





Doctoral Thesis

# **Essays in Empirical Development Economics**

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# Abstract

## Essays in Empirical Development Economics

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This thesis comprises three empirical essays in development economics on how large shocks and public policies shape human capital formation, fertility, and labour-market outcomes in low- and middle-income countries. Using microdata from Peru and Zambia linked to administrative and geospatial sources, the chapters examine the intergenerational consequences of civil conflict, the effects of adolescent-friendly reproductive health services, and the gendered impacts of climate shocks.

The first chapter, *The long shadow of conflict on human capital: Intergenerational evidence from Peru*, estimates the intergenerational impacts of mothers' exposure to the 1980-2000 Peruvian civil conflict on their children's socio-emotional skills development. Combining longitudinal data, which measures skills across a child's life, with historical geo-located conflict data and exploiting spatial and temporal variation in conflict episodes, this chapter finds that mothers' exposure to conflict has adverse intergenerational effects on their children's socio-emotional outcomes of agency and pride. These effects are present at ages 8 and 12 and are robust to alternative specifications. At age 15, mothers' conflict exposure increases children's propensity to engage in crime-related risky behaviour. The analysis of mechanisms highlights the role of reduced parental investments in children, driven by constrained household resources, a quality-quantity trade-off, and diminished maternal empowerment. Finally, an examination of the mother's migration history reveals that migration decisions of her parents during the conflict partially mitigated the adverse effects on the socio-emotional development of their grandchildren.

The second chapter, *Impact of Adolescent-Friendly Family Planning Services on Teenage Fertility in Peru*, evaluates a 2017 reform to Peru's public family planning system that standardised a rights-based approach to adolescent service delivery and introduced confidential, adolescent-friendly protocols in a subset of facilities. Using difference-in-differences designs that leverage spatial variation in proximity to upgraded health centres, and combining administrative birth records with repeated cross-sections from the Demographic and Health Surveys, I find sustained reductions in adolescent fertility, delayed cohabitation and marriage, and fewer

schooling interruptions linked to early family formation. The chapter also documents unequal gains, with larger impacts among relatively more advantaged adolescents, highlighting how universal reforms can translate into unequal effective access when stigma and provider discretion remain salient. Event-study estimates further show that the effects attenuate in 2020, during the COVID-19 interruption of non-urgent reproductive health services, and rebound when service provision is restored.

The third chapter, *The gendered effects of climate shocks on labour and welfare in Zambia*, examines the gendered consequences of climate shocks in Zambia and the extent to which formal safety nets mitigate their effects. I merge nationally representative labour-force and living-conditions surveys with high-resolution measures of rainfall and temperature shocks to characterise how extreme precipitation and heat affect employment and welfare. The results show that adverse climate conditions reduce labour force participation and reallocate work toward self-employment, with consistently larger responses among women. These labour-market adjustments translate into measurable welfare impacts: shocks reduce household income and food consumption, indicating incomplete consumption smoothing. Exploiting variation in exposure to Zambia's social cash transfer programme, I show that transfers provide only partial insurance against these climate-related losses.

Across chapters, the evidence shows that the effects of shocks and policy changes depend on when exposure occurs over the life cycle and on household and institutional channels that shape investments, labour supply adjustment, and risk management, with persistent distributional consequences by gender and socio-economic status.

# Declaration of Authorship

I, **Alessandra Nicole Hidalgo Aréstegui**, declare that this thesis titled, “Essays in Empirical Development Economics”, and the work presented in it are my own and have not been submitted, either in whole or in substantial part, in any other form for the award of a higher degree elsewhere.

Chapter 2, titled “The long shadow of conflict on human capital: Intergenerational evidence from Peru”, is a joint-authored piece of work with myself, Catherine Porter, Alan Sanchez and Saurabh Singhal, which has been published in *Journal of Development Economics* (JDE), Volume 174, 2025. A signed declaration of joint authorship can be found in Appendix.

Chapter 4, titled “The gendered effects of climate shocks on labour and welfare in Zambia”, is based on a co-authored article with myself, Patricia Justino, Gabriel Monteiro, Rodrigo Oliveira and Bruce Sianyeuka, which has been published as a WIDER Working Paper Volume 86, 2024. A signed declaration of joint authorship can be found in Appendix.

All remaining chapters are my sole-authored work. Any published or submitted material has been clearly indicated in the thesis.

An approximate estimate of the word count is: 30,699.

Alessandra Hidalgo Aréstegui

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# Chapter 1

## Introduction

My research examines how individuals and communities adapt to large structural disruptions. I study how conflict, climate, and policy shocks shape human capital formation and economic opportunity in low- and middle-income countries (LMICs), with a particular focus on gender. In a context where climate change is repeatedly breaking global temperature records, armed conflict is intensifying, and governments face pressure to design effective health and social policies, the distributional consequences of these shocks remain profound and often overlooked.

This dissertation brings these forces into a unified empirical frame. Across three papers, set in Peru and Zambia and spanning different stages of the life cycle, I analyse how large-scale shocks and policy design alter the accumulation of skills, the timing of fertility and schooling, and the allocation of labour within households and markets. A common thread is the centrality of gender in mediating exposure to shocks and access to coping mechanisms, and the way institutional design shapes human capital and mobility trajectories over the life course.

The first chapter asks whether the legacy of civil conflict spills into the next generation by affecting children's socio-emotional development through maternal exposure in Peru's 1980–2000 civil conflict. The second evaluates whether a nationwide reform that guaranteed confidential, adolescent-friendly family-planning services for minors in Peru reduced teenage fertility and altered trajectories of union formation and schooling. The third measures the gendered effects of climate shocks on labour, income, and consumption in Zambia, and assesses whether social protection can cushion welfare losses. Taken together, these chapters trace how shocks at different stages of the life course, a mother's exposure to civil war in childhood and adolescence, adolescents' access to contraceptive services, and adults' exposure to droughts and heat, operate through preferences, constraints, and institutions to influence human capital, fertility, and labour outcomes.

A large literature documents that conflict, restrictive institutions, and climatic shocks can impose persistent scars on individuals and communities. Civil conflict now touches a non-trivial share of the world's population and generates macroeconomic and social costs that endure

even after fighting ceases (Blattman and Miguel, 2010; IEP, 2022; Raleigh and Kishi, 2024; Verwimp et al., 2019). While some aggregate indicators eventually rebound (Chen et al., 2008; Waldinger, 2016), human capital and norms may not. In parallel, adolescent fertility remains stubbornly high in many LMICs despite progress in schooling and health infrastructure, raising questions about access, autonomy, and the design of reproductive health services (Aizer et al., 2022; Dupas et al., 2018; Favara et al., 2012; Kearney and Levine, 2014; Lang and Weinstein, 2015). Climate change is also altering the frequency and intensity of extreme weather events, with emerging evidence that women in many settings bear disproportionate costs in the labour market and at home (Fruttero et al., 2024; UN Secretariat, 2022). Across these domains, key policy levers are institutional: conflict legacies operate through household resources and parenting; adolescent fertility responds not only to the supply of contraceptive methods but to confidentiality and provider interactions; and climate shocks interact with social protection, informality, and sectoral structures.

The dissertation is organised around three cross-cutting questions:

1. How does a mother's exposure to civil conflict in childhood and adolescence shape her children's human capital?
2. When do adolescent-friendly family planning services, and the autonomy they provide, shift fertility and schooling at scale?
3. How do climate shocks reallocate labour and welfare across genders in rural Zambia, and can social protection mitigate these losses?

All three chapters are grounded in a human-capital production view in which skills and welfare evolve cumulatively, with dynamic complementarities and sensitive periods over the life cycle (Cunha and Heckman, 2008; Cunha et al., 2010). In Chapter 2, mothers' exposure to conflict during their own childhood and adolescence reduces later parental investments and empowerment, which in turn depresses children's socio-emotional skills. In Chapter 3, removing procedural barriers to confidential contraception shifts adolescent choices over fertility, union formation, and schooling, altering the timing of investments with long-run payoffs. In Chapter 4, weather shocks perturb household time allocation and market opportunities, with women's paid work particularly sensitive given sectoral sorting, caregiving roles, and informality; social cash transfers may soften income losses but do not necessarily stabilise consumption in the presence of broader market disruptions.

Together, these chapters demonstrate that (i) adverse shocks leave marks that extend across generations, particularly via parental investments and socio-emotional capital; (ii) institutional design that centres autonomy and confidentiality can transform adolescent choices at scale; and (iii) climate adaptation policy must acknowledge gendered constraints and stabilize both earnings and purchasing power when shocks strike. By integrating microdata with clear identification strategies and focusing on the institutional mechanisms through which

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households absorb or deflect shocks, the dissertation aims to inform policies that do more than fill gaps: they change the slope of capability accumulation over the life course.

By centring gender and life-cycle dynamics, the dissertation speaks to core debates in development economics on the formation of human capital under adversity, the design of effective social and health services, and the distributional consequences of climate change. The evidence from Peru and Zambia shows that seemingly disparate domains, post conflict recovery, adolescent reproductive health, and climate adaptation, share a common capabilities production function, whereby institutions shape constraints and incentives at sensitive ages. In identifying scalable levers that shift those constraints, the chapters offer policy-relevant evidence for governments and development partners seeking to raise both the level and the resilience of human capital in LMICs.

# Chapter 2

## The long shadow of conflict on human capital: Intergenerational evidence from Peru

### 2.1 Introduction

Civil conflicts have risen sharply since the Second World War, becoming the most common form of conflict, particularly in low- and middle-income countries. Currently, nearly one in six people live in areas affected by conflict—defined as at least one conflict-related event or victim in the past year (Raleigh and Kishi, 2024). In 2023 alone, over 147,000 conflict events were recorded, reflecting a 40% increase since 2020. In 2021, the Global Peace Index reported that violence-related losses amounted to 10.9% of global GDP (IEP, 2022). Understanding the full scope of these conflicts and informing policy responses is therefore crucial.

While macroeconomic indicators often recover post-conflict (Chen et al., 2008; Waldinger, 2016), research shows that conflict leaves lasting scars on human capital, social norms, and institutions, with effects persisting long after the violence ends (Blattman and Miguel, 2010; Verwimp et al., 2019; Vesco et al., 2025). This study contributes to the emerging literature on how the long shadow of armed conflict extends to the next generation through intergenerational human capital development. While some evidence suggests that the negative effects of conflict manifest in the next generation through in-utero exposure (Bundervoet et al., 2009), few studies examine how the human capital of those born after the conflict ends is shaped by parental exposure (Akresh et al., 2023; Phadera, 2021).

Our research builds on this limited body of evidence to evaluate the intergenerational effects of civil conflict on socio-emotional skills—an increasingly recognized yet underexplored dimension of human capital. Socio-emotional skills, such as self-esteem, agency, self-efficacy, and teamwork, are now viewed as essential components of human capital (Deming, 2022) and have demonstrated strong predictive power for future success in both developed (Heckman

et al., 2006; Almlund et al., 2011) and developing countries (Berniell et al., 2016; Cunningham et al., 2016; Mitchell et al., 2023).

We investigate how maternal exposure to Peru's prolonged civil conflict (1980–2000) affects the socio-emotional outcomes of their children, who were not directly exposed to violence. By combining longitudinal data from the Young Lives cohort study with geo-coded information on conflict incidents, we exploit variation in mothers' birth date, location, and the intensity and timing of violence. Our findings reveal that maternal exposure to conflict negatively affects children's socio-emotional skills, specifically agency and pride, at age 8. One additional year of maternal exposure to fatal violence reduces socio-emotional outcomes by 0.016 standard deviations (SD). Given the average maternal exposure of 1.12 years, this translates to a 0.018SD decline. The effect is robust across alternative specifications and sensitivity analyses.

Using novel data on maternal migration history, we find suggestive evidence that parental migration during the conflict partly mitigated the adverse effects on the children's socio-emotional skills. Specifically, the impact of conflict exposure—adjusted for maternal migration—is smaller but remains significant. Disaggregating maternal exposure by life stages shows that conflict during early childhood (ages 0–5) and adolescence (ages 11–19) has the strongest impact on children's socio-emotional development, aligning with the findings of Akresh et al. (2023) for other dimensions of child development.

The negative effect persists at age 12 but diminishes by age 15 in terms of socio-emotional skills. However, by age 15, children of conflict-exposed mothers show a higher propensity for risky behaviors, particularly criminal activity. This suggests that while direct socio-emotional deficits may fade over time, maternal conflict exposure re-emerges in other behavioral domains during adolescence, shaping long-term well-being and life outcomes.

Using a conceptual framework derived from Cunha and Heckman (2008) and Cunha et al. (2010), we examine how conflict affects key inputs in a child's socio-emotional skill production function: parental investments and the socio-emotional and cognitive skills of parents. Our analysis emphasizes the critical role of parental investments. We find that maternal conflict exposure reduces investments in children, particularly in educational expenditure and reading-related activities. This reduction is driven by a quality-quantity trade-off, constrained household resources, and diminished maternal empowerment. Conflict exposure lowers household expenditures, increases maternal fertility, reduces the age at first birth, and limits mothers' ability to make household decisions.

The contributions of this paper are threefold. First, we deepen the understanding of the long-term effects of conflict. Previous studies have documented the impact of conflict on the human capital of exposed individuals, affecting health and nutrition (Bundervoet et al., 2009; Akresh et al., 2011; Akresh et al., 2012), education and skills (Shemyakina, 2011), social capital (Cassar et al., 2013; Adhvaryu and Fenske, 2023), mental health (Singhal, 2019; Swee, 2015), and labor market outcomes (Kondylis, 2010; Bozzoli et al., 2013). To

our knowledge, only two studies have examined whether these effects extend to future generations not directly exposed to conflict. [Akresh et al. \(2023\)](#) analyze the impacts of the Nigerian civil war (1967–1970) on the health and educational outcomes of the second generation, while [Phadera \(2021\)](#) investigate the intergenerational effects of the Nepali civil conflict (1996–2006) on child health. We contribute to this literature by demonstrating that socio-emotional (or non-cognitive) skills in the next generation may also be adversely affected. Using unique maternal migration history data, we also show that while migration is driven by conflict exposure, it neither reduces overall exposure nor fully mitigates the adverse effects on children’s socio-emotional development.

This research contributes to the literature on the formation of socio-emotional (or non-cognitive) skills. Studies in developed countries show that non-cognitive skills are sensitive to childhood investments and shocks ([Cunha and Heckman, 2008](#)). These skills are shaped by maternal education, time with children, parenting styles, and family income, and can be intergenerationally transmitted ([Carneiro et al., 2013](#); [Fletcher and Wolfe, 2016](#); [Fiorini and Keane, 2014](#); [Attanasio et al., 2020](#)). This paper highlights that adverse events in a mother’s life—such as exposure to armed conflict, even years before giving birth—can disrupt the development of non-cognitive skills in her children.

Finally, our findings contribute to the body of work examining the specific legacy of the civil conflict in Peru. The literature so far has shown a variety of immediate and lasting impacts on the exposed generation. Exposure to episodes of violence led to a decrease in years of education ([León, 2012](#)), earnings and quality of jobs ([Galdo, 2013](#)), height among women ([Grimard and Laszlo, 2014](#)), the probability of incarceration ([Sara, 2020](#)), the likelihood of voting and its perception of importance ([Chavez and Malasquez, 2021](#)) and an increase in the probability of experiencing domestic violence ([Gallegos and Gutierrez, 2016](#)). Our paper contributes to this literature by showing for the first time that the adverse effects also extend to the next generation.

The paper is organized as follows: Section 2.2 outlines the conceptual framework. Section 2.3 provides background on the Peruvian civil conflict. Section 2.4 describes the data. Section 2.5 explains the identification strategy. Section 2.6 presents the results, followed by a discussion of mechanisms in Section 2.7. Section 2.8 concludes.

## 2.2 Conceptual framework

Parental exposure to armed conflict—whether during childhood, adolescence, or adulthood—can influence the socio-emotional development of their offspring, even when the children themselves are not directly exposed to violence (second generation effects). To illustrate this, drawing on the theoretical frameworks of [Cunha and Heckman \(2008\)](#) and [Cunha et al. \(2010\)](#), we consider the following static production function for the socio-emotional skills of children:

$$S^C = f(I^P, S^P, C^P, \theta) \quad (2.1)$$

where  $S^C$ , the socio-emotional skill set of the children, is determined by investments made by parents ( $I^P$ ), parent's own stock of socio-emotional skills ( $S^P$ ) and cognitive skills ( $C^P$ ), as well as other factors ( $\theta$ ) that are not directly observed. The production function  $f(\cdot)$  is assumed to be monotone increasing in its arguments.

Parents choose investments ( $I^P$ ) based on their resources and preferences. Exposure to conflict can disrupt parental education, health, and labor market opportunities, leading to long-term reductions in earnings and wealth (León, 2012; Galdo, 2013; Grimard and Laszlo, 2014; Swee, 2015). These economic disruptions diminish the resources available to parents—key factors for fostering socio-emotional development in children (Fletcher and Wolfe, 2016; Mitchell et al., 2023).

Exposure to conflict can also shift parents' investment decisions by disrupting family planning and altering the perceived costs and benefits of having children, particularly in economically vulnerable conditions. Conflict may lead to an increase in early marriages and higher fertility rates (Castro Torres and Urdinola, 2019; Ajefu and Casale, 2021), as families often view children as economic assets resulting in larger families with fewer resources per child. Consequently, following Becker's "quantity-quality" trade-off hypothesis, limited resources—such as time, finances, and emotional support from parents—can hinder each child's development (Becker and Lewis, 1973). Recent evidence indicates that increases in family size adversely affect the formation of children's socio-emotional skills (Briole et al., 2020).<sup>1</sup>

Adolescent mothers, in particular, may lack the parenting skills, maturity, and knowledge required for optimal investments in their children's development. They often have limited bargaining power within the household, reducing their influence on decisions about children's cognitive and non-cognitive skills (Perez-Alvarez and Favara, 2023). Conflict further exacerbates gender inequalities and heightens women's economic dependence on partners, restricting mothers' autonomy in child-rearing decisions (Ajefu and Casale, 2021; La Mattina, 2017).

Uncertainty about returns on investments in children, which are often realized far in the future, can further shape parental decisions. Risk and time preferences play a crucial role in influencing how parents allocate resources to develop children's skills (Tanaka and Yamano, 2015). Evidence suggests that exposure to conflict increases risk aversion and raises discount rates (Voors et al., 2012; Vesco et al., 2025), potentially discouraging long-term investments in children's development.

Research shows that socio-emotional skills are transmitted across generations (Grönqvist et al., 2017; Attanasio et al., 2020). Exposure to conflict is strongly associated with the onset of mental disorders in parents (Swee, 2015; Singhal, 2019; Vesco et al., 2025), which can

<sup>1</sup>Conversely, while focusing family resources on an only child can boost their human capital, exclusive parental attention may foster overindulgence, and the absence of sibling interaction can hinder the development of socio-emotional attributes valued in the labor market (Chen et al., 2023).

disrupt family dynamics and adversely impact children. Caregivers grappling with emotional distress may struggle to form secure attachments or provide consistent parenting practices, such as engaging positively with their children, taking an active interest in their activities, and offering comfort during distress. These difficulties can perpetuate negative developmental cycles (Cummings et al., 1994; Sánchez-Ariza et al., 2023). In low- and middle-income countries (LMICs), parents exposed to violence often adopt harsher parenting practices, including corporal punishment, further undermining children's socio-emotional health (Mitchell et al., 2010; Morales and Singh, 2016). A recent study by Sánchez-Ariza et al. (2023) found that a psychosocial program for caregivers affected by conflict and displacement in Colombia improved not only caregivers' mental health but also the quality of child-caregiver interactions and children's socio-emotional development.

Finally, parental cognitive skills also exert a significant influence on children's socio-emotional development (Carneiro et al., 2013). A substantial body of evidence indicates that conflict devastates educational outcomes in both the short and the long run—damaging school infrastructure, increasing student and teacher absenteeism, and reducing perceived returns to education (León, 2012; Shemyakina, 2011; Swee, 2015; Vesco et al., 2025)—thus potentially impairing parents' cognitive foundations and, by extension, their children's socio-emotional growth.

In the framework presented above, the arguments of the child's socio-emotional skills production function can be affected by parent's exposure to conflict at all stages of their life (see Vesco et al., 2025 for a recent review). In the rest of this paper, we focus on the transmission of maternal exposure to conflict rather than parental exposure more broadly. This choice reflects the fact that mothers are often the primary caregivers in LMICs. In the Young Lives dataset, which we use for our analysis, 98% of mothers were identified as the primary caregivers in the baseline round. As primary caregivers, mothers spend the most time with children and make critical decisions regarding their well-being and development during formative years. This role positions them at the center of the intergenerational transmission of trauma and resilience, particularly in contexts of conflict exposure.

Lastly, note that some second-generation effects discussed here may reflect third-generation impacts. Conflict exposure during a parent's childhood or adolescence can reduce resources for their children (second generation) if their own parents were affected, limiting investments in them and indirectly impacting grandchildren (third generation). As our data cannot disentangle these effects, we abstract from these differences, and only consider net intergenerational effects based on mother exposure to conflict.

## 2.3 The Peruvian Civil Conflict (1980-2000)

The civil conflict in Peru during the 1980s and 1990s is regarded as the most violent period in the country's republican history and one of the longest-running conflicts in Latin America.

In the mid-1970s, the “Communist Party of Peru – Shining Path” (PCP-SL), inspired by the Chinese Cultural Revolution, initiated a “popular war” that escalated into an armed conflict with the Peruvian government. This occurred against the backdrop of significant institutional changes and unfavorable socioeconomic conditions, including a poverty rate of 60%, extreme income inequality (with the richest 10% controlling 45% of wealth), high unemployment, and political instability, despite growth in higher education (Gonzales de Olarte et al., 1991).

These conditions fueled the spread of revolutionary ideas by the PCP-SL in the late 1970s.<sup>2</sup> The PCP-SL’s plan aimed for staged domination—first in rural highland areas, followed by cities, and ultimately the capital, Lima (McCormick, 1992). The first violent act occurred in 1980 in Ayacucho, a southern highland region. As violence spread, the Peruvian government deployed the Armed Forces to Ayacucho in 1982. Anti-terrorist operations by the police and military involved widespread human rights violations, trapping civilians between state forces and insurgents.<sup>3</sup>

Figure 2.1 illustrates the geographical spread and intensity of violence from 1980 to 2000. The pattern reflects the PCP-SL’s strategy, advancing from rural to urban areas. Official records attribute approximately 24,000 fatalities to the PCP-SL, MRTA, and government forces during the conflict (Truth and Reconciliation Commission, 2004b).<sup>4</sup> The peak period of violence occurred between 1983 and 1993, with two critical surges. The first, in 1983, followed the government’s launch of anti-terrorist operations. The second peak, in 1989, marked the PCP-SL’s strategic shift to prioritize urban warfare, particularly in Lima. This period saw intensified violence and greater mobilization in both rural and urban areas (Truth and Reconciliation Commission, 2004a). For detailed yearly victim counts, see online appendix Table B1.

In 1992, the capture of the PCP-SL leader and much of the central committee led to a sharp decline in violent activity and diminished insurgent influence. However, human rights violations continued for several years. By 2000, the end of Alberto Fujimori’s regime and the marginalization of the PCP-SL in the jungle regions marked the close of Peru’s “terrorism era”.

## 2.4 Data

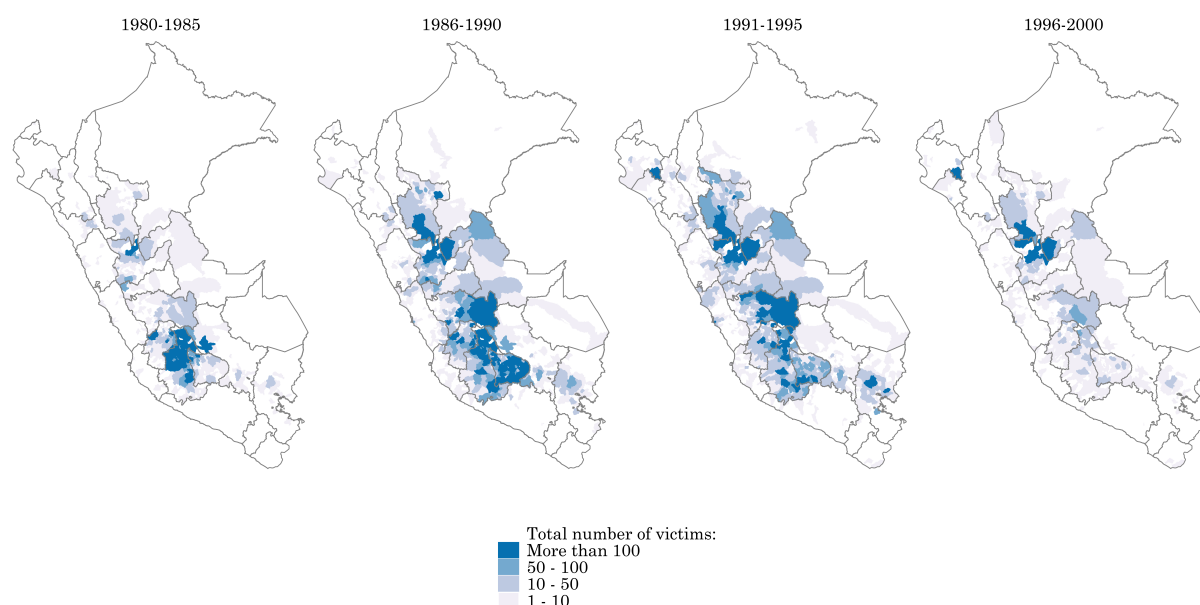
This section outlines the primary data sources and key variables used in the analysis. First, we describe the data from the Peruvian Truth and Reconciliation Commission (TRC), which

<sup>2</sup>By the end of 1982, the Marxist-Leninist group Tupac Amaru Revolutionary Movement (MRTA) was also established, contributing to terrorist violence during the 1980s and 1990s, albeit to a lesser extent (Truth and Reconciliation Commission, 2004b).

<sup>3</sup>The PCP-SL was estimated to be responsible for 53.68% of deaths and disappearances, whereas state agents, self-defense committees, and paramilitaries were responsible for 37.26% (Truth and Reconciliation Commission, 2004a).

<sup>4</sup>Peru’s Truth and Reconciliation Commission (TRC) estimated the true number of deaths and disappearances at 69,280, with a 95% confidence interval between 61,007 and 77,552.

**FIGURE 2.1 – GEOGRAPHICAL EXPANSION OF THE CONFLICT: NUMBER OF VICTIMS, BY DISTRICT**



*Notes:* The figure shows the variation over time in the total number of victims of violent acts (e.g., forced disappearances, torture, murder, kidnapping) in each district from 1980 to 2000.

*Source:* Truth and Reconciliation Commission.

provides information on the temporal and spatial trajectory of violence during the conflict in Peru at the district and year levels. Second, we introduce the Young Lives survey, which is used to examine the intergenerational impacts of the conflict. We then detail the key variables for the analysis, including the intergenerational conflict exposure measure (linking TRC and YL data) and the socio-emotional skill assessments available in the YL survey. Finally, we present descriptive statistics for the sample.

### 2.4.1 Peruvian Truth and Reconciliation Commission (TRC)

Data on the Peruvian armed conflict are drawn from Peru's Truth and Reconciliation Commission (TRC), established in 2001 to construct a "historical memory" of the Civil War period. The TRC documented human rights violations committed by terrorist organizations, state agents, and paramilitary groups between 1980 and 2000, culminating in the publication of the Final Report in 2003.<sup>5</sup>

The TRC collected testimonies from victims or their relatives through public hearings and regional offices set up across Peru. These testimonies were validated using records from NGOs, the Red Cross, and human rights organizations (Truth and Reconciliation Commission, 2004a; León, 2012). Each violent act was systematically coded by location, date, victims, perpetrators, and type of act (e.g., murder, disappearance, rape, torture, kidnapping). Al-

<sup>5</sup>The full report is publicly available at: <https://www.cverdad.org.pe/ifinal/>

though the Final Report was published in 2003, victims could continue to submit testimonies, and the database has been updated since then. The latest dataset, accessed via the Ministry of Justice's transparency portal (<https://www.transparencia.gob.pe>) in 2022, includes over 36,000 violent events. A breakdown of cases by type of violence is provided in Table B2 of the online appendix.

The TRC database provides a detailed account of the conflict's progression over time at the district level, documenting the number and nature of violent acts and their perpetrators. To facilitate data collection, the TRC established regional offices, assemblies, and public hearings in 509 districts, complemented by local administrative offices and mobile teams that traveled to more remote areas. Additionally, since data collection occurred after the conflict, reports of violence often originated from locations different from where testimonies were recorded. Testimonies were also gathered from relatives of victims, further expanding the geographic scope of the data. As a result, the TRC was able to construct a comprehensive record of the conflict's impact across Peru.<sup>6</sup> To address concerns about potential bias in self-reported violence due to unobserved characteristics, our primary measure of violence focuses on fatal events (deaths or disappearances) at the district level, as these types of acts are less likely to be under-reported or misclassified. We later demonstrate that our results remain robust to alternative measures of conflict exposure.

## 2.4.2 Young Lives longitudinal study for Peru

Data on the outcomes of interest for both mothers and their offspring come from the Young Lives (YL) longitudinal study in Peru, an international cohort study on childhood poverty. YL conducted five rounds of in-person surveys in 2002, 2006, 2009, 2013, and 2016, referred to as Rounds 1 through 5. The study tracked approximately 2,000 index children born in 2001–2002 (the “Younger Cohort”) at ages 1, 5, 8, 12, and 15 during each survey round.<sup>7</sup> The timeline of the study is illustrated in Figure 2.2.

The YL sample was selected through a multistage sampling procedure, beginning with the random selection of 20 districts in Peru (excluding the wealthiest 5%) based on a district-level poverty map. Within these districts, manzanas or centros poblados (blocks or clusters of housing, referred to as sentinel sites) were randomly chosen. Finally, 100 eligible households with a child aged 6 to 18 months were randomly selected from each sentinel site (Escobal and Flores, 2009). Although YL was not designed to be nationally representative, Escobal and Flores (2009) provide statistical evidence that key wealth and living standard indicators in the overall YL sample closely tracked Peru's regional distribution at the time of the first survey.<sup>8</sup>

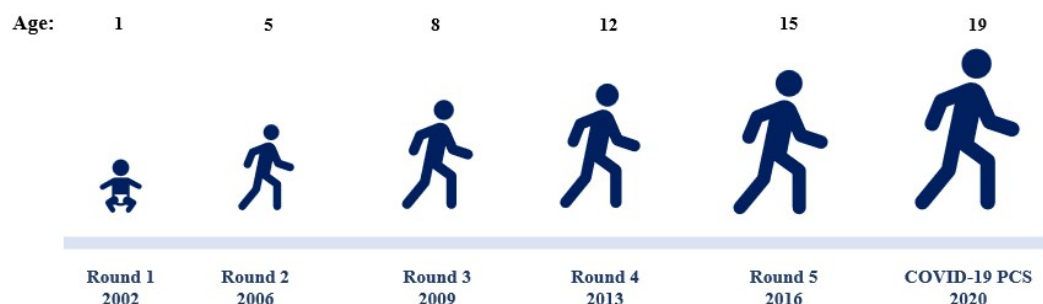
<sup>6</sup>According to the TRC, 1166 districts in the country (out of 1864) were affected by some type of violence.

<sup>7</sup>The study also tracked an “Older Cohort” of approximately 800 individuals aged 7.5 to 8.5 years old in Round 1. This cohort is excluded from the present study to avoid including individuals directly affected by the civil conflict.

<sup>8</sup>Their comparison of the YL Peru Round 1 sample (2002) with two nationally representative surveys –

Notably, YL achieved exceptionally low attrition rates compared to similar longitudinal studies in LMICs, with an annual attrition rate of only 0.6% (Sánchez and Escobal, 2020).

**FIGURE 2.2 – YOUNG LIVES STUDY TIMELINE**



The YL survey was designed to analyse the drivers and impacts of child poverty (Wilson et al., 2006). The survey captures a wide range of information on the index child, parents, household, and community, including socio-emotional dimensions such as agency, pride, and risky behaviors. Key variables for this research include the mother’s date and place of birth. The mother’s date of birth is deduced from her reported month of birth (collected in 2013) and age at the time of the interview. Place of birth (at the district level) was collected in 2006 as part of a special migration history module which recorded detailed lifetime migration histories of mothers, including geographical locations and time references for all movements up from birth to 2006.

This analysis restricts the sample to YL participants whose mothers are primary caregivers, with available data on their place and date of birth, and for whom relevant covariates, and socio-emotional outcomes are available.<sup>9,10</sup> Our primary analysis emphasizes the earliest socio-emotional measurements, collected in 2009 (YL round 3) when participants were 8 years old.

#### 2.4.2.1 Measuring conflict exposure using YL and TRC data

To impute maternal exposure to violence, we use data on fatal events (deaths or disappearances) that occurred in the mother’s district of birth from the time she was born, recorded at the month-year level.<sup>11</sup> A mother is considered affected by conflict in a given month if

the Living Standard Measurement Survey 2001 (ENAHO) and DHS 2000 – shows that (i) poverty rates in the YL sample align with national urban and rural averages from ENAHO, and (ii) the wealth distribution in the YL sample mirrors that of DHS.

<sup>9</sup>To assess whether this data limitation introduces bias, we compare the original YL sample with the analytical sample. As shown in Table B3 of the online appendix, the analytical sample does not differ significantly from the original YL sample across a range of maternal, household, and child characteristics, suggesting that the restriction is unlikely to bias the results.

<sup>10</sup>Information on place of birth is available for only a non-random subset (68%) of fathers, limiting the inclusion of paternal exposure.

