

When Protection Fails: Military Bases and Sexual Violence in Colombia*

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Abstract

Government soldiers are among the most frequently reported perpetrators of sexual violence in armed conflict, yet little is known about how expanding state military presence affects civilians. Using a novel municipality-level panel that tracks Colombian army bases from 1998 to 2016 and a staggered difference-in-differences design, we estimate the effects of military base expansion. Base introduction sharply increases recorded sexual violence, implying cumulative rises of 52% in registered and 72% in indicted cases over sixteen years. Reporting-sensitive crimes show no comparable increase. We interpret this as causal evidence that military base presence raises civilian exposure to recorded sexual violence.

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1 Introduction

War and sex are inextricably linked, extending beyond the use of sexual violence as a weapon against combatants and civilians on the enemy side. Sexual abuse and violence by government forces against their own citizens are common across historical and contemporary conflicts. Examples range from Union soldiers’ sexual assaults on civilian women in the South during the American Civil War to government soldiers raping civilian women in their homes and internal refugee camps in the Democratic Republic of Congo (Barber and Ritter, 2015; Human Rights Watch, 2014).¹ Consistent with these historical accounts, cross-country data on conflict-related sexual violence show that state armed actors are frequently reported perpetrators of wartime sexual crimes (Cohen and Nordås, 2014).² Furthermore, while individuals of any sex, sexual orientation, and gender identity can be affected, women and girls are disproportionately the victims of known sexual violence in conflict and post-conflict settings (Cohen, Green, and Wood, 2013).³

Understanding the relationship between military presence and sexual violence is important not only because of its moral and legal implications, but also because of its broader consequences for economic and political development. Recent research shows that sexual violence can disrupt labour market participation and reduce victims’ long-run earnings (Sabia, Dills, and DeSimone, 2013; Adams-Prassl et al., 2024; Adams et al., 2024). More broadly, violence committed by state actors may undermine state legitimacy, weaken citizens’ trust in institutions, and alter relations between citizens and the state, with potential downstream consequences for political and economic development (De Juan and Pierskalla, 2016; Voytas and Crisman, 2024; Acemoglu, Johnson, and Robinson, 2005).⁴

This paper asks a question: *What are the consequences of military base presence for sexual violence in host communities?* We study this question in Colombia, a country with a long history of internal armed conflict and documented allegations of sexual violence committed by members of the public forces. Media reports describe multiple cases in which Colombian soldiers sexually assaulted female civilians, often minors, during both the armed conflict

¹Bastick, Grimm, and Kunz (2007) provide a comprehensive summary of countries with civil conflicts in which soldiers of official forces commit sexual violence against civilians.

²Figure A.1 in the Appendix presents the share of conflicts involving reported sexual violence by types of armed actors from 1989 to 2021. Among 189 conflicts involving state military forces, 49.2% included reports of sexual violence by government soldiers. In contrast, the corresponding shares are lower for rebel groups (31.8%) and militias (29.6%).

³Annual reports analysing cases of conflict-related sexual violence globally from 2019 to 2023 consistently show that over 94% of victims are female (United Nations Secretary-General, 2020; United Nations Secretary-General, 2021; United Nations Secretary-General, 2022; United Nations Secretary-General, 2023; United Nations Secretary-General, 2024).

⁴These broader consequences motivate our focus but lie outside the scope of our estimates: we do not study political or developmental outcomes directly.

and the post-conflict period (Reuters, 2020; Oquendo, 2020; Turkewitz, 2020). Recognizing the severity of these allegations, Colombia’s Special Jurisdiction for Peace has launched investigations into conflict-related sexual violence committed by all armed actors, including the public forces (JEP, 2023a). We examine whether the introduction of military bases affected recorded sexual violence, and where any effect is concentrated across military units staffed primarily by drafted versus professional soldiers.

Estimating the causal effects of military presence presents two major empirical challenges. The first is the absence of comprehensive historical data on the location of military bases.⁵ To address this challenge, we construct a novel municipality-year dataset on military base presence between 1998 and 2016 using newspaper archives, military organizational records, congressional documents, legislative materials, and historical sources. The resulting dataset tracks the location and timing of brigade and battalion headquarters across Colombian municipalities and provides a new measure of military presence over time.

The second challenge is that military bases were not placed randomly. Military expansion was shaped by counterinsurgency strategies and evolving conflict conditions, creating concerns about both selection and staggered treatment timing. To address these concerns, we exploit temporal and geographic variation generated by Colombia’s large-scale military expansion between 2000 and 2016 and estimate dynamic treatment effects using the estimator developed by de Chaisemartin and D’Haultfœuille (2024). This approach compares municipalities that change treatment status to municipalities with the same baseline treatment status that have not yet changed treatment, allowing us to account for staggered treatment adoption while avoiding problematic comparisons common in conventional two-way fixed-effects estimators.

We find that military base presence substantially increased recorded sexual violence. Our estimates imply a cumulative increase of approximately 52% in registered and 72% in indicted sexual violence cases relative to pre-treatment means over the sixteen years following base introduction. This increase is largest in the first years after base arrival and attenuates thereafter, consistent with an acute response to base introduction rather than a permanent shift in its level. The effects are concentrated around standing military units staffed primarily by drafted soldiers. We report this heterogeneity to show where the response is concentrated, but interpret it descriptively rather than causally, since base type is not randomly assigned and unit types differ not only in personnel composition but also in strategic role, deployment patterns, and local conflict dynamics. By contrast, we find little evidence that military base presence affected fertility.

⁵We requested complete historical records on the location of army bases from the Colombian Ministry of Defence, but our request was denied.

Several findings support a causal interpretation of these results. Pre-treatment event-study estimates are small and jointly statistically indistinguishable from zero. We also find little evidence that military bases were systematically introduced in municipalities experiencing rising combat intensity, and our results remain similar after allowing municipalities with different pre-introduction combat intensity to follow different trends. Annual event-study estimates likewise provide little indication that the two-year aggregation masks pre-treatment dynamics. At the same time, Roth–Rambachan sensitivity analyses indicate that statistical significance becomes sensitive to moderate violations of parallel trends, suggesting that caution remains warranted when interpreting the precision of the estimates (Rambachan and Roth, 2023).

Interpreting this increase requires care, since judicial records reflect both underlying incidents and reporting behaviour. We therefore examine other explanations for the increase we attribute to base presence, including changes in reporting, recorded civilian population, and economic migration, and find that they do not account for the timing and pattern of the effects. Reporting-sensitive crimes that would also respond to a general change in reporting, such as non-homicide violence and extortion, show no comparable movement, which weighs against a reporting-driven explanation. Homicide, however, is far less sensitive to reporting, because registering a homicide requires a body or other physical evidence rather than a victim’s complaint. It increases alongside recorded sexual violence around the same units. We read this not as a rival explanation but as evidence of a possible deterioration in local security that may be one channel through which base presence raises sexual violence. We therefore interpret our estimates as causal evidence that military base presence increased civilian exposure to recorded sexual violence.

This interpretation concerns the effect of base presence, not the identity of those responsible. The data do not identify perpetrators, and historical evidence documents sexual violence by government soldiers as well as collaboration between some military units and paramilitary groups; thus, the increase may reflect the conduct of multiple actors. We therefore read the estimates as evidence on the consequences of introducing military bases into local communities rather than on who commits the recorded crimes.

Our paper contributes to three strands of literature. First, we contribute to the literature on conflict-related sexual violence. Political scientists have long documented sexual violence committed by state armed forces and other armed actors in civil conflicts (Leiby, 2009; Cohen, 2013; Cohen and Nordås, 2014; Nordås and Cohen, 2021).⁶ Recent economics work has begun to study the determinants of this variation. Most closely related, Guarnieri

⁶Nordås and Cohen (2021) provide a comprehensive review of the political science literature on conflict-related sexual violence.

and Tur-Prats (2023) show that conflict-related sexual violence is shaped by gender norms: armed actors with more gender-unequal norms are more likely to perpetrate sexual violence, and sexual violence is more intense when perpetrators are more gender-unequal than their victims. Laurent-Lucchetti, Girard, and Fourati (2023) identify an economic incentive channel, showing that increases in the value of artisanal gold mining raise conflict-related sexual violence, consistent with armed groups using sexual violence as a non-lethal tool to terrorize civilians and enforce resource extraction. Our paper highlights another determinant of conflict-related sexual violence: state military presence. Rather than explaining variation across armed actors, cultural distance, or resource environments, we estimate how the introduction of military bases affects recorded sexual violence within host communities. In doing so, we provide causal evidence that military infrastructure can increase civilian exposure to sexual violence.

Second, we contribute to the literature on the consequences of military bases. Existing economic research has largely focused on the economic effects of military-base openings and closures, particularly in the United States and Europe (Andersson, Lundberg, and Sjöström, 2007; Paloyo, Vance, and Vorell, 2010; Zou, 2018). Although historians and social scientists have documented links between military bases and local sex industries, economics research on their broader social consequences remains limited. One notable exception is Brodeur, Lekfuangfu, and Zylberberg (2017), who connect U.S. military presence to the growth of Thailand’s sex industry. We contribute to this literature by examining how military bases affect recorded sexual violence in host communities, rather than consensual commercial sex.

Third, our findings speak to a broader literature on state violence and state-citizen relations, which examines how violence and government action shape citizens’ trust in institutions and political participation (Blattman, 2009; Bateson, 2012; De Juan and Pierskalla, 2016; Dell and Querubin, 2018). While that literature has largely focused on political outcomes, we document a more direct civilian cost of expanding state presence, complementing work on the unintended consequences of efforts to strengthen state control.

The remainder of this paper is structured as follows: Section 2 describes the military expansion in Colombia that began in 2000, leading to the establishment of numerous new military bases. Section 3 details our data sources and construction method, and describes the municipalities in our sample. Section 4 discusses our empirical strategy for identifying the causal effects of military base presence. Section 5 presents our results, and discusses their robustness. Finally, Section 6 concludes with a summary of our findings and their implications for future research.

2 Context: Military Expansion in the 2000s in the Colombian Conflict

In the early 2000s, the Colombian government undertook a major military expansion aimed at reclaiming territorial control from insurgency groups and reestablishing state authority in conflict-affected regions. This initiative began under President Andrés Pastrana and intensified significantly under President Álvaro Uribe, whose Democratic Security Policy prioritized the deployment of state forces to areas with limited government presence. The strategy followed a period of intensified confrontations with numerous guerrilla groups in the late 1990s and early 2000s. It signalled a turning point in Colombia’s internal conflict, marked by a shift toward a more proactive and geographically expansive military posture. To implement this policy, the Colombian Army expanded its reach through an unprecedented increase in bases and personnel.⁷ Between 1998 and 2016, the number of municipalities hosting military bases more than doubled, growing from 69 to 145 (Figure 1).⁸

We define the term *military base* to mean the physical main centre of either a brigade or battalion. The creation of new brigades and battalions necessitated a rapid increase in military infrastructure across the country. In many cases, bases were operationalized before physical facilities were constructed: soldiers were deployed immediately to the designated areas, initially camping in the field while carrying out missions and building base infrastructure concurrently.⁹ This logistical flexibility enabled the army to establish sustained operational presence in strategic municipalities in a short period of time.

To further explain the nature of military bases, we first briefly discuss the organizational structure of the Colombian National Army, and then describe the soldiers staffing these units.¹⁰ The Colombian National Army is structured into divisions, brigades, and battalions, each with large numbers of soldiers stationed on their bases. Importantly, there are two

⁷To support this expansion, the Colombian government also received material and financial assistance from the United States, particularly under the framework of Plan Colombia. While U.S. support provided critical equipment, training, and funding, the planning and execution of base expansion were largely directed by Colombia’s internal security and counterinsurgency strategies. Figure A.2 in the Online Supplemental Appendix plots U.S. military aid through the State and Defense Departments, showing a pronounced increase beginning in 2000.

⁸Figure A.3 in the Supplemental Appendix shows the geographical distribution of military bases over the map of Colombia, and shows that this expansion spanned all regions of the country and was not confined to any single division of the Army.

⁹Figure A.4 in the Online Supplemental Appendix shows a photograph of the Julio Londoño Battalion base, newly established in 2007 in the Department of Chocó. The image captures rows of military tents and prefabricated wooden structures erected on recently deforested land, with tree stumps and cleared vegetation visible.

