Fuel Prices, Gang Violence, and Its Effects on Children's Health: Evidence from Mexico

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Abstract

I study the effects of organized crime on child health by exploiting Mexican cartels' shift into fuel theft. Using administrative birth and mortality records, I implement a difference-in-differences design that leverages the fixed pipeline network and an unexpected 20% fuel price increase in 2017, which intensified cartel violence. Pipeline municipalities experienced a 29–34% rise in homicides but no deterioration in birth outcomes; instead, infant mortality among boys declined by 6%. Evidence from stress-related hospital discharges and night-time lights suggests both physiological adaptation and local income gains. The findings challenge assumptions that violence uniformly harms health and offer new in- sights into the social consequences of organized crime in contexts of institutional fragility.

JEL codes: I12, J12, K42, O17

1 Introduction

Organized crime is responsible for 22% of global homicides and accounts for half of all homicides in the Americas (UN, 2023 [84]). Since Mexico's 2006 "war on drugs," the country has witnessed over 431,000 homicides and 218,000 disappearances—transforming what began as a militarized policy response into a structural feature of Mexican society. Nearly two decades later, drug cartels have become so economically embedded that they now rank as Mexico's fifth largest employers (Prieto-Curiel et al., 2023 [74]).

While previous research demonstrates that violence hampers economic development (Dell, 2015 [33]) and that acute violence exposure during pregnancy can harm birth outcomes through maternal stress, less is known about what happens when violence becomes persistent and woven into daily life. This distinction matters: existing literature on Mexico's drug war, such as Brown (2018 [17]), focuses on the immediate aftermath of sudden violence during 2002-2009, finding that birth weight fell by 42 grams during the conflict's initial phase. But after twenty years of sustained exposure, how do communities and health outcomes adapt to chronic insecurity?

This study tests the fetal origins hypothesis in a setting of sustained rather than acute violence, proposing two potential offsetting mechanisms that could explain adaptation over time. The first is physiological adaptation, where prolonged stress exposure reduces its biological impact on fetal development. The second involves economic spillovers: unlike violence from war and destruction, illicit market activity may increase local incomes and improve living conditions despite ongoing insecurity. Since the government's 2006 crackdown on cartels, criminal groups have diversified their activities beyond drug trafficking, increasingly turning to other illicit markets such as oil and fuel while relying on violence to evade law enforcement and dominate these sectors. Mexico's underground fuel infrastructure provides a laboratory for examining these dynamics. The theft of oil and fuel—known as "Huachicol"—from Mexico's 19,000-kilometer pipeline network has become a revenue stream that cartels are willing to fight over. Areas with greater cartel competition see more violence (Vivanco et al., 2023 [43]), intensifying stress for residents as rival groups battle for territorial control over extraction sites. Between 2009 and 2016, this illicit industry costed the Mexican government an estimated $9.28 billion in lost public finances, a sum comparable to the entire budget for scientific research and technological development during the same period. The high stakes and territorial nature of fuel theft operations have transformed pipeline corridors into contested zones where violence serves both to eliminate competitors and maintain control over extraction networks. Despite this violence, security experts note that fuel theft has become deeply embedded in local economies, with entire communities participating in or benefiting from pipeline tapping operations.

To identify the health effects of this violent yet economically productive criminal activity, I exploit two sources of quasi-exogenous variation using a municipal-level difference-in-differences strategy. First, I leverage the geographic distribution of pipeline infrastructure, which creates variation in exposure to fuel theft operations. Second, I use an unanticipated 20% fuel price hike in January 2017, which dramatically increased profit margins in the black market and intensified cartel competition around pipeline corridors.

I found that municipalities with pipelines experienced a 29-34% increase in homicides following the price shock—equivalent to roughly 7,000 additional deaths during the study period. Despite this substantial rise in violence and associated stress, I find no adverse effects on birth outcomes such as birth weight or gestational age, nor on miscarriages or stillbirths. Instead, I document a 6% decline in infant mortality among boys, with health improvements occurring during the first year of life rather than in utero.

Using stress-related hospital discharges, I provide causal estimates of physiological adaptation to persistent violence. Through nightlight data and proximity to pipeline tampering sites, I present suggestive evidence of income effects from illicit activity. Together, these findings support a two-pronged explanation: communities develop stress resilience while simultaneously benefiting economically from criminal markets.

This paper makes two key contributions to understanding violence and health in fragile states. First, it advances the fetal origins literature by showing how sustained conflict affects health outcomes differently than acute violence exposure. Second, it documents a form of rational adaptation where populations adjust to chronic insecurity through participation in illicit economies. More broadly, these findings have implications for policy responses in contexts where weak institutional capacity and limited formal employment create space for criminal markets to serve as economic stabilizers. Understanding whether communities can adapt to persistent violence—and at what cost—is crucial for designing interventions in the many Latin American countries cur-

rently experiencing similar patterns of organized criminal control.
The remainder of the paper is organised as follows. Section 2 reviews the relevant literature. Section 3 describes the institutional setting, while Section 4 outlines the identification strategy and Section 5 discusses threats to identification. Section 6 details the empirical approach, and Section 7 describes the data. Section 8 presents the main findings, followed by Section 9, which explores potential mechanisms. Finally, Section 10 concludes with policy implications and directions for future research.

2 Prenatal health and stressful shocks

The "fetal origins" hypothesis posits that conditions in utero can have lasting effects on human capital and health. Low birth weight and preterm birth—key indicators of prenatal health—are strongly associated with increased risks of infant mortality, cognitive deficits, and chronic disease (Frisbie et al., 1996 [44]; Hassan et al., 2021 [52]; Eves et al., 2023 [38]). Medical research identifies several biological pathways through which stress affects fetal development, such as elevated corticotrophin-releasing hormone (CRH) levels and stress-induced epigenetic changes, especially in early pregnancy (Wadhwa et al., 2004 [85]; Glynn et al., 2001 [47]; Copper et al., 1996 [26]).
Building on this foundation, economic research has linked adverse prenatal conditions to poorer child outcomes in contexts of economic hardship (Clark et al., 2021 [24]), pollution (Molina, 2021 [70]), famine (Scholte et al., 2015 [78]), and conflict (Mansour and Rees, 2012 [67]). Within the conflict literature, two main approaches have emerged: one focuses on acute shocks (e.g., landmine explosions, Camacho 2008 [19]); the other investigates the effects of sustained exposure to violence, including armed and criminal conflicts (e.g., Mansour and Rees, 2012 [67]).
This latter strand is especially relevant in the Latin American context, where violence originates from 'criminal wars', a conflict between the state and criminal organizations, or between criminal organizations themselves, sustained by militarized enforcement by governments (Zepeda Gil (2023 [89]). Countries such as Mexico, Colombia, Brazil, Peru and Ecuador have experienced persistent conflict of this kind, where organized crime strategically deploys violence creating widespread population-level exposure.
Given this distinct context of chronic criminal violence, understanding its health impacts requires examining how sustained exposure differs from acute conflict shocks documented elsewhere. A broad empirical literature from developing countries finds that conflict exposure—especially during the first trimester—is associated with lower birth weight and higher probability of low birth weight (Le and Nguyen, 2020 [59]; Mansour and Rees, 2012 [67]; Quintana-Domeque and Rodenas Serrano, 2017 [75]). However, recent evidence from Latin America suggests that the adverse effects of violence may be attenuated in contexts of chronic exposure. Moscoso (2022 [71]) shows that in Ecuador, the negative effect of homicides on birth weight is smaller for women with prior exposure to violence. Similarly, Foureaux et al. (2016 [55]) find modest effects on birth outcomes in Brazil, with stronger impacts in areas where violence is less endemic.
These mixed findings underscore the need for more precise causal evidence, especially in countries like Mexico, which has faced sustained exposure to organized crime violence. Since President

Felipe Calderón launched the "war on drugs" in 2006, Mexico has entered a prolonged period of elevated violence (see Figure 1). Government responses have been overwhelmingly punitive, and evidence suggests that these strategies have intensified rather than reduced conflict (Dell, 2015 [33]; Calderón et al., 2015 [18]; Massa Roldán et al., 2021 [68]). This sustained exposure to violence—whether direct, witnessed, or media-based—has been linked to increased psychological stress and mental health burdens (Rios and Rivera, 2018 [76]; Flores Martinez and Phillips, 2022 [40]).

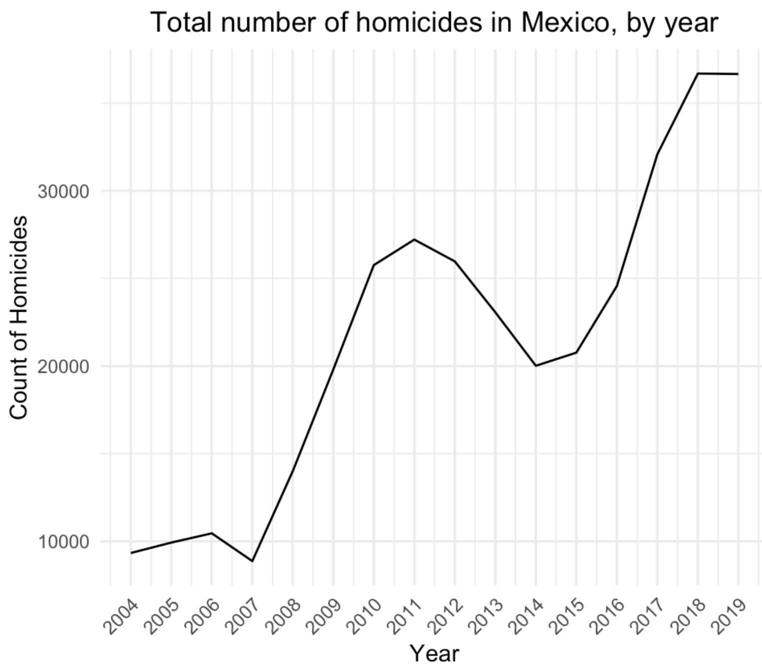


Figure 1: Total number of homicides by year. Source: INEGI's mortality records

Public perceptions mirror this climate of insecurity. In 2015, 68% of Mexico's urban population viewed their city as unsafe, rising to 73% by 2019. Among women, the figure rose from 72.6% to 77% over the same period[1].

Despite this extensive exposure, empirical research on the health consequences of Mexico's violence remains limited and focuses mainly on the early years of the conflict. Existing studies generally attribute adverse prenatal health effects to stress-related mechanisms. Torche and Villarreal (2014 [80]) find that the security crisis altered maternal behavior, with pregnant women increasing their use of prenatal care services, possibly as a compensatory strategy. Using data from the Mexican Family Life Survey (MxFLS), Brown (2018 [17]) documents a decline in birth weight following the onset of the drug war, particularly among low-income mothers. However, a major empirical challenge in this setting is identifying exogenous variation in violence after 2006, as conflict has become both widespread and persistent. This paper addresses that challenge by leveraging a natural experiment that generates localized and plausibly exogenous variation in

[1] https://www.inegi.org.mx/contenidos/programas/ensu/doc/ensu2019_diciembre_presentacion_ejecutiva.pdf

violence several years after the war's onset.

3 Setting

This section provides institutional context and describes the temporal variation central to the identification strategy: a sudden 20% increase in fuel prices, announced in December 2016 and implemented in January 2017, just months before the liberalization of Mexico's energy sector. Prior to this, fuel prices were heavily subsidized and fixed nationwide. The abrupt price hike—the largest in Mexico's history at the time— raised the profitability of stolen fuel.

Institutional background In 1938, a presidential decree established Petróleos Mexicanos (PEMEX), a state-owned enterprise with a monopoly over fuel sales and tightly regulated retail prices. From 1938 to 2016, fuel prices were set by the Ministry of Finance and applied uniformly across the country, with no geographic variation (Davis et al., 2019 [30]).
Due to declining oil production and fiscal pressures, the government approved an energy reform in 2013 to gradually liberalize the sector. However, implementation was delayed and revised multiple times. On December 20, 2016, the Energy Regulatory Commission released the final calendar which stated the dates for the liberalization process. Just eight days later, the government unexpectedly announced a 20% price increase, effective January 1, 2017[2]. The sharp adjustment was intended to cushion the population from a more abrupt transition under liberalisation. The policy sparked widespread public backlash and drove presidential approval ratings to record lows. The sharp increase in fuel prices—widely known as the gasolinazo—further boosted the profitability of fuel theft, a key component of organized crime's ongoing strategy to diversify beyond drug markets (Navarro, 2018 [72]; León Sáez, 2021 [61]; International Crisis Group, 2022 [54]).

Fuel theft Mexico's geography of violence has shifted in recent years as organized crime has moved into areas with greater profit potential (Yashar, 2018 [87]). This reallocation has transformed regions with pipelines in areas where criminal groups compete, increasing homicidal violence. Fuel in Mexico is transported through a pipeline system spanning over 19,000 km, including 9,098 km of polyducts used to carry refined products like gasoline and diesel. Constructed between 1954 and 2000[3] , much of Mexico's pipeline network predates the country's major waves of urban expansion and therefore runs beneath both long-established urban settlements and territories that later urbanised. The network crosses urban and rural areas and lies under buildings, agricultural fields and public infrastructure (see Appendix H). Because pipeline locations are classified for national-security reasons, criminal groups often bribe or coerce PEMEX staff to obtain precise site information.

[2]The fixed price remained in place until 18 February, after which daily maximum prices were introduced and allowed to vary across regions. The increase was implemented to cushion the population from the full impact of liberalisation. Nonetheless, the policy proved unpopular. Formal liberalization began on March 30, 2017, starting in Baja California and Sonora, and was rolled out gradually. By November 30, 2017, fuel prices were fully deregulated nationwide

[3]See https://www.pemex.com/nuestro-negocio/logistica/ductos/DocumentosInfra/Anexo%20Sistema%20Norte.pdf

Fuel theft generates substantial profits. A single crew of 10–20 people can siphon off US$25,000–37,500 worth of fuel during a month (León Sáez, 2022 [60]) . A former police officer reported earning US$2,600 per month from stolen fuel—over 180 times his previous salary[4]. Between 2009 and 2016, illicit fuel activity imposed substantial financial costs on the Mexican state: illegal fuel theft profits climbed from an estimated US$982 million in 2009 to US$1.7 billion in 2016, while pipeline repair costs soared nearly tenfold—reaching around 1.77 billion pesos (approximately US$95 million). Consumption of stolen fuel has also become widespread: illegal diesel and gasoline are estimated to account for 16–27% of Mexico's annual fuel use.[5]. According to the Mexican Association of Service Station Providers, there are approximately 22,000 "irregular self-consumption points" operating across the country—equivalent to 1.6 illegal outlets for every legally regulated gas station.

As shown in Figure 2, illegal fuel pipeline tappings increased markedly during the period under study, based on data from Mexico's public transparency portal (INAI). Between 2016 and 2017, the number of incidents rose by 25%. The figure presents tappings in the upper panel and fuel prices in the lower panel, allowing a visual comparison that suggests a potential link between rising fuel prices and illicit activity.

It is important to highlight that this increase on tappings might correspond to a lower bound, since in 2019, it was revealed that 170 out of 379 pipeline monitoring stations were non-functional, and 98 had never been installed.

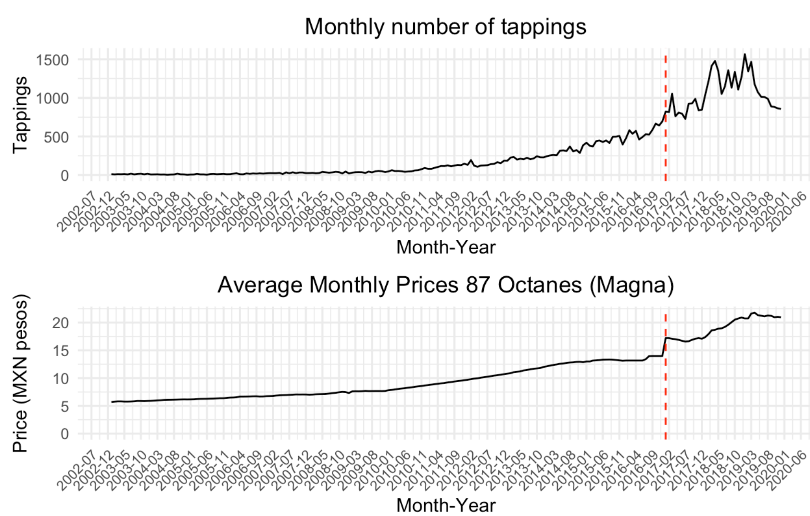


Figure 2: *A*verage monthly tappings and prices. Sources tappings: Tappings' data is from a public information request number 1857200094319. Sources for prices: Energy Information System (before 2016), public information request with number 330023824001608 (for 2016) and Energy Regulatory Commission (for 2017 onwards). Prices before 2017 are national prices determined by PEMEX and the treasury ministry. Prices are the national average for unlead gasoline (87 octanes-Magna) from 2017 onwards.

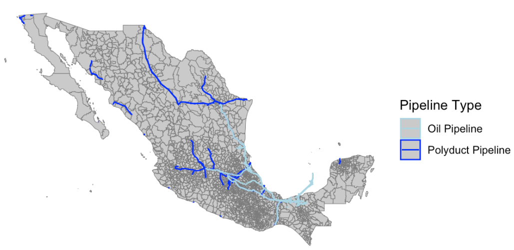
While qualitative accounts have long described rising violence near pipeline tapping sites,

[4] https://elpais.com/internacional/2017/05/23/mexico/1495496778_273384.html

[5] See https://ig.ft.com/mexico-fuel-theft/

systematic quantitative evidence is only recently emerging. López and Torrens (2023 [65]) model cartel competition and show that government crackdowns on drug trafficking have pushed criminal groups to diversify into fuel theft, which offers a profitable alternative. Because tapping sites are geographically separate from traditional drug routes, this shift has expanded the spatial reach of organized crime.
Supporting this, Vivanco et al. (2023 [43]) find that increases in international oil prices correlate with higher homicide rates in pipeline municipalities. Battiston et al. (2024 [8]) also document a rise in cartel presence in areas with pipeline infrastructure, especially where anti-trafficking political parties narrowly won local elections. Interestingly, while these areas experience more illegal tapping, no associated increase in homicides was found in the period they analyzed (2000 to 2014).

