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Essays on Development Economics

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by

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The dissertation of Camila Navajas Ahumada is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

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TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

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PUBLICATIONS

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ABSTRACT OF THE DISSERTATION

Essays on Development Economics

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Professor Gordon Dahl, Co-Chair
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This dissertation is a collection of three essays on development economics. In the first essay, we randomly assigned beneficiaries of a conditional cash transfer program in Peru to attend a 3 hour training session designed to build their trust in financial institutions. We find that the intervention: (a) significantly increased trust in banks, but had no effect on financial literacy; (b) significantly increased savings over a ten month period, and (c) had no effect of the use of accounts for transactions.

The second essay estimates crime avoidance costs in the aftermath of homicides that occur near employees' workplaces. I combine incident-level data on homicides with a matched

employer-employee dataset for São Paulo City, Brazil, and estimate causal effects by exploiting timing and hyper-local variation in how close employees work to a homicide. Exposed employees experience a significant and persistent reduction in labor earnings due to a decrease in the hourly wage rather than a reduction in hours worked. In terms of incidence, I do not find evidence of firm labor market responses to homicides. On the contrary, I find that the effects are driven by employees switching to establishments that typically pay lower wages and are located in other municipalities. In addition, workers move to establishments located farther from the crime scene and in municipalities with lower murder rates, consistent with avoiding future crime.

In the third essay, I employ a close elections regression discontinuity design to study how political alignment affects the income and assets police officers disclose. Police officers in aligned municipalities report to have 5% more total income and 52% more net assets. The effects of political alignment are greater for nonadministrative police officers, those with higher tenure and those who work in higher crime areas. Taken together, these results are consistent with a corruption-based explanation, either by an increase in extracted rents or a decrease in corrupt bureaucrats' misreporting (i.e., through an effect in the financial disclosure law's enforcement).

Chapter 1

Trust and Saving in Financial Institutions

We randomly assigned beneficiaries of a conditional cash transfer program in Peru to attend a 3 hour training session designed to build their trust in financial institutions. We find that the intervention: (a) significantly increased trust in banks, but had no effect on financial literacy; (b) significantly increased savings over a ten month period, and (c) had no effect of the use of accounts for transactions. The increase in savings is a 1.6 percentage point increase in the savings rate out of the cash transfer deposits, and a 0.5 percentage point increase in the savings rate out of household income.

1.1 Introduction

While bank accounts play a crucial role in everyday economic activities in high-income countries, fewer than 40% of the households in low- and middle-income countries (LMIC) have one (Demirgüç-Kunt et al., 2015). Instead, most poor households rely on informal, costly and risky alternatives and would benefit from access to a range of the financial services offered by formal institutions (see, for example, Bruhn and Love (2014); Célerier and Matray (2019); Dupas and Robinson (2013); Kast, Meier and Pomeranz (2018); Stein and Yannelis (2020)). Savings, in particular, facilitate investment in productive activities, education and household durables, and help smooth out income shocks. In light of these advantages, many LMIC governments and international organizations have set themselves the goal of improving these population groups' access to formal financial institutions.

One reason why poor households may not put their savings in a bank account is that they do not trust the bank to make that money available to them when it is wanted (Bold, Porteous and Rotman, 2012; Dupas et al., 2014; McKay and Seale, 2000; Bachas et al., 2018). Trust is an essential element of economic transactions and an important driver of economic development (La Porta et al., 1997; Algan and Cahuc, 2010). It is particularly crucial in financial transactions in which people exchange money for promises, and it is essential where the legal institutions that enforce contracts are weak (McMillan and Woodruff, 1999; Karlan et al., 2009).[1] A lack of trust may be one reason why randomized field experiments in three different countries have found that, even among people who take up accessible and free formal savings products, account use is low (Dupas et al., 2018). Mistrust may also account for the fact that beneficiaries of cash transfer programs withdraw most of the funds deposited in their bank accounts by the program in one lump-sum withdrawal at the beginning of each pay period; this has been found to be the case, for example, in Brazil, Colombia and South Africa (Bold, Porteous and Rotman, 2012), India (Muralidharan, Niehaus and Sukhtankar, 2016), Niger (Aker et al., 2016) and Mexico (Bachas et al., 2021).

We examine this issue with a field experiment designed to improve trust in financial institutions among beneficiaries of Peru's Juntos ("together") conditional cash transfer program. We teamed up with the Instituto de Estudios Peruanos (IEP), a well known Peruvian NGO specializing in financial inclusion, to design and implement a three-hour workshop intended to foster trust among Juntos beneficiaries and to evaluate the intervention's impact on beneficiary

[1] In developed countries, trust has been shown to be key to stock market participation (Guiso, Sapienza and Zingales, 2008), use of checks instead of cash (Guiso, Sapienza and Zingales, 2004), mortgage refinancing (Guiso, Sapienza and Zingales, 2004, 2008; Johnson, Meier and Toubia, 2019), and decisions to not withdraw deposits from financial institutions in times of financial crisis (Iyer and Puri, 2012; Sapienza and Zingales, 2012). In LMICS, there is evidence that trust affects borrowing money and the take-up of insurance (Karlan et al., 2009; Cole et al., 2013).

savings. The Juntos program sets up savings accounts for each beneficiary in the Banco de la Nacion (BN), a public institution dedicated to increasing the financial inclusion of underserved populations and regions, and has been depositing bimonthly transfers of 200 Peruvian soles (about US$ 60) into those accounts since the beginning of the program in 2005.

We find that program beneficiaries who were assigned to a financial trust workshop were more likely to report trusting the bank 12 months after the workshop. Specifically, while almost half the control group reported trusting the bank, the trust intervention caused a 40% increase in trusting the bank. A significantly larger proportion of the members of this latter group also said that they were more willing to put their savings in a bank account than to use informal alternatives such as savings in the form of assets like cattle. However, the workshops did not seem to have any effect in terms of the beneficiaries' knowledge about the banking system, their financial literacy or their understanding of how savings, loans and interest rates work.

Then, using high-frequency administrative account-level data, we examined the effect of the treatment on bank use and savings. While treatment did not affect the number of transactions (deposit and withdrawals), we did find that the financial trust workshops resulted in the treatment group saving 13 Peruvian Soles more than the control group over a ten month period. The increase in savings is close to double the savings of the treatment over the 10 month period prior to the intervention, 7 times the savings of the control group over the same period, a 1.6 percentage point increase in the savings rate out of the cash transfer deposits, and a 0.5 percentage point increase in the rate of savings out of household income.

We argue that building trust in financial institutions is a necessary condition for promoting the use of formal financial services (i.e., financial inclusion requires trust). Moreover, it is likely that trust is an important element in the effectiveness of other strategies, such as lowering transactions costs or raising interest rates. Our main contribution to this literature is to provide the first field experiment to generate evidence that trust in financial institutions can be influenced by experience and information and that higher levels of trust translate into an increase in the use of financial institutions.

Our study contributes to a small observational literature on the relationship between trust and savings (Karlan, Ratan and Zinman, 2014). Osili and Paulson (2014) show that immigrants who have experienced a systemic banking crisis in their country of origin are 11 percentage points less likely to use banks in the U.S. than otherwise similar immigrants who did not live through a crisis, and the effects are larger for people who experienced crises in countries without deposit insurance. Bachas et al. (2021) study an at-scale natural experiment in Mexico in which debit cards are rolled out to beneficiaries of a cash transfer program, who already received transfers directly deposited into a savings account. They find that after two years with a card, beneficiaries accumulate a savings stock equal to 2 percent of their annual income. Debit cards increased account usage and savings through two mechanisms: first, they reduced the transaction costs of accessing money in the account; second, they reduced monitoring costs, which leads beneficiaries to check their account balances frequently and build trust in the bank.

The rest of this paper is organized as follows. Section 1.2 describes the context and the intervention. Section 1.3 explains the research design. Section 1.4 presents the estimation strategy and Section 1.6 presents the results. Section 2.7 concludes.

1.2 Intervention

The workshop was delivered to beneficiaries of Juntos, Peru's conditional cash transfer program for poor households. Juntos gives 200 soles (approximately US$ 60) to the female head of beneficiary households once every two months provided that the household fulfills certain conditions related to schooling and to preventive health services. Juntos transfers are paid into a savings account that is opened for every beneficiary and managed by the Banco de la Nación (BN), a state-owned bank committed to service underserved populations. Typically Juntos beneficiaries withdraw all of the transfer in cash from the account soon after it is deposited. Juntos began its operation in 2005 and today covers over 700,000 beneficiaries living in 1,325 (70%) of the country's 1,874 districts.[2] Juntos beneficiaries who participated in the study has been receiving the transfers though deposits into BN accounts for at least two years prior to the intervention and therefore are already familiar with banks and bank operations.

The trust workshop was designed and implemented by Instituto de Estudios Peruanos (IEP), a well known Peruvian NGO that specializes in financial inclusion. The goal was to foster trust that money deposited in beneficiaries' bank accounts would be there when they wanted it by explaining why accounts are secure, that accounts are protected by government regulation, that there is a consumer-help telephone line available, and a trust building demonstration exercise. The workshop did not discuss the value of savings or why someone would want to save.

In addition to refreshments and snacks, the following topics were covered during the approximately 3-hour workshop:

A. *Account Access and Security*

The account into which Juntos transfers are deposited is like a lock box. The money deposited into the account will be there when wanted. The beneficiary must use an ATM card and password to withdraw money from the account. The card with the password is similar to a key that only the beneficiary can use to withdraw the money. No money can be deducted or withdrawn from the account without the card and the password. Hence, nobody else can access the account except the beneficiary.

B. *Government Consumer Protection Programs*

The Government protects all money deposited into bank accounts. The Government makes sure that all banks, including Banco de la Nacion, are safely managing and protecting your money. The Superintendencia de Banca, Seguros y AFP (Peruvian Superintendent of Bank) is in charge of making sure that banks safely manage your money including not allowing unlawful deductions from your account. Fondo de Seguro de Depositos (Deposit Insurance Fund) is in

[2]https://www.juntos.gob.pe.

charge of giving your money back in case of bankruptcy of Banco de la Nacion or fraud. If you have difficulty getting access to your funds or have a complaint you can call a toll free telephone hotline and obtain help in your own language. Cards with the free phone numbers were handed out.

C. *Multi-Red Agents*

Multi-red agents are small stores in underserved rural areas with POS machines that account holders can use to make deposits and withdrawals. The agents were fairly new so that there was some information as to who they were and how they worked. The workshop emphasized that agents were just as trustworthy as bank branches, that the accounts could only be accessed with the ATM card and password, that consumer protection laws applied to them, and that they could use the consumer hotline for problems with agents.

D. *How to Keep Money Safe*

Discussion about the relative safety of alternative places to leave money. In particular, why leaving money in Banco de la Nación is safer than keeping cash at home or purchasing animals or other assets that can be stolen or more easily appropriated by relatives, especially husbands, or friends.

E. *Trust Building Activity*

One out of the some 30 participants was randomly given 50 Soles to deposit in their account during the workshop and then asked to go to the bank to try to withdraw 30 soles later in the week and report back to the group.

1.3 Experimental Design, Analysis Sample and Compliance

The study sample was drawn from Juntos beneficiaries who live in rural villages in 17 districts in the Sierra region of Peru. These beneficiaries receive the Juntos transfers deposited in a BN savings account linked to a debit card. Beneficiaries can access their account either through the BN branch located in the district capital or through a MultiRed agent. These agents are private store owners located near rural beneficiary households and are certified as BN agents to conduct account transactions (deposits and withdrawals) for Juntos beneficiaries via a wireless point-of-sale (POS) device. In the study, we included villages with 15 or more Juntos beneficiaries who received the program transfer payment via direct deposit into their BN account. This gave us a universe of 130 villages from which we randomly assigned 64 villages to the treatment and 66 to the control group. The workshops were conducted between November 2014 and July 2015 and were rolled out over time at the district level.

At the time of the randomization, there were 4,562 Juntos beneficiaries in the 130 villages included in the study. We excluded Juntos beneficiaries who had been dropped from the program due to noncompliance with the conditionalities or who had moved away from their village (803). In addition, we trimmed off the top 0.1% of our sample to exclude outliers in the banking variables (251). Finally, we excluded households that, for scheduling reasons, had received Juntos payments twice in one bimester and that, as a result, did not receive a Juntos payment during the next period (321). This process left us with a total of 3,187 Juntos beneficiaries, of whom 1,450 live in treatment villages and 1,737 live in control villages. In all, 1,166 of the people assigned to treatment actually participated in the financial trust workshop, for a take-up rate of 80%. In addition, 198 out of the 1,737 people assigned to the control group attended the training, resulting in an 11% noncompliance rate in the control group.

1.4 Data and Measurement

Our primary source of information was administrative records from November 2013 to August 2015. Juntos provided the list of all beneficiaries living in the study villages as well as program compliance information for each of the beneficiary households. The Ministry of Development and Social Inclusion merged the information from Juntos with socio-demographic information from the national poverty mapping system (Sistema de Focalizacion de Hogares (SISFOH)) using the beneficiaries' national identification numbers (DNIs). BN then added transaction-level data on each deposit and withdrawal for each account, again using the DNI, and then provided us with the merged data after scrambling the DNIs to anonymize them. We aggregated the transaction-level data into account-level data by Juntos payment bimester, including the number of deposits, value of deposits, number of withdrawals, value of withdrawals, and savings.

Ideally, in order to study their savings behavior, we would like to know bank balances (i.e. the stock of savings) at the beginning of each payment bimester. Since that information was not provided, we instead measure the initial stock of savings as the value of all deposits minus withdrawals made during the five bimesters (10 months) prior to the intervention. Then, to compute the stock of savings in each bimester of the post-treatment period, we added to the last period's stock of savings the value of deposits minus withdrawals made during that bimester.

We have data for 11 bimesters (November 2013 to August 2015). However, information on withdrawals was accidently dropped from one bimester (July and August of 2014). We therefore exclude this bimester from the analysis. Thus, we relied on the remaining 5 pre-treatment bimesters for which we have complete data to compute the stock of savings at baseline and the 5 post-treatment bimester periods to analyze the effect of the training on savings.

In order to collect information on trust and financial literacy, we supplemented the administrative data with a household survey conducted between 12 and 18 months after the intervention. On our behalf, Innovations for Poverty Action (IPA) conducted a survey of the beneficiary households between April and May 2016. IPA enumerators were not informed about the intervention and did not know who was treatment and who was control. They identified themselves as IPA and did not refer to the workshop or IEP in anyway during the interviews. The response rate was 89.9% and was the same for treatment and control groups. BN merged the survey data with the administrative data using the DNI, and provided us an anonymized data base for analysis.

The survey collected information about household interactions with and perceptions of BN and covered the topics of trust, savings behavior and financial knowledge. The questions about trust covered trust in the bank, bank staff, and bank branch and preferences regarding saving in the bank versus holding cash in the house or purchasing assets such as animals. To measure knowledge/financial literacy, respondents were asked what a savings account was, what a MultiRed agent was, what savings and loans institutions were, and what interest rates were. The specific questions used to measure trust and knowledge/financial literacy are provided in Appendix Table A.1.

1.5 Methods

We examine the impact of treatment on two types of outcomes. The first set are measures of trust and knowledge/financial literacy obtained using data form the cross-sectional household survey. Since treatment was randomized and the experimental groups were balanced (see Table 1.1), we simply estimate the difference in the means of the treatment and control groups using the following regression:

$$Y_{iv} = \alpha + \beta\, ITT_v + \varepsilon_{iv} \tag{1.1}$$

where Y_{iv} is the outcome variable for individual i in village v, ITT_v is a dummy variable that indicates whether or not village v has been assigned to treatment and ε_{iv} is the error term. We cluster the standard errors at the village level to account for any intra-cluster correlation.

In studies with multiple outcomes, statistically significant effects may emerge simply by chance. The larger the number of tests, the greater the likelihood of incurring in a type I error. We correct for this possibility by using Bonferroni family-wise error rates to adjust the p-values of the individual tests as a function of the number of outcome variables. We rely on Bonferroni

FWER corrections at the 10% level of statistical significance in conceptually similar blocks of outcomes.[3]

The second set of outcomes are transactions and savings obtained using data from the longitudinal administration account-level data. Given that Juntos transfers are made every two months, the data is organized in bimesters, following the timing of the transfers. This allows us to examine how the treatment effect evolves over exposure – i.e., the number of bimesters since treatment.

We estimate the effect of treatment on the number of transactions and savings using the following regression specification:

$$Y_{ivt} = \alpha_i + \sum_{k \neq -1} \beta_k \, ITT_{v,k} + \lambda_t + \varepsilon_{ivt} \tag{1.2}$$

Where Y_{ivt} is the outcome variable for individual i in village v in calendar period t. $ITT_{v,k}$ takes a value of 1 if the village v is assigned to treatment and k is the number of bimesters since treatment, with treatment happening at $k = 0$. We also include bimester fixed effects (λ_t) and individual fixed effect (α_i). The individual fixed effects control for any concerns over composition effects that might have occurred due to the rollout over time by district. However, the results are almost identical with and without fixed effects. The term ε_{ivt} is a random error term that is possibly correlated within villages due treatment assignment at the village level. We therefore cluster standard errors at the village level.

The models in equations 1.1 and 1.2 estimate the intention-to-treat (ITT) impacts. Since there is some noncompliance, we also estimate the local average treatment effect (LATE) by instrumental variables using 2SLS with treatment assignment as an instrument for participating in the workshop. Again, we cluster the standard errors at the village level as a basis for statistical inference.

[3] For example, if there are 5 outcome variables, the Bonferroni corrected p-value is 0.02 (=0.1/5). Therefore, we would reject the null hypothesis of no treatment effect if the estimated coefficient is significant at the 2% level.

1.6 Results

1.6.1 Descriptive Statistics and Balance Checks

Descriptive statistics for the analysis sample at baseline for households in the treatment and control groups are presented in Table 1.1. In two cases, out of 17 contrasts, we reject the null hypothesis of equal means between groups at conventional levels of statistical significance –naturally. However, once we use using Bonferroni family-wise error rates, we never reject the null hypothesis. In Appendix Table A.2, we compare the means of baseline variables for the analysis sample and for those excluded from the analysis and find only one variable for which we reject the null hypothesis of equal means. Again, once we use using Bonferroni family-wise error rates, we never reject the null hypothesis, suggesting that the analysis sample is representative of the population of Juntos beneficiaries in the 17 districts.

The analysis sample (see Table 1.1) consists of households where the primary Juntos beneficiary is female, is on average about 40 years old, has completed 6 years of schooling and whose primary language is not Spanish. About two thirds of these beneficiaries work in agriculture but only 12% own their own farms. Very few beneficiaries have contact with formal financial institutions, as only 4% have a bank account other than their Juntos BN account and only 3% participate in a rotating savings and credit association (ROSCA). On average, individuals make one deposit (the Juntos transfer) into their BN account and one withdrawal from it each bimester (two transactions per bimester). The difference between the baseline stock of savings (i.e. the difference between all deposits and withdrawals in the 10 month period prior to the intervention) between the treatment and control group is -3.8 Peruvian Soles and is not statistically different from zero.

1.6.2 Trust and Knowledge/Financial Literacy

Baseline levels of trust are low. Only 48% of the control group trusts the bank and 36% trust bank staff. Moreover, 54% believe money is safer at home than in a bank and 29% believe it is safer to purchase animals as a store of value than keep money in a bank (Appendix Table A.3). Overall, the training workshops appear to have increased trust in the banking system substantially (see Figure 1.1, Panel A, and Appendix Table A.3). All of the treatment effects on all of the outcome variables are sizable in magnitude and significantly different from zero using conventional p-values, although trust in bank staff is not statistically significant using Bonferroni family-wise error rates. The effect of treatment increases the number of beneficiaries who reported that they trusted the BN by 19 percentage points, or 40% over the control mean (48%). Trust in BN staff increases by 6.5 percentage points, or 18% over the control mean (36%). Trust in the BN branch increases by 11 percentage points, or 14% over the control mean (78%). Treatment also increases the preference for putting savings in the bank over keeping savings at home by 21 percentage points, or 46% over the control mean (46%). Treatment increases the preference for putting savings in the bank over holding savings in the form of assets such as livestock by 18 percentage points, or 62% over the control mean (29%). Finally, treatment increases a summary measure of overall trust in banking by 30% over the control mean (49%).[4]

Another possible explanation for any increase in savings associated with the workshop is that the workshops may have also increased the beneficiaries' knowledge about the banking system and financial literacy. If that were the case, it would be hard to distinguish the effect on saving behaviour of trust from that of knowledge/financial literacy. However, there is little evidence to support this hypothesis. Baseline levels of financial literacy are large. About 74% of control households understand savings, 99% report knowing how to use a multired agent, 85% seems to understand interest rates, and 32% savings. By and large we find very small and

[4]Overall trust is the sum of the 6 trust dummy variables divided by 6. Similarly, the overall knowledge variable is the sum of the 4 knowledge dummy variables divided by 4.

statistically insignificant effects of the workshop on knowledge/financial literacy (see Figure 1.1, Panel B, and Appendix Table A.3). Regardless, there is little evidence from other studies that financial literacy leads to higher use of financial services or better financial outcomes.[5] Thus, together these results suggest that any effect on savings was likely driven by increased trust as opposed to increased knowledge/financial literacy.

1.6.3 Use of Accounts and Savings

The effect of the financial trust workshop on savings is shown in Figure 1.2 (see also Appendix Table A.4), where the local average treatment effects by bimester since the workshop was offered are presented.[6] The difference between the treatment and control groups is positive and increases over time. This suggests that treatment beneficiaries are saving more than the control group during each period and that their stock of savings is rising. After 5 bimesters (10 months), the difference in the stock of savings averaged 13 soles. At baseline (k=-1), the average stock of savings was 7 soles, which implies that, in less than a year, the treatment increased saving levels in approximately double baseline savings. In addition, this effect is almost 7 times the savings of the control group over the same period.[7] Finally, this treatment effect also translates into a 1.6 percentage point increase in the savings rate out of Juntos transfers and a 0.5 percentage point increase in the savings rate out of household income during the period studied.[8]

While the financial trust workshop had a large effect on trust, it does not seem to have affected the use of the account for transactions (see Figure 1.3 and Appendix Table A.4). One possible reason is that the closest BN branch or agent was still quite far away from most of the households. For example, on average, the closest agent was 4 kilometers away, which represents,

[5] See for example Bruhn and Love (2014); Carpena et al. (2011); Cole, Sampson and Zia (2011); Cole, Paulson and Shastry (2016); Drexler, Fischer and Schoar (2014).

[6] The intention-to-treat (ITT) results are very similar and are also reported in Appendix Table A.4.

[7] In particular, over the same period of time, the control group have saved 2 soles.

[8] After the training, JUNTOS beneficiaries have received 800 soles in four 200 soles payments and, based on the information on the Survey (2016), their average total income over the same period was 2835 soles.

on average, a total travel time of over 50 minutes. This is consistent with evidence from Mexico that transactions fall the further a household is located from bank branches and ATMs (Bachas et al., 2018).

Finally, the workshop also discussed the relatively new multi-red agent network, i.e. small shops with POS devices that beneficiaries can use to access their accounts with their ATM card and password. The agents are substantially closer to beneficiary households than bank branches and were set up to lower transaction costs of account access. Using administrative data we estimate the effect of treatment on the location of withdrawal (branch versus agent) by bimester of exposure using the specification in equation 1.2. We find no effect of the workshop on agent use (Figure 1.4). This result is consistent with the workshop not affecting knowledge of use of bank functions nor influencing any behavior related to agents.

1.7 Conclusion

We conducted a field experiment to assess the extent to which the level of trust in financial institutions among Peruvian cash transfer program beneficiaries could be raised and, if their level of trust was raised significantly, whether it would be effective in increasing their use of their bank accounts for transactions and saving. The results show that it was indeed possible to substantially increase their level of trust and thereby bring about an increase of 13 Peruvian soles in their savings account balances after 10 months as compared to an average of 7 soles at baseline. The savings effect represents a 1.6 percentage point increase in the saving of Juntos cash transfers and a 0.5 percentage point increase out of household during those 10 months.

In this study we are interested in the role of trust, i.e. the belief that deposited in account will be there when the holder wants to access it, on the use of bank. We document that the workshop did indeed build trust and then compared account use between those that were offered the workshop and those who were not. It is important to note that while there is strong evidence that the workshop built trust, there there is no evidence that the intervention increased beneficiary knowledge about the banking system or their financial literacy. Even if the workshop did improve financial literacy there is little evidence from other studies that financial literacy leads to higher use of financial services or better financial outcomes. This implies that the mechanism by which the workshop increased savings was through enhanced trust and not through enhanced financial literacy or knowledge and experience with banks.

Although the workshop used several different types of messages through which to try to build trust (account access security, consumer protection, safe savings, and a trust building exercise), we did not attempt to disentangle them empirically. Our primary interest is in the effect of trust on the use of formal financial services and thus did not design an experiment to assess which types of messages best built trust.

Our results show that trust in financial institutions is an important factor in encouraging poor households to hold their savings in bank accounts. The magnitude of the treatment effect is similar to other interventions such as lowering monetary and non-monetary transactions costs, increasing the rate of return to savings, as well as behavioral nudges and reminders (Figure 1.5). Trust is also likely to increase the effectiveness of these other other interventions as well, such as those involving a reduction in transaction costs or increased returns, in terms of influencing savings. Overall, trust may be key for the financial inclusion of the poor.

Around the world, over 100 million families participate in cash transfer programs and therefore are a vehicle to improve the financial inclusion of the poor. While many of these programs open bank accounts in beneficiaries' names, few of those bank accounts are actually used, as beneficiaries prefer to withdraw their entire transfer as soon as the cash is available (Bold, Porteous and Rotman, 2012; Muralidharan, Niehaus and Sukhtankar, 2016; Aker et al., 2016; Bachas et al., 2021). Simple cheap trust building exercises, like our workshop, maybe transformative in the financial inclusion of cash transfer program beneficiaries.

1.8 Figures and Tables

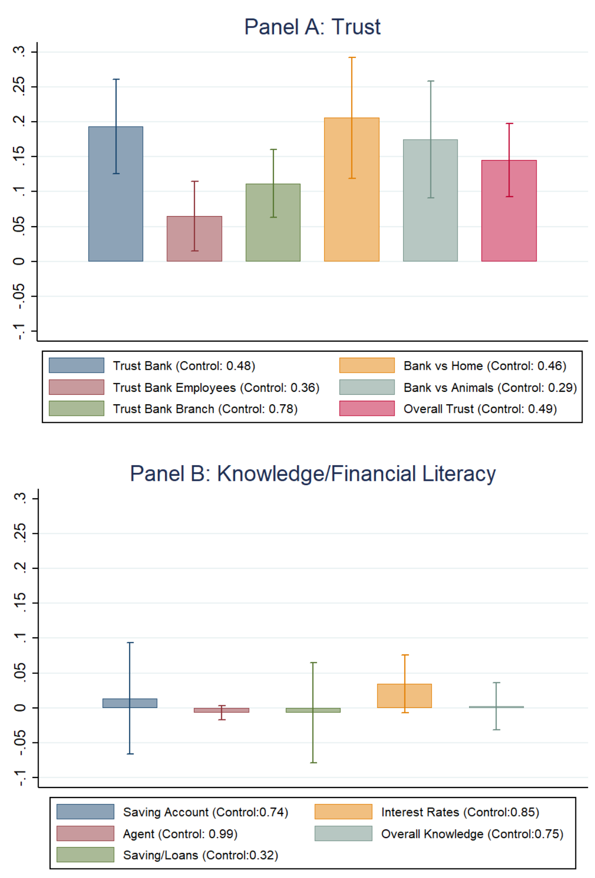

Figure 1.1: Effect of Financial Trust Workshops on Trust and Knowledge/Financial Literacy

Notes: This figure reports the differences in the mean for each variable between the treatment and control groups and the 95% confidence region for that difference based on data from the household survey. The difference in the means is the LATE estimate of the impact of the trust training workshop on the outcomes. The mean outcome for the control group is given in the key in parentheses. The point estimates, standard errors, sample sizes and means of the control groups for each of the bars are presented in Appendix Table A.3. Appendix Table A.1 reports the questions used to collect the outcome measures. The overall trust and knowledge/financial literacy measures are the sum of the responses regarding other outcome measures divided by the number of outcome measures.

