When Protection Fails: Effects of Military Bases on Sexual Violence in Colombia*

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Abstract

Sexual violence committed by soldiers is ubiquitous across the world. This paper investigates the impact of military base presence on sexual violence, fertility, and child support disputes in Colombia, a nation with an experience of large-scale growth in military base presence. Using a dataset constructed from diverse sources, we track military base locations across Colombian municipalities from 1998 to 2016. Employing an event-study approach, we identify the causal effects of military bases on host communities. Our analysis reveals that the presence of military bases significantly increases sexual violence, with a 72% rise in registered cases over the course of 16 years after the introduction of a military base. Despite this increase in sexual violence, we find no significant changes in fertility or child support disputes. These results are not driven by changes in population, security conditions, or reporting. By highlighting the link between military presence and sexual violence, this study contributes to broader discussions on the economic and institutional consequences of state-led 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] 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.[2] (Cohen et al., 2013).

Understanding the relationship between soldier presence and sexual violence is particularly relevant to economists, not only because of its moral and legal implications but also due to its lasting economic consequences. Recent studies have shown that sexual violence can disrupt victims' labor market participation, and depress earnings (Sabia et al., 2013; Adams-Prassl et al., 2024; Adams et al., 2024). Beyond individual consequences, state-led violence can also erode civilian trust in institutions and affect post-conflict governance (Voytas and Crisman, 2024). These dynamics have direct and indirect consequences for labor markets, political stability, and, ultimately, long-term economic development (Acemoglu et al., 2005).

This paper addresses the question: *What are the consequences of soldier presence for host community women?* We conduct our analysis in the context of Colombia, a country with a long history of civil conflict, where army soldiers have been accused of sexual violence against civilians. Media reports document multiple cases of Colombian soldiers sexually assaulting female civilians, often minors, both during the half-century-long armed conflict and in the post-conflict period since 2016 (Reuters, 2020; Oquendo, 2020; Turkewitz, 2020). Recognizing the severity of the issue, the Colombian government recently established a special judicial committee to investigate conflict-related sexual violence committed by all parties, including the public forces (JEP, 2023a). In this paper, we first estimate the effects of soldier presence on sexual violence. We then examine fertility and child support disputes, as consequences of sexual violence may manifest in these outcomes.

To estimate the causal effects of soldier presence, we address two significant empirical challenges. The first is the scarcity of comprehensive data on soldier presence across time and space.[3] We overcome this limitation by constructing a novel dataset on military base

[1] Bastick et al. (2007) provide a comprehensive summary of countries with civil conflicts in which soldiers of official forces commit sexual violence against civilians.

[2] Annual reports analyzing 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, 2021, 2022, 2023, 2024).

[3] We requested complete historical records on the location of army bases from the Ministry of Defense,

presence in Colombia. This dataset is compiled from diverse sources, including newspaper articles, the army's organizational charts,[4] historical records, congressional reports, and legislative documents. Our dataset provides a unique perspective, indicating the presence of army bases at the municipality-year level from 1998 to 2016. It's worth noting that our dataset differs significantly from the military structure dataset constructed by Acemoglu et al. (2020). While their dataset indicates brigade jurisdictions (each encompassing multiple municipalities), our dataset tracks the specific municipality locations of army brigade and battalion headquarters. This granular approach provides a more precise measure of military presence.

Second, the causal identification of the effects of base presence is difficult because military bases were placed non-randomly according to the Colombian government's wartime strategies and the dynamics of the war. We take advantage of the temporal and geographical variation in the introduction of military bases during the massive military expansion from 2000 to 2016. Specifically, we take an event-study approach with three specifications to identify causal effects of military base presence. The first specification is the classical two-way fixed effects model with municipality and year fixed effects to account for both time-invariant municipality characteristics and yearly trends in the outcomes. The second specification modifies the first method by including division jurisdiction-year fixed effects, instead of year fixed effects to control for the aggregate economic and conflict dynamics that affect both the presence of military bases and the outcomes.

The OLS estimations do not effectively address the staggered introduction of military bases (de Chaisemartin and D'Haultfœuille, 2022; Goodman-Bacon, 2021). Thus, the third specification uses the de Chaisemartin and D'Haultfœuille (dCdH) estimation to account for the variation in the timing of military base introduction (de Chaisemartin and D'Haultfœuille, 2024). Our analysis of the pre-treatment trends reveals that the OLS estimations tend to overestimate the effects of military base presence on sexual violence. This is due to the negative weight issue, which stems from the comparison between the switchers (which change from not having a military base to having one) to the non-switchers (which already have a base). Our preferred methodology is the dCdH estimation.

We find evidence that the presence of military bases increases the rate of sexual violence per 100,000 people, based on cases registered at the Colombian Office of the Attorney General. Our analysis indicates that military base presence leads to a 72% increase in reported sexual crime cases over the 16 years following base introduction, relative to the control mean.

but our request was denied.

[4]While our official request was denied, some snippets of official records are available online, particularly on the Internet Archive's Wayback Machine (`https://web.archive.org/`).

Furthermore, this increase appears to be driven by bases with more drafted soldiers rather than those with highly trained, well-paid, volunteer soldiers. While we find a statistically significant rise in registered cases of sexual violence, we find no corresponding increase in indictments. This discrepancy suggests, first, that increased reporting does not necessarily lead to increased prosecution. Second, it indicates that heightened state presence in the form of military bases may not strengthen local judicial capacity to prosecute sexual crimes.

We further examine whether the rise in sexual violence translates into changes in fertility and child support disputes due to unintended pregnancies from rape and find no evidence of such effects. Additionally, we find some evidence of spillover effects, with neighboring municipalities potentially experiencing increased sexual violence and reduced child support disputes. However, these findings are not consistently significant across different buffer sizes.

To better understand the drivers of this violence, we assess alternative explanations and find no evidence that the observed increase is attributable to changes in security conditions, demographic shifts, or reporting behavior. Given the well-documented collaboration between certain army units and right-wing paramilitary groups, it is possible that some of the increase in sexual violence was committed by paramilitary fighters rather than official soldiers. However, because the Colombian Army was far larger than any paramilitary group and military bases were the focal points of the observed effects, we believe that the presence of government soldiers likely explains much of the increase in sexual violence documented in this study.

While we lack data to directly observe why soldiers commit sexual violence, we discuss two broad categories of explanations using economic theories, empirical findings, and historical anecdotes. First, strategic sexual violence is used to coerce civilians, extract intelligence, and deter defection, aligning with economic models of extortion and civilian control. Historical accounts indicate that some Colombian army units used sexual violence to gather intelligence and suppress suspected guerrilla supporters Second, non-strategic sexual violence arises from weak institutional oversight, peer dynamics, and exposure to violence. We speculate that poor enforcement and lack of accountability enabled opportunistic crimes, while peer conformity in close-knit military units may have reinforced norms of impunity. Future research would benefit from more detailed data on soldier characteristics, military leadership, and unit-specific policies. Such data would allow researchers to empirically test the mechanisms proposed in this study, helping policymakers design targeted interventions to prevent sexual violence committed by government soldiers.

Our paper contributes to three strands of literature. First, we expand research on state-led violence against civilians by shifting the focus from killings to sexual violence and providing causal evidence of its occurrence. Political science has developed a robust literature,

particularly on the use of mass killings and their effectiveness as a counterinsurgency measure. A comprehensive review of this literature has identified that governments use force against civilians in civil wars to secure their collaboration and loyalty, deter cooperation with rebels, and physically reduce the sources of support for rebel groups (Hultman, 2014). The effectiveness of such force is mixed and is only achieved under limited circumstances (Valentino et al., 2004; Downes, 2007). Colombia has received particular attention for its "false-positive" cases, in which government forces executed civilians and falsely identified them as guerrilla combatants to inflate enemy casualties (Human Rights Watch, 2015). In this context, Acemoglu et al. (2020) demonstrates that these killings were driven by a pay-per-performance incentive scheme and a lack of accountability. While these studies have advanced our understanding of state-perpetrated killings, little research has examined other forms of state-led violence, such as sexual violence, despite its well-documented occurrence in conflict settings. Our study addresses this gap by providing causal estimates of the effects of military base presence on sexual violence.

Second, we extend the literature on the drivers of conflict-related sexual violence by causally linking the presence of state armed forces to sexual violence. In examining the determinants of conflict-related sexual violence, political scientists have described sexual crimes by state actors.[5] Cohen and Nordås (2014) compiled data on conflict-related sexual violence across the world from 1989 to 2009, revealing that state actors are more frequently reported as perpetrators of wartime sexual crimes than non-state armed actors such as insurgency groups. Similarly, Leiby (2009) also reports that the great majority of sexual violence cases in Guatemala and Peru are attributed to the public forces. Our paper builds on these findings by providing causal estimates of the impact of state military base presence on sexual violence.

Third, this paper contributes to the literature on the effects of military bases by focusing on sexual violence, an area that has been largely overlooked. While most studies on base placement come from military science and strategic studies, offering qualitative explorations of political, social, and environmental effects, economic research has primarily examined the impact of base closures on local economies in the U.S. and Europe. These studies have generally found no significant effects (Andersson et al., 2007; Paloyo et al., 2010), although Zou (2018) observed a decline in civilian employment in German communities

[5]Nordås and Cohen (2021) provide a comprehensive review of the political science literature on this topic. Two recent economics studies have expanded the understanding of the causes of conflict-related sexual violence. Guarnieri and Tur-Prats (2023) attribute the intensity of sexual violence to gender norms in 33 ethnic civil wars in Africa from 1989 to 2009. Laurent-Lucchetti et al. (2023) use the volatility in international gold prices to explain how armed groups use sexual violence to extract labor from local communities for mining labor-intensive resources such as gold.

following American base closures. Booth (2003) provides valuable insights by examining the effects of military bases on women's wages; however, the interpretation of the results may be influenced by omitted variable bias and reverse causality. While historians have examined the consequences of military bases, particularly those of foreign origins, on the sex trade, economics studies on this topic are rare. Among the few economics studies, Brodeur et al. (2017) use structural estimation to link U.S. military presence to the expansion of Thailand's sex industry. Our paper contributes to this literature by examining the effects of the presence of soldiers on civilians where they both belong to the same nation, and by focusing on sexual violence.

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. Section 6 investigates potential reasons why soldiers have committed sexual violence using economic theories, empirical studies, and anecdotal accounts. Finally, Section 7 concludes with a summary of our findings and their implications for future research.

2 Context

2.1 The Colombian Conflict: Military Expansion in the 2000s

From the end of the 1990s to the beginning of the 2000s, Colombia faced confrontations with numerous armed groups. In response, President Andrés Pastrana rekindled the country's relationship with the United States to receive military support; notably, this shift aligned with U.S. policy trajectories in the War on Drugs (Ruiz, 2001). From 2000 onward, U.S. military support to Colombia, primarily to counter narcotics, swelled under the banner of Plan Colombia. In 2002, President Álvaro Uribe continued military expansion under his Democratic Security Policy, targeted at augmenting the state's influence in areas experiencing the presence of non-state armed actors.[6]

Through Plan Colombia, the United States provided helicopters, weapons, ammunition, vehicles, and training to modernize the Colombian Army and increase its readiness for unconventional guerilla warfare (GAO, 2008). The material and financial assistance drastically amplified the prowess of the Colombian armed forces, particularly the army, the country's

[6]Figure A2 plots U.S. military aid through the State and Defense Departments, highlighting a pronounced rise from 2000 over a 15-year span.

largest military branch, by increasing both its size and the capacity of its personnel. Between 2000 and 2008, the size of ground forces increased by 50% as a result of the U.S. support of over US$ 104 million (GAO, 2008).

The army added new brigades and battalions to accommodate this expansion, building new bases all across the country. Once the government authorized the establishment of a new base, soldiers were deployed almost immediately to the area, without waiting to build a physical facility. Given the great need for new military bases due to the intensity of the war in the 2000s, soldiers initially camped in the area designated for the new base, while they worked on missions, which often included building the new facility. Figure 1 illustrates this expansion. The number of municipalities with military bases increased from 69 municipalities in 1998 to 145 municipalities in 2016. This expansion is spread all across the country, as shown in Figure A1.

We define the term *military base* to mean the physical main center of either a brigade or battalion. To further explain the nature of brigades and battalions, we first briefly discuss the organizational structure of the Colombian National Army, and then describe the soldiers staffing these units.

2.2 National Army of Colombia

At the top of the army hierarchy are the commander and second commander of the army in Bogotá, who directly preside over ten *divisions*.[7] Each division typically has two to five *brigades*. A brigade usually consists of two to five *battalions*. A battalion typically consists of about five companies. Each company is generally staffed with around 800 soldiers. This indicates that a brigade base can have anywhere between 8,000 and 20,000 soldiers, while a battalion base can have around 4,000 soldiers on the premises.

There are two different kinds of brigades (standing and mobile) and two different kinds of battalions (standing and counterinsurgency).These units vary in terms of the types of soldiers and strategic purposes, as explained in the rest of this subsection. We use these differences in our statistical analysis to explore the potential heterogeneity of the effects of military base presence and their mechanisms.

Standing brigades and battalions. Standing brigades and battalions are military units commonly present in regular armies. These units have a fixed location and territorial jurisdiction that rarely varies over time. These brigades and battalions are mainly staffed with conscripted soldiers, known as *basic soldiers*, who serve a mandatory minimum of 18 months up to 24 months.[8] Members of these units are usually assigned to protect roads, electrical

[7]Figure A3 shows the organization of the Colombian National Army during our analysis period.

[8]Colombia's conscription system requires all male citizens aged 18 to 24 to serve in its armed forces, with

systems, and other infrastructure that could be targeted by non-state armed actors. In addition, these brigades and battalions carry out counterinsurgency operations locally, which are mostly conducted by basic soldiers (Dávila, 1999).

Mobile brigades and counterinsurgency battalions. Mobile brigades and counterinsurgency battalions specialize in guerrilla warfare. They are the main human resources that the army uses to fight against non-state armed actors. These units are predominantly staffed by *professional soldiers* who, after completing the mandatory 18 months of military service, receive substantial and periodic training and are provided with significant compensation and health benefits, serving for up to 20 years (Human Rights Watch, 1993a). Because of the differences in age and training, professional soldiers typically are better educated than drafted soldiers.

The reinforcement of the army through increasing mobile brigades and counterinsurgency battalions is the centerpiece of the military restructuring that took place during the period under study. The army needed well-trained and disciplined soldiers to confront guerrilla and paramilitary groups in unconventional combat settings in the mountains and jungles of Colombia. As a result, soldiers in these units often move between battle zones for extended periods.

Basic and professional soldiers. As described above, there is a substantial difference between the basic and professional soldiers staffing the two broad categories of military units. Both soldier types are deployed to and reside on various military bases across the country. Both basic and professional soldiers can be transferred to multiple bases during their terms. However, professional soldiers, being more highly trained, tend to be transferred more frequently depending on military needs. This section explains the key differences between these soldier categories and describes their deployment and compensation patterns.

Professional soldiers normally go through an operational cycle. They start with a three-week training period before being deployed to the field. These trainings are not conducted on their bases but at various training centers. After this phase, soldiers are sent to conduct military operations for three to four months. The deployment period is followed by a rest phase of three weeks, during which soldiers usually go back to their places of origin to visit their parents, families, and friends.

Meanwhile, basic soldiers follow a different pattern of field deployment. The compulsory military service starts with a training phase of 10 weeks, followed by a specialization period spanning 6 to 8 weeks. After this training period, basic soldiers rest for two weeks, during which they are allowed to leave the military base. Once they return, soldiers are deployed

some exceptions. Female citizens may participate voluntarily (Suarez, 2023). This means that the great majority of soldiers at the military bases considered in this analysis are young men.

to the field for a period ranging from 12 to 14 months. During this time, basic soldiers follow the same operational cycle as professional soldiers. According to current and retired army officers, military units usually assign basic soldiers to the protection of fixed positions (i.e., military bases and infrastructure such as roads and electrical grids). Their operational cycle finishes with an adaptation-to-civilian-life phase, where they take technical courses to facilitate their reintegration into the labor force.

Both basic and professional soldiers follow a strict set of disciplinary rules while living on military bases. Naturally, their movement in and out of the bases is restricted. All soldiers must obtain permission from their superiors to leave their bases, which is granted only in special circumstances, as officers expect soldiers to attend to personal matters during their rest periods. Meanwhile, soldiers are allowed to invite guests to their bases on Sundays, if local security conditions permit. Guests are not limited to immediate families; therefore, soldiers can invite their sexual partners. Army officers mentioned that sometimes non-single professional soldiers are allowed to visit their partners outside their bases and are not limited to the regular Sunday on-base visit.

The most important difference between these two categories of soldiers, in terms of this project, is compensation. Just on the basis of monthly compensation, professional soldiers are paid over 800% more than basic soldiers.[9] Furthermore, professional soldiers, as employees of the army, receive a comprehensive package of benefits, including seniority bonus, annual service bonus, vacation bonus, Christmas bonus, travel allowances, vacation entitlement, severance pay, housing benefits, family subsidy, and burial expenses.[10] Because they also receive uniforms and necessities while living on the bases, much of these earnings are disposable income, especially when they are single.

To illustrate the difference in compensation, we compare the approximate annual compensation of hypothetical basic and professional soldiers in 2010, with a legal minimum monthly wage of US$118.06 (Datosmacro, 2022). The basic soldier's annual compensation was approximately US$215.[11] Meanwhile, the annual total compensation for the professional soldier, inclusive of annual service, vacation, and Christmas bonuses, was about US$2,231 if single, and US$2,311 if married.[12] In summary, professional soldiers earn approximately 10 times more than basic soldiers.

While we unfortunately do not have data on the composition of army soldiers by rank,

[9]Table A1 provides a comparison of compensation by soldier class.

[10]Table A2 describes these benefits in detail.

[11]US$17.92 * 12 months = US$215.04.

