaggia (2008). Vig (2008) exploits a natural experiment and a rich dataset from India to look at the relationship between law and corporate debt structure. Butler and Cornaggia (2008) exploit a sharp demand shock to the demand for corn in the US to study the relationship between finance and productivity in a quasi-experimental setting. Although, many of these papers share similar strategies to this paper, to my knowledge none study the link between finance and growth using a natural experiment and detailed micro-level data from an emerging economy.

My tests give evidence that the activity of foreign owned plants declines less around the event compared to the locally owned plants. My results are particularly strong with regard to output and productivity. Following Aitken and Harrison (1999), I estimate a log-linear production function and find that productivity is higher for foreign owned plants around the event window. This result is robust to different specifications, different event windows and different definitions of foreign owned plants. Another result is that output declines less in foreign owned plants compared to the control group. However, I find little evidence that the number of employees or capital declines more in the treated group. Therefore, this experiment shows that the sharp deterioration in financial development caused by the implementation of exchange controls worsened the relative output and productivity of domestically owned plants. In sum, the evidence suggests that the negative shock to financial development had a direct effect on some real outcomes at the plant-level. It indicates that causality can run from financial development to growth by reducing the productivity of factories. In contrast, my results do not support the conjecture that lower levels of financial development reduce growth by reducing capital accumulation.

The rest of the paper is organized as follows. Section 2 presents details about the 1983 currency reform in Venezuela, while Section 3 presents the empirical methodology. Section 4 provides information on the data. Section 5 presents the results and robustness and Section 6 concludes.

2. The Event

To identify the effect of financial development on economic growth I study the implementation of currency controls in Venezuela. Since this event is critical for my empirical design it is vital to spend time discussing the event in depth. This section will first cover the timeline and important facts about the implementation of the exchange controls. Next, I will provide convincing evidence that the event was for the most part unexpected by firms and is therefore a largely exogenous shock. Third, I will argue that the currency controls represent a negative shock to financial development in Venezuela. Lastly, I will discuss in detail how this event can be used to split the sample into plants that are likely to be affected by the exchange controls ('the control group') and plants that are not ('the treated group').[7]

The exchange controls were first announced on February 28 by President Luis Herrera Campins, however, the most famous day in local history is Friday February 18, 1983. On Black Friday as this day is know locally, the foreign exchange market in Venezuela was suspended without warning and all buying and selling of currency was banned temporarily. The following ten days were rife with speculation about what type of exchange regime would be established. Most players seemed to anticipate that some type of devaluation and/or exchange controls were at hand, however, there was substantial uncertainty until the multi-tiered exchange rate was finally introduced. It replaced a pegged exchange rate to the US dollar that had remained unchanged at 4.3 Bolivares per dollar (Bs/$) since 1960.

This new exchange rate regime had two official exchange rates and one parallel exchange rate. The first rate - a preferential rate of 4.3 Bs/$ - was available for (1) repaying principal on medium and long-term debt on the books before the event and (2) importing 'essential' products.[8] Only 'national firms' (defined as firms that had more than 51% national ownership) could use this preferential rate for debt repayment, however, there was no such restriction on importing 'essential' products. The second tier - a preferential rate of 6 Bs/$ - could be used to pay for non-essential imports. The last tier - where firms could transact local Bolivars (Bs) freely for foreign currency - plummeted to about 7-8 Bs/$ immediately after the foreign exchange markets reopened. Local sources report widespread jostling among firms about what firms and products would be considered to be 'essential importers and imports.' Ultimately, the final lists included firms engaged in industries considered national priorities and products such as food, pharmaceuticals, and some raw materials, semi-processed goods and finished products. The three-tier exchange system was further tweaked in early 1984 after the election of Jaime Lusinchi to the Presidency in general elections

[7] The treated group is typically the group to which the treatment is applied. However, for reasons of convenience the treated group in this study is the group that was not affected by the negative shock to financial development.

[8] The initial announcement did not allow repayment of private debt. However, after pressure from the business lobby, the law was changed to allow private companies to use this rate to repay debt contracted before February 18.

held in late 1983. The most important changes were that only national companies (at least 80% local ownership) now qualified and preferential foreign exchange could also be approved for interest payments (at 7.5 Bs/$) for these companies. The details of the exchange rate system are illustrated in Figure 1b.

The reform also created a new government agency known as Regimen de Cambios Diferenciales (RECADI) that was given the responsibility of processing applications for access to preferential exchange rates and for disbursing foreign exchange at those rates. As can be seen in Figure 1c and Figure 1d, the premium between the official rates and the free market rate was substantial. It peaked at 261% (the free market rate as a per cent of the official rate) in July 1983 and generally hovered at or slightly below 100% from late 1983 to 1985. This sizable premium provided firms with powerful incentives to apply for foreign exchange at the preferential rates, and the result was intense competition to get dollars approved at the subsidized rates and a large incentives for agents to file bogus or fraudulent requests for subsidized foreign exchange. To minimize such requests and prevent abuse, the application process for access to preferential rates was made difficult. As will be discussed later this was especially true for applications to use preferential rates for debt and interest payments.

2.1. Exogeneity of the event

It is critical for my study that this event is largely an exogenous shock to firms. If firms anticipated the exact nature of the exchange controls they would change their optimal behavior in advance of the announcement. This would muddle my identification strategy as the reaction of firms to the expected policy change would occur before the event date.

On the one hand, it is clear that the rapidly deteriorating macroeconomic environment in late 1982 made some policy change imminent. It was abundantly clear in late-1982 and early 1983 that Venezuela - like much of Latin America at the time - was facing an economic crisis. The sustained fall in international oil prices in the early 80s cut Venezuelan oil exports revenues from $19.3 billion in 1981 to $15 billion in 1982. Since oil exports account for about 90% of the dollar value of Venezuela's exports, this reduction in export earnings caused a severe balance of payments crisis. One symptom was the rapid decline in foreign reserves from \$18.7 billion in 1982 to \$11.5 billion in early 1983. Although Venezuela's overall debt level was manageable, it faced a significant short-run liquidity crunch. It had \$8.7 billion of short term loans due in late 1983 (out of total debts of \$18.5 billion), and international banks were resisting pressure from Venezuela to restructure the debt until reforms had been implemented. Making some type of currency reform more urgent, the overvalued exchange rate incentivized imports (\$12 billion in 1981) and reduced the value of oil exports in local currency. Judging by the increasing rate of capital flight and the evidence above it is likely many firms inferred from the macroeconomic environment that reforms were imminent.

Given this expectation, what type of reform package did the market expect? Market sentiment at the time pointed towards a gradual devaluation as the most likely outcome.[9] This was in line with the recommendations issued by the IMF that called for a devaluation and some modest curbs on capital outflows along with a structural adjustment package. Moreover, the markets speculated that the looming general election in December 1983 and bad polling numbers, made it politically expedient for President Campins to either avoid reform entirely or at least avoid implementing a risky reform program. A small devaluation and a token structural reform package (demanded by international creditors in exchange for restructing debts) may have allowed him to muddle through until the elections. This expectation reflected a widely held belief that the economy was fundamentally robust. For instance, although both S&P and Moody's lowered Venezuela's credit rating in early 1983, both expressed that Venezuela's debt position was fundamentally strong in February 1983.

The devaluation case was bolstered by the support of the powerful central bank Governor Leopoldo Diaz Bruzal. However, while Bruzal argued for a gradual devaluation (4.3 to 6.5-8 Bs/USD), the finance minister, Arturo Sosa, favored exchange controls. Sosa had first suggested exchange controls in January, but these were dismissed out of hand by President Campins. The dispute between Sosa and Bruzal was bitter and contentious, and Campins did not decide between the two alternatives until early morning on February 28. Most cabinet member did not know any-

[9]Based on a careful reading of new sources in the months before the event.

thing until the same day the reforms were announced.[10] Not only do these disagreements about policy reinforce the argument that the exchange controls were not planned nor expected, but it all but eliminates the probability of leaks to the private sector. The amateurish way the exchange controls were introduced strengthens the case that they were not planned. Not only was the foreign exchange market frozen for two weeks before the government finally issued the rules for the new foreign exchange regime, but there was also, as suggested in Jacque (1997), widespread bewilderment about the details of the exchange controls and how they would actually work. Worse, it took up to two to three weeks before clarifications were made. Major and minor changes to the law were made frequently after the exchange controls were introduced to adapt them to changes in political priorities, lobbying and other pressures. The result of this lack of planning was confusion and frustration in the business community and significant delays in disbursing funds.

