THREE ESSAYS ON DEVELOPMENT ECONOMICS

by

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Abstract

This dissertation examines how rural households in developing countries respond to two major challenges to their livelihoods: exposure to armed conflict and persistent barriers to agricultural productivity. The three chapters analyze how conflict affects household decisions and welfare, and how market-based interventions can mitigate underinvestment in agricultural inputs. Across all chapters, the dissertation highlights how poor households manage risk in settings characterized by uncertainty, weak institutions, and limited information.

The first chapter studies the behavioral responses of rural Colombian households to landmine-related events. The analysis shows that households exposed to recent nearby landmine events are less likely to engage in long-term agricultural work and are more likely to substitute into short-term labor or hire others for farm tasks. These patterns vary by land ownership, suggesting that liquidity constraints shape households' ability to adjust. The chapter also documents that landmine exposure discourages preventative healthcare-seeking behavior, highlighting the broader welfare consequences of violence.

The second chapter investigates the unintended effects of military presence on civilian populations. It leverages the expansion of the Colombian army in the 2000s to examine whether the establishment of new military bases led to increases in sexual violence in host communities. The results indicate that reported cases of sexual violence rise following the opening of a base, particularly when the base is staffed primarily by drafted soldiers. The findings underscore the importance of designing security policies that minimize harm to civilians.

The third chapter shifts focus to agricultural markets. It uses a lab-in-the-field experiment with Kenyan maize farmers to assess how the entry of a high-reputation seller affects demand for hybrid seeds. The experiment shows that access to a second, higher-quality seed option increases demand, especially among farmers initially offered a lower-quality product. These results suggest that improving market quality can spur the adoption of productivity-enhancing technologies.

Together, these chapters provide new insights into how violence and uncertainty shape economic behavior and demonstrate how market interventions can mitigate their costs.

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Introduction

People living in rural areas of developing countries face multiple challenges that affect their livelihoods. Among the most pressing are exposure to armed conflict and persistent barriers to agricultural productivity. These challenges are not only widespread—conflict and low agricultural yields are increasingly common—but they also disproportionately affect the rural poor. For example, in 2023, the number of active state-based armed conflicts reached a post-Cold War high of 59 (Davies et al., 2024). At the same time, agricultural productivity in low- and middle-income countries remains low, with the slow adoption of modern inputs such as hybrid seeds and fertilizers cited as central factor, especially in Sub-Saharan Africa (Jack, 2013).

This dissertation contributes to our understanding of how rural households in developing countries respond to these challenges. The first two chapters examine how conflict shapes household decisions and welfare. The third chapter shifts to a market-based intervention aimed at addressing underinvestment in agricultural inputs. Across all three chapters, I study how poor households manage risk in contexts where information is incomplete, institutions are weak, and decisions carry significant consequences for their livelihoods. These chapters highlight both the behavioral adjustments that arise in response to violence and uncertainty, and the potential for market mechanisms to reduce the costs of those adjustments.

In Chapter 1, coauthored with Sakina Shibuya, I study how landmine-related events shape household livelihoods in rural Colombia. I focus on two behaviors: labor market outcomes and healthcare-seeking behavior. Colombia, a country affected by a long-lasting conflict since the 1960s, has seen non-state armed actors use landmines extensively as a war strategy. These groups have placed landmines on agricultural land, walking paths, and near roads, turning agricultural labor and commuting into life-threatening activities. As a result, individuals living in contaminated areas may reallocate their labor both within and outside their farm and avoid activities that require leaving the home.

My findings show that households tend to avoid risky activities after nearby landmine events. In particular, individuals are less likely to engage in long-term agricultural work outside their farms if a landmine event occurred within 5 km of their residence in the six months prior to the planting season. Similarly, exposed households are less likely to work on agricultural tasks on their own farms and are more likely to hire others to perform them. However, I find that exposed individuals are more likely to engage in short-term agricultural labor. I hypothesize that this may be due to liquidity constraints: liquidity-constrained individuals may take on short-term jobs to offset their reduction in longer-term agricultural work. My findings support this claim. Landowning individuals—who are less likely to face liquidity constraints—reduce long-term agricultural labor after nearby landmine events but do not substitute into other work. In contrast, non-landowning individuals reduce long-term agricultural labor and increase participation in short-term agricultural day labor. Finally, I find that landmine exposure also discourages preventative healthcare-seeking behavior: adults are less likely to seek formal healthcare for preventative reasons after a nearby landmine event. These findings highlight how wealth and access to markets shape households responses to violence and underscore the wide-ranging welfare consequences of landmine contamination.

In Chapter 2, also coauthored with Sakina Shibuya, I examine the effect of military presence on sexual violence in Colombia. In the 2000s, the Colombian army underwent a major expansion as part of a broader counterinsurgency strategy, including the creation of new military units and bases. I study whether the establishment of these new bases led to an increase in reported cases of sexual violence in host communities. I find that reports of sexual violence cases registered with the Office of the Attorney General increase following the opening of a military base. This effect appears to be driven primarily by bases predominantly staffed with drafted soldiers, compared to those staffed mainly by professional soldiers. I also explore whether this increase in sexual violence has broader consequences for host communities. I find no evidence of changes in fertility or in the number of child support lawsuits in treated municipalities. These findings highlight the unintended negative consequences that military deployments may have on civilian populations and point to the importance of designing counterinsurgency policies that mitigate harm to civilians.

In Chapter 3, coauthored with Joshua Deutschmann and Emilia Tjernström, I examine how the entry of a high-reputation seller affects the demand for hybrid maize seeds—an agricultural input whose quality is difficult to observe before usage. I conduct a lab-in-the-field experiment with maize farmers in Kenya, in which participants choose between an unknown quantity of maize seeds and a known quantity of an alternative good. For a randomly selected subset of participants, a second seed option becomes available. This second option represents a high-quality entrant and offers a higher expected seed quantity than the first option. I find that participants are more likely to choose seeds over the alternative good when this second seed option is available. This effect is particularly strong among participants whose incumbent seed option has a lower expected quantity. These findings suggest that the entry of high-reputation sellers can increase demand for high-yield agricultural inputs and may accelerate the adoption of productivity-enhancing technologies, especially in settings where input quality is hard to observe.

In conclusion, this dissertation advances our understanding of how conflict shapes household behavior and how market-based interventions can promote the adoption of agricultural technologies. The evidence presented in these chapters provides insights that can help policymakers design more effective responses to the challenges faced by populations in conflict-affected areas and in settings with persistently low agricultural productivity.

Chapter 1

Watch Your Step: The Economic and Behavioral Responses of Rural Households to Landmines During Conflict

1.1 Introduction

Anti-personnel landmines, widely deployed in conflicts due to their low cost and ease of production, pose a grave threat to civilians that extends far beyond their military purpose. In 2022, landmine contamination affected at least 60 countries, with civilians accounting for approximately 85% of victims (International Campaign to Ban Landmines, 2023). Colombia exemplifies this crisis, having endured over 30 years of contamination amid a six-decade conflict, where 61% of the 12,000 documented victims were civilians (United Nations Mine Action Service, 2022). Beyond direct casualties, landmines reshape rural livelihoods by instilling fear and restricting mobility. Uncertainty about their locations confines individuals to their immediate surroundings, limiting access to farmland, hunting and fishing areas, schools, and health clinics – threatening not only food security and economic production but also access to essential services (Commission for Truth, 2022a). Yet, little is known about how households adapt their livelihood strategies and healthcare access in response to these security threats while continuing to live in affected areas.

This chapter examines how landmine exposure affects rural households' labor allocation and healthcare utilization amid ongoing conflict in Colombia. Our analysis focuses on three key dimensions. First, we assess how landmines influence labor allocation across income-generating activities with varying levels of exposure risk. Specifically, we distinguish between agricultural day labor (*jornalero*), agricultural non-jornalero, and non-agricultural non-jornalero jobs.[1] Agricultural work, particularly jornalero and agricultural non-jornalero jobs, carries a higher risk of landmine exposure because it requires working in fields, whereas non-agricultural non-jornalero jobs, such as processing harvested corn and selling tortillas from home, carry lower risk.

Second, we investigate why some farmers reduce engagement in risky work while others increase it, examining heterogeneity by land ownership, an important determinant of borrowing capacity and, consequently, risk tolerance. Finally, we explore the impact of landmine exposure on preventive healthcare utilization, an important outcome given that formal healthcare services are often distant and difficult to access for rural residents in Colombia (Ivarsson et al., 2023). By integrating these dimensions, our study provides a comprehensive perspective on how conflict-related hazards reshape economic behavior and access to essential services in rural areas.

To estimate the causal effect of landmine exposure, we address the identification challenge posed by the geographical and temporal endogeneity of landmine events. To account for pre-existing exposure and conflict dynamics, we include individual and year fixed effects. Additionally, we incorporate baseline municipality characteristics interacted with year fixed

[1] Jornaleros are individuals who work directly in agricultural production, typically paid a fixed amount or piece rate. These jobs generally offer lower wages and less stability compared to non-jornalero jobs. Agricultural non-jornalero positions may include roles such as a farm manager on a relatively large farm, responsible for maintaining the land and organizing workers.

effects to control for region- and time-varying conflict dynamics. Lastly, to address concerns about selective migration, we restrict our sample to households that remained in the same municipality across all three survey rounds.[2] This ensures that our analysis captures how households who stay in conflict-affected areas adapt to landmine exposure.

We use restricted spatial data from the Colombian Longitudinal Survey, collected every three years from 2010 to 2016. The survey includes households from regions in Colombia with varying levels of conflict intensity prior to the signing of the 2016 peace agreement, allowing us to capture household behavior in a period when non-state armed actors were continuously installing landmines. We combine this data with publicly available administrative records of landmine events dating back to 1990. The precise location and date of each landmine event enable us to determine whether these incidents occurred near a household's residence before the survey was administered.

Surprisingly, we find that farming households increase engagement in jornalero work (agricultural day labor) while reducing participation in more stable non-jornalero agricultural work following recent landmine exposure. This shift suggests that, rather than avoiding all agricultural labor, households may reallocate toward more flexible or short-term arrangements. We also find evidence that households reduce work on their own farms. These patterns are counterintuitive, as one might expect landmine contamination to reduce outdoor work across the board, particularly in agriculture, where risk of landmine exposure is high.

To understand these seemingly contradictory results, we examine heterogeneity along two key dimensions: prior exposure to landmine events and land ownership. Individuals with prior exposure may respond differently to new landmine events, potentially because they are more accustomed to risk or because they have updated beliefs about the likelihood of harm. Land ownership, in turn, is closely tied to financial security and borrowing capacity, which

[2]We test whether exposure to landmines affects the likelihood of individuals remaining in the final analysis sample. Across survey years, we find no statistically significant effect of recent landmine exposure on sample inclusion.

may influence households' ability to absorb shocks and avoid the most hazardous forms of work. In the next section, we explore how these factors shape labor allocation decisions in the aftermath of landmine contamination.

We find that individuals without prior landmine exposure adjust their labor allocation more sharply in response to new landmine events. In the immediate aftermath, they reduce participation in non-jornalero agricultural work. In contrast, those with previous exposure show more muted responses, with statistically significant reductions observed only in own-farm labor. Interestingly, both groups increase their reliance on hired labor for agricultural work on their own farms.

We find that households without land reduce participation in non-jornalero agricultural work and shift toward jornalero jobs, while landowning households reduce participation in non-jornalero work but do not take on more jornalero labor. These contrasting responses suggest that landowners may be better able to avoid high-risk employment. Further analysis using household member-level time-use data reveals that individuals in landowning households work 58% (2 hours) *fewer* hours in non-jornalero agricultural jobs, while those in non-landowning households work 133% (5 hours) *more* in the same type of work.

Our analysis reveals that landowning households seem to mitigate landmine exposure by reducing agricultural work altogether, while non-landowning households increase their participation in agricultural labor, both on and off their own farms. We consider two possible mechanisms driving this behavior. First, non-landowning households may turn to agricultural labor as a coping strategy to offset lost non-agricultural income following landmine exposure. Our data confirms that landmine events reduce non-agricultural earnings for these households, though it remains unclear whether this results from reallocating labor out of necessity or due to changed preference. Second, rural labor shortages caused by conflict may have driven up agricultural wages, incentivizing non-landowning households to take on more farm work. However, due to limited rural labor market data and household job preference information, we cannot fully test this hypothesis. While our study provides evidence on these mechanisms, future research could address them more directly through targeted data collection efforts.

The impact of landmine exposure extends beyond economic decisions, shaping access to essential services like healthcare. Just as landmines constrain movement for work, landmines can restrict access to medical care. In fact, we find that adults are 12% (8 percentage points) less likely to visit a formal medical facility without being sick soon after landmine events. In contrast, they are 60% (2 percentage points) more likely to seek alternative medicine, likely due to its closer proximity and lower transportation costs. A similar pattern emerges for children's healthcare: children exposed to landmines are 28% less likely to visit a dentist.

Taken together, these findings highlight the complex trade-offs households make in response to landmine exposure. Households that own land and have the financial capacity to absorb income losses appear to reduce risky agricultural work to avoid potential harm. In contrast, non-landowning households actually increase their engagement in riskier farm labor, although our data do not allow us to determine whether this shift is driven by necessity, changing economic incentives, or other unobserved factors. Meanwhile, our healthcare findings provide further evidence that landmine exposure restricts mobility, as affected households become less likely to seek preventive care. This suggests that the economic disruptions caused by landmines are not just about labor reallocation but also about broader constraints on movement, which limit access to essential services and may contribute to worsening overall well-being.

This paper makes three key contributions. First, we fill an important gap in the landmine literature by providing rigorous evidence on the contemporaneous effects of landmine exposure. Much of the existing research has focused on the long-term consequences of landmines (Merrouche, 2008, 2011; Takasaki, 2020; Lekfuangfu, 2022), with some notable exceptions (Camacho, 2008; Arcand et al., 2015; Vargas et al., 2024). Among these, Camacho (2008) is particularly relevant, examining the in utero impact of landmine explosions on child birth weight in Colombia and finding that maternal stress from exposure negatively affects birth outcomes. While Camacho's study focuses on health at birth, our research investigates landmine exposure's effects on household labor allocation and healthcare-seeking behavior. Similarly, Vargas et al. (2024) explores how landmine explosions near polling stations affect voter turnout in Colombia. Our work extends this approach by analyzing landmine exposure's effects on economic decisions and healthcare access, broadening our understanding of how landmines shape behavior in the short term.

Second, we contribute to the literature on the local economic effects of demining campaigns (Chiovelli et al., 2024; Prem et al., 2024). While these studies use geocoded data to analyze how demining affects outcomes like nightlight intensity, standardized test scores, and deforestation, they do not examine how individuals respond to landmine contamination itself. Our study complements this research by focusing on household-level behavioral adjustments to landmine exposure, shedding light on the microeconomic mechanisms behind the broader trends identified in demining studies.

Finally, we contribute to the literature on violent shocks by estimating the effects of landmine exposure with greater granularity and precision than previous studies (Verpoorten, 2009; Besley and Mueller, 2012; Bove and Gavrilova, 2014; Brown and Velásquez, 2017; Rockmore, 2017; Arias et al., 2019; Brück et al., 2019; Brown et al., 2019; Adelaja and George, 2019). Two notable exceptions are Callen et al. (2014) and Blumenstock et al. (2024), who use geocoded data on violent events to study the effects of conflict on risk preferences and mobile money use in Afghanistan. We extend this approach by integrating high-resolution household location data from the Colombian Longitudinal Survey (2010, 2013, and 2016)[3] with administrative records of landmine events dating back to 1990. By leveraging precise location and timing of each landmine incident, our study provides novel insights into how individuals and households adapt their labor and healthcare decisions in response to localized security threats.

The rest of the chapter is organized as follows. Section 1.2 provides context on the

[3]Access to household location data is restricted and requires a permit.

Colombian armed conflict and the strategic use of landmines by armed actors. Section 1.3 presents a conceptual framework to understand how farmers respond to landmine exposure. Section 1.4 discusses the empirical strategy, including data sources, landmine event records, and the identification approach used to estimate the effects of landmines on rural households' behavior. Section 1.5 presents the main findings and explores potential mechanisms driving these effects. Finally, section 1.6 summarizes the results and discusses their broader implications.

1.2 Background

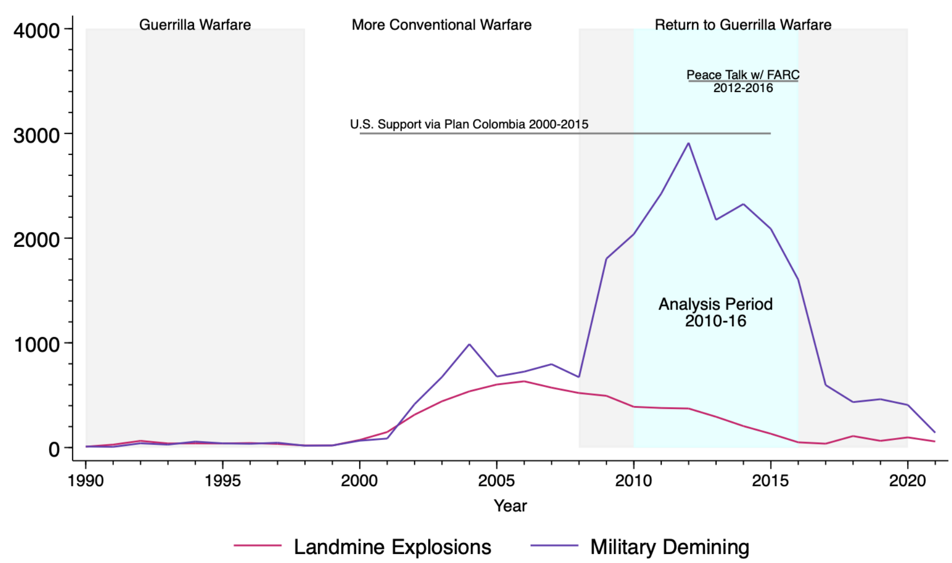
1.2.1 Colombian armed conflict

Colombia has been in one of the world's longest armed conflicts since the mid-1960s. This long-standing, low-intensity conflict involves the Colombian state and various insurgency groups, some of which have roots in peasant uprisings at the beginning of the conflict, though today entangled with drug production and trafficking and terrorism. The war is extremely violent and chaotic, involving not only fights between the states and guerrilla groups, but also conflicts among various insurgent groups (Sweig, 2002). It is estimated that at least 220,000 people were killed between 1958 and 2012, of which 80% is civilian (Centro Nacional de Memoria Histórica, 2016). While the Colombian government and the Revolutionary Armed Forces of Colombia (FARC, by its Spanish acronym), a major insurgency group, have reached a peace agreement in 2016, the conflict persists.

1.2.2 Used of improvised landmines

Although the conflict in Colombia has lasted for over half a century, the widespread use of improvised antipersonnel landmines is a relatively recent development, beginning in the 1990s. Figure 1.1 illustrates the trends in landmine explosions and military demining operations since 1990. Guerrilla groups significantly escalated their use of handmade landmines by the late 1990s, as reflected in the sharp rise in explosions. In the mid-2000s, the Colombian Army improved its capacity to detect and remove landmines with the establishment of Explosions and Demolitions Groups (EXDE, in Spanish), leading to a decline in landmine explosions and an increase in mines removed during military operations. Insurgent groups continued installing these devices until 2013, when peace negotiations with the government started.

Figure 1.1: Landmine related events and analysis period



There are two main non-governmental militia groups that used these inexpensive explosive devices: FARC and the National Liberation Army (ELN) (Centro Nacional de Memoria Histórica, 2016). These insurgency groups used hand-made landmines to compensate for the lack of military capacities relative to the government forces, and curb the advancement of their opponents.

Improvised landmines that these groups used were easy and inexpensive to produce, and very difficult to detect. Figure 1.2 provides examples of improvised anti-personnel landmines typically used by insurgency groups. Such landmines can be made with common household materials such as plastic soda bottles that can easily be found even in a very remote area of the country. One estimate suggests that such landmines can be produced and laid for USD

3 to 30 (ReliefWeb, 2001).

Figure 1.2: Examples of improvised antipersonnel landmines in Colombia

(a) Artisanal landmine

(b) Minefield in Valle del Cauca

Landmines are incredibly difficult to find once they are installed. They contain minimal metals, which make it extremely hard to locate with metal detectors which is a common tool used for landmine identification (ReliefWeb, 2017). While landmines are commonly placed underground, they are sometimes installed on trees in order to affect different parts of the body.

1.2.3 Placement of landmines

Learning about the purpose and strategies of landmine installation gives us a sense of the proximity to landmines with which rural Colombian households have lived, even though landmines were not installed to harm civilians, rather to slow down the Colombian military advancement. As such, landmines were manufactured so that they would severely injure members of the military in lieu of killing them. By injuring soldiers rather than killing them, insurgency groups can increase the high cost of the war, for the government would have to take care of landmine-affected soldiers, who are often severely mutilated and require long-term support. Landmines also exerted an enormous moral and psychological effect on the official forces.

To obstruct the State's military advancement into their territories in rural Colombia, guerrillas installed landmines in footpaths, near their valuable assets including coca fields, and near camps (Centro Nacional de Memoria Histórica, 2016, 2017). In rural Colombia where vegetation is thick and the availability of walking paths is limited, footpaths that state soldiers use are often those villagers use. These roads pass next to farming plots (lower photo in Figure 1.2), where some households also have their residences. Guerrillas also often use local schools for meetings and resting at night, as school buildings are often the only large structures in rural villages.

Insurgents kept track of the exact locations of landmines that they installed in order to avoid injuring their own members (Centro Nacional de Memoria Histórica, 2017). Such knowledge was kept in secret for obvious military strategic reasons; however, guerrillas have occasionally told villagers approximate landmine locations. Villagers often found such knowledge inadequate, because keeping one safe requires exact locations, and only knowing approximate location makes the whole area unusable, thus unproductive (Monitor and Cluster, 2018).

1.2.4 Military demining operations

Given the significant harm caused by landmines on military operations, official forces developed methods to protect troops from stepping on them. One such method involved assigning a team of five soldiers who were trained in mine removal to accompany each squad. These teams, known as Explosives and Demolition Groups (EXDE), were equipped with dogs and metal detectors to locate and remove mines.

The procedure for detecting and removing mines used by the EXDE group was as follows: when the group suspected the presence of a minefield, they would first use a trained dog to locate potential mines. The locations identified by the dog were then confirmed using a metal detector. Any mines found were either removed or detonated safely. The EXDE group also recorded the coordinates of each mine's location and the number of mines removed or destroyed.

Insurgent groups employed tactics to evade detection by demining efforts, such as masking the scent of explosive substances with coffee, and avoiding the use of metallic materials. This can be seen in the increase of demining operations during military actions following the planting of mines by guerrillas in the late 1990s, peaking around 2013. However, after 2015, there was a significant decrease in military demining operations as humanitarian demining efforts increased following the conclusion of peace talks.

The peace agreement signed between the Colombian government and FARC marks a new period in terms of the use of landmines. While the agreement led to the bilateral and definite ceasefire, and ended the use of landmines by FARC, it also included the disclosure of existing landmine locations to the Colombian government, which then provided the information to humanitarian demining operators among others. Villagers were gradually informed of exact landmine locations after the historic peace agreement. Thus, this study focuses on the period before 2016, as it investigates the role of uncertainty around landmine locations.

1.2.5 Influence of landmines on rural households' livelihoods

Given that insurgent groups tend to place landmines in areas commonly used by rural households, the presence of these devices may significantly alter household behavior, as individuals seek to avoid encountering them. Testimonies from rural Colombians offer qualitative evidence of behavioral changes resulting from the threat of landmines.

As discussed earlier, households in conflict-affected areas often lack precise information about landmine locations. Instead, individuals update their beliefs about the presence of landmines based on nearby incidents. This dynamic is captured in the words of a woman from rural Colombia: "I cannot shake off the fear that I might step on something or hear another explosion. That fear does not go away; I never leave the path, but you still live with the fear that you might have stepped off by accident" (Centro Nacional de Memoria Histórica, 2017, p. 135). People also use other conflict-related events to form expectations about landmine contamination. For example, a landmine survivor explains: "You are practically never at ease because in an area like this, the Army and the guerrilla pass through, so you cannot feel calm since the guerrilla lays their mines, and how is one supposed to know where they put them? You are always left in doubt" (Centro Nacional de Memoria Histórica, 2017, p. 136). The lack of reliable information about landmine locations amplifies their disruptive impact, forcing households to adapt their daily activities to minimize risk.

Rural households report that certain activities—especially agricultural work—become particularly hazardous in the presence of landmines. A female farmer, whose husband was injured by a landmine, recounts: "When he stepped on that mine, he was alone (...) he had gone to burn a clearing to plant cassava and plantain, and just as he was arriving at the clearing on horseback, there was an explosion. I had a feeling it was my husband" (Centro Nacional de Memoria Histórica, 2017, p. 131). The threat of landmines also deters labor outside the households' farms. As one male agricultural worker explains: "Back where we used to live, whenever there was work to clear brush or cut grass, no one would go because you would end up finding a landmine" (Centro Nacional de Memoria Histórica, 2017, p. 134). These testimonies suggest that households reallocate labor away from riskier tasks, potentially reducing income and worsening their economic well-being.

The impact of landmines extends beyond labor allocation to other activities that require leaving home. One testimony illustrates this shift in daily habits: "Yes, habits have changed; we usually stick to the road or a path, we try not to stray from it, bring our own water, try to use the bathroom before or after, and those outings... those trips, those routines, they do not really happen anymore" (Centro Nacional de Memoria Histórica, 2017, p. 135). As this suggests, landmine contamination may reduce the frequency of activities that involve leaving the home, such as accessing healthcare, attending school, among others.

Together, these testimonies illustrate how rural households adjust their behavior in response to the presence of landmines—by reallocating labor and reducing activities that require movement outside the home. In the following sections, we provide empirical evidence to further examine and support these claims.

1.3 Conceptual Framework

Landmines pose a severe threat to life, instilling fear in individuals exposed to them and discouraging participation in activities that may increase the likelihood of encountering these devices. Armed actors typically place landmines on agricultural land, walking paths, and next to roads, making activities such as working on fields and commuting potentially hazardous. As a result, in response to new landmine events, individuals may avoid activities like agricultural labor—whether on their own fields or on other households' farms—as well as any tasks requiring them to leave their home.

Our argument is based on the idea that individuals form beliefs about landmine contamination in the areas where they live, as they lack precise knowledge of the locations where non-state armed actors have installed these devices. This belief is represented as a subjective probability of encountering landmines during daily activities. An individual's subjective probability depends on their past exposure to landmine events: those previously exposed are likely to believe that similar incidents will continue to occur nearby, while individuals without past exposure may consider future events less likely in their vicinity. Consequently, individuals adjust their beliefs about landmine presence upwards after witnessing a new event. Over time, however, if no new events occur, the subjective probability declines as individuals become accustomed to living among these devices.

Consequently, individuals adjust their behavior in response to recent landmine events in three key ways. First, they tend to avoid agricultural labor—whether on their own farms or elsewhere—as well as non-agricultural work conducted outside their home, as non-state armed actors frequently place landmines in fields, along walking paths, and near roads. This avoidance mechanically leads to a decline in labor income. Second, exposed households may opt to hire agricultural workers for their plots, reducing their own involvement in farm labor while maintaining agricultural production levels. Third, individuals avoid activities that require leaving home, such as seeking healthcare, due to the need to travel to health centers, which are typically located in urban areas.

However, responses to landmine events may differ for those facing liquidity constraints. Liquidity-constrained individuals cannot afford to reduce labor activities after landmine events without lowering their consumption. Consequently, they may need to maintain or even increase participation in risky labor activities if their income from other sources declines. Specifically, liquidity-constrained individuals might continue working—or increase their work—in both agricultural jobs and non-agricultural occupations outside home, despite the associated risks. For instance, households might experience reduced income following landmine events, which could lead to a drop in local demand for goods and services. Individuals employed in these sectors may then supply less labor to these economic activities and seek work in areas with higher demand. One such option is agricultural labor, as neighboring farmers affected by landmines may prefer to hire external labor to replace their own in their fields.

Individuals' responses to new landmine events may also differ based on their prior exposure to such incidents. On one hand, those with past exposure may not significantly alter their behavior in response to new events, as they have adapted to living amid these devices. Having experienced landmine events in the past, they consider future incidents likely to occur and may have learned which actions to take to avoid landmines. On the other hand, individuals without previous exposure may react differently to new landmine events. Since they perceive such events as unlikely, they may lack strategies to navigate these risks while conducting their daily activities. Consequently, those without prior exposure may avoid activities that would increase their chances of encountering landmines after a new event occurs.

Predictions. Our framework suggests several predictions that we test empirically. First, in the absence of liquidity constraints, individuals are expected to respond to new landmine events by reducing both agricultural labor—whether on their own farm or on others'—and non-agricultural labor conducted outside home. As a result, households are likely to increase their hiring of agricultural workers, substituting their own labor with external labor. Similarly, following landmine events, individuals tend to avoid seeking healthcare, particularly when medical facilities are located far from their home and require travel.

Second, liquidity-constrained individuals may instead continue working in both agricultural jobs and non-agricultural occupations outside their home. They may even increase their participation in these jobs if they experience a reduction in income from other sources.

Third, individuals without previous exposure to landmines are expected to respond to new events by reducing both agricultural and non-agricultural labor conducted outside home. Conversely, individuals with prior exposure to landmines are predicted not to adjust their behavior in response to new landmine events.

1.4 Empirical Strategy

1.4.1 Data

To estimate the effects of landmine presence on farmers' behavior, we combine the administrative data on landmine explosions and military demining operations, and the longitudinal data from a survey that tracks households in rural Colombia.

Landmine Related Events Data

The data on landmine explosions and military demining operations used in this study was obtained from the Office of the High Commissioner for Peace (OACP). The dataset spans from 1990 to the present and includes information on the date, location, and number of civilian and military casualties for landmine explosions, as well as the number of ordnance removed or destroyed during military demining operations.

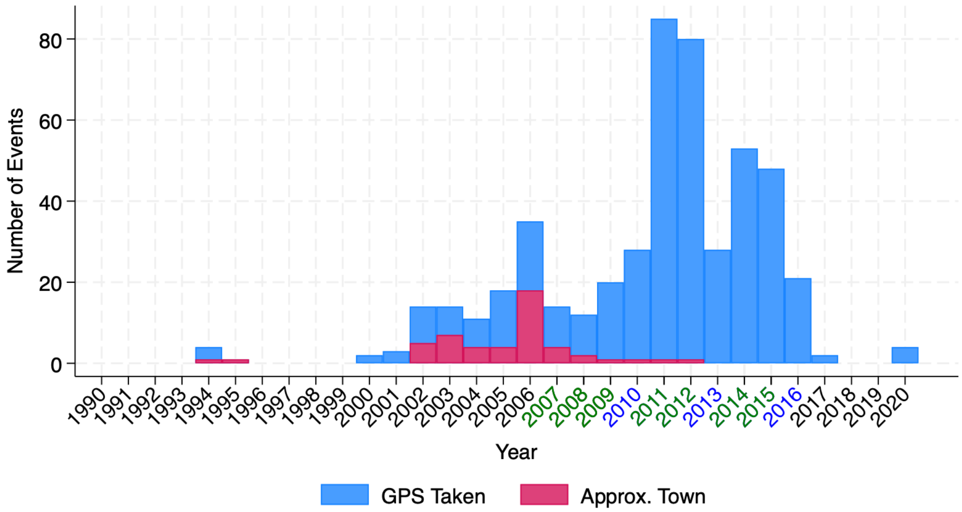
The OACP has been recording both landmine accidents and incidents[4] in the Information Management System for Mine Action (IMSMA) daily since 2002, which is the United Nations' preferred information system for managing data in UN-supported programs. Most information is sourced from local authorities, the national civil defense, national park rangers, and the armed forces. The agency also conducts interviews with survivors and affected civilians to supplement the data. For the period from 1990 to 2001, the OACP established a baseline using information from both government and non-government sources, such as newspapers and mass media. Additionally, IMSMA logs details on all demining operations conducted by the Army during this time.

The data includes the latitude and longitude coordinates for each event. The coordinates of military demining events are accurate as they were taken with GPS devices. However, the coordinates for explosions do not always correspond to the exact locations where the events occurred. Some explosions are recorded in the OACP dataset because of reported by victims or unharmed civilians. In such cases, the coordinates were often approximated to the township of the municipality where the incident occurred, as the exact location was not always known. This may pose a problem, as events approximated to the municipality's township can introduce measurement error. Households close to townships could be mistakenly considered affected by landmines, whereas some households exposed to landmine events could be treated as unexposed.

We conclude that the concern for measurement error is small. Figure 1.3 illustrates the number of landmine-related events recorded by the method of location recording in the municipalities where the surveyed households reside, as well as in the neighboring municipalities. The graph shows that, for the analysis period, we know the exact location of the majority of events.

[4]Landmine accidents are undesired events which results in harm, whereas a landmine incident is an event that gives rise to an accident or has the potential to lead to an accident.

Figure 1.3: Landmine related events and location type in surveyed and neighboring municipalities



Notes: Blue indicates the years in which ELCA data were collected, while green represents the years in which landmine exposure is considered

Household Panel Data

The Longitudinal Survey of Colombia (ELCA) is a study that tracks households and individuals over time, collecting data in 2010, 2013, and 2016. The survey is representative of urban areas in Colombia and representative of four specific micro-regions of the country at the rural level. ELCA originally targeted 4,578 rural households, comprising 8,365 adults (i.e., household heads and their spouses) and 4,411 children under nine years old. The original rural sample was located in 224 villages (*veredas* in Spanish), across 17 municipalities. The data includes household and individual characteristics, including access to and use of medical services, land ownership and use, hours spent on agricultural tasks on family and non-family farms, hours spent on non-agricultural wage labor, and crop choices. ELCA contains household GPS locations which can be accessed with permission on a secure server and dates when the surveys were administered.

We conduct the empirical analysis on a balanced panel of households who stayed in the rural area of the same municipality for all three rounds. Additionally, we exclude households where the household head changed due to the household splitting between rounds, but keep households where the household head remained the same even if the household split. Moreover, we remove from the analysis households with no follow-up subjects in all three rounds. We conclude with a sample of 3,215 households, accounting for 5,518 adults. For the children's sample, we consider individuals who appear in at least two consecutive rounds, resulting in a sample of 2,888 children from 1,763 households.

Individuals may migrate or leave the sample due to violence, particularly in response to the presence of landmines. We test this possibility by examining whether prior exposure to landmine events predicts an individual's likelihood of remaining in the sample used for the econometric analysis. Specifically, we estimate a model where the outcome variable is an indicator of whether an individual appears in both (i) 2010 and 2013, (ii) 2010 and 2016, and (iii) 2013 and 2016. The key independent variable is an indicator of whether at least one landmine event occurred within 5 km of an individual's residence in the three years preceding the survey interview. All specifications control for age, years of education, gender, and municipality fixed effects. Table 1.1 presents the results. Our findings indicate that exposure to nearby landmine events prior to the survey does not significantly predict migration and/or sample attrition.

Table 1.1: Effect of landmine events on being part of the analysis sample

	(1) Appears in 2013	(2) Appears in 2016	(3) Appears in 2016
If events in (0-36] months	0.032	-0.031	-0.034
	(0.022)	(0.029)	(0.025)
Dep Var Mean	0.810	0.676	0.836
Sample	2010	2010	2013
Observations	8,156	8,156	6,588

Notes: Standard errors clustered at the village level in parentheses. Outcome variable are indicators of whether individual was surveyed in 2013 (column 1) and 2016 (columns 2 and 3). In columns 1 and 2, sample is all the individuals surveyed in 2010, and in column 3, sample is all the individuals surveyed in 2013. Independent variable is an indicator of whether individuals experienced a landmine event within 5 km of their residence in the 36 months prior to being surveyed. All specifications include age, years of education, and an indicator of whether individual is male as covariates, and municipality fixed effects. *** 1%, ** 5%, * 10%

Treatment Variable Construction

We combine the household GPS coordinates with the locations of landmine-related events to identify which households were exposed during different time windows. To do so, we first construct a 5 km-radius buffer centered at the residence of each household in ELCA. We then indicate if there is any landmine event within the buffer which took place before the household was interviewed. For all three rounds, the surveys were administered between March and July, which coincided with the first planting season of the year from February to May. Based on this timeline, we define four different time windows.

Figure 1.4 illustrates the four treatment windows overlaid with the calendar months. First, we define the pre-survey window, which extends from March 1 to the date of the survey interview, varying by household. Next, we establish two periods during which households make most of their planting decisions. *Pre-planting window 1* spans from September of the year before the household is surveyed to February of the survey year, while *pre-planting window 2* covers the period from March to August of the same year. Finally, *history window* extends from February of the year preceding the survey to March three years before the household's survey interview. Any landmine events occurring within this timeframe are considered part of the household's historical exposure.

Figure 1.4: Planting Seasons and Landmine Exposure

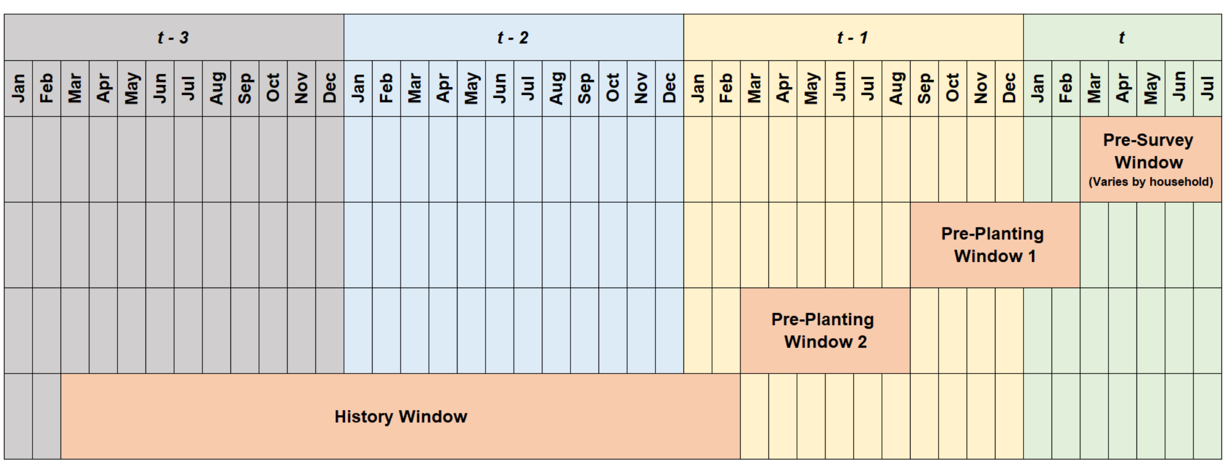


Table 1.2 presents the proportion of individuals in our sample who were exposed to

landmine events across all four time windows, as well as from 2002 until 36 months before the start of the planting season. The regions surveyed by ELCA were not as heavily affected by landmines as other parts of the country. In our individual analysis sample, 13% of individuals experienced at least one landmine event either in the six months or in the 12 to 36 months prior to the planting season. Most of these incidents occurred in 2010, a period when conflict intensity in Colombia remained high. However, landmine exposure declined after peace talks began in 2012, a period marked by a de-escalation of violence. Therefore, our findings provide insights into how individuals respond to landmine exposure in contexts of medium- or low-intensity conflict and should be interpreted with caution when extrapolating to high-intensity conflict settings.

Table 1.2: Proportion of individuals exposed to landmine events

	At Least Once	2010	2013	2016
Before survey	0.028	0.010	0.000	0.024
(0-6] months	0.129	0.073	0.034	0.041
(6-12] months	0.056	0.017	0.022	0.028
(12-36] months	0.130	0.062	0.083	0.089
Since 2002 until 36 mos.	0.237	0.104	0.202	0.219
# Individuals	5,518			

Notes: An individual is considered to be exposed to landmine events if an event occurred within 5 km of the individual's residence in the time period specified.

Outcome Variable Construction

We investigate the effect of landmine exposure on farmers' labor market decisions and healthcare seeking for preventative reasons. We use the ELCA data to construct the relevant outcome variables.

We begin by providing a brief description of the individuals in our sample. Table 1.3 presents descriptive statistics for the final sample in 2010. On average, individuals are 45 years old, indicating a middle-aged population. Additionally, the sample has relatively low educational attainment, with an average of four years of schooling—less than a complete primary education. The sample is evenly distributed between males and females. Additionally, most households are smallholders, with an average landholding of two hectares. Approximately one-third of individuals in the sample own their farmland.

