Essays on Human Capital, Labor and Development Economics

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ABSTRACT

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This dissertation contains four essays on human capital, labor and development economics. The first two chapters study how exposure to particular labor markets during childhood determines the formation of industry-specific human capital generating long-term consequences in terms of adult criminal behavior, labor outcomes and state legitimacy. The third chapter explores how criminal capital developed during childhood can be exported to other locations generating spillover effects on human capital accumulation. Finally, the last chapter studies how improving access to justice for women affects children's outcomes.

Chapter 1, **"Making a *Narco*: Childhood Exposure to Illegal Labor Markets and Criminal Life Paths"**, shows that exposing children to illegal labor markets makes them more likely to be criminals as adults. I exploit the timing of a large anti-drug policy in Colombia that shifted cocaine production to locations in Peru that were well-suited to growing coca. In these areas, children harvest coca leaves and transport processed cocaine. Using variation across locations, years, and cohorts, combined with administrative data on the universe of individuals in prison in Peru, affected children are 30% more likely to be incarcerated for violent and drug-related crimes as adults. The biggest impacts on adult criminality are seen among children who experienced high coca prices in their early teens, the age when child labor responds the most. No effect is found for individuals that grow up working in places where the coca produced goes primarily to the legal sector, implying that it is the accumulation of human capital specific to the illegal industry that fosters criminal careers. As children involved in the illegal industry learn how to navigate outside the rule of law, they also lose trust in government institutions. However, consistent with a model of parental incentives for human capital investments in children, the rollout of a conditional cash transfer program that encourages schooling mitigates the ef-

fects of exposure to illegal industries. Finally, I show how the program can be targeted by taking into account the geographic distribution of coca suitability and spatial spillovers. Overall, this paper takes a first step towards understanding how criminals are formed by unpacking the way in which crime-specific human capital is developed at the expense of formal human capital in "bad locations."

While my first chapter focuses on low-skilled labor and criminal capital, my second chapter studies the expansion of high-skilled labor markets. In Chapter 2, **"Long-term Effects of Temporary Labor Demand: Free Trade Zones, Female Education and Marriage Market Outcomes in the Dominican Republic"**, I exploit the sudden and massive growth of female factory jobs in free trade zones (FTZs) in the Dominican Republic in the 1990s, and subsequent decline in the 2000s, to provide the first evidence that even relatively brief episodes of preferential trade treatments for export industries may have permanent effects on human capital levels and female empowerment. Focusing on a sample of provinces that established FTZs and exploiting variation in the opening of zones and age of women at the time of opening, I show that the FTZs' openings led to a large and very robust increase in girls' education. The effect persists after a decline in FTZs' jobs in the 2000s following the end of a trade agreement with the U.S. and an increase in competition from Asia. The reason appears to be that the increase in *some* girls' education changed marriage markets: girls whose education increased due to the FTZs' openings married later, had better matches with more stable marriages, gave birth later, and had children who were more likely to survive infancy. In sum, the evidence in this paper indicates that labor markets can improve female outcomes in developing countries through general equilibrium effects in the education and marriage markets.

Another question I address in my dissertation is whether criminal capital developed during childhood can be exported to other locations. In the first chapter, I find that individuals take skills related to the illegal drug industry with them when they move to other districts, even when they move to districts without significant illegal industries. Chapter

3, **"Exporting Criminal Capital: The Effect of U.S. Deportations on Gang Expansion and Human Capital in Central America"**, provides new evidence on how an increase in criminal capital due to deportations from the US affects human capital investments in El Salvador. In 1996, the U.S. Illegal Immigration Responsibility Act drastically increased the number of criminal deportations. In particular, the leaders of large gangs in Los Angeles were sent back to their countries. In addition to having a direct effect, the arrival of individuals bringing criminal skills and connections may have generated important spillover effects. We exploit this policy to look at the impact that deportation policies and the subsequent arrival of criminal capital to El Salvador had on several educational and economic outcomes. Using the 1996 policy and geographical variation in the exact location and delimitation of different gang groups, we find that criminal deportations led to large increase in crime and decrease in human capital accumulation for children living in these areas. Overall, this project helps to understand one of the reasons why El Salvador is among the world's most violent peacetime countries. Understanding these effects is crucial for public policy to successfully incorporate deported criminals back into society.

While my work in the Dominican Republic and the previous literature has shown that increasing the returns to education for women incentivizes schooling, there is little evidence on how domestic violence affects human capital development and whether improving access to institutions for women can address these issues. During my field work in rural areas of Peru, I found that institutions do not usually address the problems facing women or ethnic and religious minorities. For example, the police do very little to stop domestic violence. Moreover, in many cases, women do not even trust these institutions enough to report these issues. Chapter 4, **"Inter-Generational Impacts of Improving Access to Justice for Women: Evidence from Peru"**, exploits the introduction of women's justice centers (WJCs) in Peru to provide causal estimates on the effects of improving access to justice for women and children. Our empirical approach uses variation over time in the distance from schools and households to the nearest WJC together with province-by-year fixed effects. After the opening of WJC, we find that primary school enrollment increases at schools that are within a 1km radius of a WJC and the effect decreases with distance. In addition, we also find that primary school second graders have better test scores in reading and mathematics. Moreover, we find that children in primary school living in household's located near a WJC are more likely to attend school, to pass a grade and they are also less likely to drop out of school. We also provide some evidence that these improvements might be driven by an increase in the bargaining power of women inside the household and decrease in domestic violence. In sum, the evidence in this paper shows that providing access to justice for women can be a powerful tool to reduce domestic violence and increase education of children, suggesting a positive inter-generational benefit.

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[1]For a more extensive review of the history of gangs from El Salvador see Savenije, 2009

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Chapter 1

Making a *Narco*: Childhood Exposure to Illegal Labor Markets and Criminal Life Paths

[1]

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1.1 Introduction

Illegal markets and the associated crime are critical concerns in developing countries and marginalized areas of rich countries, as exemplified by the impact of the drug-trade from Peru and Colombia to the inner cities of the U.S.[2] While research in this area has mainly focused on enforcement measures, there has been little success in pinpointing root causes that are within the reach of policy. Understanding how criminal careers begin is especially important since once an individual embarks on a criminal career, he or she is unlikely to turn back.[3]

Crime is geographically concentrated, and recent evidence documents the lifelong consequences of growing up in a bad neighborhood (Chetty et al., 2015; Chetty and Hendren, 2015; Damm and Dustmann, 2014).[4] Another literature has shown how parental decisions, particularly those related to human capital investments, can have long-term consequences (e.g., Currie and Almond, 2011; Cunha and Heckman, 2007; Heckman, 2006; Brooks-Gunn and Duncan, 1997; Becker, 1981). Might location-specific factors during childhood and parental responses catalyze criminal careers?

This paper finds evidence that exposure to illegal labor markets during childhood leads to the formation of industry-specific human capital at an early age, putting children on a criminal life path. Using an exogenous shock to illegal labor markets in Peru, I show that when the return to illegal activities increases in areas suitable for coca pro-

[2]There is a growing awareness that crime and illicit drugs are major impediments for development (UN 2012). Crime may affect development by driving away business, eroding human capital, and undermining democracy (UN 2007). This has been pointed out by several international organizations that have increased efforts to improve citizen security in developing countries. Recent surveys find that crime is the top concern for citizens in emerging and developing countries (PewResearchCenter, 2014). About half of the world's 450,000 annual homicides take place in Latin America and sub-Saharan Africa (see `http://homicide.igarape.org.br/`).

[3]See e.g., Bell et al. (2014).

[4]A large body of research demonstrates that crime is unevenly distributed across space. The high variation in crime rates across states, cities, and neighborhoods represents what Glaeser et al. (1996) called "the most puzzling aspect of crime." For example, in Newark metropolitan area in the U.S., 85% of homicides were concentrated in only a few municipalities (O'Flaherty and Sethi, 2014). Similarly, violence and narcotrafficking activities tend to be geographically concentrated in specific municipalities in Mexico (Ajzenman et al., 2014).

duction, parents significantly increase the use of child labor for coca farming. This in turn increases children's criminal capital and the chances that they remain in the cocaine industry. As adults, affected children are more likely to be incarcerated for violent and drug-related crimes, have lower earnings, and are less likely to trust state institutions. However, I show that policies that target the incentives surrounding these early investments can mitigate the effects of exposure to illegal labor markets. In particular, I show that conditional cash transfers that encourage schooling can reduce child labor in the illegal sector, and thus, drug production in coca suitable areas. This policy addresses an underlying cause of future criminality by limiting the formation of criminal capital while simultaneously increasing formal human capital.

To isolate causal channels, I take advantage of drug enforcement policies in Colombia that shifted coca leaf production to Peru, where 90% of coca production is used to produce cocaine. In 1999, Colombia, then the world's largest cocaine producer, implemented Plan Colombia, a U.S.-supported military-based interdiction intervention. One of the main components was the aerial spraying of coca crops in Colombia.[5] This resulted in higher prices and expanded coca production in Peru, where production doubled in districts with the optimal agro-ecological conditions. By 2012, Peru had become the largest producer of cocaine in the world.[6]

This setting yields three useful sources of variation: i) cross-district or cross-school/village variation in coca-growing suitability, ii) over-time variation in coca prices, and iii) differential exposure to coca growing across cohorts (during sensitive ages) within location-time cells.[7] I measure coca suitability in three ways: coca production in 1994 (prior to the events examined in my analysis), satellite image data showing where coca is grown,

[5]During the period of analysis, coca production decreased by 60% in Colombia.

[6]See "National Drug Control Strategy," Office of National Drug Control Policy, U.S. Department of State, 2012. Also see `https://www.whitehouse.gov/ondcp/news-releases-remarks/`.

[7]I use cross-district variation when examining labor and crime outcomes (there are 1,839 districts). I use cross-school/village variation since schooling outcomes are geo-coded. There are about 100,000 villages and 50,000 schools in Peru.

and a coca suitability index constructed based on the optimal conditions to grow coca.[8] Time variation comes from changes in the black market price of coca induced by the U.S.-supported eradication program in Colombia. I thus define age-specific shocks by interacting coca suitability measures and prices. Differential exposure arises since children within a district or village experience the coca boom at different ages and due to variation in coca suitability across districts, villages, and schools.

To observe these sources of variation, I build a detailed panel of administrative data for a variety of labor market, schooling, and crime outcomes.[9] First, I use a geo-coded school panel and satellite images of coca fields that allow me to link each school to a particular coca geographic cell and isolate how human capital accumulation is affected in the short-run. Second, to examine the long-run effects on criminality, I take advantage of confidential administrative data on the 2015 and 2016 universe of inmates in Peruvian prisons, which includes information on village of birth, date of birth, length of sentence, education, family characteristics, and previous occupation. Furthermore, black market prices of coca are obtained from United Nations surveys. These data allow me to track cohorts that were exposed to high coca prices during key ages across areas with different coca suitability. In this way, I am able to analyze whether exposed children are more likely to be incarcerated in adulthood.

I first show that investments in children's human capital are affected by the cocaine industry. The increase in coca prices induced by Colombia's anti-drug policy leads to a large and significant increase in child labor in areas suitable for coca production. Children between the ages of 6 and 14 are most affected, with largest effects for those between 12 and 14.[10] Consistent with an impact on child labor, when coca prices double, test scores

[8] Coca (*Erthroxylum spp.*), a plant native to South America, can be cultivated only under specific agro-ecological conditions. For instance, coca plants require specific ranges of altitude, slope, and soil conditions.

[9] I also complement these data with field work interviews. In particular, I visited the region composed of the Apurimac, Ene and Mantaro River Valleys (VRAEM), which according to the United Nations, is the place with the most coca crops and laboratories for the production of coca base and cocaine in the world.

[10] Empirical results are consistent with qualitative evidence that children aged 6 to 14 are particularly affected by the increase in prices. Most of the work is unskilled, making children an attractive input. The International Labor Organization reports that a large percentage of children are involved in the production

decline by 0.3 standard deviations for primary school students located in high coca areas. The probability that affected students failed a grade increase by more than 20%. In addition, the relatively high earnings in the cocaine industry induce some secondary school-aged children to drop out of school. In particular, there is a 27% increase in the dropout rate for students beginning secondary school. This large effect corresponds exactly to the years when most children drop out of school in Peru (i.e. the transition between primary and secondary education). Furthermore, I find that these results are not driven by violence or migration.

I then ask how early-life exposure to illegal labor markets affects children's long-run outcomes at the ages of 18 to 40.[11] I find that individuals who grew up in coca producing areas and experienced high coca prices during childhood are about 30% more likely to be incarcerated and have 20% lower earnings than their counterparts—i.e., those who were born in the same district but in a different year, and those who were born in a different district but belong to the same cohort. The effects are concentrated among children who experienced high coca prices in their early teens, the ages when child labor increases the most.[12] The fact that long-term effects are only driven by specific cohorts provides further evidence that results are not confounded by other factors such as increased enforcement in coca districts. In addition, I use coca satellite images to classify whether incarcerated individuals were born in a village inside a coca geographic cell and find that results are robust to the inclusion of district-by-year fixed effects. This controls for political decisions,

of drugs in Peru (for more information see "Ninios en zonas cocaleras. Un estudio en los valles de los rios Apurimac y Alto Huallaga.", Technical Report, United Nations, 2006). This has also been extensively reported by news outlets. See, for example, "The Mochileros: High stakes in the high Andes - the young backpackers risking their lives in cocaine valley," BBC News, November 24, 2015. Since contracts are hard to enforce, participants in the industry often rely on family and friends for different types of labor (Balbierz, 2015; Beriain, 2014; Pachico, 2012; Van Dun, 2012). In addition, children are often used because they cannot be legally prosecuted (Mejía Fritsch, 2003).

[11] These are the ages for which I am able to construct the whole history of prices during childhood.

[12] In addition, previous literature has suggested that it is at these ages when children begin to interact more with their peers and are susceptible to becoming involved in crimes (Damm and Dustmann, 2014; Ingoldsby and Shaw, 2002). Furthermore, I find no effect for individuals that had high coca prices when they were older than 14, consistent with the fact that children above these ages are more likely to be at school. The lack of long-term consequences for older cohorts is also in line with the previous literature studying the effects of neighborhood exposure during childhood in the U.S.

such as the level of enforcement, that are made at the district level.

The second focus of this paper is to understand the mechanisms driving criminal careers. There are two potential mechanisms. First, exposed children have lower formal education. Previous literature has found that lower formal human capital can lead to crime.[13] Second, individuals may acquire skills specific to the illegal sector. For example, they may develop human capital specific to the drug trade such as knowledge about transforming coca into cocaine, knowledge about smuggling, or connections to buyers.[14] The criminal capital acquired during childhood may be complimentary with future criminal capital.[15]

I find evidence that the increase in criminality is mainly driven by human capital specific to the illegal drug industry. First, those that are affected by the shock are more likely to be convicted of violent and drug-related crimes, but not other types of crimes such as property crime, sexual assault, or white collar crime. Second, I find that similar price shocks that affect legal commodities such as coffee and gold increase child labor, yet have no effect on the likelihood of future criminality. I also show that individuals from districts where most of the coca is grown for traditional medicine and religious purposes are not involved in crime later in life even though they have lower schooling. Third, I show that these negative effects are found not only for those from coca areas who remain in coca growing districts as adults, but also for those who move to districts without coca production. This implies that the long-term effects are not caused by contemporaneous exposure to the cocaine industry during adulthood but rather to exposure during childhood.[16] Fi-

[13] See e.g. Lochner and Moretti (2004) who show that schooling reduces the probability of incarceration, Anderson (2014) who shows that dropping out of school increase juvenile criminal behavior, and Deming (2011) who finds that access to better schools reduces criminal behavior. In a similar way, Aizer et al. (2015) shows that juvenile incarceration increases future criminal behavior by interrupting social and human capital accumulation during a critical period.

[14] Working in the illegal industry may also lead to exposure to criminal groups and other negative peer effects. For evidence on crime and peer interactions, see Glaeser et al. (1996), Bayer et al. (2009), Deming (2011) and Damm and Dustmann (2014).

[15] This is similar to the canonical model of human capital development (e.g. Cunha and Heckman (2007)).

[16] These results also provide further evidence that effects are not driven by an increase in enforcement in coca areas.

nally, by characterizing compliers, I find that an overwhelming majority of individuals who are in prison due to the shock report that they were involved in illegal activities before the age of 18. Moreover, most of the affected individuals reported farming as their last occupation, indicating that they likely started their criminal career growing coca.

These results also have broader implication in terms of state legitimacy. In line with the criminal capital channel, individuals involved in the illegal industry may learn to navigate an industry that operates outside of the rule of law, and consequently lose trust in public institutions. I find that children affected by the expansion of the illegal industry are less likely to believe democracy works well and have lower trust in the police and the national government when they are adults. This has important implications for state capacity, and may limit the government's ability to combat organized criminal groups and drug cartels.

Having shown that criminal careers can develop during childhood, I then analyze how policy can address the underlying mechanisms that lead to future criminality by changing parental incentives in the affected areas. I exploit the gradual rollout of a conditional cash transfer program (CCT) during the period of high coca prices. The program provided monetary transfers to parents with the condition that children attend school on a daily basis. Since the program was targeted based on poverty measures and not coca suitability I am able to examine how the CCTs interact with high coca prices in suitable coca areas.

Consistent with the hypothesis that parental responses during childhood matter, I find that conditional cash transfers can mitigate the negative effects of growing up during the expansion of the cocaine industry. I show that coca areas that implemented the program experienced a significant reduction in coca production and child labor. This leads to better schooling outcomes even when prices are high. Moreover, I show suggestive evidence that effects are mainly driven by the conditionality rather than income effects.[17]

[17] Results are robust to district poverty time trends suggesting that the effect of CCTs is not mainly driven by an income effect. Moreover, if income effects were large we would expect to see a decrease in adult labor. In contrast, I find an increase in adult labor, providing evidence that coca producers substitute away from child labor when the opportunity cost of child labor increases. Moreover, I find that policies that only

These results suggest that CCTs should be targeted toward coca-suitable districts in order to mitigate the effects of high coca prices. However, policy makers must also account for the fact that there may be a "balloon effect"—if illegal production drops in one area, it may expand in nearby areas. I show that reducing coca production by incentivizing schooling in one district leads to an increase in child labor in neighboring districts if those districts are also suitable for producing coca, mirroring the shift of coca production to Peru when eradication increased in Colombia. Using a simple algorithm, I show how a budget constrained social planner would optimally assign CCTs across districts taking into account these spillover effects. I find that if CCTs have to be allocated to only a given number of districts, the reduction in child labor and coca production can be maximized by allocating the policy to districts that have no neighboring districts with high coca suitability. These results indicate that accounting for location-specific factors and geographical spillovers when targeting a policy can increase its total impact on future crime considerably.

Overall, I provide new evidence that childhood environment and parental responses can affect criminality later in life. I argue that the formation of human capital is important for understanding the perpetuation of illegal industries and the geographic concentration of crime. In contrast, much of the previous literature on crime has mainly focused on enforcement, which can often lead to increased violence.[18] Moreover, there is a growing consensus that enforcement alone often can explain little of the variation in crime (O'Flaherty and Sethi, 2014; Fisman and Miguel, 2007; Levitt, 2004). In this paper, I find that location-specific factors and parental responses are a root cause of crime. Furthermore, I show that policies that target parents taking into account location-specific factors

increase resources, such as mining transfers, do not mitigate the cocaine industry effects.

[18]Dell (2015) shows how drug enforcement caused a large increase in homicide rates in Mexico. Also, Abadie et al. (2014) finds that aerial eradication exacerbated armed conflict and violence in Colombia, by reducing guerrillas' main source of income. Castillo et al. (2014) show how changes in drug enforcement in Colombia generated an increase in violence in Mexico. In the same line, Vargas (2014) shows how violence spiked in Chicago after the arrest of a important gang leader, by generating violent competition among gangs over market share. For a review on drug enforcement measures and violence see Werb et al. (2011) and Miron (1999), which document a positive relationship.

reduce the development of criminal careers. In other words, if location-specific factors affect parental incentives to use child labor and thus "create" criminality, then location-specific policies may be needed to target these incentives.

The remainder of the paper is organized as follows. In the next section, I discuss the previous literature in more depth. Section 1.3 presents the setting and Section 1.4 describes the data. Section 1.5 presents the empirical strategy and results for child labor and schooling outcomes. Section 1.6 presents the long-run results. Section 1.7 show the general equilibrium effects of a specific policy targeting the mechanisms behind the effects. I return to the policy implications in the final section.

1.2 Related Literature

This paper connects several literatures. First, by looking at the long-term effects of location-specific labor markets, this paper is related to the recent literature examining the long-term effects of "place" in developed countries (Damm and Dustmann, 2014; Chetty and Hendren, 2015; Chetty et al., 2015). In the context of developing countries, I contribute to this question by presenting causal evidence that place based inputs matter for whether individuals start on a criminal path. In particular, I provide new evidence that the presence of illegal labor market opportunities during childhood affects adult incarceration, earnings, and trust in institutions. I argue that the effect of place is particularly important in the context of the illegal drug industry given that it generates large externalities and regions with narco-trafficking often suffer from weak institutions.

Second, a growing number of theoretical and empirical studies have analyzed how early childhood conditions and parental investments affect later outcomes (Currie and Almond, 2011; Heckman, 2006; Brooks-Gunn and Duncan, 1997). Much of this empirical literature has analyzed how negative shocks early in life affect adult outcomes through human capital.[19] In this paper, I focus on the development of one type of industry-specific

[19]Currie and Almond (2011) provide a review of the effects of early childhood influences on later life

human capital, namely criminal skills specific to the drug industry. While much of this literature has focused on early childhood—before the age of five—I provide evidence that long-term outcomes can be affected during late childhood and early adolescence when individuals in developing countries are exposed to local labor markets.

By drawing insights from the literature on the effects of place and early childhood environment, this paper contributes to the literature by providing new evidence on the root cause of crime in developing countries. Individuals may be pushed into criminal careers by location-specific factors and parental decisions during childhood, highlighting the importance of location-specific policies that target these incentives.

This paper also provides evidence on how criminal behavior responds to changes in the private return to committing a crime. In a recent literature review, (Draca and Machin, 2015) observe that previous studies do not address such a channel, as they focus on how crime is affected by changes in the return to legal labor market opportunities and enforcement (e.g., Buonanno and Raphael, 2013; Di Tella and Schargrodsky, 2004; Levitt, 1997).[20]

This paper is also related to the previous research studying the determinants of criminality by looking at particular attributes such as age, gender, education, military service, and peers (e.g., Deming, 2011; Galiani et al., 2011; Lochner and Moretti, 2004; Glaeser et al., 1996).[21] While most of the research has focused on concurrent factors during young adulthood or conditions during early childhood, there is no empirical evidence about whether early exposure to illegal markets affect criminal careers. This mechanism potentially sheds light on why criminal activities are persistent over time and geographically concentrated.

This paper is also related to the growing literature studying how human capital investment decisions are affected by plausibly exogenous changes in legal labor market

outcomes.

[20]See also Freedman and Owens (2014).

[21]A related literature focusing on developed countries also examines whether criminality is affected by early childhood conditions such as lead exposure (Reyes, 2007) and pre-schools programs. For a review see Lochner (2011).

opportunities (Atkin, 2012a; Shah and Steinberg, 2013). In addition, recent research has examined how changes in the return to legal activities affect drug supply (Dube et al., 2015). I complement this literature by examining labor market returns in an illegal sector, which also generates effects on crime and state legitimacy. For comparison, I examine shocks to coffee and gold, which are legal commodities, and find that, although the shocks reduce human capital in the short-run, they do not effect adult criminality. This paper also contributes to the literature examining the effect of child labor (e.g., Bandara et al., 2015; Cogneau and Jedwab, 2012; Edmonds, 2007). While the previous literature has focused on how child labor effects schooling and earnings, this paper provides evidence that child labor can have broader long-term effects.

This paper also relates to the literature studying the unintended consequences of drug enforcement policies and production (Dell, 2015; Rozo, 2014; Mejía and Restrepo, 2013; Evans et al., 2012; Angrist and Kugler, 2008; Dammert, 2008). I provide evidence of international spillover effects from Colombia's drug enforcement policies and within-country spillover effects from a social program that was not intended to reduce drug production. While this literature has mainly focused on the effects of the drug industry on violence and conflict, I provide evidence on human capital development and long-term individual outcomes, highlighting the organizational structure of the industry. In addition, since Peru was not affected by rural insurgent activity when coca expanded, I can rule out any confounding factors related to civil conflicts.[22] These results are particularly important for drug policy. Since effects are driven by exposure during childhood, policies that increase the opportunity cost of child labor may reduce drug production. I provide new evidence that policies that indirectly reduce the incentive to engage in illegal activities can be more cost effective than increased enforcement.

Finally, this paper sheds light on how weak institutions may perpetuate themselves. My evidence supplements studies that consider the determinants of trust in institutions

[22] Although important in the 1980s and 1990s, civil conflict diminished sharply when the leader of the main terrorist group was captured by Peruvian authorities in 1999.

and how it may affect economic development (e.g., Nunn and Wantchekon, 2009; Alesina and Ferrara, 2002). I show that measures of state legitimacy can be affected by exposure to illegal activities at a young age.

1.3 Institutional Context

In this section, I provide background information relevant to my analysis. First, I provide an overview of the cocaine industry and describe the anti-drug policy that I exploit for identification. This paper focuses on the increase in coca and cocaine production in Peru due to eradication efforts in Colombia. Second, I review qualitative evidence indicating that children are an attractive input in the production of cocaine. Children are often used to pick coca leaves, although they are also used for other stages of the production process. Third, I also summarize my own conversations with coca farmers, school administrators, police officers, and government officials in Peru in order to better understand the potential long-term consequences of child labor in the cocaine industry. This anecdotal evidence is largely consistent with correlational evidence, as well as the more rigorous causal evidence presented in Section 1.5.

1.3.1 Spillover Effects in the Drug Industry in South America

Most coca is grown in Bolivia, Colombia, and Peru and about 90% is used to make cocaine.[23] These are the only countries that have the optimal agro-ecological conditions to grow coca. The high jungle areas on the eastern slope of the Andes Mountains are well suited for coca plants because coca grows best at altitudes over 2,000 meters with about 20 degrees of slope. Therefore, any changes in drug policy in one of these countries is likely to shift coca production within the Andean Region.

[23] In Peru, the remaining 10% is used in the traditional manner, and is highly regulated. In particular, coca leaves are chewed directly or used for tea. Most of the legal production is concentrated in the region of La Convencion y Lares.

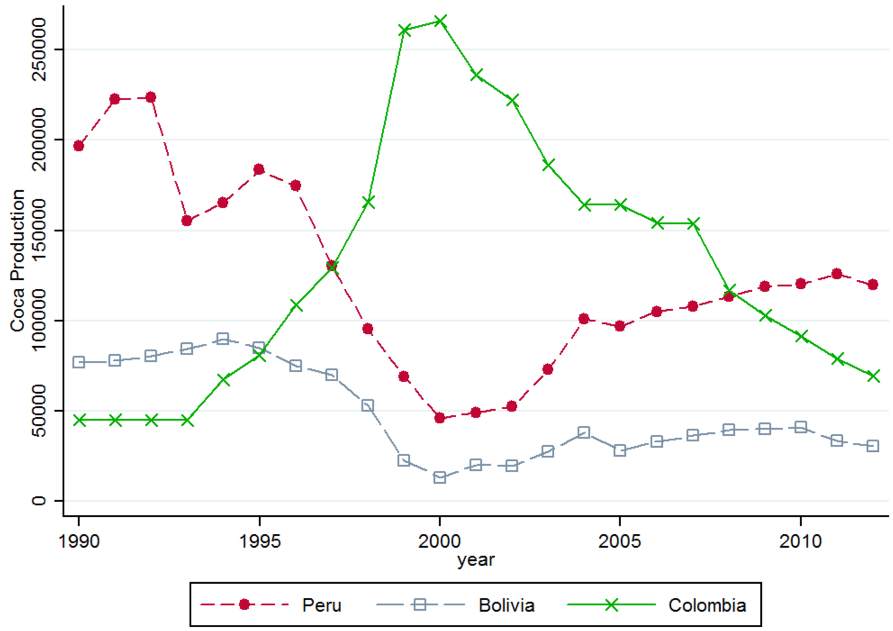


Figure 1.1: Coca production in the Andean region

Figure 1.1 shows coca production in this region. I argue that the shift in coca production between countries was exogenous. In the beginning of the 1990s, most coca was produced in Peru. However, the closing of the air bridge used to transport drugs shifted production to Colombia. Then, in the 2000s, U.S. backed eradication efforts in Colombia shifted production back to Peru—an event which is the focus of this paper. In particular, I take advantage of Plan Colombia, a coca spraying program implemented in Colombia in 1999 to reduce cocaine production. Colombian production declined sharply after 1999, followed by a steady increase in Peru.[24]

This paper primarily exploits the change in coca price in the 2000s induced by policies in Colombia, focusing on the period 1994 to 2014. I argue that this supply shock is uncorrelated with time-varying factors in Peru. In addition, I exploit the fact that this change in prices primarily affected those areas in Peru that were suitable for coca production.

[24]The phenomenon in which reducing drug production in one region causes it to expand elsewhere is often called the "balloon effect." For example, Rozo (2014) suggests that since eradication efforts in Colombia did not move production to other areas within the country, it is very likely that coca production moved to other countries with similar conditions for growing coca. In a similar vein, Mejia and Restrepo (2013) develops a model of the war on drugs to understand the effects of Plan Colombia. This model predicts a reallocation of cocaine production to other countries due to eradication efforts in Colombia.

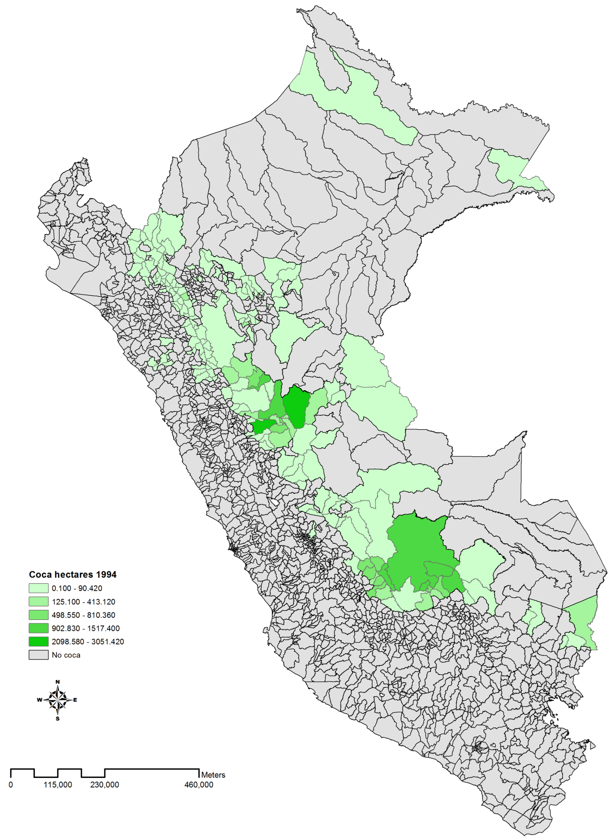


Figure 1.2: Variation in coca production in 1994 across districts

Figure 1.2 shows the 1994 distribution of coca production in Peru. There is substantial variation across districts in coca production. There are 1,839 districts in Peru and coca is grown in about 190 of these. While the Andes region in the south of Peru is highly productive, northern areas are less so. This is mainly because certain districts have better agro-ecological conditions than others. According to FAO-Eco crop there are optimal ranges of soil, precipitation, slope, and altitude to grow coca. These conditions are highly correlated with the areas that were producing in 1994—about 80% of high producing districts have the optimal conditions. I describe these conditions in detail in the data section.

In districts suitable for coca production, the economy is not only dependent on the production and selling of primary goods but also on the processing of coca into coca paste and cocaine for illegal markets. For example, about 2,000 laboratories and maceration pits were found hidden in the jungle in 2007 (UNODC, 2007). In general, local family-run organizations control production and domestic transportation of cocaine, while Colombian and Mexican intermediaries handle international trafficking.

1.3.2 The Use of Child Labor and Family Networks in the Drug Industry

Cocaine production primarily involves three phases: the collection of coca leaves, the transformation of coca into cocaine paste, and transportation. Children and adolescents an important input in every stage. There are two main reasons: children are ideally suited for the first stage of production which involves the farming of coca leaves. Also, family networks control each stage of the cocaine production and are unlikely to hire outsiders. Therefore, children are also commonly used to collect, transport, and process coca into cocaine. A qualitative survey carried out in one of the main coca valleys noted that about 90% of children who are born in coca areas work for the drug industry (Novak et al., 2011; UNICEF, 2006). While young children often pick coca leaves, they often become involved with the transformation of coca into cocaine and transportation as they get older.

Most of the cited reasons for the use of children in the cocaine industry has to do with the production function in the first stages and the illegality of the sector. Harvesting coca requires relatively low-skilled workers; the plants grow close to the ground; hence, young children are well suited for collecting coca leaves. In addition, it is often thought that small children can more effectively hide from the police. And even if they do get caught, they face less harsh penalties than adults for similar crimes(Gastelumendi, 2010; Mejía Fritsch, 2003). A testimony from a municipal authority during my field work supports this view: "From the population, almost everyone grows coca and employs children. They are fast and they can hide from the authorities easily." Data from 2012 Agriculture Census also support the view that children are used in the production of coca.[25]

[25]Given that in coca producing areas there is also processing to cocaine paste and cocaine, for the rest of

Table A1 shows that the amount of coca is positively correlated with the fraction of child laborers within farms. This contrasts with other crops, such as coffee, where there is a negative correlation between production and the share of children.

Another reason for the use of children has to do with the organization of the business. Unlike big Colombian cartels, the drug trade in Peru mostly consists of family firms that control all stages of production. Since a high level of trust is necessarily, they rely on family and friend networks. Moreover, the higher-level individuals who control the organizations are often men who started as coca farmers and worked their way up the hierarchy. This organizational structure reduces the risks of being caught (Balbierz, 2015; Beriain, 2014; Pachico, 2012; Van Dun, 2012).[26]

The farming of coca is very labor intensive. Harvests of coca can occur up to 6 times a year and at different times of the year, which means that children can be working all year in the coca fields. Moreover, children tend to work from 4am to 4pm, overlapping with school hours (usually 7am to 1pm). The harvest of an average size farm results in 125 kg of coca leaves, but only 0.3 kg of processed cocaine. Given that Peru produces 300 to 400 tones of cocaine annually, coca farming in Peru demands a large amount of child labor.

Consequently, working for the cocaine industry can have negative consequences on children's schooling. If children are in school in addition to picking coca, the long hours in the field likely affects their school performance. Other students may drop out of school entirely, especially if they join other parts of the narco-trafficking process. School census data in Peru shows that about 10% of primary school students fail a grade and more than 25% have a higher age for the grade in coca producing areas.[27]

Although children may start by picking coca leaves, they are also likely to be exposed

the paper I use the terms drug, coca and cocaine interchangeable.

[26] During my field work in the Alto Huallaga, one of main coca basins, one individual involved in the cocaine industry noted that there is a well defined system for alerting the community when the police or unknown people enter a coca area.

[27] A majority of primary school children during my field visits expressed that they spend most of their time working in coca fields. In interviews, teachers told me that older students were attracted to the potential for high earnings from working in the cocaine industry. I discuss further details on the qualitative-research methods in the Data Appendix.

to other stages of the production process as they grow older. Once coca leaves are collected, the leaves are dried and manually crushed in maceration pits. The leaves are then processed into cocaine and transported. There is anecdotal evidence that as children grow older, they become involved in all of these stages of the production process.[28]

Childhood exposure to the cocaine industry may also affect the future probability that individuals are involved in the drug trade as adults. For instance, during my field work one participant involved in the industry stated that: "A child that grows up in a coca valley [in rural Peru] will follow an employment cycle in the drug industry: first picking coca leaves, then transforming into cocaine, and then transporting drugs. It wouldn't be unusual that this early start in the business leads him to be a drug trafficker or sicario (hit-man)." In a similar way, a 21 year old man said, "I started working in coca farms when I was 8 and since I knew all of the people in the business, I was hired to work in the maceration pits."

Consistent with the hypothesis that adult crime is due to exposure during childhood, the probability of incarceration is correlated with exposure to the drug industry during childhood. In particular, those born in high intensity coca districts are more than twice as likely to be incarcerated. This is especially true for drug related crime. About 33% of offenders born in coca areas were arrested for drug trafficking versus 16% of offenders born in other areas. Moreover, about 20% of drug offenders were previously in a juvenile center, suggesting some path dependence in criminal behavior.

[28]Several local and international newspapers have documented the fact that young children in these areas often work in coca fields, while teenagers are involved in production and narco-trafficking. See, for instance, "Children are the workforce in coca fields in Peru," El Comercio, February 5, 2007. Another article notes that, "Peru now produces more cocaine than any other country. But there is no easy way to smuggle it out, so traffickers hire young men to carry it on foot [...] It's one of the most perilous jobs in the cocaine industry." See "The Mochileros: High stakes in the high Andes - the young backpackers risking their lives in cocaine valley," BBC News, November 24, 2015.

1.4 Data

This paper makes use of four main datasets that provide variation across geographic regions and time at different levels of aggregation for a variety of labor market, schooling, and crime outcomes. The first two datasets–time series data and agriculture data–provide the tools to construct the main treatment variable. Time variation comes from changes in the black market price of coca induced by eradication policies in Colombia. I interact the time series variation with measures of whether districts are suitable for growing coca. Alternatively, for precisely geocoded outcomes, I use data from satellite imagines that indicates whether a village/school is located near a coca farm and thus affected by the drug industry. The household and school level data provide information on labor and schooling outcomes as well as trust in institutions. Finally, the incarceration dataset allows me to explore whether the young individuals exposed to the coca boom are more likely to be involved in criminal activities when they are adults.

1.4.1 Agro-ecological Data

The geographic variation in coca suitability that helps define the treatment group is drawn from two sources, an Agriculture Census at the district level and geocoded satellite data on coca density. Having the treatment defined at different levels of aggregation is useful since the labor and school outcomes are also measured at varying levels of aggregation (district and school level). In addition, since the geographical measures of coca specialization are defined before the expansion of Peru's coca industry they do not reflect potentially endogenous production efforts correlated with the main outcomes over the period of analysis.

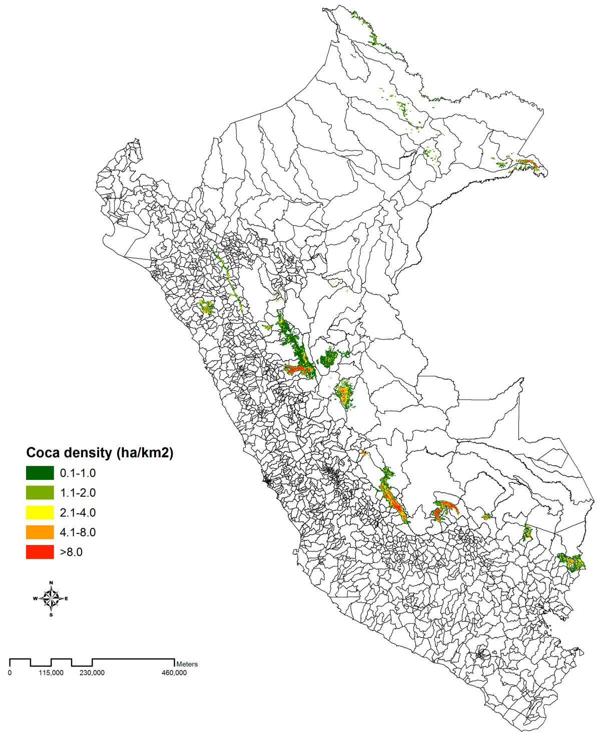
I use the Agriculture Census of 1994 to define historical coca production by the number of planted hectares of coca per district. I also use these data to create historical production of coffee by district for comparison. Given this measure is defined before the shock to

Colombia, the short-run effects would not reflect endogenous production efforts in Peru.

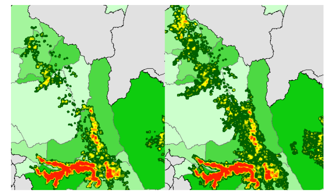
Since districts are the lowest level of disaggregation in the Agriculture Census of 1994, I also use coca density maps for the period 2002 to 2013 from the United Nations Office of Drugs and Crime (UNODC). Around 1999, the UNODC started the Illicit Crop Monitoring Programme, which uses annual satellite images to obtain the location of coca fields in Peru. The images are verified by flying over randomly chosen areas each year. The UNODC data allows me to perform a more disaggregated analysis for geocoded schooling outcomes and for a subsample of incarcerated individuals for which I can obtain geocoded information on location of birth. In addition, these data also allow me to confirm that districts that were already producing coca in 1994 are also where coca crops expanded during the 2000s.

Figure 1.3 shows the coca satellite images during the 2000s. Most coca production was located in districts that also produced coca in 1994. This suggests that 1994 coca production is a good proxy for the areas that saw an expansion during the 2000s. Panel 1.3 presents the evolution of coca crops in one of the main coca basins from 2002 to 2013, showing how crops expanded during the period when prices increased.

To rule out the possibility that results are driven by endogenous factors that may affect the outcomes of interest, I construct a coca suitability index that shows which areas have ideal agro-ecological conditions to produce coca. I use information from the FAO Eco Crop system (see Figure A1), which reports ideal ranges for precipitation, temperature, slope, altitude, and soil conditions. I define an area as "suitable" when it falls in the optimal range in every agro-ecological dimension. Figure A2 shows the areas using this definition. Most of the areas that were producing coca in 1994 are suitable to produce coca according to this index. The remaining suitable areas that do not produce coca after the expansion tend to be isolated areas.



(a) Coca satellite images during the 2000s



(b) Alto Huallaga region in Peru in 2002 and 2013

Figure 1.3: Coca crops

1.4.2 Time Series Data

The other identifying source of variation comes from changes in the price of coca over time. The UNODC program has recorded information on coca prices in the black market since 1990. The information is collected once a month by project staff through semi-structured interviews with informants who are selected from coca farmers, grocers, and people involved in the production and distribution of coca derivatives. For the short-run analysis, I use prices for 1994, 1997, and 2001 to 2013 since these are the years when child labor outcomes are available. Figure 1.4 shows that during the expansion of the drugs industry in Peru in the 2000s, coca prices doubled.

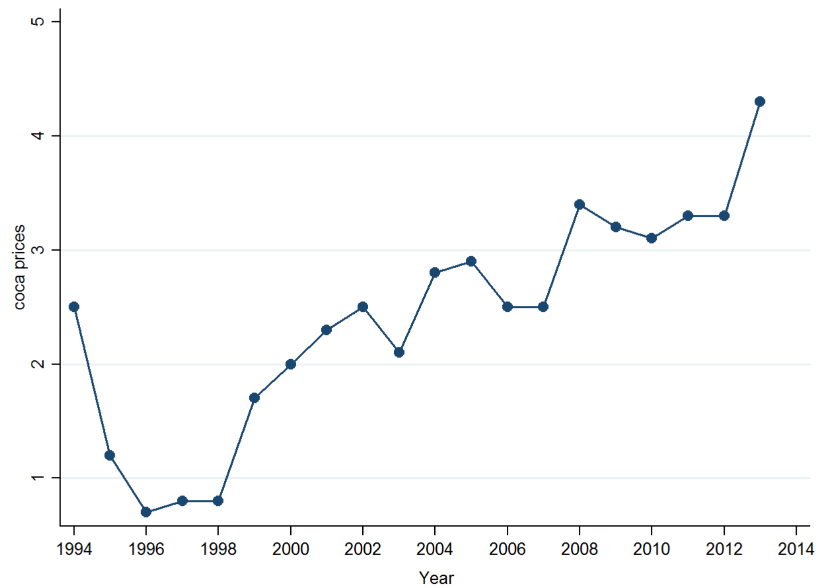


Figure 1.4: Coca prices between 1994-2014

The prices reported by UNODC represent the price of coca on the international market. After 1994, prices decline due to an effective U.S-supported air interdiction. The main air bridge used to send coca paste from Peru to Colombia was disrupted. In 1999, Plan Colombia decrease coca production and increase cocaine seizures in Colombia. I argue that changes in the price of coca are primarily due to exogenously determined drug policies in Colombia that shifted production to Peru. I discuss this in more detail in Section 1.5. In particular, I use data on the number of coca hectares in Colombia provided by the Colombia Ministry of Defense to instrument for price.

1.4.3 Household Data

I examine the effects on child labor and other labor market outcomes as well as outcomes about state legitimacy using household data from the Peruvian National Household Survey (ENAHO) covering the period from 2001 to 2013. I complement these data with the Peruvian Living Standards Measurement Survey (PLSMS) for the years 1994 and

1997. Both surveys are nationally representative.[29] My final sample consists of 34,859 household-year and 16,172 children-year observations, distributed across 1,400 district-year observations.

I also use data from National Institute of Statistics (INEI) for baseline characteristics at the district level such as poverty, distance to the main city, area, number of classrooms, and fraction of households exposed to violence during civil conflict in the 1980s. Table A22 shows summary statistics. Child labor is defined using the main activity reported by children between 6 and 14 years of age. Nationally, about 28% of children are working. It also shows that before the Colombian shock, the districts that are suitable for growing coca and the districts that specialize in other crops, such as coffee, have similar observable characteristics on average.

1.4.4 School Data

The school data I use are geocoded and cover the universe of schools in Peru during the period 1998 to 2014. The geographic coordinates allow me to combine these data with satellite image data on the location of coca production allowing for an analysis at the most disaggregated level. Using a similar identification strategy as for labor market outcomes, I can examine the effect of coca production on school outcomes. However, unlike labor market outcomes, the disaggregated data allows me to examine the effect within schools.

The schooling datasets I use are the School Censuses (Censo Escolar, CE) and Census Evaluation of Students (Evaluacion Censal de Estudiantes, ECE). The school data contain not only information on enrollment by grade, but also measures of school achievement such as test scores, age for grade, and grade repetition.[30] I complement these data with the Census Evaluation, which is a national standardized test administered every year to

[29] I use household data since it is the most precise source of information on child and adult labor outcomes in Peru.

[30] Many young children in rural areas attend some school even if they work. Therefore, primary enrollment may be less important than these measures of school achievement.

all students in second grade in all primary schools.

Figure A3 in the Appendix shows distribution of schools across Peru. Of the approximately 50,000 primary and secondary schools in Peru, about 8,000 are located in coca-growing districts.

Table A3 presents the baseline characteristics of school located in areas with high coca suitability and low coca suitability when prices were low in 1998 (before the Colombian shock). With the exception of percentage of failed students and reading test scores, there are no statistical significant differences between baseline characteristics.[31]

1.4.5 Incarceration Data

In order to examine whether children exposed to illegal labor markets are more likely to engage in crime as adults I use confidential data on the universe of individuals in prison in 2015 from the *Instituto Nacional Penitenciario* in Peru. These data allow me to track cohorts that were exposed to high coca prices during key ages across different areas with different coca suitability. I exploit variation in place of birth and date of birth to explore how childhood exposure to the drugs industry affects criminal behavior in later life. I compliment these data with a census of the incarcerated population from the first quarter of 2016 which contains additional information about the social and family environment of prisoners. I use this information to characterize the individuals that were affected by the shock.

The data contain 77,244 individuals incarcerated in Peru between the ages of 18 and 60 in 2015 (Figure A3 presents the age of arrest distribution). It contains information about their exact district and date of birth, their education, type of crime and main occupation (see FigureA4). About 60% of offenders did not complete secondary education. The percentage increases to 66% for offenders born in coca areas. The most common stated

[31] Causal identification does not require baseline characteristics to be balanced given that I utilize difference-in-difference variation.

previous occupation of offenders is manual workers. However, about 35% of offenders born in coca areas were previously agricultural workers.[32]

From this sample, I keep the individuals who were born in Peru and for whom I can construct a complete history of coca prices during childhood. The final sample contains 64,298 individuals. I collapse these to the cohort and place of birth level. There are 4.7 offenders per cohort-district of birth cell on average.[33]

From the incarceration data I construct the number of crimes by type, cohort and district of birth. Cohorts in districts that do not appear in the incarceration data take a value of zero, which means that there is no one in prison from that cohort in that specific district. I also construct incarceration rates by dividing the number of offenders by the number of people born per district and cohort. On average there are 3.4 offenders per 1,000 people (see Table A22); in coca areas it is almost twice as high.

Figure A9 shows that when coca prices are high, incarceration rates are higher for individuals who grew up in coca districts. Moreover, there is no change for individuals from non-coca areas. This helps motivate my main empirical specification.

1.5 The Direct Effect of Drug Production on Child labor and Schooling

In this section, I examine the causal effect of coca production on short-run labor market and education outcomes. First, I describe the identification strategy which exploits plausibly exogenous variation in prices and geographic coca suitability. Using this identification strategy, I examine the relationship between the cocaine industry and child labor and schooling, as well as the economic mechanisms.

[32] About 5,500 individuals do not report information on place of birth and age.

[33] As a robustness check, I also construct a sample at the cohort-village of birth level.

1.5.1 Econometric Specification

1.5.1.1 Baseline Econometric Specification

In order to estimate the causal effect of cocaine production on education and labor market outcomes I would ideally use data on who is producing cocaine. Unfortunately these data are unavailable since cocaine production is an illegal industry. As an indirect way to measure the effects of cocaine production, I combine a difference-in-difference strategy with an instrumental variables approach. First, I exploit geographic variation in coca suitability, defined by whether a district historically produced coca or whether a school is located in an area that has coca farms identified from satellite images. Second, I exploit exogenous time variation in coca prices induced by an anti-drug policy in Colombia.

To estimate the effects of the expansion of the drugs industry on child labor I use a linear probability model in which the outcome is an indicator for whether the child was working the week before the survey. The treatment variable is the price of coca on the black market interacted with a coca suitability measure indicating the number of hectares of coca in the district in 1994. All specifications include district fixed effects, year fixed effects, linear trends by department, as well as controls for poverty, type of area, population, age, and gender.

Peru is a major coca producer and may affect the prevailing price. Thus, I instrument the coca price in Peru with the coca hectares in Colombia. Equation 3.1 presents the baseline specification:

$$Y_{i,d,t} = \beta \underbrace{(PriceCoca_t \times Coca_d)}_{PriceShock_{d,t}} + \alpha_d + \phi_t + \gamma X_{i,d,t} + \sigma_r t + \epsilon_{i,d,t} \tag{1.1}$$

where $Coca_d$ is a measure of coca suitability for district d, which is defined by the number of coca hectares in thousands in district d in 1994 before the Colombian shock. $PriceCoca_t$

is the instrumented log price of coca in Peru in year t. It is instrumented by the log of the number of coca hectares in Colombia (per 100,000).[34] $Y_{i,d,t}$ is a dummy indicating whether the child in household i is working. The α_d are district fixed effect, ϕ_t year fixed effects, and $\sigma_r t$ department specific time trends.[35] By including these fixed effects, I control for invariant differences between coca and non coca producing districts, and for changes in aggregate time trends across years. Note that the regressors $PriceCoca_t$ and $Coca_d$ are collinear to the year and district fixed effects. $X_{i,d,t}$ controls for poverty, type of area, population, age, and gender. To account for serial correlation of coca prices, I cluster the standard errors at the district level.

This strategy is similar to the standard difference-in-difference model, where the estimates compare low and high suitability areas, in years following high coca prices relative to years with low coca prices. The main difference is that the treatment is a continuous variable since both the cross-sectional variation and time variations are continuous.[36]

To test how the drug industry affects schooling outcomes, I use a more disaggregated measure of treatment. In particular, I redefine the main treatment at the school level by linking each geocoded school to the geocoded data on coca geographic cells from satellite images in 2002. Thus, I am able to classify each school based on the coca intensity in the surrounding area. This is more precise than estimation at the district level. I define $Coca_d$ with $DenCoca_s$ which ranges from zero to five. The categories reflect coca intensity as follows: category zero indicates no coca, one indicates 0.1 to 1 ha of coca per km^2, two indicates 1.1 to 2 ha/km^2, three indicates 2.1 to 4 ha/km^2, four indicates 4.1 to 8 ha/km^2, and five indicates more than 8 ha/km^2.

[34]Ideally, I would like to have the share of coca hectares over all suitable land. However, I do not have access to these data. Nevertheless, the number of hectares is a good measure because my main assumption is that the more hectares of coca in a district, the more child labor is needed. In addition, as a robustness check, I control for the size of the district interacted with year fixed effects to ensure that results are not driven by larger districts.

[35]Peru is divided into 25 departments.

[36]This strategy is commonly used to estimate the effect of commodity shocks (e.g. Dube and Vargas, 2013).

$$Y_{s,t} = \beta \underbrace{(PriceCoca_t \times DenCoca_s)}_{PriceShock_{s,t}} + \alpha_s + \phi_t + \sigma_r t + \epsilon_{s,t} \tag{1.2}$$

All specifications include school and year fixed effects and standard errors are clustered at the school level. I include a vector of school level varying controls which I construct by interacting time invariant characteristics with year dummies.

1.5.1.2 Addressing Potential Concerns

In this subsection, I show that districts that were producing in 1994 are the ones expanding their production in the 2000s. In addition, I discuss the exclusion, relevance, and common trends assumptions. I show that the instrument is not weak and that the shock did not impact other outcomes such as revenue, taxes, and transfers. Finally, I present a series of robustness checks that address the potential endogeneity of coca production and differential trends across districts.

In the above specification, I assume that only districts that produced coca in 1994 were suitable for coca and responded to the shock. One concern is that non-producing districts could have started producing coca after 1994 in response to booming prices during the 2000s. However, I find that only 10% of the growth in coca production is due to the expansion of coca in previously non-producing districts. More formally, I estimate the increase in hectares allotted to coca production from 2002 to 2012 based on an indicator for growing status in 1994. Results show a strong correlation between coca intensity in the 2000s and 1994 growing status. Cultivation grew by about 300 more hectares in the growing districts than elsewhere (see Table 1.1). None of the intercepts are significant (suggesting no significant growth in the districts with no initial coca).

The results are in line with the intuition that areas with higher production in 1994 are

Table 1.1: Growing Status in 1994 and Coca Production Growth in 2002-2013

Growing status in 1994 (=1)	331.043***	509.135***	197.879***
	(24.032)	(29.265)	(18.803)
Constant	4.258	4.258	4.258
	(8.026)	(7.397)	(4.713)
Observations	1,847	1,847	1,847
R-squared	0.093	0.15	0.06
Sample	All districts	High coca	Low coca

Notes: Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

suitable for coca and respond more to the shock from Colombia.[37]

Given that I use the number of coca hectares in Colombia as an instrument for prices, I also provide a formal test for the *relevance assumption* (Imbens and Angrist, 1994) in Table 1.2. The Kleibergen and Paap F statistic is large, indicating that the weak instrument problem is not a concern. A 10% decrease in coca production in Colombia is associated with a 2.5% increase in prices in Peru.

The second assumption that must be satisfied for the validity of my identification strategy is the *exclusion restriction*. This could be violated if the local government in Peru increases their enforcement or resources in coca areas when Colombia increased their drug enforcement policies. To address this concern, Table A5 presents evidence that rules out a violation of the exclusion restriction for covariates such as district's public income, taxes, and total transfers from the central to the district governments. The table shows the estimates of a regression of the instrument on these variables. None of the regressions shows significant coefficients.

The third main identifying assumption of the baseline specification is that there would be *common trends* across districts with different levels of coca suitability in the absence of price changes. This assumption could be violated if, for instance, child labor was in-

[37] I also check whether the districts that first responded to the increase in prices are those closer to the Colombian frontier and find that it is not the case. This may be due to the lack of soil suitability in Peruvian districts close to Colombia. However, these areas started producing in recent years due to better technology developed by Colombian producers that allowed them to produce coca at lower altitudes.

creasing in suitable districts before the price shock. I address this concern by visually inspecting pre-trends and by including the years 1994 and 1997 in the main specification and controlling for coca area specific linear trends. I also include a vector of district baseline characteristics interacted with year (including classrooms, poverty index, childhood malnutrition level, the fraction of households exposed to violence during civil conflict in the 1980s, and kilometers to the main city) and coca area specific time trends. These interactions control for any potential differential trends across types of districts.

In addition, given that children beginning secondary education are more likely to drop out from school, I use a household fixed effects model to estimate the differential effect of coca prices across siblings (or other relatives) of different ages in the same household, thereby controlling for any household specific characteristics. Using this model, I can control for any differential trend across districts.

For schooling outcomes, since the data are geocoded, I also include an interaction of district-by-year fixed effects and compare schools within the same district in a given year. The identification assumption in the baseline model is that schools in high density coca areas would otherwise have changed similarly, on average, to those control schools in low or no coca geographic cells (identified by satellite images). By controlling for district-by-year fixed effects, the identification assumption is that affected schools would otherwise have changed similarly, on average, to control schools within their same district. This specification controls for any characteristic that may vary at the district and year level. This is especially relevant since most political decision are made at the district level. In particular, it rules out the concern that child labor and schooling results are driven by changes that vary by district and year such as an increase in political corruption or a decrease in district resources.

There are other potential concerns. To account for migration patterns, I check whether migration is affected by the expansion of the drug industry. Also, since the school treatment variable is constructed from 2002 satellite images—the beginning of the period of the expansion in Peru— it may be that the location of coca growing areas is endogenous to the shock. I address this concern by constructing a coca suitability index based on agro-ecological characteristics and check the robustness of the results using this index. Finally, to address whether the results are specific to the illegal industry, I estimate the effect of a legal commodity shock, coffee. Coffee is another important commodity in Peru and is produced in about 300 districts. I choose coffee since there is substantial variation across districts in coffee production and there is exogenous variation across time in coffee prices during the period of analysis. I construct the coffee shock by interacting the number of coffee hectares per district in 1994 with changes in the international coffee price. For schooling outcomes, since schools are geocoded, I use FAO-GAEZ coffee suitability index based on agro-ecological conditions that provides a finer measure. Table A23 shows that coca districts are similar to the districts that specialized in coffee production in 1997 when coca prices where low. Observable characteristics before the shock are mostly balanced across different areas.

