Is a Trade War Also a Gender War? Gendered Economic Opportunities and Partner Violence in Mexico

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Abstract

Do women's economic gains relative to men reduce intimate partner violence or trigger backlash? The 2018–2019 US–China trade war makes Mexico a natural testing ground: US tariffs on Chinese goods raised demand for Mexican exports, while tariffs on Mexican goods lowered it. We develop a household model in which, holding average exposure fixed, the sign of the violence response to the female–male exposure gap reveals whether bargaining or backlash dominates. Using a gendered shift-share design and surveys of women in 2016 and 2021, we find that a one-standard-deviation larger China-tariff exposure gap predicts a 0.46-percentage-point (7 percent) rise in past-year physical partner violence. The same gap also predicts more sadness or depression attributed to partner problems. In contrast, a larger Mexico-tariff exposure gap, which worsens women's relative position, predicts less physical violence. Under the model's assumptions, these opposing responses are consistent with backlash outweighing bargaining. Trade-induced gains in women's economic opportunities can coexist with greater partner violence, highlighting the need to complement economic opportunity with protection against abuse.

Keywords: intimate partner violence; US-China trade war; trade diversion; gender; Mexico

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1 Introduction

More than one in four ever-partnered women aged 15–49 worldwide have experienced physical or sexual violence from a partner (Sardinha et al., 2022). Improving women's economic opportunities can reduce their dependence on a partner and strengthen their ability to leave an abusive relationship (Aizer, 2010). Yet gains in a woman's position relative to her partner can also provoke backlash, as evidence from trade-induced shifts in employment suggests (Erten and Keskin, 2024). Which force prevails matters for policies that change women's economic position through transfers, employment programs and trade. Trade is especially relevant because it changes labor demand across industries and places, affecting the opportunities available to both women and men. Understanding its household consequences therefore requires distinguishing changes in women's relative position from changes in average local economic opportunities. We examine this distinction in Mexico during the US–China trade war.

The 2018–2019 US–China trade war created new export opportunities for Mexico, with different implications for women's and men's economic position. US tariffs on Chinese goods diverted demand toward competing Mexican producers (Fajgelbaum et al., 2024; Chen et al., 2025; Utar et al., 2025). Exposed Mexican exporters expanded employment and raised wages, with larger wage gains for women than men (Chen et al., 2025). Because women and men worked in different industries, the same tariff changes could shift their relative opportunities differently across municipalities. Mexico also faced direct US tariffs on its own exports, which we distinguish from tariffs on Chinese competitors. We follow these economic changes into the household using the National Survey on the Dynamics of Household Relationships (ENDIREH). Its 2016 and 2021 waves record women's experiences of physical, sexual, emotional and economic partner violence before and after the tariff increases, allowing us to connect local exposure to changes in reported abuse.

We use a gendered shift-share design that combines industry-level US tariff increases with women's and men's predetermined employment shares across Mexican municipalities. Using formal employment records from the Mexican Social Security Institute (IMSS), we construct separate exposure measures for women and men, weighting industry tariff changes by their 2017 employment shares and the share of each industry's exports sent to the US. For tariffs on China and on Mexico separately, the female-minus-male exposure gap measures women's exposure relative to men's within a municipality. We assign these municipality-level measures to ever-partnered women in ENDIREH, including women who are not employed. We compare changes in reported partner violence across municipalities, including the China-tariff exposure gap, the Mexico-tariff exposure gap and both corresponding averages in the same regression. Identification rests on industry-level tariff changes being as good as randomly assigned with respect to other determinants of changes in partner violence in the Mexican municipalities exposed to those industries. US trade-policy disputes provide the rationale for this assumption; predetermined employment shares determine how the tariff shocks translate into local exposure.

These changes in labor-market opportunities can affect partner violence in opposing ways, depending on how they alter the economic position of each spouse. We develop a model of household bargaining and violence to explain these responses. An improvement in a woman's earnings relative to her husband's strengthens her bargaining position, making violence more costly to him, but can also increase his incentive to use violence to reassert his status within the household. Whether violence increases or decreases depends on the relative strength of these bargaining and backlash forces.

Under the model's wage-pass-through and aggregation assumptions, changes in violence can be expressed, to a first-order approximation, as a function of the female–male gap in tariff exposure and average exposure. The gap captures changes in relative economic opportunities. Average exposure also matters: equal earnings increases change spouses' relative positions when initial earnings differ, while women without market earnings experience wage shocks through their husbands. The model assumes that tariffs on China raise wages in exposed Mexican industries, whereas tariffs on Mexico lower them. If backlash dominates in households with working and nonworking wives, violence rises with the China-tariff exposure gap and falls with the Mexico-tariff exposure gap, holding average exposure fixed. If bargaining dominates in both groups, these signs reverse.

A larger China-tariff exposure gap predicts an increase in physical partner violence, whereas a larger Mexico-tariff exposure gap predicts a decline. A one-standard-deviation larger China-tariff exposure gap predicts a 0.46-percentage-point increase in past-year physical violence, equivalent to 7.2 percent of its 2016 prevalence. The corresponding estimate for the Mexico-tariff exposure gap is a decline of 0.38 percentage points, and lifetime reports show the same opposing signs. This pattern is consistent with backlash outweighing bargaining under the model's assumptions. Greater average China-tariff exposure, however, accompanies less past-year physical violence. Estimates for other forms of partner violence are generally imprecise, although the Mexico-tariff exposure gap predicts more past-year sexual violence alongside less physical abuse.

The increase in physical violence accompanies gains in women's mean income and more favorable formal wage movements for women than men. A one-standard-deviation larger China-tariff exposure gap predicts 5.3 percent higher mean monthly own income, estimated by Poisson pseudo-maximum likelihood (PPML) with valid zero-income observations retained (Chen and Roth, 2024). Income gains are smaller and less precisely estimated among women with positive income, and capping or trimming high incomes reduces the mean-income estimate and its precision. Estimates for employment and women's share of recorded household income are imprecise. Monthly IMSS records show more positive estimated wage responses to the China-tariff exposure gap for women than men after 2018. In manufacturing, women's estimates turn positive in late 2018; across all sectors, their estimates remain closer to zero while men's are mostly negative. Adding women's own income to the violence regression leaves the exposure-gap estimates almost unchanged on the same sample. This pattern is consistent with our model, in which relative eco-

nomic position depends on both partners' resources.

Moreover, we find that a larger China-tariff exposure gap also predicts more severe physical violence and more reported psychological distress. In lifetime reports, the most extreme category of physical violence, which includes attempted strangulation and attacks with knives or firearms, rises by 0.28 percentage points per standard deviation, or 7.6 percent of its 2016 rate. ENDIREH also asks women whether problems with their partner left them sad or depressed. A one-standard-deviation larger China-tariff exposure gap predicts 1.5 percentage points more women reporting both partner violence and this distress, 8.6 percent of the 2016 rate. Estimates for suicidal ideation and suicide attempts, which are much rarer, are imprecise.

Comparisons with other survey outcomes and with administrative records suggest that the response is specific to partner violence and is not captured by official records. Survey reports could rise because women became more willing to disclose abuse, so we compare them with other sources. Violence in the community, at work and at school shows no detectable response to the China-tariff exposure gap, and neither do women's recollections of abuse in childhood, which predates the trade war. We also assemble municipal records of family-violence offenses from criminal investigation files. These records show no response to either exposure gap. Most partner violence in Mexico never reaches the authorities: 78 percent of women who experienced physical or sexual partner violence neither sought help nor filed a complaint (INEGI, 2022a). Analyses based only on administrative records would therefore not detect the response observed in the survey.

We subject the empirical result to a range of robustness checks, and the estimates remain consistent in sign throughout. The estimates for the China-tariff exposure gap remain positive and close to the baseline with state-by-wave fixed effects and controls for pandemic exposure and concurrent policies. They also remain positive across employment-share vintages and when we omit individual industries. Excluding industries affected by both tariff programs leaves the estimates for the China-tariff exposure gap nearly unchanged, although the estimates for the Mexico-tariff exposure gap become smaller and imprecise. Alternative trade weights reduce the magnitude and precision of the estimates for the China-tariff exposure gap but preserve their positive sign. The 2011 survey provides an inconclusive check on whether violence was already evolving differently across municipalities with different tariff exposure.

This paper makes two contributions. First, we bring the household into the study of trade diversion, which has focused on exports, firms and employment in third countries (Utar et al., 2025; Chen et al., 2025). We ask how the gains from trade diversion changed women's position relative to men's and how violence at home responded. Second, we pair a household model with an empirical design built to match it. The model shows that, holding average exposure fixed, the sign of the response to the female–male exposure gap distinguishes bargaining from backlash. We measure this gap with a gendered shift-share design, which suits the setting. The estimates answer the question the model leaves open: physical violence rises where women gain ground relative to men and falls where they lose it, consistent with backlash. The same gap also predicts

more sadness or depression that women attribute to their partners. In this sense, the trade war was also a gender war: it shifted economic opportunities between women and men, and violence and distress at home moved with them.

Specifically, we connect research on the household consequences of trade with evidence on the economic gains from trade diversion. Trade shocks reshape women's and men's employment and family choices (Keller and Utar, 2022), and studies of Cambodia and Peru document increases in partner violence following domestic trade liberalization (Chong and Velásquez, 2024; Erten and Keskin, 2024). Recent work on Mexico and Vietnam shows how US tariffs on China redirect trade and production and create new employment opportunities in third countries (Chen et al., 2025; Mayr-Dorn et al., 2026; Utar et al., 2025; Utar, 2026). We follow these opportunities into Mexican households, distinguishing exposure to tariffs on Chinese competitors from exposure to tariffs directly affecting Mexican exports. Our findings show that gains in women's mean income can accompany greater physical violence, linking the distribution of trade-induced opportunities between women and men to conflict within households.

A broader literature examines how women's economic opportunities relative to men's affect partner violence. Evidence from gender-specific wages and unemployment shows that improved opportunities for women relative to men can reduce violence (Aizer, 2010; Anderberg et al., 2016). Other studies link women's economic gains to greater abuse, emphasizing the roles of gender norms and men's control over household resources (Heath, 2014; Erten and Keskin, 2021; Tur-Prats, 2021; Zhang and Breunig, 2023). Employment can also protect women when husbands benefit from their productivity (Sanin, 2025), while randomized job offers in Ethiopia raise earnings without a detectable effect on physical violence (Kotsadam and Villanger, 2025). Studies of transfers further examine how violence responds when additional resources accrue to women or men (Haushofer et al., 2019; Bellés-Obrero et al., 2025).[1] We extend this literature by comparing the China-tariff exposure gap and Mexico-tariff exposure gap while accounting for average exposure to each program. Under the model's wage assumptions, these gaps capture improvements and deteriorations in women's relative opportunities within the same setting. Their contrasting physical-violence responses provide evidence on whether bargaining or backlash dominates.

The model connects changes in violence within couples to the local tariff exposure measured in the empirical analysis. Existing models explain how outside options constrain violence and how abuse can be used to influence household allocations or restrict women's employment (Bloch and Rao, 2002; Aizer, 2010; Eswaran and Malhotra, 2011; Anderberg and Rainer, 2013). Haushofer et al. (2019) show how responses to income changes accruing to each spouse can reveal motives for violence. Our model links these questions about household behavior to the sectoral wage changes generated by tariffs. Aggregating household responses yields an estimating equation in which the female–male exposure gap captures changes in the distribution of economic opportunities,

[1] Related studies use exchange-rate movements to examine relative earnings and police-recorded violence in Uruguay (Munyo and Rossi, 2015), and gender-specific labor demand to study relative earnings and assault-related hospital visits in Sweden (Bergvall, 2024). In the latter, increased care-seeking accounts for part of the response.

alongside average exposure. Average exposure remains relevant because equal earnings increases change spouses' relative resources when initial earnings differ, while women outside paid employment experience wage shocks through their husbands. The model establishes the conditions under which the exposure-gap coefficients distinguish whether bargaining or backlash dominates, accounting for the opposite wage effects of tariffs on China and tariffs on Mexico.

The remainder of the paper is organized as follows. Section 2 describes the institutional background. Section 3 develops the model. Section 4 introduces the data, and Section 5 presents the empirical strategy. Section 6 reports the empirical findings, heterogeneity, and mechanisms, and Section 7 concludes.

2 Institutional Background

2.1 The US–China trade war

The United States sharply increased tariffs on Chinese goods in 2018 and 2019, changing the terms on which Chinese and Mexican producers competed in the US market. The China-specific increases began in July 2018 under Section 301 and covered roughly $350 billion of imports by the end of 2019. The January 2020 Phase One agreement halted further escalation while leaving elevated tariffs in place (United States International Trade Commission, 2019; Chen et al., 2025). US buyers of the affected goods consequently had an incentive to seek suppliers outside China. Mexican producers of competing goods could gain sales without receiving a tariff reduction themselves.

Mexico also faced US tariffs on its own exports. The Section 232 duties on steel and aluminum, introduced in March 2018 at rates of 25 and 10 percent, respectively, initially exempted Mexico. That exemption ended in June 2018; the United States and Mexico agreed to remove the duties in May 2019 (United States International Trade Commission, 2019; Office of the United States Trade Representative, 2019). These measures exposed Mexican producers to two changes in demand: an opportunity to replace Chinese suppliers and a barrier to selling their own goods. Industries affected by both faced competing pressures, so exposure to tariffs on China and exposure to tariffs on Mexico need to be distinguished.

2.2 Mexico's trade position and local employment

Mexico entered the trade war with close commercial ties to both the United States and China. In 2017, the United States was its largest export destination and source of imports, while China was its second-largest source of imports. Geographic proximity and preferential access under the North American Free Trade Agreement (NAFTA) made Mexico an established supplier to US buyers. Mexican and Chinese producers also sold competing products in that market (Chen et al., 2025). Mexico's position thus combined competition with China for US customers and access to Chinese

inputs for its own production.

Export manufacturing was already organized around firms that imported inputs, processed or assembled them in Mexico, and shipped their output abroad. The *Industria Manufacturera, Maquiladora y de Servicios de Exportación* (IMMEX) program facilitates this production by allowing temporary imports for processing and subsequent export. Its manufacturing establishments employed about 2.6 million people in October 2017, according to the National Institute of Statistics and Geography (INEGI) (INEGI, 2017b). In the 2017 customs data analyzed by Utar et al. (2025), IMMEX firms represented 16 percent of exporters but accounted for 84 percent of export value. Most imported goods as well as exporting them. Mexico therefore had substantial production capacity within international supply chains before the tariff increases.

Existing export firms were an important route through which the trade war expanded Mexican production. Utar et al. (2025) find that IMMEX firms, especially foreign multinationals, accounted for the main export response to US tariffs on China. These firms also increased imports from Asia and the United States. The expansion involved greater production within established supply chains, rather than simply replacing Chinese inputs with Mexican inputs. This distinction matters for where jobs could grow: established plants and their suppliers could expand before firms built new factories or relocated entire production processes.

Those plants were concentrated in particular regions. Chihuahua, Baja California and Nuevo León together accounted for about 37 percent of IMMEX employment in October 2017, with further concentrations in central states such as Guanajuato and Querétaro (INEGI, 2017b). Local exposure depended on what these plants produced, as the tariff increases differed across goods. Municipalities with similar manufacturing employment shares could therefore face different changes in demand. Figure 1 shows the resulting geographic variation in the China-tariff exposure gap between women and men.

Women and men could benefit differently even within an expanding export sector. Using linked Mexican exporter and worker records, Chen et al. (2025) find larger wage gains from trade diversion among women than men. Differences in their distribution across industries create a further source of variation across local labor markets. Where women worked in industries receiving stronger demand, their employment opportunities could improve relative to men's; where men were more concentrated in those industries, the reverse could occur.

2.3 Partner violence in Mexico

Violence against women was widespread before the trade war and remained so afterward. In ENDIREH, the share of women aged 15 and older reporting violence at some point in their lives rose from about 66 percent in 2016 to 70 percent in 2021. These figures cover violence across settings, including schools, workplaces and public spaces. Partner violence followed a different pattern: the share of ever-partnered women reporting violence by their current or most recent partner

in the preceding twelve months fell from about 26 to 21 percent (INEGI, 2024). Despite that decline, roughly two in five ever-partnered women in 2021 reported violence during their current or most recent relationship (INEGI, 2022b).

Partner violence extended well beyond physical assault. In 2021, about 18 percent of ever-partnered women reported psychological violence in the preceding twelve months, compared with 8.1 percent reporting economic or patrimonial violence, 5.2 percent physical violence and 2.1 percent sexual violence (INEGI, 2022b). These categories can overlap within a relationship. Women also experienced psychological, economic or sexual abuse without physical assault.

Much of this violence remained outside formal systems of assistance and reporting. Among women who had experienced physical or sexual partner violence during their relationship, about 78 percent reported neither seeking help nor filing a complaint (INEGI, 2022a). Survey reports cover experiences both inside and outside formal reporting systems, including economic and emotional abuse alongside physical and sexual violence.

3 Model

A trade shock arrives as an aggregate, a change in a municipality's exposure to the tariffed sectors, but it does not reach a woman that way. It reaches her as a change in what she earns relative to her partner, set by the sectors the two of them work in and by how the tariffs treat those sectors. Whether that change makes her safer or less safe at home is the question of this paper, and it is a question about the couple. This section builds the household model that turns it into a question the municipality-level data can answer, and that says which feature of the local shock the answer should be read from: the gender composition of exposure rather than its level.

Three results organize the section. First, no matter how many sectors the tariffs touch or how men and women sort across them, the reallocation channel (the tariffs' repricing of the two spouses' work) affects a municipality's violence, to a first-order approximation, through a single scalar: the female–male gap in trade exposure. That scalar is therefore the object the empirical design should measure, and the model derives it rather than positing it. Second, the sign of the gap's effect is decided by a race between two intra-household forces: a bargaining force under which a wife's gains protect her, and a backlash force under which they provoke. Theory does not prejudge which force wins. Third, observed couples depart from the clean benchmark of equal, dual-earner spouses in two ways: they start from unequal positions, and in many of them the wife does not work for pay at all. Neither departure overturns the first two results; each instead yields an additional prediction the data can test.

The section is organized so that the cost of each result is visible. Sections 3.1 through 3.3, which deliver the first two results, impose no functional form on preferences. Section 3.4, which delivers the third, imposes one, and every statement that depends on it says so.

3.1 The couple

Consider a couple in municipality c. The husband works and earns the wage of his sector, w_m. The wife has one unit of time. She either works, and earns the wage $w_{s,f}$ of her sector, or she stays home and produces a household good worth $\omega_f > 0$.Writing $e_f \in \{0, 1\}$ for the wife's labor-force participation status, her command over resources is

$$w_f \;=\; e_f\, w_{s,f} \;+\; (1 - e_f)\, \omega_f. \tag{1}$$

Thus, $w_f > 0$ for every wife. A wife with no market earnings does not have zero potential earnings: her time is valued at ω_f, the shadow value of home production, which gives her a well-defined position in the household bargaining problem. This feature allows us to assign a well-defined bargaining weight to women who do not participate in the labor market. It also has an implication for exposure to the shocks we study. Because sectoral tariffs affect couples only through market earnings, wives who do not participate in the labor market are insulated from these shocks in our framework. For simplicity, we treat e_f as exogenous.

Both spouses value their own consumption c_g, $g \in \{m, f\}$, and the wife is harmed by the violence $v \geq 0$ the husband may inflict. Let $\rho \equiv w_f/w_m$ denote the wife's wage relative to her partner's. Preferences are

$$U_f(c_f, v) = u(c_f) - \phi_f(v), \tag{2}$$

$$U_m(c_m, v; \rho) = u(c_m) + \psi(v)\, h(\rho) - \kappa(v). \tag{3}$$

Violence enters the husband's payoff twice. It is directly costly through κ, and it yields a status return $\psi(v)\, h(\rho)$ that rises with the wife's relative wage ρ. The cost κ collects whatever makes violence directly unpleasant for him: guilt, effort, and the risk of social or legal sanction. The status term is the identity mechanism of the relative-income literature (Bertrand et al., 2015; Tur-Prats, 2021): a wife who gains ground on her husband threatens a norm he is invested in, and violence is one way he reasserts it. This term carries the *backlash* force. We impose no functional form on ψ or h: any specific choice would build in which of the two intra-household forces wins, and that race is what the data are meant to decide.

Assumption 1 (Regularity)**.** u is C^3, strictly increasing and strictly concave; ϕ_f and κ are C^2 and convex with $\phi_f(0) = \kappa(0) = 0$, $\phi_f' > 0$ and $\kappa' \geq 0$; ψ is C^2 and concave with $\psi(0) = 0$ and $\psi' > 0$; h is C^1 and positive with $h' > 0$. Outside options $\bar{U}_g(w_g)$ are increasing in own wage and independent of v. The husband's first-stage objective is strictly concave, his optimum is interior with $v^* > 0$, and both spouses' marital surpluses are strictly positive at the solution. Finally, writing

$$A(v, \rho) \;\equiv\; \psi'(v)h(\rho) - \kappa'(v) \tag{4}$$

for the husband's direct marginal return to violence, $A \geq 0$ on the region considered.

The couple decides in two stages, by backward induction. In the second stage, taking v as given, the spouses divide household income $c_m + c_f = w_m + w_f$ by generalized Nash bargaining, with disagreement equal to dissolution and with the wife's Nash weight equal to her *income share*,

$$\alpha \;=\; \frac{w_f}{w_m + w_f}. \tag{5}$$

Write S_g for the surplus spouse g draws from the marriage over dissolution, so that S_m carries the husband's consumption and his return to violence and S_f carries the wife's consumption net of the harm she suffers. In the first stage the husband chooses v, anticipating how the second-stage bargain will respond.

Three modeling choices deserve to be named rather than buried. Making the Nash weight the income share (5) lets earnings act twice — through the weight and through the outside options — which is the standard device for turning income into bargaining power (Aizer, 2010; Anderberg and Rainer, 2013). Giving the husband the first move lets him commit to a level of violence before the bargain is struck; were the order reversed, violence would carry no bargaining consequence at all. And the bargain is over utility that is not freely transferable across spouses, which is what gives violence a price at all.

One word on the last condition in Assumption 1, which is weaker than it looks. At any interior first-stage optimum $A > 0$ holds automatically, because the first-order condition (6) below equates A to the marginal consumption cost of violence, which is strictly positive. The condition therefore disciplines only the off-equilibrium paths along which the comparative statics are computed, where A is not pinned down by optimization.

3.2 Two forces on violence

Violence costs the husband consumption. The direct returns run the other way, which is why the mechanism needs stating precisely: violence harms the wife directly, through $\phi_f(v)$, while the husband's own direct return is non-negative, since $\partial S_m/\partial v = A \geq 0$ holding the split of income fixed. What costs him is not the violence but the transfer the bargain then imposes. Because violence shrinks the wife's surplus and not his, the Nash split compensates her on the consumption margin, and his own consumption falls as he grows more violent.

