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The Violence Paradox: Organized Crime and Mortality*

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Organized crime kills, but it also pays, creating opposing consequences for local communities. In this paper, we examine the impact of *huachicol*, the organized theft of fuel from Mexico's pipeline network, on two outcomes that capture these opposing effects: homicides and infant mortality. Using a difference-in-differences design, we exploit variation in pipeline presence and a fuel price shock that sharply increased the profitability of fuel theft. We find that the shock increased homicides but reduced infant mortality. We examine several potential mechanisms and find that improvements in local economic conditions provide the most plausible explanation for the decline in infant mortality. Specifically, we document an increase in nighttime lights, a proxy for economic activity, and a decline in infant hospitalizations for conditions that are particularly sensitive to household resources. Our findings reveal a violence paradox: while more profitable illicit markets increase homicides, they also reduce infant mortality, consistent with greater financial resources available to households due to fuel theft. This helps explain why some communities remain economically dependent on illicit markets despite rising violence.

Keywords: Illicit markets; homicides; infant mortality; organized crime
JEL codes: K42, O17, I10, I15

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1. INTRODUCTION

> *"When we were poor and the government turned a blind eye,*
> *he [El Chapo] was the one who lent us a hand, that's why we thank him.*
> *That's how we are in the mountains: a favor is never forgotten,*
> *it is always repaid with interest, even with life itself.*
> *That's why we protect him... "*

— Los Alegres del Barranco, *"La Gente Del Chapo"* (2012), own translation.

Organized crime can simultaneously increase violence and generate illicit income. It is often deeply rooted in local economies as an employer, a lender, and a de facto substitute for the state. Its overall effect on local communities is therefore ambiguous. The verse above, taken from a *narcocorrido*, a popular Mexican ballad that often glorifies drug lords and cartels, illustrates why this relationship is not straightforward. Indeed, Mexican municipalities with a long-established cartel presence perform better on a range of socioeconomic indicators (Murphy and Rossi, 2020). It is far less clear, however, what happens to these communities when an illicit market is disrupted by a sudden shock that may also trigger an increase in violence.

In this paper, we study the effects of *huachicol*, the organized theft of fuel from Mexico's pipeline network, on homicide and infant mortality. We exploit a natural experiment arising from Mexico's January 2017 fuel price liberalization (commonly known as the *gasolinazo*), a government policy that nationwide increased regulated retail fuel prices by up to 20%. This nationwide policy shock sharply increased the profitability of fuel theft. Because fuel can only be tapped where pipelines exist, the resulting expansion of *huachicol* took place in municipalities with a pipeline. Since the pipeline network was constructed decades before the outcomes we study, combining its predetermined geographic variation with the unexpected increase in fuel prices allows us to identify the causal effect of the increase in profitability of fuel theft. We focus on two measures of mortality that capture its opposing effects: firearm homicides among young men, a standard proxy for organized-crime violence in Mexico, and infant mortality, which reflects the broader consequences of organized crime for local communities.

The net effect of an increase in the profitability of fuel theft is theoretically ambiguous. Higher profits increase the returns to controlling pipelines, strengthening incentives for criminal organizations to compete over territory and potentially leading to greater violence. However, existing evidence on whether the earlier expansion of fuel theft increased violence is mixed (López Cruz and Torrens, 2023; Battiston et al., 2024). The implications for infant mortality are even less clear. On the one hand, if higher profitability intensifies violence, it may worsen fetal health through increased maternal stress during pregnancy and a deterioration in local living conditions, thereby increasing infant mortality. On the other hand, fuel theft is a locally organized activity whose expansion may increase local household income. If these income gains relax household budget

constraints, they may improve nutrition and sanitation during pregnancy and early childhood, thereby reducing infant mortality (Paxson and Schady, 2005; Bhalotra, 2010; Baird, Friedman and Schady, 2011).

We estimate the effects of *huachicol* using a difference-in-differences design with quarterly data. Specifically, we compare changes in homicide and infant mortality before and after the *gasolinazo* between municipalities with a pipeline and neighboring municipalities without one. To assess the validity of our treatment assignment, we draw on novel municipality-level data on illegal fuel tappings. We further investigate the underlying mechanisms using several individual-level administrative datasets and spatial satellite data. In particular, we use birth certificates and fetal death records to measure prenatal health; hospital discharge records to capture stress-related morbidity among women of reproductive age and the causes of infant hospitalizations; and harmonized satellite nighttime lights as a proxy for local economic activity.

We find that the rise in the profitability of *huachicol* sharply increased firearm homicides among young men by roughly one-third, corresponding to approximately 2,800 excess deaths, while reducing male infant mortality by about 4%. To understand these contrasting effects, we examine several potential mechanisms. We first test whether the increase in violence affected prenatal health through maternal stress. Consistent with the decline in infant mortality, we find no evidence of such a channel: birth weight, gestational age, prematurity, fetal mortality, and stress-related hospitalizations among women of reproductive age are all unaffected. We next examine whether the expansion of illicit fuel markets improved local economic conditions. Consistent with this hypothesis, satellite nighttime lights within 20 km of a pipeline increased by about 5 percent, indicating a substantial increase in local economic activity. Furthermore, infant hospitalizations declined for water- and hygiene-related conditions, which are sensitive to household resources. Taken together, these findings are consistent with local economic gains from illicit fuel markets improving infant health.

This paper contributes to three strands of literature. First, we contribute to the literature on the local consequences of organized crime by studying how an exogenous increase in the profitability of fuel theft affects violence and local welfare. Existing evidence points in different directions: criminal organizations may generate local economic benefits by providing employment, "clandestine welfare", or more stable local governance (Murphy and Rossi, 2020; Celestino, 2023; Pantaleão, 2026), yet they can also reduce investment, income, and economic activity (Pinotti, 2015; Gutiérrez-Romero and Oviedo, 2018; Fenizia and Saggio, 2024). Closest to our setting, López Cruz and Torrens (2023) show that cartel diversification into oil theft spread violence to pipeline locations of little strategic value for drug trafficking, whereas Battiston et al. (2024) find that cartel entry into oil theft did not increase homicides but reduced school attendance. We instead exploit a sharp change in the profitability of fuel theft and follow its consequences beyond violence to local economic activity and infant survival. We show that, in

a setting of rising profitability, organized crime increases violence but also raises local economic activity and lowers infant mortality, illustrating that illicit markets can generate local economic benefits even when they intensify violence.

Second, we contribute to the literature on violence and early-life health. Existing work shows that prenatal exposure to violence can worsen birth and child health outcomes, with maternal stress as an important mechanism (Camacho, 2008; Mansour and Rees, 2012; Koppensteiner and Manacorda, 2016; Quintana-Domeque and Ródenas-Serrano, 2017; Currie, Mueller-Smith and Rossin-Slater, 2022; Brown, 2018; Sardoschau, 2024). We show that, in a setting already characterized by chronic criminal activity, additional violence does not necessarily translate into worse infant survival. Instead, infant mortality declines, consistent with the income gains generated by the expansion of fuel theft. Thus, our findings highlight an economic channel through which illicit markets can affect early-life health that has received limited attention.

Finally, we contribute to the literature on how commodity and price shocks shape violence and conflict. Previous work shows that changes in commodity prices affect incentives for violent competition over valuable resources (Dube and Vargas, 2013; Berman et al., 2017), and that resource booms can increase local incomes while intensifying conflict (Angrist and Kugler, 2008). We extend this literature in two ways. First, we study a distinct source of profitability shocks: an increase in the legal retail price of fuel that raises the returns to fuel theft. Second, we examine consequences beyond conflict and income, showing that the same increase in profitability that intensifies violence also reduces infant mortality.

From a policy perspective, our findings reveal a violence paradox: the same increase in the profitability of organized crime that intensifies violence can also generate local economic benefits that improve health. This helps explain why illicit markets can persist despite their social costs and highlights that policies aimed at reducing organized crime may also remove an important source of income for affected communities. Our results therefore suggest that expanding legitimate economic opportunities may be an important complement to efforts to combat organized crime.

