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Railway Construction and Violence against Women: Evidence from Mexico's Tren Maya*

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Abstract

Large infrastructure projects are promoted as development strategies, yet their social consequences are rarely studied. We examine how the construction of Mexico's Tren Maya, a 1,554-kilometer railway, affected violence against women. Exploiting the staggered start of construction across railway sections in a difference-in-differences design, we find that construction increased assault-related hospitalisations of women by 57.8%, driven by intimate partner violence (67.5%) and non-partner assaults (50.8%). Reported crimes of sexual abuse rose by 40.2%. We find no increase in intimate partner violence against men, while estimates for other male outcomes are not informative. Construction also reshaped local labour markets, expanding jobs in the male-dominated transport sector and female-dominated food services. These shifts are consistent with two potential channels: a male backlash against women's employment, increasing intimate partner violence, and an influx of male workers, increasing assaults against women in public spaces.

Keywords: Infrastructure, Railway, Violence against women, Intimate partner violence, Mexico

JEL Codes: J16, K42, O18, O54

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1 Introduction

The Tren Maya, a USD 28 billion investment for a 1,554-kilometer railway across southeastern Mexico, is one of the most important infrastructure projects built in Mexico in the last decades to boost tourism, create employment, and reduce regional disparities (CONACYT, 2019; de la Rosa, 2024). Yet, large-scale infrastructure projects do not simply add connectivity to existing socioeconomic structures; rather, they fundamentally reorganise local economies, labour markets, and social relations (Cottafava et al., 2024; De Nito et al., 2024). Whether this reorganization ultimately improves or worsens the welfare of the local population is an open empirical question. While economic geographers have documented infrastructure’s effects on regional income and employment (Dinkelman, 2011; Donaldson, 2018), fewer studies have examined its unintended consequences such as violence and crime, especially against women.

This paper focuses on how the levels of violence against women have been affected during the period of the construction of the Tren Maya. We use hospital injury records from the General Directorate of Health Information (DGIS), distinguishing between intimate partner violence (IPV) and non-IPV assaults against women. We complement these with crime data from the Executive Secretariat of the National Public Security System (SESNSP) as a measure of violence reporting. We estimate the effect by exploiting the staggered timing of construction across railway sections whose construction started in two different years.

We find that during construction there have been large and significant increases in violence against women across multiple outcomes. Assault-related hospitalisations of women increased by 57.8%, with the effect growing across the two post-treatment years. Decomposing this aggregate by perpetrator, IPV-related assault hospitalisations rose by 67.5%, and non-IPV assault hospitalisations by 50.8%, with effects emerging in the second post-treatment year. In addition, we find a significant increase in sexual abuse crimes of 40.2%. We find no significant increase in violence against men, although most male estimates fail the parallel-trends test.

We then examine whether changes in local labour markets help explain our results. Using administrative data on formal employment, we find that employment in transport rose by 135.5% , a male-dominated sector, and by 68.8% in food services, a female-dominated sector (INEGI, 2023, 2024). These changes are consistent with two channels. First, standard household bargaining models predict that greater female employment should reduce IPV by improving women’s economic conditions (Manser and Brown, 1980; Aizer, 2010). Yet we find the opposite, which is more consistent with a male backlash model: rapid shifts in gender roles can challenge traditional male authority and generate conflict within the household (Macmillan and Gartner, 1999; Guarnieri and Rainer, 2018; Bergvall, 2024). Second, the arrival of male workers may have increased women’s exposure to violence outside the household. Consistent with this channel, assaults against women recorded in public spaces rose by 78.5%, with no comparable effects for men.

This paper contributes to several literatures. First, we provide new causal evidence on the social impacts of large-scale infrastructure projects, a recent but growing literature (Cottafava et al., 2024; De Nito et al., 2024). While most of these works examine infrastructure’s effects on aggregate crime (Calamunci and Lonsky, 2025; Schmutz, 2023; Biswas and Jain, 2025), we focus specifically on violence against women. Second, the paper adds to the evidence on economic shocks, such as extractive industry and construction booms, and their effects on gender relations (Kotsadam et al., 2017; Aragón and Rud, 2013; Aguilar-Gomez and Benshaul-Tolonen, 2023), extending this literature from mining and oil extraction to transportation infrastructure in a Latin American context. Third, we contribute to the more general literature on the gendered consequences of economic shocks related to labour market restructuring (Aizer, 2010; Bhalotra et al., 2021; Guarnieri and Rainer, 2018), showing that a dual sectoral transformation affecting men’s and women’s employment simultaneously may generate violence against women. Finally, it contributes to the broader literature that examines the differential shocks that can affect violence against women, a global public health issue (Card and Dahl, 2011; Anderberg et al., 2016; Tur-Prats, 2021; Aguilar-Gómez and Salazar-Díaz, 2025; Bollman et al., 2026). To our knowledge, this is the first study examining the impact of a large infrastructure construction in Latin America on violence against women using a causal design.

The remainder of the paper proceeds as follows. In Section 2 we describe the Tren Maya project, followed by the conceptual framework and the empirical literature in Section 3. In Section 4 we describe the data, the construction of our outcomes, and the design setting, while in Section 5 we outline the empirical method. In Section 6 we present the main results on violence against women and men, and in Section 7 we report analysis on alternative explanations and robustness checks. In Section 8 we test some hypothesised mechanisms such as the labour market, exposure in public spaces, and substance consumption. Finally, in Section 9 we discuss the findings and their policy implications.

2 The context of the Tren Maya

The Tren Maya is the flagship infrastructure project of former president López Obrador’s administration (2018–2024) (Zamora, 2025). The original estimated cost was approximately 125 billion Mexican pesos (about USD 7 billion), though final estimates reached 507 billion pesos (about USD 28 billion) (Espinosa Ovando et al., 2023; de la Rosa, 2024). The project promised to enhance local development, boost tourism, and improve the well-being of the population through job creation and better transportation in a region historically characterised by high levels of poverty and social marginalization (Zamora, 2025; CONACYT, 2019). The total population in the corridor of the railway is approximately 542,000 people, of which about one-third identify as indigenous. As the territory affected by the Tren Maya spans across five states, there is heterogeneity in

terms of poverty rates and quality of life, with some areas having a Human Development Index of 0.55–0.70, which is below the national average of 0.746 (CONACYT, 2019). At the same time, there are tourist-oriented and wealthier cities such as Tulum.

An initial government assessment reported that the construction created around 111,000 jobs (Espinosa Ovando et al., 2023). Despite these benefits, the project has been controversial since its inception. A 2019 report by the National Council for Science and Technology (CONACYT) highlighted the risk of deforestation, threats to indigenous cultures and natural resources, and potential increases in insecurity (CONACYT, 2019). Civil society organisations and scholars have reported cases of land dispossession and speculation, violations of collective land rights (ejidos), and community conflicts (CONACYT, 2019; Jiménez-Bandala and Ramírez Torres, 2025; Gasparello and Núñez Rodríguez, 2021; Informe, 2025).

The railway spans 1,554 kilometer across 41 municipalities in five states: Chiapas, Tabasco, Campeche, Yucatán, and Quintana Roo. It includes 34 stations connecting archaeological sites, cities, and natural attractions. The Tren Maya was built in seven sections (called 'Tramos') by different companies with construction starting in two different years. Construction of Tramos 1-4 began in 2020 and was carried out primarily by private consortia, including Mexican, Chinese, and Portuguese firms. Construction of Tramos 5-7 was carried out by Mexico's Ministry of National Defense (SEDENA), beginning in 2022. In total, 35 municipalities were affected by the construction since 2020 and 6 since 2022. The below Figure 1 shows the affected municipalities and their corresponding Tramos and companies.

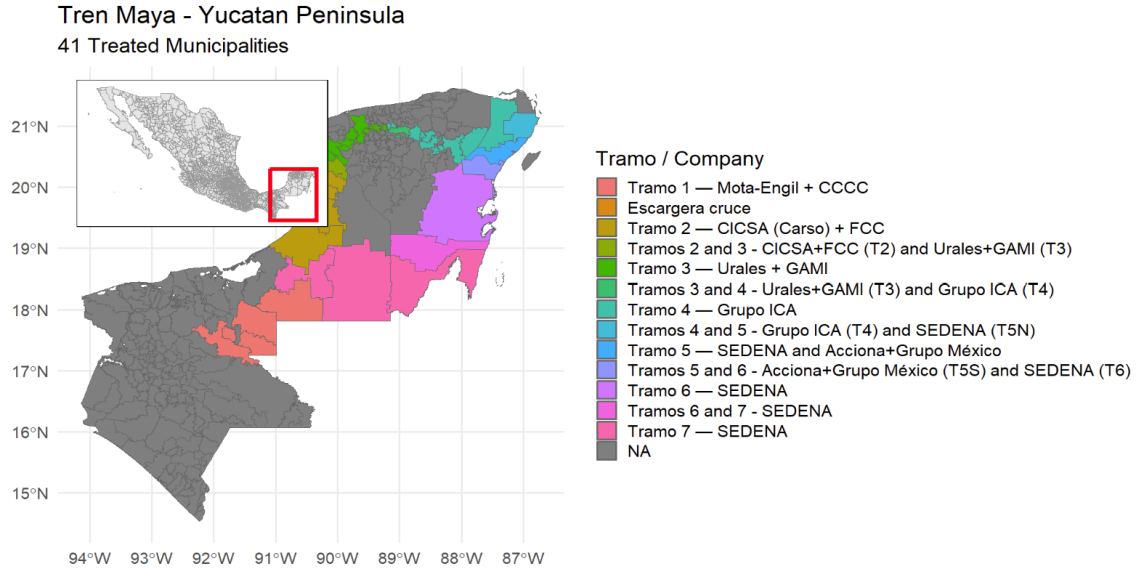


Figure 1: Map of the Tren Maya route by municipalities and companies. Source: Authors

3 Theoretical Framework and Empirical Literature

3.1 Theoretical framework

The construction of the Tren Maya altered local labour markets and the composition of the local population, as workers arrived from outside the region. These changes can raise violence against women through several channels: shifts in bargaining power within the household, male backlash, the disruption of communities and local institutions, and increased alcohol and drug consumption.

First, the construction of the Tren Maya may affect IPV by restructuring the local labour market and thereby shifting bargaining power within the household. Household bargaining models predict that an improvement in a woman’s economic position strengthens her outside options and reduces her partner’s incentive to use violence (Manser and Brown, 1980; McElroy and Horney, 1981; Lundberg and Pollak, 1993; Tauchen et al., 1991; Farmer and Tiefenthaler, 1997). In contrast, male backlash theories argue that a woman’s improved economic conditions, for instance a higher salary or better working conditions, can threaten her partner’s status and provoke a violent response, particularly when her gains coincide with a loss of relative status for him (Macmillan and Gartner, 1999; Bertrand et al., 2015).

Second, demographic changes associated with the arrival of temporary workers during the construction phase may have disrupted local social ties and altered community dynamics, increasing women’s exposure to violence. Indeed, an imbalanced sex ratio within a community may increase criminal behaviour, including violence against women (Cameron et al., 2019; Amaral and Bhalotra, 2017). In addition, routine activity theory suggests that violence can increase when motivated offenders and suitable targets converge in time and space in the absence of capable guardianship (Cohen and Felson, 1979). The arrival of a large and predominantly male temporary workforce, combined with changes in women’s daily mobility and with weaker informal supervision in rapidly growing localities, increases the frequency of such encounters and may therefore raise violence against women outside the household.

Finally, increases in male income generated by construction employment may raise alcohol or drug consumption. Higher earnings can raise disposable income and access to alcohol, which has been identified as an important risk factor for violence against women (Leonard, 2005; Luca et al., 2015). Alcohol consumption may increase violence by reducing self-control and increasing aggressive behaviours, both within households and in broader social interactions. Thus, if greater alcohol or drugs availability is associated with higher male earnings, it may affect both IPV and non-partner (non-IPV) violence against women.

3.2 Empirical Literature

The empirical evidence, overall, suggests that large economic shocks can affect violence against women through multiple channels, but the direction and magnitude of these effects depend on the mechanism at work.

On the household bargaining channel, [Aizer \(2010\)](#) finds that a one standard deviation increase in women’s relative wages in the United States reduces female assault hospitalisations by approximately 9%, concentrated among intimate partners. [Alonso-Borrego and Carrasco \(2017\)](#) finds a 2.5% lower risk of IPV among dual-earner couples in Spain, suggesting that better and more equal economic conditions for women reduce their risk of IPV. By contrast, in support of the male backlash theory, [Guarnieri and Rainer \(2018\)](#), using a regression discontinuity design in Cameroon, find that greater employment opportunities for women raise domestic violence rates by 36%, especially among partners who oppose their employment. In a different context, [Bergvall \(2024\)](#) finds that an increase in Swedish women’s earnings raises assault rates by 12.8%. [Bhalotra et al. \(2021\)](#) show that both directions can operate at the same time: in India, male unemployment and female job opportunities each independently increase domestic violence, through economic stress and backlash mechanisms, respectively.