Proposition 1 (Violence is costly to the husband)**.** *Under Assumption 1, on the interior region the second-stage Nash allocation satisfies $\partial c_m^*/\partial v < 0$, and the husband's first-stage optimum is characterized by*

$$u'(c_m^*)\,\frac{\partial c_m^*}{\partial v} \;+\; A(v^*, \rho) \;=\; 0. \tag{6}$$

The consumption cost is a non-transferable-utility phenomenon: under transferable utility c_m^* would not vary with v, the first term of (6) would vanish, and this force with it. So violence is not

a device for extracting resources — the bargain makes the husband pay for it in consumption. It is a good he wants for its own sake, at a price the marriage sets.

Now let the trade shock move the two wages. Write $v^*_{w_g} \equiv \partial v^*/\partial w_g$ for the response of equilibrium violence to each spouse's wage, and define the two objects that organize everything below:

$$\underbrace{\mathcal{N} \;\equiv\; v^*_{w_f} + v^*_{w_m}}_{\text{aggregate-income force}}, \qquad \underbrace{\mathcal{R} \;\equiv\; v^*_{w_f} - v^*_{w_m}}_{\text{reallocation force}}. \tag{7}$$

The force $\mathcal{N}$ is the response to a rise in *both* wages, a local boom that lifts men and women alike. The force $\mathcal{R}$ is the response to a shift in her wage *relative* to his, holding the total fixed. The distinction is the spine of the paper: it is $\mathcal{R}$, the response to a change in relative standing, that the design is built to isolate.

The reallocation force is a horse race. Raising the wife's wage relative to her husband's does two opposing things. It raises her income share (5) and her outside option, strengthening her in the bargain and making his violence more expensive through Proposition 1 — the *bargaining* force. And it raises $h(\rho)$, his return to reasserting status — the *backlash* force. That the first of these is protective is a property we state and carry explicitly rather than derive in general.

Assumption 2 (The bargaining channel is protective)**.** $B_m - B_f > 0$, where B_g denotes the bargaining component of H_{vw_g} defined in Appendix B.2: a transfer toward the wife lowers violence through the consumption cost by more than the husband's own gain raises it.

Assumption 2 is not free-standing conjecture. Appendix B.3 shows that in the logarithmic specification of Section 3.4, evaluated at a baseline where the two wages are equal and the spouses draw equal surplus, it is a theorem, and gives B_m and B_f in closed form. Away from that point we maintain it, as is standard in the bargaining literature on violence (Tauchen et al., 1991; Farmer and Tiefenthaler, 1996; Bloch and Rao, 2002).

Proposition 2 (The horse race)**.** *Under Assumption 1, the reallocation force is the difference between the backlash and bargaining channels,*

$$\operatorname{sgn}\mathcal{R} \;=\; \operatorname{sgn}\Big[\, \underbrace{(1+\rho)\,\psi'(v^*)\,h'(\rho)/w_m}_{\textit{backlash}} \;-\; \underbrace{(B_m - B_f)}_{\textit{bargaining}} \,\Big]. \tag{8}$$

Both signs are attainable, but not by adjusting the two channels independently, because h enters the husband's surplus and its slope therefore appears in the bargaining term $B_m - B_f$ as well as in the backlash term. One feature keeps the comparison tractable: at fixed wages, the couple's equilibrium depends on h only through its *level* at the baseline ρ, not through its slope. Raising the slope at the baseline therefore changes neither v^* nor any quantity evaluated at it, while it makes the bracket in (8) an explicit affine function of that slope. In the logarithmic benchmark of Section 3.4, Appendix B.3 uses this to sign $\mathcal{R}$: it is negative when status sensitivity is weak and, under a stated

condition on the remaining primitives, positive above an explicit threshold. If backlash dominates, $\mathcal{R} > 0$; if bargaining dominates, $\mathcal{R} < 0$.

This is the model's central point. *Theory cannot say which force wins.* The direction of the effect of relative income on violence is not a prediction to be confirmed but a property of the primitives, and the sign in the data is what reveals it.

3.3 From couples to municipalities

We do not observe the sectors a couple works in; we observe the municipality it lives in. The shock reaches a couple as a pair of wage changes, one per spouse, set by those sectors. Let $\pi_{g,s,c}$ be the pre-period share of gender-g workers in municipality c attached to sector s, and let sector s receive a wage shock Δ_s, an absolute change in the wage of the workers attached to it. We connect the shock to the tariffs by a pass-through: for a sector with tariff change τ_s and trade dependence r_s,

$$\Delta_s \;=\; \lambda\, r_s\, \tau_s. \tag{9}$$

The pass-through λ is common across sectors and, crucially, *gender-common within a sector*: the model attributes any gendered incidence of the shock to *where* women work, not to a gender-specific pass-through inside a sector, and it is that restriction that makes a gendered shift-share the right object. Section 3.5 lets λ differ across the two tariff legs, which is a statement about where each shock lands and not about gender.

Equation (9) is what makes a local exposure measure a model object. The average shock to gender g in municipality c is $\sum_s \pi_{g,s,c}\Delta_s = \lambda \sum_s \pi_{g,s,c} r_s \tau_s$, so it is proportional, with factor λ, to the gendered shift-share of tariffs onto pre-period employment shares that the empirical section constructs. We therefore distinguish the two throughout: $\bar{A}_c$ and $\mathcal{G}_c$ below are built on the wage shocks Δ_s, while the constructed regressors carry $r_s\tau_s$ in their place and differ from them only by λ.

Two further premises do the aggregation. Couples are grouped into employment cells $j \in \{E, N\}$ — earner or non-earner, according to e_f — within which every couple starts from a common point, so that the wage responses $v^*_{w_m,j}, v^*_{w_f,j}$ are constants across the cell. And the two cells face the same average shock through the husband's wage — as they do when a husband's sector is unrelated to whether his wife works. Write $p \in (0, 1]$ for the share of earner couples.

Proposition 3 (Two-force decomposition and the exposure gap)**.** *Under Assumption 1, cell-homogeneous responses, and equal average male shocks across the two employment cells, define the municipality's* average earnings change *and its* female–male exposure gap,

$$\bar{A}_c \equiv \tfrac{1}{2}\sum_s \big(\pi_{f,s,c} + \pi_{m,s,c}\big)\Delta_s, \qquad \mathcal{G}_c \equiv \sum_s \big(\pi_{f,s,c} - \pi_{m,s,c}\big)\Delta_s. \tag{10}$$

Then, to first order in the size of the shock,

$$\Delta\text{IPV}_c \;=\; \mathcal{N}^* \cdot \bar{A}_c \;+\; \tfrac{1}{2}\,\mathcal{R}^* \cdot \mathcal{G}_c, \tag{11}$$

where the participation-weighted forces are $\mathcal{N}^* = a_f + a_m$ and $\mathcal{R}^* = a_f - a_m$, with $a_f = p\, v^*_{w_f,E}$ and $a_m = p\, v^*_{w_m,E} + (1-p) v^*_{w_m,N}$: the female channel carries weight p because only earner wives receive it, the male channel weight one. At $p = 1$, $\mathcal{N}^*$ and $\mathcal{R}^*$ reduce to $\mathcal{N}$ and $\mathcal{R}$ of (7). No functional form is used.

Read v^* as a latent propensity to violence and ΔIPV_c as the change in the probability of victimization, so that (11) is the population counterpart of a linear-probability equation in the two constructed indices. It involves no approximation beyond the first-order linearization, given its two premises. Two things about it deserve emphasis.

First, to first order the gap is a sufficient statistic for the reallocation channel. All of the sectoral detail — which sectors were tariffed, by how much, and how men and women sort across them — enters the reallocation term through the single scalar $\mathcal{G}_c$. And $\mathcal{G}_c$ is a *signed* shift-share, the inner product of the gendered exposure-gap vector $(\pi_{f,s,c} - \pi_{m,s,c})_s$ with the shock vector $(\Delta_s)_s$. A booming sector raises $\mathcal{G}_c$ where women are over-represented and lowers it where men are. Because $\mathcal{G}_c$ decomposes additively by sector, one can read off which sectors drive a given municipality's gap.

Second, the sign of the gap's coefficient selects the regime. Since the constructed gap differs from $\mathcal{G}_c$ by the factor λ, the coefficient on it is $\frac{1}{2}\mathcal{R}^*\lambda$. Note that $\mathcal{R}^*$ aggregates the two cells, while Proposition 2 signs one couple's response, so reading the aggregate as the couple-level race requires the two cells to resolve that race the same way.

Corollary 1 (Sign selection)**.** *Let D denote the coefficient on the constructed exposure gap in (11), so that $D = \frac{1}{2}\mathcal{R}^*\lambda$. Then, if the two employment cells respond with a common sign, by Proposition 2, for a leg with $\lambda > 0$,*

$$D > 0 \iff \textit{backlash dominates}, \qquad D < 0 \iff \textit{bargaining dominates},$$

and the two implications reverse for a leg with $\lambda < 0$.

Estimating the sign of D therefore does not test a signed prediction; it selects which intra-household force governs the couple's response. Corollary 1 does not require the aggregate-income force to be signed, or even known — only that $\bar{A}_c$ be carried in the equation rather than dropped from it. It holds for any felicity function satisfying Assumptions 1 and 2, given the common-sign condition above.

Earner and pooled couples. The two employment cells enter (11) as a weighted average of two different responses, not as a scaling of one. Writing $\mathcal{R}_E \equiv v^*_{w_f,E} - v^*_{w_m,E}$ for the reallocation force among earner couples, and $\mathcal{R}_N \equiv -v^*_{w_m,N}$ for what a non-earner couple contributes through her husband's wage alone,

$$\mathcal{R}^* \;=\; p\,\mathcal{R}_E \;+\; (1-p)\,\mathcal{R}_N, \tag{12}$$

an identity in the weights, given (11). A non-earner couple is not inert: the shock still moves her husband's wage, and with it the couple's relative position. It therefore contributes to the reallocation force, and (12) says with what weight.

Where functional form enters. Nothing to this point restricts preferences beyond Assumptions 1 and 2: the couple problem, both forces, the horse race, the decomposition with participation, and the sign selection of Corollary 1 hold for general felicity and general outside options. What the general model cannot supply is the coefficient on the level term, and that is where a functional form becomes unavoidable.

3.4 Unequal baselines and non-participating wives

What multiplies $\bar{A}_c$ in (11) is the aggregate-income force, and nothing so far has pinned it down. Doing so requires knowing how violence responds when *both* wages rise, which is a question about the curvature of preferences rather than about the shock. One restriction answers it, and it is the only functional-form restriction the section imposes.

Under logarithmic felicity with correspondingly logarithmic outside options, equilibrium violence depends on the two wages only through their *ratio*: doubling both changes nothing.

Lemma 1 (Scale invariance)**.** *If* $u(c) = \log c$ *and* $\bar{U}_g(w_g) = \log w_g - d_g$*, then* $v^*(t\,w_m, t\,w_f) = v^*(w_m, w_f)$ *for all* $t > 0$*; equilibrium violence is homogeneous of degree zero in the wage pair.*

The logarithm is required in *both* the felicity function and the outside option, and within the constant relative risk aversion family it is the knife-edge case that delivers ratio-dependence. Scaling both wages by t shifts the husband's objective by $\log t$, an *additive* constant that leaves his optimum untouched because the violence terms carry no scale. For any other member of that family the shift is *multiplicative*, $t^{1-\sigma}$, which re-weights the consumption terms against the violence terms and moves v^* with the scale of wages. Assuming the logarithm is assuming that only relative wages matter.

Euler's theorem, applied to the degree-zero function v^* at the couple's starting point, then forces $w_m^0 v^*_{w_m} + w_f^0 v^*_{w_f} = 0$, which ties the two forces together.

A pre-existing gap. Suppose a couple starts unequal, with pre-shock relative wage $\rho^0 = w_f^0/w_m^0 < 1$.

Proposition 4 (A gap switches the aggregate-income force on)**.** *Under Assumption 1 and the specification of Lemma 1, at a pre-shock gap* ρ^0*,*

$$\mathcal{N} \;=\; \chi(\rho^0)\,\mathcal{R}, \qquad \chi(\rho) \equiv \frac{1-\rho}{1+\rho}, \tag{13}$$

an exact identity for the wage derivatives. Hence $\mathcal{N} = 0$ at parity $\rho^0 = 1$; $\mathcal{N} \neq 0$ for any $\rho^0 \neq 1$ with $\mathcal{R} \neq 0$, and is first-order in the gap $1 - \rho^0$, so small when the gap is small; and $\operatorname{sgn}\mathcal{N} = \operatorname{sgn}\mathcal{R}$ for $\rho^0 < 1$.

The benchmark $\mathcal{N} = 0$ is thus scale invariance evaluated at a symmetric point, and it fails as soon as couples start unequal. Under log only relative wages move violence, and when a couple starts unequal a shock that raises both wages by the same *absolute* amount tilts their *ratio* toward parity — and the ratio is what the husband's status return responds to. A pre-existing gender gap therefore reactivates the aggregate-income force under the very utility function that silences it at parity, in exact proportion $\chi(\rho^0)$ to the gap.

Non-participation. The two cells of Proposition 3 start from different points: earner couples at the observed earnings ratio ρ^0_E, non-earner couples at $\rho^0_N = \omega_f / w^0_m$, the value of her time at home relative to his wage. Applying Lemma 1 within each cell gives a compact statement of the two forces' ratio.

Proposition 5 (Participation as an effective relative wage)**.** *Take $\mathcal{N}^*, \mathcal{R}^*$ from Proposition 3 and apply the specification of Lemma 1 within each employment cell — so that for non-earner couples the wife's outside option is $\bar{U}_f(\omega_f) = \log \omega_f - d_f$. Suppose further that the horse race resolves with a common sign across the two cells. Then*

$$\frac{\mathcal{N}^*}{\mathcal{R}^*} \;=\; \chi(\tilde{\rho}), \qquad \tilde{\rho} \;=\; \rho^0_E + \zeta, \qquad \zeta \;=\; \frac{(1-p)\,\rho^0_N\, v^*_{w_f,N}}{p\, v^*_{w_f,E}} \;\geq\; 0. \tag{14}$$

Hence, when $\mathcal{R}^* > 0$,

$$\mathcal{N}^* < 0 \iff \tilde{\rho} > 1. \tag{15}$$

At $p = 1$ the weighted forces collapse to $\mathcal{N}, \mathcal{R}$ of (7) with $\tilde{\rho} = \rho^0_E$, and at $p = 1$, $\rho^0_E = 1$ to $\mathcal{N} = 0$.

In cell N the wife has no market wage, so $v^*_{w_f,N}$ in (14) denotes the derivative with respect to her home productivity, $\partial v^*/\partial \omega_f$ evaluated in the non-earner cell (Appendix B.5); the notation keeps the two cells symmetric. The hypothesis on the non-earner wife's outside option deserves a word, since it is the one place the logarithm is applied to home production rather than to a wage. It reads ω_f as portable: what she produces at home is also what she would command outside the marriage. That is the natural counterpart of treating ω_f as the shadow price of her time, but it is an assumption, and Section 3.6 returns to it.

The two departures pull against each other. The earnings gap on its own would make $\mathcal{N}^*$ positive, since $\chi(\rho^0_E) > 0$ for $\rho^0_E < 1$ and $\mathcal{R}^* > 0$. It is the non-earner couples that can flip the sign: they raise the effective relative wage $\tilde{\rho}$ above the earner couples' own ρ^0_E, because a wife at home still has resources ω_f at stake and so her couple responds to the husband's wage. Only if that contribution is large enough to push $\tilde{\rho}$ past parity does $\mathcal{N}^*$ turn negative. Which side of parity $\tilde{\rho}$ falls on is not something theory settles; (15) says what the answer would mean.

3.5 What the model asks of the data

The model motivates the exposure specification and organizes the interpretation of its coefficients. We include both the female–male gap and average exposure for each tariff leg, compare their signs, and ask whether conditioning on the woman's own income absorbs the gap coefficient. The model also yields implications for earner groups and sectors with balanced employment shares. Those implications clarify the mechanism but are not separate tests in the empirical analysis. Statements that depend on the logarithmic specification refer to Section 3.4.

The gap is the regressor, and its sign is the question. By Proposition 3 the reallocation channel loads on the female–male exposure gap and nothing finer, and by Corollary 1 the sign of its coefficient selects backlash from bargaining. The difference-in-differences design that follows is built to estimate exactly this coefficient. Neither statement rests on a functional form.

The level of exposure belongs in the equation. Proposition 3 carries two constructed regressors, not one: the gap $\mathcal{G}_c$ and the average $\bar{A}_c$. They are built from the same two gendered exposures — $\bar{A}_c$ is their average where $\mathcal{G}_c$ is their difference — so carrying the level alongside the gap costs no additional data. A gap-only regression omits $\mathcal{N}^* \bar{A}_c$.

The model does not sign $\mathcal{N}^*$ outright. By Proposition 5 its sign is that of $1-\tilde{\rho}$ when $\mathcal{R}^* > 0$, and where the effective relative wage falls against parity turns on how responsive non-earner couples are, which theory does not settle. What the model delivers instead is a link: $\mathcal{N}^* = \chi(\tilde{\rho})\,\mathcal{R}^*$ ties the level's coefficient to the gap's through a single function of a single number. The two coefficients therefore share a sign when $\tilde{\rho} < 1$ and carry opposite signs when $\tilde{\rho} > 1$ — a restriction across coefficients that depends on no scaling of either exposure, and one that can be inspected only if both are reported. Given $\operatorname{sgn}\mathcal{N}^*$, the direction of the omitted-level bias in a gap-only regression follows from the sign of the covariance between the two indices (Appendix B.6). The level term is thus not a robustness check the design may include but a second force the theory says belongs. Table 1 reports both gap and average-exposure coefficients for each tariff leg. This restriction rests on Lemma 1.

The two tariff legs are one force. The design exploits two tariff shocks: a demand-diverting leg that raises exposed Mexican wages and a metals leg that falls on Mexican producers directly and lowers theirs. We take the two to carry opposite-signed pass-through, $\lambda_{\text{CN}} > 0 > \lambda_{\text{ME}}$ in (9) — a premise about where each shock lands, not something the design estimates. Both then act through the *same* reallocation force $\mathcal{R}^*$, so by Corollary 1 their gap coefficients D and D' satisfy

$$\operatorname{sgn} D' \;=\; -\operatorname{sgn} D.$$

A protective response on one leg forces a provocative response on the other. This is a signed cross-leg restriction that estimation does not impose and can therefore check; economically it says the same thing twice — where the trade war improved women's relative position violence moved one way, and where it damaged that position, violence moved the other. It rests on no functional form. Table 1 permits a comparison of the estimated signs. We do not report a separate joint test of the cross-leg restriction, and the labor-market evidence does not establish opposite wage pass-through for every household.

The earner comparison depends on non-earner responses. By (12) the pooled reallocation force is a participation-weighted average of the two cells' own forces, so on a leg with $\lambda > 0$, restricting attention to couples in which the wife works raises the estimated gap coefficient exactly when non-earner couples contribute the smaller of the two, $\mathcal{R}_N < \mathcal{R}_E$. Under the specification of Section 3.4 that condition reads $\rho_N^0 \, v^*_{w_f,N} < (1+\rho_E^0) \, v^*_{w_f,E}$, which in the regime $\mathcal{R}^* > 0$ holds whenever a non-earning wife's home production is worth no more than her husband's wage and her couple is no more responsive than an earner couple. Because this compares one coefficient across two samples, it is informative only if the gap is scaled identically in both — standardized on a common reference sample rather than on each estimation sample separately — and, strictly, only in the specification that carries $\bar{A}_c$: run gap-only, the two samples inherit different omitted-level biases, and their ordering picks up a covariance the model does not restrict (Appendix B.6). The prediction is imposed nowhere in estimation, and it cuts both ways: a *smaller* earner coefficient would not contradict the model. It would say that non-earner couples contribute more than earner ones, driven by the same primitive — a large $\rho_N^0 v^*_{w_f,N}$ — that raises the effective relative wage $\tilde{\rho}$ of (14). Where participation is low, that primitive carries $\tilde{\rho}$ past parity outright: in the regime $\mathcal{R}^* > 0$, at any participation rate below one half, a smaller earner coefficient implies $\tilde{\rho} > 1$ and hence a *negative* coefficient on the level in the pooled equation (Appendix B.5 gives the sharp threshold, (34)). The two things the model asks the data to report are therefore not independent in that range: whether the earner coefficient exceeds the pooled one, and the sign of the level coefficient. Only above that participation threshold can they come apart. Both the condition and the threshold rest on Lemma 1, applied cell by cell; the weighting (12) they are applied to does not. The observed-income sample in Table 3 excludes unresolved income reports and is not a direct comparison of these model-defined earner groups. We therefore do not use that table to test their ordering.

Own income does not fix relative position. Both intra-household forces respond to the couple's relative wage, and the shock moves both of its sides. Holding the wife's resources fixed in the model fixes her side of the ratio but not the ratio ρ: the husband's channel keeps moving, and the reallocation force with it. The gap coefficient should accordingly survive an own-income control in *either* regime — non-mediation is the model's expectation, not evidence against the bargaining channel. A mediation exercise speaks instead to a reading outside the model, in which violence tracks the woman's measured resources rather than the couple's relative position. Table 3 reports

the empirical conditioning exercise. Measured monthly income is not the model's wage offer, and conditioning on an outcome of the shock does not identify a causal mediation effect.

Balanced employment shares limit the raw gap. Equation (11) also clarifies the role of gender composition. Build the exposure gap on sectors that are close to gender-balanced in the municipality ($\pi_{f,s,c} \approx \pi_{m,s,c}$). Then that gap is close to zero however large the tariff shock, carrying tariff exposure but almost no reallocation content, and it should move violence little or not at all. This concerns the size of the raw exposure gap. It does not imply a zero coefficient after a small gap is divided by its own standard deviation. We therefore do not use a split into relatively balanced and imbalanced sectors as a separate falsification test.

3.6 Scope

A word first on what this section is for. It is a framework that motivates the empirical specification, not a structural model to be estimated. Its content is the set of regressors that belong in the estimating equation, the interpretation of the sign of each coefficient, and the restrictions the signs must satisfy across the two tariff legs and across subsamples. We do not attempt to recover the model's primitives from the estimated coefficients, and none of the sign claims above depends on the units in which the exposures are scaled.

The model is static and partial-equilibrium: wages are exogenous to the couple, the marriage is intact, and there is no separation, saving, or reallocation of labor across sectors. Violence is a single scalar, so the model speaks to the *level* of partner violence and not to its composition. Five limitations are worth stating explicitly.