Table 1.3: Descriptive statistics for first round of household data

Variable	Obs.	Mean	Standard deviation	Min.	Median	Max.
Demographics						
Age	5,513	44.93	12.22	15	45	94
Education years	5,513	4.28	3.18	0	4	18
Male	5,513	0.485	0.500	-	-	-
Access to land						
Land holdings (ha)	3,213	2.16	4.32	0	0.74	78
Ownership	3,213	0.677	0.468	-	-	-
Labor outcomes						
If worked off own farm	5,513	0.330	0.470	-	-	-
If worked as jornalero	5,513	0.229	0.420	-	-	-
If worked as non-jornalero	5,513	0.164	0.370	-	-	-
If hired jornaleros	3,213	0.362	0.481	-	-	-
Preventative healthcare-seeking						
Non-alternative	5,513	0.611	0.488	-	-	-
Alternative	5,513	0.011	0.105	-	-	-

Notes: All outcomes are at the individual level (household heads + spouses), except households' number of hectares they have access to, whether they own at least one plot, and whether they hire jornaleros.

To examine the impact of landmine events on labor market outcomes, we construct five different measures. First, we assess whether farmers worked outside their farm in the past week. Specifically, we consider jobs in the private and public sector, agricultural day labor (*jornaleros*), domestic work, and self-employment. We then consider two categories based on this outcome: whether farmers work as (1) *jornaleros* or (2) any other job. Notice that these categories are non-mutually exclusive as a farmer can be a *jornalero* and also work in a different job. Additionally, we also know the number of hours per week farmers work in non-*jornalero* jobs. Finally, we explore whether households hire agricultural workers in the past 12 months.

We observe that, in 2010, one-third of the adults in our sample worked outside their own farms. Among off own farm occupations, jornalero jobs were the most common, with 22% of individuals engaged in this type of work, while only 16% were employed in non-jornalero jobs. Additionally, labor hiring was relatively common, as 36% of households hired jornaleros to work on their fields.

In addition to these outcomes, we also analyze how exposure to landmines impacts income derived from these labor sources. We calculate the income earned by each farmer in all jobs conducted outside the household's farms in the past month. To do this, we first add the income received in all non-*jornalero* jobs, which is reported by the respondents in the survey. We then calculate the income received from *jornalero* jobs. In 2013 and 2016, respondents reported how much they earned working in this type of job, so we sum these amounts. However, in 2010, farmers only reported how many days per month they worked as *jornaleros*. In this case, we use the daily wage paid to *jornaleros* in the village from the community survey and multiply it by the number of days each farmer worked in this type of job in the month before being surveyed[5].

We also construct distinct measures using ELCA's land and agricultural production module. First, we identify land ownership by looking at farmers' response to whether they claim ownership, either formal or informal, to at least one plot. We then determine whether households have access to land; in addition to ownership of a plot, we consider households with renting or sharecropping agreements to have access to land. We also examine the amount of land farmers allocate to agricultural production. Specifically, we categorize this into four different types: land cultivated with perennial crops, seasonal crops, or mixed crops (i.e., a combination of perennial and seasonal crops within the same portion of land), and land devoted to livestock raising. Additionally, we create two broader categories of land use: land devoted to agricultural production, which encompasses all four categories previously listed,

[5]The ELCA community survey was not completed in 24 out of the 224 villages in 2010. For these villages, we substituted the missing daily wage data for *jornaleros* with the average wage from other villages within the same municipality.

and land allocated to cultivation, which includes only the land with perennial, seasonal, and mixed crops.

We also use information from ELCA's time use module to examine how much time farmers spend working in agricultural jobs in their fields. Specifically, we calculate the time each farmer allocates to this activity from the time they wake up until they retire for the night in a typical day of the week prior to being surveyed. Given the substantial number of zeros, we construct four binary variables to indicate whether farmers spent more time than some predetermined thresholds.

Finally, we examine some activities farmers typically conduct outside their farms and home. One such activity is seeking for healthcare, which usually makes farmers leave their farms and travel to the closest town. We identify if farmers visited a medical professional over the past 12 months without being sick and for preventative reasons. We look at five different medical professionals for household heads and their spouses: general practitioner or any specialist (e.g., gynecologist, urologist, cardiologist, etc.), dentist, optometrist, family planning services, and alternative medicine (e.g., homeopaths, acupuncturist, etc.). Similarly, we also identify if children 0 to 9 years old in 2010 seek medical assistance for preventative care in the past 12 months. We consider the same categories as for adults, with the exception of family planning services, and we include visits to pediatricians.

Preventative healthcare-seeking is common among farmers; in 2010, 61% of individuals in the sample visited a medical professional—excluding those specializing in alternative medicine—within the past 12 months. The most frequently consulted providers were general practitioners (60%), followed by dentists (40%) and optometrists (13%). In contrast, visits to alternative and traditional medical providers were rare, with only 1% of the sample seeking this type of care. Among children, healthcare-seeking was even more prevalent, with 83% visiting a medical professional (excluding alternative medicine providers) in the past year. Most children consulted general practitioners or specialists (76%), followed by dentists (58%), pediatricians (23%), and optometrists (13%).

1.4.2 Main identification strategy

The main identification threat in estimating the effect of landmines on economic activities of rural Colombian households is the potential correlation between conflict intensity and landmine installation. Non-state armed actors installed landmines to attack official forces and to protect strongholds and strategic assets, such as camps and coca fields. Therefore, the timing and location of landmine placement are endogenous to the characteristics of households and individuals inhabiting in these areas.

To address this endogeneity concern, we exploit the longitudinal nature of the ELCA household survey and incorporate a rich set of fixed effects in our analysis. First, we include individual fixed effects to account for time-invariant farmer characteristics, such as the initial level of landmine contamination around their residence and their prior beliefs about landmine presence. Second, we incorporate year fixed effects to account for nationwide policy changes and economic trends. However, year fixed effects do not capture region-specific changes in conflict dynamics, which tend to be very common in the Colombian context. To address this, we include interactions between a set of 2005 municipality characteristics and indicators for each round of the household survey. These baseline characteristics include population density, distance to the department's capital, average altitude, homicide rate per 100,000 inhabitants, and an indicator of whether landmine events occurred in the municipality between 1990 and 2005.

Our econometric model is specified as follows. Let y_{ihmt} be an outcome for individual i of household h residing in municipality m at year t; E^{S}_{hmt} be an indicator of whether household h had a landmine event between March 1 of year t and the date h was surveyed (*pre-survey window*); $E^{(0-6]}_{hmt}$ is an indicator of whether household h had a landmine event 0 to 6 months before March 1 of year t (*pre-planting window 1*); $E^{(6-12]}_{hmt}$ is an indicator of whether household h had a landmine event 6 to 12 months before March 1 of year t (*pre-planting window 2*); $E^{(12-36]}_{hmt}$ is an indicator of whether household h had a landmine 12 to 36 months before March 1 of year t (*history window*); ϕ_i and θ_t are individual and year fixed effects, respectively;

$x_m \times \theta_t$ is an interaction term between a 2005-level municipality characteristic x_m and year fixed effects; and ε_{ihmt} is an error term. We estimate the following equation by OLS where standard errors are clustered at the village level:

$$y_{ihmt} = \beta_1 E^{S}_{hmt} + \beta_2 E^{(0-6]}_{hmt} + \beta_3 E^{(6-12]}_{hmt} + \beta_4 E^{(12-36]}_{hmt} + \phi_i + \theta_t + \sum_{x_m \in X_m} (x_m \times \theta_t) + \varepsilon_{ihmt} \tag{1.1}$$

We cluster standard errors at the village level rather than at the individual level because, although we measure exposure at the individual level, landmine exposure tends to exhibit limited variation within villages. Table A.1 presents (i) the proportion of villages with at least one individual recently exposed to landmines and (ii) the degree of within-village variation in exposure. Most villages in our sample report no landmine exposure during the analysis period. For example, even in the period of highest exposure—the six months before the 2010 planting season—only 14% of surveyed villages had at least one individual exposed to landmines. Moreover, among villages with at least one exposed individual, exposure is typically highly correlated within the village. In 2010, half of such villages had at least 65% of surveyed individuals experiencing a landmine event in the six months prior to the planting season. This concentration increases in later years: in 2013 and 2016, half of exposed villages had 94% and 100% of individuals exposed, respectively. These patterns indicate that landmine exposure is largely a village-level phenomenon, justifying the choice to cluster at that level. Nonetheless, some within-village variation remains, as there are cases where only a few households are exposed. Therefore, it remains worthwhile to analyze landmine exposure at the individual level.

For the household level analysis of agricultural labor hiring, we use household fixed effects instead of individual fixed effects, maintaining the village-level clustering standard errors.

1.5 Results

This section presents the results of our statistical analysis. First, we discuss the analysis regarding the effect of landmine events on labor allocation. We start by presenting the general results which show that an average farming household increases engagement in jornalero jobs (agricultural day labor), while decreasing working in more stable non-jornalero agricultural work. These results are counterintuitive given that one expects landmine exposure to reduce participation in any kind of agricultural work that requires working outside and poses higher risk of landmine exposure compared to non-agricultural work that is conducted indoor. To investigate this enigma, we explore the heterogeneity along previous landmine exposure and land ownership. Finally, we present the results on the effect of landmine exposure on adults' and children's usage of healthcare services.

1.5.1 Effects of landmine events on labor allocation

We begin by examining how recent landmine events affect individuals' participation in the labor market. Specifically, we estimate the effects of landmine exposure on four outcomes: (1) whether individuals worked outside their household's own agricultural fields in the past week, (2) whether they held jornalero jobs (typically short-term, daily agricultural labor), (3) whether they engaged in non-jornalero jobs, and (4) the number of hours worked in non-jornalero jobs per week. Because individuals can hold multiple roles at once, participation in jornalero and non-jornalero work is not mutually exclusive.

Table 1.4 presents the estimated effects of landmine exposure on labor allocation outcomes. We find no change in the overall likelihood of working outside one's own farm, but this masks offsetting patterns across job types. Individuals are 16% more likely to work in jornalero jobs (3.1 percentage points) following a landmine event in the six months prior to the planting season. At the same time, they are 16% less likely to engage in non-jornalero work (3.8 percentage points) and reduce hours in these jobs by 30% (2.7 hours). These negative effects appear to dissipate over time: 12–36 months after exposure, individuals are 19% more likely to work in non-jornalero jobs (4.5 percentage points) and increase hours worked by 21% (1.9 hours), suggesting partial recovery.

Table 1.4: Effects of landmine events on labor allocation outside own farm

	(1) If worked off own farm	(2) If worked jornalero	(3) If worked non-jornalero	(4) Hours worked non-jornalero
If at least one landmine event in the period...				
Before survey	0.024	0.018	0.005	0.688
	(0.040)	(0.041)	(0.039)	(2.126)
(0-6] months	0.012	0.031*	-0.038*	-2.743***
	(0.021)	(0.017)	(0.022)	(0.909)
(6-12] months	0.005	-0.006	0.005	0.986
	(0.031)	(0.027)	(0.029)	(1.778)
(12-36] months	0.027	-0.004	0.045**	1.864**
	(0.026)	(0.022)	(0.018)	(0.739)
Dep Var Mean	0.397	0.195	0.232	8.827
# Units	5,510	5,510	5,510	5,510
# Clusters	224	224	224	224
Observations	16,530	16,530	16,530	16,530

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Sample includes household heads and their spouses when they have one. *If worked off own farm* is whether individuals worked outside the household's agricultural fields in the last week. *If worked jornalero* is whether individuals worked as jornaleros (agricultural day laborers) in the past week. *If worked non-jornalero* is whether individuals had non-jornalero jobs. *Hours worked non-jornalero* is the number of hours worked on non-jornalero jobs per week and is winsorized at the top 1%. The probability values of *If worked jornalero* and *If worked non-jornalero* do not necessarily sum to the value of *If Worked Off Own Farm*, as an individual can engage in both types of jobs simultaneously. All specifications include individual and year fixed effects and municipality characteristics interacted with indicators of each year of the survey. Municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

To better understand what type of work is being reduced, we use time-use data from 2013 and 2016 to disaggregate non-jornalero jobs into agricultural and non-agricultural activities.[6]

[6] We restrict this analysis to the 2013 and 2016 survey rounds due to inconsistencies in how time-use questions were asked across waves. In those years, respondents reported the number of hours worked in the past week for each job and its associated economic activity. However, in 2010, the survey did not ask for the total number of hours worked outside their plots in all non-jornalero occupations, which prevents us from disaggregating time worked by sector for that year.

Table 1.5 shows that the decline in non-jornalero work is concentrated in agriculture: individuals reduce hours worked in agricultural non-jornalero jobs by 54% (1.9 hours) after recent landmine exposure. In contrast, we find no significant change in hours spent in non-agricultural non-jornalero work. At the same time, individuals increase hours worked in jornalero jobs by 50% (3 hours). We also find that the recovery in non-jornalero work observed 12–36 months after exposure is driven primarily by a rebound in agricultural jobs.

Table 1.5: Effect of landmine events on hours worked in the past week

	Hours worked in the past week		
	(1) Agriculture (non-jornalero)	(2) Non-agriculture (non-jornalero)	(3) Jornalero
If event before survey	0.404	0.568	2.969*
	(2.394)	(1.803)	(1.673)
If event in (0-6] months	-1.929*	-0.484	0.841
	(1.072)	(1.101)	(0.992)
If event in (6-12] months	-0.085	0.434	-1.527
	(1.636)	(1.481)	(1.316)
If event in (12-36] months	2.299***	-0.184	1.620
	(0.805)	(0.796)	(1.218)
Dep Var Mean	3.504	6.305	5.853
# Units	5,510	5,510	5,510
# Clusters	224	224	224
Observations	11,020	11,020	11,020

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Outcome variables correspond to hours worked by individuals in the past week. Sample include household heads and their spouses when they have one. Only the last two rounds of the household survey (2013, 2016) are considered. All specifications include individual and year fixed effects and municipality characteristics at baseline interacted with year FE. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

We next ask whether these shifts are reflected in labor input on households' own farms. Table 1.6 shows that recently exposed individuals are less likely to work on their own plots and more likely to hire jornalero labor. Specifically, landmine events in the 0-6 and 6-12 months before the planting season reduce the probability of spending any time on own farm by 5.5 and 4.5 percentage points, respectively, representing declines of 12% and 11% relative to the mean. At the same time, individuals are 63% (21.2 percentage points) and 21% (7.2 percentage points) more likely to hire jornaleros in the pre-survey and 0-6 month windows, respectively. These effects diminish over time.

Table 1.6: Effects of landmine events on own farm labor

	If worked on own farm for				(5)
	(1)	(2)	(3)	(4)	If hired
	> 0 hr	≥ 1 hr	≥ 2 hr	≥ 4 hr	labor
If at least one landmine event in the period ...					
Before survey	0.004	-0.009	0.016	0.017	0.212***
	(0.044)	(0.037)	(0.038)	(0.042)	(0.058)
(0-6] months	-0.055**	-0.052**	-0.035	-0.010	0.072**
	(0.024)	(0.024)	(0.026)	(0.020)	(0.028)
(6-12] months	-0.045**	-0.046*	-0.063**	-0.026	-0.085*
	(0.022)	(0.028)	(0.025)	(0.025)	(0.048)
(12-36] months	-0.025	-0.024	-0.013	-0.018	0.034
	(0.026)	(0.026)	(0.027)	(0.026)	(0.038)
Dep Var Mean	0.455	0.424	0.364	0.258	0.335
Sample	Ind.	Ind.	Ind.	Ind.	HH
# Individuals	5,485	5,485	5,485	5,485	3,213
# Clusters	224	224	224	224	224
Observations	16,455	16,455	16,455	16,455	9,639

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. In columns 1 through 4, sample includes household heads and their spouses when they have one. *If worked on own farm for ...* is whether household members spent more than 0 hours, or greater than or equal to 1, 2 or 4 hours per day on agricultural tasks on farms that households own. *If hired jornaleros* indicates whether households have hired jornaleros (agricultural day laborers) in the past 12 months. All specifications include individual/household and year fixed effects and municipality characteristics interacted with indicators of each round of the survey. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

To verify robustness, we re-estimate these models using alternative buffer sizes (4 and 6 km) and by incorporating landmine events occurring in concentric rings beyond 5 km (5–10 km and 10–20 km). Tables A.2, A.3, A.4, A.5 and A.6 present the results with these alternative buffers. The results are consistent: labor decisions appear most sensitive to recent landmine events in close proximity, with limited response to more distant events. This pattern likely reflects both perceived safety and actual mobility constraints: households prefer to work in fields closer to home, where they have more information and shorter travel distances.

Taken together, these results reveal a striking pattern. Rather than reducing agricultural work across the board, households exposed to recent landmine events shift away from more stable, often better-paid non-jornalero agricultural jobs and increase their reliance on jornalero work, an occupation typically characterized by lower pay, instability, and greater exposure to risk. This is counterintuitive: one might expect households to avoid all forms of agricultural labor in the wake of heightened physical danger. A possible explanation for this puzzle is that liquidity-constrained households may be unable to forego risky work without experiencing a significant drop in income, especially when alternative employment opportunities are limited. In the next section, we investigate whether these responses differ by prior exposure to landmine events and by land ownership—factors that may shape how households perceive risk and constrain their ability to adapt.

Heterogeneous Effects

In this section, we examine how landmine effects vary by previous exposure to landmine events and land ownership. Previous exposure to landmine is an important factor as it affects individuals' belief about landmine contamination in their vicinity, which influence their mobility. And the formation of this belief relies on and differ by past experience. In addition, land ownership is a critical element of life in rural Colombia, because it predicts wealth and determines individuals' ability to borrow. A wealthier individual who can borrow is more likely to reduce agricultural jobs as she can more easily absorb the loss of income. In this section, we first explore the heterogeneous effect by previous exposure. We then discuss the heterogeneity by land ownership, and explore the associated results in depth.

Previous Exposure

To explore the heterogeneity along previous landmine exposure, we construct an indicator of whether individuals and households experienced landmine events in the period between 2002[7] and the three years before the start of the planting season preceding the survey interview. We consider individuals who have experienced landmine events in this period as "exposed before" and those who have not as "unexposed before." We then interact this indicator with each of the binary variables indicating if individuals were exposed to landmines during the different time windows.

Figure 1.5 presents the estimated heterogeneous effects of previous exposure on the labor allocation decisions. The graphs plot the estimated coefficients on the binary variables indicating landmine exposure at each of the time windows as the effect on the unexposed before. Meanwhile, the graphs plot the linear combination of the estimated coefficients on the said binary variables and the interaction terms between each of the landmine dummies and the previous exposure dummies in a given time period as the effect on the exposed before.

We find that individuals without previous landmine exposure adjust their labor allocation more strongly in response to recent landmine events than those who had been exposed before. Specifically, among the previously unexposed, landmine events occurring in the six months prior to the planting season reduce the likelihood of working in non-jornalero jobs by 6.4 percentage points and reduce hours in these jobs by 3.6 hours per week. In contrast, we do not observe significant changes in non-jornalero or jornalero participation among individuals who were previously exposed.

To better understand which types of non-jornalero work are being reduced, we use the time-use data to disaggregate hours worked by sector. Figure 1.6 shows that the reduction is concentrated in agricultural non-jornalero work. Among previously unexposed individuals,

[7] We begin measuring past exposure in 2002 because this is the year when the Colombian government began systematically recording landmine events nationwide.

hours worked in this category decline by 112% relative to the mean in the months following a landmine event. However, this group eventually increases hours in agricultural non-jornalero work further out in time, suggesting some recovery. We do not observe meaningful changes in non-agricultural non-jornalero or jornalero jobs for either group. These patterns suggest that individuals without prior exposure initially avoid high-risk agricultural tasks, while individuals with previous exposure appear less responsive overall.

Turning to labor on own farms, we find that individuals with previous exposure reduce their work input more consistently and more substantially than the unexposed. As shown in Figure 1.7, those with prior exposure reduce the likelihood of working on their own farm for more than 0, 1, 2, and 4 hours by 7, 9, 12, and 14 percentage points, respectively, in the 6–12 months preceding the planting season. These reductions represent a 15–34% decline relative to the mean. Meanwhile, individuals without prior exposure only show significant reductions at the lower time thresholds (0 and 1 hour), and only in the 12–36 month window before the planting season.

Finally, both groups, regardless of prior exposure, increase their reliance on hired jornalero labor shortly after landmine events. Individuals unexposed in the past are 25.6 percentage points more likely to hire jornalero workers immediately after a landmine event, while those with prior exposure are 18.5 percentage points more likely to do so.

These findings suggest that prior exposure to landmine events shapes how individuals perceive and respond to subsequent risk. Those without previous exposure react more strongly to new events by avoiding agricultural non-jornalero work, particularly in the immediate aftermath. In contrast, individuals who have previously encountered landmine threats appear less responsive overall, but exhibit a greater reduction in labor on their own farms. One possible explanation for this pattern is that previously exposed individuals may have already adjusted their routines to minimize risk, leaving little room for further behavioral change—especially outside their farms. By contrast, new events represent a more salient update for those without prior exposure, prompting shifts in behavior to reduce the likelihood of landmine encounters. Regardless of exposure history, however, both groups increase their reliance on jornalero work in response to recent events. Overall, these results underscore the nuanced ways in which past experiences with conflict-related hazards influence labor decisions and highlight the importance of accounting for individuals' exposure histories when assessing the behavioral impacts of violence.

Land Ownership

We now turn to heterogeneity by land ownership, which serves as a proxy for households' wealth and borrowing capacity. We classify households into landowners, defined as those who owned at least one plot in 2010, and non-landowners, defined as those farmers who did not own any land in that year. In rural Colombia, owning land is strongly associated with greater financial security and access to credit[8]. These advantages may allow landowning households to absorb income losses and avoid high-risk work following landmine exposure. In contrast, non-landowning households may have fewer resources to buffer shocks and may be forced to continue or even increase participation in hazardous labor. In this section, we test these predictions by comparing how landmine events affect labor allocation, own-farm work, and income for landowning and non-landowning households.

Figure 1.8 shows how landmine exposure affects labor allocation differently for landowning and non-landowning individuals. We find that non-landowning individuals decrease participation in non-jornalero jobs and shift toward jornalero work, suggesting a movement toward lower-wage, higher-risk jobs. In contrast, landowning individuals also reduce participation in non-jornalero work but do not shift into jornalero jobs. This pattern is consistent with the idea that landowners are better positioned to avoid the most precarious and risky forms of labor in the aftermath of violence.

We use time-use data to further explore how landmine exposure affects the intensity and

[8]Table A.10 presents some descriptive statistics by land ownership. In our sample, landowning households were more likely to have a credit, either formal or informal, in 2010 than non-landowning households. This difference becomes larger for formal credits.

type of work across land ownership groups. Figure 1.9 shows that non-landowning individuals significantly increase hours worked in both jornalero and agricultural non-jornalero jobs, while reducing time in more stable non-agricultural non-jornalero jobs. Specifically, they increase hours worked in agricultural non-jornalero jobs by 133% (4.7 hours) and in jornalero jobs by 113% (6.6 hours), while decreasing non-agricultural non-jornalero work by 65% (4.1 hours). In contrast, landowning individuals reduce hours in agricultural non-jornalero jobs by 57% (2 hours) and show only a modest increase in jornalero work (64%, or 3.5 hours).

These differences are also reflected in work on households' own farms[9]. Figure 1.10 shows that non-landowning individuals increase both their own labor input and the likelihood of hiring jornalero labor on their farms, whereas landowners show smaller changes. More specifically, we find that non-landowners increase the probability of working more than 0 hours and hiring jornalero laborers on own farm by 50% (22.8 percentage point) and 137% (46 percentage point) relative to the mean, when exposed to landmine events before the survey interview. Meanwhile, landowners decrease the probability of working more than 0 hours on own farm by 16% (3.2 percentage point) due to landmine events in 0-6 months before the planting season. However, they also increase the likelihood of hiring jornalero by 48% (16.2 percentage point) when exposed to landmine events before the survey interview. This increase in own-farm activity among non-landowners may reflect an attempt to offset income losses from reduced non-agricultural employment or to intensify production in response to rising risk. We also find weak evidence that non-landowning households expand the area under mixed cultivation, plots growing both perennial and seasonal crops, following landmine exposure (Table A.14).

These differences are also reflected in work on households' own farms. Figure 1.10 shows that non-landowning individuals increase both their own labor input and the likelihood of hiring jornalero workers, whereas landowners show smaller changes. Specifically, among non-landowners, landmine exposure before the survey interview increases the probability

[9] Non-landowning households access land primarily through rental or sharecropping arrangements. On average, they cultivate 1.2 hectares, compared to 2.6 hectares among landowning households (Table A.10).

of working more than 0 hours on their own farm by 50% (22.8 percentage points) and of hiring jornalero laborers by 137% (46 percentage points), relative to the mean. In contrast, landowners reduce the probability of working more than 0 hours on their own farms by 16% (3.2 percentage points) following landmine events in the 0–6 months prior to the planting season. However, even among landowners, landmine exposure before the survey is associated with a 48% (16.2 percentage point) increase in the likelihood of hiring jornalero labor.

This increase in own-farm activity among non-landowning households may reflect an attempt to offset income losses from reduced non-agricultural employment or to intensify production under heightened risk. Consistent with the latter explanation, we find weak evidence that these households also expand the area under mixed cultivation—plots used to grow both perennial and seasonal crops, following landmine exposure (Table A.14).

We next examine whether the shift in labor allocation translates into changes in income sources. Figure 1.11 shows that non-landowning individuals experience a reallocation of income consistent with their increased engagement in agricultural labor. Following landmine exposure, their income from jornalero work increases by 49%, while income from non-jornalero work decreases by 47%. As a result, their overall off-farm income remains relatively stable. In contrast, landowning individuals experience an overall decline in off-farm income, driven by a 33% reduction in non-jornalero income, and no corresponding increase in jornalero earnings. This suggests that landowners respond to landmine exposure by reducing engagement in off-farm labor without substituting into higher-risk work.

While these patterns broadly mirror the labor allocation results, we cannot directly infer whether income changes are a cause or consequence of labor reallocation. Our data do not allow us to observe the sequence of decisions or underlying preferences. Nonetheless, the results suggest that landmine exposure may constrain economic options more acutely for non-landowning households, who respond by shifting toward more hazardous agricultural work.

Taken together, these findings suggest that landmine exposure interacts with pre-existing differences in household resources to shape how families respond to risk. Landowning households appear better positioned to avoid hazardous work, likely because they have greater access to resources that allow them to absorb short-term income shocks. In contrast, non-landowning households respond to the same risks by intensifying agricultural labor, both on and off their own farms—despite the heightened exposure to danger. These households also show signs of modifying production strategies, potentially to maintain income under constrained conditions. While we cannot fully disentangle whether these shifts are driven by necessity or changing preferences, the patterns are consistent with a scenario in which liquidity constraints and limited employment alternatives force more vulnerable households to accept greater risk in the wake of conflict-related violence.

1.5.2 Effects on healthcare access

We now turn to how landmine exposure affects access to healthcare services. In rural Colombia, formal medical care is typically accessed through clinics located in municipal centers, requiring considerable travel.[10] Landmine contamination may restrict this mobility, either by increasing perceived danger during travel or by physically blocking access routes. To examine these effects, we analyze changes in preventive healthcare utilization among adults and children, focusing on both formal medical services and alternative medicine. We also explore whether these effects differ by land ownership to better understand the channels through which landmine exposure shapes health-seeking behavior.

Table 1.7 presents the effects of landmine exposure on adults' use of formal and alternative medical care[11]. We find that recent exposure to landmines significantly reduces the likelihood of seeking formal preventive care. Specifically, adults exposed to a landmine event just before the survey are 12% (8.2 percentage points) less likely to visit a formal medical provider without being sick, relative to the mean. This effect is particularly strong for dentists

[10] According to ELCA community survey, only 9% of surveyed villages had a health center in 2010, and in 86% of them, patients with serious illnesses were taken to medical centers in the municipal capital.

[11] Since we do not know the number of visits and the dates when they took place, it is possible that some of these visits occurred before the landmine events.

(38%), general practitioners or specialists (15%), and optometrists (45%).

Table 1.7: Effect of landmine events on adults' healthcare seeking

	Sought medical assistance for preventative care in the past 12 months					
	(1) Any non alternative	(2) GP/ specialist	(3) Dentist	(4) Optometrist	(5) Family planning	(6) Alternative medicine
If at least one landmine event in period...						
Before survey	-0.082*	-0.090*	-0.155***	-0.059**	-0.020	0.020
	(0.048)	(0.051)	(0.051)	(0.026)	(0.022)	(0.017)
(0-6] months	-0.037	-0.043*	-0.050*	-0.022	0.019	0.018**
	(0.030)	(0.026)	(0.026)	(0.018)	(0.014)	(0.008)
(6-12] months	0.043	0.043	0.091**	0.008	0.018	0.005
	(0.045)	(0.045)	(0.038)	(0.023)	(0.016)	(0.014)
(12-36] months	0.010	0.046	0.050**	-0.007	0.005	0.029***
	(0.029)	(0.030)	(0.025)	(0.015)	(0.019)	(0.011)
Dep Var Mean	0.658	0.605	0.400	0.132	0.091	0.030
# Individuals	5,484	5,484	5,484	5,484	5,484	5,484
# Clusters	224	224	224	224	224	224
Observations	16,452	16,452	16,452	16,452	16,452	16,452

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Sample includes household heads and their spouses when they have one. All specifications include individual and year fixed effects and municipality characteristics at baseline interacted with year FE. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

While we observe some recovery in preventive dental care over time, the same is not true for other services. Adults exposed in the 6–12 and 12–36 months prior to the planting season are 23% and 13% (9.1 and 5 percentage points) more likely, respectively, to have visited a dentist, suggesting partial resumption of care. However, we do not observe a similar rebound in visits to general practitioners or optometrists.

At the same time, landmine exposure increases the use of alternative medicine, which is often provided by traditional healers and located closer to rural communities. Adults exposed in the 0–6 and 12–36 month windows are 60% and 97% (1.8 and 2.9 percentage points) more likely to seek alternative care. These patterns suggest a substitution away from formal healthcare toward more accessible, informal services in response to mobility constraints.

We now turn to healthcare access among children. Table 1.8 presents the effects of landmine exposure on children aged 0–9 in 2010—the only cohort tracked over time in the panel. The analysis uses an unbalanced panel, including children who appear in at least two consecutive rounds of the household survey. We find that landmine exposure reduces children's use of preventive dental care. Specifically, children who experienced a landmine event before the survey are 28% (16.3 percentage points) less likely to visit a dentist, relative to the mean. We do not observe statistically significant changes in visits to general practitioners or other types of formal care.

Table 1.8: Effect of landmine events on children's healthcare seeking

	Sought medical assitance for preventative care in the past 12 months					
	(1) Any non alternative	(2) GP/ specialist	(3) Dentist	(4) Optometrist	(5) Pediatrician	(6) Alternative medicine
If at least one landmine event in period...						
Before survey	-0.067	-0.106	-0.163**	-0.090	-0.057	-0.002
	(0.085)	(0.080)	(0.075)	(0.055)	(0.076)	(0.019)
(0-6] months	0.015	-0.043	-0.047	-0.002	0.015	-0.007
	(0.028)	(0.034)	(0.041)	(0.021)	(0.026)	(0.005)
(6-12] months	-0.002	0.009	0.053	0.109**	0.037	0.021***
	(0.053)	(0.053)	(0.063)	(0.047)	(0.028)	(0.006)
(12-36] months	0.029	0.044	-0.024	0.013	-0.015	0.018
	(0.038)	(0.037)	(0.044)	(0.028)	(0.028)	(0.011)
Dep Var Mean	0.836	0.759	0.583	0.133	0.231	0.011
# Individuals	2,813	2,813	2,813	2,813	2,813	2,813
# Clusters	224	224	224	224	224	224
Observations	8,170	8,170	8,170	8,170	8,170	8,170

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Sample includes children who were 0 to 9 years old in 2010 and were followed in at least two consecutive rounds. All specifications include individual and year fixed effects and municipality characteristics at baseline interacted with year FE. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

These results suggest that landmine exposure may disrupt children's access to routine health services, although the effects appear to be more limited in scope compared to adults. This difference may reflect differences in health needs, parental decision-making, or the structure of child healthcare provision in rural areas.

To further probe the role of liquidity constraints, we examine whether healthcare responses to landmine exposure also differ by land ownership. This approach mirrors our earlier analysis of labor allocation, where we found that non-landowning households, likely more financially constrained, responded to landmine exposure by increasing participation in risky agricultural work. If similar constraints influence healthcare decisions, we would expect non-landowning households to reduce formal care more sharply or switch to lower-cost alternatives.

Figure 1.12 presents the heterogeneous effects of landmine exposure on adults' healthcare-seeking behavior by land ownership status. Consistent with the idea that non-landowning households face tighter liquidity constraints, we find that non-landowning adults are significantly less likely to seek formal medical care following recent landmine exposure. Specifically, they are 31% (20.7 percentage point) less likely to visit a formal medical provider if exposed just before the survey, relative to the mean. A similar, though smaller, decline is observed for exposure during the six months prior to the planting season.

In contrast, landowning adults do not significantly reduce their use of formal care, suggesting they are better able to maintain access despite potential income shocks or mobility risks. However, one exception is dental care: both landowners and non-landowners reduce dental visits following recent landmine exposure, possibly due to the lower perceived urgency of this type of care or the longer travel distances involved.

We also find that non-landowning adults are more likely to substitute toward alternative medicine. They are 110% (3.3 percentage point) more likely to seek traditional care when exposed just before the survey, reinforcing the idea that these households shift toward lower-cost, more accessible providers when formal care becomes harder to reach or afford.

Figure 1.13 shows how landmine exposure affects children's healthcare use by land ownership status. In contrast to the adult results, we find that children in both landowning and non-landowning households do not significantly change their overall use of formal preventive care in response to landmine exposure. However, we do find that children in landowning households are less likely to visit general practitioners and specialists, while no such change is observed for children in non-landowning households. Finally, children in landowning households are more likely to use alternative medicine following exposure. One possible explanation is that baseline usage of medical care among non-landowning households is already low, leaving less room for further behavioral adjustment. In contrast, landowning households may have more flexibility to shift away from formal care when risk increases.

These results suggest that landmine exposure alters healthcare access in ways that reflect financial constraints. Adults in non-landowning households reduce formal care and turn to alternative medicine, consistent with the idea that they cannot afford the time or cost of seeking care in more distant facilities. In contrast, adults in landowning households maintain their own access to formal care, suggesting they are better able to absorb shocks without sacrificing access to essential services. Interestingly, landowners are more likely to reduce formal healthcare and increase alternative medicine for children, perhaps reflecting a heightened concern with mobility risks for children. Taken together, the patterns underscore how economic resources shape households' ability to mitigate risk.

1.6 Conclusion

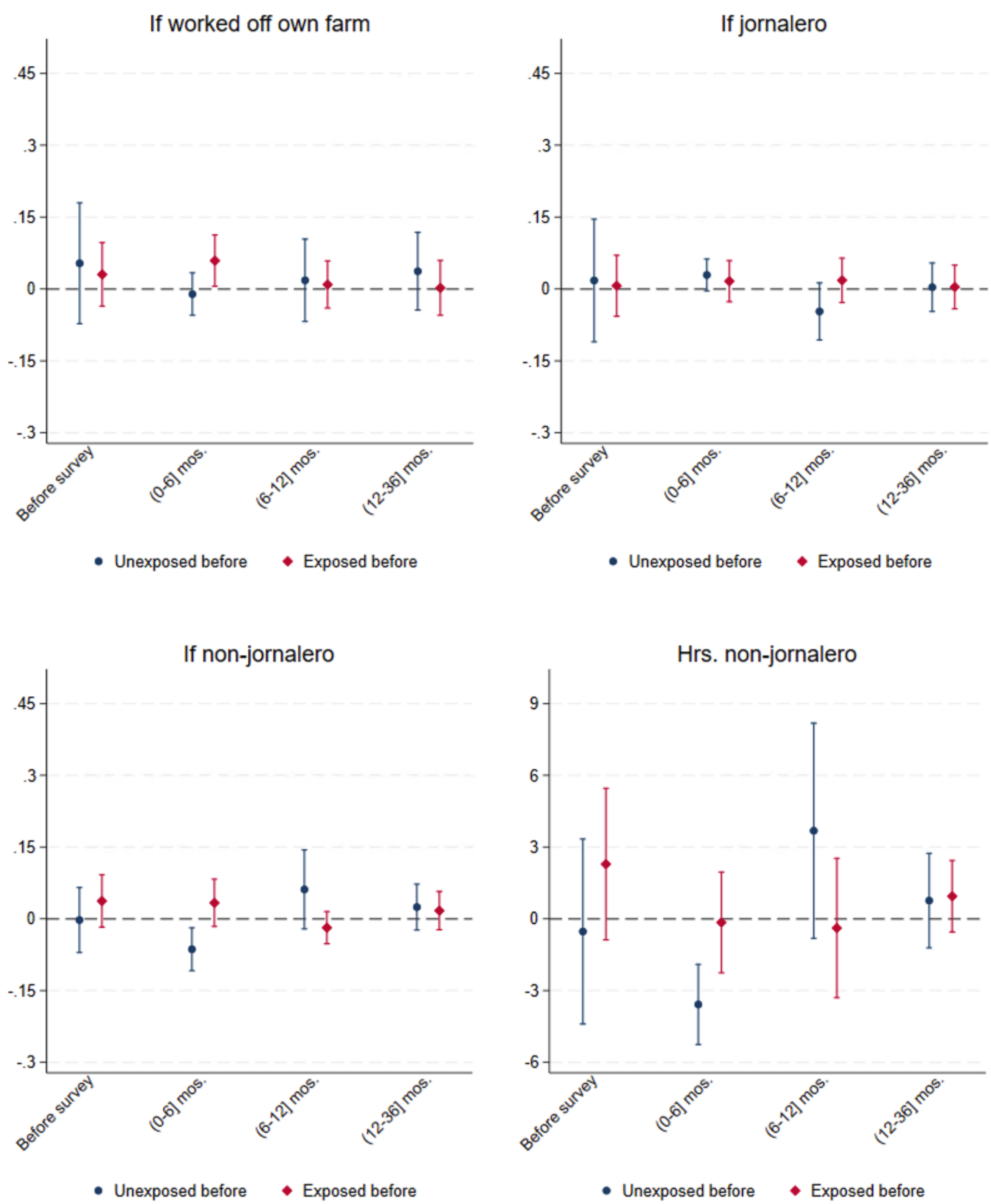
This chapter investigates how exposure to landmine contamination affects rural households' labor allocation and healthcare access in Colombia. While landmine contamination is often viewed primarily as a security concern, we show that it also has far-reaching consequences for economic behavior and use of essential services. Specifically, we estimate how recent landmine events influence individuals' decisions about where and how to work, as well as their ability to access preventive healthcare services.

Our findings reveal that landmine exposure reduces participation in more stable forms of agricultural labor and increases reliance on jornalero work, an occupation that is lower-paid, more precarious, and potentially riskier. These shifts are not uniform: individuals without prior landmine exposure react more strongly to new events, while those with prior exposure appear less responsive, possibly due to adaptation. Similarly, landowning households are better able to reduce riskier work with higher potential for landmine exposure, while non-landowning households respond by intensifying labor on others' farms and hiring more workers for their own plots, potentially due to the lack of ability to absorb negative income shocks unlike their landowning counterparts.

We also find that landmine exposure disrupts access to preventive healthcare. Adults reduce visits to formal providers and increase reliance on alternative medicine, especially in non-landowning households. Children's access is less affected, possibly because households prioritize their health even under constraints. These patterns suggest that landmine exposure shapes not only how people work but also how they care for themselves and their families by constraining movement.

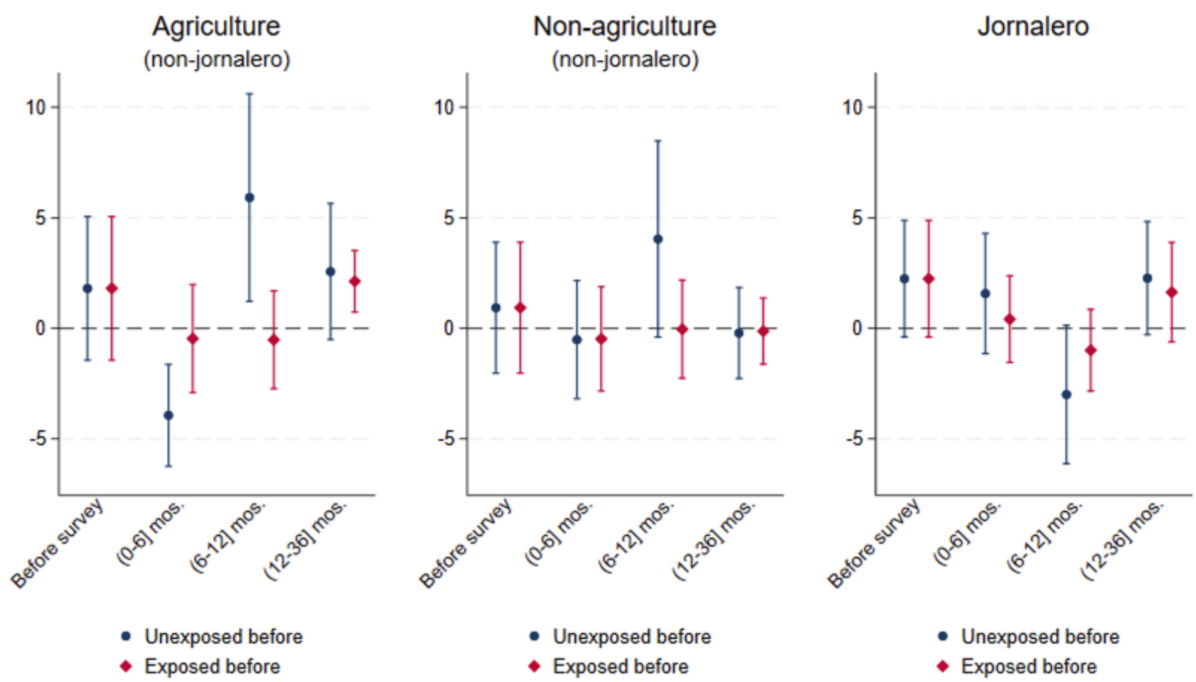
Together, our findings reveal that landmine contamination can impose substantial economic and health costs on civilians, beyond its immediate physical harm. Moreover, these burdens fall unevenly across households along the wealth line. Our study also points to several directions for future research. While we document behavioral responses to landmine exposure, we do not observe households' underlying preferences or perceptions of risk. Future work that incorporates richer data on rural labor markets and household decision-making—especially around job preferences and perceived constraints—could offer deeper insights into the mechanisms driving these responses.

