Essays in Applied Microeconomics and Development Economics

by

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Dissertation submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy in the Department of Economics
in the Graduate School of Duke University
2019

Abstract

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Abstract

This dissertation brings new causal evidence on three topics in development economics, economic demography, and political economy. In the first chapter, I study how aggregate income shocks affect the health and survival of children. I focus on the Peruvian coca industry to exploit a natural experiment. I assess the impacts of plausibly exogenous changes in the price of the coca crop on child mortality in coca-producing areas in a difference-in-difference framework. I find that child health is vulnerable to income losses. In the second chapter, I study how exposure to mass media affect household choices. In particular, I analyze the impacts of exposure to commercial television on fertility in the 1950s in the United States. I tackle this question empirically by exploiting variation in the introduction of television across time and space. I find that television is associated with fewer births. Finally, in the third chapter, I study the consequences of electing low-quality politicians to public office for the provision of public goods. I evaluate the impacts of electing criminal politicians in Peru using a regression-discontinuity design. The analysis suggests that electing a criminal politician is associated with increases in public expending; however, I show that pre-existing levels of expenditure drive these effects.

To my grandmother.

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1

Introduction

This dissertation explores three topics in development economics, economic demography, and political economy.

In the first research chapter, I study how aggregate income shocks affect child mortality. Particularly in developing countries, child health may be vulnerable to aggregate income shocks as credit constraints and other market imperfections may prevent households from fully smoothing consumption and health investments. In this chapter, I bring new evidence by exploiting quasi-exogenous variation in the price of coca leaves—the main input for cocaine production—induced by an anti-drug policy to study how sharp decreases in coca revenues affect child mortality in producing sites. The identification strategy relies on an abrupt decline in prices to compare survival rates across cohorts and areas with different levels of baseline coca cultivation in a difference-in-difference framework.

I document important increases in mortality: for the average coca district, the 50 percent price drop associated with the policy caused an effect equivalent to 6-11 percent increase in under 5 mortality. I establish that deaths occur both in-utero and during the first years of life. To do this, I use direct mortality records and a "missing children" approach that infers survival rates by comparing relative cohort sizes in census and survey data. Using data from before and after the shock, I find that households increase their labor supply to cope with the price drop; however, reductions in health investments make health vulnerable to income losses.

This research contributes to a literature on aggregate income shocks and health and a growing body of research on illegal markets and law enforcement.

In the second research chapter, I bring new evidence on how exposure to mass media affect family choices. To do this, I focus on the 1950s U.S. as a case study. Scholars have studied the underlying causes of the fertility decline that followed the post-World War II baby boom in the United States for decades. Traditional explanations consider labor market conditions, access to education, and the availability of contraceptive methods.

I exploit idiosyncratic variation in the geographic and temporal availability of television's commercial broadcasting (the 1940s and 1950s) and find that it negatively impacted fertility. The effects are economically important: television is associated with 1.3 fewer births per thousand women in childbearing age. Recent studies beyond the laboratory setting show that television content may affect the behavior of families, including fertility choices. Moreover, historical accounts document that TV represented a discrete change in the type of leisure available in the household, which may have affected the opportunity cost of non-leisure activities (e.g., childrearing). The latter is consistent with early time-use studies and evidence on the fast and broad adoption of the new technology.

This study contributes to a literature on the impacts of exposure to mass media as well as a body of research on the determinants of demographic changes. This piece also motivates future research. I discuss the use of microdata from the 1955 Growth of American Families Survey and recently released samples from the 1950 and 1960 U.S. censuses to gain more knowledge on the causal pathways connecting mass media exposure and family choices.

Finally, in the third research chapter, I study the consequences of electing criminal politicians—individuals previously sentenced for fraud, corruption, and others—to public office in Peru. An important body of evidence shows that the identity and characteristics of political leaders matter for the type and efficacy of implemented policies. Another strand of the literature documents the long-lasting effects of low-quality institutions on economic development.

To this end, I assemble a unique dataset with information on the characteristics of every politician running for public office in Peru, including (verified) criminal records and political histories, matched to detailed government expenditure records and funding sources.

Naive comparisons across municipalities in a close-election framework (regression-discontinuity design) would suggest that criminal politicians expend considerably more public funds than non-criminals. This is especially the case for infrastructure projects. A closer look, however, shows that criminal politicians are systematically more likely to be elected in areas with pre-existing high levels of public expending. This finding motivates a closer look into political selection mechanisms.

2

Price Shocks and Child Mortality: Evidence from Anti-Drug Policies in Peru

2.1 Introduction

Motivated by disparities in wealth and health, influential studies have documented a negative association between fluctuations in aggregate income and child mortality in developing countries (e.g. Baird et al., 2011; Pritchett and Summers, 1996). Child mortality is high in poor nations, where income is volatile. In rich countries, less than 1 percent of all deaths occur to children under the age of five, compared to approximately 30 percent in poor countries (Cutler et al., 2006). Moreover, poor countries exhibit greater income volatility and more frequent and severe aggregate income shocks (Koren and Tenreyro, 2007).

Despite considerable scientific progress, the causal effect of aggregate income shocks on child and infant mortality in the developing world is still elusive. Most empirical evidence comes from studies using economic recessions or economic cycles as sources of variation for aggregate income. This avenue of research has generated mixed results, yielding positive, negative, and null relationships (for a review, see Ferreira and Schady, 2009).[1] In particular, these studies face two challenges. First, it is difficult to credibly separate the effects of income on mortality from anticipated behavioral responses affecting the composition of births, such as selective fertility. Second, economic cycles and crises may affect a multiplicity of causal pathways, such as the quantity and quality of the supply of health care in addition to changes in the demand for health inputs. As a result, it is challenging to unpack the key mechanisms at play.[2]

In this paper, I exploit a natural experiment to study how a negative price shock in the Peruvian coca industry affected mortality rates among children in coca-producing cites. Coca is the primary input for cocaine production, and its cultivation only takes place in the eastern side of the Andean region due to agronomic conditions (FAO, 2007). As such, coca production is the objective of various anti-drug policy interventions that aim to curb the supply of cocaine. This paper exploits variation in the price of coca induced by a program promoted by the U.S. that sought to cut the supply chain of cocaine by enacting a shoot-down policy against narco-airplanes transporting exports of coca from Peru to Colombia. The policy caused an abrupt 50% drop in the price of coca leaves in Peru. By analyzing this context, this paper aims to contribute to a literature on aggregate income shocks and child mortality, as well as a strand of research on illegal markets and law enforcement.

[1] In addition, Pérez-Moreno et al. (2016) and Van den Berg et al. (2017) provide recent and useful overviews of the literature.

[2] Other aggregate shocks used in the literature may affect health through additional causal pathways. For instance, natural disasters may affect health and mortality directly. Shocks to the agricultural sector may compromise the availability of food for rural households. Droughts and floods may have implications for the overall disease environment.

Child mortality, or health more generally, can be affected by two main mechanisms when coca prices fall; the overall effect is theoretically ambiguous. The production function of health is assumed to depend on two arguments: consumption of health-promoting goods (e.g. nutritious food and medicines) and time allocated to health-promoting activities (e.g. taking children to preventive health visits, collect clean water, or caregiving more broadly). When coca prices drop, households may reduce consumption of health-promoting goods, negatively affecting child health (income effect). However, if the price drop is translated into a lower opportunity cost of time, caregivers may increase their allocation of time to health-promoting activities (price effect). In the end, the total effect of the coca price drop on health and mortality is theoretically ambiguous.[3]

Using variation of prices over time and of baseline levels of coca cultivation across districts, I employ a difference-in-difference design that aims to avoid key threats to identification. The empirical specification compares cohorts of children born in years of high prices with cohorts born in years of low prices and across areas with different levels of baseline coca cultivation. A salient challenge is being able to isolate the effect of the price shock on mortality from changes in the composition of cohorts, for instance, driven by selective fertility. I take advantage of the abrupt drop in coca prices to credibly isolate the effects from changes in selective fertility and other responses. The analysis compares the first cohort exposed to the shock—and conceived the year before the shock—with adjacent, older cohorts based on information on year and district of birth.

[3] In addition, maternal exposure to stressful events may affect fetal health through the release of hormones that can be harmful in high concentrations (Beydoun and Saftlas, 2008; Navara, 2010).

I overcome the challenge of not observing reliable vital records in developing countries by using census data to infer mortality rates from relative cohort sizes (i.e., "missing" individuals). I use the full 2007 Peruvian Population Census to construct population counts by district and year of birth 13 years after the implementation of the policy. The measure of (cumulative) mortality I use has several advantages, including capturing fetal deaths and delayed mortality (Jayachandran, 2009; Miller and Urdinola, 2010).

I find that the 50 percent price cut associated with the policy caused a reduction in cohort size of 0.43 to 0.53 percent for the average coca district, with the effects being particularly strong for males. In terms of under 5 mortality, this difference in cohort size represents an increase of 6 to 11 percent.[4] The effect is particularly large for boys: 0.81 to 0.89 percent decrease in cohort size for the average coca district. The pattern of higher mortality among boys is consistent with the survival disadvantage of males relative to females both in-utero and in early life (Drevenstedt et al., 2008; Navara, 2010).

Moreover, I determine that deaths take place both in-utero and during the first years of life. Using the Peruvian Demographic and Health Surveys (DHS), I find that the shock is first associated with an increase in miscarriages, especially of baby boys, as suggested by a "missing babies" approach. Although the weakest fetuses were lost before birth, newborns affected by the price drop also tend to be smaller, a marker of impoverished health. After birth, infants face higher mortality rates as well.

An analysis of the mechanisms at play suggests that both income and price effects explain the increase in mortality, contrary to recent evidence

[4] Mortality under age five is a sound benchmark as most deaths are happening before that age. This is shown in later sections of the paper.

suggesting that price effects undo income effects (e.g. Miller and Urdinola, 2010). Using general household surveys collected before and after the shock, I conclude that the downturn is associated with a reduction in overall household expenditure, including food and health. In addition, to cope with the decrease in income, increases in labor supply are in place. Individuals in school age (6 to 17 years old) and adult women supply more labor, which may crowd out time-intensive health investments. This is consistent with poor individuals supplying more labor when wages are low to compensate for losses in income (Jayachandran, 2006).

The main results are unlikely to be driven by factors other than the price shock. First, the main analysis exploits and abrupt change in coca prices and defines exposure based on district and year of birth. Thus, ex-post migration is unlikely to play any role. Moreover, I limit the analysis to cohorts 0-3 and 0-2 years of age at the time of the shock. This is, the exposed cohort was born during the price shock and (mostly) conceived the year before. This limits the influence of selective fertility in the analysis. The comparison with adjacent, older cohorts ensures that both treatment and control would have very similar life cycle experiences had it not been because of the price shock. Second, the results are consistent across a number of specifications and robustness checks. These include controlling for coca-specific time trends and a large set of baseline covariates interacted with year fixed effects, arbitrarily changing the cohorts used as the control, careful examination of the timing of the effects, among others.

The main contribution of this paper is twofold. First, it contributes to a literature on aggregate income shocks and child mortality by addressing endogeneity concerns using plausibly exogenous variation in the price of coca

(Ferreira and Schady, 2009). Moreover, contrary to papers studying economic crises, this study pins down the effects separately from changes in the public sector provision of health services by using a price shock to a non-taxable commodity. Also, the abruptness of the price change is used to carry the main analysis over a narrow window of time and prevent fertility and other behavioral responses from contaminating the results. Using a missing children approach, this paper documents that mortality takes place both in-utero and after birth. The main results suggest that the income effect and price effect have the same sign. This is consistent with poor, developing settings and arguably different from quasi-experimental evidence from more developed areas where price effects may compensate for income losses (Miller and Urdinola, 2010). More broadly, this study relates to a literature on early life shocks (Almond and Currie, 2011; Almond et al., 2017; Currie, 2009; Currie and Almond, 2011; Prinz et al., 2018) and the more general, two-way relationship between economic development and health (Deaton, 2007; Strauss and Thomas, 1998, 2007). This paper also relates to a literature on natural resources. Although natural resources are salient in developing countries, where health is more vulnerable, this literature has paid limited attention to how resource booms and busts affect health of vulnerable populations (for a review, see Aragón et al., 2015)

Second, this paper establishes that economic downturns in drug industries may have negative effects on the wellbeing of vulnerable populations. This adds to a growing body of work documenting the also negative consequences of drug booms. Angrist and Kugler (2008) document that a growth in opportunities in the coca sector in Colombia is related to a moderate increase in earnings among Colombian households accompanied by an increase in violence levels in the Colombian armed conflict. Sviatschi (2018) documents that increases in prices of coca in Peru are associated with moderate income gains, higher school dropouts, and criminal human capital accumulation. Dammert (2008) analyzes the same setting than this paper and establishes that households respond with increased child labor during the economic downturns of the coca industry. Putting this evidence together, these studies call for careful analysis of the welfare implications of anti-drug policies and drug trade, as well as new policy designs.

The paper is organized as follows. Section 2 describes the setting and coca production in Peru. Section 3 describes the natural experiment and motivates the use of exogenous sources of variation. Section 4 presents the main datasets and the empirical strategy. Results and robustness checks are presented in Section 5. Section 6 concludes.

2.2 Coca Cultivation in Peru

Coca (*Erythroxylum coca*) requires specific environmental conditions for cultivation. As such, virtually the entire supply of cocaine in the world can be traced back to only three countries: Bolivia, Colombia, and Peru (UNODC, 2007). The plant is native to South America, where the tropical and subtropical climates of the eastern side of the Andes Mountains provide suitable conditions for its growth. These conditions include annual rainfall between 1,000 to 2,100 millimeters a year, temperatures between 17 to 23 degrees Celsius, very bright sunlight intensity, and altitudes between 600 and 2,000 meters above the sea level with slopes of around 20 degrees (FAO, 2007; UNODC, 2003).

Peru was the largest producer of coca in the world in the 1980s and early

1990s, but there was substantial variation in coca cultivation across areas in the country (see World Drug Reports, e.g. 2002). Figure 2.1 shows districts by levels of coca cultivation as of 1994 grouped in quintiles.[5] Out of 1777 districts, 193 produce coca.[6] It is easy to notice that most coca districts are located to the right of an almost perfect diagonal line: coca districts are located in the eastern side of Andes Mountains, where the climate is suitable for cultivation. Sviatschi (2018) establishes that cultivation levels are positively and strongly related to climatic conditions favorable for coca farming. Bolivia and Colombia also exhibit spatial concentration of coca cultivation in particular geographies (see Illicit Crop Monitoring Reports for each country, e.g. 2002; 2005a). As I explain later, I will use variation in cultivation levels in the identification strategy.

Coca-producing sites are generally undeveloped, and estimates suggest that they depend heavily on coca as an economic activity. Over 200,000 households had an economy based on coca farming or related activities in 1995, the year in which the anti-drug policy studied in this paper was enacted (DEVIDA, 2004). Studies on the most important coca-producing regions estimate that around half of households' income comes from coca farming (Bedoya, 2003; DEVIDA, 2013). It has also been suggested that coca crowds out other sources of income (Pedroni and Yepes, 2011).[7] In addition, coca

[5] Districts are the smallest administrative unit in Peru. The average district has a population of around 12.4 thousand individuals according to the 1993 Population Census.

[6] The actual number of total districts in Peru is slightly higher than 1777. The difference arises because I collapsed less than five percent of all districts into time-consistent geographical units for the analysis. This is needed as I use various data sources collected at different points in time, and some district boundaries change. Details are provided in the data section below.

[7] Note that these are conservative estimates as they were calculated with data collected after 1995, when production of coca in Peru had sharply decreased, and the expected revenues from coca activities were lower than before 1995.

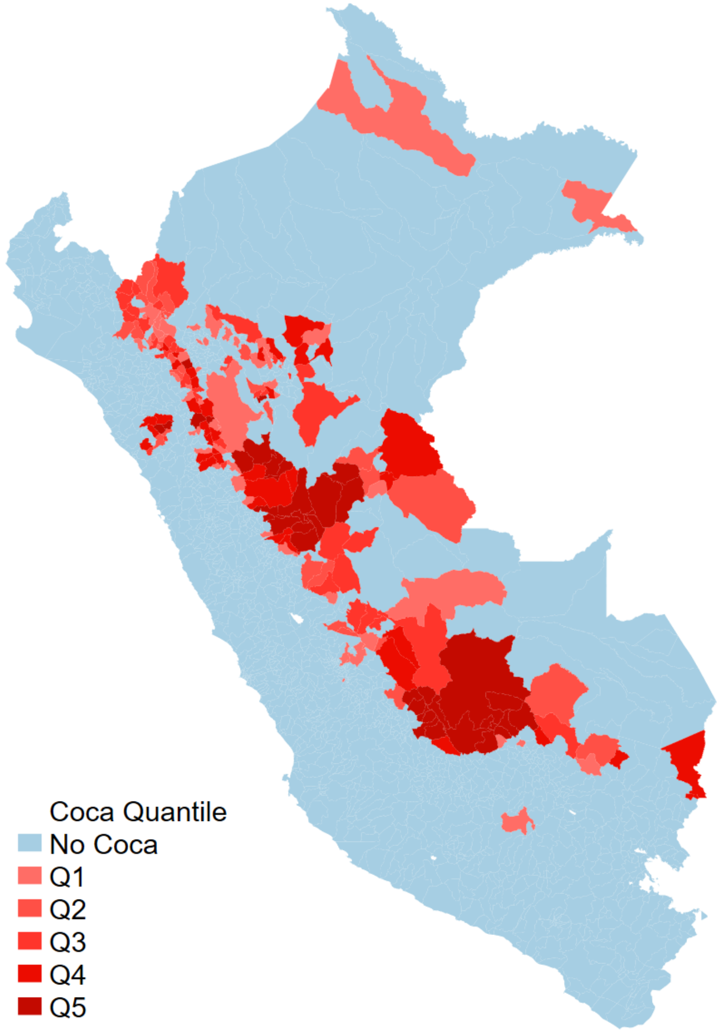

FIGURE 2.1: Coca cultivation in 1994

districts are worse off. According to the 1993 Population Census, coca districts have smaller populations, with over 70 percent of them residing in rural areas, and are less educated. Moreover, child mortality is high, reaching 6.7 percent in rural areas, and access to public health services is limited (INEI, 2001).

Coca cultivation and harvest are the first steps in the supply chain of cocaine production.[8] Coca has been produced in Peru since pre-Inka times for ceremonial and religious purposes. People living in the highlands use it to fight fatigue and altitude sickness. Since the boom of cocaine in the 1970s, however, the legal market for coca became very small relative to its illicit counterpart. Studies from the early 2000s estimated that 90% of the total amount produced of coca is sold to the illicit market (UNODC, 2003).

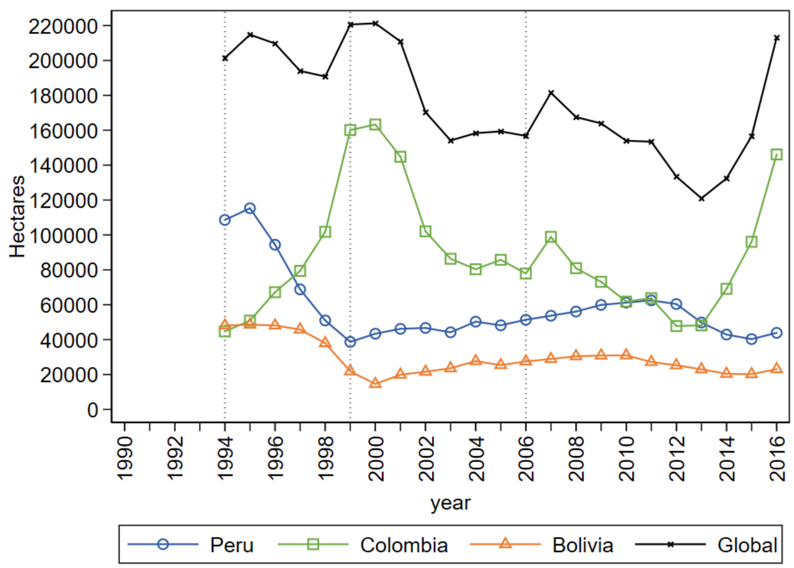

FIGURE 2.2: Coca Production by Country

Small-scale farmers produce coca as a source of monetary income. Coca plots tend to be under one hectare. The first harvest takes place after one year. Then the bushes can be regularly harvested 3-5 times a year for about 20 years. The harvest consists of picking the leaves carefully without damaging the plant's bulbs. This process is very labor intensive and needs to be done in a short period of time (about two weeks) while the leaves are "ripe". After harvest, the leaves are dried under the sun and sold to inter-

[8] This and the following paragraphs in this section draw heavily from (DOJ, 1991; García Díaz and Stöckli, 2014; UNODC, 2005b)

mediaries for cocaine production. One hectare can produce about 2.2 tons of sun-dried coca leaves a year (World Drug Report 2010). In terms of 2009 prices, one hectare produces 6600 dollars in revenues (over two times the minimum wage). Importantly, coca is not part of the nutritional intake of households. In fact, studies show that coca lacks nutritional content (Castro de la Mata and Zavaleta Martinez Vargas, 2009; Zavaleta Martinez Vargas, 2012; Zavaleta Martínez Vargas et al., 2016).

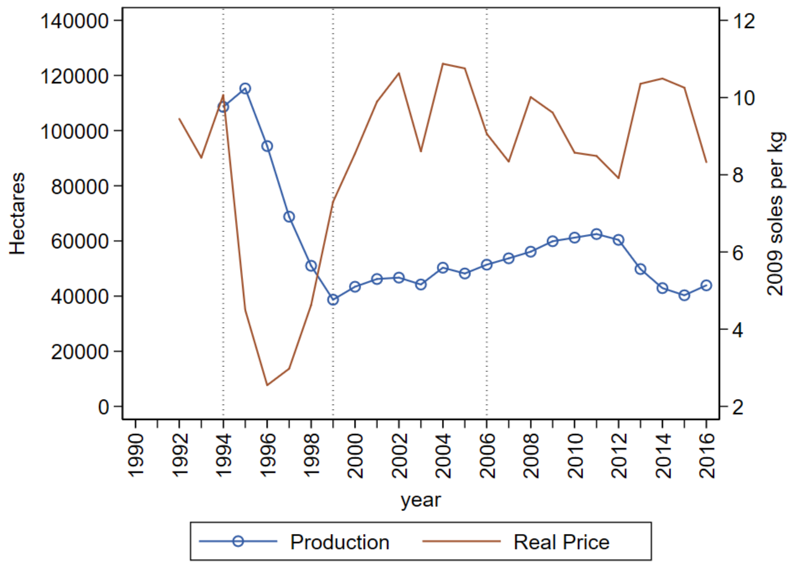

FIGURE 2.3: Coca production and real price in Peru

Sun-dried coca leaves are then transformed into coca paste and exported. Farmers sell their coca leaves to middlemen who higher local "cooks" to transform the leaves into coca paste or cocaine base, intermediate goods before the final product. This step requires "micro labs" or maceration pits, and some additional inputs, such as kerosene.[9] The process drastically reduces the volume and weight of coca leaves: estimates for the Peruvian coca

[9] Farmers own some of these, but it is not clear how many have vertically integrated this step.

industry suggest that around 450 Kg of sun-dried coca leaves are needed to produce a single kilogram of cocaine base (World Drug Report 2010).[10] This process is done locally. Then coca paste, with a much smaller volume is smuggled through roads and rivers to centers, such as the Uchiza district, where small narco airplanes would ferry coca paste from Peru into Colombia (IDL, 2012).

2.3 Price Shocks in the Peruvian Coca Industry: A Natural Experiment

The so-called "air-bridge denial" policy changed the structure of cocaine markets in South America in 1995, and I use it as a source of quasi-exogenous variation in this paper (Angrist and Kugler, 2008; Dammert, 2008).

Up to 1994, Peru specialized in producing coca. Coca would be ferried to Colombia in the form of coca paste, usually using small narco-airplanes taking off from improvised runways in the Amazon jungle. Once in Colombia, it would be transformed into cocaine and exported to final consumer markets. Peru was the largest producer of coca in the world, cultivating three times more coca than Colombia. Coca production in Peru reached levels of around 120 thousand hectares in the 80s and early 90s. Colombia and Bolivia produced about 40 thousand hectares each (see figure 2.2).

In 1995, an aggressive policy that aimed to cut the supply chain of cocaine in the Andean region represented an abrupt and negative demand shock to the Peruvian coca industry. Among other efforts, the Peruvian and Colombian Airforce implemented a shoot-down policy of aircrafts suspicious of fer-

[10] This was estimated in 1994, which is relevant for the period of analysis of this paper. Later estimates suggest increases in productivity in the 2000s after Peru became also a cocaine producer and direct exporter to final consumer markets.

rying coca and that did not follow commands of forced landing. The operation was conducted in cooperation with the U.S. government, which provided funding, aircrafts, and ground radars. US cooperation was authorized on December 8th, 1994, by President Bill Clinton's Presidential Determination and its Memorandum of Justification (CIA, 2008). In general, this strategy followed from the shift in US interdiction and seizure efforts from Central America to the source countries and the overall militarization of the war on drugs (Zirnite, 1998). Although similar strategies existed in the 80s and 90s, none had the intensity of efforts carried out in 1995 and later years.

For Peru, this policy generated an abrupt collapse in the price of sun-dried coca leaves. Figure 2.3 shows the evolution of the price in real terms over time. In a single year, from 1994 to 1995, the price dropped about 50 percent. The price remained low until 1999 when an airplane of the Peruvian Airforce suffered an accident in February of the year. This raised concerns about how the safety protocols where being implemented and the policy weakened. In 2001, the program is suspended after a civil airplane with a US missionary and her daughter was shoot down by mistake. The policy was associated with a sizable reduction in coca production in Peru and an increase in Colombia, leaving the total amount produced between 1994 and 2001 virtually unchanged (see figure 2.2).

