THREE ESSAYS ON PUBLIC POLICY IN LATIN AMERICA: LABOR MARKETS, EDUCATION, AND MEASUREMENT

BY

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DISSERTATION

Submitted in partial fulfillment of the requirements
for the degree of Doctor of Philosophy in Agricultural and Applied Economics
in the Graduate College of the
University of Illinois at Urbana-Champaign, 2017

Urbana, Illinois

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ABSTRACT

This dissertation contains three chapters that study how public policies in Latin America affect labor market outcomes, educational choices, and ways to improve policy evaluation methods.

The overarching theme of this thesis is to better understand what works to improve the living standards of individuals and households in developing countries. First, I study how government regulation affects labor markets, since the majority of people in developing countries obtain most of their income from work-related activities. Second, I analyze whether low-income students' higher education choices respond to receiving statistics on the average benefits and costs of college, information which is drawn from an online system paid for by the government. Last, I explore how widely used econometric methods fare when researchers ignore geographic correlations, which are often found in socioeconomic data. Determining the reliability of statistical tools is critical for policy evaluation, especially when the resulting estimates are intended as input for program design. Together, these three chapters provide rigorous empirical evidence on the strengths and weaknesses of public policies that can contribute to the way we think about economic development. The individual abstracts for each chapter are presented below.

Chapter 1: The Consequences of Legal Minimum Wages in Honduras

Minimum wage policies are implemented in most developing countries, so understanding their consequences is critical to determine their effectiveness. This chapter studies the labor market and poverty effects of Honduran minimum wages from 2005-2012. In this period, there were annual reforms to multiple minimum wages, a 60% increase, and changes in the number of minimum wage categories. Using 13 household surveys as repeated cross-sections, I estimate the net effects of minimum wage hikes using large variation within categories over time. Evidence shows

that employers partially comply with minimum wage laws, and respond to hikes by increasing their level of non-compliance. Higher minimum wages reduce covered (formal) employment and increase uncovered (informal) employment. Formal sector wages increase but rising labor supply in the informal sector leads to a negative net effect on wages. The latter is often expected but rarely found in the literature. I find no evidence that raising minimum wages reduces poverty.

Chapter 2: Information Policies and Higher Education Choices: Experimental Evidence from Colombia

Governments have been devoting resources to implement online Labor Observatories that provide educational and labor market statistics to help students make higher education choices. This chapter studies the extent to which this information influences low-income students' knowledge and beliefs about college, test scores, and enrollment decisions by means of a randomized controlled trial. We survey over 6,000 students in 115 public schools in Bogotá, Colombia. Students in 58 schools listened to a 35-minute presentation that provides Labor Observatory statistics: average earning premiums upon completing college, available funding options to cover costs, and the importance of test scores for admittance and financing. Average effects of the information treatment are modest and there is no evidence that some students benefit more than others. We conclude that informational campaigns based on data from online Labor Observatories are ineffective to motivate college enrollment for low-income students.

Chapter 3: How Important is Spatial Correlation in Randomized Controlled Trials?

Randomized controlled trials (RCTs) provide researchers with unbiased estimates of a program's causal effects. This chapter focuses on RCTs that assign treatment status over clusters in geographical proximity but evaluate individual-level outcomes. We study how ignoring spatial

correlation in outcomes and unobservables at the cluster-level affects individual-level difference-in-difference estimates (DD). Monte Carlo simulations reveal that spatially-correlated outcomes result in upward bias and low power while spatially-correlated unobservables only reduce power. Small RCTs are more sensitive to spatial correlation than large interventions. Spatial DD overcomes these issues, even in RCTs with few clusters. An application of our framework to Progresa, a large RCT, shows that existing estimates are robust to spatial correlation. We conclude that incorporating spatial methods in RCT evaluation provides several benefits at relatively low cost.

To all the people who have ever believed in me

ACKNOWLEDGEMENTS

Many people have directly and indirectly contributed to my career. I am lucky to have been provided with amazing opportunities from those who always believed in my capacities, even when I did not. It remains my goal to make these people proud. My apologies to anyone I may omit.

I am grateful to my advisors Kathy Baylis and Mary Arends for always guiding by example. Both have made me a better researcher and colleague in more ways than I can list here. I was honored to work alongside them. Richard Akresh and Ben Crost have contributed immensely to shape how I think and solve problems thanks to incisive comments and feedback on my work, as well as their contagious quest for excellence. All my committee members devoted great time and energy to guide me to this point, for which they deserve at the very least, a standing ovation.

Special thanks are due to Martin Perry, for believing in second chances. The same can be said of Geoffrey Hewings, Stephen Parente, Alex Winter-Nelson, Ricardo Bebczuk, and Walter Sosa-Escudero who were in my corner even in the toughest of times. I was lucky to have two homes at the University of Illinois: the Department of Agricultural and Consumer Economics and the Department of Economics. Faculty, staff, and students in both departments have been there when I needed them, providing invaluable support for which I will always be thankful.

Last but not least, my family and friends have been priceless allies. They provided me with strength when I needed it the most. My mother and uncle deserve endless thanks for being my strongest supporters. María Fernanda Latorre deserves a medal for her patience, support, and understanding. Besides a degree, I leave the University of Illinois with friends for life: Leonardo Bonilla, Nicolas Bottan, Eliana Duarte, Esteban López, Ignacio Sarmiento, María Edisa Soppelsa, and Luján Stasevicius. Thanks to the Golden Harbor folks for acting like family and keeping me well-fed. All these people have accomplished a great feat: made me into a better person.

Contents

Chapter 1

The Consequences of Legal Minimum Wages in Honduras[1]

1.1 Introduction

There is an extensive literature in developed countries, particularly the United States, that studies the consequences of minimum wage hikes. Research for the US has found higher minimum wages can lead to job losses, no effect on jobs, or even job growth.[2] In developing countries, minimum wages tend to be set higher (Maloney and Mendez, 2004), are less likely to be rigorously enforced (Kanbur and Ronconi, 2016), and labor markets are often segmented into formal and informal sectors with minimum wage policy only covering formal workers (Fields, 1990). Given these differences and that minimum wage policies are widespread in developing countries, understanding how minimum wages affect labor markets and welfare is critical for economic growth, developing

[1] I would like to acknowledge support from the Tinker Foundation and the Center for Latin American and Caribbean Studies (CLACS) at the University of Illinois at Urbana-Champaign. This paper was conceived in CEDLAS' IDRC project "Labour markets for inclusive growth in Latin America" (http://www.labor-AL.org). I am thankful to the National Statistics Institute (INE) for access to the household survey data, especially to María Auxiliadora López and René Soler. Additional data and insight were provided by Jaime Escobar at the Ministry of Labor and Social Security, Marcela Herrera at the employers' organization (COHEP), and José García at the central workers' union (CGT). Earlier versions have benefited from discussions with Richard Akresh, Mary Arends-Kuenning, Kathy Baylis, Ben Crost, Guillermo Cruces, Werner Baer, Marcelo Bérgolo, Leonardo Bonilla, Nicolas Bottan, Kristine Brown, Pablo Flores, Philip García, Tim Gindling, Carl Nelson, Mark Borgschulte, Ignacio Sarmiento, and Walter Sosa Escudero, as well as participants at numerous workshops and seminars. Any errors and omissions are entirely my own.

[2] See Card (1992), Card and Krueger (1994), Neumark and Wascher (2008), and Dube et al. (2010).

effective labor policy, and poverty alleviation.

This paper evaluates recent minimum wage policy in Honduras. Similar to other developing countries, Honduras sets high minimum wages that are weakly enforced in a segmented labor market. Assessments of minimum wage policy often rely on variation in the structure of minimum wages (Gindling and Terrell, 2009, Lemos, 2009, Alaniz et al., 2011, Comola and Mello, 2011, Khamis, 2013), large increases (Castillo-Freeman and Freeman, 1992, Harasztosi and Lindner, 2015, Muravyev and Oshchepkov, 2016), or institutional reforms to wage floor systems (Gindling and Terrell, 2007). Here, I exploit category-level variation from all three sources to quantify the consequences of minimum wages on compliance, labor market outcomes, and poverty. Estimates are drawn from 13 household surveys assembled into repeated cross-sections. These data cover eight wage floor hikes from 2005-2012 and provide information on almost 330,000 individuals in the Honduran labor force (approximately 41,000 per year).

Theoretically, the effectiveness of a country's minimum wage policy depends on whether it is able to redistribute earnings to low-paid workers without generating employment loss. Empirical work in developing countries often disagrees on which of these effects prevails.[3] Evaluating the consequences of higher minimum wages is thus an empirical question. To accurately estimate the impact of minimum wage hikes requires finding a source of exogenous variation in wage floors. Minimum wages are usually updated to account for inflation or aggregate economic conditions. Changing commodity prices cause shifts in labor supply and demand. Thus wage floors, wages, and employment are simultaneously determined, so regressing minimum wages on socioeconomic outcomes suffers from endogeneity bias.

[3] See Lemos (2007) and Neumark and Wascher (2008) for literature surveys.

Recent events in Honduras created natural experiments that generate plausibly exogenous minimum wage shocks. Honduras sets multiple minima that have differed across regional, industrial, and firm-size categories. This category-level structure is my main source of variation, which is akin to using state-level differences in the US. From 2005-2012, this variation was affected by annual minimum wage reforms, a large increase, and changes in the number of minimum wages. The largest shocks are due to the latter two events. First, President Manuel Zelaya authorized a 60% average increase in real minimum wages aiming to equalize floors across categories in 2009. Second, the number of minimum wages changed from industry firm-size minimum wages (23 categories) to regional floors (2 categories) in 2009, to region and firm-size minima (6 categories) in 2010, and returned permanently to a modified version of industry firm-size minimum wages in 2011 (37 categories). On average, real minimum wages in Honduras increased 10.8% over this period. Differential changes across categories encompass declines of -11.1 to hikes of 204.5%.

While minimum wage increases are the most visible component of this policy, enforcement and compliance are also key elements. Increasing legal minimum wages that are imperfectly enforced often results in non-compliance (Ashenfelter and Smith, 1979, Bhorat et al., 2015). About one of every three covered workers earns sub-minimum wages in Honduras (Gindling and Terrell, 2009), with some paid much less than their legally entitled wage (Ham, 2015). I take advantage of relatively constant enforcement levels over this period to test for partial compliance and approximate the effect of minimum wage hikes on non-compliance in the covered sector. The resulting evidence indicates that large employers partially comply with the regulation but small businesses do not comply. Moreover, large covered employers increase their level of non-compliance in response to higher minimum wages by 36%.

Because the Honduran labor market is segmented, I test the predictions of the dual-sector minimum wage model (Harris and Todaro, 1970, Boeri et al., 2011). In this framework, rising wage floors should lead to employment losses and higher average wages in the covered sector, and viceversa for the uncovered sector. Following the legislation, I define these sectors using occupational categories. Results provide strong and robust evidence in support of this model. A 10% increase in minimum wages lowers the likelihood of covered employment by about 8% and increases the probability of uncovered sector employment just over 5%. The data indicate that individuals substitute wage earning jobs for self-employment as a direct consequence of minimum wage hikes. Consequently, covered sector wages increase but rising labor supply in the uncovered sector leads to a negative net effect on informal wages.

Therefore, minimum wage increases contribute to the growth of the informal sector, consistent with findings in Comola and Mello (2011) and Muravyev and Oshchepkov (2016). Unlike most of the literature, I do find evidence of negative effects on wages in the uncovered sector. This result is driven by the large influx of wage earners into self-employment, suggesting that Hondurans would rather work in uncovered jobs than remain unemployed.

Since uncovered sector jobs in Honduras tend to be lower-paid part-time positions, average earnings in this sector often lie below covered sector incomes. Hence, there is a potentially adverse effect on individual well-being from a larger informal sector. I test whether minimum wages affect the likelihood of falling below national poverty lines, finding that increases in poverty for the uncovered labor force outweigh potential reductions for the covered labor force. This result indicates that higher wages for the covered workforce are unable to compensate for the resulting income losses in the uncovered sector that occur because of changes in labor force composition.

The remainder of this chapter is organized as follows. Section 1.2 outlines the dual-sector minimum wage model and reviews the empirical evidence. Section 1.3 describes minimum wage policy in Honduras and my identification strategy. Section 1.4 presents the data. Section 1.5 studies enforcement and compliance with minimum wages and Section 1.6 estimates the net effects of minimum wage increases on labor market outcomes and poverty. Section 1.7 concludes.

1.2 Minimum Wages in Developing Countries

1.2.1 Theory

Minimum wages in developing countries are commonly studied using a competitive dual-sector model that classifies workers as covered (formal) or uncovered (informal) first proposed by Harris and Todaro (1970).[4] The former are entitled to wage floors, while the latter are not. Each sector $s = \{c, u\}$ has its own labor demand and supply, so that equilibrium wages (w_s) and employment (E_s) are determined by the intersection of these curves. The key assumption is that wages in the uncovered sector, w_u, are more flexible than in the covered sector, w_c. This implies that mobility between sectors is possible, but limited. Individuals can migrate from covered to uncovered jobs freely, but moving from uncovered to covered employment is more difficult because wage rigidity causes segmentation between sectors (Mazumdar, 1989).

Figure 1.1 details the expected consequences of a binding minimum wage hike. Wages in the covered sector increase but some individuals lose their jobs. Displaced workers may either seek uncovered employment or choose to remain unemployed. If some decide to migrate, uncovered labor supply shifts from L_s to $L_s^{'}$. Since wages in the uncovered sector are flexible, this market

[4] Alternative minimum wage models may be found in Card and Krueger (1995), Manning (2003), and Boeri and van Ours (2008).

clears with higher employment but a lower equilibrium wage. In summary, the covered (uncovered) sector will have employment losses (gains) and higher (lower) average wages.

These are not the only potential consequences of minimum wage increases. Higher minima may also affect intensive margin employment by changing the amount of hours worked. A priori, effects could go either way. Differences in firm technology may lead to a rise or fall in hours (Strobl and Walsh, 2011). Effects on hours worked may also respond to different firing costs (Gindling and Terrell, 2007). If layoffs are costly, we may see a reduction in hours rather than employment, or a decline in both. But if termination costs are low, employers may downsize part-time staff while increasing hours worked by remaining employees.

Minimum wage increases may also have consequences that extend beyond the labor market. Since many workers rely on earnings as their main source of income, changing wage floors could indirectly affect poverty. If the predictions of the dual-sector model are borne out, the risk of income deprivation is expected to increase. This result is driven by covered employment loss and migration towards the lower-paid uncovered sector. However, poverty responses will also depend on whether minimum wage workers are in low income families, the level of wage floors relative to the poverty line, and intra-household factors.[5]

An unspoken assumption in this framework is that covered sector employers comply with minimum wage laws because governments effectively enforce them. However, regulation tends to be lax in most developing countries, which often leads to non-compliance (Ronconi, 2012). Enforcement affects firm-level compliance decisions, which play a key role in determining minimum wage impact. In fact, Basu et al. (2010) show that "a simple deviation from perfect to imperfect

[5]See Lustig and McLeod (1997), Saget (2001), Neumark and Wascher (2002), Fields and Kanbur (2005), and Gindling and Terrell (2010) for a more in-depth discussion of these factors.

enforcement is sufficient for theoretical predictions to be overturned".

Ashenfelter and Smith (1979) first modeled firm-level compliance decisions, with subsequent papers modifying and extending their approach.[6] Employers decide whether to comply with minimum wage laws based on their expected profits. Profits depend on revenue, costs, and the probability of getting caught non-complying ($\lambda \in [0, 1]$), which rises as enforcement becomes more strict. After a minimum wage hike, total profits decrease because labor costs rise. Under perfect enforcement, employers adjust their behavior according to theory. However, when enforcement is imperfect, firms may employ workers at wages below the minimum as long as they remain undetected. In practice, there is likely to be partial minimum wage compliance in developing countries, with both compliant and non-compliant employers (Bhorat et al., 2015).

1.2.2 Evidence

Most developing country studies find that minimum wages increase covered sector wages but have ambiguous employment effects. A few studies find no job losses (Lemos, 2009, Dinkelman and Ranchhod, 2012, Bhorat et al., 2013b), although many find evidence of modest declines in covered jobs (Bell, 1997, Fajnzylber, 2001, Maloney and Mendez, 2004, Gindling and Terrell, 2007, Alaniz et al., 2011, Comola and Mello, 2011, Bhorat et al., 2014).

Wage floor effects on the uncovered sector are unclear. Two studies find evidence of migration towards the informal sector (Comola and Mello, 2011, Muravyev and Oshchepkov, 2016). However, many authors find no effect on uncovered employment or wages. Perhaps the most striking result in the empirical literature is that minimum wage increases sometimes raise uncovered sector

[6] Bhorat et al. (2015) provide an excellent description of this literature.

wages.[7] This finding has been labeled the "lighthouse effect", since the primary explanation is that wage floors act as a numeraire in the uncovered labor market.[8]

Available evidence has differing assessments of minimum wage impact on poverty. Most studies, usually those that find null or small employment losses, report that minimum wage hikes lower deprivation (De Janvry and Sadoulet, 1995, Lustig and McLeod, 1997, de Barros et al., 2001, Saget, 2001, Devereux, 2005, Bird and Manning, 2008, Gindling and Terrell, 2010, Alaniz et al., 2011). However, these authors do not advocate wage floor policies because the potential costs of employment loss outweigh their possible distributional gains. Morley (1995) adds that poverty responses will vary depending on whether wage floor increases occur during growth or recession. Poverty will fall under the former and grow during the latter. Other papers have found that minimum wages increase poverty (Neumark et al., 2006, Arango and Pachón, 2007), often in cases when wage floors lead to adverse labor market effects.

Most research estimates minimum wage effects under weak enforcement and partial compliance. Average non-compliance in developing countries ranges between 10-70% (Rani et al., 2013). However, only a handful of studies recognize how this may affect their results and conclusions. Two countries that increase wage floors by the same amount but have different compliance rates may thus experience distinct consequences. Therefore, measuring enforcement and its subsequent impact on compliance is arguably as important to evaluate minimum wage policies than estimating its labor market and welfare effects.

Two studies analyze the effect of minimum wages in Honduras. Both define sectors using

[7] Such effects have been found in Brazil (Neri et al., 2000, Lemos, 2009), Argentina (Khamis, 2013), Costa Rica (Gindling and Terrell, 2005), and other Latin American countries (Maloney and Mendez, 2004).

[8] Alternative explanations are explored in Boeri et al. (2011).

minimum wage laws, where wage earners are covered and the self-employed are uncovered. The first, Gindling and Terrell (2009), finds a negative employment elasticity of -0.46 that dominates a positive wage effect of 0.29 for covered workers in large firms using industry-level panel data for 1990-2004. No wage or employment effects are found for wage earners in small firms and the uncovered sector. The second was carried out by the same authors on individual data from 2001-2004, and studies whether minimum wages reduce poverty (Gindling and Terrell, 2010). They find a 10% increase in mandated minima lowers the probability of extreme poverty by 2.2% but find no effect on overall poverty rates (extreme plus moderate).

This study contributes to the empirical minimum wage literature in several ways. First, it provides a comprehensive evaluation of the net labor market and welfare consequences of minimum wage policy in a developing country. Unlike previous work that often uses a single shock to quantify minimum wage effects, I exploit several sources of cross-sectional and temporal variation in multiple minimum wages. Second, it updates previous results for Honduras. Last, I also focus on enforcement and compliance with legal minimum wages. This broad approach allows to better understand how minimum wages affect labor and poverty outcomes, developing and regulating effective labor policy, and the potential of minimum wages as a tool for poverty alleviation.

1.3 Minimum Wage Policy in Honduras

1.3.1 History and attributes

Legal minimum wages in Honduras were first implemented in 1974 and are regulated by the General Directorate of Wages (DGS, in Spanish), which belongs to the Ministry of Labor. There have been about 30 updates since then, most of them during the past two decades. Annual adjustments are negotiated by a committee of Government, employer, and worker representatives. Discussions generally stall because the parties cannot agree on the amount of the increase. If this impasse cannot be resolved, a final decision is taken by the president. The resulting wage floors are published as decrees in the Senate's Newspaper, *La Gaceta*. Upon careful inspection of this legislation, several distinctive characteristics stand out.

First, multiple minimum wages exist at the same time, which vary by region, industry, and firm-size. Floors have usually been set for 23 categories, following the ISIC industrial classification: agriculture, non-metallic mining, metallic mining, manufacturing, utilities, construction, retail, transport, real estate, business services, financial services, communal and personal services, and the export (or *maquila*) sector.[9] Except for metallic mining, utilities, and the export sector, different minimum wages were set for small (1-15 employees) and large (16+ employees) firms until 2008. This structure has experienced several reforms. It changed to regional minima (2 categories) in 2009, to region and firm-size floors (6 categories) in 2010, and returned permanently to industry firm-size minimum wages in 2011 (37 categories).

Second, Honduras frequently sets daily wage floors. According to the DGS, full-time employees should be paid 30 daily minimum wages per month. Third, minima directly cover wage earners in private firms. Public employees are indirectly covered, since some are paid in multiples of the minimum wage (Gindling and Terrell, 2009). However, the public sector is not subject to labor inspections nor required to make collateral payments for mandated benefits.[10] Domestic work is

[9]The export industry in Honduras produces textiles and apparel, electric components for automobiles, imports and sells spare parts for machinery, and provides data processing services (de Hoyos et al., 2008).

[10]Employers must contribute a percentage of the worker's wage to a Christmas bonus, mid-year bonus, severance, social security payments, paid leave, contributions to the national training center (INFOP), housing contributions (RAP), and an educational transfer (COHEP, 2016).

considered a salaried occupation and thus protected by the Labor Code. Nevertheless, employers are not required to pay wage floors, so compliance is voluntary. This means that legally, employers, the self-employed, and unpaid family workers are the uncovered sector in Honduras. Fourth, covered employers can pay less than the legal minimum wage if they grant certain forms of in-kind compensation. Workers who receive food or housing may be paid 80% of the minimum wage, and 70% if provided both.

Last, similar to most countries, average minimum wage changes are indexed to inflation. Historically, the inflation rate served as a guide but was not always employed in negotiations. In 2013, a new mechanism incorporated productivity measures into minimum wage setting (García, 2011).[11] The correlation between changes in real floors and previous-year inflation is 0.594 and statistically significant at the 1 percent level. This implies that a regression of minimum wages on labor market outcomes and poverty is endogenous because wage floors, wages, and employment are simultaneously determined. To isolate the effects of rising minimum wages requires finding exogenous variation unrelated to the economic cycle. Using the attributes of Honduran minima and some unique policy circumstances, I propose several exogenous shocks.

1.3.2 Identifying exogenous variation in Honduran minimum wages

Exogenous variation in Honduran minimum wages may be obtained by exploiting category-level variation. From 2005-2012, this variation was affected by annual minimum wage updates, a large increase, and changes in the number of minimum wages. The DGS usually set 23 different industry

[11]The new mechanism is based on two equations: 1) $MW = \mathbb{E}\pi_{t+1} + P$ and 2) $MW > \pi_t$, where π denotes inflation (measured by the Central Bank) and P denotes productivity (measured by the Ministry of Labor). The first equation calculates the minimum wage increase as the sum of expected price changes and actual productivity gains or losses. The second equation requires that the calculated value is higher than actual inflation. For example, if the inflation forecast is 7% and productivity fell by 1.5%, the corresponding increase is 5.5%. If actual inflation is above this value (say 6%), then the mandated increase changes to six percent.

firm-size minimum wages in this period. For comparability, I maintain these categories throughout the analysis and convert decreed values into real hourly minimum wages.[12]

Table 1.1 shows yearly changes in real minimum wages for each industry firm-size category. Trends are plotted in Appendix Figure A.1. The average increase in real minimum wages was 10.8%. There is substantial variation across categories (the standard deviation is 26.4%), ranging from declines of -11.1% to increases of 204.5%. Hence, even if the average increase may depend on previous inflation, each category experiences different rates of change. After controlling for cross-sectional variation across categories (using industry firm-size and region effects) and the average change in the minimum wage (using time dummies), all remaining variation is arguably driven by the structure of minima and not the economic cycle.

Much of the observed variation was generated by a large increase in minimum wages. In 2009, during the last year of his elected term, minima were set unilaterally by President Manuel Zelaya. He raised average real minimum wages by about 60 percent with redistributive purposes.[13] The measure was unexpected. It was announced on December 23, decreed on the 27th, and took effect four days later. More importantly, it was unrelated to aggregate economic conditions. If endogenous, this update would respond to continuous growth and inflation, which is not supported by the data (see Appendix Table A.2). In fact, the increase was approved in spite of an anticipated economic downturn due to the global financial crisis (Cordero, 2009). An additional concern is that Zelaya operated under political motives, benefiting loyal districts who voted for his presidency four

[12] The procedure follows Gindling and Terrell (2009). I homogenize daily floors into monthly values and compute: Hourly MW = Monthly MW / (44 $\times$ 4.3). Calculated values for each industry firm-size category over time are shown in Appendix Table A.1.

[13] Appendix Figure A.2 shows this by plotting the percent change for each industry firm-size category and its pre-policy minimum wage. Categories with lower wage floors experienced the largest hikes from the policy.

years earlier. Appendix Table A.3 shows that this is not the case, as minimum wage increases in districts that voted for Zelaya were not significantly higher compared to communities who voted for the opposing candidate.

I also employ variation due to reforms in the number of minimum wage categories. In 2009, the system went from 23 minima set by industry firm-size categories to 2 regional floors, urban and rural. In 2010, the number of categories rose to six, urban and rural floors for 1-20, 21-50, 51+ employees. In 2011, setting returned to industry firm-size but was expanded to encompass four firm sizes, 1-10, 11-50, 51-150, 151+ employees, for a total of 37 minima.[14] These changes were due to concern with how to deliver minimum wages more efficiently and not in response to labor market conditions.

Jointly, these events provide variation within categories and over time in legal wage floors that is plausibly exogenous. Compared to previous studies, there is greater variation across multiple minimum wages, as Figure 1.2 shows. The Honduran case thus presents a singular opportunity to evaluate the labor market and welfare consequences of minimum wages in a developing country.

1.4 Data

I construct repeated cross-section data from Honduran household surveys, the *Encuesta Permanente de Hogares de Propósitos Múltiples* (EPHPM). The EPHPM is nationally representative and conducted twice a year —May and September— by the National Statistics Institute (INE). It gathers detailed information on demographics, education, employment, earnings, and household

[14]Ten industries have been considered since 2011: agriculture, mining, manufacturing, utilities, construction, retail, transport, financial/real estate/business services, communal and personal services, and export. Mining was unified into a single category and business services, real estate, and financial services were also aggregated.

poverty status. Thirteen waves collected between 2005-2012 are joined for this study. All variables are identically defined to ensure comparability over time. Unfortunately, panel data on labor market and welfare outcomes are unavailable.

Survey data are augmented with information from two sources. The first are minimum wage tables published in *La Gaceta*.[15] Using the decrees, I assign the corresponding wage floor to each individual based on their self-reported industry and firm-size. Since the surveys identify whether respondents receive food or housing from their employer, minimum wages are adjusted to account for this compensation. The second source is the Honduran Central Bank (BCH), which provides aggregate and industry-level information. Following standard practice, consumer price indexes are used to deflate minimum wages and actual wages. Industry-level variables are used to control for changing market conditions over time in each sector of production.[16] On one hand, I use the monthly production index for each industry (IMAE) since there is more than one survey per year. On the other, I employ the BCH's estimates of value added (VA) to account for differential yearly growth in production.

My population of interest are Hondurans in the labor force, classified into covered (formal) and uncovered (informal) sectors. Following the legislation, I define the covered sector as occupations directly and indirectly covered by minimum wages: privately employed wage earners –in large and small firms–, public sector employees, and domestic workers. The uncovered sector comprises the self-employed, unpaid family workers, and employers. To consider differences within these

[15]Appendix Table A.4 lists the selected EPHPM surveys and valid decrees at the time of data recollection. During the period, most minimum wage changes became effective on January 1st of the respective calendar year. The exception was 2010, when the update applied on September 1st. Hence, in the data, the 2009 scheme was still applicable at the time when fieldwork for the May 2010 survey was undertaken.

[16]The BCH's classification of industries does not coincide with the minimum wage decrees. However, all wage floor categories are nested within the BCH's nine aggregate groupings.

definitions, some results are presented separately by occupation.

The data provide complete information for the employed but not the unemployed. Surveys ask the latter their occupation and industry of previous employment, but do not inquire about firm size. Labor force entrants into unemployment have no information on previous occupation or industry, so are excluded from the analysis. Employed individuals are assigned their category-specific minimum wage while the unemployed are imputed the large firm wage floor for the industry of their last reported job.[17] Therefore, estimates for the entire labor force will require aggregating industry firm-size categories at the industry-level. Nevertheless, variation and trends are unchanged when using fewer categories (see Appendix Table A.5). Following the literature, the analysis focuses on adults 15 years or older. I further restrict the employed sample to individuals who report working less than 84 hours per week and earn below the 99th percentile of real wages. This leaves 327,764 valid observations, about 41,000 individuals per year (or 25,200 per wave).

Table 1.2 provides descriptive statistics by sector.[18] About 95% of the covered labor force is employed and 5% is unemployed. Employed individuals are paid an average of 13.06 Lempiras an hour (about US$1.31) and work full-time jobs, 44 hours per week. Slightly over 27% dwell in extremely poor households using the official poverty classification in Honduras.[19] Over half live in a poor household. Just under two thirds are male and less than half are married. On average, the covered workforce has 7.5 years of education, equivalent to incomplete secondary schooling. Most live in urban areas, with large families, and are not the heads of their household. Individuals

[17]Since large firm minimum wages increased less than small firms in this period (see Table 1.1), this assigns the unemployed the lowest change in minimum wages. Results are unchanged when imputing minimum wages for small firms (largest increase) or the average between the two. These estimates are not reported here but are available upon request.
[18]Appendix Table A.6 shows descriptive statistics by occupation.
[19]See Sobrado and Clavijo (2008) for a description of poverty measurement in Honduras.

in the covered sector work or have worked mostly in services, agriculture, retail, manufacturing, construction, and the export sector.

The uncovered labor force is almost entirely employed (99% vs 1%) in part-time jobs (34 hours per week) and earns approximately 10.91 Lempiras an hour (US$1.09). Compared to the covered sector, almost twice as many workers live in extremely poor households. This sector has marginally fewer men but more married individuals. Uncovered workers accumulate 5 years of formal education, less than complete primary. These individuals are usually located in rural areas and are often the household heads of large families. Hondurans in the uncovered sector are mainly attached to agriculture, retail, and manufacturing.

Table 1.3 presents annual trends in labor market outcomes and poverty. Given that average minimum wages increased throughout the period, overall employment rates change slightly in response. Figure 1.3 focuses on trends in labor force composition. The share of employed individuals in the covered sector falls while uncovered employment and overall unemployment rise. Covered sector wages increase after minimum wage hikes while uncovered wages decrease. These trends suggest that the raw data are in line with the predictions of the dual-sector minimum wage model.

1.5 Enforcement and Compliance

1.5.1 Patterns and trends

Honduran minima may affect many workers because they are set high relative to average wages. To show this, I plot a widely used measure of the minimum wage's "bite" in Figure 1.4, its ratio to the mean covered sector wage: $\bar{MW}/\bar{w}_c$. This indicator grew from 0.66 in 2005 to 1.13 in 2012. ILO (2008) estimates from for over 50 countries indicate that this estimated minimum to mean wage ratio lies within range of other developing labor markets such as Argentina, El Salvador, Guatemala, Nepal, Paraguay, and Venezuela.

Labor regulation in developing countries is often imperfectly enforced, mostly due to budget constraints (Gindling et al., 2015). Honduras is no exception, with only 139 inspectors in 20 regional offices available to monitor labor code violations (UPEG, 2016). Among other duties, inspectors visit firms to assess compliance with minimum wage laws. Gindling and Terrell (2009) point out that large firms are more likely to be inspected than small businesses. Enforcement changed slightly throughout the period (see Supplementary material for Chapter 1 Figure A.3). In fact, fewer inspections were performed after the 2009 increase. Lax regulation is also reflected in low fines. If an employer commits an infraction, lump-sum penalties range between 1000-5000 Lempiras (US$50-250) and occasionally require reinstating back pay.

Given the complexity of wage floors in Honduras, I examine compliance by analyzing the distribution of wages in covered versus uncovered occupations. Figure 1.5 plots kernel densities for the distribution of log hourly wages minus log minimum wages for occupations with valid earnings. This re-centers the distribution so that $0 = MW$. If covered firms comply with mandated minima, we should see censoring from below at zero and a higher spike at this value. I find differing levels of compliance across occupations. Minimum wages are mostly complied with in large firms and the public sector but small businesses and domestic employers do not comply. In all covered jobs, there is evidence of some non-compliance from employers. Densities for the self-employed and employers show no indication of compliance in uncovered occupations.

Table 1.4 presents non-compliance indicators for the sample. It begins with the fraction of workers earning below, at, and above the hourly minimum wage. About 47% of directly covered employees earn below mandated minima, consistent with rates in other countries (Rani et al., 2013) and previous estimates for Honduras (Gindling and Terrell, 2009). Non-compliance also varies across industries and regions (Ham, 2015), and as shown here, by occupation. On one hand, it is 31.9% and 62.4% for large and small firm wage earners, respectively. On the other, 9.5% of public employees and 66% of domestic workers earn sub-minimum wages. In the uncovered sector, almost 63% of the self-employed earn below the minimum wage while just one in four employers earns lower wages than the minimum.

While compliance rates are informative, they do not tell the entire story. Recent research argues that the depth of non-compliance is also relevant (Bhorat et al., 2013a). Similar to poverty measures, these papers report the incidence, gap, and severity of minimum wage violations. They propose computing average shortfalls, the ratio between the gap and incidence of non-compliance, to measure how far actual wages are from minimum wages. These estimates are shown in Table 1.4. Underpaid wage earners in large firms earn 36% less than their corresponding wage floor and 50% less in small firms. This shows that in addition to being paid below the legally entitled wage, some workers earn much less on average.

