Three Essays in Population Economics

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I dedicate this work to my family. I would not have gone this far without them.

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I would like to thank my advisors and my committee members. They were patient, helpful and inspiring. I am very lucky to have worked with people so brilliant and so kind.

Abstract

This dissertation explores the economic drivers and consequences of population dynamics through three distinct analyses. Chapter One examines the effects of trade-shock-induced migration and return migration on local crime rates and labor market outcomes. Utilizing the 1990s Brazilian trade reform as an exogenous shock, we analyze Brazilian census data to investigate how migrants from regions affected by trade shocks influence labor markets, particularly when they lack social networks, family support, or prior knowledge of the local economy. Chapter Two assesses the impact of increased access to education on teenage fertility. We leverage the 2012 Mexican education reform, which made high school education mandatory and led to a significant expansion of access in regions with previous high school capacity constraints, we explore how this exogenous increase in educational opportunities affects teenage fertility rates. Chapter Three investigates the differential effects of job destruction and establishment closures on teenage and adult fertility. Using county-level U.S. data on job losses due to establishment closures, we examine fertility rates by age group to understand the relationship between labor market disruptions and fertility. Additionally, we analyze individual-level data on income and employment duration to delve into the mechanisms potentially driving teenage fertility in areas affected by job destruction. This work contributes to our understanding of how economic changes influence demographic behaviors, offering insights into the interplay between migration, education, labor markets, and fertility.

Keywords: *Demographics, Fertility, Migration, Labor Market Shocks, Education, Crime.*

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Chapter 1

Introduction

The intricate dynamics of population changes are an essential component of the economic landscape. Variations in fertility and migration rates not only drive the labor force's size and composition but also impact the demand for goods and services. Furthermore, the timing of fertility decisions plays a crucial role in shaping the economic futures of families. This dissertation delves into several aspects of the relationship between population dynamics and economics.

The first chapter explores the consequences of trade-shock-induced migration, investigating its dual impact on labor markets and crime rates. By analyzing the migration responses to the Brazilian trade reforms of the 1990s—a period marked by significant tariff reductions and heightened competition from Chinese imports—this study investigates migration's influence on crime rates, differentiating between the outcomes for returning migrants and migrants. This research leverages the trade liberalization episode as a natural experiment to elucidate the causal links through which trade shocks propagate across regions, even those not directly impacted by the shock. The main identification challenge in this work is that, although the trade shock is exogenous, migration choices are not, and particular regions may attract specific profiles of migrants. To account for potential issues of migrant self-selection, we employ a gravity equation model and construct instruments for migration. This allows us to assess the consequent economic effects of migration, highlighting the differential roles of migrant types in this context.

The second chapter pivots to the domain of education, examining its influence on teenage birth rates. To do so, we focus on the 2012 education reform in Mexico, which amended the constitution to make high school compulsory. This forced all levels of government to remove previously existing high school capacity constraints. We apply a difference-in-differences approach, exploiting the variation across time and municipality-level exposure to the reform. This allows us to capture the reform's impact on teenage fertility, offering insights into how the expansion of education opportunities can alter teenagers' life trajectories.

Venturing further into fertility topics, the final chapter analyzes the differential interplay between labor market dynamics and fertility rates among adults and teenagers. Focusing on job losses due to business establishment closures, it assesses how labor market shocks differentially affect fertility decisions across age groups. This research sheds light on the potential mediating role of parents and labor market experiences, enriching our understanding of the economic factors that specifically drive teenage fertility choices. Specifically, by using detailed county-level US data on job destruction and individual-level data on family members' income and weeks worked, we examine how parents' and teenagers' labor market experiences can specifically shape teenage fertility outcomes.

In wrapping up, the dissertation synthesizes its findings to articulate the implications of education and labor market shifts on population dynamics. It advocates for these dynamics to be accounted for in policy formulation and economic analysis, emphasizing the importance of acknowledging population effects when measuring the real costs of economic disturbances and the value of strategic policy interventions to mitigate their impacts.

Chapter 2

Migration, Return Migration and the Long-Run Effects of Economic Shocks on Crime

Abstract

In this study, we examine two key dimensions related to the impact of migration flows on crime rates. Firstly, we investigate whether migration from areas affected by economic shocks has a discernible effect on crime rates. Secondly, we explore whether this effect varies depending on whether individuals are returning to their birthplaces ("returning migrants") or moving to a new one ("migrants"). Our research builds upon the existing literature discussing the trade liberalization that occurred in Brazil during the 1990s. Specifically, we demonstrate that regions that were moderately affected by the trade shock in the medium run experienced indirect effects in the long run. To account for potential issues of migrant self-selection, we employ a gravity equation framework to construct geographic components of various migration flows. We show the extent to which migration flows have transmitted the effects of the trade shock and shed light on the role played by different types of migration in this process.

Keywords: Trade liberalization, long-run crime, internal migration, return migration, spillovers, transmission

JEL classification: D31, F16, J13, C26.

2.1 Introduction

Gary Becker's groundbreaking paper introducing an economic theory of crime, published over fifty years ago, laid the foundation for understanding the rational decision-making process of potential criminals [13, 61].

According to Becker's theory, individuals' choices regarding engaging in criminal activities are influenced not only by the probability and severity of punishment but also by the opportunity cost associated with alternative activities.

In subsequent years, the empirical literature has extensively expanded this framework by examining the impact of economic shocks on labor market conditions and, consequently, the opportunity cost of crime [41, 52, 76, 32, 23, 73]. By analyzing various economic shocks, researchers have aimed to assess how changes in labor market conditions alter the trade-off between engaging in criminal behavior and pursuing legal avenues of income generation.

Indeed, this literature has recognized labor market shocks as drivers of changes in crime rates. However, it is important to acknowledge that these shocks rarely occur in isolation. Even shocks that initially target specific regions or industries can have broader effects through various channels, such as the stimulation of migration flows. Such flows can serve as a significant avenue for transmitting the effects of shocks across different regions or even countries.

There is little literature on whether local shocks can affect other regions by causing affected individuals to migrate. Understanding spillovers of local shocks is essential to design policies that help displaced workers and prevent crime in both directly and indirectly affected regions.

The literature scarcely addresses the extent to which local economic shocks generate regional spillover effects through the migration of impacted individuals. A nuanced comprehension of these spillover dynamics is necessary for the formulation of policy interventions aimed at both assisting displaced workers and mitigating crime in regions that are directly and indirectly affected.

Thus, the complex relationship between migration, labor market conditions, and crime is yet to be fully understood and is subject of ongoing debate and investigation among policymakers and economists. Understanding this relationship is challenging due to the inherent endogeneity of migration and the concurrent impact it has on both labor supply and the demand for local goods and services. As a result, identifying causal effects in this context is a difficult task.

Researchers have employed various methods, including natural experiments, to overcome the identification challenges associated with this topic. However, the existing literature has yielded conflicting findings regarding the effects of migration on both crime rates and labor market outcomes. Some studies have suggested a positive association between migration and crime, highlighting the potential for increased criminal activity in areas experiencing a significant influx of migrants [25]. Other studies have found no significant relationship or even negative associations, emphasizing the potential positive contributions that migrants can make to local economies and communities. Therefore, the literature indicates that the impact of migration on labor market outcomes and crime is influenced by various factors, including the characteristics and skills of the migrants, as well as the specific context in which migration occurs.

Using the evacuation resulting from Hurricane Katrina, [84] revealed that migration associated with the hurricane led to a dominant negative impact on wages due to the increase in labor supply outweighing any potential increases in labor demand. This highlights the challenges posed by the offsetting effects of migration on both sides of the labor market.

The influence of human capital levels among migrants further complicates the relationship. For instance, the Greek government-debt crisis prompted return migration from Albania to Greece. That is, Albanian immigrants working in Greece decided to return to their home country. However, [48] found that this return migration led to higher wages in Albania. The returning migrants possessed more experience and agricultural training than the average Albanian worker, resulting in increased labor demand in the agricultural sector. As a consequence, unemployment decreased and wages were pushed up. This demonstrates the potential positive impact of return migration with higher human capital on labor market outcomes.

As with human capital differences, the presence of both migrants and returning migrants adds further complexity to the intricate relationship between migration, labor markets, and crime. Migrants decide how much time to spend abroad based on factors such as relative prices between the origin and destination, the complementarity between consumption and location, and the returns on the human capital they have acquired at the destination. Any shocks affecting these factors can influence the timing of migrants' decisions and the consequence of migration flows [35, 2]. Returning migrants may bring back savings and human capital necessary for success upon their return, which can positively impact economic and social outcomes in their places of origin [36]. However, this positive effect may not hold true when the return is driven by negative shocks. In such instances, their migration is involuntary and may result in workers entering labor markets where their skills are not as in-demand. For example, migrants returning from industrial hubs to their agricultural hometowns may face challenges in finding suitable employment in the industrial sector.

Using the Great Recession as a shock, [22] found that migrants returning to Mexico from the United States contributed to a decrease in crime rates. [24] employed data from the Mexican Census to identify Mexican migrants who have returned from the United States. By asking respondents about their country of residence five years prior to the interview, they are able to track the movements of individuals. Their findings reveal that employment shocks experienced in the United States have significant social and economic repercussions in Mexican communities. Specifically, negative shocks in the US labor market lead to an increase in return migration, a decrease in emigration rates, a decline in remittance flows from the US to Mexico.

Collectively, these studies consistently highlight migration and return migration as significant mechanisms through which economic shocks are transmitted to other regions or countries. The characteristics and skills of migrants, along with the specific contexts and shocks involved, shape the observed effects on crime and labor market outcomes. Understanding these dynamics is essential for designing effective policies and interventions that can harness the potential benefits of migration while mitigating any adverse consequences.

Our research aims to examine the role of various migration flows in transmitting the effects of economic shocks. To accomplish this, we focus on the trade reform that took place in Brazil during the 1990s, which resulted in substantial tariff reductions and had a significant impact on local labor markets. In our study, we build upon the work of [32] and draw insights from studies conducted by [39], [22], and [48].

In our analysis, we make two critical distinctions: firstly, between migrants and returning migrants; and secondly, between migration flows originating from areas significantly affected by trade shocks versus those less affected. These distinctions represent a notable contribution to the existing literature, which has predominantly examined migration and return migration in isolation. In contrast, our study employs a joint model of domestic migration and return migration, enabling us to assess whether and how these migration flows distinctly transmit economic shocks. Furthermore, by focusing on domestic flows, our research investigates the impacts of migration and return migration without the confounding effects of institutional barriers to relocation, offering a more unadulterated analysis of movement dynamics. Lastly, our study enriches the discourse on the impacts of the Brazilian trade reform of the 1990s, shedding further light on its wide-ranging effects.

Therefore, we evaluate (1) whether domestic migration flows transmit the impacts of economic shocks to different regions, and (2) whether migrants returning to their birthplaces affect crime differently compared to other displaced workers. We leverage the Brazilian trade reform of the 1990s as an exogenous trade shock, along with geographic factors as exogenous drivers of migration. This approach enables us to estimate the impacts of the arrival of migrants and returning migrants from areas affected by the tariff reform. We define our treatment variables are as indicators of significant inflows from areas impacted by the tariff reform. Specifically, we identify areas experiencing the largest increases in migration or return migration from the regions most affected by the 1990s tariff reform. We delve into the labor market mechanisms underlying our findings, assessing the impacts of domestic migration and return migration on crime, and elucidating the long-run relationships between Brazilian trade shocks and crime as observed in the literature.

To the best of our knowledge, the impact of return migration on crime has not been previously examined or documented in the existing crime literature. Additionally, our analysis is the first to assess the impacts of return migration in a context where there are no legal barriers to migration. Furthermore, our research is significant contribution in its linkage of migration patterns to the impacts of the Brazilian 1990s trade reform.

Previous studies have explored the effect of international migration on crime [15, 26], the international transmission of local economic shocks through returning migrants [24, 22, 48] and domestic migration and crime [38]. Thus, a comprehensive model of domestic migration, return migration and crime is still a gap in the literature.

Our paper is structured as follows: In Section 2, we provide the context for our study by highlighting key features and trends in crime rates and migration patterns in Brazil. In Section 3, we delve into the details of the Brazilian 1990s trade reform, which serves as the foundation for our identification strategy. In Section 4, we describe the data sources utilized in our study. Section 5 outlines our strategy to address the issue of endogenous migration selection. Section 6 outlines our empirical strategy to examine the impacts of migration and return migration on crime. In Section 7, we present the empirical results of our analysis. Finally, in Section 8, we conclude our paper.

2.2 Migration Flows & Crime in Brazil

Brazil has been grappling with a persistent and alarming level of crime and violence in recent years. In 2017 alone, the country witnessed nearly 64,000 murders, averaging 175 deaths per day, reaching 31 homicides for every 100,000 inhabitants. This trend has been reversed, but rates remain comparatively high. For instance, Brazil's homicide rate was 22 in 2020, compared to 7 in the US [91]. International organizations have drawn attention to Brazil's crime rates, comparing them to countries in conflict zones [93]. Furthermore, the Mexico's Citizens' Council for Public Security, in its annual ranking of the world's most violent cities in 2018, revealed that out of the 50 cities listed, a striking 17 were located within Brazil. These findings serve as a sobering reminder of the magnitude and complexity of the crime problem facing the country.

During the period from 1991 to 2010, Brazil experienced a significant rise in its homicide rate, which increased from 20.67 per 100,000 inhabitants in 1991 to 30.23 in 2010, reflecting a striking 46% surge. This upward trend in crime rates becomes even more perplexing when juxtaposed with the reduction in regional inequalities during the same period. Eight out of the nine Northeastern states, highlighted in red, exhibited income growth surpassing the national average growth between 2000 and 2010 (indicated by the vertical dashed line). Interestingly, during this very period, the crime rate in these states also surpassed the national average (represented by the horizontal dashed line).

Brazil is widely recognized for its pronounced spatial inequalities, as highlighted in the existing literature [10]. The country's population and economic activities are heavily concentrated in the South and Southeast regions, while the North and Northeast regions face a higher prevalence of poverty. In the early 2000s, Brazil implemented the largest conditional cash transfer program globally, known as "Bolsa Família," which aims to assist approximately 11 million families and represents around 0.4% of Brazil's GDP. Notably, more than 50% of these income transfers were allocated to the Northeast states, which played a pivotal role in fostering income growth within the region [**?**].

The implementation of the Bolsa Família program, combined with other contributing factors, has contributed to the observed economic advancements in the Northeast region. This income redistribution initiative has played a crucial role in addressing poverty and promoting economic well-being in areas that have historically faced significant challenges. By targeting vulnerable households and providing them with financial support, the program has helped alleviate some of the spatial inequalities that persist in Brazil. Nonetheless, crime trends were not reversed by the program.

The disconnection between economic prosperity and the escalating crime rates has spurred discussions and inquiries into the underlying factors driving this phenomenon. Some studies have shed light on the lack of a clear association between violence and economic growth in the Northeast region [89]. Experts have put forward various explanations, including the persistent presence of drug trafficking, gang-related violence, systemic corruption, inadequately trained law enforcement personnel, and an inefficient judicial system. These factors have been cited as potential contributors to the observed surge in crime rates in Brazil [71, 93].

The study conducted by [32] represents a significant milestone in this literature as it suggests the Brazilian trade liberalization as an important driver of crime rates. The paper not only establishes a concrete link between economic conditions and homicide rates but also extends the understanding of trade shocks beyond their labor market implications. Moreover, it introduces a temporal perspective by examining crime dynamics over time. The findings of [32] reveal that regions affected by trade shocks experienced a medium-run increase in crime rates. However, they also note that this increase dissipates in the long run. Building upon this research, our paper aims to further expand the analysis and investigate the long-run effects of trade shocks on crime. In doing so, we place particular emphasis on exploring the role of migration and return migration as significant mechanisms through which these long-run effects operate and explain the apparent disconnection between economic prosperity and the escalating crime rates.

We define migration as the process of an individual changing their residence from one region within the country to another region. On the other hand, return migration specifically refers to the movement of an individual from any region (destination) back to the region where they were originally born (origin). In other words, a returning migrant is an individual who was born and currently resides in a particular region, but has previously lived in another region. Figure 2 illustrates the definition of migration and return migration as used in our study.

2.3 Trade Liberalization & Migration Flows

The Brazilian trade liberalization of the early 1990s is widely regarded in the literature as an exogenous shock that significantly impacted the country's economy. This reform involved substantial unilateral reductions in import tariffs and took place between 1991 and 1995. It marked a departure from a long period of strict trade barriers, high tariffs and complete import bans on certain products. As part of the reform, these bans were lifted and replaced with small tariffs, a process commonly referred to as tariffication [31].

Importantly, the Brazilian trade reform is considered a once-and-for-all event, as it occurred within a relatively short time frame, with tariffs remaining stable thereafter. The initial impact of the reform was observed in regions that were specialized in manufacturing, and were exposed to larger tariff cuts. These regions experienced significant labor market changes [31]. These changes were not limited to employment declines but also included a more pronounced decline in formal-sector employment compared to regions facing smaller tariff shocks [74]. These changes in labor market dynamics, in turn, contributed to an increase in medium-run crime rates relative to regions with lower exposure to the trade shock [32].

The process of trade liberalization in Brazil began in March 1990 when President Fernando Collor de Mello unexpectedly eliminated non-tariff barriers [31]. Figure 1 provides an approximation of the percentage change in average prices for each sector resulting from changes in tariffs. The regional impact of these tariff changes can be assessed using the indicator proposed by [59] called Regional Tariff Change (RTC_r). This variable captures the average tariff change faced by region r, taking into account the importance of each sector in regional employment.

To calculate RTC_r, we assign weights to each sector based on its significance in regional employment and wage bill. The resulting RTC_r values range from -0.008 to 0.153, with an average of 0.045. It is worth noting that our definition of RTC slightly differs from that used by [31]. In our representation, tariff reductions are expressed as positive numbers. Therefore, positive values of RTC_r indicate tariff reductions in a given region.

By quantifying the regional variation in tariff changes through the RTC_r indicator, we can better understand the differential effects of trade liberalization across different regions in Brazil. This measure allows us to assess the extent to which regions were exposed to tariff reductions and provides a basis for analyzing the subsequent economic and social impacts of the trade reform.

Both theoretical and empirical literature predict that tariff reductions have an impact on local labor markets and, subsequently, on crime rates. However, the Brazilian data reveals impacts on regions that were not directly affected by the trade shocks, which remains unexplained by existing literature. Our analysis complements previous findings suggesting that the direct impact of tariff reductions on crime diminished over time. Instead, we show that these effects are rather transmitted to regions that were initially unaffected by the trade shock. Hence, when indirect effects are accounted for, the impact of trade shocks on crime persists in the long-run. To account for indirect effects, we introduce a new element to our analysis: the role of migration flows. That is, we hypothesize that the impacts of tariff reductions on specific labor markets could be transmitted to other regions through migration flows.

To conduct a more comprehensive analysis, we categorize migration flows into two distinct types: migration and return migration. In the context of Brazil, individuals from economically disadvantaged regions (origin) historically migrated to more prosperous industrial centers (destination) in search of better job prospects. This movement is defined as migration. However, following the trade shock, some of these migrants experienced unemployment and subsequently returned to their birthplace (return migration) or relocated to a new destination (migration).

To identify returning migrants, we utilize Brazilian census data. This involves examining two key variables: (1) determining whether individuals were born in the municipality where they currently reside and (2) assessing their previous residency in other municipalities in preceding years. By analyzing these factors, we can distinguish returning migrants from other individuals in the dataset.

2.4 Data

In our study, we leverage the same datasets used by [32]. Our primary indicator for crime is the homicide rate, which we derive from mortality records obtained from the Ministry of Health. This source of data offers greater accuracy compared to police reports, as healthcare professionals have less incentive to under-report instances of violence, unlike police officers, victims, or witnesses. To access this dataset, we rely on DATASUS (Departamento de Informática do Sistema Único de Saúde), a system that consolidates various databases from the Brazilian Ministry of Health.

Additionally, we make use of the last four waves of the Brazilian Demographic Census, namely 1980, 1991, 2000, and 2010[1]. The Census data is instrumental in identifying migrants and returning migrants, as it includes questions about an individual's current place of residence, place of birth, and their previous residency five years prior to the Census.

[1]Due to the COVID-19 pandemic, the 2020 Census was postponed and is not available as of the writing of this paper

Moreover, the Census provides valuable information on income, employment, demographic characteristics, and occupation codes, allowing us to measure the number of individuals employed in law enforcement and private security within each region.

To further enrich our analysis, we incorporate geographic data such as distances and altitude differences between municipalities in different regions. These geographic details are sourced from the Brazilian Institute of Geography and Statistics (IBGE) and are publicly accessible via the IPEADATA system.

Finally, our data on tariff changes is derived from the works of [60] and [32]. Following the approach proposed by [59], [32] calculate the Regional Tariff Change (RTC), which represents the average tariff change weighted by the industry composition in each region. This measure allows us to quantify the magnitude of tariff changes experienced by different regions accurately.

$$\begin{aligned} RTC_r &= \sum_{i \in T} \psi_{ri} \Delta \log(1 - \tau_i), \quad \text{with} \\ \psi_{ri} &= \frac{\frac{\lambda_{ri}}{\phi_i}}{\sum_{j \in T} \frac{\lambda_{rj}}{\phi_j}}, \end{aligned} \tag{2.1}$$

where τ_i is the tariff on industry i, λ_{ri} is the initial share of region r workers employed in industry i, ϕ_i equals one minus the wage bill share of industry i, and T denotes the set of all tradeable industries (manufacturing, agriculture and mining). In our analysis, we treat the tariff changes that occurred between 1991 and 1995 as exogenous shocks. These tariff reductions were significant and were followed by a period of relative stability, where tariffs remained relatively constant.

2.5 Endogenous Migration

To address the potential endogeneity between crime and migration, we employ two strategies. Firstly, we construct instrumental variables for migration and return migration. This allows us to estimate the portion of migration and return migration that can be attributed to geographic differences, such as pairwise distance and altitude differences between regions. These geographic components serve as suitable instruments for migration and return migration, as they are unrelated to the crime equation.

Secondly, we estimate our model in log differences, capturing time and region-specific characteristics. This methodology helps control for unobserved factors that may simultaneously affect both crime rates and migration flows. By comparing changes in crime rates over time and across regions, we can isolate the specific effects of migration and return migration on crime.

These two strategies jointly help mitigate the potential endogeneity between crime and migration, allowing us to obtain more reliable and causal estimates of the relationship between the two variables.

In our analysis of bilateral migration flows, we incorporate time-invariant and pair-specific migration costs that are determined by geography and are not influenced by crime or migration itself. This approach is analogous to the gravity model used in trade flow analysis, as introduced by [7] and further expanded upon by [6].

By considering bilateral migration costs, we take into account factors that may deter migration flows between specific pairs of regions. These costs can vary over time or remain constant. However, when they are time-invariant, they are specific to each pair of regions and are not influenced by changes in crime rates or migration patterns. Geographical factors, such as distance or altitude differences between municipalities, are often considered as significant determinants of these migration costs.

To construct our geographic instruments, we adopt the approach proposed by [44] and [6]. For statistical purposes, the Brazilian Institute of Geography and Statistics aggregates municipalities into micro-regions, which provide a more appropriate geographic division for our analysis.

To capture the geographic challenges faced by migrants with greater precision, we construct pairwise geographic difference indicators at the micro-region level. This involves calculating indicators for pairs of municipalities within each micro-region and then averaging these values for the corresponding micro-region pairs.

For instance, to measure the distance and altitude difference between two micro-regions, we calculate the average distance and average altitude difference between the centers of each micro-region's municipalities. Similarly, we construct indicators to capture shared borders and shared biomes (e.g., the Amazon biome) between micro-regions.

Using these constructed geographic variables, we estimate a bilateral migration gravity equation that considers all possible micro-region pairs. This equation allows us to model and quantify the determinants of inter-regional migration and return migration flows. The fitted values from this gravity equation serve as our geographic instruments, which capture the exogenous geographic factors influencing migration patterns. Based on [6], the bilateral migration settlements can be expressed as:

$$M_{ij} = \frac{N_i N_j}{N} \left(\frac{\delta_{ij}}{\Gamma_i \Omega_j} \right)^{1-\theta}, \tag{2.2}$$

where

$$\Gamma_i \equiv \sum_j \left(\phi_j / \delta_{ij}^{1-\theta} \right) \Omega_j^{\theta-1}, \tag{2.3}$$

$$\Omega_j \equiv \sum_i \left(\phi_i / \delta_{ij}^{1-\theta} \right) \Gamma_i^{\theta-1}. \tag{2.4}$$

M_{ij} represents the number of migrants living in region i coming from j; N_i and N_j indicate respectively population size in destination and origin regions, while N is the total population size. The Γ_i is the appropriate 'average' portion of migration costs borne by region i to all destinations, outward multilateral resistance, and Ω_j is the average portion of migration costs borne by j from all sources, inward multilateral resistance (where $\phi_i = N_i/N$ and $\phi_j = N_j/N$). δ_{ij} represents the bilateral migration costs.

Equation (1) is analogous to the [7] gravity model for trade, while equations (2) and (3) respectively trace inward and outward multilateral resistance equations for trade (MRT), but applied to migration. Thus, the MRs are not observable but can be inferred along with δ_{ij}. The equation (1) can be split in two parts: $N_i N_j / N$ is the frictionless share of migrants in country j and $(\delta_{ij}/\Gamma_i \Omega_j)^{1-\theta}$ is the effect of migration frictions. Then, the bilateral migration costs can be represented by the following exponential function for migration cost:

$$\delta_{ij} = FC_{ij}^{\tau_1} VC_{ij}^{\tau_2}, \tag{2.5}$$

where FC_{ij} is the time invariant component of bilateral migration cost driven by the geographical cost of migration (i.e., distance, altitude difference and common border); VC_{ijt} is the time variant cost of migration based on the information cost, and τ_1 and τ_2 are parameters.

In line with [44], we contend that the distance between municipalities provides valuable information regarding bilateral migration patterns. Additionally, geographic characteristics, such as distance and altitude difference, remain unaffected by crime rates, government policies, and other factors that may influence crime. Consequently, these geographic characteristics can serve as instrumental variables in estimating the causal impact of migration on crime. By leveraging the exogenous variation provided by these geographic instruments, we can obtain instrumental variables estimates that isolate the causal effect of migration on crime rates. This approach allows us to address potential endogeneity concerns and obtain more robust and reliable estimates of the relationship between migration and crime. The structure proposed by [78] is the following:

$$M_{ij} = \exp\left(x_{ij}\beta\right)\eta_{ij}, \tag{2.6}$$

where M_{ij} is a given migration flow from i to j, x_{ij} represents the geographic variables and the multilateral resistance terms, β is a vector of parameters and, η_{ij} is a non-negative random variable. The use of an exponential model is motivated by the prevalence of zero values in migration flows, which is a common occurrence in migration settings. In many cases, there are region pairs where significant migration flows either do not exist or were not fully captured by the Census data[2].