On the community disruption channel, evidence comes primarily from resource extraction booms, where the arrival of a predominantly male workforce alters local demographic composition and disrupts existing community structures. [Kotsadam et al. \(2017\)](#) find that proximity to gold mines in sub-Saharan Africa raises the probability of sexual violence by 2–3 percentage points. [Martin et al. \(2019\)](#) report a 33% increase in IPV during the Bakken oil boom, and [James and Smith \(2017\)](#) and [Schmutz \(2023\)](#) show comparable increases in violent crime linked to strained local institutional capacity during the U.S. shale boom and the construction of Brazil’s Belo Monte dam, respectively. Evidence from transportation infrastructure is less consistent: [Calamunci and Lonsky \(2025\)](#) find that new U.S. interstate segments increase violent crime by about 5%, while [Biswas and Jain \(2025\)](#) find that improved road connectivity in India reduces crime by 5–10%, driven by improved state monitoring. These findings suggest that the direction depends on whether the specific project contributes to weakening social control, or strengthening institutional capacity.

On the routine-activity channel, [Moreira and Ceccato \(2021\)](#), studying São Paulo’s metro system, find that women face a significantly higher risk of victimisation than men at central, high-traffic stations, consistent with routine mobility patterns shaping gendered exposure to violence in a Latin American context. [Savard et al. \(2020\)](#), using U.S. incident data, similarly finds that women face significantly higher victimisation risk than men specifically in “feminine gendered spaces” such as retail and shopping locations.

On the alcohol and drug consumption channel, the association between consumption and IPV is well established ([Leonard, 2005](#)), and recent work in economics isolates the causal direction using policy variation in alcohol access and pricing ([Basu et al., 2026](#)). In the context of gold mines, [Kotsadam et al. \(2017\)](#) find that the increased cash earned by the workers is used to consume more alcohol, likely contributing to the observed higher domestic violence.

The empirical literature shows that shocks to women’s relative economic position can increase or decrease IPV depending on the context, reflecting the competing predictions of the bargain-

ing and backlash channels. Studies of resource-extraction booms consistently find increases in violence against women operating through community disruption, whereas evidence on transport infrastructure is mixed, depending on whether the project contributes to weakening social control or to strengthening institutional capacity. Studies supporting the routine-activity channel show that women face higher victimisation risk than men in public and work-related spaces, and the evidence on alcohol is consistent across settings, including in comparable resource-extraction contexts. However, at least two important gaps remain. First, most evidence on infrastructure concerns completed projects, while the construction phase, when the inflow of workers is largest and local institutions are least prepared, has received little attention. Second, studies rarely distinguish violence perpetrated by partners from violence perpetrated by others, even though the channels above carry different predictions for each. Much of this evidence also relies on police records, which cannot separate changes in incidence from changes in reporting. We use hospitalisation records alongside crime reports, which capture a different margin of the same phenomenon, although neither source is immune to changes in reporting behaviour.

4 Data

Our analysis combines several administrative data sources at the municipality level, from 2015 to 2024. Our main violence outcomes come from two complementary sources. The first is hospital injury records from the General Directorate of Health Information (DGIS), specifically the standardised SINBA-SEUL-17-P registration form used to record cases of violence and injury (DGIS-Les, 2025; DGIS, 2023). The second is the official crime records from the Executive Secretariat of the National Public Security System (SESNSP) (SESNSP, 2018). The two sources capture different stages of the violence reporting process: DGIS records cases of violence severe enough to reach health facilities, while SESNSP records offences that the victim decides to report to the authorities. We also use administrative data on employment, mortality, tourism capacity and substance-use hospitalisations, described below.

4.1 Hospitalisations data

Our main hospital-based measures are constructed from DGIS injury data, which record diagnoses and external causes of injury using the World Health Organization’s ICD-10 classification, separately for women and men³. We identify assault-related injuries within the X85–Y09 block, comprising 25 three-character categories and 241 four-character codes (WHO, 2019). Table 1 reports the official definitions of the 25 assaults in their three-character parent codes. DGIS records each case using the more granular four-character code: for every code except Y06 and Y07, the

³For brevity, we use the term *hospitalisations* throughout the paper to refer to injury and violence cases attended in health facilities and recorded on the SINBA-SEUL-17-P form, which covers emergency, outpatient and inpatient care rather than hospital admissions only. The alcohol- and drug-related measures in Section 8 instead come from hospital discharge records (SAEH) and therefore capture inpatient admissions only.

fourth digit specifies the location where the assault occurred (e.g., school, street, workplace), while for Y06 and Y07 the fourth digit specifies the perpetrator’s relationship to the victim (spouse or partner, parent, acquaintance, or unspecified). Among all assault cases in our data, Y070 (maltreatment by spouse or partner) is the single largest category at 28%, followed by Y079 (maltreatment, unspecified perpetrator) at 7%, X994 (assault by a sharp object, street or highway) at 6%, and Y044 (assault by bodily force, street or highway) at 5%. The full distribution is reported in Table [A.3](#).

[Aizer \(2010\)](#) pioneered the use of the ICD-10 assault block to study domestic violence but did not distinguish by perpetrator. Following the more targeted approach of [Bergvall \(2024\)](#) and [Aguilar-Gómez and Salazar-Díaz \(2025\)](#), we identify intimate partner violence (IPV) using the two codes that explicitly name a spouse or partner as the perpetrator: Y070, “maltreatment by spouse or partner,” which includes physical, sexual, or psychological maltreatment, and Y060, “neglect and abandonment by husband or partner.” We classify these as *IPV-related assaults* and the remaining codes as *non-IPV assaults*.

Classifying by ICD-10 code alone, however, misses partner violence recorded by mechanism: because each code captures either the mechanism or the perpetrator but not both, a partner assault coded, say, as X99 (sharp object) would be counted as non-IPV. To avoid this, we use a separate field on the DGIS registration form, the perpetrator’s relationship to the victim, which health providers complete independently of the ICD-10 external-cause code, and we exclude from the non-IPV measure every case in which this field identifies a spouse or partner. We do not reassign these cases to the IPV measure, because the relationship field is not always internally consistent with the ICD-10 code. For instance, a share of them are assigned to Y079 (other maltreatment, unspecified perpetrator), a contradiction we cannot resolve with the available data. We nonetheless report, as a robustness check, a broader IPV measure that adds to our IPV measure all cases flagged by this field. Table [A.3](#) reports the classification of each code and the number of cases excluded from the non-IPV measure.

Because our object of interest is whether an assault is classified as IPV rather than the specific mechanism involved, we do not further disaggregate by mechanism. Doing so would fragment an already limited number of cases into categories too small to analyse reliably, without adding information relevant to the household- versus community-level channels we investigate. This aggregation should not be read as implying that these mechanisms constitute the same form of violence; it reflects only our focus on IPV versus non-IPV assault-related hospitalisations.

We additionally construct a measure of assaults occurring in public spaces, which we use in Section [8](#). For all assault codes except Y06 and Y07, the fourth digit of the DGIS record indicates where the assault occurred; we classify as public spaces four of these locations: streets and highways, trade and service areas, sports and athletics areas, and schools, institutions and public administrative areas.

Table 1: Descriptions of the ICD-10 codes used to construct violence outcomes

Code	Description (WHO ICD-10)	Use in this paper
<i>Assault (violence outcomes) — Chapter XX, External causes of morbidity and mortality</i>		
X85	Assault by drugs, medicaments and biological substances	Non-IPV assault outcome ^a
X86	Assault by corrosive substance	Non-IPV assault outcome
X87	Assault by pesticides	Non-IPV assault outcome
X88	Assault by gases and vapours	Non-IPV assault outcome
X89	Assault by other specified chemicals and noxious substances	Non-IPV assault outcome
X90	Assault by unspecified chemical or noxious substance	Non-IPV assault outcome
X91	Assault by hanging, strangulation and suffocation	Non-IPV assault outcome
X92	Assault by drowning and submersion	Non-IPV assault outcome
X93	Assault by handgun discharge	Non-IPV assault outcome
X94	Assault by rifle, shotgun and larger firearm discharge	Non-IPV assault outcome
X95	Assault by other and unspecified firearm discharge	Non-IPV assault outcome
X96	Assault by explosive material	Non-IPV assault outcome
X97	Assault by smoke, fire and flames	Non-IPV assault outcome
X98	Assault by steam, hot vapours and hot objects	Non-IPV assault outcome
X99	Assault by sharp object	Non-IPV assault outcome
Y00	Assault by blunt object	Non-IPV assault outcome
Y01	Assault by pushing from a high place	Non-IPV assault outcome
Y02	Assault by pushing or placing victim before a moving object	Non-IPV assault outcome
Y03	Assault by crashing of a motor vehicle	Non-IPV assault outcome
Y04	Assault by bodily force	Non-IPV assault outcome
Y05	Sexual assault by bodily force	Non-IPV assault outcome
Y06	Neglect and abandonment by perpetrator	
Y060	Neglect and abandonment by spouse or partner	IPV-related assault outcome
Y061	Neglect and abandonment by parent	Non-IPV assault outcome
Y062	Neglect and abandonment by acquaintance or friend	Non-IPV assault outcome
Y068	Neglect and abandonment by other specified person	Non-IPV assault outcome
Y069	Neglect and abandonment by unspecified person	Non-IPV assault outcome
Y07	Other maltreatment, including mental cruelty, physical abuse, sexual abuse, and torture by perpetrator	
Y070	Other maltreatment by spouse or partner	IPV-related assault outcome
Y071	Other maltreatment by parent	Non-IPV assault outcome
Y072	Other maltreatment by acquaintance or friend	Non-IPV assault outcome
Y073	Other maltreatment by official authorities	Non-IPV assault outcome
Y078	Other maltreatment by other specified person	Non-IPV assault outcome
Y079	Other maltreatment by unspecified person	Non-IPV assault outcome
Y08	Assault by other specified means	Non-IPV assault outcome

4.2 Crime data

The SESNSP crime data provide complementary measures of gender-based violence recorded by the criminal justice system. The data are reported by type of crime, and we look specifically at gender-based violence crimes such as intimate partner violence and sexual abuse (SESNSP, 2018). These data are not reported by gender of the victim, but, assuming that victims of IPV and sexual abuse are mostly women, we compute these outcomes as rates per 100,000 women. We treat the crime measures as a proxy for violence reporting rather than incidence, in contrast to the hospitalisation data. Moreover, an increase in IPV-related hospitalisations need not be accompanied by a proportional increase in IPV crime reports, because victims may seek medical attention without formally reporting the incident to the authorities.

All SESNSP records, like the DGIS records, are assigned to the municipality where the aggression occurred rather than to the municipality of the victim’s residence. For the main analysis, we focus on assault-related hospitalisations of women, IPV-related and non-IPV cases, together with two crime-based outcomes: IPV crimes and sexual abuse crimes.

4.3 Other variables

Employment outcomes are constructed from IMSS records at the municipality-sector level. Because the data do not distinguish workers by gender, changes in sectoral employment cannot be interpreted as changes in female or male employment; instead, they characterise the broader labour-market restructuring associated with construction. We construct these variables as rates per 100,000 inhabitants for each sector and sub-sector.

We use excess mortality as a proxy for COVID-19 intensity, constructed following Msemburi et al. (2023). We compute observed minus expected deaths per 100,000 inhabitants, where expected mortality is estimated with a quasi-Poisson model fitted to municipality-level data from the 2015–2019 pre-pandemic period. As a proxy for tourism dependence, we use the number of registered hospitality rooms per municipality and year from INEGI (2017–2023). This variable captures the pre-existing tourism infrastructure and allows us to test whether the treatment effects are driven by tourism-related economic shocks.⁴

Alcohol- and drug-related hospitalisations are constructed using ICD-10 codes for substance-use disorders and related poisonings (WHO, 2019).⁵ These measures capture severe cases requiring medical attention and therefore provide an incomplete measure of overall alcohol and drug con-

⁴We rely on hotel-room capacity because, to our knowledge, no better measure of tourism intensity is available at this level of geography. Mexico’s national tourism monitoring system (DataTur) reports activity for only 70 major destinations (SECTUR, 2026), rather than for the full set of municipalities in our sample, whereas hotel-room capacity is observed for every municipality and year. For the same reason, we do not rely on direct measures of economic activity: no municipality-level series comparable to GDP is available with which to quantify the economic losses caused by the pandemic.

⁵Alcohol-related hospitalisations: codes F10 and T51; drug-related hospitalisations: codes F11–F19 and T40–T43.

sumption. Municipality-level survey measures of substance use are not available for our study period, and Mexico’s main national substance-use survey, ENCODAT, is representative only at the national, regional, and state levels (CONASAMA, 2025).

The final dataset is constructed by aggregating the data to the yearly level, and all outcomes are expressed per 100,000 women, men, or total inhabitants, depending on whether the outcome is gender-specific.⁶

4.4 Estimation sample

We identify 41 treated municipalities crossed by the Tren Maya railway. The control group consists of the municipalities in the five states traversed by the railway that are not themselves crossed by it. To reduce potential contamination from spillovers, we exclude control municipalities located within 15 kilometers of the nearest treated municipality. Construction can generate local economic and demographic spillovers through commuting workers, shared labour markets, and increased demand for goods and services, so that including municipalities immediately adjacent to the railway could attenuate the difference between treated and control areas. At 15 kilometers, the exclusion covers the municipalities most likely to absorb these spillovers. Qualitative evidence on the Tren Maya workforce reports that crews were housed in improvised camps close to the construction fronts, while engineers and specialised staff were accommodated in rented housing in the nearest towns and cities, and residents from nearby cities were employed to build the railway (Torres G3n3gora and Verde Ca3n3etas, 2024). The workforce was therefore concentrated not only in the municipalities crossed by the railway but also in those immediately surrounding them.