The remainder of Table 1.4 compares compliance before and after 2009. I conduct t-tests for the null hypothesis that estimated indicators were unchanged over time. Non-compliance rates increased significantly for all occupations. The fraction of underpaid large firm wage earners rose by 12 percentage points and small firm non-compliance increased by 23 percentage points. Changes are smaller for public employees. Differences in the average shortfall of wages from minimum wages reflect similar patterns. This evidence suggests that employers adjust both the level and depth of non-compliance after minimum wage increases.

These estimates may potentially suffer from measurement error. Perhaps transforming minimum wages into hourly values generates noise because the surveys ask respondents for their monthly labor income. To check this, I re-estimate densities and shares using monthly minimum wages and earnings for full-time workers in Figure A.4 and Table A.7 in the Appendix. Overall, the resulting conclusions are unchanged. Inability to measure some forms of non-monetary payments may also affect compliance estimates (Gindling and Terrell, 2009). For instance, apprentices may be paid below the minimum during their first six months on the job. Similarly, some industries compensate workers by piece rate (manufacturing), commissions (retail), and tips (services). Errors in these cases could go either way. However, there is no possibility to assess these factors from the available data.

This analysis reveals some patterns of the relationship between enforcement and compliance with minimum wages in Honduras. First, enforcement is weak and remained relatively stable during 2005-2012, despite multiple policy changes. Second, there are varying levels of compliance within the covered sector. Minimum wages are complied with by large firms but not small businesses, although legal wage floors apply to both employers. Interestingly, the public sector is largely compliant despite not being subject to regulation. Last, the depth of non-compliance matters, since some covered workers are substantially underpaid.

1.5.2 Testing for partial compliance with minimum wages

Obtaining estimates of minimum wage impact on employers' incentives to comply is challenging for several reasons. First, appropriate data are not always available (Hamermesh, 1991). Firm-level records can mislead researchers because employers are expected to misreport labor violations. Second, compliance decisions depend on wage floors and enforcement (Ashenfelter and Smith, 1979, Bhorat et al., 2015). Although minimum wages are readily measurable, data on enforcement tend to be scarce. Moreover, it is hard to isolate the impact of each channel. Last, clearly identifying treatment and control groups is an arduous task.

The Honduran case helps overcome some of these issues. Following the literature, I use employee data since it measures non-compliance more precisely than firm-level records. Since enforcement remained relatively stable over this period, compliance adjustments may be mostly attributed to changing wage floors. Coverage definitions and recent reforms generate a policy experiment. On the one hand, treated employers include large and small firms, since they must pay minimum wages and are actively regulated. A suitable comparison would comprise firms not required to pay legal wage floors nor subject to inspections, but which still comply. As shown beforehand, the public sector is such an employer. On the other hand, comparing non-compliance before and after 2009 provides variation over time.

Figure 1.6 plots non-compliance rates for large firm, small firm, and public employees. The public sector has the lowest non-compliance rate, followed by large and small employers, respectively. Before 2009, trends behave similarly across occupations. After 2009, non-compliance slightly increases in the public sector. Observed changes are higher for large employers and more striking for small firms. This suggests that directly affected employers are actively choosing to pay more workers below the minimum wage after a large hike.

These conditions allow using a difference-in-differences strategy to test for partial compliance with minimum wages.[20] This method assumes that in absence of changes to Honduran minimum

[20] Two studies have tested partial compliance with minimum wage laws. Dinkelman and Ranchhod (2012) employ a method that is informative as long as minimum wages have no employment effects. Bhorat et al. (2015) use a

wage policy, compliance in large and small firms would have behaved similarly to the public sector. Any significant differences between covered occupations and the public sector indicate that some regulated firms decide not to comply with the minimum wage increase, denoting partial compliance. I estimate the following equation by OLS:

$$NC_{ijt} = \alpha T + \beta(Post \times T) + \gamma X_{ijt} + \theta Z_{jt} + \lambda_j + \delta_t + u_{ijt} \tag{1.1}$$

where NC_{ijt} is a binary variable that identifies if worker i in industry firm-size category j at time t is paid below the minimum wage. $Post$ is an indicator variable equal to unity after 2009 and T identifies whether the worker is a wage earner. I also consider wage earners in large or small firms separately. The coefficient on the interaction term captures the average difference in non-compliance across treatment and control groups before and after 2009. An expanded version of Equation (1.1) is also estimated where the treatment identifier is interacted with dummy variables for each year. This allows testing the parallel trends assumption of difference-in-differences since several years of pre-policy data are available. It also permits identifying any heterogeneous effects over time. Given limitations with the data and other potential confounders, these estimates cannot be interpreted as the causal effects of wage floor hikes on non-compliance.

Since workers across occupations are different, I control for observable characteristics in X_{ijt}, including a constant, gender, marital status, years of education, potential experience and its square, and a dummy for urban residence. I also condition on time-varying industry-level attributes (Z_{jt}): the log of the monthly production index (IMAE) for each wave and the log of yearly value added

difference-in-differences strategy that compares covered and uncovered groups. My strategy is similar to the latter, which imposes fewer restrictions on expected labor market effects.

(VA), which control for changes in industry-level demand conditions. Finally, I include industry firm-size fixed effects (λ_j) to capture cross-sectional variation across minimum wage categories and time dummies for each wave to account for secular trends (δ_t). Standard errors are clustered by the 23 industry firm-size categories.

Results in Table 1.5 reveal that non-compliance rates in covered occupations increase after a large minimum hike. Panel A denotes that relative to the public sector, the share of wage earners who are paid sub-minimum wages increases by 32% on average. Separating wage earners into large and small firms shows that non-compliance increases about 36% for the former and 26% for the latter. Panel B shows results by year. There are no differential trends when comparing wage earners and large firms to the public sector, but one significant pre-policy difference for small firms (in 2006). For both firm sizes, there is an increase in 2009. However, non-compliance continues to rise in large firms but not small firms.

These findings indicate that large employers partially comply with minimum wages but small businesses do not comply. After a 60% increase, some large employers comply and others avoid the regulation. Small firms do not change their practices. These results are depicted in Appendix Figure A.5, which plots the distribution of log wages minus log minimum wages before and after 2009. The distribution for large firms compresses around the minimum wage but the lower tail increases, denoting partial compliance. The distribution shifts to the left for small firms, with no indication of bunching around the minimum wage.

1.6 The Net Consequences of Minimum Wages

1.6.1 Estimation strategy

I estimate the net effects of legal minimum wages on labor market outcomes and poverty using a specification commonly found in the literature (Neumark and Wascher, 2008):

$$y_{ijt} = \alpha + \beta MW_{jt} + \gamma X_{ijt} + \theta Z_{jt} + \lambda_j + \delta_t + u_{ijt} \quad (1.2)$$

Here, y_{ijt} is the outcome for individual i in minimum wage category j at time t. MW_{jt} is the log real hourly minimum wage corresponding to their self-reported category. This specification controls for the same individual and industry-level covariates, category, and survey wave effects in Equation (1.1). A second specification adds linear time trends to account for heterogeneous time effects across minimum wage categories (Allegretto et al., 2013).

I present estimates of minimum wage impact on employment, labor force composition, hours, wages, and poverty. Employment, composition, and poverty estimates use within industry variation in minimum wages over time since they include all Hondurans in the labor force. Hours and wage equations use within industry firm-size variation over time in minimum wages since these outcomes are available for employed individuals. The selected estimation methods are Probit for employment and poverty, Multinomial Logit for labor force composition, and OLS for hours and wages. I also consider alternative specifications, which are discussed in the next sub-section. Standard errors are clustered by industry (or industry firm-size) depending on the variation used to identify each equation.[21]

[21] Given the changes in minimum wage categories over time, multiple clustering options were tested. For comparability, I selected the 13 aggregate categories for estimates that include Hondurans in the labor force and 23 categories for employed individuals. Results are unchanged when using a different number of clusters.

The coefficient of interest in all relationships is β. Once controlling for covariates and fixed effects, this parameter captures the net effect of deviations from the average change in minimum wages within categories over time. We may interpret employment, composition, hours, and poverty estimates as elasticities, i.e. the net effect of a 1% increase in legal minimum wages. This interpretation is not possible for wages. Statistically significant wage estimates may be due to changing wage floors and/or composition effects. In the covered sector, average wages may be affected because: i) some workers are paid the new minimum wage, ii) some accept higher sub-minimum wages to keep their jobs, and iii) some lose their jobs and are no longer included in the sample to compute average wages (Gindling and Terrell, 2009). In the uncovered sector, significant wage effects could be due to "lighthouse" effects or market adjustment if there is evidence of changing labor supply in this sector.

The dual-sector model predicts that minimum wage increases should lead to employment losses and higher average wages in the covered sector, and viceversa for the uncovered sector. The effect on hours worked depends on firing costs. The Honduran labor code requires employers to pay high severance, so we should also expect a reduction in hours, at least for the covered sector.[22] Poverty impact is conditional on labor market results. If the predictions of the dual-sector model are borne out, the probability of income deprivation is expected to increase. Otherwise, poverty may decrease or remain unaffected.

[22] Severance depends on whether layoff is justified or not. If justified, employees are compensated for any remaining vacation days, as well as their accumulated mid-year and Christmas bonuses. If unjustified, they also receive two months compensation as notice and one monthly salary per year of employment.

1.6.2 Labor Market Outcomes

Table 1.6 reports the estimated net effects of minimum wages on the Honduran labor market. Employment results are presented for the full sample, regardless of sector or occupation. These coefficients report the change in the probability that an average individual is employed relative to being unemployed. A 10% increase in legal minimum wages reduces overall employment by 0.9% for the basic specification and by 1.1% when including linear category time trends.[23]

These negative coefficients may arise because wage floors reduce employment or increase unemployment by attracting more individuals into the labor force. Since the sample does not include new entrants into unemployment, employment loss is more likely. Moreover, an analysis from the raw surveys reveals that most labor market entrants have ensured jobs (93%) while very few are unemployed (7%). Therefore, minimum wages cause modest employment declines in Honduras, of similar magnitude to reported estimates in other studies.

While wage floors slightly reduce the probability of employment relative to unemployment, this does not rule out migration among sectors. To test for evidence of such movements, I estimate a Multinomial Logit model. The dependent variable identifies three categories: unemployed (0), employed in the covered sector (1), and employed in the uncovered sector (2). For comparability with the employment results, the base category is unemployment. The coefficients on the minimum wage variable identify the change in the probability that an average individual is employed in the covered or uncovered sector relative to being unemployed. Results indicate that labor force composition changes as minimum wages increase. A 10% hike in minimum wages lowers the

[23]Given that these coefficients are estimated from a Probit, they indicate that a 10% increase in the real minimum wage reduces the probability of being employed by 0.0085 and 0.0108. Relative to the mean employment rate, this indicates that a 10% increase in minimum wages reduces employment between $(0.0085/0.971) \times 100 = 0.9\%$. and $(0.0108/0.971) \times 100 = 1.1\%$.

probability of covered employment between 8 and 10 percent and increases the likelihood of employment in the uncovered sector by 5 to 7 percent. These findings suggest that the estimated employment effect for the full sample is averaging significant declines in covered jobs and gains in uncovered employment.

To further investigate this change in labor force composition, I estimate a Multinomial Logit where the individual's occupation is the dependent variable. Marginal effects are shown in Panel B, columns 4-9. The decline in covered sector employment is mainly driven by a loss of wage earning jobs, since effects on public sector and domestic workers are close to zero and precisely estimated. Rising labor supply in the uncovered sector is mainly due to a higher likelihood of self-employment and a small rise in the probability of carrying out unpaid work.

Results for intensive margin employment indicate that minimum wages lower the amount of hours worked for the full sample. This result is driven by reductions in the covered sector, where a 10% increase in minimum wages lowers hours worked by about 2%. Estimates by occupation reveal that some adjustment takes place for wage earners, but larger declines are observed for public sector employees and domestic workers. There is no evidence that minimum wages affect the number of hours worked in the uncovered sector.

Table 1.6 concludes with the wage equations. Minimum wages have no effect on wages for the full sample. Once again, this masks differences across sectors. Higher minimum wages increase covered sector wages, with coefficients ranging between 0.24 and 0.29. Since legal wage floors do not apply in the uncovered sector, parameter estimates reflect indirect consequences. Wage coefficients for the uncovered sector are negative, between -0.52 and -0.69, and statistically significant. Estimates by occupation show that an increase in mandated minima increases hourly pay for wage earners, public sector employees, and domestic workers. The negative net effect on the uncovered sector is driven by downward pressure on wages for self-employed workers since employer wages are unaffected. Unreported results that use monthly earnings as the dependent variable and control for hours worked and their square provide similar findings.

These findings are robust to alternative estimation methods, as shown in Appendix Table A.8. For overall employment, results are fairly similar when estimating OLS or IV regressions that use minimum wages lagged one year as an instrument (the latter approach follows Gindling and Terrell (2007)). Estimating labor force composition effects using Multinomial Probit, which relaxes the assumptions of Multinomial Logit, also provides similar results.[24] Alternative specifications for hours and wages in the covered sector are robust to specification choice. Uncovered sector results are noisier, due in part to lagged minimum wages being a weak instrument with a small first stage coefficient and larger standard error.

Since minimum wages vary by industry firm-size categories, another robustness exercise involves aggregating the data to this level and taking advantage of the panel structure, a method used in a previous study for Honduras (Gindling and Terrell, 2009). Results are shown in Appendix Table A.9.[25] Aggregate results are in line with my reported findings, but are mostly insignificant. Only wage effects for the covered sector are different from zero. Aggregating heterogeneous individuals is known to cause a loss of information and statistical power (Bertrand et al., 2004). Not

[24]Multinomial Logit assumes independence of irrelevant alternatives (IIA). Multinomial Probit is more flexible since it allows arbitrary correlation across alternatives. However, it has practical limitations with five or more alternatives and thus cannot be estimated by occupation. See page 649 in Wooldridge (2010) for details.

[25]I report three specifications: the within estimator (FE), the within estimator including a lag of the dependent variable (FE-LDV), and Arellano-Bond dynamic panel estimates that use lags of the dependent variable as instruments (GMM-DIF). First differences and system-GMM were estimated but not reported. Standard errors are robust to heteroscedasticity and clustering, and were estimated by block bootstrap with 200 replications when possible. Since there are 23 industry firm-size categories, block bootstrap results in less precision as forewarned by Bertrand et al. (2004).

surprisingly, confidence intervals for many coefficients on the minimum wage variable include the estimates in Table 1.6 obtained from individual-level data.

These results provide strong evidence in support of the dual-sector minimum wage model. Findings are consistent with previous evidence for Honduras, with estimates for the covered sector within the confidence intervals reported in Gindling and Terrell (2009). Although they find no effects on the uncovered sector, I do find evidence of higher employment and lower wages in that sector. Higher uncovered sector employment is consistent with findings in Comola and Mello (2011) for Indonesia and Muravyev and Oshchepkov (2016) in Russia. However, unlike these two studies, I find evidence of negative net effects on wages in the uncovered sector. This result seems to be driven by a substitution from formal to informal employment, mostly wage earners becoming self-employed workers. In line with the theory, the uncovered labor market in Honduras adjusts to this influx by lowering average wages.

1.6.3 Poverty

Given the net labor market effects of minimum wage policy in Honduras, we should expect a higher risk of deprivation, especially for the uncovered workforce. Since informal jobs are mostly lower-paid part-time positions, uncovered sector earnings often lie below covered sector income (see Appendix Figure A.6). Therefore, a growing informal sector generates income losses that may push some individuals into poverty. However, if income gains for the covered sector outweigh such losses, minimum wages may actually reduce poverty.

Income deprivation in Honduras is measured by the poverty line method, which yields two classifications of poverty: extreme and moderate. The former includes households whose per capita income impedes affording a basic food basket and the latter identifies families who are able to purchase food but cannot cover additional expenses (housing, education, health, transport, etc.). Honduras is one of the poorest countries in Latin America, with extreme poverty levels close to 50% and moderate poverty around 18%, so overall poverty is 68%.

We would like to approximate the effect of wage floors on household poverty, since deprivation is measured at this level. I follow Gindling and Terrell (2010) and multiply the survey weights by the ratio of household size and the number of workers ($\omega \times \frac{N}{N_w}$) to obtain an estimate of minimum wage effects on the average household, not just the labor force. Estimates that use unadjusted weights are not reported but provide largely similar results.

Table 1.7 presents Probit estimates of the net effects of minimum wages on extreme and overall poverty (extreme plus moderate). Relative to being non-poor, minimum wage increases have a small positive effect on extreme and overall poverty for the full sample. This result averages opposing impact across sectors. A 10% increase in minimum wages has a negative but insignificant effect on the probability of extreme poverty for the covered labor force. The same minimum wage hike significantly raises the odds of extreme deprivation for uncovered individuals between 1.6-4%. There are no effects on overall poverty for the covered workforce but positive and significant impact for the uncovered labor force.

These results hold when considering alternative specifications (see Appendix Table A.10). I also estimate separate regressions by occupation to determine whether some workers are more vulnerable to fall into poverty. Wage earners have a lower likelihood of deprivation but the effect is insignificant. Public sector workers and domestic workers are more vulnerable. Hondurans in self-employed jobs are the most adversely affected. A 10% hike in minimum wages increases the probability of extreme poverty by 2-4% and the likelihood of overall poverty between 1-2% for this uncovered occupation. Employers are mostly unaffected.

My findings oppose those in Gindling and Terrell (2010), who find that minimum wage increases modestly reduce extreme poverty. Given the findings in the previous sub-section, the scenarios are different. Hondurans obtain 90.4% of their total income from earnings, so the observed growth in lower-paid informal employment is pushing some households below poverty thresholds. From 2005-2012, increases in poverty for the uncovered labor force outweigh reductions for the covered labor force due to the large minimum wage hikes over the period. These conditions imply that raising minimum wages does not reduce poverty.

1.7 Conclusion

This paper evaluates recent Honduran minimum wage policy. Using repeated cross-section data and exploiting large category-level variation in wage floors, I estimate their net effects on compliance, labor market outcomes, and poverty. Results provide credible evidence in support of the dual-sector minimum wage model. While employment losses are small, I find changes in labor force composition. A 10% increase in minimum wages lowers the likelihood of covered employment by 8% and increases the probability of uncovered sector employment by 5%. Specifically, wage earning employment falls while self-employment rises. Covered wages increase but rising labor supply in the uncovered sector leads to a negative net effect on wages and earnings. These labor market effects result in a higher risk of poverty for the uncovered labor force that is not compensated by poverty reduction in the covered sector.

The negative impact of minimum wages occurs in an institutional context with weak enforcement of labor regulation. This setting leads to partial compliance with mandated minima, where compliant and non-compliant employers co-exist. In Honduras, large employers are mostly compliant while small businesses do not comply. After a large minimum wage hike, large firms increase their average non-compliance rate by 36%. This result suggests that compliant employers mitigate the adverse effects of minimum wage increases by avoiding the regulation. Without credible enforcement from governments, labor violations are likely to continue rising. Adverse minimum wage effects on the formal sector may worsen because of non-compliance. In Honduras, compliance is about 51%, which suggests that employment losses would double under full compliance.

While the estimated net effects of minimum wages in Honduras are seemingly robust, they are not definitive. The most important limitation in this study is the absence of panel data. Inability to track the same individuals over time does not allow observing transitions across or within sectors to estimate a structural model that captures the dynamics behind the estimated net effects. Such results would lead to a better understanding of how the adverse consequences of minimum wages come to pass. Despite these and other potential limitations, this study updates and improves upon previous work for Honduras, while also overcoming common empirical issues in the broader minimum wage literature.

The policy implication of these results is that setting high minimum wages has detrimental effects on labor markets, well-being, and compliance. While Honduran minimum wage policy is unlikely to offer a template for other nations, it provides a cautionary tale. To fully understand how minimum wage policies ultimately fare and how that differs from what we would like them to accomplish we need to better understand the informal economy, why people enter this sector, and the long-term consequences of participating in such activities.

1.8 Tables and Figures

Table 1.1. Yearly changes in real hourly minimum wages by industry firm-size categories

Category	Firm size	2006	2007	2008	2009	2010	2011	2012
Agriculture	1-15	6.9	4.2	-2.2	59.3	-3.2	-3.3	1.1
	16+	4.4	2.3	-1.3	33.8	1.1	-7.3	2.0
Non-metallic mining	1-15	6.9	4.1	-1.3	67.6	-8.3	19.7	1.1
	16+	4.4	2.3	-0.4	42.1	-6.6	21.0	0.6
Metallic mining	All	4.4	-0.5	-4.0	16.0	-7.1	15.2	-1.9
Manufacturing	1-15	6.9	4.1	-1.3	71.9	-4.8	15.2	6.7
	16+	4.4	2.3	-0.4	44.3	2.3	9.4	-0.1
Utilities	All	4.4	-0.5	-0.4	22.9	-0.8	19.5	2.1
Construction	1-15	6.9	4.1	-1.3	69.4	-4.6	20.5	6.9
	16+	4.4	2.3	-0.4	45.8	-1.6	11.2	0.4
Retail	1-15	6.9	4.1	-1.3	77.2	-3.8	13.2	5.9
	16+	4.4	2.3	-0.4	50.0	-0.1	7.4	1.0
Transport	1-15	6.9	4.1	-2.2	52.8	-6.2	19.9	5.6
	16+	4.4	2.3	-0.4	53.4	1.4	7.3	1.2
Real Estate	1-15	6.9	4.1	-2.2	58.4	0.7	9.2	5.2
	16+	4.4	2.3	-0.4	51.0	5.7	8.0	-0.8
Business Services	1-15	-3.7	-6.2	-11.1	204.5	-3.5	12.9	4.8
	16+	-3.7	-6.2	-11.1	164.7	-0.3	10.6	0.6
Financial Services	1-15	5.5	5.1	0.0	24.3	-6.1	14.8	5.2
	16+	5.5	4.1	0.0	27.3	1.5	8.4	1.6
Communal and Personal Services	1-15	6.9	4.1	-1.3	74.0	-4.0	16.1	7.8
	16+	4.4	2.3	-0.4	49.8	-2.6	7.4	0.7
Export	All	4.4	-0.5	-4.0	-5.7	2.6	6.0	1.4
Average		**4.7**	**2.0**	**-2.1**	**58.9**	**-2.1**	**11.4**	**2.6**

Source: Own calculations from real hourly minimum wage values in Appendix Table A.1.
Notes: The table shows percentage changes in legal minimum wages relative to the previous year.

Table 1.2. Descriptive statistics by sector, averages for 2005-2012

	Full sample		Covered		Uncovered	
	Mean	(SD)	Mean	(SD)	Mean	(SD)
Employment, hours, and wages						
Employment rate	0.971	(0.168)	0.947	(0.223)	0.993	(0.083)
Hours per week	38.99	(17.351)	44.12	(14.433)	34.26	(18.435)
Share full-time ($\geq$44 hpw)	0.432	(0.495)	0.577	(0.494)	0.299	(0.458)
Real Hourly Wages	12.11	(14.017)	13.06	(13.450)	10.91	(14.614)
Household poverty status						
Extremely Poor	0.376	(0.484)	0.276	(0.447)	0.477	(0.499)
Poor	0.594	(0.491)	0.519	(0.500)	0.669	(0.471)
Individual & household characteristics						
Males	0.644	(0.479)	0.653	(0.476)	0.634	(0.482)
Married	0.550	(0.497)	0.483	(0.500)	0.614	(0.487)
Years of education	6.23	(4.425)	7.51	(4.592)	5.02	(3.890)
Potential experience	24.1	(17.234)	18.5	(13.922)	29.4	(18.369)
Household size	5.44	(2.472)	5.41	(2.453)	5.47	(2.489)
Is household head	0.451	(0.498)	0.390	(0.488)	0.509	(0.500)
Lives in urban area	0.490	(0.500)	0.604	(0.489)	0.381	(0.486)
Composition across industries						
Agriculture	0.343	(0.475)	0.222	(0.415)	0.458	(0.498)
Non-metallic mining	0.002	(0.045)	0.002	(0.046)	0.002	(0.043)
Metallic mining	0.001	(0.024)	0.001	(0.032)	0.000	(0.014)
Manufacturing	0.097	(0.296)	0.099	(0.299)	0.095	(0.293)
Utilities	0.004	(0.066)	0.009	(0.092)	0.000	(0.019)
Construction	0.063	(0.243)	0.085	(0.279)	0.042	(0.201)
Retail	0.222	(0.415)	0.163	(0.369)	0.277	(0.448)
Transport	0.035	(0.185)	0.039	(0.194)	0.032	(0.175)
Real Estate	0.002	(0.041)	0.003	(0.051)	0.001	(0.029)
Business Services	0.021	(0.143)	0.030	(0.171)	0.012	(0.108)
Financial Services	0.011	(0.106)	0.023	(0.148)	0.001	(0.028)
Communal and Personal Services	0.151	(0.358)	0.251	(0.433)	0.057	(0.232)
Export	0.048	(0.214)	0.075	(0.263)	0.023	(0.149)
N	327,764		166,976		160,788	

Source: Own calculations from EPHPM surveys.
Notes: All statistics are weighted. Wages are expressed in real Lempiras. The average real exchange rate for the period is 10 Lempiras per $1 USD.

Table 1.3. Labor market and poverty trends by sector

	2005	2006	2007	2008	2009	2010	2011	2012	Average
Average Hourly MW	7.07	7.47	7.66	7.52	11.55	11.00	11.95	12.62	8.71
Employment rate									
Covered	0.946	0.952	0.960	0.957	0.949	0.934	0.939	0.943	0.947
Uncovered	0.993	0.995	0.994	0.992	0.995	0.990	0.993	0.994	0.993
Employment composition									
Covered employed	0.496	0.470	0.479	0.483	0.465	0.438	0.451	0.421	0.462
Uncovered employed	0.473	0.504	0.498	0.491	0.508	0.526	0.516	0.550	0.509
Unemployed	0.031	0.026	0.023	0.026	0.027	0.036	0.033	0.029	0.029
Hours per week									
Covered	45.93	45.44	44.49	44.26	42.66	42.78	43.67	44.32	44.12
Uncovered	37.14	35.89	35.53	34.63	31.97	31.29	34.46	34.64	34.26
Real wages									
Covered	12.07	12.48	12.90	13.13	13.74	13.46	13.38	13.04	13.06
Uncovered	10.23	10.70	12.48	11.08	11.81	10.49	10.98	9.22	10.91
Extreme Poverty									
Covered	0.304	0.273	0.278	0.251	0.261	0.289	0.276	0.286	0.276
Uncovered	0.549	0.526	0.453	0.428	0.423	0.485	0.472	0.524	0.477
Poverty									
Covered	0.529	0.500	0.535	0.499	0.500	0.532	0.525	0.535	0.519
Uncovered	0.716	0.692	0.670	0.631	0.619	0.673	0.667	0.713	0.669

Source: Own calculations from EPHPM surveys.

Notes: All statistics are weighted. Wages are expressed in real Lempiras. The average real exchange rate for the period is 10 Lempiras per $1 USD.

Table 1.4. Compliance with legal minimum wages

	Large firm wage earners	Small firm wage earners	Public sector workers	Domestic workers	Self-employed	Employers
A. Incidence measures						
Below MW	0.319	0.624	0.095	0.657	0.629	0.265
At MW	0.179	0.108	0.076	0.089	0.064	0.055
Above MW	0.502	0.269	0.830	0.254	0.307	0.680
B. Depth measures						
Shortfall from MW	0.360	0.504	0.408	0.512	0.661	0.544
C. Changes over time						
(i) Share below MW						
Pre (2005-2008)	0.264	0.509	0.072	0.518	0.524	0.148
Post (2009-2012)	0.386	0.739	0.119	0.799	0.717	0.380
Difference	0.122	0.230	0.046	0.281	0.194	0.232
H_0 : Pre = Post	0.015	0.001	0.000	0.000	0.001	0.000
(ii) Shortfall from MW						
Pre (2005-2008)	0.263	0.411	0.309	0.395	0.607	0.467
Post (2009-2012)	0.291	0.498	0.308	0.533	0.642	0.505
Difference	0.028	0.088	-0.002	0.138	0.034	0.037
H_0 : Pre = Post	0.004	0.000	0.962	0.006	0.227	0.085

Source: Own calculations from EPHPM surveys.

Notes: Incidence measures expressed in shares of workers. *Below* includes individuals with wages less than 0.90 of the hourly MW; *at* counts those earning between [0.90,1.10] of the hourly MW, and *above* refers to those earning more than 1.10 times the hourly MW. The depth of non-compliance is calculated as the shortfall indicator (Bhorat et al., 2013a), which measures how far actual wages are from minimum wages. Changes in Share Below MW over time are calculated by regression. Differences in the Shortfall from MW are estimated by block bootstrap with 100 replications. Reported p-values are drawn from t-tests where the null hypothesis is that non-compliance rates and depth are unchanged over time.

Table 1.5. Tests for partial compliance with legal minimum wages

	Wage earners (T) and Public sector (C)	Large firms (T) and Public sector (C)	Small firms (T) and Public sector (C)
Panel A: Pre/Post			
Post × T	0.123	0.096	0.132
	(0.031)***	(0.025)***	(0.066)*
R^2	0.293	0.186	0.354
N	143,095	82,029	82,492
Panel B: By year			
2006×T	0.027	0.016	0.050
	(0.018)	(0.028)	(0.011)***
2007×T	0.022	0.029	0.015
	(0.032)	(0.046)	(0.027)
2008×T	-0.019	0.000	-0.042
	(0.043)	(0.053)	(0.046)
2009×T	0.101	0.068	0.119
	(0.033)***	(0.034)*	(0.068)*
2010×T	0.099	0.084	0.101
	(0.044)**	(0.046)*	(0.080)
2011×T	0.148	0.144	0.134
	(0.058)**	(0.051)**	(0.109)
2012×T	0.177	0.146	0.174
	(0.068)**	(0.060)**	(0.116)
R^2	0.293	0.186	0.355
N	143,095	82,029	82,492
Mean non-compliance (2005-2008)	0.387	0.264	0.509

Source: Own calculations from EPHPM surveys.
Notes: Each panel and column presents a separate regression. Clustered standard errors by industry firm-size categories in parentheses. Covariates include a constant, a dummy for males, married, years of education, potential experience and its square (in years), a dummy variable for urban residence, the logarithm of industry-level IMAE index (by month) and value added (by year). All regressions are weighted.
*** Significant at 1 percent, ** 5 percent, * 10 percent.

Table 1.6. Net effects of legal minimum wages on labor market outcomes

		Sector		Occupation					
	Full Sample	Covered	Uncovered	Wage earners	Public sector	Domestic workers	Self-employed	Unpaid workers	Employers
A. Overall employment									
(1) Probit	-0.085								
	(0.023)***								
(2) Probit	-0.108								
	(0.008)***								
Mean employment rate	0.971								
Observations	327,764								
B. Labor force composition									
(1) Multinomial Logit		-0.363	0.274	-0.272	0.002	0.000	0.259	0.026	0.000
		(0.102)***	(0.092)***	(0.109)**	(0.000)***	(0.000)***	(0.076)***	(0.012)**	(0.004)
(2) Multinomial Logit		-0.446	0.338	-0.311	0.000	0.000	0.368	0.029	0.004
		(0.094)***	(0.098)***	(0.098)***	(0.000)***	(0.000)	(0.103)***	(0.014)**	(0.005)
Mean employment share		0.462	0.509	0.371	0.064	0.026	0.392	0.093	0.024
Observations		327,764		327,764					
C. Log Hours per week									
(1) OLS	-0.411	-0.173	-0.053	-0.088	-0.259	-0.410	-0.040	0.019	-0.232
	(0.166)**	(0.067)**	(0.088)	(0.026)***	(0.012)***	(0.003)***	(0.093)	(0.108)	(0.108)**
(2) OLS	-0.480	-0.194	0.008	-0.100	-0.267	-0.412	0.004	0.116	-0.181
	(0.177)**	(0.072)**	(0.096)	(0.028)***	(0.005)***	(0.002)***	(0.093)	(0.129)	(0.153)
Observations	305,441	153,695	151,746	123,173	21,797	8,725	116,955	26,930	7,861
D. Log Hourly Wages									
(1) OLS	0.136	0.244	-0.691	0.142	0.148	0.073	-0.717		0.113
	(0.161)	(0.087)**	(0.324)**	(0.040)***	(0.032)***	(0.004)***	(0.335)**		(0.188)
(2) OLS	0.205	0.286	-0.518	0.170	0.157	0.074	-0.548		0.044
	(0.175)	(0.097)***	(0.172)***	(0.048)***	(0.021)***	(0.005)***	(0.170)***		(0.243)
Observations	261,004	151,769	109,235	121,669	21,426	8,674	102,172		7,063

Source: Own calculations from EPHPM surveys.

Notes: Clustered standard errors by industry (Panels A and B) and industry firm-size categories (Panels C and D) in parentheses. Panels A, C, and D present separate regressions. Estimates in columns 2-3 and 4-10 of Panel B are from two separate Multinomial Logit regressions where the base category is unemployment. Covariates include a constant, a dummy for males, married, years of education, potential experience and its square (in years), a dummy variable for urban residence, the logarithm of industry-level IMAE index (by month) and value added (by year). Specification (1) controls for industry (or industry firm-size) and survey wave fixed effects. Specification (2) also includes linear category time trends. Coefficients for Probit and Multinomial Logit are marginal effects with all other covariates at their mean. All regressions are weighted.