In sum, even if currency controls were expected by some firms, it is unlikely that the final convoluted exchange regime with multiple exchange was expected. Certainly Venezuela was no stranger to currency controls. The reform was inspired by and modeled on a similar and ultimately successful devaluation in 1960. Various types of currency controls were also prevalent at the time, and Reinholdt and Rogoff (2004) report that dual or multiple exchange rates were commonplace in developing countries in the 1980s. Nevertheless, the exact nature of exchange controls was unique as pointed out by Dornbusch (1986).

Lobbying by business can be a powerful influence on economic policy. If Venezuelan businesses were able to determine the timing and/or nature of the exchange rate policy then the event is unlikely to be an exogenous shock to the firm. Successful lobbying would imply that changes in exchange rate policy is an endogenous variable in the firm's optimization problem, and I would no longer by able to exploit the exogenous variation in financial development and analyze its effect on plant-level variables. One indication that the lobbying story has merit is the swift extension of the exchange rules to include private debt. However, according to Bond (1987) this decision rather than reflecting Fedecameras's (the umbrella organization representing most Venezuelan private-

[10] As reported in Beroes (1990) who interviewed both Bruzal and Sosa in 1989 and extensively covered the events as a journalist for the largest Venezuelan daily El Nacional.

sector groups) lobbying power, this change was due to pressure from international creditors. In fact, many sources claim that the currency reform was adapted in face of fierce opposition from the Venezuelan business and despite lobbying by Fedecameras. Salgado (1987) and Bond (1987) both underscore this point by pointing out that Fedecameras had become a shell of its former self by the late 70s due to the increasing state presence in the Venezuelan economy. Bond makes the most forceful case stating that Fedecameras was "unable to achieve private sector consensus, lobby effectively, or withstand encroachment by the state." In sum, it is unlikely that lobbying was significant enough to make the event endogenous and jeopardize identification.

To summarize, although there is some evidence that exchange controls were expected, it is not clear that this expectation was widespread. Even if exchange controls were in the information sets of some firms it is unlikely that these companies were able to foresee the complicated design nor the timing of the exchange controls. Moreover, it is unlikely that lobbying by firms had a significant impact on the final decision or design of the exchange control system. Overall, it seems conceivable that after the 23-year run with an stable and unrestricted pegged exchange rate the regime switch came as a surprise to Venezuelan firms.

2.2. Financial Development

In this section I will present evidence that the implementation of exchange controls in Venezuela constitutes a negative shock to financial development. To do that I will first briefly discuss what is meant by financial development. In the words of Merton and Bodie (1995) financial systems arise to mitigate transaction and information costs, and by reducing these frictions facilitate the allocation of resources in the economy. Levine (2005) argues that financial systems perform five functions. They (1) facilitate the trading, hedging, diversification and pooling of risk, (2) allocate resources, (3) monitor managers and exert corporate control, (4) facilitate exchange of goods and services and (5) mobilize saving. Countries with financial systems that better perform these functions have a higher levels of financial development.

Did the reform lower the level of financial development in Venezuela? The reform represents a large intervention by the government into the market for foreign exchange.[11] Although firms were free to transact at the free-market rate, given the large differential between this rate and the official rates companies were likely to postpone a transaction if they thought they could gain access to the preferential rates. Given the confusion and frustration surrounding the rules governing the exchange controls and the ensuing slowness in disbursements, firms faced substantial uncertainty when making contracts involving foreign exchange. Venezuela was hugely dependent on imports of capital goods such as machinery, replacement parts and semi-processed goods, and the increasing import dependence during the era of high oil prices in the late 70s, made it hard for firms to substitute locally produced products for imported capital goods. The only way many firms could avoid insolvency faced with the recession, a sharp devaluation of the free market rate and price controls was to access the subsidized rates through RECADI.[12]

The effect of the reform on financial development is even more obvious when we look at the capital markets. Many Venezuelan companies had debt dominated in foreign currency and were dependent on trade credit from foreign suppliers. Therefore getting access to preferential rates was a key priority for business and the main trust behind their lobbying efforts. Despite these efforts there is abundant evidence that little foreign exchange was made available for debt repayment at preferential rates in the period 1983-84. Salgado (1987) reports that the government had demand reduction mechanisms such as "tedious bureaucratic procedures, delays, and announcing tentative decisions for some solicitants while confusing others." Conflicting views at the top of government contributed to the delays. Despite repeated promises to authorize subsidized dollars for debt, the government's efforts were stymied by Bruzal who as head of the central bank refused to deliver the dollars for legal reasons.[13] The result was paralysis. By December 1983 Business Week was

[11] An earlier central bank decision in September 1982 centralized the dollar reserves of PDVSA (Petroleos de Venezuela; the state oil monopoly). Given PDVSA's role in generating about 90% of Venezuela's export earnings, this decision combined with the exchange controls gave the government almost total control of foreign exchange markets.

[12] A temporary two month price freeze was imposed in conjunction with the exchange controls on February 28, 1983. On April 20, 1983 this was replaced by an extensive system of price controls that was left largely intact until the late-80s. On April 1 the government announced a mild austerity adjustment program aimed at curbing imports, controlling inflation and cutting government spending. Since both these measure were broad they are likely to affect all sectors of the economy and unlikely to contaminate my results.

[13] This evidence is corroborated by Bruzual in an interview granted to a local daily in 1989 that not a single request for subsidized dollars for debt repayment had been granted during his tenure at the BCV (he was replaced in February

reporting that the status of foreign debt held by private companies was in limbo with $1 billion interest now in arrears (out of total debt estimated at $7-8 billion). Various news sources agree that almost nothing had happened with private debt by February 1984, and in July 1984 the Wall Street Journal claimed that little had been paid on private debt in the past 18 months. Even by the end of 1985 only 45% of the amount solicitated had been paid out.

Although the concurrent recession contributed to a tightening of financial constraints, the exchange controls made the situation much worse. Local sources state that the delays and chronic uncertainties regarding payouts at preferential rates had caused more problems for business than devaluation. Short-term trade credit - the only kind of loan still extended to debt-ridden Latin America - almost completely disappeared. Reuters reported in July 1983 that bankers were worried that local importers were unable to pay debt at the new rate and that trade credit was drying up.[14] Illustrating the basic problems faced by companies, the President of local company Finalven announced to the local press that, "we've wasted a lot of time and we've had a very confused relationship with our creditor." PepsiCo reported that it made no shipments to Venezuela in the year ago quarter (third quarter 1983) due to so-called "currency related uncertainties." In sum, little happened to foreign debt held by private companies in 1983 or 1984 generating widespread distrust among creditors. Worse, the uncertainty generated by the exchange controls caused a spike in financial frictions and eliminated many financing options that otherwise would have been available. In conclusion, it is clear that the exchange reform made the Venezuelan financial system work less well by raising the transaction and information costs of raising capital, and making it more difficult for Venezuelan firms to contract with foreign suppliers and exchange goods and services. Therefore, it is valid to argue that the reform reduced the level of financial development in Venezuela.

2.3. Treated and Control Groups

Even though it is clear that the event worsened financial development, to identify the effect of this shock on plant-level outcomes it is critical to split the sample into firms that are likely to be affected

1984).

[14] Firms had few alternative sources of external capital. The local stockmarket was illquid and tiny, and the local banking community had been shaken by the failure of the biggest commerical bank in late 1982.

by the exchange controls ('the control group') and the firms that are not ('the treated group')? In this section I discuss the main approach used in this study and several alternative splits that I rely on for robustness. The main approach relies on the argument that foreign owned plants are less likely to be affected by the exchange controls. I also split the sample based on industries that are more likely to get access to preferential rates.