After this restriction, the crime-outcome sample contains 41 treated and 87 control municipalities, yielding 1,280 municipality-year observations over 2015–2024. For hospital-based outcomes, we additionally exclude municipalities without a functioning health facility during the period of analysis. This excludes one treated municipality, Tulum, and two control municipalities, leaving 40 treated and 85 control municipalities and 1,250 municipality-year observations. We impose this restriction because, where no hospital is present, victims may not seek hospital care when the nearest facility is far away; retaining such municipalities could then confound the absence of recorded hospitalisations with the absence of local reporting infrastructure. We identify municipalities with functioning health facilities using geolocated Ministry of Health data (DGIS, 2025). To check that the treated and control groups are sufficiently similar, we run balance tests on a set of socio-economic variables, including population by gender, gender-based violence crimes, and tourism dependence (Appendix Table A.4). Except for tourism, we find no statistically significant differences between the two groups.

⁶Annual panel data with causal identification are standard in the infrastructure literature (see Calamunci and Lonsky, 2025; Donaldson, 2018; Gibbons et al., 2019; De Borger et al., 2025).

4.5 Descriptive statistics

Table 2 reports descriptive statistics for the variables in their respective restricted estimation samples. In Panel A the hospitalisation measures show substantial variation across municipality-years. Total assaults against women average 48.99 cases per 100,000 inhabitants, of which IPV-related assaults account for 23.39 and non-IPV assaults for 20.54. The two components do not sum to the total because, as explained above, cases that the perpetrator-relationship field identifies as partner-perpetrated but that carry an ICD-10 code other than Y060 or Y070 are removed from the non-IPV measure without being added to the IPV measure. The corresponding rates for men are considerably different: total male assaults average 36.25 per 100,000, but almost all of these are non-IPV cases, which average 35.27, while IPV-related male assaults average only 0.48. This contrast highlights the distinct nature of IPV-related violence relative to other forms of assault and provides a useful benchmark for the gender-specific analysis below. Assaults occurring in public spaces show the opposite gender pattern to assaults overall. Women average 4.73 cases per 100,000, against 15.75 for men, so that violence against women is concentrated in private settings while violence against men occurs disproportionately outside the home. This distinction is relevant for the routine-activity channel, which predicts effects on violence occurring outside the household.

In Panel B, sexual abuse crimes average 5.42 cases per 100,000 inhabitants, while reported IPV crimes average 59.53. Both measures also exhibit substantial dispersion across municipality-years, with standard deviations of 11.16 and 91.11, respectively. These differences in means and dispersion reflect the considerable heterogeneity in the incidence and reporting of violence across municipalities. Because the hospitalisation and crime measures capture different stages of the violence reporting process, their levels are not directly comparable: hospital records capture cases reaching health facilities, whereas SESNSP records capture incidents formally reported to the authorities.

Panel C reports the variables on local economic conditions and the COVID-19 proxy used to run the robustness checks and test the mechanisms. The excess mortality rate averages 45.64 deaths per 100,000 inhabitants, with values ranging from -480.75 to 886.84 ; negative values correspond to municipalities recording fewer deaths than predicted by their pre-pandemic baseline. Hotel capacity is highly concentrated: the mean is 534.03 rooms per 100,000 inhabitants, but the standard deviation is 2,430.91 and the maximum 24,805.79, reflecting the coexistence of major tourist destinations and municipalities with no registered hotel capacity. Employment is similarly dispersed across the three sectors most exposed to the railway, with means of 144.04 in agriculture, 40.01 in transport, and 63.50 in food services per 100,000 inhabitants.

Table 2: Summary statistics, restricted estimation samples

Variable	N	Mean	SD	Min	Max
<i>Panel A: hospitalisation outcomes (DGIS)</i>					
Total Female Assault	1,250	48.99	97.63	0.00	876.05
IPV Female Assault	1,250	23.39	58.10	0.00	565.50
Non-IPV Female Assault	1,250	20.54	38.98	0.00	349.65
Total Male Assault	1,250	36.25	61.91	0.00	406.45
IPV Male Assault	1,250	0.48	2.29	0.00	49.00
Non-IPV Male Assault	1,250	35.27	60.23	0.00	390.27
Female Assault in Public Spaces	1,250	4.73	12.31	0.00	206.19
Male Assault in Public Spaces	1,250	15.75	32.74	0.00	262.58
<i>Panel B: Crime outcomes (SESNSP)</i>					
Sexual Abuse Crimes	1,280	5.42	11.16	0.00	105.76
IPV Crimes	1,280	59.53	91.11	0.00	520.88
<i>Panel C: Other variables</i>					
Excess Mortality Rate	1,280	45.64	120.39	−480.75	886.84
Hotel Rooms per Capita	896	757.79	2,876.32	0.00	24,805.79
Agriculture Employment	1,280	144.04	581.01	0.00	6,760.62
Transport Employment	1,280	40.01	201.60	0.00	2,654.50
Food Services Employment	1,280	63.50	125.86	0.00	1,801.30

Notes: The unit of observation is the municipality-year. Panels A and B report rates per 100,000 female or male inhabitants, depending on the outcome. Panel C reports the excess mortality rate, hotel rooms and employment per 100,000 inhabitants. Sample sizes differ across panels because the hospitalisation and crime outcomes are estimated on different restricted samples, as described in Section 4.

Figure 2 shows raw trends among treated and controls for the four outcomes of interest: female hospitalisations for IPV and non-IPV, crimes reported for IPV and sexual abuse. The vertical line marks 2020, when construction began for 85% of treated municipalities (Tramos 1–4). Treated municipalities exhibit substantially higher mean rates across all four outcomes compared to control municipalities: female IPV-related assault averages 48.79 per 100,000 in treated municipalities against 11.43 in control municipalities, and non-IPV female assault averages 36.07 against 13.24. The same pattern holds for the crime-based outcomes, sexual abuse averages 11.21 in treated municipalities against 2.69 in control municipalities, while crime-reported IPV averages 78.45 against 50.61. These raw differences across pre- and post-treatment years motivate the staggered difference-in-differences analysis used to identify a causal effect.

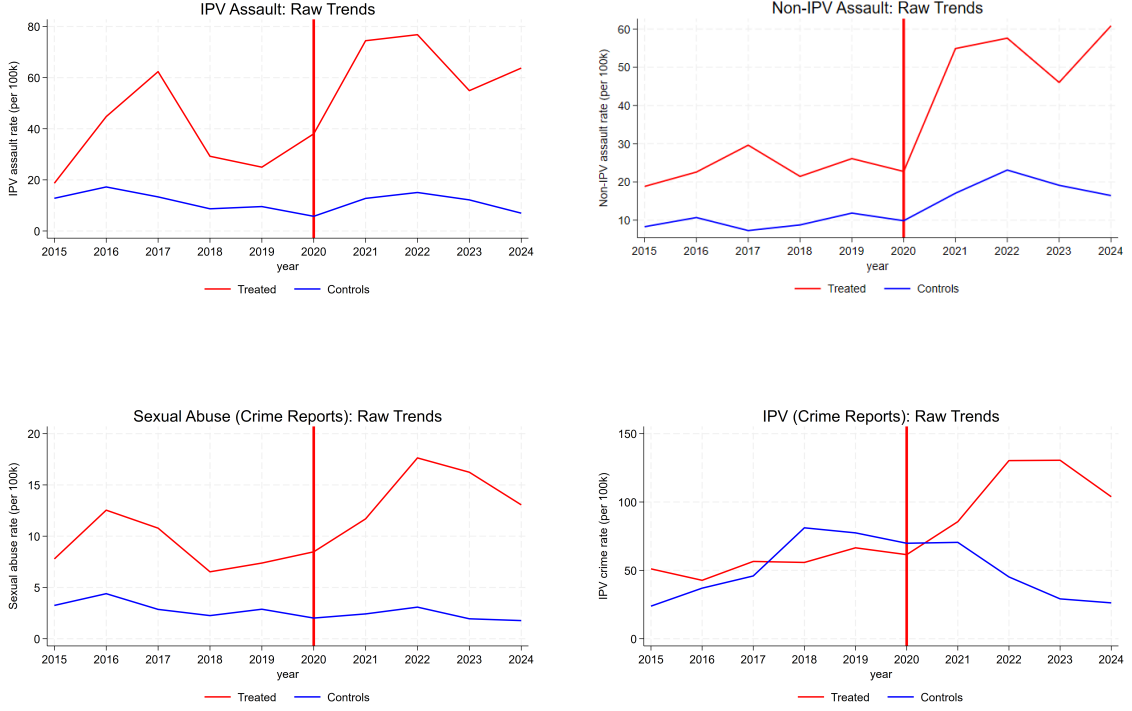


Figure 2: Raw trends in violence against women outcomes, treated vs. control municipalities, 2015–2024.

5 Empirical strategy

To estimate the effect of construction on violence against women, we exploit the staggered start of construction across municipalities in an event-study difference-in-differences design. The design is summarised by the two-way fixed-effects event-study equation

$$Y_{it} = \alpha_i + \lambda_t + \sum_{h \neq -1} \theta_h D_{it}^h + \varepsilon_{it}, \quad (1)$$

where Y_{it} is an outcome in municipality i and year t , α_i and λ_t are municipality and year fixed effects, and $D_{it}^h = \mathbf{1}\{t - E_i = h\}$ indicates that municipality i is h years from the start of construction E_i , with $E_i = \infty$ for never-treated municipalities. The parameters of interest are the horizon-specific average effects on treated municipalities, θ_h for $h \geq 0$; the pre-treatment coefficients ($h < 0$) provide a check on the identifying assumptions. Construction began in 2020 for 35 municipalities (Tramos 1-4) and in 2022 for 6 (Tramos 5-7), and ended between 2023-2024. We therefore report effects for horizons 0 to 2, the longest window observed for both cohorts and that one falls within the construction period for each of them, and summarise them by the average effect $\bar{\theta} = (\theta_0 + \theta_1 + \theta_2)/3$. Because every treated municipality is observed once at each of these horizons, $\bar{\theta}$ also equals the average of all unit-level effects in the window.

Estimated by OLS, equation (1) requires an omitted reference period, conventionally the year before construction ($h = -1$), and under staggered timing it recovers θ_h only if treatment effects are homogeneous across cohorts. When effects differ across cohorts, the OLS event-study coefficients can be contaminated by effects from other horizons (Sun and Abraham, 2021), and the static two-way fixed-effects estimator implicitly compares later-treated municipalities with already-treated ones and can assign negative weights to some unit-level effects (de Chaisemartin and D’Haultfoeulle, 2020; Goodman-Bacon, 2021). We therefore estimate θ_h with the imputation estimator of Borusyak et al. (2024), which remains valid under arbitrary heterogeneity because it never uses treated observations as controls. Rather than normalising to a single reference period, it fits the counterfactual on all untreated observations; Borusyak et al. (2024) show that this makes it the most efficient linear unbiased estimator when errors are homoskedastic and serially uncorrelated. The estimator proceeds in three steps. First, it estimates the two-way fixed-effects model for the untreated potential outcome,

$$Y_{it}(0) = \alpha_i + \lambda_t + \varepsilon_{it}, \quad (i, t) \in \Omega_0, \quad (2)$$

on the set Ω_0 of untreated observations, namely all observations of never-treated municipalities and the pre-construction observations of treated municipalities. Second, it uses the estimated fixed effects to impute the untreated potential outcome $\hat{Y}_{it}(0) = \hat{\alpha}_i + \hat{\lambda}_t$ for each treated observation and computes the unit-level effect $\tau_{it} = Y_{it} - \hat{Y}_{it}(0)$. Third, it averages these effects over treated observations at each horizon to obtain θ_h ($h = 0, 1, 2$) and $\bar{\theta}$. Standard errors are clustered at the municipality level.

Identification rests on two assumptions (Borusyak et al., 2024). The first is parallel trends: absent construction, outcomes in treated and control municipalities would have evolved in parallel, as in equation (2). The second is no anticipation: before construction began, outcomes in treated municipalities were not affected by the project, so that their pre-construction observations can serve as untreated. This second assumption deserves attention because the project was announced in 2018, before construction started. We assess both assumptions with the pre-trend test of Borusyak et al. (2024), which is conducted separately from the estimation of treatment effects: using untreated observations only, we regress the outcome on the municipality and year fixed effects and on indicators for the two years before construction, and test whether the two coefficients are jointly zero. Because the test uses only untreated observations, it does not affect the treatment-effect estimates; earlier pre-construction years and all never-treated observations serve as the benchmark. We limit the test to two leads because each additional lead removes untreated pre-construction years from this benchmark, which is short for the 2020 cohort (five pre-construction years in our panel). As a further check that our results do not depend on the choice of estimator, in Section 7 we re-estimate the main specification with the estimator of Callaway and Sant’Anna (2021).