1.5.2 Results for Short-Run Effects

I present two sets of findings related to short-run outcomes. First, the expansion of the cocaine industry in Peru significantly increased child labor. As a consequence, test scores declined and the probability of failing a grade for primary school children increased. Second, the relatively high earnings in the cocaine industry induced some secondary school-age children to drop out. Students starting secondary school were particularly affected. All of these results are robust to the inclusion of baseline covariates interacted with year fixed effects, district-by-year fixed effects, coca district specific time trends, household fixed effects, migration patterns, and using the coca suitability index. In addition, child labor and schooling effects are not driven by changes in violence or the supply of education in affected areas.

1.5.2.1 Child labor

Effect of Childhood Exposure to Illegal Activities on Child labor— Figure 1.5 shows the fraction of children working in coca and non-coca districts across time. Two observations are relevant. We can see that in periods during which the cocaine industry expands, child labor increases in areas suitable for coca production. This is the case in the beginning of the 1990s and during the 2000s. Second, while in 1997 child labor is at similar levels in coca and non-coca districts, after the 2000s, coca areas consistently have more children working.

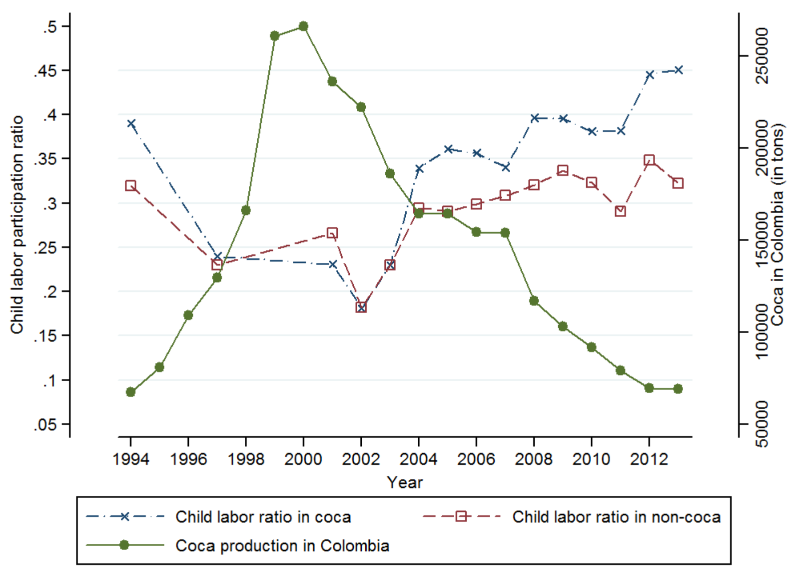


Figure 1.5: Child labor in coca and non coca districts in Peru vs coca production in Colombia

Next, I turn to estimating the causal effect of coca prices on child labor. Table 1.2 shows the results from a linear probability model. Column (1) presents the results from a regression that includes all observations from the post period (2001-2013). To gauge the magnitude of the estimated coefficients, consider the rise in child labor associated with the increase in coca prices between the low point in 1997 and the peak in 2002. During this period, coca price doubled. The estimates suggest that this increase in coca prices led to an 8 percentage point increase in child labor in the average coca area (i.e. with a suitability of 0.5). This translates to a 30% increase relative to the mean. In Column (2) I estimate a similar specification to examine the effect of coffee production on child labor. The point estimate is statistically significant, but the magnitude is much smaller. For comparison, the estimates in Column (2) indicate that a similarly sized increase in coffee prices would increase child labor by only 5%.

Robustness Checks— The rest of Table 1.2 addresses some of the concerns presented in section 3.4.0.1. First, Column (3) presents the results with department trends and the results are very similar. Column (4) includes interactions of constant variables with year dummies such as the proportion of households exposed to violence in the pre-period, distance to main city, and malnutrition. This controls for potential biases coming from differential trends in places that have more violence in the past due to civil conflicts. When I include all these covariates the coefficient is similar to the baseline estimate and significant. Second, Columns (5)-(6) include 1994 and 1997 years from ENNIV surveys in order to be able to include coca specific time trends. The estimates are robust to these specifications. To account for pre-existing differential time trends, Columns (6)-(7) include coca specific time trends and interactions of department and year fixed effects. Finally, Column (8) tests whether changes in coca production affected migration within Peru. Since illegal industries require trust and rely on family and friend networks, they are unlikely to hire workers from other districts.

Table A6 in the Appendix presents the child labor estimates by age categories. I find that results are larger for those children who are 11 to 14 years old at the time of the shock and dissipate after age 14, with no effects for those older than 18. This is in line with the fact that older individuals can be legally prosecuted. Therefore, as an additional robustness check, I compare siblings of different ages within households. Panel (A) in Table A7 includes household fixed effects and still finds an increase in child labor for siblings who were 11 to 14 years of age compared to other ages. This result provides further evidence that results are not driven by differential time trends in coca districts.

Any potential confounder needs to mimic the shock to coca prices and differentially affect children between 11 and 14 in high suitable coca districts.

To address the potential endogeneity of coca production in 1994, Panel (C) in Table A7 presents the results using the coca suitability index. Results are similar in magnitude and significance to using the 1994 coca production. Finally, another potential concern is that eradication efforts in Colombia are correlated with eradication efforts in Peru. If this is the case this would violate the exclusion restriction. Peru does not conduct aerial spaying eradication, instead they only engage in small-scale manual eradication. I check whether these eradication efforts in Peru are correlated with the eradication efforts in Colombia that form the basis of my identification strategy. I find a negative non-significant relationship over the period of analysis, providing additional evidence that the exclusion restriction is valid.[38] Table A24 in the Appendix also presents the reduced form results estimating the effect of Colombian coca hectares on child labor and results are of the same magnitude and significance. Moreover, results are robust to not instrumenting prices. This suggests that price endogeneity is not a concern.[39]

In sum, the child labor patterns are consistent with qualitative evidence presented in Section 1.3.2. I observe large effects since children are an important input in cocaine production. One limitation is that I cannot observe whether children that increased their labor participation belong to coca families and whether individuals switch between sectors. However, I do not find evidence of a decline in production of other cash commodities, such as coffee and cacao during the period of analysis. This is likely because when the returns to the illegal industry increase, this affected mostly farmers who have historically produced coca or have a relatives in the business.

[38]Moreover, most of the alternative development programs in Peru that sought to substitute coffee and cacao production for coca production were implemented at the very end of the period of analysis.

[39]These results are not included for brevity but are available upon request.

Table 1.2: Effect of coca prices on child labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dep. Variable: Child labor							Dep. Variable: Migrate
$PriceShock_{d,t}$	0.165***		0.151***	0.147***	0.155***	0.149**	.149**	0.044
	(0.043)		(0.041)	(0.039)	(0.037)	(0.055)	(0.055)	(0.101)
$PriceCoffee_t \times Coffee\ int._{d,1994}$		0.011**						
		(0.004)						
	First Stage, Dep. Variable: $PriceShock_{d,t}$							
$CocaColombia_t \times Coca_d$	-.512***		-.512***	-.511***	-.513***	-.525***	-.521***	-.51***
	(.0189)		(.0187)	(.0174)	(.0186)	(.011)	(.012)	(.016)
Kleiberg-Paap F-stat	737.804		753.253	859.976	759.453	2132.827	1917.68	1004.626
Observations	233,824	233,824	233,824	228,446	234,465	242,580	242,580	230,205
Number of districts	1,431	1,431	1,431	1,412	1,436	1,436	1,436	1,412
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Department trends			✓	✓	✓			✓
Baseline trends				✓				✓
Covariates	✓	✓	✓	✓	✓	✓	✓	✓
Coca trends							✓	
Dep*Year FE						✓	✓	
Period	2001-2013	2001-2013	2001-2013	2001-2013	1997 2001-2013	1994,1997 2001-2013	1994,1997 2001-2013	2001-2013

Notes: This table presents the results of a linear probability model for the dependent variables: *child labor and migration*. The baseline specification is presented in Equation 3.1. Column (1) presents the results for the whole post period, 2001-2013 and includes controls for gender, age, poverty at the household level and population. Column (2) replace the coca interaction by the coffee interaction. Column (3) adds department time trends. Column (4) controls for baseline characteristics interacted by year such as the proportion of villages affected by conflict in the 1980s and malnutrition rates. Column (5) adds the year 1997. Column (6) adds department-by-year fixed effects. Column (7) includes coca specific linear trends as a regressor. Column (8) replicates the baseline specification with *migrate* as the dependent variable. Standard errors are clustered at the district level. Significant at *** p<0.01, ** p<0.05, * p<0.1.

1.5.2.2 Schooling

Effect of Childhood Exposure to Illegal Activities on Human Capital— I start by analyzing the effect on primary schools outcomes. Children attend primary schools between the ages of 6 and 11. Table 1.3 presents the results. Column (1) shows that there is no effect on enrollment. This is the case for all primary school grades, which is consistent with the fact that 96% of students are enrolled in primary school. In Columns (2)-(3), achievement measures are used as the dependent variable. The increase in coca prices during the period of analysis led to a 30% increase in the proportion of students with higher age for the grade, and a 28% increase in the probability of failing compared to the baseline levels. In addition, students in the second grade exhibit lower scores in math and reading. These results imply that children working in coca fields are technically enrolled but they are spending less time studying. Moreover, the fact that there is no effect in enrollment suggests that test scores results are not biased by selection of particular students taking the exams. I also find no evidence that the proportion of students that took the national exam over the number of scheduled students is affected when coca prices increase.

Table 1.3: The effect of coca prices on primary school students

	(1)	(2)	(3)	
	Enrollment	Age for grade	Failed	
$PriceShock_{d,t}$	0.027	0.020***	0.793***	
	(0.027)	(0.003)	(0.146)	
Mean of dependent	112.03	.20	8.35%	
Observations	433,696	433,696	425,905	
Number of schools	36,874	36,874	36,860	
	Math score	Lowest math level	Reading score	Lowest reading level
$PriceShock_{s,t}$	-12.353**	4.519*	-7.915*	-3.202
	(5.272)	(2.706)	(4.322)	(2.778)
Mean of dependent	515	50%	519	21%
Observations	95,039	70,847	95,034	70,759
Number of schools	13,581	11,853	13,581	11,843

Notes: This table presents the estimates from Equation 4.1 where $PriceShock_{s,t}$ is the interaction of prices with $DenCoca_{s,2002}$ which ranges from 1-5, 1 indicates 0.1-1 ha of coca per km^2, category 2 indicates 1.1-2 ha/km^2, 3 from 2-4 ha/km^2, 4 from 4-8 ha/km^2 and 5 more than 8 ha/km^2. All specifications include school and year fixed effects as well as department specific time trends. Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.

Next, I analyze whether secondary school students are affected by the increase in coca prices. Table 1.4 presents the results on secondary school enrollment. There is a large decline in enrollment rates at 8th grade, suggesting that students may be dropping out of school at 7th grade when they are in the transition between primary and secondary education. These results are consistent with Census data that show that most dropouts occur when children are entering secondary education. Figure A4 shows a histogram of the educational attainment. There is a clear jump in 7th grade when students are about 12 and 13 years old: the probability of dropping out of school increases in this grade and decreases once students move to higher grades. [40] Therefore, this result suggests

[40]One concern with this specification is the lack of effects on enrollment at older ages given that some 9th

that students in the transition between primary and secondary education are dropping out to work full time in this industry and potentially working in the transformation and trafficking.

Table 1.4: Coca price shocks and secondary school students (enrollment)

	(1) All grades	(2) Grade 7 (Age 12)	(3) Grade 8 (Age 13)	(4) Grade 9 (Age 14)	(5) Grade 10 (Age 15)	(6) Grade 11 (Age 16)
$PriceShock_{d,t}$	-0.026	-0.011	-0.090***	-0.068*	0.019	0.037
	(0.027)	(0.021)	(0.029)	(0.041)	(0.058)	(0.064)
Mean of dependent	225.48	53.55	49.47	45.168	40.776	36.507
Observations	135,595	135,595	135,595	135,595	135,595	135,595
Number of schools	12,850	12,850	12,850	12,850	12,850	12,850

Notes: This table presents the estimates from Equation 4.1 where $PriceShock_{s,t}$ is the interaction of prices with $DenCoca_{s,2002}$ which ranges from 1-5, 1 indicates 0.1-1 ha of coca per km^2, category 2 indicates 1.1-2 ha/km^2, 3 from 2-4 ha/km^2, 4 from 4-8 ha/km^2 and 5 more than 8 ha/km^2. All specifications include school and year fixed effects as well as department specific time trends. Standard errors are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1.

Robustness Checks— Table A26 presents several robustness checks. Panel (A) adds coca specific time trends and year effects interacted with baseline characteristic to the model. The estimates are robust to this specification. Panel (B) implements a more demanding comparison by controlling for district-by-year fixed effects. All coefficients reflect only differences within districts in a given year. Although the estimates for achievement measures are in some cases smaller, they are qualitatively similar. For enrollment, I find that results are similar in significance and in magnitude. These results rule out that schooling effects are driven by any changes over time at the district level. Second, given that coca production in 2002 can be endogenous, I check the robustness of the results by replacing the main treatment variable by my coca suitability index. Panel (C) shows that results

to 11th graders were also exposed to high coca prices during grade 7th. However, since there are also 9th to 11th graders that were exposed to low coca prices during grade 7th, it is more difficult to find an effect on these grades. Nevertheless in the robustness check section I study the effects on the dropout rate rather than enrollment rate.

do not change. Results are similar in significance and in magnitude, providing further evidence that previous results are not driven by endogenous coca production measures. Finally, Panel (D) redefines my treatment using the 1994 measure at the district level and results are similar.

I further check the robustness of the results by replacing enrollment by the number of students who drop out of school as the dependent variable. This variable is only available for the years 2004 to 2014 and for a subset of schools. Again, I find that dropouts are concentrated in 7th grade. Finally, I analyze whether the achievement of secondary school children is affected by the increase in coca prices. I find positive but non-significant effects.[41] This suggests that while primary-school-age children are affected on the intensive margin, secondary-school-age children are affected on the extensive margin.

Taken together, these results show that the expansion of the drug industry in Peru had a large negative impact on children in coca districts relative to districts with little or no coca. I argue that one of the main mechanisms driving the schooling results is through participation in the drug industry. The schooling results are concentrated among 6 to 14 years old, which are the ages when child labor increases. Moreover, even within districts, effects are larger for schools located in high intensity coca cells, providing additional evidence on the child labor channel. Regression results are consistent with the qualitative evidence: younger children are harvesting coca leaves, attending school less often, or reducing their school effort. Older children who enter secondary school are likely to be involved in other parts of the cocaine industry as well, including production and transportation. The results imply that these are the students who drop out of school entirely.

1.5.2.3 Other Potential Mechanisms Behind Schooling Results

In this section, I provide evidence that schooling results are not driven by an increase in violence, cocaine consumption, or changes in the supply of education.

[41] Results are available upon request.

Changes in violence— Another possible pathway through which the expansion of the cocaine industry could impact schooling is by increasing the murder rate for young children, affecting returns to education. There is evidence that this mechanism is important for understanding the effect of the crack cocaine epidemic in the US (Evans et al., 2012). Expansion of coca production in Colombia in the 1990s also led to violence (Mejía and Restrepo, 2013). However, if schooling results are driven by violence or murder rates we should also find that secondary students achievement would be affected. I find that this is not the case. Moreover, there is no reason to believe that violence would affect schools differentially depending on the intensity of the coca cells within each district. Nevertheless, using police data at the department level I visually inspect whether homicides and terrorism in these areas increased immediately after the Colombian shock.[42]

Figure A6 presents the number of homicides and terrorist events over time across both coca and non-coca areas and there is no increase after the shock in 1999. This is consistent with qualitative evidence suggesting that violence and terrorism have not increased in these areas. According to a coca farmer with whom I spoke during my field work "There is no need to use bullets when money can get the job done." Also news articles note that "Peru has also so far avoided the levels of bloodshed that have rocked Colombia and Mexico, with Peruvian traffickers apparently relying more heavily on bribes than bullets."[43] Finally, I also check whether affected cohorts are more likely to be affected by homicides using police record of victim's district of birth and age in 2011 and 2015. I find no evidence that individuals affected by high coca prices during childhood are also more likely to be victims of homicides.

Changes in the supply of education— It is possible that schooling results are driven by changes in educational resources in affected areas. For instance, it could be the case that teachers' attendance decreases or turnover rates increase, affecting student outcomes. While I do not have access to data on teacher attendance, the fact that I find differen-

[42]Unfortunately, there is no data publicly available at the district level.
[43]Source: Business Insider, 02/09/2016.

tial effects by grade suggests that results are driven by changes in the behavior of specific cohorts and not changes that would affect the entire school. To rule out the possibility that teachers are directly affected by coca production, I check whether there is an increase in adult farm employment and find no effect. I also analyze whether there is a decrease in the number of teachers per school and find no effect. Finally, there is no effect on the quality of teachers as measured by the number of teachers with a post-secondary degree.

In addition, the effects I find are not driven by cocaine consumption. While it is possible that using cocaine affects student's schooling directly, most adolescents in Peru do not consume cocaine before the age of 16.[44] Moreover, during my sample period most cocaine consumption is concentrated in the main cities since it is an expensive product.

1.6 The Long-Term Consequences of Early Life Participation in the Illegal Industry on Crime and State Legitimacy

Thus far, the analysis has focused on how the expansion of cocaine affected children's short-run outcomes, but it may also affect long-run adult outcomes. Relative to those working in the legal sector, children who harvest coca leaves may be more likely to follow a criminal path later in life. In this section, I study the long-run effects of the drug industry on crime and trust in institutions by examining cohorts most affected by the increase in coca prices. Comparing the effects with other commodities and exploiting variation in districts where coca production is legal, I also show that effects are consistent with the development of industry-specific human capital, namely criminal capital.

[44] For more information see *Estudio sobre prevencion y consumo de drogas en la poblacion general de Lima Metropolitana y El Callao*, Technical Report, 2014, ComisiÃşn Nacional para el Desarrollo y Vida sin Drogas, Public Health Department, Peru.

1.6.1 Econometric Specification

1.6.1.1 Baseline Econometric Specification

In order to examine the long-term effects, I estimate the effect of high prices during childhood at relevant schooling ages. Identification comes from coca price variation at different ages and from coca suitability across districts of birth. For each adult individual at the time of the survey, I construct the full history of coca prices at different ages. In this way, I assess whether changes in prices during childhood have a long-term impact on adults who were born in districts with high coca suitability.

Equation 3.2 presents the specification:

$$Y_{d,c} = \beta^x \underbrace{(PriceAge_c^x \times Coca_d)}_{PriceShockAgex_{d,c}} + \alpha_d + \delta_c + \sigma_d c + \gamma_t + \epsilon_{c,d} \quad \forall x = 6, ..., 17 \tag{1.3}$$

where d indexes the district of birth and c the birth year. $Coca_d$ is the number of coca hectares in the district of birth in 1994. $PriceAge_c^x$ is the log price of coca at different ages during childhood, where x ranges from age 6 to 17. For example, if we want to see an effect of prices at the age of 10 for an individual born in 1985, $PriceAge_c^{10}$ will be equal to prices in 1995. The term δ_c indicates year of birth fixed effects and controls for specific cohort effects. The term α_d indicates district of birth fixed effects and control for time-invariant characteristics of the districts that may be correlated with both childhood exposure and future incarceration.

Control variables are not available for all years of birth. Therefore, to control for potential changes across districts of births I include district specific cohort trends, $\sigma_d c$. As a robustness check, I also include department-by-year fixed effects. District specific cohort trends account for the differential economic development and enforcement measures of each district through time. Furthermore, this isolates the deviation in the outcome from the long-run trend in a given district of birth. Given the spatial and time correlation in the error terms, I cluster standard errors by district of birth, allowing for an arbitrary variance-covariance structure within districts.

The parameter of interest is β, the effect of experiencing the boom in coca prices during childhood, which is identified from variation in prices across districts and birth cohorts. Therefore, the control group is composed of those who were born in the same district but in a different year, and those who were born in a different district but belong to the same cohort. Given that schooling and child labor results were driven by children between age 6 and 14, there should be no long-run effect for cohorts that were affected at older ages. Therefore, as a falsification test, I also analyze whether there is an effect for older individuals.

For the earnings and trust outcomes, I use household surveys from 2011 to 2015. Therefore, I also controls for year of survey fixed effects, γ_t.[45] I also control for individual-level covariates including gender, poverty, migration status, population, and region. As in the main specification, I cluster standard errors by district of birth.

1.6.1.2 Addressing Potential Concerns

One potential concern is that increased enforcement may be correlated with exposure to the coca shock. I address this issue by comparing outcomes for individuals in different cohorts within coca districts. In addition, for the sub-sample of individuals with village of birth information, I classify village of birth using the geographic cells from the satellite images.[46] This more granular classification allows for the inclusion of district-by-year fixed effects. I compare individuals who were born in the same district and year across villages with different coca density. This specification controls for any district-by-year-specific shocks to incarceration rates across districts, such as those resulting from

[45]Nevertheless, results are invariant to controlling for year of survey.

[46]The incarceration data from the first quarter of 2016 includes village of birth information, but the 2015 data does not.

changing enforcement at the district level. Given that most of the enforcement decisions are made at the district level, this helps rule out effects from differential enforcement.

Another potential concern is that the results are driven by adult exposure to criminal activity. To address this concern, I show that prices at older ages do not predict future incarceration. In addition, to directly control for adult exposure to criminal activity, I check the robustness of the results by restricting the sample to individuals that were in prisons located outside of coca districts. Identification comes from exposed individuals who move to a non-coca district, and therefore were not likely to be directly exposed to coca during adulthood.

As in the short-run analysis, areas that grew coca in 1994 are classified as suitable for coca. To address the concern that this may be endogenous, I check the robustness of the results using the coca suitability index based on agro-ecological variables and coca satellite images.

To control for the fact that incarceration may be trending upwards, but at different rates across Peru, I include department-by-year of birth fixed effects. In addition, since arrests of affected cohorts may be correlated with overall changes in policing in Peru, I control for year of arrest fixed effects. Using the incarceration data, I construct a panel of arrests by year. I measure the probability of being incarcerated in a particular year given that the individual was born in a coca district and experiences high coca prices at a particular age. For this specification, the data is aggregated so that there is one observation for each combination of year, district of birth, and year of arrest. Since there are individuals of the same age arrested in different years, I am also able to separate the age effects. Finally, I analyze whether effects are driven by differential mortality across cohorts and district of birth.

Peru was affected by intense civil conflicts between 1980 and 1993, including the rise of the Shining Path.[47] Although this is before my main period of analysis, it is possible that

[47] Leon (2012) shows how human capital accumulation decreases due to civil conflicts in Peru.

the oldest cohorts in the sample were affected during childhood. To control for the possibility that individuals affected by the coca shock could also be affected by civil conflicts, I control for the number of victims due to civil conflict per district per year of birth.

I also perform the following robustness checks. First, I check whether cohorts that were age 6 to 14 at the first peak of prices (in 2002) are most affected. In this specification, variation comes from their age in 2002 interacted with coca suitability measures. For instance, I can compare individuals who were 12 at the beginning of the coca boom versus individuals who were 17 at that time. Second, for the years that data are available I check the robustness of results by instrumenting prices by the number of coca hectares in Colombia. In Equation 3.2, I do not instrument prices given that historical data on the number of hectares in Colombia are not available. However, this is less of a concern because future incarceration rates are less likely to affect changes in coca prices during childhood. Moreover, short-term effects documented in the previous section are robust to not instrumenting coca prices. This is consistent with the view that changes in the price of coca are due to demand shocks from the U.S. and Europe and supply shocks in other source countries. Third, for comparison, I analyze the long-term effects of legal commodities which also increase child labor such as coffee and gold. Shocks to gold prices generated an increase in child labor of similar magnitudes as shocks to coca prices, making it a particularity relevant comparison.[48] In the case of gold, I define the shock as the interaction of mineral gold deposits per district in 1970s with international gold prices instrumented by gold exports of top producing countries.[49]

[48] Gold is an important commodity in Peru and children often work in gold production. For example, Santos (2014) finds that the boom in international gold prices increased child labor in Colombia.

[49] The international price of gold was obtained from World Bank (Global Economic Monitor Commodities Database). The volume of exports was obtained from `http://atlas.media.mit.edu/` and the gold deposits were obtained from the United States Geological SurveyâĂŹs Mineral Resource Database.

1.6.2 Results for Long-Run Effects

In this section, I provide evidence that adult criminal behavior is significantly affected by exposure to the drug industry during childhood. Cohorts born in coca districts who were exposed to high prices between age 11 and 14 are more likely to be incarcerated as adults.

Next, I examine the mechanisms driving these results by examining other outcomes as well as heterogeneous effects. Taken together, the evidence implies that exposure to the drug industry during childhood increase industry-specific human capital, leading to future criminality.

I show that incarceration effects are mainly driven by the increase in child labor. Moreover, I find the largest effects for violent and drug-related crimes with no effects on other crimes. In addition, the incarceration effects are concentrated in districts producing coca illegally, rather than districts producing coca for the legal section. I show that affected individuals are less likely to trust government institutions, which may affect state capacity in regions with a large illegal sector. Results are robust when examining only exposed individuals not currently living in the coca district. This implies that it is not adult exposure to the drug industry that affects adult outcomes but exposure during childhood. Moreover, there are no long-term effects for individuals who experience high coca prices after the age 14. Finally, results are not driven by an increase in violence and enforcement in coca areas.

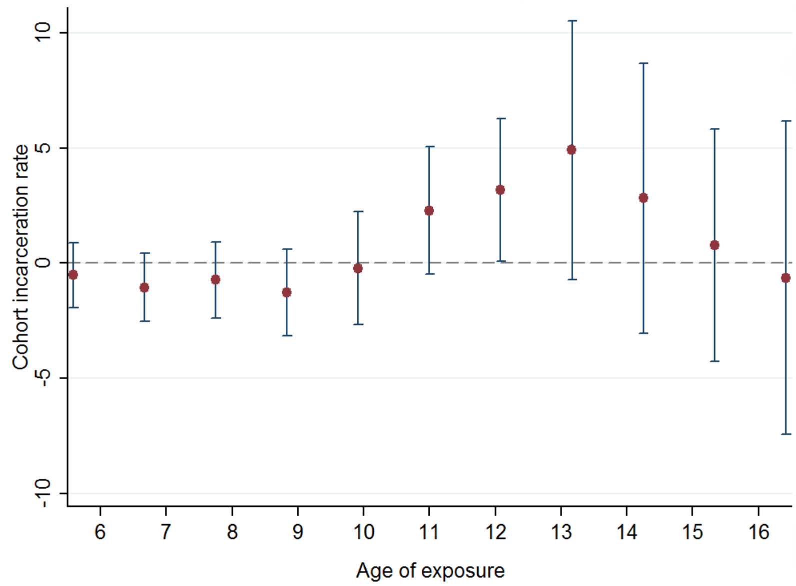
1.6.2.1 Criminal Paths

Effect of Childhood Exposure to Illegal Activities on Criminal Behavior—I start by estimating the incarceration effects of being exposed to high coca prices at different ages of childhood. The dependent variable is the number of individuals in prison per cohort-district of birth divided by the population born in that cohort-district per 1000 individuals. Figure

1.6 shows that effects start increasing when children are exposed at age 11 and dissipate if they are exposed after age 14. These results are consistent with the previous child labor estimates showing that large and significant effects are concentrated between these ages. Also, it is at these ages when children drop out of school in Peru since they are in the transition between primary and secondary education. This finding is also consistent with previous literature showing that it is at these ages when children are more susceptible to neighborhood characteristics and criminal peers (Damm and Dustmann, 2014).[50]

I then estimate Equation 3.2 but interacting coca suitability with the average prices between the ages of 11 and 14 for each individual. Table 1.5 presents the results. Results show that higher prices during the relevant ages lead to a statistically significant increase in the subsequent probability of being an offender in a district-cohort. The increase in coca prices after the eradication efforts in Colombia induces a 30% increase in the probability of incarceration of an individual who grew up in an average coca district in Peru. Column (2) presents the estimates for narco-trafficking crimes and the magnitude of the estimates double, which is consistent with an increase in industry-specific capital during childhood. Children who start working in this industry at an early age are more likely to be involved in the drug trade later on.

[50]Previous research has also shown that exposure to adverse events during the transition between primary and secondary schooling can have a long-term effects on years of schooling and earnings (Shah and Steinberg, 2013).



Notes: These graphs plot the coefficients obtained from a regression of the incarceration rate on the interaction between the coca suitability in the district of birth and price at different childhood ages. The regressions control for district of birth, district time trends, and cohort fixed effects. The Y-axis shows the estimated coefficients and the X-axis shows the ages. Standard errors are clustered at the district level.

Figure 1.6: Incarceration rate effects by age

In the incarceration data I cannot observe individuals who entered the prison and were released before the beginning of my sample. This could bias my results if these individuals are not evenly distributed across treatment and control groups. Notwithstanding, most individuals in prison are serving sentences that average eight years. To address this concern, however, Column (3) in Table 1.5 uses the length of the sentence as the dependent variable. The estimates are non-significant, suggesting that measuring the sample conditional on being in prison is a good proxy for the total number of convicted individuals in a given year.

Table 1.5: Coca prices during childhood and subsequent criminal behavior

	(1) All	(2) Drugs	(3) Sentence length
PriceShock Age11to14$_{d,c}$	3.607***	2.127***	20.277
	(0.906)	(0.680)	(19.273)
Effect for avg district	+29%	+62%	+14%
Mean of dependent	4.5	1.2	98.37

Notes: *PriceShock Age11to14*$_{d,c}$ is the interaction between the coca suitability in the district of birth and the log average prices between the ages of 11 and 14. All specifications control for district and year of birth, as well as district specific time trends. Standard errors clustered at the district of birth level. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Another potential concern with the results presented in Table 1.5 is that the time-series correlation in the exposure to illegal markets might be affecting my estimates. One way to indirectly test this is to include other age bins in the same regression. I do this in Table 1.6. Column (1) estimates the effects of childhood exposure to high coca prices on individuals who are between 18 to 30 years of age.[51] Results are consistent with the estimates of Figure 1.6. It shows that results are mostly driven by those who were below the age of 14. In particular, there are larger effects for those who experienced high prices at the critical period between the ages of 11 and 14. In Column (3), I estimate the effect for older individuals who are between 28 and 39 years old. In both samples, there are large increases in drug related crimes. This shows that childhood exposure to the boom in coca prices not only increases an individual's chance of committing a crime as a young adult (in their early 20s) but also later in life (around their 30s). This suggests that affected cohorts remain on a criminal path well into adulthood. Moreover, higher prices between the ages of 15 and 19 do not affect the probability of being incarcerated, providing further

[51] I separate the sample so that I have the same number of cohorts between young and old offenders and so that each sample has enough variation in prices. On the one hand, I have old offenders who were children in 1982 to 1999 and thus were affected by the first period of expansion of the drugs industry in Peru and also by the fall in prices from the shut down of the main air bridge. On the other hand, I have young offenders who were children in 1993 to 2009 and thus were affected by the fall in prices and the expansion induced by Colombian policies.

evidence that results are driven by exposure prior to adolescence.

Table 1.6: Coca prices during childhood and subsequent criminal behavior

	(1) All	(2) Drugs	(4) All	(5) Drugs
PriceShock Age6to7$_{d,c}$	1.239	0.543	-0.331	-0.008
	(0.817)	(0.601)	(0.932)	(0.502)
PriceShock Age8to9$_{d,c}$	1.409	0.734	1.654	1.613**
	(0.929)	(0.758)	(1.009)	(0.699)
PriceShock Age10to11$_{d,c}$	2.645**	1.604	1.609	1.720*
	(1.205)	(1.016)	(1.228)	(0.943)
PriceShock Age12to13$_{d,c}$	4.734**	2.820	1.962**	1.686**
	(2.182)	(1.767)	(0.831)	(0.748)
PriceShock Age14to15$_{d,c}$	5.003	1.097	1.393*	1.018*
	(3.284)	(2.616)	(0.756)	(0.583)
PriceShock Age16to17$_{d,c}$	-0.060	-1.286	0.271	0.036
	(2.844)	(1.955)	(0.429)	(0.369)
PriceShock Age18to19$_{d,c}$			0.566	-0.204
			(0.961)	(0.825)
Observations	23,853	23,853	22,028	22,028
Sample	18-30	18-30	28-39	28-39

Notes: *PriceShock Age6to7*$_{d,c}$ is the interaction between the coca suitability in the district of birth and log average coca prices at different ages. Results are robust to different ranges of bins. All specifications control for district and year of birth, as well as district specific time trends. Standard errors clustered at the district of birth level. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Robustness Checks— To control for the fact that incarceration may be trending upwards, but at different rates across Peru, Panel (A) in Table A10 adds department-time fixed effects. It also controls for trends that may arise because younger cohorts have had less time to be arrested, and the degree of measurement error for younger cohorts may vary by department. In addition, since convictions of affected cohorts may be correlated with overall changes in policing in Peru, in Panel (B), I control for year of arrest fixed effects.

To account for potential endogeneity of coca producing areas, Panels (C) and (D) present the results using the coca suitability index and instrumenting for coca prices during childhood using the number of hectares in Colombia. These specifications produce similar estimates.

Panel (E) employs district-by-year of birth fixed effects. Individuals from a village located inside a coca geographic cell (identified by satellite images) who experienced high prices at key ages are 50% more likely to be incarcerated for drug-related crimes. Any potential confounder needs to mimic the evolution of coca prices and differentially affect villages within a district that have higher coca suitability. The similarity with previous estimates at the district level suggests that differences across districts over time that are not accounted for in the main specification are not important for explaining variation in incarceration rates.

To rule out that effects are not driven by civil conflicts, Panel (F) controls by the number of victims per year and district of births. Results do not change.

As an alternative approach, I compare cohorts who were between 6 and 14 years of age in the year of the first peak in prices versus cohorts who were older that year. The omitted category is those older than 16. Table A11 in the Appendix presents the results. The results are consistent with the previous estimates. Being between 11 and 14 years old at the time of the peak increases the chances of being an offender by 25% and it is significant at the 1% level. However, these results should be taken with caution because different ages at the time of the boom also reflect a change in the potential years of exposure to illegal labor market activities. Therefore, these estimates are a combination of the age at the time of the peak in prices and the years of exposure.

Finally, to check that results are not driven by differential mortality across cohorts and across regions with different coca suitability, I use homicide data from the police for the years 2011 and 2013.[52] These data contain information about the district of birth and age of victim. I repeat the analysis in Equation 3.2 but replace the dependent variable with homicide victims per capita. Figure A8 presents the results. There is no differential effect by age and district of birth. I also analyze whether the size of the cohort changes and find no effect.

[52] These are the years in which there are available data.

Compliers Characteristics— To gain insight into whether individuals who are more likely to be incarcerated due to high coca prices during childhood (i.e. compliers) were the ones who were affected by child labor and schooling, I investigate the labor, schooling, and family characteristics of offenders. I can compute the proportion of compliers who have characteristic X using two-stage least squares:[53]

$$D_{c,d} = \phi Treated_{c,d} + \kappa_d + \nu_c + \pi_d c + \chi_t + \mu_{c,d} \tag{1.4}$$

$$X_{c,d} \times D_{c,d} = \beta D_{c,d} + \alpha_d + \delta_c + \sigma_d c + \gamma_t + \epsilon_{c,d} \tag{1.5}$$

where $D_{c,d}$ is the number of individuals who are in prison per cohort and district of birth. For ease of interpretation, I redefine the treatment as a discrete variable. I define $Treated_{c,d}$ for those whose average prices in the key ages were above the median and who were born in a district with coca. I also check the robustness of the the results using the continuous treatment variable (the interaction of coca prices at specific ages and coca suitability). Note that $X_{c,d} \times D_{c,d}$ is the number of individuals per cohort who are in prison and have characteristic X (e.g., less than a high school degree).

The coefficient β gives the proportion of compliers with characteristic X. Results for the characteristics of compliers can be found in Column 1 in Table 1.7.

[53] I calculate the proportion of compliers that had characteristic X as:

$$P(X_i = 1 | D_{1i} > D_{0i}) = \frac{E[X_i D_i | Z_i = 1] - E[X_i D_i | Z_i = 0]}{E[D_i | Z_i = 1] - E[D_i | Z_i = 0]}$$

where Z_i is a binary variable and takes value of 1 if the individual was induced to treatment and 0 if she was not induced to treatment D_i. D_{0i} is the value that D_i would have taken if $Z_i = 0$ and D_{i1} if $Z_i = 1$. In my case it indicates those cohorts who were induced to be in prison. X_i is an indicator for whether the individual has characteristic X.

Table 1.7: Compliers Characteristics

	(1) Compliers	(2) Population
Has less than high school education	0.819***	0.585
	(0.219)	[0.493]
Has more than high school education	0.181	0.415
	(0.219)	[0.493]
Had farming as last occupation	0.598**	0.333
	(0.264)	[0.471]
Participated in illicit activities before age 18	0.776**	0.500
	(0.381)	[0.500]
Had friends in illicit activities before age 18	0.425	0.372
	(0.340)	[0.484]
Had a family member in jail	0.425	0.314
	(0.263)	[0.464]
Experienced gangs in neighborhood during childhood	0.466	0.505
	(0.337)	[0.5]
Experienced violence in their family during childhood	0.434**	0.486
	(0.174)	[0.5]

Notes: Column (1) presents the β estimates from Equation 7, which represents the proportion of individuals in prison due to the shock that have a particular characteristic. All specifications control for district and year of birth, as well as district specific time trends. Standard errors clustered at the district of birth level are in parenthesis. Standard deviations are presented in brackets. Significant at *** p<0.01, ** p<0.05, * p<0.1.

About 80% of those who were affected by the shock had less than a high school degree. I also repeat the analysis using an indicator for whether each offender's occupation was farming and find that about 60% of affected individuals declared farming as their main previous occupation. Finally, I analyze the compliers' family characteristics and childhood conditions. When comparing these proportions to the proportions in the actual population in column (2), about 80% of compliers have participated in illicit activities before the age of 18 and 43% had at least one of their family or friends in prison. In the general population, those percentages are 50% and 31%, respectively.

These results are in line with the previous short-term results showing that the most affected individuals were those who were also affected by their schooling and had a farming background. Moreover, the fact that an overwhelming proportion of affected cohorts also participated in criminal activities before the age of 18, suggests that coca shocks put children on a criminal path and that adult criminality is likely related to criminal capital acquired during childhood. I discuss this mechanism in more detail in the next section.

1.6.2.2 Mechanisms behind the Criminal Paths

There are two main mechanisms that could be driving the effects on adult criminal activity, an increase in criminal capital or decrease in formal human capital.

On the one had, it could be the case that illegal labor market opportunities increase criminal exposure during childhood and these investments in industry-specific criminal capital increase the benefit of future involvement in the industry.[54] This criminal capital may include knowledge of how the industry works obtained from interactions with individuals at various stages of cocaine production or social capital, such as contacts with buyers.

On the other hand, results could also be explained by a reduction in schooling during childhood. As children dropout of school, they may have fewer opportunities and lower wages in the formal sector, increasing future involvement in crime. I also examine other potential mechanisms such as exposure to violence and adult criminal exposure. In this section, I argue that the results are primarily due to the development of criminal capital during childhood.

Type of Crime—Table 1.8 presents the results by type of crime. The effect is concentrated on drug-related crimes, suggesting that crime-specific human capital is developed at the

[54] Previous literature studying the effects of incarceration (Mueller-Smith, 2016; Bayer et al., 2009) suggest the development of criminal capital while in prison. Also, recent work has highlighted the importance of peers at schooling ages. Carrell et al. (2016) shows how exposure to disruptive peers in school affects adult earnings.

expense of productive human capital. I also find smaller significant effects for violent crimes and no effects for other crimes. These results are also consistent with the criminal capital channel. Children exposed to the drug industry continue to work in the drug trade as adults, leading to incarceration for industry-specific crimes such as drug trafficking and murder.

In addition, if I control for the average schooling of the offenders in the main specification, the magnitudes remain similar.[55]

Adult Criminal Exposure— An alternative hypothesis is that adult outcomes are driven by adult exposure to illegal industries rather than childhood exposure. To explore this, I examine individuals exposed to the illegal industry as children but who were incarcerated in non-coca districts as adults. In particular, I divide the sample between individuals who are in prisons in coca areas and individuals who are in prisons outside of these areas, which is a proxy for where they lived as adults.[56] Table 1.8 shows that there are significant effects for individuals who are incarcerated outside coca areas, implying that exposure during childhood affects adult criminality even if individuals are not exposed to the illegal sector as adults.[57] This finding suggests that individuals bring their criminal capital with them if they migrate to areas that do not produce coca.

[55]This result is available upon request and should be interpreted with caution. Given that schooling is also affected by the drug industry, this specification could suffer from the *bad control* problem.

[56]In Peru, the location of imprisonment is usually close to the location of the crime.

[57]Figure A9 in the Appendix presents the results for different ages and I find similar results.

Table 1.8: Coca prices during childhood and subsequent criminal behavior

	(1) All	(2) Drugs	(3) Violent	(4) Sexual	(5) White collar	(6) Other
			Panel A, All Prisons			
PriceShock Age11to14$_{d,c}$	3.607***	2.127***	1.304***	-0.107	0.046	0.237
	(0.906)	(0.680)	(0.397)	(0.193)	(0.103)	(0.355)
Effect for avg. district	+29%	+62%	+19%	-2.6%	+11%	+6%
		Panel B, Prisons located in Non-Coca Districts				
PriceShock Age11to14$_{d,c}$	2.253***	1.275*	0.881***	-0.136	0.062	0.172
	(0.815)	(0.673)	(0.327)	(0.181)	(0.091)	(0.263)
			Panel C, Women			
PriceShock Age11to14$_{d,c}$	0.384**	0.266**	0.062	0.000	-0.000	0.059
	(0.163)	(0.129)	(0.047)	(0.001)	(0.000)	(0.076)
			Panel D, Legal Coca			
PriceShock Age11to14$_{d,c}$	4.091***	2.542***	1.379***	-0.305	0.030	0.445
	(1.055)	(0.812)	(0.425)	(0.215)	(0.124)	(0.343)
PriceShock Age11to14$_{d,c}$ × *Legal*	-3.749**	-2.654***	-0.965	0.151	0.049	-1.174
	(1.817)	(0.999)	(1.779)	(0.271)	(0.170)	(1.435)

Notes: *PriceShock Age11to14*$_{d,c}$ is the interaction between the coca suitability in the district of birth and the log average prices between the ages of 11 and 14. Standard errors clustered at the district of birth level. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Women—Panel (C) in Table 1.8 presents the results for women. I find that affected women are more likely to be incarcerated for drug trafficking, but not other offenses. This is consistent with the qualitative evidence that women are primarily involved in the non-violent parts of the drug trade. Moreover, the fact that women are also affected by the drug-trade suggests that involvement in violence during childhood is not driving the results.

Coca production for the legal sector—I examine the differential effect for districts where most coca production goes to the legal sector.[58] In these districts, coca leaves are legally

[58]These districts are located in the region of La Convencion and Lares, which has historically produced coca for traditional use. To sell to the legal sector, farmers have to be registered with government agencies. In general, prices for legal coca are much lower than prices paid by the illegal sector.

sold for medicinal and religious purposes.[59] Therefore, workers have less contact with the cocaine industry. I interact the shock with a dummy indicating whether the individual is from an area where coca is produced legally. For the fully saturated model in Panel (D), I find that individuals from these districts are less likely to be involved in drug-related crimes during adulthood. The entire effect on drug-related crimes is due to areas with illegal coca, not areas with legally grown coca. This provides further evidence that exposure to the cocaine industry, rather than coca production alone, leads to the formation of industry-specific criminal capital and future criminality.[60]

Other commodities—Table A12 estimates the effects of price shocks for legal commodities-âĂŞ-coffee and gold-âĂŞ-on incarceration rates. Child labor is sensitive to price changes for both commodities. In the case of gold, the magnitude of the child labor shock is similar to that of coca. However, I find no evidence that early life participation in these sectors affects subsequent incarceration rates. This provides further evidence that it is child labor in *illegal activities* that drives the results, rather than child labor alone. This suggest that children acquire industry-specific criminal capital when working in the illegal sector, but not in the legal sector.

Other channels—I have argued that criminal capital develops during childhood from learning-by-doing through direct participation in the drug industry and interactions with other participants. However, it could also be the case that children acquire criminal capital even if they do not work in the drug industry (e.g. through peer effects). While I cannot completely rule this out, three pieces of evidence suggest that industry-specific criminal capital is developed by working in the illegal industry directly and not merely by general exposure. First, a vast majority of affected individuals (i.e. compliers) state that they were involved in illegal activities before the age of 18 and report farming as their last occupation. Second, the incarceration effects are primarily driven by exposure

[59]Coca leaves are often consumed in teas or chewed directly.

[60]Table A12 in the Appendix presents the short-run effects on child labor and I find no differential effect in areas where most coca is produced legally.

during the ages when child labor increases the most. Third, even within a district-year, effects are larger for individuals who were born in a high coca density cell compared to individuals born in low coca density cells in the same district. If it was only general exposure, all children in coca districts would likely be affected. In Section 1.7, I provide further evidence that child labor in the illegal sector can play an important role for future criminality.

Another potential mechanism could be exposure to violence. It could be that children who grew up in a household with exposure to the drug industry may also be more exposed to violence, leading to future incarceration. Three pieces of evidence suggest that this is not the main mechanism driving the results. First, if exposure to violence was the main mechanism, violence should also affect older or younger individuals. Second, I do not find evidence that violence increased in the short-term in these areas, suggesting that individuals were not exposed to significant changes in violence. In addition, the complier analysis suggests that families of affected individuals were not more violent, as measured by self-reported family violence during childhood. Finally, exposure to violence would not explain why there are larger effects for drug-related crimes.

1.6.2.3 Trust and State Legitimacy

In the previous sections, I showed that affected cohorts gain industry-specific human capital and are more likely to follow a criminal life path. In line with the criminal capital mechanism, exposure to illegal industries may have broader implications for democratic values and state legitimacy. As individuals become more involved in the drug trade they may learn how to work under weak rule of law, changing their attitude towards the state. In particular, I examine satisfaction with democracy as well as trust in various government institutions, including police, congress, judicial system, and political parties using data covering the period 2011 to 2014. These outcomes have been widely used to study state legitimacy in the context of developing countries.

Table 1.9 presents the results using the same specification as the previous section. Column (1) shows distrust in the regional government in general while Columns (2) to (4) show the coefficients for specific institutions as dependent variables. I find that individuals who were most exposed to the cocaine industry have more distrust in the police, regional government, and congress. This finding is consistent with the fact that during cocaine expansion, corruption increases, potentially affecting these institutions.[61]

Table 1.9: Coca prices during childhood and subsequent distrust in institutions

	(1) Government	(2) Police	(3) Congress	(4) Justice	(5) Democracy	(6) Politicians
PriceShock Age6to14$_{d,c}$	0.463**	0.324*	0.594**	-0.270	0.331*	0.482**
	(0.185)	(0.181)	(0.276)	(0.280)	(0.191)	(0.203)
PriceShock Age15to18$_{d,c}$	0.048	-0.004	-0.173	0.057	0.048	0.053
	(0.170)	(0.132)	(0.146)	(0.119)	(0.128)	(0.083)
Observations	30,253	30,253	30,253	30,253	30,253	30,253
District FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Department Trends	✓	✓	✓	✓	✓	✓

Notes: The dependent variable for columns 1-4 is a dummy that indicates whether the respondent does not trust the regional government (column 1), police (column 2), Congress (column 3), and Judiciary Power (column 4). Column (5) presents the results using a dummy that indicates whether the individual believes democracy does not work well in the country as the dependent variable. Column (6) is a dummy that indicates whether individuals think that democracy does not work well due to bad politicians. Standard errors clustered at the district of birth level. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Column (5) shows the estimates using satisfaction with democracy as the dependent variable. Exposure to the drug industry during childhood increases the belief that democ-

[61] While government may fight against the illegal drug business, they also often take a portion of profits. In interviews, one coca farmer noted that, "We can only trust locals, police take most of our merchandise [coca and cocaine] and re-sell it at higher prices." An NGO administrator working in the area said that "it is the police and people from outside the area who regularly steal cocaine." In addition, at least 115 local politicians have been prosecuted for involvement in drug-trafficking (Ministerio del Interior, 2015).

racy does not work. Column (6) shows that there is an increase in the belief that democracy does not work because of bad politicians. In addition, there are no effects for older cohorts.[62]

Overall, the expansion of the drug industry reduced trust in institutions for those highly exposed to the illegal industry. This may have important implications for development. Distrust in institutions can lead to lower levels of social capital, which many view as a key ingredient for development (HorvÃąth, 2013; Baliamoune-Lutz, 2011; Ahlerup et al., 2009).

1.7 Can Parental Incentives Mitigate the Effects on Child Labor, Drug Production and Crime?

So far I have shown how the expansion of the drug industry in Peru affected children, leading to long-term consequences for adult criminality and trust in institutions. In this section, I study how policy can address the underlying mechanisms that lead to future criminality by changing parental incentives. I exploit the differential rollout of a conditional cash transfer program (CCT) during the period of high coca prices. This second experiment helps further disentangle the mechanisms of the previous results and sheds light on the role of policy to mitigate the incarceration effects.

First, I examine the incentive to grow coca given the effect on the present value of lifetime earnings for parents and their children. In Section 4.9.3, I estimate the short- and long-run effects on income using the same identification strategy as in Equations 3.1 and 3.2. In the short-run there are income gains from the shock to coca prices. However, individuals who are affected by high coca prices as children earn 30% less in the long-run. I use these estimates to calculate the change in the present discounted value of earnings

[62]Ideally, it would be possible to separately estimate the effect for each age. However, due to the lack of statistical power I rely on age bins.

across generations when individuals are exposed to the coca shock relative to those not exposed. Assuming a 5% discount rate, I find evidence that the present income gains from growing coca do not compensate the future income losses for children. This is true even though the calculation does not take into account the cost associated with the increased probability of incarceration when individuals grow coca.

In Section 4.9.3, I develop a simple framework to analyze parents' choice to use child labor given the long-term effects for their children. In the model, parents choose whether to employ their children on the farm or send their children to school. Their utility is assumed to depend on the current earnings, as well as the present value of their children's future utility. Motivated by the empirical results, there are frictions that cause parents to not fully internalize the future cost of growing coca for their children. There are many potential reasons for these frictions such as credit constrains, myopia, or lack of information. The lack of information may be particularly important given that a survey from my field work shows that 65% of farmers do not believe that school is a better investment for children than working on coca farms.[63]

The model suggests that parents may decrease the use of child labor if there are additional incentives to send children to school. In the next section, I use the gradual rollout of a conditional cash transfer (CCT) to test to what extent results are driven by parental decisions in areas affected by the coca shock. In addition, I shed light on how to maximize the effects of the policy.

1.7.1 Direct Effects of Conditional Cash Transfer on Child Labor and Drug Production

In this section, I show that incentives for parents to send their children to school mitigates the effect of exposure to the illegal drug industry. In particular, the CCT program

[63] The survey was conducted on a sample of about 300 coca farmers. I describe the survey in more detail in Appendix 4.9.6.

reduces child labor and drug production, as well as improves schooling outcomes. I also provide suggestive evidence that the CCT reduces adult incarceration rates when children grow up in coca areas when coca prices are high.

The CCT program consists of a monthly lump-sum payment of about 30 dollars. This amount does not depend on the number of children in the household. The transfer is given to mothers conditional on their children having 85% school attendance, complete vaccinations, and pre- and post-natal care.

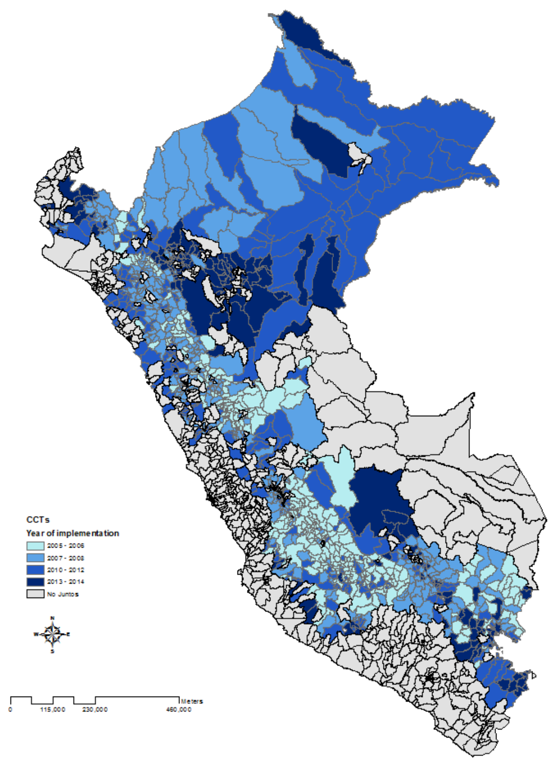


Figure 1.7: Rollout of CCTs

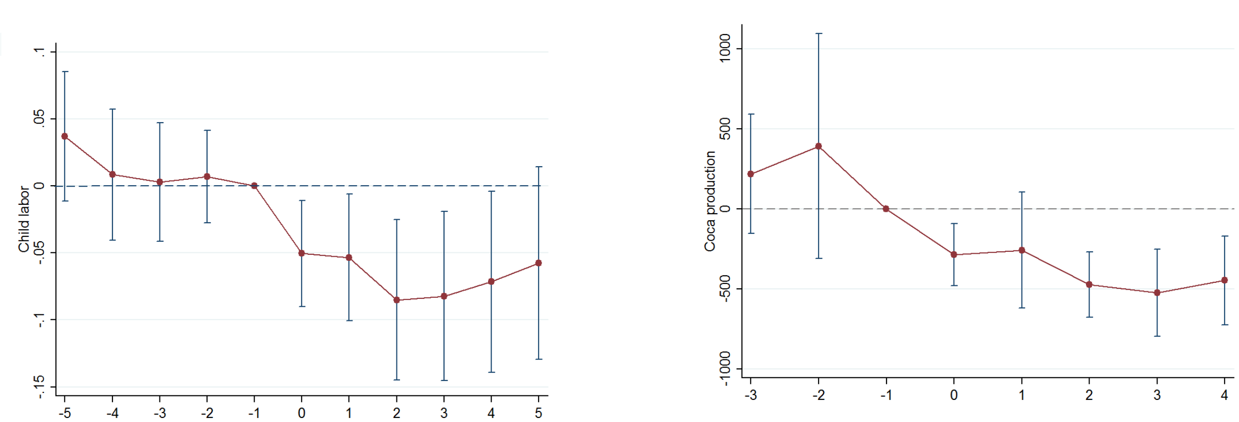
Figure 1.7 presents a map showing the rollout of the CCT program. There is substantial variation across districts and across years. By 2014, about 1,400 districts—covering 80% of the coca-growing districts—had CCTs. There were two large expansions in in 2007 and then in 2012. I find that average coca production was similar in both periods, suggesting that coca districts were not treated first. The selection of districts was based on an index that includes poverty and percentage of villages affected by violence during civil conflict.

All regressions control for trends in this index. Notice that in the previous analysis, results did not change when controlling for this index. Thus, this suggests that the pre-trends assumption is satisfied. Nevertheless, using an event study analysis, I examine whether there are any pre-trends.

I start by estimating the effect of CCTs in an event study analysis:

$$Y_{i,d,t} = \alpha + \sum_{i=-4}^{5} \beta_i(\tau_{d,t} = i) \times Coca_d + \alpha_d + \phi_t + \sigma_r t + \epsilon_{i,d,t} \tag{1.6}$$

where τ_{dt} denotes the event year, defined so that $\tau = 0$ for the year the CCT program started in that district, $\tau = 1$ for one year after the CCT started, and so on. For $\tau \leq -1$, households were untreated by the CCT. The coefficients are measured relative to the omitted coefficient ($\tau = -1$). Figure 1.8 plots the event and year coefficients from estimating Equation 1.6 using child labor as the dependent variable. The results support the validity of the identification strategy, showing an absence of a strong pre-trend and evidence of a trend break after the introduction of CCTs, decreasing child labor. This evidence suggests that potential confounders would have to mimic the timing of the CCTs' expansion extremely closely.



(a) Child labor relative to years with CCT

(b) Coca production relative to years with CCT

Figure 1.8: The effects of CCTs when coca prices are high

Table 1.10 presents a fully saturated version of Equation 3.1 including interactions with

$CCT_{d,t}$, a dummy indicating whether the district d has a CCT in year t. I find that CCTs decrease child labor by 15% in coca areas. [64]

Table 1.10: CCTs and coca price shocks on child labor

	(1)	(2)
	Child labor	Child labor
$PriceShock_{d,t}$	0.145***	0.237**
	(0.040)	(0.102)
$PriceShock_{d,t} \times CCT_{d,t}$		-0.116**
		(0.053)
Mean of dependent	0.25	0.25
Observations	233,824	233,824

Notes: This table presents the estimates of a fully saturated model of Equation 3.1 with interactions with $CCT_{d,t}$, a dummy indicating whether the district d has a CCT in year t and 0 otherwise. Results are robust to the inclusion of trends by the index for which they selected districts and differential trends by the stage of treated. Standard errors clustered at the district level are shown in parenthesis. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Next, I analyze whether this reduction in child labor led to a reduction in coca production. I use data on coca production from 2010 to 2014, the years for which data are available. Given that I only have data for years with high coca prices, I estimate a model excluding the price interactions. Panel (B) in Figure 1.8 presents the event study estimates and shows that coca production decreases after the introduction of CCTs. I find that CCTs decreased coca production by 34%.[65]

I assess whether the magnitudes are plausible by comparing the effects of the CCTs with the effects found from the increase in coca prices in the 2000s. During the boom in coca prices in the 2000s, I find that a 30% increase in child labor is associated with an 80% increase in coca production (this is the increase observed during the period of analysis). Assuming that there is no substitution between child and adult labor, a 15% decrease in child labor would then lead to a 40% decrease in coca production, close to the observed

[64] The magnitude is consistent with experimental evidence in rural Mexico where CCTs decrease child labor by 22% (Skoufias et al., 2001).

