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

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Abstract

This paper examines how communities exposed to chronic organized crime respond to increases in violence and illicit economic opportunities. I study a natural experiment in Mexico, where criminal groups expanded into fuel theft along the underground pipeline network. A 20% fuel price hike in 2017 increased the profitability of theft, leading to a 29–34% rise in homicides in pipeline municipalities. Using administrative data and a difference-in-differences design, I find no deterioration in prenatal health indicators—such as birth weight or gestational age—nor increases in miscarriages or stillbirths. Moreover, infant and child mortality among boys declined by 5–6%. These results suggest behavioral adaptation to persistent violence and potential income effects linked to participation in illicit markets. The findings challenge assumptions that violence uniformly harms health and offer new insights into the social consequences of organized crime in contexts of institutional fragility.

1 Introduction

Organized crime is a pervasive feature of weak-state environments, accounting for 22% of global homicides and 50% of those in the Americas (UN, 2023[87]). While prior research has shown how such violence undermines economic development (Dell, 2015[35]) and human capital accumulation (Manacorda et al., 2021[58]), less is known about its physiological consequences—particularly for pregnant women and infant health. Chronic exposure to violence during pregnancy may increase stress-related risks, yet some recent studies suggest these effects may attenuate over time. Why such attenuation occurs, and what mechanisms mediate it, remains unclear.

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This paper engages with the fetal origins hypothesis, which posits that prenatal conditions—including maternal stress—have long-term consequences for child health, cognitive development, and labor market outcomes (Currie Moretti, 2007[32]; Conley et al., 2006[29]). In contexts of organized violence, stress-induced effects on fetal development could be particularly severe. Yet in settings where exposure is persistent, behavioral or physiological adaptation—or compensatory income effects from illicit activity—may mitigate these outcomes.

The Mexican context is especially well-suited to explore this question. Since the 2006 launch of its militarized "war on drugs," Mexico has experienced over 431,000 homicides and 218,000 disappearances (INEGI, 2022). Facing enforcement pressure, drug cartels diversified into fuel theft by tapping the country's 19,000-km underground pipeline network. These pipelines run through municipalities of varying cartel presence, offering spatial variation in exposure to criminal control. While earlier studies have documented adverse prenatal effects during the initial years of Mexico's drug war, the long-term implications of chronic exposure remain underexplored. Brown (2018) finds that birth weight declined by an average of 42 grams in the early 2000s—an effect comparable to that of natural disasters [84]—and Torche and Villarreal (2014) show that maternal behavior shifted in response to escalating insecurity. However, nearly two decades later, the landscape has evolved significantly. Organized crime has become structurally embedded in the economy, with cartels now estimated to be the fifth-largest employers in Mexico [78].

To causally identify the effects of persistent criminal violence on health, I leverage two quasi-exogenous shocks: (a) the fixed geography of pipeline infrastructure, and (b) an unanticipated 20% fuel price hike in January 2017, which sharply increased the profitability of stolen fuel. I use administrative data on homicides, births, fetal deaths, infant and child mortality, and hospital discharges from the third quarter of 2015 to the second quarter of 2018, right before a massive presidential election that altered violence. The empirical strategy is a difference-in-differences design comparing municipalities with and without pipeline infrastructure before and after the 2017 fuel shock. Event studies are used to test for parallel trends and assess dynamic effects.

First, I show that homicide rates rose by 29–34% in pipeline municipalities after the price shock, confirming cartel responsiveness to economic incentives (Sobrino, 2019[82]; Dube Vargas, 2013[39]). Second, despite the rise in violence, there is no evidence of negative effects on any of the in-utero health outcomes such as birth weight, gestational age, miscarriages, or stillbirths. Third, I document modest (5–6%) reductions in infant and child mortality among boys.

To ensure robustness, I rule out selection effects by showing no changes in fertility, migration, abortions or maternal mortality. I also find no shifts in emergency or elective care access that could drive health improvements. Importantly, the mortality reductions occur during infancy and childhood, not in utero, and coincide with the timing of increased illicit activity—suggesting a postnatal, economic mechanism.

To unpack potential mechanisms, I show causal evidence of adaptation via stress-related hospital discharge trends and present suggestive evidence of income effects through nightlight data and proximity to pipeline tampering sites. Together, these results support a two-pronged explanation: physiological/behavioral adaptation to persistent stress, and compensation from increased illicit income.

This paper advances two literatures. First, it contributes to the fetal origins literature by showing how long-term conflict does not uniformly impair child health—particularly in contexts of chronic exposure. Second, it deepens our understanding of how communities respond to organized criminal control by documenting both stress resilience and economic coping under persistent violence. While prior work (e.g., Moscoso, 2022; Foureaux et al., 2016) has shown attenuation effects in places pesistently exposed to homicides, this study opens the "black box" by identifying and testing plausible mechanisms. The findings have broader implications for fragile states where formal employment is scarce, and illicit markets constitute a critical (albeit problematic) source of livelihood.

More broadly, the findings have implications for fragile states, where weak institutional capacity and limited formal employment lead to blurred boundaries between legal and illegal economies. In such environments, criminal markets may serve as economic stabilizers—albeit at substantial social cost. This paper thus documents a form of rational adaptation in which populations adjust to chronic insecurity not through disengagement, but through participation in non-state-compliant ways. The remainder of the paper proceeds as follows: Section 2 reviews relevant literature. Section 3 describes the institutional setting and the 2017 price shock. Sections 4 and **??** outline the identification strategy and empirical approach. Section 8 presents the results, and Section 9 explores potential mechanisms. 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[47]; Hassan et al., 2021[54]; Eves et al., 2023[41]). Medical research highlights several stress-related biological mechanisms, including elevated corticotrophin-releasing hormone (CRH) levels and epigenetic changes, particularly during early pregnancy (Wadhwa et al., 2004[88]; Glynn et al., 2001[49]; Copper et al., 1996[30]).

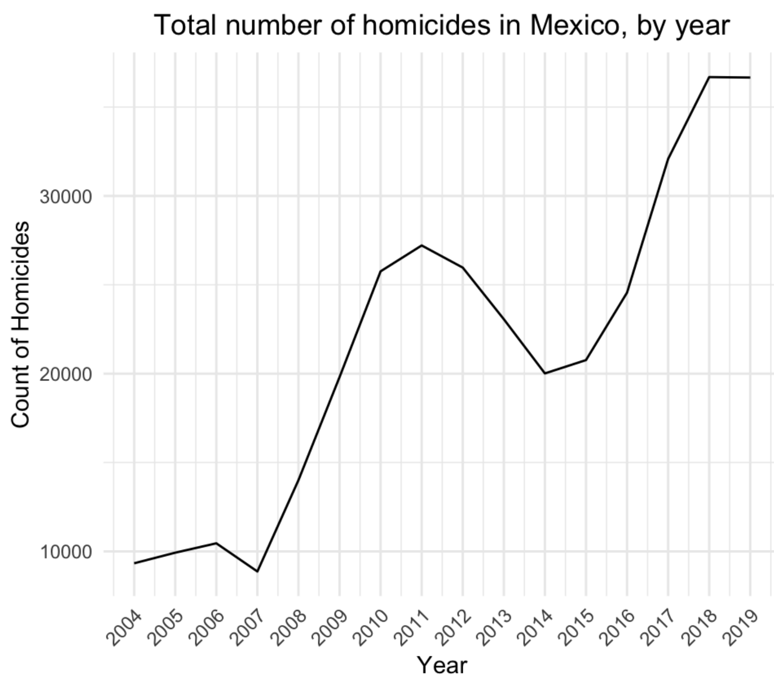
Building on this foundation, economic studies have linked prenatal stress to adverse outcomes in the context of economic hardship (Clark et al., 2021[28]), pollution (Molina, 2021[72]), famine (Scholte et al., 2015[81]), and conflict (Mansour and Rees, 2012[69]). Within the conflict literature, two main approaches have emerged: one focuses on acute shocks (e.g., landmine explosions, Camacho 2008[24]); the other investigates the effects of sustained exposure to violence, including armed and criminal conflicts (e.g., Mansour and Rees, 2012[69]).

This latter strand is especially relevant in the Latin American context, where violence is not driven by civil war but by what Zepeda Gil (2023[92]) terms "crime wars"—highly violent competition between organized criminal groups and state forces. Countries such as Mexico, Colombia, Brazil, Peru, and Ecuador have experienced persistent conflict of this kind, where organized crime strategically deploys violence against rivals and security forces, increasing population-level exposure.

A broad empirical literature from developing countries finds that conflict exposure—especially during the first trimester—is associated with lower birth weight and a higher probability of low birth weight (Le and Nguyen, 2020[61]; Mansour and Rees, 2012[69]; Quintana-Domeque and Rodenas Serrano, 2017[79]). However, recent evidence from Latin America suggests that the adverse effects of violence may be attenuated in contexts of chronic exposure. For example, Moscoso (2022[73]) 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[57]) find modest effects on birth outcomes in Brazil, with stronger impacts in areas where violence is less endemic.

Mexico presents a compelling case for further investigation. Since President Felipe Calderón launched the "war on drugs" in 2006, the country has entered a prolonged phase of elevated violence (see Figure 1). Government responses have been overwhelmingly punitive, and evidence suggests these strategies have intensified conflict (Dell, 2015[35]; Calderón et al., 2015[23]; Massa Roldán et al., 2021[70]). Exposure to violence—whether direct, witnessed, or media-based—has also been linked to increased psychological stress and mental health burdens (Rios and Rivera, 2018[80]; Flores Martinez and Phillips, 2022[43]).

Figure 1: Average number of homicides by year



Notes: Homicides from 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.

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

https://www.inegi.org.mx/contenidos/programas/ensu/doc/ensu2019_diciembre_presentacion_ejecutiva.pdf health effects to stress-related mechanisms. Torche and Villarreal(2014 [83]), 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 [22]), 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 violence several years after the war's onset.

3 Mexican Fuel Sector

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 tasked with overseeing all aspects of the energy sector, including the monopoly over fuel sales, with retail prices tightly regulated.

From 1938 to 2016, fuel prices were set by the Ministry of Finance and applied uniformly across the country, with no geographic variation. All of Mexico's approximately 11,000 gas stations were required to sell PEMEX-branded fuel at these fixed prices (Davis et al., 2019[34]).

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 schedule. Just eight days later, the government unexpectedly announced a 20% price increase, effective January 1, 2017. This fixed price lasted until February 18, after which maximum prices were set daily and allowed to vary regionally.

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.

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[74]; León Sáez, 2021[63]; International Crisis Group, 2022[56]). n regions with greater cartel competition, this shift has been linked to heightened violence and increased stress for local populations (Vivanco et al., 2023[46]).

Criminal groups including the Zetas, Caballeros Templarios, and Cártel Jalisco Nueva Generación (CJNG), as well as smaller regional gangs—especially in states like Puebla—began organizing large-scale fuel theft operations (Navarro, 2018[74]).

Fuel in Mexico is transported via a vast pipeline system spanning over 19,000 km, including 9,098 km of polyducts used to carry refined products like gasoline and diesel . Built between 1954 and 2000, these polyducts traverse urban, rural, and indigenous areas. Because their exact locations are classified for national security reasons, criminal groups often resort to bribing or coercing PEMEX personnel to access internal maps and technical data, making them prime targets for fuel theft.

Fuel theft is a highly profitable operation, typically executed by organized crews of 10–20 people, sometimes including engineers. One former police officer reported earning up to $50,000 per month from stolen fuel, compared to just $270 in his official salary. According to PEMEX, a single network can siphon off $500,000–750,000 in fuel monthly in operations lasting under two hours (León Sáez, 2022[62]).

The barriers to entry are low—a clandestine tap costs just $150—making it a far more accessible revenue source than drug production,like cocaine which an take up to six months (León Sáez, 2022[62]).

Between 2009 and 2016, fuel theft cost the Mexican government an estimated $9.28 billion, with PEMEX spending another $95 million on pipeline repairs..

As illustrated in Figure 2, there was a marked increase in illegal fuel pipeline tappings during the period under analysis, based on data obtained through Mexico's public transparency portal (INAI) and cross-referenced with PEMEX fuel pricing records. Between 2016 and 2017, the number of such incidents rose by 25%, potentially indicating a correlation between rising fuel prices and illicit activity.

It is important to highlight that this increase on tappings might correspond to a lower bound, since measuring fuel theft in Mexico is notoriously difficult. During President López Obrador's administration (2018–2024), the government reported that 46.4 million barrels were stolen during 2016 to 2018, while PEMEX's director stated the figure was 54.4 million—highlighting discrepancies in official estimates. In 2019, it was revealed that 170 out of 379 pipeline monitoring stations were non-functional, and 98 had never been installed.