[2] That is, to account for the count nature of the data and the presence of pairs with no migration, we employ the Poisson Pseudo Maximum Likelihood (PPML) estimation method. PPML is a widely used technique for count data analysis, particularly in cases where the count variable has excess zeros and the standard Poisson regression assumptions are violated. The PPML estimation allows us to model the relationship between migration flows and geographic variables, taking into account the potential zeros in the migration data. By treating the zeros as "structural zeros" (i.e., cases where migration is not possible or observed), the PPML approach provides consistent and efficient parameter estimates for the non-zero migration flows.

As with multilateral trade resistance equations for trade flows, MRs in migration contexts are not observable but can be inferred along with bilateral migration costs. In our case, the specification becomes:

$$E[F_{ijt}|\mathbf{\Theta}_{ijt}] = exp\left(\beta_1 A_{ijt} + \beta_2 S_{ijt} + \beta_3 B_{ijt} + \boldsymbol{G}_{ijt} + \boldsymbol{D_{it}} + \boldsymbol{O_{jt}} + \epsilon_{ijt}\right) \tag{2.7}$$

Where F_{ijt} represents the number of individuals migrating or returning from region i to region j, $\mathbf{\Theta}_{ijt}$ denotes our set of covariates, A_{ijt} the average altitude difference between the regions, S_{ijt} the distance between the regions, B_{ijt} an index capturing the border shared by the regions, and O_{it} and D_{jt} represent origin-by-year and destination-by-year (multilateral resistance) fixed effects, respectively. We also include a set of fixed effects G_{ijt} for regions in the same state, regions in the Amazon forest, and regions in the state of São Paulo.[3] By estimating gravity models with these augmented specifications, we are able to derive the coefficients of the geographic variables, which in turn allow us to predict the geographic component of migration (or return migration) in terms of the number of migrants.

Figure 4 presents the correlation between migration/return migration and their predicted geographic components, predicted using a gravity equation estimated by PPML. Our gravity equation includes distance, altitude difference and an index capturing the amount of shared borders. We estimate intensive margins only, using just region pairs with return migration greater than zero. We include multilateral resistance and region characteristics fixed-effects. Each observation corresponds to a micro-region, origin-destination pair. N = 68,492. The correlation between migration/return migration and the corresponding geographic component is 0.7487 and 0.7898 respectively.

2.6 Empirical Strategy

We focus on two key comparisons in our study: migration versus return migration and flows from strongly affected areas versus flows from weakly affected areas. To address the

[3]Note that we define region j as the destination and region i as the origin to adapt the notation to return migration, which consists of migrants moving from their destination back to their origin.

potential endogeneity between migration and crime, we employ two treatment variables and two placebo variables, allowing us to explore the causal relationships of interest.

To construct our treatment variables, we begin by ranking all regions according to their RTC values and identify the upper quartile as the harder-hit regions. These regions experienced more significant trade shocks compared to others. Then, using data aggregated at the micro-region level, we define as treated regions those in the top quartile of migration/return migration from most affected areas.

To calculate migration and return migration, we use the number of incoming migrants and returning migrants from the harder-hit regions per 100,000 inhabitants for each micro-region in our sample. This measure allows us to capture the relative fraction of the population composed of individuals who were previously exposed to the economic shock. Higher rates indicate a larger proportion of the population consisting of incoming migrants (and returning migrants) from the harder-hit regions, indicating greater indirect exposure to the economic shock. Using these measures of indirect exposure, we define our treatment variables. The treated group comprises micro-regions with above-median migration (and return migration) rates. Conversely, the control group consists of below-median regions in terms of migration and return migration from harder-hit regions.

To validate the robustness of our results, we create placebo treatment variables as well. These variables indicate the regions in the top quartile of migration and return migration from the least affected areas, specifically the bottom 25% of regions in terms of RTC.

Additionally, we explore the sensitivity of our results by redefining the treatment variables using alternative percentile cut-offs. By varying the threshold values, we ensure that our conclusions are not dependent on a specific choice of quartiles and demonstrate the robustness of our findings across different specifications.

To further address potential self-selection biases in the construction of our treatment variables, we employ instrumental variable (IV) estimation techniques. To construct our instruments, we recalculate our treatment indicators using predicted geographic components of migration and return migration rates from selected regions.

By using the geographic component to construct instruments, we aim to capture the exogenous variation in migration and return migration flows that is driven by geographic factors rather than endogenous factors such as crime rates or other potential confounding variables. Instrumenting the treatment variables allows us to mitigate the potential bias arising from self-selection, where individuals may choose to migrate or return based on unobserved characteristics related to crime rates.

To examine the association between regional tariff changes (RTC) and net out-migration, we conduct a first-stage analysis. In this analysis, we calculate the net migration flow per 100,000 inhabitants by subtracting the total number of individuals leaving region i for another region j from the total number of individuals coming into region i from region j during a given period $t-5, t$. This net migration measure allows us to capture overall migration patterns, irrespective of whether it represents migration or return migration.

Next, we investigate the relationship between the net migration per 100,000 inhabitants on the regional tariff changes (RTC) for each of the four periods under consideration: 1980, 1991, 2000, and 2010. This analysis helps us examine the impact of changes in tariff levels on the magnitude of net migration flows. Following the first-stage analysis, we proceed to our main specifications. Our main specification is formulated as follows:

$$\Delta log(CR_{rt}) = \beta_0 + \beta_1 T^M_{rt} + \beta_2 T^{RM}_{rt} + \beta_3 RTC_r + \boldsymbol{\gamma' \Omega_{r80}} + \boldsymbol{UF_s} + e_{rt} \tag{2.8}$$

Where $\Delta \log(CR_{rt})$ represents the change in crime rate between $t-1$ and t in region r, and $T^M rt$ and $T^{RM} rt$ are our treatment indicators (i.e., indicators of top-quartile increases in migration/return migration per capita from regions most affected by RTC). Recall that the treatment indicators are calculated using migration and return migration into region r during the last 5 years prior to time t. While our treatment variables capture indirect exposure to RTC, RTC_r represents the RTC to which region r was directly exposed. In other words, RTC_r refers to the tariff changes in region r, i.e., the direct exposure to the shock, as opposed to the indirect exposure to RTC, i.e., the RTC experienced by incoming migrants from other regions, which is captured by the treatment indicators. Utilizing both sets of variables allows us to differentiate the direct effects of RTC from spillovers mediated by migration flows. UF_s denotes state fixed effects, and Ω_{r80} represents a set of pre-reform (1980) controls, including non-white share, male share, average years of education, high income share, poverty, and extreme poverty rates. High income share, poverty rate, and extreme poverty rates are defined as follows: the share of the population earning more than 20 times the minimum wage at time t, the share of the population earning less than 1/2 of the minimum wage at time t, and the share of the population earning less than 1/4 of the minimum wage at time t, respectively. We instrument our treatment indicators using indicators constructed from the predicted geographic component of migration and return migration rates from harder-hit regions. Additionally, we implement two extra specifications as a robustness check: one replaces the treatment indicators with placebo treatments, i.e., using least-hit regions instead of hard-hit regions to construct our treatments and instruments.

2.7 Empirical Results

Our empirical analysis reveals several important findings. Firstly, we confirm some patterns suggested by the previous literature [32]. We show that the regions receiving the highest net migration were the ones most affected by the tariff changes. Once tariffs changed, this pattern was reversed, with migrants leaving regions most affected by tariffs. Therefore, we have evidence that tariff changes are associated with worker displacement.

Our main results suggest that when migration and return migration are both included in the model, return migration from harder-hit regions leads to a sizeable decreases in crime rate growth. On the other hand, migration leads to increases in crime rate growth. The magnitude of the effects is the largest for migrants incoming in the 2010-2005 period. When comparing the instrumental variable results to the OLS results we see that the direction of the impact is the same, but results have a larger magnitude and more noise when instruments are used.

Overall, our findings suggest that return migration from areas most affected by RTC plays a significant role in decreasing crime rate growth, while migration from the same areas leads to an increase in crime growth.

Our findings lead to two important conclusions. Firstly, the origin of migration flows plays a crucial role. Migrants and returning migrants from regions unaffected by shocks are likely to have planned their migration and, therefore, have better financial resources and better prospects for fitting into local labor markets. As a result, their presence does not have sizeable effects on the growth of crime rates. Secondly, the context and type of migration are also important factors to consider. Returning migrants, in particular, have family and social support. Consequently, their economic conditions and their ability to fit local labor markets are typically better than those of migrants.

The impact of returning migrants depends on the average labor market conditions in their destination regions. Our results complements the findings of [48], who observed successful skill transfer and market integration among returning migrants in Albania from Greece. Our study reveals that in the Brazilian context, migrants coming from industrial hubs to regions primarily focused on agriculture may be unable to effectively utilize their skills in the local labor markets, unless they have support from their social networks. This suggests that the transmission of economic shocks through on whether incoming workers are migrants or incoming migrants as well as the specific characteristics of local labor market contexts.

2.8 Concluding Remarks

Our findings provide valuable insights into the transmission of economic shocks through migration flows and the contextual factors that shape their impacts. We demonstrate that different migration flows can play a crucial role in explaining the cross-region spillovers of economic shocks. In particular, we find that returning migrants, who are displaced by economic shocks and seek family support in their birthplaces, can have distinct impacts on local labor markets and crime. In the case of migrants returning from Brazilian industrial hubs to their agriculture-specialized birthplaces, we observe that social networks contribute to their fit in local labor markets.

Chapter 3

Improvements in Schooling Opportunities and Teen Births

Abstract

We study the causal relationship between educational attainment and teenage birth rates by focusing on a large-scale, country-wide reform that made high school compulsory and removed previously existing school capacity constraints in Mexico. Relying on administrative data on schools and births, we implement a difference-in-differences strategy that exploits variation across time and municipality-level exposure to the reform to explore the effects of expanding educational opportunities on teenage fertility. We find that teenage birth rates decreased by 2.8 percent after the education reform in municipalities with high increases in high school availability relative to municipalities with low increases. This decline is not driven by a decline in the time teenagers had to engage in risky behaviors (incapacitation effect) but a potential change in expectations for the future.

JEL classification: I12, I21, I28, J13, J16.

Keywords: Education reform; Teenage birth rate; Human capital.

3.1 Introduction

Teenage pregnancy is a global issue, with 15 percent of women giving birth before age 18 globally [86]. Teenage motherhood has been associated with lower educational attainment, labor force participation, and income [49, 21, 56, 27, 62, 42], inferior marriage prospects [50, 40], higher reliance on cash assistance [42], and higher likelihood of falling below the poverty threshold [79]. Although teen birth rates have declined globally in the last few decades, the decline has been uneven in different regions of the world, and developing countries exhibit the highest rates worldwide. Because teenage pregnancy can change the course of a young woman's life and contribute to the inter-generational transmission of poverty, understanding its determinants is a key policy issue.[1]

Women may be more likely to embrace early childbearing in contexts where they perceive socioeconomic progress as not achievable. In contrast, when there is hope for economic and social advancement, delaying motherhood and investing in human capital can be more desirable [54]. Access to education can affect teen pregnancy in different ways. First, it may contribute to raising individuals' future earnings, increasing the opportunity cost of bearing children, and moving the optimal fertility choice towards fewer yet higher "quality" children [12].[2] Second, education can also provide teenagers with better information about contraception [77] and may reduce their available time to engage in risky behaviors [53]. Moreover, education can affect the timing of fertility [57] and change women's preferences for partners, which may indirectly affect their fertility choices [34].

Educational choices and fertility decisions are likely to influence each other, making identifying the causal effect of education on teen pregnancy difficult. We examine the causal

[1] Teen motherhood plays a role in the inter-generational transmission of poverty, as children born to teen mothers may achieve lower levels of education, have a higher probability of teenage childbearing, lower earnings [43], and a higher likelihood of engaging in criminal activity [46].

[2] That is, children with, e.g., access to better education, nutrition, healthcare, and housing.

relationship between access to education and teenage pregnancy by focusing on a large-scale, country-wide, plausibly exogenous expansion in public high school capacity in Mexico, a middle-income country where teen pregnancy rates remain high.[3]

Starting in 2012, Mexico implemented an Education Reform that included high school as one of the compulsory levels of education in the Mexican Constitution. In this setting, the government committed to offering a seat in a public high school to any student of school age by eliminating existing capacity constraints through the improvements and expansions of the existing schools and opening of new ones.

Our empirical strategy leverages the heterogeneity in the implementation of the reform across municipalities and over time by comparing municipalities that experience large increases in high school availability (*high-exposure municipalities*) to municipalities with smaller changes in availability (*low-exposure municipalities*) in a difference-in-differences setting.[4] We combine different sources of information, including annual municipality-level administrative data on enrollment, number of schools, and births.

Our findings indicate that the reform increased the number of students by 7.5 percent in high-exposure municipalities relative to low-exposure municipalities. Moreover, we find that teenage first births in high-exposure municipalities decreased by 2.8 percent after the reform, relative to low-exposure municipalities, implying that at least 23,091 teen births were avoided between 2014 and 2019 due to the increased access to high school. Our results are consistent

[3] Latin America is one of the regions with the slower decline in teenage pregnancy, with a rate of 53.2 births per 1,000 teenagers in 2021 [94]. In 2019, for example, Mexico's rate was 58.4 births per 1,000 teenagers (15 to 19 years old). This rate is still higher than Canada's teen birth rate in 1960 (57.3) and the United States in 1973 (57.3), whose rates in 2019 were 6.8 and 16.4, respectively [92]. Figure **??** in Appendix A shows the evolution of first birth rates (first parity) for teenagers (15-19), young non-teenage women (20-24), and all women of reproductive age (15-49) in Mexico. Although these birth rates have declined over time, first births to teenage mothers have consistently remained the highest, with an average of 53 births per 1,000 women between 2008 and 2019. This is followed by first births to women ages 20-24, with an average birth rate of 46 births per 1,000 women. The overall first birth rate of women of reproductive age has been, on average, 22 births per 1,000 women.

[4] We define as high-exposure municipalities those that experience a percentage increase in the average number of schools in 2013-2018, the post-reform period (relative to the average number in 2008-2012, the pre-reform period) above the cross-municipality median and an increase in 2013, the first year of implementation (relative to the year prior to the implementation, 2012) above the median. In addition, if a municipality opened its first school in 2013 or after, we consider this municipality to be high-exposure. In section 3.5.4, we show that the results are robust to considering alternative definitions of treatment.

with a decrease in teen births that is driven by teenagers changing their expectations for the future rather than an incapacitation effect (i.e., our results suggest that the effects on teenage childbearing are not driven by a reduction in the time adolescents have to engage in risky behaviors that increase the probability of early pregnancy). Importantly, we show that before the school capacity expansion, municipalities that later experienced large expansions in high school capacity were on similar teenage fertility trends as municipalities that received a lower expansion. This suggests that the intensity of the expansion in high school availability was not driven by strong demand for schooling in areas where teenage pregnancy was expected to decline.

Our paper contributes to the existing knowledge on the causal effects of education on fertility. Part of the literature provides evidence of this relationship by exploiting variation in educational attainment induced by differences in compulsory schooling regulations across regions and/or birth cohorts [16, 70, 29, 4, 90]. In that case, the identified treatment effects are typically local to students who would drop out of school. In contrast, we exploit large expansions in high school capacity. In Mexico, the reform we analyze made high school education mandatory, but, in practice, there were no penalties for those who did not comply.[5] Then, our analysis is closer to papers that rely on school constructions as a source of variation in school access. In these studies, the treatment effects are more likely to be identified by the population of school-age individuals potentially affected by the expansion in school capacity

[5] An essential element for compulsory education policies to provide plausibly exogenous variation in education is that compliance with such policies is extremely high. For example, Black et al. [16] studied increases in mandatory educational attainment through compulsory schooling policies on teenage childbearing in the U.S. and Norway. In Black et al. [17], they indicate compliance with the rule to start school in Norway when children turn seven was almost perfect for their studied cohorts. This is because otherwise, parents had to apply for an exception to the rule, which health and school specialists and the local government must approve. Similarly, DeCicca and Krashinsky [29] study the effects of education on teenage fertility by relying on variation in education induced by compulsory schooling laws in Canada. They point out that some provinces, like Ontario, introduced penalties for non-compliance and hiring school-aged children. In contrast, in Mexico, the enforceability of high school education as compulsory has been less stringent. Teenagers not enrolling or dropping out of high school are not penalized, nor are their parents or tutors. Therefore, we claim the compulsory aspect of the reform is not the fundamental policy piece that changed educational opportunities but the ease of capacity constraints.

without over-relying on students at the margin of dropping out.[6,7] With this paper, we present new evidence of how improving educational opportunities for teenagers through the expansion of high school access successfully retained them in the educational system and provided them with opportunities for social and economic advancement, resulting in the avoidance of early childbearing.

The remainder of this paper is organized as follows. In section 3.2, we discuss the characteristics of the Mexican education system and the education reform. In section 3.3, we describe our data sources. Section 3.4 discusses our identification strategy and estimation methods. In section 3.5, we discuss our main results. Section 3.6 concludes.

3.2 The Mexican educational system

The Mexican Educational System has a structure closely resembling the American system: it is divided into preschool, primary school, middle school, high school, and higher education. Primary school corresponds to grades 1 to 6, while middle and high school correspond to grades 7 to 9 and 10 to 12, respectively [83]. In the last few decades, primary and middle school have been almost universally provided, but high school has not; it has been subject to capacity constraints, with students having to undergo a competitive application process.

Mexico's public high school education system includes three school types or degrees that students can apply to. General high schools resemble high schools in the U.S. by preparing

[6] Studies using the variation of school availability include the analysis of access to tuition-free primary education in Nigeria in the context of a program that expanded the primary classrooms; the effect of primary school construction in Indonesia [18, 3, 67], and, closer to our setting, the impact of the construction of secondary schools in Brazil [58]. Our study differs from the latter in that the change in school access was induced by an education reform implemented as part of the policy changes that came with the newly elected president's term initiated in 2013. This was an unforeseen reform, as it was never disclosed during his political campaign. Moreover, the expansion of school capacity represents a large-scale, rapid shock to high school education as it mainly relied on the existing school infrastructure and resources across municipalities. In addition, we explore an incapacitation effect as a potential mechanism behind our findings and show that it does not explain the decline in teen births, suggesting a change in expectations for the future as a potential mechanism.

[7] Our results also contribute to the understanding of how changes in the effective costs of attending school affect early fertility. The literature on this matter includes the effects of access scholarships or cash transfer programs [11, 33, 34].

students for undergraduate studies and are usually run by universities. This is the traditional high school type. Technical schools provide high school curricula and classes that aim to prepare students for the labor market. These are smaller high schools with technical classes such as industrial chemistry, gastronomy, and I.T. support. Finally, vocational schools are purely professional and do not provide a high school diploma. Instead, they offer plumbing, carpentry, and welding degrees, among others [83].

Relative to elementary and middle schools, high school was not widely available historically. Therefore, the process of entering a public high school is competitive. After middle school, students apply to high schools. Depending on the region, high school applications and admissions are based on the GPA or standardized test scores. In Mexico City's Metropolitan Area, for example, the Council of High Schools (Comisión Metropolitana de Instituciones Públicas de Educación Media Superior, COMIPEMS) runs a centralized high school admission process for public schools, where a placement exam score is the only determinant of admission. In each application system, prospective students are ranked and assigned to schools according to their school preference, seats available in each school, and their position in the ranking. Before the reform, students were required to score above a point cutoff (at least 31 points) in the placement exam to be considered for admission. After the reform, the minimum score requirement was eliminated.[8]

Unlike the U.S., students are not restricted to school districts of residence and may apply and, if admitted, enroll in any school in the country with available seats. Neither is the funding tied to the district. The majority of the funds for education come from the federal government, although states and municipalities also contribute. In the past, students who

[8]The press release for the results of the 2013 high school standardized test contest in Mexico City provides information on how the reform came to be included in this contest. For example, it indicates that *"... the goal of this contest is the school seats distribution and assignment, not evaluating the performance of prospective students."* In addition, *"... no prospective student was assigned to a non-chosen option, and the requirement for a minimum point cutoff was eliminated to be consistent with the compulsory high school constitutional reform."* It also details what options non-matched students had: *"... students with an insufficient score to be matched to their preferred options or who did not meet the minimum GPA of 7 to attend a high school from the Universidad Nacional Autónoma de México or Instituto Politécnico Nacional, have the option to choose one of the remaining available seats in other schools."*[80].

were not matched to any of their chosen schools (or did not achieve the minimum admission requirements) could not be admitted to any public high school. This meant these students would have to reapply the following school year or abandon the educational system. However, as described in the following subsection, the education reform reduced capacity constraints and enhanced students' opportunities to attend public high schools.

3.2.1 The reform to high school education

By 2012, compulsory education in Mexico included preschool, elementary school, and middle school.[9] That year, the Mexican Constitution was modified to include high school as one of the compulsory levels of education.[10] Starting in the 2012-2013 academic year, the government had the obligation and commitment to guarantee access to public high school education to any student completing the basic education and of age to attend high school. The goal was accomplishing full coverage of all the students of age to attend high school by the 2021-2022 school year [69].

In February 2013, the Education Reform Act (ERA) introduced by former President Enrique Peña Nieto (EPN) was declared constitutional and signed into law. The ERA included major reforms to the educational system in Mexico; high school education was no exception. Building onto the 2012 reform to Article 3, the Education Sectoral Program 2013-2018 (Programa Sectorial de Educación 2013-2018, in Spanish) [81], one of the elements of the ERA, established the need to open new schools, improving and expanding existing schools, and providing virtual education (Prepa en línea SEP) and the open high school education mode.[11] These additional educational modes allowed the expansion and diversification of

[9]These three levels of education integrate the basic education in Mexico.

[10]On February 9, 2012, high school as a compulsory level of education was included in the first paragraph of Article 3 in the Mexican Constitution: "*All individuals have the right to receive an education. The Federal government, states, and municipalities will provide preschool school, elementary school, middle school, and* ***high school****. Preschool education, elementary school, and middle school are the basic education;* ***this and high school are compulsory.***" [30].

[11]This education mode allows students to initiate or continue high school at their own pace. Students can enroll in open high schools anytime without an admission test. There are no age or time limitations to completing the study plan. After concluding the study plan, students receive an open high school certificate.

options to attend the new high school demand [68].[12] With the ERA, capacity constraints decreased. Even students not assigned to a public high school of their choice through the matching system would be given a seat in another school or could continue their education in one of the alternative modes.

We analyze the evolution of the average number of high schools and enrolled students in a municipality from 2008 to 2018. Although both have increasing trends over time, they also show a sharp increase between the 2012-2013 and 2013-2014 school years, the latter being the first school year affected by the reform. In particular, the average number of schools in a municipality increased from around 6.2 in the 2012-2013 school year to around 7.5 schools in 2013-2014, but for the subsequent years, it became closer to 10 schools, on average. Likewise, the average number of high school students across municipalities in the school year before the reform was around 1,650. Once the reform was implemented, this average increased by approximately 300 students; by 2018, it was above 2,200. This figure provides suggestive evidence of a sharp jump between 2012 and 2013 and a sustained increase in high school availability and capacity, reflected in newly available capacity and higher enrollment after the reform implementation.

Although Article 3 in the Mexican Constitution states that high school education is compulsory, in practice, high school-age individuals not complying with the law (or their parents or tutors) are not penalized. As we show in section 3.5.1, the reform greatly impacted the number of high schools and enrollment, causing a shock to the supply of high school education in Mexico that had not been seen before. Even though the compulsory aspect of the law may not be the fundamental factor behind high school enrollment, capacity

[12]The Education Sectoral Program recognized that expanding capacity was not enough to improve students' retention and recognized the importance of minimizing the number of students dropping out of high school. It was fundamental to improve the study plan quality, standardize the study plan quality across the different high school types, and provide tools and knowledge useful for students joining the labor market after high school. It also established the importance of communication between parents and the school system to obtain their support in their children's education and, ultimately, contribute to minimizing the risk factors that affect students' ability to stay in high school. The Education Sectoral Program was also considered a priority for teachers' professional development so that they were trained and prepared to address the changes to the educational system the ERA would bring and attend to the new high school demand [81].

constraints have prevented teenagers of school age from enrolling in high school. In other words, teenagers may have wanted to attend high school even before it was compulsory, but capacity constraints prevented them from doing so.

Another interesting aspect of the ERA is how it came to be implemented. It passed in the first few months following the election of the former president, EPN, and it was not discussed throughout his political campaign. Given the short legislative process and the restricted debate preceding it, the ERA also represents a shock with no anticipation effects. Moreover, during EPN's tenure, the other major reforms focused on the tax system, the regulation of the electricity and oil sectors, labor law, and political reforms that included changes to the legislative procedure. Since none of these changes indirectly affect the educational system, it is unlikely these other policies drove the changes in enrollment after 2013 we identified. Moreover, if these other reforms impacted fertility, we would likely observe changes in fertility across age groups and municipalities, not only in teenagers living in municipalities with higher exposure to the education reform, as shown in section 3.5.2.

3.3 Data

To explore changes in public high school education availability, we rely on school-level administrative census data from Estadistica 911 collected by the Ministry of Education directly from high schools at the beginning of each academic year. We focus on analyses at the municipality level since this is the smallest government level at which decisions on the budget for education are made.[13] We restrict our analysis to the information for the academic years 2008/09-2018/19.

[13]The federal contributions to the General Branch 33 from the Federal Expenditure Budget (Ramo 33 del Presupuesto de Egresos de la Federación, in Spanish) are established as the resources the Federal government transfers to state and municipality treasuries to allocate to expenses in public education, health, infrastructure, public security, and social welfare programs. In particular, the public education budget allocation covers expenses related to education provision, infrastructure, teacher and staff training, and compensation packages [66].

Information on births comes from the Birth Information Subsystem (Subsistema de Información sobre Nacimientos, SINAC, in Spanish) [82] from the Ministry of Health, which contains data on all the birth certificates issued at birth occurrence between 2008 and 2019. This information is collected by hospitals and health facilities and reported to the Health Ministry for its validation and compilation, and it has been available since 2008 [51]. We aggregate birth records by mothers' municipality of residence, quinquennial age groups, and year. Because the impact on women's schooling decisions is more likely to be pronounced with the birth of their first child compared to subsequent children, we restrict our main analyses to the sample of first births.

We also rely on the annual estimates of the total population and population by sex and age group at the municipality level from the National Population Council (Consejo Nacional de Población, CONAPO, in Spanish).

3.4 Empirical strategy

Although the education reform was a national policy, in practice, teenagers' exposure to the reform varies due to differences in the allocation of resources to public education across municipalities. Using a difference-in-differences framework, we exploit the variation in the intensity of exposure to the reform at the municipality level to study its impacts on teenage birth rates.