(a) Pipelines



(b) CartoCritica

Figure 3: Map extracted from CartoCritica

Pipeline location Although the exact locations of the pipelines remain undisclosed for security reasons, the CartoCritica NGO assembled pipeline maps using public and leaked sources. According to their map, pipelines traversed 400 of Mexico's 2,643 municipalities, across 29 of 32 states. Of these, 80 had oil pipelines, 164 polyducts, and 156 both types. Using publicly released tapping data[6] cross-referenced with CartoCritica pipeline maps (cf. Figure 3b), I find a 98.3% match between municipalities with reported tappings and pipeline presence. Between 2000 and 2019, 83% of municipalities with pipelines experienced at least one tapping incident[7].

Other studies rely on different pipeline maps, such as the one used by Vivanco et al. (2023 [43]), which identifies pipelines in 285 municipalities. I chose the CartoCritica map—also used by López and Torrens (2023 [65]) and Battiston et al. (2024 [8])—due to its alignment with the municipalities where illegal taps have been reported.

[6]INAI request number 1857200094319

[7]When both types of pipelines pass through the same municipality, the INAI data do not indicate whether the tapping occurred on the oil pipeline or the polyduct. However, since there are tapping incidents reported in municipalities that have only oil pipelines and others that have only polyducts, it is reasonable to conclude that both types are siphoned

3.1 Commodity Prices, Crime, and Health

Prior research shows that income shocks from commodity price changes affect whether people engage in legal or illegal activities, depending on whether the commodity is capital- or labor-intensive. Dube et al. (2013 [36]) find that oil price increases in Colombia escalated conflict due to oil's capital-intensive nature, while higher coffee prices reduced conflict by boosting legal labor demand. Similarly, Berman et al. (2017 [12]) find that mineral price increases in Africa promote violence in capital-intensive extraction, whereas Axbard et al. (2021[7]) show that labor-intensive mineral sectors reduce crime by creating legitimate employment opportunities.

Nevertheless, there is limited research on whether cartel activities can also generate local economic benefits. Most existing studies, which focus on the early years of Mexico's drug war (up to 2010–2011, before major cartel diversification), find predominantly negative effects on employment and poverty (Calderon et al., 2015 [18]; Gutierrez-Romero and Oviedo, 2017 [51]). However, measuring the economic impact of organized crime is challenging because informal employment—representing roughly 60% of Mexico's economy—is difficult to quantify. High informality makes criminal activity an option for those facing precarious legal work (Zepeda Gil, 2024 [46]) and allows cartels to provide ”clandestine welfare” that state institutions often fail to deliver (Zapata, 2023 [22]).

While there is qualitative evidence of how cartel activities and local economies have become increasingly intertwined[8], recent quantitative estimates show how significant this economic role has become. Prieto Curiel et al. (2023 [74]) calculate that, given persistently high homicide rates, cartels must recruit 350–370 people every week to sustain operations, effectively making organized crime Mexico's fifth-largest employer.

Combined with cartels' diversification into sectors like agriculture (Haro,2025[31]), gas (Haro et al., 2024 [31]), and even local internet services [9], this suggests that organized crime can generate local income spillovers in the communities where it operates. This challenges the conventional view of cartels as purely extractive, highlighting their role as informal economic actors in contexts of weak state capacity.

Related work shows that such local income effects can partly offset broader harms: in Angola, firms in conflict zones gained financially during civil war due to reduced competition and oversight (Guidolin and La Ferrara, 2007 [50]). In Africa, Benshaul-Tolonen (2019 [11]) finds that mining-related pollution's negative health impacts are partially cushioned by local income gains. By connecting fuel price shocks to organized crime and child health outcomes in a low-governance context, this paper adds new quantitative evidence to a nascent but growing literature on how illicit markets and economic shocks jointly shape community welfare.

[8] Project Noria documents how, historically, certain Mexican territories have depended on the cultivation of the poppy flower to sustain local livelihoods and in the absence of the State: https://noria-research.com/mxac/es/project/proyecto-amapola-mexico/

[9] https://www.theguardian.com/world/2024/jan/04/mexican-cartel-forces-locals-pay-makeshift-wifi

4 Identification strategy

This study analyzes the effect of stress due to violence on various health outcomes, using homicides as a proxy of stress. Despite widespread under-reporting of crime,[10] homicide data remain one of the most reliable indicators of violent activity. However, the spatial distribution of violence is not exogenous, complicating causal inference.

One strategy to address this is to exploit the temporal variation introduced by the 2006 drug war, as in prior work using the MxFLS survey waves of 2002-2012 (see Alamir, 2023 [4], Tsaneva and Gunes, 2020 [83], Brown, 2018 [17] and Flores Martinez and Atuesta, 2018 [39]). However, administrative birth records with birth weight data are only available from 2008 onward. Moreover, nearly two decades into the conflict, it is unclear whether populations have adapted to persistent violence or whether shifting crime geographies continue to influence health outcomes when using either survey or administrative data sources.

To explore this, I use a 20% increase in fuel price implemented in January 2017 following an unanticipated presidential decree. This shock, combined with the fixed geography of Mexico's underground pipeline network—built between the 1930s and 2000, well before the drug war, allows me to implement a difference-in-differences design comparing 400 municipalities with pipelines (treated) to 525 neighboring municipalities without pipelines (controls) (Figure ??). Descriptive statistics of the municipalities can be found in the Appendix.

The price shock itself is plausibly exogenous. While fuel market liberalization was expected sometime in 2017 (the final schedule was announced on December 20, 2016), the public did not anticipated the 20% increase announced on December 28 and implemented just days later on January 1, 2017.

As an initial check for parallel trends in homicides, I plot them in Figure ??. A formal test of the parallel trends assumption is presented later using an event study.

[10] Only 6.4% of crimes are reported in Mexico, and just 14% of those are solved. See Impunidad Cero's report: https://www.impunidadcero.org/impunidad-en-mexico/

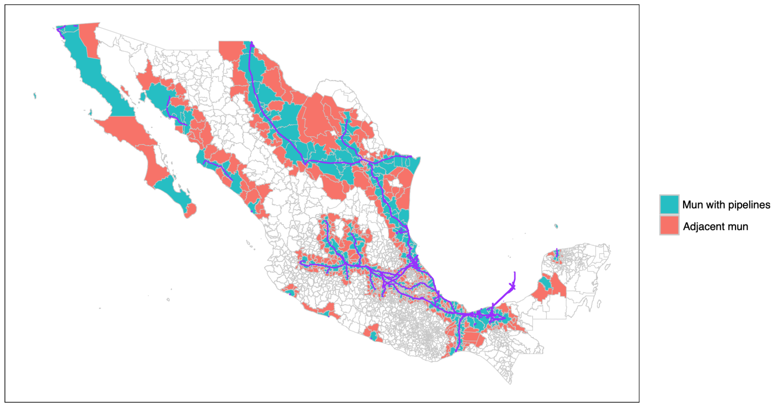
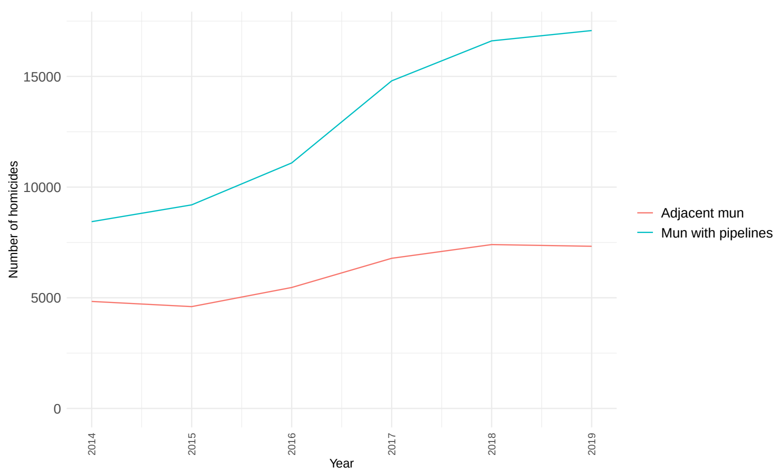


Figure 4: Municipalities with pipelines vs first-order neighbors. Municipalities with pipelines in green, first-order neighbors in orange. Source: Information from the location of the pipelines was extracted from the CartoCritica map.



Number of homicides in municipalities with pipelines vs. first-order neighbors. Homicides in municipalities with pipelines in green, first-order neighbors in orange. Information from the location of the pipelines was extracted from the CartoCritica map. Homicides are from INEGI's mortality records.

First-order neighboring municipalities serve as an appropriate control group because they share similar economic structures and institutional contexts with pipeline municipalities. Their validity as controls is supported by tests showing no evidence of homicide spillover effects from pipeline areas (see Appendix C.4). Event-study models for key outcomes (homicides, birth weight, gestational age) validate the parallel trends assumption, with pre-treatment trends not significantly different from zero. The analysis uses quarterly, municipality-level data to enhance empirical precision.

The analysis thus provides a unique opportunity to assess how acute increases in cartel-perpetrated violence—triggered by an exogenous economic shock—affect prenatal health and related outcomes in affected communities.

5 Threats to identification

In this section I discuss several potential threats to identification in my empirical strategy.

Internal Migration and Selective Fertility High levels of violence can significantly alter population behavior, particularly affecting women's fertility decisions and motivating expectant mothers to relocate due to safety concerns. Consequently, internal migration and selective fertility are two mechanisms that could confound my estimates.

Selective Fertility: Examining fertility trends over time revealed that the difference-in-differences (DiD) estimates do not show statistically significant effects or pretrends (see Appendix Table 4).

Internal Migration: Assessing internal migration is challenging due to the lack of comprehensive data tracking population movements. The limited data available comes from the decennial Censuses (2010 and 2020) and an Intercensal Survey (2015), both representative at the municipal level. The 2020 Census asks if the person resided in the same state from 2015 to 2020, while the Intercensal Survey (2015) asks if the person resided in the same municipality in the previous five years (2010).

The Intercensal Survey shows that 82% of inhabitants lived in the same municipality from 2010-2015, with a higher percentage (92%) for women of fertile age (15 to 49). This pattern holds for pipeline areas (83% overall; 91% for women) and neighboring municipalities (84%; 92%). Hence, it is plausible to state that most women do not relocate. Because administrative migration data at the municipal level are unavailable for 2016–2018, I use a geospatial approach to assess potential internal migration near pipelines. By creating buffer zones (1–20 km) around polyduct and oil pipelines, I estimate population changes using high-resolution GeoTIFF files for 2016 and 2017 (see Appendix C.2).

Following this strategy, I found a slight but statistically significant increase in population between 2016 and 2017 for areas near both types of pipelines, aligning closely with national and urban growth rates (1.1% and 1.4%, respectively). This suggests that observed population changes reflect Mexico's general demographic trends rather than internal migration triggered by security concerns. Stable growth across buffer distances also confirms that pipelines mainly connect urban centers rather than causing local population shifts. One caveat of this strategy is that it is not possible to distinguish between men's and women's movements. These findings suggest that internal migration and selective fertility are unlikely to pose a significant threat to the identification strategy.

Anticipation Effects Another potential threat to identification is the possibility of anticipation effects. The fuel price increase was officially announced on 28 December 2016 and implemented on 1 January 2017, with prices rising by 20%. If people anticipated this hike, it would be expected a rise in illegal fuel tappings before January. To test this, I plotted monthly tappings for the year before and after the increase. The results (see the Appendix, Figure 19a) show a clear increase starting in January 2017, without an earlier spike. Since tapping data are monthly, any response between the announcement and the price increase (from 28 December to 1 January) cannot be observed. Likewise, PEMEX data show no unusual spike in legal fuel purchases in the months leading up to the price change (Appendix Figure 19b).

Spillover Effects (SUTVA Violations) Another threat to identification could come from violations of the stable unit treatment value assumption (SUTVA) if spillovers from treated municipalities contaminate the control group. To assess this, I conduct a series of robustness tests by redefining treatment and control groups based on distance from pipeline municipalities.

I reassign the original control group—first-order neighbors of pipeline municipalities—as a new treated group and compare them to second- and third-order neighbors (see Figure 20), who are progressively more distant from pipelines and presumably less exposed to spillover effects. This approach tests whether violence spillovers extend beyond the immediate neighbors of pipeline municipalities (See details in Appendix section C.4).

The results show no statistically significant difference in homicides between first- and second-order neighbors, nor between second- and third-order neighbors. However, a statistically significant difference emerges when comparing first- and third-order neighbors directly. This finding suggests that spillover effects may extend beyond immediate neighbors but dissipate by the third order. The comparison between first- and third-order neighbors shows evidence of pre-treatment trends, which limits causal interpretation of this result.

6 Empirical strategy

To assess the impact of the 2017 fuel price increase on violence and stress-related outcomes, I employ two distinct analytical approaches based on the temporal nature of exposure. I distinguish between calendar-time outcomes—such as homicides, infant and child mortality, suicides, hospital discharges, and fertility rates—and gestation-based outcomes—such as birth weight, gestational age, and fetal deaths.

This distinction reflects key differences in how and when exposure to the shock could plausibly influence outcomes, particularly given the biological sensitivity of the in utero period. Both approaches employ event study and difference-in-differences designs at the municipality level, with all outcomes aggregated quarterly. This level of aggregation smooths short-term fluctuations and aligns with the gestational period, allowing for an analysis that is compatible with trimester-based windows of exposure, which are commonly used in the literature on prenatal stress and health outcomes.

The event study spans a symmetric six-quarter window from Q3 2015 to Q2 2018, capturing trends before, during, and after the policy change. This time frame maximizes exposure to violence while avoiding two potential confounds: the 2018 election period, during which cartel violence against local candidates and authorities spiked as criminal groups sought to influence political transitions, and the 2019 military crackdown on oil pipeline theft, which prompted criminal groups to diversify into gas pipeline theft (De Haro and Katovich, 2024 [32]).

Calendar-Time Outcomes (Homicides, Discharges, Mortality, Suicides) For outcomes that follow calendar time—homicides, obstetric hospital discharges, suicides, mortality (infant, child, and other causes), and fertility—I align the event study by calendar quarter, centering the analysis around Q4 2016 as the reference period immediately prior to the shock (Figure 5).

Gestation-Time Outcomes (birth certificates and fetal deaths) For health outcomes determined during pregnancy, I estimate the quarter of conception for each birth using birth date and gestational age information. Aligning by conception quarter, rather than birth quarter, ensures more accurate assessment of in utero exposure to violence and associated stress.
I set Q1 2016 as the reference period, representing conceptions occurring before babies could be affected by the price increase (Figure 5). This timing allows for meaningful comparisons: children conceived in Q2 2016 were likely born in Q1 2017, with most gestation occurring before violence onset, while those conceived later experienced violence exposure during more sensitive developmental stages, particularly the first trimester. The analysis uses the same six-quarter symmetric window around the shock, capturing conception cohorts before, during, and after the fuel price hike.

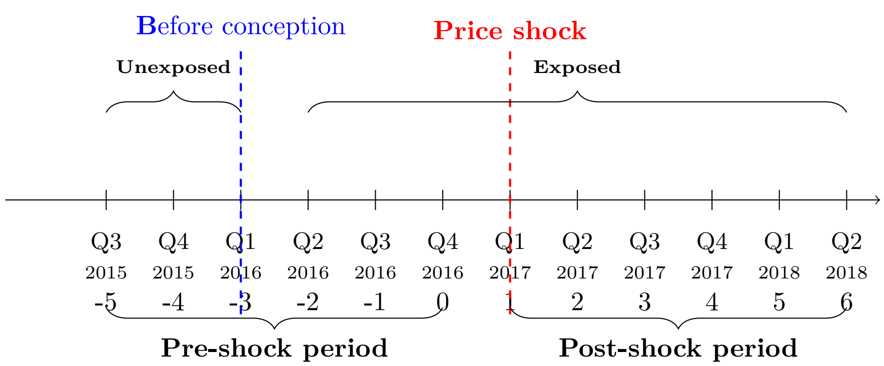


Figure 5: Timeline for analysis. Gestational cohorts used for birth and fetal health outcomes use reference period Q1.2016. Calendar periods used for outcomes like violence, hospital discharges, and mortality use reference period Q4.2016.

Hence for a set of i outcomes, outcome Y_i by municipality j and time (quarter-year) t, I estimate the following specification:

$$Y_{jt} = \mu_0 + \beta_0 \cdot \text{Pipeline}_{jt} + \sum_{k=-i,k\neq 0}^{i} \beta_k \cdot \text{Pipeline}_{jt} \cdot \mathbb{1}[\tau = k] + \eta_j + \delta_t + \varepsilon_{jt} \quad (1)$$

The term $Pipeline_{jt}$ is a dummy variable that takes the value of "1" when there is a pipeline in the municipality and "0" if the municipality is the first-order neighbor. The reference period ($Time_t = 0$) is set in December 2016, just before the price increase.
The analysis considers a pre-event window t, $k = \{-5, -4, -3, -2, -1\}$ in 2015 and 2016 and an after-event period ($k = \{1, 2, 3, 4, 5, 6\}$) from January 2017 onward. Each estimate β_k represents the change in outcome Y for municipalities with pipelines compared to their neighbors after the price increase.

The event study also incorporates quarter-by-year fixed effects δ_t to account for temporal fluctuations affecting the outcome variable dynamics differently across quarters and years, and municipality fixed-effects η_j to control for non-invariant geographical characteristics. Standard errors are clustered by municipality (Abadie et al., 2022 [1]). In addition, difference-in-difference (DiD) designs were used to assess aggregate effects before and after the price increase.

7 Data and variable definitions

I use the empirical strategy mentioned above in a variety of different health outcomes (See Appendix table 1). All outcome data come from administrative public registers collected by the National Institute of Statistics and Geography, INEGI (in Spanish), for all 2463 municipalities. Descriptive statistics are available in the Appendix.

7.1 Homicides

Homicide data are sourced from mortality records compiled by INEGI and accessed via the mxmortalitydb R package[11]. These records, from death certificates, are aggregated at the municipality–quarter–year level. I use homicides as an initial validation of treatment exposure: if municipalities with pipelines became more stressful following the January 2017 fuel price hike, it would be expected a subsequent rise in homicides. The dataset includes subcategories (e.g., firearm-related deaths) and victim characteristics (age and sex), enabling detailed analysis. As shown in Figure **??**, homicide trends were parallel for control and treatment groups until 2017, after which pipeline municipalities diverge.