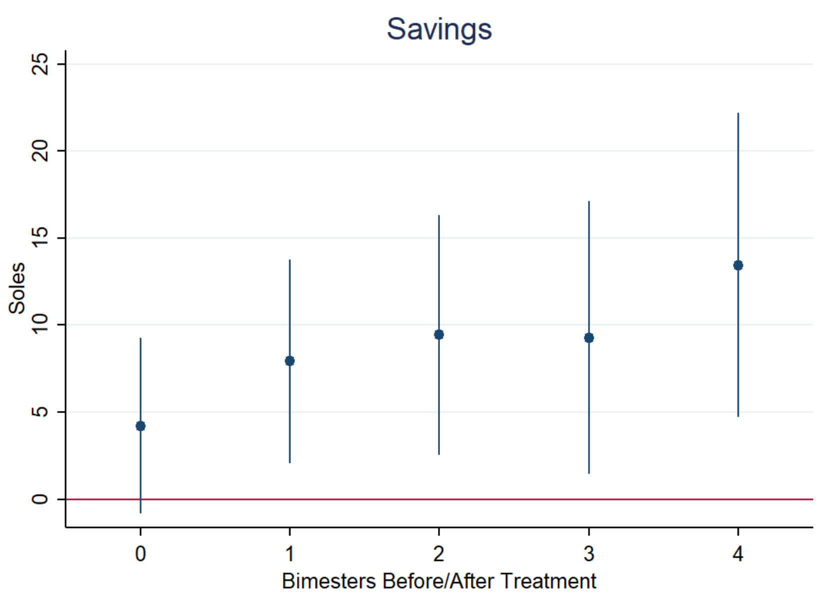

Figure 1.2: Effect of Financial Trust Workshops on Saving

Notes: This figure presents the estimated LATE treatment effects and 95% confidence regions of the financial trust workshops on the level of savings in bank accounts at the end of each bimester over time. (Treatment is based on equation (1.2) using the administrative data on 3,184 households over 6 bimesters.) The estimates associated with this figure are presented in Appendix Table A.4. The F-statistic for the first stage of the LATE estimates as well as the ITT estimates are also reported in Appendix Table A.4.

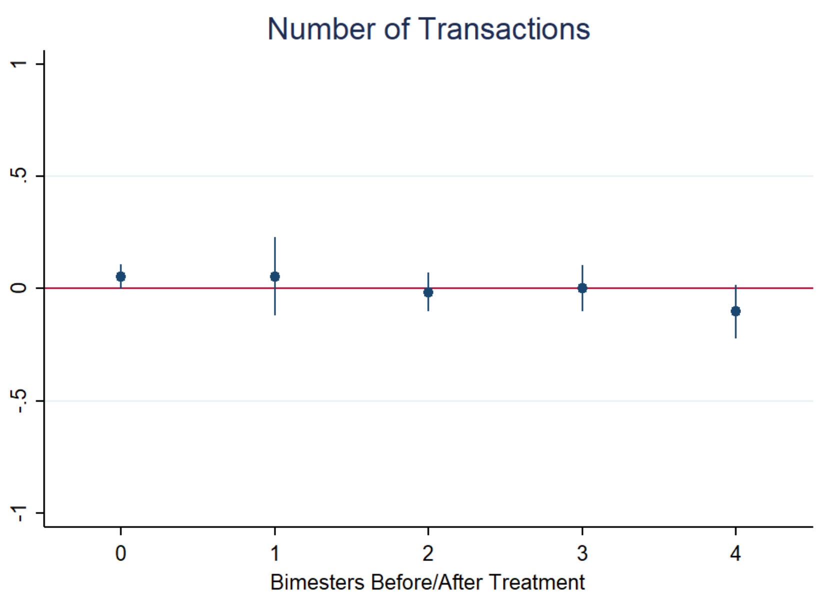

Figure 1.3: Effect of Financial Trust Workshops on the Number of Transactions

Notes: This figure presents the estimated LATE treatment effects and 95% confidence regions of the financial trust workshops on the number of transactions (deposits plus withdrawals) by bimester. (Treatment is based on equation (1.2) using administrative data on 3,184 households over 6 bimesters.) The estimates associated with this figure are presented in Appendix Table A.4. The F-statistic for the first stage of the LATE estimates as well as the ITT estimates are also reported in Appendix Table A.4.

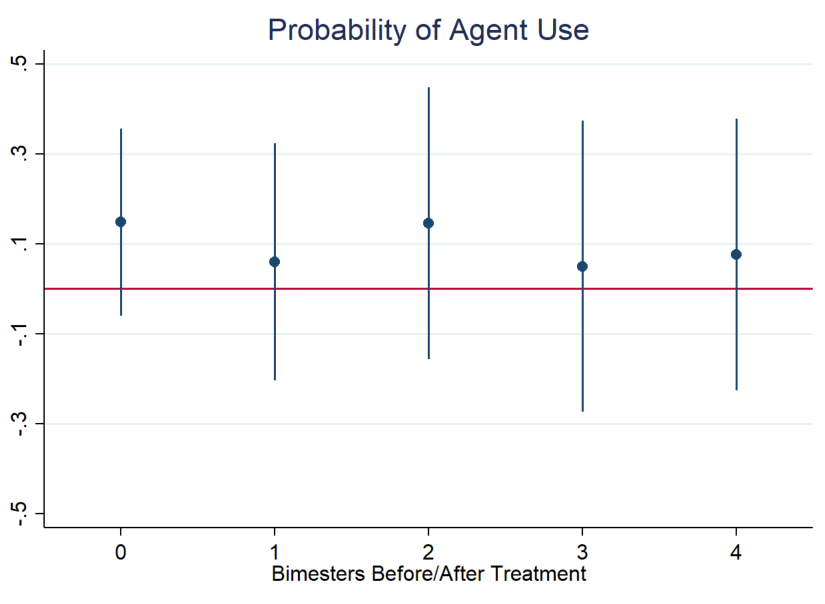

Figure 1.4: Effect of Financial Trust Workshops on Use of Agent for Withdrawal

Notes: This figure presents the estimated LATE treatment effects and 95% confidence regions of the financial trust workshops on the the use of an agent to make withdrawals by bimester. (Treatment is based on equation (1.2) using administrative data on 3,184 households over 6 bimesters.) The estimates associated with this figure are presented in Appendix Table A.4. The F-statistic for the first stage of the LATE estimates as well as the ITT estimates are also reported in Appendix Table A.4.

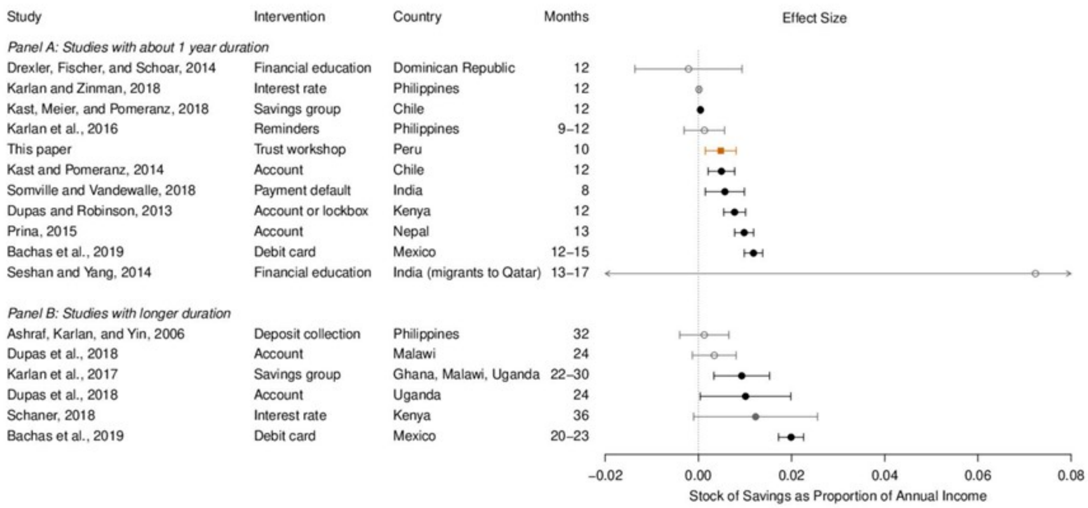

Figure 1.5: Comparison of Treatment Effects of Various Interventions on Household Savings as a Proportion of Income

Notes: Adapted from Bachas et al. (2021). This figure includes field experiments that estimates the effect of an intervention on savings and has income data available to be able to convert the effect on the stock of savings into the a savings rate out of income. Studies that did not have income information available were excluded from this comparison. Bachas et al. (2021) describes each of these studies in detail as well as the construction of this figure.

Table 1.1: Baseline Descriptive Statistics (Analysis Sample)

Variable	Treatment			Control			Means	P Value
	Mean	SD	N	Mean	SD	N	Difference	
Age	39.73	10.03	1408	40.80	10.16	1661	-1.07	0.12
Female	0.97	–	1408	0.96	–	1661	0.01	0.17
Household Size	4.77	1.61	1408	4.82	1.60	1661	-0.05	0.59
Years of Schooling	5.74	4.16	1407	6.05	4.05	1661	-0.31	0.54
Preferred Language	0.17	–	1408	0.20	–	1661	-0.03	0.54
Work at Farm	0.65	–	1257	0.65	–	1565	-0.01	0.93
Own Farm	0.12	–	1257	0.11	–	1565	0.01	0.79
Own Home	0.82	–	1408	0.79	–	1661	0.03	0.55
Have Other Bank Accounts	0.04	–	1408	0.03	–	1661	0.00	0.66
Have ROSCA	0.03	–	1392	0.03	–	1642	0.00	0.57
Number of Deposits	0.97	0.19	1408	0.98	0.15	1661	-0.01	0.05
Number of Withdrawals	0.93	0.28	1408	0.93	0.30	1661	0.01	0.83
Number of Transactions	1.90	0.44	1408	1.91	0.39	1661	-0.01	0.76
Value of Deposits	192.79	37.95	1408	195.82	28.74	1661	-3.03	0.04
Value of Withdrawals	186.23	55.37	1408	184.10	58.29	1661	2.13	0.70
Use Agent for Withdrawal	0.24	–	1408	0.26	–	1661	-0.02	0.84
Stock of Savings	7.07	53.65	1408	10.83	67.60	1661	-3.77	0.53

Notes: This table uses Survey Data (2016) for the socioeconomic variables and administrative data for the bank variables (in the bimester before the beginning of the Financial Trust Training). The Stock of Savings variable is calculated using the bank balances in the five bimesters before the beginning of the intervention. The Preferred Language variable takes 1 if Spanish or 0 if Quechua or Aymara. All monetary values are expressed in Soles.

1.9 Acknowledgements

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Chapter 2

Avoiding Crime at Work: Homicides and Labor Markets

This paper estimates crime avoidance costs in the aftermath of homicides that occur near employees' workplaces. I combine incident-level data on homicides with a matched employer-employee dataset for São Paulo City, Brazil, and estimate causal effects by exploiting timing and hyper-local variation in how close employees work to a homicide. Exposed employees experience a significant and persistent reduction in labor earnings due to a decrease in the hourly wage rather than a reduction in hours worked. In terms of incidence, I do not find evidence of firm labor market responses to homicides. On the contrary, I find that the effects are driven by employees switching to establishments that typically pay lower wages and are located in other municipalities. In addition, workers move to establishments located farther from the crime scene and in municipalities with lower murder rates, consistent with avoiding future crime. Overall, these findings demonstrate that, in addition to the costs imposed on victims, crime avoidance costs are consequential when designing and evaluating policies that cost-effectively prevent crime.

2.1 Introduction

Crime is a major concern for citizens around the world;[1] therefore, governments and international organizations have to decide how much to invest in improving citizen security. To make these decisions, policy makers need information on how costly and effective policies are at reducing crime and on the benefits of crime reduction, which implies having a comprehensive understanding of the costs of crime. From a citizen perspective, there are costs incurred by victims (ex post costs); however, significant costs may also be derived from preventing criminal victimization (ex ante costs).[2] In particular, many citizens never become victims; however, this fact does not imply that these citizens are not affected by crime because, for example, they may spend many economic resources to prevent criminal victimization. Policy makers must take these costs into account because they depend not only on the probability of becoming a victim and the ex post cost associated with it but also on the preferences and perceptions that citizens have about crime. In the case of labor markets, ex ante costs may arise, for example, when employees avoid crime near the workplace either by (a) adjusting their labor supply or (b) through labor mobility (i.e. switching to jobs located in lower crime areas).

Although there is evidence that citizens avoid crime (Cullen and Levitt, 1999; Chetty, Hendren and Katz, 2016), measuring the costs of such behavior has proven challenging. One reason is that, while ex ante costs take place *before* the occurrence of a crime, determining whether these costs are *caused* by crime (i.e., a causal effect) implies constructing a counterfactual of what would have happened in the absence of crime. In that sense, a challenge in measuring crime avoidance costs comes from the fact that most crime data relies on citizens' potentially endogenous willingness to report the crime to the authorities.[3] However, even when it is possible to count with

[1]Survey data reveals that a median of 83% of people around 34 emerging and developing economies answer that crime is a "a very big problem" in their countries (PewResearchCenter, 2014)

[2]See, for example, Domínguez and Raphael (2015) for a comprehensive discussion on both ex ante and ex post costs of crime.

[3]For example, in areas where citizens have a lower willingness to avoid crime, people might also have low incentives to report the crime to the authorities.

reliable crime data, another challenge is that crime is more likely to occur in poor neighborhoods where people may not only have different willingness to avoid crime but also different economic constraints to do so. Finally, another major challenge is finding reliable measures of crime avoidance costs. One approach would be to rely on survey data (i.e., stated preferences); however, citizens' answers may be biased and, more importantly, such bias may be correlated with their crime experiences. To address this potential concern, revealed preferences approaches focus on market outcomes and study actual decision making; however, given that behind any market outcome agents from both supply and demand sides of the market are involved, studying agents decision making and their crime avoidance costs require a simultaneous understanding on how each side of the market is affected by crime.[4]

This paper overcomes all these challenges and estimates crime avoidance costs in the aftermath of homicides that occur near employees' workplace. In particular, I combine incident-level data on all homicides in São Paulo City with a matched employer-employee dataset that contains information on all formal-sector workers. One main advantage of focusing on homicides is that, as opposed to other crimes, homicides do not rely on the victims' willingness to report the crime to the authorities. In addition, the linked employer-employee data contain information about labor demand and supply sides simultaneously, which allow me to study (a) the crime incidence between firms and employees and (b) the type of jobs that employees switch to after a crime, which is crucial to understanding the role of labor mobility in avoiding crime. By geo-coding the precise location of both establishments and homicides and calculating distances between those coordinates, I am able to define a measure of establishments and employees' exposure to homicides based on geographic proximity. Then, to address the potential endogeneity problem between homicides and labor market outcomes, I use a dynamic difference-in-differences

[4]For example, this challenge is also present in the literature that estimates the willingness to pay for an amenity using housing prices (Rosen, 1974). Such literature usually focuses on the short-run because the housing supply is considered inelastic and, therefore, any price change would be completely demand driven. See, for example, Gibbons (2004), Greenstone and Gallagher (2008), Linden and Rockoff (2008), Pope (2008), Pope and Pope (2012) and Ajzenman, Galiani and Seira (2015), among others.

design that allows me to exploit hyper-local variation in the location and timing of each homicide and compare changes over time between employees working in the same neighborhood in establishments located closer and farther away from a homicide.

Exploiting within neighborhood variation in homicides location implies that these events are hyper-local shocks that, as I show, (a) are unanticipated (i.e., they are not predicted by previous events), (b) do not predict future homicides and (c) are dissipated within a few blocks. This implies that the level of previous crime exposure and future crime risk between employees who have and have not been exposed to this type of shocks is similar overall (i.e., neighborhood characteristics are the same) and the difference in their crime exposure relies on their proximity to the crime scene. However, the salience of spatial proximity may still affect both employees and firms in several ways. From employees' perspectives, such an event near their workplace may affect, for example, their risk attitudes (Callen et al., 2014; Mejia and Restrepo, 2016; Moya, 2018; Brown et al., 2019) and perceptions about future crime (Braakmann, 2012; Mastrorocco and Minale, 2018; Esberg and Mummolo, 2018; Vinæs Larsen and Leth Olsen, 2020), triggering a crime avoidance behavior and changing their employment choices. Similarly, customers may also affect their consumption choices and affect firm sales. As a consequence, firms may need to reallocate resources to attract both employees and customers (Besley and Mueller, 2018) or to substitute those workers who leave after a crime (Jäger, 2016).

This paper offers two main results. First, employees working in establishments within five blocks from a homicide, relative to those working within five to ten blocks, experience a significant and persistent reduction in labor earnings. In particular, 5 years after the homicide, employees' weekly labor earnings are reduced by 7.7%. Avoidance costs can decrease labor earnings in two ways (i.e., hours worked or wages); therefore, I first analyze the effects on hours worked and find small and insignificant estimates.[5] Consistent with this labor supply response, I find that the effects on labor earnings are driven by an effect, about the same magnitude, on the

[5] Even though my data only contain formal workers and I cannot disentangle whether employees are unemployed or working in the informal sector, I do not find evidence supporting that homicides affect that margin.

hourly wage.

Second, I analyze the crime incidence between firms and employees and, while I do not find evidence consistent with firm labor market responses to homicides, I show that the effects on wages are driven by employees' reactions to homicides. In particular, I analyze establishment-level outcomes and find that coefficients on wage expenditure, hourly wage, number of employees, and the probability of shutting down are small and insignificant. While this setting is characterized by a high job turnover rate (Gonzaga, Maloney and Mizala, 2003), which is not significantly affected by crime exposure, I find that results are driven by the fact that being exposed to a homicide significantly affects the type of jobs that employees switch to. In particular, following Abowd, Kramarz and Margolis (1999), I estimate establishment-specific wage premiums and find that these premiums are 38% lower for exposed employees. In addition, the establishments exposed employees switch to are more likely to be located in other municipalities (i.e. outside São Paulo City). The effects on the probability of switching municipalities increase over time, reaching its maximum around 2.5 years after the homicide before decreasing. Consistent with these results, I provide suggestive evidence that some features of the Brazilian labor market and certain labor mobility costs prevent exposed employees from switching jobs earlier, which may also explain why there is not a differential effect on the job separation rate between exposed and unexposed workers.

There are two possible reasons why exposed employees switch to establishments that typically pay lower wages and are located in other municipalities. The first reason is that employees may react to homicides because, for example, they dislike working at a location where a murder took place (i.e., backward-looking behavior). However, another reason is that employees may be preventing a future crime exposure (i.e., forward-looking behavior). Employees working within 5 blocks from a homicide, relative to those working within 5 to 10 blocks, are not more likely to be exposed to another one in the future; however, the salience of the spatial proximity might alter their risk attitudes or crime perceptions, triggering a crime avoidance behavior. I conduct three exercises that provide evidence consistent with employee crime avoidance. First, I show that exposed employees are more likely to switch to establishments located farther away from the crime scene. Second, I find that workers are more likely to work in lower crime municipalities (i.e. municipalities with a lower murder rate than São Paulo City). Finally, by comparing police versus non-police homicides, I find larger effects for the former which, given the setting and period of study, might have represented an important threat to the security of the average citizen. Overall, the costs of crime (i.e., lower earnings) are estimated *after* the occurrence of a homicide; however, they have an ex ante interpretation in the sense that employees avoid *future* crime exposure by moving to safer areas (i.e., ex ante costs).

Lastly, given that there is evidence consistent with employees avoiding crime, the occurrence of a homicide can affect employees working in establishments located nearby the crime scene along with potential new employees who might find those establishments less attractive. While I do not find an effect on the separation rate, firms nearby a homicide may find it difficult to fill new vacancies and, for example, they may need to offer a compensating wage differential. However, I do not find evidence consistent with firm labor market responses to homicides. One possible reason is that, given that my estimates are locally identified, a firm effect may not be detected if, for example, new employees equally dislike all firms within the whole neighborhood that experienced a homicide. However, I find that the effects on exposed employees fade away completely at five blocks from the homicide meaning that it is possible that there is still a thick labor market to fill new vacancies and therefore exposed firms are not significantly affected by homicides.[6]

Taken together, these results highlight the importance of crime avoidance costs. Homicides increase exposed employees' probability of working in establishments that are located in safer areas (i.e. locations with lower homicides rates) but at the expense of a significant reduction in

[6]This could be if, for instance, the effects are driven by employees who directly observed the crime scene. Note, however, that there could be additional explanations that might make employees working farther away unresponsive to a homicide but still unwilling to occupy vacancies in establishments closer to the crime scene.

labor earnings. From a policy perspective, these findings provide evidence that, not only ex post costs but also the ex ante costs of crime -such as the labor market distortions presented in this paper- are consequential when designing and evaluating policies that cost-effectively prevent crime.

This paper contributes to a growing body of literature on the effects of crime.[7] In terms of the identification strategy, the most closely related research is Ang (2020) that exploits timing and detailed geographic locations to study how police shootings affect students outcomes. Using different identification strategies, there is research that explored the effects of crime on employees labor market outcomes (Velásquez, 2019; Bindler and Ketel, 2019) and businesses (Braakmann, 2009; Rosenthal and Ross, 2010; Rozo, 2018; Utar, 2018). In addition to confirming some prior findings such as the decrease in employee earnings, I contribute to this literature in the following ways.

First, while previous work studies the effects of crime on employees and firms separately, I am able to simultaneously study labor market demand and supply sides and provide evidence that the effects on workers' earnings are driven by employees' responses to homicides rather than firm labor market responses. From a policy perspective, determining whether it is supply or demand driven is important to target cost measurement efforts and policies to cope with such costs.[8]

Second, this paper provides evidence that not only are there ex post costs of crime but also significant ex ante costs. While citizens can avoid crime by spending on private security, some

[7]Previous research explored the effects on a wide range of outcomes such as education (Brown and Velásquez, 2017; Monteiro and Rocha, 2017; Koppensteiner and Menezes, 2019; Ang, 2020; Michaelsen and Salardi, 2020), health (Cornaglia, Feldman and Leigh, 2014; Dustmann and Fasani, 2016; Koppensteiner and Manacorda, 2016; Currie, Mueller-Smith and Rossin-Slater, 2018; Moya, 2018), economic behavior (Callen et al., 2014; Mejia and Restrepo, 2016; Moya, 2018; Brown et al., 2019), property prices (Gibbons, 2004; Linden and Rockoff, 2008; Pope, 2008; Pope and Pope, 2012; Besley and Mueller, 2012; Ajzenman, Galiani and Seira, 2015), businesses (Braakmann, 2009; Rosenthal and Ross, 2010; Rozo, 2018; Utar, 2018) and employees labor market outcomes (Velásquez, 2019; Bindler and Ketel, 2019)

[8]For example, if the effect on employee earnings is driven by the fact that firms' sales have been negatively affected after a homicide because customers avoid that area, one policy to cope with those effects would imply designing business strategies to improve sales. On the contrary, this paper highlight that the effect on earnings is a cost paid by employees that is driven by their own reactions to homicides.

of its benefits are not internalized, leading to its underprovision (Ayres and Levitt, 1998) and to underestimates of crime avoidance costs. In this paper, I provide evidence on crime avoidance costs that arise through switching to safer areas where citizens may be able to internalize the benefits of avoiding crime, and find that there are significant ex ante costs of crime. Overall, obtaining causal estimates of the costs associated with forward-looking behavior (i.e., costs derived from avoiding future crime) has proven challenging; however, having a comprehensive understanding of all costs of crime is necessary to guide policies to improve citizen security. This paper demonstrates that only focusing on ex post costs of crime leave behind a significant component which are the ex ante costs.

Third, the findings also highlight the importance of labor mobility in avoiding crime. Melnikov, Schmidt-Padilla and Sviatschi (2019) shows that the presence of criminal organizations restricts citizens' labor mobility and their labor market options, which leads to worse socioeconomic conditions. This paper complements that research by showing how employees who were exposed to crime use labor mobility to react to violence exposure and switch to jobs located in safer areas.

This paper is also related to the literature on the effects of crime on housing prices.[9] In the hedonic pricing model (Rosen, 1974), under some conditions, such changes in prices can be interpreted as the average willingness to pay to avoid crime. I contribute to this literature by analyzing a different shock (i.e., crime nearby the workplace) and showing that, even when a change in prices is absent (i.e., firm wages), citizens are still willing to avoid crime, as indicated by the crime avoidance costs incurred by employees who move to lower wage jobs located in safer areas. Overall, this result suggests that only focusing on how prices change after a crime may not entirely capture citizens' willingness to avoid crime.

Finally, this paper speaks to the literature on labor mobility and amenities. The existence of a compensating wage differential has been long analyzed since the work of Roback (1982). This

[9] For example, Gibbons (2004), Linden and Rockoff (2008), Pope (2008), Pope and Pope (2012), Besley and Mueller (2012), Ajzenman, Galiani and Seira (2015)

paper builds upon this literature by providing evidence on one fundamental mechanism behind compensating wage differential models: after an exogenous negative shock that could affect the overall level of amenities (Albouy, Christensen and Sarmiento-Barbieri, 2020), employees are willing to give up wages in exchange of reallocating to lower crime areas.

The remainder of this paper is organized as follows: Section 2.2 describes the background and data, Section 2.3 discusses the identification strategy, Section 2.4 presents estimation results for the main labor market outcomes, Section 2.5 explores mechanisms, Section 2.6 provides a discussion and Section 2.7 concludes.

2.2 Background and Data

2.2.1 São Paulo City

São Paulo City, the largest city in both Brazil and Latin America, is the capital of the surrounding São Paulo State and counts with a population of 12 million people. To provide a notion of the violence level faced by São Paulo City citizens, Figure 2.1 compares the homicide rate of São Paulo City with other cities in the world.

In particular, Panel A shows the homicide rate for the most populated cities in Latin America, while panel B compares São Paulo with the largest cities in the United States. With a homicide rate of 12 murders per 100,000 inhabitants, São Paulo is exactly located at the median of the distribution of the fifteen most populated cities in the region. Naturally, São Paulo's homicide rate is above the average murder rate in the US.[10] However, as panel B illustrates, cities such as Chicago or Philadelphia present higher homicides rates than São Paulo City. Overall, this makes São Paulo a good setting for this research.

The previous comparison between São Paulo and other cities is made for the period of analysis used in this paper; however, the homicide rate dramatically decreased between 2000-

[10]The average homicide rate in the United States for 2012 was five homicides per 100,000 people.

2010. In particular, over the course of that decade, the homicides rate in São Paulo City decreased by 70%.[11] Different hypotheses may explain this sharp decline in murders, and some of them draw attention to the role of the *Primeiro Comando Da Capital* (PCC), Brazil's largest criminal organization.[12] The PCC was formed in the wake of the October 1992 massacre in São Paulo's Carandiru prison, in which Brazilian security forces killed over 100 prisoners following a riot. As opposed to other Latin American cities in which different criminal organizations dispute their territories (Dell, 2015), one of the goals of the PCC was to fight for justice for the massacre through reducing the rivalry between different criminal organizations to fight an common enemy: the police. Since then, the violence of São Paulo was characterized by intermittent periods of truces and tensions between the PCC and the police.

2.2.2 Homicides Data

The data on homicides were obtained from the *Secretaria de Segurança Pública* (SSP). SSP is a secretariat within the government of São Paulo State that gathers information on all homicides in São Paulo State, including São Paulo City.[13] For each homicide, SSP collects information on some features of the incident (for example, whether the homicide was committed by the police) in addition to some demographic data on the victim such as age, gender and race.

More importantly for this analysis, these data includes the date, time and exact location of each homicide that took place in public spaces.[14] For all homicides, regardless of whether it occurred in a public or private location, we count with data on all the above variables; however, in order to protect the identity of the victim and their relatives, SSP do not share the exact location

[11]For example, according to data published by São Paulo's *Secretaria de Segurança Pública*, the total number of homicides in 2001 was 5,463 and 1,497 in 2012.

[12]The name refers to the state capital, São Paulo City. However, the PCC operates through out the entire Brazilian territory, as well as in Peru, Venezuela, Ecuador, Paraguay and Bolivia.

[13]The homicides data is based on the concept of *homicídios dolosos*, which includes all murders that were committed by the human hand and occur due to negligence, regardless of whether the crime was premeditated or not.

[14]Even though this information is available from 2010 it has been consistently collected from 2012 according to SSP.

of homicides that took place in private facilities. Given that the exact location of the homicide is a key element for the identification strategy I use in this paper, I exclude from the analysis those homicides (8%) for which that information is not available.

2.2.3 Employer-Employee Data

The data on workers and firms were obtained from the *Relação Anual de Informações Sociais* (RAIS). These data contains linked employer-employee records collected by the Brazilian Ministry of Labor and Employment (MTE). By Brazilian law, every private or public-sector employer must report this information every year and fines are levied on firms that provide inaccurate information.

At the employee level, the data includes demographic data such as age, gender, ethnicity and education. In addition, the data includes detailed information about the individual's job: occupation, type of labor contract, tenure, wage and hours worked.[15] Even though RAIS is collected annually, it contains data on the exact date of when the employee began and stopped working in a particular establishment. This information, combined with the fact that each employee and establishment are assigned unique administrative identifiers that do not change overtime, allows to study labor mobility using high-frequency data.[16] Another outcome of interest when studying the impact of homicides on labor markets is the probability of being unemployed. Given that Brazil has a large informal sector, periods in which an employee disappears from the RAIS dataset do not necessarily indicate unemployment[17]; however, I am still able to use the data to study the probability of being either unemployed or working in the informal sector.

Lastly, the data includes establishment-level information, including industry information,

[15] Occupational classifications in RAIS follow the *Classificação Brasilieira de Ocupações* (CBO) which contains 2,355 categories.

[16] In particular, I use biannual frequency. Note, however, that for those employees who remain in the same establishment the entire year, the data on wages and hours worked represents a yearly average. For that cases, I assume that those averages are smooth over the year -i,e. same value for the first and second half of the year.

[17] According to the National Household Sample Survey (PNAD), the share of unregistered employees in the entire Brazilian workforce dropped from around 33% in the 1990s to 23% in the 2010's.

legal nature information, whether the business is still active and the date of the shutdown. RAIS does not contain information about where employees live, but does contain data on the addresses of establishments, which allow me to simultaneously study the exposure of firms and their employees to homicides.