The descriptive statistics in Table 2 are computed on the full restricted panel: 1,280 municipality-year observations for the crime outcomes (128 municipalities over 2015–2024) and 1,250 for the hospital outcomes (125 municipalities). The estimates use the same panel with one exclusion. The 2023 and 2024 observations of the 2020 cohort lie at horizons 3 and 4, beyond the common window, so they are excluded in the estimation. Because these observations are treated, they would not enter equation (2) in any case, and excluding them changes none of the reported estimates; it only ensures that the reported sample size reflects the observations actually used. Since 35 treated municipalities began construction in 2020, this removes $35 \times 2 = 70$ observations from each sample, leaving 1,210 municipality-year observations for the crime outcomes and 1,180 for the hospital outcomes.

6 Results

6.1 Violence against women

Table 3 presents the estimates from the Borusyak et al. (2024) staggered difference-in-differences imputation estimator on our main violence against women outcomes, and in Figure 3 we report the corresponding event studies. Overall, we find a large and statistically significant effect.

Total female assault-related hospitalisations increased by 57.8% relative to the pre-treatment mean, and the estimates satisfy the parallel-trends assumption. Decomposing this aggregate by perpetrator, the average treatment effect on the treated (ATT) on female IPV-related assault hospitalisations corresponds to an average increase of 67.5% relative to the pre-treatment mean. For non-IPV female assaults, we find an increase of 50.8%. Sexual abuse crimes show a significant increase of 40.2%. The IPV outcome event study shows how the treatment effect increases over time, consistent with a cumulative mechanism linked to the progression of construction activity. The effect for non-IPV assaults emerges only in the second year after construction began, with sexual abuse crimes following a similar delayed pattern. These four outcomes, total female assault, IPV assault, non-IPV assault, and sexual abuse, are the ones for which we find both a significant treatment effect and no violation of parallel trends.

We also report IPV crime reports from SESNSP as a complementary outcome. Although the estimated effect is large and statistically significant, the result should be interpreted with caution because the lead two years before construction is significant. Nonetheless, its direction and magnitude (56.6%) are broadly consistent with the more robust IPV hospitalisation result. Importantly, the two measures capture different dimensions of violence. Hospital-based IPV assaults capture cases severe enough to require medical attention, whereas SESNSP records capture incidents that are formally reported to the authorities. These measures do not necessarily move together. The positive association across the two measures is therefore suggestive of a broader increase in IPV,

but the causal effect of IPV crimes should be interpreted with caution.

Table 3: Main results: Effects of Tren Maya construction on gender-based violence

Outcome	ATT	SE	Pre-treat Mean	% Change	N
Total female assault	43.06***	(14.82)	74.54	+57.8%	1,180
IPV-related assault	27.42***	(9.33)	40.64	+67.5%	1,180
Non-IPV female assault	13.32**	(5.68)	26.23	+50.8%	1,180
Sexual abuse (crimes)	4.2**	(1.83)	10.44	+40.2%	1,210
IPV (crimes)†	36.67***	(11.93)	64.68	+56.6%	1,210

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Borusyak et al. (2024) staggered difference-in-differences imputation estimator. Municipality and year fixed effects; standard errors clustered at the municipality level. The IPV and non-IPV components do not sum to it as explained in Section 4 accounting for the remaining 7.67 cases per 100,000 of the pre-treatment mean. Pre-treatment mean is computed among treated municipalities, restricted to years before each municipality’s own treatment year. Percentage change calculated relative to this pre-treatment mean. N is lower than the full estimation sample because observations for the 2020 cohort beyond the common 0–2 post-treatment horizon are not used in constructing the reported coefficients, as explained in Section 5. † Fails the parallel-trends test, interpret with caution.

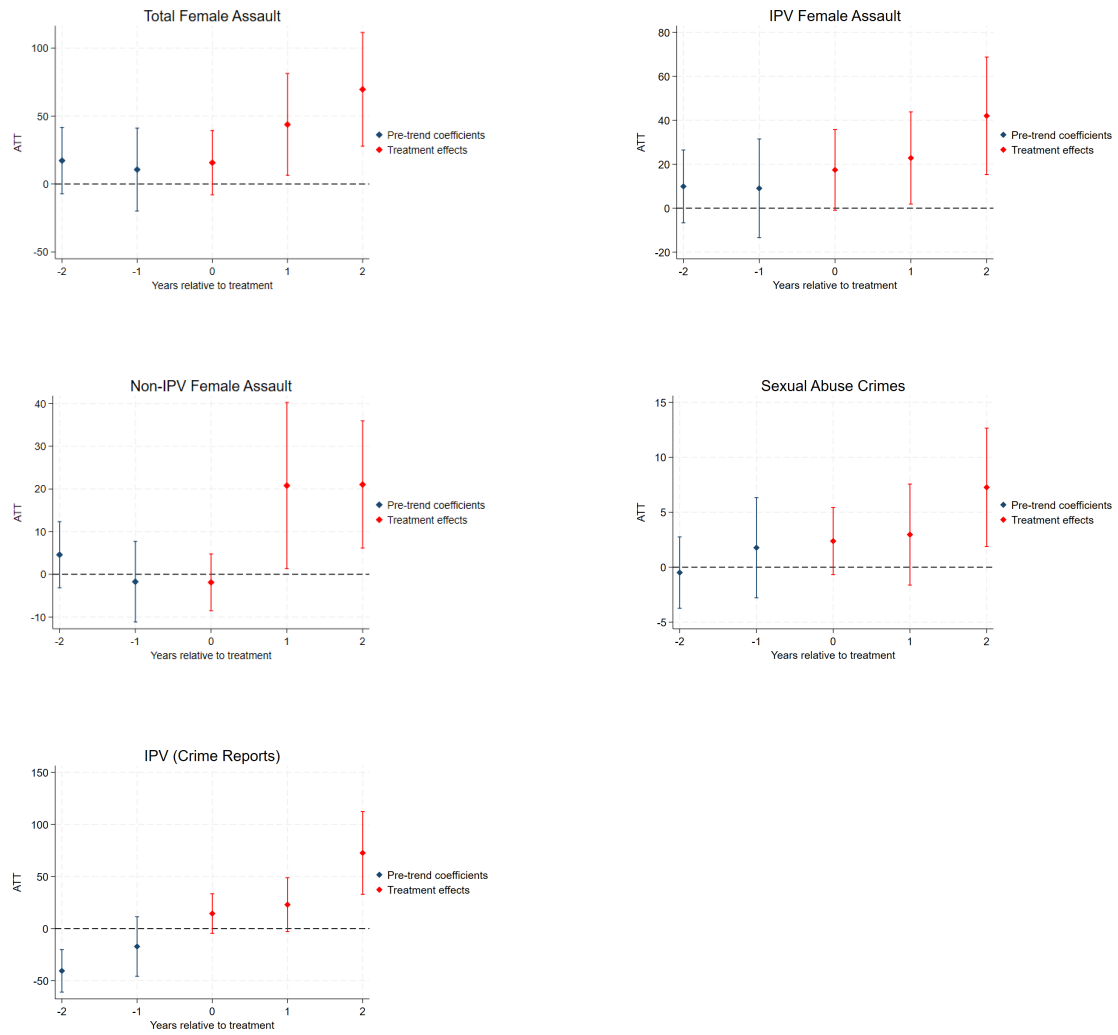


Figure 3: Event studies on the main violence against women outcomes

6.2 Did the spread of violence also affect men?

To assess whether the observed increases are gender-specific, we estimate the same event study specifications using the corresponding male violence outcomes, reported in Figure 4. For total and non-IPV assaults against men, the estimates display a mild upward trend that is not statistically significant, and the joint test rejects the null that the pre-treatment coefficients are zero, so these results are not informative in either direction. For male IPV-related assault, the estimates are flat and statistically insignificant, but the joint test satisfies the parallel trends assumption. The male results therefore provide no evidence of an increase in IPV against men, while the remaining male outcomes cannot be used to assess gender specificity.

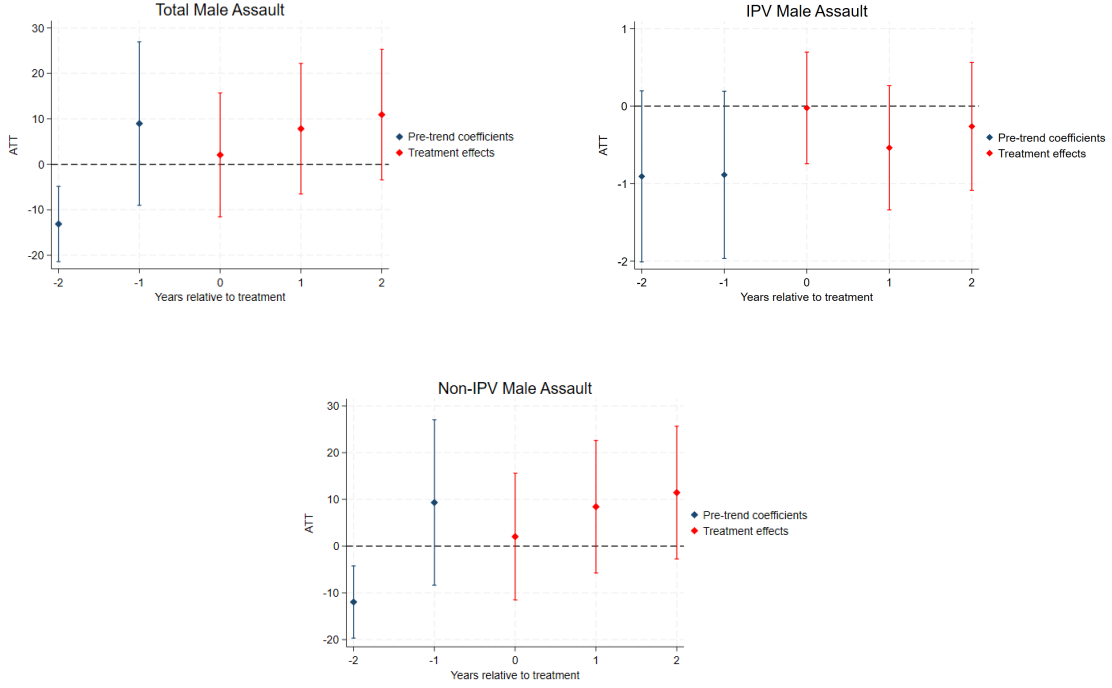


Figure 4: Effects of Tren Maya construction on male violence outcomes. Borusyak et al. (2024) imputation estimator. Municipality and year fixed effects; standard errors clustered at the municipality level.

7 Robustness checks

7.1 COVID-19 as a Potential Confounder

First, to address the concern that the pandemic confounds our estimates, we estimate a triple-interaction specification. We restrict the sample to municipalities treated in 2020 and never-treated municipalities over the period 2018–2022, so that treatment timing is common across treated units, and estimate for each main outcome Y_{it} :

$$Y_{it} = \beta_1(\text{TM}_i \times \text{Post}_t) + \beta_2(\text{ExcessMort}_i \times \text{Post}_t) + \beta_3(\text{TM}_i \times \text{ExcessMort}_i \times \text{Post}_t) + \mu_i + \lambda_t + \varepsilon_{it}$$

where TM_i indicates Tren Maya municipalities and $\text{Post}_t = \mathbf{1}[\text{year} \geq 2020]$. The level terms TM_i and ExcessMort_i are time-invariant and are absorbed by the municipality fixed effects μ_i , and Post_t by the year fixed effects λ_t . ExcessMort_i is the excess mortality rate per 100,000 inhabitants in 2020, winsorised at the 99th percentile and standardised over the estimation sample. Standard errors are clustered at the municipality level.

Because ExcessMort_i is standardised, β_1 is the treatment effect at mean pandemic severity; β_2 measures the association between local pandemic severity and the outcome after 2020 across all

municipalities; and β_3 tests whether the treatment effect varies with that severity. The evidence against COVID confounding is twofold. First, controlling for the severity gradient β_2 , the treatment effect β_1 remains statistically significant and close to our estimates, so the effect is not an artefact of treated municipalities having experienced a more intense epidemic. Second, an insignificant β_3 indicates that the effect is not concentrated in the municipalities hit hardest by the pandemic.

Table 4 confirms both. The triple interaction (Panel C) is small relative to the treatment effect and statistically insignificant across all outcomes, while the estimated treatment effect (Panel A) remains lower but statistically significant. These results suggest that the effects are not driven by differential pandemic intensity. Additional robustness exercises, such as controlling directly for excess mortality, estimating heterogeneous effects by COVID severity, and treating excess mortality as the treatment variable, lead to the same conclusion (see Appendix A.5).

Table 4: Triple Interaction: Tren Maya $\times$ COVID Severity (Excess Mortality)

	IPV Assault	Non-IPV Assault	Sexual Abuse Crimes
<i>Panel A: Tren Maya Treatment</i>			
TM $\times$ Post	23.55** (9.21)	13.67** (6.64)	3.53** (1.71)
<i>Panel B: COVID Intensity</i>			
Excess Mort. $\times$ Post	1.64 (2.05)	3.31* (1.91)	0.02 (0.59)
<i>Panel C: Differential COVID Effect</i>			
TM $\times$ Excess Mort. $\times$ Post	-4.34 (6.93)	-3.82 (5.78)	0.01 (1.49)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	600	600	610

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Sample restricted to 2020-treated and never-treated municipalities, years 2018–2022. COVID severity measured as excess mortality rate per 100,000 in 2020 (quasi-Poisson regression on 2015–2019 baseline), winsorised at the 99th percentile and standardised. Standard errors clustered at the municipality level in parentheses.