Figure 1.5: Landmine effects on labor allocation by previous exposure



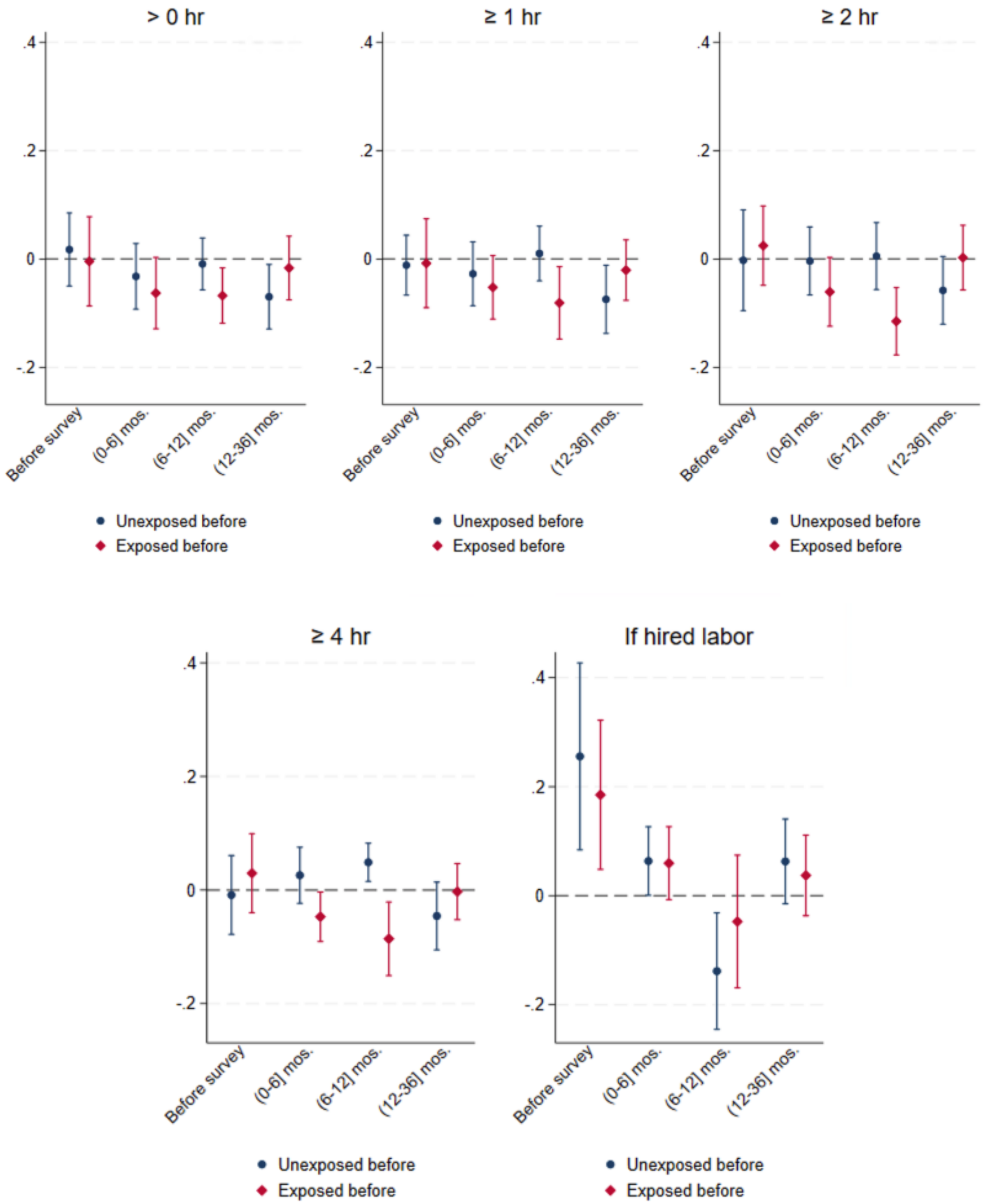
Note: Lines represent 90% confidence intervals. Exposed coefficients are the linear combination of *If events since 2002 until 36 mos.* × *If events in time window*. Table A.7 presents the full regression results.

Figure 1.6: Landmine effects on hours of labor allocated by previous exposure



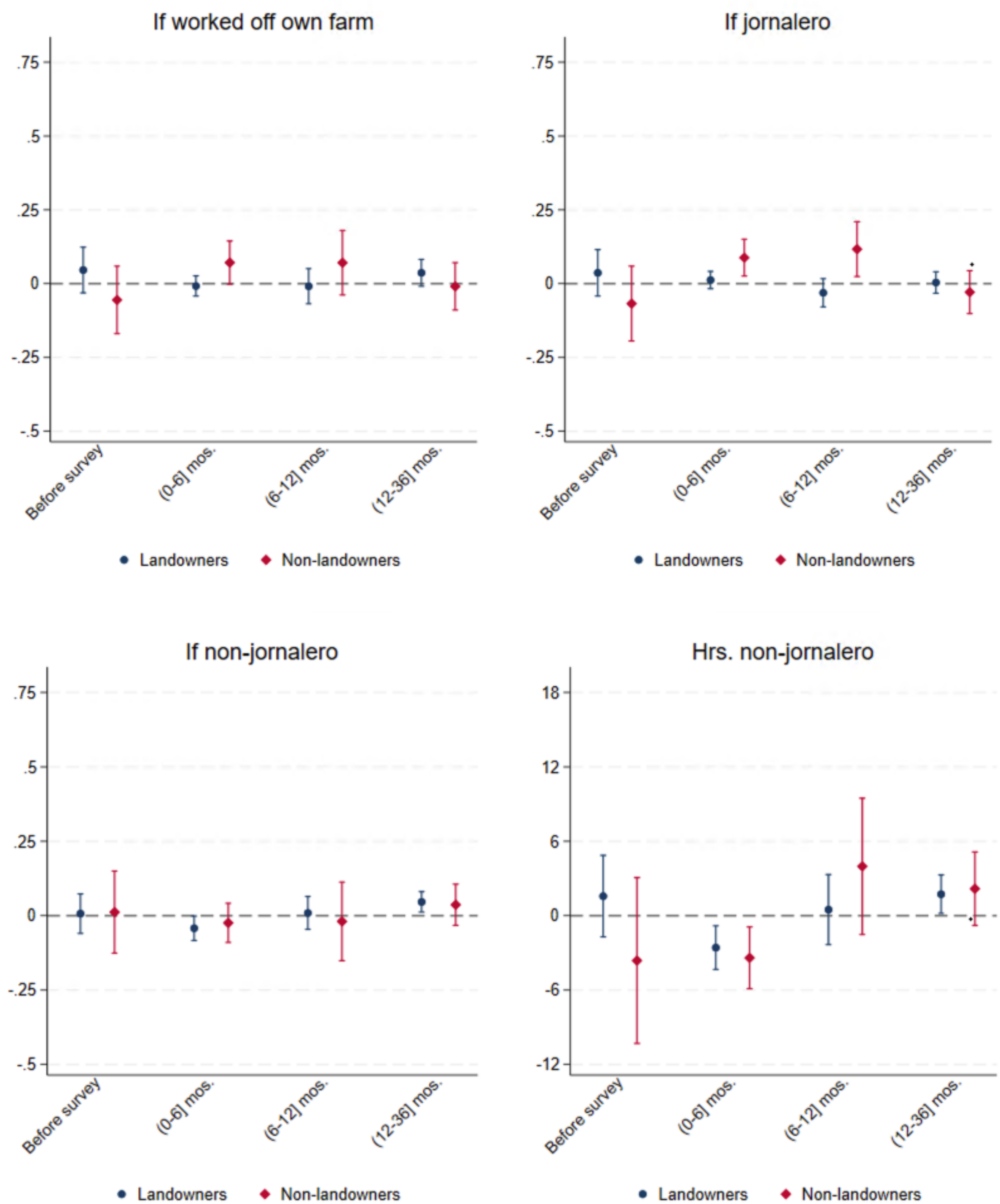
Note: Lines represent 90% confidence intervals. Exposed coefficients are the linear combination of *If events since 2002 until 36 mos.* × *If events in time window*. Table A.8 presents the full regression results.

Figure 1.7: Landmine effects on own farm labor by previous exposure



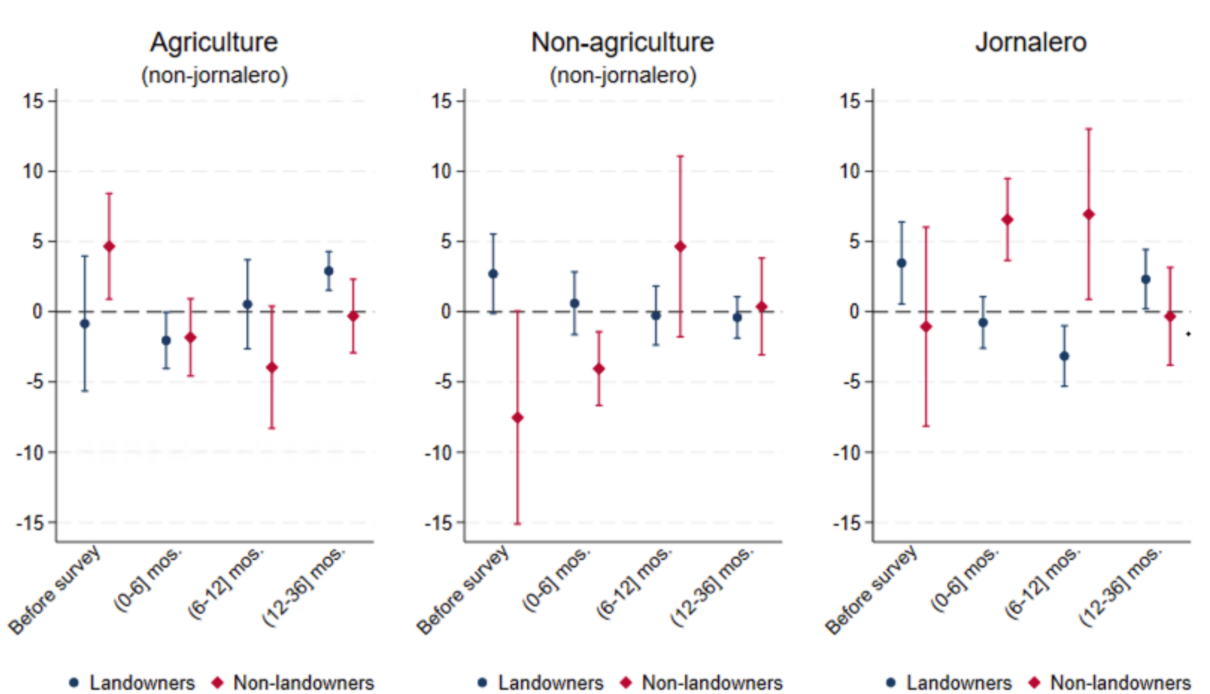
Note: Lines represent 90% confidence intervals. Exposed coefficients are the linear combination of *If events since 2002 until 36 mos.* × *If events in time window.* Table A.9 presents the full regression results.

Figure 1.8: Landmine effects on labor allocation by land ownership



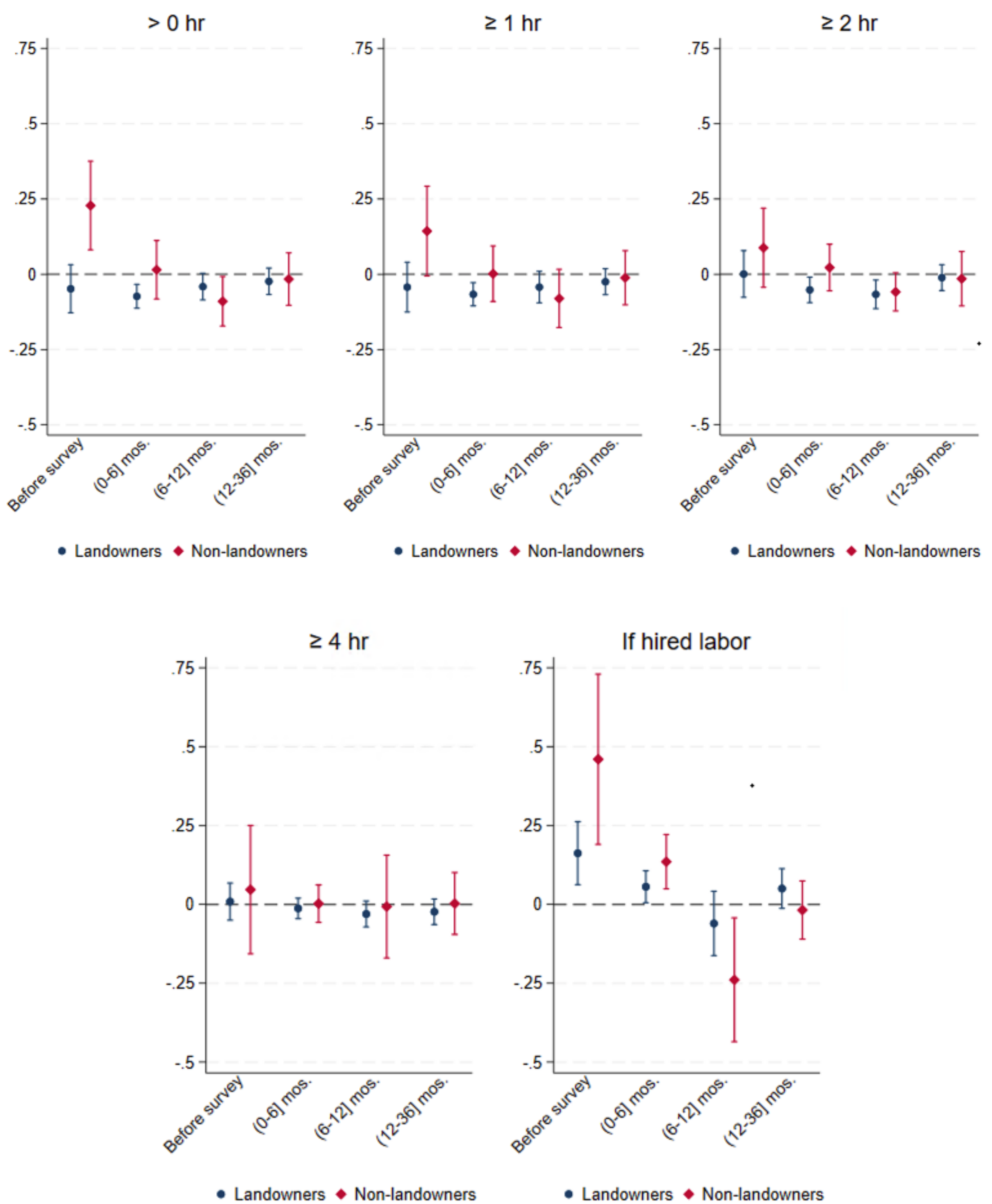
Note: Lines represent 90% confidence intervals. Lines represent 90% confidence intervals. Non-landowners point estimate corresponds to the linear combination *Owner* + *Owner* × *If event in time window*. Table A.11 presents the full regression results.

Figure 1.9: Landmine effects on time spend on agricultural and non-agricultural work by land ownership



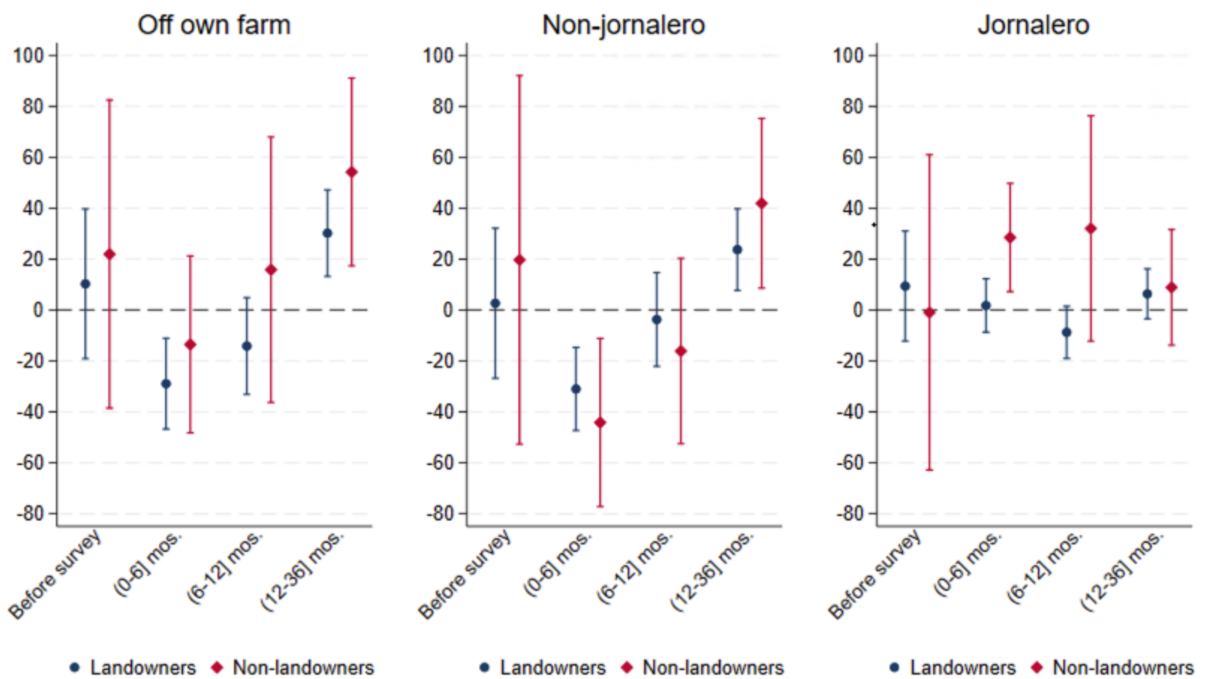
Note: Lines represent 90% confidence intervals. Sample only contains 2013 and 2016 rounds. Non-landowners point estimate corresponds to the linear combination *Owner* + *Owner* × *If event in time window*. Table A.12 presents the full regression results.

Figure 1.10: Landmine effects on labor input on own farm by land ownership



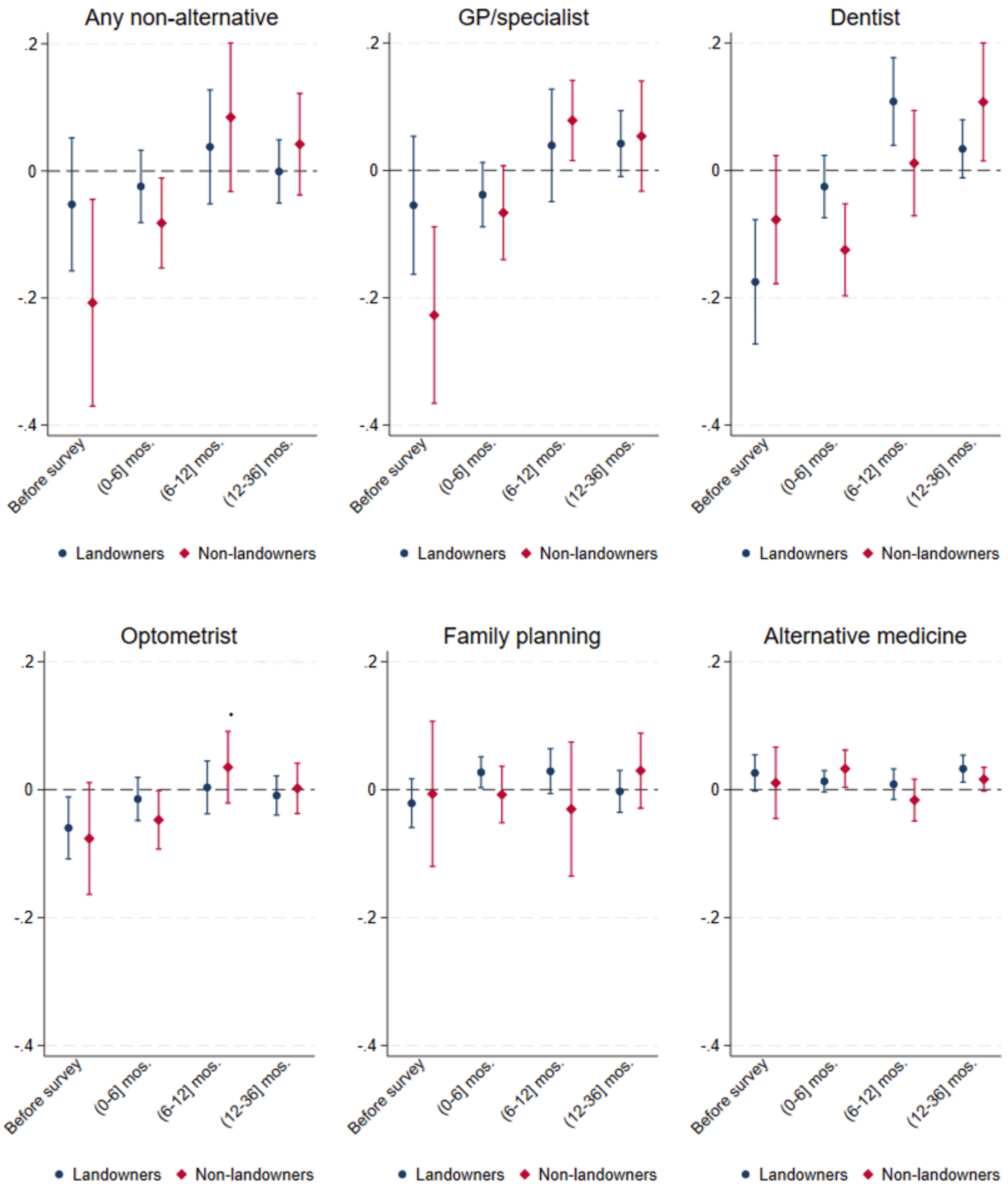
Note: Lines represent 90% confidence intervals. Individual sample is used in first four figures and household sample used in fifth figure. Non-landowners point estimate corresponds to the linear combination *Owner* + *Owner* × *If events in time window*. Table A.13 presents the full regression results.

Figure 1.11: Landmine effects on income by land ownership



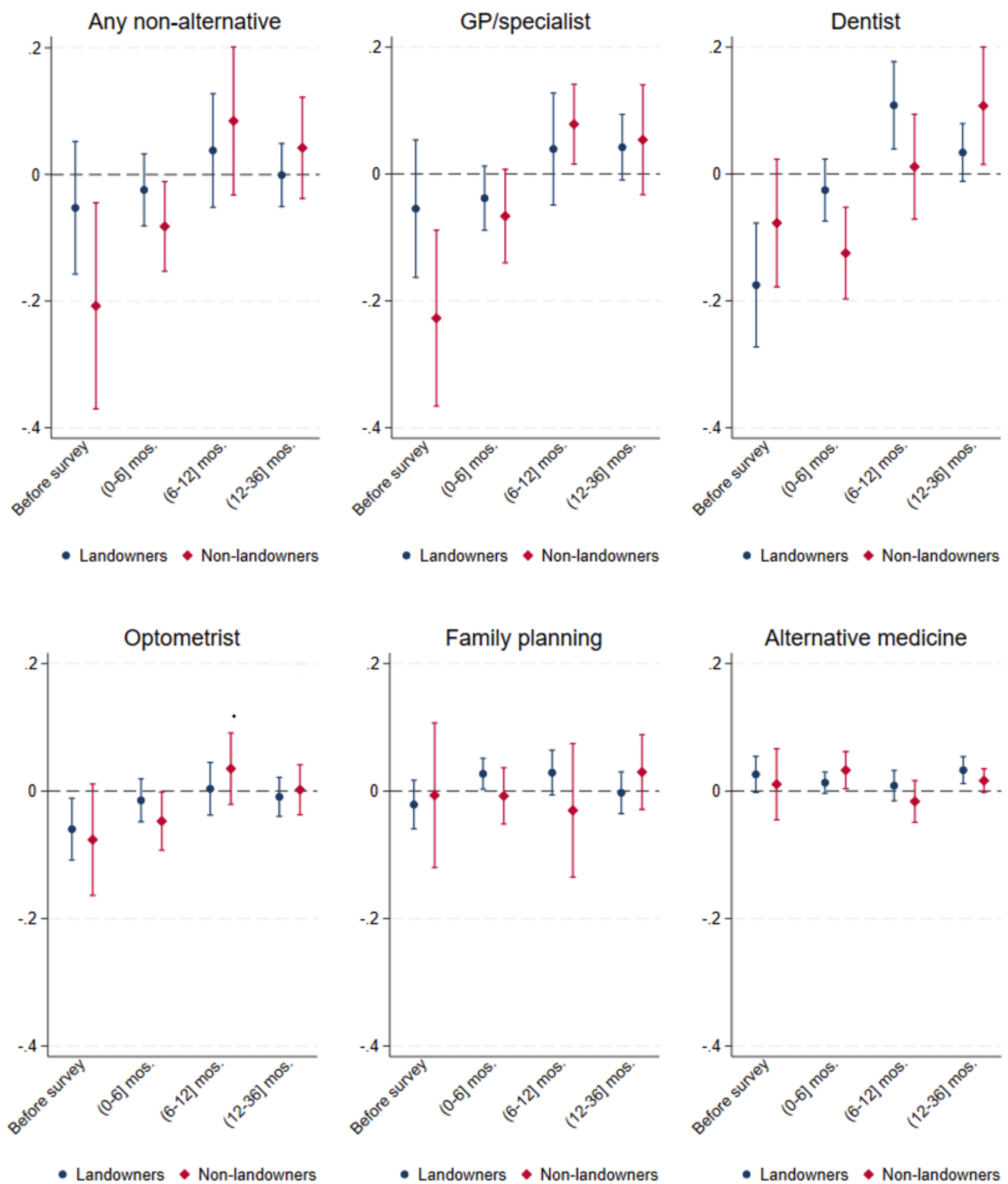
Note: Lines represent 90% confidence intervals. All monetary values expressed in thousands of COP. Non-landowners point estimate corresponds to the linear combination *If events in period* + *Non-owner* × *If events in period* Table A.15 presents the full regression results.

Figure 1.12: Landmine effects on adults' healthcare seeking by land ownership



Note: Lines represent 90% confidence intervals. Non-landowners point estimate corresponds to the linear combination *Owner* + *Owner* × *If events in time window*. Table A.16 presents the full regression results.

Figure 1.13: Landmine effects on children's healthcare seeking by land ownership



Note: Lines represent 90% confidence intervals. Non-landowners point estimate corresponds to the linear combination *Owner* + *Owner* × *If events in time window*. Table A.17 presents the full regression results.

Chapter 2

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

2.1 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 (Cohen et al., 2013).[2]

[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).

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 presence in Colombia. This dataset is compiled from diverse sources, including newspaper articles, the army's organizational charts,[4] historical records, congressional reports, and leg-

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

islative 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 (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2022). Thus, the third specification uses the de Chaisemartin and D'Haultfœuille (dCdH) estimation to account for the staggered 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 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. Furthermore, this increase appears to be driven by bases with more drafted soldiers rather than those with highly trained, well-paid, professional soldiers. While we find a statistically significant rise in registered cases of sexual violence, we observe a corresponding increase in cases resulting in formal charges. 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 evaluate alternative explanations beyond the presence of government soldiers and find no evidence that the observed increase is attributable to changes in security conditions, demographic shifts, or reporting behavior. While some of the violence may have been committed by right-wing paramilitary groups, particularly given their documented collaboration with certain army units, we note that the Colombian Army was far larger than any paramilitary group, and that military bases were the focal points of the observed effects. Taken together, these findings suggest that the increase in sexual violence is most plausibly explained by the presence of government soldiers.

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.

This chapter 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 chapter 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 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

[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.

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 chapter is structured as follows: Section 2.2 describes the military expansion in Colombia that began in 2000, leading to the establishment of numerous new military bases. Section 2.3 details our data sources and construction method, and describes the municipalities in our sample. Section 2.4 discusses our empirical strategy for identifying the causal effects of military base presence. Section 2.5 presents our results and discusses their robustness. Section 2.6 investigates potential reasons why soldiers have committed sexual violence using economic theories, empirical studies, and anecdotal accounts. Finally, Section 2.7 concludes with a summary of our findings and their implications for future research.

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

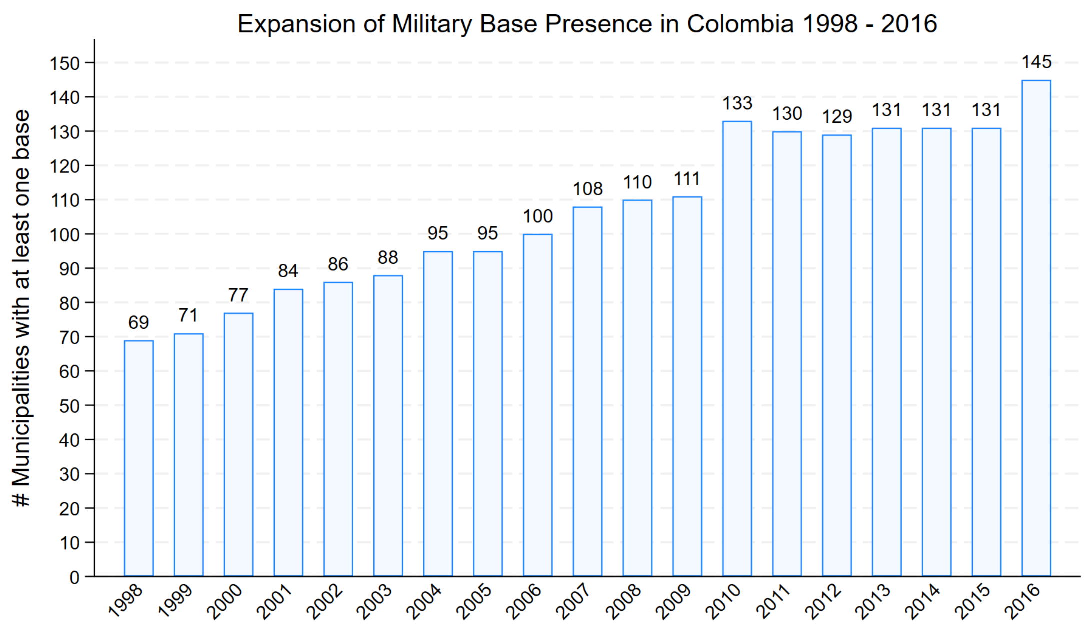
In the early 2000s, the Colombian government undertook a major military expansion aimed at reclaiming territorial control from insurgency groups and reestablishing state authority in conflict-affected regions. This initiative began under President Andrés Pastrana and intensified significantly under President Álvaro Uribe, whose Democratic Security Policy prioritized the deployment of state forces to areas with limited government presence. The strategy followed a period of intensified confrontations with numerous guerilla groups in the late 1990s and early 2000s. It signaled a turning point in Colombia's internal conflict, marked by a shift toward a more proactive and geographically expansive military posture.

To implement this policy, the Colombian Army expanded its reach through an unprecedented increase in bases and personnel.[6] Between 1998 and 2016, the number of munic-

[6] To support this expansion, the Colombian government also received material and financial assistance from the United States, particularly under the framework of Plan Colombia. While U.S. support provided critical equipment, training, and funding, the planning and execution of base expansion were largely directed

ipalities hosting military bases more than doubled, growing from 69 to 145 (Figure 2.1). Furthermore, this expansion spanned all regions of the country and was not confined to any single division of the Army (Figure B.2).

Figure 2.1: Expansion of the National Army between 1998 and 2016



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

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.

by Colombia's internal security and counterinsurgency strategies. Figure B.1 plots U.S. military aid through the State and Defense Departments, showing a pronounced increase beginning in 2000.

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 *drafted 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 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 drafted 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,

[7] Figure B.3 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 some exceptions. Female citizens may participate voluntarily (Suárez, 2023). This means that the great majority of soldiers at the military bases considered in this analysis are young men.

receive substantial and periodic training and are provided with significant compensation and health benefits, serving for up to 20 years (Human Rights Watch, 1993). 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 within their jurisdiction for extended periods.

Drafted and professional soldiers. As described above, there is a substantial difference between the drafted 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 drafted 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, drafted 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, drafted soldiers rest for two weeks, during which they are allowed to leave the military base. Once they return, soldiers are deployed to the field for a period ranging from 12 to 14 months. During this time, drafted soldiers follow the same operational cycle as professional soldiers. According to current and retired army officers, military units usually assign drafted 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 drafted 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 drafted 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 drafted and professional soldiers in 2010, with a legal minimum monthly wage of US$118.06 (Datosmacro, 2022). The drafted soldier's annual compensation

[9]Table B.1 provides a comparison of compensation by soldier class.
[10]Table B.2 describes these benefits in detail.

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 drafted soldiers.

While we unfortunately do not have data on the composition of army soldiers by rank, 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 drafted soldiers (Ministry of Defense of Colombia, 2007). Clearly, the drafted 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.

2.3 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.

[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.

2.3.1 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 and/or battalion, published from January 1, 2000 to December 31, 2010. We then used Google Cloud Vision to detect text 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 B.1.

2.3.2 Outcome data

In this section, we discuss the sources of the outcome data and the construction of the outcome variables. Table B.3 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

[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.

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, 2020). 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

[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.

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

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

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

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

2.3.3 Characteristics of municipalities in the sample for analysis

Table B.4 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:

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

1. We omit the municipalities of San Andrés and Providencia, which are small islands in the Caribbean Sea. 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 and administrative sections 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.

Table 2.1 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.

Table 2.1: 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.

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 2.4.

2.4 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:

$$y_{mt} = \sum_{\substack{j=-4 \\ j\neq -1}}^{7} \mathbb{1}(t - t^*_m = j)\beta_j + \alpha_m + \eta_t + \epsilon_{mt} \tag{2.1}$$

where y_{mt} is an outcome in municipality m in two-year period t; $\mathbb{1}(t - t^*_m = j)$ is the time

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

relative to the military base introduction period t_m^*; α_m is the municipality fixed effects; η_t is the year fixed effects; and ϵ_{mt} is a time-variant unobserved term at the municipality level. The omitted group is $j = -1$, 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 2000 to 2016 into 9 two-year periods, with the first period 2000 and 2001, and the last period containing only 2016.

Estimating Equation (2.1) mitigates potential bias from time-invariant 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 geographic and time-variant economic and conflict dynamics, we include army division jurisdiction-year fixed effects, δ_{dt}, and estimate the following equation with OLS:

$$y_{mt} = \sum_{\substack{j=-4 \\ j\neq -1}}^{7} \mathbb{1}(t - t_m^* = j)\beta_j + \alpha_m + \delta_{dt} + \epsilon_{mt}. \tag{2.2}$$

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 $j = -1$, and standard errors are clustered at the municipality level.

The parameters of interest are β_j for $j \geq 0$, which capture the average effect of military base presence on the outcome in the jth 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 establish an appropriate counterfactual when treatment assignment has variation in timing (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2022). This issue stems from the possible comparison that OLS estimation make between the switchers (control to treatment) to the non-switchers (already in treatment). In our context, municipalities receive military units at different times; therefore, treatment timing is staggered. 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. 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 B.4. 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 2.1 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_j for $j < 0$, 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 2.2, 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 host only standing units with drafted soldiers, 33% host only counterinsurgency units with professional soldiers, and 5% host both types of units (Table 2.2). 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.

Table 2.2: 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.

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.

2.5 Results

This section presents the results of our statistical analysis. First, we estimate the effects of military base presence on sexual violence (Subsection 2.5.1). Next, we explore potential drivers other than government soldier presence to assess whether the observed increase in sexual violence can be plausibly attributed to the presence of government soldiers (Subsection 2.5.2). We then examine whether this rise in sexual violence leads to changes in fertility and child support disputes (Subsection 2.5.3), 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 2.5.4).

2.5.1 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.

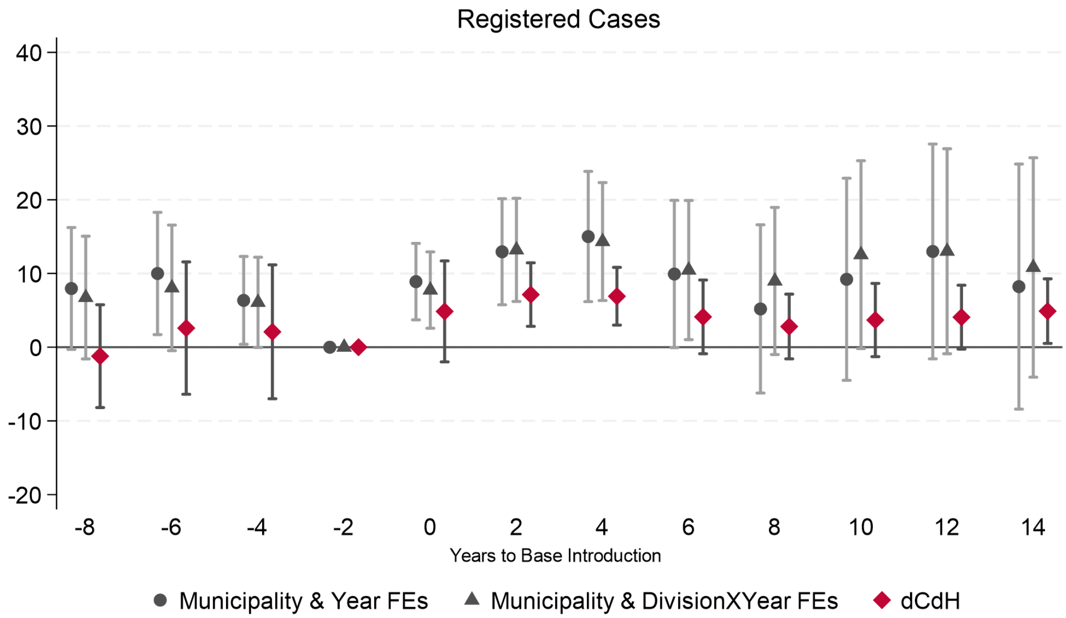
Figure 2.2 illustrates the estimated impact on the number of sexual violence cases per 100,000 inhabitants. As outlined in Section 2.3.2, *registered* refer to those acknowledged by the Office of the Attorney General's Office, while *indicted* cases involve formal accusations against suspects.

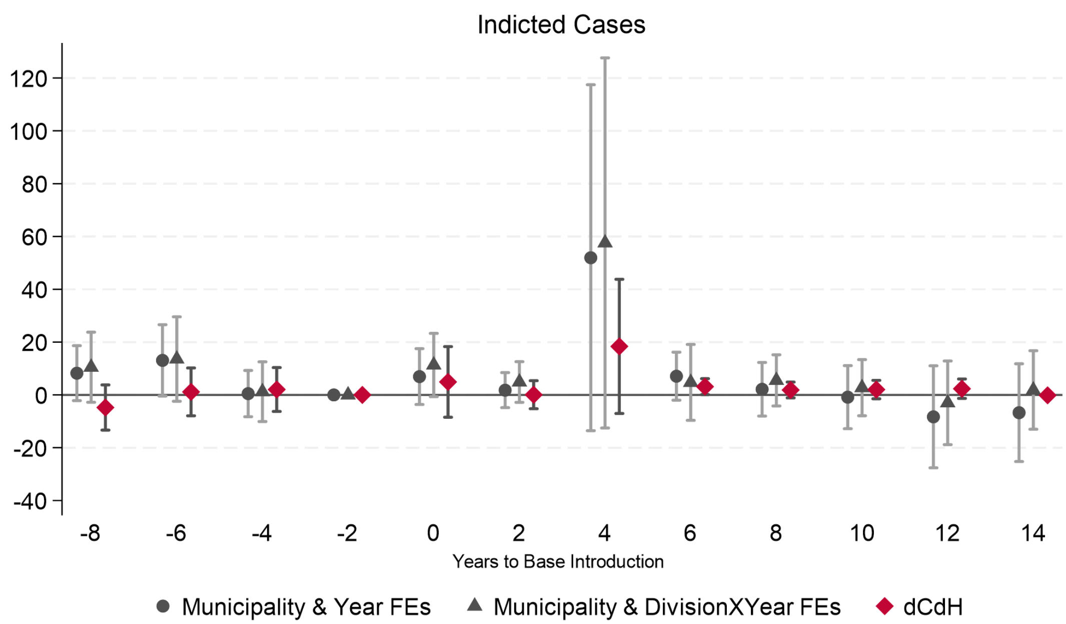
First, the dCdH estimation shows a more balanced pre-trend, especially in the two years preceding base introduction, compared to the OLS estimations, which display a positive pre-trend. This discrepancy suggests that the OLS estimates are inflated due to the negative weight issue, arising from comparisons between switchers (municipalities transitioning from no base to having one) and non-switchers (those with pre-existing bases) (de Chaisemartin and D'Haultfœuille, 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

Figure 2.2: 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 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

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. Notably, no significant change is observed for indicted cases.