2. CONTEXT, DATA, AND IDENTIFICATION

2.1. Chronic violence and the Huachicolero

Homicides in Mexico increased sharply after the 2006 launch of the war on drugs and have stayed at elevated levels ever since (Appendix Figure A1). Enforcement has been overwhelmingly punitive and, based on existing evidence, has often intensified conflict rather than reduced it (Dell, 2015; Calderón et al., 2015; Lindo and Padilla-Romo, 2018). As a result, Mexicans are routinely exposed to violence in their local communities, and perceptions of insecurity are persistently high. Around the time of the *gasolinazo*, roughly 70% of Mexicans reported that their city was unsafe (INEGI, 2020). Our setting therefore allows us to study the effects

of an increase in violence within an already chronically violent environment, rather than the onset of violence itself.

Within this broader context of chronic violence, *huachicol*, the theft of fuel from Mexico's national pipeline network, had become one of the country's most lucrative illicit markets (Leon Saez, 2022). Its profitability depended on the regulated retail price of gasoline, because stolen fuel is resold at a discount to the pump price. Since 1938, Petróleos Mexicanos (PEMEX), Mexico's state-owned oil company, had held a monopoly over fuel retailing, with gasoline prices set nationally and little geographic variation (Davis, McRae and Bejarano, 2019). Although the government had announced a gradual transition toward market-based fuel pricing following the 2013 energy reform, it unexpectedly announced on December 27, 2016, that retail gasoline prices would increase by up to 20% effective January 1, 2017. The higher retail price increased the value of stolen fuel, sharply raising the profitability of fuel theft. The response is visible in the tapping records themselves: the number of illegal tappings recorded on the national pipeline network increased by roughly 52% between 2016 and 2017.[1]

Three features make this episode well suited for identification. First, the timing and magnitude of the price increase were announced only a few days before implementation, leaving little scope for anticipation. Second, the increase applied nationwide, independent of pipeline presence. Third, the pipeline network had been constructed decades before the reform, making its geographic location predetermined with respect to the policy.

2.2. Data

To estimate the effects of the January 2017 increase in the profitability of fuel theft, we construct a balanced panel of 925 municipalities observed over twelve quarters (2015Q3–2018Q2), covering the six quarters before and the six quarters after the January 2017 fuel price shock. The sample comprises 400 municipalities containing a fuel pipeline (treated) and their 525 first-order neighboring municipalities (controls). The resulting panel contains 11,100 municipality-quarter observations. Summary statistics are reported in Appendix Table A1 for the pooled sample and in Appendix Table A2 at the reference period.

To identify treated municipalities, we use the location of Mexico's national pipeline network. Because pipeline locations are classified, we rely on a reconstruction by the NGO CartoCrítica based on public and leaked sources, which has also been used by López Cruz and Torrens (2023) and Battiston et al. (2024).[2]

[1] Based on the authors' own calculations. This is likely a lower bound, as a subsequent audit found that 170 of 379 pipeline monitoring stations were not operating, implying that recorded tappings understate the true expansion of fuel theft. Furthermore, as stolen fuel is resold on a black market at a discount to the pump price, the pass-through from the regulated price increase of up to 20% to the margin on stolen fuel is not observed directly and need not be one-for-one. Our empirical design requires only that the margin moved in the same direction, which the acceleration of tappings supports.

[2] We validate this treatment assignment using monthly municipality-level records of illegal pipeline tappings obtained through a freedom-of-information request to Mexico's National Institute for Trans-

The mapped network crosses 400 of Mexico's 2,463 municipalities in 30 of the country's 32 states. Figure 1 presents the resulting treatment and control groups.

All outcomes are constructed from administrative data compiled by Mexico's National Institute of Statistics and Geography (INEGI). Violence is measured using firearm homicides among men aged 15–39, a standard proxy for organized crime violence in Mexico (Calderón et al., 2015; Magaloni et al., 2020). Early-life health is measured using birth certificates covering 3,826,695 live births, together with 29,590 fetal death certificates and vital statistics on infant and child mortality. From these records, we construct measures of prenatal health (birth weight, gestational age, prematurity, and prenatal care utilization), fetal survival (miscarriage and stillbirth rates), and postnatal mortality (infant and child deaths).[3] Consistent with the violence outcome, our infant mortality measure is based on deaths among boys, who are more vulnerable to nutritional and environmental stress (Chao et al., 2023).

We further use administrative hospital-discharge records to examine infant hospitalizations by diagnosis and stress-related hospitalizations among women of reproductive age. To measure local economic activity, we use harmonized DMSP-VIIRS satellite nighttime lights on a 5 km grid (Li et al., 2020), while gridded GlobPOP population estimates are used to assess population displacement (Liu et al., 2024).[4]

2.3. Empirical strategy

For each outcome Y_{mt} in municipality m and quarter t, we estimate the following event study:

$$Y_{mt} = \sum_{\substack{k=-5 \\ k\neq 0}}^{6} \beta_k \left(\text{Pipeline}_m \times \mathbf{1}[\tau_t = k]\right) + \eta_m + \delta_t + \varepsilon_{mt}, \tag{1}$$

where Pipeline_m equals one for municipalities with a pipeline, τ_t denotes the number of quarters between quarter t and the last quarter of 2016, and $\mathbf{1}[\tau_t = k]$ equals one if $\tau_t = k$. The last quarter of 2016, immediately preceding the *gasolinazo*, serves as the reference period ($k = 0$).

Municipality fixed effects η_m absorb time-invariant characteristics of a municipality, including the level effect of Pipeline_m, while quarter-year fixed effects

parency, Access to Information and Personal Data Protection (INAI, request no. 1857200094319). The correspondence between recorded tappings and mapped pipeline presence is 96.5%, and 83% of municipalities containing a pipeline recorded at least one illegal tapping between 2000 and 2019.

[3]Infant mortality captures deaths before age one, whereas child mortality captures deaths between ages one and five.

[4]ENIGH, the national income and expenditure survey, is representative only at the state level, so we proxy local activity with satellite nighttime lights (Henderson, Storeygard and Weil, 2012; Donaldson and Storeygard, 2016). Furthermore, because reliable quarterly municipality-level infant population denominators are unavailable, infant and child mortality and the hospitalizations of male infants are analyzed as quarterly counts rather than rates.

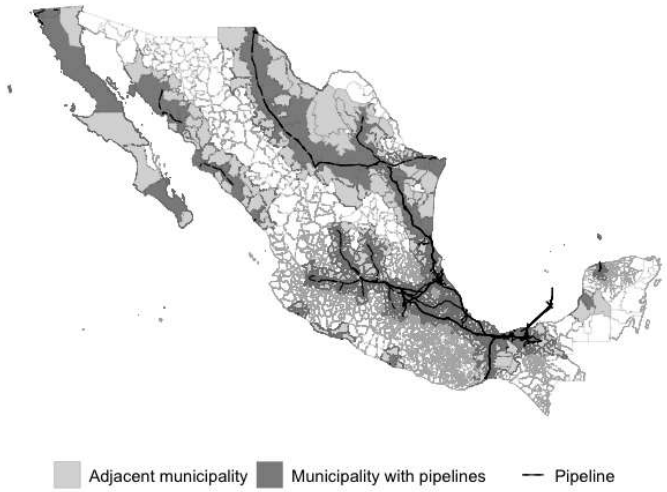


Figure 1. **The pipeline network: Treated and control municipalities**

Notes: Municipalities containing a fuel pipeline (treated) are shown in dark gray, and their first-order neighboring municipalities (controls) are shown in light gray. The pipeline network is drawn in black. Pipeline locations are taken from the NGO CartoCrítica.

δ_t absorb shocks common to all municipalities in a given quarter-year, such as national policy changes, macroeconomic conditions, and seasonality. Standard errors are clustered at the municipality level.

The coefficients β_k capture the evolution of treatment effects over time. Pre-treatment coefficients provide a test of the parallel-trends assumption, while post-treatment coefficients capture the dynamic effects of the increase in the profitability of fuel theft. The estimation window is symmetric around the *gasolinazo* and ends before the 2018 presidential election and the nationwide anti-fuel-theft campaign launched in early 2019, both of which substantially altered the behavior of organized crime (De Haro Lopez and Katovich, 2025).