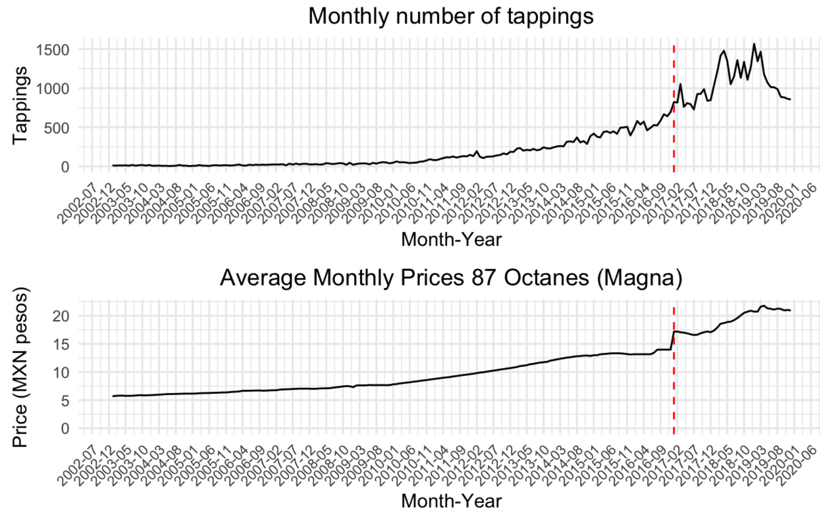
Recent estimates suggest that illegal diesel and gasoline account for between 16% and 27% of Mexico's annual fuel consumption. According to the Mexican Association of Service Station Providers, there are approximately 22,000 "irregular self-consumption points" operating across the country—equivalent to 1.6 illegal outlets for every legally regulated gas station.

Polyducts or multipurpose pipelines started to be built in 1954 with different expansions in 1960, 1981, 1991, 1995, and 2000. See https://www.pemex.com/nuestro-negocio/logistica/ductos/DocumentosInfra/Anexo%20Sistema%20Norte.pdf

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

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

Figure 2: Average monthly tappings and prices



Notes: Monthly tappings and prices. Tappings' data source is from a public information request with folio number 1857200094319. Sources for prices: Energy Information System (before 2016), public information request with folio number (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.

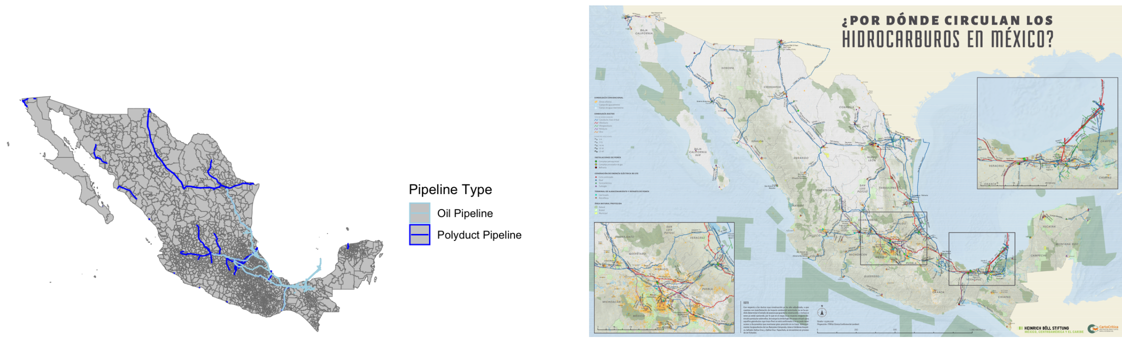
Qualitative accounts suggest that violence has intensified in areas where tappings occur. However, while quantitative evidence on the link between pipeline tapping and violence remains limited, it is steadily increasing.

López and Torrens (2023[67]) modelled competition behavior amongst cartels and found that when the government cracks down on drug trafficking, it triggers criminal diversification into other illegal activities like fuel theft. Because fuel theft opportunities are geographically dispersed and do not align with the drug trafficking areas, organized crime spreads to new locations. López and Torrens (2023[67]) also found an increase in homicides in places with pipelines.

On a similar note, Vivanco et al. (2023 [46]) show that an increase in international oil prices is associated with higher homicide rates in municipalities with pipelines. Lastly, Battiston et al. (2024 [10]) found that municipalities with pipelines experienced a significant increase in cartel presence compared to those without pipelines. They also found that among municipalities with pipelines, there are more illegal taps in areas where the political party supporting anti-drug trafficking policies narrowly won local elections. However, they did not find an increase in homicides.

Although the exact locations of the pipelines remain undisclosed for security reasons, the CartoCrtica NGO assembled pipeline maps using public and leaked sources. According to their map, pipelines traversed 400 of Mexico's 2,643 municipalities in 2017, across 29 of 32 states. Of these, 80 had oil pipelines, 164 polyducts, and 156 both types. Using publicly released tapping data (INAI folio 1857200094319) cross-referenced with CartoCritica pipeline maps (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.

(a) Pipelines *(b)* CartoCritica



Notes: Map extracted from CartoCritica

Other studies rely on different pipeline maps, such as the one used by Vivanco et al. (2023[46]), which identifies pipelines in 285 municipalities. I chose the CartoCritica map—also used by López and Torrens (2023[67]) and Battiston et al. (2024[10])—due to its strong alignment with the municipalities where illegal taps have been reported.

3.1 Commodity Prices, Crime, and Health

Recent literature examines how exogenous income shocks, particularly those driven by fluctuations in commodity prices, may influence conflict dynamics through competing mechanisms. One view, rooted in standard economic theory, posits that higher commodity prices increase legal sector wages, reducing the attractiveness of criminal activity by raising the opportunity cost of violence (Becker, 1968 [13]). Another posits that higher prices increase the returns to theft or violence, thereby escalating conflict (Grossman, 1991 [51]). Empirical findings remain mixed and context-dependent, varying with the type of commodity (e.g., oil, minerals, cocoa) and the institutional capacity of the affected country.

Dube et al. (2013 [39]) provide evidence of the link between price changes and conflict, reporting a positive relationship between oil price hikes and conflict escalation in Colombia, especially in oil-rich municipalities. The capital-intensive nature of the oil sector encourages increased extraction (i.e., rapacity) rather than a shift of labor towards legal sectors. Berman et al. (2017[16]) find similar patterns for mineral extraction in Africa. Conversely, Axbard et al. (2021[9]) report that rising mineral prices reduce crime, as labor-intensive sectors absorb more workers.

Further evidence highlights how commodity prices can influence crime patterns beyond violent conflict. Draca et al. (2019[38]) show that prices of consumer goods, including fuel, are positively correlated with property crime in London. Braakmann et al. (2024[21]) find that gold price spikes shift burglary patterns within the UK, highlighting criminals' responsiveness to expected returns.

Meanwhile, research on the relationship between changes in commodity prices and health outcomes has been analyzed separately from conflict-related studies. Adhvaryu et al. (2019[2]) found that

low cocoa prices at the time of birth in cocoa-producing regions of Ghana increased the incidence of severe mental distress. For the Mexican context, Adhvaryu et al. (2024[3]) document how adverse rainfall shocks—affecting agricultural wages—diminish physical health, with conditional cash transfers mitigating some effects.
There is also evidence that income increases can wear off negative shocks . In Angola, firms operating in resource-abundant regions benefited financially from civil war due to reduced competition and regulatory oversight, making low-intensity conflict more profitable than peace (Guidolin and La Ferrara, 2007[53]). Also in Africa, the adverse health effects of pollution from mining activities are partially offset by local income gains, illustrating how extractive economies can simultaneously harm and support affected populations (Benshaul-Tolonen, 2019[15]). This last literature, although not connected to price increases, are related to local income increases that can offset negative effects. By connecting fuel price shocks to crime and health outcomes in a low-governance context, this paper contributes to a nascent but growing literature exploring the interplay between economic shocks, illegal markets, and human welfare.

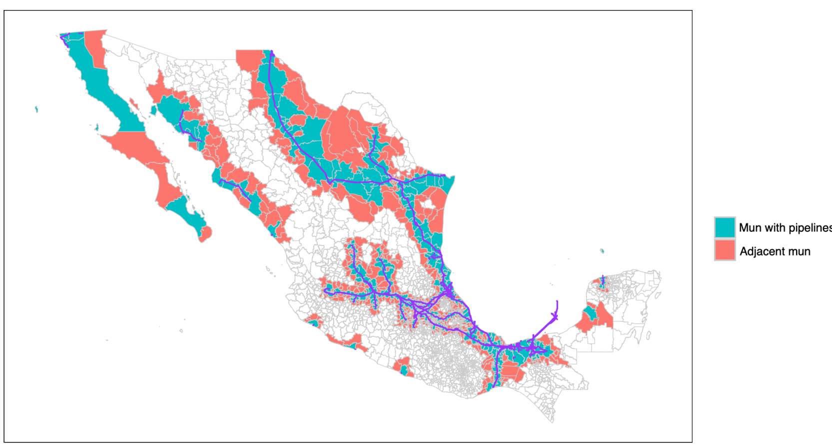
4 Identification strategy

This study analyzes the effect of stress due to violence on various health outcomes, focusing on the context of Mexico, where homicides have been on the rise since the declaration of the war against drugs in 2006. Homicide counts are among the most reliable crime data in Mexico, despite the serious problem of underreporting , and can serve as a proxy for measuring stress. However, the persistent level of violence across municipalities is not exogenous.
To address this challenge, one strategy is to examine health outcomes before and after the 2006 declaration of the war against drugs using the MxFLS survey whose temporal variation allows to explore this time frame (see Alamir, 2023 [5], Tsaneva and Gunes, 2020 [86], Brown, 2018 [22] and Flores Martinez and Atuesta, 2018 [42]). This type of analysis is not plausible using birth certificate records, as relevant health data, such as birth weight, were only consistently documented starting in 2008. Furthermore, given almost two decades of ongoing conflict, it is crucial to investigate whether the population has normalized stress or if changes in the geographical distribution of crime, and consequently homicides, have affected health outcomes.
Therefore, I leverage quasi-exogenous variation from the pipeline network's geography and a significant 2017 fuel price hike derived from a presidential decree. Thus, my design consists of municipalities with pipelines used as treatment units, and the control units consist of adjacent municipalities (first-order neighbors) to ensure that both groups are overall comparable in terms of regional trends and institutions. To validate the parallel trends assumption, I conduct event-study analyzes, which for the main outcomes show no statistically significant differences in trends between treatment and control groups prior to the fuel price shock. Therefore, despite baseline differences, the chosen control group remains appropriate for causal identification.
To enhance my empirical design, my data is not only disaggregated at the municipal level, but also

In Mexico, out of every 100 crimes committed, only 6.4 are reported. Of every 100 crimes reported, only 14 are solved. For more information see Impunidad Cero's report https://www.impunidadcero.org/impunidad-en-mexico/

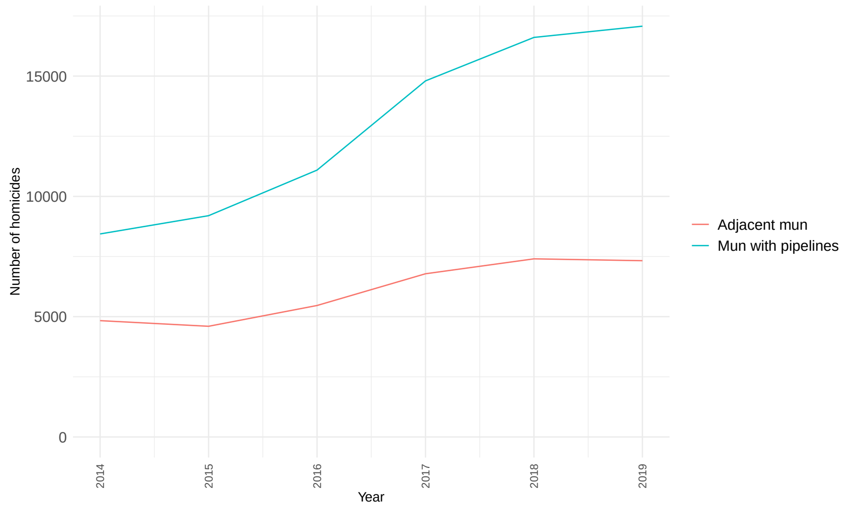
accounts for the quarterly variation of my outcome variables.
Furthermore, the location of the pipelines is unrelated to the current violence in Mexico, as the infrastructure was constructed between the 1930s and the year 2000, well before the onset of the "war against drugs" policy.
The design leverages a price hike, which may raise concerns about exogeneity. However, the shock itself is exogenous, as the public anticipated price liberalization at some point in 2017 (the final schedule was announced on December 20, 2016). However, they did not expect the 20% price increase announced on 28 December 2016 and implemented on 1 January 2017. The measure sparked surprise and discontent, partly because the government had promoted liberalization as a way to lower fuel prices, which did not happen. This led to a sharp decline in the president's approval rating to 12%, the lowest ever recorded for a Mexican president.
To claim that places with pipelines are more violent and, therefore, more stressful, it is first necessary to show that there are indeed more homicides in places with pipelines compared to places without pipelines. Hence, the design in Figure 4 is implemented, comparing places with pipelines to their first-order neighbors. This approach allows for a comparison of the effects of homicides in municipalities where they are presumably unexpected and potentially more traumatic than in municipalities where homicides are frequent.
Having defined the identification strategy, I show that after the price increase, homicides increased in places with pipelines. A first insight into the increasing number of homicides is given by comparing both trends (Figure 5). Nevertheless, by conducting an event study and employing differences-in-differences designs, it is confirmed that there is a rise in homicides after the price increase. This provides an opportunity to elucidate the impact of stress due to homicidal violence perpetrated by drug trafficking organizations (DTOs) on prenatal and other health outcomes.

Figure 4: Municipalities with pipelines vs first-order neighbors



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

Figure 5: Number of homicides in municipalities with pipelines vs. first-order neighbors



Notes: 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.

5 Threats to identification

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.

Examining fertility trends over time revealed that the Difference-in-differences (DiD) estimates show no statistically significant effects nor pretrends (see Appendix Table 3).

