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Using Administrative Data to Evaluate Municipal Reforms:

An Evaluation of the Impact of Minas Fácil Expresso *

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Abstract

This study uses administrative data to evaluate the impact of Minas Fácil Expresso, a program in the state of Minas Gerais, Brazil, which expanded a business start-up simplification program to more remote municipalities. Using difference-in-differences with 56 months of registration data for 822 municipalities, the analysis finds introducing the program actually led to a reduction in registration rates, and no change in tax revenues. The paper uses this evaluation to illustrate the design choices and issues involved in using administrative data to evaluate reforms, also providing a template that can be used for evaluating similar reforms elsewhere.

Keywords: Entry reform; Difference-in-Differences; Informality.

JEL codes: O17; O12; D22 ; L26.

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1. Introduction

Influenced by the work of Hernando de Soto (1989), and the World Bank/IFC's *Doing Business* project, efforts to make it easier for individuals to register a business have been the most common type of regulatory reform over the past decade. Since 2004, 75 percent of the countries included in the *Doing Business* survey have adopted at least one reform making it easier to register a business (IFC, 2009). There is some evidence that, starting from a point of burdensome entry, big reductions in barriers to entry are associated with increases in firm registrations. Djankov et al. (2002) show that countries with more burdensome entry regulations have larger informal sectors, while Klapper et al. (2006) find that costly entry regulations are associated with the creation of fewer limited liability companies. Reforms which dramatically reduced the time taken to start a business led to increases in formalization in Mexico (Bruhn, 2011; Bruhn 2012; Kaplan et al, 2011), Peru (Mullainathan and Schnabl, 2010), Colombia (Cárdenas and Rozo, 2009) and Portugal (Branstetter et al, 2010).

However, recent research has suggested there is little benefit to reform if the reduction in the barriers to entry are not large (Klapper and Love, 2011), and despite these reforms, the majority of firms in many developing countries remain informal. That is, although some informal firms register when a reform makes it easier to do so, this number is small compared to the overall stock of informal firms. While there is evidence that distance and information serve as barriers to some firms registering who would benefit from doing so (McKenzie and Sakho, 2010), several recent experiments which have provided firms with information on how to register, and which have lowered the cost of registering, have found very few informal firms formalize as a result (de Mel et al, 2013 in Sri Lanka; Jaramillo, 2009 in Peru; Andrade et al, 2013 in Brazil, de Giorgi and Rahman 2013 in Bangladesh). One explanation appears to be that very few informal firms seem to benefit from formalizing. This raises the question of whether government efforts to extend simplification efforts to firms outside the main cities will lead to increased formalization rates, given that such firms may be even less likely than firms in large cities to benefit from many of the supposed benefits of formalizing like increased customer bases, access to government programs and contracts, and availability of lower cost financing sources. Furthermore, we know of no evidence to show whether governments gain more from such a process in taxes than they spend implementing these reforms.

This paper helps answer these questions through an evaluation of the impact of the Minas Fácil Expresso program in Minas Gerais, Brazil. This was a program designed to extend a business simplification program already present in the most populous municipalities in the state to its remaining

municipalities. It was implemented first in 55 of the 822 unserved municipalities. We have 56 to 58 months of administrative data on firm registration rates and business tax receipts for each municipality, and show how difference-in-differences estimation can be used with such data to evaluate the impact of municipal reforms. Many of the non-treated municipalities are significantly smaller in population and had far fewer registrations pre-reform than the treated municipalities, which may make the difference-in-differences assumption of common underlying trends less believable. We therefore use propensity-score matching and nearest neighbor matching to obtain a control group of more comparable enterprises, as well as using a group of municipalities in which the units were planned to be implemented but were not yet implemented as an alternative control group.

Our preferred estimates show that municipalities opening a Minas Fácil Expresso office had a reduction of about 1.3 firms per month registering as a result of the reform, approximately a 10 percent reduction. This impact appears to be concentrated in the month of opening and month immediately after, with registration levels suffering less of a reduction in subsequent months. This result is robust to multiple different choices of a control group, to omitting one month with extraordinary registration patterns, and to the use of either difference-in-differences or fixed effects specifications. Consistent with no big changes in new registrations, we find municipalities do not receive any increase in businesses taxes as a result of the reform.

We suggest three reasons why these impacts contrast with the positive impacts found after reforms in Peru, Colombia, and Mexico. First, the size of the reform in our case is not quite as dramatic as some of those studied: the time taken to register dropped from an existing time of somewhere between 15 days and several months to 9 days, whereas the Mexican reform dropped the number of days taken to start a business from 30 to 1.4. Second, the benefits of being registered are likely to be lower in less populous areas than they are in the main cities that have been subject to previous evaluations. Third, as in these other cases, introducing a one-stop shop approach to registration removes the flexibility firms have to become partially-formal by registering with one government agency and not another. In other cases this might be outweighed by the benefits of full formalization, but in the case of Brazil, which has more onerous taxes and requirements for being fully formal, this may have lead to firms which would have partially registered opting not to register at all.

In addition to documenting the impact of this particular reform, we hope this paper can help serve two further related purposes. First, the administrative data used in the paper should be available for many other developing countries and the methods used here could thus be readily mimicked to evaluate other

such municipal reforms. Such analysis is very cheap, and so we hope this paper might motivate other governments to carry out similar evaluations. Second, there is often a concern of impact evaluation publication bias, whereby results which show little impact or impacts that are counter to the hopes of the institution being evaluated are often not published. We hope that sharing the results of this study will be a contrast to this pattern, and be consistent with increasing interest in “learning from failure”.

The remainder of the paper is structured as follows: Section 2 explains the intervention which took place, Section 3 summarizes our main data sources, and Section 4 our estimation methods. The results are found in Section 5, while Section 6 concludes.

2. The Intervention

The State of Minas Gerais is the second most populous in Brazil, with a population of approximately 20.5 million, and a land area slightly larger than that of France. Minas Fácil (Minas Easy) is a program developed by the State Government of Minas Gerais, with the aim of simplifying and streamlining the process of opening a business in the state. The program set up a “one-stop shop” for registrations by integrating together the different municipal, state, and federal entities involved in registering a firm. Physical offices were set up in 31 of the state’s 853 municipalities, covering the more populous municipalities with highest levels of firm registrations. The simplifications resulted in a reduction from 8 to 4 steps in the number of procedures required to register, and a reduction in the time taken to register from 28 to 9 days (Barbosa et al, 2011). This applied to firms registering companies, including under the Brazilian SIMPLES program, but not for sole proprietors registering under an individual entrepreneur (MEI) category designed for individually-owned businesses with low incomes.

In order to extend the benefits of this program to firms in the other 822 municipalities in the state (which collectively still have a population of approximately 12 million), the government began a program initially called *Minas Fácil Virtual* (Virtual Minas Easy), which was later renamed as *Minas Fácil Expresso* (Express Minas Easy). This was intended as a quick and low-cost way to integrate the remaining municipalities, with the goals of facilitating the opening of businesses in these municipalities, reducing informality, and increasing state and municipal tax collection (Barbosa et al, 2011). The units of Minas Fácil Expresso provide similar services to the traditional Minas Fácil units, but scan and email documents received from entrepreneurs to the central state offices (Descomplicar, 2012).