Table 2.3 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'Haultfœuille, 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.

Table 2.3: Average Total Effects on Sexual Violence (dCdH)

Outcome: Number of Cases per 100,000 Inhabitants

	Registered (1)	Indicted (2)
Has Army base	16.399**	16.254
	(7.329)	(10.416)
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 calculates the *average total effect*, which is the weighted sum of the effects of all periods. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

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 2.2 and 2.4, 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 2.3 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 2.6.

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 2.4, 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 2.5 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, 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 2.4 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.

Table 2.4: Average Total Effects on Sexual Violence (Intensive Margin)
Outcome: Number of Cases per 100,000 Inhabitants

	Registered (1)	Indicted (2)
N. Army base	12.230***	6.779
	(3.299)	(5.662)
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 calculates the *average total effect*, which is the weighted sum of the effects of all periods. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

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.

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.

2.5.2 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 2.6 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 B.5. 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 2.7 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 2.8 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 reporting 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 (Ruiz, 2001; Commission for Truth, 2022b). Investigating this potential mechanism is crucial for ruling out confounding factors and ensuring accurate attribution of the observed effects.

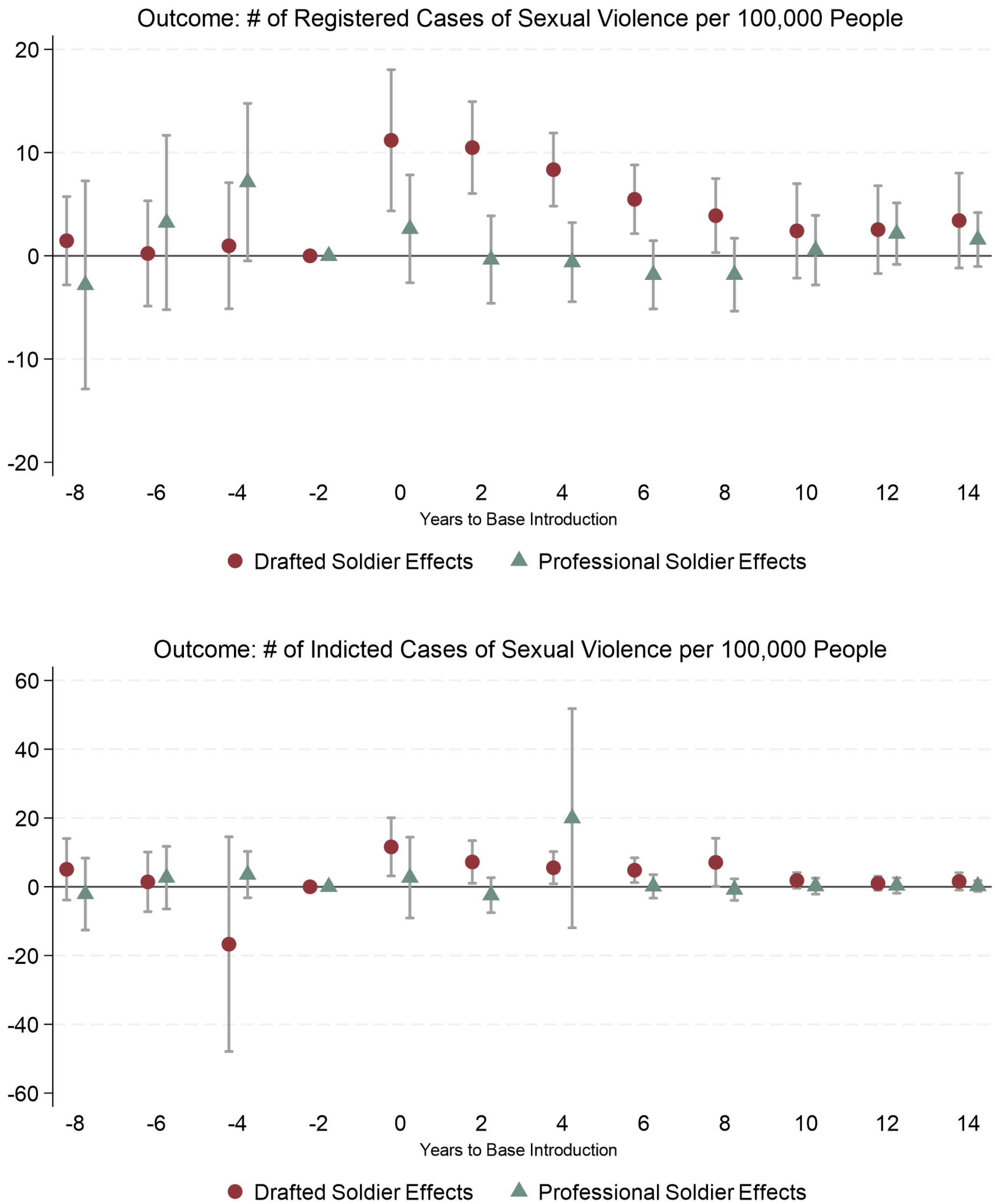
We hypothesize that the increase in sexual violence in municipalities with military bases may, at least in part, be attributed to the presence of right-wing paramilitary organizations rather than left-wing guerrilla groups. This hypothesis is grounded in historical evidence of collaboration between certain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

2.5.3 Potential consequences of increased sexual violence

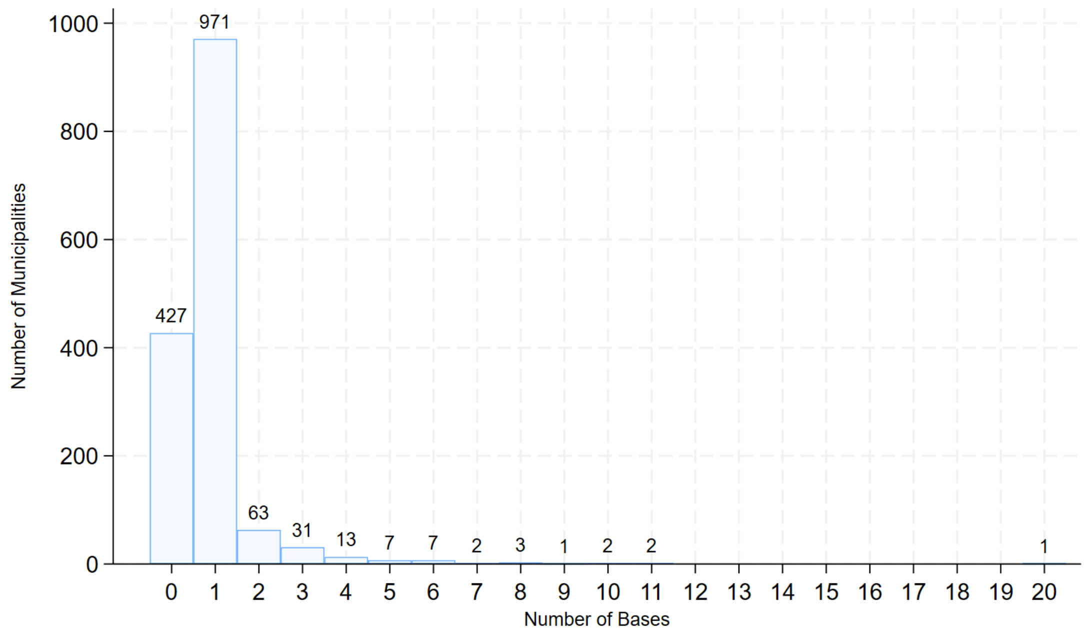
We have established that military base presence leads to an increase in sexual violence. In this section, we investigate whether this increase translates into unintended consequences on fertility and child support disputes. Theoretically, increased sexual violence could result in unintended pregnancies,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

Figure 2.3: Effects on Sexual Violence by Base Type



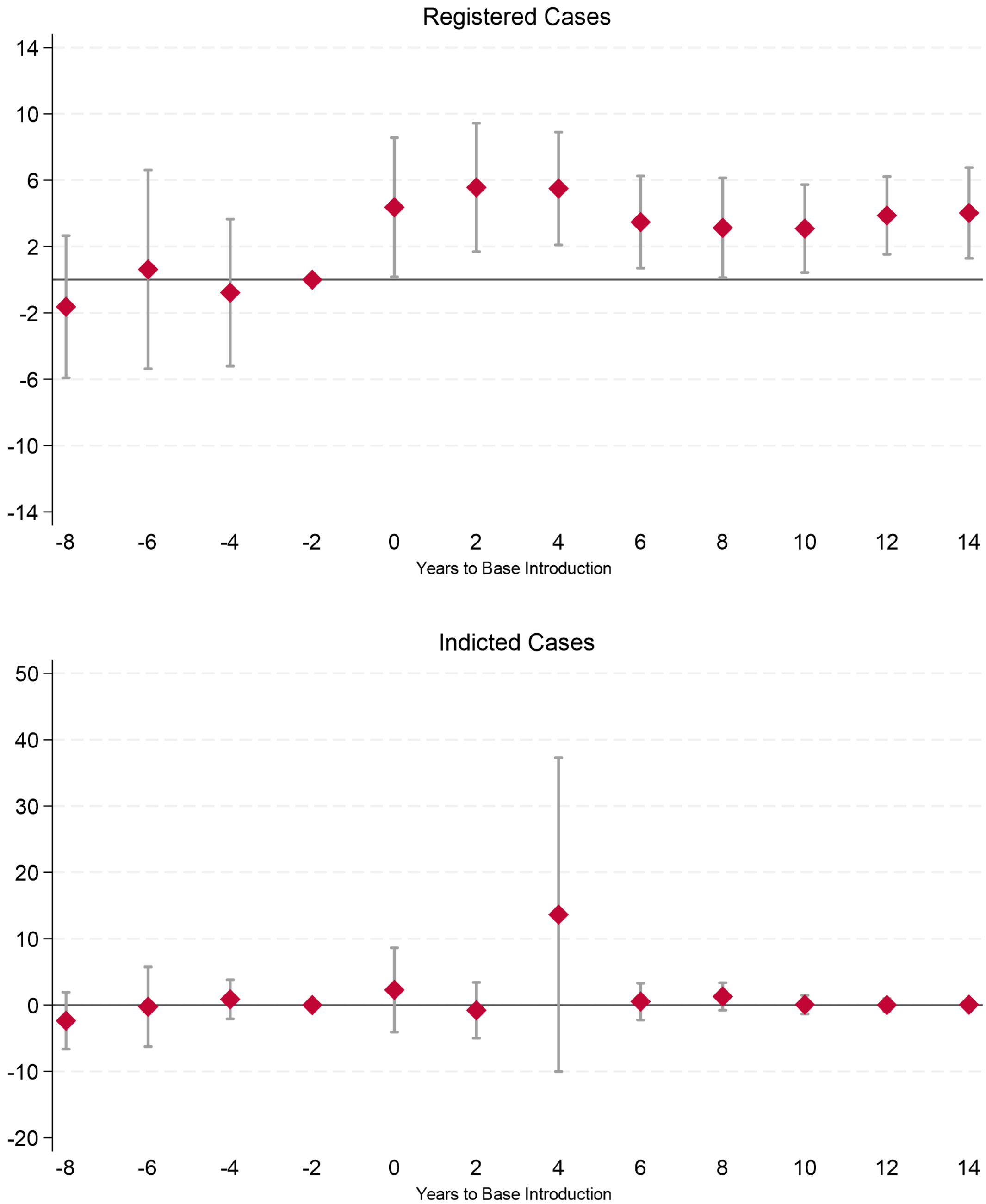
Note: These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). *Drafted soldier effects* refer to the estimated coefficients on an indicator variable 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 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 2.4: Distribution of Observations across the Number of Bases



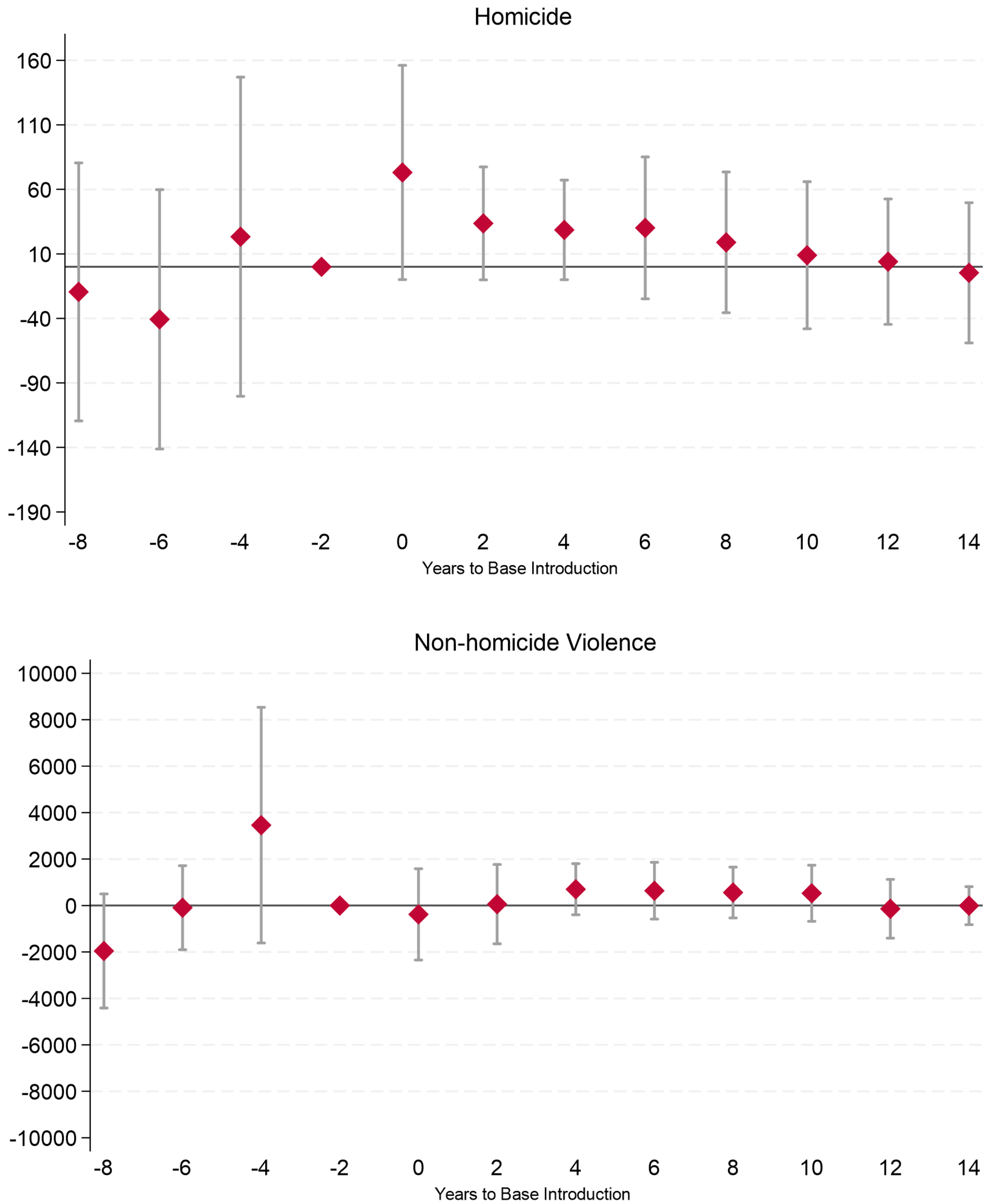
Note: This graph plots the distribution of municipalities across the number of military bases. There are 1,530 municipality-year group observations.

Figure 2.5: Intensive-Margin Effects on Sexual Violence
Outcome: Number of Cases per 100,000 Inhabitants



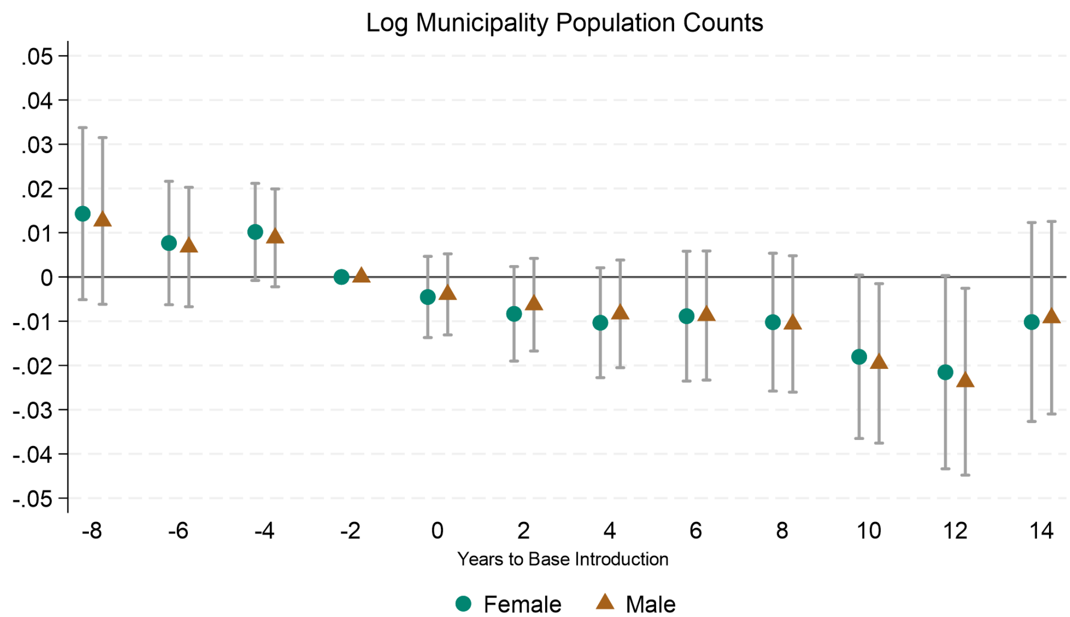
Note: These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the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

Figure 2.6: Effects on Homicides and Non-homicide Crimes
Outcome: Number of Cases per 100,000 Inhabitants



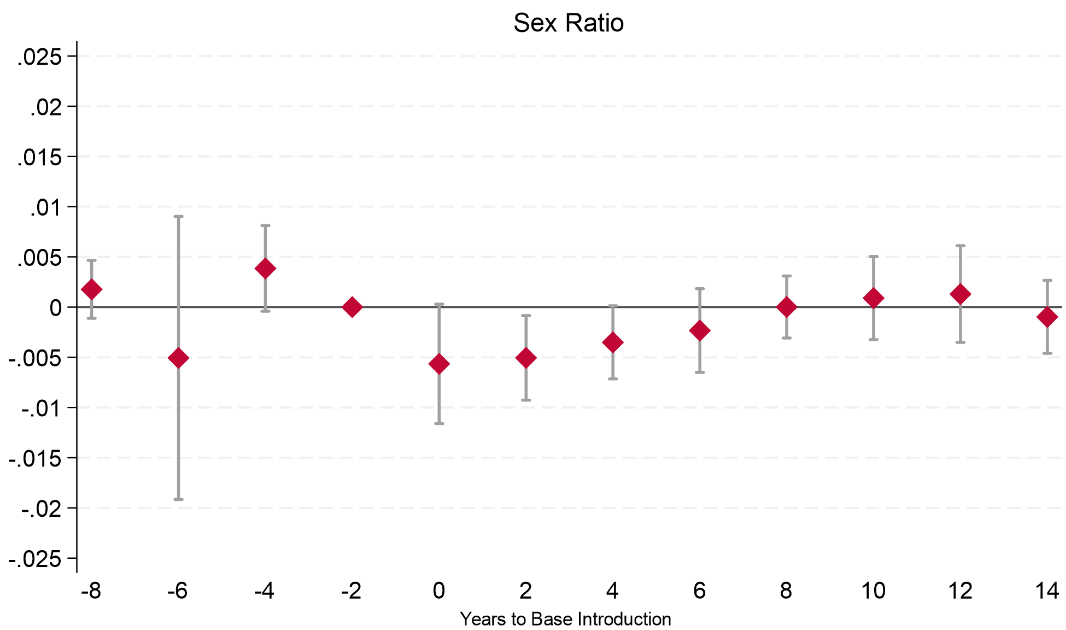
Note: These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the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

Figure 2.7: 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 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

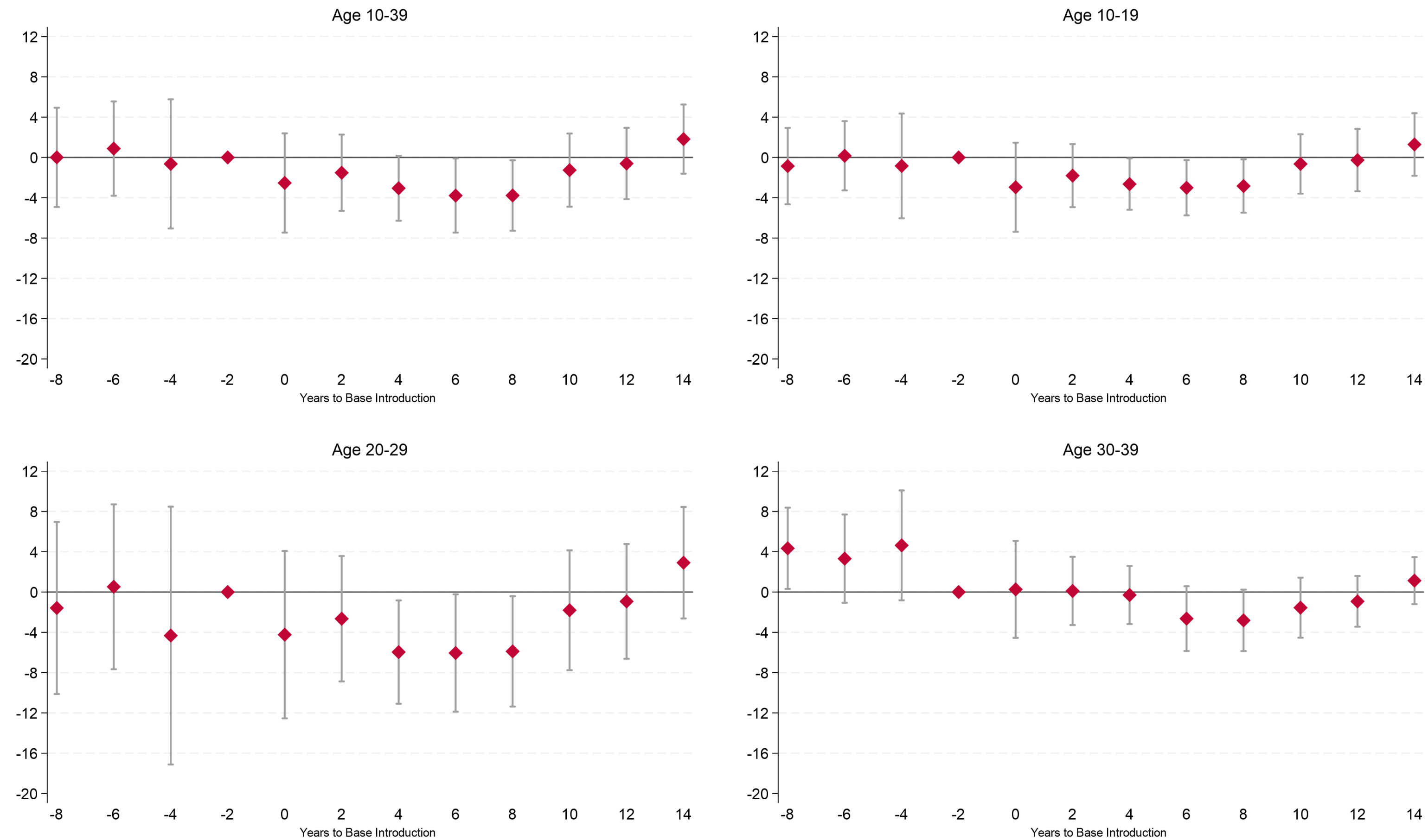
Figure 2.8: Effects on Female-to-Male Sex Ratio



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 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure 2.9: Effects on Fertility

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



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 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

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 2.9 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 2.5. 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 2.10.

Table 2.5: 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
	(6.287)	(4.986)	(10.070)	(5.223)
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 calculates the *average total effect*, which is the weighted sum of the effects of all periods. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

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.[17] Figure 2.11 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,[18] consensual relationships with soldiers, and positive income effects.[19] However, our previous analyses rule 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 B.6. 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

[17]While this is an important margin, we note that marital status is endogenous as it can also be affected by the presence of military bases. We analyze the extent of this endogeneity by estimating the base effect on the share of people currently married or in union with the two rounds of the Census data (Table B.5). Our estimation with the limited data shows no evidence of the relationship between base presence and marriage.

[18]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.

[19]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 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.

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.

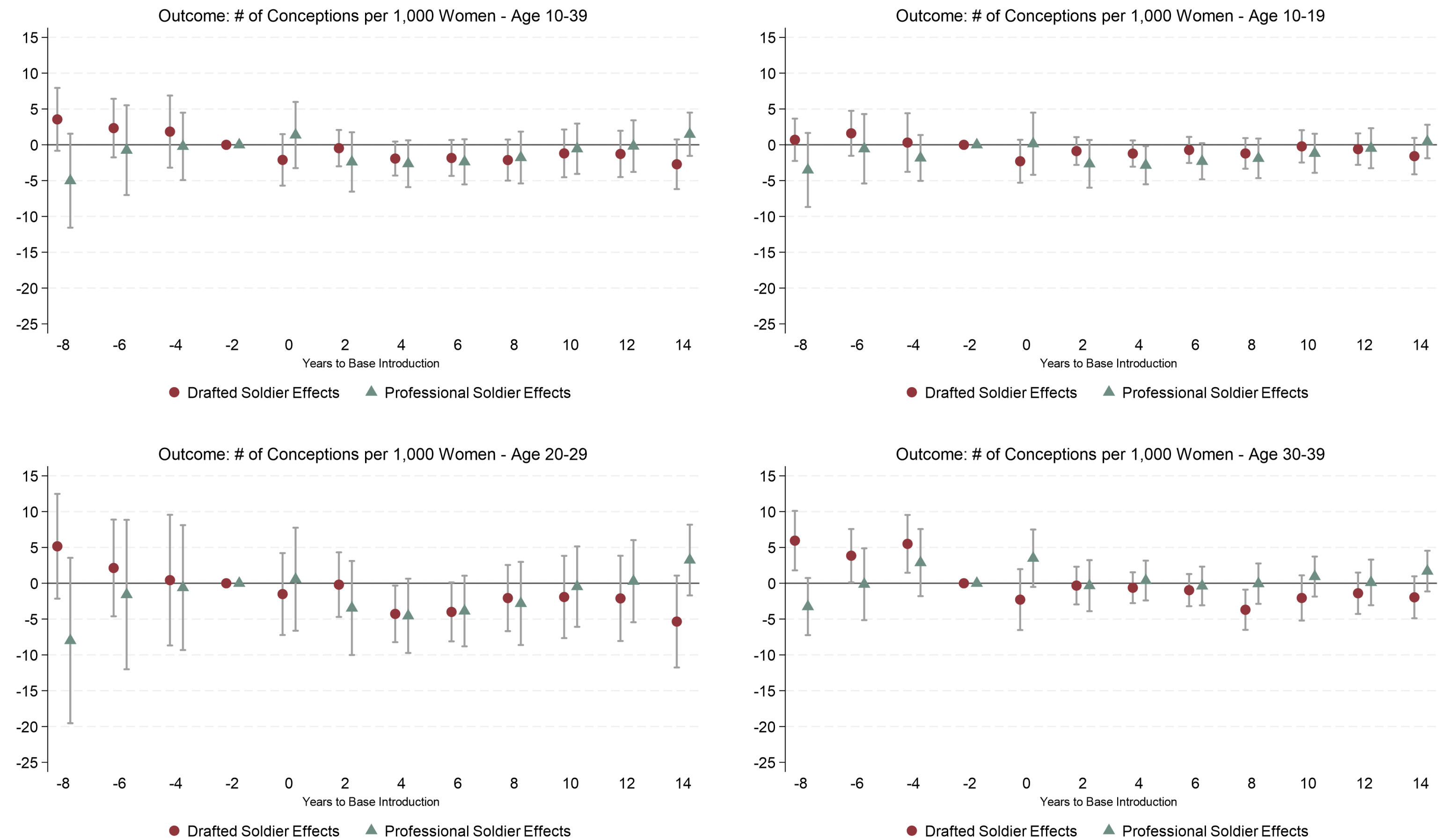
Figure 2.12 presents the estimated effects of military base presence on child support disputes per 100,000 inhabitants, disaggregated into registered and indicted cases. Registered cases refer to disputes formally acknowledged by the judicial system, while indicted cases involve formal legal accusations 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 2.6 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 inhabitants over the course of 16 years after base introduction, which translates to a 51% reduction relative to the control mean.

Table 2.6: Average Total Effects on Child Support Disputes (dCdH)
Outcome: Number of Cases per 100,000 Inhabitants

	Registered (1)	Indicted (2)
Has Army base	-21.901	-29.530^{*}
	(17.730)	(17.292)
Obs.	1,224	1,224
Control mean	66.47	59.22

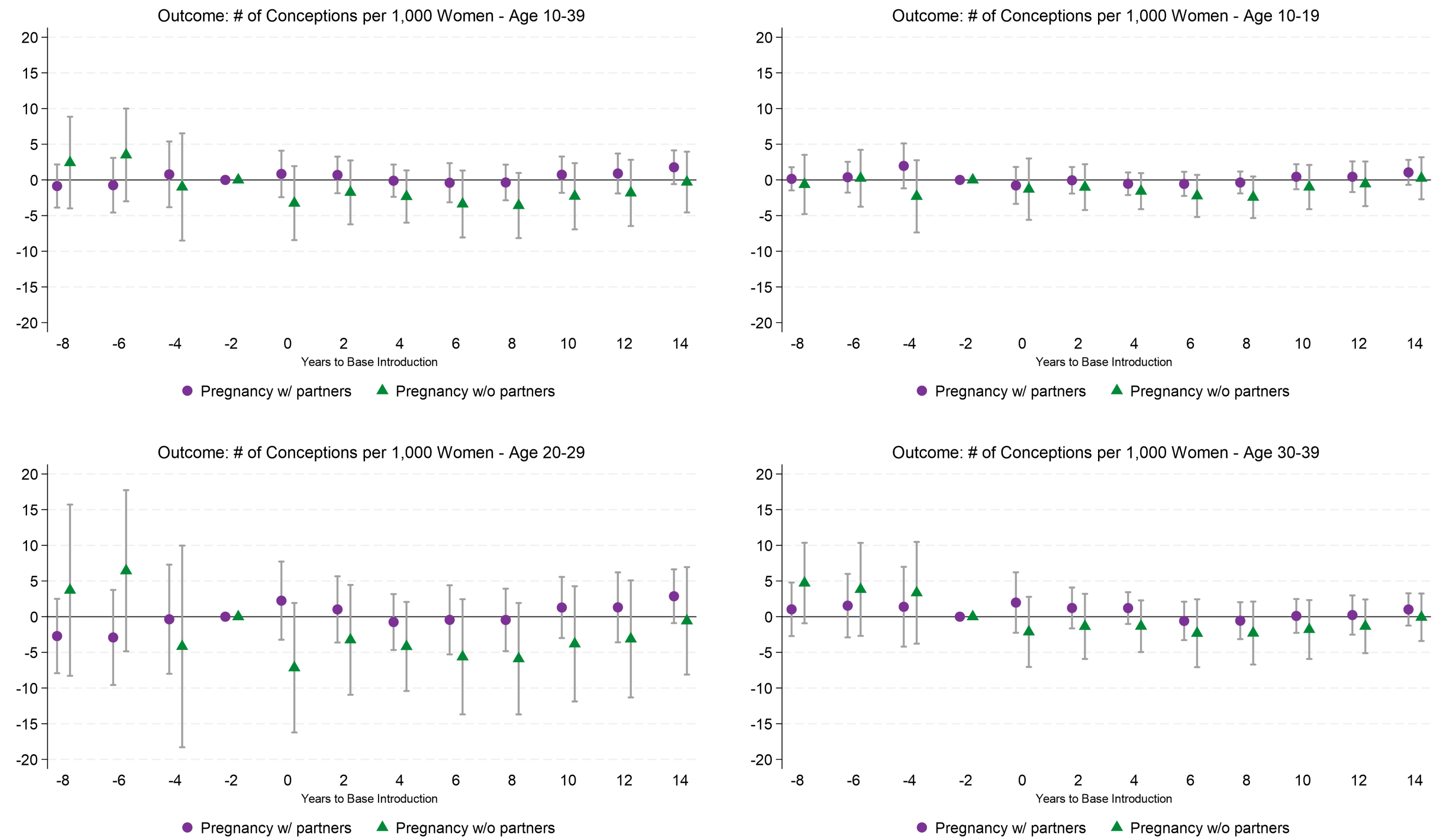
Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors in parentheses clustered at the municipality level. The de Chaisemartin and D'Haultfœuille estimator calculates the *average total effect*, which is the weighted sum of the effects of all periods. This analysis sample includes 153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

Figure 2.10: Effects on Fertility by Base Type



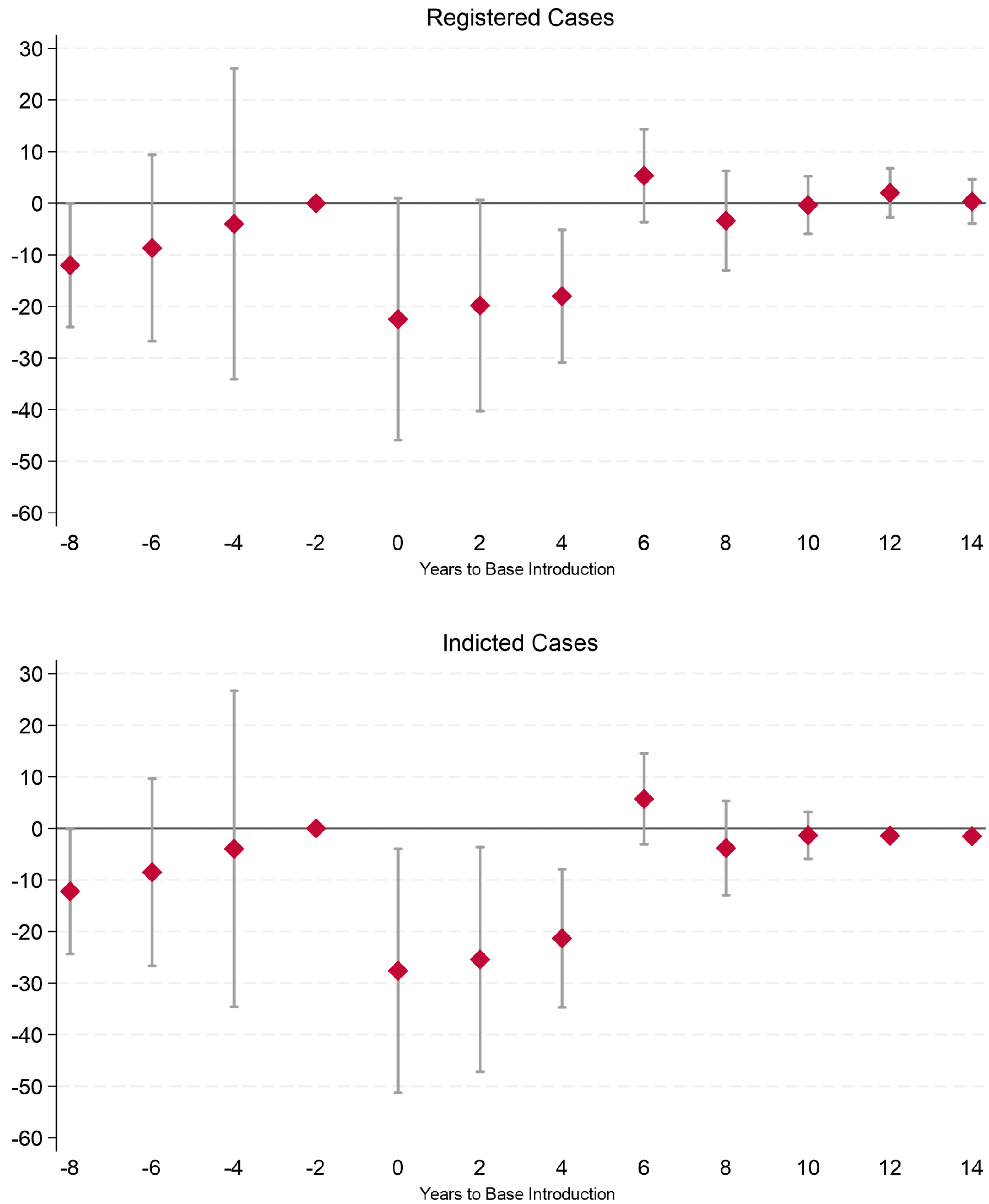
Note: These graphs plot the estimated coefficients for each two-year period relative to the period of military base introduction, calculated using the estimator proposed by de Chaisemartin and D'Haultfœuille (2024). The lines are the 90% confidence intervals. Robust standard errors are clustered at the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

Figure 2.11: Effects on Single-mother Fertility by Mothers' Age Groups



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 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

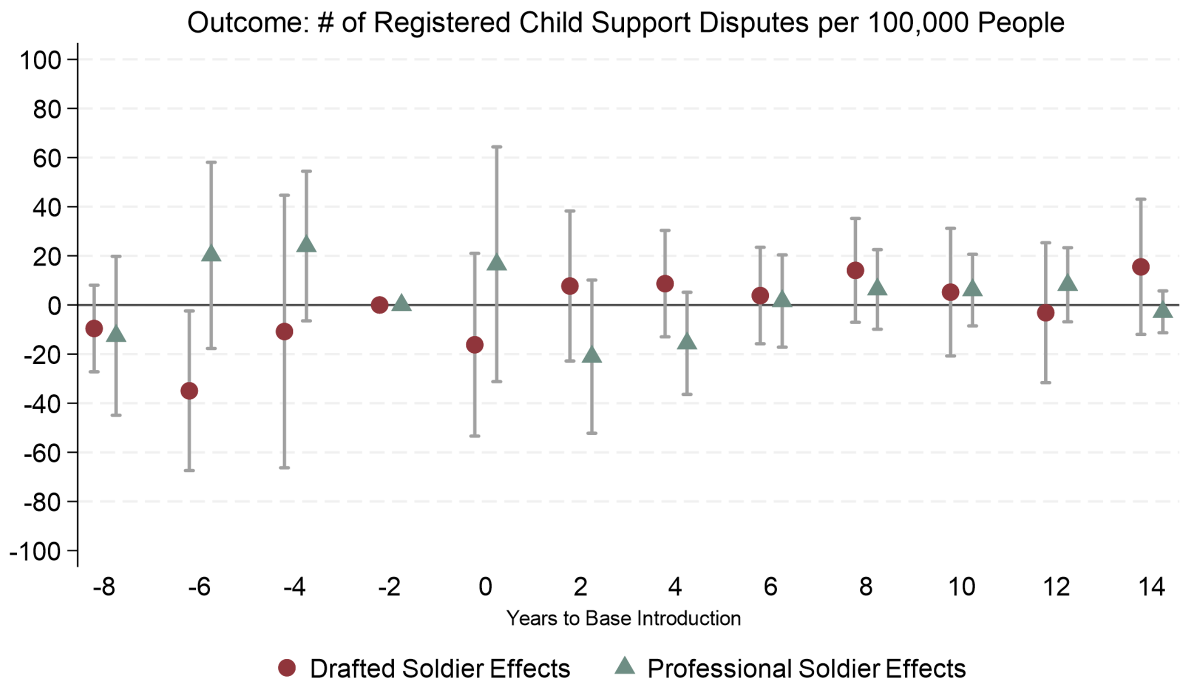
Figure 2.12: Effects on Child Support Disputes
Outcome: Number of Cases per 100,000 Inhabitants



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 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

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 indicted cases, but we find negative coefficients for counterinsurgency bases with professional soldiers, as shown in Figures 2.13 and 2.14.