7.2 Tourism dependency

A related concern is that treated municipalities experienced larger pandemic-induced economic shocks because of greater pre-existing tourism dependence, a concern reinforced by our balance tests, which show significantly higher pre-treatment hotel rooms per capita in treated municipalities (Table A.4). If so, the observed increase in violence, particularly IPV, could reflect the collapse

of the tourism sector with subsequent economic shocks and tensions in the household, leading to increasing violence. To address it, we re-estimate our baseline specification allowing year effects to vary with pre-pandemic tourism intensity, measured as the log of average hotel rooms per capita in 2017–2019 and interacted with year indicators. This absorbs tourism-related shocks, including the collapse in travel during the pandemic, while avoiding controlling for hotel capacity after construction began, which the railway itself may have affected. Table 5 reports the results (event studies in Figure A.6). The effects on IPV-related assaults and sexual abuse crimes remain positive and statistically significant 62% and 34% of the pre-treatment mean respectively, somewhat smaller than in the main specification. The effect on non-IPV assaults remains positive but is smaller and no longer statistically significant ($p = 0.14$), indicating that this outcome is more sensitive to tourism-specific shocks.⁷

Table 5: Main outcomes analysis controlling for tourism

Outcome	ATT	SE	% Change	Pretrend p-value	N
IPV-related assault	25.27***	(9.58)	+62.2%	0.339	1,180
Non-IPV female assault	9.13	(6.14)	+34.8%	0.202	1,180
Sexual abuse (crimes)	3.55**	(1.76)	+34.0%	0.419	1,210

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Borusyak et al. (2024) staggered difference-in-differences imputation estimator, controlling for the log of average 2017–2019 hotel rooms per capita interacted with year indicators. SEs in parentheses, clustered at the municipality level. ATT is the average of the three post-treatment period coefficients. Percentage change is relative to the pre-treatment mean of treated municipalities. Pretrend p is the p -value of the joint test on the two pre-treatment coefficients. All specifications include municipality and year fixed effects.

As an additional test, we classify municipalities as high- or low-tourism according to whether their pre-2020 average hotel capacity per capita lies above the sample median, and re-estimate the main specification within each group (Table 6).

For IPV-related assaults, our primary outcome, the effect is essentially the same in both groups: 28.4 in high-tourism and 24.0 in low-tourism municipalities, both statistically significant, both satisfying parallel trends, with an insignificant difference among them. The effect therefore does not depend on tourism exposure, which is inconsistent with the tourism-collapse channel. Non-IPV assaults show no significant difference between groups. For sexual abuse, the effect is concentrated in high-tourism municipalities, but the low-tourism estimate fails the parallel-trends test and is therefore not identified, so we draw no conclusion about tourism for this outcome. We note that the low-tourism group contains only 11 treated municipalities, so the within-group estimates, and especially the between-group comparisons, are imprecise. Taken together, and most clearly for our

⁷We use the log of hotel capacity because its distribution is highly skewed. With a linear interaction, the estimates for total and non-IPV assaults become imprecise, because the control is then driven by a few municipalities whose hotel capacity lies far above that of any control municipality.

primary IPV outcome, the evidence does not support the hypothesis that differential exposure to the tourism collapse explains our findings.

Table 6: Treatment effects by tourism intensity

Outcome	ATT	SE	<i>p</i> -value	Treated munic
<i>Panel A. High-tourism municipalities</i>				
IPV-related assault	28.39**	(12.47)	0.295	29
Non-IPV female assault	8.18	(6.48)	0.159	29
Sexual abuse	6.35***	(2.32)	0.163	30
<i>Panel B. Low-tourism municipalities</i>				
IPV-related assault	24.00**	(9.76)	0.755	11
Non-IPV female assault	21.03*	(12.22)	0.575	11
Sexual abuse	−2.55 [†]	(1.99)	0.006	11
<i>Panel C. Difference (Low − High)</i>				
IPV-related assault	−4.39	(15.83)	0.782	
Non-IPV female assault	12.85	(13.83)	0.353	
Sexual abuse	−8.90 [†]	(3.05)	0.004	

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Borusyak et al. (2024) staggered difference-in-differences estimation, applied separately within each tourism group. Municipalities are classified as high- or low-tourism according to whether their pre-2020 average hotel capacity per capita (rooms per 100,000 inhabitants) lies above the sample median. ATT is the average of the three post-treatment period coefficients; SEs clustered at the municipality level in parentheses. In Panels A and B the *p*-value column reports the joint pre-trends test on the two pre-treatment coefficients; in Panel C it reports the between-group difference test (independent-sample *z*-test, Low − High). All specifications include municipality and year fixed effects. [†] The low-tourism sexual-abuse cell fails the pre-trends test ($p = 0.006$), so that estimate and the corresponding difference are not identified and should not be interpreted.

7.3 Other robustness checks

Alternative estimator

The imputation estimator of Borusyak et al. (2024) recovers the counterfactual by fitting a two-way fixed-effects model on all untreated observations. To check that our results do not depend on this approach, we re-estimate the main specification with the estimator of Callaway and Sant’Anna (2021), which rests on a different set of assumptions: it uses never-treated municipalities as the comparison group and the single period immediately before treatment as the baseline, rather than the full pre-treatment history. It is therefore less sensitive to the earliest years of the panel, when injury-based rates in some small municipalities are volatile.⁸ Table A.6 shows that the estimates

⁸The spikes in 2016–2017 (Figure 2) coincide with the transition to the new DGIS information systems (SIS,

are consistent with our main findings in sign, magnitude, and significance, indicating that our conclusions are not an artefact of the imputation estimator’s specification.

Alternative IPV definition

Our primary IPV measure relies only on the two ICD-10 codes that name a spouse or partner as perpetrator (Y060, Y070), which excludes partner-perpetrated assaults recorded by mechanism. As a robustness check, we construct a broader measure that additionally classifies as IPV every assault-block case in which the independent perpetrator-relationship field identifies a spouse or partner, regardless of the ICD-10 code. This adds 5,871 cases to the IPV measure (a 21% larger case base). The relationship field is recorded consistently across 2015–2024, with no discontinuity at treatment, so the broader measure does not mechanically track the construction timing. The estimated effect is very close to the baseline: an increase of 61.6% above the pre-treatment mean, against 67.5% in the main specification, and the pre-trends test continues to pass (Table A.9). Our results are therefore not driven by the conservative, code-based definition of intimate partner violence.

Sensitivity to influential municipalities

Outcome rates in municipalities with few women can fluctuate considerably from year to year, raising the concern that our estimates are driven by a small number of such municipalities. We do not weight by female population, since five municipalities account for 69% of the treated female population and would dominate a weighted estimate. Instead, we re-estimate our main specification excluding municipalities below the 25th percentile of the 2019 female population across all municipalities (fewer than 5,843 women), which removes 13 of the 40 treated municipalities. The estimates are somewhat smaller than in the main specification but remain large and significant (Table A.10). We also exclude each treated municipality in turn; the estimates for IPV and non-IPV assaults remain of similar magnitude and statistically significant in every sample (Appendix Figures A.7a and A.7b).

8 The household and community level mechanisms

8.1 The restructuring of the labour market

Table 7 reports effects on formal employment in the two sectors that map onto the mechanisms discussed in Section 3: transport, directly linked to construction activity, and food services, expected to expand with the arrival of the workforce (event studies in Appendix Figure A.8). Food services employment increases by 68.8% relative to the pre-treatment mean and transport employment by 135.5%. In both sectors the effect grows over the construction period, from the year the works begin to the second year after, consistent with a cumulative response to the progression of SAEH, Urgencias, Lesiones) and are most pronounced in municipalities with low baseline counts.

construction. The food-services estimate should be read with some caution: its joint pre-trends test is borderline ($p = 0.06$), which may reflect anticipatory expansion of local businesses ahead of the announced arrival of the workforce. Among the remaining 96 IMSS sub-sectors we find either no statistically significant effect or a clear rejection of the pre-trends test.

Table 7 also reports registered agricultural employment, which declines by 82.5% relative to the pre-treatment mean. We report this result for completeness but do not interpret it as a reallocation of workers towards the expanding sectors as the IMSS data do not allow us to do so. These data record the number of formal jobs by sector and municipality, not individual employment histories, so we cannot establish whether workers who left registered agricultural jobs moved into transport, construction, informal work, or other municipalities. The decline may equally reflect changes in land use along the railway corridor, including land acquired for the right of way, or a shift of agricultural work into the informal sector, where informality is already widespread. We therefore read it as evidence of a broader disruption of registered rural activity rather than as part of the mechanisms we examine. More generally, because IMSS records cover formal employment only, all these estimates capture registered jobs and understate changes in sectors where informality is common.

These shifts are informative when read through a gender lens. Transport is heavily male-dominated in Mexico, with women accounting for 21% of sectoral employment (INEGI, 2024), whereas food services employ a disproportionate share of women: 60% of jobs in the sector were held by women in 2022, against 40% economy-wide (INEGI, 2023). The simultaneous expansion of male-dominated transport and female-dominated food services is consistent with two complementary dynamics: direct demand for male labour generated by construction activity, and the indirect expansion of services catering to the incoming workforce.

These changes may affect violence against women through two channels. First, within the household, women who find formal jobs in food services may gain independent income and improve their economic position relative to their partners. Household bargaining models predict that such gains reduce IPV, whereas backlash models predict that they can increase it when they challenge traditional gender roles; our IPV results are consistent with the latter. Second, outside the household, the expansion of transport and construction brings more male workers into public spaces, while the growth of food services brings more women into work outside the home, often in jobs involving contact with the public. Both changes may raise women’s exposure to violence by non-partners, which we examine in the next section. We stress, however, that we observe formal employment at the sector level, without information on workers’ gender, earnings or households, so we cannot establish that the women who experience violence are those whose employment changed.

Table 7: Employment mechanisms: effects of Tren Maya construction on formal employment

Sub-sector	ATT (SE)	% Change	Pretrend p-value	N
<i>Panel A. Sectors linked to the mechanisms</i>				
Food services	80.77*** (30.93)	+68.8%	0.06	1,210
Transport	52.33** (25.93)	+135.5%	0.36	1,210
<i>Panel B. Agriculture</i>				
Agriculture	-83.10** (32.87)	-82.5%	0.30	1,210

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Borusyak et al. (2024) staggered difference-in-differences imputation estimator on IMSS formal employment data. ATT expressed as the number of formal jobs per 100,000 inhabitants; standard errors in parentheses, clustered at the municipality level. Percentage change is relative to the pre-treatment mean. All specifications include municipality and year fixed effects. The food-services pre-trend is borderline (joint $p = 0.06$), consistent with some anticipatory expansion ahead of the workforce arrival. The agricultural estimate is reported for completeness; the data do not allow us to trace workers across sectors (see text).

8.2 Aggressions in public spaces

The second channel predicts that construction increased women’s exposure to violence by non-partners outside the household. To test it, we use the measure of assaults occurring in public spaces described in Section 4: hospital-recorded assaults whose location is a street or highway, a trade or service area, a sports area, or a school or public administrative area.

Table 8 reports the results. Among women, assaults in public spaces increased by 78.5% relative to the pre-treatment mean, and the parallel trends assumption is satisfied. This large percentage reflects a low baseline: in levels, the effect is about five additional cases per 100,000 women. The event study (Appendix Figure A.11) shows the same timing as our main results, with no effect in the first year of construction and a marked increase in the following two years. The increase is not driven by partner violence occurring in public: excluding cases in which the perpetrator field identifies a spouse or partner leaves the estimate essentially unchanged (Panel B).

For men, the estimate is of similar magnitude but fails the pre-trend test, so we do not interpret it causally. As a result, we cannot determine whether the increase for women reflects risks specific to women or a broader rise in violence in public spaces. Either interpretation, however, is consistent with the second channel: after construction began, women faced more violence outside the household from people other than their partners.

Table 8: Effects of Tren Maya construction on assaults recorded in public locations

Outcome	ATT	SE	Pre-treatment Mean	% Change	Pretrend p
<i>Panel A: All assault codes in public spaces</i>					
Assaults against women	4.98**	(2.12)	6.34	+78.5%	0.888
Assaults against men†	5.38*	(3.16)	16.68	+32.3%	0.006
<i>Panel B: Excluding cases recorded with a spouse or partner perpetrator in the perpetrator column</i>					
Assaults against women	4.57**	(2.06)	5.76	+79.3%	0.873
Assaults against men†	5.22*	(3.17)	16.61	+31.5%	0.005

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Borusyak et al. (2024) imputation estimator, 1,180 municipality-year observations in all specifications. Rates per 100,000 inhabitants. Public locations are defined by the fourth digit of the ICD-10 external cause code and comprise schools, institutions and public administrative areas, sports and athletics areas, streets and highways, and trade and service areas. Pre-treatment mean is computed among treated municipalities, restricted to years before each municipality’s own treatment year. Municipality and year fixed effects; standard errors clustered at the municipality level. † Fails the parallel-trends test, interpret with caution.