*** Significant at 1 percent, ** 5 percent, * 10 percent.

Table 1.7. Net effects of legal minimum wages on poverty

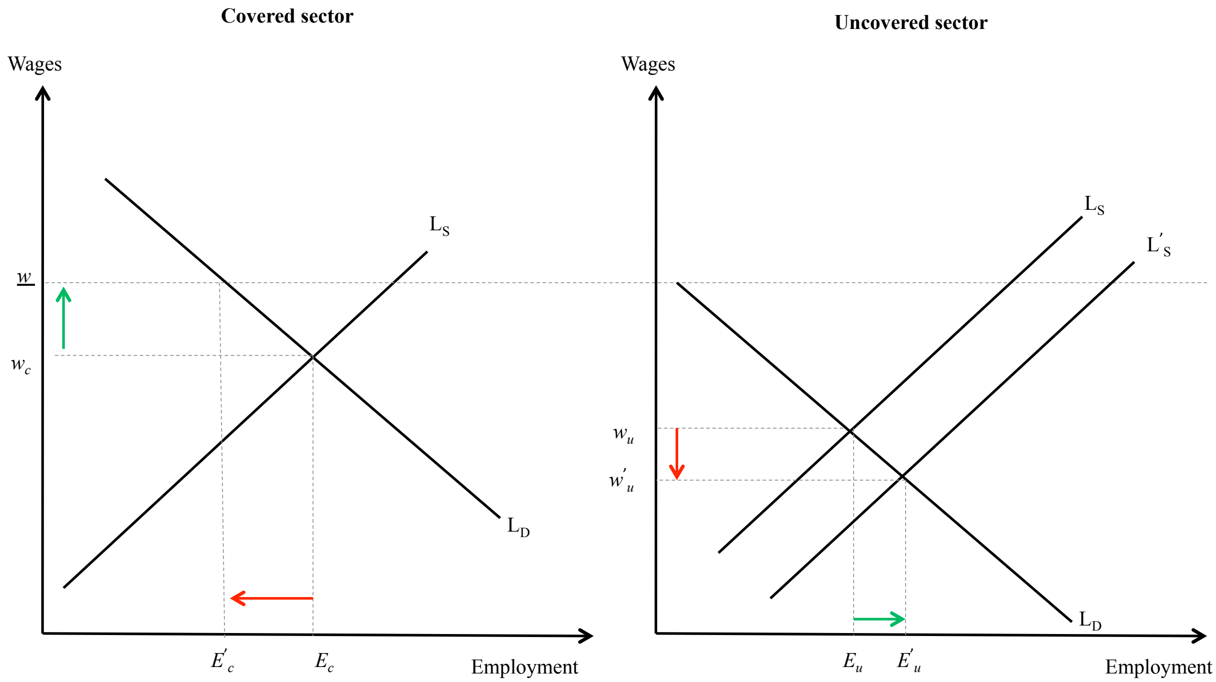
		Sector		Occupation					
	Full Sample	Covered	Uncovered	Wage earners	Public sector	Domestic workers	Self-employed	Unpaid workers	Employers
A. Extreme Poverty									
(1) Probit	0.051	-0.014	0.229	-0.015	0.062	0.307	0.227	0.159	0.201
	(0.025)**	(0.021)	(0.041)***	(0.023)	(0.003)***	(0.003)***	(0.051)***	(0.044)***	(0.042)***
(2) Probit	0.042	-0.013	0.089	-0.015	0.064	0.307	0.093	0.039	0.129
	(0.038)	(0.027)	(0.037)**	(0.033)	(0.003)***	(0.002)***	(0.039)**	(0.022)*	(0.080)
Mean poverty rate	0.459	0.368	0.554	0.404	0.115	0.424	0.561	0.628	0.202
Observations	313,852	165,035	148,817	133,156	23,137	9,354	115,674	24,643	7,817
B. Poverty									
(1) Probit	0.057	0.026	0.147	-0.002	0.106	0.383	0.151	0.078	0.086
	(0.031)*	(0.032)	(0.035)***	(0.015)	(0.005)***	(0.001)***	(0.039)***	(0.057)	(0.037)**
(2) Probit	0.058	0.035	0.044	0.007	0.111	0.384	0.060	-0.013	-0.037
	(0.037)	(0.033)	(0.023)*	(0.021)	(0.003)***	(0.001)***	(0.027)**	(0.028)	(0.071)
Mean poverty rate	0.680	0.624	0.738	0.667	0.337	0.663	0.750	0.778	0.392
Observations	313,852	165,035	148,817	133,156	23,137	9,360	115,674	24,651	7,866

Source: Own calculations from EPHPM surveys.

Notes: Each column corresponds to a separate regression. Clustered standard errors by industry categories in parentheses. Covariates include a constant, a dummy for males, married, years of education, potential experience and its square (in years), a dummy variable for urban residence, the logarithm of industry-level IMAE index (by month) and value added (by year). Specification (1) controls for industry (or industry firm-size) and survey wave fixed effects. Specification (2) also includes linear category time trends. Coefficients for Probit and Multinomial Logit are marginal effects with all other covariates at their mean. All regressions are weighted.

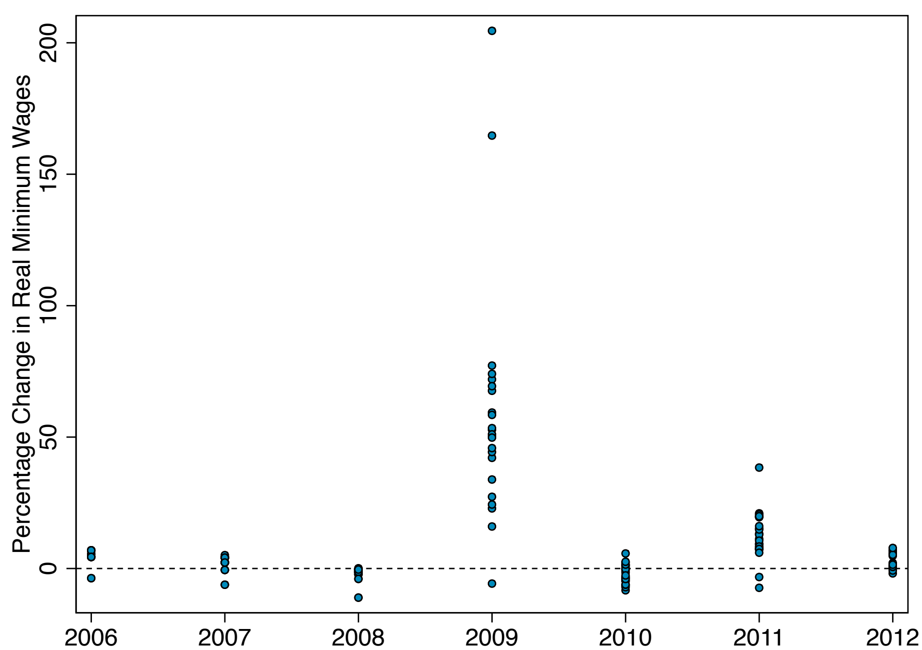
*** Significant at 1 percent, ** 5 percent, * 10 percent.

Figure 1.1. Predicted effects of a minimum wage increase in a dual labor market



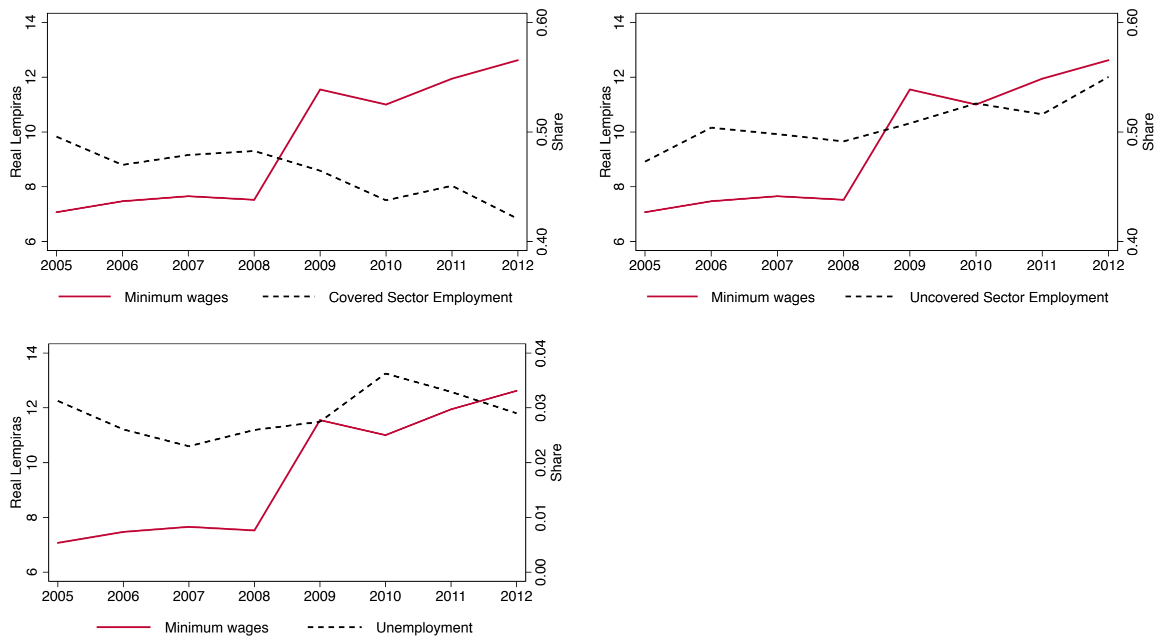
Source: Own elaboration from Harris and Todaro (1970) and Boeri et al. (2011).

Figure 1.2. Variation in real minimum wages by year



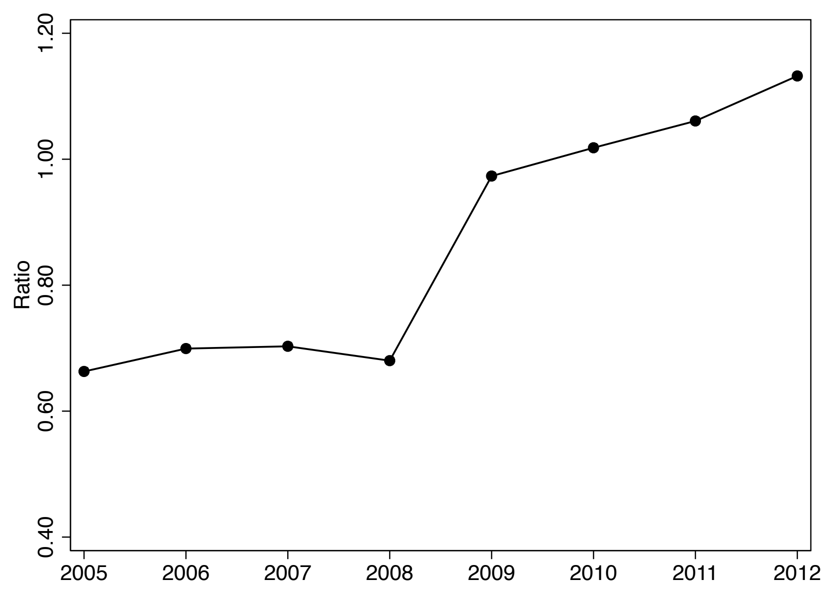
Source: Own calculations from Honduran minimum wage decrees.

Figure 1.3. Minimum wage and labor force composition trends, 2005-2012



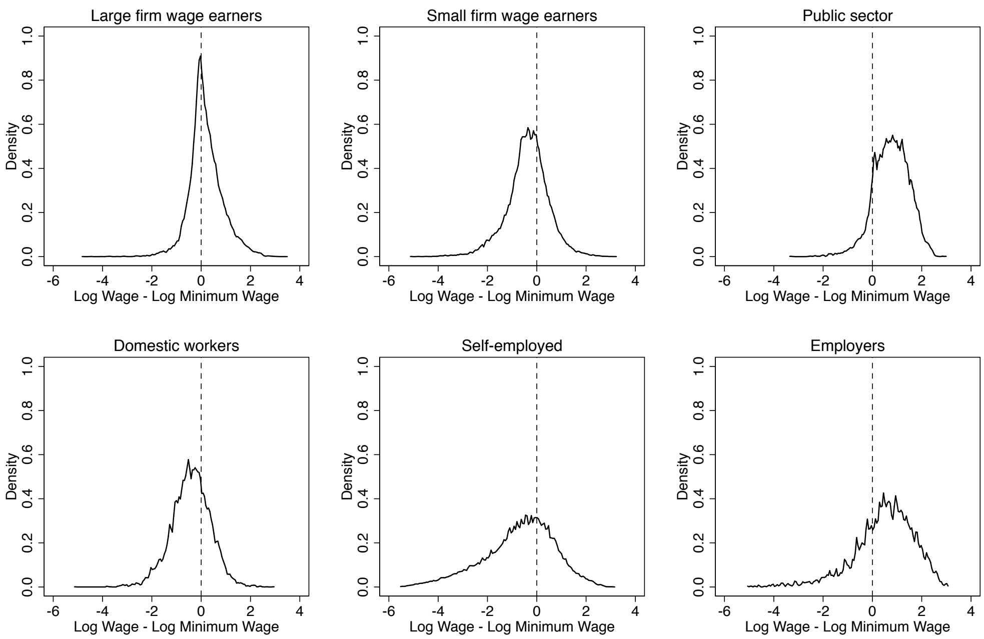
Source: Own calculations from EPHPM surveys.

Figure 1.4. Ratio of minimum wages to mean covered sector wages, 2005-2012



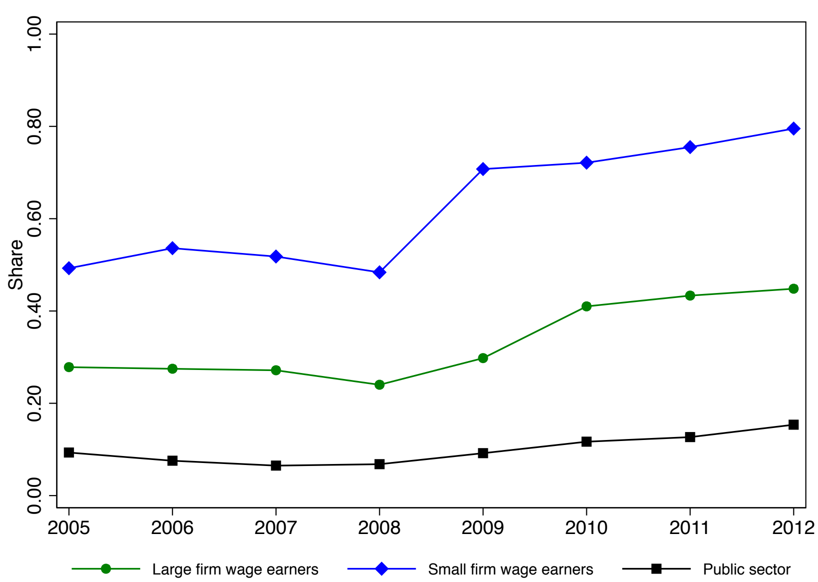
Source: Own calculations from EPHPM surveys.

Figure 1.5. Kernel densities of log wages minus log minimum wages



Source: Own calculations from EPHPM surveys.
Notes: These densities are average distributions from 2005-2012 and are centered so that $MW = 0$.

Figure 1.6. Non-compliance rates for large, small, and public workers, 2005-2012



Source: Own calculations from EPHPM surveys.

Chapter 2

Information Policies and Higher Education Choices: Experimental Evidence from Colombia[1]

with Leonardo Bonilla and Nicolas L. Bottan

2.1 Introduction

Motivated by the increasing demand for higher human capital in the labor market, governments (and in some cases, non-profits) in developing countries have created Labor Market Observatories. These institutions are tasked with improving the information available to high school students deciding whether to pursue higher education and for universities determining what programs to offer. They typically compile and publish statistics on average starting wages, higher education supply, and employment rates for recent graduates by colleges, degrees and fields.[2] Despite the

[1]We would like to thank the Department of Economics at the University of Illinois for financial support. Special thanks are due to Richard Akresh and Martin Perry for their guidance, advice, and encouragement. We are grateful to the Secretary of Education of Bogotá for its interest in this project and authorization to visit the schools, to ICFES and the Ministry of Education for the exit exam data and enrollment data sets, and to the field work team. Earlier versions benefited from useful comments by Geoffrey Hewings, Dan Bernhardt, Adam Osman, Rebecca Thornton, Marieke Kleemans, Seema Jayachandran, Paul Glewwe, Walter McMahon, Julian Cristia, Felipe Barrera-Osorio, Francisco Gallego, Oscar Mitnik, Alejandro Ganimian, Guillermo Cruces, Cristian Pop-Eleches, and participants at various seminars and conferences. This project was reviewed and approved in advance by the Institutional Review Board for the protection of human subjects of the University of Illinois at Urbana-Champaign (IRB #13570). The approval letter is in the Supplementary material for Chapter 2. All remaining errors and omissions are our sole responsibility.

[2]These data are typically constructed from matching college graduates to social security data.

recent interest and allocated resources in implementing these online information systems, there remains relatively little evidence on the role they play on high school students' college decisions.

In this paper we explore the extent to which this information influences students' knowledge and beliefs about college, test scores, and higher education decisions. For this purpose, we conduct a randomized controlled trial among senior high school students in 115 public schools in Bogotá, Colombia, the country with the longest running Labor Observatory in Latin America.

A month after the beginning of the 2013 school year, 11th grade students in 58 schools were given a 35-minute presentation delivered by young Colombian college graduates along with a handout. The presentation covered three main topics: i) showing average statistics on the earning premiums associated with graduating from college and mean salary differences between selected colleges, degrees, and fields (also introducing the Labor Observatory website for students to visit on their own), ii) the availability of student loan programs for financing higher education, and iii) the importance of exit exam scores for college admission and obtaining financial aid. We collect survey data that measures students' knowledge of information and funding programs, as well as their beliefs on average earnings for different levels of education. The experimental data are then matched to administrative records that contain high school exit exam test scores and college enrollment information (i.e. whether the student enrolled and their institution, degree, and field).

At baseline, our sample is mostly unaware of the existence of the Labor Observatory, funding programs, and is misinformed about the average earnings of college graduates. Students overestimate the returns to college by almost 100%, consistent with previous evidence (Gamboa and Rodríguez, 2014). The average effects of the information treatment on knowledge and earning beliefs are modest. Student awareness of the Labor Observatory and a city-wide funding program

(FESBO) are unaffected. The treatment does increase their familiarity with ICETEX, the largest funding program in Colombia, by around 6.6%. We also find that the intervention does not significantly change earning beliefs, a result that is robust to different definitions of student perceptions.

Test scores and college enrollment rates show virtually no change, consistent with previous studies that assess similar programs. We do find that students who enroll attend more prestigious colleges because of the intervention. Though small in magnitude (between 0.5-0.6 percentage points), this effect is economically significant and robust. It represents an increase of approximately 50% with respect to the control group's average. This result indicates that providing information nudges some students to more selective colleges, achieving comparable effects to more personalized and expensive information programs (Hastings et al., 2015, Busso et al., 2016).

Our estimates for the average effects of information on these outcomes are correctly sized, since we have enough statistical power to detect small effects on knowledge, beliefs, test scores, and higher education choices. The experimental design is comparable to previous studies that evaluate information policies. We also allow for the possibility that our findings may be driven by chance rather than the treatment, unlike most other papers. We adjust all reported p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group, following Aker et al. (2012). Therefore, we are confident that our design is correct and estimates do not capture spurious correlations or may be driven by specification choice.

We then test for suggestive evidence that some students may benefit from Labor Observatory information more than others. Selected attributes include gender, family income, direction of error in baseline beliefs (underestimating or overestimating), students' perceived academic ranking, perceived self-efficacy, risk aversion, and perceived likelihood of college enrollment. Results consistently show no indication of heterogeneous impact of information across these dimensions.

These results may be interpreted as evidence that campaigns which employ data from online information systems are unable to affect knowledge and beliefs, test scores, and higher education choices for low-income students in a developing country. Since this is the objective of Labor Market Observatories, their ability to motivate educational investment is perhaps overestimated.

Our study is part of a rapidly growing literature that explores how low-cost information treatments affect educational choices. Given the success of these programs in primary schools (Nguyen, 2008, Jensen, 2010), the literature has proceeded to study their effectiveness at keeping students in high school (Dinkelman and Martínez, 2014, Avitabile and De Hoyos Navarro, 2015) and motivating college enrollment (Booij et al., 2012, Loyalka et al., 2013, Oreopoulos and Petronijevic, 2013, Hastings et al., 2015, Pekkala-Kerr et al., 2015, Busso et al., 2016, Fryer Jr., 2016, McGuigan et al., 2016, Rao, 2016). We contribute to a relevant policy discussion because governments are currently devoting resources to maintaining online information systems and relying on dissemination campaigns due to their documented effectiveness and low cost. However, these programs vary in their capacity to increase enrollment rates for low-income students at secondary and higher educational levels, as this paper and most of the related literature shows.

The results in this study also address concerns about unequal access to college education. Despite the significant returns to higher education, enrollment among disadvantaged students remains low (McMahon, 2009). Three different policies are often proposed to reduce this inequity: alleviating credit constraints (Manski, 1992, Kane, 1994, Ellwood and Kane, 2000, Solis, 2013), increasing financial aid take-up among the poor (Bettinger et al., 2012, Dynarski and Scott-Clayton, 2013), and eliminating the gap between expected and actual returns to college (Manski, 1993a).

In our analysis, we test how the latter of these initiatives affects decisions by low-income students and compare our results to existing evidence on the other two policies in Colombia.

The remainder of this chapter is organized as follows. Section 2.2 provides background on Colombia's higher education system. Section 2.3 outlines the experimental framework and describes the informational intervention based on Labor Observatory data. Section 2.4 presents the data and empirical strategy. Section 2.5 reports average and heterogeneous impacts of the intervention. Section 2.6 concludes by discussing our findings and potential directions for future research.

2.2 Higher education in Colombia

The Colombian higher education system consists of many institutions offering different degrees in multiple fields. In total, there are 327 colleges in the country, with 132 located in the Bogotá metropolitan area. These institutions differ in the degrees they grant, their administration, and prestige. They offer vocational (2-year) and academic (4-year) degrees across 55 different fields of study. Vocational degrees are granted in 92 technical/technological institutes, while 40 universities supply most of the academic programs. There are 23 public and 109 private institutions. Six of the top-10 universities in Colombia are located in Bogotá.[3]

Each institution has its own admissions criteria. Most colleges use a merit-based system contingent on educational performance and a minimum test score on the SABER 11 high school exit exam, but thresholds vary widely across institutions. While not always required to graduate from high school, the majority of students take the exit exam at least once.[4] However, it is a requirement

[3]The best universities in Colombia are ranked based on their students' average performance on university exit exams, the SABER PRO. In 2012, six institutions in Bogotá were in the top-10: *Universidad de los Andes*, *Universidad Nacional*, *Universidad del Rosario*, *Universidad Externado*, *Universidad de la Sabana*, and *Pontificia Universidad Javeriana*. All institutions are private, except for the *Universidad Nacional*.

[4]Students are allowed to retake the SABER 11 exam for a fee of US$21.

for college entry and as described below, a determinant factor for financial aid applications.

Higher education in Colombia is not free, and tuition costs are markedly different across colleges. On the one hand, students in public institutions pay tuition under a progressive system based on family income. Costs per semester in public colleges can be as low as 0.1 minimum wages[5], about $29 (or $58 per year). On the other hand, tuition costs at top-tier private universities may rise to 13.2 minimum wages or $3,800 per semester ($7,600 a year).

Two funding sources are available to college students in Bogotá. The Colombian Public Student Loans Institution (ICETEX) runs the largest student loan program in the country. It funds vocational, academic, and postgraduate studies in Colombia and abroad. Approximately 22% of college students in Bogotá received funding from this source during 2013. Recent reforms have introduced zero-interest loans for low-income students at great success (Melguizo et al., 2016). Bogotá's Secretary of Education offers a second funding option for low-income students educated in the city's public schools: the Fund for Higher Education of Bogotá (FESBO). This fund has two financing options. The first targets high achieving students and offers loans for careers in any college, degree, and field. The second provides non-targeted loans to pursue vocational degrees. In both cases a fraction of the debt can be forgiven upon degree completion.

Students must fulfill several requirements in order to obtain a student loan. These include Colombian citizenship, voluntarily providing their socioeconomic information, having an admittance letter from an accredited college, and obtaining a minimum score on the SABER 11 high school exit exam.[6] However, perhaps the most important requirement is that all loans must be

[5]Hereafter, we express monetary variables in monthly minimum wages, a commonly used measure in Colombia. The 2013 monthly minimum wage was 535,600 Colombian Pesos (roughly 288 US dollars).
[6]Specific requirements on test scores have changed over time, since ICETEX offers different loan types. See http://www.icetex.gov.co/dnnpro5/en-us/cr%C3%A9ditoeducativo/pregrado.aspx for more information.

backed by an approved co-debtor. This restriction is particularly binding for low-income families because proposed co-debtors must pass a credit check and show evidence of financial capacity to repay the full cost of the loan. In this sense, the Colombian system is less flexible compared to Chile, where the government often backs student loans.[7]

The average benefits of higher education in the labor market are also heterogeneous. Figure 2.1 plots mean monthly earnings for college graduates in their first three years as well as the interquartile range of these salaries (25th and 75th percentile). Differences in average salaries and their spread are sizable across colleges, degrees, and fields. For example, mean earnings for recent graduates from public institutions are 2 minimum wages versus 2.9 minimum wages for private college graduates. The interquartile range shows that private college graduates in the 25th percentile earn more than public institution graduates at the 75th percentile. On average, earnings are higher for individuals with academic degrees, from top-10 institutions, and whose field is classified as Science, Technology, Engineering, and Mathematics (STEM).[8] Earnings inequality is substantial when comparing salaries between prestigious colleges and fields.

Given this heterogeneity in the Colombian higher education system, the government started the Labor Observatory for Education in 2005 (http://www.graduadoscolombia.edu.co/). Its mission is to "provide valuable information about the relevance of educational investment and help students make higher education decisions". It is the longest running labor observatory in Latin America, predating similar initiatives in Mexico and Chile. The observatory provides statistics on average starting wages for college graduates, how long it takes them to gain employment, and paints a

[7]See González-Velosa et al. (2015) for a detailed comparison of higher education systems in Chile and Colombia.
[8]STEM fields include agronomy, animal sciences, veterinary medicine, medicine, bacteriology, biology, physics, mathematics, chemistry, geology, business, accounting, economics, and all engineering programs.

picture of labor demand patterns across fields and regions. We will study students' awareness of the Labor Observatory and whether an informational intervention that uses data from this source affects their beliefs, test scores, and higher education choices.

2.3 Experimental setting

In order to answer our research question, we conduct a randomized control trial in Bogotá. Public school students enrolled in their senior year of high school (11th grade) are our population of interest. Public schools face a disadvantage in higher education access relative to private schools (see Supplementary material for Chapter 2 Table B.1). Most students in the public schooling system come from low socioeconomic status and perform consistently worse on the SABER 11 exit exam, and therefore have lower enrollment rates; especially in academic programs, prestigious colleges, and STEM fields.

Using administrative data, we select a representative sample of 120 public school-shifts out of the 570 that offer an academic track.[9] These institutions are all mixed-sex high schools with at least 20 students enrolled in eleventh grade the year before our intervention. Half of the selected schools are randomly assigned to receive an informational talk detailing earning premiums by college, degree, and field while also discussing funding opportunities based on data from the Labor Observatory. The remaining institutions serve as our comparison group. Despite numerous attempts, we were unable to visit five schools, yielding a final sample of 115 schools. Schools we could not reach corresponded to 3 treatment and 2 control schools. The inability to interview

[9]Most public high schools in Bogotá have two shifts: morning and afternoon. Each shift has different students and most importantly, different teachers and staff. Hence, each school-shift may be considered as an independent educational institution. In what follows, we refer to school-shifts as schools.

students at these schools does not affect the validity of our randomization procedure as our balance tests will show. Overall, schools in our sample cover almost all neighborhoods in Bogotá, with treatment and control schools relatively spread out over the city (see Supplementary material for Chapter 2 Figure B.1).

Because selected schools vary in size, we decided to interview at most two classrooms in each visit. In schools with only one or two classrooms of seniors, all eleventh grade students were interviewed. In larger schools, we randomly selected two grade 11 classrooms to take part in our study. Our sample consists of all students present in school on the days of our visits.

The timing of our intervention is summarized in Figure 2.2. In line with the academic cycle for public institutions, which begins in February and ends in December, we arranged our visits at certain key points over the 2013 school year. Baseline fieldwork and the intervention took place during March, about a month after the beginning of the schooling cycle. We carried out one follow up visit in August, just before students took the high school exit exam. Using these scores, students apply to college between September and December. We use administrative data to measure test scores and enrollment outcomes, which are described in the next section.

Baseline visits to all schools involved students responding a survey designed for this study. After the surveys were collected, visits concluded in control schools. Students in treatment schools were given a 35-minute presentation delivered by young Colombian college graduates.[10] The presentation covered three main topics: i) showing average statistics on the earning premiums associated with graduating from college and mean salary differences between selected colleges, degrees, and fields (while introducing websites where students could find this information on their

[10]We opted for local college graduates based on findings in Nguyen (2008), where information provided by role models is shown to be more effective in comparison to researchers.

own), ii) the availability of student loan programs for financing higher education, and iii) the importance of exit exam scores for college admission and obtaining financial aid.

The first topic began by showing statistics on the average monthly earnings for individuals with incomplete (0.9 minimum wages) and complete secondary (1.1 minimum wages) in graphical form. These values were then compared to the mean monthly salary for individuals who have completed higher education, differentiating by vocational (2 minimum wages) and academic degrees (2.9 minimum wages).[11] Next, we introduced students to two websites where they could find very detailed information on the labor market outcomes of college graduates: the Labor Observatory and *Finanzas Personales*.[12] Using these resources, students were shown how to find information on average wages by means of examples (e.g. geographer v. geologist at the same institution and medicine at different universities), the supply of degrees and fields across institutions, and the average employment probabilities by college, degree, and field.

The second part of the talk focused on the two main funding programs available to students in Bogotá: ICETEX and FESBO. For each program, we provided basic information regarding benefits, application requirements, and deadlines. Students were encouraged to visit each program's website to collect more information on their own. We emphasized the fact that college education can be affordable, even if they choose a relatively expensive university.

In the final portion of the talk, we highlighted the importance of good performance on the SABER 11 high school exit exam. As mentioned in the previous section, this test is a determinant factor for admission in most colleges and minimum scores are required to obtain funding. Students

[11] Reference earnings for incomplete and complete secondary were estimated from household survey data for 2011.

[12] This site is maintained by *Semana* publications, one of the leading media groups in Colombia. Its information system is based on data from the Labor Observatory presented in a user-friendly way. The page is located at http://www.finanzaspersonales.com.co/calculadoras/articulo/salarios-profesion-para-graduados/45541.

were allowed time for questions and were given a one-page handout summarizing the main points of the talk, which also provided links to all websites mentioned during the presentation.[13]

2.4 Data and estimation strategy

2.4.1 Data

We employ two sources of data in our analysis: surveys and administrative records. Students in selected schools answered a baseline and follow up questionnaire. The baseline survey was completed by 6,601 students, and inquired about demographics, family background, socioeconomic status, educational history, knowledge and beliefs about the higher education system, aspirations, and attitudes towards risk. The follow up survey was completed by 5,503 students in the same schools. It mainly followed up on baseline questions about knowledge, beliefs, and aspirations. Given the lower response rates at follow up, we test for evidence of selective attrition. There is no indication that baseline and follow up respondents differ by treatment status (Supplementary material for Chapter 2 Table B.2).[14] Observed attrition is likely due to absences on the day of our second visit since we are able to match most of the baseline sample to administrative data collected at the end of the year.

Administrative records are matched to the experimental sample to measure exit exam performance and college enrollment after the intervention. First, we use data from the Colombian Institute for the Promotion of Higher Education (ICFES). These data contain students' raw scores on the high school exit exam across eight different subject areas and their overall performance.

[13]The original and translated copy of this handout may be found in the Supplementary material for Chapter 2.

[14]In unreported results, we also compared observable characteristics across respondents and non-respondents and found little difference between both samples.

They also gather information on demographics, family background, and socioeconomic attributes. When constructing individual and household-level controls we use administrative data, replacing any missing information from our surveys. The matching rate with ICFES data is approximately 95.7%, and shows no significant differences across treatment and control groups (column 2 of Table B.2). Second, enrollment information is provided by the National Information System for Higher Education (SNIES). These data help us track students in our sample who enrolled in college, and identify their institution, degree, and field. About 95.4% of the experimental sample with valid test scores are matched to SNIES data (column 3 of Table B.2).

Given our objective lies in exploring whether an information treatment using Labor Observatory data affects knowledge and beliefs, test scores, and higher education choices; we focus on three sets of variables:

1. *Knowledge and beliefs*. In both baseline and follow up surveys, we asked students to indicate whether they were familiar with the Labor Observatory website and available funding programs (ICETEX and FESBO). Their answers were in Yes/No form, and we construct dummy variables equal to one if Yes and zero otherwise.

   Next, we elicited beliefs about expected earnings across different levels of education. Specifically, we asked: "How much do you think an average individual who recently began to work earns per month (in minimum wages) in each of the following situations? a) completed high school but does not go to college, b) completes a vocational degree, and c) completes an academic degree." The options range between 1 to 10 or more minimum wages. Using the responses, we construct *perceived earning errors* for vocational and academic degrees as the percentage error in beliefs relative to earning estimates from household surveys. These measures are similar to the ones used in Hastings et al. (2015).

2. *Test scores*. We examine the effect of our intervention on students' overall, math, and language scores (the two most heavily weighted fields).[15] Since our matched data contains raw scores, these values are standardized to have mean zero and standard deviation of 1 with respect to the control group for ease of interpretation.

3. *Higher education choices*. Because the matched data contains information on observed career choices, we first measure enrollment with a dummy variable equal to one if a student enrolled in any higher education program and zero otherwise (*College Enrollment*). Then, we take advantage of data on the institution, degree, and field of enrollment. We define four outcomes: a dummy variable that indicates whether the student is pursuing an *Academic Degree*, a dummy variable that equals one if enrolled in a *Private College*, a dummy variable that equals one if enrolled in a *Top-10 College*, and a dummy variable that equals one if their program of study is classified as a *STEM field*.

Table 2.1 presents baseline statistics for knowledge and belief outcomes, student attributes, and school characteristics across treatment and control schools. The final column presents p-values for the hypothesis that means are equal across groups, which are estimated by regression with clustered standard errors at the school-level. Both groups are statistically identical before the intervention, indicating that our randomization was successful. A joint test for the significance of

[15] The official weights are: mathematics (3), language (3), social sciences (2), biology (1), physics (1), chemistry (1) and philosophy (1).

student and school variables on the likelihood of attending a treatment school indicates that they are uncorrelated (p-value of 0.239).

