December 2018

Three Essays on Labor and Public Economics

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Abstract

This dissertation consists of three chapters in the area of applied microeconomics. Using a variety of quasi-experimental research designs, they study the labor market effects of a wage subsidy in a highly informal economy, the impact of a tax-breaks program on small formal firms' entrance decision and the probability of survival, and the impact of exposure to neighborhood crime on school absenteeism, respectively.

The first chapter studies the labor market effects of a wage subsidy introduced in Colombia's First Job Act. It exploits changes in the labor earnings distribution to measure how the policy change affected net employment, the relative sizes of the formal and informal labor markets, and the subsidy's excess burden. The results indicate the policy caused sizable shifts of workers across the formal and informal labor markets, but relatively little net employment growth. The policy's marginal excess burden ranges from 0.1% to 4% of corporate income tax revenues, which the paper argues represent a lower bound for what the benefits of moving workers across sectors should be, for the policy to be welfare enhancing.

The second chapter, coauthored with Julio Romero, studies how taxes affect small formal firms' entrance decision, the probability of survival, employment and average per-worker compensation. It exploits variation in tax treatment from a cohort-based, tax breaks program implemented in Colombia in 2010. Exploiting the fact that to qualify firms must have registered after December 31, 2010, it compares firms created soon before the cut-off date, to firms created soon after. It finds the availability of the tax breaks did not affect any of the outcomes considered.

The third chapter, coauthored with Amy Ellen Schwartz, studies whether exposure to neighborhood violence causes school absenteeism. Exploiting variation in the timing of violent crimes, it compares absenteeism days immediately after exposure, to days immediately before. It finds an increase in average absenteeism after exposure of 5% to 10%. The response is statistically significant for both genders, most race/ethnic groups, and grade levels, and varies by violent crime type, being stronger for cases of exposure to homicides.

Students exposed multiple times respond strongly to a second event, regardless of the violent crime type, but not to the subsequent ones.

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Dissertation

Submitted in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy in Economics

Syracuse University

December, 2018

Acknowledgments

I would like to thank my advisor, Professor Jeffrey Kubik, and my boss, co-author and in many instances co-advisor, Professor Amy Ellen Schwartz for their guidance and support throughout this journey.

I would like to thank Professor Hugo Jales for the many constructive discussions about research ideas and methods. Your passion for teaching and research is inspiring, and I will see with joy the many successes that are ahead of you in your career.

I would like to thank my dissertation committee members, Professor Amy Ellen Schwartz, Professor Perry Singleton, Professor Alfonso Flores-Lagunes, Professor Hugo Jales, and Professor Katherine Michelmore for their support throughout this process.

I would like to thank my friends and career mentors Alicia Bozzi, Jaime Bonet, Javier Baez, Juan Sebastian Llerar and Brian Kovak. Without your example, guidance and support I would not be writing these words.

I would like to thank my friends Valentina, Catalina, Daniel, Jonathan, Dany, Pedro, Carlos, Sergio, Ricardo, Boqian, Laura, Yusun, Michelle, Emily, Diana, Carlos, Julie, Pipe, Melissa, Jerome, Lucia, Jud, Agustina, among many others, for their moral support all these years. I am blessed for your friendship and will be forever grateful.

I would like to thank my cohort-mates, Lin, Shulin, Yang, Boqian, Jindong, Jaewoo, and Serkan. I have learned a lot over these years from your kindness, work ethic, and tenacity. Best wishes to all of you.

Special thanks to my parents, Fabio and Tere, my brother Rodrigo and my sister Maria Alejandra for their unconditional love, as well as their moral and emotional support.

Special thanks to God, who blessed me and gave me strength at all times, but especially the hard ones, that were many throughout these years.

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Chapter 1

Wage Subsidies in Labor Markets with High Informality: Evidence from Colombia's First Job Act

1.1 Introduction

Wage subsidies are a widely used policy tool aimed at putting to work people with poor employment prospects. A critical question is whether they succeed in creating new jobs, or increase employment of eligible workers at the expense of non-eligible ones. The effectiveness and efficiency of a wage subsidy largely depends on the answer to this question; however, credibly identified evidence on the extent of workers' displacement is relatively scarce.[1]

[1] Katz (1996) assesses evidence on workers' displacement as either inconclusive or non-credible, based on studies of wage subsidy programs in the United States and other OECD countries. Kluve (2016) remarks workers' displacement is the aspect of wage subsidies which we known the least about. Three studies that look explicitly at displacement effects are Crepon et al. (2013), Gautier et al. (2014) and Martins and e Costa (2014). They address the question in the context of job search assistance programs. The first two use field experiments carefully design to answer the question, and find evidence of displacement effects, while the latter exploits discontinuities in program rules, and finds no evidence of displacement.

In developing economies, where a substantial portion of the labor market operates informally, workers move across sectors too, making more uncertain the subsidy's potential net employment effect. Even if the policy objective is putting people to work in the formal sector, it does not preclude the possibility of displacing non-eligible workers, or subsidizing wages of individuals who would have had a formal job anyway.

This paper investigates how a wage subsidy introduced in Colombia's First Job Act affected the labor market. It allows formal firms to rebate in the form of tax credits from the corporate income tax, 11.0 percentage points of payroll taxes paid on newly hired workers either younger than 28 years old, or with labor earnings between one and one and a half minimum wages. By introducing discontinuous changes in the marginal payroll tax rate, the subsidy creates a strong incentive for formal firms to locate their newly hired employees in the 'correct' part of the distribution. If only job creation occurs, mass must increase in the parts of the formal sector labor earnings and/or age distributions where the subsidy applies, and no change anywhere else. If displacement of non-eligible formal workers takes place, mass must drop in areas adjacent to the targeted one; if informal workers shift sector to take subsidized jobs, they should come from an area similar to the affected one in the formal sector distributions. The presence and extent of mass changes is tested using individual level data on sector, and labor earnings and age from the Colombia's Continuous Household Survey, a cross-sectional study representative for the entire labor market.

I estimate the empirical formal and informal sector labor earnings and age distributions using Kernel methods. Contrasting them before and after the program started shows mass increases in the formal sector in the earnings' range where the subsidy applies, and drops in areas neighboring it. In the informal sector mass drops around the minimum wage, and increases around one and a half minimum wages. Meanwhile, there is no sign of firms responding on the age margin. In addition to showing firms responded to the policy, this simple exercise reveals ignoring or assuming away displacement of non-eligible workers, draw an incomplete picture of the subsidy's impact on the labor market.

Next, I exploit mass changes to assess the subsidy's impact on employment. In the absence of distortions, excess mass in the formal sector distribution gives total employment growth. Missing mass in areas adjacent to the targeted one, permit to account for within formal sector displacement of workers, and in a similar manner, mass changes in the informal sector for displacement between sectors. To estimate mass changes I bootstrapped the following procedure. I reestimate the empirical distribution for the pre-subsidy period, this time fitting a flexible polynomial, and use the parameters to predict the number of workers per earnings level in the post-policy period. The difference between observed and predicted number of workers gives the mass change. I find an 8% increase in employment in the part of the distribution targeted by the policy. Former informal workers took 75% of these jobs, the formal sector displaced workers 15%, and job seekers the remaining 10%.

Unless there are welfare gains from inducing the shift of workers across sectors and earnings levels, the subsidy's excess burden must be substantial. I derive a deadweight loss formula from a static, general equilibrium model that allows for labor market's segmentation, heterogeneity in workers ability and firm's productivity, and introduces a wage subsidy that only covers the formal labor market. The formula is a function of the change in the probability of holding a formal job, and the slope of the labor demand curve. I recover the latter from the subsidy's effect on employment, and use a difference-in-difference strategy, where parts of the distribution unaffected by the subsidy control for economy-wide trends and contemporaneous shocks, to estimate the change in the probability of working formally. Computation of the formula shows the subsidy's marginal excess burden ranges from 0.1% to 4.0% of corporate tax revenues, or, in per-capita terms, from U$30 to U$2,000 (PPP).

Attributes of formal jobs such as safer working conditions, access to health insurance and to a retirement pension could render social benefits if, for example, they improve workers health, productivity and lifetime consumption. Although estimating the benefits of formality is out of the scope of this study, the deadweight loss calculation indirectly informs it since it serves as a lower bound for what the benefits should be for the subsidy to be welfare enhancing.

The results indicate the subsidy caused sizable shifts across the formal and informal labor markets, and to a lesser extent across earnings levels, but relatively little net employment growth. Therefore, this study suggests more attention must be given to general equilibrium effects when subsidizing wages, and to the welfare implications they might have. If the primary policy objective was creating new jobs, the program is ineffective and highly inefficient. Nonetheless, if the objective was creating formal jobs, the effectiveness assessment might turn positive, and its efficiency will depend on whether external benefits accrue from shifting workers across sectors, and if so, the extent of them.

The remaining sections of the paper are organized as follows. The next section describes the wage subsidy program, firm's expected response, and data used in the empirical analyses. Section 1.3 shows firms in fact responded, estimates the policy net impact on employment, and the changes it induced in the relative sizes of the formal and informal labor markets. Section 1.4 estimates the policy's marginal excess burden, Section 1.5 discusses the results, and Section 1.6 concludes.

1.2 Institutional Setting, Conceptual Framework and Data

1.2.1 Colombia's First Job Act

In December 2010, Colombia's Congress passed Law 1429, also known as the First Job Act. It introduced a tax credit equivalent to 11.0 percentage points of payroll taxes paid on newly hired workers. The credit can be claimed for two consecutive fiscal years, as long as newly hired workers: i) increase the number of employees on payroll, and the firm's wage bill; and ii) if male, are younger than 28 years old, if female, are younger than 28 or older than 40 years old, or earn between one and one and a half minimum wages.

In addition to the section containing the tax credit, the Act has three more. One mandates the government to put in place microcredit and training programs to support young entrepreneurs. Another introduces tax breaks for newly registered small firms, and the last one eliminates a set of labor, commercial, legal and accounting procedures formal firms were required to conduct with certain regularity. None of the other Act's measures confound the intended effect of the tax credit. New small firms benefiting from the tax breaks might delay their response to the credit, but it should not obscure any aggregate effect since most (around two thirds) formal wage-employees work at firms with more than 50 workers.

Economic incentives to secure formal jobs for new labor market entrants was part of the campaign promises of the winning political coalition in the 2010 election cycle. The media reported on the proposal, but presented it in terms of desired outcomes, not specifics. The latter became known when it reached congress[2] in the second half of 2010, and was subject to changes as it passed through the legislative process. Moreover, the Act's name and its actual content have a weak match, therefore it is unlikely anticipatory responses are of great extent.

A tax reformed passed in late 2012 reduced payroll taxes for firms. For the First Job Act tax credit rates, it implies they dropped from 11 to 4.5 percentage points starting in 2013. Even though this policy change affected the credit's generosity, it leaves unaltered the identifying mechanism this paper exploits, which is the presence of discontinuities in the effective payroll tax rate across the age and labor earnings distributions.

1.2.2 Conceptual Framework

If labor demand shifts out in response to an incremental employer-side wage subsidy that targets a certain group of workers (e.g., low earning, or under 28 years old), and labor supply for this group is not perfectly inelastic, economic theory predicts an increase in em-

[2]The legislative year starts on July 20, and Presidential inaugurations take place on August 7. The first draft of the Act reached congress on August 19, 2010, was debated in late November, and signed into law on December 29, 2010.

ployment of targeted workers. Workers taking subsidized jobs can come from three places only: the pool of job seekers and non-participants, the pool of non-targeted workers, and in an economy with a large informal labor market, the pool of informal workers as well. The extent to which each of these sources supply workers depends on how substitutable they are with respect to the targeted group.

To illustrate this idea, consider a simple two sector model. Production in the *formal* sector uses unskilled (L_U), semiskilled (L_S) and skilled workers (L_H), with production function:

$$Y = F(L_U, L_S, L_H) \tag{1.1}$$

while in the *informal* sector production is carried out using unskilled workers only:

$$X = G(L_U) \tag{1.2}$$

Labor demand depends on the wage:

$$L^*_{s,j} = L_{s,j}(w^*_j) \tag{1.3}$$

where s indexes sector, and j skill level.[3]

For simplicity, assume the supply of all type of labor is fixed: $\bar{L} = L_U + L_S + L_H$, with $L_U = L^f_U + L^i_U$. The government decides to introduce a wage subsidy for formal unskilled workers. After log-linearizing and totally differentiating (3), the across and within sector substitution of unskilled workers are:

$$\hat{L}^i_U - \hat{L}^f_U = \sigma_1(w^f_U - \delta - w^i_U) \tag{1.4}$$

$$\hat{L}^f_S - \hat{L}^f_U = \sigma_2(w^f_U - \delta - w^f_S) \tag{1.5}$$

[3]Galiani and Weinschelbaum (2012) model firms sector and labor demand decisions. In the model production is a function of labor and managerial ability. The model predicts sorting; firms with lower managerial ability become informal. It also predicts informal firms are smaller, and hire unskilled workers.

$$\hat{L}_H^f - \hat{L}_U^f = \sigma_3(w_U^f - \delta - w_H^f) \tag{1.6}$$

where σ_1, σ_2 and σ_3 are the elasticities of substitution between formal unskilled workers, and informal unskilled, formal semiskilled, and formal skilled workers, respectively. Therefore, as long as elasticities of substitution are different from zero, movement of workers across and within sectors result in an increase in the number of formal unskilled workers, and drops in the number of informal unskilled, formal semiskilled, and formal skilled workers.[4]

This framework helps in thinking about the effects of the subsidy in a world with a continuous distribution of skills. To begin with, it is safe to assume substitutability decreases the farther apart two given skill levels are. Thus, in the formal sector the number of targeted workers should increase, drop in skill levels in close proximity to it, and remain unchanged for skill levels sufficiently high in the distribution. In the informal sector, where the skills distribution is skewed towards the bottom, overlapping substantially with the skill range where jobs are subsidized, mass drops should also appear.

The fixed labor supply assumption implies the total number of workers taking subsidized jobs equals the sum of workers displaced from the informal sector and from other skill levels in the formal sector. Relaxing the assumption implies the equality no longer holds. This is the case since the shifts in labor demand alter the equilibrium wage, and with it the size of the pool of labor market participants. Therefore, in addition to the informal sector and other parts of the formal sector skills distribution, workers taking subsidized jobs can come from out of the market.

Taking the predictions to the wage subsidy under study, the density of formal work-

[4]Defining labor categories in terms of age instead of skill or ability, lead to qualitatively identical predictions. Let L_L and L_H denote young (e.g. $18 \leq age < 35$) and mature (e.g. $35 \leq age < 65$) workers, respectively. Assume all mature workers are equally productive, but within young workers productivity varies between unexperienced (e.g. $18 \leq age < 28$, and denoted by L_U) and experienced (e.g. $28 \leq age < 35$, and denoted by L_S) workers. Equation (1.4) to (1.6) hold, and a wage subsidy targeting young, unexperienced workers, increases labor demand for this group, and its share in total employment.

ers earning between one and one and a half minimum wages, and/or of formal workers younger than 28 years old must have increased. If the subsidy displaced workers within the formal sector, mass drops must appear in earnings regions adjacent to the targeted one, and/or near but to the right of 28 years old. If displacement occurred in the informal sector, the density must drop around one and one and a half minimum wages and/or below 28 years of age. It can be that the only density change occurs in the earnings and/or age regions targeted by the subsidy. If this is the case, all extra workers must have came from outside the market. Before taking these predictions to the data, the next subsection describes the dataset used to perform the empirical analyses.

1.2.3 Colombia's Continuous Household Survey

All empirical analyses in this paper use Colombia's Continuous Household Survey[5] for years 2008 to 2015. It is a nationally representative cross sectional survey conducted by DANE -which is a Spanish acronym for 'National Statistics Institute'- designed to monitor the Colombian labor market. It captures general demographic characteristics, and for all individuals 10 years old or older, asks a comprehensive set of questions about their labor market status. Demographic characteristics include gender, age, family composition, and educational attainment. Working-age individuals who are labor market participants, and have a job, report monthly earnings, hours of work, tenure, occupation, compliance with social security regulations, and firm size, among several other characteristics of their current job.

To classify workers as formal or informal I use questions about compliance with social security contributions: formal if comply, informal otherwise. To assess how accurately it approximates the sizes of the formal and informal labor markets, I check the extent of mass below the minimum wage in the formal sector and the fraction of formal public sector workers. Since no formal worker should earn less than the minimum wage, a good approximation should place very few individuals in that area. It is also reasonable to assume

[5] In Spanish, *Gran Encuesta Integrada de Hogares.*

the government complies with all labor regulations when acting as employer, thus, if measurement error is not systematic, the vast majority of public sector workers should be classified as formal. Figures 1.1b and 1.1c show this definition of formality places 95% or more of public sector workers in the formal sector, and less than 10% of formal workers below the minimum wage, respectively.

In the Colombian context, month is the reference period commonly used for salaries, even in the case of low paying jobs. Accordingly, the survey asks for monthly earnings. To express them in terms of 'minimum wages', the unit used in the First Job Act, I rescale them using the minimum wage series for the years under study.[6] The calculation is transparent, since a presidential decree adjusts annually the minimum wage, it covers all industries, and its nominal value is the same nationwide.

Years 2008 to 2010 serve as pre-policy period, and years 2012 to 2015 as post-policy period. Changes in the survey's methodology impede adding more pre-policy years. Nearly half the workers are formal (figure 1.1a). Consistent with La-Porta and Shleifer (2014) characterization of the formal and informal labor markets, in the latter, the average worker is less educated (8.2 years of education versus 12.5 of formal workers), less productive (almost 70% earn less than the minimum wage) and works at smaller firms (90% at firms of 5 or less employees, while that fraction among formal workers is 15%). Despite the differences, figures 1.1e and 1.1f show substantial overlap between the formal and informal labor earnings and age distributions. Lastly, figures 1.1c and 1.1d show the discontinuous jumps in the effective payroll tax rate occur in dense parts of the distributions; nearly 50% of formal workers earn between one and two minimum wages, and about 40% are in the 24 to 44 age range.

[6] The minimum wage series from 1984 to date can be downloaded from: http://www.banrep.gov.co/es/indice-salarios

1.3 Empirical Analysis

The first part of this section presents evidence formal firms responded to the policy. It does so by contrasting formal labor earnings and age distributions before and after the wage subsidy. Thereafter, I incorporate the informal sector into the analysis, and estimate the magnitude of the response in terms of net employment growth and change in the size of the formal sector.

1.3.1 Graphical Evidence

I estimate the empirical distributions using Kernel methods. The estimator uses an Epanechnikov kernel function, and a 'rule-of-thumb' approach for choosing the optimal bandwidth. All empirical distributions are based on counts of non-public sector, full time workers.

Figure 1.2a shows that the empirical labor earnings distribution exhibits the expected changes: i) a mass increase in the targeted region ($[MW - 1.5 * MW)$), and ii) mass drops to the left ($(0.0 - 0.8 * MW)$) and right ($[1.5 * MW - 1.9 * MW]$) of one, and one and a half minimum wages, respectively (figure 1.3 zooms in into each of these regions).

On the other hand, figure 1.2b shows formal firms did not respond on the age margin, a finding consistent with most of the existent evidence about active labor market programs that target young workers (see Katz (1996), Card et al. (2015), and Kluve (2016)). Proposed explanations for why labor demand is not responsive for this segment of the labor market include the relatively higher uncertainty employers have about young workers' skills, and their low levels of job attachment. In the Colombian context, another possible explanation is that other incentives to hire young workers might compete in generosity with the wage subsidy. It can be the case of the 'apprenticeship' contract. Legislated in 2002, it offers formal firms the option of paying below the minimum wage, plus a payroll tax discount, for up to two years for every apprentice they retain.[7]

[7]Another explanation for why formal employment of workers 28 years old or younger does not increase, is that labor supply for this segment of the labor market is perfectly inelastic. If this is the case, the shift in labor demand caused by the wage subsidy increases the equilibrium wage, but leaves employment un-

The rest of the paper focuses on the labor earnings margin. In terms of response dynamics, figure 1.4 shows: i) there is no sign of mass changes before the introduction of the wage subsidy, ii) emergence of excess mass in the targeted earnings region is not transitory, and iii) appearance of missing mass lags appearance of excess mass. Potential confounders are contemporaneuous industry-specific or region-specific policies. Appendix A.2 shows the described changes in the earnings distribution are present across most industries and regions.

1.3.2 Employment Effects

Workers forming the excess mass in the formal sector labor earnings distribution can only come from three places: i) other parts of the formal labor earnings distribution, ii) the informal sector, and iii) the pool of job-seekers. Regarding the first source, figues 1.2a and 1.3 show the subsidy caused the displacement of formal workers from earnings ranges adjacent to the targeted region.

With respect to displacement of informal workers, theory does not provide a sharp prediction where they should come from in the distribution. To procure an answer, I exploit two facts of the informal sector. First, its labor earnings distribution is less dispersed than its formal sector counterpart, with about 70% of informal workers earning less than the minimum wage, and over 90% earning less than two minimum wages. Therefore, by searching for mass changes over the $(0.0 - 2.0 * MW]$ range, I am covering almost the entire informal labor earnings distribution. Second, informal workers are more homogeneous, in terms of observable characteristics (e.g., age, education, family composition), than formal workers are. An informal worker earning the minimum wage is very much alike an informal worker earning two minimum wages, which is not true in the formal sector. Since informal workers are more homogeneous, they could come from below the minimum wage, but also come from higher parts of the distribution, since some might be willing to trade

changed. Appendix A.1 presents evidence neither employment nor labor earnings changed for the age group targeted by the subsidy.

off higher gross earnings for the mandated benefits attached to formal jobs.

Despite the above-mentioned, convenient facts, it remains true there is no theoretical guidance regarding where in the distribution displaced informal workers should come from, therefore I cannot tell with certainty which mass changes, if any, were caused by the wage subsidy. The best I can do then, is a bounding exercise. I will search for mass changes in the $(0.0 - 2.0 * MW]$ range in the informal sector labor earnings distribution, and say that, if the net number of workers forming these mass changes took subsidized jobs, it represents an upper bound for the size of the across sector displacement of workers. Figure 1.5 shows missing mass appears in the $[0.4 * MW - 1.2 * MW]$ range, and excess mass in the $[1.3 * MW - 1.8 * MW]$ range of the informal labor earnings distribution, the latter possibly reflecting across sector movement of workers from the spillover region located to the right of 1.5 minimum wages in the formal labor earnings distribution.

Lastly, workers coming from the pool of job seekers is the residual after accounting for displacement of formal and informal workers. Note that since I am estimating an upper bound for the number of informal workers taking subsidized jobs, the number of jobs that went to job seekers represents a lower bound.

To fix ideas, denote by Δ_1, Δ_2, and Δ_3, the number of workers coming from the informal sector, other parts of the formal labor earnings distribution, and the pool of job seekers, respectively. The following expressions define these objects:

$$\Delta_1 = M_{informal_{[0.4-1.2]}} \tag{1.7}$$

$$\Delta_2 = M_{formal_{(0.0-0.8]}} + M_{formal_{[1.5-1.9]}} - E_{informal_{[1.3-1.8]}} \tag{1.8}$$

$$\Delta_3 = E_{formal_{[1.0-1.5)}} - \Delta_1 - \Delta_2 \tag{1.9}$$

where E and M denote excess and missing mass, respectively.

Estimation of mass changes makes use of the following, reduced form specification:

$$c_{j,t_0} = \sum_{i=0}^{P} \beta_i (z_{j,t_0})^i + \sum_{i=z_L}^{z_U} \gamma_i \mathbf{1}[z_{j,t_0} = i] + v_{j,t_0} \tag{1.10}$$

where c_{j,t_0} is the number of workers in bin j, z_{j,t_0} the earnings level in bin j, and z_L, z_U earnings levels immediately to the left and right of the minimum wage, respectively. The latter terms intend to reproduce the pronounced spike at the minimum wage. p denotes the order of the polynomial, and t_0 pre-policy years. The counterfactual distribution is given by:

$$\hat{c}_{j,t_1} = \sum_{i=0}^{p} \hat{\beta}_i (z_{i,t_1})^i + \sum_{i=z_L}^{z_U} \hat{\gamma}_i \mathbf{1}[z_{j,t_1} = i] \tag{1.11}$$

where t_1 denotes post-policy years. Excess (E) and missing (M) mass areas are measured as the difference between observed and counterfactual bin counts: $\hat{E} = \sum_j (c_{j,t_1} - \hat{c}_{j,t_1})$ and $\hat{M} = \sum_j (\hat{c}_{j,t_1} - c_{j,t_1})$.

Before estimating excess and missing mass areas, I assess whether the reduced form estimate of the formal and informal labor earnings distributions reproduce the changes observed when they are estimated using kernel methods. Bins' length in the reduced form specification match the optimal bandwidth used in the kernel estimation ($j = 0.1$ for the formal sector distribution, and $j = 0.2$ for the informal sector distribution). The order of the polynomial that better fits the data is $p = 3$ for the formal sector distribution, and $p = 4$ for the informal sector distribution (table 1.1).

Figures 1.6 and 1.7 show the reduced form estimates replicate all observed changes. Concretely, in the formal sector distribution, missing mass is in the $(0.0 - 0.8 * MW)$ and $[1.5 * MW - 1.9 * MW]$ ranges, and excess mass in the targeted earnings range $[MW - 1.5 * MW)$. In the informal sector distributions, missing and excess mass are found in the $[0.4 * MW - 1.2 * MW]$ and $[1.3 * MW - 1.8 * MW]$ ranges, respectively.

For estimation of all objects (E, M and $\Delta's$) defined in this section, I resort to a bootstrapping procedure that uses a 10% random sample and 500 replications. Table 1.2 contains the results. The subsidy added over $700,000$ jobs in the $[MW - 1.5 * MW)$ labor earnings range, a roughly 8% increase with respect to the number of workers that would had located there absent the subsidy. Workers who absent the wage subsidy would had already been working in the formal sector took 13.4% of the subsidized jobs. The rest of the jobs, approximately 85%, were taken by workers who would had been working informally or searching for jobs. From our bounding exercise, workers who would have been working in the informal sector took up to 75% of the subsidized jobs, and the remaining 10.8% would have been workers searching for jobs.

1.3.3 Change in the Size of the Formal Labor Markets

Denote by $\lambda_{j,t}$ the faction of formal workers in bin j, at time t. In the presence of secular trends in the size of the formal sector, $\Delta\lambda_j = \lambda_{j,t1} - \lambda_{j,t0}$ does not give the causal effect of the wage subsidy on formality. To account for economy-wide trends and contemporaneous shocks, I use as control group a set of earnings bins unaffected by the wage subsidy. It should necessarily locate above two minimum wages, since, as the previous section shows, below it are the treated areas. Individuals located too high in the distribution do not serve as a credible counterfactual either, since they are more likely different in dimensions that are both unobservable and correlated with the probability of having a formal job. Therefore, as a control group I use individuals located not too far from the affected areas in the labor earnings distribution. Concretely, I use individuals earning between three and four minimum wages.

Consider the following regression specification:

$$\lambda_{i,j} = \alpha_0 + \alpha_1 T_t + \alpha_2 G_j + \alpha_3 (T_t * G_j) + X_i \delta + \mu_{i,j} \tag{1.12}$$

where $\lambda_{i,j}$ takes 1 if worker i, in earnings bin j is formal, 0 otherwise. T_t is a time indicator, G_j a group indicator, X_i a matrix of observable characteristics, and $\mu_{i,j}$ the error term. T_t takes 1 for years 2012 to 2015, 0 for years 2008 to 2010. G_j takes 1 if labor earnings falls within bin j, 0 if within the $[3.0 * MW - 4.0 * MW]$ range, where j indexes the earnings ranges affected by the subsidy in the formal sector: $(0.0-MW)$, $[MW-1.5*MW)$

and $[1.5 * MW - 1.9 * MW]$. The matrix of observable characteristics includes a set of education level indicators, experience, experience squared, marital status indicators, industry indicators, and a set of job history variables.

The identification assumption for $\hat{\alpha}_3$, to give the causal effect of the wage subsidy on the size of the formal sector, is that in its absence, treatment and control groups would had evolved similarly. I begin showing treated and control earnings levels share common trends before the introduction of the wage subsidy. Figure 1.8 shows that the fraction of formal workers, until 2010, gravitated around 0.4 and 0.7 for the treatment and control groups, respectively. The fraction of formal workers starts increasing steadily for the treatment group since 2011, the year the wage subsidy became effective, while for the control group it remains gravitating around 0.7 until 2012. At the end of 2012 a tax reform dropped the payroll tax rate formal firms pay for workers earning less than 10 minimum wages. It might explain what seems to be an upward trend in the fraction of formal workers in the control group starting in 2013, and also implies the increase in the treatment group from 2013 on is partly driven by it. If the drop of the payroll tax rate affects uniformly different parts of the labor earnings distribution, the control group accounts for it, and the identification strategy pursued here gives the causal effect of the wage subsidy on the size of the formal sector.

Table 1.3 contains raw difference-in-difference estimates. All standard errors are clustered at the state level. Panel A shows the fraction of formal workers in the control group increases by 2.5 percentage points, while below the minimum wage drops by 0.06 percentage points, as a result of a drop in the number of both formal and informal workers. The difference-in-difference estimate of the change in the size of the formal sector in the $(0.0 - MW)$ earnings range is -2.6 percentage points.

In the earnings region targeted by the wage subsidy (panel B), the fraction of formal workers increases by 4.9 percentage points as a result of an increase in the number of formal workers, plus a drop in the number of informal workers around the minimum wage. There is also an increase in the number of informal workers around 1.5 minimum wages, but the net number of informal workers in the entire region drops significantly. The difference-in-difference estimate of the change in the size of the formal sector in this earnings region is 2.4 percentage points.

In the $[1.5 * MW - 1.9 * MW]$ earnings range (panel C) the number of formal workers decreases, while it increases in the case of informal workers. As a result, the difference-in-difference estimate of the change in the size of the formal sector in this earnings region is -1.6 percentage points.

Table 1.4 presents the results adding covariates. It slightly attenuates the magnitude of the change in the size of the formal sector in the targeted region, and increases it in the $[1.5 * MW - 1.9 * MW]$ earnings region. Below the minimum wage it barely changes the results. In all cases the difference-in-difference estimates are statistically significant.

Figure 1.9 shows the change in the size of the formal sector is not homogeneous within the earnings ranges affected by the wage subsidy. Below the minimum wage the effect is persistently negative up until approximately $0.8 * MW$. As the mass changes point out, it is the result of a drop in the number of formal workers in this entire region, and a drop in the number of informal workers only in the upper end of it.

The drop in the number of informal workers occurs mostly between 0.8 and 1.2 minimum wages. It explains why the size of the formal sector starts trending up as it approaches the minimum wage from the left, and why the greatest change happens around 1.2 minimum wages. In the upper end of the targeted region, the change in the size of the formal sector starts trending downwards as a result of a relatively small number of additional formal workers in this area, plus an increase in the number of informal workers. Finally, in the affected area located above 1.5 minimum wages, the change in the size of the formal sector is negative as a result of less formal workers and more informal workers.

The results described here are quantitatively similar, and qualitatively the same, if instead of a linear probability model, I estimate α_3 using non-linear probabilistic models, and if I use alternative control groups (see appendix A.3).

This section has shown the wage subsidy caused a sizable shift of workers across the formal and informal labor markets, and to a lesser extent across earnings levels within the formal sector, but relatively little net employment growth, which suggests the excess burden of the policy must be substantial. The next section derives a deadweight loss formula to assess the welfare cost of the wage subsidy.

1.4 Welfare Analysis

This section follows Chetty (2009) in deriving a deadweight loss ('DWL') formula to assess the welfare cost of a wage subsidy in an economy with a large informal labor market. Consider an economy with P firms, whose objective is to maximize profits by choosing to operate formally ($s = 1$) or informally ($s = 0$), and the optimal level of labor (and combination of labor types), given the technology at their disposal. Technology is a function of labor and an exogenous, known, firm-specific productivity parameter (z_i) drawn from a smooth distribution with CDF $H_z(x)$. Firms operating formally are subject to a flat corporate income tax (τ_c), a flat payroll tax (τ_d), and can access a wage subsidy by claiming a tax credit equivalent to $0 \leq \delta \leq 1$ of the payroll tax rate for hiring workers of ability type $k \leq j < K < M$. Firms operating informally evade all taxes, but face the risk of detection, and full confiscation of profits, with probability ρ. The firm's problem is:

$$max_{\{L_1,..L_K,..L_M\}} \Pi_{s,i}$$

$$= \begin{cases} (1-\tau_c)[pY_i - (1+\tau_d)\sum_{j=1}^{M} w_j L_j] + \delta\tau_d \sum_{j=1}^{K} w_j L_j & \text{if } s = 1 \\ (1-\rho)[pY_i - \sum_{j=1}^{M} w_j L_j] & \text{if } s = 0 \end{cases} \tag{1.13}$$

s.t.

$$Y_i \leq F(L_1, ..L_K, ..L_M, z_i)$$

$$L_1, ..L_K, ..L_M \geq 0$$

On the consumer side, there is a heterogeneous population of $L = \sum_{j=1}^{M} L_j$ individuals, where 'j' indexes ability type. The individual's problem is to choose consumption ($c_{i,j}$), labor market sector (s), and how much labor to supply ($l_{i,j}$). The individual's problem is:

$$\begin{gathered} max_{\{c_{i,j},l_{i,j}\}}\, u_{i,j}(c_{i,j}, l_{i,j}) = c_{i,j} - \Psi_s(l_{i,j}) \\ s.t. \\ c_{i,j} \leq w_j l_{i,j} \\ c_{i,j}, l_{i,j} \geq 0 \end{gathered} \tag{1.14}$$

where Ψ captures the disutility of work, which might vary across sectors, and w_j denotes wage. Workers choosing to supply labor in the formal sector hold a -formal- job with probability $\lambda_j(\delta)$, and with complementary probability $1 - \lambda_j(\delta)$ spend their time in the second best alternative (an informal job). The wage subsidy affects the probability of holding a formal job through its effect on formal sector labor demand. The informal labor market is frictionless, therefore workers choosing to supply labor in the informal sector hold a job with probability one.