The Minas Fácil Expresso reform aimed to both simplify the time taken to open a business and the steps required. Communication that previously had to happen in person could now take place electronically.

Before the opening of Minas Fácil Expresso, entrepreneurs had to travel to a city with a Minas Fácil office to register their business with federal and state level authorities, in addition to registering with the local municipality.¹ With the electronic system, entrepreneurs only have to visit their municipal registration office since it can process federal and state level licenses. This change lowered the time taken to open a business and also saved travel costs. Another feature of Minas Fácil Expresso was the elimination of municipal inspections prior to opening a business, instead issuing a provisional operating license (provisional *ALF*) for activities of low risk, allowing them to start operations without the need for pre-registration surveys and inspections by authorities. Overall, the time taken to start a business would fall to around 9 days, compared to estimates of “15 days” (Jornal S’Passo, 2011) to “months” (Gxp, 2011) in some municipalities previously.

In some municipalities, there was also a reduction in the fees required for registering. The licensing process requires a feasibility consultation (*Consulta de Viabilidade*) to determine whether the firm can operate under the desired name at the desired location. In the absence of Minas Fácil Expresso, this consultation was conducted by the local authorities for a fee that varied between 15 and 100 Reais (US\$7 and 48), depending on the municipality. With Minas Fácil Expresso, the consultation is done online for free. In addition, many municipalities had previously required that owners pay back any debt relating to unpaid property taxes before registering, and this was now no longer required.

The State Government set a target of opening 50 Minas Fácil Expresso offices by the end of 2011, and then expanding further to the rest of the state. An initial list of 50 municipalities were selected based on the volume of registrations occurring in 2010, as well as whether the municipalities had expressed interest in having the program (through the Mayor, Professional Associations, Legislative Hall, or State Legislature). In practice actual implementation occurred for 32 of these 50 municipalities, beginning in September 2011, along with a further 23 municipalities for a total of 55 municipalities by the end of August 2012. Figure 1 shows the number of municipalities receiving a Minas Fácil Expresso unit each month.

Municipalities wishing to implement the Minas Fácil Expresso unit had to implement a General Municipal Law (Complementary Law 123) which relates to starting a business, as well as support the implementation of the project. Deploying a unit costs approximately 40,000 Reais (US\$19,000) for the first year, which includes the cost of training, computer equipment, furniture, and running costs.

The opening of the Minas Fácil Expresso offices was covered by local newspapers in at least some of the municipalities (Jornal S'Passo, 2011; Gxp, 2011), although it is unclear how widespread advertisement of the new system to firms was.

3. Administrative Data

We use three forms of administrative data, all provided by the *Junta Comercial de Minas Gerais (JUCEMG)*, the Minas Gerais Chamber of Commerce. The first is a list of the municipalities in which the express office was planned to be implemented, and of the municipalities in which it was actually implemented along with the implementation dates. The second is monthly data from January 2008 until August 2012 on the number of firms registering as formal entities in each municipality. This includes registration of firms under all registration categories except the low income self-employed registering as individual microentrepreneurs (MEIs). In particular, it includes firms who register for SIMPLES, the type of registration which is the focus of the one-stop shop. The third type of administrative data is the monthly total municipal tax payments received in each municipality from January 2008 to October 2012. The tax paid is the *Imposto Sobre Serviços de Qualquer Natureza (ISS)* – a tax on all firms in the service sector, which is paid as part of the SIMPLES payment whereby a firm makes a single payment that covers a range of different government taxes.

There are 853 municipalities in Minas Gerais. Thirty-one of these, including Belo Horizonte, the state capital, have a physical Minas Fácil office and are generally much larger in size. Our dataset consists of the monthly formalization and tax data by municipality for the remaining 822 municipalities, along with the details of which of these received a Minas Fácil Expresso office.

In order to provide a template that will make it easier for other researchers to replicate this work with other types of administrative data, we have provided the data and a replication Stata do file in the World Bank's microdata library (<http://microdata.worldbank.org/index.php/catalog/1534>).

4. Estimation Methodology

Figure 2 plots the mean monthly number of registrations for the 55 municipalities that had a Minas Fácil Expresso office implemented at some point during our study period (treated municipalities), as well as the monthly mean number of registrations for the 767 municipalities in which it was not introduced. Vertical lines show the period during which the Minas Fácil Expresso offices were introduced into the treated municipalities in a staggered fashion, as shown in Figure 1.

Measuring the impact of this reform requires coming up with a counterfactual of what registration rates would have been in treated municipalities had this reform not occurred. A simple before-after comparison would use the registration rates in the period before the reform as the counterfactual for what rates would have been in the post-reform period had this reform not occurred. There are two reasons to not believe this. First, Figure 2 shows considerable month-to-month variation in the number of firms registering, suggesting seasonal factors and other changes in the economy are likely to influence the registration rate in any given month. Second, even if registration rates were completely constant over the 44 months of data pre-intervention, there is still the concern that some other change in government policy or in the economy may induce a change in the registration rate at the same time as our intervention is occurring. Figure 2 suggests this may have occurred in September 2011, where there is a large reduction in the number of firms registering. During this month the Federal Government passed a law relaxing the eligibility criteria for individuals to register under the MEI status, and so some individuals who might have previously registered under SIMPLES might have instead decided not to register during this month until they observed what the changes in law would be. Alternatively, this could just be a reduction due to other events occurring in the economy.

4.1 Difference-in-differences and Fixed Effects

Difference-in-differences estimation provides a way to subtract out these other events occurring in the economy by comparing the before-after changes in the treatment group to those in a control group. This method has a long history of use in economics (e.g. Ashenfelter and Card, 1985; a textbook exposition is in Angrist and Pischke, 2009), and in our context requires estimating the following equation for municipality i at time t :

$$Registrations_{i,t} = \alpha + \beta Treat_i + \sum_{t=2}^{56} \gamma_t \delta_t + \theta PostTreat_{i,t} + \pi' X_i + \varepsilon_{i,t} \quad (1)$$

Where $Treat_i$ is a dummy variable for whether or not i is one of the 55 treated municipalities, δ_t are month-year dummy variables covering each month from February 2008 until August 2012, and $PostTreat_{i,t}$ is a dummy variable which takes value one if treated municipality i has actually received its Minás Facil Expresso office by time period t , and is zero otherwise. X_i are additional controls – we control for the average number of registrations in the municipality in each of 2008, 2009, and 2010, and for the 2010 municipal population. We choose these controls because they control for the existing size of the municipality and any existing trend in registration patterns. If one had data on the number of informal firms in each municipality pre-reform, or on time-varying municipal level economic outcomes

that were unaffected by the reform, these would also be useful to control for, but such variables are unlikely to be available in most applications.

Then θ is our coefficient of interest, and measures the difference in monthly registrations between the treated and control municipalities before the reform compared to after the reform. Following McKenzie (2012) we estimate this as a common effect, averaging over multiple months of data to increase statistical power. The standard errors are clustered at the municipality level, following the recommendations of Bertrand et al. (2004).

Alternatively, we can estimate a fixed effects specification, which controls for any time-invariant characteristics of the municipality through a fixed effect α_i :

$$Registrations_{i,t} = \alpha_i + \sum_{t=2}^{56} \gamma_t \delta_t + \theta PostTreat_{i,t} + \varepsilon_{i,t} \quad (2)$$

where again we cluster the standard errors at the municipality level. This controls for unobserved characteristics of the municipality, such as the level of existing informality among its firms, which might be expected to influence registration rates.