[65] On average, the CCT reduces the area in which coca is grown by 200 hectares. On average, districts were producing 600 hectares during this period.

reduction of 34%.[66]

Table A17 presents the effect of CCTs on schooling outcomes. I find that the CCTs mitigate the negative effects on schooling. The exception is the age for grade outcome, where I find no statistically significant effect.

Finally, I turn to incarceration outcomes. Only 3% of the incarceration sample was affected by both the coca shock and the CCTs during the relevant ages. Nevertheless, Table A18 suggests that incarceration effects are also mitigated when individuals have the CCTs in their early teens. There are no effects at older ages.

These findings have two implications. First, they help to disentangle the mechanisms driving the previous estimates. If long-term effects were driven by other factors apart from child labor in the criminal sector, increasing the returns to schooling would not mitigate the effects. Second, they shed light on the role of policy. Policy makers can actually solve some of the problems related to criminal involvement by incentivizing the development of formal human capital. Not only does this have implications for reducing coca production in the short-run, but these policies could also reduce future criminality by putting individuals on a non-criminal path.

1.7.1.1 What is Driving the Effects? Is it the Conditionality?

In this section I discuss two mechanisms that could explain these results: income effects and schooling-work substitution effects. Monetary transfers increase incomes, potentially reducing the incentives for those who are economically motivated to produce coca for the illegal market. Previous literature has showed how welfare transfers in the U.S. reduced economically motivated crimes (e.g., Jacob et al., 2015; Foley, 2011). Also, Chioda et al. (2015) find that a CCT program in Brazil reduced youth crime by 18% and argue that the results are driven by income effects. CCTs may also affect drug production

[66] Moreover, if they are imperfect substitutes, one would expect coca production to decrease by less than 40 percent.

by incentivizing schooling. Children may be substituting time working on the farm and illegal activities with time spent in school. This is in line with the schooling incapacitation effect found in the crime literature. For example, Anderson (2014) finds that an increase in the minimum high school dropout age led to a reduction in juvenile arrest rates in the US. Similarly, the CCT program in Peru may increasing the time children spent in school, reducing the time for coca production.

To analyze the income component of CCTs, I check whether effects are driven by lower income areas. I also examine how adult labor responds to the introduction of the CCT. I find that results do not change when stratifying by poverty, suggesting that income effects are small. Moreover, if CCT effects were mainly driven by income effects, we would expect no effects on adult labor. However, Table A28 shows that adult labor increases after the introduction of the CCTs.

To shed additional light on the schooling-work substitution mechanism, I provide qualitative evidence showing that children have to work many hours in coca farming. Testimonies from my field work indicate that the coca harvest is done from 4 am to 4 pm during the week and from 6 am to 2 pm over the weekends. This implies that any increase in schooling is likely to reduce the available time for picking coca leaves.

1.7.2 Geographic Targeting of Conditional Cash Transfer Program

In this section, I explore the optimal geographic allocation of the CCT program. First, if the government objective is to reduce child labor and coca production, and thus future criminality, CCTs should be targeted toward coca districts where the likelihood of becoming a criminal is higher. Second, it is important to take into account spatial spillovers. In the same way that coca production spilled over from Colombia to Peru, CCTs can create spillovers across coca suitable areas in Peru. Previous research has found that increased enforcement can shift criminal or illegal activities to adjacent areas.[67] Therefore, I examine whether CCTs shift coca production and child labor to other suitable districts that did not have the policy and then use these estimates to provide an alternative allocation of CCTs that accounts for these potential spillovers.

1.7.2.1 Spillover Effects

In this subsection, I show that when a CCT program is introduced, child labor and coca production increase in neighboring coca districts where parental incentives for schooling are not provided.

In order to test whether the CCTs impact coca production in other districts without the policy, it is necessary to specify where spillovers are likely to occur. Using data from my field interviews, I assume that spillovers move to suitable neighboring districts since intermediaries may have specific contacts and resources such as trucks that are relatively immobile in a particular area. In addition, qualitative evidence suggests that when coca production decreases in one district, local intermediaries increase coca prices, generating an increase in coca production in nearby districts. In Peru there are about 13 coca/cocaine markets, each market containing several districts. Each market has intermediaries who supply the international market. These intermediaries have social capital (e.g. reputation with coca farmers) and physician capital (e.g. trucks) specific to each markets.[68] Therefore, if coca production decreases in one district, coca production is likely to move to neighboring coca district that are more suitable.

When CCTs are introduced in one district, child labor decreases, costs to produce increase, and therefore, production decreases in the district. Although there may be substitution to adult labor, it is more expensive. In response to the shift in supply, the intermediary increases the coca price in the area and the neighboring districts are now willing

[67]For example, Dell (2015) shows that drug enforcement policy in certain municipalities in Mexico diverted drug trafficking to other municipalities.

[68]The intermediary can be a local family firm or a foreign drug cartel.

to produce more.[69] Although, I am not able to test the price mechanism, the qualitative evidence suggests the presence of spillovers effects. I empirically test for these spillover effects in the next section.

1.7.2.2 Empirical Strategy and Results

In order to estimate spillover effects of CCTs on neighboring coca areas, I first define the set of districts over which I expect the spillovers to be positive. These districts are those who are neighbors of at least one coca district based on the number of hectares produced in 1994. For these districts, I analyze the impact of neighboring districts with coca and CCTs. In other words, I study whether districts that neighbor a coca district that has the CCT program experience more child labor due to price shocks. I use the following specification to test spillovers:

$$\begin{aligned} Y_{d,t} = {} & \beta_1 PriceShock_{d,t} + \beta_2 PriceShock_{d,t} \times NeighborCCT_{d,t} \\ & + NeighborCCT_{d,t} + \alpha_d + \phi_t + \sigma_r t + \epsilon_{d,t} \end{aligned} \tag{1.7}$$

where $PriceShock_{d,t}$ is defined as the interaction of coca prices and coca production in 1994 instrumented by hectares in Colombia (see Equation 3.1) and $NeighborCCT_{d,t}$ is coded as 1 if the neighboring district has CCTs in year t.[70] The sample excludes years when district has CCTs themselves. For example if district d_1 has a neighbor d_2 that has the program in 2005 and district d_1 gets CCTs in 2009, district d_1 will only be coded as 1 for the period 2005 to 2009. The main regressor $PriceShock_{d,t} \times NeighborCCT_{d,t}$ is coded as 1 if district d has a neighboring district with both coca and CCTs in year t. I interact

[69] Note that Rozo (2014) finds evidence that enforcement in Colombia shifted production internationally rather than to neighboring districts. This may be due to the fact that coca farmers in Colombia expected enforcement in all districts, whereas coca farmers in Peru were not affected by major drug enforcement policies.

[70] I estimate the full saturated model that includes interactions with $NeighborCCT_{d,t}$. To simplify the exposition, these interactions are not shown in Equation 1.7.

this dummy with the coca intensity and the logarithm of coca prices. β_2 measures how shocks to prices affect districts that are neighboring coca areas that have CCTs. A positive β_2 means that there is a larger impact for districts that are bordering a coca district with CCTs and also have a high share of coca compare to districts that are bordering coca but do not have CCTs.

Table 1.11 shows that coca districts that are bordering a district with CCTs experience a larger increase in child labor than those that are not bordering CCTs. β_2 is positive and significant, implying that there are positive spillover effects arising from coca districts that are bordering a district that receives the transfer. Child labor increases in neighboring coca districts by 7%. In an alternative specification, I keep the sample of coca districts that are bordering another coca area and estimates the effect of having CCTs. Column (2) shows that results do not change under this specification. I also test to what extent farmers may send their children to work in the neighboring districts that do not have CCTs. In Columns (3) and (4) I check whether there are spillover effects on migration of adults and children and I find no effect.

Table 1.11: CCTs and child labor, spillovers in neighboring districts

	(1)	(2)	(3)	(4)
	Child labor	Child labor	Migration	Migration
$PriceShock_{d,t}$	0.200*		-0.090	-0.015
	(0.115)		(.110)	(0.113)
$PriceShock_{d,t} \times Neighbor\ CCT_{d,t}$	0.046**	0.038**	0.0161	0.011
	(0.021)	(0.017)	(0.021)	(0.013)
Observations	170,814	46,581	170,814	998,647
Sub-sample of coca neighbors		✓		

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the estimates of a fully saturated model of Equation 3.1 that includes interactions with $NeighborCCT_{d,t}$, a dummy indicating whether the neighboring district d has a CCT in year t and 0 otherwise. Results are robust to the inclusion of trends by the index for which they selected districts and differential trends by the stage of treated. Standard errors clustered at the district level are shown in parenthesis.

Next, I conduct a falsification test. If spillover effects are driven by a reduction in coca production of neighboring districts due to CCTs, districts that are bordering a non-coca district with CCTs should not have spillover effects. I find that all the effects are driven by the districts that are bordering areas with CCTs but also had coca. There are no effect due to neighboring district with CCTs that are not producing coca. This suggests that it is not the direct effect of CCTs but the fact that the transfer program is reducing coca production in the bordering district.

In Table A20, using coca production from 2010 to 2014, I directly study the spillover effects on coca production and find that it increases in the neighboring districts by 19%. In this specification, I do not include the price interaction because I have no price variation. Results are robust to the inclusion of department and coca specific trends. Moreover, effects are robust to clustering the standard errors at the regional level.

1.7.2.3 Cost-Benefit Analysis of Using CCTs to Reduce Drug Production

The aim of the CCT program was to reduce poverty and increase human capital investments. In the previous sections, I show that it also has the benefit of reducing coca production by about 15% after taking into account the negative spillover effects. In 2012, the total cost of the program was about $212 million, providing CCT payments to about 600,000 households (the annual cost per household is about $340).[71] The average number of households in coca districts is 4,000. Even assuming that all households in coca districts are given the transfer, these estimates imply that it would cost an average of $1.4 million to implement the CCT in each coca district.

I can use these estimates to compare the cost of using CCTs to reduce coca production with the cost of increased enforcement. Spending $1.4 million on the CCT reduces coca production by about 100 hectares. For comparison, Mejía et al. (2015) find that reducing 800 hectares of coca through an eradication program would cost between $20 and $27 mil-

[71] These figures come from Peru's Ministerio de Desarrollo e InclusiÃşn Social.

lion. Therefore, it costs about $11 million to reduce coca production by the same amount through CCTs. Moreover, eradication programs have been shown to have unintended consequences by increasing violence and affecting local economic development (Abadie et al., 2014; Rozo, 2014).

These estimates suggest that, relative to standard enforcement measures, CCTs can be a much more cost effective way to reduce production in the illegal drug market. Other policies that encourage schooling or limit the develop of criminal capital by promoting the legal sectors may also have positive returns. It is important to note that there may still be important interactions between increased enforcement and other policies that affect incentives for formal human capital.

1.7.2.4 Alternative Allocations of CCTs

In this section, I use my previous estimates to shed light on the optimal allocation of CCTs taking into account spillovers based on coca suitability. I assume that the objective function of the social planner is to reduce child labor in coca districts subject to a binding constraint that limits the number of districts with the CCT program. I develop an algorithm that maximizes this objective function. Results imply that that there are two potential strategies for a social planner. First, it may be optimal to allocate CCTs to districts with high suitability that neighbor districts with low suitability in order to reduce spillovers. Alternatively, it may be optimal to implement the CCT program in clusters of high suitable districts that all neighbor one another.

The idea of this exercise can be illustrated with the following example. Suppose we have two coca markets: coca market 1 is composed by two districts, A and B that are both very suitable for coca production, and coca market 2 where district C is very suitable and district D has very low suitability. Now given these markets, where is it better to implement CCTs if we could only choose one district? Given that spillovers occur in productive neighboring districts, it may be better to allocate CCTs to C since the elasticity of production with respect to price is low for the neighboring district. I formalize this idea in the following model.

The main assumptions in the model are: i) the government objective function is to minimize child labor in coca districts but can only choose a subset of districts, ii) spillovers can only occur in neighboring districts that can produce coca and are part of the same coca labor market, iii) if a district has the policy it does not receive spillovers from other districts, and iv) the spillover effects of each district are independent of whether other districts in the market have the policy.

Let $\mathcal{S} = \{1, 2, \ldots, S\}$ be the set of *districts* and A be an $S \times S$ matrix where each cell (i, j) is the spillover effect of *district* i on district j if the policy is implemented in i but not in j, and the diagonal elements of A ($[a_{ii}]$) is the effect district i gets when the policy is implemented in i, which depends on each district coca suitability.

We have two sets of binary decision variables: n_i ($i \in \mathcal{S}$) and m_{ij} ($i, j \in \mathcal{S}; i \neq j$). n_i is equal to one if the policy is implemented in i, zero otherwise, and m_{ij} is equal to one, if the policy is implemented in i but not in j, zero otherwise.

The objective is to maximize the effect of policy on districts:

$$\max \sum_{i \in \mathcal{S}} a_{ii} n_i + \sum_{i \in \mathcal{S}} \sum_{j \in \mathcal{S}, i \neq j} a_{ij} m_{ij} \tag{1.8}$$

subject to three sets of constraints:

- *Spillover constraint*,

$$m_{ij} = \begin{cases} 1, & \text{if } n_i = 1, n_j = 0 \\ 0, & \text{otherwise} \end{cases} \quad , \quad i, j \in \mathcal{S}, i \neq j$$

which can be written in linear form as

$$2m_{ij} \leq 1 + n_i - n_j, \qquad i, j \in \mathcal{S}, i \neq j \tag{1.9}$$

$$m_{ij} \geq n_i - n_j. \qquad i, j \in \mathcal{S}, i \neq j \tag{1.10}$$

- *Budget constraint*,

$$\sum_{i \in \mathcal{S}} n_i \leq b. \tag{1.11}$$

where b is the maximum number of *Districts* to be selected for policy.

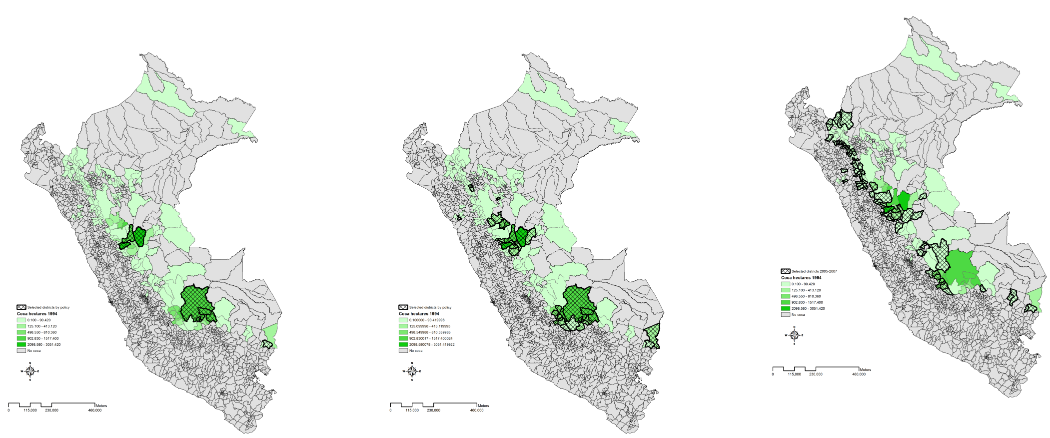
- the decision variables are binary,

$$n_i \in \{0,1\}, m_{ij} \in \{0,1\}. \quad i,j \in \mathcal{S}, i \neq j \tag{1.12}$$

Therefore, the policymaker maximizes the following problem:

$$\begin{aligned}
\text{min} \quad & \sum_{i\in\mathcal{S}} a_{ii} n_i + \sum_{i\in\mathcal{S}} \sum_{j\in\mathcal{S}, i\neq j} a_{ij} m_{ij} \\
\text{subject to} \quad & 2m_{ij} \leq 1 + n_i - n_j,\ i,j \in \mathcal{S}, i \neq j \\
& m_{ij} \geq n_i - n_j,\ i,j \in \mathcal{S}, i \neq j \\
& \sum_{i\in\mathcal{S}} n_i \leq b, \\
& n_i \in \{0,1\},\ i \in \mathcal{S} \\
& m_{ij} \in \{0,1\},\ i,j \in \mathcal{S}, i \neq j
\end{aligned}$$

Panel (A) in Figure 1.9 presents the results of this problem when the government can allocate the CCT to 10 districts (i.e. $b = 10$). We can see that the selected districts are those that are next to districts with low suitability or those that are next to other high suitability district with the CCT program. High suitable districts that are within a cluster of other high suitability districts are only given the CCTs if their neighbors also get the CCTs.



(a) Targeting 10 **(b)** Targeting 40 **(c)** Actual 2005-2007 allocation

Figure 1.9: Targeting considering coca suitability

If we increase the government constraint to 40 districts, it is optimal to target more clusters rather than individual coca districts. These results show that there are two reasons why policy may want to target specific places: i) to reduce spillovers to neighbors, the social planner should target districts that do not neighbor other high suitability coca districts, and ii) to reduce spillovers in the entire area, the social planner should target a cluster of suitable districts together.[72]

Next, in Panel (C) of Figure 1.9, I compare the optimal allocation of CCTs with the actual government selection between 2005 to 2007. During these years, the government implemented the policy in 84 coca districts based on their allocation rule. Most of the selected districts were those with low suitability or those with high suitability neighboring other high suitability districts, generating spillover effects. If the objective is to reduce child labor and reduce future criminality in these areas, these results suggest that there is significant room to improve outcomes by taking into account location-specific characteristics (i.e. the presence of illegal labor markets that are due to coca suitability).

1.8 Conclusion

This paper provides evidence that childhood exposure to illegal markets leads to a substitution from formal human capital to criminal capital, putting children on a criminal life path. I contribute to the literature by showing that geographic conditions can generate future criminality and perpetuate illegal industries, providing an explanation for the persistence of crime and violence in specific locations. I then provide evidence that these effects can be mitigated by changing parental incentives through conditional cash transfers that incentivize schooling.

I emphasize that there are large externalities associated with growing up in an area that specializes in illicit activities. The results suggest that in the long-term, exposure could lead to the formation of criminal groups, undermining state legitimacy. Though the situation in Peru is unique in some ways, there are many other examples of illegal labor markets which might have similar unintended consequences for children. For example, children are heavily involved in opium poppy cultivation in Afghanistan, and often recruited by armed groups funded by the heroin trade.[73] Similarly, in rural Mexico, there are reports that many children work in opium poppy fields rather than go to school. Although child labor is rare in developed countries, children may still be exposed to illegal

[72]Notice that targeting all high coca suitable districts may generate international spillovers, which I am not considering in the analysis.

[73]See "The Opium Economy in Afghanistan," United Nations Office on Drugs and Crime, 2003.

industries, increasing their criminal capital and future propensity to commit crime.[74]

This paper also provides evidence that exposure to illegal industries generates distrust in state institutions, however there may be other important consequences related to state capacity. The presence of illegal labor markets may affect the quality and honesty of elected politicians. This is a particular concern since politicians in areas with large illegal sectors are often found to have connections to the narco-trafficking industry.[75] Relatedly, lower trust in government institutions may lead to substitution towards informal institutions, limiting the ability of the state to reduce drug trafficking. Understanding to what extent informal mechanisms of justice or informal groups may develop in areas exposed to illegal industries, as well as the determinants of politician quality in these areas, is an area for further research.

In the second half of this paper, I focus on whether policy can mitigate the effects of exposure to illegal industries. If location-specific factors affect parental incentives to use child labor in the illegal market and thus create criminality, location-specific policies may be needed to target these incentives. In particular, I find that CCTs reduced drug production and future criminality by increasing the costs of child labor, one of the main inputs. This is relevant not only for developing countries where the expansion of drug production and trafficking has led to high levels of violence in the last decades, but also for international efforts to combat drug production. I argue that the judicious use of policies that decrease criminal capital can potentially be more cost effective than increased enforcement.

Overall, this paper provides a first step at understanding how illegal labor markets function and criminality develops, motivating the use of policies that address the root causes of crime and illegal industries.

[74]In the U.S., drug-related crime is geographically concentrated (e.g. urban Chicago is a hot-spot for heroin trafficking). It is possible that criminal capital plays an important role in this context as well.

[75]In Peru, at least 115 local politicians have been prosecuted for involvement in drug-trafficking (Ministerio del Interior, 2015).

Chapter 2

Long-term Effects of Temporary Labor Demand: Free Trade Zones, Female Education and Marriage Market Outcomes in the Dominican Republic

[1]

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2.1 INTRODUCTION

Rapid periods of industrialization in developing countries have been characterized by increases in female labor demand in the export manufacturing industry (Duflo, 2012; Mammen and Paxson, 2000). Evidence from Bangladesh suggests that at least in the short term, the increase in demand for female factory workers has raised returns to education, encouraging women to invest in human capital, delay marriage, and access the formal labor market (Heath and Mobarak, 2012). But what about the long-term effects? Can temporary labor market opportunities shift developing countries to a "good equilibrium" in female education and associated outcomes? If so, even relatively brief episodes of preferential trade preferences for export industries may have permanent effects on human capital levels and female empowerment.

This paper presents evidence that human capital investments are sustained even when labor market opportunities decrease. Using data from the Dominican Republic, I find a 26% decrease in female dropouts from school during the expansion of the export manufacturing industry, an increase that is sustained up to 10 years after the removal of the main commercial tariff agreement with the US and Asian competition. I argue that temporary increases in female factory jobs can lead to lasting improvements in female status through general equilibrium effects in the education and marriage markets. For instance, when women increase their schooling after a factory opens, they also delay marriage and have a lower chance of divorce. Thus, while individuals are initially more likely to make human capital investments because of larger economic returns to education, the benefits may persist because of changes to the marriage and labor market.

To study the long-term effects of female factory jobs on women's status, I exploit the sudden and massive growth in the textile industry, along with its subsequent decline. In the 1990s, textile manufacturing boomed as free trade zones (FTZs) opened in the Dominican Republic.[2] Since textiles were the biggest source of formal employment for women–about 60% of workers were women–female employment rose.[3] However, in the 2000s, labor market opportunities for women decreased as the textile sector contracted due to

[2] According to the *Consejo Nacional de Zonas Francas* (CNFZ), the free zones are defined as "geographic areas of the country, submitted to special customs and tax regimes, established by law, which permit the installation of companies that focus their production or services towards foreign markets. Free zones are areas limited by gates or walls, where the entrance and exit of people, vehicles or cargo, is supervised by personnel of the General Customs Office. Textile is the activity that has been more developed within free zones companies, since the country is an important exporter to the United States. Other industries of importance are footwear, jewelry, assembly of medical and electronic components, tobacco processing, data services and telecommunications, among others."

[3] The share of women employed in tourism and agro-industry was only 29% and 19%, respectively (Castro et al., 1993).

Asian competition, and female employment declined by about 45%; by 2008, about 70% of women who were displaced from the textile industry were still unemployed. Thus, the decline of FTZs in the 2000s provides insight into whether the effects are sustained in the long-term, even in the absence of these labor market opportunities.

This paper uses a difference-in-difference-in difference (DDD) empirical strategy. I compiled a new data set on FTZs' opening at the province level between 1986 and 2007. I use the timing and location of FTZs' opening as well as the age of women at the time of opening to isolate the impact of labor demand on education and marriage outcomes. Using only provinces that experienced an opening, this approach relies on province variation in FTZs' opening among cohorts of women who are plausibly unaffected by the openings as controls for potentially unobserved confounding factors. Thus, my estimates are identified using differences in the availability of female labor market opportunities among different cohorts of women living in the same province and year.

I show that new opportunities for female employment led to a large and robust increase in women's educational attainment and age of marriage. The opening of an FTZ increases women's educational attainment by 0.3 years, mostly due to an increase in secondary school enrollment. In 1990, 46% of women in the Dominican Republic married before the age 18, the largest share in Latin America. In 2010, after the opening of FTZs, only 36% of women were married before the age 18. I corroborate these results with several other pieces of evidence. First, the pre-existing characteristics of women in each province do not allow one to predict the opening of FTZs. Second, I find that female labor force participation, women's educational attainment, school enrollment, and age of marriage do not have a clear trend before opening of FTZs, but increase afterwards. Third, while the opening of FTZs affects women who are less than 16 years of age at the time the FTZs opened, it does not affect older cohorts. In addition, the opening did not directly affect men's educational attainment. These estimates are robust to different data sources and to the inclusion of household fixed effects.

My results suggest that women increase their education level because additional schooling is rewarded in the labor market. The FTZs created significant demand for female labor, employing the majority of women over the period of analysis. Even though most of the jobs created by FTZs were unskilled, they were better-paying than other labor market opportunities. In equilibrium, I observe that most women working in FTZs have some level of secondary education higher than average educational attainment in other sectors. Given the better pay, women may have competed for these jobs by increasing their educational attainment. In addition, extra years of schooling may be rewarded if education works as a signal of discipline or responsibility during factory hours.

Due to a decline in the demand for textiles, the FTZ no longer provided the same job opportunities in the 2000s. When analyzing the negative shocks, I find that women still increase their educational attainment and postpone marriage in the absence of the economic returns in the labor market. While schooling and age of marriage are affected by the opening of FTZs, these outcomes do not revert back to their previous level after the negative shock. The evidence suggests that this may be because higher educational attainment among some women changes marriage markets. Due to the opening of FTZs, some women obtain more education and delay their age of marriage, changing the equilibrium in the marriage market. These women also match with a higher-quality husband, give birth later, and have children that are more likely to survive infancy.

This paper contributes to the literature studying the effect of labor market opportunities on female education. In particular, my results are closely related to the literature examining how the effect of the expansion of Information Technology (IT) service jobs encouraged women to remain in school and delay marriage by increasing the economic returns to education (Oster and Steinberg, 2013; Jensen, 2010; Munshi and Rosenzweig, 2006). To my knowledge, this paper is the first to present evidence that a temporary shock to labor markets, in this case the rise and fall of FTZs, can have a permanent affect on women's outcomes, which may be due to a change in the marriage market equilibrium. For instance, even after labor market returns decrease, women have an incentive to get more education so they can compete with the older, highly educated women already in the marriage market. Using a long panel, this paper exploits different sources of variation that are relevant for policy since they demonstrate the long-term effects for different cohorts of women.

The evidence in this paper suggests that increases in female educational attainment can produce positive externalities through the marriage market. It also provides evidence regarding the mechanisms driving these changes. In particular, delayed marriage is primarily driven by education rather than female labor force participation. Sivasankaran (2013) argues that a longer tenure in the textile industry reduces early marriage; this paper suggests that while the boom in textiles did increase women's participation, it is not the direct explanation for the delay in marriage age. This paper is also related to previous research that studied how the introduction of oral contraception in the US allowed women to remain in school and pursue longer-term careers without facing a penalty in the marriage market (Bailey, 2010, 2006; Goldin and Katz, 2002). Similarly, female factory jobs decreased the cost of marriage delay and altered the rankings of women as potential marriage partners, favoring those with higher levels of education.

Finally, this paper contributes to the trade literature studying the growth of export processing zones (Atkin, 2012b; Liberato and Fennell, 2007; Schrank, 2008; Raynolds, 2002; Willmore, 1995) by providing evidence on differential gender effects. Atkin (2012b) finds evidence that the expansion of unskilled jobs at attractive wages increases school dropout rates for men of legal working age in Mexico. I come to the opposite conclusion when examining women in the Dominican Republic. In this context, access to well-paid unskilled jobs does not increase the opportunity cost of schooling for workers. This is likely because women are more prone to drop out of school in order to get married, rather than join labor force. In this way, the Dominican Republic is similar to the rest of Latin America–more than 70% of those who drop out of school due to labor force participation are men while 97% of those who drop out of school because of marriage, maternity or household activities are women (SITEAL 2013).

Overall, this paper provides the first evidence that temporary labor demand can move societies to a “good equilibrium" that persists even after job opportunities taper off. The evidence in this paper indicates that labor markets can significantly improve female outcomes in developing countries through general equilibrium effects in the education and marriage markets. While improving female labor market opportunities increases the economic returns to education and thus encourages human capital investments, other factors such as changes in the marriage market, social norms, or intergenerational effects might perpetuate these effects even in absence of economic gains.

The paper proceeds as follows: I provide information about FTZs, background on the Dominican Republic, and data in Section 2. Section 3 describes the empirical methods. Section 4 discusses the results on schooling and mechanisms. Section 5 presents the results on marriage, mechanisms, and some robustness checks. Section 6 examines the effect of negative female labor demand shocks and Section 7 concludes.

2.2 Background and Data

2.2.1 Background on Education and Early Marriage in the Dominican Republic

The Dominican Republic has historically been faced with a number of challenges pertaining female labor market participation. In particular, few women complete secondary school, often because they marry early. More recently, the prevalence of export manufacturing and the high level of female participation in this sector has made the Dominican

Republic well-suited for an analysis of changes in the labor and marriage markets.[4]

The school system in the Dominican Republic is divided into three levels: Initial Level (Preschool) covers children up to 6 years of age; Basic Level (Primary) begins at 6, lasts 8 years (6 to 13 years old) and is compulsory, and Middle Level (Secondary education) covers students from 14 to 18 years of age, lasts 4 years and is not mandatory. According to data from the World Bank, at the secondary level, Dominican Republic is far behind other countries in Latin America. While the average gross enrollment rate for Latin America and the Caribbean was 87.2%, in the Dominican Republic it was about 60%. Moreover, only 40% of students in primary level continue to secondary education (Gajardo, 2007).

One of the main reasons women drop out of school is early marriage. About 42% of women between 20 and 49 years-old married before the age of 18,[5] making the Dominican Republic the country with the highest female marriage rates for this age range in Latin America and the Caribbean.[6] Parents in the Dominican Republic often encourage their daughters to marry as a consequence of poverty and lack of labor market opportunities (ONE, 2010). The social importance given to motherhood also may drive early marriages and early motherhood. Many young women marry early with the intention of becoming mothers. Although these pregnancies are planned, motherhood before 20 is associated with a higher obstetric risk (Pérez and Vargas, 2011). Several previous studies of the Dominican Republic have shown that young women have a higher risk of child and maternal mortality (Caceres, 1998).[7]

2.2.2 History of Free Trade Zones in the Dominican Republic

In this section, I argue that the opening of FTZs was a national policy that was likely uncorrelated with province-specific female educational trends. In addition, I provide qualitative evidence that FTZs created widespread labor market opportunities for women that did not exist prior to the opening. It provided higher wages than other sectors and women working in FTZs tended to be older and more educated. Specifically, I examine the levels of education in FTZs, tourism and agriculture.

[4] The Dominican Republic is the second largest country in the Caribbean with an area of 48,445 km^2 and a population of 193.6 inhabitants per km^2. There are 2.2 million women between 15 and 49 years old in the Dominican Republic (Díaz et al., 2002). The main sectors of activity are the FTZs, tourism and agriculture.

[5] It has similar levels to most Asian countries, where 46% of women are married before the age of 18.

[6] In Latin America and the Caribbean, 29% of women are married by the age of 18.

[7] In addition, a high percentage of married or in union women between 15 to 19 years-old in the Dominican Republic have experienced emotional, physical or sexual violence by their husband or partner (DHS 2007). Early marriage or union might also compromise their ability to negotiate the use of contraceptive methods. There is a higher incidence of HIV among women between 15 and 24 years that are married or in a union (ONE, 2010).

The process of opening FTZs started in 1969 in the province of Romana as part of a national policy that involved import substitution and export promotion. However, it was not until 1984 that the industrial free zones attracted a significant number of new companies and foreign direct investment. Two national policies promoted this expansion: the transition to a free exchange rate and preferential tariff treatment from the United States, such as the Caribbean Basin Initiative (Schrank, 2008).[8]

In 1996, about 500 firms had factories in these zones, an average of 10 firms per FTZ. FTZs became one of the main economic sectors, surpassing the agricultural sector. In 2001, exports from these zones accounted for 32% of the Dominican Republic total exports (Liberato and Fennell, 2007). The free zones were the main generator of foreign earnings and generated 4% of GDP.[9] By 1995, more than 50% of the provinces had at least one FTZ. During the period of analysis, the free zones were the main generator of employment in the country (Buitelaar et al., 1999). In 1996, employment in these zones represented 7.5% of the economically active population.[10] Between 1984 and 1994 employment grew at an average rate of 22% annually, creating a total of 149,185 jobs (see Figure 2.1). This rate was particularly high considering the unemployment rate was 20%. For most of the workers, the alternative to working in FTZs was often unemployment or returning to village subsistence life (Madani, 1999).

The development of FTZs was considered one of the primary reasons for the increase in women's labor market participation (Castro et al., 1993). About 60% of workers in the FTZs were women. This can be explained by employment in textile manufacturing, which was one of the main activities in the FTZs, employing 70% of the labor. The high share of women in textile industries is not particular of the Dominican Republic, Do et al. (2011) provide evidence that across a broad sample of countries, the share of female labor in these industries is the highest among manufacturing sector. Female jobs were concentrated in floor production positions (CNZF 2002). Nevertheless, in contrast with other FTZs in Latin America, the Dominican Republic had many supervisory positions held by women (Madani, 1999).

According to Madani (1999), women became the unintended beneficiaries of FTZs, since many of them did not have access to other formal market employment (with its higher salary and potential benefits).[11] For instance, the share of women employed in

[8]Excluding Mexico, the Dominican Republic received the most of foreign direct investment in the Caribbean and Central America region.

[9]This number goes up to 21% if we consider the industrial zones value added over the manufacturing GDP.

[10]In contrast, the traditional manufacturing sector employed 2.6% of the economically active population.

[11]Garcia Dominguez (2012) has also pointed out the lack of female labor opportunities in the Dominican Republic.

tourism and agro-industry was only 29% and 19%, respectively (Castro et al., 1993). Moreover, wages in these other sectors were lower than in FTZs. In the case of agro-industry, jobs were not only poorly paid but also unstable (Raynolds, 2002). Therefore, the alternatives for women were to be employed in the informal market or to stay at home. Several authors have suggested that the free trade zones were an important factor explaining the decrease in female poverty and unemployment during the 1990s (Willmore, 1995; ILO, 2013).[12]

The average wage in FTZs was higher than the average wage outside the zones (Madani, 1999; Castro et al., 1993). FTZs in the Caribbean and in Central America paid 5%-20% higher salaries than domestic firms.[13] In addition to their wages, workers in FTZs were often paid bonuses based on productivity as well as payments for overtime and piece work (Romero, 1995). According to a 1991 survey, the average monthly wage in FTZs was US$176.10, higher than wages for workers in the agro-industry and tourism industry (Romero, 1995; Castro et al., 1993). Moreover, in the case of the Dominican Republic, the FTZs provided training courses on English, computer use, and sewing (Buitelaar et al., 1999).

Most workers had completed primary education and secondary education. Table A22 presents the percentage distribution of workers based on sex, education and economic sectors in 1991. Only 3% of female workers had no education. Moreover, when compared with agriculture and tourism, the FTZs had a higher share of women who obtained a university degree. Many authors have argued that free trade zones include workers with higher levels of education than other formal sectors (Calzada et al., 2007). By the 2000s, about 90% of women working in free trade zones had a primary or secondary education. This evidence suggests that education might have been an important requirement for getting a job in the FTZs.

[12]Female unemployment rates are particularly high in the Dominican Republic (about 24%). The official definition of unemployment in the Dominican Republic includes people without job that are available to work but did not look for a job in the last week because they think that there are either no job opportunities or too many obstacles. This distinction was made to account for women.

[13]Atkin (2012b) points out that most trade literature finds higher wages and larger returns to skill among workers at exporting firms (Frıas et al., 2009; Goldberg and Pavcnik, 2007; Bernard et al., 1995).

Figure 2.1: Number of Jobs in FTZ between 1970-2010

2.2.3 Data

I use the Demographic Health Surveys (DHS) for the years 1986, 1991, 1996, 2002, and 2007.[14] These surveys provide information on important health, nutrition, and demographic indicators for the Dominican Republic. The target population for DHS is defined as all women of reproductive age (15 to 49 years old) and their young children under five years of age living in ordinary residential households. Table A23 presents descriptive statistics for the sample. A total of 55,956 observations are available for estimation.

I obtain the industry data from the "Consejo Nacional de Zonas Francas", which provides information on the date of opening of each FTZ and the number of female and male employees in each zones. There are a total of 54 FTZs with around 500 firms. In 1986 there were about 10 FTZs which increased to 54 by 2007. On average, each firm has a total of 400 employees. Figure A1 and A2 depict the evolution of FTZs across provinces over time. By 2010, about 75% of the provinces had opened an FTZ. The largest growth in free zones was between 1986 and the 2000s. There is a large degree of variation across provinces and years in the openings. In this paper, I make use of the variation in the opening of FTZs between the years 1986 and 2007, when most new zones opened. I exclude the provinces in which a FTZ opened before 1986 since there is no variation. In addition, I exclude provinces that did not experience any opening (those in white color in Figure A2). Therefore, I exploit variation in the timing of FTZ openings as well as the age of women at the time FTZs open.

[14]This covers all available years.

2.2.4 Geographic Location of FTZs

In my analysis I exploit exogenous variation in the timing of openings controlling for province and year fixed effects. This identification strategy is valid if time-varying characteristics of each province are not correlated with the timing of FTZ openings. However, it is possible that the industrial free zones were opened in places where female education was growing faster. In such a scenario, women may have increased their educational attainment for reasons other than the opening of a FTZ. In this paper, I focus on export industries, which tend to locate near ports or where land and inputs are available (Madani, 1999). Interviews with FTZs' administrators also suggest FTZs were located based on these factors. I assume that these characteristics are not associated with changes in women's years of education and age of marriage. As a first approach to test this assumption, I follow Bailey (2006) by generating province-level characteristics from the 1986 DHS survey. For each province, I construct a dependent variable that indicates the years elapsed between 1986, when there was a large expansion of the free industrial zones, and the year an FTZ opened in each province.[15]

Table A24 reports the results of cross-province regressions of this new dependent variable "time to opening" on 1986 baseline characteristics.[16] Panel A reports the results for demographic characteristics, such as the proportion of women in different age groups, the proportion of households that own farm land, and the proportion of households living in rural areas. Panel B includes social characteristics such as the mean years of education of women and men and the rate of literacy as a proportion of married or in-union women, mean age at first marriage, and mean age at first birth. Finally, Panel C presents the results for labor market characteristics such as the proportion of women working, the proportion of women earning wages or salary, the proportion of women working for their family, and the proportion of women working before and after marriage. None of the characteristics are statistically significant. Moreover, the low r-squared and the fact that FTZs do not seem to be correlated with female education lends credibility to the identification strategy treating the opening of free trade zones as exogenous.[17]

Local governments may have also invested in necessary infrastructure, such as improvement of roads, ports, and airports near the designated zones. However, there is no qualitative evidence suggesting an increase in education, health, or housing investments

[15] In the next sections I will provide further tests of these assumptions.

[16] For this analysis, I did not include those provinces in which an FTZ opened before 1986.

[17] I also repeat the analysis with household characteristics such as type of residence (urban or rural), whether the main source of drinking water comes from piped water, type of toilet facilities, whether the household has electricity, radio, television, refrigerator and car, main floor material, main wall material, and number of household members. I find that none of these characteristics explain the allocation process.

near the zones.[18] Nevertheless, unobservable characteristics may still be correlated with the opening of FTZs. To mitigate this issue, I also include province time trends and touristic zones specific time trends in the main specification and provide a cohort level analysis, an event study analysis, and a falsification test in the following sections.

2.3 Empirical Framework

I exploit the boom in free zones in the Dominican Republic as an exogenous shock to female labor market demand. Figure 2.2 plots the proportion of women working with respect to the year of opening. The x-axis indicates the number of years before or after the FTZ opened.[19] I observe that female labor force participation was increasing after the FTZs opened. I use two identification strategies that exploit this variation. First, I use a difference-in-difference strategy comparing educational attainment across provinces that had FTZ open at different times. Second, I exploit differences in the age of women at the time of FTZ openings using a difference-in-difference-in-difference approach. In this way, I am able to assess which age group was most affected by the policy.

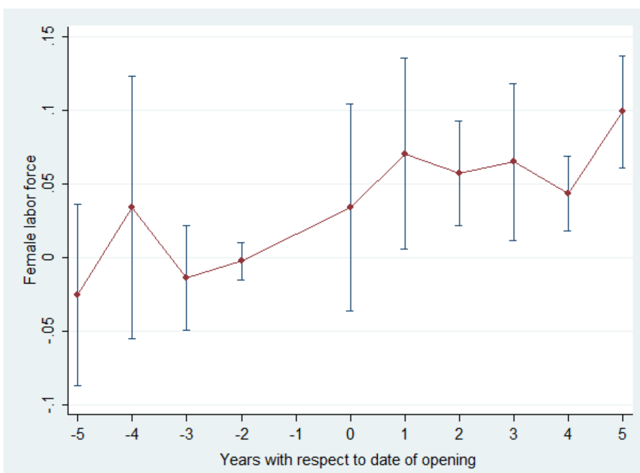


Figure 2.2: Timing of Female Labor Force Participation

Notes: This graph plots the coefficients obtained from a regression of the outcome on the interaction between the treated province dummy and year dummies. The regressions control for province, year and province time trends. The Y-axis shows the estimated coefficients and the X-axis shows the years. Standard errors are clustered at the province level.

[18] In Section 5.4, I also check the effect of FTZs opening on investments in the construction, tourism and municipal expenditures and I find no statistically significant effects.

[19] The x-axis variable was constructed by subtracting the year of the opening from each year of the survey.

2.3.1 Difference-in-difference (DD)

In the first approach, I use the timing of FTZs' openings to isolate the impact of labor demand on women's education and age of first marriage. The provinces chosen for analysis are all provinces in the country with at least one FTZ. I proceed to estimate the effects of FTZs with the following equation:

$$\begin{aligned} Outcome_{i,h,p,t} =& \alpha + \beta FTZ_{p,t} + \delta Province_p + \pi Year_t \\ &+ \theta Trend_p + \gamma X_{h,p,t} + \nu X_{p,t} + \varepsilon_{i,h,p,t} \end{aligned} \tag{2.1}$$

$Outcome_{i,h,p,t}$ is the outcome of women i in household h in province p and year t. $FTZ_{p,t}$ is a dummy variable that indicates the existence of an FTZ in province p in year t. I also include year and province fixed effects, as well as province time trends. Using province fixed effects I am able to control for time-invariant characteristics of the province. $Trend_p$ are province linear time trends to control for any omitted characteristics that vary linearly over time within the province. $X_{h,p,t}$ is a vector of covariates that controls for socioeconomic variables at the level of household h, such as the type of residence, literacy rates, if the main source of drinking water comes from piped water, type of toilet facilities (if they use flush or pour flush toilet), if the household has electricity, main floor material, main wall material, age of respondent and number of household members. $X_{p,t}$ is the number of construction permits per province per year. Moreover, in some specifications, I also include cohort fixed effects and province cohort trends. The goal is to provide precise estimates of β, the causal effect of FTZs. In all the models, the standard errors allow for potential correlation within province and province year clusters.

2.3.2 Difference-in-difference-in-difference (DDD)

As a second strategy, I exploit variation in the age of women at the time of the opening using thresholds in key ages. In the Dominican Republic, basic education is compulsory for those 6 to 14 years-old. Secondary education, which is also public, is not compulsory. Assuming that the FTZs affect women who were less than 15 or 16 years of age, I can exploit variations across cohorts and across households. Moreover, these ages are when most dropouts occur and when the decision to continue high school is made. By using older individuals as a control group, common confounding factors are removed from the estimates and the effects of the FTZs are more precisely measured. This strategy compares the outcomes of women who are affected by the opening to the outcomes of women who are not affected by the opening (first difference) in provinces with an "earlier" FTZ versus provinces with “later" FTZ (a second difference) over time (the third difference). Now Equation 2.1 is identified from joint variation in outcomes in three dimensions: i) provinces that opened FTZs relative to others, ii) after the FTZ opening relative to before and iii) cohorts most affected by the opening relative to other cohorts of young women.

This estimation strategy addresses at least three important endogeneity problems. First, there is a strong association between age and schooling. As a result, comparing the schooling of women less than 16 years old to those over it raises some concerns. This method, however, alleviates this issue because it also compares outcomes of 16-year-old in provinces with “earlier" FTZs to outcomes of 16-year-old in provinces with “later" FTZs. Also, this approach mitigates any concern coming from differences between provinces because it compares individuals of different ages within provinces within FTZs. This technique controls for the potential endogeneity of FTZs opening by differentiating over time. As a result, permanent differences in the characteristics of provinces are taken into account.

2.4 How Can Female Factory Jobs Change Education for Women?

This section discusses the results of the two identification strategies and potential mechanisms for which schooling is affected by the emergence of female factory jobs. The results imply that the increase in female factory jobs led to larger returns to education, creating an incentive for women to increase their educational attainment. While school age women are affected by the opening of FTZs, there are no statistically significant effects for women who were older than 16 years. This is consistent with the fact that most of women tend to drop out of school during high school in order to get married. In addition, I demonstrate that the results are unlikely to be driven by migration, income or government investments in education.

2.4.1 Main Findings

Table 2.1 presents the results of estimating Equation 2.1. I find that the presence of a FTZ increases educational attainment by 0.4 years , equivalent to an increase of about 5% relative to the mean (7.8 years). This result is robust to the inclusion of province time trends and other covariates. In addition to increases in primary, secondary or university education, there also may have been an increase in job training (which may be included in educational attainment since it is a self-reported measure). Therefore, more insight into what level of education the FTZs were affecting is obtained by examining the impact on school enrollment. Columns 2 and 4 present the estimates of the effect on school enrollment and completion. Enrollment in secondary school increases by 4 percentage points (column 3). This is an increase of about a 9% relative to mean enrollment. I do not find any effect on primary school enrollment or completion.

Table 2.1: Schooling and Female Factory Jobs, 1986-2007

	(1)	(2)	(3)	(4)	(5)	
	Years of education	Enrollment in primary	Enrollment in secondary	Complete primary	Complete secondary	Men's years of education
Panel A						
FTZ	0.359***	0.007	0.046**	0.010	0.038**	0.208
	(0.127)	(0.022)	(0.017)	(0.021)	(0.013)	(0.167)
N	55,894	27,975	51,991	39,244	51,949	38,269
R^2	0.124	0.043	0.154	0.145	0.118	0.089
Panel B						
FTZ×age6to16	0.262**	-0.008	0.028**	0.023**	0.025***	-0.015
	(0.122)	(0.009)	(0.013)	(0.010)	(0.009)	(0.173)
N	46,026	23,784	46,067	34,503	46,026	17,234
R^2	0.174	0.042	0.142	0.131	0.118	0.139
Mean of dependent	7,82	0.9	0.46	0.4	0.24	7.28

Notes: In the DDD the control group consists of women between 16 to 30 years of age. Sample restrictions for panel B: I eliminate those women who were more than 30 years of age at the opening. Standard errors are corrected for clustering at the province level. Significant at *** p<0.01, ** p<0.05, * p<0.1

Figure 2.3 decomposes the effects by age and shows that most effects are driven by women who were of schooling age. About 50% of the sample was less than 16 years of age at the time of the opening. Each dot in the solid line is the coefficient of the interaction of a dummy for being a given age at the time of opening and a dummy for an FTZ (a 95% confidence interval is plotted with vertical lines).[20] Each dot summarizes the effect of the between-province variations for a given cohort and can be interpreted as an estimate of the impact of the program on a given cohort. For example, a woman aged less than 16

[20]The omitted category is a dummy for being more than 30 years old at the time of the opening. Ideally, I would like to estimate a coefficient for each age less than 16 but due to the lack of statistical power I rely on age bins. Moreover, if I add extra interaction terms with older cohorts (dummies for 30-40 and 50-60 years old. I find that effects are close to zero and non significant.

at the time of the opening receives 0.3 additional years of education if she is in a region with FTZs.[21] I find that most of the results decrease as the age of the woman at the time of opening increases.

As expected, the FTZ did not have an effect on the education of cohorts not exposed to it and it had a positive effect on the education of younger cohorts. The increase in years of education and enrollment in secondary school is mostly driven by women who were less than 16 years of age at the time of opening. This is consistent with the fact that the average age of marriage in the 1980s was 17 years of age and most women tend to drop out of school in order to get married.

Instead of testing whether each coefficient is equal to 0 for ages that are over 16, I can impose this restriction and estimate a DDD specification. Since most of the effects are driven by women who are less than 16, the omitted group is now composed of women aged 16 or more at the time of opening. Panel B in Table A1 presents the DDD results. All specifications include province, year fixed effects as well as cohort fixed effects and province year of birth trends. Consistent with the results presented in Figure 3, the estimates in column 1 suggest that the opening of an FTZ increases the education of the youngest women by about 0.3 years. In column 2, I present the effect on school enrollment. As before, female labor market opportunities increase enrollment in secondary school by about 3 percentage points but have no effect on primary school enrollment.[22] The opening of an FTZ also in a province increases primary completion by 2 percentage points.

I also check whether men's educational attainment is affected using husbands information. Column 6 shows that there is no effect for men, providing evidence about the importance of the FTZ's for women. In addition, it suggests that results are not mainly driven by investments in the number of schools or any other improvement in education. If this was the case, men would also be affected.

To provide additional insight into the magnitude of these results, I also examine the effect on school dropout rates. Assuming that women between 13 and 16 years of age are at risk of dropping out, an increase of 0.3 years of education is equivalent to a 24% reduction in the dropout rates.[23] About one fourth of the young women who would have

[21]This effect is slightly higher than what was found in Duflo (2001), where the effect of one school built per 1,000 children increased the education of exposed cohorts by 0.2 years. In the case of FTZs, one FTZ is equivalent to 10 jobs per 1,000 inhabitants

[22]These results are similar to the effect found for other developing countries. For instance, ITES centers in India increased enrollment by about 4.1 percentage points (Oster and Steinberg, 2013). Jensen (2010) found an increase of 5.2 percentage point due to an increase in recruitment services for call centers that employed women.

[23]This calculation was done summing up: the number of women who drop out in their 7th grade multi-

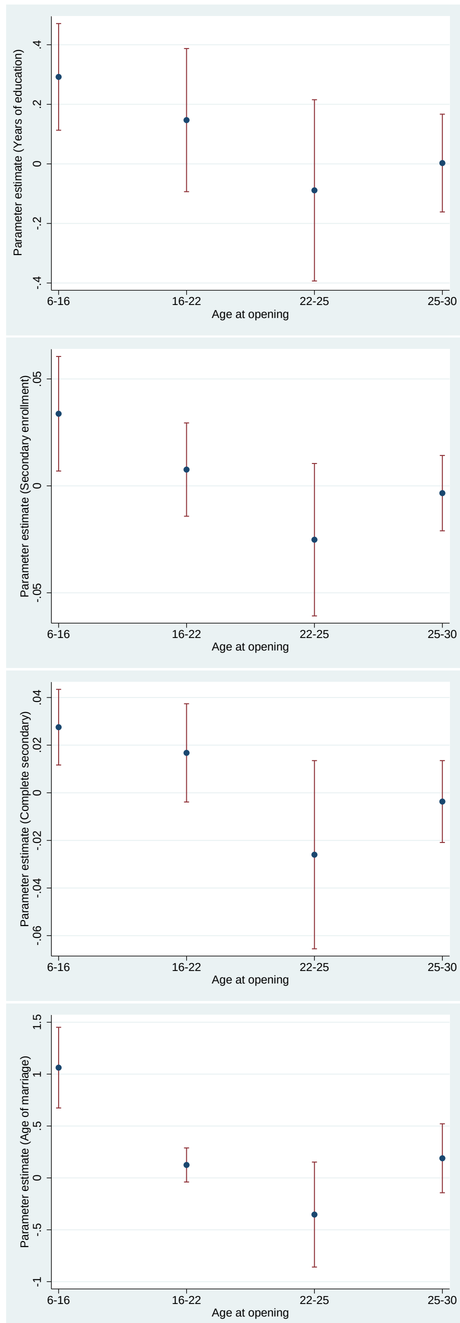


Figure 2.3: Effects by Age at Opening

Notes: These graphs plot the coefficients obtained from a regression of the corresponding outcome on the interaction between the treated province dummy and age at opening dummies. The regressions control for province, year, province time trends, cohort fixed effects, province of birth trends and socioeconomic variables. The Y-axis shows the estimated coefficients and the X-axis shows the age at opening.

dropped out of school are induced to continue their education due to the opening of FTZs (average size of 400 workers per firm).

2.4.2 Mechanisms Behind the Increase in Education

I examine three mechanisms by which FTZs increase the years of education: income, infrastructure investments, and labor market economic returns. First, female factory jobs may generate a direct income effect: as women have access to the labor market, they have more earnings and thus they can increase the education levels of their children. Second, FTZs may have promoted government investments in infrastructure and thus, an expansion in the number or size of schools.

A third possible explanation is that women increased their secondary school attainment because they expected FTZs to reward additional schooling. Even though most of the FTZ' s jobs were floor positions, they were better paid than other labor market opportunities and provided the main source of female employment over the period of analysis. In addition, most of workers had completed secondary, suggesting that education was an important requirement. Competition for these jobs is also a possible explanation for the found education effect. If education increases marginal productivity or provides a signal for beneficial abilities such as discipline or responsibility, women will be also encouraged to educate themselves in order to increase their chance of obtaining a job in this industry.

Table A21 presents the results. I provide evidence that infrastructure investments and changes in income explain only a small fraction of the increase in schooling. Column 1 shows that larger FTZs are not associated with larger educational effects, suggesting that income alone cannot be explaining all the results. I also find an increase in schooling even when there are no women working in the household (column 3). These results are consistent with other research suggesting that women do not continue their schooling due to marriage and childbirths (ONE, 2009). Nevertheless, in the robustness check section, I use other surveys that have data on income to directly control for this channel and find that the magnitude of the effects do not change. [24] In column 2, I find that the results are robust to adding controls for construction permits, which serve as a proxy for government investments in education.

I also check whether results are driven by the expansion of the tourism and construction sectors. The share of GDP from the construction sector increase from 4.9% in 1970

plied by 5 (the potential years of education to finish secondary school), those in 8th grade multiplied by 4, the number of drop out women in their 9th grade multiplied by 3 and the number of drop out in 10th grade multiplied by 2 years. This is comparable with the results found by Oster and Steinberg (2013), where about 26% of out-of-school children are enrolled one year after the introduction of IT centers.

[24] In the Appendix I provide more information about these other surveys.

to 10,4% in 2005 and tourism from 0.4% to 7.1%. Particularly in tourism, the share of the female work force increased from 48% in 1997 to 55% in 2011. Thus, a similar trend could have been the reason of the expansion in the tourism or construction sectors. To address these concerns, I include an specific trend by touristic zones and proximity to the coast and I find that results do not change. Moreover, most of the provinces that experience an opening were not touristic zones. For instance, the province of *Altagracia* that has the main touristic zone (*Punta Cana*) is not in my sample given that it had never experienced an FTZ opening. Thus, results are unlikely driven by trends in the tourism market. I also run the main specification, but replacing the dependent variable schooling with measures of changes in the economic activity: municipal earnings, investments in construction sector and number of hotels (columns 7-8). I find no statistical significant effects on these outcomes, suggesting that changes in tourism and economic activity are not the ones leading to a positive impact in education and subsequently in the marriage market.

Finally, I check the increase in economic returns to education channel by analyzing whether the gap in labor force participation between women with high education and women with low education increases after the FTZs open. I find that there is a greater probability of working for educated women after the FTZs open. Before the FTZs open, about 33% of highly educated women were working in contrast to 43% after the opening. In relation to the type of jobs, after the FTZs opened there is a greater proportion of women working in professional, managerial, technical and skilled manual positions than before the opening. This suggests that the returns to education increased after FTZs opened by providing more employment choices. Moreover, in equilibrium, I observe that most women working in FTZs had some level of secondary education and university education (65%). Furthermore, previous evidence shows that a large portion of wages was based on workers' performance likely making education important.[25]

This mechanism is in line with previous literature that suggests that the introduction of new local job opportunities changes the perceived and actual returns to schooling (Oster and Steinberg, 2013; Jensen, 2010).[26] Although most of this literature focuses on high-skilled jobs, Heath and Mobarak (2012) also provide evidence that the garment industry expansion in Bangladesh increased schooling for women, suggesting an increase in re-

[25]Interviews with workers at FTZs also suggest that these jobs demanded female workers with high levels of education. One reason stated is that education is associated with discipline and responsibility.

[26]Due to the lack of data on the exact location of households, I cannot assess whether the effects are local and therefore driven by women who lived close to the factories. In addition, I cannot disentangle whether effects are driven by better information about jobs opportunities. Nevertheless, given that FTZs were big and provinces in the Dominican Republic are small, it is likely that the effects will extend to a larger geographical area such as a province level.

turns to education even if the female labor opportunities are low-skill.

2.5 What Are the Effects of Female Factory Jobs on Marriage?

In this section, I provide evidence that female factory jobs increase the age of marriage and reduce the probability of marriage before the age of 18. In addition, I show that the same cohort of women that increase their educational attainment also marry later. Furthermore, the results in the marriage are not driven by an increase in female labor force participation. In particular, women increase their educational attainment before marrying and participating in the labor market. Finally, I provide evidence that delaying marriage has positive effects on both infant and mother health including lower child mortality and less domestic violence.