<sup>11</sup>This definition aligns with the literature on the impacts of the Peruvian civil conflict. See: León (2012): pp. 1006, Galdo (2013): pp. 800, Morales and Singh (2016): pp. 14-15, Grimard and Laszlo (2014): pp. 142 and Chavez and Malasquez (2021): pp. 14.

at least one fatal event occurred in her district of birth during that time. Since the conflict spanned 20 years, potential cumulative exposure ranges from 0 to 240 months. For simplicity of interpretation, we divide the total by 12 to express exposure in years. This yields the primary conflict exposure variable, defined as:

$$\text{MotherExposure}_{dtm} = \text{Years of conflict}_{d,tm} = \frac{\sum_{tm=1}^{240} \mathbf{1}(\text{casualty}_{d,tm})}{12} \quad (2.2)$$

where  $\mathbf{1}(\text{casualty}_{d,tm}) = 1$ , if  $\text{casualty}_{d,tm} > 0$

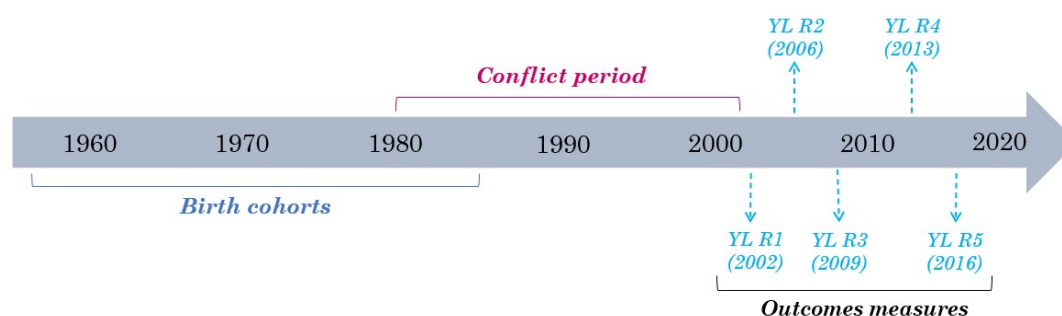
Here, *Years of conflict*<sub>*d,tm*</sub> represents the cumulative years of exposure to conflict in district *d*, where *t* and *m* denote the year and month of the mother's birth, and *tm* covers all month-year combinations since birth. The variable *casualty*<sub>*d,tm*</sub> reflects the number of casualties in district *d* for a given month-year period. For the primary analysis, we consider only months with fatal events—deaths or disappearances—to minimize potential bias from self-reported violence. To reduce measurement error and avoid over-reliance on extreme cases, we use a binary indicator for the presence of violence (*casualty* > 0), ensuring the results are not driven by outliers or inconsistencies in casualty reporting. In Section 2.6.2, we show that the results remain robust when non-fatal events are included and when using casualty counts as a measure of conflict intensity. While the primary analysis uses cumulative years of conflict exposure, we also examine how the results vary based on the mother's exposure at different stages of her life (Section 2.6.4). This measure also assumes no migration occurred between the mother's birth and the first YL survey in 2002, an assumption that is relaxed and tested in Section 2.6.3.

By combining individual-level YL data with district-level conflict data from the TRC, we construct measures of maternal exposure to violence, allowing for an assessment of inter-generational impacts. YL mothers were born between 1954 and 1989, overlapping with the Peruvian civil conflict from 1980 to 2000. The timeline of the conflict and YL data collection rounds is shown in Figure 2.3, while Figure B1 in the online appendix illustrates the overlap between YL mothers' birth years and the conflict period. Based on the mother's district of birth, the YL sample spans 428 districts, representing 22.8% of all districts in Peru. Of these, 66.1% experienced at least one victim of violence (fatal or non-fatal) during the conflict, higher than the national distribution of affected districts (54.2%). Descriptive statistics on maternal exposure to violence are presented in Section 2.4.3.

#### 2.4.2.2 Socio-emotional skills

Socio-emotional skills, also sometimes referred to as “non-cognitive” skills, include behavioral attributes such as motivation and self-esteem. These skills are vital for adolescent development and are positively correlated with future social and economic opportunities (Heckman et al., 2006). Additionally, the development of cognitive and socio-emotional skills is

**FIGURE 2.3 – CIVIL CONFLICT IN PERU AND YOUNG LIVES STUDY TIMELINE**



mutually reinforcing, with gains in one domain supporting progress in the other (Heckman et al., 2006; Dercon and Krishnan, 2009; Mitchell et al., 2023; Cunha and Heckman, 2008).

In the YL study, socio-emotional skills were measured through indicators such as agency and pride, derived from educational psychology literature and adapted to the cultural context through extensive piloting.<sup>12</sup> Agency refers to an individual's perception of control over their life, signifying the belief that outcomes result from one's actions rather than external factors such as luck, fate, or others' involvement (Rotter, 1966). Individuals with high levels of internal agency, or locus of control, attribute outcomes to their own efforts or behaviors (Bandura et al., 2006). In the YL study, the agency scale is adapted from existing scales of locus of control developed by Rotter (1966) and Bandura (1977), modified to be relevant for children. It focuses on specific life domains such as time use, school, and work, ensuring cultural and contextual relevance (Ogando and Yorke, 2018). Pride, on the other hand, assesses an individual's self-evaluation and is a modified version of the Rosenberg Self-Esteem Scale (Rosenberg, 1965), and is linked to specific aspects of the child's living circumstances such as school, employment, attire and housing (Dercon and Krishnan, 2009).

The YL study measures socio-emotional skills from age 8 through self-reported assessments of agency and pride, administered in the 2009, 2013, and 2016 survey rounds. Each indicator consists of five statements rated on a 4-point Likert scale, from strong disagreement to strong agreement, as detailed in Table B4 of the online appendix. Additionally, assessments of self-efficacy (Schwarzer and Jerusalem, 1995), linked to the concept of agency, and self-esteem (Shavelson et al., 1976), associated with pride, were collected at ages 12 and 15.<sup>13</sup> For the main analysis, we focus on data collected at age 8 (agency and pride), which provides the earliest measurement of socio-emotional skills. Results using later rounds of agency and pride, as well as additional measures of self-efficacy and self-esteem, are presented in Section 2.6.4.2.

The construction of these indices followed a systematic process: statements with negative connotations were reverse-coded to ensure higher scores represent more positive outcomes. Each item was standardized by the child's age to have a mean of zero and a standard deviation of one. The final indices were calculated as the simple mean of non-missing standardized item

<sup>12</sup>For details on measurement and validation during the YL surveys, see Ogando and Yorke (2018).

<sup>13</sup>Details on the statements comprising each variable can be found in Table B4.

scores.<sup>14</sup> Histograms of the standardized scores are presented in the online appendix [Figure B3](#) and [Figure B4](#).

### 2.4.3 Descriptive statistics

[Table 2.1](#) presents descriptive statistics for the balanced sample of 1,752 YL index children and their mothers (sample without missing values in outcomes or relevant variables). Results are reported for the full sample (columns 1-2) and disaggregated by whether the mother was ever exposed to conflict (columns 2-3 and 4-5, respectively). Panel A summarizes maternal characteristics: on average, mothers were 27 years old in 2002, 14% had some tertiary education, 30% had an indigenous native tongue, and 30% were born in a location different from their residence during the first-round interviews in 2002.

Panel B provides child-level characteristics: 50% of YL individuals are female, 39% were the mother's firstborn, and they started first grade at an average age of 5.65. On average, they were about 12 months old during the first wave. Panel C presents maternal conflict exposure: 64% of mothers experienced conflict at some point in their lives, with an average exposure of 1.13 years for the full sample (column 1) and 1.76 years for those exposed (column 2). A similar pattern holds across different age periods.

Finally, column 7 of [Table 2.1](#) tests differences in mother and child characteristics based on whether the mother was ever exposed to conflict. Exposed mothers are more likely to have lower education levels, an indigenous native tongue, a rural background, and belong to larger households (Column 4). In contrast, there are no significant differences in children's demographic characteristics based on maternal exposure to conflict.

## 2.5 Empirical Strategy

Following [León \(2012\)](#), the identification strategy relies on the nature of the Peruvian Civil Conflict (1980-2000), in particular the exogenous temporal and geographical variation of violent acts. As discussed in [Section 2.3](#), the strategy used by the PCP-SL was political and used erratic, violent forces designed to establish a generalised sense of terror and fear in the Peruvian territory.

The following equation is used to estimate the effect of the conflict on the socio-emotional skills of the next generation:

$$Y_{oidtm} = \beta_0 + \beta_1 \text{MotherExposure}_{dtm} + X'_{oidt} \beta + Z'_p \varphi + \gamma_r + \delta_t + \theta_o + \phi_k + \epsilon_{ijdtm} \quad (2.3)$$

where  $Y_{oidtm}$  is the outcome of the child  $o$ , born to a woman  $i$ , who was born in district  $d$ , in year  $t$  and month  $m$ . The outcomes are socio-emotional skills of *Agency*, *Pride* and

<sup>14</sup>Following [Onatski and Wang \(2021\)](#), this method was preferred over principal component analysis (PCA).

**TABLE 2.1 – SUMMARY STATISTICS, BY MOTHER’S EXPOSURE TO VIOLENCE**

	Total		Exposed		Not exposed		Difference
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	
<b>Panel A: Mother characteristics</b>							
Mother’s age (years), 2002	27.04	(6.68)	26.90	(6.83)	27.29	(6.40)	0.39
Mother has complete primary or less	0.49	(0.50)	0.52	(0.50)	0.44	(0.50)	-0.08**
Mother has complete (or incomplete) secondary	0.37	(0.48)	0.34	(0.47)	0.41	(0.49)	0.06**
Mother’s native tongue is indigenous	0.30	(0.46)	0.38	(0.49)	0.15	(0.36)	-0.23***
Mother lived in a different district than her birthplace by 1999	0.30	(0.46)	0.30	(0.46)	0.30	(0.46)	0.00
Mother lived in urban area, 2002	0.69	(0.46)	0.66	(0.47)	0.73	(0.44)	0.07**
Caregiver is the head’s household	0.07	(0.25)	0.08	(0.27)	0.06	(0.23)	-0.02
Household size, 2002	5.69	(2.33)	5.77	(2.39)	5.53	(2.20)	-0.25*
Mother’s number of children, 2002	2.69	(2.09)	2.77	(2.20)	2.54	(1.88)	-0.23*
Wealth index, 2002	0.43	(0.24)	0.43	(0.24)	0.42	(0.22)	-0.01
Mother does not have a permanent partner, 2002	0.12	(0.33)	0.14	(0.34)	0.10	(0.30)	-0.04*
<b>Panel B: YL child characteristics</b>							
YL is female	0.50	(0.50)	0.50	(0.50)	0.49	(0.50)	-0.01
Age in months, 2002	11.55	(3.53)	11.49	(3.51)	11.65	(3.58)	0.17
YL is the oldest sibling	0.39	(0.49)	0.39	(0.49)	0.37	(0.48)	-0.03
Child’s age at start of grade 1	5.65	(0.55)	5.66	(0.56)	5.62	(0.52)	-0.04
<b>Panel C: Mother’s exposure to conflict</b>							
Mothers affected by conflict (percent)	0.64	(0.48)	N.A.	N.A.	N.A.	N.A.	N.A.
Mother’s exposure to conflict	1.13	(2.25)	1.76	(2.60)	N.A.	N.A.	N.A.
Mother’s exposure to conflict at ages 0-5	0.16	(0.61)	0.24	(0.74)	N.A.	N.A.	N.A.
Mother’s exposure to conflict at ages 6-10	0.27	(0.72)	0.42	(0.86)	N.A.	N.A.	N.A.
Mother’s exposure to conflict at ages 11-19	0.44	(1.04)	0.69	(1.23)	N.A.	N.A.	N.A.
Mother’s exposure to conflict at ages 20+	0.25	(0.98)	0.40	(1.20)	N.A.	N.A.	N.A.

*Notes:* The conflict exposure variables are defined as the total number of months a mother was exposed to at least one fatal event due to conflict in her birth district across a given period of her life (the variable is re-scaled in years). The wealth index is a measure of multidimensional poverty used in YL (see Briones, 2017). Summary statistics are reported for the full sample (Columns 1-2), and according to whether or not the mothers were ever exposed to the conflict (Columns 3-6). Column 7 tests for differences between means among non-exposed and exposed mothers. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

an average of the two (called *Index*). The main independent variable,  $MotherExposure_{dmt}$ , measures maternal years of exposure to conflict throughout her life, based on the mother’s birth in district  $d$  in month  $m$  and year  $t$ . Exposure is calculated in months and rescaled to years for simplicity. The coefficient  $\beta_1$  then captures the effect of an additional year of maternal conflict exposure during the Peruvian civil war.

The model includes fixed effects for the mother’s region of birth ( $\gamma_r$ , district of birth  $d$  belonging to region  $r$ ) to control for persistent regional effects of violence, and year of birth ( $\delta_t$ ) to account for cohort effects.<sup>15</sup> At the child level, we control for birth cohort (month-year) fixed effects ( $\theta_o$ ) and YL sentinel site fixed effects ( $\phi_k$ ), addressing time-invariant site-specific characteristics. Additional controls include child’s sex and mother’s native tongue (a proxy for ethnicity) in vector  $X_i$  and pre-conflict provincial characteristics in vector  $Z_p$  (province of birth  $p$  belonging to region  $r$ ), drawn from the 1972 Peruvian census.<sup>16</sup> The error term,  $\epsilon_{ijdt}$ ,

<sup>15</sup>Month of birth fixed effects are excluded due to limited variation in the relatively small sample.

<sup>16</sup>Pre-conflict provincial controls include population size (log), female illiteracy rate, and access to piped

captures unexplained variation in the outcome, and standard errors are clustered at the YL sentinel site level, reflecting the multistage sampling procedure (Abadie et al., 2023).

The crucial assumption is that any relationship between exposure to violence and children's development outcomes is unlikely to be driven by omitted variables once geographic and cohort-specific variables are controlled for, such as differential inequality trends within the country. Section 2.6.3 discusses these factors and how they could affect our main results.

## 2.6 Main Results

### 2.6.1 Intergenerational effects of conflict

Table 2.2 presents the effects of maternal conflict exposure on children's socio-emotional outcomes at age 8, including agency, pride, and a combined socio-emotional index. The results show that maternal exposure to conflict has a significant negative impact on these outcomes, measured years after the conflict ended. Each additional year of exposure lowers the agency index by 0.016SD, the pride index by 0.017SD, and the combined socio-emotional index by 0.016SD — all statistically significant at the 1% level. For the average exposure of 1.13 years, this corresponds to a reduction of 0.018SD. As shown in Table B5 in the online appendix, the point estimates remain stable with the inclusion of additional controls and fixed effects.

**TABLE 2.2 – INTERGENERATIONAL EFFECT OF CONFLICT EXPOSURE ON CHILD'S SOCIO-EMOTIONAL OUTCOMES AT AGE 8**

	Agency (1)	Pride (2)	Index (3)
Mother's exposure to conflict	-0.016*** (0.004)	-0.017*** (0.005)	-0.016*** (0.003)
Observations	1,752	1,752	1,752
R-squared	0.099	0.108	0.136
Estimated mean effect	-0.018 (0.004)	-0.019 (0.005)	-0.018 (0.003)

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). The estimated mean effect coefficient is calculated by multiplying the coefficient and the average exposure of the mother to conflict. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

For the main specification, we use the number of years a mother was exposed to any type of fatal event as a proxy for her cumulative conflict exposure. However, the results water, with data available only at the provincial level.

are robust to alternative definitions of violence, holding regardless of the type of violent act considered—whether deaths, disappearances, sexual violence, or torture—as detailed in subsection 2.6.2. Additionally, the results are not driven by selective migration or region-level inequality, as demonstrated in subsection 2.6.3.

To contextualize our results, we compare the estimated impact of maternal conflict exposure with other adverse events affecting children’s agency and pride at this age. [Sánchez \(2017\)](#) report that a one standard deviation increase in height at age 1 (a proxy for improved early-life nutrition) enhances socio-emotional skills at age 8 by 0.026SD. Similarly, [Bedoya et al. \(2020\)](#) find that maternal exposure to alcohol-induced intimate partner violence (IPV) reduces a child’s agency score at age 8 by 0.14SD. The intergenerational effects observed in our study are slightly smaller but align with broader literature suggesting that external shocks generally have limited impacts on socio-emotional skills—both in absolute terms and relative to cognitive skills. For instance, [Cunha and Heckman \(2008\)](#) find that parental investments yield smaller effects on non-cognitive skills (0.15SD) compared to cognitive skills (0.23SD). Similarly, [Dercon and Sánchez \(2013\)](#) show that while maternal socio-emotional skills significantly predict child development, they explain only a modest share of the variation in children’s socio-emotional outcomes.

Finally, [Table B6](#) in the online appendix explores heterogeneous effects by sex, socioeconomic status (bottom tercile of wealth distribution), and area of residence (urban vs. rural). The results indicate no significant differences by wealth or area of residence. However, maternal conflict exposure has a stronger negative impact on girls compared to boys, highlighting potential gendered vulnerabilities in socio-emotional development.

## 2.6.2 Robustness checks

This subsection demonstrates the robustness of the results to alternative definitions of violence exposure, including variations in the type of violence experienced by mothers, the exclusion of mothers least or most affected by violence, and alternative standard error clustering.

We test the robustness of the main results by using alternative definitions of conflict. While our preferred measure focuses on maternal exposure to fatal violence (deaths and disappearances) to minimize reporting errors, we expand this to include both fatal and non-fatal violent events. Panel A of [Table 2.3](#) shows that the results remain qualitatively consistent with those in [Table 2.2](#). The findings also hold when restricting the measure to non-fatal violent events.

Next, we assess robustness by replacing years of exposure with the number of victims reported in the mother’s birth district after her birth.<sup>17</sup> Using detailed data from the TRC

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<sup>17</sup>The regression coefficients reflect the effect of each additional 100 victims for the corresponding type of violence.

dataset, we calculate victims under the following classifications: total victims (fatal and non-fatal), total fatalities (deaths and disappearances), and non-fatal victims (sexual violence, torture, and other violent events).<sup>18</sup> Panel B of Table 2.3 shows that higher victim counts in all these categories consistently have adverse effects on children's socio-emotional outcomes irrespective of the type of violent act. Notably, the coefficients for the sub-component sexual violence are larger than those for other types of violence.

**TABLE 2.3 – ALTERNATIVE MEASURES OF MOTHER'S EXPOSURE TO CONFLICT VIOLENCE**

	Agency (1)	Pride (2)	Index (3)
<i>Panel A. Measured in number of:</i>			
i. Years (at least one fatal or non-fatal violent victim)	-0.013*** (0.004)	-0.010** (0.005)	-0.012*** (0.003)
ii. Years (at least one non-fatal victim)	-0.017** (0.006)	-0.012* (0.006)	-0.014*** (0.004)
<i>Panel B. Measured in number of victims of violence:</i>			
i. Total victims (fatal and non-fatal)	-0.016*** (0.005)	-0.014*** (0.003)	-0.015*** (0.003)
ii. Fatal victims	-0.026*** (0.006)	-0.023*** (0.005)	-0.025*** (0.004)
a. Death	-0.039*** (0.009)	-0.034*** (0.008)	-0.036*** (0.006)
b. Disappearance	-0.078*** (0.021)	-0.068*** (0.012)	-0.073*** (0.011)
iii. Non-fatal victims	-0.018* (0.009)	-0.014*** (0.003)	-0.016*** (0.005)
a. Sexual violence	-0.238* (0.129)	-0.130** (0.049)	-0.184** (0.067)
b. Torture	-0.024** (0.010)	-0.018*** (0.005)	-0.021*** (0.006)
c. Other events	-0.036*** (0.013)	-0.028*** (0.005)	-0.032*** (0.007)
Observations	1,752	1,752	1,752

*Notes:* Each row corresponds to an independent regression. Independent variables for Panel B stand for the total number of victims per 100 individuals per affectation type in a given year in the mother's birth district due to the conflict. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Index outcome refers to the average of both agency and pride indexes. Standard errors clustered at the YL sentinel site (location in 2002) and presented in parenthesis. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

In our primary specification, the main independent variable is the number of years of exposure (calculated with month-level precision), with 35% of mothers in the sample having

<sup>18</sup>Other non-fatal violent events include physical violence, wounded or injured victims and kidnapping.

zero months of exposure. This zero value may reflect two scenarios: either the mother was born in a district unaffected by violence throughout the conflict, or she resided in a district that experienced violence, but not the fatal types included in our definition (deaths and disappearances). To account for potential systematic differences in districts never affected by violence, we restrict the sample to mothers from districts that experienced at least some fatal violence during the Peruvian civil war. Results presented in [Table B7](#) of the online appendix remain consistent with the baseline findings, showing that maternal conflict exposure negatively affects children's socio-emotional development. To further confirm that our results are not driven by extreme events, we also re-estimate our baseline results by excluding the most heavily affected region (Ayacucho). Once again, the results are robust ([Table B8](#) of the online appendix).

Finally, while we have followed [Abadie et al. \(2023\)](#) to cluster the standard errors at the YL sentinel site level to account for the YL survey sampling process, the results are also robust to clustering at the level of the district ([Table B9](#) of the online appendix).

### 2.6.3 Threats to identification

In this subsection we discuss several potential challenges to our identification strategy. First, we examine the role of migration in biasing the results. Second, we explore whether region-level inequality trends might explain our main results. Furthermore, we carefully assess the composition of our sample, as any inherent biases or demographic differences could significantly affect the generalizability of our conclusions. Finally, we check for attrition-related bias in long-term follow-up surveys.

#### 2.6.3.1 Internal Migration

A critical implicit assumption in our analysis has been that mothers stayed in their district of birth during the conflict years (1980-2000). In reality, internal migration — particularly from rural to urban areas — was common in Peru, often in search of better opportunities or marriage. Furthermore, there might be a relationship between violence and migration. As violence escalated due to the dual threats of the PCP-SL insurgency and government counterinsurgency operations, residents of vulnerable rural areas were often forced to migrate to safer urban centers, with Lima emerging as a primary destination ([Truth and Reconciliation Commission, 2004a](#)). Previous research highlights that exposure to violence influences migration decisions, which may introduce bias in the estimated effects of conflict ([Pivovarova and Swee, 2015](#)). If wealth and the formation of socio-emotional skills are positively correlated and transmitted between generations ([Sánchez-Ariza et al., 2023](#)), greater exposure to conflict-induced migration could lead to a downward bias in our estimates. In contrast, as noted by [León \(2012\)](#) and [Sara \(2020\)](#), migrants often face discrimination, including limited labor opportunities and imperfect access to public goods and services, which can negatively

affect maternal mental health and well-being. In this case, relocation may fail to improve children's development, potentially introducing upward bias in the estimates.

To examine the role of internal migration, we begin by comparing mothers who migrated to those who did not. As shown in [Table 2.1](#), 30% of mothers migrated (i.e., moved to another district) at least once between their birth and the first YL survey in 2002. [Table B10](#) in the online appendix provides demographic comparisons between migrant and non-migrant mothers. The results indicate that mothers who migrated during the conflict period were more likely to come from wealthier and larger households and less likely to speak an indigenous language as their mother tongue.

The availability of detailed maternal migration histories allows us to conduct a more granular analysis. First, we construct a panel at the individual-year level to examine whether conflict in period  $t - 1$  predicts migration in period  $t$  during 1980–2000.<sup>19</sup> The results in [Table B11](#) of the online appendix provide evidence that conflict influenced migration decisions. Specifically, the occurrence of at least one fatal event in the district of residence in the previous year ( $t - 1$ ) increased the probability of migration in the following year ( $t$ ; column 1) and migration to capital cities (column 2), though it did not significantly predict migration to non-violent areas (column 3). However, when using the number of casualties as an alternative conflict measure, we find that casualties at  $t - 1$  increased the likelihood of migration to districts without violence ( $t$ ).

Second, we adjust the main independent variable in [Equation 3.1](#) to account for migrant mothers' district of residence in each year of the conflict.<sup>20</sup> Replicating our main results with this adjusted conflict exposure variable, we find muted—but still significant—negative intergenerational effects of conflict on children's socio-emotional skills ([Table B12](#) in the online appendix). These results suggest that migration partially mitigated the adverse impacts of conflict on socio-emotional development.

Although the point estimates are smaller, it is notable that our main results remain consistent when using adjusted exposure to conflict. One possible explanation is that migration did not always allow individuals to escape violence entirely. While the early years of the conflict primarily affected rural Andean areas, it later expanded to urban centers. As a result, individuals who migrated from rural to urban areas — whether to escape violence or seek better economic opportunities — may have eventually been affected as well.<sup>21</sup> Using the migration adjusted violence variable, [Table B13](#) of the online appendix compares conflict exposure between migrant and non-migrant mothers. We find that migrant mothers experienced slightly higher exposure (although the difference is not statistically significant). Interestingly, for mi-

<sup>19</sup>The maternal migration history module from the 2006 survey provides retrospective information on birthplaces, migration destinations, and ages at migration, enabling us to reconstruct individual migration panels.

<sup>20</sup>To minimize measurement error, the adjusted violence exposure variable for each year ( $t$ ) is calculated as the average exposure in the district of residence in  $t$ ,  $t - 1$ , and  $t + 1$ .

<sup>21</sup>The YL data show that mothers' migration during the conflict period was primarily rural-to-urban, with the capital city being the most common destination.

grant mothers, adjusted exposure to conflict is 0.188 years higher than unadjusted exposure, suggesting that migration, if used as a strategy to reduce violence exposure, was not entirely successful. To sum, this analysis highlights that while migration was used as a strategy to mitigate conflict exposure during the Peruvian civil war, it was not entirely successful in offsetting the negative inter-generational effects.

### 2.6.3.2 Inequality trends

Another potential threat to our identification strategy arises if region-level inequality trends during the conflict period were correlated with conflict location, such as differences in economic development or infrastructure investment. These factors can influence both political violence and child development. However, we argue this is unlikely, as the conflict's progression was driven by the strategic decisions of the PCP-SL and the government, including the shift from rural to urban areas and retreat to the Amazon jungle— factors unrelated to inequality trends. To address this concern, in addition to the pre-conflict provincial characteristics included in the main specification (Equation 3.3), we also control for region-specific linear time trends. As shown in Table B14 of the online appendix, our baseline results remain robust.

### 2.6.3.3 Sample composition

Our results could also be biased by selective mortality. To assess this, we examine whether birth cohort size correlates with conflict exposure. Using the 2007 Peruvian census data, we construct a district-year panel where the outcome of interest is the district-level birth cohort size for the years covered by the YL sample (1954–1987). As shown in Table B15 of the online appendix, we find no systematic relationship between conflict exposure and cohort size. This result aligns with León (2012), who similarly found no statistically significant impact of conflict exposure on district-level population density using the 1993 and 2007 Peruvian censuses.

### 2.6.3.4 Selective attrition in YL sample

The final concern with longitudinal data analysis is attrition, where individuals cannot be located or refuse to participate in later survey rounds, potentially leading to unrepresentative samples. However, this is not a significant issue for our study using the YL dataset, which has one of the lowest attrition rates among longitudinal studies in developing countries (Sánchez and Escobal, 2020). For the younger cohort in Peru, attrition rates were only 5.3% in 2009 and 9.4% in 2016. Furthermore, to investigate the possibility that attrition linked to our primary explanatory variable, we assess the probability of an individual exiting the YL sample by 2009, 2013, and 2016 rounds, conditional on being present in 2001 round and the availability of all relevant maternal variables. Our results show no significant correlation between attrition

and conflict exposure, confirming that attrition does not bias our estimates (Table B16 of the online appendix).

## 2.6.4 Additional results

This section examines additional findings on the intergenerational impact of conflict. We first investigate whether the effects on children vary based on the mother's age during conflict exposure. Next, we assess the persistence of socio-emotional impacts as children grow older. Finally, given strong linkages between socio-emotional competencies and risky behaviors, we explore whether maternal conflict exposure influences children's engagement in risky behaviors.

### 2.6.4.1 Critical periods of conflict exposure

The timing of conflict exposure is crucial, as understanding which life stages are most affected can inform more effective remedial and intervention strategies. While our primary analysis focuses on cumulative years of maternal conflict exposure, we also examine how the timing of exposure during different stages of the mother's life influences child outcomes.

The critical importance of fetal and early childhood periods (up to age 5) for shaping long-term outcomes has been documented well in the literature (Almlund et al., 2011; Grantham-McGregor et al., 2007). However, there is a growing body of research suggesting that later developmental stages, particularly adolescence, may also play a pivotal role in long-term socio-economic well-being (Akresh et al., 2023; Gangadharan et al., 2022; León, 2012; Sara, 2020; Van den Berg et al., 2014). To investigate this further, we disaggregate maternal conflict exposure into four life-cycle periods: early childhood (0–5 years), primary school years (6–10 years), secondary school years/adolescence (11–19 years), and adulthood (20 years or older). These indicators are constructed following Equation 3.1, with the violence variable conceptualized as the cumulative months (rescaled to years) of conflict exposure during these critical age intervals.

Table 2.4 shows a negative impact of maternal conflict exposure on the socio-emotional development of offspring, with early childhood (0–5 years) and adolescence (11–19 years) being particularly significant. Each additional year of maternal conflict exposure during early childhood and adolescence is associated with a 0.07SD and 0.02SD decrease in the child's socio-emotional index, respectively. This evidence contributes to the expanding literature identifying later childhood stages as crucial periods for development.

### 2.6.4.2 Persistent effects

We now assess whether maternal conflict exposure has a persistent effect on a child's long-term development by including all socio-emotional outcomes measured in later rounds of the

**TABLE 2.4 – INTERGENERATIONAL EFFECT OF CONFLICT EXPOSURE AT AGE 8: BY DIFFERENT AGE OF EXPOSURE PERIODS**

	Agency (1)	Pride (2)	Index (3)
Mother's exposure to conflict at ages 0-5	-0.053** (0.021)	-0.083*** (0.022)	-0.068*** (0.019)
Mother's exposure to conflict at ages 6-10	-0.000 (0.022)	0.047 (0.030)	0.023 (0.018)
Mother's exposure to conflict at ages 11-19	-0.012 (0.012)	-0.030** (0.012)	-0.021** (0.009)
Mother's exposure to conflict at ages 20+	-0.017* (0.009)	-0.016 (0.023)	-0.017 (0.014)
Observations	1,752	1,752	1,752
R-squared	0.100	0.110	0.138

Notes: The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

YL study (i.e., the 2013 and 2016 rounds when the child was aged 12 and 15, respectively).<sup>22</sup> As shown in Table 2.5, maternal conflict exposure continues to have a sustained adverse impact on socio-emotional outcomes at age 12 but not at age 15, the final measurement point.<sup>23</sup> This lack of persistence at age 15 may reflect adolescents' increased exposure to external influences, such as peers, family, and school, compared to their more limited social environment at younger ages.

In the 2013 and 2016 rounds, the YL study collected information on the socio-emotional outcomes (agency and pride) of the immediate younger siblings of the YL index children. Therefore, it is possible to re-estimate the analysis presented in Table 2.5 by including the younger siblings in the sample. Results in Table B17 of the online appendix show consistent significant negative effects on socio-emotional outcomes measured in the 2013 round.

### 2.6.4.3 Implications for risky behaviors in adolescence

The economic literature has increasingly recognized the role of socio-emotional skills, such as self-esteem and locus of control, in influencing risky behaviors and long-term socioeconomic outcomes (Heckman et al., 2006; Cunha et al., 2010; Favara and Sanchez, 2017). Building on the established link between maternal conflict exposure and socio-emotional development,

<sup>22</sup>In the 2013 and 2016 rounds, Self-efficacy and Self-esteem indexes were included. Details on the statements comprising each variable can be found in Table B4.

<sup>23</sup>The sample size in Table 2.5 differs from previous tables as it includes only children present in all three survey rounds.

**TABLE 2.5 – INTERGENERATIONAL EFFECT OF CONFLICT EXPOSURE ON CHILD’S SOCIO-EMOTIONAL OUTCOMES AT AGE 12 AND 15**

	Agency (1)	Pride (2)	Self-efficacy (3)	Self-esteem (4)	Index (5)
<i>Age 12 outcomes</i>					
Mother’s exposure to conflict	-0.017*** (0.006)	-0.017*** (0.006)	-0.005 (0.007)	-0.016*** (0.005)	-0.014*** (0.004)
<i>Age 15 outcomes</i>					
Mother’s exposure to conflict	0.012 (0.010)	0.003 (0.016)	-0.009 (0.007)	0.012 (0.010)	0.004 (0.006)
Observations	1,656	1,656	1,656	1,656	1,656

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother’s birth year, mother’s birth region, child’s birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother’s native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. The sample size differs from the baseline results as it includes only observations with non-missing information data for all outcomes across survey rounds. Standard errors clustered at the YL sentinel site (location in 2002) and presented in parenthesis. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

it is plausible that these impacts extend to other interconnected outcomes as children mature.

At age 15, the YL study began collecting data on risky behaviors, including age at first sex, alcohol and tobacco consumption, drug use, and criminal activity. Given the sensitive nature of these topics, the YL study employed a self-administered questionnaire with protocols designed to ensure confidentiality and minimize under-reporting.<sup>24</sup> Risky behaviors were defined as follows: early sex refers to sexual initiation at age 15 or earlier, drug consumption refers to ever using illegal drugs (e.g., marijuana, cocaine paste, methamphetamines); alcohol and tobacco consumption refers to having consumed alcohol to the point of intoxication or smoked in the last month; and criminal behavior is measured using a composite index capturing actions such as being arrested, being sentenced, carrying a weapon, or gang involvement.

Results summarized in Table 2.6 show that maternal exposure to conflict significantly increases the likelihood of children engaging in criminal behaviors by age 15. For the average maternal conflict exposure (1.13 years), each additional year of exposure increases the probability of engaging in criminal behavior by approximately 1.12 percentage points (or 14.6% of the sample mean).

## 2.7 Mechanisms

This paper finds a robust, significantly negative association between maternal conflict exposure and the socio-emotional development of offspring. We now examine underlying pathways discussed in the conceptual framework (Section 2.2) by estimating the following:

<sup>24</sup>Fieldworkers informed participants about the sensitive nature of the questions and assured them of their autonomy to complete the questionnaire and choose which questions to answer.

**TABLE 2.6 – INTERGENERATIONAL EFFECT OF CONFLICT EXPOSURE ON CHILD’S RISKY BEHAVIOUR AT AGE 15**

	Early sex (1)	Drugs (2)	Criminal behaviour (3)	Alcohol (4)
Mother’s exposure to conflict	-0.002 (0.009)	-0.004 (0.005)	0.011*** (0.003)	-0.004 (0.006)
Observations	1,578	1,578	1,578	1,578
R-squared	0.069	0.081	0.084	0.086
Mean dep. variable	0.258	0.085	0.085	0.107

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Fixed effects included in all regressions are: mother’s birth year, mother’s birth region, child’s birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother’s native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Risky behaviour outcomes are asked in the 2016 round. The sample size differs from the baseline results as it includes only observations with non-missing data for all outcomes. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

$$Y_{idtm} = \beta_0 + \beta_1 Exposure_{dt} + X'_{idtm}\beta + \gamma_r + \delta_t + \phi_k + \epsilon_{ijdtm} \quad (2.4)$$

where  $Y_{idtm}$  denotes the outcome for mother  $i$ , born in district  $d$ , in year  $t$  and month  $m$ .  $X_i$  controls for indigenous native tongue. All the other terms in the right-hand side of the equation have the same meaning as those presented in Equation 3.1.