2.2.4 Exposure to Homicides

To calculate the exposure to homicides, I geo-coded the location of each homicide and establishment in the above datasets. Figure 2.2 shows the coordinates for both establishment and homicides.[18] A dense number of establishments are located through out São Paulo City. While more relatively scarce and more concentrated in certain areas, there are also homicides throughout the entire territory. Notably, only few neighborhoods had never experienced a homicide during the sample period.

Next, I calculate the distance between firms and homicides coordinates. In my primary specification, I classify employees as exposed if they were working in an establishment situated between 0 and 500 meters (or, equivalently, five blocks) from a homicide. The empirical rationale of choosing this definition of homicides exposure relies on the fact that, as I demonstrate in Section 4.3, the effects of homicides on labor markets fade to zero near this 500 meters mark.

This measure of exposure is highly consistent with recent work by Ang (2020), who studies the effect police shooting in students outcomes classifying as exposed those students who live within half a mile (four blocks) of an incident location. Similarly, as indicated by Ang (2020), Chetty et al. (2018) finds that "a child's immediate surroundings -within about half a mile- are responsible for almost all of the association between children's outcomes and neighborhood characteristics".

Exploiting within neighborhood variation in homicides location implies that these events

[18] Some of these addresses were misspelled. To check the accuracy of the coordinates found, I did a reverse geo-coding -i,e. find the address corresponding to each coordinate. I eliminated from the sample those establishments ($< 5\%$) for which there was a mismatch between the address and coordinate found.

are hyper-local shocks that, as I discuss in Section 3.2, are unanticipated (i.e., they are not predicted by previous events) and do not predict future homicides. This implies that, overall, the level of previous crime exposure and future crime risk between employees who have and have not been exposed to these type of shocks is similar (i.e., neighborhood characteristics are the same) and the only difference relies on their proximity to the crime scene. However, the salience of spatial proximity may still affect both employees and firms in several ways. From employees perspectives, such event near their workplace may affect, for example, their risk attitudes (Callen et al., 2014; Mejia and Restrepo, 2016; Moya, 2018; Brown et al., 2019) and perceptions about future crime (Braakmann, 2012; Mastrorocco and Minale, 2018; Esberg and Mummolo, 2018; Vinæs Larsen and Leth Olsen, 2020), triggering a crime avoidance behavior and changing their employment choices. In a similar way, customers may also change their consumption choices, which can affect firms sales. As a consequence, firms may need to reallocate resources to attract both employees and customers (Besley and Mueller, 2018) or to substitute those workers who leave after a crime (Jäger, 2016).

2.2.5 Sample Selection

Given that RAIS is available until 2018, to be able to analyze labor market outcomes in the long-run (i.e., until five years after the homicide) using a balanced panel, the above definition of exposure is based on those homicides that took place in 2012 and 2013. Therefore, I restrict the employer-employee data for the years 2007 to 2018 (i.e., five years before and after the homicide). In addition, I focus on employees who are between 18 and 65 years old.[19]

Finally, as I exploit hyper-local variation in exposure to crime within neighborhoods, I restrict the sample to those employees working in establishments located within 1 kilometer (or, equivalently, 10 blocks) from a homicide.

[19]To minimize the influence of outliers and obvious measurement error, I also trim observations ($< 1\%$) with very large and unusual earnings.

Overall, my sample contains 263,489 employees working in 33,944 establishments located nearby 2,027 geographic coordinates where the 2012 and 2013 homicides occurred. As I show in Table 2.1, employees in my sample are 31 years old on average. 60% of employees are male and 4% are racial minorities. More than half of the employees in my sample have completed high school as their highest level of education. More importantly, employees in treatment and control group have similar sociodemographic characteristics which suggests that employees working in establishments located between 5 to 10 blocks away is an effective control group.

2.3 Identification Strategy

2.3.1 Difference-in-Differences

The main obstacle to causal identification is that homicides are not random and may be more likely to occur in neighborhoods where both firms and employees face different socioeconomic conditions. To account for this potential endogeneity, I rely on a difference-in-differences approach. This design exploits detailed panel data and compares changes on treatment employees outcomes before and after the exposure of a homicide to changes over time among control workers located farther away. By doing so, this strategy accounts for any level differences that may exist between employees who were exposed to homicides and those who were not. The validity of this design is further bolstered by the data's granularity, which allows me to control for unobserved neighborhood time trends at detailed geographic levels.

In particular, I exploit the timing and location of homicides by estimating the following equation:

$$Y_{ijt} = \delta_i + \lambda_{n,t} + \sum_{\tau \neq -1} \beta_\tau Homicide_{j\tau} + \varepsilon_{ijt} \tag{2.1}$$

where Y_{ijt} represents the outcome for employee i, who works in firm j, in period t. δ_i

and $\lambda_{n,t}$ are individual and neighborhood-time fixed effects, respectively. $Homicide_{j\tau}$ are relative time to treatment indicators that are set to one for treatment employees if t is τ periods from the time of the homicide. In the main specification, the treatment group is composed by employees who work in establishments located between 0 and 500 meters (or, equivalently, 5 blocks) from a homicide whereas establishments located between 500 meters and 1000 meters serve as the control group. Then, the coefficient of interest, β_τ, represents the average change between time τ and the last period before treatment (i,e. the omitted period) among employees exposed to a homicide relative to that same change over time among unexposed employees in the same neighborhood.[20] Standard errors are clustered by zip code.[21]

2.3.2 Threats to Causal Identification

The difference-in-differences approach relies crucially on a parallel trend assumption. Estimates of β_τ for $\tau < 0$ allows me to test for common trends prior to treatment; however, additional threats to causal identification may still be present.

One of these threats would be the existence of unobserved post-treatment shocks that affect treatment and control differently. While it is not possible to address this issue directly, the design takes advantage of the data's granularity which implies that any potential post-treatment shock, in order to bias the results, would have to be hyper-local, differently affecting employees within zero to five and five to ten blocks from a homicide.

Another threat to causal identification would be generated by the auto-correlation structure of homicides. My design exploits homicides' location; thus, if a previous event can predict another one, then homicides are not as good as "random". However, given that I compare employees within zero to five versus five to ten blocks from a homicide, the serial correlation would only

[20]For the 12% of establishments who were exposed to multiple homicides, I define treatment according to the first homicide. In the robustness checks, I show that I find similar results by restricting the sample to those employees who were only exposed to one homicide.

[21]Results are robust to alternative methods of calculating standard errors. See Tables B.6-B.9

be a threat as long as a homicide today predicts another homicide within 5 blocks but does not predict one within 5 to 10 blocks. In other words, in order to bias my results, the auto-correlation structure would have to be different within a 0 to 5 block radius from the location of a previous event relative to a 5 to 10 block radius.[22] I rule out this potential concern by directly exploring the serial correlation structure of homicides. Ideally, we would like to study whether homicides before 2012 predicts homicides in 2012 and 2013 (the ones used to define homicide exposure in this study); however, given that the homicides data begin in 2012, I instead explore whether these events are correlated with future homicides. I conduct two tests to explore this issue. First, I calculate the number of homicides in 2014-2018 within a 0 to 5 block radius from all homicides in the study (2012/2013) and compare it to the number of homicides in a 5 to 10 block radius. As table B.1 illustrates, the difference in the average number of homicides is small (-0.02) and I fail to reject the null hypothesis of no difference with a p value of 0.65. Second, to further explore the auto-correlation, I conduct two autorregressive models (one for the homicides located between 0 to 5 blocks away and another one for the homicides between 5 to 10 blocks). In Figure B.1, I show that the two autorregressive coefficients are small but, more importantly, they are not statistically different from each other.

Finally, Abraham and Sun (2018) shows that, when there is variation in treatment timing and treatment heterogeneity, the coefficient on a lead or lag can be contaminated by effects from other periods. In other words, under treatment heterogeneity, a spurious non-zero lead coefficient can exist even when there is no pre-trend. The authors propose an alternative method in which they estimate the treatment effect for each cohort, and then calculate the average of these cohort-specific estimates, with weights representative of the cohort share. Overall, there is no evidence of treatment heterogeneity contaminating my results.[23]

[22] On the contrary, in wider areas, such as neighborhoods, homicides are likely to be auto-correlated which constitute one of the reasons of why it is necessary to count with their precise location to estimate a causal effect. In the identification strategy described above, the auto-correlation of homicides at the neighborhood level would not constitute a threat to causal identification because it would equally affect treatment and control.

[23] Notice that, given that I focus on those employees who experienced homicides in 2012 or 2013, there are few cohorts in my sample. Consistently, when I employ the interaction-weighted estimator proposed by Abraham and

2.4 Main Results

In this section, I examine the effects of exposure to homicides on employee labor market outcomes by estimating Equation 2.1 on four key outcomes: (a) the probability of being unemployed or working in the informal sector, (b) weekly labor earnings, (c) weekly hours worked and (d) hourly wage. In addition, I show that these results are robust to a host of alternative specifications.

2.4.1 Unemployment/Informal Sector Employment

First, I explore how homicides affect the probability of working in the informal sector or being unemployed. As it was explained in previous sections, RAIS contains the universe of formal workers. However, given that Brazil has a considerable informal sector, periods in which an employee disappears from the RAIS dataset could imply that this individual is unemployed or working in the informal sector.

Estimates of my preferred specification -employing neighborhood-time fixed effects and treatment (control) defined within a 0-5 (5-10) blocks radius from the homicides- are displayed in Figure 2.3 and Column 1 of Table 2.2. I do not find evidence of homicides affecting the probability of working in the informal sector or being unemployed. All coefficients are less than 0.001 in magnitude and do not reach statistical significance.

Having showed that the probability of working in the informal sector is not significantly affected by homicides, in the following subsections, I focus on labor market outcomes *conditional* on being employed in the formal sector.[24]

Sun (2018), I find similar results.

[24]This implies that no information is available on wages and hours worked for the periods when the individual is either unemployed or working in the informal sector. As an alternative approach, I impute zeros when that variables take missing values and get similar results.

2.4.2 Labor Outcomes in the Formal Sector

I present the results for weekly labor earnings in Figure 2.4 and Column 2 of Table 2.2. Prior to the homicide, I find little evidence of differential group trends. For $\tau < 0$, all treatment coefficients are very small in magnitude and never reach statistical significance, even at the 10 percent level. This finding is consistent with exogeneity and unpredictability of these homicides.

After a homicide, weekly labor earnings start to decrease for exposed workers. This effect is persistent over time: five years after a homicide, there still is a negative and statistically significant effect on earnings. In particular, after 5 years, weekly labor earnings fall, on average, by 20 reais, which, relative to a baseline mean of 261 Brazilian Reais, represents a decrease of 7.7 %.

This effect on labor earnings could be either explained by a decrease on hours worked or a decrease on the hourly wage. I estimate the effect of homicides on weekly hours worked and do not find significant results: both pre and post homicide, the coefficients are not statistically significant and very small in magnitude (less than 0.03 hours; see Figure 2.5 and Column 3 of Table 2.2). Overall, these results imply that the negative effect found on earnings is not driven by a decrease in hours worked.

Finally, in Figure 2.6 and Column 4 of Table 2.2, I show the effects of homicides on hourly wage. The pattern is very similar to the effects on labor earnings: there is a negative and persistent effect, even five years after a homicide. Similarly, after 5 years, exposed employees' wages are, on average, 7.6% less than the wages of employees in the control group. This finding explains why labor earnings decrease without any large and significant change on hours worked.

2.4.3 Robustness

In this section, I perform several robustness checks. Tables B.2 - B.5 presents full estimation results for the four main outcome variables using alternative specifications and samples.

In particular, I show that results are robust to the use of alternative time trends (grid-time instead of neighborhood-time) and to the inclusion of time-varying controls (quadratic and cubic terms in age fully interacted with educational attainment).[25] In addition, I employ an alternative sample that excludes multiple treaters (i,e. those employees who are exposed to more than one homicide in our time frame). Tables B.6-B.9 demonstrate that results are robust to the use of alternative standard errors (i.e., multi-way clustering with zip code and time and clustering by grid). In all cases, I obtain similar results with insignificant estimates prior to treatment and significant estimates in the periods following homicides.

Given that the difference-in-differences approach used as the main specification relies on the choice of treatment and control groups, I perform a host of robustness checks. First, I replicate the primary analysis defining exposure at 0-100, 100-200, 200-300 and 400-500 meters (see Figure 2.7). In all cases, the control group remains constant at 500-1000 meters. Comparing results across models, I find that treatment estimates decrease as we get closer to the homicide and, eventually dissipates completely at the 400-500 meters bandwidth. This exercise provides a rationale to the definition of treatment (0-500m) and control (500-1000m) groups used in the main specification. However, as a second robustness check, in Figure 2.8 and Tables B.12-B.15, I employ alternative treatment bandwidths (i.e., 0-300 meters) and alternative control groups (i.e., 600-1000 meters).[26]

In addition, I also employ an alternative identification strategy. While the specification in Equation 2.1 exploits both the timing and location of each homicide, it is also possible to only exploit the timing and use as a counterfactual all employees who have not experienced a homicide yet but will be treated later on (event-study approach). In particular, I estimate:

[25] Because neighborhoods vary in area, I instead sub-divided São Paulo into 1 kilometer-squared grids.

[26] In addition, in Tables B.12-B.15 I show that results are robust to changing both treatment (i.e., 0-300 meters) and control bandwidths (i.e., 600-1000 meters) simultaneously.

$$Y_{ijt} = \sum_{\tau \neq -1} \beta_\tau Homicide_{j\tau} + \alpha X_{i,t} + \lambda_{n,t} + \varepsilon_{ijt} \tag{2.2}$$

where Y_{ijt} represents the outcome for employee i, who works in firm j, in period t. $Homicide_{j\tau}$ are relative time to treatment indicators that are set to one for treatment employees if t is τ periods from the time of the homicide. I define as treated those employees who work in establishments located between 0 and 5 blocks from a homicide. $\lambda_{n,t}$ are neighborhood-time fixed effects and $X_{i,t}$ are a set of time varying controls which includes the age of the employee. In Table B.10 I show that results are also robust to this alternative specification.

2.5 Mechanisms

In the previous sections, I show that homicides have a significant, negative and persistent effect on employees labor earnings and hourly wages. In this section, I explore possible mechanisms behind these results. In particular, I explore the effects of homicides on establishment-level outcomes and employees' labor mobility.

2.5.1 Establishment-Level Outcomes

Homicides can potentially affect firms. For example, establishment sales could be negatively affected if customers avoid areas which have been recently exposed to a homicide. As a consequence, firms might react in several ways, for example, by adjusting the average wage paid to employees, by reducing the number of employees or by shutting down the establishment. If that is the case, firms labor market responses could be the reason why employees exposed to homicides have lower earnings and wages. To explore this potential mechanism, I estimate a version of Equation 2.1 at establishment-level. In particular, I estimate:

$$Y_{jt} = \delta_j + \lambda_{n,t} + \sum_{\tau \neq -1} \beta_\tau Homicide_{j\tau} + \varepsilon_{jt} \tag{2.3}$$

where Y_{jt} represents the outcome for establishment j in period t. δ_j and $\lambda_{n,t}$ are establishment and neighborhood-time fixed effects, respectively. $Homicide_{j\tau}$ are relative time to treatment indicators that are set to one for treatment firms if t is τ periods from the time of the homicide. Similar to Equation 2.1, the treatment group is composed by establishments located between 0 and 5 blocks from a homicide while those located between 5 and 10 blocks serve as the control group. Standard errors are clustered at zip code.[27]

In Figure 2.9 and Table B.11, I show that homicides do not have significant effects on establishments' wage expenditure (Panel A), hourly wage (Panel B), number of employees (Panel C) or the probability of shutting down (Panel D). In all cases, the coefficients are small and do not reach statistical significance.

2.5.2 Labor Mobility Outcomes

Labor mobility is widespread in this setting. For example, on average, employment tenure is about three years. Consistently with that fact, I find that most of the employees (95%) leave the establishment at some point within five years after the homicide and the probability of ever leaving the establishment within that time frame does not change by being exposed to treatment (i.e., eventually, almost all employees leave the establishment).

Consistent with small and insignificant effects on establishments' number of employees, in Figure 2.10 and Table B.16 Column 1, I estimate Equation 2.1 on the probability of switching establishments period by period and find no significant effects. Overall, this implies that employees exposed to homicides and employees in the control group are equally likely to eventually leave the establishment, and at a similar pace (i,e. small and insignificant effects of switching

[27] Results are also robust to clustering at establishment level.

establishments at a given period of time).

The fact that employees who were exposed to a homicide are not more likely to leave the establishment does not imply that the effects found on hourly wage are not driven by mobility-related outcomes. While employees in the treatment and control group might be equally likely to switch establishments, being exposed to a homicide might affect the type of jobs they are switching to, which could explain why we find an effect of homicides on the hourly wage. In the rest of this section, I provide several pieces of evidence that demonstrate how the effects of homicides exposure on certain labor mobility outcomes is, in fact, the main mechanism that explains the results found in employees earnings and wages.

Establishment "Stayers" and "Movers"

First, in Table 2.3, I explore the effects of homicides among those employees who remain at or leave the establishment after the homicide (establishment "stayers" or "movers", respectively). In particular, I estimate Equation 2.1 on hourly wage by replacing time to treatment indicators with a post-treatment dummy and using three samples: all employees (Column 1), employees who leave the establishment after the homicide or "Movers" (Column 2) and employees who remain at the establishment after the homicide or "Stayers" (Column 3).[28]

Consistent with the establishment-level outcomes presented in the previous section, the effects found are completely driven by those employees who leave the establishment (i,e. the effect of homicides on hourly wage among establishment "stayers" has a positive sign and does not reach statistically significance).

[28] Diving the data in these three subsamples could be potentially endogenous; however, in the previous section I showed that being exposed to a homicide have small and insignificant effects on the probability of leaving the establishment (i.e., the probability of being a "mover").

Establishment-Specific Wage Premiums

Second, following a growing body of research originated with Abowd, Kramarz and Margolis (1999) (AKM), I study the role of establishment-specific wage premiums as one possible reason of why establishment movers face a lower hourly wage (see Card et al. (2018) for a recent overview). Specifically, I estimate the following two-way fixed effects model with worker and establishment fixed effects:

$$Y_{ijt} = \alpha_i + \psi_{J(i,t)} + \gamma_t + x'_{i,t}\beta + r_{ijt} \tag{2.4}$$

where Y_{ijt} is the hourly wage of individual i who works in establishment j at time period t. α_i is an individual fixed effect, $\psi_{J(i,t)}$ is an establishment fixed effect, γ_t is a time period indicator, $x'_{i,t}$ are time varying covariates which include quadratic and cubic terms in age fully interacted with educational attainment, and $J(i,t)$ is a function that indicates the establishment individual i is employed at in time period t.

We interpret the establishment effect $\psi_{J(i,t)}$ as a proportional pay premium (or discount) that is paid by establishment j to all employees. As noted by AKM, OLS estimation of Equation 2.4 will only yield unbiased estimates of the establishment wage premiums if the "exogenous mobility" assumption is satisfied. This assumption does not allow workers to sort based on idiosyncratic match effects which implies that firms offer a proportional wage premium to all workers regardless of their skill level and job. Although this might be a restrictive assumption, previous research has shown that it seems to hold in several settings (Card, Heining and Kline, 2013; Song et al., 2019). Importantly for this paper, Alvarez et al. (2018) using the RAIS dataset performs several tests proposed by Card, Heining and Kline (2013) that validates the AKM model assumptions in the context of Brazil.

To study whether the effect of homicides on hourly wage is driven by employees moving to establishments with lower wage premiums, I estimate:

$$\hat{\psi_{J(i,t)}} = \delta_i + \lambda_{n,t} + \sum_{\tau \neq -1} \beta_\tau Homicide_{j\tau} + \varepsilon_{ijt} \tag{2.5}$$

where $\hat{\psi_{j,it}}$ is the estimated establishment-specific wage premium that employee i receives in period t for working at the establishment j. Figure 2.11 and Table B.16 Column 2 display the results.

As it is shown, employees who were exposed to a homicide face a significantly smaller establishment-specific wage premium relative to employees in the control group. In particular, after 5 years, the establishment-specific wage premium for the treatment group decreases, on average, by 38%.

In addition, being exposed to a homicide not only could affect the type of establishments employees switch to, but it may affect the likelihood of being employed in different industries or occupations. Potentially, this could explain the results if, for example, those occupations, establishments or industries pay a lower wage on average. However, Figures B.2 and B.3 and Table B.17 explore this issue and do not find evidence consistent with exposed employees choosing different occupations or industries.

Establishment Location

I also study whether the establishments that exposed employees are switching to are also different in another dimension: their location. In Figure 2.12 and Table B.16 Column 3, I estimate Equation 2.1 on the probability of switching to another municipality (i.e., the probability of leaving São Paulo City).

Two important results can be derived from Figure 2.12. First, employees in the treatment group are considerably more likely to work in an establishment located outside São Paulo City. For example, two and a half years after a homicide the coefficient on the the probability of switching to another municipality is 0.012, which represents an increase of 11% relative to the control group (i,e. the baseline mean is 0.11). Given that exposed employees are more likely to switch to establishments located in other municipalities, another possible reason why employees face lower wages is because those municipalities, on average, provide lower wages than São Paulo City. However, in Figure B.5, I explore this issue by analyzing the average wage in each municipality and I do not find evidence that this is smaller for exposed employees.[29]

Figure 2.12 also demonstrate that the probability of leaving São Paulo increases over time, reaching it maximum around two and a half years after the homicide before decreasing. This result indicates that treated employees switch municipalities at different paces which is consistent with the existence of labor mobility costs.

Finally, the dynamics on labor earnings and hourly wage in which they gradually decrease over time are consistent with the dynamics on the effects of homicides on establishment-specific wage premiums and establishment locations. Overall, the results suggest that after a homicide employees gradually switch to establishments that typically pay a lower wage and are located in other municipalities.

Labor Mobility Timing and Costs

Labor mobility is the main mechanism behind why treated employees suffered a significant wage loss. However, labor mobility takes place gradually over time. In this section, I provide suggestive evidence that certain labor mobility costs prevent employees from switching jobs earlier.

One important labor mobility cost might arise from the fact that, in this setting, it is highly costly for employees to resign from their jobs. Most labor contracts are for indefinite time (95% in this study) which implies that they must be ended either by the employer or the employee.

[29]Note that this result on municipalities wages probably suggest that the average prices in the municipalities they switch to are also similar, implying that the decrease in employees wages are not compensated by a decrease in prices.

However, significant economic gains result when employees wait for their employer to end the labor contract. In particular, employees who resign cannot receive severance pay given by the *Fundo de Garantia do Tempo e Servico* (FGTS). The amount received is formed by monthly deposits made by the employer equivalent to 8% of employee salary. In principle, employees can access these benefits when they are dismissed without cause. However, in this setting there is evidence of fake dismissals (Gonzaga, Maloney and Mizala (2003)) which implies that, in practice, most employees find ways to receive the severance payment on separation, even when they are not dismissed without cause. Overall, this particularity of the Brazilian labor market suggests that employees do not have economic incentives to immediately leave their employer after crime exposure. On the contrary, there are incentives to wait for opportunities to get dismissed without cause (or fake a dismissal) and receive a significant economic compensation. Consistent with that evidence, in my sample, less than 25% of job separations are resignations. This probability is not affected by homicide exposure, which suggests that these costs might be binding and-even though exposed employees might want to leave after a homicide-it is convenient for them to wait until the employer ends their labor contract (see Figure B.4).

Another reason why labor mobility does not take place earlier could be that employees are trying to minimize wage loss. In other words, it might be optimal for the employees to wait for a job offer that does not represent a significant wage loss.

To confirm this idea, I compare the effects on skilled and unskilled workers.[30] In particular, in Figure 2.13 and Table B.18, I compare the probabilities of switching to another municipality for skilled and unskilled workers and find that unskilled workers switch municipalities earlier. This result is consistent with the idea that, given that unskilled workers face on average a lower wage, it takes less time for them to find a job in another municipality relative to skilled workers who would, on average, accept offers with higher wages. In other words, the set of offers that unskilled workers would accept is larger than that for skilled workers; thus, unskilled workers are

[30]The distinction between skilled and unskilled workers is based on educational attainment. In particular, workers whose maximum educational attainment is high school (67%) are classified as unskilled.

more likely to obtain an offer they would accept.

In addition, if switching municipalities is less costly for unskilled workers -which would explain why they switch earlier than skilled workers-, then the wage loss would be lower for this group. In table 2.4, I confirm this idea by showing that the effects on hourly wage are considerably smaller for unskilled workers. Overall, these results suggest that employees might try to minimize wage loss by waiting for a higher wage offer, which occurs first for unskilled workers.

The existence of labor mobility costs not only can explain why employees do not switch establishments earlier but can also provide some suggestive evidence of why employees in the treatment group, relative to control employees, are not more likely to leave the establishment at a given period (i.e., small and insignificant effect on the job separation rate). First, as it was previously mentioned, Brazilian labor markets are characterized by a very high turnover rate (i.e., in my sample, the average job tenure is about three years). Given that workers might have economic incentives to wait for the employer to end their labor contract, exposed and unexposed employees might switch jobs at a similar rate because the decision of leaving the firm would be not be driven (at least, entirely) by the employee but also would depend on the employer's decision (i.e., because employees would have to find opportunities to fake a dismissal or get fired without a cause). Second, it is important to highlight that being exposed to a homicide might affect two margins simultaneously. On the one hand, it would make firm departure more attractive (which tends to increase the departure rate) but, on the other hand, it also affects the type of jobs that employees switch to (i.e. lower establishment-specific wage premiums located in other municipalities). The latter has an ambiguous effect on the departure rate and, more precisely, could have a negative effect if exposed employees require more time to obtain those type of job offers. Overall, these results suggest that, even with a similar job separation rate, labor mobility could still play an important role in explaining the decrease in employees' earnings because exposed employees who leave establishments are more likely to switch to other type of job.

2.5.3 Crime Avoidance

Employees exposed to a homicide are more likely to work in establishments with lower wage premiums which translates into a decrease of almost 8% in their hourly wage. In addition, these establishments are also more likely to be located outside São Paulo City.

There are two possible interpretations of why exposed employees switch to establishments that typically pay lower wages and are located in other municipalities. The first one is that employees may be preventing a future crime exposure (i.e., forward-looking behavior). Exposed employees are not more likely to be exposed to another homicide, relative to employees in the comparison group; however, the salience of the spatial proximity might alter their risk attitudes or perceptions about crime, thus triggering a crime avoidance behavior. However, a second interpretation is that employees may react to homicides because, for example, they dislike working nearby a crime scene (i.e., backward-looking behavior). A forward-looking interpretation would imply that the costs estimated contain an ex ante cost component. From a policy perspective, this result would implicate that, in addition to the ex post costs of crime, it is also crucial to take ex ante costs into account.

In this section, I explore whether crime avoidance is an important mechanism by conducting three exercises. First, I show that treated employees are more likely to switch to establishments that are located farther away from the crime scene. In particular, by calculating distances between São Paulo City and the municipalities that employees are switching to, I compute the probability of switching to an establishment located more than 50 kilometers away (or equivalently, 31 miles). In Figure 2.14 and Table B.19 Column 1, I show that the effects on this probability follows a similar pattern to the probability of switching municipality: it increases over time, reaches its maximum two and a half years after the homicide and then decreases until eventually fades away. In particular, at the maximum, treated employees are 4.4% more likely to switch to establishments located farther away from the crime scene. While the RAIS dataset do not count with employees' addresses, it is not possible to examine whether treated employees are more likely to change their residences. However, this exercise provides some evidence that employees may change residences because the new job locations are far away from their previous establishments.

Second, I study whether the municipalities that employees are switching to have lower homicides rates than São Paulo City. Specifically, I run Equation 2.1 on probability of switching to a municipality with a lower homicide rate than São Paulo. As it is shown in Figure 2.15 and Table B.19 Column 2, employees who were exposed to a homicide are more likely to work in establishments located in municipalities that on average have lower homicide rates. In particular, the effect reaches it maximum two and a half years after the homicide and workers are 5.6% more likely to switch to lower crime municipalities.[31]

Finally, I compare the effects of police and non-police homicides. Most of the violence during the period of this study was related to the rivalry between the PCC and the police.[32] A significant number of deaths occurred among both groups in São Paulo during 2012 and 2013. These homicides were not limited to the neighborhoods where the PCC presence was stronger because many police officers have been killed execution-style while off duty.[33] As a result, many citizens have found themselves in the middle of a shooting and even some citizens have been unintentionally murdered as a result of this confrontation.[34] As opposed to other type of crimes in which citizens can, at some extent, predict whether they are at risk, citizens' homicides that are product of a shooting between the PCC and the police are harder to anticipate and to protect from because they are considered to be "collateral damage".[35] The homicides dataset does not

[31]As a robustness check, I estimate the effect on the probability of switching to municipalities with *higher* homicides rates. I find that all coefficients are small and insignificant, suggesting that the effect found on the probability of switching municipality is driven by switches to safer locations.

[32]See Willis (2015) for a comprehensive description about São Paulo's violent crime and the role of PCC and São Paulo's police.

[33]For example, in one case that has drawn widespread attention, officer Marta da Silva was gunned down in front of her 11-year-old daughter.