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). For municipalities with pipelines, 83% of the population remained in the same municipality, compared to 84.33% in neighboring municipalities. For women of fertile age, the percentages were 91.25% and 91.71% respectively. Due to the lack of administrative data at the municipal level for 2016, 2017 and 2018 I used a geospatial strategy to review possible internal migration near pipelines. This approach allows observation of population dynamics beyond municipal boundaries, offering a clearer picture of potential internal migration and growth near polyduct and oil pipelines. Data was analyzed focusing on the years closer to the precie increase, 2016 and 2017 (in the lack of geospatial quarterly data), through buffers of varying distances (1 km to 20 km), using high-resolution GeoTIFF files to estimate population counts within each buffer zone (see Appendix for complete analysis). 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 or other local factors. The stable growth pattern across different buffer distances supports the theory that the presence of pipelines has not significantly influenced migration patterns. This also confirms that the pipelines connect urban centers, despite passing through various territories.

These findings suggest that internal migration and selective fertility are unlikely to pose a significant threat to the identification strategy.

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 approximately 20%. If people anticipated this hike, we would expect 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 Appendix, Figure 22a) show a clear rise starting in January 2017, with no spike beforehand. Since tapping data is monthly, any response between the announcement and year-end cannot be observed. Overall, this pattern suggests no significant anticipation effect.

6 Empirical strategy

To assess the impact of the 2017 fuel price increase on violence and stress-related outcomes, I divided the outcome data in two different time frames. Specifically, 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 birthweight, gestational age and fetal deaths. This distinction reflects key differences in how and when exposure to the shock could plausibly influence outcomes, especially considering the biological sensitivity of the in-utero period.

Both strategies employ a combination of event study and difference-in-differences designs, using municipalities as the unit of analysis and aggregating all outcomes at the quarterly level. This approach facilitates analysis of both immediate and short-term responses to the shock. The event study spans a symmetric six-quarter window—from Q3 2015 to Q2 2018—capturing trends before, during, and after the policy change.

Calendar-Time Outcomes (Homicides, Discharges, Mortality, Suicides) For outcomes determined at or near the time of occurrence—such as homicides, obstetric hospital discharges, suicides, mortality (infant, child and other causes of death), and fertility—I align the event study by calendar quarter. I centre the analysis around the fourth quarter of 2016 (Q4 2016), which serves as the reference period prior to the shock (Figure 6)

Gestation-Time Outcomes (birth certificates and fetal deaths) For health outcomes that are determined during pregnancy, I estimate the quarter of conception for each birth using information on the date of birth and gestational age. Aligning by conception quarter, rather than

birth quarter, ensures a more accurate assessment of in-utero exposure to the rise in violence and associated stress.
I set Q1 2016 (January-March 2016) as the reference period, a quarter before conception of the babies affected at least one period by the price increase and therefore, stress. Children conceived in the second quarter of 2016, are most likely born in Q1 2017, with most of their gestation occurring before the onset of violence. In contrast, those conceived later were likely exposed to violence during more sensitive stages of gestation, particularly the first trimester.
As with the calendar-time outcomes, I use a six-quarter symmetric window around the shock. This setup includes conception cohorts before, during, and after the fuel price hike.

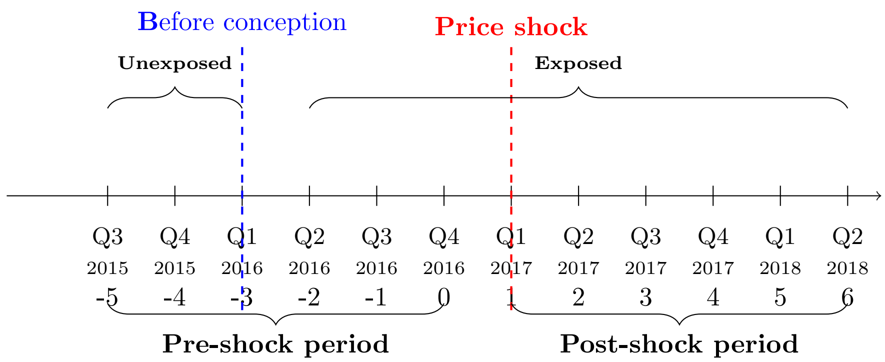


Figure 6: Timeline for analysis. Gestational cohorts used for birth and fetal health outcomes use reference period Q1.2016. Calendar periods used for outcomes like violence, hospital discharges, and mortality use reference period Q4.2016.

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

$$Y_{jt} = \mu_0 + \beta_0 \cdot \text{Pipeline}_{jt} + \sum_{k=-5, k\neq 0}^{6} \beta_k \cdot \text{Pipeline}_{jt} \cdot \mathbb{1}[\tau = k] + \eta_j + \delta_t + \varepsilon_{jt} \tag{1}$$

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 table 2). All outcome data come from administrative registers collected by the National Institute of Statistics and Geography, INEGI (in Spanish), for all 2463 municipalities.

7.1 Homicides

Homicide data are sourced from mortality records compiled by INEGI and accessed via the mx-mortalitydb R package. These records, based on death certificates, are aggregated by municipality, quarter, and year. I use homicides as an initial validation of treatment exposure: if municipalities with pipelines became more stressful following the January 2017 fuel price hike, we would expect 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 5, homicide trends were similar across groups until 2017, after which pipeline municipalities show a sharper increase relative to neighboring areas.

7.2 Health outcomes

Birth registers and fetal mortality I analyze data from birth registers and from the fetal mortality records 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 has been collected since 1986, however, for this analysis, I focus on the period from the third quarter of 2015 to the second quarter of 2018. This dataset includes detailed information on newborns, such as weight (collected since 2008), gestational age, and sex, as well as maternal characteristics like age, education, and municipality of residence. I studied all births during this period and collapsed 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 babies born with anomalies, sex ratios, and low Apgar scores (less than 7).
Data inconsistencies, such as babies weighing less than 500 grams and mothers over 60 years old,

https://github.com/diegovalle/mxmortalitydb were excluded to ensure data accuracy.

Fetal deaths This dataset records the characteristics of products of conception (POC). For fetal mortality, I included an array of outcomes aggregated by municipality, quarter, and year that includes rates such as deaths that presented complications during pregnancy, deaths at birth and pregnancy, neonatal deaths of male fetuses, stillbirths (after 20 weeks of gestation) and miscarriages (20 weeks of gestation or less). These rates were calculated by dividing the number of cases by the total number of women in their childbearing years in the municipality in that specific quarter and year, times 1000. More details about the population counts are provided in the Appendix (12). As the birth certificate sample, the dataset spans from the third quarter of 2015 to the second quarter of 2018.

Child and infant mortality Child mortality is defined as death before age five, and infant mortality as death between 28 days and one year of age. I construct these outcomes using quarterly mortality data from INEGI, based on the number of registered deaths within the relevant age ranges. Due to the lack of reliable quarterly birth counts, I do not calculate mortality rates; using approximated denominators could introduce measurement error. Instead, I rely on fixed effects to account for variation in population size and seasonal trends, mitigating potential bias in the estimates.

7.3 Other data sources

I draw on multiple data sources for the Mechanisms, Threats to Identification, and Robustness Checks sections, with further details provided in their respective parts of the paper.

For the Mechanism analysis, I examine two channels: stress resilience and income effects. Stress-related hospital discharges are used to proxy physiological stress, while income is measured using geospatial data. Specifically, I use harmonized nightlight datasets—DMSP/OLS (Li and Zhou, 2017[64]) and VIIRS from the Colorado School of Mines (Li et al., 2020[65])—along with spatial GDP per capita (PPP) estimates from Kummu et al. (2025[60]).

To address potential threats to identification, I examine internal migration and fertility changes. For migration, I use population estimates from GLOBPOP to assess spatial shifts in the resident population. For fertility, I construct rates using birth certificate data from SINAC, divided by imputed municipal-level counts of women of reproductive age.

For robustness checks, I use INEGI data on medical procedures and obstetric hospital discharges. These indicators help determine whether observed effects could be driven by broader healthcare shifts, unrelated health shocks, or behavioral changes such as contraceptive use or abortion.

8 Baseline results

This section shows the event-study design that compares places with pipelines (treated) vis-a-vis neighboring municipalities (control) amid the fuel price increase in January 2017.

8.1 Are there more homicides in places with pipelines?

As observed in other Latin American countries, Mexico's geography of violence has changed substantially in the last years due to the reallocation of crime in places where there is profit (Yashar, 2018 [90]). This has transformed regions where these groups compete for goods such as fuel in places where homicidal violence is extreme.

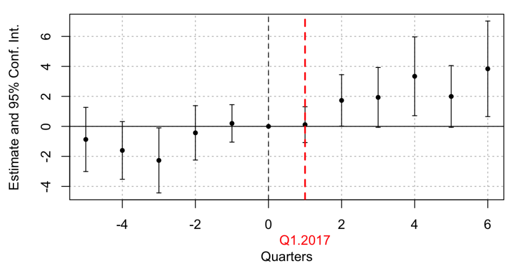
Given the above scenario, firstly, I aim to test if places with pipelines became more stressful to live in; it is necessary to check if they became more violent. A way to measure the latter is to verify if homicides, which are the most reliable proxy of violence in Mexico.

Using the research design (Figure 6) and an event study (based on Eq.1), I analyzed homicides, using the reference period as the fourth quarter of 2016.

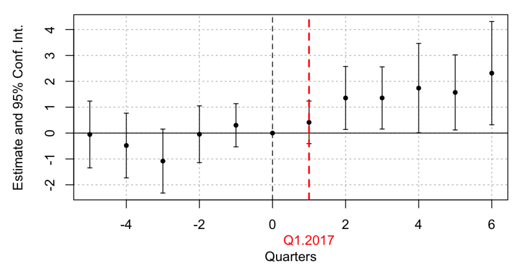
In Figure 7a, the event study shows that homicides began to increase in the second quarter of 2017 and remained positive and significant in the subsequent quarters. For firearm homicides of young men (Figure 7b), I found the same persistent increase in the second quarter of 2017. In the DiD estimates (Table 11 in the Appendix), I observe an increase of almost 30% in all types of homicides in comparison to the average.

Similarly, for the firearm homicides of young men (14 to 44), a type of homicide correlated with organized crime, I found an increase of 33% . All corresponding regression tables are available in the Appendix.

(a) Effect of price increase in all homicides



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



Notes: 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 foruth quarter of 2016, a quarter before the price increase in 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.

I observed a similar pattern in the event studies and DiD estimates for firearm, male, and young male homicides when analyzed separately (see Appendix 14 for tables and figures). For

(2.957/9.87)*100
(1.607/4.11)*100

female homicides, I do observe an increase after the price increase, but with pre-trends.

These findings reinforce the idea that Drug Trafficking Organizations adjust their strategies in response to external market pressures and new avenues of profit. These results are in line with Sobrino's (2019[82]) study, which investigated the demand shock for heroin resulting from the 2010 OxyContin reformulation. Sobrino found that an increase in the number of cartels entering a geographically defined market, such as opium poppy cultivation, is associated with increased homicide rates in that region.

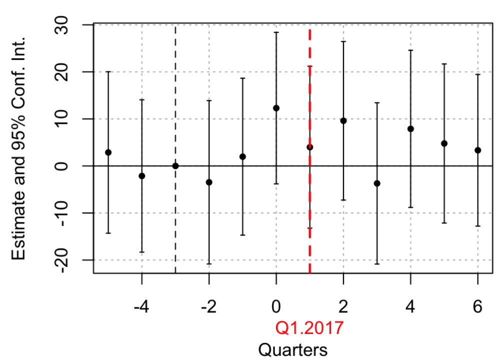
Battinson et al. (2024[10]) analyzed cartel competition in the context of fuel theft and found that the 2006 war against drugs (government crackdowns on drug-related violence and drug-trafficking routes) pushed cartels into this illegal activity without finding an increase in homicides during the 2000-2014 period. Nevertheless, the evidence presented in this paper suggests that the 2017 price shock motivated cartels to compete aggressively against each other for this good.

8.2 Results health outcomes

This section presents the results for outcomes from birth certificates and fetal mortality records. The analysis follows a time frame aligning by conception quarter, rather than birth quarter. The event studies correspond to the specification outline in equation 1, including time and municipality fixed-effects. The reference period is the first quarter of 2016.

Figure 8a presents the birth weight. The event-study plot shows no point estimates different from zero. Likewise, the DiD interaction term is not significant.

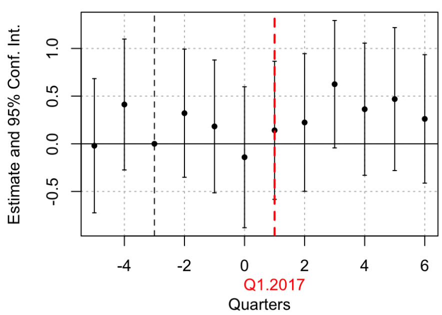
(a) Effects of the price increase on birth weight



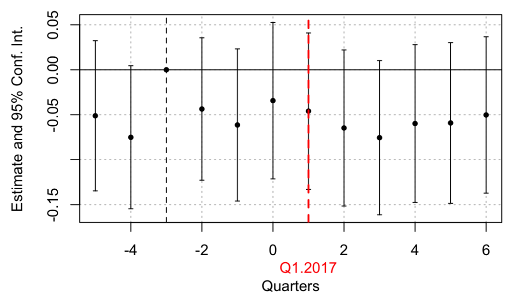
Notes: Results from estimating equation 1, where the dependent variable is birth weight from the third quarter of 2015 to the second quarter of 2018, from INEGI's birth certificates dataset. The X-axis represents the quarters. The reference period is the first quarter of 2016, a quarter before conception 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.