2.4 Data and Empirical Strategy

I exploit variation in the price of coca leaves generated by a counter-narcotic policy to measure its impact on child survival and health. The policy significantly reduced farm gate prices of coca leaves during 1995-1999, with an unprecedented initial drop of 50% from 1994 to 1995. This period is known as the “crisis of coca”. I compare cohorts born in years of high prices with cohorts born in years of low prices across districts with different levels of coca suitability. The identification strategy focuses on the effects of price shocks during the first year of life. Life is fragile and mortality high both in-utero and during the first year of life. In additional specifications I allow for effects at different ages.[11]

2.4.1 Data

The measure of coca revenues combines prices of sun-dried coca leaves in Peru and cultivation levels of coca crops by districts measured *before* the price collapse. Yearly prices are collected by the United Nations Office on Drugs and Crime and reported in U.S. dollars.[12] I transform this price sequence to 2009 real Nuevos Soles for the analysis.[13] Coca cultivation is obtained from the 1994 Agricultural Census. I construct cultivation levels by district measured as the total area covered with coca crops in thousands of hectares. This is the measure of coca intensity of this paper. Note that coca intensity is measured in 1994, while the price drop took place in 1995.

I use the full 2007 Peruvian Population Census to construct cohort sizes by year-and-district-of-birth cells. Cohort sizes will be used as an indirect measure of cumulative survival (mortality) 13 years after the price shock (Jayachandran, 2009; Miller and Urdinola, 2010). The empirical specification

[11] Variations of the empirical specification that allow for effects before and after the first year of life corroborate that shocks at other ages have little to null effects.Econometrically, we have more confidence in the assignment of the treatment at the time of birth using district-of-birth and year-of-birth than years after birth when exact locations are not known.

[12] Price data are available at the month-valley level since 1998. Unfortunately, I cannot use these data because the identification strategy is based on the quasi-exogenous drop of prices in 1995. Prices across regions are highly correlated.

[13] Exchange rate and price index data are from the Central Bank of Peru.

below establishes the conditions under which cohort size can be interpreted as cumulative survival.

In addition, I use Peru's DHS to draw detailed data on births and direct measures of child mortality. To have wider coverage over coca and non-coca districts, I pool information from the two earliest waves after the price drop: 1996 and 2000. DHS records information on birth histories, child mortality, maternal characteristics, and women and children's health for nationally representative samples of women ages 15-49. I use these surveys to infer how the shock is associated with mortality in-utero and after birth.

Finally, I use the 1994 and 1997 Peruvian Living Standards Measurement Surveys (LSMS)—which cover precisely the period before and after the price shock—to characterize how households expenditure and labor decisions were affected.

District boundaries were mostly constant over the period of analysis. However, to ensure a correct match across data sources, all districts were transformed to the 1993 administrative division using information from National Decrees published in the official newspaper El Peruano. Less than 5 percent of districts suffered boundary changes in the period under analysis.

2.4.2 Empirical Strategy

Cohort Size

I first analyze how cohort size relates to district and year of birth variation in coca revenues generated by the policy. Using the full 2007 Population Census, I construct population counts by cohort (year) and district of birth, which are linked to a measure of revenue that combines yearly prices of sun-dried coca leaves and coca cultivation intensity by district.

In particular, to investigate this relationship, I estimate equation 2.1

$$ln(CohortSize_{dt}) \;=\; \beta(P_t \times Coca_d) \; + X'_{dt}\pi \; + \alpha_d \; + \gamma_t \; + \delta_r \times t \; + \epsilon_{dt} \quad (2.1)$$

Where $CohortSize_{dt}$ is the number of individuals (that survived until 2007) born in district d and year t, and the measure of coca revenue is the product of the year-of-birth price of sun-dried coca leaves in real soles per kg, P_t, and the district-of-birth measure of coca intensity, $Coca_d$. I use the 1994 Agricultural Census, which precedes the 1995 price shock, to calculate the total area of cultivated land with coca crops in thousands of hectares as a measure of coca intensity.

The estimation is conditioned on a set of covariates. District and year of birth fixed effects are represented by α_d and γ_t, respectively. The specification also includes state-level linear trends, $\delta_r \times t$. There are 25 states in Peru, and many policies are executed at this level.[14] These trends partial out factors (linearly) changing over time in each state. The vector X'_{dt} represents year-of-birth effects interacted with a set of agricultural controls including district-level intensity of cacao and coffee measured in thousands of hectares in the 1994 Agricultural Census. This controls for volatility of other commodities beyond variation in international prices, such as pests. In addition, to control for other changing conditions in the agricultural sector, I control for the total area of cultivated land as of the 1994 Agricultural Census in each district interacted with year fixed-effects.

The coefficient of interest is β. Holding all other independent variables

[14] Peru has three layers of administrative division: 25 states (including Callao Constitutional Province) which are divided into approximately 180 provinces, and these are subdivided into over 1800 districts.

constant, increases in the price of coca imply pro-cyclical (larger) cohort size if $\beta > 0$. Alternatively, cohort size is counter-cyclical if $\beta < 0$.

To interpret changes in cohort size as excess cumulative mortality, the regression analysis focuses on the 1995 abrupt price decline and compares the 1995 cohort to immediately preceding cohorts. The 1995 cohort is the first cohort exposed to the shock and was (mostly) conceived in 1994, before the shock. Thus, it is unlikely that changes in the demand for children drive the main results unless the price drop was anticipated. Further analysis on shows that this is unlikely. Including later cohorts or exploiting other sources of price variation that are not as abrupt, however, could complicate the interpretation of the results as these could include a combination of mortality and compositional effects across cohorts, such as selective fertility or selective migration as a response to the change in coca revenues. In addition, the analysis uses district-of-birth and not district-of-residence at the time of the census; therefore, cohort counts are more likely to be correctly assigned to exposure to treatment.

To compare cohorts that would have shared very similar life-cycle events in the absence of the price shock during the year of birth, I perform the analysis over the 1993-1995 and 1994-1995 cohorts. This is a sample of individuals ages 0-3 and 0-2 at the time of the shock.[15] When analyzing the 1993-1995 sample, I show results with and without a coca-specific linear

[15] The 2007 Population Census does not record date of birth directly but age in years as of Census day (October 21st, 2007). Thus, individuals who report being X years of age are assigned to the Oct/21/(2007-X-1) to Oct/20/(2007-X) year of birth. By assigning calendar year prices to census birth years, the oldest individual in a given census cohort (born in October 21st) is matched to prices during months 2 to 14 of age. On the other hand, the youngest individual of the census cohort (born in October 20) is match to prices during months -9 to 2 of age. I do not find evidence of age stacking. The cohorts under analysis are 12-14 years of age at the time of the census.

trend of the form $\delta \times 1\left[Coca_d > 0\right]$. Conditional on this trend, changes in coca revenues are identified as deviations from the average evolution of coca districts over time. As robustness checks, I show results for alternative definitions of the control cohorts. The Peruvian economic crisis of the late 1980s and the abrupt economic reforms of the early 1990s, however, prevent me from analyzing much longer time intervals.

Working with census data and inferring cumulative survival (mortality) rates from relative cohort sizes has two advantages over using Vital Statistics. First, cumulative survival captures fetal mortality and not only mortality after birth (Jayachandran, 2009; Miller and Urdinola, 2010) Second, the quality of Vital Statistics in developing countries may not be adequate. In Peru, mortality and birth certificates data suffer from underreporting in rural districts. The census program cover all districts. A drawback of the census is that it does not have detailed information on each individual and household, and although survival rates can be inferred, little information is collected on households and mothers that experience the death of child. Moreover, the timing of deaths (in-utero and after birth) cannot be distinguished. As I explain below, I also use survey data to overcome these challenges.

Two main identifying assumptions are key to interpret β causally. First, exposed and unexposed cohorts should be statistically exchangeable. This is, there should be no differences in the composition of cohorts based on the measure of exposure. Second, there are no other factors varying over time in a way that is proportional to coca intensity and affecting the outcomes of interest.

I provide tests for these assumptions and evidence on further robustness checks in later sections. Importantly, to address cohort composition, I study maternal characteristics of the cohorts exposed to the shock and show results after controlling for these. Although newborns and children could have been severely affected by the shock, pre-determined characteristics of older cohorts should have not. To study if the effects are driven by other confounding factors correlated with coca revenues, I show results after controlling for a range of time-varying observables. Finally, I also consider robustness to alternative explanations, such as the Peruvian civil conflict, the expansion of health services to non-coca areas, and alternative anti-drug enforcement efforts.

Note that the identification strategy using the 1993-1995 cohorts relies on price variation preceding the shock and not only on the 1994-1995 price drop. This could be problematic if the 1993-1994 variation is a result of factors driving both prices and the mortality of children and is a relevant source of variation. Thus, in addition to conducting close comparisons between the 1994 and 1995 cohorts, I also replicate the 1993-1995 analysis with a difference-in-difference estimation that drops the price sequence and uses a post-treatment variable for 1995. I also use these dummy as an instrument for prices.

Miscarriages, Mortality, and Birth Outcomes

I test if the exposed and unexposed cohorts are statistically exchangeable in terms of maternal characteristics using microdata from the 1996 and 2000 DHS. Mothers with different Socio-Economic Status (SES) may select into having children if they are able to anticipate the shock.[16] At the same time, worsened nutritional conditions and stress can be associated with unexpected

[16] DHS records histories of all live births, including those not alive at the time of the survey, for all women ages 15-49.

termination of births and selection in-utero (Beydoun and Saftlas, 2008; Navara, 2010). Unfortunately, reliable data on miscarriages since the time of conceptions is rarely available.[17] Thus, I use the histories of live births for all women in the sample and then contrast this analysis with predictions on the probability of birth that relate to miscarriages.

$$y_{imdt} \;=\; \beta(P_t \times Coca_d) \;+ X'_{dt}\pi \;+ \alpha_d \;+ \gamma_t \;+ \delta_r \times t \;+ \epsilon_{imdt} \qquad (2.2)$$

I estimate equation 2.2, where y_{imdt} is a maternal outcome for birth i in year t to mother m and district d (for example, years of education, number of preceding births, among others). The regressions include a survey-wave fixed effect, and all other variables are defined as before. The estimation is done over the 1993-1995 and 1994-1995 cohorts.

The test of interest is if $\beta = 0$, or the lack of ability of the measure of exposure to predict pre-determined characteristics of the mothers of children exposed to the shock. Note that both in-utero mortality or changes in the demand for children could cause this test to reject the null.

Then I test if exposure is related to changes in the probability of giving birth, and if these changes are associated with miscarriages rather than changes in the demand for children. To do this, I create a yearly panel for all women in the DHS sample and estimate equation 2.3

$$birth_{mdt} \;=\; \beta(P_t \times Coca_d) \;+ X'_{mdt}\pi \;+ \alpha_d \;+ \gamma_t \;+ \delta_r \times t \;+ \epsilon_{mdt} \qquad (2.3)$$

[17] Even if available, data on self-reported conceptions and miscarriages may be of poor quality. Miscarriages are more likely to happen early in pregnancy. See Orzack et al. (2015) for recent efforts on studying in-utero mortality.

where $birth_{mdt}$ indicates if woman m in district d gave birth in year t. This sample includes childless women that had not given birth at the time they were interviewed. Moreover, in addition to the controls specified in equation 2.1 and survey-wave fixed effects, I will show results with and without a set of maternal characteristics. The notation of X'_{mdt} is now changed to allow for this. Maternal characteristics include sets of fixed effects for the following variables: mother's year of birth, preceding number of births, marital status at birth, mother's years of schooling, and mother's ethnicity (proxied by language).

If miscarriages—and not some behavioral effect in anticipation of the shock—are the main driver of missing births, they should be affecting more male fetuses than female fetuses, as the former are much more likely to be miscarried overall (Navara, 2010). To test for this, I replace the outcome of equation 2.3 for an indicator variable for gender-specific births and test for the difference in magnitude of these estimates.[18] Another implication of miscarriages being the main driver of missing births is that the observed male-to-female ratio should be changing within the shock year. Male fetuses are more likely to be miscarried during the first trimesters of pregnancy, while female fetuses are more likely to be miscarried during the last trimester of pregnancy. Thus, the male-to-female ratio should be lower at end of 1995—when both male and female babies are missing—than at the beginning of 1995—when mostly baby girls are missing—, and relative to the control cohorts. I also implement this test.

Finally, I study if exposure to the shock also affected mortality probabil-

[18] The dependent variable takes value 1 if the newborn is born alive *and* is of a particular gender. For instance, for females, live births of female babies takes value 1 and zero if there was no birth or the new born was male.

ities and other birth outcomes for those children that survived to in-utero exposure. To do this, I estimate equation 2.4 at the individual (child) level,

$$o_{imdt} \;=\; \beta(P_t \times Coca_d) \;+ X'_{imdt}\pi \;+ \alpha_d \;+ \gamma_t \;+ \delta_r \times t \;+ \epsilon_{imdt} \qquad (2.4)$$

where o_{imdt} is the outcome of interest for individual i in district d born in year t and to mother m, such as health markers at the time of birth or survival status up to a particular age. As before, the controls included here are those of equation 2.1, survey wave effects, gender and multiple birth dummies, mother's age at first birth, and the set of maternal characteristics described in equation 2.3.

Note that DHS do no record district of birth. However, these surveys record the number of years mothers have been living in the current place of residence. Using this information, I show evidence that migration is not a problem for identification with DHS data by (i) predicting migrant status with the treatment variable and (ii) re-doing the analysis with the non-migrant sample.

Mechanisms: Income Effects and the Value of Time

I turn to study the mechanisms at play using LSMS data. In particular, I analyze how the price shock affected households' expenditures and labor force allocation. The LSMS survey waves of 1994 and 1997 cover the periods exactly before and after the price drop. LSMS have information on migration for individuals 15 years of age or more at the province level. Thus, I relocate individuals to their pre-shock province of residence. For individuals 14 years of age or younger, I assume they were located in the same province than the household head. Since provinces are larger administrative divisions than districts, I aggregate all district-level variables to the province level.[19]

For an individual or household i in pre-shock location d and year t, I estimate equation 2.5

$$h_{idt} \;=\; \beta(P_t \times Coca_d) \;+ X'_{idt}\pi \;+ \alpha_d \;+ \gamma_t \;+ \delta_r \times t \;+ \epsilon_{idt} \tag{2.5}$$

Where h_{idt} is an outcome of interest, and all other variables are defined as in equation 2.1. I will estimate effects on labor force participation by gender and age group (in school age, 6-17 years, and adults, 18-59 years).[20] In addition, for individual-level regressions, I include age, age squared, and ethnicity fixed effects (proxied by language). In general, I do not include years of education as a control because this is an endogenous variable for individuals in school-age (Dammert, 2008).[21] Moreover, I do not include any additional controls to those of equation 2.1 in household-level regressions as household composition may be endogenous.

2.5 Results

2.5.1 Missing Children: Mortality In-Utero and After Birth

Table 2.1 presents results of estimating the relationship between coca revenues and cohort size through equation 2.1. The dependent variable is the natural logarithm of the population count born in a district-year cell. Thus, the coefficients of interest, β, can be interpreted roughly in percentage terms.

[19] On average, a province encompasses 9 districts

[20] Defining age groups differently do not affect the results significantly.

[21] The results do not change when I include education as a control for individuals in the age group 18-59

To ease interpretation further, I show implied effects for the average coca district and a *price drop* of 5.56 real Peruvian Soles—the price collapse between 1994 and 1995. The first two columns show results for individuals born between 1993 and 1995 (ages 0-3 at the time of the shock) with and without coca-specific time trends. The third column focuses on the adjacent cohorts 1994 and 1995 (ages 0-2 at the time of the shock) and does not include a coca-specific time trend. Results are stratified by gender in each panel: Panel A for all individuals, Panel B for males only, and Panel C for females only. The bottom of the table presents model specifications.

Panel A of Table 2.1 shows evidence of pro-cyclical cohort size across specifications: the 1995 price collapse is associated with a 0.3 to 0.5 percent smaller cohort for the average coca district. The estimates for the 1993-1995 cohorts with or without a coca-specif trend (columns 1 and 2) are positive and statistically significant. A similar result emerges from the adjacent-cohort comparison (column 3).

The magnitudes are large. Attributing this reduction in cohort size to under 5 mortality, the effects would imply and increase of approximately 6 to 11 percent.[22] Mortality under age 5 is a good benchmark given that, as shown in later sections in an analysis using DHS data, most deaths are taking place in-utero and during the first years of life. As a benchmark, the Peruvian economic crisis of the late 80s, which was associated with a 30% drop in per capita GDP, increased infant mortality in 2.5% (Paxson and Schady, 2005). In addition, note that this estimates are likely a lower bound:

[22] This calculation is based on a child mortality rate of 47 per thousand individuals under age 5 (Peruvian Demographic and Health Survey, 2000). Then $0.003 \times 1000/47 = 0.063$ and $0.005 \times 1000/47 = 0.106$. In terms of the rural child mortality rate of 64 per thousand, these effects are 0.047 and 0.078, respectively.

Table 2.1: Effects of Coca Price Shocks on Cohort Size

Dependent Variable:	Log cohort size		
Cohorts:	93-95		94-95
Model:	(1)	(2)	(3)
	Panel A: All individuals		
Price at t × Coca Intensity	0.0065*	0.0079**	0.0044*
	(0.0034)	(0.0036)	(0.0026)
Implied Effect (%)	-0.43	-0.53	-0.29
Districts	1777	1777	1777
Observations	5331	5331	3554
	Panel B: Males		
Price at t × Coca Intensity	0.0121***	0.0134***	0.0116***
	(0.0040)	(0.0043)	(0.0038)
Implied Effect (%)	-0.81	-0.89	-0.77
Districts	1777	1777	1777
Observations	5331	5331	3554
	Panel C: Females		
Price at t × Coca Intensity	0.0001	0.0017	-0.0037
	(0.0049)	(0.0050)	(0.0049)
Implied Effect (%)	-0.01	-0.12	0.25
Districts	1777	1777	1777
Observations	5331	5331	3554
Model Specifications:			
District FE	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes
Coca Trend	No	Yes	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Implied effects at the average coca district (119.95 hectares) and for a -5.56 real Nuevos Soles price change.

not every individual in treatment areas is actually treated, and cohort sizes in the main analysis are constructed using the entire population in each district-year cell. Untabulated results document that the effects are twice as large for districts with above median levels of coca cultivation. A well executed paper by Miller and Urdinola (2010) estimates that coffee price shocks in Colombia are associated with cohort sizes through a *negative* elasticity between -0.01 and -0.04. These effects are large according to Ferreira and Schady (2009) review of the literature. The implied elasticity of the coca price shocks is *positive* and around 0.01. In later sections I come back to discussing the difference in signs between this study and (Miller and Urdinola, 2010).

Table 2.2 presents the results of estimating equation 2.2 and studies how *pre-determined* maternal characteristics of the exposed cohorts relate to the price shock using DHS data. The analysis is performed for the 93-95 cohorts (columns 1-3) and 94-95 cohorts (columns 4-5). The table shows estimated coefficients, implied effects, and dependent variables means, for a range of maternal outcomes. Importantly, this is a sample of live births, regardless of their survival status at the time of the survey, linked to maternal outcomes.

The evidence suggests that, conditional on observing a live birth, exposure to the shock is associated with higher maternal SES, if anything. In other words, there are missing live births to lower SES women. Table 2.2 shows that, out of nine outcomes proxying for demographic and economic characteristics, two are statistically significant: exposed women who gave birth to a child at the time of the shock were more educated and literate. Out of the other seven outcomes, five of them show point estimates that would suggest a similar intuition, but these are not statistically significant: women are older, and older at the time of first birth, with fewer preceding

Table 2.2: Exposure to Coca Price Shocks and Maternal Characteristics

Treatment:	Price at t × Coca Intensity					
Cohorts:	93-95			94-95		
	(1) Estimate (S.E.)	(2) Imp. Effect	(3) Var. Mean	(4) Estimate (S.E.)	(5) Imp. Effect	(6) Var. Mean
Depedent Variable:						
Mother's age in years at time of birth	-0.027 (0.199)	0.018	27.278	-0.004 (0.188)	0.003	27.272
Mother was married at time of birth (=1)	0.035 (0.027)	-0.024	0.661	0.050 (0.034)	-0.033	0.654
Mother's age in years at first birth	-0.011 (0.075)	0.007	20.299	-0.013 (0.070)	0.009	20.264
Mother's number of preceding births	0.019 (0.043)	-0.013	2.566	0.028 (0.044)	-0.019	2.594
Mother's first birth (=1)	0.008 (0.005)	-0.005	0.244	0.008 (0.005)	-0.005	0.243
Mother is illiterate (=1)	0.017** (0.007)	-0.011	0.194	0.013** (0.007)	-0.009	0.205
Mother's years of education	-0.141*** (0.046)	0.094	5.394	-0.100** (0.043)	0.067	5.361
Mother was born in a rural area (=1)	0.011 (0.015)	-0.008	0.399	0.008 (0.015)	-0.005	0.407
Mother is a Quechua speaker (=1)	-0.000 (0.002)	0.000	0.174	-0.002 (0.003)	0.001	0.187
Model Specifications:						
District FE	Yes			Yes		
Year of Birth FE	Yes			Yes		
Agro Controls	Yes			Yes		
Region Trend	Yes			Yes		
Coca Trend	Yes			No		

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Standard errors clustered at the district level are in parenthesis. Implied effects at the average coca district (119.95 hectares) and for a -5.56 real Nuevos Soles price change.

births, and less likely to be the first birth (first births usually have higher probability of complications), and these women are less likely to be born in rural areas).

Panel A of Table 2.3 shows the results of estimating equation 2.3 and further confirms that exposure to the shock is associated with missing live births. The estimates are statistically significant across all specifications, including corrections for maternal controls. The implied effect for the average coca district is a *decrease* of 0.56-0.60 points in the probability of birth in a given year, or 4.7% to 5.1% with respect to the mean.

Table 2.3: Effects of Coca Price Shocks on Probability of Birth

Dependent Variable:	Live birth (=1)					
Maternal controls:	No Maternal Controls			With Maternal Controls		
Cohorts:	93-95		94-95	93-95		94-95
Model:	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Any birth					
Price at t × Coca Intensity	0.0084***	0.0090***	0.0088***	0.0084***	0.0090***	0.0087***
	(0.0025)	(0.0026)	(0.0024)	(0.0026)	(0.0027)	(0.0024)
Implied Effect	-0.0056	-0.0060	-0.0059	-0.0056	-0.0060	-0.0058
Dep. Var. Mean	0.1187	0.1187	0.1180	0.1187	0.1187	0.1180
Districts	847	847	847	847	847	847
Observations	164835	164835	109890	164835	164835	109890
	Panel B: Male births					
Price at t × Coca Intensity	0.0061***	0.0066***	0.0073***	0.0061***	0.0066***	0.0072***
	(0.0015)	(0.0014)	(0.0015)	(0.0014)	(0.0013)	(0.0015)
Implied Effect	-0.0041	-0.0044	-0.0048	-0.0041	-0.0044	-0.0048
Dep. Var. Mean	0.0595	0.0595	0.0602	0.0595	0.0595	0.0602
Districts	847	847	847	847	847	847
Observations	164835	164835	109890	164835	164835	109890
	Panel C: Female births					
Price at t × Coca Intensity	0.0023	0.0024	0.0015	0.0023	0.0024	0.0015
	(0.0023)	(0.0023)	(0.0024)	(0.0023)	(0.0023)	(0.0024)
Implied Effect	-0.0015	-0.0016	-0.0010	-0.0015	-0.0016	-0.0010
Dep. Var. Mean	0.0592	0.0592	0.0578	0.0592	0.0592	0.0578
Districts	847	847	847	847	847	847
Observations	164835	164835	109890	164835	164835	109890
Model Specifications:						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes	Yes	Yes
Maternal Controls	No	No	No	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	No	Yes	No	No	Yes	No

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Standard errors clustered at the district level are in parenthesis. Implied effects at the average coca district (119.95 hectares) and for a -5.56 real Nuevos Soles price change.

I then establish that the effects are consistent with the survival disadvantage of males both in-utero and in early life documented in biological studies (Drevenstedt et al., 2008; Navara, 2010). First, male fetuses are more likely to be miscarried than female fetuses (Navara, 2010). One could argue that the positive selection in maternal characteristics I find is a product of an anticipated fertility response over those women for which the shock is more relevant: low SES women. On the other hand, if babies are being miscarried, and this is the main factor driven the results, then it is likely that we would observe a gender difference in this pattern.

Separately estimating equation 2.1 by gender shows strong and statistically significant effects for males but not for females. This is consistent across all specifications. Panel B of table 2.1 shows the results for the cohorts of males. The implied effect is a reduction of approximately 0.8 to 0.9 percent in cohort size. Panel C shows the results for females; all estimates are statically indistinguishable from zero. The difference between males and females is statistically different.

The evidence on missing live births is also consistent with a gender-specific pattern. Panels A and B of Table 2.3 show estimates of the price shock stratifying equation 2.3 by gender. Again, the effects are strong and significant for males and smaller and not statistically different from zero for females. The effects for males and females are statistically different.

Moreover, an additional pattern consistent with miscarriages is that the male-to-female ratio of life births should have decreased over time within the shock year, 1995, relative to control years. This is because male fetuses are more likely to be miscarried at the beginning of pregnancy, while female fetuses are more likely to be miscarried at the end of pregnancy (Orzack et al., 2015). Thus, more male live births should be missing at the end of 1995 because they were miscarried earlier in 1995, while this pattern should be less pronounced for females. I test this hypothesis by fully interacting equation 2.2 with an indicator variable that takes value 1 for the second half

Table 2.4: Effects of Coca Price Shocks on Male to Female Ratio and Miscarriages

Dependent Variable:	Newborn is male (=1)		
Maternal controls:	With Maternal Controls		
Cohorts:	93-95		94-95
Model:	(1)	(2)	(3)
Price at t × Coca Intensity × Second Half of Year	0.0302*	0.0299*	0.0309**
	(0.0158)	(0.0158)	(0.0156)
Implied Effect	-0.0201	-0.0200	-0.0206
Dep. Var. Mean	0.5014	0.5014	0.5109
Districts	836	836	823
Observations	19294	19294	12689
Model Specifications:			
District FE	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes
Maternal Controls	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes
Coca Trend	No	Yes	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Implied effects at the average coca district (119.95 hectares) and for a -5.56 real Nuevos Soles price change.

of the year and using male births as an outcome. I focus on the coefficient of the triple interaction between prices, coca intensity, and the indicator for the second half of the year.