There are several reasons why foreign subsidiaries are less likely to be affected by the exchange controls. Most importantly they are likely to have better access to capital from their parent companies abroad to avoid the spike in financial constraints faced by local companies. In contrast to locally owned plants who faced a dearth of financing, foreign subsidiaries could access the internal capital markets of their parent. This lifeline enabled foreign subsidiaries to continue pursuing growth opportunities and enabled them to exploit this advantage in the product markets.

This behavior is consistent with the literature on the behavior of multinationals. Desai, Foley and Hines (2004) show that multinationals adjust their financial choices according to local capital conditions. They further document that subsidiaries of multinationals prefer to borrow from the parent company rather than unrelated parties if local market conditions are unfavorable. Further, Desai, Foley and Forbes (2004) find that foreign subsidiaries outperform local firms during a currency crisis. In sum, there is support in the literature for the view that foreign subsidiaries can circumvent capital controls using internal capital markets.

Is assignment to the treated group random? If the assignment is not random and the variable determining the assignment is correlated with plant performance, then this will imperil identification. For instance, if foreign investors choose to have stakes in plants that are more likely to outperform when faced with capital control, then this choice and not financial development may explain why foreign owned plants outperform. Although assignment is not random in my study, I consider it to be a plausible assumption in the short-window around the event. First, rules on foreign investment in Venezuela prohibit investors from buying direct equity stakes in existing companies. Therefore foreign ownership is likely to be more sticky than in other countries since the costs to changing your ownership position are significant. Second, since the event was largely unanticipated it is unlikely that the variables that induced the foreign investors to invest in the plant are correlated with how the plant is likely to fare under capital controls. This is merely one of many variables that determine whether a plant is foreign owned.

My hypothesis is therefore that foreign owned plants will outperform locally owned plants in a window around the event. It is important to note that this is not the outcome that policymakers had in mind. In fact, foreign subsidiaries were not supposed to benefit from the reform. The 1983 rules prohibited foreign subsidiaries from accessing the preferential rates for repayment of debt. However, as documented in the previous section, almost no debt repayments were authorized until late 1984, and it is therefore unlikely that locally owned plants benefited from the preferential rates until the beginning of 1985 at the earliest.

Another natural approach to splitting the data relies on differential treatment of industries. The lowest preferential rate was reserved for 'essential imports' as defined by government priorities. Although the lists of products and industries that had access to subsidized exchange changed in response to both industry pressure and changing government priorities some common themes emerge. Dornbusch (1986) concludes that food, pharmaceutical and petroleum industries were considered high priority. Unfortunately it is likely that the approval process was gamed by corrupt businesses and bureaucrats. For instance incoming administration of Lusinchi in early 1984 discovered that a lax disbursement process had led to disbursements to businesses not involved with 'essential imports.' In response the rules were tightened and the head of approving 'import applications' was jailed, but corrupt practices are still likely to muddle what industries actually gained access to subsidized currency. Moreover investigations in 1989-1990 revealed massive corruption in RECADI. Although most of this occurred in the later years of the system, it indicates that corruption may have blurred the lines between 'essential industries' and other industries to such a large extent so as to make splitting the sample along these lines problematic.

3. Empirical Design

As I argued in the previous section, the implementation of exchange controls in Venezuela allows me to take advantage of a source of exogenous variation in financial development. This is useful in this context as most causal studies linking financial development and economic outcomes suffer from endogenous variation due to problems with omitted variables, selection and simultaneity. However, as the shock to financial development coincided with - and was partially a consequence of - a sharp recession (see Figure 1a), it is not sufficient to compare plant performance before and after the event. For instance, a decline in economic variables after 1983 could be caused by unobservables such as a contractions in domestic demand instead of the shock to financial development. It is therefore key to carefully disentangle the effects of the recession on plant behaviors from the effect of financial development. To do this I implement a difference-in-differences (DID) empirical design.

Intuitively, DID compares the effect on an event (exchange controls in this study) on a group that is affected by the event (the treated group) to a group that is unaffected (the control group). To find the effect of the event on plant performance we can subtract the performance of the plants before the event from the performance of the plants after the event. This difference gives the change in plant performance during the event window. However, other variables that impact plant performance may also have changed during the event window. Therefore, we need a control group to control for common shocks that can affect plant performance. To achieve this subtract plant performance before the event from plant performance after the event for both treated and control groups. This difference tells us how the event affects each group individually. The effect of the event on the control group gives us the counterfactual: if it were not for the treatment (exchange controls) how would treated plants have performed during the event window. To get the second difference we compare the differences in the treated group to the differences in the control group. This second differences allows us to control for common shocks that can affect both groups (i.e. the recession in this study). Thus, differencing in this manner eliminates any biases on the treatment group that may arise from any other common changes that could affect the treatment group during the event window other than the reform.

More formally, let y_{it} be the plant-level variable of interest and d_t be event dummy that is one after the event occurs and zero before. Let z_{it} be a vector of controls that contains factors that affect y_{it} and that changed around the event. I estimate:

$$y_{it} = \alpha + \beta d_t + z_{it}\delta + \epsilon_{it}$$

The identifying assumption is that $\beta = 0$ in the absence of treatment, or equivalently assuming that $E(z_{it}, d_t|\epsilon_t) = 0$. If there are important unobservable variables missing from z_{it} then this assumption does not hold and the simple differences estimator β is a biased estimate of the effect of the event on y_{it}. As discussed above this is unlikely to hold in this study. To avoid this confounding affect on β we split the sample into a treated group that is affected by d_t and and control group that is not. Then we estimate the full specification:

$$y_{it} = \alpha_j + \gamma_t + \beta d_t + \omega T_i + \theta d_t T_i + z_{it}\delta + \epsilon_{it} \tag{1}$$

Here t indexes times, i indexes plants, j indexes industries, α_j is an industry dummy, γ_t is a time dummy and T_i is an indicator variable that is one if the plant belong to the treatment group and zero otherwise. The coefficient on the interaction term θ gives the difference-in-differences estimate of the affect of the event on plant performance, y_{it}. Unbiased estimation of θ is not affected by common trends in both the treated and control group, but trends that differentially affect treatment and control groups can induce bias in θ. This known as the 'parallel trends' assumption. For instance, if foreign owned plants are hit with an unobserved technological shock that increases their productivity around the event window this will bias as my results towards finding a positive θ. To guard against the presence of differential trends I use matching estimators to find comparable plants in the control group for each treated plant. As is discussed in more detail in the next section, I carefully implement these estimators using the advice in Smith and Todd (2003), Dehejia and Wahba (1999) and Abadie and Imbens (2006). This matching exercise ensures that the treated and control groups share similar characteristics and are more likely to experience common trends around the event window.

4. Data

I utilize a rich plant level data set from Venezuela that has been used in previous studies by Aitken and Harrison (1999) and Aitken, Harrison and Lipsey (1996). Whereas these papers focus on the effects of foreign investment over the whole 1976-1989 sample period, I study the whether the 1983 currency reform reduced plant performance. These authors obtained the data set directly from Venezuela's National Statistical Bureau (OCEI). This survey is conducted at annual intervals during the sample period.[15] During the sample period OCEI collected survey data from all plants with more than 50 workers. In addition, they also surveyed a large sample of smaller plants. In the interests of confidentiality, the survey was released without firm identifiers. To remedy this Aitken and Harrison (1999) carefully relink plants over time by using data items such as capital stocks, location, ownership, industry, number of employees and other information. The final dataset, that I use directly, includes 43,010 plant year observations. The data is not a balanced panel as the total number of plants varies across each year of the sample. The dataset has information on plant ownership, output, employment, input costs, capital, number of employees and location.