7.2 Health outcomes

The health outcomes examined in this research are prenatal and neonatal outcomes, as well as infant mortality (under one year of age) and child mortality (ages one to less than five years old). As outlined in the empirical strategy, the key distinction is that prenatal and neonatal outcomes are tied to conception timing, whereas infant and child mortality are recorded according to calendar time.

These outcome categories reflect fundamentally different causal mechanisms. Prenatal outcomes (fetal deaths, stillbirths, miscarriages) and neonatal outcomes (birth weight, gestational age) are primarily determined by conditions during pregnancy, like maternal stress (Miller, 2022[69]). These outcomes thus capture the biological effects of violence exposure during gestation.

In contrast, infant and child mortality can operate through postnatal mechanisms. While some infant deaths stem from birth conditions, many result from factors that emerge after delivery—household economic conditions, care practices, and environmental exposures. An examination of leading causes of death among Mexican children under five during this period illustrates this

[11] https://github.com/diegovalle/mxmortalitydb

distinction: perinatal conditions account for 41.6% of deaths, congenital malformations for 23.3%, and respiratory diseases for 6.9%. Whereas perinatal conditions and congenital anomalies are largely determined in utero, respiratory diseases are more strongly influenced by postnatal living conditions and care quality.
This mechanistic distinction is crucial for interpretation, as it suggests that fuel price shocks may affect infant and child health through multiple pathways rather than solely through prenatal channels.

Birth registers and fetal mortality To test the "fetal origins" hypothesis, that establishes that conditions in the womb can have long-term effects on individual's human capital, I analyze data from birth registers and from fetal mortality records. Both datasets contain prenatal outcomes (fetal deaths, stillborns, miscarriages) and neonatal outcomes (birthweight, gestational age, neonatal mortality), as they represent a contrast between more "resilient" pregnancies that managed to reach their full term, as well as to characterize the ones that could not make it.
Data on birth certificates are based on administrative records from civil registry certificates that provide insights into common maternal ages at conception, timing and completeness of birth, and the mother's usual place of residence, among other factors.
Neonatal mortality records are obtained from Fetal Death Certificates, which capture both deaths at birth and the loss of a product of conception before it is fully expelled or extracted from the mother's body, regardless of the duration of the pregnancy. This administrative data set compiles all death certificates, as required by the Civil Registry.

Birth certificates The birth certificate dataset includes detailed information on newborns, such as weight (collected since 2008), gestational age, and sex, as well as maternal characteristics like age, and municipality of residence. I studied all births (3,953,922) during this period in the treatment and control municipalities and aggregated the data by quarter, year, and municipality. I calculated various metrics such as averages of the following variables: birth weight (in grams), gestational age (in weeks), mother's age, number of prenatal visits, and Apgar scores. Additionally, I estimated the percentages of premature births (less than 37 weeks), percentage of teenage mothers, the percentage of low Apgar scores (less than 7) and sex ratios.
This dataset covers the third quarter of 2015 through the second quarter of 2018.

Fetal deaths This dataset, compiled by INEGI, records characteristics of products of conception (POC) and includes 39,031 registered deaths over the study period. For fetal mortality, I analyze multiple outcomes aggregated by municipality, quarter, and year: deaths with pregnancy complications, deaths at birth or during pregnancy, neonatal deaths of male fetuses, stillbirths (after 20 weeks gestation), and miscarriages (at or before 20 weeks). Rates are calculated as the number of cases per 1,000 women of reproductive age in each municipality and period (see Appendix B for population details). Like the birth certificate data, this dataset covers the third quarter of 2015 through the second quarter of 2018.

Child and infant mortality Infant mortality captures deaths before age one and child mortality is defined as deaths from one to five. Using quarterly INEGI mortality data, I construct these outcomes from registered deaths within each age group. Without matching quarterly birth counts, calculating true mortality rates would introduce denominator error. Additionally, the absence of gestational age data limits age-specific mortality definitions to chronological age at death. I therefore report raw death counts and employ fixed effects to control for population size variation and seasonal trends.

7.3 Other Data Sources

Additional data sources support mechanism analysis, address identification threats, and enable robustness checks. For mechanism analysis, I examine stress resilience proxied by stress-related hospital discharges, and income effects measured using nightlight data—DMSP/OLS and VIIRS—plus spatial GDP per capita estimates.

To address identification threats, I test for internal migration using GLOBPOP population estimates and examine fertility shifts using INEGI birth certificate data with imputed municipal counts of reproductive-age women. Robustness checks employ INEGI data on medical procedures and obstetric hospital discharges to verify that results are not driven by unrelated healthcare changes or behavioral responses.

8 Baseline results

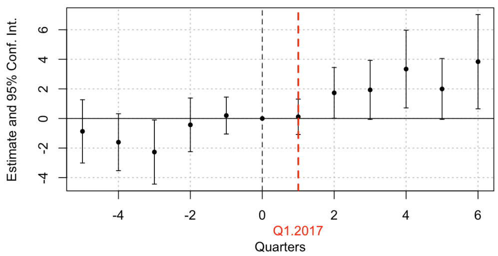
This section presents three sets of findings. First, I document that municipalities with pipelines experienced a marked rise in homicides after the January 2017 fuel price increase. Second, I show that this increase in local violence did not affect prenatal or neonatal outcomes. Third, I provide evidence that infant mortality (less than one year) declined following the price increase. In the Appendix, I show that these results are not explained by internal migration, selective fertility, abortions, maternal mortality, or changes in healthcare access.

8.1 Are there more homicides in places with pipelines?

To assess whether areas with pipelines became more stressful to live in, I begin by examining whether they became more violent. Using the research design outlined in Figure 5 and an event study specification (Equation (1)), I compare homicides relative to the fourth quarter of 2016. Figure 6a shows that total homicides began to rise in the second quarter of 2017 and remained persistently higher thereafter. For firearm homicides of young men (ages 14–44)—a type of homicide correlated with organized crime—the pattern is similar, with a sustained increase starting in 2017Q2 (Figure 6b).

The DiD estimates (Table 16 in the Appendix) indicate an increase of nearly 30% in total homicides relative to the mean, and a 33% rise in firearm homicides of young men. Scaling the estimates by the number of treated municipalities and post-shock quarters implies approximately 7,100 excess total homicides and 4,000 excess firearm homicides of young men in pipeline municipalities over the six quarters following the price increase.

(a) Effect of price increase on all homicides



(b) Effect of price incr. on young male hom. firearm

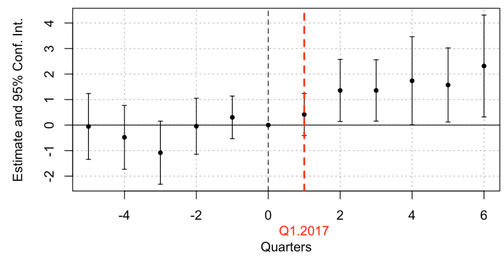


Figure 6: Results from estimating equation 1, where the dependent variables are the number of homicides and firearm homicides of young men (14 to 44 years old) from INEGI's mortality population counts from the third quarter of 2015 to the second quarter of 2018. The X-axis represents the quarters. The reference period is fourth quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the number of homicides. I control for time and municipalities' fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

I observed a similar pattern in the event studies and DiD estimates for firearm, male, and young male homicides when analyzed separately (see Appendix E for tables and figures). For female homicides, I also detect an increase after the price shock, but with significant pre-trends that complicate interpretation. Full regression results are provided in the Appendix.

These findings suggest that organized crime groups rapidly adjusted to the new market opportunity created by the fuel price hike, intensifying violent competition in affected municipalities. This evidence echoes Sobrino (2019 [79]), who shows that the 2010 reformulation of OxyContin in the U.S. triggered a heroin demand shock, raising homicide rates in Mexican poppy-growing regions. Likewise, Battiston et al. (2024 [8]) document cartel entry into fuel theft following the 2006 drug war, though without detecting homicide increases in the 2000–2014 period. In contrast, my results indicate that the 2017 fuel price increase created a sharp new incentive for cartels to violently compete over this resource, leading to a sizable rise in local homicides.

8.2 Prenatal and neonatal outcomes

This section presents results for outcomes derived from birth certificates (prenatal) and fetal mortality (neonatal) records.

Following the gestation-based approach described in Section 6, the analysis aligns outcomes by conception quarter, with the first quarter of 2016 serving as the reference period. All estimates correspond to the event study specification in equation (1), including municipality and time fixed effects.

Figure 7 displays the event study results for birth weight. The plot reveals no statistically significant effects of the fuel price increase on birth weight in pipeline municipalities compared to neighboring areas. All point estimates across the study period remain statistically indistinguishable from zero, suggesting that the violence shock did not detectably affect average birth weight.

Figure 7: Effects of the price increase on birth weight

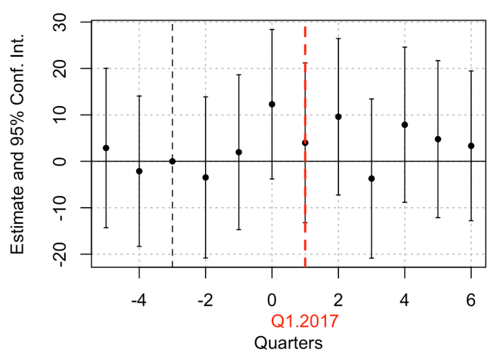
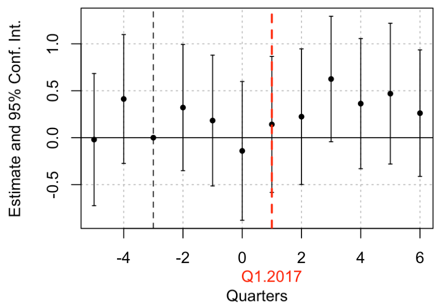


Figure 8: Results from estimating equation (1), where the dependent variable is birth weight from the third quarter of 2015 to the second quarter of 2018, from INEGI's birth certificates dataset. The X-axis represents the quarters. The reference period is the first quarter of 2016, a quarter before conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the weight in grams. I control for time and municipalities' fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

To evaluate the precision of this null finding, I compare my estimated confidence intervals with those reported in related studies. Camacho (2008[19]) finds that exposure to landmines explosions during early pregnancy reduces birth weight by 2.717 to 12.343 grams. My estimates yield a comparable confidence interval of -0.999 to 10.171 grams (see Appendix Table 17), indicating sufficient statistical power to detect effects of similar magnitude.

(a) Effects of the price increase on % low weight



(b) Effects of the price increase on gest. age

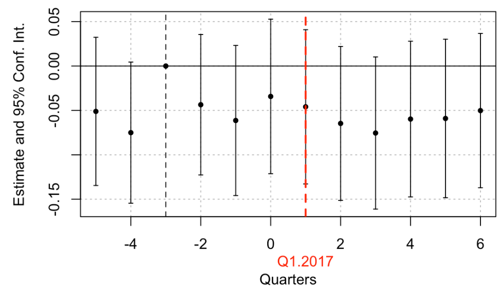
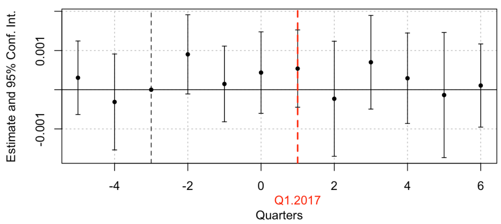


Figure 9: Results from estimating equation (1), where the dependent variables are the percentage of low birth weight and average gestational age (weeks) from the third quarter of 2015 to the second quarter of 2018, from INEGI's birth certificates dataset. The X-axis represents the quarters. The reference period is the first quarter of 2016, a quarter before conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the weight in grams. I control for time and municipalities' fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

Likewise, there is no detected effects on the percentage of low birth weight or in gestational age. Furthermore, my estimates reveal no statistically significant effects on the percentage of babies with macrosomia, premature births, or births to teenage mothers. Similarly, no detectable effects are observed on average maternal age, prenatal care utilization, or sex ratios. However, pre-treatment trends are detected for Apgar scores and the percentage of births with low Apgar scores (less than 7), precluding causal interpretation of these outcomes.
Having established that in utero exposure to the fuel price increase does not affect birth weight among live births, I examine whether violence affected neonatal mortality, outcomes that may be more sensitive to acute maternal stress.
Analysis of neonatal death records yields no statistically significant estimates for either stillbirth or miscarriage rates. The difference-in-differences interaction terms for both outcomes remain statistically indistinguishable from zero.

No statistically significant changes and no pre-trends are observed in the total number of prenatal deaths (although the coefficient is positive), complications during pregnancy, prenatal care, and death at birth (see Tables 19 and 20). Additionally, there is no increase in the percentage of neonatal deaths among male fetuses, who are generally more biologically fragile than females (Kraemer, 2000 [57]).
The absence of detectable effects on prenatal and neonatal health outcomes, despite clear evidence of increased violence, diverges from findings in the existing literature linking violence exposure to adverse birth outcomes.
The affected populations may exhibit high resilience to stress, potentially developed through prior

(a) Effects of the price increase on stillbirths rate among women



(b) Effects of the price increase miscarriage rate among women

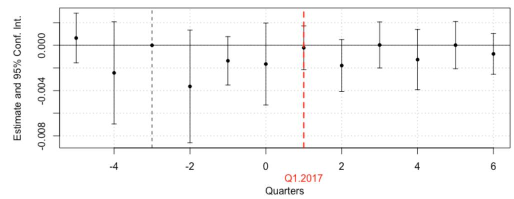


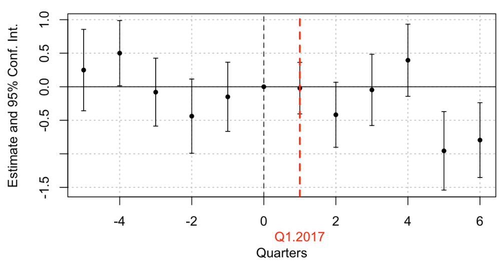
Figure 10: Results from estimating equation (1), where the dependent variables are the rate of stillbirths in that municipality that presented a neonatal death and the rate of miscarriages from the third quarter of 2015 to the second quarter of 2018, from INEGI's birth certificates dataset. The X-axis represents the quarters. The reference period is the first quarter of 2016, a quarter before conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the weight in grams. I control for time and municipalities' fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

exposure to violence-related uncertainty. Alternatively, economic benefits derived from increased fuel theft activities in pipeline areas may have improved material conditions for pregnant women, offsetting stress-related health effects. These potential mechanisms are explored in detail in Section 9.

8.3 Infant and child mortality

Contrary to established literature documenting adverse effects of conflict on child health (Wagner et al., 2018 [86]), I find that infant mortality decreased by 6.1% in pipeline municipalities following the fuel price shock, despite documented increases in homicides. The conflict literature, primarily focused on African contexts, consistently shows that violence exposure increases infant and child mortality through reduced healthcare access, nutritional stress, and maternal health deterioration—effects that would predict increased mortality in affected pipeline areas. Gender-disaggregated analysis strengthens these findings. Male infant mortality declined by 6.29%, while female infant mortality also decreased, though pre-existing trends complicate causal inference for girls. The larger effects for boys align with medical evidence of greater male vulnerability to environmental stressors (Chao et al., 2023[23]), suggesting boys may be more responsive to improved economic conditions accompanying the violence shock. Notably, I find no effects on child mortality (ages 1 to 5), indicating that benefits were concentrated in the first year of life when infants are most sensitive to household economic conditions.

Regarding dynamic effects, a decline in overall infant mortality (Figures 11a and male infant mortality 11b) is observed in the first and second quarters of 2018 (January–March and April–June 2018), corresponding to children born around 2017Q2–2018Q1. These cohorts were conceived between late 2016 and mid-2017, with many conceived before the January 2017 fuel price increase. Since homicides increased sharply from 2017Q2, infants dying in early 2018 experienced their *(a)* Effects of the price increase on infant mortality



(b) Effects of the price incr. on boy infant mortality

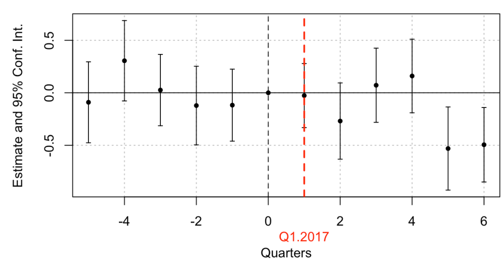


Figure 11: Results from estimating equation (1), where the dependent variables are infant and boy infant mortality from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's mortality records. The X-axis represents quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the changes in mortality per quarter. I control for time and municipalities' fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

entire postnatal period during peak violence yet showed improved survival—strongly suggesting postnatal rather than prenatal mechanisms. These dates are rough estimates as the mortality data set contains a birth date but no gestational age. I analyze the leading causes of infant death in Mexico (respiratory distress, birth asphyxia, congenital heart malformations; see Appendix Tables 22, 23, and 24) and find no statistically significant effects for any individual cause of death. This suggests that the observed decrease in mortality represents the joint effect of small improvements across multiple causes rather than a large reduction in any single specific condition.

Postnatal mechanism So far, the evidence presented in this research supports a postnatal rather than a prenatal improvement conditions. First, I find no effects on any in utero outcomes such as birth weight, gestational age, prematurity, miscarriages, or stillbirths. Second, selection mechanisms are absent—fertility and abortions remained stable (Appendix C.1), maternal mortality was unaffected (Appendix D.3), and obstetric hospital discharges (like births) show no significant changes (Appendix D.2).

To test whether improved healthcare access explains the mortality decline or the lack of in utero adverse effects, I analyze urgent and elective medical procedures across multiple demographic groups—including procedures specific to newborns, children, adults, and the elderly[12] (Appendix D.1). This approach allows to observe systematic changes in hospital capacity or access, as violence-induced healthcare disruptions or improvements would manifest in the delivery of unrelated medical procedures across age groups. The analysis reveals no consistent changes in these procedures, indicating that neither increased strain on healthcare services nor infrastructure improvements likely drove the mortality decline.

[12] Following the approach of Guidetti et al. (2021) [49] and Deryugina et al. (2019) [34].