[12]If single, US$165.29 (monthly salary) * 12 months + US$82.65 (annual service) + US$82.65 (vacation) + US$82.65 (Christmas) = US$2,231.43. The annual family subsidy of US$79.34 is added if the professional soldier is married.

the 2007 Ministry of Defense report provides some insight. In 2007, the report states that professional soldiers represented about 39% of the army's soldiers, while the remaining composition included regular soldiers (48%), village soldiers (12%), and bachelor soldiers (1%), all of which are categorized as basic soldiers (Ministry of Defense of Colombia, 2007). Clearly, the basic soldier class dominates in number, though the professional soldier class had a significant presence.

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, fertility patterns, and child support disputes. This section describes the mechanisms through which base presence can influence these outcomes.

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. This dataset, crafted from national and local newspaper articles, covers the period between 2000 and 2010, and has been expanded through additional research up to 2016. In analyzing fertility, we utilize comprehensive birth certificate data spanning from 1998 to 2016.

3.1 Treatment Data: Military Bases

We obtain the data on military base presence from national and local newspaper articles published between 2000 and 2010.[13] We obtained these articles from the newspaper database called Digital Press Archive, offered by the Popular Research and Education Center/Program for Peace (Cinep/PPP). The database provides access to over 700,000 digitized publications from 10 national and regional press sources since 1997, 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 used two keywords to narrow our search for relevant articles; brigade (*brigada*) and battalion (*batallón*). Thus, our military base data come from approximately 11,000 scanned newspaper articles that contain the words brigade

[13]The 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 Defense of Colombia, but our requests were denied multiple times.

and/or battalion, published from January 1, 2000 to December 31, 2010. We then used Google Cloud Vision to detect texts in the scanned articles. We used the combination of ChatGPT and human detection to construct a municipality-year panel dataset that indicates the geographical and temporal existence of brigades and battalions, as shown below. We describe this process in detail in Appendix A.3.

3.2 Outcome Data

In this section, we discuss the sources of the outcome data and the construction of the outcome variables. Table A3 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.

Sexual Violence and Child Support Lawsuits Data. We obtained the data on sexual crime and child support lawsuits recorded between 2000 and 2021 from the Office of the Attorney General of Colombia, which collects information on all lawsuits in the country through its mandate to investigate crimes, prosecute offenders, and review judicial processes. A case is registered in the institution's system when either an investigation is opened by the office itself or a person reports an incident to a police station or the Attorney General's Office. There are two types of cases in this administrative database. *Registered* cases, or *procesos* in Spanish, are those where the office acknowledges the existence of such reported cases. *Indicted* cases, or *indiciados*, are those for which suspects are formally accused by the office.

The 2000-2010 dataset that we received was already aggregated by the specific law violated (related to sexual crimes and child support) by year and municipalities where crimes were reported to have occurred. These data only contain the number of cases per law per municipality per year. Unfortunately, we do not have any further information about these cases, such as the sex of the denouncers and the accused. We then counted the numbers of registered and indicted cases of all sexual crimes and child support violations in this dataset for each municipality in each year. The 2009-2021 dataset that we obtained was also aggregated, but by specific law violated. We took this dataset and counted the numbers of registered and indicted cases of sex crimes and child support violations for each year and municipality of the event. In addition to the counts of sex crime and child support cases, we also calculated the cases per 100,000 inhabitants by dividing the counts by the annual municipal population.

There are two important considerations regarding the judicial records on sexual crimes. First, while sexual crimes are notoriously underreported in many contexts, including the

Colombian armed conflict, we believe that the data from the Attorney General's Office is the most comprehensive source for this analysis. Alternative data sources, such as diagnostic records from the Ministry of Health, have reported doctors' assessments of potential sexual violence since 2004. However, the temporal coverage of this dataset is limited, as data before 2009 are currently unavailable. This narrow temporal range would significantly reduce our sample size, particularly excluding periods when many military bases were newly established. Therefore, we rely on the judicial records due to their broader temporal coverage, which better fits our research context.

Although underreporting remains a concern, we believe that judicial records are not more susceptible to underreporting than health diagnostics data. Victims of sexual violence in Colombia often do not seek medical help after sexual assault due to financial costs, limited access to healthcare facilities, and societal stigma (Center for Reproductive Rights, 2020a). Given these barriers, it is unlikely that the health diagnostics data provide a more accurate representation of sexual violence incidents. Therefore, the judicial records offer the most reliable and consistent dataset available for analyzing the effects of military base presence on sexual violence.

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. Investigations conducted by Colombian government agencies concluded that 85-89% of reported cases of sexual violence involved women and girls (JEP, 2023b; Amnesty International, 2011). At least one of these investigations also analyzed the data from the Attorney General's Office used in this paper. Therefore, we believe that the great majority of sexual crime cases counted in our dataset also involved women and girls.[14]

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.[15] We then divide the number of conceptions by age-appropriate population to

[14]We acknowledge that sexual violence impacts people across all gender identities and sexual orientations, not just women. Gender and sexual minorities face targeted violence that is even less likely to be captured in official crime registries. This is partially due to the relatively small population size of these groups, but also due to widespread under-reporting stemming from stigma around non-traditional sexuality and gender expressions. While we do not have data to quantify this, we are aware that such cases against gender and sexual minorities were perpetrated by non-state armed groups, in particular, during the conflict (Colombia Diversa et al., 2015). The official statistics on sexual violence are likely an underestimate, especially for those whose identities lie outside of the male/female binary categories.

[15]The average gestation period is based on vital statistics showing that approximately 98% of pregnancies

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 2026 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 Municipalities in the Sample for Analysis

Table A4 presents the number of unique municipalities included in our analysis for each year from 1998 to 2016, ranging from 1,089 to 1,111 municipalities. As of 2024, Colombia comprises a total of 1,123 municipalities. Our study excludes certain areas for specific reasons:

1. We omit the municipalities of San Andrés and Providencia, which are small islands in the Caribbean Sea, because they had no army brigade or battalion during our study period.

2. We also exclude the seven major cities: Barranquilla, Bogotá, Bucaramanga, Medellín, Cali, Cartagena, and Cúcuta. These cities are outliers in terms of population size and have a large number of military institutions, including many specialized units that differ significantly from standard brigades and battalions in terms of soldier composition and function.

These exclusions ensure that our analysis focuses on municipalities that are more representative of the typical Colombian context and have comparable military presence.

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).

Table 1 describes the basic characteristics of all municipalities in the sample for analysis in the earliest year of data availability, before the large-scale military expansion occurred. 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.

On average, municipalities with base presence exhibit several distinct characteristics compared to those without bases. They are larger in size, situated at a lower altitude, and have substantially larger populations. The total, female, and male populations of municipalities with bases are almost twice as large as those of municipalities without bases. However, there is no meaningful difference in the female to male sex ratio between the two groups.

The data also reveal significant disparities in violence levels. The mean homicide rate is about 70% higher in municipalities with bases, while the mean kidnapping rate is 160% higher. Additionally, municipalities with bases show higher rates of forced displacement.[16]

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 the location and timing of military base introduction are not exogenous to unobservable municipality characteristics. Military units are placed strategically, and in the context of Colombia's conflict, they are particularly positioned for counterinsurgency. To address this issue, we leverage the longitudinal nature of the municipality panel data, employing an event-study approach to estimate the effect of military units on sexual violence, fertility, and child support disputes.

Therefore, we estimate:

$$y_{it} = Base_i \times \sum_{\substack{m=-4 \\ m\neq -1}}^{7} \mathbb{1}(t - t_i^* = m)\beta_m + \alpha_i + \eta_t + \epsilon_{it} \tag{1}$$

where y_{it} is an outcome in municipality i in two-year period t; $Base_i$ is a binary variable indicating whether municipality i has had at least one military base between 1998 and 2016; $\mathbb{1}(t - t_i^* = m)$ is the time relative to the military base introduction period t_i^*; α_i is the

[16]Forced displacement is an involuntary movement of people from their home due to conflict, violence, or human right violations.

municipality fixed effects; η_t is the year fixed effects; and ϵ_{it} is a time-variant unobserved term at the municipality level. The omitted group is $m = -1$, the period before the military base introduction. We cluster the standard errors at the municipality level.

We conduct our analysis at the two-year period level by aggregating the number of sexual crimes, births, and child support disputes in each municipality over two years. This is because sexual violence and disputes over child support at the municipality level are relatively rare. Aggregating every two years helps detect changes in these outcomes. For 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. We split the analysis timeline from 1998 to 2016 into 10 two-year periods, with the first period spanning from 1998 to 1999, and the last period containing only 2016. Therefore, the maximum number of periods a municipality can be treated is 10.

Estimating Equation 1 mitigates potential bias from time-consistent municipality characteristics that affect the number of conceptions and yearly trends in the outcome. However, reproductive outcomes can be influenced by aggregate economic and conflict dynamics that vary across time and geography. For instance, increased economic activities may make people more or less willing to have children, while exacerbated conflict intensity may affect these decisions due to security concerns and instability. Moreover, the Ministry of Defense likely considered factors such as conflict intensity and economic relevance when determining the allocation of military bases. Failure to account for these factors could lead to omitted variable biases.

To address geographically and temporally variant economic and conflict dynamics, we include army division jurisdiction-year fixed effects, δ_{dt}, and estimate the following equation with OLS:

$$y_{it} = Base_i \times \sum_{\substack{m=-4 \\ m\neq -1}}^{7} \mathbb{1}(t - t_i^* = m)\beta_m + \alpha_i + \delta_{dt} + \epsilon_{it}. \tag{2}$$

A division in the Colombian Army is a class of units that presides over the brigades within its hierarchy. Each division is assigned a portion of Colombian territory for which it is responsible. For consistency, we use the 1999 division classification throughout the period of analysis. In 1999, there were five divisions collectively responsible for the security of Colombia's entire land territory. Again, the omitted category is $m = -1$, and standard errors are clustered at the municipality level.

The parameters of interest are β_m for $m \geq 0$, which capture the average effect of military base presence on the outcome in the mth period after the base introduction. We hypothesize

that the presence of a military unit, on average, leads to an increase in sexual violence, fertility, and child support disputes, in municipalities with military units compared to those without.

OLS estimation may not produce unbiased estimates of military base effects, because it may fail to account for variation in treatment timing. In our context, municipalities receive military units at different times, and treatment timing is potentially endogenous to municipality characteristics. To mitigate this challenge, we use the dCdH estimator (de Chaisemartin and D'Haultfœuille, 2024). This estimator does not require the treatment to be all-absorbing, unlike other recent difference-in-differences (DID) estimators, and accommodates treatment with non-random variation in treatment timing. Because a municipality can lose a military base before the end of the period of analysis, this estimator is suitable for our treatment allocation. We visualize the treatment duration for each municipality in Figure A4. Standard errors are also clustered at the municipality level for this estimation method.

All three approaches require two assumptions for successful estimation of military base effects: parallel trends and no anticipation. As Table 1 suggests, municipalities with bases likely have different trends than those without. To address this concern, in addition to using different combinations of fixed effects in the OLS estimation and using the dCdH estimation, we will exclude the never-treated municipalities, because not-yet-treated municipalities are intuitively better control units. In the following section on results, we will investigate the pre-treatment trends on all the outcomes to see if the parallel trends assumption can be plausibly met. Additionally, the non-random variation in treatment timing can contribute to failing to satisfy the parallel trends assumption. As the timing of military base introduction was largely determined by the conflict dynamics, we will inspect the pre-treatment trends on the security measures in the following section. Furthermore, we believe that the immediate deployment of soldiers after the decision to establish a new base allows us to satisfy the no-anticipation assumption. By estimating β_m for $m < 0$, we will check whether the pre-trends are sufficiently balanced across the treated and control groups to plausibly satisfy these assumptions.

As described in Section 2, army units are diverse. Therefore, we consider the following categories of military base presence:

1. Whether a municipality has at least one military base (standing brigade, standing battalion, mobile brigade, or counterinsurgency battalion)

2. Whether a municipality has at least one standing unit (standing brigade or standing battalion with drafted soldiers)

3. Whether a municipality has at least one counterinsurgency unit (mobile brigade or

counterinsurgency battalion with professional soldiers)

The first category is the broadest, encompassing all types of military units. The second one focuses on the presence of standing units which are largely staffed with drafted soldiers. The third one indicates the presence of counterinsurgency units that are mostly operating with professional soldiers. In our sample for analysis, 62% of the treated observations have at least one standing unit with drafted soldiers, 33% have at least one counterinsurgency unit with professional soldiers, and 5% have both (Table 2). To explore the heterogeneity by standing and counterinsurgency units, we estimate the effect of standing or counterinsurgency units on the outcomes of interest, controlling for the presence of the other type of unit.

The municipality-level data on sexual violence and child support disputes contain a large number of observations with zeros, around 35% for registered cases and 55% for indicted cases. In economics, it is common to transform skewed outcomes using the natural logarithm or inverse-hyperbolic sine (IHS) to achieve normally distributed residuals. However, we chose not to transform our outcomes, and deal with the mass of zero issue by simply aggregating the outcome data by two years. We make this choice because recent studies have shown that these transformations can be problematic when the outcome includes a significant number of zeros. Mullahy and Norton (2024) demonstrate that, in linear regressions, estimates from transformed data with few zeros are similar to those from scaled linear probability models. However, when the data contain many zeros, estimates can vary significantly depending on the parameters chosen for the logarithm or IHS transformation. Furthermore, Chen and Roth (2023) suggest that estimates from transformed outcomes with a high proportion of zeros cannot be straightforwardly interpreted as percentage changes, complicating standard interpretation.

5 Results

This section presents the results of our statistical analysis. First, we estimate the effects of military base presence on sexual violence (subsection 5.1). Next, we explore alternative explanations to assess whether the observed increase in sexual violence can be plausibly attributed to the presence of government soldiers (subsection 5.2). We then examine whether this rise in sexual violence leads to changes in fertility and child support disputes (subsection 5.3), with a focus on potential heterogeneity by mothers' marital and partnership status. Although we cannot observe victims' marital status for sexual violence cases, we investigate whether single women, who may be more exposed to soldiers, experience different fertility outcomes. Finally, we analyze potential spillover effects of military bases on neighboring municipalities (subsection 5.4).

5.1 Effects on Sexual Violence

We now discuss the results on sexual violence. Our analysis shows that the presence of military bases leads to a significant increase in registered sexual violence cases, particularly in municipalities hosting standing units with drafted soldiers. This effect is most pronounced in the early years following base introduction.

Figure 2 illustrates the estimated impact on the number of sexual violence cases per 100,000 inhabitants. As outlined in Section 3.2, *registered* refer to those acknowledged by the Office of the Attorney General's Office, while *indicted* cases involve formal accusations against suspects.

First, the dCdH estimation shows a more balanced pre-trend, especially in the two years preceding base introduction, compared to the OLS estimations, which display a positive pre-trend. This discrepancy suggests that the OLS estimates are inflated due to the negative weight issue, arising from comparisons between switchers (municipalities transitioning from no base to having one) and non-switchers (those with pre-existing bases) (de Chaisemartin and D'Haultfoeuille, 2022). Consequently, OLS estimates of post-treatment effects are consistently higher than those from dCdH. Given this, we focus on the dCdH estimates in the subsequent analysis.

Second, the dCdH estimation indicates that military bases may have contributed to an increase in sexual crime rates. Specifically, registered cases rise by approximately seven per 100,000 inhabitants during years 2 and 3 and years 4 and 5 following base introduction, although the increase in years 0 and 1 is not statistically significant. This initial rise diminishes after years 6 and 7, with no further significant changes for the remainder of the analysis period. Notably, no significant change is observed for indicted cases.

Table 3 presents the average effects across 16 years for both registered and indicted cases, calculated using the dCdH estimator. These total average effects are weighted sums of all two-year period effects, with weights corresponding to the number of observations in each period (de Chaisemartin and D'Haultfoeuille, 2022). According to the dCdH estimates, registered case rates increased by 16 per 100,000 inhabitants, which is statistically significant at the 5% level. This corresponds to a 72% rise in registered sexual violence cases relative to the control mean of 22 cases per 100,000 inhabitants over 16 years.

To investigate if a particular type of soldiers drive this increase, we disaggregate the effects by base type, focusing on dominant soldier characteristics. As described in sections 2 and 4, military bases are categorized into: 1) standing units primarily composed of drafted soldiers who receive minimal stipends and 2) counterinsurgency units staffed by professional soldiers who are better educated, more extensively trained, and receive higher salaries with comprehensive benefits.

Figure 3 presents the disaggregated results. We find that the increase in registered cases is primarily driven by municipalities hosting standing units with drafted soldiers. The data show that the presence of standing units with more drafted soldiers increases registered cases, especially in the first eight years (first four two-year periods) after the base introduction. In these areas, registered cases rise significantly during the first eight years (first four two-year periods) following base introduction. Conversely, we observe no substantial change in indicted cases, though a marginally significant increase is noted in the first two years for standing units with drafted soldiers.

Meanwhile, we find no statistically meaningful change in either registered or indicted cases due to the presence of counterinsurgency bases predominantly occupied by better-paid, well-trained professional soldiers. However, these results may not necessarily be interpreted as evidence that professional soldiers do not commit sexual crimes. One possible explanation is that the Army may have stronger incentives to conceal misconduct by professional soldiers, as they are less replaceable and more strategically important in the Colombian conflict than drafted soldiers. This potential reporting bias highlights the role of institutional accountability in shaping observed patterns of sexual violence. We further discuss this possibility in Section 6.

To further investigate the relationship between military presence and sexual violence, we examine the intensive-margin effect, considering how the number of military bases in a municipality influences sexual violence rates. As shown in Figure 4, the vast majority of treated municipalities host only one military base, while a small minority have multiple bases. Given this distribution, we expect the intensive-margin effect to closely resemble the extensive-margin effect.

Figure 5 presents the estimated effects of the number of military bases on sexual violence rates per 100,000 inhabitants. We find that the impact of additional bases on registered cases of sexual violence is similar in magnitude to the previously discussed extensive-margin effect. This suggests that the introduction of a single base is sufficient to drive most of the observed increase in sexual violence, with additional bases contributing only marginally to further increases.

However, the intensive-margin analysis reveals a more persistent positive effect over time. Unlike the extensive-margin results, where the effect diminishes after years 6 and 7, we find statistically significant increases in years 10 to 13 as well. This persistence suggests that municipalities with multiple bases may experience prolonged exposure to risk factors associated with military presence, potentially due to sustained interactions between soldiers and the local population.