8.3 Alcohol and drug-related hospitalisations

An additional concern is that construction activity may have increased alcohol consumption or drug use, which are well-documented risk factors for intimate partner violence and social disruption (Leonard, 2005). To explore this channel, we estimate the same event study specifications using alcohol-related and drug-related hospitalisations as outcomes for men and women. The results for male alcohol- and drug-related hospitalisations are reported in Figure A.9. The alcohol-related cases show a marginally significant decrease, which may reflect the reduced alcohol availability during the early construction period due to COVID-19 restrictions, including alcohol sales bans implemented across the southeastern Mexican States in 2020 (Gobierno del Estado de Yucatán, 2020; Various authors, 2021). Male drug-related hospitalisations show small positive but statistically insignificant effects. We found no significant effects for women (Figure A.10). These results do not support alcohol or substance use as a primary mechanism driving the observed increases in violence against women, though we acknowledge that hospitalisation data capture only the most severe cases and may not reflect changes in overall consumption patterns.

9 Discussion

In this paper we examine how the construction of the Tren Maya affected violence against women in southeastern Mexico. Exploiting the staggered timing of construction across municipalities, we find that construction substantially increased violence against women. Assault-related hospitalisations of women rose by 57.8%, driven by a 67.5% increase in IPV-related cases and a 50.8% increase

in non-IPV assaults, alongside a 40.2% increase in reported sexual abuse crimes, with effects strengthening over the construction period. For men, we find no increase in IPV-related assaults, while the results for other male outcomes are not informative. Women therefore appear to have borne a disproportionate share of the social costs of the project during its construction, both inside and outside the household.

Our results suggest that the construction shock reached violence through two routes. Within the household, construction coincided with an expansion of formal employment in female-dominated food services and male-dominated transport. Growing employment opportunities for women are consistent with the backlash channel, although our sector-level data do not allow us to link these jobs to the women who experienced violence. Outside the household, assaults recorded in public locations rose by 78.5% among women, consistent with greater exposure in public and work-related settings as the local economy and population changed.

Our IPV results are consistent with studies supporting the male backlash hypothesis rather than household bargaining. [Aizer \(2010\)](#) and [Alonso-Borrego and Carrasco \(2017\)](#) find that improvements in women’s economic position reduce IPV in the United States and Spain. By contrast, [Guarnieri and Rainer \(2018\)](#) find a 36% increase in domestic violence when women’s employment opportunities expand in Cameroon, and [Bergvall \(2024\)](#) and [Bhalotra et al. \(2021\)](#) document similar backlash effects in Sweden and India. Our estimates point in the same direction as this second group, although our increase in IPV-related hospitalisations is larger than the effects they report, which are measured on different outcomes. A plausible reason for this pattern is the nature of the shock: unlike the gradual changes in relative wages studied by [Aizer \(2010\)](#), the Tren Maya produced an abrupt expansion of female-dominated services, the configuration under which backlash theory predicts higher violence ([Macmillan and Gartner, 1999](#)).

More broadly, our results resemble those documented for resource extraction booms, which bring a large inflow of predominantly male workers and have been associated with higher violence against women. [Martin et al. \(2019\)](#) find a 33% increase in IPV during the Bakken oil boom, while [Kotsadam et al. \(2017\)](#), [James and Smith \(2017\)](#) and [Schmutz \(2023\)](#) report higher sexual violence and violent crime around mines, shale extraction, and dam construction. One difference concerns alcohol: whereas [Kotsadam et al. \(2017\)](#) find that higher earnings increased alcohol consumption around gold mines, we find no increase in alcohol- or drug-related hospitalisations, although these capture only severe cases and are an imperfect proxy for consumption. Our findings bring also a contribution to the mixed evidence on transport infrastructure, although from a different perspective, the construction phase rather than the effect of the infrastructure itself. [Calamunci and Lonsky \(2025\)](#) find that new interstate highways increase violent crime in the United States, while [Biswas and Jain \(2025\)](#) find that improved road connectivity in India reduces crime through better state monitoring. Although studies examine completed infrastructure, whereas we study the construction phase, when the inflow of workers is plausibly largest and local institutions

may be least prepared to absorb it. In this respect our setting may be closer to an extraction boom than to completed infrastructure, which may instead strengthen state presence, although we cannot test this distinction directly because we observe neither the workforce nor institutional capacity at the municipal level. The rise in assaults in public spaces is in line with evidence that women face higher victimisation risk in high-traffic and service settings (Moreira and Ceccato 2021; Savard et al. 2020). Finally, we find no evidence that the assault effects are mainly driven by the pandemic, and the estimates remain significant when the smallest municipalities are excluded and when each treated municipality is dropped in turn, suggesting that they are not driven by a few tourist destinations or by small municipalities with volatile rates.

We acknowledge several limitations when interpreting our results. Firstly, our main measure of violence using hospitalisation data detects only the most severe cases, leaving out all those victims who cannot reach the hospital, and other less severe cases. Secondly, we cannot test two channels that may have accompanied construction. The first is demographic: construction attracted a large temporary workforce, predominantly male, which may have shifted local sex ratios and weakened informal social monitoring in small communities. The second is the presence of security forces: three of the seven sections were built by the army, and military deployment, in the four states for which data are available, rose from about 23,000 personnel in 2020 to 29,500 in 2022 (Table A.2). A larger security presence could either deter violence or alter local social dynamics, so its net effect is ambiguous. Because neither the workforce nor military deployment is reported at the municipal level, we cannot separate these channels from the effect of construction itself. Thirdly, our employment data cover formal jobs only. Informal employment is substantial in agriculture and local services and may also have changed, but INEGI's household surveys are not representative at the municipal level, so our labour-market results describe the formal sector only. Finally, our findings come from a single project in southeastern Mexico, and whether they extend to other infrastructure projects, countries, or institutional settings remains an open question.

Our findings carry implications for how large infrastructure investments are appraised. Megaprojects are typically evaluated in terms of growth, employment and regional development, whereas our results indicate that violence against women may be an unintended consequence. In practice, this means incorporating gender-based violence into social impact assessments, strengthening local prevention and response services before construction begins rather than after, and monitoring violence throughout implementation.

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A Appendix

A.1 Contextual information

Municipality	State	Tramo(s)	Station	Treatment year
<i>Panel A. Treated in 2020 (Tramos 1–4)</i>				
Palenque	Chiapas	1	Yes	2020
Tenosique	Tabasco	1	Yes	2020
Balancán	Tabasco	1	Yes	2020
Candelaria	Campeche	1	Yes	2020
La Libertad	Chiapas	1	No	2020
Escárcega	Campeche	1, 2, 7	Yes	2020
Champotón	Campeche	2	Yes	2020
Campeche	Campeche	2	Yes	2020
Tenabo	Campeche	2	Yes	2020
Hecelchakán	Campeche	2	Yes	2020
Calkiní	Campeche	2, 3	Yes	2020
Maxcanú	Yucatán	3	Yes	2020
Umán	Yucatán	3	Yes	2020
Mérida	Yucatán	3	Yes	2020
Kanasín	Yucatán	3	Yes	2020
Tixkokob	Yucatán	3	Yes	2020
Halachó	Yucatán	3	No	2020
Chocholá	Yucatán	3	No	2020
Kopomá	Yucatán	3	No	2020
Cacalchén	Yucatán	3	No	2020
Tixpehual	Yucatán	3	No	2020
Izamal	Yucatán	3, 4	Yes	2020
Tinúm	Yucatán	4	Yes	2020
Valladolid	Yucatán	4	Yes	2020
Lázaro Cárdenas	Q. Roo	4	Yes	2020
Puerto Morelos	Q. Roo	4, 5N	Yes	2020
Benito Juárez	Q. Roo	4, 5N	Yes	2020
Bokobá	Yucatán	4	No	2020
Tekantó	Yucatán	4	No	2020
Tunkás	Yucatán	4	No	2020
Quintana Roo	Yucatán	4	No	2020
Dzitas	Yucatán	4	No	2020
Kaua	Yucatán	4	No	2020
Cuncunul	Yucatán	4	No	2020
Chemax	Yucatán	4	No	2020
<i>Panel B. Treated in 2022 (Tramos 5–7, SEDENA)</i>				
Solidaridad	Q. Roo	5N, 5S	Yes	2022
Tulum	Q. Roo	5S, 6	Yes	2022
Felipe Carrillo Puerto	Q. Roo	6	Yes	2022
Bacalar	Q. Roo	6, 7	Yes	2022
Othón P. Blanco	Q. Roo	6, 7	Yes	2022
Calakmul	Campeche	7	Yes	2022
Treated in 2020: 35	Treated in 2022: 6	Total treated: 41		

Table A.1: Treated municipalities, tramos crossing each municipality, presence of a station, and year of treatment. A municipality is assigned to the cohort of the earliest construction start among the tramos that cross it; Escárcega, Puerto Morelos, and Benito Juárez are crossed by both a 2020 and a 2022 tramo and are therefore assigned to 2020. Some municipalities have more than one

Table A.2: Military personnel deployed by state and per capita rates, 2020 and 2022

Year	State	SEDENA	National Guard	SEMAR	Total	Per 100,000 inh.
2020	Chiapas	3,536	3,466	1,504	8,506	148.4
2020	Campeche	1,088	1,301	1,314	3,703	370.1
2020	Yucatán	1,577	768	434	2,779	123.0
2020	Quintana Roo	1,242	1,619	724	3,585	208.0
2020	Tabasco	2,049	1,301	1,135	4,485	174.3
2020	Totals TM States				23,058	
2020	Oaxaca	2,626	3,770	821	7,217	174.1
2020	Veracruz	3,596	4,230	4,885	12,711	148.9
2022	Chiapas	7,831	3,438	1,561	12,830	218.8
2022	Campeche	–	–	–	–	–
2022	Yucatán	2,579	968	704	4,251	175.7
2022	Quintana Roo	1,707	1,780	1,889	5,376	271.9
2022	Tabasco	2,368	2,615	2,050	7,033	162.4
2022	Totals TM States				29,490	
2022	Oaxaca	6,661	4,507	1,840	13,008	279.2
2022	Veracruz	6,487	4,302	5,217	16,006	196.8

Source: Military data for 2020 were elaborated by Alberto Hidalgo with information from Causa Común (UNAM 2021). The data for 2022 are retrieved from the report of the *Mision civil de observacion sobre el Tren Maya* (Informe 2025) using data provided by SEDENA (no data on Campeche were provided).

The totals per capita were elaborated by the authors by using data on total annual population per state provided by INEGI for 2020 (INEGI 2020) and the population projections for 2022 by CONAPO (CONAPO 2024).

Oaxaca and Veracruz, the two states adjacent to the Peninsula, are added as a reference of comparison.

A.2 Data and descriptive statistics

Table A.3: ICD-10 assault codes (X85–Y09): share of assault-block cases, classification, and cases excluded from the non-IPV measure by the perpetrator-relationship field. Codes below 0.10% are grouped in “Other codes”; the full classification comprises 241 four-character codes.

Code	Description	% Cases	Outcome	Cases excluded
Y070	Other maltreatment by spouse or partner	27.85	IPV	–
Y079	Other maltreatment by unspecified perpetrator	7.19	non-IPV	763
X994	Assault by sharp object, street and highway	5.80	non-IPV	31
Y044	Assault by bodily force, street and highway	5.20	non-IPV	112
Y040	Assault by bodily force, home	5.05	non-IPV	1,526

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Table A.3 – continued from previous page

Code	Description	% of Cases	Outcome	Excluded Cases
Y078	Other maltreatment by other specified perpetrator	4.62	non-IPV	260
Y071	Other maltreatment by parent	4.04	non-IPV	213
X990	Assault by sharp object, home	3.90	non-IPV	451
Y050	Sexual assault by bodily force, home	3.56	non-IPV	955
X999	Assault by sharp object, unspecified place	3.17	non-IPV	112
Y004	Assault by blunt object, street and highway	2.48	non-IPV	22
Y059	Sexual assault by bodily force, unspecified place	2.06	non-IPV	217
Y072	Other maltreatment by acquaintance or friend	2.01	non-IPV	85
Y049	Assault by bodily force, unspecified place	1.90	non-IPV	123
Y000	Assault by blunt object, home	1.79	non-IPV	229
X998	Assault by sharp object, other specified place	1.57	non-IPV	21
Y009	Assault by blunt object, unspecified place	1.32	non-IPV	22
X934	Assault by handgun discharge, street and highway	1.08	non-IPV	2
Y048	Assault by bodily force, other specified place	1.05	non-IPV	50
Y034	Assault by crashing of a motor vehicle, street and highway	0.88	non-IPV	4
X954	Assault by other and unspecified firearm discharge, street and highway	0.80	non-IPV	0
Y060	Neglect and abandonment by spouse or partner	0.74	IPV	–
Y008	Assault by blunt object, other specified place	0.65	non-IPV	11
Y058	Sexual assault by bodily force, other specified place	0.63	non-IPV	22
Y099	Assault by unspecified means, unspecified place	0.57	non-IPV	84
Y090	Assault by unspecified means, home	0.50	non-IPV	153
Y054	Sexual assault by bodily force, street and highway	0.50	non-IPV	4
Y080	Assault by other specified means, home	0.48	non-IPV	135
X959	Assault by other and unspecified firearm discharge, unspecified place	0.43	non-IPV	0
Y084	Assault by other specified means, street and highway	0.36	non-IPV	2
X930	Assault by handgun discharge, home	0.36	non-IPV	3
X950	Assault by other and unspecified firearm discharge, home	0.36	non-IPV	9

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Table A.3 – continued from previous page