Our identification strategy exploits the interaction between the predetermined location of Mexico's fuel pipeline network and the unexpected nationwide fuel price increase in January 2017. Since the price increase applied to all municipalities regardless of pipeline presence, we identify the effects of the increased profitability of fuel theft by comparing changes in outcomes in pipeline municipalities with changes in their first-order neighbors before and after the *gasolinazo*. These neighboring municipalities share regional labor markets, climate, health infrastructure, and exposure to state-level policies and criminal dynamics, but lack pipelines and thus direct access to fuel tapping. Identification relies on the assumption that, absent the *gasolinazo*, outcomes in the two groups would have followed parallel trends. Because the pipeline network was built decades

before the reform and the price increase was announced only days before taking effect, neither treatment assignment nor pre-reform outcomes could respond to the shock. For our main outcomes, the pre-treatment event-study coefficients are jointly insignificant, consistent with parallel trends. Section 5 discusses remaining threats to identification and alternative interpretations of the results.

3. RESULTS

Figure 2 presents the event-study estimates for our two mortality outcomes. Panel (a) shows a clear and persistent increase in firearm homicides among young men following the shock. The pre-treatment coefficients are jointly insignificant, and only one pre-treatment coefficient is borderline significant, with a point estimate of the opposite sign. Averaged across the post-shock quarters, the estimated effect is 1.165 additional homicides per municipality-quarter. Relative to the baseline mean of 3.562 homicides in treated municipalities, this corresponds to, on average, an increase of roughly 33%, or approximately 2,800 excess deaths across the 400 pipeline municipalities over the six post-shock quarters.

Table A3 in the Appendix reports the corresponding difference-in-differences estimates. It also shows that these results are robust to alternative definitions of homicide. Although the estimated effect for females is also positive and statistically significant, its magnitude is more than ten times smaller than that for males. Thus, as expected, the overall effect is driven by homicides among men.

For male infant mortality, Panel (b) reveals the opposite pattern to homicides, with the effect following a different dynamic. The response is substantially delayed as the estimated effects remain close to zero during the first four post-shock quarters before turning sharply negative in the fifth and sixth post-shock quarters (point estimates of -0.533 and -0.497, respectively). Averaged across the post-shock quarters, the estimated effect is a reduction of 0.182 deaths per municipality-quarter. Relative to the baseline mean of 4.678 deaths in treated municipalities, this corresponds to a decline, on average, of about 4%, or approximately 437 fewer male infant deaths across the six post-shock quarters. The pre-treatment coefficients are statistically insignificant.

The corresponding difference-in-differences estimates are reported in Appendix Table A4. Furthermore, the results show that the estimated effect is also negative for female infant mortality, although its magnitude is smaller than that for males, consistent with evidence that male infants are biologically more vulnerable and exhibit higher mortality risks during the first year of life than female infants (e.g., Drevenstedt et al., 2008; Austad and Fischer, 2016). In contrast, we find no evidence of an effect on child mortality, defined as deaths among children between the ages of one and five. Together, these findings suggest that the mortality gains are concentrated in the first year of life, consistent with evidence that infants are substantially more susceptible to adverse shocks than older children, particularly during the neonatal period (Lawn, Cousens and Zupan, 2005).

The delayed response in infant mortality is consistent with a mechanism op-

erating *in utero*. Accordingly, for the prenatal outcomes examined in the next section, we redefine the reference period based on conception rather than birth. Specifically, for outcomes determined *in utero*, we use the first quarter of 2016 as the reference period, corresponding to the last cohort whose entire gestation period preceded the *gasolinazo*.

4. MECHANISMS

4.1. The health channel

We begin our analysis of the underlying mechanisms with the direct health channel. A change in the profitability of an illicit market could shape early-life health by altering conditions in utero that determine infant survival.[5] Appendix Figure A2 shows event-study estimates for six outcomes measuring birth quality, fetal survival, and maternal stress.[6] If the shock affected infant survival through conditions *in utero*, these outcomes should respond after the shock.

We find no evidence that this channel explains our results. Panels (a) and (b) show that the share of low-birth-weight infants and average gestational age are unchanged among cohorts exposed to the shock in utero, indicating no deterioration in conditions at birth. Panels (c) and (d) show no change in the stillbirth or miscarriage rate, suggesting that fetal survival was also unaffected.

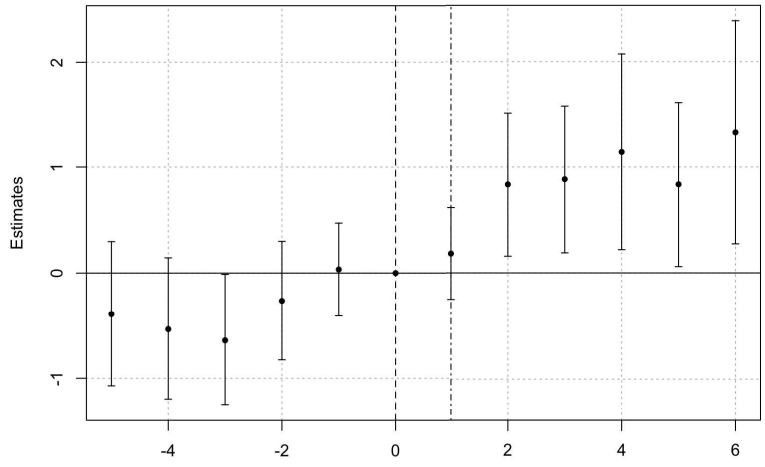
We also find no evidence of a stress response among women of reproductive age. Panels (e) and (f) present event-study estimates for hospital discharges related to anxiety and stress-induced cerebrovascular conditions. Both remain flat despite the sustained increase in homicides, which may reflect weaker psychological responses to additional violence in settings where violence is already chronic. Taken together, the findings provide no evidence that the increase in the profitability of the illicit market leads to a decrease in infant mortality through direct health effects operating via a prenatal channel.

4.2. The income channel

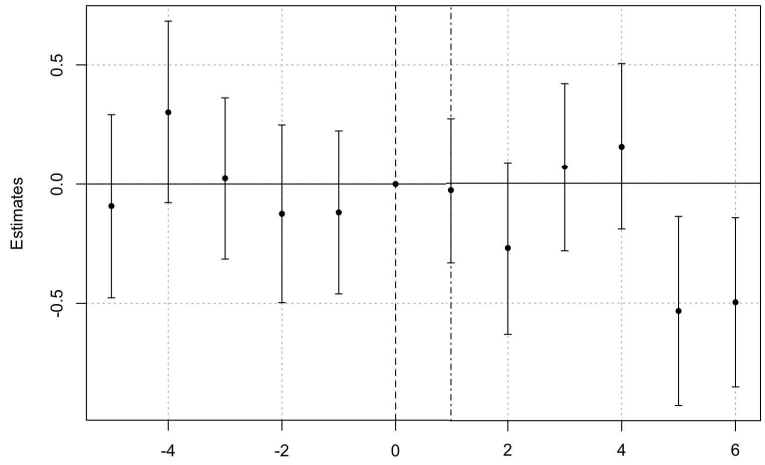
Since we find no evidence of a direct health effect, we ask whether our results can instead reflect higher income generated by the illicit market. Higher *huachicol* profitability could shape local welfare in two opposing ways. By raising the value of a contested, appropriable resource, it increases the returns to controlling it and can intensify violence, in line with the literature on rapacity and the resource curse (Dube and Vargas, 2013; Berman et al., 2017; Couttenier, Grosjean and Sangnier,

[5]The existing literature shows, for example, that exposure to maternal stress can shorten gestation and reduce birth weight (Camacho, 2008; Mansour and Rees, 2012; Koppensteiner and Manacorda, 2016; Currie, Mueller-Smith and Rossin-Slater, 2022).

[6]The samples differ across outcomes because they are constructed from distinct administrative records. Birth-related outcomes are based on live-birth certificates, while fetal mortality is measured using fetal-death certificates. Thus, the sample for outcomes among live births consists of mothers whose pregnancies resulted in a live birth, whereas the fetal-death sample consists of mothers whose pregnancies ended in a recorded fetal death.