Output is defined as the value of sales less the change in inventories, deflated by an annual producer price deflator which varies across four-digit industries (ISIC). The dataset also includes detailed information on the inputs into production. Skilled and unskilled labor is defined as the number of workers. The number of workers is used since the number of hours worked is not available across all plants. Capital stock is the beginning of year capital stock deflated by the GDP deflator. Material costs are adjusted for changes in inventories, and then deflated by a production price deflator. Electricity consumption and expenditure are both available, but as an input electricity consumption is preferred due to the presence of an array of subsidies and a complex pricing structure for electricity. The location of the plant is defined at the regional, district or municipal levels. There are also several variables that proxy for regional differences that may affect plant performance. Real wages is a regional index of real wages for skilled workers where the plant is located. Electrical prices are regional energy prices. Aitken and Harrison (1999) find that an increased share of foreign

[15] The sample does not include 1980; the survey was not conducted in census years.

ownership in the sector negatively impacts the performance of locally owned plants. As in their paper the share of foreign ownership is defined as labor-weighted share of foreign ownership in the industry. Industries are defined by the four-digit ISIC classification which varies from 3111 to 3999.

Foreign ownership is defined as the percentage of subscribed capital (equity) owned by foreign investors. A foreign owned plant (or equivalently foreign subsidiary) is defined as all plants that had less than 80% local ownership. Venezuelan law considers companies with more than 80% local ownership to be national companies; companies with 50-80% local ownership are mixed; and companies with less than 50% local ownership are foreign. Neither foreign nor mixed companies had access to preferential rates for debt repayment after early-1984 when mixed companies were excluded. Therefore that fact that mixed companies were initially included in 1983 should not affect my tests as almost no requests for debt repayment were granted during 1983. One concern with the decision to extend the definition of foreign owned plants to include mixed companies is that the willingness of parents to let subsidiaries access their internal capital markets declines with ownership share. However, the mean level of foreign ownership in my main sample is 60% a level where this concern is unlikely to be a significant factor. To further alleviate this concern I consider more restrictive definitions of foreign ownership in robustness checks.

As I am investigating the impact of an event, it is critical to define the event window carefully. Since the event occurred in February 1983, a natural event window would be to compare plant performance in 1982 (pre-event) to plant performance in 1983 (after the event). The advantage of this design is that it is centered around the event and therefore gives the cleanest identification of the effect of the exchange controls on plant-level variables. Using observations that are further away from the event risks picking up the effects of other events that may affect plant performance potentially biasing the results. However, it is likely that it will take some time for the effect of the event to affect firm behavior and outcomes. To account for this lag and to have more plant-year observations in my tests, I define my main event window with 1982 being the pre-event baseline and comparing this to the post-event performance from 1983 to 1984. As RECADI started to authorize and disburse more money for debt repayment in late 1984 and 1985, I avoid having 1985 as part of my event window. I explore many other variations of how to define the event window in the results section. For instance, a natural extension is to use more than one year in the pre-event period to get a more accurate predictor of the plant's performance before the event.

5. Results

5.1. Summary Statistics

The means and standard deviations of the data are presented in Table 1. One important observation is that the locally owned plants (the control group) are significantly smaller than foreign owned plants (the treated group). Both in the whole sample in the second column and the event sample the control group is much bigger when measured by capital, output or total employees. The average foreign owned plant has three times as many workers as a domestic plant. To have basis of comparison the average size of a foreign plant is my sample is about 20% smaller in term of numbers of employees compared to the average plant for publicly traded US companies.[16] In untabulated results foreign owned plants tend to cluster in different industries (defined at the four-digit ISIC level) than domestic plants. The share of foreign ownership also varies significantly across industries.

It is crucial for my empirical design that the plant's treatment status does not change around the event window (i.e. from treatment to control). I therefore eliminate plants that either change status or do not have at least three consecutive observations during the event window. As can be seen in the second column, this reduces the sample to 7674 plant-year observations. As can be observed by comparing the two samples, the significant drop in means indicates that many smaller plants were eliminated. For instance, the average plant now employs more than twice as many workers. It is comforting to note that there is not much difference between the event sample (1982-84) in column three and the whole sample. The average plant is a little smaller which likely reflects the economic downturn during the event window.

An important take-away from Table 1 is that foreign ownership is low on average. In the whole sample only 2.5% of equity is owned by foreign investors. This translates into a small treatment

[16] This is the average of plant-level employees from the Longitudinal Research Database - Compustat matched sample in Bertrand and Mullainathan (2003).

group compared to the much larger control group. For instance, during the main event window from 1982 to 1984 I only have 39 plants in the treated group compared to 708 in the control group. In robustness checks I use less stringent criteria when selecting plants with the goal of maximizing the number of plant-level observations. For instance, I keep plants that have at least one observations on either side of the event window. In this larger sample the results are qualitatively similar.

5.2. Response of productivity

Interesting anecdotal evidence about how the behavior of the treated and control may have varied around the event window can be gleaned from Figure 2. It plots trends in the log of output, number of employees and capital from 1976 to 1989 for foreign and locally owned plants. Overall, both sets of plants show similar trends over the sample period. Further both groups show declines in economic activity starting in the early 80s. These declines are particularly pronounced around the event driving home the importance of controlling for the effect of the business cycle on plant-level outcome. Focusing on the trends around the event (indicated by the circle), shows that while number of employees and capital displayed similar tendencies after 1983, output is markedly different between the two groups. Treated plants show a significantly smaller drop around the implementation of exchange controls than the control group of locally owned plants. It is interesting to note that after 1984-85 both groups show very similar trends in output. Naturally, these results are only indicative given the lack of control variables, but they are striking nonetheless.

I implement (1) by estimating a log-linear production using plant-level panel data. Given that my data contains detailed information on the factors of production, I expect (1) to be well specified in my sample. As a result any differences in relative output can be attributed to a total factor productivity effect. Unfortunately there are no good models for estimating (1) for other interesting plant-level outcomes such as total labor and capital using my dataset. Table 2 presents the results from estimating (1) on various subsamples of the data. The first column of Table 2 indicates that foreign owned plants have higher productivity than local plants. Also the event dummy enters with a negative sign, but this result is not robust across specifications. The inputs into production all enter with the predicted positive signs and these results are very robust and stable across all specifications and samples.

All the remaining columns in Table 2 investigate the effect of the event on productivity using a difference-in-differences framework. All specifications include year dummies to control for aggregate economic shocks. Some specifications also control for productivity differences across industries by including industry dummies at the four-digit ISIC level. The results indicate that the foreign owned firms tend to outperform local firms in response to the shock to financial development. The coefficient on the interaction term is positive and significant across most specifications and robust to changes in the event window. The coefficient estimates mean that the event affects to scale of production of foreign owned plants less negatively than local competitors. More precisely output is likely to be about 8% higher after the event if the plant was foreign owned. Since I already control for the differences in inputs, this improvement is a total factor productivity gain. This result indicates that the event has large effects of relative output and productivity, and suggests that effect of financial development on plant outcomes is potentially large and economically significant.

Columns 8 and 9 present the results of sample splits based on size. Small firms tend to face larger financial frictions than larger firms. For instance, estimation results in Hennessy and Whited (2007) suggest financing costs are almost twice as large for small firms. Since larger firms tend to have larger plants, I predict that larger plants are less likely to be affected by the credit crunch as a result of the implementation of exchange controls. The rationale is that having access to the internal capital markets abroad is likely to be more valuable if the plant faces a higher level of financial constraints. Large plants are defined as those that have more than 150 workers, but I also do robustness checks in which I define size by capital stock and use higher thresholds for splitting the sample. The results are consistent with this prediction. In smaller plants the effect of the event on output is substantially larger (11% vs. 3%), however, the coefficient is only significant at the 10% level. Moreover, in the sample of large plants the coefficient on the difference-in-differences variable is no longer statistically significant. However, these results are not robust as I increase the threshold for splitting the sample.