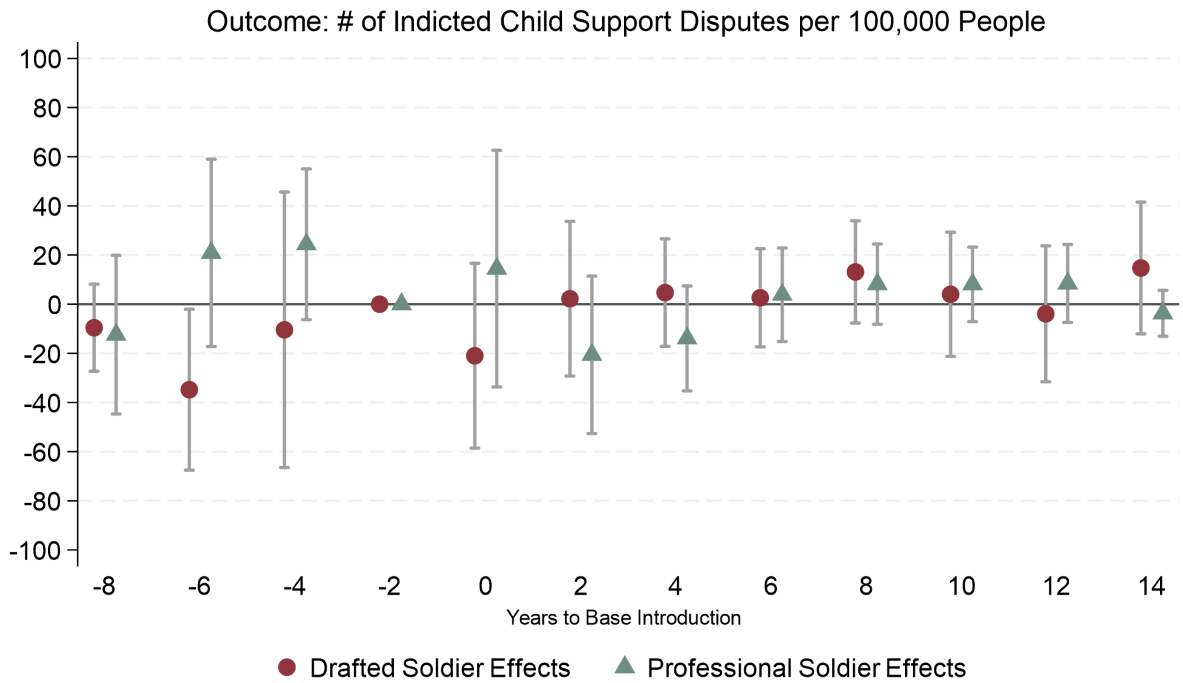
Figure 2.13: Effects on Child Support Disputes by Base Type (Registered Cases)



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). *Drafted soldier effects* refer to the estimated coefficients on an indicator variable for each period in which a municipality has at least one standing unit with more drafted soldiers. *Professional soldier effects* refer to the estimated coefficients on an indicator variable for each period in which a municipality has at least one counterinsurgency unit with more professional soldiers. Drafted soldiers are typically less educated and only given a small monthly stipend. Professional soldiers, who are more educated, better trained, and receive a regular salary with generous benefits. The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

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 communities. However, given the lack of robust statistical significance, we interpret this pattern with

Figure 2.14: Effects on Child Support Disputes by Base Type (Indicted Cases)



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). *Drafted soldier effects* refer to the estimated coefficients on an indicator variable for each period in which a municipality has at least one standing unit with more drafted soldiers. *Professional soldier effects* refer to the estimated coefficients on an indicator variable for each period in which a municipality has at least one counterinsurgency unit with more professional soldiers. Drafted soldiers are typically less educated and only given a small monthly stipend. Professional soldiers, who are more educated, better trained, and receive a regular salary with generous benefits. The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

caution.

2.5.4 Spillover Effects

As mentioned in Section 2.2, 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 2.7[20] 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 2.8 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 municipalities, reinforcing the interpretation that military base presence does not significantly

[20]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.

Table 2.7: 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
	(6.95)	(11.46)
Obs.	1,224	1,224
Panel B: 50 km Buffer		
Has treated neighbor	19.74	8.44
	(13.07)	(5.61)
Obs.	996	996
Panel C: 75 km Buffer		
Has treated neighbor	25.34*	8.70
	(14.78)	(7.16)
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 calculates the *average total effect*, which is the weighted sum of the effects of all periods. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

impact reproductive outcomes in either host or neighboring areas.

Table 2.9 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. How-

Table 2.8: 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
	(6.97)	(5.48)	(11.60)	(5.54)
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
	(7.47)	(5.90)	(13.62)	(8.85)
Obs.	996	996	996	996
Panel C: 75 km Buffer				
Has treated neighbor	7.35	5.80	16.02	-0.56
	(10.32)	(6.61)	(19.27)	(12.15)
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 calculates the *average total effect*, which is the weighted sum of the effects of all periods. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

ever, 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.

2.6 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 mil-

Table 2.9: 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**
	(13.75)	(13.78)
Obs.	1,224	1,224
Panel B: 50 km Buffer		
Has treated neighbor	13.69	3.44
	(14.89)	(11.58)
Obs.	996	996
Panel C: 75 km Buffer		
Has treated neighbor	10.70	2.20
	(18.08)	(15.75)
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 calculates the *average total effect*, which is the weighted sum of the effects of all periods. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

itary 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.

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 (1993) 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, 2020). 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.

2.7 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 2.6. Further investigation could help policymakers identify effective interventions to prevent sexual violence committed by government soldiers in conflict settings.

Chapter 3

Boosting Adoption of Agricultural Inputs: Lab-in-the-Field Evidence from the Entry of a High-Quality Seller

3.1 Introduction

The adoption of high-yield agricultural inputs, such as hybrid maize seeds and fertilizers, is essential for increasing agricultural productivity. However, the uptake of these technologies has remained low in many developing countries (Bridle et al., 2020). The presence of low-quality inputs in local markets—or even the perception that such inputs might be common—can discourage adoption (Bold et al., 2017; Michelson et al., 2021; Hoel et al., 2024). Some interventions that help farmers identify low-quality products by providing market information have proven effective at encouraging the use of high-yield inputs(Hsu and Wambugu, 2022; Hasanain et al., 2023). In the absence of such mechanisms, however, farmers' trust in local agro-dealers and seller's reputations becomes a key determinant of adoption.

This chapter examines whether the entry of a high-reputation seller increases the adoption of high-yield agricultural inputs. The presence of such seller may stimulate demand for these inputs by making buyers more confident that they will obtain high-quality products. We test this hypothesis using a lab-in-the-field experiment conducted in several markets in Western Kenya before the start of the main planting season. Maize farmers—who typically purchase inputs in these markets—are invited to participate in a game in which they choose between hybrid maize seeds and an unrelated alternative good. To mimic the quality uncertainty farmers face in real markets, the quantity of seeds participants receive in each round is uncertain, while the quantity of the alternative good is fixed. To simulate the entry of a high-quality seller, we offer a second option—with a higher expected quantity—to a random half of participants.

We also investigate whether the impact of a high-quality seller's entry depends on the market's underlying quality distribution prior to the arrival. In particular, the entry may have a larger effect when average quality in the market is low. To test this, participants are randomly assigned to groups characterized by different quality distributions. These experimental markets vary along two dimensions: average quality (high or low) and quality dispersion (high or low variance).

Because participants may base their decisions on perceived rather than actual quality distributions, we elicit their beliefs about the incumbent's quality before the entry of the new seller. To do this, we ask participants to imagine selecting hybrid maize seeds over several additional rounds from the incumbent seller. For each round, they report the quantity of seeds they expect to receive. These belief elicitation responses allow us to assess whether participants' choices are driven by observed quality signals or by the expectations they form through repeated interactions with the seller.

Our findings show that the entry of a high-quality seller significantly increases demand for hybrid maize seeds. Participants in experimental markets with entry are 14% more likely to purchase seeds than those in markets without entry. This increase in demand comes from two sources: farmers who previously purchased seeds and switch to the new option when available, and those who had not purchased seed before but begin to do so after the newcomer's entry. Among the former group, participants are 90% less likely to buy from the incumbent seller after the new option becomes available. These results suggest that the availability of a higher-quality option increases adoption, even among farmers who were previously deterred by uncertainty about quality input.

We also find that the impact of the newcomer's entry diminishes when participants either observe high-quality inputs in the market or believe they are likely to receive them. Specifically, the newcomer's effect on seed demand declines as the average quantity of seeds received increases, and as participants' expectations about future seeds improve. These patterns are most evident in markets with high quality variance, where prior beliefs play a larger role in the belief-updating process. These findings suggest that the newcomer's entry primarily boosts demand in settings where farmers have observed low-quality inputs or perceive a high risk of receiving them.

Our results suggest that the effect of entry—and its heterogeneity across quality distributions —can be explained by the gap between farmers willingness to pay (WTP) for hybrid maize seeds and the market price. On average, participants' WTP falls below the prevailing market price, and this gap widens as the underlying quality in the market declines. When a higher-quality option becomes available, however, farmers are willing to pay more—sometimes even slightly above the market price—indicating a strong preference for improved quality.

This chapter contributes to the literature that examines the potential explanations for the slow adoption of modern agricultural inputs in the developing world. As noted above, one reason for low uptake is the presence of low-quality inputs in local retail stores (Bold et al., 2017). Additionally, even when inputs meet quality standards, farmers may perceive them as low quality, which can also deter adoption (Michelson et al., 2021; Hoel et al., 2024).

Interventions that provide farmers with information about input quality have been shown to increase adoption (Hasanain et al., 2023; Hsu and Wambugu, 2022). Similarly, incentivizing sellers to offer high-quality inputs has improved both adoption rates and the overall quality on inputs available in local markets (de Brauw and Kramer, 2023). This paper extends this strand of literature by exploring a new mechanism that may increase adoption of high-yield agricultural inputs: the entry of a high-reputation seller into the market.

This chapter also relates to the literature examining how reputation building can improve efficiency in markets where the quality of goods is uncertain. Laboratory experiments have shown that allowing for reputation building—by making seller identities known and enabling the sharing of past experiences among buyers—increases trade volumes in markets for goods whose quality is only revealed after consumption (Huck et al., 2012, 2016). However, reputation alone may not be sufficient to boost demand for such goods. For example, in markets where quality is only imperfectly observed even after consumption, additional interventions may be necessary to raise trade volumes (Dulleck et al., 2011). Moreover, reputation mechanisms may fail to improve the overall quality available in the market if buyers are pessimistic about the quality they can expect (Bai, 2025). This paper contributes to this strand of the literature by studying whether the entry of a high-reputation seller can increase demand in markets characterized by quality uncertainty.

The reminder of this chapter is organized as follows. Section 3.2 presents a theoretical model that intends to explain the effect of entry on consumers. Section 3.3 presents the experimental design, and section 3.4 presents the specification we estimate to test the effects of entry on demand and possible mechanisms. Section 3.5 presents the main results and discusses potential mechanisms. Finally, section 3.6 concludes.

3.2 Theoretical Model

This section presents a theoretical model showing how consumers react to the entry of a high-quality seller.

3.2.1 Setup

Consider a risk-neutral buyer who chooses between two goods over T periods: a known-quality good and an unknown-quality good. In the first $\tau - 1 < T$ periods, the buyer may purchase the unknown-quality good from an incumbent seller, denoted by I. Starting in period τ, the buyer can choose between the incumbent I and an entrant seller, denoted by E.

In each period t, the buyer receives utility $x_{i,t}$ if she purchases the unknown-quality good from seller $i \in \{E, I\}$, and a deterministic payoff z if she selects the known-quality good, which is the same for all periods. The utility from the unknown-quality good is realized upon consumption and is independently drawn from a normal distribution with mean $\tilde{x}_i$ and variance $\tilde{\sigma}_i^2$. Note that each seller has a distinct underlying quality distribution. We assume the entrant offers higher average quality than the incumbent, so $\tilde{x}_E > \tilde{x}_I$.

The buyer does not observe $\tilde{x}_i$, but instead holds a prior belief about it. This belief is normally distributed with mean $\hat{x}_i$ and variance $\hat{\sigma}_i^2$. Furthermore, the buyer believes that the entrant offers a higher average quality than the incumbent, implying $\hat{x}_E > \hat{x}_I$.

3.2.2 Equilibrium

In period $t = 1$, the buyer chooses the unknown-quality good—offered only by the incumbent seller—if the expected utility from doing so exceeds the utility from the known-quality alternative, that is, if $\mathbb{E}(x_{I,1}) \geq z$. Since the buyer has not yet observed any realization of quality, she relies on her prior belief about the incumbent's average quality. Therefore, $\mathbb{E}(x_{I,1}) = \hat{x}_I$, and the condition for choosing the unknown-quality good simplifies to $\hat{x}_I \geq z$.

After consuming the unknown-quality good, the buyer observes the realized payoff $x_{I,1}$. She interprets this outcome as a signal about the incumbent seller's average quality and updates her belief accordingly. Assuming the buyer is a Bayesian learner, her posterior belief about the incumbent's quality $\tilde{x}_I$ remains normally distributed. By Bayes' theorem, the posterior distribution has mean:

$$\frac{\hat{x}_I \tilde{\sigma}_I^2 + x_{I,1} \hat{\sigma}_I^2}{\hat{\sigma}_I^2 + \tilde{\sigma}_I^2} \tag{3.1}$$

and variance:

$$\frac{\hat{\sigma}_I^2 \tilde{\sigma}_I^2}{\hat{\sigma}_I^2 + \tilde{\sigma}_I^2}. \tag{3.2}$$

In period $t = 2$, the buyer chooses the unknown-quality good if $\mathbb{E}(x_{I,2}) \geq z$. The value of $\mathbb{E}(x_{I,2})$ depends on the buyer's choice in the previous period. If the buyer selected the known-quality good in $t = 1$, she did not receive any new information about the unknown-quality good and thus does not update her belief. In this case, her expectation remains $\mathbb{E}(x_{I,2}) = \hat{x}_I$, and since $\hat{x}_I < z$, she will again choose the known-quality good.

Conversely, if the buyer chose the unknown-quality good in $t = 1$, she updated her belief using the observed payoff $x_{I,1}$. In this case, her expectation for period 2 is given by the posterior mean in equation (3.1). If she chooses the unknown-quality good again in $t = 2$, she observed a new realization $x_{I,2}$ and uses it to update her belief further. The resulting posterior is again normally distributed, now with mean:

$$\frac{\hat{x}_I \tilde{\sigma}_I^2 + \hat{\sigma}_I^2 (x_{I,1} + x_{I,2})}{2\hat{\sigma}_I^2 + \tilde{\sigma}_I^2}$$

and variance:

$$\frac{\hat{\sigma}_I^2 \tilde{\sigma}_I^2}{2\hat{\sigma}_I^2 + \tilde{\sigma}_I^2}.$$

We can generalize this result to any period $t < \tau$. In each of these periods, the buyer chooses the unknown-quality good if $\mathbb{E}(x_{I,t}) \geq z$, where

$$\mathbb{E}(x_{I,t}) = \frac{\hat{x}_I \tilde{\sigma}_I^2 + \bar{x}_{I,t-1} D_{t-1} \hat{\sigma}_I^2}{D_{t-1} \hat{\sigma}_I^2 + \tilde{\sigma}_I^2}. \tag{3.3}$$

Here, $d_t \in \{0, 1\}$ is an indicator equal to 1 if the buyer selected the unknown-quality good in period t, and 0 otherwise. The term $D_{t-1} = \sum_{j=1}^{t-1} d_j$ denotes the number of periods in which the buyer chose the unknown-quality good by the start of period t. The average of the observed quality signals is given by $\bar{x}_{I,t} = \sum_{j=1}^{t-1} d_j x_{I,j} / \sum_{j=1}^{t-1} d_j$, which is well defined whenever $D_{t-1} > 0$.

An important implication of this structure is that the buyer chooses the unknown-quality good in period t if and only if she also chose it in period $t-1$. To establish this formally, we introduce the following proposition.

Proposition 1. *If the buyer chooses the known-quality good in any period $t < \tau$, she will continue to choose the known-quality good in all subsequent periods.*

Proof. Suppose the buyer chooses the known-quality good in period $t = 1$. By the decision rule, this implies $\hat{x}_I < z$. Since the buyer does not observe any signal about the unknown-quality good, her belief remains unchanged, so $\mathbb{E}(x_{I,2}) = \hat{x}_I$. In period $t = 2$, the buyer again compares $\hat{x}_I$ to z, and since $\hat{x}_I < z$, she once again chooses the known-quality good.

Now suppose the buyer chooses the known-quality good in some period $t = t' - 1$. Then it must be that $\mathbb{E}(x_{I,t'-1}) = \hat{x}_{I,t'-1} < z$. Because she does not observe any new signal, her belief remains the same in period t', that is, $\hat{x}_{I,t'} = \hat{x}_{I,t'-1}$. Therefore, $\mathbb{E}(x_{I,t'}) = \hat{x}_{I,t'} < z$, and she again chooses the known-quality good.

By induction, if the buyer selects the known-quality good in any period $t < \tau$, she will continue to do so in all subsequent periods. $\square$

Entry of the new seller

In period $t = \tau$, a new seller enters the market. From this period onward, the buyer may purchase the unknown-quality good from either the incumbent or the entrant seller. The buyer will choose the unknown-quality good if the expected utility from at least one seller exceeds the utility from the known-quality alternative—that is, if either $\mathbb{E}(x_{E,\tau}) \geq z$ or $\mathbb{E}(x_{I,\tau}) \geq z$. In contrast, the buyer chooses the known-quality good if both $\mathbb{E}(x_{E,\tau}) < z$ and $\mathbb{E}(x_{I,\tau}) < z$ hold.

3.2.3 Testable Hypotheses

From the model, we derive the following hypotheses that we test in our empirical analysis.

Hypothesis 1. *The entry of the new seller induces buyers who were previously choosing the known-quality good to switch to the unknown-quality good, provided the buyer's prior belief about the entrant's quality is sufficiently high.*

Proof. Consider a buyer who was choosing the known-quality good before the entry of the new seller. By the decision rule, this implies that $z > \mathbb{E}(x_{I,\tau})$. Upon entry, the buyer now evaluates both unknown-quality options. If the expected utility from the entrant satisfies $\mathbb{E}(x_{E,\tau}) \geq z$, the buyer strictly prefers the entrant's good to the known-quality alternative and switches to the unknown-quality good. Therefore, entry induces a switch in behavior whenever the buyer's belief about the entrant's average quality is high enough to satisfy this inequality. $\square$

Hypothesis 2. *The entry of the new seller induces a switch from the known-quality alternative to the unknown-quality good only among buyers whose prior belief about the incumbent's quality is sufficiently low.*

Proof. Consider two buyers, A and B, such that $\hat{x}_I^A > \hat{x}_I^B$. Since the expectation of the unknown-quality good is increasing in the prior—i.e., $\partial\mathbb{E}(x_{I,t})/\partial\hat{x}_I > 0$—it follows that $\mathbb{E}(x_{I,t}^A) > \mathbb{E}(x_{I,t}^B)$ for all $t \in \{1, \ldots, T\}$.

Suppose buyer A chooses the unknown-quality good from the incumbent before entry, while buyer B chooses the known-quality alternative. This implies:

$$\mathbb{E}(x^A_{I,t}) \geq z > \mathbb{E}(x^B_{I,t}).$$

Now assume that both buyers hold the same prior about the entrant's quality: $\hat{x}^A_E = \hat{x}^B_E = \hat{x}_E$, with $\hat{x}_E > \mathbb{E}(x^A_{I,\tau})$. In this case, buyer A continues to choose the unknown-quality good but switches from the incumbent to the entrant. Buyer B, who previously chose the known-quality alternative, now switches to the unknown-quality good because $\hat{x}_E > z > \mathbb{E}(x^B_{I,\tau})$.

Thus, entry only induces a change in purchasing behavior—from the known-quality good to the unknown-quality good—for buyers with relatively low priors about the incumbent's average quality. □

Hypothesis 3. *The entry of the new seller induces a switch from the known-quality alternative to the unknown-quality good only among buyers who, upon entry, have observed an average of quality signals from the incumbent that is sufficiently low.*

Proof. Consider two buyers, A and B, such that $\bar{x}^A_{I,\tau-1} > \bar{x}^B_{I,\tau-1}$, where $\bar{x}_{I,\tau-1}$ denotes the average of the quality signals observed from the incumbent up to period $\tau - 1$. Since the expected utility from the unknown-quality good is increasing in the average of observed signals—that is, $\partial\mathbb{E}(x_{I,t})/\partial\bar{x}_{I,t} > 0$—it follows that $\mathbb{E}(x^A_{I,t}) > \mathbb{E}(x^B_{I,t})$ for all $t \in \{1, \ldots, T\}$.

Suppose buyer A chooses the unknown-quality good from the incumbent in period τ, while buyer B chooses the known-quality alternative. Then:

$$\mathbb{E}(x^A_{I,\tau}) \geq z > \mathbb{E}(x^B_{I,\tau}).$$

Assume both buyers have the same prior about the entrant, such that $\hat{x}^A_E = \hat{x}^B_E = \hat{x}_E$, and that $\hat{x}_E > \mathbb{E}(x^A_{I,\tau})$. In this case, buyer A continues to purchase the unknown-quality good but switches from the incumbent to the entrant. Buyer B, in contrast, is now willing to switch from the known-quality good to the entrant's unknown-quality good, since $\hat{x}_E > z > \mathbb{E}(x^B_{I,\tau})$.

Therefore, entry induces a change in purchasing behavior—from the known-quality good to the unknown-quality good—only among buyers who, on average, have observed low-quality signals from the incumbent prior to entry. □

Hypothesis 4. *The effect of entry is more likely to induce a switch to the unknown-quality good among buyers with low priors about the incumbent in markets where the variance of the incumbent's quality distribution is sufficiently high.*

Proof. Consider two buyers, A and B, such that $\hat{x}^A_I > \hat{x}^B_I$. Also consider two markets: a high- and low-variance markets, H and L, where $\tilde{\sigma}^2_{I,H} > \tilde{\sigma}^2_{I,L}$.

Since the expected utility from the unknown-quality good is increasing in the buyer's prior, we have $\mathbb{E}(x^A_{I,\tau}) > \mathbb{E}(x^B_{I,\tau})$. Let $\mathbb{X} = \mathbb{E}(x^A_{I,\tau}) - \mathbb{E}(x^B_{I,\tau})$ denote the difference in expected utility between the two buyers. Because the expected marginal utility of the buyer's prior about the incumbent is increasing in the variance of the incumbent's quality distribution—that is, $\partial^2\mathbb{E}(x_{I,t})/\partial\hat{x}_I\partial\tilde{\sigma}^2_I > 0$—it follows that this difference is larger in market H than in market L, i.e., $\mathbb{X}_H > \mathbb{X}_L$.

Now suppose $\hat{x}^A_I$ is the same across both markets, so $\mathbb{E}(x^{A,H}_{I,\tau}) = \mathbb{E}(x^{A,L}_{I,\tau})$. Given $\mathbb{X}_H > \mathbb{X}_L$, this implies:

$$\mathbb{E}(x^{A,H}_{I,\tau}) = \mathbb{E}(x^{A,L}_{I,\tau}) > \mathbb{E}(x^{B,L}_{I,\tau}) > \mathbb{E}(x^{B,H}_{I,\tau}).$$

Suppose buyer B purchases the unknown-quality good in market L but not in market H, and that $\hat{x}_E > \mathbb{E}(x^{A,H}_{I,\tau})$. Then:

$$\mathbb{E}(x^{A,H}_{I,\tau}) = \mathbb{E}(x^{A,L}_{I,\tau}) > \mathbb{E}(x^{B,L}_{I,\tau}) \geq z > \mathbb{E}(x^{B,H}_{I,\tau}).$$

This implies that in the low-variance market L, both buyers would already purchase the unknown-quality good regardless of entry. In contrast, in the high-variance market H, buyer A continues to purchase the unknown-quality good (switching from the incumbent to the entrant), while buyer B—who previously did not purchase the unknown-quality good—now switches due to the entry of the higher-quality seller.

Thus, the entry effect on demand is more pronounced in high-variance markets, particularly among buyers with relatively low priors about the incumbent. □

These hypotheses motivate our empirical analysis. The first hypothesis suggests that the entry of a high-quality seller increases demand for unknown-quality goods, like agricultural inputs, as buyers who were previously hesitant to purchase due to quality uncertainty are more likely to do so once a higher-quality option becomes available. It also implies that buyers who were already purchasing the unknown-quality good from the incumbent are likely to switch entirely to the entrant. The second and third hypotheses highlight that the effect of entry varies with the market's underlying quality distribution prior to the entrant's arrival. Specifically, when the incumbent offers relatively low average quality, more buyers opt for the known-quality alternative. In such markets, the arrival of a high-quality entrant leads to a greater increase in demand for the unknown-quality good than in markets where average quality is higher. Finally, the fourth hypothesis posits that this heterogeneous effect of entry depends on the variance of the incumbent's quality distribution. When quality variance is high, buyers place more weight on their prior beliefs than on the signals they observe from individual purchases. As a result, their beliefs update more slowly, making the arrival of a new, high-quality seller a more significant improvement in the perceived quality available in the market.

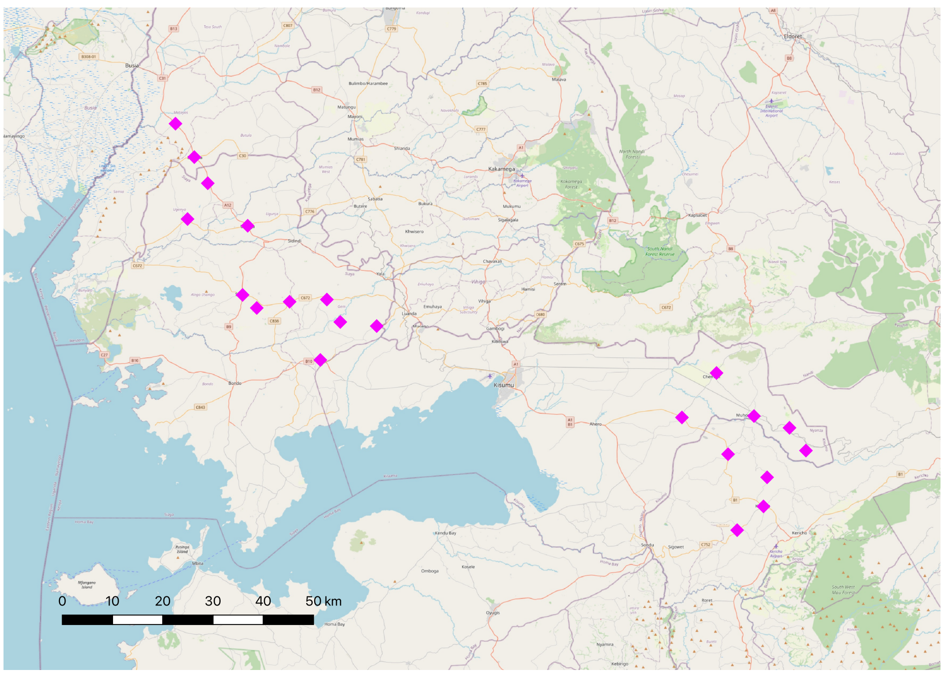
3.3 Experimental Design

3.3.1 Simulated agricultural input markets

We measure the effect of the entry of a high-quality seller on agricultural inputs demand by recreating a market for hybrid maize seeds. Participants are smallholder maize farmers who typically purchase their agricultural inputs in local markets in Central and Western

Kenya. Specifically, we invited farmers from 21 market centers in rural Kenya, where One Acre Fund, a non-profit organization providing technical assistance to smallholders in several countries across Eastern Africa, had opened stores selling high-quality agricultural inputs. The locations of these market centers are shown in Figure 3.1. The experimental procedures were conducted in March 2023, just before the long rains season begins, which coincides with the main planting period in these regions. In each market, we held three to four sessions per day, with each session comprising four to five farmers[1].

Figure 3.1: Market Centers Location



Notes: Each diamond corresponds to a market center where we conducted the lab-in-the-field experiments.

In each session, participants play a total of 20 rounds, during which they decide whether to purchase a bag of hybrid maize seeds. To mimic the uncertainty in input quality that farmers typically face in these markets, we randomly vary the quantity of seeds participants receive in each round. Specifically, participants draw 10 marbles from a bag containing both black and white marbles. For each black marble drawn, they receive 100 grams of hybrid

[1] The AEA registry and the corresponding pre-analysis plan can be found at `https://www.socialscienceregistry.org/trials/11135`

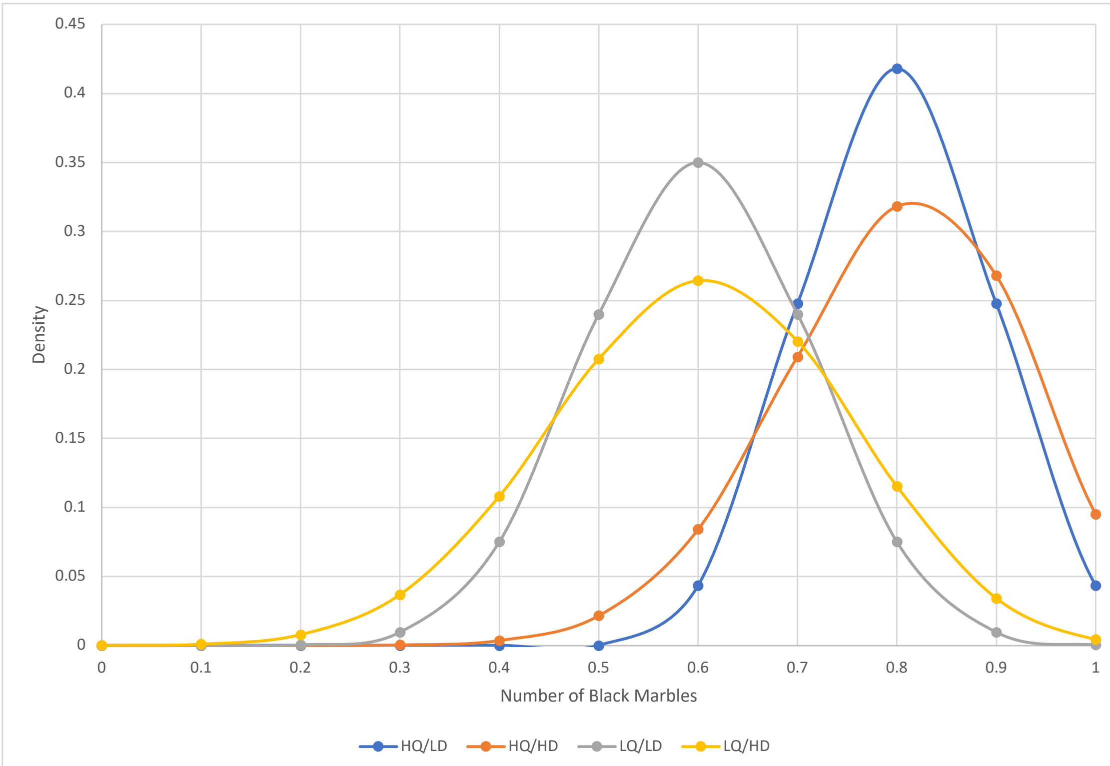
maize seeds with germination rates exceeding 90%, the threshold established by Kenyan guidelines. Participants are unaware of the number of black and white marbles in the bag and only learn the number of black marbles they draw at the end of each round. This design ensures that the quantity of seeds they receive is only known ex-post.

Participants may also choose to opt out of purchasing the bag of seeds. In such cases, they receive a good of comparable monetary value but unrelated to hybrid maize seeds, selected from a predetermined set of options. We selected goods with similar monetary value to 1 kg of hybrid maize seeds to prevent farmers from opting out in order to receive a higher value alternative. Additionally, the selected good is unrelated to hybrid maize seeds to ensure it is not perceived as a substitute for agricultural production. Participants may choose between 3 kg of maize flour, 2 kg of sugar, or 1 liter of cooking oil.

In each session, participants draw marbles from two different bags—one for the first set of ten rounds and another for the second set. The composition of black and white marbles in each bag is randomly determined. Specifically, each set of ten rounds is randomly assigned to one of four groups: high-quality/high-dispersion (HQ/HD), high-quality/low-dispersion (HQ/LD), low-quality/high-dispersion (LQ/HD), and low-quality/low-dispersion (LQ/LD). Within the same session, each of the two bags was assigned to a different group, ensuring that no session had both bags from the same group.

In high-quality bags, the ratio of black to white marbles is 4:1, resulting in an expected mean of 8 black marbles per 10 draws. In contrast, low-quality bags have a 3:2 ratio, leading to an expected mean of 6 black marbles per 10 draws. Additionally, we manipulate dispersion by varying the total number of marbles in each bag. High-dispersion bags contain 100 marbles, while low-dispersion bags contain 20 marbles. Consequently, the number of black marbles drawn by participants in each round follows a hypergeometric distribution. Figure 3.2 presents the probability mass function for each of the four groups. This experimental design allows us to systematically vary the quality distribution participants encounter in these markets.

Figure 3.2: Incumbent's quality distributions



Notes: Probability mass functions for the number of black marbles (successes) drawn in each round for a total of ten draws. High-quality (HQ) distributions consider a proportion of 4 black marbles to 1 white marbles, whereas low-quality (LQ) distributions consider a proportion of 3 black marbles to 2 white marbles. High-dispersion (HD) distributions have a total number of 100 marbles, whereas low-dispersion marbles (LD) have a total of 20 marbles.

Since participants do not know the exact composition of each bag, we provide some information to help them form beliefs about its contents. Specifically, at the beginning of each set of rounds (i.e., rounds 1 and 11), enumerators visibly place the designated number of white marbles into each bag, while the number of black marbles remains hidden from participants. Additionally, before participants begin the game, we conduct 10 preliminary rounds and record the number of black and white marbles drawn each time on a whiteboard, ensuring that all participants can observe the results. Participants may also use the outcomes of their own draws during the game—if they choose seeds over the alternative good—to further learn about the contents of the bag. Enumerators recorded the number of black and white marbles each participants drew in each round using the survey instrument; this information was private and only available to the individual participant.

3.3.2 Elicitation of beliefs about quality

After rounds 5 and 15, we elicit farmers' beliefs about the quality of the seeds sold by the only store available in each set of ten rounds up to that point. We ask farmers to imagine they will purchase seeds from the same store for the next 20 rounds. To facilitate this process, we present them with a chart containing 20 numbered boxes, each representing one of these hypothetical rounds (see Figure C.1). We then provide them with a handful of beans, each representing a white marbles they expect to draw in a given round. Participants are instructed to distribute the beans across the boxes according to their expectations. For instance, if they believe they will draw five white marbles in the first hypothetical round, they should place five beans in box 1.

With this approach, we aim to reconstruct participants' empirical distribution of perceived quality for the incumbent store. Specifically, we compute the average number of black marbles they expect to draw, which is derived by subtracting the number of beans placed in each box from ten. These values serve as empirical analogs of the parameters in the theoretical model. One potential concern is that our method relies on distributions constructed from at most 15 data points (i.e., 10 preliminary draws plus 5 draws during the game). This limitation could affect the accuracy of farmers' perceptions of their experimental market's quality distribution. However, our primary objective is to capture participants' beliefs about the quality in their experimental market, irrespective of whether their perceptions are accurate.

3.3.3 Entry of high-quality seller

After eliciting participants' beliefs about quality, we randomly assign them to one of two groups: entry and no-entry. Participants in the entry group have the opportunity to purchase seeds from a new seller, whereas those in the no-entry group continue purchasing seeds from the same seller they have been interacting with. Notably, a participant may be assigned to the entry group in the first set of 10 rounds but not in the second, creating variation in treatment at the individual level.

We represent the new seller with a new bag containing a higher ratio of black to white marbles. Specifically, this bag includes 18 black marbles and 2 white marbles, following a hypergeometric distribution with a mean of 9 and lower variance compared to incumbent bags. Similar to the incumbent's bag, enumerators visibly place two white marbles into the new bag and keep the number of black marbles hidden from the participant. This new seller represents a high-quality store, as its entry enhances the overall quality available in the experimental market. Participants still have the option to opt out of purchasing seeds and instead receive the alternative good.

We incentivize the game by randomly selecting one of the 20 rounds at the end. Participants receive the item they chose in that selected round.

3.3.4 Elicitation of farmers' valuation for high-quality seeds

At the end of the experiment, we elicit participants' WTP for the seeds offered by both incumbents and the entrant, even for farmers who were assigned to a market with no newcomer entry[2]. To do so, we conduct a multiple price list approach, which is a variation of the original Becker-DeGroot-Marschak mechanism (Becker et al., 1964). In particular, we ask participants if they are willing to purchase a bag of hybrid maize seeds from both incumbent stores and the entrant at a determined price, which is taken from a list previously constructed by the research team. The list ranges from Ksh. 0 to Ksh. 300, the market price, which is informed to farmers to generate a natural anchor, which is common in real-life demand decisions (Cole et al., 2020). We ask this question for all prices in the list to check for inconsistent choices among participants in the form of multiple or single switching behavior[3]

[2]For participants assigned to the no-entry group in both sets of ten rounds, we mention that the entrant's bag contains only two white marbles.

[3]Among the 317 participants, only 6% provided inconsistent responses (i.e., stating they would not purchase at a given price but later indicating they would buy at a higher price).

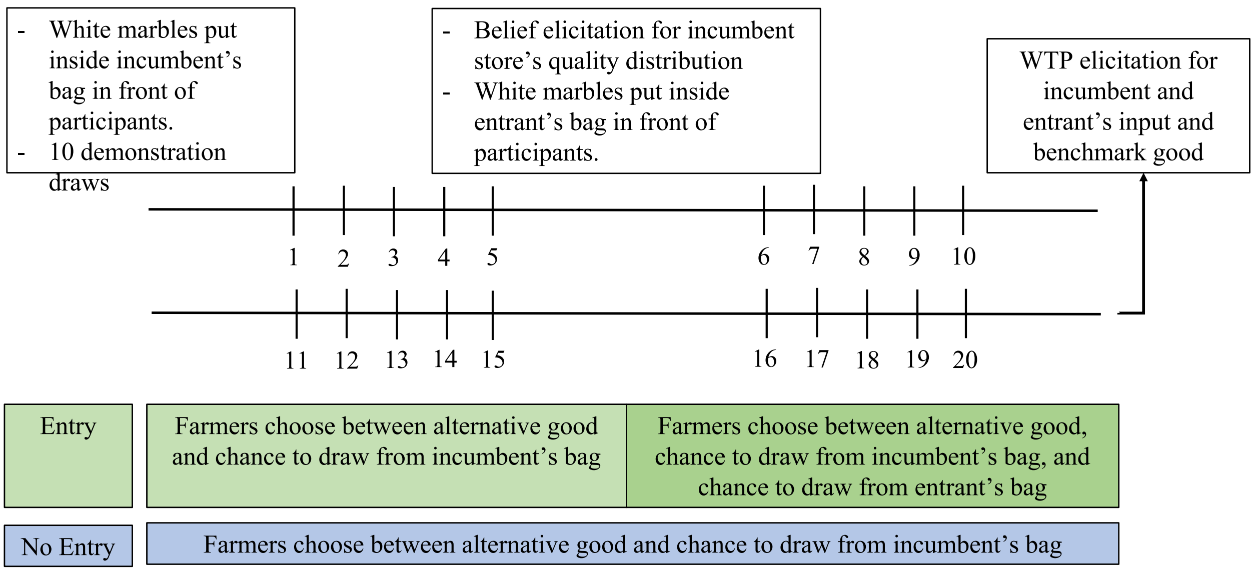
(Jack et al., 2022). We present each choice separately to keep participants from knowing the price distribution. We also randomize the order in which the choices are presented to farmers to take into account any framing effect on the participants' valuations. Recent studies eliciting WTP in low- and middle-income countries have found that respondents' valuations vary depending on the order of the questions (Channa et al., 2021; Jack et al., 2022; Squires, 2024). Specifically, we randomize whether we start asking a farmer the question with the smallest price and then continue in ascending order, or we begin with the biggest and then proceed in descending order.

To incentivize participants to truthfully reveal their WTP, we inform them that one price from the list will be randomly selected. If the drawn price is less than or equal to their stated WTP—that is, the first price at which they indicated willingness to purchase—then they buy the good at the randomly drawn price. Otherwise, they do not purchase the good. To increase statistical power, we elicit WTP for the two incumbents and the entrant store, and incentivize the three of them. Specifically, after completing the WTP elicitation, we randomly select one seller and then draw a price from the list.

To improve the accuracy and precision of the valuation for the hybrid maize seeds offered by the different sellers in the game, we elicit participants' WTP for a benchmark good, which is unrelated to agricultural inputs (Dizon-Ross and Jayachandran, 2022). We include the benchmark good's WTP as a control in the specifications using the input's valuation as the dependent variable to correct for variations in the seeds' measured WTP that are unrelated to the farmers' true WTP. In addition, we conduct the multiple price list approach with an item of small value to practice with participants the procedure and check their understanding. In this case, the MPL approach is not incentivized. We conduct the practice round and the benchmark good elicitation before the seeds MPL.