In addition to these basic specifications, we also examine whether the impact of the Minas Fácil Expresso office introduction differs over time by estimating:

$$Registrations_{i,t} = \alpha + \beta Treat_i + \sum_{t=2}^{56} \gamma_t \delta_t + \theta_1 Six\ months\ pre - treat_{i,t} + \theta_2 ImmediateTreat_{i,t} + \theta_3 LongTreat_{i,t} + \pi' X_i + \varepsilon_{i,t} \quad (3)$$

and the fixed effects analog. Here *Six months pre – treat* is a dummy variable which takes value one in the six months prior to the opening of the Minas Fácil Expresso office in municipality i , *ImmediateTreat* takes value one in the month of opening and month immediately after opening, and *LongTreat* takes value one in months two or more after the opening. The inclusion of the pre-treatment dummy allows us to test whether there is a trend in registrations in the run-up to opening, while the latter two variables separate out the immediate and slightly longer-term impacts of treatment.

The key underlying assumption in both the difference-in-differences and the fixed effects specifications is that after controlling for differences in levels among municipalities, these municipalities would have exhibited the same time trend in registrations in the absence of the introduction of Minas Fácil Expresso. This assumption is more credible when the treated and control municipalities exhibit the same

time trends prior to the reform, and the more similar are the treated and control municipalities. It would, however, be violated if officials choose municipalities with better growth prospects as places in which to target the introduction. It does not appear that officials had any more administrative data than us in this regardⁱⁱ and, as described in Section 2, they stated that past registration rates were the basis for selecting municipalities.

The first two columns of Table 1 compare the characteristics of the treated municipalities with those of the full set of non-treated municipalities, while Figure 2 plots the time paths of registration rates for the two series. Consistent with the reform program targeting municipalities with higher existing registration rates we see a large and significant difference in levels of registration between the two groups: the mean number of registrations per month in a treated municipality was approximately 12-13 in each of the three years prior to the reform, whereas it was only 1-2 for the average control municipality. Moreover, not only are the levels different, but so is the variability around these levels, with the standard deviation across months of the number of registrations almost three times as large in the treated municipalities as the full set of control municipalities. The treated municipalities have larger populations (mean of 56,000 versus mean of 12,000 for the control municipalities), and a higher share of industry in municipal GDP (26% versus 15%).

These large differences mean that month-to-month changes over time in the number of registrations in the full group of non-treated municipalities are unlikely to be an accurate counterfactual of what the change would be in the treated municipalities in the absence of the introduction of Minas Fácil Expresso. However, because there is no noticeable trend in registrations, difference-in-differences may still give a reasonable picture of what the average level of registrations over multiple months would have been. We can formally test whether the pre-reform time trend is the same in treated and non-treated municipalities by estimating first a test of whether they have a common linear trend over the period January 2008 to August 2011:

$$Registrations_{i,t} = \alpha + \beta Treat_i + \gamma Time_t + \rho Treat_i * Time_t + \omega_{i,t} \quad (4)$$

Where $Time_t$ is a linear time trend, and we test whether $\rho = 0$. We can also test whether the treated and non-treated municipalities have the same non-parametric time trend by estimating over the same period:

$$Registrations_{i,t} = \alpha + \beta Treat_i + \sum_{t=2}^{44} \gamma_t \delta_t + \sum_{t=2}^{44} \gamma_t \rho_t Treat_i + u_{i,t} \quad (5)$$

The last few rows of Table 1 show that we can reject that the treated and non-treated municipalities follow a common linear trend or a common non-parametric trend over the 44 months of pre-treatment data. This therefore casts doubt on the use of difference-in-differences or fixed effects using the full sample of non-treated municipality as a control group. However, it should be noted that although there is a differential linear trend in the non-treated municipalities, it is very small in magnitude, averaging 0.02 firms per month, so may not have much practical impact on the difference-in-differences estimate.

4.2 Obtaining a Better Control Group

One approach that is sometimes used to deal with differences in scales is to take logs. This then requires that municipalities exhibit the same trends in percentage changes in registrations, rather than in levels of registrations. This is appealing in that one might expect it to be more likely that an aggregate economic shock would lead to a constant percentage change in registrations, rather than a constant level increase in registrations across municipalities. However, the problem we would face in doing this is that 36 percent of the municipality-time observations are zeroes – many of the smaller municipalities have no firms registering in them in some months.

We therefore instead restrict the set of municipalities that we are comparing to a subset of the full sample which are more comparable to one another. To do this, we employ the method of propensity-score matching (Dehejia and Wahba, 2002). Our base specification, which we specified at a World Bank evaluation workshop in June 2011 (before any of the Minas Fácil Expresso offices were put in place) matches on the number of registrations in a municipality each month from January 2008 through to December 2010, along with 2010 municipal populationⁱⁱⁱ. This proceeds by estimating a probit regression of the likelihood of being a treated municipality as a function of these characteristics, and taking the predicted probabilities from this estimation which form the propensity score. Appendix Table 1 provides this selection equation, and shows (as in Table 1) that larger municipalities, i.e. the ones that have a higher population and thus a higher level of registrations, were more likely to get the program. It also shows that the past number of registrations in specific months does help predict program use, but that this comes through a mixture of positive and negative coefficients. There thus doesn't appear to be any pattern whereby municipalities with growing number of registrations are the ones most likely to be selected.

The common support is the range of overlap of the distributions of the propensity score for the treatment and the control groups. In our case this ranges from 0.008 to 1. Restricting the untreated

municipalities to this range of common supports eliminates 410 municipalities that are not very similar to the treated municipalities. Column 3 of Table 1, and the other plotted line in Figure 2 are then for the sample of 357 municipalities within this common support. We see this group has more registrations on average and a larger population size than the full set of untreated municipalities, but is still significantly different in size and in pre-reform trend.

We therefore further restrict the sample by using nearest neighbor matching. This selects (with replacement) the control municipality that is the closest match to each treated municipality in terms of the propensity score. This results in the use of only 29 of the 767 non-treated municipalities. The fourth column of Table 1 shows that this control group is much more similar to the treatment group. In particular, there is no significant difference in the average number of registrations or their variability, population size averages 50,000 compared to 56,800 for the treated municipalities, and there is only a marginally significant difference in the share of industry in municipal GDP. Moreover, although we only matched on pre-2011 data, the level and variability of registration rates are not significantly different over the eight pre-reform months of 2011, which provides a nice falsification test. Figure 3 plots the registration data for this nearest neighbor control group. We see it is much closer in level to the treatment municipalities, and displays similar trends. We cannot reject a common linear time trend, although do reject a common non-parametric time trend over the 44 pre-intervention months. This appears to be driven by a difference in January 2011, and so the last row of Table 1 shows that we cannot reject common time trends for all other pre-intervention months.

Finally, we also consider an alternative control group consisting of the 18 municipalities which were initially selected for the Minas Fácil Expresso program, but where it was not yet implemented by the end of our study period. The last column of Table 1 shows the characteristics of these municipalities, while Figure 3 plots their time path of registrations. We see these municipalities are slightly larger in population and had slightly more registrations per month than the treated municipalities, but not significantly so. Their time path of registrations appears to visually track that of the treated municipalities, but because the variability is slightly larger, we reject equal non-parametric trends pre-treatment.