3.4.1 Intensity of exposure to the education reform

We define the intensity of exposure to the education reform of municipality m based on two elements. First, we consider the percentage change in the number of schools between 2012 and 2013 to capture the discontinuity in school availability as a response to the reform.

$$Growth_m^{12-13} = \frac{S_m^{2013} - S_m^{2012}}{S_m^{2012}} \tag{3.1}$$

Where S_m^{2012} and S_m^{2013} are the number of high schools in municipality m in 2012 and 2013, respectively.[14]

Second, we account for the relative and sustained growth in the number of schools pre-post reform to capture the change in school availability that persists over time once school capacity is expanded through the reform.

$$Growth_m^{pre-post} = \frac{S_m^{\bar{post}} - S_m^{\bar{pre}}}{S_m^{\bar{pre}}} \tag{3.2}$$

Where $S_m^{\bar{pre}}$ and $S_m^{\bar{post}}$ are the average number of high schools in municipality m before and after the reform took place in 2013, respectively; that is, the municipality's average number of schools between 2008-2012 and 2013-2018, respectively.

A municipality m is defined as being highly exposed to the education reform if both $Growth_m^{12-13}$ and $Growth_m^{pre-post}$ are *above the corresponding median* of the distributions across municipalities.[15] Using this information, we construct an indicator variable for municipalities with high exposure ($HighExposure_m$) to the reform and zero otherwise. The intuition behind this definition of treatment is that the sharp and persistent change in available schools in a municipality after the reform is likely to be exogenous to the teenagers in the age range to attend high school. In the remainder of the paper, we will refer to municipalities with high exposure to the reform as high exposure. We will denote the rest of the municipalities as low-exposure municipalities.[16]

Appendix 2 shows the yearly average number of students enrolled in high schools in low and high-exposure municipalities. The red triangles represent the average for high-exposure municipalities, and the blue circles show the average for low-exposure municipalities. Prior to the education reform, the difference in the average number of high school students between

[14] In our analysis, when a high school has multiple shifts, the number of schools is defined as the number of shifts in that high school.

[15] In addition, if a municipality opened its first school in 2013 or after, we consider this municipality as being treated.

[16] In section 3.5.4, we present results by varying the cutoff to define what municipalities are treated according to their locations in the schools' growth distributions. In particular, we show results varying the cutoffs for municipalities with a growth in the number of schools in the 40, 45, and 55 or above percentiles. Although some results become noisier, the main findings hold under these different high-exposure definitions.

high-exposure and low-exposure municipalities exhibited a persistent gap. However, this gap significantly increased starting in 2013. This figure suggests that the reform implementation was not homogeneous across the country and that the changes in enrollment in low-exposure municipalities provide a good counterfactual for the corresponding changes in high-exposure municipalities.

We analyze the geographic distribution of high-exposure and low-exposure municipalities across the country: 511 municipalities (21 percent) fall within the first category, and 1,945 (79 percent) fall within the second category. There is variation across the country's regions in municipalities' exposure to the reform.

Why some municipalities were more affected by the reform than others? Our results show that even before the implementation of the reform, high-exposure municipalities had an average number of schools above the average in low-exposure municipalities. This implies that areas, where high school availability was already higher, are those that were more affected by the reform. This is likely explained by the high school education goals in the Education Sectoral Program, which mainly targeted the implementation of reform through the use and expansion of the existing infrastructure and resources [81].[17]

3.4.2 Estimation Method

We explore the effects of the education reform on teenage birth rates by leveraging variation in the municipality's exposure to the reform. We compare the number of births of women

[17] The Education Sectoral Program emphasized the necessity of increasing high school coverage by taking advantage of the existing resources. Selected excerpts from this Program highlight that: *"Resources are scarce. So, it will be necessary to take advantage of the existing capacity and simultaneously increase and diversity the education supply with new education types." "It is a challenge to increase the education supply. Therefore, it is fundamental to improve education planning capacity. Increases in capacity should respond to the best possible use of existing resources."* The strategies to achieve the goals of this program regarding high school education included the prioritization of investments aimed at expanding physical infrastructure in schools that had space and whose educational model allowed it, the promotion of programs that fully took advantage of the available capacity in existing infrastructure, and the promotion of federal financial support for education options that offered better results in relation to costs.

in population group g in municipalities with low vs. high exposure to reform by estimating the following Poisson model by Pseudo-Maximum Likelihood:[18]

$$E[Births_{gmrt}|HighExposure_{mt}, \alpha_m, \alpha_{rt}, pop_{gmt}] = exp\left(\sum_{j=-6}^{j=5} \delta_j HighExposure^j_{mt} + \alpha_m + \alpha_{rt} + 1ln(pop_{gmt}) + \epsilon_{gmrt}\right) \quad (3.3)$$

where $Births_{gmrt}$ represents the number of first births of women in age group g, living in municipality m, region r, and year t, α_m are municipality fixed effects, $HighExposure^j_{mt}$ indicates if municipality m is high-exposure j periods from the reform year (2014) and zero otherwise, and ϵ_{gmrt} is an error term that we allow to be correlated within municipalities.[19] We control for the population of women, pop_{mgt}, in age group g, municipality m, and year t, as the exposure variable and restrict its coefficient to be unity.[20] We also include region-by-year fixed effects, α_{rt}, to compare changes in outcomes in high and low-exposure municipalities within the same region.[21]

We omit the year before the policy change as the comparison year. The parameter δ_j indicates the average impact of the reform on the rate of first births of women in age group g, j years later. We also estimate a static version of equation (3.3) as follows:

$$E[Births_{gmrt}|HighExposure_{mt}, \alpha_m, \alpha_{rt}, pop_{gmt}] = exp\left(\delta HighExposure_{mt} \times Post_t + \alpha_m + \alpha_{rt} + 1ln(pop_{gmt}) + \epsilon_{gmrt}\right) \quad (3.4)$$

where $Post_t$ is an indicator variable for the period 2014-2019. The remaining variables are the same as those in equation (3.3). In this case, δ recovers the average effect of the reform on the first birth rates of women in age group g.

[18] We consider quinquennial age groups: 15-19, 20-24, 25-29, 30-34, 35-39, 40-44, and 45-49.

[19] We define 2014 as the first year affected by the reform to account for a nine-month pregnancy period.

[20] Algebraically, by including as the exposure variable the log of the corresponding population and constraining its coefficient to be equal to 1, this is equivalent to having the birth rate as a ratio of the population of women as the dependent variable. We implement this estimation using the *ppmlhdfe* Stata command with the relevant population in the *exposure* option.

[21] We consider Mexico's eight regions: northeast, northwest, north-center, south-center, east, west, southeast, and southwest.

The identifying assumption underlying our research design is that in the absence of the education reform, teenage birth rates in high and low-exposure municipalities (within the same region) would have followed the same trends in the years after the reform. We provide empirical evidence supporting this assumption in section 3.5.

3.5 Estimated Results

3.5.1 Impacts of the education reform on high school capacity

For the education reform to change teenage fertility trends, municipalities must expand their high school availability, and teenagers must perceive these changes in educational opportunities and take advantage of them, which would reflect increases in enrollment. In section 3.4.1, we presented suggestive evidence of differential trends after 2013 in the high school enrollment between high-exposure and low-exposure municipalities. We formalize this evidence in our event-study, which estimates corresponding to a slightly modified version of equation (3.3) in which the outcome variable is the number of students enrolled in high school.[22]

Before the reform, enrollment followed similar trends in high-exposure and low-exposure municipalities, providing support for the parallel trends assumption. Estimates show that after the reform, enrollment increased by 6.5 percent in 2013 in high-exposure municipalities relative to low-exposure municipalities, and this increase is statistically significant at the 1 percent level. Our results show an increase in enrollment of 6.5 percent: $[exp(0.063)-1]\times 100$. The average increase in enrollment in the post-reform period is approximately 7.5 percent.

We also examine if the education reform affected the enrollment decisions of males and females differently. The event-study estimates by sex and corresponding confidence for males

[22] In this case, the post-reform period starts in 2013, when the expansion of high school availability started. Since we consider data on school enrollment for 2008/09-2018/19, we recover the estimated effects for $j \in [-5, +5]$. In addition, the exposure variable in this analysis is pop_{mt}, the population of teenagers in municipality m in year t.

and females overlap, suggest that males and females took advantage of the expansion in high school capacity similarly.

3.5.2 Impacts of the education reform on teenage birth rates

In our analysis of the effect of the expanded high school availability on first births, we focus on three age groups: 15-19 years old (teenagers), 20-24 years old (non-teenage young women), and 15-49 years old (all women of reproductive age).[23] We expect to observe the changes in first births to be concentrated in the 15-19-year-old women group since they are the ones of age to attend high school. In addition, as most 20-24-year-old women in our analysis were not directly exposed to the reform, we expect the reform to have negligible or significantly smaller effects on this group than on the 15-19-year-old women.

Since the education reform should have only affected the fertility of women of high school age, overall birth trends are unlikely to change significantly between high vs. low-exposure municipalities after the reform, other than due to its effect on high-school-aged women. Otherwise, our estimates could capture other factors that generate differential trends between high and low-exposure municipalities besides the education reform.

Before diving into the estimated effects of the education reform on (first) teen births, we discuss the descriptive information on the evolution of first birth rates by females' age group across municipalities' exposure to the education reform. For the period included in our analysis (2008-2019), high-exposure municipalities have higher first birth rates for these three groups of women relative to low-exposure municipalities. However, the difference is relatively small. Birth rates by age group and for all women of reproductive age show a decline after the reform (2008-2013 vs. 2014-2019) for all age groups in high and low-exposure municipalities. Nonetheless, first-birth rates declined more after the reform in high-exposure municipalities than in low-exposure municipalities. In particular, teen first birth rates in low-exposure municipalities went from 52.7 to 50.5 births per teenage women, whereas in high-exposure

[23]The group of all women of reproductive age contains information for the following quinquennial age groups: 15-19, 20-24, 25-29, 30-34, 35-39, 40-44, and 45-49.

municipalities, they declined from 55.9 to 51.6. We observe a similar pattern for the 20-24 age group, although the difference in birth rate declines between low vs. high-exposure municipalities is more modest.

Our event-study corresponds to the estimates of δ_j in equation (3.3). We begin with the estimates for the first births of all women of reproductive age. The estimated results show that the education reform did not differentially change birth trends across municipalities. The point estimates in the post-reform period are negative but insignificant. A potential explanation for this is that the reform negatively impacted teenage pregnancy jointly with no effect on older women's fertility. We examine this possibility by estimating the model for women in different age groups.

Next, we restrict the sample to births of 15-19 years old and confirms the effects of the reform on fertility are concentrated among teenagers. Before the education reform, first births to teenagers trended similarly across high- and low-exposure municipalities, which provides evidence that supports the validity of our parallel trends assumption. However, once the reform was implemented, teen births decreased more in high-exposure municipalities than in low-exposure municipalities. Although births show a lagged response to the reform, on average, they decreased by 3.8 percent in high-exposure vs. low-exposure municipalities three to five years after the reform. Panel (c) shows the estimates for the sample of first births to 20-24-year-old women. The reform did not change this group's birth trends in high vs. low-exposure municipalities.

We also estimate average effects of our specification in equation (3.4), considering all women of reproductive age, 15-19-year-old women, and 20-24-year-old women. The estimated effects indicate that the education reform decreased first teen births by 2.8 percent in high-exposure municipalities relative to low-exposure municipalities.[24] In the case of first births of all women of reproductive age and 20-24-year-old women, the education reform does not induce statistically significant changes at conventional significance levels.

[24] $[exp(-0.028) - 1] \times 100 = 2.76$

3.5.3 Future opportunities vs. contemporary incapacitation

With the education reform, teenagers who would not have been able to attend a public high school before had the opportunity to enroll, thanks to the easing of capacity constraints. However, the reduction in teenage fertility may be driven by several factors. On the one hand, students may perceive the expansion in educational opportunities as a means of social and economic advancement, which changes their expectations and aspirations for the future and incentivizes them to avoid early fertility. On the other hand, the reform may not have changed students' expectations for the future and only represents an incapacitation effect. So, when teenagers are out of school, they may still engage in risky behaviors that increase the chances of an early birth.

To examine these possibilities, we test for differences in the effects on summer pregnancies, defined as first births with an associated month of conception during June to August (i.e., when teenagers are not "incapacitated" in school) and non-summer pregnancies (first births with an associated month of conception during the rest of the year). We calculate each pregnancy's approximate conception date using information on the birth date and weeks of gestation. If the reform had only affected non-summer pregnancies, this would suggest that our results are likely to be explained by an incapacitation effect for teenagers rather than a change in their aspirations for the future.

We estimate the event studies for teenage births with associated summer and non-summer pregnancies. Although the event study for summer pregnancies is noisier due to fewer births happening in these months than the rest of the year, this figure provides evidence of no differences in the effects between teenage pregnancies during and out of the school year. This suggests that students were no more likely to engage in risky sexual behaviors during the summer than the rest of the year. Therefore, the increase in access to high school is likely to have been perceived by teenagers as a potential improvement in their future social and economic opportunities.

3.5.4 Robustness checks

Our identification relies on a definition of treatment (i.e., high-exposure municipalities) that uses a threshold considering both the increased access to high school during the first year of the education reform and the sustained growth in high school access over time. By construction, however, the group of low-exposure municipalities includes areas treated to a lesser extent. This implies that our estimated results should be interpreted as lower bounds for the effects of the education reform on teenage pregnancy. We illustrate this idea and show robustness to our results by following two strategies. First, we show how our estimates change when we vary the treatment threshold. Second, we exclude from the control group sets of municipalities that are more likely to be significantly affected by the reform (i.e., municipalities with growth in high school access closer to the threshold).

In our treatment definition, we consider a municipality as highly exposed to the reform if both the relative growth in the number of schools pre-post reform and the relative growth in the number of schools between 2012 and 2013 are above the median (i.e., the percentile 50) in their corresponding distributions. We test the robustness of our estimates to changes in the distribution thresholds for a municipality to be considered highly exposed to the education reform. In particular, we redefined the rule for a municipality to be considered treated by setting the treatment inclusion criteria that the municipality's number of schools' relative growths are above the percentile x in the corresponding growth distributions, where $x = 40, 45, 55$. Appendix 2 shows the estimated effects of these alternative definitions of treatment. The results show that after the reform implementation, teenage first births declined in high-exposure municipalities relative to low-exposure in all the cases. However, the more restrictive the threshold becomes (i.e., the higher the x), the less likely we can identify statistically significant effects of the reform on teenage birth because the control groups contain municipalities with higher exposure to the reform relative to when the threshold is less restrictive (i.e., $x = 40, 45$).[25]

[25]Appendix 2 shows the event studies for each of the different thresholds.

We also redefine what municipalities we consider as low-exposure. In particular, we excluded municipalities whose schools' growth was close to the 50th percentile to reduce the potential contamination in the comparison group. We exclude municipalities with growths in the number of schools between 45-50, 40-50, and 35-50 percent. For example, in the last case, we consider high-exposure municipalities with a growth in the number of schools above the 50th percentile in the growth distributions vs. municipalities with a growth in the number of schools below the 35th percentile. Then, using these comparison groups, we estimate the effects on teenage first births using equation (3.4). The results show that the estimated effects of the education reform on teenage births are robust to excluding these municipalities from the comparison group and that as we get a cleaner counterfactual, the estimated effects on birth rates increase from 2.8 to 5.1 percent when we exclude growths between 35 and 50 percent from the control group.[26]

Finally, as non-school-age women are more likely to have had children in the past than teenagers, we estimate the effects of the reform considering subsequent births (i.e., not-first births). We find that the reform did not affect the births among females who already had children. This is true both for teenagers and older women. These results suggest that increased access to high school education reduced teenage births among those who did not have children before the program's implementation. These results make sense since teenagers and older women who already had children are expected to be less affected by a high school reform because they are less likely to continue their educational investments as a response to the reform relative to childless teenagers.

[26]Appendix 2 shows the event studies varying the municipalities considered low-exposure according to their location in the growth distributions.

Bayesian Structural Time Series Method

As a robustness check, We additionally use a Bayesian Structural Time Series Method (BSTS) to construct a counterfactual for the national changes in teenage births in the absence of the reform.

Our event study and DiD estimates are a lower bound of the true effect as they rely on comparing teenage births in high-intensity municipalities vs. low-intensity municipalities. To understand how underestimated our event study and DiD estimates are, we rely on a Bayesian Structural Time Series Method (BSTS).

Our BSTS estimations indicate that, after the reform, teenage births decreased by around 104,041 births, representing a 6.7 percent reduction. This finding indicates our DiD estimate is a lower bound for the reform's effect on teenage fertility. Therefore, the education reform reduced teenage fertility by 3.1 to 6.7 percent.

The Bayesian Structural Time Series Method (BSTS) proposed by [20] addresses the difficulty in constructing counterfactuals that truly account for the pre-treatment trend in outcomes because possible confounding factors such as seasonality [88] may be ignored. A common methodology used to construct counterfactuals is Synthetic Control Method (SCM). However, this method usually assumes stationarity, which might be too restrictive [1]. BSTS allows constructing a counterfactual relaxing this assumption.

Intuitively, BSTS provides a forecast of what the outcome would have been in the absence of the treatment. Then, it compares the actual outcome to the counterfactual and provides an estimate of the causal effect. The counterfactual is constructed using a trend component, seasonal effects, and relevant covariates to forecast the outcome. Such covariates, which [20] refers to as control variables, are analogous to the donor's pool in SCM.

The BSTS approach is particularly useful when an intervention is implemented country-wide, i.e., all units are treated simultaneously, and standard SCM assumptions might not hold, such as in our case. Along with the advantage of being an autoregressive model, it has the advantage of accounting for seasonality, and residual autocorrelation, with time-varying local trends and coefficients. The assumptions are that the structure itself is not changed by the treatment and that there are no treatment spillovers to the control series.

These assumptions are likely to hold in our setting since we use macroeconomic aggregates to model fertility, and the education reform did not have immediate, short-run impacts on macroeconomic aggregates. To assess whether such assumptions hold, we can examine the control time series and look for structural breaks in the intervention date.

We draw from the literature about *baby booms* to select relevant forecasting variables which link economic cycles to fertility. Among the economic drivers explored by the literature are: unemployment, interest rates, pensions, and government subsidies for having children [75]; divorce, marriage, and infant mortality rates [14]; education [16], per-capita income, hourly wages, and hours worked [45]. Since we have high-frequency fertility data, we can link fertility and standard national-level aggregates. This allows us to construct a counterfactual for the post-reform period.

Formally, the system of equations that defines the BSTS is as follows:

$$\begin{aligned} births_t &= \boldsymbol{Z}_t^T \boldsymbol{\alpha}_t + \epsilon_t \\ \alpha_{t+1} &= \boldsymbol{T}_t \alpha_t + \boldsymbol{R}_t \gamma_t \\ \boldsymbol{Z}_t^T &= \sum_{j=1}^{J} x_{j,t} \beta_{j,t} \\ \boldsymbol{\beta}_{j,t+1} &= \beta_{j,t} + \eta_{\beta,j,t} \end{aligned} \tag{3.5}$$

where $births_t$ is the number of births of 15-19-year-old mothers in year t. This variable is modeled as a function of the state vector $\alpha_t + 1$ and the output vector Z_t. The state evolves according to a local linear time trend component and a seasonality component, captured by the control matrix R_t and the transition matrix T_t as described in [20]. The error terms $\epsilon_t \sim \mathcal{N}(0, \sigma_t^2)$ and $\eta_t \sim \mathcal{N}(0, Q_t)$ are the scalar error term and the q-dimensional system error with a $q \times q$ state-diffusion matrix, respectively. The output vector is a linear function of each control $x_{j,t}$. To confirm our results, we also run the same specification replacing $births_t$ with births of mothers of other age groups.

Equations (7) and (8) show in detail the two variations of $x_{j,t}\beta_{j,t}$ used. Despite being linear, this component has dynamic coefficients, i.e., coefficients vary across time and evolve as a random walk.

Our specification uses monthly time series from January 2008 to December 2018, and it is defined as follows:

$$\sum_{j=1}^{J} x_{j,t}\beta_{j,t} = births_t^u\beta_{1,t} + pop_t\beta_{2,t} + divorce_t\beta_{3,t} + marriage_t\beta_{4,t} + unemp_t\beta_{5,t} + \pi_t\beta_{6,t} \tag{3.6}$$

Where $births_t^u$ is the number of births among women aged 25 to 29 (untreated), pop_t the total Mexican population, $divorce_t$ is the number of divorces, $marriage_t$ the number of marriages, $unemp_t$ the unemployment rate, and π_t the inflation rate, all these variables defined in month-year t.

To estimate causal impacts, we choose a time cutoff from which individuals are treated. We use the series before the cutoff to estimate the model and then use the estimated model to predict the dependent variable after the cutoff. The comparison between the predicted and the observed births allows us to assess the causal impact of the reform. In our case, we select as the cutoff January 2014 to assume 2014 as the first year births were affected by the reform to account for a nine-month pregnancy period.

Regarding inference, the method follows a Bayesian approach and specifies a prior distribution to model parameters and initial state values, which are updated. Then it is possible to generate samples using MCMC (Markov Chain Monte Carlo) and a Kalman filter. Posterior inference proceeds by simulating draws of the model parameters and state vector. These yield a simulated posterior predicted distribution of the counterfactual time series. This distribution also allows calculating a simulated distribution of the difference between the counterfactual outcome and the observed one. That is, with posterior predictive distribution over counterfactual, it is possible to construct a posterior distribution of the causal effect and the distribution of the cumulative effect of the intervention over time.

Our BSTS estimates rely on monthly data for the following variables to construct the counterfactual for teenage births. These variables are: (1) the number of 20-24-year-old women giving birth, (2) the annual population, (3) the number of divorces, (4) the number of marriages, (5) the unemployment rate, and (6) inflation measured by the national price index variation. The dependent variable is the births of 15-19-year-old women.

Using this methodology, we generate a counterfactual for the changes in teen births in the absence of education and compare it with the observed teen births to estimate the changes in births. The ultimate goal of using this methodology is to learn more about the extent of the reform's effect on births, given that our DiD estimates provide us with a lower bound of the true effect of the reform.

In Appendix 2 we show the evolution of actual and predicted births of 15-19-year-old women (first parity) using 2014 as the cutoff to start the prediction. The first graph (original) plots the predicted counterfactual (dashed line), its 95 percent confidence interval, and the observed birth series. The second graph (pointwise) plots the estimated pointwise treatment effect and its corresponding 95 percent confidence interval. Finally, the third graph (cumulative) depicts the cumulative treatment effect over the predicted period.

Between 2014 and 2018, monthly teenage births were, on average, 17,977 births. In contrast, in the absence of the reform, we would have expected 19,711 monthly average teen births (with confidence intervals, CI:[19,233, 20,546]). Comparing the counterfactual number of births with the observed births indicates that the reform induced a decrease of 1,740 monthly teen births (CI:[-2,569, -1257]).

The cumulative effect of the reform between 2014-2018 is the avoidance of 104,041 teenage births (CI:[-174,870, -26,993]). This cumulative effect is the difference between the actual cumulative teen births (1,078,599) and the predicted births by the counterfactual (1,182,640). This is equivalent to an approximate 6.7 percent decrease in births relative to the number of teen births in 2012.[27] Given the approximate percent decrease in teen births we estimate

[27] This percent was calculated by dividing the cumulative avoided births by the number of years in 2014-2018, which assumes each year had the same number of teen births. Then, we took the ratio between

with the BSTS model, we confirm that our 3.1 percent DiD estimate is a lower bound for the effect of the reform on teenage fertility.

As a placebo, we reestimate the model using, as the dependent variable, the births of 20-24-year-old women. Overall, we do not identify statistically significant differences between the observed and predicted births for 20-24-year-old women. If any, statistically significant changes in births start showing off at the end of the impulse response. This may be explained by the fact that the first women treated by the reform (those 15 years old in 2013) became part of the 20-24 age group 5 years later (turned 20 in 2018). Therefore, some women in this group may have been affected by the reform in their teens. Nonetheless, we do not identify a change in births for these women induced by the reform.

3.6 Conclusions

We explore the causal relationship between educational attainment and teenage birth rates by focusing on a large-scale, country-wide, exogenous shock to public high school capacity. This shock eased high school capacity constraints by constructing new schools and creating new shifts in the existing ones.

Although the education reform was a national policy, its implementation was not homogeneous across the country due to differences in capacity and budget constraints at the municipality level. Using a differences-in-differences approach, we exploit these differences in municipalities' exposure to the reform. This reform increased enrollment by 7.5 percent in high-exposure municipalities relative to low-exposure municipalities. Moreover, birth rates of 15-19-year-old women decreased by 2.8 percent in high-exposure municipalities relative to low-exposure municipalities. As a consequence of the improved high school access, 1,124,465

the avoided annual teen births and the actual teen births (first parity) in 2012–a year before the reform was implemented:

$$\%\delta births = \frac{104,401/5}{312,587} = 0.067$$

additional students enrolled in high school during the 2008/09-2018/19 period, and at least 23,091 births to teenagers were avoided. This implies that for every 100 students (45 females and 55 males) who gained access to high school in the context of the reform, two births to teenage women were avoided. We do not observe statistically significant changes in the number of births for other age groups after the reform.

These findings shed light on the importance of providing teenagers with educational opportunities that can change their expectations and aspirations for the future and incentivize them to delay parenthood and continue their human capital investments. In the context of the U.S., previous research has suggested that policies specifically aimed at preventing teen pregnancy, such as sex education or increased access to contraception, are unlikely to considerably improve outcomes for disadvantaged teenage women; however, policies improving economic opportunities, reducing poverty, and improving prospects for adulthood have more potential to decrease teenage fertility [54]. In particular, financial aid for higher education and early childhood education programs have been identified as interventions that could effectively reduce teen pregnancy [63]. In this paper, we present evidence of an education policy targeting teenagers that successfully created opportunities for them and retained them in school, potentially changing their aspirations and expectations for the future and ultimately reducing early births.

Chapter 4

My Parents Are Not Home: Parental Employment and Teenage Fertility

Abstract

This study investigates the influence of labor market dynamics on teenage fertility rates and assesses the degree to which parents mediate these effects. Furthermore, it investigates whether job destruction exerts distinct impacts on teenage fertility relative to other labor market shocks analyzed by the literature. Leveraging the timing and the number of jobs lost due to establishment closures, we examine how parents' and teenagers' labor market experiences can shape teenage fertility outcomes. We explore potential mechanisms underlying these findings, examining the repercussions of job destruction on the income and labor supply of mothers, fathers, and teenagers. Additionally, we construct gender-specific measures of job destruction and investigate its effects on fertility, income, and labor supply, across different genders and age groups. We corroborate prior findings that negative labor market shocks exert opposing effects on teenage and adult fertility. Specifically, we present evidence suggesting rises in teenage fertility caused by job destruction and identify fathers' and teenage girls' incomes as the primary link between job destruction and teenage behavior.