Farmers receive Ksh. 500 for participation in the experiment, which may be utilized by them to buy the good randomly selected from the WTP elicitation exercise. After the elicitation exercise, we administer a small survey to participants where we ask them about their agricultural production practices, input usage and purchasing decisions, and we elicit their risk aversion and time preferences using questions from the Global Preferences Survey (Falk et al., 2016). Figure 3.3 shows a timeline summarizing the events described in this section.

Figure 3.3: Experimental Framework Timeline



3.4 Empirical Analysis

Using data from the experimental exercises, we estimate the effect of the entry of a high-quality seller on the demand for hybrid maize seeds and examine how this effect varies with the experimental market's quality distribution and buyer's beliefs. Specifically, we test several hypotheses derived from the theoretical model. First, we assess whether the entry of the newcomer increases demand for hybrid maize seeds. Second, we test whether this effect is stronger when participants face an incumbent with lower average quality. In particular, we examine whether the entry induces a larger increase in demand when the market's average quality is low, when participants have observed lower-quality signals, or when their beliefs about the incumbent's quality are low. Finally, we assess whether these effects are more pronounced in high-dispersion markets compared to low-dispersion markets. To identify these effects, we exploit the random assignment of the entrant's availability and the exogenous variation in quality distributions across experimental markets. We explore these hypotheses using two types of outcome variables: the probability that a farmer chooses to purchase seeds and their stated willingness to pay.

3.4.1 Likelihood of purchasing seeds

We start our analysis by first examining the effect of the newcomer's entry on the likelihood of purchasing seeds. To do so, we estimate the following linear probability model by OLS:

$$y_{isr} = \alpha_0 + \alpha_1 E_{isr} + \alpha_2 Q_{isr} + \alpha_3 D_{isr} + \delta_i + \phi_r + \varepsilon_{isr} \tag{3.4}$$

where y_{isr} is an indicator of whether participant i in session s chose seeds in round r; E_{isr} is an indicator of whether i was able to purchase seeds from the newcomer in round r; Q_{isr} is a measure of the market's underlying quality distribution, which can be (i) an indicator of whether the market is of high-quality, (ii) the standardized average number of black marbles taken during the 10 demonstration rounds, and (iii) the standardized average number of black marbles i expects to obtain in 20 hypothetical rounds; D_{isr} is an indicator of whether the market is of high-dispersion; δ_i are individual fixed effects; ϕ_r are round fixed effects; and ε_{isr} is the error term. We cluster the standard errors at the session level. The parameter of interest is α_1, as it measures the causal effect of the newcomer's entry on individual seeds demand. According to the theoretical model, we expect $\alpha_1 > 0$ because the newcomer's entry represents an overall improvement in the market's quality distribution.

Although we exploit experimental variation in both treatment and the market's quality distribution, participants' beliefs about the quality they expect to obtain in future purchases are endogenous. To address this issue, we instrument this variable with the standardized average number of black marbles observed during the demonstration rounds. We argue that this instrument is relevant, as participants use this information to form beliefs about the market's underlying quality distribution. Furthermore, the instrument satisfies the exclusion restriction, as it can only influence the probability of choosing seeds through the beliefs participants form about the quality distribution. Since the preliminary draws are entirely random, there is no reason to believe they would affect the probability of choosing seeds through any other channel.

The theoretical model predicts that the effect of entry varies depending on the market's quality distribution and buyer's perceptions of that quality. To test for heterogeneous effects, we estimate the following linear probability model using OLS:

$$\begin{aligned} y_{isr} = \beta_0 + &\beta_1 E_{isr} + \beta_2 Q_{isr} + \beta_3 D_{isr} + \beta_4 E_{isr} \times Q_{isr} + \beta_5 E_{isr} \times D_{isr} \\ &+ \beta_6 Q_{isr} \times D_{isr} + \beta_7 E_{isr} \times Q_{isr} \times D_{isr} + \delta_i + \phi_r + \nu_{isr}. \end{aligned} \tag{3.5}$$

According to the model, entry is more likely to induce a switch from the known-quality good to the unknown-quality good among buyers with low prior beliefs about the incumbent's quality and among those who have observed low-quality signals. Therefore, the effect of entry is expected to decrease as the market's average quality improves, or as buyers observe higher-quality signals or expect to obtain high-quality products in future purchases. To capture these relationships empirically, we focus on the marginal effect of entry across different levels of quality and dispersion. In high-dispersion markets, this marginal effect is given by $\beta_4 + \beta_7$, which we expect to be negative. In low-dispersion markets, the marginal effect is simply β_4, and we expect it to be smaller in magnitude than the effect in high-dispersion markets, since buyers place less weight on their beliefs when observed quality is more consistent.

Finally, we exploit the panel structure of the data and estimate an event-study specification to examine how the effect of treatment varies over time. In particular, we estimate the following equation using OLS:

$$y_{isr} = \gamma_0 + \sum_{j=1}^{4} \gamma_j 1[r = Entry_{is} - j] + \sum_{j=6}^{10} \gamma_j 1[r = Entry_{is} + j] + \delta_i + \phi_r + \epsilon_{isr} \tag{3.6}$$

where $Entry_{is}$ is an indicator of whether i was assigned to the entry group in the first set of ten rounds. We estimate the same specification for the second set of ten rounds. In both cases, we cluster the standard errors at the session level. We expect to observe $\gamma_j > 0$ for $j \geq 6$, which indicates that entry has a positive impact on the probability of choosing seeds only in the rounds where the entry becomes available. Before that, we should observe $\gamma_j = 0$, indicating that there is no difference in the likelihood of choosing seeds between treated and untreated participants.

3.4.2 Willingness to pay for seeds

We then focus on how the newcomer's entry and the market's quality distribution affects the willingness to pay for seeds sold by both incumbents and the entrant. For this analysis, we use the data obtained from the WTP elicitation exercise for seeds coming from the incumbent and newcomer stores. When looking at the incumbents, we estimate the following equation using OLS:

$$WTP^I_{it} = \lambda_0 + \lambda_1 E_{it} + \lambda_2 Q_{it} + \lambda_3 D_{it} + \delta_i + \varphi_t + \xi_{it} \tag{3.7}$$

where WTP^I_{it} is the participant i's willingness to pay for the seeds sold by the incumbent of set of rounds t ($t \in \{1, 2\}$); E_{it} is an indicator of whether i was assigned to entry in t; Q_{im} is a measure of the quality distribution i was assigned to in t; D_{im} is an indicator of whether i was assigned to a high-dispersion incumbent in t; δ_i are individual fixed effects; φ_t are set of rounds fixed effects, and ξ_{it} is the error term. We cluster the standard errors at the session level.

When we use the willingness to pay for the entrant as the outcome variable, we are only able to estimate a cross-sectional model, as we observe this value only once for each participant. In particular, we estimate the following equation:

$$WTP_i^E = \vartheta_0 + \vartheta_1 E_{1,i} + \vartheta_2 E_{2,i} + \vartheta_3 Q_{1,i} + \vartheta_4 Q_{2,i} + \vartheta_5 D_{1,i} + \vartheta_6 D_{2,i} + X_i'\Gamma + \varsigma_i \tag{3.8}$$

where WTP_i^E is i's willingness to pay for the entrant; $E_{j,i}$ is an indicator of whether i was assigned to entry in set of rounds $j \in \{1, 2\}$; $Q_{j,i}$ is a measure of the average quality of the incumbent of set of rounds j; $D_{j,i}$ is an indicator of whether i was assigned to a high-dispersion incumbent in set of rounds j; X_i is a vector of individual controls, including the participant's age, risk aversion, time preferences, altruism, and indicators for the market center; and ς_i is the error term. Again, we cluster the standard errors at the session level.

3.5 Results

The entry of a high-quality seller boosts the demand for hybrid maize seeds when quality is unobserved to farmers ex-ante. Table 3.1 shows the effect of entry of such type of seller on the probability of choosing seeds. We find that participants assigned to a market with entry are 8.2 percentage points more likely to choose seeds over the alternative good; this represents a 13.5% increase in the probability of selecting seeds relative to the control mean. This result is robust to adding several markers of the initial quality distribution of the market to the base specification. This finding indicates that farmers who were discouraged from purchasing hybrid maize seeds due to the uncertainty in their quality decide to purchase it once a better alternative becomes available. Thus, the presence of a high-quality seller in the market may correct some of the inefficiencies that lead to a reduced demand for goods with unobserved quality ex-ante.

In addition to the presence of a high-quality seller, the market's underlying quality distribution also influences farmers' decisions to purchase hybrid maize seeds. Table 3.1 presents evidence on how the likelihood of choosing seeds varies with the market's initial quality distribution. In column 2, we find that participants assigned to a high-quality market are

Table 3.1: Effect of entry and market's quality distribution on likelihood of choosing seeds

Dep. var. : Indicator of whether respondents choose seeds					
				Beliefs	
	(1)	(2) Dummies	(3) Signals	(4) OLS	(5) 2SLS
Entry	0.082***	0.083***	0.083***	0.083***	0.083***
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Quality		0.034**	0.022***	0.021**	0.040***
		(0.014)	(0.008)	(0.009)	(0.014)
High-Dispersion		0.018	0.013	0.021	0.019
		(0.015)	(0.014)	(0.014)	(0.014)
No Entry Mean	0.605	0.605	0.605	0.605	0.605
First Stage F-stat					241.84
Participants	311	311	311	311	311
Observations	6220	6220	6220	6220	6220

Notes: Standard errors clustered at the session level in parenthesis. The dependent variable is an indicator of whether the participant chooses seeds in a particular round. The quality variable is different in each column. Column 2 uses an indicator of whether the incumbent is of high- or low-quality. Column 3 uses the standardized average of the number of black marbles drawn in the demonstration rounds. Columns 4 and 5 use the standardized average of the number of black marbles the participant believes he/she would take out in 20 imaginary rounds. In column 5, the average number of black marbles drawn in the demonstration rounds is used as instrument. F-statistic of first stage (only the instrument) is reported. All specifications include participant and round fixed effects. *** 1%, ** 5%, * 10% significance.

3.4 percentage points more likely to choose seeds than those in a low-quality market. This result suggests that markets for agricultural input markets—where farmers do not observe the quality of the inputs ex-ante—might be less likely to collapse when the incumbent stores are more likely to offer high-quality inputs.

Nonetheless, buyers typically do not observe the market's underlying quality distribution. As a result, they rely on their previous purchases from incumbent stores, which are merely draws from the quality distribution. To test this, we use the average number of black marbles drawn during the demonstration rounds as a measure of the quality observed by consumers. In column 3, we find that a one standard deviation increase in this measure raises the probability of choosing seeds by 3.3 percentage points. This result confirms that buyers use their past purchases of hybrid maize seeds to infer the market's underlying quality distribution, as these purchases are strong predictors of demand for such inputs. Additionally, farmers respond positively to good draws from the quality distribution, increasing their likelihood of purchasing hybrid maize seeds.

Another possibility is that buyers interpret their previous purchases from the incumbent as signals and use them to form beliefs about the market's quality distribution. We test this by examining the average number of black marbles participants expect to obtain in 20 hypothetical draws. Since participants' beliefs about subsequent draws from the initial quality distribution are endogenous, we use the average number of black marbles drawn during the demonstration rounds as an instrument. Table C.1 presents the first-stage results, providing suggestive evidence that participants rely on previous draws from the quality distribution to form expectations about the quantity of seeds they would receive in future purchases from the incumbent. Specifically, a one standard deviation increase in the average number of black marbles drawn during the demonstration rounds raises the average number of black marbles participants expect to obtain in subsequent rounds by 0.55 standard deviations. Additionally, we obtain a high F-statistic of 241.84, which exceeds the standard thresholds suggested in the literature, indicating that this is a strong instrument.

The results suggest that buyers' beliefs about the quality distribution influence the likelihood of purchasing hybrid maize seeds. In column 5, we find that a one standard deviation increase in the average number of black marbles participants expect to obtain in future draws raises the probability of choosing seeds by 4 percentage points. Intuitively, when buyers expect to obtain high-quality seeds from the existing sellers, they are more likely to select this good over the one with ex-ante known quality. Conversely, when buyers consider it is unlikely to obtain high-quality goods, they prefer to purchase the good with known quality.

The entry of the high-quality seller may also impact the existent sellers in the market. Buyers may decide to purchase hybrid maize seeds from the entrant rather than the incumbent stores. The experimental design allows us to test this by using an indicator of whether the participants select seeds offered by the incumbent as the dependent variable. Table 3.2

presents the results for this estimation. We find, in effect, that incumbent's sales drop after the high-quality seller enters the market. Specifically, participants assigned to a market with entry are 54.3 percentage points less likely to choose seeds from the incumbent, which represents an 89.7% decrease with respect to the control mean. This result reveals that once a better alternative is available in the market, nearly all buyers who were already purchasing seeds switch to the seller with a better quality distribution, reducing drastically incumbents' sales.

Table 3.2: Effect of entry and market's quality distribution on likelihood of choosing incumbent's seeds

Dep. var. : Indicator of whether respondents choose seeds from the incumbent					
				Beliefs	
	(1)	(2) Dummies	(3) Signals	(4) OLS	(5) 2SLS
Entry	-0.543***	-0.543***	-0.543***	-0.543***	-0.543***
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
Quality		0.022	0.014*	0.015*	0.026*
		(0.014)	(0.008)	(0.009)	(0.014)
High-Dispersion		0.014	0.011	0.016	0.015
		(0.015)	(0.015)	(0.015)	(0.015)
No Entry Mean	0.605	0.605	0.605	0.605	0.605
First Stage F-stat					241.84
Participants	311	311	311	311	311
Observations	6220	6220	6220	6220	6220

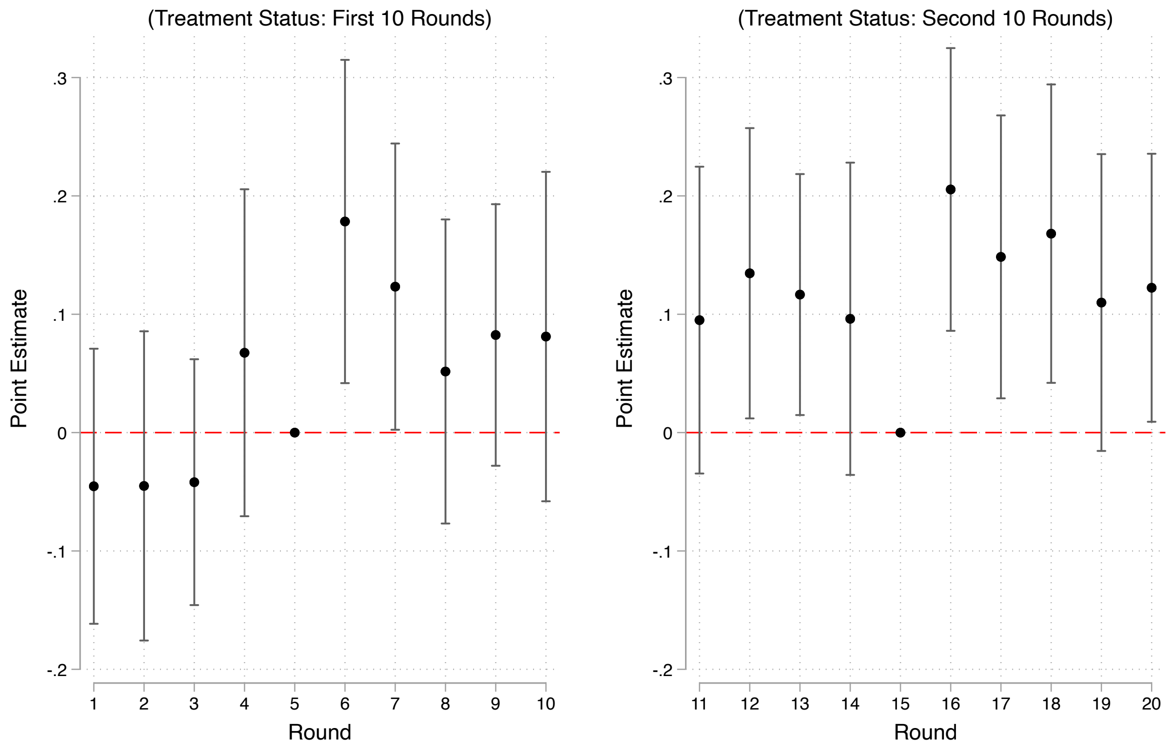
Notes: Standard errors clustered at the session level in parenthesis. All specifications include participant and round fixed effects. The dependent variable is an indicator of whether the participant chooses seeds in a particular round from the incumbent. The quality measurements are different in each column. Column 2 uses an indicator of whether the incumbent is of high- or low-quality. Column 3 uses the standardized average of the number of black marbles drawn in the demonstration rounds. Columns 4 and 5 use the standardized average of the number of black marbles the participants believes he/she would take out in 20 imaginary rounds. In column 5, the average of the number of black marbles drawn in the demonstration rounds is used as instrument. We report the F-statistics of the first stage (only the instrument). *** 1%, ** 5%, * 10% significance.

The experimental design also allows us to examine how buyers respond to entry over time. Figure 3.4 presents estimates from an event-study analysis for the first and last ten rounds of the game, separately. We find that the average treatment effect is primarily driven by the initial post-entry rounds. Specifically, participants are more likely to choose seeds in the first two to three rounds following entry; after that, the effect dissipates. In the second set of ten rounds, we observe that untreated participants are more likely to choose seeds than treated participants in round 15—a pattern that should not arise, given that treatment was randomly assigned and groups should be balanced in pre-entry rounds. To address this imbalance, Figure C.2 excludes round 15 from the analysis. After doing so, we find that the effect of entry is concentrated in the first post-entry round, consistent with the pattern observed in the first set of ten rounds.

These results suggest that buyers who were previously hesitant to purchase hybrid maize seeds due to uncertainty in quality tend to start buying them after the high-quality seller enters the market. However, this effect fades in subsequent rounds. This pattern could be explained by two possibilities: either treated participants received low-quality draws from the entrant early on—discouraging further seed purchases—or untreated participants obtained unexpectedly high-quality draws from the incumbent, which increased their likelihood of choosing seeds over the alternative good.

Additionally, we confirm that the randomization into treatment was successful, as there is no difference in the likelihood of choosing seeds between treated and untreated units before entry. To further validate our results, we conduct a falsification test in which we use the treatment assignment for the second (first) set of ten rounds to estimate its effect on the likelihood of choosing seeds in the first (second) ten rounds. Since treatment should only affect behavior within the assigned period, we expect the treatment status of the second ten rounds to have no impact on seed choices during the first ten rounds, and vice versa. Figure C.3 presents the estimates from this falsification event-study. As expected, we find that treatment assignment in the second ten rounds does not influence the likelihood of choosing seeds in the first ten rounds. Similarly, the treatment assignment for the first ten rounds has

Figure 3.4: Event-study estimates of the effect of entry on the likelihood of choosing seeds



Notes: Each dot represents the point estimate for that round in an event study estimation where the outcome variable is an indicator of whether participants chooses seeds and independent variables are indicators of whether participants had the chance to choose from the entrant in each round. Individual and round fixed effects included. Standard errors clustered at the session level. Lines represent 95% confidence intervals. Left panel shows the estimates for the first set of ten rounds. Right panel shows the estimates for the second set of ten rounds.

no effect on seed choices in the second ten rounds[4].

3.5.1 Heterogeneous effects by market's initial quality

The evidence presented so far demonstrates that the entry of a high-quality seller increases demand for hybrid maize seeds. The primary mechanism driving this result is that buyers who previously refrained from purchasing seeds due to quality uncertainty begin purchasing

[4]Figure C.4 presents the estimates for the second set of ten rounds, excluding round 15. We find that treatment status of the first ten rounds does not have an effect on the likelihood of choosing seeds in the post-entry rounds.

them once a seller offering higher quality seeds becomes available. Additionally, we find that buyers are more likely to purchase the agricultural input when they have previously obtained high-quality products or expect to receive high-quality products in future purchases from incumbent sellers.

These findings suggest that the impact of entry on demand for hybrid maize seeds may depend on the market's underlying quality distribution. The rationale behind this argument is that as the market's initial quality improves, fewer buyers refrain from purchasing seeds due to quality uncertainty. We test this hypothesis empirically by estimating the heterogeneous effects of entry based on the market's underlying quality distribution. Since buyers typically lack precise knowledge of the market's quality distribution, we also consider the quality they have observed and their beliefs about the quality they would receive in future purchases from incumbent sellers. Table C.2 presents the estimates for these heterogeneous effects on the probability of choosing seeds.

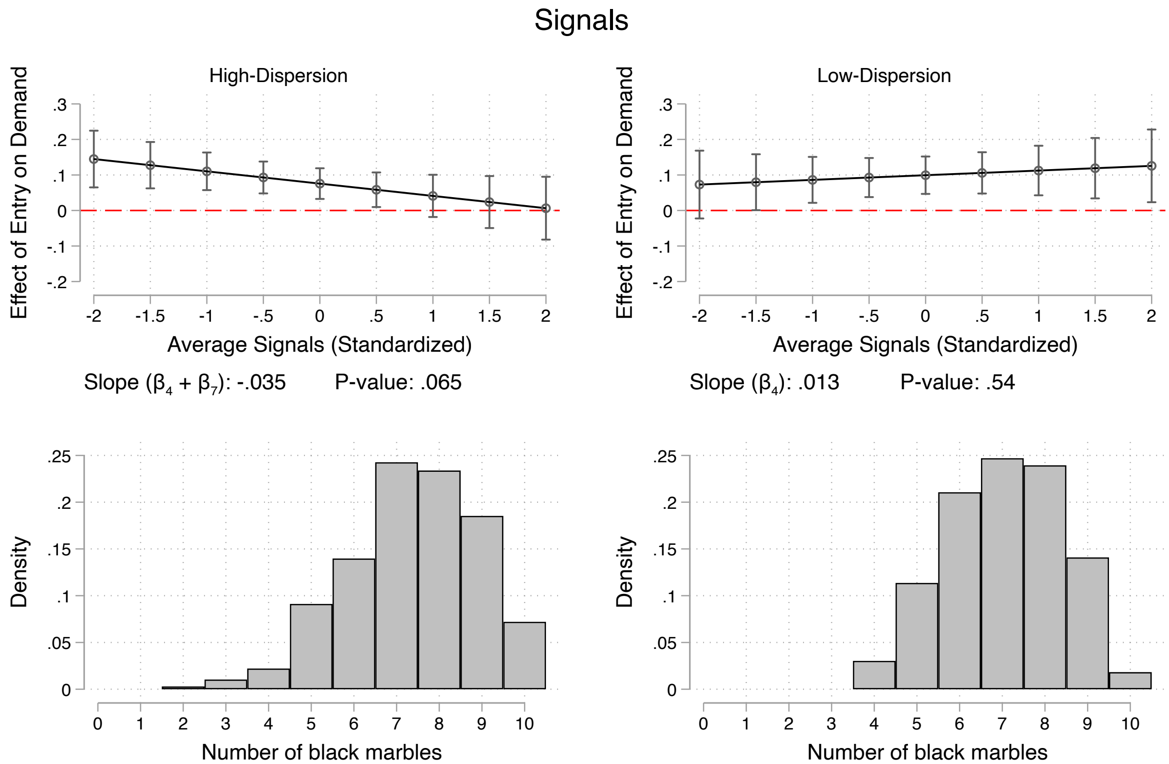
The effect of entry does not vary with the mean of the quality distribution. In high-dispersion markets, treated participants are more likely to choose seeds regardless of whether they face a high- or low-quality incumbent. However, these effects are not statistically different from each other (as indicated by $\beta_4 + \beta_7$ in column 1). A similar pattern emerges in low-dispersion markets, where entry increases the likelihood of participants choosing seeds in both high- and low-quality settings, but the effects are not statistically distinct (as indicated by β_4 in column 1). Since buyers are unaware of the market's true quality distribution, their response to entry may not depend on its actual mean, explaining the absence of a differential treatment effect by quality in both high- and low-dispersion markets.

Since buyers learn about the market's quality distribution through their previous purchases, the effect of entry may depend on the signals they receive. We test this hypothesis by using the average number of black marbles drawn during the demonstration rounds as a measure of the market's observed quality. Figure 3.5 presents the effects of entry across different levels of the standardized average number of black marbles. In high-dispersion markets, we find that the effect of entry on demand decreases as the average number of black marbles drawn during the demonstration rounds increases. Specifically, a one standard deviation increase in the average number of black marbles drawn during the demonstration rounds reduces the effect of entry by 3.5 percentage points. Conversely, we find no evidence of such variation in low-dispersion markets. These results indicate that the entry of a new seller has a greater impact on the demand for hybrid maize seeds when buyers have previously encountered low-quality products from the incumbent—a scenario more common in high-dispersion markets. In this context, entry represents a significant improvement in the market's perceived quality. As a result, buyers who were previously hesitant to purchase seeds due to quality uncertainty now do so because a more reliable option is available.

As discussed earlier, buyers use previous draws as signals to form beliefs about the market's quality distribution. We also examine whether the effect of entry varies based on buyers' quality beliefs. Figure 3.6 presents the effects of entry across different levels of the standardized average number of black marbles participants expect to obtain in 20 hypothetical rounds. We find that the effect of entry decreases as buyers perceive a higher likelihood of obtaining high-quality products in future purchases. Specifically, in high-dispersion markets, a one standard deviation increase in the expected average number of black marbles reduces the effect of entry by 5.8 percentage points. In contrast, we find no evidence of a differential effect of entry based on buyers' beliefs in low-dispersion markets. One possible explanation for this pattern is that participants in high-dispersion markets place greater weight on their beliefs than on the quality signals they observe. As a result, when participants believe that the incumbent offers low average quality, they perceive the entry of a high-quality seller as a meaningful improvement in the market—particularly in high-dispersion markets where beliefs play a larger role, as opposed to low-dispersion markets where observed quality signals carry more weight.

These findings align with the results based on observed quality signals, suggesting that buyers respond more strongly to the entry of a new seller when they believe the market's av-

Figure 3.5: Marginal effects of entry by observed quality on the likelihood of choosing seeds



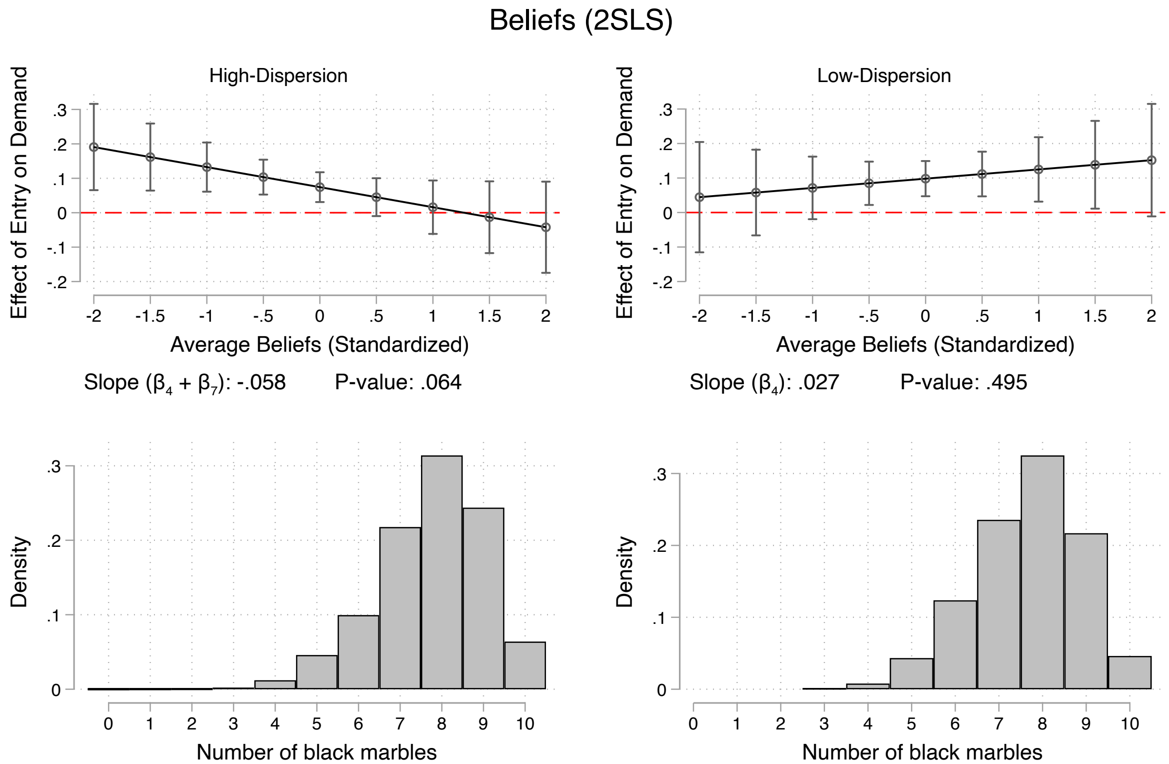
Notes: Upper panels present the marginal effects of entry on the probability of choosing seeds by the standardized average number of black marbles drawn during the demonstration rounds on high-dispersion (left) and low-dispersion (right) markets, based on the regression results shown in table C.2 in column 2. The estimate for the slope ($\beta_4 + \beta_7$ for high-dispersion and β_4 for low-dispersion markets) and its p-value are reported. Lines indicate confidence intervals at the 95%. Bottom panels present the distribution of black marbles drawn during the demonstration rounds in high- and low-dispersion markets.

erage quality to be low. When buyers expect the incumbent to provide low-quality products, the entry of a high-quality seller represents a significant improvement from their perspective. Consequently, buyers who hold beliefs consistent with a low-quality market are more likely to respond to the new seller's entry.

3.5.2 Mechanism: Willingness-to-pay for the entrant's good

Our findings so far indicate that the entry of a high-quality seller increases demand for hybrid maize seeds when quality is unobserved ex-ante, with a stronger effect in markets

Figure 3.6: Marginal effects of entry by expected quality on the likelihood of choosing seeds

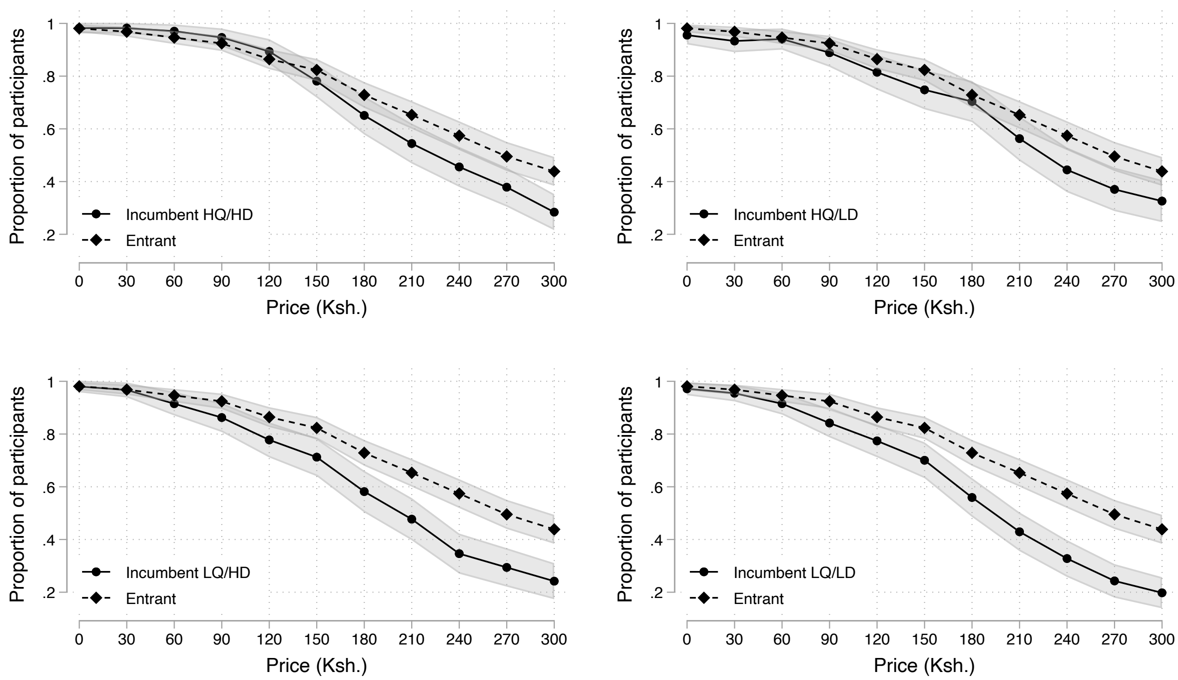


Notes: Upper panels present the marginal effects of entry on the probability of choosing seeds by the standardized average number of black marbles participants expect to drawn in 20 hypothetical rounds on high-dispersion (left) and low-dispersion (right) markets, based on the regression results shown in table C.2 in column 4. The estimate for the slope ($\beta_4 + \beta_7$ for high-dispersion and β_4 for low-dispersion markets) and its p-value are reported. Lines indicate confidence intervals at the 95%. Bottom panels present the distribution of black marbles drawn during the demonstration rounds in high- and low-dispersion markets.

where the initial underlying quality distribution is low. One possible mechanism driving these results is that buyers may place value on having a higher-quality option available in their local markets. In this section, we examine the extent to which buyers value the presence of a high-quality seller and how their willingness-to-pay for hybrid maize seeds offered by incumbent sellers varies depending on the market's quality distribution. To investigate this, we analyze participants' elicited willingness-to-pay for each good they encounter during the game.

We begin by examining how participants' willingness-to-pay for the entrant's seeds compares to that for the incumbent's seeds, depending on the market's quality distribution. Figure 3.7 shows the proportion of participants willing to purchase hybrid maize seeds at each price level in the elicitation exercise. We find that participants consistently exhibit a higher willingness-to-pay for the entrant's good than for the incumbent's. Moreover, this gap widens as the price increases, suggesting that demand for the incumbent's seeds is more elastic than demand for the entrant's. Additionally, the gap is larger when the incumbent offers lower-quality goods, indicating that the market's underlying quality distribution also influences buyers' willingness-to-pay for the incumbent's product.

Figure 3.7: Elicited willingness-to-pay for incumbent and entrant's seeds



Notes: Each point represents the proportion of participants who stated they would purchase seeds from a specific seller at each price asked during the willingness-to-pay elicitation. Confidence intervals are reported at the 95% level and obtained from the standard error for the mean for each price. Dashed lines correspond to entrant seller's seeds, while solid lines correspond to the incumbent seller's seeds. Upper left panel presents the demand curve for high-quality/high-dispersion incumbent. Upper right panel presents the demand curve for high-quality/low-dispersion incumbent. Bottom left panel presents the demand curve for low-quality/high-dispersion incumbent. Bottom right panel presents the demand curve for low-quality/low-dispersion incumbent.

As we have argued earlier, participants may take into account the quality they have observed through previous purchases and the beliefs they form through these signals to decide the value they assign to hybrid maize seeds, as they do not observe the market's quality distribution. For that reason, we explore whether signals observed by participants and their corresponding beliefs impact participants' willingness-to-pay for the seeds sold by the incumbent. Table 3.3 presents the estimates of a panel regression of the willingness-to-pay for the incumbent's seeds on markers of the market's underlying quality distribution.

Table 3.3: Effect of entry and market's quality distribution on willingness-to-pay for incumbent's seeds

Dependent variable: WTP for incumbent's seeds					
				Beliefs	
	(1)	(2) Dummies	(3) Signals	(4) OLS	(5) 2SLS
Entry	-1.91	-1.69	-1.89	-1.81	-1.86
	(4.42)	(4.41)	(4.42)	(4.63)	(4.77)
Quality		21.61***	11.52***	14.71***	21.22***
		(4.22)	(2.30)	(3.15)	(4.06)
High-Dispersion		3.55	1.56	5.58	4.90
		(4.52)	(4.54)	(4.43)	(4.47)
Mean Dep. Var.	196.18	196.18	196.18	196.18	196.18
First Stage F-stat					228.21
Participants	317	317	317	317	317
Observations	634	634	634	634	634

Notes: Standard errors clustered at the session level in parentheses. All specifications include participant and round fixed effects. The dependent variable is the participant's willingness-to-pay for the seeds sold by the incumbent. The average quality measurements are different in each column. Column 2 uses an indicator of whether the incumbent is of high- or low-quality. Column 3 uses the standardized average number of black marbles drawn in the demonstration rounds. Columns 4 and 5 use the standardized average number of black marbles the participants believe he/she would take out in 20 imaginary rounds. In column 5, the standardized average number of black marbles drawn in the demonstration rounds is used as an instrument. *** 1%, ** 5%, * 10% significance.

Buyers are willing to pay more for the incumbent's seeds when they have observed high-quality products sold by the incumbent or when they expect to receive high-quality seeds from it in the future. Additionally, participants' valuation of the incumbent's seeds is not influenced by whether they had the option to purchase hybrid maize seeds from the entrant. First, we find that participants' willingness to pay for the incumbent's good is 11% higher in high-quality markets, consistent with the patterns observed in the raw willingness-to-pay data. Moreover, willingness to pay for the incumbent's seeds increases when buyers have previously purchased high-quality seeds or expect to receive high-quality seeds in the future. Specifically, a one standard deviation increase in the average number of black marbles drawn during the demonstration rounds raises participants' willingness to pay for the incumbent's seeds by 5.9%. Likewise, a one standard deviation increase in the average number of black marbles participants expect to obtain in future hypothetical rounds increase their willingness to pay by 10.8%. These findings confirm that buyers are willing to pay more for hybrid maize seeds with unobserved ex-ante quality as the market's underlying quality distribution improves, leaving less room for the entrant to enhance the perceived quality distribution upon entry.

The results presented earlier should be interpreted with caution, as the estimates may be biased due to the right-censoring of the willingness-to-pay data at Ksh. 300 (see Figure C.5). To address this issue, we estimate Tobit models to correct for censoring and present the results in Table C.3[5]. The results of our preferred specification remain quantitatively robust under Tobit estimation, with the only difference being that the latter method yields less precise estimates. Additionally, OLS appears to underestimate the effects of quality markers on participants' willingness to pay, suggesting that our estimates may represent a lower bound of the true effect[6]. Furthermore, the cross-sectional estimates suggest that the observed effects may be primarily driven by the second incumbent. A possible explanation is that participants interacted with this incumbent more recently, making it easier for them to recall both their quality signals and beliefs associated with this seller.

The market's underlying quality distribution may also influence buyers' willingness to

[5]Since Tobit estimation with panel data produces biased and inconsistent estimators when fixed effects are included due to the incidental parameters problem (Lancaster, 2000), we estimate the Tobit model using cross-sectional data for each incumbent separately.

[6]For comparison, Table C.4 presents cross-sectional OLS estimates.

pay for the entrant's seeds, as they may be more willing to pay for high-quality hybrid maize seeds when the only low-quality incumbents are available. To test this, we estimate the effect of various markers of the market's quality distribution on participants' willingness to pay for the entrant's seeds. Since each participant's willingness to pay for the entrant's seeds is observed only once, we use cross-sectional data and put all variables for the two sets of ten rounds in the same specification. Table 3.4 presents the results.

Table 3.4: Effect of entry and market's quality distribution on willingness-to-pay for entrant's seeds

Dependent variable: WTP for entrant's seeds				
			Beliefs	
	(1) Dummies	(2) Signals	(3) OLS	(4) 2SLS
Entry (1st set)	4.69	4.68	5.00	4.20
	(8.13)	(8.12)	(8.04)	(7.96)
Entry (2nd set)	-3.24	-3.30	-2.73	-3.79
	(8.76)	(8.81)	(8.72)	(8.66)
Quality (1st set)	-0.80	-2.44	-3.58	-5.74
	(10.87)	(5.29)	(4.76)	(7.03)
Quality (2nd set)	13.05	3.84	-5.76	6.91
	(11.11)	(5.51)	(4.67)	(11.30)
High-Dispersion (1st set)	6.29	6.37	5.68	5.31
	(10.20)	(10.13)	(10.34)	(10.02)
High-Dispersion (2nd set)	5.75	4.69	4.89	5.61
	(10.22)	(10.56)	(10.48)	(10.17)
Mean Dep. Var.	201.58	201.58	201.58	201.58
Observations	317	317	317	317

Notes: Standard errors clustered at the session level in parentheses. The dependent variable is the participant's willingness-to-pay for the entrant's seeds. The average quality measurements are different in each column. Column 1 uses an indicator of whether the incumbent is of high- of low-quality. Column 2 uses the standardized average number of black marbles drawn in the demonstration rounds. Columns 3 and 4 use the standardized average number of black marbles the participants believe he/she would take out in 20 imaginary rounds. In column 5, the standardized average number of black marbles drawn in the demonstration rounds is used as an instrument. First-stage robust F-statistics reported. All specifications use individuals' measurements for risk aversion, time preferences, altruism, the participant's age, and an market center fixed effects as controls. *** 1%, ** 5%, * 10% significance.