Five of these 18 planned placement municipalities overlap with the 29 nearest neighbor municipalities, whilst the remainders are different. Appendix 1 provides a map showing where the municipalities are located. We see both treatment and control municipalities are dispersed throughout the state, and it is

not the case that we are comparing all treated municipalities from one region of the state to controls in another.

This then gives us a range of plausible counterfactuals, and so we can see how sensitive our results are to the choice of comparison group. Implicitly, in all our specifications, we are also relying on the staggered introduction of the treatment to use municipalities which were treated later as controls for those treated earlier in our regressions.

5. Results

We first estimate the impacts on the number of registrations per month, and then the impact on taxes received in the municipality.

5.1. Average Impact on Firm Registrations

Table 2 reports the estimated impact of opening a Minas Fácil Expresso office on registrations in a municipality under different specifications. Panel A provides the difference-in-difference results, while Panel B shows the results under a fixed effects specification – we see the point estimates of similar magnitudes and statistical significance levels regardless of which of these two specifications are used. Regardless of the control group we use, we estimate that the opening of the office resulted in an average reduction in registrations of about one firm per month in the municipalities opening these offices. This result is statistically significant at the 5 percent level using all four possible control groups. The similarity of results across treatment groups reflects the fact that registration rates, while varying a lot from month to month, exhibited no strong secular trend.

On average the treated municipalities were registering 12.55 firms per month in the year prior to the reform, so the reduction of 1.3 firms registering estimated by our preferred 36-month nearest neighbor estimator represents a 10.4 percent fall in registrations.

Figures 2 and 3 show that there was a large drop in registrations in September 2011, which happened to coincide with the month the first three treated municipalities introduced the Minas Fácil Expresso office. In order to ensure that the drop in registrations in that month is not driving our results, Panels C and D conduct two robustness exercises. The first, reported in Panel C, is to simply drop all data for the month of September 2011, while the second, reported in Panel D, instead drops the three municipalities which opened their Minas Fácil Expresso offices in September 2011. We see that both robustness checks result

in only minor changes in the estimated treatment effects, and still lead us to estimate a 1.1 to 1.2 firm per month reduction in registrations in the treated municipalities.

5.2. Time Path of Impacts on Firm Registrations

Table 3 reports the difference-in-differences and fixed effects estimates from estimating equation (3). We see that the negative impact of the reform is concentrated in the month of the introduction of the Minas Fácil Expresso office and the month immediately after opening. Our preferred estimator shows a 1.7 to 1.8 firm per month per municipality reduction in newly registered firms during this period. Then in months two or more after the introduction, the negative impact falls to only a 0.6 to 0.7 reduction, which is statistically significant with our larger samples, but not using our preferred estimator. In contrast, there is no evidence of a declining trend in registrations in the six months preceding implementation – we find no significant change in registrations compared to the preceding three years when we use the full sample of untreated firms, the sample in the common support, or our 36 month nearest neighbor control sample, and a marginally significant positive pre-implementation impact using the planned implementation control group.

The 55 municipalities that received Minas Fácil Expresso offices collectively registered a total of 8,283 firms over the 12 months prior to the first offices being opened. Our point estimates suggest that opening these offices resulted in 187 fewer firms registering in these municipalities during the month of opening and month immediately afterwards, and perhaps another 154 fewer firms registering during the next four months post-implementation. We cannot extrapolate the results much further than this given the time elapsed since implementation, but it appears to date that the reforms have resulted in a relatively small reduction in registrations.

One reason why registrations might fall during the period of opening is that delays in registration may occur during an adjustment period until the new system is running smoothly. However, this should have led to increases in completed firm registrations in later months, which we do not see in the data. It is possible that delays accumulate in the first couple of months, while the resulting backlog is cleared more slowly, so that the subsequent increase is not as pronounced and detectable. An additional reason why we find no positive effect of Minas Fácil Expresso may be that some firms do not want to register with all three levels of government. Before the new system unified municipal, state, and federal procedures, firms had the option of registering with only one or two of these authorities, thereby evading fees and taxes associated with the others. Taking away this flexibility may make some firms less likely to register.

This same logic applies to one-stop shop reforms in other countries as well, but given that the full costs of being formal are rather high for small firms in Brazil (Andrade et al, 2013), this logic may apply more in this case.

5.3 Impact on Municipal Tax Collections from Firms

One motivation for governments to attempt to bring firms into the formal sector is to broaden its tax base. We are unaware of any study which has looked empirically at this issue, and the administrative data available in Brazil offers the opportunity. Given that the flow of newly registered firms is likely to result in only a small change in the total stock of registered firms in a municipality, we should not expect to see large changes in tax revenues unless a reform leads to a dramatic change in the number of firms registering, which the above analysis shows was not the case. Nevertheless, estimating the treatment impact offers a check on this, as well as illustrating how the choice of a control group matters a lot more when there are pre-treatment trends.

Figure 4 plots the time path of tax revenues received over time. Unlike registrations which show no visible trend, we see business tax revenues have been trending upwards since January 2010 in the municipalities which later received the Minas Fácil Expresso offices. In contrast, average tax revenues in the full sample of untreated municipalities have not been growing as fast in level terms. We again use propensity score matching to obtain a more similar control group, this time matching on the time path of pre-reform business tax receipts instead of registration rates. Figure 4 also plots the time path of the nearest neighbor-matched municipalities, which are lower in levels, but exhibit a similar time path to the treated municipalities.

Table 4 reports our treatment estimates for the impact of introducing the Minas Fácil Expresso offices on municipal tax revenues. The first two columns, which use the full set of municipalities or those in the common support, would suggest that treatment led to a large increase in municipal business tax revenues. However, this reflects the large gap which opened up in revenues between treatment and control municipalities in 2010, after revenues had been similar in the two groups in 2008 and 2009 – that is, it reflects a pre-existing trend. Columns 3 and 4 of Table 4, which use more similar control groups, find instead no significant impact on tax revenues, which is consistent with what we would expect given the relatively small change in registration rates.

6. Conclusions

One-stop shops and making it easy for individuals to formally register firms have been mainstays of the policy recommendations pushed by Hernando de Soto and the Doing Business reports, and have been widely implemented. However, recent evidence has called into question the view that most informal firms would like to formalize, noting that there are often few benefits to them of doing so. This is particularly likely to be the case once one goes outside the main centers to more isolated municipalities.

We use government administrative data to evaluate the impact of a reform – the opening of Minas Fácil Expresso offices to ease registration in more remote municipalities in the state of Minas Gerais in Brazil. We find that opening these offices actually led to a small reduction in the number of firms registering in the month of implementation and following month, and zero increase or perhaps small decreases in registration rates over subsequent months. There was also no significant change in tax revenue. This reform therefore does not appear to have succeeded in its goal of increasing formality in these municipalities, suggesting the need for policymakers to consider further what the other constraints to firms formalizing are – and in particular, whether it is worth it on a cost-benefit basis to try and draw more of them into the formal sector.

More generally, this paper illustrates how the rich administrative data collected by a state in Brazil allow for rigorous evaluation of municipal level business reforms. Typically when reforms are not randomly introduced, one is concerned about the plausibility of non-experimental approaches to evaluate these reforms. However, in cases like this one, where a large time series of data are available pre-reform, one can test and have more confidence in the assumptions required for difference-in-differences to hold than is the case in evaluations with only one or two periods of pre-treatment data. Given that data of this sort should be collected as part of normal government functioning in most countries, the evaluation was very cheap (requiring only researcher and government time costs), and so this sort of analysis of many other government municipal-level programs should be possible.