Table 4 presents the total effect of military bases at the intensive margin. On average,

each additional base increases registered sexual violence cases by 12 per 100,000 people over the 16-year period following base introduction, translating to a 55% increase relative to the control mean. In contrast, we observe no statistically significant effects on indicted cases, consistent with the baseline findings on the presence of military bases.

To sum, we find that base presence has led to an increase in registered cases of sexual crime but no in indictment. This finding implies that increased reporting does not necessarily lead to increased prosecution. Second, it suggests that heightened state presence in the form of military bases may not strengthen local judicial capacity to prosecute sexual crimes.

Furthermore, our findings suggest that the presence of military bases leads to an initial surge in sexual violence that gradually subsides, largely driven by the presence of standing bases with drafted soldiers. This temporal pattern may indicate that less-trained drafted soldiers engage in misconduct during the early stages of base establishment. The gradual return to baseline levels of sexual violence over time could be attributed to several potential explanations.

First, the Army may have learned to better manage and discipline the behavior of drafted soldiers as base operations became more established. This is particularly plausible given that the rapid military expansion likely outpaced the institution's capacity to effectively oversee and train new recruits. Second, the decline may reflect improved concealment of misconduct over time as management practices evolved. However, this explanation is less likely, as the learning curve for concealment would likely be shorter than the observed decline, which spans eight years after the third year of base introduction.

Lastly, members of host communities may learn to better cope with the presence of military bases. For example, community members may have learned to avoid areas with a high likelihood of encountering soldiers, thereby reducing opportunities for sexual violence. This is consistent with anecdotal evidence from a report on conflict-related sexual violence in Colombia published by ABColombia (2023). The report documents that women in Buenaventura "strategically altered their daily activities, avoiding public spaces and night outings to reduce the risk of sexual violence." Similarly, in Chocó, indigenous women "restricted their movements and avoided traditional gathering places" to minimize contact with armed actors. These adaptive responses illustrate the complex interactions between military presence and community behavior over time, highlighting the agency of local populations in navigating conflict environments.

5.2 Mechanisms

To better understand the drivers of increased sexual violence in municipalities with military bases, this section explores potential mechanisms beyond the direct presence of army soldiers. Specifically, we examine changes in security conditions, demographic shifts, and the influence of other armed actors as alternative channels through which military bases may affect sexual violence. Investigating these mechanisms helps rule out confounding factors and strengthens the causal interpretation of the observed effects as resulting from the presence of army soldiers.

Change in Security. One potential mechanism driving the observed increase in sexual violence is a change in security conditions in municipalities with military bases. It is possible that these municipalities become hotspots for crime and violence as military bases attract attacks from opponents, leading to general security deterioration and increased vulnerability for civilians, including a rise in sexual violence. Alternatively, the army may have strategically placed bases in areas already experiencing elevated violence. To investigate these possibilities, we examine the effects of military base presence on various forms of violence.

Figure 6 shows the estimated effect of military base presence on cases of homicide and non-homicide violent crimes per 100,000 people. Non-homicide violent crimes include intimidation, terrorism, kidnapping, and forced displacement. First, we find no strong evidence of pre-treatment differences between treated and not-yet-treated municipalities, supporting the parallel trends assumption. Second, we observe no statistically significant changes in these outcomes due to base presence. These results indicate that military bases do not meaningfully affect actual or perceived security levels in host communities.

We present the detailed results for each type of non-homicide violent crime in Figure A5. Consistent with the aggregated results, we find no significant effects on intimidation, terrorism, kidnapping, or forced displacement. Together, these findings suggest that changes in general crime and violence are unlikely to explain the observed increase in sexual violence, reinforcing the interpretation that the rise is directly linked to the presence of military personnel.

Demographic Change. Another potential mechanism driving the observed increase in sexual violence is demographic change in municipalities with military bases. Military bases can affect migration through job creation, altering the demographic composition and potentially leading to increased sexual violence. For 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 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). To test these mechanisms, we examine the effects of military base presence on municipality population counts disaggregated by sex and on sex ratio.

Figure 7 presents the estimated effects of military bases on log municipality population counts by sex. It is important to note that the population data likely do not include soldiers because they are estimated based on the census. Therefore, the results should be interpreted as changes in the *civilian* population. We find no statistically significant effects of military base presence on either female or male civilian population. Figure 8 presents the estimated effects of military bases on female to male sex ratio. We find no strong evidence of change in sex ratio due to base presence.

These results suggest that military bases do not meaningfully impact civilian demographic composition, ruling out population change and sex ratio shifts as mechanisms driving the observed increase in sexual violence. This finding reinforces the interpretation that the rise in sexual violence is directly linked to the presence of military personnel rather than shifts in the civilian population. By ruling out these demographic channels, the analysis strengthens the causal interpretation of the base effects on sexual violence.

Change in Reporting. The introduction of military bases, as extensions of government presence, may influence reporting behavior, particularly in remote municipalities with historically weak central government presence. This is especially relevant for sexual violence, which is known to be severely underreported in Colombia (González Támara and Barragán Moreno, 2024). If military base presence increases citizens' willingness to report incidents of sexual violence to the authorities, it could artificially inflate the observed crime rates without reflecting a true increase in incidents.

To rule out this scenario, we refer to our results on non-homicide violence. Non-homicide violence includes intimidation, terrorism, kidnapping, and forced displacement, all of which are largely based on reporting. Unlike homicide, which requires the identification of corpses and is therefore less prone to underreporting, these forms of violence depend heavily on citizens' willingness to report incidents. If military base presence influenced reporting behavior, we would expect to see similar increases in the reporting of these other forms of violence.

However, we find no statistically significant changes in intimidation, kidnapping, or forced displacement due to military base presence. These null effects suggest that changes in re-

porting behavior are unlikely to explain the observed increase in sexual violence. By ruling out reporting bias as a confounding factor, this finding strengthens the causal interpretation that the rise in sexual violence is directly linked to the presence of military personnel.

Presence of Other Armed Actors. The presence of military bases may coincide with the presence of other armed actors, who either oppose or collaborate with the official state forces. In Colombia, fighters from non-state armed organizations, rather than army soldiers, could be responsible for the observed rise in sexual violence. While we are unable to conduct a statistical analysis due to a lack of data on non-state armed actors, historical accounts indicate that the Colombian conflict has been characterized by the presence of both left-wing guerrilla groups and right-wing paramilitary organizations. Investigating this potential mechanism is crucial for ruling out confounding factors and ensuring accurate attribution of the observed effects.

We hypothesize that the increase in sexual violence in municipalities with military bases may, at least in part, be attributed to the presence of right-wing paramilitary organizations rather than left-wing guerrilla groups. This hypothesis is grounded in historical evidence of collaboration between certain units within the Colombian government forces and right-wing paramilitary organizations, including joint military operations and informal political cooperation (Human Rights Watch, 2001; Eva, 2002; Acemoglu et al., 2013). Moreover, paramilitary groups have been frequently implicated in human rights violations, including sexual violence (Commission for Truth, 2022). This close, albeit illicit, relationship between state forces and paramilitary groups suggests that at least part of the observed increase in sexual violence could be due to the presence of paramilitary fighters.

However, it is important to acknowledge that the Colombian National Army is by far the largest military institution in the nation, and the presence of government soldiers generally likely to surpass that of paramilitary fighters. Although historical data on the numbers of army and paramilitary soldiers are scarce, reports indicate that the ELN, the most prominent paramilitary group, had around 4,500 fighters at its peak in 2000 (WOLA, 2020), whereas the Colombian Army had approximately 154,000 military personnel in 2002 (Ministry of Defense, 2007). Given this stark difference in military presence, we believe that the observed increase in sexual violence is still largely attributable to the presence of government soldiers.

5.3 Potential Consequences of Increased Sexual Violence

We have established that military base presence leads to an increase in sexual violence. In this section, we investigate whether this increase translates into unintended consequences on fertility and child support disputes. Theoretically, increased sexual violence could result in

unintended pregnancies, leading to changes in fertility patterns or disputes over child support if victims are able to identify perpetrators. By examining these potential consequences, we aim to provide a comprehensive understanding of the broader social impact of military bases.

Effects on Fertility. Sexual violence, including rape, can lead to unintended pregnancies, potentially affecting fertility rates in host communities. To test this hypothesis, we examine the effects of military base presence on conception rates by age group. Figure 9 shows the estimated effects on conception rates per 1,000 women, calculated by dividing the number of conceptions by the female population in each age group.

First, we find no strong evidence of pre-treatment differences in fertility trends between municipalities with and without military bases, supporting the parallel trends assumption. Second, we observe no statistically significant changes in conception rates across all age groups following the introduction of military bases. We also find no statistically significant total effects over 16 years after the base introduction in Table 5. These null results remain consistent when disaggregating the effects by type of bases (standing units with drafted soldiers vs. counterinsurgency units with professional soldiers), as shown in Figure 10.

While the overall null results suggest that military base presence does not significantly impact fertility, we acknowledge that individual women within host communities may be affected differently. To explore potential heterogeneity, we disaggregate conception rates by marital status, focusing on single mothers versus women with partners. We hypothesize that single women may be more vulnerable to sexual violence or have different reproductive responses compared to married or partnered women. Figure 11 presents the estimated effects on conception rates for single mothers and non-single mothers. We find no statistically significant evidence that military base presence affects fertility differently by marital status, nor do we find differences across age groups (10-19, 20-29, and 30-39).

The null results on fertility may reflect competing channels that influence fertility in opposing directions. Theoretically, military base presence could increase fertility through migration that brings in more people of child-bearing age, improved security,[17] consensual relationships with soldiers, and positive income effects.[18] However, our previous analyses rule

[17]Economic theory suggests that mortality can affect fertility by changing the cost of producing a surviving child (Becker, 1992). In conflict settings, improved security could reduce mortality, potentially increasing fertility by lowering the need for replacement births. Conversely, continued insecurity could suppress fertility by increasing child mortality and discouraging childbearing.

[18]Military base presence could influence fertility through local economic effects, but theoretical predictions about the direction of this relationship are ambiguous. On one hand, increased local income from job creation and economic activities could raise the demand for children, consistent with children being normal goods (Jones et al., 2008). On the other hand, higher income could decrease fertility by increasing the opportunity cost of parents' time, leading to a preference for fewer but "high-quality" children (Becker, 1992). Empirical literature generally suggests a positive relationship between men's income and fertility (Doepke et al., 2023). In the U.S., where military bases significantly impact host communities, Zou (2018) finds that reductions

out the first two channels. Although we cannot directly observe consent in relationships, we find no evidence that military bases impact local income, as indicated by null effects on municipality GDP in Figure A6. Given the absence of evidence for these channels, we conclude that increased sexual violence did not lead to detectable changes in fertility in host communities.

Effects on Child Support Disputes. Unintended pregnancies resulting from increased sexual violence can potentially lead to disputes over child support, particularly if victims are able to identify perpetrators. While the previous subsection found no evidence of increased fertility, it remains possible that sexual violence could lead to legal disputes even without significant changes in overall fertility rates. To investigate this potential consequence, we examine the effects of military base presence on child support disputes recorded by the judicial system.

Figure 12 presents the estimated effects of military base presence on child support disputes per 100,000 inhabitants, disaggregated into registered and indicted cases. Registered cases refer to disputes formally acknowledged by the judicial system, while indicted cases involve formal legal accusations against alleged perpetrators. Our analysis provides no statistically significant evidence that military base presence increases child support disputes for either registered or indicted cases. However, we find some evidence that base presence might have led to an initial decrease in indicted cases of child support disputes during the first six years following base introduction. The aggregated effect on indicted cases further support this result. Table 6 presents the total effects on child support disputes over 16 years after base introduction. We find no statistically significant total effects on registered cases but observe weak evidence of a decrease in indicted cases by 30 cases per 100,000 inhabitant over the course of 16 year after base introduction, which translates to a 51% reduction relative to the control mean.

When disaggregating the effects by base type, distinguishing between standing units with drafted soldiers and counterinsurgency units with professional soldiers, we find statistically insignificant estimates on both registered and indited cases, but we find negative coefficients for counterinsurgency bases with professional soldier, as shown in Figures 13 and 14.

While the overall null results suggest that military base presence does not significantly impact child support disputes, we observe a potential decline in disputes during the first six years following base introduction. Although these decreases are not consistently statistically significant, they may reflect unobserved behavioral or social changes within host commu-

in military personnel led to civilian job losses, indicating positive local economic effects. However, this mechanism is less likely in the Colombian context, as communities during the study period faced ongoing armed conflict and economic instability. The absence of positive income effects in our analysis further supports this contextual distinction.

nities. For example, if the presence of military bases leads to increased fear of retaliation, victims may be less likely to pursue child support claims. However, given the lack of robust statistical significance, we interpret this pattern with caution.

5.4 Spillover Effects

As mentioned in Section 2, soldiers from any brigade or battalion can be deployed outside their bases, implying the potential for spillover effects in surrounding municipalities. Military personnel often move across administrative boundaries for security operations, military exercises, or temporary deployments, potentially influencing neighboring areas through increased military presence, altered security dynamics, or social interactions. These military activities suggest that the observed increase in sexual violence could extend beyond host municipalities. Testing for spillover effects is crucial to determine whether the impacts are localized or influence broader regional patterns.

To investigate whether the effects of military base presence extend into neighboring municipalities, we use the dCdH estimator to estimate the impact of having at least one treated neighbor within a 25, 50, or 75 km radius of the population center on sexual violence, child support disputes, and fertility. We control for whether municipalities have at least one base, isolating potential spillover effects from localized impacts. By analyzing multiple buffer sizes, we capture a range of potential spillovers while accounting for variation in soldier mobility and operational reach.

We find some evidence that the presence of military bases in neighboring municipalities might have increased sexual violence and decreased child support disputes. However, we find no evidence of spillover effects on fertility.

Table 7[19] presents the regression results on sexual violence rates per 100,000 people. We find weak evidence of positive spillover effects for registered cases of sexual violence from treated neighbors within 25 and 75 km. Specifically, registered cases might have increased by 12 per 100,000 people at the 25 km buffer and by 25 per 100,000 people at the 75 km buffer. Both estimates are statistically significant at the 10% level, translating to increases of 55% and 113% relative to the control mean over the 16-year period after base introduction, respectively. However, the estimates are not consistently significant across buffer sizes, and we observe no statistically significant changes for indicted cases.

Table 8 shows the results on the number of conceptions per 1,000 women in neighboring municipalities. We find no statistically significant changes in fertility across all age groups or buffer sizes. These results are consistent with the null findings on fertility in host mu-

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

nicipalities, reinforcing the interpretation that military base presence does not significantly impact reproductive outcomes in either host or neighboring areas.

Table 9 presents the estimated effects on child support disputes per 100,000 people. We find that the presence of treated neighbors within 25 km is associated with a decrease of 28 cases per 100,000 people for indicted cases, statistically significant at the 5% level. This estimate translates to a 47% decrease over 16 years. The magnitude of this spillover effect suggests that the negative impact on child support disputes in neighboring municipalities could be as substantial as the direct effect observed in treated municipalities. This finding indicates that military base presence may discourage victims from pursuing formal legal actions not only in host communities but also in neighboring areas.

Our analysis provides some evidence that military base presence led to an increase in sexual violence and a decrease in child support disputes in neighboring municipalities. However, the weak statistical significance and inconsistency across buffer sizes caution against strong causal claims. The observed spillover effects on sexual violence are not consistently significant across distances, suggesting that any potential spillovers are likely localized and not broadly influencing the region. The null results on fertility further reinforce the interpretation that military base presence does not significantly impact reproductive outcomes beyond host municipalities.

6 Discussion: Explaining Soldiers' Behaviors

Our analysis has revealed that the presence of government soldiers is associated with a substantial increase in sexual violence. This raises a critical question: What motivates soldiers to commit these crimes? While we lack direct data on individual soldier characteristics or military institutional policies, we can gain insights by drawing on theoretical frameworks from economics and political science. These disciplines offer explanations for why sexual violence occurs in conflict settings and help contextualize the patterns observed in our study.

Research on conflict-related sexual violence often distinguishes between strategic and non-strategic motives (Nordås and Cohen, 2021). Strategic sexual violence serves a military or organizational function, such as controlling civilians, enforcing taxation, or fostering group cohesion. In contrast, non-strategic sexual violence results from weak institutional oversight, peer influences, and exposure to violence, allowing opportunistic crimes to persist.

In this section, we discuss these two broad categories of explanations, drawing on both economic and political science theories. We first explore the strategic use of sexual violence by military organizations, followed by non-strategic explanations related to discipline, impunity, and social influences.

Strategic Use of Sexual Violence. Research on conflict-related sexual violence suggests that military organizations and armed groups may use sexual violence strategically to achieve two primary objectives: controlling civilian populations and enforcing compliance, or fostering group cohesion and reinforcing ideological commitment. Importantly, the strategic use of sexual violence does not necessarily require a top-down institutional order. Military leaders at different hierarchical levels can independently issue direct orders, tolerate ongoing misconduct, or even deliberately ignore abuses as a way to advance their objectives (Wood and Cohen, 2015). This section examines these two mechanisms by first presenting the economic theories that explain them, followed by empirical evidence and case studies that illustrate their operation in practice.

One strategic function of sexual violence is to subjugate civilians, extract resources, and deter defection. This mechanism aligns with the economic model of extortion, which predicts that armed actors must use force as an up-front investment to establish credibility in their threats and extract resources in the long run (Konrad and Skaperdas, 1997). The logic is straightforward: when civilians perceive future retaliation to be highly likely, they are more likely to comply with soldiers and avoid resistance.

Empirical research supports this idea. Laurent-Lucchetti et al. (2023) show that armed groups in Africa increase their use of non-lethal violence, including sexual violence, in response to rising commodity prices, particularly in areas where labor-intensive artisanal gold mining is prevalent. Their findings suggest that coercion is strategically employed to enforce taxation and control economic resources. These results align with earlier observations by Whitaker et al. (2019), who find that sexual violence is more prevalent when rebel groups rely on extortion rather than smuggling to finance their activities. The rationale is that extortion requires a visible display of force, while smuggling necessitates cooperation with civilians, who might otherwise report them to authorities. This distinction highlights how different financing strategies shape the likelihood of armed actors resorting to sexual violence.