¹⁰Section A.3.1 in the Online Supplemental Appendix presents a detailed overview of the National Army of Colombia.

broad types of military units: standing brigades and battalions, and mobile brigades and counterinsurgency battalions. This distinction reflects both the strategic purpose of the units and the type of soldiers who staff them, which is central to our analysis.

Standing brigades and battalions are fixed territorial units, primarily staffed by drafted soldiers serving mandatory 18–24 month terms. These young conscripts are typically assigned to guard infrastructure such as roads and electrical systems and to conduct localized counterinsurgency operations. By contrast, *mobile brigades and counterinsurgency battalions* specialize in guerrilla warfare and are predominantly staffed by professional soldiers. These soldiers serve long-term contracts of up to 20 years, receive intensive training, and are better compensated, enabling the army to deploy them flexibly in conflict zones such as mountains and jungles.

The difference between drafted and professional soldiers is thus substantial. Drafted soldiers are numerous but less trained and poorly compensated, while professional soldiers represent a smaller but highly skilled and better-paid segment of the army. We use these differences to examine whether the effects of military base presence are more pronounced around bases staffed by particular types of soldiers.

Given the context, the introduction of a military base in a municipality can be characterized as the arrival of a group of young men who are visible outsiders in uniforms, associated with the central government through their membership in the army, and who have a regular, albeit small, monthly cash inflow. Their presence can affect host community women through various channels, including non-consensual and consensual sexual relations, which can then manifest as changes in sexual violence and fertility patterns.

3 Data

One of the key contributions of this paper is the development of a unique municipality-level dataset on the presence of military bases from 1998 to 2016, constructed primarily from national and local newspaper articles and official publications of the Colombian National Army.

3.1 Treatment Data: Military Bases

We construct the data on military base presence from the combination of national and local newspaper articles published between 2000 and 2010, along with government publications and other archival sources.¹¹ Drawing from a diverse set of materials allows us to cross-verify

¹¹The ideal source for this information would be a legislative document detailing the opening or closure of military units. We requested these documents from the Ministry of Defence of Colombia, but our requests

the timing and location of base operations.

Our data collection starts with two snapshots of all army bases as of 1998 and 2016. These provide us with a universe of military installations at the start and end of our analysis period. We then use newspaper sources to identify when and where new bases were established between these two points.

The primary source for newspaper coverage is the Digital Press Archive maintained by the Popular Research and Education Center / Program for Peace (Cinep/PPP). The database provides access to over 600,000 digitized publications from 10 national and regional newspapers since 1992, categorized into five groups: 1) church and conflict, 2) politics and government, 3) drug trafficking, 4) society and culture, and 5) ecology and environment. A sub-category, armed conflict and actions for peace, makes the database particularly relevant for this project. We restricted our search to articles published between January 1, 2000, and December 31, 2010, and used two Spanish keywords to narrow our search for relevant articles: brigade (*brigada*) and battalion (*batallón*).

This search yielded approximately 11,000 scanned newspaper articles containing either or both keywords. We used Google Cloud Vision to extract text from the scanned articles, followed by a combination of ChatGPT-assisted and manual text processing, along with cross-verification using external sources, to build a municipality-year panel dataset that tracks the presence of brigades and battalions over time and space.¹² Further details on this process are provided in Section A.3.2 in the Online Supplemental Appendix.

3.2 Outcome Data

We now discuss the sources of the outcome data and the construction of the outcome variables.¹³

Sexual Violence Data. We obtained the data on sexual crime recorded between 2000 and 2021 from the Office of the Attorney General of Colombia, which records all criminal lawsuits through its mandate to investigate crimes, prosecute offenders, and review judicial processes. A case is registered when an investigation is either initiated by the office or triggered by a citizen report to the police or the Attorney General. There are two types of cases: registered (*procesos*), where the office formally records a reported case, and indicted (*indiciados*), where a suspect has been formally charged.¹⁴

were denied multiple times.

¹²We documented sources for each military base, which are available upon request.

¹³Table B.1 in the Appendix provides an overview, including the available years for each data source, and the years that overlap with the treatment data years and therefore are used in the current paper.

¹⁴We exclude one municipality-year observation for *indicted* sexual violence in Vistahermosa in 2007. This observation reports 3,304 indicted sexual violence cases per 100,000 inhabitants, more than six times

Registered sexual-violence cases in these data capture incidents that entered the criminal-justice system through the Attorney General’s Office. Under the Attorney General’s procedural framework, a criminal report (*noticia criminal*) may originate from a complaint (*denuncia*) submitted verbally or in writing, as well as from reports by police or other authorities. The complaint receiver is instructed to collect information from the complainant and complete the Single Criminal Report Form (*Formato Único de Noticia Criminal*), after which the case is assigned for investigation (Fiscalía General de la Nación, 2009). For sexual offenses, the Attorney General’s Office has also issued specific complaint-intake guidance, and victims may be served through specialized Centers for Comprehensive Attention and Investigation for Victims of Sexual Crimes (*Centros de Atención e Investigación Integral a las Víctimas de Delitos Sexuales*, CAIVAS), where available (Fiscalía General de la Nación, 2016; Fiscalía General de la Nación, 2018). Thus, we interpret the data as records of cases that became known to the Attorney General’s Office through ordinary or specialized intake channels, rather than as a complete measure of underlying victimization.

The 2000–2010 dataset was pre-aggregated by violated statute and by the year and municipality in which the crime occurred. It includes only the number of cases per law per municipality per year. The 2009–2021 dataset was similarly aggregated. For both, we counted the number of registered and indicted cases for sexual crimes per municipality per year, and computed rates per 100,000 inhabitants using annual municipal population estimates.¹⁵

There are two important considerations regarding the judicial records on sexual crimes. First, sexual crimes are widely known to be underreported, particularly in conflict settings such as Colombia, due to stigma, entrenched gender norms, and weak institutional response (Rennison, 2002; Amnesty International, 2011; Abrahams et al., 2014; Perreault, 2015; Borumandnia et al., 2020). Nevertheless, we argue that the Attorney General’s records offer the most suitable dataset for our research question. Alternative sources, such as the Ministry of Health’s diagnostic registry, are temporally limited, given that the data before 2009, when many military bases were established, are unavailable.¹⁶ They suffer from similar or greater barriers to reporting, as victims often do not seek medical care following assault

the next-largest value in the sample. Because indicted cases are a subset of registered cases, this value is implausible: the same municipality-year records only 63.39 registered cases per 100,000 inhabitants, so the indicted figure almost certainly reflects a data-entry or aggregation error.

¹⁵We use total population rather than female population as the denominator to maintain consistency with other crime outcomes, which are not gender-specific.

¹⁶The temporal limitation of the health data also prevents us from examining gender-disaggregated health or mental health outcomes as downstream consequences of sexual violence. To our knowledge, no municipality-level mental health data suitable for quantitative analysis of this kind exist for the relevant period.

due to cost, inaccessibility, or stigma (Center for Reproductive Rights, 2020). Thus, the health registry does not necessarily provide a more accurate or complete picture than the legal records.

Underreporting is an important limitation, though its implications for our estimates depend on the structure of the reporting process, not on the level of underreporting alone. If the share of incidents that go unrecorded is stable and does not change with base introduction, a constant fraction of cases is missing in every period, and our estimates recover the effect on underlying incidents up to a scaling factor; the reported-case results would then understate the true effect and can be read as a lower bound. If instead base presence itself shifts reporting behaviour, the direction of the bias is no longer signed: base introduction could suppress reporting (if victims fear retaliation or distrust military authorities) or raise it (if proximity to state institutions lowers the cost of filing a report), so the recorded effect could over- or understate the effect on incidents. We therefore do not treat the lower-bound interpretation as automatic, and we probe the reporting channel directly in Section 5.4 using outcomes subject to similar reporting dynamics.

Second, we consider the sexual crime outcomes as “women-related” in this particular context, because the overwhelming majority of known sexual crime cases involve women as victims. Official investigations report that 85–89% of sexual violence cases in Colombia involved female victims, and at least one such study relied on the same data from the Attorney General’s Office used here (JEP, 2023b; Amnesty International, 2011). While we recognize that sexual violence affects people of all gender identities and sexual orientations, official statistics significantly underrepresent cases involving gender and sexual minorities due to stigma and institutional invisibility (Colombia Diversa, Caribe Afirmativo, and Santamaría Fundación, 2015). These dynamics are especially relevant for violence committed by non-state armed actors. Although our data do not allow us to disaggregate by victim gender, the overwhelming majority of recorded cases likely involve women and girls.

Fertility Data. We sourced our fertility data from Colombia’s complete set of birth certificates, provided by the National Department of Statistics (DANE). This dataset includes detailed information on births, maternal and paternal attributes, and miscarriages from 1998 to 2022. To estimate pregnancy rates across Colombian municipalities, we used these data from 1998 to 2016 to first obtain the *number of conceptions*. We define the date of conception by subtracting 10 months before the date of delivery, the average gestation period in Colombia.¹⁷ We then divide the number of conceptions by age-appropriate population to

¹⁷The average gestation period is based on vital statistics showing that approximately 98% of pregnancies last longer than 9 months but less than 10 months. Additionally, the national average gestation length in Colombia is 38.82 weeks, or 9.71 months (Pinzón-Rondón et al., 2015).

obtain pregnancy rates.

The availability of maternal and paternal characteristics is more comprehensive for completed pregnancies but limited for pregnancies that ended in fetal death. In particular, we do not observe fathers' age or pregnancy history for unsuccessful pregnancies. Thus, we only count successful pregnancies for those analyses that use these data. However, we believe this does not limit the regression exercises in any substantial way, since unsuccessful pregnancies constitute only 3.7% of the whole data.

Demographics Data. We use the population projection data calculated by the DANE based on the National Census of Population and Livelihood *Censo Nacional de Población y Vivienda*. The population data are available from 1995 to the present by age and sex. We use these data from 1998 to 2016 in our analysis and also calculate the female to male sex ratio.

Violence and Security. We obtained the number of cases of homicide, intimidation, terrorism, kidnapping, and forced displacement from 1993 to 2019 from the Conflict and Violence module of the Municipality Panel dataset compiled by the Center for Economic Development Studies at the University of Los Andes. We combine the data from 1998 to 2016 with the population data to calculate the rate of each of these forms of violence per 100,000 inhabitants.

3.3 Characteristics of Colombian Municipalities

Table B.2 describes the basic municipality characteristics in the earliest year of data availability, before the large-scale military expansion occurred.¹⁸ It compares the average characteristics of municipalities that had at least one military base during the analysis period to those that have never had a military base. Point estimates show these differences, and p-values indicate their statistical significance.

Municipalities that hosted a base differ systematically from those that never did during the analysis period. They are larger and lower in altitude, with total populations nearly twice those of never-hosted municipalities, though the two groups are similar in their female-to-male sex ratio. They are also considerably more violent at baseline: relative to municipalities that never hosted a base, the mean homicide rate is about 70% higher and the mean kidnapping rate about 160% higher, and rates of forced displacement are elevated as well.¹⁹

¹⁸We exclude islands (San Andrés and Providencia) and Colombia's seven largest cities (Barranquilla, Bogotá, Bucaramanga, Medellín, Cali, Cartagena, and Cúcuta) to ensure that our sample reflects municipalities with more typical population sizes and military presence, making the analysis more representative of the broader Colombian context.

¹⁹Forced displacement is an involuntary movement of people from their home due to conflict, violence, or human rights violations.

These statistically and economically meaningful differences between municipalities with and without bases are expected, because military bases are never randomly assigned. This exercise confirms the necessity of carefully constructing an appropriate comparison group. We explain our approach in Section 4.

4 Empirical Strategy

The main identification challenge is that both the location and timing of military base introduction are non-random. As described in Section 2, the expansion of military infrastructure in Colombia was driven by counterinsurgency strategies, with bases strategically placed in municipalities based on conflict intensity, geographic importance, and operational considerations. These same factors may also directly affect outcomes such as sexual violence. In addition, the timing of base introduction is determined by evolving conflict conditions and policy priorities, and therefore cannot be viewed as randomly assigned across municipalities.

Military bases are also introduced at different points in time across municipalities, generating staggered treatment timing. In such settings, municipalities that have already changed treatment status may serve as controls for municipalities treated later, potentially leading to inappropriate counterfactual comparisons in standard two-way fixed effects estimators (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfoeulle, 2022). This setting, thus, requires an empirical framework specifically designed for staggered treatment adoption.