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Table 1: Municipality Characteristics for Treated and Control Groups

		Control Municipalities			
		All	36-month Common Support	Nearest Neighbors	Planned Implementation Candidates
	Treated	Non-treated			
<i>Variables used for level matching</i>					
Average monthly registrations in 2008	11.9	2.1***	3.3***	10.4	14.7
Average monthly registrations in 2009	12.8	2.3***	3.7***	11.2	15.3
Average monthly registrations in 2010	12.4	2.2***	3.4***	11.0	15.3
Standard deviation of monthly registrations 2008 to 2010	4.1	1.4***	1.8***	3.3	5.0*
Population in 2010	56847	11754***	17172***	50423	65632
<i>Variables not used for matching</i>					
Average monthly registrations in first eight months of 2011	12.8	2.2***	3.5***	12.1	16.0
Standard deviation of monthly registrations in first eight months of 2011	4.1	1.3***	1.7***	3.2	4.4
Share of Industry in Municipal GDP (%)	26.3	14.9	17.4***	19.4*	35.8*
Total GDP per capita growth 2003 to 2008 (%)	182.3	181.7	180.2	166.5*	179.2
GDP per capita in 2008	11773	9381	10088	9250	18792**
Average monthly Municipal taxes received from firms in 2009	12220	15272	22451	40122	6978
Average monthly Municipal taxes received from firms in 2010	40851	5405***	9552***	49521	50077
<i>p-value for testing that:</i>					
Common linear trend to treatment over 2008 to August 2011		0.030	0.045	0.495	0.612
Common non-parametric time trend to treatment over this period		0.000	0.000	0.0004	0.000
Common non-parametric time trend to treatment except January 2011		0.000	0.000	0.1078	0.000
Number of Municipalities	55	767	357	29	18

Notes: *, **, and *** denote means statistically different at the 10, 5 and 1 percent levels respectively

Note that propensity score matching is done on the 36 monthly registration variables, we just show the means for each year and the standard deviation over these 36 months in this table to summarize the data.

Table 2: Impact of Minas Facil Expresso Introduction on Formalization
Dependent Variable: Number of Firms Registering in the Municipality in a Month

	Full Sample	Common Support	36-month Nearest Neighbor	Planned Implementation Candidates
<i>Panel A: Difference-in-Differences</i>				
Treatment Effect	-1.206*** (0.285)	-1.129*** (0.293)	-1.334** (0.619)	-1.425*** (0.537)
<i>Panel B: Fixed Effects</i>				
Treatment Effect	-1.167*** (0.280)	-1.085*** (0.288)	-1.256** (0.621)	-1.334** (0.523)
<i>Panel C: Fixed Effects (dropping September 2011)</i>				
Treatment Effect	-1.189*** (0.278)	-1.096*** (0.287)	-1.195* (0.638)	-1.235** (0.524)
<i>Panel D: Fixed Effects (dropping the three municipalities introducing program in Sept. 2011)</i>				
Treatment Effect	-1.037*** (0.267)	-0.956*** (0.274)	-1.096* (0.598)	-1.161** (0.504)
Sample Size	46,032	23,072	4,704	4,088
Number of Municipalities	822	412	84	73
Treatment Mean Sept 2010-Aug 2011	12.55	12.55	12.55	12.55

Notes:

Panel A controls for the mean number of formalizations in the municipality in each of 2008, 2009 and 2010.

Sample sizes are for panels A and B.

Robust standard errors in parentheses, clustered at the municipality level.

*, **, and *** indicate significance at the 10, 5, and 1 percent levels respectively.

Table 3: Time Path of Treatment Impact on Formalization Rates**Dependent Variable:** Number of Firms Registering in the Municipality in a Month

	Full Sample	36-month Common Support	36-month Nearest Neighbor	Planned Implementation Candidates
<i>Panel A: Difference-in-Differences</i>				
Average impact six months before Minas Facil Expresso introduced	-0.0860 (0.326)	0.0559 (0.330)	0.560 (0.580)	0.800* (0.475)
Average impact in month of implementation and month after	-2.331*** (0.449)	-2.173*** (0.449)	-1.799** (0.836)	-1.585** (0.632)
Average impact in months two or more after implementation	-0.796** (0.310)	-0.712** (0.320)	-0.755 (0.771)	-0.718 (0.674)
<i>Panel B: Fixed-Effects</i>				
Average impact six months before Minas Facil Expresso introduced	-0.0783 (0.327)	0.0650 (0.331)	0.594 (0.586)	0.850* (0.477)
Average impact in month of implementation and month after	-2.322*** (0.449)	-2.161*** (0.449)	-1.739** (0.842)	-1.496** (0.630)
Average impact in months two or more after implementation	-0.731** (0.303)	-0.641** (0.313)	-0.612 (0.782)	-0.536 (0.658)
Sample Size	46,032	23,072	4,704	4,088
Number of Municipalities	822	412	84	73
Treatment Mean Sept 2010-Aug 2011	12.55	12.55	12.55	12.55

Notes:

Panel A controls for the mean number of formalizations in the municipality in each of 2008, 2009, and 2010.

Robust standard errors in parentheses, clustered at the municipality level.

*, **, and *** indicate significance at the 10, 5, and 1 percent levels respectively.

Table 4: Impact of Minas Facil Expresso Introduction on Municipal Tax Receipts
Dependent Variable: Total Business Taxes Received in the Municipality in a Month

	Full Sample	36-month Common Support	36-month Nearest Neighbor	Planned Implementation Candidates
<i>Panel A: Difference-in-Differences</i>				
Treatment Effect	32,746*** (5,388)	27,879*** (4,925)	5,210 (5,229)	-7,549 (5,917)
<i>Panel B: Fixed Effects</i>				
Treatment Effect	21,586*** (7,667)	28,603*** (5,031)	5,902 (5385)	-7,152 (6,122)
Observations	47,676 853	41,644 718	5,046 84	4,234 73
Treatment Mean Sept 2010-Aug 2011	48367	48367	48367	48367

Notes:

Panel A controls for the mean number of formalizations in the municipality in each of 2008, 2009 and 2010.

Sample sizes are for panels A and B.

Robust standard errors in parentheses, clustered at the municipality level.

*, **, and *** indicate significance at the 10, 5, and 1 percent levels respectively.