Keywords: Job destruction, establishment closure, labor market, teenage pregnancy, fertility, human capital, labor supply.

JEL classification: J13, J16, J22.

4.1 Introduction

Teenage pregnancy is a persistent concern in numerous developed countries and in the majority of the developing world. In 2021, approximately 15% of women worldwide gave birth before the age of 18 [87]. The implications of such early pregnancies are profound, with maternal health complications ranking among the top five leading causes of death and disability among teenage women [87]. These alarming figures underscore the urgent need to comprehensively examine the factors driving teenage pregnancy rates.

The effects of teenage pregnancy extend beyond health outcomes, potentially imposing substantial constraints on a young woman's future economic prospects. Extensive research indicates that early and unplanned pregnancies are strongly linked to diminished educational attainment, marital stability, and income levels, among other socioeconomic outcomes [49, 21, 56, 27, 55, 42, 43, 85]. This literature demonstrates that the consequences of teenage pregnancy can extend beyond young mothers themselves, yielding spillovers upon other family members, such as siblings [37]. This literature also reveals that children born to teenage mothers are particularly more likely to experience adverse social and economic outcomes, similar to those experienced by their mothers [43]. The intergenerational transmission of these adverse outcomes underscores the far-reaching impacts of teenage pregnancy. Moreover, from this literature, we may also conclude that teenage childbearing amplifies racial and gender disparities, as it demonstrates that it (1) exerts adverse effects and (2) disproportionately affects minority populations [85]. Consequently, a comprehensive understanding of the factors contributing to teenage pregnancy is a vital step in the implementation of effective early pregnancy prevention strategies that ultimately address inequality and disrupt the intergenerational cycle of poverty. Nonetheless, existing literature has not comprehensively explored the mechanisms linking teenage fertility rates to economic outcomes. Moreover, it has not delved into the gender-specific dynamics potentially connecting parental labor market outcomes to the likelihood of early childbearing. Finally, the literature has yet to examine the specific effects of job destruction and establishment closures.

These questions are relevant as teenagers differ from adults in their fertility decision-making processes, facing unique challenges and vulnerabilities such as limited information access, lacking confidence and experience, and incomplete formal education. These factors often render teenagers less prepared to make informed decisions regarding their reproductive health, setting them apart from their adult counterparts. Moreover, teenagers' mental well-being is more vulnerable to changes in the stress levels prevailing in their household environment. Because of that, data indicates that the trends in teenage fertility rates diverge from those observed among adults. [8], for example utilized the National Longitudinal Survey of Youth and state-level unemployment rates to examine the association between economic cycles and teenage pregnancy. The study revealed that teenage fertility rates tend to increase during periods of economic recession, while adult fertility rates exhibit a contrasting decrease during such periods. [5] obtains contrasting findings, suggesting that teenage births decrease following economic downturns. Research from fields outside of economics has consistently emphasized the role of parental communication in preventing teenage pregnancy. This literature suggests that effective communication between parents and teenagers serves as a mechanism for providing valuable information, guidance, and support to teenagers who may be struggling with decisions related to their reproductive health [47]. Hence, it is a plausible hypothesis to test that parents serve as the link between economic fluctuations and teenage pregnancy. Conversely, teenagers, permitted to work from age 14, may be directly impacted by job destruction if they participate in the workforce (Fair Labor Standards Act, 1938). Disentangling these mechanisms is pivotal for crafting policies that adequately support teenagers. Thus, we broaden this literature by evaluating whether previous findings persist in the context of job destruction and exploring the gender and age-specific mechanisms potentially driving such relationships.

Understanding the relationship between job destruction and teenage fertility, as well as the mechanism linking them, presents a significant challenge, as labor market conditions can affect family dynamics through various interconnected pathways, some of which may counteract each other. For example, when a father faces adverse labor market conditions, reductions in his income can potentially resonate throughout the household, influencing factors such as overall stress levels, parental division of labor, or time spent with teenage children. Additionally, the teenagers may concurrently face their own labor market challenges, furthering the complexity of proper identification. Determining the prevailing impact of labor market conditions in this context requires an empirical strategy that considers these possible mechanisms, as well as underlying heterogeneities at play.

In this paper, we focus on job destruction within the American context and across all of income levels. Furthermore, we delve into the potentially differential impacts of mothers', fathers and teenagers' layoffs on teenage pregnancy outcomes. Lastly, we supplement these findings by examining all teenagers up to the age of 19 and assessing the direct impact of job destruction on this demographic. Therefore, we explore the potential mechanisms that connect labor market conditions and teenage fertility rates. We build upon the work of [64] and [19] and employing job destruction due to establishment deaths, that is, layoffs arising from establishment closures, instead of all mass layoffs, as exogenous labor market shocks.

Similar topics have been examined by the previous literature. Drawing on data from the National Education Longitudinal Survey of 1988, [65] argues that the impact of labor market conditions on teenage fertility outcomes can vary significantly. His study suggests an income-dependent relationship between a mother's employment and her teenage daughter's likelihood of giving birth. Specifically, the impact of a mother's employment status differs across schools with varying income levels. In a study examining aggregate data from Illinois, [85] showed that teenage pregnancy rates vary with income levels, the share of the white population, and the proportion of two-parent families. The analysis indicates that teenage pregnancy tends to decrease as income increases and as the share of the white population and two-parent families rises. Another study by [5] uses data from the North Carolina Job Loss databank to investigate the effects of mass layoffs resulting from economic downturns on teenage pregnancy using North Carolina data from 1990 to 2010. These findings indicate that while the overall effects on teenage pregnancy are relatively small, they are negative and significant for black teens and exhibit variation based on income levels. A study conducted by [19] combined individual-level administrative datasets from Brazil to explore the effects of parental job loss during mass layoffs and access to unemployment insurance on various child outcomes within low-income, welfare program beneficiary families. The research assessed impacts on metrics such as school completion, engagement in informal work, involvement in criminal activities, and early pregnancy, focusing specifically on mass layoffs, defined as the dismissal of over one-third of a workforce within a year. The study found positive impacts of mass layoffs on early pregnancy (10 to 15 years-old), but found neither substantial support for changes in the relative time allocation between mothers and fathers nor significant differences in the income reductions experienced by either parent. The research concludes that parental job loss elevates the risk of fertility among children aged 10 to 15 by approximately 18%, with no distinction between job loss experienced by fathers or mothers, though access to unemployment insurance mitigates this effect. Interestingly, these findings contrast with [5], which found a 2% decrease in teenage fertility for a 1% increase in jobs lost.

We contribute to this literature by further investigating the potential mechanisms that link labor market conditions to teenage fertility rates, building upon the work of [64] and [19]. We innovate by employing job destruction due to establishment deaths, i.e. layoffs resulting from establishment closures and by decomposing such measure by gender. Furthermore, we compare the effects on fathers, mothers and teenagers to assess the mechanisms driving our findings. We also contribute to the literature by sampling all income levels, for all of the US. This is relevant, considering that household labor allocation dynamics may differ for populations with different income levels and that the previous literature found contrasting effects for Brazil and the US. To accomplish this, we utilize data from three primary sources:

(1) the Business Dynamics Statistics (BDS), (2) the American Community Survey (ACS), and (3) the National Center for Health Statistics (NCHS). Our first step is investigating whether the occurrence of job destruction during the year of conception can explain the prevalence of teenage pregnancy cases. Additionally, we explore the family and individual-level dynamics using information from the ACS, including specific details on income and weeks worked by parents of teenagers during the year of conception.

Our findings demonstrate that job destruction leads to higher teenage fertility and lower adult fertility. We show a similar pattern in other risky behaviors, such as property crime, where male teenage property crime notably surges in response to job destruction compared to other age cohorts. Lastly, we offer evidence suggesting that the primary channels through which job destruction might influence teenage fertility revolve around the income of teenage girls and their fathers. These particular outcomes are the most profoundly affected by job destruction, whereas mothers and teenage boys do not exhibit significant responses in terms of income and weeks worked. Changes in teenagers' household income, as well as fluctuations in teenage girls' personal income, affect social dynamics that can potentially lead to teenage pregnancy.

The structure of the paper is as follows: Section 2 provides an overview of the data sources we utilize in our analysis. In Section 3, we outline our empirical strategy. Sections 4, 5 and 6 present the empirical analysis of the effects of job destruction on teenage fertility rates and household labor market outcomes. Finally, in Section 7, we conclude.

4.2 Data

As previously stated, we have two primary objectives: (1) to establish a link between the job destruction experienced by both parents and teenagers and subsequent teenage fertility rates; and (2) to explore the potential mechanisms underlining this link, examining the impacts across all family members and considering heterogeneities in the relationship, incorporating factors such as access to abortion, race, and ethnicity.

To address the first objective, we employ data from the National Center for Health Statistics (NCHS), which provide birth record data at both the individual and aggregated county levels. Data aggregated at the county level, spanning from 2000 to 2019, provides teenage birth rates. For our purposes teenagers are individuals aged 15 to 19 years-old. Two distinct sets of county-aggregated data are available for this age: The first provides fertility rates, calculated utilizing the U.S. Census Bureau's (USCB) county-level population-by-age estimates. In this set counties with populations under 100,000 within each state combined together into a single unit. Conversely, the second set offers county-level teenage fertility rates calculated with the NCHS's Hierarchical Bayesian model population estimates, which does not aggregate smaller counties. Additionally, these datasets can be merged with lagged data from the USCB's Small Area Income and Poverty Estimates (SAIPE), allowing us to incorporate county-level teenage poverty rate estimates into our analysis.

Our treatment variable is derived from the USCB's Business Dynamics Statistics (BDS) data, which we term "Adjusted Job Destruction". The BDS data provides detailed information on net employment changes at the establishment level, categorizing changes stemming from establishment openings (referred to as "births") and closures ("deaths"), as well as establishment expansions and contractions. Positive labor market shocks can trigger resource reallocation among sectors or corporate restructuring, wherein job destruction in one domain may be offset by job creation in another. Moreover, we might have formal establishment closures that are just *de facto* changes in establishment ownership. Consequently, our measure of Adjusted Job Destruction is computed by subtracting the jobs created from establishment openings from the jobs lost due to establishment closures. More specifically:

$$AJD_{sct} = \frac{\sum_{i=1}^{n}(JDD_{isct} - JCB_{isct})}{POP_{sct}} \tag{4.1}$$

Where JDD_{isct}, JCB_{isct} and POP_{sct} are respectively job destruction due establishment deaths, job creation due to establishment births and population for state s county c, industry i and time t. Although the BDS identifies job destruction as the aggregate of employment losses from both contracting and closing establishments from year $t-1$ to year t, our treatment variable is restricted to establishment closures, omitting employment changes due to establishment contraction. Furthermore, changes in employment, as articulated in the BDS, are compiled and presented by county, firm size, firm age, industry, and year. With the availability of employment change data by industry, we utilize a strategy similar to [9], employing industry gender shares to construct county-by-year-by-gender metrics of employment change. For conciseness in our discussion, we will refer to our treatment variable measure simply as "Adjusted Job Destruction" (AJD) throughout.

The American Community Survey (ACS) data allows us to address objective (2) and, to a limited extent, objective (1). To tackle (2), we examine the individual-level data of the groups under consideration, namely teenagers and their parents, utilizing the same treatment variable, i.e. yearly AJD. We evaluate whether individuals subjected to increased job destruction experience lower income levels or reduced work weeks.[1]

To tackle objective (1) and use the ACS to investigate fertility, there are two possible strategies: Firstly, we identify teenagers who recently transitioned into parenthood at the time of the survey and associate them with their cohabiting parents. The probability of a teenager having borne a child in the preceding year is then modeled as a function of the demographic characteristics and the AJD occurring during the previous year, along with

[1] To convert county-level variables into PUMA-level (Public Use Microdata Areas) variables, we utilize the crosswalks provided by the U.S. Census Bureau, which allows us to merge the county-level data with the ACS PUMA-level data.

a set of fixed effects. Secondly, for all cohabitating children and parents, we utilize their age difference to determine whether they became parents as teenagers in the past, even if they are not teenagers at the time of the survey. This enables us to identify the year of conception and align it with the respective AJD. We model the probability of childbirth at a specific age across generations, utilizing demographics as controls and the AJD individuals were exposed to in the previous year, while incorporating generation fixed-effects, i.e., fixed effects for the year when they reached that particular age. However, this provides merely limited evidence. The American Community Survey encompasses a sample, and the observed number of teenage parents is small, rendering estimates subject to noise. Furthermore, when making cross-generational comparisons, despite the increased the number of observed teenage parents, we encounter the limitation of migration uncertainty. For instance, if we identify an individual who is 33 and resides with a 17-year-old child at the time of the survey, we acknowledge that this individual was a teenage parent 17 years ago. We can recover the AJD they were exposed to at the time of conception, but only under the assumption that their residence has remained consistent. This is less of a concern for the case of labor market outcomes, as we analyze their relationship with AJD occurring the year prior to the survey. An additional limitation of the ACS data lies in the fact that observations are restricted to families residing within the same household. Therefore, the treatment effects derived from this analysis will be local to this specific group.

Lastly, we verify whether our findings related to teenage fertility are applicable to other risky behaviors exhibited by teenagers. We want to show that increased teenage fertility following job destruction is not planned, but rather product of risky behavior. Therefore, we leverage supplementary data from the FBI's Uniform Crime Report and revisit the NCHS data. From these datasets, we extract: (1) county-by-year property crime data categorized by the age of the perpetrators, and (2) county-by-year mortality rate data classified by selected external causes and age. Because our focus is risky behavior, we omit mortality rates resulting from medical errors, natural disasters, or airplane accidents, restricting our rates exclusively to causes such as suicides, homicides, overdoses, confrontations with the police, and accidents involving vehicles, falls, explosions, drownings, among others.

By using Ajusted Job Destruction (AJD) along with these datasets, we aim at facing the endogeneity issues arising from typical labor market variables. Consider, for example, that unobserved alcoholism of a parent can influence both parents' labor market outcomes, their relationship with their children and the probability of teenage childbearing. Likewise, reverse causality may be at play: a mother might exit the labor force upon learning of her teenage daughter's pregnancy. Given the endogeneity issues intertwined with standard observable labor market outcomes, like employment status, it is essential to explore alternative shocks less prone to bias. Beyond the concerns of endogeneity, establishment closures distinguish themselves from other forms of labor market shocks in several pivotal ways. Notably, such closures tend to unleash more persistent and widespread impacts on local labor markets, often mirroring swift structural shifts. These shifts affect employment and with time may affect wage structures and transform industry compositions, as well as the demand for specific skills. While Adjusted Job Destruction (ADJ) might initially signify the exogenous displacement of workers, it potentially saturates local labor markets in the longer run, exerting downward pressure on wages [28]. For these reasons, we opted to use AJD, which impacts all workers, irrespective of their personal attributes. This methodological choice is inspired by the research conducted by [64] and [19], who incorporated mass layoffs into their analyses. However, AJD has one limitation: while establishment closures involve non-selective layoffs, establishment openings might exhibit selectivity in hiring. Therefore, to ensure the robustness of our findings with net job destruction, we also estimate all models omitting JCB_{isct} from the calculation of AJD. The results do not change significantly when we do so.

Exploration of our data reveals that teenage fertility robustly correlates with both teenage poverty (Figure 5) and unemployment (Figure 5.15). Nonetheless, Adjusted Job Destruction

(AJD) exhibits correlation with unemployment and unemployment insurance filings, thereby offering an exogenous component to labor market fluctuations (Figures 5.18 and 5.19).

To calculate Adjusted Job Destruction (ADJ) disaggregated by gender, we employ gender shares from 1990 as a baseline. Essentially, we apply state-wide industry gender shares to both the number of jobs destroyed and created, providing a gender-specific ADJ rates:

$$AJD_{gsct} = \frac{\sum_{i=1}^{n} \gamma_{gsi90}(JDD_{isct} - JCB_{isct})}{POP_{gsct}} \tag{4.2}$$

Where JDD_{isct}, JCB_{isct} and POP_{sct} are respectively job destruction due establishment deaths, job creation due to establishment births and population for state s, county c, industry i and time t. γ_{gsi90} is the share of gender g among workers of the workers of industry i in state s in 1990. Then, AJD_{gsct} is the adjusted job destruction for gender g. To calculate gender shares in 1990, we utilize data from the 1990 U.S. Census, aggregating working individuals by state, industry, and gender, subsequently enabling the computation of gender shares.

By utilizing gender shares per sector obtained from the 1990s U.S. Census, we can compute AJD by gender at the county and year levels. It is important to note that we exclude healthcare and education sectors from our analysis due to their potential influence on teenage pregnancy through channels other than teenagers and their parents. Moreover, to ensure consistency and comparability across datasets, we employ the Census' NAICS (North American Industry Classification System) and PUMA-county (Public Use Microdata Areas to county) crosswalks. These crosswalks enable us to harmonize the industry codes and geographic divisions employed in our analysis. By aligning these datasets, we can effectively explore the relationship between labor market conditions and teenage fertility.

Lastly, to account for time-varying state-level shifts in abortion regulations, we employ a state-by-year index that reflects teenagers' access to abortion. Specifically, this index reflects laws that mandate parental consent for minors seeking abortion care. These regulations also reflect the overall policy landscape surrounding reproductive rights and the consequential implications for pregnant teenagers. Notably, these regulations also serve as a proxy for other abortion access-related policies. Our measurement of these regulations is anchored in the exhaustive dataset compiled by [72], which encompasses a thorough aggregation of annotated statutes, judicial decisions, attorney general opinions, and advisory articles in medical journals. It also integrates secondary sources like newspaper articles and policy environment overviews from various scholars, advocates, and governmental entities. With [72] offering a coding of policy environments spanning over six decades, our analysis can appropriately account for heterogeneous access to abortion both across states and through time.

4.3 Empirical Methods

Our initial specification consists a model of teenage birth rates by county, year and age groups, using the data derived from the National Center for Health Statistics (NCHS):

$$BR_{ct} = \beta_0 + \beta_1 NW_{ct-1} + \beta_2 H_{ct-1} + \beta_3 P_{ct-2} + \boldsymbol{\gamma_1}' \boldsymbol{AJD}_{ct-1} + \boldsymbol{\theta_s} \times \boldsymbol{T_t} + \kappa_c + \epsilon_{ct} \tag{4.3}$$

where BR_{ct} is the teenage birth rate at county c, year t, NW_{ct-1} is the share of black and native American population, H_{ct-1} the share of Hispanic population and $\boldsymbol{AJD_{ct-1}}$ is either AJD in county c, year $t-1$ or the vector of gender-specific AJD measures for county c, year $t-1$. Again, these gender-specific measures are a decomposition of net job destruction using the gender shares of each industry for each state in 1990. P_{ct-1}, $\boldsymbol{\theta_s} \times \boldsymbol{T_t}$ and κ_c are respectively, a lagged index of teenage poverty, state-by-year fixed effects, and county fixed effects. To manage the correlation across time, we cluster standard errors at the county level. It is worth reiterating that we use two datasets for teenage fertility: one aggregates smaller counties within each state, while the other does not. Consequently, for the second dataset, we weight observations by county population and abstain from doing so for the first, as the aggregation inherently plays such a role in the initial dataset. In Section 4, we expand our estimation to accommodate various age groups and introduce lags and leads of the treatment variable.

Our subsequent estimations involve analyzing birth rates utilizing ACS data. We use two approaches. Initially, we fix the age and identify when individuals, in all ACS iterations, turned a specific age (e.g., 17), given they are 17 or older at the time of the survey. Next, we determine whether they became parents that year, and recover the AJD they experienced the preceding year. Consequently, we model the probability of having children at, for example, age 17 as a function of race, ethnicity, and the AJD encountered at age 16, incorporating state-by-year fixed effects, with the reference year being when individuals turned 17. Essentially, we are comparing teenagers across counties and generations, modeling their probability of parenthood as a function of AJD exposure. This encompasses all teenagers of a designated age, spanning generations, with a dummy variable indicating whether they had children at that age, and the job destruction they were exposed to in the year prior to reaching that age. ACS person weights are utilized in this regression.

Our second approach also expands the number of teenagers in the dataset in a similar way. We evaluate each surveyed individual during the time they belong to a certain age group. For every participant in the ACS, we trace the AJD they experienced at diverse ages, such as 14, 15, 16, and so forth. Consequently, we restructure our data so each entry represents an individual at a specified age, allowing for the possibility of the same individual appearing multiple times within the dataset. To elaborate, each row corresponds to an individual at a certain age, containing information regarding whether they had a child at that age, the ADJ experienced in the preceding year, the year they were surveyed, and their residential location at the time of the survey. Subsequently, we model the probability of childbearing in a given year as a function of age, race, ethnicity, region, state-by-year fixed effects, and PUMA fixed effects. The respective first and second approaches are as follows:

$$P(c_{it}|a_{it}) = \beta_0 + \boldsymbol{\Psi'\delta_i} + \boldsymbol{\gamma'}\boldsymbol{AJD}_{it-1} + P_p + \theta_s \times T_t + +\epsilon_{it} \tag{4.4}$$

$$P(c_{it}) = \beta_0 + \beta_1 a_{it} + \beta_2 {a_{it}}^2 + \boldsymbol{\Psi'\delta_i} + \boldsymbol{\gamma'}\boldsymbol{AJD}_{it-1} + P_p + \theta_s \times T_t + \epsilon_{it} \tag{4.5}$$

where $P(c_{it}|a_{it})$ is the probability that an individual i bears children at time t conditional on such individual being age a at time t, $P(c_{it})$ is the probability that an individual i bears children at time t. $\boldsymbol{\delta}_i$ is a vector of demographic controls, i.e. the individual's race, ethnicity and gender. $\boldsymbol{AJD}_{it}$ is the AJD the individual was exposed to at time $t-1$ (which coincides to the conception year for individuals that had children at time t). Finally, we have PUMA fixed-effects P_p and state-by-year fixed-effects $\theta_s \times T_t$. Additionally, we include an indicator of the year the individual was surveyed.

Lastly, we introduce the specifications designed to investigate underlying mechanisms. In an effort investigate the dynamics of income and labor supply, we rely two outcome variables: the number of weeks worked and log of total income. Given the absence of data regarding labor market outcomes across time in the survey, our reference point defaults to the survey time. Therefore, we employ the following specification:

$$LM_{it} = \beta_0 + \beta_1 a_{it} + \beta_2 {a_{it}}^2 + \boldsymbol{\Psi'\delta_i} + \boldsymbol{\gamma'}\boldsymbol{AJD}_{it-1} + P_p + \theta_s \times T_t + \epsilon_{it} \tag{4.6}$$

Where LM_{it} is a labor market outcome, either the log income or weeks worked reported by individual i at the time of the survey t, a is the age of individual i at time t, $\boldsymbol{\delta_i}$ a vector of demographic controls, i.e. race and ethnicity, JD_{it-1} is the lagged net job destruction (or a vector of gender-specific job destruction measures). Finally, we include PUMA fixed-effects

P_p and state-by-year fixed effects $\theta_s \times T_t$. Note that we omit gender as a control variable, as this model is estimated separately for mothers, fathers, teenage boys, and teenage girls.

4.4 Results

4.4.1 Teenage Fertility

We begin our results discussion with county-level birth rate models, utilizing population estimates from the UCSB. Our findings suggest that, in fertility terms, teenagers are most significantly impacted by job destruction. Al0igning with existing literature, our results show that, while fertility diminishes with AJD across most age groups, it notably increases for teenagers. As shown in Figure 5.21, the impacts of AJD are positive and statistically significant for teenagers aged 15 to 19, while conversely, they are negative and significant for individuals aged 20 to 44. For all other age groups, the effects are not statistically significant.

Figure 5.22 shows that the outcomes for teenagers remain consistent when switching to the dataset that encompasses all counties and employs NCHS population estimates. In both scenarios, the point estimates of job destruction coefficients linger between 25 and 35. Furthermore, as shown in Figure 5.23, the incorporation of controls does not substantially impact the coefficient magnitudes, suggesting that our treatment operates as random. More specifically, the minor difference between the estimation with and without controls originates from the lagged teenage poverty controls, not from the controls for race and ethnicity.

Additionally, we explore the heterogeneities in our findings by splitting our samples based on diverse county characteristics. Specifically, we estimate models separately for counties with above and below median shares of Hispanic and Black populations, and abortion restrictions as of 1990. Additionally, we conduct separate analyses for large and fringe metropolitan areas compared to the remaining, less densely populated regions. Our findings suggest that the impact of AJD on fertility is more pronounced in counties with above-median shares of Hispanic and Black populations and in those with more restrictive abortion policies, as shown in Figure 5.24.

4.4.2 Robustness and Other Outcomes

To evaluate the robustness of our findings, we modify AJD to examine its alternative lags and leads. If job destruction induces riskier behaviors in teenagers, leading to increased pregnancies, we should observe past AJD affecting fertility, not future. Our results confirm this, demonstrating positive impacts of past AJD on teenage fertility, while revealing minor and insignificant effects from job destruction at birth or thereafter, as shown in Figure 5.25.

If AJD prompts teenagers to engage in riskier behaviors, we might anticipate impacts on other outcomes, such as mortality from selected external causes and property crime rates. That is, if teenage fertility increases are not planned, but rather product of risky behavior, we should see other consequences of risky behavior as well. In fact, teenage property crime rates exhibit the most pronounced response to AJD, with notable, though smaller and still significant, effects also witnessed among individuals aged 10-14, 20-24, 30-34, 35-39, and 40-44, as seen in Figure 5.26. Similarly, our heterogeneity analysis underscores that teenage property crime, like teenage fertility, spikes more sharply with AJD in counties with above-median black and Hispanic population shares. Notably, an exception is observed regarding access to abortion heterogeneity (5.27).

Regarding mortality due to selected external causes (which encompass accidents, overdoses, homicides, and suicides) the influence of AJD proves to be insignificant for teenagers and nearly all other age demographics, as seen in Figure 5.28.

Lastly, we use the American Community Survey (ACS) data to verify aforementioned impacts on teenage fertility. As detailed in Section 3, we can expand the dataset, ensuring each observation corresponds to an individual i at time t, due to our access to the ages of individuals and of their cohabitating children. Subsequently, we model the probability of childbirth at time t as a function of age, race, ethnicity, geography, and exposure to ADJ in the prior year. Again, this approach yields limited evidence due to two primary constraints: (1) uncertainty regarding an individual's residence in each year, with only their location at the survey time being observed, and (2) the modest number of observed teenage parents at the time of the survey.

While our findings echo the mechanism identified in our aggregate results, they are notably noisier. As shown in Figure 5.29, the point estimates denote a reduction in fertility rates as a response to AJD for all age groups above 25 years. Conversely, for groups up to 24 years of age, fertility appears to rise with job destruction, although these effects are not statistically significant. Recall that we incorporate state-by-year fixed effects to adjust for temporal and regional variations and that the treatment variable, AJD, is the same as previously utilized, but adjusted to the Public Use Microdata Areas (PUMAs) where individuals reside.