[34]See, for example, Kawaguti (2012)

[35]For example, in the case of homicides in the context of property theft, the likelihood of being a victim depend on the value of the asset (Becker, 1968). The higher the value of the property, the higher the risk of being victimized; therefore, citizens can protect themselves of this type of homicides, for example, by hiding the asset. On the contrary, homicides involving the PCC and the police are much harder to anticipate and to protect from because all citizens, regardless of their assets or behavior, have some probability of becoming victims.

specify whether each homicide has been a product of the confrontation between the police and PCC; however, the data do specify whether the homicide was committed by the police.[36]

In Table 2.5, I compare the effects of homicides committed by the police force versus those committed by another citizen. I find that, while both type of homicides have a significant and negative effect on hourly wage, the effects of police killings are much larger.[37] Taken together, these three exercises provide evidence consistent with crime avoidance being an important mechanism behind employees' labor market responses.

Why exposed employees may avoid crime?

Employees exposed to homicides are more likely to move to safer areas at an expense of a significant wage loss. Although exposed employees do not actually face a greater risk than those in the control group, employees could avoid crime if being exposed to a homicide might alter their perceptions or attitudes about future crime.

The existence of an important gap between crime and crime perceptions has been documented for a wide number of countries. Not only there is descriptive evidence on this gap, but also a growing literature studies the determinants of crime misperceptions and shows that they are caused by factors such as the exposure to news media (Mastrorocco and Minale, 2018; Esberg and Mummolo, 2018) and information (Vinæs Larsen and Leth Olsen, 2020). In our setting, a change in crime perceptions can arise if, for example, exposed employees update their priors according to the information available to them (i.e., the homicide) affecting their expectations of future homicides.[38]

In addition, previous research has found a relationship between violence and risk atti-

[36]This information, however, is only available from 2013 onwards.

[37]Ang (2020) studies the effects of police versus non police killings on students outcomes in Los Angeles, and also found larger effects for police homicides that are driven by black and Hispanic minorities. Racial disparities in the police use of force also exist in Brazil; thus, I explore Ang (2020) channel by analyzing the effects by race. However, I do not find that racial minorities are more affected by homicides.

[38]Note that this could be either by the case where (a) there was a crime misperception before the homicide and employees are correctly updating their beliefs after the homicide or (b) their priors were correct, and the homicide exposure provokes a crime misperception.

tudes.[39] In that case, expectations of future crime could be the same (i,e. no changes in crime perceptions); however, if exposed employees' risk attitudes have changed, they might still decide to move to safer areas.

Finally, results can also be consistent with a model of salience in decision making. In that framework, individuals attach disproportionately high weight to salient attributes (Bordalo, Gennaioli and Shleifer, 2013). In our setting, for example, exposed employees may switch to establishments in lower crime municipalities because, after a homicide, they attach disproportionately high weight to alternatives that represent a lower victimization probability.

While my data does not allow me to disentangle whether homicide exposure affects expectations about future crime or risk attitudes or, similarly, whether the results are explained by a model of salience in decision making, by exploring the autocorrelation of homicides within neighborhood, I find that exposed employees do not face a greater risk than those in the control group. This is an important result from a policy perspective because, even though more research is needed, it suggests that being exposed to crime may provoke additional distortions (e.g. crime misperceptions) and, therefore, policy designed to cope with the effects of crime should consider ways to mitigate such distortions.

2.6 Discussion: Compensating Wage Differentials

While firms can be directly affected by homicides (i.e. a "first order" effect), they could also experience a "second order" effect given that the firms' employees are affected by homicides. In particular, after a homicide, not only might current employees want to leave the firm but also potential new ones might find the firm less attractive. However, I do not find a large and significant effect on the job separation rate, which suggest that exposed firms are not significantly more likely to lose employees after a homicide.

[39]See, for example, Callen et al. (2014), Moya (2018) and Brown et al. (2019)

Yet, firms might still find it difficult to attract new workers and fill new vacancies for which they would need to offer a compensating wage differential. However, I do not find a firm labor market response to homicides. One possible reason is that, given that my estimates are locally identified, a firm effect may not be detected if, for example, new employees equally dislike all firms within the wider neighborhood that experienced a homicide.

However, I find that the effects on exposed employees fade away completely at five blocks from the homicide. This result suggests that the salience of the spatial proximity may be driving the results, for example, by those employees who observe the crime scene or recall the violent event every time they walk nearby the place where the homicide took place.[40] In that case, only few potential new employees would find treated establishments unattractive (i.e. only those who were working within a few blocks from the homicide) which would suggest that there is still a thick labor market to fill new vacancies, thus explaining why exposed firms are not significantly affected by homicides.[41]

2.7 Conclusion

This paper estimates crime avoidance costs in the aftermath of homicides that take place near employees workplaces in São Paulo, Brazil. I combine incident-level data on the universe of homicides with a matched employer-employee dataset that contains information on all formal-sector workers. To address the potential endogeneity problem between homicides and labor market outcomes, I rely on a dynamic difference-in-differences design that allows me to exploit hyper-local variation in the location and timing of each homicide and compare changes over time between employees working in establishments located closer and farther away from a homicide but within the same neighborhood.

[40] In fact, Callen et al. (2014) shows that individuals exposed to violence, when primed to recall fear, exhibit an increased preference for certainty.

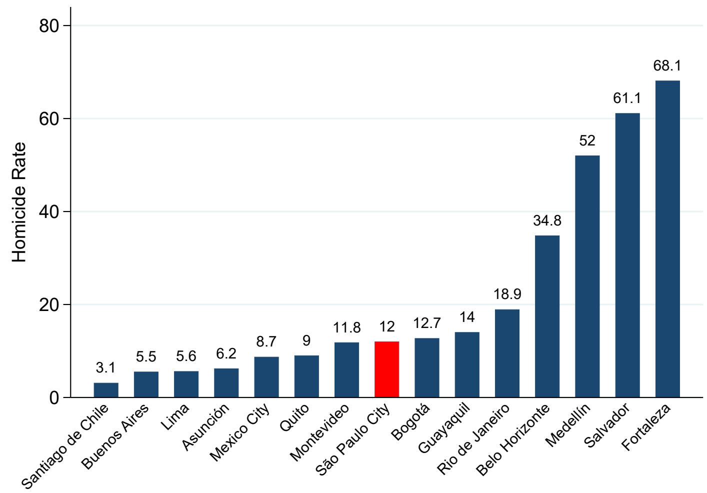
[41] Note, however, that there could be additional explanations that might make employees working farther away unresponsive to a homicide but still unwilling to occupy vacancies in establishments closer to the crime scene.

Relative to others in the same neighborhood, treated employees experience a significant and persistent reduction in labor earnings and hourly wages. These effects can be supply or demand driven; therefore, I study the incidence between firms and employees and do not find evidence of firm labor market responses to homicides. On the contrary, I find that the effects are driven by employees reactions to homicides: exposed employees switch to establishments that typically pay lower wages and are located in other municipalities (i.e., outside São Paulo City).

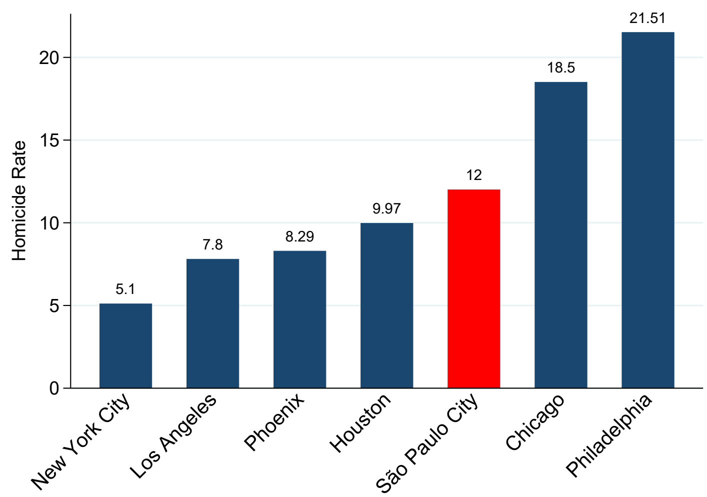
These results are consistent with a forward-looking (crime avoidance) explanation. Although exposed employees, relative to employees in the comparison group, are not more likely to be exposed to another homicide, the salience of the spatial proximity might trigger crime avoidance behavior, for example, by altering risk attitudes or perceptions about crime. I show that treated employees are more likely to switch to establishments located farther away from the crime scene and in lower crime municipalities, consistent with avoiding future crime.

Taken together, these results highlight the importance of crime avoidance costs. Homicides increase employees' probability of moving to establishments that are located in safer areas (i.e. locations with lower homicides rates), but at the expense of a significant reduction in labor earnings. From a policy perspective, these findings demonstrate that, in addition to the costs imposed on victims, crime avoidance costs are consequential when designing and evaluating policies that cost-effectively prevent crime.

2.8 Figures and Tables



Panel A: São Paulo versus Latin American Cities



Panel B: São Paulo versus US Cities

Figure 2.1: São Paulo's Homicides Rate Comparison

Notes: This figure compares the homicide rate of São Paulo City with other cities in the world. Panel A shows the homicide rate for the most populated cities in Latin America and panel B compares São Paulo with the largest cities in the United States. Homicides are calculated as the total number of homicides per 100,000 people in 2012. Data for the Latin American cities were obtained from the Igarapé Institute and data for the US cities were collected from the FBI.

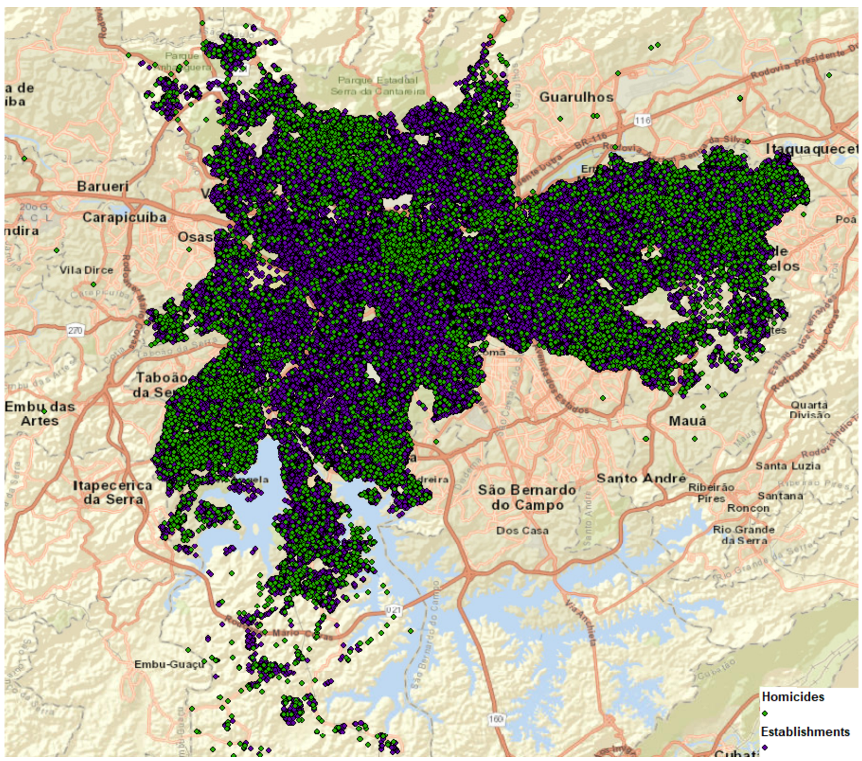


Figure 2.2: Map of Establishments and Homicides

Notes: This figure shows the location of each establishment and homicides in São Paulo City between 2012 and 2018. Green (purple) circles correspond to homicides (establishments).

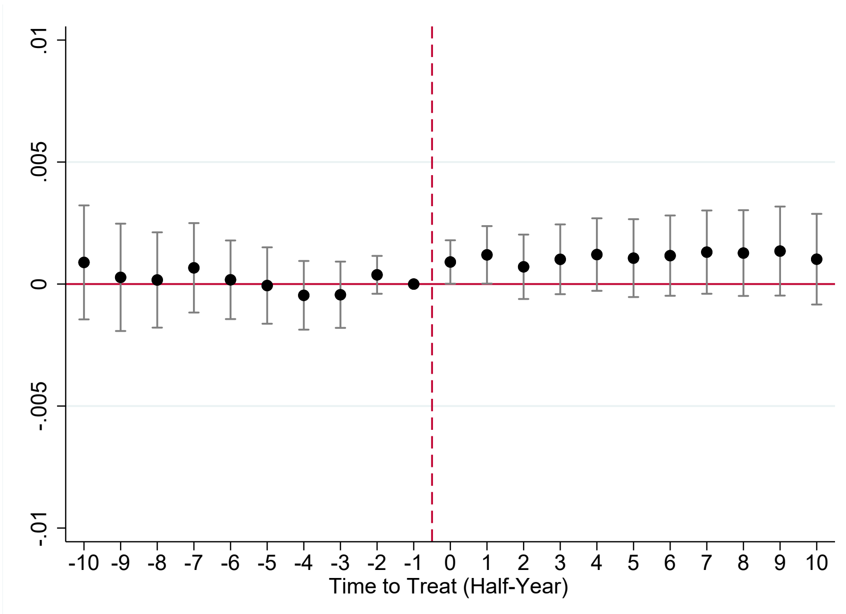


Figure 2.3: Effects on Unemployment/Informal Sector Employment

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of being unemployed or working in the informal sector. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table 2.2 Column 1.

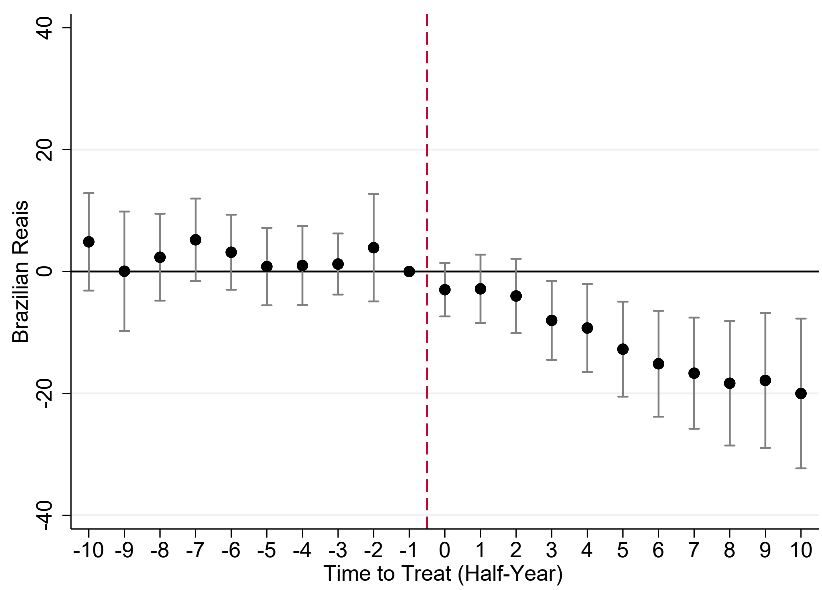


Figure 2.4: Effects on Weekly Labor Earnings

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on weekly labor earnings. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table 2.2 Column 2.

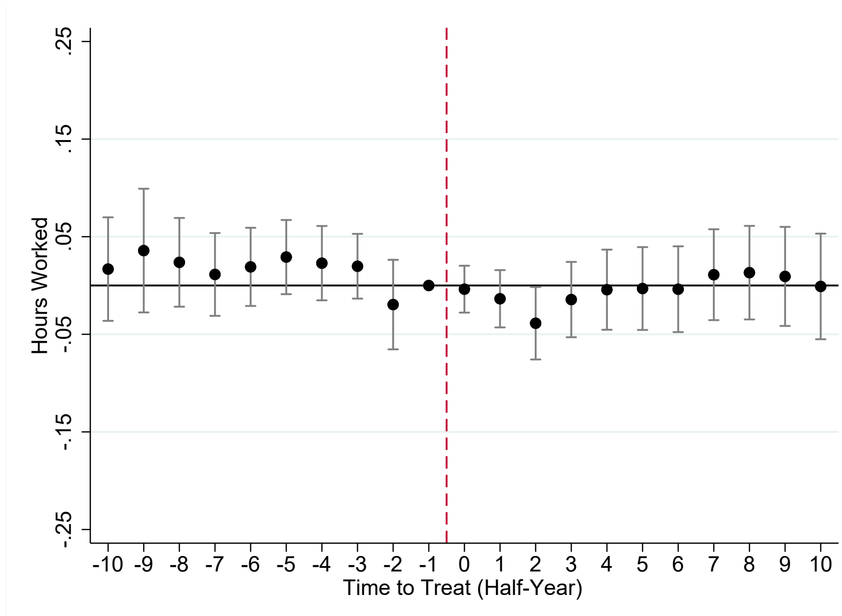


Figure 2.5: Effects on Weekly Hours Worked

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on weekly hours worked. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table 2.2 Column 3.

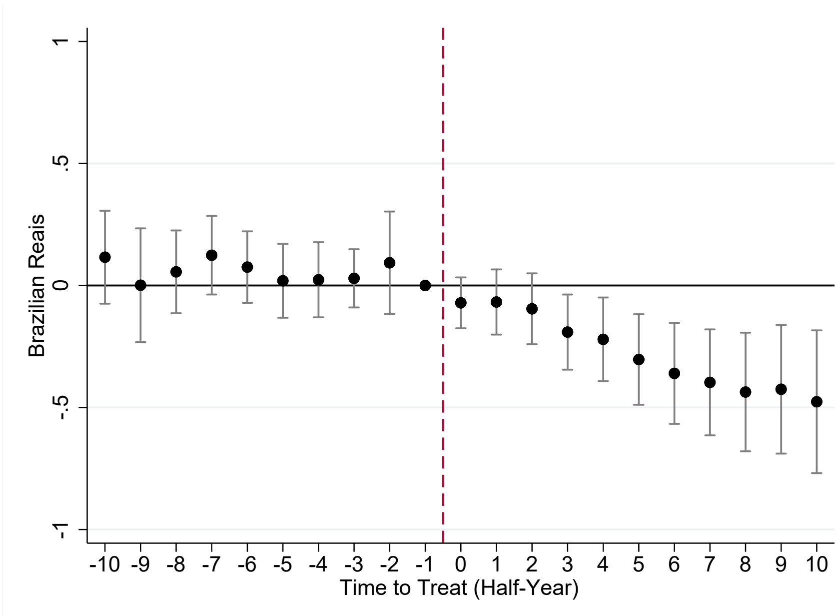


Figure 2.6: Effects on Hourly Wage

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on hourly wage. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table 2.2 Column 4.

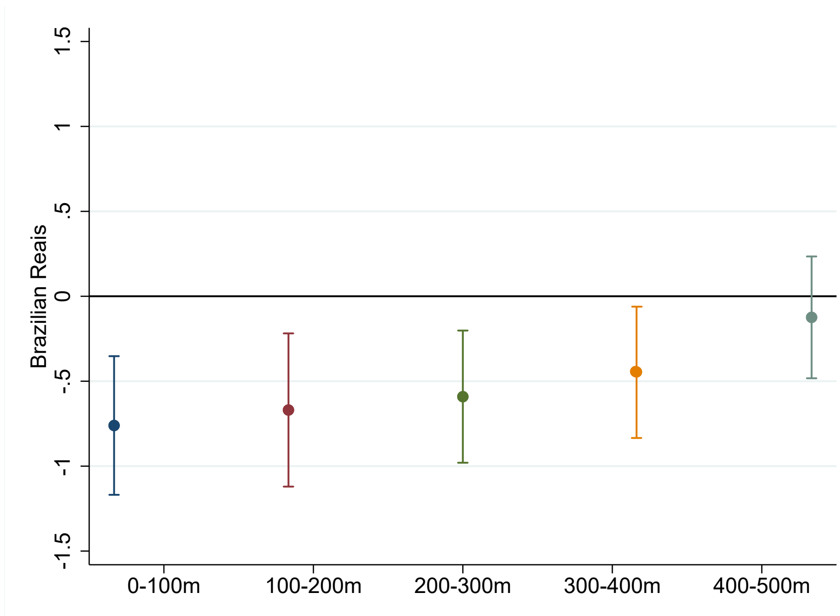
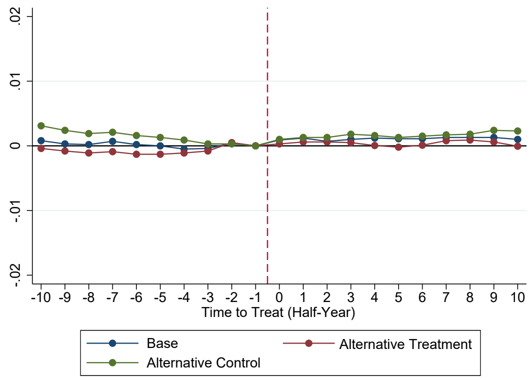
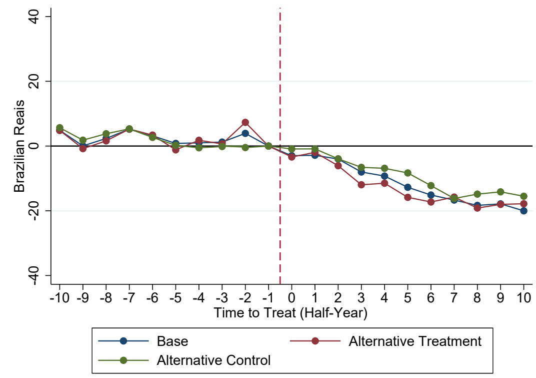


Figure 2.7: Effects on Hourly Wage by Distance

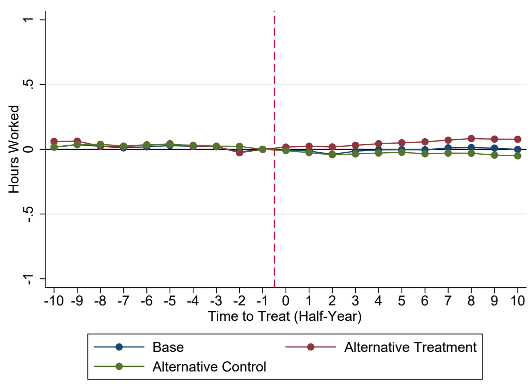
Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on hourly wage, replacing time to treatment indicators with a post-treatment dummy based on every 100 meters (0-100, 100-200 etc). In all cases, control group defined as employees working in an establishment within 500-1000 meters from a homicide. Standard errors clustered by zip code.



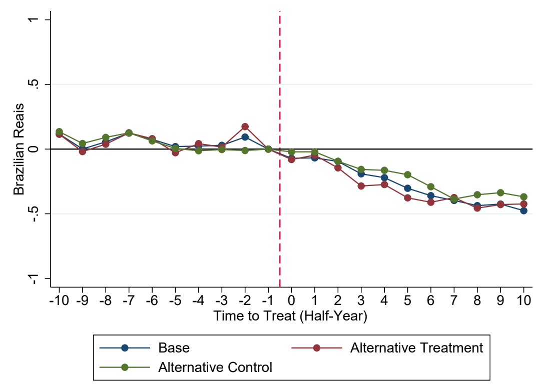
Panel A: Unemployment/Informal Sector



Panel B: Weekly Labor Earnings



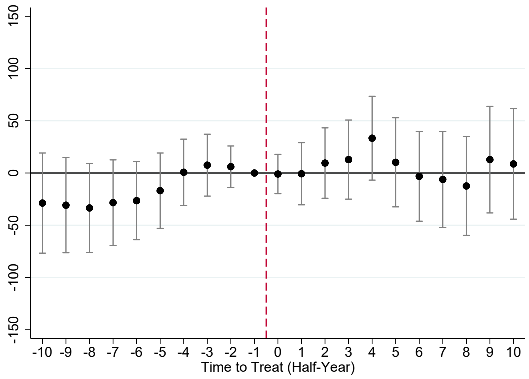
Panel C: Weekly Hours Worked



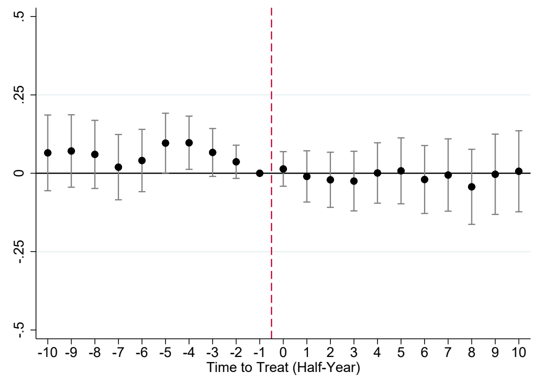
Panel D: Hourly Wage

Figure 2.8: Alternative Treatment and Control Groups

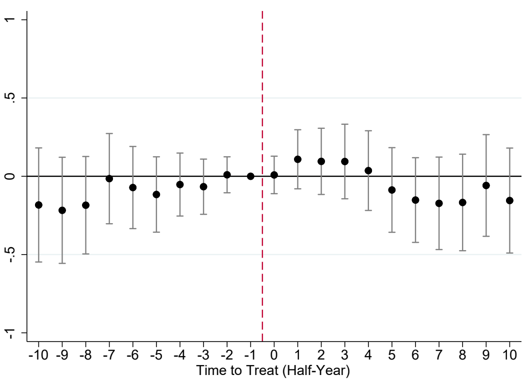
Notes: Graph shows DD coefficients from estimation of Equation 2.1 on the probability of being unemployed or working in the informal sector (Panel A), weekly labor earnings (Panel B), weekly hours worked (Panel C) and hourly wage (Panel D) using an alternative treatment (0-300m) and control group (600-1000m). Full estimation results displayed in Tables B.12-B.15.



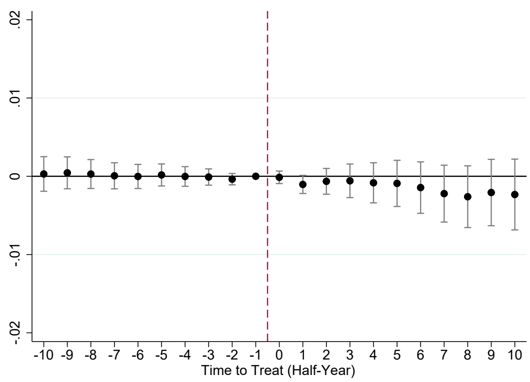
Panel A: Wage Expenditure



Panel B: Hourly Wage



Panel C: Number of Employees



Panel D: Probability of Shutdown

Figure 2.9: Effects on Establishment-Level Outcomes

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.3 on establishment-level outcomes. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table B.11.

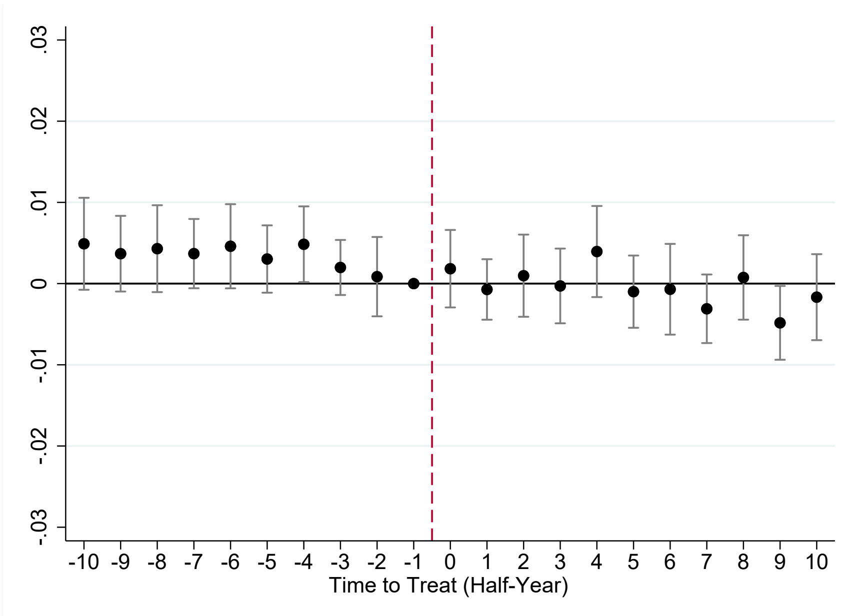


Figure 2.10: Effects on the Probability of Switching Establishments

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of switching establishments. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table B.16 Column 1.

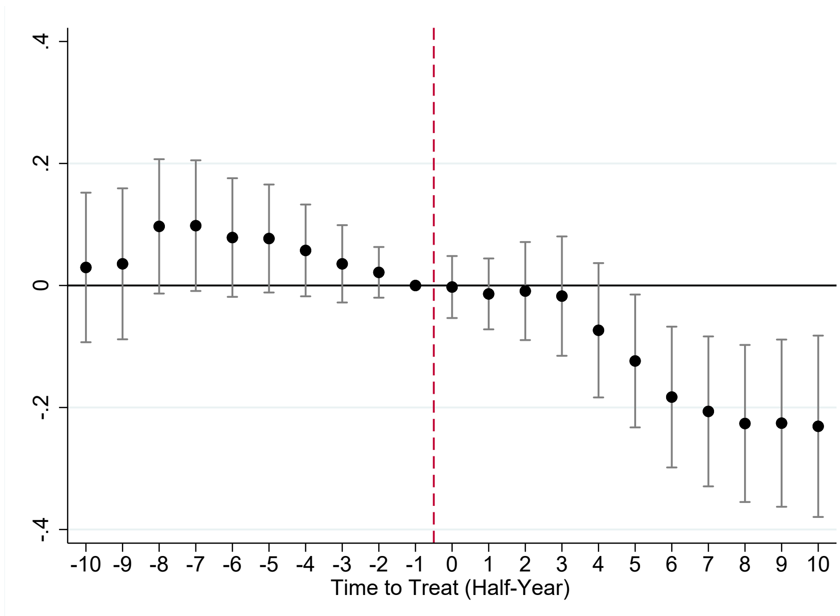


Figure 2.11: Effects on the Establishment-Specific Wage Premiums

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.5. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table B.16 Column 2.