While these studies primarily focus on resource extortion by rebel groups, they support the broader possibility that sexual violence is also used as a means of extracting information and political support. Non-lethal violence, such as rape, can instill long-lasting fear and control without eliminating potential supporters and collaborators, whereas lethal violence, such as extrajudicial killings, risks reducing the size of a group's support base. This logic suggests that sexual violence can function as a strategic alternative to lethal repression, allowing military forces to deter defection while maintaining civilian populations under their influence.

The Colombian conflict provides further evidence that sexual violence was used as a mechanism of civilian control. Reports indicate that government forces engaged in sexual

violence to extract intelligence from civilians and to deter local populations from supporting left-wing guerrilla groups. Human Rights Watch (1993b) documents cases in which soldiers coerced women into providing information about guerrilla movements through acts of sexual violence. Similarly, Amnesty International (2008) reports that military personnel used sexual violence to punish and intimidate communities suspected of harboring insurgents. These patterns suggest that sexual violence was not merely opportunistic misconduct by individual soldiers but rather a coercive strategy designed to enforce compliance and consolidate state control.

A second explanation suggests that sexual violence fosters group cohesion within military organizations. Examining the role of social identity in organizations, Akerlof and Kranton (2005) proposes an economic model predicting that participation in shared experiences can function as a non-monetary incentive, increasing in-group loyalty and commitment. A model of identity formation further suggests that organization leaders have incentives to enforce extreme "identity-producing activities" to deter free-riding behavior (Carvalho, 2016). Under this framework, participation in gang rape or other collective acts of violence can serve as a ritual of membership, ensuring that individuals fully integrate into the group. This logic explains why, in many insurgent organizations, non-participation in sexual violence is lightly punished but not entirely prohibited; the goal is to ensure that sufficient participation occurs to strengthen internal bonds (Cohen, 2013, 2017).

However, the relevance of this mechanism for Colombia's state military remains questionable. Unlike insurgent groups that rely on forced recruitment and informal hierarchies, Colombia's military operated under a structured hierarchy with professional training. While discipline and oversight varied across units, the presence of formal ranks, national identity, and centralized command structures suggests that the military did not require sexual violence as an identity-producing mechanism to build internal cohesion.

The available evidence suggests that sexual violence in Colombia was primarily a tool of civilian control rather than group cohesion. Reports of coerced intelligence extraction and counterinsurgency suppression align closely with economic theories of coercion and compliance. In contrast, the hypothesis that sexual violence was used to foster group cohesion appears less applicable to Colombia's military. Unlike insurgent groups that rely on forced recruitment and weak internal structures, the Colombian armed forces operated under a national training system with a structured hierarchy, reducing the need for identity-producing acts of violence.

While it is possible that some unit leaders tolerated or encouraged sexual violence as a form of bonding, the institutional structure of the Colombian military makes it unlikely that this was a widespread or necessary practice. Given the patterns of violence observed

in Colombia, sexual violence was more likely a tool of repression and control rather than an internal mechanism for military unity.

Non-strategic Sexual Violence. While some instances of sexual violence in conflict settings are driven by strategic objectives, others arise from opportunism, weak enforcement, and behavioral influences. This section examines two key mechanisms underlying non-strategic sexual violence: lack of discipline and supervision, and peer influence and exposure to violence. For each, we present the economic theories that explain the behavior, and empirical evidence and real-world case studies that illustrate their relevance.

A major driver of non-strategic sexual violence is the failure of military institutions to enforce discipline and penalize misconduct. In economic terms, weak supervision and penalty enforcement lower the expected cost of misconduct, increasing the likelihood of opportunistic crimes such as sexual violence (Becker, 1968). The "rational cheater" model, developed by Nagin et al. (2002), further explains why individuals engage in opportunistic behavior when monitoring is costly and punishment is weak. According to this model, individuals will exploit gaps in oversight when the probability of being caught is low. The model predicts that as monitoring costs rise, enforcement weakens, and the likelihood of opportunistic crime increases. Additionally, research suggests that peer monitoring alone is often ineffective at deterring misconduct. Olken (2007) finds that community-level monitoring had no significant effect on reducing corruption among village contractors, whereas government audits significantly reduced financial discrepancies. This suggests that strong, top-down enforcement is necessary to prevent opportunistic abuses.

In the context of Colombia, victim testimonies suggest that sexual violence often resulted from weak military oversight rather than direct orders from superiors. Reports indicate that soldiers abused their relative power and exploited the economic vulnerability of women and girls to coerce sex (Center for Reproductive Rights, 2020b). Additionally, cases of rape occurring in close proximity to or even within military bases suggest a lack of effective monitoring. ABColombia (2013) reports a 2012 case in which a woman was raped by a soldier on the side of a road only 100 meters away from his base, and another in 2005 involving an 11-year-old girl who was raped and held captive until the next morning by a soldier at his base. These cases highlight two possible explanations. First, some ranking officers may have deliberately ignored misconduct, either due to indifference or a belief in impunity. Second, the rapid military expansion may have strained institutional capacity, weakening the military's ability to enforce discipline and monitor soldiers' actions effectively.

Our finding that the presence of counterinsurgency bases with better-trained, well-paid professional soldiers does not lead to a statistically significant increase in sexual violence may be explained by the role of opportunism. For ranking officers, professional soldiers are

far more irreplaceable than drafted soldiers due to their longer experience and specialized training in counterinsurgency operations. As a result, officers may have stronger incentives to overlook instances of sexual misconduct by professional soldiers, thereby lowering the expected cost of committing such crimes. In turn, professional soldiers may respond to this implicit leniency by engaging in sexual violence. This dynamic could explain why we do not observe a statistically significant increase in sexual violence in municipalities hosting counterinsurgency bases.

A second explanation for non-strategic sexual violence is the influence of peer dynamics and exposure to violence, both of which can shape individual behavior in military environments. The economic theory of conformity, developed by Bernheim (1994), suggests that individuals adjust their behaviors to align with group norms, particularly when others' true preferences are not directly observable. When individuals value their social standing within a group, they are more likely to engage in behaviors that reinforce group identity, even if those behaviors are harmful. A more general peer effect model, developed by Boucher et al. (2024), further expands on this idea. The authors find that conformism plays a dominant role in shaping risky behaviors, indicating that individuals are more likely to engage in harmful actions if such behaviors are normalized within their peer group. Wood (2018) notes that soldiers often live and work in close-knit units, sharing meals and sleeping quarters, creating a particularly strong peer environment. Additionally, exposure to trauma and violence during conflict may make soldiers more likely to commit sexual violence. Research on veterans of U.S. and U.K. armed forces finds that combat exposure is associated with increased rates of incarceration, intimate partner violence, and other forms of aggression (MacManus et al., 2015; Kwan et al., 2020; Lane et al., 2022; Lucas et al., 2022). While these studies do not establish a causal link, they suggest a strong correlation between exposure to violence and later violent behavior.

While some instances of sexual violence in Colombia were likely driven by strategic military objectives, much of it appears to have been opportunistic, enabled by weak supervision, peer influence, and exposure to violence. Reports of sexual violence near military bases, lack of enforcement, and exploitation of vulnerable women strongly suggest that many cases were not premeditated acts of war, but rather crimes of opportunity. Ultimately, the distinction between strategic and non-strategic sexual violence is critical for understanding military accountability and its policy implications. Recognizing that weak enforcement and peer influence contributed to opportunistic crimes highlights the need for institutional reforms. Strengthening military oversight, enforcing discipline at various levels of the military hierarchy, and addressing the psychological impact of combat exposure may be essential steps in preventing future violence.

7 Conclusion

This study provides compelling evidence that the presence of military bases led to a substantial and sustained increase in sexual violence in Colombia. We find that registered cases of sexual violence per 100,000 inhabitants rose by approximately 72% relative to the control mean over a 16-year period following base introduction. This increase was driven primarily by municipalities hosting standing units with drafted soldiers, who were younger, had lower levels of training, and received lower pay compared to professional soldiers. In contrast, counterinsurgency units composed of professional soldiers did not exhibit statistically significant effects on sexual violence rates. These findings suggest that the composition and discipline of military units play a crucial role in shaping patterns of violence against civilians.

Despite the significant increase in sexual violence, we find no evidence that this rise translated into changes in fertility rates or child support disputes. This suggests that survivors may have taken measures to prevent pregnancies, been unable or unwilling to pursue child support claims, or that pregnancies resulting from sexual violence were not substantial enough to influence aggregate fertility patterns. Moreover, our analysis identifies potential spillover effects, indicating that municipalities neighboring military bases may also experience increased sexual violence and reduced child support disputes, though these findings are not consistently significant across different buffer sizes.

To better understand the drivers of this violence, we examined alternative explanations beyond the direct presence of military personnel. Our findings indicate that the increase in sexual violence was not driven by changes in security conditions, demographic shifts, or reporting behavior. If heightened insecurity were responsible, we would expect to see corresponding increases in homicides, kidnappings, and forced displacement, yet we find no such effects. Similarly, while sexual violence is notoriously underreported, we find no evidence that military base presence increased reporting rates for other crimes. This suggests that the observed increase reflects a real rise in incidents rather than a shift in reporting behavior.

The question of who committed these crimes remains difficult to answer with the available data. Given the historically documented collaboration between some groups in the army and right-wing paramilitary groups, it is possible that some of the increase in sexual violence was committed by paramilitary fighters operating in coordination with state forces. However, considering that the Colombian Army was vastly larger than any paramilitary group, and that military bases themselves were the focal points of the observed effects, we believe that government soldiers were the primary drivers of the increase in sexual violence documented in this paper.

While we lack data to empirically investigate why soldiers commit sexual violence, we

provide a discussion grounded in economic theory, empirical findings, and historical accounts. We explore two broad categories of explanations: strategic and non-strategic sexual violence. Strategic explanations suggest that sexual violence was used to coerce civilians, extract information, and deter defection, consistent with the economic model of extortion and civilian control. Historical accounts support this interpretation, documenting cases where government forces used sexual violence as a tool for intelligence gathering and counterinsurgency operations. Conversely, we find little anecdotal evidence that sexual violence was used as a mechanism for group cohesion or ideological reinforcement, explanations that are more commonly associated with insurgent groups.

Non-strategic explanations point to institutional weaknesses and social dynamics that enabled sexual violence. Weak enforcement, lack of accountability, and peer influences likely played a significant role in facilitating opportunistic crimes. The rational cheater model explains how weak oversight and low risk of punishment increase misconduct. Victim testimonies reinforce this view, documenting sexual violence occurring near or within military bases, implying a failure of command responsibility. Moreover, economic models of peer conformity suggest that soldiers stationed in close-knit units, sharing meals and sleeping spaces, may have been influenced by prevailing norms of impunity and aggression.

Future research would benefit from more detailed data on soldier characteristics and institutional functions within the Colombian Army. Such data would allow researchers to empirically test the hypotheses and speculations presented in section 6. Further investigation could help policymakers identify effective interventions to prevent sexual violence committed by government soldiers in conflict settings.

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8 Figures

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

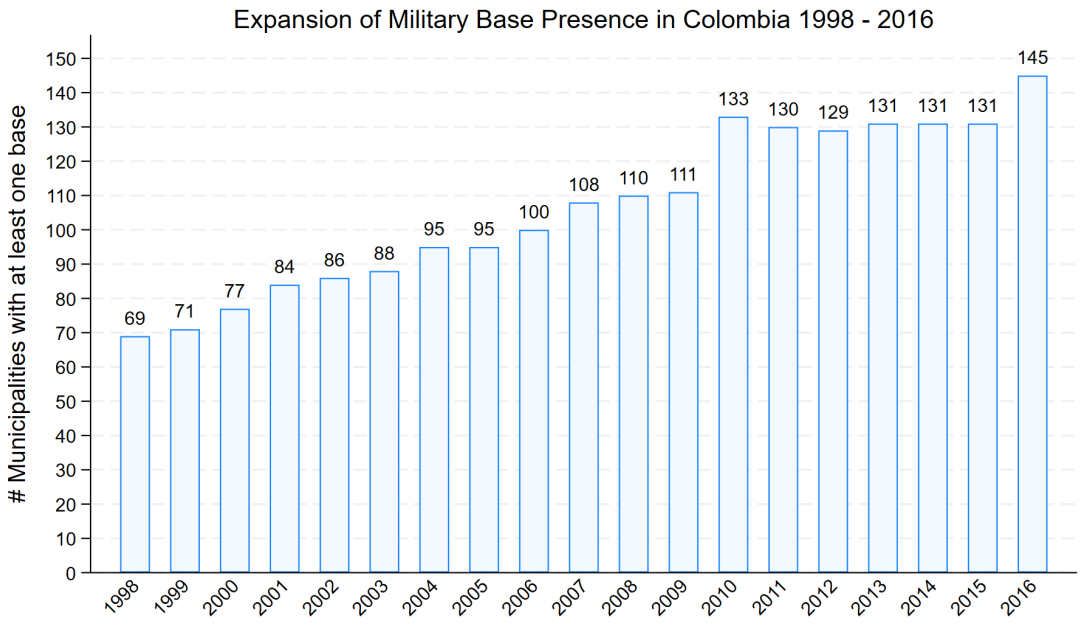
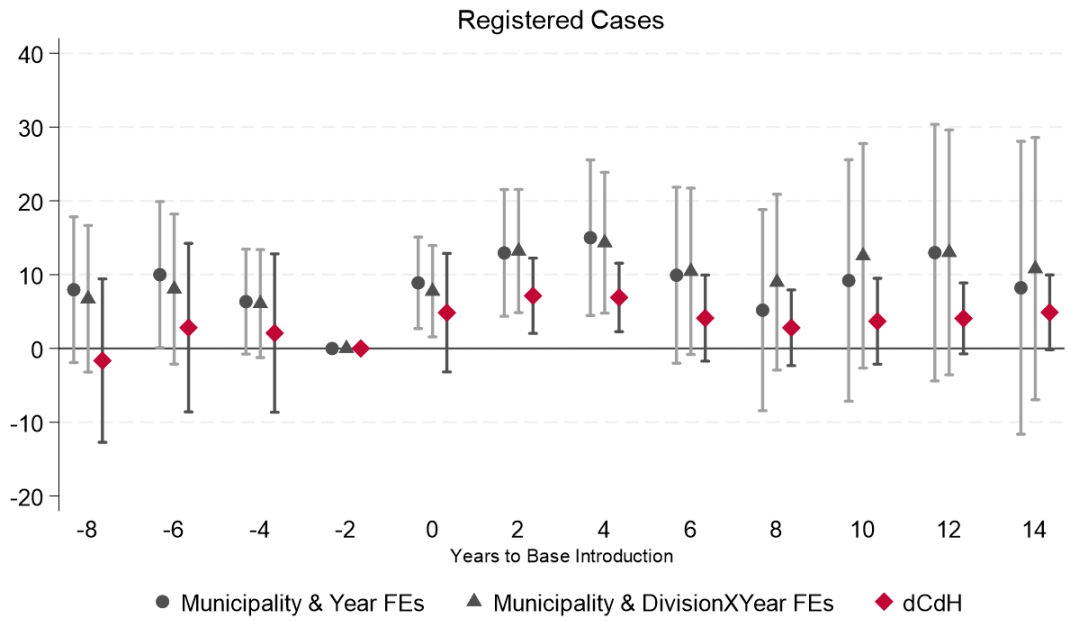
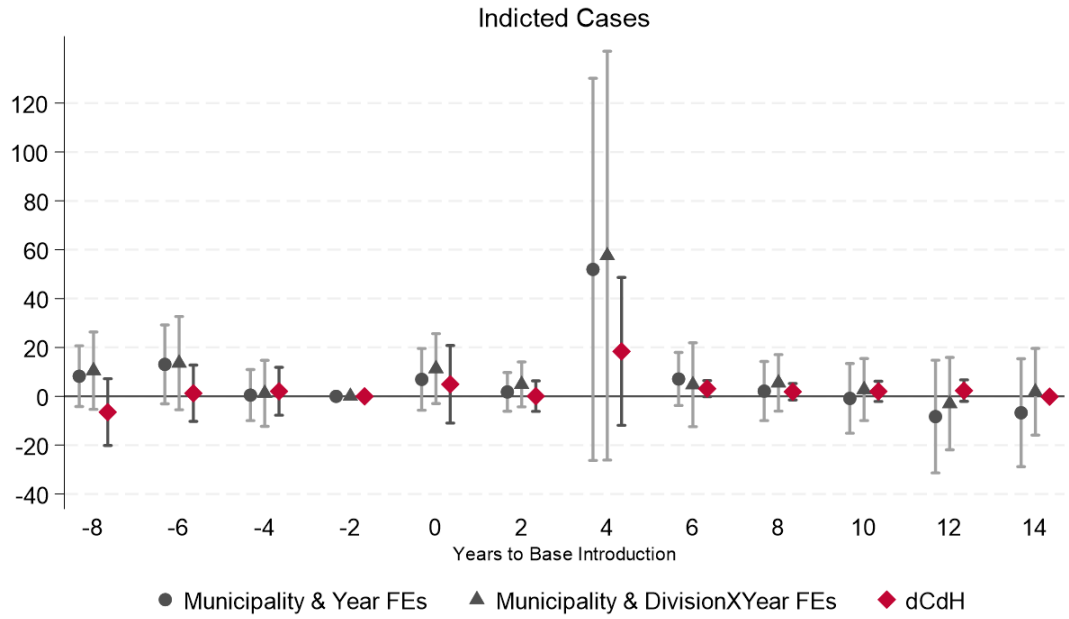