To construct valid comparisons in this staggered setting, we use the estimator proposed by de Chaisemartin and D’Haultfoeulle (2024), which we call the dCdH estimator. This approach compares municipalities that change treatment status to municipalities with the same baseline treatment that have not yet changed treatment at each event horizon. We use this estimator because it accommodates non-absorbing treatments, allowing treatment status to change over time. This feature is relevant in our context because a small number of municipalities experience base removals during the sample period.

Because never-treated municipalities differ substantially from municipalities that ever host military bases, we restrict the estimation sample to municipalities that host at least one military base during the sample period.²⁰ The resulting analysis sample contains 153 municipalities: 76 are treated at baseline, including 11 municipalities that later lose bases, and 77 are untreated at baseline but receive a base during the sample period. Therefore, our main specification relies on variation in the timing of treatment changes among municipalities that have military bases during the sample period. Table 1 summarizes the composition of

²⁰ As shown in Table B.2, never-treated municipalities differ systematically from ever-treated municipalities in geographic characteristics, population size, and conflict-related outcomes.

the analysis sample, and Table 2 reports baseline characteristics for municipalities treated at baseline and future entrants.

Formally, let Y_{gt} denote the outcome of municipality g at time t , and let D_{gt} be a binary indicator equal to one if municipality g has a military base at time t . Let F_g denote the first period in which municipality g changes treatment status, and let $\ell \geq 0$ index event time relative to this treatment change. Following de Chaisemartin and D’Haultfoeuille (2024), we first define the municipality-specific treatment effect for ℓ periods

$$\delta_{g,\ell} = E \left[Y_{g,F_g-1+\ell}(D_{g,1}, \dots, D_{g,F_g-1+\ell}) - Y_{g,F_g-1+\ell}(D_{g,1}, \dots, D_{g,1}) \mid D \right]. \quad (1)$$

This parameter measures the difference between municipality g ’s observed outcome at $F_g - 1 + \ell$ and the counterfactual outcome that would have been realized had treatment status remained unchanged from the initial period to $F_g - 1 + \ell$.

We then aggregate these municipality-level effects across municipalities that experience treatment changes during the sample period:

$$\delta_\ell = \frac{1}{N_\ell} \sum_{g:F_g-1+\ell \leq T_g} [\mathbf{1}\{D_{g,F_g} > D_{g,1}\} - \mathbf{1}\{D_{g,F_g} < D_{g,1}\}] \delta_{g,\ell}. \quad (2)$$

Thus, δ_ℓ captures the average effect of deviating from baseline military base status for ℓ periods, relative to the counterfactual path in which treatment status remained unchanged. For municipalities that receive bases during the sample period, this corresponds to the effect of being exposed to a military base relative to remaining without one. For municipalities that are treated at baseline and later lose bases, the sign adjustment in the aggregation converts the effect of losing a base into the corresponding effect of greater military base exposure. As a result, the estimated event-study effects can be interpreted as average effects of military base presence across treatment-changing municipalities. Because most treatment changes in our data are base introductions, the estimates are primarily driven by municipalities that receive bases during the sample period.²¹

Because treatment status change over time, we report normalized estimates that scale δ_ℓ by the cumulative change in treatment exposure over the post-treatment horizon. These normalized estimates can be interpreted as the average effect of a one-unit change in military base presence. In our setting, where treatment reversals are rare, this interpretation corresponds closely to the effect of introducing a military base.

²¹We retain municipalities treated at baseline, including those that later lose bases, in the main specification to preserve identifying variation and statistical precision. As a robustness check, we also restrict the sample to municipalities that are untreated at baseline and receive bases during the sample period, so that identification comes only from base introductions. The main results are similar in this restricted sample.

The dCdH framework relies on two key assumptions: parallel trends and no anticipation. The parallel trends assumption requires that, absent changes in military base presence, municipalities that change treatment status and municipalities with the same baseline treatment that have not yet changed treatment would have followed similar outcome trends over time. The no-anticipation assumption requires that outcomes do not respond prior to changes in military base presence. In our setting, this assumption is plausible because military deployment decisions are largely driven by conflict dynamics and operational considerations, and the deployment of soldiers typically occurs immediately following the establishment of a new base.

We assess the plausibility of these assumptions by estimating placebo effects in pre-treatment periods using the same comparison structure as the main estimator. Because the timing of military base introduction is closely linked to conflict dynamics, we additionally examine placebo effects for security-related outcomes. We report joint tests of the null hypothesis that placebo effects are jointly equal to zero across pre-treatment periods. In addition, we report joint tests of the null hypothesis that post-treatment effects are jointly equal to zero.

Finally, we assess the robustness of the estimated effects to potential violations of the parallel trends assumption using the sensitivity analysis proposed by Rambachan and Roth (2023). Specifically, we implement relative magnitude bounds that allow post-treatment deviations from parallel trends to be proportional to deviations observed during the placebo periods. This approach evaluates the extent to which the estimated effects remain statistically significant under bounded departures from the identifying assumptions.

We report normalized event-study estimates and compute analytical standard errors clustered at the municipality level.²² In addition to the main post-treatment estimates, we estimate placebo effects in pre-treatment periods to assess the plausibility of the identifying assumptions discussed above.

Because many municipality-period observations contain zero reported cases of sexual violence, we aggregate outcomes into two-year periods.²³ This aggregation reduces noise arising from low-frequency outcomes while preserving the interpretation of the estimates in levels. However, we also present the estimation results with one-year periods to show that

²²We implement the estimator using the `did_multiplegt_dyn` Stata package developed by de Chaisemartin, Ciccia, et al. (2024).

²³For the indicator of military base presence, we consider a municipality to be treated in a two-year period if it has at least one military base in at least one year. The number of two-year periods depends on each outcome’s data availability. Sexual-violence and extortion outcomes, recorded by the Office of the Attorney General beginning in 2000, span 2000–2016 (nine two-year periods, from 2000–2001 through a final period containing only 2016). Fertility, demographic, and other violence outcomes are available beginning in 1998 and span 1998–2016 (ten two-year periods, from 1998–1999 through 2016).

the two-year aggregation does not necessarily mask pre-treatment trends. We do not apply nonlinear transformations such as the inverse hyperbolic sine transformation because recent work shows that these transformations can complicate interpretation and generate sensitivity to scale choices when outcomes contain a large mass of zeros.²⁴

As described in Section 2, army units differ substantially in organizational structure and personnel composition. We therefore consider three treatment definitions: (1) whether a municipality hosts any military base, including standing brigades, standing battalions, mobile brigades, and counterinsurgency battalions; (2) whether a municipality hosts at least one standing unit with drafted soldiers; and (3) whether a municipality hosts at least one counterinsurgency unit staffed primarily by professional soldiers. To examine heterogeneity across unit types, we estimate the effect of standing and counterinsurgency units while controlling for the presence of the other unit type.

In our sample, 62% of treated municipality-period observations host only standing units, 33% host only counterinsurgency units, and 5% host both types of units.²⁵

5 Results

This section presents our main findings. Section 5.1 reports the effects of military base introduction on recorded sexual violence. Section 5.2 examines the sensitivity of these results to pre-treatment dynamics and potential violations of parallel trends. Section 5.3 explores heterogeneity by base type. Section 5.4 evaluates alternative interpretations, including reporting behaviour, the general security environment, demographic changes, and the presence of other armed actors. Finally, Sections 5.5 and 5.6 examine consequences for fertility and spillover effects on neighbouring municipalities.

5.1 Effects of Military Base Presence on Sexual Violence

We first examine the effect of military base introduction on recorded sexual violence. Figure 2 reports dCdH event-study estimates for registered and indicted sexual violence cases per 100,000 inhabitants. Registered cases are cases recorded by the Office of the Attorney General, while indicted cases are cases in which a suspect is formally charged.

The pre-introduction estimates do not provide clear evidence of differential trends in

²⁴Sexual violence data contain many zeros, as they are relatively rare events. Around 35% of registered and 55% of indicted case observations are zero. Given that Mullahy and Norton (2024) show that such transformations can yield inconsistent estimates and complicate interpretation when zeros are frequent, we do not apply natural log or inverse hyperbolic sine transformations.

²⁵Table A.2 in the Supplemental Appendix reports the distribution of military bases by unit type.

recorded sexual violence before base introduction. Most placebo coefficients are close to zero and imprecisely estimated, and the joint tests fail to reject the null that the pre-treatment placebo effects are jointly equal to zero for both registered cases ($p = 0.368$) and indicted cases ($p = 0.149$). We note that two pre-treatment coefficients for registered cases, at six and four years before introduction, are positive rather than centred on zero. These estimates are individually imprecise and do not lead the joint test to reject, but we return to them below, since the size of the largest pre-treatment deviation is what the Roth–Rambachan sensitivity analysis in Section 5.2 scales against. These patterns are consistent with the identifying assumptions, although they do not by themselves rule out all time-varying factors correlated with base placement; we examine this concern more directly in Section 5.2. We also do not find evidence of anticipatory changes in recorded sexual violence in the periods before base introduction.

The post-introduction estimates are positive for both outcomes. For registered cases, the estimates increase to roughly 6–9 additional cases per 100,000 inhabitants during the first six years after base introduction, with statistically significant effects in the two- and four-year event periods. For indicted cases, the estimates are also positive and larger in the early post-introduction periods, rising by approximately 10–16 cases per 100,000 inhabitants during the first six years. The joint tests reject the null of no post-treatment effects for both registered cases ($p = 0.046$) and indicted cases ($p = 0.018$).

The cumulative total effects imply an average cumulative increase of 16.4 registered cases and 24.4 indicted cases per 100,000 inhabitants over the post-introduction period of 16 years.²⁶ These changes correspond to an approximately 52% increase in registered cases relative to the baseline mean of 32.34 cases per 100,000 inhabitants (the pre-treatment mean among municipalities that later receive a base), and an approximately 72% increase in indicted cases relative to the baseline mean of 33.85 cases per 100,000 inhabitants.

The increase in indicted cases is informative because indictment requires a further prosecutorial step beyond registration. This pattern does not prove that underlying incidents increased, since indicted cases may also rise when more cases are registered, and indictment does not establish guilt. However, it suggests that the increase in registered cases is not driven solely by weak or unsubstantiated reports; part of the increase reflects cases that advanced to formal accusation.

²⁶The cumulative total effect summarizes the average cumulative effect of introducing a base over the post-introduction period of 16 years. It aggregates the estimated effects across event periods and normalizes by cumulative treatment exposure. Because treatment is binary and base removals are rare in our setting, we interpret it as the average cumulative effect of base presence over the 16 years following base introduction. These estimates are reported in Figure 2. In addition, Table B.4 reports the intensive margin effect of military bases, and we find similar results.

5.2 Pre-Treatment Dynamics and Sensitivity to Trend Violation

We next examine whether the main sexual-violence results are sensitive to pre-treatment dynamics and violations of parallel trends. This concern is particularly relevant because base deployment decisions reflected military strategy and may have responded to evolving conflict conditions. If municipalities received bases when conflict intensity was already changing, and those changes also affected sexual violence, the estimates could partly capture time-varying determinants of base placement. We address this concern using combat-episode diagnostics and combat-trend-adjusted estimates, Roth–Rambachan sensitivity analysis, and annual event-study estimates.

Combat episodes. We first examine combat episodes as a direct proxy for evolving conflict intensity around base introduction. If bases were systematically introduced in municipalities where conflict was already escalating, we would expect the number of combat episodes to increase before base opening.²⁷ Figure 3 does not show such a pattern. The pre-introduction estimates are generally negative or close to zero, and the joint test fails to reject the null that the placebo estimates are jointly equal to zero ($p = 0.204$). The post-introduction estimates are also small and statistically indistinguishable from zero. These results do not rule out all strategic determinants of base placement, but they provide little evidence that base openings were systematically preceded by rising combat intensity.

Combat-trend-adjusted estimates. Although the combat-event-study estimates do not indicate rising conflict intensity before base introduction, conflict dynamics remain a central concern in this setting. We therefore estimate an additional specification that allows municipalities with different pre-introduction combat intensity to follow different linear counterfactual trends. Specifically, we construct each municipality’s average number of combat episodes before base introduction and interact this measure with calendar time. We do not control for contemporaneous combat episodes, since combat may itself respond to base introduction.