The market-clearing condition $L^D(w) = L^S(w)$, with $L^D(w) = L^D(w)_{s=1} + L^D(w)_{s=0}$ and $L^S(w) = L^S(w)_{s=1} + L^S(w)_{s=0}$, closes the model. Product and factor markets are competitive, and utility and production functions exhibit standard properties: $u' > 0$, $u'' < 0$, $F' > 0$, $F'' < 0$. Social welfare in this economy is given by:

$$\begin{gathered} W(\delta) = \Bigg\{ \sum_{j=1}^{M} \sum_{i=1}^{L} \Big[max\Big\{ (\lambda_j(\delta)(w_j l_{i,j,s=1} - \Psi_{s=1}(l_{i,j,s=1}) \\ +(1 - \lambda_j(\delta))\overline{w}_{i,j,s=0})); \overline{w}_{i,j,s=0} \Big\} \Big] \Bigg\} \\ + \Bigg\{ max\Big\{ ((1 - \tau_c)[pF(L, z_i) - (1 + \tau_d)wL] + \delta\tau_d \sum_{j=1}^{K} w_j L_j); \\ (pF(L, z_i) - wL) \Big\} \Bigg\} - \delta\tau_d \sum_{j=1}^{K} w_j L_j \end{gathered} \tag{1.15}$$

where $\overline{w}_{i,j,s=0} = (1-\rho)[w_j l_{i,j,s=0} - \Psi_{s=0}(l_{i,j,s=0})]$.

Conceptually, the deadweight loss of the wage subsidy is the amount of surplus lost net of labor costs saved by firms due to the subsidy. The first term in curly brackets is the consumers' surplus, which given quasi-linearity in the utility function is money-metric. The second term in curly brackets is the firms' surplus. The last term corresponds to revenues collected from firms, in the form of a lump-sum tax, for an amount equal to the subsidy they received. Taking the derivative of (1.15) with respect to δ, and using envelope conditions, yields the following expression for the deadweight loss of the wage subsidy:

$$\frac{dW}{d\delta} = \sum_{j=1}^{K\leq m\leq M} \sum_{i=1}^{L_1} \left[\frac{d\lambda_j}{d\delta} \Big(w_j l_{i,j,s=1} - \Psi_{s=1}(l_{i,j,s=1}) - \overline{w}_{i,j,s=1} \Big) \right] \tag{1.16}$$

$$-\delta\tau_d \sum_{j=1}^{K} w_j \frac{dL_j}{d\delta}$$

To make the formula more tractable, I fully parametrize the utility function. The specification incorporates labor earnings taxation in the form of a flat payroll tax rate (τ_c), enforceable in the formal sector only. The utility function is:

$$u_{i,j} = \begin{cases} z_{i,j}(l_{i,j}) - \tau_c(l_{i,j}) - \frac{l_{i,j}^{1+\frac{1}{\alpha_1}}}{1+\frac{1}{\alpha_1}} & \text{if } l_{i,j,s=1} > 0 \\ \overline{w}_{i,j} & \text{if } l_{i,j,s=1} = 0 \end{cases} \tag{1.17}$$

where $z_{i,j} = w_j l_{i,j}$ denotes pre-tax labor earnings, and $\overline{w}_{i,j}$[8] net informal sector labor earnings. An individual that chooses to supply labor in the formal sector decides how many hours to supply by solving the following problem:

$$max_{\{l_{i,j}\}} u_{i,j} = w_j l_{i,j} - \tau_c w_j l_{i,j} - \frac{l_{i,j}^{1+\frac{1}{\alpha_1}}}{1+\frac{1}{\alpha_1}} \tag{1.18}$$

from which the optimal number of hours is:

$$l_{i,j} = [(1-\tau_c)w_j]^{\alpha_1} \tag{1.19}$$

[8] $\overline{w}_{i,j} = (1-\rho)\left[z_{i,j}(l_{i,j}) - \frac{l_{i,j}^{1+\frac{1}{\alpha_0}}}{1+\frac{1}{\alpha_0}} \right]$

The individual chooses to work in the formal sector if the following condition holds:

$$\frac{[(1-\tau_c)w_j]^{1+\alpha_1}}{1+\alpha_1} \geq \overline{w}_{i,j} \tag{1.20}$$

The expected utility of an individual that chooses to supply labor in the formal sector is:

$$\lambda_j(\delta)\left[\frac{[(1-\tau_c)w_j]^{1+\alpha_1}}{1+\alpha_1}\right] + (1-\lambda_j(\delta))\overline{w}_{i,j} \tag{1.21}$$

Taking the derivative of (1.21) with respect to δ, and plugging the result into (1.16) yields:

$$\frac{dW}{d\delta} = \sum_{j=1}^{K\leq m\leq M}\sum_{i=1}^{L_1}\left[\frac{d\lambda_j}{d\delta}\left[\frac{[(1-\tau_c)w_j]^{1+\alpha_1}}{1+\alpha_1} - \overline{w}_{i,j}\right]\right] - \delta\tau_d\sum_{j=1}^{K} w_j\frac{dL_j}{d\delta} \tag{1.22}$$

The marginal excess burden formula is a function of two sufficient statistics: i) the change in the probability of having a formal job, potentially across the whole ability distribution, and ii) the change in labor demand induced by the subsidy within the ability ranges targeted by the policy. Absent the informal sector, the first term collapses, and the formula resembles closely the expression of the well-known Harbenger triangle, where the labor demand slope ($\frac{dL_j}{d\delta}$) approximates the triangle's height, and the size of the subsidy ($\delta\tau_d$) its base. In a segmented labor market, subsidizing wages in one sector might change the sectors' relative sizes. Moreover, the sector size changes might not happen exclusively in the ability levels targeted by the policy, but could affect non-targeted groups depending on the degree of substitutability among targeted and non-targeted ability levels.

Before computing the deadweight loss of the wage subsidy, I discuss the main assumptions made to derive the formula: i) informal labor market is frictionless, and ii) no income effects. Relaxing the frictionless assumption in the informal labor market adds no complication, but would not enrich the model in a meaningful way. Firms access the wage subsidy through the tax system, thus its first order effect is on formal firms. Additionally, the model captures the subsidy's effect on across sector mobility through $\lambda(\delta)$. Allowing for income effects leaves the formula unchanged, but will require α_1 to be an uncompensated elasticity. The no income effects assumption is a standard one in the labor supply and personal income taxation literatures, and attempts to recover the size of it suggest that it is rather small (Saez et al. (2012)).

1.4.1 Deadweight Loss Computation

In addition to the sufficient statistics, a set of parameters undistorted by the wage subsidy enter the formula. They are the elasticity of labor earnings with respect to the payroll tax rate (α_1), the net value of workers outside option ($\overline{w}_i$), the workers payroll tax rate (τ_c), and the size of the wage subsidy relative to the employer payroll tax rate ($\delta\tau_d$). I simulate the deadweight loss by plugging in the sufficient statistics estimated in the previous section, and using, for α_1, values in the range $[0.1 - 0.4]$, which the literature on personal income taxation assesses as the most credible ones (Saez et al. (2012)), for $\overline{w}_i$, values in the range $[0 * w_{i,j} - 0.8 * w_{i,j}]$, and for τ_c and $\delta\tau_d$, their statutory values, 0.08 and 0.11, respectively.

Table 1.5 summarizes the results. Not reported in that table is the amount of payroll tax revenue lost due to the tax credit, which amounts to 25 billion pesos. The calculation for job seekers assumes they all are looking for formal jobs within the targeted region. Under this assumption, they are the only group experiencing a welfare gain. Were they, absent the subsidy, searching for informal jobs, or formal jobs in earnings ranges other than the targeted one, the subsidy will impose a burden on them as well. The deadweight loss of the wage subsidy ranges from \$30 to \$1,787 billion pesos. As a fraction of corporate income tax revenues, it ranges from 0.1% to 4.0%. In per-capita terms, using the set of workers affected by the wage subsidy as the reference population, the DWL ranges from 65 thousand (U\$30 PPP) to 6 millon pesos (U\$2,000 PPP). The largest contributors are formal workers displaced from the spillover region located next to 1.5 minimum wages, follow by informal workers displaced from around the minimum wage, and the set of formal workers displaced from below the minimum wage.

Figure 1.10 plots total DWL for different values of α_1 and $\overline{w}$.[9] The net value of workers outside option ($\overline{w}$) makes little difference on the magnitude of the DWL, while it changes

[9] DWL figures in Figure 1.10 include the amount of payroll tax revenue lost due to the tax credit

non-linearly with the elasticity of labor earnings.

Implicit in these calculations is the belief the upper bound of the number of subsidized jobs taken by workers who, absent the subsidy, would had been working informally, is the most credible estimate for the across sector displacement of workers. To assess the implications of this belief, assume the mass changes found in the informal sector labor earnings distribution are orthogonal to the wage subsidy, then approximately 85% of the subsidized jobs were taken by workers who, absent the subsidy, would had been seeking jobs. Under this assumption, the effectiveness assessment of the wage subsidy changes, job creation is now significant, but the efficiency assessment would not change. It would attenuate the size of the DWL, but not considering all job seekers were searching for jobs paying in the earnings range targeted by the policy, the subsidy yields a welfare gain. This is the case because the job seekers' gains are not greater than the losses from workers displaced within the formal sector.

1.5 Discussion

This section intends to shed light into three questions: how does the deadweight loss estimate compare with existing ones in the literature?; Could there be social gains from moving workers out of the informal sector?; and, Who are they relevant for?

1.5.1 *How does the deadweight loss estimate compare with existing ones in the literature?*

Busso et al. (2013) assess the efficiency cost of the Empowerment Zones (EZ) tax credit.[10] They find no statistically significant effect of EZ designation on zone amenities, rents in

[10] EZ is a place based, federal program, intended to boost economic activity in the most disadvantaged communities in the United States. It offers employment tax credits to firms located in the areas designated as EZ, and grants for local governments to spend in business assistance, infrastructure and housing within these areas.

non-designated neighboring areas, rents within designated areas, or wages of non-resident EZ workers. They do find a positive effect on the wages of EZ resident workers, and estimate the DWL of the tax credit amounts to 6.9 million dollars (corporate income tax revenues were over 170,000 billion dollars in 2000, the year they use as reference point for all welfare calculations).

The literature on non-place based wage subsidies is vast, but has restricted its attention almost exclusively to employment and earnings effects (See Katz (1996) and Card et al. (2015)). It is common to find references to potentially large DWL, but they are all speculative. Bell et al. (1999) argues, regarding a wage subsidy targeting individuals between ages 18 and 24 in the United Kingdom, deadweight losses must be large because they target a population transition probabilities suggest moves frequently from unemployment to employment. Researchers have also pointed to potentially large displacement effects to argue DWL must be large. However, evidence on displacement effects is scarce and, until recently, not credibly identified (Katz (1996)). Exceptions are Blundell et al. (2004), Crepon et al. (2013), Gautier et al. (2014) and Martins and e Costa (2014), who use research designs aimed at identifying displacement effect. They find mixed results, and none embark on a DWL calculation.

DWL estimates are more common in the literatures on taxation and large social insurance programs such as unemployment insurance. The nature of these government interventions differ from the one studied in this paper, but they help in assessing the magnitude of the wage subsidy's DWL. A classical reference is Harberger (1964), who finds the US income tax generates a DWL of about 1 billon per year, or 2.5% of revenues raised.[11]

[11] Other authors that followed Harbeger's approach were Ballard et al. (1985), Browning (1987) and Stuart (1984). The results they report vary according to the modeling assumptions they made. Ballard et al. (1985) assesses the efficiency costs all major US taxes by calibrating a multi-sector, dynamic general equilibrium model, finding that a one percent increase in tax revenues generates a loss ranging from 17 to 56 cents. Browning (1987) and Stuart (1984) restrict attention to taxes on labor earnings, but the former computes DWL from a partial-equilibrium model, and the latter from a general-equilibrium model. Browning (1987) reports losses ranging from 10 to 300 percent of marginal tax revenues, while Stuart

Feldstein (1999) argues previous attempts to quantify the marginal excess burden of the US personal income tax underestimate the true losses, since they ignore responses on margins other than labor supply. He derives a DWL formula where the only sufficient statistic needed for welfare analysis is the elasticity of taxable income with respect to the marginal tax rate, and finds losses amount to approximately 32% of the personal income tax revenue. Feldstein (1999) bases his calculation on an estimate of the elasticity of taxable income with respect to the marginal tax rate of 1.04. This parameter became of great interest in the taxation literature not only for purposes of efficiency cost evaluation, but for optimal tax design as well. As such, intense empirical research followed trying to obtain convincing, cleaner estimates of it, with the most credible ones ranging from 0.12 to 0.4 (Saez et al. (2012)). Since the DWL's size in Feldstein's formula is proportional to the elasticity of taxable income, given the newer estimates, its lower bound is close to Harberger's estimate, and its upper bound is around 12% of personal income tax revenues.

Chetty (2008) finds that a 10% increase in Unemployment Insurance (UI) benefits leads to a welfare gain of about 5.9 billion dollars, or approximately 0.05% of GDP, when the baseline UI replacement rate is near 50%. The author shows increasing UI benefits produces a gain rather than a loss, since at a 50% replacement rate, the 'liquidity effect' explains 60% of the unemployment duration's increase, while disincentives to search explains the remaining 40%. The 'liquidity effect' is socially desirable since it counteracts credit and private insurance market failures.[12]

The DWL caused by the wage subsidy studied in this paper is larger than the DWL of the EZ tax credit, but smaller than the available DWL estimates of the personal income tax or unemployment insurance. The former is hardly an informative benchmark since it applies to narrow geographical areas. The latter, on the other hand, covers larger fractions of the population. In the case of the policy studied here, it covers the formal sector only, and within it, those at the margin of increasing payroll must simultaneously have a strictly

(1984) finds losses in the order of 20 to 25 percent of marginal tax revenues.

[12]Shimer and Werning (2008) provides another DWL estimate of increasing UI benefits. They report a net welfare gain of approximately 2.4 billion from a 1% increase in benefits level, or about 0.2% of GDP.

positive corporate income tax liability. Overall, wage subsidy's DWL does not seem out of proportion when compared to existing evidence.

1.5.2 *Could there be social gains from moving workers out of the informal sector?*

Any market intervention is welfare enhancing if the distortion it intends to correct is at least as great as the loss it induces. Social gains from formalizing workers can come from the stop of free riding of government services funded via social security contributions. In the Colombian context, access to health services is highly subsidized for informal workers and their dependents. Adding them as contributors frees fiscal resources that could be given more productive uses. Gains can also result from greater productivity and better health, since working conditions are superior in formal jobs; plus, access to social insurance programs enhances individuals' ability to deal with adverse shocks.

Contrasting the DWL estimated in the previous section with the social gains of moving workers out of the informal sector, would tell whether the policy enhanced welfare or not. Unfortunately, there are not credible estimates of the social benefits of being formal, and estimating them exceeds the scope of this paper. Alternatively, the DWL can be seen as a lower bound for what the size of the gains should be for the wage subsidy to take the labor market to a superior equilibrium.

1.5.3 *Who are the results informative for?*

The results are for the most part informative for economies with large informal sectors and limited informational enforcement capacity. The US has experimented with non-place based, non-industry specific wage subsidies channeled through the tax system in the form of tax credits; however, advanced economies have more recently chosen the unemployment insurance system as the mean to implement them. The latter ensures better targeting - possibly at the expense of otherwise higher take up rates- and therefore overall smaller distortions. In developing countries unemployment insurance is rare, and where it exists has limited coverage. Additionally, if shrinking the size of the informal sector is a policy goal, targeting workers receiving UI benefits is of little help.

1.6 Conclusion

This paper studies the labor market effects of a wage subsidy introduced in Colombia's First Job Act in 2010 by exploiting changes it induced in the labor earnings distribution. Using cross sectional data from a nationally representative sample of working age individuals, I estimate the employment effect net of displaced workers. I find the subsidy caused a substantial shift of workers across the formal and informal labor markets, to a lesser extent across earnings levels within the formal sector, but relatively little net employment growth.

This finding contributes to a small but growing literature that has been investigating the presence and scale of displacement effects in active labor market programs. This study takes a step forward, and by estimating the subsidy's deadweight loss, provides a lower bound for what the benefits of moving workers across labor markets should be for the wage subsidy to enhance welfare. The deadweight loss of the wage subsidy ranges from 0.1% to 4.0% of corporate tax revenues.

In spite of the fact this analysis shows the subsidy created relatively few extra jobs, and that former informal workers took most subsidized jobs, extrapolation requires caution. These results should not serve as the basis for a prediction of what the employment effects of a large scale wage subsidy, that targets low earnings workers, will be -not even for the case of economies with large informal labor markets. More variation in dimensions such as the subsidy's generosity, eligible population, and means of implementation, among others, is necessary before trying to generalize the findings.

Table 1.1: OLS Results, Polynomial Fitted to Reproduce the Pre-Wage Subsidy Empirical Distribution

VARIABLES	**Formal**	**Informal**
Labor Earnings	568.450***	739.853***
	(120.331)	(207.366)
(Labor Earnings)^2	-0.260***	-0.544***
	(0.057)	(0.160)
(Labor Earnings)^3	0.000***	0.000***
	(0.000)	(0.000)
(Labor Earnings)^4		0.000***
		(0.000)
Observations	100	50
R-squared	0.858	0.980

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.2: Mass Changes and Decomposition of Excess Mass In the Subsidy's Targeted Region

A. *Mass Changes*				
RANGE	**Statistic**	**Std. Err.**	**[95% Conf.**	**Interval]**
Formal Sector				
$\hat{M}_{(0.0-0.8)}$	73 092.87	596.77	71 923.19	74 262.55
$\hat{E}_{[1.0-1.5)}$	739 564.50	8817.08	722 283.03	756 845.97
$\hat{M}_{[1.5-1.9]}$	185 665.80	4466.00	176 912.44	194 419.16
Informal Sector				
$\hat{M}_{[0.4-1.1]}$	559 749.70	5700.75	548 576.23	570 923.17
$\hat{E}_{[1.3-1.8]}$	159 088.50	2379.10	154 425.46	163 751.54

B. *Decomposition of* $\hat{E}_{Formal_{[1.0-1.5)}}$				
OBJECT	**Statistic**	**Std. Err.**	**[95% Conf.**	**Interval]**
$\hat{\Delta}_1$	559 564.50	5700.75	548 576.23	570 923.17
$\hat{\Delta}_2$	99 670.17	5035.08	89 801.42	109 538.92
$\hat{\Delta}_3$	80 144.62	11 657.67	57 295.58	102 993.66

Note: All objects were estimated using a bootstrapping procedure with a 10% random sample (with replacement) of the population, clustered at the State level, and 500 replications. Standard errors and confidence intervals in panel B account for the covariances among the terms that form them.

Table 1.3: Change in the Size of the Formal Sector, Difference-in-Difference Estimator, No Controls

A. *Earnings Range* $(0.0 - MW)$

	Before	After	Difference
Treatment Group	0.0749***	0.0743***	−0.0006
	(0.012 9)	(0.012 2)	(0.004 9)
	n=138016	n=264892	n=363115
Control Group	0.7130***	0.7386***	0.0255***
	(0.019 3)	(0.020 6)	(0.005 7)
	n=138016	n=264892	n=39793
Difference	−0.6381***	−0.6642***	−0.0261***
	(0.011 7)	(0.012 6)	(0.005 7)
	n=138016	n=264892	n=402908

B. *Earnings Range* $[MW - 1.5 * MW)$

	Before	After	Difference
Treatment Group	0.6016***	0.6508***	0.0492***
	(0.028 7)	(0.025 6)	(0.007 9)
	n=156578	n=364423	n=481208
Control Group	0.7130***	0.7386***	0.0255***
	(0.019 3)	(0.020 6)	(0.005 7)
	n=156578	n=364423	n=39793
Difference	−0.1114***	−0.0877***	0.0237***
	(0.013 3)	(0.009 4)	(0.007 0)
	n=156578	n=364423	n=521001

C. *Earnings Range* $[1.5 * MW - 1.9 * MW]$

	Before	After	Difference
Treatment Group	0.6468***	0.6566***	0.0097
	(0.027 8)	(0.025 3)	(0.010 3)
	n=43258	n=92873	n=96338
Control Group	0.7130***	0.7386***	0.0255***
	(0.019 3)	(0.020 6)	(0.005 7)
	n=43258	n=92873	n=39793
Difference	−0.0662***	−0.0819***	−0.0157**
	(0.010 7)	(0.007 1)	(0.007 3)
	n=43258	n=92873	n=136131

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.4: Change in the Size of the Formal Sector, Difference-in-Difference Estimator, with Controls

VARIABLES	$(0.0 - MW)$	$[MW - 1.5 * MW)$	$[1.5 * MW - 1.9 * MW]$
$\hat{\alpha}_3$	−0.027***	0.013*	−0.023***
	(0.007)	(0.007)	(0.008)
Time Indicator	0.015**	0.010*	0.008
	(0.006)	(0.005)	(0.006)
Group Indicator	−0.455***	−0.017	0.025**
	(0.017)	(0.014)	(0.010)
Male	−0.006	−0.067***	−0.011
	(0.004)	(0.015)	(0.010)
Married	−0.007*	−0.018**	−0.032***
	(0.003)	(0.007)	(0.004)
HS Dropout	−0.359***	−0.305***	−0.380***
	(0.021)	(0.015)	(0.017)
HS Degree	−0.286***	−0.109***	−0.172***
	(0.017)	(0.011)	(0.016)
Associate Degree	−0.165***	−0.016	−0.020*
	(0.011)	(0.014)	(0.010)
Experience	0.001	0.001	−0.006***
	(0.001)	(0.001)	(0.001)
Experience^2	0.000	0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Second Job	0.016***	0.070***	0.047***
	(0.005)	(0.004)	(0.011)
Not Worked Before	0.043***	0.145***	0.131***
	(0.007)	(0.010)	(0.012)
Tenure	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Service Sector	−0.003	−0.046***	−0.059***
	(0.004)	(0.014)	(0.016)
Constant	0.742***	0.505***	0.661***
	(0.012)	(0.017)	(0.021)
Observations	402,711	520,592	135,979
R-squared	0.390	0.104	0.161

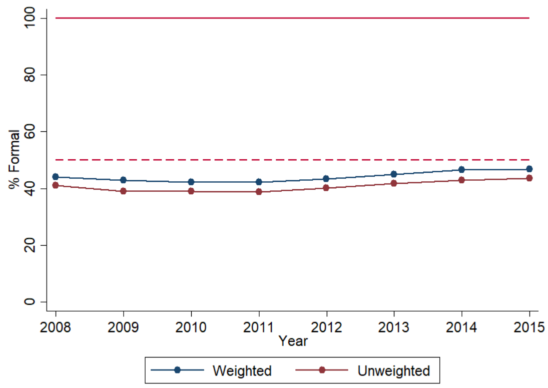
Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Deadweight Loss Calculation

PARAMETERS	$\hat{M}_{formal_{(0.0-0.1)}}$	$\hat{M}_{informal_{[0.4-1.2]}}$	$\hat{M}_{formal_{[1.5-1.9]}}$	**Job Seekers**
n	73,093	559,750	185,666	80,145
w	343,263	660,802	1,098,383	735,535
$\frac{d\lambda}{d\delta}$	-0.0269	-0.0211	-0.0231	0.0130
α_1	[0.1-0.4]	[0.1-0.4]	[0.1-0.4]	[0.1-0.4]
$\overline{w}$	[0.0*w-0.8*w]	[0.0*w-0.8*w]	[0.0*w-0.8*w]	[0.0*w-0.8*w]
DWL (in billions)	[-70.2, -1.5]	[-780.9, -11.9]	[-1,055.8, -18.5]	[1.9, 111.9]
Total DWL	[-1,787,-30]			

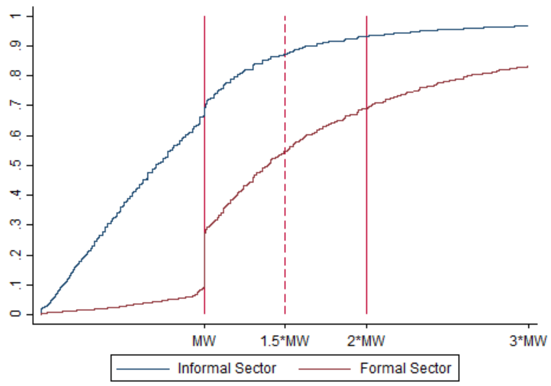
Note: All monetary figures are in 2015 Colombian pesos. n denotes the number of workers forming the corresponding mass changes. w denotes the mean earnings' level within the corresponding earnings' range. DWL calculations using the median rather than the mean barely change the results. $\frac{d\lambda}{d\delta}$ denotes the change in the probability of being formal in the corresponding earnings range. α_1 and $\overline{w}$ denote the elasticity of labor supply and the net value of the worker's outside option, respectively.

Figure 1.1: Size of the Formal Sector, Fraction of Formal Public Sector Workers, and Empirical Labor Earnings and Age CDFs and PDFs, 2008-2015

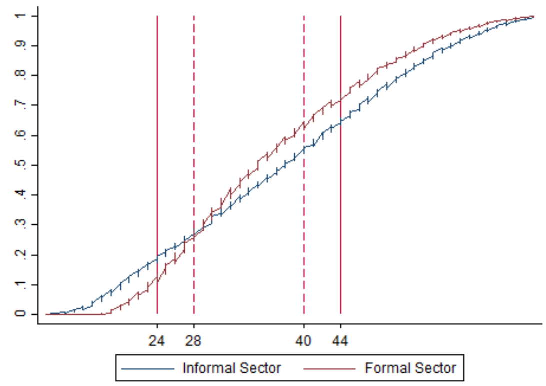


(a) Size of Formal Sector

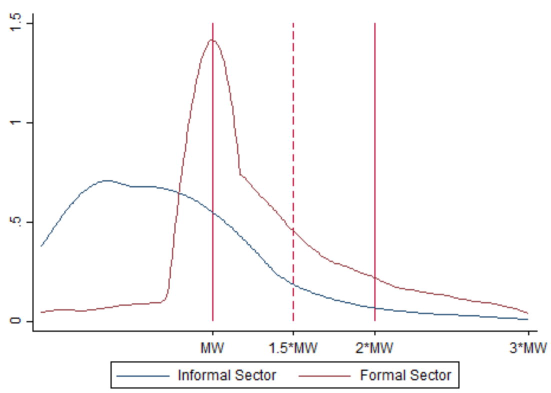
(b) Public Sector Workers



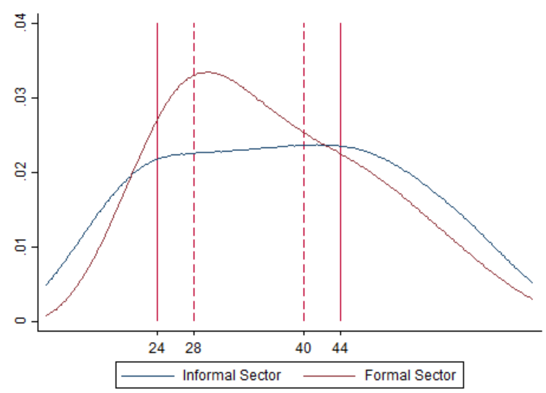
(c) Labor Earnings CDF, 2008-2015



(d) Age CDF, 2008-2015



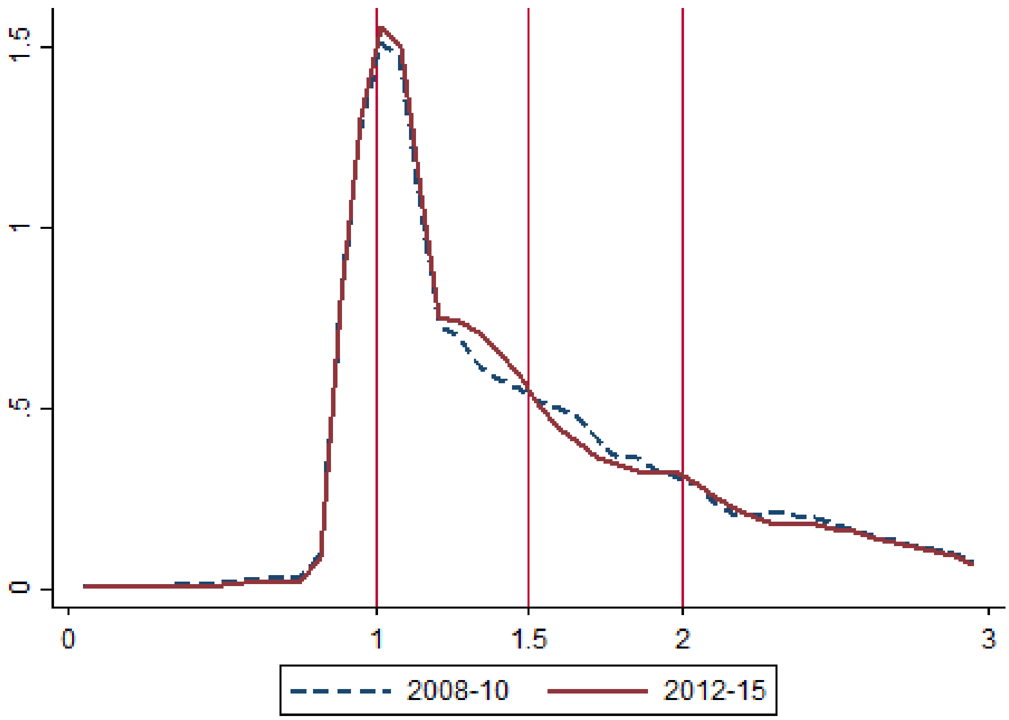
(e) Labor Earnings PDF, 2008-2015



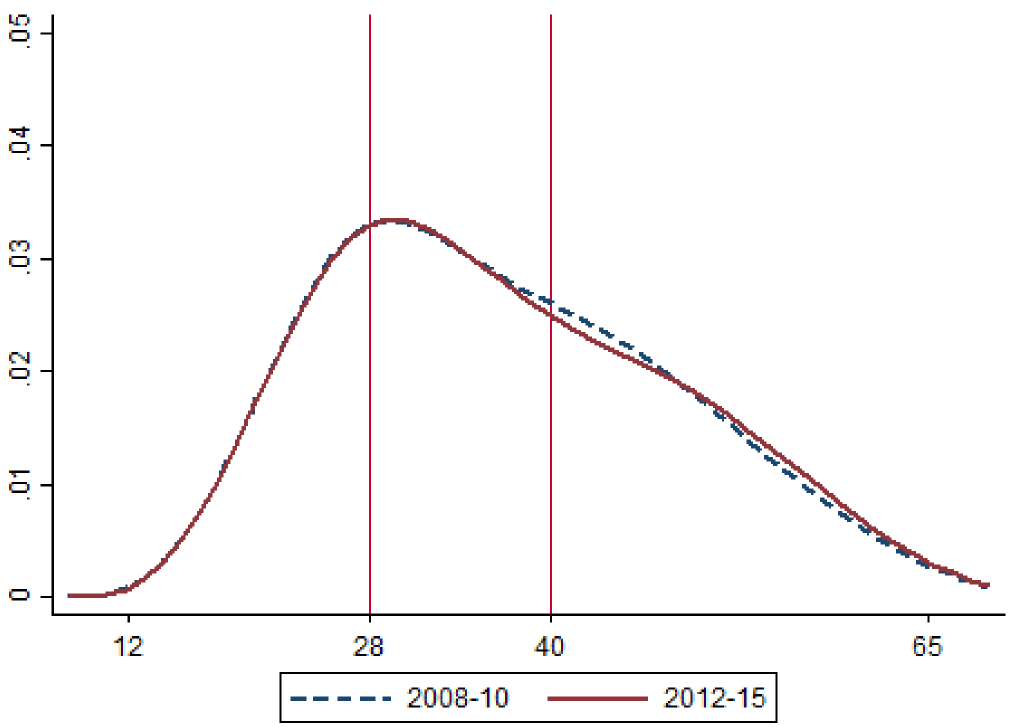
(f) Age CDF, 2008-2015

Notes: Due to the long right tail, figures 1.1c and 1.1e zoom in in the (0-3*MW) range of the earnings distribution. Empirical PDFs and CDFs in figures 1.1c to 1.1f use survey weights. Dashed vertical lines in figures 1.1c to 1.1f denote the earnings and age points where the effective payroll tax rate jumps beacuse of the introduction of the wage subsidy. Solid vertial lines arbitrarily delimit a range where both the subsidy applies and does not apply.