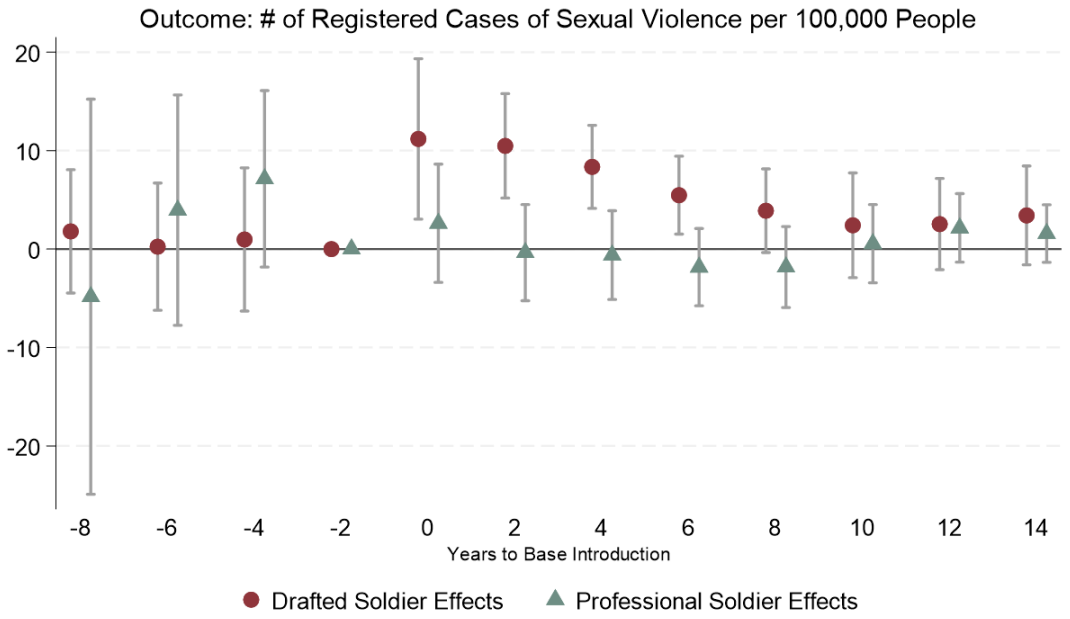
Figure 2: Effects on Sexual Violence
Outcome: Number of Cases per 100,000 Inhabitants

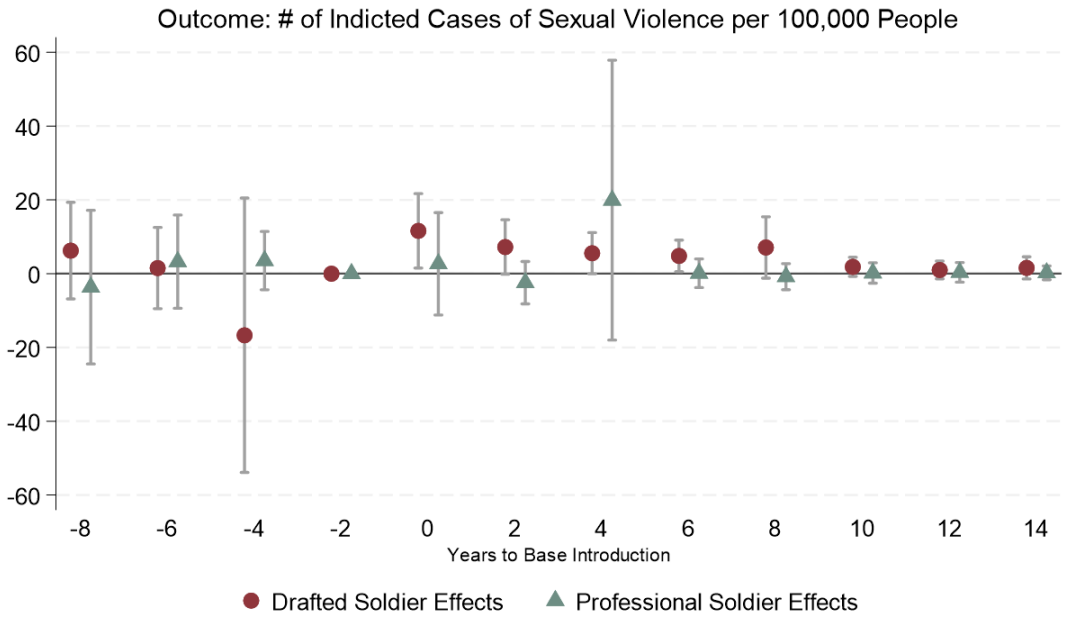




Note: These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction. The *Municipality & Year FEs* and *Municipality & DivisionXYear FEs* estimates are calculated with OLS, while the *dCdH* estimates are calculated with the de Chaisemartin and D'Haultfœuille estimator (de Chaisemartin and D'Haultfœuille, 2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

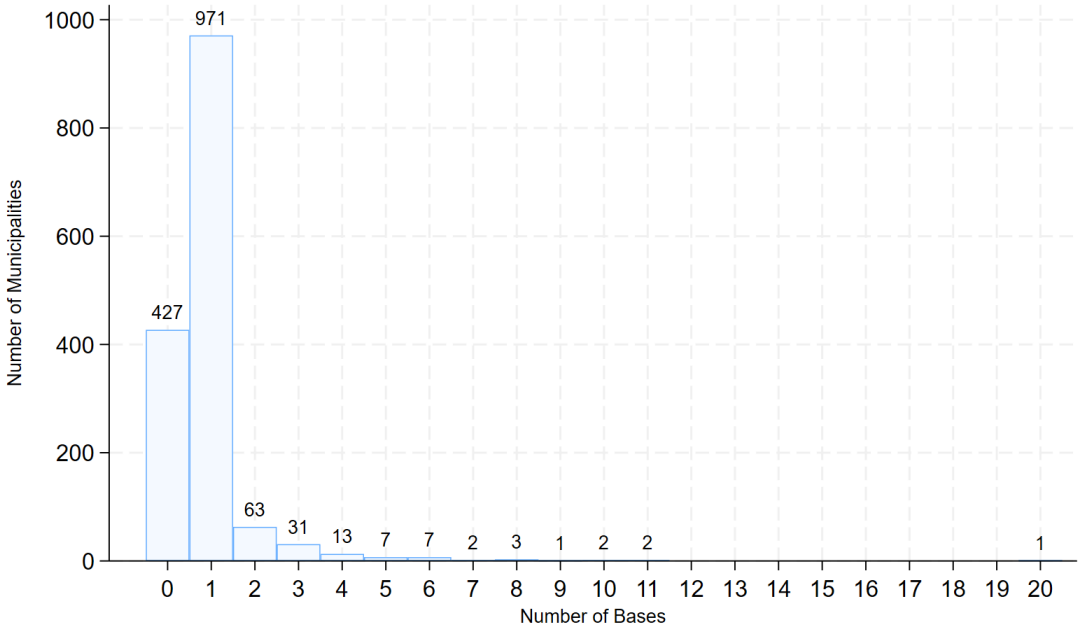
Figure 3: Effects on Sexual Violence by Base Type





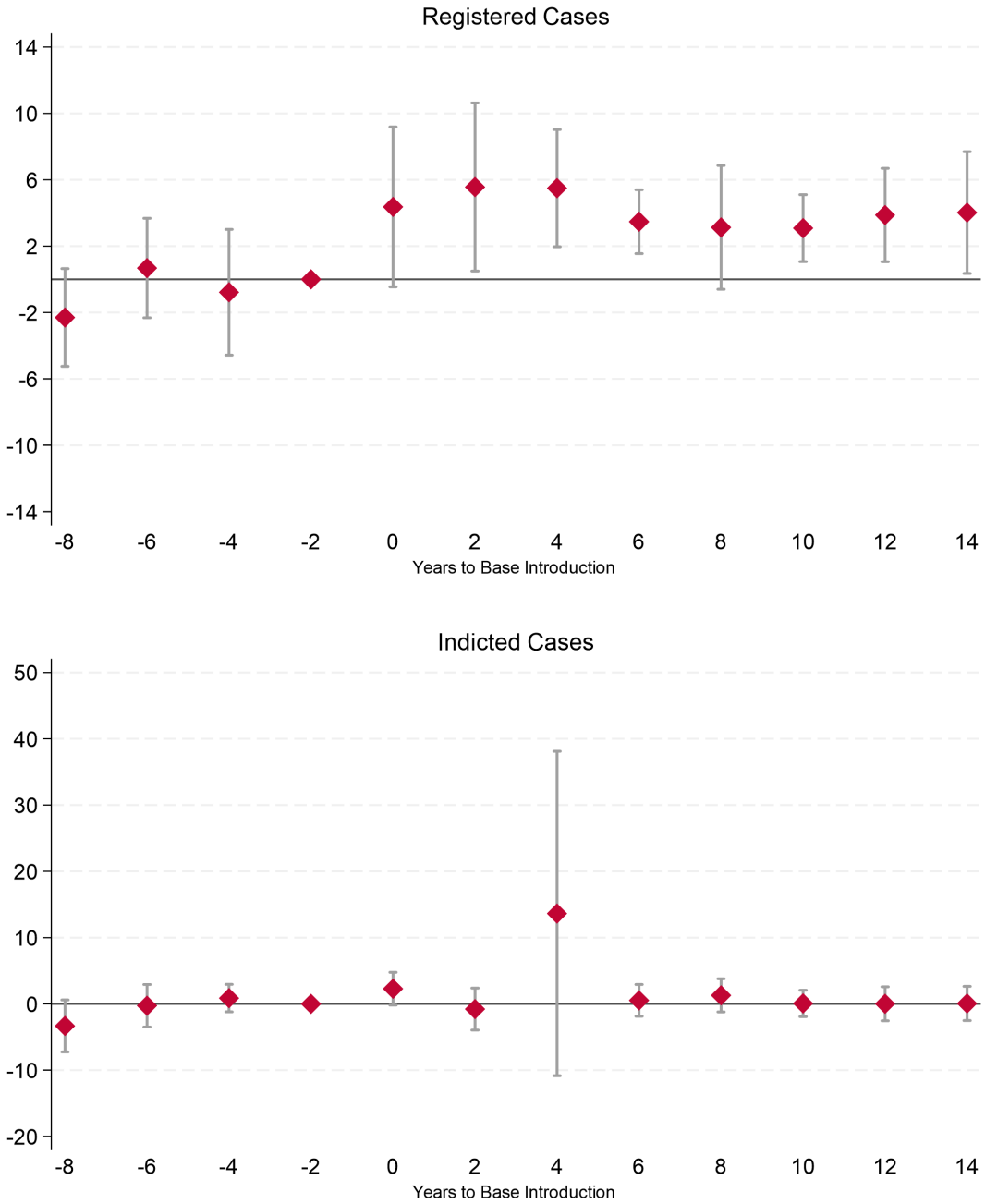
Note: These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). *Drafted soldier effects* refer to the estimated coefficients on an indicator variable 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 95% 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: Distribution of Observations across the Number of Bases



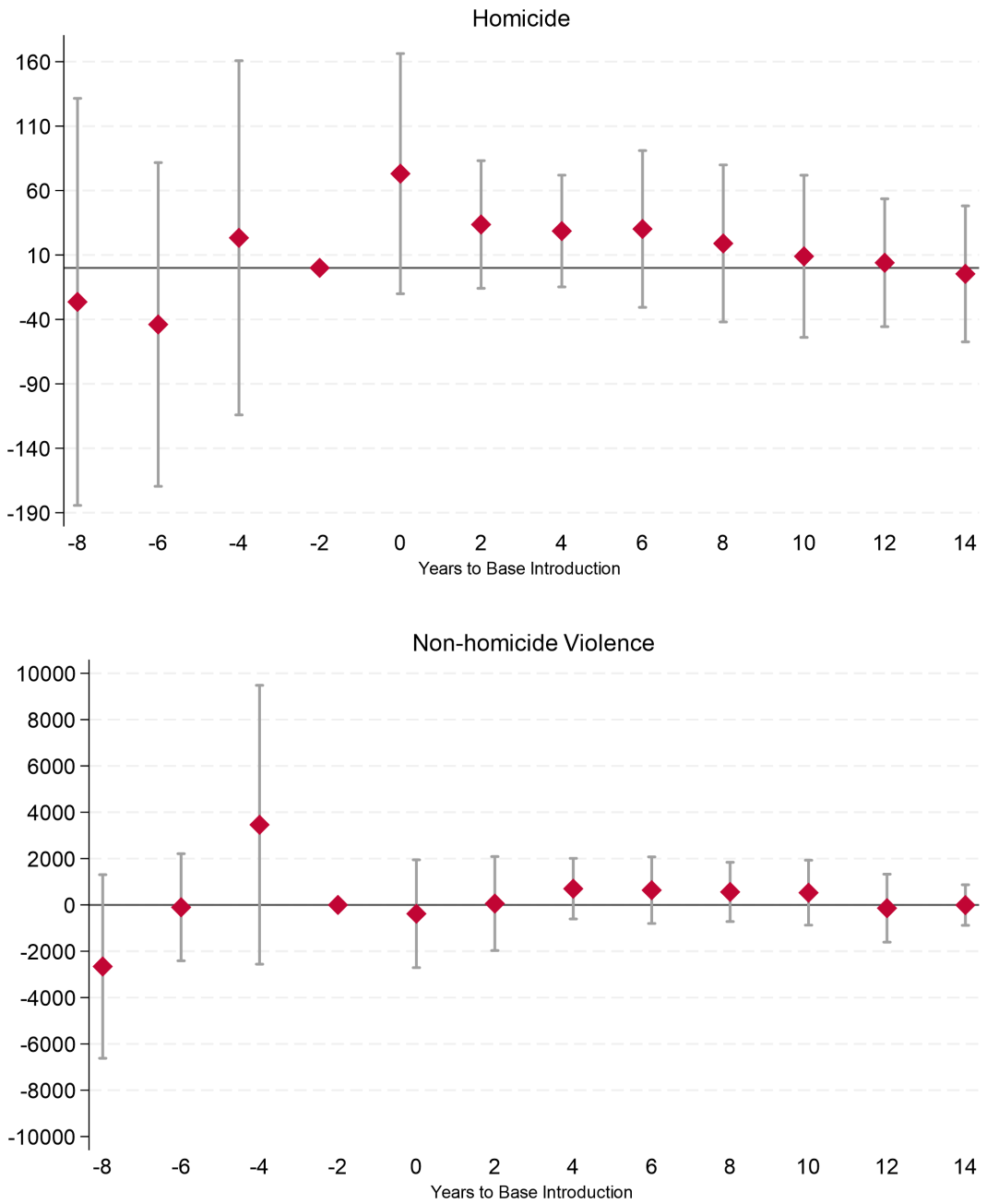
Note: This graph plots the distribution of municipalities across the number of military bases. There are 1,530 municipality-year group observations.

Figure 5: Intensive-Margin Effects on Sexual Violence
Outcome: Number of Cases per 100,000 Inhabitants



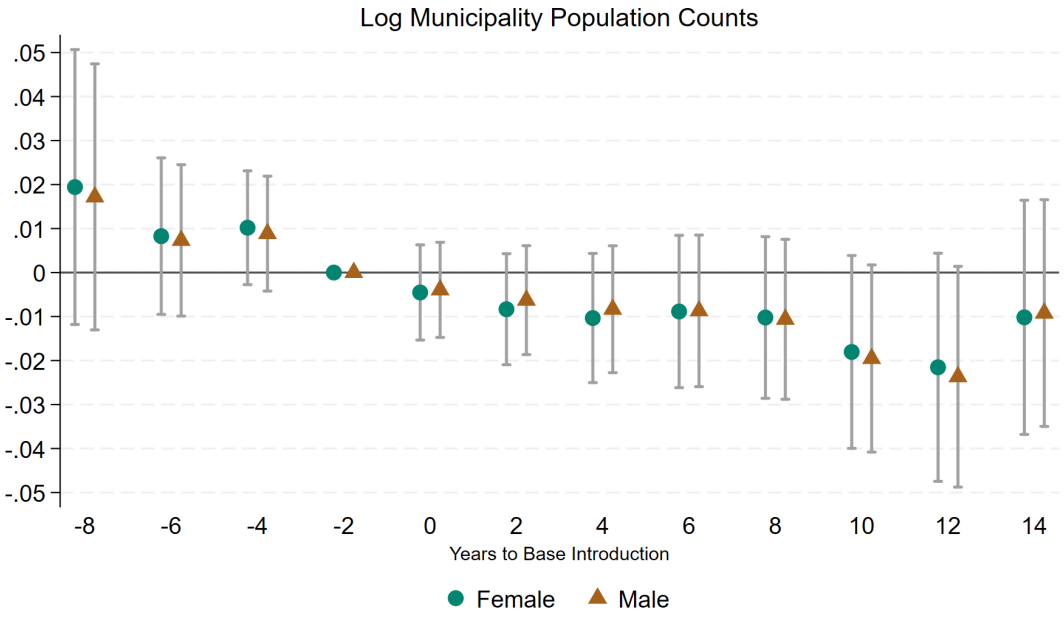
Note: These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 6: Effects on Homicides and Non-homicide Crimes
Outcome: Number of Cases per 100,000 Inhabitants



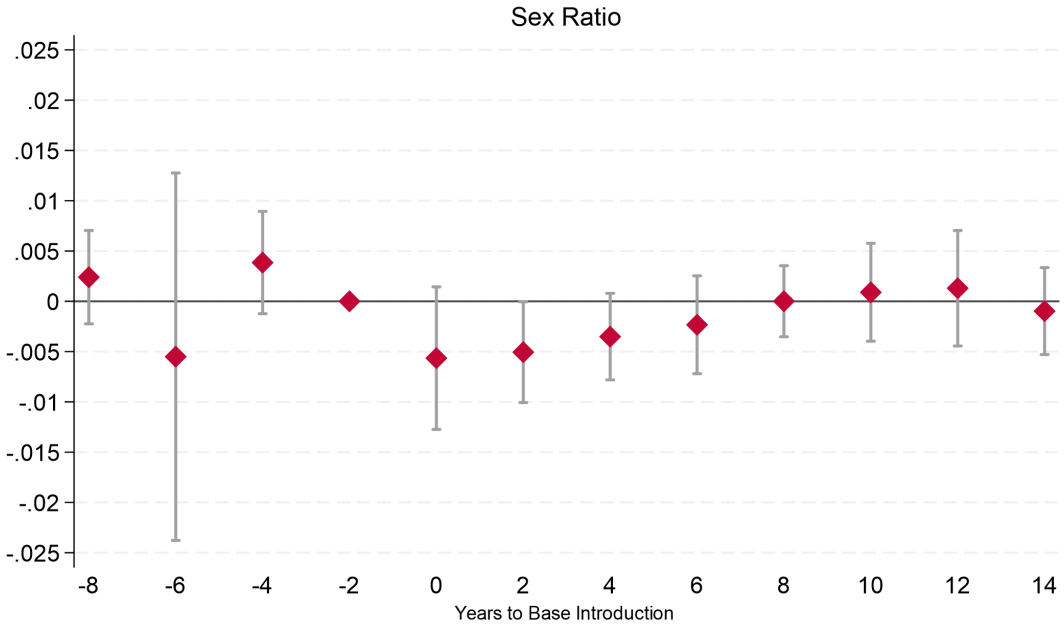
Note: These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 7: Effects on Female and Male Populations