It could be that these results are driven by unobserved locational factors. For instance, it could be that foreign owned plants are more productive because they locate in regions where they are more likely to benefit from better infrastructure or positive externalities from neighboring industry clusters. Table 3 addresses this concern by introducing regional control variables. The dataset has information on the location of the plant by region or district. To control for regional differences in productivity I include location dummies in some specifications. Moreover, I consider some proxies for regional productivity. One is real wages. As suggested in Aitken and Harrison (1999) this variable may pick up unobserved differences in the quality of labor or other locational advantages. Another is electricity prices. The Venezuelan government had a policy of trying to attract industry to certain regions, and differential energy prices were used to achieve this goal. Therefore electricity prices may pick up unobserved government policies that favor certain industries. Finally, I include plant-fixed effects in some specifications. If the effects of location on productivity are fixed over time then fixed effects will reduce the any bias in my coefficient estimates.

The results are consistent with my findings in Table 2. The coefficient estimates on the difference-in-differences variable is positive and significant across specifications. It is robust to various regional controls, different event windows and including firm fixed effects. The result that the effect of the event on output is stronger for small firms is replicated in column 9. More importantly, it is significant and much more robust when controlling for locational factors. This indicates that foreign ownership is particularly useful for alleviating high financial constraints in smaller plants. It also tentatively suggests that improving financial development can boost growth by alleviating financial constraints for small firms. This is consistent with the conclusions of Beck, Demirguc-Kunt, and Maksimovic (2004) who find that the extent to which financial development constrains firm growth depends on firm size.

A key identifying assumption behind the my empirical strategy is that in the absence of treatment the difference-in-difference variable would be zero. Intuitively, this means that if there were no exchange controls then, controlling for other variables that affect output, we would expect to see the same trends in output across both groups. Equivalently if we estimated (1), and the event had not occurred, then the change in output would be the same for both foreign or locally owned plants. One way to check that this assumption (often called the 'parallel trends' assumption) is satisfied is to examine the trends in the control variables during the pre-event period for both the treatment and control group. Table 4 undertakes this exercise in the pre-event period from 1976 to 1982. These results are troubling since all the means are significantly different from each other. Treated plants are significantly larger. They are also located in different industries and regions than locally owned plants. This indicates that the parallel trends assumption may not be satisfied in this sample, and opens the door for alternative explanations for my results. For instance since treated firms are more capital intensive, other factors that are correlated with size and differentially affect productivity during the event window may bias my estimates. To mitigate these concerns I undertake a matching approach.

5.3. Matching

A matching estimator helps satisfy the 'parallel trends' assumption by matching similar plants in the treated and control groups. By reducing the differences between the treated and control groups we are more likely to isolate the effect of the exchange controls (the treatment) on output. Traditional matching estimators faced a 'curse of dimensionality problem', but recent advances have allowed a multi-dimensional matching exercise to be implemented. This paper implements propensity score matching as described in Smith and Todd (2003) and Dehejia and Wahba (1999). Intuitively the propensity score is the probability of being treated conditional on pre-treatement variables. To place this in the context of this paper, it is the probability that a plant is foreign owned given a set of plant attributes. To estimate this probability I estimate a probit regression at the plant level of an indicator variable of being treated on set of plant-level characteristics.

It is common to select a set of conditioning variables that maximizes the probability of successfully predicting the treatment decision. In this setting that means finding variables that predict the propensity of plants to be foreign owned. However, Heckman and Navarro-Lozano (2004) find that these selection rules are not always successful in reducing bias. Instead they suggest using the 'minimum relevant' set of conditioning variables. These are variables that affect both the likelihood of being a foreign plant and plant-level output. Since foreign plants tend to concentrate in certain industries and regions, I include foreign ownership share and real wages. Further, as shown in Aitken and Harrison (1999) foreign investors tend to invest in larger and more productive plants. To proxy for this effect I include the pre-event averages for inputs and output. The results from estimating this model are in Table 5. Most variables in the conditioning set enter significantly. Many of coefficients are sensible i.e. foreign ownership or output in the plant's sector increases the likelihood of a plant being foreign. The pseudo R-squared is a respectable 0.36 indicating that the specification scores well on the goodness-of-fit criteria for selecting control variables.

The effectiveness of the matching procedure is revealed by studying Table 4. A new comparison in column 2 between treatment and control groups shows that the two groups are significantly more similar than before the matching exercise was carried out. Most paired t-tests comparing the difference in means are now unable to reject null that the means are similar. Additional evidence is provided by re-running the probit on the matched sample. The R-squared has declined to 0.06 and most conditioning variables are now insignificant. This underlines that the matching process has removed most meaningful differences between the two groups. Therefore it is likely that in the post-match sample the 'parallel trends' assumption holds.

Matching is done based on nearest neighbor matching. After calculating propensity scores for all plants, each treated plant is placed in a group with the plants that have similar propensity scores. The procedure allows plants to be used more than once. This technique of matching with replacement has, as is emphasized in Abadie and Imbens (2006), the advantage of reducing bias while increasing variance. These authors also suggest using four matches to minimize MSE. In addition, I have many more controls than treated plants suggesting I can incorporate more information without reducing the quality of my matches. Nevertheless, I use three matches in the reported specification because I find that the 'parallel trends sample' is more likely to be violated if I use more than three matches. To make sure the matching procedure performs well I study the distribution of propensity scores. This is done to make sure that there are no ranges of propensity scores where there are no close matches.

Table 6 reports the difference-in-differences estimates on the matched sample. Since the plants are already been matched on most of the control variables used in Table 2 and 3, I simply compare differences. This has the added advantage of giving insight into how capital and labor changed in response to the shock to financial development. Unlike production there is no well-specified model that I could use to estimate (1) using labor and capital. The results indicate that total employees increased 2% more for foreign firms while capital decreased by 3% more compared to the performance of locally owned plants. However, both estimates are insignificant. Of more interest is the estimate of how the event affects production. The estimate on output indicates that output increased 16% relative to the performance of the control group. This makes it clear that the productivity of foreign owned plants suffered much less than that of locally owned firms in response to the implementation of exchange controls. Thus, the negative shock to financial development seems to have a first-order effect of firm production and productivity. To ensure that these results are not an artifact of the matching procedure I used, I perform various robustness checks on the matching procedure. The procedure is reestimated using different sets of conditioning variables and using different number of matches. In unreported results the estimates are qualitatively similar. The estimates are also robust to the bias-adjustment in Abadie and Imbens (2006). One concern is that the number of treated plants is only 39. The reflects the low level of foreign ownership in the sample. To alleviate this concern I used a less stringent sample selection criteria to maximize the number of plants in the sample i.e. it was no longer required that the firm have three consecutive observations in the event window. This yielded a bigger sample of 85 treated firms and largely similar results.

6. Conclusion

In this study I used an innovative empirical design to gain new insight into the link between financial development and growth. The sudden implementation of exchange controls in Venezuela in 1983 lead to widespread bewilderment and uncertainty in the credit markets leading to increased financial constraints. The subsequent crunch in the capital markets caused even trade credit - a vital component in daily operations for import-dependent Venezuelan firms - to largely disappear. It is therefore hard to argue that this event was not a negative shock to the level of financial development in Venezuela. Foreign subsidiaries should have an easier time accessing internal capital markets of their parents abroad to alleviate this crunch and to continue normal operations. Therefore I argue that foreign owned plants are less likely to be affected by the shock to financial development and can be used as a treatment group. I compare the performance of foreign owned plants to locally owned plants in a window surrounding the event using a difference-in-differences methodology. By directly analyzing the effect an a sharp policy change this quasi-experimental design has several advantages compared to the cross-country panel regressions that are used in the literature to identify the effect of finance on growth (see Goldsmith (1969), Levine (1993), Rajan and Zingales (1998), Beck et.al (2000) and La Porta et.al (1998)). My approach avoids having to find a good proxy for financial development, and most importantly, by focusing on a policy shock, my design comes closer to being a true experimental setting for dealing with the simultaneity of growth and finance.