Figure 1.2: Formal Labor Earnings and Age PDF

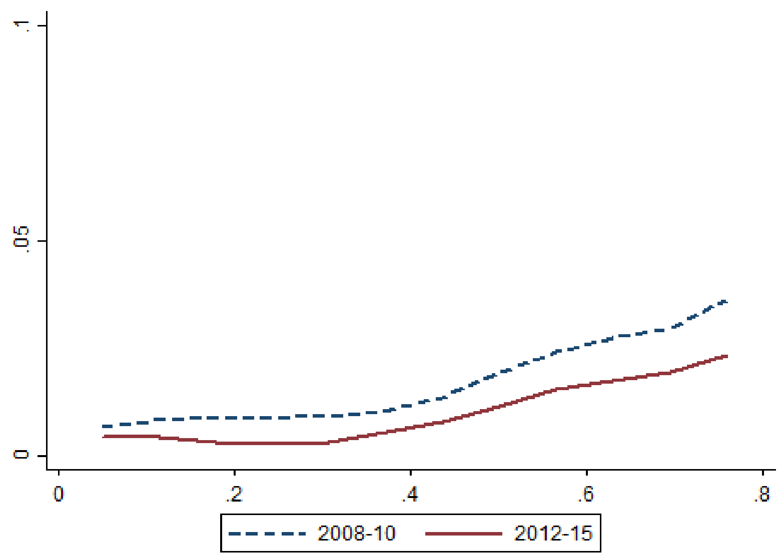


(a) Empirical Labor Earnings PDF

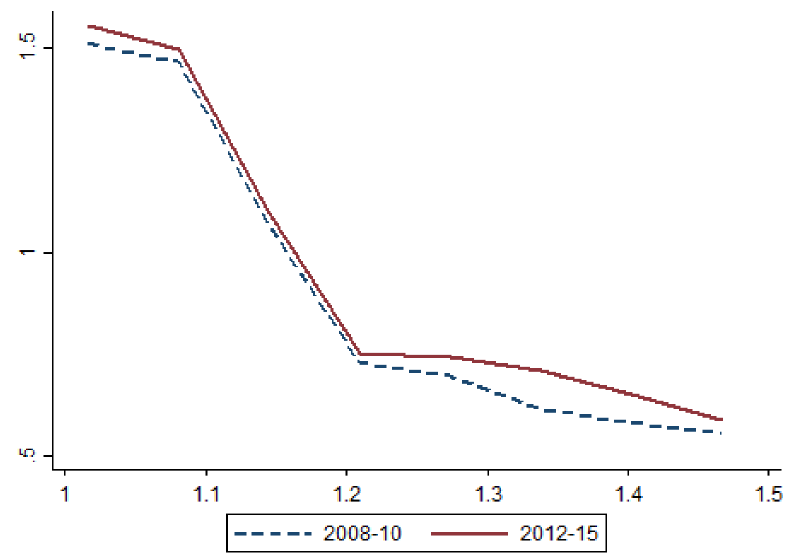


(b) Empirical Age PDF

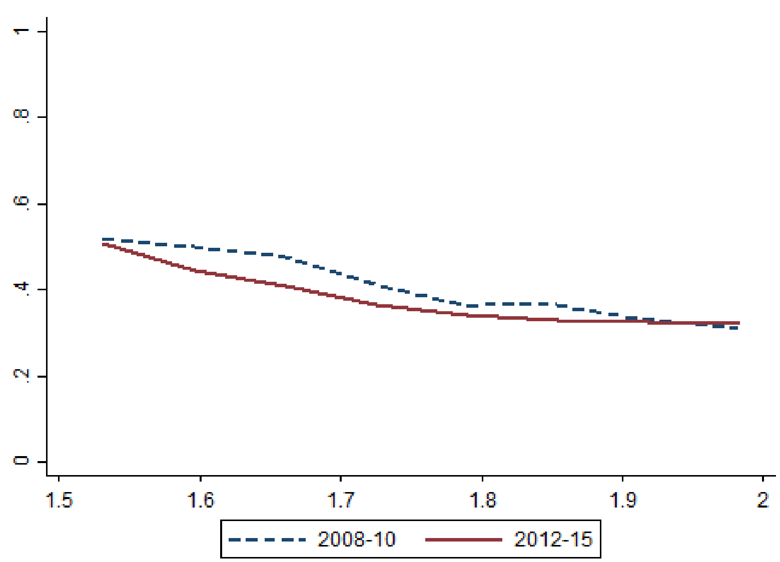
Figure 1.3: Formal Labor Earnings PDF, by Earnings Range



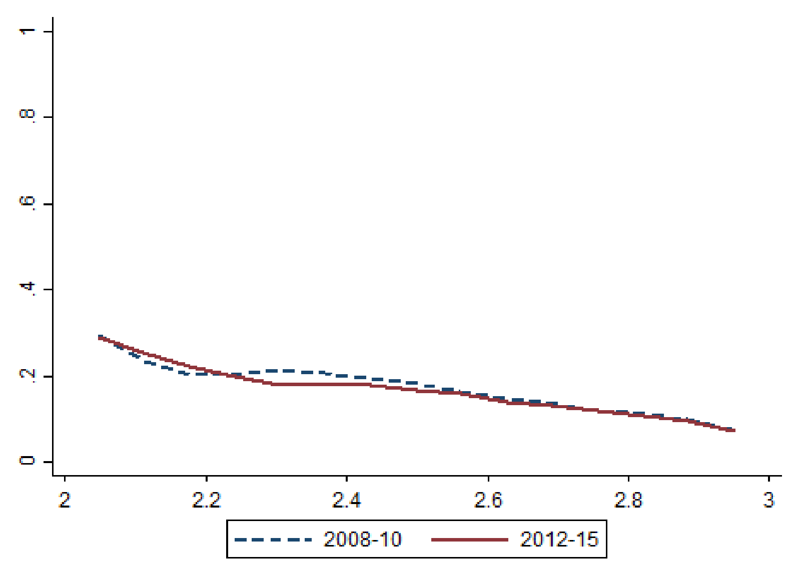
(a) (0.0-0.8*MW)



(b) [1.0*MW-1.5*MW)

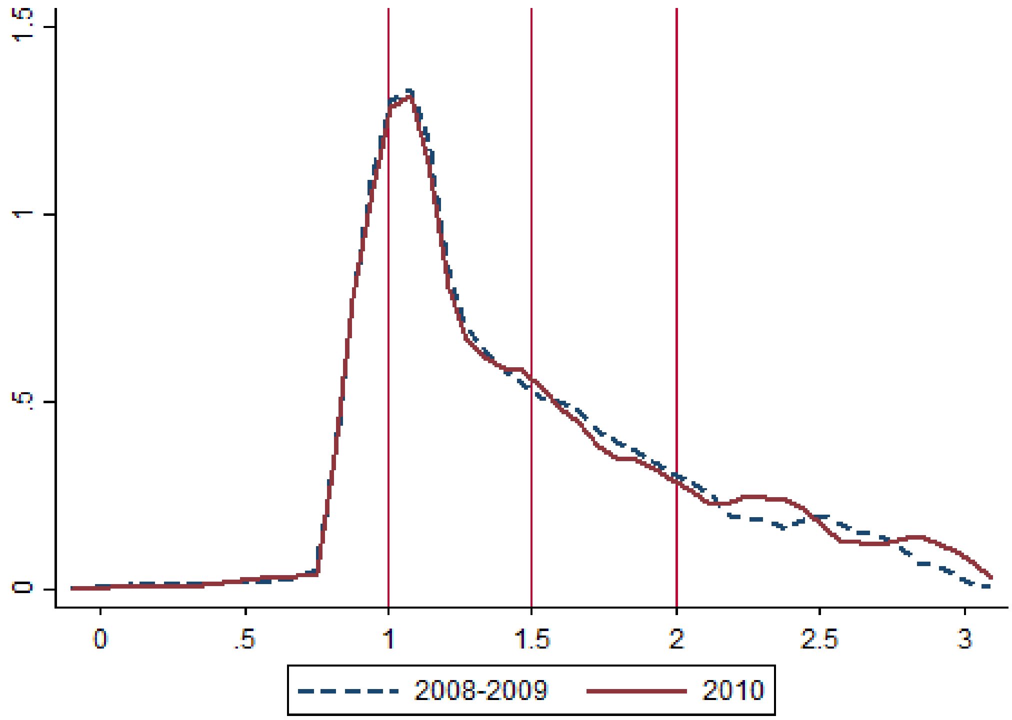


(c) [1.5*MW-2.0*MW]

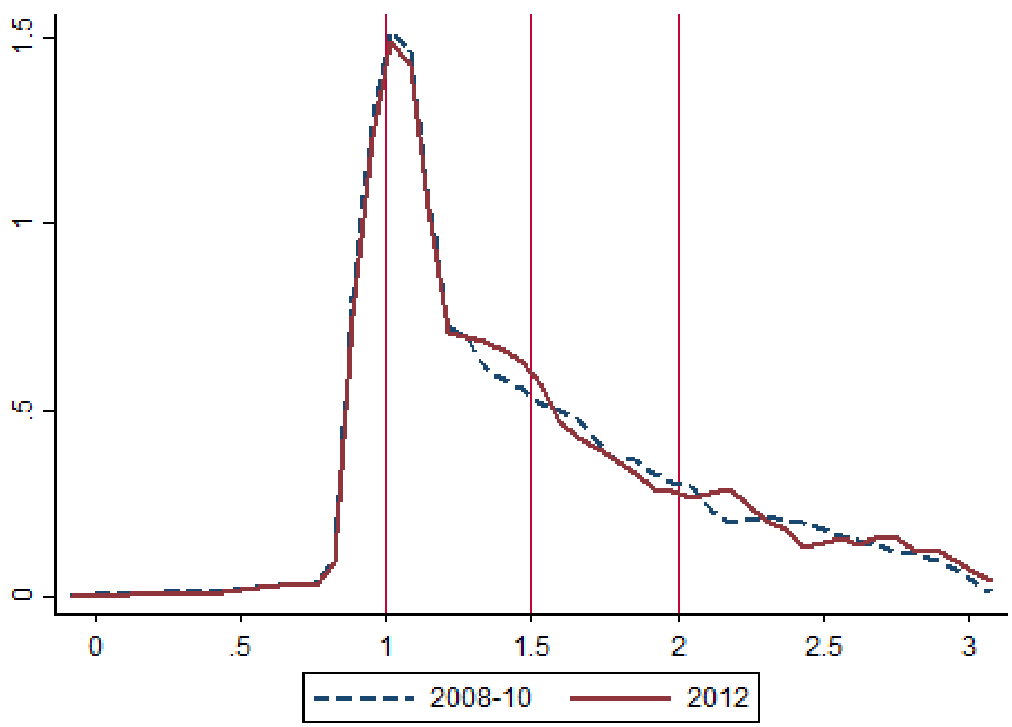


(d) (2.0*MW-3.0*MW)

Figure 1.4: Formal Labor Earnings PDF, by Year

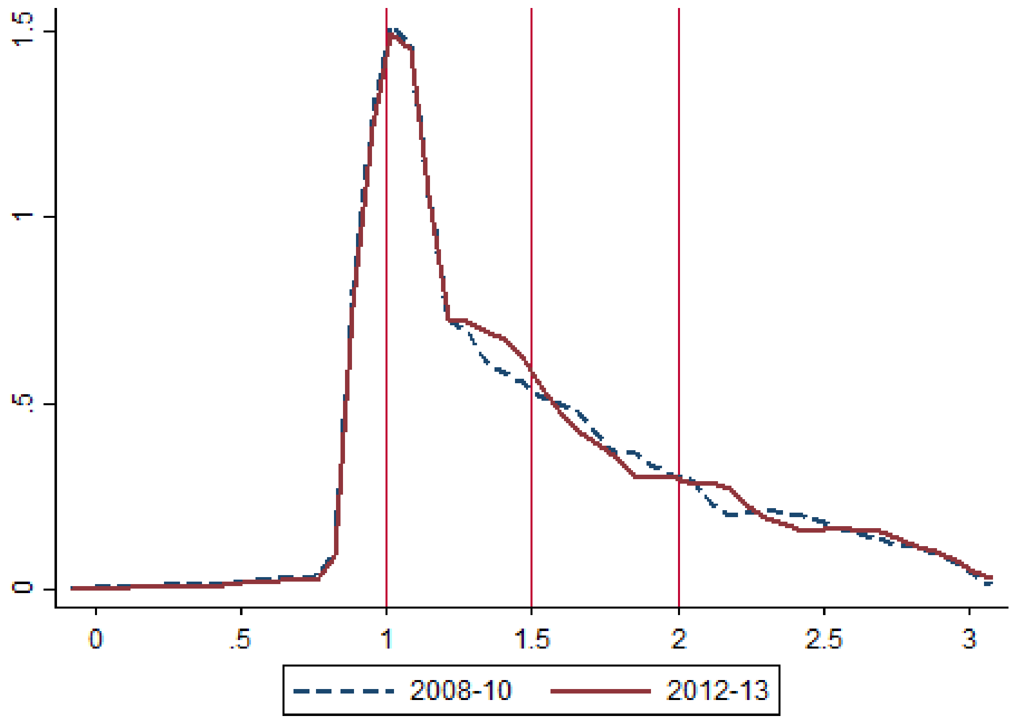


(a)

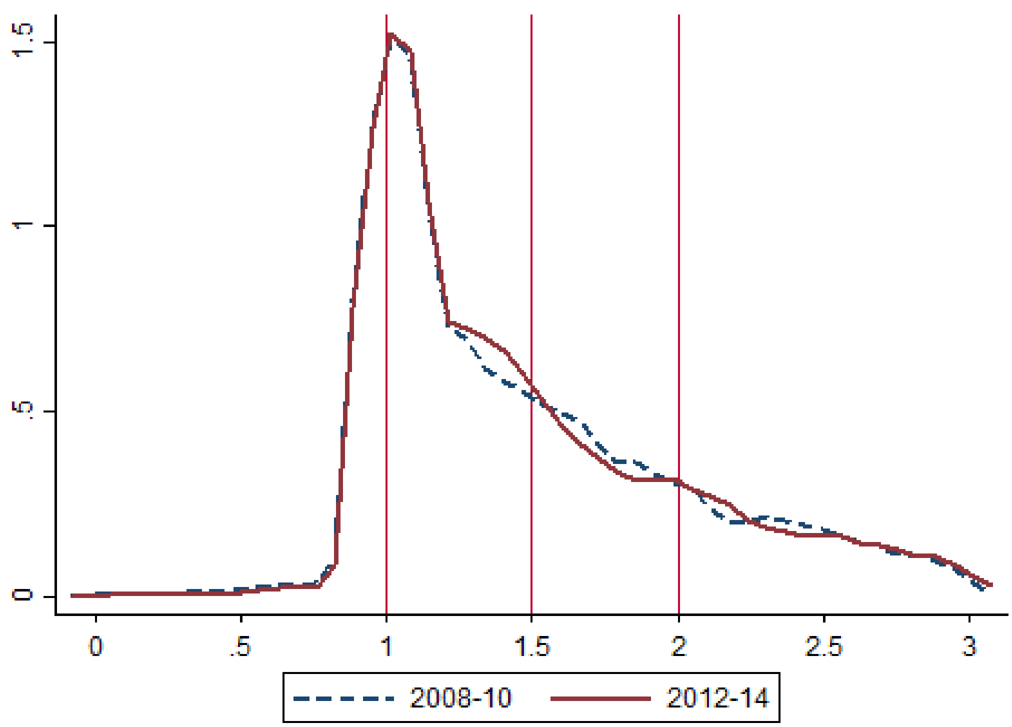


(b)

Figure 1.4 (cont.): Formal Labor Earnings PDF, by Year

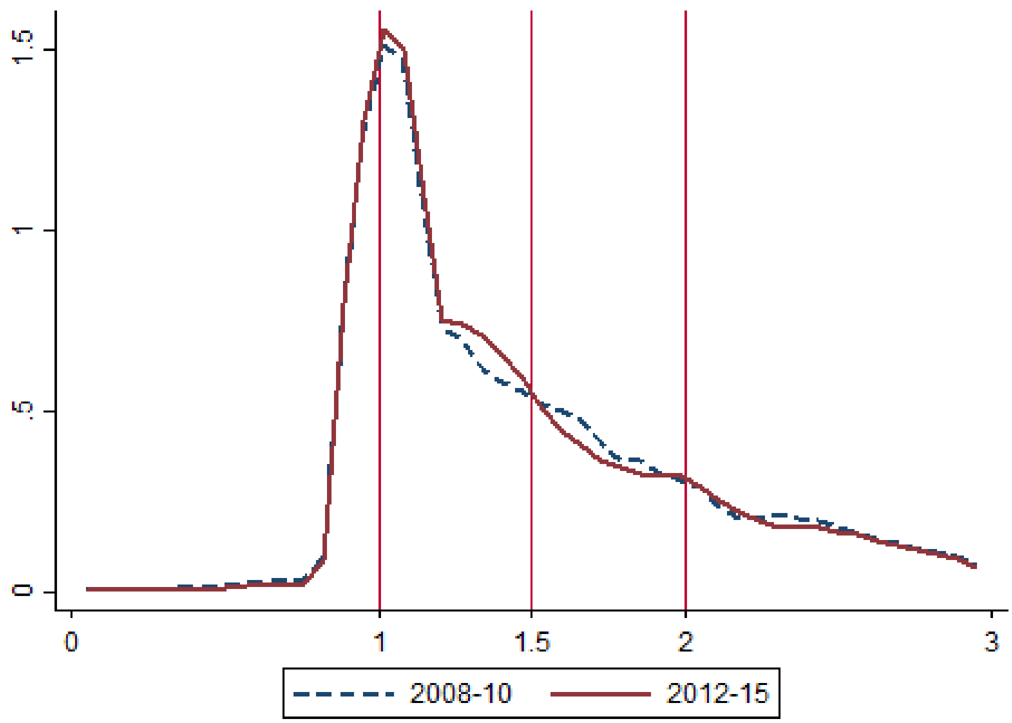


(c)



(d)

Figure 1.4 (cont.): Formal Labor Earnings PDF, by Year



(e)

Figure 1.5: Informal Labor Earnings PDF Before and After the Policy Change

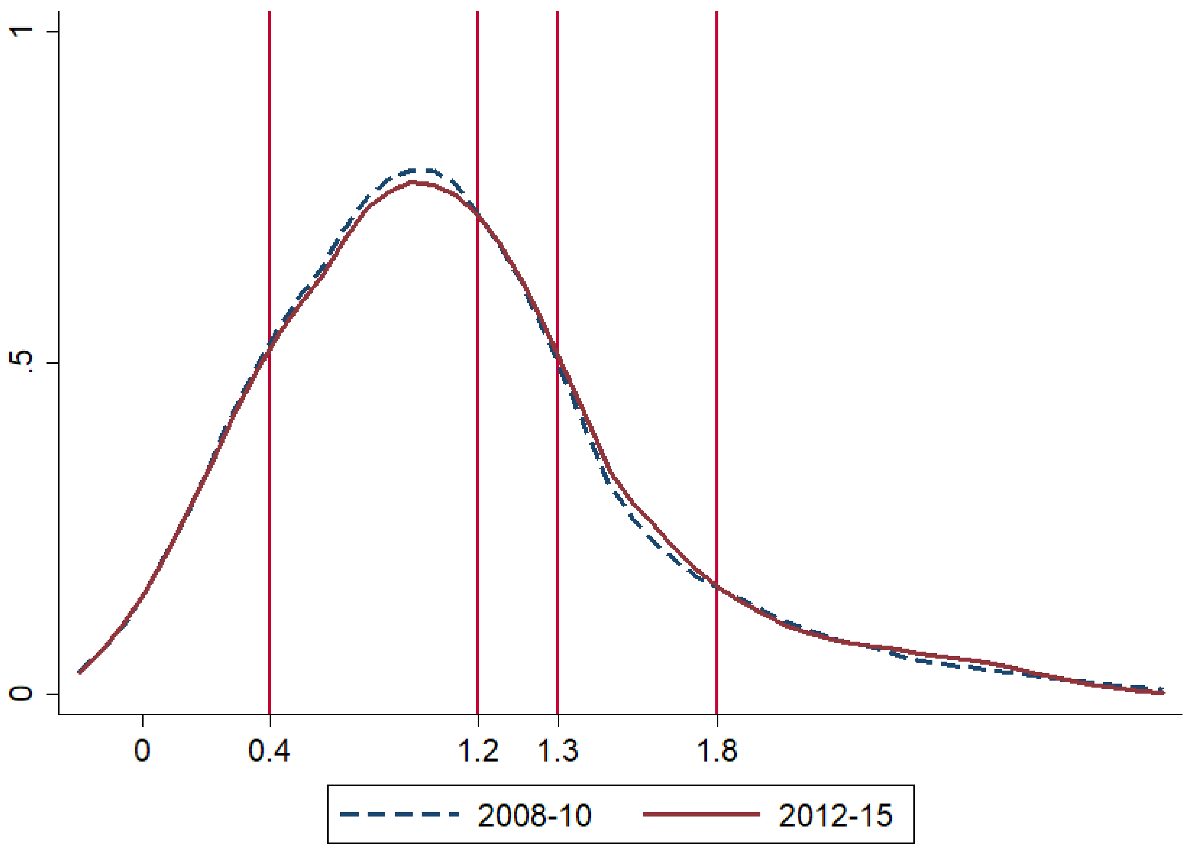
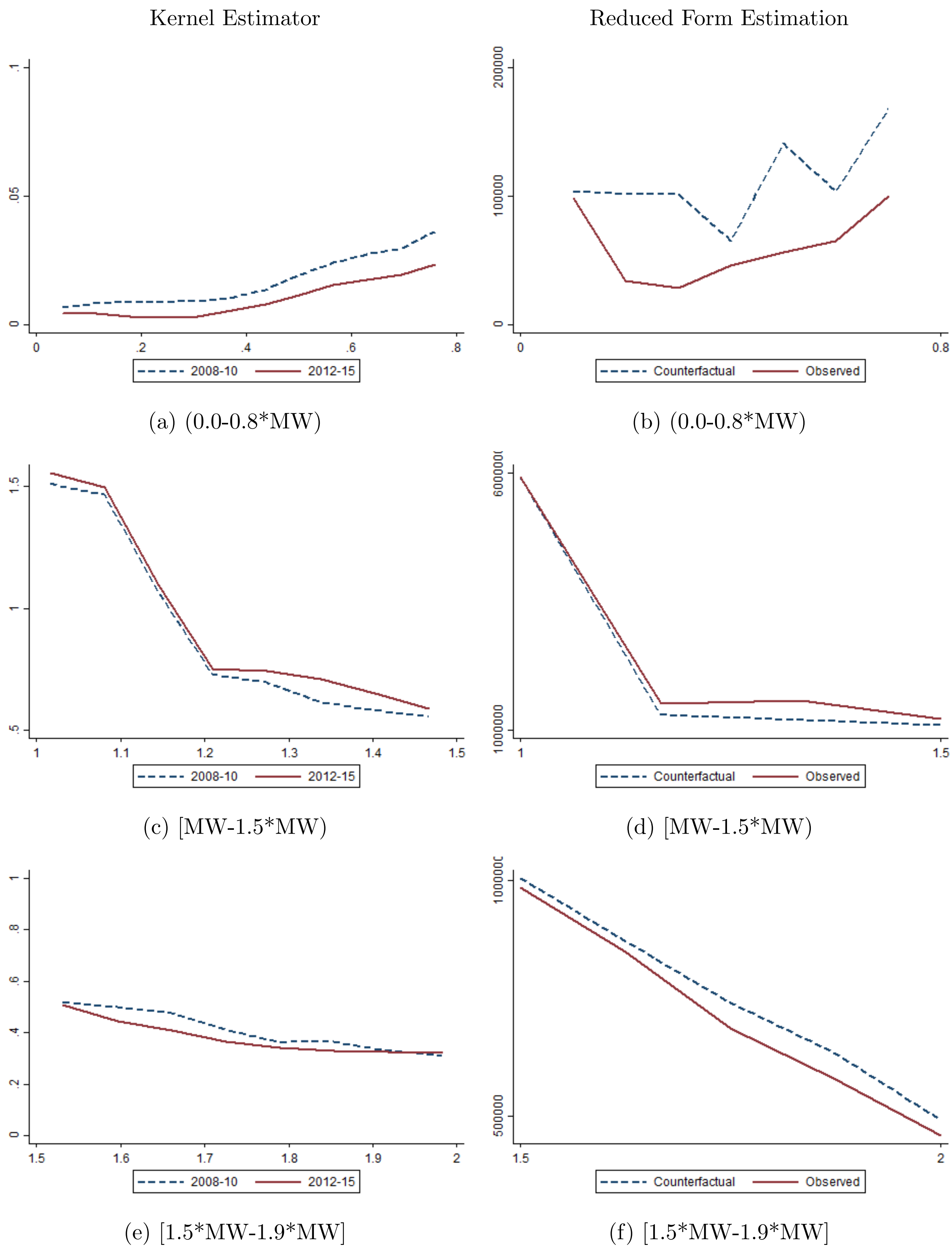


Figure 1.6: Formal Labor Earnings PDF, Kernel versus Reduced Form Estimates



(a) (0.0-0.8*MW)

(b) (0.0-0.8*MW)

(c) [MW-1.5*MW)

(d) [MW-1.5*MW)

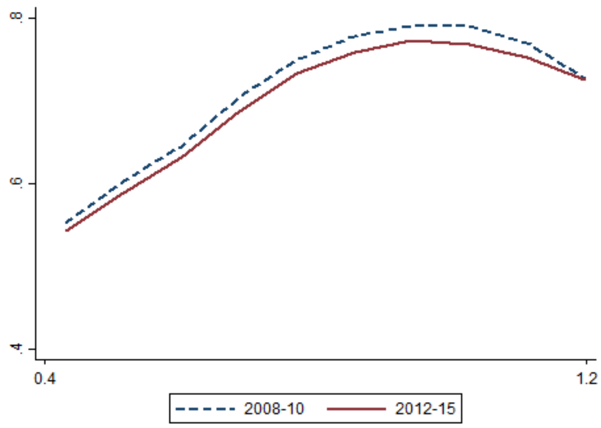
(e) [1.5*MW-1.9*MW]

(f) [1.5*MW-1.9*MW]

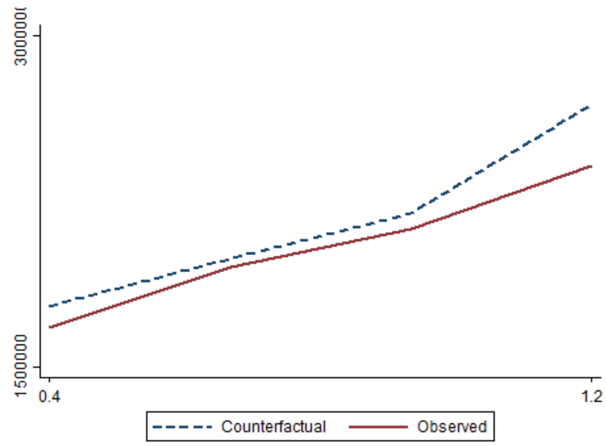
Figure 1.7: Informal Labor Earnings PDF, Kernel versus Reduced Form Estimates

Kernel Estimator

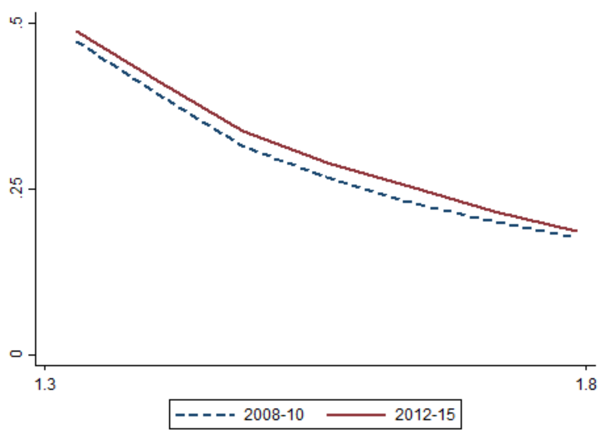
Reduced Form Estimation



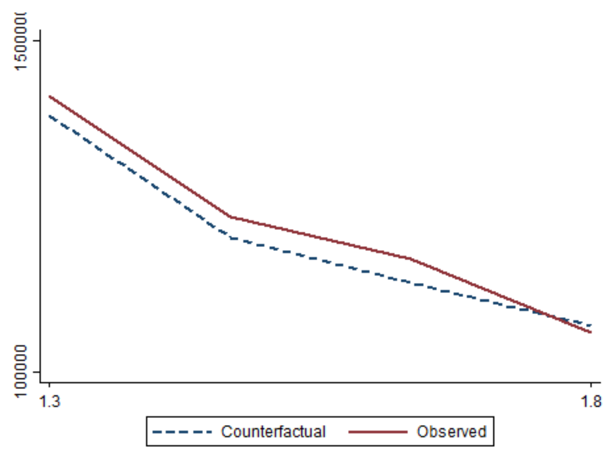
(a) [0.4*MW-1.2*MW]



(b) [0.4*MW-1.2*MW]



(c) [1.3*MW-1.8*MW]



(d) [1.3*MW-1.8*MW]

Figure 1.8: Fraction of Formal Workers in Treatment and Control Labor Earnings Regions