Collectively, these robustness checks (Appendix D) support an interpretation that postnatal improvements in household economic conditions, possibly linked to increased illicit income, contributed to the observed reductions in child and infant mortality. This interpretation is supported by a concurrent reduction in child mortality (under age one), affecting children born well before the price change (Figures 11a and 11b).

9 Mechanisms

I examine potential mechanisms driving the results. First, to explain the absence of detectable effects on in utero outcomes despite the substantial increase in homicides, I consider the possibility of adaptation to persistently high levels of stress among expectant mothers. Since Mexico has faced widespread violence since 2006, populations may have adjusted to its psychological burden. To explore this mechanism, I examine stress-related hospital discharges, focusing on anxiety, substance use disorders, and a selection of cardiovascular and cerebrovascular diseases.

A second, complementary explanation is that violence associated with illegal economic activity may generate local economic benefits that offset or even outweigh its detrimental health effects. Fuel theft is a highly profitable activity, and communities located near pipelines may benefit from these rents directly or indirectly through broader economic spillovers. To capture these effects, I use night-time light intensity as a proxy for local economic activity, given the absence of granular and timely income data in Mexico. Economists have used satellite light data to supplement official income growth measures, especially in regions lacking comprehensive statistics (Henderson et al., 2012[53]). An increase in local economic activity could also help explain the observed decrease in infant mortality, since these deaths are less directly tied to prenatal conditions and more influenced by postnatal factors such as nutrition, healthcare access, and living conditions.

While the first mechanism can be tested in a causal framework, the second should be interpreted more cautiously, as it is inherently more difficult to isolate from confounding factors. Nonetheless, considering both pathways is important for interpretation, as it highlights that violence can influence maternal and child health through multiple, and potentially countervailing, channels.

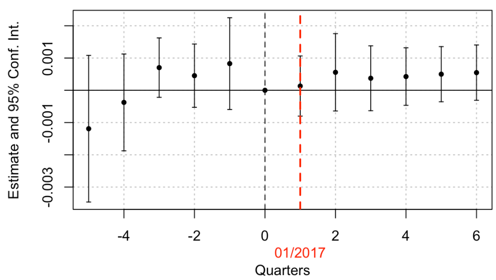
9.1 Adaptation

To assess the effects of rising violence on stress, I analyze stress-related hospital discharges from INEGI using the event study framework in Equation (1). While much of the evidence linking stress and mental health comes from administrative records in high-income countries or surveys, limited data availability in Mexico restricts timely assessments of population-level stress. Hence, I focus on conditions plausibly linked to stress, including anxiety, substance use disorders, and a selection of cardiovascular and cerebrovascular diseases (See categorization in the Appendix table 29). Since the administrative data do not directly identify pregnant women, I restrict the main analysis to women of reproductive age and report additional estimates for men, young men, all women, and the general population in Appendix F. Most of these additional outcomes are not statistically significant.

Discharge rates are constructed quarterly by municipality, scaled per 1,000, and adjusted using population estimates (see Appendix B). As a complementary check for extreme psychological outcomes, I examine suicides[13] among women—particularly those in their childbearing years—and other subgroups (Appendix D.4). I find no significant changes, which helps mitigate concerns about unobserved psychological distress.

The DiD and event study estimates for anxiety (Figure 12a) and substance use disorders (Figure 13a) are statistically indistinguishable from zero. For cerebrovascular diseases (Figure 12b), while a pretrend appears in Q3 2015, neither the DiD interaction term nor the event study coefficients show significant effects overall. In contrast, hospitalizations for heart disease (Figure 13b) show rising pretrends before the shock, which persist briefly during the shock and then flatten. The DiD coefficient for women in their childbearing years is not statistically significant, despite the coefficient for the whole population is positive and statistically significant but is driven mostly by young men.

(a) Effects of the price increase on anxiety disorders rate among women



(b) Effects of the price increase on cerebrov.-stress diseases rate among women

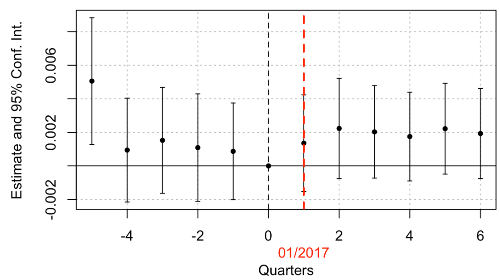


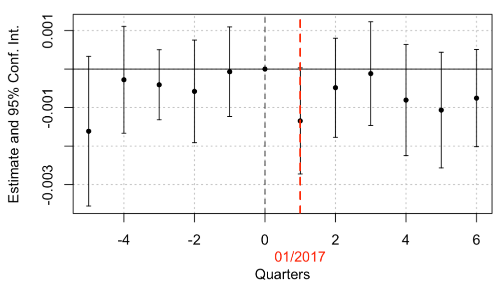
Figure 12: Results from estimating equation (1), where the dependent variables are the rate of anxiety disorders and cerebrovascular-stress-related diseases from the third quarter of 2015 to the second quarter of 2018. The data source are INEGI's hospital discharges. The X-axis represents the quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the number of homicides. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

9.2 Income effect

Due to the localized nature of fuel and oil theft, it is plausible that income increases close to pipeline areas may be linked to the proceeds of such activities. For example, individuals involved in fuel theft might invest in local businesses, purchase goods, or build infrastructure. This increase in income could also translate into better prenatal or general health conditions, which could neutralize the adverse effects of stress due to homicides.

[13] I report raw suicides counts and employ fixed effects to control for population size variation and seasonal trends

(a) Effects of the price increase on substance use disorders rate among women



(b) Effects of the price increase on heart disease rate among women

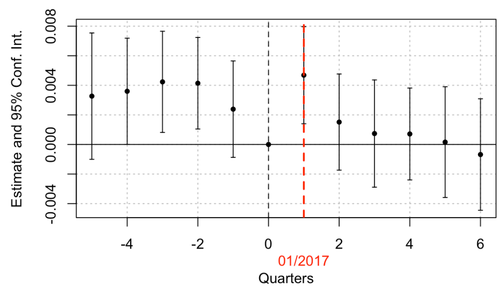


Figure 13: Results from estimating equation (1), where the dependent variables are the rate of substance use disorders and heart-stress-related diseases from the third quarter of 2015 to the second quarter of 2018. The data source are INEGI's hospital discharges. The X-axis represents the quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the number of homicides. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

In Mexico, municipal-level income data are not available at sufficient spatial or temporal granularity. To proxy for local economic activity, I use satellite nightlight intensity, a widely used indicator of income and consumption in data-scarce contexts [53, 35]. Increases in light intensity are generally associated with rising incomes and development. To strengthen robustness, I also use a geospatial gridded dataset for GDP (see Appendix D.5).

To analyze light intensity, I create buffer zones around both polyduct and oil pipelines at varying distances (1, 5, 10, 15, and 20 km), extending equally on both sides of each pipeline. For each buffer, I calculate the average nightlight intensity and compare values between 2016 and 2017, bracketing the January 2017 fuel price increase.

I use two nightlight datasets: (1) harmonized DMSP/OLS data (Li et al., 2017 [62]), which provides long-term historical consistency but lower spatial resolution, and (2) VIIRS data from the Colorado School of Mines (Li et al., 2020 [63]), which offers finer spatial detail but lacks year-to-year harmonization.

DMSP Analysis of DMSP data shows that, regardless of pipeline type, light intensity increases as proximity to the pipeline decreases, reflecting the original purpose of pipelines supplying fuel and oil to urban centers. Moreover, both polyduct and oil pipelines display a statistically detectable increase in light intensity between 2016 and 2017 (Figures 14a and 14b).

(a) Light intensity polyduct *(b)* Light intensity oil

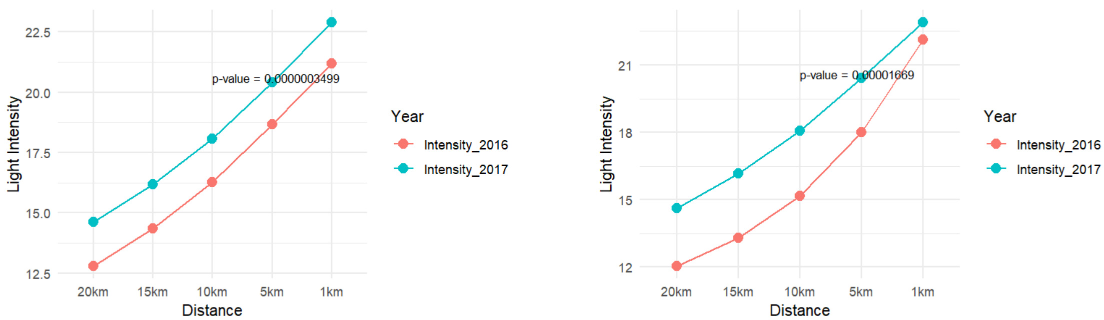


Figure 14: Average light intensity for 2016 and 2017 for both type of pipelines: oil and polyduct. Data from DMSP dataset (Li and Zhou, 2017 [62]).

VIIRS VIIRS data reveal a similar spatial pattern: light intensity rises near pipelines. However, year-to-year changes differ by pipeline type. For polyduct pipelines, light intensity increased in 2017 relative to 2016 (paired t-test, p = 0.0003), while for oil pipelines, changes were small and not statistically significant.

This divergence may reflect differences in the economic incentives of tapping each type of pipeline: polyducts transport fuel and diesel, which are easier to extract and more profitable, whereas oil pipelines carry crude oil, making illicit extraction technically complex and less profitable. Differences in sensor resolution may also contribute: DMSP smooths short-term local variation, capturing broader trends, while VIIRS detects localized changes more precisely.

Overall, nightlight data suggest that illicit activity near polyduct pipelines may be associated with increases in local economic activity. To corroborate this, I examine geospatial GDP data (Appendix D), which shows income growth around both pipeline types. These patterns support the interpretation that local economic effects from fuel theft could partially offset the negative health consequences of violence.

(a) Light intensity polyduct *(b)* Light intensity oil

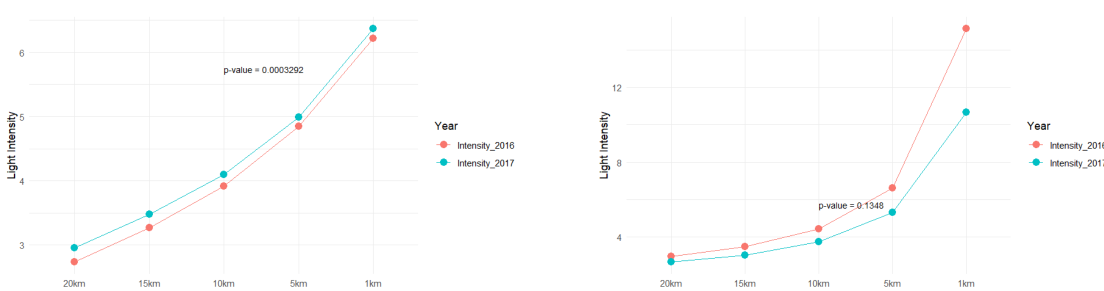


Figure 15: Average light intensity for 2016 and 2017 for both type of pipelines: oil and polyduct. Data from VIIRS dataset (Li et al., 2020 [63]).

10 Conclusion

This paper examines the health consequences of violence linked to organized crime, leveraging two sources of quasi-exogenous variation: the geographic distribution of Mexico's underground fuel pipeline network and a sudden, nationwide fuel price increase in January 2017. document a sharp rise in homicides—between 29% and 34%,in municipalities with pipelines following the shock, illustrating how cartels respond to changes in the price of commodities they can exploit for profit.

Despite this escalation in violence, I find no evidence of adverse effects on in utero health outcomes, including birth weight, gestational age, prematurity, fetal deaths including stillbirths and miscarriages. Moreover, I found declines (6%) in infant mortality among boys. These results are not driven by changes in fertility, abortion rates, maternal mortality, or obstetric hospital discharges (births, c-sections), and I also rule out improvements in healthcare supply through elective or urgent procedures. Collectively, the evidence suggests that reductions in infant mortality, alongside stable prenatal outcomes, may reflect the presence of palliative mechanisms.

To explore underlying pathways, I examine both stress and income channels. Satellite-based nightlight data indicate increased luminosity—and by extension, local income—in pipeline areas following the price shock, consistent with localized economic gains likely linked to participation in or proximity to illicit fuel markets. On the stress side, hospitalizations for stress-related conditions among women of reproductive age remain largely unchanged, and suicides across multiple demographic groups also remain stable, suggesting limited acute psychological deterioration during this period.

Taken together, these findings suggest that in contexts of chronic violence, populations may exhibit adaptive non-state compliant responses that mitigate health harms, particularly when economic conditions improve. However, such adaptation occurs at a high social cost, as evidenced by rising homicides.

These results have important policy implications: militarized crackdowns on organized crime risk undermining community welfare if they sever critical (albeit illicit) income streams without providing legal economic alternatives. Effective conflict and public health strategies must therefore balance enforcement with investments in formal livelihoods and local development.

More broadly, these findings challenge assumptions that violence uniformly deteriorates health and highlight the need for policy responses that account for both the costs of persistent conflict and the rational economic choices households make in fragile state settings. Future research should strengthen micro-level evidence on household coping and illicit income flows to better inform integrated security, health, and labor market interventions.

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Fuel Prices, Gang Violence, and Its Effects on Children's Health: Evidence from Mexico
Online Appendix

Fernanda Gutierrez Amaros

A Data sources

Table 1: Outcome variables

Type of Data	Outcome	Source (INEGI)
Homicides	All homicides	Mortality registers
	Male homicides	
	Firearm homicides	
	Young men (14 to 44) homicides	
	Male and firearm homicides	
	Young male homicides with firearm	
	Female homicides	
	Female in their fertile age (14 to 49) homicides	
Fetal mortality	Rate of prenatal deaths	Fetal death statistics
	Rate of complications during pregnancy	
	Rate of stillbirths	
	Rate of death during birth	
	Gestational age	
	Weight	
	Rate of death of male fetus	
	Rate of miscarriages	
	Prenatal care (number of visits)	
Birth certificates	Birth weight	Birth certificates (SINAC)
	Gestational age	
	Percentage of low birth weight (less than 2500g)	
	Percentage of very low birth weight (less than 1500g)	
	Percentage of macrosomia (more than 4000g)	
	Prenatal care (number of visits)	
	Percentage premature	
	Apgar	
	Low Apgar (below 7)	
	Fertility rate	
	Sex ratios	
	Percentage of teenage mothers	
Mortality Records	Infant Mortality (less 1 year)	Mortality records
	Child mortality (1 to 5)	
	Suicides	
	Maternal mortality	

Outcome data sources related to violence and health indicators

Table 2: Outcome variables

Type of Data	Outcome	Source (INEGI)
Stress related hospital discharges	Anxiety Disorders	Hospital discharges
	Substance Use Disorders	
	Heart Diseases	
	Cerebrovascular Diseases	
Medical procedures	EEG monitoring	Hospital discharges
	Phimosis surgery	
	Appendectomy	
	Fracture repair	
	Hernia repair	
	Cholecystectomy	
	Hysterectomy	
	Prostatectomy	
	Hip replacement	
	Circumcision	
	Neonatal intubation	
Obstetric hospital discharges	Birth rate	Hospital discharges
	C-sections rate	
	Dystocic rate	
	Contraceptive rate	
	Abortion rate	

Outcome data sources related to hospital discharges

Table 3: Outcome variables

Type of Data	Outcome	Source
Satellite data	VIIRS	Colorado School of Mines
	Harmonized DMSP/OLS	Li, et al., 2020
Geospatial data	GDPP	Kummu et al., 2025
	Population	Liu et al., 2024

Outcome data sources related to spatial sources

B Population Adjustment Methodology

Population data by municipality are limited to the availability of the Mexican Census every 10 years and an Intercensal Census (Encuesta Intercensal) in 2015. To estimate the population for 2016, 2017 and 2018, I used linear interpolation using census data from 2015 and 2020. Data on the total number of women by municipality and in their fertile window (15 to 49 years) and young men (14 to 44 years) come from the Intercensal Census 2015. I obtained the percentages of women per municipality from this sample and imputed them with the same linear interpolation method. To improve my population measurements, I subtracted the number of homicides from monthly population counts. Thus, for all the municipalities' populations, I subtracted all homicides; for the female population, I subtracted the number of all female homicides; and for women in their reproductive age counts, I subtracted female homicides in the same range of age.

C Threats to identification

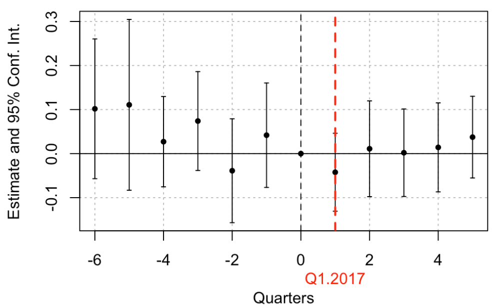
One potential threat to the validity of my previous analysis is the assumption that mothers changed their behavior in response to the price increase and subsequent violence, which could influence the relationship between health and local violence. This behavior change could be selective fertility or migration to "safer" places. I tested these threats using an event study of fertility and satellite data that proxy population counts.

C.1 Fertility

I challenge my results by analyzing fertility over time using event-study type analysis and difference-in-differences estimates. I calculate the fertility rate by using the total number of births divided by the number of women of fertile age in each municipality. The event study and the DiD show no effects or pretrends on fertility, suggesting that this variable did not change during the study period, conditional on all the fixed effects.

I conducted the same analysis on a subsample of women with undergraduate education or higher (almost 15% of the sample), who typically have better adherence to birth control and are more likely to relocate (Aldeco et al., 2022 [5]). This subsample showed a slight decrease statistically different from zero in the point estimate in April 2017 (see Figure 16b).

(a) Effects of the price increase on fertility rate



(b) Effects of the price incr. on fert. rate of highly edu.