2.5.1 Main Findings

I start by estimating Equation 2.1 by age groups using $AgeMarriage_{i,h,c,p,t}$ as the dependent variable, which is the reported age at first marriage or unions. As for the education outcomes, most of the effects are driven by younger cohorts (see Figure 3). This suggests that one possible mechanism by which women increase their age of marriage is by staying in school longer. The lack of results for women who are not at school at the time the FTZs open is consistent with the context of the Dominican Republic. Data from 2002 census shows that marriage is the main variable explaining dropout rates among women under the age of 18. About 80% of women married within 2 years after stopping their studies. This is consistent with a framework where increasing educational attainment is equivalent to marrying at an older age.

Table 2.2 presents the DDD and the results are similar to the ones presented graphically in Figure 3. I find that FTZs increase the age of marriage by 1.2 years for those not married before the FTZ opened. I also estimate a similar model replacing the dependent variable with a dummy that takes the value of 1 if the woman was married before age 18.[27] I find that the probability of early marriage declines by 10 percentage points for the youngest cohorts, representing a change of 21% from baseline.

I assess the importance of two channels through which the opening of FTZs may have increased the age of marriage: women's labor market participation and education. In

[27]In this case, I am not censoring the data for married women since I am measuring the proportion of married women before the age of 18 among all women in the sample.

order to test the schooling mechanism, I add years of education as a control variable in my main specification. I find that the coefficient on FTZs becomes smaller, suggesting that education may be an important channel. I also find that women that increase their education are more likely to be employed in the future (see column 7 in Panel A).

In addition, I checked which cohort of women responded to the labor market demand in the period immediately after the opening of factories. The results demonstrate that women who were older than 25 at the time of the opening increased their labor force participation in the short run.[28] Therefore, it does not seem to be the case that younger cohorts drop out of school in order to participate in the labor market and then get married when they are older. In addition, the fact that there is no effect on marriage for older cohorts (who are the ones that work in the factories at the time of the opening) suggests that labor force participation is not the main mechanism for the delay in the age of marriage.[29]

In panel B I explore health outcomes that may be affected by these changes. I find that FTZs reduce the probability of teenage births by 9 percentage points. This finding is particularly important in Latin America and the Caribbean, which is the only region of the world where the rate of early births has risen over the past 30 years. Moreover, the opening of FTZs increases the chances that a child is alive by 1.3 percentage points. Finally, while I did not find evidence that female factory jobs increase women's power in relevant household decision-making, I did find evidence that women are less likely to justify domestic violence (see Table A25 in the Appendix).

[28] As a robustness check, I estimate all the specifications of this section using *EarlyMarriage* and using the DD specifications and results do not change. By increasing their years of education, women are less likely to marry before the age of 18. Using the DD specification I find larger effects on female labor force participation than when I estimate the DDD. This is expected given that women who were older than 25 were the ones responding to the increase in female factory jobs during the first years after the opening.

[29] I also estimate a similar model to Equation 1 that includes the proportion of women who work as a control variable. The coefficient on female factory jobs does not vary much (see Table 2, columns 3 and 6). This suggests that changes in women's labor force participation are not the main channel driving the change in early marriage.

Table 2.2: Early Marriage and Female Factory Jobs (DDD), 1986-2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A							
	Age of marriage	Age of marriage	Age of marriage	Early marriage	Early marriage	Early marriage	Female labor
FTZ×age6to16	1.214***	0.882***	1.197***	-0.103***	-0.068***	-0.101***	0.035**
	(0.196)	(0.127)	(0.191)	(0.018)	(0.012)	(0.018)	(0.015)
Years of education		0.429***			-0.046***		
		(0.016)			(0.001)		
Female labor			0.598***			-0.064***	
			(0.133)			(0.008)	
Mean of dependent	17.94	17.94	17.94	0.46	0.46	0.46	0.24
N	33,897	33,863	33,839	46,069	46,026	45,987	45,987
R^2	0.123	0.298	0.128	0.056	0.189	0.060	0.107
Panel B							
	Age at first birth	Early birth	Age at first intercourse	Early intercourse	Desired fertility	Out-of-wedlock birth	Child survival
FTZ×age6to16	0.924***	-0.093***	0.725***	-0.046***	-0.111***	0.008	0.013**
	(0.143)	(0.015)	(0.113)	(0.014)	(0.030)	(0.005)	(0.006)
Mean of dependent	19.31	0.24	17.31	0.39	3.2	0.036	0.9
N	31,151	46,069	26,779	46,069	46,069	31,151	29,184
R^2	0.138	0.038	0.110	0.049	0.087	0.017	0.017

Notes: each cell represents a separate regression. Other covariates control for household and province characteristics. The control group consists of women between 16 to 30 years of age. Standard errors are corrected for clustering at the province level. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$

In the Appendix, I check the robustness of the results by estimating the effects for populations that should not be affected by the opening, such as women who were already married women by the time of the opening (Table A26). As another robustness check, I include quadratic trends when estimating equation 2.1, and the results do not change. Moreover, limited dependent variable models yield nearly identical results.[30] I also exclude from the analysis provinces that contain the country's major cities, such as the National District, Santo Domingo and Santiago. I do this to verify that my results are not driven by provinces where many factories are located. Table A27 in the Appendix presents the results and estimates are similar.[31] Finally, it shows that results are robust to using other data sources. Using data from the Inter American Development Bank leads to the same magnitude of estimates (Table A28 and Figures A3-A4).

2.5.2 Possible Concerns

The two main concerns regarding the validity of the identification strategy are the endogenous mobility of women who move after the opening and the possibility of pre-existing trends in education in the places where the FTZs opened first. In this section, I provide evidence that migration is not the main mechanism driving the results and that there was no increasing trend in female education and marriage in the period before the openings.

The main results could be biased due to selective migration into the provinces after the opening of an FTZ. Although most of the FTZs tend to hire local women and most migration in the Dominican Republic occurs at municipality (rather than the province) level, it is still possible that at least some of the results are explained by the endogenous mobility of women. It could be the case that migrants who moved to the free industrial zones differ in ways that would bias results. For example, if movers are more educated, the main parameter could be overestimated. To address this concern, I restrict the sample by eliminating those individuals that moved to the area after the free industrial zones opened. Results are presented in Table A29, column 1 and do not change under this new specification, suggesting that selective migration is not an important concern.

There also may be concern that people moved to provinces where they were expecting factories to open. In order to address this, I did not include those who moved to the area within two years of the opening in my sample. In contrast with the above specification presented in column 1, column 2 presents the results for the sample excluding recent movers. Again the results hold under this new specification.

[30] Results are available upon request.

[31] Results are also unchanged if I estimate the DDD specification.

Rather than restricting the sample, I add a dummy variable for women who had moved. I find that the dummy coefficient is negative and significant at a one percent level. However, the effect of opening FTZs remains consistent with the baseline estimates, suggesting that migration is not a concern. Moreover, since movers are less educated than non-movers in my sample, this could only bias the results downward.[32]

Finally, I construct migration rates for each year from 1986 to 2007 using the year that individuals migrated to the province. A household is considered a mover when the year of arrival is equal to the year of the sample. Most migrants are married at younger ages, less educated, have fewer members in their family, and have worse housing facilities. Thus, if migration is a concern, it leads to underestimation of the results. Nevertheless, I estimate whether the FTZs affected migration. I estimate Equation 2.1 but using $Migration_{p,t}$, the proportion of women that move in year t in province p, as the dependent variable. I find an effect of 0.17 percentage points at the 10 percent level of statistical significance. This means that out of 1,000 women, an FTZ opening caused fewer than two migrants to move. This finding suggests that the economic significance of the effect is close to zero. Moreover, once I include socioeconomic controls, I find that the opening of FTZs has no effect on migration rates. Overall, migration results suggest that even if FTZs affect migration rates, this factor is not the main mechanism affecting education and marriage.[33]

As already discussed above, another central threat to the validity of the estimates is the possibility that FTZs anticipate educational attainment increases rather than causing them. For instance, FTZs may have located in areas that were expected to have a highly educated female workforce. In this section I use two approaches to check that the results are not driven by pre-existing trend in the places where the FTZs opened first. First, I look at whether education and age of marriage appears to be increasing in provinces with FTZs prior to the openings. If openings determine the main outcomes rather than

[32] Another test of the validity of the identification strategy is to estimate the effects for those women that were more than 16 years of age. If the identification strategy is valid, then labor market of opportunities before the age of 16 should have a larger effect on a woman's education than opportunities she has when she is past the usual age of secondary school attendance and age of marriage. Conversely, if educated women with more than 16 years of age move to the provinces with FTZs, then openings at 16 or older should be stronger predictors of a woman's educational attainment. Results of this estimation were presented in Figure 3 and I find smaller and non significant effects for women who were 16 or older at the time of the opening. Moreover, if migration is driving the results I would find greater effects on the outcomes of interest in places where more jobs were available. In order to check for this bias I rerun all my specifications, including the number of industrial parks as a control variable in each province. I find not only does the magnitude and significance of the results not change but the number of industrial parks is not a significant predictor of the outcomes.

[33] This is consistent with the fact that migration in the Dominican Republic occurs across municipalities in the same province rather than across provinces.

vice versa, I should find little evidence of a pre-trend in the outcomes of interest prior to the FTZ opening. Second, I compare women that belong to the same household using age thresholds. Using the three different approaches, I find no evidence of pre-existing trends in education.

My first approach to deal with this concern is to construct falsification tests. First, I pretend the FTZs opened one, two, or three years after the real opening in each province and then use only post-treatment data. Second, I pretend the FTZs opened one, two or three years before the real opening in the same place and then only use pre-treatment data. I rerun Equation 2.1 using these false treatments and find no significant effects on the outcomes of interest.[34]

The estimates presented in previous sections suggest that the effect of FTZs is identified by the discrete jump after the year of opening and its impact on the outcome of interest. In particular, I showed in the previous analysis that results are not sensitive to the inclusion of province time trends and province birth cohort trends. Moreover, in Table A24, I have showed that the year of opening is not correlated with baseline characteristics. However, there still may be a concern that the results are driven by trends in province education and marriage outcomes that are correlated with the opening of FTZs in a way that province linear trends do not capture. This proposition can be evaluated more directly in an event study analysis. Formally, I will estimate the following regression:

$$\begin{aligned} Outcome_{i,h,p,t} =& \alpha + \sum_{i=-4}^{5} \beta_i(\tau_{p,t} = i) + \delta Province_p + \pi Year_t + \theta Trend_p \\ &+ \rho Cohort_i + \mu Province * Cohort_p + \varepsilon_{i,h,p,t} \end{aligned} \tag{2.2}$$

where τ_{pt} denotes the event year, defined so that $\tau = 0$ for the year the FTZ began operations in that province, $\tau = 1$ for one year after the FTZ began operation, and so on. For $\tau \leq -1$, households were untreated by the FTZ (marriages before the program started). The coefficients are measured relative to the omitted coefficient ($\tau = -1$). I also include province, year, and province-specific linear time trends.[35] Figure 2.4 plots the event and year coefficients from estimating Equation 2.2 on age of marriage, years of education and secondary enrollment.

[34]Results are available upon request.

[35]The dummy for $\tau = 5$ is a dummy that takes the value of one for more than five years after the FTZ began operations. $\tau = -4$ is a dummy that takes the value one for more than 4 years before the park began operations.

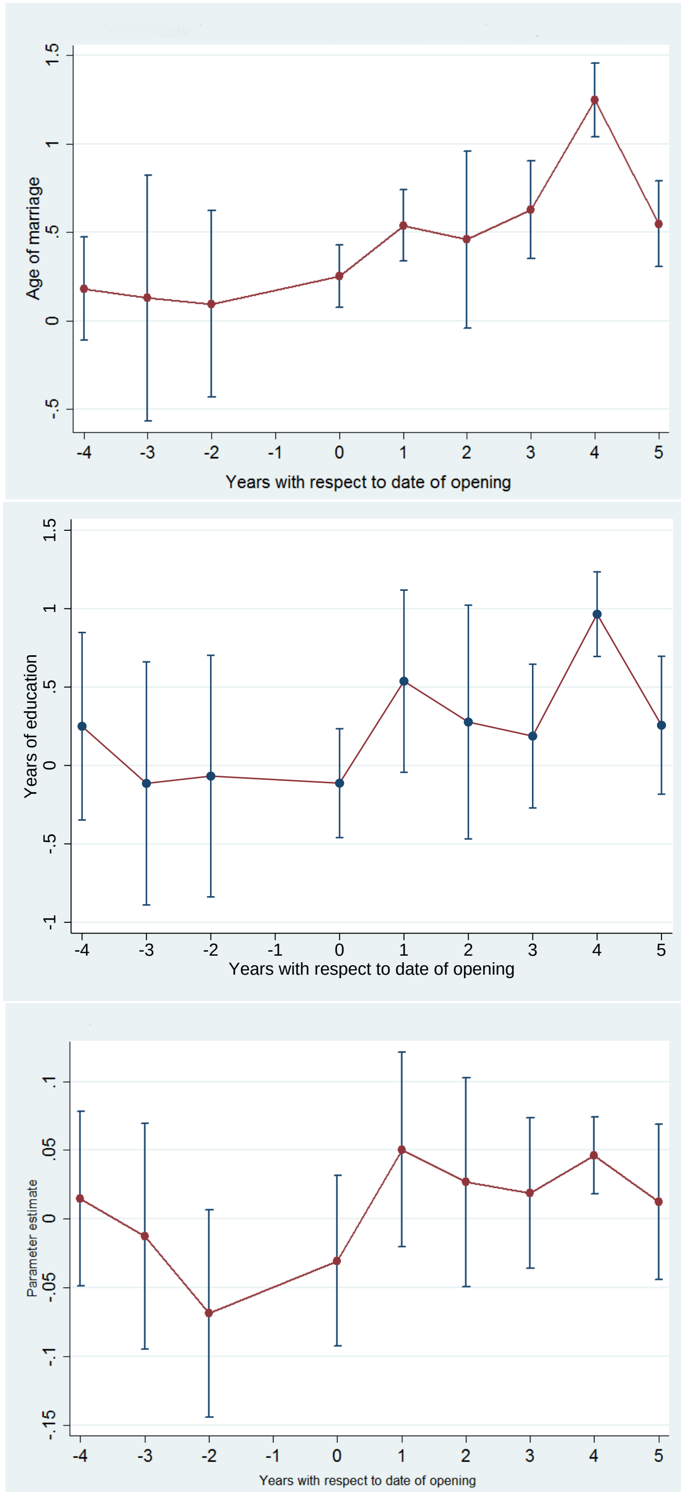


Figure 2.4: Timing of Schooling and Marriage Effects

Notes: These graphs plot the coefficients obtained from a regression of the outcome on the interaction between the treated province dummy and year dummies. The regressions control for province, year and province time trends. The Y-axis shows the estimated coefficients and the X-axis shows the years.

The results support the validity of the identification strategy, showing an absence of a strong pretrend and evidence of a trend break after FTZs opened, increasing the years of education and age of marriage for women.[36] This evidence suggests that potential confounders would have to mimic the timing of the FTZs' expansion extremely closely.[37] In addition, the similar timing of effects on education and marriage provides further evidence that women might be delaying their age of marriage by increasing their years of schooling.

To check the plausibility of these effects, I proceed to use only women that are on the relevant margin. Assuming that most women tend to dropout at the age of 16, I repeat the analysis using only women who were in school but close to finishing. First, I define the year in which each woman in the whole sample was 16 years of age and then I subtract the year in which the FTZ opened. For instance, if the new variable takes the value of 1 it means that a woman was 15 years old when the FTZ opened and therefore had only one year of treatment (since after 16 should not be treated). If the variable takes the value of -1 it means the woman was 17 years of age when the FTZ opened and therefore should not be affected. Figure 2.5 presents the results. The estimates are consistent with the DDD estimation: I found an increase of about 0.3 for those women that were less than 16 years of age at the time of opening.

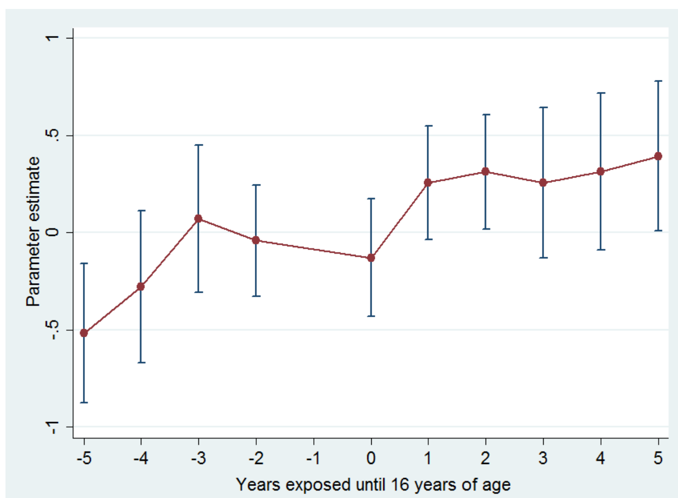


Figure 2.5: Schooling Effects for Women at the Margin

Notes: This graph plots the coefficients obtained from a regression of the outcome (years of education) on dummies of years exposed until 16. I define year exposed until 16 by subtracting from the year of opening the year when each woman was 16 years of age. The regressions control for province, year and province time trends. The Y-axis shows the estimated coefficients and the X-axis shows the years.

Finally, since the DHS surveys cover all women in the household that are between 15 to

[36] This is consistent with Oster and Steinberg (2013) who find effects on school enrollment an year after the introduction ITES centers in India. In a similar way, the rapid response in schooling could be explained by older girls (those who were in high school).

[37] Results hold if I use *EarlyMarriage* as the dependent variable.

49 years of age, I can include household fixed effects. By doing so, I can compare women inside the same household who were less than 16 years of age with those who were more than 16 years old. I find that results do not change under this new specification. By using household fixed effects, the results are consistent with the previous findings (see Table A30). Although estimates are higher due to potential differences between women of the same households (such as differential parenting behavior) and changes in the sample (it only considers households were there is more than one woman), the estimation results using this specification are similar to previous estimates.

Overall, results from different approaches (differences in differences, difference-in-difference-in-differences using age at opening, event study analysis, and household fixed effects) provide strong evidence that the effect of female labor market opportunities created by FTZs on schooling and marriage is causal.

2.6 ARE THESE EFFECTS LONG LASTING?

In this section I study whether the education and marriage effects reverted in the presence of negative female labor demand shocks. The growth competition from Asian countries in 2000 and the end of the preferential commercial agreement with the US led to a decline in the importance of FTZs.[38] Using this as an exogenous negative shock, I find that even when labor market opportunities decrease, educational attainment increases are sustained.

During the 2000s demand for female labor collapsed. The commercial agreements granting preferential market access to the United States ended in 2005. At the same time, China entered the World Trade Organization and competition coming from Asian countries grew.[39] These shocks caused a large decrease in textile manufacturing activities in FTZs between 2000 and 2007. Before 2000, total exports from this sector represented 53% of total production; however, in 2006 they represented only 35%. These shocks had a larger negative effect on FTZs that produced apparel. Textile sector employment was reduced by about 45% (see Figure A5). As a result, female unemployment rose in the 2000s following a decade of decline, suggesting that new sectors were not able to absorb the extra female labor in the FTZs (ILO (2013), Isa and Cruz (2007)). According to a survey on displaced workers in 2008, 70% of women who were displaced from the textile industry due to these shocks were still unemployed.

[38]This commercial agreement granted quotas of preferential market access to the United States. During the 2000s the imports of textile products coming from the Dominican Republic were reduced by 50%.

[39]In addition to these shocks, the appreciation of the exchange rate reduced the competitiveness of the sector.

Since these shocks affected the textile sector the most, I will use this variation across sectors in FTZs to analyze the effect of reducing female labor demand. Therefore, I estimate the following equation:

$$\begin{aligned} Outcome_{i,h,p,t} =& \alpha + \beta_1 FTZ_{p,t} + \beta_2 Shock_t + \beta_3 Textile_p + \beta_4 Shock_p \times Textile_t \\ & + \delta Province_p + \pi Year_t + \theta Trend_p + \gamma X_{h,p,t} + \nu X_{p,t} + \varepsilon_{i,h,p,t} \end{aligned} \tag{2.3}$$

where $Shock_t$ is a dummy variable for after 2000 and $Textile_p$ is the proportion of firms in the textile industry before 2000 in province p. The interaction of both variables controls for the effect of the negative shock. In this way, I will be able to distinguish the effect of the FTZ opening and the effect of the negative shock.[40] Another way is to interact the variable $FTZ_{p,t}$ with a variable that takes the value of zero in province p in the year 2000 and onwards if that province has a large share of firms in the textile industry before the shock. Formally, I estimate the following model:

$$\begin{aligned} Outcome_{i,h,p,t} =& \alpha + \beta_1 FTZ \times (1 - 1_{\{Year \geq 2000 \text{ \& } Textile \geq 0.5\}})_{p,t} + \delta Province_p \\ & + \pi Year_t + \theta Trend_p + \gamma X_{h,p,t} + \nu X_{p,t} + \varepsilon_{i,h,p,t} \end{aligned} \tag{2.4}$$

where $(1 - 1_{\{Year \geq 2000 \text{ \& } Textile \geq 0.5\}})$ takes the value of 0 after the year 2000 if the province has more than 50% of firms in the textile industry in 1996.[41]

If the results persist in the presence of the negative shock I expect β_1 in Equations 2.3 and 2.4 to be similar in magnitude to the effects found in the main specification presented in Equation 2.1.

I find that, even in the presence of the negative shock, women educational attainment increases by 0.3 years. Table 2.3 columns 1, 3 and 4 in Panel A presents the results of estimating Equation 2.3. Conditional on the negative shock, the opening of a FTZ still has a positive effect on women's educational attainment. Moreover, the magnitude is similar to that found previously. Columns 2, 3 and 6 show the estimates of Equation 2.4; and the results still hold. I also estimate a difference-in-difference-in-differences model to check whether younger cohorts are affected by the negative shock. Table 2.3 Panel B presents the results. I find that women who were between 6 and 16 years of age at the time of the shock increase their educational attainment by 0.3 years even after controlling for the negative shock.[42]

[40] For example, if a province has a 60% of the firms in the manufacturing industry, the variable shock is equal to 0 for the years before 2000 and 60% after 2000.

[41] I only have information for the year 1996 on the number of firms in the textile industry per province.

[42] I find similar results using the 2005 shock and using as age of marriage and birth as the dependent variables.

Table 2.3: Are Schooling and Marriage Effects Long Lasting?

	Years of education		Enrollment in secondary		Complete secondary	
FTZ	0.329**		0.036**		0.030**	
	(0.156)		(0.015)		(0.012)	
Shock×textile	-0.075		-0.016		-0.006	
	(0.243)		(0.022)		(0.019)	
$FTZ\times(1-1_{\{Year\geq 2000\ \&\ Textile\geq 0.5\}}$		0.341**		0.028***		0.0214***
		(0.128)		(0.009)		(0.007)
N	55,894	55,894	55,894	55,894	55,894	55,894
R^2	0.124	0.125	0.104	0.104	0.079	0.079
FTZ×age6to16	0.268**		0.029**		0.025***	
	(0.126)		(0.013)		(0.008)	
Shock×textile ×age6to16	0.201		0.030		0.023	
	(0.494)		(0.039)		(0.038)	
$FTZ\times$age6to16$\times(1-1_{\{Year\geq 2000\ \&\ Textile\geq 0.5\}}$		0.274**		0.029**		0.026***
		(0.111)		(0.011)		(0.008)
N	46,026	46,026	46,026	46,026	46,026	46,026
R^2	0.174	0.174	0.142	0.142	0.117	0.117

Notes: $Shock_t$ is a dummy variable for after 2000 and $Textile_p$ is the proportion of firms in the textile industry before 2000 in province p. The interaction between both variables control for the effect of the negative shock. $(1-1_{\{Year\geq 2000\ \&\ Textile\geq 0.5\}})$ takes the value of 0 after the year 2000 if the province has more than 50% of firms in the textile industry in 1996 (before the negative shock). Standard errors are corrected for clustering at the province level. Significant at *** p<0.01, ** p<0.05, * p<0.1

Finally, I estimate the previous specifications for only the years after the shock (2000-2007). I find positive and slightly larger effects. This finding suggests that even after the end of the preferential trade agreement, women continue investing in education. [43]

In order to analyze the robustness of these results, I provide two additional pieces of evidence that suggest that the negative shock did not the educational attainment gains for subsequent cohorts in places with FTZs. First, I find that the increase in educational attainment is similar for cohorts of women affected by the opening of FTZs in the 1990s and cohorts that were affected by the negative shocks in the 2000s. I compare cohorts that were over 16 at the time of the negative shock (which means that they were less than 16 at the time of expansion of FTZs in the 1990s) with those that were less than 16 years of

[43]One possible concern with this methodology is that the negative shock could be affecting a different type of women than the one affected by the positive shock. If the compliers (those women that change their behavior due to the shock) are different under the two different treatments (FTZs' opening and negative shock), the results should be interpreted differently. For instance, one could argue that while the opening of FTZs considerably affects those women with high socioeconomic status (SES), the negative shocks do not affect this same group of women. To address the this concern I analyze if there exists a differential effect between the positive and negative shock based on an index of socioeconomic status. I find the same interacted effect for both shocks. Therefore, a positive shock is affected in the same way by socioeconomic status as a negative one. Results are available upon request.

age at the time of the negative shock. For instance, a woman who was 17 in the year of the negative shock (2000) was affected only by the positive shock in her key ages (since she was less than 16 at the time of openings in the 1990s).

In Figure A6 I estimate the effect of the negative shock by cohort for provinces affected by the openings of FTZs during the 1990s. Since the negative shock affected the entire country, I am not able to include time fixed effects. The omitted category is age 15. I find some evidence of a decrease in educational attainment for those women who were less than 16 years of age at the time of the negative shock. However, these effects are not statistically significant. This suggests that there is no evidence that cohorts affected by the negative shock changed their behavior.

I also find that the negative shocks had no schooling effects in provinces with FTZ compared to provinces that never had an FTZ. I exploit a sample of provinces which was not included in none of my previous analysis: provinces that did not experience an FTZs' opening at any point in time. In Table A29 I present the results of the second test, I compare the years of schooling in provinces with FTZs against provinces that never experienced an opening before and after the negative shock. I find no significant effect on schooling.[44] Moreover, I test if there are differential effects based on the proportion of textile factories in FTZs and I find that even in those places where the proportion of textile factories was high (more than 50%), there was no effect of the negative shock compared to the control group. The key assumption for any difference-in-differences strategy is that the outcome in treatment and control group would follow the same trend in the absence of treatment. In Figure A7 I show that there is no evidence of different trends before 2000. Moreover after 2000 the difference is positive suggesting an increase after the shock. I observe the same pattern with age of marriage. However, this difference is not significant.

Overall, these results suggest that there are long-term effects from a temporary increase in female labor market demand. Second, the persistence of the effects may be driven by mechanisms other than the labor market. If a temporary increase in female labor demand reduces early marriage and increases schooling through labor market gains, the competitive model would predict that that educational attainment and early marriage would revert back to previous levels when there is no longer labor market returns to education. However, this is not what is observed in the data.

Of course, the negative shocks may not be of the same magnitude as the positive shocks created by FTZs' openings. Figure A5 demonstrates that although the negative shocks

[44] I estimate the following model $Outcome_{i,p,t} = \alpha + \mu FTZ_p + \lambda Shock_t + \gamma(FTZ_p * Shock_t) + \epsilon_{i,p,t}$, where FTZ_p is a dummy which is equal to 1 if woman i is in a province with a FTZ, $Shock_t$ is a dummy equal to 1 if the observation is from 2000 (post). The coefficient of interest is γ (the difference-in-differences estimate).

were large, the unemployment rate was not as large as before the opening of FTZs. Most of the recovery of FTZs started in 2009 and it was driven by industries in which male employment is predominant. In addition, probably FTZs did not disappear altogether, and thus still generate some demand for female labor affecting schooling decision. I address this concern by checking whether there are schooling effects for cohorts that were exposed more years to the negative shocks. I find that even for cohorts that were exposed more than 2 years there is still an increase in education.

In the following section I provide potential explanations for the persistence of the results.

2.6.1 Mechanisms Behind the Long-Lasting Effects: Spillovers in the Marriage Market

In order to interpret why educational attainment does not decrease for new cohorts in the absence of economic gains in the labor market, I propose that the persistence of results can be partially explained by marriage market gains. Women may face a lower penalty for delaying marriage if a previous generation of women had been educated.

The idea is that the opening of FTZs created incentives for some women to increase their education and invest in their careers. By doing so FTZs created two indirect effects, a reduction in the penalty for delaying marriage for future generations and access to better matches for these type of women. Intuitively this is due to the fact that women have greater returns to education in the marriage market (and a smaller "waiting" penalty) if past generations had high educational attainment. For example, one can assume that men have two periods to find a match in the marriage market but they are uncertain about each woman's type. Naturally, the probability of finding a more educated woman in the second period is increasing in the proportion of women that invested in schooling. Hence, whenever the proportion of girls with high educational attainment increases, men have a larger incentive to wait until the last period to match if they wish to match with high education women. This reduces the penalty of delaying marriage for women. The importance of women's education in the marriage market can be thought of as a relative concept. As the average level of schooling of women increases, a man's cost of not marrying a highly educated woman outweighs the cost of waiting for the match. Hence, after schooling, women have more men waiting for them.

This simple framework illustrates that FTZs not only increased the perceived labor market returns to education but also may have increased the marriage market returns by reducing the penalty of delaying marriage. Another related empirical prediction is that positive assortive mating on education also increased. I find evidence that after the FTZs opened, high educated women tend to match with highly educated men (see Figures A8 and A9). In particular, wives with only a primary school education affected by FTZs have a lower probability of marrying up than wives with the same level of education who were not affected.[45] While wives with secondary schooling or higher now have a high chance of being matched with a husband of their same education level or higher. In the case of husbands, I observe a similar pattern suggesting an increase in positive assortive matching. Moreover, for each education category, I estimate the probability that women are married to a husband with a given level of education. I find that women with only a primary education are negatively effected in the marriage market. They are about 4 percentage points less likely to be married with someone with a higher education status after the opening of a FTZ.

In order to explore the gains in the marriage market, I test to what extent increases in educational attainment and age of marriage (due to the opening of FTZs) can affect the quality of marriage matches. The framework above predicts that more educated women will have access to better matches in the marriage market. Moreover, by delaying their age of marriage they might be able to better understand their preferences and reduce the chance of divorce (Goldin and Katz, 2002). I identify divorces by examining women that had more than one union at the time of the survey. I find that women affected by FTZs have a lower probability of divorce. For those women who were younger than 16 at the time of the opening, the probability of divorce is reduced by 2.5 percentage points.[46]

Another outcome to explore is the quality of women's match as measured by their husband's education. While the FTZs did not have a direct effect on men's education, it might be affected by increasing female educational attainment. I find that the FTZs increase the husband's years of education by 0.6 years for only those women who increase their schooling. Therefore, women who are increasing their years of education and marrying after the age of 18 due to FTZs are the ones who are seeing a reduction in divorce rates and a husband with greater education.

These two results provide further evidence that FTZs increase the marriage market gains of women by increasing education attainment and delaying marriage. These results are maintained even if we restrict the sample to women that are not working at the time of

[45] I also repeat the analysis just comparing women who were between 13-16 years of age versus women who were 17-21 and I find the same pattern.

[46] Since my framework predicts that by increasing educational attainment, women receive gains in the marriage market, I also use the opening of the FTZs as an instrument for women's years of education. In this case, conditional on women's participation in the labor market and other socioeconomic variables, I find that one year of education reduces the probability of divorced by 3.5 percentage points.

the survey. This suggests that FTZs affect women's behavior through mechanisms apart from labor force participation. I also repeat the analysis using only the observations after the negative shock and find similar results.

I also examine alternative measures of husband quality. These include the probability that the husband resides at home with the wife, the difference in age between husband and wife and the skill level of the husband's job.[47] Table A31 presents the results which are of the expected sign: for those women who were affected by the opening of FTZs, the probability that the husband stays at home increases, the difference in age decreases and the probability that the husband has a skilled job increases.

Another direct explanation for the persistence of effects could be a change in beliefs. The idea is that if the opening of free trade zones affects women through a change in beliefs, then in the face of a negative shock in the following period, the high educational attainment and late marriage phenomenon should not be reverted. That is, even if labor market opportunities for women decrease, it should not affect women's education and marriage age since a new belief or social norm has been formed within the community (e.g. better education signals other abilities such as childcare).[48]

2.7 Conclusions

Economic development has lad to more female empowerment by expanding job opportunities for women (Duflo, 2012). Historically, industrialization and the rise of factory work suitable for women increased demand for female labor. More recently, the rise of service sector jobs due to outsourcing has done the same. Little is known, however, about the long-term effects of female labor demand on women's educational attainment and marriage outcomes. This paper exploits the sudden and massive growth of female jobs in FTZs in the Dominican Republic in the 1990s, and subsequent decline in the 2000s, to provide the first evidence that temporary improvements in female labor demand can move societies to a "good equilibrium" that persists even after job opportunities taper off.

While most of the previous literature has studied the effects of female labor markets during periods of expansion, I provide evidence that even temporary improvements in female labor demand can have long-term effects on girls' schooling and early marriage.

[47] I define skilled work as those that include professional, managerial, clerical and manual skilled positions.

[48] Another possibility is that women continue increasing their schooling because their mothers were previously affected by the openings. I check if this is the case and do not find that that this is driving the results.I do this by interacting the main treatment variable with an indicator whether the mother was affected by the opening.

Although I cannot rule out if the persistence of effects might be driven by a change in social norm caused by a possible permanent jump on expectations due to an FTZs' opening, I provide evidence that marriage market gains might be an important factor to explain women's higher levels of education during contractions.

While this paper has shown suggestive evidence of important interactions between labor markets and marriage markets, more work remains to identify and characterize the spillovers involved. Given that women's expectations about labor market returns and expectations about marriage market returns are both important, additional research is needed on how these joint expectations form and whether information is enough to change behavior.

Chapter 3

Exporting Criminal Capital: The Effect of U.S. Deportations on Gang Expansion and Human Capital in Central America

Joint with Josefa Aguirre

3.1 Introduction

Between 1998 and 2014, U.S. immigration authorities logged almost 300,000 deportations of immigrants with criminal records to Central America, including an untold number of gang members. Although these policies were primarily aimed at reducing criminal activity by breaking up Los Angeles street gangs, it may have have backfired and helped spread gangs across Central America and back into other parts of the United States. A good example of this is the Mara Salvatrucha, or MS-13, which is a violent transnational criminal organization that started in Los Angeles and is now active across the United States and many Central American countries. Recent estimates place MS-13 membership in the United States between 8,000 and 10,000 individuals across 33 states.[1]

This paper provides new evidence on how an increase in criminal capital due to deportations from the US affects human capital investments in El Salvador. Although these deported criminals may have generated a direct effect on crime when released in El Salvador, we are particularly interested in the potential spillover effects. In particular, we ask whether peer effects generate changes in education investments in the regions where deported criminals are released. Furthermore, we argue that there is a self-reinforcing circle through which gang presence lowers education investments and economic activity, which in turn helps to increase gang adherence.

To analyze these issues, we employ a difference-in-difference approach using geographical variation in gangs location and time variation in deportation policies. In 1996, the U.S. Illegal Immigration Responsibility Act drastically increased the number of criminal deportations. In particular, the leaders of large gangs in Los Angeles were sent back to their countries. By comparing outcomes across municipalities with and without gang presence in El Salvador, we are able to measure how the increase in the number of deportees affected homicide rates and educational outcomes.

Our identification strategy relies on the idea that gang deportees from the U.S. tended to locate themselves in places that had some gang presence at baseline. Although we do not have a measure of gang presence before deportation policies begun in the 1990s, we do have a measure of gang criminal activity for 2002 which is just before deportations begun to increase exponentially. Because this variable could potentially be endogenous, we also examine estimates from an instrumental variable approach were we instrument gang presence in 2002 with the municipalities of birth of the major gang leaders. Looking at previous trends we are also able to test our basic identifying assumption, that is, that places with and without gang presence in 2003 would have followed similar trends if the

[1]See http://www.migrationpolicy.org/article/national-policies-and-rise-transnational-gangs.

number of criminals deported had not increased.

Our results show that deportation policies led to an increase in homicide rates and a decrease in educational outcomes in the places where gang members are located. In the short-run we find that children in primary school are less likely to attend school after the arrival of gangs. This is consistent with the fact that the most common age of initiation for gangs is between 10 and 12, which is also when children are at particular risk of dropping out of school when gang presence increases. Interestingly, we observe that in the long-run, educational attainment decreases more heavily for younger cohorts who were exposed to gang presence during childhood. We also show that these effects are not driven by selective migration or an increase in homicides rate, suggesting that gang's presence is driving the results.

These results show how criminal deportations deteriorate economic and educational outcomes in the countries of destination, and how this may ultimately help to extend criminal organizations. More broadly, the Salvadoran case provides a unique opportunity to understand how criminal capital can be exported from one place to another. In a previous paper, Sviatschi (2017) shows that exposing children to illegal labor markets makes them more likely to be criminals, irrespective of where they live as adults, indicating that individuals take their criminal capital from one place to another. This paper takes a step further and exploits the increase in the number of US criminal deportees to look at the effect of a large increase in criminal capital in a particular location. Hence, providing additional evidence for the result in Sviatschi (2017): increases in illegal activities affect the career path (or at least educational investment) of children in affected areas. Relatedly, this paper adds to the literature on migration showing how specific human capital acquired in places of birth can be exported to other locations generating spillover effects.

This paper is also related to recent working papers studying deportations (Jakubowski, 2010; Blake, 2015; Anders et al, 2016; Kalsi, 2017). While most of the literature has relied on cross-country variation, our data allow us to estimate effects on short and long-term outcomes at a more dissagregated level using the whole universe of individuals and municipalities in El Salvador. The closest related previous paper is by Anders et al (2016) who study the effects of criminal and non-criminal deportations from the US to Mexico in municipalities near repatriation points. Unlike this paper, they find no effects on homicides rates. In contrast to the individuals deported from the US to Mexico, many of the individuals deported to El Salvador were known gang leaders in Los Angeles. This is important, as it implies that these individuals potentially bring criminal capital back to El Salvador. In work conducted in parallel, Kalsi (2017) also examines the effects of criminal deportations in El Salvador. We complement this paper by using data that allow us to know the exact location of gangs and estimate effects on short and long-term outcomes at a more dissagregated level using the whole universe of individuals and municipalities in El Salvador.

3.2 Historical Background [3]

Los Angeles was the destination of thousands of people from El Salvador fleeing civil war in the 1980s. During this period, Salvadoran immigrants were living in poor and overcrowded neighborhoods and often faced discrimination. In a typical immigrant family both parents worked, leaving children without major supervision (Savenije, 2009).

During this period, many Central American youth, who were often on their own in the streets of Los Angeles, joined the Eighteenth Street Gang or Barrio 18, a gang formed mainly by Mexican youth that become one of the biggest gangs in Los Angeles (DeCesare, 1998; Dunn, 2007; Lopez and Connell, 1996). At the same time, a group of Salvadoran youth came together in what would later be called the MS-13 or Mara Salvatrucha (Hayden, 2005). The MS-13 partly originated as a self-defense group in response to discrimination from people of other countries and threats from other Mexican gangs (Johnson, 1989). Although relatively peaceful at first, this changed in the mid-1980s when some of the MS-13 members spent time in prison. Prison served as a place where Ms-13 members could learn illegal practices, trading information on the best ways to run criminal operations. By 1985, the MS-13 had evolved, and started taking up small-scale drug trafficking or extorting money from corner drug dealers in their areas (Ramsey, 2012). They also developed a fierce rivalry with the Eighteenth Street Gang which has persisted to this day.

In the late 1980s, the U.S. resorted to the deportation of gang members to try to reduce violence and crime in Los Angeles. The Immigration and Naturalization Service (INS) begun to actively look for and deport gang members (Davis, 2006). After the civil war peace agreements of El Salvador in 1992, the INS increased these efforts through what was called the 'Violent Gang Task Force', which focused on looking for immigrants with criminal records and deporting them to their countries of origin (DeCesare, 1998).

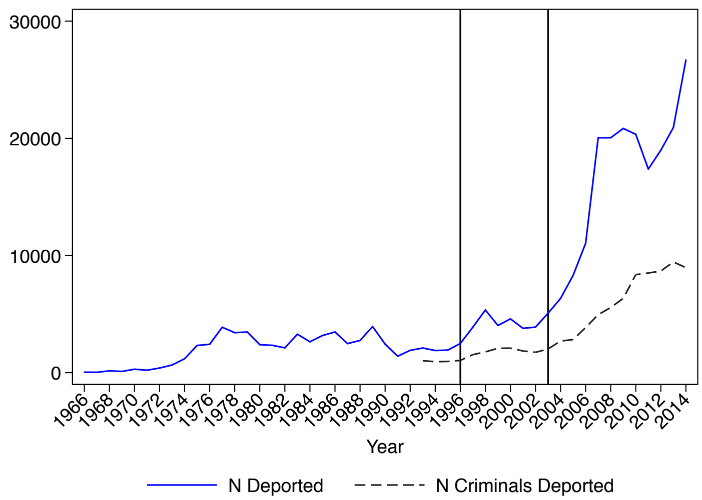
Deportations increased further in 1996 and 2003 due to the Illegal Immigration Reform and Immigration Responsibility Act (IIRIRA) and the Homeland Security Act. The IIRIRA of 1996 drastically increased immigration enforcement, by creating expedited removal procedures, adding new grounds for deportation, and increasing the number of border patrol agents. The Homeland Security Act approved in 2002 and implemented in

[3] For a more extensive review of the history of gangs from El Salvador see Savenije, 2009

2003 created the Department of Homeland Security with the mandate of preventing and reducing the vulnerability of the United States to terrorist attacks. In practice, this had a profound impact on the number of forced removals by U.S. immigration authorities, which increased sharply after that year.

Figure 3.1 shows the number of individuals being deported to El Salvador from 1966 to 2014. As of 1993 there is information on whether the deported individuals were criminal or not. As shown in the figure, there was a substantial increase in the number of deported individuals after 1996 and after 2003.

Figure 3.1: Deportations to El Salvador by year



Source: Department of Homeland Security

Changes in US deportation policies led to an increase in criminal capital in El Salvador. Deported gang members not only increased the number of criminals living in El Salvador but may have recruited others and spread criminal knowledge acquired in US prisons. Studies have shown that the majority of gang members in El Salvador have never been outside the country.[4] The deportations may have indirectly affected these gangs, increasing crime.

In the 1990s, when deportation begun to increase, gangs members did not receive much attention from police authorities in El Salvador. At that time, youth gangs seemed to be primarily be involved with feuds with rival groups rather than confrontations with the police. This, however, changed in 2003 with the enactment of the *Mano Dura* Act. *Mano Dura* was a wide government effort to tackle gangs and to gain public support for

[4]Studies include Cruz and Peña (1998) who indicate that only one in every ten gang members joined the gang organization in the U.S. Similarly, Giralt and Concha-Eastman (2001) indicate that only 12% of gang members said they had been to the US.

the approaching elections of 2004 (Aguilar, 2004). A law was enacted in July 2003 aimed at detaining and prosecuting suspected gang members based on the newly classified felony of "illicit association" and gang membership (Thale and Falkenburger, 2006). By mid-2004, this state initiative evolved to a superlative form called *Super Mano Dura*. The *Super Mano Dura* gave complete authority to the police and military personnel to carry out arrests based on arbitrary decisions and thin evidence. Police could use the presence of tattoos, hand signals, some dress codes, and physical appearance as evidence of gang membership (Hume, 2007).

The *Super Mano Dura* led to the capture and mass incarceration of gang members, thus saturating and overpopulating the prison system. This in turn provided gangs with an opportunity to control several prisons across the country, strengthening gangs and promoting their organization. The *Mano Dura* and *Super Mano Dura* are seen by many authors as having provided gangs with an opportunity to organize, unite their cliques, and develop a regional and national leadership (Cruz, 2014).

MS-13 and Barrio 18 are currently the two major youth gangs in El Salvador and Central America. Between 2002 and 2006, both gangs comprised more than 87% of gang membership in El Salvador (Aguilar and Miranda, 2006; USAID, 2006). The gangs are known not only because of their control of Salvadoran neighborhoods and most of the prisons, but also because they have evolved to become powerful criminal groups with an extortion networks across the region. Salvadoran authorities estimate that 60,000 to 70,000 people belong to gangs and that half a million more—relatives, business partners, corrupt politicians and police—are financially dependent on them (Maslin, 2016). Salvadorans pay $756m a year, about 3% of GDP, to gangs, according to a study by the country's central bank and the UN Development Program (Penate et al., 2016). El Salvador's shocking high murder rate is largely due to wars between them for control of territory. Penate et al. (2016) estimate that the total cost of violence, including the amount households spend on extra security and the lost income of people deterred from working, is nearly 16% of GDP, the highest level in Central America.

Figure A10 presents a timeline of the main events taking place between 1979 and 2006.

3.3 Data

This study brings together data from multiple sources in order to examine the effect of deportations on El Salvador. Data on deportations comes from the Immigration Statistics of the United States Department of Homeland Security (DHS). This data set includes annual information on the number of individuals deported from 1966 to 2013, including country to which they are deported. Beginning in 1993, the data on deportations can be divided by criminal and non-criminal status. Criminal status includes those cases in which the DHS has evidence of a conviction. Between 1993 and 2013, approximately 40% of the deportations to El Salvador are criminal.

Data on educational outcomes comes from the 2007 census. Data on the census allows us to look at the total years of education completed by people who are born before 1989, because these are the people who have turned 18 by 2007 and have probably already completed their education. We also use the 1992 census to provide information on baseline characteristics across municipalities with gang presence.

We complement data from the Census with data from Household Surveys (Encuesta de Hogares de Propósitos Múltiples) that have been conducted annually in El Salvador since 1995. Household survey include information regarding demographic variables, educational enrollment and attainment, health, labor force participation, income and consumption of Salvadoran individuals and their households. Each survey consists of a stratified sample of over 20,000 households, for a total sample size of over 85,000 individuals. Figure 3.2 plots the years of schooling in gang and non-gang areas. It shows that individuals starting school after mid 1990s (when gangs expanded) have less years of schooling completed in gang areas. While individuals starting school in 1990 had more years of schooling in gang related areas than in non-gang areas by 1996 the gap is reduced by half. We find the same pattern when we divide the results by gender and when using data from the census (see Figure A11 in the Appendix).

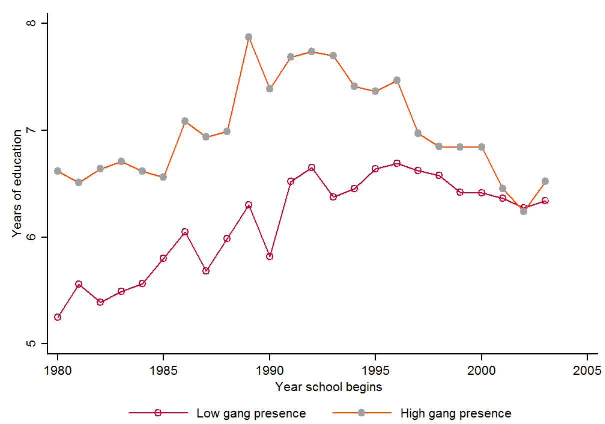


Figure 3.2: Average years of education in gang and non gang municipalities in El Salvador using household surveys in 2012 and 2013

Data on municipal-level homicides for the years 1995, 1999-2010 was provided by the National Civil Police of El Salvador. We complement this information with homicides committed by gangs from 2002 to 2010. Using this information we define whether a municipality has gang presence as the municipalities that experienced at least one homicide committed by gangs in 2002 data. Table 3.1 shows that in 1992, gang areas have higher education than non-gang areas.

Table 3.1: Baseline 1992 gangs and non gang municipalities

Variable	Non-gang	Gang	Diff.	$\|t\|$	$\Pr(\|T\| > \|t\|)$
Complete family	0.543	0.549	0.006	0.92	0.3577
Members living abroad	0.132	0.117	-0.016	1.60	0.1105
Males living abroad	0.064	0.066	0.001	0.32	0.7491
Participated in the armed force	0.009	0.008	-0.000	0.12	0.9051
Years of education	4.849	5.991	1.142	7.94	0.0000***
School attendance 1995	0.654	0.682	0.028	3.07	0.0021***
Homicides rates in 1995	17.014	23.551	6.537	1.11	0.2699

Data on gangs' leaders was collected from a special investigation done by one of the main newspapers in El Salvador, El Faro, which provided the names of the main gang leaders. Most of these gangs leaders grew up in the US but were born in El Salvador. To obtain information on their place of birth, we use data from criminal sentences from the Ministry of Justice in El Salvador, the US Department of Treasury, and newspapers investigations.

3.4 Empirical Strategy

In order to estimate the causal effect of criminal deportations on crime and education outcomes we would ideally use data on where the deportees arrived. Unfortunately, this information is unavailable. Therefore, to measure the effects of criminal deportations from the US, we combine a difference-in-difference strategy with an instrumental variables approach. First, we exploit geographic variation in gangs location, defined by whether the police reported violent crime associated with gangs in 2002, just before the second period of expansion of criminal deportations from the US. Second, we exploit arguably exogenous time variation in criminal deportations induced by 1996 and 2003 laws in US.

The treatment variable is the total number of criminal deportees to El Salvador interacted with a dummy variable that equals one if a given municipality has gang presence. Since gangs presence is measured after the first criminal shock which brought the gang leaders, we use as instrumental variable that consists of an indicator for whether the main gang leader was born in that municipality. Equation 3.1 presents the baseline specification:

$$Y_{m,t} = \beta \underbrace{\left(Gang\ Presence_m \times Criminal\ Deportations_{t-1}\right)}_{CrimeShock_{m,t}} + \alpha_m + \phi_t + \gamma X_{m,t} + \sigma_d t + \epsilon_{m,t} \quad (3.1)$$

where $Gang\ Presence_m$ is a measure of gang activity for municipality m, which is defined by whether there was an homicide committed by gangs members in municipality m in 2002 before the second shock. $Criminal\ Deportations_t$ is the instrumented number of criminal deportations from the US in year $t-1$. $Gang\ Presence_m$ is instrumented by a dummy indicating whether a main gang leader was born in that municipality. $Y_{m,t}$ is the homicide rate per 100,000 population in municipality m in year t. The α_m are municipality fixed effect, ϕ_t year fixed effects, and $\sigma_d t$ department-by-year fixed effects.[5] By including these fixed effects, we control for invariant differences between gang and non-gang municipalities, and for changes in aggregate time trends across years. By controlling for department-by-year fixed effects, the identification assumption is that affected municipalities would otherwise have changed similarly, on average, to control municipalities within their same department. This specification controls for any characteristic that may vary at the department and year level. This is especially relevant since most political decision are made at the department level. In particular, it rules out the concern that crime and schooling results are driven by changes that vary by department and year such as an increase in police corruption or a decrease in department resources. $X_{d,t}$ controls for time trends in baseline characteristics in 1992 such as poverty, crime and education. To account for serial correlation of criminal deportations, we cluster the standard errors at the municipality level.

In order to examine the long-term effects of criminal deportations on schooling, we estimate the effect of being exposed to criminals during childhood at relevant schooling ages. Identification comes from variation in the years of exposure to criminal deportees at different ages and from gang presence across municipalities of birth. Equation 3.2 presents the specification:

$$Y_{m,c} = \beta \underbrace{\left(Age\ 1996_c \times Gangs\ Presence_m\right)}_{CrimeShockAgex_{m,c}} + \alpha_m + \delta_c + \sigma_m c + \epsilon_{m,c} \quad (3.2)$$

where m indexes the municipality of birth and c the birth year. $Gangs\ Presence_m$ is a dummy indicating whether gangs arrived in the municipality of birth. $Age1996_c$ is the

[5] El Salvador is divided into 14 departments.

age in 1996. The term δ_c indicates year of birth fixed effects and controls for specific cohort effects. The term α_m indicates municipality of birth fixed effects and control for time-invariant characteristics of the municipality that may be correlated with both childhood exposure and schooling. Given that individuals in non-gang areas have less years of schooling at baseline, we also include a vector of municipality baseline characteristics interacted with year (including years of schooling in 1992 and crime rates in 1995). These interactions help control for potential differential trends across types of municipalities.

The parameter of interest is β, the effect of experiencing criminal deportations during childhood, which is identified from variation in criminal deportees across municipalities and birth cohorts. Therefore, the control group is composed of those who were born in the same municipality but in a different year, and those who were born in a different municipality but belong to the same cohort.

3.4.0.1 Addressing Potential Concerns

In this subsection, we show that municipalities that had gang presence are the ones expanding their gang activities in the 2000s. In addition, we discuss the exclusion, relevance, and common trends assumptions. Finally, we present a series of robustness checks that address the potential endogeneity of gang presence and differential trends across municipalities.

In the above specification, we assume that only municipalities that had gang activities in 2002 responded to the criminal deportation shock. One concern is that other municipalities could have expanded gangs in response to criminal deportations during the 2000s. To address this concern, we estimate the increase in homicides by gangs from 2002 to 2010 based on an whether there was gang presence in 2002. Results show a strong correlation between homicides committed by gang members in the 2000s and 2002 gang status. Homicides committed by gangs more than duplicated in areas that had gang presence (see Table 3.2).

Table 3.2: Gangs presence in 2002 and violent crimes committed by gangs in 2003-2010

	MS13	M18	Other
Gangs presence in 2002 (=1)	2.132***	2.006***	0.126
	(0.384)	(0.491)	(0.126)
Observations	2,096	2,096	2,096
R-squared	0.15	0.24	0.14

Notes: Standard errors clustered at the municipality level. *** p<0.01, ** p<0.05, * p<0.1.

Given that we use the place of birth of gang leaders as an instrument for gang activity in 2002, we also provide a formal test for the *relevance assumption* in Table A1. The Kleibergen and Paap F statistic is large, indicating that the weak instrument problem is not a concern.

The second assumption that must be satisfied for the validity of the identification strategy is the *exclusion restriction*. This could be violated if the local government in El Salvador increases their enforcement or resources in gang areas when the US increased their criminal deportations. To address this concern, we estimate the effects on crimes that are not related to gang activities. We also limit the sample to the period where enforcement has not increased.

The third main identifying assumption of the baseline specification is that there would be *common trends* across municipalities with gangs in the absence of deportations. This assumption could be violated if, for instance, violent crime was increasing in areas where gangs arrived before the deportation shock. We address this concern by visually inspecting pre-trends and by including municipality specific linear trends. We also include a vector of municipalities baseline characteristics interacted with year. These interactions help control for potential differential trends across types of municipalities.

3.5 Results

We present two sets of findings related to short and long-run outcomes. First, the introduction of criminals significantly increased violent crime and reduce school attendance for children at primary school, in particular those between 10 and 12 years old. As a consequence, individuals that were exposed during childhood have less years of schooling and less likely to complete primary education. All of these results are robust to the inclusion of baseline covariates interacted with year fixed effects, department-by-year fixed effects, municipality specific time trends, household fixed effects, and migration patterns.

3.5.1 Gang's Expansion and Violent Crime

Figure 3.3 shows the homicides rates in gang and non-gang municipalities across time. Two observations are relevant. We can see that in periods during which the criminal deportations from the US increase, homicides rate increase. Second, while in 1995 homicides rates are at similar levels in areas where the gangs arrived, after the 2000s, gangs areas experience an increase in homicides rates.

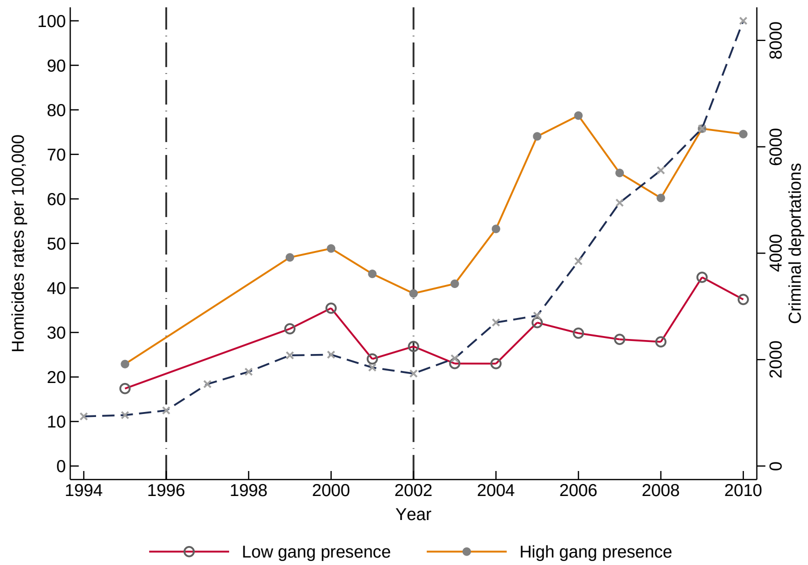


Figure 3.3: Homicides rates in gang and non gang municipalities in El Salvador vs criminal deportations from the US

Next, we turn to estimating the causal effect of criminal deportations on homicides rates. Table A1 presents the results. Column (1) presents the results from a regression that includes all observations from 1965, 1995 and 1999-2010. To gauge the magnitude of the estimated coefficients, consider an increase in 1,000 criminal deportees. The estimates suggest that homicides rates increase by 4 individuals per 100,000 (this is equivalent to a 20% increase relative to the baseline in 1995). In Column (2) adds department-by-year fixed effects. The point estimate is statistically significant, but the magnitude is smaller.

To address the potential endogeneity of gang presence in 2002, Column (3) uses as instrument whether a gang leader was born in the municipality. Results are similar in magnitude and significance. Given that gangs are an urban phenomenon, Column (4) focuses in urban municipalities and results do not change.