We begin by examining the role of parental investment in Table 2.7 using three measures: (i) the proportion of household monthly expenditure allocated to education; (ii) an index capturing reading encouragement, including parental support for reading habits and household reading resources; and (iii) an index reflecting parental involvement, measured by parents’ knowledge of their child’s daily life.<sup>25</sup> Results in columns 1–3 of Panel A show that maternal conflict exposure significantly reduces investment in children’s education and reading skills, while no significant effects are observed for parental involvement (column 3).

Lower investments in children may stem from: (i) limited resources; (ii) a “quality-quantity” trade-off, where fewer resources are available per child; (iii) lack of parental skills or reduced maternal empowerment; and (iv) parental risk and time preferences regarding investments. First, in columns 4–5 of Panel A (Table 2.7), we examine the relationship between maternal conflict exposure and household resources. The results show that conflict exposure is significantly associated with lower monthly real household food expenditure per capita, indicating constrained resources. However, there is no evidence of a direct effect on household wealth.<sup>26</sup>

Second, results suggest a potential “quality-quantity” trade-off at play, with maternal

<sup>25</sup>Details of the questions are provided in Table B18 of the online appendix.

<sup>26</sup>The wealth index is a continuous measure ranging between 0 and 1, representing the poorest and wealthiest households, respectively. For further details, refer to Briones (2017).

exposure to conflict significantly associated with a higher probability of teenage pregnancy, lower age at first birth, and higher fertility (columns 1–3 of Panel B). This channel is further corroborated by examining effects on children’s time use. From the YL 2009 survey, we have data on the number of hours children spend on various activities in an average day. Using these data, we find that maternal conflict exposure is significantly associated with fewer hours spent by the index child studying and playing, and more hours dedicated to caring for other household members and doing household chores (columns 1–4 of Table 2.8). The “quality-quantity” trade-off can also imply a weakened parent-child relationship. The YL survey collected data on this relationship from the child’s perspective using the Marsh Self-Description Questionnaire-II (Marsh, 1990) in the 2013 survey round where a higher score indicates a more positive relationship.<sup>27</sup> Using this index as an outcome variable in column 5 of Table 2.8, we find that maternal conflict exposure significantly weakens the parent-child relationship.

Third, we examine the role of parental skills, maturity, and maternal empowerment. Given the earlier finding that conflict exposure increases the likelihood of teenage pregnancy and lowers age at first birth, parental skills and maturity may partly explain our primary effects. We proxy maternal empowerment using two variables: whether the mother is engaged in wage labor and whether she is the primary decision-maker regarding household assets and activities.<sup>28</sup> While we do not find any significant effects on mothers’ wage employment, we find that conflict exposure significantly reduces the probability of the mother being the primary decision-maker in the household (columns 4–5 of Panel B, Table 2.7).

Fourth, as outlined in the conceptual framework, maternal conflict exposure may increase risk aversion or raise discount rates, discouraging long-term investments in children’s socio-emotional development. However, we lack the data to directly assess this pathway.

Lastly, we examine the potential role of mothers’ cognitive and socio-emotional skills in explaining our results. We find no significant effects of conflict exposure on mothers’ educational attainment (likelihood of completing primary school) or socio-emotional outcomes, such as mental health, agency, and self-esteem measures (Table B20 of the online appendix).<sup>29</sup>

To summarize, our findings suggest that the negative effects of maternal conflict exposure on children’s socio-emotional skills are driven by reduced parental investments, which may result from constrained resources, quality-quantity trade-offs, and diminished maternal empowerment within the household.

<sup>27</sup>This questionnaire includes 9 questions directly administered to the child, assessing whether the child felt liked or understood by their parent.

<sup>28</sup>The latter is a dummy variable equal to 1 if the mother is responsible for any of the following: (i) making key decisions about land, (ii) making key decisions about animals, or (iii) deciding on wage work activities.

<sup>29</sup>We also explore partner-related outcomes, as they may influence household dynamics. Specifically, we assess how conflict exposure affects the likelihood of the mother’s partner getting drunk, his presence in the household, and his employment status. Results in Table B19 of the online appendix suggest that conflict exposure is negatively associated with the partner’s household presence.

**TABLE 2.7 – EFFECT OF CONFLICT EXPOSURE: PARENTAL INVESTMENT, HOUSEHOLD RESOURCES AND PREFERENCES OUTCOMES**

<i>Panel A. Parental investments</i>					
	Education expenditure (1)	Reading encouragement (2)	Parental involvement (3)	Food HH expenditure (4)	Wealth index (5)
Mother's exposure to conflict	-0.002*** (0.001)	-0.009** (0.003)	0.003 (0.003)	-0.010* (0.005)	-0.001 (0.003)
Mean dep. variable	0.112	0.683	0.782	5.206	0.430
<i>Panel B. Mother's outcomes</i>					
	Teenage pregnancy (1)	Age at first birth (2)	Number of children (3)	Waged worker (4)	Makes HH decisions (5)
Mother's exposure to conflict	0.014*** (0.004)	-0.127*** (0.042)	0.043* (0.022)	-0.006 (0.004)	-0.012** (0.004)
Mean dep. variable	0.371	21.534	2.523	0.182	0.437
Observations	1,752	1,752	1,752	1,752	1,752

*Notes:* Independent variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district. Fixed effects included in all regressions are: mother's birth year, mother's birth region and location in the 2002 round. The control variables included in the regressions was the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE 2.8 – EFFECT OF CONFLICT EXPOSURE: CHILDREN'S TIME USE (IN HOURS) AND CHILD-PARENT RELATIONSHIP**

	Play (1)	Care (2)	Chores (3)	Study (4)	Child-parent relationship (5)
Mother's exposure to conflict	-0.034* (0.019)	0.022* (0.011)	0.020*** (0.006)	-0.023* (0.012)	-0.022** (0.010)
Mean dep. variable	4.123	0.491	0.877	1.881	0.015
Observations	1,752	1,752	1,752	1,751	1,697

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). The first four columns refer to the average hours dedicated to each activity, and the last column refers to a child-parent relationship perception index. All outcomes refer to the YL child. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

## 2.8 Conclusion

In summary, this study enhances our understanding of the long-term repercussions of civil conflict, providing new evidence that intergenerational impacts extend to socio-emotional skills—a critical yet understudied dimension of human capital. By exploiting spatial and time-

varying exposure to conflict, we identify a significant relationship between maternal exposure to violence and adverse effects on children's socio-emotional outcomes, particularly agency and pride during early childhood. These effects persist into early adolescence (age 12) but diminish by age 15, likely reflecting the uneven developmental trajectories and socio-emotional changes characteristic of adolescence (Blakemore, 2008).

While some socio-emotional effects may fade over time, early-life reductions in agency and pride can have lasting downstream consequences. Notably, we find that maternal conflict exposure significantly increases the likelihood of children engaging in criminal behavior at age 15. This suggests that even modest disruptions in socio-emotional development can compound over time, shaping long-term well-being and life outcomes.

The analysis of migration patterns presents a complex picture. Migration partially alleviated some adverse effects of conflict, but accounting for migration also reveals higher exposure to violence, reflecting the evolving nature of the Peruvian conflict, which eventually affected urban centers—initially safe havens for economic migrants.

Our investigation into mechanisms highlights that maternal conflict exposure reduces parental investments in children. This is driven by a quality-quantity trade-off, constrained household resources, and diminished maternal empowerment. Conflict exposure increases maternal fertility, lowers age at first birth, and reduces mothers' ability to make household-level decisions.

This study has some limitations. The cohort design means mothers were selected because they had a child of a specific age, and geographic diversity is limited to 20 sites. Additionally, we examine impacts only up to age 15, leaving questions about whether these effects persist into adulthood, particularly for risky behaviors. Future research should explore these longer-term outcomes.

Our findings document the enduring and wide-ranging effects of the Peruvian civil conflict on individuals and their families. They underscore the need for comprehensive support systems, including targeted interventions to improve socio-emotional outcomes and prevent criminal behavior among adolescents, not just adults (Addison et al., 2016). By understanding the mechanisms driving these impacts, policymakers can design strategies to mitigate inter-generational harm and foster resilience in affected communities.

# Chapter 3

## Impact of Adolescent-Friendly Family Planning Services on Teenage Fertility in Peru

### 3.1 Introduction

Teenage pregnancy continues to be a persistent policy challenge across many low- and middle-income countries (LMICs), despite its central place in the Sustainable Development Goals (SDGs). While adolescent fertility has declined in parts of the world, it remains high in regions where access to sexual and reproductive health services is limited, stigmatised, or conditional. Early childbearing is associated with well-documented negative consequences on educational attainment, labor market participation, and upward mobility, making it both a public health and development concern (Aizer et al., 2022; Azevedo et al., 2012; Lang and Weinstein, 2015). In Latin America, adolescent fertility remains elevated despite broad improvements in education, health infrastructure, and declining total fertility rates (Azevedo et al., 2012; Sanhueza et al., 2023).

Peru illustrates this divergence particularly clearly. The total fertility rate has fallen from around 6.7 children per woman in the 1970s to just over 2.2 in recent years (World Bank, 2025a). Yet adolescent fertility in Peru remained high, around the mid-50s births per 1,000 girls aged 15–19 in the mid-2010s, and exceeds levels observed in several Latin American comparators and higher-income benchmarks (e.g., 49.1 vs. 23.7 in 2018 for Peru versus Chile) (World Bank, 2025b). This gap is also large relative to the OECD average (8.7 per 1,000) (OECD, 2023). Regional and urban–rural disparities are also marked: nearly 10 percent of Peruvian girls aged 15–19 have begun childbearing, with much higher rates in rural and Amazonian regions such as Loreto and Ucayali and markedly lower rates in Lima and other urban areas (Instituto Nacional de Estadística e Informática (INEI), 2022). These patterns suggest that aggregate progress in contraceptive coverage has not fully addressed the specific

institutional and social barriers faced by adolescents.

This paper evaluates a major reform to Peru's public family planning system that explicitly targeted such barriers. Studying this reform provides new evidence on a policy that specifically focused on adolescents and aimed to reduce barriers related to provider discretion, trust in confidential care, and adolescent autonomy. In 2017, the Ministry of Health implemented the Family Planning Technical Guideline (hereafter, FPTG), which standardised a rights-based approach to service delivery for adolescents and introduced protocols for adolescent-friendly family planning services for individuals under 18. In particular, the guideline strengthened protections around confidentiality, privacy, and voluntary decision-making, and promoted differentiated delivery conditions for adolescents (e.g., dedicated spaces and service hours beyond school time). A subset of existing public health centres meeting minimum infrastructure and staffing requirements was designated for upgrade to deliver adolescent-friendly services (hereafter, HCA), rather than expanding supply through new facilities. This feature generates quasi-experimental variation in access across districts and communities, against a backdrop of historically contested and uneven family planning provision in Peru ([Aramburú, 2014](#); [Defensoría del Pueblo, 2005](#)).

I combine administrative and survey data with a difference-in-differences design to estimate the impact of this reform on adolescent fertility and related life transitions. The main analysis uses the universe of birth records (Certificado de Nacido Vivo, CNV) from 2013 to 2019 to construct age-specific fertility rates by district, age group, and year for girls aged 13–17, which I link to information on whether the district contains at least one HCA centre. I estimate a two-way fixed effects model and an event-study specification around the 2017 reform to trace dynamic effects. I complement this with individual-level evidence from the Peruvian Demographic and Health Surveys (DHS) for 2013–2019, exploiting georeferenced cluster locations to compare adolescents living within a short distance of an HCA centre to those whose nearest public centre did not receive the adolescent-friendly upgrade. Together, these data allow me to document changes in fertility, early union formation, schooling, contraceptive use, and reproductive health knowledge.

The results show that increasing access to adolescent-friendly services substantially reduced teenage fertility. In the population-level CNV birth records, exposure to an HCA centre is associated with a persistent decline in fertility among girls aged 13–17, amounting to roughly a 15% reduction relative to comparable districts without an HCA centre. The estimated effects are larger for older adolescents and in areas located closer to upgraded centres. Evidence from the individual-level DHS is consistent with this pattern: the reform reduces the probability that a girl has given birth before age 18 by around 1.5–2 percentage points (approximately 13–17% of the baseline), with stronger effects in urban areas.

The benefits of the reform, however, were not evenly distributed: fertility reductions were concentrated among adolescents from relatively more advantaged backgrounds, those living in wealthier, male-headed, and larger households, underscoring the persistence of inequalities

in programme uptake despite the policy's universal design. Event-study estimates indicate that the impact strengthens in the years following implementation; however, attenuates in 2020, coinciding with Peru's COVID-19 lockdown and the temporary suspension of non-urgent reproductive health services, temporarily nullifying the gains. Notably, fertility declines resume once services are restored, reinforcing the importance of continuity in adolescent reproductive health services. This COVID-period disruption provides an unexpected stress test, highlighting that the reform's gains are sensitive to interruptions in service availability.

The reform also shifted other key life outcomes. Adolescents in treated areas are less likely to enter early cohabiting unions and are less likely to interrupt or leave school because of pregnancy, childcare responsibilities, or marriage/cohabitation, consistent with greater control over the timing of family formation and reduced role conflicts between schooling and family duties. Across outcomes, the estimates indicate effects not only on adolescent fertility, but also on early union formation and schooling, consistent with improvements in adolescents' ability to access services under conditions of confidentiality, discretion, and trust. Analysis of mechanisms suggests that these changes are driven by shifts in contraceptive practices and information sources: adolescents in treated areas move away from traditional methods toward more autonomous methods such as condoms, and they become less reliant on family members and peers for family planning information, consistent with increased engagement with confidential, institution-based counselling.

This study contributes to several strands of the literature on contraceptive access, teenage fertility behavior, family planning policies, and service design and delivery. First, it adds to a robust body of work studying how access to modern contraception shapes fertility decisions and broader life-cycle outcomes. Vast research in high-income settings has shown that contraceptive access can dramatically alter women's educational, marital, and labor market trajectories<sup>1</sup> (Bailey, 2010; Bailey and Lindo, 2017; Goldin and Katz, 2002; Guldi, 2008; Lindo et al., 2020; Ragan, 2025). In their seminal paper, Goldin and Katz (2002) provided foundational evidence that access to oral contraception delayed marriage and childbearing among young women in the 1970s, potentially enhancing higher educational attainment and increasing labor market participation<sup>2</sup>.

In contrast to high-income settings, evidence from developing countries, though extensive, has focused more on fertility outcomes, and shows mixed results. For instance, Dupas et al. (2024) find that free provision of medical contraception in Burkina Faso was insufficient to alter reproductive behavior, even when accompanied by interventions aimed at addressing potential

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<sup>1</sup>Notably, prior research on these dynamics has largely centered on the birth control pill, given its importance as the most transformative breakthrough in contraceptive technology since the 1960s (Doepke et al., 2023).

<sup>2</sup>Extensive complementary research examining socioeconomic outcomes finds that access to contraception and family planning programs significantly increases years of education and the likelihood of high school graduation, while also contributing to a reduction in the gender gap and the likelihood of intergenerational poverty risk (Ananat and Hungerman, 2012; Bailey et al., 2012, 2019; Lindo et al., 2020; Myers, 2017; Stevenson et al., 2021)

misperceptions. This finding aligns with [Ashraf et al. \(2013\)](#) who report similarly limited effects in Zambia following an intervention that provided vouchers for free and immediate access to long-acting contraceptive methods. On the other hand, [Miller \(2010\)](#) shows that the expansion of family planning services in Colombia significantly reduced fertility and improved women's educational attainment. Similarly, [Pörtner et al. \(2011\)](#) find that access to family planning in Ethiopia significantly reduced fertility in women without education, but had no effect on women with some schooling, suggesting that family planning and formal education may act as substitutes in this setting.

Second, this paper advances the literature on fertility policy by focusing on adolescents, an empirically understudied population in causal work on reproductive health in LMICs, who face distinct institutional and social constraints, including concerns around confidentiality and third-party involvement, provider discretion, and stigma around sexuality<sup>3</sup> ([Colman et al., 2013](#); [Guldi, 2008](#); [Myers and Ladd, 2020](#); [Sabia and Anderson, 2016](#)). For instance, in the United States,<sup>4</sup> several studies demonstrate that expanding access to contraception and abortion services can significantly reduce teenage fertility and improve educational trajectories ([Fischer et al., 2018](#); [Kelly et al., 2020](#); [Lindo and Packham, 2017](#); [Stevenson et al., 2021](#)). Yet, most of this evidence comes from high-income settings, where legal and health system contexts differ markedly from those in middle-income countries. Although limited, prior causal studies in LMICs find that integrated family planning programs, which combine contraceptive access ([Ferre et al., 2023](#); [Galárraga and Harris, 2021](#); [Kaytan et al., 2025](#)), along with counseling and information campaigns ([Branson and Byker, 2018](#); [Kaytan et al., 2025](#); [Strupat, 2017](#)), can have a significant impact on addressing teenage pregnancies and early childbearing. This paper advances this literature by studying the effects of a nationwide policy reform that specifically targeted adolescent access and discretion, in a setting where adolescents' access to contraceptive services had previously been more contingent on discretionary gatekeeping and concerns about confidentiality.

Third, this study speaks to broader debates on how the source and framing of reproductive health information influence behavior. While supply-side policies may increase availability, their effectiveness will often depend on service delivery and perceived accessibility, and the extent to which demand is constrained by knowledge gaps, perceived barriers, or financial costs ([Bailey et al., 2023](#); [Branson and Byker, 2018](#); [Grossman et al., 2025](#); [Kearney and Levine, 2015](#)). Strong evidence from a range of settings particularly suggests that autonomy, discretion, and provider interaction can shape both take-up and outcomes ([Ashraf et al., 2014, 2013](#); [Grossman et al., 2025](#); [Jones and Pineda-Torres, 2024](#); [Myers, 2022](#)). By documenting shifts

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<sup>3</sup>From a policy perspective, these constraints matter not only for immediate access to services, but also due to the strong association between early childbearing and lower educational attainment and socio-economic mobility ([Bailey, 2025](#); [Heath and Jayachandran, 2016](#); [Herrera Almanza and Sahn, 2018](#); [Todd and Wolpin, 2006](#)).

<sup>4</sup>By the 1970s, the United States had a notably higher teen pregnancy rate compared to other developed nation ([Kearney and Levine, 2012](#)).

not only in fertility but also in information sources, knowledge, and downstream outcomes like marriage and schooling, this paper highlights the importance of service design and user autonomy as key mechanisms in shaping adolescent reproductive behavior. Specifically, this paper contributes to the existing body of work by providing new causal evidence from a middle-income country context, showing that alleviating supply-side barriers may not be sufficient in the presence of societal and institutional frictions.

The remainder of the paper is structured as follows: Section 3.2 provides background on teenage fertility in Peru and the 2017 Family Planning Technical Guideline. Section 3.3 describes the administrative and survey data used in the analysis. Section 3.4 outlines the empirical strategy and identification approach. Section 3.5 presents the main results on adolescent fertility, including heterogeneity analyses, complementary outcomes, and COVID-19 dynamics. Section 3.6 reports robustness checks. Section 3.7 examines underlying mechanisms using individual-level data, focusing on changes in contraceptive use, sources of reproductive health information, and knowledge of contraceptive methods. Section 3.8 concludes.

## 3.2 Background

### 3.2.1 Adolescent Fertility in Peru

Despite decades of shifting political and ideological approaches to family planning in Peru, adolescent fertility has remained a persistent policy challenge.<sup>5</sup> Although Peru's total fertility rate declined from 6.7 in 1970 to just over 2.2 in recent years ([World Bank, 2025b](#)), nearly 13% of girls aged 15 to 19 have begun childbearing, with substantial disparities across regions, income levels, and education ([Instituto Nacional de Estadística e Informática \(INEI\), 2022](#)). In rural areas, adolescent motherhood reaches 22%, compared to 10% in urban zones, and exceeds 20% in Amazonian regions such as Loreto and Ucayali. These figures contrast with Lima, where the rate falls below 8%. Peru's adolescent fertility rate is more than twice the OECD average and remains significantly higher than other upper-middle-income countries ([OECD, 2023](#)). This persistence suggests that national progress in contraceptive coverage has not fully addressed the distinct institutional and social barriers faced by adolescents.

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<sup>5</sup>Initial efforts in the 1960s–early 1980s were weak and largely symbolic, but the first national population policy in 1985 broadened access to publicly funded contraceptives, heavily supported by international cooperation. Under President Fujimori (1996–2000), the program expanded aggressively, prioritising sterilisation and reaching nearly 200,000 procedures, but allegations of coercion and reported deaths triggered a major scandal that discredited public family planning and fuelled conservative opposition ([Defensoría del Pueblo, 2005](#); [Valdivia, 2005](#)). After 2000, family planning was downgraded and integrated into maternal–child health, with growing emphasis on “natural” methods, often promoted by religious actors, reducing access particularly for rural and less-educated women. Even after regime changes, family planning budgets and political support remained limited, while Peru's upper-middle-income status reduced external aid. By 2013, nearly 40% of contraceptive users relied on private providers ([Aramburú, 2014](#)), reflecting persistent gaps in public provision. These historical legacies help explain why, despite increased contraceptive coverage, adolescents continued to face institutional gatekeeping, provider discretion, and stigma in accessing modern methods.

The institutional constraints that shape access to contraception in Peru, and the 2017 reform studied in this chapter, are defined around the legal threshold of adulthood. The FPTG explicitly introduced adolescent-friendly protocols and removed age-based restrictions for individuals under the legal age, 18. Accordingly, the empirical analysis focuses on fertility among minors, where confidentiality, provider discretion, and stigma are most likely to bind, and where the reform was intended to operate. In the administrative CNV data, this corresponds to restricting the main fertility outcomes to births to mothers aged 13–17.

Legal and economic constraints further reinforce these gaps. While comprehensive sex education was introduced in 2012, and emergency contraception was legalized for public distribution in 2019, abortion remains illegal in nearly all circumstances.<sup>6</sup> This restrictive environment is compounded by inconsistencies in legal norms governing adolescent sexual and reproductive rights. The age of sexual consent in Peru is 14, and until a legislative reform in 2023, adolescents could legally marry below the age of 18 with judicial authorization.<sup>7</sup> Although the law thus granted adolescents a degree of sexual autonomy, institutional barriers often limited their effective access to reproductive services.

Out-of-pocket contraceptive costs further limit access. When not publicly subsidized, a monthly pack of oral contraceptives can cost around USD 10 (PROMSEX, 2021), exceeding the daily minimum wage. In contrast, IUDs may cost between USD 160 and 320, making them largely inaccessible for low-income and adolescent users. These affordability issues highlight the urgent need for reforms that expand both access and autonomy in reproductive healthcare.

### 3.2.2 Family Planning Technical Guideline (FPTG)

In 2016, the Peruvian Ministry of Health approved the FPTG, officially enacted through Ministerial Resolution No. 652-2016/MINSA and updated in 2017.<sup>8</sup> The guideline, released and implemented in 2017, established standardized protocols for the delivery of contraceptive services across all public health centres.<sup>9</sup> The FPTG aimed to improve the quality and safety of family planning services by unifying procedures across different levels of care. It emphasized a rights-based approach to reproductive health, with a focus on gender equity, cultural inclusion,

<sup>6</sup>According to the Center for Reproductive Rights, in Peru, therapeutic abortion is permitted under Article 119 of the Penal Code; it is not punishable when performed by a physician to save the life of the pregnant woman or to prevent serious harm to her physical or mental health. Nonetheless, access remains highly restricted in practice due to regulatory barriers, the lack of standardized protocols, and providers' use of conscientious objection. In 2025, the Ministry of Health revised the Technical Guidelines for therapeutic abortion, explicitly removing considerations related to mental health risk, with experts have describing it as a setback, particularly in cases of adolescent pregnancy resulting from sexual violence.

<sup>7</sup>The Peruvian Civil Code previously permitted marriage from age 16 for boys and 14 for girls with judicial consent. Law No. 31839, enacted in 2023, eliminated these exceptions and established 18 as the minimum legal age for marriage.

<sup>8</sup>While compulsory for public health institutions, the FPTG served as a reference for the private sector.

<sup>9</sup>The FPTG functions as a legally binding technical guideline, comparable to implementation directives in the U.S. Department of Health and Human Services or the UK's NHS. It regulates how national reproductive health policies are operationalized.

and non-discrimination.<sup>10</sup>

A key innovation of the 2017 reform was to formalise a rights-based approach to adolescent access within public family planning services by explicitly codifying safeguards for confidentiality, privacy, and voluntary decision-making. In doing so, it reduced a long-standing source of institutional ambiguity in Peru's legal environment: although the age of sexual consent is 14, adolescents' ability to seek reproductive care in practice has often been constrained by uncertainty and discretionary gatekeeping. The guideline therefore emphasises protocols that protect adolescents' autonomy during consultations, limit disclosure of health information to third parties, and prohibit pressure or coercion in contraceptive choices.<sup>11</sup>

Implementation was selective. The guideline defines adolescent "differentiated services" as continuous, quality comprehensive sexual and reproductive health and family planning care, delivered in *differentiated hours* and *exclusive environments* for adolescents, and aligned with national standards for adolescent services. It also notes that adolescent attention may be provided through youth development centres (CDJ), combining counselling, training workshops, socio-cultural activities, and clinical care. In practice, this service model requires minimum infrastructure and staffing capacity, so only a subset of public health centres were authorised to deliver adolescent-friendly family planning services (HCA).

While the reform did not introduce a national outreach strategy exclusively targeted to adolescents, it set out general dissemination guidelines for sexual and reproductive health and family planning. Communication efforts were expected to combine mass media (press, radio, television) with alternative media and local campaigns, including posters and printed materials, public murals and advertisements in public transport, and community-based channels such as meetings with local authorities and loudspeaker announcements (*perifoneo*). When adolescents were the intended audience, coordination with the Ministry's Adolescent Life Stage Unit (*Etapa de Vida Adolescente*) was explicitly encouraged.<sup>12</sup>

Throughout the paper, I define the start of the reform period as January 2017, when the Family Planning Technical Norm became operational and adolescent-focused services began to be offered in public health centres. All conceptions (and survey observations) from 2017 onwards are therefore treated as post-reform.

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<sup>10</sup>Specifically, the law stated: (i) that any consultation or mention of your case is carried out discreetly and that there are no persons present who are not directly involved in your care without your consent; (ii) no information should be given to third parties or family members about the reasons for care, diagnoses or treatment, unless authorized by the user, except as provided by law; (iii) to make decisions related to his/her health care; (iv) to be free from any pressure or coercion to choose a contraceptive method. More detail on the materials and service implementation can be found at [Figure C4](#), [C5](#), [C6](#).

<sup>11</sup>For instance, the guideline requires privacy during counselling and examinations, establishes confidentiality of communications and clinical records, restricts disclosure of information without the user's authorization, and explicitly prohibits coercion or inducements in method choice.

<sup>12</sup>MINSA (2017), diffusion and communication guidelines; implementation is coordinated across national and regional health authorities, and adolescent-oriented campaigns are coordinated with the *Etapa de Vida Adolescente*.

## 3.3 Data

### 3.3.1 Data sources

To examine the effects of FPTG on outcomes for women, I use administrative birth records covering the universe of live births (CNV), population estimates from the Peruvian Ministry of Health, and microdata sourced from the Peruvian Demographic and Health Survey (DHS). The main pre-pandemic analysis uses data from 2013–2019 (CNV and DHS). For the event-study analysis of COVID-19 disruptions, I extend the CNV sample to 2013–2023.

#### 3.3.1.1 Birth records data

I use an administrative dataset from the Peruvian Ministry of Health (MINSA), based on the *Certificado de Nacido Vivo (CNV)*.<sup>13</sup> The system was launched in March 2012 and operates nationwide, incorporating data from all healthcare providers, both public and private. It records every live birth at the moment of delivery. It collects detailed information on the mother (such as age and educational level), the newborn (including sex and birthweight), and the pregnancy (such as gestational age).

This information allows me to construct the universe of registered births in Peru, disaggregated by year, mother's age, and district of residence, covering the period 2013 to 2019.<sup>14</sup> I restrict the sample to births to mothers aged 13 to 17, in line with the specific target population of the FPTG. Using census-based population estimates for these age groups, I construct a district-by-age-group-by-year panel of age-specific fertility rates, which serves as the main outcome variable in the first part of the analysis.

Because the FPTG reform targets access and service protocols for minors, I align the main administrative outcome with this policy-relevant cutoff and construct fertility measures for girls below age 18.

#### 3.3.1.2 Individual data

I also use data from the Peruvian Demographic and Health Surveys (DHS, or ENDES in its local designation), a nationally representative survey conducted by the National Institute of Statistics and Informatics (INEI). The DHS has been fielded continuously since 2004, with quarterly data collection that supports the construction of annual cross-sectional estimates. For this study, I use waves from 2013–2019. The DHS adopts a repeated cross-sectional design to ensure national and regional representativeness. Using stratified, multi-stage cluster

<sup>13</sup>Data can be accessed upon request from the Ministry of Health. Detailed information available at: <https://www.minsa.gob.pe/cnv/>

<sup>14</sup>The CNV records all centres-based births; although home or unregistered births may be more common in remote areas, there is no evidence that their incidence changed differentially across districts with and without an HCA during the study period, so any resulting undercoverage is likely time-invariant and absorbed by district fixed effects.

sampling, the survey selects clusters of households within strata defined by geography and urban/rural status, and then randomly samples households within each selected cluster.

The survey collects detailed demographic and health information for women aged 15 to 49, including fertility histories, marital and employment status, household decision-making, and socio-economic characteristics. In this study, I focus on adolescent childbearing and related life transitions. Because the DHS interviews women aged 15–49, adolescents aged 13–14 are not consistently observed in the repeated cross-sections. I therefore restrict the DHS analysis sample to girls aged 15–17 at the time of interview, so that all respondents are minors (i.e., under 18) in the pre-reform and/or implementation period. To maintain a policy-relevant estimand tied to the reform’s legal cutoff and to ensure comparability with the administrative measures, I define DHS outcomes using pre-18 thresholds (e.g., having given birth before age 18, and entering marriage/cohabitation before age 18).

While the CNV covers the universe of registered births, the DHS provides rich information on behaviours, knowledge, and schooling for a representative sample of adolescents. Although the two sources cannot be linked at the individual level, they offer complementary evidence on the reform’s fertility and behavioural impacts.

The inclusion of GPS coordinates for DHS household clusters enables me to compute each cluster’s proximity to the nearest health centre.<sup>15</sup> However, because the survey does not report the name of each cluster (*centro poblado*), district is the smallest administrative unit at which I can condition the analysis (e.g., through district fixed effects).

### 3.3.1.3 Health centres location data

I complement this information with the GPS locations of all public health centres providing differentiated services to adolescents (*HCA centres*). This information is publicly available upon request from the Ministry of Health in Peru (*MINSA*). As illustrated in [Figure C1](#), HCA centres are present in the 25 regions of Peru but not in all districts, only in 72.63% of the universe of Peruvian provinces.<sup>16</sup>

## 3.3.2 Measuring exposure to HCA

To capture exposure to HCA centres, I employ two complementary measures, tailored to the structure of each dataset. First, in the administrative birth records, geographic precision is limited to the district level. I therefore define exposure based on whether at least one health centre in the district was accredited as an HCA under the reform period. The HCA list was determined prior to the 2017 reform and remained unchanged over my study period, so this indicator is time-invariant at the district level. Treatment variation then comes from

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<sup>15</sup>In the Peruvian DHS, clusters contain on average 25 households, ranging from 1 to 50.

<sup>16</sup>Region corresponds to the first administrative level of the country, province to the second, and district to the third administrative level.

interacting this fixed district characteristic with the common post-reform period.

Second, using the DHS, which provides georeferenced coordinates for survey clusters, I construct a more granular measure of exposure based on proximity to HCA-accredited centres. Specifically, I calculate the Euclidean distance between each DHS household cluster and the nearest HCA centre from the pre-reform list. A household cluster is considered exposed if an HCA centre falls within a given distance radius of its location, irrespective of administrative boundaries. Because HCA status is fixed over time, this exposure measure is time-invariant at the cluster level and is only activated in the post-2017 period of the analysis.<sup>17</sup> The main results section examines how varying the buffer size affects the estimates and demonstrates that the findings are robust across alternative thresholds.<sup>18</sup> This individual-level exposure definition is applied consistently in both difference-in-differences and event study models, allowing for greater precision while maintaining alignment with the district-level analysis. [Figure 3.1](#) illustrates this spatial approach by mapping the Euclidean buffers for four selected regions in Peru: Arequipa, Iquitos, Lima, and Trujillo.

One potential concern related to the DHS GPS locations is the precision of this information, since all GPS points provided in the DHS dataset are displaced before public disclosure of the datasets to preserve individuals' confidentiality. The GPS displacement is done randomly, with differences between urban and rural clusters. Urban clusters are uniformly displaced up to 2 kilometres, and rural clusters are displaced up to 5 kilometres. Additionally, all the displacement points are restricted, so they stay within the province level (second administrative unit). To address any concerns related to the displacement of GPS locations, I established a reasonable buffer distance to the HC, rather than the closest distance to the HC centre.<sup>19</sup> In that sense, I provide evidence that the main results are not sensitive to different groups of Euclidean buffer distances. Additionally, since the displacement is less pronounced in urban locations than in rural areas, I present my results using both the full sample and the urban cluster sample.