Our findings roughly align with previous results, but lack statistical significance. Again, these results must be interpreted cautiously due to the sample's limitations. Nonetheless, for examining mechanisms, the ACS offers a valuable reference as we show in the next section.

In conclusion, our findings consistently illuminate the impact of AJD on teenage pregnancy. In the following section, we delve into the mechanisms underpinning these findings.

4.5 Mechanisms

To identify mechanisms, we model individual outcomes as functions of demographics along with the same AJD measures previously applied to model aggregate fertility rates. Specifically, look into the log income and weeks worked of teenagers and their parents. Changes in labor market conditions have the potential drive family decisions, for example, prompting one of the parents to increase their labor supply to offset earnings losses. Furthermore, income losses can escalate stress and domestic conflicts, which in turn might sway teenagers' risk-taking behavior. To investigate these dynamics, we harness the extensive data encapsulated in the American Community Survey, exploring the reverberations of job destruction on weeks worked and income.

Examining results by age, we assess AJD's impact on personal income across diverse age groups to discern its direct effects on teenagers and potential ramifications for their parents. Within our sample, teenagers are defined as individuals aged 15 to 19, with an average age of 17.03, while parents of teenagers average at 47.48 and 44.86 years for fathers and mothers, respectively. Notably, the average age of teenage parents is 18.14. Median ages for parents are 47 for fathers and 45 for mothers. Interestingly, as shown in Figure 5.30, our findings indicate that the income of the 40-44 age group is most significantly impacted by AJD, both in point estimates and statistical significance. This evidence supports the claim that parent's income are one of the links between teenage fertility and job market conditions.

Our findings indicate no significant impact of job destruction on labor supply, with a notable exception for teenage girls. While point estimates for all groups suggest a reduction in labor supply, it's exclusively teenage girls that exhibit a significant decrease, as shown in Figure 5.32. Regarding income, different effects emerge: both fathers and teenage girls encounter significant negative impacts, as seen in Figure 5.33. Consequently, since we observe tangible effects on fathers' income and female teenagers' income, the main driver of teenage fertility remains elusive. Nevertheless, the data implies that if parents serve as a link between teenage fertility and labor market conditions, income fluctuations may wield more influence than alterations in time allocation. Furthermore, due to our reliance on age-group data, pinpointing specific age-related fertility increases remains a challenge. Older teenagers, being more likely to participate in the labor market, are more likely to be directly impacted by such shocks. Hence, if effects are predominantly experienced by 18 and 19-year-olds, it bolsters the argument for a direct impact, unmediated by parental influences. A deeper dive using ACS data, modelling the likelihood of childbearing for teenagers at distinct ages as a function of the preceding year's AJD, uncovers significant effects solely for 14-year-olds, as shown in Figure 5.34. While these findings lean towards supporting the hypothesis that parents act as mediators, the inferential limitations arising from using birth data from the ACS limits the conclusiveness of this evidence.

4.6 Gender-specific Job Destruction

In this section, we employ gender-specific AJD calculated using 1990 gender shares per industry. Thus, here we leverage variations in county-level industry composition and state industry gender shares. The results are shown in Figure 5.35. Despite the inherent limitations of this decomposition, given we do not observe the exact number of each gender laid off, analyses of the re-estimated models suggest teenage fertility predominantly responds to female job destruction. The effects of male job destruction are negligible and insignificant, whereas those of female job destruction emerge as positive and significant, albeit wide confidence intervals hinder precise interpretation.

While revisiting the impacts on income and weeks worked for mothers, fathers, teenage girls, and teenage boys, results displayed in Figures 5.37 and 5.36, show that the only significant negative effect emerges from male AJD influencing fathers' income. Nevertheless, again results deriving from gender-specific are excessively noisy, preventing conclusive interpretations.

4.7 Conclusion

Our findings show that AJD leads to higher teenage fertility rates and lower adult fertility rates. Heterogeneity analysis shows that the effects are more intense in regions predominantly Black or Hispanic as well as in states with strict abortion regulations. The effect is also observed for crime, which supports our risky behavior hypothesis.

Individual-level birth data yield point estimates similar to the ones obtained at the county level, but with wider standard errors. These data also show that the 40-44 age cohort is the most affected, while the average age of teenagers' parents is 46.17. Individual-level results also show that weeks worked are not significantly affected by AJD, but fathers' and teenage girls' incomes are negatively affected. The effects on teenage fertility are driven by female AJD, while the effects on fathers' income are driven by male AJD.

These results confirm previous findings regarding teenage and adult fertility for other labor market shocks. Fathers are shown to be affected, but teenage girls are also affected. Moreover, teenage fertility is affected by female AJD more than male AJD, which drives changes in fathers' income. Therefore, although parents may be a link between the labor market and teenage fertility, we cannot rule out direct effects as well.

In conclusion, our investigation consistently reveals impacts of AJD on teenage fertility, and opposing effects on adult fertility. The effects of AJD, as evidenced by our results, demonstrate robustness across varied measures of teenage fertility and diverse specifications. In terms of mechanisms, our data does not permit us to rule out a direct impact, suggesting that while parental income may serve as a mechanism, a direct influence on teenage girls also remains plausible.

Chapter 5

Conclusion

In this dissertation, we have illuminated some aspects of the relationship between economic and population dynamics, underscoring the significance of these interactions for both policy-making and economic theory.

In the first chapter, we demonstrated that trade shocks displace workers, and the impact of these workers' arrival depends on whether they are returning migrants or new migrants. Because returning migrants potentially possess knowledge of the local economy and access to social networks, their impact on local economies and social indicators can be more beneficial than that of regular migrants.

In the second chapter, we showed that increased access to high school education significantly reduces teenage fertility. We also found that these effects are not associated with merely constraining teenagers' free time, suggesting that the observed reductions in fertility likely stem from improved access to information and enhanced future economic prospects for teenagers.

Lastly, in the third chapter, we revealed that labor market shocks affect adults' and teenagers' fertility in divergent ways. While negative labor market shocks lead to a decrease in fertility among adults, teenagers exhibit an increase in fertility during economic downturns. Our evidence supports the view that this differential impact is due to increased risky behavior among teenagers. Although parents are affected by the shocks, our analysis suggests a direct impact on teenage girls in particular, who participate in the labor force and are thus directly influenced by labor market conditions.

Through these findings, we have illustrated how changes in the economic prospects of individuals influence their fertility and migration decisions. These decisions, in turn, can affect future economic and social prospects, highlighting the importance of accounting for population dynamics in policy design.

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Appendix 1

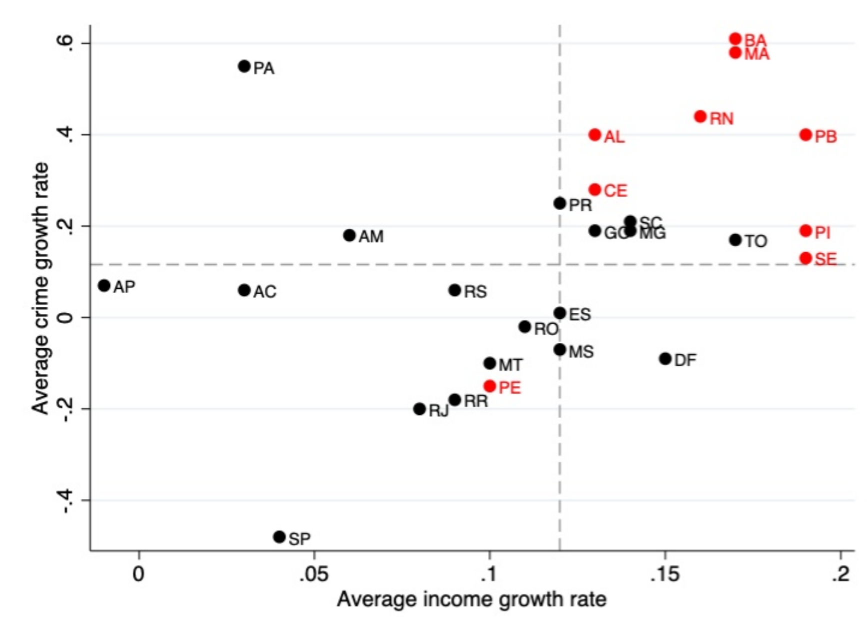


Figure 5.1: Average growth rate of crime vs average growth rate of income: Brazilian states – 2000 to 2010.

Notes: Northeastern states highlighted in red. Horizontal dashed line represents the average of the crime growth rate. Vertical dashed line represents the average of the income growth rate.

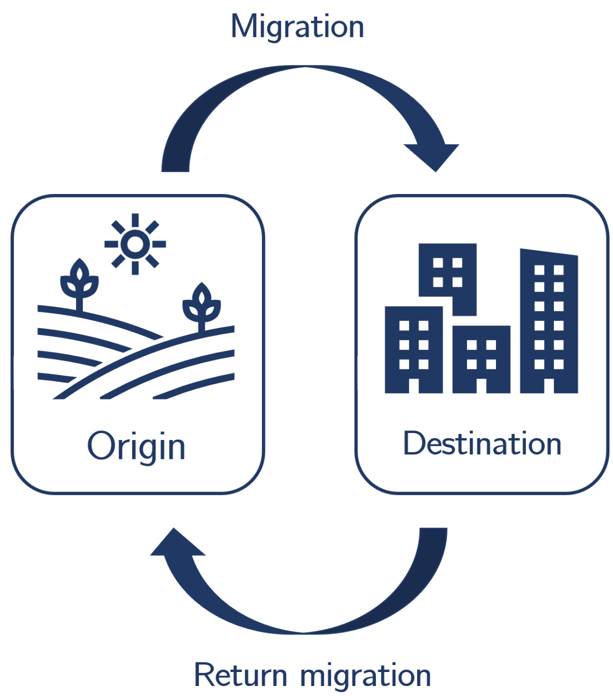


Figure 5.2: Migration and return Migration

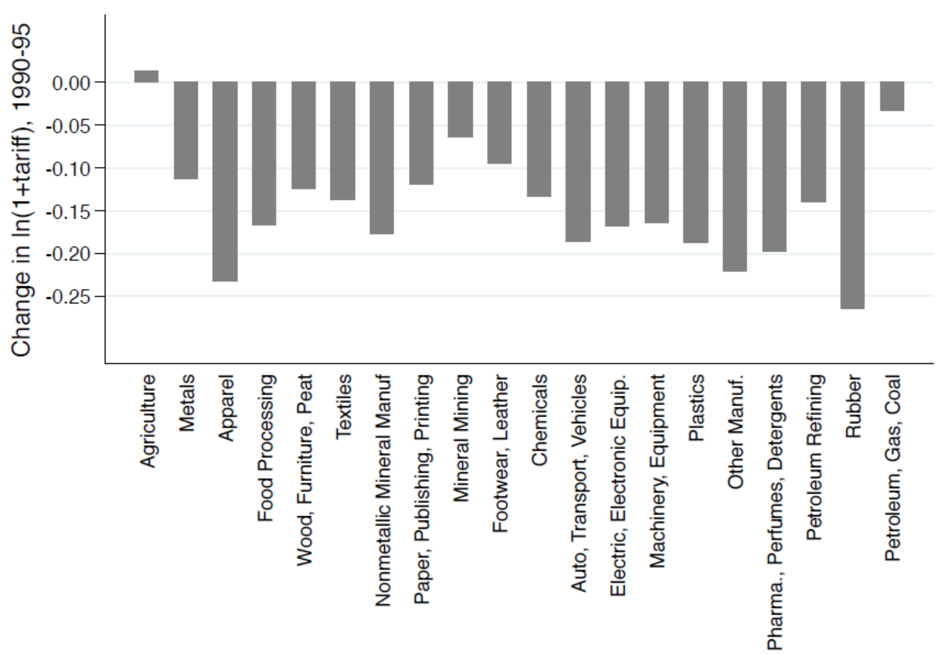


Figure 5.3: Changes in average nominal tariff by industry - 1990-1995.

Source: [60].

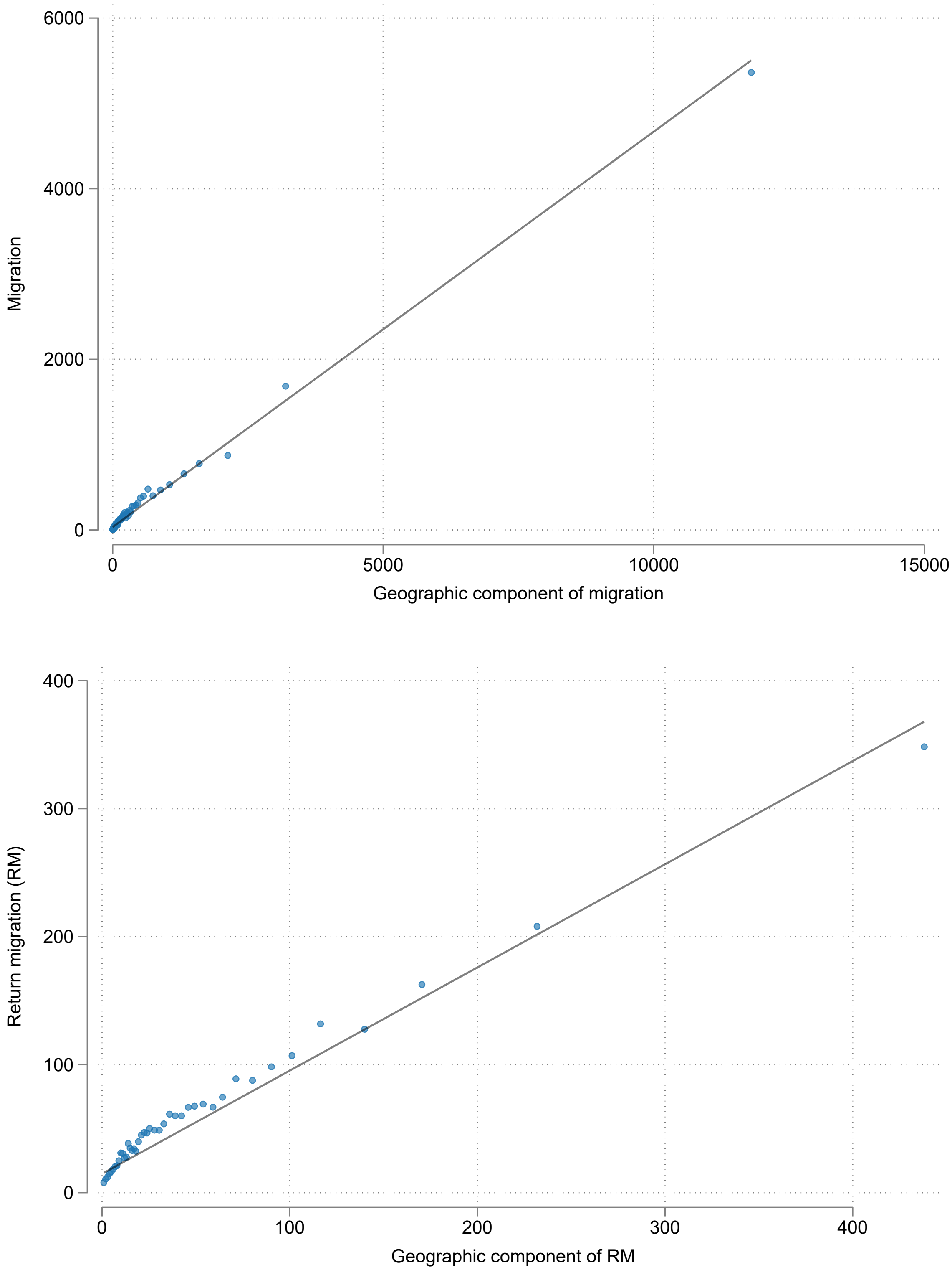


Figure 5.4: Geographic components of migration and return migration

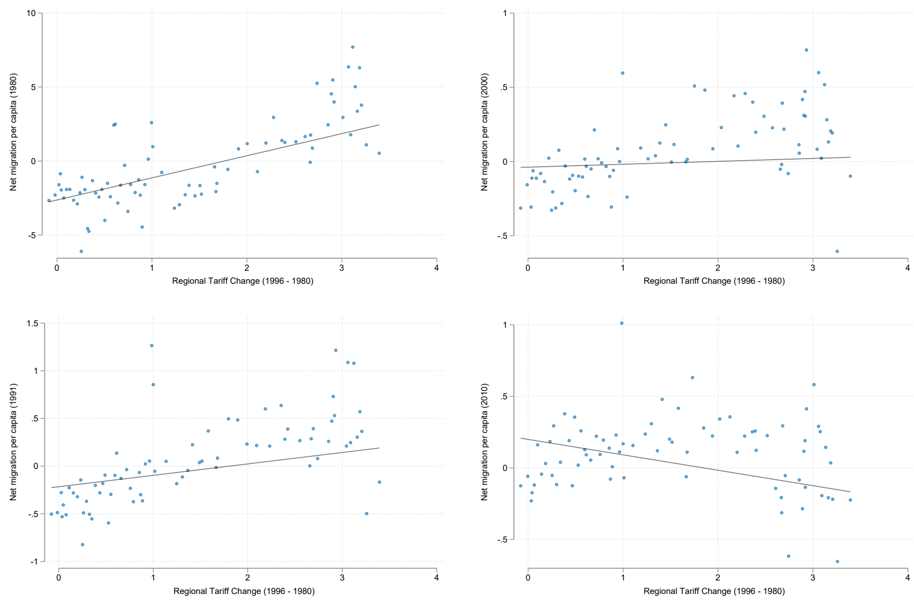


Figure 5.5: Impacts of RTC on net migration across time

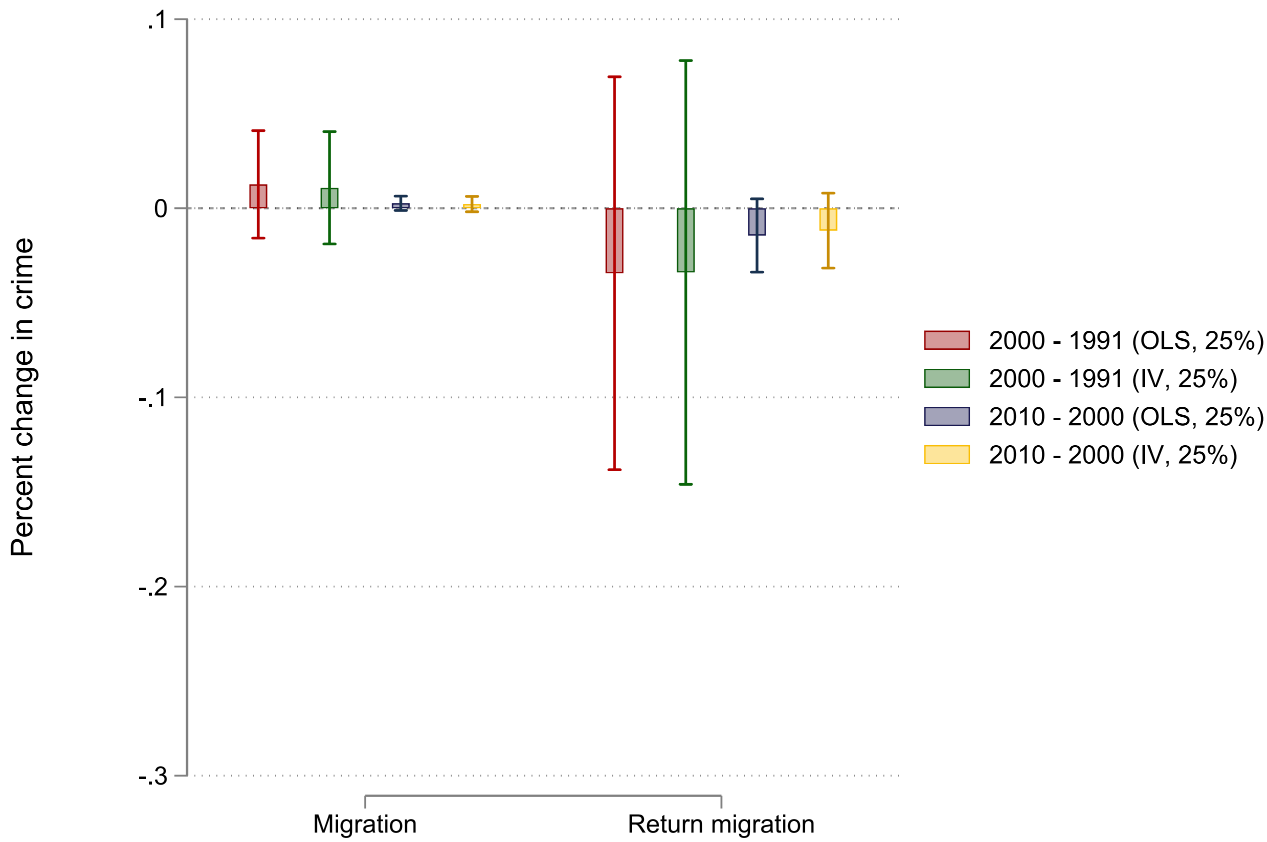


Figure 5.6: Impacts of migration and return migration from most affected areas on crime growth

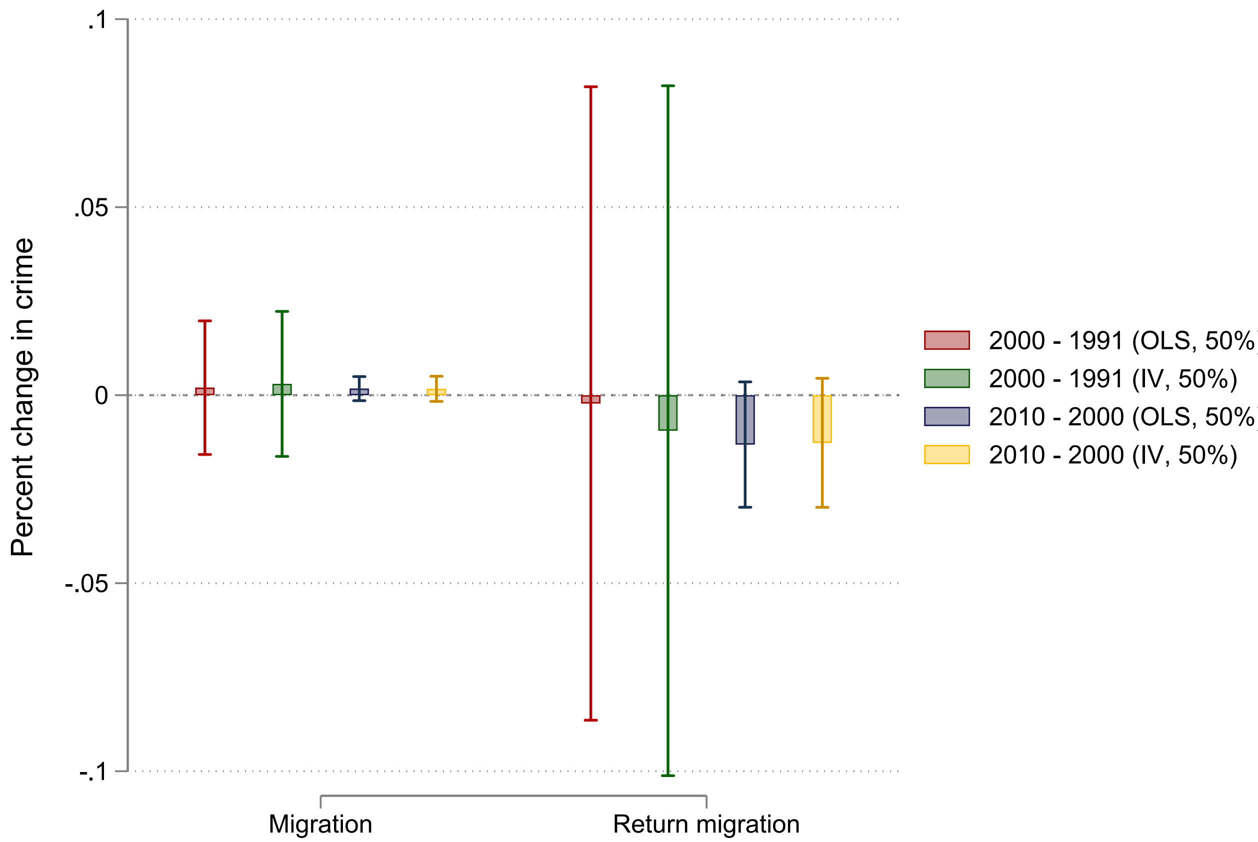
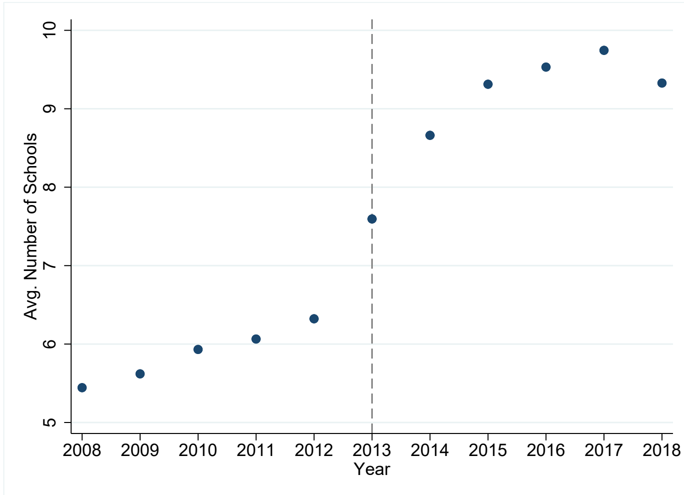


Figure 5.7: Impacts of migration and return migration from most affected (top 50% RTC) areas on crime growth

Appendix 2

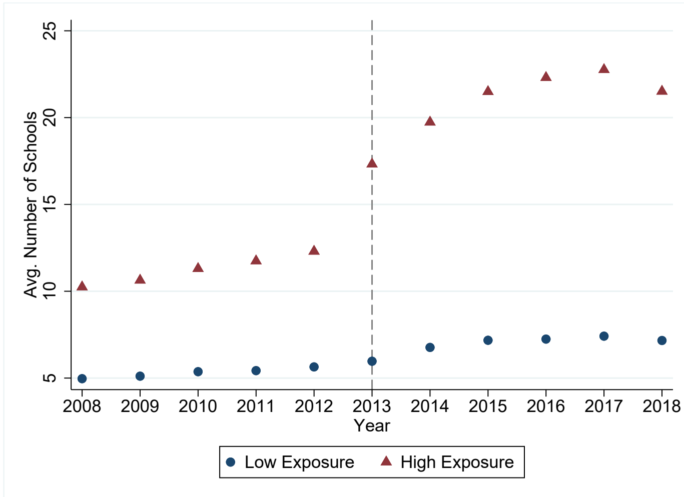


(a) Schools

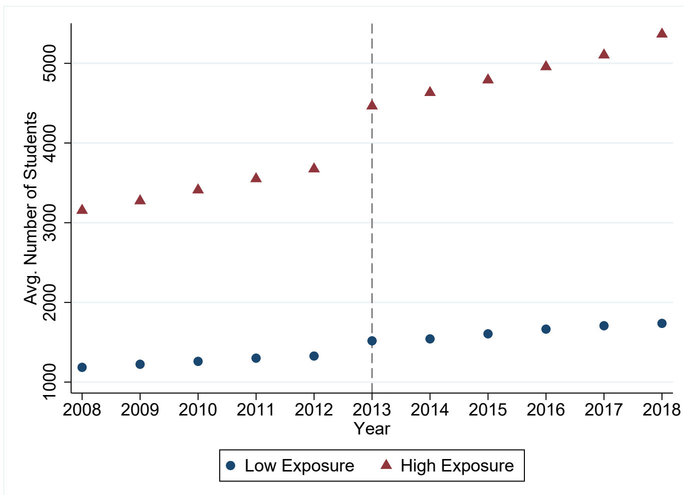
(b) Students

Figure 5.8: Municipalities' average number of schools and students

Notes: The vertical line indicates the beginning of the first affected academic year affected by the education reform (2013/14). Source: Own elaboration using information from Estadistica 911.