The results indicate that the interaction between the fuel price increase and pipeline exposure does not have a statistically discernible effect on average birth weight (see Appendix Table 12). To assess the precision of this null result, I compare the estimated confidence intervals with those reported in related studies. For instance, Camacho (2008[24]) finds that exposure to homicides during early pregnancy is associated with a reduction in birth weight, ranging from –12.343 to –2.717 grams. In contrast, my estimate yields a slightly wider interval, from –0.999 to 10.171 grams (see Appendix Table 12. Furthermore, when analyzing the entire sample of births, there is a minor and temporary increase in the incidence of low birth weight during the third quarter of 2018, which subsequently reverts to earlier levels. No discernible change is observed in gestational age outcomes.

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



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



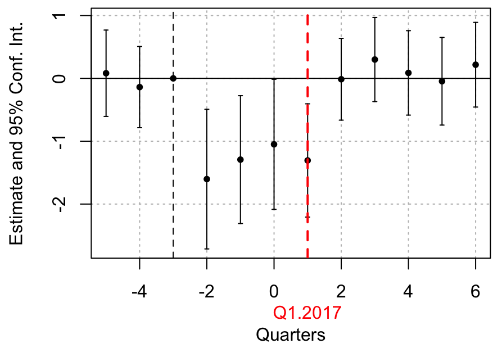
Notes: Results from estimating equation 1, where the dependent variables are the percentage of low birth weight and average gestational age (weeks) from the third quarter of 2015 to the second quarter of 2018, from INEGI's birth certificates dataset. The X-axis represents the quarters. The reference 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.

The event study analysis reveals a sustained decline in the percentage of births with anomalies for cohorts conceived prior to the fuel price increase and exposed to it at some point during gestation, as well as for those conceived during the first quarter of 2017. From the second quarter of 2017 onward (+2 to +6), the point estimates are not statistically different from zero (See Figure 10a).

The results suggest that fetuses whose first trimester, the most critical developmental phase, occurred before the onset of violence may have benefited from the income spillovers associated with the price increase (see Mechanism 9). In contrast, from the second quarter of 2017 onward, when violence had already escalated (See Figure 7a), the estimated effects are negligible and not statistically significant. This implies that income effects may have mitigated potential adverse impacts on fetuses whose first trimester was spent in a violent environment. Moreover, the Difference-in-

Differences (DiD) interaction coefficient is negative and statistically significant, corresponding to a 7.5% reduction in the percentage of birth anomalies. In further sections, I show that there is no evidence of selection in births, as there were no observed increases in miscarriages or stillbirths.

(a) Effects of the price increase on % of anomalies



Notes: Results from estimating equation 1, where the dependent variable is the percentage of anomalies 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.

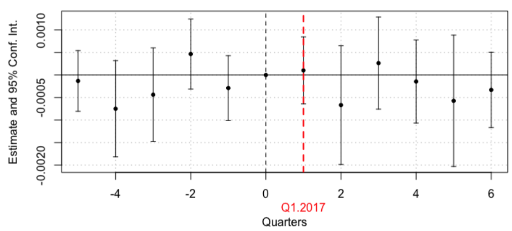
No effects were found for the percentage of babies with macrosomia, premature babies, and teenage mothers. In addition, no effects were found on the average of mother's age, prenatal care and sex ratios. Pretrends were detected for Apgar score and the percentage of births with low Apgar scores (less than 7).

Having established that in-utero exposure to the price increase and pipelines does not have any effect on birth weight, I next examined whether this configuration affected a different sample of pregnant women who could not carry their pregnancies to term.

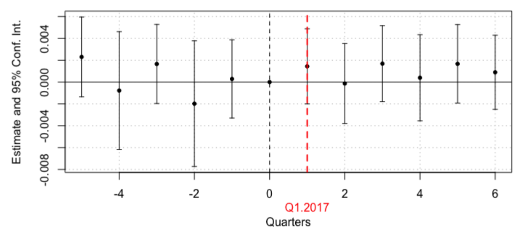
From the neonatal death, there were no estimates different from zero on the rate of stillbirths or for miscarriages. The DiD interaction term for both outcomes are also not statistically significant.

(-1.231/15.918)*100

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



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



Notes: 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.

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 14 and 15). Additionally, there is no increase in the percentage of neonatal deaths among male fetuses, who are generally more biologically fragile than females (Kraemer, 2000 [59]).

In conclusion, there are no effects on birth weight, although there are transitory effects on the percentage of low birth weight, gestational age, and anomalies. By analyzing prenatal deaths and, hence, a different sample of mothers, I found slight increases in stillbirths and a transitory increase statistically different from zero in miscarriages.

Despite finding an increase in homicides and contrary to what has been found in the literature, there is no effect on birth weight. It is plausible that other forces at interplay neutralize the adverse effect. In another section, I discuss two plausible mechanisms: high resilience to stress and an income effect derived from fuel theft that might improve prenatal conditions.

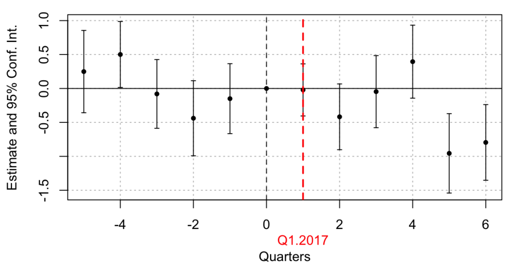
8.3 Child and infant mortality

Conflict is typically associated with deteriorating child health outcomes. For instance, Wagner et al. (2018[89]) find that in Africa, children born within 50 km of an armed conflict face a 5.2 per 1,000 higher risk of dying before age one. These effects often operate through both direct exposure and indirect channels, such as environmental disruption and changes in parental behavior.

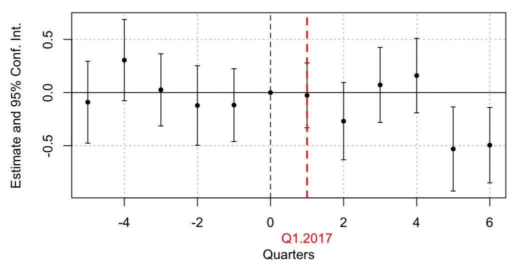
In contrast to this literature—largely focused on African contexts—the Mexican case presents a different pattern. In contrast, the Mexican case reveals a different pattern. Using event-study and difference-in-differences designs, I find infant (under one) and child (under five) mortality declined by approximately 6% and 5%, respectively. While aggregate trends exhibit pre-trends before treatment, disaggregated analyses show no pre-trends for boys and significant reductions in infant (5.3%) and child (6.3%) mortality. Mortality for girls also declines, though pre-existing trends limit causal claims.

These patterns are consistent with evidence suggesting that boys being biologically weaker and more susceptible to diseases and premature death (Chao et al.,2023[27]), making them more responsive to improved postnatal conditions—potentially driven by income-related mechanisms.

(a) Effects of the price increase on infant mortality

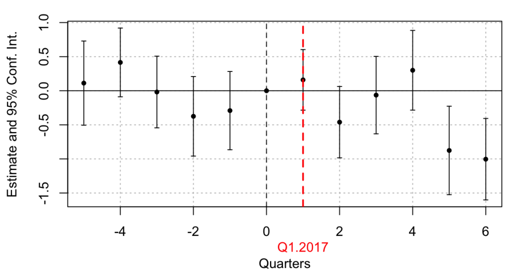


(b) Effects of the price increase on boy infant mortality

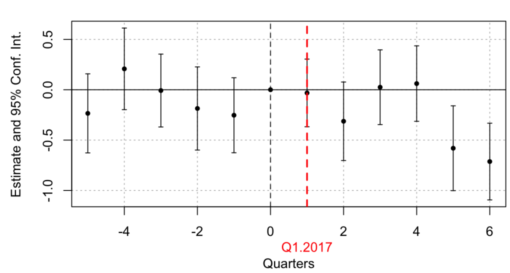


Notes: Results from estimating equation 1, where the dependent variables are infant and boy infant mortality from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's mortality records. The X-axis represents quarters. The reference period is the fourth quarter of 2016, 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.

(a) Effects of the price increase on child mortality



(b) Effects of the price increase on boy mortality



Notes: Results from estimating equation 1, where the dependent variables are child and boys child 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.

Regarding dynamic effects, infant mortality (Figures 13a and 13b) declines are observed in the first and second quarters of 2018 (January–March and April–June 2018), corresponding to children born between January 2017 and the beginning of June 2018 (Q1 2017–Q1 2018). These cohorts were conceived between April 2016 and June 2017 (Q2 2016–Q2 2017), meaning many were conceived before the fuel price hike in January 2017 (Q1 2017) or they spent the initial pregnancy months -the most vulnerable ones- not exposed to violence. Homicide rates began to rise sharply in the second quarter of 2017 (Q2 2017), suggesting that infants born earlier in 2017 were primarily exposed postnatally to both elevated violence and potential income gains from increased fuel theft activity. The lack of any impact on in utero outcomes—such as birth weight, gestational age, percentage of prematures, miscarriages, or stillbirths—further supports a postnatal exposure mechanism. I also find no changes in fertility, abortion rates, maternal mortality, or obstetric discharges that would indicate selection or maternal behavioral responses. Additionally, there are no shifts in urgent or elective hospital procedures, ruling out improved healthcare access as a driver. Taken together, the evidence suggests that reductions in infant mortality are likely driven by postnatal improvements in household economic conditions, particularly for children exposed during infancy to the effects of the 2017.

This interpretation is supported by a concurrent reduction in child mortality (under age five), affecting children born well before the price change (12a and 12b).

This finding is consistent with existing literature on the positive effects of wealth on child well-being, wherein increased income alleviates stress and improves material conditions, ultimately outweighing potential negative consequences (see Mechanisms section: Income Effect). An illustrative case is provided by Benshaul-Tolonen (2019, [15]), who documents a decline in infant mortality associated with income generated from mining activities, despite the adverse environmental impact of pollution. In comparison to the findings of Benshaul-Tolonen (2019, [15]), which report a 50% reduction in infant mortality, my results indicate a more modest effect, documenting a 5-6% change under similar conditions.

9 Mechanisms

In this section, I discuss two potential mechanisms that may explain the absence of effects on birth weight and other in-utero outcomes despite the rise in homicides: adaptive behaviors in response to stressful living conditions and an increase in local income driven by fuel theft activities.

9.1 Adaptative behavior

Community violence can adversely affect health through physiological stress responses and behavioral changes (Ahern et al., 2018[4]). While much of the evidence linking violence to stress and mental health comes from administrative records, RCTs, or survey data in high-income countries, limited data availability in Mexico restricts timely assessments of population-level stress, leaving policymakers with few mental health indicators.

To assess the effects of rising violence on stress, I analyze stress-related hospital discharges using the event study framework in Equation 1. I focus on conditions plausibly linked to stress, including anxiety, substance use disorders, and cardiovascular and cerebrovascular diseases. While administrative data do not allow direct identification of pregnant women, I restrict the main analysis to females of reproductive age, and report additional estimates for men, young men, all women, and the general population in Appendix 15.

Discharge rates are constructed quarterly by municipality, scaled per 1,000 women, and adjusted using population estimates (see Appendix 12). As a complementary check for extreme psychological outcomes, I also analyze suicide rates using INEGI mortality records, again finding no statistically significant effects across subgroups.

Table 1: Outcome variables

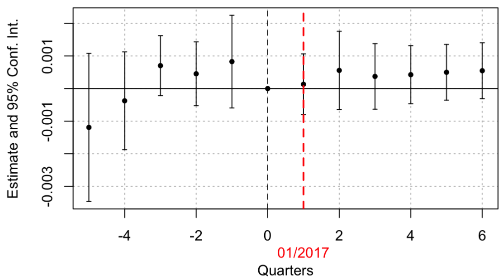
Reference Period	**Type of Data**	**Outcome**	**Source (INEGI)**
December 2016	Stress related hospital discharges	Anxiety Disorders	Hospital discharges
(Month before the price increase)		Substance Use Disorders	
		Heart Diseases	
		Cerebrovascular Diseases	

Data sources of stress-related hospital discharges

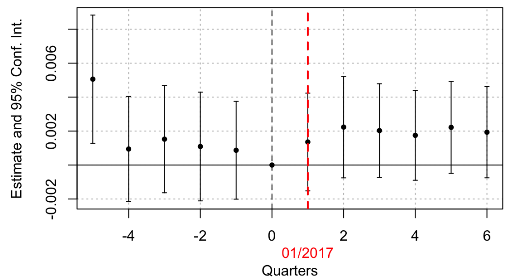
The descriptive statistics from these hospital discharges show an overall decrease in anxiety disorders during the period after the price increase. Nevertheless, there is an increase in substance use disorders and heart-stress-related conditions.

The DiD and event study estimates for anxiety and substance use disorders are statistically indistinguishable from zero. For cerebrovascular diseases, while a pretrend appears in Q3 2015, neither the DiD interaction nor the event study coefficients show significant effects overall. In contrast, Heart disease hospitalizations show rising pretrends before the shock, which persist briefly during the shock and then flatten. This inflection aligns with the observed spike in homicides beginning in Q2 2017. The subsequent stabilization—despite rising violence—suggests a potential income effect, where improved economic conditions may have offset stress-related cardiovascular risks. The negative and statistically significant DiD coefficient supports this interpretation.