Code	Description	% of Cases	Outcome	Excluded Cases
X939	Assault by handgun discharge, unspecified place	0.34	non-IPV	0
Y094	Assault by unspecified means, street and high-way	0.32	non-IPV	3
X938	Assault by handgun discharge, other specified place	0.28	non-IPV	0
X995	Assault by sharp object, trade and service area	0.28	non-IPV	4
Y042	Assault by bodily force, school, institution and public administrative area	0.27	non-IPV	4
Y043	Assault by bodily force, sports and athletics area	0.26	non-IPV	9
Y039	Assault by crashing of a motor vehicle, unspecified place	0.25	non-IPV	1
Y089	Assault by other specified means, unspecified place	0.24	non-IPV	10
Y045	Assault by bodily force, trade and service area	0.22	non-IPV	10
X996	Assault by sharp object, industrial and construction area	0.19	non-IPV	3
X993	Assault by sharp object, sports and athletics area	0.18	non-IPV	3
X958	Assault by other and unspecified firearm discharge, other specified place	0.17	non-IPV	0
Y061	Neglect and abandonment by parent	0.16	non-IPV	5
X992	Assault by sharp object, school, institution and public administrative area	0.16	non-IPV	3
Y046	Assault by bodily force, industrial and construction area	0.14	non-IPV	3
Y062	Neglect and abandonment by acquaintance or friend	0.13	non-IPV	6
Y038	Assault by crashing of a motor vehicle, other specified place	0.13	non-IPV	1
X997	Assault by sharp object, farm	0.12	non-IPV	0
Y088	Assault by other specified means, other specified place	0.12	non-IPV	1
Y005	Assault by blunt object, trade and service area	0.11	non-IPV	1
Y006	Assault by blunt object, industrial and construction area	0.11	non-IPV	2

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Table A.3 – continued from previous page

Code	Description	% of Cases	Outcome	Excluded Cases
Y073	Other maltreatment by official authorities	0.11	non-IPV	10
Y003	Assault by blunt object, sports and athletics area	0.10	non-IPV	0
X910	Assault by hanging, strangulation and suffocation, home	0.10	non-IPV	39
Y052	Sexual assault by bodily force, school, institution and public administrative area	0.10	non-IPV	4
Y055	Sexual assault by bodily force, trade and service area	0.10	non-IPV	5
Other	149 further codes, each < 0.10% codes	2.32	non-IPV	123
<i>IPV-related cases (Y060, Y070)</i>				27,597
<i>Non-IPV cases, after cleaning</i>				63,059
<i>Cases excluded from both measures</i>				5,871

Table A.4: Balance tests: Pre-treatment differences between treated and control municipalities

Variable	Coefficient	SE	p-value
Total Population	38210.09	32498.89	0.242
Female Population	18951.45	16518.10	0.253
Male Population	19258.64	15989.61	0.231
Gender-based violence rate crimes	3.32	15.81	0.834
Hotel Rooms per Capita	862.996	383.129	0.026
Observations	625		

Note: Regressions of each pre-treatment covariate on a treatment indicator over 2015–2019, with standard errors clustered at the municipality level, on the main sample of estimation which excludes neighbouring municipalities within 15 km, and the three municipalities without opened hospitals in the period of interest. Coefficient is the difference between treated and control municipalities. Gender-based violence rate crimes include the sum of the crimes categorized as intimate partner, sexual abuse, femicide, sexual harassment, rape, and other 'gender violence', per 100,000 inhabitants.

A.3 Additional results

Table A.5: Effects of Tren Maya construction on other crime outcomes

Crimes Outcomes	ATT	SE (ATT)	% Change (ATT)	Pretrend p
Feminicides	-0.101	0.206	-36.0%	0.002
Rape	6.856***	1.744	73.8%	0.004
Sexual Stalking	1.702***	0.580	115.6%	0.152
Sexual Harassment	0.155	0.342	22.3%	0.908
Gender-Based Violence (Other)	-0.045	0.035	-52.3%	0.390

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Borusyak et al. (2024) imputation estimator on crime outcomes from SESNSP. ATT is the average of θ_0 , θ_1 and θ_2 . Pretrend p is the joint test on the two pre-treatment leads (β_{-2} , β_{-1}). Standard errors clustered at the municipality level. All specifications use 1,210 municipality-year observations.

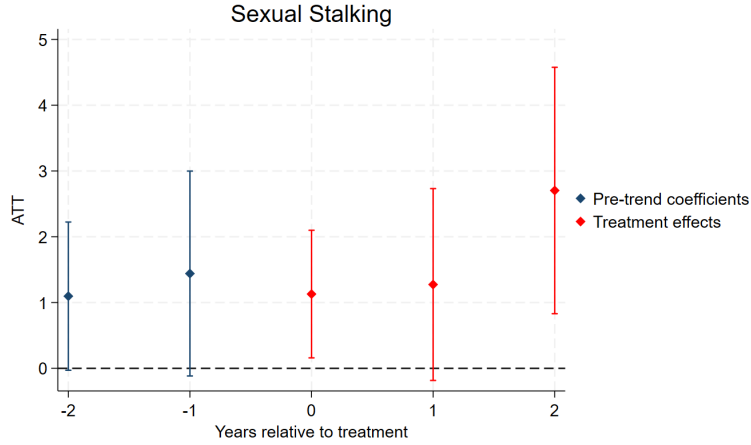


Figure A.1: Event study: Sexual stalking (*acoso sexual*). Borusyak et al. (2024) imputation estimator, two pre-treatment leads and horizons 0–2. The pre-trend joint test is not significant at conventional levels ($p = 0.152$) but the individual pre-treatment coefficients lie close to the boundary of statistical significance, as reflected in their confidence intervals.

A.4 Robustness checks and alternative explanations

Table A.6: Robustness: Callaway and Sant'Anna (2021) estimator

Outcome	ATT	SE	p-value	N
Total female assault	44.96***	(12.99)	(0.00)	1,250
IPV-related assault (females)	27.69***	(7.21)	(0.00)	1,250
Non-IPV assault (females)	16.26**	(8.34)	(0.05)	1,250
Sexual abuse (crimes)	3.7*	(1.76)	(0.05)	1,280

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Callaway and Sant'Anna (2021) staggered difference-in-differences estimator with wild bootstrap standard errors. Municipality and year fixed effects. Rates per 100,000 population.

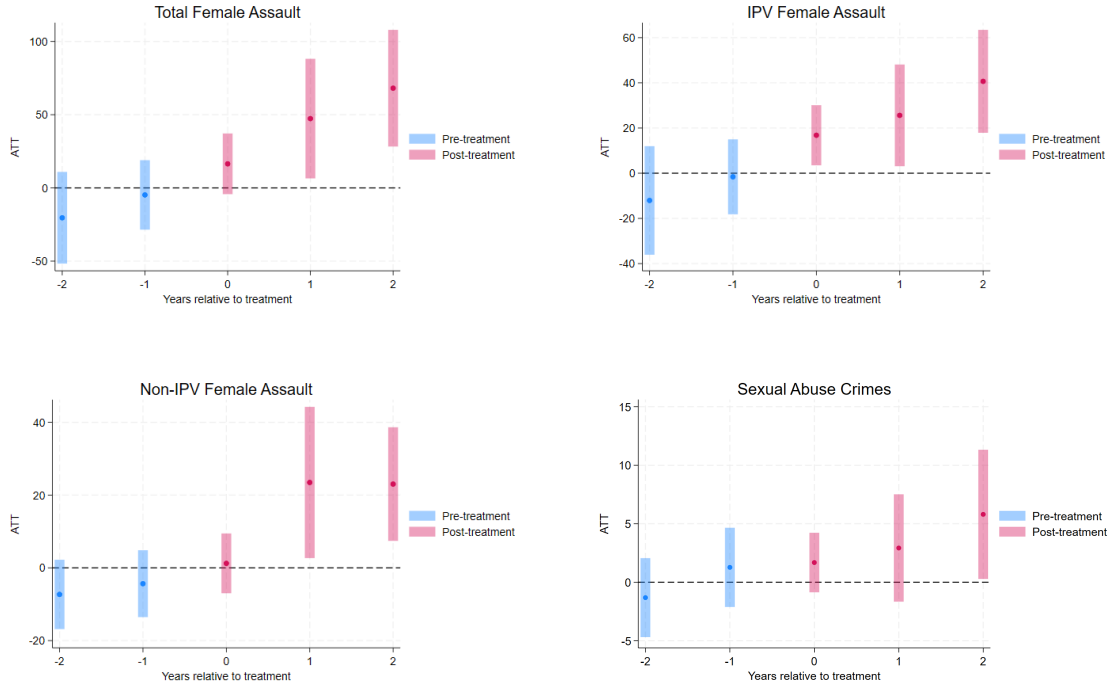


Figure A.2: Robustness: Callaway and Sant'Anna (2021) event studies. Results are consistent with the main Borusyak et al. (2024) estimates reported in Figure 3. Municipality and year fixed effects; wild bootstrap standard errors.

A.5 COVID robustness checks

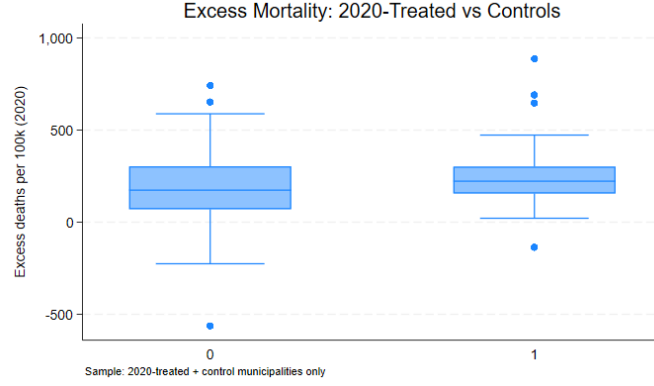


Figure A.3: Excess mortality in 2020: Treated vs Control municipalities. The boxplot shows the distribution of excess mortality rates per 100,000 population in 2020 across treated (Tren Maya construction) and control municipalities. The difference is not statistically significant ($p=0.103$).

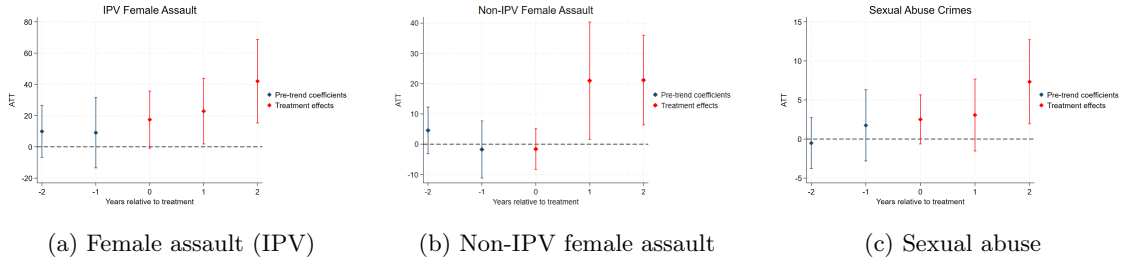
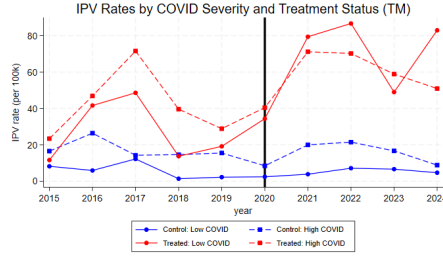


Figure A.4: Event studies controlling for excess mortality. Borusyak et al. (2024) imputation estimator with municipality-level excess mortality rate per 100,000 as a control variable. Municipality and year fixed effects; standard errors clustered at the municipality level.

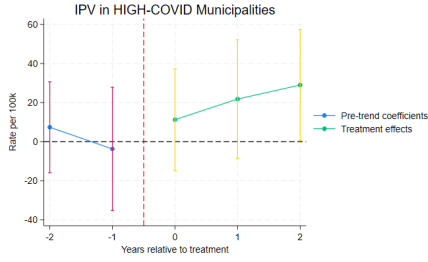
Table A.7: Heterogeneous treatment effects by COVID-19 severity: ATT estimates

	High COVID			Low COVID		
	ATT	SE	Pretrend p	ATT	SE	Pretrend p
IPV female assault	20.68*	(12.20)	0.553	37.78***	(12.77)	0.252
Non-IPV female assault	10.07	(7.21)	0.488	18.23**	(8.02)	0.153
Sexual abuse	3.05	(2.49)	0.883	6.12**	(2.71)	0.267
Observations	664 / 664 / 674			516 / 516 / 536		

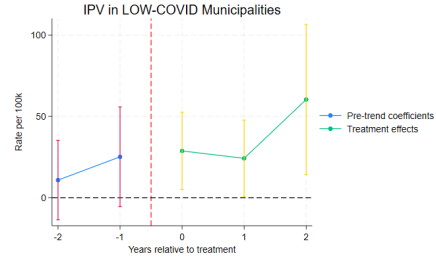
Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Borusyak et al. (2024) imputation estimator. Sample split at the median of the 2020 excess mortality rate. ATT is the average of the three post-treatment period coefficients (τ_0, τ_1, τ_2). Pretrend p is the joint test on the two pre-treatment leads (β_{-2}, β_{-1}). Municipality and year fixed effects; standard errors clustered at the municipality level.