(a) Schools



(b) Students

Figure 5.9: Municipalities' average number of schools and students by treatment exposure

Notes: The vertical line indicates the beginning of the first academic year affected by the education reform(2013/14). *High exposure* municipalities are those with a growth rate in the number of high schools from the pre- to the post-treatment period in the top quartile of the growth distribution. Own elaboration using information from Estadistica 911.

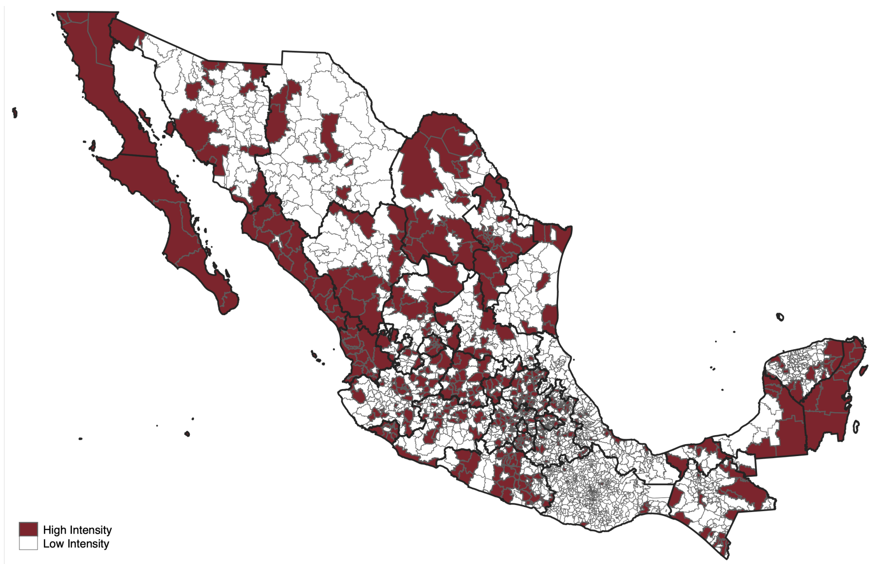
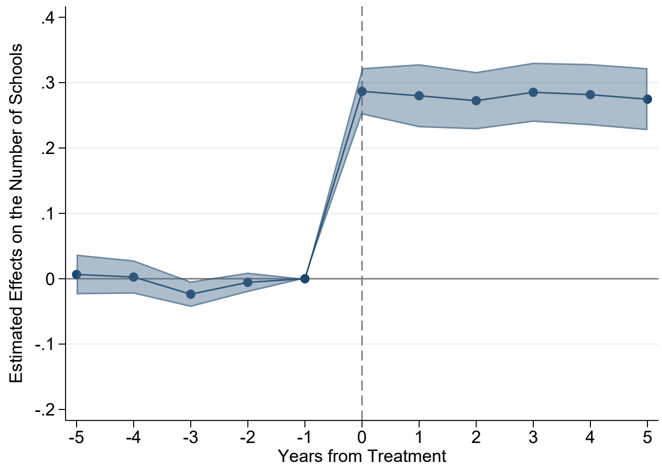
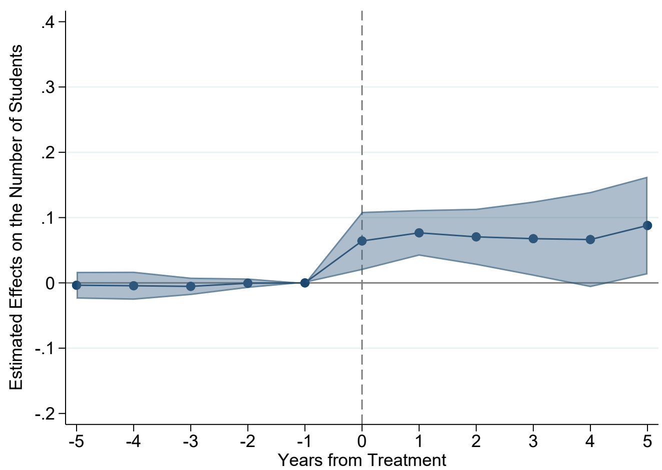


Figure 5.10: Municipalities by intensity of treatment to the educational reform

Notes: This map displays the municipalities in Mexico by their intensity of exposure to the educational reform, according to the definition in section 3.4.1. The map contains information for 2,456 municipalities. We categorize 517 as high-exposure and the remaining 1,625 as low-exposure municipalities. The remaining are excluded from the analyses because they do not have any schools during our analyzed period. Source: Own elaboration using information from Estadistica 911.



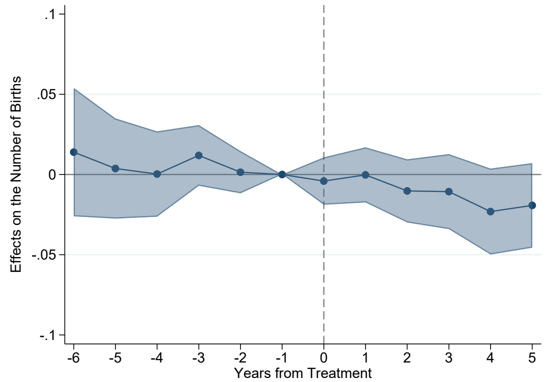
(a) Schools



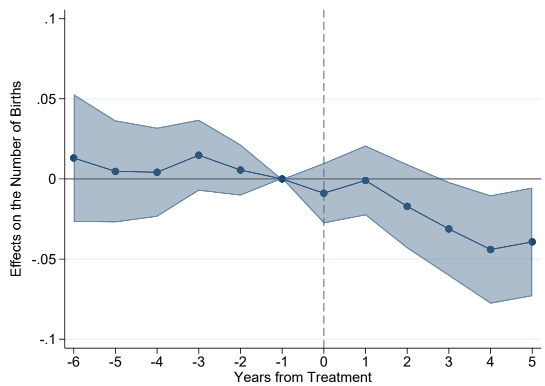
(b) Students

Figure 5.11: Estimated effects of the education reform on schools and students

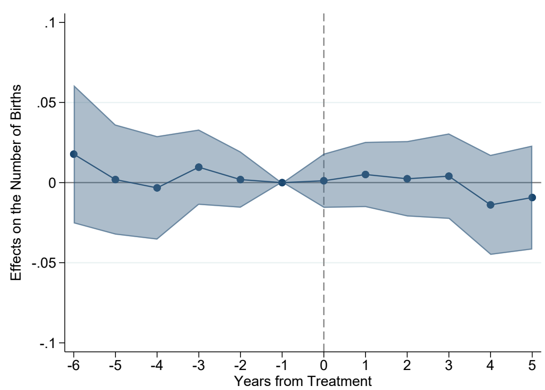
Notes: These estimates correspond to the δ_j. These are Pseudo-Maximum Likelihood estimations of a Poisson model (*ppmlhdfe*). All estimates in each panel come from a single specification that includes municipality of residence fixed effects, year fixed effects, and region-by-year fixed effects. Standard errors for confidence intervals are clustered at the municipality level.



(a) All women



(b) Ages 15-19



(c) Ages 20-24

Figure 5.12: Estimated effects of the education reform on births

Notes: The estimates correspond to β_j. The dependent variable is the births of women of the age group indicated in the subtitle. These are Pseudo-Maximum Likelihood estimations of Poisson models (*ppmlhdfe*). All estimates in each panel come from a single specification that includes municipality of residence fixed effects, year fixed effects, and region-by-year fixed effects. Standard errors for confidence intervals are clustered at the municipality level.

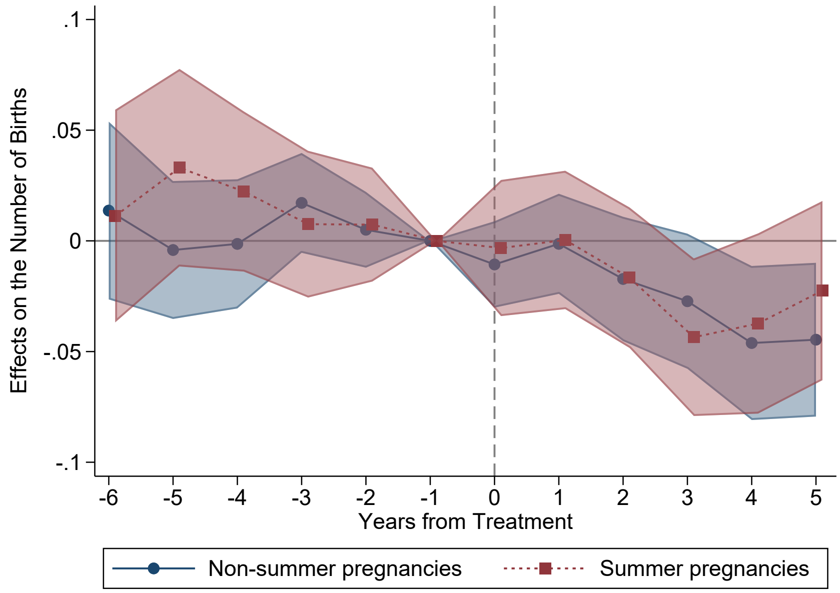


Figure 5.13: Estimated effects of the education reform on teenage births by approximate pregnancy period

Notes: The estimates correspond to β_j. The dependent variable is the births of women of the age group indicated in the subtitle. These are Pseudo-Maximum Likelihood estimations of Poisson models (*ppmlhdfe*). All estimates in each panel come from a single specification that includes municipality of residence fixed effects, year fixed effects, and region-by-year fixed effects. Summer pregnancies refer to births with an associated pregnancy during the months of June to August. Non-summer pregnancies refer to births with an associated pregnancy during the rest of the year. Standard errors for confidence intervals are clustered at the municipality level.

Table 5.1: Average number of high schools by municipality intensity of exposure to the education reform

	Low-exposure			**High-exposure**		
	2008-2012	2013-2018	2008-2018	2008-2012	2013-2018	2008-2018
High schools	5.303	6.955	6.204	11.244	20.853	16.485
	(14.560)	(18.376)	(16.769)	(19.511)	(33.243)	(28.259)
General high schools	4.295	5.612	5.013	8.641	15.151	12.192
	(11.708)	(13.775)	(12.893)	(14.963)	(23.386)	(20.261)
Technical high schools	1.008	1.343	1.191	2.603	5.702	4.293
	(4.001)	(5.306)	(4.760)	(5.613)	(11.617)	(9.503)
Morning shift high schools	3.630	4.494	4.101	7.333	11.643	9.684
	(9.730)	(11.435)	(10.702)	(12.955)	(18.957)	(16.639)
Other shifts high schools	1.673	2.462	2.103	3.911	9.210	6.801
	(5.087)	(7.308)	(6.407)	(7.396)	(15.421)	(12.709)
General morning shift high schools	2.949	3.613	3.311	5.711	8.415	7.186
	(8.036)	(9.002)	(8.583)	(10.262)	(13.950)	(12.482)
General other shift high schools	1.346	1.999	1.702	2.930	6.736	5.006
	(3.972)	(5.199)	(4.693)	(5.537)	(10.502)	(8.814)
Technical morning shift high schools	0.682	0.881	0.790	1.622	3.229	2.499
	(2.474)	(3.069)	(2.816)	(3.355)	(6.027)	(5.056)
Technical other shift high schools	0.327	0.462	0.401	0.981	2.474	1.795
	(1.601)	(2.477)	(2.125)	(2.441)	(6.076)	(4.837)

Notes: This table shows the averages of the number of schools across municipalities by intensity category and pre-post the education reform. See section 3.4.1 for information on the definition of municipalities' intensity of exposure to the education reform. Source: Own elaboration using information from Estadistica 911.

Table 5.2: Birth rates by mothers' age group

	Low-exposure			High-exposure		
	2008-2013	2014-2019	2008-2019	2008-2013	2014-2019	2008-2019
15-19	52.644	50.399	51.492	56.022	51.642	53.743
	(15.226)	(15.101)	(15.203)	(11.837)	(12.023)	(12.132)
20-24	46.103	43.747	44.895	47.543	44.201	45.804
	(13.082)	(11.568)	(12.385)	(9.530)	(8.638)	(9.228)
All women	22.546	21.119	21.814	23.509	21.449	22.437
	(5.897)	(5.121)	(5.559)	(4.342)	(4.020)	(4.302)

Notes: This table presents the average and standard deviation (in parenthesis) of births per 1,000 women by the municipality of residence's intensity of treatment to the education reform, pre (2008-2013) / post (2014-2018) reform. These averages only include first births to mothers. The averages are weighted by the 15-49-year-old female population in the municipality. Low-exposure municipalities are those whose change in the average number of high schools pre/post-reform is among the first three quartiles of the distribution across municipalities. High-exposure municipalities are those whose change in the number of high schools pre/post-reform is in the top quartile of the distribution across municipalities. See section 3.4.1 for more information on the definition of the intensity of exposure to the education reform. The births per 1,000 women for each age group were calculated using, as the denominator, the number of women in a municipality in the corresponding age group.

Table 5.3: Estimated effects of the education reform on births

	All women	**15-19**	**20-24**
High Exposure × *Post*	-0.015	-0.030**	-0.006
	(0.011)	(0.012)	(0.013)
Observations	29472	29472	29460

Notes: These coefficients correspond to the estimate of β from equation **??**. The dependent variable is the number of births of the corresponding age group indicated in the column. Each column represents a different regression. *High Exposure* is defined as having a growth rate in the number of high schools from the pre- to the post-treatment period in the top quartile of the growth distribution. See section 3.4.1 for more information on the definition of the intensity of exposure to the reform at the municipality level. *Post* is an indicator variable for the period 2013-2018, which corresponds to the period after the implementation of the education reform. The estimates additionally control for municipality, year, and region-by-year fixed effects (only column 3). The standard errors are clustered at the municipality level.
*, **, *** Significant at the 10%, 5%, and 1% levels, respectively.

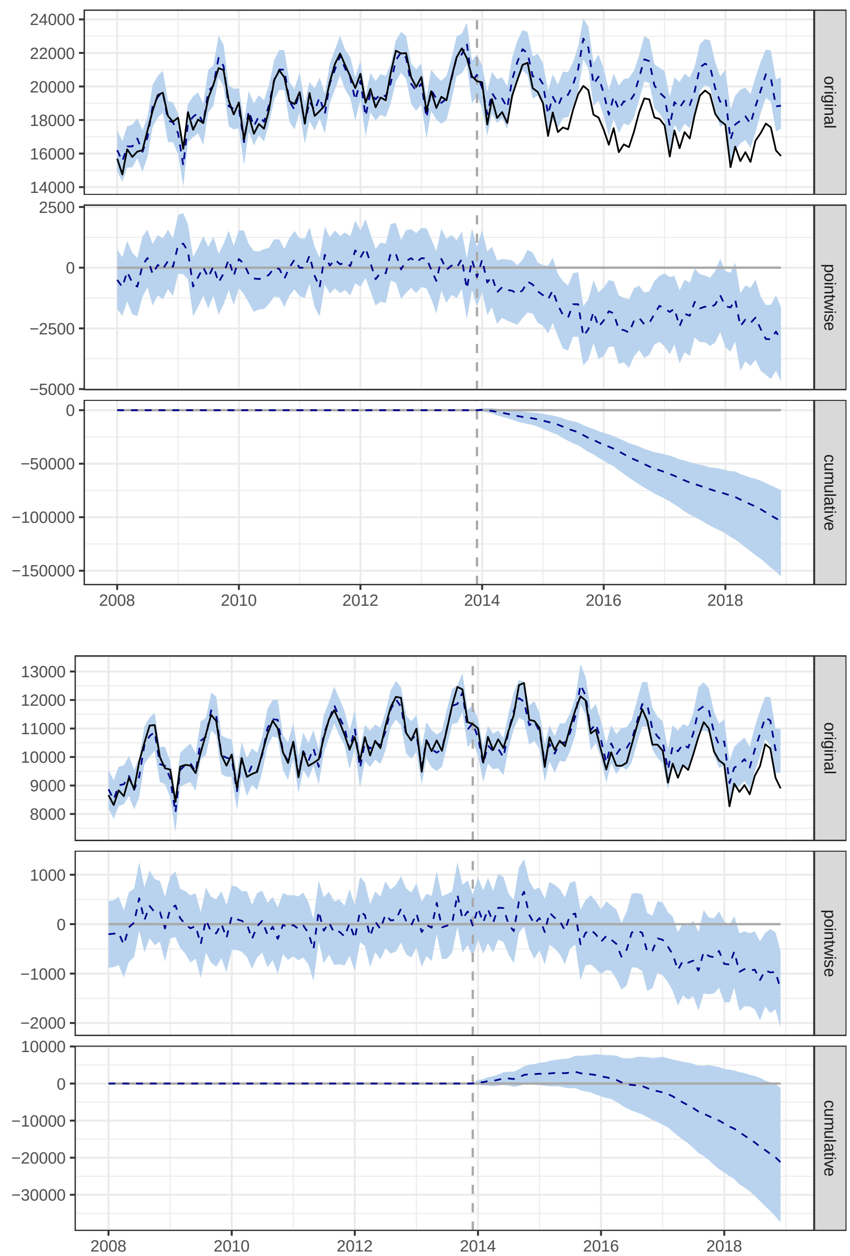
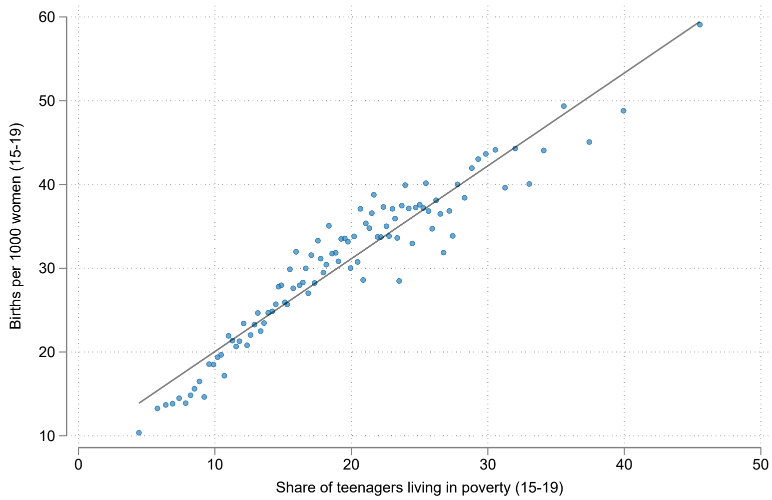


Figure 5.14: Bayesian structural time series method forecast for monthly births. 15-19-year-old and 20-24-year-old women

Notes: These figures correspond to the Bayesian Structural Time Series Method (BSTS). The analyses rely on monthly births and control variable time series. The horizontal axis represents the year, and the vertical axis represents births for each age group. The first graph presents the results for 15-19-year-old women, while the second graph presents the results for 20-24-year-old women The first graph (original) compares the original birth series vs. the predicted counterfactual (dashed line) and its corresponding 95 percent confidence interval. The second graph plots the estimated pointwise treatment effects and their corresponding 95 percent confidence interval. Finally, the third graph depicts the cumulative effect over the period.

Appendix 3



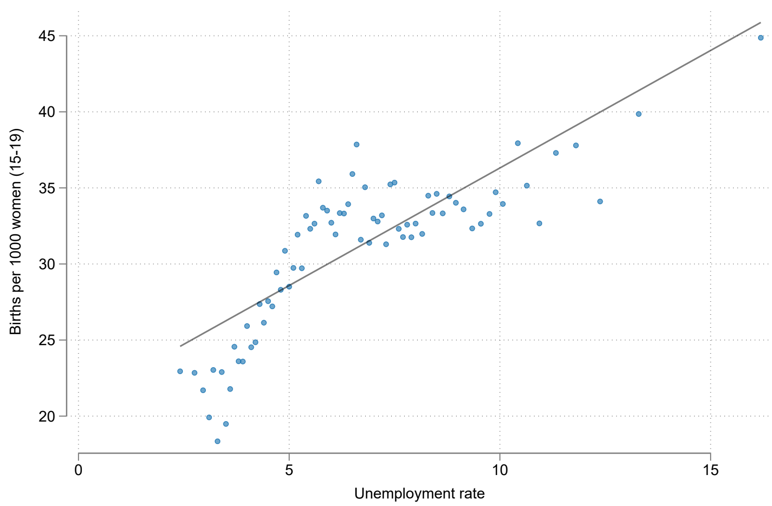


Figure 5.15: This figure presents the county-level relationship between teenage birth rates and the share of teenagers living in poverty from 2002 to 2019. Observations are individual counties at year t weighted by their population.

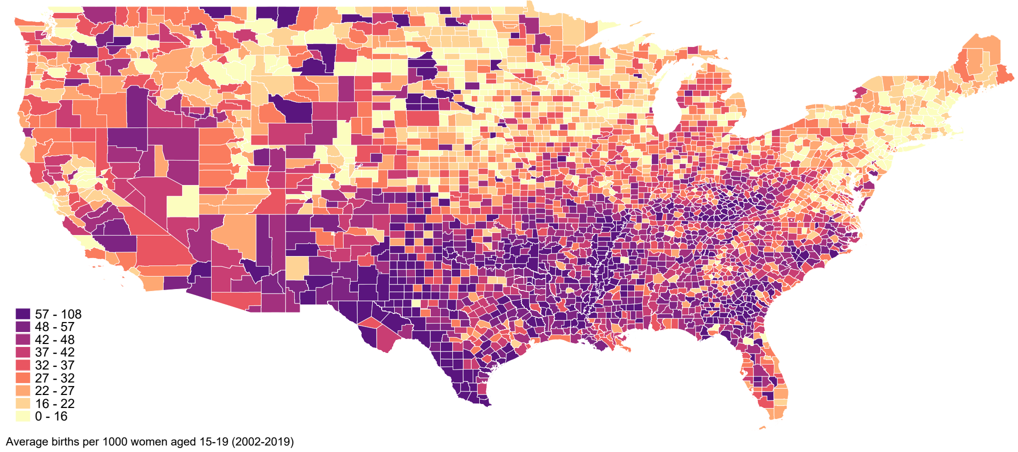


Figure 5.16: This figure presents the county-level relationship between teenage birth rates and unemployment rates from 2002 to 2019. Observations are individual counties at year t weighted by their population.

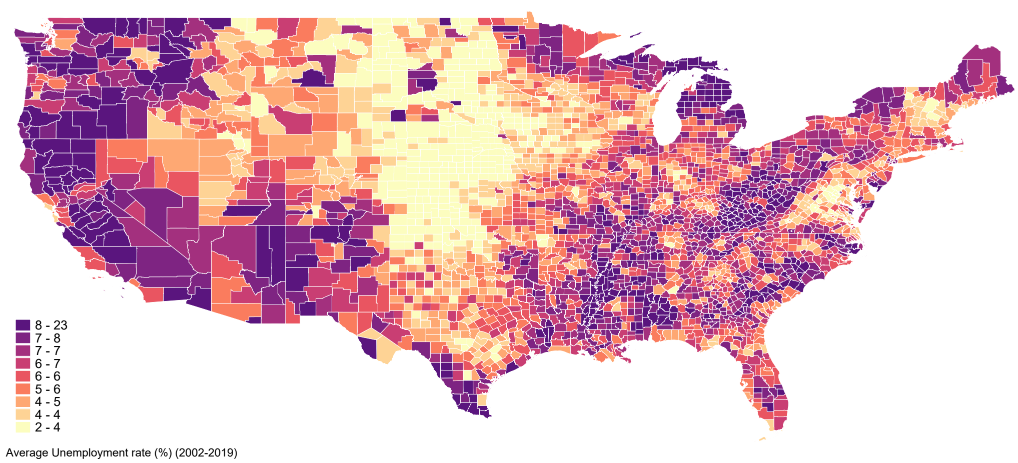


Figure 5.17: This map presents the average county-level teenage birth rates from 2002 to 2019.

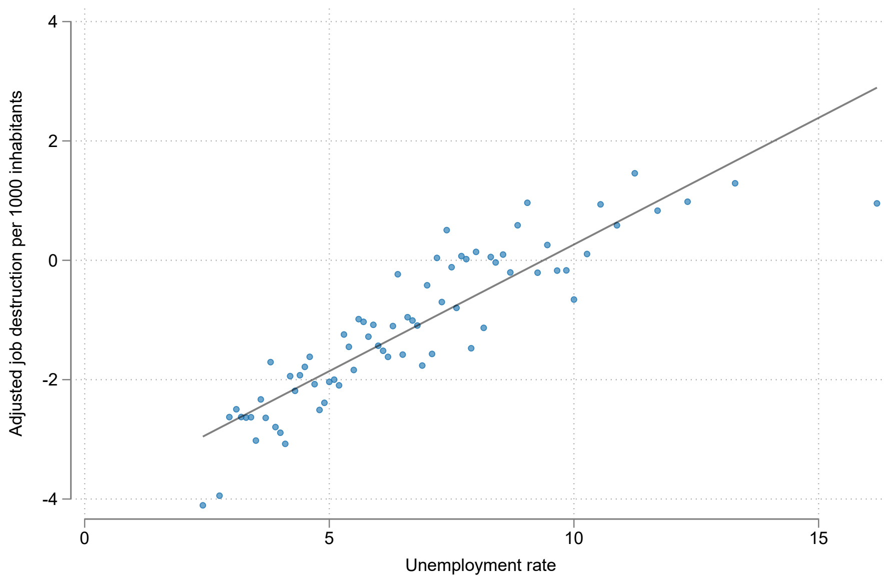


Figure 5.18: This map presents the average county-level unemployment rates from 2002 to 2019.