My findings indicate that financial development affects growth mostly through productivity. I find robust results that the productivity of foreign owned plants declined less over the event window compared to the control sample of locally owned plants. On the other hand, I find little evidence that domestic plants cut capital or factors of production such as labor more than foreign plants. This supports the literature that suggests that the main transmission channel from finance to growth is through productivity.[17] It also is a sign that a more sophisticated financial system facilitates the process of creative destruction by allocating capital to the more productive firms. By using a sharp policy change and rich plant level data from an emerging country I am able to shed new light on the importance of financial development. I hope to use similar identification techniques to further analyze the link between complex concepts such as financial development and firm behavior in future studies.

[17] Jorgensen (1995, 2005) finds that capital accumulation does not have a large effect on long-term growth rates. Similarly Easterly and Levine (2001) conclude that the 'residual' rather than factor accumulation accounts for most of the differences in growth and income between countries. Moreover, Beck, Levine and Loayza (2000) find that financial intermediaies boost growth by boosting productivity. In comparison they find little evidence that financial development increases physical capital accumulation or national saving rates.

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Figure 1

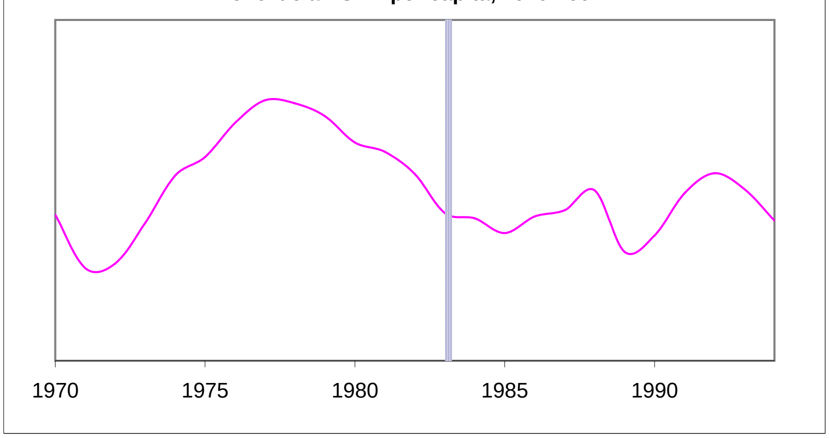


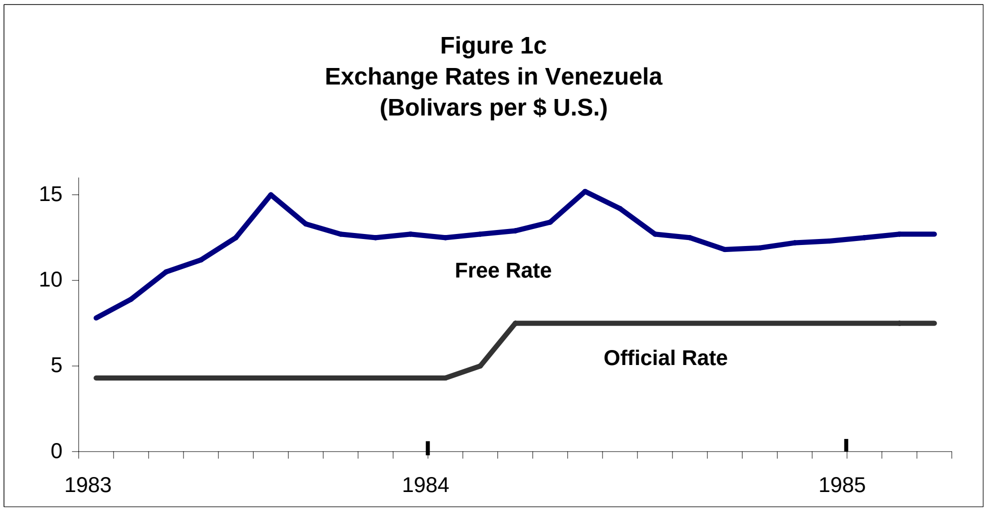
Figure 1a
Venezuelan GDP per capita, 1970-1994

Figure 1a plots Venezuela's log GDP per capita from 1970 to 1994. GDP is measured in US dollars at constant prices. The GDP data series is from the World Penn Tables. The vertical line represents when the exchange controls were introduced.

Figure 1b
Venezuelan Exchange Controls 1983-1985

Date	Bolivars/$	Category
2/1983	4.3	petroleum exports, basic food and debt service
	6.0	most imports
	free market	all other transactions
2/1984	4.3	basic food, debt service
	6.0	petroleum exports
	7.50	services, most imports, interest payments
	free market	non-traditional exports, non-essential imports, capital accounts transactions

Figure 1b illustrates the Venezuelan exchange rate system from 1983 to 1985. The data is adapted from Dornbusch (1986). The first column represents when the exchange rates were reformed; the second column gives the exchange rate; the third column contains the type of transaction that qualified for that exchange rate. Debt service in 1983 was available to both mixed and national companies (less than 50% foreign ownership) for the debt contracted before February 18, 1983 for repayment of principal only. After February 1984 mixed companies were no longer eligible for this program. For national companies the program was extended to also include interest payments.



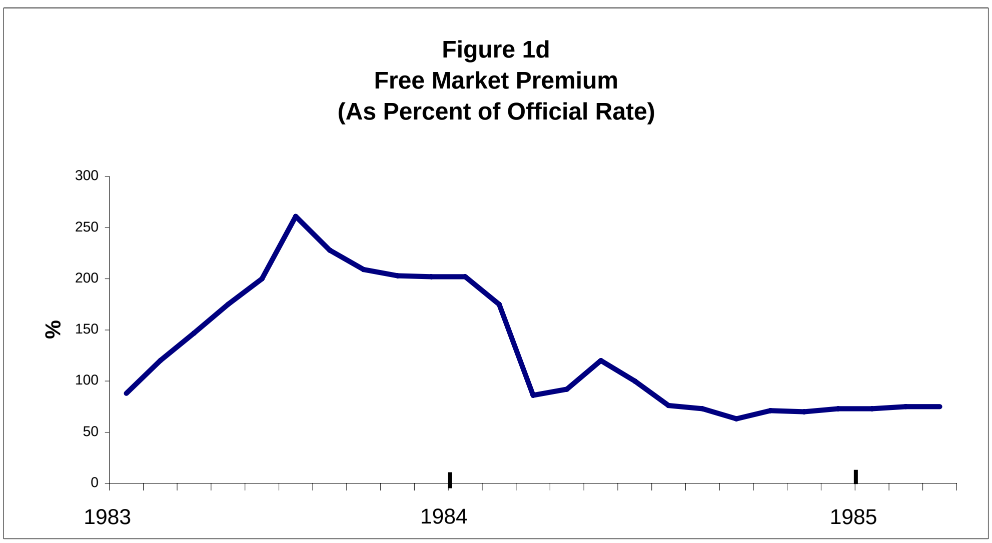
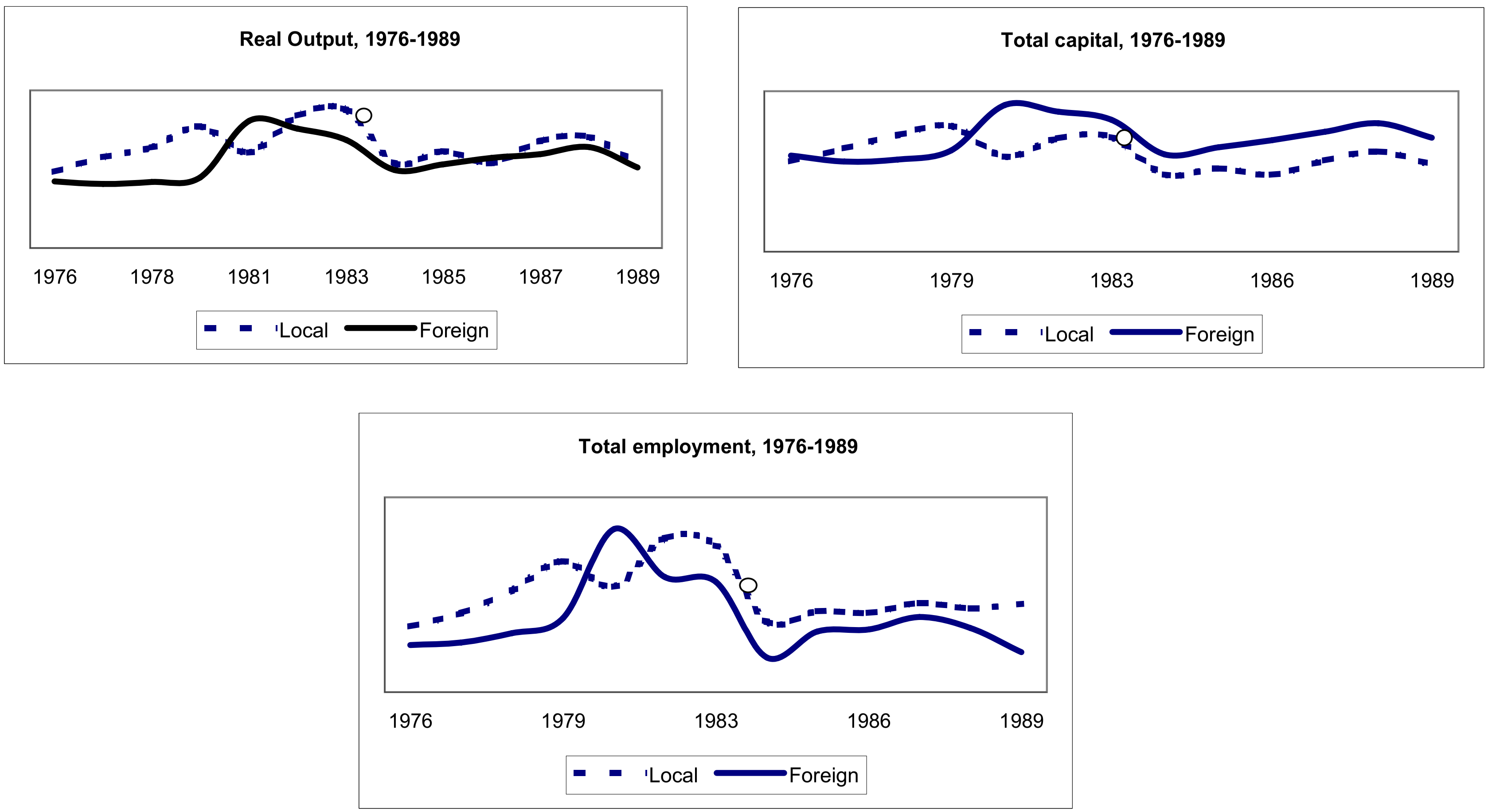