The results in Table 2.4 are consistent with the hypothesis that the likeness of observing male newborns relative to female newborns is *lower* in coca areas when prices are low and during the second half of the year. This is consistent across specifications. This effects are unlikely to be driven by selective abortion. First, abortion is illegal in Peru. Second, even if available informally, it would require a technology that allows mothers in rural Peru to screen the gender of the fetus and then act upon this information. This seems unlikely given the lack of health infrastructure in the country. For instance, only 60 percent of babies born in 1995 in all Peru (including urban districts and the capital region, Lima, which holds one third of the population) were weighed at birth. Third, it seems unlikely that, even if the above conditions are met, (baby boys) and not baby girls would have been the subject of selective abortions. Finally, as I show later, this gender-specif pattern of deaths also takes place after birth, which is consistent with the hypothesis that the price shock affects the weakest (baby boys both in-utero and after birth) more.

Next I analyze if, conditional on in-utero survival, exposure during the year of birth increased mortality rates of children. Live births were, if anything, higher SES according to maternal characteristics. Relative to unexposed newborns and conditional on in-utero survival status, exposed newborns are probably stronger that unexposed newborns had they not been affected by the price drop. Thus, to investigate the effects of exposure to the price drop on child mortality and birth outcomes, I will show results with and without controlling for maternal characteristics.

Table 2.5 shows that exposure to the price drop also increased mortality after birth. As expected, the impacts are slightly stronger after controlling for maternal characteristics—once sample selection is partially addressed. Table 2.6 explores the timing of these effects. I show the effects of the price shock on deaths under 24 months in columns 1 to 3 and deaths under 60 months (i.e., child mortality) in columns 4 to 6. For simplicity, I show results for males and females in the 93-95 cohorts after including all controls of the previous analysis and the coca-specific time trend. The price shock caused an increase in mortality under 24 months equivalent to 6.8% of the mean (0.4396/6.5002) for the average coca district (column 1). The effect is statistically significant for males (column 2) but not for females (column 3), although the latter has the expected sign. I reject the null that the effect for males and females is the same (p-value = 0.0939). Columns 4 to 6 shows a similar pattern. The effect is significant when pooling males and females, but the estimated coefficient is statistically significant for males and not for females. Again, I reject the null that the effects for males are statistically equal than for females (p-value = 0.0845). In addition, this tables shows that the bulk of the effect takes place during the first two years of life, as the estimated coefficients for mortality under 24 months and 60 months are observationally very similar.

I also show that the effects on mortality after birth are consistent with the effects on the health of the child. I find that newborns are smaller at birth as reported by the subjective assessment of their mothers (Table 2.7).[23]

Overall, the evidence suggests that the price drop is associated with more missing children, particularly boys. The effect is consistent with increased mortality in-utero affecting more males than female fetuses. This male-to-female pattern is grounded in research in biology and reproductive science, as well as in the characteristics of the setting. First, it has been documented that the ratio of males to females at birth decreases under stressful events and adverse conditions both in mammals in general and humans (Navara, 2010). What is more, males have a survival disadvantage both in-utero and in early life relative to females; in general, perinatal conditions, neonatal care, and infectious diseases affect more males than females (see Drevenstedt et al.,

[23] Size at birth is self-reported by mothers as a subjective assessment of the size of their children at the time of birth relative to other children. Weight at birth is not available for over 40% of the sample. Size at birth is only available in the 1996 wave. Thus, a more limited number of districts are covered in the sample.

Table 2.5: Effects of Coca Price Shocks on Mortality

Dependent Variable:	Child not alive (=1)					
Maternal controls:	No Maternal Controls			With Maternal Controls		
Cohorts:	93-95		94-95	93-95		94-95
Model:	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Males and Females					
Price at t × Coca Intensity	-0.0063**	-0.0058**	-0.0103***	-0.0080***	-0.0075***	-0.0117***
	(0.0026)	(0.0027)	(0.0034)	(0.0022)	(0.0023)	(0.0034)
Implied Effect	0.0042	0.0039	0.0069	0.0053	0.0050	0.0078
Dep. Var. Mean	0.0680	0.0680	0.0695	0.0680	0.0680	0.0695
Districts	836	836	823	836	836	823
Observations	19294	19294	12689	19294	19294	12689
	Panel B: Males					
Price at t × Coca Intensity	-0.0071	-0.0063	-0.0123**	-0.0088*	-0.0081*	-0.0144**
	(0.0045)	(0.0047)	(0.0061)	(0.0045)	(0.0046)	(0.0062)
Implied Effect	0.0047	0.0042	0.0082	0.0059	0.0054	0.0096
Dep. Var. Mean	0.0753	0.0753	0.0770	0.0753	0.0753	0.0770
Districts	812	812	783	812	812	783
Observations	9779	9779	6529	9779	9779	6529
	Panel C: Females					
Price at t × Coca Intensity	-0.0047	-0.0045	0.0026	-0.0090	-0.0087	-0.0001
	(0.0100)	(0.0100)	(0.0089)	(0.0111)	(0.0111)	(0.0121)
Implied Effect	0.0031	0.0030	-0.0018	0.0060	0.0058	0.0001
Dep. Var. Mean	0.0608	0.0608	0.0617	0.0608	0.0608	0.0617
Districts	813	813	766	813	813	766
Observations	9515	9515	6160	9515	9515	6160
Model Specifications:						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes	Yes	Yes
Maternal Controls	No	No	No	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	No	Yes	No	No	Yes	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Implied effects at the average coca district (119.95 hectares) and for a −5.56 real Nuevos Soles price change.

2008, and citatios therin). Second, the effect on females may not be large enough to be statistically significant given sample sizes because the male-to-female ratio in rural Peru was already low. Regular male to females ratios are around 1.05, but rural Peru exhibited 1.02 in 1994 and 1.01 in 1993-1994, the control years in the analysis. This may suggest that the female babies born in the control years were already pretty resilient. In addition, there is little

Table 2.6: Effects of Coca Price Shock on Mortality by Age of the Child

Cohorts:	93-95					
Dependent Variable:	Died under 24 months (=100)			Died under 60 months (=100)		
Sample	All (1)	Male (2)	Female (3)	All (4)	Male (5)	Female (6)
Price at t × Coca Intensity	-0.6592*** (0.2531)	-0.7708* (0.4595)	-0.4862 (1.0167)	-0.6961*** (0.2638)	-0.8302* (0.4806)	-0.5050 (1.0172)
Dep. Var. Mean	6.5002	7.1949	5.8015	6.7666	7.4985	6.0305
Districts	836	812	813	836	812	813
Observations	19294	9779	9515	19294	9779	9515
Implied Effect of Price Drop (pp)	0.4396*** (0.1688)	0.5141* (0.3065)	0.3242 (0.6781)	0.4642*** (0.1759)	0.5537* (0.3205)	0.3368 (0.6784)
H_0 : Males = Females (p-value)			0.0939			0.0845
Model Specifications:						
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	Yes	Yes	Yes	Yes	Yes	Yes

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Implied effects at the average coca district (119.95 hectares) and for a −5.56 real Nuevos Soles price change.

evidence of differential investments in health by gender in Peru (Attanasio et al., 2017), which contrasts with evidence from other countries such as India and China.

2.5.2 Mechanisms: Expenditure and Labor Markets

In this section, I explore two plausible mechanisms related to health investments: changes in health and food expenditures and changes in the allocation of time of the primary caregiver.

Table 2.8 documents that households' real expenditure decreases as a result of the price shock, in particular, expenditures in health. Panel A shows results for real expenditure in logs, while Panel B shows results for real expenditure per capita in logs. The evidence suggests that households decrease total expenditures in 5.7 percent. The analysis in per capita terms leads to a similar conclusion (a decrease of 7.9 percent). Expenditures in food also

Table 2.7: Effects of Coca Price Shocks on Size at Birth

Dependent Variable:	Small at Birth (=1)					
Maternal controls:	No Maternal Controls			With Maternal Controls		
Cohorts:	93-95		94-95	93-95		94-95
Model:	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Males and Females					
Price at t × Coca Intensity	-0.0233***	-0.0233***	-0.0204***	-0.0240***	-0.0241***	-0.0188**
	(0.0048)	(0.0047)	(0.0070)	(0.0050)	(0.0049)	(0.0076)
Implied Effect	0.0155	0.0156	0.0136	0.0160	0.0161	0.0125
Dep. Var. Mean	0.2231	0.2231	0.2215	0.2231	0.2231	0.2215
Districts	476	476	472	476	476	472
Observations	10246	10246	6756	10246	10246	6756
	Panel B: Males					
Price at t × Coca Intensity	-0.0280***	-0.0283***	-0.0238***	-0.0314***	-0.0318***	-0.0238***
	(0.0104)	(0.0106)	(0.0054)	(0.0118)	(0.0119)	(0.0069)
Implied Effect	0.0187	0.0189	0.0159	0.0210	0.0212	0.0159
Dep. Var. Mean	0.2038	0.2038	0.1991	0.2038	0.2038	0.1991
Districts	460	460	446	460	460	446
Observations	5192	5192	3506	5192	5192	3506
	Panel C: Females					
Price at t × Coca Intensity	-0.0115	-0.0117	-0.0009	-0.0134	-0.0134	0.0082
	(0.0157)	(0.0157)	(0.0308)	(0.0166)	(0.0168)	(0.0306)
Implied Effect	0.0077	0.0078	0.0006	0.0089	0.0089	-0.0054
Dep. Var. Mean	0.2425	0.2425	0.2459	0.2425	0.2425	0.2459
Districts	467	467	447	467	467	447
Observations	5054	5054	3250	5054	5054	3250
Model Specifications:						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes	Yes	Yes
Maternal Controls	No	No	No	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	No	Yes	No	No	Yes	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Implied effects at the average coca district (119.95 hectares) and for a −5.56 real Nuevos Soles price change.

decrease, although by a smaller fraction. Changes in food expenditures may affect the nutrition of household members. As a fraction of the total budget, however, food is not decreasing, indicating that households are reallocating their budgets to this category, or that food is part of the home production function and less likely to present overall reductions if there are no changes in inputs. Health expenditures, on the other hand, decrease across all specifications. The effect amounts for a decrease of 30 to 20 percent. Although health expenditures represent about 5 percent of the total household budget, reductions in this category could be of particular importance for vulnerable populations, such as young children, pregnant women, and the elderly.

I then examine the effects of the price shock on labor markets and time allocation for different household members. Table 2.9 shows results on the impacts of the shock for adults. Columns 1 and 2 focus on labor force participation and total weekly hours worked. Columns 3 and 4 report results on participation in household chores and weekly hours allocated to them. Although the surveys do not break down this category further, some activities may relate to caregiving and child health directly or indirectly (e.g., cooking, cleaning, taking care of young children). Panel A focuses on adult females and Panel B on adult males.

As a result of the negative price shock, more adult females supply labor and allocate less time into household chores. This is suggestive evidence of a decrease in the amount of time allocated into the production function of child health. For the average province, the price drop increases the probability of observing females ages 18 to 59 supplying labor in 2.9 percentage points or a 5.4 percent increase with respect to the baseline level (column 1). There are no significant effects, however, on the total number of hours allocated to labor (column 2). The increase in labor supply is likely related to the observed reduction in hours allocated into household chores. Finally, adult males do not significantly change their labor supply. Most adult males were already working at baseline (84.23 percent). If anything, adult males may be working fewer hours, but this is not statistically significant. This group also seems to participate less from household chores because of the shock, but

Table 2.8: Effects of Coca Price Shocks on Household Real Expenditure

	Real Expenditure		
Expenditure Category:	Total (1)	Food (2)	Health (3)
	Panel A: Log Expenditure		
Price at t × Coca Intensity	0.0343**	0.0177**	0.1917***
	(0.0159)	(0.0087)	(0.0666)
Implied Effect (%)	-5.72	-2.95	-31.98
Dep. Var. Mean	17937.40	7980.28	945.48
Provinces	139	139	139
Observations	7453	7453	7453
	Panel B: Log Expenditure Per Capita		
Price at t × Coca Intensity	0.0475**	0.0309**	0.1278**
	(0.0223)	(0.0125)	(0.0534)
Implied Effect (%)	-7.92	-5.16	-21.32
Dep. Var. Mean	4259.49	1798.36	221.56
Provinces	139	139	139
Observations	7453	7453	7453
	Panel C: Budget Shares		
Price at t × Coca Intensity	. . .	-0.0073	0.0037***
	. . .	(0.0051)	(0.0012)
Implied Effect	. . .	0.0122	-0.0062
Dep. Var. Mean	. . .	0.5305	0.0471
Provinces	. . .	139	139
Observations	. . .	7453	7453
Model Specifications:			
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the province level are in parenthesis. Implied effects at the average coca province (300 hectares) and for a -5.56 real Nuevos Soles price change.

Table 2.9: Effects of Coca Price Shocks on Labor Margins and Time Allocation for Adults

Type of Work:	Market Work		Household Work	
Dependent Variable:	Any work (=1) (1)	Total Hours (2)	Any work (=1) (3)	Total Hours (4)
	Panel A: Adult Females (18-59 years)			
Price at t × Coca Intensity	-0.0174*	-0.5655	0.0012	1.2903*
	(0.0102)	(0.3688)	(0.0016)	(0.6998)
Implied Effect	0.0290	0.9432	-0.0021	-2.1522
Dep. Var. Mean	0.5391	20.7156	0.9567	34.0830
Provinces	141	141	141	141
Observations	9639	9639	9639	9637
	Panel B: Adult Males (18-59 years)			
Price at t × Coca Intensity	-0.0064	0.5969	0.0208***	0.0818
	(0.0054)	(0.4037)	(0.0071)	(0.1052)
Implied Effect	0.0107	-0.9956	-0.0347	-0.1364
Dep. Var. Mean	0.8423	42.6093	0.6569	7.3331
Provinces	139	139	139	139
Observations	8887	8887	8887	8887
Model Specifications:				
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Ind Controls	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes
Coca Trend	No	No	No	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the province level are in parenthesis. Implied effects at the average coca province (300 hectares) and for a -5.56 real Nuevos Soles price change.

this does not affect the average number of hours allocated into this category.

In table 2.10, I explore the effects of the price shock on the labor responses of individuals in school age. As before, I focus on labor supply and time allocated to household chores. Panel A presents results for females ages 6 to 17 years, while Panel B focuses on males in the same age group.

Individuals in school-age increase their labor supply. The strongest impacts are concentrated among females in school-age. More young females supply labor (column 1). The effect is sizable with respect to the baseline. For the average province, the price drop increases the likeliness of observing young females working in 5.8 percentage points. This effect is almost one-third of the baseline. In addition, there is also an increase in the number of hours worked. Interestingly, they do not decrease their participation in household chores (columns 3 and 4). Previous research in Peru suggests that young females play an important role in the caregiving of younger siblings (Levison, 1998). This may come at a temporary cost in terms of schooling. Dammert (2008) studies the same shock that this paper and shows that children drop out of school at the time of the drop in coca prices. I do not find evidence of more young males supplying labor; however, those already participating in the labor force increase the number of hours worked (column 2). This group also show reductions in household chores.

Overall, the price drop is consistent with changes in expenditures and time allocation that affect health investments. Household budgets are reduced as shown by decreases in total expenditures. In addition, I document moderate decreases in food expenditures and important drops in expenditures in health. Moreover, the evidence on labor market responses and time allocated into household chores suggests reductions in the amount of time allocated into the production function of child health. Adult females and individuals in school-age supply more labor and this comes at the cost of allocating less time into household-related tasks. The findings here suggest that health is vulnerable to incomes losses and that decreases in the relative price of maternal time do not undo the adverse effect of decreased expenditures (Bhalotra, 2010). Importantly, this pattern contrasts with evidence from more developed settings (Miller and Urdinola, 2010).

Table 2.10: Effects of Coca Price Shocks on Labor Margins and Time Allocation for Individuals in School Age

Type of Work:	Market Work		Household Work	
Dependent Variable:	Any work (=1)	Total Hours	Any work (=1)	Total Hours
	(1)	(2)	(3)	(4)
	Panel A: Females in school age (6-17 years)			
Price at t × Coca Intensity	-0.0347***	-1.3053***	-0.0000	-0.1733
	(0.0092)	(0.1664)	(0.0081)	(0.2064)
Implied Effect	0.0580	2.1772	0.0001	0.2891
Dep. Var. Mean	0.1761	3.6901	0.8268	12.9886
Provinces	130	130	130	130
Observations	5416	5416	5416	5415
	Panel B: Males in school age (6-17 years)			
Price at t × Coca Intensity	-0.0047	-0.6187***	0.0322***	0.3309**
	(0.0134)	(0.2362)	(0.0044)	(0.1501)
Implied Effect	0.0078	1.0319	-0.0537	-0.5520
Dep. Var. Mean	0.2620	5.9355	0.7018	7.7000
Provinces	132	132	132	132
Observations	5524	5523	5524	5524
Model Specifications:				
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Ind Controls	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes
Coca Trend	No	No	No	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the province level are in parenthesis. Implied effects at the average coca province (300 hectares) and for a −5.56 real Nuevos Soles price change.

2.5.3 Robustness and Pre-Trends Analysis

In this section, I show evidence that the main findings of this paper are unlikely to be driven by pre-existing time-trends. Thus far, I have shown that the main results on missing children, explained by increases in in-utero mortality and mortality after birth, are not the result of anticipated behavioral responses to the shock, such as selective fertility or selective migration. I show this by studying the maternal characteristics of the cohorts exposed and unexposed to the shock as well as by bringing evidence consistent with the biological survival disadvantage of males in-utero and after birth. In particular, I show that the effects are stronger for males both in-utero and during the first years of life. Any confounding behavior in anticipation of the shock would have thus needed to affect more males than females, even in-utero. A possible confounding factor, however, could be that coca districts have been evolving over time in ways that worsened child health, affecting more males than females.

I provide three pieces of evidence that show that pre-trends are not a problem for the analysis. First, in all sections, I showed results with and without including a coca-specific time-trend. In every case, the main estimates are not affected by the inclusion of this additional control.

Second, the main results are not affected by the inclusion of additional covariates. I add to equation 2.1 a set of district-level controls from the 1993 population census interacted with year-effects. These covariates include log population, male to female ratio, share rural, share Quechua, shares by age groups (0-14, 15-49, 50+), average years of education (for population 21+), and share unemployed (for population 6+). Panel A of table 2.11 shows the baseline result of the effect the price drop on cohort size for the 1993-1995 cohorts: all individuals in columns 1 and 2, males in columns 3 and 4, and females in columns 5 and 6. Columns 2, 4 and 6, include a coca-specific time-trend. In panel Panel B, I replicate the analysis conducted for Panel A, but I add the additional set of controls constructed using the 1993 population census. In every column, the result in Panel A is virtually the same than in Panel B.

Table 2.11: Effects of Coca Price Shock on Cohort Size – Robustness to 1993 controls

Dependent Variable:	Log cohort size					
Cohorts:	93-95					
Sample:	All Individuals		Males Only		Females Only	
	(1)	(2)	(3)	(4)	(5)	(6)
	PANEL A: BASELINE SPECIFICATION					
Price at t × Coca Intensity	0.0065*	0.0079**	0.0121***	0.0134***	0.0001	0.0017
	(0.0034)	(0.0036)	(0.0040)	(0.0043)	(0.0049)	(0.0050)
Districts	1777	1777	1777	1777	1777	1777
Observations	5331	5331	5331	5331	5331	5331
Equation 1 controls	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	No	Yes	No	Yes	No	Yes
1993 controls × year effects	No	No	No	No	No	No
	PANEL B: ADDING 1993 CENSUS CONTROLS					
Price at t × Coca Intensity	0.0060*	0.0072*	0.0119***	0.0130***	-0.0013	0.0000
	(0.0035)	(0.0037)	(0.0044)	(0.0046)	(0.0050)	(0.0051)
Districts	1777	1777	1777	1777	1777	1777
Observations	5331	5331	5331	5331	5331	5331
Equation 1 controls	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	No	Yes	No	Yes	No	Yes
1993 controls × year effects	Yes	Yes	Yes	Yes	Yes	Yes

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Columns 1 to 6 from Panel A replicate Table 2.1. Panel B shows results for each column after adding the following 1993 census controls interacted with year fixed effects: log population, male to female ratio, share rural, share Quechua, shares by age groups (0-14, 15-49, 50+), average years of education (for population 21+), and share unemployed (for population 6+).

Third, I use an event-study framework to bring observational evidence that the effects take place when the prices dropped and not before (i.e., evidence of no pre-trends). To do this, I estimate equation 2.6 below

$$ln(CohortSize_{dt}) \;=\; \sum_{s\neq 1994} \beta_s(Coca_d \times year_s) \;+X'_{dt}\pi \;+\alpha_d \;+\gamma_t \;+\delta_r \times t \;+\epsilon_{dt} \tag{2.6}$$

which is a slight modification of the main equation of this paper (equation

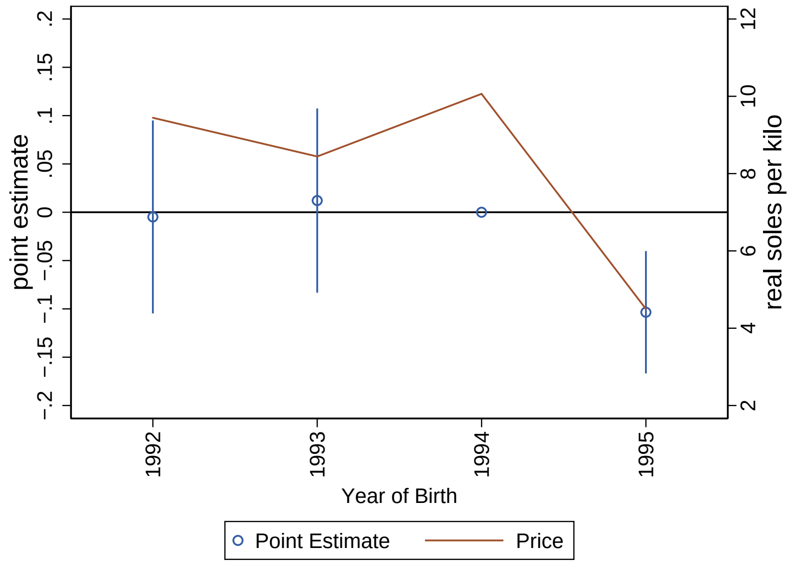

FIGURE 2.4: Event-study framework: Effect of price shock on male cohort size

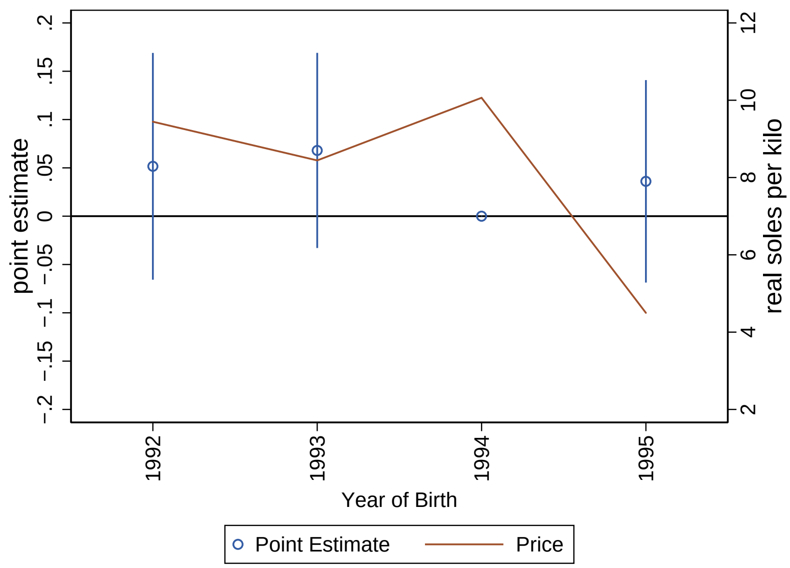

FIGURE 2.5: Event-study framework: Effect of price shock on female cohort size

2.1). In particular, I drop the price variable and interact the measure of coca cultivation with a year-specific dummy, $year_s$. This is an event-study design with 1994 as the omitted year, and where β_s recovers the difference in the dependent variable across districts experiencing different intensities of coca cultivation relative to 1994–the pre-shock year. All other variables are defined as before. I conduct the analysis for the 1992-1995 cohorts (the first cohort exposed to the shock and three cohorts immediately before).

Figures 2.4 and 2.5 show the estimated coefficients β_s for males and females, respectively. For males, the estimated coefficient for 1992 and 1993 is not statistically different from the omitted category, 1994. The estimated coefficient for 1995, on the other hand, is negative and statistically different from 1994. This pattern suggests that the effects only start in the year of the shock, 1995, and not before. The results for females show that there are no effects in 1995; in fact, no single coefficient is statistically different from the omitted category. I take this as evidence of no pre-existing trends in the main analysis.

2.6 Conclusion

This paper studies how price shocks in the Peruvian cocaine industry affect the health of children in areas that produce coca—the main input for cocaine production. The empirical strategy is based on a difference-in-difference design that exploits variation across baseline levels of coca cultivation across areas and abrupt drops in the price of coca leaves over time due to an anti-drug policy. I find that price drops increase mortality rates among children. A combination of data sets and approaches allows me to establish that survival is compromised, especially for boys, both in-utero and after birth. The increase in mortality is consistent with the behavioral responses of the household. When coca prices drop, household income decreases. To smooth the income shock, individuals in school age and adult females increase their labor supply. Despite this coping strategy, household cannot fully smooth consumption: expenditures in food fall, and expenditures on health drop sharply. In addition to reduced health investments, the increase in labor supply and the reduction of time allocated to household chores is consistent with a setting in which the decrease in the opportunity cost of time is not translated to increases in time-intensive health investments.

The empirical strategy used here addresses important threats to identification. First, to overcome potential endogeneity issues between coca revenues and child health, I use an antidrug policy as a quasi-exogenous source of variation for coca prices over time. Importantly, this policy focused on the interception and shootdown of narco-airplanes ferrying coca from Peru to Colombia and caused an abrupt decline in coca prices in Peru. Although this policy was later followed by law enforcement efforts on the ground, during the period I analyze, market forces as a result of the negative demand shock for Peruvian coca are the only factors at play. Second, the abrupt fall in coca prices allows me to confidently identify the effects of the price on mortality isolated from confounding factors, such as selective fertility. In particular, I compare cohorts of children that were conceived the year before the shock with older, adjacent cohorts. In addition, the price shock does not predict pre-determined characteristics of mothers, which supports the hypothesis that the policy and subsequently drop in prices was not anticipated.