First, participation is predetermined. The model does not rationalize the observed non-participation as a free choice — under free choice a wife would stay home only when home production is worth more than the wage she could earn, and too few women work for that to be the whole story. We therefore read ω_f as a summary of how much a non-earning wife has at stake, not as a measured shadow price to be inverted, and we do not use the model to interpret the participation margin itself. Second, and relatedly, Proposition 5 treats ω_f as portable into dissolution. A wife whose home production is worth less outside the marriage than inside it would have a weaker position than the model gives her, which would damp the non-earner contribution ζ without changing its sign. Third, the shares $\pi_{g,s,c}$ that build the exposure indices are measured on formal employment while the participation rate p is measured on all employment, so the earner group mixes women who receive the shock with women who do not; this attenuates $\mathcal{R}^*$ without touching its sign. Fourth, the model gives the husband a job. Couples in which he is not employed receive no shock through his channel, which by (11) attenuates both forces without reversing either. Fifth, Proposition 3 requires a husband's sector to be unrelated to whether his wife works. If the two are associated — if men in the sectors the tariffs reach are systematically more or less

likely to have an earning wife — the two cells no longer face the same average shock through the husband, and (11) acquires a further term in the covariance between his shock and e_f, of a sign the model does not restrict.

4 Data

We link women's reports of partner violence and economic circumstances to the industrial composition of female and male employment in their municipalities before the trade war. ENDIREH supplies the individual outcomes; IMSS employment records, product-level tariffs and international trade flows supply the local exposure measures. Administrative wage and offense records and a separate household income survey provide complementary evidence on economic conditions and violence.

4.1 ENDIREH: sample and individual outcomes

ENDIREH surveys women aged 15 and older about specific acts of violence and their relationship to the perpetrator (INEGI, 2016b, 2021a). Its partner module records physical, sexual, emotional and economic abuse. We use the 2016 and 2021 waves, which fall before and after the tariff increases. The two waves provide repeated cross sections of women in the same municipalities.

Our sample includes ever-partnered women in current or ended marriages, cohabiting unions and dating relationships. We use the survey's person weights and link individual respondents to municipality-level exposure. ENDIREH is designed to provide representative estimates nationally and by state; we pool respondents across municipalities rather than estimate municipality-specific prevalence.

For each type of violence, an indicator records whether the respondent reports at least one of the acts in that category. Any violence combines the four categories. We construct each outcome for the past year and for the history of the relationship, labeled lifetime in the exhibits. The past-year measure captures recent abuse; lifetime violence includes experiences before the tariff changes and can remain positive after abuse has ceased. Appendix A.1 gives the act definitions and reference periods.

We study municipalities with employment of both women and men in industries affected by each of the two tariff measures. This restriction selects 566 municipalities using prewar IMSS employment; 565 contain ENDIREH respondents. The pooled sample contains 156,267 ever-partnered women before outcome-specific restrictions. Table A1 reports their characteristics and outcomes. In the 2016 sample, about 6.4 percent report past-year physical partner violence and 18 percent report physical violence over the relationship. About 44 percent of women in the 2016 sample currently work.

The survey records women's current work status and work income, together with partner income and transfers. We use current work status to measure employment and convert reported earnings to monthly pesos using each respondent's reporting period. A reported zero is retained, including when the woman works. A blank earnings amount following an explicit nonwork response is also coded zero. Missing earnings for a working woman remain missing, as does a positive amount with an unknown reporting period.

We examine four measures of women's economic position: log monthly work income, monthly work income in levels estimated by PPML, current work status, and the woman's share of recorded income (Table 2). Log income uses positive earnings, while income in levels includes valid zeros. The income share divides own work income by own and partner work income plus transfers; it is defined for currently partnered women with all components observed and a positive total. Appendix A.2 documents the income construction and sample restrictions.

ENDIREH also asks about sadness or depression, suicidal ideation and suicide attempts attributed to problems with the partner. The questions follow the screening for partner violence. We measure joint reports of partner violence and its psychological consequences among all ever-partnered women in the sample. A confirmed skip after a negative violence screen contributes zero to this joint outcome; an unresolved response remains missing. These items record self-reported psychological consequences attributed to partner problems. The additional routing for suicide attempts and the resulting differences in outcome coverage are described in Appendix A.3.

4.2 Local employment and tariff exposure

IMSS records registered formal employment by sex, municipality and economic activity (Instituto Mexicano del Seguro Social, 2024). We average monthly employment over 2017 to measure each municipality's industrial composition before the trade war. Separate industry shares for women and men allow the same tariff change to generate different exposure for the two workforces. The denominator is all mapped formal employment of the relevant sex in the municipality, including employment in industries unaffected by tariffs. Thus exposure depends on how much local employment is in affected industries as well as which industries those are.

Product-level US tariff changes come from the files accompanying Fajgelbaum et al. (2020, 2024). We distinguish tariffs on Chinese goods from tariffs on Mexican goods and aggregate the maximum increases recorded in 2019 to three-digit industries. An industry's weight is the share of its Mexican exports sent to the United States in 2017, calculated from the Base pour l'Analyse du Commerce International (BACI) trade database (Gaulier and Zignago, 2010). This weight captures how strongly its exports were oriented toward the market imposing the tariffs. The 2019 maximum includes duties subsequently removed.

For women and men separately, local exposure sums each industry's employment share multiplied by its US export share and tariff increase. We then calculate the female-minus-male exposure

gap and the average of female and male exposure for each tariff measure. A positive gap means that the affected industries accounted for greater weighted exposure among women's jobs than men's. Average exposure measures the overall magnitude of exposure across the two workforces. Both enter the empirical specification in Section 5. We assign these measures of local employment opportunities to each respondent using her municipality's prewar employment structure. Appendix A.5 provides the industry concordances, aggregation and standardization.

4.3 Complementary wage, income and offense records

Monthly IMSS records trace formal wages throughout the tariff episode, from July 2016 through December 2021. Mean daily contribution pay divides the recorded wage bill by registered jobs with reported wages. We also compare wages and employment at the December 2016 and December 2021 endpoints. These records describe the formal jobs in a municipality; changes in average pay can reflect changes in who holds those jobs as well as wage adjustments. These municipality-level records complement the individual economic outcomes observed in ENDIREH.

The National Survey of Household Income and Expenditure (ENIGH) provides a broader measure of household resources through quarterly current income in 2016 and 2020 (INEGI, 2016a, 2020b). We calculate the household-weighted mean of log income within each municipality and wave among households with positive income. The change in that mean can be measured for 402 eligible municipalities observed in both waves. This comparison covers total household current income, while the ENDIREH outcomes distinguish women's earnings and their share of the recorded income components.

The Executive Secretariat of the National Public Security System (SESNSP) records family-violence offenses in investigation files (Secretariado Ejecutivo del Sistema Nacional de Seguridad Pública, 2025). We use annual municipal counts in 2015–2017 and 2019–2021, omitting the transition year 2018, and show annual estimates through 2024. These records capture incidents reaching the criminal-justice system and cover a broader set of victims and relationships than ENDIREH's partner questions. Changes in the counts can therefore reflect reporting and enforcement as well as changes in violence. Appendix A.7 details the coverage and construction of these administrative and household-income measures.

4.4 Pandemic exposure and concurrent policies

Municipalities experienced different health and employment shocks during the coronavirus disease 2019 (COVID-19) pandemic. We measure mortality using confirmed deaths in the health ministry's surveillance records through October 31, 2021, relative to population in the 2020 Census (Secretaría de Salud, 2021; INEGI, 2020a). National industry employment changes from January to December 2020, measured in IMSS records, are combined with prewar female and male employment shares to capture gender differences in exposure to the pandemic employment shock.

We also combine those shares with a broad industry classification based on the federal essential-activity rules to measure exposure to mandated closures (Secretaría de Salud, 2020).

The concurrent-policy controls use the municipalities listed in the northern-border fiscal decree, prewar IMSS wage bins and ENDIREH reports of Prospera receipt in 2016 (Presidencia de la República, 2018; Instituto Mexicano del Seguro Social, 2024; INEGI, 2016b). We also account for the municipality's 2017 apparel and textile employment share from IMSS. Appendix A.8 describes how these sources enter the additional-control comparison.

5 Empirical Strategy

We compare changes in partner violence between 2016 and 2021 across municipalities with different tariff exposure. Women's and men's employment was concentrated in different industries before the trade war, so the tariff increases generated different exposure for the two groups. Our main comparison asks whether violence changed more in municipalities where women were more exposed than men to US tariffs on Chinese goods, holding average exposure fixed. The regression also includes the gender gap and average exposure to US tariffs on Mexican goods, together with municipality and wave fixed effects and individual controls.

5.1 Gender differences in tariff exposure

For gender $g \in \{F, M\}$, municipality c, and tariff leg $d \in \{CN, ME\}$, exposure is given by

$$A_c^{g,d} = \sum_s E_{cs,2017}^g \, r_s \, \tau_s^d, \tag{16}$$

where $E_{cs,2017}^g$ is industry s's share of gender g's formal employment, measured using average employment in 2017. The share denominator includes employment in industries unaffected by tariffs. The sector tariff increase, τ_s^d, measures US tariffs on Chinese goods for the China leg and on Mexican goods for the Mexico leg. The two measures capture different changes in access to the US market: Chinese competitors face higher duties under the first, while Mexican exporters face higher duties under the second.

An industry's orientation toward the US market determines its weight in exposure. We use

$$r_s = \pi_s^{US} = \frac{X_{s,US}^{2017}}{X_{s,World}^{2017}}, \tag{17}$$

where $X_{s,US}^{2017}$ and $X_{s,World}^{2017}$ are Mexican exports in industry s to the United States and to all destinations, respectively. Employment shares and trade weights thus precede the tariff increases. The weight measures where exports go; the appendix considers alternative weights that measure exports relative to output or employment, as well as a specification with equal industry weights.

We summarize gender differences and common exposure by

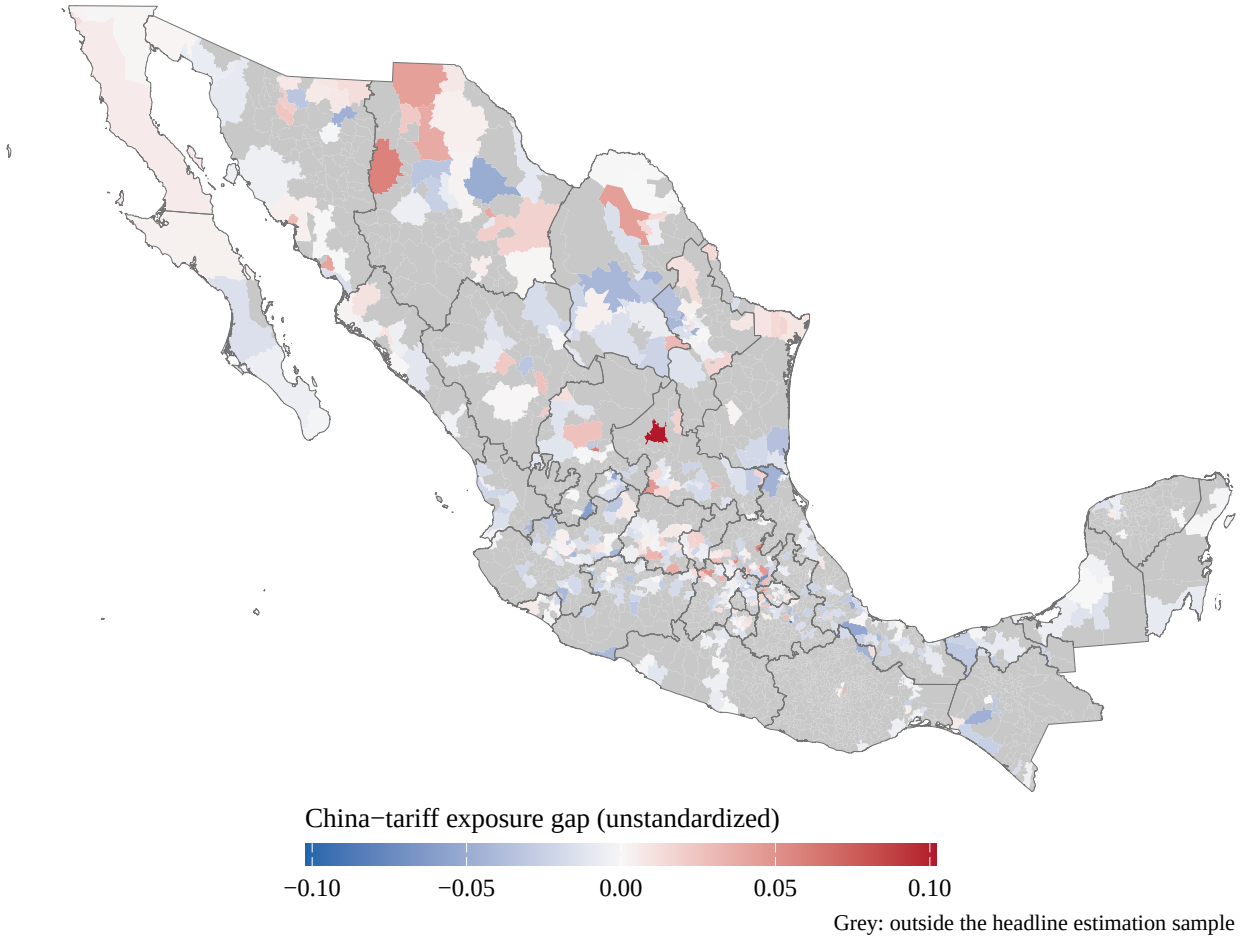
$$G_c^d = A_c^{F,d} - A_c^{M,d}, \tag{18}$$

$$\bar{A}_c^d = \tfrac{1}{2}\left(A_c^{F,d} + A_c^{M,d}\right). \tag{19}$$

At a given average exposure, a larger gap places more of the exposure on women's employment and less on men's. Including both indices allows violence to respond differently to this change in relative position and to a change shared by the two workforces, as in Proposition 3. Because the two tariff programs overlap in industry coverage, we include the gap and average for each leg jointly.

Each woman is assigned the female and male exposure measures for her municipality of residence. These measures capture tariff exposure through the local labor market, including the opportunities available to women who work outside tariffed industries or are not employed. ENDIREH records where respondents live but does not identify their industry of employment. Figure 1 maps the China-tariff exposure gap across municipalities in the estimation sample. Red municipalities have greater female exposure, while blue municipalities have greater male exposure. Both signs appear in northern and central Mexico, with differences across municipalities within the same state.

Figure 1: China-tariff exposure gap across municipalities



Notes: Colours show the unstandardized female-minus-male China-tariff exposure gap used in Table 1, constructed from 2017-average IMSS formal employment shares, sector tariff changes and BACI 2017 US export-destination weights. Red indicates greater female exposure and blue greater male exposure. The symmetric colour scale is centred on zero and is not clipped. The map covers the 565 municipalities with ever-partnered women in the ENDIREH 2016 and 2021 headline estimation sample; other municipalities are grey. Thin lines mark state boundaries. Boundaries are from the Global Administrative Areas (GADM) database, version 4.1, linked through the municipal crosswalk. The map describes exposure geography, not estimated effects on violence.

5.2 Estimating the exposure gradient

We form the gaps and averages in Equations (18) and (19) before standardizing each of the four indices. In the regressions below, G_c^d and $\bar{A}_c^d$ denote these standardized indices: each is centered and divided by its own standard deviation (SD). The reference population consists of ever-partnered ENDIREH respondent rows in the eligible municipalities, pooled across 2016 and 2021 before outcome-specific exclusions. Means and standard deviations are unweighted across these rows; estimation uses the survey's person weights. Keeping the reference scales fixed allows coefficients to be compared when missing income or other outcome restrictions change the estimation sample.

Appendix A.5 documents the scales and the separate normalization used for alternative exposure constructions.

For woman i in municipality c and survey wave t, the linear-probability model, estimated by ordinary least squares (OLS), is given by

$$\begin{aligned} Y_{ict} = {} & \beta_{CN}\,\text{Post}_t G_c^{CN} + \beta_{ME}\,\text{Post}_t G_c^{ME} \\ & + \theta_{CN}\,\text{Post}_t \bar{A}_c^{CN} + \theta_{ME}\,\text{Post}_t \bar{A}_c^{ME} \\ & + X_{ict}'\gamma + \mu_c + \delta_t + \varepsilon_{ict}, \end{aligned} \tag{20}$$

where Y_{ict} indicates reported partner violence, Post_t equals one in 2021, and X_{ict} contains age and educational attainment. Municipality fixed effects, μ_c, account for time-invariant differences across municipalities. The exposure indices are fixed at their prewar values, so their coefficients are estimated through interactions with the post indicator. Wave fixed effects, δ_t, absorb changes common to the sample. The interaction coefficients measure how the change in violence varies with prewar exposure after accounting for the other regressors.

The coefficient β_{CN} is the difference in the change in victimization probability associated with a one-standard-deviation larger China gap, holding the Mexico gap and both averages fixed. We report binary-outcome coefficients multiplied by 100, in percentage points. This is a continuous comparison among exposed municipalities, estimated as a linear relationship. The headline analysis covers physical violence and the other domains in Table 1; the discussion leads with past-year physical violence. Lifetime reports cover the history of the relationship and can include abuse before the tariffs, so changes in that outcome also reflect accumulated experiences, recall and the composition of respondents.

The model gives the gap and average coefficients distinct economic roles. Under its pass-through and aggregation assumptions, the gap coefficient reflects the response to women's relative economic position, while the average coefficient reflects the response to average economic resources. Standardization changes their magnitudes, but not their signs. Reading a gap coefficient as evidence about bargaining or backlash additionally requires the pass-through sign and common-sign condition in Corollary 1. The regression estimates exposure responses; it does not estimate the causal effect of a woman's realized income on violence.

Our sample comprises ever-partnered women in the 566 municipalities meeting the prewar employment criterion described in Section 4; 565 have survey observations. Each eligible municipality has employment of both sexes in industries relevant to each tariff leg. The comparison therefore concerns this exposed population, with the survey weights preserving women's relative population weights within it. Mexico City is excluded because IMSS employment cannot be assigned to its individual municipalities. The appendix includes it using citywide exposure and a common cluster for its constituent survey municipalities (Table A6).

We cluster standard errors by municipality, allowing errors to be correlated among women in

the same municipality and across waves. Municipalities with similar industry composition may also share disturbances, the dependence emphasized by Adão et al. (2019). The headline Results footnote therefore reports metro-zone clustering and the null-imposed exposure-robust procedure of Adão, Kolesár and Morales (AKM0) for the same China-gap coefficients.

For the economic outcomes defined in Section 4, we retain both exposure gaps and both averages, age and education controls, and municipality and wave fixed effects. We estimate the log-income, employment and income-share regressions by OLS and the income-level regression by PPML. For income in levels, exponentiating an exposure coefficient gives the ratio of conditional mean income associated with a one-SD increase in that exposure, holding the other regressors fixed.

5.3 Identification and supporting comparisons

Recent methodological work distinguishes two sources of identification in shift-share designs: exogenous exposure shares and exogenous shocks. Goldsmith-Pinkham et al. (2020) develop the exposure-share interpretation, while Borusyak et al. (2022) establish conditions for identification from quasi-random shocks even when shares are endogenous. Borusyak et al. (2025) synthesize these approaches and the assumptions each requires. Our identification argument centers on the tariff shifts; prewar employment shares determine how those shifts reach female and male workers in each municipality.

The US tariff increases originated in trade-policy disputes with China and Mexico and changed the competitive conditions facing Mexican producers. Tariffs on Chinese goods raised the cost of competing suppliers in the US market, while tariffs on Mexican goods raised the cost of Mexican exports themselves. The same industry-level policy changes generated different local exposure because women and men worked in different industries across municipalities. We measure that exposure using employment shares and export-destination weights fixed before the tariff increases. The regression compares changes in violence across these pre-existing patterns of exposure, including both tariff programs and separating each gender gap from average exposure.

The identifying assumption is that the tariff-induced variation in exposure is unrelated to other determinants of changes in partner violence, conditional on the included regressors. Foreign policy decisions provide the rationale for this assumption, but their industry targeting can still coincide with other sector-specific shocks. Prewar shares fix the allocation of exposure before the policy change; they can also reflect industrial specialization associated with different subsequent trends. In the two-period regression, the required conditional parallel-trends restriction is that, absent the tariff episode, changes in violence would not vary systematically with the China gap after accounting for the Mexico gap, both averages and the controls. Corresponding restrictions apply to the other exposure coefficients.

The robustness checks address these possible links between exposure and other changes in lo-

cal conditions. State-by-wave fixed effects absorb shocks common to municipalities within a state (Table A5). Additional post interactions account for pandemic mortality, the sector employment shock, average closure exposure and its gender gap, northern-border policy, minimum-wage exposure, Prospera coverage and apparel/textile employment (Table A11). We compare the adjusted regression with a baseline fitted to the same sample. Pandemic measures can themselves respond to economic conditions after the tariffs, so this comparison conditions on potentially post-treatment variables.

The 2011 ENDIREH wave provides an earlier comparison for assessing pre-tariff differences. We harmonize eight physical-violence acts and restrict all three waves to currently or previously married or cohabiting women, using 2016 as the reference year and including state-by-wave fixed effects (Figure A1). Differences in interview selection and weighting, together with imprecise estimates, limit what this comparison establishes about trends in the main sample. Appendix A.6 describes the harmonization.

Sector-level checks examine whether the estimates depend on particular industries or tariff assignments. We omit one sector at a time, exclude overlapping tariff sectors from China exposure and compare the estimate with a reference distribution obtained by reassigning China tariffs across sectors (Figures A4 and A5 and Table A7). The overlap check retains the original employment-share denominators, Mexico exposures and estimation sample. Reassigned tariffs provide a sensitivity diagnostic; actual tariffs were not randomly allocated across industries. Metropolitan-zone clustering and AKM0 provide alternative inference, reported in the headline Results footnote.

We also vary how exposure and the estimation sample are constructed. The comparisons include alternative trade weights (equal weights, exports relative to output and exports per worker), employment-share vintages, 2010 Census formal-worker shares, tariff shocks transformed using the inverse hyperbolic sine (IHS), restriction to currently partnered women and inclusion of Mexico City (Tables A6 and A8 to A10). The Census alternative changes source and vintage together. Exposure-quintile estimates examine departures from a linear gradient (Figure A6). For economic outcomes, own-sex exposure specifications and direct estimates of the female–male wage difference complement the gap-and-average specification (Table A2 and Figures A2 and A3).

Reported violence depends on disclosure as well as behavior. Violence severity, violence in other settings and reports of childhood violence provide complementary outcome and reporting comparisons, with different reference periods and populations (Tables A12 to A14). Administrative family-violence offenses provide evidence from a separate reporting system and a longer annual series (Table 5 and Figure A7). These sources capture different parts of underlying violence through their respective reporting and disclosure processes.