Individuals in our sample are almost 18 years old and about 47.3% are male. Students mainly come from low socioeconomic status: only 16.5% of their parents have completed college and 68% report that family income lies below 2 minimum wages ($576 per month). About 17% are employed while attending high school. To measure academic self-concept, we asked students to rank their academic performance relative to their peers on a Likert-scale from 1-10 where the latter is the highest value. As a measure of self-efficacy, students rated how often they achieved their goals (from 1 to 10, where 1 is never and 10 is always). Individuals above the median response are classified as high academic self-concept and self-efficacy, while those below constitute the low group. Given that risk aversion has been found to play an important role in educational decisions, students were asked to play a game at baseline.[16] The resulting classification indicates that 85% of our sample is risk averse. We also asked students' perceived probability of college enrollment. Over 84% reported in the baseline survey that they were likely to enroll.

2.4.2 Estimation strategy

Given the random assignment of treatment status, we quantify the effect of providing Labor Observatory information to public school students in Bogotá using cross-sectional or difference-in-difference regressions depending on whether outcomes are observed once or twice.

For cross-sectional outcomes that are only observed after the intervention we estimate:

[16]Students face the following hypothetical scenario: They were just hired for a new short-term job and can choose between a fixed salary or a lottery in which earnings are determined by a coin flip. By varying the optimistic scenario payment, we classify students in a scale from 1 to 4 where 1 is extremely risk averse and 4 is risk loving. We consider a student risk averse if they are classified 1 or 2.

$$y_{is,t=1} = \alpha + \beta T_s + \theta X_{is,t=0} + u_{is,t=1} \tag{2.1}$$

where $y_{is,t=1}$ is the outcome for student i attending school s at the follow up, $t = 1$. We include an intercept, α, and control for baseline student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on the exit exam in previous years, whether the school has a computer lab, shift indicators, and school size), and neighborhood fixed effects in $X_{is,t=0}$. Given that take-up depends on the level of attention placed by students, β captures the intent-to-treat effect of the informational intervention. $u_{is,t+1}$ is a mean-zero error term assumed to be uncorrelated with the treatment indicator since it was randomly assigned. Equation (2.1) is estimated by Ordinary Least Squares (OLS)[17] with clustered standard errors at the school-level.

For outcomes available at both baseline and follow up, we employ two specifications. First, we estimate Equation (2.1), but include the baseline outcome as a control. This ANCOVA approach may provide additional power when autocorrelation in outcomes is low (McKenzie, 2012). Second, we estimate a difference-in-difference specification with student-level fixed effects:

$$y_{ist} = \alpha Post + \beta(T_s \times Post) + \mu_i + u_{ist} \tag{2.2}$$

where $Post$ is a dummy variable that equals one after information exposure and zero otherwise. α estimates changes in the outcome over time and μ_i is a student-specific effect that controls for time-invariant characteristics. Again, β is our coefficient of interest, which measures the intent-to-treat effect of the information treatment. Standard errors are clustered at the school-level.

[17]We also estimate Probit regressions. Because results are largely unchanged, only OLS estimates are shown.

Given that we are testing whether the intervention affected multiple outcomes, there is the possibility that our findings may be driven by chance rather than by the treatment. For all our estimates we adjust the resulting p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group, following Aker et al. (2012).[18] We distinguish between three groups of outcomes when calculating adjusted p-values: knowledge and beliefs (5 outcomes), test scores (3 outcomes), and higher education choices (5 outcomes).

2.5 Results

2.5.1 Average effects

Descriptive statistics for knowledge and beliefs are presented in Table 2.1. Only 7.7% of students are aware of the existence of the Labor Observatory. Funding programs are slightly better known. Almost 70% of students express familiarity with ICETEX and 17.5% with FESBO. Students in our sample tend to overestimate college earnings. They believe average monthly earnings for vocational and academic graduates exceed observed salaries by about 64% and 95%, respectively. Figure 2.3 plots the distribution of these errors to study belief dispersion. Individual perceptions are not far from actual vocational earnings, with 76.2% reporting beliefs within one standard deviation of the true salary. Beliefs for academic degrees are more disperse: 43.1% of students are within one standard deviation, 46.2% between one and three standard deviations, and 10.7% more than three standard deviations. Knowledge and belief outcomes are balanced at baseline. These results are consistent with previous evidence for Colombia (Gamboa and Rodríguez, 2014) and other countries (Pekkala-Kerr et al., 2015, Hastings et al., 2015, McGuigan et al., 2016).

[18]The adjusted p-value for the k-th variable in a group of K different outcomes is given by $1-(1-p_k)^{K^{1-r_{-k}}}$ where p is the usual p-value and r_{-k} is the average correlation among all other outcomes excluding k.

The effects of the information treatment on knowledge and beliefs are reported in Table 2.2. Panel A reports ANCOVA regressions and Panel B presents difference-in-difference estimates with individual fixed-effects. On average, student awareness of the Labor Observatory was unchanged by the informational talk. Results are robust to specification choice and are further confirmed by adjusting p-values for multiple hypothesis testing. The intervention does have a positive and significant effect on student knowledge of the largest funding program, ICETEX. Average awareness increases by at least 4.6 percentage points, or 6.6% of the baseline mean. This estimate remains significant even after correcting for multiple testing, with p-values between 0.009 and 0.051. The evidence also indicates that knowledge of FESBO was unaffected.

Results show that earning beliefs are unchanged for the sample. ANCOVA estimates are close to zero and insignificant while difference-in-difference coefficients are slightly positive but also not significant at any conventional level. In fact, adjusted p-values are close to 1, indicating that the information treatment does not change students' expectations about the average earnings for college graduates. Perhaps students are not considering an average individual as a reference point but themselves. To explore this possibility, we compare reported beliefs with salaries for students' aspired careers. In the survey, they were asked to list their ideal college, degree, and field. Using this alternate comparison point, we find similar results (see Supplementary material for Chapter 2 Table B.3). We also estimate effects on aspirations: ideal college, degree, and field, and find no impact (Supplementary material for Chapter 2 Table B.4).

Given these results, we now explore whether the intervention changes test scores and higher education choices. We present estimates from cross-section regressions for students interviewed at baseline that are matched to the administrative records (full sample) and students observed in both in-school surveys who are successfully matched to administrative data (balanced sample).

The SABER 11 exit exam was taken by students in our sample five months after the intervention. We focus on overall performance and scores on the two most heavily weighted subjects: mathematics and language.[19] All scores are standardized with mean zero and standard deviation one with respect to the control group. Columns 1 to 3 of Table 2.3 present the average effects of the treatment on test scores. Overall, there is no evidence that students exposed to Labor Observatory information adjust their effort on the exam. While estimated coefficients are consistently positive for mathematics and only marginally insignificant even when adjusting for multiple hypothesis tests, we conclude that the information intervention had no effects on test scores.

We now study higher education choices. College enrollment rates for our sample are low, since only about 44% attend a higher education institution (including both vocational and academic programs). The majority pursue vocational degrees (34.6%) and the rest choose academic careers (9.6%). Few public school students attend private institutions (15%) and top-10 colleges (1.1%). Only about 5.2% opt for careers in STEM fields. Columns 4 to 8 of Table 2.3 show treatment effect estimates for these outcomes. We find no effect of information on the probability of enrolling in college, which is robust across samples and to multiple hypothesis corrections. Results for degree type and private colleges are slightly positive but insignificant. There is a positive effect on the likelihood of enrolling in a top-10 college. The estimated effects lie between 0.5 and 0.6 percentage points, depending on the sample, and remain statistically significant after adjusting for multiple testing. Though small in magnitude, this impact is economically significant. It represents an increase of approximately 50% with respect to the control group's average. Estimated effects

[19] Estimated treatment effects for other subjects show no effects.

on careers in STEM fields are also positive but not statistically significant.

Our results are consistent with previous literature. Most work analyzing information treatments finds no effect of disclosing information on college enrollment (Booij et al., 2012, Oreopoulos and Dunn, 2013, Pekkala-Kerr et al., 2015, Wiswall and Zafar, 2015, Fryer Jr., 2016, McGuigan et al., 2016). Intensive margin effects on college type are comparable to interventions that focus on students who are already applying to college and have a high probability of enrollment (Hoxby and Turner, 2013, Hastings et al., 2015). Opting for a top-10 college may have large implications on long-run earnings (conditional on graduating). Recall from Figure 2.1 that students who graduate from a top-10 college in Colombia earn significantly more than non-top college students (1 minimum wage more on average). Therefore, while providing information may not lead more individuals to attend college, it does affect what colleges are chosen by those who do enroll.

Overall, an informational nudge using data from an online information system that intends to help students make educational decisions is ineffective on a representative sample of public high school students in a developing country. Our experimental design has enough statistical power to detect small effects (see Supplementary material for Chapter 2 Table B.5) and most of our estimates are fairly precise, so an explanation for the lack of impact lies elsewhere. In particular, we now test whether the informational intervention affects only a small fraction of students and discuss other potential obstacles that limit the success of information policies in the conclusion.

2.5.2 Heterogeneous effects

Even though we find modest effects of providing Labor Observatory information on average, it may be possible that some students benefited more than others from our intervention. Given the richness of our data, we test for differential effects across student and household-level attributes. These include gender, family income, direction of error in baseline beliefs (underestimating or overestimating), students' perceived academic ranking, perceived self-efficacy, risk aversion, and perceived likelihood of college enrollment. These results should be interpreted as suggestive, because the data and experiment were not stratified by these characteristics. Despite this limitation, the analysis may provide further insight into whether and how information programs work.

Depending on the outcomes, we estimate ANCOVA or difference-in-difference regressions that interact a dummy variable for each group with the treatment indicator and all other right hand side variables. This procedure estimates differential effects for the informational intervention but also allows the coefficients on included controls to vary across groups. Regressions are estimated by OLS with clustered standard errors at the school-level. We adjust all p-values for multiple hypothesis testing using the procedure in Aker et al. (2012), also accounting for the fact that we calculate three coefficients: the reference group treatment effect, an interaction, and their sum.

Table 2.4 presents estimates for knowledge and belief outcomes by gender, family income, and direction of perceived errors at baseline. Results for knowledge of the Labor Observatory are mostly insignificant and do not differ across groups. Estimated coefficients on familiarity with ICETEX are significant for boys and students who underestimate college returns at baseline but are not statistically different from effects on girls and overestimators. We find no differential effects on knowledge of FESBO. Effects on beliefs are mostly insignificant, although we note that students who underestimate at baseline seem to adjust their earning expectations upwards. However, these estimates are noisy. In the Appendix, we also report effects by self-perception and risk aversion (see Table B.6). Similar to the above results, only coefficients on knowledge of ICETEX are significant. Students with low perceived academic ranking and high self-efficacy report increased awareness of this funding program. However, we cannot reject that these coefficients are different from the estimates for students with high perceived ranking and low self-efficacy, respectively.

Differential effects for test scores are presented in the first three columns of Table 2.5. Overall, we do not find any heterogeneous impact across gender, income, and baseline error direction. There is some suggestive evidence that the treatment improved language scores for high self-efficacy students, and the difference is significant compared to individuals with low perceived self-efficacy (see Table B.7). The analysis finds no other differences across baseline attributes. We also explore potential heterogeneity across the score distribution in test scores by estimating quantile specifications of our cross-sectional regressions. Figure B.2 in the Appendix shows no evidence of heterogeneous impact across the distribution of test scores.

The remainder of Table 2.5 presents heterogeneous effects for higher education choices. There are no subgroup differences across the selected characteristics, including self-perception and risk aversion (Table B.7). Some coefficients are positive, but most are statistically insignificant. While we found positive and robust average effects on enrollment in a top-10 college, there is no indication from our data that certain students enrolled in these institutions more than others.

This analysis indicates that in addition to modest average effects, the information treatment had no differential impact on students. However, these results are relatively imprecise and only suggestive since data limitations restrict our ability to identify small heterogeneous effects.

2.6 Conclusion

The growth in online information systems by developing country governments has led to an increase in available data on educational and labor market outcomes. These websites are targeted towards high school students in the process of making higher education decisions. This paper evaluates whether an information treatment that uses data from Colombia's Labor Observatory, the longest running college information system in Latin America, affects the decision-making process of public school students in the capital city of Bogotá. We conduct a randomized controlled trial in 115 schools, 58 of which received a 35-minute presentation by young college graduates that discussed the average earning premiums upon completing college, available funding options to cover costs, and the importance of test scores for college admission and obtaining financing.

Using surveys and administrative data, we find modest effects of this informational nudge on individual outcomes. Students do not increase their knowledge of the Labor Observatory or significantly change their beliefs, but do become more familiar with the ICETEX funding program. Test scores and college enrollment are also unaffected, although students who enroll choose more prestigious colleges (top-10 institutions) because of the intervention. We also explore the possibility that some students benefit from information more than others, but find little evidence to suggest that is the case. Our findings are consistent with previous studies that assess similar programs in different settings. We interpret these results as evidence that campaigns to raise awareness for government-maintained information systems are unable to affect knowledge and beliefs, test scores, and higher education choices for low-income students in a developing country.

Given the success of information programs in primary and secondary, our results suggest that these policies are less effective in college. Information is thus unable to raise enrollment at all levels, as has been implied in the literature. Access to higher education is challenging in Colombia, since financial, academic, and information constraints play a role. In our follow up survey, we asked students to list what they believed was the most significant barrier to enroll in college. 64.5% reported that they believed college was unaffordable while 32% claimed that obtaining admission was the largest obstacle. Our results here show that incorrect information is also a problem, but that low cost informational interventions are unable to raise enrollment rates by themselves. This evidence suggests that we should be less optimistic about the ability for online information systems to motivate education. In Colombia, programs that provide funding through zero-interest loans (Melguizo et al., 2016) and merit-based scholarships (Londoño-Vélez et al., 2017) have been highly successful in increasing college enrollment rates in Colombia, especially among poorer students.

However, informational programs have potentially more to offer. Given their low costs, additional investment on personalized interventions may successfully target students who are more likely to benefit. Studies evaluating these initiatives find slightly better results than we report here (Bettinger et al., 2012, Hastings et al., 2015, Avitabile and De Hoyos Navarro, 2015, Busso et al., 2016). Improving informational campaigns for education requires further research. Who should be targeted by information policies? What is the relevant information for higher education decisions? When is the best time to disclose this knowledge? How can we properly ensure and measure information take-up? We are hopeful that the growing literature to which our study contributes will seek answers to these unanswered questions to help governments improve college access among the poor, thereby facilitating upward social and economic mobility.

2.7 Tables and Figures

Table 2.1. Baseline outcomes and characteristics for experimental sample

	Control		Treatment		Difference
	Mean	(SD)	Mean	(SD)	p-value
Knowledge					
Knows Labor Observatory	0.072	(0.258)	0.082	(0.274)	0.200
Knows ICETEX	0.699	(0.459)	0.688	(0.463)	0.646
Knows FESBO	0.181	(0.385)	0.168	(0.374)	0.254
Perceived earning errors					
Vocational	0.656	(0.978)	0.617	(0.939)	0.308
Academic	0.976	(0.834)	0.923	(0.834)	0.120
Student attributes					
Male	0.475	(0.499)	0.472	(0.499)	0.831
Age	17.639	(0.925)	17.663	(0.942)	0.504
Parent completed secondary	0.398	(0.489)	0.392	(0.488)	0.719
Parent completed higher education	0.176	(0.381)	0.155	(0.362)	0.270
Family income (<1 minimum wage)	0.136	(0.343)	0.151	(0.358)	0.289
Family income (1-2 minimum wages)	0.538	(0.499)	0.539	(0.499)	0.941
Family income (>2 minimum wages)	0.320	(0.467)	0.307	(0.461)	0.589
Student works	0.164	(0.370)	0.176	(0.381)	0.352
Perceived high academic ranking	0.424	(0.494)	0.395	(0.489)	0.128
Perceived high self-efficacy	0.350	(0.477)	0.355	(0.479)	0.749
Risk averse	0.857	(0.350)	0.845	(0.362)	0.374
Perceived in likelihood of enrollment	0.841	(0.366)	0.844	(0.363)	0.832
School characteristics					
Number of students (2010-2012)	95.264	(48.292)	92.349	(31.826)	0.718
SABER 11 score (2010-2012)	0.160	(0.216)	0.118	(0.275)	0.381
Morning shift	0.647	(0.478)	0.625	(0.484)	0.811
Afternoon shift	0.330	(0.470)	0.359	(0.480)	0.748
Single shift	0.023	(0.150)	0.016	(0.125)	0.803
School has computer lab	0.969	(0.173)	0.958	(0.201)	0.749
Total number of students	3,224			3,377	
Total number of schools	58			57	

Source: Authors' calculations from surveys matched to administrative data.

Notes: Using date of birth, we compute each student's age on December 31, 2013. The number of students is the average number of individuals who sat for the SABER 11 exam in each year from 2010-2012. SABER 11 scores are standardized with respect to each year's national average. The last column presents the p-value of the difference in the attribute between treatment and control groups calculated by regression with clustered standard errors at the school-level. A joint significance test for student and school variables accepts that these characteristics are unable to explain the likelihood of attending a treatment school, with an estimated p-value of 0.239.

Table 2.2. Average effects on knowledge and beliefs

	Knowledge			Perceived earning errors	
	Labor Observatory	ICETEX	FESBO	Vocational	Academic
A. ANCOVA					
Treatment	0.008	0.049***	0.016	-0.002	0.001
	(0.007)	(0.016)	(0.012)	(0.027)	(0.029)
Adjusted p-value	0.761	0.009	0.608	1.000	1.000
Observations	5,080	5,365	5,112	5,121	5,169
B. Difference-in-differences					
Treatment × Post	-0.005	0.046**	0.007	0.037	0.035
	(0.010)	(0.018)	(0.014)	(0.038)	(0.035)
Adjusted p-value	0.978	0.051	0.986	0.844	0.832
Observations	10,556	10,861	10,591	10,599	10,656
Baseline mean	0.077	0.694	0.175	0.636	0.949

Source: Authors' calculations from survey data.
Notes: Each column and panel correspond to separate OLS regressions. Panel A presents coefficients of ANCOVA regressions that control for student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on exit exam in previous years, has computer lab, shift indicators, and school size), and neighborhood fixed effects. Panel B presents coefficients for difference-in-difference regressions that control for individual fixed-effects. Standard errors in parentheses are clustered at school-level. We report adjusted p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 2.4.2 for details).
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.3. Average effects on test scores and higher education choices

	Test scores			Higher education choices				
	Overall score	Math	Language	College enrollment	Academic degree	Private college	Top-10 college	STEM field
A. Full sample								
Treatment	-0.002	0.045	-0.004	0.004	0.008	0.013	0.005**	0.005
	(0.038)	(0.042)	(0.033)	(0.022)	(0.008)	(0.012)	(0.003)	(0.006)
Adjusted p-value	0.997	0.343	0.952	0.997	0.754	0.593	0.086	0.872
Observations	6,318	6,318	6,318	6,298	6,298	6,298	6,298	6,298
B. Balanced sample								
Treatment	0.019	0.065	0.011	-0.001	0.010	0.012	0.006**	0.006
	(0.039)	(0.041)	(0.035)	(0.023)	(0.008)	(0.013)	(0.003)	(0.006)
Adjusted p-value	0.858	0.144	0.826	1.000	0.601	0.719	0.082	0.779
Observations	5,427	5,427	5,427	5,414	5,414	5,414	5,414	5,414
Baseline mean				0.438	0.096	0.150	0.011	0.052

Source: Authors' calculations from surveys matched to administrative data.

Notes: Each column and panel correspond to separate OLS regressions that control for student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on exit exam in previous years, has computer lab, shift indicators, and school size), and neighborhood fixed effects. Panel A presents results for students interviewed at baseline that are matched to the administrative records (full sample) and Panel B for students observed in both in-school surveys who are matched to administrative data (balanced sample). Standard errors in parentheses are clustered at school-level. We report adjusted p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 2.4.2 for details).

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.4. Heterogeneous effects on knowledge and beliefs

	Knowledge			Perceived earning errors	
	Labor Observatory	ICETEX	FESBO	Vocational	Academic
A. Gender					
Female	-0.012	0.033	-0.005	0.047	0.068
	(0.013)	(0.023)	(0.019)	(0.053)	(0.046)
Male	0.002	0.060*	0.021	0.025	-0.003
	(0.015)	(0.024)	(0.019)	(0.042)	(0.042)
p-value (Female=Male)	0.998	0.963	0.969	1.000	0.891
Observations	10,556	10,861	10,591	10,599	10,656
B. Family income					
Low (≤2 MW)	-0.003	0.051	0.004	0.020	0.032
	(0.011)	(0.021)	(0.016)	(0.048)	(0.039)
Middle (>2 MW)	-0.009	0.035	0.013	0.073	0.045
	(0.016)	(0.024)	(0.025)	(0.047)	(0.051)
p-value (Low=Middle)	1.000	0.997	1.000	0.996	1.000
Observations	10,556	10,861	10,591	10,599	10,656
C. Perceived earning errors (academic)					
Under or equal	-0.010	0.162**	0.080	0.195	0.119
	(0.040)	(0.051)	(0.048)	(0.100)	(0.090)
Over	-0.006	0.038	0.002	0.025	0.022
	(0.011)	(0.019)	(0.015)	(0.037)	(0.035)
p-value (Under=Over)	1.000	0.141	0.738	0.603	0.978
Observations	10,147	10,422	10,178	10,318	10,417

Source: Authors' calculations from survey data.

Notes: Each column and panel correspond to separate difference-in-difference regressions that interact a dummy variable for each group with the treatment indicator and all controls. Standard errors in parentheses are clustered at school-level. Reported significance levels and p-values are adjusted for multiple hypothesis testing.

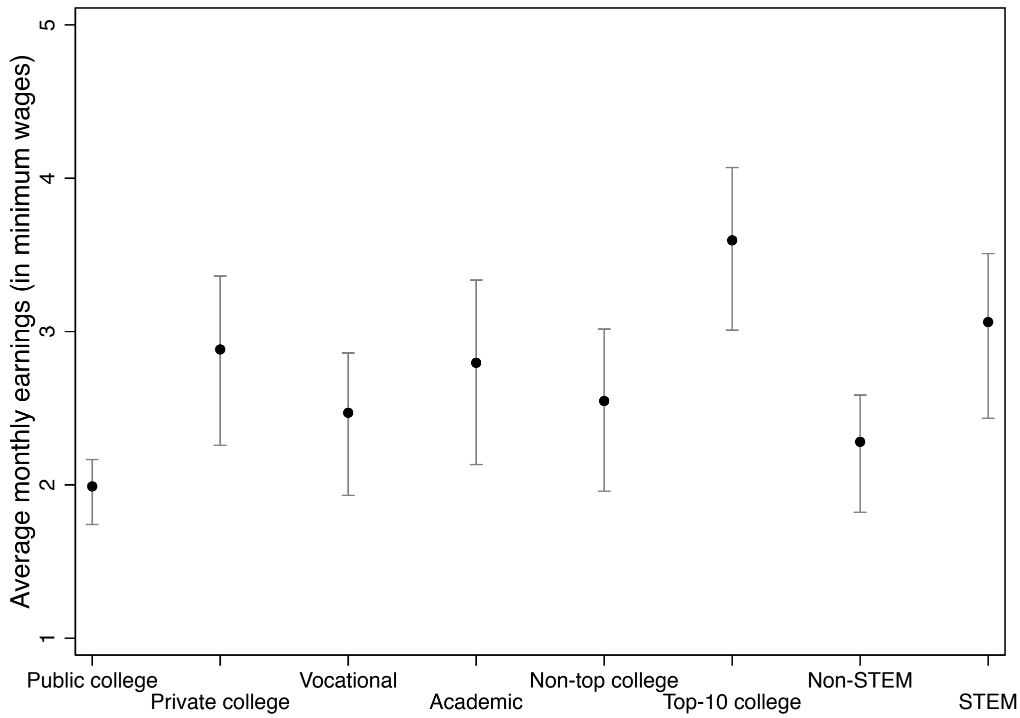
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2.5. Heterogeneous effects on test scores and higher education choices

	Test scores			Higher education choices				
	Overall score	Math	Language	College enrollment	Academic degree	Private college	Top-10 college	STEM field
A. Gender								
Female	-0.030	0.029	-0.045	-0.014	0.007	0.004	0.006	0.001
	(0.043)	(0.047)	(0.041)	(0.026)	(0.015)	(0.003)	(0.011)	(0.007)
Male	0.030	0.063	0.043	0.025	0.021	0.007	0.011	0.008
	(0.048)	(0.050)	(0.041)	(0.024)	(0.014)	(0.004)	(0.013)	(0.010)
p-value (Female=Male)	0.632	0.677	0.133	0.659	0.961	0.991	1.000	0.998
Observations	6,318	6,318	6,318	6,298	6,298	6,298	6,298	6,298
B. Family income								
Low (≤2 MW)	-0.022	0.022	-0.017	0.006	0.024	0.004	0.013	0.005
	(0.042)	(0.043)	(0.039)	(0.023)	(0.011)	(0.003)	(0.009)	(0.007)
Middle (>2 MW)	0.042	0.096	0.026	0.000	-0.010	0.009	-0.002	0.004
	(0.049)	(0.055)	(0.046)	(0.027)	(0.021)	(0.006)	(0.016)	(0.013)
p-value (Low=Middle)	0.541	0.221	0.591	1.000	0.492	0.974	0.986	1.000
Observations	6,318	6,318	6,318	6,298	6,298	6,298	6,298	6,298
C. Perceived earning errors (academic)								
Under or equal	0.004	0.081	0.033	0.056	0.027	-0.001	0.030	0.032
	(0.099)	(0.099)	(0.094)	(0.045)	(0.034)	(0.008)	(0.033)	(0.018)
Over	-0.008	0.043	-0.011	-0.006	0.010	0.006	0.004	0.002
	(0.037)	(0.041)	(0.034)	(0.022)	(0.013)	(0.003)	(0.009)	(0.007)
p-value (Under=Over)	1.000	0.853	0.826	0.636	0.998	0.959	0.994	0.620
Observations	6,021	6,021	6,021	6,003	6,003	6,003	6,003	6,003

Source: Authors' calculations from surveys matched to administrative data.
Notes: Each column and panel correspond to separate OLS regressions that interact a dummy variable for each group with the treatment indicator and all controls. Standard errors in parentheses are clustered at school-level. Reported significance levels and p-values are adjusted for multiple hypothesis testing.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 2.1. Average monthly earnings of recent college graduates



Source: Authors' elaboration from Labor Observatory data.
Notes: Monthly earnings are expressed in minimum wages (1 minimum wage ≈ $288 US dollars), and correspond to the average entry-level salaries for recent graduates by college, level, and field (in the first three years). The lower and upper bounds identify the 25th and 75th percentiles.

Figure 2.2. Intervention timing and primary data collection

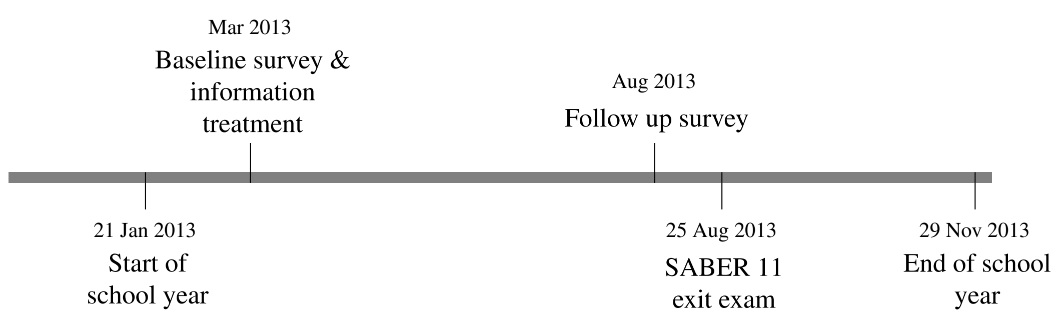
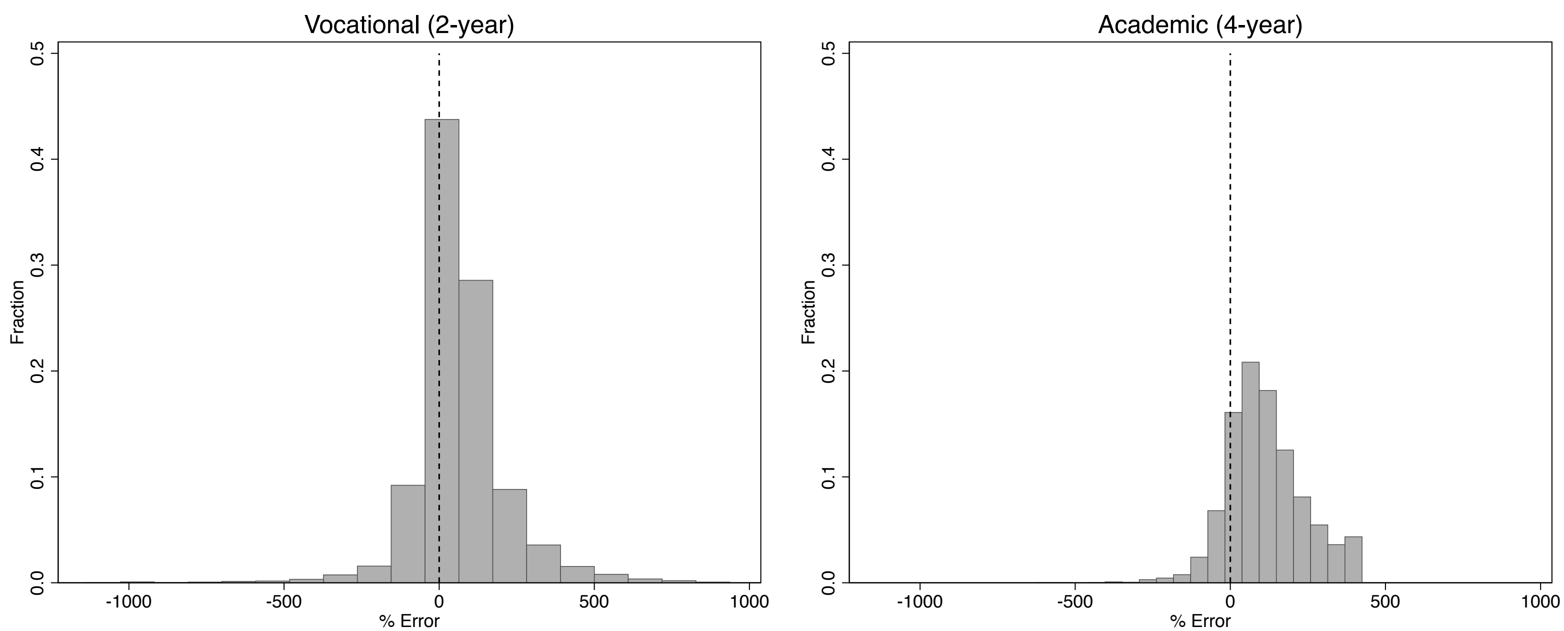


Figure 2.3. Distribution of perceived earning errors at baseline



Source: Authors' elaboration from survey data.

Notes: We calculate perceived earning errors as the difference between perceived and actual earnings divided by actual earnings. Let y^j denote earnings, with $j = \{\text{actual,perceived}\}$. Errors are calculated as $(y^{perceived} - y^{actual})/y^{actual}$.

Chapter 3

How Important is Spatial Correlation in Randomized Controlled Trials?[1]

with Kathy Baylis

3.1 Introduction

Socioeconomic data tend to be correlated over time and space. However, randomized controlled trials (RCTs) inspired by clinical experiments do not account for the latter relationship because cross-sectional correlation is uncommon in medical data. RCTs provide researchers with a simple and credible way to estimate the unbiased causal effect of a randomized intervention (Duflo et al., 2008), which has led to a vast amount of evidence on the effectiveness of educational, financial, health, labor market policies, information, and other programs on individual and household-level outcomes.[2]

This paper studies the consequences of ignoring cross-sectional correlation over space on re-

[1]We would like to thank Richard Akresh, Giuseppe Arbia, Mary Arends-Kuenning, Andre Avelino, Anil Bera, Leonardo Bonilla, Ben Crost, Raymond Florax, Don Fullerton, Rafael Garduño-Rivera, Geoffrey Hewings, Roger Koenker, Esteban López, Daniel McMillen, Dusan Paredes, Paolo Postiglione, and Ignacio Sarmiento for fruitful and engaging discussions on this research; as well as seminar participants at the University of Illinois, the 2014 North American Regional Science Conference, the 2015 Midwest International Development Conference, and the 2015 AAEA & WAEA Joint Meeting. Marin Thompson provided outstanding research assistance. All remaining errors and omissions are entirely our own and do not necessarily reflect the views of the University of Illinois.

[2]RCTs have also been criticized. The most debated issues include their lack of external validity and replicability (Rodrik, 2008), agnosticism with respect to behavioral changes (Ravallion, 2009), partial equilibrium approach (Deaton, 2010), and interference or spillover effects (Manski, 1993b).

searchers' ability to evaluate randomized programs using difference-in-differences (DD). Treatment status in RCTs is often assigned at the individual or cluster-level. This study focuses on the latter designs. We evaluate DD methods because they are commonly used to estimate treatment effects in settings that collect data before and after treatment. When treatment varies at the cluster-level and outcomes are measured at the individual-level, most authors cluster DD standard errors to correct for within-cluster dependence and serial correlation (Bertrand et al., 2004, Cameron and Miller, 2015), which assumes that between-cluster correlation is zero.

Many outcomes of interest in RCTs are variables that at least in other contexts, show some degree of spatial correlation (Barrios et al., 2012). Using baseline data from Mexico's Progresa program, we estimate spatial correlation coefficients for secondary enrollment rates and log per capita income (Moran, 1948). Figure 3.1 plots the empirical relationship between villages and their nearest neighbor. Spatial correlation in secondary enrollment is 0.285, indicating that more educated individuals tend to cluster in neighboring villages. We find a stronger relationship for per capita income, which has a coefficient of 0.427. Assuming zero between-cluster correlation is therefore at odds with RCT data. Despite the availability of spatial econometric methods to deal with spatial correlation, they remain largely overlooked in the RCT literature.