Figure 1: Number of Municipalities Introducing Minas Facil Virtual by Month

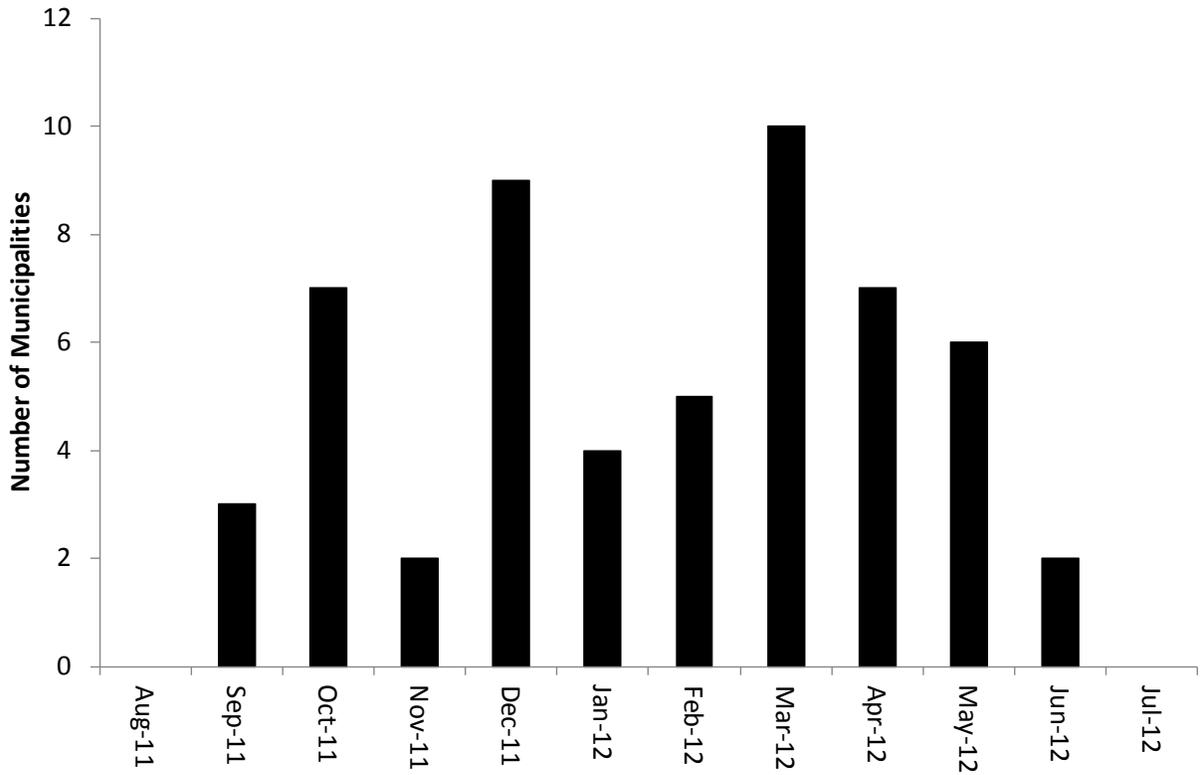


Figure 2: Monthly Registrations for Treatment and Control Municipalities

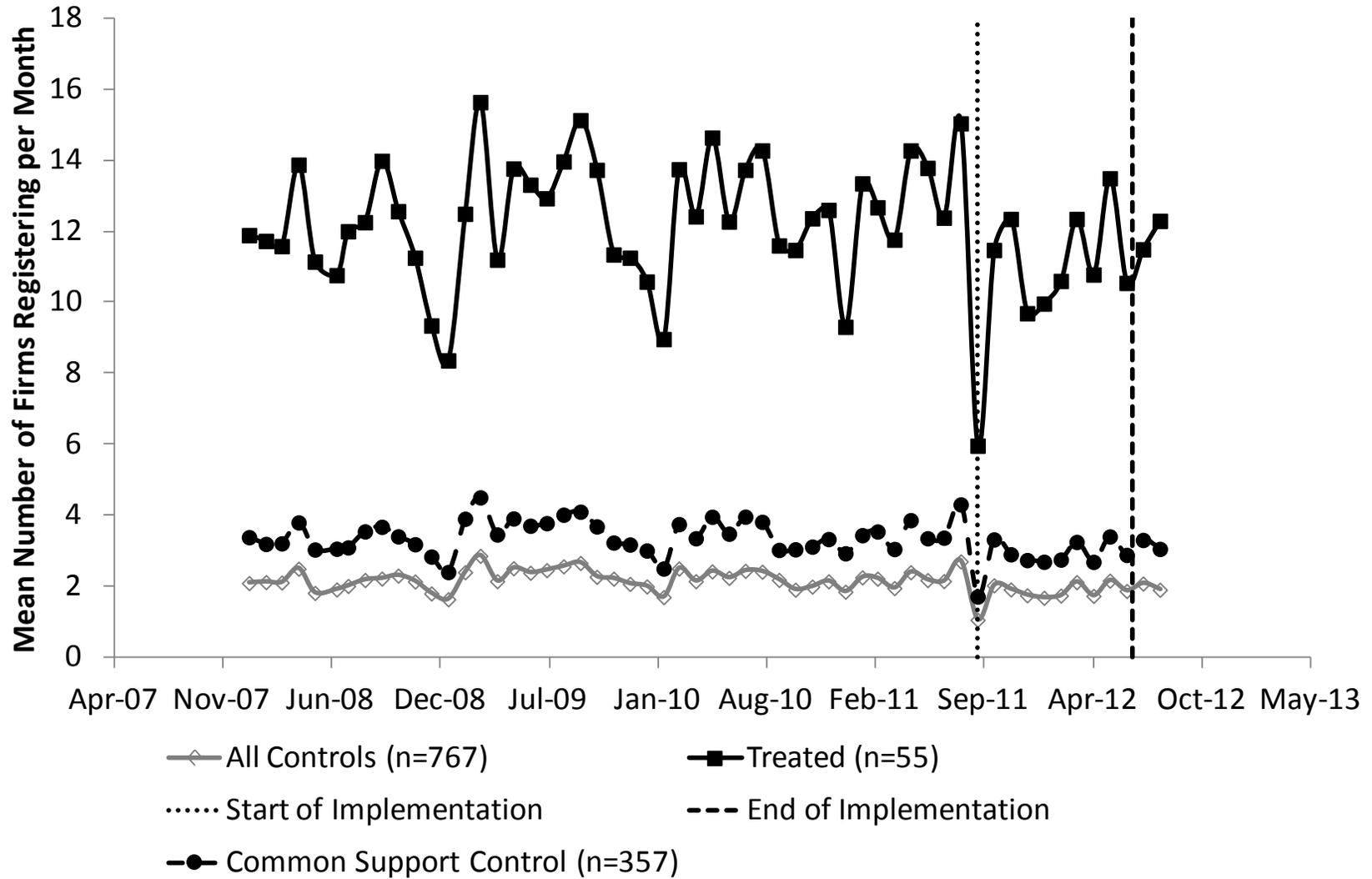


Figure 3: Monthly Registrations for Treatment and Nearest Neighbor and Control Non-Complier Municipalities