In addition, the setting and source of shocks allow me to focus on the behavioral responses of the household as the main mechanisms at play. Previous studies using aggregate income shocks, such as financial crises or deviations from the economic cycle, may affect child health through additional causal pathways including the provision of public goods (Ferreira and Schady, 2009). In addition, other sources of shocks commonly used in the literature, such as weather shocks and natural disasters, may affect health directly (Almond and Currie, 2011; Almond et al., 2017; Prinz et al., 2018). This paper isolates the effects of market forces on health through household responses.

3

Mass Media and Demographic Transitions: Evidence from the Introduction of Television to the U.S.

3.1 Introduction

The underlying causes of the fertility decline that followed the post-World War II baby boom in the United States have puzzled scholars for decades (Van Bavel and Reher, 2013). Although, traditional explanations emphasize the role of labor market conditions, access to education, and the availability of contraceptive methods in reducing the demand for children (Bailey, 2010), new evidence from developing countries points out the relevance of exposure to mass media, and in particular to television, on shaping demographic patterns (Jensen and Oster, 2009; La Ferrara et al., 2012). Interestingly, the fertility decline that started in the 1960s was preceded by the massive introduction of commercial television during the late 1940s and mid-1950s.

Recent studies conducted beyond the laboratory setting show that exposure to television can indeed affect the attitudes and behavior of families

(Price and Dahl, 2012). For instance, Jensen and Oster (2009) study the roll-out of cable television in rural villages in India during the 2000s, and Chong and La Ferrara (2009) and La Ferrara et al. (2012) examine the case the introduction of a particular television network in Brazil during the 1980s. In both countries, the authors find that television, through the portrayal of "progressive" family lives and role models, prompts reductions in fertility and attitudes associated with the traditional role of genders.

Exploiting idiosyncratic variation in the geographic availability of television's commercial broadcasting in the United States, I find that the beginning of the television era was negatively associated with fertility. Interestingly, the timing of these effects coincides with the moderation of the post-World War II baby boom and the later drop in fertility rates.

Three characteristics of this setting regarding the timing of introduction of television to different areas allow me to identify a causal relationship (for a similar strategy, see Gentzkow, 2006; Gentzkow and Shapiro, 2008b). First, through an area-specific licensing process that started in 1941, commercial broadcasting was licensed for different areas at different points in time. Although the order of the introduction was not random (on average richer and more populated areas received television first), idiosyncratic spacing in the exact timing of the introduction was added by two historical events: World War II and a technical freeze imposed by the Federal Communications Commission (FCC) from 1948 to 1952. Second, the somewhat fixed technology of broadcasting stations allowed a single antenna to reach a number of heterogeneous counties at once. As a result, not only big cities but also small towns received television early on. Third, the adoption of television (i.e., take-up) was fast and demographically broad. Between 1948 and 1955, approximately two-thirds of the households in the United States installed a television set (Spigel, 1992); and, by 1950, viewing time among television owners had already reached four and half hours a day (Bogart, 1958).

I find that the negative effect of television on fertility is economically important and statically robust. My most conservative estimate is that the introduction of television is associated with 1.3 fewer births per thousand women in childbearing age. The magnitude of this effect is around one-sixth of that of the introduction of the contraceptive pill (Bailey, 2010). Specification checks examining the exact timing of the effects suggest that the main findings are not driven by unobserved factors slowly evolving over time: the effects of television are detectable right after the introduction year but not before. Moreover, the main conclusions are robust to the inclusion of flexible time-polynomials, World War II controls, and proxies for other events that threat the validity of the results.

Qualitative evidence suggesting that television crowded out other (leisure and non-leisure) activities and increased household's consumption points towards an increase in the opportunity cost of childbearing as an important mechanism. In particular, television may reduce fertility as a consequence of an increase in the value of leisure of the primary caregiver that translates into a higher opportunity cost of time allocated into childrearing. On the other hand, television's role in boosting consumerism may have increased the opportunity cost of children by increasing female labor force participation (needed to contribute to the family income) or by increasing the standards (and costs) of living.

3.2 Institutional Background: The Introduction of Television to the United States

The Federal Communications Commission (FCC) licensed television's commercial broadcasting for different areas at different points in time. The process started in 1941 and virtually reached all counties in the United States by the mid-1950s, leaving approximately a 15-year gap between the earliest and latest receivers.

It must be acknowledged that the introduction of television was not random: on average richer and more populated areas received television first. Previous studies have pointed out that, while the FCC aimed at maximizing the size of the population covered by television signal, broadcasting companies applied for licenses to broadcast over markets with higher purchasing power (Gentzkow and Shapiro, 2008b). As a consequence, the area's average income and population size are strong predictors of entrance (Gentzkow, 2006).[1]

Although the *order* in which television arrived to different areas was not random, two key historical events affected the spacing of the timing of the introduction: World War II and a technical freeze imposed by the FCC. After the first groups of counties received television in 1941, the government banned the construction of new stations during the war in order to preserve construction material. Areas where broadcasting started before the war, however, were not affected by the ban. The construction of new stations was resumed in 1945, introducing a four-year gap between the first and the more recent receivers. Then, the post-war years witnessed a rapid geographical

[1] As far as I know, there is no evidence that the licensing process strictly followed a particular rule that determined the sequence of licensed areas.

expansion until 1948, when a freeze imposed by the FCC prevented the assignment of new broadcasting licenses until 1952. The freeze was established because of technical issues regarding the allocation of air spectrum and the future arrival of color television. After the freeze, most of the remaining areas received television signal in only a couple of years.

An additional source of idiosyncratic variation comes from heterogeneity within the areas covered by television signal. In particular, given the somewhat fixed technology of broadcasting antennas, a single television station reached on average 15 counties. Thus, developed and populated cities were not the only early receivers; small towns that happen to lie within a 100-mile radius from the cities' antennas also received television early on (Gentzkow, 2006).

The adoption of television in areas covered by commercial broadcasting (i.e., take-up) evolved at record speeds and was demographically broad. Between 1948 and 1955, approximately two-thirds of the households in the United States installed a television set; and, by 1960, television penetration reached nine out of ten households (Spigel, 1992). Surveys annually collected by the Market Research Corporation since 1949 (and reported by Bogart, 1958) suggest that television ownership was not concentrated among the richest; as early as 1949, for instance, television ownership was evenly spread across the first three upper quartiles of the income distribution.[2]

Moreover, television watching was significant since the early days. By 1948, prime time schedule was almost full (Spigel, 1992). According to the Nielsen Company, the average household allocated four and a half hours into

[2] The first three upper quartiles of the income distribution owned 30, 30, and 27 percent of the total number of television sets in 1949, respectively. The bottom fourth owned 13 percent.

television viewing as early as in 1950, and this statistic only increased over time (Bogart, 1958). Women, in particular, allocated a substantial number of hours into television watching. The earliest piece of evidence I am aware of is a state-wide survey conducted in Iowa in 1954: women over 18 years old spent between 4.41 and 5 hours in front of the screen during weekdays, which represented about one-third of their time "awake and at home".[3] In 1955, a nation-wide survey corroborated that women were spending on average 30.08 hours per week watching television (excluding daytime viewing during weekends).[4]

Time allocated in front of the screen crowded out other activities. Television reduced time allocated into other media, face-to-face interactions, conversation, sleep, and others (see Bogart (1958) for a summary of these findings). Moreover, popular discussions regarding television and the family sphere hypothesized that television may have affected marital and parent-child relationships as well as female house-related work (Spigel, 1992).

3.3 Empirical Strategy

3.3.1 Data Sources

I use three main sources of data for the main analysis:

County-level natality data: the main measure of fertility in this paper will be constructed using county-level birth counts available in published volumes of Vital Statistics from the US National Center for Health Statistics. The National Bureau of Economic Research (NBER) entered the relevant

[3] I am not aware of information on time spent in television watching during weekends.

[4] These two surveys are reported by Bogart (1958).

natality tables for the period 1940-1968, and I use them in this study.[5] All births were recorded in the county of residence of the mother. Unfortunately, little information besides the counts is available.[6]

County-level economic and demographic characteristics: All county-level characteristics in this study come from published census volumes of the US Census Bureau (1940-1970) and several volumes of the County and City Data Book (1947, 1949, 1952, 1956, 1962, 1967 and 1972). These figures were entered and made available by Haines (2010), and I use them in this paper.

Television data: I use data on the availability of commercial broadcasting by county from Gentzkow and Shapiro (2008a). In particular, this data set provides the date in which the first broadcasting station started operations in each television market.[7]

3.3.2 Idiosyncratic Variation in the Introduction of Television

Figure 3.1 shows the share of the 1950 US population that, in each year, received television signal for the first time (i.e., share of the population by year of introduction of television). It is clear that television reached three groups of the population at three different points in time. A first group received television before World War II (1941 or earlier), a second group after World War II but before the FCC freeze (1945-1950), and a third group after the FCC freeze (1952 or later). I will name these three groups as pre-WWII

[5] I acknowledge financial support from NIA grant P30-AG012810 through the NBER.

[6] Race of the mother is available, although not systematically for every county and year. No information on parity or mother's age is available during this period.

[7] Television markets, and therefore the counties that compose it, were defined based on Nielsen's definition of Direct Marketing Areas (DMA) in 2003. Previous studies have argued that the early definitions of television market closely match the 2003 DMAs (Gentzkow, 2006; Gentzkow and Shapiro, 2008b).

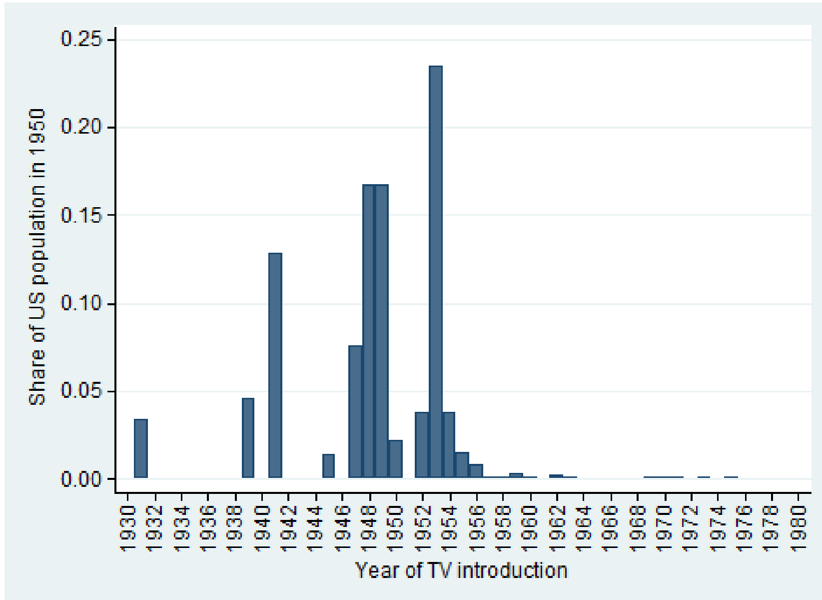

Figure 3.1: Share of 1950 US Population by Year of Introduction of Television.

(1941 or earlier), pre-freeze (1945-1950) and post-freeze (1952 or later).[8]

Figure 3.2, shows the share of counties that received television in each year and illustrates that only a handful of areas received television before World War II, with the bulk of the geographic expansion happening before and after the FCC freeze. Moreover, the comparison between Figure 3.1 and Figure 3.2 suggests that those areas that received television first may have been considerably denser than those that receive it later.

Table 3.1 explores this more formally by comparing the characteristics of the average county in each of the three groups. Averages for the pre-WWII, pre-freeze, and post-freeze counties are shown in columns 1 through 3, respectively. Column 4 shows the difference between the pre-WWII and

[8] Two clarifications are needed. First, although commercial television only became available in 1941, some counties already had experimental stations before 1941. The graphs show the year of entrance of the experimental station if there was one.

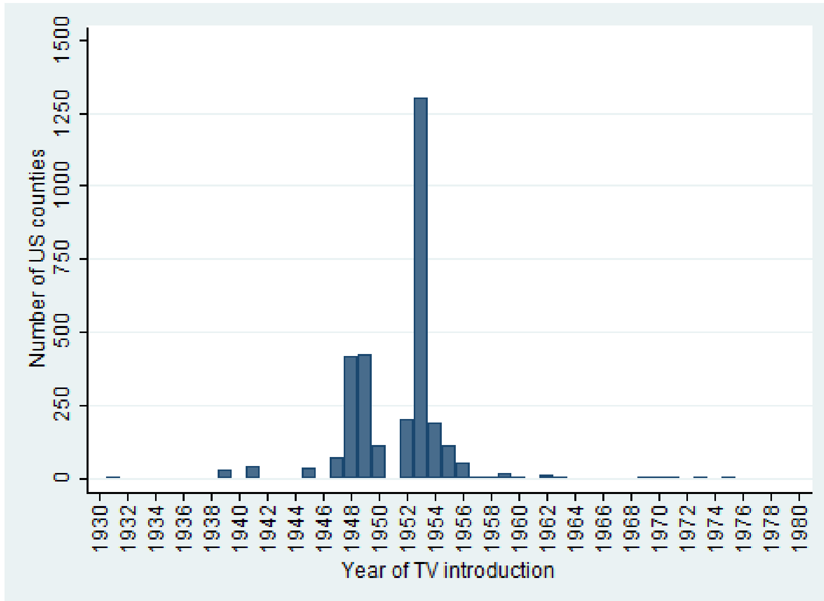

FIGURE 3.2: Number of Counties by Year of Introduction of Television.

pre-freeze groups while column 5 does the same for the pre-freeze and post-freeze groups. All variables were measured in 1940 except income per capita, which was measured in 1959.[9]

It is clear that the counties that received television first, those in the pre-WWII group, are on average richer, more populated, and with higher levels of education. Also, more females participate in the labor force, although the same is not true for males, perhaps because an important proportion of young men over 14 years old are still investing in education in these areas. The differences are less dramatic between the pre-freeze and post-freeze groups than between the pre-WWII and pre-freeze groups. The biggest differences are in terms of income, population size, and population density.

The problem with these differences is that a simple comparison between groups may result in spurious estimates if the evolution over time of observed

[9] Income per capita was obtained from the census website.

Table 3.1: 1940 County Characteristics by Period of TV introduction

	(1) pre-WWII	(2) pre-freeze	(3) post-freeze	(1)-(2) difference	(2)-(3) difference
1959 income per capita	7962.58	5452.13	5109.75	2510.45***	342.39***
	(170.76)	(46.76)	(34.75)	(179.54)	(58.40)
Total population ('000)	335.38	53.35	24.45	282.03***	28.91***
	(19.86)	(5.44)	(4.04)	(33.44)	(3.06)
Density (population per sq mile)	1120.12	170.25	51.47	949.86***	118.79***
	(89.00)	(24.37)	(18.11)	(148.78)	(21.21)
Total white population (%)	96.23	89.32	87.74	6.91***	1.58*
	(2.00)	(0.55)	(0.41)	(1.81)	(0.69)
Population 25+ with high school (%)	23.93	19.75	20.11	4.18***	−0.36
	(0.85)	(0.23)	(0.17)	(0.88)	(0.29)
Males 25+ with high school (%)	22.26	17.28	17.16	4.98***	0.12
	(0.77)	(0.21)	(0.16)	(0.83)	(0.26)
Females 25+ with high school (%)	25.70	22.38	23.44	3.32***	−1.06**
	(0.99)	(0.27)	(0.20)	(0.96)	(0.34)
Males 14+ in labor force (%)	77.47	78.43	79.48	−0.96	−1.05***
	(0.46)	(0.13)	(0.09)	(0.52)	(0.16)
Females 14+ in labor force (%)	25.82	18.56	18.14	7.26***	0.42
	(0.73)	(0.20)	(0.15)	(0.79)	(0.25)
N	80	1067	1932		

Columns 1 to 3 show point estimates of averages across counties for each group. The forth column shows the difference between the pre-WWII and the pre-freeze groups. The fifth column shows the difference between the pre-freeze and the post-freeze group. Standard errors are shown in parenthesis. Stars represent statistical significance: *** p<0.01, *** p<0.05, and * p<0.10.

and unobserved factors correlates with fertility and the introduction of television. For instance, fertility in the US is usually negatively correlated with income within a cross section (Jones and Tertilt, 2006). Population density may affect the cost of children if it is seen as a proxy for the price of space. Parental education may be related to higher levels of investment in children and lower fertility. Murphy et al. (2008) document that the US trends for population density and parental education are negatively correlated with fertility.

I will address these concerns in several ways. First, I will restrict my attention to the pre-freeze and post-freeze counties, which seems safest. Second, in the main empirical strategy, I will try to control for a number of factors that may have had differential impacts over time. Third, I will exploit the exact timing of the introduction of television to different markets to test for observed or unobserved pre-existing trends that may have confounded the effect of television availability.

3.3.3 Pre-Freeze and Post-Freeze Markets

Figure 3.3 depicts the evolution of the average General Fertility Rate (GFR) in the pre-freeze and post-freeze markets from 1940 to 1968.[10] Despite the differences found in terms of 1940 characteristics in Table 1, the GFRs evolve very similarly for the period 1940 to 1947 – the years that preceded the consolidation of the television industry. This pattern preliminarily suggests that no differential trends in observed or unobserved characteristics affected the evolution of fertility differently across groups. Also, notice that the spike in births after World War II takes place in both groups, which preliminarily suggests that the return of soldiers probably affected the two groups similarly.[11]

The first television market that received commercial broadcasting in the pre-freeze group did in 1945 (vertical red line). Once the television industry had taken off and reached more areas in 1947, we start observing a separation between the two groups: the pre-freeze groups has a lower GFR relative to that of the post-freeze group. In 1952 (vertical red line) and especially in 1953, most of the counties in the post-freeze group are reached by television signal. Potentially, as a result, the gap between the two groups starts to close since 1955.

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[10] I construct the GFR by dividing the number of live births in each county-year by the number of women ages 15 to 44 and multiply this ratio by a thousand. The relevant population of women is interpolated across intercensal years.

[11] By 1947, 90 percent of the soldiers that were mobilized had already returned to the US.

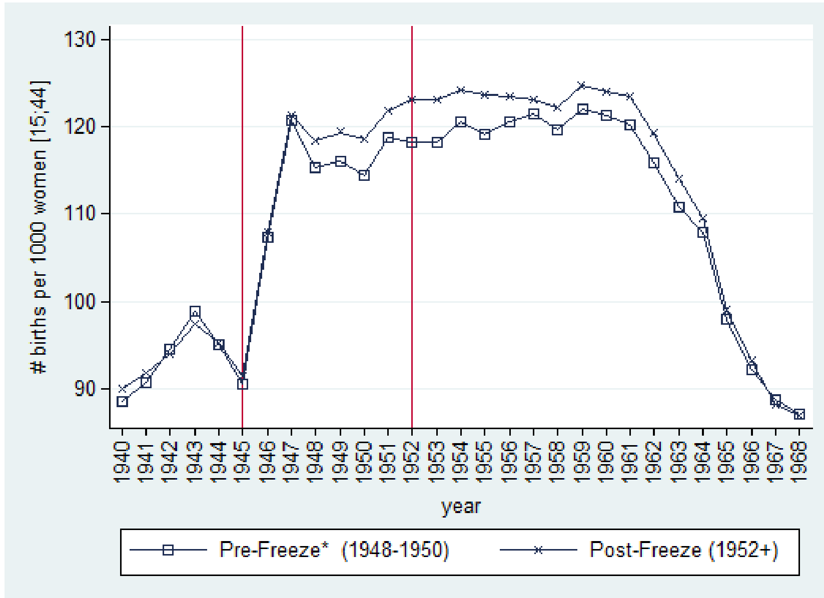


FIGURE 3.3: Fertilty Rate by Pre-Freeze and Post-Freeze Counties

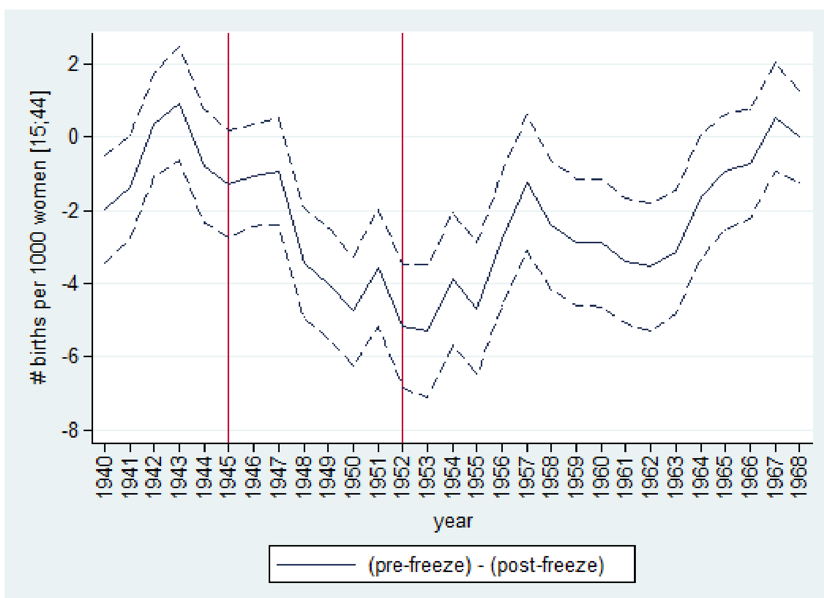


FIGURE 3.4: Difference in Fertilty Rate by Pre-Freeze and Post-Freeze Counties

Figure 3.4 shows the evolution of this gap (calculated as the GFR of the pre-freeze group minus the GFR of the post-freeze) with 95 percent confidence intervals. The average difference between 1940 and 1947 is not statistically different from zero. The gap reached its most extreme value in 1953 and then starts to shrink a year after television reached the post-freeze markets.

3.3.4 Main Equations

Restricting the analysis to the pre-freeze and post-freeze groups, the main empirical strategy of this paper will exploit the timing of introduction to different areas. Notice that although the order in which the introduction took place is not random (as suggested by Table 1), the spacing in time caused by the FCC freeze possibly is. To investigate the effects of exposure to television on county-level GFRs I estimate equation 1:[12]

$$GFR_{ct} \;=\; \beta \times TV_{ct} \;+\; \alpha_c \;+\; \gamma_{rt} \;+\; X'_{ct}\Pi \;+\; \epsilon_{ct} \tag{3.1}$$

where GFR_{ct}, the General Fertility Rate for county c in year t, is constructed by dividing the number of live births in each county-year by the population (in thousands) of women ages 15 to 44. The denominator is linearly interpolated across intercensal years.

County-level fixed-effects, α_c, and year-region fixed-effects, γ_{rt} , are included. These sets of controls should correct for constant county-level characteristics and for aggregate patterns across time that are correlated with the introduction of television and fertility, respectively. The variable of interest is TV_{ct}, an indicator that takes value 1 if the county started to receive commercial broadcasting in year $t-1$. Note that this definition considers the delayed

[12] This follows the specification in Gentzkow (2006) adapted to the study of fertility.

effect of television on births due to pregnancy. Thus, $TV_{ct} = 1[t > T_c]$, where T_c is the year in which county c received commercial broadcasting for the first time. The coefficient of interest is β, the change in the GFR as a result of exposure to commercial television broadcasting.

The vector X'_{ct} includes county-level characteristics to correct for differential trends that may be correlated with the timing of adoption and the outcome of interest. In general, I show results for different sets of covariates measured at baseline (either in 1940, 1950 or both) interacted with high-order time polynomials. In particular, I progressively control for baseline GFR, log population, log income, county's age structure, population density, percentage white, education controls by gender, percentage of the population in the labor force (by gender), and employment (by gender).

The assumption for identification in equation 3.1 is that conditional on the set of covariates, no unobserved trends or shocks are simultaneously correlated with exposure to television and the dependent variable.

Notice that the time interactions will only correct for smooth spurious correlations over time as long as they can be predicted from the baseline. Thus, I follow two approaches to investigate potential issues. First, in the absence of yearly county-level data, I drop the time interactions and control with linearly interpolated covariates across decennial censuses. The advantage of these "synthetic" yearly measures is that they contain more information than baseline characteristics. The disadvantage is that they are less flexible (i.e., linear between censuses). Second, I formally exploit the timing of the introduction of television to verify if preexisting trends drive the results. In particular, equations 3.2 and 3.3 address this issue.

Equation 3.2 offers a way of studying if pre-existing trends that systematically correlated with the introduction of television could be driving the results. The coefficients of interest are those of the set of dummy variables $RelativeYear^{j}_{ct}$,with $j = -11, 10, \ldots, 11$. Each of these indicators takes value 1 if the calendar year t is the $j-th$ relative year for county c since television was introduced. The year of introduction is denoted as zero. In particular, this specification will allow us to determine if "future" introduction has an effect on fertility:

$$GFR_{ct} = \sum_j \delta_j \times RelativeYear^{j}_{ct} + \alpha_c + \gamma_{rt} + X'_{ct}\Pi + \epsilon_{ct} \tag{3.2}$$

Finally, equation 3.3 is a simplification of equation 1 that allows a better inspection of potential unobserved confounding factors. $PreFreeze_c$ is a dummy variable that takes value 1 if county c is part of the pre-freeze group and 0 otherwise. The set of variables $Year_t$ are dummy variables for each calendar year. All other variables are the same as in equation 1. The coefficients of interest are ρ_t, with $t = 1940, 1941, \ldots, 1968$. These coefficients should only show effects once the pre-freeze markets receive television (1945+) and not before, and the effects should become smaller once the post-freeze group receives television (1952+).[13]

$$\begin{aligned} GFR_{ct} &= \rho \times PreFreeze_c \\ &+ \sum_t \lambda_t \times Year_t + \sum_t \rho_t \times Year_t \times PreFreeze_c \\ &+ \alpha_c + \gamma_{rt} + X'_{ct}\Pi + \epsilon_{ct} \end{aligned} \tag{3.3}$$

[13] Note that ρ and the set of parameters λ_t are not identifiable since county and year-region fixed effects are included. I have preferred to display them as part of equation 3.3 for a clearer exposition.