### 3.3.3 Descriptive statistics

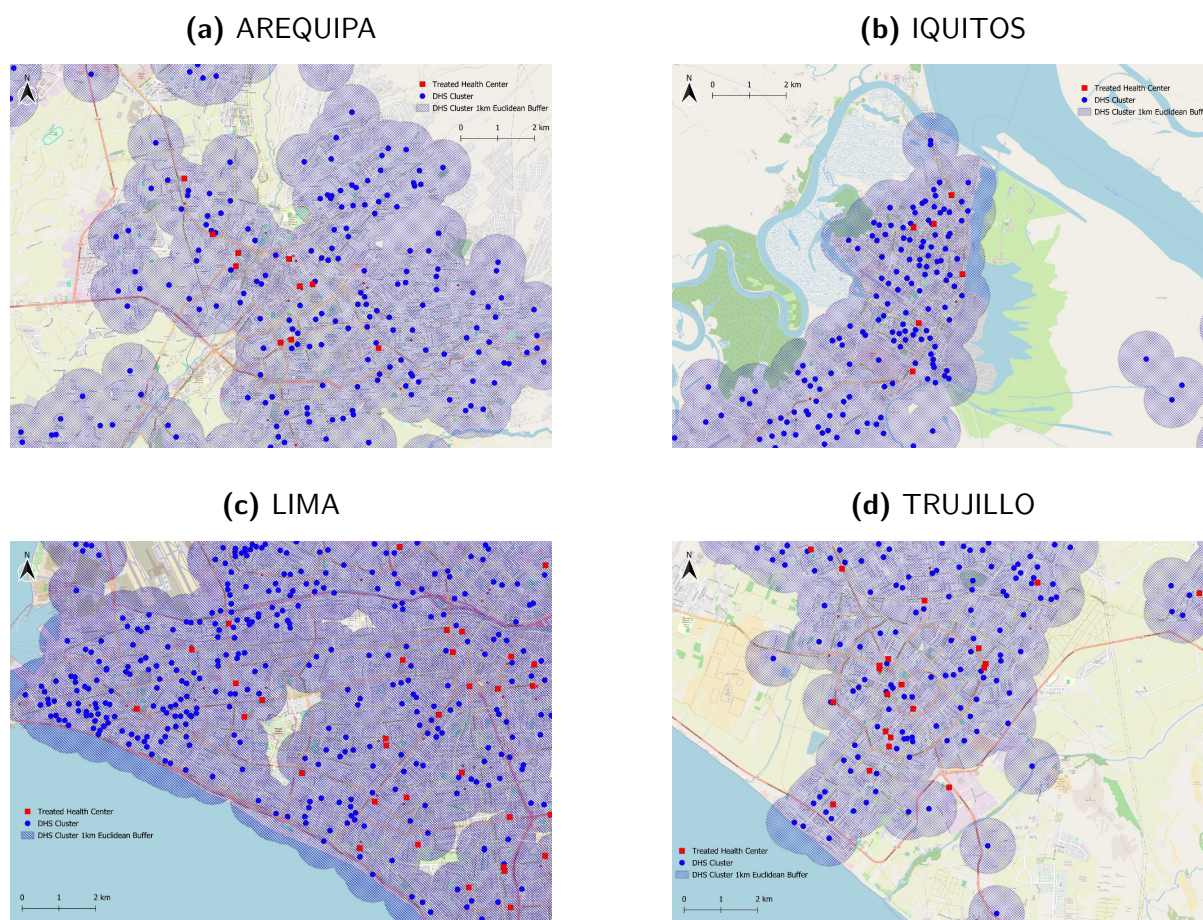
I restrict the sample to women under 18 years old; this cut-off corresponds to how the FPTG reform targets adolescents under age of 18 in Peru. In the case of the birth records dataset, the analysis sample comprises a balanced panel of 1872 districts (out of 1890 districts in the country) spanning the 2013-2019 conception period. [Figure C2](#) plots the distribution

<sup>17</sup>Importantly, no additional centres were accredited as HCA during 2013–2019, so the reform did not generate staggered adoption across districts.

<sup>18</sup>See Section 3.5 for robustness checks using different distance buffers (e.g., 2–5 km). Results remain stable across specifications.

<sup>19</sup>Because DHS cluster coordinates are randomly displaced within provinces and treatment is defined using relatively broad distance buffers (2–5 km), any misclassification arising from GPS displacement is likely to attenuate estimated effects toward zero rather than generate spurious impacts.

**FIGURE 3.1 – EUCLIDEAN DISTANCE BUFFERS AND HCA (DHS CLUSTERS OF HOUSEHOLDS)**



Notes: This graph illustrates the location coordinates of the household DHS clusters, using a 1km buffer for illustrative purposes, and the HCA centres in 4 different locations of Peru.

of teenage fertility rates across districts with and without an HCA.<sup>20</sup> It is essential to mention that the treated districts are considerably smaller than the comparison districts. Only 27% of provinces have an HCA.

In the case of the Peruvian DHS, I restrict the individual dataset to women aged 15, 16 or 17 during the reform period or survey year and who are not sterilised.<sup>21</sup> Table C1 provides information about the sample distribution for the DHS 2013-2019 period. Table C2 presents descriptive statistics for the pre-reform period (2013–2016) at the individual and household level for the full sample and by treatment status, where treatment is defined using the baseline measure of living within a 4-kilometre Euclidean buffer of an HCA centre (results are robust to alternative buffer sizes).

34% of the sample falls within a 4km distance to their closest health centre. Moreover,

<sup>20</sup>Calculated as the number of births divided by the estimated population per 1000 people in a given district, year and age group (15, 16 and 17).

<sup>21</sup>The Peruvian DHS only includes women under the age of 15 if they are already mothers in the households selected for interviews. However, women under 15 are not included in the sample design.

it is observed that there are some slight differences between treatment and control at the household characteristics measuring proxy for wealth and assets accumulation. Regarding the characteristics of women, there are not many differences between the two groups, except for the variable of urban residence. Due to the location of the existing HCA, the treatment group primarily resides in urban areas.

## 3.4 Methodology

To estimate the effect of increasing access to adolescent-friendly contraceptive (HCA) services, I use a difference-in-differences framework estimated with a two-way fixed effects (TWFE) specification (Roth et al., 2023). The empirical strategy exploits the sharp timing of the 2017 reform, the age cut-off that defines adolescents, and the fact that HCA services were introduced in a fixed set of pre-existing public health centres (Figure C1). I first present the specification using district-level data and then the specification using individual-level data.

### 3.4.1 Identification and event-study design

The centres that implemented the adolescent-friendly family planning protocol were not randomly selected. Prior to the reform, HCA centres tended to be larger centres with more infrastructure and personnel than other public health centres, reflecting the requirement to allocate dedicated space and staff for adolescent services. These level differences motivate the inclusion of rich fixed effects and controls in the empirical analysis.

The identifying assumption is that, conditional on these time-invariant differences and common shocks, adolescent outcomes in areas served by HCA centres and other centres would have followed parallel trends in the absence of the reform. Consistent with this assumption, the event-study estimates presented below show no systematic differences in pre-2017 trends between areas served by adolescent-friendly centres and areas served by other centres.

### 3.4.2 District-level specification

I estimate a two-way fixed effects (TWFE) model that compares the fertility rate of 15–17-year-olds before and after the introduction of the family planning reform across districts with and without an HCA facility. The list of HCA centres is fixed over my study period: all treated centres were already constructed and accredited before the 2017 reform, and no new centres gained HCA status during 2013–2019. Thus, the design combines a single, common policy date (2017) with pre-existing cross-sectional variation in the presence of an HCA across districts. The key identifying assumption is that, in the absence of the reform, the change in the average teenage birth rate observed in comparison districts would have provided a good counterfactual for the change in treated districts.

In this approach, I construct a panel at the district level and define the outcome of interest and the relevant age group accordingly. The equation can be formalised as:

$$Y_{dty} = \beta_0 + \beta_1(Treated_d \times Post_t) + \theta_{r(d)t} + \delta_d + \epsilon_{dty}, \quad (3.1)$$

where  $Y_{dty}$  is the fertility rate for age group  $y$  in district  $d$  and conception year  $t$ .<sup>22</sup>  $Post_t$  indicates whether the conception year is after the reform became operational, that is, for conceptions occurring in 2017, 2018, and 2019.  $Treated_d$  indicates whether there is at least one HCA facility in district  $d$ . The coefficient  $\beta_1$  captures the change in adolescent fertility in treated districts after the reform, relative to the change in comparison districts, under the assumption of parallel trends. The terms  $\theta_{r(d)t}$  and  $\delta_d$  are region-by-year fixed effects and district fixed effects, respectively.<sup>23</sup> Finally,  $\epsilon_{dty}$  is an error term. In this specification, I cluster the standard errors at the district level, i.e., the level of treatment (Abadie et al., 2023).<sup>24</sup>

To examine how the impact of the 2017 family planning reform evolved in the years before and after its implementation, I estimate an event-study specification of the following form (Miller, 2023):

$$Y_{dt} = \sum_{j \in \{-4, \dots, -1, 1, \dots, 4\}} \beta_j (Treated_d \times \mathbf{1}\{t - 2017 = j\}) + \alpha_d + \delta_t + \varepsilon_{dt}, \quad (3.2)$$

Here,  $Y_{dt}$  denotes the outcome of interest (e.g., the adolescent fertility rate for 15–17-year-olds) in district  $d$  and year  $t$ . The indicator  $Treated_d$  equals one for districts that contain at least one HCA facility in the pre-reform accreditation list, and zero otherwise. The term  $\mathbf{1}\{t - 2017 = j\}$  is an indicator for calendar years relative to the first post-reform year (2017), so that  $j < 0$  indexes leads and  $j > 0$  indexes lags. The coefficients  $\beta_j$  measure how the difference in outcomes between treated and comparison districts evolves  $j$  years before and after the reform, relative to the year just before implementation ( $j = -1$ ), which is omitted from the summation. As before,  $\alpha_d$  and  $\delta_t$  denote district and year fixed effects, respectively, and  $\varepsilon_{dt}$  is an error term.

Estimates for  $j < 0$  capture pre-reform trends and allow for tests of the parallel trends assumption, while those for  $j > 0$  quantify the policy's effects in the years following implementation. As shown in later sections, there is no statistical evidence that pre-treatment trends differ between treated and comparison districts, supporting the parallel trends assumption.

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<sup>22</sup>The main outcome is an age-specific fertility rate at the district-by-age-group-by-year level. I first use the information in the birth records to assign each live birth to an approximate month and year of conception, and then aggregate all conceptions that occur between January and December of calendar year  $t$  for each district–age cell. The fertility rate for district  $d$ , age group  $y$  and conception year  $t$  is defined as the number of births conceived in year  $t$  to women aged  $y$  residing in district  $d$ , divided by the corresponding female population in that district–age group and multiplied by 1,000.

<sup>23</sup>In Peru, regions (or departments) correspond to the first administrative level below the national government. Districts are grouped into provinces, and provinces are grouped into regions/departments. There are 25 regions in total.

<sup>24</sup>Standard errors are clustered at the district level in all district-level regressions.

### 3.4.3 Individual-level specification

To complement the district-level estimates in section 3.4.2 based on population birth records, I additionally exploit individual-level DHS microdata to estimate the reform's effects on adolescent childbearing and related behaviours. The DHS adds value for two reasons. First, it allows me to construct a more precise measure of exposure based on residential proximity to upgraded HCA centres (using DHS cluster coordinates), rather than relying solely on an indicator for whether an HCA centre operates within the district. Second, the DHS contains individual outcomes and mediating behaviours (e.g., sexual activity, contraceptive knowledge and use) that are not observable in administrative birth records, providing complementary evidence on mechanisms.

Using repeated cross-sectional DHS surveys from 2013 to 2019, I implement a standard difference-in-differences (DiD) design to estimate the effects of the reform on adolescent (under-18) fertility outcomes and related behaviours, including sexual activity, contraceptive practices, and reproductive health knowledge. This approach relies on the accessibility and distribution of services within regions, which can vary depending on proximity to households, particularly in smaller districts. In order to estimate the impact of FPTG reform on adolescents' fertility decisions, the following specification is employed:

$$Y_{idt} = \beta_0 + \beta_1(Treated_{id}^{(R)} \times Post_t) + \beta_2 X_{it} + \gamma_d + \delta_t + \epsilon_{idt}, \quad (3.3)$$

where  $Y_{idt}$  represents the outcome of interest for woman  $i$ , residing in district  $d$  in year  $t$ . treatment is defined based on residential proximity to HCA services. Let  $Treated_{id}^{(R)}$  be an indicator equal to one if woman  $i$ 's DHS cluster lies within  $R$  kilometres of the nearest HCA facility, and zero otherwise.<sup>25</sup>  $Post_t$  takes the value 1 if the survey year is 2017, 2018, or 2019. The vector  $X_{it}$  includes individual covariates: age (and age squared), an indicator for mother tongue different from Spanish, and the DHS wealth index.  $\gamma_d$  and  $\delta_t$  are district-of-residence fixed effects and survey-year fixed effects, respectively. Finally,  $\epsilon_{idt}$  is the error term. I cluster the standard errors at the district level.

Because the DHS is a repeated cross-section, the set of observed ages varies by survey year and the number of very young adolescents is limited. I therefore align the DHS analysis with the population of interest in this chapter—minors—by restricting the estimation sample to girls who are at most 17 years old at interview.<sup>26</sup> To maintain comparability across survey years and avoid differential exposure windows by interview age, I focus on outcomes defined strictly before age 18 (e.g., having had a birth before age 18), so that the outcome period remains within the policy-relevant age range for all respondents.

<sup>25</sup>In robustness checks, I consider alternative radius  $R \in \{1, 2, 3, 4, 5, \dots\}$  to assess how the estimated effects vary with the size of the buffer area.

<sup>26</sup>The DHS's questionnaire sample design only includes women aged 15 and older; consequently, the DHS cannot capture consistently outcomes for ages 13–14, which are observed in the CNV population birth records.

## 3.5 Main Results

This section presents the primary findings on the effects of the 2017 FPTG reform on adolescent fertility in Peru. I begin by assessing pre-trends and reporting estimates from district-level specifications using administrative birth records. I then turn to individual-level results from the DHS survey and explore heterogeneity by subgroups and age cohorts. I complement the main results with additional outcomes related to adolescent cohabitation and schooling. Finally, I conclude with the results, including the COVID-19 disruption.

### 3.5.1 Effects on teenage fertility rates

To verify the plausibility of the identifying assumptions, I start by examining whether adolescent fertility trends differed between treated and untreated areas before the reform. Using administrative data, I estimate event study models that flexibly allow for dynamic effects before and after the policy rollout, as shown in [Equation 3.2](#). [Figure 3.2](#) shows no statistically significant differences in fertility trends between treatment and comparison groups prior to 2017, consistent with the parallel trends assumption. The event-study estimates show that treated and comparison districts follow parallel trends prior to 2017 and that adolescent fertility declines sharply in treated districts immediately after the reform, with effects persisting over subsequent post-reform years.

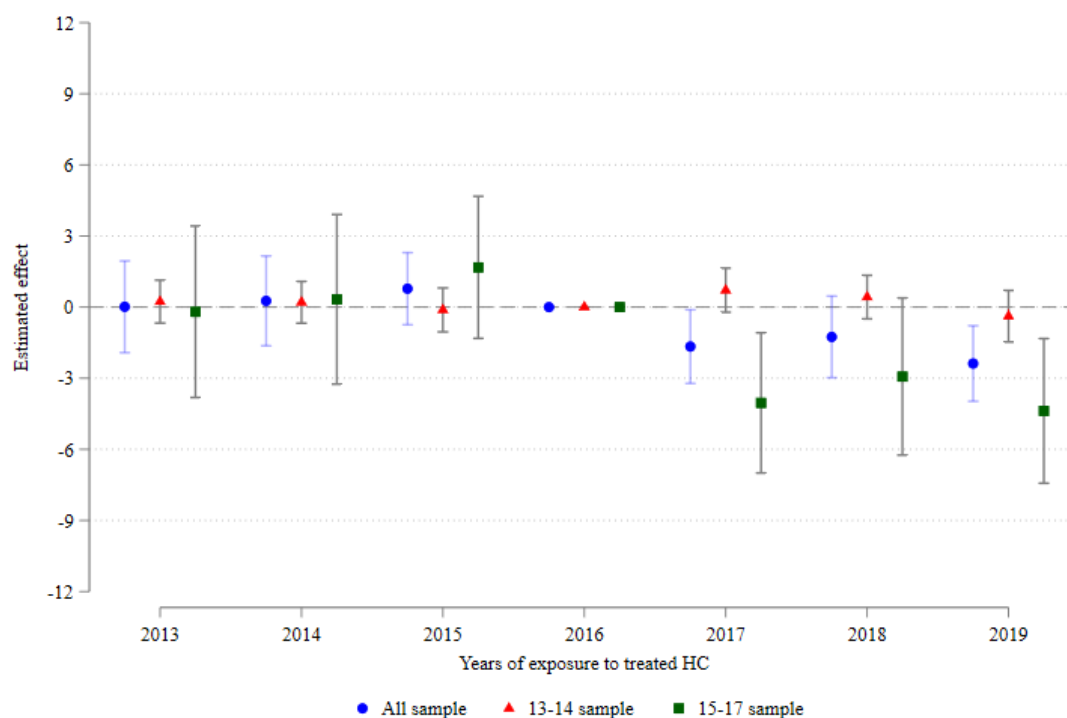
I estimate a difference-in-differences specification using administrative birth records covering the universe of registered births in Peru between 2013 and 2019. [Table C3](#) reports the results for adolescents aged 13–17 (Column 1), as well as for the 13–14 (Column 2) and 15–17 (Column 3) age cohorts separately. The reform is associated with a statistically significant reduction of about 2.1 births per 1,000 adolescents (around 13% relative to the control mean) in the full sample. When disaggregating the analysis by age group, the effect is concentrated among older adolescents (ages 15–17), for whom the reform led to a reduction of about 4.3 births per 1,000 (around 14–15% relative to the control mean).<sup>27</sup> These findings highlight the demographic significance of removing discretion-based barriers to contraceptive access for adolescents, particularly among those nearing the legal age of adulthood.

In addition to adolescent fertility rates, I also examine whether the reform influenced outcomes related to birth characteristics, utilising the administrative CNV dataset. Specifically, I examine average birth weight, birth height, and gestational age among newborns of adolescent mothers. Across all specifications, I find no statistically significant effects of the reform on these indicators ([Table C4](#)). These null results suggest that while the reform influenced fertility behavior, such as the timing and likelihood of adolescent births, it did not substantially alter maternal health or birth outcomes in the short term.

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<sup>27</sup>This is consistent with very low baseline fertility among ages 13–14 in the control group (about 2.2 births per 1,000), which limits the scope for large absolute reductions and reduces precision in this subgroup.

**FIGURE 3.2 – EVENT STUDY FOR TEENAGE FERTILITY RATES – *CERTIFICADO NACIDO VIVO* (2013–2019)**



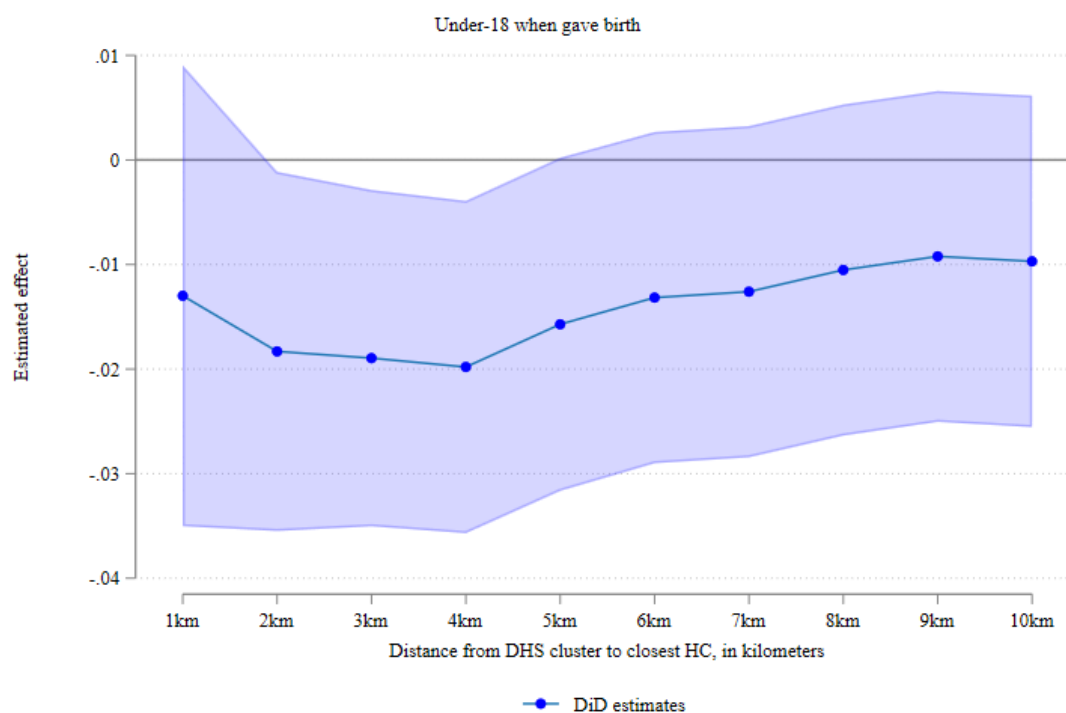
Notes: This graph presents summary difference-in-differences coefficients of Equation 3.1, estimated by Ordinary Least Squares (OLS) using the national birth records dataset. All regressions include district and year-department fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 95% level are plotted.

While the district-level approach provides broad coverage, it relies on administrative boundaries that may mask local variation in exposure. To address this, I implement an individual-level difference-in-differences model using DHS data, where treatment is defined based on Euclidean proximity for a given buffer to health centres providing adolescent-friendly services. A key empirical question is how far these services can realistically influence adolescent reproductive behavior. To address this, I estimate a series of difference-in-differences regressions using concentric distance buffers around each treated facility, ranging from 1 km to 10 km in radius. Figure 3.3 plots the estimated treatment effects across distance bands, along with 95% confidence intervals.

The results indicate that the reform significantly reduced adolescent fertility among girls living in close proximity to treated centres. The most significant and robust effects are concentrated within 2 to 4 kilometres, where the estimated declines are both statistically significant and economically meaningful. Beyond 4 km, the effects attenuate and become statistically indistinguishable from zero. This pattern suggests that geographic proximity is an important determinant of practical exposure, likely reflecting reduced time and travel costs, increased likelihood of contact with youth-focused providers, and higher take-up of confidential services.

The results, reported in Table 3.1, indicate that the reform significantly reduced adoles-

**FIGURE 3.3 – DIFFERENT DISTANCE BUFFERS FOR TEENAGE PREGNANCY – DHS (2013–2019)**



Notes: Each point represents the coefficient from a separate difference-in-differences regression estimating the effect of exposure to an HCA following the 2017 FP reform, following Equation 3.2 and using the DHS dataset. All regressions include covariates: age, age squared, Indigenous native tongue (=1), and wealth index. All regressions include district and survey year fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 95% level are plotted.

cent fertility among girls living near treated centres, corroborating the district-level findings. All specifications between 2 and 5 kilometres lead to negative and statistically significant coefficients, with the largest effects concentrated within the 3–4 km range. Specifically, the reform reduced the probability of having given birth before age 18 by around 1.5–2 percentage points (roughly 13–17 percent of the baseline). The magnitude and consistency of these results suggest that geographic proximity is a key determinant of program effectiveness, likely reflecting lower access costs, greater familiarity with services, and increased confidentiality for adolescents. Importantly, this pattern is consistent with the administrative birth records results, reinforcing the robustness of the evidence across data sources.

In light of this evidence, subsequent individual-level analyses restrict the sample to adolescents residing within 4 kilometres of a treated health facility, which captures the zone where the policy’s impact is most pronounced. To assess the validity of the identifying assumptions in this setting, I also estimate an event study specification using the DHS sample restricted to girls living within 4 kilometres of a treated facility. As shown in Figure C3, the pre-reform coefficients are small in magnitude and not statistically significant, providing no evidence of differential trends before the reform. The post-reform trajectory mirrors the pattern found in the administrative data, with a clear and sustained decline in teenage pregnancy following the

**TABLE 3.1 – IMPACT OF FP REFORM ON TEENAGE PREGNANCY RATES - DHS (2013-2019)**

Specification	Coefficient	Std. Error	Mean Dep. Var.	N
<i>Panel A. Post × HCA within 2km</i>				
All sample	-0.018**	(0.009)	0.1150	22,819
Only urban clusters	-0.016*	(0.009)	0.0921	14,976
<i>Panel B. Post × HCA within 3km</i>				
All sample	-0.019**	(0.008)	0.1170	22,819
Only urban clusters	-0.015*	(0.009)	0.0926	14,976
<i>Panel C. Post × HCA within 4km</i>				
All sample	-0.020**	(0.008)	0.1166	22,819
Only urban clusters	-0.019**	(0.009)	0.0889	14,976
<i>Panel D. Post × HCA within 5km</i>				
All sample	-0.016*	(0.008)	0.1176	22,819
Only urban clusters	-0.015	(0.009)	0.0886	14,976

Notes: This table presents summary difference-in-differences coefficients of Equation 3.2, estimated by Ordinary Least Square (OLS) using the DHS dataset. The outcome is a dichotomical variable taking the value of 1 if the individual gave birth at age 17 or younger. All regressions include age, age square, an indigenous native tongue (=1), and wealth index as well as district and survey year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

roll-out of adolescent-friendly services. These results reinforce the consistency and robustness of the main findings across data sources and identification strategies.

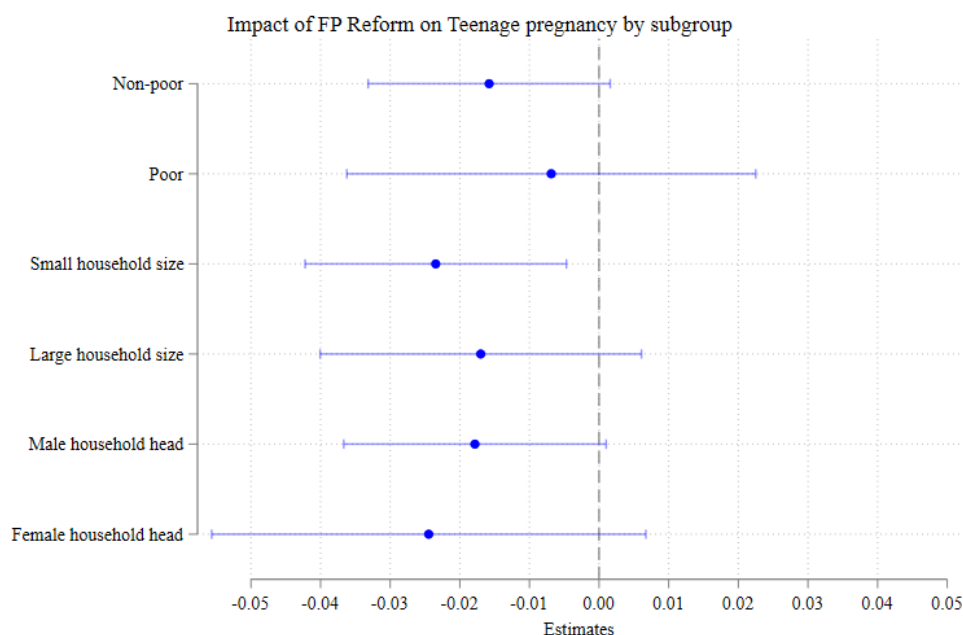
### 3.5.2 Heterogeneity Analysis

To better understand who benefited most from the reform, I explore heterogeneous effects across key socioeconomic and demographic dimensions. Using the DHS sample, I estimate subgroup-specific difference-in-differences models by wealth status, household size, and the gender of the household head. The results are presented in Figure 3.4.

Consistent with earlier findings, the reform's impact is concentrated among adolescents from relatively more advantaged backgrounds (see Table C5). Non-poor adolescents experience a statistically significant reduction in teenage pregnancy rates of approximately 1.6 percentage points compared to an insignificant effect among poorer peers.

Notably, additional heterogeneity is observed in household-level characteristics. Adolescents living in larger households and those in households headed by men exhibit significantly stronger fertility declines. The fertility rate among girls in large households drops by 2.3 percentage points, and the effect for those living in male headed households is approximately 1.8 percentage points; both estimates are statistically significant at conventional levels. These patterns likely reflect deeper structural advantages: adolescents in wealthier, male-headed, or larger households may face fewer mobility constraints, benefit from stronger linkages to the formal health system, or be better positioned to navigate newly expanded service options.

**FIGURE 3.4 – IMPACT OF FPTG REFORM ON TEENAGE PREGNANCY RATES, HETEROGENEITY ANALYSIS – DHS (2013–2019)**



Notes: This graph presents summary difference-in-differences coefficients of Equation 3.1, estimated by Ordinary Least Squares (OLS) using the birth records dataset, separately for each of the following subgroups: individuals in the upper half of the wealth index distribution, those in households below the median household size, and those living in male-headed households. All regressions include district and survey year–department fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 95% level are plotted.

These households may also be more responsive to policy reforms, especially when services require navigating institutional norms or procedures.

These results reveal the importance of intersectional barriers in shaping the impact of adolescent-focused policy reforms. Although the reform eliminated legal age-based restrictions and introduced adolescent-friendly services, its benefits were unevenly distributed. Targeted outreach or complementary support strategies may be necessary to ensure that adolescents from poorer, indigenous, or female-headed households are equally able to access and benefit from expanded reproductive health services.

### 3.5.3 Effects on Cohabitation, Marriage and Education

I investigate whether the reform has more wide-ranging effects beyond its effects on teenage pregnancy, the reform also appears to shift family formation and schooling trajectories. As shown in Table 3.2, adolescent girls living close to health centres that offered adolescent-friendly services were significantly less likely to report being married or cohabiting before age 18. The estimates imply declines of about 2.5–3 percentage points from pre-reform means of 11–13 percent, i.e. roughly a one-fifth reduction in early unions. At the same time, the reform reduced the total number of children ever born by around 0.03 among adolescents (column 3), corresponding to a sizeable decline relative to the baseline mean. These patterns suggest

that access to confidential, adolescent-centered services not only lowers the probability of any teen birth but also dampens the intensity of early childbearing and delays union formation.

Schooling outcomes show a consistent picture. [Table 3.3](#) documents that girls exposed to the reform are less likely to report leaving school because of childbearing or marriage: the probability of citing pregnancy/childbearing as the main reason for stopping school falls by about 1.7 percentage points (almost one-third of the pre-reform mean), with a smaller decline for marriage-related reasons. By contrast, there is little evidence that the reform affected the likelihood of stopping school for economic reasons, because the respondent had already completed her studies or academic disengagement, suggesting that the policy mainly altered fertility-related causes of dropout rather than broader schooling constraints. Taken together, these additional results indicate that the declines in early cohabitation, marriage, and fertility operate through a re-timing of key life-course transitions, allowing adolescents to remain in school and accumulate human capital for longer. These patterns are consistent with a broader literature documenting the interplay between reproductive autonomy, early marriage, and human capital investment ([Bailey, 2012](#); [Goldin and Katz, 2002](#); [Stevenson et al., 2021](#)).

**TABLE 3.2 – IMPACT OF FPTG REFORM ON COHABITATION AND MARRIAGE - DHS (2013-2019)**

	(1) Cohabitation-18	(2) Marriage-18	(3) Total Children
<i>Panel A. All sample</i>			
4km HC × Post	-0.025*** (0.008)	-0.029*** (0.008)	-0.035*** (0.009)
Observations	22,819	22,819	22,819
Mean Dep. Var.	0.11	0.13	0.14
<i>Panel B. Only urban</i>			
4km HC × Post	-0.016* (0.008)	-0.024*** (0.009)	-0.023** (0.010)
Observations	14,976	14,976	14,976
Mean Dep. Var.	0.08	0.10	0.11

*Notes:* This table reports difference-in-differences estimates from [Equation 3.3](#), estimated by ordinary least squares (OLS) using DHS data. The outcomes are: (i) an indicator equal to 1 if the individual is cohabiting (living with a partner) before age 18, (ii) an indicator equal to 1 if the individual married before age 18, and (iii) total children ever born (continuous). All regressions include age, age square, an indigenous native tongue (=1), and wealth index as well as district and survey year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

### 3.5.4 Post-Reform Dynamics and the COVID-19 Disruption

An essential aspect of evaluating the reform's effectiveness is understanding how its impacts evolved over time, particularly in response to external disruptions. Using CNV data extended to 2023, [Figure 3.5](#) presents the dynamic treatment effects by year since exposure to a treated health facility, disaggregated by adolescent age group. The reform begins to show

**TABLE 3.3 – IMPACT OF FPTG REFORM ON PATHWAYS OUT OF SCHOOLING: FERTILITY, MARRIAGE, AND SCHOOL PROGRESSION - DHS (2013-2019)**

	(1)	(2)	(3)	(4)	(5)
	Childbearing	Marriage	Economic	Graduated or short-course	Academic disengagement
<i>Panel A. All sample</i>					
4km HC × Post	-0.017** (0.008)	-0.008** (0.003)	0.012 (0.008)	-0.011 (0.007)	0.000 (0.005)
Observations	22,819	22,819	22,819	22,819	22,819
Mean Dep. Var.	0.06	0.03	0.08	0.04	0.03
<i>Panel B. Only urban</i>					
4km HC × Post	-0.015* (0.009)	-0.007* (0.004)	0.011 (0.008)	-0.015 (0.009)	-0.002 (0.005)
Observations	14,976	14,976	14,976	14,976	14,976
Mean Dep. Var.	0.06	0.01	0.07	0.06	0.02

*Notes:* This table presents summary difference-in-differences coefficients of Equation 3.3, estimated by Ordinary Least Square (OLS) using the DHS dataset. The outcomes are binary indicators for the main reason the respondent stopped attending school: (i) childbearing/pregnancy, (ii) marriage, (iii) economic constraints, (iv) already graduated or enrolled in a short course/academy, and (v) academic disengagement (did not want to continue studying). All regressions include age, age square, an indigenous native tongue (=1), and wealth index as well as district and survey year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

statistically significant reductions in fertility for the older cohort (15–17) in the years immediately following its roll-out. However, these effects appear to dissipate in 2020, coinciding with the onset of the COVID-19 pandemic.