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

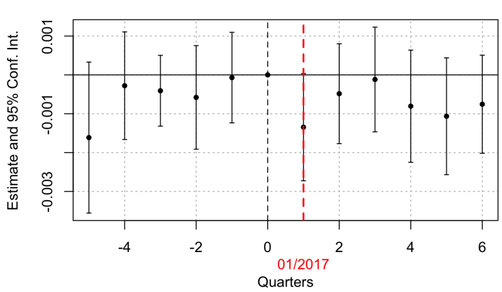


(b) Effects of the price increase on cerebrov.-stress diseases rate among women

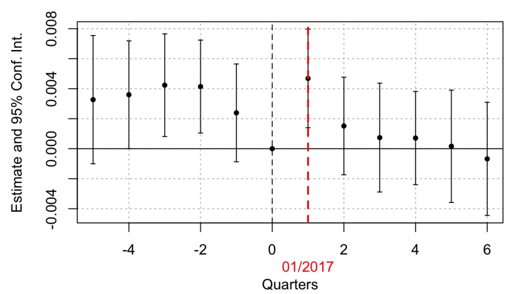


Notes: Results from estimating equation 1, where the dependent variables are the rate of anxiety disorders and cerebrovascular-stress-related diseases from the third quarter of 2015 to the second quarter of 2018. The data source are INEGI's hospital discharges. The X-axis represents the quarters. The reference period is the fourth quarter of 2016, a quarter before the price increase in the first quarter of 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 on substance use disorders rate among women



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



Notes: 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 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.

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.

In Mexico, as in many countries, obtaining detailed annual income data at a granular level is challenging. Hence, to assess a possible income increase I will leverage geospatial data, particularly from nightlight data and a gridded dataset for Gross Domestic Product (GDP) as a robustness check.

To analyze the geospatial data, I split the analysis into both types of pipelines, polyduct and oil, and clipped buffer zones around the pipelines at varying distances (20 km, 15 km, 10 km, 5 km and 1 km). The buffer zones extend equally on both sides of each pipeline. So, when it is specified a buffer width of, for example, 5 kilometres, it means the buffer is 5 kilometres on both sides of the pipeline. Hence a width of 10 km across the buffer zone, centered on the pipeline. I calculate the average value of light intensity or GDPP per bandwidth and compare them in 2016 and 2017.

9.3 Nightlight data

To assess potential income gains in municipalities near pipelines, I use satellite-based nighttime light intensity as a proxy for local economic activity. This approach builds on the growing literature linking luminosity to income and consumption, particularly in data-scarce contexts [55, 37]. Increases in light intensity are typically associated with rising incomes and economic development. I use two datasets to strengthen robustness: (1) the harmonized DMSP/OLS data [64], and (2) VIIRS data from the Colorado School of Mines [65]. Both are provided as annual GeoTIFF raster files. Given the annual frequency, I focus on changes between 2016 and 2017, bracketing the January 2017 fuel price hike.

The DMSP/OLS dataset records digital numbers ranging from 0 to 63 with approximately 1 km spatial resolution and is harmonized across satellites and years for temporal consistency.

VIIRS offers finer spatial detail (approximately 500 meters) and measures radiance in physical units ($nW/m^2/sr$), capturing a broader range of light emissions in both rural and urban areas. While VIIRS enables more granular spatial analysis, the harmonized DMSP/OLS data are useful for comparing historical trends. I rely on both sources to explore whether regions exposed to fuel theft-related violence experienced corresponding increases in light intensity, consistent with income effects driven by illicit economic activity.

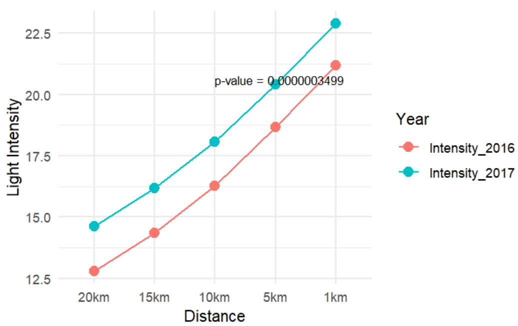
9.3.1 DMSP

Analysis of the DMSP data for 2016 and 2017 reveals that, irrespective of pipeline type or year, light intensity consistently increases as proximity to the pipeline decreases. This pattern likely reflects the original purpose of the pipelines—many of which were constructed beginning in the 1950s—to supply fuel and oil to urban areas.

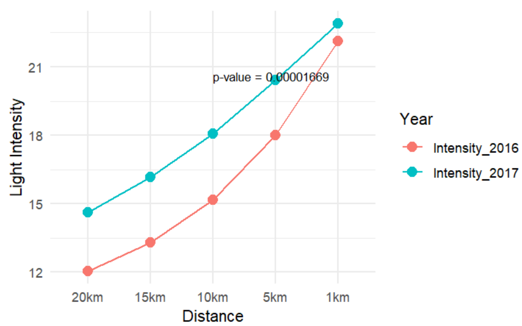
Quarterly night-time light data are not available; as a result, I exclude the last two quarters of 2015 and the first two quarters of 2018.

Moreover, for both types of pipelines, there is a statistically detectable increase in light intensity between 2016 and 2017 (see Figures 16a and 16b).

(a) Light intensity polyduct



(b) Light intensity oil



Notes: Average light intensity for 2016 and 2017 for both type of pipelines: oil and polyduct. Data from DMSP dataset (Li and Zhou, 2017[64]).

9.3.2 VIIRS

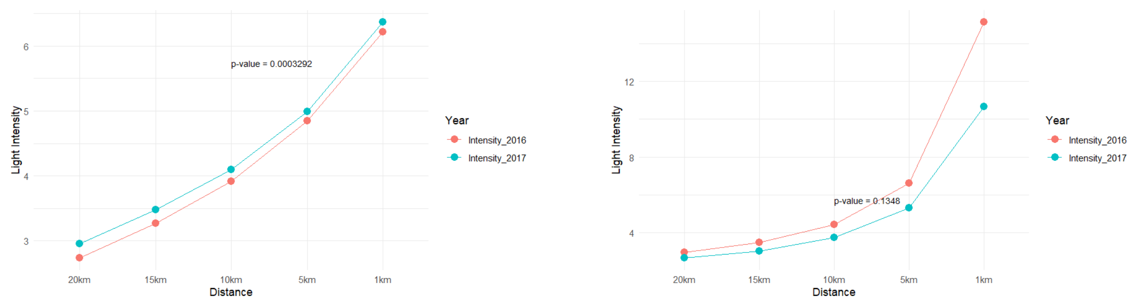
The VIIRS data (see Appendix Figures 17a and 17b) show a similar spatial pattern to the DMSP data: light intensity increases as one moves closer to the pipeline. However, the year-to-year changes differ by pipeline type. For polyduct pipelines, light intensity increased in 2017 compared to 2016. A paired t-test on buffer-level light intensity values confirms that this increase is statistically significant ($p = 0.0003$).

In contrast, for oil pipelines, light intensity was slightly higher in 2016 than in 2017, with no statistically significant difference between the two years.

The divergence between the DMSP and VIIRS datasets may be explained by two complementary factors. First, it may reflect the differing economic incentives for tapping each type of pipeline. Polyduct pipelines, which transport fuel and diesel, are easier to extract from and offer higher resale value, making them more attractive targets for illicit activity. In contrast, oil pipelines—mainly used for crude oil—are less frequently tapped due to the technical complexity and lower profitability of extraction. Second, the discrepancy could stem from differences in the technical properties of the two satellite sensors. The DMSP sensor, with its lower spatial resolution and harmonized annual composites, tends to smooth short-term fluctuations and instead capture broader patterns of urban expansion. VIIRS, by contrast, has higher spatial precision and is better equipped to detect subtle, localized changes in night-time activity. This may explain why VIIRS shows no statistically significant changes in light intensity near oil pipelines in 2017, even as DMSP suggests an increase.

(a) Light intensity polyduct

(b) Light intensity oil



Notes: Average light intensity for 2016 and 2017 for both type of pipelines: oil and polyduct. Data from VIIRS dataset (Li et al.,2020 [65]).

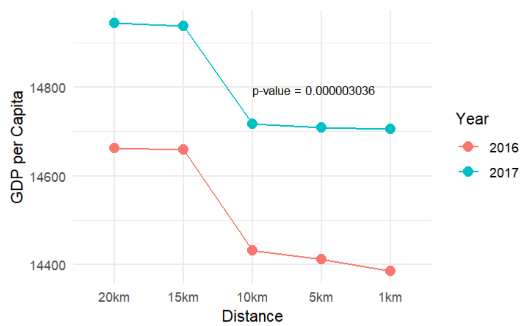
Although nightlight data suggest a general increase in luminosity—and by extension, potential economic activity—around polyduct pipelines, the evidence is less consistent for oil pipelines. To further investigate whether these trends reflect actual income dynamics, I conduct a complementary analysis using geospatial income data.

9.4 GDPP

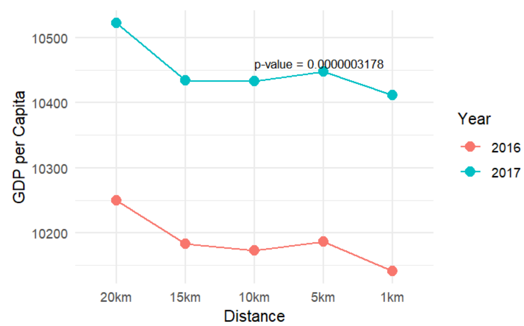
9.5 GDP per Capita (PPP)

As a robustness check, I use the downscaled global GDP per capita (PPP) dataset by Kummu et al. ([60]) to explore whether income levels—measured in international dollars per year at purchasing power parity—increased in areas near the pipelines. This high-resolution dataset covers 43,501 administrative units worldwide (admin-2 level) and applies advanced downscaling techniques, including machine learning models, achieving high predictive accuracy ($R^2 = 0.79$ in cross-validation and $R^2 = 0.80$ on the test set). Annual estimates are available in both gridded and polygon formats at national, provincial, and municipal levels.
Compared to light intensity data, the interpretation of the GDP dataset differs. GDP per capita tends to increase with distance from the pipeline, likely because larger buffer zones capture more people. Nonetheless, both polyduct and oil pipeline zones exhibit an overall increase in GDP per capita from 2016 to 2017 (see Figures 18a and 18b), suggesting a broader pattern of economic growth in these regions.

(a) GDP polyduct



(b) GDP oil



Notes: 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. ([60]) .

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. I document a sharp rise in homicides—between 29% and 34%—in municipalities with pipelines following the shock. Despite this escalation, I find no significant adverse effects on in utero health outcomes, including birth weight, gestational age, prematurity, fetal deaths including stillbirths and miscarriages. Moreover, I found modest declines (5-6%) in infant and child mortality amongst boys. Having discarded selection into birth by not observing increases in fetal development I also find no changes in fertility, abortion rates, maternal mortality, or obstetric discharges that would indicate selection or maternal behavioral responses. I also revised if there was any improvement in health supply by ruled out changes in urgent or elective hospital procedures. Taken together, the evidence suggests that reductions in infant mortality and no in utero effect may reflect the presence of palliative mechanisms. To explore underlying mechanisms, I examine both stress and income channels. Satellite-based nightlight data and spatial GDPP income, indicate increased luminosity and income in pipeline areas following the price shock, consistent with localized economic gains—likely from participation in or proximity to illicit fuel markets.

On the stress side, hospitalizations for stress-related conditions amongst women in their childbearing years remain flat across most categories. Suicide rates—examined across several demographic subgroups—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 responses that mitigate health harms—particularly when economic conditions improve, even through illicit channels. However, such adaptation occurs at a high social cost, as evidenced by rising homicide rates. These results challenge the conventional view that violence uniformly deteriorates health and highlights the importance of accounting for local economic dynamics in conflict settings.

These findings offer new insights into community responses to chronic violence, though some limitations—such as the inability to track individual-level stress among pregnant women or to causally identify income effects—highlight the need for further research using richer microdata.

These conclusions carry important policy implications. Militarized strategies may deepen community reliance on illicit economies, with complex consequences for public health. Interventions in high-violence contexts must consider both the direct costs of conflict and the how populations might adapt in non-state-complaint ways.

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11 Appendix

12 Main data

Table 2: 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	
Neonatal 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	
	Percentage of anomaly	
Mortality Records	Infant Mortality	Mortality records
	Child mortality	
	Suicides	

Outcome data sources related to violence and health indicators

Population Adjustment Methodology

Population data by municipality is 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 regarding the total number of women by municipality and in their fertile window (15 to 49 years) and young men (14 to 44 years) comes 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.

Threats to identification: Fertility, migration and anticipation to the shock

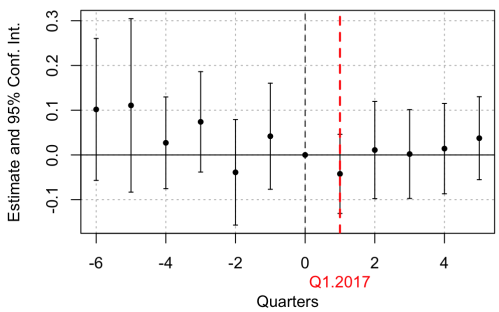
One potential threat to the validity of my previous analysis is the assumption that mothers changed their behavior in response to the price increase and subsequent violence, which could be influencing the relationship between health and local violence. This behavior change could be selective fertility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

12.1 Fertility

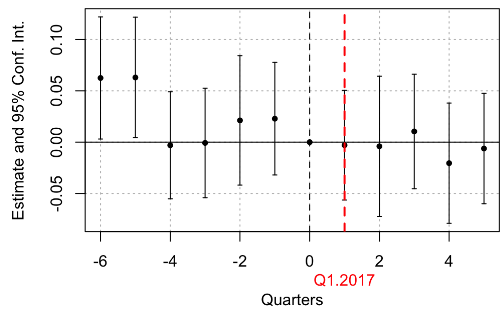
I challenge my results by analyzing fertility over time using event-study type analysis and difference-in-differences estimates. I calculate the fertility rate by using the total number of births divided by the number of women of fertile age in each municipality. The event study and the DiD show no effects 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[6]). This subsample showed a slight decrease statistically different from zero in the point estimate in April 2017 (see Figure 19b).