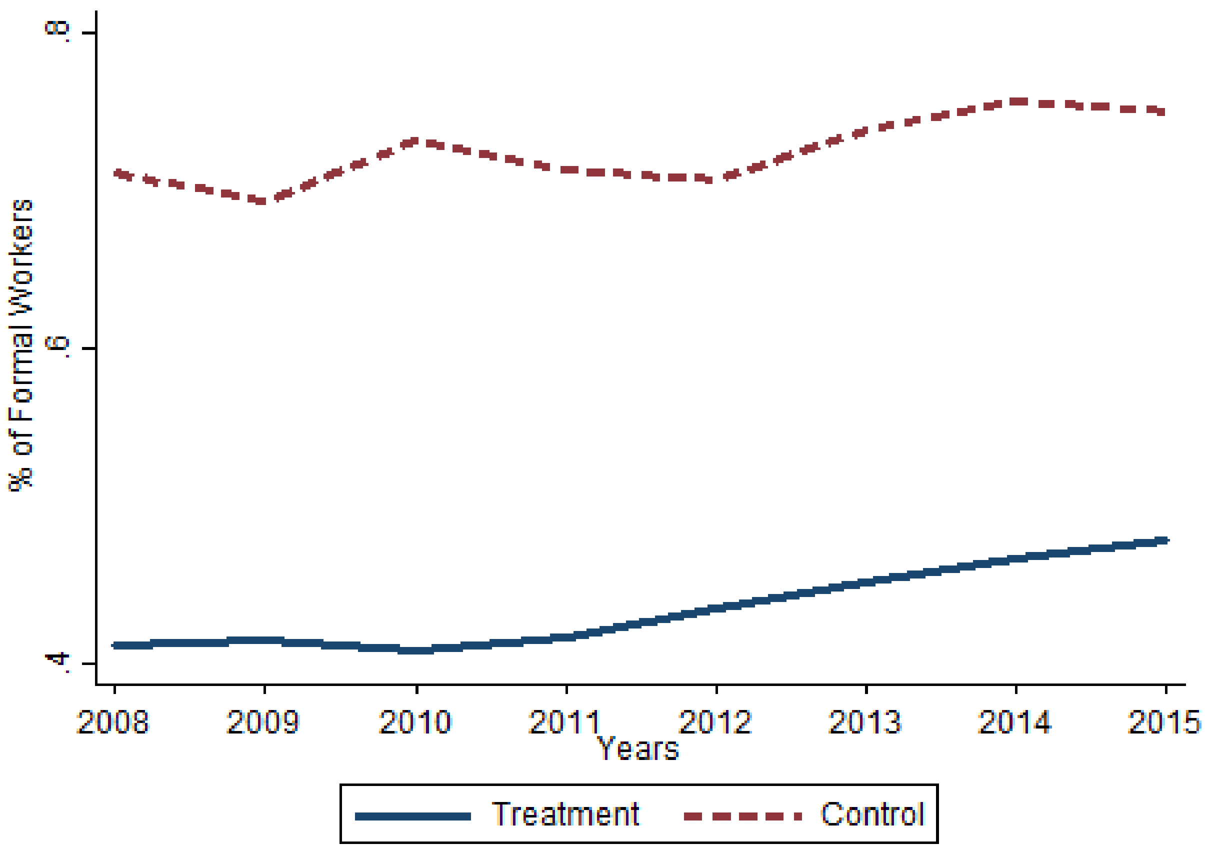


Figure 1.9: Change in the Size of the Formal Sector, By Earnings Bins

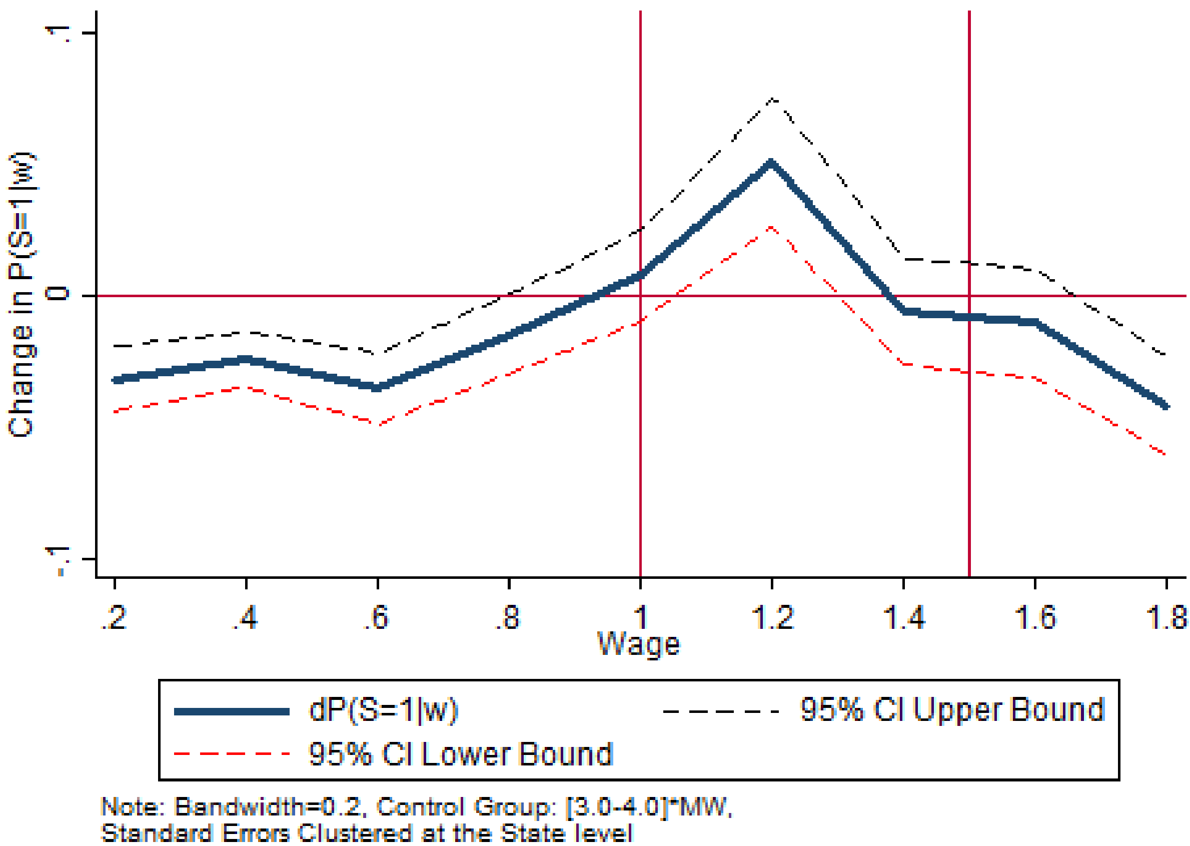
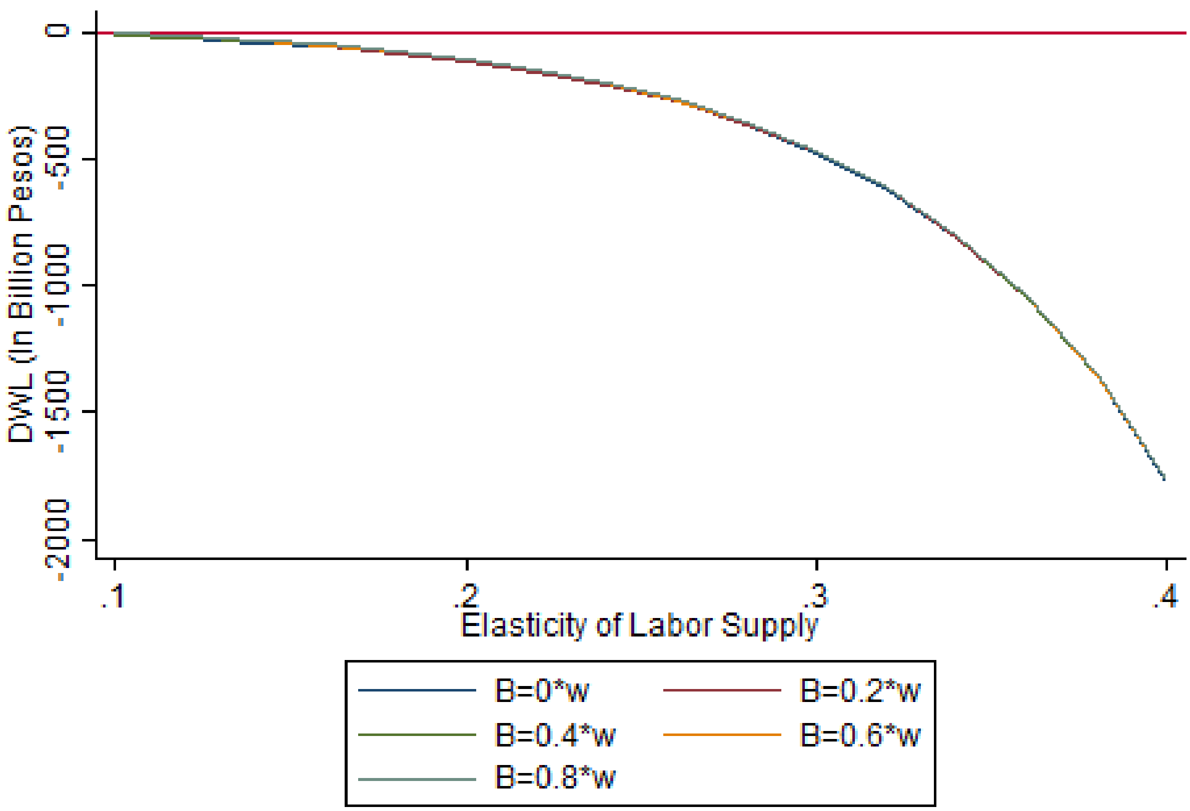


Figure 1.10: Deadweight Loss Calculation



Note: B denotes the worker's outside option net value.

Chapter 2

Do Tax Incentives Increase Small Firms' Survival Rate? (with J. Romero)

2.1 Introduction

Creative destruction is a solidly-rooted idea in economics. More creative innovative firms outperform competitors, making them eventually abandon the market. It implies, absent any market friction, that firms' mortalitiy is optimal and survivors are a key force driving endogenous aggregate output growth (Aghion and Howitt (1992)). However, markets are not frictionless. Entrepreneurs have limited access to capital, market conditions are uncertain, and existing firms exercise entry deterrence strategies. These, among other factors, call into question the efficiency of the observed number of market entrants and survivors.

Several government policies reflect this concern. To overcome entrepreneurs' limited access to capital, governments create business incubators, insure small businesses' bank loans and/or give venture capitalists tax incentives. To prevent monopolistic practices governments set up market regulator bodies, and to help small businesses navigate their early years they offer differentiated tax treatments or tax breaks. Yet, solid evidence about the impact of these type of policies on firms' entrance and survival is scarce.[1]

[1] Gale and Brown (2013)'s review of the literature of the effects of tax policy on small businesses and innovation reflects the relatively little attention that creation and survival of small firms has received.

This paper investigates the effects on entrance and survival of a cohort-based tax breaks program introduced in Colombia late in 2010. The program offered tax breaks to newly created, qualified small firms. The tax exemptions were phased out over a five-year period, and covered corporate income and payroll taxes, as well as the annual business registration fee. Dynamic models of firms' entrance and production under mild assumptions predict that subsidizing firms' costs, even temporarily, should have a positive impact on entrance and survival.

Using social security data at the firm level, we first tested whether firms manipulated the entry decision. We did not find evidence of changes in the patterns of firm creation, nor that firms created before the introduction of the tax breaks gamed the policy by moving their payroll to 'new' firms. Moreover, we did not find evidence that the introduction of the tax breaks program increased firm creation. To study the policy's impact on survival, we exploited the fact that to qualify, firms must have been created after December 31, 2010. We used duration models to compare the probability of survival, over a five years period, of firms created soon after the cut-off date, to firms created soon before.

The tax breaks could have also shifted labor demand out, increasing employment and wages.[2] Next, we compared current average employment and per-worker compensation levels of firms created soon after the cut-off date to firms created soon before. Since we observed imperfectly who the beneficiary firms were, we did not pursue an estimation of the local average treatment effect on the compliers (fuzzy regression discontinuity design), but an average intention to treat effect instead.

We found the availability of the tax break program had no effect on survival, employment or average per-worker compensation. The benefits take up rate is below 20%, which we attribute to the strictness of eligibility rules other than day of entry into the market and size. The relatively small take up rate, combined with the imperfect observability of

[2] It is a widely held view small firms create most of the jobs. Haltinwanger et al. (2013) confirm it is partially true. They find that conditioning on age, there is no systematic relationship between firm size and job growth, but that it is young businesses, which typically start small, where most of the job creation occurs.

what firms received the treatment, impeded disentangling whether the absence of effects is due to a small, near-zero, short-term elasticity of the outcomes under study with respect to the tax rates, or that the small take up rate makes any intention to treat effect undetectable.

This paper contributes primarily to the literature concerned with how taxes affect small businesses' entrance decision (Holtz-Eakin et al. (1994), Bruce (2000), Bruce (2002), Gentry and Hubbard (2004b), Gentry and Hubbard (2004a), Cullen and Gordon (2007), Gurley-Calvez and Bruce (2008)) and probability of survival (Bruce (2002), Gurley-Calvez and Bruce (2008)).[3] Most of this literature exploits differential tax treatments across corporate forms, which makes difficult to tell to what extent the estimated effects are reflecting true economic responses rather than avoidance behavior. The variation we use overcomes this limitation, since it applies to all corporate forms, and depends instead on day of entrance into the market.

The paper proceeds as follows. Section 2.2 describes the program, the data used, and the theoretical framework from which we draw predictions. Section 2.3 describes the identification strategy and how we implement it empirically. Section 2.4 presents the results. Finally, sections 2.5 and 2.6 discuss the results and conclude, respectively.

2.2 Institutional Setting, Conceptual Framework and Data

2.2.1 Formal Sector Regulations and the Tax Breaks Program

All formal firms in Colombia are subject to the following sets of regulations. At the national level, they must comply with corporate income, wealth, value added and payroll taxes. At the subnational level, they must comply with municipal sales and property taxes,

[3]Other authors have looked at employment and investment (Carroll et al. (1998b), Carroll et al. (1998a), Carroll et al. (2000)), financing (Evans and Jovanovic (1989), Holtz-Eakin et al. (1994)) and corporate form (Gordon and MacKie-Mason (1990), Carroll and Joulfaian (1997), Golsbee (2004)).

and state business registration and vehicle circulation taxes. Additional regulations may apply depending on industry (e.g. environmental regulation if in mining), location (e.g. special labor regulations if operating in areas with protected or vulnerable communities present), or size (e.g. tax or financial regulations if assets exceed certain thresholds).

Law 1429-2010 introduced a set of tax breaks for firms with 50 workers or less and assets valued at 5,000 minimum wages or less (U$1.2-1.5 million[4]). The tax breaks consisted of 100% of the Corporate Income ('CI') and payroll taxes[5] during the firm's first two years of existence, 75% in the third year, 50% in the fourth year, and 25% in the fifth year. From the sixth year on, they had to pay the CI and payroll taxes in full. It also exempted qualified firms of 100% of the business registration fee in their first year of existence, 50% in the second year, and 25% in the third year. From the fourth year on they had to pay the annual business registration fee in full. Additionally, beneficiary firms were not subject to CI tax withholding during the entire phaseout period, and had the possibility of carrying losses forward for up to five consecutive fiscal years.

In addition to satisfying the employee and asset thresholds, for firms to qualify they had to be *new* businesses, registered after the policy was passed -it was signed into law on December 29, 2010- and before December 31, 2014. The law explicitly excluded newly registered businesses whose capital and stockholders were the same as firms that existed before the policy. To qualify there was no requirement other than be a newly registered business, with 50 workers or less, and assets valued at 5,000 minimum wages or less. Firms from all industries, and located anywhere in the country could become beneficiaries.[6]

The law's original text was introduced in congress late in August, 2010, but it was not until November when the legislative process began. The relatively short period of time over which it was discussed and approved suggests there was limited room for anticipatory

[4]PPP adjusted, using OECD PPP rates.

[5]The payroll tax exemption applies over 10.5 percentage points (out of 52), or approximately 20% of all mandatory charges that have payroll as a tax base.

[6]The phase out period for three southern, remote states (Amazonas, Guainía and Vaupés) is ten rather than five years. Combined, these three states account for 0.3% of the country's population.

responses.

2.2.2 Conceptual Framework

The spirit of the tax breaks program (Law 1429-2010) was to incentivize the creation of formal firms by lowering the cost of regulation temporarily. It was not aimed at ameliorating non-competitive practices or limited access to capital, two other barriers often invoked by entrepreneurs. Therefore, given the nature of the program, we follow Arkolakis et al. (2018)'s model of firms' learning to formulate predictions on a firm's entrance and survival.

The Model: Environment and Solution

Consider an economy where time is discrete and denoted by t. Consumers maximize lifetime consumption, are endowed with one unit of labor they supply inelastically, and receive wage w_t in return. Additionally, they own an equal share of all firms.

Monopolistically competitive firms supply each good in the economy. Firms' objective is to maximize the sum of discounted expected profits. Firms' technology is linear in labor, the only input in production. Firms' heterogeneity is captured by productivity parameter z_i, which is known by the firm, exogenous, and time-invariant. In every period firms incur a fixed cost of production, measured in terms of units of labor. Demand uncertainty is captured by $a_t = \theta + \epsilon_t$, where θ is a time-invariant, unknown parameter that denotes the firm's product appeal, and ϵ_t is a transitory preference shock. Firms do not know a_t, but have a prior about its distribution, which they update every period after observing prices. Firms' expected per-period profits function is:

$$E\pi(z, b(\overline{a}, n)) = \frac{(\sigma-1)^{\sigma-1}}{\sigma^{\sigma}} b(\overline{a}, n)^{\sigma} \left(\frac{e^z}{w}\right)^{\sigma-1} \frac{Y}{P^{1-\sigma}} - wf \tag{2.1}$$

where $b(\overline{a}, n) \equiv E_{a_t|\overline{a},n}(e^{\frac{a_t}{\sigma}})$ is the expected per-period demand shock.

In every period there are J potential entrants, who decide to enter if the sum of expected discounted profits -given their prior about a_0- is greater than or equal to zero:

$$V(z,b,0) = E\pi(z,b) + \beta(1-\delta)E_{b'|b,n}V(z,b',1) \geq 0 \tag{2.2}$$

If they enter, they choose how much to produce. Existing firms decide to stay in the market if the sum of expected discounted profits -given that their updated estimate of a_t- is greater or equal to zero:

$$V(z,b,n) = E\pi(z,b) + \beta(1-\delta)E_{b'|b,n}V(z,b',n+1) \geq 0 \tag{2.3}$$

and if they decide to stay, choose how much to produce.[7]

Predictions on Firms' Entrance and Survival

Consider the case if the government subsidizes a fraction ρ of the fixed costs in the first year, and without lost of generality, assume the economy lasts two periods. A potential entrant chooses to enter if $V(z,b_0,0) \geq 0$. Solving for z:

$$e^z \geq \left[\frac{(1-\beta(1-\delta))wf - \rho f}{\frac{M}{\beta(1-\delta)}(b_0^\sigma - b_0'^\sigma)}\right]^{\frac{1}{\sigma-1}} w \tag{2.4}$$

where $M = \frac{(\sigma-1)^{\sigma-1}Y}{\sigma^\sigma P^{1-\sigma}}$. Taking the natural logarithm of (2.4), and making the relation hold with equality, yield $\underline{z}_1$, the minimum productivity level compatible with non-negative discounted expected profits. Note that absent the subsidy, the minimum productivity level observed among entrants, $\underline{z}_1'$, is larger. In other words, the subsidy allows less productive firms to be profitable. The number of entrant firms with and without the subsidy is given by:

$$[1 - H(\underline{z}_1)]J > [1 - H(\underline{z}_1')]J \tag{2.5}$$

where $H(z)$ is the productivity's CDF.

In period 2, operating firms decide to stay in the market and produce if $V(z,b_1,1) \geq 0$. Solving for z:

[7] For a full characterization of the equilibrium see Arkolakis et al. (2018).

$$e^z \geq \left[\frac{wf}{Mb_1^\sigma}\right]^{\frac{1}{\sigma-1}} w. \tag{2.6}$$

Taking the natural logarithm of (2.6), and making the relation hold with equality, yield $\underline{z}_2$, the minimum productivity level compatible with non-negative expected profits among operating firms. The number of surviving firms (firms that choose to operate in period 2) is:

$$[1 - G(\underline{z}_2)]J = [1 - \pi H(\underline{z}_2)]J \tag{2.7}$$

where $\pi = \frac{1}{1-H(\underline{z}_1)}$.

From (2.7) it follows the number of surviving firms is larger when fixed costs are subsidized in period 1:

$$\pi = \frac{1}{1 - H(\underline{z}_1)} < \frac{1}{1 - H(\underline{z}_1')} = \pi' \tag{2.8}$$

Note that expressions (2.5) and (2.8) hold under much simpler market structures. Only heterogeneity in productivity is necessary to generate these predictions. We can easily accommodate within this framework, then, ideas other than uncertainty about market conditions, such as small firms' limited access to capital, as in Carpenter and Petersen (2002).

The primary objective of this paper is to test the predictions derived from expressions (2.5) and (2.8) by exploiting the variation created by the tax break program described above. Before explaining the empirical strategy we pursued, the next subsection describes the dataset used.

2.2.3 Data

The empirical analyses of this paper use social security records at the firm level from the PILA. The PILA is the electronic platform through which payroll taxes are paid. Before the PILA, firms had to file separate forms for each of their payroll tax charges.[8] With the PILA, they file a single form and have the option of paying electronically. We have monthly data for the 2008-2016 period, but since it was not until 2009 that usage of the platform became mandatory, we shortened the period to June, 2009 to December, 2016.

The dataset contains monthly firm-level records of the number of employees and the wage bill's value. It also contains a flag that indicates whether the firm was a beneficiary of the tax break program. The only sample restriction was the exclusion of self-employees. We used a unique firm identifier to construct birth and mortality indicators; the first and last appearance in the dataset determined birth and mortality, respectively.

There are three main limitations of the dataset. First, by construction we only observe firms operating in the formal sector. Second, we do not observe any firm characteristic other than number of employees and the wage bill's amount. Third, we only know if firms were benefiting from the payroll tax breaks. From the latter, it follows we might underestimate the probability of receiving treatment if some firms were claiming the CI and business registration breaks, but not the payroll tax ones. In the results section we discuss the implications of this limitation of the data, and how it shaped our econometric approach.

Table 2.1 shows that over 300,000 formal firms operated at any given time between 2009 and 2015. Of them, three quarters had five or less employees, 15% 6-10, 8% 11-50, and 2% had more than 50 employees. The number of entering firms ranged from 3,500-4,000 per year, and the size distribution closely resembled that of all operating firms. The number of exiting firms was slightly below the entering ones, except in 2015 when it exceeded the number of newly created firms. Its distribution, on the other hand, was more skewed towards the bottom, with approximately 95% of exiting firms having 10 or less employees. Finally, since the introduction of the tax breaks approximately 15% of entering firms claimed benefits.

[8] Separate forms were required for the health contribution, the retirement contribution, workers' compensation and to each of the *parafiscales* (contributions to the CAJAS, SENA and ICBF).

2.3 Empirical Strategy

As access to the tax breaks depend on day of entry into the market, the empirical strategy consists of comparing firms created before and after the introduction of the policy. Since firm characteristics vary over time -e.g. more recent entrants might make more intensive use of technology, therefore are more productive, and more likely outperform competitors- we cannot directly compare firms created before and after the policy. However, we should expect differences to shrink, the closer the day of entry into the market is to the day the tax breaks became effective. This feature leads naturally to a regression discontinuity design ('RDD'), where we compare firms created just before the introduction of the tax breaks, to firms created just after.

To identify the effect of the tax breaks on the probability of survival, employment and average per-worker compensation, we run regressions of the form:

$$y_i = \alpha_0 + \beta_0 \cdot \mathbf{1}(t_i \geq 0) + \sum_{k=1}^{K} \alpha_k \cdot t_i^k + \sum_{k=1}^{K} \beta_k \cdot t_i^k \cdot \mathbf{1}(t_i \geq 0) + X\gamma + \epsilon_{t_i} \tag{2.9}$$

where y_i is the outcome variable, t_i the month of entry into the market of firm i, normalized so that $t = 0$ at the cut-off line of January, 2011, and X denotes a vector of control variables that include, either or both, month and firm size fixed effects. Lastly, k denotes the order of the polynomial.

The parameter of interest is β_0. It gives the average effect of the availability of the tax breaks program on firms' survival, employment and average per-worker compensation. Validity of the empirical strategy will depend on the absence of manipulation of firms' decisions about entry into the market. Therefore we first test for abnormal changes in the rate of firm creation around the time the program became effective. Furthermore, to assess the robustness of the RDD results, we include higher order polynomials ($k = \{1, 2, 3\}$), and use local linear regression. Likewise, following best practices, we accompany regression estimates with plots of the average outcome variables by month of entry, and draw on them a quadratic fit to the left and right of the cut-off date. If the introduction of the tax breaks program had an impact on survival, employment and/or average per-worker compensation, it should be visible graphically, and confirmed by the regression analysis.

2.4 Results

2.4.1 Creation of Firms

The rate of firm creation is one of the margins potentially impacted by the policy. However, it is also one firms could have manipulated. The concern is that firms that were planning to start operating late in 2010, when aware of tax breaks being discussed in Congress, decided to postpone their opening. Figure 2.1 shows strong seasonality in firm creation. It peaks early in the year, and drops significantly towards the end. However, the drop at the end of 2010, and the jump at the beginning of 2011 do not seem out of order with respect to other observed years. Figure 2.1 also shows firm creation did not trend up after the introduction of the policy. Moreover, there are no abnormal changes in the rate of firm creation around December, 2014, the last month when newly created firms could have started to claim the tax breaks benefits.

We tested separately for discontinuous changes ($\beta_0 \neq 0$) in the number of newly created firms right after the policy was put in place (January, 2011) and right before its expiration (December, 2014). Table 2.2 confirms that there are discontinuous changes in neither case. Around the start of the policy, parameters are statistically significant only when the polynomial is linear. Around the end of the program, no polynomial order yields statistically significant estimates. Table 2.3 confirms firm creation did not trend up during the time the tax breaks program was in place.

The evidence suggests firms did not manipulate the timing of the entry decision when the tax breaks program was introduced nor when it expired. The absence of a disruption in the pattern of firm creation around the time the tax breaks became effective, which we need for the validity of our empirical strategy, might reflect the short time firms had for anticipation. However, there might be other explanations for the absence of an upward trend in firm creation during the time the program was in place, and for the dearth of a disproportionate increase in firm creation before the benefits expired. The benefit's take up rate, at which we look in the next subsection, will shed light into the program's attractiveness.

Two other gaming strategies involve firms created before January, 2011. They might have had an incentive to set up new firms once the tax breaks program was in place, and move there part of their payroll and new investments. We can partially test if firms engaged in these gaming strategies, by looking at whether the average number of employees, and new capital investments dropped post introduction of the program. Data on capital investments come from the Colombia's Annual Census of Manufacturing Firms.[9] The evidence shows, for firms that existed before January, 2011, no change in the average number of employees on payroll, nor, in the case of manufacturing firms, any change in capital investments (see Appendix A.4).

2.4.2 Take Up of Benefits

Figure 2.2 shows, for firms created within the first six months the tax breaks were in place, the take-up of benefits went up from nearly zero to approximately 10-12%, remained at that level for firms created in the second semester of 2011, and increased progressively to 15% among firms created in 2012.[10] The stringent requirements to become eligible, and in particular, the requirement newly created firms cannot have the same stockholders than existing ones, should to a large extent explain the relatively low take-up rate. Setting up controlled firms is a prevalent practice, mostly motivated by tax and commercial considerations.

Note that we might be underestimating the take-up rate if newly created firms claim the CIT and/or business registration fee benefits, but not the payroll tax ones. If it were the case, it would result in an upwardly biased estimation of the local average treatment

[9]It is a census of firms with ten or more employees. We do not use this dataset more extensively because firms start being followed only when they reach the ten employees threshold, therefore for most firms the first year they are observed differ from the year of creation.

[10]The take up rate among 2013 and 2014 entrants remained around 15%.

effect on the compliers (benefit takers). To see why, recall the take up rate enters in the denominator of the RDD estimator (Imbens and Lemieux (2008)):

$$\tau_{RDD} = \frac{\tau^Y}{\tau^W} = \frac{lim_{x\downarrow c}E[Y|X=x] - lim_{x\uparrow c}E[Y|X=x]}{lim_{x\downarrow c}E[W|X=x] - lim_{x\uparrow c}E[W|X=x]} \tag{2.10}$$

where W denotes the treatment indicator.[11] Therefore, given the uncertainty about the potential underestimation of the probability of receiving the treatment (take up rate), we conduct an intention to treat analysis.[12]

2.4.3 Survival Analysis

In the survival analysis the dependent variable is the logarithm of the hazard rate ($y_i = log\lambda_i$), which is indicative of the probability of firm i disappearing in period t. The constant will in this case be the baseline hazard ($\alpha_0 = \lambda_0$), or the risk of disappearance for firms created soon before the introduction of the tax breaks. All other terms are as defined in equation (2.9). In estimating the Cox proportional hazard model, and plotting the survival probabilities, we use data on newly created firms from July 2010 to June 2011. All survival analyses are over a 5 year horizon (right censoring at 60 months).

Figure 2.3 compares the survival probabilities of firms created within three (2.3a) and six (2.3b) months, around January, 2011. They show that availability of the tax breaks have no effect on the probability of survival over a 5 year horizon. Regardless of whether firms entered into the market soon before or after the tax breaks became effective, approximately 15% of newly created firms disappear by month 12^{th}, a quarter by month 30^{th}, and almost half by month 60^{th}. By firm size at time of birth, those that started with five or less workers registered the same probability of survival over a 5 year period, regardless of whether they entered the market soon before or after the introduction of the policy; but those that started with more than five workers register a slightly higher probability of survival. The differences, though, for the latter group are either borderline significant, or not

[11] The RD is a sharp design if $|\tau^W|= 1$, a fuzzy design if $|\tau^W|< 1$.

[12] In RDD terminology, we use a sharp RD design ($|\tau^W|= 1$).

statistically significant (see figures 2.4 and 2.5).

Tables 2.4 and 2.5 contain the results from estimating the hazard model. The former reports on the coefficients, the latter on the hazard ratios. It confirms what the graphic evidence shows; availability of the tax breaks has no effect on the probability of survival over a 5 year period. The no-effect result is robust to adding linear, quadratic and cubic trends. Likewise, adding size-at-birth fixed effects, or interacting size-at-birth with the exposure indicator, does not alter the results.[13/14]

2.4.4 Other Outcomes: *Employment and Average Per-Worker Compensation*

If job creation is sensitive to taxes, and to the cost of regulation more generally, firms created after the introduction of tax breaks should on average be larger, and as a result of that, wages might, due to the outward shift in labor demand, also be higher. We compare average employment and per-worker compensation for years 2013-2016, by month of entry into the market. Figure 2.6 and 2.7 show there is not a sharp increase in employment, nor in average per-worker compensation, as a result of the introduction of the tax breaks program, respectively.

Table 2.6 tells a similar story. Panel A shows the employment estimates are not robust neither to the regression specification nor the estimation method. Parametric estimates yield statistically significant results when using a linear and cubic polynomial, but with reversed parameters' sign. A quadratic polynomial yields a positive, non-statistically significant estimate, while local linear regression gives a statistically significant negative effect. Panel B contains the results for per-worker compensation. In this case, when using a quadratic polynomial and local linear regression the effect is negative and statistically significant, whereas when using linear and cubic polynomials, the effect is positive but not

[13]Tables A.7 and A.8 in Appendix A.4 present results including size-at-birth fixed effects. Results including interactions of size-at-birth with the exposure indicator are available upon request.

[14]Evidence the proportionality assumption is satisfied is also available upon request.

statistically significant.

The lack of robustness is indicative of the absence of a significant effect on employment and average per-worker compensation. These results are quantitatively similar and qualitatively identical whether we use more or fewer years (e.g. years 2012 to 2016 or 2015 to 2016) or when adding size-at-birth fixed effects.[15]

2.5 Discussion

The main challenge for our identification strategy is a change in the composition of entrant firms as a result of the policy. In particular, the tax breaks pulled in a disproportionate number of low productivity firms. Since there is no reason to believe the policy distorted the entrance decisions of firms that were sufficiently productive, firms that would have entered even absent the policy, a clear sign of a massive compositional change would be a spike in firm creation soon after the introduction of the tax breaks. However, as figure 2.1 and table 2.2 show, the patterns of firm creation did not change. They did not change around the time the tax breaks became effective, nor during the time they were in place, nor soon before they expired.

Using data of manufacturing firms created between 2005 and 2015, from the Colombia's Annual Census of Manufacturing Firms, we also tested for differences in observable characteristics. We tested for differences in regional location, energy consumption, total and net investment, total non-financial assets, value added, number of employees and average salary. Table A.9 in Appendix A.5 shows firms created after 2010 are not statistically different in any of the above-mentioned characteristics, but, marginally and in an unexpected direction, in value added and average salary.

Lastly, we show in Appendix A.4 that firms created before January, 2011 did not reduce the number of workers they had on payroll. Had they done this, it would have raised

[15] Tables with results including size-at-birth fixed effects, or a larger or shorter set of years, are available upon request.

suspicions they gamed the program by setting up new firms, and moving their payroll there. Even though all this evidence suggests a change in the composition of firms does not seem to be an issue, we cannot rule out that it might have happened to some extent. We should be cautious and restate that, the internal validity of the results rest on the assumption we are comparing firms that are very much alike, except that those created since January, 2011 could benefit from the tax breaks program.

If we believe the approach we pursued is internally valid, why did the availability of the tax breaks result in a low take up rate, and no effects on survival, employment and average per-worker compensation? We can only propose possible answers. Beginning with the low take up rate, the most likely explanation is the strictness of the eligibility rules. To verify it, we will need to know the identity of stockholders of both new and old firms, information that is not available. Other possible explanations are firms' inattention, or aversion to being scrutinize by the National Tax Authority, but none of them, again, is verifiable with the data at hand.

Caution is also necessary in interpreting the absence of effects on the probability of survival, employment and per-worker compensation. It might be reflecting small, near-zero short-term elasticities of each of these variables with respect to the tax rates, or to the costs of regulation more generally. However, we cannot rule out the possibility that the relatively small take up rate makes undetectable any effect on the outcome variables we studied. Moreover, the availability of the tax breaks program might have impacted outcomes not studied here, such as output growth or the debt-to-assets ratio, or it may be that it is the long term not the short term elasticities that matter. Richer data and a longer time horizon should help disentangle some of these unresolved questions.

Policies aimed at stimulating the creation and growth of small businesses are permanently in the policy agenda of both developed and developing countries. This evaluation of the Colombia's 2011 tax breaks program seeks to inform this debate. Yet, before extrapolating its conclusions to other countries or contexts, we should give careful consideration to the program's characteristics, and the environment in which its introduction happened.

It was a temporary program, implemented in a highly informal economy, in a period of robust economic growth. Whether the results found here carry on to the case of a permanent program, or a permanent drop in tax rates for small firms, or to an institutional environment where firms' activities can be monitored more closely, or during a period of slow growth, will require further research.

Lastly, from an efficiency perspective incentivizing the entrance of firms should render welfare gains by means of lower prices, greater product variety and/or higher wages (Arkolakis et al. (2018)). Even though the program's efficiency is not the focus of this paper, our findings provide evidence the wage channel does not seem to be operating in this case.

2.6 Conclusions

This paper studies the impact on entrance, survival, employment and average per-worker compensation of a cohort-based tax break program introduced in Colombia late in 2010. The program offered tax breaks to newly created, qualified small firms over a five years period. Exploiting the fact that, to qualify firms must have been created after December 31, 2010 we compared firms created soon before the cut-off date to firms created soon after. We found the availability of the tax breaks had no effect on any of the outcomes we considered.

Programs aimed at stimulating the creation and growth of small firms are permanently in the policy agenda. They are typically justified as job creation programs. More subtle efficiency arguments also back them. This paper seeks to inform this policy debate by providing a well-identified intention to treat estimate of the impact of one of such programs on entrance, survival, employment and average per-worker compensation. Extrapolation of the findings of this paper requires caution. The program evaluated here was a temporary one, implemented in a highly informal economy in a period of robust economic growth. Whether the results found here carry on to the case of a permanent program, or a permanent drop in tax rates for small firms, or to an institutional environment where firms' activities can be monitored more closely, or during a period of slow growth will require further research.

Table 2.1: Number of Operating, Entering, Exiting and New Beneficiary Firms, 2009-2015.

	2009	2010	2011	2012	2013	2014	2015
A. All Operating Firms (Average)							
< 5 Workers	207796	208654	214317	223266	226857	231806	235934
$6-10$ Workers	37174	37333	38952	41691	44070	49227	53520
$11-50$ Workers	21359	21687	22963	25110	26896	30505	32983
$51+$ Workers	7218	7471	7888	8454	9107	10055	10793
B. Entering Firms							
< 5 Workers	4834	2787	2718	2873	2493	2829	2240
$6-10$ Workers	511	473	550	575	573	755	659
$11-50$ Workers	212	195	241	257	246	300	264
$51+$ Workers	23	22	30	37	30	36	30

Table continues in the next page.

	2009	2010	2011	2012	2013	2014	2015
C. Exiting Firms							
< 5 Workers	2083	1830	2025	2454	2635	3173	3490
$6 - 10$ Workers	297	265	295	308	303	390	414
$11 - 50$ Workers	110	101	115	116	119	143	159
51+ Workers	12	11	11	9	10	11	13
D. New Beneficiary Firms (Average)							
< 5 Workers	-	-	207	384	363	464	200
$6 - 10$ Workers	-	-	64	116	108	139	49
$11 - 50$ Workers	-	-	32	51	42	60	21
51+ Workers	-	-	1	2	2	5	3

Notes: The data comes from social security records at the firm level. Self-employees are not included. First and last appearance in the database defines firm creation and mortality, respectively.