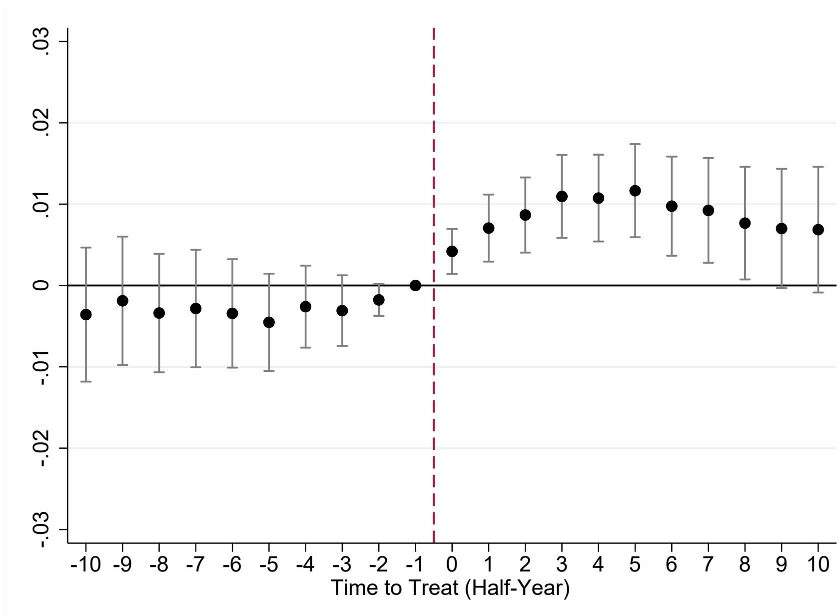


Figure 2.12: Effects on the Probability of Switching Municipality

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of switching municipality. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table B.16 Column 3.

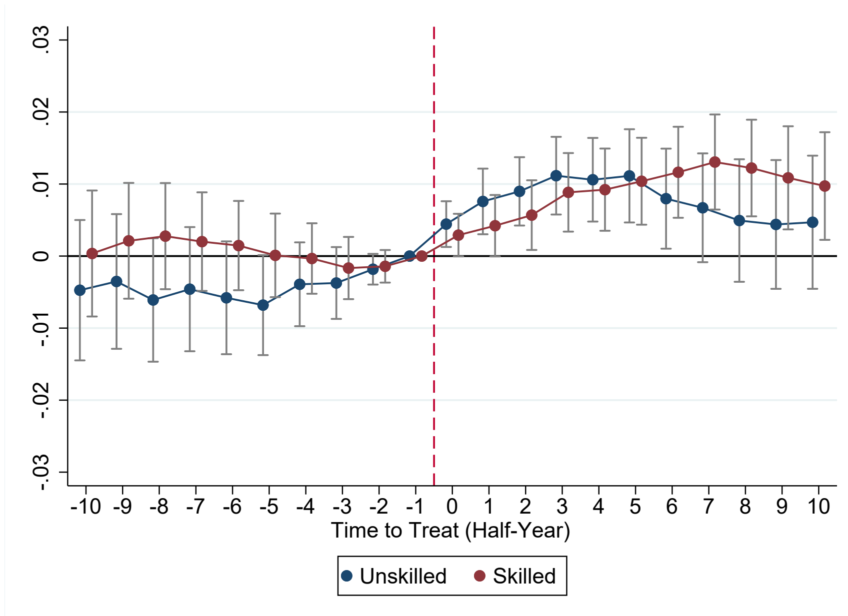


Figure 2.13: Effects on Switching Municipality: Skilled and Unskilled Workers

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of switching municipality for skilled and unskilled workers. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table B.18

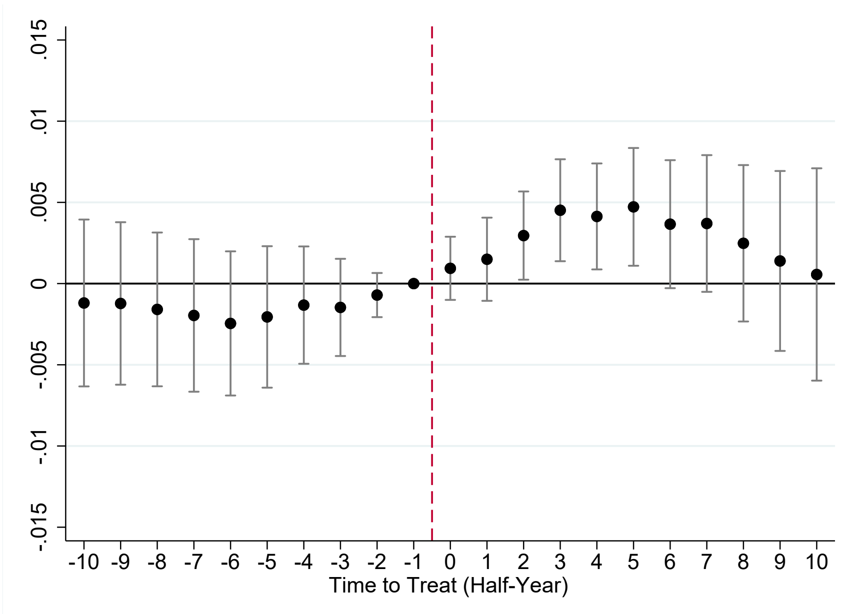


Figure 2.14: Effects on Switching to Farther Away Municipalities

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of switching to establishments located in municipalities farther away from São Paulo (more than 50kms). Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table B.19 Column 1.

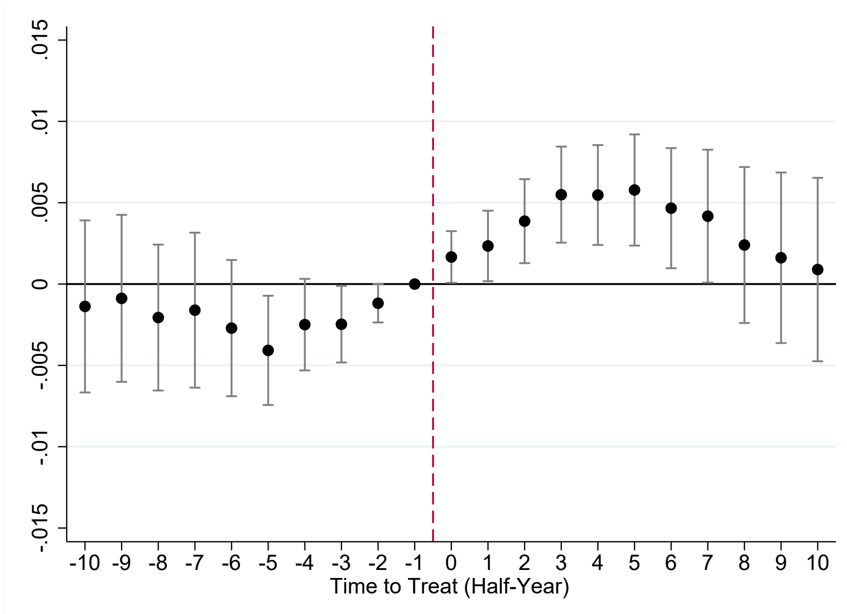


Figure 2.15: Effects on Switching to Lower Crime Municipalities

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of switching to establsihments located in municipalities with a lower homicide rate than São Paulo. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment. Full estimation results displayed in Table B.19 Column 2.

Table 2.1: Summary Statistics

	Treatment 0-5 blocks	Control 5-10 blocks
Age (years)	31.6	31.5
Gender (=1 if men)	0.59	0.61
Racial Minority	0.04	0.04
Education (Highest Level)		
Less than High School	0.12	0.1
High School	0.56	0.57
More than High School	0.32	0.33
Employees	65,873	197,616

Notes: This table provides summary statistics for the employee sample, disaggregated by those who were working in an establishment near/far from a homicide in 2012-2013. Racial minority is a dummy variable that takes one for employees who are black, asian or indigenous.

Table 2.2: The effects of Homicides: Main Results

Time to Treat	Informal Sector / Unemployed (1)	Weekly Labor Earnings (2)	Hours Worked (3)	Hourly Wage (4)
-10	0.0008	4.870	0.0168	0.116
	(0.0012)	(4.080)	(0.0271)	(0.0971)
-9	0.0003	0.043	0.0358	0.0010
	(0.0011)	(4.997)	(0.0323)	(0.119)
-8	0.0002	2.347	0.0237	0.0559
	(0.0009)	(3.636)	(0.0232)	(0.0866)
-7	0.0007	5.208	0.0113	0.124
	(0.0009)	(3.448)	(0.0216)	(0.0821)
-6	0.0002	3.166	0.0191	0.0754
	(0.0008)	(3.140)	(0.0204)	(0.0748)
-5	0.0000	0.811	0.0291	0.0193
	(0.0007)	(3.243)	(0.0194)	(0.0772)
-4	-0.0005	0.987	0.0229	0.0235
	(0.0007)	(3.299)	(0.0194)	(0.0786)
-3	-0.0004	1.231	0.0197	0.0293
	(0.0007)	(2.558)	(0.0169)	(0.0609)
-2	0.0004	3.913	-0.0196	0.0932
	(0.0004)	(4.501)	(0.0234)	(0.107)
-1	-	-	-	-
	-	-	-	-
0	0.0009	-2.987	-0.0038	-0.0711
	(0.0005)	(2.234)	(0.0122)	(0.0532)
1	0.0012	-2.842	-0.0136	-0.0677
	(0.0006)	(2.861)	(0.0150)	(0.0681)
2	0.0007	-4.013	-0.0387	-0.0955
	(0.0007)	(3.114)	(0.0189)	(0.0741)
3	0.0010	-8.018	-0.0144	-0.191
	(0.0007)	(3.300)	(0.0197)	(0.0786)
4	0.0012	-9.269	-0.0043	-0.221
	(0.0008)	(3.675)	(0.0209)	(0.0875)
5	0.0011	-12.74	-0.0031	-0.303
	(0.0008)	(3.974)	(0.0216)	(0.0946)
6	0.0011	-15.13	-0.0038	-0.36
	(0.0008)	(4.432)	(0.0224)	(0.106)
7	0.0013	-16.68	0.0111	-0.397
	(0.0008)	(4.652)	(0.0237)	(0.111)
8	0.0013	-18.33	0.0131	-0.437
	(0.0009)	(5.210)	(0.0244)	(0.124)
9	0.0013	-17.86	0.0093	-0.425
	(0.0009)	(5.648)	(0.0259)	(0.134)
10	0.0010	-20.01	-0.0009	-0.476
	(0.0009)	(6.268)	(0.0276)	(0.149)
Baseline Mean	0.29	261.4	42	6.3
Obs	5,269,780	3,746,367	3,746,367	3,746,367
R-Sq	0.923	0.540	0.648	0.540

Notes: DD coefficients from estimation of Equation 2.1 on the probability of being unemployed or working in the informal sector (Column 1), weekly labor earnings (Column 2), weekly hours worked (Column 3) and hourly wage (Column 4). Columns 2 and 4 expressed in Brazilian Reais. Standard errors clustered by zip code.

Table 2.3: Hourly Wage: "Stayers" vs "Movers"

	All (1)	Movers (2)	Stayers (3)
Exposure	-0.261 (0.076)	-0.380 (0.076)	0.204 (0.187)
Obs	3,746,367	3,548,800	197,567
R-Sq	0.538	0.534	0.728

Notes: DD coefficients from estimation of Equation 2.1 on hourly wage, replacing time to treatment indicators with a post-treatment dummy ("Exposure") and using three samples: all employees (Column 1), employees who leave the establishment after the homicide or "Movers" (Column 2) and employees who remain at the establishment after the homicide or "Stayers" (Column 3). Standard errors clustered by zip code.

Table 2.4: Hourly Wage: Unskilled vs Skilled Workers

	All (1)	Unskilled (2)	Skilled (3)
Exposure	-0.261 (0.076)	-0.070 (0.038)	-0.686 (0.133)
Baseline Mean	6.3	5.3	9.4
Obs	3,746,367	2,510,065	1,236,302
R-Sq	0.538	0.564	0.528

Notes: DD coefficients from estimation of Equation 2.1 on hourly wage, replacing time to treatment indicators with a post-treatment dummy ("Exposure") and using three samples: all employees (Column 1), unskilled (Column 2) and skilled workers (Column 3). Standard errors clustered by zip code.

Table 2.5: Hourly Wage: Police vs Non-Police Homicides

	All	Police	Non-Police
	(1)	(2)	(3)
Exposure	-0.261	-1.71	-0.319
	(0.076)	(0.630)	(0.123)
Obs	3,746,367	227,904	1,167,634
R-Sq	0.538	0.644	0.567

Notes: DD coefficients from estimation of Equation 2.1 on hourly wage, replacing time to treatment indicators with a post-treatment dummy ("Exposure") and using three type of homicides: all homicides (Column 1), Police Homicides (Column 2) and Non-Police Homicides (Column 3). Column 1 uses all homicides in 2012 and 2013. Column 2 and 3 use data for which this information (police vs non-police) is available (2013). Standard errors clustered by zip code.

Chapter 3

Political Alignment, Bureaucratic Corruption and Disclosure Laws: Evidence for the Police Force

This paper employs a close elections regression discontinuity design to study how political alignment affects the income and assets police officers disclose. Police officers in aligned municipalities report to have 5% more total income and 52% more net assets. The effects of political alignment are greater for nonadministrative police officers, those with higher tenure and those who work in higher crime areas. Taken together, these results are consistent with a corruption-based explanation, either by an increase in extracted rents or a decrease in corrupt bureaucrats' misreporting (i.e., through an effect in the financial disclosure law's enforcement).

3.1 Introduction

Bureaucratic corruption and its pernicious economic effects have been well documented across the developing world (Olken and Pande, 2012; Svensson, 2005). While scholars and policy makers agree about the importance of reducing corruption, there is less agreement about which policies are the most effective to achieve that goal. Bureaucrat's accountability and combatting corruption through punishments, rewards or monitoring all rely on information (Djankov et al., 2010; Gans-Morse et al., 2018). As a result, many governments and international agencies have made efforts in recent years to improve transparency through freedom of information (FOI) and financial disclosure laws.

Income and asset disclosures may point out discrepancies or outside conflicts and shed light on bureaucrats' misconduct (Di Tella and Weinschelbaum, 2008). Once these inconsistencies are identified, other mechanisms of accountability, such as law enforcement or media exposure may come into play (Djankov et al., 2010). Disclosure laws may potentially serve as a corruption-controlling device; however, their effectiveness ultimately rests on the information they provide. Therefore, understanding which factors affect the information these transparency initiatives provide is crucial to determine how these policies may serve to fight corruption.

In this paper, I ask how political-party heterogeneity affects police officers' disclosure of their income and assets in the context of a recent financial disclosure law in Buenos Aires Province, Argentina. While police officers are hired by the provincial government, in practice, they work in a particular municipality and interact with politicians at both provincial and municipal levels. Therefore, there are several ways in which political alignment could affect police officers' disclosure of income and assets. One possibility is that political alignment may facilitate the implementation and enforcement of the financial disclosure law, therefore increasing the compliance rate and accuracy of the information provided. Another possibility is that political party heterogeneity affects the rents extracted by corruption itself. For instance, politicians of the same political party might team-up and combine resources to monitor the bureaucrats. In addition, there could be stronger incentives to fight corruption in aligned regimes because only one party takes responsibility for the outcomes while it could be harder to identify which political party is responsible for high levels of corruption in unaligned regimes. However, political alignment could also have the opposite effect. A possible reason could be that corrupt bureaucrats might need to reach an agreement with politicians from different levels of government to extract rents. Therefore, reaching an agreement with politicians from the same party could be more straightforward than bargaining with politicians from several political parties. In other words, political party heterogeneity might be an obstacle to bureaucratic corruption.

To answer how political alignment between provincial and municipal levels of government affects police officers' financial disclosure, I construct a novel dataset which combines electoral results from Buenos Aires Province' municipalities with information from a recent disclosure law which allows me to collect detailed data on police officers' income, assets and liabilities. Then, I rely on a close elections regression discontinuity design to address the potential endogeneity between political-party heterogeneity and income and assets disclosure.

My results suggest that political alignment has a positive and significant effect on the income and assets police officers report. In particular, police officers in aligned municipalities report to have 5% more total income and 52% more net assets. The effect on total income is driven by the income obtained by alternative sources rather than by an effect on the income obtained by their main occupation (specifically, police officers in aligned municipalities have 42% more income from other sources that those in unaligned municipalities). In addition, the effect on net assets is driven by an affect on total assets rather than an effect on debts. In particular, police officers in aligned municipalities have 36% more total assets which is mostly explained by an effect on movable and immovable assets rather than financial assets.

There are several reasons why police officers in aligned municipalities may report having more income and assets. For example, previous research suggests that political alignment may have an impact on economic growth (Asher and Novosad, 2017) or tax evasion (Cullen, Turner and Washington, 2018) which could potentially explain these results. However, another explanation may be based on police corruption, either through an increase on the extracted rents or through a decrease in corrupt police officers misreporting income and assets (i.e., political alignment affects the enforcement of anti-corruption devices such as this financial disclosure law). Thus, I conduct several heterogeneity exercises to understand whether my findings are consistent with a corruption-based explanation. First, I compare administrative versus nonadministrative police officers. Administrative police officers experience much better monitoring and have fewer opportunities to extract rents from corruption. Therefore, if these results are consistent with a corruption-based explanation, we would expect a larger effect of political alignment on nonadministrative police officers rather than on administrative ones. While there is a positive and significant effect on nonadministrative police officers, I fail to reject a null effect of political alignment on administrative police officers. In addition, I compare the effects of political alignment between police officers with high versus low tenure and those who work in high versus low crime areas. The results are greater for police officers with higher tenure and for police officers who work in high crime areas. Taken together, these results are consistent with a corruption-based explanation, as opposed to political alignment affecting local economic growth or tax compliance.

Either by an increase in the extracted rents or through a decrease in income and assets misreporting (i.e., through an effect in the financial disclosure law's enforcement), the financial disclosure law provides evidence that political alignment plays an important role in understanding bureaucratic corruption. My findings have two main policy implications. First, they highlight the importance of transparency initiatives in providing information that could be crucial to deter corruption. Second, given that the interaction between politicians and bureaucrats seems to play an important role, these findings suggest that policies that aim to reduce bureaucratic corruption which merely focus on bureaucrats' incentives might not be as effective as policies that target both bureaucrats' and politicians' incentives.

This paper makes three main contributions to the literature. First, it contributes to literature on bureaucratic corruption which suggests that incentives such as wages and future rents (Di Tella and Schargrodsky, 2003; Niehaus and Sukhtankar, 2013) and monitoring devices (Nagin et al., 2002; Olken, 2007) play an important role in reducing corruption. I make a contribution to this literature by providing evidence that the interaction between politicians and bureaucrats plays an important role and, therefore, effective anti-corruption policies should target both bureaucrats and the politicians they interact with.

Second, this paper contributes to the extensive literature on political alignment, which points out that political alignment affects intergovernmental transfers (Ansolabehere and Snyder Jr, 2006; Brollo and Nannicini, 2012), tax evasion (Cullen, Turner and Washington, 2018), public sector services (Callen, Gulzar and Rezaee, 2020) and economic growth overall (Asher and Novosad, 2017). This paper contributes to that literature by showing that political alignment has an impact on corruption either through an effect in financial disclosure law's enforcement or an increase in extracted rents.

Finally, this paper also contributes to a relatively recent literature which studies disclosure laws. This paper highlights the importance of these laws in providing researchers with new available tools to study corruption (Djankov et al., 2010; Fisman, Schulz and Vig, 2014, 2016) and policy makers with information that could be useful to design policies that aim to reduce corruption.

The rest of the paper is organized as follows. Section 3.2 explains the setting and the data while Section 3.3 discuss the empirical strategy. Section 3.4 presents the main results and Section 3.5 concludes.

3.2 Setting and Data

Buenos Aires Province is Argentina's largest province. Divided into 135 municipalities, the area has a total population of 17 million (approximately 38% of Argentina's total population). The Police Department of Buenos Aires Province (PBA), formed in 1880, has 150,000 employees and constitutes one of the largest police forces in Argentina. In 150 year history, the PBA has experienced several scandals of corruption, which is why it is known as one of the most corrupt bureaucracies in Argentina. For example, many police officers have been involved in serious crimes such as drug and weapons trafficking.

Police officers are hired by the provincial government but in practice they are assigned to a police station in a particular municipality; therefore, they interact with politicians at both provincial and municipal levels. However, all decisions related to the police (e.g., wages, hiring and firing decisions) are made at provincial level. In particular, police officers within the same police rank face the exact same wage, regardless of which municipality they work in.

The data I use relies on two different sources. The first data source comes from both provincial (governor) and municipal (mayor) elections, which took place in October 2015 across Buenos Aires Province's 135 municipalities. I collected information on the governor's political party from the provincial elections, and information on the political party of the winner, opposition and differences in the vote shares from the municipal elections.

The second data source comes from a recent anti-corruption law, which established that some public sector employees must report their income and assets and that information must be publicly available. According to this law, in 2017, approximately 15,000 police officers had to provide detailed information about their income, assets, and liabilities. In addition to their own income and assets, police officers must report their spouses' and dependent family members' incomes and assets. This requirement prevents the simple concealment of assets by putting them under the names of immediate family members. In particular, police officers must report they income obtain from their main occupation (i.e., police officer), their income from other sources (i.e., a second occupation, their spouse and dependent's income, etc.), movable and immovable assets, financial assets and debts.[1]

The disclosure law's compliance rate was very high (around 95%). According to the information provided by the Auditoria de Asuntos Internos, an anti-corruption office in charge of

[1] All these variables are expressed in Argentinian pesos. The exchange rate in 2017 was 1 USD = 17 pesos

reviewing and analyzing the disclosure forms, there are two main reasons why police officers are incentivized to comply with this law. First, this office is in charge of cross-checking the information police officers report in the disclosure forms with official records (provided by the tax agency, automobile registry, etc.). Once they find a discrepancy between what is reported and the official records (or, directly, when police officers do not comply with filing the disclosure form), this office starts an investigation and the police officer is not allowed to work until the investigation is complete. In addition, according to this office, another reason why incentives to comply with the financial disclosure were high came from the fact that, in practice, it is really difficult to prove that income and assets came from illegal sources (i.e., corruption). Consistent with the information provided by this office, police officers, besides complying with filling out the disclosure forms, in some cases, reported having extraordinary amounts of income and assets.[2]

3.3 Empirical Strategy

3.3.1 Estimation

The identification strategy relies on a Sharp Regression Discontinuity Design:

$$Y_{im} = \beta_0 + \beta_1 \mathbf{1}(Margin_m > 0) + \beta_2 Margin_m + \beta_3 Margin_m \times \mathbf{1}(Margin_m > 0) + \varepsilon_{im}$$

where *Margin* is defined as the difference between the vote share of the political party ruling the Provincial Government and the opposition's vote share:

$$Margin_m = \frac{v_m^r - v_m^o}{v_m^{tot}}$$

Therefore, the municipal and provincial levels are politically aligned when the variable *Margin* takes positive values. This specification includes separate linear trends at both sides of the

[2]For example, a case appeared on the news in which a police officer reported to have a helicopter. See, for instance, "The "millionaire policeman", owner of helicopters and more than a hundred properties", La Nacion, March 19, 2018.

discontinuity and clustered standard errors at the municipality level.[3] The dependent variable, Y_{im}, is the reported income, assets and liabilities. Therefore, β_1 identifies the causal effect of aligned municipalities.

3.3.2 Balance Tests

The identifying assumption of the regression discontinuity is that police officers and municipalities where the ruling party candidate barely wins have similar unobservable characteristics to municipalities where the ruling party candidate barely loses. According to recent work by Grimmer et al. (2011), candidates who enjoy structural advantages in US elections disproportionately win elections that are very close. This would violate the identifying assumptions if, for example, powerful parties manipulated specific close elections, based on unobserved characteristics. Eggers et al. (2015) finds that Grimmer et al. (2011)'s results are an exception and that most U.S. elections in fact support the identifying assumption. Nevertheless, I perform tests to demonstrate that these types of advantages do not drive the outcomes of close elections in Buenos Aires Province.

I test for continuity of all baseline covariates around the treatment threshold, as well as the density of the running variable. The McCrary test of continuity in the density of the running variable around the treatment threshold of zero (McCrary, 2008) does not reject continuity in the running variable at the win/loss threshold, indicating that candidates do not have the ability to selectively push themselves across the win margin.[4]

Figure 3.1 shows that we reject a discontinuity in some observable characteristics of police officers such as sex, age, tenure, and a dummy, which indicates whether they are chiefs. In addition, we also reject the discontinuity in some municipalities' characteristics such as population, area, the number of police stations and total crimes per capita. Consistent with Figure

[3]To see robustness to other specifications, refer to section 3.4.3
[4]The point estimate for the discontinuity is 0.02, with a standard error of 0.09

3.1, Table 3.1 presents the regression analysis and shows that there is no noticeable difference between municipalities narrowly won and narrowly lost by ruling party candidates.

3.4 Results

3.4.1 Income

Figure 3.2 presents the effect of political alignment on total income and its two components: income from their main occupation and income from other sources. I find that political alignment has a positive and significant effect on total income, in particular, driven by the income obtained by other sources. In other words, the coefficient that captures the effect of political alignment on the income police officers obtained from their main occupation is small and not significant. This result is consistent with what was explained in the above sections: police officers within the same police rank face the exact same wage, regardless of which municipality they work in.

Table 3.2 provides the regression analysis, which confirms the evidence we obtained from the graphical results. Police officers in aligned municipalities have 5.2% more total income. This effect is driven by an effect on the income they obtained from other sources: while the effect on their income from their main occupation is small and not significant, police officers in aligned municipalities have 42.5% more income from other sources.

3.4.2 Net Assets

Figure 3.3 and Table 3.3 present the effect of political alignment on net assets and its two components: total assets and liabilities. Political alignment has a positive and significant effect on net assets. In particular, police officers in aligned municipalities have 51.7% more net assets.[5]

[5]While this increase in a two-year period might appear quite significant, it is important to highlight that police officer's net assets are low to start with. In particular, police officers' net assets are around 230,000 Argentinian

A positive effect on net assets could be driven by an increase on net assets, a decrease in debts, or a combination of both. In this case, the effect on net assets is completely driven by an increase in total assets (36% more total assets than police officers in unaligned municipalities) rather than a decrease in liabilities (i.e., the effect on debts is small and not significant).

Given that total assets include a combination of physical (movable and immovable) and financial assets, I then proceed to analyze which kind of assets political alignment has an effect on. As Figure 3.4 and Table 3.4 show, the effect on total assets is driven by physical assets (both movable and immovable) rather than financial assets.

3.4.3 Robustness of the Results

In this section, I explore the robustness of the results found in the previous section. In particular, I consider specifications without police fixed effects, quadratic trends, and a smaller bandwidth (5% win margin). Table 3.5 shows that the results are robust to these alternative specifications.

3.4.4 Interpretation of the Results

In the previous sections, I find that police officers in aligned municipalities report having more income and assets than police officers in unaligned municipalities. There could be several explanations behind these results.

First, it could be that police officers in aligned municipalities are compensated for their work with a higher wage than police officers in unaligned municipalities. However, by law, all police officers within the same police rank, given that they are hired by the provincial government, should earn the exact same wage, regardless of which municipality they work in. Consistent with this law, I do not find that political alignment has an effect on the income police officers obtain from their main occupation (i.e., as police officers).

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pesos on average, equivalent to approximately 13,000 U.S. dollars.

Second, it could be that these results are driven by economic growth. In particular, Asher and Novosad (2017) shows that political alignment has an effect on economic growth. If that is also the case in this setting, and police officers in aligned municipalities experience better economic conditions, that could be a reason why they report having more income and assets (for example, spouses might be able to find higher paid jobs in aligned municipalities).

Third, tax compliance could be greater in aligned municipalities. For example, Cullen, Turner and Washington (2018) shows that, when there is a match between own party and presidential party in the U.S., evasion is lower. If, for some reason, tax evasion is greater in unaligned municipalities, the effect found would be explained by greater tax compliance.

A fourth possible explanation could be that aligned municipalities better enforce the disclosure law given that it is a provincial law. If that is the case, maybe police officers in aligned municipalities have incentives to comply with this law and more accurately report their income and assets than those in unaligned municipalities. In that case, an increase in *reported* income and assets rather than a real increase in income and assets would explain the effect found in the previous section .