(a) Effects of the price increase on fertility rate



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



Notes: 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 January 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 dirven by pretrends.

Table 3: 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)
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 $\mathrm{AfterPrice_t}$ indicator, which takes the value of one at January 2017. The outcome variable analyzed is Fertility, which is a rate created by the total number of births divided by the women in their childbearing years living in the municipality. I include time and municipality-fixed-effects. Specifications have clustered standard errors at the municipality level in parentheses.

12.2 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.

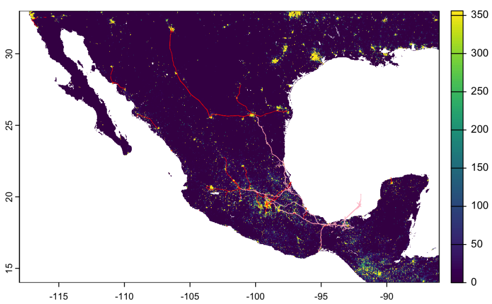
Due to the lack of administrative data, I use a geospatial strategy to revise possible internal migration close to the pipelines. This approach provides a glimpse of population dynamics beyond municipal administrative borders. If violence influences migration decisions, a decline in population close 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[66]), a gridded dataset with 30 arc-second resolution, available in population count and density formats.

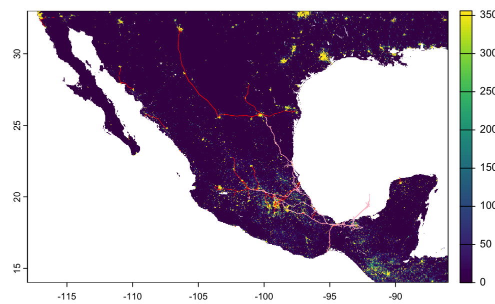
To estimate the population near the pipelines, I created buffers at distances of 1 km, 5 km, 10 km, 15 km, and 20 km around the pipelines and applied them to the GlobPOP dataset. Using the population counts per grid, I calculated the mean population per grid cell within each buffer. After determining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

in each proximity zone.
Figure 20a and figure 20b 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

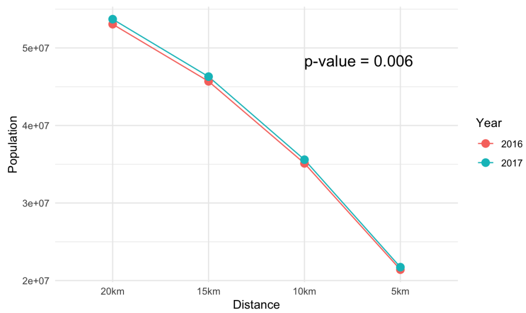


Notes: Population Distribution across Mexico in 2016 and 2017 (Rescaled for Mexico's minimum and maximum values per grid cell), based on GlobPOP global gridded population data with a 30-arcsecond (1 km^2) resolution. The dataset provides population counts for each grid cell. Source: GlobPOP Global gridded population) from Liu et al.,2024. Red pipelines correspond to polyducts, and pink are oil pipelines.

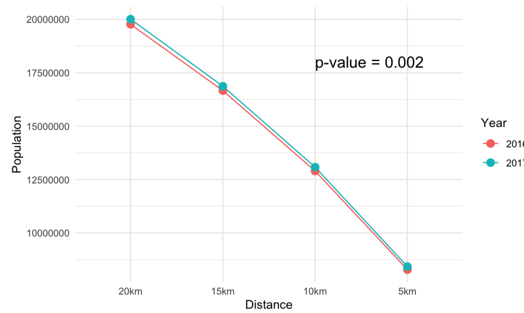
As mentioned, the main concern of internal migration is that the population, particularly women, are migrating to "safer" places to avoid stress exposure due to homicides and therefore affecting the estimates. I estimated the average population counts per bandwidth and found that places close to pipelines 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.

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



Notes: 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 4, 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 4: 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

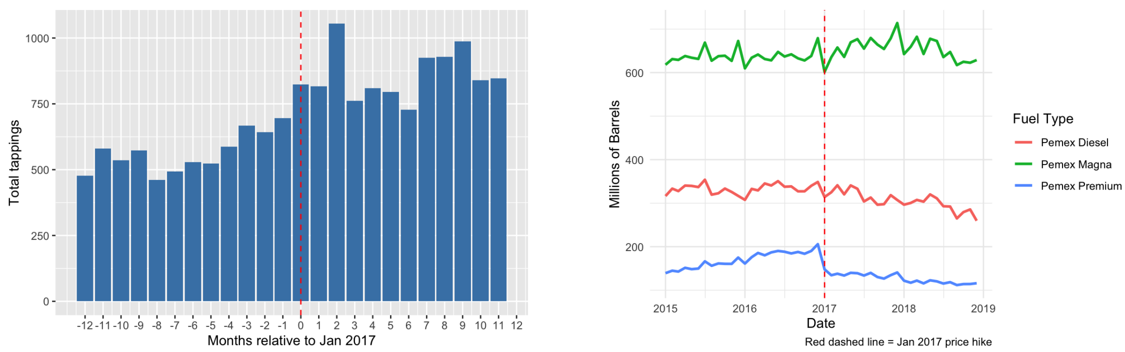
Comparison of population estimates for different bandwidths around pipelines. It contrasts the actual population counts for 2016 and 2017 with the expected counts for 2017 based on two growth rates: the national growth rate of 1.1% and the urban growth rate of 1.4%. The percentage differences between 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.

12.3 Anticipation

To further validate the findings, I built a bar plot showing monthly tapping activity before and after the price hike to discard anticipatory effects—that is, significant increases in tapping before the price change—which would challenge the causal interpretation of the policy impact. The absence of any such anticipation in the months preceding January 2017 strengthens the evidence that the price increase triggered a shift from legal to illegal fuel consumption rather than a preemptive response.

(a) Tappings *(b)* Sold barrels



Notes: Monthly data of tappings and sold barrels. Data from tappings is from a public information request with folio number 1857200094319. Data from sold barrels is from PEMEX.

The analysis includes legal sales of Pemex fuel—Magna, Premium, and Diesel—to provide an important benchmark for understanding changes in fuel consumption patterns around the 2017 price increase. 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. Tracking legal sales allows us to check whether the observed increase in illegal fuel tapping coincides with a decline in official fuel purchases, suggesting substitution toward illegal sources. This comparison helps rule out alternative explanations, such as seasonal fluctuations or changes in unrelated demand, strengthening the evidence that the price hike triggered a change from legal to illegal fuel consumption.

13 Robustness Checks

To assess whether the findings are robust to alternative explanations, I analyze medical procedures and obstetric hospital discharges.

13.1 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—we follow recent literature (Guidetti et al., 2021 [52]; Deryugina et al. 2019 [36]) and analyze the frequency of selected surgical procedures as placebo outcomes. The rationale is that if the increase in violence triggered widespread disruptions or improvements in healthcare infrastructure—whether due to increased strain on services or additional funding—we would expect to observe changes in the provision of unrelated medical procedures. We analyze a range of surgeries that vary in urgency and demographic specificity to detect any systematic changes in hospital capacity or access. Table 5 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

Table 5: 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. We classified these procedures using ICD-9-CM codes, detailed in Tables 6 and 7, and standardized their incidence by population or sex-specific subpopulations (e.g. men or women).

Table 6: Regression table Part 1 with the effects of the price increase on elective and urgent medical procedures

	(1) eeg_monitoring	(2) phimosis_surgery	(3) appendectomy	(4) hernia_repair
DD price	12.36	76.11	164.9	-91.47
inc.*pipeline	(16.53)	(69.50)	(178.7)	(168.4)
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 EEG monitoring, phimosis surgery, appendectomy, fracture repair, and hernia repair. I include time and municipality-fixed-effects. Both specifications have clustered standard errors in parentheses.

Table 7: Regression table Part 2 with the effects of the price increase on elective and urgent medical procedures

	(1)	(2)	(3)	(4)
	cholecystectomy	Hysterectomy	prostatectomia	hip_replace
DD price	69.97	0.0661	0.0426	-11.42
inc.*pipeline	(226.9)	(0.0694)	(0.0502)	(24.80)
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 $\mathrm{Post}_{\mathrm{t}}$ 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 Cholecystectomy, hysterectomy prostatectomy, and hip replacement. I include time and municipality-fixed-effects. Both specifications have clustered standard errors in parentheses.

13.2 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 both general 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 8: Regression table with the effects of the price increase in obstetric hospital discharges

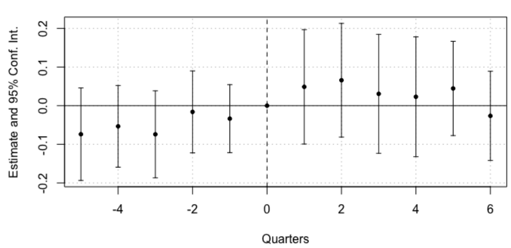
	(1) Abortion	(2) Contracep.	(3) C-sect	(4) Dystocic	(5) Birth
DD price inc.*pipeline	0.000501	-0.0225	0.00652	-0.00163	0.0729
	(0.00717)	(0.0463)	(0.0222)	(0.00417)	(0.0494)
Munic. F.E.	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.352	0.656	0.569	0.257	0.686
N	11100	11100	11100	11100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

Standard errors in parentheses

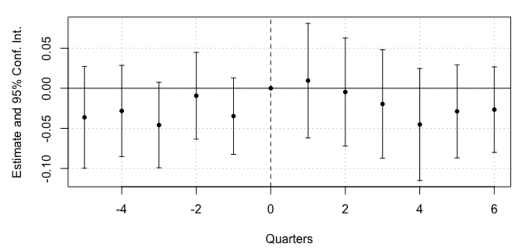
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. The outcome variables were obstetric hospital discharges. The data was obtained from INEGI. I include time and municipality-fixed-effects. Both specifications have clustered standard errors in parentheses.

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



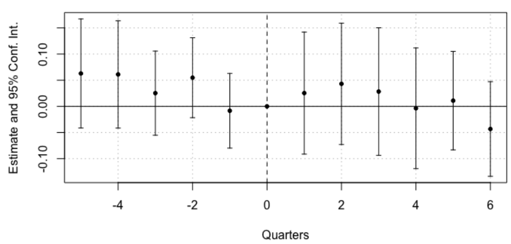
(b) Effects of the price increase on C-section rate among fertile women



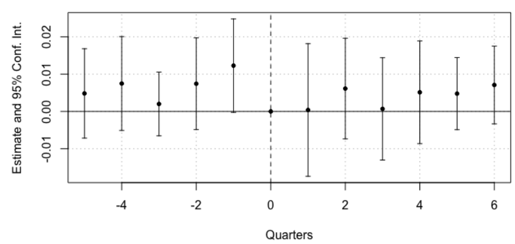
Notes: Results from estimating equation 1, where the dependent variable is the rate of births and C-sections from the third quarter of 2015 to the second quarter of 2018. The data source is the obstetric hospital discharges records from INEGI. The X-axis represents the quarters. The reference period 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 19a) within the study's time frame. Previous studies by Brown (2018 [22]) and Floridi et al. (2023 [44]) 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

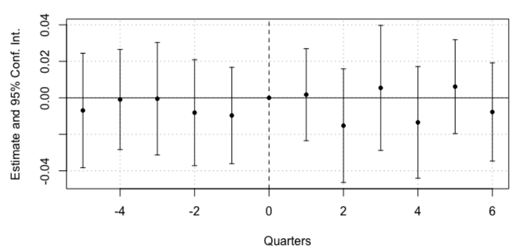


(b) Effects of the price increase on dystocic birth rate among fertile women



Notes: 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 25: 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.

13.3 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 9: Regression table with the effects of the price increase on maternal mortality

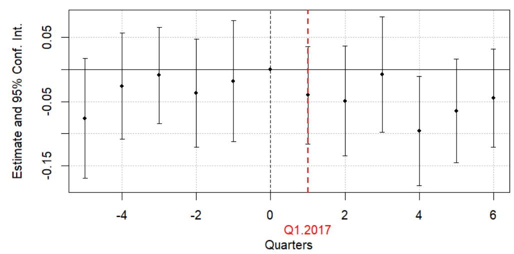
	(1) Maternal mort.
DD price	-0.0227
incr.*pipeline	(0.0167)
Municipality F.E.	Yes
Time F.E.	Yes
Adjust R2	11008
AIC	11520.9

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 $\text{AfterPrice}_\text{t}$ 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.

(a) 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.