Figure 4 shows that the main sexual-violence estimates are similar under this adjustment. The pre-introduction estimates remain close to zero and statistically insignificant for both registered and indicted cases. The post-introduction estimates also remain positive and sta-

²⁷We calculate combat episodes as the sum of total subversive actions (*acciones subversivas*) and total armed contacts (*contactos armados*). Subversive actions are defined as military actions involving armed interaction between guerrilla groups and the public security forces, and armed contacts as armed confrontations initiated by the public security forces. These data are collected by the Ministry of Defence, and made available in the conflict and violence panel by the Centro de Estudios sobre Desarrollo Económico (CEDE) at the University of Los Andes.

tistically meaningful. The cumulative total effects are 17.1 registered cases and 23.8 indicted cases per 100,000 inhabitants, compared with 16.4 and 24.4 in the baseline specification. These results suggest that the main sexual-violence estimates are not driven by differential linear trends associated with pre-introduction combat intensity.

Roth–Rambachan sensitivity. We next use Roth–Rambachan sensitivity analysis to assess how sensitive our results are to violations of the parallel-trends assumption. Our main concern is that base openings may coincide with other conflict-related shocks, such as changes in combat activity or broader changes in state military presence. These concerns are better represented as shocks rather than smooth trends. We therefore use the relative-magnitude bounds of Rambachan and Roth (2023), which allow post-treatment violations of parallel trends up to $\bar{M}$ times the largest observed pre-treatment violation. We report robust confidence intervals across values of $\bar{M}$ for the post-treatment horizons where the baseline estimates are statistically meaningful: 2, 4, and 12 years after base introduction.

Figure 5 reports the resulting sensitivity intervals. The baseline point estimates are positive, but the robust confidence intervals widen as larger violations of parallel trends are allowed. The intervals include zero once post-treatment deviations are permitted to reach roughly 20 to 40 percent of the largest pre-treatment deviation. We therefore do not describe the statistical significance of the estimates as insensitive to trend violations: it is sensitive to parallel-trends violations that are small relative to the deviations already visible in the pre-treatment period.

This sensitivity bounds how much weight the significance levels can bear under adversarial assumptions about trends. The robust confidence sets include zero at moderate values of $\bar{M}$, so we cannot rule out a null effect under those assumptions. However, the sets do not lie entirely below zero: they straddle it rather than establishing a sign reversal, indicating that the data are consistent with the baseline positive effect even under the posited bias.

Annual aggregation. Finally, we re-estimate the main event studies using annual rather than two-year periods. This exercise addresses the concern that aggregating outcomes into two-year intervals may mask short-run changes before base introduction. Figure 6 reports the resulting estimates using annual periods. The annual estimates provide little evidence of such hidden pre-trends. The pre-introduction coefficients are small and statistically indistinguishable from zero for both registered and indicted cases, and the joint tests fail to reject the null of no pre-treatment placebo effects ($p = 0.886$ for registered cases and $p = 0.756$ for indicted cases). The individual post-introduction estimates are noisier and weaker than in the two-year specification, as expected given the lower frequency of annual cases, and

some periods hug zero. Nonetheless, the annual estimates reproduce the same front-loaded shape as the two-year specification, with the positive effects concentrated in the first several years after base introduction rather than sustained across the full window. The joint post-treatment tests reject the null of no effect for both registered ($p = 0.0033$) and indicted ($p = 0.0013$) cases, so the aggregate signal survives even as the period-by-period annual estimates are individually less precise. We therefore find little evidence that the two-year aggregation masks pre-opening changes in recorded sexual violence.

5.3 Heterogeneity by Base Type

We next examine whether the effects are concentrated around particular types of military units. This exercise is motivated by institutional differences across units, but we interpret it cautiously. Standing units are staffed primarily by drafted soldiers, while counterinsurgency units are staffed primarily by professional soldiers. For ease of exposition, we refer to estimates for standing units as “drafted-soldier effects” and estimates for counterinsurgency units as “professional-soldier effects.” These labels reflect the dominant personnel composition of each unit type, not direct evidence on individual perpetrators. Figure 7 reports event-study estimates separately for the two base types, controlling for the presence of the other type of unit.

For standing units, the pre-introduction estimates provide little evidence of differential pre-trends: for both registered and indicted cases, the placebo estimates are close to zero. For counterinsurgency units, some pre-treatment estimates are positive, particularly in earlier periods, although they are imprecisely estimated and not statistically distinguishable from zero. Given these noisier pre-treatment patterns, we do not interpret the base-type estimates as providing a clean causal contrast between standing and counterinsurgency units. Rather, we use them to describe where the main sexual-violence effect appears to be concentrated.

The positive post-introduction pattern is more pronounced for standing units. For registered cases, the estimates rise after base introduction and remain positive for much of the post-introduction window. For indicted cases, the estimates show a similar pattern, with positive effects in the early post-introduction periods and mostly positive estimates thereafter. The fact that this pattern appears for both registered and indicted cases echoes the main results and suggests that the increase is not limited to weak or unsubstantiated reports.

The estimates for counterinsurgency units staffed primarily by professional soldiers are smaller and less consistently positive. However, we do not interpret this as evidence of a precise null effect for professional-soldier units. Their post-treatment confidence intervals are wide and include economically meaningful positive effects, so the estimates cannot rule

out a response of the kind we find for standing units; they are best read as an imprecise null rather than as evidence of no effect. The pre-treatment estimates for these units are also less reassuring than those for standing units, suggesting that municipalities receiving counterinsurgency units may have been on different trajectories before base introduction. Moreover, unit types may differ not only in personnel composition but also in deployment logic, strategic role, and the local conflict dynamics that precede base introduction.

This caution is reinforced by the combat-episode estimates by base type (Figure A.7). While the aggregate combat results above do not show evidence of rising combat intensity before base introduction, the disaggregated combat estimates are less stable and suggest that conflict dynamics differ across municipalities receiving different types of units. However, when controlled for pre-introduction combat trends, the estimates do substantially change (Figure A.8). We, therefore, use the base-type results primarily to describe where the main sexual-violence effect is concentrated, rather than to isolate the causal role of drafted versus professional status.

Taken together, the base-type estimates indicate that the increase in recorded sexual violence is especially evident around standing units, which are staffed primarily by drafted soldiers. This pattern is consistent with personnel composition or other institutional features of standing units shaping civilian risks. However, the estimates do not identify individual perpetrators and should not be interpreted as direct evidence that drafted soldiers committed the reported crimes.

5.4 Interpreting the Increase in Recorded Sexual Violence

The results in Section 5.1 show that base introduction is followed by an increase in recorded sexual violence. Interpreting this increase requires caution. Judicial records reflect underlying incidents, reporting behaviour, costs of reporting, and prosecutorial processing. Moreover, military bases may be introduced alongside broader changes in security conditions, civilian population composition, or the presence and behaviour of other armed actors. We therefore examine several channels and alternative interpretations. These analyses do not allow us to identify perpetrators directly, but they help assess whether the main estimates are likely to reflect changes in reporting, conflict-related confounding, or other changes associated with base placement.

Reporting. One concern is that military base introduction may change reporting behaviour or access to judicial institutions. In the Colombian context, sexual violence is severely underreported (González Támara and Barragán Moreno, 2024). Military base presence could

further discourage reporting if victims fear retaliation, distrust military authorities, or believe the perpetrators are connected to government forces. At the same time, base presence could increase reporting if it brings citizens closer to state institutions or increases their willingness and ability to report crimes to the authorities by reducing the costs of reporting. Thus, changes in recorded sexual violence may reflect changes in reporting behaviour or costs, rather than changes in underlying incidents.

We examine this possibility using other report-sensitive outcomes.²⁸ The first is non-homicide violence, which includes intimidation, terrorism, kidnapping, and forced displacement.²⁹ These outcomes depend on reporting more than homicide does, since registering a homicide requires a body or other physical evidence.³⁰ These outcomes therefore provide a useful benchmark for whether base introduction generated a broader increase in recorded violence. Figure 8 shows no statistically meaningful increase in non-homicide violence after base introduction. The post-treatment estimates are imprecise and the joint test fails to reject the null of no post-treatment effects. This pattern does not support a broad reporting response across recorded violent outcomes.

The second comparison outcome is extortion. This is a particularly useful check because, like the sexual-violence outcomes, extortion cases come from the Office of the Attorney General. If the increase in recorded sexual violence reflected a general increase in use of this reporting and prosecution system, we might expect recorded extortion cases to rise as well. Figure 9 shows no such pattern. Registered extortion does not increase after base introduction, and indicted extortion cases are also imprecisely estimated and not jointly statistically significant.

Because the main effects differ across base types, Appendix Figures A.11 and A.12 report the same checks separately for standing and counterinsurgency units. The disaggregated estimates show some positive post-treatment estimates for standing units, particularly, but the timing of these estimates does not follow the event-study pattern for sexual violence. Thus, they do not provide clear evidence of a broad reporting response that would explain the main sexual-violence results.

Taken together, these results make a pure reporting-only explanation less consistent with the evidence. We do not find similar increases in other report-sensitive outcomes, including

²⁸We do not have access to municipality-level data on intimate partner violence, which would provide an additional reporting check given its similar underreporting dynamics.

²⁹We present the detailed results for each type of non-homicide violent crime in Figure A.9 in the Appendix. Consistent with the aggregated results, we find no significant effects on intimidation, terrorism, kidnapping, or forced displacement.

³⁰Homicide can still be underreported, for instance when bodies never surface, but its measurement does not respond to the willingness-to-report and institutional-access channels that could otherwise explain the recorded increase in sexual violence and non-homicide crimes.

an outcome recorded by the same institution as sexual violence. The base-type disaggregated checks are somewhat noisier and show some positive estimates for standing units, but they do not reveal a timing pattern that mirrors the increase in sexual violence. We therefore interpret these checks as evidence against a broad increase in reporting, while recognizing that they cannot fully rule out changes specific to the reporting or prosecution of sexual violence. The homicide results discussed below provide further evidence against a pure reporting explanation, since homicide is not reporting-sensitive yet rises alongside sexual violence around standing units.

General security environment. A related concern is that the observed increase in sexual violence may reflect broader changes in local security conditions around base introduction. Military bases could attract attacks by non-state armed actors or otherwise intensify local violence, increasing civilian vulnerability and contributing to sexual violence. Alternatively, the army may have strategically placed bases in municipalities where violence was already increasing. To investigate these possibilities, we examine whether base introduction is associated with changes in other forms of violence.

Figure 10 reports estimates for homicide. The pre-introduction estimates do not provide clear evidence of differential trends before base introduction. In the aggregate specification, the post-introduction estimates are positive in some periods but imprecisely estimated, and the joint post-treatment test does not reject the null of no effect, so the aggregate estimates do not indicate a clear average increase in homicide. The base-type disaggregation is more revealing, however: municipalities with standing units exhibit positive and relatively sustained post-treatment homicide estimates, while the estimates for counterinsurgency units are smaller and less persistent (Appendix Figure A.13). The homicide response is thus concentrated around the same standing units that drive the sexual-violence result.

This co-movement is central to interpreting the main result. As discussed above, homicide is far less sensitive than recorded sexual violence to the willingness-to-report and institutional-access channels that could otherwise explain the recorded increase. That homicide and recorded sexual violence rise together around standing units is therefore difficult to reconcile with a pure reporting or institutional-access explanation, and points instead to a genuine increase in recorded violence around these units. At the same time, we do not read the homicide pattern as ruling out a broader security channel: a deterioration in local security conditions may itself be part of how base introduction raises civilian exposure to violence, particularly around standing units. Taken together with the combat-episode results in Section 5.2, these findings indicate that broad recorded violence is unlikely to be the sole explanation for the increase in recorded sexual violence, even as evolving conflict intensity

and local security conditions remain part of the context in which the effects emerge.

Demographic and local population changes. Another potential mechanism driving the observed increase in sexual violence is demographic change in municipalities with military bases. Military bases may affect local population dynamics through employment opportunities, demand for local services, or changes in perceived security.³¹ We, thus, examine whether military base presence affects municipality population counts, separately by sex, and the female-to-male sex ratio. A decline in the female-to-male ratio indicates that the recorded population becomes more male-skewed, either because the male population rises relative to the female population or because the female population falls relative to the male population.