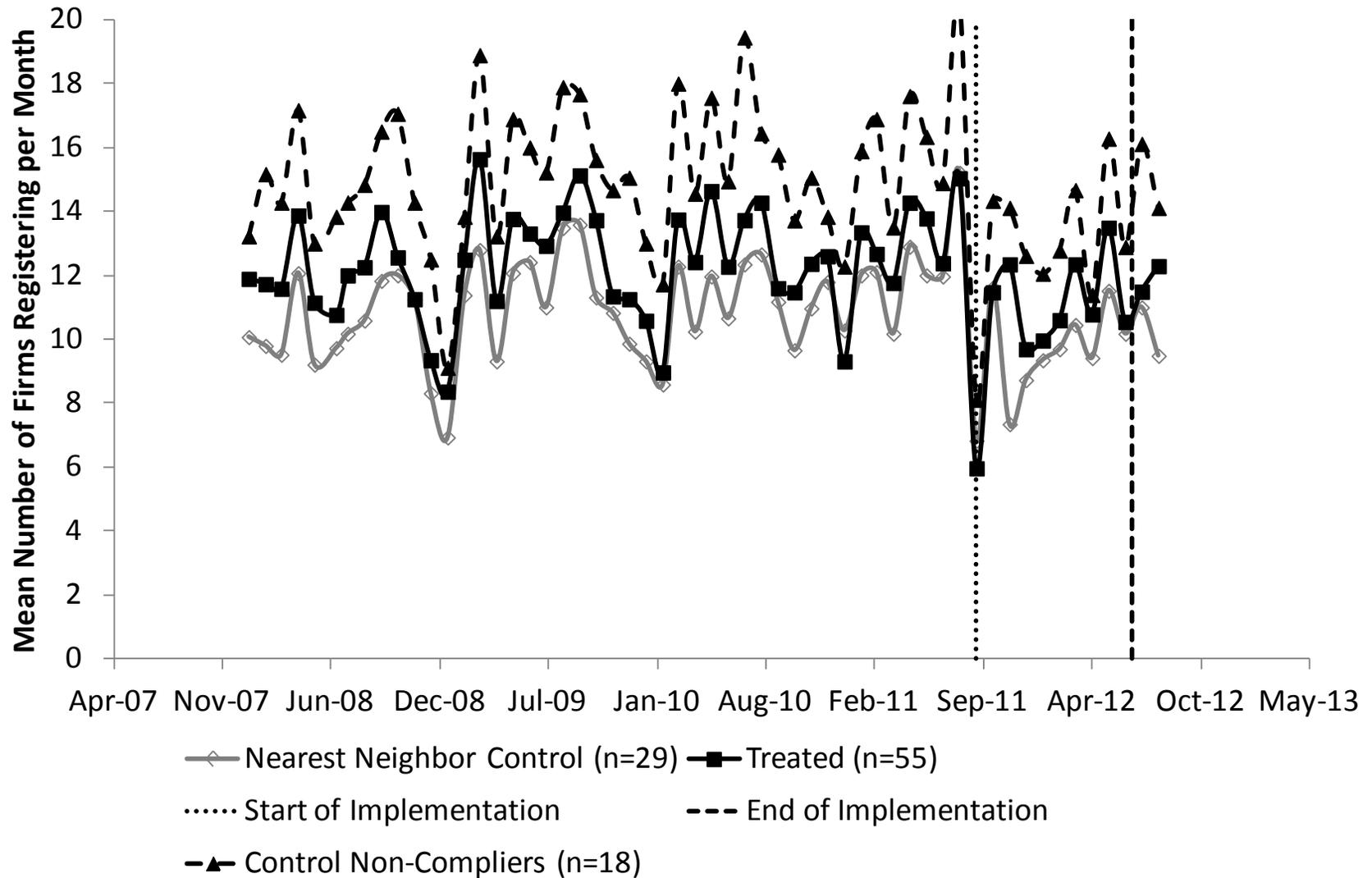
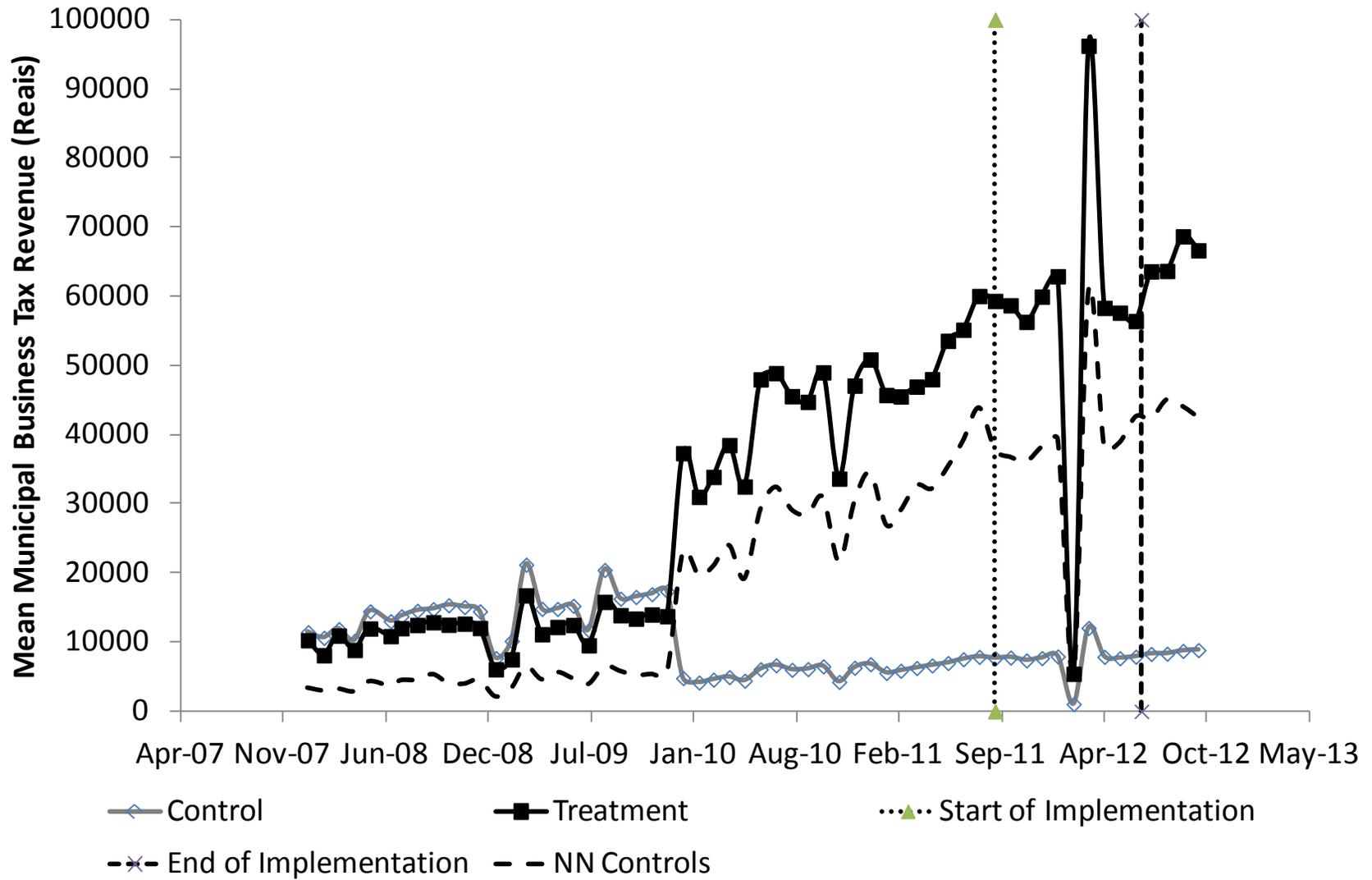
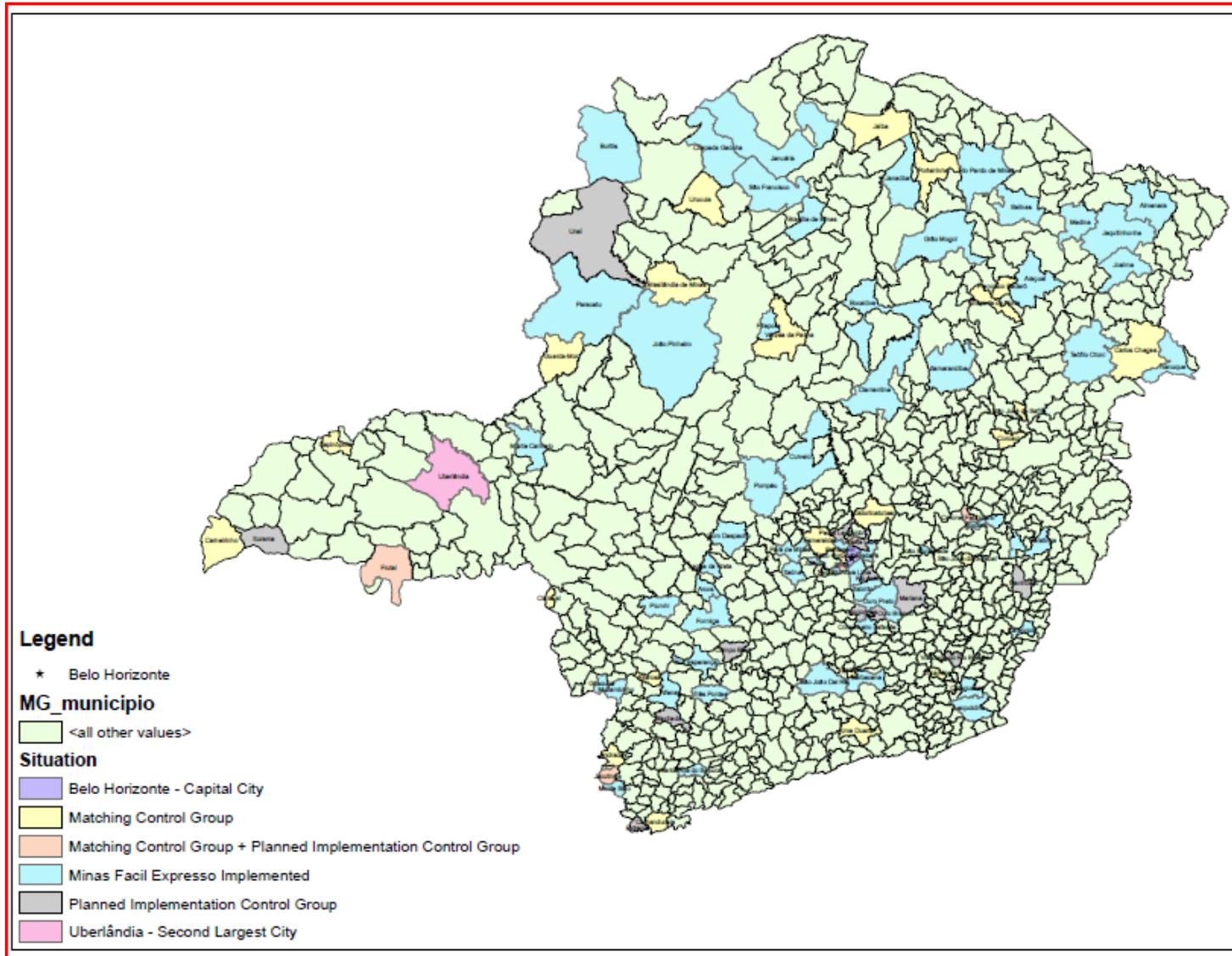