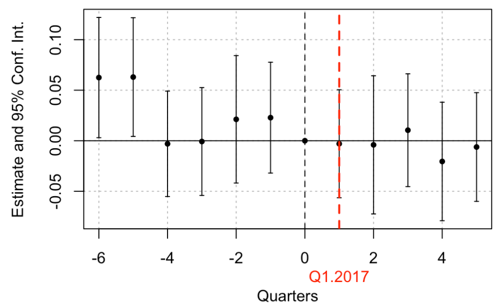


Figure 16: Results from estimating equation (1), where the dependent variable is the fertility rate (number of births divided by the number of women of fertile age), and a subsample focusing on women with higher educational attainment (completed undergraduate education or more). The analysis covers the period from the third quarter of 2015 to the second quarter of 2018. Data are drawn from INEGI birth certificate records and adjusted population counts. The X-axis shows calendar quarters, with the fourth quarter of 2016 serving as the reference period—just before the fuel price increase in the first quarter of 2017. The plot displays the estimated β_k coefficients, which capture changes in fertility rates relative to the reference period. All models include time and municipality fixed effects. 95% confidence intervals are based on standard errors clustered at the municipality level

As mentioned above, the DiD estimates are not statistically significant for overall fertility. However, for highly educated women (15% of the sample), there is a decrease in fertility that is statistically significant but mostly driven by pretrends.

Table 4: Effects of the price increase in fertility

	(1)	(2)
	Fertility	Highly educated fert.
DD price inc.*pipeline	-0.0490	-0.0365***
	(0.0430)	(0.0088)
Mean	3.99	.38
Munic. F.E.	Yes	Yes
Time F.E.	Yes	Yes
Adjust R2	0.768	0.732
N	11075	11075
AIC	23502.4	-7142.0

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by month and year), I replace them by an $\mathrm{AfterPrice}_{\mathrm{t}}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are fertility rate (number of births divided by the number of women of fertile age), and a subsample focusing on women with higher educational attainment (completed undergraduate education or more). I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

C.2 Migration

There is a lack of data documenting internal migration in Mexico. The Mexican census, conducted every ten years, shows that in 2020 only 4% of the population reported moving due to security concerns. Still, they do not ask for more information, such as the exact origin and final locations. Additionally, population data is limited since it is contained at the municipal level in the decennial Censuses (2010 and 2020) and the Intercensal Survey (2015). In previous sections, I used census data sources to impute population to estimate the rates of different outcomes, but these data falls short of providing a clearer picture regarding migration.

Due to the lack of administrative data, I use a geospatial strategy to revise possible internal migration close to the pipelines. This approach provides a glimpse of population dynamics beyond municipal administrative borders. If violence influences migration decisions, a decline in population close to pipelines would be expected.

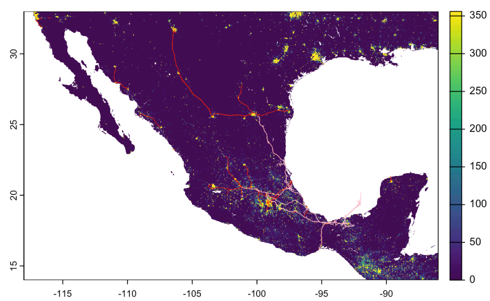
I analyzed the spatial distribution of populations near polyduct and oil pipelines in Mexico using GeoTIFF population data from the GlobPOP (Liu et al., 2024 [64]), a gridded dataset with 30 arc-second resolution, available in population count and density formats.

To estimate the population near the pipelines, I created buffers at distances of 1 km, 5 km, 10 km, 15 km, and 20 km around the pipelines and applied them to the GlobPOP dataset. Using the population counts per grid, I calculated the mean population per grid cell within each buffer.

After determining the area of each buffer, I use these mean values to estimate the total population in each proximity zone[14].

Figure 17a and Figure 17b show the population distribution in 2016 and 2017 were it is observe that the pipeline network in central Mexico passes by important urban centres such as part of Mexico City whereas the Northern part of the country is less populated.

(a) Gridded population 2016



(b) Gridded population 2017

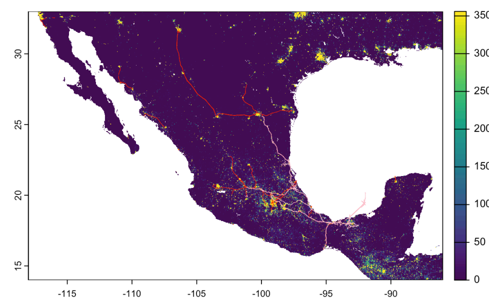
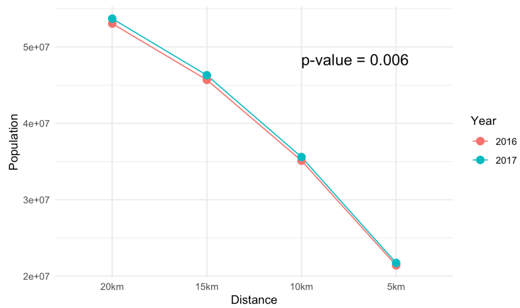


Figure 17: Population Distribution across Mexico in 2016 and 2017 (Rescaled for Mexico's minimum and maximum values per grid cell), based on GlobPOP global gridded population data with a 30-arcsecond (1 km²) resolution. The dataset provides population counts for each grid cell. Source: GlobPOP Global gridded population) from Liu et al.,2024. Red pipelines correspond to polyducts, and pink are oil pipelines.

As mentioned, the main concern of internal migration is that the population, particularly women, are migrating to "safer" places to avoid stress exposure due to homicides and therefore affecting the estimates. I estimated the average population counts per bandwidth and found that places close to pipelines are very populated. The average population in the 5 kilometre bandwidth (so in a vicinity of 10 kilometres) there is 21 million inhabitants for polyducts and 8 million inhabitants for oil pipelines. I also found that the population slightly increased in places closer to both pipelines from 2016 to 2017.

[14]To improve accuracy, I address overlapping buffers by merging them into non-overlapping areas and recalculating population sums for these adjusted zones.

(a) Polyduct estimated population counts



(b) Oil estimated population counts

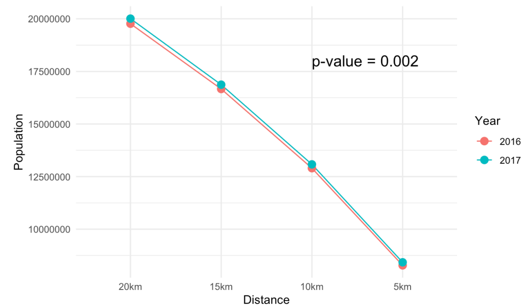


Figure 18: Average population counts per type of pipeline (polyduct and oil), estimated by calculating the mean population per grid cell within each buffer zone (5 to 20 kilometres) based on GlobPOP global gridded population data with a 30-arcsecond (1 km²) resolution. The dataset provides population counts for each grid cell. Source: GlobPOP Global gridded population) from Liu et al.,2024.

The population increase from 2016 to 2017 is statistically significant for both types of pipelines. This growth may be attributed to Mexico's demographic trends during that period, with a national growth rate of 1.1% and an urban growth rate of 1.4%, and not due to internal migration. To investigate this hypothesis, I compared the actual population counts for 2017 with the expected counts based on two growth rates: the national growth rate of 1.1% and the urban growth rate of 1.4%.).

Based on the results shown in Table 5, it can be observed that the actual populations for both the national and urban areas were quite close to the expected values based on the growth rates of 1.1% for national and 1.4% for urban populations. This suggests that the growth rates for estimating the expected populations were reasonable and reflective of actual demographic trends. This conclusion is further supported by earlier results indicating that fertility and birth rates have not increased.

Table 5: Comparison of Actual and Expected Population Counts Around Pipelines

Bandwidth (km)	2016 Population	2017 Population	Expected Growth Comparison		Urban Expected Growth Comparison	
			Expected (1.1%)	Diff. (%)	Expected (1.4%)	Diff. (%)
Polyduct Pipelines						
20	53,091,857	53,728,963	53,708,917	+0.04	53,836,142	-0.20
15	45,750,510	46,330,688	46,257,766	+0.16	46,388,017	-0.12
10	35,219,548	35,659,912	35,606,964	+0.15	35,712,617	-0.15
5	21,435,107	21,740,672	21,671,893	+0.32	21,735,188	+0.03
1	7,753,152	7,879,334	7,838,437	+0.52	7,861,708	+0.22
Oil Pipelines						
20	19,755,573	19,985,602	19,974,934	+0.05	20,033,175	-0.24
15	16,636,390	16,852,645	16,819,449	+0.20	16,869,295	-0.10
10	12,888,700	13,066,739	13,030,476	+0.28	13,069,159	-0.02
5	8,253,942	8,401,371	8,344,736	+0.68	8,369,497	+0.38
1	3,510,639	3,595,207	3,549,256	+1.29	3,559,790	+0.99

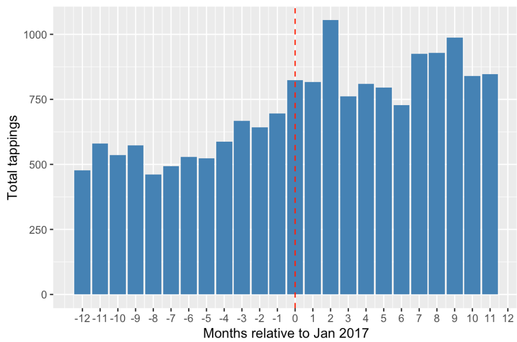
Comparison of population estimates for different bandwidths around pipelines. It contrasts the actual population counts for 2016 and 2017 with the expected counts for 2017 based on two growth rates: the national growth rate of 1.1% and the urban growth rate of 1.4%. The percentage differences between the actual and expected populations are provided, highlighting how closely the observed values align with demographic expectations.

Furthermore, the consistency of the results across different bandwidths (20km, 15km, 10km, 5km and 1km) indicates that the growth patterns are stable regardless of the distance from the pipelines. This uniformity might suggest that the impact of pipelines on population growth or internal migration is minimal. However, it is important to note that the location of the pipeline is quite heterogeneous since it passes populated areas, such as central Mexico and historically empty ones, such as the northern part of the country.

C.3 Anticipation

I examine whether there was any anticipatory behavior prior to the 2017 fuel price hike that could undermine the causal interpretation of the results. Figure 19a presents the monthly tapping activity before and after the policy change; there is no evidence of a pretreatment increase.

(a) Tappings



(b) Sold barrels

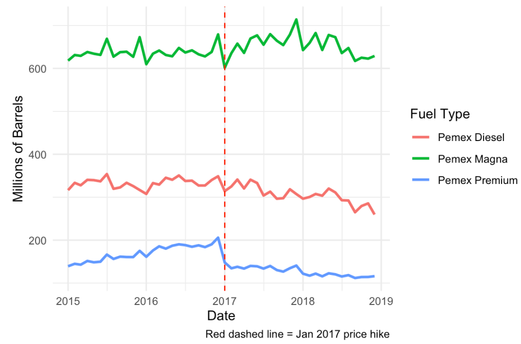


Figure 19: Panel (a) presents monthly data on illegal pipeline tappings, obtained through a public information request (number 1857200094319). Panel (b) displays monthly data on barrels sold, sourced from PEMEX records.

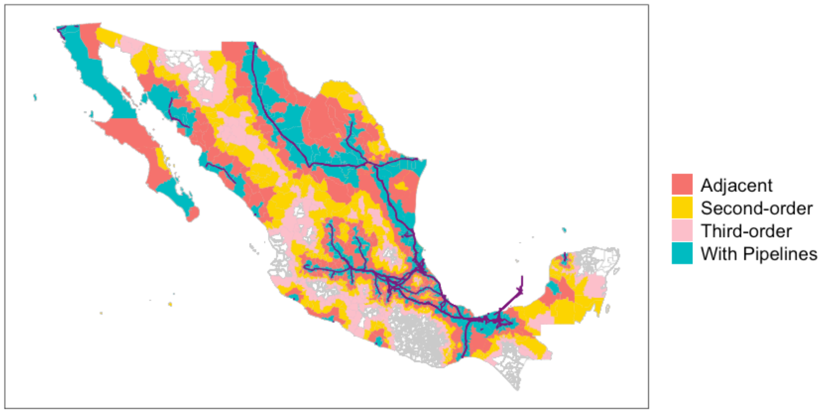
Furthermore, PEMEX data on barrels show no unusual spike in legal fuel purchases in the months leading up to the price change (Figure 19b). The analysis includes legal sales of Pemex fuel—Magna, Premium, and Diesel [15].

C.4 Spillovers

To test for spatial spillover of homicides, I follow the DiD estimate outlined in Equation (1) and redefine the treated group as those neighboring municipalities with pipelines and compare them with the second and third order neighbors (Figure 20).

[15] Magna, the most widely used gasoline by everyday drivers, Premium, consumed primarily by luxury vehicles, and Diesel, used primarily in the commercial and agricultural sectors, each represent different segments of fuel demand and are transported through pipelines

Figure 20: Different control and treatment groups



Notes: Information from the location of the pipelines was extracted from the CartoCritica map.

The results show no statistically significant effects when comparing first-order neighbors to second-order neighbors, and similarly no significant effects when comparing second-order to third-order neighbors, indicating that spillover effects do not extend beyond the immediate vicinity of pipeline municipalities. While the comparison between first and third-order neighbors shows significant effects, the absence of effects in the second vs third-order comparison, combined with the presence of differential pretrends, suggests these results reflect pre-existing differences between these groups rather than spillover effects. These findings confirm that first-order neighbors provide a suitable control group, as they are not contaminated by spillover effects from homicides in municipalities with pipelines.

Table 6: Spillover Analysis: First vs Second-order Neighbors

	(1) All	(2) Male	(3) Firearm	(4) Young male	(5) Fire. young male	(6) Female	(7) Fem fert.
After × First-order	0.895	0.799	0.800	0.710	0.611	0.0962	0.0666
	(0.511)	(0.471)	(0.447)	(0.381)	(0.338)	(0.0684)	(0.0508)
Mean	5.440	4.839	3.646	3.527	2.584	0.587	0.420
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.780	0.770	0.704	0.749	0.689	0.520	0.448
N	4,799	4,799	4,799	4,799	4,799	4,799	4,799
AIC	28,527.4	27,759.9	27,142.4	25,578.8	24,412.6	12,140.4	10,161.7

Clustered Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\text{AfterPrice}_\text{t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are all homicides accounted together, male homicides, firearm homicides, youn gamle homicides, young male homicides with firearm, female homicides, and homicides of women in their childbearing years. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 7: Spillover Analysis: Second vs Third-order Neighbors

	(1) All	(2) Male	(3) Firearm	(4) Young male	(5) Fire. young male	(6) Female	(7) Fem fert.
After × Second-order	0.194	0.110	0.340	0.0852	0.222	0.101	0.0683
	(0.442)	(0.409)	(0.352)	(0.325)	(0.265)	(0.0629)	(0.0468)
Mean	4.285	3.818	2.827	2.712	1.966	0.448	0.318
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.714	0.700	0.676	0.672	0.639	0.361	0.307
N	3,459	3,459	3,459	3,459	3,459	3,459	3,459
AIC	18,553.9	18,046.8	16,986.8	16,538.3	15,354.8	7,571.6	6,198.6

Clustered Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\text{AfterPrice}_\text{t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are all homicides accounted together, male homicides, firearm homicides, youn gamle homicides, young male homicides with firearm, female homicides, and homicides of women in their childbearing years. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 8: Spillover Analysis: First vs Third-order Neighbors

	(1) All	(2) Male	(3) Firearm	(4) Young male	(5) Fire. young male	(6) Female	(7) Fem fert.
After × First-order	1.089*	0.910*	1.135**	0.795*	0.832*	0.194**	0.132*
	(0.505)	(0.458)	(0.435)	(0.377)	(0.325)	(0.0741)	(0.0535)
Mean	5.448	4.841	3.647	3.500	2.561	0.590	0.417
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-sq	0.771	0.763	0.688	0.737	0.672	0.516	0.433
N	4,434	4,434	4,434	4,434	4,434	4,434	4,434
AIC	26,528.7	25,778.2	25,232.5	23,822.7	22,714.6	11,275.3	9,410.6

Clustered Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\text{AfterPrice}_\text{t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are all homicides accounted together, male homicides, firearm homicides, youn gamle homicides, young male homicides with firearm, female homicides, and homicides of women in their childbearing years. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

D Robustness Checks

To assess whether the findings are robust to alternative explanations, I analyze medical procedures, obstetric hospital discharges, suicides and maternal mortality. To show that fuel theft had economic spillovers I used the downscaled global GDP per capita (PPP) dataset by Kummu et al. (2025[58]).

D.1 Medical procedures

To assess whether the main results might be driven by broader changes in healthcare supply or unrelated health shocks, rather than by effects specific to prenatal health, I follow recent literature (Guidetti et al., 2021 [49]; Deryugina et al., 2019 [34]) and analyze the frequency of selected surgical procedures as placebo outcomes. Furthermore, I evaluate a different surgeries that vary in urgency and demographic specificity to detect any systematic changes in hospital capacity or access. Table 9 provides details on the selected procedures.

Procedure	ICD-9-CM Code(s)	Type	Notes
EEG monitoring	8914, 8919	Elective	Used for epilepsy diagnosis or evaluation; non-invasive, not sex-specific
Phimosis surgery	640, 641	Elective	Male-only procedure; removal of foreskin (circumcision or correction)
Appendectomy	470	Urgent	Common emergency surgery for acute appendicitis; not sex-specific
Fracture repair	79∗	Urgent	Broad class of procedures for bone fracture fixation; not sex-specific
Hernia repair	530∗	Elective	Often elective unless complicated (e.g. strangulated hernia); not sex-specific
Cholecystectomy	5122, 5123	Mostly elective	Gallbladder removal; typically elective but can be urgent in acute cholecystitis; not sex-specific
Hysterectomy	683–689	Elective	Female-only procedure; removal of uterus for various reasons (fibroids, cancer, etc.)
Prostatectomy	602, 605, 606	Elective	Male-only procedure; for prostate enlargement or cancer
Hip replacement	8151	Elective	Total hip replacement; usually elective, for degenerative disease or injury; not sex-specific
Circumcision (neonatal)	640, 641	Elective	Male-only neonatal procedure; foreskin removal, typically planned and non-urgent
Neonatal intubation	9604	Urgent	Emergency airway management in newborns; critical, life-saving procedure

Table 9: ICD-9-CM surgical procedures used as placebo outcomes

Elective and non-urgent procedures—such as EEG monitoring, phimosis surgery, hernia repair, hysterectomy, prostatectomy, and hip replacement—can be postponed when healthcare systems face strain. Meanwhile, urgent procedures like appendectomies or fracture repairs are less likely to be rescheduled. I classified these procedures using ICD-9-CM codes, detailed in Tables 10 and 11, and standardized their incidence by the total population or by sex-specific subpopulations (e.g. men or women) per 100,000 inhabitants. Furthermore I add an elective-non-urgent procedure and an urgent procedure specifically for newborns, which are circumcision and neonatal intubation, respectively.