(a) Homicides



(b) Infant mortality

Figure 2. **The violence paradox: Event-study estimates**

Notes: Panel (a) reports the event-study coefficients from Equation (1), where the dependent variable is the quarterly count of firearm homicides among men aged 15–39. Panel (b) reports the corresponding coefficients where the dependent variable is the quarterly count of deaths among boys under one year of age. The fourth quarter of 2016 serves as the reference period, denoted by the dashed line. The dot-dash line denotes the first quarter of 2017, when the fuel price shock began. All specifications include municipality and quarter-year fixed effects. Error bars denote 95% confidence intervals based on standard errors clustered at the municipality level.

2017). At the same time, by raising local incomes it can relax household budget constraints and improve infant survival (Baird, Friedman and Schady, 2011).

First, we examine whether the increase in *huachicol* profitability indeed generated local economic gains. Because the national income and expenditure survey (ENIGH) is representative only at the state level, we instead use annual nighttime light intensity as a proxy for local economic activity (Henderson, Storeygard and Weil, 2012; Donaldson and Storeygard, 2016). In particular, we estimate

$$y_{ij} = \beta \left(\text{Post}_j \times \text{Near}_i\right) + \alpha_i + \delta_{mj} + \varepsilon_{ij}, \tag{2}$$

where i denotes 5 km $\times$ 5 km grid cells, m the municipality containing cell i, and $j \in \{2016, 2017\}$ the year. Near_i identifies cells within 20 km of a pipeline, while α_i and δ_{mj} denote cell and municipality-year fixed effects, respectively.[7] Thus, β identifies the differential change in nighttime lights for more- versus less-exposed cells within the same municipality, relative to the pre-treatment period, after accounting for municipality-year shocks common to all cells.

Panel A of Table 1 shows that the rise in *huachicol* profitability was accompanied by a substantial increase in local economic activity. Cells within 20 km of a pipeline experienced a 0.519-unit increase in luminosity relative to more distant cells, corresponding to roughly 5 percent of the pre-treatment mean. The estimate is stable across specifications and alternative definitions of the treatment and control groups.

Panel B examines whether these economic gains reached infant health. Water- and hygiene-related hospitalizations, such as intestinal, skin, and urinary-tract infections, decline by 0.093 admissions per municipality-quarter, a statistically significant reduction. In contrast, we find no significant change in hospitalizations related to nutrition, housing and respiratory conditions, or severe complications. This pattern points to a water-and-sanitation mechanism: safe water is scarce and costly in Mexico, so higher household income can relax this constraint by allowing families to purchase, treat, and safely store water or improve sanitation. The finding is consistent with a large literature showing that better access to safe water reduces infant and child mortality in developing countries (Galiani, Gertler and Schargrodsky, 2005; Gamper-Rabindran, Khan and Timmins, 2010).

[7]We use a 20 km threshold because, among pipeline municipalities, the median municipality has 100% of its population living within 20 km of the pipeline. Appendix Table A5 shows that the results are robust to alternative threshold definitions, including a continuous distance measure. While illegal tapping itself may generate light through nocturnal siphoning, transport, and related infrastructure, the increase extends beyond areas immediately surrounding the pipelines, suggesting that it does not merely reflect light from theft activity itself.

Table 1— The income channel

Panel A: Local Economic Activity

	(1) Baseline	(2) Restricted	(3) All Mexico
Post × Near Pipeline	0.519***	0.531***	0.576***
	(0.098)	(0.099)	(0.072)
Grid Cell FE	Yes	Yes	Yes
Year FE	–	–	Yes
Municipality-Year FE	Yes	Yes	No
Adj. R^2	0.982	0.981	0.973
N	47,914	45,886	157,118

Panel B: Hospitalizations

	(1) Water & Hygiene	(2) Housing	(3) Nutrition	(4) Severe Complications
DD price inc. × pipeline	-0.0931***	0.0487	0.0129	-0.0094
	(0.0256)	(0.101)	(0.0115)	(0.0159)
Municipality FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Adj. R^2	0.588	0.672	0.495	0.425
N	11,100	11,100	11,100	11,100

Notes: Panel A reports estimates of the effect of pipeline proximity on nighttime light intensity. The dependent variable is mean nighttime light intensity in 5km×5km grid cells. Post = 1 for 2017 relative to the 2016 baseline. Near Pipeline indicates grid cells within 20km of any pipeline segment. Column (1) reports our baseline specification, which includes municipality-year fixed effects, so that identification comes from comparing grid cells near and far from a pipeline within the same municipality. The sample is restricted to the 383 municipalities containing both types of cells. Column (2) further restricts the sample to 313 municipalities containing at least three near and three far grid cells. Column (3) uses all grid cells in Mexico and compares cells near a pipeline with all other cells, with year fixed effects. All Panel A specifications include grid-cell fixed effects, which also absorb any time-invariant characteristic of the municipality. Panel B reports estimates of the effect of the price increase on hospitalizations among male infants under age one. The treatment group is interacted with an AfterPrice_t indicator, which takes the value of one from the first quarter of 2017 onward. Water/hygiene-related hospitalizations include intestinal infectious diseases (ICD-10 A00-A08), skin and subcutaneous tissue infections (L00-L08), and unspecified urinary tract infection (N39.0). Housing-related hospitalizations include upper and lower respiratory infections (ICD-10 J00-J06 and J12-J21). Nutrition-related hospitalizations include protein-energy malnutrition (E40-E46) and nutritional anemia (D50-D53). Severe complications include dehydration (E86) and sepsis (A40-A41). All Panel B specifications include municipality and time fixed effects. The analysis covers the period from the third quarter of 2015 to the second quarter of 2018. Standard errors clustered at the municipality level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Taken together, the evidence suggests that the same shock generated both higher violence and improved infant health through its effects on local economic activity. The increase in *huachicol* profitability raised local economic activity by roughly 5 percent, increasing the value of the resource available for appropriation and thereby contributing to the rise in firearm homicides, while the associated income gains relaxed household budget constraints, for example, by improving access to safe water and sanitation, thereby improving infant survival. The income channel therefore provides a coherent explanation for the apparent paradox of rising violence alongside declining infant mortality.

5. THREATS TO IDENTIFICATION AND ALTERNATIVE EXPLANATIONS

5.1. Spillovers

Our design assumes that the shock does not spill over into control municipalities. If the effects in pipeline municipalities reach their first-order neighbors, the controls become partially treated, which compresses the contrast between treated and control municipalities and attenuates the estimates toward zero. We test for such spillovers by re-estimating the design on successive spatial rings, comparing first- with second-order neighbors and second- with third-order neighbors. Appendix Figures A3 and A4 show no systematic change after the shock in either homicides or male infant mortality across rings. This indicates that both effects are concentrated in pipeline municipalities and that first-order neighbors provide a suitable control group. For infant mortality, the concern is sharper because the effect plausibly operates through local economic activity, which can diffuse across municipal borders. If neighboring municipalities shared in these gains, their infant mortality would also fall, and our estimate would understate the true decline. The alternative, that pipeline municipalities grew at the expense of their neighbors, would produce a negative luminosity gradient around the pipelines. Instead, Appendix Table A5 shows that the estimated gains extend outward with the exposure radius. Any remaining spillovers are therefore more likely to attenuate than to amplify the estimated decline in infant mortality.

5.2. Migration and compositional change

A further concern is that the shock triggered differential migration, changing the population at risk rather than mortality rates. Because reliable quarterly population denominators are unavailable, we measure both outcomes as counts, which makes them sensitive to changes in population size. Rising violence could push families out of pipeline municipalities, while income from fuel theft could draw migrants in. For infant mortality, either flow would alter the size and composition of the infant population. For homicides, out-migration would reduce the population at risk and work against our finding, whereas an inflow of young men drawn by illicit income could raise homicide counts mechanically.

We find little support for either concern. Using gridded GlobPOP data (Liu et al., 2024), we compare the observed population around the pipeline network in 2016 and 2017 with the counts expected under national and urban demographic growth. Appendix Table A6 reports the results for distance buffers of 1 to 50 km around the pipeline network. Observed growth tracks these benchmarks, and no buffer shows either the decline that out-migration would produce or the surge that large-scale in-migration would require to account for a one-third increase in homicides. For infant mortality, the 2015 Intercensal Survey points the same way. It records that 92% of women of reproductive age had lived in the same municipality over the previous five years, compared with 84.5% of the population as a whole, implying low baseline mobility for the group that matters most. The stability of the population at risk suggests that both results reflect changes in mortality rather than in who lives near a pipeline.