6 Results

6.1 Relative exposure and partner violence

Physical partner violence increases more between 2016 and 2021 in municipalities where women are more exposed than men to US tariffs on Chinese goods. Holding the Mexico gap and both average exposures fixed, a one-SD larger China-tariff exposure gap predicts a 0.46-percentage-point larger change in past-year physical violence (Table 1). This difference is about 7.2 percent of the 2016 prevalence of 6.4 percent. The lifetime estimate is 0.84 percentage points, or 4.8 percent of its baseline prevalence. The past-year and lifetime estimates are statistically significant at the ten and five percent levels, respectively.[2]

The Mexico-tariff exposure gap has the opposite relationship with physical violence. A one-SD larger gap predicts a 0.38-percentage-point smaller change in past-year violence and a 0.55-percentage-point smaller change in lifetime violence, both statistically significant at the ten percent level. The past-year estimate is about 6.0 percent of baseline prevalence. The two tariff programs place opposite pressures on Mexican producers: tariffs on Chinese competitors can expand demand for Mexican goods, while tariffs on Mexico restrict it. Under the pass-through and common-sign assumptions of Corollary 1, the positive China-gap and negative Mexico-gap coefficients are consistent with backlash outweighing the protective bargaining force. This interpretation concerns the response to relative economic opportunities; the coefficients do not identify the effect of a woman's own realized income.

Greater average exposure to the China tariffs accompanies a smaller increase in past-year physical violence. The estimate is -0.44 percentage points per SD, statistically significant at the ten percent level, whereas the lifetime estimate and both average Mexico-exposure estimates for physical violence are imprecise. Thus relative and average exposure have different relationships with recent physical violence: greater exposure among women relative to men accompanies more violence, while greater exposure shared across the two workforces accompanies less. The model separates these forces and leaves the sign of the average-exposure response unrestricted, in part because couples with a non-earning woman also respond through the husband's economic circumstances.

The evidence is less precise for broader measures of partner violence. The China-gap estimates for any violence are positive—0.68 percentage points for the past year and 0.84 for lifetime violence—but neither is statistically significant at the ten percent level. Its estimates for sexual, emotional and economic violence are also positive and individually imprecise. The Mexico gap predicts more past-year sexual violence, by 0.24 percentage points per SD, statistically significant at the ten percent level, alongside the decline in physical violence.

The clearest violence response is in physical abuse: a larger China-tariff exposure gap accompa-

[2]For the China-tariff exposure-gap coefficient, clustering by metropolitan zone gives p-values of 0.089 and 0.009 for past-year and lifetime physical violence, respectively; AKM0 shock-level inference gives 0.003 and less than 0.001. Both calculations retain the specification and sample of Table 1.

nies more violence, while a larger Mexico-tariff exposure gap accompanies less. Given the opposite demand pressures of the two tariff programs, these signs are consistent with violence rising when economic opportunities improve for women relative to men. Estimates for other forms of abuse are less precise.

Table 1: Tariff exposure and partner violence

	(1) Any violence	(2) Physical	(3) Sexual	(4) Emotional	(5) Economic
Panel A: Past 12 months					
Post × China-tariff exposure gap	0.68	0.46*	0.05	0.78	0.32
	(0.51)	(0.24)	(0.13)	(0.51)	(0.35)
Post × Mexico-tariff exposure gap	0.48	−0.38*	0.24*	0.53	0.46
	(0.53)	(0.20)	(0.14)	(0.58)	(0.31)
Post × Average China-tariff exposure	−0.36	−0.44*	0.11	−0.47	−0.25
	(0.57)	(0.26)	(0.16)	(0.55)	(0.34)
Post × Average Mexico-tariff exposure	−0.65	0.24	−0.14	−0.41	−0.70**
	(0.46)	(0.24)	(0.18)	(0.42)	(0.29)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267	156,267
Mean of dep. var. (2016, %)	26.0	6.4	2.2	23.6	10.2
Panel B: Lifetime					
Post × China-tariff exposure gap	0.84	0.84**	0.28	1.16	0.71
	(0.68)	(0.38)	(0.22)	(0.72)	(0.48)
Post × Mexico-tariff exposure gap	0.72	−0.55*	0.21	0.72	0.00
	(0.73)	(0.31)	(0.23)	(0.77)	(0.42)
Post × Average China-tariff exposure	0.74	−0.39	0.23	0.56	−0.07
	(0.81)	(0.43)	(0.29)	(0.83)	(0.56)
Post × Average Mexico-tariff exposure	−1.14*	−0.24	−0.46	−0.98	−0.48
	(0.65)	(0.40)	(0.29)	(0.65)	(0.51)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267	156,267
Mean of dep. var. (2016, %)	44.0	17.6	6.5	40.3	21.0

Notes: Each column in each panel reports a difference-in-differences regression for the indicated type of partner violence. Any violence indicates at least one of the four listed types within the panel's reference period. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Of the 566 eligible municipalities, 565 have survey observations. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

6.2 Women's income and formal wages

Women's mean own income rises with the China-tariff exposure gap, although the gains are less precisely estimated among women with positive earnings. The PPML estimate in Table 2, which includes valid zeros, implies 5.3 percent higher conditional mean monthly income per SD and is statistically significant at the five percent level. The log-income estimate among positive-income respondents implies an increase of 1.0 percent and is imprecise. Comparing the two estimates involves both a change in the sample and a change in the conditional mean being estimated. On the same positive-income sample, PPML yields a smaller increase of 2.4 percent, also imprecise. The mean-income result is therefore partly sensitive to the inclusion of women with zero earnings.[3]

Employment and the woman's share of recorded income show no precisely estimated response to the China gap. These outcomes help distinguish higher mean earnings from entry into work or a larger share of household resources. The Mexico gap predicts 0.78 percentage points less employment per SD, statistically significant at the ten percent level, while its income and income-share estimates are imprecise. The survey evidence thus points to higher mean own income under the China gap, with limited evidence on whether women gain resources relative to their partners. The own-sex exposure comparison appears in Table A2.

Formal wages move more favorably for women than men in municipalities with a larger China-tariff exposure gap. In manufacturing, women's coefficients turn positive in the latter part of 2018 and generally remain above men's through 2021 (Figure 2). Across all sectors, women's coefficients stay closer to zero, while men's are mostly negative after the tariff increases. The directly estimated female–male wage difference is generally positive after 2018, with substantial uncertainty and fluctuations already visible before the tariffs (Figure A2). These patterns connect the exposure gap to relative labor-market conditions, while the prewar movements and changes during the pandemic complicate attribution of the full wage path to tariffs. Mean formal wages also change when the composition of registered jobs changes. The own-sex specification is reported in Figure A3.

Registered female employment also rises with the China gap in the December 2016–December 2021 comparison, whereas endpoint wage estimates and the female wage-bill share are imprecise (Table A3). Formal jobs can expand through movements into registered employment as well as through entry into work, so this finding can coexist with an imprecise survey employment response. Household-income estimates from ENIGH are likewise imprecise (Table A4).

Higher mean own income and more favorable formal wage movements for women than men accompany the increase in physical violence in municipalities with a larger China gap. The survey estimates provide less precise evidence of changes in employment or women's share of household resources. The division of these economic gains within couples remains uncertain.

[3] Capping positive income at the wave-specific 95th or 99th percentile, or trimming observations above the 99th percentile, gives PPML estimates between 3.7 and 4.1 percent per SD. These estimates are statistically significant at the ten percent level but not the five percent level. The upper tail strengthens the baseline estimate and its precision, while the estimated probability of positive income remains imprecise.

Table 2: Tariff exposure and women's economic outcomes

	(1) Log own income	(2) Own income (PPML)	(3) Currently working	(4) Woman's income share
Post × China-tariff exposure gap	0.010	0.051**	0.70	0.09
	(0.010)	(0.025)	(0.63)	(0.29)
Post × Mexico-tariff exposure gap	0.015	−0.054	−0.78*	−0.22
	(0.012)	(0.034)	(0.47)	(0.30)
Post × Average China-tariff exposure	0.018	−0.001	0.44	0.02
	(0.012)	(0.024)	(0.59)	(0.36)
Post × Average Mexico-tariff exposure	−0.001	0.043	0.13	0.02
	(0.011)	(0.033)	(0.59)	(0.25)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	63,551	148,388	156,267	75,900
Mean of dep. var. (2016)	8.345	2,293.8	44.2	18.4

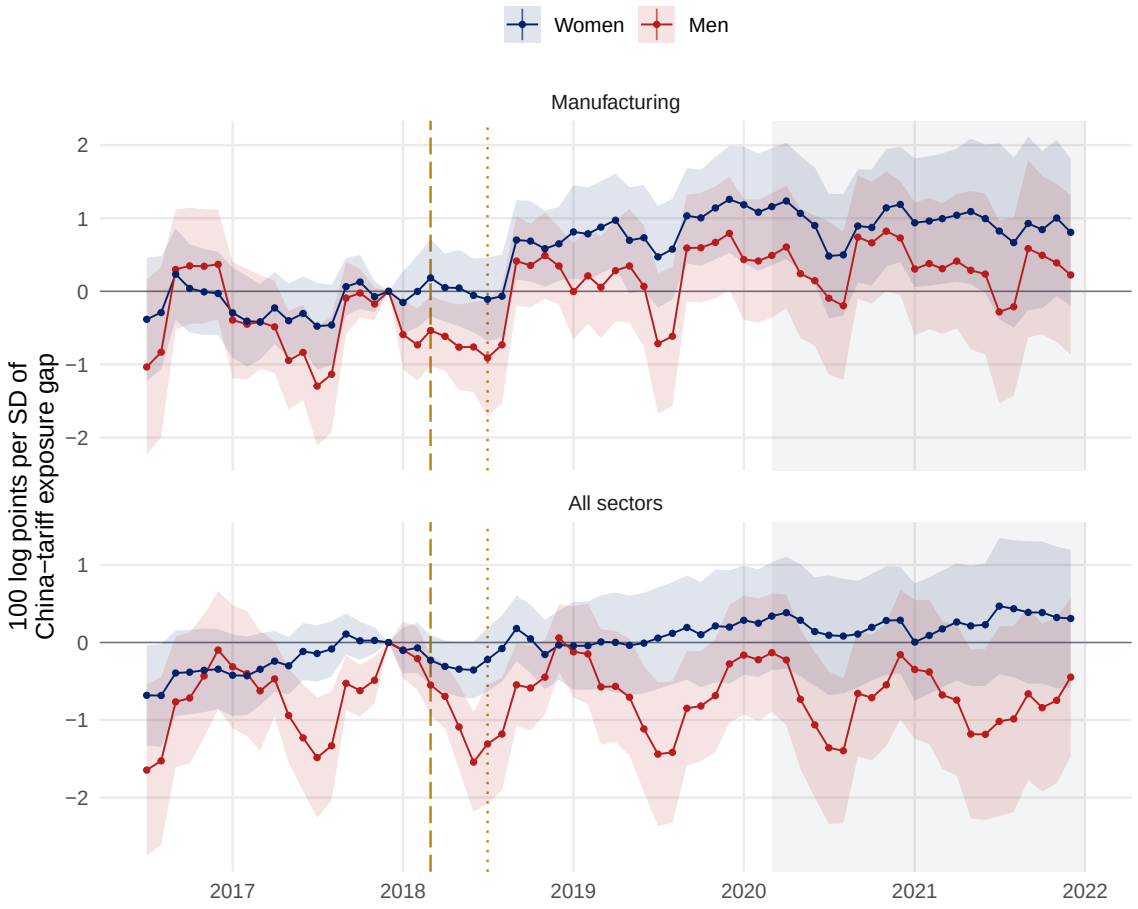
Notes: Each column reports a regression for the indicated outcome, using OLS except for PPML in column (2). All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are per one-SD increase: log points in columns (1)–(2), percentage points in columns (3)–(4). Income is monthly; employment means currently working. Column (4) measures own income divided by own income, partner income and transfers combined. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Column (1) requires positive income; column (2) retains valid zeros. Unresolved or censored income is excluded. Column (4) requires a current partner and a known, positive income total. Outcome means are weighted 2016 means in log pesos, pesos, percent and percent, respectively. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

6.3 Own income and the relative-position mechanism

The relationship between tariff exposure and physical violence remains after conditioning on women's own income. On the sample with observed income, adding monthly income in levels leaves the China-gap estimate at 0.46 percentage points for past-year violence and changes the lifetime estimate from 0.82 to 0.81 percentage points (Table 3). The Mexico-gap coefficients also change little. Comparing specifications on this common sample separates the effect of adding income from the exclusion of unresolved income reports. The coefficients on own income are positive and imprecise.

Own income captures only part of the economic position that matters in the model. At a given level of a woman's earnings, her partner's resources and her relative position can still change. The persistence of the exposure-gap coefficient fits this distinction, and the model permits it under either bargaining or backlash. Because measured income can respond to the tariff shock and to household circumstances affecting violence, the conditioning exercise describes a partial association rather than a causal mediation share.

Figure 2: China-tariff exposure gap and formal wages by sex



Notes: Separate regressions for women and men in manufacturing and all sectors include month interactions with both exposure gaps and both average exposures. Points report the China-gap coefficients. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Coefficients describe log mean daily contribution wages relative to December 2017, in log points per one-SD increase in exposure. Coefficients and confidence intervals are multiplied by 100. Exposure scales use the ENDIREH reference sample of Table 1. All regressions include municipality and month-by-year fixed effects. The sample comprises municipality-month observations from IMSS administrative records in the 566 eligible municipalities, from July 2016 to December 2021, using equal weights. Each regression retains observations with positive wages for the indicated sex and sector group. Vertical lines mark March and July 2018; grey shading begins in March 2020. Shaded bands show pointwise 95% normal-approximation confidence intervals with standard errors clustered by municipality.

6.4 Psychological consequences and recorded offenses

The rise in reported physical violence is accompanied by more reported sadness or depression attributed to partner problems. A one-SD larger China-tariff exposure gap predicts about 1.5 percentage points more such reports, statistically significant at the five percent level and equivalent to 8.6 percent of the 2016 mean (Table 4). Suicidal ideation and suicide attempts have imprecise responses, as do the three Mexico-gap estimates. These outcomes record joint reports of partner

Table 3: Tariff exposure and physical partner violence: controlling for own income

	(1) Full sample	(2) Income observed	(3) Income controlled
Panel A: Past 12 months			
Post × China-tariff exposure gap	0.46*	0.46*	0.46*
	(0.24)	(0.26)	(0.26)
Post × Mexico-tariff exposure gap	−0.38*	−0.31	−0.31
	(0.20)	(0.22)	(0.22)
Post × Average China-tariff exposure	−0.44*	−0.48*	−0.48*
	(0.26)	(0.26)	(0.26)
Post × Average Mexico-tariff exposure	0.24	0.19	0.19
	(0.24)	(0.22)	(0.22)
Own monthly income (1,000 pesos)			0.005
			(0.006)
Municipality FE	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes
Observations	156,267	148,426	148,426
Mean of dep. var. (2016, %)	6.4	6.4	6.4
Panel B: Lifetime			
Post × China-tariff exposure gap	0.84**	0.82**	0.81**
	(0.38)	(0.38)	(0.38)
Post × Mexico-tariff exposure gap	−0.55*	−0.52	−0.51
	(0.31)	(0.32)	(0.32)
Post × Average China-tariff exposure	−0.39	−0.40	−0.40
	(0.43)	(0.43)	(0.43)
Post × Average Mexico-tariff exposure	−0.24	−0.30	−0.31
	(0.40)	(0.39)	(0.39)
Own monthly income (1,000 pesos)			0.042
			(0.027)
Municipality FE	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes
Observations	156,267	148,426	148,426
Mean of dep. var. (2016, %)	17.6	17.7	17.7

Notes: Each column in each panel reports a difference-in-differences regression for physical partner violence. Column (1) uses the full sample; columns (2)–(3) use the same sample with observed income. Column (3) adds own monthly income in thousands of pesos. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. The income coefficient is in percentage points per 1,000 pesos. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Columns (2)–(3) retain valid zeros and exclude unresolved or censored income. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

violence and its psychological consequences in the survey population. They extend the evidence to the distress women attribute to their relationships; the questionnaire routing and treatment of skips are described in Section 4.

Table 4: Tariff exposure and reported psychological consequences of partner problems

	(1) Sadness or depression	(2) Suicidal ideation	(3) Suicide attempt
Post × China-tariff exposure gap	1.45**	0.18	−0.01
	(0.59)	(0.20)	(0.12)
Post × Mexico-tariff exposure gap	−0.23	0.18	0.06
	(0.39)	(0.24)	(0.11)
Post × Average China-tariff exposure	−0.60	−0.04	−0.06
	(0.57)	(0.23)	(0.14)
Post × Average Mexico-tariff exposure	−0.08	−0.35	−0.09
	(0.46)	(0.28)	(0.09)
Municipality FE	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes
Observations	154,059	154,205	154,205
Mean of dep. var. (2016)	16.9	3.7	1.6

Notes: Each column reports a difference-in-differences regression for reported psychological consequences of partner problems. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. The indicators equal one for an affirmative report. Women who report none of the listed partner-violence acts and therefore skip the consequences module are coded zero; these are joint reporting outcomes, not measures of general mental health. Attempts skipped after a negative ideation response are also zero. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Unspecified responses and unresolved skips remain missing. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Administrative offenses show no clear counterpart to the survey exposure-gap pattern. Both tariff-gap estimates in Table 5 are small and imprecise under IHS and PPML. Average-exposure results vary across the two estimators: the negative average China estimate is statistically significant at the five percent level only under IHS, while average Mexico exposure has a positive IHS coefficient and a negative PPML coefficient, statistically significant at the ten and five percent levels, respectively. The annual estimates through 2024 provide the longer comparison in Figure A7. Investigation-file offenses cover a broader population and set of family relationships than ENDIREH and depend on reporting to authorities and institutional processing.

6.5 Exposure measurement and alternative comparisons

The China-gap relationship with physical violence remains when the comparison is made within states. State-by-wave fixed effects yield estimates of 0.49 percentage points for past-year violence

Table 5: Tariff exposure and recorded family-violence offenses

	(1) Count (IHS)	(2) Count (PPML)
Post × China-tariff exposure gap	−0.010	0.011
	(0.027)	(0.033)
Post × Mexico-tariff exposure gap	0.001	0.001
	(0.026)	(0.029)
Post × Average China-tariff exposure	−0.087**	−0.012
	(0.039)	(0.039)
Post × Average Mexico-tariff exposure	0.047*	−0.077**
	(0.028)	(0.036)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
Observations	3,370	3,346
Mean of dep. var. (2015–2017)	4.161	219.034

Notes: Each column reports a difference-in-differences regression for annual family-violence offenses recorded in investigation files. Column (1) uses the IHS of the count; column (2) estimates the count by PPML. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2019–2021 relative to 2015–2017; the transition year 2018 is excluded. Exposure coefficients and standard errors are per one-SD increase: transformed-count units in column (1), log points in column (2). Exposure scales use the ENDIREH reference sample of Table 1. All regressions include municipality and year fixed effects. The sample comprises observed municipality-year counts from SESNSP municipal crime records in 565 eligible municipalities represented in both periods, using equal weights. Reported zeros are retained; missing records are not coded zero. PPML drops four municipalities with zero counts in every included year. Outcome means are unweighted 2015–2017 means on each column's dependent-variable scale. Records reflect reporting and enforcement and are not restricted to female victims or intimate partners. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

and 0.88 for lifetime violence, close to the headline estimates of 0.46 and 0.84 (Table A5). On a common sample, adding the pandemic and concurrent-policy controls changes the corresponding estimates from 0.46 to 0.50 and from 0.84 to 0.93 percentage points (Table A11). The positive estimates therefore survive both adjustments, subject to the post-treatment qualification for pandemic controls discussed in Section 5.3.

The results are more sensitive to trade weights than to the vintage of employment shares. Alternative annual IMSS vintages retain positive China-gap estimates (Table A9). With equal industry weights, exports relative to output or exports per worker, the past-year estimates range from 0.28 to 0.37 percentage points and are imprecise (Table A8). The lifetime estimates are also smaller, especially under output and employment denominators. The baseline export-destination weights consequently matter for both magnitude and precision. Using 2010 Census formal-worker shares produces a smaller, imprecise past-year estimate and a lifetime estimate close to the baseline. IHS-transformed tariff shocks leave the estimates nearly unchanged; restricting the sample to currently partnered women or including Mexico City retains positive China-gap coefficients with less preci-

sion (Tables A6 and A10).

The China-gap result extends beyond industries tariffed under both programs. Removing the four overlapping sectors from both China exposure measures yields estimates of 0.45 percentage points for past-year physical violence and 0.85 for lifetime violence, both statistically significant at the five percent level (Table A7). These are close to the baseline, while the Mexico-gap estimates shrink and become statistically indistinguishable from zero. The estimated Mexico relationship is therefore more sensitive to the construction of China exposure. Omitting one sector at a time also leaves positive China-gap coefficients (Figure A4).

The quintile and tariff-reassignment comparisons provide limited evidence about the linear exposure gradient. The observed coefficients lie within the broad distributions generated by reallocating tariff values across sectors (Figure A5); these are conditional reference distributions because actual tariff assignment was not random. Quintile estimates have wide intervals and do not establish a smooth increase in violence across exposure groups (Figure A6). In the earlier-wave comparison, the 2011 China-gap coefficients are positive relative to 2016, as are the 2021 coefficients, with wide intervals (Figure A1). This pattern and the survey comparability limits described in Section 5.3 leave the parallel-trends assumption unresolved.

Estimates for violence in other settings and recalled childhood abuse are imprecise. China-gap estimates for mild, severe and extreme physical violence are positive over both reference periods, although their precision varies (Table A12). The China-gap estimates for community, workplace and school violence are imprecise, as are those for recalled physical and emotional abuse in childhood (Tables A13 and A14). These comparisons offer no clear evidence of a broad shift across reporting domains, while their differing populations and reference periods limit their ability to rule out changes in disclosure of partner violence.

7 Conclusion

Trade diversion can bring economic gains and household conflict to the same communities. In Mexico, a larger China-tariff exposure gap predicts an increase in reported physical partner violence between 2016 and 2021, while a larger Mexico-tariff exposure gap predicts a decline. A one-standard-deviation larger China-tariff exposure gap predicts a 0.46-percentage-point increase in past-year physical violence, equivalent to 7.2 percent of its 2016 prevalence. These findings connect changes in export opportunities arising from foreign trade policy to women's safety within the household.

The model explains why the distribution of economic opportunities between women and men matters alongside their average level. Better opportunities can strengthen a woman's bargaining position while increasing her partner's incentive to reassert status through violence. Aggregating household responses gives the gender exposure gap and average exposure separate roles in the

empirical specification. Under the model's assumptions, the opposing physical-violence responses to the two tariff programs are consistent with backlash outweighing bargaining. Greater average China-tariff exposure, however, accompanies less past-year physical violence. Economic improvements shared across the local workforce can therefore have different implications from changes in women's position relative to men's.