Spatial econometrics has mainly studied the consequences of spatial correlation in cross section and panel data settings on regional data (Anselin, 1988, Arbia, 2006). Our approach considers differences-in-difference scenarios that use individual-level data but where spatial correlation occurs at the cluster-level, which also happens to be the randomization unit. In theory, omitting spatially-correlated outcomes results in biased and inefficient estimates while ignoring spatially-correlated unobservables lowers efficiency. We therefore simulate a scholarship program that raises test scores by 0.25 standard deviations to test the performance of difference-in-differences with clustered standard errors for both types of spatial correlation.

Monte Carlo results confirm these predictions, especially for strong levels of spatial correlation. When the outcome of interest is spatially correlated, difference-in-differences will estimate program effects between 0.29 and 0.98 standard deviations, a bias ranging from 15-300 percent. Researchers will find a statistically significant effect of the scholarship program only about 32-64 percent of the time. Power can be improved by increasing the number of clusters, but bias remains unaffected. Omitting a spatially-correlated unobservable will not affect bias but reduces statistical power, correctly rejecting the null hypothesis of no effect 13-70 percent of the time. Program size improves efficiency unless spatial correlation is persistent ($\geq$0.90). Thus, smaller RCTs are more sensitive to spatial correlation than larger interventions.

We evaluate several methods to account for spatial correlation in RCTs. Adding cluster or individual-level fixed effects to control for time-invariant attributes provides no visible gains with either individual or village-level data. The best performing alternative is a spatial difference-in-difference specification that controls for correlation in the outcome or unobservable. Spatial DD provides satisfactory results in terms of bias and power even with few clusters. While efficiency for all estimation procedures improves as program size grows, only spatial DD is able to account for bias or inefficiency due to persistent levels of spatial correlation.

The empirical framework is then applied to Mexico's conditional cash transfer program, Progresa. We test previously evaluated outcomes in the literature for evidence of spatial correlation, finding statistically significant relationships across villages. Primary and secondary enrollment, female labor supply, and household income show evidence that the source of correlation is the outcome itself, while a spatially-correlated unobservable is more likely for per capita consumption. However, estimated correlations are low. Given our Monte Carlo results and the size of the program, we find that difference-in-difference estimates are robust to spatial correlation when using either individual or village-level data for different spatial networks. We also argue that spatial methods can facilitate the estimation of direct and indirect effects of randomized programs and identify geographic mechanisms that drive results.

Our research contributes to the randomization literature by rigorously inspecting the reliability of difference-in-difference estimators. It shares similar motivation with previous work that studies this method's performance in the presence of correlation over time (Bertrand et al., 2004, McKenzie, 2012), although we focus on cross-sectional dependence. It also complements the growing body of evidence on the consequences of spillovers in RCTs (Hudgens and Halloran, 2008, Aronow, 2012, Aronow and Samii, 2013). However, unlike the majority of papers analyzing within-cluster spillovers[3], we address between-cluster spillovers and show that randomization *per se* does not guarantee that the influence of spatial correlation is eliminated.

This paper also contributes to the spatial econometrics literature. Studies in this area have mostly focused on aggregate units of measurement but not microdata. Recent research has studied the effects of a spatially-correlated treatment variable in cross-section (Barrios et al., 2012) and difference-in-difference settings (Dubé et al., 2014, Delgado and Florax, 2015, Chagas et al., 2016). However, our approach takes a further step by implementing spatial models that account for correlation at the cluster-level without sacrificing individual-level variation. To our knowledge,

[3]Within-cluster spillovers have been more prominently studied. Some recent papers analyzing within interference in randomized settings include Duflo and Saez (2003), Angelucci and DeGiorgi (2009), Bobonis and Finan (2009), Lalive and Cattaneo (2009), Babcock and Hartman (2010), Barrera-Osorio et al. (2011), and Baird et al. (2014).

this paper and Arbia et al. (2016) are among the first studies in this direction.

The remainder of this chapter proceeds as follows. Section 3.2 surveys the RCT literature to characterize its treatment of spatial correlation. Section 3.3 derives the expected consequences of ignoring spatial correlation on difference-in-difference estimators. Section 3.4 provides Monte Carlo evidence on the performance of difference-in-differences when outcomes or unobservables are spatially correlated. We then test alternative DD specifications to deal with the resulting consequences. Section 3.5 analyzes spatial correlation patterns and applies our estimation framework to data from Mexico's Progresa. Section 3.6 summarizes and concludes.

3.2 Spatial correlation in the RCT literature

We compiled RCT papers from six economics journals between 2000 and 2014.[4] Studies were included if they estimate the effect of a randomly allocated treatment. In total, we found 86 such papers. Table 3.1 summarizes the randomization method, journal of publication, setting, and main outcomes of the selected studies.

Fifty-five papers evaluate an individual-level treatment (64%) while thirty-one assess a cluster-randomized treatment (36%). Forty-two articles study interventions in developing countries and forty-four in industrialized contexts. The countries with the largest number of published RCTs are the United States (37), Kenya (8), Mexico (8), and India (7).

The main outcomes in each paper were classified into six categories.[5] We counted thirty-four articles analyzing educational outcomes, sixteen observing consumer behavior, seven discussing

[4]The journals are the American Economic Review, the American Economic Journal: Applied Economics, the American Economic Journal: Policy Economics, the Quarterly Journal of Economics, the Journal of Political Economy, and the Review of Economic Studies.

[5]Papers could fall into more than one category. Unfortunately, 16 of the 86 papers were not directly classifiable and were included in the 'Other' category.

health, five dealing with micro-credit, four focusing on insurance, and four evaluating effects on investment. Because most studies collect panel data for two points in time (pre and post exposure), treatment effects are usually estimated by difference-in-differences (DD) with clustered standard errors that control for within-cluster dependence and serial correlation.

Many socioeconomic outcomes tend to be correlated across space. Consider a frequent outcome in RCTs, years of education. Evidence shows positive and significant spatial correlation in educational attainment between US states, which tells us that more educated individuals are located in contiguous regions (Barrios et al., 2012). Assuming no between-state correlation is thus directly at odds with observable patterns in the data, since this correlation does not vanish across geographic boundaries. Other studies have also found spatial correlation in school enrollment (Bobba and Gignoux, 2016), consumer behavior (Case, 1991), health outcomes (Duncan et al., 1993, Miguel and Kremer, 2004), and investment (Cohen and Paul, 2004).

Over a third of the surveyed papers evaluate a cluster-randomized program, but the majority do not consider how correlation across clusters may affect their results.[6] Only two studies have addressed the issue directly. The first, Miguel and Kremer (2004), calculates the effect of a de-worming treatment randomized across schools on education and health outcomes. Using the density of treatment within villages as their source of regional treatment variation, they find that for every additional 1,000 dewormed children living within a 3 kilometer radius, enrollment further increases by 2 percentage points and rates of moderate to heavy worm infections fall by 26%. The second, Bobba and Gignoux (2016), estimates the spatial spillovers of Progresa on secondary en-

[6]A larger fraction of surveyed studies analyze how ineligible individuals within a cluster may be affected by their eligible peers. See Banerjee et al. (2007), Kremer and Miguel (2007), Angelucci and DeGiorgi (2009), Bobonis and Finan (2009), Glewwe et al. (2010), Duflo et al. (2011), Muralidharan and Sundararaman (2011), Attanasio et al. (2012).

rollment. They find that besides the 9.7% increase in treated villages, living within five kilometers of a treatment community further raises secondary attendance by 2.9%.

Two main points are worth noting from this review. First, many RCTs are randomized at the cluster-level and evaluate their results using difference-in-difference methods that adjust standard errors for within-cluster dependence and serial correlation. Second, almost no papers control for between-cluster correlation or discuss how it may affect their estimates. Studies that address spatial relationships in their analysis find this dimension to be highly relevant to their results. Ignoring observed spatial correlation in outcomes or unobservables may potentially affect the resulting conclusions of employing difference-in-difference estimators in RCTs.

3.3 Spatial correlation in difference-in-difference estimation

We now derive the consequences of ignoring spatial correlation on difference-in-difference estimates of treatment effects. We first present the standard randomization framework and then introduce spatial correlation in outcomes and unobservables, respectively.[7] This exercise will provide testable implications for our Monte Carlo simulations.

3.3.1 Randomization benchmark

Randomization facilitates the estimation of causal effects because it solves the counterfactual problem.[8] To illustrate, consider the potential outcomes framework first proposed by Rubin (1974), where outcomes are determined by a binary indicator, T, that is equal to 1 if an individual is

[7]See Barrios et al. (2012) and Delgado and Florax (2015) for the expected consequences when the treatment variable is spatially-correlated.
[8]For an in-depth treatment of randomization, see Morgan and Winship (2007), Pearl (2009), and Imbens and Rubin (2015).

exposed to a treatment and 0 otherwise. For individual i we observe: $y_i = Ty_i^1 + (1-T)y_i^0$.

Suppose that y_i are test scores and T indicates whether the person receives a scholarship. Researchers observe the outcome for individual i if they have the scholarship (y_i^1) or if they do not (y_i^0). Hence, calculating individual-specific treatment effects is impossible since we cannot monitor the same person in both states. This situation is known as the counterfactual problem.

One solution to this problem is to randomly assign T across clusters, for example villages. Let D_j denote the assignment variable. Randomization makes students in recipient and non-recipient villages comparable on average observable and unobservable characteristics, and different only in treatment status. Since data on outcomes and covariates are frequently collected pre and post-exposure, researchers will generally estimate the average treatment effect of an intervention using difference-in-difference regressions:

$$y_{ijt} = \alpha P + \beta(P \times D_j) + \gamma D_j + \boldsymbol{\theta}\mathbf{X} + \varepsilon_{ijt} \tag{3.1}$$

The parameter α captures secular time trends. The coefficient on the interaction term, β, is the program's average treatment effect.[9]. γ controls for pre-existing differences between treatment and control groups, and $\mathbf{X}$ is a matrix of covariates generally included to control for relevant attributes and increase efficiency. Last, ε_{ijt} is a mean-zero normally distributed error term assumed to be uncorrelated with all right-hand side variables.

In Equation (3.1), treatment varies at the cluster-level but outcomes are measured at the individual level and may be correlated over time. Therefore, most authors cluster their standard errors

[9] Technically, this coefficient represents the intent-to-treat effect. Under full take-up however, this effect is identical to the average treatment effect.

to correct for within-cluster dependence and serial correlation (Bertrand et al., 2004).

There is one additional assumption in this model: the Stable Unit Treatment Value Assumption (SUTVA). SUTVA requires that outcomes for treated and control units depend solely on their own treatment status and not on what treatment others receive. If this assumption does not hold, there is interference between units and randomization no longer provides unbiased causal effects because of spillovers across units.

Three types of SUTVA violations may be identified from the literature, which depend on the randomization unit. First, when treatment assignment occurs at the individual-level, interference may be driven by peer effects. Because contamination occurs inside shared networks such as schools or villages, we refer to these as 'within' spillovers. Second, if randomization occurs at the cluster-level, then correlation between clusters generates interference. Since contamination occurs among larger units where the internal population are all treated or untreated, we designate them as 'between' spillovers. Finally, both types of interference could be present at once, generating 'mixed' spillovers.

In what follows, we argue that depending on whether spatial correlation exists and in what variables —outcomes or unobservables—, difference-in-difference estimates of treatment effects with clustered standard errors may be inconsistent, inefficient, or both.

3.3.2 Spatial correlation in randomized settings

Clusters may be interdependent in a number of ways, but the First Law of Geography frames it succinctly: "everything is related to everything else, but near things are more related than distant things" (Tobler, 1970). Two types of spatial correlation have been the most widely studied in the regional economics literature: i) correlated outcomes or *spatial lag* processes, and ii) correlated unobservables or *spatial error* processes.[10]

Spatial lag: correlated outcomes over space

The spatial lag model tells us that outcomes for village j are influenced by outcomes in neighboring villages k. Supply constraints may generate this situation. If schools are built to serve multiple villages, then test scores in villages without a school depend on how the village with the school performs. Formally, we can rewrite Equation (3.1) as:

$$y_{ijt} = \alpha P + \beta(P \times D_j) + \gamma D_j + \rho \mathbf{W} \bar{y}_{kt} + \boldsymbol{\theta}\mathbf{X} + \varepsilon_{ijt} \qquad \text{for} \quad j \neq k \tag{3.2}$$

where $\mathbf{W}$ is a $J \times J$ proximity matrix that describes the spatial network, whose typical element w_{jk} is 1 if villages j and k are neighbors and zero otherwise. The degree of spatial correlation is captured by ρ. If positive, villages with higher test scores will have neighbors whose average scores are also high. If negative, higher scoring villages will have lower performing neighbors. For most socioeconomic outcomes, we expect a positive spatial correlation coefficient.[11]

What does the existence of a spatial lag imply for a cluster randomized treatment? Equation (3.2) tells us that $y_{ijt} = f(D_j, \bar{y}_{kt}(D_k), P, \mathbf{X})$. SUTVA is violated because the treatment status of neighboring villages indirectly affects observed outcomes. While randomization is still valid, between interference occurs because of existing geographic relationships in outcomes. The expected consequences are identical to those from omitted variables bias. If $\rho > 0$, then difference-in-differences will overestimate the true treatment effect.

[10] For a textbook treatment, see Anselin (1988) and Arbia (2006).

[11] While positive spatial correlation is the norm, some outcomes may be negatively correlated. See Kao and Bera (2016) for an example.

Note that the dimension of the outcome vector is $NT \times 1$ but the proximity matrix is $J \times J$. To overcome this dimensionality issue, we assume that if two villages are neighbors, then their inhabitants are also neighbors. To expand $\mathbf{W}$, we first take the Kronecker product of the proximity matrix and a symmetric matrix of ones for the population density in each village, $\mathbf{W}_N = \mathbf{W} \otimes \mathbf{1}_{n_j}$. Then, we take the Kronecker product of an identity matrix for the number of time periods and $\mathbf{W}_N$ to obtain the panel version of the neighbors matrix, $\mathbf{W}_{NT} = I_T \otimes \mathbf{W}_N$.[12]. Since all terms have the same dimension, we can rewrite Equation (3.2) to facilitate interpretation:

$$y_{ijt} = (I_{NT} - \rho \mathbf{W}_{NT})^{-1} \left[\alpha P + \beta(P \times D_j) + \gamma D_j + \boldsymbol{\theta}\mathbf{X} + \varepsilon_{ijt}\right] \tag{3.3}$$

The average treatment effect is now β times the inverse of $(I_{NT} - \rho \mathbf{W}_{NT})$, or the sum of direct and indirect effects. Difference-in-differences will provide an estimate that cannot differentiate between the two. Moreover, the error term is also multiplied by this inverse, so variance estimates will also be affected. Therefore, if an outcome is spatially correlated and this dependence is ignored, difference-in-differences will be inconsistent and inefficient.

Spatial error: correlated unobservables over space

The spatial error model assumes that there is some unobservable attribute that is spatially correlated and affects the outcome of interest. A prime example is agricultural productivity. Productivity measures are generally absent in practice and tend to be correlated over space since higher yielding farming areas tend to be clustered together. Agricultural productivity has been linked with educational outcomes through the labor market (Ferreira and Schady, 2009). For instance, a positive

[12]This procedure assumes that the spatial network is time-invariant and was chosen for simplicity. The expansion of the proximity matrix follows Millo and Piras (2012).

productivity shock may increase the demand for child labor, attracting students to the fields and out of the classroom. By exam time, some children may under-perform because of work-related absences.

Suppose village-level productivity may be modeled as spatially-correlated random noise, $u_{jt} \sim N(0, \sigma^2)$:

$$(I - \lambda \mathbf{W})^{-1} u_{jt} \tag{3.4}$$

where as above, $\mathbf{W}$ is the $J \times J$ weights matrix. To differentiate from a spatial lag, we measure the strength of this correlation with the parameter λ. Equation (3.4) tells us that village j's agricultural productivity is related to its neighbors' productivity level, unless $\lambda = 0$.

Once again, we expand $\mathbf{W}$ to the individual-level and simplify:

$$\nu_{ijt} = (I_{NT} - \lambda \mathbf{W}_{NT})^{-1} u_{jt} \tag{3.5}$$

and outcomes are:

$$y_{ijt} = \alpha P + \beta(P \times D_j) + \gamma D_j + \boldsymbol{\theta}\mathbf{X} + \nu_{ijt} + \varepsilon_{ijt} \tag{3.6}$$

The spatial error model does not lead to such dire consequences as the spatial lag model. Since outcomes are not directly or indirectly affected by their neighbors' treatment status, SUTVA is not violated and difference-in-differences provides an unbiased treatment effect. However, variance estimates will be inefficient at higher values of λ because of between-cluster correlation in the error term. Even when controlling for within-cluster dependence and serial correlation, between-cluster noise may not be eliminated (Cameron and Miller, 2015). Therefore, omitting spatially-correlated unobservables will lead to consistent but inefficient difference-in-difference estimates of treatment effects.

3.4 Monte Carlo Evidence

Suppose a government implements a pilot scholarship program to improve standardized test scores in the poorest region of the country. The intervention will be scaled up if successful and discarded otherwise. Mindful about the benefits of randomization to quantify the impact of social programs, a team of independent researchers designs the pilot so that scholarships are randomly allocated at the village-level. The team also decides that to avoid within spillovers, all students in treated villages will receive the transfer while those in control villages will not.

Using simulated panel data for two periods, before and after the intervention, we analyze the performance of difference-in-difference estimates (DD) with clustered standard errors when ignoring different degrees of spatial correlation in outcomes and unobservables, respectively.

3.4.1 Simulation setup

The simulations are carried out in six steps.[13] First, our procedure involves creating villages. We generate $J = \{50, 100, 200, 500\}$ villages to test different program sizes and locate them using random (x, y) coordinates. Second, we define a spatial network using a k-nearest neighbor approach.[14] Results are presented for the simplest case, $k = 1$, but were also calculated with $k = 2$ for robustness. Each network is summarized in a proximity matrix, $\mathbf{W}^k$. Figure 3.2 plots a random

[13]Replication codes for all Monte Carlo simulations are available upon request.

[14]We do not assume that links between villages are symmetric. Hence, village j may be linked to village k, but the converse need not apply.

draw of village locations, treatment status, and their links for the $k = 1$ spatial network. Figure C.1 in the Supplementary material for Chapter 3 shows the network for $k = 2$ neighbors.

Third, villages are randomly assigned into treatment and control groups. We draw a random number from a uniform distribution on the unit interval as a proxy poverty score and set an exogenous eligibility threshold at 0.50. Villages with a score above this threshold are allocated to the treatment group and those below the cut-off are classified as controls. In the fourth step, we populate villages. For simplicity, we assume each village has 20 students.[15] Our results easily generalize to settings where villages differ in population size. The fifth step simulates the data generating process (DGP) for a spatially-correlated outcome and unobservable using Equations (3.3) and (3.6). Finally, we follow the literature and estimate the effects of the scholarship program using difference-in-differences with clustered standard errors at the village-level.

Parameter values are: $\alpha = 0.04$, $\beta = 0.25$, $\gamma = 0$, $\boldsymbol{\theta} = (5, 0.025, 0.07, 0.12)$. The effect of the scholarship program is β, which indicates that test scores should increase by 0.25 standard deviations. To ensure that the design is appropriate to estimate the program's impact, we calculate the minimum detectable effect (MDE) for the selected number of clusters (Duflo et al., 2008). Figure C.2 shows that 50 clusters provide enough statistical power to detect the scholarship program's effect when within-cluster correlation is below 0.05. Program sizes of 100, 200, and 500 are robust to within-cluster correlations of 0.15, 0.35, and 1.

In what follows, the villages and spatial network remain fixed, while steps three to six are repeated 1,000 times each.

[15] Including more observations in each cluster presents higher computation costs with small benefits compared to increasing the number of clusters, as is well-documented in the randomization literature (Duflo et al., 2008).

3.4.2 The consequences of omitting spatial correlation

The existence of a spatial lag indicates that outcomes in a village depend on their neighbors' outcomes. In this hypothetical country, we assume that schools are built to serve multiple communities, and thus high test scores in one village spur similar results in neighboring villages.

The degree of spatial correlation varies between $\rho = \{0, 0.10, 0.25, 0.50, 0.75, 0.90\}$, from low to strong dependence. Given the derivations in the previous section, we expect that this type of spatial correlation will lead to inconsistent and inefficient difference-in-difference estimates.

Monte Carlo results for spatially-correlated outcomes are presented in Panel A of Table 3.2. For 50 clusters, we encounter bias when the spatial correlation parameter is 0.50 or higher, with difference-in-differences overestimating treatment effects by around 15 to 300 percent. In our example, researchers would estimate that scholarships raise test scores between 0.29 and 0.98 standard deviations. At the same time, rejection rates deteriorate as spatial correlation increases. When correlation is above 0.50, researchers will find a statistically significant effect of the scholarship program about 32-64 percent of the time (when it should be at least 80 percent, ideally 95). Results slightly improve when considering a 2 nearest neighbor network, although bias and power issues are still sizable, as Table C.1 shows. The remaining rows in Panel A of Table 3.2 show findings for 100, 200, and 500 clusters. Increasing program size improves statistical power but does not eliminate the resulting bias from spatial correlation.

These findings indicate that difference-in-difference methods provide an incorrect and upward biased estimate of program impact for any RCT size when the outcome of interest is spatially-correlated. Such consequences are analogous to the findings in Blundell and Bond (2000), who find that strongly dependent data over time leads to inconsistency in dynamic panel data estimators.

Furthermore, small-scale experiments will also suffer from inefficiency caused by low statistical power, while larger programs are unaffected in terms of efficiency.

If researchers are unable to control for a spatially-correlated unobservable that is unrelated to all other covariates, then difference-in-difference estimates will be inefficient. This situation may occur if the scholarship program was rolled out in rural areas and agricultural productivity measures are not collected. We use the same parameter values to evaluate difference-in-difference performance, denoting spatial correlation in the unobserved variable with λ.

Panel B of Table 3.2 presents the results. There is no bias from omitting a spatially-correlated unobservable for any value of lambda. Estimates of the scholarship program's effect will be consistently estimated but rendered statistically insignificant when spatial correlation is above 0.50. While rejection rates are acceptable at lower levels of dependence, they decline rapidly after this threshold, especially when the number of clusters is small. Increasing the number of villages would solve this problem because it augments statistical power. However, persistent spatial correlation ($\lambda \geq 0.90$) would still result in inefficiency regardless of program size. These findings also hold for the $k = 2$ spatial network (Panel B in Table C.1). Therefore, omitting spatially-correlated unobservables will cause inefficiency in small but not large RCTs.

Some of these results are well established in the spatial econometrics literature for cross-section and panel data (Anselin, 1988, Arbia, 2006, Arbia et al., 2016). Recent research has studied the effects of a spatially-correlated treatment variable in cross-section (Barrios et al., 2012) and difference-in-difference settings (Dubé et al., 2014, Delgado and Florax, 2015, Chagas et al., 2016). Our approach takes a further step by incorporating spatial econometric models into differences-in-difference scenarios that use individual-level data but where spatial correlation occurs at the cluster-level, the latter which also happens to be the randomization unit.

These simulations provide evidence on how spatial correlation affects difference-in-difference estimates of treatment effects given a reasonable sample size and number of clusters. On the one hand, ignoring spatial correlation in the outcomes of interest will lead to bias and power issues. On the other hand, omitting a spatially-correlated unobservable will not affect bias but hamper power. Program size matters, since some of these consequences may be resolved by design if conditions allow for larger rather than smaller RCTs. Given that most socioeconomic data reveals the existence of geographic patterns, estimating the degree of spatial correlation is important to further ensure the internal validity of randomized programs.

In the case of the pilot scholarship program example used here, researchers may estimate an incorrect treatment effect (for a spatial lag) or be unable to detect a significant impact of the program (spatial lag and error), conditional on program size. Either way, despite the rigorous randomization, estimated results may be invalid and no policy lessons can be learned. Unaccounted spatial correlation could thus lead policymakers to discard a successful program.

3.4.3 Correcting for spatial correlation

Suppose these researchers inspect the data for spatial patterns across villages and effectively encounter that geographic correlation is strong (above 0.50). What procedure could they use to avoid the above issues and correctly estimate the scholarship program's effect?

We test three alternatives on individual-level data: i) difference-in-differences with village fixed effects, ii) difference-in-differences with individual fixed effects, and iii) spatial difference-in-differences that controls for correlation in outcomes or unobservables. The analysis also considers specifications i) and iii) on aggregated data at the village-level. Performance is evaluated by comparing the resulting bias and rejection rates for different degrees of spatial correlation. The ideal solution would lead to no bias and a rejection rate of at least 80 percent.

Table 3.3 shows the performance of these procedures for the case of 50 villages. For either type of spatial correlation, adding cluster or individual-level fixed effects to control for time-invariant attributes does not provide visible gains with individual or village-level data.

The best performing alternative is the spatial difference-in-difference specification that controls for correlation in the outcome or unobservable. This requires estimating Equations (3.3) and (3.6) by maximum likelihood, using the expanded neighbors matrix at the individual-level to model the spatial network. Spatial DD provides satisfactory results in terms of bias and rejection rates compared to methods that ignore spatial correlation. Even with few clusters, the method allows researchers to consistently estimate the effect of the scholarship program and identify statistically significant effects. The results also apply when aggregating the data to the cluster-level, where spatial DD provides unbiased estimates and higher statistical power.

All estimation methods perform better in terms of efficiency as program size grows, as shown in Figures 3.3 and 3.4.[16] However, when spatial correlation is strong ($\geq$0.75) or persistent ($\geq$0.90), only spatial DD provides consistent estimates and correctly sized hypothesis tests. These findings continue to hold when we consider a more complex spatial network.

Spatial correlation affects difference-in-difference estimates of a randomized program as well as their precision. The most effective way to correct for these issues requires modeling the spatial interaction. However, it is not the only possible solution. A common critique of spatial economet-

[16]Supplementary material for Chapter 3 Tables C.2, C.3, and C.4 provide the full estimates.

ric methods is that they impose too many restrictions (Corrado and Fingleton, 2012, Gibbons and Overman, 2012), but more flexible methods are available (Anselin and Florax, 1995, McMillen, 2012, Elhorst and Vega, 2013). ANCOVA methods that control for spatial correlation may also be explored, since McKenzie (2012) has shown them to be more effective than DD under certain conditions. Some of the losses generated by spatial correlation seem to affect statistical power. Several researchers have been studying the benefits and costs of multi-way clustering (see Cameron and Miller (2015) for an overview), which represents an interesting extension to spatial analysis. Hierarchical methods also seem a natural extension when cluster-level processes influence individual-level outcomes (Banerjee et al., 2003).

Overall, our results indicate that there are visible gains from incorporating spatial econometric methods into randomized evaluations. While spatial difference-in-differences may not be relevant in all RCT settings, researchers should rule out this possibility to ensure internal validity and test the robustness of their findings. We therefore revisit publicly available data from Progresa, Mexico's conditional cash transfer program, to provide a guide for practitioners.

3.5 Spatial correlation in Mexico's Progresa program

3.5.1 Program description

Mexico's *Programa de Educación, Alimentación y Salud* (Progresa) began in 1997 and remains in place today. It is the largest social program in the country and widely considered a landmark cash transfer program.[17] The intervention's objective was to alleviate poverty and foster the accumulation of human capital by providing transfers to eligible households conditional on regular school

[17]The program has been renamed over time, usually coinciding with its expansions. It is currently called *Prospera*.

attendance and periodic health check-ups of beneficiary children.

We revisit the first phase of Progresa, that took place between 1997-1999. The advantage of using this period is that 506 rural villages were chosen for randomized evaluation of the transfers.[18] Eligibility was determined in two-stages. First, villages were randomly assigned to either receive transfers or not. The resulting allocation selected 320 villages into treatment and 186 communities to control. Second, households within treatment villages were classified as eligible or ineligible by a poverty score, with the poorest becoming transfer recipients. On average, 78% of households in treatment villages were eligible to receive the transfers.

3.5.2 Data and methods

Data for Progresa's rural phase are publicly available on the website of the Mexican Ministry of Social Development (SEDESOL). It constitutes a census of beneficiaries spanning four waves: a baseline (September 1997 - March 1998) and three follow-ups in November 1998, March 1999, and November 1999. For simplicity, we aggregate the data into two time periods, before and after transfers. The data are geo-referenced using shapefiles from the National Statistics Institute (INEGI), which contain the latitude and longitude of all villages in Mexico.[19] The leftmost panel of Figure 3.5 plots these locations. Targeted communities were mostly located in the central-south states, roughly coinciding with some of the poorest areas in Mexico.

In order to gauge the importance of spatial correlation before transfers are paid out, we first study baseline data. The analysis is restricted to outcomes previously evaluated in the literature,

[18] Progresa's first phase covered approximately 2.6 million families in 50,000 rural villages nationwide, approximately 40% of rural households and 10% of all Mexican households (Gertler, 2004).

[19] The shapefiles we employ for geo-referencing villages are publicly available and may be downloaded here.

including: enrollment, labor supply, and per capita income and consumption. Subsequently, we return to the individual-level data to evaluate the program using difference-in-differences.

We use commonly employed methods in regional economics for our analysis.[20] First, we employ Moran's I (Moran, 1948). This index is a correlation coefficient that takes values between $[-1, 1]$ and whose statistical significance may be tested.[21] Second, given our simulation results, it is critical to determine whether correlation occurs on the outcome or some unobservable attribute. Anselin et al. (1996) developed a series of Lagrange multiplier tests based on the residuals from OLS regressions. These tests are robust to local misspecification and are widely employed to determine the source of spatial correlation. Based on the resulting evidence, we proceed to evaluate the program using standard and spatial difference-in-difference methods.

3.5.3 Baseline spatial correlation

To compare with the Monte Carlo simulations, we model interactions between villages using a k-nearest neighbor spatial network. Two relationships are considered: a village is related to its closest neighbor, $k = 1$ or its two nearest neighbors, $k = 2$. A visual depiction of these interactions among Progresa villages is shown in the two right hand panels of Figure 3.5.

Table 3.4 provides estimates of Moran's I for the selected outcomes and spatial networks. First, we notice that treatment assignment shows no significant spatial correlation. This finding provides further confirmation that randomization was successful, since treatment status should not be correlated across villages. All remaining outcomes show positive and statistically significant spatial correlation regardless of the chosen network, except for male labor supply. Most correlations are

[20] See Fischer and Getis (2009) for a textbook treatment.

[21] The index's expected value under the null of no spatial correlation is $\mathbb{E}(I) = -1/(N-1)$. Hence, it is not actually zero, but tends asymptotically to this limit.

positive, implying that villages with better outcomes tend to be clustered together. For instance, villages with higher enrollment rates tend to be near other villages that also have high enrollment rates. Female labor supply, income, and consumption show slightly higher values of Moran's I, indicating marked geographic patterns in this region of Mexico.

A statistically insignificant Moran's I in the treatment variable is insufficient evidence that SUTVA holds. We need to identify the source of the dependence to test its validity. A spatially-correlated outcome indicates that outcomes in one village affect its neighbors, which violates the assumption. A spatially-correlated unobservable will not affect whether SUTVA holds.

Table 3.5 presents p-values from Lagrange multiplier tests to determine the source of spatial correlation in Progresa outcomes. These statistics compare OLS residuals to estimated errors from spatial lag and spatial error models. Furthermore, they are independent, with statistics for spatial lag robust to the presence of a spatial error process and viceversa.[22] Results show that conditional on the network, the source of spatial correlation varies. Several statistics are insignificant for the nearest neighbor network but reveal stronger patterns for the two nearest neighbor network. For this relationship, there seem to be spatially-correlated unobservables are likely across villages for per capita consumption. Most of the remaining outcomes however, are consistent with evidence of spatial correlation in the outcome itself.

These results suggest that the SUTVA assumption may be compromised. SUTVA does seem to hold locally, since spatial correlation for the nearest neighbor network is weaker. This finding was also highlighted by Delgado and Florax (2015) for the case of spatially-correlated treatment. Therefore, the choice of spatial network matters. Educational outcomes may be affected by a

[22] The possibility that both forms of spatial correlation are present is also testable, but requires strict assumptions for estimation. See Anselin (1988), LeSage and Pace (2010), and Arbia (2014) for these generalized spatial models.

different set of neighbors than labor market outcomes. Researchers should assess the patterns of regional dependence to determine which network is appropriate.

3.5.4 Accounting for spatial correlation in Progresa

Given these results at the village-level, we return to the individual-level data to implement our empirical framework. We estimate difference-in-difference regressions with clustered standard errors and their spatial counterparts on balanced data for two time periods. The village-level weight matrices for $k = 1$ and $k = 2$ spatial networks are expanded following the procedure in Section 3.3. The choice of spatial lag or error model is based on the test statistics in Table 3.5. Since male labor supply is uncorrelated over villages, we omit this outcome from the analysis.

Table 3.6 presents unadjusted and spatial difference-in-difference estimates of Progresa's effect on the selected outcomes. Overall, DD estimates are unaffected when controlling for spatial correlation, as expected given the simulation results and the size of Progresa. In particular, estimated spatial correlation parameters are below 0.20, denoting weak relationships across space. Estimating the same regressions at the village-level provides similar conclusions, but less overall power due to losses from aggregation (see Supplementary material for Chapter 3 Table C.5). Therefore, difference-in-difference estimates of Progresa's impact are robust to spatial correlation when using either individual or village-level data for different spatial networks.

Even though DD estimates are unchanged in Progresa when controlling for cross-sectional dependence over space, spatial difference-in-difference estimators constitute an additional robustness test. More visible benefits may be seen in smaller programs and in settings where spatial correlation is stronger, as shown from our Monte Carlo simulations. In either case, these methods are implementable at low cost and provide many potential benefits to researchers.

Moreover, incorporating spatial methods into RCT evaluation can provide additional insight into how programs work. Although we did not expand on the interpretation of coefficients, finding significant spatial correlation in outcomes or unobservables informs us about possible spillovers. For instance, secondary enrollment shows evidence of a spatial lag process, indicating that enrollment in one village affects surrounding villages. Bobba and Gignoux (2016) found evidence of such spillovers in Progresa. Future work can focus on how to accurately estimate these spillovers and determine whether and how geographic mechanisms can lead to externalities similar to the procedure used by Miguel and Kremer (2004).