5.3. Selective fertility

A further concern is that the shock altered the composition of births rather than infant survival prospects. Because infant mortality is measured among live births, changes in who conceives or carries a pregnancy to term could affect the estimates: if higher violence or income deterred conceptions among higher-risk mothers, or selectively reduced pregnancies through miscarriage or abortion, the surviving cohort could appear healthier even absent any improvement in early-life conditions. We find no evidence of such selection. Appendix Table A7 shows no differential change in the fertility rate or in abortion-related hospitalizations, and Appendix Table A8 shows that the composition of births is stable across maternal age, prenatal-care utilization, and the sex ratio at birth.

5.4. Healthcare capacity

Finally, changes in healthcare capacity could provide an alternative explanation for our findings. For instance, the increase in violence may raise demand for hospital services, potentially straining local healthcare capacity. If capacity became constrained, fewer patients might be treated or recorded, potentially affecting both outcomes and the mechanisms we study. Guidetti, Pereda and Severnini (2021) show that binding hospital-capacity constraints can displace elective procedures while leaving urgent procedures largely unaffected. Appendix Table A9 shows no systematic changes in either elective or urgent procedures, suggesting that changes in healthcare capacity are unlikely to account for the observed effects.

6. CONCLUSION

Organized crime can generate both substantial social costs and local economic benefits. Exploiting Mexico's 2017 fuel price shock as an exogenous increase in

the profitability of fuel theft, we show that the expansion of *huachicol* sharply increased firearm homicides among young men while reducing infant mortality. We find no evidence that the increase in violence worsened prenatal health through maternal stress. Instead, our results are consistent with improved local economic conditions, reflected in higher nighttime light intensity and fewer infant hospitalizations for water- and hygiene-related conditions, driving the decline in infant mortality.

Our findings reveal the violence paradox at the heart of organized crime: illicit markets can simultaneously generate substantial social costs through violence and local economic benefits that improve health. This helps explain why communities continue to rely on illicit markets despite rising violence and underscores the importance of considering both dimensions when evaluating policies aimed at reducing organized crime.

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Online Appendix

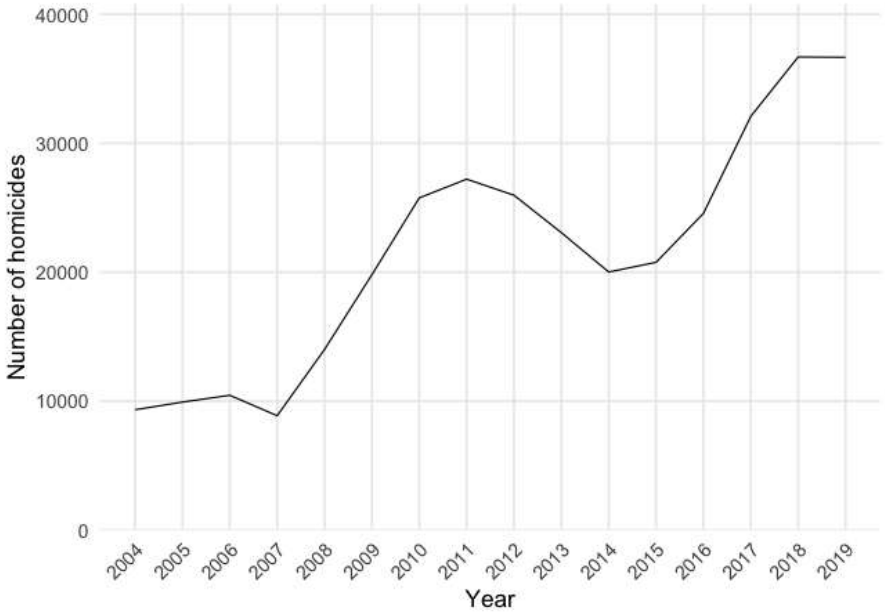


Figure A1. Homicides in Mexico, by year

Notes: Total homicides recorded in the INEGI vital-statistics mortality files. Homicides rise sharply after the 2006 launch of the war on drugs and remain high, the chronic-violence baseline against which the 2017 shock is an increment (Section 2.1). Data source: INEGI mortality records.

Table A1— Summary statistics by treatment status: Pooled sample

	Treated		Control	
	Mean	StD	Mean	StD
Panel A: Violence[a]				
All homicides	9.032	29.141	2.868	9.076
Male homicides	8.004	26.176	2.547	8.174
Firearm homicides	6.112	21.442	1.933	6.672
Young male homicides	5.176	17.000	1.594	5.336
Fire. young male homicides	3.826	13.356	1.175	4.209
Female homicides	0.999	3.149	0.314	1.080
Fem. fertile-age homicides	0.748	2.466	0.224	0.802
Panel B: Infant and Child Mortality[b]				
Infant mortality	8.062	20.921	2.902	14.286
Infant boy mortality	4.522	11.829	1.606	7.950
Infant girl mortality	3.506	9.206	1.281	6.352
Child mortality	1.275	3.322	0.612	2.795
Boy child mortality	0.682	1.828	0.338	1.576
Girl child mortality	0.592	1.693	0.274	1.324
Panel C: Fertility and Reproductive Outcomes[c,d]				
Fertility (births per 1,000 women 15–49)	14.117	5.739	14.897	5.648
Mother's age (years)	25.187	1.077	25.152	1.474
Prenatal care visits	7.713	0.917	7.414	1.051
Sex ratio (girls per boy)	0.975	0.246	1.013	0.490
Birth weight (g)	3147.292	91.264	3148.510	116.868
Gestational age (weeks)	38.723	0.272	38.777	0.386
Low birth weight (%)	6.083	3.090	5.998	4.595
Premature (%)	6.334	3.153	6.307	5.158
Panel D: Fetal Death and Pregnancy Complications[e]				
Miscarriage (per 1,000 women 15–49)	0.016	0.041	0.017	0.096
Stillbirth (per 1,000 women 15–49)	0.003	0.021	0.003	0.029
Panel E: Obstetric Hospital Discharges[f]				
Abortions (per 1,000 women 15–49)	0.750	0.616	0.842	0.812
Panel F: Procedures (per 100,000 inhabitants)[g]				
EEG monitoring	0.049	0.521	0.026	0.294
Phimosis surgery	0.018	0.458	0.002	0.056
Hernia repair	2.523	4.372	2.897	6.264
Cholecystectomy	6.105	8.863	6.115	11.497
Hysterectomy (per 100,000 women)	2.881	5.924	2.984	7.859
Prostatectomy (per 100,000 men)	0.904	4.228	1.064	5.404
Hip replacement	0.099	0.565	0.110	1.136
Appendectomy	4.237	7.655	4.272	8.274
Neonatal intubation	0.033	0.308	0.039	0.692
Panel G: Infant Hospitalizations (boys under one)[h]				
Water & hygiene	0.272	0.965	0.093	0.453
Housing (respiratory)	3.427	9.992	1.373	4.077
Nutrition	0.126	0.545	0.052	0.288
Severe complications	0.180	0.656	0.059	0.302
Panel H: Stress-Related Hospital Discharges (per 1,000 women 15–49)[h]				
Anxiety	0.004	0.019	0.004	0.034
Stress-related cerebrovascular	0.010	0.042	0.011	0.060
Panel I: Nighttime Lights[i]				
Mean radiance (5 km × 5 km grid cells)	10.564	13.331	3.366	5.251

Notes: Treated are municipalities containing a fuel pipeline. Controls are their first-order neighboring municipalities without a pipeline. Statistics are pooled across all quarters from 2015Q3 to 2018Q2, except for Panel I (2016–2017). Homicides, infant and child mortality, and infant hospitalizations are counts per municipality-quarter. Fertility, miscarriage, stillbirth, abortions, and stress-related discharges are rates per 1,000 women aged 15–49 per quarter; procedures are rates per 100,000 inhabitants (hysterectomy per 100,000 women, prostatectomy per 100,000 men). In Panel I the unit is the 5 km × 5 km grid cell, "Treated" are cells within 20 km of a pipeline and "Control" are cells farther away. Births and fetal deaths are assigned to the quarter of conception, imputed from gestational age, to align exposure during pregnancy with the timing of the *gasolinazo*. Sources: [a] INEGI homicide-specific mortality records. [b] INEGI all-cause mortality records. [c] Constructed from birth certificates and the CONAPO-corrected population denominator. [d] INEGI birth certificates (live births only). [e] INEGI fetal-death certificates (pregnancies not resulting in a live birth). [f] INEGI hospital-discharge records, obstetric procedures. [g] INEGI hospital-discharge records, elective and urgent procedures. [h] INEGI hospital-discharge records. [i] Harmonized DMSP-VIIRS nighttime lights (Li et al., 2020), restricted to the 383 municipalities with grid cells on both sides of the 20 km threshold (baseline sample of Table 1).