Figure 11 presents estimated effects of military base introduction on log civilian population by sex.³² We find little evidence that base introduction increased the recorded civilian population of host municipalities. Estimated effects on male and female population counts are small and move closely together, suggesting that changes in the population denominator are unlikely to mechanically drive the increase in recorded sexual violence and providing no support for the hypothesis that base introduction substantially increased the recorded civilian male population in host municipalities. The sex-ratio estimates point the same way: Figure 12 shows small and generally imprecise effects on the overall female-to-male ratio, and while the age-specific estimates in Figure A.14 offer some suggestive evidence of a relative decline among younger age groups, these too are imprecise and do not indicate large shifts in the overall civilian sex composition.

Taken together, these results provide limited support for the hypothesis that the increase in sexual violence is driven by economic migration or by an influx of civilian men into municipalities with bases.³³

³¹For example, in the American context, Zou (2018) finds that the contraction of military personnel increases outward migration and discourages inward migration due to civilian job losses. There are two main channels through which base presence could contribute to rising sexual violence. First, a decrease in the total population would mechanically inflate sex crime rates by reducing the denominator used to calculate crime rates. Second, changes in the sex ratio could directly influence crime rates, including sexual violence. For example, in China, a male-skewed sex ratio resulting from the one-child policy contributed to increased rates of violence and property crimes (Edlund et al., 2013). In contrast, in Rwanda, a female-leaning sex ratio imbalance due to the 1994 genocide likely contributed to a decline in female bargaining power, leading to increased domestic violence against women (La Mattina, 2017).

³²The population data are based on census projections and likely do not include soldiers stationed at military bases. We therefore interpret these estimates as changes in the recorded civilian population rather than direct measures of military personnel presence.

³³We also examine whether base introduction affected local economic conditions. Appendix Figure A.15 shows no evidence that bases increased real municipal GDP per capita, though these data are only available for 2000–2009.

Presence of other armed actors. A final attribution concern is that military base introduction may have coincided with changes in the presence or behaviour of other armed actors. The Colombian conflict involved left-wing guerrilla groups, right-wing paramilitary organizations, and documented collaboration between some state forces and paramilitary groups (Ruiz, 2001; Human Rights Watch, 2001; Evans, 2002; Acemoglu, Robinson, and Santos, 2013; Commission for Truth, 2022). As a result, the increase in recorded sexual violence need not reflect misconduct by army soldiers alone. It could also reflect violence by armed actors operating alongside, in response to, or in collaboration with state forces.

The combat-episode results in Section 5.2 provide little evidence that base openings were systematically preceded or followed by increases in combat activity. This reduces the concern that the estimates are driven by a broad escalation in insurgent activity around base introduction. However, combat episodes are an imperfect proxy for the presence of other armed actors, especially paramilitary groups or informal collaboration with state forces. We therefore interpret the estimates as capturing the effect of introducing military bases into local communities, rather than as direct evidence on the identity of perpetrators.

5.5 Consequences for Fertility

We next examine whether the increase in recorded sexual violence is associated with changes in fertility. Sexual violence, including rape, can lead to unintended pregnancies and may therefore affect conception rates in host communities. At the same time, fertility is an indirect and imperfect downstream outcome: changes in fertility could reflect many channels other than sexual violence, including migration, security conditions, consensual relationships, or local economic changes.

Figure 13 reports estimated effects on conception rates per 1,000 women ages 10 to 39. We find little evidence that military base introduction affected fertility. The pre-introduction estimates do not indicate systematic differential trends, and the post-introduction estimates are small and statistically indistinguishable from zero. The results are similar when we disaggregate conception rates by women’s age group, as shown in Appendix Figure A.17.

These null effects should be interpreted cautiously. Fertility is a coarse measure of the consequences of sexual violence, and any fertility response could be offset by changes in migration, contraceptive behaviour, abortion, consensual relationships, or local economic conditions. We therefore do not interpret the fertility results as evidence against the increase in recorded sexual violence. Rather, they indicate that the increase in recorded sexual violence did not translate into detectable changes in aggregate conception rates.

5.6 Spillover Effects

As discussed in Section 2, brigades and battalions may operate outside the municipality where they are based. Soldiers may be deployed across administrative boundaries for security operations, military exercises, or temporary assignments. As a result, the effects of base introduction may extend beyond host municipalities. We therefore examine whether neighbouring municipalities also experience changes in recorded sexual violence.

To estimate spillover effects, we use the dCdH estimator and define treatment as having at least one municipality with a military base within a 25, 50, or 75 km radius of the municipality population centre. In these specifications, we control for whether the municipality itself hosts a base, so that the estimates capture exposure to nearby bases rather than direct exposure to a local base. Using multiple buffer sizes allows us to examine whether the results vary with proximity to treated municipalities.

Table 3 reports the spillover estimates for registered and indicted sexual violence cases per 100,000 inhabitants.³⁴ For registered cases, the estimates are positive and statistically significant at the 10% level for treated neighbours within 25 km and 75 km, corresponding to increases of approximately 12 and 25 cases per 100,000 inhabitants over the post-introduction period. For indicted cases, the estimates are positive and statistically significant at the 5% level for the three buffer sizes considered, corresponding to increases between 23, 32, and 34 cases per 100,000 inhabitants over the post-introduction period.

6 Conclusion

This paper examines whether the introduction of military bases increased sexual violence in Colombia. We construct a novel municipality-year panel of military base presence from 1998 to 2016 and link it to administrative records on sexual violence, fertility, conflict, crime, and local demographic conditions. To account for staggered treatment timing, we estimate dynamic treatment effects using the de Chaisemartin and D’Haultfœuille estimator, restricting comparisons to municipalities with military bases at some point during the sample period.

We find that military base presence led to a substantial increase in recorded sexual violence. The event-study estimates show positive effects on both registered and indicted sexual-violence cases after base introduction. The increase in indicted cases is particularly informative because indictment requires a further prosecutorial step beyond registration, suggesting that the observed increase is not limited to weak or unsubstantiated reports. The

³⁴The number of observations decreases as the buffer size increases because fewer municipalities switch from not having a base to having one at larger distances.

effects are concentrated around standing units, which are staffed primarily by drafted soldiers rather than professional soldiers. We examine this heterogeneity to see where the response is concentrated, but interpret it descriptively rather than causally: base type is not randomly assigned, and unit types differ not only in personnel composition but also in strategic role, deployment patterns, and the conflict dynamics surrounding base introduction.

Several pieces of evidence support a causal interpretation of the main results. The pre-treatment estimates for sexual violence are small, imprecise, and jointly statistically indistinguishable from zero. We also find little evidence that base openings were systematically preceded by rising combat intensity, and the results remain similar after allowing municipalities with different pre-introduction combat intensity to follow different linear counterfactual trends. Annual event-study estimates provide little evidence that the two-year aggregation masks short-run pre-treatment changes. Roth–Rambachan sensitivity analyses show that the statistical significance of the estimates is sensitive to moderate violations of parallel trends, though the robust confidence sets straddle zero rather than lying entirely below it. Taken together, these checks support the interpretation that base introduction increased recorded sexual violence, while warranting caution about statistical precision under possible deviations from parallel trends.

Having attributed the increase in recorded sexual violence to base presence, we consider other explanations for that increase. We do not find strong evidence that it is driven by a broad increase in reporting, by changes in the recorded civilian population, or by economic migration. The reporting-sensitive placebo outcomes speak directly to this concern: because they respond to the same reporting behaviour as sexual violence, their flatness is hard to square with a general increase in the recording or reporting of crime. Neither non-homicide violent crime nor extortion, the latter recorded by the same institution as sexual violence, rises systematically after base introduction. The demographic results likewise provide limited support for large changes in the recorded civilian population or sex composition, though census-based population data likely do not capture soldiers stationed at bases. These checks do not rule out every channel, but they indicate that reporting changes, demographic shifts, and migration do not account for the timing and pattern of the sexual-violence effects.

Homicide is the one comparison outcome that does rise, and it is informative. Because homicide is recorded from physical evidence rather than from victim complaints, it is far less sensitive to the reporting behaviour that could confound recorded sexual violence. That homicide and recorded sexual violence rise together around the same standing units is difficult to reconcile with a pure reporting explanation and points instead to a genuine deterioration in local security around these units. We therefore do not read this pattern as a rival explanation for the main result but as a plausible mechanism behind it: base presence may raise civilian

exposure to recorded sexual violence in part by worsening general security conditions. We accordingly interpret our estimates as causal evidence that military base presence increased civilian exposure to recorded sexual violence, with deteriorating security as one channel through which this exposure may operate.

A central limitation of the paper is that the data do not identify perpetrators. The estimates capture the effect of introducing military bases into local communities, not a perpetrator-specific effect. Some of the increase in recorded sexual violence may have been committed by soldiers stationed at bases, but some may also reflect changes in civilian behaviour, violence by other armed actors, or interactions between state forces and non-state groups. Historical evidence from Colombia documents sexual violence by government soldiers as well as collaboration between some army units and paramilitary groups. The results are therefore best interpreted as evidence on the consequences of introducing military bases into local communities, rather than as direct evidence on the identity or motives of individual perpetrators.

Although we cannot identify who commits the recorded crimes, several mechanisms in the literature on conflict-related sexual violence are consistent with our findings, including the strategic use of violence to control civilians and non-strategic channels such as weak oversight, impunity, and the after-effects of deployment. Appendix Section C.3 discusses these interpretations and the studies behind them. The concentration of effects around standing units is consistent with institutional features of these units shaping local risk, but, as noted above, our evidence cannot separate personnel composition from differences in deployment logic, command structure, or surrounding conflict conditions.

The findings complicate a simple security-benefit narrative of military expansion. Military bases are often justified as instruments for restoring order, protecting civilians, and strengthening state authority in conflict-affected areas. Our results show that such interventions can also impose substantial civilian costs—costs potentially borne largely by women and girls, who make up the great majority of recorded sexual-violence victims in these data. Even when the state expands its territorial presence in the name of security, that presence may expose civilians in host communities to new forms of harm unless accompanied by credible oversight, accountability, and survivor-centred reporting institutions. Future research should collect data that link incidents to perpetrators, military units, command structures, and local institutional responses. Such data would allow researchers to distinguish more clearly between soldier misconduct, violence by other armed actors, and broader changes in civilian behaviour around military deployment.