Figure 1c plots the time series of the free market rate and the lowest preferential rate from 1983 to 1985. Figure 1d plots the free market rate as a percentage of the official rate in Figure 1c. The data is from Dornbusch (1986).

Figure 2: Comparing the treatment and control groups



The graphs plot time trends in plant level variables. The sample period is 1976 to 1989; since I do not have data for 1980, 1980 is plotted by interpolation. Foreign are plants that have at least 20% foreign ownership in the sample year; local plants have less than 20% foreign ownership. The imposition of currency control (the event) occured in February 1983 and it is indicated by a circle in the figures.

Table 1

Summary Statistics

	(1) 1976-1989	(2) 1976-1989		(3) 1982-1984	
		Foreign plants	**Local plants**	**Foreign plants**	**Local plants**
Capital stock (log)	7.71	10.60	8.55	10.70	8.57
	(2.01)	(1.51)	(1.96)	(1.35)	(1.93)
Electrictiy Consumption (log)	11.41	13.18	11.40	13.30	11.20
	(1.92)	(1.53)	(1.81)	(1.46)	(1.60)
Output (log)	15.32	18.07	16.21	17.98	16.10
	(1.72)	(1.14)	(1.61)	(1.10)	(1.57)
Materials (log)	14.49	17.07	15.34	16.93	15.13
	(1.84)	(1.33)	(1.74)	(1.20)	(1.73)
Unskilled Employees (log)	1.36	4.06	2.12	4.09	2.00
	(1.53)	(1.07)	(1.57)	(1.01)	(1.57)
Skilled Employees (log)	3.06	4.98	3.74	4.80	3.59
	(1.27)	(1.12)	(1.25)	(1.12)	(1.21)
Total Employees	76.45	359.52	128.58	314.54	107.94
	(289.84)	(401.81)	(241.13)	(331.40)	(195.13)
Real Wages	4.84	5.01	4.88	5.11	4.98
	(0.34)	(0.26)	(0.30)	(0.14)	(0.25)
Foreign ownership	0.03	0.60	0.00	0.60	0.00
	(0.12)	(0.22)	(0.02)	(0.21)	(0.02)
Event	0.51	0.57	0.58		
	(0.50)	(0.50)	(0.49)		
Number of observations	43010	442	7232	117	2124

These are plant level observations from the annual industrial survey (Encuesta Industrial) conducted by the Venezuelan National Statistical Bureau (OCEI). The sample period is 1976-1989; there is no data for 1980 (the survey in not conducted in census year). Dataset (1) contains the whole sample from Aikten and Harrison (1999). The second dataset (2) deletes all plants that (1) switch ownership in the event window or (2) do not have at least three consecutive observations over the event window. The third dataset restricts attention to the event window around the imposition of exchange control on February 1983. The main event window is from 1982 to 1984.

I define the treated group as all plants that have at least 20% foreign ownership. Foreign ownership is the percentage of equity that is owned by foreign investors. Event is an indicator variable that is one if the plant-year is after the imposition of exchange controls. Output is the natural logarithm of the value of sales less the change in inventories, deflated by an annual producer price deflator which varies across four-digit industries (ISIC). Skilled and unskilled labor is the natural logarithm of the number of workers. Capital stock is the natural logarithm of the beginning of year capital stock deflated by the GDP deflator. Material costs is the natural logarithm of matieral costs adjusted for changes in inventories, and then deflated by a production price deflator. Real wages is a regional index of real wages for skilled workers where the plant is located.

Table 2

The response of productivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Difference-in-differences		**0.08**	**0.08**	**0.07**	**0.09**	**0.09**	**0.09**	0.118**	0.03
(Foreign ownership * Event)		(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.06)	(0.04)
Event	-0.02	-0.01	-0.01	**0.02**	-0.028**	0.00	**0.02**	0.00	**0.04**
	(0.02)	-(0.01)	-(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Foreign Ownership	**0.18**	0.02	0.07	0.09	0.10	0.11	0.1334**	0.06	0.10
	(0.07)	(0.08)	(0.08)	(0.07)	(0.08)	(0.07)	(0.07)	(0.15)	(0.09)
Capital Stock	**0.07**	**0.11**	**0.07**	**0.08**	**0.07**	**0.07**	**0.08**	**0.08**	**0.10**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
Unskilled labor	**0.13**	**0.15**	**0.14**	**0.14**	**0.14**	**0.14**	**0.14**	**0.13**	**0.17**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.05)
Skilled Labor	**0.30**	**0.29**	**0.30**	**0.30**	**0.30**	**0.30**	**0.30**	**0.32**	0.172**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.09)
Material costs	**0.55**	**0.51**	**0.54**	**0.53**	**0.55**	**0.54**	**0.53**	**0.53**	**0.54**
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
Number of observations	2241	2241	2241	2598	3500	4430	2598	2014	584
Treated observations	117	117	117	137	189	246	58	41	96
Control observations	2124	2124	2124	2461	3311	4184	2540	1973	488
Adjusted R-squared	0.96	0.95	0.96	0.96	0.96	0.96	0.95	0.92	0.9
Industry dummies	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Years	82-84	82-84	82-84	81-84	82-86	81-87	81-84	81-84	81-84
Sample splits	No	No	No	No	No	No	No	Small	Large
Foreign ownership threshold	0.2	0.2	0.2	0.2	0.2	0.2	0.5	0.2	0.2

This table reports results for the regression in equation (2) in the paper. The base specification regresses log of real output on plant-level inputs, foreign ownership, an event dummy and the interaction of these two variables. Inputs include material costs, unskilled and skilled labor and capital stock; all input variables are in logs and are defined in detail in the caption following Table 1. Event is an indicator that takes on the value one if the plant-level observation is after 1982 and zero otherwise; foreign ownership is an indicator that is one if foreign ownership exceeds 20% of equity; specification (6) uses a threshold of 50% for foreign ownership. Small plants are defined as plants with fewer than 150 total employees (unskilled and skilled); larger plants have more than 150 employees.