Note: This graph plots the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

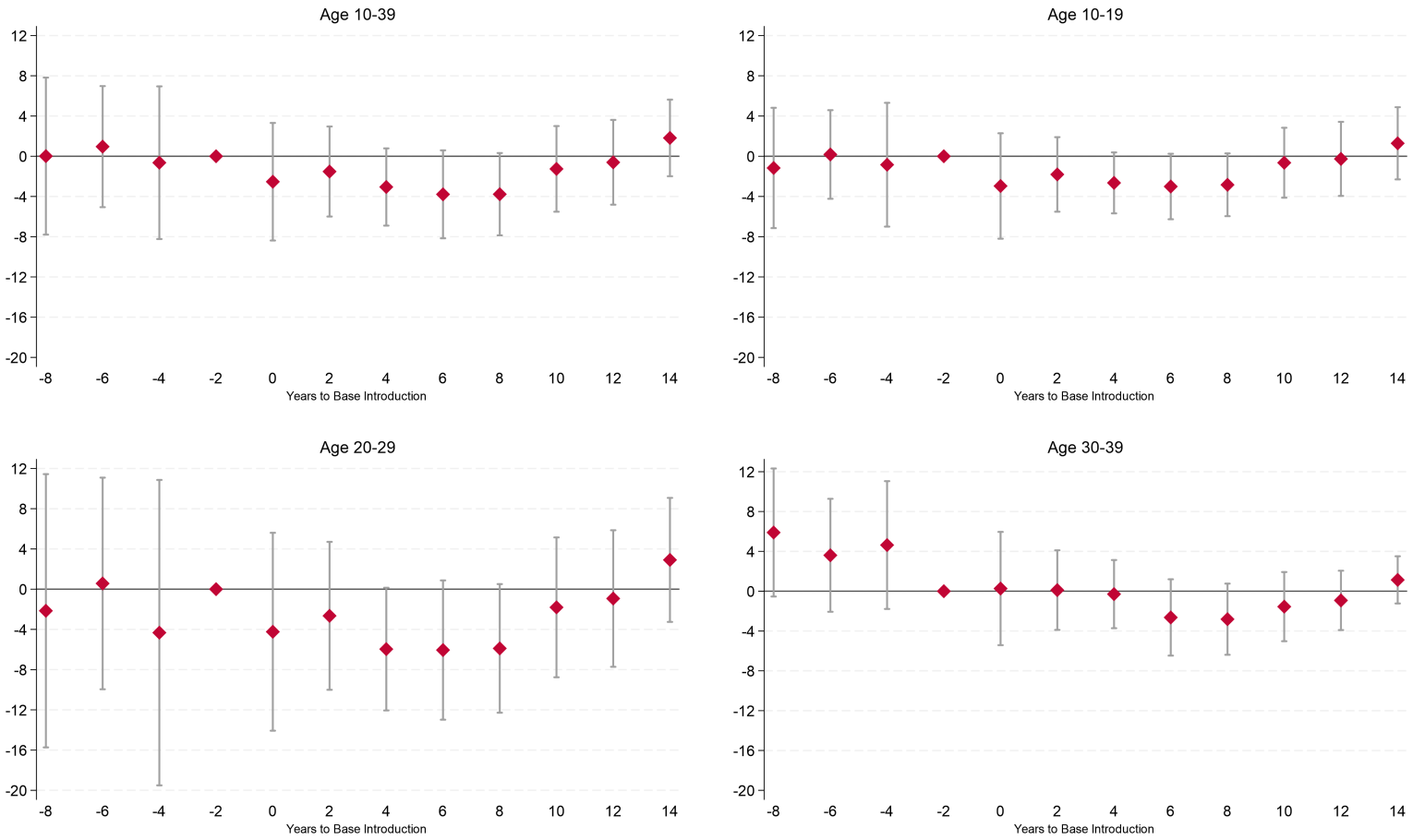
Figure 8: Effects on Female-to-Male Sex Ratio



Note: This graph plots the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

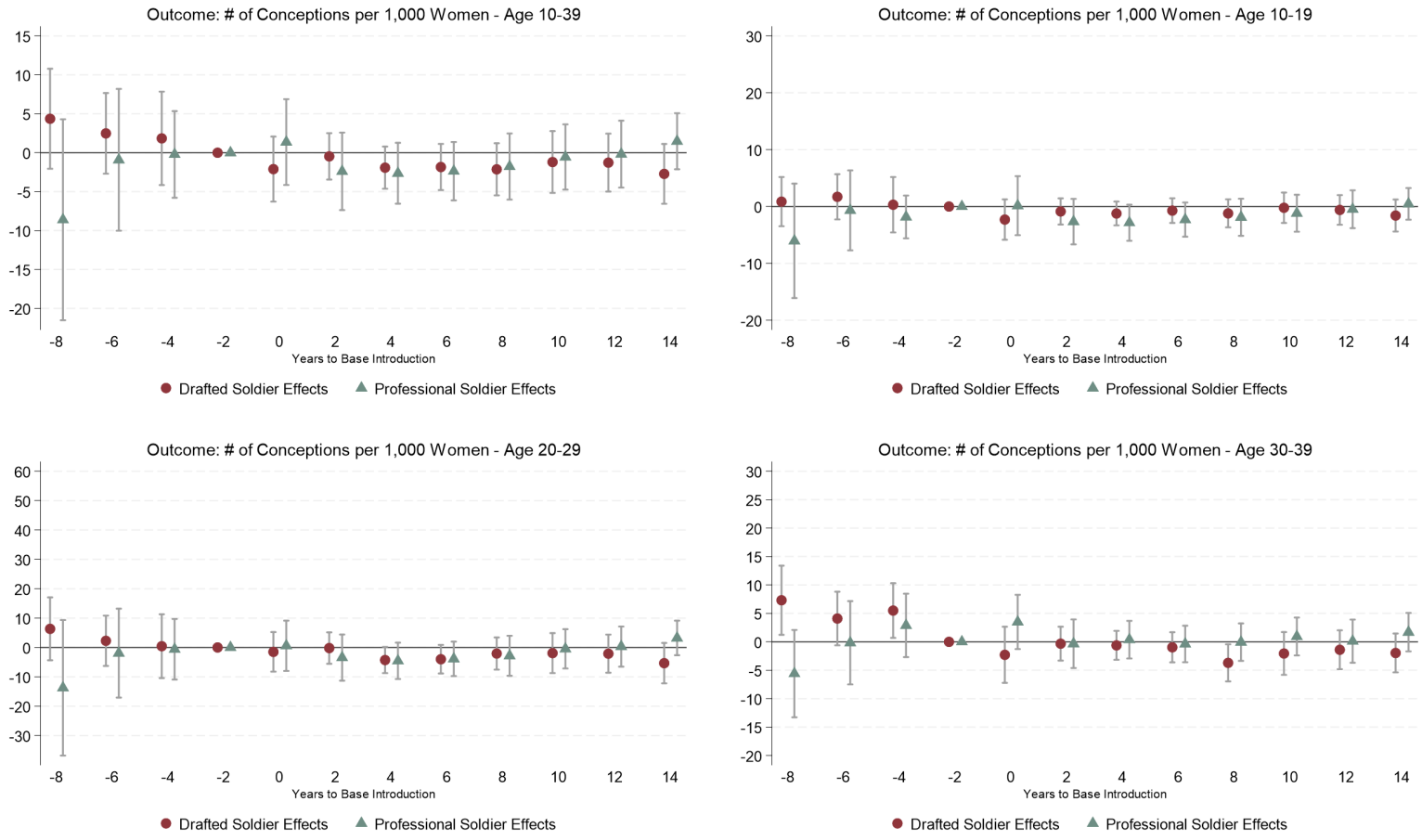
Figure 9: Effects on Fertility

Outcome: Number of Conceptions per 1,000 Women by Mothers' Age Groups



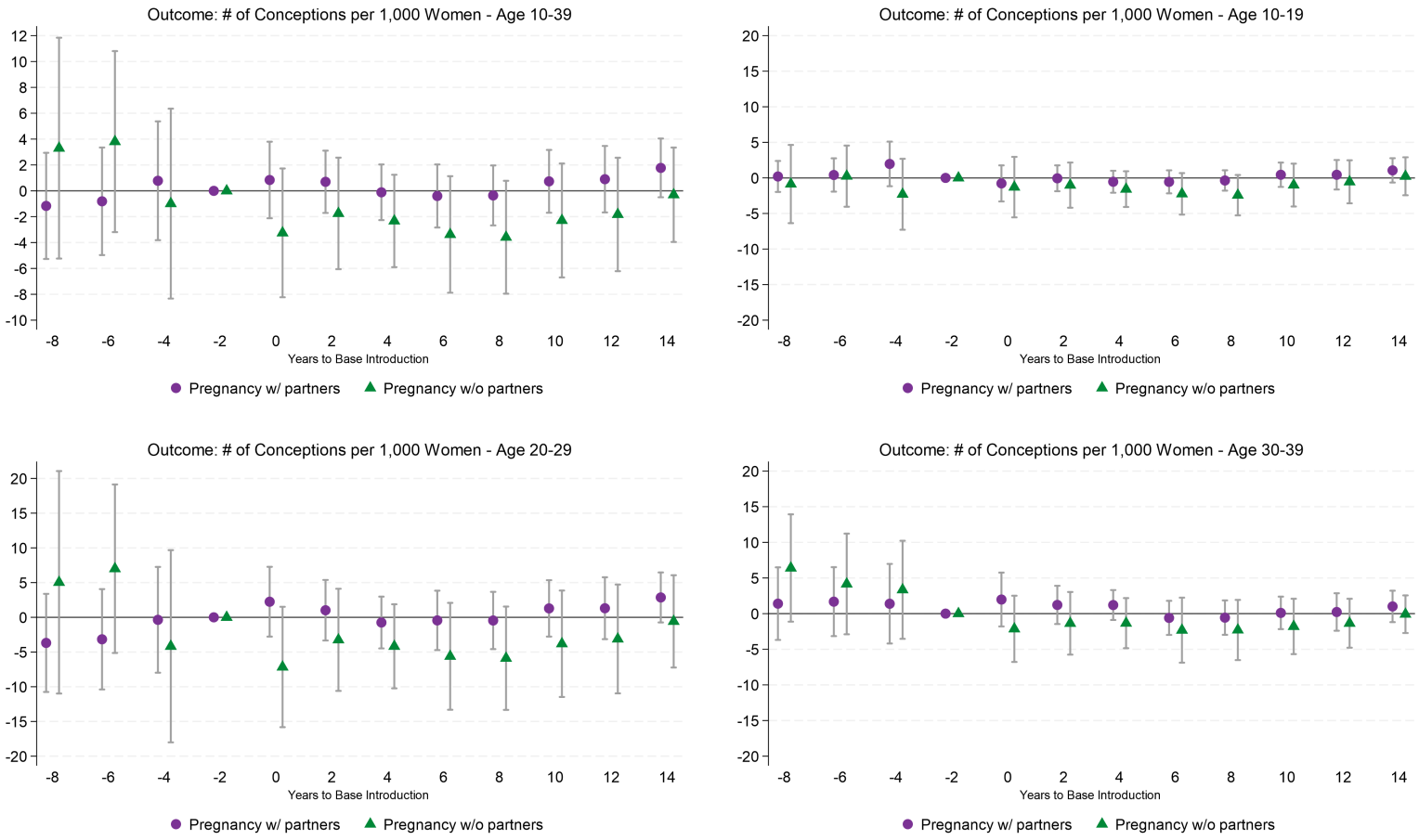
These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 10: Effects on Fertility by Base Type



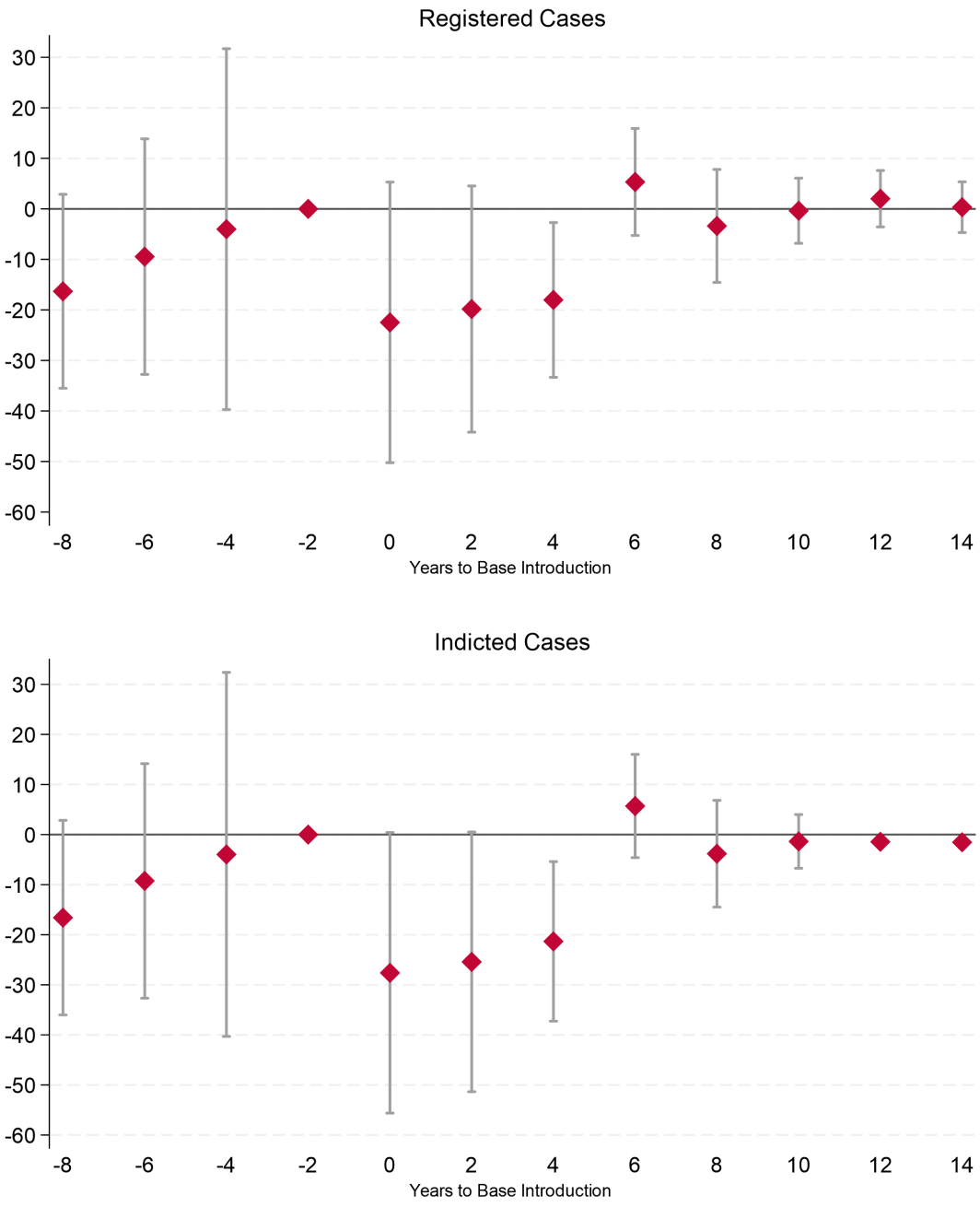
These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 11: Effects on Single-mother Fertility by Mothers' Age Groups



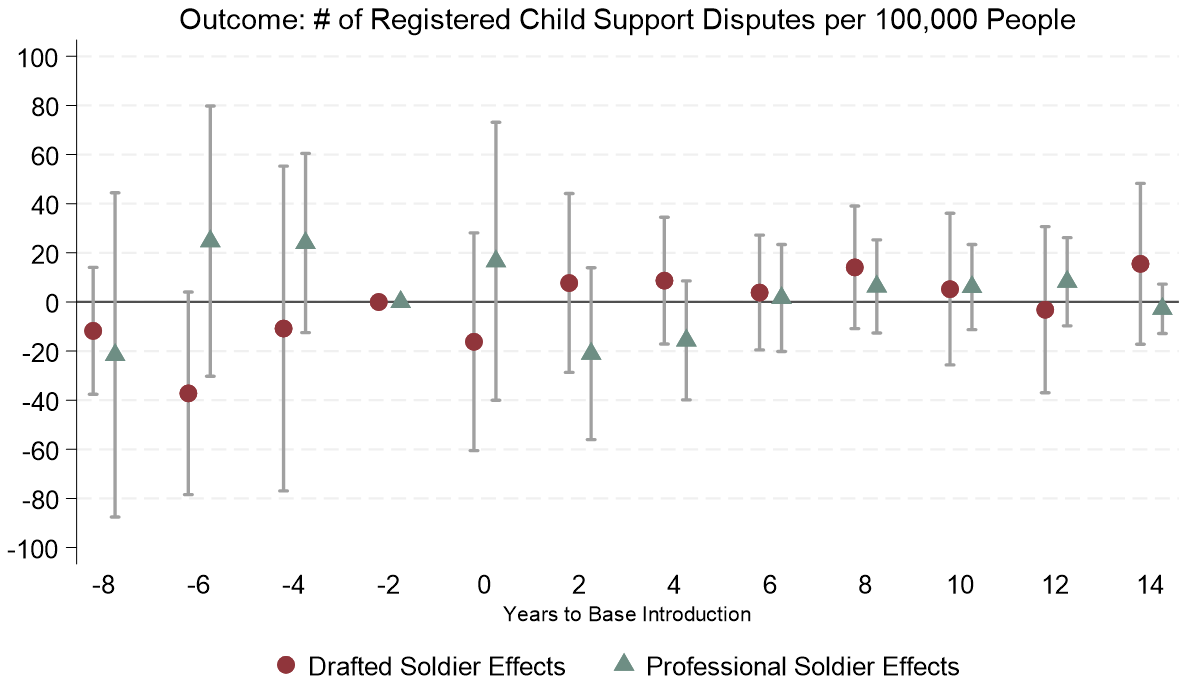
These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 12: Effects on Child Support Disputes
Outcome: Number of Cases per 100,000 Inhabitants