Table 3.3: Criminal deportations from the US and homicides rates in El Salvador

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Gangs_m \times CrimDep_t$	0.005***	0.003**	0.006**	0.004***	0.008***	0.006**	0.009***	0.011***
	(0.001)	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
First Stage, Dep. Variable: $Gangs_m \times CrimDep_t$								
$Leader\ born_m \times CrimDep_t$			0.715***					0.83***
			(0.131)					0.104
Kleiberg-Paap F-stat			32.16					67.135
Observations	3,668	3,668	3,668	1,456	2,882	2,882	1,145	1,145
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Dep*Year FE	NO	YES	YES	YES	YES	YES	YES	YES
Years	1965, 1995	1965, 1995	1965, 1995	1965, 1995	1965, 1995	1965, 1995	1965, 1995	1965, 1995
	1999-2011	1999-2011	1999-2011	1999-2011	1999-2006	1999-2006	1999-2006	1999-2006
Municipality Time Trends						YES	YES	YES
Urban Municipalities				YES			YES	YES
IV			YES					YES

Notes: $Gangs_m \times CrimDep_t$ is the interaction of a dummy indicating gangs presence at the municipality and the number of criminal deportations in year t-1. The baseline specification is presented in Equation 3.1. Column (1) presents the results for the whole period and includes controls for municipality and year fixed effects. Column (2) adds department*year fixed effects. Column (3) presents the results using as an instrument of gang presence whether a leader of the gang was born in that municipality. Column (4) restricts the analysis to urban municipalities. Column (5)-(8) restricts the analysis to the years before the Super Mano dura. Standard errors are clustered at the municipality level. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

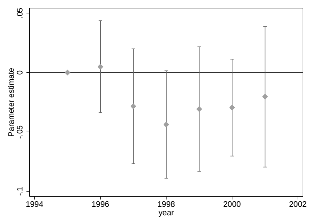
Finally, another potential concern is that criminal deportations from the US are correlated with enforcement efforts in El Salvador. If this is the case this would violate the exclusion restriction. Thus, Columns (5)-(7) restricts the analysis to the period before *Super Mano Dura*.[6]

While we do not have information on other crimes at the municipality level, we use department level data to shed light on whether gangs members who were deported brought criminal knowledge from the US by analyzing the effects by type of crime. Gangs members in the US developed specific criminal capital in prisons in the US for small drug-trafficking and extortion. Consistent with this, at least in the short run we find no effects on thefts, minor injuries, sexual violence and kidnapping, but we do find effects on violent crime and extortion which are the main crimes associated with these gangs in the US in the 1980s.

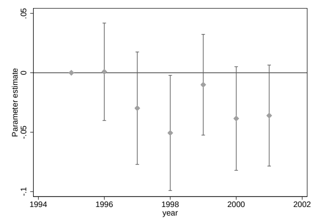
3.5.2 The Effects of Gang's Recruitment on Children

Short-run effects on school attendance and child labor: we start by estimating the effects on the probability that children are attending school at the time of the survey. Figure 3.4 presents the results by ages in an event study. We divide children by ages covering the four different cycles of primary and secondary education. After the arrival of gang leaders, school attendance declines. In particular, children aged 10 to 12 year old reduce school attendance by 5 percentage points (which is equivalent to 10 percent decline from the baseline). These are the children who are in the second cycle of basic education. We find no differential effects by gender.

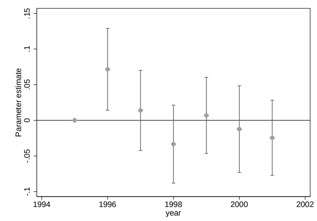
[6] Results are also robust to restricting the analysis to the period before *Mano Dura*.



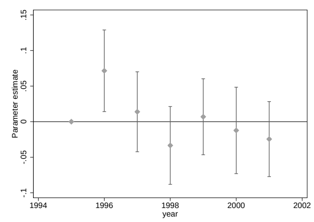
(a) Age 7-9



(b) Age 10-12



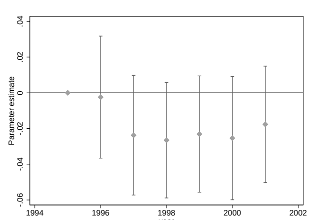
(c) Age 13-15



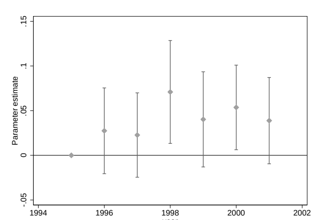
(d) Age 16-18

Figure 3.4: School attendance effects 1995-2001

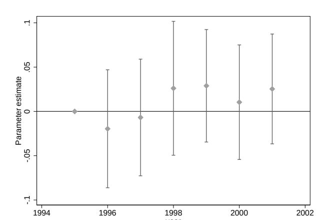
Next, we study whether individuals that are not attending school are working. We have information for individuals older than 10. Figure 3.5 presents the results. We find that there is an increase on the labor supply of adolescents, which is mainly driven by boys who are combining school and work. We find no effects for 10-12 year olds who are the ones not attending school after the shock. This is consistent with the fact that the main reason for which children drop out of school in El Salvador is not for economic reasons but mainly because they moved and gang's delinquency (MINED, 2014).



(a) Age 10-12



(b) Age 13-15



(c) Age 16-18

Figure 3.5: Child labor participation effects 1995-2001

Long-run effects on total years of schooling: Table 3.4 presents the results of being exposed to deportees criminals at different ages of childhood. The dependent variable is the years of schooling of individuals between 18 and 45 per cohort-municipality of birth. Individuals that were during childhood to the gang leaders have less schooling relative to those who were older than 16 at the time of the first shock. Columns (2) and (3) show the results dividing the sample between individuals that have lived in the same neighborhood all their life and individuals that were born in a different neighborhood. Results are also robust to using as instrumental variable the place of birth of gang leaders and using other age bins and including only 18 to 30 years old at the time of the census. Overall, results show that after the arrival of the main gang leaders children are attending less school and also being affected on their total years of education.

Table 3.4: Criminal deportations from the US and years of schooling in El Salvador

	(1)	(2)	(3)
$CrimeShockAge7x8_{m,c}$	-0.401***	-0.473***	-0.508***
	(0.149)	(0.145)	(0.162)
$CrimeShockAge9x10_{m,c}$	-0.174	-0.220*	-0.411***
	(0.122)	(0.120)	(0.141)
$CrimeShockAge11x12_{m,c}$	-0.178**	-0.159*	-0.409***
	(0.087)	(0.085)	(0.105)
$CrimeShockAge13x14_{m,c}$	-0.056	-0.085	-0.281***
	(0.074)	(0.067)	(0.101)
$CrimeShockAge15x16_{m,c}$	0.081	0.022	-0.088
	(0.067)	(0.065)	(0.077)
Observations	1,502,481	1,029,375	470,823
Municipality FE	YES	YES	YES
Cohort FE	YES	YES	YES
Dep*Year FE	YES	YES	YES
Non-migrants		YES	NO

Cluster standard errors at the municipality level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Potential mechanisms behind the schooling effects: we consider three potential mechanisms for which the arrival of gangs may have affected schooling: increase of homicides, changes in the supply of education, and gang's presence.

To analyze whether these effects are driven by violent crime or gang's presence, we exploit a recent truce in 2012 where gangs leaders committed to reduce homicides rates in exchange of jobs and movement of gangs leaders to better prison facilities. The truce lasted two years and by 2015 was completely broken. While we do find evidence that violent crime declined in gang areas by 50 percent during the truce, we find no evidence that schooling outcomes improved during that period.

These results suggest that effects may be driven by other factors associated with gangs and not only violent crime. Even though homicides declined during the truce, extortion practices continued and even increased during the truce. Furthermore, if effects were mainly driven by the direct effect of violence or changes in the supply of education, we would also see effects for individuals aged 13 to 18 and we do not find any effects. Most of the effects are concentrated among children that are in primary school which are the ones particularly targeted by gangs. Finally, the fact that the labor force participation of children in key ages was not affected in the short-run suggests that this may not be the main channel.

One of the main mechanisms can be gang recruitment for boys who are used to help in extortion practices and delinquency in the streets. According to a recent report from the Ministry of Education (MINED), the percentage of dropouts due to delinquency increased by 120% in the last years. This has to do with the lack of security given threats from gangs and the perils of crossing gang boundaries. In addition, gangs often recruit children at schools, making schools a dangerous zone. MINED estimates that about 65 percent of schools are affected by the presence of gangs, while almost 30 percent sees it's internal security threatened by gangs. A school located in gang territory is generally considered property of the gang. Gangs threaten and extort directors, teachers and students; prevent students from crossing the frontiers of gangs and to attend school.

3.6 CONCLUSION

This paper takes a first step to understand how criminal deportations may have affected gangs development and human capital in El Salvador. Although it has been suggested by many that U.S. deportation policies contributed to the development of gangs in el Salvador, this paper is the first to provide causal evidence on this matter.

Initial results from this paper show that the increase in U.S. criminal deportations led to an increase in homicide rates in places with gang presence at baseline. Moreover, results also show that, in such places, an increase in criminal deportations decreased educational outcomes for young cohorts who were presumably exposed to higher gang presence during their adolescence years. In the future we intend to extend these results by showing how deportation policies impacted other economic outcomes such as child mortality and health investments and also by trying to disentangle some of the mechanisms driving the results. In particular, we are interested in understanding whether increased gang presence decreased educational outcomes because it increased violence, decreased economic outcomes, or increased gang recruitment.

Results from this paper are highly relevant from a public policy perspective. El Salvador is nowadays called the murder capital of the world. Understanding what factors contributed to this outcome is, therefore, highly relevant for policymakers. At the same time, the MS-13 has been designated by the U.S. as a global criminal organization on a par with the Zetas of Mexico, or the Yakuza of Japan. It is, therefore, crucial that we understand what factors may have contributed to the expansion of this criminal organization and to determine what policies could potential help undermine gang activity.

Chapter 4

Inter-Generational Impacts of Improving Access to Justice for Women: Evidence from Peru

Joint with Guadalupe Kavanaugh and Iva Trako[1]

[1]We gratefully acknowledge all the helpful comments and suggestions from Karen Macours, Martin Ravallion, Dominique van de Walle, Denis Cogneau, Sylvie Lambert and all the participants of the Development Seminar at Paris School of Economics. We are responsible for all remaining errors.

4.1 INTRODUCTION

An accessible and fair justice system is thought to be important for economic development, so much so that it was recently added as a United Nations Sustainable Development Goal.[2] Access to justice may be particularly important for vulnerable groups in developing countries, since these groups are often unable to legally challenge violence and discrimination. In particular, women are often unable to seek justice for domestic violence or receive equitable treatment during a divorce (Duflo, 2012; Revilla, 1999). While research in this area has mainly focused on addressing these issues through economically empowering women (e.g. Bobonis et al., 2013; Angelucci, 2008; Aizer, 2010), there has been very little evidence on the impact of improving access to justice for women in developing countries. Justice for women is also important for understanding educational choices and the persistent gender gap across generations. In addition, understanding the mechanisms through which better access to justice for women can improve outcomes has implications for household bargaining more generally.

Domestic violence or intimate partner violence (IPV) remains a worldwide pressing social problem, as every year about 30% of women suffer physical and/or sexual intimate partner violence (WHO, 2013).[3] To address this issue, many developing countries have enacted more comprehensive legislations aimed at reducing violence against women, but these legal reforms have not necessarily benefited women. For example, when women want to file a domestic violence complaint, the police may ignore them by claiming that "domestic disputes" are not a police matter or courts may dismiss their case. For this reason, women often do not trust these institutions enough to report these issues (e.g. Jubb et al., 2010; Boesten, 2012).[4]

As a result, during the same period, Peru, Mexico, Brasil, El Salvador and Ecuador, among others have launched or expanded a special network of *Women's Police Stations* or *Women's Justice Centers* (WJCs). The WJCs are specialized police or judicial institutions whose main purpose is to improve access to justice for victims of domestic violence

[2]The 2030 Agenda for Sustainable Development is a set of 17 "Global Goals". See `http://www.undp.org/content/dam/undp/library/corporate/brochure/SDGs_Booklet_Web_En.pdf`.

[3]Women who suffer from abuse in the household are more likely to report physical, mental, sexual and reproductive health problems (Campbell, 2002). Domestic violence may also limit their ability to take care of their children. An growing literature on domestic violence shows that childhood exposure to domestic violence is associated with a number of emotional and behavioral problems (e.g. Pollak, 2004; Carlson, 2000; Huth-Bocks et al., 2001; Koenen et al., 2003; Carrell and Hoekstra, 2010).

[4]Furthermore, in cases of family violence in rural Peruvian communities, traditional justice systems often discriminate against women themselves, assuming that women are at least partially to blame for conflict (Revilla, 1999). For this reason, these institutions are thought to do a poor job of addressing the needs of women and children who are victims of violence (Franco and González, 2009).

by providing legal, psychological and medical support. Even though WJCs have been gaining popularity with policy-makers, little attention has been paid on to the actual effectiveness of such centers.

In this paper, we examine how the introduction of WJCs across Peru affects children's education outcomes and the prevalence of domestic violence. Our findings reveal that providing better access to justice for women can reduce domestic violence and have positive spillovers on children's education outcomes. Peru is a particularly interesting context given that the level of physical and/or sexual intimate partner violence has been among the highest in the world during the last decade (e.g. Garcia-Moreno et al., 2006; Bott et al., 2012). As a response to this endemic problem in the country, the *Peruvian Ministry for Women and Vulnerable Populations* decided to create the WJCs in 1999 as part of the National Program against Sexual and Family Violence. During the period 1999-2014, the number of WJCs has grown from 13 in the first year to 226 by the end of 2014, covering 100% of the 24 regions of Peru and 96% of the provinces.

To identify the casual effect of the WJCs we use a difference in-differences strategy which exploits variation created by the differential timing in the opening of the WJCs and the spatial variation in the exposure of a school/household to a WJC, together with province-by-year fixed effects. We geo-match schools and households with detailed data on WJCs' locations and founding years in order to construct two different measures of exposure to the WJC: presence of a WJC within 1 km of the school/household and presence of a WJC in school's/household's district.[5]

This empirical strategy allows us to compare changes in outcomes of schools and households (children and women) already residing in the proximity of a WJC ("treatment schools/ households") to those not yet reached by the program ("control schools/ households"). Using the panel nature of the school level data, we control for school fixed-effects and initial school characteristics in order to analyze how enrollment changes within a same school and among initially-similar treatment and control schools after the introduction of a nearby WJC.

This paper takes advantage of three different types of geo-coded datasets: school level data, individual and household-level data and administrative data on WJC that allow us to analyze the effects at a very disaggregated level. First, our school level data comes from the *Peruvian School Census*, which is a large geo-coded panel dataset on primary and secondary school enrollment that covers the universe of schools in Peru during the period 1998 to 2014. Second, our individual and household-level data comes from the *Peruvian Demographic Health Survey*, which is a nationally representative cross-sectional

[5]There are about 2,000 districts in Peru.

survey that contains rich information on demographic and socioeconomic characteristics of the household members, as well as a detailed domestic violence module for married or cohabiting women. The Peruvian DHS covers the period 2000 to 2014 and is geo-coded at the cluster level. Lastly, the administrative data on WJCs comes from the *Peruvian Ministry for Women and Vulnerable Populations* (MIMP) and consists of a geo-coded directory of all WJCs in Peru along with their founding dates from 1999 to 2014.

Our main finding is that children's education is positively affected by the introduction of the WJCs. We first show that the introduction of a WJC within 1km of a school is associated with an increase between 2% and 3% in the number of children enrolled in primary school in the year after the center introduction. We also find evidence that after the opening of a WJC, children in primary and secondary school living in households located near a WJC are significantly more likely to attend school, have better national test scores, are more likely to pass the grade and are also less likely to drop out of school. More specifically, the probability that children affected by the WJCs attended school and passed a grade increases by approximately 2 percentage points, while drop outs decrease by 1.8 percentage points. These effects are localized within a few kilometers and they are mostly driven by girls living in urban areas. These results are also robust to including district-specific trends and to limiting the sample to urban clusters and districts which ever have a WJC, and to the use of different datasets that measure the same outcomes.

The main threat to our identification strategy is time-varying unobservables that are correlated with both the timing of the WJC introduction and changes in education outcomes. To ensure that our results are not driven by selection or time-varying unobservables, we perform several falsification exercises and robustness checks. First, in order to control for the nonrandom placement of the WJCs, we also include a province-by-year fixed effect which controls for any characteristics that may vary at the province and year level. By using province-by-year fixed effects, our identification assumption is that treatment schools/households would otherwise have changed similarly, on average, to control schools/households within their same province.

Second, we focus our analysis in the middle of the rollout period for which identifying assumptions are likely to hold. In particular, we show that schools/households reached by the WJCs from 2006 to 2014 had similar pre-program trends and that the introduction of WJCs was not anticipated by changes in schooling. Third, we show that WJC placement was not anticipated by changes in enrollment.[6] Finally, we limit the samples to areas

[6]A central issue in our analysis in the fact that WJCs are not placed randomly. Conversations with policymakers and WJC managers suggest they choose where to locate primarily based on population density, the level of infrastructure and proximity to several institutions, but there was no mention of locating based on anticipated increases in schooling or previous years schooling increases. We use the panel nature of the

most comparable to the those with WJC presence: urban schools and urban clusters of households, since the WJCs were more likely to be located in more densely populated areas. We further examine the results by limiting the sample to districts which ever had a WJC.

The next focus of this paper is to pin down the mechanisms driving these results. We propose two potential mechanisms. First, the introduction of WJCs may have affected intra-household bargaining, giving women more say in household decisions by changing the threat-point. If women have a greater preference for investing in children's education than their partner, this could explain the improvement in educational outcomes after the introduction of a WJC.[7] Second, the reduction in domestic violence from the introduction of the WJC may improve the ability of women to take care of their children, allowing them to attend school. We provide evidence that both these mechanisms may be present.[8]

To distinguish between these possible mechanisms, we use the Domestic Violence Module of the Peruvian DHS, which allows us to estimate the impact of the WJCs on: (a) the prevalence of domestic violence (physical and emotional) (b) decision-making and bargaining power in the household. We find evidence that women who live within 1km of a WJC are significantly less likely to suffer from physical and emotional violence by their spouse. These results are mostly driven by older women who live in urban areas. We also find suggestive evidence of an improvement in the bargaining power of women in the household. In particular, we find that women living near a WJC are more likely to make joint decision-making with their husband, less likely to earn less than their husband and more likely to earn as much as their husband.

To the best of our knowledge, this is the first quantitative analysis that attempts to explore the impact of an unexamined dimension of institutional intervention which provides better access to justice for women, on the prevalence of domestic violence and its spillover effects on children's education outcomes. This study does not only provide ev-

school database to analyze the impact of WJCs introduced in future years on current enrollment. If WJC managers are targeting areas which have more rapidly increasing schooling, future WJCs should also correlate with changes in schooling. We do not find evidence for an impact of future WJCs and the inclusion of future WJCs does not affect our estimate of the impact of current WJC. This placebo test suggests that WJC placement was not anticipated by changes in schooling. Moreover, we also address another concern — that the results reflect changes in population after a WJC opening — and argue that this issue does not drive our results after controlling for total district population.

[7]Several economic theories of household bargaining power suggest that policies aimed at affecting spouse's outside option from a marriage may also affect within-household distribution through changes in their relative bargaining positions (McElroy and Horney, 1981; Manser and Brown, 1980).

[8]The introduction of WJC may also have contributed to breaking the silence regarding violence against women and turning it into a public issue, with the intention of improving access to justice. By making domestic violence more visible, these specialized institutions may be changing the discriminatory social values and power structures that underlie violence against women.

idence of the effectiveness of an important component of Peru's public policy aimed at curbing domestic violence, but it also contributes to the literature on gender development by providing a new insight on women's empowerment in developing countries and its indirect effect on children's education outcomes. It thereby connects the literature on domestic violence with a wider body of literature that assesses the role played by economic development in the reduction of gender inequalities (Duflo, 2012).[9] It also relates to the literature on effective policies aimed at increasing school enrollment in the developing countries (e.g. Duflo et al., 2012; Kremer et al., 2005; Kremer, 2003; Duflo, 2000). As a policy implication, our results suggest that providing better access to justice for women may be a effective tool to increase education, particularly for girls. Moreover, the fact that the results are mostly driven by girls suggests that if a mother experiences gender discrimination (due to lack of institutions preventing conflict) then this gets passed down to the next generation since daughters receive less education. In this way, the WJCs generate an inter-generational effect on gender inequality.

The remainder of this paper is organized as follows. In Section 4.2 we discuss the previous literature on domestic violence in more depth. Section 4.3 presents a brief background on the prevalence of domestic violence in Peru and on the WJC intervention. Section 4.4 describes the data. Section 4.5 presents the empirical strategy. Section 4.6 presents the main results. Section 4.7 provides supporting evidence consistent with the identification assumptions. Section 4.8 investigates the channels through which WJC introduction affects schooling. Section 4.9 concludes.

4.2 Previous Literature

There are two bodies of literature on domestic violence. The first one focuses on the risk factors for domestic violence, while the second one focuses on the effects of intimate partner violence on women's outcomes, including those for children living in households

[9]For instance, economic empowerment of women is often considered a major tool in the fight against intimate partner violence, even though its impact might be ambiguous. On the one hand, employment opportunities such as conditional cash transfers or access to welfare services may empower women by increasing their resources within the household; improve their outside options and bargaining status in their relationships; and decrease their exposure to violence (Farmer and Tiefenthaler, 1996; Stevenson and Wolfers, 2006; Aizer, 2010; Hidrobo and Fernald, 2013). On the other hand, an increase in the resources available to women may strengthen the incentives of men to use violence or threats of violence in order to control these newly obtained resources or to regain decision-making power within the household. As a result, women may become more vulnerable to mistreatment (Bobonis et al., 2013; Eswaran and Malhotra, 2011; Bloch et al., 2004). A possible explanation for this is the fact that women in developing countries do not generally count with an effective judicial protection or a credible threat in cases of domestic violence. In this study we analyze an unexplored empowerment channel for women which is better access to justice.

with domestic violence. While most fundamental studies on the causes and effects of domestic violence center in developed countries, especially United States, a new wave of literature has expanded the scope of study to developing countries due to this form of violence's perceived obstacle to the broader development agenda.

There is a growing literature on causal channels that impinge on the prevalence of domestic violence. One type of this literature focuses on the intra-household bargaining channels that affect domestic violence through improvements in women's outside options. For example, Aizer (2011) shows that a decline in the gender wage gap reduces violence against women in California. The author's interpretations is that a relative improvement in female income reduces her exposure to spousal violence by increasing her bargaining power. Stevenson and Wolfers (2006) find that the adoption of unilateral divorce laws in the United States resulted into a drop in female homicide and domestic violence. In a more recent study, Brassiolo (2016) finds a decline in spousal conflict and in extreme partner violence in response to introducing less stringent divorce legislation in Spain. Using victimization data from the US, Miller and Segal (2016) find that as female representation increases among police officers in an area, violent crimes against women in that area, and especially domestic violence, are reported to the police at significantly higher rates. They also show that increases in female officer representation are followed by significant declines in intimate partner homicide rates and in rates of repeated domestic abuse.

Another strand of this literature focuses on the heterogenous effects of conditional cash transfer programs on domestic violence. Bobonis et al. (2013) analyze the effect of the Mexican program *Oportunidades* on domestic violence and find that beneficiary women are less likely to be victims of physical violence but are more likely to receive threats of violence. Using the same randomized evaluation, Angelucci (2008) finds that among households that received small transfers, alcohol-related domestic violence declined, whereas in households that received large transfers, the level of spousal abuse from husbands with particularly low levels of education increased.

In addition, recent research has observed that in many contexts, increased autonomy and women's entry into the formal labor market is often associated with a higher likelihood of experiencing violence in Colombia (Friedemann-Sánchez and Lovatón, 2012), Bangladesh (Heath, 2012; Rahman et al., 2011) and India (Eswaran and Malhotra, 2011). Indeed, the paid employment or the non-labor income of a female intimate partner may be threatening for some men, especially those who are unemployed. Abusive partners may perceive a loss of status and power and use violence or coercion to regain control.

However, less literature has been written on the consequences of intimate partner violence (IPV) on children's outcomes, especially in developing countries. Previous research has shown that children exposed to domestic violence are associated with a number of health, emotional and behavioural problems including, low birthweight, aggressive behaviour, bullying, depression, violence in adulthood and also diminishing academic performance.[10] With respect to children's education outcomes, studies conducted in the United States have found lower reading levels among teenagers who have been exposed to domestic violence (Thompson and Whimper, 2010), lower academic achievement in math and reading for children in elementary and middle school (Kiesel et al., 2011), lower scores on standardized tests for children ages 6 to 17 - especially for girls and children younger than 12 years old (Peek-Asa et al., 2007)- and more grade repetition and truancy among children 6 to 15 years old (Emery, 2011). Moreover, Carrell and Hoekstra (2010) show that exposure to school peers from troubled families significantly decreases reading and math test scores and increases misbehaviour in the classroom.

Among the scattering studies conducted in developing countries, Jayasinghe et al. (2009) show that children who were directly or indirectly exposed to domestic violence at home had poor school attendance and lower academic achievement on average. Similarly, Durand et al. (2011) find that Brazilian children 5 to 12 years old who lived with mothers exposed to psychological, physical and sexual domestic violence were more likely to be among those dropping out of school or failing a school year.

What is perhaps most striking about this literature is that rigorous studies attempting to evaluate the effectiveness of various intervention strategies aimed at curbing domestic violence are quite scarce. This is mainly due to the difficulties and ethical considerations on collecting reliable data on domestic violence. Another difficulty is dealing with the endogeneity problem. Randomized experiments, for instance, are extremely rare. In addition, even though WJCs are one such intervention that has been gaining popularity, little attention has been paid on to the actual effectiveness of such centers on eradicating violence against women and, particularly, there is very little evidence on the extent of spillovers on their children. Two exceptions are the studies of Agüero (2013) and Perova and Reynolds (2017), which exploit the variation stemming from the gradual municipality/district -level rollout of the WPS/WJCs in Peru and Brazil, respectively. One concern with these previous studies is that they fail to establish a casual relationship and a sufficiently sound research design.

To sum up, the research to date has outlined much of the domestic violence problem and provided some fundamental understanding of its causes and consequences, but has

[10] See Edleson (1999); Wolfe et al. (2003); Pollak (2004); Fantuzzo et al. (1997); Koenen et al. (2003); Holt et al. (2008); Baldry (2003); Carlson (2000); Currie (2006); Black et al. (2010); Aizer (2011).

left policy makers with little on which to build effective interventions. In this light, our paper contributes to the literature on domestic violence by focusing on an unexplored empowerment channel for women which is better access to justice and the role of women's justice centers (WJC) in breaking the cycle of violence and generating a spillover effect on their children's outcomes.

4.3 Background

4.3.1 Domestic Violence in Peru

Domestic violence or intimate partner violence (IPV) is one of the most pressing social problems in Latin America and the Caribbean. Even though the region has received much attention on conflict, crime, political and economic instability, it is easily overlooked that violence against women is among the most pervasive types of violence in the region (Fregoso and Bejarano, 2009; Heinemann and Verner, 2006; Londoño et al., 2000).

Among the Latin American countries, Peru has gained a considerable amount of attention in recent years, largely due to the high prevalence and severity of domestic violence in this country. According to a study carried out in 10 countries by the World Health Organization in 2006, the prevalence of physical violence by a male partner ranges from 13% in Japan's urban regions to 61% in rural areas of Peru and 49% in urban areas of Peru (Garcia-Moreno et al., 2006; Morrison et al., 2007). Flake and Forste (2006) study the relationship between household characteristics and the likelihood of experiencing domestic violence in Colombia, Dominican Republic, Haiti, Nicaragua and Peru. They find that although the prevalence of domestic violence is high in all five countries, Peru had the highest percentage of instances at 38.9% followed by Nicaragua (26.1%), Dominican Republic (22.6%), Colombia (19%) and then Haiti (15.7%). Data collected by *Instituto Nacional de Estadistica e Informatica* (INEI) through the Demographic Health Surveys have found that although the prevalence of violence (physical and/or sexual) affecting women has declined from 41.2% to 32.6% from 2000 to 2015, it still remains quite high (INEI, 2001, 2015).[11] All this evidence suggests that Peru is very high on the world ranking of registered cases of domestic violence and among the leaders in Latin America in terms of prevalence of violence against women.

While the majority of IPV is perpetrated within the domestic sphere, Peru's institutions also have a reputation for gender-based violence, including sexual violence. For many decades, women in Peru have been subject to abuse- even by the one entity supposed to

[11] See Figure A13

protect them: the state. For instance, in the 1990s and early 2000s, Peru witnessed one of the most heinous violations of women's rights in recent history: under the administration of Alberto Fujimori, thousands of women were forcibly sterilized in an attempt to prevent overpopulation and poverty. The state is also complicit in institutional violence against women, ranging from insults to injury in its hospitals, health centers and schools (Boesten, 2012).

Despite legislative progress in identifying and addressing the problem, the legal system has constantly been characterized as ill-equipped to efficiently process complaints. In the early 1990s, Peru was one of the first countries in the region to develop legislation and policy to address violence against women. The Law for Protection from Family Violence was first adopted in 1993 and strengthened in 1997, attempting to codify IPV as a criminal offence while producing a distinct and expedited procedure for victims to lodge complaints. However, these legal reforms in the area of violence against women lacked a clear legal framework and have done very little to curb its persistence. In short, "many women do not bother to file complaints because the legal system is too slow to act" (UNHCR, 2010).

4.3.2 *Centros de Emergencia para Mujeres (WJC)* Program

In the last 15 years, there have been significant efforts to prevent, punish and eradicate violence, particularly in the case of violence against women. As a response to one of the highest rates of domestic violence in Latin America, the Peruvian Ministry for Women and Vulnerable Populations decided to create in 1999 the women's justice centers (*Centros de Emergencia para Mujeres*) as part of the National Program against Sexual and Family Violence.[12]

The *Centros de Emergencia para Mujeres (WJC)* are public centers which offer specialised attention to victims of domestic and sexual violence, from an inter-disciplinary and integral approach that includes legal, social and psychological dimensions. This program is aimed at strengthening the capacities of the police, prosecutors and judicial officers to detect the risk of domestic violence and also assist the victims. Aside from that, their aim is also to undertake awareness-raising and rehabilitation programs for the victims of domestic violence. In this regard, the WJCs have put in practice courses for training justice promoters *'facilitadoras en accion"*, which are volunteer women involved in activities

[12]The Peruvian Ministry for Women and Vulnerable Populations, known as *Ministerio de la Mujer y Poblaciones Vulnerables* - (MIMP) used to be called as Ministry for Women and Social Development (*Ministerio de la Mujer y Desarrollo Social - MIMDES*) when the WJC program was rollout in 1999. http://www.mimp.gob.pe/contigo/contenidos/pncontigo-articulos.php?codigo=14

and campaigns that raise awareness about the problem of domestic violence (MIMDES, 2007).[13]

Basically, the idea of these centers is to centralize the different stages that a victim of domestic violence has to go through - police station, attorney's office and medical doctor- in order to reduce as much as possible the time dedicated to issue the complaint and to follow the legal procedure in the corresponding court of justice. The service in these centers is provided free of change and is staffed by representatives of various government institutions such as police officers, prosecutors, counsellors, psychologists and public welfare agents in order to help the victims of domestic abuse.

The first women's justice center was located in the District of Lima in 1999. In order to provide more protection and access to justice to more victims of domestic violence, every year more centers have been implemented at the national level. During the period 1999-2014, the number of centers has grown from 13 in the first year to 226 by the end of 2014, covering 100% of the 24 regions of Peru and 96% of the provinces (188 of 196 provinces) (Figure 4.1). However, the program has been implemented more intensively between 2006 and 2014: from 48 WJCs in 2006 to 226 in 2014, which is an important measure. From a geographical coverage point of view, by 2014 most of the WJCs were concentrated in Metropolitan Lima and Lima Provinces (31 WJCs); in the Callao region there were 4 WJCs; the rest of the coastal region had 46 WJCs; in the sierra region there were 117 WJCs and in the jungle region there were 28 WJCs. The location of these centers is distributed mostly in urban areas.

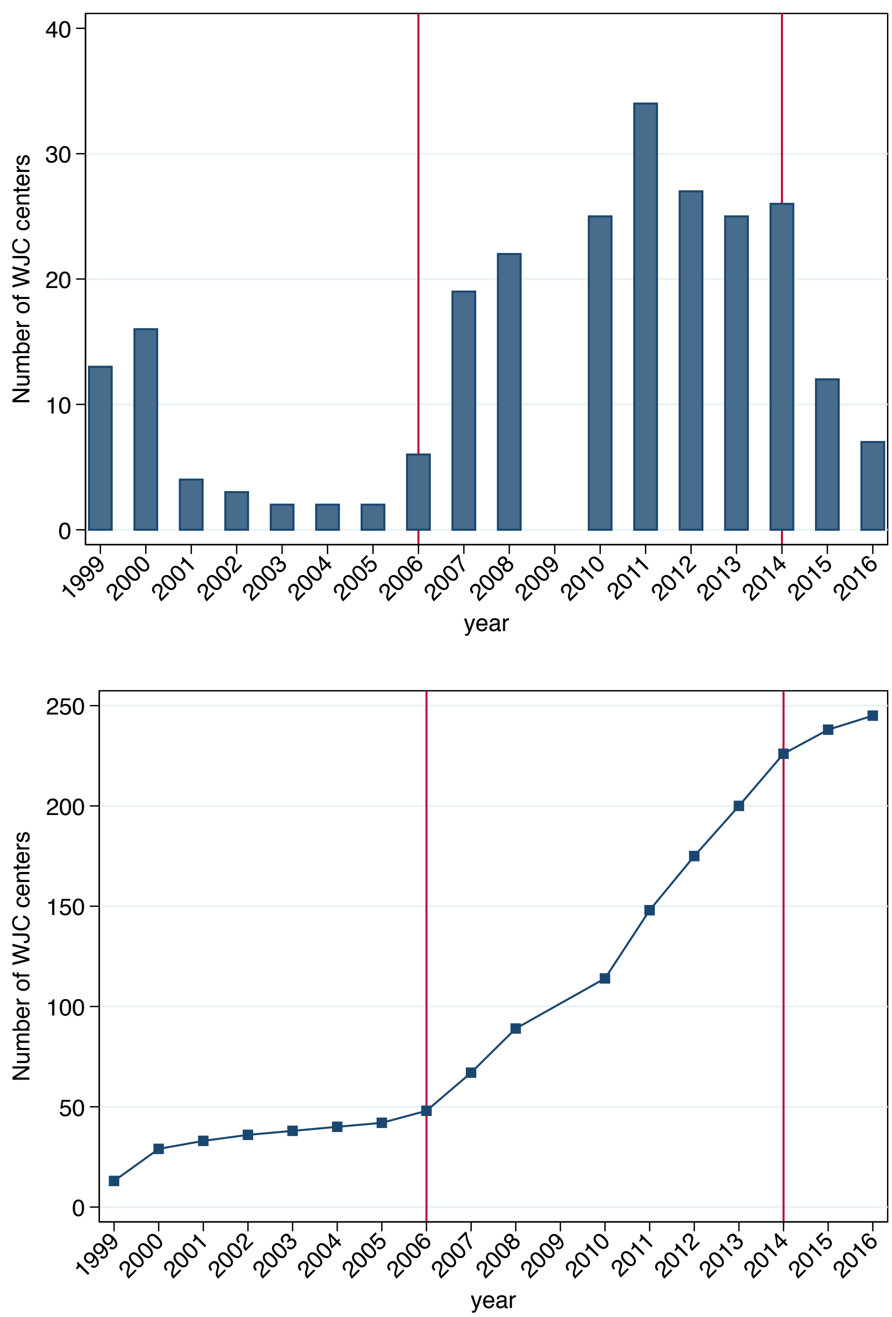
According to statistics from the MIMP, the number of domestic violence cases registered in the WJCs has increased substantially: from 29,759 in 2002 to 50,485 in 2014 (See Figure A14). Most of the domestic violence cases reported in the WJCs are of women between 25 and 45 years old (40%). However, the WJCs also receive many reports of children and teenagers (0-17 years old), which constitute around 30% of the total cases. A report from the Peruvian Ministry for Women and Vulnerable Populations in 2009, which consists of surveys and interviews in 51 women's justice centers located all over Peru during 2006-2008, shows that for the majority of the women (75%) who attended a WJCs, domestic violence stopped during and after the intervention of the WJC. However, a smaller proportion of women (25%) indicated that domestic violence did not stop in the household even after having attended a WJC (MIMDES, 2009).[14]

[13]Ministerio de la Mujer y Desarrollo Social. 2007. Â£Que son los Centros de Emergencia Mujer?. Available at `http://www.mimp.gob.pe/files/programas_nacionales/pncvfs/Centros_Emergencia_Mujer_MIMDES1.pdf`

[14]Ministerio de la Mujer y Desarrollo Social. 2009. Investigacion operativa: "Eficacia de la intervencion de los Centros Emergencia Mujer". Available at `http://www.mimp.gob.pe/files/programas_`

Therefore, it is particularly important to evaluate not only whether the opening of the WJCs has an effect on the incidence of domestic violence, but also whether these centers act as mechanism of women empowerment which indirectly might also enhance women's ability to care for their children through better social protection and access to justice.

Figure 4.1: Distribution and Growth of the Openning of the *Women's Justice Centers* (WJCs) by Year - Peru (1999-2016)



nacionales/pncvfs/estadistica/eficacia_intervencion_cem.pdf

4.4 The Data

This paper makes use of three different types of datasets which provide variation across geographical regions and time at different levels of aggregation: school level data, individual and household-level survey data and administrative data on WJCs at the district level.

4.4.1 School Level Data

The school level datasets we use are the Peruvian School Census (*Censo Escolar*, CE) and the Census Evaluation of Students (*Evaluacion Censal de Estudiantes*, ECE). The Peruvian School Census is a large panel dataset on primary and secondary school enrollment, which covers the universe of schools in Peru during the period 1998 to 2014. This dataset is collected on a yearly basis by the Peruvian Ministry of Education, with exception of the year 2003 and it contains a rich set of information at the school level. More specifically, the School Census collects comprehensive data on the total number of enrolled students by age, grade and gender. These data are designed to reflect enrollment (not attendance) statistics corresponding to the months of May-July. The School Census also collects data on school characteristics, such as language of instruction, public or private, urban or rural area and other physical plant characteristics (i.e. electricity, piped water etc). We complement these data with the Census Evaluation of Students, which contains the standardized test scores of a national exam administered every year to all primary school students in second grade during the period 2007-2014. This exam has two portions: math and language (Spanish) skills.

Each school in these datasets is given a unique ID number, which allows us to follow schools over time. In addition, one of the main advantages of these school datasets is that they are geo-coded, which means that we can observe the exact location of the school. The geographic coordinates of the schools allow us to combine these data with the WJC's locations, in order to see whether the area/district of the school is located near the WJC and thus affected by the opening of these centers that provide specialized attention to victims of domestic and sexual violence.[15]

Panel A of Table A33 shows the years of data coverage and the number of schools from 2006 till 2014, which is the period of analysis of our study. In the later years, the dataset covers a larger share of schools. It is important to note that this dataset is not a balanced panel because during the period of study some schools have closed, while

[15]See Figures A15 and A16.

others have opened. In addition, as mentioned above, there is no data available for the year 2003, since data for this year was not collected. Although this means we do not have a balanced panel, by including school fixed effects we ensure that we compare the same schools over time. The main analysis, then, draws on a nine-year unbalanced panel dataset of 36.994 primary schools (grades one through six) and 12.811 secondary schools (grades one through five).[16]

Panel C of Table A33 provides some summary statistics on school enrollment and school characteristics. The average primary school in our sample has 95.9 students, while the average secondary school has 175 students. The proportion of primary schools is higher in rural areas, while secondary schools are more likely to be found in urban areas. The majority of primary schools are public and teach in Spanish language, but there is also a small proportion that teach in Quechua and other native languages. In contrast, a large proportion of secondary schools (40%) are private and in almost all of them the language of instruction is Spanish.

A final important issue of the School Census data is that it measures total number of children enrolled, not enrollment/attendance rates. This may lead to the concern that our results reflect changes in population. However, we discuss this issue in greater detail in Section 4.5. In addition, we also use, as a robustness check, the Peruvian Demographic Health Survey (2006-2014) to estimate the share of children who are attending school.

4.4.2 Individual and Household Level Data

Since we do not observe enrollment rates with the School Census, we also use the *Encuesta Demografica y de Salud Familiar* (ENDES), which is the Peruvian version of a Demographic and Health Survey (DHS) to estimate the share of children in primary and secondary level who are enrolled and attending school. In order to be consistent with the school data, for this analysis we use the Peruvian DHS which also covers the period 2000-2014. The Peruvian DHS is exceptionally a continuous survey, which means that the data is collected every year. These surveys are cross-sections designed to be representative at the national and regional levels. The DHS employs a stratified random cluster sampling procedure in which the country is divided into several primary sampling units (in this case, districts) and clusters of households are randomly selected.

In addition to the standard survey which includes demographic and socioeconomic characteristics of the household members (especially for women and children), the Peruvian DHS also includes a domestic violence module which asks eligible women if they

[16]The primary-school sample covers between 4.1 and 3.5 million students each year, whereas the secondary school sample covers between 2.3 and 2.7 million students.

have ever experienced physical, sexual or emotional abuse from their current or previous partner in the last 12 months.[17] While all women between the ages of 15 to 49 are asked to participate in the standard survey, only one women in each household, who has ever been married or partnered, is randomly selected to complete the domestic violence module. Women who are never married or never cohabited are excluded from the sample. This selection process is taken by the DHS program in order to minimize underreporting of domestic violence events.[18]

The DHS captures four different types of domestic violence: moderate physical violence, severe physical violence, sexual violence and emotional violence.[19] Since the last one is less visible and more difficult to measure, in this study we define exposure to a domestic violence event if the woman has ever experienced any type of moderate, severe or sexual violence during the last 12 months. The main advantage of using this household survey is that we can link children's school attendance status with their mother's self-reported domestic violence. This information is crucial in order to be able to understand the mechanisms behind the results.

Panel B of Tables A34 and A35 provides summary statistics on children's school attendance status and on women's domestic violence during 2006-2014, respectively. According to the Peruvian DHS, the school attendance rate in primary level is 97% for both boys and girls, which is almost universal. The school attendance rate in secondary level is also quite high (89%) and very similar between genders. Given that secondary school

[17] It should be noted that though this is an important measure of domestic violence, it does not report the different forms of gender-based violence that affect women beyond spouses and inter-family relationships.

[18] The domestic violence module of questions is implemented only to a subsample of the women selected for the Peruvian DHS sample. There are three security and ethical precautions increasingly mandated by the DHS program for the collection of data on domestic violence. The first requires that the interviewer does not continue with the questions on domestic violence if privacy cannot be ensured. In general, the interviewers are women trained to elicit trust from the respondents. The second requires that only one eligible woman in each selected household is to be administered the module questions. In sample households where more than one woman is eligible for the DHS survey, the domestic violence module is administered to only one randomly selected woman. By interviewing only one woman in each household, possible security breaches, due to other persons in the household knowing that information on domestic violence was given, are minimized. The third requires that the domestic violence questions should be only administered to ever-married or cohabiting women, even though the DHS sample includes all women age 15-49. Underreporting of domestic violence events is quite low, as only 1% of the eligible women was not interviewed because privacy was not made possible in the household.

[19] The DHS defines *moderate physical violence* if the woman experienced at least one of these acts from their spouse or partner:(a) spouse ever pushed, shook or threw something, (b) spouse ever slapped respondent, (c) spouse ever punched respondent with fist or something harmful, (d) spouse ever kicked or dragged respondent. *Severe physical violence* is defined if the woman experienced at least one of the following acts:(e) spouse ever tried to strangle or burn, (f) spouse ever threatened with knife/gun or other weapon, (g) spouse ever attacked with knife/gun or other weapon. *Sexual violence* is defined if the woman experienced at least one of the following acts: (h) spouse ever physically forced sex when not wanted, (i) spouse ever forced other sexual acts when not wanted (j) spouse ever twisted arm or pulled hair.

is not compulsory, the drop-out rate reaches 9% of the students in this educational level. As for the prevalence of domestic violence, the data indicate that 39% of ever-partnered Peruvian women declared to have experienced abuse from their spouse during the last 12 months.

In addition, the Peruvian DHS also records GPS coordinates for every cluster of households in a certain district, which allows us to measure not only presence of WJC in the district of residence but also proximity to the WJC. Although this data was collected yearly, in this study we were able to obtain the GPS cluster locations only for the 2000, 2004-2008, 2009, 2010, 2011 and 2014 Peruvian DHS Surveys.[20] Since the DHS does not disclose the name of the villages (*centros poblados*) were the clusters are located, the final sample is a repeated cross-section of individuals (children and women), where the lowest geographical unit we can condition on is the district.

Our concern with this database is linked to the fact that GPS locations of the sampled DHS clusters of households are displaced before public release to preserve confidentiality of respondents. The GPS displacement is randomly carried out so that: urban clusters are uniformly displaced up to 2 kilometers and rural clusters are displaced up to 5 kilometers, with 1% of the rural clusters displaced up to 10 kilometers. In addition, the displacement is restricted so that the points stay within the second administrative level, which is the province. Therefore, the GPS displacement procedure introduces a random error, which can substantively affect the results of the analysis (Burgert et al., 2013). Perez-Heydrich et al. (2013) propose several recommendations in order to reduce any distance measurement error. Firstly, they suggest that the amount of measurement error depends on the spatial density of the resource facilities. As the density of the resource facilities decreases, the probability that a DHS cluster is linked to the correct closest WJC increases for all types of locations (urban and rural). In Peru, there are a total of 226 WJCs by 2014, which means that the spatial density of the WJCs is quite low and, thus, the measurement error is quite reduced. Secondly, the authors recommend to study the effect of the service within a reasonable buffer distance, rather than using the closest-distance to the resource facility. For this reason, we are going to measure exposure to the WJC through different groups of Euclidean distance buffers. Lastly, we are also going to limit the analysis to urban areas because in these locations the range of displacement is less than in rural areas.

[20]See Figure A17.

4.4.3 Administrative Data on WJCs

Information on the rollout of the WJCs was provided by the *Peruvian Ministry for Women and Vulnerable Populations* (MIMP) and consists of a directory of WJCs across all over Peru. This directory contains the name of the WJCs, their founding dates (date-month-year), their administrative locations (district-province-department) and their addresses during the period 1999 to 2014. By using the administrative locations and addresses provided in the directory of the MIMP, we were able to geo-code all the WJCs, which allows us to have not only the district where they are located but also their exact GPS location.

This data collection project resulted in a dataset of 226 WJCs from 1999 till 2014. Figure 4.1 shows a histogram of WJC founding dates and it also illustrates the evolution of the opening of WJCs since 1999 till 2016, while Figure 4.2 maps the rollout of the WJCs at the national level, which allows to visualize the extensiveness and national scope of the program. From both graphs, we can clearly see a substantial growth in the number of centers over time, where 81% of them are founded after the year 2006.

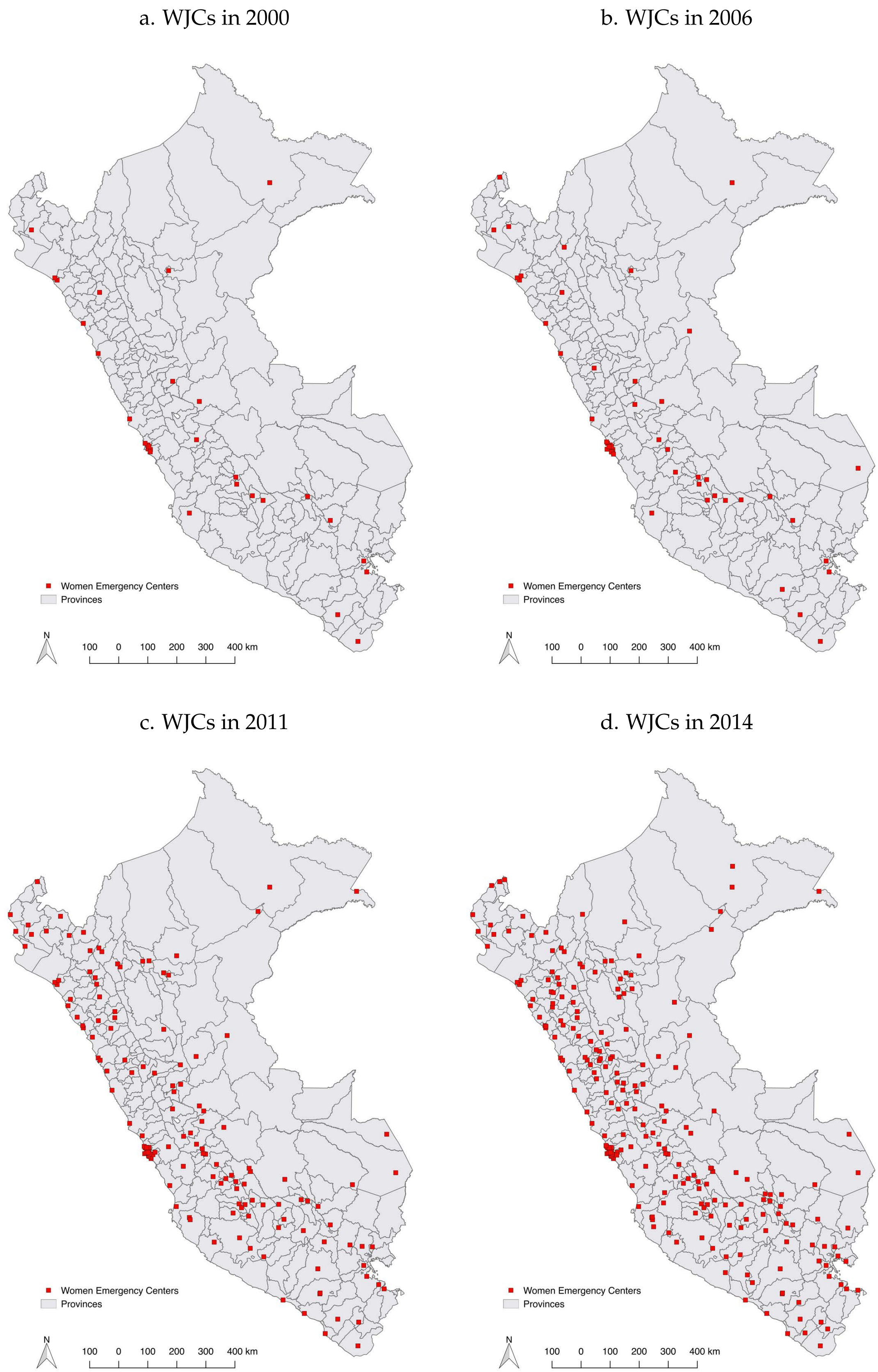
4.4.4 Measuring Exposure to the WJCs

In order to be able to match the data on WJCs with the data on education, we construct two measures of exposure to the program. The first measure uses the GPS coordinates of the child's school/DHS cluster of residence and the GPS coordinates of the WJCs in order to measure a 1 kilometer Euclidean buffer distances from every school/DHS cluster. For this method, the Euclidean buffer of 1km was first centered on each school/DHS cluster and then each school/DHS cluster was linked to a WJC if the WJC falls within the buffer, without consideration of district administrative borders. For instance, a school/DHS cluster located within 1km of a WJC founded in 2008 is coded as having a WJC within 1km of the school/DHS cluster since the 2008 school year. Figure A12 shows a visual representation of the Euclidean buffers for two specific regions in Peru, Lima and Tumbes.

The second measure matches the presence of a WJC in the district based on its date of opening and location, with the district of the school/DHS cluster of residence. For instance, a school/DHS cluster in the district of Lima (150101) with a WJC introduced in 2006 is coded as having a WJC in the district of Lima since the 2006 school year.

Our preferred measure is the one that uses the Euclidean buffer since we want to estimate the impact of having a WJC in the neighborhood of the school/household. The second measure is used as a robustness check because it might not always capture accurately the impact of the WJCs due to the fact that districts in Peru have very different sizes. Some districts are very big, while other are very small. Panel B of Table A33 and Panel A of Tables A34 and A35 shows descriptive statistics of exposure to the WJCs at the school and individual level. The main reason for our choice of a 1km distance buffer instead of a larger buffer is not only because centers may have a very localized effect at the neighborhood level, but also because the measure of exposure using a 5km Euclidean buffer seems to be very similar to the one that uses presence of WJC in the district.

Figure 4.2: Rollout of the WJCs across Time and Space (1999-2014)



4.5 EMPIRICAL STRATEGY

4.5.1 Placement of WJCs

A central methodological issue in our analysis is the fact that WJCs are not placed randomly across the country. Even though our analysis will take advantage of variation over time, which will account for any fixed differences across areas, it still remains important to understand what drives placement since placement decisions may not be orthogonal to other factors that could affect education outcomes.

We address this concern in a number of ways which lead us to believe that the link between the opening of the WJCs and education outcomes is casual. First, we had several discussions with the Peruvian policymakers and WJC managers about the location choices. Since the foundation of the first WJC in 1999 till the end of 2005, the primary criteria they cited when deciding where to locate were population density and level of infrastructure at the regional level. In this stage, capitals and large cities were prioritized locations to open a WJC. Starting from 2006, after the decentralization process which transferred the responsibility of the WJCs to the local governments (districts), the Peruvian policymakers decided to open new WJCs at the district level and they incorporated additional criteria such as proximity to police stations, district attorney offices (known as *fiscalias*) and health establishments. Even though program guidelines suggested that priority should be given to poorer districts with sufficient judicial and medical infrastructures, in several occasions, political representatives had certain autonomy in deciding the order in which districts received the program. However, our conversations with the Peruvian policymakers suggest that educational considerations, and in particular enrollment rates or schooling performance, were never factored into program placement decisions.

We are able to evaluate this endogenous placement statistically using our data. To do this we estimate, at the district level: (a) the determinants of having a WJC by the end of the sample in 2014 and (b) the determinants of adding a WJC during 2006-2014, which is the period when the program grew substantially. We focus on several variables at the district level cited by the Peruvian policymakers such as: number of justice courts, number of district attorney offices, number of police stations and number of health establishments. We also control for district population at baseline and department fixed-effects. Moreover, in order to verify that education patterns before the program do not predict where the WJCs are introduced, we also control for pre-program changes in primary and secondary school enrollment at the district level.

The results from these regressions are shown in Table A36. In general, the results corroborate the evidence we collected from our conversations with the Peruvian policy-makers and WJC managers. Districts with more police stations, more district attorney offices, more health establishments and more densely populated are more likely to have WJCs by 2014 and more likely to add them during 2006-2014. Clearly, urban areas with more infrastructure development are more likely to have these specialized centers for women. In addition, pre-program changes in primary and secondary district enrollment do not seem to have any impact. Neither coefficient is statistically significant and both are very small. This result suggests that WJC placement between 2006-2014 was not based on pre-program changes in schooling.

Another concern related to WJC placement is that if we estimate the impact of the WJCs on all areas, our results might be identified off of rural areas which are not at risk of having a WJC and these may not be an accurate comparison for those areas which get a WJC. Given this, we will focus our analysis on a specification in which we limit the sample to urban areas (urban school and households), which are the ones more “at risk" for opening a WJCs. As a further robustness check, we will also limit our samples to districts which ever have a WJC during the sample period.

4.5.2 School Level Specification

We use a difference-in-difference empirical strategy to estimate the impact of WJCs on education outcomes. We exploit the variation created by the differential timing in the opening of the WJCs and also the spatial variation in the exposure to a WJC. First, we study the overall effect of WJCs on education outcomes at the school level by using the following regression equation:

$$Y_{st} = \beta_0 + \beta_1 WJC_{st} + \alpha_s + \lambda_{pt}\gamma_t X_s^{'} + \varepsilon_{st} \tag{4.1}$$

where (Y_{st}) is the education outcome (i.e. total number of children enrolled, standardized test scores) in school s at year t, (WJC_{st}) is an indicator variable that takes the value of one if the school has a WJC within 1km/in the district of the school, (α_s) is a school fixed-effect, (λ_{pt}) is a province-by-year fixed-effect, $(\gamma_t X_s^{'})$ is a year-interacted vector of school's initial characteristics (including initial school enrollment, presence of electricity, presence of piped water, school language (Spanish), urbanization and public school dummy) and (ε_{st}) is a random error term. The inclusion of school fixed-effects accounts for any time-invariant characteristics at the school level. However, this does not account for any differential trends in education outcomes associated with WJC placement. To address this, we allow the year fixed-effects to differ by province and by measures of school's baseline enrollment and baseline infrastructure. Firstly, province-by-year fixed effects rule out the concern that our results are driven by changes that vary by province and year such as an increase in political corruption or a decrease in provincial resources. Secondly, because initially-different schools might be more likely to change differently, this empirical specification focuses on comparing changes in treatment and control schools with similar initial characteristics that might drive WJC placement.

The coefficient of interest is (β_1), which captures the average change in enrollment in schools that are located near the WJCs or in districts with WJC, to the average change in enrollment in schools that did not have a WJC. The identification assumption is that treatment schools located in the proximity of a WJC/in districts with WJC would otherwise have changed similarly, on average, to those controls schools that are not exposed to the services of a WJC. In practice, by controlling for province-by-year fixed-effects (λ_{pt}) and by variables that drive WJC placement, the identification assumption is that treatment schools would otherwise have changed similarly, on average, to control schools within their same province and with similar initial characteristics. Throughout this analysis, we cluster our standard errors at the school level. We also estimate this regression including district-specific time trends.