13.4 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, all men, young men (ages 14–45), 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 results 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 10: Regression table with the effects of the price increase in suicides

	(1)	(2)	(3)	(4)	(5)
	Suicide	Men_suicide	Women_suicide	Young_men_suicide	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)
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

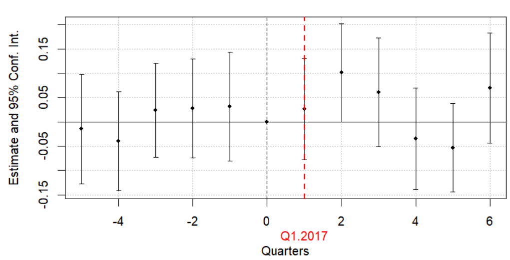
Clustered standard errors in parentheses

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

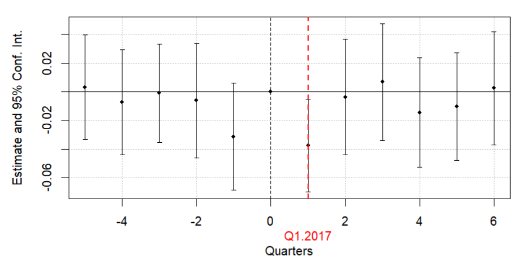
Notes:Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables were stress-related hospital discharges. 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.

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



Notes: 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.

14 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%.

Table 11: 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

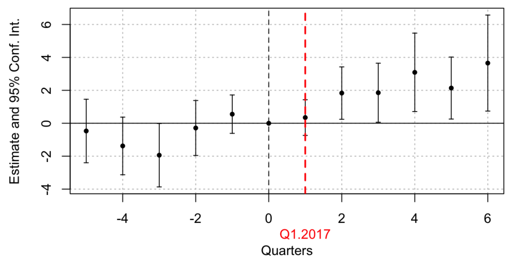
Clustered Standard errors in parentheses

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

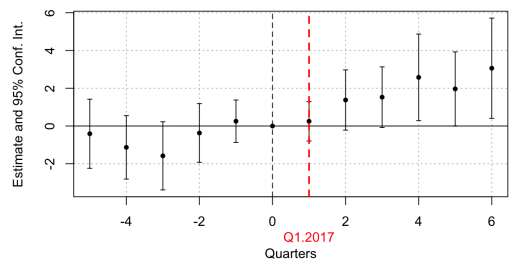
Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an $\mathrm{AfterPrice_t}$ indicator, which takes the value of one at the first quarter of 2017. I include time and municipality-fixed-effects.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

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

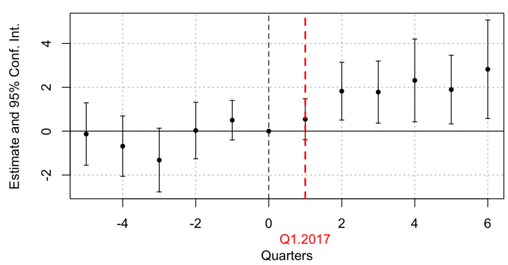


(b) Effects of the price increase hom. firearm

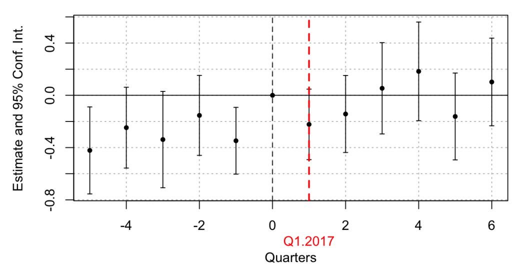


Notes: Results from estimating equation 1, where the dependent variables are the number of male homicides and firearm homicides from INEGI's mortality population counts from the third quarter of 2015 to the second quarter of 2018. The X-axis represents the quarters. The reference period is the fourth 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

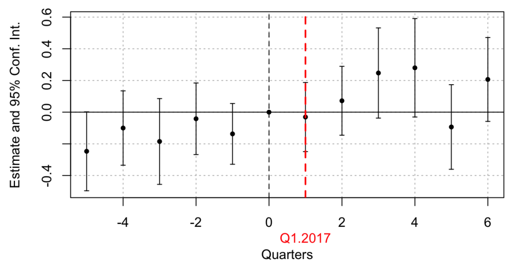


(b) Effects of the price increase female homicides



Notes: 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.

(a) 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.

15 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 were split into two different tables.

15.1 Birth certificate outcomes

Table 12: 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 inc.*pipeline	4.586	-0.0570	-0.0791	0.0514	0.00398	-0.163
	(2.848)	(0.160)	(0.0491)	(0.122)	(0.0110)	(0.158)
Munic. F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.480	0.119	0.013	0.170	0.319	0.108
N	10437	10437	10437	10437	10437	10437
AIC	119702.7	56557.0	33779.0	52950.3	2195.0	58438.6

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 $\mathrm{Post_t}$ indicator, which takes the value of one at the first quarter of 2017. Our outcome variables were the several prenatal outcomes. The data was obtained from the birth certificate registers collected by INEGI from the theird 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.

Table 13: Regression table part 2 with the effects of the price increase in birth outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	% teen	Prenatal care	Sex ratio	Apgar	% low Apgar	% anomaly
DD price	-0.253	0.00347	-1.375	0.00801	-0.104	-1.231***
inc.*pipeline	(0.334)	(0.0226)	(1.380)	(0.00726)	(0.0834)	(0.339)
Munic. F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.254	0.741	0.036	0.292	0.176	0.314
N	10437	10437	10405	10437	10437	10437
AIC	69698.6	14249.5	104425.1	-14515.4	38215.3	62665.7

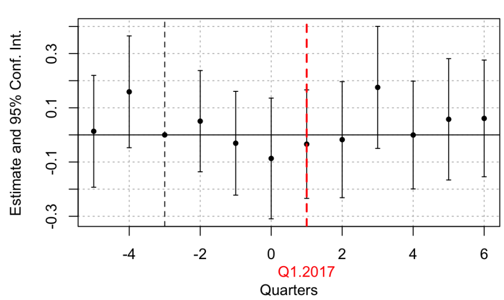
Standard errors in parentheses

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

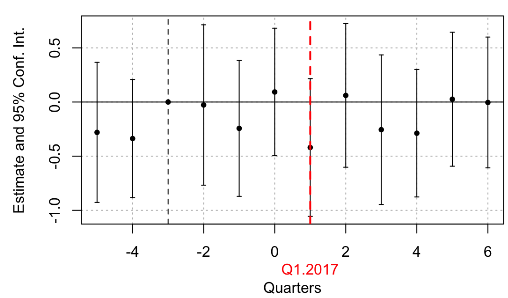
Notes: Results from estimating a version of equation (1) where instead of interacting the treatment group with time dummies (by quarter and year), I replace them by an Post_t indicator, which takes the value of one at the first quarter of 2017. Our outcome variables were the several prenatal outcomes. The data was obtained from the birth certificate registers collected by INEGI from the theird 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.

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



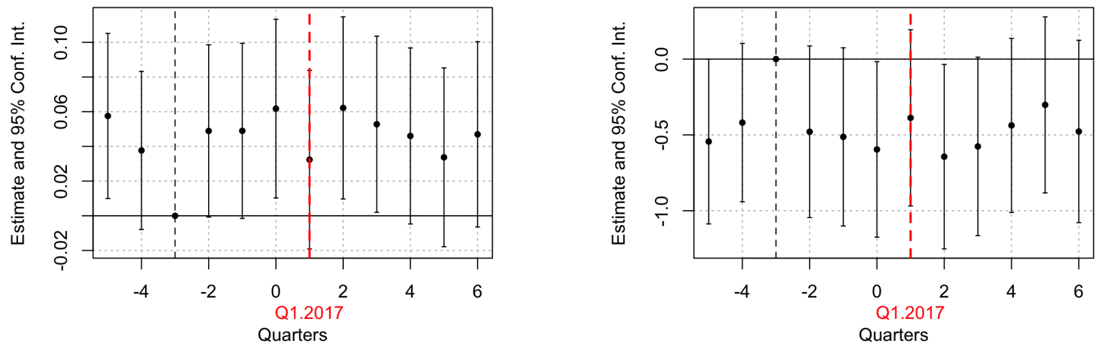
(b) Effects of the price incr. on % of macrosomia



Notes: 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

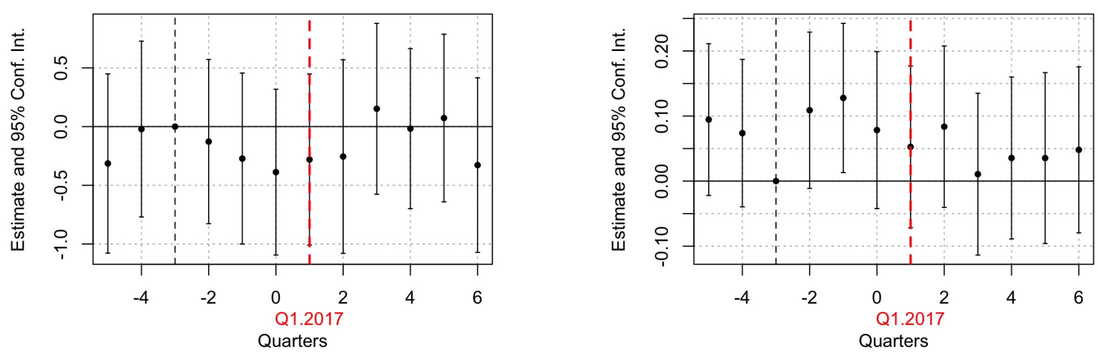
(b) Effects of the price incr. on % low Apgar



Notes: Results from estimating equation 1, where the dependent variables are the percentage of Apgar score and percentage of low Apgar (5 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.

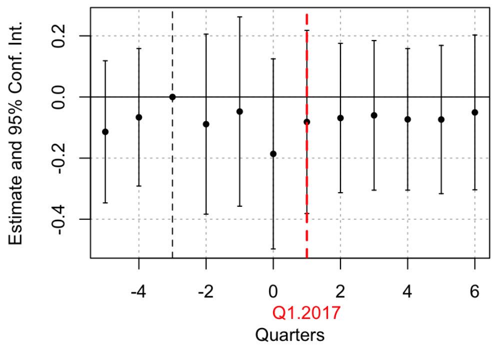
(a) Effects of the price increase on the percentage of premature babies

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

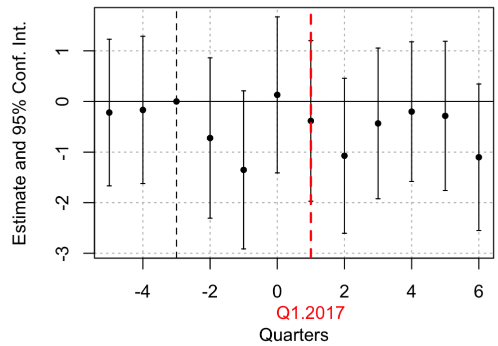


Notes: Results from estimating equation 1, where the dependent variables are the percentage of premature babies and average prenatal care (total number of visits) from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's birth certificates dataset. The X-axis represents 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

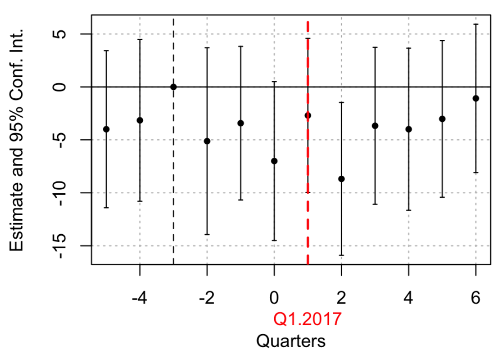


(b) Effects of the price incr. on % teen mothers



Notes: 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.

(a) 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.

15.2 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 14: 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	0.000388	0.000596	0.000854	-0.000404	0.511
inc.*pipeline	(0.00137)	(0.000897)	(0.00126)	(0.000479)	(0.428)
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: This table reports estimates from a difference-in-differences model as specified in Equation 1, where the treatment effect is captured by a post-policy indicator equal to 1 from 2017Q1 onwards. Outcomes include the number of prenatal deaths, complications during pregnancy, deaths during pregnancy and at birth, and average gestational age. All models include municipality and quarter fixed effects. Standard errors are clustered at the municipality level. The data come from INEGI's Death and Fetal Death Certificates covering 2015Q3 to 2018Q2.

Table 15: 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)
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

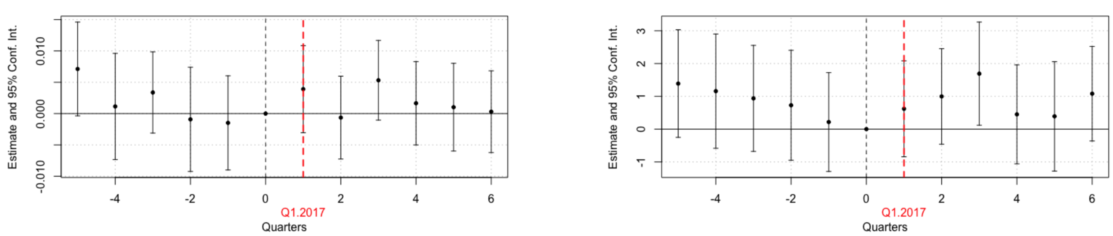
Standard errors in parentheses

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

Notes:This table presents results from the same specification as in Equation 1. The dependent variables include birth weight, rate of male fetus deaths, miscarriage rate, stillbirth rate, average number of prenatal visits, and maternal age. All models control for municipality and quarter fixed effects, and standard errors are clustered at the municipality level. The data source is INEGI's Death and Fetal Death Certificates from 2015Q3 to 2018Q2.