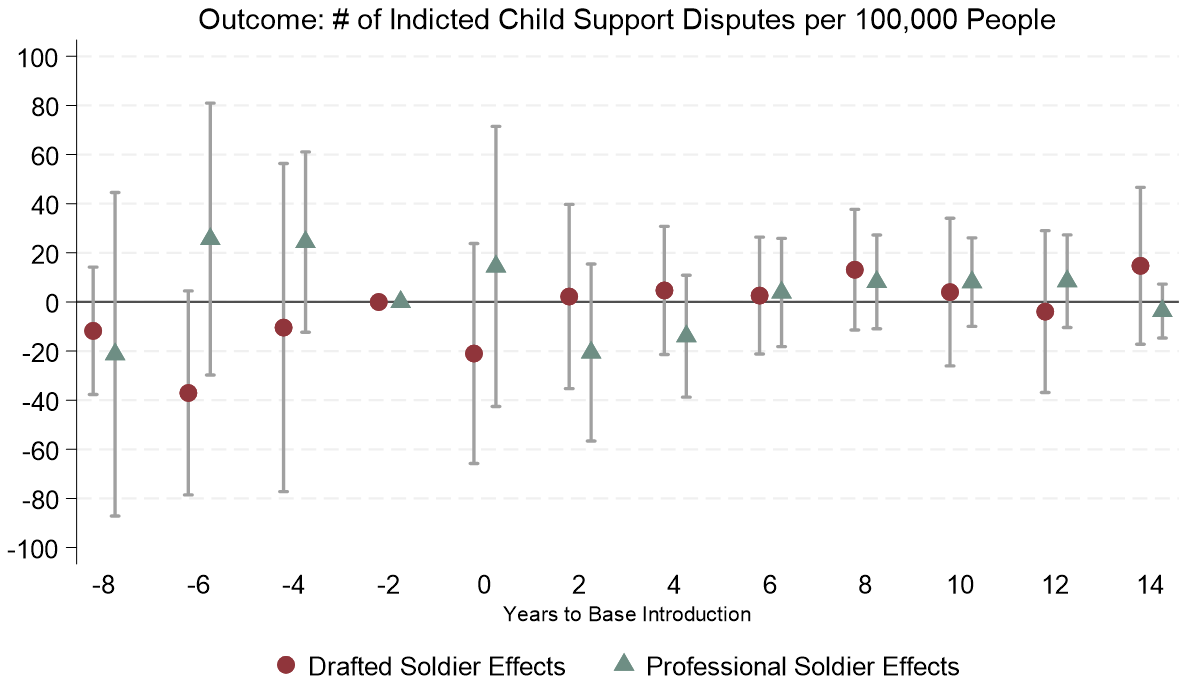
This graph plots the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 13: Effects on Child Support Disputes by Base Type (Registered Cases)



This graph plots the estimated coefficients for each two-year period relative to the period of military base introduction, calculated 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 95% 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 14: Effects on Child Support Disputes by Base Type (Indicted Cases)



This graph plots the estimated coefficients for each two-year period relative to the period of military base introduction, calculated 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 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

9 Tables

Table 1: Pre-Expansion Municipality Characteristics

	Year	Control Mean	Difference	p-value
Area (km^2)	-	602.21	2,743.20	0.00
Altitude above the sea level (meters)	-	1,215.04	-427.71	0.00
Real GDP per capita (million peso constant 2010)	2000	10.75	0.26	0.83
Total population	1998	17,964.98	40,570.43	0.00
Female	1998	8,961.65	20,936.97	0.00
Male	1998	9,003.33	19,633.46	0.00
Sex ratio (Female:Male)	1998	0.95	0.01	0.40
Cases of violence per 100,000 inhabitants				
Homicide	1998	161.51	118.26	0.00
Intimidation	1998	52.92	38.36	0.17
Terrorism	1998	14.87	43.97	0.33
Kidnapping	1998	40.14	81.60	0.00
Forced displacement	1998	1,307.38	483.52	0.19

Note: There are 1,104 municipalities in the analysis. Each year indicates the earliest year in which the data for each variable is available. Altitude and area sizes are constant across years. Difference is the estimated coefficient of the indicator that a municipality has ever had at least one military base in the anlaysis period in the regression of each characteristic.

Table 2: Share of Treated Observations by Base Types

Base Types	Counts	Share (%)
Standard bases	578	62.42
Counterinsurgency bases	306	33.05
Both	42	4.54
Total	926	100.00

Note: The total number of treated and not-yet treated observations is 1,530.

Table 3: Average Total Effects on Sexual Violence (dCdH)

Outcome: Number of Cases per 100,000 Inhabitants

	Registered	Indicted
	(1)	(2)
Has Army base	16.399**	16.254
	(7.174)	(10.335)
Obs.	1,224	1,224
Control mean	22.44	14.31

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 calcuates 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.

Table 4: Average Total Effects on Sexual Violence (Intensive Margin)
Outcome: Number of Cases per 100,000 Inhabitants

	Registered	Indicted
	(1)	(2)
N. Army base	12.230***	6.779
	(4.041)	(6.292)
Obs.	1,359	1,359
Control mean	22.44	14.31

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 calcuates 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.

Table 5: Average Total Effects on Fertility (dCdH)
Outcome: Number of Conceptions per 1,000 Women

	Mothers' Age Groups			
	10-39	10-19	20-29	30-39
	(1)	(2)	(3)	(4)
Has Army base	-7.359	-6.126	-12.327	-3.802
	(6.268)	(4.976)	(10.022)	(5.206)
Obs.	1,224	1,224	1,224	1,224
Control mean	128.06	88.28	204.50	103.52

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 calcuates 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.

Table 6: Average Total Effects on Child Support Disputes (dCdH)

Outcome: Number of Cases per 100,000 Inhabitants

	Registered	Indicted
	(1)	(2)
Has Army base	-21.901	-29.530*
	(17.587)	(17.191)
Obs.	1,224	1,224
Control mean	66.47	59.22

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 calcuates 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.

Table 7: Spillover Effects on Sexual Violence (dCdH)
Outcome: Number of Cases per 100,000 Inhabitants

	Registered	Indicted
	(1)	(2)
Panel A: 25 km Buffer		
Has treated neighbor	12.16*	15.90
	(6.66)	(11.38)
Obs.	1,224	1,224
Panel B: 50 km Buffer		
Has treated neighbor	19.74	8.44
	(13.10)	(5.37)
Obs.	996	996
Panel C: 75 km Buffer		
Has treated neighbor	25.34*	8.70
	(14.30)	(6.76)
Obs.	966	966
Control mean	22.44	14.31

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 calcuates 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.

Table 8: Spillover Effects on Fertility (dCdH)
Outcome: Number of Conceptions per 1,000 Women

	Mothers' Age Groups			
	10-39	10-19	20-29	30-39
	(1)	(2)	(3)	(4)
Panel A: 25 km Buffer				
Has treated neighbor	-9.75	-7.68	-15.19	-7.51
	(6.94)	(5.46)	(11.54)	(5.52)
Obs.	1,224	1,224	1,224	1,224
Panel B: 50 km Buffer				
Has treated neighbor	-0.56	-0.19	1.98	-6.08
	(7.48)	(5.92)	(13.56)	(8.88)
Obs.	996	996	996	996
Panel C: 75 km Buffer				
Has treated neighbor	7.35	5.80	16.02	-0.56
	(9.97)	(6.54)	(18.59)	(11.88)
Obs.	966	966	966	966
Control mean	128.06	88.28	204.50	103.52

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 calcuates 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.

Table 9: Spillover Effects on Child Support Dispute (dCdH)
Outcome: Number of Cases per 100,000 Inhabitants

	Registered	Indicted
	(1)	(2)
Panel A: 25 km Buffer		
Has treated neighbor	-18.51	-27.87**
	(13.41)	(13.27)
Obs.	1,224	1,224
Panel B: 50 km Buffer		
Has treated neighbor	13.69	3.44
	(14.37)	(10.51)
Obs.	996	996
Panel C: 75 km Buffer		
Has treated neighbor	10.70	2.20
	(17.24)	(14.16)
Obs.	966	966
Control mean	66.47	59.22

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 calcuates 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.

A Appendix

A.1 Figures

Figure A1: Geographical Distribution of Military Bases 1999 - 2016

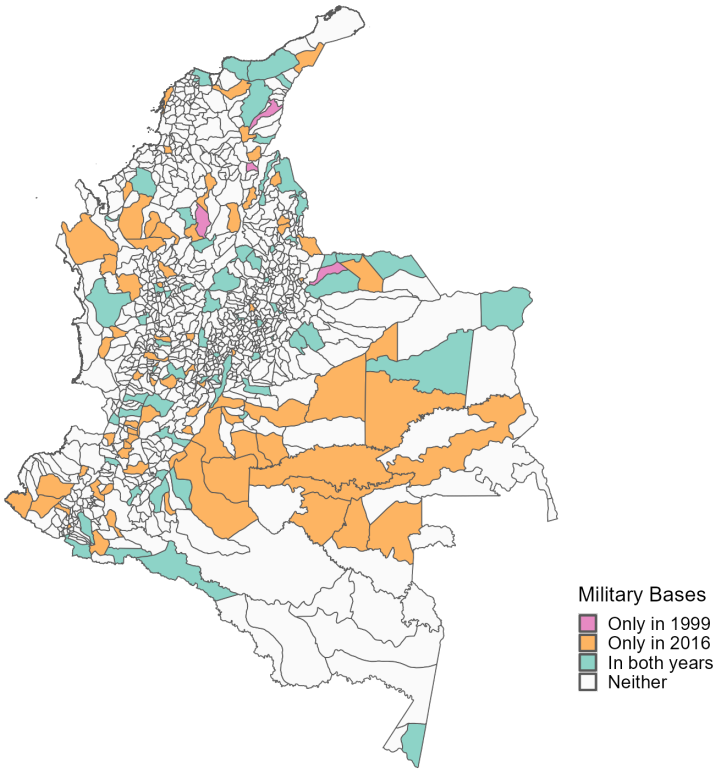
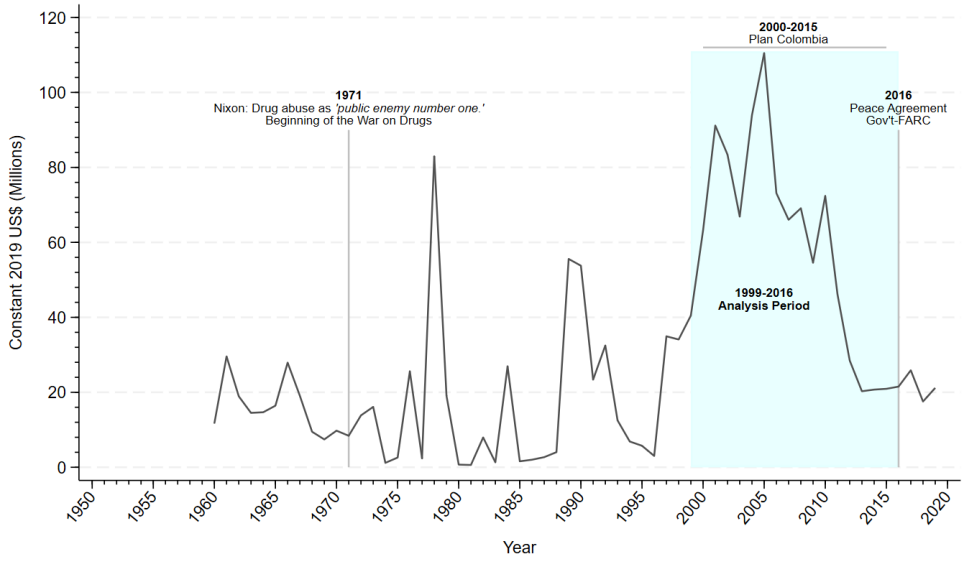
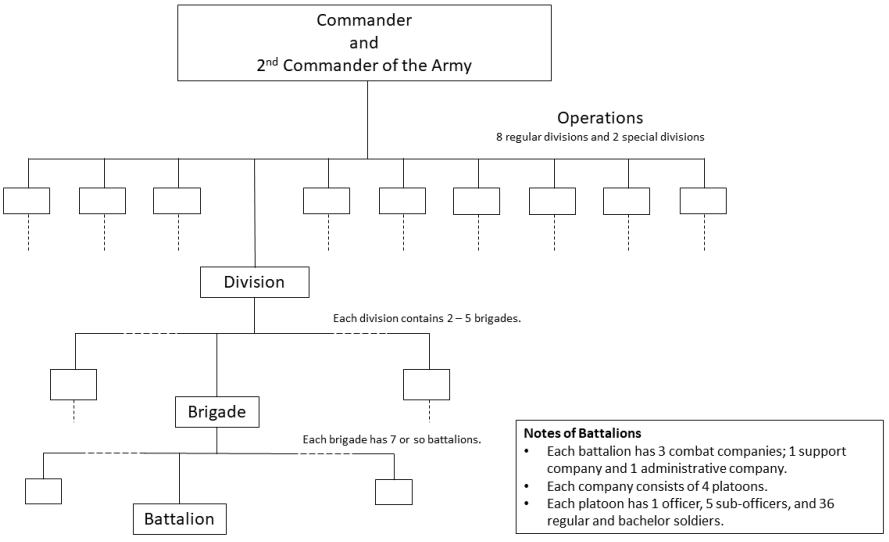


Figure A2: U.S. Military Assistance to Colombia



Source: U.S. Overseas Loans and Grants (Greenbook), USAID

Figure A3: The Organization of the Colombian National Army



Note: The presented organization chart reflects the organization during the analysis period.