The Differences-in-differences (DID) variable capture the differences-in-differences effect. Industry dummies are defined at the four-digit ISIC level. All specification include year dummies. Standard error are in parenthesis under the coefficient estimates; bold estimates are significant at the 5% level. Double asterisk indicates that the estimate is significant at the 10% (but not 5%) level. Standard error are clustered at the firm level.

Table 3

Regional controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Difference-in-differences	**0.08**	**0.08**	**0.07**	**0.08**	**0.07**	**0.12**	0.05	**0.13**
(Foreign ownership * Event)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.06)
Foreign Ownership	0.07	0.07	0.04	0.07	-0.40	-0.06	0.04	0.01
	(0.07)	(0.08)	(0.08)	(0.07)	(0.31)	(0.07)	(0.09)	(0.15)
Real wages			0.103**	**0.16**	0.01	0.00	0.07	**0.18**
			(0.05)	(0.04)	(0.06)	(0.04)	(0.10)	(0.05)
Electrical prices			0.00	0.00	0.00	-0.01	0.04	-0.01
			(0.02)	(0.01)	(0.01)	(0.01)	(0.03)	(0.02)
Foreign ownership in sector		-0.26	-0.15	-0.24	**-0.42**	**-0.47**	-0.46	-0.25
		(0.17)	(0.12)	(0.17)	(0.15)	(0.12)	(0.40)	(0.19)
Electricity Consumption	**0.07**		**0.08**	**0.07**	**0.04**	**0.06**	-0.01	**0.09**
	(0.01)		(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Number of observations	2227	2241	2241	2241	2241	4048	485	1737
Treated observations	117	117	117	117	117	246	96	42
Control observations	2110	2124	2124	2124	2124	3802	389	1695
Adjusted R-squared	0.96	0.95	0.96	0.97	0.99	0.99	0.92	0.95
Industry dummies	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Years	82-84	82-84	82-84	82-84	82-84	81-87	81-84	81-84
Sample splits	No	No	No	No	No	No	Large	Small
Regional fixed effects	No	No	Yes	No	No	No	No	No
Plant level fixed effects	No	No	No	No	Yes	Yes	No	No

This table reports results for the regression in equation (2) in the paper. The base specification regresses log of real output on plant-level inputs, foreign ownership, an event dummy and the interaction of these two variables. Inputs include material costs, unskilled and skilled labor and capital stock; all input variables are in logs and are defined in detail in the caption following Table 1. Event is an indicator that takes on the value one if the plant-level observation is after 1982 and zero otherwise; foreign ownership is an indicator that is one if foreign ownership exceeds 20% of equity. The Differences-in-differences (DID) variable capture the differences-in-differences effect. Industry dummies are defined at the four-digit ISIC level. All specification include year dummies. Standard error are in parenthesis under the coefficient estimates; bold estimates are significant at the 5% level. Double asterisk indicates that the estimate is significant at the 10% (but not 5%) level. Standard error are clustered at the firm level.

Electricity consumption is the natural log of the amount of electricity consumed at the plant level. Real wages is an index of regional wage levels for skilled workers. Electrical prices are regional energy prices. Foreign ownership in sector is the labor weighted share of foreign ownership at the industry level. The coefficient estimates on the other controls variables are suppressed in the interests of brevity.

Table 4

Pairwise Comparisons

Variable	Pre-Match Treated	Pre-Match Control	Pre-Match P-value	Post Match Treated	Post Match Control	Post Match P-value
Capital Stock	10.70	8.56	(0.00)	10.70	10.28	(0.04)
	(1.39)	(1.90)		(1.39)	(1.63)	
Unskilled Employees	4.09	2.02	(0.00)	4.09	3.85	(0.11)
	(0.97)	(1.57)		(0.97)	(1.16)	
Skilled Employees	4.87	3.61	(0.00)	4.87	4.65	(0.15)
	(1.10)	(1.23)		(1.10)	(1.18)	
Output	18.04	15.62	(0.00)	18.04	17.75	(0.08)
	(1.39)	(1.62)		(1.39)	(1.27)	
Materials	16.91	14.75	(0.00)	16.91	16.77	(0.15)
	(1.44)	(1.72)		(1.44)	(1.42)	
Electricity consumption	13.36	10.69	(0.00)	13.36	12.81	(0.02)
	(1.50)	(1.71)		(1.50)	(1.56)	
Real Wages	5.09	5.00	(0.00)	5.09	5.07	(0.18)
	(0.15)	(0.26)		(0.15)	(0.15)	
Electricity Price	-0.98	-1.14	(0.02)	-0.98	-1.11	(0.08)
	(0.56)	(0.63)		(0.56)	(0.54)	
Foreign ownership in sector	0.12	0.05	(0.00)	0.12	0.10	(0.21)
	(0.07)	(0.06)		(0.07)	(0.07)	
Observations	211	3398		211	522	

This table presents summary statistics for the pre-match and post-match samples. Treatment is determined by foreign ownership. The treated group contain plants that have 20% or more foreign ownership. All other plants are part of the control group. Plants are deleted from the sample if they (1) switch from treated to control or vice versa during the event window or (2) do not have consecutive observations from 1982 to 1984. Both samples are from the main the pre-event sample that runs from 1976-1982. The variables are defined in Table 1 and 3. The matching procedure is a nearest neighbor match of treatment and controls by estimated propensity scores. P-values are from a test of difference in means between treatment and control groups. Standard errors are in parenthesis under the variable means.

Table 5

Probit Regression Results

Variable	Pre-Match		Post Match	
Intercept	-14.30	(0.00)	-7.16	(0.06)
Capital Stock	-0.03	(0.84)	0.15	(0.10)
Unskilled Employees	**0.46**	**(0.01)**	0.14	(0.43)
Skilled Employees	**-0.50**	**(0.03)**	**-0.32**	**(0.04)**
Output	**0.88**	**(0.01)**	0.26	(0.39)
Materials	**-0.42**	**(0.04)**	-0.11	(0.58)
Real Wages	1.17	(0.19)	0.68	(0.26)
Foreign ownership in sector	**3.36**	**(0.02)**	2.49	(0.07)
Industry dummies	Yes		Yes	
Observations	743		122	
Treated	39		39	
Control	704		83	
Pseudo R-squared	0.36		0.05	
Chi-Square P-value	0.00		0.06	

This table presents parameter estimates from the probit model that was used to estimate propensity scores for the treatment and control groups. The dependent variables in an indicator that is one if the observation in part of the treated group. Treatment is defined as the plants that have foreign ownership higher than 20%. The probit is run at the plant level and all the covariates are averages over the pre-event period (1976-1982). Covariates are defined as in Table 1. The pre-match results are parameter estimates of the probit model estimated on the entire sample prior to matching; the post match results are parameter estimates of the probit model estimated on the treated and control groups after

Table 6

Response of capital, labor and output

Variable	Capital	Labor	Output
Average difference in treated	-0.20	-0.06	0.08
S.E.	(0.06)	(0.03)	(0.06)
Average difference in control	-0.17	-0.09	-0.08
S.E.	(0.04)	(0.03)	(0.04)
Diff-in-diff	-0.03	0.02	**0.16**
T-stat	-0.47	0.62	2.78

Panel A presents the following summary statistics for the plants: Average difference is the average difference between the plants after the event. S.E. is the standard error of the average. Diff-in-diff is the difference between the two average. The sample in this panel is from 1982-1984. Matching is based on the propensity score matched sample from Table 4 and 5.