3.6 Conclusion

This paper explores how ignoring spatial correlation in outcomes and unobservables at the cluster-level affects difference-in-difference estimates at the individual-level. We derive predictions using spatial econometric theory and then conduct Monte Carlo simulations to test these expectations. Results show that ignoring spatial correlation in the outcomes of interest leads to upward bias and low power, while omitting a spatially-correlated unobservable does not affect bias but reduces power. Program size matters, since small RCTs are more sensitive to spatial correlation than larger interventions. Researchers can account for spatial correlation using spatial difference-in-differences, which is shown to outperform other commonly used specifications for all program sizes, especially when spatial correlation is strong or persistent.

We analyze data from Mexico's Progresa to gauge the extent of spatial correlation in a large RCT and test our empirical framework. Many outcomes denote significant spatial correlation, although the degree of the estimated relationships are low. Most variables show evidence of being spatially-correlated while fewer are consistent with the existence of a spatially-correlated unobservable. Given our simulation results, we find that difference-in-difference estimates are robust to spatial correlation when using either individual or village-level data for different spatial networks. Therefore, our estimates for Progresa agree with existing empirical evidence.

Given that most socioeconomic data reveals the existence of geographic patterns, accounting for these relationships may help us better understand individual and household behavior. Data collection in the field has improved substantially, with many programs now routinely collecting geographical coordinates of clusters and in some cases, even individuals. Researchers could analyze their data for spatial correlation, test underlying assumptions, and determine the most appropriate estimation method. Because the consequences of ignoring spatial correlation do not completely disappear when program size increases, this applies to both small and large scale interventions. If researchers find low levels of spatial correlation, then our empirical framework becomes an additional robustness test that validates using a simple approach. When spatial correlation is strong or persistent, spatial difference-in-difference estimators provide reliable results in terms of bias and power. Either way, incorporating spatial methods to the RCT literature provides several benefits at relatively low costs.

Geo-referenced data is becoming more common and less costly to collect. Taking full advantage of these data may help validate assumptions, determine an estimation strategy, allow estimation of direct and indirect effects, and inform researchers about geographic mechanisms that drive their results. This would allow for a more complete understanding of how randomized programs work and help improve the design and evaluation of future interventions.

3.7 Tables and Figures

Table 3.1. Survey of RCT studies

Total Number of RCTs	86
Randomization method	
Individual-level	55
Cluster-level	31
Journal	
AER	30
AEJ: Policy	7
AEJ: Applied Economics	17
JPE	6
QJE	21
REStud	5
Setting	
Developing countries	42
Mexico	8
Kenya	8
India	7
Developed countries	44
USA	37
Main outcome	
Education	34
Consumer Behavior	16
Health	7
Micro-credit	5
Insurance	4
Investment	4
Other	16

Notes: Authors' calculations from a survey of published articles in six journals between 2000 and 2014: the American Economic Review, the American Economic Journal: Applied Economics, the American Economic Journal: Policy Economics, the Quarterly Journal of Economics, the Journal of Political Economy, and the Review of Economic Studies. We define an article as a randomized controlled trial if it estimates the impact of a randomly allocated treatment on one or more outcomes.

Table 3.2. Difference-in-difference performance under spatial correlation

	Spatial correlation parameter					
	0.00	0.10	0.25	0.50	0.75	0.90
Panel A: Spatially-correlated outcome						
50 clusters						
Bias (in %)	-4.5	-5.2	-2.1	16.4	85.0	290.4
Rejection rate	80.7	75.7	74.5	64.4	47.2	32.1
100 clusters						
Bias (in %)	-5.0	-4.0	0.0	14.8	64.1	235.7
Rejection rate	97.0	97.6	96.9	91.7	65.2	45.8
200 clusters						
Bias (in %)	-4.2	-2.6	0.5	16.3	78.5	263.5
Rejection rate	100.0	100.0	100.0	99.7	95.1	80.6
500 clusters						
Bias (in %)	-4.2	-3.9	-0.5	14.3	69.0	235.6
Rejection rate	100.0	100.0	100.0	100.0	100.0	98.5
Panel B: Spatially-correlated unobservable						
50 clusters						
Bias (in %)	-2.4	-4.2	-3.5	-3.4	-5.0	-1.8
Rejection rate	71.9	70.1	68.6	64.0	37.8	13.8
100 clusters						
Bias (in %)	-5.2	-3.1	-4.5	-5.3	-3.8	-5.3
Rejection rate	93.0	94.5	92.3	88.2	65.9	17.9
200 clusters						
Bias (in %)	-3.5	-5.2	-4.8	-3.8	-4.6	-5.5
Rejection rate	99.7	99.9	99.7	99.2	89.8	32.9
500 clusters						
Bias (in %)	-4.3	-4.6	-4.2	-4.5	-4.8	-3.9
Rejection rate	100.0	100.0	100.0	100.0	100.0	66.9

Source: Authors' calculations from 1,000 Monte Carlo Simulations.
Notes: Reported values are averages from the total number of replications. Bias is calculated as the difference between the average estimate and the true value $([\hat{\beta} - \beta]/\beta) \times 100$. The rejection rate measures the percentage that DD finds a statistically significant effect when the null hypothesis is false (equivalent to statistical power).

Table 3.3. Alternative estimation procedures with 50 clusters

	Spatially-correlated outcome						Spatially-correlated unobservable					
	0.00	0.10	0.25	0.50	0.75	0.90	0.00	0.10	0.25	0.50	0.75	0.90
Using individual-level data												
DD with village fixed effects												
Bias (in %)	-4.5	-5.2	-2.1	16.4	85.0	290.4	-2.4	-4.2	-3.5	-3.4	-5.0	-1.8
Rejection rate	79.9	75.2	73.7	63.1	46.2	31.3	71.3	69.4	68.1	63.6	36.5	13.4
DD with individual fixed effects												
Bias (in %)	-4.5	-5.2	-2.1	16.4	85.0	290.4	-2.4	-4.2	-3.5	-3.4	-5.0	-1.8
Rejection rate	51.7	48.4	45.4	34.7	18.4	11.7	41.0	39.3	39.7	32.6	14.5	2.8
Spatial DD												
Bias (in %)	-5.0	-6.3	-6.7	-5.3	-2.9	-4.0	-2.8	-4.1	-3.6	-3.0	-5.7	-2.7
Rejection rate	78.9	76.3	76.8	78.2	80.1	78.9	71.8	69.4	70.5	71.1	69.4	76.9
Using village-level data												
DD with village fixed effects												
Bias (in %)	-4.5	-5.2	-2.1	16.4	85.0	290.4	-2.4	-4.2	-3.5	-3.4	-5.0	-1.8
Rejection rate	47.6	44.3	40.8	30.3	15.6	9.7	37.0	35.8	36.5	27.7	12.2	2.3
Spatial DD												
Bias (in %)	-5.0	-6.4	-6.8	-5.5	-3.4	-4.5	-2.8	-4.1	-3.7	-3.0	-5.7	-2.4
Rejection rate	81.2	77.4	80.0	79.9	81.0	80.6	74.4	71.3	72.5	69.7	59.8	62.1

Source: Authors' calculations from 1,000 Monte Carlo Simulations.

Notes: Reported values are averages from the total number of replications. Bias is calculated as the difference between the average estimate and the true value $([\hat{\beta} - \beta]/\beta) \times 100$. The rejection rate measures the percentage that DD finds a statistically significant effect when the null hypothesis is false (equivalent to statistical power).

Table 3.4. Spatial correlation in Progresa villages at baseline

		1 nearest neighbor		2 nearest neighbors	
	Mean	Moran's I	p-value	Moran's I	p-value
Treatment status	0.632	-0.024	0.693	-0.020	0.653
Primary enrollment	0.965	0.114	0.039	0.148	0.000
Secondary enrollment	0.485	0.285	0.000	0.293	0.000
Male labor supply	0.929	0.014	0.771	0.077	0.050
Female labor supply	0.190	0.365	0.000	0.389	0.000
Log per capita income	5.25	0.427	0.000	0.436	0.000
Log per capita consumption	5.18	0.288	0.000	0.310	0.000

Source: Authors' calculations from Progresa baseline data aggregated at village-level.
Notes: Reported p-values are obtained from conducting a two-tailed test of $H_0 : \mathbb{E}(I) = -1/(N-1)$.

Table 3.5. Robust Lagrange Multiplier Tests for source of spatial dependence

	1 nearest neighbor		2 nearest neighbors	
	Outcome	Unobservable	Outcome	Unobservable
Primary enrollment	0.095	0.148	0.156	0.394
Secondary enrollment	0.820	0.331	0.052	0.911
Male labor supply	0.972	0.975	0.672	0.879
Female labor supply	0.002	0.056	0.000	0.022
Log per capita income	0.002	0.276	0.000	0.085
Log per capita consumption	0.283	0.502	0.480	0.035

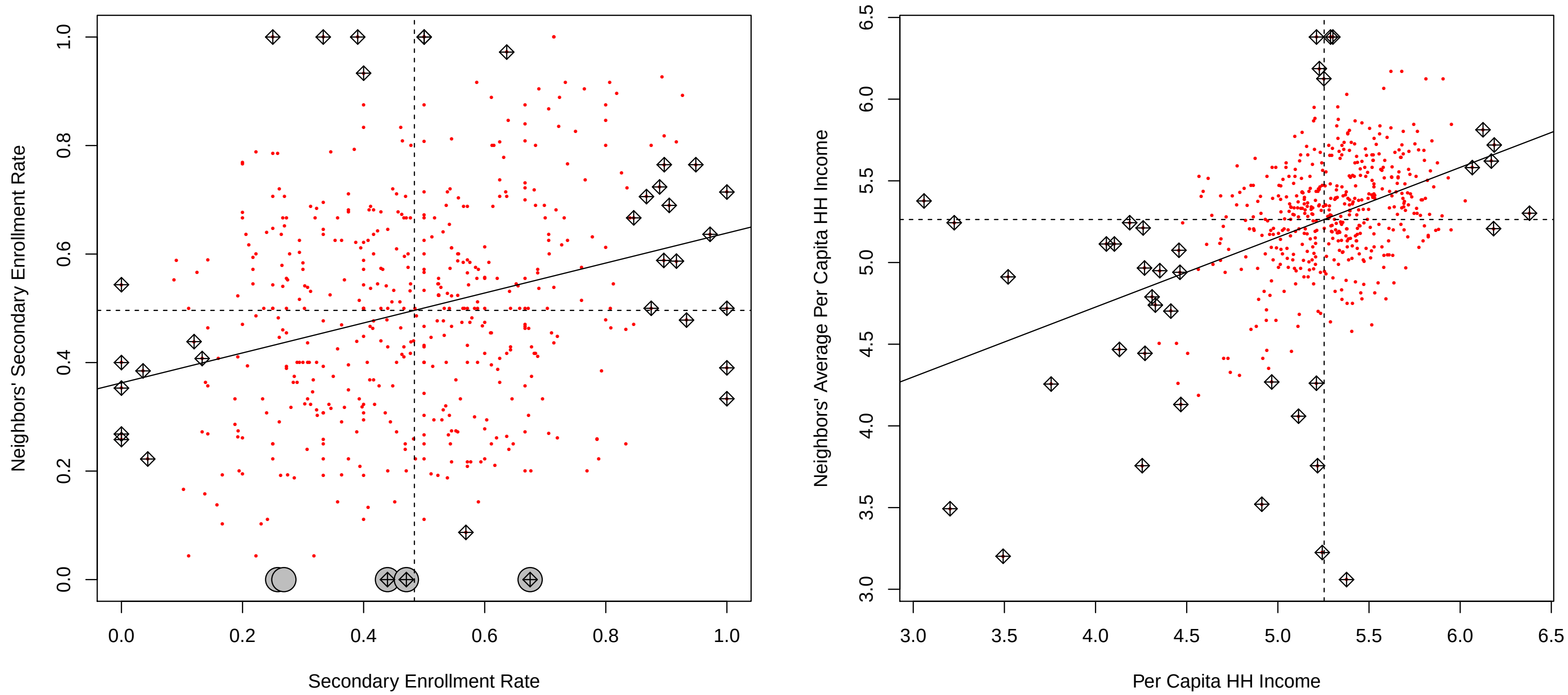
Source: Authors' calculations from Progresa baseline data aggregated at village-level.
Notes: These tests are two-directional, distributed as χ^2_2. See Anselin et al. (1996) for details. The reported p-values are obtained from the following regression: $y_j = \alpha + \beta T_j + \theta X_j + u_j$. Covariates in X_j include: average village household size, fraction of male headed households, average years of schooling of the household head, average number of children (0-17 years), average number of adults (+18 years), number of adult workers, fraction of dwellings owned, fraction of precarious dwellings, and the fraction households that have: drinking water, a toilet, sewer access, and electricity.

Table 3.6. Difference-in-differences estimates for Progresa at the individual-level

			Spatial DD	
		Unadjusted	1 nearest neighbor	2 nearest neighbors
Primary enrollment	β	0.010	0.013	0.013
(Spatial lag)		(0.004)***	(0.002)***	(0.002)***
	ρ		0.003	-0.001
			(0.000)*	(0.000)
	Observations	41,704	41,704	41,704
Secondary enrollment	β	0.031	0.031	0.031
(Spatial lag)		(0.014)**	(0.014)**	(0.014)**
	ρ		0.053	0.089
			(0.011)***	(0.015)***
	Observations	20,080	20,080	20,080
Female labor supply	β	-0.009	-0.009	-0.009
(Spatial lag)		(0.012)	(0.007)	(0.007)
	ρ		0.086	0.176
			(0.068)***	(0.016)***
	Observations	43,058	43,058	43,058
Per capita income	β	0.053	0.053	0.053
(Spatial lag)		(0.028)*	(0.018)***	(0.018)***
	ρ		-0.001	0.003
			(0.000)	(0.001)
	Observations	41,706	41,706	41,706
Per capita consumption	β	0.083	0.083	0.083
(Spatial error)		(0.028)***	(0.013)***	(0.013)***
	λ		0.145	0.181
			(0.016)***	(0.015)***
	Observations	41,130	41,130	41,130

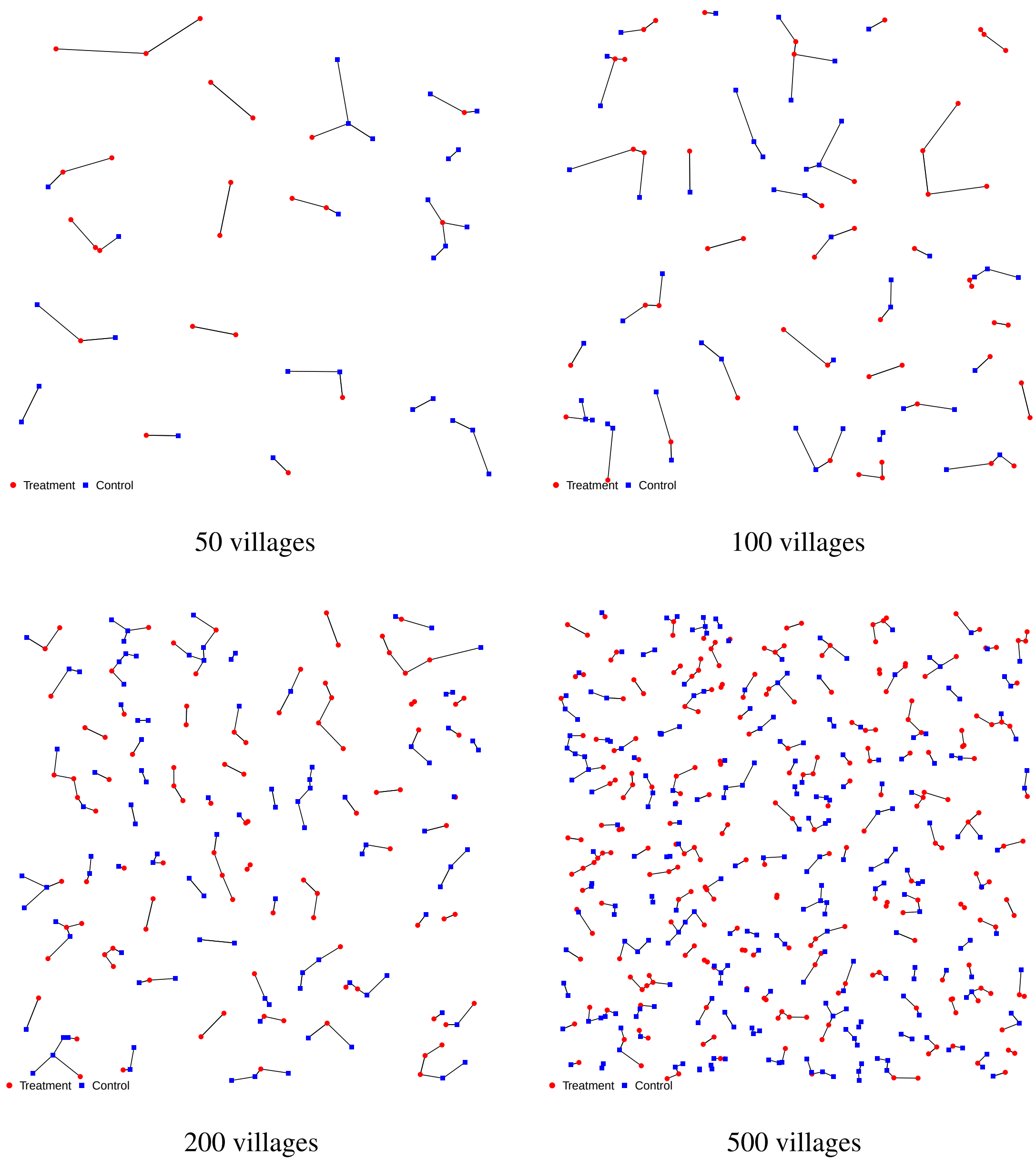
Source: Authors' calculations from Progresa data at the individual-level.
Notes: We estimate Equation (3.1) on individual data with no controls and use expanded weight matrices for spatial specifications. Each set of coefficients corresponds to a separate regression. Standard errors are clustered at the village-level.
*** Significant at 1 percent, ** 5 percent, * 10 percent.

Figure 3.1. Spatial correlation in secondary enrollment and household income in Progresa villages



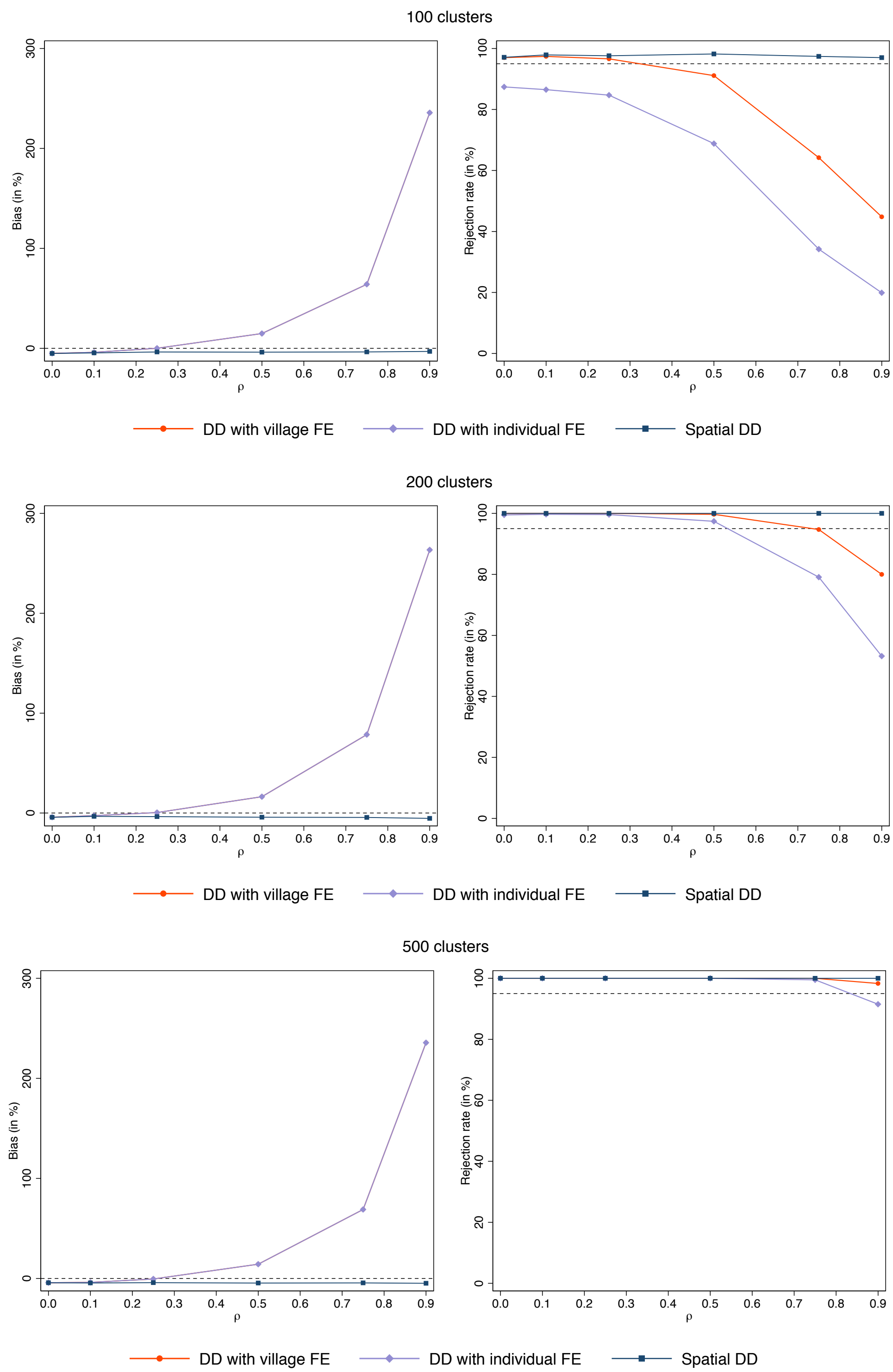
Source: Authors' elaboration from Progresa baseline data aggregated at village-level.
Notes: The figure plots the village-level mean of secondary enrollment and per capita household consumption in a village and the average for its closest neighbor.

Figure 3.2. Simulated spatial networks: 1 Nearest neighbor



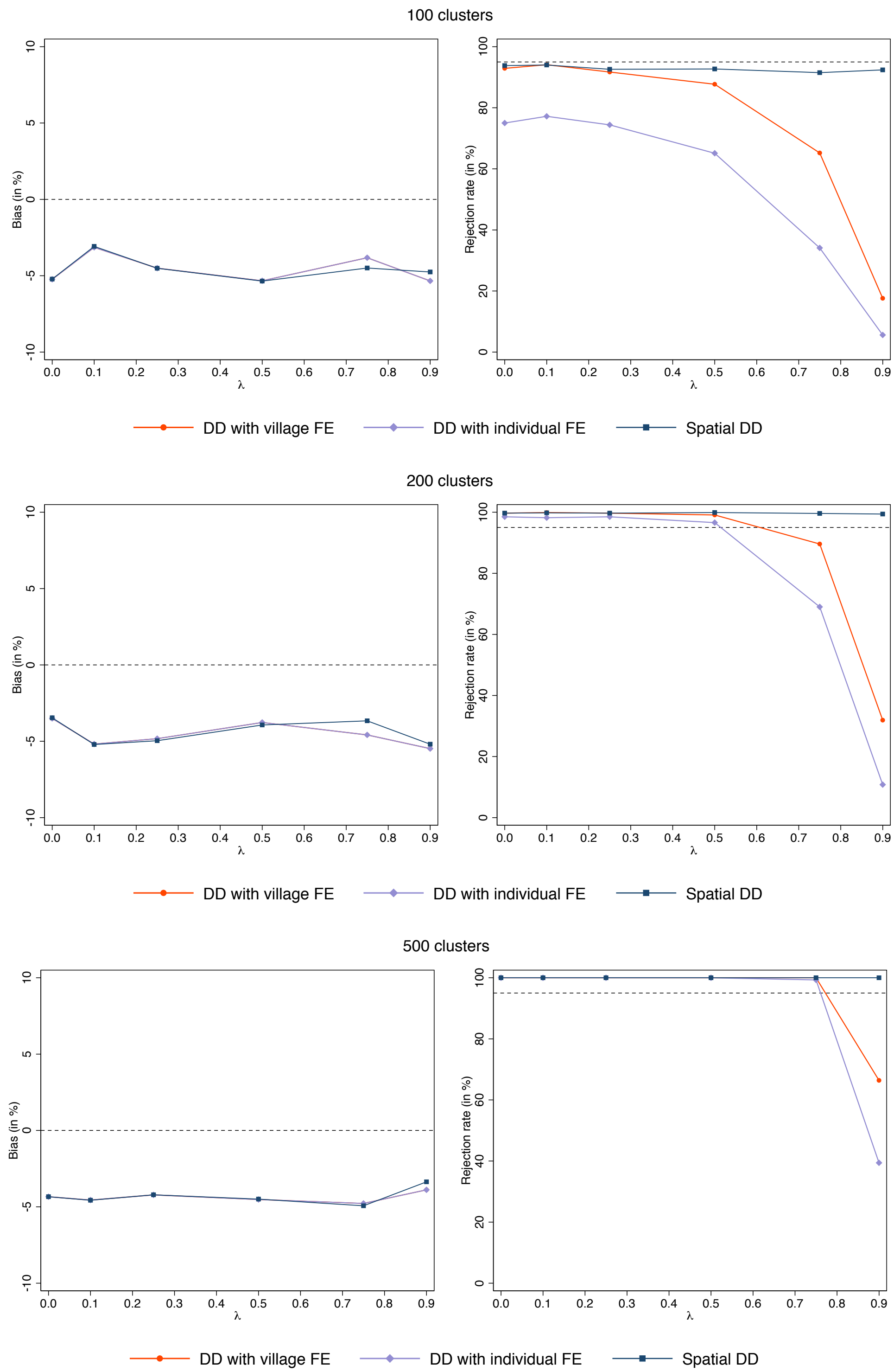
Source: Authors' elaboration from simulated data.
Notes: This graph shows a random draw of villages by allocated treatment status. Lines between villages denote the that two locations are neighbors.

Figure 3.3. Alternative specification performance for spatially-correlated outcome



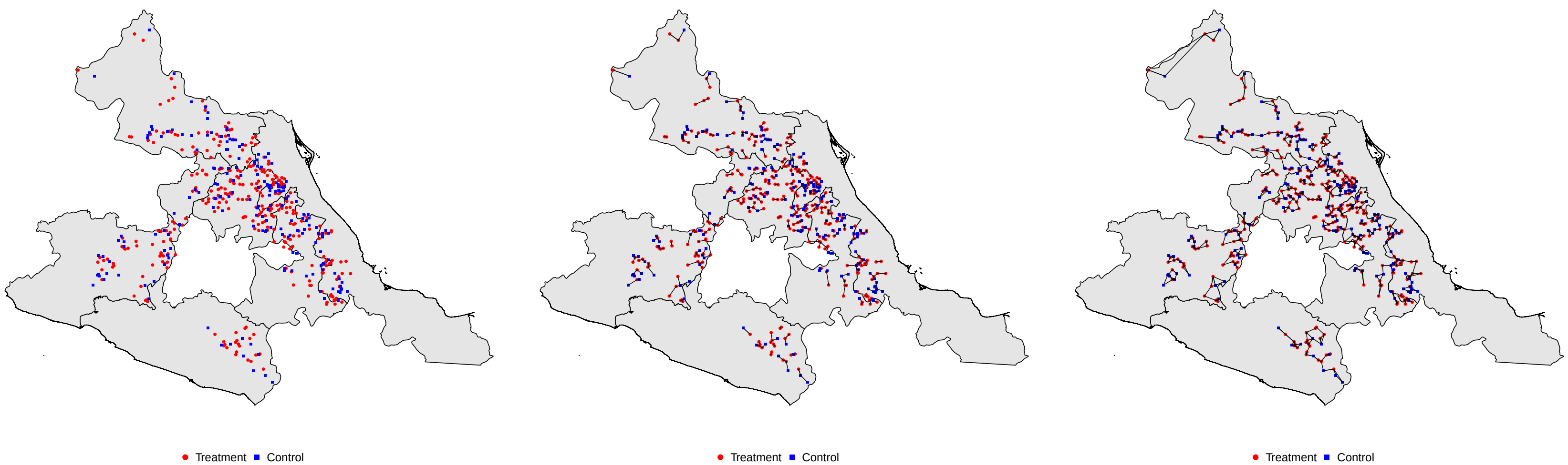
Source: Authors' calculations from 1,000 Monte Carlo Simulations.

Figure 3.4. Alternative specification performance for spatially-correlated unobservable



Source: Authors' calculations from 1,000 Monte Carlo Simulations.

Figure 3.5. Progresa village locations and selected spatial networks



Village locations | 1 nearest neighbor network | 2 nearest neighbor network

Source: Authors' elaboration from official shapefiles from the Instituto Nacional de Estadística, Geografía e Informática (INEGI).
Notes: Progresa villages were identified by using the state, municipality, and village identifiers in the official program data downloaded from SEDESOL. Lines between villages denote the that two locations are neighbors.

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Appendix A

Supplementary material for Chapter 1

Table A.1. Real hourly minimum wages by industry firm-size categories

Category	Firm size	2005	2006	2007	2008	2009	2010	2011	2012
Agriculture	1-15	6.01	6.43	6.69	6.55	10.43	10.10	9.77	9.88
	16+	7.78	8.12	8.31	8.20	10.97	11.10	10.28	10.48
Non-metallic mining	1-15	6.60	7.06	7.35	7.26	12.16	11.15	13.35	13.49
	16+	8.28	8.64	8.84	8.80	12.51	11.68	14.13	14.21
Metallic mining	All	9.59	10.01	9.96	9.57	11.09	10.30	14.26	13.99
Manufacturing	1-15	6.60	7.06	7.35	7.26	12.48	11.88	13.69	14.61
	16+	8.28	8.64	8.84	8.80	12.70	13.00	14.23	14.21
Utilities	All	9.59	10.01	9.96	9.92	12.19	12.09	14.45	14.75
Construction	1-15	6.60	7.06	7.35	7.26	12.29	11.73	14.14	15.11
	16+	8.28	8.64	8.84	8.80	12.83	12.63	14.05	14.11
Retail	1-15	6.60	7.06	7.35	7.26	12.86	12.37	14.00	14.83
	16+	8.28	8.64	8.84	8.80	13.21	13.19	14.17	14.31
Transport	1-15	7.52	8.04	8.37	8.19	12.51	11.73	14.07	14.86
	16+	8.03	8.38	8.57	8.54	13.09	13.27	14.24	14.41
Real Estate	1-15	7.52	8.04	8.37	8.19	12.97	13.06	14.26	15.00
	16+	8.03	8.38	8.57	8.54	12.89	13.63	14.72	14.61
Business Services	1-15	5.35	5.15	4.83	4.30	13.09	12.63	14.26	14.94
	16+	6.20	5.97	5.60	4.98	13.19	13.15	14.54	14.63
Financial Services	1-15	9.59	10.12	10.63	10.64	13.22	12.42	14.26	15.00
	16+	9.59	10.12	10.54	10.54	13.41	13.61	14.75	14.99
Communal and Personal Services	1-15	6.60	7.06	7.35	7.26	12.63	12.12	14.08	15.18
	16+	8.28	8.64	8.84	8.80	13.19	12.84	13.79	13.88
Export	All	9.59	10.01	9.96	9.57	9.02	9.25	9.80	9.94

Source: Honduran minimum wage decrees.
Notes: Real minimum wages are calculated from monthly values as Hourly MW=(Monthly MW/44 x 4.3) following Gindling and Terrell (2009). Values are expressed in real Lempiras. The average real exchange rate for the period is 10 Lempiras per $1 USD.

Table A.2. Macroeconomic and labor market indicators, 2005-2012

Year	GDP growth (real)	Inflation rate (%)	Labor force participation	Employment rate	Unemployment rate
2005	6.1	8.8	61.7	58.7	4.9
2006	6.6	5.6	59.9	57.7	3.6
2007	6.2	6.9	58.9	57.1	3.1
2008	4.2	11.4	59.0	57.2	3.1
2009	-2.4	5.5	61.3	59.3	3.3
2010	3.7	4.7	61.9	59.4	4.1
2011	3.8	6.8	59.9	57.2	4.4
2012	4.1	5.2	58.4	56.2	3.7

Source: Honduran Central Bank and EPHPM surveys.
Notes: Growth is calculated using constant GDP levels at December 1999 prices, the inflation rate denotes percentage changes in prices (inter-annual variation in December), and labor market indicators are weighted averages computed from individual-level EPHPM survey data for adults ($\geq$15 years old).

Table A.3. Changes in legal minimum wages by electoral preferences (municipal-level data)

	(1)	(2)	(3)
2006×Voted for Zelaya in 2005 election	-0.009	-0.005	0.000
	(0.008)	(0.006)	(0.006)
2007×Voted for Zelaya in 2005 election	-0.002	-0.005	-0.001
	(0.009)	(0.006)	(0.006)
2008×Voted for Zelaya in 2005 election	-0.003	-0.003	-0.001
	(0.009)	(0.006)	(0.006)
2009×Voted for Zelaya in 2005 election	0.007	0.007	0.009
	(0.011)	(0.008)	(0.008)
2010×Voted for Zelaya in 2005 election	-0.013	-0.000	-0.002
	(0.015)	(0.010)	(0.010)
2011×Voted for Zelaya in 2005 election	0.003	0.003	0.005
	(0.011)	(0.009)	(0.007)
2012×Voted for Zelaya in 2005 election	0.022	0.022	0.011
	(0.014)	(0.010)**	(0.007)
Municipality effects	Yes	Yes	Yes
Survey wave effects	Yes	Yes	Yes
Share of workers in each MW category	No	Yes	Yes
Linear time trend in Share of Workers per Category	No	No	Yes
R^2	0.946	0.968	0.974
N	3,514	3,514	3,514

Source: Own calculations from EPHPM surveys aggregated to the municipal-level and electoral results.
Notes: Each column is a separate regression. Clustered standard errors by municipality in parentheses. Estimates correspond to $\hat{\beta}$ from the following regression: $\log MW_{dt} = \alpha + \beta_t(Zelaya_d \times \delta_t) + \phi_d + \bar{\mu}_j + \delta_t + u_{dt}$. Covariates include the share of males, average age, mean years of education, mean potential experience and its square, fraction of urban residents, and the share of workers in each industry firm-size category.
*** Significant at 1 percent, ** 5 percent, * 10 percent.