This change in trend coincides with Peru’s national lockdown and the temporary suspension of non-urgent health services, including family planning. Public health centres were repurposed to manage COVID-19 cases, and in-person services were restricted, plausibly limiting adolescents’ access to contraceptive counselling and supplies. While this narrative is consistent with administrative reports documenting service disruptions, these patterns should be interpreted cautiously. The pandemic generated multiple simultaneous shocks, including economic, social, behavioural, and institutional changes, that are difficult to separate empirically in this context.<sup>28</sup>

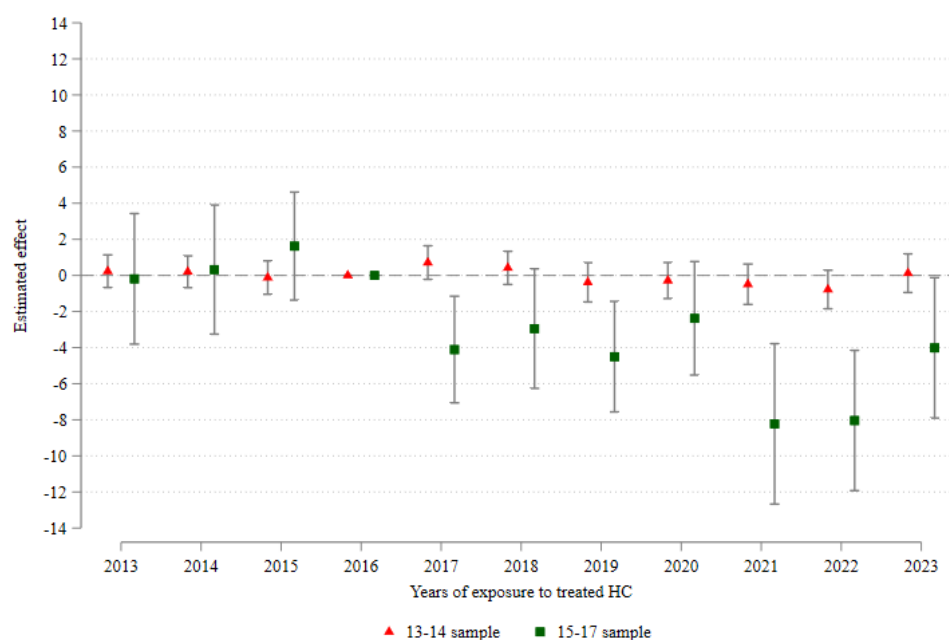
Interestingly, fertility declines among the 15–17 cohort reappear and strengthen from 2021 onward, once services began to resume. This trend may reflect the reinstatement of adolescent-focused service delivery, but it may also capture other post-pandemic dynamics. In contrast, the 13–14 age group consistently shows null effects throughout the period, consistent with earlier findings that the reform primarily benefited older adolescents closer to reproductive maturity.

Overall, these patterns suggest that institutional design is critical, but sustained service delivery is also key to long-term behavioral change. At the same time, they underscore

<sup>28</sup>As a result, the empirical design does not permit a causal attribution of the 2020 attenuation to pandemic-related service disruptions, given the presence of concurrent unobserved shocks.

the vulnerability of adolescent reproductive services to systemic shocks, and how crises like COVID-19 can complicate efforts to measure and sustain policy impact in real time.

**FIGURE 3.5 – EVENT STUDY FOR FERTILITY RATES – *CERTIFICADO NACIDO VIVO* (2013–2023)**



Notes: This graph presents summary difference-in-differences coefficients of Equation 3.1, estimated by Ordinary Least Squares (OLS) using the national birth records dataset for the period 2013–2023. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 95% level are plotted.

## 3.6 Robustness

I perform several robustness checks to verify the consistency and reliability of the main findings. First, a key identifying assumption is that, in the absence of the FPTG, the teenage fertility rates among districts with and without health centres would have followed parallel trends. Following standard practice in the literature (Roth et al., 2023), I statistically test for differences in trends before the implementation of the FPTG. Results are presented in Table C7, and I cannot reject the null hypothesis that the two groups were on parallel trends before 2017.

To assess the validity of the identifying assumption underlying the difference-in-differences design, I examine whether the timing of the reform is correlated with pre-existing trends in adolescent fertility. Specifically, I test whether districts that gained exposure to adolescent-friendly services already exhibited differential fertility trajectories prior to the reform. Using the universe of administrative birth records, I estimate regressions of pre-reform changes in fertility rates on indicators for the year a treatment facility became available in a given district. The results presented in Table C8 show no evidence of differential trends in treated versus

untreated areas before the reform was implemented. This suggests that the timing of the adolescent-focused family planning rollout was not systematically related to prior changes in fertility, supporting the plausibility of the parallel trends assumption. Overall, event study graphs and formal tests of pre trends (Table C7 and Table C8) show no evidence that treated and comparison districts differed in their adolescent fertility trajectories prior to the reform, supporting the parallel trends assumption. Consistent patterns also emerge in the DHS sample (see Table C9 and Table C10).

Second, the results are not driven by any particular region nor the metropolitan area of the capital city Lima (Table C11 and Table C12). The estimated coefficients remain stable when I omit one region at a time from the database. Third, to account for potential spatial correlation in treatment exposure, I also compute Conley standard errors (Conley, 1999), which allow for spatial autocorrelation based on geographic distance. Results remain robust and statistically significant across a range of distance cut-offs, indicating that spatial dependence does not drive the main findings.

Additionally, I re-estimate the district-level DiD specifications using the CNV data aggregated at the quarter and semester level instead of annually. The estimated effect of exposure to an adolescent-friendly health facility remains negative and statistically significant in both panels (Table C13), with magnitudes similar to the baseline results. This confirms that the estimated reduction in teenage fertility is not driven by the particular choice of temporal aggregation and that the reform's impact is robust to using finer time variation in births data. Finally, I present individual-level results estimated using an alternative functional form, a probit regression model, in Table C14. The results remain consistent with the baseline findings across the different distance thresholds.

Overall, these robustness checks collectively reinforce the main conclusion: proximity to HCA facility after the FPTG reform is consistently associated with decreased teenage pregnancy rates.

### 3.6.1 Age-gradient analysis

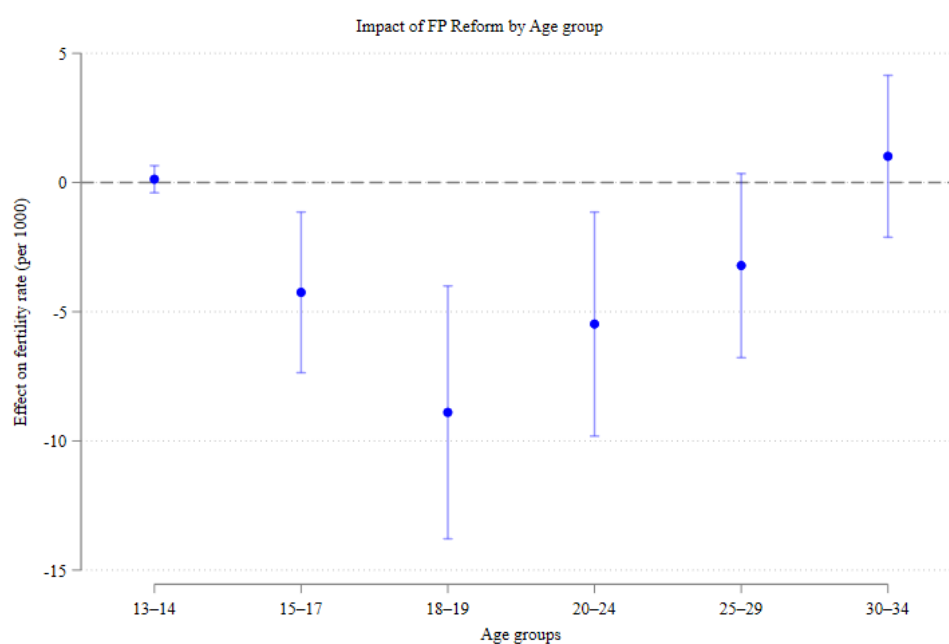
As an additional robustness exercise, I examine whether the FP reform affected women beyond the formally targeted group. I estimate separate difference-in-differences models for six age cohorts: 13–14, 15–17, 18–19, 20–24, 25–29, and 30–34. Each model follows the main district-level specification but is estimated independently for each age group, allowing me to trace how the reform's impact varies across the age distribution.

The results, presented in Figure 3.6, reveal no significant change in fertility among girls aged 13–14, consistent with their low baseline fertility and limited demand for reproductive health services at that age. Fertility declines significantly among 15–17-year-olds, which is the group formally targeted by the reform, and even larger reductions are observed among women aged 18–19 and 20–24. These patterns suggest that improved access to and quality

of contraceptive services at HCA centres also benefited slightly older women who use the same centres and providers. Fertility also falls modestly among women aged 25–29, with no discernible effect for those aged 30–34. Together, these results indicate a clear age gradient in treatment effects, with the largest declines concentrated among adolescents and younger adults.

This pattern has direct implications for identification. The presence of substantial treatment effects among 18–24-year-olds undermines their validity as an untreated comparison group in a triple-difference (DDD) design, since they are also exposed to the upgraded centres and the broader improvements in service delivery triggered by the reform. As a result, DDD estimates that use older women as a control group are likely to be biased toward zero and understate the true effect on the intended teenage population. The age-gradient analysis therefore supports interpreting the reform as a broader improvement in local reproductive health services, and it justifies relying on age-specific DiD estimates rather than DDD as the preferred identification strategy.<sup>29</sup>

**FIGURE 3.6 – FERTILITY RESPONSES TO FPTG REFORM ACROSS AGE COHORTS - CNV (2013–2019)**



*Notes:* Each point represents the coefficient from a separate difference-in-differences regression estimating the effect of exposure to an HCA following the 2017 FP reform. The outcome is the annual fertility rate (births per 1,000 women) for each age cohort. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 90% level are plotted. Sample period: 2013–2019.

<sup>29</sup>One potential alternative would be to exploit the DHS, which includes a large cross-sectional sample of women aged 15–49. However, the DHS is a repeated cross-section, not a panel, making it difficult to track exposure consistently across cohorts over time. Since different individuals are observed in each wave and assignment to treatment relies on age at exposure, comparisons between adjacent cohorts (e.g., 15–17 vs. 18–19) risk conflating age, period, and sample-composition effects. In contrast, the national births dataset provides a complete panel of fertility outcomes by district and age group over time, allowing for cleaner implementation of cohort-based DiD and robustness analyses.

### 3.7 Potential mechanisms

To better understand the mechanisms behind the observed decline in adolescent fertility, I use the DHS microdata to examine changes across three domains: (i) contraceptive use and knowledge, (ii) family planning communication channels, and (iii) sexual behavior.

Panel A of [Table C6](#) shows that adolescents living near centres offering adolescent-friendly services were significantly less likely to use traditional methods (e.g., withdrawal or rhythm) and more likely to report current condom use. The estimated 4–5 percentage-point increase in condom use represents about a 20–25% rise relative to the pre-reform mean, consistent with the magnitude of the fertility decline.

While there is no statistically significant evidence of an overall rise in modern contraceptive uptake requiring clinical involvement (such as hormonal methods or IUDs), the increase in condom use suggests a shift toward less provider-dependent and more accessible methods. This behavioral shift is accompanied by a modest rise in contraceptive knowledge, especially regarding modern methods, indicating improved awareness and a greater ability to make informed choices.

These findings suggest that the reform enhanced access to methods that better align with adolescents' preferences for autonomy and discretion. A plausible interpretation is that the rise in condom use reflects greater comfort with over-the-counter options and improved privacy, facilitated by confidential service delivery. Consistent with this view, Panel B shows a reduction in reported discussions about family planning with partners, relatives, and peers. This decline should not necessarily be read as disengagement, but may instead indicate a shift away from informal and potentially judgemental sources of advice toward professional guidance offered in adolescent-friendly clinics. Although the DHS does not directly record whether health providers were the primary source of information, the simultaneous decline in reliance on family and peers, together with increased contraceptive knowledge and condom use, is suggestive of a shift toward more formal channels.

Panel C finds no significant changes in sexual behavior indicators, such as age at first intercourse or partner age gaps, which suggests that fertility reductions were not driven by delayed sexual debut but rather may have resulted from safer and more autonomous contraceptive practices. Taken together, these patterns imply that the reform's main contribution was less about altering sexual behavior per se and more about enabling adolescents to manage reproductive decisions with greater privacy, reliability, and control.<sup>30</sup>

Together, these findings suggest that the reform primarily operated by enhancing access to reliable contraception and improving the informational environment.<sup>31</sup> Adolescents responded

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<sup>30</sup>I also explore changes in fertility preferences, as measured by the number of children adolescents report wanting in the future. I find no statistically significant change associated with the reform, suggesting that shifts in long-term fertility goals do not drive the observed fertility effects.

<sup>31</sup>The available data only allow me to observe women's fertility outcomes and contraceptive behaviour. I do not have comparable information on men's contraceptive use or service utilisation, either in the birth records or in the DHS. As a result, the mechanism analysis focuses on women's side of contraceptive

by switching to more effective and accessible methods like condoms, reducing reliance on informal advice, and making more autonomous reproductive decisions.<sup>32</sup> While changes in reported sexual behaviour were limited, the increase in contraceptive knowledge and condom use points to a meaningful shift in how adolescents engage with family planning in settings where stigma and institutional barriers have historically limited their agency.

## 3.8 Conclusion

This study provides causal evidence that institutional design plays a critical role in shaping adolescent reproductive behaviour. The 2017 Peruvian family planning reform reduced institutional ambiguity and discretionary barriers around adolescent access, and introduced adolescent-focused protocols in a subset of public health centres. These changes led to sustained reductions in adolescent fertility, particularly among older girls and those living closer to centres offering the upgraded services. The findings demonstrate the importance of proximity and the quality of service delivery in determining programme effectiveness.

The reform also influenced other key life transitions. Adolescents exposed to the policy were less likely to enter early unions and less likely to leave or interrupt school because of pregnancy, childcare responsibilities, or marriage/cohabitation. These results point to broader improvements in reproductive autonomy and decision-making, suggesting that access to adolescent-centred services can delay family formation and reduce role conflicts between schooling and family duties. Such changes represent important investments in human capital during a critical period of development.

Analysis of potential mechanisms reveals that the reform shifted contraceptive behaviour toward more autonomous and accessible methods. Adolescents in treated areas reported lower use of traditional methods and a modest increase in condom use, consistent with a preference for discreet options that do not require clinical intervention. In parallel, adolescents became less likely to rely on family members or peers for reproductive health information. While the data do not directly record whether health providers were the primary source of information, the simultaneous decline in reliance on family and peers, together with increased contraceptive knowledge and condom use, is suggestive of a shift toward more formal channels for contraception and counselling.

Despite these gains, the benefits of the reform were unevenly distributed. Fertility reductions were significantly larger among adolescents from wealthier households, non-indigenous backgrounds, urban areas, male-headed families, and larger households. These disparities reveal the persistence of structural and cultural barriers that continue to shape access, even

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decision-making.

<sup>32</sup>Because contraceptive use and sexual behaviour are self-reported and potentially subject to social desirability bias, these mechanism estimates should be interpreted cautiously; nonetheless, the patterns are internally consistent with the fertility results.

after the reform strengthened confidentiality protections and standardised adolescent-friendly service protocols, and highlight how ostensibly universal policies can disproportionately benefit more advantaged groups.

The qualitative evidence discussed next helps interpret why the reform's effects are larger in urban and more advantaged settings, and why effective access may not improve uniformly across space. In many regions, adolescent-friendly services remained scarce: for instance, fewer than ten percent of centres in Piura offered differentiated services in 2017, and even in Arequipa only a minority provided private, adolescent-appropriate spaces (PROMSEX, 2017). These services were concentrated in urban centres, leaving rural and peripheral areas underserved. Adolescents also faced stigma when seeking contraception, including concerns about confidentiality, fear of judgement by providers, and community disapproval. Reports from peri-urban areas describe moralising behaviour and denial of services (PROMSEX, 2023), while household dynamics, particularly in conservative or indigenous families, further constrained adolescents' autonomy to make confidential reproductive health decisions.

Taken together, these findings suggest that while the 2017 reform expanded institutional opportunities and formal protections, cultural, social, and operational barriers limited its reach. The distinction between rights on paper and effective access is critical. Closing this gap requires adapting implementation to local conditions, fostering community engagement, training providers in non-discriminatory practices, and monitoring service quality.

This study contributes to the broader literature on reproductive health policy by showing that adolescent outcomes can improve in middle-income contexts when legal reforms are combined with trusted and confidential service delivery. In high-income countries, access to contraception has been linked to delayed fertility, higher educational attainment, and improved labour market outcomes (Bailey, 2012; Goldin and Katz, 2002). In Latin America, the findings complement Miller (2010) on Colombia, which documents fertility and education gains following family planning expansion. The Peruvian case adds new evidence by focusing on adolescents and highlighting mechanisms related to discretion, trust, and autonomy. By comparison, studies from Zambia and Burkina Faso emphasise that interventions which expand supply alone often yield more limited results because of demand-side frictions and institutional resistance (Ashraf et al., 2013; Dupas et al., 2024). The results therefore suggest that policies prioritising confidentiality and adolescent agency may be better positioned to overcome stigma and increase contraceptive uptake in settings where adolescent fertility remains high.

The temporary reversal of gains during the COVID-19 pandemic further highlights the vulnerability of adolescent reproductive services to external shocks. Event-study estimates show that the fertility reductions associated with the reform were effectively nullified in 2020, when non-urgent reproductive health services were suspended, but resumed once services were restored. This interruption illustrates the need for resilient systems that can preserve access during public health emergencies; continuity of care must be a core priority in future policy planning.

Overall, the findings underscore that expanding access to modern contraceptive methods is not sufficient on its own; the design of services and the conditions under which adolescents seek care are central to reducing early childbearing. In the Peruvian context, this suggests that scaling up adolescent-friendly models such as HCA to underserved and rural areas, ensuring sustained training and supervision of providers, and monitoring stigma and confidentiality breaches should be policy priorities. More broadly, the results speak to other middle-income settings where adolescent fertility remains high despite nominal contraceptive availability: reforms that combine method access with confidential, judgement-free counselling and clear, age-appropriate protocols may yield sizable gains in delaying first births and supporting girls' educational trajectories.

While the analysis is specific to Peru's health system and legal framework, the patterns documented here are likely relevant for other Latin American countries with persistently high adolescent fertility, such as Colombia, Ecuador, and Bolivia. Many of these settings share features such as relatively high contraceptive prevalence among adults, decentralised public health systems, and strong social norms around sexuality and motherhood. At the same time, cross-country differences in the scope of reproductive rights, the organisation of primary care, and the extent of opposition to comprehensive sexuality education may shape both the feasibility and the impact of adolescent-friendly service models. As such, the estimates in this paper should be interpreted as evidence of what can be achieved when discretionary constraints and confidentiality-related barriers are reduced, and adolescent-friendly delivery conditions are implemented, rather than as mechanical predictions for other contexts.

In summary, this study offers clear lessons for governments seeking to reduce adolescent fertility within restrictive reproductive environments. Legal reforms are necessary, but not sufficient. To be both effective and equitable, reforms must be accompanied by institutional investments that expand real access, build trust, and promote adolescent autonomy. Ensuring that services are confidential, culturally appropriate, and physically accessible is critical to empowering young people to make informed decisions about their futures.

# Chapter 4

## The gendered effects of climate shocks on labour and welfare in Zambia

### 4.1 Introduction

Climate change intensifies economic and social vulnerabilities worldwide, with its impacts often magnified along gender lines (UN Secretariat, 2022). As extreme weather events such as droughts, floods, and hurricanes become more frequent and severe, households face increased pressures, spending more time on domestic responsibilities while also striving to offset income losses. In low-income settings, these shocks test households' ability to smooth consumption through informal insurance, savings, and labour reallocation, yet risk-sharing is typically incomplete, especially under covariate shocks that hit entire local economies (Dercon, 2002; Dercon and Krishnan, 2000; Morduch, 1995; Townsend, 1994; Udry, 1994). These constraints can be particularly salient for women given gender gaps in labour market opportunities, access to resources, and the unequal distribution of unpaid care and domestic work. Moreover, when coping occurs through within-household reallocation of time and work, existing gender divisions of labour can shift adjustment burdens toward women, with implications for labour supply and well-being (Dercon, 2002; Dercon and Krishnan, 2000). Yet systematic evidence on gender-differentiated labour responses and welfare losses from climate shocks in low-income settings remains limited, particularly in Sub-Saharan Africa and for work that jointly links labour supply and time-use adjustments to household welfare. Existing evidence for the region has focused mainly on specific margins such as migration and demographic responses, with comparatively fewer studies quantifying gender-differentiated impacts on labour, income, and consumption.<sup>1</sup>

In this paper, we study the impacts of climate shocks on labour outcomes, time use, income, and consumption in Zambia. We use two main data sources: the Labour Force

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<sup>1</sup>See, for example, Anglewicz and Myroniuk (2018); Dillon et al. (2011); Gray and Mueller (2012); Lee et al. (2021); Marchetta et al. (2019).

Surveys (LFS) for 2017, 2018, 2019, 2020, and 2021 from the International Labour Organization (ILO); and the Living Conditions Measurement Survey (LCMS) 2015 and 2022 from the Zambia Statistical Agency (ZAMSTAT).<sup>2</sup> Both datasets have information on individuals' socioeconomic characteristics, district of residence, and the quarters in which they were interviewed. We merge these data with granular rainfall and temperature measures to construct district-by-period indicators of extreme climate shocks. We use the LFS and LCMS to organise important evidence on the Zambian labour market and household welfare over this period. While headline figures are disseminated through annual reports, access to the underlying microdata is limited, so our analysis also helps to systematise and make visible a rich but rarely used data infrastructure. For instance, in 2021, 23% of the population had a job, 4% was unemployed, and 72% was outside the labour force, that is, neither employed nor actively looking for work. Women represented 37% of the labour force. Services was the sector with the most individuals in the labour force (47%), followed by agriculture (22%) and manufacturing (12%). The monthly food consumption per capita in 2022 was about US\$18, around half of households fell below the basic food basket value per capita, and 26% of households had a woman as the household head.

We study an important and representative context. Zambia is a predominantly agricultural country with a large presence of mining companies. Its status is often between low- and lower-middle-income countries, with a profile similar to many African countries. In 2024, Zambia faced the worst drought in the last 60 years (Sinyangwe and Savage, 2024). Even though the country is not ranked as one of the most vulnerable countries in terms of climate index, it has been facing many extreme weather episodes such as floods, flash floods, and droughts. One of the most recent household surveys, from 2024, suggests that about 60% of the population is below the poverty line and that a large portion of Zambia's infrastructure is inadequate. These climate shocks pose a serious threat to economic stability and growth, food security, and overall public health.

Climate shocks may affect labour outcomes through impacts on businesses (Feriga et al., 2024; Kala et al., 2023), but also through direct reductions in households' well-being. Several channels may explain these welfare losses, including declines in income and consumption, destruction of family assets, withdrawal of children from school, and higher incidence of health problems. Guided by a standard consumption-smoothing framework, we focus on reductions in well-being measured by income and, especially, by consumption. The LCMS has detailed information on the goods consumed by families. Although the LFS also includes wages and self-employment income, researchers usually rely on consumption as a better measure of well-being in low-income countries, as it is more closely tied to permanent resources and less volatile than income in settings with high poverty and informality (Meyer and Sullivan, 2003). Many families may report no income in household surveys but still maintain a minimum level of

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<sup>2</sup>We also use the In-Depth Vulnerability Survey for 2023 from the Disaster Mitigation and Management Unit (DMMU) in the descriptive statistics.

consumption for subsistence. Unfortunately, information on consumption (LCMS) is available at the household level but not at the individual level.

Because within-district variation in extreme rainfall and temperature is plausibly exogenous to household and individual decisions, and we control for district and time fixed effects (year in the LCMS and year–quarter in the LFS), we interpret the estimated effects of climate shocks as causal impacts under standard assumptions. In both datasets, we focus on extreme high temperature and extreme low/high precipitation; extreme low temperatures are rare in Zambia and provide insufficient variation for reliable estimation.

Using the LCMS, we first document that short-run weather shocks have sizeable negative impacts on household food consumption value per capita. On average, extreme high temperatures in the past month are associated with a decline in food consumption value of about 32%, while extremely low precipitation in the past month is associated with a decline of about 12%. Consistent with households substituting between market purchases and non-market sources of food, the corresponding estimates for cash food expenditures are larger in magnitude but less precisely estimated. Given the limited number of survey years and the concentration of interviews within specific periods, there is uncertainty around the exact magnitude of these effects, with relatively wide confidence intervals.

We then study how these welfare responses differ by the gender of the household head. We find that climate shocks widen gender gaps in cash food expenditures: female-headed households experience larger reductions in food spending than male-headed households, particularly during extreme heat and episodes of unusually high precipitation, even though differential effects on total food consumption value are small and statistically insignificant. This pattern is consistent with female-headed households adjusting more through home production and in-kind sources of essential items, which can help smooth total consumption but may also reinforce a poverty trap (Ghatak, 2015; Klasen et al., 2014).

Next, we turn to labour supply and time-use adjustments as key mechanisms through which households attempt to stabilise consumption in the face of shocks. Using the LFS, we show that quarters with extreme rainfall reduce labour force participation on average, while extreme high temperatures are associated with higher labour force participation, driven primarily by men. These average effects hide sharp gender differences. Men increase labour supply during drought-like conditions (very low precipitation), largely through informal work. Women, by contrast, are less likely than men to participate in the labour force during both dry and wet extremes and are more likely to shift toward self-employment and household-production activities when experiencing extreme rainfall and extreme heat, with corresponding declines in formal and informal wage employment. We also find that, conditional on working, women are more likely to be in services while men are more likely to work in manufacturing during climate shocks. Taken together with the consumption results, these patterns are consistent with women reallocating time away from market work and toward home production and subsistence activities that help maintain basic consumption levels.

Finally, by interacting the climate shock measures with indicators of whether the household receives a social protection programme, we provide suggestive evidence on the potential mitigating role of social protection. This analysis is particularly relevant because Zambia is a low-income country with one of the oldest unconditional cash transfer schemes (initiated in 2003) and several other programmes such as the Food Security Pack ([Ministry of Community Development and Social Services, 2021](#)) and the Women's Livelihoods Programme ([Girls' Education and Women's Empowerment and Livelihoods Project, nd](#)). Our results suggest that social protection, especially social cash transfers, can partially cushion income and consumption losses during climate shocks and may therefore attenuate the need for extreme labour-supply adjustments.

This paper has two contributions to the literature. First, we expand the literature on the gendered impacts of climate shocks and labour reallocation to a low-income African country. The existing evidence for low-income countries focuses on other outcomes and mostly relies on small samples or experimental settings. We exploit the two most important nationally representative surveys in Zambia to provide new evidence that can inform future research and policy in similar contexts. Because of the lack of data availability, the existing evidence is highly concentrated in developed and upper-middle-income countries such as India ([Afridi et al., 2022](#); [Colmer, 2021](#); [Liu et al., 2023](#)), Brazil ([Branco and Féres, 2021](#); [Xie, 2024](#)), Mexico ([Jessee et al., 2018](#)), and China ([Garg et al., 2020](#); [Huang et al., 2020](#); [Zhang et al., 2018](#)). For instance, many of these studies rely on panel data spanning more than a decade, with detailed information on location and interview timing, which is not the case in Zambia. Our paper is closely related to [Afridi et al. \(2022\)](#), who study gendered effects of climate change in India, but we find much stronger effects of climate shocks on female labour force participation. The limited evidence for African countries has mainly examined margins such as migration and demographic responses ([Anglewicz and Myroniuk, 2018](#); [Dillon et al., 2011](#); [Gray and Mueller, 2012](#)), with less work linking gender-differentiated labour and time-use adjustments to household welfare.

Second, we contribute by jointly measuring the impacts of climate shocks on household income, consumption, and labour allocation in one of the world's poorest countries. Prior work for developed and upper-middle-income countries has shown systematic negative impacts of climate shocks on economic activity but mixed evidence on income inequality ([Dang et al., 2024](#); [Tol, 2018, 2022](#)). Measuring welfare using consumption is important because consumption inequality is typically more stable, as consumption depends more on permanent rather than current (possibly transitory) income ([Meyer and Sullivan, 2003](#)), and because consumption is better measured than income in low-income settings. Much of the existing literature has instead focused on aggregate outcomes such as GDP per capita, income, productivity,<sup>3</sup> and mortality ([Deschênes and Greenstone, 2011](#); [Heutel et al., 2021](#); [Kahn, 2005](#)).

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<sup>3</sup>See [Garg et al. \(2020\)](#), [Tol \(2018\)](#), [Burke and Emerick \(2016\)](#), [Adhvaryu et al. \(2020\)](#), and [Somanathan et al. \(2021\)](#).

By linking household consumption responses to gendered labour and time-use adjustments, our study sheds light on how households, especially women, attempt to smooth consumption in the face of climate shocks and on the extent to which social protection can support this process.

The remainder of the paper is structured as follows. Section 4.2 presents the economic framework. Section 4.3 provides background on Zambia and its climate vulnerability. Section 4.4 describes the data sources and the construction of the main variables. Section 4.5 outlines the empirical strategy. Section 4.6 presents the main results on household welfare and labour-market outcomes, together with complementary analysis on the role of social protection. Section 4.7 concludes.

## 4.2 Economic framework

To interpret the empirical results, I use a simple household framework with incomplete insurance and gendered labour supply. This setup builds on standard models of household time allocation and home production (Becker, 1965; Gronau, 1977). While the agricultural household model (Singh et al., 1986) provides the natural reference point for rural households, Zambia’s labour markets are characterised by substantial rural-to-urban migration and a large urban informal sector, meaning that many households, including urban ones, remain partially exposed to agricultural income risk. I therefore use a general household framework that nests both rural and urban settings, while acknowledging that the transmission mechanisms differ across geographies.

I adopt a unitary household model, treating the household as a single decision-making unit with a joint utility function. This abstracts away from intra-household bargaining (?), but is appropriate here for two reasons. First, the data do not allow identification of individual-level bargaining weights. Second, the primary interest of this chapter is in household-level consumption responses and individual-level labour market adjustments conditional on household membership. Gendered differences in outcomes are instead captured through gender-specific wage schedules and time endowments, as described below.

A household faces income risk from climate shocks and chooses consumption and time allocation to maximise expected utility. Let  $C$  denote household consumption and  $l^g$  leisure for adult  $g \in \{m, f\}$  (man, woman).<sup>4</sup>

The household solves:

$$\max_{C, l^m, l^f, h^m, h^f, n^m, n^f} \mathbb{E}[U(C, l^m, l^f)], \quad (4.1)$$

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<sup>4</sup>I allow labour endowments to vary by headship: female-headed households may have a lower endowment of adult male labour because it is more likely that no male partner is resident.

subject to time constraints:

$$T = l^g + h^g + n^g \quad \text{for } g \in \{m, f\}, \quad (4.2)$$

where  $h^g$  is time in market work and  $n^g$  time in home production and self-employment (subsistence agriculture, food processing, care and domestic work). Home production allows households to substitute own-produced goods for market purchases when income falls, and therefore constitutes a key margin of consumption smoothing.

The budget constraint is:

$$C = w_m(\theta)h^m + w_f(\theta)h^f + p_H(\theta)H(n^m, n^f; \theta) + Y_0 + B, \quad (4.3)$$

where  $w_g(\theta)$  are gender-specific wages,  $H(\cdot)$  is a home-production technology with value  $p_H(\theta)$ ,  $Y_0$  denotes other income (assets, remittances, informal transfers),  $B$  captures public transfers and social protection, and  $\theta$  is the realisation of a climate shock. I model  $\theta$  as a district-level covariate shock — affecting all households in a district simultaneously — that is transitory in nature but potentially persistent in its welfare effects given the limited insurance available to households (Dercon, 2002). Specifically,  $\theta$  takes an adverse realisation when rainfall or temperature crosses an extreme threshold, reducing agricultural productivity and local labour demand. As a result, both wages  $w_g(\theta)$  and the value of home production  $p_H(\theta)$  fall when  $\theta$  is adverse.

With incomplete insurance, households rely on a combination of assets, informal transfers, public transfers  $B$ , and adjustments in  $(h^m, h^f, n^m, n^f)$  to smooth consumption (Dercon, 2002; Fafchamps, 2003). A sufficiently bad shock leads to a reduction in  $C$ , especially for poor households with few assets. This motivates our focus not only on mean consumption but also on its distribution.

**Transmission mechanisms and gendered predictions.** The framework yields different predictions depending on household geography and gender, which we test empirically in Sections ?? and ??.

*Rural households.* For households engaged in agriculture, an adverse  $\theta$  constitutes a simultaneous shock to home production and to local labour demand. The value of own-produced food falls, reducing the return to time in home production  $n^g$ , while lower agricultural wages reduce the return to market work  $h^g$ . This double-exposure motivates **Prediction 1**: rural households will experience larger consumption losses than urban households following an adverse climate shock.

*Urban households.* For urban households, the primary channel is an income and price shock transmitted through food markets and local informal labour demand, rather than a direct productivity shock. The effect on consumption is therefore expected to be smaller in magnitude. This motivates **Prediction 2**: the consumption-smoothing margin will be more

binding in rural than urban areas.

*Gendered labour market adjustments.* Men and women face different wage schedules and sectoral opportunities,  $w_m(\theta) \neq w_f(\theta)$ , and women typically bear a larger share of childcare and domestic work. Following an adverse shock, men are more likely to expand total labour supply into informal or casual jobs to replace lost agricultural income. Women face tighter constraints: care responsibilities limit sectoral mobility, and female-intensive sectors (services, petty trade) may contract alongside agriculture. Women are therefore more likely to shift into self-employment and home-production activities, or to exit the labour force entirely when demand contracts. This motivates **Prediction 3**: climate shocks will produce larger reductions in formal and informal wage employment for women than for men, with women reallocating relatively more towards self-employment and home production.

*Social protection.* Social protection enters through  $B$ : more generous or shock-responsive transfers relax the budget constraint, dampen the fall in  $C$ , and reduce the need for sharp labour adjustments. When coverage or benefit levels are low, insurance remains partial. This motivates **Prediction 4**: access to social protection will mitigate the consumption and labour market effects of climate shocks, with larger protective effects in rural areas where exposure is greatest. Note, however, that programme beneficiaries are unlikely to be randomly selected (??), so the estimated effects of  $B$  should be interpreted as associations rather than causal impacts.