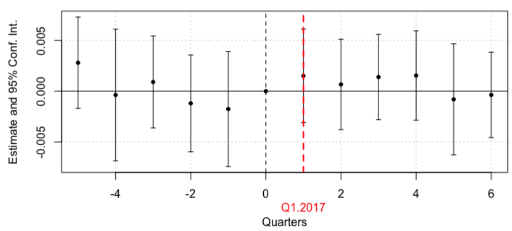
(a) Effects of the price increase on prenatal death rate among fertile women

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

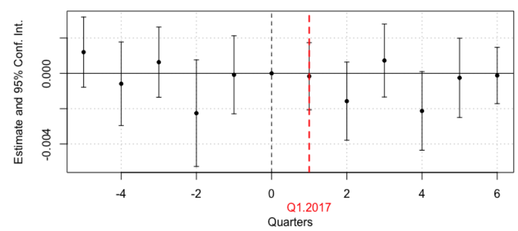


Notes: Results from estimating equation **??**, where the dependent variables are the percentage of women in that municipality that suffered a prenatal death and the average prenatal care (number of visits to the doctor) of women in that municipality from the third quarter of 2015 to the second quarter 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

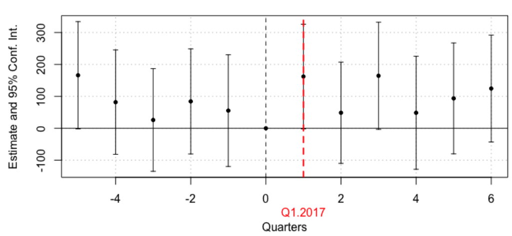


(b) Effects of the price increase on death at a birth rate among fertile women

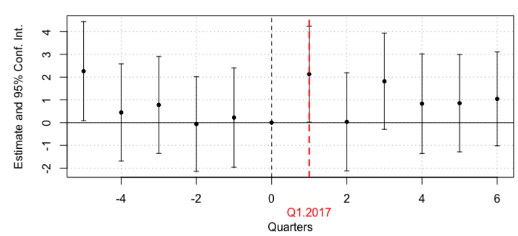


Notes: Results from estimating equation 1, where the dependent variables are the rate of women in that municipality who presented complications during pregnancy and those who suffered the death of the baby at birth from the third quarter of 2015 to the second quarter of 2018, both outcomes from INEGI's 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

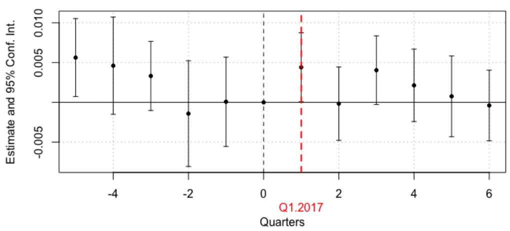


(b) Effects of the price increase on gest. age



Notes: 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.

(a) 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.

15.3 Infant and child mortality

In this section, I display the results from estimating equation 1 on infant and child mortality by gender. Furthermore, I show the event studies for girl's infant and child mortality.

Table 16: Regression table

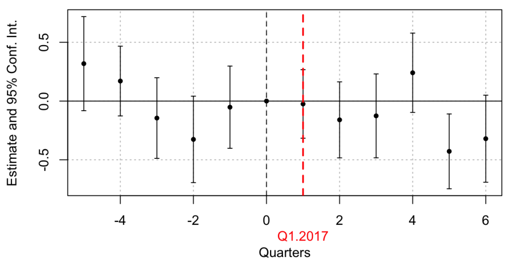
	(1) infant mort.	(2) infant boy mort.	(3) infant girl mort.	(4) child mort.	(5) boy child mort.	(6) girl child mort.
DD price inc.*pipeline	-0.320*** (0.121)	-0.182** (0.0843)	-0.131** (0.0634)	-0.298** (0.128)	-0.180** (0.0888)	-0.110 (0.0697)
Munic. F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjust R2	0.975	0.962	0.952	0.978	0.966	0.960
N	11008	11008	11008	11008	11008	11008
AIC	53097.9	44985.2	42086.4	54730.1	46564.9	43428.1

Standard errors in parentheses

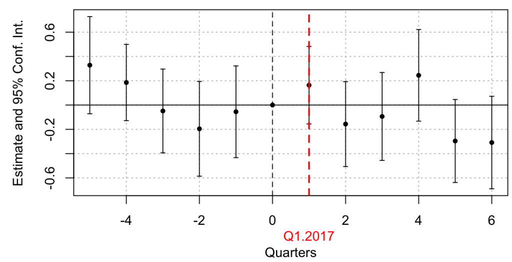
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 $\mathrm{Post_t}$ indicator, which takes the value of one at January 2017. The outcome variables consist on infant and child mortality. 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. Both specifications have clustered standard errors at the municipality level in parentheses.

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



(b) Effects of the price increase on girl child mortality



Notes: Results from estimating equation 1, where the dependent variables are girl infant and child 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.

15.4 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 17: Regression table with the effects of the price increase in stress hospital discharges

	(1)	(2)	(3)	(4)	(5)
	stress	stress_fert_women	stress_women	stress_young_men	stress_men
DD price inc.*pipeline	0.0000577 (0.000137)	0.000352 (0.000292)	0.000251 (0.000173)	-0.000180 (0.000286)	-0.000129 (0.000193)
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 were 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.

Table 18: Regression table with the effects of the price increase in psychotropic hospital discharges

	(1)	(2)	(3)	(4)	(5)
	psychotropic	psychot._fert_women	psychot._women	psychot._young_men	psychot._men
DD price inc.*pipeline	-0.000566 (0.000378)	-0.000271 (0.000375)	-0.000156 (0.000210)	-0.00264* (0.00137)	-0.00101 (0.000711)
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 were 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.

Table 19: 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)
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 Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables were 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.

Table 20: 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)
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 Post_t indicator, which takes the value of one at the first quarter of 2017. The outcome variables were 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.

15.5 Stress-related ICD codes

Table 21: 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

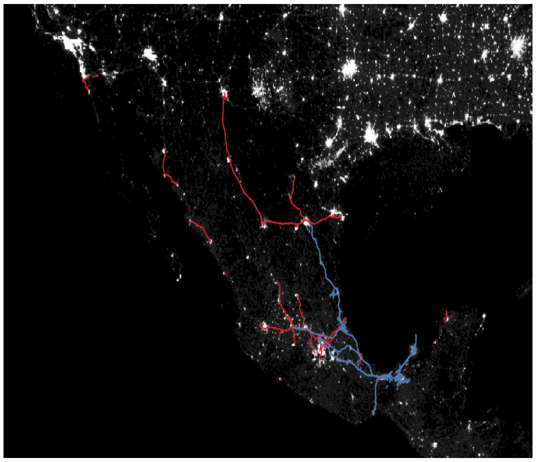
Diseases Related to Stress by General Category

16 Geospatial data

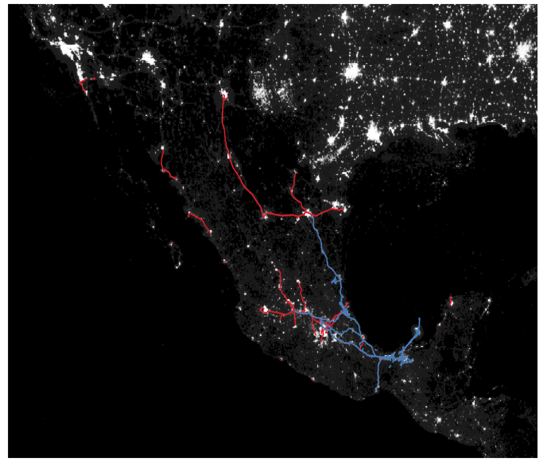
In this section, I visualize the entire Mexican territory using the original geospatial data from all sources employed in the analysis. The map overlays national pipeline infrastructure—sourced from CartoCritica onto municipal boundaries, providing a comprehensive view of areas potentially affected by 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 41a) and 2017 (Figure 41b).

(a) Nightlight and pipelines in 2016



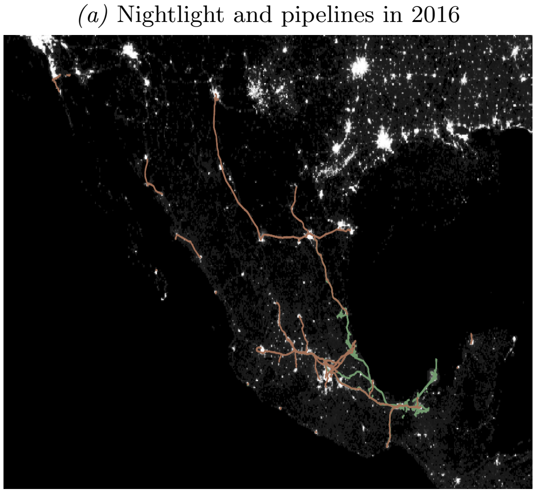
(b) Nightlight and pipelines in 2017



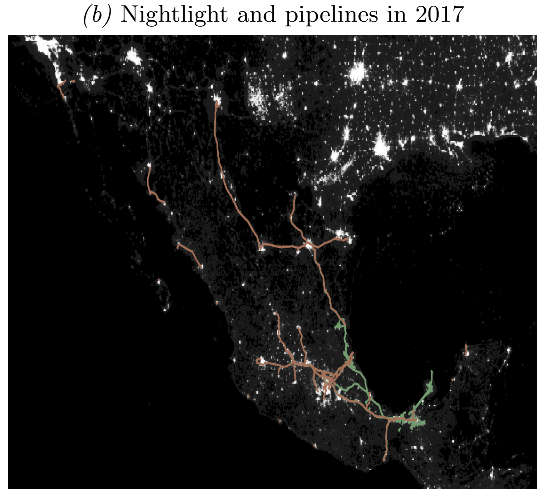
Notes: GeoTIFF files (raster image file types) with light intensity (radiance) by year with polyducts or multipurpose pipelines in red and oil pipelines in blue. Source: VIIRS (Visible Infrared Imaging Radiometer Suite) data from the Colorado School of Mines (Li et al., 2020[65]).

In the following figures it is shown the satellite data from DMSP/OLS in 2016 (Figure 42a) and 2017 (Figure 42b).

(a) Nightlight and pipelines in 2016



(b) Nightlight and pipelines in 2017

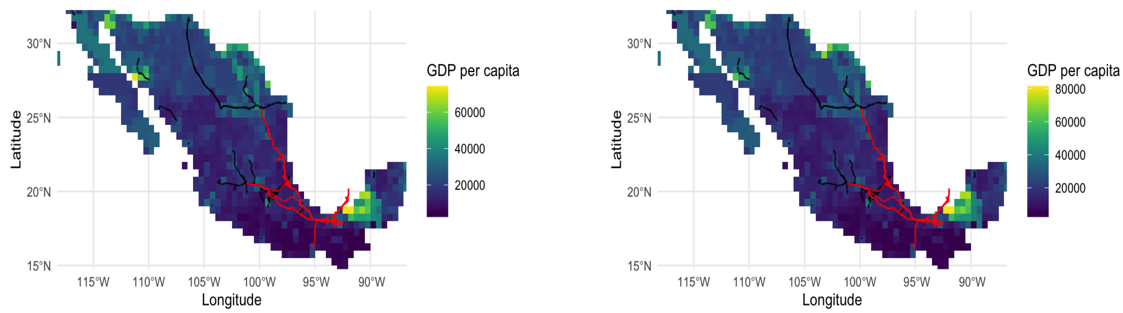


Notes: GeoTIFF files (raster image file types) with light intensity (radiance) by year with polyducts or multipurpose pipelines in orange and oil pipelines in green. Source: DMSP/OLS (Li and Zhou, 2017[64])

In Figures 18a and 18b it is shown data from downscaled global GDP per capita (PPP) dataset by Kummu et al. ([60]) with the oil and polyduct ppelines.

(a) GDPP and pipelines in 2016

(b) GDPP and pipelines in 2017

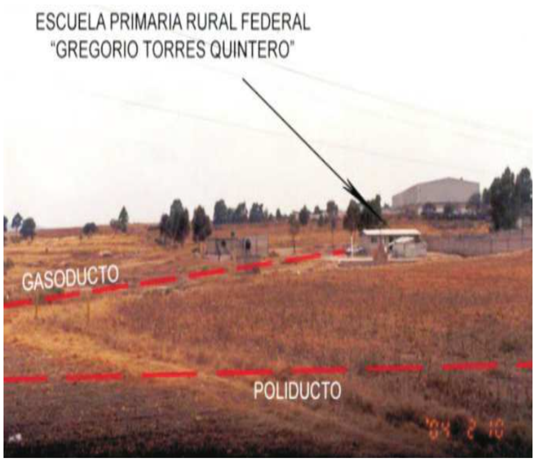


Notes: GeoTIFF files (raster image file types) with global GDP per capita (PPP) dataset by Kummu et al. ([60]) by year with polyducts or multipurpose pipelines in balck and oil pipelines in red. Source: DMSP/OLS (Li and Zhou, 2017[64])

17 Additional figures

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

(a) Pipelines



(b) Pipelines

Notes: Photos from Mexico State risk report about pipeline location

18 Descriptive statistics

IN DEVELOPMENT

Table 22: 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 23: 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 24: 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
% anomaly	6.40	5.11	4.54	3.86

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 25: 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 were 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 26: 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 were 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 27: 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 were 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.