Table A.4. Valid minimum wage decrees at each survey wave

Year	Wave	Decree Number	Setting	Effective date
2005	September	STSS-029-05	Negotiated	January 1, 2005
2006	May September	027-STSS-06	Negotiated	January 1, 2006
2007	May September	STSS-041-07	Set unilaterally	January 1, 2007
2008	May September	STSS-258-07	Set unilaterally	January 1, 2008
2009	May	STSS-374-08	Set unilaterally	January 1, 2009
2010	May September	STSS-342-2010	Set unilaterally	September 1, 2010
2011	May September	STSS-223-2011	Set unilaterally	January 1, 2011
2012	May	STSS-001-2012	Negotiated	January 1, 2012

Source: National Statistics Institute (INE) and General Directorate of Wages (DGS).
Notes: Minimum wage decrees are available at http://www.trabajo.gob.hn/?page_id=921.

Table A.5. Yearly changes in real hourly minimum wages by industry categories

Category	2006	2007	2008	2009	2010	2011	2012
Agriculture	5.5	3.1	-1.7	45.2	-1.0	-5.4	1.5
Non-metallic mining	5.5	3.1	-0.8	53.6	-7.4	20.3	0.8
Metallic mining	4.4	-0.5	-4.0	16.0	-7.1	38.4	-1.9
Manufacturing	5.5	3.1	-0.8	56.8	-1.2	12.2	3.2
Utilities	4.4	-0.5	-0.4	22.9	-0.8	19.5	2.1
Construction	5.5	3.1	-0.8	56.4	-3.0	15.7	3.7
Retail	5.5	3.1	-0.8	62.3	-1.9	10.2	3.4
Transport	5.6	3.2	-1.3	53.1	-2.3	13.2	3.4
Real Estate	5.6	3.2	-1.3	54.7	3.2	8.6	2.2
Business Services	-3.7	-6.2	-11.1	183.1	-1.9	11.7	2.7
Financial Services	5.5	4.6	0.0	25.8	-2.3	11.4	3.4
Communal and Personal Services	5.5	3.1	-0.8	60.7	-3.3	11.6	4.3
Export	4.4	-0.5	-4.0	-5.7	2.6	6.0	1.4
Average	**4.4**	**1.5**	**-2.3**	**60.4**	**-2.3**	**14.4**	**2.1**

Source: Own calculations from real hourly minimum wages aggregated at the industry-level.
Notes: The table shows percentage changes in legal minimum wages relative to the previous year.

Table A.6. Descriptive statistics by occupation, averages for 2005-2012

	Wage earners	Public sector	Domestic workers	Self-employed	Unpaid workers	Employers
Employment, hours, and wages						
Share of sample	0.393	0.067	0.028	0.396	0.093	0.024
Hours per week	44.62	39.13	49.16	34.52	30.66	44.54
Share full-time ($\geq$44 hpw)	0.622	0.277	0.670	0.310	0.191	0.558
Real Hourly Wages	11.08	28.03	4.91	10.06		24.71
Household poverty status						
Extremely Poor	0.307	0.075	0.319	0.478	0.562	0.158
Poor	0.562	0.247	0.562	0.676	0.730	0.320
Individual & household characteristics						
Males	0.729	0.450	0.071	0.626	0.649	0.722
Married	0.483	0.595	0.216	0.700	0.215	0.756
Years of education	6.92	11.85	5.38	4.53	6.23	8.50
Potential experience	18.1	21.0	19.5	33.5	12.1	29.3
Household size	5.47	5.00	5.54	5.26	6.45	4.95
Is household head	0.392	0.451	0.200	0.612	0.032	0.671
Lives in urban area	0.569	0.794	0.656	0.386	0.286	0.681
Composition across industries						
Agriculture	0.275	0.001	0.001	0.441	0.589	0.238
Non-metallic mining	0.003	0.000	0.000	0.002	0.001	0.003
Metallic mining	0.001	0.000	0.000	0.000	0.000	0.000
Manufacturing	0.122	0.002	0.001	0.094	0.086	0.135
Utilities	0.005	0.036	0.000	0.000	0.000	0.001
Construction	0.105	0.004	0.001	0.051	0.010	0.028
Retail	0.202	0.001	0.002	0.271	0.269	0.413
Transport	0.042	0.036	0.000	0.037	0.005	0.041
Real Estate	0.003	0.000	0.000	0.001	0.000	0.005
Business Services	0.036	0.005	0.002	0.011	0.004	0.055
Financial Services	0.026	0.010	0.000	0.001	0.000	0.003
Communal and Personal Services	0.088	0.904	0.992	0.065	0.025	0.061
Export	0.093	0.001	0.000	0.026	0.010	0.015
N	134,190	23,375	9,411	124,829	27,680	8,279

Source: Own calculations from EPHPM surveys.
Notes: All statistics are weighted. Wages are expressed in real Lempiras. The average real exchange rate for the period is 10 Lempiras per $1 USD.

Table A.7. Compliance with legal minimum wages (Monthly values)

	Covered sector				Uncovered sector	
	Large firm wage earners	Small firm wage earners	Public sector workers	Domestic workers	Self-employed	Employers
A. Incidence measures						
Below MW	0.237	0.661	0.104	0.628	0.731	0.284
At MW	0.221	0.101	0.095	0.097	0.055	0.068
Above MW	0.542	0.238	0.801	0.275	0.214	0.648
B. Depth measures						
Shortfall from MW	0.368	0.550	0.443	0.506	0.715	0.582
C. Changes over time						
(i) Share below MW						
Pre (2005-2008)	0.195	0.546	0.075	0.465	0.620	0.173
Post (2009-2012)	0.290	0.778	0.134	0.796	0.824	0.393
Difference	0.095	0.232	0.059	0.331	0.204	0.220
H_0 : Pre = Post	0.007	0.005	0.000	0.000	0.001	0.000
(ii) Shortfall from MW						
Pre (2005-2008)	0.253	0.466	0.365	0.366	0.665	0.490
Post (2009-2012)	0.258	0.543	0.303	0.531	0.705	0.531
Difference	0.005	0.077	-0.062	0.165	0.040	0.041
H_0 : Pre = Post	0.801	0.000	0.010	0.000	0.135	0.035

Source: Own calculations from individual EPHPM surveys.

Notes: Incidence measures denote shares of full-time workers. *Below* includes individuals with earnings less than 0.90 of the monthly MW; *at* counts those earning between [0.90,1.10] of the monthly MW, and *above* refers to those earning more than 1.10 times the monthly MW. The depth of non-compliance is calculated as the shortfall indicator (Bhorat et al., 2013a), which measures how far actual earnings are from monthly minimum wages. Changes in Share Below MW over time are calculated by regression. Differences in the Shortfall from MW are estimated by block bootstrap with 100 replications. Reported p-values are drawn from t-tests where the null hypothesis is that non-compliance rates and depth are unchanged over time.

Table A.8. Alternative specifications for labor market outcomes

		Sector		Occupation					
	Full Sample	Covered	Uncovered	Wage earners	Public sector	Domestic workers	Self-employed	Unpaid workers	Employers
A. Employment rate									
(1) OLS	-0.099								
	(0.018)***								
(2) OLS	-0.111								
	(0.018)***								
(1) IV	-0.132								
	(0.027)***								
F-statistic	1,788.1								
(2) IV	-0.148								
	(0.031)***								
F-statistic	485.9								
Observations	327,764								
B. Labor force composition									
(1) Multinomial Probit		-0.350	0.254						
		(0.094)***	(0.085)***						
(2) Multinomial Probit		-0.431	0.307						
		(0.087)***	(0.091)***						
Observations		327,764							
C. Log Hours per week									
(1) IV	-0.512	-0.205	-0.226	-0.111	-0.319	-0.441	-0.165	-0.682	-0.252
	(0.174)***	(0.072)***	(0.185)	(0.032)***	(0.011)***	(0.003)***	(0.204)	(0.149)***	(0.183)
F-statistic	1,634.5	4,525.8	31.7	4,250.8	1,178.9	913,810.3	33.0	8.7	61.5
(2) IV	-0.591	-0.225	-0.675	-0.121	-0.328	-0.443	-0.563	-2.220	0.097
	(0.179)***	(0.075)***	(0.259)***	(0.032)***	(0.006)***	(0.002)***	(0.337)*	(0.667)***	(0.671)
F-statistic	494.6	1,234.2	66.2	891.0	641.1	1,603,362.0	54.1	23.6	35.5
Observations	305,441	153,695	151,746	123,173	21,797	8,725	116,955	26,930	7,861
D. Log Hourly Wages									
(1) IV	0.220	0.272	-0.360	0.153	0.155	0.084	-0.352		1.252
	(0.158)	(0.098)***	(0.341)	(0.043)***	(0.041)***	(0.005)***	(0.353)		(0.569)**
F-statistic	1,934.1	4,485.3	28.9	4,213.2	1,229.3	1,103,095.3	27.1		51.6
(2) IV	0.275	0.306	0.295	0.172	0.168	0.085	0.335		4.449
	(0.179)	(0.108)***	(0.370)	(0.056)***	(0.028)***	(0.006)***	(0.376)		(1.533)***
F-statistic	554.6	1,221.8	61.8	885.0	682.8	1,743,636.0	56.5		32.3
Observations	261,004	151,769	109,235	121,669	21,426	8,674	102,172		7,063

Source: Own calculations from EPHPM surveys.

Notes: See notes for Table 1.6. (1) controls for industry (or industry firm-size) and survey wave fixed effects and (2) includes linear category time trends. IV specifications use minimum wages lagged one year as an instrument. The table reports first-stage F-statistics. All coefficients are marginal effects.

*** Significant at 1 percent, ** 5 percent, * 10 percent.

Table A.9. Effects of legal minimum wages on labor market outcomes (industry firm-size panel data)

		Sector		Occupation				
	Full Sample	Covered	Uncovered	Wage earners	Public sector	Self-employed	Unpaid workers	Employers
A. Log Employment								
FE	-0.065	-0.225	0.110	-0.170	0.877	0.171	-0.771	0.256
	(0.157)	(0.222)	(0.223)	(0.207)	(0.589)	(0.333)	(0.434)	(0.226)
	299	299	257	299	128	232	129	226
FE-LDV	-0.080	-0.254	0.190	-0.212	0.085	0.066	-0.490	0.272
	(0.198)	(0.243)	(0.228)	(0.238)	(1.012)	(0.235)	(0.469)	(0.248)
	276	276	225	276	98	199	108	188
GMM-DIF	-0.064	-0.258	0.194	-0.210	0.114	0.077	-0.549	0.221
	(0.220)	(0.259)	(0.218)	(0.255)	(1.124)	(0.246)	(0.545)	(0.237)
	253	253	203	253	85	178	96	167
B. Log Hours per week								
FE	-0.041	-0.064	-0.259	-0.059	-0.013	-0.086	0.118	-0.180
	(0.039)	(0.071)	(0.199)	(0.070)	(0.083)	(0.160)	(0.206)	(0.099)*
	299	299	257	299	128	231	129	226
FE-LDV	-0.026	-0.054	-0.284	-0.049	0.060	0.038	0.036	-0.302
	(0.045)	(0.077)	(0.293)	(0.078)	(0.111)	(0.146)	(0.206)	(0.145)**
	276	276	225	276	98	198	108	188
GMM-DIF	-0.027	-0.052	-0.310	-0.046	0.048	0.047	0.047	-0.264
	(0.045)	(0.077)	(0.342)	(0.078)	(0.152)	(0.131)	(0.247)	(0.144)*
	253	253	203	253	85	178	96	167
C. Log Hourly Wages								
FE	0.198	0.215	-0.159	0.204	0.199	0.263		0.050
	(0.068)***	(0.113)*	(0.265)	(0.115)*	(0.236)	(0.392)		(0.392)
	299	299	256	299	128	231		222
FE-LDV	0.233	0.256	0.051	0.249	-0.115	0.355		-0.165
	(0.071)***	(0.117)**	(0.270)	(0.120)**	(0.298)	(0.398)		(0.399)
	276	276	224	276	98	198		183
GMM-DIF	0.245	0.262	0.063	0.253	-0.064	0.365		-0.270
	(0.066)***	(0.117)**	(0.288)	(0.120)**	(0.370)	(0.422)		(0.366)
	253	253	202	253	85	178		163

Source: Own calculations from EPHPM surveys aggregated to the industry firm-size level.

Notes: Clustered standard errors by industry firm-size categories in parentheses. Panel A uses the log of weighted employment for each industry firm-size category. Panels B and C use weighted means of hours and wages. The specifications are: the within estimator (FE), the within estimator including a lag of the dependent variable (FE-LDV), and Arellano-Bond dynamic panel estimator that uses lags of the dependent variable as instruments (GMM-DIF). All regressions control for the share of males, average years of education, mean potential experience and its square, share of urban workers, the logarithm of industry-level IMAE index (by month) and value added (by year), industry-firm size effects, survey wave effects, and linear category-specific time trends.

*** Significant at 1 percent, ** 5 percent, * 10 percent.

Table A.10. Alternative specifications for poverty

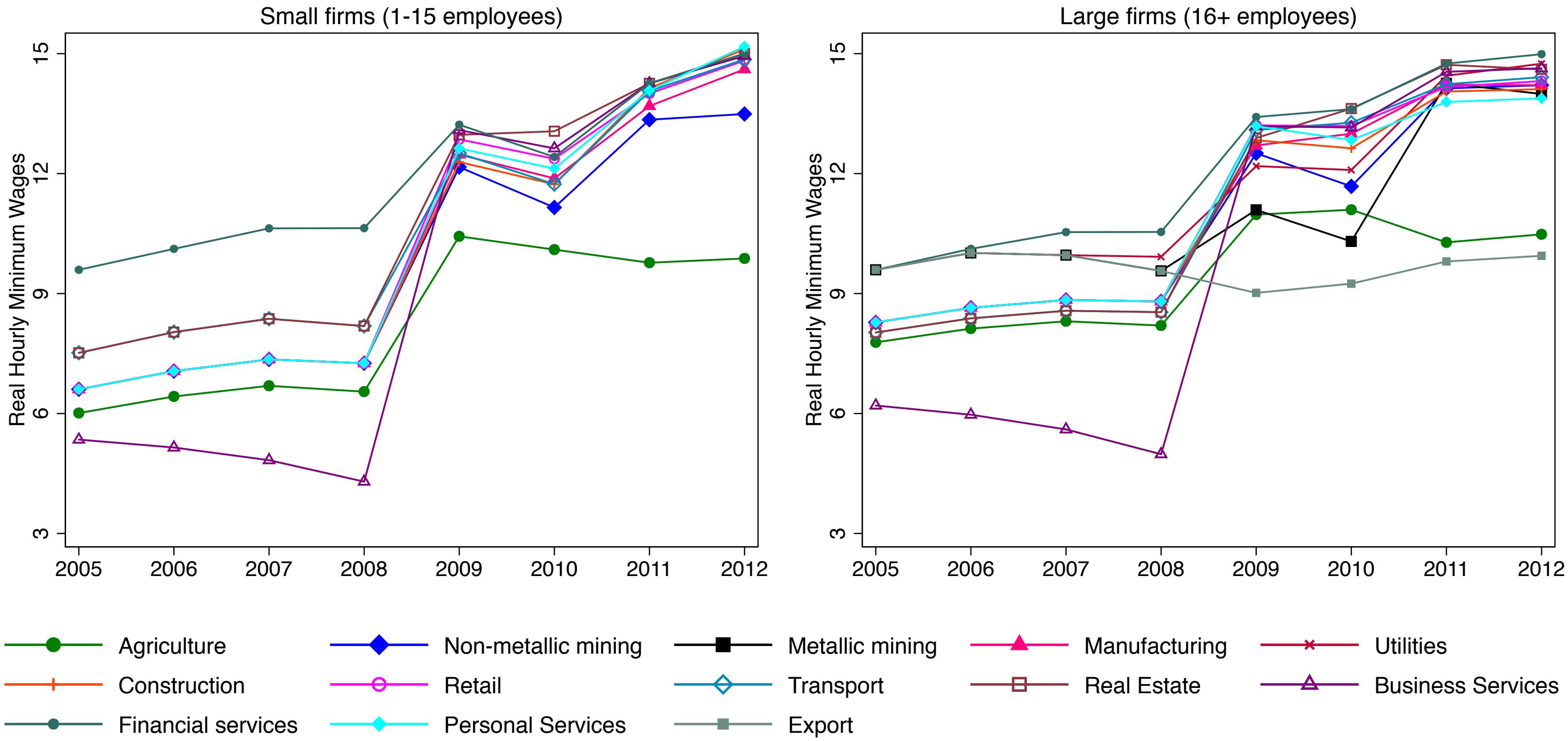
		Sector		Occupation					
	Full Sample	Covered	Uncovered	Wage earners	Public sector	Domestic workers	Self-employed	Unpaid workers	Employers
A. Extreme poverty									
(1) OLS	0.044	-0.008	0.176	-0.012	0.090	0.277	0.179	0.136	0.185
	(0.021)*	(0.017)	(0.026)***	(0.019)	(0.007)***	(0.003)***	(0.033)***	(0.032)***	(0.034)***
(2) OLS	0.039	-0.007	0.082	-0.012	0.096	0.277	0.087	0.056	0.104
	(0.030)	(0.021)	(0.034)**	(0.026)	(0.004)***	(0.003)***	(0.035)**	(0.020)**	(0.064)
(1) IV	0.027	-0.024	0.305	-0.033	0.108	0.293	0.333	0.329	-0.133
	(0.035)	(0.028)	(0.085)***	(0.034)	(0.004)***	(0.002)***	(0.111)***	(0.093)***	(0.147)
F-statistic	2,067.5	5,789.6	40.4	3,405.1	2,014.9	15,527,689.2	39.0	9.5	80.1
(2) IV	0.027	-0.022	0.494	-0.032	0.111	0.293	0.651	0.542	-0.581
	(0.043)	(0.032)	(0.191)***	(0.041)	(0.003)***	(0.002)***	(0.201)***	(0.327)*	(0.264)**
F-statistic	537.8	1,363.6	71.1	784.2	1,255.7	11,493,050.9	61.2	29.9	40.8
Observations	313,852	165,035	148,817	133,156	23,137	9,364	115,674	24,653	7,868
B. Poverty									
(1) OLS	0.044	0.015	0.141	-0.003	0.081	0.339	0.147	0.075	0.076
	(0.026)	(0.026)	(0.026)***	(0.013)	(0.007)***	(0.001)***	(0.031)***	(0.051)	(0.025)**
(2) OLS	0.047	0.024	0.067	0.006	0.085	0.339	0.081	0.022	-0.021
	(0.032)	(0.027)	(0.024)**	(0.017)	(0.005)***	(0.001)***	(0.029)**	(0.027)	(0.058)
(1) IV	0.037	0.009	0.253	-0.015	0.117	0.361	0.261	0.188	-0.082
	(0.034)	(0.032)	(0.085)***	(0.022)	(0.011)***	(0.002)***	(0.094)***	(0.113)*	(0.086)
F-statistic	2,067.5	5,789.6	40.4	3,405.1	2,014.9	15,527,689.2	39.0	9.5	80.1
(2) IV	0.044	0.018	0.419	-0.005	0.119	0.362	0.499	0.282	-0.339
	(0.040)	(0.034)	(0.257)	(0.028)	(0.014)***	(0.002)***	(0.292)*	(0.252)	(0.274)
F-statistic	537.8	1,363.6	71.1	784.2	1,255.7	11,493,050.9	61.2	29.9	40.8
Observations	313,852	165,035	148,817	133,156	23,137	9,364	115,674	24,653	7,868

Source: Own calculations from EPHPM surveys.

Notes: See notes for Table 1.7. (1) controls for industry (or industry firm-size) and survey wave fixed effects and (2) includes linear category time trends. IV specifications use minimum wages lagged one year as an instrument. The table reports first-stage F-statistics. All coefficients are marginal effects.

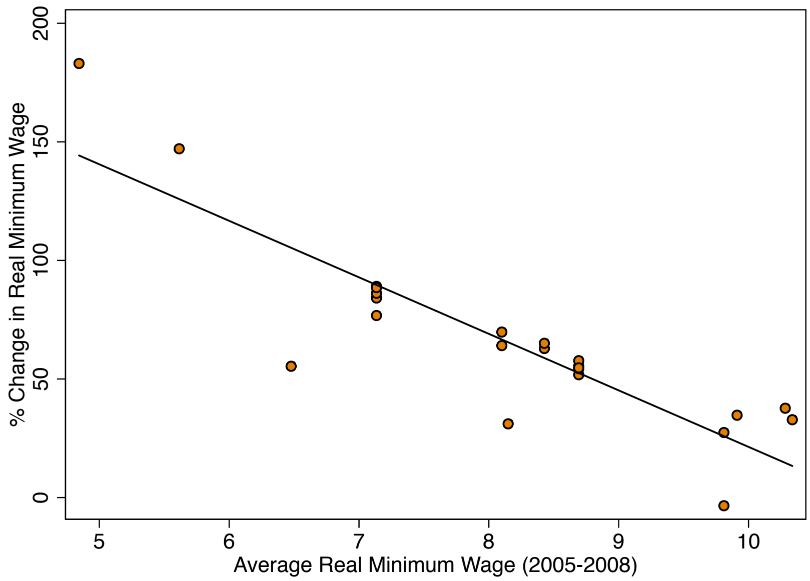
*** Significant at 1 percent, ** 5 percent, * 10 percent.

Figure A.1. Trends in minimum wages by industry firm-size categories



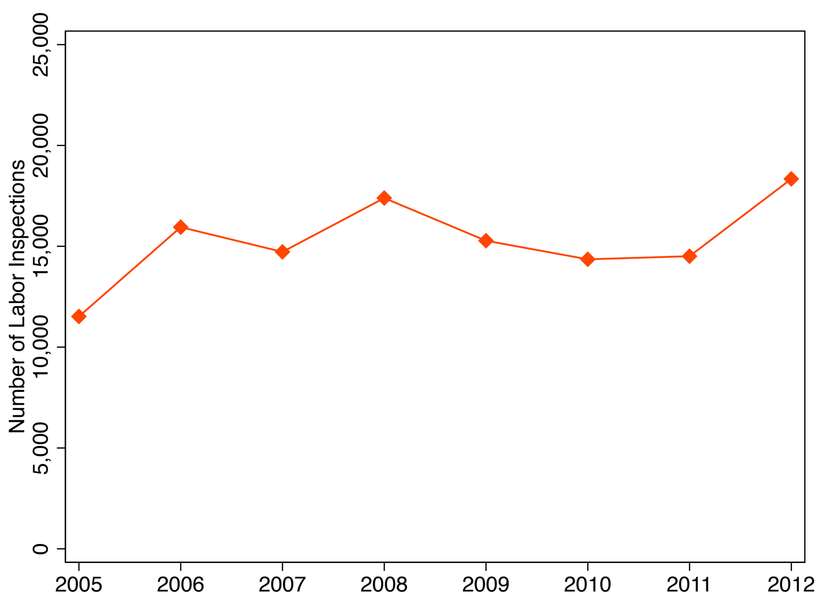
Source: Own elaboration from Honduran minimum wage decrees.

Figure A.2. Minimum wage changes by industry firm-size category before and after 2009



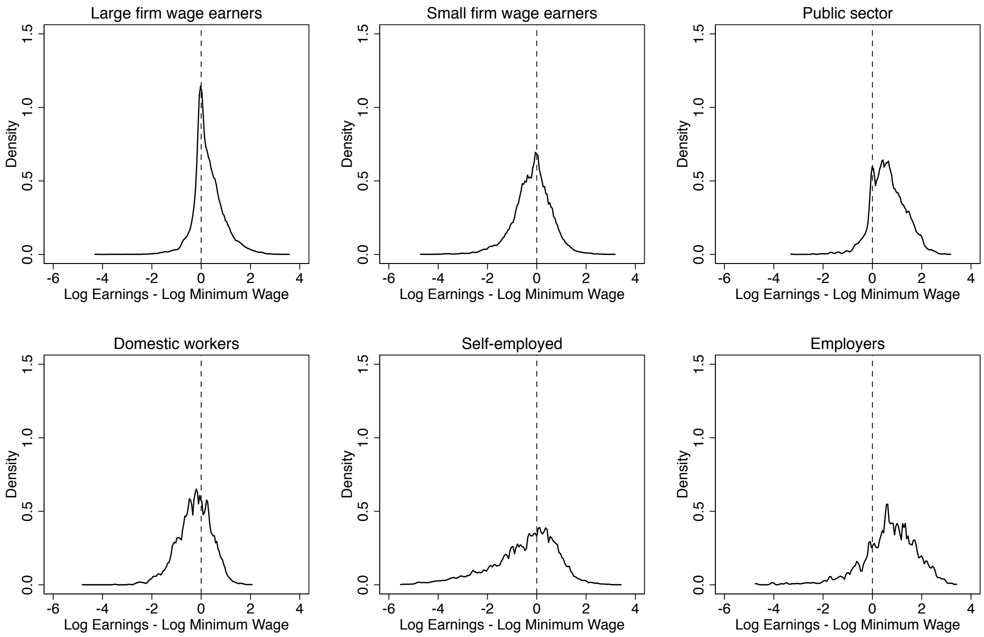
Source: Own calculations from EPHPM surveys aggregated to the industry firm-size level.

Figure A.3. Number of labor inspections, 2005-2012



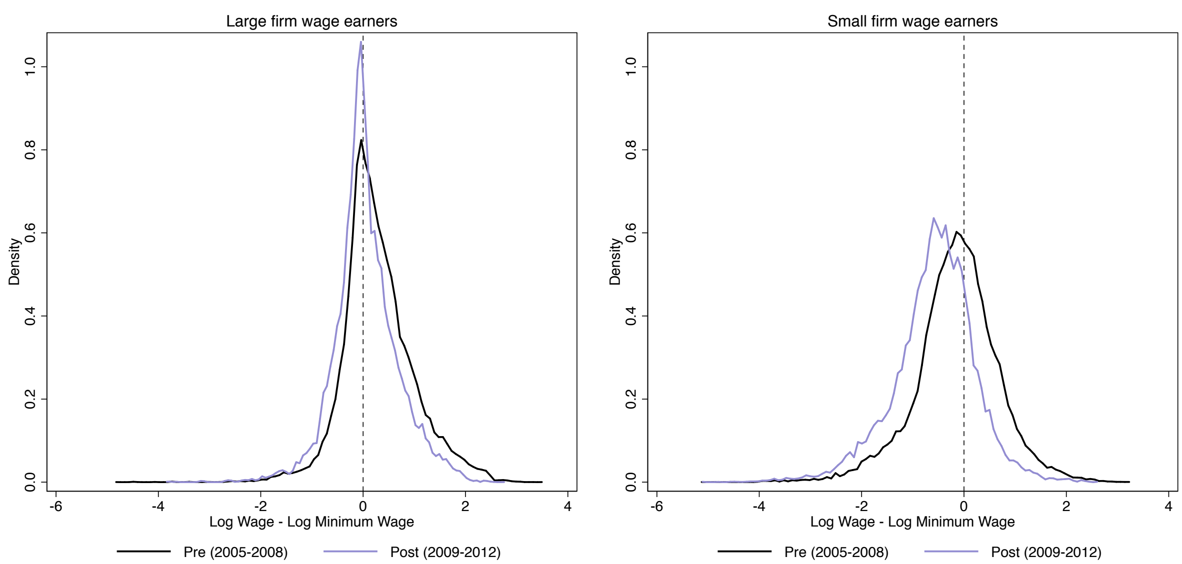
Source: Honduran Ministry of Labor (UPEG, 2016).

Figure A.4. Kernel densities of log earnings minus log monthly minimum wages



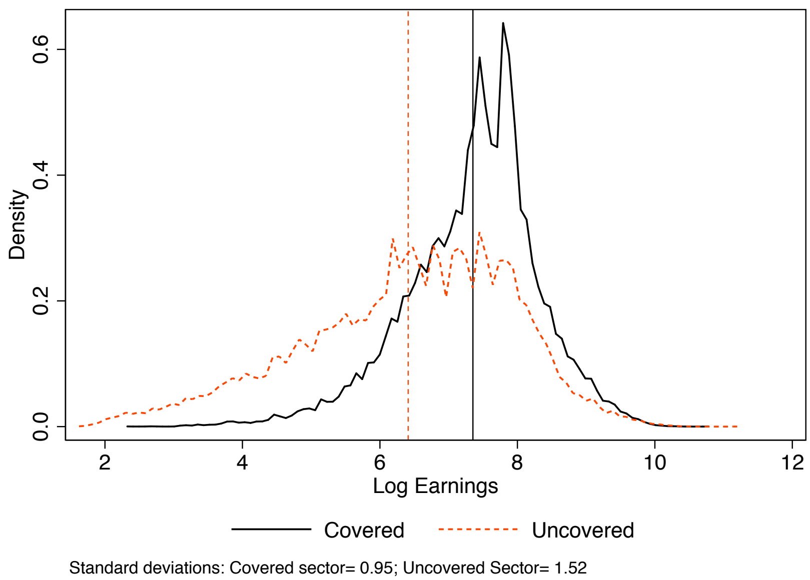
Source: Own calculations from EPHPM surveys.
Notes: These densities are average distributions from 2005-2012 and are centered so that $MW = 0$.

Figure A.5. Kernel densities of log wages minus log minimum wages, before and after 2009



Source: Own calculations from EPHPM surveys.
These densities are centered so that $MW = 0$.

Figure A.6. Kernel densities of log earnings by sector



Source: Own calculations from EPHPM surveys.
Notes: These densities are average distributions from 2005-2012.

Appendix B

Supplementary material for Chapter 2

UNIVERSITY OF ILLINOIS
AT URBANA-CHAMPAIGN

Office of Vice Chancellor for Research

Institutional Review Board
528 East Green Street
Suite 203
Champaign, IL 61820

February 14, 2013

Richard Akresh
Economics
470E Wohlers Hall
1206 South Sixth Street
M/C 706

RE: *The role of information on students' career choice and school effort*
IRB Protocol Number: 13570

Dear Dr. Akresh:

Thank you for submitting the completed IRB application form for your project entitled *The role of information on students' career choice and school effort*. Your project was assigned Institutional Review Board (IRB) Protocol Number 13570 and reviewed. It has been determined that the research activities described in this application meet the criteria for exemption at 45CFR46.101(b)(1).

This determination of exemption only applies to the research study as submitted. **Exempt protocols are approved for a maximum of three years.** Please note that additional modifications to your project need to be submitted to the IRB for review and exemption determination or approval before the modifications are initiated.

We appreciate your conscientious adherence to the requirements of human subjects research. If you have any questions about the IRB process, or if you need assistance at any time, please feel free to contact me or the IRB Office, or visit our website at http://www.irb.illinois.edu.

Sincerely,

Dustin L. Yocum

Dustin L. Yocum, Human Subjects Research Exempt Specialist, Institutional Review Board

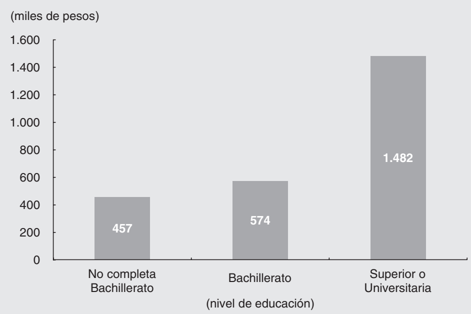
c: Leonardo Bonilla
Andres Ham Gonzalez

Student Handout

¡La educación superior paga!

La relación entre estudios e ingresos

La educación superior es un factor determinante de la situación económica y por tanto la calidad de vida de las familias. En el siguiente gráfico se presentan los salarios promedio por nivel educativo en Bogotá.



Fuente: Encuesta de Hogares 2011, DANE

Como se puede observar, mayor educación se traduce en salarios más altos. Sólo con terminar el Bachillerato se pasa de ganar 457.000 a 574.000 por mes. El salto es más evidente para aquellos con un título de nivel superior, ya que el salario promedio mensual crece a 1.482.000. Estas estadísticas presentan un mensaje claro: vale la pena estudiar.

¿Cómo puedo averiguar cuanto ganaría en la carrera que a mí me interesa?

Es probable que usted ya tenga una idea sobre las carreras que le interesarían y la institución donde quisiera realizar estos estudios. Si es así, ¿hay alguna manera de saber cuánto puede esperar ganar en su situación específica?

Existen dos lugares donde pueden consultar el salario promedio de los graduados por institución y carreras. Estas son:

1. Calculadora de salarios promedios para graduados: www.finanzaspersonales.com.co

Esta página cuenta con una herramienta que le permite consultar el salario promedio por región, institución educativa, programa de estudio y género de las personas que obtuvieron su título entre 2001-2011.

¿Cómo funciona?

- Acceda al enlace y busque la *Calculadora de Salario por profesión para Graduados*
- Escoja la región donde quiere realizar la búsqueda (por ejemplo, Bogotá)
- Seleccione la institución donde quiere realizar sus estudios y el programa que planea cursar

2. Observatorio laboral del Ministerio de Educación: www.graduadoscolombia.edu.co

Está página también provee información sobre los salarios promedios de personas con título de educación superior para toda Colombia. Además, le permite conocer las perspectivas laborales del programa de estudio de su interés.

¿Cómo funciona?

- Acceda al enlace y busque el botón rojo que dice *Sistema de información del Observatorio Laboral*.
- Si quiere conocer el número de graduados por carrera, acceda a la pestaña que dice "Perfil nacional". Después, escoja el departamento donde planea estudiar y obtendrá los datos de graduados por área de estudio.

Si desea saber cuántos individuos en su área de interés tienen un empleo formal (cotizando a la seguridad social) y cuanto ganan en promedio vaya a ¨Vinculación laboral recién graduados¨. Aquí tiene la opción de buscar por institución o por carrera.

Recuerde que estas páginas le permiten conocer el salario promedio de los profesionales graduados en su área de interés.