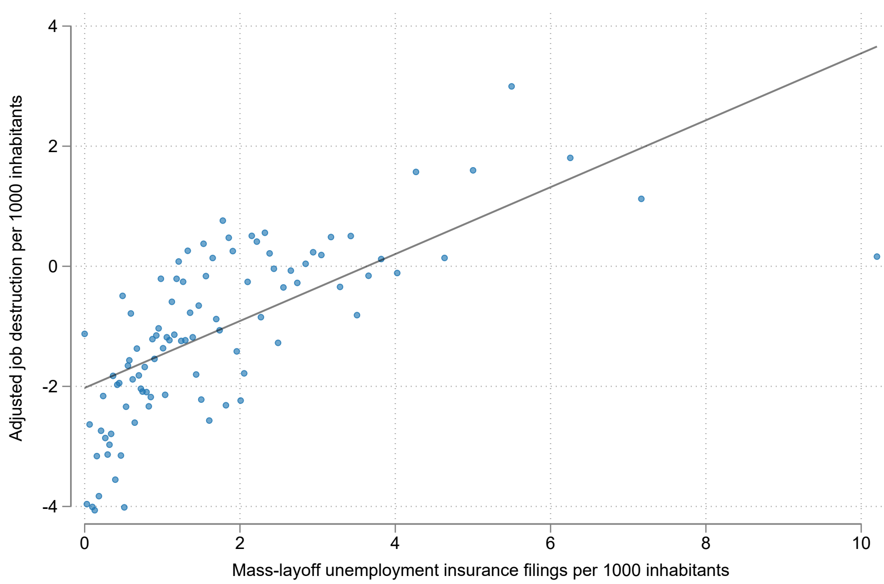


Figure 5.19: This figure presents the county-level relationship between adjusted job destruction and unemployment rates from 2002 to 2019. Observations are individual counties at year t weighted by their population.

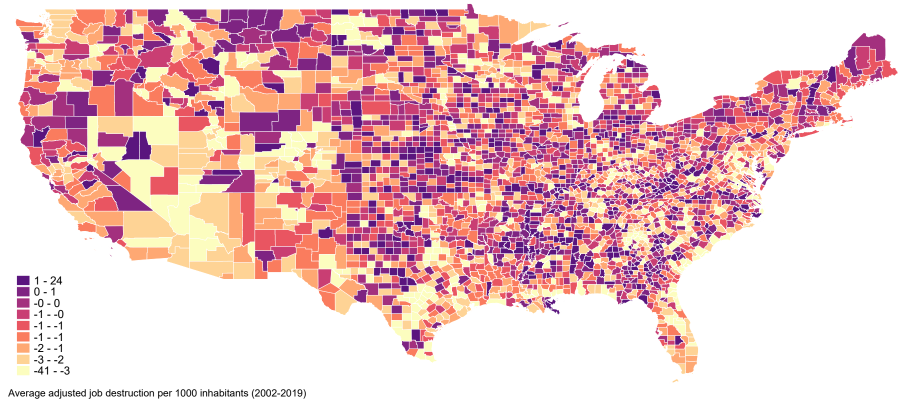


Figure 5.20: This figure presents the county-level relationship between adjusted job destruction and mass layoff-related unemployment insurance filings from 2002 to 2012. Observations are individual counties at year t weighted by their population.

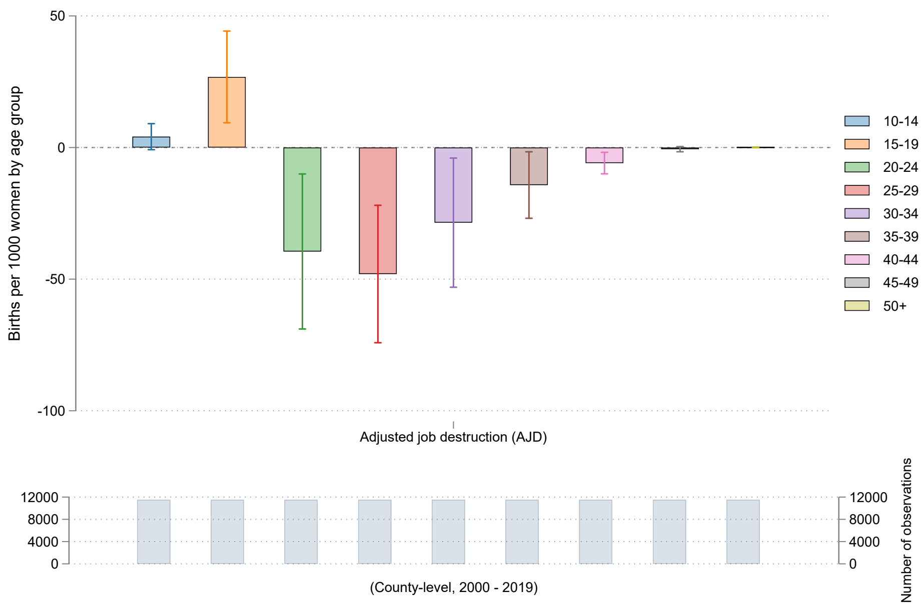


Figure 5.21: This map presents the average county-level per-capita adjusted job destruction (AJD) from 2002 to 2019.

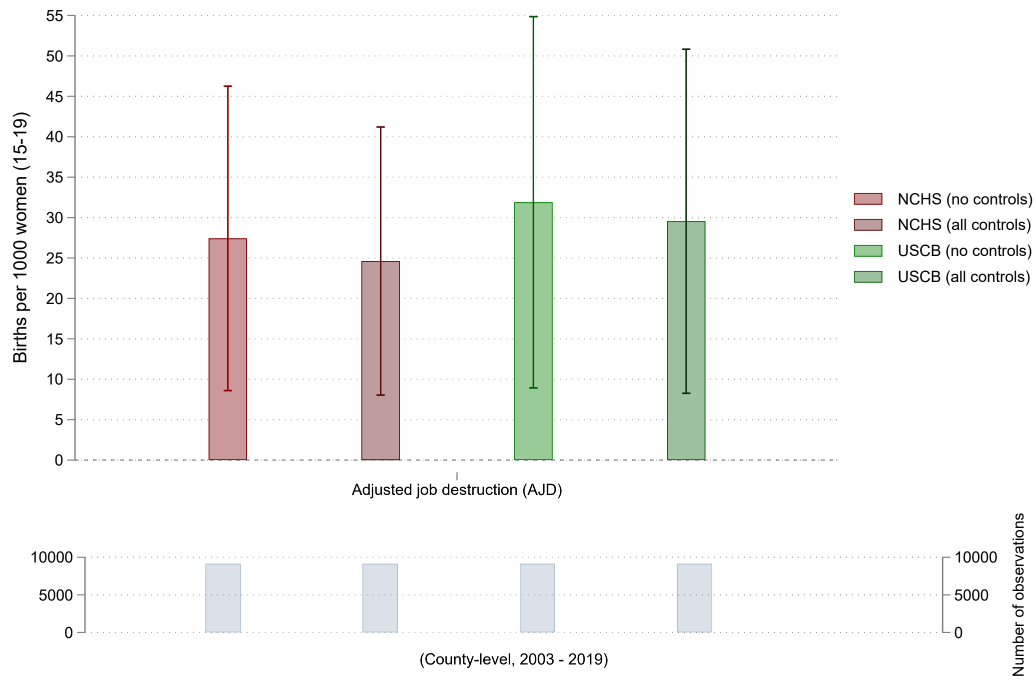


Figure 5.22: This figure presents coefficient plots for the impact of adjusted job destruction (AJD) on birth rates by age from 2002 to 2019. Cofficients were obtained from a fixed-effects model that examines the birth rate by age group and adjusted job destruction (AJD) per 1000 inhabitants. The observations are at the county-by-year level. We include demographic control variables, such as share of hispanic population, share of non-white population and the lagged share of teenagers living below the poverty line. County and state-by-year fixed effects are included to account for time and location-specific factors. Our treatment variable is AJD, i.e. the difference between jobs lost due establishment closures and jobs created due to establishment openings per capita. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time. In this fertility dataset, counties with less than 100,000 inhabitants in a state are aggregated. Thus, we aggregate other variables for such counties accordingly.

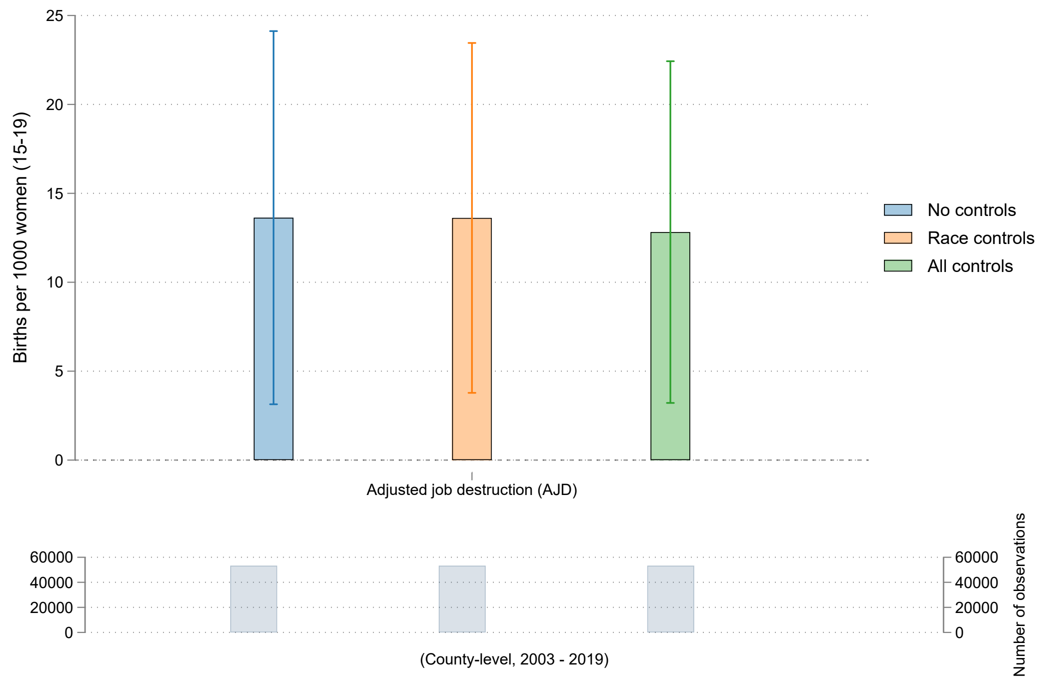


Figure 5.23: This figure presents coefficient plots of fixed-effects models that examines the effect of Adjusted Job Destruction on different teenage birth rate estimates. We compare the results using two teenage birth rates: One is calculated using NCHS Hierarchical Bayesian population estimates and other using USCB population estimates. Because the second dataset omitted counties with less than 100,000 inhabitants, we keep only counties that are present in both datasets. The observations are at the county-by-year level. We include demographic control variables, such as share of hispanic population, share of non-white population and the lagged share of teenagers living below the poverty line. County and state-by-year fixed effects are included to account for time and location-specific factors. Our treatment variable is the AJD (adjusted job destruction), i.e. the difference between jobs lost due establishment closures and jobs created due to establishment openings per capita. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time.

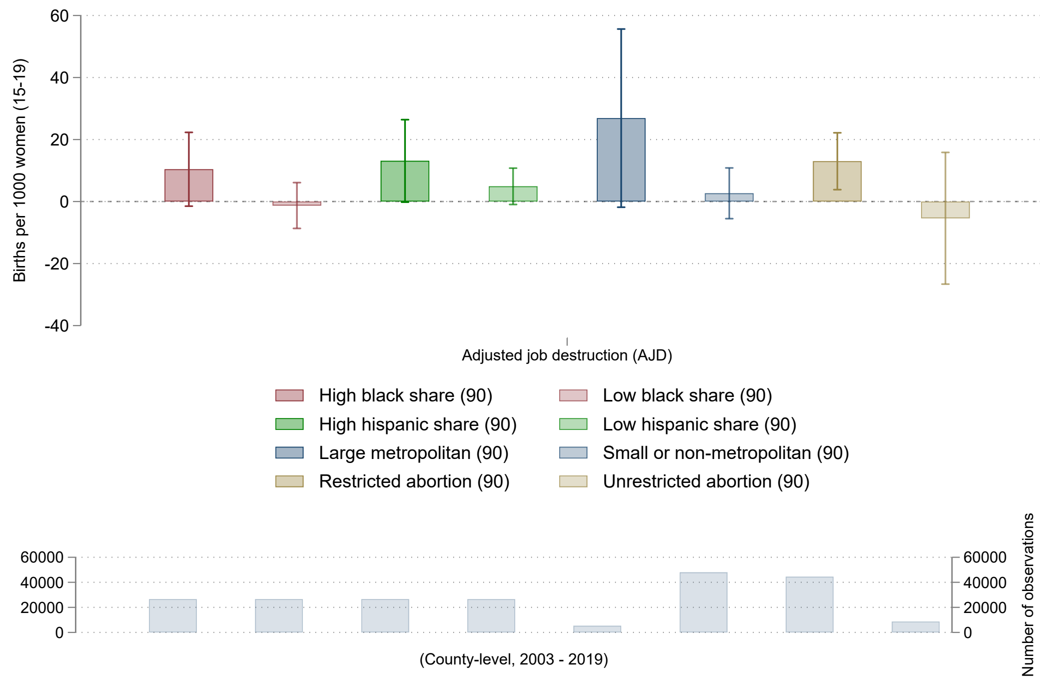


Figure 5.24: This figure presents coefficient plots for fixed-effects model that examines teenage birth rate (15-19) as estimated by the CDC. The observations are at the county-by-year level. We include demographic control variables, such as share of hispanic population, share of non-white population, urban area and the lagged share of teenagers living below the poverty line. County and state-by-year fixed effects are included to account for time and location-specific factors. Our treatment variable is the adjusted job destruction (AJD), i.e. the difference between jobs lost due establishment closures and jobs created due to establishment openings per capita. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time. Observations are weighted by the total population in the county. Our analysis covers all counties and states for the period from 2002 to 2019.

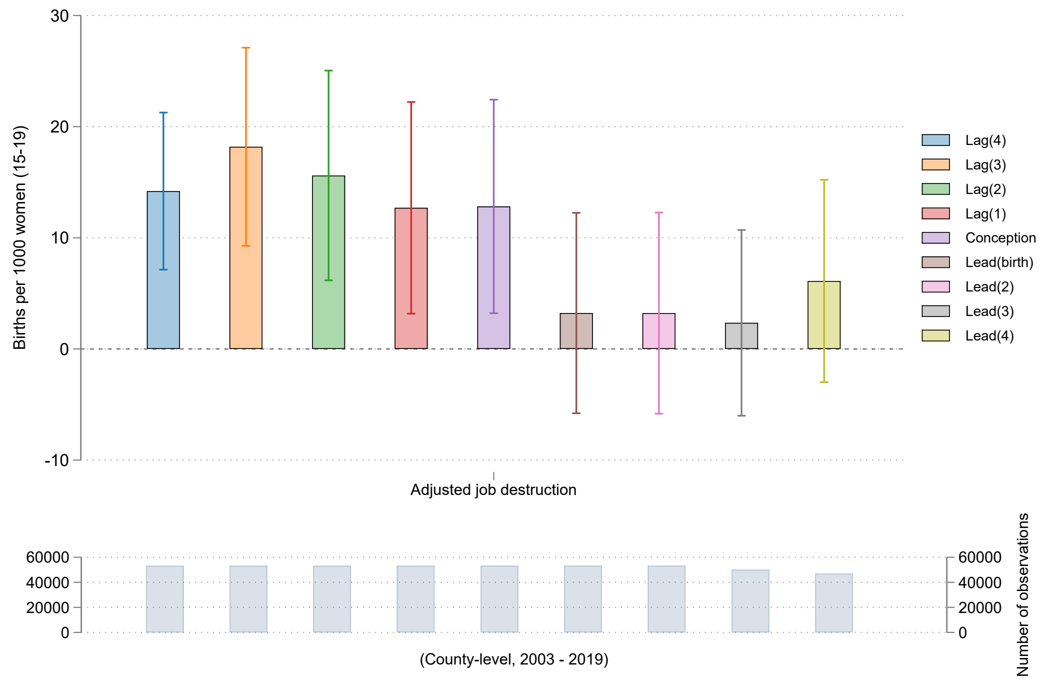


Figure 5.25: This figure presents coefficient plots for fixed-effects models that examine teenage birth rate (15-19) by year and county as estimated by the CDC. These models explore heterogeneities by splitting samples based on historical county characteristics (1990). Specifically, we estimate models separately for counties with above and below median shares of Hispanic and Black populations, large metropolitan areas and abortion restrictions as of 1990. To assess whether a county is located in a state with restrictive abortion regulations, we use the index proposed by Myers (2020) regarding parental involvement laws. We consider counties in states with an index greater than 0.6 to be restrictive with regards to abortion. The observations are at the county-by-year level. We include demographic control variables, such as share of hispanic population, share of non-white population, urban area and the lagged share of teenagers living below the poverty line. County and state-by-year fixed effects are included to account for time and location-specific factors. Our treatment variable is the adjusted job destruction, i.e. the difference between jobs lost due establishment closures and jobs created due to establishment openings. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time. Observations are weighted by the total population in the county. Our analysis covers all counties and states for the period from 2002 to 2019.

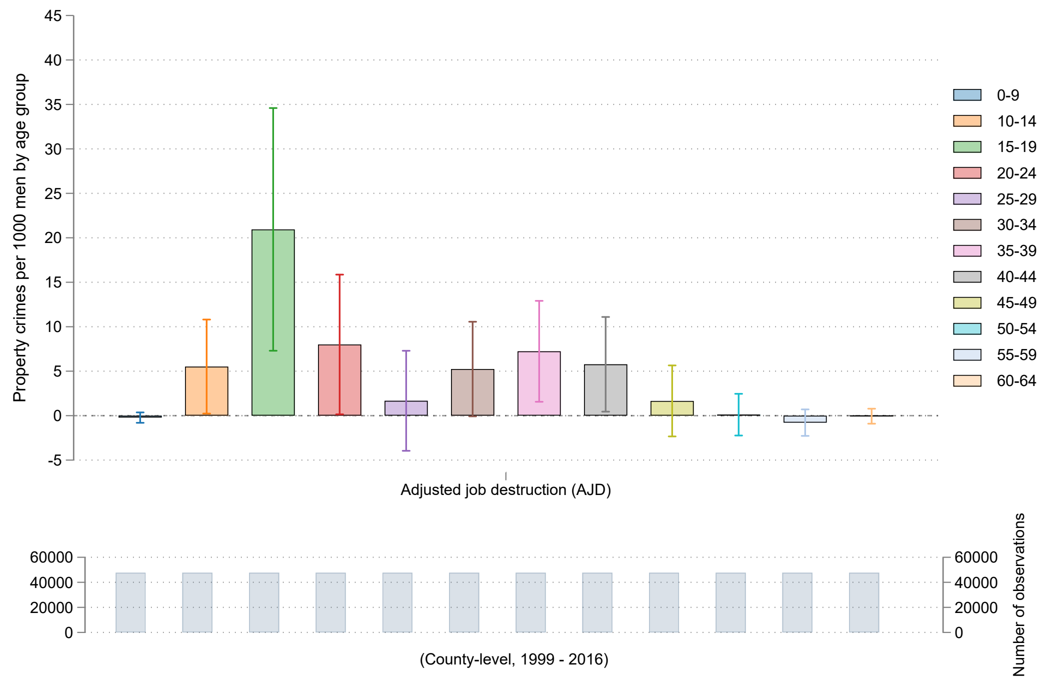


Figure 5.26: This figure presents coefficient plots of fixed-effects models that examines teenage birth rate (15-19) by year and county as estimated by the CDC. We change the treatment variable, adjusted job destruction (AJD) to its lags and leads for each specification. That is, we regress teenage fertility at time t on AJD at time t (i.e. birth year), time $t-1$ (i.e. conception year) and so on. The observations are at the county-by-year level. We include demographic control variables, such as share of hispanic population, share of non-white population, urban area and the lagged share of teenagers living below the poverty line. County and state-by-year fixed effects are included to account for time and location-specific factors. Our treatment variable is the net job destruction, i.e. the difference between jobs lost due establishment closures and jobs created due to establishment openings. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time. Observations are weighted by the total population in the county. Our analysis covers all counties and states for the period from 2002 to 2019.

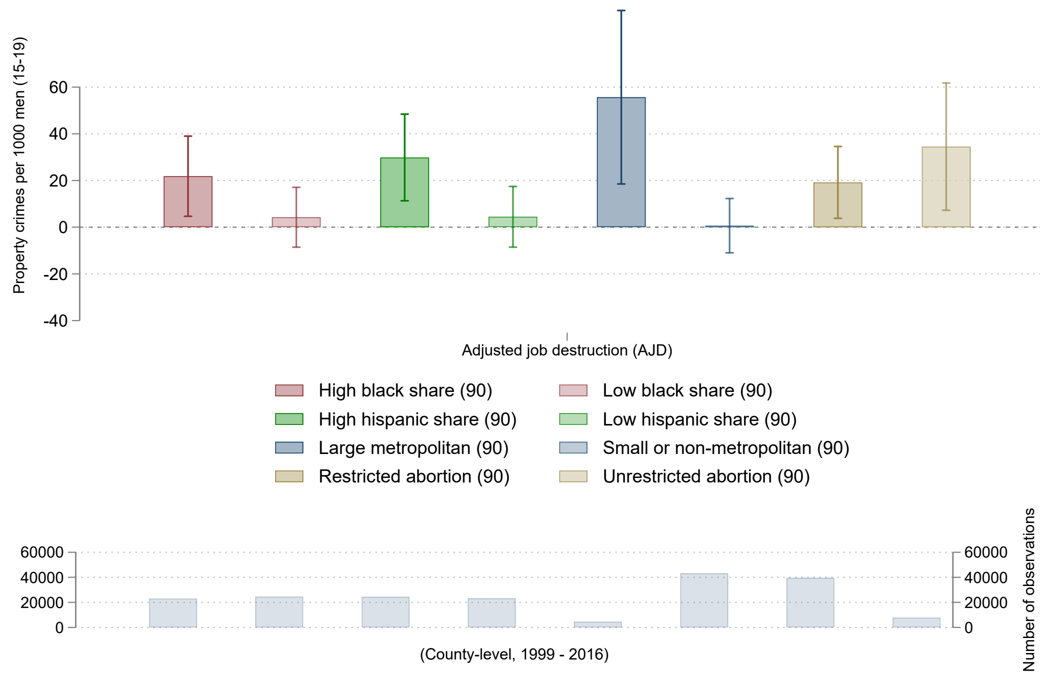


Figure 5.27: This figure presents coefficient plots for fixed-effects models that examine the effect of adjusted job destruction (AJD) on property crime rate by age group by year and county, as reported by the UCR. We include demographic control variables, such as share of hispanic population, share of non-white population, urban areas and the lagged share of teenagers living below the poverty line. County and state-by-year fixed effects are included to account for time and location-specific factors. Our treatment variable is the adjusted job destruction, i.e. the difference between jobs lost due establishment closures and jobs created due to establishment openings per capita. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time. Observations are weighted by the total population in the county. Observations are weighted by the total population in the county. Our analysis covers all counties and states for the period from 1999 to 2016.

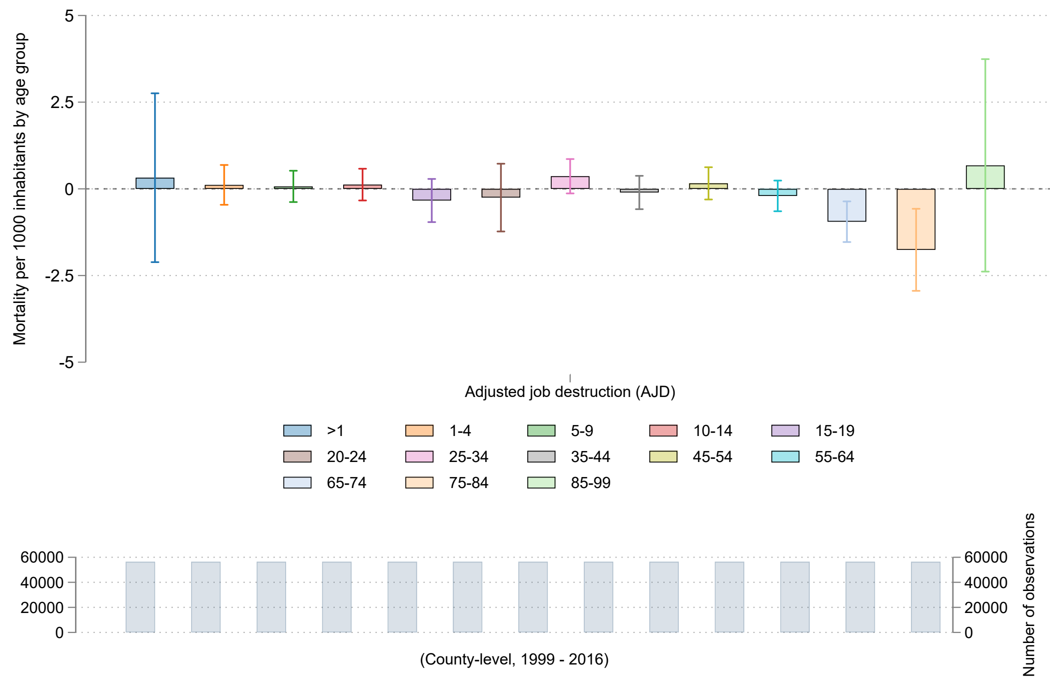


Figure 5.28: This figure presents coefficient plots for fixed-effects models that examine teenage property crime rate (15-19) by year and county as estimated by the UCR. These models explore heterogeneities by splitting samples based on historical county characteristics (1990). Specifically, we estimate models separately for counties with above and below median shares of Hispanic and Black populations, large metropolitan areas and abortion restrictions as of 1990. To assess whether a county is located in a state with restrictive abortion regulations, we use the index proposed by Myers (2020) regarding parental involvement laws. We consider counties in states with an index greater than 0.6 to be restrictive with regards to abortion. The observations are at the county-by-year level. We include demographic control variables, such as share of hispanic population, share of non-white population, urban area and the lagged share of teenagers living below the poverty line. County and state-by-year fixed effects are included to account for time and location-specific factors. Our treatment variable is the adjusted job destruction, i.e. the difference between jobs lost due establishment closures and jobs created due to establishment openings. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time. Observations are weighted by the total population in the county. Our analysis covers all counties and states for the period from 1996 to 2016.