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7 Figures

Figure 1: Expansion of the National Army between 1998 and 2016

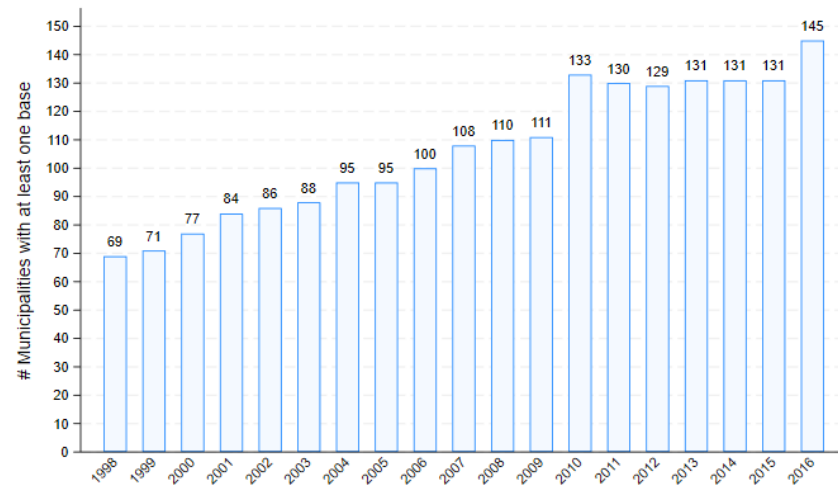
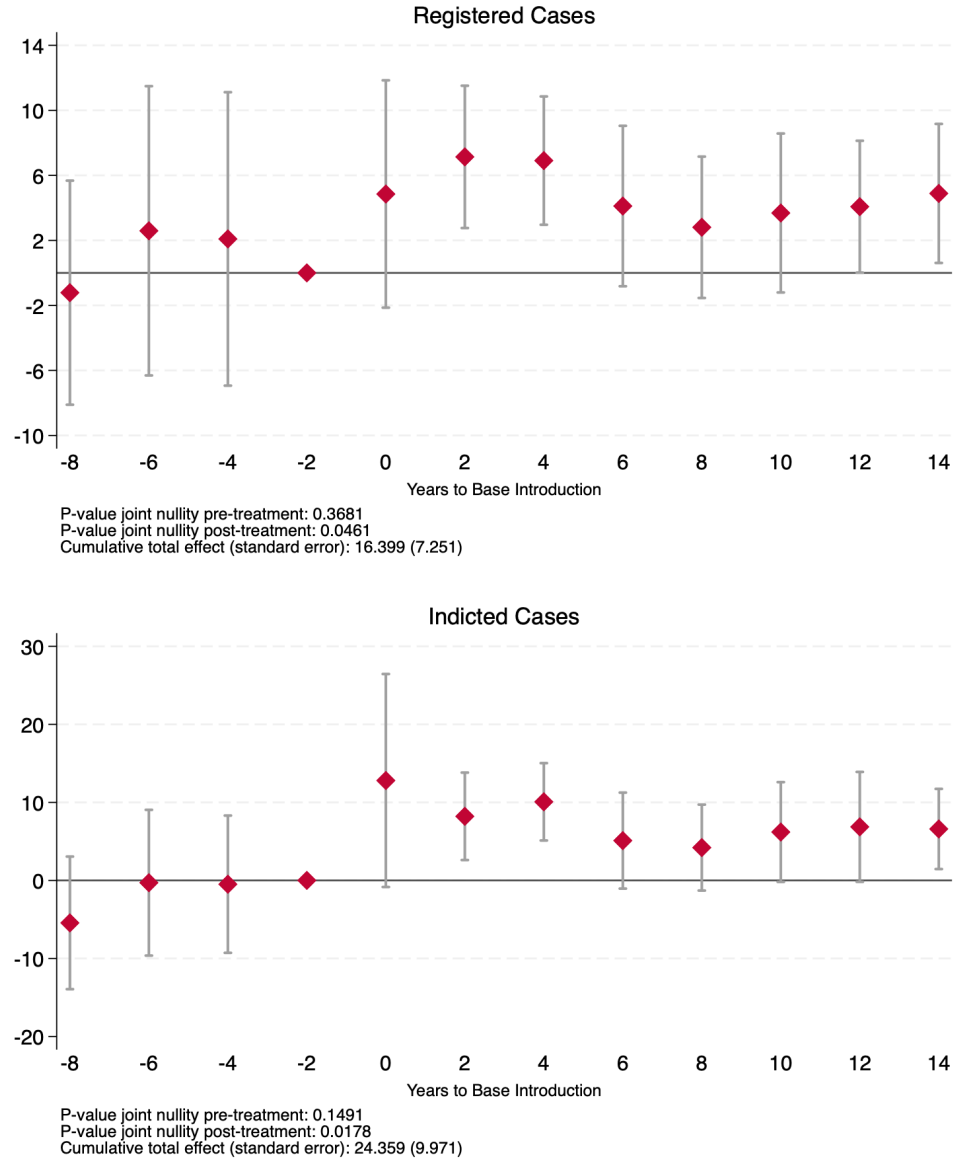


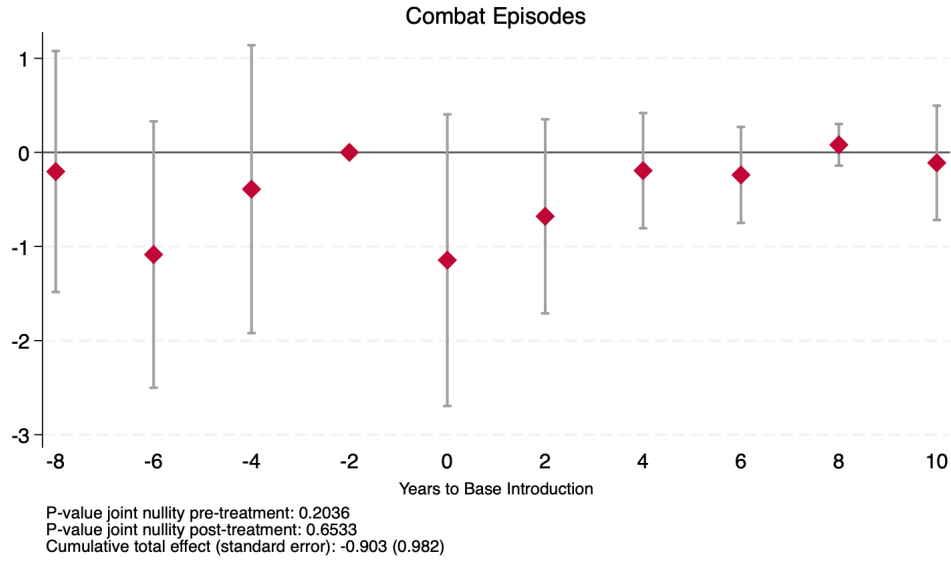
Figure 2: Effects on Sexual Violence
Outcome: Number of Cases per 100,000 Inhabitants



Note: This figure dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

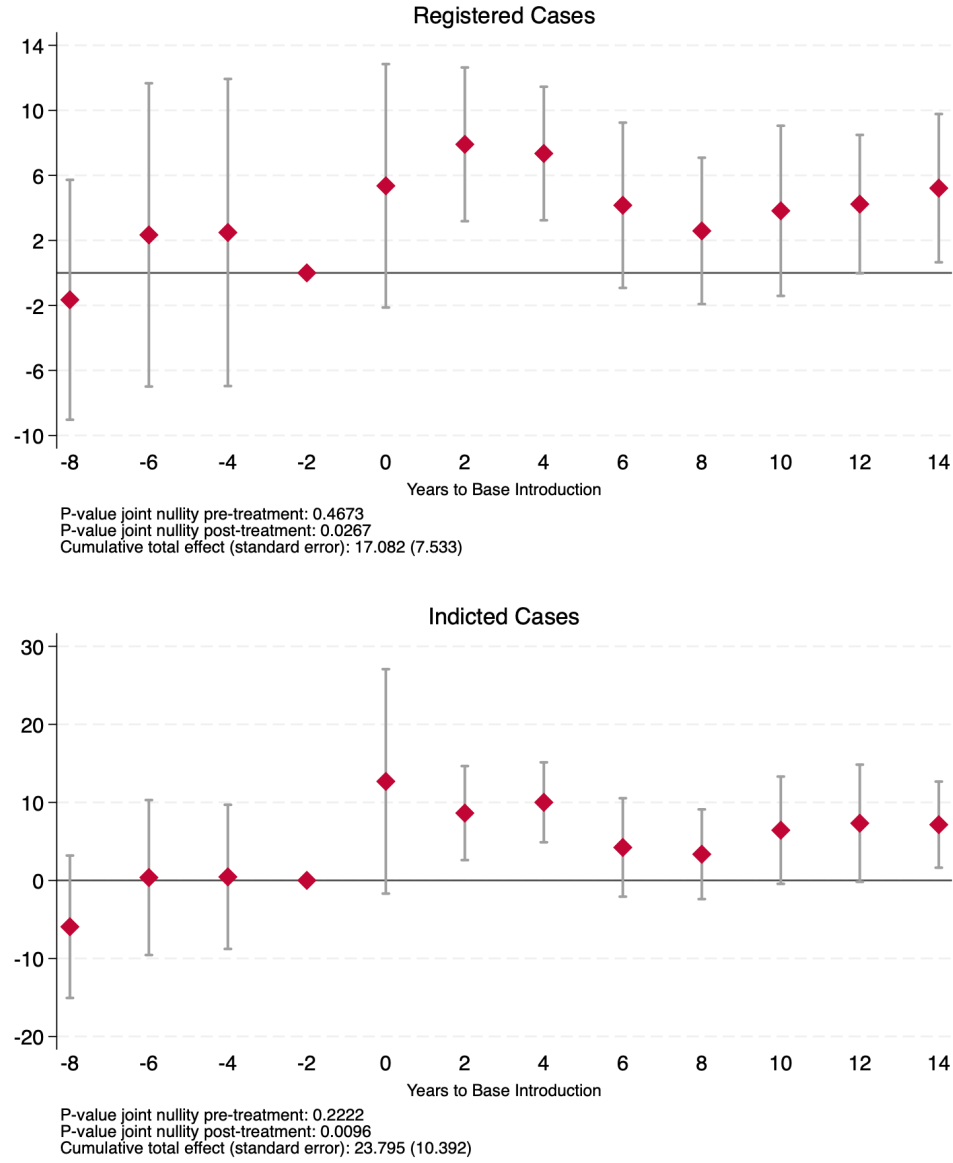
Figure 3: Effects on Combat Episodes

Outcome: Number of Cases per 100,000 Inhabitants



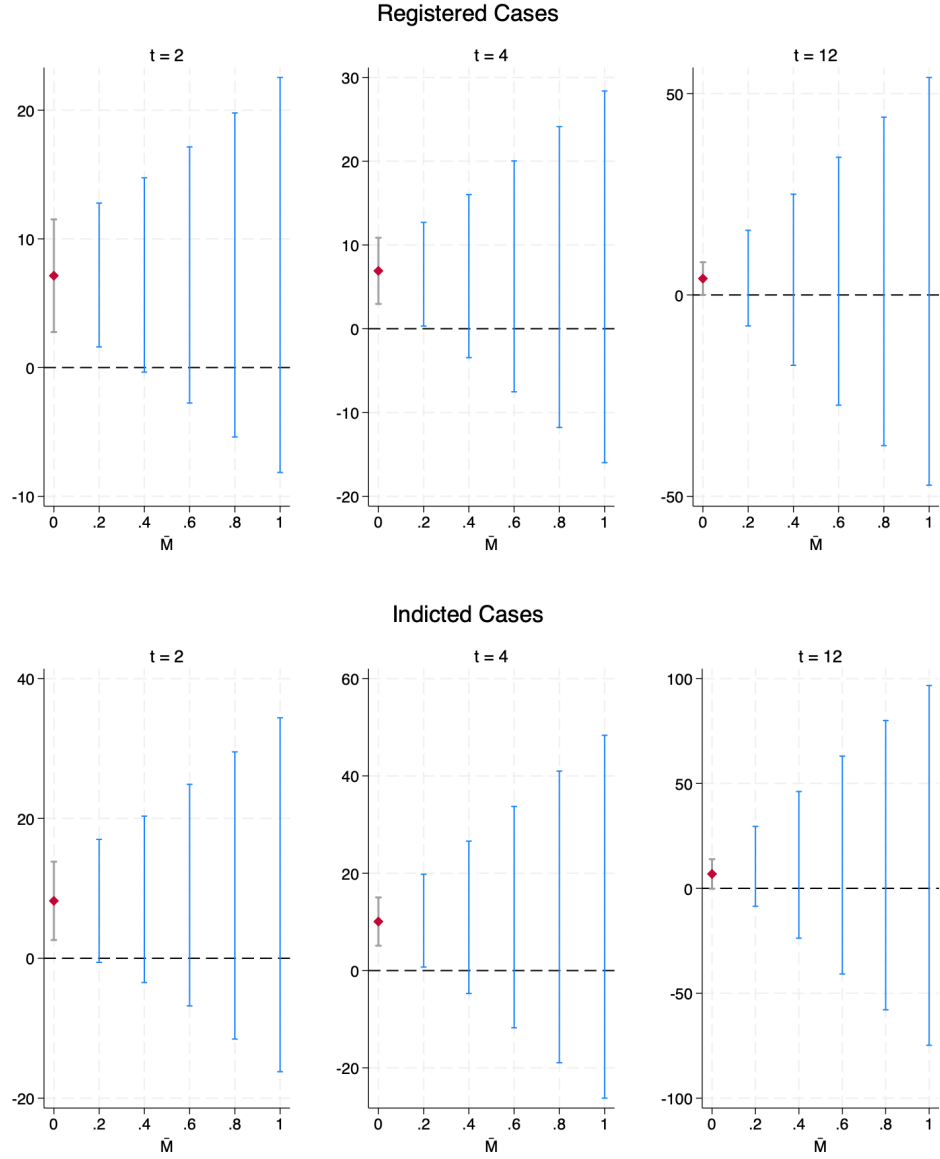
Note: This figure plots dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 4: Effects on Sexual Violence, Adjusting for Pre-Introduction Combat Trends
Outcome: Number of Cases per 100,000 Inhabitants