A final possible reason behind my results is a corruption-based explanation. There are three ways in which corruption could play a role in explaining my results. The first way relates to the previous explanation: If political alignment allows a better enforcement of provincial laws and the goal of this financial disclosure law is to increase the monitoring of corrupt bureaucrats, then corrupt police officers in aligned municipalities would be better monitored and, as a result, their reported income and assets would be greater.[6] A second way in which corruption could explain my findings may be that police officers have incentives to report more income and assets in aligned regimes because these regimes are more lenient with police officers.[7] A third corruption-based

[6]Note that the previous explanation would imply a decrease of misreporting of *all* police officers while this other (i.e., corruption-based) explanation would imply an effect only on corrupt bureaucrats.

[7]Given that police officers who report to have an extraordinary amount of income and assets are supposed to be investigated for corruption, this second corruption-based explanation would also imply that political alignment affects the enforcement of the financial disclosure law (i.e., police officers report more income and assets in aligned regimes because they are less likely to be investigated for corruption in the case of a discrepancy or inconsistency in

explanation is that political party heterogeneity may directly obstruct police officers' opportunities to extract rents, resulting in an actual decrease on income and assets.

To understand which of the above explanations is behind my results, I conduct several heterogeneity exercises. First, I compare the effect of political alignment on administrative police officers versus nonadministrative ones. Administrative police officers work on administrative tasks and do not leave the police station and, consequently, are much better monitored. Given the type of corruption we are dealing with in this setting (e.g., police officers teaming-up with criminals), nonadministrative police officers have many more opportunities to be corrupt than administrative ones. Therefore, if the reason behind my results is a corruption-based explanation, we would expect to observe a greater effect of political alignment on nonadministrative police officers rather than on administrative ones. However, if my results are driven by any of the above explanations, we would not expect a differential impact on administrative versus nonadministrative police officers. In other words, if the results are driven by economic growth, better enforcement of the disclosure law in aligned municipalities or different tax compliance incentives, we would not find a differential effect of political alignment on administrative and nonadministrative police officers' income and assets because the only difference between these two categories of police officers is the type of tasks they are assigned to.

In Table 3.6, I show the effect of political alignment on administrative police officers (Column (a)) and on nonadministrative ones (Column (b)). While I still find a positive and significant effect on nonadministrative police officers' total income and net assets, the effect on administrative ones is small and not statically significant. In Panel B, I employ an alternative specification and, instead of estimating separate regressions (Panel A), I estimate the effect of political alignment on administrative and nonadministrative police officers using a combined regression for which I test whether the coefficients for each group are the same. Both specifications are consistent with the hypothesis that the effect of political alignment is different (and greater)

their income or assets).

for nonadministrative police officers.

Second, I explore the heterogeneity by tenure in Table 3.7. Possibly, police officers with higher tenure are able to extract more rents than those police officers who have been working for a shorter period of time. Consistent with this idea, I find that the effect of political alignment on both income and net assets is greater for those police officers who have higher tenure.

Finally, I compare the effects of political alignment between low and high crime areas in Table 3.8. The main motivation behind this heterogeneity exercise comes from the fact that, in this setting, police officers team-up with criminals to conduct very serious crimes, such as drug and weapons trafficking. Therefore, we would expect that police officers who work in higher crime areas have more opportunities to be corrupt than those who work in low crime areas. Naturally, one concern about this heterogeneity exercise is that crime is possibly endogenous to political alignment. However, in Figure 3.5, I provide evidence that this is not the case: the effect of political alignment on total crimes per capita is small and insignificant. In other words, the definition of low and high crime areas (which is based on the total crimes per capita in a municipality) is not affected by political alignment. Consistent with a corruption-based explanation, political alignment has a positive and significant effect on the income and net assets of those police officers who work in high crime areas while the effect on those who work in low crime areas is small and insignificant.

Through these heterogeneity exercises, we learn that the effect of political alignment is greater for nonadministrative police officers, for those police officers who have been working for a longer periods of time (higher tenure) and for those who work in higher crime areas. Overall, these results provide suggestive evidence that these results are likely to be driven by a corruption-based explanation (either by a decrease in corrupt police officers' misreporting or an increase in the extracted rents from corruption) rather than by the other alternative explanations discussed above.

3.5 Conclusion

The negative effects of bureaucratic corruption have been well documented across the developing world (Olken and Pande, 2012; Svensson, 2005). Disclosure laws may potentially serve as a corruption-controlling device; however, their effectiveness ultimately rests on the information they provide. Therefore, understanding which factors affects the information these transparency initiatives provide is crucial to determine how these policies may serve to fight corruption.

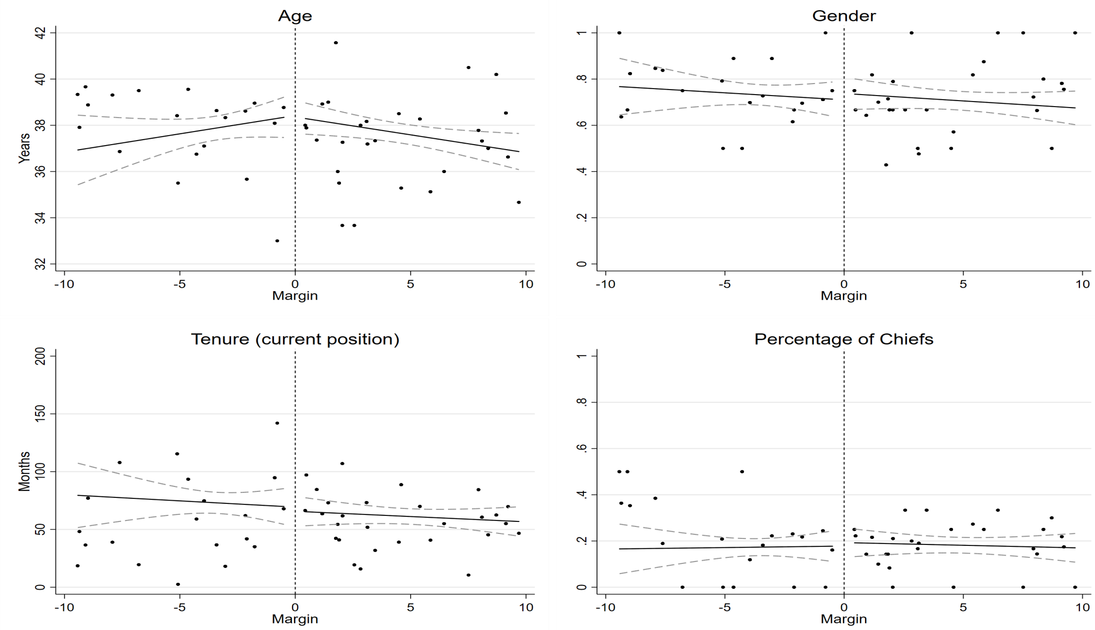
In this paper, I employ a close elections regression discontinuity design to ask how political party heterogeneity affects police officers' disclosure of their income and assets in the context of a recent financial disclosure law in Buenos Aires Province, Argentina. While the provincial government hires police officers, in practice, each officer works in a particular municipality and interacts with politicians at both provincial and municipal levels.

My results show that police officers in aligned municipalities have more total income (5.2%) and net assets (51.7%) than police officers in unaligned municipalities. While the effect on the income officers obtain through their main occupation is small and insignificant, the effect on their total income is driven by the income they obtain via alternative sources (specifically, police officers in aligned municipalities have 42% more income from other sources than those in unaligned municipalities). In addition, the effect on net assets is driven by an effect on total assets rather than an effect on debts. In particular, police officers in aligned municipalities have 36% more total assets which is mostly explained by an effect on movable and immovable assets rather than financial assets.

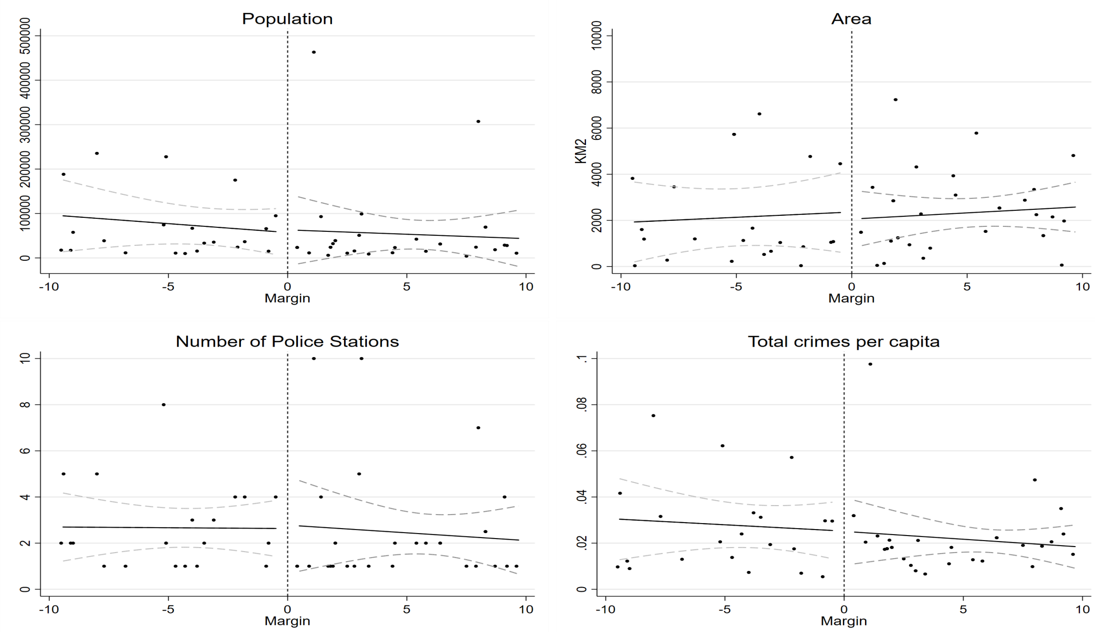
By comparing these effects on nonadministrative versus administrative police officers, I find that these effects only hold on nonadministrative police officers. In addition, I conduct other heterogeneity exercises in which I compare the effects of political alignment between high versus low tenure and high versus low crime areas and find greater effects for police officers with higher tenure and those who work in high crime areas. These results are consistent with a corruption-based explanation, rather than political alignment affecting wages, economic growth or tax compliance.

While the data do not allow me to disentangle whether an increase on extracted rents or a decrease in corrupt bureaucrats' misreporting explain these results, my findings highlight the importance of political alignment and transparency initiatives in understanding bureaucratic corruption. Overall, the fact that political alignment significantly affects the information obtained from these financial disclosure laws has important policy implications. First, it points out the importance of transparency initiatives in providing information that could be crucial to deter corruption. Second, it highlights that policies aiming to reduce bureaucratic corruption which merely focus on bureaucrats' incentives might not be as effective as policies that target both bureaucrats' and politicians' incentives.

3.6 Figures and Tables



Panel A: Police Officers



Panel B: Municipalities

Figure 3.1: Balance Tests

Notes: Each observation is the average of police officers/municipalities in 0.1 percentage margin bin. The dashed vertical line denote the cutoff of zero win margin. Points to the right (left) of zero are aligned (unaligned) municipalities.

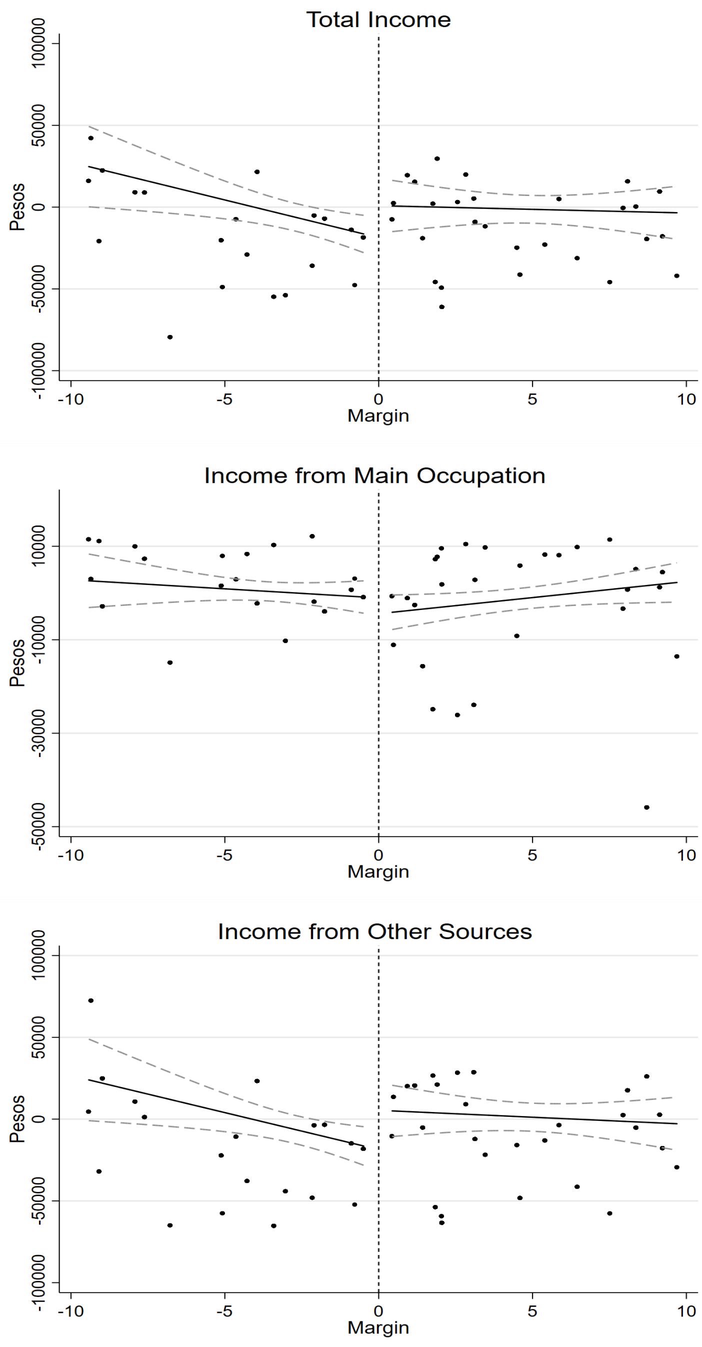


Figure 3.2: Effects on Income

Notes: Each observation is the average of police officers in 0.1 percentage margin bin. The dashed vertical line denote the cutoff of zero win margin. Points to the right(left) of zero are police officers in aligned (unaligned) municipalities.

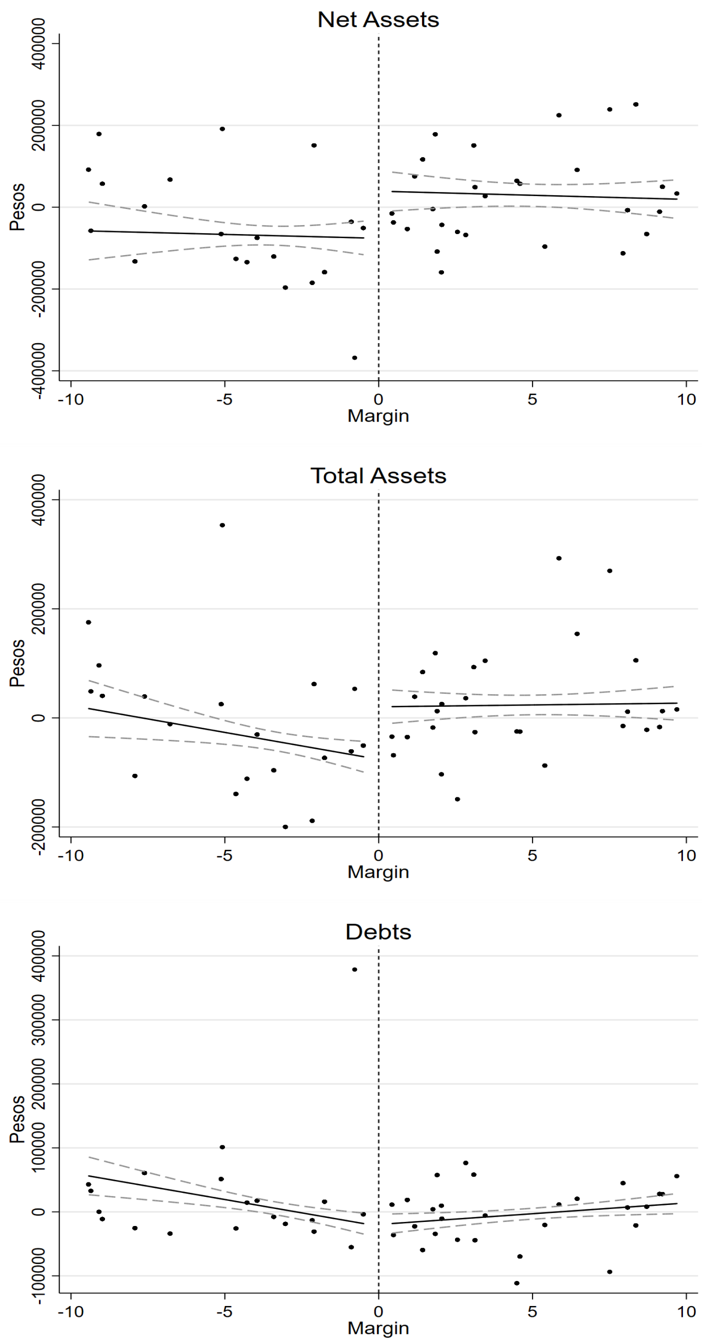


Figure 3.3: Effects on Assets

Notes: Each observation is the average of police officers in 0.1 percentage margin bin. The dashed vertical line denote the cutoff of zero win margin. Points to the right (left) of zero are police officers in aligned (unaligned) municipalities.

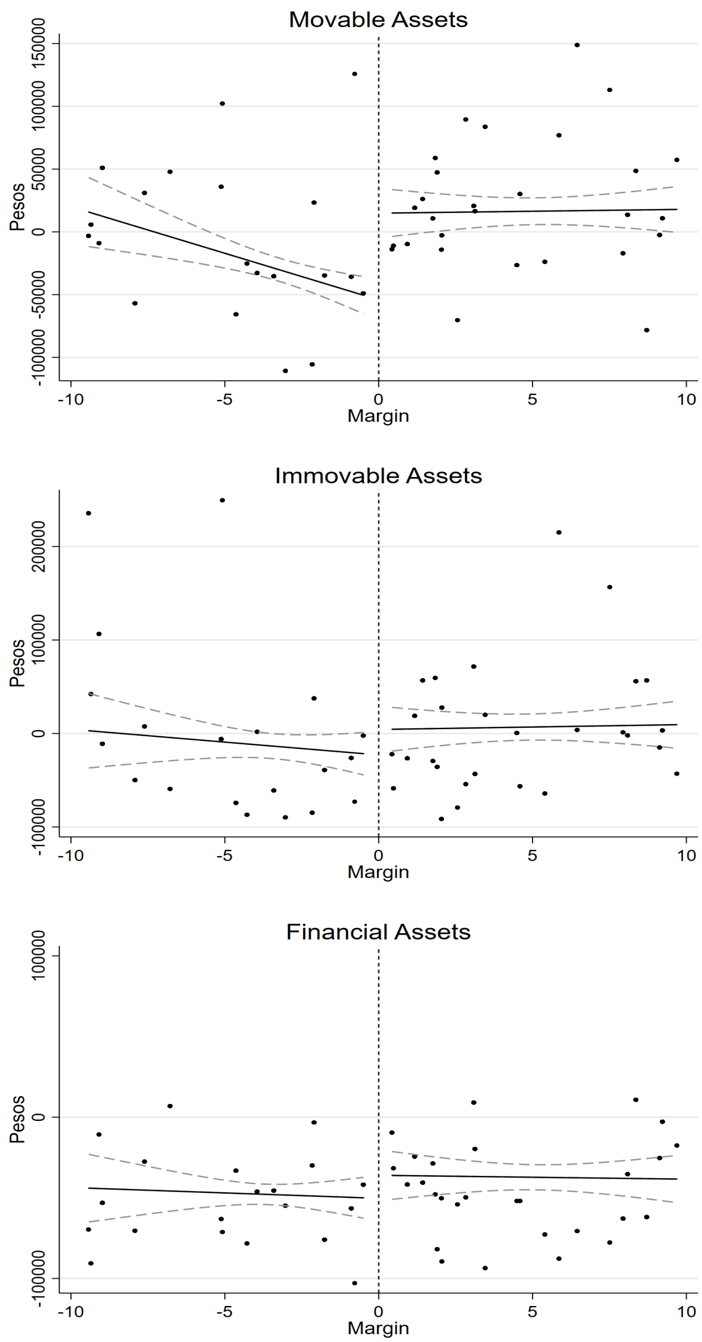


Figure 3.4: Effects on Total Assets By Category

Notes: Each observation is the average of police officers in 0.1 percentage margin bin. The dashed vertical line denote the cutoff of zero win margin. Points to the right (left) of zero are police officers in aligned (unaligned) municipalities.

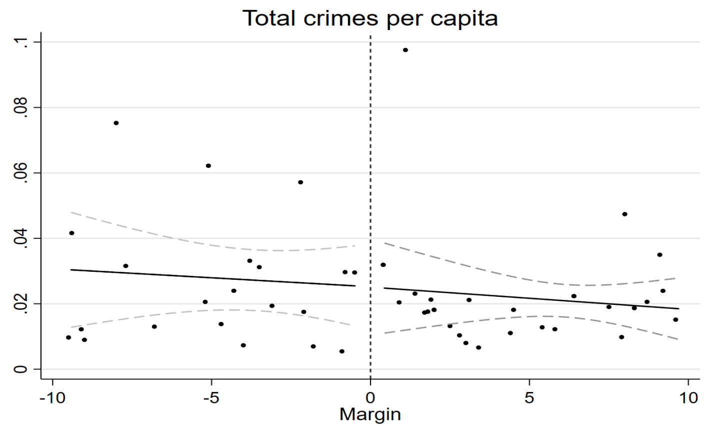


Figure 3.5: Effects on Total Crimes per Capita

Notes: Each observation is the average of municipalities in 0.1 percentage margin bin. The dashed vertical line denote the cutoff of zero win margin. Points to the right (left) of zero are police officers in aligned (unaligned) municipalities.

Table 3.1: Balance Tests

	Margin>0	Margin<0	RD estimate	t-stat
Panel A: Police Officers				
Age (years)	37.74	37.89	-0.06	-0.12
Sex	0.73	0.74	0.03	0.49
Tenure current position (months)	66.85	71.56	-3.31	-0.30
Percentage of Chiefs	0.18	0.19	0.01	0.36
Panel B: Municipalities				
Total Population	54,183	76,387	5,830	0.12
Area (km2)	2,302	2,143	-305.18	-0.29
Police Stations	2.47	2.67	0.15	0.12
Total Crimes per capita	0.02	0.03	0.00	-0.01

Notes: Panel A: Sample size is 980 which corresponds to police officers in municipalities in a 10 percentage win margin bandwidth. Column 1 and 2 shows mean for aligned and unaligned municipalities. Column 3 estimates the discontinuity at the zero win margin cutoff using separate linear trends and triangular weights. Column 4 is the t-statistic for those estimates.

Table 3.2: Regression Analysis-Income

	Total Income	Main Occupation	Other Sources
Margin>0	22,436**	-3,483	25,919**
	(10,280)	(2,375)	(10,393)
Mean Dep Var	429,501	368,586	60,915
Observations	980	980	980

Notes: Specifications include police officers in municipalities in a ten percentage win margin bandwidth, separate linear trends on each side of the discontinuity and employ triangular weights. Standard errors clustered by municipality. Fixed effects by police hierarchy.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 3.3: Regression Analysis-Assets

	Net Assets	Total Assets	Debts
Margin>0	118,697***	108,554***	2,785
	(34,565)	(24,853)	(18,561)
Mean Dep Var	229,175	300,776	113,745
Observations	980	980	980

Notes: Specifications include police officers in municipalities in a ten percentage win margin bandwidth, separate linear trends on each side of the discontinuity and employ triangular weights. Standard errors clustered by municipality. Fixed effects by police hierarchy.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 3.4: Regression Analysis-Total assets by category

	Movable Assets	Immovable Assets	Financial Assets
Margin>0	66,158***	29,549*	12,927
	(9,524)	(16,115)	(9,316)
Mean Dep Var	153,177	106,157	42,144
Observations	980	980	980

Notes: Specifications include police officers in municipalities in a ten percentage win margin bandwidth, separate linear trends on each side of the discontinuity and employ triangular weights. Standard errors clustered by municipality. Fixed effects by police hierarchy.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 3.5: Robustness Checks

Panel A: Income			
	Total Income	Main Occupation	Other Sources
No fixed effects	14,641	-12,053	26,694**
	(20,196)	(11,988)	(11,437)
Quadratic Trends	44,175***	-7,205*	51,379***
	(12,394)	(3,849)	(12,953)
5% bandwidth	45,487***	-4,952	50,439***
	(9,202)	(3,417)	(9,712)

Panel B: Assets			
	Net Assets	Total Assets	Debts
No fixed effects	121,004***	111,169***	2,352
	(41,752)	(29,125)	(19,782)
Quadratic Trends	71,791*	72,019*	19,887
	(41,886)	(35,833)	(26,666)
5% bandwidth	81,574**	86,634***	16,955
	(36,510)	(28,486)	(23,695)

Notes: Specifications employ triangular weights. Standard errors clustered by municipality. ***, ** and * significant at the 1, 5 and 10 percent level, respectively.

Table 3.6: Heterogeneity: Administrative vs Nonadministrative Police Officers

Panel A: Separate Regressions		
	(a)	(b)
	Administrative	Nonadministrative
1. Total Income		
Margin>0	3,200	26,462**
	(5,247)	(11,288)
Mean Dep Var	409,860	432,404
Obs	116	874
2. Net Assets		
Margin>0	-44,449	104,268***
	(67,291)	(28,210)
Mean Dep Var	190,129	135,609
Obs	116	874
Panel B: Combined Regressions Contrasts (p values)		
1. Total Income	0.03	
2. Net Assets	0.00	

Notes: Specifications include police officers in municipalities in a ten percentage win margin bandwidth, separate linear trends on each side of the discontinuity and employ triangular weights. Standard errors clustered by municipality. Fixed effects by police hierarchy.
***, ** and * significant at the 1, 5 and 10 percent level, respectively.

Table 3.7: Heterogeneity: High vs Low Tenure

Panel A: Separate Regressions		
	(a)	(b)
	Low Tenure	High Tenure
1. Total Income		
Margin>0	17,331	56,423**
	(12,483)	(25,808)
Mean Dep Var	432,577	427,762
Obs	839	151
2. Net Assets		
Margin>0	96,996***	140,637*
	(29,598)	(75,149)
Mean Dep Var	139,425	157,382
Obs	839	151
Panel B: Combined Regressions Contrasts (p values)		
1. Total Income	0.06	
2. Net Assets	0.08	

Notes: Specifications include police officers in municipalities in a ten percentage win margin bandwidth, separate linear trends on each side of the discontinuity and employ triangular weights. Standard errors clustered by municipality. Fixed effects by police hierarchy. ***, ** and * significant at the 1, 5 and 10 percent level, respectively.

Table 3.8: Heterogeneity Low vs High Crime Areas

Panel A: Separate Regressions		
	(a)	(b)
	High Crime Areas	Low Crime Areas
1. Total Income		
Margin>0	39,125***	4,175
	(10,835)	(17,189)
Mean Dep Var	439,525	424,938
Obs	498	492
2. Net Assets		
Margin>0	117,592***	54,477
	(35,445)	(57,116)
Mean Dep Var	136,991	145,843
Obs	498	492
Panel B: Combined Regressions Contrasts (p values)		
1. Total Income	0.03	
2. Net Assets	0.03	

Notes: Specifications include police officers in municipalities in a ten percentage win margin bandwidth, separate linear trends on each side of the discontinuity and employ triangular weights. Standard errors clustered by municipality. Fixed effects by police hierarchy. ***, ** and * significant at the 1, 5 and 10 percent level, respectively.

Appendix A

Supplementary Tables for Chapter 1

Table A.1: Definitions of Trust and Knowledge/Financial Literacy Variables

Variable Name	Survey Questions
Trust Variables	
Trust bank	Do you trust the bank? (=1 Yes; =0 No)
Trust bank staff	Do you trust the bank staff? (=1 Yes; =0 No)
Trust bank branch	Do you trust your bank branch? (=1 Yes; =0 No)
Prefer to save in bank vs home	Do you feel safer having your savings in a bank or at home? (=1 Bank; =0 Home)
Prefer to save in bank vs assets (livestock)	Do you feel safer having your savings in a bank or in the form of assets (livestock)? (=1 Bank; =0 Livestock)
Overall trust	Share correct = Sum of correct answers to trust questions divided by the total number of questions (5)
Knowledge/Financial Literacy Variables	
Savings account	Do you know what a savings account is? (=1 having money in the bank; =0 otherwise)
Savings/loans	Do you think you understand savings and loans? (=1 Yes; =0 No)
Agent	Do you know what a MultiRed Agent is? (=1 Yes; =0 No)
Interest rates	Suppose Bank A offers a savings account with an annual interest rate of 15% while Bank B offers an interest rate of 18%. Which bank do you think is better for saving? (=1 Bank A; =0 Bank B)
Overall knowledge	Share correct = Sum of correct answers to knowledge questions divided by the number of questions (4)

Table A.2: Comparison of Full Sample with Analysis Sample

Variables	Analysis Sample Mean	SD	N	Sample Excluded Mean	SD	N	Difference in Means	P-Value
Age	40.31	10.11	3069	40.91	11.43	887	-0.60	0.14
Female	0.97	–	3069	0.95	–	887	0.02	0.04
Household size	4.80	1.60	3069	4.83	1.92	887	-0.03	0.68
Years of schooling	5.90	4.10	3068	6.13	4.18	887	-0.22	0.29
Preferred language	0.19	–	3069	0.19	–	887	-0.01	0.70
Work on farm	0.65	–	2822	0.63	–	824	0.02	0.31
Own farm	0.12	–	2822	0.09	–	824	0.02	0.08
Own home	0.81	–	3069	0.81	–	886	0.00	0.86
Have other bank accounts	0.04	–	3069	0.03	–	887	0.00	0.62
Participate in a ROSCA	0.03	–	3034	0.03	–	873	0.00	0.71

Notes: This table is based on 2016 household survey data.