Table 2.2: Firm Creation

PARAMETER	I	II	III	IV	V	VI
A. *Beginning of the Program*						
β_0	0.1958***	0.1870***	0.1478	0.0746	0.1818	−0.0741
	(0.057 3)	(0.049 4)	(0.101 6)	(0.069 1)	(0.125 9)	(0.096 3)
# of Observations	67	67	67	67	67	67
B. *End of the Program*						
β_0	−0.0578	−0.0736	−0.1509	−0.1465**	−0.1905	−0.2169*
	(0.069 8)	(0.056 0)	(0.104 8)	(0.072 8)	(0.150 3)	(0.111 2)
# of Observations	72	72	72	72	72	72
Month FE		YES		YES		YES
Linear Trend	YES	YES	YES	YES	YES	YES
Quadratic Trend			YES	YES	YES	YES
Cubic Trend					YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: To produce this table we used monthly data on total number of newly created firms from June, 2009 to December, 2016. Month/year of first appearance in the dataset determines firm's birth.

Table 2.3: Firm Creation Post Introduction of the Tax Breaks Program

PARAMETER	I	II	III	IV	V	VI
β_1	0.0180	0.0509	−0.0493	0.0164***	−0.0035	0.0114
	(0.011 1)	(0.026 7)	(0.061 5)	(0.004 8)	(.017 6)	(0.040 7)
β_2		0.0020	−0.0181		−0.0025	0.0027
		(0.002 9)	(0.014 6)		(0.001 5)	(0.009 6)
β_3			−0.0012			0.0002
			(0.000 9)			(0.000 6)
# of Observations	73	73	73	73	73	73
Month FE	NO	NO	NO	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: To produce this table we used monthly data on total number of newly created firms from June, 2009 to December, 2016. Month/year of first appearance in the dataset determines firm's birth. Results from using a linear polynomial are columns I and IV, from using a quadratic polynomial in columns II and V, and from using a cubic polynomial in columns III and VI.

Table 2.4: Probability of Survival, Hazard Model Coefficients

PARAMETER	I	II	II	IV
A. *+/-3 Months Around January, 2011*				
β_0	−0.0394	−0.0373	−0.0518	−0.0092
	(0.035 3)	(0.087 9)	(0.073 3)	(0.015 3)
# of Observations	34,520	34,520	34,520	34,520
B. *+/-6 Months Around January, 2011*				
β_0	−0.0694***	−0.0640	−0.0006	0.0519***
	(0.023 1)	(0.039 9)	(0.073 4)	(0.010 7)
# of Observations	70,795	70,795	70,795	70,795
Linear Trend	YES	YES	YES	NO
Quadratic Trend	NO	YES	YES	NO
Cubic Trend	NO	NO	YES	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Probability of Survival, Hazard Ratios

PARAMETER	I	II	II	IV
A. *+/-3 Months Around January, 2011*				
β_0	0.9612	0.9633	0.9494	0.9907
	(.034 0)	(0.084 6)	(0.069 6)	(0.015 1)
# of Observations	34,520	34,520	34,520	34,520
B. *+/-6 Months Around January, 2011*				
β_0	0.9328***	0.9379	0.9993	1.0533***
	(0.021 5)	(0.037 4)	(0.073 4)	(0.011 3)
# of Observations	70,795	70,795	70,795	70,795
Linear Trend	YES	YES	YES	NO
Quadratic Trend	NO	YES	YES	NO
Cubic Trend	NO	NO	YES	NO

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

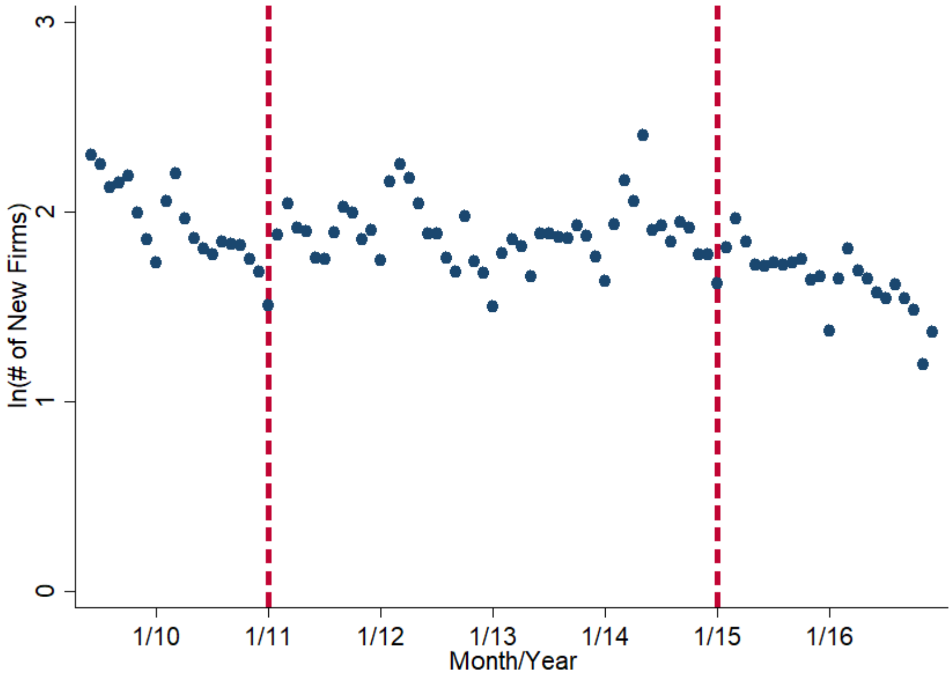
Table 2.6: Employment and Average Per-Worker Compensation, Intention to Treat Effect, 2013-2016

PARAMETER	I	II	II	IV
A. *Employment*				
β_0	0.2508***	0.0699	−1.2209***	−0.2497**
	(0.041 4)	(0.069 9)	(0.108 2)	(0.116 6)
# of Observations	4,959,029	4,959,029	4,959,029	4,959,029
B. *Per-Worker Compensation*				
β_0	0.0131	−0.3685***	0.0150	−0.0643***
	(0.009 0)	(0.015 0)	(0.023 1)	(0.023 7)
# of Observations	4,930,780	4,930,780	4,930,780	4,930,780
Month FE	YES	YES	YES	YES
Firm Size FE	YES	YES	YES	YES
Linear Trend	YES	YES	YES	NO
Quadratic Trend	NO	YES	YES	NO
Cubic Trend	NO	NO	YES	NO
LLR	NO	NO	NO	YES

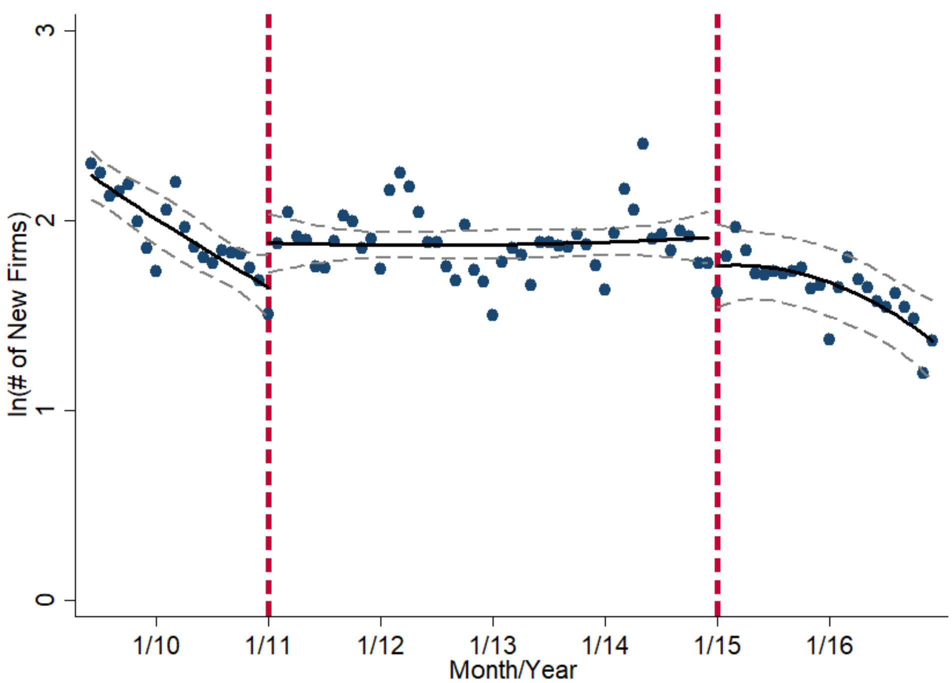
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: LLR stands for Local Linear Regression. The LLR estimate uses a triangular kernel and the Imbens-Kalyanaraman optimal bandwidth selector.

Figure 2.1: Monthly Average of the Number of Newly Created Firms, 06/2009 to 12/2016



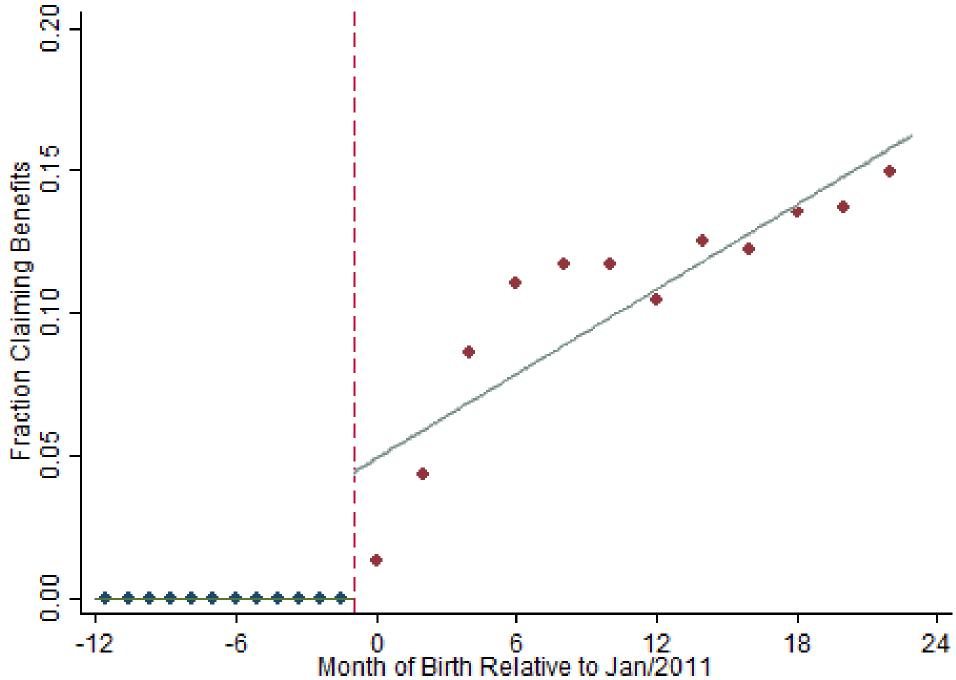
(a) Raw Data



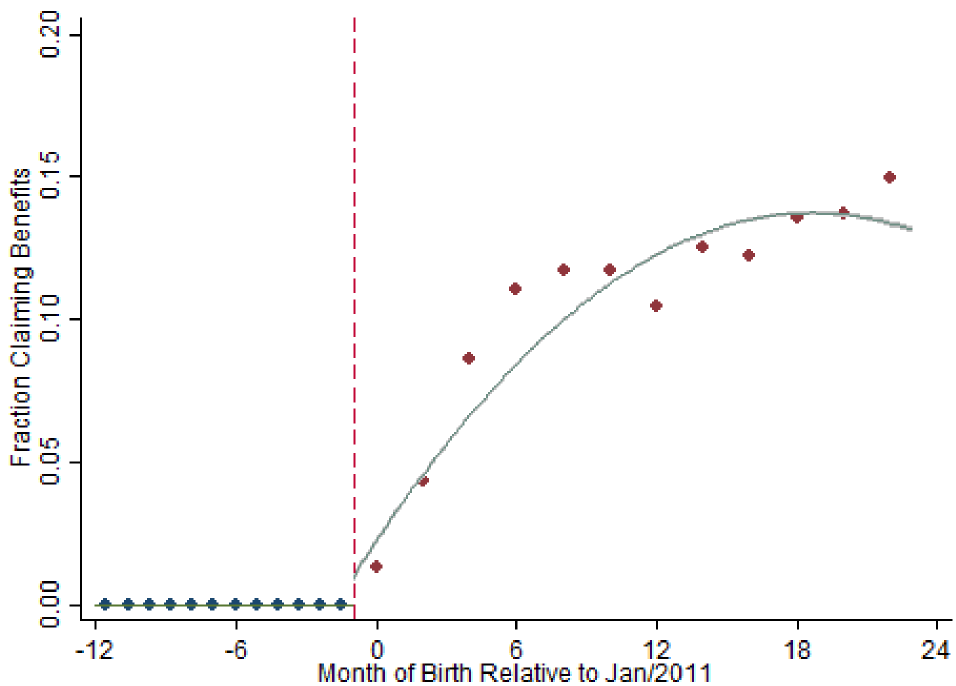
(b) Quadratic Fit

Notes: The dashed vertical lines denote the month/year when the tax breaks became effective and expired, respectively. Fitted lines in panel 2.1b come from the estimation of a quadratic polynomial. Each line was estimated separately.

Figure 2.2: Fraction of Newly Created Firms Claiming Benefits, by Month of Entry



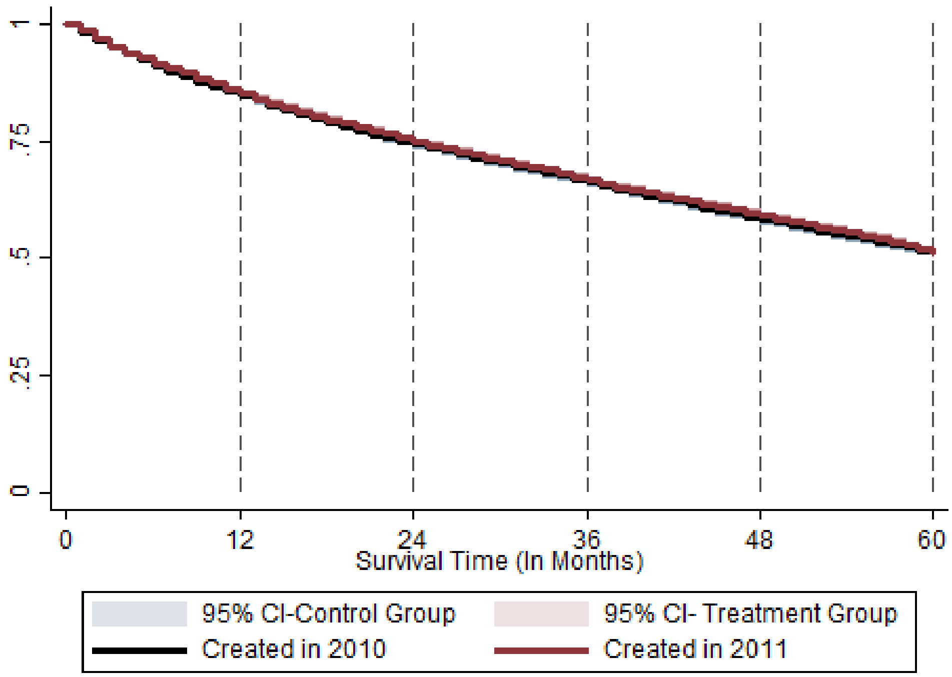
(a) Linear Fit



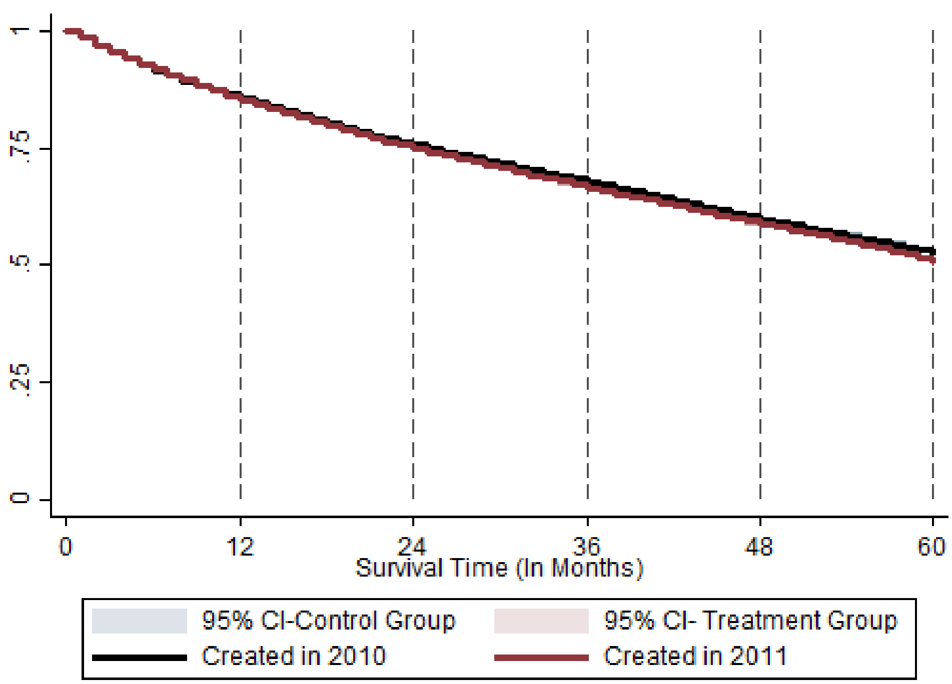
(b) Quadratic Fit

Notes: The graphs plot local estimates of the fraction of newly created firms claiming benefits. It includes firms newly created between January, 2010 and December, 2012. Take up of benefits correspond to payroll tax breaks only.

Figure 2.3: Kaplan-Meier Survival Estimates, 5 Year Horizon



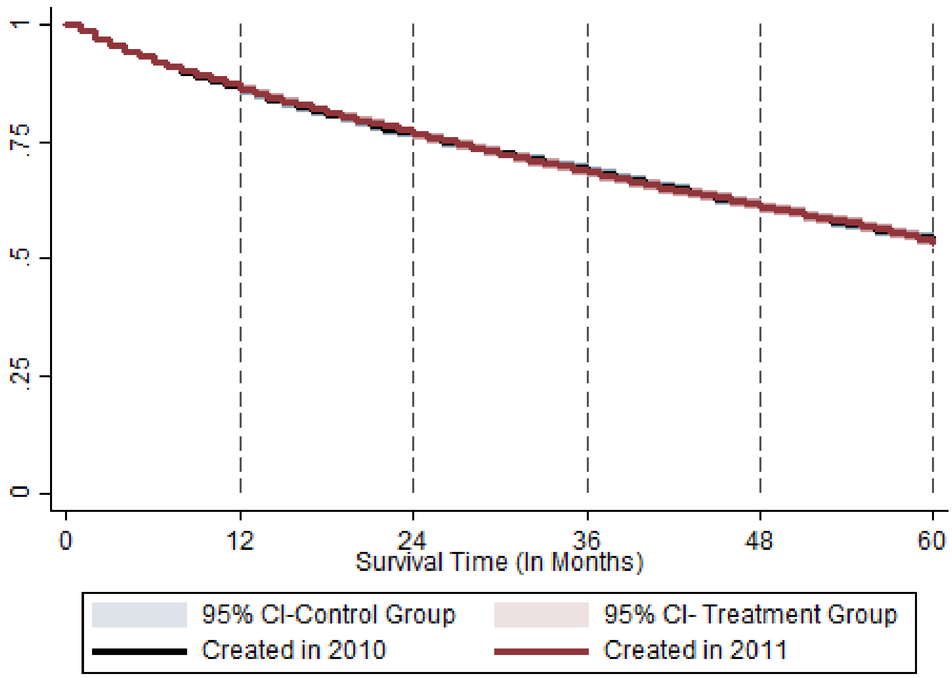
(a) +/-3 Month Around January, 2011



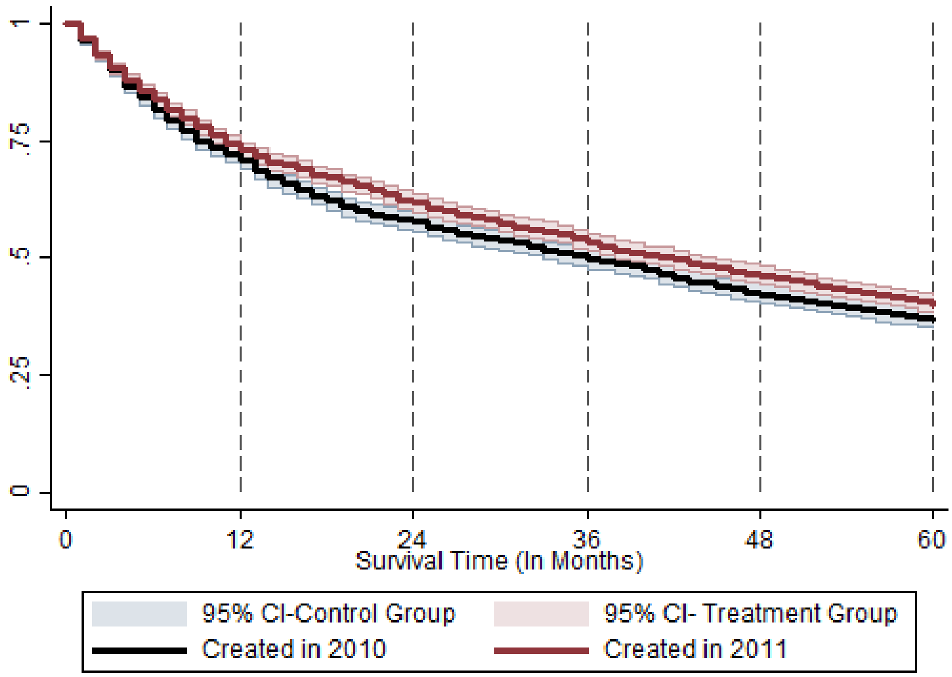
(b) +/-6 Month Around January, 2011

Notes: Survival time is censored at 60 months. The Kaplan-Meier method estimates the probability of survival non-parametrically, using an Epanechnikov kernel function and a fixed bandwidth.

Figure 2.4: Kaplan-Meier Survival Estimates, 5 Year Horizon, by Firm Size at Birth, +/-3 Months Around January, 2011



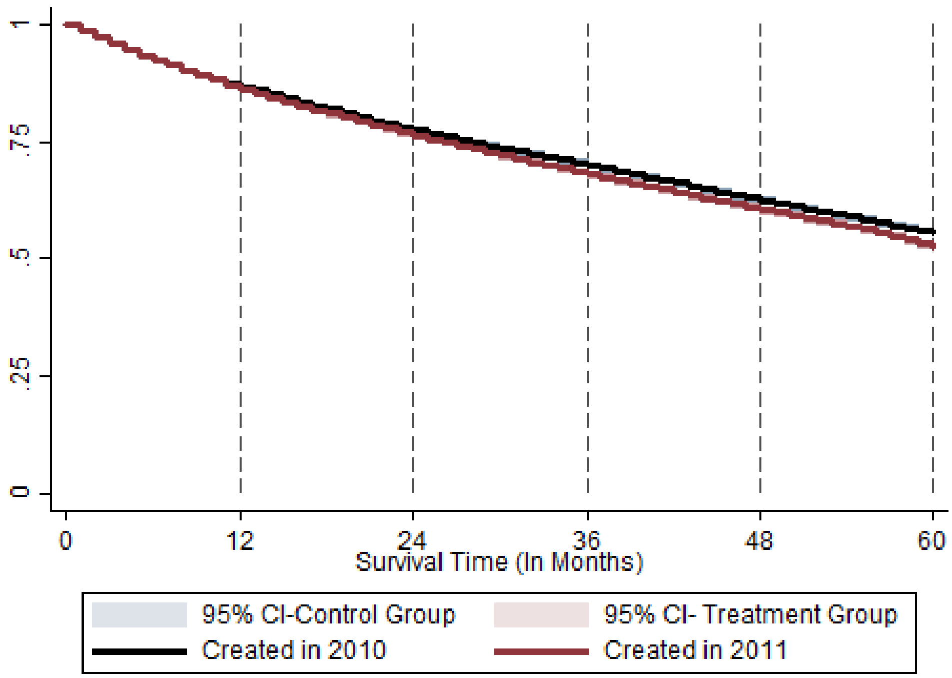
(a) Five or Less Workers



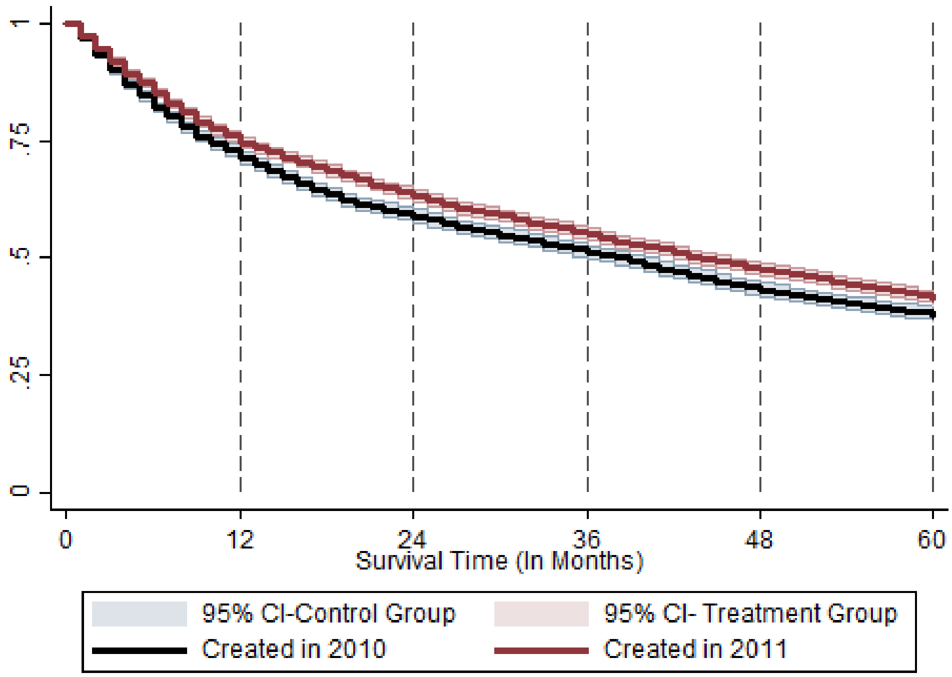
(b) More than Five Workers

Notes: Survival time is censored at 60 months. The Kaplan-Meier method estimates the probability of survival non-parametrically, using an Epanechnikov kernel function and a fixed bandwidth.

Figure 2.5: Kaplan-Meier Survival Estimates, 5 Year Horizon, by Firm Size at Birth, +/-6 Months Around January, 2011



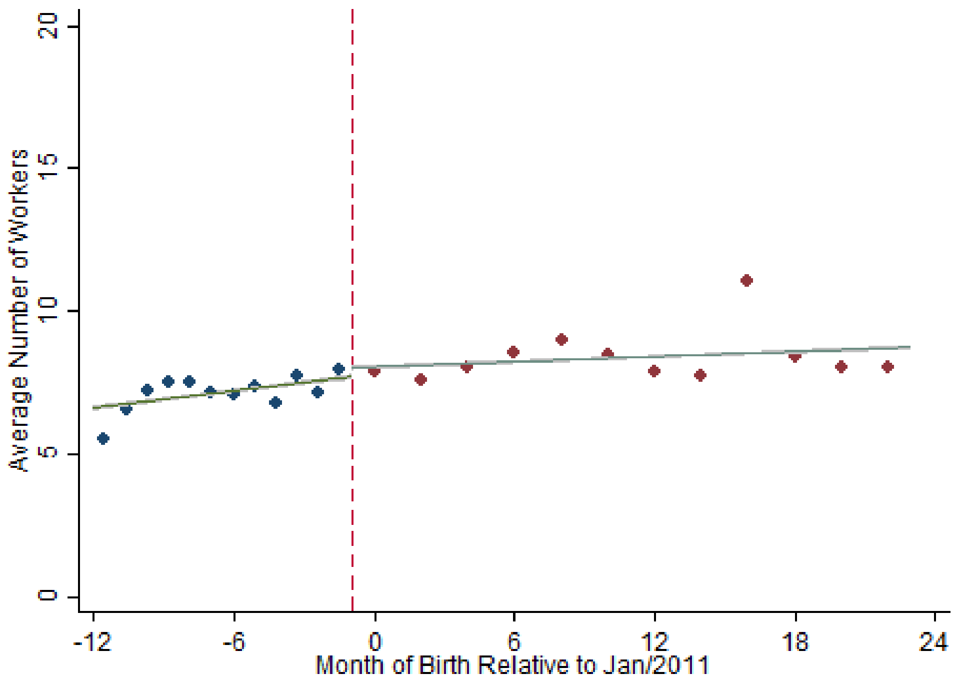
(a) Five or Less Workers



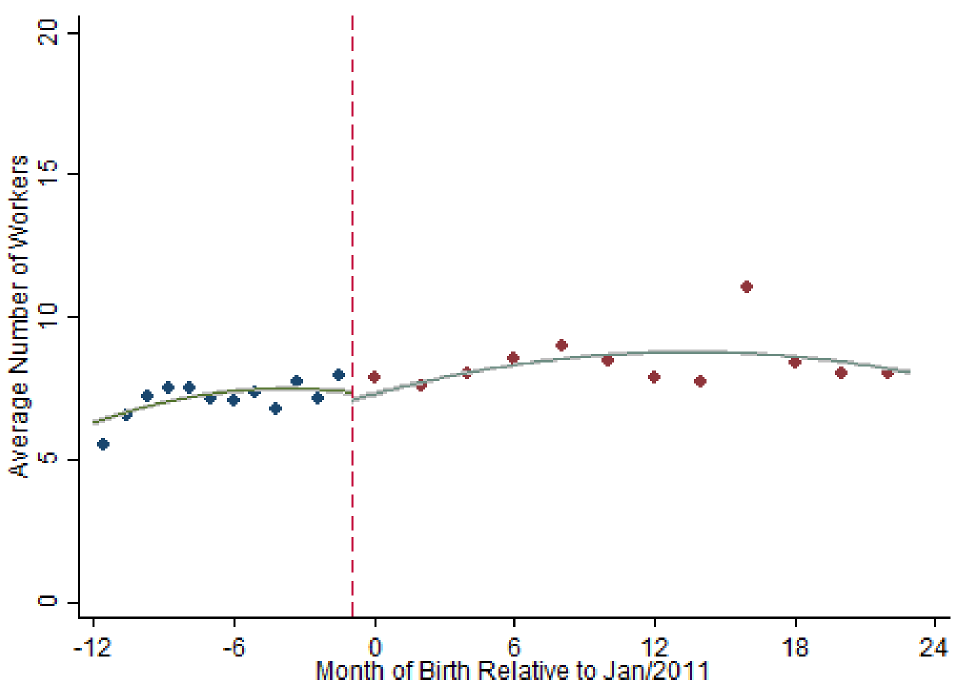
(b) More than Five Workers

Notes: Survival time is censored at 60 months. The Kaplan-Meier method estimates the probability of survival non-parametrically, using an Epanechnikov kernel function and a fixed bandwidth.