As noted in the introduction, we are concerned about the possibility that the results are driven by time-varying variables which might influence both the opening of the WJCs and school enrollment. A related issue is the possibility that WJC managers consciously decide to introduce centers where school enrollment is increasing. To address both of these issues, we use the panel nature of the school data in order to construct a placebo treatment based on the timing of the WJCs introduction. We estimate whether *future* WJCs predict current enrollment using equation 4.2 below:

$$Y_{st} = \beta_0 + \beta_1 WJC_{st} + \beta_2 WJC_{st+1} + \alpha_s + \lambda_{pt} + \gamma_t X_s^{'} + \varepsilon_{st} \tag{4.2}$$

where (WJC_{st+1}) is an indicator variable that takes the value of one if the school has a WJC within 1km/in the district of the school in year $t+1$. If $\beta_2 > 0$ is positive and significant, this would indicate that WJCs are being introduced in areas where schooling is increasing more rapidly. While, if $\beta_2 = 0$ this would indicate that WJCs are introduced in areas in which school enrollment is growing for other reasons.[21] Therefore, the coefficient β_2 effectively captures the effect of future openings for areas that are not covered by the

[21] This technique has already been used to address this concern by LaFerrara et al, 2012; Oster and Steinberg, 2013.

WJCs in t. Our hypothesis for the placebo regression is that total enrollment in schools that do not have a WJC within 1km/in the district should *not* be affected by the fact that a WJC may open in the future in the proximity of these schools.

4.5.3 Individual Level Specification

Since our school level data contain number of students enrolled, but not enrollment rates, we then use, as a further robustness check, the *Peruvian DHS* to estimate the impact of WJCs on children's school attendance status. The main individual outcomes is a dummy variable indicating whether the child is attending school during the year of the survey. We also use additional individual outcomes, which are defined as a changes in school attendance status between one year and the next, conditional on the child being enrolled in school. Therefore, the dependent variable can be classified as: (a) currently attending school, (b) passed grade (c) repeated grade (d) dropped out and (e) left school more than 2 years ago. Using the same identification strategy, the regression equation is the following:

$$y_{it} = \gamma_0 + \gamma_1 WJC_{it} + \alpha_d + \lambda_{pt} + \delta X_{it}^{'} + \varepsilon_{it} \tag{4.3}$$

where (y_{it}) is one of the previously discussed school attendance statuses for child i at year t, (WJC_{it}) is an indicator variable that takes the value of one if there is a WJC within 1km of the child's household/in the district of residence of child i in year t, (α_d) is a district fixed-effect, (λ_{pt}) is a province-by-year fixed-effect, $(X_{it}^{'})$ is a vector individual-level characteristics (including age, gender, household's head years of education, number of children in the household aged 0-18, number of children in the household aged 0-5, number of female adults, number of male adults, rural residence dummy) and (ε_{it}) is a random error term. Standard errors are clustered at the district level and we also include district-specific time trends.

The coefficient of interest is (γ_1), which captures the average change in school attendance status of children that are located near the WJCs or in districts with WJC, to the average change in school attendance status of children that are not reached by a WJC. The identification assumption is that in the absence of the WJCs, treatment households would otherwise have changed similarly, on average, to control households within their same province. Note that in this specification we cannot control for individual fixed-effects because the DHS database is a repeated cross-section.

4.6 Results

4.6.1 Impact of WJCs on School Enrollment

4.6.1.1 Main Results

This section analyzes our estimates of the impact of the WJCs on education outcomes at the school level. From estimating equation 4.1, Table 4.1 and Table A37 present estimated impacts of WJCs on average enrollment in primary schools and secondary schools, respectively. While Table A38 presents the impact of WJCs on standardized test scores for second grade students in primary level.

Panel A of Table 4.1 shows our primary school enrollment estimates when exposure to the program is measured through the presence of a WJC within a 1km Euclidean buffer. Column 1 presents the results using the entire sample. The coefficient on WJC within 1km is positive and statistically significant. This result indicates that the introduction of a WJC within 1km of a school is associated with an increase of 2.8% in the number of children enrolled in primary school in the year after the center was opened. Column 2 shows this regression after including district-specific trends to address the concern that districts that have a WJC are trending differently than those that do not. The coefficient is almost unchanged (2.7%) and still highly significant. In Column 3, we include district population as a time-varying control in order to rule out the concern that our results might be driven by mechanical changes in population, especially due to the fact that our school data measure number of students enrolled, not enrollment rates. After controlling for district population, the impact of WJCs on primary school enrollment is even larger (3.3%) and statistically significant. Our preferred specifications are shown in Columns 4 and 5, in which we limit the sample to just urban schools and districts that ever have a WJC, which means that control schools are most comparable to those which are affected by a WJC. Although this restricts the sample significantly, the coefficient for urban schools in Column 5 is also larger in magnitude to the overall sample (3.2%) and highly significant. Lastly, the impact for districts that ever have a WJC is bit smaller in magnitude (2.4%) and significant, despite the fact that we restrict the sample size even further.

In Panel B of Table 4.1 we explore the impact of WJCs on primary school enrollment by using the alternative measure of exposure, presence of a WJC in the district. We use this alternative explanatory variable as a robustness check and also to explore whether the opening of a WJC matters in broader surroundings. Panel B shows that introducing a WJC in the district also has a positive and significant effect, but the coefficient is a bit

Table 4.1: The Effect of WJCs on Primary School Enrollment (2006-2014)

Dep. variable	**Log (Primary School Enrollment)**				
Sample	All schools	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard	Standard
	(1)	(2)	(3)	(4)	(5)
Panel A: WJC within a distance buffer from the school					
WJC within 1km	0.028***	0.027***	0.033***	0.032***	0.024**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)
Log (District Population)			0.443***	0.424***	0.415***
			(0.023)	(0.031)	(0.055)
Observations	315,221	315,221	315,221	119,232	103,662
Number of schools	36947	36947	36947	14405	12413
Mean dep. var	95.9	95.9	95.9	177.8	127.7
Panel B: WJC in the district of the school					
WJC in the district	0.009*	0.002	0.005	0.012**	0.019**
	(0.005)	(0.004)	(0.005)	(0.006)	(0.009)
Log (District Population)			0.439***	0.417***	0.398***
			(0.023)	(0.031)	(0.056)
Observations	315,407	315,407	315,407	119,270	103,730
Number of schools	36994	36994	36994	14412	12427
Mean dep. var	95.9	95.9	95.9	177.8	127.7
School FE	YES	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES

Robust standard errors in parentheses

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

Notes: The dependent variable is the logarithm of enrollment plus one. The independent variables measures the number of WJCs within a 1km Euclidean buffer from the school and presence of WJC in school's district. Standard errors (in parentheses) are clustered at the school level. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (spanish), urban and public school dummy).Source: Peruvian School Census 2006-2014.

lower (1%), indicating that the effect probably decreases with distance. Focusing on our preferred specifications in Columns 4 and 5, we find that adding a WJC in the district increases the total number of children in primary school between 1.2% and 1.9%.

Table A37 shows the impact of WJCs on secondary school enrollment, using the different measures of exposure to the program. We also find a positive impact on the number of children enrolled in secondary school (2.9%) when we use the entire sample, but the effect is not robust to controlling for district specific trends and to limiting the sample to districts which ever have WJC. The specification with urban schools is the only one that yields a positive and significant coefficient of 3.4% for secondary school enrollment.

Lastly, consistent with these results, we also find some suggestive evidence of a positive effect on standardized test scores for primary school children located in schools near a WJC. Table A38 shows that test scores of children in schools located in the proximity of a WJC are 0.02 - 0.05 standard deviations higher. Even though these results are not robust to all the different specifications, they are positive and highly significant for urban schools.

Table 4.2: The Effect of WJCs on Primary Level 2nd Grade Test Scores - (2006-2014)

Dep. variable	**Standardized Test Scores (2nd Grade)**			
Sample	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the school				
WJC within 1km	0.028*	0.018	0.040**	0.027
	(0.017)	(0.019)	(0.018)	(0.021)
Observations	181,240	181,240	92,666	69,822
Number of schools	29737	29737	13507	10858
Mean dep. var	508.9	508.9	536.9	526.9
Panel B: WJC in the district of the school				
WJC in the district	0.026**	-0.020	0.050***	0.050***
	(0.011)	(0.016)	(0.013)	(0.016)
Observations	181,279	181,279	92,681	69,838
Number of schools	29747	29747	13510	10862
Mean dep. var	508.9	508.9	537.0	527.0
School FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

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

Notes: The dependent variable is the average of the standardized reading and math test scores for 2nd grade of primary school. The independent variables measures the number of WJCs within a 1km Euclidean buffer from the school and presence of WJC in school's district. Standard errors (in parentheses) are clustered at the school level. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (spanish), urban and public school dummy). Source: Peru ECE 2007-2014.

All these findings suggest a strong connection between the presence of WJCs and total number of children in school. They also indicate that these findings are localized to within few kilometers and they are mostly driven by urban areas. In Table A51 of the Appendix, we also show these effects broken down by gender and grade. We find that these effects are similar for boys and girls, even though they seem to be driven mostly by girls. We also find that the impact is equally distributed among the different grades.

4.6.1.2 Placebo regression: *Future* WJCs

As mentioned earlier, the main threat to our identification strategy is the possibility that WJCs were rolled out in response to changes in enrollment, rather than causing them. This is strongly linked to the issue of endogenous WJC placement. Even though, we account for characteristics which are constant over time through school fixed-effects, one concern that remains is the possibility that WJCs are placed in areas where enrollment is increasing more rapidly since center managers or policymakers are targeting more densely populated areas. Another concern is posed by time-varying unobservables correlated to both the timing of the WJCs and school enrollment. For example, it could be that areas reached by the WJCs are also hit by a positive economic shock or there are improvements in public welfare programs at the time they are opening the WJCs. We already account for this by controlling for province-by-year fixed effects.

However, another way to address the concern that WJCs are located in areas that are changing in other ways that we do not observe is by constructing a placebo treatment based on the timing of the WJC openings. We estimate analogous regressions to the ones in Tables 4.1 and A37 (our baseline school-level specification), but instead of only looking at the effects of opening a WJC on current enrollment, we also look at the effects of *future* openings. The idea is that if *future* WJC openings predict current enrollment, this would suggest that WJC placement anticipates changes in schooling, rather than causing them. Table A39 and Table A40 show the results for this falsification exercise for primary and secondary school enrollment, respectively. We find that the effect of future WJCs is virtually zero and not statistically precise, suggesting no strong evidence of pre-trends. In addition, the inclusion of future WJCs does not affect our estimate of the impact of current WJC on school enrollment.

4.6.2 Impact of WJCs on Children's School Attendance

The evidence above suggests that overall primary school enrollment increases in response to WJC introduction. Here, we analyze the impact of WJCs on children's school attendance and their attendance status as an additional robustness check since a downside of our school-level data is that we observe number of students enrolled, not enrollment rates. Table A41 and Table A42 summarize the estimated impacts of WJCs on children's school attendance in primary and secondary level, respectively, from estimating equation 4.3. While, Table 4.3 presents the results for children's attendance status.

First, Panel A of Table A41 indicates that children in primary school living in households located near a WJC are significantly more likely to attend school. More specifically, living in the proximity of a WJC increases children's school attendance by approximately 2 percentage points. Focusing on our preferred specifications in Columns 3 and 4, we find a positive and statistically significant effect on children's primary school attendance after the opening of a WJC in the proximity of the household but also in the district of residence. These results are robust to using the different measures of exposure to the program and they are also similar in magnitude to the results found with the school-level data, which is reassuring.

Second, in Table A42 we also find a positive and statistically significant impact of WJCs on secondary school attendance for those children living within 1km of the center. These effects range between 2 to 3 percentage points. However, this effects disappear when we use presence of a WJC in the district as a measure of exposure.[22]

Lastly, the impact of WJCs on school attendance status - grade advancement conditional on staying in school, repeating grade, recent drop-out and old drop-out was also estimated using the same method as reported for school attendance. Results in Table 4.3 show that children located near a WJC are significantly more likely to pass a grade and they are also significantly less likely to drop out of school. However, we do not find an effect on grade repetition nor on having left school more than two years before the opening of the centers. These results are robust to using different samples of children (i.e. children of the women selected for the domestic violence module). What we find, overall, is that investments in children's human capital are affected positively by the introduction of the WJCs.

[22]Due to the GPS displacement issue in the Peruvian DHS data, we also estimate the impact of WJCs using two additional Euclidean buffers: 3km and 5km. Results in Tables A52 and A53 show that when we analyze the effect of the WJC in broader surroundings we do not find a significant impact for both primary and secondary school attendace rates.

Table 4.3: School Attendance Status and Proximity to a WJC - (2006-2014)

Sample	**Primary School Attendance Status** Children: 6-11 years old				**Secondary School Attendance Status** Children: 12-16 years old			
Dep. variables	Passed grade (1)	Repeated grade (2)	Dropped out (3)	Left school +2 years ago (4)	Passed grade (5)	Repeated grade (6)	Dropped out (7)	Left school +2 years ago (8)
Sample A: All Children								
WJC within 1km	0.020**	-0.004	-0.018**	0.001	0.020*	-0.000	-0.017*	-0.002
	(0.010)	(0.005)	(0.009)	(0.001)	(0.013)	(0.005)	(0.012)	(0.009)
Observations	64,921	64,921	64,921	64,921	53,378	53,378	53,378	53,378
Number of districts	1165	1165	1165	1165	1161	1161	1161	1161
Mean dep. var.	0.917	0.048	0.023	0.002	0.778	0.036	0.094	0.085
Sample B: Children of the women selected for the DV Module								
WJC within 1km	0.023***	-0.006	-0.019***	0.001	0.030**	-0.007	-0.018	-0.003
	(0.008)	(0.005)	(0.007)	(0.001)	(0.013)	(0.005)	(0.012)	(0.009)
Observations	48,213	48,213	48,213	48,213	30,380	30,380	30,380	30,380
Number of districts	1155	1155	1155	1155	1135	1135	1135	1135
Mean dep. var.	0.919	0.048	0.022	0.002	0.782	0.038	0.090	0.084
District FE	YES	YES	YES	YES	YES	YES	YES	YES
Province-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

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

Notes: The dependent variable is a dummy indicating the school attendance status of the child. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the child's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample for primary level includes children between the ages of 6 and 11 and the sample for secondary level includes children between the ages of 12 and 16. Covariates include age, gender, household's head years of education, number of children in the household aged 0-18, number of children in the household aged 0-5, number of female adults, number of male adults, rural residence dummy, district fixed effect and province-by-year fixed effect. Source: Peru DHS 2006-2014.

4.7 Internal Validity Checks

In this section we present several robustness checks that support the validity of the identification assumption of the paper. The main threat to the identification strategy is the correlation between the order of the opening of the WJCs and the trends in education patterns before the rollout of the program. Basically, the average effect of the WJCs would be biased if the timing of WJCs creation was correlated with pre-program changes in education outcomes. In order to test this, we begin by estimating a regression of pre-program changes in school enrollment on indicators for the year the WJC was introduced within a 1km radius of the school:

$$\Delta Y_{st} = Y_{st-1} - Y_{st} = \gamma + \alpha_t + \sum_{k \geq t} \delta_k I(WJCyear_{<1km,s} = k) + \varepsilon_{st} \tag{4.4}$$

The dependent variable is ΔY_{st} is the change in primary/secondary total school enrollment from year $t-1$ to year t. The set of dummy variables $(WJCyear_{<1km} = k)$ take the value of 1 in the year in which a WJC was opened within 1km of the school. Year fixed-effects are denoted as α_t. The data for this test is derived exclusively from the School Census (CE) database and the sample is restricted to those schools that were reached by the program between 2006 and 2014. The reference group is the opening of a WJC in 2006. If $(WJCyear)$ effects are jointly significant it would indicate that year of WJC creation within 1km of the school was correlated with pre-program changes in total school enrollment.

We cannot perform the same test with the *Peruvian DHS* since we do not observe the same clusters of households over time. This means that we cannot exploit the variation generated by proximity to the WJC through Euclidean buffers. However, we can still check whether the timing of WJC introduction in the district is correlated with changes in school attendance in the district. For this case, we regress pre-program changes in district-level school attendance rates on yearly indicators of WJC introduction in the district:

$$\Delta y_{dt} = y_{dt-1} - y_{dt} = \gamma + \alpha_t + \sum_{k \geq t} \delta_k I(WJCyear_d = k) + \varepsilon_{dt} \tag{4.5}$$

We do not find evidence that pre-program trends in education patterns were correlated with the order of the WJC implementation during the period 2006-2014. Table A43 and A45 report the results of estimating equation (4.4) and (4.5), respectively.[23] Results in Table A43 indicate that opening a WJC within 1km of the school does not significantly

[23] We also estimate equation (4.5) at the school level. Results are shown in Table A44.

explain pre-program changes in primary and secondary school enrollment between 1998-2005. Similarly, results in Table A45 show that the opening of a WJC in the district is not correlated to pre-program changes in school attendance. Columns 2, 3, 5 and 6 repeat this exercise by gender. In all cases, we are unable to reject the null hypothesis of the joint test. These findings strongly suggest that pre-program time trends for the education outcomes of interest are not correlated with the introduction of the WJCs between 2006-2014.[24]

4.8 Mechanisms

In this section we provide some initial evidence on the mechanisms that might potentially drive these effects. We distinguish two possibilities. Firstly, the introduction of WJCs may have contributed to break the silence regarding violence against women and turning it into a public issue. By making domestic violence more visible, these specialized institutions may be changing the discriminatory social values and power structures that underlie violence against women. Alternatively, the presence of a WJC in the neighborhood or in proximity of the household may generate a more credible threat to the potential offenders through greater chances of demanding police involvement and criminal penalties. This distinction is potentially important when thinking about public policy implications.

Both mechanisms lead to the conclusion that WJC's intervention in households with abuse may change the behavior of offenders and victims. In other words, the opening of WJCs might be a powerful tool to reduce the incentives of the spouse to choose domestic violence through an improvement of the bargaining power of women in the household and, in turn, it might also improve women's ability to take care of their children. Several economic theories of household bargaining power suggest that policies aimed at affecting spouse's outside option from a marriage may also affect within-household distribution through changes in their relative bargaining positions (McElroy and Horney, 1981; Manser and Brown, 1980).

In order to distinguish between these possible mechanisms, we use the Domestic Vio-

[24]In Tables A55 and A56 of the Appendix, we also show results on three different windows of pre-program changes in education outcomes , 1998-1999, 1998-2005 and 1998-2010, at the school and district level, respectively. These findings show that pre-program changes in education at the begnining of the roll-out are correlated with the timing of the WJC introduction. While, the other two windows of pre-program education results, 1998-2005 and 1998-2010, indicate that the rollout year is never significant. For this reason, we decide to focus our analysis in the middle of the rollout for which identifying assumptions are likely to hold. We also perform the same test with other outcomes used in the study such as standardized test scores and domestic violence and we do not find evidence of pre-program trends. See results in Tables A57 and A58

lence Module of the Peruvian DHS, which allows us to estimate the impact of the WJCs on: (1) the prevalence of domestic violence during the last 12 months (physical and emotional) (2) decision-making and bargaining power in the household. Table A46 presents the results of regressing the likelihood of experiencing domestic violence in the last 12 months against the presence of a WJC within 1km/in the district after controlling for age, age at first marriage, number of children, years of education, number of household members, number of households in the dwelling, marital status, rural residence dummy, district fixed-effects and province-by-year fixed effects. These findings show that women living within 1km of a WJC or in a district with WJC are significantly less likely to suffer from physical violence by their spouse.[25] These results are robust to including district specific trends and to limiting the sample to urban clusters and districts which ever have a WJC. In Table A54 of the Appendix, we also show that these results are driven by older and more educated women, which are the ones that are more likely to have better outside options. In addition, in Table A47 we present the impact of the WJCs on different types of emotional violence. In general, we find a negative but not statistically significant effect.

Central to the analysis on the mechanisms behind the results is the relationship between household decision-making and the WJC introduction. In order to test this, we also use the Peruvian DHS which records who has the final say on a variety of household decisions. For example, a woman is asked "*who makes the final decision on large household purchases?*" or "*who makes the final decision on money husband earns?*". Responses include: respondent only, jointly with partner and partner only. For these categories, we construct three measure of equal decision-making. The first one is a score that ranges from 0 to 6 and counts the number of times the respondent makes decision jointly with partner. The second one is also a score that ranges from 0 to 1 and counts the share of decisions made jointly with partner. The third one is a dummy that takes the value of 1 when at least one decision is made jointly with the partner. In addition to decision-making, we also estimate the effect of WJCs on women's earnings relative to their husbands.

Table A48 provides the estimates of the impact of WJCs on decision-making and bargaining power. We find suggestive evidence of an improvement in the bargaining power of women in the household. In particular, we find that women living near a WJC are more likely to make joint decision-making with their husband. The are also less likely to earn less than their husband and more likely to earn as much as their husband. We also

[25]The full sample of women in the Peruvian DHS surveys consists of 210.847 respondents aged 15 to 49 over the period 2000-2014. However, this sample is reduced to 121.404 eligible women since we only include partnered women who are eligible for the domestic violence module. When we run estimations using the geo-coded cluster locations during the period 2006-2014, this sample is reduced even further to 64.366 observations of women.

Table 4.4: The Effect of WJCs on Domestic Violence - (2006-2014)

Dep. variable	**Domestic Violence in last 12 months**			
Sample	All women	All women	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the cluster of residence				
WJC within 1km	-0.022**	-0.018*	-0.029***	-0.017
	(0.010)	(0.011)	(0.010)	(0.012)
Observations	64,363	64,363	38,395	27,996
Number of districts	1167	1167	485	215
Mean dep. var	0.390	0.390	0.399	0.397
Panel B: WJC in the district of residence				
WJC in district	-0.024**	-0.060***	-0.023*	-0.032*
	(0.011)	(0.020)	(0.014)	(0.018)
Observations	96,560	96,560	58,579	42,393
Number of districts	1293	1293	531	225
Mean dep. var	0.387	0.387	0.397	0.394
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

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

Notes: The dependent variable is a dummy indicating whether the women suffered any type of domestic violence (less severe, severe or sexual violence) during the last 12 months. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the women's cluster of residence and presence of WJC in the women's district. Robust standard errors (in parentheses) are clustered at the district level. The sample includes women between the ages of 15 and 49. Women who were never married or never cohabited are excluded from the sample. Covariates include age, age at first marriage, number of children, years of education, number of household members, number of households in the dwelling, marital status (married=1), rural residence dummy, district fixed-effects and province-by-year fixed effects. Source: Peru DHS 2006-2014.

analyze whether the WJCs have an effect on women's labor force participation. Results in Table A49 indicate that women's labor supply does not seem to be affected by the opening of these centers.

4.9 Conclusion

In this paper we argue that the opening of WJCs in Peru has a positive impact on children's human capital investment, and these impacts are concentrated in the very local areas around the WJC. To the best of our knowledge, this is the first quantitative analysis that attempts to explore the impact of an unexamined dimension of institutional intervention that provides better access to justice for women, namely the WJCs, on education outcomes and the prevalence of domestic violence.

We deal with the potential endogeneity in the WJC placement by exploiting the variation generated by the rollout of the women's justice centers in Peru. Basically, in order to ensure that our results are not driven by selection or time-varying unobservables, we use a difference in-differences strategy, which exploits variation created by the differential timing in the opening of the WJCs and also the spatial variation in the exposure of a school/household to a WJC, together with province-by-year fixed effects. We provide evidence in support of the identifying assumptions, and account for two key time-varying confounders: the fact that WJC introduction might anticipate changes in schooling and unobservable changes in variables that might affect both the timing of the WJCs and the education outcomes.

Our main finding is that investments in children's human capital are affected positively by the introduction of the WJCs. In particular, we find that introducing a WJC within 1km of a school causes an increase of 3% in the total number of children enrolled in primary school. In addition, we also find suggestive evidence that primary school second graders have better test scores in reading and mathematics. Moreover, we find that children in primary school living in householdâĂŹs located near a WJC are significantly more likely to attend school, to pass a grade and they are also significantly less likely to drop out of school. These effects are localized within a few kilometers and they are mostly driven by urban areas.

We also test whether this pattern of results could be caused by potential mechanisms. Lastly, we provide evidence that these improvements might be driven by a reduction in the prevalence of domestic violence and by an increase in the bargaining power of women inside the household. From a public policy standpoint, our analysis implies that providing better access to justice for women can be a powerful tool to reduce domestic violence and increase human capital investment of children, suggesting a positive intergenerational benefit of the program.

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Appendix

4.9.1 Coca Index based on Agro-ecological Conditions

Ecocrop

Search | Find plant | News | About

Find plant » Data sheet

Data sheet

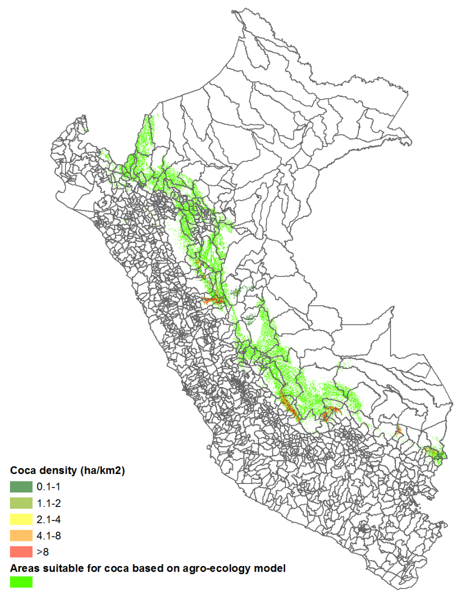
View crop Data sheet EcoPort

Erythroxylum coca

Description			
Life form	shrub, tree	Physiology	single stem, multi stem
Habit	erect	Category	medicinals & aromatic
Life span	perennial	Plant attributes	grown on small scale

Ecology							
	Optimal		Absolute			Optimal	Absolute
	Min	Max	Min	Max	Soil depth	deep (>>150 cm)	medium (50-150 cm)
Temperat. requir.	17	23	14	27	Soil texture	medium	heavy, medium, light
Rainfall (annual)	1000	2100	700	4000	Soil fertility	high	moderate
Latitude	-	-	40	40	Soil Al. tox		
Altitude	---	---	-	-	Soil salinity	low (<4 dS/m)	low (<4 dS/m)
Soil PH	5.5	6.5	4.3	8	Soil drainage	well (dry spells)	well (dry spells)
Light intensity	very bright	very bright	very bright	clear skies			

(a) Optimal ecological conditions for coca plants



(b) Coca suitability based on agro-ecological variables

Figure A1: Coca suitability across Peru

4.9.2 Additional Summary Statistics and Results

Table A1: Correlation between fraction of children and coca and coffee production 2012

	Coca production/total arable land	Coca production/total arable land	Coffee production/ total arable land	Coffee production/ total arable land
No.children/No. total labor in farm	.013	.026	-.015	-.015
	(.004)***	(.004)***	(.005)***	(.005)***
District fixed effects		✓		✓
Observations	11,086	11,086	11,086	11,086
R^2	0.005	0.136	0.01	0.113

Notes:Coefficients of the fraction of children amongst total labor per farm. Data for coca and coffee production are from 2012 Agriculture Census. Standard errors clustered at the district level. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Summary statistics of main variables

Variable	Unit	Obs	Mean	Std. Dev.
Panel level variables				
Child labor (6-14)	Individual	242,580	.283	.451
Adult labor (21-60)	Individual	522,887	.772	.412
Self-employment (21-60)	Individual	434,694	.438	.496
Population	District	11,785	19264.1	35790.9
Primary enrollment	School	434,309	112.032	178.54
Age for grade in primary	School	434,309	.199	.169
% Failed in primary	School	426,523	8.349	8.992
Secondary enrollment	School	136,083	225.475	317.544
Age for grade in secondary	School	136,083	.2305	.207
% Failed in secondary	School	132,342	6.19	7.624
Incarceration rate in non coca	District-cohort	40,196	.0034	.0205
Incarceration rate in coca district	District-cohort	40,196	.0055	.0146
District variables				
Coffee intensity, thousands of hectares, 1994	District	1441	.14	.821
Coca indicator, 1994	District	1441	.126	.332
Coca intensity, thousands of hectares, 1994	District	1441	.016	.014
Poverty index in coca districts, 2000	District	188	34.185	8.032
Poverty index in coffee districts, 2000	District	393	32.383	7.675
Population in coca districts, 2000	District	188	12970.64	20020.57
Population in coffee districts, 2000	District	393	12894.18	24279.97
Health posts, 2000 in coca districts, 2000	District	188	7.681	10.176
Health posts, 2000 in coffee districts, 2000	District	393	7.679	11.091
Classrooms, 2000 in coca districts, 2000	District	188	86.09	93.314
Classrooms, 2000 in coffee districts, 2000	District	393	82.97	109.759
% of coca villages affected by violence, 2000	District	188	1.356	3.986
% of coffee villages affected by violence,2000	District	393	1.225	3.404
Time-level variables				
Log international coffee price	Year	15	.959	.466
Log internal coca price	Year	15	1.06	.193
Eradicated coca hectares in Colombia, hundred thousands of hectares	Year	15	1.529	.467
Log coca hectares in Colombia, hundred thousands of hectares	Year	15	-.258	.304

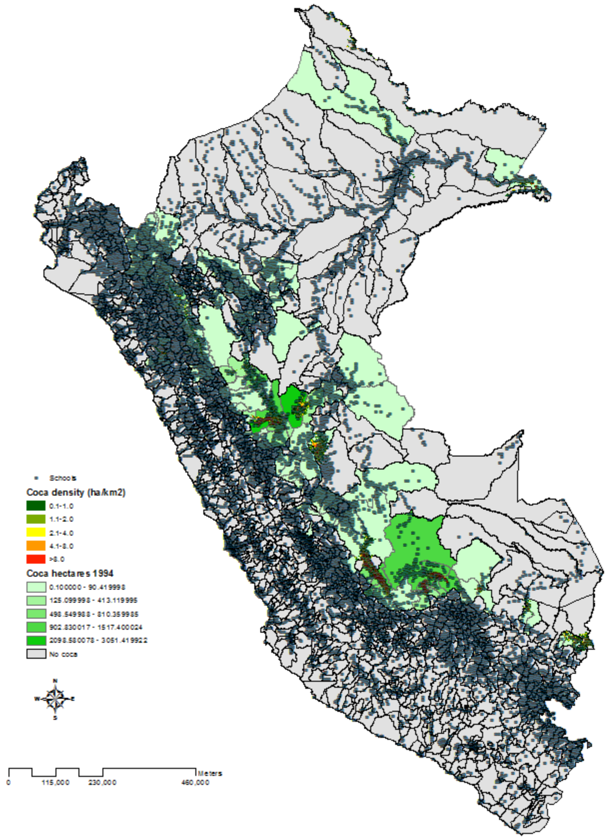


Figure A2: Schools distribution

Table A3: Baseline 1998 schools located inside and outside coca geographic cells

Variable	Outside	Inside	Diff.	$\|t\|$	$Pr(\|T\| > \|t\|)$
Primary schools					
No of students	74.895	70.782	-4.113	1.00	0.3153
Age for grade	0.327	0.335	0.009	1.32	0.1861
% Failed students	13.856	11.476	-2.381	5.63	0.000***
No of teachers with degree	5.542	5.022	-0.520	0.81	0.4168
Math average (national exam second grade)	474.850	477.140	2.290	0.50	0.6190
Reading average (national exam second grade)	439.839	446.952	7.113	1.80	0.0715*
Secondary schools					
No of students	147.093	107.276	-39.817	1.28	0.2015
Age for grade	0.466	0.519	0.053	1.86	0.0635*
% Failed students	8.840	6.879	-1.961	1.59	0.1120
No of teachers with degree	19.101	23.649	4.548	0.78	0.4371

Table A4: Baseline 1997 coca and coffee districts

Variable	Coffee	Coca	Diff.	$\|t\|$	$Pr(\|T\| > \|t\|)$
Household characteristics					
Water inside hh	0.392	0.571	0.180	1.87	0.065*
Public drain	0.293	0.300	0.008	0.64	0.5234
Rural	0.477	0.453	-0.024	-0.23	0.816
Number of rooms	2.909	2.838	-0.071	-0.42	0.672
Male	0.504	0.511	0.007	0.55	0.5843
Age	23.542	23.469	-0.073	0.15	0.8842
Self-employed	0.439	0.451	0.012	0.87	0.3867
Years of education of household head	3.797	3.657	-0.140	-1.22	0.228
Married	0.283	0.292	0.009	0.61	0.5441
Household size	6.320	5.907	-0.414	-1.67	0.101
Children characteristics					
Male	0.506	0.508	0.002	0.06	0.9533
Age	9.418	9.253	-0.165	1.31	0.1902
HH work	0.808	0.814	0.005	0.24	0.8066
Hh work hours	1.493	1.474	-0.018	0.28	0.7780
Self-employed	0.224	0.232	0.008	0.36	0.7157
Self-employed days	3.965	3.629	-0.336	1.31	0.1904
Self-employed hours	3.445	3.776	0.331	1.43	0.1524
Read and write	0.845	0.812	0.033	1.58	0.1139
School enrollment	0.970	0.947	-0.023	2.15	0.0315**
Years of schooling	2.144	2.145	0.001	0.01	0.9941

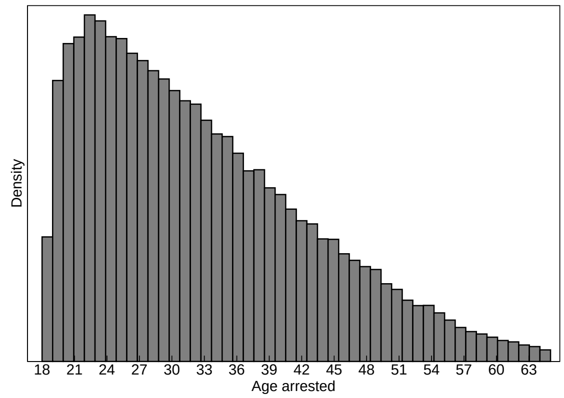
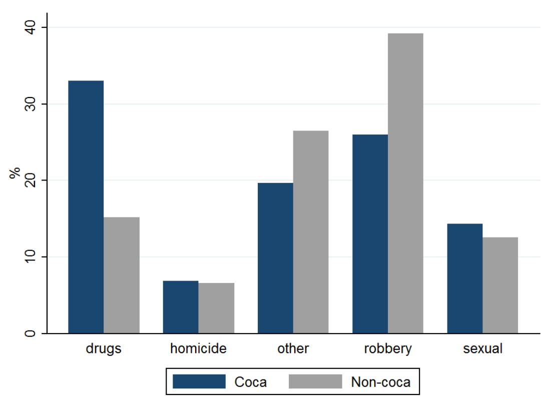
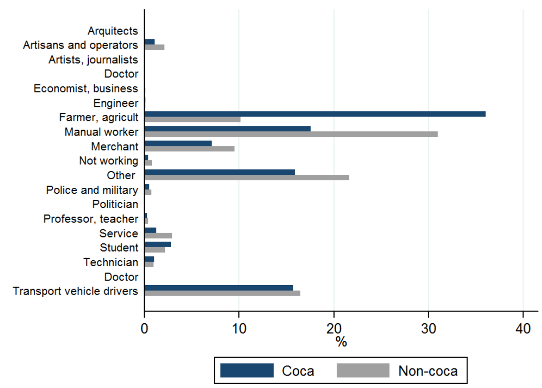


Figure A3: Age distribution of incarcerated individuals



(a) Type of crime



(b) Main occupation of incarcerated individuals

Figure A4: Incarceration data

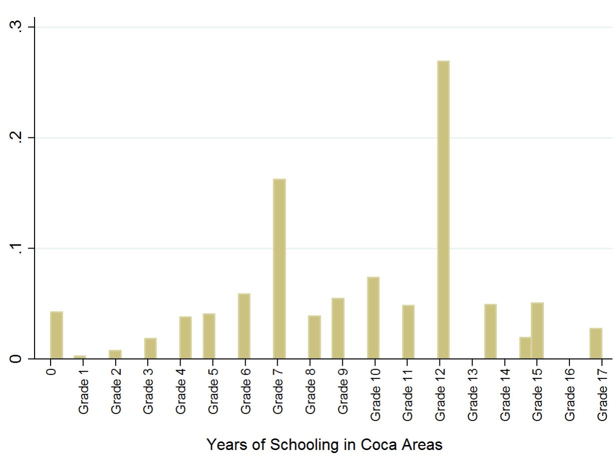


Figure A5: Histogram of educational attainment from 2007 Census

Table A5: Internal validity of the exclusion restriction

	(1) Taxes	(2) Income taxes	(3) Donations	(4) Income	(5) Canon	(6) Customs	(7) Special transfers	(8) Other transfers
$Coca_d \times CocaColombia_t$	-0.152	-0.181	0.005	0.182	4.382	0.024	0.181	0.524
	(0.138)	(0.136)	(0.010)	(0.114)	(3.826)	(0.016)	(0.239)	(0.479)
Observations	18,306	18,425	18,301	18,301	18,301	18,301	18,301	18,301
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: All variables are expressed in real values and come from the National Institute of Statistics (INEI). Canon corresponds to the tax income collected through natural resource exploitation. Transfers are made from the central to the local governments every year. Clustered standard errors at the district level are presented in parentheses. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A6: The effect of coca prices on child labor in Peru, by age bins

	(1)	(2)	(3)	(4)
Ages	6-10	11-14	15-18	19-21
$PriceShock_{d,t}$	0.104***	0.276***	0.243**	0.204
	(0.038)	(0.089)	(0.12)	(0.193)
Effects for avg. district	21%	35%	26%	18.5%
Observations	126,414	107,410	102,756	62,026
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Department trends	✓	✓	✓	✓
Covariates	✓	✓	✓	✓

Notes: Column (1) presents the results for children who are between 6 to 11 years old. Column (2) presents the results for those between 11-14 years old. Column (3) and (4) presents the results for those between 15-18 and 19-21 years old respectively. I separate the sample between the ages that students are in primary and secondary school. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Robustness checks, child labor

Panel A, Household Fixed Effects	
$PriceShock_{d,t} \times Age11x14_i$	0.073***
	(0.015)
Observations	161,746
Panel B, Coca Suitability Index	
$PriceShock_{d,t}$	0.309**
	(0.143)
Effects for avg. district	38%
Observations	233,753
Panel C, Controling for Violence	
$PriceShock_{d,t}$	0.163***
	(0.036)
Effects for avg. district	34%
Observations	233,727

Notes: All specifications control for district fixed effects, year fixed effects, department specific trends and covariates. Building upon the specification presented in Equation 3.1, Panel A adds a fully saturated model with interactions with *Age11x14*, a dummy indicating whether the individual is between 11 and 14 years old at the time of the survey, allowing for household fixed effects. Panel B defines $PriceShock_{d,t}$ using the coca suitability index instead of coca production in 1994. Panel C controls for trends by violence in 2000. Significant at *** p<0.01, ** p<0.05, * p<0.1

Table A8: Reduced form estimates: the effect of hectares in Colombia on child labor in Peru

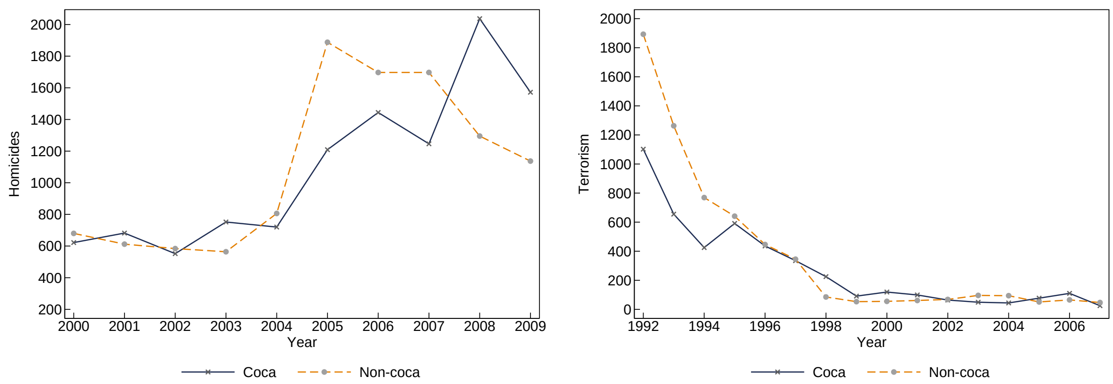
	6-14	6-14	6-14	6-10	11-14
$Coca_d \times CocaColombia_t$	-0.086***	-0.079***	-0.085***	-0.056*	-0.122*
	(0.022)	(0.020)	(0.024)	(0.028)	(0.060)
Observations	234,473	234,473	233,832	126,438	107,394
R-squared	0.295	0.295	0.323	0.318	0.327
District FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Department trends		✓	✓	✓	✓
Covariates			✓	✓	✓

Notes: Standard errors clustered at the district level are shown in parenthesis. Significant at *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Robustness checks, schooling outcomes

	(1)	(2)	(3)	(4)
	Failed	Age for grade	Grade 8	Grade 9
Panel A, Including Baseline and District Trends				
$PriceShock_{s,t}$	0.706***	0.019***	-0.095***	-0.085**
	(0.163)	(0.003)	(0.030)	(0.042)
Effect for avg school	25%	28.5%	28.5%	25.5%
Observations	379,824	386,248	111,846	111,846
Panel B, Including District-Year Fixed Effects				
$PriceShock_{s,t}$	0.033	0.010**	-0.115**	-0.051
	(0.286)	(0.004)	(0.045)	(0.062)
Effect for avg. school	0.8%	15%	33%	15%
Observations	424,638	432,421	128,790	128,790
Panel C, Coca Suitability Index				
$PriceShock_{s,t}$	3.534***	0.071***	-0.298**	-0.311*
	(0.687)	(0.013)	(0.129)	(0.185)
Effect for avg. school	41%	35%	30%	31%
Observations	425,862	433,634	135,593	135,593
Panel D, Coca 1994				
$PriceShock_{s,t}$	2.207***	0.044***	-0.203***	-0.171*
	(0.457)	(0.008)	(0.069)	(0.104
Observations	425,862	425,892	135,593	135,593
Panel E, Coffee Shock				
$PriceCoffee_t \times$ $Coffee\ int._d$	0.090	-0.001	0.033**	0.061***
	(0.117)	(0.002)	(0.016)	(0.021)
Observations	382,603	389,502	123,400	123,400

Notes: All specifications include school and year fixed effects, and department specific trends. Standard errors are clustered at school level. Significant *** p<0.01, ** p<0.05, * p<0.1.



(a) Number of homicides **(b)** Number of terrorist events

Figure A6: Is violence driving the schooling results?

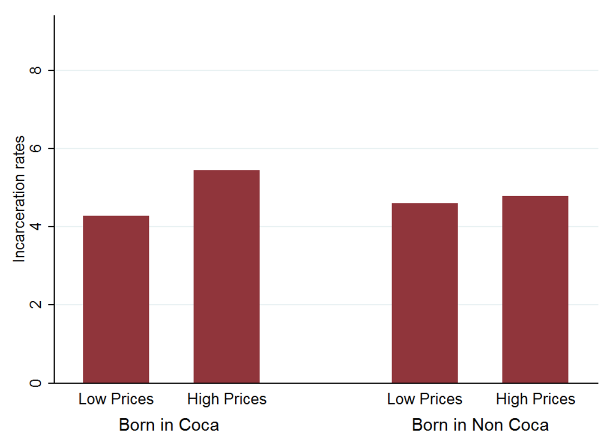


Figure A7: Differences in incarceration rates by period

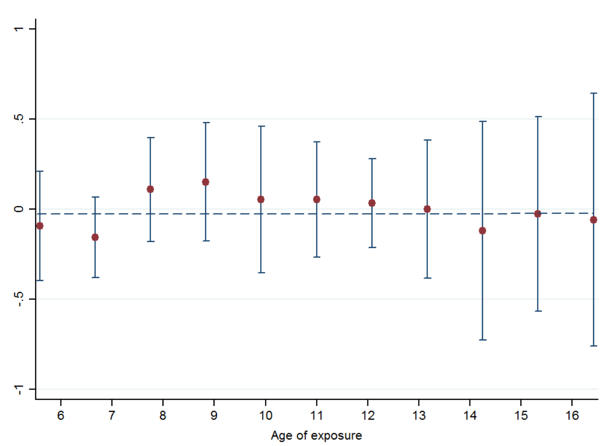


Figure A8: Effects on victims of homicides by age using police data

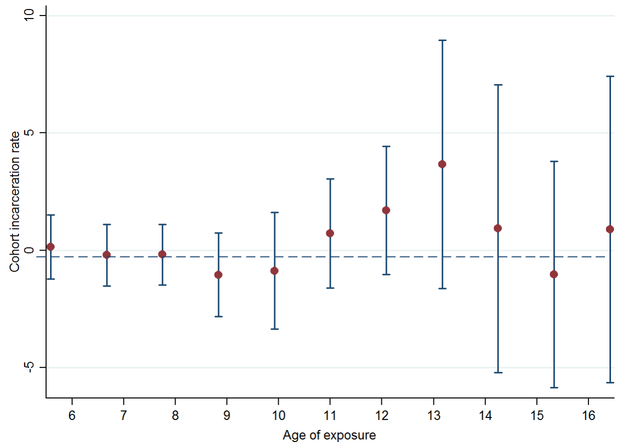


Figure A9: Incarceration rate effects by age using prisons located in non-coca districts

Table A10: Robustness checks, incarceration outcomes

	(1)	(2)	(3)
	All crimes	Drugs	Sentence length
Panel A, Including Department by Yob Fixed Effects			
PriceShock Age11to14$_{d,c}$	3.405***	2.160***	22.956
	(0.903)	(0.661)	(19.506)
Panel B, Including Year of Arrest Fixed Effects			
PriceShock Age11to14$_{d,c}$	0.129***	0.076***	-23.140
	(0.031)	(0.023)	(23.058)
Panel C, Instrumenting Prices			
PriceShock Age11to14$_{d,c}$	2.586***	1.482**	-24.282
	(0.654)	(0.645)	(46.932)
Panel D, Coca Suitability Index			
PriceShock Age11to14$_{d,c}$	2.217***	0.863**	19.897
	(0.597)	(0.337)	(13.284)
Panel E, Including District by Yob Fixed Effects [1]			
PriceShock Age11to14$_{v,c}$	0.006***	0.004***	
	(0.001)	(0.001)	
Panel F, Including Number of Victims During Civil Conflicts			
PriceShock Age11to14$_{d,c}$	2.888***	2.064***	23.433
	(1.061)	(0.691)	(19.021)
District FE	✓	✓	✓
Year FE	✓	✓	✓
Cohort FE	✓	✓	✓
District Trends	✓	✓	✓

Notes: *PriceShock Age11to14*$_{d,c}$ is the interaction of log average price of coca between 11 to 14 years old and coca suitability measure of the district or village of birth. [1] Panel E uses 2016 census to obtain information of the village of birth but does not have information on the length of the sentence. Standard errors clustered at the district of birth level. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Age at time of the coca boom and subsequent criminal behavior

	(1)	(2)	(3)	(4)
Coca boom Age6x7$_c$ × *Coca*$_{d,1994}$	0.097	0.231**	0.227*	
	(0.080)	(0.093)	(0.125)	
Coca boom Age8x9$_c$ × *Coca*$_{d,1994}$	0.419	0.578*	0.572	
	(0.296)	(0.306)	(0.369)	
Coca boom Age10x11$_c$ × *Coca*$_{d,1994}$	0.551***	0.737***	0.728**	0.484***
	(0.207)	(0.253)	(0.293)	(0.182)
Coca boom Age12x13$_c$ × *Coca*$_{d,1994}$	0.551***	0.762***	0.751**	0.532**
	(0.187)	(0.218)	(0.291)	(0.218)
Coca boom Age14x15$_c$ × *Coca*$_{d,1994}$		0.556**	0.543*	0.349
		(0.276)	(0.285)	(0.222)
Coca boom Age16$_c$ × *Coca*$_{d,1994}$			-0.034	-0.209
			(0.493)	(0.412)
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
District Trends	✓	✓	✓	✓

Table A12: Shocks to other commodities

Short-run effects on child labor					
Coca					
PriceShock$_{d,t}$ × *Legal*	0.195**				
	(0.096)				
Effect for avg. district	33%				
Coffee					
PriceCoff$_t$ × *Coff int.*$_{d,1994}$	0.011**				
	(0.004)				
Effect for avg. district	5%				
Gold					
PriceGold$_t$ × *Gold int.*$_{d,1975}$	0.019**				
	(0.008)				
Effect for avg. district	30%				
Long-term effects on incarceration					
	All	Drugs	Violent	Sexual	White collar
PriceCoff Age11x14 × *Coff int.*$_{d,1994}$	-0.414	0.047	-0.328**	0.091	-0.069**
	(0.260)	(0.058)	(0.165)	(0.217)	(0.033)
PriceGold Age11x14 × *Gold int.*$_{d,1975}$	-0.041	0.020	-0.052	0.010	0.000
	(0.121)	(0.033)	(0.084)	(0.030)	(0.005)

4.9.3 Does Coca Production Generate Large Income Effects?

Although coca production may have negative effects for children in the future, it is possible that present income gains may compensate the long-term negative effects. In this section, I show that the lifetime income for these individuals is negative.

To explore this, first I estimate whether income increases when coca price increases in the short-run. The household surveys record the total income and the earned income. I replace the dependent variable in Equation 3.1 for these income measures. Table A13 presents the results. Columns (1) and (2) present the results with the log of total income and log of earned income among adults. Columns (3) to (4) present the results for youth. All four coefficients are positive although only significant for earned income. Earnings increase by 10% for adults and 25% for youth. This implies that there are immediate income gains from the increase in coca prices. Moreover, the fact that I find effects only for youth is consistent with the story that coca prices increased wages of younger individuals.

Next, I explore how future income is affected for individuals who have high coca prices during childhood. I start first by analyzing whether children exposed to high prices at the ages of 6 to 14 experience lower income using household surveys from 2011 to 2014. Then, I check whether future income is particularly affected when children are exposed at a particular age.

Table A14 presents the results. Column (1) shows that cohorts affected by the coca boom when they are schooling age have lower income. The rest of the table provides robustness checks. For example, Columns (2) and (3) separates the sample by migration status and results are similar. Column (4) adds coca specific cohort trends. Given that some adult individuals who are 17 may still be at school, I check the robustness of the results by analyzing the effects on adults over the age of 18. Columns (5) and (6) show that results are robust to different sample sizes. Finally, in Column (7) I present the results with more dissaggreated bins. The coefficients are negative for all ages but stronger and significant for those between 11 and 14 years of age.

I also analyze whether the earnings results are driven by children who experienced high prices at specific ages during childhood. Table A15 presents the results with more dissaggreated bins. Although, the estimates lose some precision, most of the effects are concentrated among adults that experienced high coca prices at the age of 12. This is the age of most students in 7th grade, which is the grade where most dropouts occur.

Table A13: The effects of coca prices on present income

	(1) log income	(2) log earned income	(4) log income	(5) log earned income
$PriceShock_{d,t}$	0.204	0.286*	0.476*	0.603**
	(0.264)	(0.153)	(0.256)	(0.265)
Observations	340,560	166,449	128,322	89,456
R-squared	0.060	0.094	0.158	0.216
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Department trends	✓	✓	✓	✓
Covariates	✓	✓	✓	✓
Ages 21-60	✓	✓		
Ages 14-21			✓	✓

Notes: Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

Table A14: Coca prices during childhood and adult earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PriceShock Age6to14$_{d,c}$	-0.601**	-0.604*	-0.556	-0.487**	-0.554*	-0.461*	
	(0.244)	(0.339)	(0.593)	(0.240)	(0.285)	(0.276)	
PriceShock Age15to16$_{d,c}$	0.160	0.288	-0.021	0.148	0.118	0.113	
	(0.360)	(0.416)	(0.695)	(0.347)	(0.386)	(0.371)	
PriceShock Age6to10$_{d,c}$							-0.317
							(0.245)
PriceShock Age11to14$_{d,c}$							-0.791**
							(0.389)
Observations	64,300	37,199	27,101	64,300	56,209	56,209	56,209
R-squared	0.060	0.087	0.048	0.058	0.056	0.054	0.049
District FE	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓
Year of the survey FE	✓	✓	✓	✓	✓	✓	✓
Department trends	✓	✓	✓		✓		✓
Coca time trends				✓		✓	
Sample	All	Movers	Non Movers	All	All	All	All
Ages	16-30	16-30	16-30	16-30	18-30	18-30	18-30

Notes: The baseline specification is presented in Equation 3.2. Column (1) presents the results for the whole sample and includes controls for gender, age, department trends, cohort fixed effects and year of the survey fixed effect. Columns (2) and (3) present the estimates for the sample of movers and non movers. Column (4) includes coca specific time trends. Column (5) and (6) replicates Columns (1) and (4) for adults who are more than 18 years old at the time of the survey. Column (7) presents the results with more dissaggreated bins. Standard errors are clustered at the district level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A15: Coca prices during childhood and adult earnings

	(1)	(2)
$PriceShock\ Age6_{d,c}$	0.297	0.279
	(0.345)	(0.332)
$PriceShock\ Age7_{d,c}$	-0.081	-0.044
	(0.384)	(0.372)
$PriceShock\ Age8_{d,c}$	-0.310	-0.277
	(0.265)	(0.251)
$PriceShock\ Age9_{d,c}$	0.522*	0.497
	(0.306)	(0.309)
$PriceShock\ Age10_{d,c}$	-0.405	-0.441
	(0.527)	(0.492)
$PriceShock\ Age11_{d,c}$	0.251	0.325
	(0.601)	(0.560)
$PriceShock\ Age12_{d,c}$	-0.557***	-0.595***
	(0.172)	(0.203)
$PriceShock\ Age13_{d,c}$	0.702	0.744
	(0.605)	(0.587)
$PriceShock\ Age14_{d,c}$	-0.625	-0.547
	(0.464)	(0.442)
$PriceShock\ Age15_{d,c}$	0.958	0.789
	(0.698)	(0.609)
$PriceShock\ Age16_{d,c}$	-0.915	-0.961
	(1.144)	(1.081)
Observations	56,203	56,203
R-squared	0.050	0.056
District FE	✓	✓
Cohort FE	✓	✓
Department time trends	✓	✓
Year of the survey FE	✓	✓
Department*year FE		✓
Coca time trends		✓
Ages	18-30	18-30

Notes: The specification is presented in Equation 3.2. Standard errors clustered at the district level.*** p<0.01, ** p<0.05, * p<0.1.

Table A16 addresses some concerns such as under-reporting of income and I find no evidence that affected cohorts are reporting no or lower income. Column (1) uses an indicator for whether the individual has completed high school as the dependent variable. I use this specification to test whether using pooled household surveys leads to an accurate measure of the long-term effect. I find that the probability is increased by 30% for affected cohorts which is similar to the results found using school census data in Section 1.5.2.2.

Another potential concern is that income may be under-reported for those individuals who are more likely to be in the illegal sector. I test this by estimating the same model but replacing the dependent variable by the probability of reporting a low or no income. Columns (3) and (4) report the results and there is no evidence that affected cohorts have incentives to under-report.

Finally, I examine whether these results are driven by the reduction in education. I recover the returns to schooling by dividing my earnings estimates by the schooling estimates. If I expect students who stay in school after age 12 to get 4.2 more years of education on average, this implies the return to an extra year of education is about 18%.

Table A16: Coca prices during childhood and adult earnings

	(1) High School drop out	(2) High School drop out	(3) Missing earnings(=1)	(4) Low earnings(=1)
PriceShock Age6to14$_{d,c}$	0.110**	0.105*	-0.042	0.037
	(0.054)	(0.055)	(0.098)	(0.035)
PriceShock Age15to16$_{d,c}$	-0.024	-0.027	0.083	0.008
	(0.043)	(0.042)	(0.058)	(0.051)
Observations	56,203	56,203	56,203	56,203
R-squared	0.037	0.038	0.078	0.050
District FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Department trends	✓			
Coca time trends		✓	✓	✓
Sample	All	All	All	All
Ages	18-30	18-30	18-30	18-30

Overall, these results suggest that individuals affected by coca prices during childhood are experiencing higher earnings in the short term at expenses of lower earnings when they are adults. Using these estimates and assuming that each household has a discount rate of 0.95, I calculate the present discounted life income of households. I calculate that individuals have present income gains for 4 years which is the average number of years individuals in my sample were exposed. I find that the present discounted income of households is negative.[26] In the next section, using a simple framework I discuss how these results are consistent with a model of parental incentives.

[26]Note that in Table A5 I also rule out the existence of income gains coming from local development at the district level.

4.9.3.1 Theoretical Framework to Understand the Role of Parental Incentives

In this section, I provide a model in which parents are not able to internalize the costs of child labor. This could be because they are credit constraint or present low inter-generational altruism or misguided beliefs about schooling as well as they may discount the future more heavily. However, I show that once the social planner introduces a transfer to parents with the condition that children have to attend school, all the negative effects are mitigated even if coca prices are high.

I build on qualitative evidence from my field work suggesting that many parents do not internalize the future costs of children working in coca farms. In particular, parents do not believe that school is a better investment for children than working on coca farms. From about 300 farmers, 65% of coca farmers reported that school is not more important for their children future than working in coca farming (I describe more about this survey in Appendix 4.9.6).

I present a two-period model in which parents choose whether their children work or go to school.[27] In the first period, parents receive income if their child works growing coca. In the second period, children are grown and returns to schooling are realized. Using this simple framework, I examine the effect of a conditional cash transfer that incentivizes schooling.

In particular, when parents put children to work in coca fields they get the following utility:

$$U_i^l = U(I_i + P_t h_c) + \beta\delta_i U(H - h_c)$$

If parents decide to invest in their children's schooling:

$$U_i^s = U(I_i) + \beta\delta_i U(H)$$

In the first period, parents have exogenous income I. I assume I is distributed with CDF $F(I)$ and positive support over $[\underline{I}, \bar{I}]$. In addition, parents receive additional income from coca that depends on the price of coca, P_t, and number of hours children work, h_c . If children do not work, then they attend school and parents receive no additional income.

In the second period, children are grown and have earnings that depend on the amount of human capital accumulated in the first period. If children worked in the first period, they receive earnings $H - h_c$, where H is the maximum amount of human capital. Therefore, if they do not work in the first period, earnings are H.