Women's mean own income rises with the China-tariff exposure gap, and formal wage estimates generally move more favorably for women than men. Estimates for employment and women's share of recorded household income are imprecise, leaving the division of these gains within couples uncertain. Reports of sadness or depression attributed to partner problems also rise with the China-tariff exposure gap. The consequences recorded in the survey thus extend beyond physical acts of violence to the psychological distress women attribute to their relationships.

Following the same couples over time would clarify how local economic changes affect both partners' resources and their experiences of violence. Alternative trade weights preserve the positive estimates for the China-tariff exposure gap but reduce their magnitude and precision, while the earlier survey leaves pre-existing trends unresolved. More frequent, comparable observations before and after changes in labor demand would strengthen the comparison and help establish whether the violence response persists or fades as household roles adjust. Linking these observations to both partners' employment would also help distinguish changes in relative economic position from other pressures on the household.

These findings give a reason to include women's safety in the design and evaluation of policies that expand economic opportunity. Employment initiatives in growing export communities could be linked to confidential referral services and access to legal and psychological support, with services also available to women outside paid work. Community programs addressing norms that legitimize violence or restrict women's economic independence offer another potential complement. Evaluating these combinations would help identify which forms of support allow women to benefit from new opportunities with greater security at home. Assessments of who gains from trade should account for household safety alongside earnings and employment.

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Appendices

A Data Appendix

A.1 Survey population and violence measures

The 2016 and 2021 ENDIREH samples contain women aged 15 and older interviewed about violence in several settings (INEGI, 2016b, 2021a). We link the survey modules within each wave using the respondent identifier and attach exposure through the municipality code. The main analysis retains women who have had a partner, including women whose relationships ended and women with dating relationships. In the harmonized questionnaire classification, these are instruments A1, A2, B1, B2 and C1; C2 identifies never-partnered women and is excluded. Regressions use the person expansion weights supplied for each wave.

Partner violence is recorded in Section XIII in 2016 and Section XIV in 2021. We distinguish physical, sexual, emotional and economic violence using the act-level responses. The frequency categories "many times," "a few times" and "once" identify a reported act. For each domain, the outcome records whether a woman reports any of its acts. The lifetime measures refer to the history covered by the partner battery. The past-year questions refer to the period from October of the year before fieldwork to the interview date, rather than a rolling window of exactly twelve months. We retain the conventional past-year label in the text and exhibits.

Any violence equals one if at least one of the four domain indicators is one. It equals zero when all four are observed and zero; if none is positive and one or more are missing, the combined indicator remains missing. The two horizons are constructed separately. The national prevalence figures in Section 2 describe the survey population, whereas the sample means in Table A1 use the eligible geography and ever-partnered denominator of the analysis. All reported survey outcome means use person weights and the observations available for that outcome.

The severity outcomes group the nine physical acts recorded in both 2016 and 2021. Mild violence comprises pushing or hair-pulling, slapping, and throwing objects. Severe violence comprises tying up, kicking, punching or hitting with an object, attempted strangulation, knife attacks and firearm attacks. Extreme violence comprises the last three acts and is therefore a subset of severe violence. A woman can report acts in more than one group. The act count sums the nine indicators to measure the number of distinct types of physical abuse reported. Women outside the lifetime physical battery have missing severity outcomes. Within a nonempty lifetime battery, affirmative act reports are one and all other entries, including blank or unspecified items, contribute zero to the severity indicators and act count. Past-year blanks generated by the lifetime screening likewise contribute no reported act.

The supplementary community, workplace and school outcomes use lifetime reports, the com-

parable reference horizon available across the two main waves. Recalled childhood violence concerns events before age 15. Its timing precedes the trade war, although retrospective reporting and the composition of surveyed women can differ across waves.

A.2 Work income and income shares

Own and partner work income are constructed from the amount and reporting period in Section IV. We multiply weekly amounts by 4.33 and fortnightly amounts by two; monthly amounts are unchanged. Converted amounts are rounded to whole pesos. A positive amount requires a recognized reporting period to yield monthly income. Positive income is converted using that person's own reporting period.

The coding distinguishes zero income from a missing amount. An explicitly reported zero remains zero, including for women who report working. A blank amount following an explicit nonwork response is also zero. A blank amount for a working woman, an unknown or unspecified amount, and a positive amount with an unresolved period remain missing. In the released amount fields, codes 999998 and 999999 denote unknown or unspecified information where applicable; 999997 denotes a censored amount and is excluded from point-valued income. A reported zero does not require a reporting period because its monthly value is unchanged by the conversion. These distinctions use the questionnaire's separate amount, period and routing information (INEGI, 2021b).

The labor-supply outcome records whether a woman currently works. The log-income outcome uses strictly positive observed monthly income, and the PPML outcome retains both positive values and valid zeros. We do not trim or winsorize income in these baseline outcomes. The IHS alternative retains observed zeros. Table 3 compares violence regressions on the same observed-income sample when monthly own income in thousands of pesos is added as a control, so that changing the sample is separated from adding the regressor.

Partner income follows the same amount-and-period rules. Transfer components are monthly amounts: confirmed nonreceipt contributes zero, while an unknown or censored component makes the total missing. The woman's income share is own work income divided by the sum of own work income, partner work income and transfers. We calculate it only for currently partnered women with all components observed and a strictly positive denominator. Observed zero own income yields a zero share when the remaining components are observed and their sum is positive. The denominator covers the work-income and transfer components recorded in ENDIREH.

A.3 Psychological consequences of partner problems

The consequence questions follow the screening for acts of partner violence. Sadness or depression is a combined response to question 13.28, option 4, in 2016 and question 14.29, option 4, in 2021.

Suicidal ideation is recorded in questions 13.29 and 14.30, and suicide attempts in questions 13.30 and 14.31. The questions attribute the experiences to problems or conflicts with the partner; the sadness/depression item is not a clinical diagnosis.

An affirmative response is coded one and an explicit negative response zero. When all applicable partner-violence screening items are answered "never," the questionnaire skips the consequences module. For these women, zero denotes the absence of a joint report of partner violence and the specified consequence. The attempt question has an additional skip after a negative answer on suicidal ideation; that confirmed skip is also zero. Unspecified responses, unresolved screening answers and other unexplained blanks remain missing. Each outcome uses its own available observations, accounting for the different sample sizes in Table 4.

A.4 Employment shares and municipality coverage

We use monthly IMSS registered-employment counts to measure the industrial composition of women's and men's local employment (Instituto Mexicano del Seguro Social, 2024). Counts are aggregated by municipality, sex and three-digit industry and averaged over calendar year 2017. Industry shares divide these averages by all mapped employment of the same sex in the municipality. Employment in industries with no tariff shock remains in the denominator. Thus a municipality with little employment in affected industries receives little exposure even if those industries account for most of its tradable employment.

The IMSS activity classification is linked to three-digit industries in Mexico's *Sistema de Clasificación Industrial de América del Norte* (SCIAN) through a researcher concordance, using economic-group assignments with overrides for more detailed activity codes. Bundled classifications require assignments that are not an official one-to-one equivalence between the two systems. A geographic concordance maps IMSS municipality identifiers to INEGI's five-digit codes, preserving leading zeros.

Eligibility requires positive employment of each sex in at least one industry affected by tariffs on China and at least one affected by tariffs on Mexico. We intersect these four conditions using the 2017 employment counts, obtaining 566 municipalities. This set is held fixed in the main robustness comparisons; outcome availability can further reduce the realized sample. One eligible municipality has no ENDIREH respondents, leaving 565 in the headline regressions. Mexico City is excluded from this construction because the IMSS records do not permit employment to be assigned separately to its survey boroughs. The supplementary comparison assigns citywide exposure to those boroughs and clusters them together as one unit (Table A6).

A.5 Tariffs, trade weights and exposure scales

The product-level tariff files accompanying Fajgelbaum et al. (2020, 2024) record increases in US import tariffs during 2018 and 2019. We use the 2019 maximum-increase measure separately for goods from China and Mexico. This records the maximum increase during the year, not an annual average tariff paid or the tariff prevailing at the 2021 interview. The source documentation for the 2018–19 update is dated June 1, 2021. Tariffs imposed in retaliation by China or Mexico are not part of the main exposure measures.

Product codes in the Harmonized System (HS) are aggregated from ten digits (HS10) to six digits (HS6) by averaging tariff increments, and sector tariff shocks average the mapped product increments. The product concordance uses the 2017 mapping between HS and the North American Industry Classification System (NAICS): each HS6 product is assigned to the modal six-digit NAICS industry among its HS10 entries and then to a three-digit industry. Using these industries as counterparts to three-digit SCIAN is a researcher approximation. The resulting concordance defines the industry content of both the tariff measures and the export weights.

The baseline trade weight is the share of a sector's Mexican exports sent to the United States. We calculate the numerator and denominator from 2017 BACI trade flows, using the HS2017 classification and release V202501 (Gaulier and Zignago, 2010). Mexican exports are summed across mapped products within the sector; the numerator includes only the US destination, and the denominator includes all destinations. Sectors absent from the mapped export data receive zero weight. This share measures orientation toward the US market.

For each tariff measure, female and male exposure sum the products of the sex-specific employment share, the sector trade weight and the sector tariff increase. We first form the female-minus-male gap and the arithmetic mean of female and male exposure. Each of these four regressors is then centered and divided by its own standard deviation. The reference distribution consists of ever-partnered respondent rows in the eligible municipalities across the two main waves, before outcome-specific exclusions. These means and standard deviations are unweighted across respondent rows, so municipalities contribute in proportion to their sample counts; the regressions themselves use person weights. The same scales are retained when missing income or a sample restriction removes respondents and in the complementary IMSS and ENIGH estimates.

Alternative exposure constructions use their own reference standard deviations; the three-wave comparison uses its separate harmonized reference sample.

The alternative trade weights change the economic importance attached to each sector. One sets all weights to one. Another uses US-bound gross exports divided by gross output from the Organisation for Economic Co-operation and Development (OECD) Trade in Value Added (TiVA) database, using the 2025 edition's observations for 2017 (OECD, 2025); both quantities come from the same national accounting system. Its industry classification is coarser than three-digit SCIAN, so several subsectors share an industry weight.

The exports-per-worker alternative divides 2017 US-bound BACI exports by employment and excludes oil and gas. Employment totals come from the four quarterly rounds of the National Survey of Occupation and Employment (ENOE), averaged over 2017 (INEGI, 2017a). ENOE's public industry categories do not distinguish the required manufacturing subsectors, so group totals are allocated using their shares in national IMSS employment. Within-group differences in these denominators consequently come from IMSS rather than separate ENOE subsector estimates.

A.6 Alternative employment shares and the 2011 comparison

The Census alternative uses the long-form 2010 Population Census sample (INEGI, 2010). We sum person expansion weights by municipality, sex and the first three digits of reported industry, restricting workers to those reporting employer benefits. Shares are formed within municipality and sex and combined with the same tariff shocks and baseline export-destination weights. This comparison changes the date, classification and operational definition of formal employment together. The alternative IMSS vintages instead retain the administrative source and change the period used to construct shares.

The 2011 comparison harmonizes physical violence across three waves without replacing the main 2016–2021 outcomes (INEGI, 2011). The common battery includes pushing or hair-pulling, tying up, kicking, throwing objects, punching or hitting with an object, attempted strangulation, knife attacks and firearm attacks. Slapping, included in the later nine-item battery, is excluded because it lacks a matching item in the 2011 battery. Both past-year and lifetime indicators are constructed from these eight acts.

The comparable 2011 files cover currently or previously married or cohabiting women. We impose that narrower universe on the later waves as well, excluding their dating-only group from this exercise. The 2011 construction uses the available dwelling expansion factor, whereas the later waves use their person factors; selection of women within dwellings also differs. The common-item series harmonizes acts and partnership coverage; differences in weights and within-dwelling selection remain. The figure uses 2016 as its reference year and includes municipality and state-by-wave fixed effects, with wave interactions for both exposure gaps and both average exposures, age and education controls, and municipality-clustered intervals.

A.7 Complementary income and administrative outcomes

The monthly IMSS wage panel covers July 2016 through December 2021, including every calendar month. Mean daily contribution wages divide contribution wage mass by jobs with reported wages. The endpoint comparison uses December 2016 and December 2021, with female employment measured by registered jobs. The female wage-bill share divides female contribution wage mass by the combined female and male total and requires both components to be observed. These measures describe the changing population of registered jobs in each municipality. Changes can

reflect entry, exit and workforce composition as well as pay; payroll underreporting documented by Kumler et al. (2020) is an additional measurement consideration.

ENIGH supplies quarterly household current income in 2016 and 2020 (INEGI, 2016a, 2020b). Within each municipality and wave, we calculate the household-weighted mean of log income among households with positive income and valid positive survey weights. The dependent variable is the 2020 mean minus the 2016 mean, not the log change in mean income. The sample contains 402 eligible municipalities observed in both waves. One regression weights municipalities equally; the other weights them by the smaller of their two endpoint household counts. ENDIREH's exposure scales are retained. ENIGH was not designed to provide representative municipality estimates, so these local means have sampling uncertainty beyond the differences in their household counts. The second endpoint also falls in the pandemic.

The administrative family-violence data are SESNSP municipal offense records, using the December 2025 data vintage (Secretariado Ejecutivo del Sistema Nacional de Seguridad Pública, 2025). We sum complete monthly modality records to annual municipality counts. A missing month leaves the corresponding annual source record unknown; absent municipality-years are not filled with zeros. The table uses observed complete years in 2015–2017 and 2019–2021, requiring at least one observation in each period but not a balanced six-year panel. It excludes 2018. The IHS and PPML regressions include municipality and year fixed effects and the four exposure interactions, with equal observation weights and municipality-clustered standard errors. Municipalities with zero counts in every included year drop from PPML estimation.

The administrative annual figure extends through 2024 and uses 2017 as the reference year. It includes year interactions for both gaps and both average exposures, with municipality and year fixed effects. The dependent variable is the IHS of the annual offense count. These records measure offenses reaching investigation files, cover victims and relationships beyond the female partner-violence sample, and depend on reporting and enforcement.

A.8 Concurrent shocks and exposure diagnostics

The additional-control comparison combines municipal mortality, pandemic employment shocks and closure exposure with prewar measures of exposure to concurrent policies. Mortality comes from the health ministry's December 31, 2021 surveillance snapshot (Secretaría de Salud, 2021). We retain confirmed cases in classification categories 1–3 and count deaths dated through October 31, 2021 by municipality of residence. Deaths per 1,000 residents use municipal population totals from the 2020 Census (INEGI, 2020a).

The pandemic employment measure uses the national percentage change in each three-digit industry's IMSS employment between January and December 2020. We multiply these changes by the municipality's 2017 female-minus-male employment shares and sum across industries. The national totals include the municipality's own employment; the leave-municipality-out version is

a separate diagnostic and does not enter the reported joint-control specification.

Closure exposure combines 2017 employment shares with a researcher-coded approximation to the federal essential-activity rules (Secretaría de Salud, 2020). The coding treats agriculture and related activities (111–115), oil and gas extraction (211), mining (212), food manufacturing (311) and chemicals (325) as essential, and other mapped industries as nonessential. We construct the average of female and male exposure to nonessential industries and the female-minus-male gap. This broad industry classification measures potential exposure to closures; it does not observe which establishments actually closed or the duration of their closures.

Northern-border policy exposure is an indicator for municipalities covered by the December 2018 fiscal decree (Presidencia de la República, 2018). The minimum-wage control uses December 2017 IMSS wage bins: the implemented upper-bound measure includes jobs in the first two bins, covering wages up to twice the minimum. Unclassified wage observations are assigned to the lowest group and included in this measure. It is an upper-bound proxy for the share near the minimum wage. Prospera coverage is the person-weighted municipal share of 2016 ENDIREH respondents reporting receipt in the preceding twelve months, calculated from the existing survey panel. Apparel and textile exposure is the share of 2017 IMSS employment in industries 313–315. Interacting this prewar share with the post indicator allows the change in violence to vary with local specialization in apparel and textiles. This checks whether the tariff-exposure estimates reflect broader changes in municipalities concentrated in these industries.

Each control enters interacted with the post indicator. Missing municipal controls are not replaced by zero, and the baseline is re-estimated on the adjusted regression's sample. Because pandemic measures occur after the tariff shock, this is a conditional robustness comparison rather than a mediation analysis.

The sector-omission exercise reconstructs the China-tariff gap after removing one sector's contribution and standardizes that gap on its reference sample. The Mexico gap and both average exposures retain their baseline values. Sector 314 has a nominal China tariff but no matched employment in the exposure data, so it contributes zero; the omission figure covers the 26 China-tariffed sectors with employment. The exercise measures sensitivity to the sector composition of the China gap while holding the other three exposure measures fixed.

The random-tariff diagnostic assigns the observed nominal positive China tariff values to the employment-sector universe 1,000 times. Employment shares, trade weights, the Mexico gap and both average exposures remain fixed. Each assigned China gap is standardized using its own reference standard deviation before the regression is estimated. The resulting distribution describes how the coefficient changes under these hypothetical assignments. Actual tariffs were not randomized, so the coefficient's rank in this distribution is a conditional diagnostic rather than a design-based randomization probability.

B Proofs

Throughout, Assumption 1 holds; we write $y = w_m + w_f$, $\alpha = w_f/y$, $\rho = w_f/w_m$, and $A(v, \rho)$ as in (4). Assumption 1 includes $A \geq 0$, which is what the implicit-function paths below require.

B.1 Proof of Proposition 1

Write $c_f = y - c_m$. The second-stage surpluses are $S_m = u(c_m) + \psi(v)h(\rho) - \kappa(v) - \bar{U}_m(w_m)$ and $S_f = u(c_f) - \phi_f(v) - \bar{U}_f(w_f)$. The interior Nash first-order condition is the residual

$$F(c_m, v; w_m, w_f) \;\equiv\; (1-\alpha)\frac{u'(c_m)}{S_m} - \alpha\frac{u'(y-c_m)}{S_f} \;=\; 0. \tag{21}$$

By the implicit function theorem $\partial c_m^*/\partial v = -F_v/F_{c_m}$. Only S_m, S_f depend on v, with $\partial S_m/\partial v = A(v,\rho)$ and $\partial S_f/\partial v = -\phi_f'(v)$, so

$$F_v = -(1-\alpha)\frac{u'(c_m)A(v,\rho)}{S_m^2} - \alpha\frac{u'(c_f)\phi_f'(v)}{S_f^2} \;<\; 0$$

whenever $A \geq 0$ (the second term is strictly negative regardless), and

$$F_{c_m} = (1-\alpha)\frac{u''(c_m)S_m - u'(c_m)^2}{S_m^2} - \alpha\frac{-u''(c_f)S_f + u'(c_f)^2}{S_f^2} \;<\; 0,$$

since $u'' < 0$ and $S_m, S_f > 0$. Hence $\partial c_m^*/\partial v < 0$. The husband's first-stage objective is $H(v) \equiv u(c_m^*(v)) + \psi(v)h(\rho) - \kappa(v)$; its first-order condition $H_v = 0$ is $u'(c_m^*)\,\partial c_m^*/\partial v + A(v^*, \rho) = 0$, which is (6). Note that (6) delivers $A(v^*, \rho) = -u'(c_m^*)\,\partial c_m^*/\partial v > 0$ at any interior optimum, so the hypothesis $A \geq 0$ binds only off the equilibrium path. Under transferable utility the Nash split of (21) is replaced by an efficient transfer, c_m^* ceases to depend on v, and the first term of (6) vanishes. □

B.2 Proof of Proposition 2

Applying the implicit function theorem to $H_v = 0$ gives $v_{w_g}^* = -H_{vw_g}/H_{vv}$ with $H_{vv} < 0$ by strict concavity (Assumption 1). Differentiating H_v,

$$H_{vw_g} \;=\; \underbrace{u''(c_m^*)\frac{\partial c_m^*}{\partial w_g}\frac{\partial c_m^*}{\partial v} + u'(c_m^*)\frac{\partial^2 c_m^*}{\partial v\,\partial w_g}}_{\equiv\; B_g \text{ (bargaining)}} \;+\; \underbrace{\psi'(v)h'(\rho)\,\frac{\partial \rho}{\partial w_g}}_{\text{backlash}}. \tag{22}$$

Since $\rho = w_f/w_m$, $\partial\rho/\partial w_f = 1/w_m$ and $\partial\rho/\partial w_m = -\rho/w_m$. Substituting into (22) and using $v^*_{w_g} = -H_{vw_g}/H_{vv}$ with $H_{vv} < 0$,

$$\operatorname{sgn}\mathcal{N} = \operatorname{sgn}\big[(B_m + B_f) + (1-\rho)\,\psi'(v^*)h'(\rho)/w_m\big], \tag{23}$$

$$\operatorname{sgn}\mathcal{R} = \operatorname{sgn}\big[(1+\rho)\,\psi'(v^*)h'(\rho)/w_m - (B_m - B_f)\big], \tag{24}$$

and (24) is (8). This uses Assumption 1 only. Assumption 2 is what licenses reading the second bracket as protective, and hence the *dominates* vocabulary of the text; Appendix B.3 establishes it as a theorem in the logarithmic benchmark. The backlash term is strictly positive, since $\psi' > 0$, $h' > 0$ and $v^* > 0$ by Assumption 1. □

Remark (*backlash leaks into $\mathcal{N}$ off symmetry*). At a symmetric baseline $\rho = 1$ the coefficient $(1-\rho)$ in (23) is zero, so backlash is a pure reallocation force and $\operatorname{sgn}\mathcal{N} = \operatorname{sgn}(B_m + B_f)$. Away from symmetry $(1-\rho) \neq 0$ and backlash enters $\mathcal{N}$; this is the analytic root of Proposition 4.

B.3 The bargaining channel is protective in the logarithmic benchmark

This appendix establishes Assumption 2 as a theorem in the specification of Lemma 1, evaluated at a symmetric baseline $w^0_m = w^0_f = w^0$ at which the dissolution constants are chosen so that the two baseline surpluses coincide, $S_m = S_f \equiv S^* > 0$. Such baselines exist by construction: fix any $S^* > 0$, let v^* solve $\phi'_f(v) = A(v,1)(1+2S^*)$, and set $d_f = S^* + \phi_f(v^*)$ and $d_m = S^* - \psi(v^*)h(1) + \kappa(v^*)$. This is a restriction on the primitives rather than a normalization, and it is imposed nowhere else in the paper; what makes it cheap here is that d_g shifts the level of $\bar{U}_g$ only, leaving $\bar{U}'_g(w^0) = u'^*$, the sole property of the outside option used below.