Figure 2.6: Average Employment on Years 2013-2016, by Month of Entry

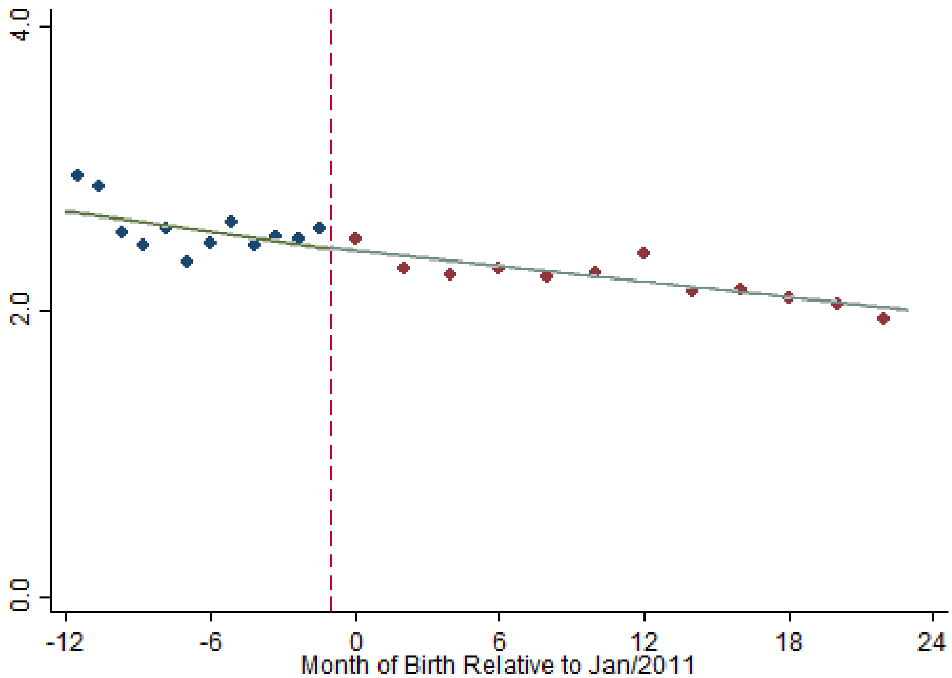


(a) Linear Fit

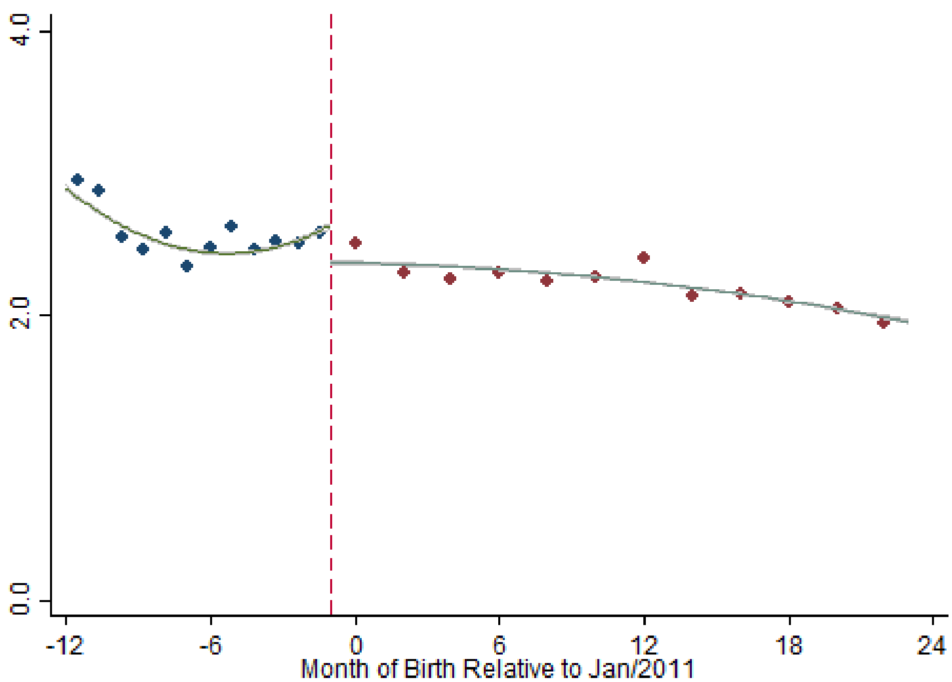


(b) Quadratic Fit

Figure 2.7: Average Per-Worker Compensation on Years 2013-2016, by Month of Entry



(a) Linear Fit



(b) Quadratic Fit

Chapter 3

Does Neighborhood Crime Cause Kids to Miss School? Evidence from NYC Daily Absenteeism Microdata (with A.E. Schwartz)

3.1 Introduction

While mounting evidence documents the impact of neighborhood violence on children's performance on standardized tests, less is known about the underlying mechanisms driving this effect. One leading hypothesis is that exposure to violence increases absenteeism, forcing kids to miss critical instruction and reducing their performance on subsequent standardized tests. There is, however, little evidence documenting a causal link between neighborhood violence and absenteeism due in part to a dearth of appropriate data. In this paper we exploit daily absenteeism data for NYC public school children, combined with detailed, blockface-level crime data, to estimate the impact of exposure to neighborhood violence on absenteeism. Our results provide credible causal estimates of the impact of neighborhood violence on absenteeism, contributing both to the ongoing debate about how neighborhoods affect kids' outcomes and to the growing literature on the causes of school absenteeism.

Street violence is one of the channels through which neighborhood conditions impact individuals. The most direct consequence is a higher likelihood of becoming a victim, but it goes far beyond that. Witnessing violence, knowing a victim or seeing others suffering a loss could lead to different forms of trauma from very young ages (Fitzpatrick and Boldizar (1993); Osofsky (1995); Osofsky (1999); Fowler et al. (2009)). Restricting outdoor activities, often a response to neighborhood crime, limits consumption of neighborhood public goods such as parks or sport facilities and isolates individuals from peers and, activities that are not only potentially beneficial in themselves but that could also counteract some of the harm done by exposure to violence.

School outcomes are natural candidates for trauma originated in neighborhood violence to manifest among children and teenagers. The school is after home the place where they spend most of their time and learning is a complex process, sensitive to an array of biology, family and environmental factors. Attention-deficit, irritability, aggressiveness, anxiety and depression, are all potential manifestations of trauma arising after exposure to violence, which could dampen the effectiveness of the learning process (Armsworth and Holaday (1993), Fowler et al. (2009), Sharkey et al. (2012), McCoy et al. (2015)).

Attending school is critical for learning,[1], yet is one of the margins parents, kids and even teachers might respond on due to exposure to neighborhood crime. Parents and kids might feel safer staying home to minimize exposure to retaliatory violence, or the experience could induce the child to behave disturbingly, so that parents, teachers, or both, might consider is better for him and others to stay home for a few days.

No one school day is more important than another is. Except maybe for someone who is about to perpetrate a crime, it is not possible to anticipate when a crime will occur. This paper uses the latter fact to identify whether kids miss school days as a result of exposure to a violent crime. Using daily absenteeism, and daily, blockface-level crime data, we compare absenteeism days immediately after exposure to days immediately before, and

[1]Goodman (2014) uses variation on winter weather conditions to instrument for school absences and closures, and finds that an additional school day missed due to snowy conditions drops performance in Math and English by 0.05 and 0.01 standard deviations respectively, both statistically significant estimates. The author also finds school closures due to severe weather conditions do not harm students' performance on standardized tests.

find that exposure to the first violent event increases absenteeism by 0.4 percentage points, a 5% to 10% increase in average absenteeism. The effect is statistically significant for boys and girls, all ethnic groups except Asians, from whom is not statistically different from zero, all grade levels and all types of violent crime. Students exposed repeatedly throughout the academic year, respond strongly to the second violent event (compared to the first one), but do not respond to the subsequent ones.

This paper contributes to two literatures. One is the literature concerned with understanding whether, and how, neighborhood violence affects student outcomes. The study's findings add to it by providing causal evidence that neighborhood violence affects school outcomes other than test scores, which had been the focus of previous research. The other is the small but growing literature on the causes of absenteeism, an outcome that, due to its meaningful implications for students, schools and families, has been increasingly receiving attention. For children it means less instructional time, therefore poorer academic achievement, while for schools and parents it poses coordination challenges. When a student shows up after missing a day or more of classes, teachers have to find a way to bring her up to speed with class material, while trying not to slow down the learning process of those who showed up consistently. For working parents, it costs at least leisure or work time, in trying to find someone to watch the child.

The rest of this paper is organized as follows: Section 3.2 reviews the existing literature on how neighborhood crime affect kids' outcomes, and on the causes of school absenteeism, Section 3.3 describes the data used, Section 3.4 describes the empirical strategy pursued to estimate the causal effect of exposure to neighborhood violence on school absenteeism, Sections 3.5 and 3.6 present and discuss the results, respectively, and Section 3.7 concludes.

3.2 Literature Review

Good schools, plenty of drug-free open public spaces, abundant offers of recreational facilities, little or no crime, and educated, well-connected, highly motivated neighbors (peers), are characteristics typically listed in descriptions of 'good' neighborhoods. The absence of these features is believed to adversely impact children's life trajectories, a belief partially supported by the latest long-term evaluations of the Moving to Opportunity ('MTO') experiment[2]. Chetty et al. (2016) find that children under 13 years old by the time their family moved to low-poverty neighborhoods more likely attended college, and in adulthood had higher earnings, lived in better neighborhoods, and were less likely single to be parents.

MTO does not inform about how much each of the channels or mechanisms through which a good or bad neighborhood might affect children contribute. One such potential channel or mechanism is crime. Sharkey (2010) and Sharkey et al. (2014) compare math and English scores of students exposed to a homicide[3] in the week before the test day to students exposed the week after, finding the former group had a poorer performance. This relatively recent evidence, even though a significant contribution to a literature that had mostly relied on observational or self-reported perception data, leaves critical questions unanswered. Standardized tests are intended to assess the accumulated knowledge on a certain subject, thus exposure to violence soon before the test day is most likely working through some form of trauma that negatively disturbs performance on the test. More permanent effects, or effects on outcomes whose consequences may last longer as might be the case with absenteeism are not explored yet.

The best teachers, best school practices, best school facilities and largest school bud-

[2] MTO started in 1994. The experiment randomly selected a sample of families with children living in public housing, and offered the treatment group housing vouchers valid only if they moved to low-poverty neighborhoods. Another treatment group received traditional housing vouchers, and the control group received nothing. For more details see http://www.nber.org/mtopublic/.

[3] A student was exposed if the homicide happened at the blockface where she lives.

get are all meaningless unless kids show up for classes consistently. Aligned with the intuitive idea that less instructional time leads to poorer academic achievement is the well documented negative correlation between absenteeism and test scores (Gottfried (2009); Gottfried (2010); Gottfried (2013); Gottfried (2014); Gottfried (2015); Gershenson et al. (2017)). Causality is harder to establish; the cleanest piece of evidence is probably Goodman (2014), who uses variation in weather conditions to instrument for school absences and closures, and finds that an additional school day missed due to heavy snow drops performance on Math and English by 0.05 and 0.01 standard deviations, respectively.[4]

A question that follows naturally is what the causes of absenteeism are. The literature addressing this question is for the most part qualitative, and points to three type of factors: the family, the school and the environment (Epstein and Sheldon (2002); Epstein and Sheldon (2004); Dude and Orpinas (2009)). Among family factors the list typically includes dysfunctional parental relationships, low valuation of school outcomes, and lack of resources to reinforce or supplement learning at home. At the school level, violence and bullying are factors, and at the environmental level neighborhood violence and the availability of complementary resources such as parks, sport facilities or libraries are included.

Summarizing, neighborhoods seem to matter for kids' outcomes in the long run, but a limited understanding of how neighborhood attributes affect daily choices persists. On the other hand, even though recognized as critical for learning, the study of the causes of absenteeism is in its infancy. This paper contributes to these two lines of research. It contributes to the one concerned with understanding how neighborhoods affect student outcomes by testing whether exposure to community violence causes absenteeism. To the other, the literature concerned with what induces students to miss school days, it contributes by providing credible causal evidence of one concrete factor, neighborhood violence.

[4]Goodman (2014) also finds school closures do not harm students' performance on standardized tests.

3.3 Data

To investigate the effect of exposure to neighborhood violence on school absenteeism, we needed to observe students' attendance, where they live, and where violent crimes occur. From the New York City Department of Education ('DOE') we obtained daily absenteeism records, plus information on students' socioeconomic characteristics and home addresses for academic year 2009-2010, and from the New York City Policy Department ('NYPD') daily, geocoded crime data for the 2009-2010 period.

3.3.1 Absenteeism

The DOE provides monthly absenteeism files for the universe of public school students. A student-day entry appears each time a student does not show up for classes. It does not report the absence motive, nor whether it is excused or unexcused. To complete each student's attendance history, we added school days with no absenteeism records, and marked them 'present' on those days.

We used the official academic calendar[5] to flag school breaks, and days when attendance was not mandatory (e.g religious holidays), or not required (e.g. fall and spring one-day teachers and administrators retreat). We also flagged days when schools closed due to heavy snow. Lastly, we flagged all September and June school days since there are attendance exceptions on several days in those months. In all empirical analyses we treat flagged days as non-school days.

Figures 3.2a and 3.2b show two stylized facts about absenteeism. First, it starts at a low point early in the academic year, trend up during the fall semester, and levels off at around 9% once students return from the Winter break. Second, absenteeism is higher on Mondays and Fridays, while typically reaches its lowest point on Wednesdays.

[5]To download the official NYC school calendar visit: http://schools.nyc.gov/Calendar/Archives

3.3.2 Crime and Definition of Exposure

We observe when and where every reported crime in NYC happened. Since we will not rely on students being a victim or witness of a crime, to consider her exposed, we restrict attention to the more salient forms of crime: homicides and aggravated assaults.[6] Combined, they represent 18%-20% of all crime in the city during the years under study.

Even though we can map the latitude and longitude of the crimes to the properties where students live, such definition of exposure is too restrictive. Violent crimes are disturbing, noticeable events, that typically have also attached substantial police and communication displays. We opt instead to geocode crimes at the block-face level (see Figure 3.1).[7] It allows to use a definition of exposure that incorporates students living in close proximity to the exact location of the crime, not just those living right where it occurred.

A total of 494 homicides and $19,847$ aggravated assaults happened in the course of academic year 2009-2010. Not all violent events result in exposure, since some occur in block-faces where no student lives. On the other hand, an event exposes several students if they live in the same block-face. Therefore, there is not a predefined correspondence between when and where a violent crime occurs, and how many students become exposed.

3.3.3 Exposure to Neighborhood Violence

To identify exposed students we need to have a common identifier ('id') in both the crime and attendance datasets. Such identifier is the block-face id. We begin geocoding crimes location to the block-face level. Then, we geocode student home address to the block-face level, and link it to the absenteeism dataset using the unique student id. Finally, we link crime and absenteeism datasets using the block-face id, and keep only students exposed to at least one violent crime.

[6] Homicides include murder and manslaughter. We will use the term 'violent crime' when we add homicides and aggravated assaults. This definition of violent crimes differ from the one used in the FBI Uniform Crime Report, which includes forcible rape and robbery.

[7] A block-face consists of both sides of the street between two intersections.

Critical for the identification strategy is variation across time and space in exposure to homicides and aggravated assaults. Figures 3.2c and 3.2d show the distribution of exposure by month and day-of-the-week, respectively. Exposure is well spread across months, while is higher on weekends, especially for homicides. Lastly, figures 3.2e and 3.2f show violent crime is also well spread across the city. High crime areas are not random, though. Zones of intense criminal activity are mostly on the Brooklyn side, near the Brooklyn-Queens border, and the Bronx.

The non-random spatial distribution of violent crimes results in sets of exposed and non-exposed students that are not alike. Exposed students are poorer, by an almost ten percentage points' difference, than non-exposed students (see Table 3.1), a reflection of the fact violent crime is more prevalent in disadvantaged neighborhoods. It reflects also on the poorer performance of exposed students on standardized math and reading tests, and on blacks and Hispanics being overrepresented. Not surprisingly, the greater the degree of exposure, the more disadvantage the population becomes. As we move across columns, the fraction poor increases, as does the fraction that is black or Hispanic, and the worse the math and reading scores are.

3.4 Empirical Strategy

The primary econometric exercise we pursue consists of comparing absenteeism days immediately after exposure to a violent crime, to absenteeism days immediately before. Equation (3.1) outlines the baseline specification:

$$absent_{i,t} = \alpha_0 + \alpha_1 Post_i + \theta_{dw} + \pi_{dc} + \mu_i + \epsilon_{i,t} \tag{3.1}$$

where $absent_{i,t}$ takes 1 if student i, on school day t, was absent, 0 otherwise, and $Post_i$ takes 1 for calendar days 0 to 14 (after exposure), 0 for days -1 to -14 (before exposure). θ_{dw} and π_{dc} denote a set of day-of-the-week ('DoW'), and day-of-exposure ('DoEX') fixed effects, intended to account for the weekly patterns of absenteeism, and the potential imbalances in the composition of what days we use to compare absenteeism before and after exposure, respectively.

The identification assumption for α_1 to give the average causal effect of exposure to a violent crime on school absenteeism, is that students cannot anticipate, within a relatively narrow window of time, when a violent crime will occur.[8] A potential concern is that neighborhood choice is a strong predictor of exposure. If students living in high crime neighborhoods are less attached to schools, our estimate of α_1 might be upwardly biased. The strategy we pursue, though, is well suited to address this concern, because what it requires for identification is that conditional on violence level, individuals cannot predict precisely when and where a crime will occur. We remove all students time-invariant unobserved characteristics by adding student fixed effects (μ_i).

We restrict the comparison of absenteeism around exposure to two weeks before and after. We opt for estimating a short-run, acute effect, since the farther apart from the day of exposure, the more likely underlying trends in absenteeism might bias the results. An alternative would be to open the window, and add flexibly a variable that measures the number of days relative to the day of exposure. This approach, equivalent to a parametric regression discontinuity design, has the disadvantage, in this context, that we run quickly into an unbalanced sample, because the number of days before and after exposure varies greatly across students. Our approach to estimate $\hat{\alpha_1}$ using (3.1), is in fact equivalent to estimating a regression discontinuity design specification using local linear regression, given the plus/minus 14 days window is the optimal bandwidth. Optimal bandwidth calculations suggest it ranges from approximately 8 to 21 days, depending on the optimization criterion, and the sample used (see Table 3.2).

Over 40% of exposed students live in blockfaces where more than one violent crime

[8]The assumption is not testable. Standard balance tests on observable characteristics are not informative, since we are comparing average absenteeism before and after exposure for the same set of individuals. Searching for a discontinuity in the forcing variable, in this case the number of calendar days relative to the day of exposure, around the value that determines treatment, is not informative either, since in this context it is continuous by construction.

occurred throughout the academic year.[9] We adjust (3.1) to accommodate exposure to multiple events, using the following specification:

$$absent_{i,t} = \alpha_0 + \sum_{j=1}^{J} \alpha_{1,j} Post_{i,j} + \sum_{j=1}^{J} \alpha_{2,j} Window_{i,j} + \theta_{dw} + \pi_{dc} + \mu_i + \epsilon_{i,t} \tag{3.2}$$

where we now use the entire academic year, not only the two weeks before and after the violent crime. J indexes the crime number, and variables $absent_{i,t}$, $Post_{i,j}$, θ_{dw}, π_{dc} and μ_i are defined as above. As in (3.1), the parameter of interest is $\alpha_{1,j}$, which is identified under the assumption students cannot predict within a narrow window of time when the crime will occur. The new term in the specification, $Window_{i,j}$, takes 1 for days -14 to 14, 0 otherwise, and measures average absenteeism in the two weeks before and after exposure, relative to the rest of the academic year.

One of the questions that raises repeated exposure is how the response to the first violent crime, interacts with exposure to subsequent events within the academic year. Table 3.3 shows a non-negligible fraction of repeated exposure cases happen within a relatively short time distance. We slightly modify (3.2) to dig deeper into this question:

$$\begin{aligned} absent_{i,t} = \alpha_0 &+ \sum_{j=1}^{J} \alpha_{1,j} Post_{i,j} \\ &+ \sum_{j=2}^{J} \alpha_{2,j} Distance_{i,j} + \sum_{j=2}^{J} \alpha_{3,j} Post_{i,j} * Distance_{i,j} \\ &+ \sum_{j=1}^{J} \alpha_{4,j} Window_{i,j} + \theta_{dw} + \pi_{dc} + \mu_i + \epsilon_{i,t} \end{aligned} \tag{3.3}$$

where $Distance_{i,j}$ measures how far in terms of calendar days two consecutive events are, and $\alpha_{3,j}$ will tell how the response to an event other than the first changes as the temporal distance in exposure increases by one day. Notice that $Distance_{i,2}$ is defined only for

[9] Of the 345,436 students exposed to at least one violent crime during academic year 2009-10, 188,953 (54.7%) were exposed to one violent event, 81,868 (23.7%) to two, 36,064 (10.4%) to three, and the remaining 38,551 (11.2%) to four or more. Restricting attention to the 16,108 students exposed to homicides, 15,593 (96.8%) were exposed to one, and the remaining 515 to two.

students exposed to at least two violent crimes, $Distance_{i,3}$ only for students exposed to at least three violent crimes, and so on. If we estimate (3.3) pooling all exposed students together, we will be recovering the parameters only for those exposed the greatest number of times. What we will do instead, is to stratify the sample by number of times exposed, and run (3.3) separately on each.

We further explore for heterogeneous responses, by interacting the pre-post exposure indicator in equation (3.1) with gender, race/ethnicity and grade level indicators, as well as with dummies for when within the week exposure happened.

3.5 Results

We begin presenting results for homicides. They have two characteristics that favor a clean implementation of our empirical strategy. First, it is the most salient form of violent crime, therefore the less likely to go unnoticed. Second, there are few cases of repeated exposure. Subsequently, we replicate the analyses adding aggravated assaults, and present evidence of the results' robustness.

All estimates are based on cases of exposure that happened between October 15, 2009 and May 31, 2010, except those that took place two weeks before or after the winter, mid-winter and spring breaks. The latter exclusion seeks to ensure a balanced number of absenteeism observations before and after exposure for most analyzed students.

3.5.1 Homicides

Since the vast majority, over 96%, of students exposed to homicides were so to only one, we start showing the effect on absenteeism of exposure to the first homicide. Column I in table 3.4 contains the result of estimating equation (3.1) without any of the fixed effects. This raw comparison of average absenteeism in the 14 days after exposure, to the 14 days before, yields a statistically significant estimate of 0.54 percentage points ('pp'), a 5% increase with respect to average absenteeism at baseline (14 days before). Column

II adds DoW fixed effects, which slightly increases the coefficient to 0.56, and column III adds DoEX fixed effects, which increases it further to 0.65. Column IV contains our preferred specification, which includes student fixed effects. The within student comparison in absenteeism increases the coefficient to 0.75, an over 7% increase in average absenteeism.

We then investigate differences by day-of-exposure, gender, race/ethnicity and grade level. Panel A in table 3.5 shows absenteeism in the two weeks after exposure increases by 1.0 pp, a 10% increase in average absenteeism, if it occurs from Monday to Friday, while if it happens on Saturdays or Sundays the change is not statistically different from zero. This result might be reflecting systematic differences in the geography of crime between weekdays and weekends, and/or differences in the extent and type of family support kids receive depending when during the week exposure to crime happens.

Boys and girls elicit an equally strong response when exposed to a homicide (panel B in table 3.5); the increase in absenteeism for boys is 0.75 pp, and for girls 0.74 pp, but the difference is not statistically different from zero. Among race/ethnic groups (panel C in table 3.5) the response is strongest for whites, 1.62 pp, follow by Hispanics, 1.04 pp, and blacks, 0.5 pp. For Asians it is not statistically different from zero. These differences among race/ethnic groups suggest there is not a clear correlation between the likelihood of being exposed, and the response's magnitude. Both Asians and whites are underrepresented among exposed students, but for the former the response is not distinguishable from zero, while for the latter it is the largest. Similarly, blacks and Hispanics are overrepresented among exposed students, but the response of the latter groups is twice as large as the response of the former.

By grade level we observe the largest response among middle school students, for whom absenteeism increases by 1.5 pp, follow by the 0.7 pp increase of K-5 students. Neither for special education students, nor for high school students the effect is statistically significant. For special education students the coefficient is relatively large, but imprecisely estimated, while for high school students is both small and imprecisely estimated.

Taken together, these results suggest: i) the effect of exposure to a homicide on absenteeism is fairly robust to alternative specifications, ii) average absenteeism increases by 5% to 8% as a result of exposure to a homicide, iii) the effect is not driven by a single gender, race/ethnic group or grade level, and iv) the effect of a homicide on absenteeism varies among race/ethnic groups, grade levels, and depending on whether exposure happens on weekdays or weekends.

Next, we investigate how students respond to a second homicide (no student was exposed to more than two homicides). First, we estimate separately the response to each homicide. Columns I and II in table 3.6 compare the response to the first homicide of students exposed to one homicide only, and two homicides only, respectively. Note the response of students exposed to one homicide only is essentially the same we obtain, when we estimate the response to the first homicide pooling all students expose to homicides. On the other hand, the response to the first homicide of students exposed to more than one, is significantly bigger. However, it is imprecisely estimated and we cannot tell whether it is different from zero. Column III shows the response to the second homicide. It is substantially bigger and precisely estimated. In the two weeks after exposure to the second homicide, absenteeism increases by 4.65 pp.

Stacking all events of exposure in order to estimate the response to the first and second homicide in the same regression specification, yields quantitatively similar, and qualitatively identical results. Column I in table 3.7 shows, in line with previous findings, exposure to the first homicide increases absenteeism by 0.7 pp. The response to a second homicide is smaller compared to what we obtain when we estimate the response to each event separately, but still several orders of magnitude greater than the response to the first homicide. Exposure to a second homicide increases absenteeism by 3.1 pp. Column I in table 3.7 also shows absenteeism in the +/- 14 days around exposure, is not different, neither for the first homicide nor the second, than absenteeism in the rest of the academic year. The latter finding is reassuring because a potential concern is that an underline trend in absenteeism might confound the results, but what this shows is that absenteeism in the vicinity of exposure does not seem to deviate from what it is on average.

Column II in table 3.7 shows the results of estimating the response to the first and second homicide in a single regression, but this time adding the interaction of exposure to the second event with the distance between the first and second homicide. Note these results correspond to students exposed to two homicides only, because the distance between events is only defined for them. As before, we find a substantially stronger response to the second homicide. The interaction of the exposure indicator of the second homicide with the distance between the two homicides suggest the farther apart the two homicides are, the smaller the increase in absenteeism. Even though the coefficient of the interaction term is not statistically significant at conventional levels, its magnitude is not trivial. Compared to the two homicides happening on the same day, the increase in absenteeism is roughly 16% smaller (-0.04*30/7.3) if the distance between them is 30 days.

These findings about the effect of repeated exposure to homicides on absenteeism, point to students becoming 'sensitize' to neighborhood violence, at least in terms of their response to the outcome under study in this paper.[10] However, we might be concerned the population exposed to two homicides is different than the rest of the NYC public school students, and inferences based on them will not be informative for other segments of the population. To assess whether the results described so far are generalizable to a larger population, we next add aggravated assaults to our definition of exposure. By adding this type of violent crime the exposed population grows from less than 1% to over 30% of public schools students, and the cases of repeated exposure increase substantially, since over 40% of exposed students live in block-faces where more than one violent crime happened during the academic year. Adding aggravated assault will also permit to explore whether the magnitude of the response differs depending on the type of violent crime.

[10] We borrow the term 'sensitize' from the 'sensitization' hypothesis proposed in the psychology literature on adaptation to violence (Ng-Mak et al. (2004), McCart et al. (2007)). This literature also proposes the 'desensitization' hypothesis, and there does not seem to be a consensus on which of the two dominates.

3.5.2 All Violent Crime

To expedite computations, estimates for all violent crime use a 5% random sample of students exposed in academic years 2009-10 or 2010-11. We focus on the response to up to the third violent crime, since about 90% of exposed students were so to a maximum of three violent crimes.

Column I in table 3.8 shows that for students exposed to only one violent crime, exposure increases absenteeism by 0.4 pp, an approximately 5% increase in average absenteeism in the two weeks after exposure compare to the two weeks before (average absenteeism in the two weeks before exposure for this group is 8.7%). The first thing to notice is that the response to aggravated assaults is less strong than to homicides,[11] a finding consistent with the fact aggravated assaults typically involve less police display, thus are more likely to go unnoticed. On the other hand, even if students are aware of their occurrence, they might be less traumatic.

Columns II to III in table 3.8 show, for students exposed twice, the response to each event estimated separately. The response to the first violent crime is of a similar magnitude, 0.33 pp, than the response of students exposed once. The response to the second violent crime on the other hand, is substantially larger, 1.0 pp. For students exposed three times (see columns IV to VI), the response is statistically significant for none of the events. The size of the coefficient for the first event is small and negative, while for the second and third is around 0.3 pp each, but imprecisely estimated. Note also average absenteeism in the +/- 14 days around exposure to the first event is below the academic year average, around the second event is not statistically different than the average, and for the third event is above the average, a pattern consistent with an upward trend in absenteeism, since by construction the first exposure tend to cluster on days at the beginning of the academic year, and exposure to a third violent crime on days towards the end of the academic year. We look deeper into this issue in the coming section.

[11] The magnitude of the response is driven by students exposed to aggravated assaults. Excluding students exposed to homicides leave the size of the coefficient unchanged.

Next, we simultaneously estimate the responses to the first, second and third event for students exposed to at least one violent crime. Column I in table 3.9 shows exposure to the first violent crime increases absenteeism by 0.2 pp, and exposure to the second by .35 pp. The effect of exposure to a third event is small and not statistically significant. In columns II and III we estimate the model for students exposed two and three times only, respectively. We find the same patterns, but the estimates are in no case statistically significant. Furthermore, we interact the post exposure indicators with the distance between the first and second, and second and third event. An extra day between two violent crimes has a near zero, not statistically significant effect on absenteeism.

Adding aggravated assaults to the definition of crime used here, attenuates the magnitude of the effect exposure to neighborhood violence has on absenteeism, but it remains statistically significant. Similar to what we found for homicides alone, the response to exposure to a second violent crime is stronger than to the first, but interestingly, it is not the case for exposure to a third violent crime, for which we find a zero-effect. We dig deeper into the latter finding by exploring whether accumulated absences constraint the response to events of exposure that happen towards the end of the academic year. Concretely, first exposure tend to happen early in the academic year, whereas the third exposure in the last months. Since students accumulate absences as the academic year progresses, a greater risk of truancy might limit their ability to miss school days when exposed late in the academic year. To test whether this is what might be happening, we restrict attention to exposure to the first violent crime, and conduct the following two exercises. First, we interact the post exposure indicator with a semester (fall/spring) indicator, and second, we interact the exposure-times-semester indicator with the fraction of school days missed up to the day before exposure. If accumulated absences constraint students ability to miss school, the effect of exposure should be less strong in spring, and conditional on semester, less strong among students who have missed a higher fraction of school days.

Tables 3.10 and 3.11 show the effect of exposure to a homicide or aggravated assault has a greater effect when it happens in the fall semester. When exposure occurs in the spring semester, the effect is small and not statistically significant. They also show absenteeism increases for students who has missed less than 5% of school days (up to the day of exposure) regardless of whether it happened in fall or spring, but the effect is small and not significant for those who have missed near 10% or more school days. It suggest school institutions and norms interact with the dynamics of exposure to neighborhood crime. Schools must keep track of student absences, and warn them and their families of the consequences of not showing up for classes.

3.5.3 Robustness Checks

We perform three robustness checks. First, we do a placebo test in which we assign exposed students random days of exposure. Second, to address concerns of potential unaccounted time trends, we strengthen our preferred specification by adding month and calendar-day fixed effects. Lastly, we estimate equation 3.1 using local linear regression:

$$\min_{\{\alpha_0 \dots \alpha_3\}} \sum_{i=1}^{n} \Big(absent_{i,t} - \alpha_0 - \alpha_1 POST_i - \alpha_2 (D_{i,t} - c_i) \qquad (3.4)$$
$$- \alpha_3 POST_i \cdot (D_{i,t} - c_i) \Big)^2 \cdot K(h)$$

where $absent_{i,t}$ and $POST_i$ are defined as above. $D_{i,t}$ and c_i denote calendar-day and day of exposure, respectively, K denotes the kernel function and h the bandwidth. We use a triangular kernel and the fully data-driven, mean squared error minimizer bandwidth proposed by Imbens and Kalyanaraman (2012) for sharp designs ('IK bandwidth').

Row 1 in table 3.12 shows the result of the placebo test. Assigning randomly the day of exposure yields a small, noisy, non-statistically different from zero estimate of the placebo day of exposure on absenteeism. Row 2 shows replacing semester fixed effects by month fixed effects barely change the results. Adding calendar-day fixed effects attenuates the response's size, and makes it relatively less precise, but it remains statistically significant at conventional levels (row 3). Finally, using the IK bandwidth (row 4) does not change the estimate of the response's size; it becomes slightly less precise, but remains statistically significant.

3.6 Discussion: *Why Do Kids Miss School As a Result of Exposure to Violent Crimes?*

The empirical approach pursued here to identify the causal average effect of exposure to violent crimes on school absenteeism is silent regarding why students are more likely to miss school days after getting exposed to a violent crime. We consider two linked channels to be the most probable causes. One is fear of retaliatory violence. This might be especially important for students living in neighborhoods with prevalent gang activity. The other mechanism is the development of a form of trauma. There is abundant evidence documenting the appearance, beginning at very young ages, of different forms of trauma as a result of exposure to community violence. They manifest in different forms, from mood changes to physical symptoms, and differ in intensity from one individual to another (Sharkey et al. (2012), Fowler et al. (2009), McCoy et al. (2015)). What mechanism dominates, or to what extent each explains the effect we are identifying, remains an open question.

For students living in high crime neighborhoods, exposure to violence might become frequent. Psychology offers explanations for both greater sensitization and greater desensitization as a result of repeated exposure to violence. In a world with heterogeneous individuals, some becoming sensitized, some desensitized, interpreting responses to repeated exposure is not a straightforward exercise. Our results suggest school institutions might also play a role in constraining students' capacity to respond on the attendance margin, which makes it even harder to interpret results from students exposed multiple times. A more comprehensive study of repeated exposure will require following students across several academic years, a task we intend to pursue once appropriate data becomes available.

Finally, extrapolating from the finding that absenteeism is sensitive to severe winter weather conditions (Goodman (2014)), students living in poor neighborhoods are, in any academic year, possibly exposed to many more violent events than to severely bad weather days. Additionally, absenteeism due to exposure to violent crime is potentially more damaging, since the learning impairing effects of trauma associated with exposure to violence could still be present after the student returned to classes.

3.7 Conclusions

This paper explores the role neighborhood violence plays on school absenteeism. It exploits across-space variation in the timing of violent crimes in NYC during academic year 2009-2010, to identify the average effect of exposure to a violent crime on school absenteeism. Detailed administrative data on the universe of NYC public school students, including their daily absenteeism records and residential address as well as daily blockface level crime data, allows us to identify students exposed to every single violent crime, and compare their absenteeism days immediately after the event to the days immediately before.

Exposure to the first violent crime a student is exposed to during the academic year increases absenteeism in the two weeks after exposure by 0.4 percentage points, a 5% to 10% increase in average absenteeism. The effect is present across genders, race/ethnic groups, grade levels, and violent crime types. Students exposed repeatedly throughout the academic year respond strongly to the second event, regardless of whether it is a homicide or aggravated assault, but do not respond to subsequent ones. We cannot tell with certainty what the drivers of the dynamics of responses to repeated exposure are, but the evidence suggests one factor should be being at risk of truancy, which suggests school institutions play an important role in molding students' absenteeism behavior.