Parents have discount rate δ_i, which is distributed with CDF $G(\delta_i)$ and positive support over

[27] In this model, I study the effects of prices and transfer on the extensive margin of child labor. This is mainly because in my data I do not have access to number of hours worked by children. A potential concern is that I am not being able to identify income effects (whether when prices increase, households reduce the number of hours children are working). While previous literature has found mixed effects, in the coca areas it is more likely for substitution effects to operate. This is mainly because children are an important input in the production process. Moreover, if income effects were dominating, schooling outcomes would improve and I find no evidence of this.

$[\underline{\delta}, \overline{\delta}]$. In addition, β is the degree to which parents internalize the future utility of their children. It can also be a measure of inter-generational altruism.[28] Note that $\beta = 1$ if their children's earnings are fully internalized. If parents place less weight on their children's utility or do not believe working will effect their children's future utility, then $\beta < 1$.

Parents will decide to invest in schooling if:

$$U_i^s > U_i^l$$

$$U(I) + \beta\delta U(H) > U(I + P_t h_c) + \beta\delta U(H - h_c)$$

$$\delta > \delta^* = \frac{U(I + P_t h_c) - U(I)}{\beta[U(H) - U(H - h_c)]}$$

Note that δ^* is defined as the threshold and households with $\delta < \delta^*$ children will work. $G(\delta^*)$ is the proportion of parents that are doing child labor.

When P_t increases,

$$\frac{\partial \delta^*}{\partial P_t} = \frac{U'(I + P_t h_c) h_c}{\beta[U(H) - U(H - h_c)]} > 0$$

The threshold increases $\delta^*(P') > \delta^*(P)$ when $P' > P$ and a larger proportion of parents choose child labor. The opposite happens when exogenous income I increases.

When $\beta < 1$, the equilibrium outcome is inefficient since parents do not fully internalize the cost of child labor. As β decreases, a larger proportion of parents will choose child labor, increasing the inefficiency.

The social planner fully internalizes the cost of child labor, setting $\beta = 1$. Therefore, the δ^{SP} threshold set by the social planner is

$$\delta^{SP} = \frac{U(I + P_t h_c) - U(I)}{U(H) - U(H - h_c)}$$

When $\beta < 1$, this is lower than δ^*.

Now if we add a transfer conditional on schooling, T, parents will decide to invest in schooling if:

[28] Also it could be that parents may discount the future more heavy than the children or that interest of parents are not so well align with interest of children. In addition, β could also represent some measure of present biased beliefs. It is possible that parents decisions hold persistently misguided beliefs about either the nature of the process of investments in child education or the subsequent returns to these investments. For instance, parents may believe that earnings respond to education less elastically than they actually do. In addition, β can be define by $\frac{1}{1+r}$ and we could think that parents who are credit constraint face a high interest rate.

$$U(I+T)+\beta\delta U(H) > U(I+P_t h_c)+\beta\delta U(H-h_c)$$

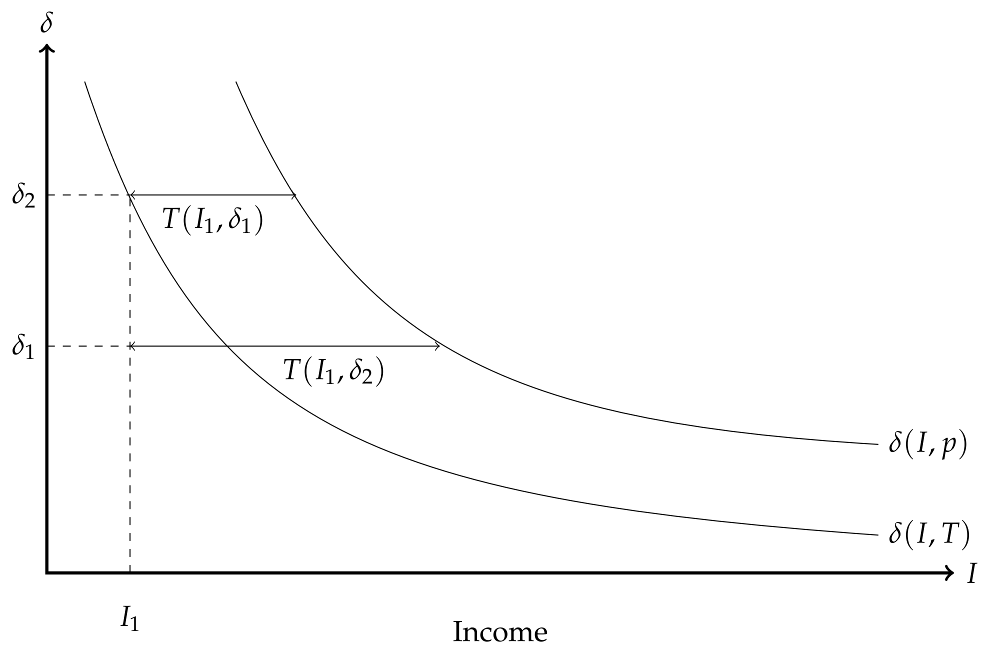
Then the threshold for child labor becomes:

$$\delta^*(T) = \frac{U(I+P_t h_c) - U(I+T)}{\beta[U(H)-U(H-h_c)]}$$

When T increases,

$$\frac{\partial \delta^*(T)}{\partial T} = \frac{-U'(I+T)}{\beta[U(H)-U(H-h_c)]} < 0$$

the threshold decreases ($\delta^{P'} > \delta^{P} > \delta^{T}$) moving the indifference curve to the left. In this way, the transfer can compensate for an increase in coca prices and correct the market failure by reducing the proportion of parents choosing child labor (the type of parents between $\delta(I,p)$ and $\delta(I,T)$).



Prediction 1: a conditional cash transfer will induce some parents to not use child labor even when coca prices are high.

Moreover, at higher prices a larger transfer is needed to mitigate the effects. To see this result note that the the social planner chooses T^* such that the threshold under $\beta = 1$ is equal to the threshold with transfer:

$$\frac{U(I+P_t h_c) - U(I)}{\beta[U(H)-U(H-h_c)]} = \frac{U(I+P_t h_c) - U(I+T)}{[U(H)-U(H-h_c)]}$$

$$U(I+T^*) = (1-\beta)U(I+P_t h_c) + \beta U(I)$$

Using implicit function theorem, I calculate $\frac{\partial T^*}{\partial P}$:

$$\frac{\partial T^*}{\partial P} = -\frac{(1-\beta)U'(I + P_t h_c)h_c}{U'(I+T)} > 0$$

Similarly, when parents do not internalize the cost of child labor and have a low β, a larger transfer is needed to compensate for high prices:

$$\frac{\partial T^*}{\partial \beta} = -\frac{U'(I + P_t h_c)h_c}{U'(I+T)} > 0$$

Even though I did not formally introduce a parameter measuring the land suitability for coca, it is clear that the effect is similar to an increase in coca prices and a larger transfer may be needed to get to the social planner solution when districts are more suitable for coca.

Prediction 2: when coca price or coca suitability increases, a larger transfer is needed to incentivize parents to not do child labor. [29]

4.9.4 Additional Results, CCTs

Table A17: CCTs and coca price shocks on schooling

	(1) Failed	(2) Age for grade	(3) Grade 8	(4) Grade 9
$PriceShock_{d,t}$	0.600**	0.023***	-0.162***	-0.180***
	(0.238)	(0.005)	(0.038)	(0.061)
$PriceShock_{d,t} \times CCT_{d,t}$	-0.381***	-0.001	0.042**	0.063**
	(0.114)	(0.002)	(0.020)	(0.025)
Observations	424,096	431,790	135,123	135,123

Notes: This table presents the estimates of a fully saturated model of Equation 4.1 with interactions with $CCT_{d,t}$, a dummy indicating whether the district d has a CCT in year t and 0 otherwise. Results are robust to the inclusion of trends by the index for which they selected districts and differential trends by the stage of treated. Standard errors clustered at the district level are shown in parenthesis. Significant at *** p<0.01, ** p<0.05, * p<0.1.

[29] The model could also be extended by assuming that coca production also leads to an increase in crime, generating an additional externality. In this case, parents do not only internalize the cost of future crime in addition to the future effect on children's earnings. This model would have similar implications.

Table A18: CCTs and coca price shocks on incarceration, drug-related crimes

	Affected Cohorts	Unaffected Cohorts
$PriceShock\ Age11to14_{d,c}$	2.003***	
	(0.896)	
$PriceShock\ Age11to14_{d,c} \times CCTs\ Age11to14$	-0.355*	
	(0.200)	
$PriceShock\ Age15to16_{d,c}$		-0.073
		(1.488)
$PriceShock\ Age15to16_{d,c} \times CCTs\ Age15to16$		0.141
		(0.204)

Table A19: CCTs and coca price shocks on adult labor

	(1) Hours worked	(2) Hours worked	(3) Self-employment	(4) Self-employment
$PriceShock_{s,t}$	-4.803**	-8.183**	0.052	-0.010
	(1.951)	(3.428)	(0.054)	(0.045)
$PriceShock_{s,t} \times CCT_{d,t}$		3.564***		0.031
		(1.097)		(0.027)
Observations	430,574	430,574	522,409	522,409
R-squared	0.038	0.038	0.087	0.087
Number of District	1,440	1,440	1,440	1,440
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Department trends	✓	✓	✓	✓
Covariates	✓	✓	✓	✓
Ages 21-60	✓	✓	✓	✓

4.9.5 Spillover Effects

Table A20: CCTs and coca production 2010-2014, spillovers in neighboring districts

	(1)	(2)
$Coca_d \times Neighbor\ CCT_{d,t}$	248.057***	251.664***
	(69.540)	(75.463)
Observations	3,455	688
Mean coca	671	671
District FE	✓	✓
Year FE	✓	✓
Valley Trends	✓	✓
Sub-sample of coca neighbors		✓

Notes: This table presents the estimates of Equation 1.7, a fully saturated model of Equation 3.1 with interactions with $NeighborCCT_{d,t}$, a dummy indicating whether the neighboring district d has a CCT in year t and 0 otherwise. The dependent variable is the number of coca hectares between 2010 and 2014. Results are robust to the inclusion of trends by the index for which they selected districts and differential trends by the stage of treated. Standard errors clustered at the district level are shown in parenthesis. Significant at *** p<0.01, ** p<0.05, * p<0.1.

4.9.6 Qualitative Data Appendix

This research project developed from a set of interviews that I conducted during the months of April and July of 2015 to understand what factors influence child labor decisions in coca areas in Peru. In this Appendix, I describe the interview methodology.

I conducted structured interviews in 11 districts in Peru: Monzon, Rupa-Rupa, Daniel Alomia Robles, Mariano Damaso Beraun, Jose Crespo y Castillo, Tocache, Pichari, San Francisco, Llochegua, Otari, and Catarata. These districts are part of two main cocaine basins: Huallaga and the valleys of the rivers Apurimac, Ene and Mantaro (VRAEM). The total population in these areas is about 1.1 millon. Both regions have about 30 narcotraffic organizations with a production of between 200 and 500 kg of cocaine per month. Research participants were drawn from an arbitrary and convenient sample found in fields: households (parents and children), farmers, schools (principals, teachers, and children), NGOs, and government officials. In the VRAEM regions, the sample included 50 private individuals (farmers, fieldworkers, parents, children), 10 government officials, and 10 school officials. The research participants in the Huallaga area were 32 private citizens and 4 school officials. In addition to the interviews, I helped designing a survey given to 270 farmers in the district of Mariano Damaso Beraun. These farmers were part of an Alternative Development Project to substitute coca production for coffee and cacao. The survey included questions about production, earnings from coca and other activities, and beliefs about education.

Interview lengths ranged from 30 minutes to two hours. Participants answered questions about working and schooling decisions. The interviews were usually conducted in participants' homes, offices, classrooms, and farms. For children, the questions included: whether they attended school, whether they liked school and why, what they wished was different about school, what were the reasons they did or did not attend school, what were their main activities after school, and what were the main difficulties they faced finishing their studies. For parents and farmers, the questions included whether children attend school and why, whether they used child labor, farming decisions such as how and when they decided to grow coca or other crops, whether prices influenced their choices, and main sources of employment (family, friends, external labor, etc.). For school teachers and headmasters, the questions included why they think children do not attend school, their satisfaction in their jobs, and whether children dedicated time to activities related to drug production. For government officials, the questions included: what they thought were the main problems in drug producing areas, why they think children did or did not attend school, and their understanding of the local environment.

I also conducted less-structured interviews in which I embedded myself in the community. I also accompanied government officials on the Alternative Development in Mariano Damaso Beraun project, in which ex-coca farmers traveled to villages to teach skills and encourage farmers to grow legal crops. This included living with local families during the visits and understanding their production and labor decisions.

Table A21: Mechanisms Behind the Schooling Effects

	(1) Years of education	(2) Years of education	(3) Years of education	(4) Enroll-ment in secondary	(5) Enroll-ment in secondary	(6) Enroll-ment in secondary	(7) Invest-ments on construc-tion	(8) Province Earnings	(9) Number of hotel rooms
FTZ× age6to16	0.259**	0.262**	0.236**	0.030**	0.028**	0.021*	-125.751	92.143	-1.164
	(0.122)	(0.122)	(0.103)	(0.012)	(0.013)	(0.010)	(86.245)	(494.052)	(1.842)
N^o parks	0.085			0.005					
	(0.057)			(0.004)					
N^o construction permits		-0.001			-0.000*				
		(0.001)			(0.000)				
N	46,026	46,026	29,808	49,716	46,067	29,831	437	228	456
Subsample of non-working women			YES			YES			

Notes: each cell represents a separate regression. Other covariates control for household and province characteristics. The control group consists of women between 16 to 30 years of age. In this table I show that the effects on schooling is driven by other mechanism apart from an increase in women's earnings and school infrastructure. Standard errors are corrected for clustering at the province level. Significant at *** p<0.01, ** p<0.05, * p<0.1

Table A22: Education Based on Sex and Sectors (%)

	FTZ		Tourism		Agro-industry	
	Men	Women	Men	Women	Men	Women
No education	2.3	3.4	2.1	1.0	4.2	1.2
Incomplete primary	10.4	7.3	7.9	12.9	17.8	14.3
Complete primary	8.1	7.8	12.5	2	17.8	14.3
Incomplete secondary	23.1	18.9	25	15.8	23.2	16.7
Complete secondary	23.1	16.6	20	23.8	11.3	9.5
Tecn. secondary	2.7	5.3	3.8	7.9	4.2	10.7
Tecn. university	5	8.7	5.4	7.9	3.1	8.3
Incomplete university	16.7	15.5	14.5	19.8	10.5	19
Complete university	14.9	16	8.8	8.9	8.5	9.5
Other	0.1	0.6	0	0	0.8	6

Source: Reyes Castro et al. (1993) based on Encuesta Nacional de Mano de Obra (ENMO'91). BID-FUNDAPEC.

Table A25: Women Empowerment and Female Factory Jobs

	(1) Final say	(2) Violence	(3) Spend	(4) Earns more
FTZ×age6to16	-0.067	-0.013**	-0.0148	0.093***
	(0.228)	(0.0047)	(0.018)	(0.032)
Mean of dependent	0.511	0.08	0.796	0.28
N	9,695	32,760	12,854	4,612
R^2	0.025	0.037	0.174	0.035

Notes: dependent variable in column (1) indicates whether the respondent has the final say in households decisions. Dependent variable in column (2) indicates whether the respondent justifies domestic violence. Dependent variable in column (3) is a dummy that indicates whether the respondent decides how to spend money. Dependent variable in column (4) indicates whether the respondent earns more than her husband. Women empowerment variables are only available in 2002 and 2007 surveys. Standard errors are corrected for clustering at the province level. Significant at *** p<0.01, ** p<0.05, * p<0.1

Table A23: Descriptive Statistics, 1986-2007

Variable	Obs	Mean	Std. Dev.	Min	Max
Age of Respondent	55,956	29.15	9.88	15	49
Women Years of Education	55,894	7.83	4.38	0	22
Number of HH Members	55,956	5.26	2.35	1	22
Current Marital Status	55,955	0.76	0.42	0	1
Age of Marriage	42,784	17.94	4.1	8	47
Age at First Intercourse	33,190	17.31	3.66	8	46
Age at First Birth	39,711	20.00	4.08	10	46
Working (=1)	55,850	0.36	0.48	0	1
Ever attended only primary school (=1)	30,445	0.9	0.3	0	1
Ever attended only secondary school (=1)	55,953	0.46	0.5	0	1
Complete only primary school (=1)	42,490	0.40	0.49	0	1
Complete only secondary school (=1)	55,959	0.24	0.43	0	1
Age of Husband*	27,095	38.14	10.85	15	95
Husband Years of Education	38,269	7.28	4.68	0	20

*only available for 1996, 2002, 2007 surveys

Table A24: Predictors of FTZ's Openings 1986

(A) Demographic Characteristics	
Proportion of Women in Age 15-21	5.098
	(11.977)
Proportion of Women in Age 22-30	-2.819
	(11.69)
Proportion of Women in Age 31-45	7.131
	(11.005)
Proportion of Households in Urban Areas	-1.342
	(1.763)
Proportion of Owners of Land Worked	-.351
	(2.644)
Port (=1)	2.145
	(1.682)
R-squared	0.023
(B) Social Characteristics	
Average Years of Education for Women	-0.681
	(0.805)
Proportion of Literated Women	0.671
	(6.890)
Average Years of Education for Men	1.888
	(5.890)
Average Age of First Marriage	2.369
	(2.493)
Average Age of First Birth	0.967
	(0.979)
Proportion of Married Women	7.296
	(5.897)
Average Age of First Intercourse	-3.681
	(2.924)
R-squared	0.100
(C) Labor Characteristics	
Proportion of Women Earning a Salary	0.344
	(2.783)
Proportion of Women Working for a Non-Family Member	-2.201
	(2.686)
Proportion of Women Working Before Marriage	2.319
	(5.628)
R-squared	0.03
Observations	107

Notes: the dependent variable is the year in which the FTZ opened in each province minus 1986, the year of the beginning of greatest expansion. Results from including all regressors variables in a single regression do not change. Robust standard errors are reported in parenthesis.

Table A26: Schooling, Marriage and Female Factory Jobs (Unaffected Women)

	(1) Years of education	(2) Enrollment in secondary	(3) Complete secondary	(4) Age of marriage	(5) Early marriage
FTZ	-0.145	-0.020	0.004	0.203	-0.002
	(0.228)	(0.020)	(0.016)	(0.165)	(0.025)
N	22,709	22,735	22,737	20,867	20,867
R^2	0.073	0.053	0.043	0.112	0.082
Province FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Province trends	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES

Notes: each cell represents a separate regression. I estimate the effect of FTZs on a group of women who should not be affected: women who were already married by the time of the opening. Other covariates control for household and province characteristics. Standard errors are corrected for clustering at the province level. Significant at *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A27: Schooling, Marriage and Female Factory Jobs (Excluding Main Cities)

	(1) Years of education	(2) Enrollment in secondary	(3) Complete secondary	(4) Age of marriage	(5) Early marriage
FTZ	0.436*	0.046**	0.037**	1.039***	-0.073***
	(0.211)	(0.017)	(0.013)	(0.168)	(0.014)
N	51,949	51,991	51,993	24,905	33,829
R^2	0.188	0.154	0.118	0.150	0.085
Province FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Province trends	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES
Province year of birth trends	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES

Notes: each cell represents a separate regression. I estimate the model in Equation 2.1 but excluding from the analysis provinces that contain the main cities such as the National District, Santo Domingo and Santiago. Standard errors are corrected for clustering at the province level. Significant at *** p<0.01, ** p<0.05, * p<0.1

Results Using Other Surveys

In order to check the robustness of the results I rely on other household surveys assembled by the Inter-American Development Bank. These surveys cover the period 2000-2011 and are part of the Program for the Improvement of Surveys and the Measurement of Living Conditions (MECOVI), sponsored by the Inter-American Development Bank, the UN Economic Commission for Latin America and the Caribbean, and the World Bank. The variables I use to measure education outcomes are similar to the ones used in the DHS: reported number years of schooling, last type of studies pursue and school enrollment. With these variables, I construct enrollment and attainment measures that are age-specific. This data has some disadvantages. First, similar as DHS they are self-declared reports and households could over-estimate the years of schooling. However, Urquiola (2006) notice that estimates coming from these surveys in general are smaller than from official statistics. Second, these surveys only contain information for my schooling outcomes.

Table A28: Schooling and Female Factory Jobs (Using IDB Surveys)

	(1)	(2)
	Years of education	Years of education
FTZ×age6to16	0.229*	0.249*
	(0.123)	(0.125)
Household income		5.81e-05***
		(1.85e-06)
N	110,968	110,706
R^2	0.394	0.425

Notes: each cell represents a separate regression. In this table I show that the effect of FTZs on schooling is robust to the use of different surveys and it is also driven by other mechanism apart from an increase in earnings. Standard errors are corrected for clustering at the province level. Significant at *** p<0.01, ** p<0.05, * p<0.1

Table A29: Migration and Female Factory Jobs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Years of education	Years of education	Years of education	Years of education	Age of marriage	Age of marriage	Age of marriage	Age of marriage
FTZ	0.423**	0.385***	0.488***	0.350**	1.323***	1.276***	1.332***	1.337***
	(0.164)	(0.127)	(0.160)	(0.127)	(0.209)	(0.247)	(0.224)	(0.245)
Movers				-0.761***				-0.330***
				(0.108)				(0.0855)
Mean of dependent	7,82	7,82	7,82	7,82	17.94	17.94	17.94	17.94
N	41,985	54,778	40,869	55,894	17,732	25,714	17,506	25,940
R^2	0.157	0.125	0.159	0.131	0.039	0.026	0.038	0.0276
Non-migrants	YES		YES		YES		YES	
Without just movers		YES	YES			YES	YES	

Notes: columns (1) and (5) presents estimates using only the subsample of non-migrants. Columns (2) and (6) eliminates from the whole sample those who moved to the area just before the FTZ opened. Columns (4) and (8) adds a dummy that indicates whether the household moved before the FTZ opened. Standard errors are corrected for clustering at the province level. Significant at *** p<0.01, ** p<0.05, * p<0.1

Table A30: Schooling, Marriage and Female Factory Jobs (Including Household Fixed Effects)

	(1) Years of education	(2) Enrollment in secondary	(3) Complete secondary	(4) Age of marriage	(5) Early marriage
FTZ	0.609**	0.067*	0.0715*	1.428**	-0.091**
	(0.276)	(0.036)	(0.039)	(0.726)	(0.038)
N	15,890	14,667	14,648	9,971	14,668
R^2	0.795	0.773	0.737	0.822	0.706
Province FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Province year of birth trends	YES	YES	YES	YES	YES
Relationship	YES	YES	YES	YES	YES
Household FE	YES	YES	YES	YES	YES
Age	YES	YES	YES	YES	YES

Notes: in columns (1)-(3) there are approximately 7,000 households with more than one women inside and in columns (4)-(5), 5,000 households. Standard errors are corrected for clustering at the province birth level. Significant at *** p<0.01, ** p<0.05, * p<0.1

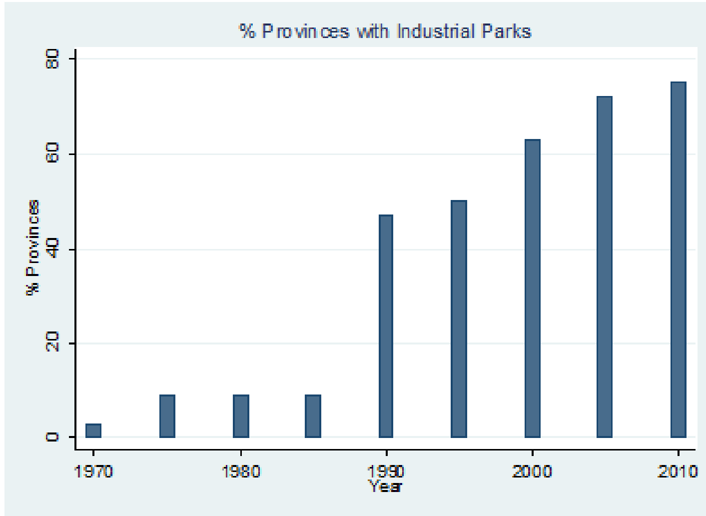


Figure A1: Provinces with FTZ between 1970-2010

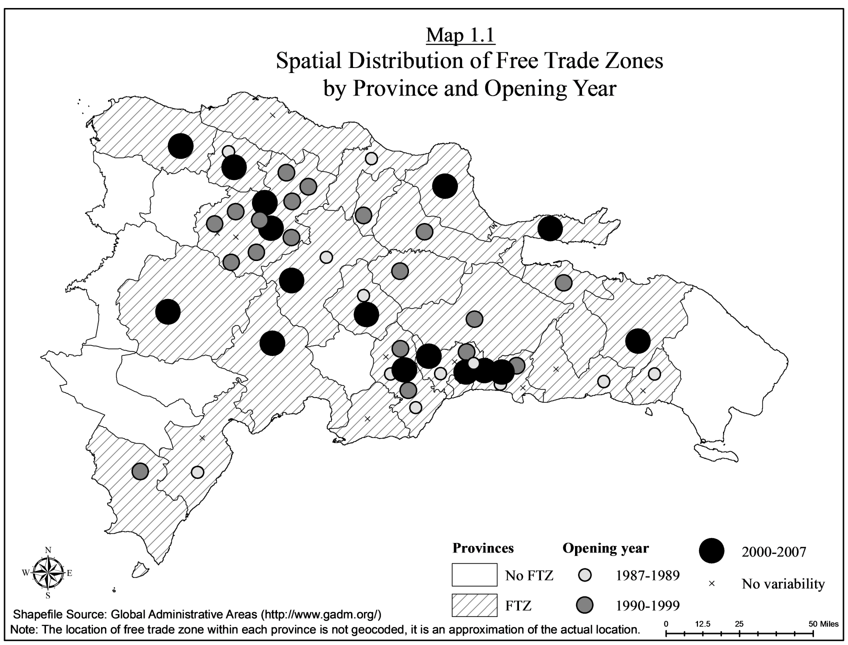


Figure A2: FTZs Distribution between 1970-2010

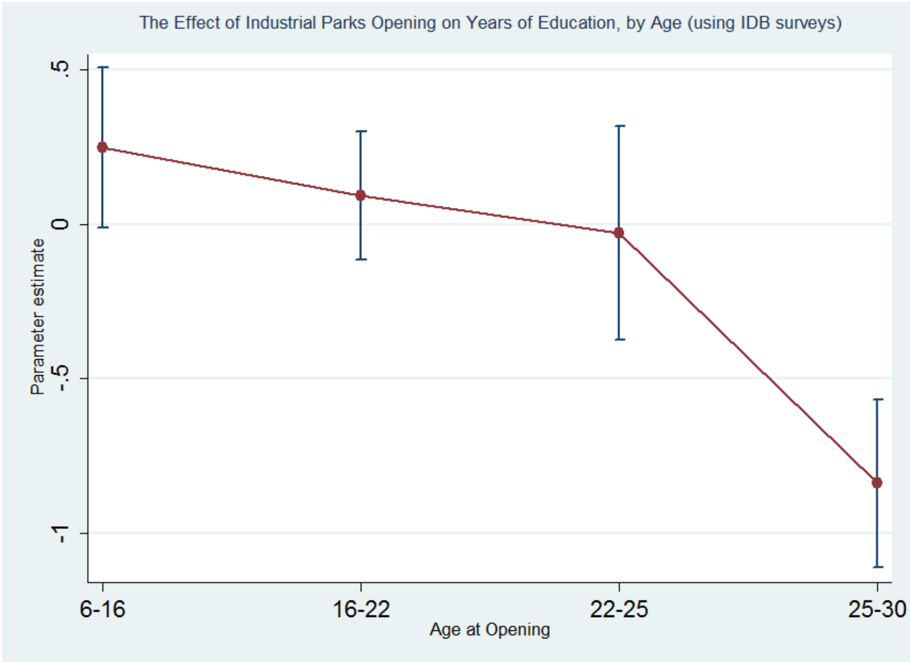


Figure A3: Educational Attainment by Age at Opening (Using IDB Surveys)

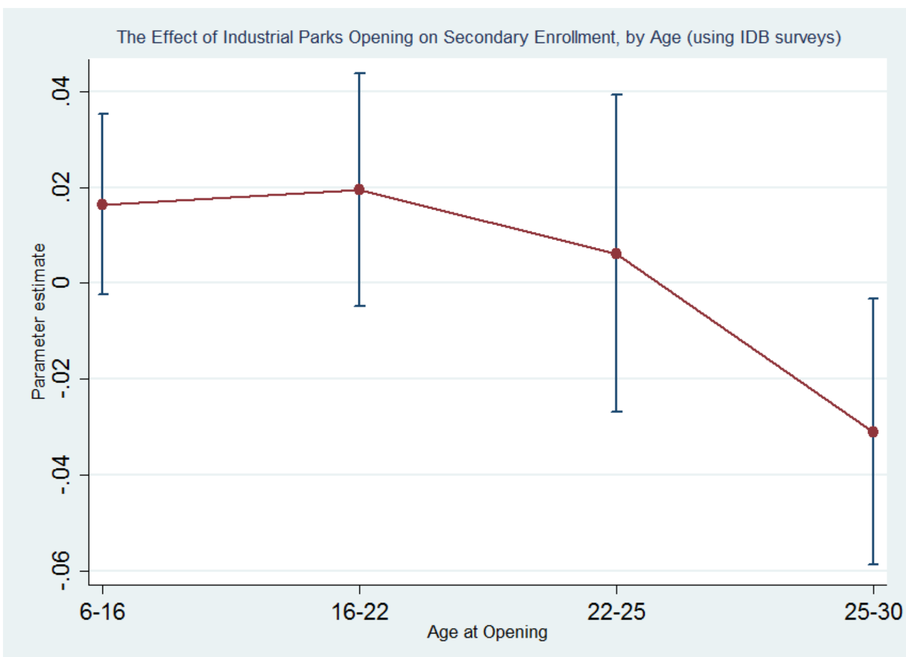


Figure A4: Enrollment in Secondary School by Age at Opening (Using IDB Surveys)

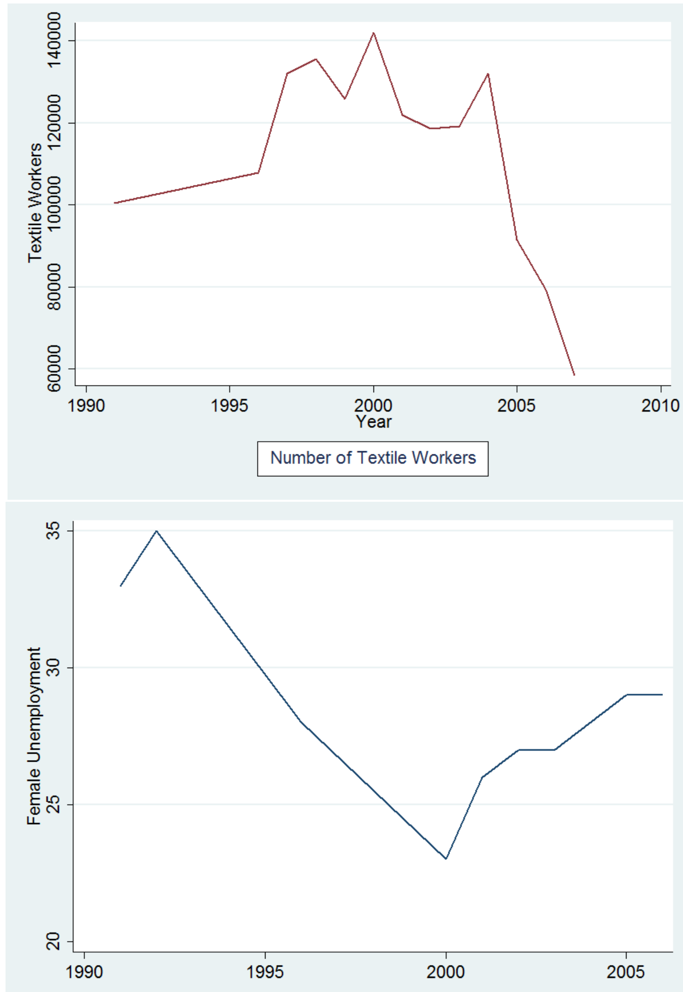


Figure A5: Female Unemployment

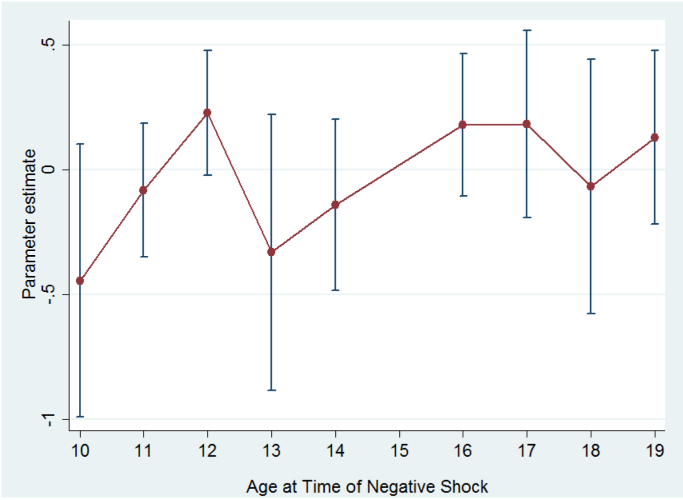
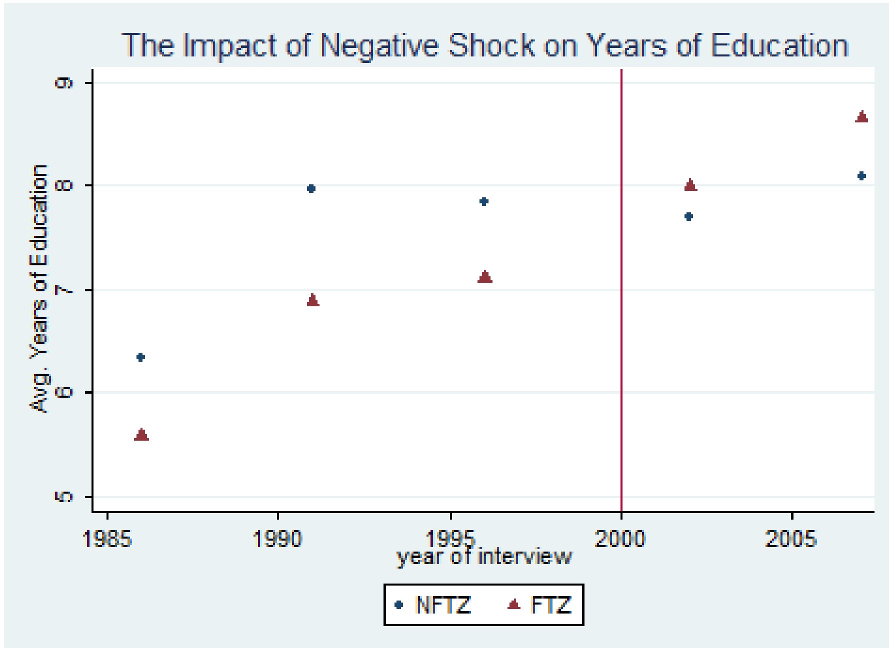


Figure A6: The Effect of Negative Shocks on Education by Age at the Time of the Shock

Notes: This graph plots the coefficients obtained from a regression of years of education on age at the time of the negative shock dummies, controlled by province time trends. The Y-axis shows the estimated coefficients and the X-axis shows the age at the time of the shock. Standard errors are clustered at the province level.



Notes: This graph plots the average years of education in each year of the survey before and after the negative shock. It shows that there is no evidence of different trends in schooling before 2000. The same pattern is also find if I plot average age of marriage.

Figure A7: Average Years of Education Before and After the Negative Shocks (Using as Control Group Prvinces with No FTZ)

Table A31: Marriage Market Gains and Female Factory Jobs

	(1) Divorce	(2) Husband's education	(3) Husband in high skilled job	(4) Difference in age	(5) Husband stays at home
FTZ×age6to16	-0.025**	0.672***	0.033**	-0.724**	0.003
	(0.013)	(0.168)	(0.014)	(0.310)	(0.012)
Mean of dependent	0.365	7.278	0.436	6.133	0.898
N	34,576	31,224	19,020	21,598	23,544
R^2	0.05	0.174	0.074	0.044	0.02
Province FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Province trends	YES	YES	YES	YES	YES
Cohort FE	YES	YES	YES	YES	YES
Province year of birth trends	YES	YES	YES	YES	YES

Notes: each cell represents a separate regression. Other covariates control for household and province characteristics. Standard errors are corrected for clustering at the province level. Significant at *** p<0.01, ** p<0.05, * p<0.1

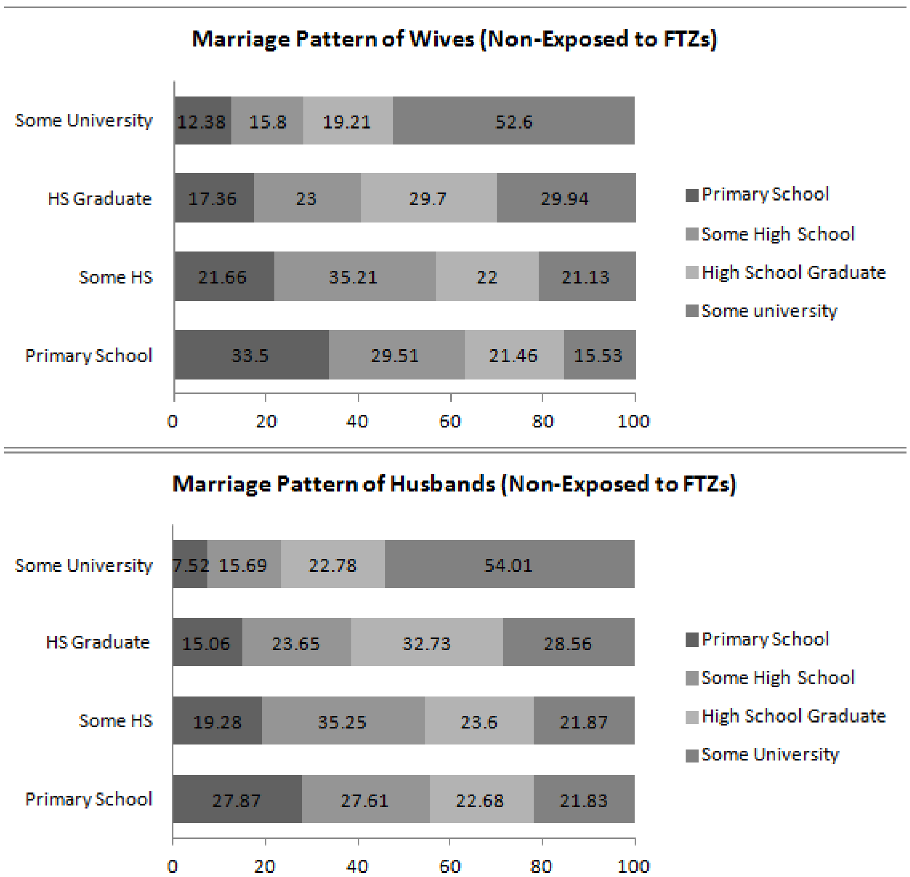


Figure A8: Spouse Education by Own Education, Ages 25-49, Non-Exposed Cohorts

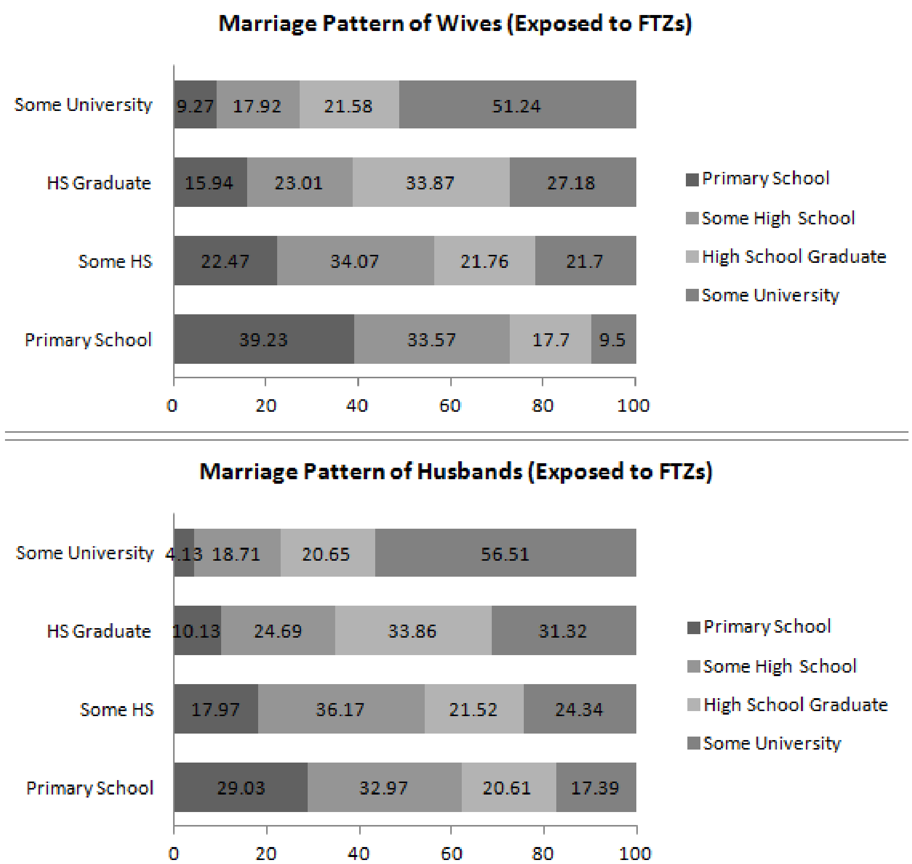
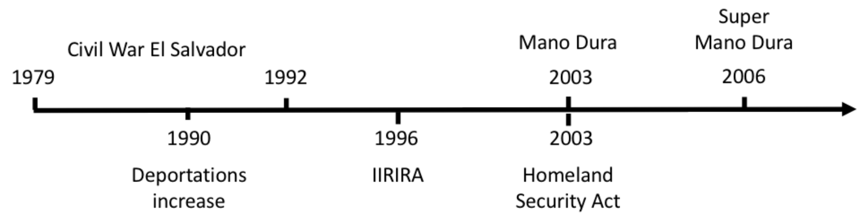


Figure A9: Spouse Education by Own Education, Ages 25-49, Exposed Cohorts

Figure A10: Timeline of Events



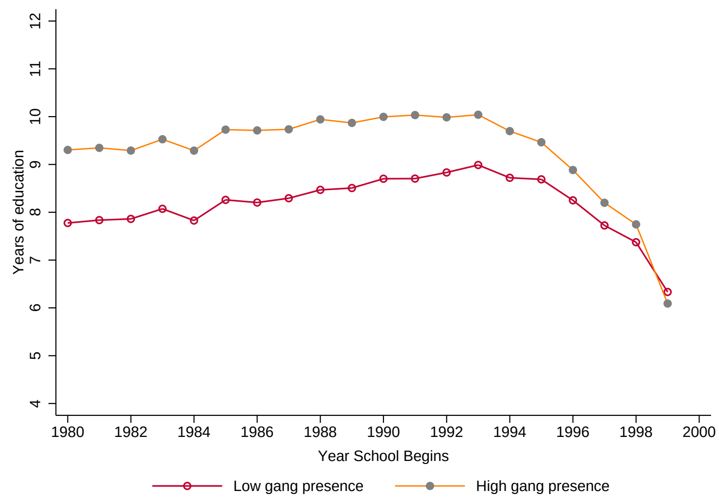


Figure A11: Years of education in gang and non gang municipalities in El Salvador

Table A32: Criminal deportations from the US and primary schooling in El Salvador

	(1)	(2)	(3)
$CrimeShockAge5x6_{m,c}$	-0.029***	-0.039***	-0.034***
	(0.010)	(0.010)	(0.010)
$CrimeShockAge7x8_{m,c}$	-0.022**	-0.032***	-0.028***
	(0.009)	(0.009)	(0.008)
$CrimeShockAge9x10_{m,c}$	-0.013	-0.024***	-0.024**
	(0.008)	(0.008)	(0.010)
$CrimeShockAge11x12_{m,c}$	-0.018***	-0.017**	-0.034***
	(0.007)	(0.007)	(0.008)
$CrimeShockAge13x14_{m,c}$	-0.007	-0.009*	-0.020**
	(0.005)	(0.005)	(0.008)
$CrimeShockAge15_{m,c}$	-0.005	-0.006	-0.003
	(0.006)	(0.006)	(0.011)
Observations	950,979	708,200	241,873
Municipality FE	YES	YES	YES
Cohort FE	YES	YES	YES
Dep*Year FE	YES	YES	YES
Sample	16-30	16-30	16-30

Cluster standard errors at the municipality level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure A12: Euclidean Distance Buffers and WJCs (Schools and DHS Clusters of Households) - Lima and Tumbes

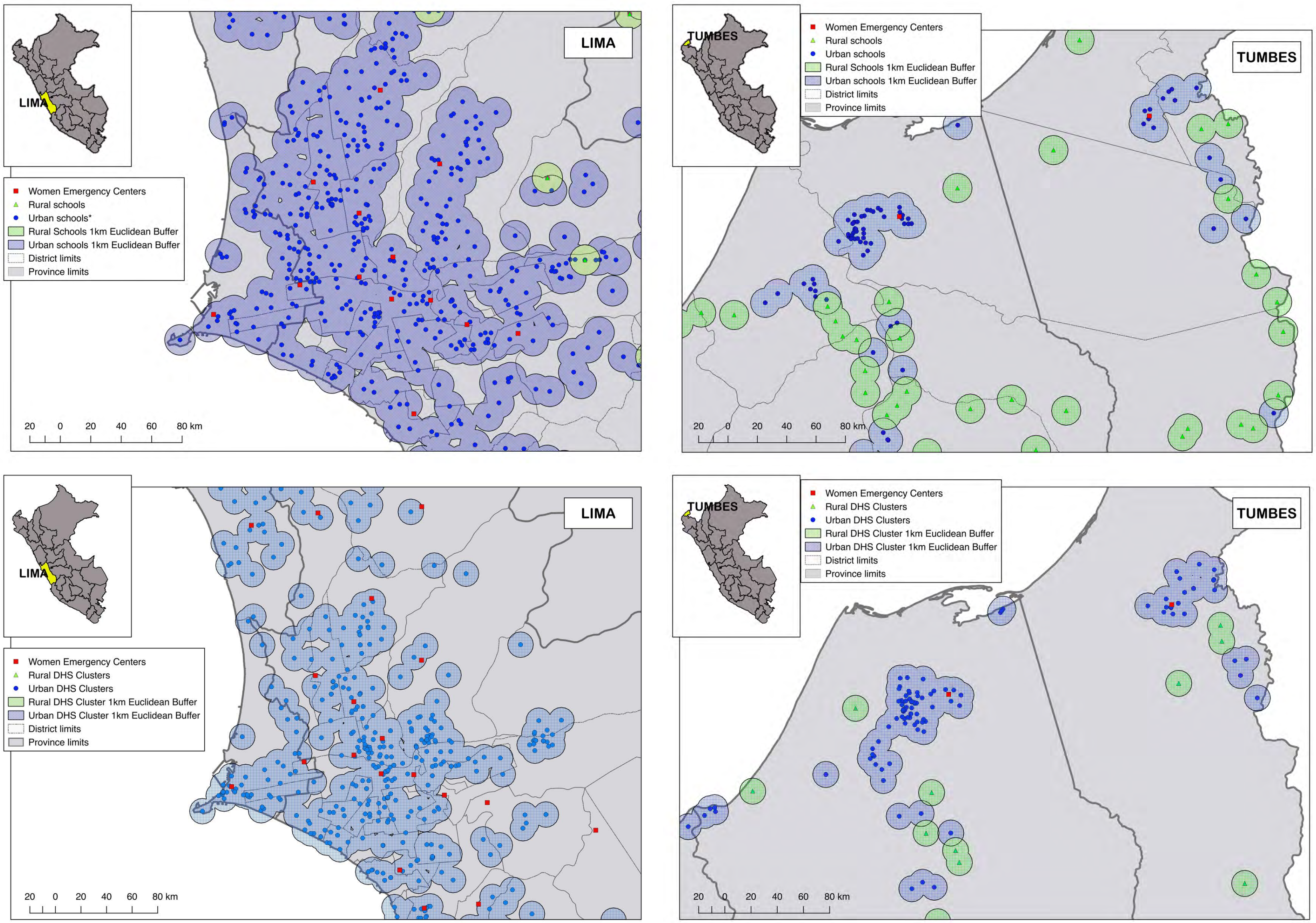


Figure A13: Domestic Violence in Peru (2003-2015)

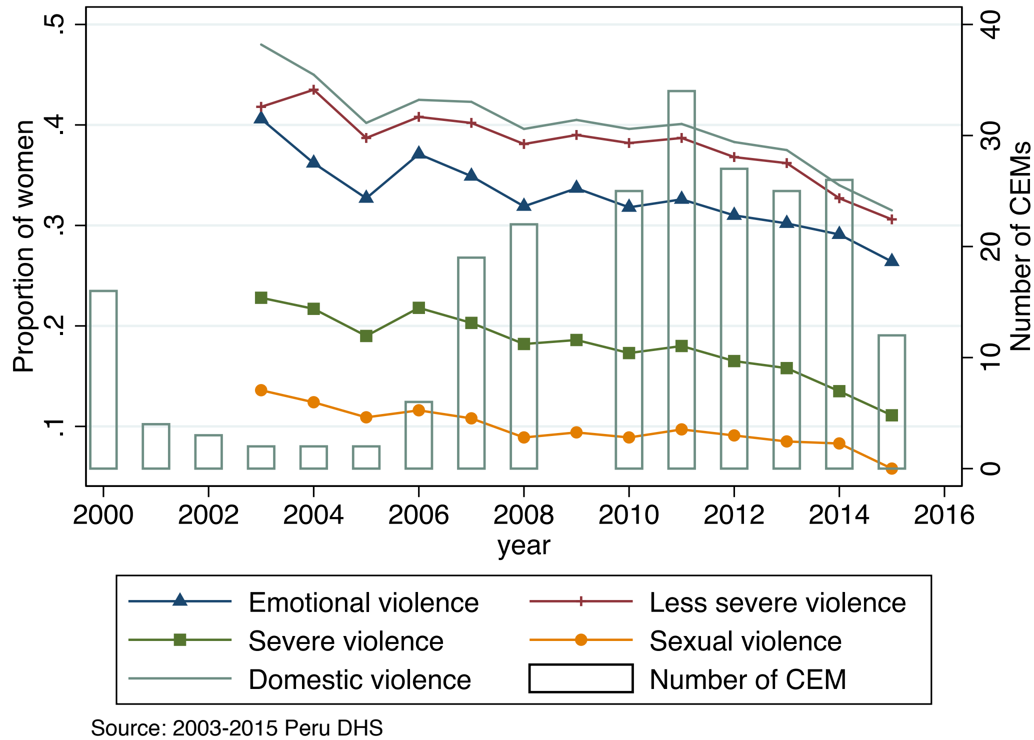


Figure A14: Total Number of Persons Attended in WJCs by Year (2002-2016)

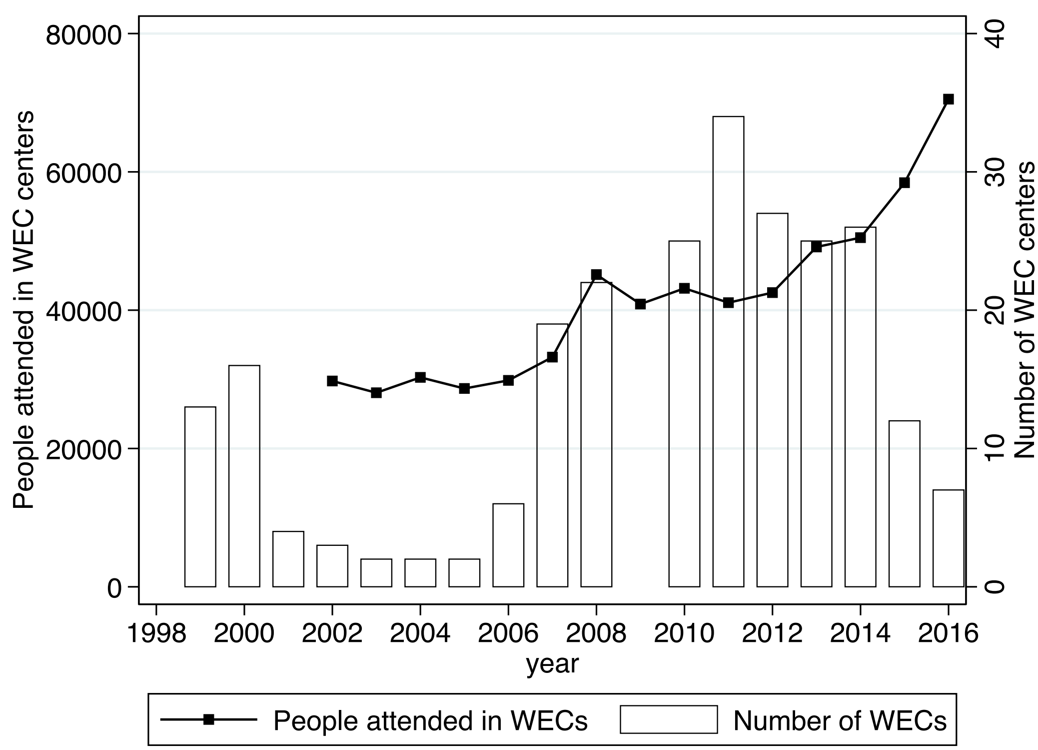


Table A33: Descriptive Statistics: School Enrollment and WJC Exposure (2006-2014)

	Primary Schools (1st - 6th Grade)			**Secondary Schools (1st - 5th Grade)**		
	All	Urban	Rural	All	Urban	Rural
Panel A: Years of coverage and number of schools						
Number of schools in						
First year of coverage (2006)	32,817	12,007	20,810	9,693	6,822	2,871
Last year of coverage (2014)	36,859	14,325	22,534	12,773	8,488	4,285
Panel B: Number of schools by exposure to a WJC						
Never had WJC within 1km	34,372	11,883	22,489	11,287	7,018	4,269
WJC within 1km	2,575	2,524	51	1,522	1,504	18
Never had WJC within 5km	26,418	5,095	21,323	7,282	3,164	4,118
WJC within 5km	10,529	9,312	1,217	5,527	5,358	169
Total of schools	36,947	14,407	22,540	12,809	8,522	4,287
Never had WJC in the district	24,439	6,530	17,909	7,481	4,040	3,441
WJC in the district	12,555	7,884	4,671	5,330	4,484	846
Total of schools	36994	14,414	22,580	12,811	8,524	4,287

	Primary Schools (1st - 6th Grade)			**Secondary Schools (1st - 5th Grade)**		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Panel C: School Summary Statistics						
Total Enrollment	315,221	95.9	142.5	102,685	174.8	206.8
Female Enrollment	315,221	46.9	73.6	102,685	84.4	114.9
Male Enrollment	315,221	49.0	75.4	102,685	90.4	113.1
School Characteristics						
Public School	315,221	0.797	0.402	102,685	0.636	0.481
Urban School	315,221	0.378	0.485	102,685	0.679	0.466
School Language (Spanish)	315,221	0.815	0.387	102,685	0.905	0.292
School Language (Quechua)	315,221	0.124	0.330	102,685	0.000	0.242
School with electricity	315,221	0.671	0.469	102,685	0.872	0.334
Schools with piped water	315,221	0.729	0.444	102,685	0.845	0.361
Reading test-scores (2nd grade)	181,240	510.18	73.08			
Math test-scores (2nd grade)	181,240	507.74	81.68			
Both test-scores (2nd grade)	181,240	508.9	73.44			

Notes: The GPS data was not available for 49 schools (47 primary schools and 2 secondary schools) in the Peruvian School Census. Source: Peru School Census (2006-2014)

Table A34: Descriptive Statistics: Children's School Attendance and WJC Exposure (2006-2014)

	Primary Level (Children: 6-11 years old)			Secondary Level (Children: 12-16 years old)		
	All	Urban	Rural	All	Urban	Rural
Panel A.1: Number of children by exposure to a WJC - (GPS data)						
No WJC within 1km	42,914	19,654	23,260	29,494	14,282	15,212
WJC within 1km	5,789	5,740	49	4,025	3,991	34
No WJC within 5km	32,066	9,706	22,360	21,691	7,087	14,604
WJC within 5km	16,637	15,688	949	11,828	11,186	642
Total of children	48,703	25,394	23,309	33,519	18,273	15,246
Panel A.2: Number of children by exposure to a WJC - (All data)						
No WJC in the district	48,895	19,250	29,645	33,392	13,999	19,393
WJC in the district	22,971	19,084	3,887	16,069	13,490	2,579
Total of children	71,866	38,334	33,532	49,461	27,489	21,972

	Primary Level (Children: 6-11 years old)			Secondary Level (Children: 12-16 years old)		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Panel B: Children's Summary Statistics						
Currently Attending	48,703	0.970	0.169	33,519	0.895	0.305
Female Attendance	24,689	0.970	0.169	18,549	0.899	0.300
Male Attendance	24,014	0.970	0.169	14,970	0.891	0.311
Passed Grade	48,213	0.919	0.271	30,380	0.782	0.412
Repeated Grade	48,213	0.048	0.215	30,380	0.038	0.191
Dropped Out	48,213	0.022	0.146	30,380	0.090	0.287
Left School +2 years ago	48,213	0.002	0.047	30,380	0.084	0.278
Children's Characteristics						
Age	48,703	8.467	1.700	33,519	13.786	1.384
Head's Years of Education	48,703	8.602	7.159	33,519	8.348	7.025
Urban Cluster	48,703	0.521	0.499	33,519	0.545	0.497
# Female Adults in HH	48,703	1.219	0.532	33,519	1.218	0.541
# Male Adults in HH	48,703	1.101	0.611	33,519	1.120	0.669
# HH Members 0-18 years old	48,703	3.166	1.522	33,519	3.248	1.551

Notes: The GPS data was not available for the years 2012 and 2013 in the Peru DHS. Source: Peru DHS (2006-2014)

Table A35: Descriptive Statistics: Women's Domestic Violence and WJC Exposure (2006-2014)

	Women: 15-49 years old		
	All	Urban	Rural
Panel A.1: Number of women by exposure to a WJC (GPS data)			
No WJC within 1km	55,323	29,432	25,891
WJC within 1km	9,040	8,965	75
No WJC within 5km	38,603	13,841	24,762
WJC within 5km	25,760	24,556	1,204
Total of women	64,363	38,397	25,966
Panel A.2: Number of women by exposure to a WJC (All data)			
No WJC in the district	61,946	28,540	33,406
WJC in the district	34,614	30,041	4,573
Total of women	96,560	58,581	37,979

	Women: 15-49 years old		
	Obs	Mean	Std. Dev.
Panel B: Women's Summary Statistics			
Domestic violence last 12 months	64,363	0.390	0.487
Emotional violence	64,363	0.323	0.467
Less severe violence	64,363	0.376	0.484
Severe violence	64,363	0.174	0.379
Sexual violence	64,363	0.093	0.291
Women's Characteristics			
Age	64,363	33.93	8.336
Age at first marriage	64,363	20.14	4.739
# Total children ever born	64,363	2.811	1.993
# Years of education	64,363	8.577	4.481
# Household Members	64,363	4.626	1.818
Married	64,363	0.356	0.478
Living together	64,363	0.517	0.499
Widowed	64,363	0.007	0.089
Divorced	64,363	0.118	0.319
Urban cluster	64,363	0.596	0.490

Notes: The GPS data was not available for the years 2012 and 2013 in the Peru DHS. Source: Peru DHS (2006-2014)

Table A36: Placement of WJCs in the District

Dependent variables	**WJC in district, by 2014**	**Added WJC in district during 2006-2014**		
	(1)	(2)	(3)	(4)
# Criminal Attorney Offices	-0.022*	-0.050***	-0.050***	-0.050***
	(0.013)	(0.015)	(0.015)	(0.015)
# Family Attorney Offices	0.090**	0.110***	0.111***	0.110***
	(0.036)	(0.040)	(0.040)	(0.040)
# Mixed Attorney Offices	0.106***	0.069	0.071*	0.069
	(0.033)	(0.043)	(0.043)	(0.043)
# Criminal Courts	0.005	-0.001	-0.001	-0.001
	(0.018)	(0.024)	(0.024)	(0.024)
# Family Courts	-0.093**	-0.126**	-0.127**	-0.127**
	(0.040)	(0.058)	(0.058)	(0.058)
# Mixed Courts	0.183***	0.233***	0.233***	0.233***
	(0.035)	(0.041)	(0.042)	(0.042)
# Police Stations	0.082***	0.049***	0.048***	0.049***
	(0.012)	(0.015)	(0.015)	(0.015)
# of Health Establishments	0.246***	0.194***	0.193***	0.196***
	(0.043)	(0.050)	(0.049)	(0.050)
Log. Population (2000)	0.017***	0.012**	0.012**	0.013**
	(0.005)	(0.005)	(0.005)	(0.005)
△ Primary Enrollment, (1998-2005)		0.0002		0.0002
		(0.0002)		(0.0002)
△ Secondary Enrollment, (1998-2005)			0.00008	0.00006
			(0.00008)	(0.00009)
Observations (Districts)	1,843	1,843	1,843	1,843
R-squared	0.703	0.535	0.534	0.535
Department FE (25)	YES	YES	YES	YES

Robust standard errors in parentheses

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

Notes: This table shows the effects of district characteristics on WJCs placement. The left hand side variable in Columns 1 is the number of WJCs in the district by 2014; in Columns 2 to 4 it is whether any centers were added during the sample period 2006-2014. Standard errors are in parentheses, clustered at the district level. Source: MIMP (*Ministerio de la Mujer y Poblaciones Vulnerables)*

Table A37: The Effect of WJCs on Secondary School Enrollment (2006-2014)

Dep. variable	**Log (Secondary School Enrollment)**				
Sample	All schools	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard	Standard
	(1)	(2)	(3)	(4)	(5)
Panel A: WJC within a distance buffer from the school					
WJC within 1km	0.029**	0.017	0.030**	0.034***	-0.005
	(0.012)	(0.014)	(0.012)	(0.013)	(0.019)
Log (District Population)			0.427***	0.426***	0.442***
			(0.038)	(0.043)	(0.082)
Observations	102,685	102,685	102,685	69,686	41,324
Number of schools	12809	12809	12809	8516	5175
Mean dep. var	174.8	174.8	174.8	215.3	195.3
Panel B: WJC in the district of the school					
WJC in the district	0.023***	-0.004	0.014*	0.019**	-0.005
	(0.008)	(0.008)	(0.008)	(0.008)	(0.013)
Log (District Population)			0.420***	0.417***	0.448***
			(0.038)	(0.043)	(0.083)
Observations	102,691	102,691	102,691	69,692	41,324
Number of schools	12811	12811	12811	8518	5175
Mean dep. var	174.8	174.8	174.8	215.3	195.3
School FE	YES	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES

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

Notes: The dependent variable is the logarithm of enrollment plus one. The independent variables measures the number of WJCs within a 1km Euclidean buffer from the school and presence of WJC in school's district. Standard errors (in parentheses) are clustered at the school level. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (spanish), urban and public school dummy).Source: Peruvian School Census 2006-2014.