Note: This figure plots dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

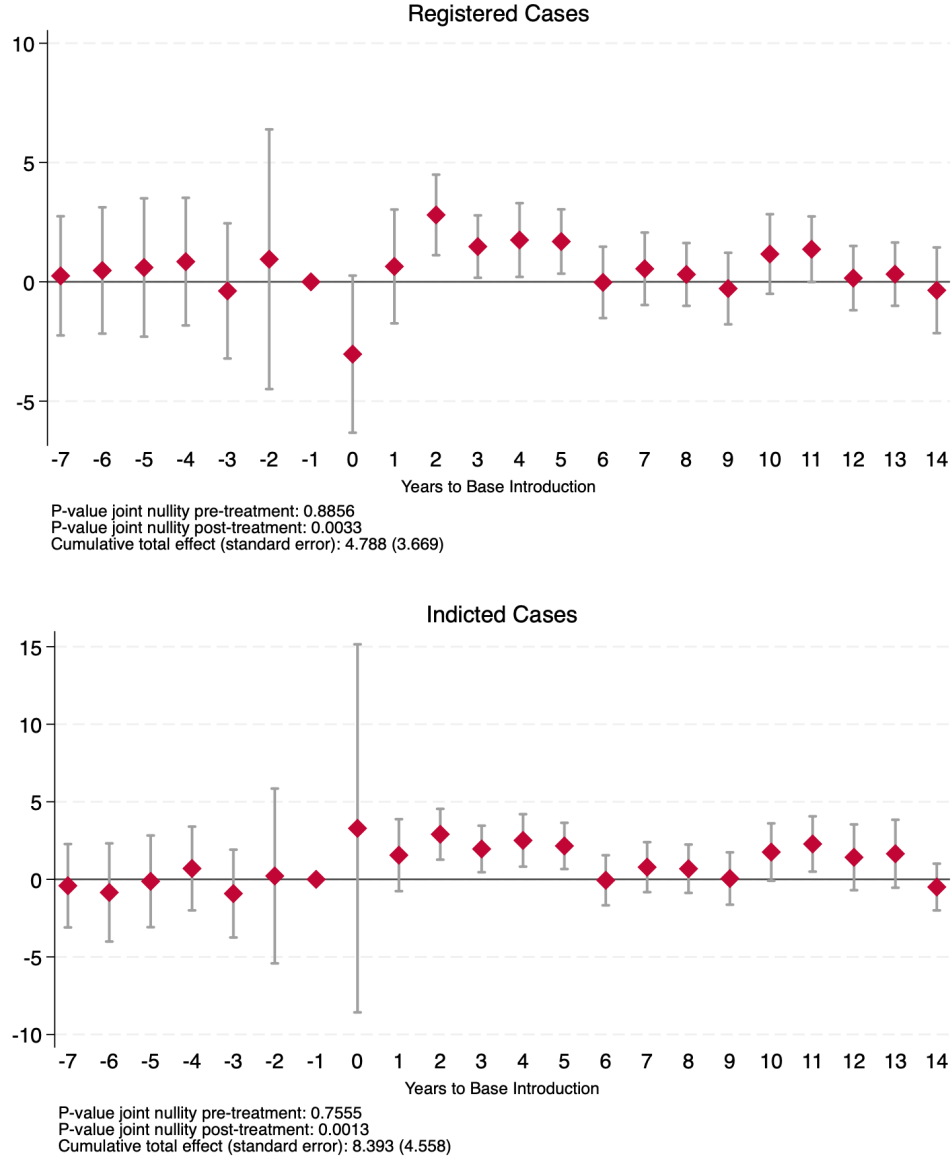
Figure 5: Roth-Rambachan Test: Effects on Sexual Violence



Note: This figure reports Roth–Rambachan sensitivity intervals using relative-magnitude bounds for the two-year event-study estimates. Each panel reports the baseline estimate at $\bar{M} = 0$ and robust confidence intervals allowing post-treatment violations of parallel trends to be up to $\bar{M}$ times the largest observed pre-treatment violation. The figure reports the post-treatment horizons where the baseline event-study estimates are statistically meaningful: 2, 4, and 12 years after base introduction. The outcome is the number of cases per 100,000 inhabitants. Standard errors are clustered at the municipality level.

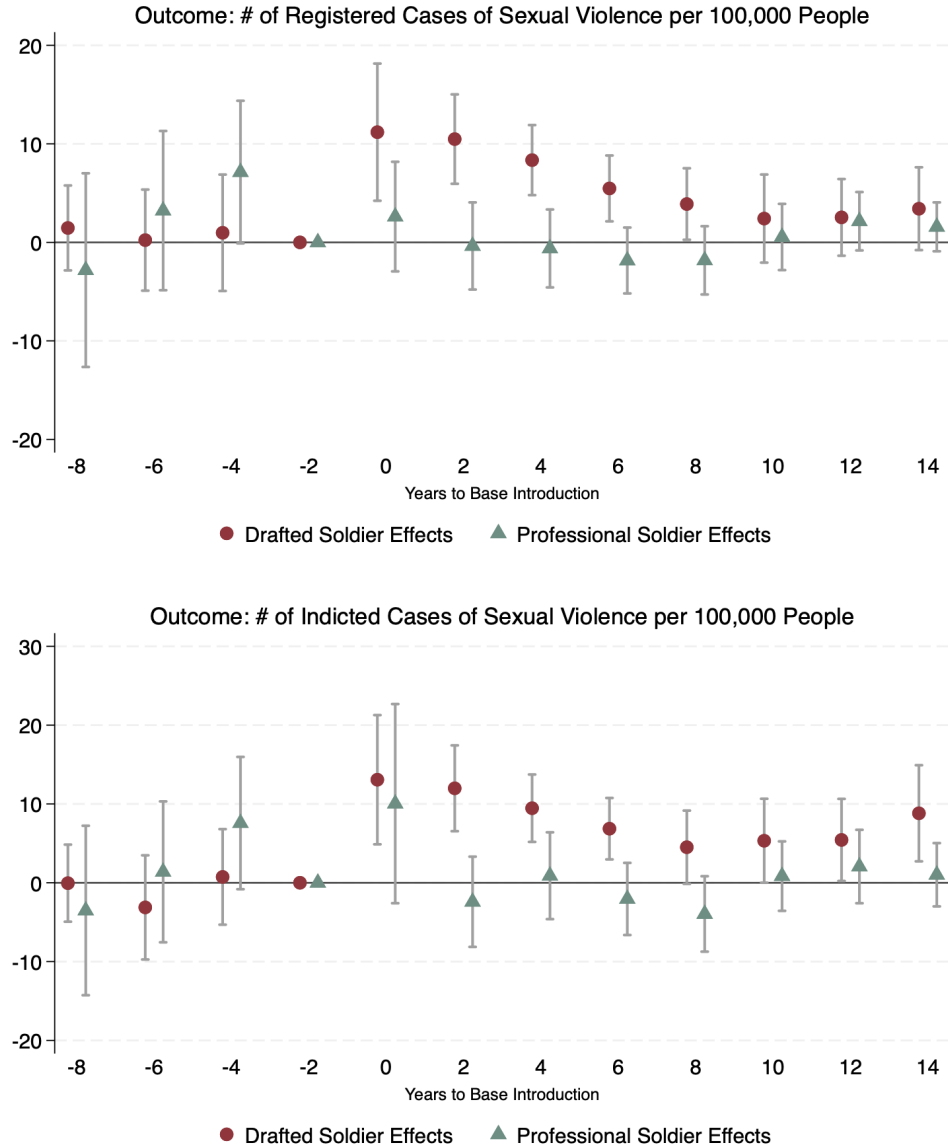
Figure 6: Effects on Sexual Violence, Annual Aggregation

Outcome: Number of Cases per 100,000 Inhabitants



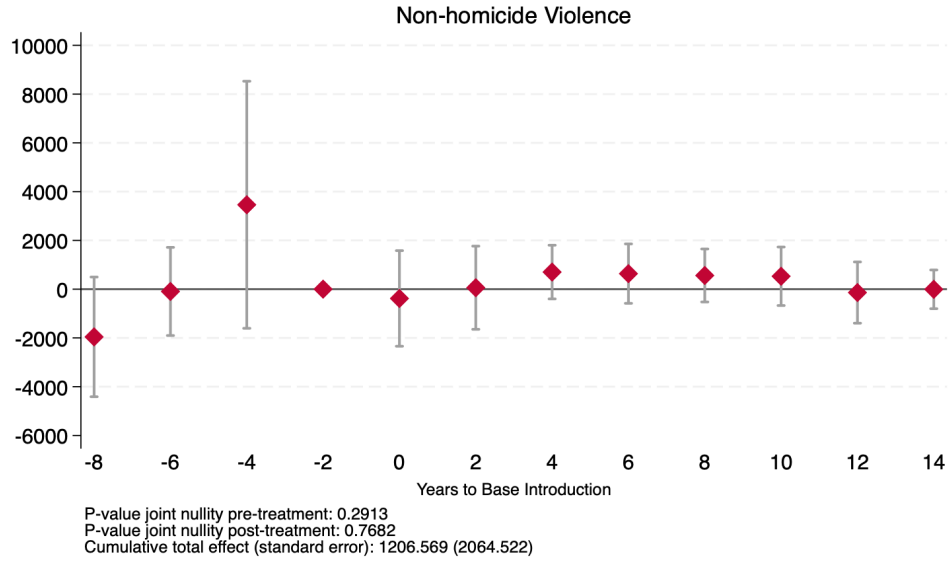
Note: This figure reports dCdH event-study estimates using annual outcomes, relative to the year immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfoeulle (2024). Vertical lines report 90% confidence intervals based on standard errors clustered at the municipality level. The outcome is the number of registered or indicted sexual violence cases per 100,000 inhabitants. The analysis sample includes 153 municipalities that host a military base during the sample period and excludes 959 municipalities that never host a base.

Figure 7: Effects on Sexual Violence by Base Type
Outcome: Number of Cases per 100,000 Inhabitants



Note: This figure plots dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). *Drafted soldier effects* refer to the estimated coefficients on an indicator variable for each period in which a municipality has at least one standing unit with more drafted soldiers. *Professional soldier effects* refer to the estimated coefficients on an indicator variable for each period in which a municipality has at least one counterinsurgency unit with more professional soldiers. Drafted soldiers are typically less educated and only given a small monthly stipend. Professional soldiers, who are more educated, better trained, and receive a regular salary with generous benefits. The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

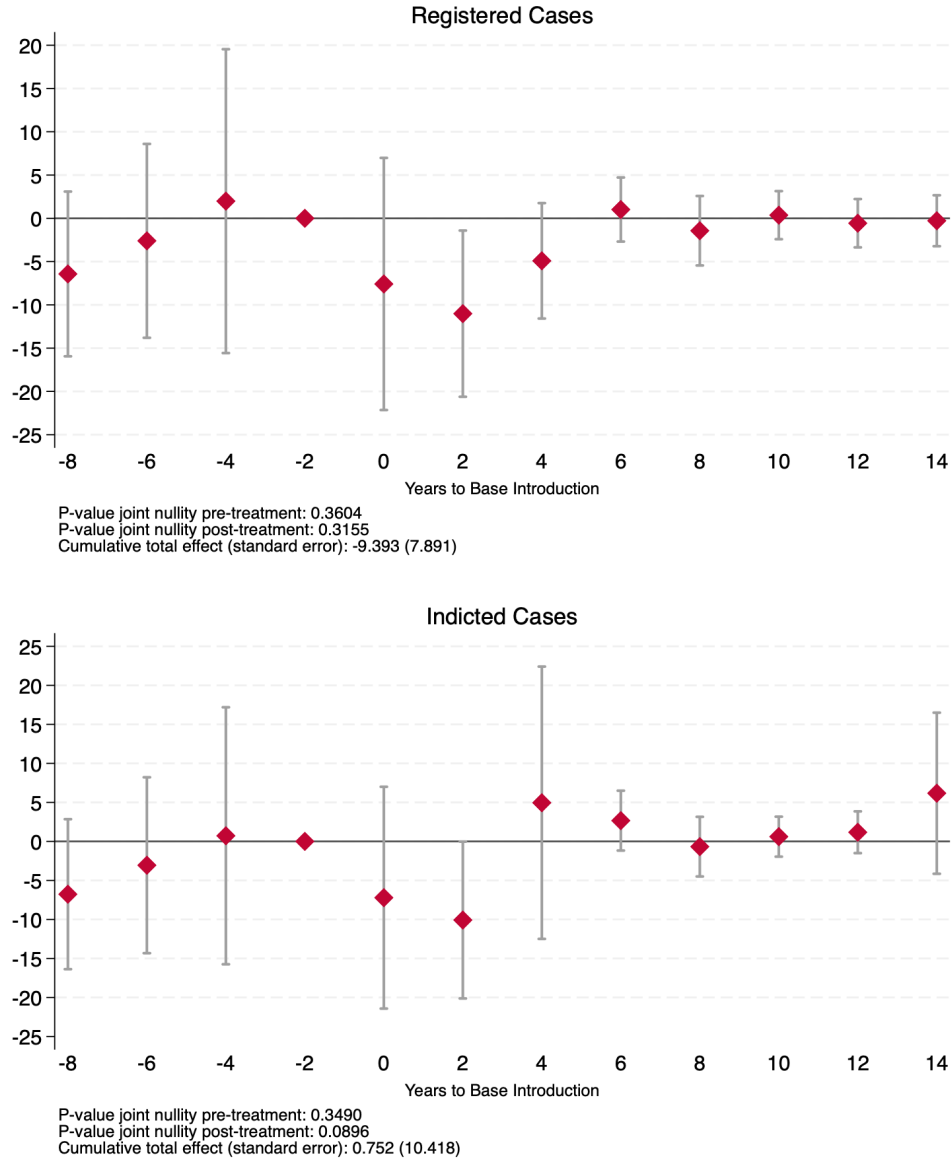
Figure 8: Effects on Non-homicide Crimes
Outcome: Number of Cases per 100,000 Inhabitants