This study's results have implications for school outcomes that are sensitive to absenteeism, such as test scores, grade promotion and high school graduation. However, extrapolation requires caution. In a strict sense, our findings do not imply a causal relationship between exposure to community violence and these other outcomes. More research is needed to establish, in a causal manner, those links. Likewise, the results are for the most part informative of students living in high crime neighborhoods.

Table 3.1: Observable Characteristics, Non-exposed versus Exposed Students, Academic Year 2009-2010

	Non-exposed	Any Exposure	Agg. Assaults Only	Homicides Only	Agg. Assaults and Homicides
% Chronically Absent	20.0	26.3	26.2	25.7	30.3
Average Daily Absenteeism	7.5	9.0	8.8	9.0	10.1
% Poor	74.8	84.7	84.6	84.5	85.7
Math Z-Score (3rd to 8th Grade)					
Mean	0.09	-0.19	-0.18	-0.21	-0.30
Reading Z-Score (3rd to 8th Grade)					
Mean	0.08	-0.17	-0.17	-0.18	-0.23
By Ethnicity (%)					
Asian	19.1	9.7	9.8	10.0	6.2
Hispanic	35.1	47.5	47.8	47.3	40.5
Black	27.3	37.4	37.0	38.9	50.7
White	18.5	5.3	5.4	3.8	2.6
Number of Students	715,303	345,436	329,328	4,839	11,269
% of Public School Students	67.4	32.6	31.1	0.5	1.1

Notes: Chronic absenteeism refer to missing 10% or more of school days for any reason. 'Any Exposure' refers to exposure to at least one violent crimes. 'Aggravated Assaults Only', 'Homicides Only' and 'Aggravated Assaults and Homicides' refer to exposure to at least one aggravated assault, at least one homicide, and at least one aggravated assault and one homicide, respectively. Poverty is measured as access to free and reduced price lunch at school.

Table 3.2: Optimal Bandwidth Calculations

Method/Sample	1st Event Left of c	1st Event Right of c	2nd Event Left of c	2nd Event Right of c	3rd Event Left of c	3rd Event Right of c
Imbens & Kalyanaraman I	13.180	13.180	21.744	21.744	16.281	16.281
Imbens & Kalyanaraman II	13.902	14.211	16.340	14.736	15.536	16.675
Imbens & Kalyanaraman III	8.349	8.349	-	-	-	-
Imbens & Kalyanaraman IV	10.469	8.982	-	-	-	-

Notes: I&K I and I&K II are estimated using a 5% random sample of all students exposed to at least one violent crime during academic years 2009-10 and 2010-11. I&K III and I&K IV are estimated using the sample of all students exposed to a homicide event during academic year 2009-10. These are only computed for the first event since the vast majority of students exposed to homicides are exposed to one. All estimators correspond to the Imbens and Kalyanaraman (2012) optimal bandwidth selectors implemented in Stata by Calonico, Cataneo and Titiunik (2014). Rows 1 and 3 show results using the selector that imposes the same bandwidth size on both size of the threshold, while in rows 2 and 4 the one that allows them to differ.

Table 3.3: Distance Between Violent Events

A. *Violent Crimes*

Range of Days	1st to 2nd Event Freq. (%)	2nd to 3rd Event Freq. (%)	1st to 3rd Event Freq. (%)
1-9 Days	7.6	5.7	0.0
10-19 Days	9.1	6.2	1.2
20-29 Days	8.1	9.0	1.2
30-39 Days	7.9	8.5	2.3
40-49 Days	6.3	9.4	3.2
50-59 Days	6.9	7.3	3.5
60-69 Days	5.5	6.3	3.5
70-79 Days	4.8	4.1	3.6
80-89 Days	4.0	5.1	2.7
90-99 Days	3.3	3.0	3.6
100+ Days	36.7	35.4	75.3

B. *Homicides Only*

Range of Days	1st to 2nd Event Freq. (%)		
1-19 Days	10.2	-	-
20-39 Days	13.0	-	-
40-99 Days	58.0	-	-
100+ Days	18.8	-	-

Notes: Panel A is estimated using a 5% random sample of students exposed during academic years 2009-10 and 2010-11. Panel B is estimated using all students exposed to at least one homicide during academic year 2009-10. No student was exposed to more than two homicides.

Table 3.4: Effect of Exposure to a Homicide on Absenteeism, First Homicide, AY 2009-10

	I	II	III	IV
Post	0.0054**	0.0056*	0.0065***	0.0075***
	(0.002 0)	(0.002 0)	(0.001 7)	(0.001 7)
DoW Fixed Effects	NO	YES	YES	YES
DoEX Fixed Effects	NO	NO	YES	YES
Student Fixed Effects	NO	NO	NO	YES
Bandwidth	14 Days	14 Days	14 Days	14 Days
Observations	123,365	123,365	123,365	123,365
# of Exposed Students	6,319	6,319	6,319	6,319
R-Squared	0.000	0.001	0.004	0.001

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Students exposed to any homicide between October 15, 2009 and May 31, 2010. Homicides at the beginning, during and at the end of multi-day school breaks (thankgiving, winter, mid-winter and spring breaks) are excluded. Standard errors clustered at the block-face level. 'DoW' and 'DoEX' stand for Day-of-the-Week and Day-of-Exposure, respectively.

Table 3.5: Effect of Exposure to a Homicide on Absenteeism by Day-of-Exposure, Gender, Race/Ethnicity and Grade Level, Preferred Specification, First Homicide, AY 2009-10

	Post	Std. Err.	# of Observations	# of Exposed Students	Diff. (p-value)
A. *By Day-of-Exposure*					
Weekdays	0.0101***	(0.0023)	74,177	3,711	0.0067 (0.077)
Weekends	0.0034	(0.0027)	49,188	2,608	-
B. *By Gender*					
Boys	0.0075***	(0.0026)	63,253	3,294	0.0001 (0.983)
Girls	0.0074**	(0.0030)	58,001	3,025	-
C. *By Race/Ethnicity*					
Asians	0.0036	(0.0057)	11,227	581	-0.0126 (0.312)
Hispanics	0.0104***	(0.0023)	49,511	2,640	-0.0058 (0.534)
Blacks	0.0050*	(0.0027)	56,323	2,877	-0.0112 (0.279)
Whites	0.0162*	(0.0092)	4,193	221	-
D. *By Grade Level*					
K-5th	0.0070**	(0.0030)	51,826	2,706	0.0036 (0.512)
6th-8th	0.0150***	(0.0040)	22,657	1,182	0.0116 (0.039)
9th-12th	0.0034	(0.0040)	33,516	1,744	-
Special Ed.	0.0087	(0.0057)	10,408	540	0.0053 (0.442)

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Notes: Students exposed to any homicide between October 15, 2009 and May 31, 2010. Homicides at the beginning, during and at the end of multi-day school breaks (winter, mid-winter and spring breaks) are excluded. Standard errors clustered at the block-face level. All estimates are obtained from a specification that include day-of-the-week, day-of-exposure and student fixed effects. Reference groups are girls, whites, high school students and weekends.

Table 3.6: Effect of Exposure to a Homicide on Absenteeism, Multiple Exposure, Response to Each Homicide Estimated Separately, AY 2009-10

	Students Exposed to One Homicides Only	Students Exposed to Two Homicides Only	
		1st Homicide	2nd Homicide
	I	II	III
Post	0.0070***	0.0182	0.0465**
	(0.001 7)	(0.011 5)	(0.005 6)
DoW Fixed Effects	YES	YES	YES
DoEX Fixed Effects	YES	YES	YES
Student Fixed Effects	YES	YES	YES
Bandwidth	14 Days	14 Days	14 Days
Observations	117,217	2,110	1,290
# of Students	6,058	108	102
R-squared	0.001	0.008	0.013

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Students exposed to any homicide between October 15, 2009 and May 31, 2010. Homicides at the beginning, during and at the end of multi-day school breaks (thanksgiving, winter, mid-winter and spring breaks) are excluded. Standard errors clustered at the block-face level. 'DoW' and 'DoEX' stand for Day-of-the-Week and Day-of-Exposure, respectively.

Table 3.7: Effect of Exposure to a Homicide on Absenteeism, Multiple Exposure, Response to Each Homicide Estimated in One Regression, AY 2009-10

	# of Homicides	
	At Least One	Two Only
	I	II
1st Event Window, Days -14 to 14	0.0005	−0.0063
	(0.0047)	(0.0068)
1st Event, Days 0 to 14	0.0070***	0.0186
	(0.0018)	(0.0098)
2nd Event Window, Days -14 to 14	−0.0087	−0.0065
	(0.0166)	(0.0192)
2nd Event, Days 0 to 14	0.0313*	0.0736*
	(0.0173)	(0.0327)
Post 2nd Event*Distance 1st and 2nd Event		−0.0004
		(0.0003)
DoW Fixed Effects	YES	YES
DoEX Fixed Effects	YES	YES
Student Fixed Effects	YES	YES
Bandwidth	14 Days	14 Days
Observations	1,025,362	15,544
# of Students	6,319	116
R-squared	0.000	0.002

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Students exposed to any homicide between October 15, 2009 and May 31, 2010. Homicides at the beginning, during and at the end of multi-day school breaks (thanksgiving, winter, mid-winter and spring breaks) are excluded. Standard errors clustered at the block-face level. ‘DoW’ and ‘DoEX’ stand for Day-of-the-Week and Day-of-Exposure, respectively.

Table 3.8: Effect of Exposure to a Violent Crime on Absenteeism, Multiple Exposure, Response to Each Violent Crime Estimated Separately, AYs 2009-10 and 2010-11

	One Event Only	Two Events Only		Three Events Only		
		1st Event	2nd Event	1st Event	2nd Event	3rd Event
	I	II	III	IV	V	VI
Post	0.0040***	0.0033*	0.0101***	−0.0002	0.0033	0.0032
	(0.001 2)	(0.001 8)	(0.001 9)	(0.003 1)	(0.002 9)	(0.003 4)
AY Fixed Effects	YES	YES	YES	YES	YES	YES
DoW Fixed Effects	YES	YES	YES	YES	YES	YES
Semester Fixed Effects	YES	YES	YES	YES	YES	YES
Student Fixed Effects	YES	YES	YES	YES	YES	YES
Bandwidth	14 Days	14 Days	14 Days	14 Days	14 Days	14 Days
Observations	229,085	103,947	94,826	38,831	43,321	35,094
# of Students	11,961	5,459	4,873	2,084	2,246	1,769
R-squared	0.001	0.001	0.001	0.001	0.001	0.001

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: 5% random sample of students exposed to at least one violent crime between October 15, 2009 and May 31, 2010, or October 15, 2010 and May 31, 2011. Violent crimes at the beginning, during and at the end of multi-day school breaks (thankgiving, winter, mid-winter and spring breaks) are excluded. Standard errors clustered at the block-face level. 'AY' and 'DoW' stand for Academic Year and Day-of-the-Week, respectively.

Table 3.9: Effect of Exposure to a Violent Crime on Absenteeism, Multiple Exposure, Response to Each Homicide Estimated in One Regression, AYs 2009-10 and 2010-11

	# of Violent Crimes		
	At Least One	Two Only	Three Only
	I	II	III
1st Event Window, Days -14 to 14	−0.0037***	−0.0050***	−0.0099***
	(0.000 8)	(0.001 2)	(0.002 2)
1st Event, Days 0 to 14	0.0023***	0.0009	−0.0006
	(0.000 9)	(0.001 3)	(0.002 4)
2nd Event Window, Days -14 to 14	0.0011	0.0010	0.0026
	(0.001 2)	(0.001 2)	(0.002 2)
2nd Event, Days 0 to 14	0.0035**	0.0024	0.0013
	(0.001 4)	(0.002 1)	(0.003 5)
3rd Event Window, Days -14 t0 14	0.0060**		0.0065***
	(0.002 3)		(0.002 3)
3rd Event, Days 0 to 14	−0.0006		0.0012
	(0.002 8)		(0.004 4)

Table continues in the next page.

2nd Event, Days 0 to 14	0.0035**	0.0024	0.0013
	(0.001 4)	(0.002 1)	(0.003 5)
3rd Event Window, Days -14 t0 14	0.0060**		0.0065***
	(0.002 3)		(0.002 3)
3rd Event, Days 0 to 14	−0.0006		0.0012
	(0.002 8)		(0.004 4)
Post 2nd Event*Distance 1st to 2nd Event		1.2×10^{-5}	-2.6×10^{-5}
		(1.9×10^{-5})	(3.9×10^{-5})
Post 3rd Event*Distance 2nd to 3rd Event			-2.6×10^{-5}
			(4.3×10^{-5})
AY Fixed Effects	YES	YES	YES
DoW Fixed Effects	YES	YES	YES
Semester Fixed Effects	YES	YES	YES
Students Fixed Effects	YES	YES	YES
Observations	4,252,370	2,122,758	683,308
# of Students	23,673	11,794	3,805
R-squared	0.002	0.002	0.001

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: 5% random sample of students exposed to at least one violent crime between October 15, 2009 and May 31, 2010, or October 15, 2010 and May 31, 2011. Violent crimes at the beginning, during and at the end of multi-day school breaks (thankgiving, winter, mid-winter and spring breaks) are excluded. Standard errors clustered at the block-face level. 'AY' and 'DoW' stand for Academic Year and Day-of-the-Week, respectively.

Table 3.10: Effect of Exposure to a Homicide on Absenteeism, First Homicide, By Semester of Exposure and Accumulated Absences before Exposure, AY 2009-10

	Fall I	Spring II	Fall III	Spring IV
Post	0.0123***	0.0034	−0.0378***	−0.0039
	(0.003 4)	(0.002 7)	(0.007 4)	(0.006 5)
$Post$*$\mathbf{1}$(0% $\leq$ % $Days\ Absent$< 2%)			0.0716***	0.0175**
			(0.008 2)	(0.007 0)
$Post$*$\mathbf{1}$(2% $\leq$ % $Days\ Absent$< 4%)			0.0611***	0.0150**
			(0.009 3)	(0.006 5)
$Post$*$\mathbf{1}$(4% $\leq$ % $Days\ Absent$< 6%)			0.0661***	0.0055
			(0.013 2)	(0.007 1)
$Post$*$\mathbf{1}$(6% $\leq$ % $Days\ Absent$< 8%)			0.0593***	0.0108
			(0.012 6)	(0.008 8)
$Post$*$\mathbf{1}$(8% $\leq$ % $Days\ Absent$< 10%)			0.0546***	−0.0111
			(0.016 8)	(0.010 9)

Table continues in the next page.

DoW Fixed Effects	YES	YES	YES	YES
DoEX Fixed Effects	YES	YES	YES	YES
Student Fixed Effects	YES	YES	YES	YES
Month Fixed Effects	YES	YES	YES	YES
Bandwidth	14 Days	14 Days	14 Days	14 Days
Observations	44,463	78,902	44,463	78,902
# of Students	2,352	3,967	2,352	3,967
R-squared	0.000	0.000	0.000	0.000

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Students exposed to any homicide between October 15, 2009 and May 31, 2010. Homicides at the beginning, during and at the end of multi-day school breaks (thankgiving, winter, mid-winter and spring breaks) are excluded. Standard errors clustered at the block-face level. 'DoW' and 'DoEX' stand for Day-of-the-Week and Day-of-Exposure, respectively.

Table 3.11: Effect of Exposure to a Violent Crime on Absenteeism, First Violent Crime, By Semester of Exposure and Accumulated Absences before Exposure, AYs 2009-10 and 2010-11

	Fall I	Spring II	Fall III	Spring IV
Post	0.0061***	0.0005	−0.0283***	−0.0261***
	(0.0012)	(0.0015)	(0.0047)	(0.0047)
$Post$*$\mathbf{1}(0\% \leq \%\ Days\ Absent < 2\%)$			0.0551***	0.0269***
			(0.0047)	(0.0048)
$Post$*$\mathbf{1}(2\% \leq \%\ Days\ Absent < 4\%)$			0.0440***	0.0266***
			(0.0052)	(0.0053)
$Post$*$\mathbf{1}(4\% \leq \%\ Days\ Absent < 6\%)$			0.0359***	0.0265***
			(0.0063)	(0.0059)
$Post$*$\mathbf{1}(6\% \leq \%\ Days\ Absent < 8\%)$			0.0305***	0.0214***
			(0.0070)	(0.0068)
$Post$*$\mathbf{1}(8\% \leq \%\ Days\ Absent < 10\%)$			0.0210**	0.0065
			(0.0088)	(0.0082)

Table continues in the next page.

AY Fixed Effects	YES	YES	YES	YES
DoW Fixed Effects	YES	YES	YES	YES
Student Fixed Effects	YES	YES	YES	YES
Month Fixed Effects	YES	YES	YES	YES
Bandwidth	14 Days	14 Days	14 Days	14 Days
Observations	197,618	174,245	197,618	174,245
# of Students	10,795	8,701	10,795	8,701
R-squared	0.000	0.000	0.000	0.000

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: 5% random sample of students exposed to at least one violent crime between October 15, 2009 and May 31, 2010, or October 15, 2010 and May 31, 2011. Violent crimes at the beginning, during and at the end of multi-day school breaks (thankgiving, winter, mid-winter and spring breaks) are excluded. Standard errors clustered at the block-face level. 'AY' and 'DoW' stand for Academic Year and Day-of-the-Week, respectively.

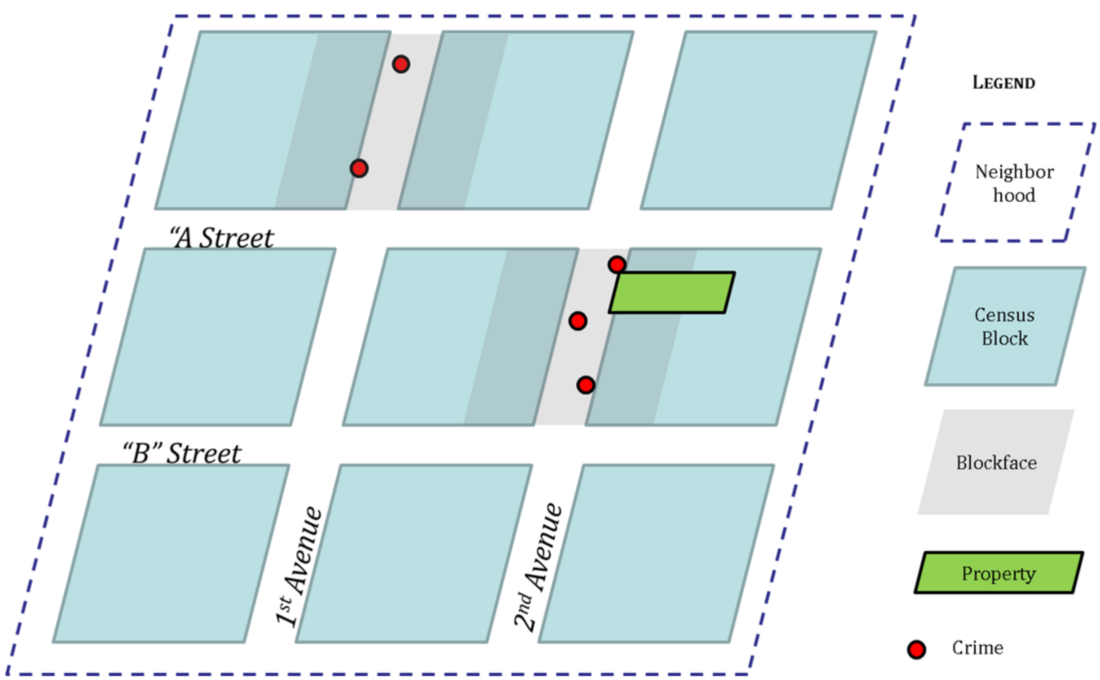
Table 3.12: Robustness Checks

Description	Statistic	Std. Err.	[95% Conf.	Interval]
Placebo Test	−0.0009	(0.0018)	-0.0044	0.0026
Month Fixed Effects	0.0062***	(0.0022)	0.0019	0.0105
Calendar-Day Fixed Effects	0.0013*	(0.0007)	-0.0001	0.0025
I&K Bandwidth Selector	0.0044***	(0.0013)	0.0019	0.0069

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

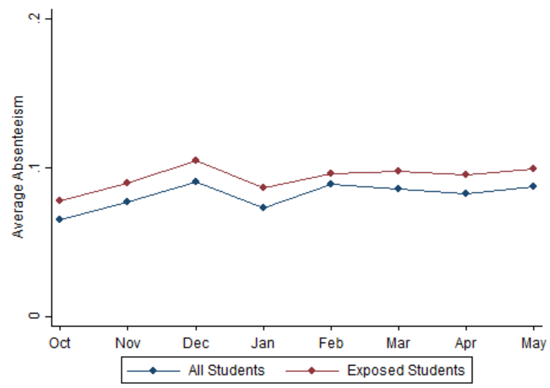
This table reports on α_1, the average treatment effect of exposure to a violent crime on absenteeism. Placebo Test was estimated using a fixed 14 days bandwidth, DoW fixed and student fixed effects. Rows 2 substitutes the semester fixed effects in equation 3.1 with month fixed effects, and row 3 substitutes both DoW and semester fixed effects in equation 3.1 with calendar-day fixed effects. The estimate using the IK bandwidth selector does not add fixed effects. Rows 2 to 4 estimate response to first event of exposure.

Figure 3.1: Definition of Exposure, Graphical Representation

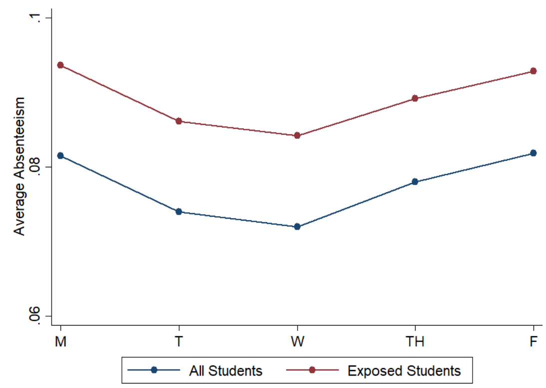


Notes: Students linging in the shaded parts of adjacent census blocks would be coded as residing on the same block-face, and exposed to the same incidents of crime (Schwartz et al. (2016)).

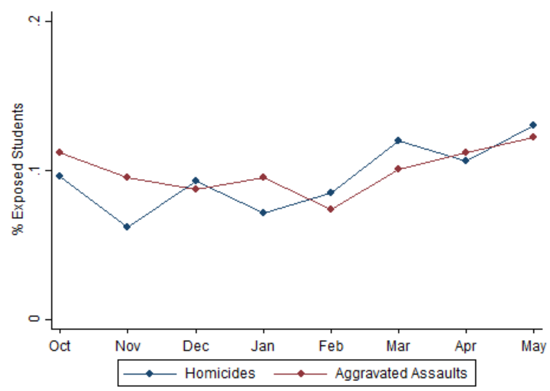
Figure 3.2: Temporal and Spatial Distribution of Absenteeism, Exposed Students and Violent Crimes, Academic Year 2009-2010.



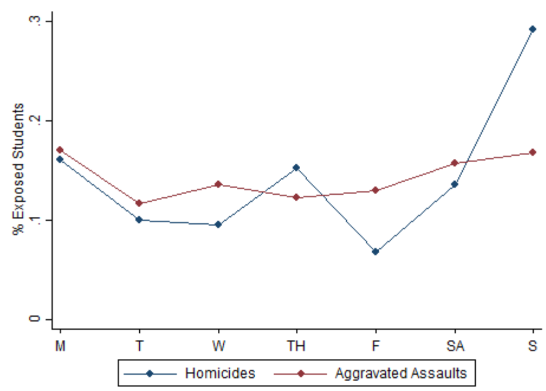
(a) By Month



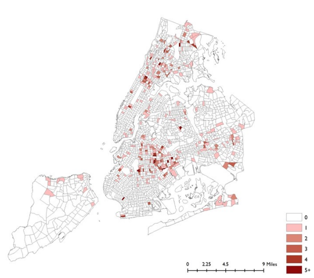
(b) By Day-of-the-Week



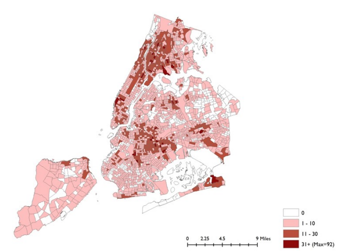
(c) By Month



(d) By Day-of-the-Week



(e) Homicides



(f) Violent Crime

Notes: In figures 3.2a and 3.2b ‘exposed students’ correspond to exposure to at least one homicide. In figure 3.2c and 3.2d counts of exposure are based on events, not students. Therefore, students exposed multiple times are counted every time they are exposed. Figures 3.2e and 3.2f show the count of homicides and violent crimes from the first to the last day of school. Violent crimes include homicides and aggravated assaults.

Appendices

A.1 Firms Response on the Age Margin

An explanation for why formal employment of workers 28 years old or younger does not increase, is that labor supply for this segment of the labor market is perfectly inelastic. If this is the case, the shift in labor demand caused by the wage subsidy increases the equilibrium wage, but leaves employment unchanged. I use a difference-in-difference strategy to test whether monthly earnings of workers younger than 28 years old increase. I use the following regression specification:

$$y_i = \alpha_0 + \alpha_1 T_t + \alpha_2 G_i + \alpha_3 (T_t * G_i) + X_i \delta + \mu_i \tag{A.1}$$

where $y_{i,j}$ is the outcome variable, T_t a time indicator, G_i a group indicator, X_i a matrix of observable characteristics, and $\mu_{i,j}$ the error term. In addition to monthly earnings (expressed in terms of number of minimum wages), I test two additional outcome variables: a binary variable that takes 1 if workers i has a formal job, 0 otherwise, and weekly hours of work. T_t takes 1 for years 2012 to 2015, 0 for years 2008 to 2010. G_i takes 1 if worker i is between 24 and 28 years old, 0 if is between 32 and 40. The matrix of observable characteristics include a set of education level indicators, experience, experience squared, marital status indicators, industry indicators, and a set of job history variables.

The identifying assumption for $\hat{\alpha}_3$ to give the causal effect of the wage subsidy on the outcome of interest, is that in its absence treatment and control groups would had evolved similarly. Tables A.1 contains the results. Columns I and II show results for formal employment, III and IV for monthly labor earnings, and V and VI for weekly hours of work. For all outcomes the difference-in-difference estimate of the average effect of the wage subsidy is not statistically different from zero. Table A.2 presents the results when the treatment group are workers between 29 and 31 years of age, the age range more likely affected if the wage subsidy cause displacement of workers. None of the outcomes show a statistically significant change for this alternative treatment group.

The absence of changes in both formal employment and monthly earnings suggest firms did not respond on the age margin. These findings reinforce the hypothesis the wage subsidy is not attractive for this segment of the labor force, since other incentives, such as the ‘apprenticeship’ contract, compete in generosity with it.

Table A.1: Difference-in-Difference Estimates, Average Treatment Effect on Employment, Labor Earnings, and Hours of Work, Age Group Targeted by First Job Act

VARIABLES	I	II	III	IV	V	VI
$\hat{\alpha}_3$	−0.006	−0.003	−0.002	−0.008	−0.171	−0.219
	(0.004)	(0.004)	(0.021)	(0.021)	(0.135)	(0.133)
Time Indicator	0.047***	0.007	0.016	−0.044***	−1.024***	−0.802***
	(0.005)	(0.006)	(0.013)	(0.010)	(0.253)	(0.267)
Group Indicator	0.058***	−0.078***	−0.320***	−0.530***	−0.489***	1.331***
	(0.013)	(0.006)	(0.060)	(0.084)	(0.172)	(0.258)
Controls	NO	YES	NO	YES	NO	YES
Observations	679,769	633,875	703,060	652,147	767,052	713,044
R-squared	0.005	0.224	0.004	0.177	0.001	0.086

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.2: Difference-in-Difference Estimates, Average Treatment Effect on Employment, Labor Earnings, and Hours of Work, Alternative Treatment Group

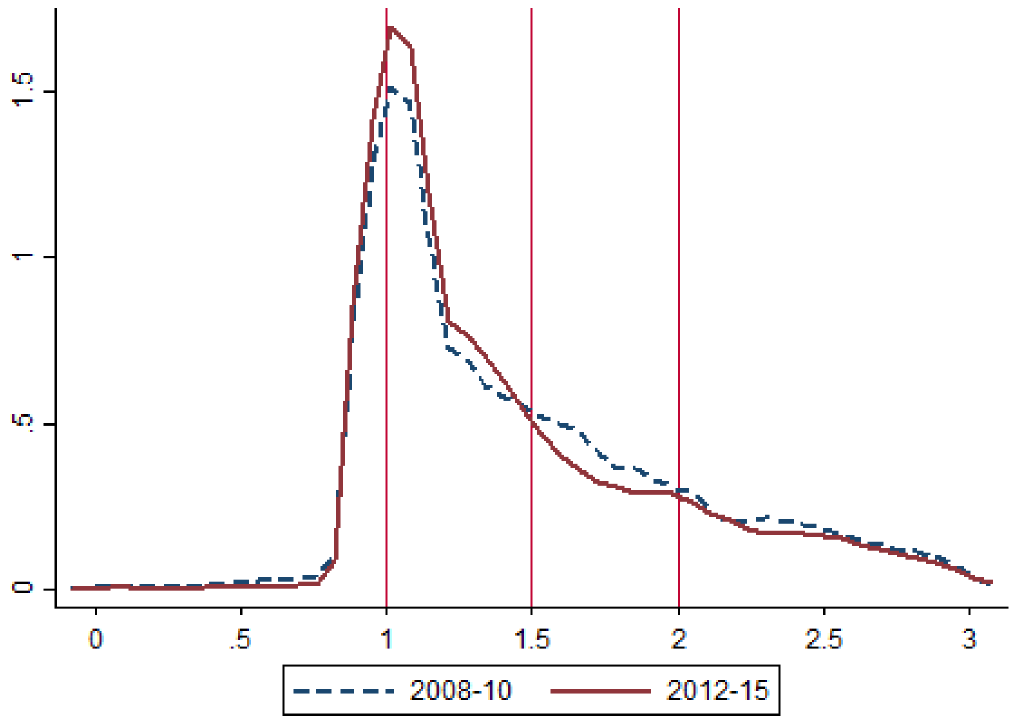
VARIABLES	I	II	III	IV	V	VI
$\hat{\alpha}_3$	−0.005	0.002	0.003	0.016	−0.009	−0.094
	(0.004)	(0.003)	(0.022)	(0.024)	(0.137)	(0.121)
Time Indicator	0.047***	0.007	0.016	−0.043***	−1.024***	−0.809***
	(0.005)	(0.006)	(0.013)	(0.011)	(0.253)	(0.263)
Group Indicator	0.056***	−0.047***	−0.115***	−0.459	−0.060	0.453**
	(0.009)	(0.006)	(0.037)	(0.093)	(0.137)	(0.169)
Controls	NO	YES	NO	YES	NO	YES
Observations	584,443	545,743	599,094	556,424	654,259	609,039
R-squared	0.004	0.232	0.000	0.170	0.000	0.095

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

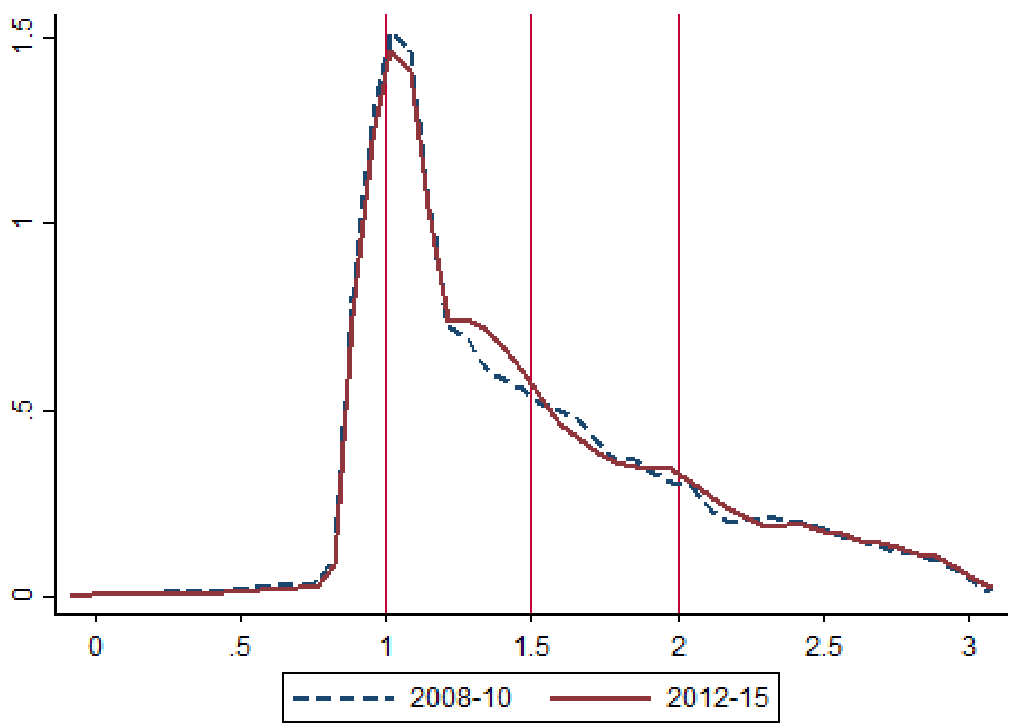
A.2 Formal Labor Earnings Distribution by Industry and Region

Empirical densities are estimated as in section 1.3, using an Epanechnikov kernel function, and a 'rule-of-thumb' approach for choosing the optimal bandwidth. All empirical distributions are based on counts of non-public sector, full time workers.