We find that buyers' willingness to pay for the entrant's seeds does not vary with the market's underlying quality distribution. Moreover, participants do not adjust their willingness to pay for the entrant's seeds based on the quality of previous draws from the incumbent's quality distribution. Similarly, buyers' beliefs about the market's quality distribution do not influence their willingness to pay for the entrant's good. These results should be interpreted with caution, as censoring in the willingness-to-pay data may lead to biased and inconsistent estimates. To address this issue, we estimate Tobit models to correct for censoring and present the results in Table C.5. As observed previously, OLS appears to underestimate the effect of the market's quality distribution on participant's willingness to pay. In this case, while the estimated effects increase in magnitude under Tobit estimation, they remain statistically insignificant. These findings suggest that the differential effect of entry by the market's underlying quality distribution is likely driven by a lower valuation of low-quality goods available in the market rather than a higher willingness to pay for high-quality goods in low-quality markets.

Finally, we test whether the market's quality distribution influences the gap between the buyers' willingness to pay for the entrant's and incumbent's seeds. We estimate the effect using cross-sectional data for the two markets separately, considering the incumbent corresponding to each market to calculate the difference. Table 3.5 presents the results.

The gap in willingness to pay is smaller in high-quality markets, consistent with the patterns observed in the raw willingness-to-pay data. Additionally, this gap decreases when buyers previously purchased high-quality products from the incumbent and when they expect to obtain high-quality products from this seller in the future. However, these results should be interpreted with caution due to measurement error in the outcome variable. Specifically, a zero gap between willingness to pay for the entrant's and the incumbent's seeds does not necessarily indicate that buyers are willing to pay the same price for both. Since both willingness-to-pay values are right censored, some zero gaps may simply reflect that participants' actual willingness to pay remains unobserved in both cases.

Our findings indicate that buyers' willingness to pay for hybrid maize seeds increases as

Table 3.5: Effect of entry and market's quality distribution on the difference between willingess-to-pay for entrant and incumbent

Dep. var. : Difference between entrant's and incumbent's WTPs	Market 1			Market 2		
	(1) Dummies	(2) Signals	(3) Beliefs	(4) Dummies	(5) Signals	(6) Beliefs
Entry	9.10	9.19	9.53	-3.68	-3.89	0.08
	(7.35)	(7.34)	(7.07)	(7.69)	(7.65)	(7.99)
Quality	-17.09**	-7.61*	-11.21**	-28.56***	-13.71***	-35.49***
	(8.12)	(4.02)	(5.39)	(6.74)	(2.64)	(6.74)
High-Dispersion	-8.84	-7.65	-9.75	5.72	8.12	3.53
	(7.23)	(7.68)	(6.73)	(5.69)	(5.67)	(6.86)
Mean Dep. Var.	24.98	24.98	24.98	35.77	35.77	35.77
First Stage F-stat			228.20			52.01
Observations	317	317	317	317	317	317

Notes: Standard errors clustered at the session level in parentheses. The dependent variable is the difference between the participant's willingness-to-pay for the seeds sold by the entrant and the incumbent. The average quality measurements are different in each column. Column 1 and 4 use an indicator of whether the incumbent is of high- or low-quality. Column 2 and 5 use the standardized average number of black marbles drawn in the demonstration rounds. Columns 3 and 6 use the standardized average number of black marbles the participants believe he/she would take out in 20 imaginary rounds, where the standardized average number of black marbles drawn in the demonstration rounds is used as instrument. First-stage robust F-statistics reported. All specifications use individuals' measurements for risk aversion, time preferences, altruism, the participant's age, and an market center fixed effects as controls. *** 1%, ** 5%, * 10% significance.

the market's underlying quality distribution improves. Moreover, buyers' valuations follow a similar pattern when considering either the quality signals they observe or their beliefs about the market's quality distribution. These findings are critical for understanding why markets for agricultural inputs where quality is unobserved ex-ante may exhibit low demand—buyers may be unwilling to pay the market price when there is a risk of receiving low-quality products. Consequently, demand for these goods may rise when incumbents offer high-quality products or when a new seller enters the market and improves the overall quality distribution. In such cases, the gap between the market price and buyers' willingness to pay narrows, increasing the number of buyers willing to purchase hybrid maize seeds.

3.6 Conclusion

This chapter provides experimental evidence on how the entry of a high-quality seller affects demand for hybrid maize seeds when farmers do not observe quality ex-ante. Using a lab-in-the-field experiment with maize farmers in rural Kenya, we find that the presence of a high-quality entrant increases demand for hybrid maize seeds by 14%, particularly among farmers who previously refrained from purchasing due to quality uncertainty. Our results suggest that improving consumers' beliefs about product quality can significantly enhance demand, mitigating market inefficiencies associated with unobserved product quality.

Moreover, we document that the effect of entry is stronger in markets where buyers have previously encountered low-quality goods or expect to receive low-quality products from incumbent sellers. In such cases, the introduction of a seller with a more reliable quality distribution represents a substantial improvement, encouraging hesitant buyers to enter the market. In contrast, when buyers have observed high-quality signals or hold optimistic beliefs about future purchases, the impact of entry diminishes.

These findings contribute to the broader literature on competition and agricultural input markets by highlighting the role of quality perceptions in shaping demand. While previous research has examined the effects of reputation-building and third-party verification, our study emphasizes the importance of direct market interventions—such as the entry of a high-quality seller—in addressing quality uncertainty.

Our results have important policy implications for agricultural markets in developing countries. Interventions that increase the availability of verifiably high-quality inputs, such as accreditation systems, certification programs, or market entry incentives for reputable sellers, could play a key role in enhancing farmers' adoption of productivity-enhancing inputs. Future research could explore how long-lasting these demand shifts are and whether they translate into broader changes in market dynamics, including pricing, competition, and sellers' incentives to improve product quality.

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Appendix A

Supplemental Materials for Chapter 1

Table A.1: Landmine exposure within villages

	(1)	Prop. of exposed inds.		
	Prop. villages with exposure	(2) Minimum	(3) Median	(4) Maximum
Year: 2010				
Before survey	0.036	0.036	0.146	0.852
(0-6] months	0.138	0.028	0.650	1.000
(6-12] months	0.054	0.024	0.113	0.600
(12-36] months	0.125	0.040	0.436	1.000
Year: 2013				
Before survey	0.000	.	.	.
(0-6] months	0.036	0.267	0.767	1.000
(6-12] months	0.027	0.051	0.945	1.000
(12-36] months	0.089	0.118	0.967	1.000
Year: 2016				
Before survey	0.031	0.083	0.667	1.000
(0-6] months	0.054	0.120	1.000	1.000
(6-12] months	0.031	0.083	0.939	1.000
(12-36] months	0.112	0.033	1.000	1.000

Notes: Column 1 reports the proportion of villages with at least one individual exposed to landmines within 5 km of their residence during each time window. Column 2 reports the lowest proportion of exposed individuals observed among villages with at least one exposed individual. Column 3 reports the median proportion of exposed individuals across these villages. Column 4 shows the highest proportion of exposed individuals among these villages.

Table A.2: Effects of landmine events on labor allocation outside own farm (4 km buffer)

	(1) If worked off own farm	(2) If worked jornalero	(3) If worked non-jornalero	(4) Hours worked non-jornalero
If at least one landmine event in the period...				
Before survey	0.016	-0.008	0.009	0.858
	(0.040)	(0.044)	(0.043)	(2.424)
(0-6] months	0.003	0.026	-0.042*	-2.651**
	(0.024)	(0.019)	(0.025)	(1.123)
(6-12] months	0.033	0.023	0.009	1.422
	(0.044)	(0.036)	(0.030)	(1.698)
(12-36] months	0.019	0.009	0.017	0.672
	(0.020)	(0.017)	(0.021)	(0.831)
Dep Var Mean	0.397	0.195	0.232	8.827
# Units	5,510	5,510	5,510	5,510
# Clusters	224	224	224	224
Observations	16,530	16,530	16,530	16,530

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 4 km from its residence in the specified windows. Sample includes household heads and their spouses when they have one. *If worked off own farm* is whether individuals worked outside the household's agricultural fields in the last week. *If worked jornalero* is whether individuals worked as jornaleros (agricultural day laborers) in the past week. *If worked non-jornalero* is whether individuals had non-jornalero jobs. *Hours worked non-jornalero* is the number of hours worked on non-jornalero jobs per week and is winsorized at the top 1%. The probability values of *If worked jornalero* and *If worked non-jornalero* do not necessarily sum to the value of *If worked off own farm*, as an individual can engage in both types of jobs simultaneously. All specifications include individual/household and year fixed effects and municipality characteristics interacted with indicators of each year of the survey. Municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.3: Effects of landmine events on labor allocation outside own farm (6 km buffer)

	(1) If worked off own farm	(2) If worked jornalero	(3) If worked non-jornalero	(4) Hours worked non-jornalero
If at least one landmine event in the period...				
Before survey	-0.044	0.002	-0.059	-1.558
	(0.033)	(0.034)	(0.039)	(1.855)
(0-6] months	0.002	0.027*	-0.048**	-3.085***
	(0.019)	(0.016)	(0.021)	(0.870)
(6-12] months	0.040	0.006	0.037*	1.684
	(0.026)	(0.025)	(0.021)	(1.370)
(12-36] months	0.030	-0.015	0.059***	2.563***
	(0.026)	(0.021)	(0.021)	(0.791)
Dep Var Mean	0.397	0.195	0.232	8.827
# Units	5,510	5,510	5,510	5,510
# Clusters	224	224	224	224
Observations	16,530	16,530	16,530	16,530

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 6 km from its residence in the specified windows. Sample includes household heads and their spouses when they have one. *If worked off own farm* is whether individuals worked outside the household's agricultural fields in the last week. *If worked jornalero* is whether individuals worked as jornaleros (agricultural day laborers) in the past week. *If worked non-jornalero* is whether individuals had non-jornalero jobs. *Hours worked non-jornalero* is the number of hours worked on non-jornalero jobs per week and is winsorized at the top 1%. The probability values of *If worked jornalero* and *If worked non-jornalero* do not necessarily sum to the value of *If worked off own farm*, as an individual can engage in both types of jobs simultaneously. All specifications include individual/household and year fixed effects and municipality characteristics interacted with indicators of each year of the survey. Municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.4: Effects of landmine events on own farm labor (4 km buffer)

	If worked on own farm for				(5)
	(1) > 0 hr	(2) ≥ 1 hr	(3) ≥ 2 hr	(4) ≥ 4 hr	If hired labor
If at least one landmine event in the period ...					
Before survey	0.020	0.013	0.022	0.014	0.135**
	(0.056)	(0.047)	(0.055)	(0.058)	(0.058)
(0-6] months	-0.030	-0.034	-0.028	-0.006	0.067**
	(0.028)	(0.028)	(0.026)	(0.022)	(0.034)
(6-12] months	-0.060*	-0.069*	-0.068*	-0.031	-0.071
	(0.034)	(0.041)	(0.040)	(0.039)	(0.055)
(12-36] months	-0.059**	-0.044**	-0.040	-0.035*	0.059*
	(0.023)	(0.022)	(0.026)	(0.021)	(0.035)
Dep Var Mean	0.455	0.424	0.364	0.258	0.335
Sample	Ind.	Ind.	Ind.	Ind.	HH
# Individuals	5,485	5,485	5,485	5,485	3,213
# Clusters	224	224	224	224	224
Observations	16,455	16,455	16,455	16,455	9,639

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 4 km from its residence in the specified windows. In columns 1 through 4, sample includes household heads and their spouses when they have one. *If worked on own farm for ...* is whether household members spent more than 0 hours, or greater than or equal to 1, 2 or 4 hours per day on agricultural tasks on farms that households own. *If hired labor* indicates whether households have hired jornaleros (agricultural day laborers) in the past 12 months. All specifications include individual/household and year fixed effects and municipality characteristics interacted with indicators of each round of the survey. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.5: Effects of landmine events on own farm labor (6 km buffer)

	If worked on own farm for				(5)
	(1) > 0 hr	(2) ≥ 1 hr	(3) ≥ 2 hr	(4) ≥ 4 hr	If hired labor
If at least one landmine event in the period ...					
Before survey	-0.020	-0.044	-0.018	0.019	0.158***
	(0.049)	(0.043)	(0.045)	(0.045)	(0.054)
(0-6] months	-0.070***	-0.069***	-0.059***	-0.033*	0.048*
	(0.023)	(0.022)	(0.022)	(0.019)	(0.027)
(6-12] months	-0.025	-0.023	-0.053	-0.032	-0.052
	(0.032)	(0.031)	(0.033)	(0.032)	(0.032)
(12-36] months	-0.004	-0.011	-0.007	-0.016	0.020
	(0.023)	(0.024)	(0.025)	(0.023)	(0.039)
Dep Var Mean	0.455	0.424	0.364	0.258	0.335
Sample	Ind.	Ind.	Ind.	Ind.	HH
# Individuals	5,485	5,485	5,485	5,485	3,213
# Clusters	224	224	224	224	224
Observations	16,455	16,455	16,455	16,455	9,639

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 6 km from its residence in the specified windows. In columns 1 through 4, sample includes household heads and their spouses when they have one. *If worked on own farm for ...* is whether household members spent more than 0 hours, or greater than or equal to 1, 2 or 4 hours per day on agricultural tasks on farms that households own. *If hired labor* indicates whether households have hired jornaleros (agricultural day laborers) in the past 12 months. All specifications include individual/household and year fixed effects and municipality characteristics interacted with indicators of each round of the survey. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.6: Effects of landmine events on labor allocation outside own farm (donut analysis)

	(1) If worked off own farm	(2) If worked jornalero	(3) If worked non-jornalero	(4) Hours worked non-jornalero
Inner circle [0-5 km]				
If event before survey	0.044	0.028	0.008	1.092
	(0.044)	(0.043)	(0.042)	(2.033)
If event in (0-6] months	0.003	0.030	-0.046*	-3.440***
	(0.023)	(0.020)	(0.024)	(0.995)
If event in (6-12] months	0.002	-0.001	0.002	0.630
	(0.030)	(0.027)	(0.027)	(1.642)
If event in (12-36] months	0.036	-0.002	0.048**	2.206***
	(0.026)	(0.023)	(0.019)	(0.789)
Inner ring (5-10 km]				
If event before survey	0.019	0.007	0.015	1.138
	(0.033)	(0.019)	(0.031)	(1.158)
If event in (0-6] months	-0.019	-0.007	-0.025	-1.669**
	(0.021)	(0.014)	(0.019)	(0.745)
If event in (6-12] months	-0.041*	-0.025	-0.002	-1.070
	(0.023)	(0.019)	(0.023)	(0.844)
If event in (12-36] months	0.005	-0.011	0.029*	1.324*
	(0.017)	(0.014)	(0.015)	(0.712)
Outer ring (10-20 km]				
If event before survey	0.010	0.004	-0.001	0.241
	(0.019)	(0.013)	(0.017)	(0.733)
If event in (0-6] months	-0.013	-0.024**	-0.004	0.546
	(0.016)	(0.011)	(0.016)	(0.679)
If event in (6-12] months	-0.034	-0.001	-0.037*	-1.874**
	(0.021)	(0.013)	(0.020)	(0.749)
If event in (12-36] months	0.026*	0.010	0.023	1.167*
	(0.015)	(0.013)	(0.015)	(0.634)
Dep Var Mean	0.397	0.195	0.232	8.827
# Units	5,510	5,510	5,510	5,510
# Clusters	224	224	224	224
Observations	16,530	16,530	16,530	16,530

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event (i) within 5 km, (ii) 5 to 10 km, (iii) 10 to 20 km from its residence in the specified windows. Sample includes household heads and their spouses when they have one. *If worked off own farm* is whether individuals worked outside the household's agricultural fields in the last week. *If worked jornalero* is whether individuals worked as jornaleros (agricultural day laborers) in the past week. *If worked non-jornalero* is whether individuals had non-jornalero jobs. *Hours worked non-jornalero* is the number of hours worked on non-jornalero jobs per week and is winsorized at the top 1%. The probability values of *If worked jornalero* and *If worked non-jornalero* do not necessarily sum to the value of *If worked off own farm*, as an individual can engage in both types of jobs simultaneously. All specifications include individual/household and year fixed effects and municipality characteristics at baseline interacted with year FE. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.7: Effect of landmine events on labor allocation outside own farm by previous exposure

	(1) If worked off own farm	(2) If worked jornalero	(3) If worked non-jornalero	(4) Hours worked non-jornalero
If at least one landmine event in the period...				
Before survey	0.054	0.018	-0.002	-0.529
	(0.076)	(0.077)	(0.041)	(2.340)
(0-6] months	-0.011	0.029	-0.064**	-3.577***
	(0.027)	(0.020)	(0.027)	(1.013)
(6-12] months	0.018	-0.047	0.061	3.683
	(0.052)	(0.036)	(0.050)	(2.722)
(12-36] months	0.037	0.004	0.025	0.764
	(0.049)	(0.031)	(0.029)	(1.196)
Since 2002 until 36 mos.	0.008	-0.028	0.033	1.441
	(0.032)	(0.026)	(0.022)	(0.877)
If events since 2002 until 36 mos. × If events ...				
... Before survey	-0.023	-0.011	0.040	2.818
	(0.077)	(0.066)	(0.045)	(2.636)
... (0-6] months	0.070*	-0.013	0.097***	3.428**
	(0.038)	(0.030)	(0.037)	(1.471)
... (6-12] months	-0.009	0.065*	-0.080*	-4.066
	(0.052)	(0.037)	(0.047)	(2.774)
... (12-36] months	-0.035	0.000	-0.007	0.181
	(0.058)	(0.038)	(0.039)	(1.507)
Linear combs. (If events in time window + If events in time window × Exposed before)				
Before survey	0.030	0.007	0.037	2.289
(0-6] months	0.059*	0.016	0.034	-0.149
(6-12] months	0.009	0.018	-0.018	-0.383
(12-36] months	0.002	0.004	0.017	0.945
Dep Var Mean	0.397	0.195	0.232	8.827
# Units	5,510	5,510	5,510	5,510
# Clusters	224	224	224	224
Observations	16,530	16,530	16,530	16,530

Notes: Standard errors clustered at the village level in parenthesis. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Last four regressors refer to interactions with an indicator of whether household experienced a landmine event since 2002 until 36 months prior to March 1 of the year the household was surveyed. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. Sample includes heads and their spouses when they have one. Hours worked per week excluding agricultural daily laborers winsorized at the top 1%. All specifications include individual and year fixed effects, and municipality characteristics interacted with indicators for each round of the survey. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.8: Effects of landmine events on hours of labor allocated outside of own farm by previous exposure

	Hours worked in the past week		
	(1) Agriculture (non-jornalero)	(2) Non-agric. (non-jornalero)	(3) Jornalero
If at least one landmine event in the period...			
Before survey	1.804	0.931	2.243
	(1.966)	(1.793)	(1.596)
(0-6] months	-3.933***	-0.515	1.573
	(1.391)	(1.616)	(1.644)
(6-12] months	5.914**	4.039	-2.993
	(2.841)	(2.685)	(1.893)
(12-36] months	2.566	-0.211	2.268
	(1.861)	(1.244)	(1.548)
Since 2002 until 36 mos.	3.376**	2.095*	1.291
	(1.349)	(1.177)	(1.230)
If events since 2002 until 36 mos. × If events ...			
... Before survey	0.000	0.000	0.000
	(.)	(.)	(.)
... (0-6] months	3.467*	0.036	-1.155
	(2.043)	(2.140)	(2.031)
... (6-12] months	-6.433***	-4.077*	2.003
	(2.468)	(2.318)	(2.111)
... (12-36] months	-0.439	0.082	-0.636
	(2.130)	(1.505)	(1.775)
Linear combs. (If events in period + If events in period × Exposed before)			
Before survey	1.804	0.931	2.243
(0-6] months	-0.466	-0.478	0.418
(6-12] months	-0.519	-0.039	-0.990
(12-36] months	2.127**	-0.129	1.632
Dep Var Mean	3.504	6.305	5.853
# Units	5,510	5,510	5,510
# Clusters	224	224	224
Observations	11,020	11,020	11,020

Notes: Standard errors clustered at the village level in parenthesis. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Last four regressors refer to interactions with an indicator of whether household experienced a landmine event since 2002 until 36 months prior to March 1 of the year the household was surveyed. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. Sample includes heads and their spouses when they have one and the two last rounds of the survey. All specifications include individual and year fixed effects, and municipality characteristics interacted with indicators for each round of the survey. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.9: Effects of landmine events on own farm labor by previous exposure

	If worked on own farm for				(5)
	(1) > 0 hr	(2) ≥ 1 hr	(3) ≥ 2 hr	(4) ≥ 4 hr	If hired labor
If at least one landmine event in the period...					
Before survey	0.018	-0.011	-0.002	-0.009	0.256**
	(0.041)	(0.034)	(0.056)	(0.042)	(0.104)
(0-6] months	-0.032	-0.027	-0.004	0.026	0.064*
	(0.037)	(0.036)	(0.038)	(0.030)	(0.038)
(6-12] months	-0.009	0.010	0.005	0.049**	-0.138**
	(0.029)	(0.031)	(0.037)	(0.020)	(0.065)
(12-36] months	-0.070*	-0.074*	-0.058	-0.046	0.063
	(0.036)	(0.038)	(0.038)	(0.036)	(0.047)
Since 2002 until 36 mos.	0.062*	0.078**	0.059*	0.053*	-0.041
	(0.034)	(0.031)	(0.031)	(0.028)	(0.031)
If events since 2002 until 36 mos. × If events...					
... Before survey	-0.022	0.004	0.027	0.038	-0.070
	(0.052)	(0.060)	(0.074)	(0.052)	(0.135)
... (0-6] months	-0.031	-0.025	-0.057	-0.073*	-0.004
	(0.055)	(0.051)	(0.053)	(0.039)	(0.053)
... (6-12] months	-0.058	-0.091*	-0.120**	-0.135***	0.091
	(0.043)	(0.049)	(0.055)	(0.042)	(0.099)
... (12-36] months	0.053	0.054	0.060	0.043	-0.026
	(0.047)	(0.045)	(0.043)	(0.038)	(0.047)
Linear combs. (If events in period + If events in period × Exposed before)					
Before survey	-0.004	-0.008	0.025	0.030	0.185**
(0-6] months	-0.063	-0.052	-0.061	-0.047*	0.060
(6-12] months	-0.067**	-0.081**	-0.115***	-0.086**	-0.047
(12-36] months	-0.017	-0.020	0.003	-0.003	0.037
Dep Var Mean	0.455	0.424	0.364	0.258	0.335
Sample	Ind.	Ind.	Ind.	Ind.	HH
# Units	5,485	5,485	5,485	5,485	3,213
# Clusters	224	224	224	224	224
Observations	16,455	16,455	16,455	16,455	9,639

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Last four regressors refer to interactions with an indicator of whether household experienced a landmine event since 2002 until 36 months prior to March 1 of the year the household was surveyed. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. Sample includes heads and their spouses when they have one for the first for columns and households in the fifth column. All specifications include individual and year fixed effects, and municipality characteristics interacted with indicators for each round of the survey. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.10: Descriptive statistics by land ownership

	Owners		Non-owners		Difference	Standard error
	Mean	Standard deviation	Mean	Standard deviation	Owners − Non-owners	
Age	46.98	12.12	40.82	11.34	6.16	0.339
Education years	4.23	3.13	4.38	3.26	-0.14	0.091
Male	0.484	0.500	0.488	0.500	-0.004	0.014
Land holdings (ha)	2.62	4.48	1.18	3.78	1.44	0.161
Credit	0.379	0.485	0.291	0.454	0.088	0.018
Formal credit	0.273	0.445	0.098	0.298	0.174	0.015

Notes: Owners correspond to households who owned at least one plot in 2010, whereas non-owners correspond to those who did not own plots in 2010. Difference between owners and non-owners and its standard error are reported. All outcomes are at the individual level (household heads + spouses), except households' number of hectares they have access to, whether household took a credit, and whether household took a formal credit.

Table A.11: Effects of landmine events on labor allocation outside own farm by land ownership

	(1) If worked off own farm	(2) If worked jornalero	(3) If worked non-jornalero	(4) Hrs. worked non-jornalero
If at least one landmine event in the period ...				
Before survey	0.046	0.037	0.007	1.577
	(0.047)	(0.048)	(0.040)	(1.992)
(0-6] months	-0.008	0.012	-0.042*	-2.576**
	(0.021)	(0.018)	(0.025)	(1.063)
(6-12] months	-0.009	-0.031	0.009	0.489
	(0.036)	(0.029)	(0.033)	(1.710)
(12-36] months	0.037	0.004	0.046**	1.739*
	(0.027)	(0.022)	(0.021)	(0.940)
Non-owner × If events ...				
... Before survey	-0.101	-0.104	0.005	-5.190
	(0.083)	(0.091)	(0.084)	(3.272)
... (0-6] months	0.079*	0.076*	0.018	-0.819
	(0.044)	(0.040)	(0.044)	(1.797)
... (6-12] months	0.080	0.148**	-0.028	3.491
	(0.079)	(0.057)	(0.090)	(3.012)
... (12-36] months	-0.046	-0.033	-0.010	0.440
	(0.051)	(0.045)	(0.048)	(2.241)
Linear combs. (If events in period + Non-owner × If events in period...)				
Before survey	-0.055	-0.067	0.012	-3.613
(0-6] months	0.071	0.088**	-0.024	-3.395**
(6-12] months	0.071	0.117**	-0.019	3.980
(12-36] months	-0.009	-0.029	0.037	2.178
Dep Var Mean	0.397	0.195	0.232	8.827
# Units	5,510	5,510	5,510	5,510
# Clusters	224	224	224	224
Observations	16,530	16,530	16,530	16,530

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Sample includes household heads and their spouses when they have one. *If worked off own farm* is whether individuals worked outside the household's agricultural fields in the last week. *If worked jornalero* is whether individuals worked as jornaleros (agricultural day laborers) in the past week. *If worked non-jornalero* is whether individuals had non-jornalero jobs. *Hrs. worked non-jornalero* is the number of hours worked on non-jornalero jobs per week and is winsorized at the top 1%. Households classified on whether they owned land when they were surveyed in 2010. All specifications include individual and year fixed effects and municipality characteristics at baseline interacted with indicators for each survey round. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.12: Effects of landmine events on agricultural and non-agricultural work by land ownership

	Hours worked in the past week		
	(1) Agriculture Non-jornalero	(2) Non-agriculture non-jornalero	(3) Jornalero
If at least one landmine event in period...			
Before survey	-0.843	2.696	3.471*
	(2.906)	(1.712)	(1.764)
(0-6] months	-2.040*	0.601	-0.762
	(1.211)	(1.349)	(1.106)
(6-12] months	0.531	-0.271	-3.152**
	(1.919)	(1.271)	(1.300)
(12-36] months	2.903***	-0.402	2.315*
	(0.834)	(0.891)	(1.279)
Non-owner × If events...			
... Before survey	5.501*	-10.227**	-4.530
	(3.113)	(4.628)	(4.420)
... (0-6] months	0.211	-4.652**	7.326***
	(1.902)	(2.027)	(2.168)
... (6-12] months	-4.484	4.916	10.094***
	(2.773)	(3.710)	(3.110)
... (12-36] months	-3.208*	0.770	-2.643
	(1.666)	(2.268)	(2.141)
Linear combs. (If events in period + Non-owner × If events in period ...)			
Before survey	4.658**	-7.531	-1.059
(0-6] months	-1.828	-4.052**	6.565***
(6-12] months	-3.953	4.645	6.942*
(12-36] months	-0.305	0.368	-0.328
Dep Var Mean	3.504	6.305	5.853
# Units	5,510	5,510	5,510
# Clusters	224	224	224
Observations	11,020	11,020	11,020

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Outcome variables correspond to hours worked by individuals in the past week. Sample include household heads and their spouses when they have one. Only the last two rounds of the household survey (2013, 2016) are considered. All specifications include individual/household and year fixed effects and municipality characteristics at baseline interacted with year FE. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.13: Effects of landmine events on labor input on own farm by land ownership

	If worked on own farm for				(5)
	(1) > 0 hr	(2) ≥ 1 hr	(3) ≥ 2 hr	(4) ≥ 4 hr	If hired labor
If events before survey	-0.048	-0.043	0.001	0.009	0.162***
	(0.048)	(0.050)	(0.047)	(0.036)	(0.060)
If events in (0-6] months	-0.073***	-0.066***	-0.052**	-0.013	0.056*
	(0.024)	(0.023)	(0.026)	(0.020)	(0.031)
If events in (6-12] months	-0.041	-0.043	-0.067**	-0.030	-0.060
	(0.027)	(0.032)	(0.029)	(0.025)	(0.062)
If events in (12-36] months	-0.023	-0.025	-0.011	-0.023	0.050
	(0.026)	(0.026)	(0.026)	(0.025)	(0.038)
Non-owner × ...					
... If events before survey	0.276***	0.186	0.087	0.038	0.298*
	(0.102)	(0.118)	(0.099)	(0.121)	(0.174)
... If events in (0-6] months	0.088	0.068	0.074*	0.015	0.079
	(0.059)	(0.055)	(0.044)	(0.034)	(0.055)
... If events in (6-12] months	-0.049	-0.038	0.008	0.024	-0.179
	(0.063)	(0.069)	(0.046)	(0.104)	(0.151)
... If events in (12-36] months	0.008	0.013	-0.004	0.026	-0.068
	(0.054)	(0.054)	(0.051)	(0.058)	(0.047)
Linear combinations (If event in period + Non-owner × If event in period...)					
Before survey	0.228**	0.143	0.088	0.047	0.460***
(0-6] months	0.015	0.002	0.022	0.002	0.135**
(6-12] months	-0.090*	-0.080	-0.058	-0.007	-0.239**
(12-36] months	-0.016	-0.012	-0.015	0.003	-0.018
Dep Var Mean	0.455	0.424	0.364	0.258	0.335
Sample	Ind.	Ind.	Ind.	Ind.	HH
# Units	5,485	5,485	5,485	5,485	3,213
# Clusters	224	224	224	224	224
Observations	16,455	16,455	16,455	16,455	9,639

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Sample includes household heads and their spouses when they have one. In columns 1 through 4, sample includes household heads and their spouses when they have one. *If worked on own farm for...* is whether household members spent more then 0 hours, or greater than or equal to 1, 2 or 4 hours per day on agricultural tasks on farms that households own. *If hired labor* indicates whether households have hired jornaleros (agricultural day laborers) in the past 12 months. Individuals classified based on whether they belong to a household that does not own land when surveyed in 2010. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. All specifications include individual/household and year fixed effects and municipality characteristics interacted with indicators for each round of the survey. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.14: Effects of landmine events on land use by land ownership

	Number of hectares allocated to				
	(1)	(2)	(3)	(4)	(5)
	Cultivations	Perennial	Seasonal	Mixed	Livestock raising
If events before survey	-0.002	-0.215	0.091	0.098	0.096
	(0.167)	(0.346)	(0.078)	(0.206)	(0.256)
If events in (0-6] months	-0.117*	-0.110	0.028	-0.003	-0.063
	(0.065)	(0.069)	(0.034)	(0.038)	(0.122)
If events in (6-12] months	-0.022	0.044	-0.038	0.015	-0.211
	(0.075)	(0.226)	(0.064)	(0.165)	(0.172)
If events in (12-36] months	0.085	0.075	0.004	-0.013	-0.201*
	(0.091)	(0.075)	(0.040)	(0.052)	(0.111)
Non-owner × ...					
... If events before survey	0.555**	0.360*	0.008	0.185	0.362
	(0.268)	(0.200)	(0.100)	(0.302)	(0.428)
... If events in (0-6] months	0.206**	0.176**	0.029	-0.003	0.162
	(0.087)	(0.071)	(0.049)	(0.059)	(0.190)
... If events in (6-12] months	-0.391**	-0.329**	-0.022	-0.066	0.136
	(0.168)	(0.138)	(0.077)	(0.190)	(0.222)
... If events in (12-36] months	0.028	-0.047	0.022	0.070	0.088
	(0.130)	(0.078)	(0.045)	(0.096)	(0.222)
Linear combinations					
Before survey	0.553	0.146	0.098	0.283**	0.458
(0-6] months	0.089	0.066	0.057	-0.006	0.098
(6-12] months	-0.413**	-0.285	-0.060**	-0.051	-0.075
(12-36] months	0.112	0.028	0.026	0.057	-0.112
Dep Var Mean	0.785	0.368	0.218	0.148	0.889
# Units	3,213	3,213	3,213	3,213	3,213
# Clusters	224	224	224	224	224
Observations	9,639	9,639	9,639	9,639	9,639

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. All outcomes are measured in hectares at the household level and are winsorized at the top 1%. Individuals classified based on whether they belong to a household that does not own land when surveyed in 2010. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.15: Effects of landmine events on income by land ownership

	Income earned in the past month		
	(1) Off own farm work	(2) Non-jornalero	(3) Jornalero
If events before survey	10.30	2.67	9.42
	(17.801)	(17.831)	(13.066)
If events in (0-6] months	-28.91***	-30.99***	1.80
	(10.788)	(9.878)	(6.346)
If events in (6-12] months	-14.13	-3.70	-8.74
	(11.504)	(11.131)	(6.200)
If events in (12-36] months	30.21***	23.74**	6.38
	(10.279)	(9.700)	(5.922)
Non-owner × ...			
... If events before survey	11.67	17.02	-10.35
	(37.953)	(36.053)	(37.784)
... If events in (0-6] months	15.41	-13.17	26.69*
	(24.189)	(22.487)	(14.361)
... If events in (6-12] months	29.97	-12.41	40.78
	(36.016)	(25.263)	(25.644)
... If events in (12-36] months	24.03	18.20	2.55
	(24.473)	(22.114)	(13.150)
Linear combs. (If events in period + Non-owner × If events in period...)			
Before survey	21.97	19.69	-0.94
(0-6] months	-13.50	-44.16**	28.48**
(6-12] months	15.84	-16.10	32.03
(12-36] months	54.24**	41.93**	8.93
Dep Var Mean	153.33	93.45	57.87
# Individuals	5,489	5,489	5,489
# Clusters	224	224	224
Observations	16,467	16,467	16,467

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Sample includes household heads and their spouses when they have one. All monetary values are expressed in thousands of Colombian Pesos (base December 2018) and winsorized at the top 1%. Households classified on whether they owned land when they were surveyed in 2010. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.16: Effect of landmine events on adults' healthcare seeking by land ownership

	Sought medical assistance for preventative reasons in the past 12 months					
	(1) Any non alternative	(2) GP/ specialist	(3) Dentist	(4) Optometrist	(5) Family planning	(6) Alternative medicine
If at least one landmine event in period...						
Before survey	-0.053	-0.055	-0.175***	-0.060**	-0.021	0.026
	(0.063)	(0.066)	(0.059)	(0.029)	(0.023)	(0.017)
(0-6] months	-0.024	-0.038	-0.026	-0.015	0.027*	0.013
	(0.034)	(0.031)	(0.030)	(0.020)	(0.015)	(0.010)
(6-12] months	0.038	0.039	0.108***	0.003	0.029	0.008
	(0.054)	(0.053)	(0.042)	(0.025)	(0.021)	(0.014)
(12-36] months	-0.001	0.042	0.034	-0.009	-0.003	0.033**
	(0.030)	(0.031)	(0.028)	(0.019)	(0.020)	(0.013)
Non-owner × If events in ...						
... Before survey	-0.155	-0.172	0.098	-0.017	0.015	-0.016
	(0.138)	(0.124)	(0.085)	(0.056)	(0.073)	(0.037)
... (0-6] months	-0.058	-0.028	-0.099**	-0.033	-0.035	0.019
	(0.049)	(0.052)	(0.049)	(0.032)	(0.026)	(0.021)
... (6-12] months	0.047	0.039	-0.097	0.032	-0.059	-0.025
	(0.099)	(0.071)	(0.059)	(0.038)	(0.074)	(0.021)
... (12-36] months	0.043	0.012	0.074	0.011	0.032	-0.017
	(0.047)	(0.053)	(0.061)	(0.031)	(0.036)	(0.013)
Linear combinations (If events in period + Non-owner × If events in period...)						
Before survey	-0.207**	-0.227***	-0.077	-0.076	-0.007	0.011
(0-6] months	-0.082*	-0.066	-0.125***	-0.047*	-0.008	0.033*
(6-12] months	0.085	0.078**	0.011	0.035	-0.030	-0.016
(12-36] months	0.042	0.054	0.107*	0.002	0.030	0.016
Dep Var Mean	0.658	0.605	0.400	0.132	0.091	0.030
# Units	5,484	5,484	5,484	5,484	5,484	5,484
# Clusters	224	224	224	224	224	224
Observations	16,452	16,452	16,452	16,452	16,452	16,452

Notes: Standard errors clustered at the village level in parentheses. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Individuals classified based on whether they belong to a household that does not own land when surveyed in 2010. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Table A.17: Effect of landmine events on chilldren's healthcare seeking by land ownership

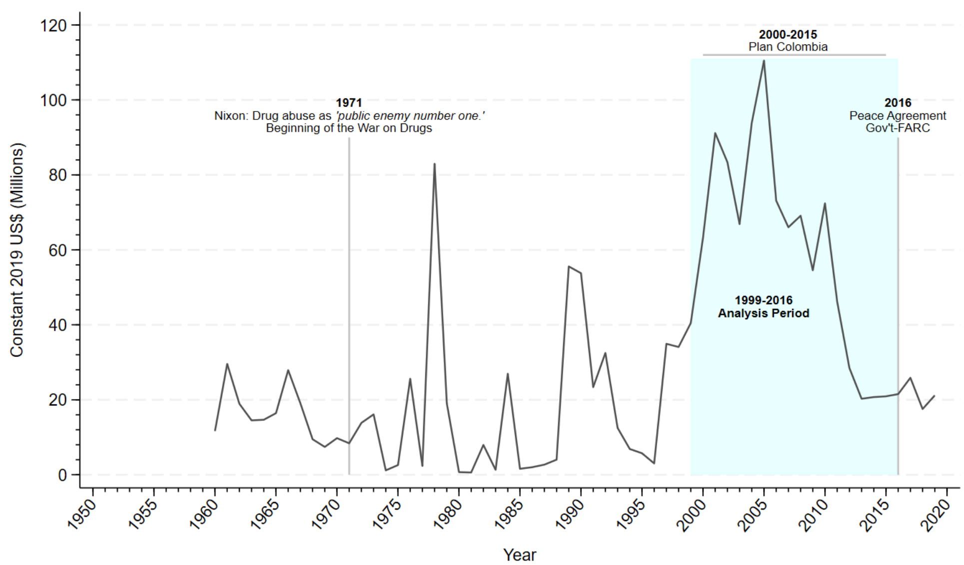
	Sought medical assistance for preventative reasons in the past 12 months					
	(1)	(2)	(3)	(4)	(5)	(6)
	Any non alternative	GP/ specialist	Dentist	Optometrist	Pediatrician	Alternative medicine
If at least one landmine event in period...						
Before survey	-0.086	-0.147*	-0.167**	-0.111	-0.099	0.002
	(0.082)	(0.078)	(0.069)	(0.071)	(0.090)	(0.024)
(0-6] months	0.028	-0.047	-0.028	-0.003	0.027	-0.008
	(0.031)	(0.038)	(0.044)	(0.022)	(0.033)	(0.007)
(6-12] months	0.008	0.040	0.088	0.125**	0.042	0.028***
	(0.053)	(0.056)	(0.066)	(0.057)	(0.034)	(0.008)
(12-36] months	0.018	0.048	-0.049	0.015	-0.035	0.027*
	(0.044)	(0.048)	(0.048)	(0.036)	(0.029)	(0.015)
Non-owner × If events in...						
... Before survey	0.083	0.231	0.068	0.117	0.180*	-0.007
	(0.181)	(0.205)	(0.124)	(0.104)	(0.108)	(0.026)
... (0-6] months	-0.043	0.025	-0.065	0.009	-0.030	0.004
	(0.061)	(0.073)	(0.084)	(0.041)	(0.062)	(0.009)
... (6-12] months	-0.055	-0.176*	-0.168**	-0.091	-0.048	-0.033***
	(0.083)	(0.095)	(0.076)	(0.084)	(0.050)	(0.009)
... (12-36] months	0.038	-0.009	0.082	-0.004	0.076	-0.031*
	(0.074)	(0.080)	(0.071)	(0.056)	(0.055)	(0.017)
Linear combinations (If events in period + Non-owner × If events in period...)						
Before survey	-0.003	0.084	-0.100	0.007	0.081	-0.005
(0-6] months	-0.014	-0.022	-0.093	0.006	-0.003	-0.004
(6-12] months	-0.047	-0.137	-0.081	0.033	-0.006	-0.005
(12-36] months	0.056	0.040	0.033	0.011	0.041	-0.004
Dep Var Mean	0.836	0.759	0.583	0.133	0.231	0.011
# Units	2,813	2,813	2,813	2,813	2,813	2,813
# Clusters	224	224	224	224	224	224
Observations	8,170	8,170	8,170	8,170	8,170	8,170