At $\alpha = \frac{1}{2}$ the Nash rule (21) reads $u'(c_m)/S_m = u'(c_f)/S_f$, so equal surpluses give $c^*_m = c^*_f = w^0$. Hence $u'^* = 1/w^0$ and $u''^* = -1/(w^0)^2$, and logarithmic outside options give $\bar{U}'_g(w^0) = u'^*$: at the baseline the marginal value of the outside option equals the marginal utility of consumption. Write A^*, ψ^*, ψ'^* for the baseline values of $A(v^*,1)$, $\psi(v^*)$ and $\psi'(v^*)$; by Assumption 1, $A^* \geq 0$, and $\psi^*, \psi'^* > 0$ because $\psi(0) = 0$, $\psi' > 0$ and $v^* > 0$. The first-stage condition (6) reduces to $\phi'^*_f = A^*(1+2S^*)$, and differentiating the second-stage rule (21) at the baseline gives the consumption responses

$$\sigma_f \equiv \frac{\partial c^*_m}{\partial w_f} = -\frac{\psi^* h'(1)}{2(1+S^*)} < 0, \qquad \sigma_m \equiv \frac{\partial c^*_m}{\partial w_m} = 1 + \frac{\psi^* h'(1)}{2(1+S^*)} > 1, \tag{25}$$

so that $\sigma_f + \sigma_m = 1$. That identity owes nothing to the logarithm: raise both wages by a common amount and pass that amount to each spouse's consumption; α and ρ are unchanged, and the two surpluses shift equally precisely when the outside options have equal slope at the baseline, $\bar{U}'_m(w^0) = \bar{U}'_f(w^0)$, so the Nash rule continues to hold and his consumption has risen one for one. What the logarithm supplies here is the closed form, and with it the *sign* of σ_f — a transfer toward

the wife lowers his consumption.

Substituting (25), the first-stage condition, and the logarithmic values of u'^*, u''^*, u'''^* into the definition of B_g in (22) yields

$$2(1+S^*)^2 w^0\, B_f = -\Big[A^*\big((1+S^*)^2 + S^*\psi^* h'(1)\big) + (1+S^*)\psi'^* h'(1)\Big] \;<\; 0, \tag{26}$$

$$2(1+S^*)^2 w^0\, B_m = \quad A^*\big((1+S^*)^2 + S^*\psi^* h'(1)\big) + (1+S^*)\psi'^* h'(1) \;>\; 0. \tag{27}$$

The strict signs follow from $A^* \geq 0$, $S^* > 0$, $\psi^*, \psi'^* > 0$ and $h'(1) > 0$, all supplied by Assumption 1: the second summand alone would sign the result even in the degenerate limit $A^* \to 0$. Differencing,

$$2(1+S^*)^2 w^0\,(B_m - B_f) \;=\; 2A^*\big[(1+S^*)^2 + S^*\psi^* h'(1)\big] + 2(1+S^*)\psi'^* h'(1) \;>\; 0, \tag{28}$$

so $B_m - B_f > 0$: the bargaining channel is protective under Assumption 1, the specification of Lemma 1, and the equal-surplus restriction. □

Remark. Adding (26) and (27) gives $B_m + B_f = 0$, so by (23) the aggregate-income force vanishes at the symmetric log baseline — which is $\chi(1) = 0$ in (13), reached here by a different route. This second identity needs no equal-surplus restriction: at $\rho = 1$ the two backlash terms in (22) cancel, so $B_m + B_f = H_{vw_m} + H_{vw_f}$, which vanishes by Lemma 1 alone.

The same closed forms settle whether the horse race of Proposition 2 is genuinely two-sided. The difficulty is that h is not separable from the bargaining channel: it enters the husband's surplus, hence the second-stage rule, hence the consumption responses inside B_g — as (26) and (27) display. What *is* separable is the slope h' at the baseline, because at fixed wages the couple's two-stage problem depends on h only through the number $h(1)$. Varying $h'(1)$ therefore moves the comparative statics and nothing else.

Proposition 6 (The race is two-sided)**.** *Under Assumption 1, the specification of Lemma 1 and the equal-surplus restriction, at the symmetric baseline*

$$\operatorname{sgn}\mathcal{R} \;=\; \operatorname{sgn}\big[\,C\,h'(1) - A^*(1+S^*)^2\,\big], \qquad C \;\equiv\; (1+S^*)(1+2S^*)\,\psi'^* - A^* S^* \psi^*. \tag{29}$$

If $C > 0$, then $\mathcal{R} < 0$ for $h'(1) < h'^*$ and $\mathcal{R} > 0$ for $h'(1) > h'^*$, where $h'^* = A^*(1+S^*)^2/C$; if $C \leq 0$, then $\mathcal{R} < 0$ for every $h'(1) > 0$. A sufficient condition for $C > 0$ is $S^*\psi(v^*)h(1) < (1+S^*)(1+2S^*)$. The sweep in $h'(1)$ leaves $v^*, S^*, A^*, \psi^*, \psi'^*$ unchanged.

Proof. At $\rho = 1$ the backlash term of (24) is $2\psi'^* h'(1)/w^0$. Substituting (28) and multiplying by $(1+S^*)^2 w^0 > 0$,

$$\operatorname{sgn}\mathcal{R} = \operatorname{sgn}\Big[2\psi'^* h'(1)(1+S^*)^2 - A^*\big((1+S^*)^2 + S^*\psi^* h'(1)\big) - (1+S^*)\psi'^* h'(1)\Big],$$

and collecting the terms in $h'(1)$ with $2(1+S^*)^2-(1+S^*)=(1+S^*)(1+2S^*)$ gives (29). The bracket is affine in $h'(1)$ with intercept $-A^*(1+S^*)^2\le 0$, so it is non-positive as $h'(1)\to 0$ — strictly negative whenever $A^*>0$, which (6) delivers at any interior optimum — and turns positive for large $h'(1)$ if and only if its slope C is positive; the threshold is the root h'^*. For the sufficient condition, $A^*\le \psi'^* h(1)$ by (4) and $\kappa'\ge 0$, so $C\ge \psi'^*\big[(1+S^*)(1+2S^*)-S^*\psi^* h(1)\big]$. Finally, both stages hold the wage pair fixed at the symmetric point, where $\rho\equiv 1$, so h enters S_m — and hence (21), c_m^*, H, H_v and H_{vv} — only through the number $h(1)$. Hence v^*, and with it $S^*, A^*, \psi^*, \psi'^*$, are invariant to $h'(1)$, as are the regularity conditions of Assumption 1 themselves, so the sweep stays inside the admissible class. □

Proposition 6 does not invoke Assumption 2: it substitutes the closed form (28) in place of a sign restriction. Note that the protective bargaining channel is *not* what makes the race two-sided — $C>0$ is a separate restriction, and where it fails the bargaining force wins however sensitive to relative standing the husband's status return is made.

B.4 Proof of Lemma 1 and Proposition 4

Scale invariance. Let $u(c)=\log c$ and $\bar U_g(w_g)=\log w_g-d_g$, and scale $(w_m,w_f)\to(t\,w_m,t\,w_f)$, $t>0$. Then ρ and α are unchanged. Try $c_g\to t\,c_g$, feasible since the budget scales by t. Each surplus is invariant, because $\log(t\,c_g)-\log(t\,w_g)=\log c_g-\log w_g$ and the violence terms $\psi(v)h(\rho),\phi_f(v),\kappa(v)$ carry no scale. So the second-stage problem is invariant up to the common factor t on consumption, $c_m^*(v;t\,w)=t\,c_m^*(v;w)$, and the first-stage objective obeys $H(v;t\,w)=H(v;w)+\log t$, an additive constant with the same maximizer. Hence $v^*(t\,w_m,t\,w_f)=v^*(w_m,w_f)$. The argument uses the logarithm in *both* the felicity and the outside option; log felicity alone leaves the $\log t$ uncancelled. That the logarithm is the only member of the constant relative risk aversion family with this property is shown in the text following the lemma. □

The force at a gap. Euler's theorem applied to the degree-zero function v^* at the baseline (w_m^0,w_f^0) gives $w_m^0 v^*_{w_m}+w_f^0 v^*_{w_f}=0$, i.e. $v^*_{w_m}=-\rho^0 v^*_{w_f}$ with $\rho^0=w_f^0/w_m^0$. Substituting into (7),

$$\mathcal{N}=(1-\rho^0)\,v^*_{w_f},\qquad \mathcal{R}=(1+\rho^0)\,v^*_{w_f}\;\Longrightarrow\;\mathcal{N}=\frac{1-\rho^0}{1+\rho^0}\,\mathcal{R}=\chi(\rho^0)\mathcal{R},$$

which is (13). At $\rho^0=1$, $\mathcal{N}=0$. For $\rho^0\neq 1$ and $\mathcal{R}\neq 0$, $\mathcal{N}\neq 0$ and is first-order in $1-\rho^0$; and $\operatorname{sgn}\mathcal{N}=\operatorname{sgn} v^*_{w_f}=\operatorname{sgn}\mathcal{R}$ for $\rho^0<1$. □

B.5 Proof of Propositions 3 and 5

Decomposition. Index couples by cell $j\in\{E,N\}$; within a cell every couple starts at a common point, so the wage responses $v^*_{w_m,j}, v^*_{w_f,j}$ are cell constants. The shock reaches the wife only if she works, so a couple of type (s_m,s_f) in cell j has $\Delta v^*=v^*_{w_m,j}\Delta_{s_m}+e_f\,v^*_{w_f,j}\Delta_{s_f}+O(\|\Delta\|^2)$. This is

additively separable in the two spouses' shocks, so averaging over the municipality's composition requires only the two marginal distributions of sectors and not their joint distribution. With $p = \Pr(e_f = 1)$, the husband employed, and his sector unrelated to whether his wife works — so that his mean shock is the same in both cells —

$$\Delta\text{IPV}_c = a_m\,\bar{\Delta}_{m,c} + a_f\,\bar{\Delta}_{f,c}, \quad \bar{\Delta}_{g,c} = \sum_s \pi_{g,s,c}\Delta_s, \quad a_f = p\,v^*_{w_f,E}, \;\; a_m = p\,v^*_{w_m,E} + (1-p)v^*_{w_m,N}. \tag{30}$$

The female channel carries weight p because only earner wives receive it; the male channel carries weight one. Applying $ax + by = \frac{1}{2}(a+b)(x+y) + \frac{1}{2}(a-b)(x-y)$ with $(a,b) = (a_m, a_f)$, $(x,y) = (\bar{\Delta}_{m,c}, \bar{\Delta}_{f,c})$ gives (11), with $\bar{A}_c, \mathcal{G}_c$ as in (10) and

$$\mathcal{N}^* = a_f + a_m, \qquad \mathcal{R}^* = a_f - a_m. \tag{31}$$

No functional form is used here; the two-index decomposition and the first-order sufficiency of the gap hold for general primitives. Setting $p = 1$ gives $\mathcal{N}^*, \mathcal{R}^* = \mathcal{N}, \mathcal{R}$ of (7). □

Earner and pooled samples. A sample restricted to earner couples is the same computation with cell N empty, so its reallocation force is $\mathcal{R}_E = v^*_{w_f,E} - v^*_{w_m,E}$. That subsample can be run on the same municipality-level indices: its female exposure is $\bar{\Delta}_{f,c}$ by definition, since $\pi_{f,s,c}$ is a share among working women, and its male exposure is $\bar{\Delta}_{m,c}$ under the same cell-mean premise used for (30). From (30) and (31),

$$\mathcal{R}^* = p\big(v^*_{w_f,E} - v^*_{w_m,E}\big) - (1-p)v^*_{w_m,N} = p\,\mathcal{R}_E + (1-p)\,\mathcal{R}_N, \qquad \mathcal{R}_N \equiv -v^*_{w_m,N},$$

which is (12); subtracting gives $\mathcal{R}_E - \mathcal{R}^* = (1-p)(\mathcal{R}_E - \mathcal{R}_N)$. Participation therefore scales the difference between the two samples but does not enter the condition for its sign. Under the specification of Lemma 1 the within-cell Euler relation of Appendix B.4 gives $\mathcal{R}_E = (1+\rho^0_E)v^*_{w_f,E}$ and $\mathcal{R}_N = \rho^0_N v^*_{w_f,N}$, so $\mathcal{R}_N < \mathcal{R}_E$ reads $\rho^0_N v^*_{w_f,N} < (1+\rho^0_E)v^*_{w_f,E}$. Throughout, the comparison is between the population gap coefficients of (11), the specification that carries $\bar{A}_c$, and it requires $\mathcal{G}_c$ to be scaled identically in the two samples, since coefficients in different units order nothing. Appendix B.6 records what changes when both samples are estimated gap-only. □

Effective relative wage. This step, unlike the decomposition, uses the specification of Lemma 1 *within each cell*: logarithmic felicity and logarithmic outside options for earner couples, and for non-earner couples the reading $\bar{U}_f(\omega_f) = \log\omega_f - d_f$, under which home production is portable into dissolution. Under that specification Lemma 1 applies cell by cell, and the Euler relation of Appendix B.4 holds in each with the cell's own baseline ratio: $v^*_{w_m,j} = -\rho^0_j v^*_{w_f,j}$. Substituting into (31) and writing $v_j \equiv v^*_{w_f,j}$:

$$\mathcal{N}^* = p(1-\rho^0_E)v_E - (1-p)\rho^0_N v_N, \tag{32}$$

$$\mathcal{R}^* = p(1+\rho^0_E)v_E + (1-p)\rho^0_N v_N. \tag{33}$$

With $\zeta \equiv (1-p)\rho_N^0 \upsilon_N/(p\,\upsilon_E)$ and $\tilde{\rho} \equiv \rho_E^0 + \zeta$, dividing (32) by (33) cancels $p\,\upsilon_E$ and gives

$$\frac{\mathcal{N}^*}{\mathcal{R}^*} = \frac{(1-\rho_E^0)-\zeta}{(1+\rho_E^0)+\zeta} = \frac{1-\tilde{\rho}}{1+\tilde{\rho}} = \chi(\tilde{\rho}),$$

which is (14), and which requires $p > 0$. Under the maintained hypothesis that the horse race resolves with a common sign across cells, $\operatorname{sgn}\upsilon_E = \operatorname{sgn}\upsilon_N$, both terms of (33) carry that common sign, so $\operatorname{sgn}\mathcal{R}^* = \operatorname{sgn}\upsilon_E$ and $\zeta \geq 0$. When $\mathcal{R}^* > 0$ this yields $\mathcal{N}^* < 0 \iff \tilde{\rho} > 1$, which is (15). As $\rho_N^0 \to 0$ the non-earner couples cease to respond, $\zeta \to 0$ and $\tilde{\rho} \to \rho_E^0 < 1$, restoring $\mathcal{N}^* > 0$; setting $p = 1$ gives $\tilde{\rho} = \rho_E^0$, and $p = 1$ with $\rho_E^0 = 1$ gives $\mathcal{N}^* = 0$. □

When the two comparisons lock together. The quantities just assembled also fix when the ordering of the gap coefficient across the two samples settles the sign of the level coefficient. Maintain the specification and the common-sign hypothesis above, take the regime $\mathcal{R}^* > 0$, and let $p < 1$ so that both cells are occupied. Write $q \equiv \mathcal{R}_N/\mathcal{R}_E$. The common-sign hypothesis gives $q \geq 0$, and with $\mathcal{R}^* = \mathcal{R}_E[\,p+(1-p)q\,]$ it makes $\mathcal{R}_E > 0$ equivalent to the regime; since $\mathcal{R}_E - \mathcal{R}^* = (1-p)(\mathcal{R}_E - \mathcal{R}_N)$, the earner force is then the smaller of the earner and pooled forces exactly when $q > 1$ — and, on a leg with $\lambda > 0$, so is the earner coefficient. Substituting $\mathcal{R}_E = (1+\rho_E^0)\upsilon_E$ and $\mathcal{R}_N = \rho_N^0\upsilon_N$ into ζ gives $\zeta = (1-p)(1+\rho_E^0)q/p$, so

$$\tilde{\rho} > 1 \iff q > \bar{q} \equiv \frac{p\,(1-\rho_E^0)}{(1-p)(1+\rho_E^0)}, \qquad \text{and} \qquad \bar{q} \leq 1 \iff 2p \leq 1+\rho_E^0. \tag{34}$$

Both are exact equivalences under the specification of Lemma 1 applied cell by cell, not approximations; the only linearization in the chain is the first-order step behind (11). Hence if $p \leq \frac{1}{2}(1+\rho_E^0)$, then $q > 1$ implies $\tilde{\rho} > 1$ and so $\mathcal{N}^* < 0$ by (15): a smaller earner coefficient forces a negative level coefficient in the *pooled* equation. It says nothing about the earner sample's own level coefficient, which is positive there, since that sample has $\tilde{\rho} = \rho_E^0 < 1$. Equivalently, a non-negative pooled level coefficient requires the earner coefficient to be at least as large as the pooled one. Since $\rho_E^0 > 0$, any participation rate below one half satisfies the bound. The bound cannot be weakened within the algebra: for $p > \frac{1}{2}(1+\rho_E^0)$ one has $\bar{q} > 1$, and every $q \in (1, \bar{q})$ breaks the implication. Whether primitives realizing such a q are themselves admissible is a separate question, not settled here and not needed for the sufficient condition. □

B.6 The direction of the omitted-level bias

Equation (11) is the population regression the model implies. If it is estimated with the gap alone, the level term $\mathcal{N}^*\bar{A}_c$ is omitted and the probability limit of the gap coefficient is the least-squares expression

$$\operatorname{plim}\widehat{D} \;=\; \tfrac{1}{2}\mathcal{R}^* + \mathcal{N}^*\,\frac{\operatorname{Cov}(\bar{A}_c, \mathcal{G}_c)}{\operatorname{Var}(\mathcal{G}_c)}. \tag{35}$$

Units: this appendix normalizes the constructed regressors so that the leg loading λ of Corollary 1 equals one; reinstating λ multiplies both terms of (35) by λ and moves no sign. The model does not sign the second term outright, because it does not sign $\mathcal{N}^*$: by (15), $\operatorname{sgn}\mathcal{N}^* = \operatorname{sgn}(1-\tilde{\rho})$ when $\mathcal{R}^* > 0$, and theory does not place $\tilde{\rho}$ against parity. What it does deliver is the tie $\mathcal{N}^* = \chi(\tilde{\rho})\mathcal{R}^*$ of Propositions 4 and 5. Given that tie, the bias is signed by two objects: the side of parity on which $\tilde{\rho}$ falls, and the sign of $\operatorname{Cov}(\bar{A}_c, \mathcal{G}_c)$, which is positive when the two gendered exposures are positively related across municipalities. The bias is first-order in $1-\tilde{\rho}$ and so vanishes as $\tilde{\rho} \to 1$.

Because (35) is a least-squares identity, the fact that adding $\bar{A}_c$ moves the gap coefficient in the direction implied by the estimated $\operatorname{sgn}\mathcal{N}^*$ is arithmetic, not evidence: it is true of any omitted regressor and confirms nothing about the model. The model's content is the tie between the two coefficients, which is a restriction on the pair.

The same identity disciplines the earner-versus-pooled comparison of Appendix B.5. Both samples are drawn from the same municipalities and carry the same two indices, so they share $\operatorname{Cov}(\bar{A}_c, \mathcal{G}_c)/\operatorname{Var}(\mathcal{G}_c)$; what they do not share is the level force, since $\mathcal{N}_E - \mathcal{N}^* = (1-p)\big[(1-\rho_E^0)\upsilon_E + \mathcal{R}_N\big]$. Estimated gap-only in both, the difference in probability limits is therefore

$$(1-p)\Big\{\tfrac{1}{2}\big(\mathcal{R}_E - \mathcal{R}_N\big) + \frac{\operatorname{Cov}(\bar{A}_c, \mathcal{G}_c)}{\operatorname{Var}(\mathcal{G}_c)}\big[(1-\rho_E^0)\upsilon_E + \mathcal{R}_N\big]\Big\}, \tag{36}$$

which need not carry the sign of $\mathcal{R}_E - \mathcal{R}_N$ unless $\operatorname{Cov}(\bar{A}_c, \mathcal{G}_c) = 0$. The prediction that the earner coefficient is the larger is thus a statement about (11), the specification carrying $\bar{A}_c$; two gap-only regressions compare biased estimands whose ordering depends on a covariance the model does not restrict. □

C Additional Tables

Table A1: Summary statistics for the exposed sample

Panel A: Individual outcomes and characteristics

	2016			2021		
	Mean	SD	N	Mean	SD	N
Past-year physical violence	6.4	24.4	78,328	5.1	22.1	77,939
Past-year sexual violence	2.2	14.6	78,328	2.1	14.3	77,939
Past-year emotional violence	23.6	42.5	78,328	18.7	39.0	77,939
Past-year economic violence	10.2	30.3	78,328	8.2	27.5	77,939
Lifetime physical violence	17.6	38.1	78,328	16.3	37.0	77,939
Lifetime sexual violence	6.5	24.6	78,328	6.8	25.1	77,939
Lifetime emotional violence	40.3	49.0	78,328	35.5	47.8	77,939
Lifetime economic violence	21.0	40.8	78,328	19.2	39.4	77,939
Age (years)	41.0	16.9	78,328	42.5	17.1	77,939
Any schooling	94.9	22.1	78,323	95.9	19.9	77,939
Currently working	44.2	49.7	78,328	48.1	50.0	77,939
Monthly own income (pesos)	2,293.3	4,750.6	73,794	3,311.6	12,346.3	74,632
Log monthly own income	8.3	0.8	29,474	8.6	0.8	34,083
Own income share	18.4	26.9	39,020	21.4	28.1	36,881

Panel B: Municipality tariff exposure

	Mean	SD	Min.	Max.	N
China-tariff exposure gap	−0.0064	0.0197	−0.0885	0.1019	565
Mexico-tariff exposure gap	−0.0017	0.0084	−0.0481	0.0476	565
Average China-tariff exposure	0.0625	0.0392	0.0026	0.1906	565
Average Mexico-tariff exposure	0.0084	0.0171	0.0000	0.2067	565

Notes: Panel A reports person-weighted summary statistics; Panel B reports unweighted statistics for the four unstandardized exposure measures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Binary outcomes and the income share are in percent. Income is monthly. Any schooling excludes unspecified education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Each row in Panel A uses nonmissing responses. Valid income zeros are retained, including nonwork zeros; unknown or censored amounts and unresolved reporting periods remain missing. Log income requires a positive amount. The income share requires a current partner and a known, positive total of own income, partner income and transfers. Panel B counts each of the 565 municipalities with survey observations once; exposures use 2017-average IMSS employment shares and BACI 2017 US export-destination weights.