Table A.3: Effect of Treatment on Trust and Knowledge

	Panel A: Trust					
	Trust bank	**Trust bank staff**	**Trust bank branch**	**Bank vs home**	**Bank vs livestock**	**Overall trust**
ITT: OLS $\hat{\beta}$	0.133	0.045	0.078	0.141	0.120	0.101
Standard error	(0.028)	(0.021)	(0.019)	(0.032)	(0.032)	(0.021)
P Value	[0.000]	[0.037]	[0.000]	[0.000]	[0.000]	[0.000]
LATE: TSLS $\hat{\beta}$	0.193	0.065	0.111	0.206	0.175	0.145
Standard error	(0.041)	(0.030)	(0.029)	(0.052)	(0.051)	(0.032)
P Value	[0.000]	[0.034]	[0.000]	[0.000]	[0.001]	[0.000]
First-stage F-statistic	2968	2968	1977	2720	2752	1774
Mean control group	0.48	0.36	0.78	0.46	0.29	0.49
Observations	3,187	3,187	2,060	3,021	2,979	1,866
	Panel B: Knowledge/Financial literacy					
	Savings account	**Agent**	**Savings/Loans**	**Interest rates**	**Overall knowledge**	
ITT: OLS $\hat{\beta}$	0.009	-0.005	-0.005	0.024	0.002	
Standard error	(0.034)	(0.004)	(0.030)	(0.017)	(0.014)	
P Value	[0.777]	[0.232]	[0.878]	[0.163]	[0.905]	
LATE: TSLS $\hat{\beta}$	0.014	-0.007	-0.007	0.035	0.002	
Standard error	(0.048)	(0.006)	(0.043)	(0.025)	(0.020)	
P Value	[0.777]	[0.248]	[0.877]	[0.168]	[0.905]	
First-stage F-statistic	1723	2968	2199	2674	1304	
Mean control group	0.74	0.99	0.32	0.85	0.75	
Observations	1,828	3,187	2,223	2,894	1,432	

Notes: This table shows the results of the estimation of equation 1.1, which are used to construct Figure 1.1 in the main text. The data source for estimation is the 2016 household survey data. Clustered standard errors are given in parentheses (village) and p-values in brackets. For Bonferroni corrected p-value, we contrast the p -value against 0.02 for a significance level of 0.1. The exact questions used to measure the trust and knowledge/financial literacy outcomes are presented in Appendix Table A.1

Table A.4: Effect of Treatment on Transactions, Savings and Use of Agent

Bimester since treatment					
	K = 0	K = 1	K = 2	K = 3	K = 4
Number of Transactions (Deposits + Withdrawals)					
ITT: OLS $\hat{\beta}_k$	0.040	0.042	-0.019	-0.01	-0.10
Standard error	(0.019)	(0.07)	(0.03)	(0.04)	(0.05)
LATE: TSLS $\hat{\beta}_k$	0.053	0.055	-0.016	0.00	-0.10
Standard error	(0.03)	(0.09)	(0.04)	(0.05)	(0.06)
First-stage F-statistic	293.36	303.14	279.49	255.8	168.62
Savings					
ITT: OLS $\hat{\beta}_k$	2.96	5.79	7.50	7.15	10.85
Standard error	(1.93)	(2.25)	(2.81)	(3.19)	(3.60)
LATE: TSLS $\hat{\beta}_k$	4.21	7.91	9.43	9.28	13.43
Standard error	(2.58)	(2.97)	(3.52)	(3.99)	(4.46)
First-stage F-statistic	294.70	303.17	286.16	234.88	200.14
Use of Agent for Withdrawal					
ITT: OLS $\hat{\beta}_k$	0.11	0.04	0.11	0.03	0.05
Standard error	(0.08)	(0.09)	(0.12)	(0.13)	(0.12)
LATE: TSLS $\hat{\beta}_k$	0.15	0.06	0.15	0.05	0.08
Standard error	(0.11)	(0.13)	(0.15)	(0.17)	(0.15)
First-stage F-statistic	293.36	303.14	279.49	255.80	168.62
Individuals	3,187	3,187	3,187	3,187	3,187
Observations	18,754	18,754	18,754	18,754	18,754

Notes: This table shows the results of the estimation of equation 1.2, which are used to construct Figures 1.2, 1.3 and 1.4 in the main text. The data source for estimation is the administrative account level data. Calendar time and individual fixed effects are included but not reported. Clustered standard errors are reported in parentheses (village).

Appendix B

Supplementary Figures and Tables for Chapter 2

Table B.1: Homicides Mean in 2014-2018 from Homicides in 2012/13

	0-5 blocks	5-10 blocks	Difference	p-value
Mean	1.42	1.44	-0.02	0.65

Notes: This table calculates the average number of homicides in 2014/2018 per year per km2 in a radius of 0-5 and 5-10 blocks from all 2012/2013 homicides.

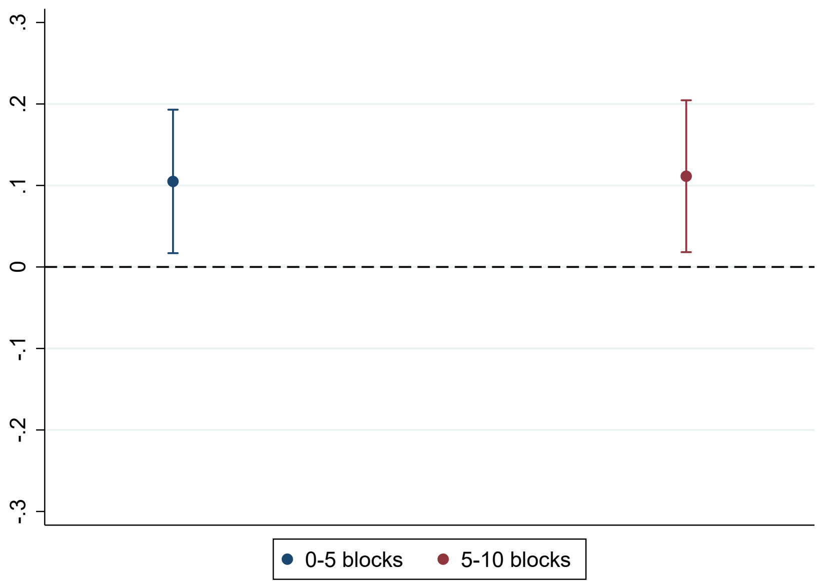


Figure B.1: Homicides Autocorrelation

Notes: Graph shows autoregressive coefficients and 95 percent confidence intervals from two AR(1) models: homicides within (a) 0-5 blocks and (b) 5-10 blocks from all 2012/13 homicides. The method employed is system-GMM (Blundell and Bond, 1998) using a two-step approach. The resulting variance co-variance matrix has been corrected to account for potential small sample bias (Windmeijer, 2005).

Table B.2: Unemployed/Informal Sector: Robustness

Time to Treat	*Base*	*Alternative Time Trends*	*Alternative Sample*	*Alternative Controls*
-10	0.0008	0.0012	0.0009	0.0012
	(0.0012)	(0.0011)	(0.0012)	(0.0011)
-9	0.0003	0.0004	0.0003	0.0005
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
-8	0.0002	0.0002	0.0002	0.0003
	(0.0009)	(0.0009)	(0.0009)	(0.0009)
-7	0.0007	0.0005	0.0006	0.0005
	(0.0009)	(0.0009)	(0.0009)	(0.0009)
-6	0.0002	0.0000	0.0002	0.0000
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
-5	0.0000	-0.0001	0.0000	-0.0001
	(0.0007)	(0.0007)	(0.0008)	(0.0007)
-4	-0.0005	-0.0003	-0.0005	-0.0003
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
-3	-0.0004	-0.0003	-0.0004	-0.0003
	(0.0007)	(0.0006)	(0.0007)	(0.0006)
-2	0.0004	0.0004	0.0004	0.0004
	(0.0004)	(0.0003)	(0.0004)	(0.0004)
0	0.0009	0.0008	0.0009	0.0008
	(0.0005)	(0.0004)	(0.0005)	(0.0004)
1	0.0012	0.0012	0.0012	0.0012
	(0.0006)	(0.0005)	(0.0006)	(0.0005)
2	0.0007	0.0006	0.0007	0.0006
	(0.0007)	(0.0006)	(0.0007)	(0.0006)
3	0.0010	0.0008	0.0010	0.0008
	(0.0007)	(0.0007)	(0.0008)	(0.0007)
4	0.0012	0.0009	0.0012	0.0009
	(0.0008)	(0.0007)	(0.0008)	(0.0007)
5	0.0011	0.0008	0.0011	0.0008
	(0.0008)	(0.0007)	(0.0008)	(0.0007)
6	0.0011	0.0009	0.0012	0.0009
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
7	0.0013	0.0011	0.0013	0.0010
	(0.0008)	(0.0008)	(0.0009)	(0.0008)
8	0.0013	0.0010	0.0013	0.0009
	(0.0009)	(0.0008)	(0.0009)	(0.0008)
9	0.0013	0.0012	0.0014	0.0011
	(0.0009)	(0.0009)	(0.0009)	(0.0009)
10	0.0010	0.0008	0.001	0.0008
	(0.0009)	(0.0009)	(0.0009)	(0.0009)
Time Trends	Neigh.	Grid	Neigh.	Neigh.
Multiple Treaters	Yes	Yes	No	Yes
Time-Varying Controls	No	No	No	Yes
Obs	5,269,780	5,269,780	4,758,623	5,269,780
R-Sq	0.540	0.590	0.546	0.601

Notes: DD coefficients from estimation of Equation 2.1 in addition to alternative specifications on the probability of being unemployed or working in the informal sector. Time-varying controls include quadratic and cubic terms in age fully interacted with educational attainment. Standard errors clustered by zip code.

Table B.3: Weekly Labor Earnings: Robustness

Time to Treat	*Base*	*Alternative Time Trends*	*Alternative Sample*	*Alternative Controls*
-10	4.870	1.208	3.554	5.654
	(4.080)	(3.571)	(4.375)	(3.750)
-9	0.0432	0.687	-2.186	2.790
	(4.997)	(3.429)	(5.455)	(4.121)
-8	2.347	1.342	0.971	3.918
	(3.636)	(3.257)	(3.973)	(3.338)
-7	5.208	4.232	4.308	5.063
	(3.448)	(3.147)	(3.740)	(3.302)
-6	3.166	0.999	2.119	3.605
	(3.140)	(3.111)	(3.463)	(3.010)
-5	0.811	-0.558	-0.634	1.256
	(3.243)	(3.428)	(3.710)	(3.129)
-4	0.987	-1.134	0.0580	1.265
	(3.299)	(2.885)	(3.818)	(3.220)
-3	1.231	-0.840	-0.0657	0.815
	(2.558)	(2.682)	(2.956)	(2.479)
-2	3.913	-2.216	3.622	3.937
	(4.501)	(2.330)	(5.429)	(4.473)
0	-2.987	-0.614	-2.405	-3.101
	(2.234)	(1.675)	(2.620)	(2.102)
1	-2.842	-2.343	-2.475	-3.745
	(2.861)	(2.807)	(3.387)	(2.700)
2	-4.013	-3.068	-4.994	-5.741
	(3.114)	(2.848)	(3.384)	(2.826)
3	-8.018	-6.433	-7.892	-10.28
	(3.300)	(3.597)	(3.695)	(3.029)
4	-9.269	-9.655	-9.712	-12.34
	(3.675)	(4.080)	(4.107)	(3.300)
5	-12.74	-11.07	-14.13	-15.91
	(3.974)	(4.326)	(4.438)	(3.713)
6	-15.13	-11.09	-15.44	-18.81
	(4.432)	(4.714)	(4.864)	(4.240)
7	-16.68	-11.49	-17.25	-20.39
	(4.652)	(5.211)	(5.098)	(4.531)
8	-18.33	-12.92	-18.81	-22.68
	(5.210)	(5.611)	(5.666)	(5.108)
9	-17.86	-12.2	-18.4	-20.3
	(5.648)	(6.243)	(6.156)	(5.489)
10	-20.01	-14.48	-20.33	-22.87
	(6.268)	(7.304)	(6.692)	(6.070)
Time Trends	Neigh	Grid	Neigh	Neigh
Multiple Treaters	Yes	Yes	No	Yes
Time-Varying Controls	No	No	No	Yes
Obs	3,746,367	3,746,367	3,385,617	3,746,367
R-Sq	0.540	0.590	0.546	0.545

Notes: DD coefficients from estimation of Equation 2.1 in addition to alternative specifications on weekly labor earnings. Time-varying controls include quadratic and cubic terms in age fully interacted with educational attainment. Standard errors clustered by zip code.

Table B.4: Weekly Hours Worked: Robustness

Time to Treat	*Base*	*Alternative Time Trends*	*Alternative Sample*	*Alternative Controls*
-10	0.0168	-0.0126	0.0365	0.0092
	(0.0271)	(0.0266)	(0.0283)	(0.0271)
-9	0.0358	-0.0124	0.0591	0.0248
	(0.0323)	(0.0250)	(0.0342)	(0.0295)
-8	0.0237	-0.0138	0.0452	0.0153
	(0.0232)	(0.0235)	(0.0245)	(0.0228)
-7	0.0113	-0.0195	0.0272	0.0067
	(0.0216)	(0.0222)	(0.0232)	(0.0218)
-6	0.0191	-0.00559	0.0391	0.0143
	(0.0204)	(0.0205)	(0.0220)	(0.0205)
-5	0.0291	0.0198	0.0498	0.0250
	(0.0194)	(0.0196)	(0.0214)	(0.0194)
-4	0.0229	0.0106	0.0349	0.0199
	(0.0194)	(0.0176)	(0.0218)	(0.0195)
-3	0.0197	0.0093	0.0243	0.0189
	(0.0169)	(0.0159)	(0.0190)	(0.0169)
-2	-0.0196	-0.0012	-0.0217	-0.0205
	(0.0234)	(0.0102)	(0.0280)	(0.0234)
0	-0.0038	-0.0040	0.0106	-0.0027
	(0.0122)	(0.0113)	(0.0129)	(0.0122)
1	-0.0136	-0.0237	-0.0028	-0.0101
	(0.0150)	(0.0151)	(0.0167)	(0.0150)
2	-0.0387	-0.0333	-0.0166	-0.0328
	(0.0189)	(0.0161)	(0.0210)	(0.0187)
3	-0.0144	-0.0166	0.0063	-0.0068
	(0.0197)	(0.0193)	(0.0218)	(0.0196)
4	-0.0043	0.0037	0.0294	0.0056
	(0.0209)	(0.0209)	(0.0229)	(0.0207)
5	-0.0031	-0.0046	0.0369	0.0077
	(0.0216)	(0.0221)	(0.0238)	(0.0219)
6	-0.0038	0.0085	0.0321	0.0090
	(0.0224)	(0.0227)	(0.0246)	(0.0231)
7	0.0111	0.0199	0.0457	0.0247
	(0.0237)	(0.0240)	(0.0259)	(0.0253)
8	0.0131	0.0137	0.0425	0.0293
	(0.0244)	(0.0248)	(0.0265)	(0.0260)
9	0.0093	0.0199	0.0378	0.0230
	(0.0259)	(0.0257)	(0.0278)	(0.0276)
10	-0.0009	0.0068	0.0337	0.0149
	(0.0276)	(0.0281)	(0.0295)	(0.0298)
Time Trends	Neigh	Grid	Neigh	Neigh
Multiple Treaters	Yes	Yes	No	Yes
Time-Varying Controls	No	No	No	Yes
Obs	3,746,367	3,746,367	3,385,617	3,746,367
R-Sq	0.540	0.590	0.546	0.648

Notes: DD coefficients from estimation of Equation 2.1 in addition to alternative specifications on weekly hours worked. Time-varying controls include quadratic and cubic terms in age fully interacted with educational attainment. Standard errors clustered by zip code.

Table B.5: Hourly Wage: Robustness

Time to Treat	*Base*	*Alternative Time Trends*	*Alternative Sample*	*Alternative Controls*
-10	0.116	0.0288	0.0846	0.135
	(0.0971)	(0.0850)	(0.104)	(0.0893)
-9	0.00103	0.0163	-0.0520	0.0664
	(0.119)	(0.0816)	(0.130)	(0.0981)
-8	0.0559	0.0319	0.0231	0.0933
	(0.0866)	(0.0775)	(0.0946)	(0.0795)
-7	0.124	0.101	0.103	0.121
	(0.0821)	(0.0749)	(0.0890)	(0.0786)
-6	0.0754	0.0238	0.0505	0.0858
	(0.0748)	(0.0741)	(0.0825)	(0.0717)
-5	0.0193	-0.0133	-0.0151	0.0299
	(0.0772)	(0.0816)	(0.0883)	(0.0745)
-4	0.0235	-0.0270	0.00138	0.0301
	(0.0786)	(0.0687)	(0.0909)	(0.0767)
-3	0.0293	-0.0200	-0.00156	0.0194
	(0.0609)	(0.0638)	(0.0704)	(0.0590)
-2	0.0932	-0.0528	0.0862	0.0937
	(0.107)	(0.0555)	(0.129)	(0.107)
0	-0.0711	-0.0146	-0.0573	-0.0738
	(0.0532)	(0.0399)	(0.0624)	(0.0501)
1	-0.0677	-0.0558	-0.0589	-0.0892
	(0.0681)	(0.0668)	(0.0806)	(0.0643)
2	-0.0955	-0.0730	-0.119	-0.137
	(0.0741)	(0.0678)	(0.0806)	(0.0673)
3	-0.191	-0.153	-0.188	-0.245
	(0.0786)	(0.0856)	(0.0880)	(0.0721)
4	-0.221	-0.23	-0.231	-0.294
	(0.0875)	(0.0971)	(0.0978)	(0.0786)
5	-0.303	-0.264	-0.336	-0.379
	(0.0946)	(0.103)	(0.106)	(0.0884)
6	-0.36	-0.264	-0.368	-0.448
	(0.106)	(0.112)	(0.116)	(0.101)
7	-0.397	-0.274	-0.411	-0.485
	(0.111)	(0.124)	(0.121)	(0.108)
8	-0.437	-0.308	-0.448	-0.54
	(0.124)	(0.134)	(0.135)	(0.122)
9	-0.425	-0.291	-0.438	-0.483
	(0.134)	(0.149)	(0.147)	(0.131)
10	-0.476	-0.345	-0.484	-0.545
	(0.149)	(0.174)	(0.159)	(0.145)
Time Trends	Neigh	Grid	Neigh	Neigh
Multiple Treaters	Yes	Yes	No	Yes
Time-Varying Controls	No	No	No	Yes
Obs	3,746,367	3,746,367	3,385,617	3,746,367
R-Sq	0.540	0.590	0.546	0.545

Notes: DD coefficients from estimation of Equation 2.1 in addition to alternative specifications on hourly wage. Time-varying controls include quadratic and cubic terms in age fully interacted with educational attainment. Standard errors clustered by zip code.

Table B.6: Unemployed/Informal Sector: Alternative Standard Errors

Time to Treat	Coef	Cluster Zip (1)	Cluster Zip, Time (2)	Cluster Grid (3)
-10	0.0008	0.308	0.492	0.323
-9	0.0003	0.690	0.835	0.696
-8	0.0002	0.799	0.949	0.802
-7	0.0007	0.579	0.819	0.586
-6	0.0002	0.963	0.798	0.964
-5	0.0000	0.862	0.653	0.866
-4	-0.0005	0.649	0.516	0.663
-3	-0.0004	0.652	0.543	0.666
-2	0.0004	0.291	0.416	0.342
-1	-	-	-	-
0	0.0009	0.038	0.023	0.038
1	0.0012	0.022	0.021	0.026
2	0.0007	0.285	0.360	0.291
3	0.0010	0.206	0.261	0.206
4	0.0012	0.162	0.244	0.154
5	0.0011	0.260	0.385	0.255
6	0.0011	0.221	0.307	0.225
7	0.0013	0.184	0.272	0.181
8	0.0013	0.214	0.270	0.195
9	0.0013	0.179	0.216	0.156
10	0.0010	0.340	0.423	0.306

Notes: This tables displays the p-values of Equation 2.1 with standard errors calculated with various methodologies. Coefficients and zip code-clustered standard errors (shown in Column 1) are derived from main estimation results displayed in Figure 2.3 and Column 1 of Table 2.2.

Table B.7: Weekly Labor Earnings: Alternative Standard Errors

Time to Treat	Coef	Cluster Zip (1)	Cluster Zip, Time (2)	Cluster Grid (3)
-10	4.870	0.233	0.257	0.235
-9	0.0432	0.993	0.993	0.993
-8	2.347	0.519	0.529	0.524
-7	5.208	0.131	0.197	0.137
-6	3.166	0.313	0.413	0.320
-5	0.811	0.802	0.821	0.806
-4	0.987	0.765	0.776	0.767
-3	1.231	0.630	0.701	0.635
-2	3.913	0.385	0.463	0.386
-1	-	-	-	-
0	-2.987	0.181	0.388	0.181
1	-2.842	0.321	0.455	0.321
2	-4.013	0.197	0.299	0.197
3	-8.018	0.015	0.019	0.016
4	-9.269	0.012	0.012	0.012
5	-12.74	0.001	0.000	0.001
6	-15.13	0.000	0.000	0.001
7	-16.68	0.000	0.000	0.000
8	-18.33	0.000	0.000	0.000
9	-17.86	0.002	0.000	0.002
10	-20.01	0.001	0.000	0.001

Notes: This tables displays the p-values of Equation **??** with standard errors calculated with various methodologies. Coefficients and zip code-clustered standard errors (shown in Column 1) are derived from main estimation results displayed in Figure 2.4 and Column 2 of Table 2.2.

Table B.8: Weekly Hours Worked: Alternative Standard Errors

Time to Treat	Coef	Cluster Zip (1)	Cluster Zip, Time (2)	Cluster Grid (3)
-10	0.0168	0.439	0.511	0.528
-9	0.0358	0.194	0.211	0.276
-8	0.0237	0.250	0.288	0.304
-7	0.0113	0.621	0.600	0.601
-6	0.0191	0.324	0.359	0.345
-5	0.0291	0.128	0.145	0.134
-4	0.0229	0.229	0.236	0.238
-3	0.0197	0.279	0.291	0.242
-2	-0.0196	0.407	0.495	0.408
-1	-	-	-	-
0	-0.0038	0.824	0.825	0.756
1	-0.0136	0.365	0.421	0.359
2	-0.0387	0.042	0.0315	0.0465
3	-0.0144	0.560	0.430	0.470
4	-0.0043	0.980	0.817	0.839
5	-0.0031	0.923	0.863	0.887
6	-0.0038	0.932	0.839	0.867
7	0.0111	0.667	0.559	0.647
8	0.0131	0.725	0.491	0.596
9	0.0091	0.963	0.637	0.719
10	-0.0009	0.881	0.963	0.972

Notes: This tables displays the p-values of Equation 2.1 with standard errors calculated with various methodologies. Coefficients and zip code-clustered standard errors (shown in Column 1) are derived from main estimation results displayed in Figure 2.5 and Column 3 of Table 2.2.

Table B.9: Hourly Wage: Alternative Standard Errors

Time to Treat	Coef	Cluster Zip (1)	Cluster Zip, Time (2)	Cluster Grid (3)
-10	0.116	0.233	0.257	0.235
-9	0.0010	0.993	0.993	0.993
-8	0.0559	0.519	0.529	0.524
-7	0.124	0.131	0.197	0.137
-6	0.0754	0.313	0.413	0.320
-5	0.0193	0.802	0.821	0.806
-4	0.0235	0.765	0.776	0.767
-3	0.0293	0.630	0.701	0.635
-2	0.0932	0.385	0.463	0.386
-1	-	-	-	-
0	-0.0711	0.181	0.388	0.181
1	-0.0677	0.321	0.455	0.321
2	-0.0955	0.197	0.299	0.197
3	-0.191	0.015	0.0190	0.016
4	-0.221	0.011	0.0127	0.012
5	-0.303	0.001	0.000	0.001
6	-0.36	0.001	0.000	0.001
7	-0.397	0.000	0.000	0.000
8	-0.437	0.000	0.000	0.000
9	-0.425	0.002	0.000	0.002
10	-0.476	0.001	0.000	0.001

Notes: This tables displays the p-values of Equation 2.1 with standard errors calculated with various methodologies. Coefficients and zip code-clustered standard errors (shown in Column 1) are derived from main estimation results displayed in Figure 2.6 and Column 4 of Table 2.2.

Table B.10: The effects of Homicides: Robustness (Event-Study)

Time to Treat	Informal Sector/ Unemployed (1)	Weekly Labor Earnings (2)	Hours Worked (3)	Hourly Wage (4)
-10	0.0005	5.576	-0.0707	0.133
	(0.0022)	(13.08)	(0.150)	(0.311)
-9	0.0001	6.154	-0.0230	0.147
	(0.0020)	(12.55)	(0.138)	(0.299)
-8	0.0011	7.150	-0.0199	0.170
	(0.0018)	(11.94)	(0.124)	(0.284)
-7	0.0012	9.805	-0.0310	0.233
	(0.0016)	(11.42)	(0.111)	(0.272)
-6	0.0004	7.537	-0.0106	0.179
	(0.0014)	(10.09)	(0.0957)	(0.240)
-5	0.0000	6.177	-0.0119	0.147
	(0.0012)	(8.593)	(0.0822)	(0.205)
-4	-0.0004	4.794	-0.0295	0.114
	(0.0009)	(8.041)	(0.0682)	(0.191)
-3	-0.0003	1.206	-0.0104	0.0287
	(0.0008)	(5.441)	(0.0504)	(0.130)
-2	0.0004	6.174	-0.0447	0.147
	(0.0004)	(6.725)	(0.0396)	(0.160)
-1	-	-	-	-
0	0.0010	-9.684	0.0269	-0.231
	(0.0004)	(7.157)	(0.0393)	(0.170)
1	0.0014	-14.35	-0.00320	-0.342
	(0.0006)	(7.873)	(0.0522)	(0.187)
2	0.0008	-17.12	-0.0341	-0.408
	(0.0007)	(8.763)	(0.0680)	(0.209)
3	0.0011	-21.27	-0.0237	-0.507
	(0.0008)	(10.45)	(0.0832)	(0.249)
4	0.0012	-24.76	-0.0196	-0.589
	(0.0008)	(12.03)	(0.0971)	(0.286)
5	0.0009	-31.13	-0.0381	-0.741
	(0.0009)	(14.13)	(0.110)	(0.337)
6	0.0009	-34.72	-0.0671	-0.827
	(0.0010)	(16.17)	(0.122)	(0.385)
7	0.0009	-39.30	-0.0729	-0.936
	(0.0011)	(18.11)	(0.135)	(0.431)
8	0.0007	-44.37	-0.106	-1.056
	(0.0011)	(20.11)	(0.144)	(0.479)
9	0.0009	-47.82	-0.127	-1.139
	(0.0012)	(21.80)	(0.153)	(0.519)
10	0.0005	-52.96	-0.153	-1.261
	(0.0013)	(23.42)	(0.160)	(0.558)
Obs	1,317,460	922,222	922,222	922,222
R-Sq	0.911	0.185	0.139	0.185

Notes: DD coefficients from estimation of Equation 2.2 on the probability of being unemployed or working in the informal sector (Column 1), weekly labor earnings (Column 2), weekly hours worked (Column 3) and hourly wage (Column 4). Columns 2 and 4 expressed in Brazilian Reais. Standard errors clustered by employee.