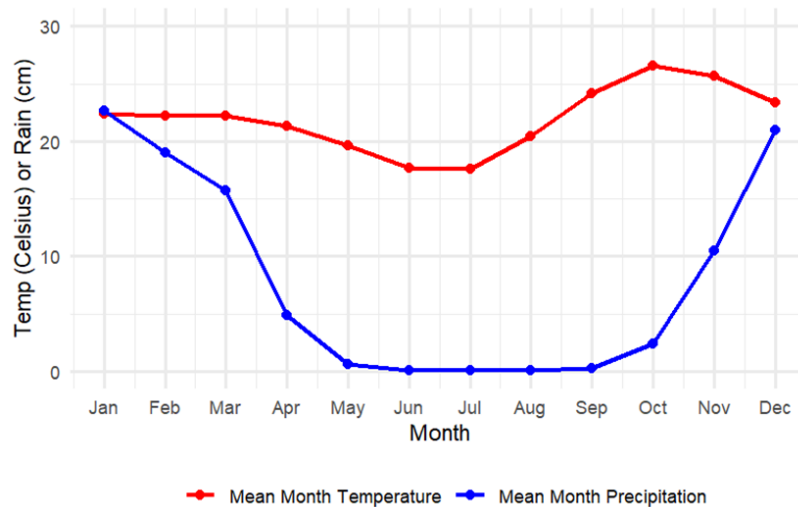
**From framework to empirical strategy.** This framework directly motivates the empirical approach in later sections. We use the LCMS to estimate reduced-form effects of district-level climate shocks  $\theta_{dt}$  on food and total consumption, testing Predictions 1 and 2 by comparing rural and urban households. We then use the LFS to examine how men and women adjust labour-force participation, employment status, and sector of work following shocks (Prediction 3), and whether access to social protection  $B$  moderates these adjustments (Prediction 4). Throughout, we interpret labour market changes as an adjustment margin that households use — alongside assets, transfers, and home production — to cope with income fluctuations caused by climate shocks.

### 4.3 Zambia profile and climate vulnerability

Zambia is a large, landlocked country located in south-central Africa. It is bordered by Angola, Botswana, the Democratic Republic of Congo, Malawi, Mozambique, Namibia, Tanzania, and Zimbabwe. Zambia has a tropical climate with three marked seasons: cool and dry (May to August), hot and dry (September to November), and warm and wet (December to April). Figure 4.1 shows these patterns of temperature and precipitation. In the last decade, the country has experienced substantial economic growth as Africa's second-largest copper producer. Other main exports include cobalt, cotton, coffee, flowers, tobacco, and

maize. Despite the economic growth and partially due to the vulnerability of falling commodity prices, the country still has almost two-thirds of the population living in poverty conditions (see ZAMSTAT, 2022). Due to the low income level, low human capital development, and high economic vulnerability, Zambia has been classified as one of the world's 45 least developed countries (United Nations, nd). Moreover, Zambia encounters many environmental challenges, including loss of biodiversity, deforestation, air pollution, droughts, floods, and extreme temperatures.

**FIGURE 4.1 – AVERAGE RAINFALL AND TEMPERATURE PER MONTH**



Notes: The figure displays the average rainfall (cm) and temperature (Celsius) for each month in Zambia between 1980 and 2022. Averages were calculated considering equally weighted districts.

Using data from Zambia's 2023 In-Depth Vulnerability Survey, with interviews in June and July 2023, we present contextual information on climate vulnerability in Table D1. This survey is conducted by the DMMU in Zambia. The survey targets low-income families living in areas affected and at risk of being affected by climate shocks. Estimates from the survey<sup>5</sup> indicate that, in 2023, around 33% of individuals were in households impacted by drought since October 2022, and 30% were in households impacted by flood. We look at how being impacted by one of these shocks is related to indicators of hunger and coping strategies asked in the survey. Comparisons are made with simple OLS regressions of indicators on drought and flood dummies using household sample weights.

From Table D1, Panel A shows that 45% of people were in households that lacked food in the past 30 days. Additionally, drought or floods increased the lack of food by 6.5 and 5.9 percentage points (p.p.), respectively. Individuals who were impacted by these disasters were also more likely to be in households where a member slept hungry or went a full day without eating compared to those who were not impacted. These differences are especially

<sup>5</sup>The estimates are intended to capture correlations only. We estimate a linear regression such as  $Y_i = \beta_0 + \beta_1 Shock + \varepsilon$ . Each  $Y_i$  is displayed in the table rows, while the shocks are a self-reported indication of being affected by a drought or flood.

pronounced for households that experienced drought, with an 11 p.p. higher rate than the baseline of having a household member go a full day without eating.

Panel B from Table D1 shows the relationship between natural disasters and households' reduced coping strategies, illustrating how households adjust. Droughts and floods are both associated with higher rates of adjustment, such as consuming less expensive food, having limited portions at mealtime, and restricting adult consumption to favour children. Interestingly, individuals who faced drought did not present a higher rate of resorting to borrowing food than the baseline, while households that faced floods had a 9 p.p. higher rate.

Panel C of Table D1 shows that individuals in households impacted by floods had higher rates than baseline for borrowing money for food and health needs, selling oxen or mature female livestock, and withdrawing children from school. Both drought and flood are associated with higher rates of begging.

## 4.4 Data

### 4.4.1 Living Conditions Measurement Survey

Zambia's LCMS is a nationwide survey conducted by the country's Central Statistical Office with the main purpose of measuring the well-being of the population. Among other information, the survey measures the household consumption of a variety of food and non-food items. We use two waves of the survey in our analysis: (1) the 2015 LCMS, which conducted interviews between April and May 2015, and (2) the 2022 LCMS, which had interviews from June 2022 to July 2022. We are able to observe each household's district and the date of the interview.

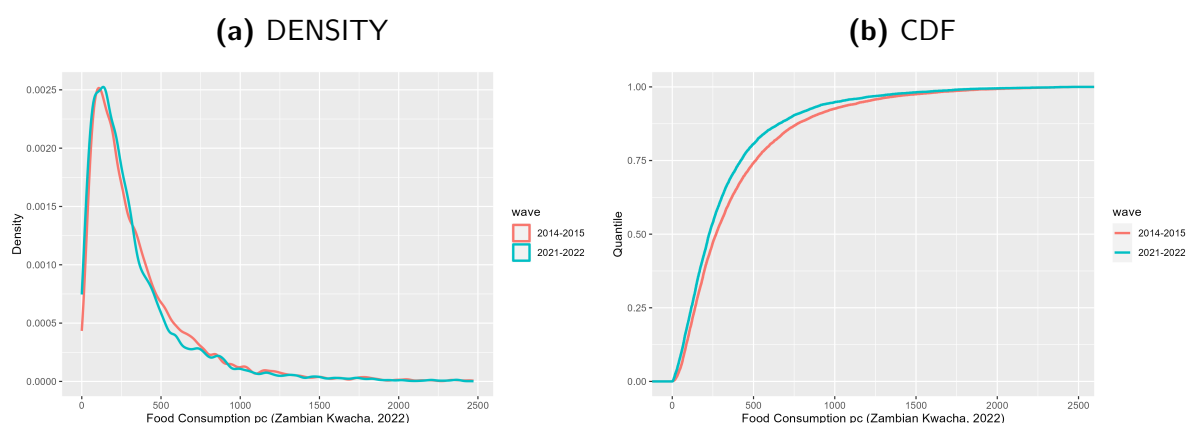
The main variable of interest in our analysis is the household's value of food consumption per capita over a one-month period. The consumption measure includes expenditure on products bought and estimates for self-produced goods and gifts. The original data considered different time spans of household consumption for different goods (7 days, 14 days, and 30 days). We adjusted all consumption to the monthly equivalent and divided by the number of people in the household to obtain the monthly consumption value per capita. All values are adjusted to 2022 prices. Table 4.1 summarizes the main variables used in this study.

Figure 4.2 shows the density and cumulative distribution functions of monthly household food consumption value per capita in our sample for 2015 and 2022. Considering information from Zambia's 2022 Poverty Assessment, the total food basket for a family of six costs ZMW1,522, translating to ZMW254 per capita (ZAMSTAT, 2023). Based on the empirical distribution of food consumption per capita, around half the households fall below the basic food basket value per capita.

**TABLE 4.1 – LIVING CONDITIONS MEASUREMENT SURVEY: DESCRIPTIVE STATISTICS**

<i>Panel A. Household head characteristics</i>	
Household head age	43.71 (0.2981)
Female household head	0.2643 (0.0068)
Household head never attended school	0.1022 (0.0112)
Household head highest education grade	7.788 (0.2915)
<i>Panel B. Household characteristics</i>	
Total household wages per capita (2022, ZMW)	479.1 (105.7)
Food consumption per capita (2022, ZMW)	358.3 (24.92)
Number of people in household	5.115 (0.0441)
Number of children under six years	0.5633 (0.0264)
<i>Panel C. Participation in social programmes (only 2022)</i>	
Keeping Girls in School	0.0126 (0.0022)
School Feeding Program	0.0346 (0.0100)
Social Cash Transfer	0.0715 (0.0109)

Notes: This table displays the descriptive statistics of the main variables used in this study using the LCMS 2015 and 2022. The total number of household-level observations is 19,788.

**FIGURE 4.2 – DISTRIBUTION OF FOOD CONSUMPTION VALUE PER CAPITA**

Notes: This figure presents the distribution (a) of food consumption per capita by LCMS survey year, and the cumulative distribution of consumption per capita (b) by LCMS survey year.

#### 4.4.2 Labour Force Survey (LFS)

The LFS is a nationally representative household-level sample survey designed to collect information on labour market activities.<sup>6</sup> From the survey, we also observed the district where the household is located and the quarter of the year the survey interview took place. The quarters in our sample are divided according to the order of the month in a year: January to March (Q1), April to June (Q2), July to September (Q3), and October to December (Q4). The LFS comprises a series of cross-sectional studies over time; for this analysis we use the 2017, 2018, 2019, 2020, and 2021 surveys.<sup>7</sup>

For each survey year, the final sample size was calculated in such a way as to have appropriate power to produce estimates for the country, area of residence (urban and rural), and each province.<sup>8</sup> In order to draw sampling units, a two-stage cluster sampling technique was involved. The primary sampling units are standard enumeration areas (SEAs), identified from an original sampling frame compiled from the 2010 Census of Population and Census.

Table 4.2 summarizes the main variables used in this study.<sup>9</sup> The outcomes of interest for our analysis are related to the individuals' labour characteristics. This concept includes variables such as labour force participation (employed or actively seeking a job), employment type (formal sector, informal sector, or self-employed), industry type, and hours worked in the main activity. Table D3 in Appendix shows the sample size for the analytical sample by survey years, and Table D4 shows the distribution by household location at the province level.

#### 4.4.3 Climate Data and Construction of Climate Shock Variables

This study uses historical data on temperature (degrees Celsius) and precipitation (centimetres) for the climate-related variables from 2014 to 2022, enveloping the interview periods of all surveys. The temperature data comes from the Modern-Era Retrospective Analysis from Research and Applications (MERRA-2) provided by NASA, offering daily mean temperatures measured at a 2-metre height.<sup>10</sup> The precipitation data comes from the Global Precipitation Climatology Centre (GPCC) of the National Oceanic and Atmospheric Administration (NOAA), offering monthly cumulative data based on global station data.<sup>11</sup> In both cases, the data was aggregated at the district level and later integrated into the individual-level data.

We are specifically interested in climate shocks during our period of analysis, i.e., events in which the temperature or precipitation diverge from the usual for the country. To construct

<sup>6</sup>The LFS is conducted by the Zambia Statistics Agency in partnership with the Ministry of Labour and Social Security.

<sup>7</sup>The 2018 survey year information only comprises data on one-quarter of interviews for 16.9% of the individuals.

<sup>8</sup>Zambia has a total of nine provinces: Central, Copperbelt, Eastern, Luapula, Lusaka, Northern, North-Western, Southern, and Western.

<sup>9</sup>Appendix Table D2 reports the main LFS variables of interest and their definitions.

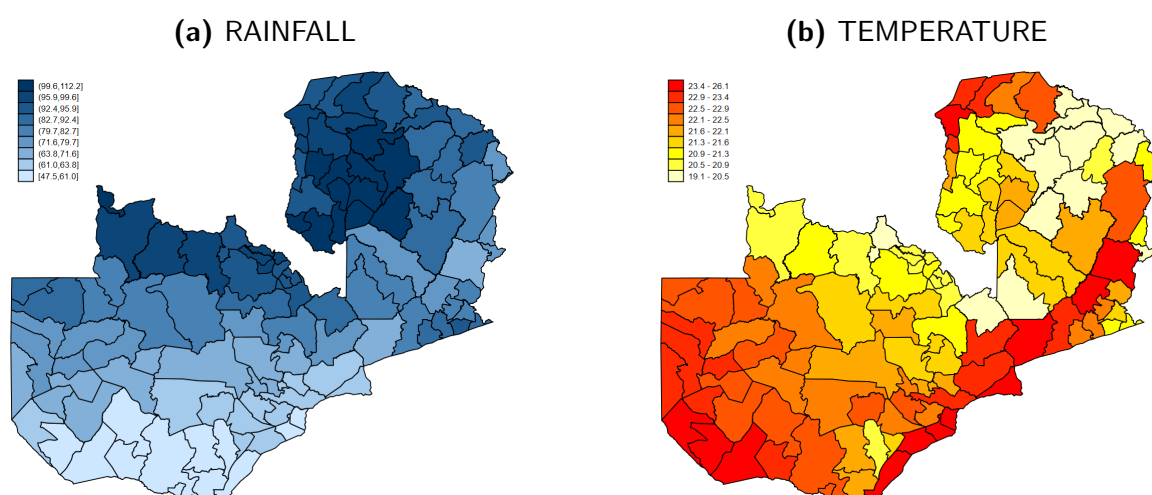
<sup>10</sup>The data was represented in grids with a spatial resolution of  $0.5 \times 0.625$  degrees.

<sup>11</sup>Data was represented in grids of  $1 \times 1$  degree resolution.

**TABLE 4.2 – LABOUR FORCE SURVEY: DESCRIPTIVE STATISTICS**

	Total	Male	Female
<i>Panel A. Individual characteristics</i>			
Age	22.33 (18.26)	22.51 (18.27)	22.16 (18.25)
Lives in urban (=1)	0.35 (0.48)	0.35 (0.48)	0.36 (0.48)
Education (in years)	6.52 (3.84)	6.84 (3.93)	6.19 (3.71)
Household size	6.43 (3.56)	6.56 (3.57)	6.47 (3.47)
<i>Panel B. Employment characteristics</i>			
In labour force (=1)	0.35 (0.48)	0.44 (0.50)	0.26 (0.44)
Employed at the formal sector (=1)	0.24 (0.43)	0.31 (0.46)	0.16 (0.37)
Employed at the informal sector (=1)	0.30 (0.46)	0.34 (0.47)	0.25 (0.43)
Self-employed/ HH consumption (=1)	0.46 (0.50)	0.35 (0.48)	0.59 (0.49)
Industry: Elementary services (=1)	0.40 (0.49)	0.33 (0.47)	0.53 (0.50)
Industry: Agriculture (=1)	0.29 (0.45)	0.30 (0.46)	0.26 (0.44)
Industry: Manufacture (=1)	0.13 (0.33)	0.16 (0.37)	0.06 (0.24)
Hours worked in the last week in main activity	11.51 (20.93)	15.84 (23.60)	7.92 (17.54)
Hours worked in the last week in secondary activity	0.27 (3.14)	0.40 (3.81)	0.16 (2.42)

*Notes:* This table displays descriptive statistics for the main variables used in this study using the LFS 2017, 2019, 2020, and 2021. The 2018 survey is only partially available and is used in the regressions but excluded from this descriptive table. The sample includes  $N = 92,579$  individuals in total, of whom  $N_F = 35,728$  are women and  $N_M = 35,742$  are men. These counts are based on the estimation sample, i.e., individuals with non-missing values for the full set of variables used in the analysis.

**FIGURE 4.3 – RAINFALL AND TEMPERATURE IN ZAMBIA BY DISTRICT**

*Notes:* The figure displays the average rainfall (mm) and temperature (Celsius) in Zambia between 2014 and 2020, by district.

our climate shocks variables we gather district-month average temperature and precipitation at the district level from 2014 to 2022, and obtain the threshold values of the 10th and

90th percentiles of the empirical distribution. Climate shocks are then defined as cases in which the average temperature or precipitation in that district-month was above (under) the 90th (10th) percentile of that measure's distribution. We consider the distinct effects of each weather extreme separately, but focus on three measures: high temperature, low precipitation, and high precipitation. The choice of these measures was because the remaining extreme categories occur too infrequently in our sample to support reliable estimation.

Due to the different levels of information on interview timing between the LFS and LCMS datasets, we work with both quarter and month level measures of climate shocks. In the LFS, the information on interview timing is only available at the quarter-year level, which is the main time-window for weather variables used in our analysis. However, in the LCMS we have information on the month-year of interview, which also let us consider shorter-term month weather shocks. To obtain quarter-year level variables for extreme weather events, we calculate the number of months the average temperature and precipitation were in the extremes of their distributions (below 10th and above 90th percentiles), and classify the quarter as having a high temperature or high/low precipitation when at least one month was a tail-of-the-distribution occurrence in that measure. Quarter-year climate shocks (and month-level shocks when possible) are matched to individual's interviews according to district and timing of interview.

## 4.5 Methodology

This paper aims to quantify how climate shocks affect household welfare and individual labour-market outcomes in Zambia. We exploit two complementary data sources: the 2015 and 2022 LCMS for welfare measures, and the 2017–2021 LFS for labour outcomes. In both cases, the identification strategy relies on within-district variation over time in extreme temperature and precipitation, controlling flexibly for district and time fixed effects. The construction of the climate shock variables is described in the Data Section.

To study welfare impacts, measured by household food expenditures and food consumption value, we estimate the short- and medium-run effects of climate shocks on average and across the consumption distribution. Short-run shocks are defined using tail events in accumulated precipitation and mean temperature over the previous month, while medium-run shocks are constructed analogously over the previous quarter.

$$Y_{idt} = \beta_T \text{Temp}_{dt} + \beta_P \text{Prpc}_{dt} + \theta_X X_{idt} + \mu_d + \phi_y + \varepsilon_{idt}, \quad (4.4)$$

where  $Y_{idt}$  is a measure of household food expenditures or food consumption value per capita for household  $i$  in district  $d$  interviewed at time  $t$ . For average effects,  $Y_{idt}$  is the log of per capita food expenditures or the log of per capita food consumption value, depending on the outcome. For distributional effects,  $Y_{idt}$  is the re-centred influence function of log

food consumption at a given decile, following [Firpo et al. \(2009\)](#). The variables  $\text{Temp}_{dt}$  and  $\text{Prcp}_{dt}$  are indicators for extreme high temperature (above the 90th percentile) and extreme low/high precipitation (below the 10th percentile and above the 90th percentile), with non-extreme weather as the omitted category.<sup>12</sup> Past-month and past-quarter shocks are estimated in separate regressions to avoid mechanical overlap. The vector  $X_{idt}$  includes demographic and socioeconomic characteristics of the household head (age, age squared, an indicator for female headship, an indicator for never having attended school, and the highest grade completed), as well as an indicator for never attending school. All regressions include district fixed effects  $\mu_d$  and year fixed effects  $\phi_y$ . Additional specifications that examine the mitigating role of social protection add controls for benefit receipt and interactions between benefit receipt and climate shocks. Under this setup, the coefficients  $\beta_T$  and  $\beta_P$  capture the reduced-form effects of extreme temperature and precipitation events on consumption and related welfare measures.

To study labour-market responses, we use the 2017–2021 LFS and analyse contemporaneous impacts of precipitation and temperature shocks on individual labour outcomes. We focus on labour force participation, employment type, hours worked, and sector of work as dependent variables, and estimate the fixed-effects model:

$$Y_{idt} = \beta_T \text{Temperature}_{d,t-1} + \beta_P \text{Precipitation}_{d,t-1} + X'_{idt} \beta_X + \delta_d + \theta_t + \varepsilon_{idt}, \quad (4.5)$$

where  $Y_{idt}$  is an individual labour outcome for person  $i$  in district  $d$  at year–quarter  $t$ , and the weather variables are indicators for at least one month of extreme temperature and high/low precipitation in the previous quarter.<sup>13</sup> The vector  $X_{idt}$  includes standard individual controls (gender, age, area of residence and education level), and all regressions include district fixed effects  $\delta_d$  and year–quarter fixed effects  $\theta_t$ , with standard errors clustered at the district level.<sup>14</sup>

Within the framework described in Section 4.2, the LCMS estimates provide evidence on how far households are from fully smoothing consumption in response to climate shocks, while the LFS estimates characterise how men and women adjust labour-force participation, employment type and sectoral allocation as one of the main margins through which households attempt to cope with these shocks. Interactions with social protection indicators allow us to assess whether access to transfers attenuates both consumption losses and the associated gendered labour reallocations.

<sup>12</sup>We do not include extreme low temperature because such events are very rare in our sample and provide insufficient variation for reliable estimation.

<sup>13</sup>We focus on extreme high temperature and do not include extreme low temperature because low-temperature extremes are rare over our study period and do not provide sufficient within-district variation.

<sup>14</sup>For migration outcomes, we estimate analogous linear probability models using yearly weather shocks (defined over the previous 12 months) as regressors and the same set of controls and district and year-quarter fixed effects.

## 4.6 Results

### 4.6.1 The effects on welfare

In this subsection, we study the impacts of climate shocks on household welfare measured by consumption reported in the LCMS. Consumption is a better measure of welfare in low-income countries as it is less volatile than income (Meyer and Sullivan, 2003). In a country highly dependent on agricultural production and limited insurance markets and mechanisation, climate shocks can generate substantial volatility in food prices. Shocks may raise food prices significantly, and even if families have mechanisms to cushion income losses, these may not be sufficient to stabilise consumption. Therefore, we turn to the analysis of the LCMS.

**TABLE 4.3 – IMPACT OF WEATHER SHOCKS ON HOUSEHOLD FOOD EXPENDITURES AND FOOD CONSUMPTION VALUE PER CAPITA (LCMS)**

	Log Food Expenditures / Food Consumption		Value per capita	
	Expenditures	Consumption	Expenditures	Consumption
	Shock in Past Quarter		Shock in Past Month	
	(1)	(2)	(3)	(4)
<i>Panel A. Baseline results</i>				
Temperature Above P90	-0.526 (0.567)	-0.358 (0.233)	-0.972* (0.509)	-0.391** (0.192)
Precipitation Above P90	0.204 (0.154)	-0.079 (0.098)	-0.232 (0.182)	-0.142 (0.158)
Precipitation Below P10	-0.118 (0.108)	-0.064 (0.083)	-0.167* (0.098)	-0.125** (0.055)
Observations	19,621	19,682	19,621	19,682
R-squared	0.414	0.289	0.417	0.290
<i>Panel B. Interaction with female head</i>				
Temperature Above P90	-0.438 (0.524)	-0.376 (0.228)	-0.889* (0.478)	-0.394* (0.200)
Precipitation Above P90	0.238 (0.148)	-0.081 (0.098)	-0.180 (0.187)	-0.108 (0.152)
Precipitation Below P10	-0.121 (0.115)	-0.057 (0.086)	-0.184* (0.097)	-0.115** (0.056)
Female Head × Temperature Above P90	-0.380** (0.190)	0.078 (0.115)	-0.352* (0.178)	0.009 (0.108)
Female Head × Precipitation Above P90	-0.158*** (0.055)	0.010 (0.040)	-0.228* (0.116)	-0.145 (0.159)
Female Head × Precipitation Below P10	0.001 (0.065)	-0.023 (0.069)	0.055 (0.050)	-0.029 (0.045)
Observations	19,621	19,682	19,621	19,682
R-squared	0.415	0.289	0.417	0.290

*Notes:* this table shows the results of experiencing an extreme weather event in the previous quarter or month according to Equation 4.4. The dependent variable is the logarithm of household food expenditures per capita (cash purchases) or household food consumption value per capita (which additionally includes the imputed value of home-produced and in-kind food consumption). All specifications control for household-head characteristics: age (and age squared), an indicator for female headship, an indicator for never having attended school, and the highest grade completed. Standard errors are clustered at the district level. All regressions include district and year fixed effects. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

Given the information on interview timing in the LCMS, Table 4.3 distinguishes shocks occurring in the past quarter (Columns 1–2) from more immediate shocks in the past month (Columns 3–4), and reports results for both food expenditures and total food consumption value per capita. Throughout, “food expenditures” capture cash purchases only, whereas “food consumption value” additionally includes the imputed value of home-produced and in-kind food consumption.

Panel A shows that short-run shocks are strongly associated with welfare losses. In particular, an extreme temperature shock in the past month is associated with sizeable declines in both cash food expenditures and total food consumption value. The point estimate for expenditures implies a very large reduction when experiencing extreme high temperatures but it is only significant at the 10% level, while the decline in total food consumption is more precisely estimated, showing a reduction of approximately 40% under the same shock. Similarly, extremely low precipitation in the past month is associated with reductions in both expenditures and total food consumption value per capita, although coefficients are significantly smaller than when temperature is extremely high. On the other hand, experiencing extreme precipitation does not display systematic effects on either outcome over these short horizons.

Panel B explores heterogeneity by the gender of the household head. The main coefficients capture the effects for male-headed households, while the interaction terms report the differential effect for female-headed households. Two patterns stand out. First, female-headed households experience additional reductions in food expenditures during extreme events, particularly for high temperatures and high precipitation. In contrast, the corresponding differential effects on the total value of food consumption are close to zero and not statistically significant. Quantitatively, the quarter-based interaction estimates imply additional expenditure declines of roughly 38% under extreme heat and about 16% under extreme high precipitation, with similar signs and magnitudes for the past-month interactions. These patterns suggest that women-headed households reduce market purchases more sharply during some shocks but might partially offset these declines through additional sources of food consumption, which may involve greater reliance on home production and in-kind consumption.

The results from Table 4.3 are consistent with suggestive evidence from the 2023 In-Depth Vulnerability and Needs Assessment (IVA). The reduction in the value of food consumption per capita in response to climate shocks is consistent with increases in hunger indicators and in measures of limited food expenditures and consumption among households impacted by droughts or floods/high rainfall.

Appendix Figure D2 summarises the distributional impacts of extreme precipitation shocks using unconditional quantile estimates based on Equation (4.4). The figure focuses on past-month rainfall extremes and reports effects separately for cash food expenditures (Panels (a) and (c)) and total food consumption value per capita (Panels (b) and (d)). Two patterns emerge. First, both unusually low rainfall and unusually high rainfall are associated with the largest welfare losses at the bottom of the distribution: estimated declines in expenditures and

consumption are most pronounced for poorer households and generally attenuate as one moves toward higher deciles. Second, the effects are close to zero and statistically weaker at the top of the distribution, indicating that better-off households are comparatively insulated from short-run precipitation shocks. Overall, the results suggest that past-month rainfall extremes are regressive, consistent with poorer households facing tighter liquidity and insurance constraints and therefore being less able to protect consumption when shocks occur.

## 4.6.2 The gendered effects on labour

Panel A of Table 4.4 shows that a past quarter with at least one month of extreme precipitation reduces the probability of being in the labour force by 3.3 p.p. Column (1) also shows that, using the entire sample, labour force participation increases by 3.6 p.p. when experiencing a past quarter with at least one month of extreme temperatures. Panel B indicates that these average effects mask important gender differences. The negative impact of extreme rainfall on labour force participation is concentrated among women, while the increase in labour supply associated with extreme temperatures is mainly driven by men. This is the same pattern observed for episodes of drought, as captured by past quarters with at least one month of precipitation below the 10th percentile.

Columns 2–5 provide additional evidence on the adjustment margins. The gender differences are substantial. In response to extreme weather, men tend to shift toward informal employment, while women disproportionately reallocate toward self-employment and household-production activities. For example, following quarters with unusually high precipitation (precipitation above the 90th percentile), the interaction estimates imply that women's probability of working for self-consumption/self-employment increases by 16.5 p.p. relative to men, while their probability of holding a formal job falls by 7.3 p.p. relative to men and their probability of informal wage work falls by 9.2 p.p. relative to men. Combining the main and interaction coefficients implies that women experience an approximately 11.0 p.p. increase in self-consumption/self-employment, alongside declines of roughly 5.4 p.p. in formal employment and 5.6 p.p. in informal employment. Across specifications, extreme shocks consistently reduce women's probability of holding a formal job, highlighting a vulnerability given that formal employment can buffer income losses during crises (Lastunen et al., 2021).

Table 4.5 shows that climate shocks also affect sectoral allocation. While Panel A does not show strong evidence of sectoral reallocation when pooling all individuals, Panel B again reveals important gendered effects. A past quarter with at least one month of an extreme climate shock increases the probability that men work in manufacturing and decreases their probability of working in the services sector, with no clear impacts on agriculture. In contrast, women experiencing climate shocks in the past quarter are more likely to work in services and less likely to work in manufacturing and agriculture.

Unfortunately, the lack of panel data does not allow us to fully explore the mechanisms that

**TABLE 4.4 – IMPACT OF WEATHER SHOCKS ON EMPLOYMENT OUTCOMES (LFS)**

	Labour force (1)	Hours worked (2)	Formal job (3)	Informal job (4)	Self-employed or HH consumption (5)
<i>Panel A. Baseline results</i>					
Precipitation below p10 (past quarter)	0.019 (0.013)	0.464 (0.491)	-0.023 (0.016)	0.054** (0.025)	-0.031 (0.024)
Precipitation above p90 (past quarter)	-0.033* (0.018)	-0.425 (0.584)	-0.015 (0.021)	-0.005 (0.019)	0.020 (0.021)
Temperature above p90 (past quarter)	0.036* (0.021)	1.199* (0.636)	-0.018 (0.023)	0.042 (0.029)	-0.024 (0.031)
Observations	64,530	81,341	30,217	30,217	30,217
R-squared	0.190	0.181	0.275	0.189	0.340
<i>Panel B. Interaction with female</i>					
Precipitation below p10 (past quarter)	0.064*** (0.020)	0.470 (0.505)	-0.013 (0.017)	0.061** (0.026)	-0.048* (0.029)
Precipitation above p90 (past quarter)	0.017 (0.018)	-0.465 (0.590)	0.019 (0.022)	0.036* (0.021)	-0.055*** (0.020)
Temperature above p90 (past quarter)	0.090*** (0.022)	1.283** (0.636)	0.026 (0.025)	0.031 (0.030)	-0.057 (0.035)
Female × precipitation below p10	-0.095*** (0.025)	-3.718** (1.677)	-0.027 (0.028)	-0.017 (0.016)	0.044 (0.040)
Female × precipitation above p90	-0.102*** (0.013)	-8.676*** (1.833)	-0.073*** (0.019)	-0.092*** (0.013)	0.165*** (0.018)
Female × temperature above p90	-0.109*** (0.017)	-5.617*** (1.905)	-0.099*** (0.023)	0.034*** (0.012)	0.065** (0.031)
Observations	64,530	64,530	30,217	30,217	30,217
R-squared	0.186	0.181	0.274	0.188	0.333
Year-quarter FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes

*Notes:* This table shows the results of the impact of experiencing at least one month of an extreme weather event in the past quarter on the main employment outcomes according to Equation 4.5. Column (1) is an indicator of being in the labour force. Column (2) shows the number of hours in the main job. Column (3) is an indicator of whether the individual has a formal job contract. Column (4) is an indicator of whether the individual does not have a formal job contract. Column (5) is an indicator of whether the individual works for self-consumption only. Controls include gender, age group, education, rural residence. Standard errors are clustered at the district level. All regressions include district and year-quarter fixed effects. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

may explain these patterns. Our contribution here is to document these gender-differentiated responses and highlight the need for future research to understand why men's and women's labour supply and sectoral allocation respond so differently to climate shocks in Zambia.

Taken together, these results reinforce the framework's prediction that, relative to men, women respond to climate shocks by reallocating time away from formal and informal wage work and into self-employment and home production that helps stabilise basic consumption but may worsen long-run earnings prospects.

### 4.6.3 Do social protection policies cushion welfare losses?

Social protection programs that are not specifically focused on extreme weather events can also provide insurance against climate disasters (Deryugina, 2017). As these shocks negatively

**TABLE 4.5 – IMPACT OF WEATHER SHOCKS ON EMPLOYMENT SECTOR (LFS)**

	Manufacturing (1)	Agriculture (2)	Services (3)
<i>Panel A: Baseline results</i>			
Precipitation below p10 (past quarter)	0.010 (0.010)	0.014 (0.020)	-0.021 (0.016)
Precipitation above p90 (past quarter)	-0.015 (0.014)	-0.006 (0.022)	0.009 (0.020)
Temperature above p90 (past quarter)	-0.019 (0.016)	0.021 (0.029)	0.021 (0.025)
Observations	24,972	24,972	24,972
R-squared	0.055	0.303	0.177
<i>Panel B: Interaction with female</i>			
Precipitation below p10 (past quarter)	0.036** (0.017)	0.023 (0.020)	-0.054*** (0.019)
Precipitation above p90 (past quarter)	0.021 (0.015)	0.004 (0.021)	-0.050** (0.019)
Temperature above p90 (past quarter)	0.010 (0.021)	0.038 (0.029)	-0.035 (0.027)
Individual is female × precipitation below p10	-0.074** (0.029)	-0.026* (0.014)	0.094*** (0.022)
Individual is female × precipitation above p90	-0.091*** (0.018)	-0.028** (0.014)	0.151*** (0.020)
Individual is female × temperature above p90	-0.075*** (0.023)	-0.047*** (0.014)	0.146*** (0.018)
Observations	24,972	24,972	24,972
R-squared	0.048	0.303	0.168
Year-quarter FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes

*Notes:* This table shows the results of the impact of experiencing at least one month of an extreme weather event in the past quarter on the probability of working in manufacturing (column 1), agriculture (column 2), or services (column 3) according to Equation 4.5. Controls include gender, age group, education, rural residence. Standard errors are clustered at the district level. All regressions include district and year-quarter fixed effects. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

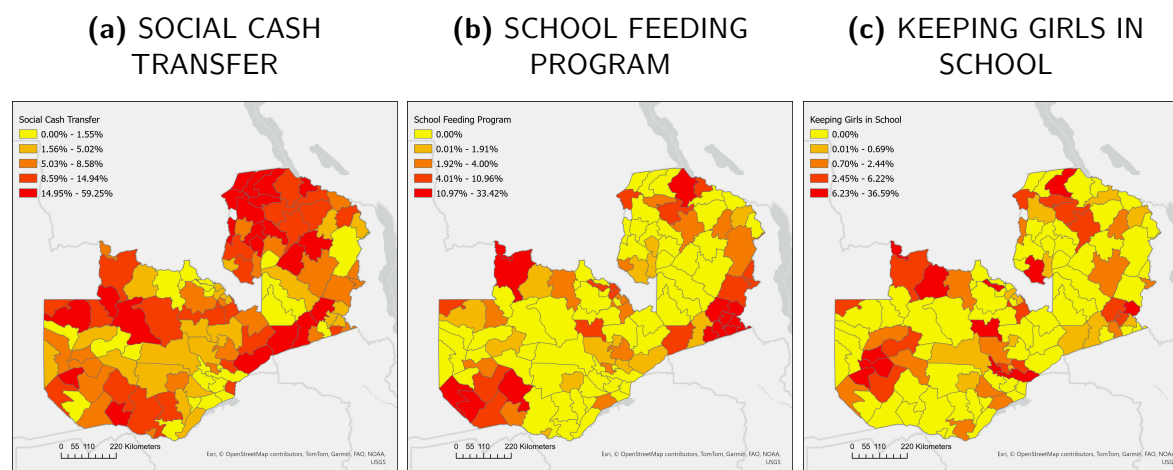
impact local economies, more households might become eligible for social protection programs. Furthermore, the occurrence of natural disasters can increase the salience of socioeconomic conditions in impacted areas, and in the presence of limited state capacity, policymakers might relocate funds to these regions. However, this is an ideal scenario that requires sufficient fiscal space to expand coverage, which is not the case in most low-income countries, including Zambia (Banda et al., 2024; Hirvonen et al., 2024).