Notes: Standard errors clustered at the village level in parenthesis. Independent variables indicate if household experienced a landmine event within 5 km from its residence in the specified windows. Individuals classified based on whether they belong to a household that does not own land when surveyed in 2010. Linear combinations correspond to the estimate of the sum of uninteracted plus interacted term of the same time period. Sample includes children who were 0 to 9 years old in 2010 and were followed in at least two consecutive rounds. All specifications include individual and year fixed effects and municipality characteristics at baseline interacted with year FE. Baseline municipality characteristics include average altitude, population density in 2005, distance to the department's capital, homicide rate in 2005, and indicator of landmine events between 1990 and 2005. *** 1%, ** 5%, * 10%

Appendix B

Supplemental Materials for Chapter 2

Figure B.1: U.S. Military Assistance to Colombia



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

Figure B.2: Geographical Distribution of Military Bases 1999 - 2016

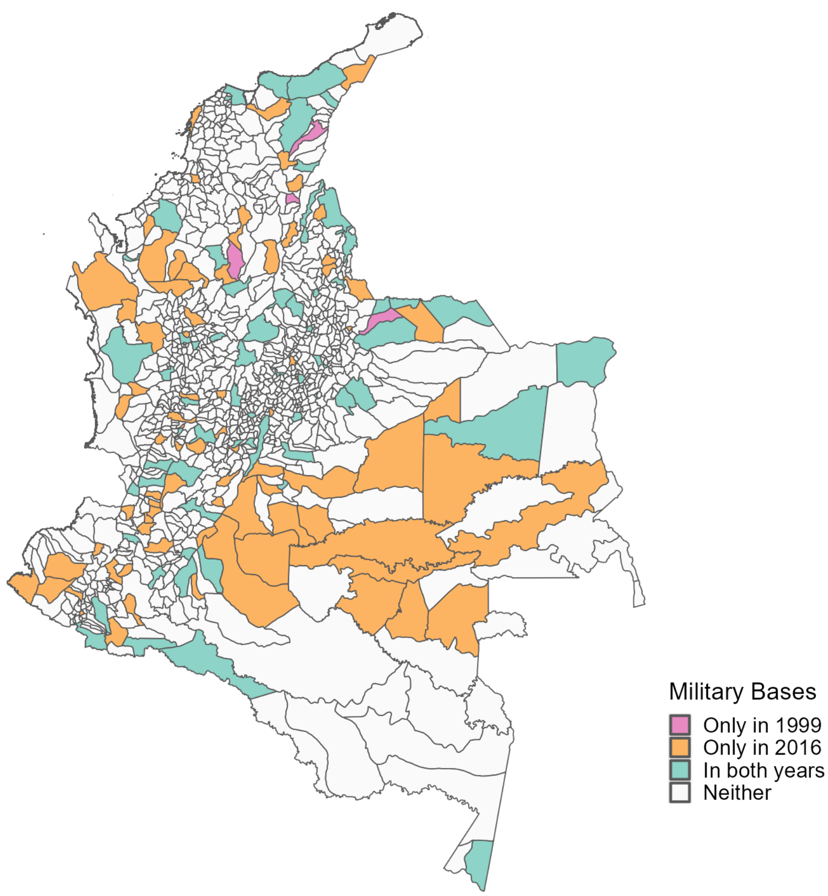
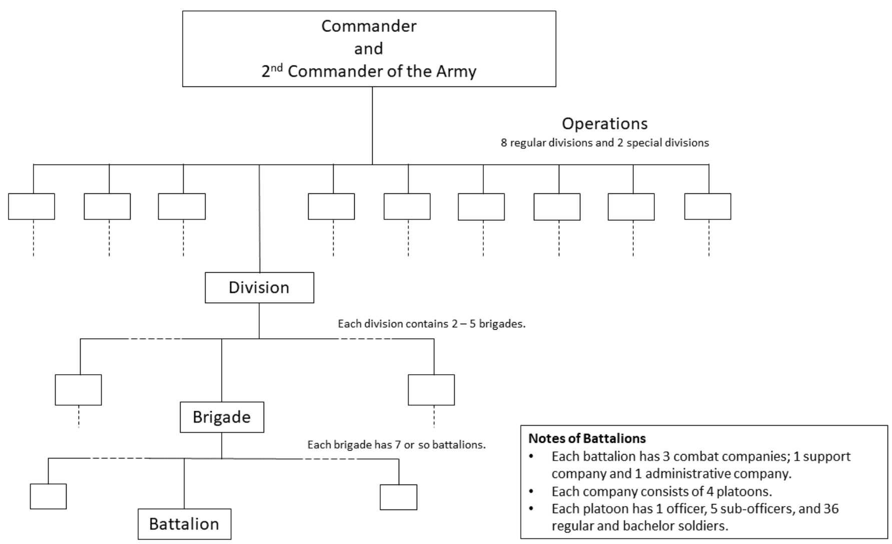


Figure B.3: The Organization of the Colombian National Army



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

Figure B.4: Military Base Presence and Duration

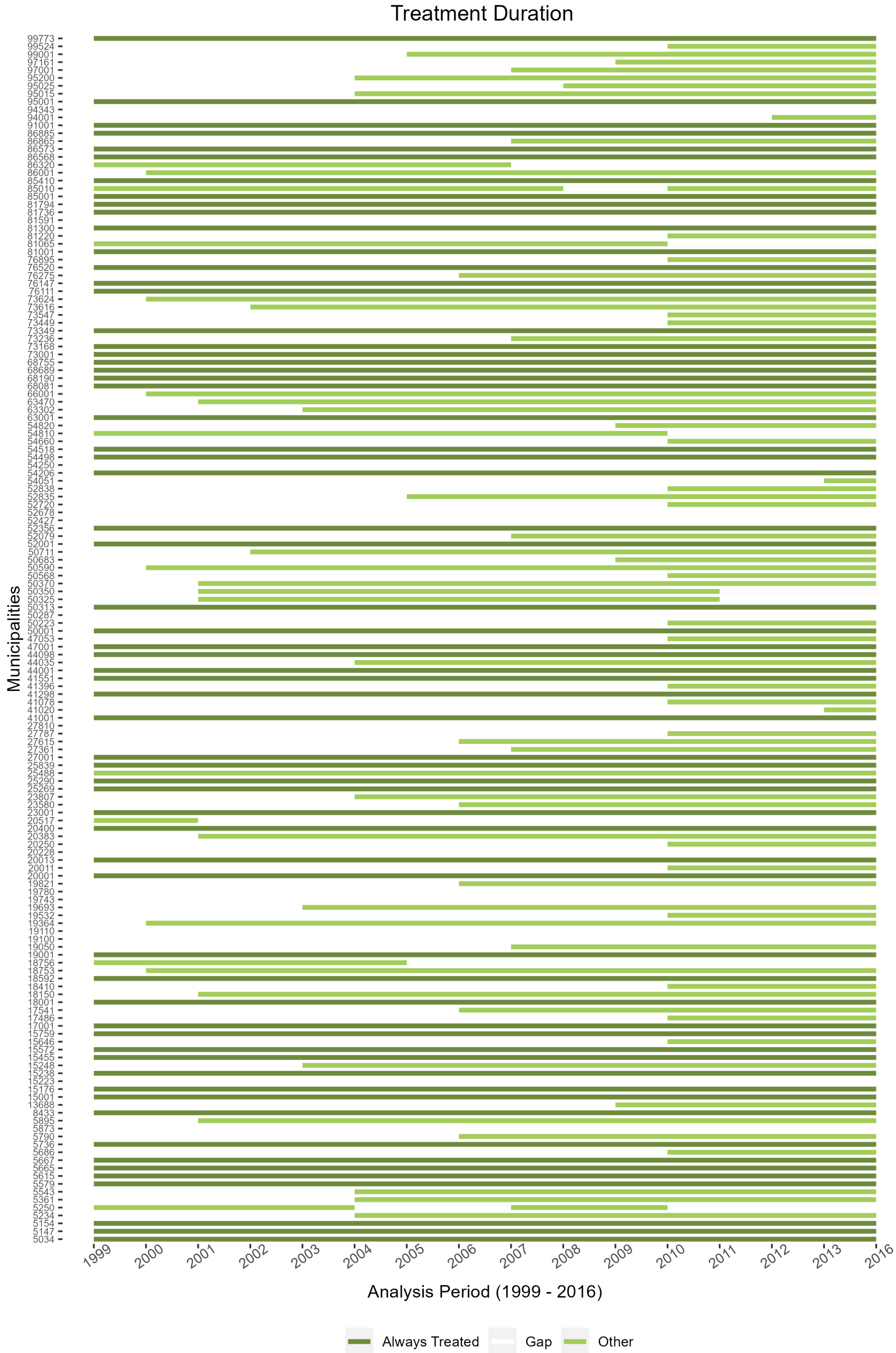
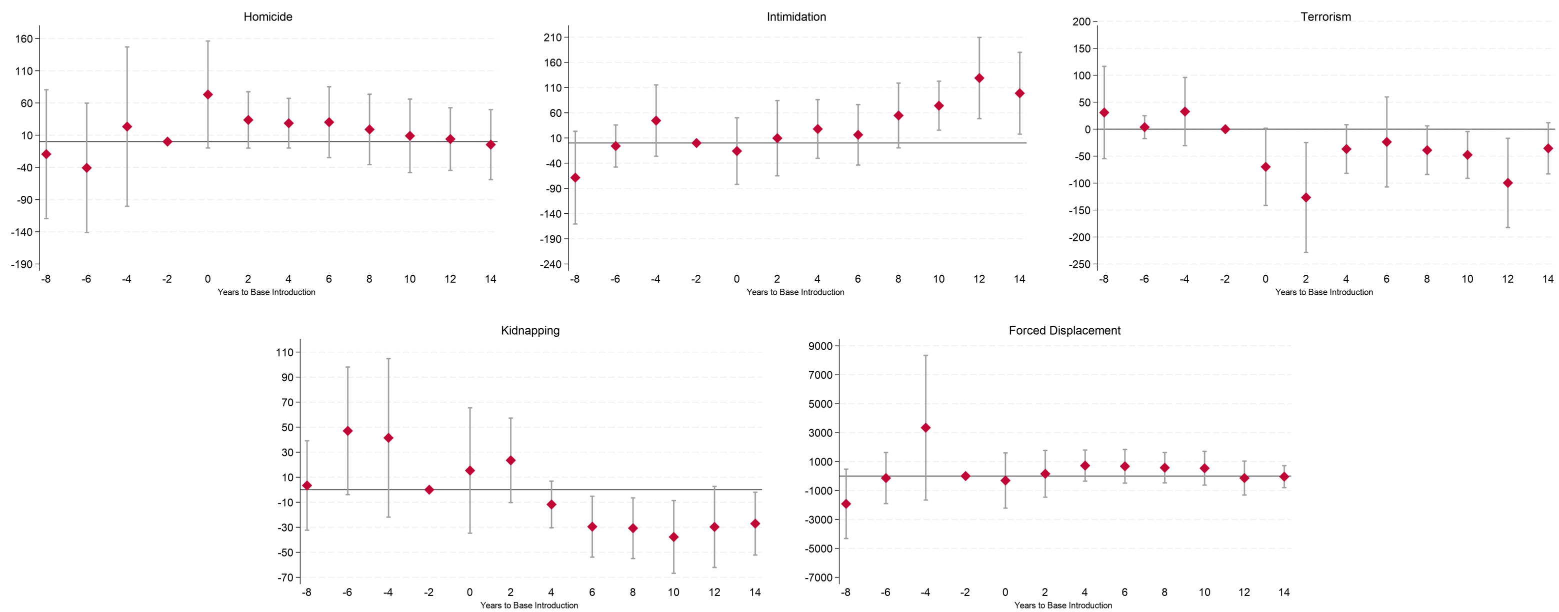


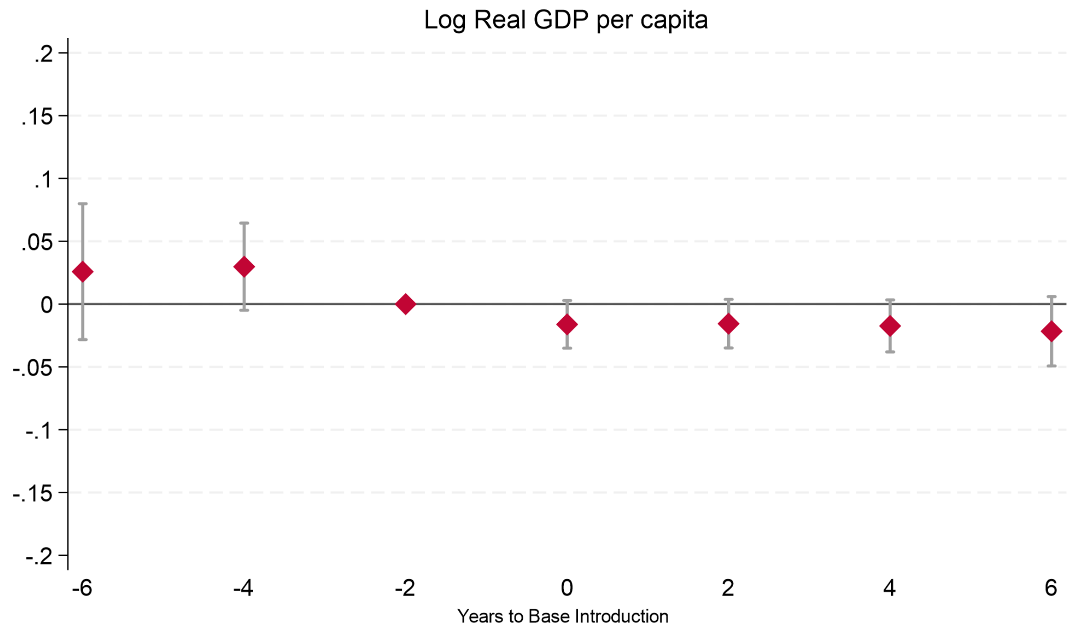
Figure B.5: Effects on Crime Rates by Types

Outcome: Cases per 100,000 Inhabitants



Note: The lines are the 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities.

Figure B.6: 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 90% confidence intervals. Robust standard errors are clustered at the municipality level. This analysis sample includes 153 unique municipalities and excludes 959 never-treated municipalities. The data on municipal GDP per capita is only available from 2000 to 2009, and is made available by CEDE (2021).

Table B.1: 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 B.2: 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: Decree 1794 of 2000

Table B.3: 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 B.4: 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.

Table B.5: Effects on Female Marriage (Currently Married or In Union) - Level Outcomes

	Share of Women Currently Married or In Union by Age Groups				
	(1)	(2)	(3)	(4)	(5)
Panel A: Municipality and Year FEs					
If at least one Army base	-0.01	-0.00	0.01	-0.00	0.01
	(0.01)	(0.01)	(0.03)	(0.01)	(0.03)
Panel B: Municipality and Division X Year FEs					
If at least one Army base	-0.01	-0.00	0.01	-0.00	0.01
	(0.01)	(0.01)	(0.03)	(0.01)	(0.03)
Obs.	236	236	236	236	236
Control Mean	0.48	0.11	0.62	0.76	0.73

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors in parentheses clustered at the municipality level. A *division* in the Colombian Army is a larger unit within its hierachy that govern brigades and battalions, and is assigned parts of the country as its jurisdiction. This analysis sample includes 118 unique municipalities and excludes 0 never-treated census units from the 1993 and 2005 general census.

B.1 Construction of the Military Base Data

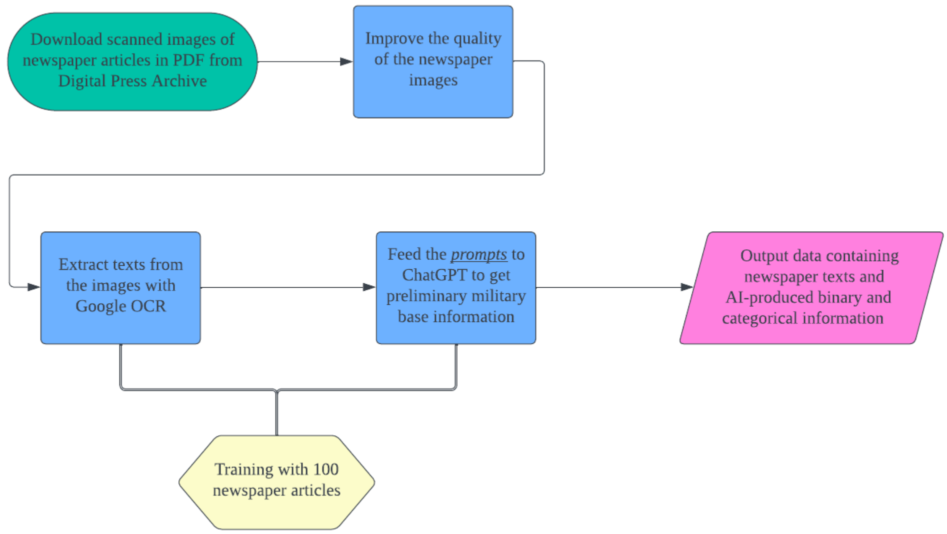
As the data on military base locations were not made available, we constructed them from newspapers published in Colombia from 2000 to 2010. Figure B.7 provides an overview of this data cleaning process, and we describe it in detail in the following.

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

Figure B.7: Process to Extract Text from Newspaper Images



contrast.[1]

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 B.1 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.

[1] 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.

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.

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.

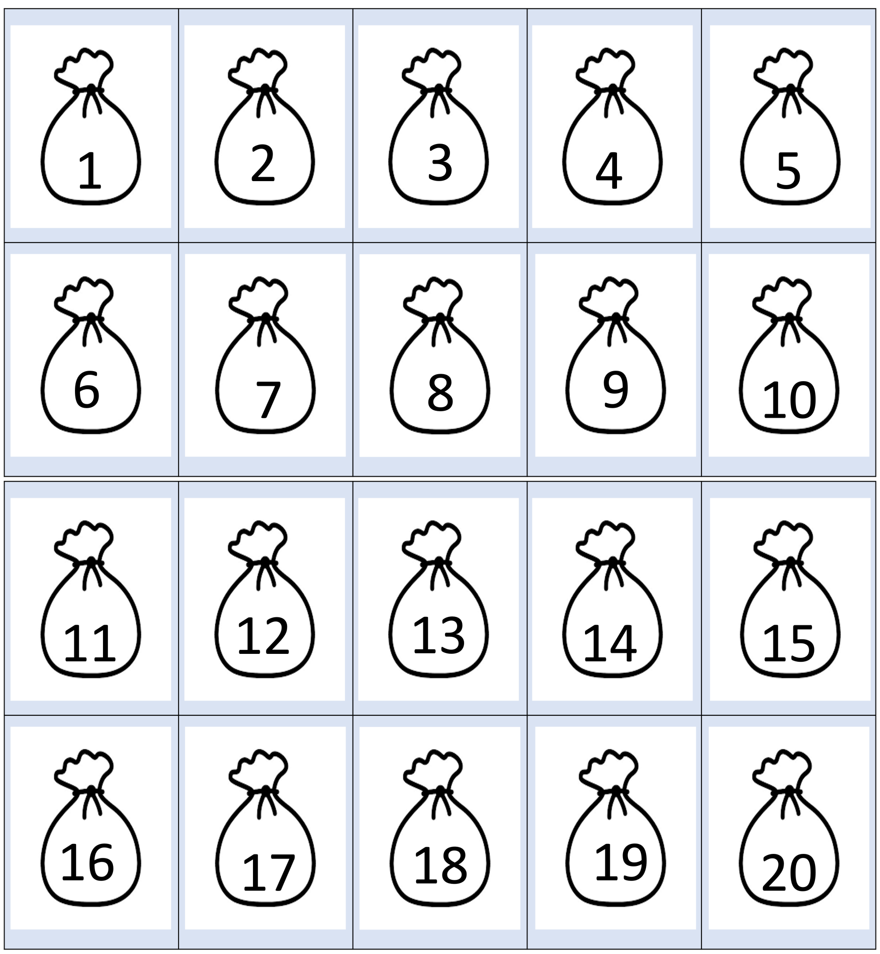
14. You should not show the corrected article. Just the JSON

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.

Appendix C

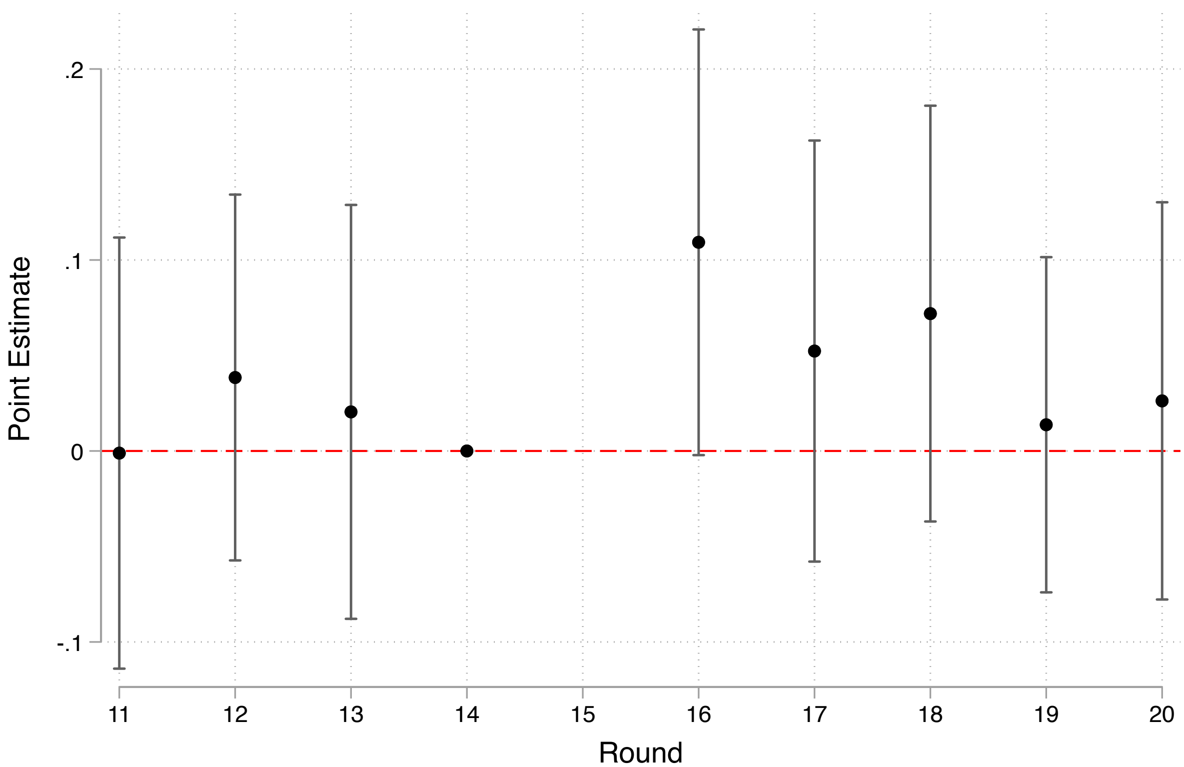
Supplemental Materials for Chapter 3

Figure C.1: Chart used in belief elicitation



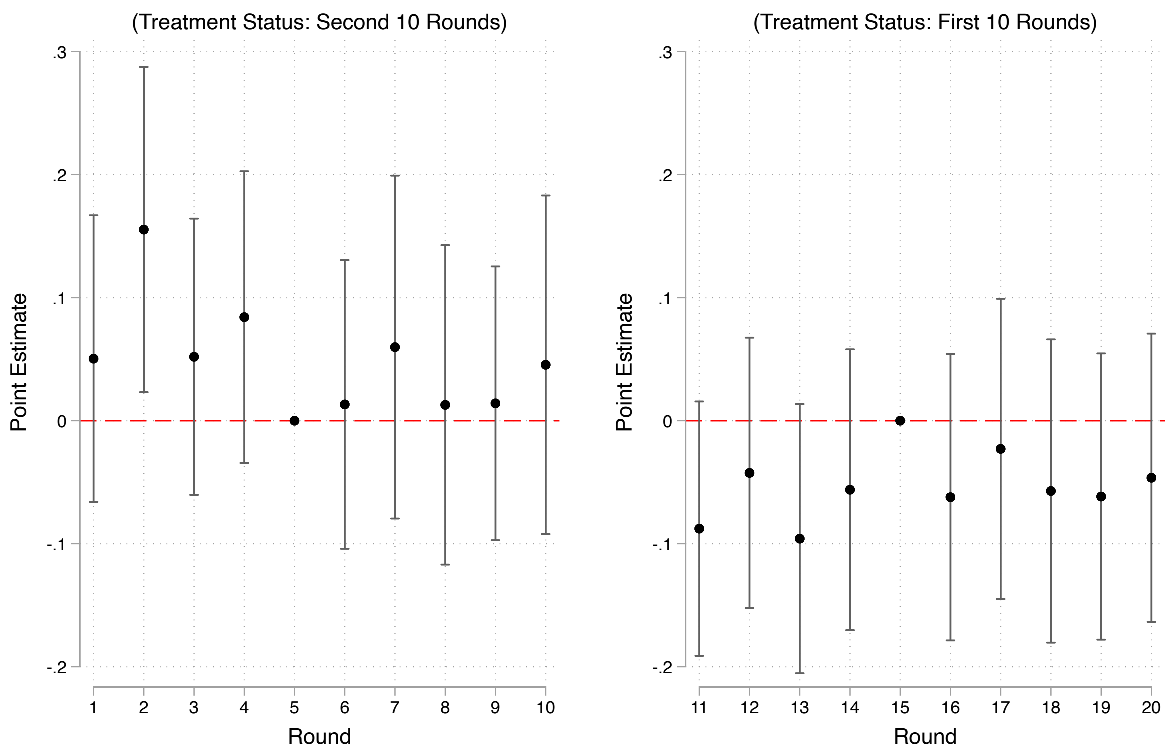
Notes: Participants were asked to place one bean per black marble they expected to obtain in 20 hypothetical draws from the bag they have been playing with for the last five rounds.

Figure C.2: Event-study estimates of the effect of entry on the likelihood of choosing seeds (excluding round 15)



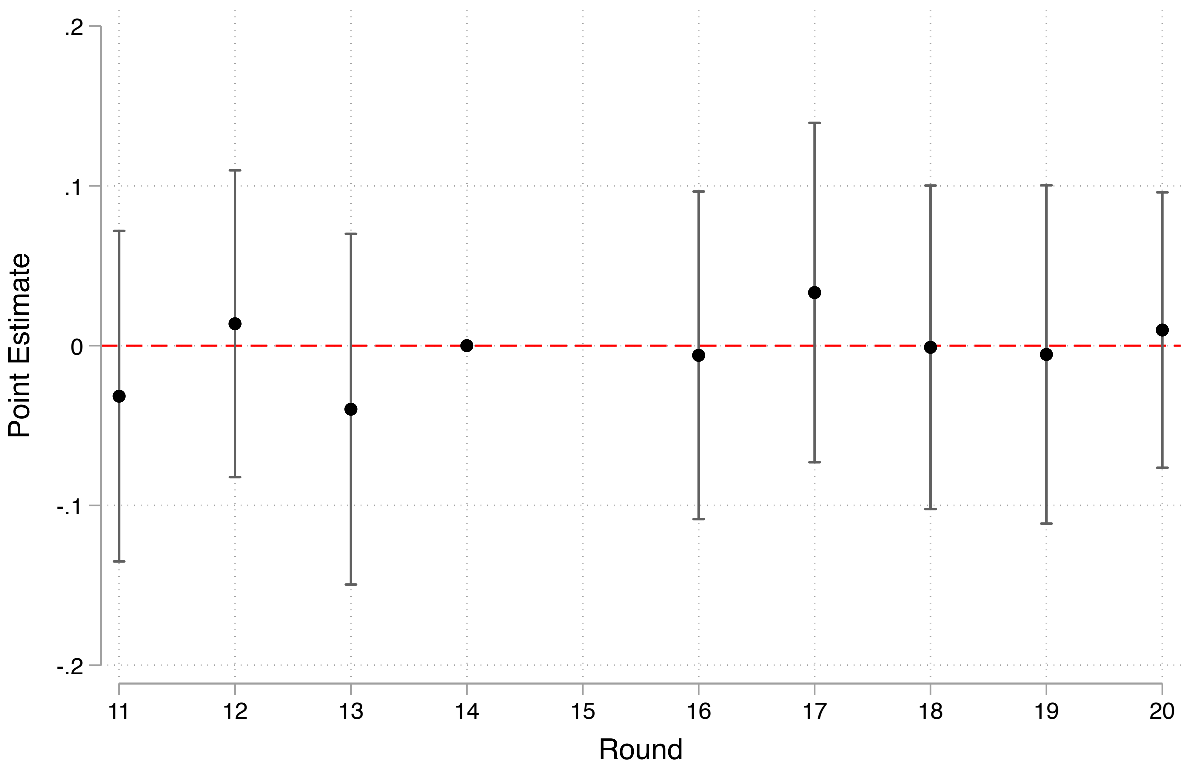
Notes: Each dot represents the point estimate for that round in an event study estimation where the outcome variable is an indicator of whether participants chooses seeds and independent variables are indicators of whether participants had the chance to choose from the entrant in each round. Round 15 is excluded from the analysis. Individual and round fixed effects included. Standard errors clustered at the session level. Lines represent 95% confidence intervals.

Figure C.3: Event-study estimates of the effect of other market's entry on the likelihood of choosing seeds



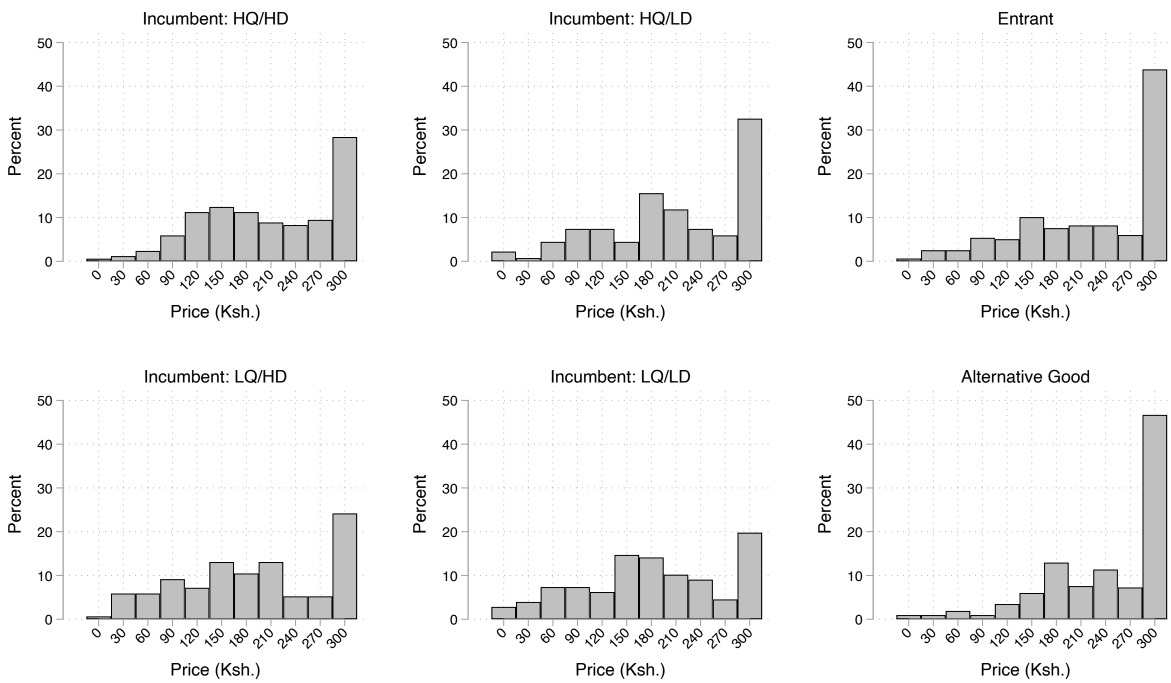
Notes: Each dot represents the point estimate for that round in an event study estimation where the outcome variable is an indicator of whether participants chooses seeds and independent variables are indicators of whether participants had the chance to choose from the entrant in each round. The treatment status of the second set rounds is used for the first set of ten rounds, and the treatment status of the first set of ten rounds is used for the second set of ten rounds. Individual and round fixed effects included. Standard errors clustered at the session level. Lines represent 95% confidence intervals. Left panel shows the estimates for the first set of ten rounds. Right panel shows the estimates for the second set of ten rounds.

Figure C.4: Event-study estimates of the effect of other market's entry on the likelihood of choosing seeds (excluding round 15)



Notes: Each dot represents the point estimate for that round in an event study estimation where the outcome variable is an indicator of whether participants chooses seeds and independent variables are indicators of whether participants had the chance to choose from the entrant in each round. The treatment status of the first set of ten rounds is used. Round 15 is excluded from the analysis. Individual and round fixed effects included. Standard errors clustered at the session level. Lines represent 90% confidence intervals.

Figure C.5: Elicited willingness-to-pay histograms



Notes: Willingness-to-pay is calculated as the maximum price for which each participant said he would buy the good. The alternative good is either 3 kg of maize flour, 2 kg of sugar or 1 liter of cooking oil, which was chosen by the participant at the beginning of the game.

Table C.1: Effect of signals on beliefs (first-stage results)

Dependent variable: Avg. Belief Black Marbles (standardized)	
	(1)
Avg. Black Marbles (standardized)	0.552***
	(0.036)
Entry	-0.005
	(0.044)
High-Dispersion	-0.161**
	(0.077)
F-stat instument	241.840
Participants	311
Observations	6220

Notes: Standard errors clustered at the session level in parentheses. The specification includes participant and round fixed effects. The dependent variable is the standardized average of the number of black marbles participants believe they would take out in 20 imaginary rounds. We report F-statistic of instrument (standardized average number of black marbles drawn during preliminary rounds). *** 1%, ** 5%, * 10% significance.

Table C.2: Heterogeneous effects of entry by market's quality distribution

Dependent variable: Indicator of whether respondents choose seeds			Beliefs	
	(1) Dummies	(2) Signals	(3) OLS	(4) 2SLS
Entry (β_1)	0.091***	0.099***	0.098***	0.098***
	(0.034)	(0.027)	(0.026)	(0.026)
Quality (β_1)	0.031	0.016	0.018	0.030
	(0.023)	(0.011)	(0.012)	(0.020)
High-Dispersion (β_3)	0.019	0.019	0.027	0.025
	(0.024)	(0.016)	(0.017)	(0.016)
Entry×Quality (β_4)	0.013	0.013	0.026	0.027
	(0.042)	(0.021)	(0.021)	(0.039)
Entry×High-Dispersion (β_5)	-0.004	-0.024	-0.025	-0.024
	(0.045)	(0.032)	(0.031)	(0.031)
High-Dispersion×Quality (β_6)	0.011	0.018	0.012	0.027
	(0.028)	(0.013)	(0.015)	(0.023)
Entry×High-Dispersion×Quality (β_7)	-0.046	-0.048*	-0.068**	-0.085*
	(0.057)	(0.028)	(0.030)	(0.050)
F-test p-value ($\beta_4 + \beta_7 = 0$)	0.388	0.065	0.033	0.064
No Entry Mean	0.605	0.605	0.605	0.605
First Stage F-stat				70.18
Participants	311	311	311	311
Observations	6220	6220	6220	6220

Notes: Standard errors clustered at the session level in parenthesis. All specifications include participant and round fixed effects. The dependent variable is an indicator of whether the participant chooses seeds in a particular round. The quality measurements are different in each column. Column 1 uses an indicator of whether the incumbent is of high- or low-quality. Column 2 uses the standardized average of the number of black marbles drawn in the demonstration rounds. Columns 3 and 4 use the standardized average of the number of black marbles the participants believes he/she would take out in 20 imaginary rounds. In column 4, the average of the number of black marbles drawn in the demonstration rounds is used as instruments. The F-statistic reported corresponds to the test of whether the excluded instruments are jointly significant in the first stage corresponding to the uninteracted quality measurement.

Table C.3: Tobit estimates on willingness-to-pay for incumbent's seeds

Dependent variable: WTP for incumbent's seeds	Market 1			Market 2		
	(1)	(2)	(3)	(4)	(5)	(6)
	Dummies	Signals	Beliefs	Dummies	Signals	Beliefs
Entry	-7.95	-8.07	-8.08	6.47	6.65	2.74
	(13.17)	(13.13)	(13.04)	(13.21)	(13.19)	(14.48)
Quality	9.22	-0.50	-0.73	30.05*	13.60	34.93*
	(14.14)	(7.28)	(10.73)	(16.62)	(8.40)	(20.75)
High-Dispersion	21.29*	23.34*	23.23*	13.79	11.51	16.06
	(12.73)	(12.92)	(12.86)	(12.11)	(12.62)	(12.72)
Mean Dep. Var.	201.58	201.58	201.58	190.79	190.79	190.79
Observations	317	317	317	317	317	317

Notes: Standard errors clustered at the session level in parentheses. The dependent variable is the participant's willigness-to-pay for the incumbent's seeds. Maximum likelihood used to obtain Tobit estimates. The average quality measurements are different in each column. Column 1 and 4 use an indicator of whether the incumbent is of high- or low-quality. Column 2 and 5 use the standardized average number of black marbles drawn in the demonstration rounds. Columns 3 and 6 use the standardized average number of black marbles the participants believe he/she would take out in 20 imaginary rounds, where the standardized average number of black marbles drawn in the demonstration rounds is used as instrument. First-stage robust F-statistics reported. All specifications use individuals' measurements for risk aversion, time preferences, altruism, the participant's age, and an market center fixed effects as controls. *** 1%, ** 5%, * 10% significance.

Table C.4: Cross-sectional estimates on willingness-to-pay for incumbent's seeds

Dependent variable: WTP for incumbent's seeds	Market 1			Market 2		
	(1) Dummies	(2) Signals	(3) Beliefs	(4) Dummies	(5) Signals	(6) Beliefs
Entry	-2.56	-2.58	-2.62	2.18	2.34	-0.80
	(10.10)	(10.08)	(9.49)	(10.26)	(10.25)	(10.71)
Quality	7.80	0.91	1.34	23.79*	10.83	28.05*
	(10.37)	(5.39)	(7.53)	(13.26)	(6.79)	(15.88)
High-Dispersion	17.95*	18.88*	19.13**	8.39	6.61	10.24
	(9.73)	(9.87)	(9.32)	(10.03)	(10.46)	(9.97)
Mean Dep. Var.	201.58	201.58	201.58	190.79	190.79	190.79
First Stage F-stat			228.20			52.01
Observations	317	317	317	317	317	317

Notes: Standard errors clustered at the session level in parentheses. The dependent variable is the participant's willigness-to-pay for the incumbent's seeds. The average quality measurements are different in each column. Column 1 and 4 use an indicator of whether the incumbent is of high- or low-quality. Column 2 and 5 use the standardized average number of black marbles drawn in the demonstration rounds. Columns 3 and 6 use the standardized average number of black marbles the participants believe he/she would take out in 20 imaginary rounds, where the standardized average number of black marbles drawn in the demonstration rounds is used as instrument. First-stage robust F-statistics reported. All specifications use individuals' measurements for risk aversion, time preferences, altruism, the participant's age, and an market center fixed effects as controls. *** 1%, ** 5%, * 10% significance.

Table C.5: Tobit estimates on willingness-to-pay for entrant's seeds

Dependent variable: WTP for entrant's seeds			Beliefs	
	(1) Dummies	(2) Signals	(3) Tobit	(4) IV Tobit
WTP for entrant				
Entry (1st set)	14.87	14.86	15.09	13.64
	(13.55)	(13.58)	(13.36)	(13.52)
Entry (2nd set)	0.12	-0.12	0.50	-1.01
	(14.94)	(15.04)	(14.80)	(15.00)
Quality (1st set)	-3.59	-4.74	-7.81	-12.71
	(19.60)	(9.64)	(8.76)	(12.80)
Quality (2nd set)	28.13	10.00	-8.21	17.91
	(19.97)	(9.72)	(8.04)	(20.33)
High-Dispersion (1st set)	14.43	14.29	12.86	12.31
	(17.93)	(18.05)	(18.18)	(18.10)
High-Dispersion (2nd set)	16.23	13.82	14.73	16.17
	(17.89)	(18.38)	(18.25)	(18.06)
Mean Dep. Var.	201.58	201.58	201.58	201.58
Observations	317	317	317	317

Notes: Standard errors clustered at the session level in parentheses. The dependent variable is the participant's willigness-to-pay for the entrant's seeds. Maximum likelihood is used to estimate the Tobit models. The average quality measurements are different in each column. Column 1 uses an indicator of whether the incumbent is of high- of low-quality. Column 2 uses the standardized average number of black marbles drawn in the demonstration rounds. Columns 3 and 4 use the standardized average number of black marbles the participants believe he/she would take out in 20 imaginary rounds. In column 5, the standardized average number of black marbles drawn in the demonstration rounds is used as an instrument. First-stage robust F-statistics reported. All specifications use individuals' measurements for risk aversion, time preferences, altruism, the participant's age, and an market center fixed effects as controls. *** 1%, ** 5%, * 10% significance.