Table 10: Regression table Part 1 with the effects of the price increase on elective and urgent medical procedures

	(1)	(2)	(3)	(4)
	eeg_monitoring	phimosis_surgery	appendectomy	hernia_repair
DD price	12.36	76.11	164.9	-91.47
inc.*pipeline	(16.53)	(69.50)	(178.7)	(168.4)
Mean	34.00	367.81	2657.89	1785.13
Munic. F.E.	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes
Adjust R2	0.011	0.021	0.144	0.088
N	10832	10832	10832	10832
AIC	160224.4	192144.2	213286.9	210666.1

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables are from INEGI hospital discharge data on surgical procedures. The outcome variables are rates (per 100,000 inhabitants) of the following procedures: EEG monitoring, phimosis surgery, appendectomy, fracture repair, and hernia repair. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 11: Regression table Part 2 with the effects of the price increase on elective and urgent medical procedures

	(1) cholecystectomy	(2) Hysterectomy	(3) prostatectomia	(4) hip_replace
DD price inc.*pipeline	69.97	0.0661	0.0426	-11.42
	(226.9)	(0.0694)	(0.0502)	(24.80)
Mean	3630.78	0.76	0.26	69.98
Munic. F.E.	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes
Adjust R2	0.167	0.233	0.059	0.037
N	10832	11090	11090	10832
AIC	216120.6	41474.1	35204.3	168856.3

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables are from INEGI hospital discharge data on surgical procedures. The outcome variables are rates (per 100,000 inhabitants) of the following procedures: Cholecystectomy, hysterectomy prostatectomy, and hip replacement. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 12: Regression table Part 3 with the effects of the price increase on elective and urgent medical procedures

	(1) perc_circumcision	(2) perc_neonatal_intubation
DD price inc.*pipeline	76.11	-24.55
	(69.50)	(26.72)
Mean	367.80	42.45
Munic. F.E.	Yes	Yes
Time F.E.	Yes	Yes
Adjust R2	0.021	0.055
N	10,832	10,832
AIC	192144.2	172821.8

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are from INEGI hospital discharge data on surgical procedures. The outcome variables are rates (per 100,000 inhabitants) of the following procedures: circumcision and neonatal intubation. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

D.2 Obstetric hospital discharges

To assess changes in healthcare utilization and reproductive behavior, I examine obstetric hospital discharges among women of reproductive age (15–49). The dataset includes diagnoses such as C-sections, abortions, dystocic births, and contraceptive procedures, categorized using ICD codes based on the primary condition treated at discharge. These records cover discharges from general hospitals and specialized hospitals. I construct quarterly rates per 1,000 women by aggregating discharges at the municipality level and normalizing by the population of women in this age group. This data provides insight into whether exposure to violence influences fertility-related health behaviors or medical decision-making. The contraceptive rate, consists of hospital appointments for contraceptives such as oral hormonal, monthly and bimonthly injectable, subdermal implants, intrauterine devices (IUD), female condoms, male condoms, and medicated IUDs.

Table 13: Regression table with the effects of the price increase in obstetric hospital discharges

	(1) Abortion	(2) Contracep.	(3) C-sect	(4) Dystocic	(5) Birth
DD price inc.*pipeline	0.000501	-0.0225	0.00652	-0.00163	0.0729
	(0.00717)	(0.0463)	(0.0222)	(0.00417)	(0.0494)
Mean	0.205	1.182	0.692	0.021	1.907
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.352	0.656	0.569	0.257	0.686
N	11100	11100	11100	11100	11100
AIC	-11053.7	17833.8	6316.6	-29370.7	22714.6

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables are obstetric hospital discharges. The data was obtained from INEGI. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

(a) Effects of the price increase on birth rate among fertile women

(b) Effects of the price increase on C-section rate among fertile women

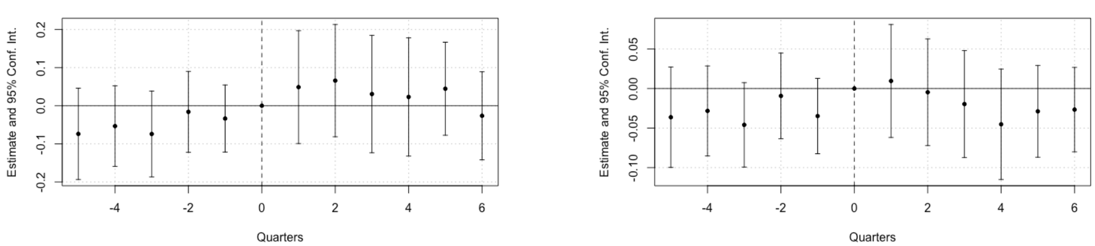
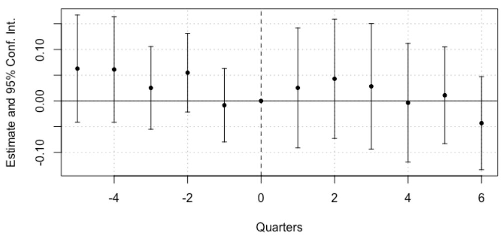


Figure 21: Results from estimating equation (1), where the dependent variable is the rate of births and C-sections from the third quarter of 2015 to the second quarter of 2018. The data source is the obstetric hospital discharges records from INEGI. The X-axis represents the quarters. The reference period is the last quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the change in the rate of each outcome. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

It is also noticeable that births, C-sections, and abortions remained constant during the study period which coincides with the lack of statistically significant changes in fertility (Figure 16a) within the study's time frame. Previous studies by Brown (2018 [17]) and Floridi et al. (2023 [41]) also found no evidence of selective fertility due to violence; nevertheless, these studies used data from the MxFLS covering the same period (2002-2009) across the entire country.

(a) Effects of the price increase on contraceptive rate among fertile women



(b) Effects of the price increase on dystocic birth rate among fertile women

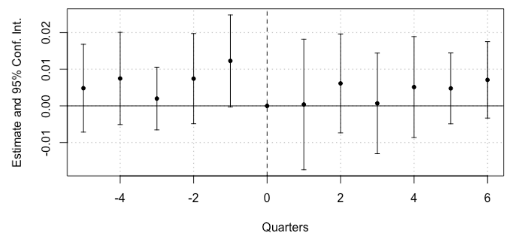
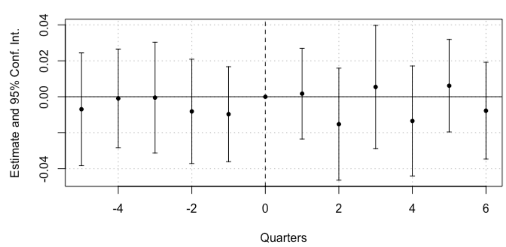


Figure 22: Results from estimating equation (1), where the dependent variable is the rate of contraceptive use and dystocic births from third quarter of 2015 to the second quarter of 2018. The data source is the obstetric hospital discharges records from INEGI. The X-axis represents the quarters. The reference period is the last quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the change in the rate of each outcome. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

Figure 23: Effects of the price increase on abortion rate among women



Notes: Results from estimating equation (1), where the dependent variable is the abortion rate from the third quarter of 2015 to the second quarter of 2018. The data source is the obstetric hospital discharges records from INEGI. The X-axis represents the quarters. The reference period is the last quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the change in the rate of each outcome. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

D.3 Maternal mortality

To rule out selective survival of women during pregnancy or childbirth as a potential mechanism, I examine maternal mortality using INEGI's mortality records. Specifically, I estimate event study and difference-in-differences models to assess whether the 2017 fuel price shock affected maternal deaths. I find no statistically significant effects on maternal mortality. As with other outcomes, I include fixed effects to control for time-invariant municipal characteristics and seasonal variation, helping to mitigate bias due to unobserved population changes.

Table 14: Regression table with the effects of the price increase on maternal mortality

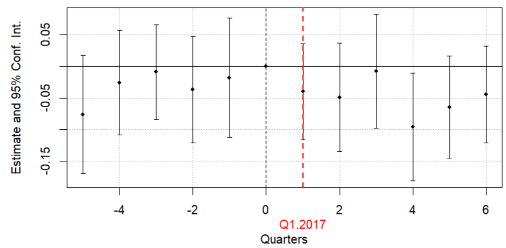
	(1) Maternal mort.
DD price incr.*pipeline	-0.0227 (0.0167)
Mean	.183
Municipality F.E.	Yes
Time F.E.	Yes
Adjust R2	11008
AIC	11520.9

Clustered Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Result from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an AfterPrice_t indicator, which takes the value of one at the first quarter of 2017. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Figure 24: Effects of the price increase in maternal mortality



Notes: Results from estimating equation (1), where the dependent variable is maternal mortality from INEGI's mortality population counts from the third quarter of 2015 to the second quarter of 2018. The X-axis represents the quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of January 2017. The plot shows the β_k coefficients that capture the number of homicides. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

D.4 Suicides

In addition to analyzing stress-related hospital discharges, I examine suicides as an extreme manifestation of psychological distress. Using INEGI mortality records, I estimate the impact of the fuel price shock on suicide rates across several subpopulations: women of reproductive age (15–49), all women, young men (ages 14–45), all men and the overall population.

I apply both event study and difference-in-differences models, using the same empirical strategy as for other mortality outcomes. Across all specifications, I find no statistically significant effects on suicides following the shock. These null effects provide further evidence against severe psychological deterioration as a mechanism. As with other analyses, I include municipality and time fixed effects to account for unobserved heterogeneity and seasonality.

Table 15: Regression table with the effects of the price increase in suicides

	(1) Suicide	(2) Men_suicide	(3) Women_suicide	(4) Young_men_suicide	(5) Fertilewomen
DD price	0.0253	0.00107	0.0235	-0.0239	-0.00239
inc.*pipeline	(0.0624)	(0.0523)	(0.0203)	(0.0432)	(0.00956)
Mean	1.284	1.044	0.240	0.711	0.041
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.871	0.852	0.621	0.811	0.223
N	11008	11008	11008	11008	11008
AIC	35964.5	32996.0	15722.4	28167.9	-4624.3

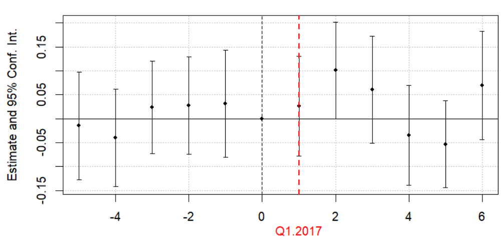
Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes:Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables are suicides, male suicides, female suicides, suicides of young men (14 to 44) and suicides of women in their childbearing years (15-49). The data was obtained from INEGI mortality records from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

I report event study estimates for all women and for women of reproductive age. Results for additional subgroups are available upon request.

(a) Effects of the price increase on suicides among women



(b) Effects of the price increase on suicides among fert. women

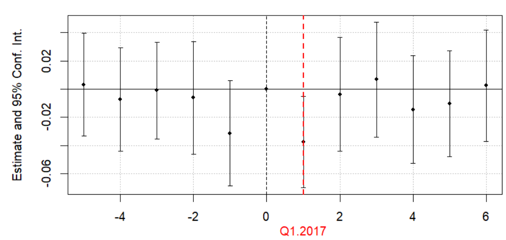


Figure 25: Results from estimating equation 1, where the dependent variables are suicides amongst women and women in their childbearing years from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's mortality records. The X-axis represents quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the changes in each type of suicide per quarter. I control for time and municipalities' fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

D.5 GDP per Capita (PPP)

As a robustness check, I use the downscaled global GDP per capita (PPP) dataset by Kummu et al. (2025 [58]) to examine income changes near pipelines. This high-resolution dataset, covering 43,501 admin-2 units worldwide, employs advanced machine learning techniques with strong predictive accuracy. Annual GDP estimates are available in gridded and polygon formats at multiple spatial levels.

Unlike light intensity data, GDP per capita tends to increase with distance from the pipeline, likely because larger buffers include more populous and wealthier areas. Nonetheless, both polyduct and oil pipeline zones show an overall increase in average GDP per capita from 2016 to 2017 (see Figures 26a and 26b), indicating regional economic growth.

For instance, within 1 km of the polyduct pipeline, GDP per capita increased by approximately 2.2% between 2016 and 2017[16]. However, as these buffers are nested cumulative averages, it is not possible to precisely estimate income changes by narrower distance bands or gradients without more granular data.

(a) GDP polyduct   *(b)* GDP oil

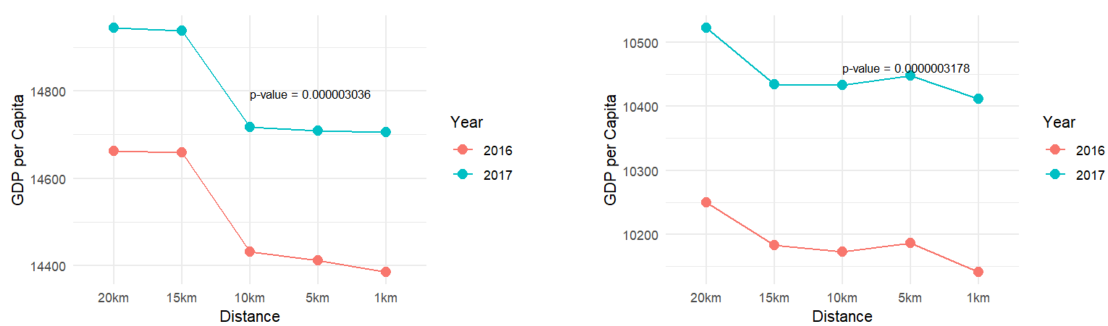


Figure 26: Average GDP per bandwidth for both type of pipelines: oil and polyduct. Data from the downscaled global GDP per capita (PPP) dataset developed by Kummu et al. ([58]) .

E Homicides results

This section shows the results from the difference-in-differences estimates in all homicides, male homicides, homicides with a firearm, young male homicides, homicides of young males with a firearm, female homicides and females in their fertile age (15 to 49). The interaction coefficients are all statistically significant except for all women homicides. The percentage of increase spans from almost 30% to 34.63%.

[16] from \$14,385 to \$14,706) expressed in international dollars at purchasing power parity (Int\$PPP)

Table 16: Regression table with the effects of the price increase in homicides

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Male	Firearm	Young male	Fire. young male	Female	Fem fert.
DD price	2.957*	2.709*	2.311*	2.108*	1.670*	0.220	0.231**
incr.*pipeline	(1.187)	(1.085)	(0.987)	(0.880)	(0.752)	(0.114)	(0.0832)
Mean	9.87	8.75	6.67	6.53	4.81	1.08	.746
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.848	0.845	0.806	0.836	0.799	0.751	0.720
N	6154	6154	6154	6154	6154	6154	6154
AIC	45469.8	44315.4	43246.7	41300.8	39591.3	21291.6	17991.5

Clustered Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\text{AfterPrice}_\text{t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are all homicides accounted together, male homicides, firearm homicides, youn gamle homicides, young male homicides with firearm, female homicides, and homicides of women in their childbearing years. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Furthermore, I include the event studies of the rest of the types of homicides that serve as a robustness check of how the price increase increased homicides in places with pipelines in comparison to their first-order neighbors.

(a) Effects of the price increase male homicides

(b) Effects of the price increase hom. firearm

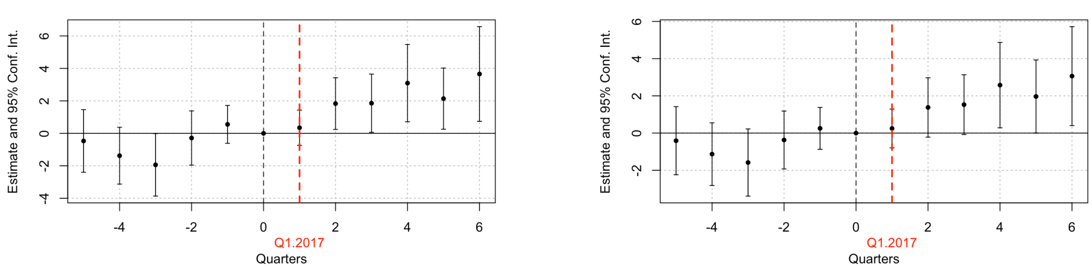
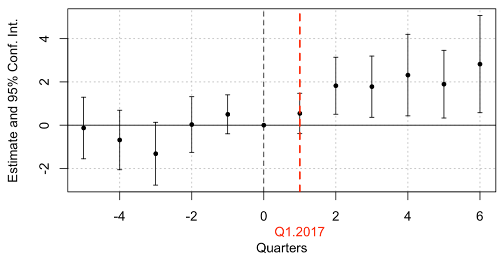


Figure 27: Results from estimating equation (1), where the dependent variables are the number of male homicides and firearm homicides from INEGI's mortality population counts from the third quarter of 2015 to the second quarter of 2018. The X-axis represents the quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of January 2017. The plot shows the β_k coefficients that capture the number of homicides. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

(a) Effects of the price increase young male homicides



(b) Effects of the price increase female homicides

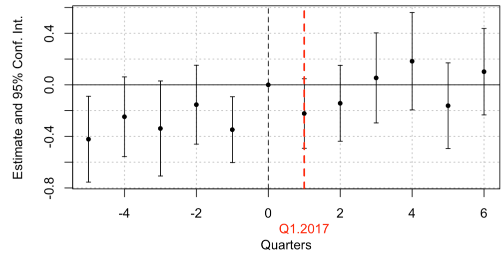
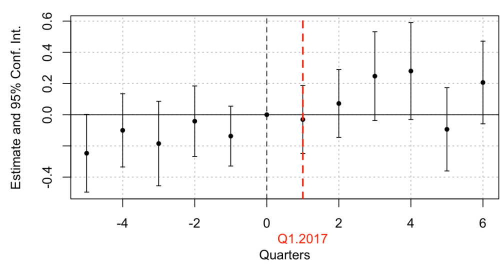


Figure 28: Results from estimating equation (1), where the dependent variables are the number of young male (14 to 44 years old) homicides and female homicides from INEGI's mortality population counts from the third quarter of 2015 to the second quarter of 2018. The X-axis represents the quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of January 2017. The plot shows the β_k coefficients that capture the number of homicides. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

Figure 29: Effects of the price increase female in their fertile age homicides



Notes: Results from estimating equation (1), where the dependent variables are the number of female in their childbearing years homicides from INEGI's mortality population counts from the third quarter of 2015 to the second quarter of 2018. The X-axis represents the quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of January 2017. The plot shows the β_k coefficients that capture the number of homicides. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

F Health outcomes results

In this section, I present the results from the difference-in-differences estimates and event studies for prenatal outcomes from birth certificates.