Table A2— Summary statistics by treatment status: Reference period

	Treated		Control	
	Mean	StD	Mean	StD
Panel A: Violence[a]				
All homicides	8.662	26.904	2.935	8.889
Male homicides	7.638	24.040	2.667	8.180
Firearm homicides	5.768	19.219	1.994	6.487
Young male homicides	4.850	15.596	1.646	5.131
Fire. young male homicides	3.562	12.027	1.202	3.971
Female homicides	1.008	3.022	0.261	0.890
Fem. fertile-age homicides	0.698	2.145	0.185	0.623
Panel B: Infant and Child Mortality[b]				
Infant mortality	8.310	21.339	3.000	14.943
Infant boy mortality	4.678	11.975	1.669	7.742
Infant girl mortality	3.610	9.494	1.312	7.163
Child mortality	1.288	3.490	0.596	2.598
Boy child mortality	0.735	1.870	0.312	1.475
Girl child mortality	0.550	1.775	0.284	1.271
Panel C: Fertility and Reproductive Outcomes[c,d]				
Fertility (births per 1,000 women 15–49)	12.632	6.186	13.247	6.114
Mother's age (years)	25.134	1.110	25.076	1.426
Prenatal care visits	7.741	0.918	7.464	1.041
Sex ratio (girls per boy)	0.975	0.207	1.010	0.513
Birth weight (g)	3148.575	89.050	3153.308	107.421
Gestational age (weeks)	38.751	0.272	38.784	0.355
Low birth weight (%)	5.973	3.048	6.008	4.596
Premature (%)	6.306	2.842	6.087	4.498
Panel D: Fetal Death and Pregnancy Complications[e]				
Miscarriage (per 1,000 women 15–49)	0.016	0.046	0.012	0.047
Stillbirth (per 1,000 women 15–49)	0.002	0.012	0.003	0.035
Panel E: Obstetric Hospital Discharges[f]				
Abortions (per 1,000 women 15–49)	0.738	0.772	0.808	0.756
Panel F: Procedures (per 100,000 inhabitants)[g]				
EEG monitoring	0.045	0.359	0.032	0.298
Phimosis surgery	0.009	0.166	0.002	0.046
Hernia repair	2.555	4.363	2.759	6.393
Cholecystectomy	6.287	10.177	6.117	13.256
Hysterectomy (per 100,000 women)	2.394	4.717	2.937	7.144
Prostatectomy (per 100,000 men)	0.586	1.744	0.484	2.102
Hip replacement	0.176	0.831	0.103	0.646
Appendectomy	3.903	5.972	4.321	8.630
Neonatal intubation	0.022	0.237	0.021	0.300
Panel G: Infant Hospitalizations (boys under one)[h]				
Water & hygiene	0.263	0.978	0.088	0.371
Housing (respiratory)	4.938	12.461	1.863	4.887
Nutrition	0.115	0.564	0.057	0.291
Severe complications	0.157	0.615	0.053	0.257
Panel H: Stress-Related Hospital Discharges (per 1,000 women 15–49)[h]				
Anxiety	0.003	0.015	0.004	0.026
Stress-related cerebrovascular	0.010	0.040	0.016	0.094
Panel I: Nighttime Lights[i]				
Mean radiance (5 km × 5 km grid cells)	9.602	13.423	2.765	5.025

Notes: Treated are municipalities containing a fuel pipeline. Controls are their first-order neighboring municipalities without a pipeline. Statistics are computed at the reference period. Panels A, B, E, F, G, and H use 2016Q4, the quarter immediately preceding the *gasolinazo*. Panels C and D use 2016Q1, the last conception cohort whose entire pregnancy preceded the shock. Panel I uses 2016, the pre-shock year; its unit is the 5 km × 5 km grid cell, with "Treated" being cells within 20 km of a pipeline and "Control" cells farther away. Homicides, infant and child mortality, and infant hospitalizations are counts per municipality-quarter. Fertility, miscarriage, stillbirth, abortions, and stress-related discharges are rates per 1,000 women aged 15–49 per quarter; procedures are rates per 100,000 inhabitants (hysterectomy per 100,000 women, prostatectomy per 100,000 men). Births and fetal deaths are assigned to the quarter of conception, imputed from gestational age, to align exposure during pregnancy with the timing of the *gasolinazo*. Sources: [a]INEGI homicide-specific mortality records. [b]INEGI all-cause mortality records. [c]Constructed from birth certificates and the CONAPO-corrected population denominator. [d]INEGI birth certificates (live births only). [e]INEGI fetal-death certificates (pregnancies not resulting in a live birth). [f]INEGI hospital-discharge records, obstetric procedures. [g]INEGI hospital-discharge records, elective and urgent procedures. [h]INEGI hospital-discharge records. [i]Harmonized DMSP-VIIRS nighttime lights (Li et al., 2020), restricted to the 383 municipalities with grid cells on both sides of the 20 km threshold (baseline sample of Table 1).

Table A3— Effect of the fuel-price shock on homicides

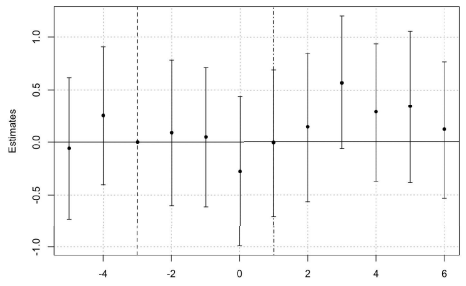
	(1) Fire. young male	(2) All	(3) Male	(4) Firearm	(5) Young male	(6) Female	(7) Fem. fert.
DD price	1.165***	2.380***	2.174***	1.908***	1.424***	0.187***	0.204***
incr.*pipeline	(0.400)	(0.764)	(0.699)	(0.631)	(0.465)	(0.0704)	(0.0579)
Munic. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.815	0.861	0.857	0.821	0.851	0.773	0.744
N	11100	11100	11100	11100	11100	11100	11100

Notes: Results from a difference-in-differences version of Equation (1), in which the event-time indicators are replaced by a single interaction between Pipeline_m and an AfterPrice_t indicator equal to one from the first quarter of 2017 onward. The outcomes are firearm homicides of men aged 15–39 (column 1), all homicides (column 2), male homicides (column 3), firearm homicides (column 4), homicides of men aged 15–39 (column 5), female homicides (column 6), and homicides of women aged 15–49 (column 7). The analysis covers the period from the third quarter of 2015 to the second quarter of 2018. Data are drawn from INEGI mortality records. All models include municipality and time fixed effects. Standard errors clustered at the municipality level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

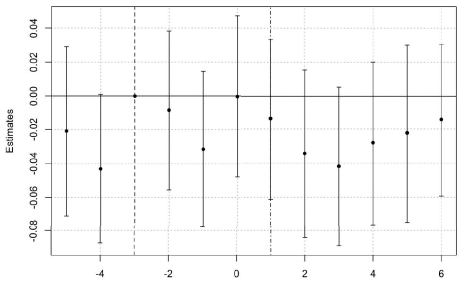
Table A4— Effect of the fuel-price shock on infant and child mortality