¿Qué necesito para entrar a la Universidad y la carrera que me interesa?

1. Buenos resultados académicos: Uno de los criterios más importantes a la hora de buscar admisión a una institución de educación superior es el rendimiento académico. Muchas instituciones utilizan el puntaje del ICFES (SABER 11), y otras instituciones como la Universidad Nacional que tienen su propio examen de admisión. En cualquier caso, estudiar aumenta las posibilidades de ser admitido y también las posibilidades de acceder a becas o financiación.

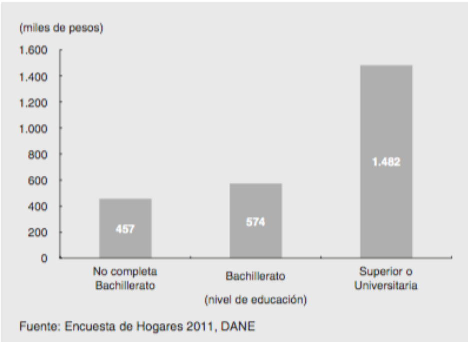
2. Financiación: Existen varias maneras de financiar la educación superior en Colombia. En general, tendrán preferencia los alumnos de escasos recursos y buen desempeño académico. Las siguientes son algunas opciones a tener en cuenta:

- Becas proveídas por cada institución por mérito académico y/o escasos recursos. Consulte las políticas de beca ya que estas son diferentes para cada institución.
- ICETEX: http://www.icetex.gov.co
- Secretaría de Educación de Bogotá (Banco de cupos, Fondo de Financiamiento de Educación Superior de Bogotá): http://www.sedbogota.edu.co/index.php/educacion-superior.html

Post-secondary education pays!

The relation between studies and income

Higher education is a determining factor of wages and the quality of life of families. The following figure presents average wages by level of completed education in Bogotá:



Clearly, more education is related with higher wages. By only finishing high school, wages move from 457,000 to 574,000 pesos each month. The difference is even more marked for those with a college degree, since their average monthly wage increases to 1,492,000. These statistics present a clear pattern: studying is worth it.

How can I learn about how much people earn who finished the degree I'm interested in?

It is very likely that you already have a good idea about the degrees and institutions where you would like to pursue your studies. If this is true, is there a way to know how much I could expect to earn?

There are two places where you can obtain information on average wages for graduates by institution and degree. These are:

1. Average wage calculator for graduates: www.finanzaspersonales.com.co

This website counts with a tool that allows to calculate average wages by region, institution, degree and gender of people who graduated between 2001 and 2011.

How does it work?

- Visit the website and search for *Wage calculator by degree for Graduates.*
- Select the region where you are interested in searching (e.g. Bogotá)
- Select the institution and the degree you are interested in evaluating

2. Labor Observatory of the Ministry of Education: www.graduadoscolombia.edu.co

This website also provides information about average wages for the whole country. Additionally, you can learn about the labor prospects for your degree of interest

How does it work?

- Visit the website and click on the red button reading *Information System of the Labor Observatory*
- If you would like to know the number of graduates by degree, click on the "National Profile" tab. Next, select the department where you plan to study and you will find data on graduates by degree.

If you are interested in the number of individuals who pursued your degree of interest who have a formal job (paying social security) and how much they earn on average, select "*labor link of recent graduates"*. Here you have the option to search by institution and degree.

Remember that these websites allow to learn about the average wages of recent graduates for your degree of interest.

What will I need to enroll in a University and in my degree of interest?

1. **Good academic results**: One of the main criteria for admissions in University if academic performance. Many institutions use the ICFES (SABER 11) score, and other institutions like the National University also have their own admissions test. Nevertheless, studying will increase the probability of being admitted and also of obtaining financial aid or financing.
2. **Financing:** There are many ways to finance higher education in Colombia. In general, financing institutions have preferences for students of low income and good academic performance. The following are some organizations to keep in mind:

- Scholarships provided by each institution according to academic merit of financial need. Consult the scholarship policies for each institution given that they may differ.
- ICETEX: http://www.icetex.gov.co
- Secretary of Education in Bogotá (FDFESBO): http://www.sedbogota.edu.co/index.php/ed- ucacion-superior.html

Table B.1. Descriptive statistics for universe of students in Bogotá, by public and private schools

	Public schools		Private schools		Difference
	Mean	(SD)	Mean	(SD)	p-value
A. Student attributes					
Male	0.458	(0.498)	0.492	(0.500)	0.007
Age	17.641	(0.873)	17.648	(0.907)	0.825
Parent completed secondary	0.395	(0.489)	0.288	(0.453)	0.000
Parent completed higher education	0.156	(0.363)	0.580	(0.494)	0.000
Family income (<1 minimum wage)	0.144	(0.351)	0.028	(0.165)	0.000
Family income (1-2 minimum wages)	0.559	(0.497)	0.246	(0.431)	0.000
Family income (>2 minimum wages)	0.297	(0.457)	0.726	(0.446)	0.000
B. SABER 11 exit exam					
Overall Score	0.138	(0.841)	0.864	(1.192)	0.000
Math	0.046	(0.884)	0.708	(1.231)	0.000
Language	0.156	(0.870)	0.702	(1.060)	0.000
C. Higher education choices					
Enrolled	0.426	(0.495)	0.571	(0.495)	0.000
Academic degree (4-year)	0.098	(0.298)	0.370	(0.483)	0.000
Vocational degree (2-year)	0.328	(0.469)	0.201	(0.400)	0.000
Public College	0.278	(0.448)	0.147	(0.354)	0.000
Private College	0.148	(0.355)	0.424	(0.494)	0.000
Top-10 College	0.011	(0.106)	0.160	(0.366)	0.000
STEM field	0.054	(0.227)	0.211	(0.408)	0.000
Total number of students	37,787		37,068		
Total number of schools	570		790		

Source: Authors' calculations from administrative data.
Notes: These statistics include the universe of public and private schools offering an academic track. SABER 11 exam scores are standardized with respect to the national average. The last column presents the p-value for a difference in means test between public and private schools.

Table B.2. Attrition diagnostics

	Surveys: Baseline to Follow-Up	Baseline survey to ICFES	Baseline survey to ICFES-SNIES
	(1)	(2)	(3)
A. Attrition Rates			
Baseline N	6,601	6,601	6,601
Final N	5,503	6,323	6,303
Attrition Rate	0.166	0.043	0.046
B. Random attrition tests (OLS)			
Treatment	0.015	-0.012	-0.012
	(0.027)	(0.013)	(0.014)

Source: Authors' calculations from surveys matched to administrative data.
Notes: Standard errors in parentheses are clustered at the school-level.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.3. Average effects on perceived earning errors with alternative reference point

Reference earnings by:	Vocational		Academic	
	College, degree & field	Public/private college, degree & field	College, degree & field	Public/private college, degree & field
A. ANCOVA				
Treatment	0.009	-0.001	0.010	-0.010
	(0.024)	(0.023)	(0.038)	(0.037)
Adjusted p-value	0.829	0.989	0.884	0.893
Observations	2,782	3,972	2,802	4,009
B. Difference-in-differences				
Treatment × Post	0.033	0.039	0.038	0.049
	(0.029)	(0.028)	(0.040)	(0.040)
Adjusted p-value	0.356	0.228	0.444	0.297
Observations	5,691	8,152	5,715	8,196
Baseline mean	0.096	0.217	0.944	1.147

Source: Authors' calculations from survey data.

Notes: Each column and panel correspond to separate OLS regressions. Panel A presents coefficients of ANCOVA regressions that control for student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on exit exam in previous years, has computer lab, shift indicators, and school size), and neighborhood fixed effects. Panel B presents coefficients for difference-in-difference regressions that control for individual fixed-effects. Standard errors in parentheses are clustered at school-level. We report adjusted p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 2.4.2 for details).

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.4. Average effects on educational aspirations

	Aspirations				
	College enrollment	Academic degree	Private college	Top-10 college	STEM field
A. ANCOVA					
Treatment	0.002	0.010	0.004	0.010	0.015
	(0.003)	(0.017)	(0.020)	(0.013)	(0.013)
Adjusted p-value	0.982	0.967	1.000	0.909	0.757
Observations	5,503	5,503	5,503	5,503	5,503
B. Difference-in-differences					
Treatment × Post	-0.001	0.003	-0.004	0.004	0.007
	(0.004)	(0.016)	(0.023)	(0.013)	(0.014)
Adjusted p-value	1.000	1.000	1.000	0.997	0.983
Observations	11,006	11,006	11,006	11,006	11,006
Baseline mean	0.983	0.228	0.449	0.877	0.410

Source: Authors' calculations from survey data.

Notes: Each column and panel correspond to separate OLS regressions. Panel A presents coefficients of ANCOVA regressions that control for student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on exit exam in previous years, has computer lab, shift indicators, and school size), and neighborhood fixed effects. Panel B presents coefficients for difference-in-difference regressions that control for individual fixed-effects. Standard errors in parentheses are clustered at school-level. We report adjusted p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 2.4.2 for details).

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.5. Minimum detectable effects

Outcome	MDE in standard deviations	MDE in percentage points
Knowledge		
Knows Labor Observatory	0.0520	0.0138
Knows ICETEX	0.1219	0.0562
Knows FESBO	0.0535	0.0203
Perceived earnings error		
Vocational	0.0955	0.1441
Academic	0.1019	0.1272
Test scores		
Overall score	0.1981	
Math	0.2154	
Language	0.1869	
Higher education choices		
College enrollment	0.1043	0.0518
Academic degree	0.0953	0.0281
Private college	0.0888	0.0317
Top-10 college	0.0389	0.0040
STEM field	0.0690	0.0153

Source: Author's calculations from survey and administrative data.
Notes: These calculations follow Duflo et al. (2008). We assume 50 students per school (6000/115), calculate intra-cluster correlations from the data and set the test level at 0.10 and statistical power at 0.80.

Table B.6. Heterogeneous effects on knowledge and beliefs, perceptions and risk aversion

	Knowledge			Perceived earnings error	
	Labor Observatory	ICETEX	FESBO	Vocational	Academic
A. Perceived academic ranking					
Low	0.000	0.065**	0.008	0.042	0.002
	(0.012)	(0.022)	(0.019)	(0.045)	(0.039)
High	-0.016	0.016	0.002	0.029	0.077
	(0.015)	(0.024)	(0.021)	(0.051)	(0.049)
p-value (Low=High)	0.986	0.520	1.000	1.000	0.826
Observations	10,480	10,780	10,514	10,524	10,578
B. Perceived self-efficacy					
Low	0.004	0.024	0.015	0.041	0.009
	(0.011)	(0.022)	(0.017)	(0.044)	(0.040)
High	-0.025	0.083***	-0.013	0.027	0.076
	(0.016)	(0.024)	(0.024)	(0.058)	(0.053)
p-value (Low=High)	0.697	0.230	0.977	1.000	0.958
Observations	10,473	10,773	10,504	10,514	10,571
C. Risk aversion					
Low	-0.048	0.047	0.085	0.013	-0.072
	(0.028)	(0.042)	(0.037)	(0.102)	(0.078)
High	0.004	0.047	-0.002	0.031	0.043
	(0.010)	(0.019)	(0.015)	(0.041)	(0.037)
p-value (Low=High)	0.529	1.000	0.221	1.000	0.858
Observations	10,194	10,487	10,229	10,248	10,300
D. Perceived likelihood of enrollment					
Low	-0.030	0.098	-0.009	0.112	0.112
	(0.022)	(0.039)	(0.039)	(0.095)	(0.083)
High	0.002	0.038	0.010	0.031	0.024
	(0.011)	(0.020)	(0.016)	(0.039)	(0.038)
p-value (Low=High)	0.854	0.720	1.000	0.997	0.985
Observations	10,083	10,372	10,118	10,137	10,196

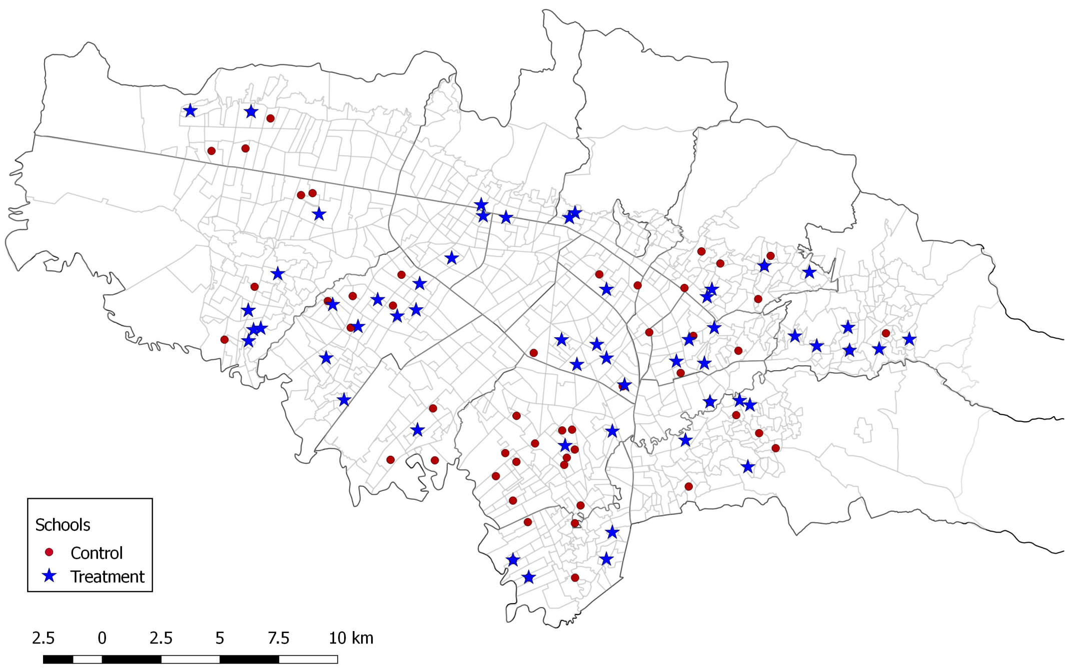
Source: Authors' calculations from survey data.
Notes: Refer to Table 2.4
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table B.7. Heterogeneous effects on test scores and higher education choices, perceptions and risk aversion

	Test scores			Higher education choices				
	Overall score	Math	Language	College enrollment	Academic degree	Private college	Top-10 college	STEM field
A. Perceived academic ranking								
Low	0.015	0.066	-0.010	0.005	0.016	0.005	0.007	0.004
	(0.045)	(0.049)	(0.042)	(0.025)	(0.013)	(0.003)	(0.009)	(0.007)
High	-0.002	0.038	0.024	0.007	0.008	0.006	0.011	0.008
	(0.047)	(0.050)	(0.043)	(0.027)	(0.018)	(0.005)	(0.015)	(0.011)
p-value (Low=High)	0.993	0.768	0.692	1.000	0.999	1.000	1.000	1.000
Observations	6,268	6,268	6,268	6,248	6,248	6,248	6,248	6,248
B. Perceived self-efficacy								
Low	-0.034	0.032	-0.052	0.003	0.014	0.004	0.002	0.000
	(0.044)	(0.049)	(0.039)	(0.023)	(0.013)	(0.003)	(0.011)	(0.008)
High	0.076	0.091	0.094*	0.005	0.012	0.008	0.021	0.013
	(0.048)	(0.051)	(0.046)	(0.027)	(0.017)	(0.005)	(0.012)	(0.010)
p-value (Low=High)	0.103	0.423	0.011	1.000	1.000	0.973	0.892	0.871
Observations	6,257	6,257	6,257	6,237	6,237	6,237	6,237	6,237
C. Risk aversion								
Low	0.020	0.081	0.039	0.031	0.019	0.016	0.032	0.032
	(0.085)	(0.090)	(0.074)	(0.039)	(0.024)	(0.009)	(0.018)	(0.015)
High	-0.012	0.035	-0.015	-0.002	0.011	0.004	0.005	0.002
	(0.040)	(0.041)	(0.036)	(0.022)	(0.013)	(0.003)	(0.009)	(0.007)
p-value (Low=High)	0.992	0.762	0.668	0.950	1.000	0.720	0.719	0.200
Observations	6,085	6,085	6,085	6,066	6,066	6,066	6,066	6,066
D. Perceived likelihood of enrollment								
Low	-0.039	0.014	-0.057	0.015	0.004	-0.001	0.008	-0.001
	(0.056)	(0.058)	(0.062)	(0.032)	(0.016)	(0.003)	(0.014)	(0.008)
High	0.001	0.044	0.002	0.005	0.015	0.007	0.010	0.007
	(0.039)	(0.044)	(0.036)	(0.023)	(0.014)	(0.003)	(0.010)	(0.007)
p-value (Low=High)	0.941	0.831	0.539	1.000	0.994	0.313	1.000	0.980
Observations	6,023	6,023	6,023	6,004	6,004	6,004	6,004	6,004

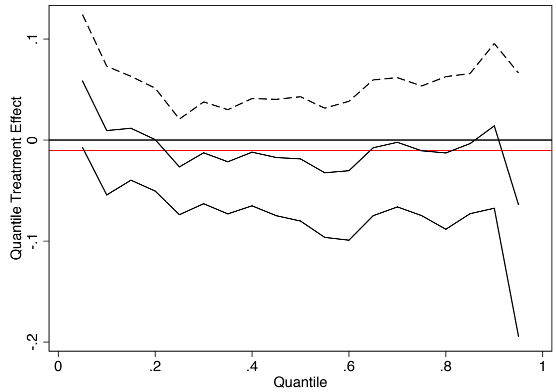
Source: Authors' calculations from surveys matched to administrative data.
Notes: Refer to Table 2.5
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Figure B.1. Geographic distribution of 115 treatment and control schools

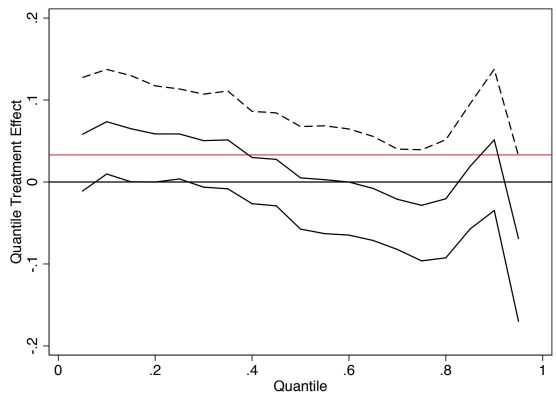


Source: Authors' elaboration from Secretary of Education's school census and survey data.

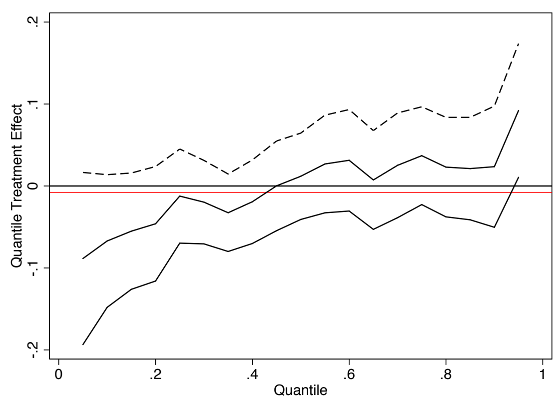
Figure B.2. Quantile treatment effects for SABER 11 test scores



Overall score



Math



Language

Source: Authors' elaboration from surveys matched to administrative data.
Notes: 90% Confidence intervals in black dashed/red dotted lines. OLS estimate in red line.

Appendix C

Supplementary material for Chapter 3

Table C.1. Difference-in-difference performance under spatial correlation, 2 nearest neighbors

	Spatial correlation parameter					
	0.00	0.10	0.25	0.50	0.75	0.90
Panel A: Spatially-correlated outcomes						
50 clusters						
Bias (in %)	-5.4	-2.5	-1.5	7.6	40.8	132.6
Rejection rate	78.4	79.4	78.1	68.7	50.7	27.2
100 clusters						
Bias (in %)	-4.0	-4.0	-3.4	6.1	34.7	112.8
Rejection rate	97.5	96.5	97.2	94.4	78.1	44.2
200 clusters						
Bias (in %)	-4.4	-4.4	-2.1	5.9	34.2	105.5
Rejection rate	100.0	100.0	99.8	99.6	96.1	73.3
500 clusters						
Bias (in %)	-4.3	-4.0	-2.5	7.5	39.3	124.8
Rejection rate	100.0	100.0	100.0	100.0	100.0	98.3
Panel B: Spatially-correlated unobservables						
50 clusters						
Bias (in %)	-2.8	-5.2	-3.2	-2.6	-4.6	-4.0
Rejection rate	71.7	68.9	69.3	66.8	51.5	25.1
100 clusters						
Bias (in %)	-3.8	-3.6	-4.4	-4.8	-4.4	-3.7
Rejection rate	94.7	94.5	94.7	90.6	80.2	38.0
200 clusters						
Bias (in %)	-4.3	-4.0	-3.4	-4.0	-4.9	-5.2
Rejection rate	99.9	99.8	99.7	99.6	97.5	63.1
500 clusters						
Bias (in %)	-4.9	-3.9	-4.1	-4.5	-3.9	-4.0
Rejection rate	100.0	100.0	100.0	100.0	100.0	92.9

Source: Authors' calculations from 1,000 Monte Carlo Simulations.

Notes: Reported values are averages from the total number of replications. Bias is calculated as the difference between the average estimate and the true value $([\hat{\beta} - \beta]/\beta) \times 100$. The rejection rate measures the percentage that DD finds a statistically significant effect when the null hypothesis is false (equivalent to statistical power).

Table C.2. Alternative estimation procedures with 100 clusters

	Spatially-correlated outcome						Spatially-correlated unobservable					
	0.00	0.10	0.25	0.50	0.75	0.90	0.00	0.10	0.25	0.50	0.75	0.90
Using individual-level data												
DD with village fixed-effects												
Bias (in %)	-5.0	-4.0	0.0	14.8	64.1	235.7	-5.2	-3.1	-4.5	-5.3	-3.8	-5.3
Rejection rate	97.0	97.4	96.6	91.1	64.2	44.8	92.9	94.1	91.7	87.7	65.2	17.6
DD with individual fixed-effects												
Bias (in %)	-5.0	-4.0	0.0	14.8	64.1	235.7	-5.2	-3.1	-4.5	-5.3	-3.8	-5.3
Rejection rate	87.4	86.5	84.7	68.8	34.2	19.9	75.0	77.2	74.4	65.1	34.1	5.6
Spatial DD												
Bias (in %)	-5.1	-4.6	-3.7	-3.9	-3.6	-3.1	-5.2	-3.1	-4.5	-5.4	-4.5	-4.8
Rejection rate	97.1	97.9	97.6	98.2	97.4	97.0	93.8	94.0	92.6	92.7	91.5	92.4
Using village-level data												
DD with village fixed-effects												
Bias (in %)	-5.0	-4.0	0.0	14.8	64.1	235.7	-5.2	-3.1	-4.5	-5.3	-3.8	-5.3
Rejection rate	86.5	85.6	83.8	67.2	32.6	18.0	73.8	75.5	72.6	62.9	32.3	5.2
Spatial DD												
Bias (in %)	-5.1	-4.6	-3.7	-4.0	-3.8	-3.4	-5.3	-2.9	-4.6	-5.4	-3.9	-4.8
Rejection rate	97.2	97.9	97.7	98.2	97.7	97.5	93.6	95.4	92.3	88.4	66.4	18.8

Source: Authors' calculations from 1,000 Monte Carlo Simulations.

Notes: Reported values are averages from the total number of replications. Bias is calculated as the difference between the average estimate and the true value $([\hat{\beta} - \beta]/\beta) \times 100$. The rejection rate measures the percentage that DD finds a statistically significant effect when the null hypothesis is false (equivalent to statistical power).

Table C.3. Alternative estimation procedures with 200 clusters

	Spatially-correlated outcome						Spatially-correlated unobservable					
	0.00	0.10	0.25	0.50	0.75	0.90	0.00	0.10	0.25	0.50	0.75	0.90
Using individual-level data												
DD with village fixed-effects												
Bias (in %)	-4.2	-2.6	0.5	16.3	78.5	263.5	-3.5	-5.2	-4.8	-3.8	-4.6	-5.5
Rejection rate	100.0	100.0	100.0	99.7	94.7	80.0	99.7	99.9	99.7	99.1	89.6	31.9
DD with individual fixed-effects												
Bias (in %)	-4.2	-2.6	0.5	16.3	78.5	263.5	-3.5	-5.2	-4.8	-3.8	-4.6	-5.5
Rejection rate	99.5	99.7	99.6	97.4	79.1	53.2	98.5	98.2	98.5	96.6	69.0	10.8
Spatial DD												
Bias (in %)	-4.2	-3.3	-3.6	-4.2	-4.4	-5.3	-3.5	-5.2	-5.0	-3.9	-3.7	-5.2
Rejection rate	100.0	100.0	100.0	100.0	100.0	100.0	99.7	99.8	99.7	99.9	99.6	99.4
Using village-level data												
DD with village fixed-effects												
Bias (in %)	-4.2	-2.6	0.5	16.3	78.5	263.5	-3.5	-5.2	-4.8	-3.8	-4.6	-5.5
Rejection rate	99.4	99.7	99.5	97.2	78.2	52.0	98.4	98.2	98.5	96.6	68.1	10.3
Spatial DD												
Bias (in %)	-4.2	-3.3	-3.6	-4.3	-4.5	-5.5	-3.5	-5.2	-4.9	-3.8	-4.6	-5.4
Rejection rate	100.0	100.0	100.0	100.0	100.0	100.0	99.8	99.9	99.7	99.3	90.5	33.6

Source: Authors' calculations from 1,000 Monte Carlo Simulations.
Notes: Reported values are averages from the total number of replications. Bias is calculated as the difference between the average estimate and the true value $([\hat{\beta}-\beta]/\beta)\times 100$. The rejection rate measures the percentage that DD finds a statistically significant effect when the null hypothesis is false (equivalent to statistical power).

Table C.4. Alternative estimation procedures with 500 clusters

	Spatially-correlated outcome						Spatially-correlated unobservable					
	0.00	0.10	0.25	0.50	0.75	0.90	0.00	0.10	0.25	0.50	0.75	0.90
Using individual-level data												
DD with village fixed-effects												
Bias (in %)	-4.2	-3.9	-0.5	14.3	69.0	235.6	-4.3	-4.6	-4.2	-4.5	-4.8	-3.9
Rejection rate	100.0	100.0	100.0	100.0	100.0	98.3	100.0	100.0	100.0	100.0	100.0	66.4
DD with individual fixed-effects												
Bias (in %)	-4.2	-3.9	-0.5	14.3	69.0	235.6	-4.3	-4.6	-4.2	-4.5	-4.8	-3.9
Rejection rate	100.0	100.0	100.0	100.0	99.5	91.5	100.0	100.0	100.0	100.0	99.3	39.4
Spatial DD												
Bias (in %)	-4.3	-4.5	-4.2	-4.6	-4.4	-4.8	-4.3	-4.6	-4.2	-4.5	-4.9	-3.4
Rejection rate	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Using village-level data												
DD with village fixed-effects												
Bias (in %)	-4.2	-3.9	-0.5	14.3	69.0	235.6	-4.3	-4.6	-4.2	-4.5	-4.8	-3.9
Rejection rate	100.0	100.0	100.0	100.0	99.5	91.3	100.0	100.0	100.0	100.0	99.3	38.7
Spatial DD												
Bias (in %)	-4.3	-4.5	-4.2	-4.6	-4.4	-4.9	-4.3	-4.5	-4.2	-4.5	-4.8	-3.8
Rejection rate	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	67.4

Source: Authors' calculations from 1,000 Monte Carlo Simulations.

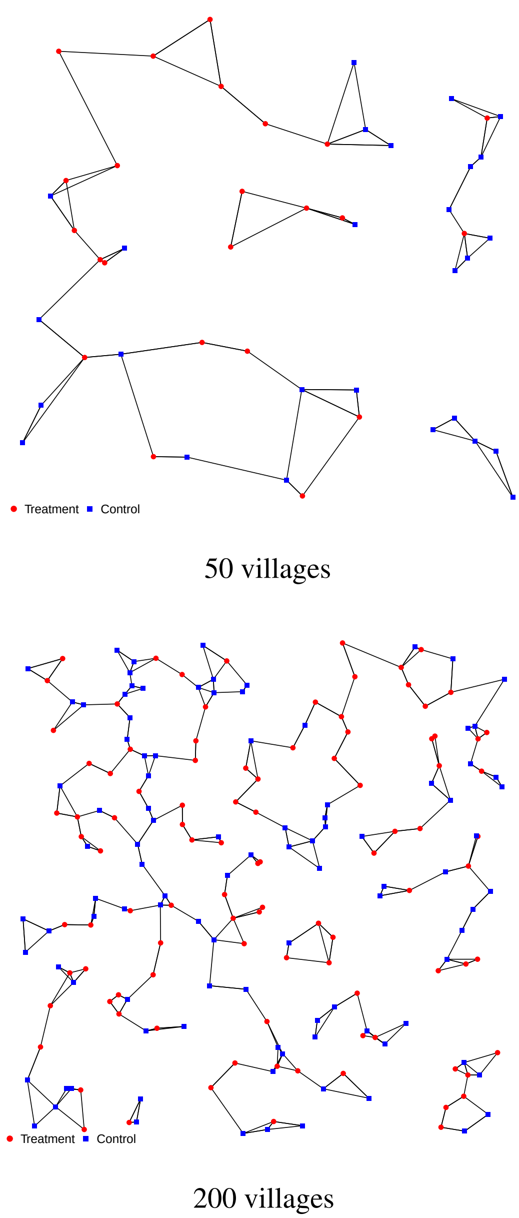
Notes: Reported values are averages from the total number of replications. Bias is calculated as the difference between the average estimate and the true value $([\hat{\beta} - \beta]/\beta) \times 100$. The rejection rate measures the percentage that DD finds a statistically significant effect when the null hypothesis is false (equivalent to statistical power).

Table C.5. Difference-in-differences estimates for Progresa at the village-level

			Spatial DD	
		Unadjusted	1 nearest neighbor	2 nearest neighbors
Primary enrollment	β	0.007	0.007	0.006
(Spatial lag)		(0.004)*	(0.005)	(0.005)
	ρ		0.097	0.179
			(0.024)***	(0.032)***
	Observations	1,010	1,010	1,010
Secondary enrollment	β	0.010	0.008	0.009
(Spatial lag)		(0.014)	(0.024)	(0.024)
	ρ		0.155	0.224
			(0.023)***	(0.030)***
	Observations	1,010	1,010	1,010
Male labor supply	β	-0.006	-0.006	-0.006
(No spatial correlation)		(0.008)	(0.009)	(0.009)
	ρ		0.004	0.017
			(0.010)	(0.015)
	Observations	1,012	1,012	1,012
Female labor supply	β	-0.011	-0.012	-0.011
(Spatial lag)		(0.013)	(0.015)	(0.015)
	ρ		0.024	0.069
			(0.016)	(0.023)***
	Observations	1,010	1,010	1,010
Per capita income	β	0.040	0.038	0.035
(Spatial lag)		(0.031)	(0.044)	(0.044)
	ρ		0.153	0.254
			(0.023)***	(0.030)***
	Observations	1,012	1,012	1,012
Per capita consumption	β	0.082	0.080	0.087
(Spatial error)		(0.026)***	(0.039)**	(0.038)**
	λ		0.127	0.238
			(0.024)***	(0.031)***
	Observations	1,010	1,010	1,010

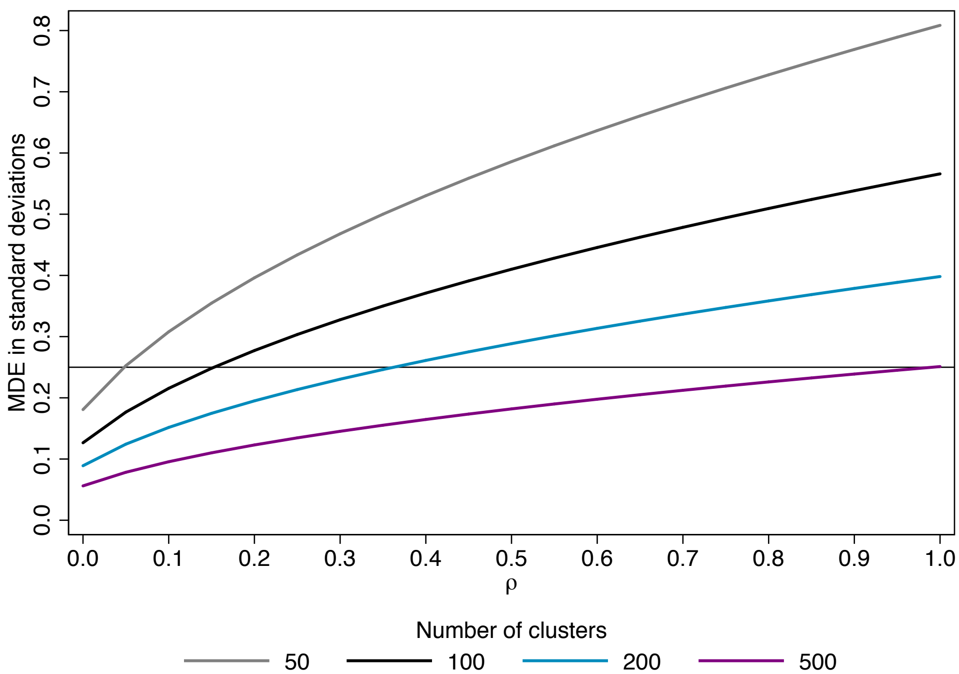
Source: Authors' calculations from Progresa data aggregated at village-level.
Notes: We estimate Equation (3.1) on village data with no controls and use regional weight matrices for spatial specifications. Each set of coefficients corresponds to a separate regression. Standard errors are clustered at the village-level.
*** Significant at 1 percent, ** 5 percent, * 10 percent.

Figure C.1. Simulated spatial networks: 2 nearest neighbors



Source: Authors' elaboration from simulated data.
Notes: This graph shows a random draw of villages by allocated treatment status. Lines between villages denote the that two locations are neighbors.

Figure C.2. Minimum detectable effects for simulated scholarship program



Source: Authors' calculations.