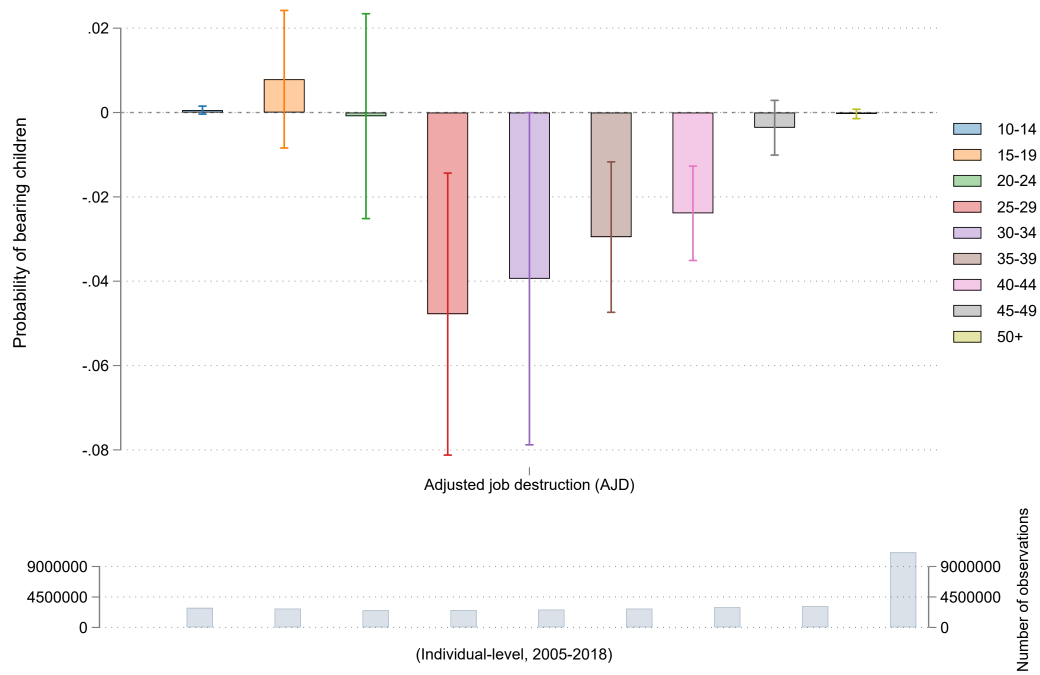


Figure 5.29: This figure presents coefficient plots for fixed-effects models that examine mortality rate by selected external causes by age group and adjusted job destruction AJD. Observations are at the county-by-year level. We include demographic control variables, such as share of hispanic population, share of non-white population, urban areas and the lagged share of teenagers living below the poverty line. County and state-by-year fixed effects are included to account for time and location-specific factors. Our treatment variable is the adjusted job destruction (AJD), i.e. the difference between jobs lost due establishment closures and jobs created due to establishment openings. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time. Mortality by selected external causes comprises all deaths by external causes excluding medical error, airplane crashes, natural disasters and war. Our analysis covers all counties and states for the period from 2002 to 2019.

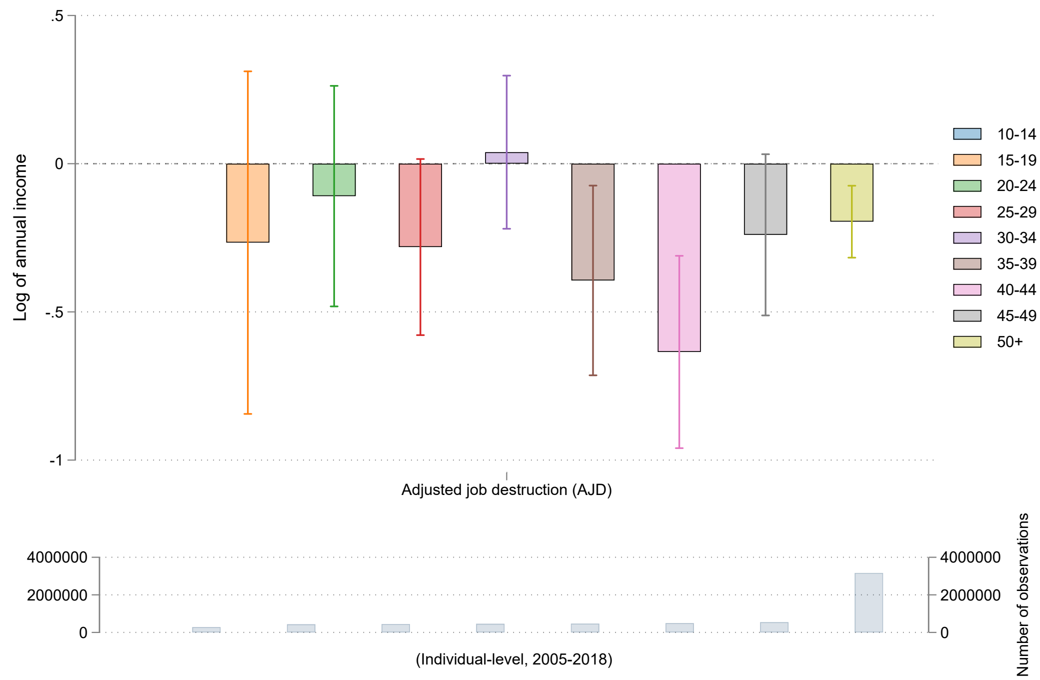


Figure 5.30: This figure presents coefficient plots for linear probability models of childbearing. Each observation corresponds to an individual at time t. We also observe in which PUMA and state the individual resides. The treatment variable is the exposure to adjusted job destruction, i.e. the AJD at the the PUMA of residence at time $t-1$. We split the sample into the age groups used by the CDC. We control for individuals age, race and ethnicity. We also include PUMA fixed-effects, state-by-year fixed effects and survey year fixed effects. To obtain this data, we use ACS information on cohabitating parents and children age and expand the data so that we have individual-by-year data with an indicator whether the individual had a child that year. That is, we leverage the age difference between parents and their children to know when they became parents. We cluster the data at the state-by-year level and we use ACS/Census person weights.

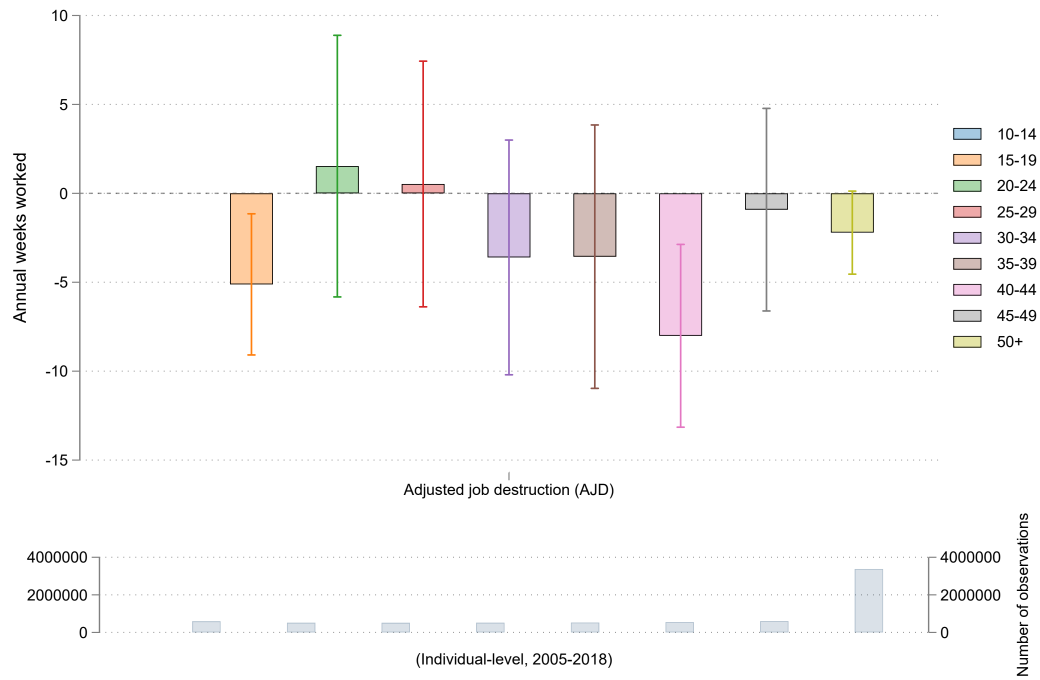


Figure 5.31: This figure presents coefficient plots for models of log income at the individual-level by age group sub-samples. The data includes in which PUMA and state individuals reside. The treatment variable is the exposure to adjusted job destruction, i.e. the AJD at the the PUMA of residence at time $t-1$. We split the sample into the age groups used by the CDC. We control for individuals age, race and ethnicity. We also include PUMA fixed-effects, state-by-year fixed effects. We cluster the data at the state-by-year level and we use ACS/Census person weights.

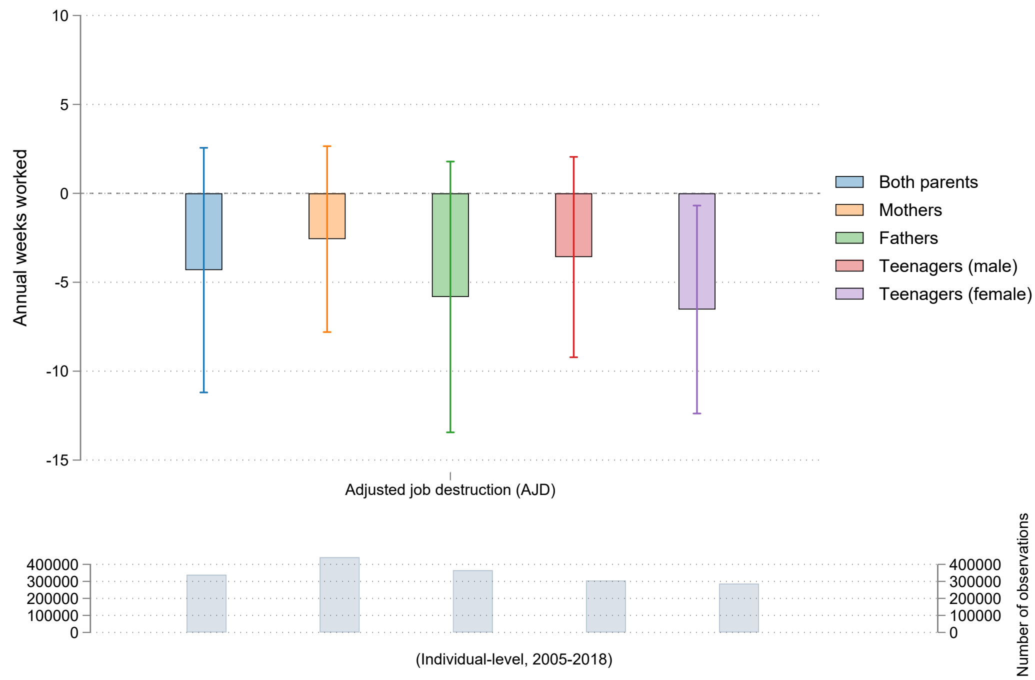


Figure 5.32: This figure presents coefficient plots for models of weeks worked at the individual-level by age group sub-samples. The data includes in which PUMA and state individuals reside. The treatment variable is the exposure to adjusted job destruction, i.e. the AJD at the the PUMA of residence at time $t-1$. We split the sample into the age groups used by the CDC. We control for individuals age, race and ethnicity. We also include PUMA fixed-effects, state-by-year fixed effects. We cluster the data at the state-by-year level and we use ACS/Census person weights.

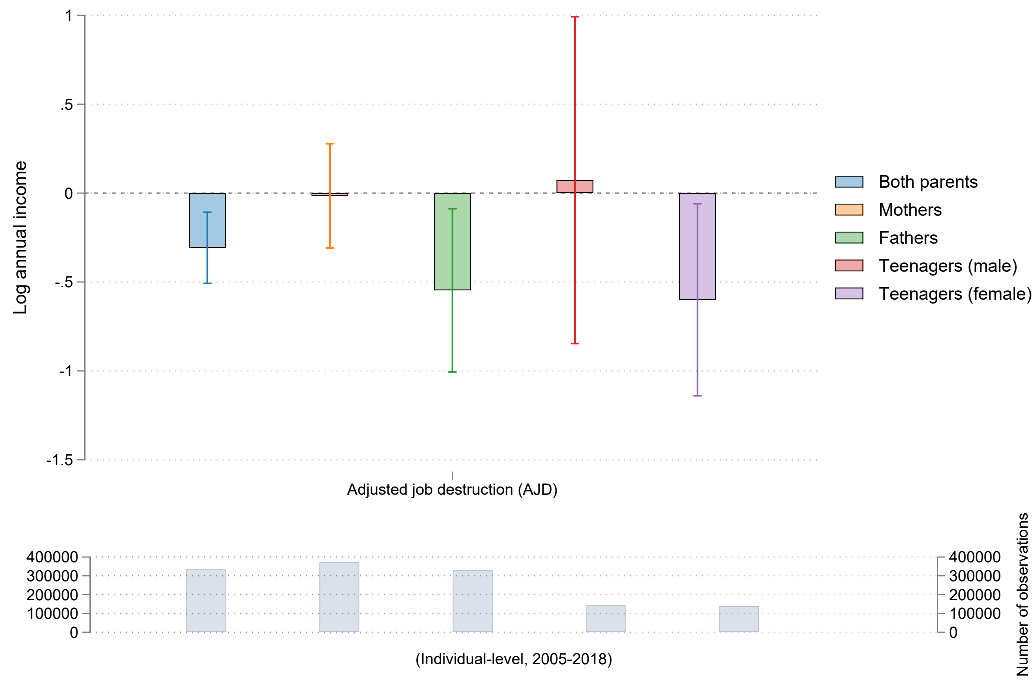


Figure 5.33: This figure presents coefficient plots for models of weeks worked at the individual-level by teenagers' family member. The data includes in which PUMA and state individuals reside. The treatment variable is the exposure to adjusted job destruction, i.e. the AJD at the the PUMA of residence at time $t-1$. We split the sample into the age groups used by the CDC. We control for individuals age, race and ethnicity. We also include PUMA fixed-effects, state-by-year fixed effects. We cluster the data at the state-by-year level and we use ACS/Census person weights.

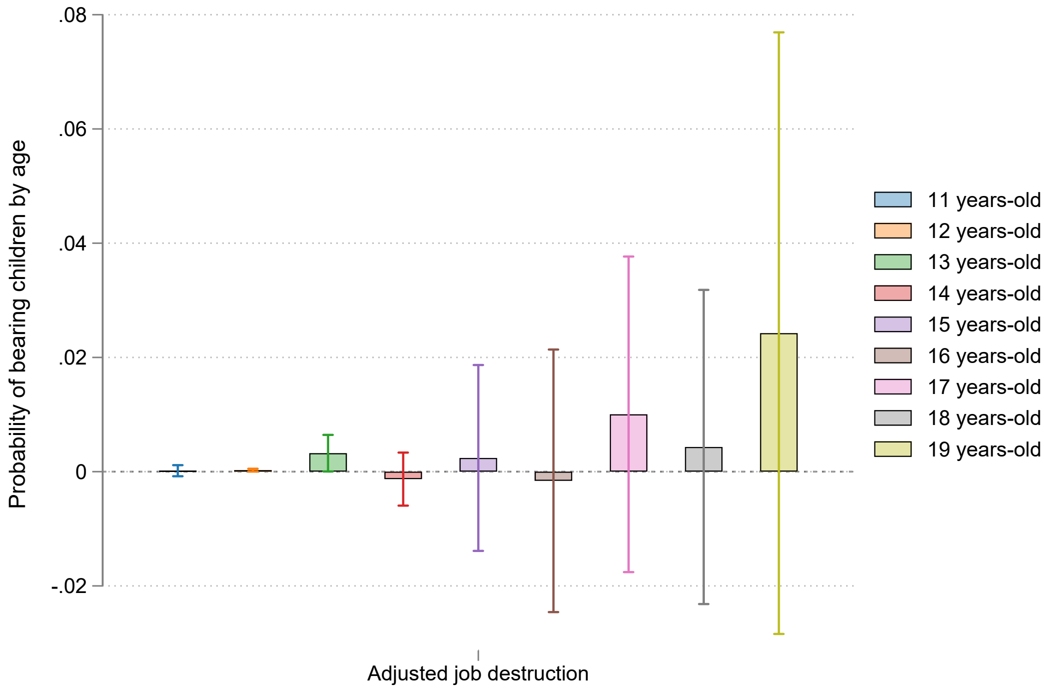


Figure 5.34: This figure presents coefficient plots for models of log income at the individual-level by teenagers' family member. The data includes in which PUMA and state individuals reside. The treatment variable is the exposure to adjusted job destruction, i.e. the AJD at the the PUMA of residence at time $t-1$. We split the sample into the age groups used by the CDC. We control for individuals age, race and ethnicity. We also include PUMA fixed-effects, state-by-year fixed effects. We cluster the data at the state-by-year level and we use ACS/Census person weights.

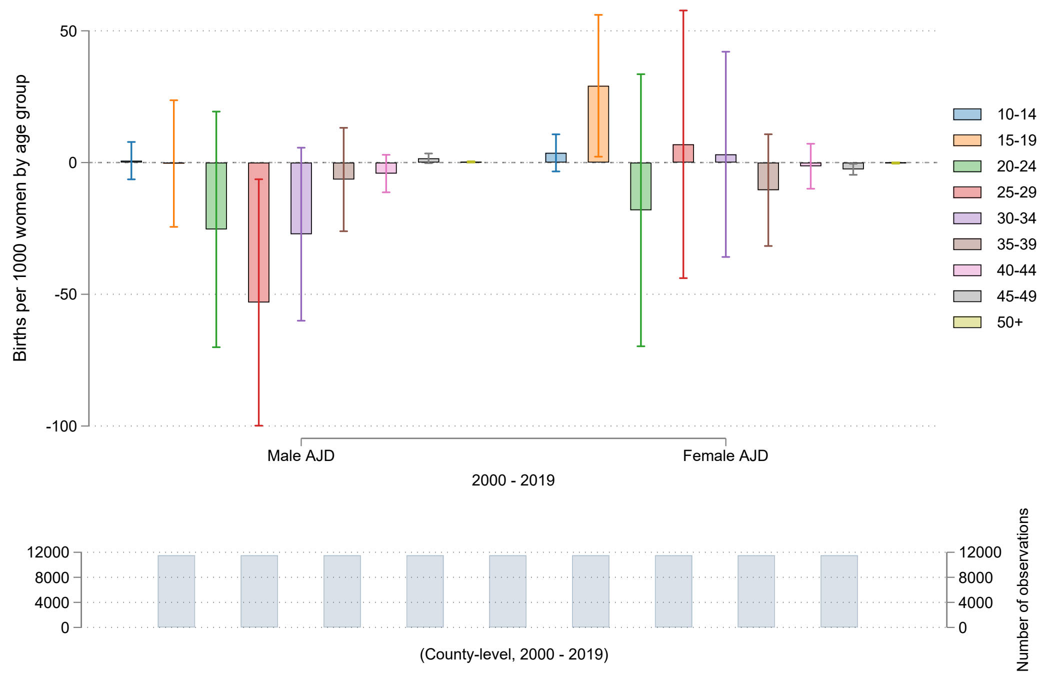


Figure 5.35: This figure presents coefficient plots for models of log income at the individual-level by teenager age sub-samples. The data includes in which PUMA and state teenagers reside. Here, the treatment variable is the exposure adjusted job destruction, i.e. the AJD at the the PUMA of residence at time $t-1$. We split the sample into the age groups used by the CDC. We control for individuals age, race and ethnicity. We also include PUMA fixed-effects, state-by-year fixed effects. To obtain this data, we use ACS information on cohabitating parents and children age and expand the data so that we have individual-by-year data with an indicator whether the individual had a child that year. That is, we leverage the age difference between parents and their children to know when they became a teenage parent. We cluster the data at the state-by-year level and we use ACS/Census person weights.

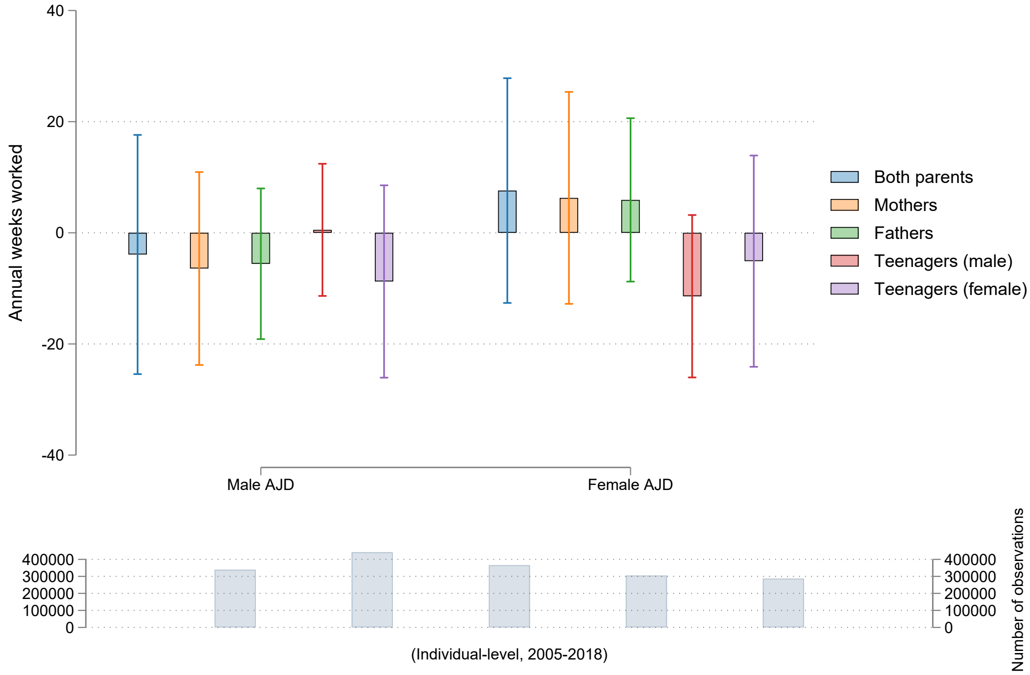


Figure 5.36: This figure presents coefficient plots for linear probability models of childbearing. Each observation corresponds to an individual at time t. We also observe in which PUMA and state the individual resides. The treatment variables are the exposure to gender-specific adjusted job destruction, i.e. the gender-specific AJD at the the PUMA of residence at time $t-1$. We split the sample into the age groups used by the CDC. We control for individuals age, race and ethnicity. We also include PUMA fixed-effects, state-by-year fixed effects and survey year fixed effects. To obtain this data, we use ACS information on cohabitating parents and children age and expand the data so that we have individual-by-year data with an indicator whether the individual had a child that year. That is, we leverage the age difference between parents and their children to know when they became parents. We cluster the data at the state-by-year level and we use ACS/Census person weights.

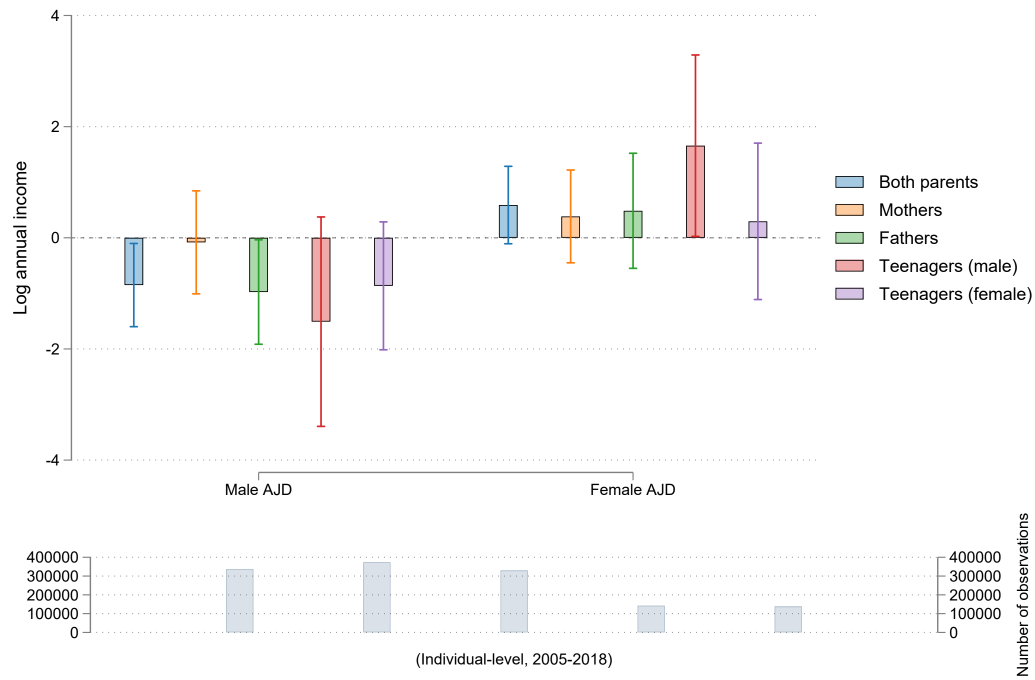


Figure 5.37: This figure presents coefficient plots for models of weeks worked at the individual-level by teenagers' family member. The data includes in which PUMA and state individuals reside. The treatment variables are the exposure to gender-specific adjusted job destruction, i.e. the gender-specific AJD at the the PUMA of residence at time $t-1$. We split the sample into the age groups used by the CDC. We control for individuals age, race and ethnicity. We also include PUMA fixed-effects, state-by-year fixed effects. We cluster the data at the state-by-year level and we use ACS/Census person weights.

Table 5.4: Impact of AJD on teenage fertility (NCHS)

	(1)	(2)	(3)
Adjusted job destruction (AJD)	13.6306**	13.6180***	12.8222***
	(5.3509)	(5.0172)	(4.8999)
Hispanic population share		-96.2697***	-89.7979***
		(19.8409)	(19.6807)
Non-white population share		73.1677***	77.4444***
		(17.0506)	(16.5663)
L.Teenage poverty rate			-0.2198***
			(0.0477)
Constant	30.5681***	35.2331***	37.8029***
	(0.0073)	(3.8936)	(4.1186)
Observations	53227	53227	53227
Adjusted R^2	0.961	0.963	0.964

Teenage fertility rate (15-19) is computed by the CDC for using all counties using NCHS population estimates (2002-2019). County and state-by-year fixed effects included. Adjusted job destruction (AJD) is the difference between jobs lost due establishment deaths and jobs created due to establishment births per capita, as reported by the BDS. We employ the estimator proposed by Correia (2016). Standard errors are clustered at the county level to account for potential correlation across time. Observations are weighted by the total population in the county.

Vita

Lucas Nogueira Garcez is an applied microeconomist. He was born in São Paulo and raised in Jundiaí, Brazil. He graduated from University of São Paulo, Fundação Getulio Vargas and University of Delaware before attending the Ph.D. at University of Tennessee.