	(1) infant boy mort.	(2) infant mort.	(3) infant girl mort.	(4) child mort.	(5) boy child mort.	(6) girl child mort.
DD price	-0.182**	-0.320***	-0.131**	0.0219	0.0018	0.020
inc.*pipeline	(0.083)	(0.12)	(0.063)	(0.036)	(0.0304)	(0.024)
Munic. FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R2	0.962	0.975	0.952	0.898	0.824	0.806
N	11100	11100	11100	11100	11100	11100

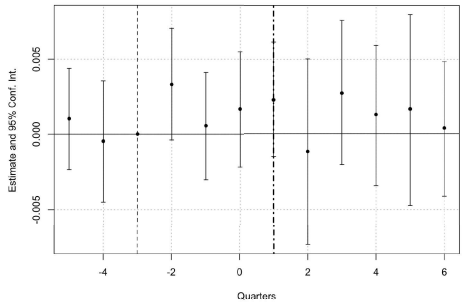
*Notes:*Results from a difference-in-differences version of Equation (1), in which the event-time indicators are replaced by a single interaction between Pipeline_m and an AfterPrice_t indicator equal to one from the first quarter of 2017 onward. The outcomes are infant mortality (deaths before age one) for boys, all children, and girls in columns 1–3, and child mortality (deaths between ages one and five) for all children, boys, and girls in columns 4–6. The analysis covers the period from the third quarter of 2015 to the second quarter of 2018. Data are drawn from INEGI mortality records. All models include municipality and time fixed effects. Standard errors clustered at the municipality level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.



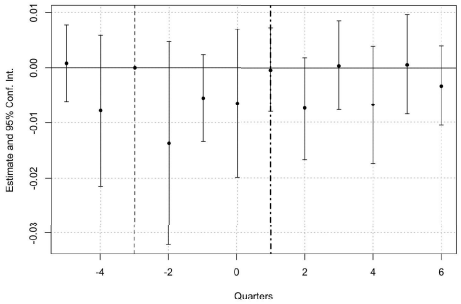
(a) % low birth weight



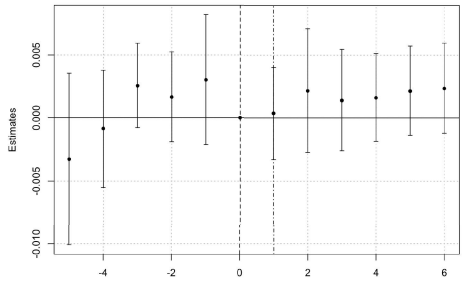
(b) Gestational age



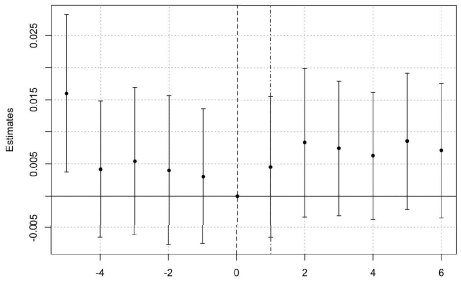
(c) Stillbirth rate



(d) Miscarriage rate



(e) Anxiety, women 15–49



(f) Stress-related cerebrovascular, women 15–49

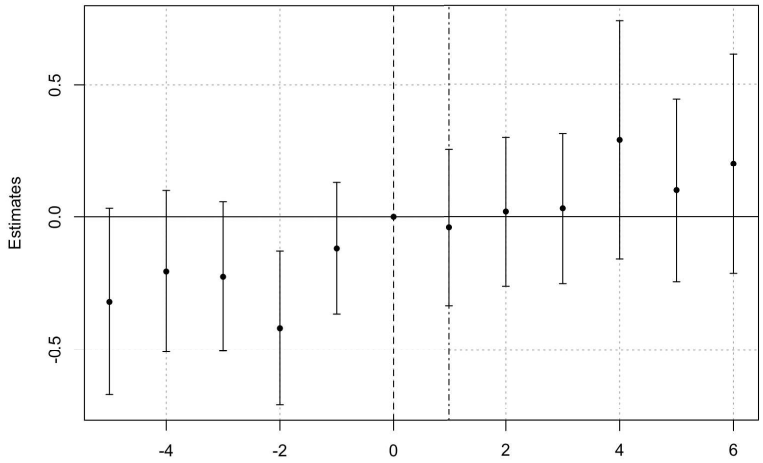
Figure A2. The health channel: Event study estimates

Notes: Panels (a)–(f) report event-study coefficients from Equation (1). Panels (a)–(d) are prenatal outcomes aligned by conception quarter (2016Q1 reference): the share of low-birth-weight babies, gestational age, the stillbirth rate, and the miscarriage rate. Panels (e)–(f) are stress-related hospital discharges among women aged 15–49 in calendar time (2016Q4 reference). Error bars denote 95% confidence intervals based on standard errors clustered at the municipality level. Sources: INEGI birth certificates, fetal-death certificates, and hospital-discharge records.

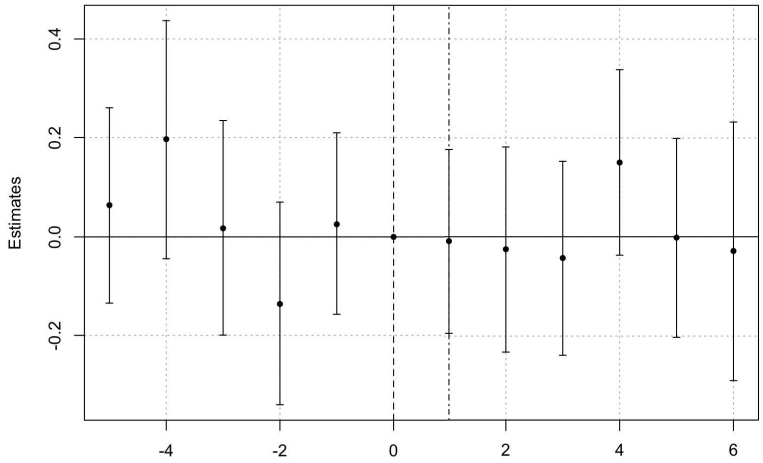
Table A5— Nighttime lights: Robustness to alternative exposure definitions

Panel A: Alternative Distance Thresholds						
	5km	10km	15km	20km	25km	30km
Post × Threshold	0.196	0.284**	0.381***	0.519***	0.670***	0.793***
	(0.150)	(0.128)	(0.106)	(0.098)	(0.099)	(0.109)
Grid Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	31,742	36,718	43,012	47,914	45,324	42,704
Number of municipalities	152	224	296	383	289	217
Panel B: Continuous Distance Specification						
Post × Distance			−0.728 ∗ ∗			
			(0.187)			
Grid Cell FE			Yes			
Municipality-Year FE			Yes			
N			47,914			
Number of municipalities			383			

Notes: Panel A redefines high exposure using alternative distance thresholds around the pipeline network. For each threshold, the sample includes only municipalities containing grid cells on both sides of the cutoff, ensuring within-municipality identification. Effects generally increase as the exposure radius expands, consistent with spillovers extending beyond the immediate pipeline corridor. Panel B replaces the binary exposure definition with continuous standardized distance to the nearest pipeline, estimated within the same sample as the preferred specification. The negative coefficient indicates monotonic attenuation with distance. Clustered standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.