In Figure 4.4, we document the coverage of three government programs: (i) Social Cash Transfer, (ii) School Feeding Program, and (iii) Keeping Girls in School. In 2022, only the Social Cash Transfer programme covered all districts of Zambia, while the School Feeding Program was present in 39 districts. For a household to receive Social Cash Transfer, it needs to satisfy three criteria: residency at the locality for six months or more, meeting incapacitation and destitution conditions, and passing a formal welfare assessment.<sup>15</sup> The

<sup>15</sup>To meet incapacitation and destitution status, the household needs to satisfy one of the following conditions: (i) female-headed household with three children or more; (ii) household headed by a child aged 18 years or below; (iii) household with a person who is chronically ill on palliative care or with severe disability (with certification from a government health facility); (iv) household with an elderly member aged 65 years

selection of districts to be covered by the School Feeding Programs is driven by a need-based set of criteria: high dropout, low net enrolment and attendance rates, high rates of underweight, and extreme poverty.

**FIGURE 4.4 – COVERAGE OF SOCIAL PROTECTION PROGRAMMES, LCMS 2022**



Notes: This figure shows the coverage of social protection programmes per district in Zambia. Coverage is the share of households receiving the social protection programme divided by the total number of households.

In addition, Appendix Tables D5 and D6 show some distributional statistics of social protection in Zambia using the LFS. Around 7% of the rural population and 2% of the urban population report receiving some kind of social protection programme. Coverage is low overall and remains modest even among poor and female-headed households, who are more exposed to climate risk. Rural areas exhibit somewhat higher coverage than urban areas, reflecting the rural focus of the Fertiliser Input Support Programme and Social Cash Transfer, but a large share of poor households, especially those headed by women, still receive no programme at all. These patterns reinforce that existing schemes are not designed or scaled to systematically insure the most vulnerable against climate shocks.

Using data from the 2022 Living Conditions Measurement Survey (LCMS), which includes household participation in government programmes, we provide suggestive evidence on whether exposure to extreme weather shocks differs for households receiving the Social Cash Transfer. Because detailed information on social protection receipt is only available in the 2022 wave and interviews were conducted over a short fieldwork window (two months), there is limited identifying variation in both programme coverage and local weather conditions. Appendix Figure D3 shows no clear geographical correlation between district-level Social Cash Transfer coverage and rainfall patterns in the months preceding the interview.

Table D7 presents our estimates on how government programs, particularly Social Cash Transfer, interact with weather shocks and affect households' food expenditures and consumption. Due to the limited variation in weather shocks during this short period, it is only or above (with proper documentation).

possible to estimate the impacts of high and low rainfall in the past quarter and low precipitation in the past month. Our estimates provide suggestive evidence that social cash transfers can help cushion the economic conditions of households impacted by high precipitation in the quarter prior to the interview, although the precision of the estimates is limited by the short observation window.

Finally, we complement the LCMS evidence with income data from the LFS. Appendix Table D8 estimates the effects of extreme rainfall and temperature in the past quarter on household income per capita for poor households, and how these effects vary with receipt of any social benefit, the Social Cash Transfer, or the input fertiliser programme. A quarter with at least one month of very high rainfall reduces income per capita by about 10 per cent, but this loss is substantially attenuated – and in some cases almost fully offset – for beneficiary households, as indicated by the positive and statistically significant interaction terms. This pattern is similar for female-headed households and is consistent with our consumption results, suggesting that existing social protection provides at least partial insurance against income losses induced by heavy-rainfall shocks.

#### 4.6.4 Migration Responses to Climate Shocks

Finally, we examine whether climate shocks affect migration decisions using the LFS question on whether an individual migrated in the past 12 months. We consider two outcomes: (i) any migration, and (ii) migration reported as work-related. Table D9 presents the results using weather shocks over the previous year, defined as precipitation below the 10th percentile, precipitation above the 90th percentile, and temperature above the 90th percentile of the national distribution. Overall, migration responses to climate shocks are limited in magnitude. The only systematic average effect is for extremely high rainfall: a year with precipitation above the 90th percentile increases the probability of migrating by about 0.6 percentage points (Column 1). In contrast, extreme low precipitation and extreme high temperature are not associated with statistically significant changes in overall migration. For work-related migration (Column 2), extreme high rainfall is associated with a small increase in the probability of reporting migration due to labour of around 0.2 percentage points, while the average effect of extreme high temperature is close to zero.

When disaggregating by gender (Panel B), we find that women are less likely than men to report labour-related migration in response to extreme weather, with negative and statistically significant interaction coefficients for both high precipitation and high temperature. Taken together, these results suggest that while migration responses to climate shocks are rare, there is some indication that extreme rainfall in particular is associated with slightly higher migration, and that women are less likely to rely on labour-related migration as a coping mechanism. This reinforces the broader pattern in the paper that adjustment occurs primarily through changes in labour supply and sectoral allocation rather than through large-scale geographic

mobility.

## 4.7 Conclusion

This paper examines the impact of exogenous climate shocks on household welfare and gendered labour-market responses in Zambia, a low-income country facing increasing climate variability and frequent extreme weather events. Exploiting within-district variation in extreme temperature and precipitation, and combining nationally representative labour and living conditions surveys, we show that climate shocks have sizeable and unequal effects on both consumption and labour outcomes.

On the welfare side, the LCMS results indicate that short-run shocks, particularly extreme heat and very low precipitation, lead to large declines in per capita food consumption value, with losses concentrated among poorer households. These consumption losses are consistent with evidence from the 2023 In-Depth Vulnerability and Needs Assessment, which documents higher hunger and negative coping strategies among households exposed to droughts and floods. When we distinguish by the gender of the household head, we find that weather shocks widen gender gaps in cash food expenditures: female-headed households cut food spending more than male-headed households, especially during extreme heat and unusually wet periods. By contrast, we do not detect statistically significant differential effects by headship on total food consumption value, suggesting that women-led households partially offset reduced purchases through home production and in-kind sources of consumption. Consistent with the conceptual framework, female-headed households appear to use home production more intensively as a consumption-smoothing margin.

Turning to labour-market adjustments, the LFS results show that quarters with unusually high precipitation reduce overall labour-force participation, while extreme heat raises participation on average. These aggregate responses mask gender differences. Men are more likely to remain in the labour force under drought-like conditions and high temperatures, largely by shifting into informal work. Women, by contrast, are less likely to participate in the labour force across dry, wet, and heat extremes, and among those who remain active, the probability of self-employment or work for home consumption rises while formal and informal wage employment decline. Sectoral results indicate that, during climate shocks, women are more likely to work in services and less likely to work in manufacturing and agriculture, whereas men are more likely to shift toward manufacturing. Taken together with the welfare results, these patterns are consistent with the model's prediction that women reallocate time away from market work and into home production and subsistence activities that help stabilise basic consumption, but at the cost of detachment from formal and informal wage jobs.

We also provide suggestive evidence on the role of social protection. We find indications that Social Cash Transfers can partially cushion weather-related income losses and, more tentatively, food expenditure declines among poorer households. However, coverage and

benefit levels appear low relative to the scale of losses, implying that existing schemes offer only partial insurance. Migration responses, in contrast, are limited: exposure to extreme weather in the previous year is associated with at most small changes in migration, and women are less likely than men to report labour-related migration in response to shocks. This suggests that, in the Zambian context, labour reallocation across sectors and into home production, rather than geographic mobility, is the main adjustment margin.

Overall, the findings highlight the need for climate and social protection policies that explicitly account for gendered constraints in labour and time use. Strengthening the generosity and coverage of cash transfer schemes, improving women's access to productive assets and off-farm opportunities, and reducing barriers to formal employment could enhance households' ability to smooth consumption without pushing women further into low-paying self-employment and unpaid home production. More broadly, the analysis underscores the importance of investing in high-quality microdata in low-income settings: the lack of panel data and consistent health and anthropometric measures limits our ability to trace the full dynamic and intergenerational consequences of climate shocks. Despite these limitations, the paper provides some of the first nationally representative evidence on how climate shocks affect both welfare and gendered labour-market outcomes in Zambia, and points to concrete margins where policy can strengthen resilience to a changing climate.

# Chapter 5

## Conclusion

This thesis has examined how individuals in low- and middle-income countries navigate large structural disruptions, and how institutions can either amplify or mitigate their effects. Across three empirical studies set in Peru and Zambia, I showed that civil conflict, reproductive health policy, and climate shocks shape human capital, fertility, and labour outcomes through gendered constraints and the design of services.

The second chapter, *The long shadow of conflict on human capital: Intergenerational evidence from Peru*, documents that the legacy of civil conflict extends to children who were never directly exposed. Mothers who experienced more intense conflict violence in childhood and adolescence have children with lower socio-emotional skills in middle childhood and early adolescence. The evidence points to reduced parental investments and constrained maternal empowerment as key channels, highlighting how conflict scars operate through the next generation of parents.

The third chapter, *Impact of Adolescent-Friendly Family Planning Services on Teenage Fertility in Peru*, studies a nationwide reform that guaranteed confidential contraceptive access for minors and designated adolescent-friendly health centres. Combining administrative birth records and DHS data with variation in proximity to these centres, I show that access to adolescent-friendly services substantially reduced teenage fertility, delayed cohabitation and marriage, and lowered school interruptions due to pregnancy, childcare, or unions. Mechanism evidence suggests increased use of modern methods, especially condoms, and a shift away from relying on family and peers toward more formal information channels. Yet gains were concentrated among relatively advantaged adolescents, and the temporary suspension of services during COVID-19 erased impacts before they resumed, underscoring both inequities and the importance of continuity.

The fourth chapter, *The gendered effects of climate shocks on labour and welfare in Zambia*, turns to extreme weather shocks in a highly climate-vulnerable setting. Merging labour and living conditions surveys with high-resolution rainfall and temperature data, I find that extreme precipitation and heat reshape labour supply, sectoral allocation, and household welfare. Women are more likely to exit the labour force or move into self-employment following

shocks, with corresponding declines in wage work. At the household level, extreme rainfall and very low precipitation reduce income and per-capita food consumption, especially among poorer households. Social cash transfers cushion some income losses but do not fully stabilise consumption, plausibly because climate shocks also raise food prices.

Taken as a whole, the chapters show that shocks at different points in the life course—a mother's exposure to civil war, adolescents' access to reproductive health services, and adults' exposure to climate extremes—are linked by a common production process of capabilities. Early life and adolescence emerge as sensitive periods: conflict exposure in these windows depresses the next generation's socio-emotional skills; adolescent-friendly family planning can shift fertility, union, and schooling trajectories at scale; and climate shocks to adults' work and income likely spill over to children's time use and nutrition. Across settings, who holds power and information, especially women's agency over fertility, work, and resources, critically shapes how shocks translate into long-run outcomes.

A central message of this thesis is that institutional design matters as much as the presence of resources. The conflict chapter emphasises that rebuilding human capital after violence requires strengthening caregivers' economic security and decision-making power, not just restoring schooling. The family planning chapter shows that removing age-based barriers and designing services around confidentiality and judgement-free counselling can generate sizeable behavioural change, but that coverage, quality, and resilience are essential to avoid uneven gains and backsliding. The climate chapter suggests that social protection can buffer income but must be complemented by measures that stabilise prices and expand climate-resilient opportunities, particularly for women.

Overall, the evidence in this thesis emphasises that the long-run consequences of conflict, reproductive health reforms, and climate shocks depend on the timing of exposure over the life cycle and on gendered access to resources and decision-making. Early life and adolescence reappear as recurrent margins where shocks and policies have particularly persistent effects on skills, fertility, schooling, and work. By quantifying these links in two different country contexts and highlighting the channels through which they operate, the chapters contribute to a more precise understanding of how human capital is formed and eroded under adversity, and point to concrete margins for future empirical and policy work.

# Appendix A

## Joint Authorship

### A.0.1 Joint Authorship Statement for Chapter 2

We certify that Alessandra Hidalgo Arestegui was involved in the conception, design, and procurement of data for the work in Chapter 2 titled: “The long shadow of conflict on human capital: Intergenerational evidence from Peru”. All data cleaning and data analysis were carried out by Alessandra, and she was the primary contributor to the empirical design and drafting of the chapter. The final version of the chapter has been published in *Journal of Development Economics* (JDE), Volume 174, 2025.

Catherine Porter

Alan Sanchez

Saurabh Singhal

## **A.0.2 Joint Authorship Statement for Chapter 4**

We certify that Alessandra Hidalgo Arestegui was involved in the conception, design, and procurement of data for the work in Chapter 4 titled: “The gendered effects of climate shocks on labour and welfare in Zambia”. Most of data cleaning and data analysis were carried out by Alessandra, and she was the primary contributor to the empirical design and drafting of the chapter.

Patricia Justino  
Gabriel Monteiro  
Rodrigo Oliveira  
Bruce Sianyeuka

# Appendix B

## First chapter appendix

### 1. Tables

**TABLE B1 – TOTAL NUMBER OF VICTIMS CAUSED BY THE CIVIL CONFLICT DURING 1980-2000, BY YEAR**

Year	Number of Victims
1980	1,046
1981	1,275
1982	4,039
1983	10,471
1984	20,472
1985	10,185
1986	7,880
1987	8,916
1988	11,541
1989	14,680
1990	14,618
1991	8,806
1992	10,239
1993	6,657
1994	4,245
1995	3,729
1996	2,606
1997	2,327
1998	1,869
1999	1,302
2000	641
Total	147,544

*Notes:* The information is provided by the Truth and Reconciliation Commission and reports the total number of victims during the conflict period (1980-2000) by year.

**TABLE B2 – TOTAL NUMBER OF CIVIL CONFLICT INCIDENTS PER AFFECTATION TYPE**

Affectation Type	Number of Cases
Forced displacement	71,616
Torture	49,435
Death	24,144
Arbitrary detention	17,543
Kidnapping	14,475
Wounded or Injured Victims	13,266
Enforced disappearance	8,993
Sexual violence	6,758
Forced Recruitment	2,198
Prison being Innocent	1,515
Victims with Disabilities	1,161
Minor member of CAD	652
Undocumented	47
Unduly requisitioned	37
Null	3
<b>Total</b>	<b>211,843</b>

Notes: The information is provided by the Truth and Reconciliation Commission and reports the total number of victims during the conflict period (1980-2000) by affectation type.

**TABLE B3 – COMPARING ANALYTICAL SAMPLE WITH YOUNG LIVES R1**

	R1 Original sample			Final Analytical sample			Difference (7)
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	
<i>Panel A. Mother characteristics</i>							
Mother's age (years), R1	27.04	(7.02)	2047	27.04	(6.68)	1752	0.00
Mother has complete primary or less	0.49	(0.50)	1956	0.49	(0.50)	1752	0.00
Mother has complete (or incomplete) secondary	0.36	(0.48)	1956	0.37	(0.48)	1752	-0.01
Mother has some level of higher education	0.15	(0.36)	1956	0.14	(0.35)	1752	0.01
Mother's native tongue is indigenous	0.29	(0.45)	2051	0.30	(0.46)	1752	-0.01
Mother lived in a different district than her birthplace by 1999	0.30	(0.46)	1872	0.30	(0.46)	1746	0.00
Mother lived in urban area, R1	0.69	(0.46)	2051	0.69	(0.46)	1752	-0.00
Caregiver is the head's household, R1	0.08	(0.26)	2051	0.07	(0.25)	1752	0.01
Household size, R1	5.70	(2.33)	2051	5.69	(2.33)	1752	0.02
Mother's number of children, R1	2.67	(2.10)	2050	2.69	(2.09)	1752	-0.02
Wealth index, R1	0.42	(0.24)	2046	0.43	(0.24)	1752	-0.01
Mother does not have a permanent partner, R1	0.11	(0.32)	2051	0.12	(0.33)	1752	-0.01
Mother is the primary caregiver	0.95	(0.21)	1953	1.00	(0.08)	1752	-0.05***
Natural region is Coast	0.35	(0.48)	2051	0.35	(0.48)	1752	-0.01
Natural region is Andean/Mountain	0.50	(0.50)	2051	0.50	(0.50)	1752	0.01
Natural region is Jungle	0.15	(0.36)	2051	0.15	(0.36)	1752	0.00
<i>Panel B. YL child characteristics</i>							
YL is female	0.50	(0.50)	2051	0.50	(0.50)	1752	0.00
Age in months, R1	11.53	(3.54)	2051	11.55	(3.53)	1752	-0.01
YL is the oldest sibling	0.40	(0.49)	2051	0.39	(0.49)	1752	0.01

Notes: The wealth index is a measure of multidimensional poverty used in YL. For more information about this variable, see Briones (2017). R1 original sample (columns 1-3) corresponds to the original sample collected in 2002. Columns 4-6 correspond to the analytical sample used in the baseline results. Column 7 tests for differences between means original R1 YL data and the final analytical sample. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

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**TABLE B4 – CHILD’S SOCIO-EMOTIONAL SCALES IN YOUNG LIVES**

Scale	YL Round	Statements
<b>Agency Index</b>	3, 4 and 5	<ol style="list-style-type: none"><li>1. If I try hard, I can improve my situation in life</li><li>2. Other people in my family make all the decisions about how I spend my time</li><li>3. I like to make plans for my future studies and work</li><li>4. If I study hard at school, I will be rewarded by a better job in the future</li><li>5. I have no choice about the work I do – I must do this sort of work</li></ol>
<b>Pride Index</b>	3, 4 and 5	<ol style="list-style-type: none"><li>1. I am proud of my shoes or of having shoes</li><li>2. I am proud of my clothes</li><li>3. I am never embarrassed because I do not have the right books, pencils or other equipment</li><li>4. I am proud that I have the correct uniform</li><li>5. I am proud of the work I have to do</li></ol>
<b>Self-efficacy Index</b>	4 and 5	<ol style="list-style-type: none"><li>1. I can always manage to solve difficult problems if I try hard enough</li><li>2. If someone opposes me, I can find the means and ways to get what I want</li><li>3. It is easy for me to stick to my aims and accomplish my goals</li><li>4. I am confident that I could deal efficiently with unexpected events</li><li>5. Thanks to my resourcefulness, I know how to handle unforeseen situations</li><li>6. I can solve most problems if I invest the necessary effort</li><li>7. I can remain calm when facing difficulties because I can rely on my coping abilities</li><li>8. When I am confronted with a problem, I can usually find several solutions</li><li>9. If I am in trouble, I can usually think of a solution</li><li>10. I can usually handle whatever comes my way</li></ol>
<b>Self-esteem Index</b>	4 and 5	<ol style="list-style-type: none"><li>1. I do lots of important things</li><li>2. In general, I like being the way I am</li><li>3. Overall, I have a lot to be proud of</li><li>4. I can do things as well as most people</li><li>5. Other people think I am a good person</li><li>6. A lot of things about me are good</li><li>7. I'm as good as most other people</li><li>8. When I do something, I do it well</li></ol>

**TABLE B5 – INTERGENERATIONAL EFFECT OF CONFLICT EXPOSURE ON CHILD'S SOCIO-EMOTIONAL OUTCOMES AT AGE 8:  
FULL CONTROLS**

	Agency					Pride					Index				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Mother's exposure to conflict (in number of years)	-0.011** (0.005)	-0.015*** (0.003)	-0.015*** (0.003)	-0.016*** (0.003)	-0.016*** (0.004)	-0.015* (0.009)	-0.018** (0.006)	-0.017*** (0.005)	-0.017*** (0.004)	-0.017*** (0.005)	-0.013* (0.006)	-0.017*** (0.004)	-0.016*** (0.003)	-0.016*** (0.003)	-0.016*** (0.003)
Observations	1,752	1,752	1,752	1,752	1,752	1,752	1,752	1,752	1,752	1,752	1,752	1,752	1,752	1,752	1,752
R-squared	0.002	0.090	0.092	0.098	0.099	0.003	0.094	0.098	0.108	0.108	0.004	0.122	0.125	0.135	0.136
Mother's Birth Year FE		X	X	X	X		X	X	X	X		X	X	X	X
Mother's Birth Region FE		X	X	X	X		X	X	X	X		X	X	X	X
Location in Round 1 FE		X	X	X	X		X	X	X	X		X	X	X	X
Mother-level controls			X	X	X			X	X	X		X	X	X	X
Child's Birth Month-Year FE				X	X			X	X	X		X	X	X	X
Child-level controls				X	X			X	X	X		X	X	X	X
Province-level controls					X				X	X				X	X

Notes: The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B6 – INTERGENERATIONAL EFFECT OF CONFLICT EXPOSURE ON CHILD’S SOCIO-EMOTIONAL OUTCOMES AT AGE 8: HETEROGENEITY ANALYSIS**

	Agency (1)	Pride (2)	Index (3)
<i>Panel A. Female</i>			
Mother’s exposure to conflict	-0.011** (0.005)	-0.007 (0.005)	-0.009** (0.004)
Interaction	-0.010 (0.007)	-0.020* (0.010)	-0.015* (0.007)
<i>Panel B. Bottom WL tercile</i>			
Mother’s exposure to conflict	-0.015** (0.006)	-0.010* (0.005)	-0.012** (0.004)
Interaction	-0.001 (0.012)	-0.012 (0.008)	-0.006 (0.009)
<i>Panel C. Rural location in 2002</i>			
Mother’s exposure to conflict	-0.023*** (0.007)	-0.019*** (0.006)	-0.021*** (0.004)
Interaction	0.014 (0.010)	0.003 (0.013)	0.009 (0.008)
Observations	1,752	1,752	1,752

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother’s birth year, mother’s birth region, child’s birth month-year and location in the 2002 round. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002) and presented in parenthesis. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B7 – EXCLUDING DISTRICTS THAT NEVER EXPERIENCED VIOLENCE**

	Agency (1)	Pride (2)	Index (3)
Mother’s exposure to conflict	-0.014*** (0.004)	-0.019*** (0.005)	-0.017*** (0.003)
Observations	1,326	1,326	1,326
R-squared	0.127	0.128	0.163

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother’s birth year, mother’s birth region, child’s birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother’s native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B8 – EXCLUDING THE MOST VIOLENTLY AFFECTED REGION DURING THE CONFLICT PERIOD: AYACUCHO**

	Agency (1)	Pride (2)	Index (3)
Mother's exposure to conflict	-0.010 (0.006)	-0.016*** (0.005)	-0.013** (0.005)
Observations	1,562	1,562	1,562
R-squared	0.103	0.112	0.139

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B9 – BASELINE RESULTS, DIFFERENT LEVELS OF CLUSTERING STANDARD ERRORS**

	Agency (1)	Pride (2)	Index (3)
<i>Panel A. Clustered at YL sentinel site level</i>			
Mother's exposure to conflict	-0.016*** (0.004)	-0.017*** (0.005)	-0.016*** (0.003)
<i>Panel B. Clustered at district of birth level</i>			
Mother's exposure to conflict	-0.016* (0.009)	-0.017** (0.007)	-0.016*** (0.005)
Observations	1,752	1,752	1,752

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B10 – SUMMARY STATISTICS, BY MOTHER’S MIGRATION STATUS**

	Total		Not migrant		Migrant		Difference
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	(7)
Violence in mother's house'	0.36	(0.48)	0.35	(0.48)	0.38	(0.49)	-0.04
Mother's native tongue is indigenous	0.29	(0.45)	0.33	(0.47)	0.25	(0.43)	0.07**
Mother has completed high school	0.36	(0.48)	0.35	(0.48)	0.40	(0.49)	-0.05
Wealth index, R1	0.42	(0.24)	0.41	(0.24)	0.48	(0.22)	-0.08***
Household size, R1	5.70	(2.34)	5.87	(2.43)	5.29	(2.05)	0.58***

Notes: Column 4 tests for differences between means among non-migrant (Columns 3-4) and migrant mothers (Columns 5-6). Migrant mothers are defined as those mothers who lived in a different district than their birthplace by 1999. Violence in the mother's house is a dummy equal 1 if the mother recalls any violence in their parent's house. The wealth index is a measure of multidimensional poverty used in YL. For more information about this variable, see Briones (2017). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B11 – ASSOCIATION BETWEEN MIGRATION AND CIVIL CONFLICT: EVIDENCE FROM YL PANEL OF MIGRATION HISTORY**

	Migration		Migration to capital		Safe migration	
	(1)	(2)	(3)	(4)	(5)	(6)
At least one casualty ( $t - 1$ )	0.015*** (0.003)		0.005*** (0.002)		0.001 (0.001)	
Number of casualties/100 ( $t - 1$ )		0.007 (0.010)		0.004 (0.007)		0.002*** (0.000)
Observations	36,276	36,276	36,276	36,276	36,276	36,276
R-squared	0.100	0.100	0.058	0.058	0.055	0.055

Notes: Independent variables stand for (i) at least one fatal event due to the conflict in her residing district or (ii) number of casualties divided by 100 in her residing district. The panel comprises information between 1980 and 2000. All regressions include year-fixed effects as well as district-individual fixed effects. Standard errors clustered at the mother's district of birth level. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B12 – INTERGENERATIONAL EFFECTS OF CONFLICT ON CHILD'S SOCIO-EMOTIONAL OUTCOMES AT AGE 8: ADJUSTING EXPOSURE TO CONFLICT FOR MIGRANT MOTHERS**

	Adjusted for migration		
	Agency (1)	Pride (2)	Index (3)
Mother's exposure to conflict (adjusted)	-0.005 (0.008)	-0.017*** (0.005)	-0.011*** (0.003)
Observations	1,752	1,752	1,752
R-squared	0.098	0.108	0.135

*Notes:* The independent variable for all models stands for the total number of months re-scaled in years; a mother was exposed to at least one fatal event due to the conflict in her residing district across her whole life. Exposure to conflict variables is adjusted, so it includes the district of residence of migrant mothers. Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother's birth year, mother's birth region and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B13 – MOTHER'S EXPOSURE TO CONFLICT ADJUSTED BY MIGRATION**

	Total		Not migrant		Migrant		Difference (7)
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	
<i>Exposure to conflict</i>							
Mother's exposure to conflict	1.20	(2.26)	1.18	(2.29)	1.24	(2.17)	-0.06
Mother's exposure to conflict at ages 0-5	0.26	(0.76)	0.28	(0.79)	0.17	(0.65)	0.11*
Mother's exposure to conflict at ages 6-10	0.33	(0.78)	0.35	(0.78)	0.28	(0.75)	0.08
Mother's exposure to conflict at ages 11-19	0.45	(1.04)	0.45	(1.05)	0.44	(1.02)	0.01
Mother's exposure to conflict at ages 20+	0.35	(1.12)	0.34	(1.10)	0.38	(1.18)	-0.04

*Notes:* The conflict exposure variables are defined as the total number of months a mother was exposed to at least one fatal event due to conflict in her residing district across a given period of her life (the variable is re-scaled in years). Exposure to conflict variables in this table are adjusted, so the variables include the district of residence for migrant mothers. Column 4 tests for differences between means among non-migrant (Columns 3-4) and migrant mothers (Columns 5-6). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B14 – ACCOUNTING FOR BIRTH REGION TIME TRENDS**

	Agency (1)	Pride (2)	Index (3)
Mother's exposure to conflict (in number of years)	-0.024*** (0.005)	-0.020*** (0.006)	-0.022*** (0.004)
Observations	1,634	1,634	1,634
R-squared	0.234	0.254	0.268

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B15 – VARIATION IN COHORT SIZE AND COMPOSITION DURING THE CONFLICT PERIOD: EVIDENCE FROM PERU CENSUS DATA**

	District cohort size		District cohort female percent	
	(1)	(2)	(3)	(4)
Any violent event	0.003 (0.009)		-0.002 (0.005)	
Any violent event with fatalities		0.016 (0.012)		-0.006 (0.006)
Observations	62,280	62,280	61,942	61,942
R-squared	0.97	0.97	0.24	0.24

*Notes:* Information comes from the Peruvian Census 2007 for the sample born between 1954 and 1987 to make it comparable with the YL mothers sample. Outcomes are the logarithm of the population per year per district and the percentage of female inhabitants per district per year. Each column corresponds to a different regression. Independent variables are as follows: Column (1) measures the presence of violence by any violent event that happened due to conflict in that year, and column (2) only counts the presence of violence in those events with at least one fatality (death or forced disappearing). All models include fixed effects at the district level and cohort year. Standard errors clustered at the district level in parentheses. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B16 – PROBABILITY OF EXITING THE YOUNG LIVES SAMPLE**

	Not found in Round 3	Not found in Round 4	Not found in Round 5
	(1)	(2)	(3)
Mother's exposure to conflict	-0.001 (0.002)	-0.000 (0.003)	-0.002 (0.005)
Observations	1,827	1,827	1,827
R-squared	0.070	0.083	0.073

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth month-year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B17 – INTERGENERATIONAL EFFECT OF CONFLICT EXPOSURE:  
INCLUDING YOUNGER SIBLINGS**

	Agency (1)	Pride (2)	Index (3)
<i>Round 2013 outcomes</i>			
Mother's exposure to conflict	-0.016*** (0.003)	-0.018*** (0.004)	-0.019** (0.007)
<i>Round 2015 outcomes</i>			
Mother's exposure to conflict	-0.004 (0.005)	-0.003 (0.017)	0.006 (0.010)
Observations	2,293	2,293	2,293

*Notes:* The conflict variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district across her whole life (the variable is re-scaled in years). Index outcome refers to the average of both agency and pride indexes. Outcomes were collected when the YL index child was age 12 and 15, respectively. Fixed effects included in all regressions are: mother's birth year, mother's birth region, child's birth year and location in the 2002 round. The control variables included in all regressions are the sex of the child and the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE B18 – MECHANISMS VARIABLES DESCRIPTION**

Variable name	Type	Description
<b>1. Education expenditure</b>	Dichotomical	Mother has a job and received a salary (R2).
<b>2. Reading encouragement</b>	Index	Composed of 4 yes/no questions made to the parents and 1 standardized continuous answer (R3): a. Do you encourage your child to read? b. Does your child read texts for fun (i.e. not assigned as homework)? c. Do you have a dictionary at home? d. Does your child use the dictionary at home? e. How many books do you have at home, excluding school textbooks?
<b>3. Parental involvement</b>	Index	Composed of 6 yes/no questions made to the parents (R3): a. Do you know the names of your child's friends? b. Do you know what your child does after school/work? c. Do you feel a close relationship with your child? d. Do you know the parents of your child's friends? e. Do you know your child's teacher? f. Do you talk to your child about politics or political participation?
<b>4. Household expenditure in food</b>	Continuous	The amount of money spent on food items in the last month, in per capita real terms (R3)
<b>5. Wealth index</b>	Index	A measure of household socio-economic status (R1), ranging between 0 and 1, representing the poorest and wealthiest households, respectively.
<b>6. Teenage pregnancy</b>	Dichotomical	Mother has given birth before the age of 20.
<b>7. Age at first birth</b>	Continuous	Mother's age at first birth.
<b>8. Number of children born</b>	Continuous	Mother's number of children born (alive).
<b>9. Mother is a waged worker</b>	Dichotomical	Mother has a job and received a salary (R2).
<b>10. Mother makes household decisions</b>	Dichotomical	Mother is the primary household member making decisions in the household about (i) the land, (ii) the animals, or (iii) work for wage activities (R2).
<b>11. Mother's educational level</b>	Dichotomical	Mother's highest educational level is complete primary (R2).
<b>12. Agency Index</b>	Index	Composed of 5 5-point scale questions (1: strongly agree, 0: strongly disagree) made to the caregiver (R3): a. If I try hard, I can improve my situation in life b. I like to make plans for my future c. I have no choice about which should to send my child to d. If my child gets really sick, I can do little to help him/her get better e. I can do little to help my child do well in school, no matter how hard I try
<b>13. Self-esteem Index</b>	Index	Composed of 5 5-point scale questions (1: strongly agree, 0: strongly disagree) made to the caregiver (R3) a. I feel proud to show my friends or other visitors where I live b. I feel proud of my clothes c. I feel proud of the job done by my spouse d. The job I do makes me feel proud e. I feel proud of my children
<b>14. Mental Health Index</b>	Index	Composed of 20 2-point scale; 1: Agrees; 0: disagrees questions made to the caregiver (R3) a. Do you often have headaches? b. Is your appetite poor? c. Do you sleep badly? d. Are you easily frightened? e. Do your hands shake? f. Do you feel nervous, tense, or worried? g. Is your digestion poor? h. Do you have trouble thinking clearly? i. Do you feel unhappy? j. Do you cry more than usual? k. Do you find it difficult to enjoy your daily activities? l. Do you find it difficult to make decisions? m. Is your daily work suffering? n. Are you unable to play a useful part in life? o. Have you lost interest in things? p. Do you feel that you are worthless person? q. Has the thought of ending your life been on your mind? r. Do you feel tired all the time? s. Do you have uncomfortable feelings in your stomach? t. Are you easily tired?