Note: This figure plots dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

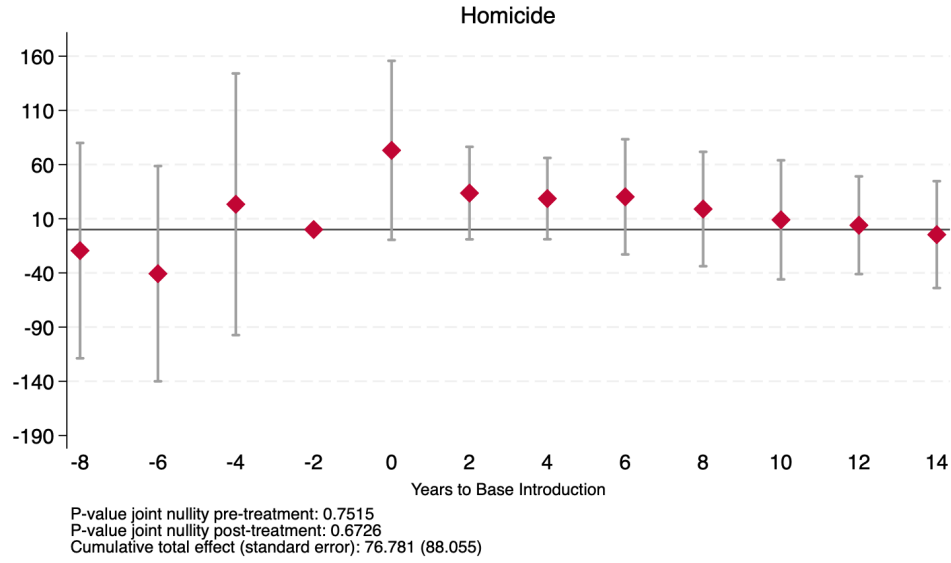
Figure 9: Effects on Extortion

Outcome: Number of Cases per 100,000 Inhabitants



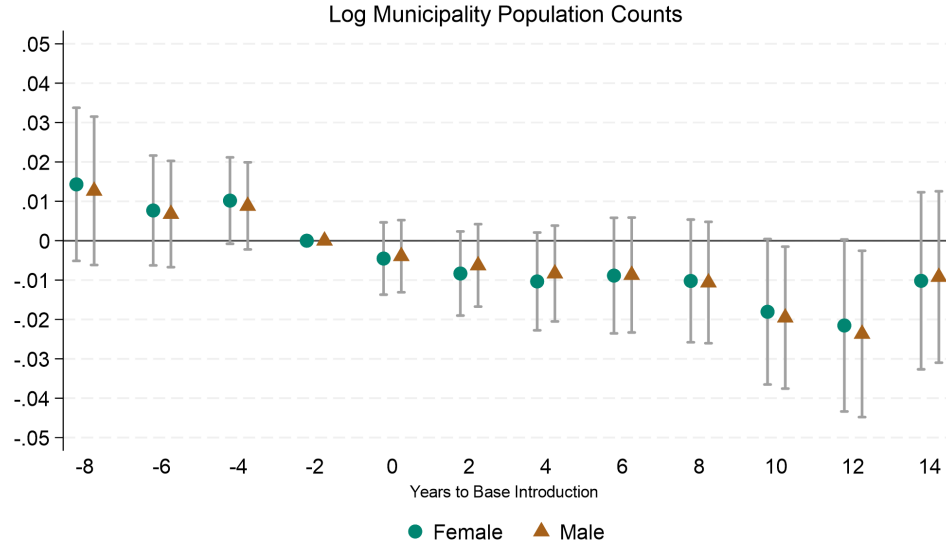
Note: This figure plots dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfoeulle (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 10: Effects on Homicides Crimes
Outcome: Number of Cases per 100,000 Inhabitants



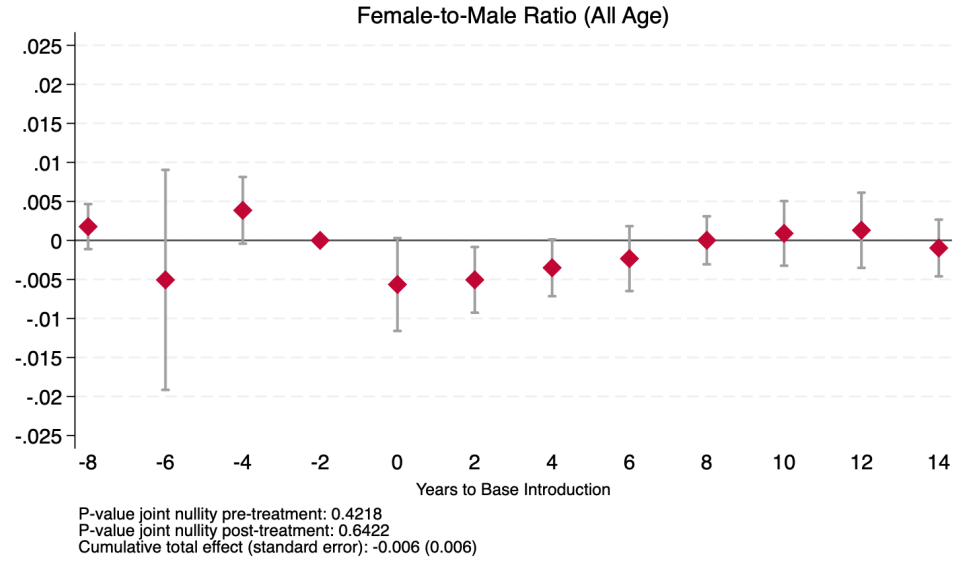
Note: This figure plots dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 11: Effects on Population



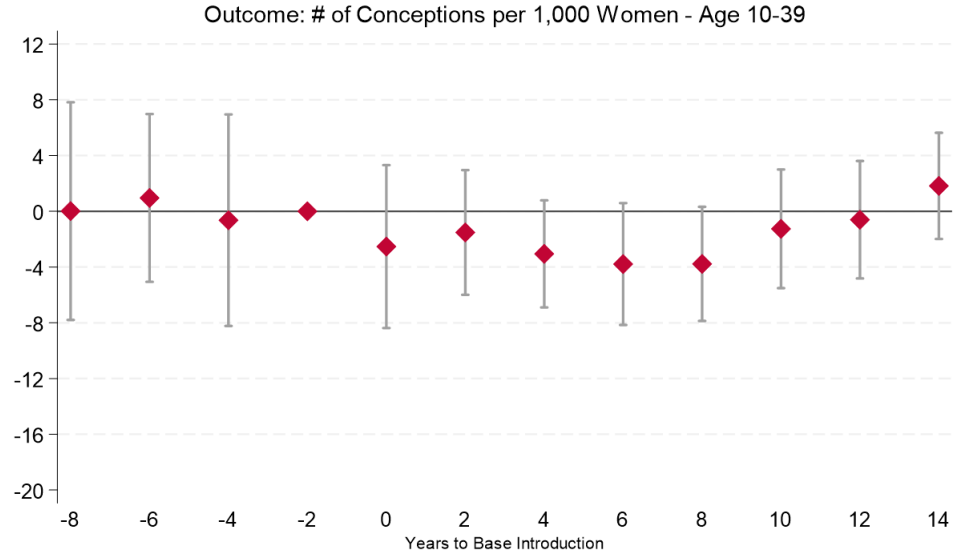
Note: These graphs plot dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 12: Effects on Sex Ratio



Note: These graphs plot dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D’Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 13: Effects on Fertility



Note: These graphs plot dCdH event-study estimates for each two-year event period, relative to the two-year period immediately preceding military base introduction. The estimates are obtained using the estimator proposed by de Chaisemartin and D'Haultfoeuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

8 Tables

Table 1: Share of Municipalities by Treatment Status

Treatment Status	Counts	Share (%)
Always Treated	65	42.48
Future Entrants	77	50.33
Lost Bases	11	7.19
Total	153	100.00

Note: Total number of municipalities included in the sample is 153.

Table 2: Municipality Characteristics in Analysis Sample

	Treated at Baseline				Future Entrants			
	Mean	Std. Dev.	Min.	Max.	Mean	Std. Dev.	Min.	Max.
Area (km ²)	3301.33	8980.26	108.00	65674.00	3352.56	5102.80	70.00	20141.00
Altitude above the sea level (meters)	769.12	837.26	2.00	2897.00	796.39	830.88	3.00	3051.00
Total population	92772.30	110567.34	6137.00	447594.50	29665.66	49421.49	2034.50	400242.00
Female	47790.54	58141.43	1586.00	237059.00	14667.86	25140.16	931.50	204020.00
Male	44981.76	52476.87	3491.50	210535.50	14997.80	24290.99	1103.00	196222.00
Sex ratio (Female:Male)	1.00	0.12	0.35	1.25	0.94	0.10	0.57	1.10
Cases of violence per 100,000 inhabitants								
Homicide	717.04	998.63	0.00	5807.05	429.38	465.46	0.00	1871.03
Intimidation	135.00	253.43	0.00	1431.40	238.72	281.87	0.00	1425.41
Terrorism	20.86	73.07	0.00	618.89	91.18	353.88	0.00	2947.94
Kidnapping	258.33	425.53	0.00	2785.39	206.80	375.88	0.00	2118.72
Forced displacement	5587.74	10297.26	0.00	64628.19	7161.79	8227.18	18.29	40191.92
Cases of sexual violence per 100,000 inhabitants								
Registered	12.42	25.32	0.00	87.50	32.34	42.59	0.00	153.36
Indicted	15.65	34.53	0.00	195.68	33.85	44.03	0.00	168.48

Note: There are 153 municipalities in the analysis. The first four columns report statistics for 76 municipalities that were treated at baseline (hosted at least one military base at the start of the analysis period); this includes 11 municipalities that later lost their military bases during the analysis period. For these municipalities, we report statistics for the first two-year period in the analysis period. The last four columns report statistics for 77 municipalities that received a military base during the analysis period. For these municipalities, we report statistics for the two-year period before they received the military base. Altitude and area sizes are constant across years.

Table 3: Spillover Effects on Sexual Violence (dCdH)

Outcome: Number of Cases per 100,000 Inhabitants		
	Sexual Violence	
	Registered	Indicted
	(1)	(2)
Panel A: 25 km Buffer		
Has treated neighbor	12.16*	23.33**
	(6.78)	(9.57)
Obs.	1,224	1,223
Panel B: 50 km Buffer		
Has treated neighbor	19.74	31.28***
	(13.02)	(12.04)
Obs.	996	995
Panel C: 75 km Buffer		
Has treated neighbor	25.34*	34.68**
	(14.25)	(14.19)
Obs.	966	965
Baseline mean	32.34	33.85

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors in parentheses clustered at the municipality level. The de Chaisemartin and D'Haultfœuille estimator calculates the *average total effect*, which is the weighted sum of the effects of all periods. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities. Sexual violence is expressed as the number of cases per 100,000 inhabitants.