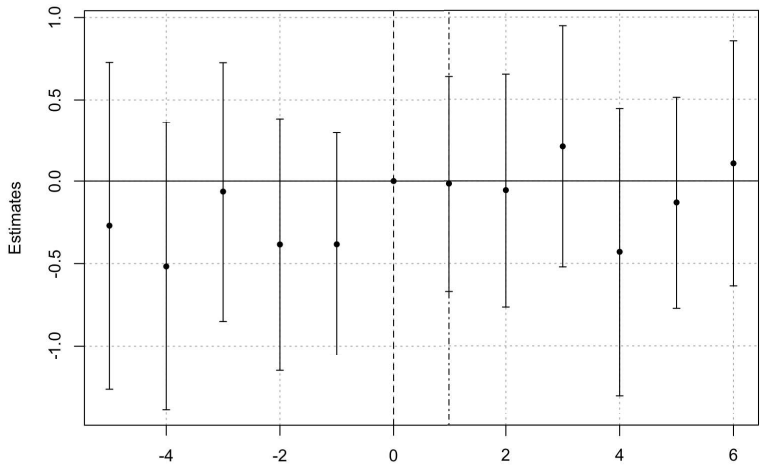
(a) Homicides



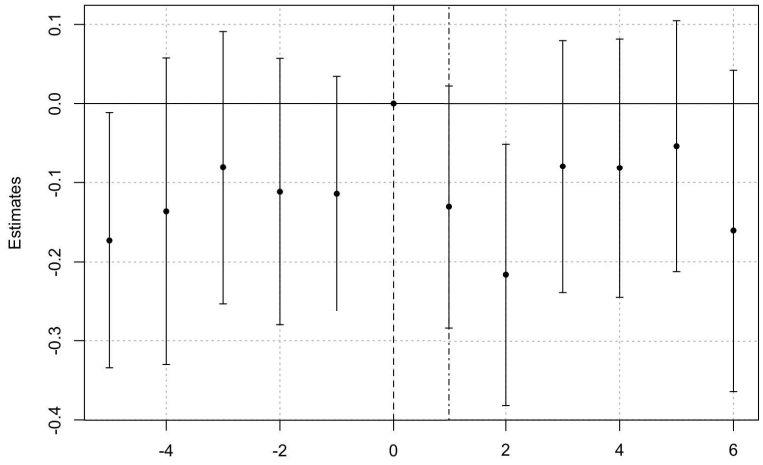
(b) Infant mortality

Figure A3. Spillovers: First-order vs. Second-order neighbors

Notes: Panel (a) reports the event-study coefficients from Equation (1), where the dependent variable is the quarterly count of firearm homicides among men aged 15–39, comparing first-order to second-order neighboring municipalities. Panel (b) reports the corresponding coefficients where the dependent variable is the quarterly count of deaths among boys under one year of age. The fourth quarter of 2016 serves as the reference period, denoted by the dashed line. The dot-dash line denotes the first quarter of 2017, when the fuel price shock began. All specifications include municipality and quarter-year fixed effects. Error bars denote 95% confidence intervals based on standard errors clustered at the municipality level.



(a) Homicides



(b) Infant mortality

Figure A4. Spillovers: Second-order vs. Third-order neighbors

Notes: Panel (a) reports the event-study coefficients from Equation (1), where the dependent variable is the quarterly count of firearm homicides among men aged 15–39, comparing second-order to third-order neighboring municipalities. Panel (b) reports the corresponding coefficients where the dependent variable is the quarterly count of deaths among boys under one year of age. The fourth quarter of 2016 serves as the reference period, denoted by the dashed line. The dot-dash line denotes the first quarter of 2017, when the fuel price shock began. All specifications include municipality and quarter-year fixed effects. Error bars denote 95% confidence intervals based on standard errors clustered at the municipality level.

Table A6— Observed versus expected population growth around the pipeline network, 2016–2017

Distance to pipeline network (km)	2016 Population	2017 Population	National (1.1%)		Urban (1.4%)	
			Expected	Diff. (%)	Expected	Diff. (%)
50	97,029,392	98,171,616	98,096,715	+0.08	98,387,803	−0.22
45	94,881,680	95,992,704	95,925,378	+0.07	96,210,024	−0.23
40	92,774,784	93,863,456	93,795,307	+0.07	94,073,631	−0.22
35	90,515,296	91,576,048	91,510,964	+0.07	91,782,510	−0.22
30	88,038,704	89,081,424	89,007,130	+0.08	89,271,246	−0.21
25	84,898,432	85,914,736	85,832,315	+0.10	86,087,010	−0.20
20	81,197,728	82,172,008	82,090,903	+0.10	82,334,496	−0.20
15	74,509,344	75,412,408	75,328,947	+0.11	75,552,475	−0.19
10	61,716,372	62,462,700	62,395,252	+0.11	62,580,401	−0.19
5	39,386,512	39,876,412	39,819,764	+0.14	39,937,923	−0.15
1	9,702,723	9,872,619	9,809,453	+0.64	9,838,561	+0.35

Notes: Observed population living within the stated distance of the pipeline network (polyducts and oil pipelines together, the network used to define treatment) in 2016 and 2017, compared with the 2017 counts expected under the national (1.1%) and urban (1.4%) annual growth rates applied to the 2016 observed population; "Diff." is the percentage gap between observed and expected 2017 population. Growth rates: World Bank, Mexico (SP.POP.GROW, SP.URB.GROW); a later data pull will show different figures for the same year due to routine revisions. Population from gridded GlobPOP data (Liu et al., 2024). Buffers are dissolved to avoid double-counting overlapping pipeline segments; distances computed in a Mexico-centred LAEA projection.

Table A7— Effect of the fuel-price shock on fertility and abortions

	(1) Fertility	(2) Abortions
DD price inc.*pipeline	-0.365 (0.3704)	-0.0231 (0.0262)
Munic. F.E.	Yes	Yes
Time F.E.	Yes	Yes
Adj. R^2	0.561	0.333
N	11,100	11,100

Notes: Results from a difference-in-differences version of Equation (1), in which the event-time indicators are replaced by a single interaction between Pipeline_m and an AfterPrice_t indicator equal to one from the first quarter of 2017 onward. Column 1 reports the fertility rate (births per 1,000 women of reproductive age), constructed from INEGI birth certificate counts and interpolated municipal population data from CONAPO; this measure includes all registered births, even when information on individual characteristics such as birth weight or gestational age is missing. Column 2 reports the rate of abortion-related hospital discharges per 1,000 women of reproductive age, drawn from INEGI hospital-discharge records. Neither estimate is statistically significant. The analysis covers the third quarter of 2015 to the second quarter of 2018. All models include municipality and time fixed effects, with standard errors clustered at the municipality level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8— Effect of the fuel-price shock on maternal composition at birth

	(1) Mother's age	(2) Prenatal care	(3) Sex ratio
DD price	0.0338	-0.0382	0.00826
inc.*pipeline	(0.0377)	(0.0258)	(0.0143)
Munic. F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes
Adj. R^2	0.394	0.709	0.030
N	11,076	11,076	11,032

Notes: Results from a difference-in-differences version of Equation (1), in which the event-time indicators are replaced by a single interaction between Pipeline_m and an AfterPrice_t indicator equal to one from the first quarter of 2017 onward. Outcomes are the average age of mothers in years (column 1), the average number of prenatal-care visits (column 2), and the sex ratio at birth, expressed as female over male (column 3), all drawn from INEGI birth certificates. These regressions exclude records with missing individual-level information, which is why the sample sizes are smaller than in Table A7. None of the estimates is statistically significant, providing no evidence of a compositional change among mothers. The analysis covers the third quarter of 2015 to the second quarter of 2018. All models include municipality and time fixed effects, with standard errors clustered at the municipality level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A9— Effect of the fuel-price shock on elective and urgent medical procedures

Panel A: Elective Procedures

	(1) EEG monitoring	(2) Phimosis surgery	(3) Hernia repair	(4) Cholecystectomy	(5) Hysterectomy	(6) Prostatectomy	(7) Hip replacement
DD price inc. × pipeline	0.00629	0.0181	-0.151	-0.404	0.177	0.103	-0.00425
	(0.0174)	(0.0133)	(0.185)	(0.376)	(0.249)	(0.190)	(0.0338)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.016	−0.001	0.268	0.416	0.237	0.052	0.045
N	11,100	11,100	11,100	11,100	11,100	11,100	11,100

Panel B: Urgent Procedures

	(1) Appendectomy	(2) Neonatal intubation
DD price inc. × pipeline	-0.147	-0.0200
	(0.254)	(0.0219)
Municipality FE	Yes	Yes
Time FE	Yes	Yes
Adj. R^2	0.363	0.037
N	11,100	11,100

Notes: The table reports difference-in-differences estimates of the effect of the price increase on hospital procedures. All specifications include municipality and time fixed effects, with standard errors clustered at the municipality level. Panel A reports elective procedures and Panel B reports urgent procedures. The classification of EEG monitoring, phimosis surgery, and appendectomy follows Guidetti, Pereda and Severnini (2021).$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$