I cluster standard errors at the television market level in all equations.

3.4 Results

3.4.1 The Effect of Television on General Fertility Rates

Table 3.2 reports the effect of television availability on GFRs by estimating equation 3.1. The first column includes county and region-year fixed effects but no other controls. The reported coefficient is negative and statically significant: the introduction of television is associated with 1.76 fewer births per thousand women ages 15-44.

The introduction of television, however, was not random and the characteristics that determined the suitability for the new medium may have evolved differently for the early receivers relative to later receivers. Thus, I progressively add different set of controls interacted with 4th order time-polynomials in columns 2 through 6.

Column 2 reports the effect of television after controlling for 1940 baseline GFR and proxies for the main determinants of the introduction. The estimated effect is still negative and precisely estimated. In particular column 2 includes 1959 log per capita income, log population, percentage urban and ensity (population per square mile). Moreover, the specification also controls for the age structure of the relevant female population: women ages 15-19, 20-24, and 25-34 as a percentage of the population of women ages 15-44 (the omitted category is women ages 35-44).[14] This set of controls (excluding 1959 log per capita income and 1940 baseline FGR) are labeled as "1940 baseline controls set 1" in the table.

Additional controls are added in column 3, but the results are not largely

[14] It was not possible to reconstruct 5-year age groups from every census.

changed – television is associated with 1.32 fewer births per thousand women in childbearing age, and this effect is statistically significant at 1 percent levels. The additional set of controls (labeled as 1940 baseline controls set 2 in the table) include percentage white and the following variables by gender: percentage of males and females ages 25+ with completed high school, percentage of males and females ages 25+ that completed from 1 to 4 years of college, percentage of males and females age 14+ participating in the labor force, and percentage of employed males and females age 14+. The education and labor market condition proxies should correct for the usually negative correlations between education and fertility (e.g. Murphy et al., 2008) and female labor force participation and fertility (e.g. Bailey, 2006).

Columns 4 and 5 are analogous to columns 2 and 3, respectively, but they include baseline covariates measured in 1950. Arguably, the decision to introduce television to the pre-freeze and post-freeze markets is better described by covariates from the 1950 census. The controls for baseline GFR and log per capita income, however, remain as in previous columns. Again, the effect of television is negative and precisely estimated. Column 6 pools together the 1940 and 1950 "baseline" controls. The main conclusions do not change.

Columns 7 and 8 try to address the concern that baseline characteristics interacted with high-order time polynomials may not be able to predict the actual evolution of different variables. In the absence of yearly data, I linearly interpolate the relevant controls using information from the 1940-1970 Decennial Censuses and several County and City Data Books (Haines, 2010). These interpolated controls have more information than measurements at baseline. However, they are less flexible (i.e., linear). Column 7 keeps the

1940 baseline GFR and 1959 log income and adds a first set covariates equivalent to those of columns 2 and 4. Column 8 additions further controls, but these are not exactly analogous to those in columns 3 and 6. Unfortunately, information by gender is not available for some covariates. Thus, the controls in Column 8 are percentage white, percentage of population ages 25+ with completed high school, percentage of males and females 14+ participating in the labor force. Moreover, labor force in 1960 and 1970 was defined as civilian labor force only. With these caveats in mind, the evidence suggests that the estimate of the parameter of interest remains mainly unchanged.

In conclusion, the evidence drawn from Table 2 suggests that television had a negative effect on fertility, measured as the GFR. The effect is robust in magnitude and statistical significance to the inclusion of covariates potentially associated with both the introduction of television and fertility behavior. These results, however, could be spurious if unobserved or observed but not well-measured factors are systematically correlated with the introduction of television and the GFRs. For instance, the pre-freeze and post-freeze counties may have been catching-up with the fertility patterns of the most developed (pre-WWII) counties, and this process could have taken place in the pre-freeze counties first and in the post-freeze counties later on. In the next section, I discuss the extent to which potential threats like this may be driving the results.

Table 3.2: Impacts of Television Exposure on Fertility

Dependent variable	Number of births per 1000 women ages 15 to 45							
Sample	Pre-freeze and Post-Freeze Counties							
TV	-1.7594^{***}	-1.6287^{***}	-1.3228^{***}	-1.6859^{***}	-1.5659^{***}	-1.4257^{***}	-1.6867^{***}	-1.8380^{***}
	(0.6143)	(0.5973)	(0.4556)	(0.5850)	(0.4676)	(0.4279)	(0.6188)	(0.6112)
County FE	X	X	X	X	X	X	X	X
Year-region FE	X	X	X	X	X	X	X	X
Controls interacted with 4th order time-polynomials:								
1940 fertility		X	X	X	X	X	X	X
1959 income		X	X	X	X	X	X	X
1940 controls set 1		X	X			X		
1940 controls set 2			X			X		
1950 controls set 1				X	X	X		
1950 controls set 2					X	X		
Interpolated set 1							X	X
Interpolated set 2								X
N	86906	86906	86906	86906	86906	86906	86906	86906
# of Counties	2999	2999	2999	2999	2999	2999	2999	2999
# of TV markets	199	199	199	199	199	199	199	199
Average GFR	109.98	109.98	109.98	109.98	109.98	109.98	109.98	109.98

TV is a dummy variable for the availability of television broadcasting. Associated standard errors clustered at the television market level shown in parenthesis. Stars represent statistical significance: *** $p<0.01$, *** $p<0.05$, and * $p<0.10$.

3.4.2 Specification Checks: Exploiting the Timing of the Introduction

It is always difficult to rule out the existence of omitted factors evolving over time that may be correlated with the treatment under analysis; however, a large family of potential threats can be dismissed by comparing the evolution of the outcome of interest before and after the sharp start of the treatment. In particular, if any confounding omitted factors is both smoothly evolving over time and correlated with the treatment, then the outcome of interest may start to behave as if the treatment were already in place but before it has begun. The implication for the case under analysis is that future access to television should not predict a decrease in GFR.

The estimation of equation 3.2 should be useful to illustrate if any factors that evolve smoothly over time are a threat to identification. Examples of these variables could be economic development, the convergence of fertility patterns across geographic areas, infant mortality trends, maternal health trends, changes in the return to education for children, etc.

The point estimates of the parameters associated with the $RelativeYear_{ct}^{j}$ dummies in equation 3.2 are plotted in Figure **??** along with 95 percent confidence intervals. The set of controls employed are those of column 3 in Table 3.2, which yielded the most conservative estimate. The omitted category is relative year zero, the year in which television enters each county – given that the outcome is the GFR, no major effects should be expected within the year of introduction.

The graph suggests that the negative effects of television on the GFR start right after its introduction and not before. In other words, future introduction (relative years -1 and before) are not associated with a decline

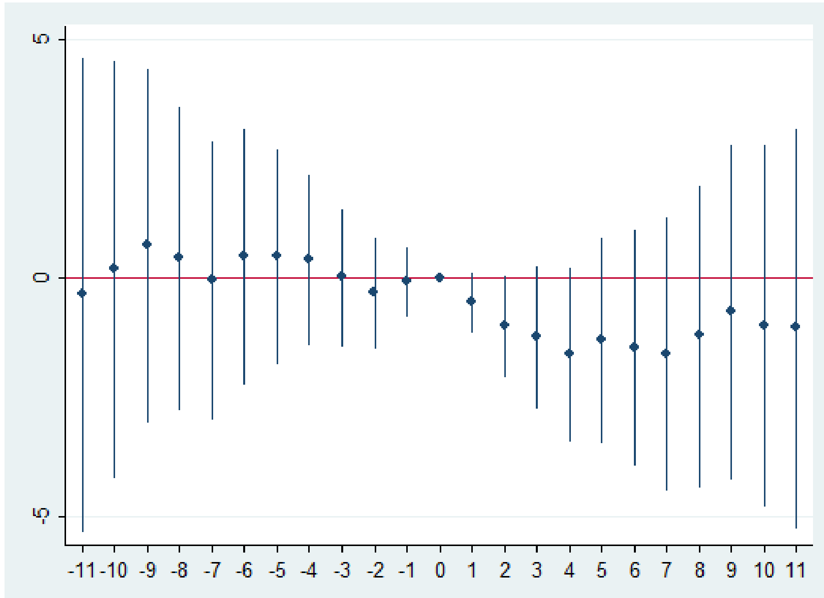


FIGURE 3.5: Effect of Television by Relative Year

in fertility. Indeed, averaging across the first two years after the introduction of television (relative years 1 and 2) already yields a negative and statistically significant difference with respect to the omitted category (relative year 0): average = -0.7424 and SE=0.4016 (p-value=0.066). On the other hand, the average of the two years previous to the introduction (relative years -1 and -2) yields an average = -0.1886 and SE = 0.4530 (p-value = 0.678).

The fact that the effects can start to be detected right after the introduction is consistent with previous works studying the roll-out of cable television in India (Jensen and Oster, 2009), the spread of a particular television network across different areas in Brazil (La Ferrara et al., 2012), and with the effects of mass media more generally.

The last piece of evidence comes from Figure 3.6, which plots the coefficients of the parameters associated with the difference in difference strategy of equation 3.3 (ρ_t,"for" t=1941,...,1968) along with 95 percent confidence

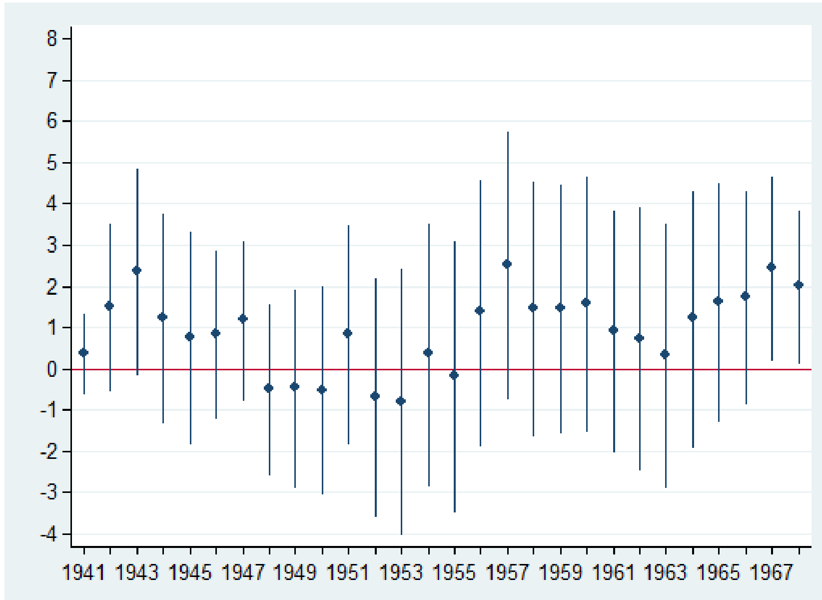


Figure 3.6: Effect of Television by Year

intervals. The baseline category is the calendar year 1940. Again, the controls used for this estimation are those of column 3 in Table 2.

Three conclusions can be drawn from this Figure. First, the general timing of the effect of television seems correct. The point estimates for the pre-freeze group during the years 1948-1953 are shifted downwards compared to the point estimates for other years. In particular, the downward shift coincides with the timing of the introduction of television in the pre-freeze counties, while the jump to "pre-treatment" levels in 1954 and 1955 is consistent with the post-freeze group receiving television in 1952 and mostly in 1953. On average, the point estimates from 1941 to 1947 are not any different than those from 1954 to 1968.[15] However, on average, those from 1948 to 1953 represent 1.618 (SE=0.523, p-value = 0.002) fewer births when compared to the other years. Second, the effects are not driven by a sin-

[15] The difference between the two averages is -0.1282 (SE= 1.029, p-value=0.901)

gle one-time shock correlated with the presence of television in one of the groups. Third, any confounding variable would need to follow the pattern described by the figure: it would need to create a gap between the GFRs of the pre-freeze and post-freeze exactly from 1948 to 1953, and this gap would need to close-up right after the post-freeze group received television.

The findings from this section can be summarized as follows. First, the empirical evidence suggests a negative effect of television on GFRs that is statistically significant and robust to different specifications. Second, the effects are well timed, and future introduction of television do not predict lower GFRs. This suggests that the effects are not being driven by unobserved factors slowly evolving over time. In fact, the effects of television are detectable right after the introduction year (but not before). Third, the effects are not driven by a sharp, one-time shock in one of the groups. Fourth, if a confounding factor is driving the effects, then it must be one that reduces the GFR in the pre-freeze group relative to the post-freeze group from 1948 to 1953 and then closes the gap between the two groups right after television reaches the post-freeze markets.

3.4.3 Mechanisms

There are two channels through which television may have impacted fertility, and both can simultaneously play a role in explaining the reduced-form results found in the previous sections.[16] First, under the assumption that childrearing is costly in terms of time, television may reduce fertility as a consequence of an increase in the value of leisure of the primary caregiver. Even more trivially, television may crowd out time previously allocated into sex-

[16] This section heavily draws from Bogart (1958); Spigel (1992); Howard-Williams and Katz (2013)

ual intercourse.[17] On the other hand, exposure to information flows through television (i.e., content) may affect attitudes and behavior in ways that result in lower demand for children.

In this section, I briefly elaborate on how early television may have impacted fertility through each of these mechanisms.

The Value of Leisure

The introduction of television can be understood as an increase in the value of a particular leisure activity. As such, it is theoretically possible that this translates into a higher opportunity cost of time allocated to other leisure and non-leisure activities. Indeed, previous qualitative research and survey evidence focusing on the early days of television in the US have emphasized changes in the time-allocation of television adopters that seems consistent with such prediction. It is still and empirical question, however, whether this could have reduced the demand for children (a good that imposes a non-trivial time cost for the primary caregiver) or simply crowded out time allocated into intercourse.

The amount of time allocated into television viewing was non-negligible since its early days. According to measurements of the Nielsen Company, the average household allocated four and a half hours into television viewing as early as in 1950. By 1956, this average had increased to five hours a day. Women were responsible for the highest number of hours in front of the

[17] Although it is possible to argue that television may have the opposite effect on sexual intercourse, this seems unlikely during its early days. First, survey evidence from 1954 indicates that television sets were physically located in living rooms in 85 percent of the cases, 3 percent in bedrooms, and 12 percent in rooms. Second, television watching was considered a family activity during the evenings. For instance, survey evidence from 1954 shows that children amounted for an important proportion of the evening audience (6:00 PM to sign-off): 21 percent during weekdays, 38 percent on Saturdays, and 18 percent on Sundays.

screen. A statewide survey among television owners conducted in 1954 in Iowa recorded the time spent watching television during weekdays for different individuals (as opposed to households): women over 18 years old living in urban areas spent on average 4.41 hours while those in less densely populated areas spent over 5 hours. Although at this point in history women probably spent more time at home than other household members, this viewing habit represented, on average, 29 percent of their "in-home and awake" time during weekdays. Men over 18 years, on the other hand, allocated 34 percent (around 3 hours). One year later, a nationally representative survey conducted by the American Research Bureau found that women spent on average 30.08 hours per week watching television while men spent 19.62.

Time allocated in front of the screen crowded out many other activities. For instance, other media, face-to-face interactions, conversation, sleep, and social events outside home were affected by television. Moreover, popular discussions at the time of the introduction of television questioned if the new medium would have pervasive effects on a number of family matters. For instance, marital relationships, children's homework and school performance, and housewives' house-related work (Gentzkow and Shapiro, 2008b; Howard-Williams and Katz, 2013; Spigel, 1992).

From this point of view, it is theoretically possible that television, through the increase in the value of leisure, reduced fertility by reducing sexual intercourse or the demand for children per-se (or both).

Television Content

The content of early television may have affected the attitudes and behavior of its audience in ways that resulted in a lower demand for children.

Previous work has shown that specific content can affect fertility. Studies in India and Brazil suggest that portrayals of urban family lives and role models are capable of changing gender attitudes and fertility behavior in less densely populated areas, where women usually have more children and play a less "progressive" role in the family circle (La Ferrara et al., 2012; Jensen and Oster, 2009). In a different context, two recent studies focusing on the US found that females in teenage years altered their reproductive behavior when exposed to a specific show aired in the 2000s that depicted the lives of pregnant teenagers (Kearney and Levine, 2015; Trudeau, 2015).

Interestingly, the golden era of television (the 1950s and 1960s) has been known for emphasizing the traditional role of genders: women were overwhelmingly represented as mothers and "ideal" housewives (Press, 2009). This, however, does not necessarily imply that female spectators would react by mimicking the role models portrayed in television. The relationship between the medium and its audience can be far more complex. Here I enumerate two hypotheses that could reconcile the findings in the empirical section (lower fertility) with the content of early television.

First, some authors argue that it was precisely the programming emphasizing traditional role of genders what contributed to women's social self-recognition as a disadvantaged minority with respect to men. Moreover, sitcoms, a genre in which an increasing number of female actors played central roles, often used the traditional role of genders as a source of comedy that could have implicitly challenged the status quo (Howard-Williams and Katz, 2013). From this point of view, television may have (perhaps paradoxically) triggered in households a progressive attitude and behavior.

Second, television was recognized as a powerful medium for advertisement since its very beginning, and its role in boosting the consumer's product industry may be associated with lower fertility. The profitability of broadcasting stations was determined by the advertisers that saw in the broadcasting area a market where to sell their goods. Survey evidence from the early days of television supports the view that advertisement in television was effective and welcomed by its audience. Some authors argue that to keep up with the post-WWII new standards of consumption, many married women joined the labor force to contribute to the family income (Spigel, 1992). Thus, if television prompted higher levels of expenditure in its audience, higher standards and costs of living may have been translated into lower fertility. Moreover, if higher consumption implied increased female labor supply, an important and well-documented trade-off between labor participation and demand for children may have been in place (Doepke et al., 2015).

3.5 Concluding Remarks and Further Research

In this paper, I show causal evidence of the impacts of the introduction of television to the United States on fertility. To get at causality, I exploit temporal and spatial variation on the availability of commercial broadcasting in a difference-in-difference framework. The main results are robust to a number of specification checks including the inclusion of additional control variables and pre-trends analysis. In addition, I document qualitative evidence consistent with likely mechanisms at play. Overall, I present results that enrich our understanding of the complex demographic patterns of the 1950 and 1960 U.S.

In this vein, this study motivates further work in two directions. First, the county-level empirical strategy employed in previous sections has some shortcomings. For instance, little information can be obtained from the dependent variable besides the number of births per county-year. Variables such as parity and age of the mother are not available. Moreover, most of the controls were limited to information available in published volumes of the census and County and City Data Books (Haines, 2010). These factors complicate specification checks, placebo tests, and the exploration of mechanisms.

To overcome this challenge, I propose analyzing individual and household level data from the 1960 U.S. Census.[18] The 1960 decennial census can be used to reconstruct the history of births for each woman in childbearing age. In particular, women in the age interval 15 to 59 at the time of the survey (i.e., born from 1921 through 1945) and recreate their most recent 15-year history of births by exploiting information on family relationships from household rosters complemented with data on year-of-birth for each family member.[19] Focusing on this subgroup of women ensures observing most female "at risk" of birth (i.e., in the age interval 15 to 44) at every point in time from 1945 to 1960. Note that this period coincides with the timing of television introduction into the pre and post freeze markets.In addition, one of the advantages of an individual-level strategy is that it is possible to study effects on different parities and spacing.

The 1960 census included an extended questionnaire that was applied to all household members in one out of four households (which makes it the "densest" census in the history of the US). This questionnaire asked a num-

[18] Geographic county-level identifiers are not available in IPUMS for the censuses starting in 1950. I need these identifiers for the treatment variable to establish if a county is covered by television signal in a particular point in time.

[19] The 1960 census recorded month and year of birth for every household member.

ber of items that are relevant to the topic under analysis (veteran status, the length of residence in the current dwelling unit, education and occupation for all household members, etc.). A disadvantage of census questionnaires, however, is that they are not designed to extract information about pregnancies and fertility choices.

Thus, I propose complementing the analysis using the Growth of American Families Survey (GAF) from 1955. This survey has rich information on birth histories and pregnancies, use of contraceptives, attitudes towards pregnancy and child-rearing, etc. Despite its relevant information for this project, it seems safest to only use the results of the GAF as a complement to those of the Census: only 97 counties were part of the survey, which complicates executing specification checks that exploit county heterogeneity within television markets. Moreover, the census seems to have better information on demographics.

Second, these individual-level datasets can be used to investigate the potential causal pathways (the value of leisure and television content). For instance, TV may have been associated with other changes such as increases in female education or in age at first marriage. Evidence along these lines would suggest that "progressive" or "urban" norms are at play. In this vein, future research may have the potential to enrich the qualitative description of previous studies.

4

Electing Criminal Politicians to Public Office

4.1 Introduction

Do criminal politicians—individuals previously convicted for corruption, financial fraud, among other crimes—provide public goods any differently? The exchange theory suggests that voters may condone rent-seeking behaviors in exchange for other benefits, such as the delivery of public goods, economic growth or private gains (Winters and Weitz-Shapiro, 2017). In addition to direct private gains, voters in different settings, such as in the world's largest democracy, India, or the country under analysis here, Peru, may be willing to condone rent-seeking behaviors in exchange for public goods that promote economic development (Datum, 2014; Vaishnav, 2017); even in some settings, rent-seeking behaviors may be associated with competence (Levine and Rubinstein, 2017).

This paper aims to test if potential rent-seekers in Peru—criminal politicians—provide public goods any differently. In weak institutional settings, corruptible politicians may react to rent-seeking incentives and could provide different public goods, prioritizing those that offer higher rents.

To do so, this study uses a unique data set with information on the characteristics of every politician running for public office in Peru, including criminal records and political histories, that is matched to detailed project-level government spending records and infrastructure data in Peruvian municipalities. Using a regression-discontinuity design in a close election framework to gain causal identification, this paper will document the consequences of electing a criminal politician for mayoral office on (i) public spending, (ii) the actual provision of public goods, (iii) local development, and (iv) political institutions, such as reelection rates and corruption cases. Current results show a strong correlation between electing a criminal politician for mayor and increased public expenditure. However, the causality of this relationship is challenged by pre-existing levels of expenditure and budget sizes prior to the electoral result. I find evidence of selection of criminal politicians into municipalities with high levels of pre-existing expenditure even within the close-election framework.

Broadly, this paper relates to three bodies of research. First, it refers to a strand of work on corruption dynamics in countries with weak institutions (Olken and Pande, 2012). Second, it contributes to our understanding of factors driving the political selection of potentially low-quality politicians (Besley, 2005; Caselli and Morelli, 2004). Finally, it relates to a growing literature documenting that political leadership and the characteristics of those in office matter for the type and implementation of public policies (Besley et al, 2011; Chattopadhyay and Duflo, 2004; Jones and Olken, 2005; Meyersson, 2014; Pande, 2003, Prakash et al, 2017).

4.2 Context: Municipalities and Close Elections in Peru

Municipalities are the main form of local governance in Peru. Broadly, the Peruvian government has three layers of administrative division: a central government, 26 regional governments, and over 1800 local governments or *municipalities*.[1] Municipalities are important as they can act as decentralized units of the executive branches of the central and regional governments and have direct competence over the territory they govern. For instance, municipalities propose and develop local development plans, provide basic infrastructure, and share responsibility with the central government in managing welfare programs.

In general, municipalities are not resource constraint, and they have a larger budget than what they get to spend (Loayza et al., 2014). Most of the municipal budget is composed by direct transfers from the central government. Municipalities have little competence to raise funds through debt and local taxation. Over this paper's period of analysis, municipalities represented about one-fifth of the total budget of the public sector.

Mayors are the highest municipal authority and are democratically elected every four years following a plurality system (i.e., the candidate with the largest vote share wins). Mayors do not face reelection limits, and there are many candidates per constituency. Moreover, political parties are highly fragmentized. In the electoral period under analysis, 7 political groups participated in the municipal election in the average constituency.

The mayor leads both the executive and the legislative bodies of the municipality. The latter is known as the Municipal Council, and its members

[1] The metropolitan municipality of Lima, Peru's capital, has regional government status.

are elected along with the mayor. The Peruvian electoral rules automatically assign the majority of council seats to the mayor's party, while the remaining seats are distributed among the parties of the nonelected candidates based on their respective vote shares. No significant opposition is expected for the winner party as the council decisions are taken by simple majority rule.[2] Moreover, the municipal council plays a secondary role, and their members are part-time municipal employees.

Although there are province-level and district-level municipalities, this paper will only focus on the latter. Peru's 25 regions are divided into over 180 provinces, which are subdivided into over 1800 districts. Province municipalities have direct competence over the capital district of the province and shared jurisdiction over all districts within the province. On the other hand, district municipalities only have competence within the boundaries of the district. Since there is no sensible way of analyzing the outcomes of capital districts, districts, and provinces at the same time, I restrict my analysis to (noncapital) district municipalities.

Previous studies on the Peruvian context have analyzed the role of municipalities and mayors on local development. For instance, Ardanaz and Maldonado (2016) uncover a non-monotonic relationship between municipal rents from natural resource booms, public good provision, and wellbeing, while Aguero et al. (2016) find that these effects lead to higher school achievement as a consequence of improved school infrastructure and teacher

[2] The number of available council seats is determined before the election based on each constituency' population size. Most municipalities in my sample have either five or seven council seats. Because of the electoral rules, at least 4 out 5 and 5 out of 7 seats are given to the mayor's party. Thus, there is at most one or two council members from opposition parties per council. In addition, the elected mayor is also part of the council and holds a casting vote.

quality. Moreover, Pique (2017) studies mayoral incentives and establishes that higher mayoral wages are associated with increases in political fragmentation and opposition, which translate to negative impacts on the efficiency of municipal investment. Other characteristics, however, such as the mayor's political party affiliation (national vs. local) and incumbency are not related to differences in expenditure and investment patterns (Aragon and Pique, 2016; Aragon et al., 2017). Loayza et al. (2014) study the factors associated with the inability of Peruvian municipalities to spend their allocated budgets and propose that local capacity is one of the main drivers.