Table A2: Robustness: Own-sex tariff exposure

	(1) Log own income	(2) Own income (PPML)	(3) Currently working
Post × female China-tariff exposure	0.019	0.022	0.75
	(0.012)	(0.020)	(0.57)
Post × female Mexico-tariff exposure	0.010	0.017	−0.22
	(0.012)	(0.023)	(0.59)
Municipality FE	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes
Observations	63,551	148,388	156,267
Mean of dep. var. (2016)	8.345	2,293.8	44.2

Notes: Each column reports a regression for the indicated outcome, using OLS except for PPML in column (2). All columns include female China- and Mexico-tariff exposures. Post indicates 2021. Exposure coefficients and standard errors are per one-SD increase: log points in columns (1)–(2), percentage points in column (3). Income is monthly; employment means currently working. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Column (1) requires positive income; column (2) retains valid zeros. Unresolved or censored income is excluded. Outcome means are weighted 2016 means in log pesos, pesos and percent, respectively. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A3: Tariff exposure and formal labor market outcomes

	(1) Log daily wage	(2) Daily wage (PPML)	(3) Registered jobs (PPML)	(4) Female wage-bill share
Post × China-tariff exposure gap	0.007	0.006	0.025**	−0.10
	(0.005)	(0.005)	(0.011)	(0.18)
Post × Mexico-tariff exposure gap	0.005	0.004	−0.005	0.12
	(0.007)	(0.007)	(0.010)	(0.26)
Post × Average China-tariff exposure	−0.004	−0.010	0.078***	0.13
	(0.006)	(0.006)	(0.010)	(0.19)
Post × Average Mexico-tariff exposure	0.001	0.005	−0.023**	−0.05
	(0.006)	(0.007)	(0.011)	(0.16)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	1,132	1,132	1,132	1,132
Mean of dep. var. (2016)	5.309	211.359	9,041.095	27.9

Notes: Each column reports a regression for the indicated outcome, using OLS in columns (1) and (4) and PPML in columns (2)–(3). All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates December 2021 relative to December 2016. Exposure coefficients and standard errors are per one-SD increase: log points in columns (1)–(3), percentage points in column (4). Exposure scales use the ENDIREH reference sample of Table 1. Daily wages equal contribution wage mass divided by jobs with reported wages. The female wage-bill share is female contribution wage mass divided by the combined female and male total. The sample comprises municipality-level observations from IMSS administrative records in the 566 eligible municipalities in December 2016 and December 2021, using equal weights. Outcome means are unweighted December 2016 means in log pesos, pesos, jobs and percent, respectively. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A4: Tariff exposure and household income in ENIGH municipalities

	(1) Equal weights	(2) Household-count weights
China-tariff exposure gap	0.008	0.003
	(0.012)	(0.010)
Mexico-tariff exposure gap	0.003	0.006
	(0.010)	(0.008)
Average China-tariff exposure	0.030**	0.046***
	(0.013)	(0.012)
Average Mexico-tariff exposure	−0.036**	−0.024**
	(0.015)	(0.011)
Municipality FE	No	No
Wave FE	No	No
Observations	402	402
Mean of dep. var.	0.151	0.137

Notes: Each column reports a regression for the 2016–2020 change in municipality mean log quarterly household current income. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Exposure coefficients and standard errors are in log points per one-SD increase. Exposure scales use the ENDIREH reference sample of Table 1. The sample comprises 402 eligible municipalities with positive-income household observations in both ENIGH 2016 and 2020. Municipality means use household survey weights. Column (1) uses equal municipality weights; column (2) weights municipalities by the smaller of their two endpoint household counts. Outcome means are the mean changes under the corresponding regression weights. ENIGH is not representative at municipality level, and the second endpoint is in the pandemic. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A5: Robustness: State-by-wave fixed effects

	(1) Physical	(2) Sexual	(3) Emotional	(4) Economic
Panel A: Past 12 months				
Post × China-tariff exposure gap	0.49*	0.04	0.66	0.26
	(0.27)	(0.14)	(0.43)	(0.33)
Post × Mexico-tariff exposure gap	−0.51**	0.22	0.54	0.62*
	(0.20)	(0.15)	(0.58)	(0.33)
Post × Average China-tariff exposure	−0.51*	0.09	−0.41	−0.27
	(0.30)	(0.19)	(0.61)	(0.38)
Post × Average Mexico-tariff exposure	0.13	−0.12	−0.13	−0.65***
	(0.20)	(0.20)	(0.35)	(0.25)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
State × wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016, %)	6.4	2.2	23.6	10.2
Panel B: Lifetime				
Post × China-tariff exposure gap	0.88**	0.26	1.02*	0.62
	(0.41)	(0.22)	(0.60)	(0.42)
Post × Mexico-tariff exposure gap	−0.71**	0.20	0.71	0.04
	(0.34)	(0.23)	(0.79)	(0.43)
Post × Average China-tariff exposure	−0.19	0.20	0.24	−0.23
	(0.50)	(0.32)	(0.87)	(0.59)
Post × Average Mexico-tariff exposure	−0.36	−0.35	−0.29	−0.25
	(0.37)	(0.32)	(0.54)	(0.50)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
State × wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016, %)	17.6	6.5	40.3	21.0

Notes: Each column in each panel reports a difference-in-differences regression for the indicated type of partner violence. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. Exposure scales use the ENDIREH reference sample of Table 1. All regressions control for age and education. All regressions include municipality and state-by-wave fixed effects. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Of the 566 eligible municipalities, 565 have survey observations. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A6: Robustness: Including Mexico City

	(1) Physical	(2) Sexual	(3) Emotional	(4) Economic
Panel A: Past 12 months				
Post × China-tariff exposure gap	0.37	0.03	0.53	0.19
	(0.24)	(0.13)	(0.52)	(0.35)
Post × Mexico-tariff exposure gap	−0.35*	0.24*	0.59	0.49
	(0.20)	(0.14)	(0.56)	(0.30)
Post × Average China-tariff exposure	−0.26	0.15	0.08	0.05
	(0.29)	(0.15)	(0.65)	(0.38)
Post × Average Mexico-tariff exposure	0.21	−0.15	−0.48	−0.72**
	(0.23)	(0.18)	(0.41)	(0.29)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	162,822	162,822	162,822	162,822
Mean of dep. var. (2016, %)	6.4	2.2	23.9	10.2
Panel B: Lifetime				
Post × China-tariff exposure gap	0.71*	0.23	0.75	0.51
	(0.38)	(0.21)	(0.75)	(0.49)
Post × Mexico-tariff exposure gap	−0.50	0.22	0.83	0.06
	(0.31)	(0.22)	(0.74)	(0.41)
Post × Average China-tariff exposure	−0.10	0.32	1.47	0.39
	(0.46)	(0.27)	(1.01)	(0.61)
Post × Average Mexico-tariff exposure	−0.28	−0.47	−1.10*	−0.54
	(0.39)	(0.28)	(0.64)	(0.50)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	162,822	162,822	162,822	162,822
Mean of dep. var. (2016, %)	17.7	6.6	41.2	21.3

Notes: Each column in each panel reports a difference-in-differences regression for the indicated type of partner violence, adding Mexico City. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. Exposure scales are recalculated for the extended sample. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 582 eligible municipalities and boroughs, including Mexico City, using person weights. The 16 Mexico City boroughs receive a common exposure constructed from citywide employment shares; their survey outcomes remain separate. Of the 582 eligible municipalities and boroughs, 581 have survey observations. Outcome means are weighted 2016 percentages. Standard errors in parentheses cluster all Mexico City boroughs together and other municipalities separately. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A7: Robustness: Excluding overlapping tariff sectors

	Past-year physical violence		Lifetime physical violence	
	(1) Baseline	(2) Exclude overlap	(3) Baseline	(4) Exclude overlap
Post × China-tariff exposure gap	0.46*	0.45**	0.84**	0.85**
	(0.24)	(0.22)	(0.38)	(0.35)
Post × Mexico-tariff exposure gap	−0.38*	−0.12	−0.55*	−0.09
	(0.20)	(0.17)	(0.31)	(0.26)
Post × Average China-tariff exposure	−0.44*	−0.38*	−0.39	−0.40
	(0.26)	(0.23)	(0.43)	(0.36)
Post × Average Mexico-tariff exposure	0.24	0.03	−0.24	−0.47
	(0.24)	(0.17)	(0.40)	(0.30)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016)	6.4	6.4	17.6	17.6

Notes: Each column reports a difference-in-differences regression for the indicated physical partner-violence outcome. Columns (2) and (4) exclude SCIAN sectors 331, 332, 334 and 335, which face both China- and Mexico-tariff shocks, from both China exposure measures. Employment-share denominators and Mexico exposure measures are unchanged. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. Each exposure construction uses its own SD on the same reference sample. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A8: Robustness: Alternative trade weights

Panel A: Past-year physical violence

	(1) Baseline	(2) No trade weight	(3) US exports / output	(4) Exports per worker
Post × China-tariff exposure gap	0.46*	0.33	0.37	0.28
	(0.24)	(0.24)	(0.34)	(0.44)
Post × Mexico-tariff exposure gap	−0.38*	−0.30	−0.21	−0.09
	(0.20)	(0.19)	(0.23)	(0.35)
Post × Average China-tariff exposure	−0.44*	−0.29	−0.47	−0.88**
	(0.26)	(0.23)	(0.31)	(0.37)
Post × Average Mexico-tariff exposure	0.24	0.13	0.28	0.60**
	(0.24)	(0.20)	(0.26)	(0.29)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016)	6.4	6.4	6.4	6.4

Panel B: Lifetime physical violence

	(1) Baseline	(2) No trade weight	(3) US exports / output	(4) Exports per worker
Post × China-tariff exposure gap	0.84**	0.74*	0.30	0.09
	(0.38)	(0.40)	(0.51)	(0.68)
Post × Mexico-tariff exposure gap	−0.55*	−0.46	−0.08	−0.14
	(0.31)	(0.30)	(0.33)	(0.59)
Post × Average China-tariff exposure	−0.39	−0.25	−0.27	−0.67
	(0.43)	(0.38)	(0.49)	(0.60)
Post × Average Mexico-tariff exposure	−0.24	−0.36	−0.32	0.07
	(0.40)	(0.34)	(0.39)	(0.43)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016)	17.6	17.6	17.6	17.6

Notes: Each column in each panel reports a difference-in-differences regression for physical partner violence. Columns change the trade-dependence weight: baseline US export share, no trade weight, US exports over output, and exports per worker excluding oil and gas. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. Each exposure construction uses its own SD on the same reference sample. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A9: Robustness: Alternative employment-share vintages

Panel A: Past-year physical violence

	(1) 2017 average	(2) 2015 average	(3) 2016 average	(4) 2015–17 average
Post × China-tariff exposure gap	0.46*	0.50**	0.47**	0.47**
	(0.24)	(0.22)	(0.23)	(0.23)
Post × Mexico-tariff exposure gap	−0.38*	−0.36*	−0.39**	−0.37*
	(0.20)	(0.20)	(0.20)	(0.20)
Post × Average China-tariff exposure	−0.44*	−0.54**	−0.47*	−0.49*
	(0.26)	(0.27)	(0.26)	(0.26)
Post × Average Mexico-tariff exposure	0.24	0.24	0.24	0.24
	(0.24)	(0.24)	(0.23)	(0.24)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016)	6.4	6.4	6.4	6.4

Panel B: Lifetime physical violence

	(1) 2017 average	(2) 2015 average	(3) 2016 average	(4) 2015–17 average
Post × China-tariff exposure gap	0.84**	1.08***	0.99***	0.98***
	(0.38)	(0.37)	(0.37)	(0.37)
Post × Mexico-tariff exposure gap	−0.55*	−0.55*	−0.57*	−0.55*
	(0.31)	(0.31)	(0.32)	(0.31)
Post × Average China-tariff exposure	−0.39	−0.42	−0.43	−0.43
	(0.43)	(0.43)	(0.43)	(0.43)
Post × Average Mexico-tariff exposure	−0.24	−0.30	−0.25	−0.26
	(0.40)	(0.38)	(0.39)	(0.39)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016)	17.6	17.6	17.6	17.6

Notes: Each column in each panel reports a difference-in-differences regression for physical partner violence. Each column reconstructs both exposure gaps and both average exposures using the indicated IMSS employment-share vintage. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. Each exposure construction uses its own SD on the same reference sample. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A10: Robustness: Alternative exposures and partnership sample

Panel A: Past-year physical violence

	(1) Baseline	(2) 2010 Census formal	(3) IHS tariff shock	(4) Currently partnered
Post × China-tariff exposure gap	0.46*	0.24	0.46*	0.38
	(0.24)	(0.24)	(0.24)	(0.32)
Post × Mexico-tariff exposure gap	−0.38*	−0.19	−0.38*	−0.05
	(0.20)	(0.21)	(0.20)	(0.34)
Post × Average China-tariff exposure	−0.44*	−0.49*	−0.44*	−0.18
	(0.26)	(0.28)	(0.26)	(0.37)
Post × Average Mexico-tariff exposure	0.24	0.49*	0.24	−0.18
	(0.24)	(0.27)	(0.24)	(0.44)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	102,700
Mean of dep. var. (2016)	6.4	6.4	6.4	8.1

Panel B: Lifetime physical violence

	(1) Baseline	(2) 2010 Census formal	(3) IHS tariff shock	(4) Currently partnered
Post × China-tariff exposure gap	0.84**	0.80**	0.85**	0.78*
	(0.38)	(0.39)	(0.38)	(0.42)
Post × Mexico-tariff exposure gap	−0.55*	−0.47	−0.55*	0.01
	(0.31)	(0.31)	(0.31)	(0.41)
Post × Average China-tariff exposure	−0.39	−0.15	−0.39	−0.22
	(0.43)	(0.46)	(0.43)	(0.52)
Post × Average Mexico-tariff exposure	−0.24	−0.04	−0.24	−0.62
	(0.40)	(0.44)	(0.40)	(0.54)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	102,700
Mean of dep. var. (2016)	17.6	17.6	17.6	16.9

Notes: Each column in each panel reports a difference-in-differences regression for physical partner violence. Column (1) uses the baseline exposure construction. Column (2) uses Census 2010 formal-worker employment shares, changing both source and vintage. Column (3) applies the IHS to sector tariff shocks before constructing exposure. Column (4) restricts the sample to currently partnered women. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. Alternative exposure measures in columns (2) and (3) use their own reference SDs; column (4) retains the baseline scales. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Column (4) includes only currently partnered women. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A11: Robustness: Pandemic and concurrent-policy controls

	Past-year physical violence		Lifetime physical violence	
	(1) Matched sample	(2) Joint controls	(3) Matched sample	(4) Joint controls
Post × China-tariff exposure gap	0.46*	0.50**	0.84**	0.93**
	(0.24)	(0.25)	(0.38)	(0.42)
Post × Mexico-tariff exposure gap	−0.38*	−0.50**	−0.55*	−0.52
	(0.20)	(0.21)	(0.31)	(0.36)
Post × Average China-tariff exposure	−0.44*	−0.87***	−0.39	−0.66
	(0.26)	(0.29)	(0.43)	(0.52)
Post × Average Mexico-tariff exposure	0.24	0.25	−0.24	−0.19
	(0.24)	(0.25)	(0.40)	(0.48)
Post × Sector pandemic shock, 2020		−38.61		−136.51***
		(29.06)		(50.00)
Post × COVID deaths per 1,000		−0.25		0.26
		(0.18)		(0.40)
Post × Closure exposure		−1.15		0.26
		(1.87)		(3.19)
Post × Closure exposure gap		4.02		0.24
		(3.51)		(5.91)
Post × Northern border zone		2.07***		2.55**
		(0.58)		(1.23)
Post × Minimum-wage exposure		−0.18		−1.44
		(1.82)		(3.24)
Post × Prospera coverage		−2.78		6.53
		(2.61)		(4.39)
Post × Apparel and textile share		5.22*		2.98
		(2.72)		(4.27)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	155,310	155,310	155,310	155,310
Mean of dep. var. (2016)	6.4	6.4	17.6	17.6

Notes: Each column reports a difference-in-differences regression for the indicated physical partner-violence outcome. Within each outcome group, columns compare the baseline specification and the model with additional controls on the same sample. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. All regressions control for age and education. Additional post interactions cover the sector pandemic shock, COVID deaths, closure exposure and its gender gap, northern border policy, minimum-wage exposure, Prospera coverage and apparel/textile employment. Pandemic measures can be post-treatment; these are conditional comparisons, not a mediation analysis. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. All columns require non-missing additional controls. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A12: Tariff exposure and the severity of physical partner violence

Panel A: Past-year physical violence				
	(1) Mild	(2) Severe	(3) Extreme	(4) Act count
Post × China-tariff exposure gap	0.44*	0.23	0.06	0.012*
	(0.23)	(0.16)	(0.10)	(0.007)
Post × Mexico-tariff exposure gap	−0.39**	−0.12	−0.05	−0.009*
	(0.19)	(0.14)	(0.07)	(0.005)
Post × Average China-tariff exposure	−0.43*	−0.10	−0.06	−0.006
	(0.26)	(0.17)	(0.08)	(0.007)
Post × Average Mexico-tariff exposure	0.31	0.03	−0.00	0.004
	(0.25)	(0.12)	(0.06)	(0.005)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016)	6.2	3.2	0.8	0.148
Panel B: Lifetime physical violence				
	(1) Mild	(2) Severe	(3) Extreme	(4) Act count
Post × China-tariff exposure gap	0.72*	0.43*	0.28*	0.019
	(0.37)	(0.24)	(0.15)	(0.012)
Post × Mexico-tariff exposure gap	−0.50	−0.22	−0.01	−0.010
	(0.31)	(0.24)	(0.15)	(0.010)
Post × Average China-tariff exposure	−0.25	−0.27	0.07	−0.004
	(0.43)	(0.29)	(0.20)	(0.013)
Post × Average Mexico-tariff exposure	−0.20	−0.04	−0.40*	−0.010
	(0.39)	(0.27)	(0.21)	(0.013)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016)	16.8	9.5	3.7	0.483

Notes: Each column in each panel reports a difference-in-differences regression for the indicated measure of physical partner violence. Columns report mild, severe and extreme indicators and the count of reported acts; severity tiers may overlap. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are per one-SD increase: percentage points in columns (1)–(3), numbers of acts in column (4). All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Outcome means are weighted 2016 means in percent in columns (1)–(3) and numbers of acts in column (4). Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A13: Tariff exposure and lifetime violence across settings

	(1) Partner	(2) Community	(3) Workplace	(4) School
Post × China-tariff exposure gap	0.84**	−0.00	0.36	0.71
	(0.38)	(0.77)	(0.44)	(0.61)
Post × Mexico-tariff exposure gap	−0.55*	0.50	0.80*	−0.13
	(0.31)	(0.60)	(0.48)	(0.55)
Post × Average China-tariff exposure	−0.39	−0.33	0.82	0.03
	(0.43)	(0.74)	(0.56)	(0.67)
Post × Average Mexico-tariff exposure	−0.24	0.12	0.69	−0.59
	(0.40)	(0.56)	(0.83)	(0.52)
Municipality FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes
Observations	156,267	156,267	156,267	156,267
Mean of dep. var. (2016)	17.6	41.4	13.3	24.4

Notes: Each column reports a difference-in-differences regression for lifetime violence in the indicated setting. The partner outcome measures physical violence; the non-partner indicators cover the available setting-specific violence measures, not necessarily physical violence alone. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

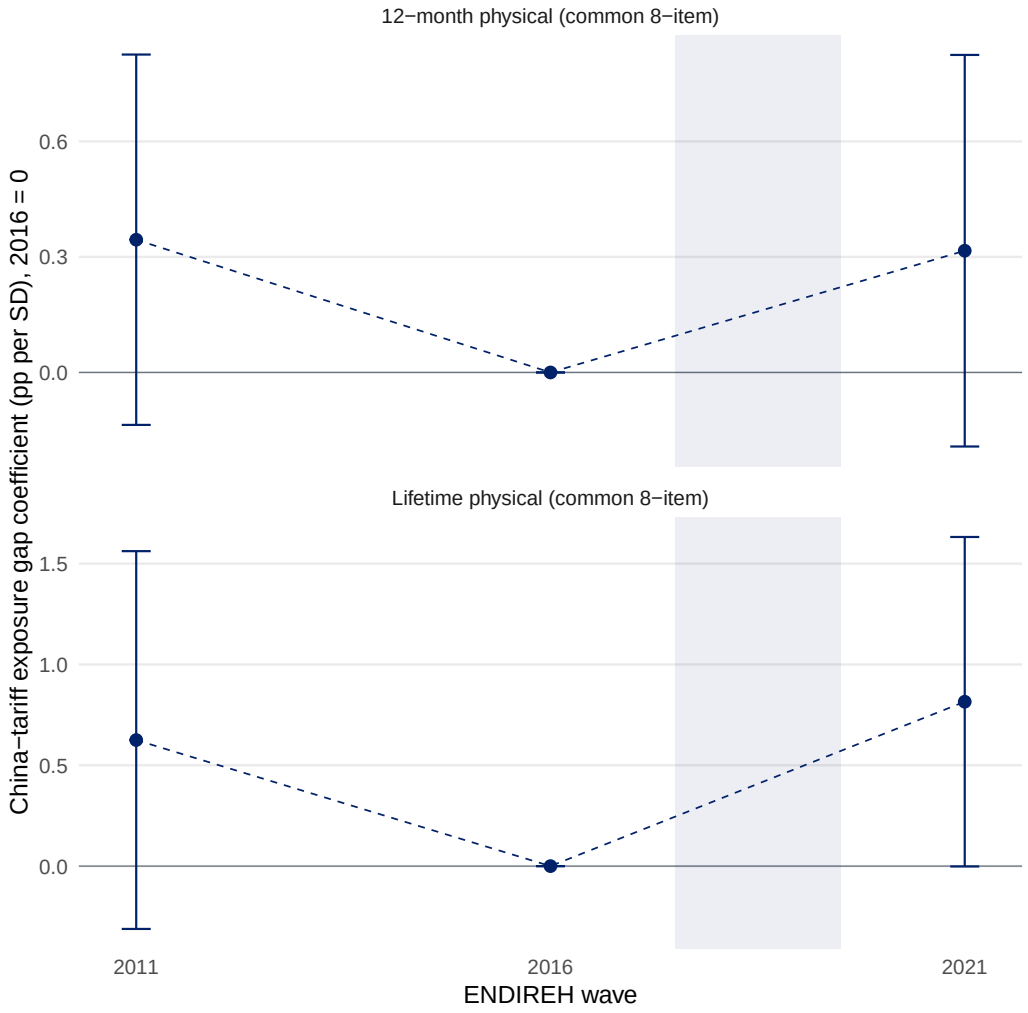
Table A14: Tariff exposure and reports of childhood violence

	(1) Physical	(2) Emotional
Post × China-tariff exposure gap	0.40	−0.23
	(0.52)	(0.42)
Post × Mexico-tariff exposure gap	−0.57	0.02
	(0.42)	(0.42)
Post × Average China-tariff exposure	−0.89	−0.31
	(0.56)	(0.52)
Post × Average Mexico-tariff exposure	−0.64	−0.25
	(0.52)	(0.59)
Municipality FE	Yes	Yes
Wave FE	Yes	Yes
Observations	156,267	156,267
Mean of dep. var. (2016)	32.3	18.2