Table B.11: Establishment-Level Outcomes

Time to Treat	Wage Expenditure (1)	Hourly Wage (2)	Number of Employees (3)	Prob of Shutdown (4)
-10	-28.77	0.0650	-0.183	0.0003
	(24.44)	(0.0616)	(0.186)	(0.0011)
-9	-30.79	0.0710	-0.217	0.0004
	(23.21)	(0.0589)	(0.173)	(0.0010)
-8	-33.44	0.0602	-0.185	0.0003
	(21.74)	(0.0554)	(0.159)	(0.0009)
-7	-28.38	0.0195	-0.0151	6.33e-05
	(20.88)	(0.0531)	(0.147)	(0.0009)
-6	-26.48	0.0407	-0.0719	-1.96e-05
	(19.05)	(0.0507)	(0.134)	(0.0007)
-5	-16.89	0.0963	-0.116	0.0002
	(18.38)	(0.0486)	(0.123)	(0.0007)
-4	0.729	0.0973	-0.0528	-2.28e-05
	(16.17)	(0.0433)	(0.103)	(0.0006)
-3	7.533	0.0664	-0.0666	-9.37e-05
	(15.11)	(0.0390)	(0.0899)	(0.0005)
-2	6.062	0.0366	0.0095	-0.0004
	(10.11)	(0.0270)	(0.0586)	(0.0004)
-1	-	-	-	-
0	-0.955	0.0138	0.0086	-0.0001
	(9.616)	(0.0282)	(0.0611)	(0.0004)
1	-0.714	-0.0101	0.109	-0.0010
	(15.16)	(0.0417)	(0.0963)	(0.0006)
2	9.529	-0.0209	0.0953	-0.0006
	(17.17)	(0.0449)	(0.108)	(0.0008)
3	12.85	-0.0250	0.0944	-0.0006
	(19.30)	(0.0485)	(0.121)	(0.0011)
4	33.33	0.0009	0.0363	-0.0008
	(20.46)	(0.0492)	(0.130)	(0.0013)
5	10.25	0.0077	-0.0874	-0.0009
	(21.75)	(0.0536)	(0.138)	(0.0015)
6	-3.161	-0.0199	-0.152	-0.0014
	(21.90)	(0.0553)	(0.138)	(0.0017)
7	-6.121	-0.0056	-0.173	-0.0022
	(23.42)	(0.0589)	(0.151)	(0.0019)
8	-12.42	-0.0433	-0.167	-0.0026
	(24.08)	(0.0611)	(0.157)	(0.0020)
9	12.80	-0.0033	-0.0587	-0.0021
	(26.02)	(0.0653)	(0.166)	(0.0022)
10	8.704	0.0064	-0.155	-0.0023
	(26.96)	(0.0659)	(0.171)	(0.0023)

Notes: DD coefficients from estimation of Equation 2.3 on establishment-level outcomes. Standard errors clustered by zip code. N=678,883.

Table B.12: Unemployed/ Informal Sector: Alternative Treatment and Control Groups

Time to Treat	*Base*	*Alternative Treatment*	*Alternative Control*	*Alternative Treatment & Control*
	(1)	(2)	(3)	(4)
-10	0.0008	-0.0004	0.0031	0.0014
	(0.0012)	(0.0010)	(0.0011)	(0.0009)
-9	0.0003	-0.0008	0.0024	0.0015
	(0.0011)	(0.0010)	(0.0010)	(0.0009)
-8	0.0002	-0.0011	0.0019	0.0015
	(0.0009)	(0.0009)	(0.0009)	(0.0008)
-7	0.0007	-0.0009	0.0021	0.0016
	(0.0009)	(0.0009)	(0.0009)	(0.0008)
-6	0.0002	-0.0013	0.0016	0.0015
	(0.0008)	(0.0008)	(0.0008)	(0.0007)
-5	0.0000	-0.0013	0.0013	0.0015
	(0.0007)	(0.0008)	(0.0008)	(0.0007)
-4	-0.0005	-0.0011	0.0009	0.0011
	(0.0007)	(0.0007)	(0.0007)	(0.0006)
-3	-0.0004	-0.0008	0.0003	0.0002
	(0.0007)	(0.0007)	(0.0006)	(0.0006)
-2	0.0004	0.0005	0.0003	0.0001
	(0.0004)	(0.0003)	(0.0004)	(0.0003)
0	0.0009	0.0003	0.0010	0.0007
	(0.0005)	(0.0005)	(0.0004)	(0.0003)
1	0.0012	0.0006	0.0013	0.0008
	(0.0006)	(0.0006)	(0.0005)	(0.0005)
2	0.0007	0.0006	0.0013	0.0009
	(0.0007)	(0.0006)	(0.0006)	(0.0005)
3	0.0010	0.0005	0.0018	0.0009
	(0.0007)	(0.0006)	(0.0006)	(0.0005)
4	0.0012	5.05e-05	0.0016	0.0007
	(0.0008)	(0.0006)	(0.0007)	(0.0005)
5	0.0011	-0.0002	0.0013	0.0005
	(0.0008)	(0.0007)	(0.0007)	(0.0005)
6	0.0011	0.0001	0.0015	0.0005
	(0.0008)	(0.0007)	(0.0008)	(0.0005)
7	0.0013	0.0008	0.0017	0.0007
	(0.0008)	(0.0007)	(0.0008)	(0.0006)
8	0.0013	0.0009	0.0018	0.0008
	(0.0009)	(0.0007)	(0.0008)	(0.0006)
9	0.0013	0.0006	0.0024	0.0009
	(0.0009)	(0.0008)	(0.0008)	(0.0006)
10	0.0010	-4.97e-05	0.0023	0.0006
	(0.0009)	(0.0008)	(0.0009)	(0.0006)
Treatment	0-500	0-300	0-500	0-400
Control	500-1000	500-1000	600-1000	600-1000
Obs	5,269,793	4,980,147	4,996,174	4,917,463

Notes: DD coefficients from estimation of Equation 2.1 on the probability of being unemployed or working in the informal sector using an alternative treatment group (Column 2), an alternative control group (Column 3) and both (Column 4). Standard errors clustered by zip code.

Table B.13: Weekly Labor Earnings: Alternative Treatment and Control Groups

Time to Treat	*Base* (1)	*Alternative Treatment* (2)	*Alternative Control* (3)	*Alternative Treatment & Control* (4)
-10	4.870	4.829	5.665	6.003
	(4.080)	(3.635)	(4.058)	(5.827)
-9	0.0432	-0.804	1.818	0.690
	(4.997)	(5.387)	(4.543)	(4.791)
-8	2.347	1.598	3.803	2.892
	(3.636)	(3.624)	(3.422)	(4.035)
-7	5.208	5.245	5.309	3.150
	(3.448)	(3.591)	(3.224)	(3.855)
-6	3.166	3.386	2.672	0.518
	(3.140)	(3.337)	(2.965)	(3.102)
-5	0.811	-1.172	0.227	-0.602
	(3.243)	(3.935)	(3.290)	(3.198)
-4	0.987	1.775	-0.537	-1.555
	(3.299)	(4.293)	(2.964)	(2.959)
-3	1.231	0.656	-0.146	0.00623
	(2.558)	(3.394)	(2.509)	(2.799)
-2	3.913	7.317	-0.457	-0.243
	(4.501)	(7.452)	(1.749)	(1.968)
0	-2.987	-3.382	-0.894	-2.539
	(2.234)	(3.107)	(1.978)	(1.931)
1	-2.842	-1.961	-0.870	-5.761
	(2.861)	(3.428)	(2.802)	(2.102)
2	-4.013	-6.077	-3.947	-8.029
	(3.114)	(3.261)	(3.304)	(2.573)
3	-8.018	-11.96	-6.579	-12.76
	(3.300)	(3.683)	(3.649)	(4.325)
4	-9.269	-11.49	-6.873	-12.08
	(3.675)	(4.152)	(4.052)	(4.938)
5	-12.74	-15.84	-8.311	-13.5
	(3.974)	(4.057)	(4.398)	(5.132)
6	-15.13	-17.28	-12.22	-16.53
	(4.432)	(4.495)	(5.031)	(5.596)
7	-16.68	-15.77	-16.2	-16.06
	(4.652)	(4.497)	(5.699)	(6.197)
8	-18.33	-19.15	-14.85	-16.13
	(5.210)	(4.660)	(6.630)	(6.693)
9	-17.86	-18.02	-14.15	-15.61
	(5.648)	(5.045)	(6.626)	(6.626)
10	-20.01	-17.82	-15.5	-16.19
	(6.268)	(5.790)	(6.490)	(7.012)
Treatment	0-500	300	500	0-400
Control	500-1000	500-1000	600-1000	600-1000
Obs	3,746,367	3,456,721	3,472,748	3,394,037

Notes: DD coefficients from estimation of Equation 2.1 on weekly labor earnings using an alternative treatment group (Column 2), an alternative control group (Column 3) and both (Column 4). Standard errors clustered by zip code.

Table B.14: Weekly Hours Worked: Alternative Treatment and Control Groups

Time to Treat	*Base* (1)	*Alternative Treatment* (2)	*Alternative Control* (3)	*Alternative Treatment & Control* (4)
-10	0.0168	0.0611	0.0176	-0.0248
	(0.0271)	(0.0283)	(0.0277)	(0.0353)
-9	0.0358	0.0628	0.0366	0.0108
	(0.0323)	(0.0348)	(0.0310)	(0.0316)
-8	0.0237	0.0245	0.039	-0.006
	(0.0232)	(0.0239)	(0.0225)	(0.0271)
-7	0.0113	0.0221	0.0249	-0.0107
	(0.0216)	(0.0227)	(0.0218)	(0.0254)
-6	0.0191	0.0304	0.0354	0.00443
	(0.0204)	(0.0218)	(0.0198)	(0.0221)
-5	0.0291	0.0428	0.0362	0.00877
	(0.0194)	(0.0214)	(0.0200)	(0.0208)
-4	0.0229	0.0211	0.0313	0.0177
	(0.0194)	(0.0225)	(0.0176)	(0.0179)
-3	0.0197	0.0209	0.0245	0.0101
	(0.0169)	(0.0176)	(0.0161)	(0.0170)
-2	-0.0196	-0.0264	0.0227	0.0134
	(0.0234)	(0.0368)	(0.0140)	(0.0142)
0	-0.0038	0.0171	-0.0102	-3.56e-05
	(0.0122)	(0.0143)	(0.0120)	(0.0120)
1	-0.0136	0.0241	-0.0262	-0.00519
	(0.0150)	(0.0162)	(0.0175)	(0.0181)
2	-0.0387	0.0181	-0.042	-0.0224
	(0.0189)	(0.0209)	(0.0211)	(0.0218)
3	-0.0144	0.0310	-0.0355	-0.0128
	(0.0197)	(0.0215)	(0.0219)	(0.0242)
4	-0.0043	0.0432	-0.0288	-0.0086
	(0.0209)	(0.0221)	(0.0222)	(0.0263)
5	-0.0031	0.0503	-0.0231	-0.0003
	(0.0216)	(0.0228)	(0.0222)	(0.0245)
6	-0.0038	0.0578	-0.0347	-0.0193
	(0.0224)	(0.0239)	(0.0222)	(0.0248)
7	0.0111	0.0707	-0.0292	-0.0398
	(0.0237)	(0.0254)	(0.0227)	(0.0255)
8	0.0131	0.083	-0.0305	-0.0471
	(0.0244)	(0.0257)	(0.0234)	(0.0271)
9	0.0093	0.0787	-0.0448	-0.0671
	(0.0259)	(0.0267)	(0.0244)	(0.0284)
10	-0.0009	0.0776	-0.0504	-0.0776
	(0.0276)	(0.0285)	(0.0250)	(0.0315)
Treatment	0-500	0-300	0-500	0-400
Control	500-1000	500-1000	600-1000	600-1000
Obs	3,746,367	3,456,721	3,472,748	3,394,037

Notes: DD coefficients from estimation of Equation 2.1 on weekly hours worked using an alternative treatment group (Column 2), an alternative control group (Column 3) and both (Column 4). Standard errors clustered by zip code.

Table B.15: Hourly Wage: Alternative Treatment and Control Groups

Time to Treat	*Base*	*Alternative Treatment*	*Alternative Control*	*Alternative Treatment & Control*
	(1)	(2)	(3)	(4)
-10	0.116	0.115	0.135	0.143
	(0.0971)	(0.0866)	(0.0966)	(0.139)
-9	0.00103	-0.0191	0.0433	0.0164
	(0.119)	(0.128)	(0.108)	(0.114)
-8	0.0559	0.0380	0.0905	0.0689
	(0.0866)	(0.0863)	(0.0815)	(0.0961)
-7	0.124	0.125	0.126	0.0750
	(0.0821)	(0.0855)	(0.0768)	(0.0918)
-6	0.0754	0.0806	0.0636	0.0123
	(0.0748)	(0.0795)	(0.0706)	(0.0739)
-5	0.0193	-0.0279	0.00540	-0.0143
	(0.0772)	(0.0937)	(0.0783)	(0.0761)
-4	0.0235	0.0423	-0.0128	-0.0370
	(0.0786)	(0.102)	(0.0706)	(0.0705)
-3	0.0293	0.0156	-0.00348	0.0001
	(0.0609)	(0.0808)	(0.0597)	(0.0666)
-2	0.0932	0.174	-0.0109	-0.0058
	(0.107)	(0.177)	(0.0416)	(0.0469)
0	-0.0711	-0.0805	-0.0213	-0.0605
	(0.0532)	(0.0740)	(0.0471)	(0.0460)
1	-0.0677	-0.0467	-0.0207	-0.137
	(0.0681)	(0.0816)	(0.0667)	(0.0500)
2	-0.0955	-0.145	-0.0940	-0.191
	(0.0741)	(0.0776)	(0.0787)	(0.0613)
3	-0.191	-0.285	-0.157	-0.304
	(0.0786)	(0.0877)	(0.0869)	(0.103)
4	-0.221	-0.274	-0.164	-0.288
	(0.0875)	(0.0989)	(0.0965)	(0.118)
5	-0.303	-0.377	-0.198	-0.321
	(0.0946)	(0.0966)	(0.105)	(0.122)
6	-0.36	-0.411	-0.291	-0.394
	(0.106)	(0.107)	(0.120)	(0.133)
7	-0.397	-0.375	-0.386	-0.382
	(0.111)	(0.107)	(0.136)	(0.148)
8	-0.437	-0.456	-0.353	-0.384
	(0.124)	(0.111)	(0.158)	(0.159)
9	-0.425	-0.429	-0.337	-0.372
	(0.134)	(0.120)	(0.158)	(0.158)
10	-0.476	-0.424	-0.369	-0.385
	(0.149)	(0.138)	(0.155)	(0.167)
Treatment	0-500	0-300	0-500	0-400
Control	500-1000	500-1000	600-1000	600-1000
Obs	3,746,367	3,456,721	3,472,748	3,394,037

Notes: DD coefficients from estimation of Equation 2.1 on hourly wage using an alternative treatment group (Column 2), an alternative control group (Column 3) and both (Column 4). Standard errors clustered by zip code.

Table B.16: Effect on Labor Mobility Outcomes

Time toTreat	Prob Switching Establishment (1)	Establishment-Specific Wage Premium (2)	Prob Switching Municipality (3)
-10	0.0049	0.0296	-0.0036
	(0.0029)	(0.0475)	(0.0042)
-9	0.0037	0.0355	-0.0019
	(0.0024)	(0.0479)	(0.0040)
-8	0.0043	0.0969	-0.0034
	(0.0027)	(0.0427)	(0.0037)
-7	0.0037	0.0981	-0.0028
	(0.0022)	(0.0415)	(0.0037)
-6	0.0046	0.0786	-0.0034
	(0.0026)	(0.0377)	(0.0034)
-5	0.003	0.0770	-0.0045
	(0.0021)	(0.0343)	(0.0031)
-4	0.0048	0.0575	-0.0026
	(0.0024)	(0.0291)	(0.0026)
-3	0.0019	0.0355	-0.0031
	(0.0017)	(0.0245)	(0.0022)
-2	0.0009	0.0215	-0.0018
	(0.0025)	(0.0161)	(0.0010)
-1	-	-	-
0	0.0018	-0.0025	0.00419
	(0.0024)	(0.0197)	(0.0014)
1	-0.0007	-0.0138	0.0071
	(0.0019)	(0.0225)	(0.0021)
2	0.0009	-0.0091	0.0087
	(0.0026)	(0.0311)	(0.0024)
3	-0.0003	-0.0174	0.011
	(0.0023)	(0.0379)	(0.0026)
4	0.0039	-0.0734	0.011
	(0.0029)	(0.0426)	(0.0027)
5	-0.0009	-0.124	0.012
	(0.0022)	(0.0422)	(0.0029)
6	-0.0007	-0.183	0.0098
	(0.0029)	(0.0447)	(0.0031)
7	-0.0031	-0.206	0.0092
	(0.0022)	(0.0476)	(0.0034)
8	0.0008	-0.226	0.0077
	(0.0027)	(0.0499)	(0.0035)
9	-0.0048	-0.226	0.0070
	(0.0023)	(0.0531)	(0.0037)
10	-0.0017	-0.231	0.0069
	(0.0027)	(0.0576)	(0.0039)
Baseline Mean	0.33	0.59	0.11
Obs	3,746,367	3,746,367	3,746,367
R-Sq	0.585	0.632	0.588

Notes: DD coefficients from estimation of Equation 2.1 on the probability of switching establishments (Column 1), municipality (Column 3). DD coefficients from estimation of Equation 2.5 on establishment-specific wage premiums (Column 2). Standard errors clustered by zip code.

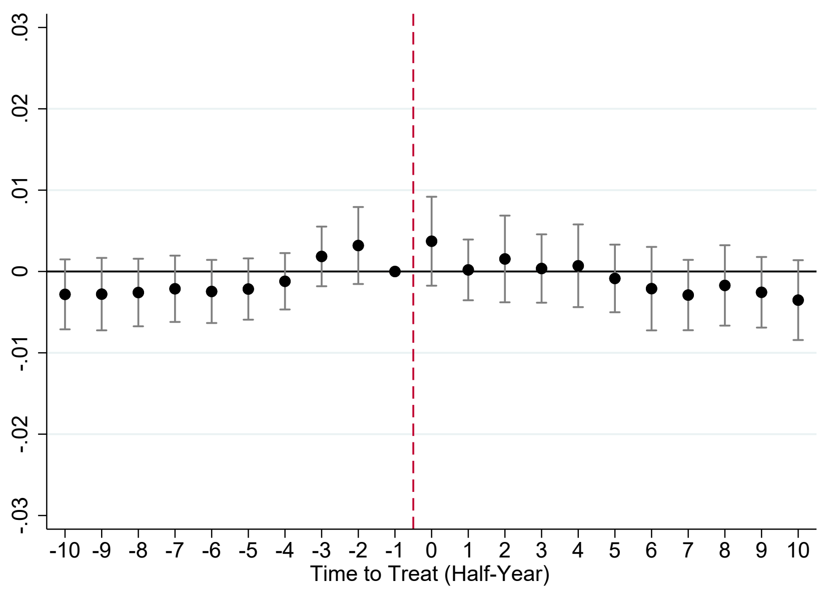


Figure B.2: Effects on the Probability of Switching Occupation

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of switching occupations. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment.

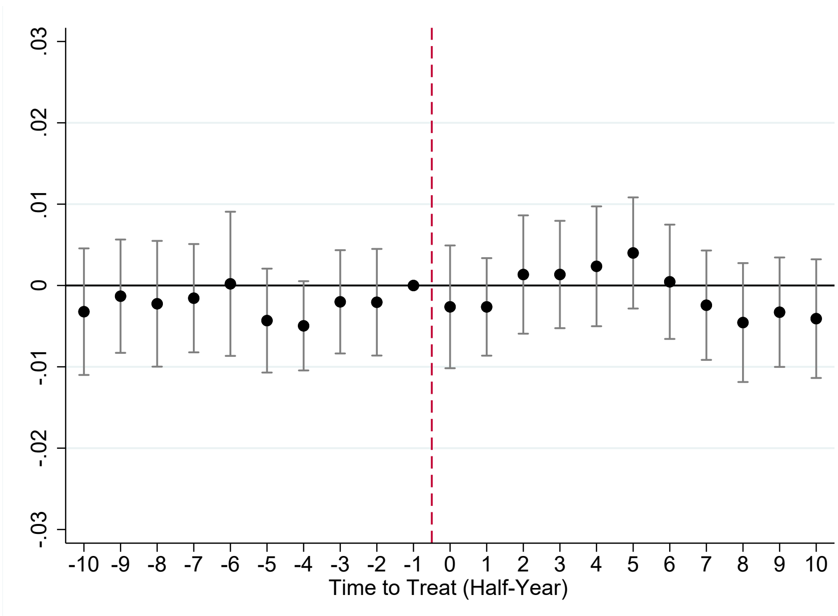


Figure B.3: Effects on the Probability of Switching Industry

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of switching industry. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment.

Table B.17: Effects on Switching Industry and Occupation

Time to Treat	Prob Switching Industry (1)	Prob Switching Occupation (2)
-10	-0.003	-0.003
	(0.003)	(0.002)
-9	-0.001	-0.003
	(0.003)	(0.002)
-8	-0.002	-0.003
	(0.003)	(0.002)
-7	-0.002	-0.002
	(0.003)	(0.002)
-6	0.0002	-0.003
	(0.003)	(0.002)
-5	-0.004	-0.002
	(0.003)	(0.002)
-4	-0.005	-0.001
	(0.002)	(0.002)
-3	-0.002	0.002
	(0.002)	(0.001)
-2	-0.002	0.003
	(0.003)	(0.002)
-1	-	-
0	-0.003	0.003
	(0.003)	(0.002)
1	-0.003	0.0002
	(0.002)	(0.002)
2	0.001	0.002
	(0.003)	(0.002)
3	0.001	0.0004
	(0.003)	(0.002)
4	0.002	0.0007
	(0.003)	(0.002)
5	0.004	-0.001
	(0.003)	(0.002)
6	0.0005	-0.002
	(0.002)	(0.002)
7	-0.002	-0.003
	(0.003)	(0.002)
8	-0.005	-0.001
	(0.003)	(0.002)
9	-0.003	-0.003
	(0.003)	(0.002)
10	-0.005	-0.004
	(0.003)	(0.001)
Baseline Mean	0.33	0.11
Obs	3,746,367	3,746,367
R-Sq	0.585	0.588

Notes: DD coefficients from estimation of Equation 2.1 on the probability of switching establishments (Column 1), municipality (Column 3). DD coefficients from estimation of Equation 2.5 on establishment-specific wage premiums (Column 2). Standard errors clustered by zip code.

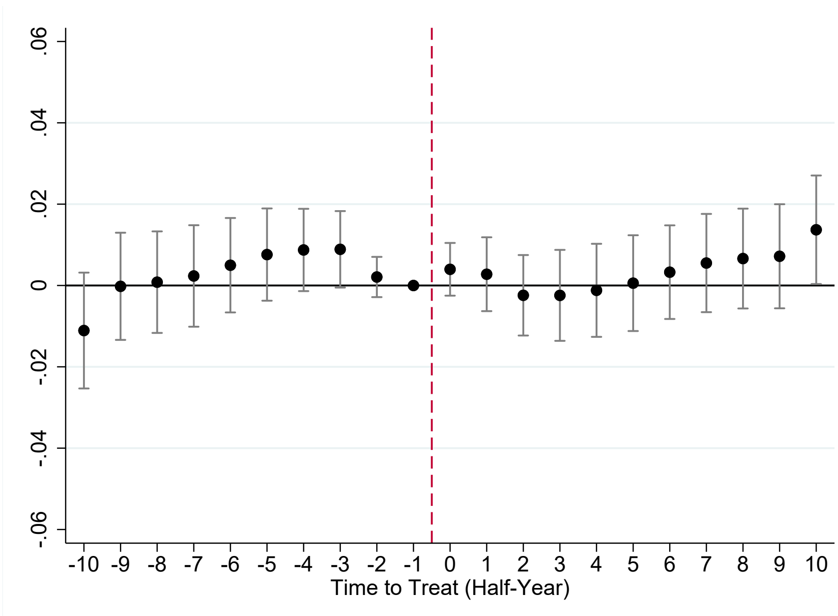


Figure B.4: Probability of Resigning Job

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the probability of resigning the current job. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment.

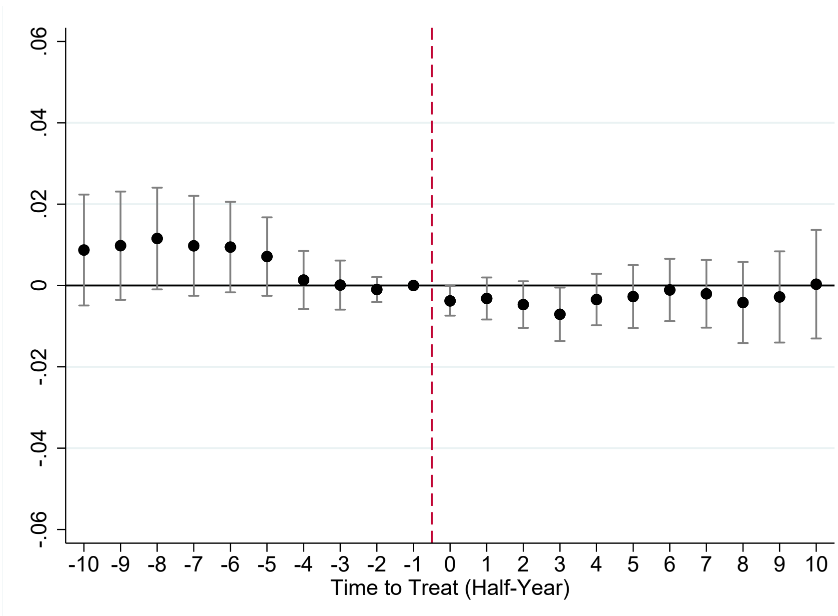


Figure B.5: Average Municipality Wage

Notes: Graph shows DD coefficients and 95 percent confidence intervals from estimation of Equation 2.1 on the the average municipality wage. Standard errors clustered by zip code. Treatment (Control) defined as employees working in an establishment within 0-500 (500-1000) meters from a homicide. Red vertical line represents time of treatment.

Table B.18: Effects on Switching Municipality: Skilled and Unskilled Workers

Time to Treat	Unskilled Workers	Skilled Workers
	(1)	(2)
-10	-0.0047	0.0004
	(0.0049)	(0.0045)
-9	-0.0035	0.0021
	(0.0048)	(0.0041)
-8	-0.0061	0.0028
	(0.0044)	(0.0038)
-7	-0.0046	0.0020
	(0.0044)	(0.0035)
-6	-0.0058	0.0015
	(0.0040)	(0.0032)
-5	-0.0068	0.0000
	(0.0035)	(0.0029)
-4	-0.0039	-0.0003
	(0.0029)	(0.0025)
-3	-0.00374	-0.0017
	(0.0025)	(0.0022)
-2	-0.0018	-0.0014
	(0.0011)	(0.0012)
-1	-	-
0	0.0044	0.0029
	(0.0016)	(0.0015)
1	0.0076	0.0042
	(0.0023)	(0.0022)
2	0.0089	0.0057
	(0.0024)	(0.0025)
3	0.0112	0.0089
	(0.0026)	(0.0028)
4	0.0106	0.0092
	(0.0029)	(0.0029)
5	0.0111	0.0104
	(0.0033)	(0.0031)
6	0.0079	0.0116
	(0.0035)	(0.0032)
7	0.0067	0.0131
	(0.0039)	(0.0032)
8	0.0049	0.0122
	(0.0043)	(0.0034)
9	0.0044	0.0109
	(0.0046)	(0.0037)
10	0.0047	0.0097
	(0.0047)	(0.0038)
Obs	2,510,065	1,236,302
R-Sq	0.612	0.559

Notes: DD coefficients from estimation of Equation 2.1 on the probability of switching municipality for unskilled (Column 1) and skilled (Column 2) workers. Standard errors clustered by zip code.

Table B.19: Effects on Switching to Farther Away and Lower Crime Municipalities

Time to Treat	Prob Farther Away Municipality (1)	Prob Lower Crime Municipality (2)
-10	-0.0012	-0.0014
	(0.0019)	(0.0027)
-9	-0.0012	-0.0009
	(0.0019)	(0.0026)
-8	-0.0016	-0.0021
	(0.0018)	(0.0023)
-7	-0.0019	-0.0016
	(0.0018)	(0.0024)
-6	-0.0025	-0.0027
	(0.0017)	(0.0021)
-5	-0.0021	-0.0041
	(0.0017)	(0.0017)
-4	-0.0013	-0.0025
	(0.0014)	(0.0014)
-3	-0.0015	-0.0025
	(0.0017)	(0.0012)
-2	-0.0007	-0.0019
	(0.0005)	(0.0006)
-1	-	-
0	0.0009	0.0017
	(0.0008)	(0.0008)
1	0.0015	0.0023
	(0.0010)	(0.0011)
2	0.0030	0.0039
	(0.0011)	(0.0013)
3	0.0045	0.0055
	(0.0012)	(0.0015)
4	0.0041	0.0055
	(0.0013)	(0.0015)
5	0.0047	0.0058
	(0.0014)	(0.0017)
6	0.0037	0.0047
	(0.0015)	(0.0019)
7	0.0037	0.0042
	(0.0016)	(0.0021)
8	0.0025	0.0024
	(0.0019)	(0.0025)
9	0.0014	0.0016
	(0.0022)	(0.0027)
10	0.0006	0.0009
	(0.0025)	(0.0029)
Baseline Mean	0.09	0.09
Obs	3,746,367	3,746,367
R-Sq	0.551	0.609

Notes: DD coefficients from estimation of Equation 2.1 on the probability of switching to establishments located in municipalities farther away from São Paulo ($> 50km$) (Column 1) and municipalities with a lower homicide rate than São Paulo (Column 2). Standard errors clustered by zip code.

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