Figure A4: Military Base Presence and Duration

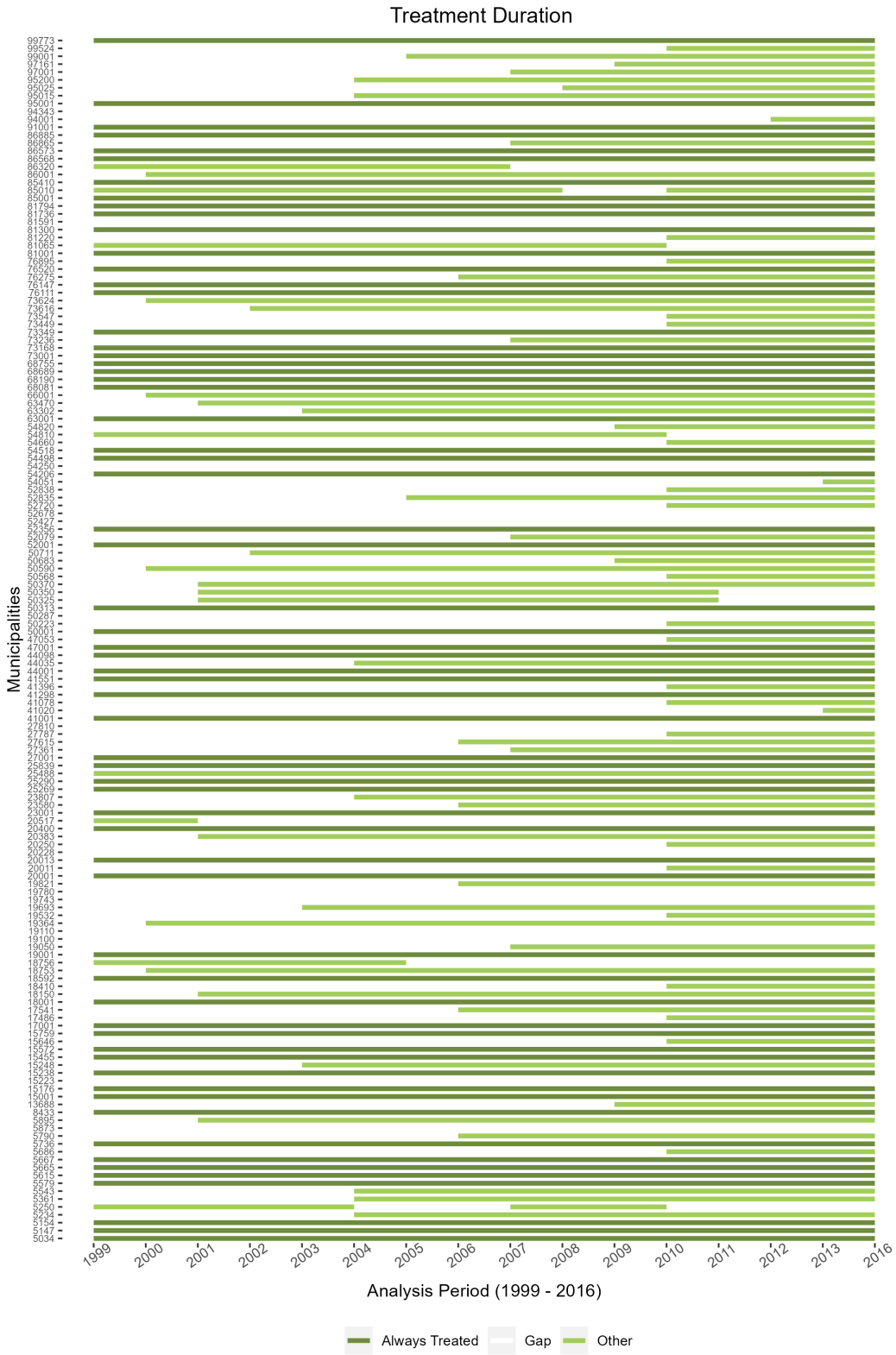
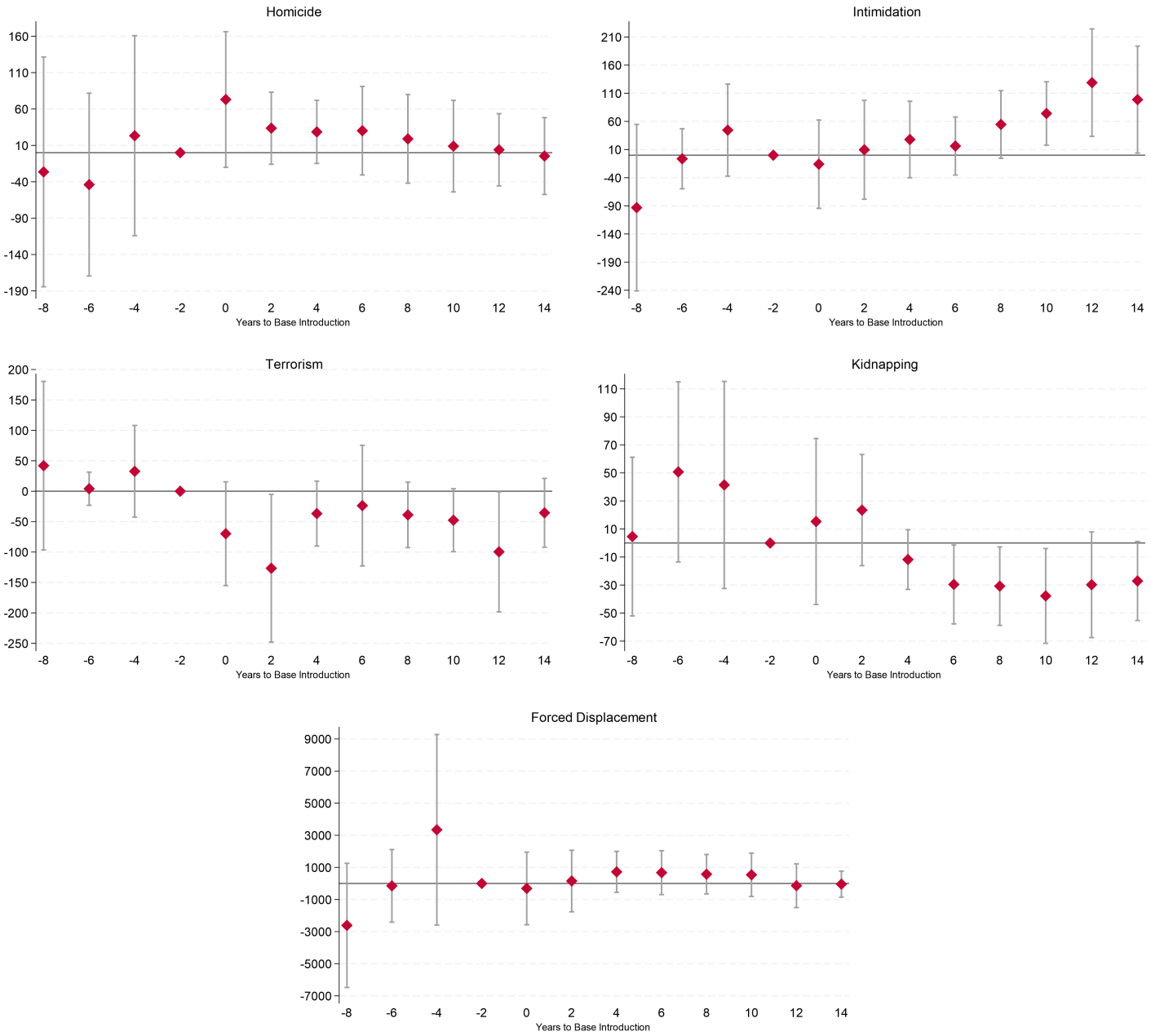


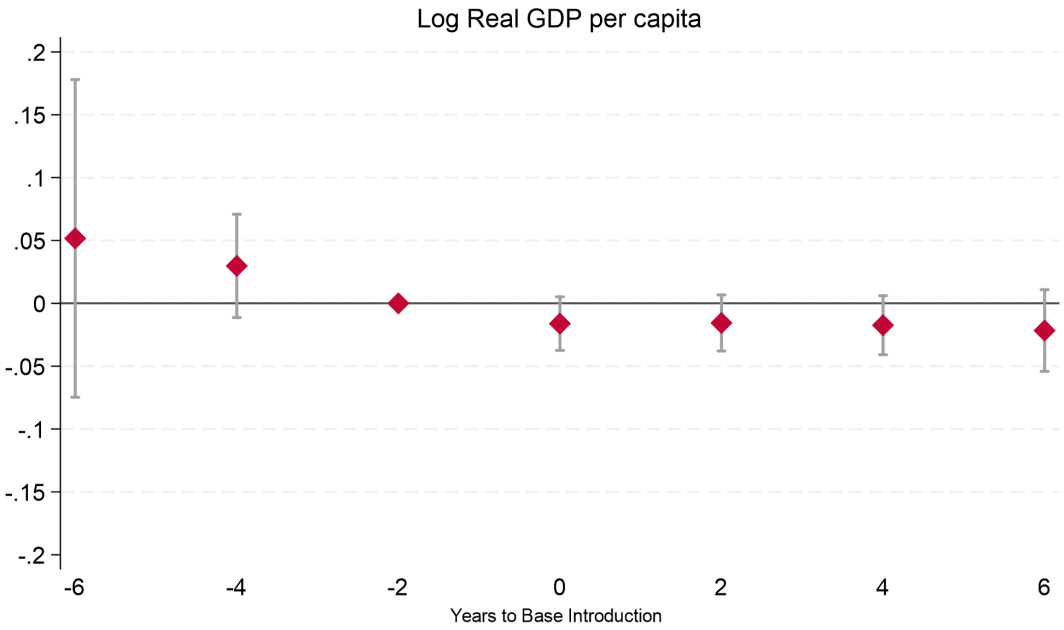
Figure A5: Effects on Crime Rates by Types

Outcome: Cases per 100,000 Inhabitants



The lines are the 95% 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 A6: Effects on Municipal Economies



Note: This graph plots the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 95% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities. The data on municipal GDP per capita is only available from 2000 to 2009, and is made available by CEDE (2021).

A.2 Tables

A.2.1 Colombian Army Soldiers

Table A1: Monthly Compensation by Soldier Categories

	Basic Soldiers	Professional Soldiers	% Difference
2000	US$9.20	US$83.48	807.06
2001	US$10.11	US$91.79	807.52
2002	US$10.93	US$99.17	807.53
2003	US$11.74	US$106.56	807.55
2004	US$12.66	US$114.90	807.54
2005	US$13.49	US$122.44	807.58
2006	US$14.43	US$130.95	807.57
2007	US$15.34	US$139.20	807.57
2008	US$16.32	US$148.12	807.56
2009	US$17.57	US$159.48	807.56
2010	US$17.92	US$165.29	822.16

Source: Authors' calculation based on Decrees 1794 and 2724 of 2000, 2737 of 2001, 745 of 2002, 3552 of 2003, 4158 of 2004, 923 of 2005, 407 of 2006, 1515 of 2007, 673 of 2008, 737 of 2009 , 1530 of 2010, and the yearly minimum wage from Datosmacro (2022). Values in Colombian pesos (COP) are converted to the U.S. dollar (USD) values using the 2023 average conversion rate of COP 4,362 to USD 1.

Note: The compensation for conscripted soldiers are called bonus (*bonificación* in Spanish), which is meant to as an allowance to supplement the supply of uniforms, and basic necessities including food and hygiene products. Conscripted soldiers can receive a 40% increase in their monthly bonus if their performance is exceptional. Meanwhile, the compensation for professional soldiers is a salary, and determined as 140% of the legal minimum wage. Volunteer soldiers, as professional soldiers were known before 2000, who have already served before December 31, 2000 receive the 160% of minimum wage.

Table A2: Benefits for Professional Soldiers

Benefit	Description
Seniority bonus	After two years of service, a professional soldier is entitled to a monthly seniority bonus equal to 6.5% of their basic salary. This bonus increases by 6.5% for each additional year of service, up to a maximum of 58.5%.
Annual service bonus	Soldiers are entitled to an annual service bonus equivalent to 50% of their basic monthly salary plus the seniority bonus. This is paid in the first 15 days of July each year.
Vacation bonus	Soldiers receive a vacation bonus equal to 50% of their basic monthly salary plus the seniority bonus for each year of service. This is calculated for vacations accrued from February 1 of the year following the decree's enactment.
Christmas bonus	A Christmas bonus equivalent to 50% of the basic salary earned in November, plus the seniority bonus, is paid in December each year.
Travel allowances	Soldiers are entitled to travel allowances for individual transfers within the country and for individual service commissions.
Vacation entitlement	Soldiers are entitled to 30 calendar days of paid vacation for each year of service.
Severance pay	Soldiers are entitled to severance pay equivalent to one basic salary plus the seniority bonus for each year of service, which is annually liquidated and deposited in a designated fund.
Housing benefits	Soldiers can participate in housing plans and programs offered by the Military Housing Promotion Fund and other entities.
Family subsidy	Married soldiers or those in a marital union are entitled to a monthly family subsidy equal to 4% of their basic monthly salary plus the seniority bonus.
Burial expenses	The Ministry of Defense covers the burial expenses of soldiers who die in active service or while receiving a pension, up to eight times the legal minimum monthly wage.

Source: Degree 1794 of 2000

A.2.2 Data Overview

Table A3: Overview of the Outcome Data

	Data Description	Link	Years Available	Years Used in This Paper
Fertility	Birth certificate data from the Vital Statistics	https://www.datos.gov.co/widgets/kk5w-ugzm	1979 - 2022	1998 - 2016
Demographics	Population projection based on the National Census of Population and Livelihood	https://www.dane.gov.co/index.php/estadisticas-por-tema/demografia-y-poblacion/proyecciones-de-poblacion	1995 - 2026	1998 - 2016
Sexual violence and child support	Lawsuit data by the Office of Attorney General	-	2000 - 2021	2000 - 2016
Violence and security	The Conflict and Violence module of the Municipality Panel compiled by the Center for Economic Development Studies	https://datoscede.uniandes.edu.co/es/catalogo-de-microdata	1993 - 2019	1998 - 2016
Education	Census of Educational Establishments by the Ministry of Education	https://microdatos.dane.gov.co/index.php/catalog/EDU-Microdatos	2004 - 2022	2004 - 2016

Table A4: Number of Unique Municipalities by Year

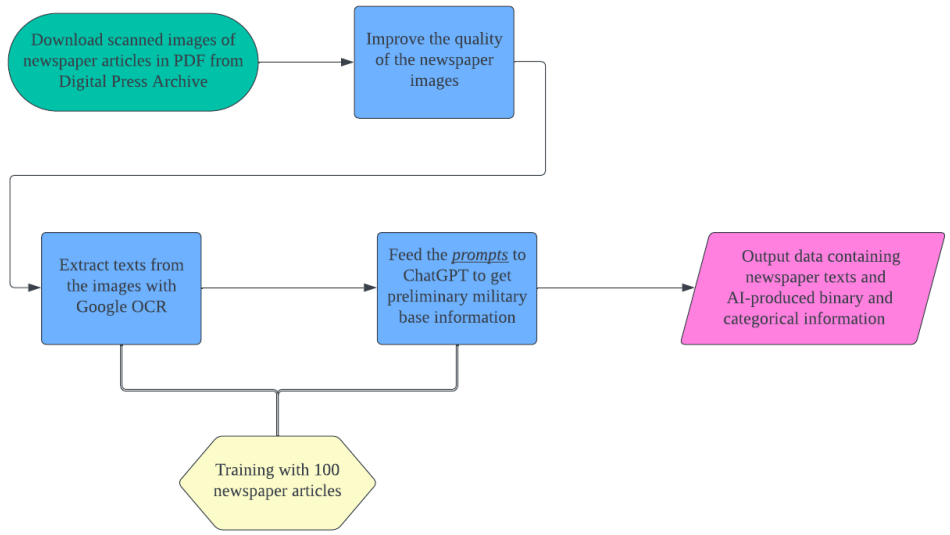
Year	N. Unique Municipalities
1998	1,089
1999	1,099
2000	1,104
2001	1,105
2002	1,107
2003	1,104
2004	1,106
2005	1,105
2006	1,104
2007	1,109
2008	1,107
2009	1,109
2010	1,108
2011	1,109
2012	1,110
2013	1,111
2014	1,110
2015	1,111
2016	1,111

Note: The analysis sample excludes the seven major cities which are Barranquilla, Bogotá, Bucaramanga, Medelliín, Cali, Cartagena, and Cucuta.

A.3 Construction of the Military Base Data

As the data on military base locations were not made available, we constructed them from newspapers published in Colombia from 2000 to 2010. Figure A7 provides an overview of this data cleaning process, and we describe it in detail in the following.

Figure A7: Process to Extract Text from Newspaper Images



A.3.1 Text Data Extraction from Newspaper Images

The first step is to collect newspapers published in Colombia from 2000 to 2010 related to military units. To do so, we downloaded relevant newspaper articles from the Digital Press Archive, a newspaper database provided by the Popular Research and Education Center/Program for Peace (Cinep/PPP). The database offers over 70,000 digitized publications from 10 national and regional newspapers since 1979, categorized into five groups; 1) church and conflict, 2) politics and government, 3) drug trafficking, 4) society and culture, and 5) ecology and environment. We use two keywords in Spanish, *brigada* (brigade) and *batallón* (battalion), to restrict our search, which has yielded about 11,000 articles published from January 1, 2000 to December 31, 2010. All the digitized materials are scanned images of newspaper articles in the PDF format with a range of image quality.

The second step is to improve the quality of the article images. We processed all the scanned newspaper articles to smooth, reduce noise, and binarize the images, and adjust contrast.[20]

[20]Image binarization is a process to convert a gray-scale image to a binary (black and white) image that can be used to identify the foreground of the image. This process helps extract texts from noise in the articles.

The third step is to read the improved article images with Google Cloud Vision, which can detect text data from images using Optical Character Recognition (OCR) and machine learning. Since texts in newspaper articles are organized in irregularly shaped chunks and font sizes (as opposed to, for example, texts in an academic paper in paragraphs), detecting texts in proper orders that form sentences can be challenging. Therefore, we used the manually extracted data from 100 articles to train a machine learning model more suitable to detect texts from newspaper articles. This step created an initial text dataset containing the texts from all newspaper articles.

The final step is to use ChatGPT 3.5 to get basic information about each newspaper article using the text data from the previous step. To optimize this process, we again used the training dataset from the same 100 articles to train ChatGPT to accurately obtain information of interest. More specifically, we wanted to ChatGPT to extract names, locations (municipalities and department), and activation and deactivation dates of military bases. The prompt we gave ChatGPT is found in Box A.3.2 below. We use the AI-extracted data to inform and speed up the later treatment variable creation, not necessarily to take the data to directly create the treatment variables without manual inspection of the content of the relevant newspaper articles.

A.3.2 Prompt for ChatGPT

Please note that this journalistic article from Colombia has been extracted using OCR software, which could result in spelling errors, incomplete words and incorrect word separation. Your task is to correct these errors and normalize the words according to the spelling rules standard before continuing with information extraction.

The article is: *ArticleText*

Now that the article has been corrected, perform the following tasks consistently:

1. Identify and list all mentions of departments only in Colombia and save them in the "departments" field
2. Identifies and lists all mentions of locations in Colombia, such as Capital district (Bogotá), tourist district (Cartagena de Indias), municipalities, townships, paths, towns and rural areas that appear in the article. It also includes any relevant Colombia-only locations in the field called "municipalities".
3. For the departure of insurgent forces, take into account guerrillas, self-defense or paramilitary groups and drug trafficking groups.

4. Includes in the list of army units only those that are mentioned in the article, covering names of commands, battalions, divisions, brigades, Companies, Platoons and Squads. The names of these units may consist of personal names, Roman numerals, or ordinal numbers, as II Brigade, II Brigade, José María Battalion and Seventh Brigade. You do not generically include the army, national army, insurgent forces or names of generals.

5. Identify and list all the government institutions mentioned in the following article. Institutions to consider include the Ombudsman's Office, Attorney's Office, Prosecutor's Office, mayor's offices and governorships.

6. To identify the department (Save it in *ColumnName*) and/or municipality, township or vereda (Save it in *ColumnName*) headquarters of the newspaper:
   (a) Search on this line: *ArticleText*
   (b) If nothing is found, search in the first 100 characters
   (c) If neither is found, look to see if the word after the title is a location. The title is *ArticleText*
   (d) If neither is found, look to see if the last word of the text is the name of a location.

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7. Includes the list of units of the national navy only those that are mentioned in the article, including marine infantry, coast guard commands, Naval Operations Command, surface units.

8. Includes to the list of air force units only those that are mentioned in the article, covering Air Combat Command (CACOM), Air Combat Group (GAC), squads.

9. Make sure you don't include duplicates in your lists, even if an item is mentioned multiple times in the article. Do not include anything that is not present in the article.

10. Check if the article contains information on the creation (foundation) and/or deactivation (Closing or dismantling) of formal Colombian military units (battalions, divisions, brigades, companies, bases) and not temporary ones

11. In case you find founded Colombian military units, extract the date of creation, the name of the unit and the text where its creation is specified (No more than 20 words). Returns the information in *ColumnName*.

12. In case you find deactivated Colombian military units, extract the creation date, the name of the unit and the text where its deactivation, dismantling or closure is specified (No more than 20 words), only the paragraph or phrase where this was specified. Return the information on *ColumnName*.

13. In case you cannot find the name of a created or disabled drive, it returns an empty record. And it only returns military units or divisions from Colombia.

14. You should not show the corrected article. Just the JSON

15. Only show data found in the text of the article. Do not make inferences or add locations that are not explicitly mentioned in the content of the article and make sure they are from Colombia.