Figure A.1: Formal Labor Earnings PDF, by Industry

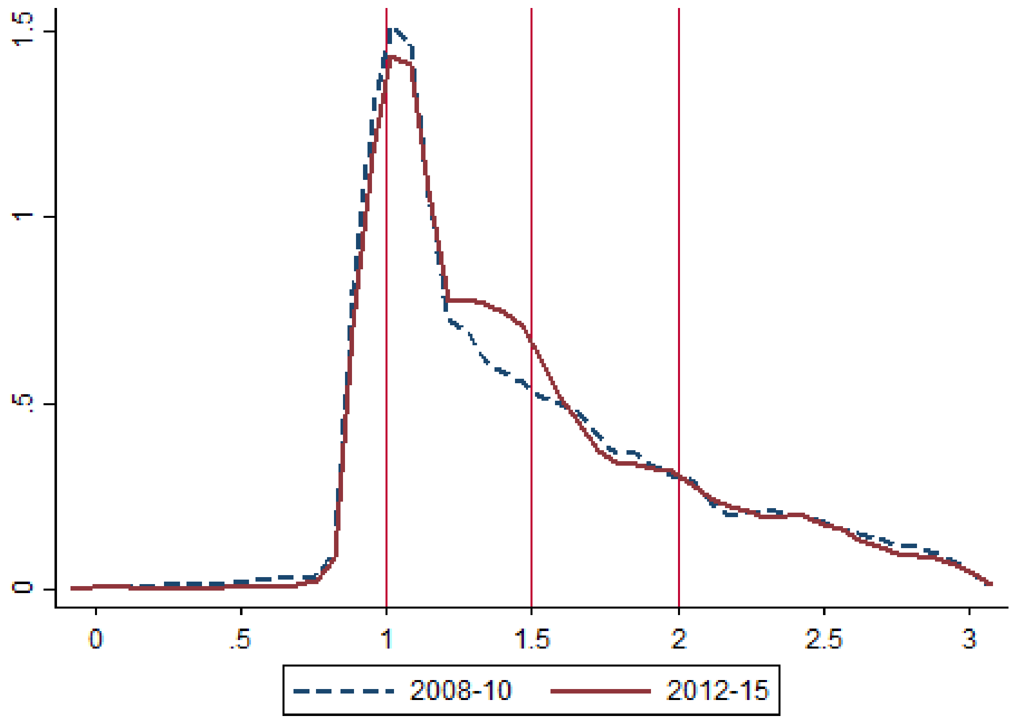


(a) Manufacturing

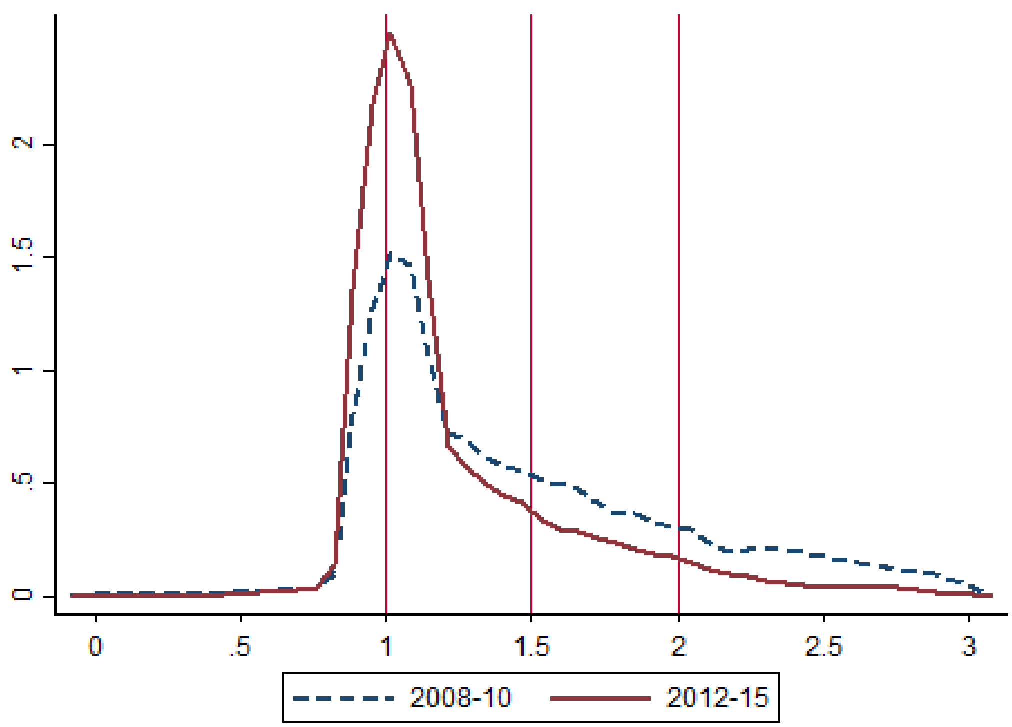


(b) Service

Figure A.1 (cont.): Formal Labor Earnings PDF, by Industry

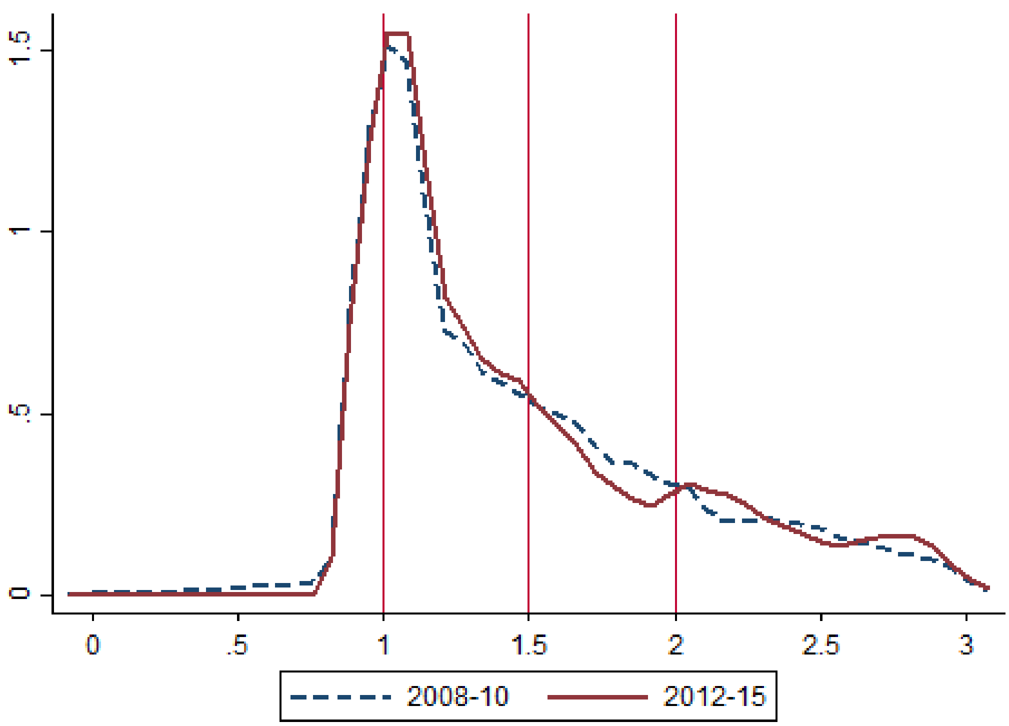


(c) Commerce



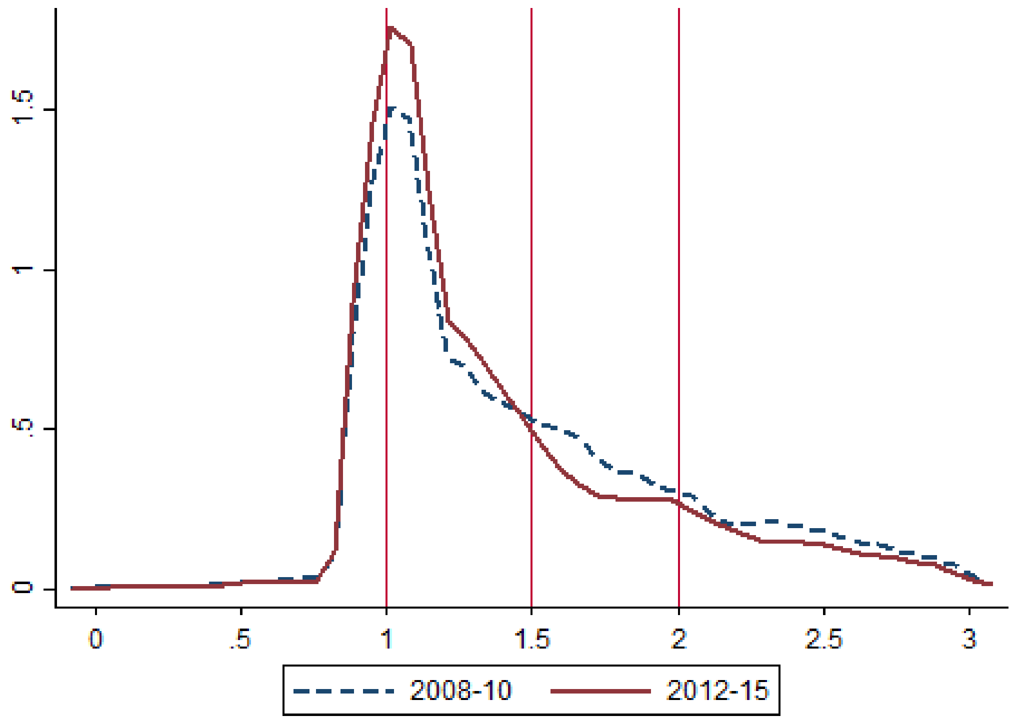
(d) Agriculture and Mining

Figure A.1 (cont.): Formal Labor Earnings PDF, by Industry

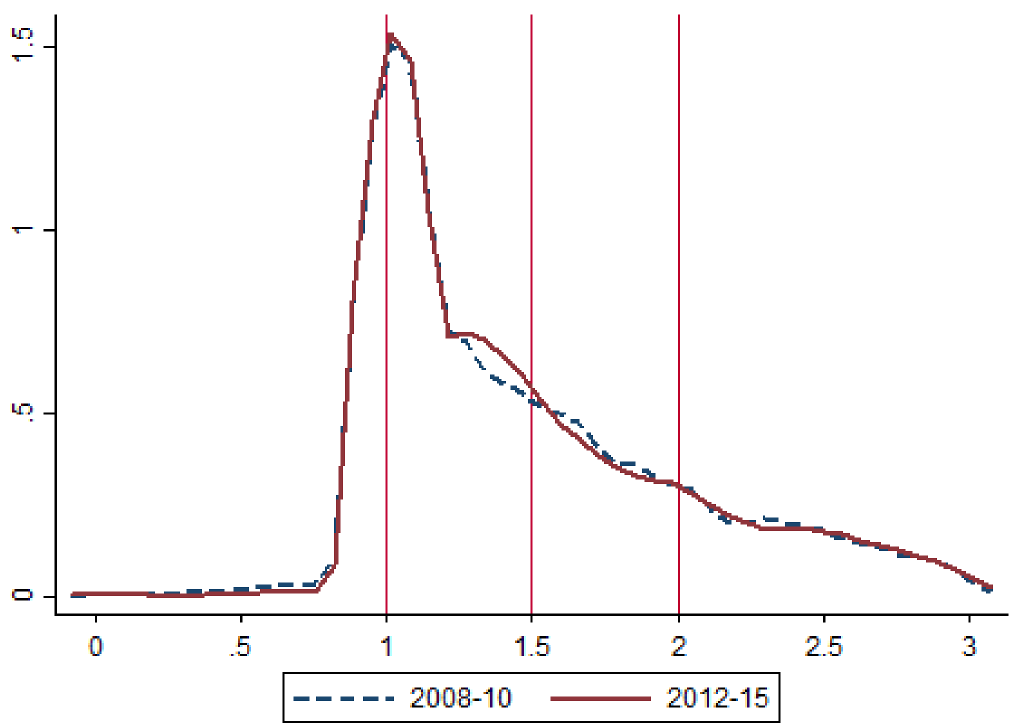


(e) Construction

Figure A.2: Formal Labor Earnings PDF, by Region

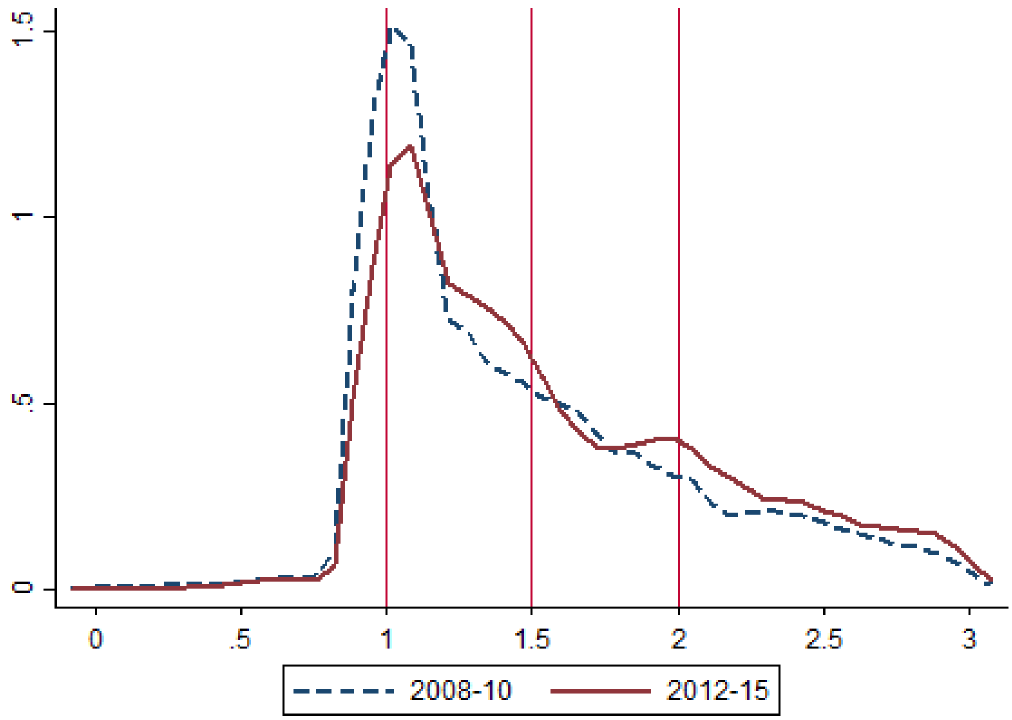


(a) North

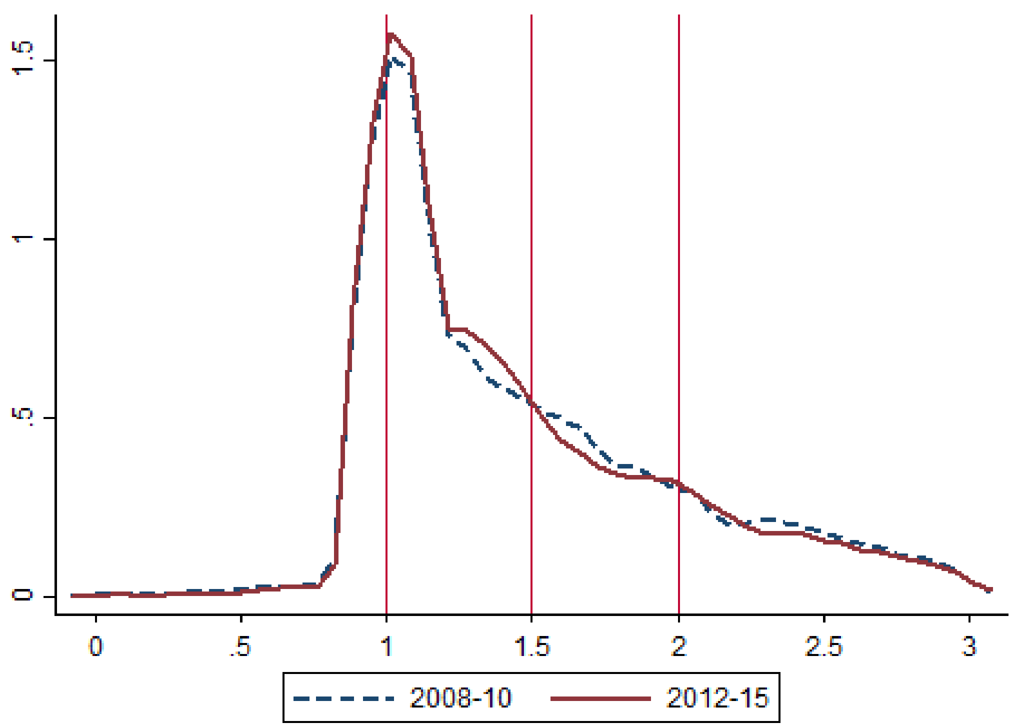


(b) Centre

Figure A.2 (cont.): Formal Labor Earnings PDF, by Region



(c) East



(d) West

A.3 Robustness Checks

Table A.3 contains the results of estimating regression equation (1.12) using as control group individuals earning between two and four minimum wages. Table A.4 shows the average marginal effect resulting from estimating regression equation (1.12) with a probit model.

Table A.3: Change in the Size of the Formal Sector, Difference-in-Difference Estimator, Alternative Control Group, with Controls

VARIABLES	$(0.0 - MW)$	$[MW - 1.5 * MW)$	$[1.5 * MW - 1.9 * MW]$
$\hat{\alpha}_3$	−0.035***	0.018**	−0.027***
	(0.005)	(0.009)	(0.009)
Time Indicator	0.020***	0.014***	0.012**
	(0.006)	(0.005)	(0.005)
Group Indicator	−0.479***	−0.020*	0.008
	(0.015)	(0.010)	(0.006)
Male	−0.016***	−0.076***	−0.020**
	(0.004)	(0.013)	(0.007)
Married	−0.012***	−0.013*	−0.029***
	(0.004)	(0.007)	(0.003)
HS Dropout	−0.341***	−0.276***	−0.376***
	(0.014)	(0.017)	(0.018)
HS Degree	−0.252***	−0.112***	−0.165***
	(0.013)	(0.014)	(0.013)
Associate Degree	−0.084***	−0.023*	−0.009
	(0.011)	(0.012)	(0.006)
Experience	−0.001	−0.003***	−0.006***
	(0.001)	(0.001)	(0.001)
Experience^2	0.000**	0.000	0.000**
	(0.000)	(0.000)	(0.000)
Second Job	0.020***	0.063***	0.048***
	(0.005)	(0.004)	(0.005)
Not Worked Before	0.054***	0.161***	0.123***
	(0.007)	(0.011)	(0.010)
Tenure	0.000***	0.000**	0.000**
	(0.000)	(0.000)	(0.000)
Service Sector	−0.019***	−0.058***	−0.066***
	(0.005)	(0.013)	(0.017)
Constant	0.759***	0.549***	0.693***
	(0.013)	(0.017)	(0.014)
Observations	486,211	604,092	219,479
R-squared	0.500	0.111	0.169

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Average Marginal Effect of the Wage Subsidy on the Size of the Formal Sector, Difference-in-Difference Estimator, with Controls

LABOR EARNINGS RANGE	$\frac{dy}{dx}$
$(0.0 - MW)$	-0.016***
	(0.004)
$[MW - 1.5 * MW)$	0.017**
	(0.008)
$[1.5 * MW - 1.9 * MW)$	-0.018***
	(0.004)

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A.4 Other Gaming Strategies and Robustness Checks

Firms created before January, 2011 might have had an incentive to set up new firms once the tax breaks program was in place, and move part of their payroll and new investments there. We can partially test if firms engage in these gaming strategies by looking at whether the average number of employees and new capital investments drop after introduction of the program. To test it we used monthly employment data from social security records, and yearly new capital investment data from the Annual Census of Manufacturing Firms.

Figure A.3 shows there are no discontinuous changes in average number of workers or new capital investments right after the introduction of the tax breaks. Furthermore, it shows there are no trend changes during the period the tax breaks were in place. Regression estimates confirm the graphical evidence. Tables A.5 and A.6 show the results of estimating equation 2.9 using, for firms created before 2011, the number of workers and new capital investment amount as outcome variables. This confirms that, for firms that existed before January, 2011, there is no change in the average number of employees on payroll nor, in the case of manufacturing firms, any change in capital investments.

Table A.5: Employment, Firms Created Before 2011

PARAMETER	I	II	III
β_0	−0.1804	−0.1791	−0.0274
	(0.146 4)	(0.207 1)	(0.259 4)
# of Observations			
Calendar Month FE	YES	YES	YES
Year of Birth FE	YES	YES	YES
Firm Size FE	YES	YES	YES
Linear Trend	YES	YES	YES
Quadratic Trend	NO	YES	YES
Cubic Trend	NO	NO	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Estimates obtained from monthly data on number of workers on payroll, from social security records for the January, 2010 to December, 2016 period.

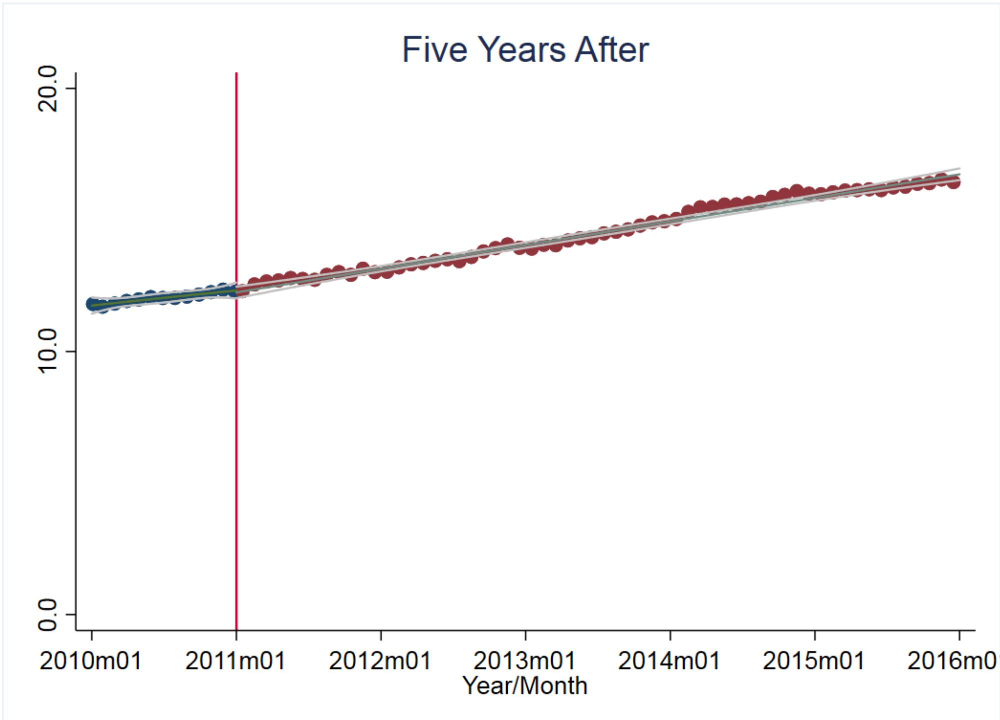
Table A.6: New Capital Investments, Manufacturing Firms Created Before 2011

β_0	Linear	Quadratic
Total	−82.59	−424.2
	(171.2)	(632.6)
Land	18.30	55.13
	(18.57)	(55.80)
Property	−51.15	56.23
	(54.56)	(93.57)
Machinery	−109.50	−455.9
	(162.4)	(552.2)
Technology	33.19	−24.28
	(27.36)	(28.42)
Offices	26.45	−29.77
	(18.28)	(19.72)
Transport	0.07	−25.52
	(19.54)	(24.51)
# of Observations	101,200	101,200
Firm FE	YES	YES
Year FE	YES	YES

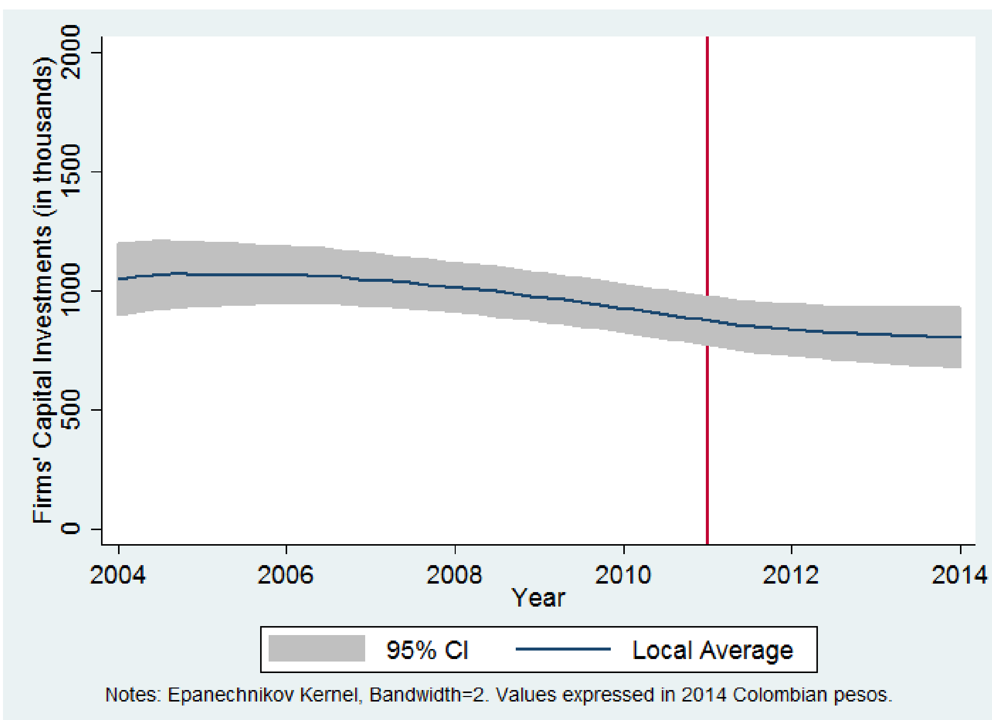
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Estimates obtained from yearly data on new capital investments of manufacturing firms for the 2004 to 2014 period. The source of the data is the Colombia's Annual Census of Manufacturing Firms.

Figure A.3: Average Employment and Average New Capital Investments, Firms Created Before 2011



(a) Employment



(b) New Capital Investments

Notes: Figure A.3a uses monthly data from social security records from January, 2010 to December 2016. Figure A.3b uses yearly data on new capital investments of manufacturing firms from 2004 to 2014.

Table A.7: Probability of Survival, Hazard Model Coefficients, Adding Size-at-Birth Fixed Effects

PARAMETER	I	II	II	IV
A. *+/-3 Months Around January, 2011*				
β_0	−0.0334	−0.0279	−0.0497	−0.0128
	(0.035 5)	(0.088 2)	(0.073 6)	(0.015 4)
# of Observations	34,520	34,520	34,520	34,520
B. *+/-6 Months Around January, 2011*				
β_0	−0.0653***	−0.0581	0.0086	0.0417***
	(0.023 1)	(0.040 0)	(0.073 6)	(0.010 8)
# of Observations	70,795	70,795	70,795	70,795
Size-at-Birth FE	YES	YES	YES	YES
Linear Trend	YES	YES	YES	NO
Quadratic Trend	NO	YES	YES	NO
Cubic Trend	NO	NO	YES	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A.8: Probability of Survival, Hazard Ratios, Adding Size-at-Birth Fixed Effects

PARAMETER	I	II	II	IV
A. *+/-3 Months Around January, 2011*				
β_0	0.9671	0.9724	0.9514	0.9872
	(0.034 3)	(0.085 8)	(0.070 0)	(0.015 2)
# of Observations	34,520	34,520	34,520	34,520
B. *+/-6 Months Around January, 2011*				
β_0	0.9367***	0.9435	1.0087	1.0426***
	(0.021 7)	(0.037 8)	(0.074 3)	(0.011 2)
# of Observations	70,795	70,795	70,795	70,795
Size-at-Birth FE	YES	YES	YES	YES
Linear Trend	YES	YES	YES	NO
Quadratic Trend	NO	YES	YES	NO
Cubic Trend	NO	NO	YES	NO

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A.5 Testing Differences in Observable Characteristics Among Manufacturing Firms

Differences in observable characteristics among manufacturing firms were tested using the following regression specification:

$$y_{i,t} = \beta_0 + \beta_1 treatment_i + e_{i,t}$$

where $treatment_i$ takes 1 if the firm was created between 2011 and 2015, 0 if created between 2005 and 2010. As outcome variables we use: regional location, energy consumption, total and net investment, total non-financial assets, value added, number of employees and average salary. The data comes from the Colombia's Annual Census of Manufacturing Firms, which is a census of manufacturing firms with 10 or more employees. Table A.9 reports the $\hat{\beta}_1$'s and its corresponding standard errors.

Table A.9: Differences in Observable Characteristics Among Manufacturing Firms, 2005-2015

	Coef.	Std. Err.	[95% Conf. Interval]		n
A. *Metropolitan Areas*					
Barranquilla-Cartagena	0.0226	(0.027 2)	−0.0309	0.0762	461
Bogota	−0.0208	(0.049 2)	−0.1177	0.0759	461
Cali	−0.0152	(0.031 3)	−0.0767	0.0463	461
Medellin	0.0133	(0.041 4)	−0.0681	0.0948	461
Eje Cafetero	−0.0240	(0.023 6)	−0.0705	0.0225	461
Rest of the Country	0.0241	(0.045 1)	−0.0646	0.1128	461
B. *Firm Characteristics*					
Energy Consumption	94.3070	(557.708 7)	−1001.6720	1190.2860	461
Total Investment	5977.5930	(4 490.872 0)	−2847.6250	14 802.8100	461
Net Investment	5541.2690	(4 383.964 0)	−3073.8590	14 156.4000	461
Total Non-Financial Assets	9612.9460	(5 907.131 0)	−1995.4940	21 221.3900	461
Value Added	7347.2040*	(3 875.597 0)	−268.9093	14 963.3200	461
Number of Employees	8.0544	(9.138 2)	−9.9035	26.0123	461
Average Salary	1.1266*	(0.665 8)	−0.1818	2.4352	461

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Data comes from Colombia's Annual Census of Manufacturing Firms, which is a census of manufacturing firms with 10 or more employees.

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  - During my first summer as part of Deloitte's New York City TP team, I will be working on Transfer Pricing documentation and planning projects.
- **Inter-American Development Bank** — Washington, DC
  Fiscal Analyst — *Apr. 2010 - Jul. 2011*
  - Produced five policy documents on fiscal decentralization in small, non-federal Latin American and Caribbean countries.
  - Organized a one-day seminar where Bank's specialists and government representatives debated about fiscal decentralization in small, non-federal Latin American and Caribbean countries.
  - Produced two studies on subnational tax revenue potential, one on the Mexico's state payroll tax, the other on the Guatemala's municipal property tax.
  - All knowledge products (policy documents, seminar conclusions, and studies) served as analytical inputs in the dialogue the bank permanently has with governments, exploring areas where they can work together.
  - As a result of the work I did on Guatemala's subnational public finance, I participated of a team that advised the Guatemalan government on subnational fiscal rules.
- **Fundesarrollo** — Barranquilla, Colombia
  Consultant — *Oct. 2009 - Mar. 2010*
  - Produced a study about the state and trends of Barranquilla's quality-of-life. The assessment used as benchmark a holistic, internationally accepted, and objectively measurable set of socioeconomic and environmental indicators.
  - Consolidated, processed and analyzed the database needed to meet the studys objectives.
  - Wrote and defended the findings' report.
- **Cartagena Como Vamos Project** — Cartagena, Colombia
  Technical Director — *Feb. 2005 - Mar. 2010*
  - Produce for five consecutive years an annual city's quality-of-life report. It required collecting and analyzing data from a variety of sources, including field work, and pursuing different validation methods to ensure the content's accuracy.
  - Produced several evidence-based, highly technical studies on poverty, inequality, labor markets, school outcomes and public finance.
  - Managed the relations with research institutions, and a network of over 70 specialists in different fields.
  - Coordinated a biannual field work designed to monitor the efficiency of the city's public transportation system.

- Collected the data to develop, and maintain updated, the georeferenced, interactive web tool '*Comunas Como Vamos*'.
- Through its reports and studies, *Cartagena Como Vamos* became a reference in the city's public policy debates, and oriented on several occasions policy decisions.

Academic Experience

- **Center for Policy Research** Syracuse, NY
  Graduate Associate *May 2015 - May 2018*
  - Wrote my doctoral dissertation, in which I studied how tax policy affects the labor market, as well as small firms' birth rate, survival probability and personnel decisions.
  - Coauthored a research project that investigated how the neighborhood environment affects school outcomes in New York City.
- **Syracuse University** Syracuse, NY
  Teaching Assistant *Aug. 2013 - May 2015*
  - Economic Ideas and Issues (Fall, 2013), The World Economy (Spring 2014, Fall 2014), Game Theory and Economic Strategy (Spring 2015).
- **Tecnologica de Bolivar University** Cartagena, Colombia
  Lecturer *Jan. 2007 - Dec. 2008*
  - Microeconomics I (Spring 2007, Fall 2007, Fall 2008), Econometrics (Fall 2008).

Languages

English (fluent), Spanish (native).

Last updated: September 8, 2018