Table A38: The Effect of WJCs on Primary Level 2nd Grade Test Scores - (2006-2014)

Dep. variable	**Standardized Test Scores (2nd Grade)**			
Sample	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the school				
WJC within 1km	0.028*	0.018	0.040**	0.027
	(0.017)	(0.019)	(0.018)	(0.021)
Observations	181,240	181,240	92,666	69,822
Number of schools	29737	29737	13507	10858
Mean dep. var	508.9	508.9	536.9	526.9
Panel B: WJC in the district of the school				
WJC in the district	0.026**	-0.020	0.050***	0.050***
	(0.011)	(0.016)	(0.013)	(0.016)
Observations	181,279	181,279	92,681	69,838
Number of schools	29747	29747	13510	10862
Mean dep. var	508.9	508.9	537.0	527.0
School FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

Robust standard errors in parentheses

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

Notes: The dependent variable is the average of the standardized reading and math test scores for 2nd grade of primary school. The independent variables measures the number of WJCs within a 1km Euclidean buffer from the school and presence of WJC in school's district. Standard errors (in parentheses) are clustered at the school level. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (spanish), urban and public school dummy). Source: Peru ECE 2007-2014.

Table A39: Placebo regressions, Impact of Future WJCs on Primary School Enrollment

Dep. variable	**Log (Primary School Enrollment)**			
Sample	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the school				
WJC within 1km, t	0.023***	0.022***	0.026***	0.019**
	(0.008)	(0.007)	(0.008)	(0.009)
WJC within 1km, $t+1$	0.007	0.008	0.003	0.006
	(0.008)	(0.007)	(0.008)	(0.009)
Observations	315,221	315,221	119,232	103,518
Number of schools	36947	36947	14405	12398
Mean dep. var	95.9	95.9	177.8	127.7
Panel B: WJC in the district of the school				
WJC in the district, t	0.008*	0.003	0.017***	0.030***
	(0.004)	(0.004)	(0.006)	(0.008)
WJC in the district, $t+1$	0.001	-0.001	0.004	0.003
	(0.004)	(0.004)	(0.006)	(0.007)
Observations	315,407	315,407	119,270	103,586
Number of schools	36994	36994	14412	12412
Mean dep. var	95.9	95.9	177.8	127.7
School FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

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

Notes: The dependent variable is the logarithm of enrollment plus one. The independent variable measures the presence of a WJC within 1km/in the district in year t and controls for openings of future WJCs in year $t+1$. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (spanish), urban and public school dummy). Source: Peruvian School Census: 2006-2014.

Table A40: Placebo regressions, Impact of Future WJCs on Secondary School Enrollment

Dep. variable	**Log (Secondary School Enrollment)**			
Sample	All schools	All schools	Only urban schools	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the school				
WJC within 1km, t	0.032**	0.022*	0.037	0.005
	(0.013)	(0.013)	(0.013)	(0.019)
WJC within 1km, $t+1$	-0.004	-0.008	-0.005	-0.019
	(0.013)	(0.013)	(0.013)	(0.018)
Observations	102,685	102,685	69,686	41,277
Number of schools	12809	12809	8516	5170
Mean dep. var	174.8	174.8	215.3	195.3
Panel B: WJC in the district of the school				
WJC in the district, t	0.024***	0.002	0.031**	0.013
	(0.008)	(0.007)	(0.008)	(0.012)
WJC in the district, $t+1$	-0.002	-0.008	-0.0009	-0.003
	(0.008)	(0.007)	(0.009)	(0.013)
Observations	102,691	102,691	69,692	41,277
Number of schools	12811	12811	8518	5170
Mean dep. var	174.8	174.8	215.3	195.3
School FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

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

Notes: The dependent variable is the logarithm of enrollment plus one. The independent variable measures the presence of a WJC within 1km/in the district in year t and controls for openings of future WJCs in year $t+1$. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (spanish), urban and public school dummy). Source: Peruvian School Census: 2006-2014.

Table A41: The Effect of WJCs on Children's Primary School Attendance - (2006-2014)

Dep. variable	**Currently Attending Primary Level**			
Sample	All children 6-11 y.o	All children 6-11 y.o	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the cluster of residence				
WJC within 1km	0.019**	0.018*	0.027***	0.023***
	(0.008)	(0.009)	(0.009)	(0.008)
Observations	48,703	48,703	25,391	19,563
Number of districts	1159	1159	485	215
Mean dep. var	0.970	0.970	0.971	0.969
Panel B: WJC in the district of residence				
WJC in the district	0.005	-0.005	0.016**	0.022**
	(0.007)	(0.011)	(0.008)	(0.009)
Observations	71,866	71,866	38,330	29,051
Number of districts	1286	1286	531	225
Mean dep. var	0.970	0.970	0.970	0.967
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

Robust standard errors in parentheses

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

Notes: The dependent variable is a dummy indicating whether the child is currently attending primary school. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the child's cluster of residence and presence of a WJC in the child's district. Robust standard errors (in parentheses) are clustered at the district level. The sample includes children between the ages of 6 and 11. Covariates include age, gender, household's head years of education, number of children in the household aged 0-18, number of children in the household aged 0-5, number of female adults, number of male adults, rural residence dummy, district fixed effect and province-by-year fixed effect. Source: Peru DHS 2006-2014.

Table A42: The Effect of WJCs on Children's Secondary School Attendance - (2006-2014)

Dep. variable	Currently Attending Secondary Level			
Sample	All children 12-16 y.o	All children 12-16 y.o	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the cluster of residence				
WJC within 1km	0.022*	0.027*	0.029**	0.027**
	(0.012)	(0.014)	(0.012)	(0.013)
Observations	33,519	33,519	18,266	13,570
Number of clusters	1140	1140	480	215
Mean dep. var	0.895	0.895	0.916	0.908
Panel B: WJC in the district of residence				
WJC in the district	0.012	0.039**	0.027	0.036
	(0.016)	(0.018)	(0.020)	(0.024)
Observations	49,461	49,461	27,482	20,275
Number of districts	1270	1270	528	224
Mean dep. var	0.896	0.896	0.913	0.904
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

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

Notes: The dependent variable is a dummy indicating whether the child is currently attending secondary school. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the child's cluster of residence and presence of a WJC in the child's district. Robust standard errors (in parentheses) are clustered at the district level. The sample includes children between the ages of 12 and 16. Covariates include age, gender, household's head years of education, number of children in the household aged 0-18, number of children in the household aged 0-5, number of female adults, number of male adults, rural residence dummy, district fixed effect and province-by-year fixed effect. Source: Peru DHS 2006-2014.

Table A43: Relationship between WJCs centers within 1km and pre-program school enrollment

	Schools matched to WJC within 1km, Pre-WJC △1998-2005					
	△ Primary School Enrollment			**△ Secondary School Enrollment**		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Female	Male	All	Female	Male
WJC within 1km in 2006	*(excluded group)*					
WJC within 1km in 2007	2.409	1.555	0.855	0.843	1.802	-0.958
	(2.376)	(1.241)	(1.456)	(4.072)	(2.246)	(3.063)
WJC within 1km in 2008	3.469	1.773	1.696	5.111	4.336	0.775
	(2.436)	(1.388)	(1.537)	(4.853)	(2.787)	(3.158)
WJC within 1km in 2009	-	-	-	-	-	-
WJC within 1km in 2010	3.121	1.877	1.244	0.287	2.349	-2.062
	(2.286)	(1.193)	(1.423)	(4.210)	(2.276)	(2.853)
WJC within 1km in 2011	2.232	1.726	0.506	-0.865	2.295	-3.160
	(2.284)	(1.208)	(1.416)	(3.999)	(2.335)	(2.751)
WJC within 1km in 2012	0.961	0.787	0.174	-5.425	-0.176	-5.248*
	(2.550)	(1.718)	(1.823)	(4.342)	(2.348)	(2.957)
WJC within 1km in 2013	1.150	0.601	0.549	-3.199	0.045	-3.244
	(2.638)	(1.397)	(1.645)	(4.361)	(2.372)	(3.014)
WJC within 1km in 2014	3.148	1.596	1.553	-0.862	1.649	-2.511
	(2.485)	(1.282)	(1.522)	(4.507)	(2.533)	(2.943)
Observations	6,372	6,372	6,372	3,400	3,400	3,400
Number of schools	1247	1247	1247	710	710	710
Year FE	YES	YES	YES	YES	YES	YES
P-value joint test	0.783	0.789	0.877	0.375	0.522	0.314

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

Notes: The dependent variable in columns 1-6 is the change in the total number of enrolled students at the school level between 1998-2005. The observations correspond to the pre-WJC period for each school (1998-2005), it includes all schools that are located within a 1km Euclidean buffer of a WJC, which opened between 2006 and 2014. Standard errors that allow for clustering at the school level are reported in parentheses. All regressions include year fixed-effects, the excluded group is presence of a WJC within 1km in 2006.

Table A44: Relationship between WJCs in the district and pre-program school enrollment

	Schools matched to WJC in the district, Pre-WJC $\triangle$1998-2005					
	$\triangle$ **Primary School Enrollment**			$\triangle$ **Secondary School Enrollment**		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Female	Male	All	Female	Male
WJC in district in 2006	*(excluded group)*					
WJC in district in 2007	-1.080	-0.544	-0.536	4.238**	2.537**	1.701
	(0.760)	(0.387)	(0.386)	(2.019)	(1.003)	(1.210)
WJC in district in 2008	-0.040	-0.010	-0.030	-1.045	0.387	-1.432
	(1.005)	(0.488)	(0.536)	(2.895)	(1.262)	(1.900)
WJC in district in 2009	-	-	-	-	-	-
WJC in district in 2010	1.034	0.491	0.544	3.165	1.777	1.388
	(1.014)	(0.508)	(0.516)	(2.265)	(1.177)	(1.263)
WJC in district in 2011	0.515	0.391	0.124	3.611	2.348	1.264
	(1.005)	(0.499)	(0.522)	(3.222)	(1.853)	(1.527)
WJC in district in 2012	-0.158	-0.130	-0.028	1.464	1.114	0.350
	(0.679)	(0.349)	(0.343)	(2.157)	(1.019)	(1.336)
WJC in district in 2013	0.563	0.266	0.297	2.642	1.487*	1.155
	(0.529)	(0.277)	(0.269)	(1.598)	(0.855)	(1.031)
WJC in district in 2014	1.026	0.433	0.593*	3.397**	1.827**	1.570*
	(0.671)	(0.339)	(0.345)	(1.489)	(0.864)	(0.925)
Observations	37,397	37,397	37,397	10,274	10,274	10,274
Number of schools	7,055	7,055	7,055	2,189	2,189	2,189
Number of districts	184	184	184	184	184	184
Year FE	YES	YES	YES	YES	YES	YES
P-value joint test	0.235	0.278	0.211	0.658	0.740	0.652

Robust standard errors in parentheses

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

Notes: The dependent variable in columns 1-6 is the change in the total number of enrolled students at the school level between 1998-2005. The observations correspond to the pre-WJC period for each school (1998-2005), it includes all schools which are located in districts with WJC, which opened between 2006 and 2014. Standard errors that allow for clustering at the school level are reported in parentheses. All regressions include year fixed-effects, the excluded group is presence of a WJC in the district in 2006.

Table A45: Relationship between WJCs in the district rollout and pre-program school attendance

	Districts matched to WJC locations, Pre-WJC △1996-2005					
	△ Primary School Attendance			**△ Secondary School Attendance**		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Female	Male	All	Female	Male
WJC in the district in 2006	*(excluded group)*					
WJC in the district in 2007	0.010	-0.006	0.013	0.033	0.038	0.024
	(0.015)	(0.029)	(0.016)	(0.051)	(0.056)	(0.063)
WJC in the district in 2008	0.012	-0.003	0.009	-0.013	-0.026	-0.001
	(0.014)	(0.031)	(0.013)	(0.046)	(0.067)	(0.059)
WJC in the district in 2009	-	-	-	-	-	-
WJC in the district in 2010	0.011	0.001	-0.004	0.015	-0.022	0.045
	(0.014)	(0.028)	(0.016)	(0.045)	(0.060)	(0.055)
WJC in the district in 2011	-0.002	-0.015	-0.001	0.008	0.019	-0.001
	(0.016)	(0.030)	(0.016)	(0.036)	(0.042)	(0.042)
WJC in the district in 2012	0.020	0.002	0.021	-0.040	-0.026	-0.064
	(0.014)	(0.029)	(0.013)	(0.042)	(0.049)	(0.051)
WJC in the district in 2013	0.006	-0.011	-0.010	0.002	-0.048	0.037
	(0.021)	(0.038)	(0.032)	(0.055)	(0.081)	(0.059)
WJC in the district in 2014	0.020	0.001	0.024	-0.049	-0.153	-0.004
	(0.054)	(0.065)	(0.048)	(0.074)	(0.109)	(0.067)
Observations	186	182	183	184	180	179
Number of districts	106	103	106	106	103	105
Year FE	YES	YES	YES	YES	YES	YES
P-value joint test	0.676	0.936	0.394	0.712	0.568	0.554

Robust standard errors in parentheses

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

Notes: The dependent variable in columns 1-6 is the change in the school attendance rate at the district level between 1996-2005. The observations correspond to the pre-WJC period for each district (1996-2005), it includes all districts which have a WJC cener that opened between 2006 and 2014. Standard errors that allow for clustering at the district level are reported in parentheses. All regressions include year fixed-effects, the excluded group is presence of a WJC in the district in 2006.

Table A46: The Effect of WJCs on Domestic Violence - (2006-2014)

Dep. variable	**Domestic Violence in last 12 months**			
Sample	All women	All women	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the cluster of residence				
WJC within 1km	-0.022**	-0.018*	-0.029***	-0.017
	(0.010)	(0.011)	(0.010)	(0.012)
Observations	64,363	64,363	38,395	27,996
Number of districts	1167	1167	485	215
Mean dep. var	0.390	0.390	0.399	0.397
Panel B: WJC in the district of residence				
WJC in district	-0.024**	-0.060***	-0.023*	-0.032*
	(0.011)	(0.020)	(0.014)	(0.018)
Observations	96,560	96,560	58,579	42,393
Number of districts	1293	1293	531	225
Mean dep. var	0.387	0.387	0.397	0.394
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

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

Notes: The dependent variable is a dummy indicating whether the women suffered any type of domestic violence (less severe, severe or sexual violence) during the last 12 months. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the women's cluster of residence and presence of WJC in the women's district. Robust standard errors (in parentheses) are clustered at the district level. The sample includes women between the ages of 15 and 49. Women who were never married or never cohabited are excluded from the sample. Covariates include age, age at first marriage, number of children, years of education, number of household members, number of households in the dwelling, maritual status (married=1), rural residence dummy, district fixed-effects and province-by-year fixed effects. Source: Peru DHS 2006-2014.

Table A47: Impact of WJCs on Emotional Violence - (2006-2014)

Dep. variables	Emotional violence	Spouse ever humiliated	Spouse ever threatened with harm	Spouse ever threatened to take children
	(1)	(2)	(3)	(4)
Sample A: All women 15-49 years old				
WJC within 1km	-0.010	-0.002	-0.003	-0.017*
	(0.010)	(0.009)	(0.006)	(0.010)
Observations	64,364	64,364	64,364	64,364
Number of districts	1167	1167	1167	1167
Mean dep.var.	0.323	0.229	0.119	0.206
Sample B: Only women in urban clusters				
WJC within 1km	-0.018	-0.009	-0.007	-0.024**
	(0.011)	(0.010)	(0.007)	(0.011)
Observations	38,396	38,396	38,396	38,396
Number of districts	485	485	485	485
Mean dep.var.	0.337	0.239	0.114	0.219
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

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

Notes: The dependent variable is a dummy indicating whether the women suffered any type of emotional violence during the last 12 months. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the women's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample includes women between the ages of 15 and 49. Women who were never married or never cohabited are excluded from the sample. Covariates include age, age at first marriage, number of children, years of education, number of household members, number of households in the dwelling, marital status (married=1), rural residence dummy, district fixed-effects and province-by-year fixed effects. Source: Peru DHS 2006-2014.

Table A48: Impact of WJCs on Decision Making and Bargaining Power in the Household - (2006-2014)

Dep. variable	Joint decision-making		
	score (0-6)	score (0-1)	(0/1)
	(1)	(2)	(3)
Sample: Married or cohabiting women 15-49 years old			
WJC within 1km	0.040	0.007	0.017*
	(0.047)	(0.008)	(0.009)
Observations	72,009	72,009	72,009
Number of clusters	1168	1168	1168
Mean dep.var.	2.238	0.373	0.798
Dep. variable	**Earnings compared to husband**		
	Earns more than husband	Earns Less than husband	Earns the same as husband
Sample: Married or cohabiting women 15-49 years old			
WJC within 1km	0.008	-0.034*	0.029**
	(0.011)	(0.018)	(0.014)
Observations	33,767	33,767	33,767
Number of districts	1094	1094	1094
Mean dep.var.	0.125	0.676	0.189
District FE	YES	YES	YES
Province*Year FE	YES	YES	YES
Covariates	YES	YES	YES

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

Notes: In the DHS women are asked who makes decisions on a variety of household issues. For instance, a women is asked "who makes the final decision on your own health care?" "*who makes the final decision on large household purchases?*" etc. Responses include: respondent only, jointly with partner, and partner only. From these replies, we construct three measures of equal decision-making, that is, when the women makes decisions jointly with the partner. Robust standard errors (in parentheses) are clustered at the district level. The sample includes women between the ages of 15 and 49. Covariates include age, age at first marriage, number of children, years of education, number of household members, number of households in the dwelling, marital status (married=1), rural residence dummy, district fixed-effects and province-by-year fixed effects. Source: Peru DHS 2006-2014.

Table A49: Impact of WJCs on Women's Labor Force Participation - (2006-2014)

Dep. variables	Currently working (1)	Works for family (2)	Works for someone else (3)	Self-employed (4)
Sample A: All women 15-49 years old				
WJC within 1km	-0.010	-0.004	-0.010	0.005
	(0.010)	(0.005)	(0.008)	(0.007)
Observations	113,785	113,786	113,786	113,786
Number of clusters	1168	1168	1168	1168
Mean dep.var.	0.646	0.211	0.305	0.236
Sample B: Married or cohabiting women selected for the DV module				
WJC within 1km	-0.009	-0.004	-0.024	0.017
	(0.014)	(0.009)	(0.017)	(0.011)
Observations	64,354	64,354	64,354	64,354
Number of districts	1167	1167	1167	1167
Mean dep.var.	0.684	0.209	0.269	0.300
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

Robust standard errors in parentheses

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

Notes: The dependent variable is a dummy indicating women's labor force participation during the last 12 months. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the women's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample includes women between the ages of 15 and 49. Covariates include age, age at first marriage, number of children, years of education, number of household members, number of households in the dwelling, marital status (married=1), rural residence dummy, district fixed-effects and province-by-year fixed effects. Source: Peru DHS 2006-2014.

Figure A15: Geographical Distribution of Primary Schools and WJCs

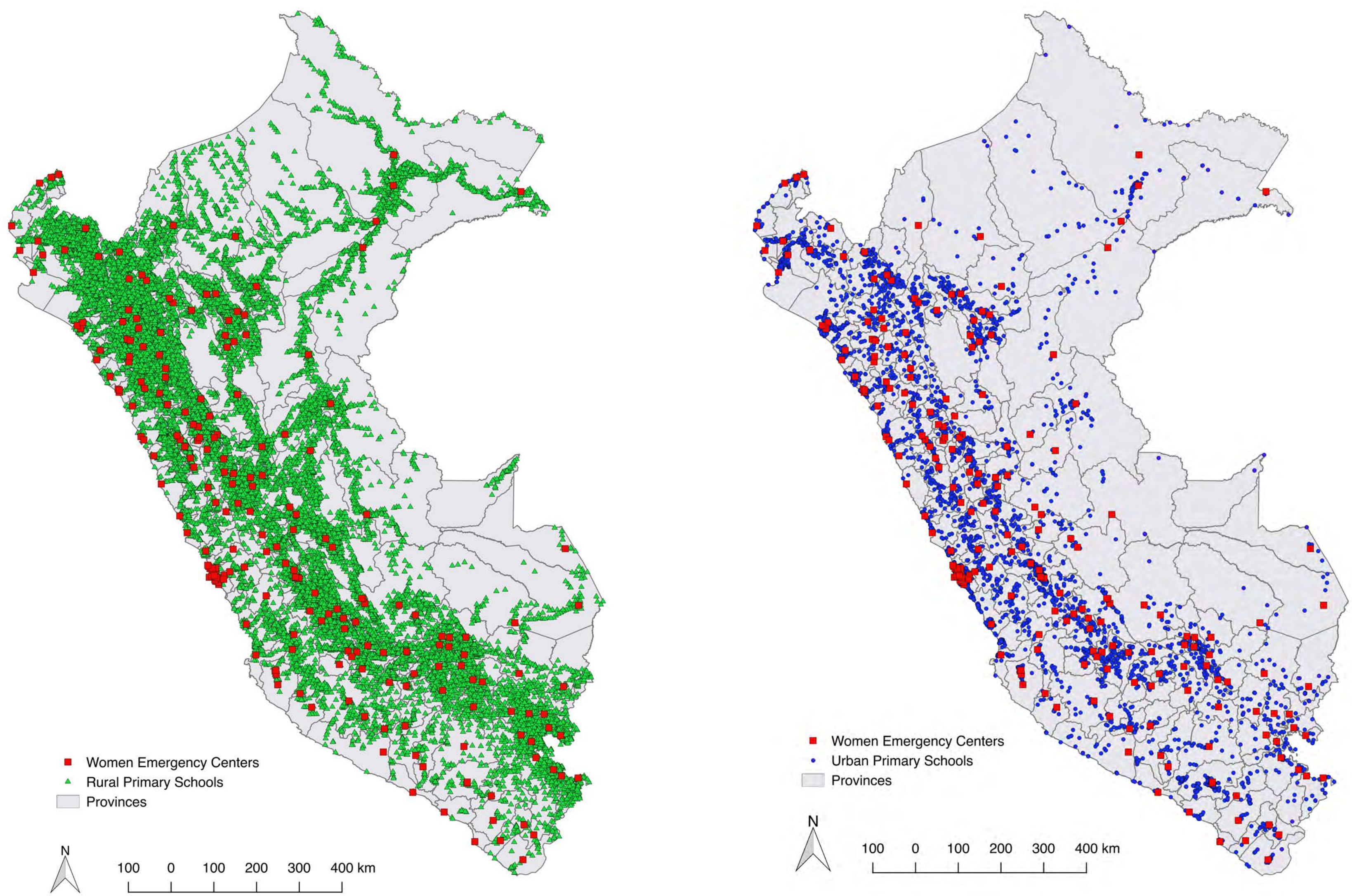


Figure A16: Geographical Distribution of Secondary Schools and WJCs

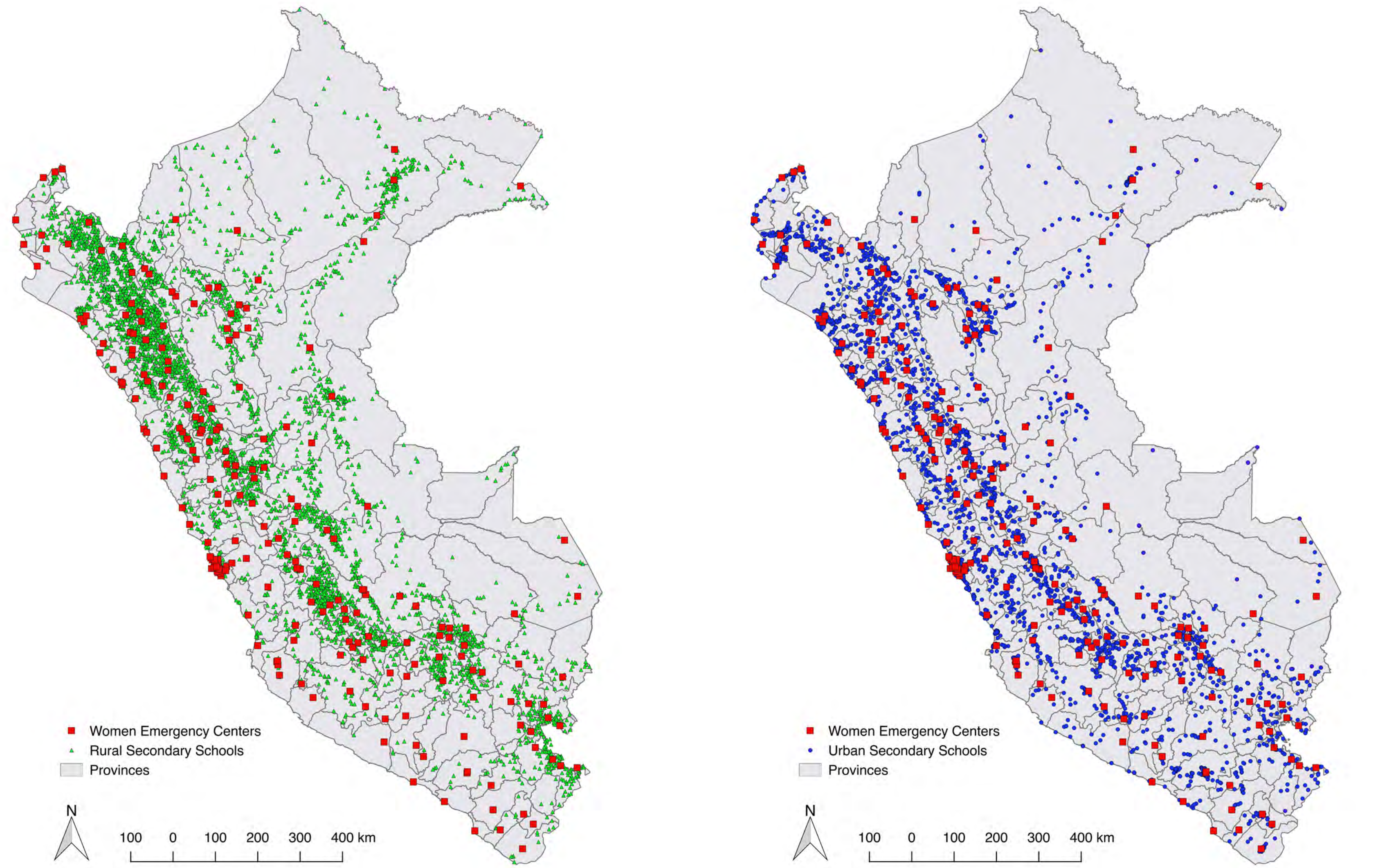
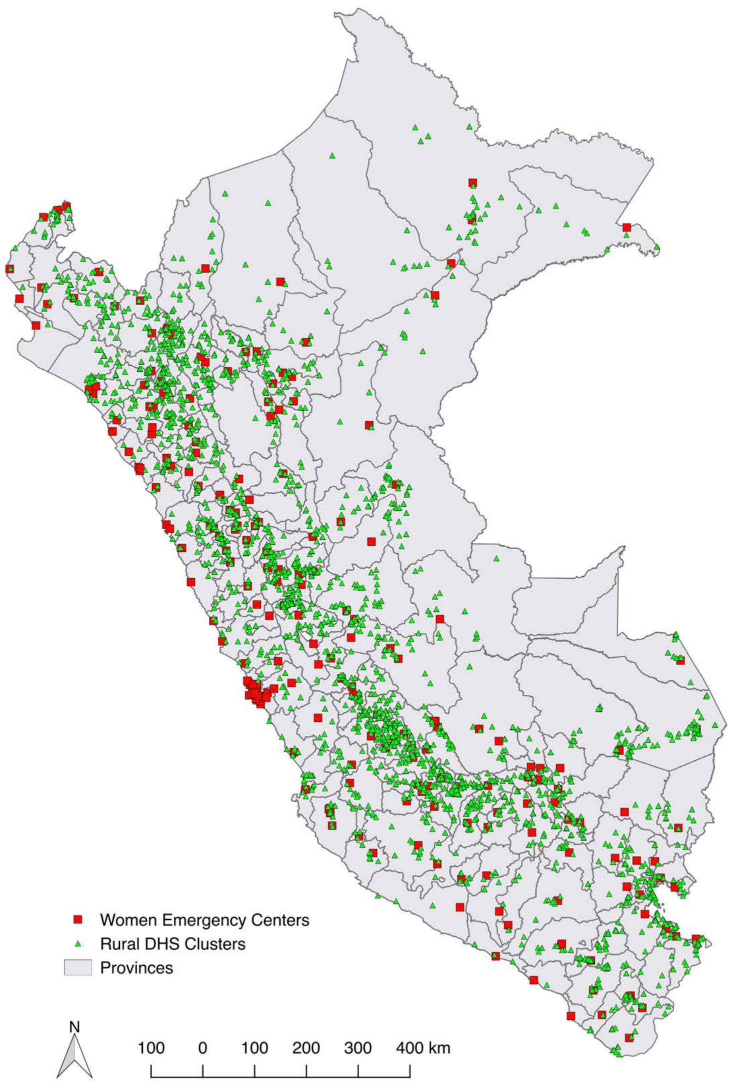


Figure A17: Geographical Distribution of DHS clusters and WJCs



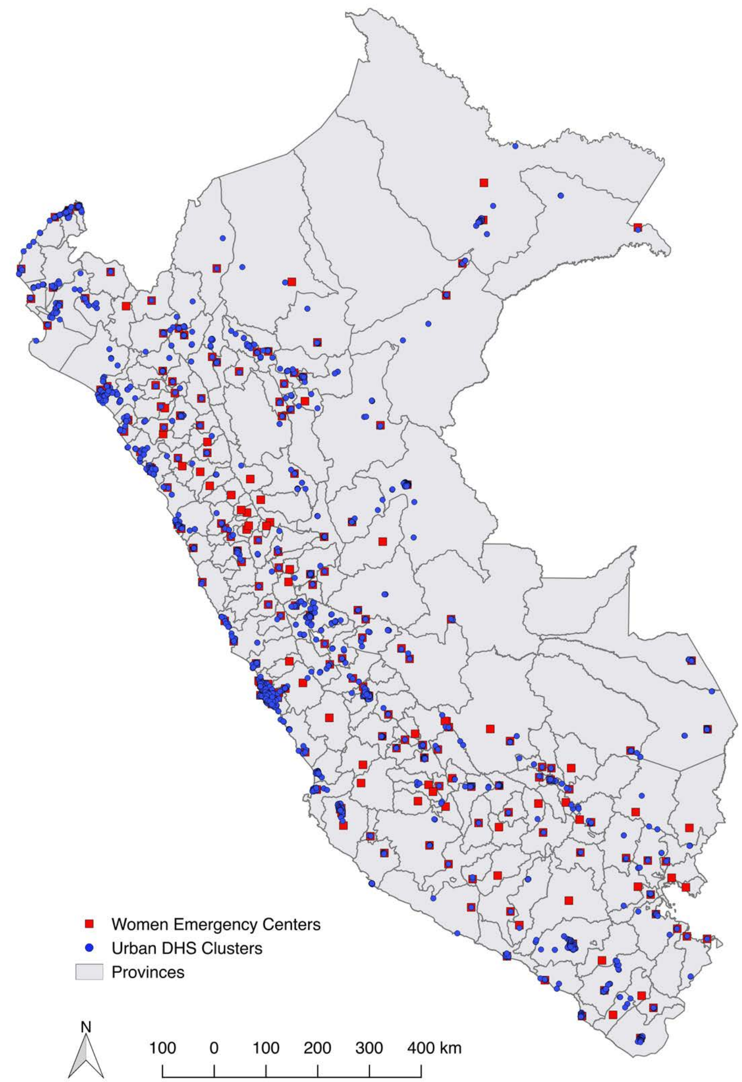


Table A50: School Enrollment Effects by Gender and Grade

Dep. variable	School Enrollment					
	Primary Schools			Secondary Schools		
	Obs.	Mean	WJC within 1km	Obs.	Mean	WJC within 1km
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Results for Schools by Gender						
Log(Female enrollment)	315,221	46.9	0.033***	102,685	84.42	0.009
			(0.010)			(0.017)
Log(Male enrollment)	315,221	49.9	0.021	102,685	90.40	0.067***
			(0.013)			(0.014)
Panel B: Results for Schools by Grade						
Grade 1 enrollment	315,221	15.57	0.019*	102,685	40.97	0.027**
			(0.010)			(0.014)
Grade 2 enrollment	315,221	17.08	0.030***	102,685	38.18	0.034**
			(0.009)			(0.014)
Grade 3 enrollment	315,221	16.55	0.026***	102,685	35.18	0.023
			(0.009)			(0.015)
Grade 4 enrollment	315,221	16.07	0.031***	102,685	31.84	0.043**
			(0.009)			(0.018)
Grade 5 enrollment	315,221	15.70	0.023**	102,685	28.64	0.044**
			(0.009)			(0.019)
Grade 6 enrollment	315,221	14.97	0.033***			
			(0.009)			
School FE			YES			YES
Province*Year FE			YES			YES
Covariates			YES			YES

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

Notes: The dependent variable is the logarithm of enrollment plus one. The independent variables measures the number of WJCs within a 1km Euclidean buffer from the school and presence of WJC in school's district. Standard errors (in parentheses) are clustered at the school level. All regressions are weighted by initial school enrollment level. Covariates include school fixed effects, year fixed effects, year-by-province fixed effects, and a vector of controls of baseline school characteristics interacted with academic year (including initial school enrollment, presence of electricity, presence of piped water, school language (spanish), urban and public school dummy).Source: Peruvian School Census 2006-2014.

Table A51: School Enrollment and Children's School Attendance Effects by Gender

Dep. variable	Currently Attending School					
	Primary Level Children 6-11 y.o.			Secondary Level Children: 12-16 y.o.		
	Obs.	Mean	WJC within 1km	Obs.	Mean	WJC within 1km
	(1)	(2)	(3)	(4)	(5)	(6)
Attendance Results for Children by Gender						
Female attendance	23,575	0.972	0.022**	14,855	0.899	0.036*
			(0.010)			(0.021)
Male attendance	24,545	0.976	0.012	18,474	0.891	0.029*
			(0.008)			(0.015)
District FE			YES			YES
Province*Year FE			YES			YES
Covariates			YES			YES

Robust standard errors in parentheses

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

Notes: The dependent variable is a dummy indicating whether the child is currently attending primary or secondary school. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the child's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample for primary level includes children between the ages of 6 and 11 and the sample for secondary level includes children between the ages of 12 and 16. Covariates include age, gender, household's head years of education, number of children in the household aged 0-18, number of children in the household aged 0-5, number of female adults, number of male adults, rural residence dummy, district fixed effect and province-by-year fixed effect. Source: Peru DHS 2006-2014.

Table A52: The Effect of WJCs on Children's Primary School Attendance - (2006-2014) - Alternative Euclidean Buffers

Dep. variable	**Currently Attending Primary Level**			
Sample	All children 6-11 y.o	All children 6-11 y.o	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the cluster of residence				
WJC within 3km	0.007	0.004	0.015	0.010
	(0.011)	(0.012)	(0.014)	(0.016)
Observations	48,703	48,703	25,391	19,563
Number of districts	1159	1159	485	215
Mean dep. var	0.970	0.970	0.971	0.969
Panel B: WJC in the district of residence				
WJC within 5km	-0.007	-0.004	0.005	0.006
	(0.008)	(0.008)	(0.011)	(0.007)
Observations	48,703	48,703	25,391	19,563
Number of clusters	1159	1159	485	215
Mean dep. var	0.970	0.970	0.970	0.967
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

Robust standard errors in parentheses

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

Notes: The dependent variable is a dummy indicating whether the child is currently attending primary school. The independent variables measures the presence of a WJC within a 3km and 5km Euclidean buffer of the child's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample includes children between the ages of 6 and 11. Covariates include age, gender, household's head years of education, number of children in the household aged 0-18, number of children in the household aged 0-5, number of female adults, number of male adults, rural residence dummy, district fixed effect and province-by-year fixed effect. Source: Peru DHS 2006-2014.

Table A53: The Effect of WJCs on Children's Secondary School Attendance - (2006-2014) - Alternative Euclidean Buffers

Dep. variable	Currently Attending Secondary Level			
Sample	All children 12-16 y.o	All children 12-16 y.o	Only urban clusters	Ever WJC in district
Controls	Standard	District trends	Standard	Standard
	(1)	(2)	(3)	(4)
Panel A: WJC within a distance buffer from the cluster of residence				
WJC within 3km	0.008	0.009	0.016	0.012
	(0.012)	(0.014)	(0.014)	(0.017)
Observations	33,519	33,519	18,266	13,570
Number of clusters	1140	1140	480	215
Mean dep. var	0.895	0.895	0.916	0.908
Panel B: WJC in the district of residence				
WJC within 5km	-0.011	-0.001	-0.001	-0.003
	(0.013)	(0.015)	(0.016)	(0.019)
Observations	33,519	33,519	18,266	13,570
Number of clusters	1140	1140	480	215
Mean dep. var	0.896	0.896	0.913	0.904
District FE	YES	YES	YES	YES
Province*Year FE	YES	YES	YES	YES
Covariates	YES	YES	YES	YES

Robust standard errors in parentheses

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

Notes: The dependent variable is a dummy indicating whether the child is currently attending secondary school. The independent variables measures the presence of a WJC within a 3km and 5km Euclidean buffer of the child's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample includes children between the ages of 12 and 16. Covariates include age, gender, household's head years of education, number of children in the household aged 0-18, number of children in the household aged 0-5, number of female adults, number of male adults, rural residence dummy, district fixed effect and province-by-year fixed effect. Source: Peru DHS 2006-2014.

Table A54: Domestic Violence Effects by Age, Education Level and Type of Domestic Violence - (2006-2014)

Dep. variable	Domestic violence in last 12 months					
	Obs.	Mean	WJC within 1km	Obs.	Mean	WJC in the district
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Results for Women by Age						
Women 15-33 years old	31,442	0.349	-0.004	47,136	0.355	-0.013
			(0.018)			(0.016)
Women 34-49 years old	32,886	0.402	-0.038***	49,380	0.418	-0.038***
			(0.019)			(0.018)
Panel B: Results for Women by Education Level						
No education	2,254	0.374	-0.102	3,380	0.374	0.134
			(0.110)			(0.119)
Primary Level	22,198	0.402	-0.035	32,844	0.390	-0.025
			(0.026)			(0.024)
Secondary Level	24,989	0.415	-0.018	37,834	0.394	-0.042**
			(0.015)			(0.016)
Higher Level	14,033	0.331	-0.029*	21,435	0.316	0.013
			(0.016)			(0.025)
Panel C: Results for Women by Type of Domestic Violence						
Less severe violence	64,366	0.376	-0.029***	96,560	0.373	-0.018
			(0.010)			(0.012)
Severe violence	64,366	0.171	-0.014*	96,560	0.171	-0.006
			(0.009)			(0.009)
Sexual violence	64,366	0.092	0.001	96,560	0.092	-0.007
			(0.006)			(0.007)
District FE			YES			YES
Province-Year FE			YES			YES
Covariates			YES			YES

Robust standard errors in parentheses

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

Notes: The dependent variable is a dummy indicating whether the women suffered any type of domestic violence (less severe, severe or sexual violence) during the last 12 months. The independent variables measures the presence of a WJC within a 1km Euclidean buffer of the women's cluster of residence. Robust standard errors (in parentheses) are clustered at the district level. The sample includes women between the ages of 15 and 49. Women who were never married or never cohabited are excluded from the sample. Covariates include age, age at first marriage, number of children, years of education, number of household members, number of households in the dwelling, maritual status (married=1), rural residence dummy, district fixed-effects and province-by-year fixed effects. Source: Peru DHS 2006-2014.

Table A55: Relationship between WJCs within 1km rollout and pre-program school enrollment

	Schools matched to WJC within 1km, Pre-WJC period					
	△ Primary School Enrollment			**△ Secondary School Enrollment**		
	(1)	(2)	(3)	(4)	(5)	(6)
	△98-00	△98-05	△98-10	△98-00	△98-05	△98-10
WJC within 1km in 2002	6.830			-1.800		
	(5.817)			(16.789)		
WJC within 1km in 2003	-6.794			33.105		
	(7.941)			(23.978)		
WJC within 1km in 2004	-7.294			-21.079		
	(6.470)			(14.668)		
WJC within 1km in 2005	-29.836			-89.897***		
	(45.571)			(12.935)		
WJC within 1km in 2006	-6.567			-14.840		
	(4.682)			(15.354)		
WJC within 1km in 2007	0.325	2.409		-13.416	0.843	
	(3.406)	(2.376)		(13.549)	(4.072)	
WJC within 1km in 2008	-3.087	3.469		-10.913	5.111	
	(3.895)	(2.436)		(13.136)	(4.853)	
WJC within 1km in 2009	-	-		-	-	
WJC within 1km in 2010	-4.750	3.121		-11.608	0.287	
	(3.469)	(2.286)		(12.931)	(4.210)	
WJC within 1km in 2011	-5.827*	2.232		-19.936	-0.865	
	(3.395)	(2.284)		(13.055)	(3.999)	
WJC within 1km in 2012	3.255	0.961	0.443	-16.233	-5.425	-1.862
	(6.375)	(2.550)	(1.381)	(13.172)	(4.342)	(1.982)
WJC within 1km in 2013	-8.550**	1.150	0.258	-12.097	-3.199	-0.216
	(3.613)	(2.638)	(1.699)	(13.086)	(4.361)	(2.454)
WJC within 1km in 2014	-1.605	3.148	1.170	-14.980	-0.862	-0.394
	(3.538)	(2.485)	(1.295)	(13.735)	(4.507)	(2.695)
Observations	2,190	6,372	6,157	1,115	3,400	3,540
Number of schools	1179	1247	678	607	710	404
Year FE	YES	YES	YES	YES	YES	YES
P-value joint test	0.043	0.783	0.848	0.000	0.375	0.729

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

Notes: Standard errors that allow for clustering at the school level are reported in parentheses. The dependent variable in columns 1-6 is the change in the level of school enrollment. The observations correspond to three windows of pre-WJC period for each school. All regressions include year fixed-effects.

Table A56: Relationship between WJCs in the district rollout and pre-program school attendance

	Districts matched to WJC locations, Pre-WJC △1996-2005					
	△ Primary School Attendance			**△ Secondary School Attendance**		
	(1)	(2)	(3)	(4)	(5)	(6)
	△96-00	△96-05	△96-10	△96-00	△96-05	△96-10
WJC in the district in 2002	0.002			-0.071		
	(0.036)			(0.060)		
WJC in the district in 2003	-0.056			0.032		
	(0.060)			(0.062)		
WJC in the district in 2004	-0.005			0.041		
	(0.036)			(0.082)		
WJC in the district in 2005	0.016			-0.051		
	(0.036)			(0.060)		
WJC in the district in 2006	-0.057			-0.078		
	(0.052)			(0.087)		
WJC in the district in 2007	-0.031	0.010		-0.065	0.033	
	(0.040)	(0.015)		(0.109)	(0.051)	
WJC in the district in 2008	-0.011	0.012		-0.008	-0.013	
	(0.039)	(0.014)		(0.098)	(0.046)	
WJC in the district in 2009	-	-	-	-	-	-
WJC in the district in 2010	-0.026	0.011	-0.009	-0.062	0.015	-0.013
	(0.040)	(0.014)	(0.008)	(0.071)	(0.045)	(0.028)
WJC in the district in 2011	-0.034	-0.002	-0.016	0.030	0.008	-0.029
	(0.041)	(0.016)	(0.009)	(0.067)	(0.036)	(0.024)
WJC in the district in 2012	0.012	0.020	0.006	0.022	-0.040	-0.052
	(0.039)	(0.014)	(0.008)	(0.076)	(0.042)	(0.041)
WJC in the district in 2013	-0.008	0.006	-0.012	0.055	0.002	-0.015
	(0.049)	(0.021)	(0.011)	(0.101)	(0.055)	(0.030)
WJC in the district in 2014	-0.073	0.020	-0.007	-0.152	-0.049	-0.030
	(0.076)	(0.054)	(0.038)	(0.125)	(0.074)	(0.054)
Observations	90	186	228	90	184	226
Number of districts	90	106	102	90	106	102
Year FE	YES	YES	YES	YES	YES	YES
P-value joint test	0.000	0.676	0.222	0.000	0.712	0.778

Robust standard errors in parentheses

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

Notes: Standard errors that allow for clustering at the district level are reported in parentheses. The dependent variable in columns 1-6 is the change in the level of school attendance at the district level. The observations correspond to three windows of pre-WJC period for each district. All regressions include year fixed-effects.

Table A57: Relationship between WJCs within 1km rollout and four windows of pre-program standardized test scores (2nd grade - Primary School)

	Schools matched to WJC within 1km Pre-WJC period			
	△ **Standradized Test Scores**			
	(1)	(2)	(3)	(4)
	△07-09	△07-10	△07-11	△07-12
WJC within 1km in 2011	0.002			
	(0.034)			
WJC within 1km in 2012	0.045	-0.009		
	(0.046)	(0.029)		
WJC within 1km in 2013	-0.023	-0.029	-0.001	
	(0.066)	(0.038)	(0.034)	
WJC within 1km in 2014	0.042	-0.019	-0.009	-0.025
	(0.060)	(0.039)	(0.033)	(0.034)
Observations	1,565	1,675	1,068	734
Number of schools	821	600	292	168
Year FE	YES	YES	YES	YES
P-value joint test	0.670	0.895	0.828	

Robust standard errors in parentheses

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

Notes: Standard errors that allow for clustering at the school level are reported in parentheses. The dependent variable in columns 1-4 is the change in standardized reading and math z-scores at the school level. The observations correspond to the pre-WJC period for each school, it includes all schools which are located within 1km of a WJC which opened between 2010-2014, 2011-2014, 2012-2014 and 2013-2014. All regressions include year fixed-effects.

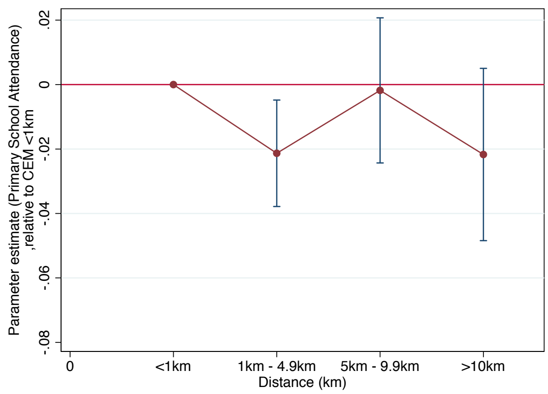
Table A58: Relationship between WJCs in the district and four windows of pre-program domestic violence

	Districts matched to WJC locations, Pre-WJC period			
	△ Domestic violence in last 12 months			
	(1)	(2)	(3)	(4)
	△2000-2005	△2000-2008	△2000-2010	△2000-2013
WJC in the district in 2007	-0.021			
	(0.088)			
WJC in the district in 2008	-0.001			
	(0.087)			
WJC in the district in 2009	-	-		
WJC in the district in 2010	-0.018	-0.006		
	(0.082)	(0.035)		
WJC in the district in 2011	0.013	0.007	-0.026	
	(0.078)	(0.034)	(0.042)	
WJC in the district in 2012	-0.025	0.060	-0.011	
	(0.093)	(0.041)	(0.041)	
WJC in the district in 2013	0.041	0.013	0.005	
	(0.098)	(0.061)	(0.050)	
WJC in the district in 2014	0.071	0.119**	-0.036	-0.016
	(0.074)	(0.078)	(0.042)	(0.020)
Observations	105	161	239	128
Number of districts	78	99	83	38
Year FE	YES	YES	YES	YES
P-value joint test	0.416	0.103	0.433	-

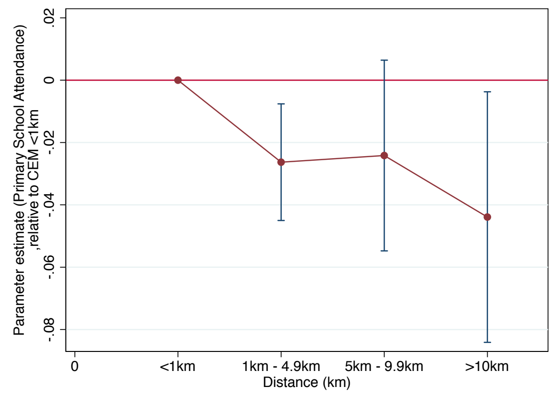
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors that allow for clustering at the district level are reported in parentheses. The dependent variable in columns 1-4 is the change domestic violence at the district level. The observations correspond to the pre-program period of the WJC rollout for each district, it includes all districts that ever had a WJC which opened between 2006-2014, 2009-2014, 2010-2014 and 2013-2014. All regressions include year fixed-effects.

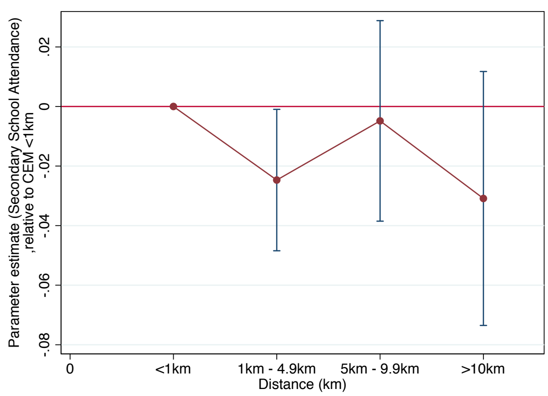
Figure A18: The effect of distance to closest WEC center on school attendance and domestic violence



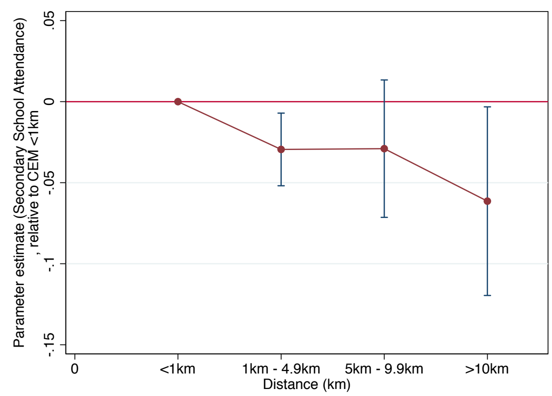
a. Primary School Attendance - All clusters



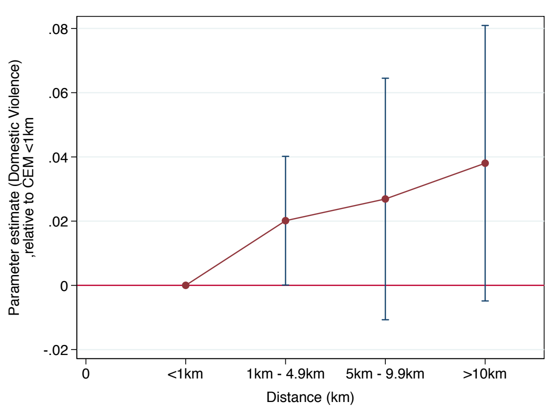
b. Primary School Attendance - Urban clusters



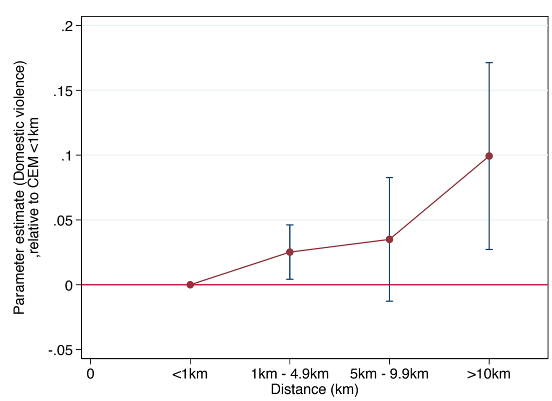
c. Secondary School Attendance - All clusters



d. Secondary School Attendance - Urban clusters



e. Domestic violence - All clusters



f. Domestic violence - Urban clusters

Notes: This graphs plot the coefficients and 95% confidence intervals from a regression of the outcomes (primary school attendance, secondary school attendance and domestic violence) on four distance categories for minimum distance to a WEC center: less than 1 km to a WEC, which is our reference category; between 1 km and 4.9km km; between 5 km and 9.9 km; and greater than 10km to a WEC center.