4.3 Data and Summary Statistics

4.3.1 Data Sources

This paper uses a unique data set containing verified (i.e. not self-reported) criminal records for the universe of candidates running for mayor in Peru. Electoral results and candidate-level characteristics are from the National Jury of Elections (NJE). The NJE started to collect candidate-level data since 2006 in the form of affidavits, which included demographics, education, and working experience. In addition, in the 2010 election, the NJE verified the criminal records of all candidates. I have access to those data and use them in the main analysis.

Data on municipal accounts are from the Peruvian Ministry of Finance. These data include both budgeted and actual expenditures at the account-level by municipality and year. I use these data to construct municipality-level measures of expenditure by year.

I use satellite data on nighttime lights intensity from the National Oceanic and Earth Administration (NOAA) Earth Observation Group (EOG) as a proxy for economic development. Henderson et al. (2012) establish a strong relationship between GDP growth and light growth at the country level and propose using lights as a complement for national accounts, especially for countries with poor statistical systems. Chen and Nordhaus (2011) find a similar result for sub-national levels of economic activity. Moreover, exploiting the high spatial resolution and panel structure of these data, several studies have relied on nighttime lights as a proxy for economic activity in the absence of other measures (see Donaldson and Storeygard 2016 for applications of remote sensing data to economics and Baskaran et al. 2015, Hodler and Raschky 2014, and Prakash et al. 2016 for studies closely related to this paper).

Two overlapping nighttime light data products elaborated by the EOG are available for the period of analysis.[3] The first product is a set of annual, cloud-free composites of stable lights at the earth's surface for the period 1992-2013, which is constructed based on low-light images of the earth collected by satellites from the Defense Meteorological Satellite Program (DMSP) with onboard Operational Linescan System (OLS) sensors. Each data file is a grid of 1Km$\times$1Km pixels covering the globe recording the intensity of nighttime lights by pixel-year in a scale from 0 (no light) to 63. These satellites scan every point on the earth between 8:00PM and 10:30PM local time in a daily basis, so the intensity of lights recorded in these files corresponds to averages within pixels over the entire calendar year. I refer to this first product as DMSP-OLS. On the other hand, the second product is a set of monthly, cloud-free composites available since May 2012 and

[3] This description is based on Baugh et al. (2010), Elvidge et al. (2017), Li et al. (2017), and NOAA (2014).

represents an improvement with respect to DMSP-OLS. In particular, these composites are built using data from the Visible Infrared Imaging Radiometer Suite (VIIRS) Day/Night Band (DNB) from NOAA's weather satellite Suomi NPP, which has a finer resolution and enhanced low-light capabilities relative to DMSP-OLS. Each pixel-month is around 0.5Km$\times$0.5Km and records average radiance values (nanoWatts/cm2/sr) taken over daily shots at around 01:00AM local time. I refer to the second product as VIIRS-DNB.

I construct a consistent measure of annual growth of nightlight intensity at the municipality level for the period of analysis using both DMSP-OLS and VIIRS-DNB data. The main challenge is that DMSP-OLS data are not available for 2014, while VIIRS-DNB data are only available since 2012. Thus, I first calculate annual growth rates for all years up to 2013 using DMSP-OLS data and then complete the series with annual growth rates starting in 2014 with the VIIRS-DNB data, where the first growth rate (from 2013 to 2014) is calculated using the 2013 VIIRS-DNB data as a base year (i.e., growth rates are calculated within each data product). Before calculating growth rates with the VIIRS data, I follow Li et al. (2017) in subtracting 0.3 nanoWatts/cm2/sr to all pixels to account for low levels of light detected in VIIRS-DNB but not in DMSP-OLS, [4] setting all negative values to zero (no light), and averaging within pixels across months for each calendar year.

Municipal-level characteristics are from the 2007 Housing and Population Census. Areas for each municipality as of 2007 are from the Ministry of Environment.

[4] Explain that this only reduces VIIRS capacity to detect changes in light intensity, but it useful when calculating other measures, such as percent of pixels lit.

4.3.2 Summary Statistics

Table 4.1 shows evidence that municipalities with criminal mayors are different from those with noncriminal mayors in terms of pre-determined characteristics. In particular, column 4 tests for group mean differences across municipalities by mayor type. Municipalities that elect criminal mayors seem more developed. They are more urban, have a higher parentage of households with access to basic services, and have a younger population with higher levels of employment. They are not, however, any different in terms of household size, population size, education levels, or geographic area.[5] Although these differences are informative, they do not address the validity of the empirical strategy employed below.

4.4 The Regression Discontinuity Design

The key problem for identification is that municipalities that elect a criminal candidate for mayor may be systematically different than those that do not, and the observed and unobserved factors correlated with the difference in electoral results may directly affect the outcomes of interest. For instance, municipalities that elect low-quality mayors may exhibit poor economic performance, but these patterns may well be the result of low education levels in those areas. On the other hand, criminal politicians may prefer to run for public office in municipalities that are developing faster as these present more opportunities for rent-extraction. If a higher share of criminal candidates running for office translates to a higher likeliness of electing a criminal mayor, a reverse causality problem could arise when stating that municipalities that elect a criminal mayor do economically better as a result.

[5] Education levels are proxied by school enrollment.

Table 4.1: Summary Statistics by Mayor Type, Municipality-level Characteristics

	Mayor Type			Difference
	All	Criminal	Noncriminal	(2)−(3)
	(1)	(2)	(3)	(4)
	Mean	Mean	Mean	Est. Diff.
	(S.D.)	(S.D.)	(S.D.)	(S.E.)
Log population	8.33	8.47	8.32	0.15
	(1.27)	(1.27)	(1.27)	(0.13)
Population aged 14 or less (%)	33.66	33.40	33.67	-0.27
	(6.58)	(5.96)	(6.63)	(0.59)
Population aged 15-24 (%)	17.11	17.48	17.09	0.39
	(2.85)	(2.89)	(2.85)	(0.28)
Population aged 25-44 (%)	25.59	26.17	25.55	0.62*
	(3.93)	(3.62)	(3.95)	(0.36)
Population aged 45-64 (%)	15.13	14.95	15.15	-0.19
	(3.05)	(2.71)	(3.07)	(0.27)
Population aged 65 or more (%)	8.51	7.99	8.54	-0.55*
	(3.93)	(3.33)	(3.97)	(0.33)
Household size	3.80	3.81	3.79	0.01
	(0.61)	(0.55)	(0.62)	(0.05)
Log area (in km^2)	5.28	5.37	5.28	0.09
	(1.43)	(1.37)	(1.43)	(0.14)
HHs in uran areas (%)	43.70	48.71	43.33	5.39*
	(29.21)	(30.72)	(29.07)	(3.01)
HHs with water (%)	30.45	35.63	30.06	5.56*
	(28.70)	(29.15)	(28.64)	(2.87)
HHs with electricity (%)	53.67	55.27	53.55	1.72
	(25.12)	(23.23)	(25.26)	(2.30)
Female Employment (%)	16.10	18.14	15.95	2.19**
	(10.21)	(10.53)	(10.18)	(1.03)
Male Employment (%)	40.06	44.26	39.75	4.51***
	(13.83)	(12.60)	(13.87)	(1.25)
School Enrollment: age group 3-5 (%)	45.73	45.93	45.71	0.22
	(17.52)	(17.69)	(17.52)	(1.74)
School Enrollment: age group 6-11 (%)	94.07	94.39	94.05	0.34
	(4.65)	(4.35)	(4.67)	(0.43)
School Enrollment: age group 12-16 (%)	86.24	85.97	86.26	-0.29
	(9.42)	(10.37)	(9.35)	(1.01)
Municipalities	1589	110	1479	1589

To overcome this challenge, the identification strategy proposed here compares municipalities where the electoral result was determined by a narrow margin. In particular, I compare municipalities that, by a small difference in voting shares, elect a criminal candidate over a noncriminal runner-up, with municipalities that elect a noncriminal candidate over a criminal runner-up. The (testable) identifying assumption is that the probability of electing a particular mayor type should be the same for municipalities that barely elect a criminal mayor and those that barely elect a noncriminal mayor. In other words, municipalities that have virtually the same probability of electing a particular type of mayor should not be systematically different.[6] If this assumption holds in the data, then municipalities that barely elect a noncriminal mayor are a good counterfactual for those that barely elect a criminal mayor.

In particular, this paper uses an RD design in a close-election framework to estimate the causal effect of electing criminal politicians for public office. RD designs exploit a discontinuity in a known treatment assignment mechanism to leverage plausible quasi-random variation in treatment status and thus gain causal identification of treatment effects (Imbens and Lemieux, 2008; Lee, 2008; Lee and Lemieux, 2010). In general, the treatment assignment mechanism is based on an observed *forcing* variable and a known *cutoff* value, c, at which the discontinuity in treatment status takes place. In this paper's electoral context, let the binary variable $CriminalMayor_i$ indicate treatment status for municipality i, where treatment is defined as electing a candidate with criminal records (i.e., $CriminalMayor_i = 1$). Moreover, de-

[6] However, this setting allows municipal-level characteristics to correlate (and imperfectly influence) the probability of electing a particular mayor type.

fine the forcing variable “margin of victory”, MV_i, as the difference between the largest vote share obtained by a candidate with criminal records minus the largest vote share obtained by a candidate without criminal records in a given municipality. Hence, MV_i takes positive values when a criminal mayor is elected (treatment) and negative values when a non-criminal mayor is elected (control).[7] Thus, assignment to treatment status follows the deterministic function $CriminalMayor_i = \mathbb{1}\left[MV_{it} \geqslant 0\right]$, where $\mathbb{1}\left[.\right]$ is the indicator function, and the discontinuity takes place at $c = 0$. Note that only municipalities with both types of candidates (with and without criminal records) are considered for the analysis. Then, the RD treatment effect[8] of electing a criminal candidate for mayor on a given outcome y_i is given by

$$\text{RD Treatment Effect} = \lim_{MV_i \uparrow 0} \mathrm{E}\left[y_{it}|MV_i\right] - \lim_{MV_i \downarrow 0} \mathrm{E}\left[y_{it}|MV_i\right] \tag{4.1}$$

Which can be estimated through the following equation

$$y_i = \alpha + \beta CriminalMayor_i + f(MV_i) + \epsilon_i \tag{4.2}$$

Where $f(.)$ is an n-order polynomial on the forcing variable, and β represents the RD Treatment Effect. The preferred specification in this paper uses local linear regressions (Imbens and Lemieux, 2008). This approach fits a polynomial of degree one on the forcing variable at each side of the cutoff ($c = 0$) and restricts the estimation sample to municipalities in a neighborhood around

[7] For example, imagine a municipality where the largest share of votes for a candidate with criminal records is 15%, and the largest share of votes for a candidate without criminal records is 10%. Then, $MV_i = 5$, and a mayor with criminal records is elected. Alternatively, imagine a different municipality where $MV_j = -3$; this would indicate that the candidate without criminal records is elected for mayor.

[8] The RD design does not identify the Average Treatment Effect (ATE) for the entire population; instead, it identifies a weighted treatment effect for the entire population where observations that are more likely to be near the cutoff receive more weight (Lee, 2008).

it, such that $MV_i \in (c-h, c+h)$. The bandwidth, h, determines the width of the interval for estimation and is here determined using Calonico et al. (2014) data-driven algorithm for each outcome. To explore the robustness of this estimation framework, I show results for various bandwidth choices, polynomial orders, and estimation techniques. Standard errors are clustered at the municipality level.

4.4.1 Validity of the Regression Discontinuity Design

Here I present two tests for the validity of the RD design: continuity of the forcing variable and covariate smoothness (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). The main assumption for identification in the RD design is that the probability function of the forcing variable is continuously differentiable around the cutoff. The first testable implication of this assumption is that the density function of the forcing variable should not exhibit any "jumps" at $MV_i = 0$. This is, for a given municipality, the probability of barely electing a criminal candidate should have been the same than barely electing a noncriminal candidate. Second, conditional on MV_i, moments of the distributions of pre-determined characteristics should not exhibit any discontinuities at $MV_i = 0$. Thus, "barely treaded" municipalities should not be different than "barely controls".

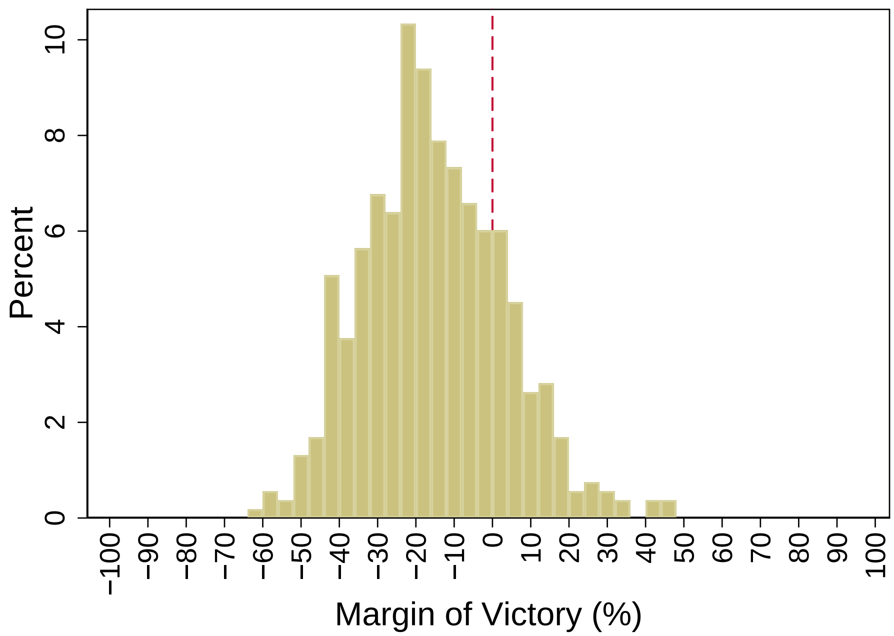
First, I find no evidence of discontinuities in the distribution of the forcing variable around the cutoff. Figure 4.1(a) plots the histogram of the margin of victory of criminal candidates in bins of four percentage points. There are no apparent discontinuities at $MV_i = 0$. Moreover, panel 4.1(b) shows the result of conducting McCrary (2008) density test, which fails to reject the null hypothesis of no discontinuity around the cutoff. Taken together, this is preliminary evidence of no manipulation of the forcing variable.

Second, I find no evidence of discontinuities in a large set of predetermined characteristics measured in 2007. Figures 4.2 and 4.3 plot, for each variable, smoothed linear regressions with 95% confidence intervals at each side of the cutoff against the margin of victory of the criminal candidate. Raw data points and averages for quintiles of the data at each side of the cutoff are also shown.[9]

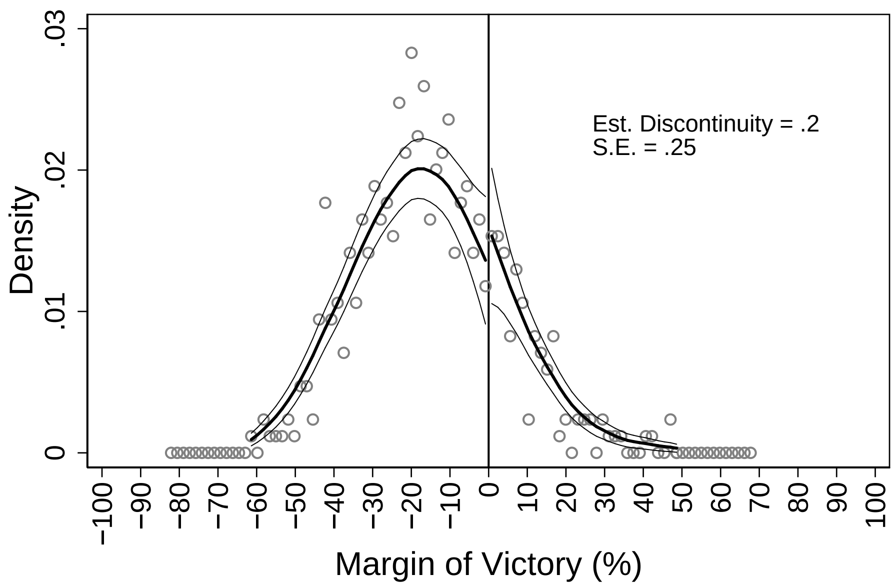
In general, there is little evidence suggesting discontinuities around the cutoff, and some confidence intervals are very wide. Some discontinuities, however, such as that of female employment and school enrollment may require additional inspection. Although very nosily estimated, the plots suggest a potential discontinuity for those variables.

Thus, Table 4.2 presents a more detailed analysis by estimating the RD effect of electing a criminal candidate for mayor on each predetermined characteristic following several methods. First, I estimate the RD effect by ordinary least squares using an optimal bandwidth for each dependent variable. Column 1 shows the estimated bandwidth size and corresponding estimation sample, while column 2 reports the estimated RD effects with standard errors in parenthesis. No result yields a statistical significant coefficient. Second, columns 3 through 8 present estimates for several arbitrary bandwidth choices. Column 3 and 4 use bandwidths that correspond to the optimal choices for the main dependent variables in this paper: 8.1 for log municipal expenditure and 9.5 for growth of nightlights. Again, no estimates are statically significant. Finally, columns 5 through 7 report estimates for band-

[9] The graphs only display observations for margins of victory within 20 percentage points, which accounts for most of the density when a criminal mayor is elected as indicated by 4.1(a).

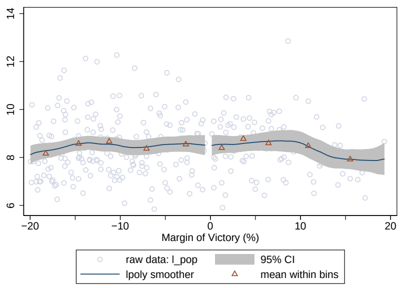


(a) Global Histogram

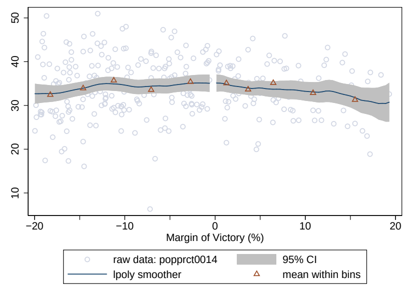


(b) McCrary (2008) Density Test

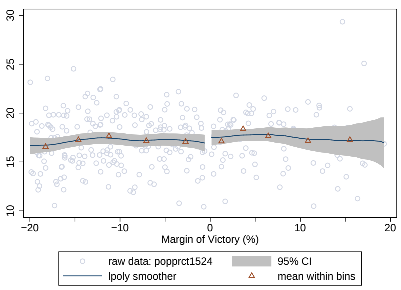
FIGURE 4.1: Continuity of Forcing Variable



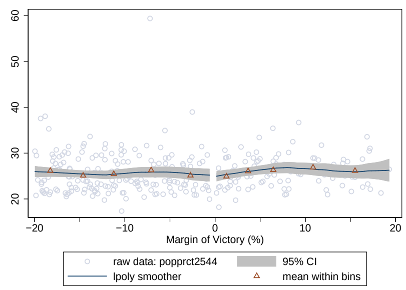
(a) Log Population



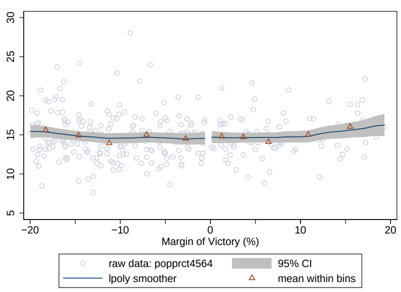
(b) Age 14 or less (%)



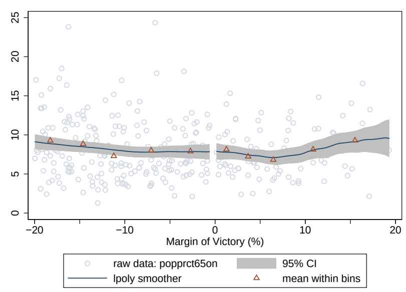
(c) Ages 15-24 (%)



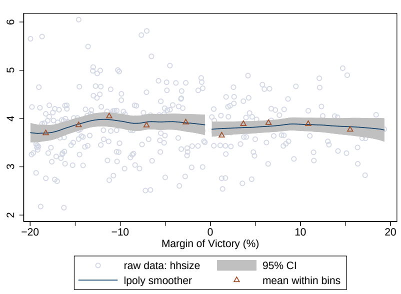
(d) Ages 25-44 (%)



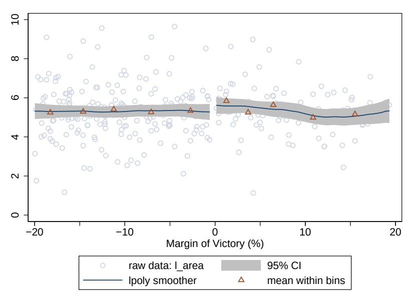
(e) Ages 45-64 (%)



(f) Age 65 or more (%)

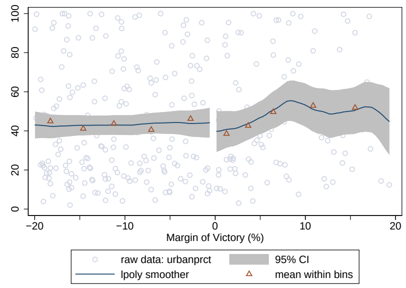


(g) Household size

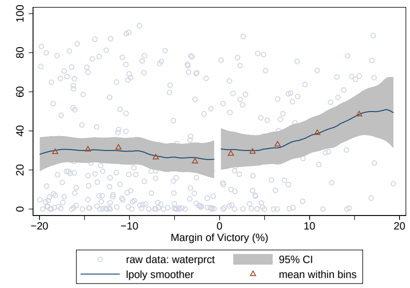


(h) Log area (in km^2)

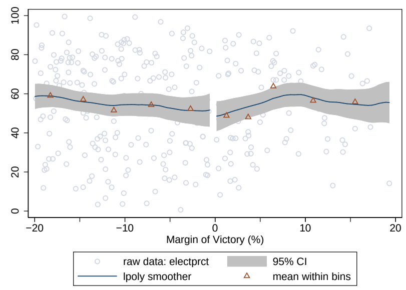
FIGURE 4.2: Covariate Smoothness – Demographic and Geographic Covariates



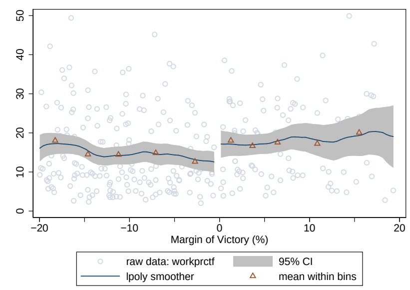
(a) Urban (%)



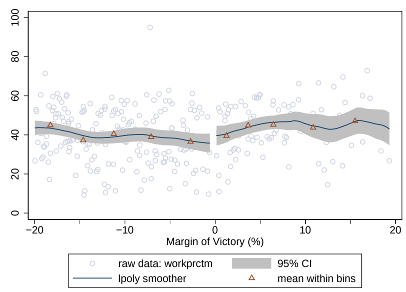
(b) With water (%)



(c) With electricity (%)

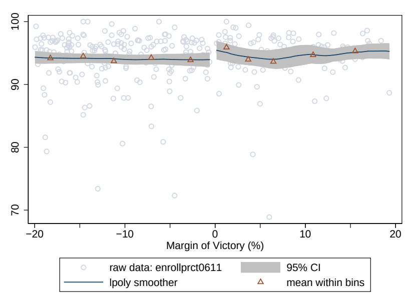


(d) Female employment (%)

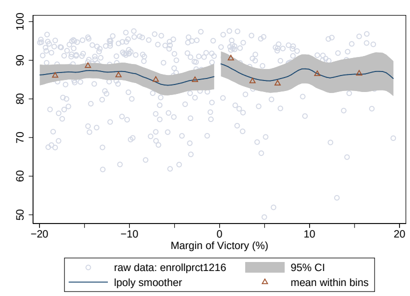


(e) Male employment (%)

(f) School: ages 3-5 (%)



(g) School: ages 6-11 (%)



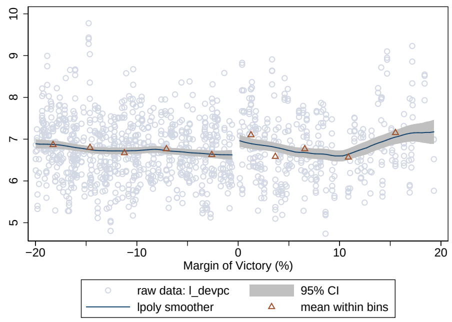
(h) School: age 12-16 (%)

Figure 4.3: Covariate Smoothness – Economic Covariates

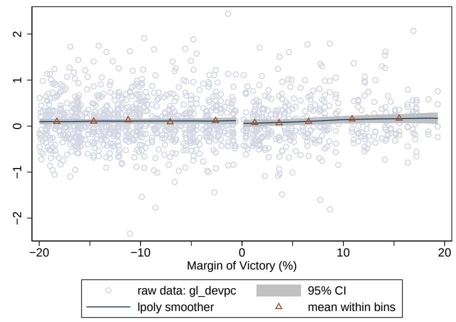
Table 4.2: Covariate Smoothness: RD Effect of Criminal Mayor on Predetermined Characteristics