The DiD estimates are split into two different tables.

F.1 Birth certificate outcomes

Table 17: Regression table part 1 with the effects of the price increase on birth outcomes

	(1) Birth weight	(2) % low bw	(3) % vlbw	(4) % macros	(5) Gest. age	(6) % premature
DD price	2.303	0.224	0.0261	-0.0223	-0.0157	0.0799
inc.*pipeline	(2.922)	(0.137)	(0.0444)	(0.124)	(0.0136)	(0.157)
Mean	3149.92	5.94	0.60	2.8	38.77	6.22
Munic. F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.464	0.121	0.024	0.166	0.284	0.105
N	10844	10844	10844	10844	10844	10844
AIC	124882.3	58331.8	33162.1	54592.9	5000.6	60717.2

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. Our outcome variables are birth weight, percentage of low birth weight, percentage of very low birth weight, percentage with macrosomia, average gestational age and percentage of premature babies. The data was obtained from the birth certificate registers collected by INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 18: Regression table part 2 with the effects of the price increase in birth outcomes

	(1)	(2)	(3)	(4)	(5)
	% teen	Prenatal care	Sex ratio	Apgar	% low Apgar
DD price	-0.179	-0.0365	-0.0341	0.00323	-0.0462
inc.*pipeline	(0.302)	(0.0263)	(1.497)	(0.00762)	(0.0869)
Mean	20.79	7.53	99.59	8.89	0.91
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.242	0.685	0.020	0.219	0.141
N	10844	10844	10844	10844	10844
AIC	73298.5	17591.9	109348.2	-10353.9	43147.8

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables are percentage of teenage mothers, average prenatal care, sex ratio, average Apgar score, and percentage of low Apagar score (below 7). The data was obtained from the birth certificate registers collected by INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

In this section, I report the event studies with null effects or those that presented pretrends.

(a) Effects of the price increase on % very low birth weight

(b) Effects of the price incr. on % of macrosomia

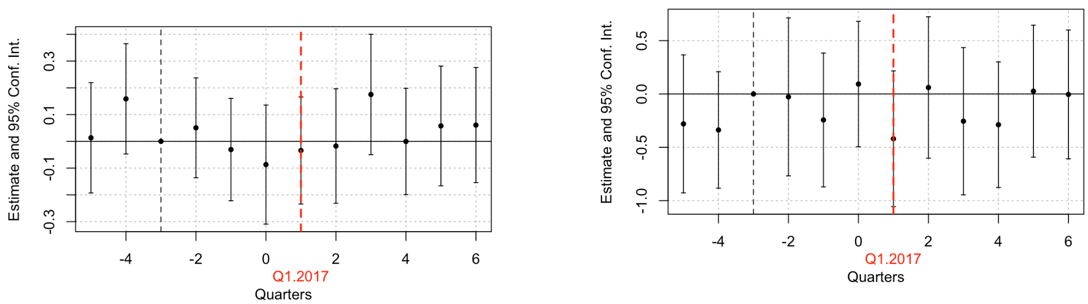


Figure 30: Results from estimating equation (1), where the dependent variables are the percentage of very low birth weight (vlbw) and percentage of babies with macrosomia from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's birth certificates dataset. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the change in the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

(a) Effects of the price increase on Apgar

(b) Effects of the price incr. on % low Apgar

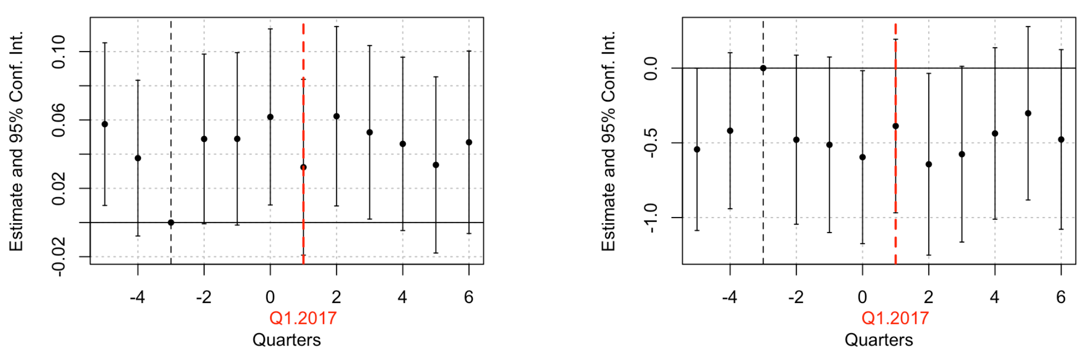
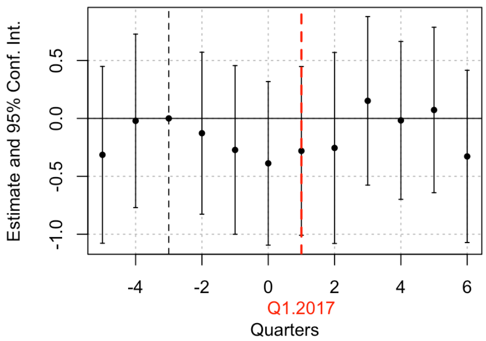


Figure 31: Results from estimating equation (1), where the dependent variables are the percentage of Apgar score and percentage of low Apgar (7 or less) from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's birth certificates dataset. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the change in the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

(a) Effects of the price increase on the percentage of premature babies



(b) Effects of the price incr. on average prenatal care

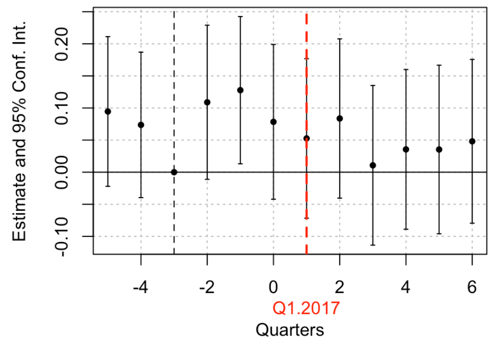


Figure 32: Results from estimating equation (1), where the dependent variables are the percentage of premature babies and average prenatal care (total number of visits) from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's birth certificates dataset. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the change in the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

(a) Effects of the price increase on mother's age

(b) Effects of the price incr. on % teen mothers

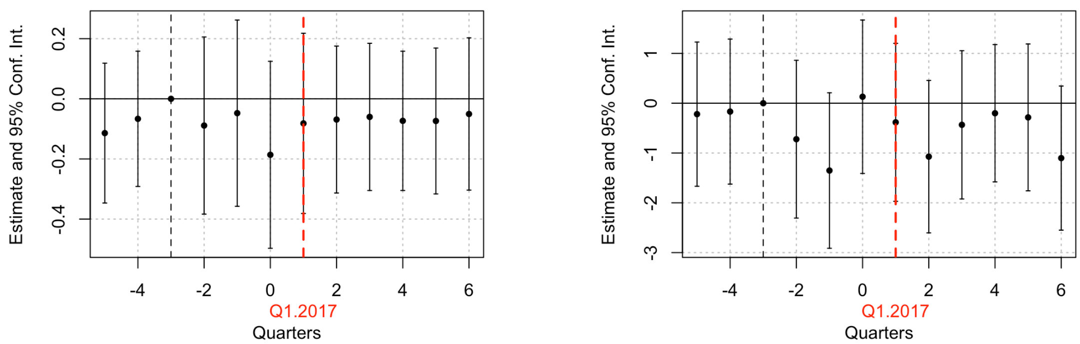
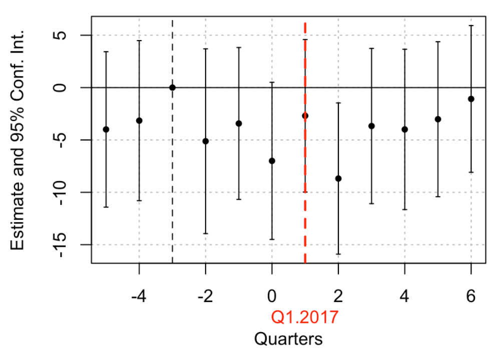


Figure 33: Results from estimating equation (1), where the dependent variables are the average mother's age and percentage of teenage mothers (less than 20 years old) from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's birth certificates dataset. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the change in the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

Figure 34: Effects of the price increase on sex ratio



Notes: Results from estimating equation (1), where the dependent variable is sex ratio (birth of girls divided by the birth of boys) from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's birth certificates dataset. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the change in the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

F.2 Neonatal mortality outcomes

In this section, I include the difference-in-differences estimates and event studies of the outcomes from prenatal mortality records that did not show any statistically significant effect. The array of outcomes consists of the following: number of deaths, rate of complications during pregnancy, rate of death during pregnancy, rate of death during birth, gestational age of the POC, weight of the POC, rate of death of male fetuses, rate of death due to violence and prenatal visits.

Table 19: Regression table Part 1 with the effects of the price increase in mortality outcomes

	(1) N. deaths	(2) Complication	(3) Preg. death	(4) Birth death	(5) Gest. age
DD price inc.*pipeline	0.000388 (0.00137)	0.000596 (0.000897)	0.000854 (0.00126)	-0.000404 (0.000479)	0.511 (0.428)
Mean	0.0220	0.0078	0.0185	0.0034	13.66
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.102	0.029	0.083	0.023	0.403
N	11100	11100	11100	11100	11100
AIC	-41283.0	-50696.8	-42778.1	-65302.1	84375.7

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables are the number of prenatal deaths, complications during pregnancy, deaths during pregnancy and at birth, and average gestational age. The data was obtained from Death and Fetal Death Certificates collected by INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 20: Regression table Part 2 with the effects of the price increase in mortality outcomes

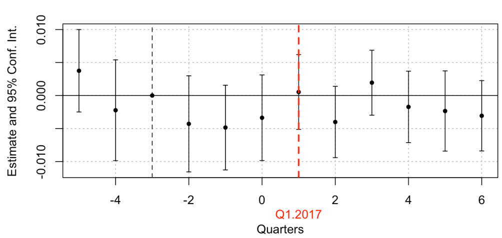
	(6) Weight	(7) Rate male	(8) Miscarriage	(9) Stillbirth	(10) Prenatal visits	(11) Women's age
DD price	38.04	-0.000239	0.000744	-0.0000345	0.133	0.631
inc.*pipeline	(34.12)	(0.000997)	(0.000719)	(0.000232)	(0.343)	(0.395)
Mean	734.66	0.0127	0.0042	0.0007	3.50	12.94
Munic. F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.223	0.072	0.033	0.009	0.087	0.445
N	11100	11100	11100	11100	11100	11100
AIC	181799.5	-46901.2	-55468.7	-81635.6	77767.0	82090.9

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables are birth weight, rate of male fetus deaths, miscarriage rate, stillbirth rate, average number of prenatal visits, and maternal age. The data was obtained from Death and Fetal Death Certificates collected by INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

(a) Effects of the price increase on prenatal death rate among fertile women



(b) Effects of the price increase on prenatal care visits

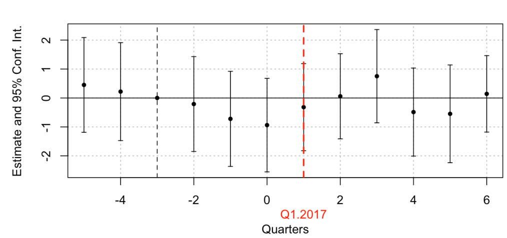
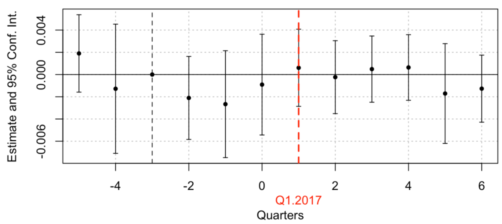


Figure 35: Results from estimating equation (1), where the dependent variables are the percentage of women in that municipality that suffered a prenatal death and the average prenatal care (number of visits to the doctor) of women in that municipality from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's fetal mortality registers. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

(a) Effects of the price increase on complication during pregnancy rate among fertile women



(b) Effects of the price increase on death at a birth rate among fertile women

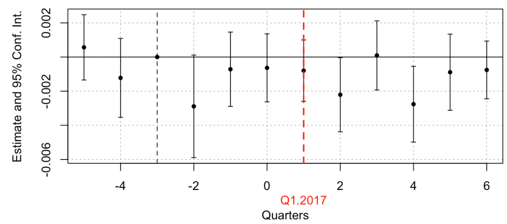
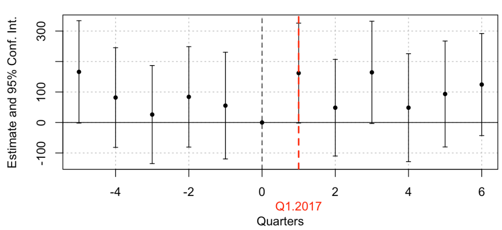


Figure 36: Results from estimating equation (1), where the dependent variables are the rate of women in that municipality who presented complications during pregnancy and those who suffered the death of the baby at birth from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's fetal mortality registers. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

(a) Effects of the price increase on average weight



(b) Effects of the price increase on gest. age

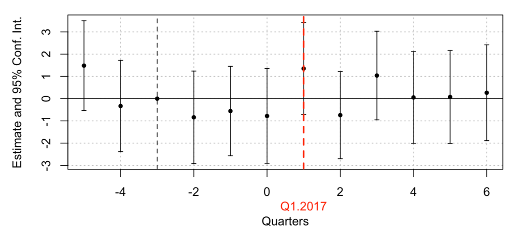
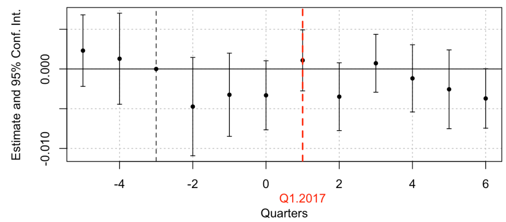


Figure 37: Results from estimating equation (1), where the dependent variables are the average weight and gestational age (weeks) of POC from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's fetal mortality registers. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

Figure 38: Effects of the price increase on the male POC rate among fertile women



Notes: Results from estimating equation (1), where the dependent variables are the rate of male POC from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's fetal mortality registers. The X-axis represents quarters. The reference period is the first quarter of 2016, a month before the conception of babies affected by the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the percentages or average outcomes. I control for time and municipalities fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

F.3 Infant and child mortality

In this section, I display the results from estimating equation (1) on infant (less than one year old) and child mortality (between 1 and less than five years old) by gender. Furthermore, I show the event studies for girl's infant mortality.

Table 21: Regression table

	(1) infant mort.	(2) infant boy mort.	(3) infant girl mort.	(4) child mort.	(5) boy child mort.	(6) girl child mort.
DD price	-0.320***	-0.182**	-0.131**	0.0222	0.00208	0.0205
inc.*pipeline	(0.121)	(0.0843)	(0.0634)	(0.0368)	(0.0306)	(0.0249)
Mean	5.18	2.89	2.26	.906	.49	.45
Munic. F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.975	0.962	0.952	0.898	0.823	0.806
N	11008	11008	11008	11008	11008	11008
AIC	53097.9	44985.2	42086.4	29790.0	22922.1	21261.5

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at January 2017. The outcome variables consist on infant (less than one year) and child mortality (from one to less than five years old) by gender. The data was obtained from the mortality records collected by INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

(a) Effects of the price increase on infant girl mortality

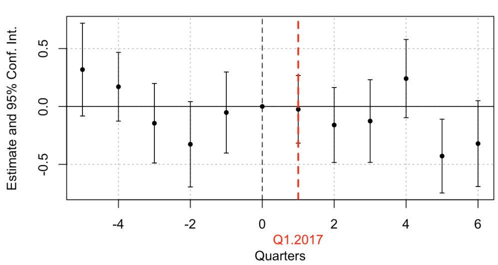


Figure 39: Results from estimating equation (1), where the dependent variables are girl infant mortality from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's mortality records. The X-axis represents quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of 2017. The plot shows the β_k coefficients that capture the changes in mortality per quarter. I control for time and municipalities' fixed-effects. Confidence interval 95% based on standard errors clustered at the municipality level.

F.4 Infant main causes of mortality

Using INEGI's mortality records, I analyze the main causes of infant mortality (less than 1 year old) to assess which are the mortality causes decreasing.