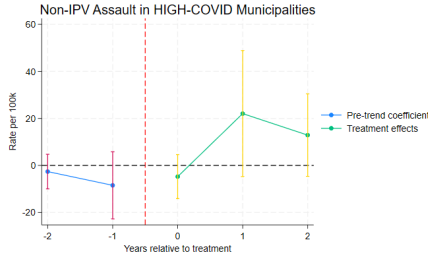
(a) IPV assaults raw trends by COVID severity and treatment status



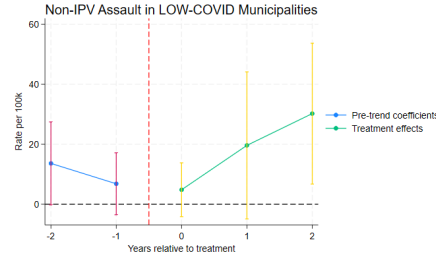
(b) IPV: High-COVID municipalities



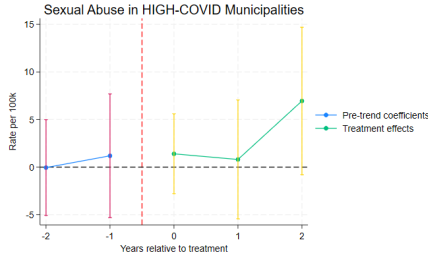
(c) IPV: Low-COVID municipalities



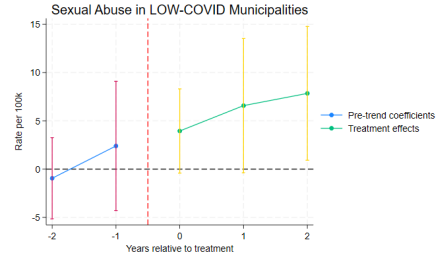
(d) Non-IPV assault: High-COVID municipalities



(e) Non-IPV assault: Low-COVID municipalities



(f) Sexual abuse: High-COVID municipalities



(g) Sexual abuse: Low-COVID municipalities

Figure A.5: Heterogeneous treatment effects by COVID-19 severity, across the three main outcomes. Panel (a) shows raw trends in female IPV assault rates by COVID severity (median cutoff on 2020 excess mortality) and treatment status. Remaining panels present separate Borusyak et al. (2024) event studies for high- and low-COVID municipalities. For all three outcomes, estimated effects are larger in low-COVID municipalities than in high-COVID municipalities, a pattern that runs counter to a pandemic-severity explanation for the observed increases in violence. All pre-trend tests pass at conventional levels (Table A.7). Results are robust to using the mean as an alternative cutoff.

Table A.8: Covid as the treatment on female IPV-assaults

	(1)	(2)	(3)	(4)	(5)
	Continuous	Continuous + TM	Binned	Binned + TM	Controls Only
<i>Panel A: COVID Intensity (Excess Mortality)</i>					
Excess Mort. $\times$ Post	0.593 (2.350)	-1.307 (2.071)			1.085 (1.434)
Medium COVID $\times$ Post			3.921 (8.895)	-2.753 (7.912)	
High COVID $\times$ Post			-2.631 (8.323)	-5.620 (7.823)	
<i>Panel B: Tren Maya Treatment</i>					
TM $\times$ Post		27.779*** (10.597)		27.683** (10.635)	
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1,250	1,250	1,250	1,250	850

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable: female IPV assault rate per 100,000. We report this analysis for IPV only, the outcome most likely affected by COVID-19 restrictions. Standard errors in parentheses;

Columns (1)–(2) use standardised continuous excess mortality interacted with post-2020. Columns (3)–(4) use terciles of excess mortality (reference: Low COVID). Column (5) restricts the sample to control municipalities only (no Tren Maya construction). Standard errors clustered at the municipality level in parentheses.

A.6 Tourism analysis

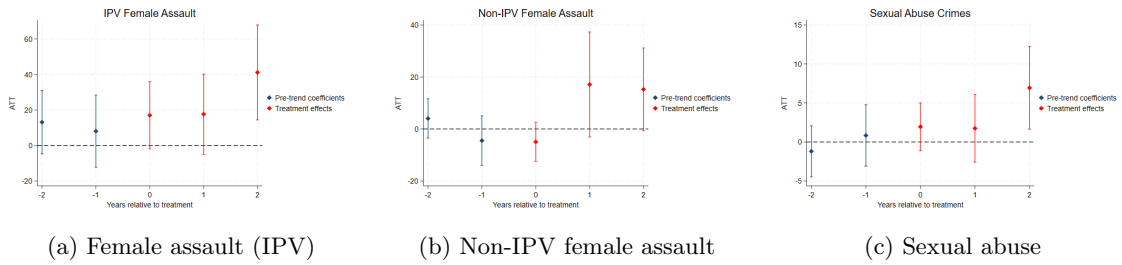


Figure A.6: Event studies controlling for tourism capacity. Borusyak et al. (2024) imputation estimator with hotel rooms per capita as a control variable. Municipality and year fixed effects; standard errors clustered at the municipality level.

A.7 Other robustness

Alternative IPV broad measure

Table A.9: Robustness: broad definition of intimate partner violence. The broad measure adds to the strict IPV outcome (ICD-10 codes Y060, Y070) all assault-block cases flagged as partner-perpetrated by the DGIS perpetrator-relationship field. Imputation estimator (Borusyak et al. 2024), municipality and year fixed effects, standard errors clustered by municipality; horizons 0–2, pre-trend test with two leads.

	Strict IPV (Y060, Y070)	Broad IPV (+ relationship field)
ATT (per 100,000 women)	27.42*** (9.33)	29.74*** (10.09)
Effect relative to baseline	67.5%	61.6%
Pre-trend test (p)	0.51	0.38
Baseline mean	40.64	48.31
Added IPV cases	—	5,871
Observations	1,180	1,180

*** $p < 0.01$.

Small municipalities test

Table A.10: Robustness: excluding the smallest municipalities

Outcome	ATT	SE	Pre-treat Mean	% Change	Pretrend p	N
Total female assault	48.49**	(20.02)	98.93	+49.0%	0.234	896
IPV-related assault	30.51**	(13.13)	55.50	+55.0%	0.492	896
Non-IPV female assault	12.72*	(6.52)	35.25	+36.1%	0.161	896

Notes: Estimates exclude municipalities in the bottom quartile of the distribution of female population in 2019 (fewer than 5,843 women), leaving 27 of the 40 treated municipalities. ATT is the average of the effects at horizons 0 to 2 estimated with the imputation estimator of (Borusyak et al. 2024). Because fewer than 30 treated municipalities remain, estimates are obtained setting the minimum effective sample size to zero, and standard errors may be slightly understated. Pre-treatment mean refers to treated municipalities in this sample. Pretrend p reports the joint test that the two pre-treatment coefficients are zero. Standard errors clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

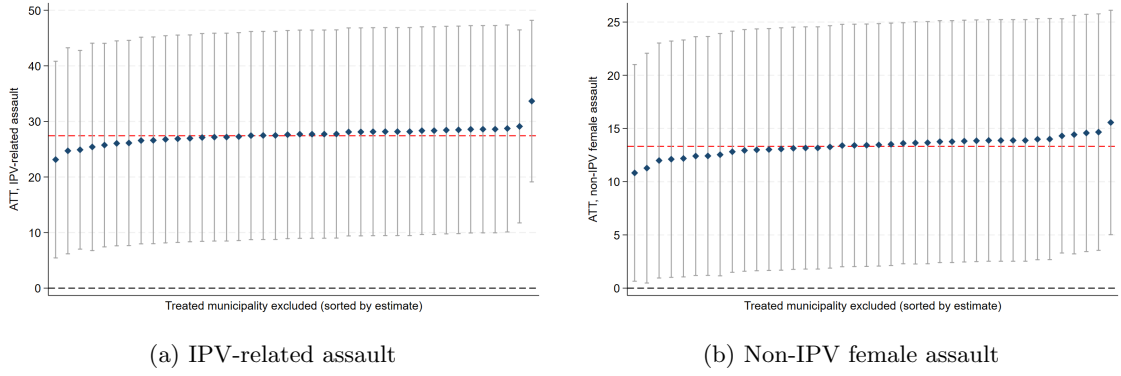


Figure A.7: Leave-one-out estimates

Notes: Each point is the ATT estimated with the imputation estimator of [Borusyak et al. \(2024\)](#), excluding one treated municipality at a time, with 95% confidence intervals based on standard errors clustered at the municipality level. Estimates are sorted by magnitude. The dashed red line indicates the estimate on the full sample; the dashed black line indicates zero.

A.8 Mechanisms

A.8.1 Employment event-studies

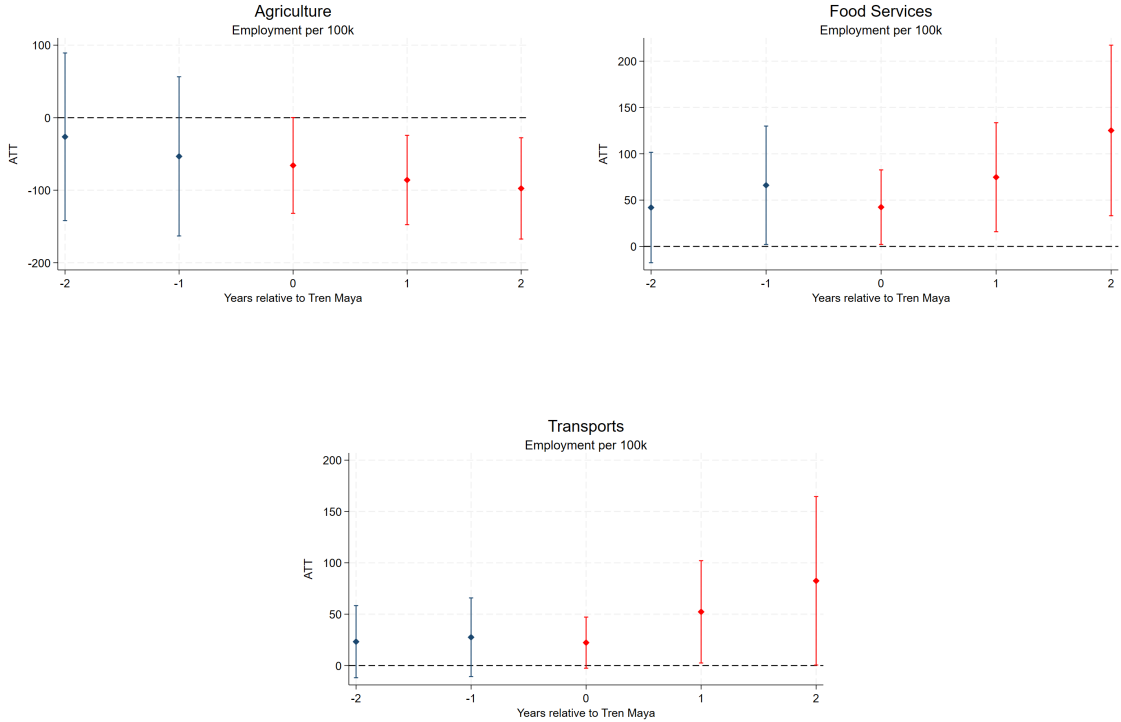


Figure A.8: Event studies: effects of Tren Maya construction on formal employment. [Borusyak et al. \(2024\)](#) imputation estimator, two pre-treatment leads and horizons 0–2. Municipality and year fixed effects; standard errors clustered at the municipality level.

A.8.2 Alcohol and drugs event-studies



Figure A.9: Event studies on alcohol and drug-related hospitalisations. Borusyak et al. (2024) imputation estimator. Municipality and year fixed effects; standard errors clustered at the municipality level.



Figure A.10: Event studies on female alcohol and drug-related hospitalisations. Borusyak et al. (2024) imputation estimator. Municipality and year fixed effects; standard errors clustered at the municipality level.

A.8.3 Assaults in public spaces event-studies

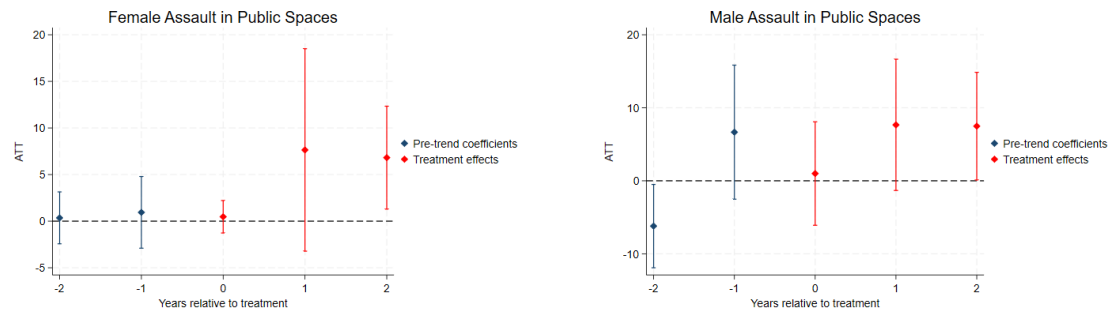


Figure A.11: Assaults recorded in public locations. Borusyak et al. (2024) estimator. Municipality and year fixed effects; standard errors clustered at the municipality level. The male series fails the joint pre-trend test and is reported for comparison only.