Figure 4: Municipal Business Tax Revenue for Treated and Control Municipalities



Appendix 1: Map Showing Treatment and Control Municipalities



Appendix Table 1: Probit of the Likelihood of having Minas Facil Expresso Implemented
 Marginal effects evaluated at the sample means.

	Likelihood of Minas Facil Expresso
Number of Firms Registering in Jan 2008	0.00162 (0.00163)
Number of Firms Registering in Feb 2008	-0.00380** (0.00184)
Number of Firms Registering in Mar 2008	2.15e-05 (0.00143)
Number of Firms Registering in Apr 2008	-0.00405** (0.00159)
Number of Firms Registering in May 2008	0.00436** (0.00197)
Number of Firms Registering in June 2008	0.00182 (0.00181)
Number of Firms Registering in July 2008	-0.00190 (0.00210)
Number of Firms Registering in Aug 2008	0.00150 (0.00216)
Number of Firms Registering in Sept 2008	0.00324* (0.00179)
Number of Firms Registering in Oct 2008	-0.00527*** (0.00180)
Number of Firms Registering in Nov 2008	-0.00358* (0.00187)
Number of Firms Registering in Dec 2008	0.00171 (0.00205)
Number of Firms Registering in Jan 2009	-0.00208 (0.00206)
Number of Firms Registering in Feb 2009	0.00449*** (0.00170)
Number of Firms Registering in Mar 2009	0.00268** (0.00136)
Number of Firms Registering in Apr 2009	0.00307** (0.00148)
Number of Firms Registering in May 2009	0.00151 (0.00202)
Number of Firms Registering in June 2009	0.00152 (0.00181)
Number of Firms Registering in July 2009	0.000408 (0.00155)
Number of Firms Registering in Aug 2009	-0.000777

	(0.00179)
Number of Firms Registering in Sept 2009	-0.000794
	(0.00137)
Number of Firms Registering in Oct 2009	0.00239*
	(0.00137)
Number of Firms Registering in Nov 2009	-0.00492**
	(0.00200)
Number of Firms Registering in Dec 2009	-0.000336
	(0.00183)
Number of Firms Registering in Jan 2010	0.000384
	(0.00181)
Number of Firms Registering in Feb 2010	-0.00270
	(0.00185)
Number of Firms Registering in Mar 2010	-0.00108
	(0.00153)
Number of Firms Registering in Apr 2010	0.00203
	(0.00195)
Number of Firms Registering in May 2010	0.00323*
	(0.00174)
Number of Firms Registering in June 2010	0.000205
	(0.00158)
Number of Firms Registering in July 2010	0.00133
	(0.00150)
Number of Firms Registering in Aug 2010	0.00294*
	(0.00175)
Number of Firms Registering in Sept 2010	-0.00870***
	(0.00274)
Number of Firms Registering in Oct 2010	0.00210
	(0.00181)
Number of Firms Registering in Nov 2010	-0.00163
	(0.00195)
Number of Firms Registering in Dec 2010	0.000809
	(0.00179)
Population in 2010/10,000	0.0110**
	(0.0053)

Number of Municipalities 822

Robust standard errors in parentheses

*, **, and *** indicate significance at the 10, 5, and 1 percent levels respectively.

ⁱ Even though firms would need to travel physically to another municipality to register, they would still be registering as a firm from the municipality in which they operate, and hence appear as registered in their actual municipality in the data.

ⁱⁱ To the extent officials targeted municipalities expected to grow, we will overstate the positive impact of the reform – so our finding of a zero to negative impact will still be valid.

ⁱⁱⁱ We selected these variables based on the fact that officials stated that they targeted the largest municipalities with the highest number of registrations for the reform. In addition, our previous research finds that past values of the outcome of interest are the ones that are most strongly correlated with the future outcome (Bruhn and McKenzie, 2009). That is, since our outcome of interest is registration rates, matching on past registration rates is likely to yield the most comparable groups of municipalities in the post-reform period.