	Optimal Bandwidth ($\hat{h}$)		Arbitrary Bandwidth					
	(1) $\hat{h}$ N	(2) Est. (S.E.)	(3) Est. (S.E.)	(4) Est. (S.E.)	(5) Est. (S.E.)	(6) Est. (S.E.)	(7) Est. (S.E.)	(8) Est. (S.E.)
Depedent Variable:								
Log population	8.7	-0.167	-0.214	-0.242	0.102	-0.323	0.100	0.055
	136	(0.404)	(0.404)	(0.381)	(0.558)	(0.376)	(0.274)	(0.403)
Population aged 14 or less (%)	11.4	0.124	-1.494	-0.842	2.086	0.008	-0.446	0.336
	169	(1.643)	(2.082)	(1.760)	(2.723)	(1.769)	(1.367)	(1.934)
Population aged 15-24 (%)	9.9	0.376	0.597	0.607	1.323	0.376	0.422	0.865
	148	(0.829)	(0.918)	(0.860)	(1.241)	(0.829)	(0.601)	(0.893)
Population aged 25-44 (%)	9.2	-0.051	0.498	-0.318	-1.665	-0.611	0.171	-1.038
	138	(1.294)	(1.598)	(1.248)	(1.661)	(1.187)	(0.923)	(1.273)
Population aged 45-64 (%)	11.0	0.176	0.311	0.447	-1.066	0.219	0.006	0.087
	163	(0.717)	(0.829)	(0.810)	(1.105)	(0.765)	(0.589)	(0.782)
Population aged 65 or more (%)	9.2	0.031	0.088	0.105	-0.678	0.008	-0.154	-0.250
	138	(1.056)	(1.133)	(1.022)	(1.393)	(1.007)	(0.771)	(1.080)
Household size	14.2	-0.092	-0.141	-0.204	0.101	-0.186	-0.190	-0.143
	212	(0.157)	(0.222)	(0.192)	(0.277)	(0.186)	(0.139)	(0.201)
Log area (in km^2)	11.3	0.355	0.552	0.322	0.822	0.301	0.325	0.558
	168	(0.354)	(0.422)	(0.398)	(0.615)	(0.388)	(0.288)	(0.399)
HHs in uran areas (%)	10.7	-7.267	-10.961	-11.794	-5.674	-11.168	-1.897	-12.081
	159	(9.501)	(10.910)	(10.178)	(14.524)	(9.820)	(6.898)	(10.451)
HHs with water (%)	11.4	7.695	4.247	4.751	11.951	3.824	1.236	8.647
	168	(8.694)	(10.044)	(9.551)	(12.678)	(9.307)	(6.700)	(9.960)
HHs with electricity (%)	10.5	-2.990	-6.777	-3.068	-1.335	-4.479	1.467	-6.661
	158	(8.153)	(9.207)	(8.415)	(11.844)	(8.374)	(5.886)	(8.897)
Female Employment (%)	15.0	3.557	4.893	3.984	4.976	3.652	4.390**	3.947
	223	(2.453)	(3.215)	(2.888)	(3.970)	(2.808)	(2.199)	(3.255)
Male Employment (%)	10.3	4.372	5.402	3.378	0.494	3.947	6.460**	2.184
	153	(4.300)	(5.254)	(4.585)	(6.632)	(4.413)	(3.173)	(4.905)
School Enrollment: age group 3-5 (%)	12.5	-1.230	-3.684	-3.754	-3.015	-2.791	-1.129	-3.625
	187	(5.001)	(6.116)	(5.625)	(8.631)	(5.578)	(4.169)	(6.047)
School Enrollment: age group 6-11 (%)	8.2	1.139	1.483	1.388	1.655	1.240	0.869	1.458
	126	(1.076)	(1.098)	(1.023)	(1.755)	(1.025)	(0.949)	(1.158)
School Enrollment: age group 12-16 (%)	8.4	1.623	2.311	3.618	5.426	3.185	3.077	4.487
	131	(2.819)	(2.897)	(2.696)	(3.988)	(2.676)	(2.170)	(2.857)
Arbitrary Bandwidth			8.1	9.5	5.0	10.0	20.0	20.0
Polynomial Order			1	1	1	1	1	2
Municipalities (N)			123	143	79	148	292	292

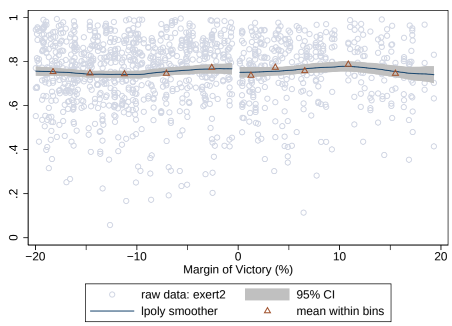
widths of 5, 10, and 20 percentage points, while column 8 replicates the analysis in 7 but allowing for a second order polynomial on the forcing variable at each side of the cutoff. The specification in column 8 recognizes that the bandwidth is already large and a non-linear specification could present a better fit. Overall, most effects are noisy across specifications and sometimes even flip signs. Only the coefficients for employment are significant in



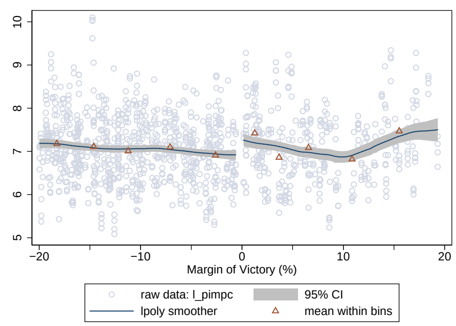
(a) Log expenditure per capita



(b) Expenditure annual log-growth



(c) Excecution rates



(d) Log budget per capita

FIGURE 4.4: RD Effect of Criminal Mayor on Municipal Expenditure

column 7 but not in other specifications. I take this as suggestive evidence of no systematic discontinuities around the cutoff on baseline characteristics.

4.5 Results

Figure 4.4 shows graphical evidence of the RDD effect on municipal expenditure and budgets. Each subfigure shows raw data on an outcome variable for each municipality-year in the period 2011-2014 plotted against the margin of victory of the criminal candidate. As before, at each side of the cutoff, the figures also show smoothed linear regressions with 95% confidence intervals and averages of the raw data within quintiles.

The graphical evidence suggests a positive RD effect of electing a criminal mayor on public expenditure, and that this increase is the result of a higher municipal budget. Figure 4.4(a) suggests a sizable and positive discontinuity in the log of per capita expenditure. This is, municipalities with a criminal candidate expend more public funds than their non-criminal counterparts. This increase, however, is not related to higher log growth rates of per capita municipal expenditure (figure 4.4(b)), or higher execution rates defined as actual expenditure over budgeted expenditures (figure 4.4(c)). Instead, the increase in per capita expenditure seems to be related to an increase in the municipal budget (figure 4.4(d)).

Table 4.3 shows regression results for the estimation of the RD point estimates. Each panel in table 4.3 shows estimated RD effects for each of the expenditure outcomes defined above using two methods: (i) bias-corrected point estimates and robust standard errors following Calonico et al. (2014) in columns 1 and 2, and (ii) OLS as in Imbens and Lemieux (2008) in columns 3 through 7. All columns but 1 and 3 include municipality-level controls for log population, percent urban, male and female employment, percentage of households with water supply, log household size, education controls (percentage of population enrolled by age brackets), and 2010 turnout. Mayor level controls include age, age squared, dummies for political party type (national level and regional level), and a dummy variable indicating previous experience as a mayor. Moreover, columns 5 through 7 explore the robustness of the results by re-estimating the model in column 4 (OLS with controls) with half the bandwidth (column 5), twice the bandwidth (column 6), and twice the bandwidth with a polynomial of order two at each side of the cutoff (column 7). Every point estimate corresponds to a separate regression, where

Table 4.3: RD Effect of Criminal Mayor on Municipal Expenditure

'	RDD Robust		RDD by OLS				
Specification	No Controls (1)	+ Controls (2)	No Controls (3)	+ Controls (4)	h/2 (5)	h × 2 (6)	h × 2 & p=2 (7)
	Dependent variable: log expenditure per capita						
Criminal Mayor	0.6517**	0.7107***	0.5173**	0.5358***	0.8399***	0.2408	0.5110**
	(0.2860)	(0.1904)	(0.2274)	(0.1875)	(0.2996)	(0.1496)	(0.2042)
	[0.0227]	[0.0002]	[0.0246]	[0.0050]	[0.0067]	[0.1089]	[0.0130]
Bandwidth	8.1	8.1	8.1	8.1	4.0	16.2	16.2
Municipalities	123	123	123	123	64	237	237
Observations	492	492	492	492	256	948	948
	Dependent variable: expenditure annual log-growth						
Criminal Mayor	-0.0894*	-0.1010**	-0.0766*	-0.0735	-0.1002	-0.0206	-0.0600
	(0.0525)	(0.0499)	(0.0416)	(0.0451)	(0.0668)	(0.0314)	(0.0465)
	[0.0886]	[0.0429]	[0.0669]	[0.1041]	[0.1363]	[0.5117]	[0.1979]
Bandwidth	14.9	14.9	14.9	14.9	7.5	29.8	29.8
Municipalities	223	223	223	223	115	409	409
Observations	892	892	892	892	460	1636	1636
	Dependent variable: execution rate						
Criminal Mayor	-0.0447	-0.0325	-0.0379	-0.0211	-0.0140	0.0136	-0.0123
	(0.0310)	(0.0291)	(0.0268)	(0.0287)	(0.0395)	(0.0213)	(0.0283)
	[0.1488]	[0.2650]	[0.1582]	[0.4629]	[0.7236]	[0.5239]	[0.6638]
Bandwidth	12.6	12.6	12.6	12.6	6.3	25.1	25.1
Municipalities	188	188	188	188	97	360	360
Observations	752	752	752	752	388	1440	1440
	Dependent variable: log budget per capita						
Criminal Mayor	0.6723**	0.7208***	0.5087**	0.5304***	0.9796***	0.2179	0.5348***
	(0.2973)	(0.2058)	(0.2364)	(0.2015)	(0.3057)	(0.1499)	(0.2059)
	[0.0237]	[0.0005]	[0.0333]	[0.0095]	[0.0021]	[0.1472]	[0.0100]
Bandwidth	8.3	8.3	8.3	8.3	4.2	16.6	16.6
Municipalities	127	127	127	127	64	248	248
Observations	508	508	508	508	256	992	992

municipality-year observations for the period 2011-2014 are pooled together. Clustered standard errors at the municipality-level are shown in parenthesis, and corresponding p-values are in squared brackets.

The point estimates of the RD effect are consistent with the graphical evidence. Both the robust and OLS estimation show an increase in log per capita expenditure of around 65 to 70 percent (columns 1 and 3), and the point estimated is not changed by the inclusion of controls (columns 2 and

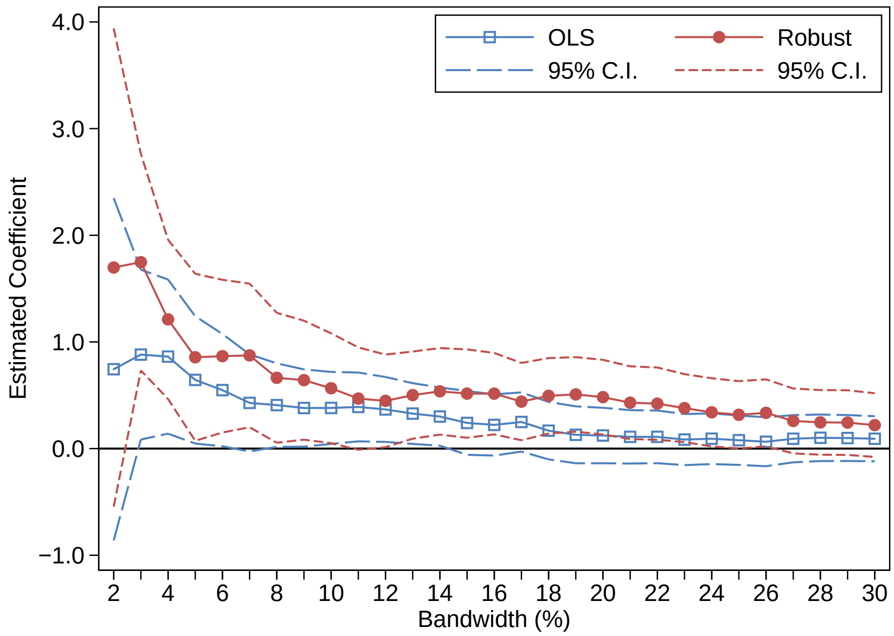


FIGURE 4.5: RDD Effect by Arbitrary Bandwidths on Log Expenditure per Capita

4). If anything, the controls increase the precision of the estimate. Moreover, arbitrary changes in the optimal bandwidth and order of the control polynomial do not significantly change the results. The only not statistically significant coefficient is that of column 6. This can be explained by the U-shaped pattern to the right of the cutoff, as suggested by figure 4.4(a) and the result from column 7. Additional evidence on the robustness of the effect is shown in figure 4.5, where point estimates are shown for arbitrary bandwidths. The effect on annual log-growth of per capita expenditure is, if anything, negative: municipalities that elect a criminal politician observe annual growth in expenditure of on average 7 to 10 percentage points lower than municipalities with a noncriminal mayor. The effect, however, is not robust across different bandwidths and estimation methods. Similarly, the increase in expenditure is not associated with higher execution rates. There is,

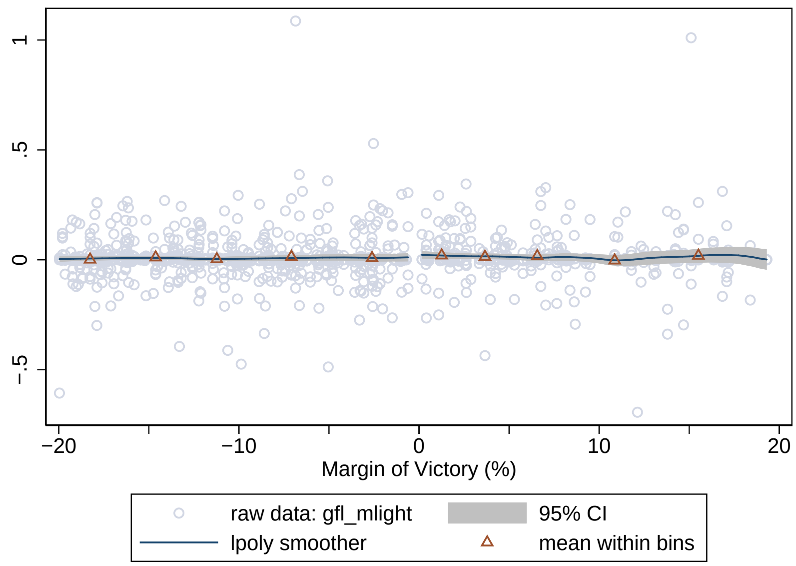
Table 4.4: RD Effect of Criminal Mayor on Night Lights

Specification	RDD Robust No Controls (1)	RDD Robust + Controls (2)	RDD by OLS No Controls (3)	RDD by OLS + Controls (4)	RDD by OLS h/2 (5)	RDD by OLS h × 2 (6)	RDD by OLS h × 2 & p=2 (7)
	Dependent variable: Annual log-growth of average lights						
Criminal Mayor	0.0073	-0.0007	0.0106	0.0074	-0.0171	0.0019	0.0100
	(0.0131)	(0.0122)	(0.0111)	(0.0121)	(0.0150)	(0.0094)	(0.0125)
	[0.5765]	[0.9558]	[0.3442]	[0.5391]	[0.2579]	[0.8362]	[0.4254]
Bandwidth	9.5	9.5	9.5	9.5	4.7	19.0	19.0
Municipalities	142	142	142	142	72	280	280
Observations	568	568	568	568	288	1120	1120
	Dependent variable: Log average lights						
Criminal Mayor	0.0202	0.2042	-0.0303	0.1386	0.1217	0.0350	0.2538**
	(0.2126)	(0.1292)	(0.2052)	(0.1134)	(0.1672)	(0.0822)	(0.1193)
	[0.9244]	[0.1142]	[0.8827]	[0.2234]	[0.4686]	[0.6710]	[0.0342]
Bandwidth	10.3	10.3	10.3	10.3	5.2	20.6	20.6
Municipalities	154	154	154	154	85	308	308
Observations	616	616	616	616	340	1232	1232

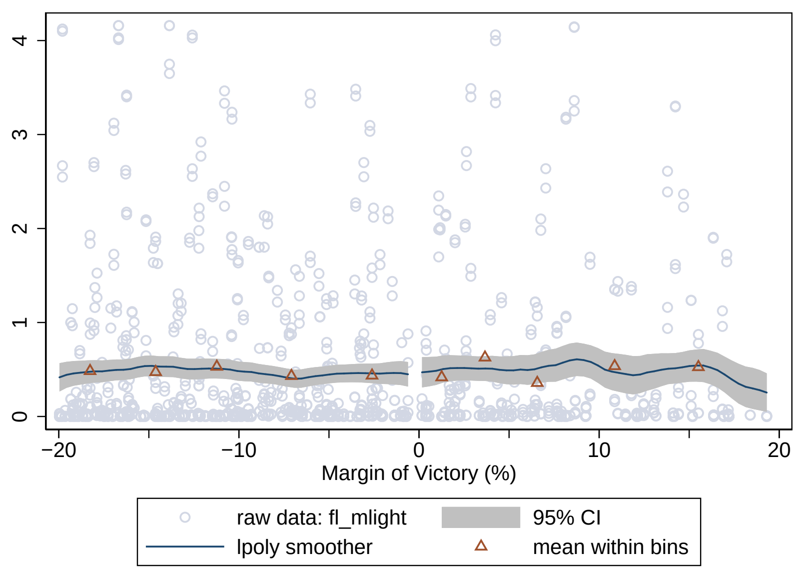
however, and almost one-to-one increase in the size of the municipal budget. The effects in expenditure and budget size are similar in size and magnitude and are robust across specifications.

Figure 4.6 and table 4.4 show results of a similar analysis on economic activity proxied by night-lights. I find no effect of electing a criminal politician on annual log-growth of lights or the log-level of light intensity. The null result is consist across various specifications.

Figure 4.7 studies the parallel trends assumption for municipal expenditures and budgets. Each figure plots RD point estimates against calendar years. Each figure corresponds to a linear regression where the indicator variable for criminal mayor is interacted with year dummies for the period 2007-2014. By allowing the intercept to vary by year, each point estimate corresponds to the effect of electing a criminal politician with respect to a non-criminal politician for each year in the sample. To ease comparisons

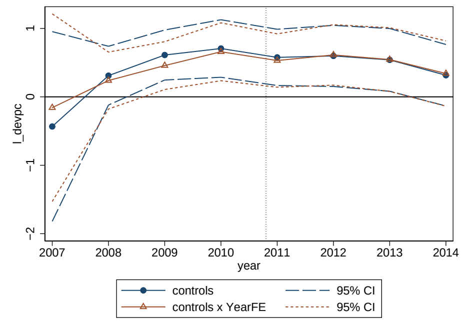


(a) Annual log-growth of average lights

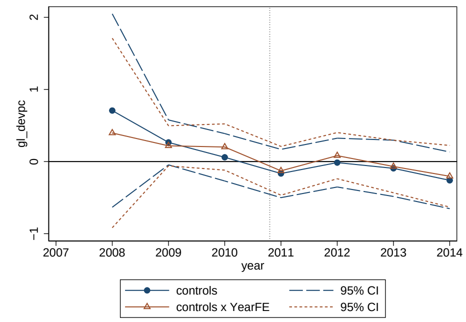


(b) Log average lights

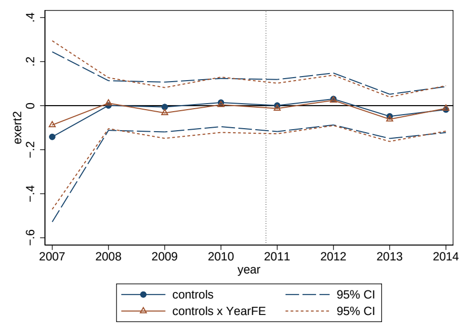
FIGURE 4.6: RD Effect of Criminal Mayor on Night Lights



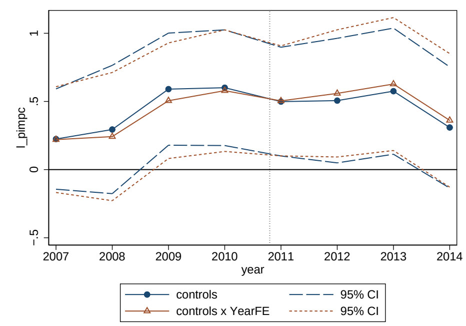
(a) Log expenditure per capita



(b) Expenditure annual log-growth



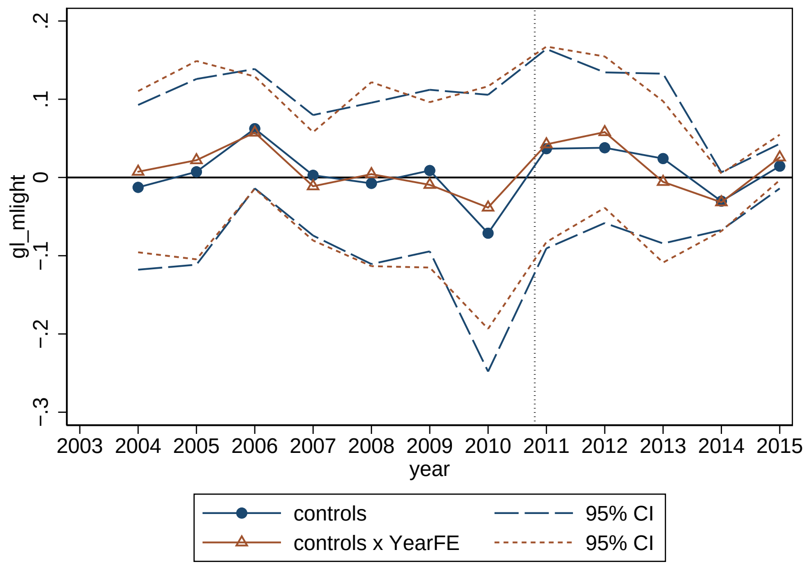
(c) Excecution rates



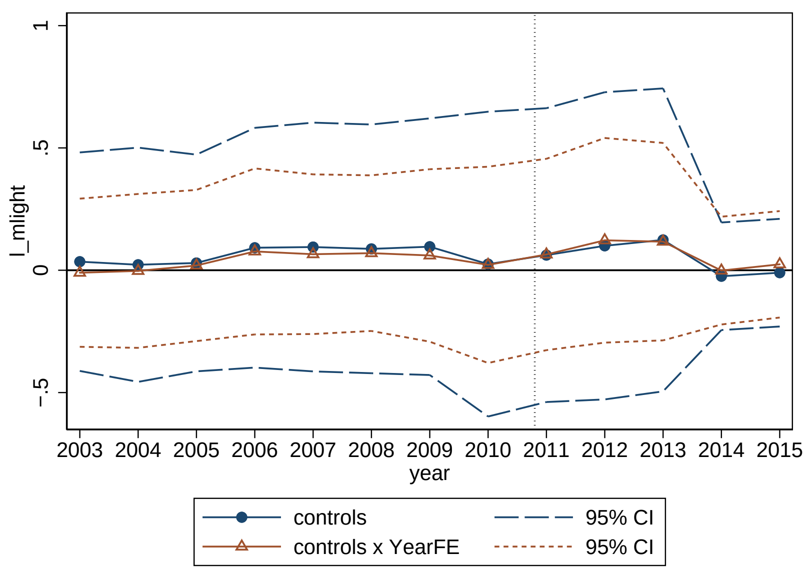
(d) Log budget per capita

FIGURE 4.7: RDD Effect of Criminal Mayor on Municipal Expenditure by Year, Constant Bandwidth

across graphs, the estimation is done in a constant sample defined by the optimal bandwidth calculated in table 4.3 for log expenditure per capita (8.1 percentage points). Two models are estimated in each graph, one with controls with a constant coefficient across years, and one with controls interacted with year dummies. Only pre-determined characteristics are included in each model as controls. Thus, relative to the model in table 4.3, I drop all mayor-level controls and the turnout variable. All other variables were measured in the 2007 census, and I take them as given.



(a) Annual log-growth of average lights



(b) Log Average lights

Figure 4.8: RDD Effect on Night Lights by Year, Constant Bandwidth

Table 4.5: RDD Effect of Criminal Mayor on Log Municipal Expenditure Per Capita by Year, Constant Bandwidth

	RDD by OLS						
Specification	No Controls (1)	+ Controls (2)	Controls × Yr (3)	h/2 (4)	h × 2 (5)	h × 2 & p=2 (6)	global & p=3 (7)
Criminal Mayor x 2007	-0.4238	-0.4332	-0.1579	-1.0679	-0.2371	-0.9174	-0.6726
	(0.7028)	(0.7006)	(0.6927)	(1.0802)	(0.4923)	(0.7779)	(0.5704)
	[0.5477]	[0.5375]	[0.8200]	[0.3266]	[0.6305]	[0.2394]	[0.2389]
Criminal Mayor x 2008	0.3194	0.3100	0.2386	0.1724	0.1099	0.1436	0.0970
	(0.2737)	(0.2169)	(0.2101)	(0.3123)	(0.1450)	(0.2342)	(0.1821)
	[0.2455]	[0.1556]	[0.2583]	[0.5828]	[0.4493]	[0.5405]	[0.5944]
Criminal Mayor x 2009	0.6209**	0.6115***	0.4570**	0.6819**	0.1838	0.4566**	0.2212
	(0.2443)	(0.1846)	(0.1767)	(0.2791)	(0.1317)	(0.2037)	(0.1559)
	[0.0123]	[0.0012]	[0.0109]	[0.0174]	[0.1643]	[0.0260]	[0.1564]
Criminal Mayor x 2010	0.7161***	0.7066***	0.6590***	0.5952	0.2974*	0.5750**	0.3238*
	(0.2543)	(0.2123)	(0.2137)	(0.3693)	(0.1596)	(0.2549)	(0.1929)
	[0.0057]	[0.0012]	[0.0025]	[0.1121]	[0.0636]	[0.0250]	[0.0938]
Criminal Mayor x 2011	0.5863**	0.5769***	0.5308***	0.9481***	0.2972**	0.4235*	0.2443
	(0.2669)	(0.2077)	(0.1970)	(0.3222)	(0.1500)	(0.2280)	(0.1858)
	[0.0299]	[0.0063]	[0.0081]	[0.0045]	[0.0487]	[0.0645]	[0.1891]
Criminal Mayor x 2012	0.6078**	0.5984***	0.6130***	0.9761**	0.4012**	0.7028**	0.3594*
	(0.2625)	(0.2255)	(0.2234)	(0.3940)	(0.1718)	(0.2707)	(0.2066)
	[0.0222]	[0.0090]	[0.0070]	[0.0159]	[0.0204]	[0.0100]	[0.0824]
Criminal Mayor x 2013	0.5498**	0.5404**	0.5450**	0.8621**	0.3354*	0.6917***	0.3412*
	(0.2617)	(0.2316)	(0.2351)	(0.3998)	(0.1738)	(0.2514)	(0.2032)
	[0.0377]	[0.0213]	[0.0221]	[0.0349]	[0.0548]	[0.0064]	[0.0938]
Criminal Mayor x 2014	0.3255	0.3160	0.3411	0.5287	0.0694	0.4175*	0.0750
	(0.2483)	(0.2270)	(0.2411)	(0.3951)	(0.1659)	(0.2452)	(0.1841)
	[0.1925]	[0.1663]	[0.1596]	[0.1856]	[0.6760]	[0.0899]	[0.6839]
Bandwidth	8.1	8.1	8.1	4.0	16.2	16.2	100.0
Municipalities	123	123	123	64	237	237	532
Observations	984	984	984	512	1896	1896	4256

The graphical evidence suggests that criminal politicians are systematically elected in municipalities where there was increased expenditure and higher budget before the election, which is a violation of the RDD assumptions. In particular, figure 4.7(a) and 4.7(d) show statistically significant placebo effects since year 2009. The first year in the term of the criminal mayors in office is 2011. Expenditure annual log-growth and execution rates do not seem to exhibit this pattern (figures 4.7(b) and 4.7(c), respectively). Tables 4.5 to 4.8 report the point estimates for these models. However, the parallel trends assumption is not violated for the measures of night-lights, as shown by figure 4.8.