Table 22: Effects of price increase on congenital malformations (Q00–Q99) and respiratory distress of newborn (P220)

	Congenital (Q00–Q99)			Respiratory distress (P220)		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Boys	Girls	All	Boys	Girls
After × Pipeline	-0.0763	-0.0496	-0.0247	-0.0621	-0.0520	-0.0075
	(0.0602)	(0.0412)	(0.0348)	(0.0425)	(0.0288)	(0.0257)
Mean	1.355	0.721	0.622	0.551	0.319	0.230
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.950	0.911	0.904	0.815	0.740	0.693
N	11008	11008	11008	11008	11008	11008
AIC	34864.3	27526.0	25540.1	27408.3	20016.9	15733.5

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables are stress-related hospital discharges. The data was obtained from INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 23: Effects of price increase on birth asphyxia (P369) and congenital malformation of heart (Q249)

	Birth asphyxia (P369)			Heart malformation (Q249)		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
After × Pipeline	-0.0479	-0.0516	0.0059	-0.0347	-0.0167	-0.0192
	(0.0509)	(0.0314)	(0.0284)	(0.0326)	(0.0214)	(0.0196)
Mean	0.565	0.323	0.240	0.412	0.230	0.180
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.863	0.811	0.736	0.807	0.712	0.663
N	11008	11008	11008	11008	11008	11008
AIC	27368.7	19286.6	17484.4	21119.5	14121.9	11206.2

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are stress-related hospital discharges. The data was obtained from INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 24: Effects of price increase on extremely low birth weight (P072) and pneumonia (J189)

	Extremely low birth weight (P072)			Pneumonia (J189)		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
After × Pipeline	-0.00735	0.000868	-0.00979	-0.0128	-0.00567	-0.00632
	(0.0205)	(0.0132)	(0.0125)	(0.0160)	(0.00972)	(0.0107)
Mean	0.169	0.093	0.075	0.110	0.063	0.047
Municipality F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.643	0.538	0.451	0.519	0.434	0.315
N	11008	11008	11008	11008	11008	11008
AIC	13665.0	5153.9	4637.5	8646.1	822.0	-1679.4

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are stress-related hospital discharges. The data was obtained from INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

F.5 Stress-related hospital discharges

In this section, I report the results from the DiD interaction term for every stress-related hospital discharge for women residents in the selected municipalities as well as the event studies of stress-related substance use disorders and stress-related heart diseases.

For all discharges, except for cerebrovascular diseases, DiD interaction coefficients are non-statistically significant.

Table 25: Regression table with the effects of the price increase in anxiety hospital discharges

	(1) stress	(2) anxi._fert_women	(3) anxiety_women	(4) anxi._young_men	(5) anxiety_men
DD price	0.0000577	0.000352	0.000251	-0.000180	-0.000129
inc.*pipeline	(0.000137)	(0.000292)	(0.000173)	(0.000286)	(0.000193)
Mean	0.273	0.118	0.173	0.076	0.101
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.045	0.018	0.038	0.020	0.017
N	11075	11075	11075	11075	11075
AIC	-93975.4	-75791.3	-87645.5	-79891.7	-86685.2

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are anxiety stress-related hospital discharges. The data was obtained from INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 26: Regression table with the effects of the price increase in psychotropic hospital discharges

	(1) psychotropic	(2) psychot._fert_women	(3) psychot._women	(4) psychot._young_men	(5) psychot._men
DD price	-0.000566	-0.000271	-0.000156	-0.00264*	-0.00101
inc.*pipeline	(0.000378)	(0.000375)	(0.000210)	(0.00137)	(0.000711)
Mean	2.616	0.297	0.347	1.714	2.269
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.311	0.089	0.106	0.205	0.298
N	11075	11075	11075	11075	11075
AIC	-72018.0	-72250.2	-85484.7	-41117.4	-57876.9

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes:Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are psychotropic stress-related hospital discharges. The data was obtained from INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 27: Regression table with the effects of the price increase in cerebrovascular hospital discharges

	(1)	(2)	(3)	(4)	(5)
	cerebrovascular	cerebr._fert_women	cerebr._women	cerebr._young_men	cerebr._men
DD price	0.00121*	0.000340	0.00118	0.00100*	0.00132
inc.*pipeline	(0.000648)	(0.000546)	(0.000809)	(0.000604)	(0.000951)
Mean	3.564	0.264	1.737	0.254	1.827
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.311	0.089	0.106	0.205	0.298
N	11075	11075	11075	11075	11075
AIC	-72018.0	-72250.2	-85484.7	-41117.4	-57876.9

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes:Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are cerebrovascular stress-related hospital discharges. The data was obtained from INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

Table 28: Regression table with the effects of the price increase in heart-stress-related hospital discharges

	(1)	(2)	(3)	(4)	(5)
	stress_heart	heart_fert_women	heart_women	heart_young_men	heart_men
DD price	0.000471	-0.00175**	-0.00141	0.000239	0.00233
inc.*pipeline	(0.00108)	(0.000829)	(0.00118)	(0.00102)	(0.00167)
Mean	0.570	0.570	2.856	0.497	2.935
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.313	0.077	0.239	0.062	0.184
N	11075	11075	11075	11075	11075
AIC	-52320.6	-54592.1	-46999.0	-52196.5	-43614.5

Clustered standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes:Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables are heart stress-related hospital discharges. The data was obtained from INEGI from the third quarter of 2015 to the second quarter of 2018. I include time and municipality-fixed-effects. Both specifications have clustered standard errors at the municipality level in parentheses.

The event study type of models for all, male, young male and all female population are available upon request.

F.6 Stress-related ICD codes

Table 29: Outcome variables

Category	ICD Code	Disease Name
	F40	Phobic anxiety disorders
	F41	Other anxiety disorders
	F42	Obsessive-compulsive disorder
Anxiety Disorders (F40-F48)	F43	Reaction to severe stress, and adjustment disorders
	F44	Dissociative (conversion) disorders
	F45	Somatoform disorders
	F48	Other neurotic disorders
	F10	Mental and behavioral disorders due to use of alcohol
	F11	Mental and behavioral disorders due to use of opioids
	F12	Mental and behavioral disorders due to use of cannabinoids
	F13	Mental and behavioral disorders due to use of sedatives or hypnotics
Substance Use Disorders (F10-F19)	F14	Mental and behavioral disorders due to use of cocaine
	F15	Mental and behavioral disorders due to use of other stimulants, including caffeine
	F16	Mental and behavioral disorders due to use of hallucinogens
	F17	Mental and behavioral disorders due to use of tobacco
	F18	Mental and behavioral disorders due to use of volatile solvents
	F19	Mental and behavioral disorders due to multiple drug use and other psychoactive substances
	I20	Angina pectoris
	I21	Acute myocardial infarction
	I10	Essential (primary) hypertension
Heart Diseases Related to Stress	I47	Paroxysmal tachycardia
	I48	Atrial fibrillation and flutter
	I42	Cardiomyopathy
	I50	Heart failure
	I51.81	Takotsubo cardiomyopathy
	I60	Subarachnoid hemorrhage
	I61	Intracerebral hemorrhage
	I62	Other nontraumatic intracranial hemorrhage
	I63	Cerebral infarction
Cerebrovascular Diseases (I60-I69)	I64	Stroke, not specified as haemorrhage or infarction
	I65	Occlusion and stenosis of precerebral arteries, not resulting in cerebral infarction
	I66	Occlusion and stenosis of cerebral arteries, not resulting in cerebral infarction
	I67	Other cerebrovascular diseases
	I68	Cerebrovascular disorders in diseases classified elsewhere
	I69	Sequelae of cerebrovascular disease

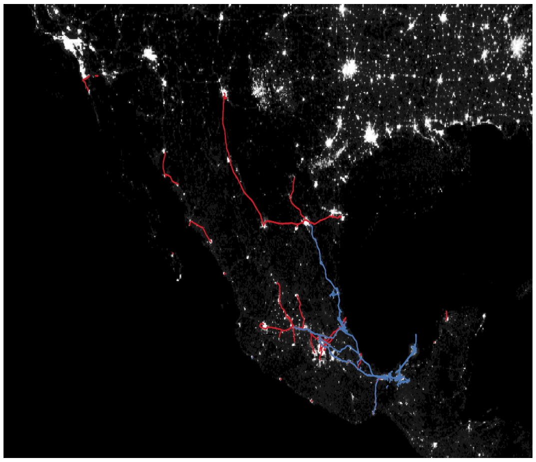
Diseases Related to Stress by General Category

G Geospatial data

In this section, I visualize the entire Mexican territory using geospatial data from all sources used in the analysis. The map overlays national pipeline infrastructure—sourced from Carto-Critica on to municipal boundaries, providing a comprehensive view of areas potentially affected by pipeline-related violence and economic shifts. This visualization serves to contextualize the spatial dimension of the empirical strategy.

The following Figures show satellite-derived TIFF files from VIIRS source that contain the average light intensity for the years 2016 (Figure 40a) and 2017 (Figure 40b).

(a) Nightlight and pipelines in 2016



(b) Nightlight and pipelines in 2017

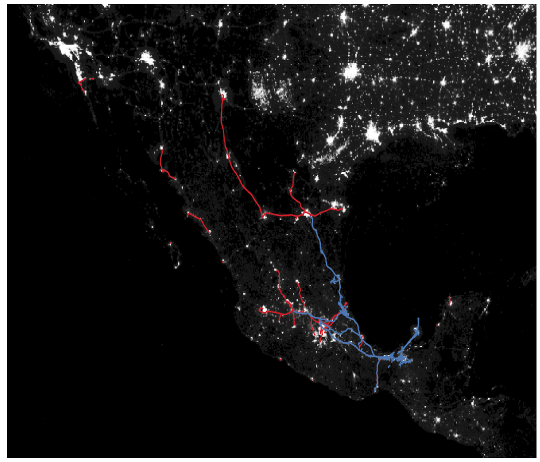
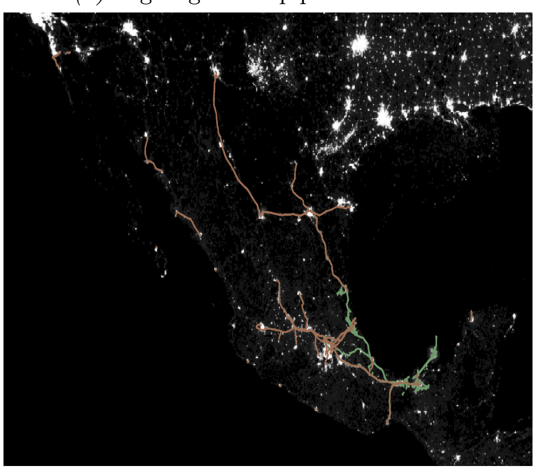


Figure 40: GeoTIFF files (raster image file types) with light intensity (radiance) by year with polyducts or multipurpose pipelines in red and oil pipelines in blue. Source: VIIRS (Visible Infrared Imaging Radiometer Suite) data from the Colorado School of Mines (Li et al., 2020 [63]).

In the following figures it is shown the satellite data from DMSP/OLS in 2016 (Figure 41a) and 2017 (Figure 41b).

(a) Nightlight and pipelines in 2016



(b) Nightlight and pipelines in 2017

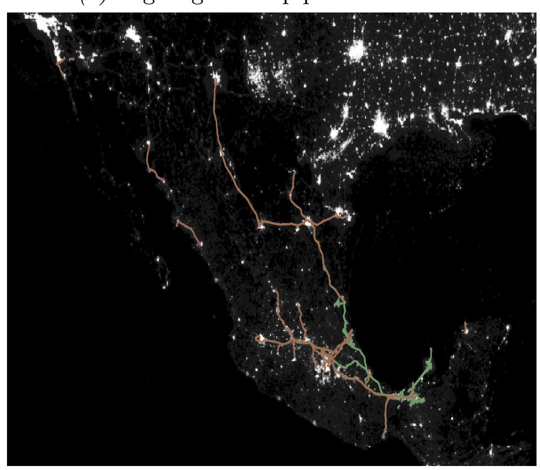


Figure 41: GeoTIFF files (raster image file types) with light intensity (radiance) by year with polyducts or multipurpose pipelines in orange and oil pipelines in green. Source: DMSP/OLS (Li and Zhou, 2017 [62])

In Figures 26a and 26b it is shown data from downscaled global GDP per capita (PPP) dataset by Kummu et al. ([58]) with the oil and polyduct ppelines.

(a) GDPP and pipelines in 2016 *(b)* GDPP and pipelines in 2017

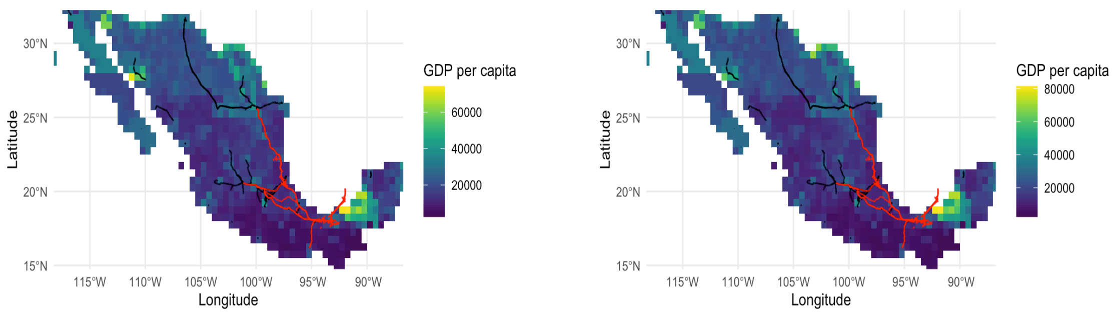
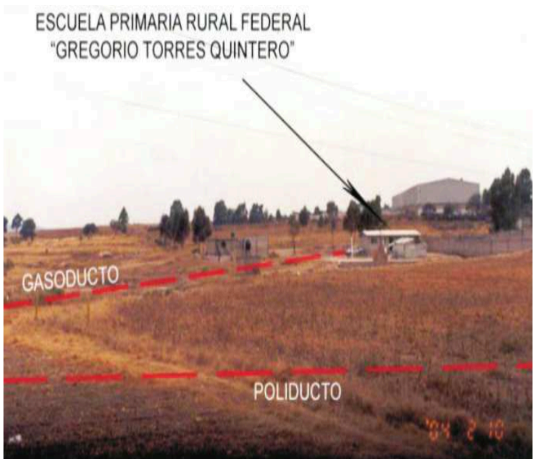


Figure 42: GeoTIFF files (raster image file types) with global GDP per capita (PPP) dataset by Kummu et al. ([58]) by year with polyducts or multipurpose pipelines in balck and oil pipelines in red. Source: DMSP/OLS (Li and Zhou, 2017 [62])

H Additional figures

These photos are from a report done by the Mexico State in 2006 in which they show risky locations of the pipelines. Figure 43a shows pipelines located close to an elementary school. Figure 43b shows pipelines passing below houses.

(a) Pipelines



(b) Pipelines

Figure 43: Photos from Mexico State risk report about pipeline location

I Descriptive statistics

Table 30: Descriptive statistics by group

Variable	**Control (%)**	**Treatment (%)**
Population in poverty	58.21	49.15
Population in extreme poverty	12.02	7.33
Population lacking access to health services	14.10	15.90

Notes: The table reports percentages for selected socioeconomic indicators across control and treatment municipalities. Data from CONEVAL 2015.

Table 31: Geographic characteristics by group

Variable	**Control**	**Treatment**
Average elevation (metres)	1,285.52	1,202.54
Exposure to hydrometeorological hazards	2.66	2.93
Exposure to geological hazards	0.40	0.282

Notes: Elevation is measured in metres above sea level. Hazard exposures are indices or scores reflecting the level of risk to each type of natural hazard. Data from CONEVAL 2015.

Table 32: Descriptive Statistics

	Pre-price increase		Post-price increase	
Variable	Control	Treatment	Control	Treatment
Birth weight (grams)	3153.03	3151.71	3141.69	3142.64
Mean gestational age (weeks)	38.80	38.76	38.76	38.70
% of low birth weight	5.75	5.75	6.19	6.40
% of very low birth weight	.59	.56	.60	.67
% of macrosomia	2.85	2.89	2.87	2.89
Mean prenatal care	7.39	7.72	7.42	7.71
% premature	6.32	6.22	6.26	6.43
Mean Apgar	8.89	8.90	8.89	8.90
% low Apagar	.97	.83	.99	.86
Fertility rate (per 1000)	4.44	4.24	5.12	4.51
Sex ratio	1.04	1.01	1.05	1.02
Mother's age	24.78	24.84	25.23	25.29

Notes: Descriptive statistics of birth certificate data from INEGI. These descriptive statistics encompass the third quarter of 2015 to the second quarter of 2018

Table 33: Descriptive statistics of neonatal mortality

	Pre-price increase		Post-price increase	
Variable	Control	Treatment	Control	Treatment
Rate of prenatal deaths	.020	.019	.016	.016
Rate of complications during pregnancy	.008	.008	.0063	.0069
Rate of death during pregnancy	.02	.019	.016	.016
Rate of death during birth	.003	.003	.0032	.002
Average gestational age (weeks)	11.56	15.9	11.74	16.63
Average weight (grams)	635.22	853.82	628.58	885.21
Rate of death of male fetus	.013	.013	.011	.011
Rate of miscarriages	.004	.004	.003	.003
Prenatal care	3..05	4.38	2.74	4.2
Rate of stillbirths	.0005	.0006	.0008	.0009

Notes: Descriptive statistics of neonatal deaths from INEGI. The rates are obtained by dividing the number of cases by the women in their childbearing age population in the municipality.These descriptive statistics encompass the third quarter of 2015 to the second quarter of 2018.

Table 34: Descriptive statistics of obstetric hospital discharges

	Pre-price increase		Post-price increase	
Variable	Control	Treatment	Control	Treatment
Birth rate	2.41	2.16	1.58	1.41
C-sections rate	.52	.479	.56	.50
Dystocic rate	.015	.014	.01	.014
Contraceptive rate	1.64	1.6	.76	.69
Abortion rate	.24	.22	.18	.16

Notes: Descriptive statistics of women's obstetric hospital discharges from INEGI. The variables include births at hospitals, C-sections, dystocic births, contraceptives, and abortions. The rates are obtained by dividing the total number of women patients by the women in their fertile age population in the municipality and multiplying by 1000. These descriptive statistics encompass the third quarter of 2015 to the second quarter of 2018.

Table 35: Descriptive statistics of stress-related hospital discharges

	Pre-price increase		Post-price increase	
Variable	Control	Treatment	Control	Treatment
Anxiety disorders	.00069	.00051	.00047	.00049
Substance use disorders	.00061	.00076	.00071	.00084
Heart diseases	.0127	.0116	.0126	.0118
Cerebrovascular diseases	.0087	.0078	.0067	.0071

Notes: Descriptive statistics of women's hospital discharges related to stress. This data is collected by INEGI and includes anxiety and substance disorders and heart and cerebrovascular stress-related diseases. The rates are obtained by dividing the total number of women patients by the women's population in the municipality and multiplied by 1000. These descriptive statistics encompass the third quarter of 2015 to the second quarter of 2018.