Notes: Each column reports a difference-in-differences regression for recalled physical or emotional violence by household members before age 15. The events precede the tariff shock, but retrospective reporting and sample composition may change. All columns include both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. Exposure coefficients and standard errors are in percentage points per one-SD increase. All regressions control for age and education. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Outcome means are weighted 2016 percentages. Standard errors in parentheses are clustered by municipality. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

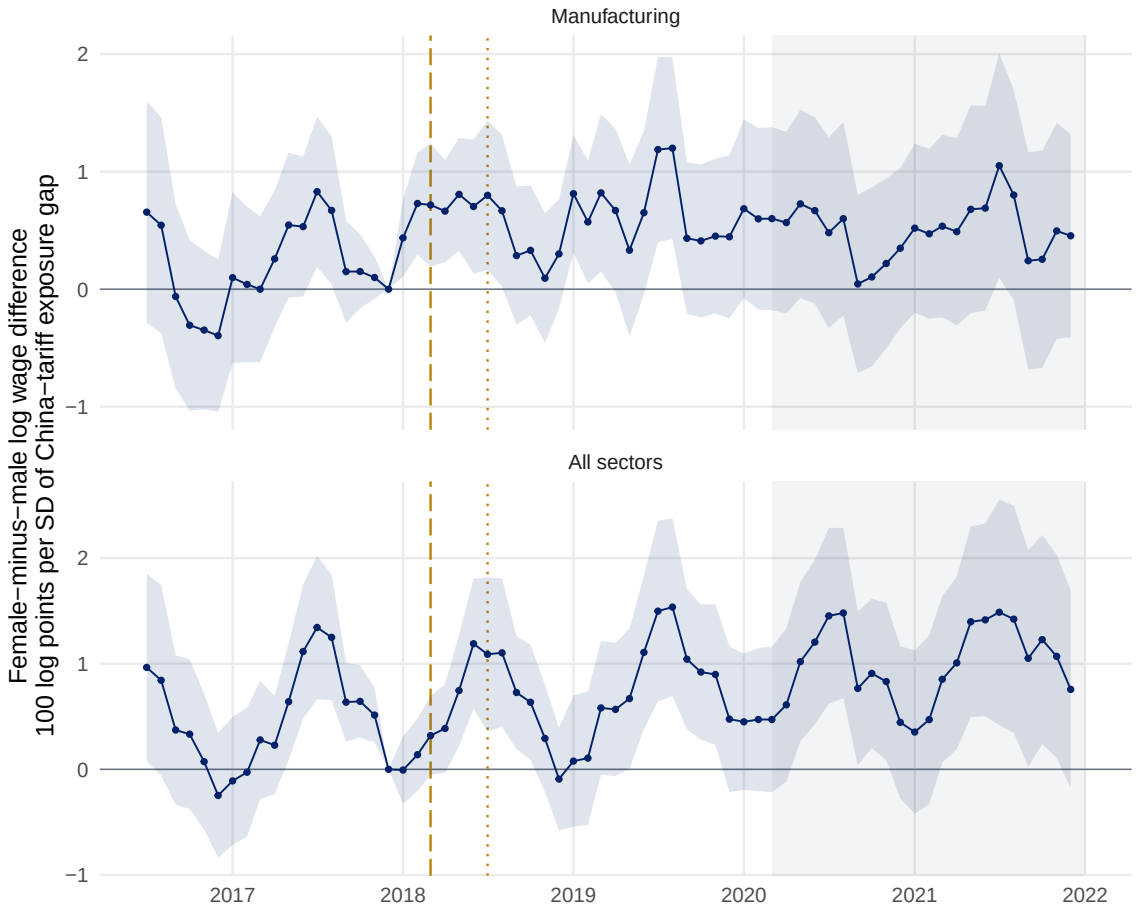
D Additional Figures

Figure A1: Tariff exposure and physical partner violence, 2011–2021



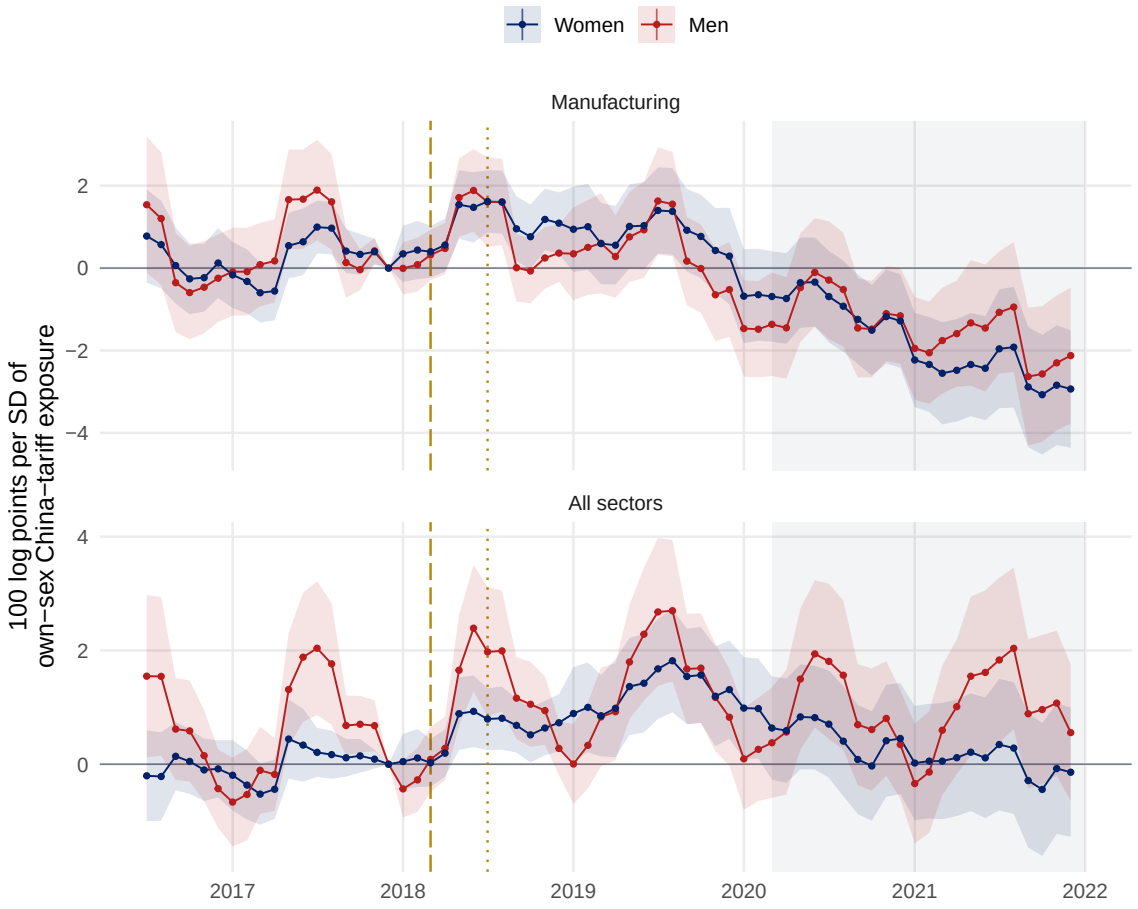
Notes: Points report China-tariff exposure-gap coefficients for physical partner violence measured with eight common questions, with 2016 as reference. Both exposure gaps and both average exposures interact with wave. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Coefficients are in percentage points per one-SD increase in the original three-wave reference sample. All regressions control for age and education. All regressions include municipality and state-by-wave fixed effects. The sample comprises ever-married or cohabiting women, including former unions, in ENDIREH 2011, 2016 and 2021 in the 566 eligible municipalities, using dwelling weights in 2011 and person weights in later waves. Survey design and the reference universe differ from the headline sample. Bars show pointwise 95% normal-approximation confidence intervals with standard errors clustered by municipality.

Figure A2: China-tariff exposure gap and the female–male formal wage gap



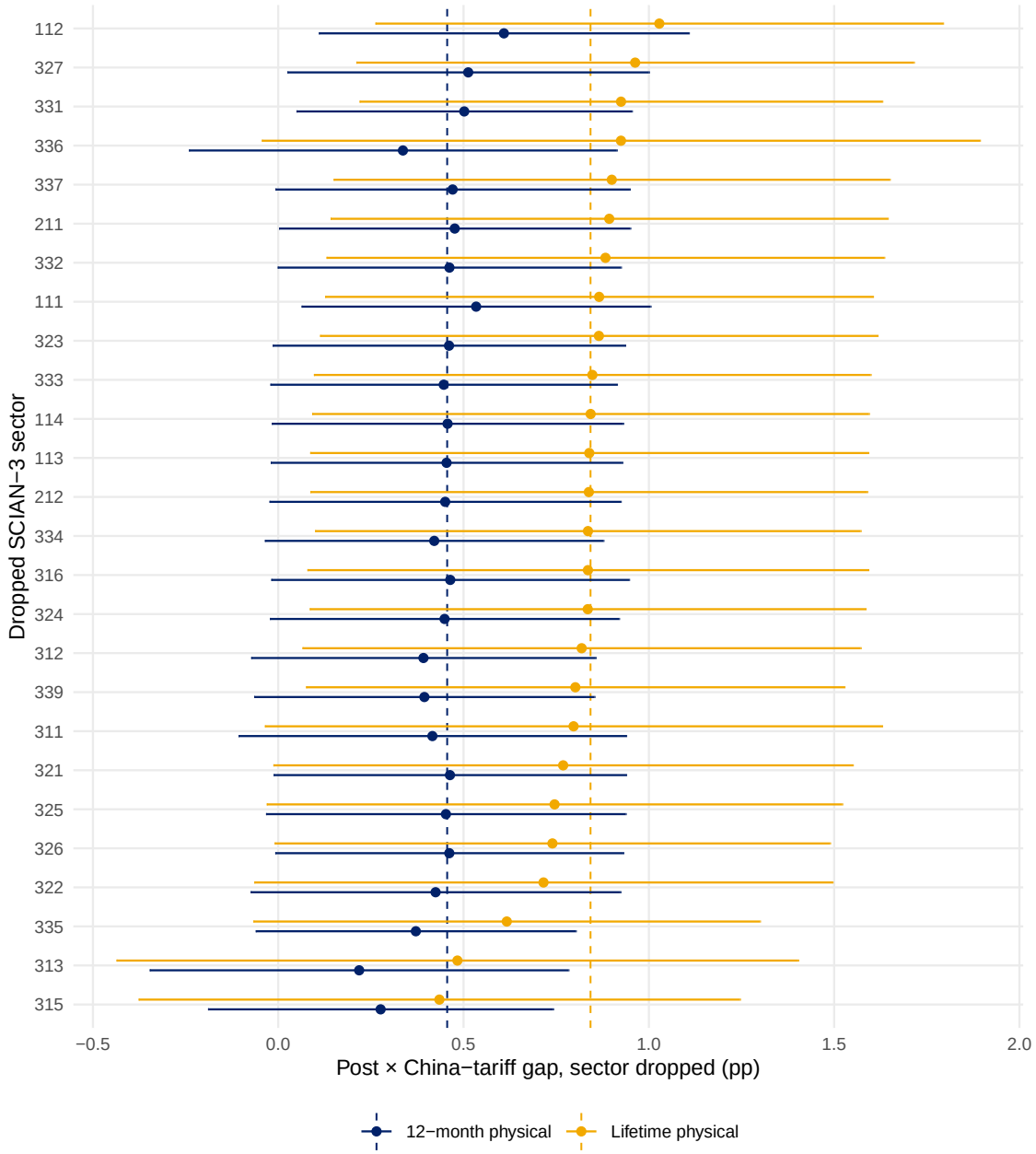
Notes: Separate regressions for manufacturing and all sectors relate log female minus log male mean daily contribution wages to month interactions with both exposure gaps and both average exposures. Points report the China-gap coefficients relative to December 2017, in log points per one-SD increase. Coefficients and confidence intervals are multiplied by 100. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Exposure scales use the ENDIREH reference sample of Table 1. All regressions include municipality and month-by-year fixed effects. The sample comprises municipality-month observations from IMSS administrative records in the 566 eligible municipalities, from July 2016 to December 2021, using equal weights. Each regression retains observations with positive wages for both sexes in the indicated sector group; the manufacturing sample therefore differs from the separate-sex samples in Figure 2. Vertical lines mark March and July 2018; grey shading begins in March 2020. Shaded bands show pointwise 95% normal-approximation confidence intervals with standard errors clustered by municipality.

Figure A3: Formal wages by sex: own-sex tariff exposure



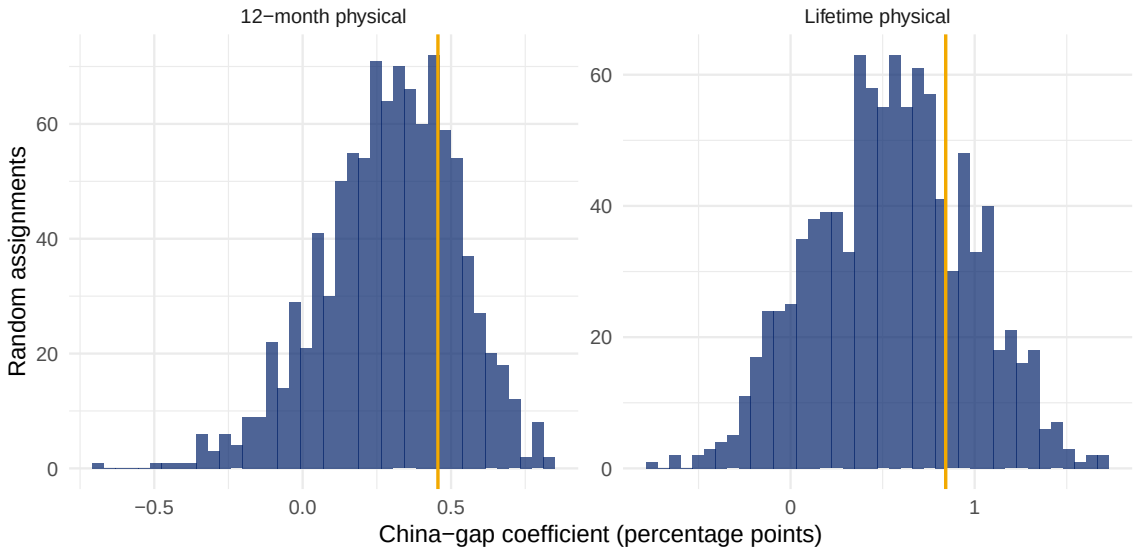
Notes: Separate regressions for women and men in manufacturing and all sectors include month interactions with own-sex China- and Mexico-tariff exposures. Points report the own-sex China coefficients. Coefficients describe log mean daily contribution wages relative to December 2017, in log points per one-SD increase in exposure. Coefficients and confidence intervals are multiplied by 100. Exposure scales use the ENDIREH reference sample of Table 1. All regressions include municipality and month-by-year fixed effects. The sample comprises municipality-month observations from IMSS administrative records in the 566 eligible municipalities, from July 2016 to December 2021, using equal weights. Each regression retains observations with positive wages for the indicated sex and sector group. Vertical lines mark March and July 2018; grey shading begins in March 2020. Shaded bands show pointwise 95% normal-approximation confidence intervals with standard errors clustered by municipality.

Figure A4: Physical partner violence: omitting individual sectors from China exposure



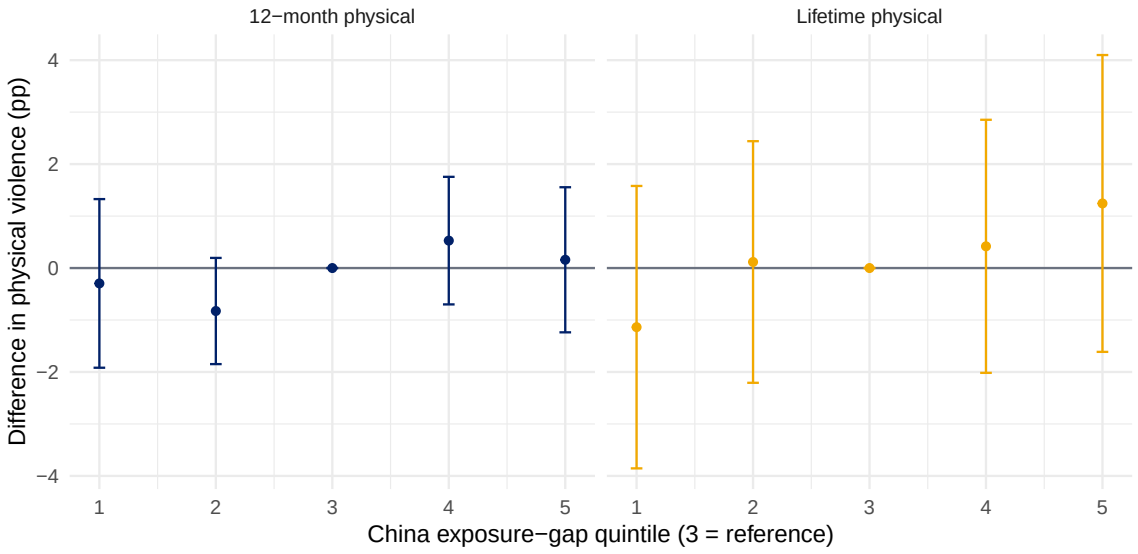
Notes: Points report China-tariff exposure-gap coefficients for physical partner violence after omitting the indicated sector from the China gap. The Mexico gap and both average exposures remain fixed. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Coefficients are in percentage points per one-SD increase; each omitted-sector gap uses its own reference SD. All regressions control for age and education. All regressions include municipality and wave fixed effects. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Dashed lines show the baseline coefficients. Bars show pointwise 95% normal-approximation confidence intervals with standard errors clustered by municipality.

Figure A5: Physical partner violence under reassigned China tariffs



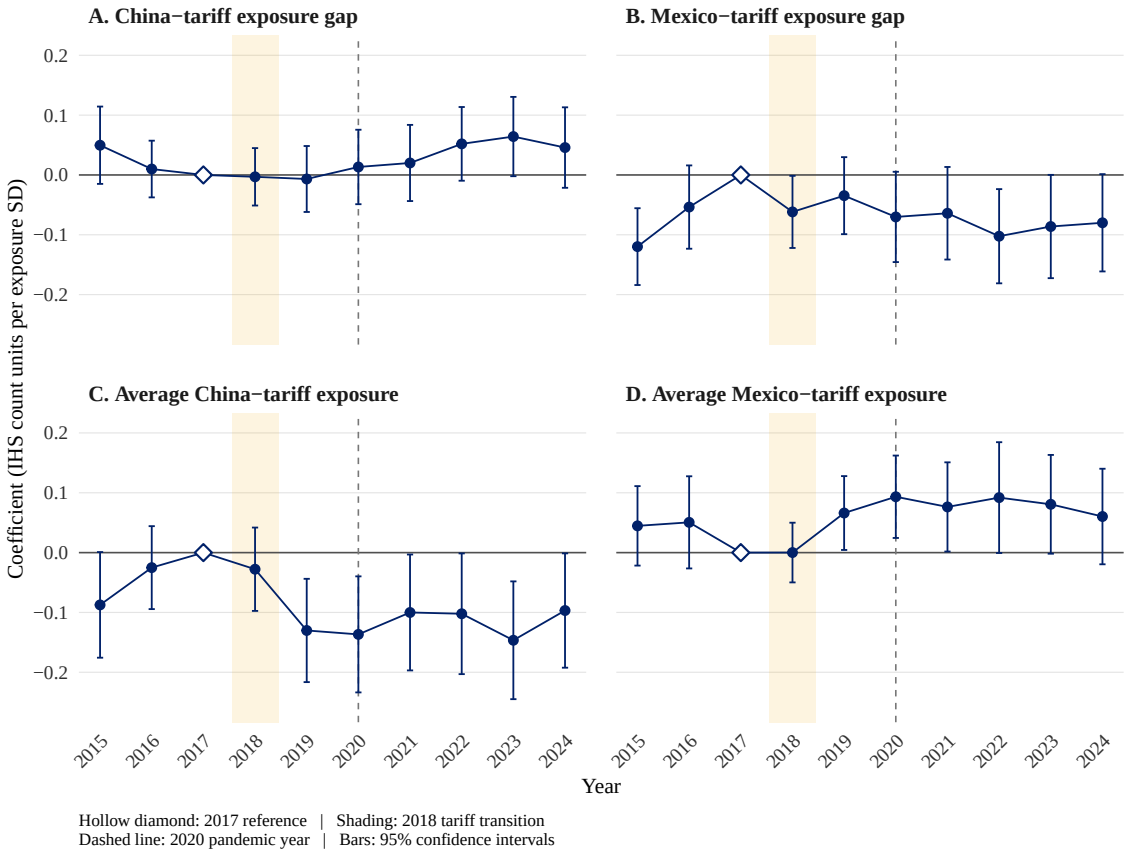
Notes: Histograms show physical-violence coefficients under 1,000 assignments of the observed nominal China tariff values to the employment-sector universe. Employment shares, trade weights and the other three exposure regressors are held fixed. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Coefficients are in percentage points per one-SD increase; each assigned China gap uses its own reference SD. All regressions control for age and education. All regressions include municipality and wave fixed effects. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Vertical lines mark the actual-assignment coefficients. This conditional reference distribution is not a design-based randomization test.

Figure A6: Physical partner violence by China-tariff exposure-gap quintile



Notes: Points report post-period coefficients for China-gap quintiles relative to the middle quintile, in percentage points. Quintiles use the unweighted distribution of China-gap exposure across women in the ENDIREH reference sample of Table 1. The Mexico gap and both average exposures are retained. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Post indicates 2021. All regressions control for age and education. All regressions include municipality and wave fixed effects. The sample comprises ever-partnered women in ENDIREH 2016 and 2021 in the 566 eligible municipalities, using person weights. Bars show pointwise 95% normal-approximation confidence intervals with standard errors clustered by municipality.

Figure A7: Tariff exposure and recorded family-violence offenses



Notes: Panels report annual coefficients from one regression of the IHS of recorded family-violence offense counts on year interactions with both exposure gaps and both average exposures. Exposure gaps are female minus male exposure; average exposure is the mean of female and male exposure. Coefficients are in IHS count units per one-SD increase, not percentages. Exposure scales use the ENDIREH reference sample of Table 1. All regressions include municipality and year fixed effects. The sample comprises 5,630 observed municipality-years from SESNSP municipal crime records in 565 eligible municipalities from 2015 to 2024, using equal weights. Reported zeros are retained; missing records are not coded zero. Records reflect reporting and enforcement and are not restricted to female victims or intimate partners. The 2017 coefficients are normalized to zero, shown as hollow diamonds without confidence intervals. All panels use the same vertical scale. Shading marks the 2018 tariff-transition year; dashed lines mark 2020, the first pandemic year. Unlike Table 5, the annual regression includes 2018 and extends through 2024. Bars show pointwise 95% normal-approximation confidence intervals with standard errors clustered by municipality.