Thus, I re-estimate the main effects on expenditure outcomes controlling

Table 4.6: RDD Effect of Criminal Mayor on Execution Rates by Year, Constant Bandwidth

Specification	RDD by OLS						
	No Controls	+ Controls	Controls × Yr	h/2	h × 2	h × 2 & p=2	global & p=3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Criminal Mayor x 2007	-0.1581	-0.1417	-0.0879	-0.3700	-0.1226	-0.2398	-0.2275
	(0.1962)	(0.1951)	(0.1933)	(0.2938)	(0.1327)	(0.2107)	(0.1503)
	[0.4219]	[0.4690]	[0.6499]	[0.2125]	[0.3564]	[0.2561]	[0.1307]
Criminal Mayor x 2008	-0.0157	0.0007	0.0112	-0.1211	0.0343	0.0142	0.0257
	(0.0560)	(0.0569)	(0.0584)	(0.0952)	(0.0398)	(0.0619)	(0.0468)
	[0.7794]	[0.9903]	[0.8479]	[0.2080]	[0.3895]	[0.8182]	[0.5834]
Criminal Mayor x 2009	-0.0225	-0.0061	-0.0331	-0.0963	-0.0359	-0.0094	-0.0515
	(0.0552)	(0.0572)	(0.0583)	(0.1000)	(0.0397)	(0.0576)	(0.0443)
	[0.6837]	[0.9146]	[0.5710]	[0.3391]	[0.3668]	[0.8702]	[0.2460]
Criminal Mayor x 2010	-0.0024	0.0140	0.0040	-0.0816	-0.0328	-0.0182	-0.0260
	(0.0544)	(0.0553)	(0.0635)	(0.0745)	(0.0353)	(0.0571)	(0.0395)
	[0.9649]	[0.8006]	[0.9502]	[0.2778]	[0.3548]	[0.7504]	[0.5104]
Criminal Mayor x 2011	-0.0158	0.0006	-0.0128	-0.1040	-0.0232	-0.0575	-0.0508
	(0.0558)	(0.0597)	(0.0580)	(0.0853)	(0.0379)	(0.0618)	(0.0434)
	[0.7775]	[0.9921]	[0.8263]	[0.2272]	[0.5411]	[0.3532]	[0.2425]
Criminal Mayor x 2012	0.0138	0.0302	0.0241	0.0474	0.0270	-0.0083	-0.0117
	(0.0590)	(0.0595)	(0.0578)	(0.1043)	(0.0391)	(0.0603)	(0.0433)
	[0.8161]	[0.6133]	[0.6777]	[0.6509]	[0.4896]	[0.8909]	[0.7870]
Criminal Mayor x 2013	-0.0649	-0.0485	-0.0617	-0.0860	0.0208	-0.0509	0.0037
	(0.0514)	(0.0508)	(0.0510)	(0.0818)	(0.0395)	(0.0554)	(0.0456)
	[0.2091]	[0.3413]	[0.2286]	[0.2970]	[0.5993]	[0.3590]	[0.9353]
Criminal Mayor x 2014	-0.0343	-0.0179	-0.0125	-0.1427	-0.0184	-0.0412	-0.0103
	(0.0488)	(0.0527)	(0.0521)	(0.0953)	(0.0384)	(0.0571)	(0.0429)
	[0.4833]	[0.7348]	[0.8101]	[0.1393]	[0.6310]	[0.4719]	[0.8107]
Bandwidth	8.1	8.1	8.1	4.0	16.2	16.2	100.0
Municipalities	123	123	123	64	237	237	532
Observations	984	984	984	512	1896	1896	4256

for pre-existing levels of expenditure and budget sizes in several ways in tables 4.9 to 4.12. Each table corresponds to the analysis of one of the main four expenditure outcomes and is divided in five vertical panels. The first panel (columns 1 to 3) shows baseline results—this is, without controlling for any lagged outcomes—and serve for comparison purposes. The second panel (columns 4 to 6) controls for average expenditure in the period 2007-2010, while the third panel (columns 7 to 9) controls for average budget size in the same period. Finally, the fourth (columns 10 to 12) and fifth (columns 13 to 15) panels control for expenditure and budget size in 2010, respectively. All controls are in logs of per capita terms. Each panel has three columns. The

Table 4.7: RDD Effect of Criminal Mayor on Log-Growth of Municipal Expenditure Per Capita by Year, Constant Bandwidth

	RDD by OLS						
Specification	No Controls	+ Controls	Controls × Yr	h/2	h × 2	h × 2 & p=2	global & p=3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Criminal Mayor x 2008	0.7432	0.7074	0.3965	1.2403	0.3470	1.0610	0.7696
	(0.6924)	(0.6773)	(0.6637)	(1.0387)	(0.4820)	(0.7542)	(0.5548)
	[0.2853]	[0.2983]	[0.5513]	[0.2369]	[0.4723]	[0.1608]	[0.1660]
Criminal Mayor x 2009	0.3015**	0.2657*	0.2185	0.5095**	0.0739	0.3130*	0.1242
	(0.1423)	(0.1572)	(0.1404)	(0.2314)	(0.1070)	(0.1593)	(0.1312)
	[0.0362]	[0.0935]	[0.1223]	[0.0313]	[0.4902]	[0.0506]	[0.3443]
Criminal Mayor x 2010	0.0952	0.0594	0.2019	-0.0867	0.1136	0.1184	0.1025
	(0.1591)	(0.1660)	(0.1620)	(0.3081)	(0.1155)	(0.1813)	(0.1380)
	[0.5509]	[0.7209]	[0.2148]	[0.7793]	[0.3261]	[0.5143]	[0.4577]
Criminal Mayor x 2011	-0.1298	-0.1655	-0.1282	0.3528	-0.0002	-0.1515	-0.0795
	(0.1625)	(0.1690)	(0.1709)	(0.2811)	(0.1141)	(0.1742)	(0.1299)
	[0.4262]	[0.3294]	[0.4544]	[0.2140]	[0.9983]	[0.3853]	[0.5410]
Criminal Mayor x 2012	0.0215	-0.0142	0.0822	0.0280	0.1040	0.2793	0.1151
	(0.1641)	(0.1709)	(0.1618)	(0.2717)	(0.1155)	(0.1833)	(0.1299)
	[0.8960]	[0.9338]	[0.6123]	[0.9182]	[0.3685]	[0.1289]	[0.3758]
Criminal Mayor x 2013	-0.0580	-0.0937	-0.0680	-0.1139	-0.0658	-0.0111	-0.0183
	(0.1773)	(0.1971)	(0.1841)	(0.3238)	(0.1201)	(0.1990)	(0.1503)
	[0.7441]	[0.6352]	[0.7125]	[0.7261]	[0.5840]	[0.9555]	[0.9034]
Criminal Mayor x 2014	-0.2243	-0.2601	-0.2039	-0.3335	-0.2659*	-0.2742	-0.2662*
	(0.2003)	(0.1984)	(0.2159)	(0.4061)	(0.1406)	(0.2117)	(0.1591)
	[0.2648]	[0.1923]	[0.3468]	[0.4147]	[0.0598]	[0.1965]	[0.0949]
Bandwidth	8.1	8.1	8.1	4.0	16.2	16.2	100.0
Municipalities	123	123	123	64	237	237	532
Observations	861	861	861	448	1659	1659	3724

first column pools municipality-year observations for the period 2011-2014. The second column replicates the estimation for the period 2012-2014, as the first year of the electoral term may be different because of inertia from the previous term. Finally, the third column conducts the analysis flexibly by calendar year. Controls are as in the main specification. To ease the analysis, the bandwidth is kept constant across specifications.

The statistical significance and magnitude of the findings depend on the lagged outcomes used in the regression. The effects of electing a criminal politician on log expenditure per capita are moderated when controlling for the average expenditure or average budget in 2007-2010; however, the effects are reduced and are not longer statistically significant when controlling for the 2010 expenditure or budget (Table 4.9). As before, there are no effects

Table 4.8: RDD Effect of Criminal Mayor on Log Budget Per Capita by Year, Constant Bandwidth

	RDD by OLS						
Specification	No Controls (1)	+ Controls (2)	Controls × Yr (3)	h/2 (4)	h × 2 (5)	h × 2 & p=2 (6)	global & p=3 (7)
Criminal Mayor x 2007	0.2896	0.2241	0.2201	0.4458	-0.0089	0.1626	-0.0221
	(0.2539)	(0.1859)	(0.1960)	(0.2834)	(0.1399)	(0.2100)	(0.1642)
	[0.2562]	[0.2303]	[0.2636]	[0.1207]	[0.9494]	[0.4397]	[0.8929]
Criminal Mayor x 2008	0.3598	0.2942	0.2421	0.3684	0.0655	0.1309	0.0594
	(0.2983)	(0.2377)	(0.2375)	(0.3628)	(0.1662)	(0.2625)	(0.1982)
	[0.2300]	[0.2182]	[0.3101]	[0.3137]	[0.6941]	[0.6184]	[0.7645]
Criminal Mayor x 2009	0.6559**	0.5903***	0.5053**	0.8142**	0.2333	0.4650*	0.2921
	(0.2663)	(0.2077)	(0.2141)	(0.3452)	(0.1536)	(0.2369)	(0.1788)
	[0.0152]	[0.0053]	[0.0199]	[0.0215]	[0.1302]	[0.0508]	[0.1029]
Criminal Mayor x 2010	0.6663**	0.6007***	0.5790**	0.6832*	0.3379**	0.5383**	0.3364*
	(0.2710)	(0.2143)	(0.2252)	(0.3718)	(0.1660)	(0.2593)	(0.1967)
	[0.0154]	[0.0059]	[0.0113]	[0.0708]	[0.0429]	[0.0390]	[0.0879]
Criminal Mayor x 2011	0.5644**	0.4988**	0.5041**	1.0752***	0.3263**	0.4800**	0.3020
	(0.2661)	(0.2015)	(0.2039)	(0.3076)	(0.1558)	(0.2316)	(0.1885)
	[0.0359]	[0.0147]	[0.0148]	[0.0009]	[0.0372]	[0.0393]	[0.1097]
Criminal Mayor x 2012	0.5717**	0.5061**	0.5591**	0.8412**	0.3508*	0.6960**	0.3603*
	(0.2823)	(0.2308)	(0.2356)	(0.4035)	(0.1797)	(0.2821)	(0.2157)
	[0.0450]	[0.0302]	[0.0192]	[0.0411]	[0.0521]	[0.0143]	[0.0955]
Criminal Mayor x 2013	0.6409**	0.5753**	0.6277**	1.0027**	0.2697	0.7448***	0.3026
	(0.2664)	(0.2336)	(0.2463)	(0.4286)	(0.1800)	(0.2598)	(0.2073)
	[0.0177]	[0.0152]	[0.0121]	[0.0225]	[0.1353]	[0.0045]	[0.1450]
Criminal Mayor x 2014	0.3745	0.3089	0.3612	0.7011*	0.1082	0.5013*	0.0910
	(0.2517)	(0.2244)	(0.2475)	(0.3909)	(0.1714)	(0.2547)	(0.1887)
	[0.1393]	[0.1712]	[0.1470]	[0.0777]	[0.5285]	[0.0503]	[0.6297]
Bandwidth	8.1	8.1	8.1	4.0	16.2	16.2	100.0
Municipalities	123	123	123	64	237	237	532
Observations	984	984	984	512	1896	1896	4256

on execution rates (Table 4.10), and the effects on expenditure growth are again, if anything, negative (Table 4.11). This is not compatible with a story of increased expenditure starting from a similar baseline. This is confirmed by the results in table 4.12, where the statistical significance and magnitude echo those from log per capita expenditure.

Table 4.9: RDD effect on log expenditure per capita, including lagged outcomes (optimal bandwidth)

	RDD by OLS														
Lagged variable included	None			Expenditure 2007-2010			Budget 2007-2010			Expenditure 2010			Budget 2010		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Criminal Mayor	0.5358***	0.5034**		0.3949**	0.3499*		0.2430*	0.1947		0.1029	0.0745		0.1504	0.1111	
	(0.1875)	(0.1990)		(0.1771)	(0.1855)		(0.1402)	(0.1466)		(0.1436)	(0.1570)		(0.1302)	(0.1415)	
	[0.0050]	[0.0127]		[0.0276]	[0.0617]		[0.0856]	[0.1865]		[0.4751]	[0.6362]		[0.2503]	[0.4341]	
Criminal Mayor x 2011			0.6329***			0.5299**			0.3878**			0.1882			0.2684
			(0.2233)			(0.2175)			(0.1955)			(0.1823)			(0.1863)
			[0.0054]			[0.0163]			[0.0496]			[0.3038]			[0.1522]
Criminal Mayor x 2012			0.6452***			0.4905**			0.3349*			0.1868			0.2311
			(0.2431)			(0.2315)			(0.1985)			(0.2026)			(0.1806)
			[0.0090]			[0.0362]			[0.0941]			[0.3583]			[0.2031]
Criminal Mayor x 2013			0.6336**			0.4709*			0.3156			0.1652			0.2337
			(0.2543)			(0.2427)			(0.2210)			(0.2265)			(0.2269)
			[0.0141]			[0.0547]			[0.1557]			[0.4670]			[0.3049]
Criminal Mayor x 2014			0.2314			0.0882			-0.0663			-0.1287			-0.1317
			(0.2784)			(0.2710)			(0.2382)			(0.2557)			(0.2397)
			[0.4074]			[0.7453]			[0.7812]			[0.6156]			[0.5839]
Years Included	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Equation × YearFE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Bandwidth	8.1	8.1	8.1	8.1	8.1	8.1	8.1	8.1	8.1	8.1	8.1	8.1	8.1	8.1	8.1
Municipalities	123	123	123	123	123	123	123	123	123	123	123	123	123	123	123
Observations	492	369	492	492	369	492	492	369	492	492	369	492	492	369	492

Table 4.10: RDD effect on execution rates, including lagged outcomes (optimal bandwidth)

	RDD by OLS														
Lagged variable included	None			Expenditure 2007-2010			Budget 2007-2010			Expenditure 2010			Budget 2010		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Criminal Mayor	-0.0211	-0.0132		-0.0204	-0.0143		-0.0104	-0.0054		-0.0155	-0.0094		-0.0042	-0.0006	
	(0.0287)	(0.0307)		(0.0290)	(0.0308)		(0.0286)	(0.0305)		(0.0288)	(0.0307)		(0.0285)	(0.0302)	
	[0.4629]	[0.6688]		[0.4813]	[0.6431]		[0.7165]	[0.8584]		[0.5915]	[0.7597]		[0.8840]	[0.9853]	
Criminal Mayor x 2011			-0.0449			-0.0388			-0.0253			-0.0337			-0.0150
			(0.0468)			(0.0467)			(0.0478)			(0.0481)			(0.0483)
			[0.3387]			[0.4065]			[0.5978]			[0.4841]			[0.7570]
Criminal Mayor x 2012			-0.0251			-0.0254			-0.0208			-0.0230			-0.0146
			(0.0477)			(0.0476)			(0.0476)			(0.0473)			(0.0474)
			[0.5995]			[0.5937]			[0.6626]			[0.6276]			[0.7577]
Criminal Mayor x 2013			-0.0270			-0.0295			-0.0181			-0.0230			-0.0132
			(0.0492)			(0.0492)			(0.0488)			(0.0491)			(0.0488)
			[0.5843]			[0.5498]			[0.7115]			[0.6394]			[0.7874]
Criminal Mayor x 2014			0.0125			0.0119			0.0225			0.0178			0.0261
			(0.0521)			(0.0531)			(0.0531)			(0.0546)			(0.0534)
			[0.8100]			[0.8222]			[0.6719]			[0.7447]			[0.6251]
Years Included	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Equation × YearFE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Bandwidth	12.6	12.6	12.6	12.6	12.6	12.6	12.6	12.6	12.6	12.6	12.6	12.6	12.6	12.6	12.6
Municipalities	188	188	188	188	188	188	188	188	188	188	188	188	188	188	188
Observations	752	564	752	752	564	752	752	564	752	752	564	752	752	564	752

Table 4.11: RDD effect on log growth of expenditure per capita, including lagged outcomes (optimal bandwidth)

	RDD by OLS														
Lagged variable included	None			Expenditure 2007-2010			Budget 2007-2010			Expenditure 2010			Budget 2010		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Criminal Mayor	-0.0735	-0.0870		-0.0631	-0.0883		-0.0539	-0.0848		-0.0365	-0.0754		-0.0436	-0.0783	
	(0.0451)	(0.0553)		(0.0434)	(0.0549)		(0.0435)	(0.0559)		(0.0401)	(0.0555)		(0.0423)	(0.0560)	
	[0.1041]	[0.1169]		[0.1474]	[0.1089]		[0.2162]	[0.1306]		[0.3642]	[0.1757]		[0.3048]	[0.1636]	
Criminal Mayor x 2011			-0.0331			0.0125			0.0387			0.0802			0.0607
			(0.1331)			(0.1230)			(0.1250)			(0.1239)			(0.1272)
			[0.8036]			[0.9193]			[0.7573]			[0.5182]			[0.6337]
Criminal Mayor x 2012			0.0613			0.0438			0.0476			0.0535			0.0439
			(0.1389)			(0.1359)			(0.1384)			(0.1404)			(0.1407)
			[0.6595]			[0.7473]			[0.7312]			[0.7038]			[0.7555]
Criminal Mayor x 2013			0.0013			-0.0061			-0.0010			-0.0017			0.0057
			(0.1413)			(0.1416)			(0.1420)			(0.1416)			(0.1408)
			[0.9926]			[0.9654]			[0.9942]			[0.9907]			[0.9676]
Criminal Mayor x 2014			-0.3235*			-0.3027*			-0.3008*			-0.2780			-0.2845
			(0.1727)			(0.1721)			(0.1742)			(0.1720)			(0.1743)
			[0.0624]			[0.0800]			[0.0855]			[0.1075]			[0.1040]
Years Included	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Equation × YearFE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Bandwidth	14.9	14.9	14.9	14.9	14.9	14.9	14.9	14.9	14.9	14.9	14.9	14.9	14.9	14.9	14.9
Municipalities	223	223	223	223	223	223	223	223	223	223	223	223	223	223	223
Observations	892	669	892	892	669	892	892	669	892	892	669	892	892	669	892

Table 4.12: RDD effect on log budget per capita, including lagged outcomes (optimal bandwidth)

	RDD by OLS														
Lagged variable included	None			Expenditure 2007-2010			Budget 2007-2010			Expenditure 2010			Budget 2010		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Criminal Mayor	0.5304***	0.5218**		0.3803**	0.3672*		0.1908	0.1782		0.0716	0.0675		0.0798	0.0778	
	(0.2015)	(0.2131)		(0.1892)	(0.1990)		(0.1457)	(0.1557)		(0.1513)	(0.1645)		(0.1327)	(0.1497)	
	[0.0095]	[0.0157]		[0.0466]	[0.0673]		[0.1925]	[0.2547]		[0.6369]	[0.6824]		[0.5485]	[0.6040]	
Criminal Mayor x 2011			0.5559**			0.4195*			0.2287			0.0839			0.0859
			(0.2246)			(0.2166)			(0.1813)			(0.1830)			(0.1551)
			[0.0146]			[0.0550]			[0.2093]			[0.6473]			[0.5808]
Criminal Mayor x 2012			0.6245**			0.4664*			0.2829			0.1502			0.1611
			(0.2488)			(0.2400)			(0.2024)			(0.2010)			(0.1828)
			[0.0133]			[0.0542]			[0.1646]			[0.4562]			[0.3799]
Criminal Mayor x 2013			0.7271***			0.5644**			0.3731*			0.2424			0.2778
			(0.2621)			(0.2461)			(0.2110)			(0.2208)			(0.2185)
			[0.0064]			[0.0235]			[0.0795]			[0.2743]			[0.2059]
Criminal Mayor x 2014			0.2139			0.0709			-0.1215			-0.1902			-0.2054
			(0.2761)			(0.2662)			(0.2297)			(0.2479)			(0.2304)
			[0.4399]			[0.7904]			[0.5978]			[0.4443]			[0.3743]
Years Included	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14	11-14	12-14	11-14
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Equation × YearFE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Bandwidth	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3
Municipalities	127	127	127	127	127	127	127	127	127	127	127	127	127	127	127
Observations	508	381	508	508	381	508	508	381	508	508	381	508	508	381	508

4.6 Concluding Remarks and Future Work

The findings of this study suggest a positive relationship between electing a criminal politician for mayor in Peru and increase municipal expenditure. However, these effects are driven by preexisting levels of expenditure and budget sizes and not by the electoral result. This results motivates studying how criminal politicians get elected. Previous evidence from Peruvian municipalities suggests that candidates use formal accountability mechanisms to oust elected officials and disqualify competitors during electoral races. The Peruvian setting is ideal to study this research question. First, there are data on the characteristics of every politician running for public office in Peru, including (verified) criminal records and political histories. These data also include information on electoral races, disqualification of candidates, and recall referendums. Most settings only have information on elected politicians. Moreover, these data can be matched to detailed government expending records and funding sources. Second, to gain plausibly exogenous variation in preexisting levels of municipal budgets, researchers may exploit redistribution rules that allocate resources from the central government to municipalities. The central government collects a considerable amount of taxes from extracting activities (e.g., mining) carried out in some municipalities. Importantly, the rules assign a share of these tax revenues to municipalities where the economic activity is not taking place.

5

Conclusions

This dissertation brings causal evidence on three topics in development economics, economic demography, and political economy.

In the first chapter, I study how an anti-drug policy that generated a sudden, negative price shock in the Peruvian coca industry affected the health of children in coca producing cites. I employ a difference-in-difference design to compare cohorts of children born in years of high prices with cohorts born in years of low prices and across areas with different levels of coca cultivation. The results show that the price cut caused by the policy increased mortality rates across children. I establish that deaths took place both in-utero and during the first years of life by using both direct mortality records and a "missing children" approach to infer mortality from census and survey data.

The evidence is consistent with two mechanisms. First, the price shock represented a negative income effect for households in producing sites, which reduced health expenditures considerably (and may have increased maternal stress, which is negatively related with in-utero survival). Second, to compensate for the income loss, adult women and individuals in school-age increased their labor supply, which could have crowded out time-intensive health investments. Several robustness checks indicate that the results are unlikely to be driven by factors other than the price shock.

This study builds on a literature on aggregate income shocks and child mortality by bringing new evidence using plausibly exogenous variation in the price of coca. This setting allows me to focus on the behavioral mechanisms that economic conditions trigger in the household. Previous studies using economic crises, fluctuations in economic cycles, or weather shocks as sources of variation study settings where "shocks" may affect health and mortality through additional causal pathways. For instance, economic crises may affect the supply of healthcare services; floods may impoverish the overall health environment or affect the food security of the household in agricultural settings. In addition, I use the abruptness of the price change to credibly isolate the effect from fertility and other confounding behavioral responses.

Overall, the evidence I show is different from quasi-experimental evidence from more developed settings where economic downturns are associated with improvements in child health (explained by an increase in time-intensive preventive health care and health investments that compensate for income losses).

This paper also contributes to a body on research on illegal markets and law enforcement efforts. It establishes that sharp economic downturns in drug industries may have negative effects on the wellbeing of vulnerable populations in drug producing sites. Previous research had documented how drug booms have adverse effects in various dimensions, such as violence, school dropouts, child labor, among others. This study supplements the narrative depicted by previous studies.

The second chapter brings causal evidence on how mass media affects behavior. I study how the introduction of commercial television to the United States in the 1950s affected households' fertility choices. To establish the main reduced-form effects, I exploit idiosyncratic variation in the timing and geographic availability of television's commercial broadcasting. I find that the availability of television reduced fertility in 1.3 births per thousand women in childbearing age.

Previous research in developing countries suggests that the negative association between fertility and exposure to modern television is explained by TV content, such as the portrayal of progressive gender roles. Alternative explanations include an increase in the value of leisure that translates into a higher opportunity cost of time allocated into childrearing and other activities. Historical accounts coincide in that the introduction of commercial television entailed a discrete change in the type of leisure activities available in the household: the new technology was adopted at record speeds, and, as early as 1950, viewing time had already reached four and half hours per day. Between 1948 and 1955, approximately two-thirds of the households in the United States installed a television set. Survey evidence at the time of the introduction suggests that television owners were spending more time at home than before, and that screen time crowded out other activities. Importantly, increases in the value of leisure and the projection of progressive roles have different predictions for other non-fertility margins, such as labor force participation. I propose testing this with recently released microdata from the 1950 and 1960 U.S. censuses.

Finally, in the third chapter, I study the impacts of electing criminal politicians to public office for local development in Peru. A regression-discontinuity design in a close-election framework suggests that criminal politicians expend more, particularly in infrastructure projects. A closer look, however, shows evidence of selection: criminal politicians are more likely to be elected in areas of with pre-existing high levels of expenditure. This chapter thus emphasizes the importance of accounting for political selection mechanisms when studying the implications of the characteristics of politicians elected to office.

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Biography

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