**TABLE B19 – EFFECT OF CONFLICT EXPOSURE: PARTNER-RELATED OUTCOMES**

	Drunk Partner (1)	Father is not present daily (2)	Father is not employed (3)
Mother's exposure to conflict	-0.002 (0.007)	0.005* (0.003)	-0.003 (0.004)
Mean dep. variable	0.511	0.168	0.074
Observations	1,752	1,752	1,752

*Notes:* Independent variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district. Fixed effects included in all regressions are: mother's birth year, mother's birth region and location in the 2002 round. The control variables included in the regressions was the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

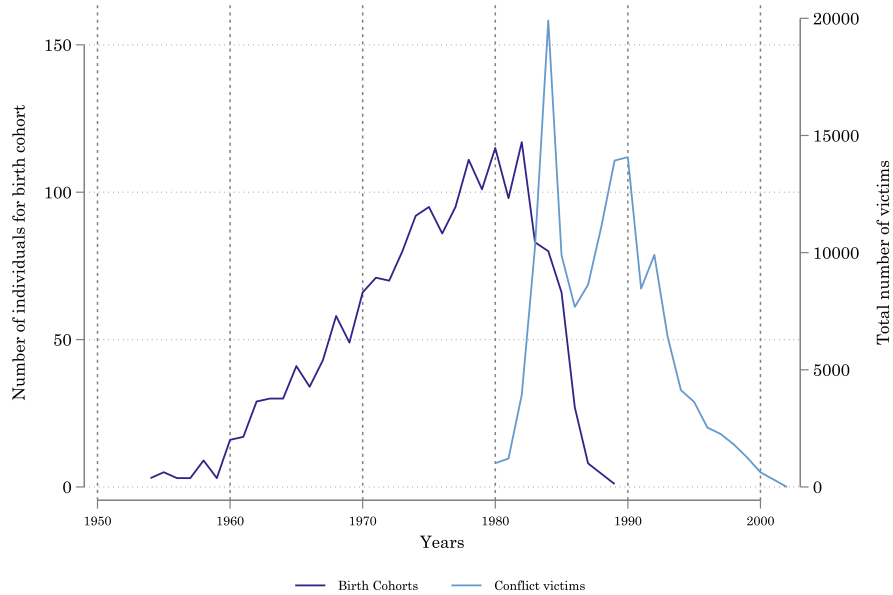
**TABLE B20 – EFFECT OF CONFLICT EXPOSURE: MOTHER'S COGNITIVE AND NON-COGNITIVE OUTCOMES**

	Mother's cognitive outcomes	Mother's Socio-emotional outcomes		
	Years of education (1)	Mental health (2)	Agency (3)	Self esteem (4)
Mother's exposure to conflict	-0.009 (0.007)	0.026 (0.051)	-0.024 (0.016)	-0.024 (0.021)
Mean dep. variable	0.491	3.642	5.850	5.594
Observations	1,752	1,752	1,752	1,752

*Notes:* Independent variable for all models stands for the total number of months a mother was exposed to at least one fatal event due to the conflict in her birth district. Fixed effects included in all regressions are: mother's birth year, mother's birth region and location in the 2002 round. The control variables included in the regressions was the mother's native tongue. Also, province-level pre-conflict variables are included: (logarithm of) population size, female illiteracy rate and access to piped water rate. Standard errors clustered at the YL sentinel site (location in 2002). Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

## 2. Figures

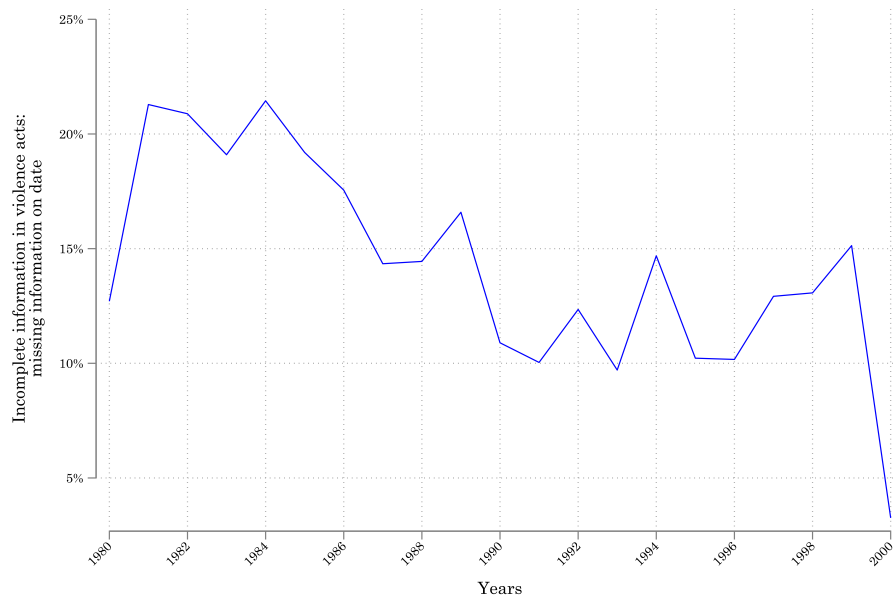
**FIGURE B1 – DISTRIBUTION OF YL BIRTH COHORTS CROSSED WITH CONFLICT-RELATED VIOLENCE DATA**



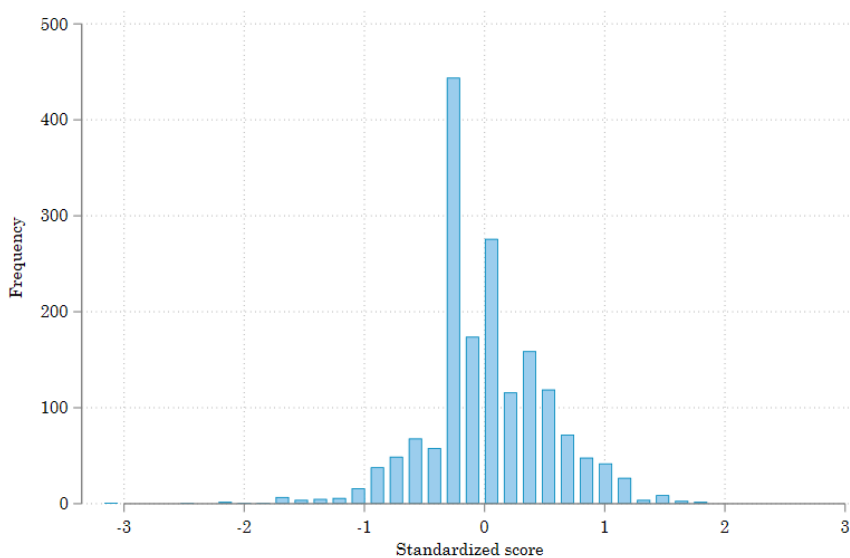
*Notes:* The figure depicts the overlapped distribution between the Young Lives mothers' birth cohorts (purple), and the duration and intensity of the civil conflict (blue).

*Source:* Young Lives Longitudinal Study and Truth and Reconciliation Commission.

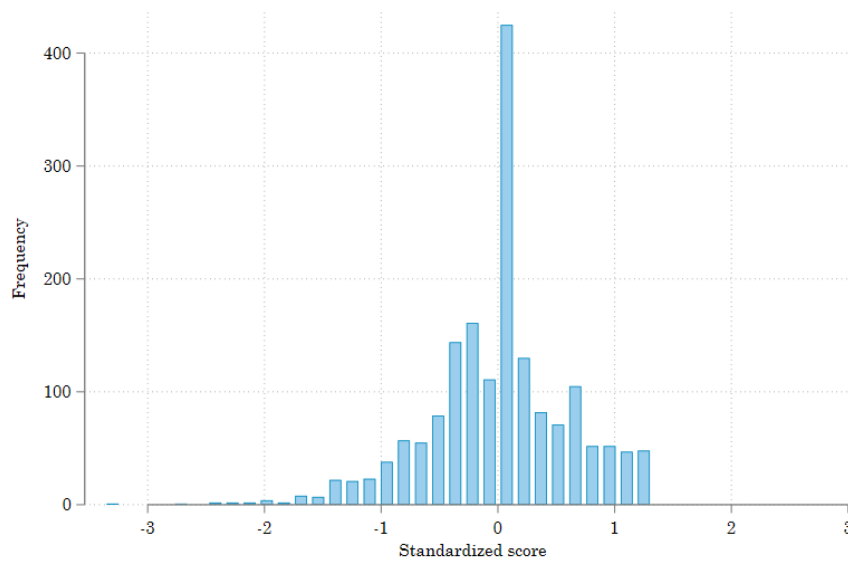
**FIGURE B2 – IMPRECISE MEASURE IN THE TRC CONFLICT DATA**



**FIGURE B3** – HISTOGRAM OF AGENCY STANDARDISED SCORE, AT AGE 8



**FIGURE B4** – HISTOGRAM OF PRIDE STANDARDISED SCORE, AT AGE 8



# Appendix C

## Third chapter appendix

### 1. Tables

**TABLE C1 – SAMPLE DISTRIBUTION AND HEALTH CENTER EXPOSURE – DHS  
(2013–2019)**

Survey year	All	Urban	Rural
2013	2587	1598	989
2014	2759	1744	1015
2015	3272	2124	1148
2016	3017	1982	1035
2017	2944	1944	1000
2018	3854	2598	1256
2019	4668	3157	1511

*Notes:* This table summarizes the DHS sample distribution for the analytical sample of women aged 17 or younger during the survey year or at the implementation of the FPTG reform.

**TABLE C2 – WOMEN'S DESCRIPTIVE STATISTICS, ONLY URBAN SAMPLE, FOR THE PRE-REFORM PERIOD (2013-2016)**

	Total		Treated		Control	
	Mean	SD	Mean	SD	Mean	SD
<i>Panel A. Treatment variable</i>						
HCA within 4 km	0.34	(0.47)	-	-	-	-
<i>Panel B. Household characteristics</i>						
Has electricity (%)	0.91	(0.29)	0.99	(0.12)	0.87	(0.33)
Wealth (standardised) index	-0.10	(1.00)	0.46	(0.87)	-0.39	(0.93)
Age of head of household	45.88	(12.08)	46.68	(12.33)	45.48	(11.94)
Head of household is male (%)	0.75	(0.43)	0.71	(0.45)	0.78	(0.42)
<i>Panel C. Women characteristics</i>						
Age in years	15.98	(0.81)	16.03	(0.82)	15.95	(0.81)
Residence is in urban area (%)	0.64	(0.48)	0.97	(0.18)	0.47	(0.50)
Highest level of education is high school (%)	0.90	(0.30)	0.94	(0.25)	0.89	(0.32)
Under-18 when gave birth (%)	0.08	(0.27)	0.07	(0.25)	0.09	(0.28)
Total children ever born	0.09	(0.29)	0.07	(0.26)	0.09	(0.31)

Notes: This table presents descriptive statistics using the DHS dataset for the pre-FPTG period between treated and control districts. The sample corresponds to women aged 17 or younger. The pre-FPTG reform period includes the years 2013, 2014, 2015 and 2016. Treatment districts contain an HCA center within a 4km buffer distance from the DHS household cluster.

**TABLE C3 – THE EFFECT OF FPTG REFORM ON TEENAGE FERTILITY RATES - CERTIFICADO NACIDO VIVO (2013-2019)**

	All sample	13-14	15-17
HCA in district x Post	-2.066** (0.817)	0.128 (0.268)	-4.250*** (1.586)
Observations	26,173	13,076	13,097
Mean Dep. Var.	15.80	2.17	29.41
Year - Region FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes

Notes: This table presents summary difference-in-difference (DiD) coefficients of Equation 3.1, estimated by Ordinary Least Squares (OLS) using the universe of birth records dataset. The outcome is the number of births per 1000 women in each age group, weighted by the population in 2019 in the corresponding age group. Column 1 presents estimates for all women teens (13 to 17 years old). Column 2 shows the results for women aged 13 to 14, and Column 3 shows the results for women aged 15 to 17. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 per cent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C4 – IMPACT OF FPTG REFORM ON OTHER OUTCOMES - *CERTIFICADO NACIDO VIVO* (2013-2019)**

	Weight at birth	Pregnancy duration	Height at birth
	(1)	(2)	(3)
HCA in district x Post	12.321 (14.608)	-0.045 (0.054)	6.046 (8.540)
Observations	10,221	10,221	10,221
Mean Dep. Var.	3075.52	38.59	58.28
Year-Region FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Squares (OLS) using the universe of birth records dataset. The outcomes are at the individual level: weight at birth in grams, dichotomous for more than one child, pregnancy duration in weeks and height at birth in centimetres. All regression includes district, year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C5 – IMPACT OF FPTG REFORM ON TEENAGE PREGNANCY RATES, HETEROGENEITY ANALYSIS – DHS (2013–2019)**

	Native	Non-native	Poor	Non-poor	Small hhsiz	Large hhsiz	Female head	Male head
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
4km HCA x Post	-0.010 (0.060)	-0.021** (0.008)	-0.007 (0.015)	-0.016* (0.009)	-0.017 (0.012)	-0.023** (0.010)	-0.024 (0.016)	-0.018* (0.010)
Observations	3,387	19,220	12,837	9,813	10,102	12,439	5,542	16,952
Mean Dep. Var.	0.13	0.11	0.14	0.06	0.11	0.12	0.10	0.12
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents summary difference-in-difference coefficients of Equation 3.2, estimated by Ordinary Least Squares (OLS) using the DHS dataset. The subsample analysis corresponds to the native mother tongue, wealth index, household size, and sex of household head. All regressions include covariates. Covariates include age, age squared, an indigenous native tongue (=1), and wealth index, as well as district and survey-year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C6 – MECHANISMS – DHS (2013-2019)**

	(1)	(2)	(3)
<i>Panel A. Contraceptive use</i>			
	Use of traditional method	Currently uses condom	Knowledge of modern methods
	(1)	(2)	(3)
4km HC x Post	-0.034** (0.017)	0.043* (0.023)	0.037** (0.017)
Observations	6,124	5,294	22,819
Mean Dep. Var.	0.12	0.19	0.47
<i>Panel B. Family Planning services</i>			
	FP with partner	FP with family	FP with friends
4km HC x Post	-0.027*** (0.009)	-0.033** (0.014)	-0.026* (0.014)
Observations	22,819	22,819	22,819
Mean Dep. Var.	0.12	0.26	0.23
<i>Panel C. Sexual behaviour</i>			
	Under-18 first intercourse	Age at first intercourse	Age difference with first partner
4km HC x Post	-0.014 (0.013)	0.068 (0.069)	-0.237 (0.230)
Observations	22,819	6,124	6,084
Mean Dep. Var.	0.27	14.94	4.18
Covariates	Yes	Yes	Yes
Survey year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes

Notes: This table presents summary difference-in-differences coefficients of Equation 3.2, estimated by Ordinary Least Square (OLS) using the DHS dataset. Covariates include age, age square, an indigenous native tongue (=1), and wealth index. All regression includes district and survey year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C7 – PRE-TRENDS - CERTIFICADO NACIDO VIVO (2013-2016)**

	All sample (1)	13–14 (2)	15–17 (3)
HCA in district × 2014	0.249 (0.663)	-0.030 (0.332)	0.526 (1.272)
HCA in district × 2015	0.762 (0.999)	-0.351 (0.409)	1.875 (1.860)
HCA in district × 2016	-0.013 (0.986)	-0.232 (0.461)	0.198 (1.844)
Observations	14,938	7,470	7,468
Pre-trends testing pvalue	0.745	0.796	0.655
Mean Dep. Var. (control)	17.71	6.22	28.20

Notes: Each row reports OLS estimates from separate regressions of the district-level teenage fertility rate (births per 1,000 girls aged 13–17) on an indicator for whether an adolescent health center (HCA) was present in the district in a specific pre-reform year. These regressions test for differential trends before the implementation of the 2017 family planning reform. All regressions include district and survey year-department fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C8 – PLACEBO YEARS - CNV (2013-2016)**

	2014 (1)	2015 (2)	2016 (3)
HCA in district × Post=2014	-0.725 (0.762)		
HCA in district × Post=2015		-1.044 (0.802)	
HCA in district × Post=2016			-1.677 (0.665)
Observations	26,145	26,145	26,145
Mean Dep. Var.	15.20	15.20	15.20
Year - Department FE	Yes	Yes	Yes
Age group - Department FE	Yes	Yes	Yes

Notes: Each column reports OLS estimates from separate regressions that assign placebo implementation dates for the 2017 reform. The outcome is the district-level teenage fertility rate (births per 1,000 girls aged 13–17). In each specification, the treatment indicator equals 1 if an HCA is located in the district and the placebo reform is assumed to begin in the specified pre-reform year. All specifications include region fixed effects to absorb time-invariant regional heterogeneity. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C9 – PRE-TRENDS - DHS (2013-2016)**

	All sample (1)	Only urban (2)
HCA within 4 km × 2014	0.013 (0.014)	0.024 (0.016)
HCA within 4 km × 2015	-0.012 (0.014)	-0.021 (0.017)
HCA within 4 km × 2016	0.007 (0.016)	0.006 (0.017)
Observations	11,428	7,333
Pre-trends testing pvalue	0.340	0.061
Year - Department FE	Yes	Yes
Age group - Department FE	Yes	Yes

Notes: Each column reports separate OLS estimates from difference-in-difference coefficients of Equation 3.2 using the DHS dataset. These regressions test for differential trends before the implementation of the 2017 family planning reform. All regressions include covariates. Covariates include age, age squared, an indigenous native tongue (=1), and wealth index, as well as district and survey-year fixed effects. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C10 – PLACEBO YEARS - DHS (2013-2016)**

	All sample			Only urban		
	2014 (1)	2015 (2)	2016 (3)	2014 (4)	2015 (5)	2016 (6)
HCA within 4km × Post=2014	-0.007 (0.010)			-0.007 (0.011)		
HCA within 4km × Post=2015		-0.011 (0.008)			-0.013 (0.009)	
HCA within 4km × Post=2016			-0.009 (0.008)			-0.007 (0.008)
Observations	22,819	22,819	22,819	14,976	14,976	14,976
Mean Dep. Var.	0.12	0.12	0.12	0.09	0.09	0.09
Year - Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Age group - Department FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports OLS estimates from separate regressions that assign placebo implementation dates for the 2017 reform using the DHS. The outcome is whether the individual gave birth before the age of 18. In each specification, the treatment indicator equals 1 if an HCA is located within a 4km distance and the placebo reform is assumed to begin in the specified pre-reform year. All regressions include covariates. Covariates include age, age squared, an indigenous native tongue (=1), and wealth index, as well as district fixed effects. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C11 – EXCLUDING METROPOLITAN LIMA - *CERTIFICADO NACIDO VIVO***  
(2013-2019)

	All sample (1)	13-14 (2)	15-17 (3)
HCA in district x Post	-1.770*** (0.681)	0.255 (0.276)	-3.801*** (1.293)
Observations	25,543	12,772	12,771
Mean Dep. Var.	15.32	2.08	28.56
Year - Department FE	Yes	Yes	Yes
Age group - Department FE	Yes	Yes	Yes

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Squares (OLS) using the universe of birth records dataset. Column 1 presents estimates for all women teens (13 to 17 years old). Column 2 shows the results for women aged 15 to 17 years old. All regression includes district, year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C12 – EXCLUDING ONE REGION AT A TIME - *CERTIFICADO NACIDO VIVO***  
(2013-2019)

Excluded Region	Coef. (SE)	R-squared
Amazonas	-2.406** (1.123)	0.419
Ancash	-2.369* (1.220)	0.415
Apurimac	-2.504** (1.108)	0.411
Arequipa	-2.504** (1.108)	0.416
Ayacucho	-2.604** (1.145)	0.420
Cajamarca	-2.205* (1.169)	0.411
Callao	-2.571** (1.126)	0.413
Cusco	-2.615** (1.062)	0.404
Huancavelica	-2.162* (1.133)	0.418
Huanuco	-1.981* (1.100)	0.410
Junin	-2.504** (1.108)	0.410
Ica	-3.209*** (1.127)	0.412
La Libertad	-2.326** (1.175)	0.411
Lambayeque	-2.331** (1.132)	0.411
Lima	-3.515*** (1.269)	0.424
Loreto	-2.612** (1.107)	0.411
Madre de Dios	-2.481** (1.116)	0.413
Moquegua	-2.504** (1.108)	0.413
Pasco	-2.378** (1.128)	0.413
Piura	-2.487** (1.139)	0.409
Puno	-2.504** (1.108)	0.408
San Martin	-2.623** (1.156)	0.410
Tacna	-2.504** (1.108)	0.415
Tumbes	-2.547** (1.117)	0.412
Ucayali	-2.331** (1.106)	0.411

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Square (OLS) using the universe of births records dataset. Each row represents the excluded region in the corresponding regression analysis. All regression includes district, survey year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C13** – MAIN RESULTS USING QUARTERLY AND SEMESTER DATA -  
*CERTIFICADO NACIDO VIVO (2013-2019)*

	Quarterly (1)	Semester (1)
HC in district $\times$ Post	-0.376** (0.157)	-0.802** (0.323)
Observations	51,182	25,593
Mean Dep. Var.	3.73	7.83
Survey year FE	Yes	Yes
District FE	Yes	Yes

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Squares (OLS) using the universe of birth records dataset in different temporary data: quarterly and semester data. All regression includes district, year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE C14** – PROBIT ANALYSIS - DHS (2013-2019)

Specification	Coefficient	Std. Error	Mean Dep. Var.	N
<i>Panel A. Post <math>\times</math> HCA within 2km</i>				
All sample	-0.010	(0.009)	0.1446	19,726
Only urban clusters	-0.009	(0.008)	0.1074	13,400
<i>Panel B. Post <math>\times</math> HCA within 3km</i>				
All sample	-0.014*	(0.008)	0.14466	19,726
Only urban clusters	-0.014*	(0.007)	0.1074	13,400
<i>Panel C. Post <math>\times</math> HCA within 4km</i>				
All sample	-0.015*	(0.008)	0.1446	19,726
Only urban clusters	-0.015*	(0.009)	0.1074	13,400
<i>Panel D. Post <math>\times</math> HCA within 5km</i>				
All sample	-0.016*	(0.009)	0.1446	19,726
Only urban clusters	-0.017*	(0.010)	0.1074	13,400

Notes: This table presents summary difference-in-difference (DiD) coefficients, estimated by Ordinary Least Square (OLS) using the universe of births records dataset. Each panel presents the regression for the complete sample and only the urban cluster sample, using different distance buffers (2-5km). All regression includes district, survey year-department fixed effects and sex of the child as controls. Standard errors are clustered at the district level. Significance levels at the 1, 5, and 10 percent levels are denoted by \*\*\*, \*\*, and \*, respectively.

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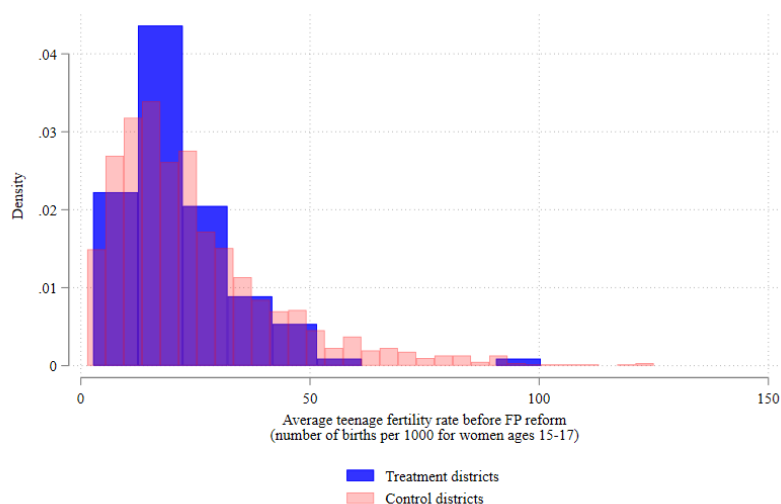
## 2. Figures

**FIGURE C1 – LOCATION OF HEALTH CENTERS PROVIDING A FAMILY PLANNING SERVICE IN PERU (DHS)**



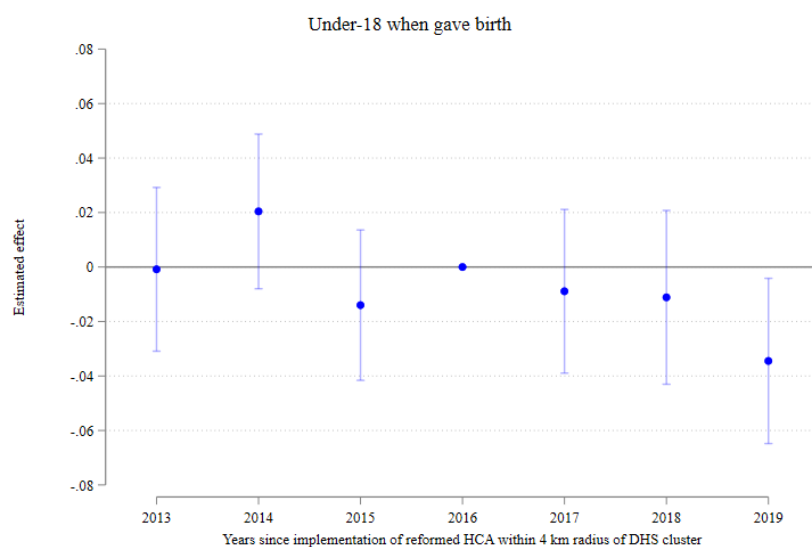
*Notes:* This graph illustrates the location coordinates of the HCA centers and other health centers at the national level using data from the Ministry of Health.

**FIGURE C2 – DISTRIBUTION OF TEEN FERTILITY RATES BEFORE 2017**



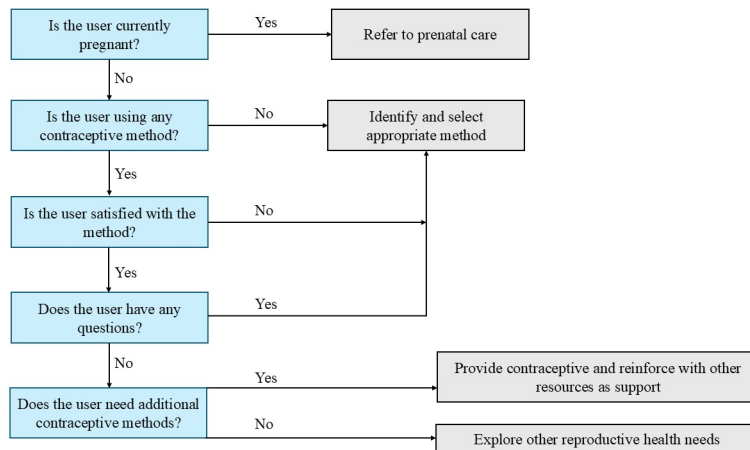
*Notes:* This figure shows the distribution of average teenage fertility rates (age groups 15, 16, and 17) in the pre-FPTG reform period for the 1,872 districts in the main sample, separately for treatment and comparison districts. The pre-FPTG reform period includes the years 2013, 2014, 2015, and 2016. Treatment districts contain HCA centers, while comparison districts have other health centers.

**FIGURE C3 – EVENT STUDY FOR TEENAGE PREGNANCY RATES – DHS (2013–2019)**

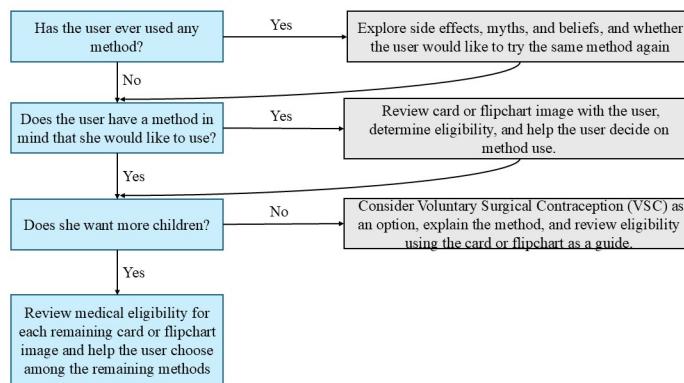


*Notes:* This graph presents summary difference-in-difference coefficients of Equation 3.2, estimated by Ordinary Least Squares (OLS) using the DHS dataset. All regressions include covariates: age, age squared, indigenous native tongue (=1), and wealth index. All regressions include district and survey year fixed effects. Standard errors are clustered at the district level. Confidence intervals at the 90% level are plotted.

**FIGURE C4 – FLOWCHART FOR COUNSELING IN FAMILY PLANNING CONSULTATIONS**



**FIGURE C5 – SELECTING A FAMILY PLANNING METHOD**



**FIGURE C6 – SERVICE IMPLEMENTATION**

**(a) COUPLES SERVICES**



**(b) INFORMATION CAMPAIGNS**



Notes: Photograph taken during monitoring visit by mass media. No identifiable information is disclosed.

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**TABLE C15 – MATERIALS, EQUIPMENT, AND FURNISHINGS REQUIRED FOR SERVICE PROVISION**

**2 Materials**

- Pamphlets on contraceptive methods, STI prevention, breast self-exams, HIV/AIDS.
- Violence screening form.
- Flip charts or visual cards of contraceptive methods.
- Anatomical models (female and male reproductive tracts).
- Displays of contraceptive methods.

**Contraceptive Methods and Supplies**

- Contraceptive methods.
- Microscope slides.
- Ayre spatulas.
- Cytobrushes.
- 70 alcohol.
- Sterile gloves.
- Lidocaine 2% without epinephrine (5 cm syringe, 21G needle).
- Rapid HIV tests.

**Equipment**

- Vaginal speculums (small, medium, large).
- IUD insertion/removal equipment.
- Female genital exam equipment.
- Male genital exam equipment.

**Information Records**

- Medical record.
- Standard prescription form.
- Lab test request form.
- Imaging request form.
- Appointment control card.
- SIS Unique Care Format.
- Family Planning follow-up form.
- FP Activity Report (SIS 240–D).
- Daily Services Log (HIS).

**Furnishings**

- Desk.
- Chairs.
- Gynecological exam table.
- Rotating stool.
- Gooseneck lamp.
- Screen divider.
- Metal shelves.
- Scale.
- Stethoscope.
- Aneroid sphygmomanometer.

# **Appendix D**

## **Fourth chapter appendix**

### **1. Tables**

**TABLE D1 – WEATHER SHOCKS ON 2023 IN-DEPTH VULNERABILITY SURVEY**

	Constant	Drought	Floods
<i>Panel A. Hunger (30 days)</i>			
No food in the house due to lack of resources	0.4549*** (0.0287)	0.0650** (0.0287)	0.0594* (0.0327)
Household member slept hungry	0.4218*** (0.0277)	0.0775** (0.0305)	0.0595* (0.0335)
Household member had a full day without eating	0.2695*** (0.0240)	0.1162*** (0.0307)	0.0537* (0.0289)
<i>Panel B. Reduced coping strategies (7 days)</i>			
Less expensive food	0.6434*** (0.0283)	0.0915*** (0.0250)	0.0504* (0.0277)
Borrowed food	0.4267*** (0.0200)	0.0363 (0.0238)	0.0916*** (0.0253)
Limited portions at mealtime	0.5113*** (0.0337)	0.0990*** (0.0312)	0.1033*** (0.0324)
Restricted adult consumption to favour children	0.3937*** (0.0258)	0.0592** (0.0256)	0.0773** (0.0329)
<i>Panel C. Livelihood coping strategies (30 days)</i>			
Reduced expenditures on essential non-food items	0.4484*** (0.0249)	0.0378 (0.0260)	0.0129 (0.0286)
Borrowed money for food or health needs	0.3659*** (0.0244)	0.0230 (0.0242)	0.0606*** (0.0228)
Used savings for food or health needs	0.2046*** (0.0200)	0.0245 (0.0242)	0.0096 (0.0197)
Sold oxen or mature female livestock	0.0549*** (0.0067)	0.0163 (0.0118)	0.0154* (0.0090)
Withdrew children from school	0.0662*** (0.0085)	-0.0009 (0.0139)	0.0222* (0.0113)
Begged	0.2104*** (0.0203)	0.0740*** (0.0242)	0.0411* (0.0236)
Engaged in child labour	0.0765*** (0.0108)	-0.0176 (0.0159)	0.0248 (0.0151)
Share of individuals impacted	–	0.3266*** (0.0346)	0.2999*** (0.0284)

Notes: this table displays estimates intended to capture correlations between being affected by a climate shock and vulnerability indices. We estimate a linear regression such as  $Y_i = \beta_0 + \beta_1 Shock + \varepsilon$ . Each  $Y_i$  is displayed in the table rows, while the shocks are a self-reported indication of being affected by a drought or flood. Standard errors are clustered at the district level. All regressions have a sample size of 11,247 households and are weighted according to household sample weights. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

Source: In-Depth Vulnerability Survey conducted by the DMMU.

**TABLE D2 – VARIABLE OF INTEREST DEFINITIONS (LFS)**

Variable name	Type	Description
1. Labour force	Dichotomical	Equals 1 if an individual, aged 15 or older, is employed or actively seeking a job at the time of the survey.
2. Formal sector	Dichotomical	Equals 1 if an employed individual is in the formal sector (e.g. has medical insurance, a formal contract, etc.).
3. Informal sector	Dichotomical	Equals 1 if an employed individual does not meet the criteria to be considered in a formal sector job.
4. Self-employed or household consumption	Dichotomical	Equals 1 if an individual is self-employed or works on farming, rearing animals, or fishing and the production is used for family consumption.
5. Manufacture	Dichotomical	Equals 1 if an individual can be categorized as a plant and machine operator or assembler.
6. Services	Dichotomical	Equals 1 if an individual can be categorized as a services and sales worker.
7. Agriculture	Dichotomical	Equals 1 if an individual works as an agricultural, forestry, or fishery worker.
8. Hours worked (main)	Continuous	Total number of hours dedicated to the main activity in a week.
9. Hours worked (secondary)	Continuous	Total number of hours dedicated to the secondary activity in a week.
10. Receiving SP	Dichotomical	Equals 1 if at least someone in the household is receiving at least one social benefit from a list of seven programmes.

**TABLE D3 – SAMPLE DISTRIBUTION BY SURVEY YEAR (LFS 2017–21)**

	(1) Total	(2) 2017	(3) 2018	(4) 2019	(5) 2020	(6) 2021
<i>Area of residence</i>						
Rural	57.27 (38,092,306)	57.91 (9,129,912)	58.02 (1,655,044)	57.25 (9,839,965)	56.74 (10,050,016)	57.06 (7,417,369)
Urban	42.73 (28,425,274)	42.09 (6,635,527)	41.98 (1,197,581)	42.75 (7,348,814)	43.26 (7,661,637)	42.94 (5,581,715)
<i>Sex</i>						
Male	49.56 (32,968,564)	48.38 (7,627,972)	72.58 (2,070,457)	48.65 (8,363,126)	48.09 (8,517,561)	49.15 (6,389,449)
Female	50.44 (33,549,016)	51.62 (8,137,467)	27.42 (782,169)	51.35 (8,825,653)	51.91 (9,194,093)	50.85 (6,609,634)
Observations	160,701	43,862	8,258	33,654	44,952	29,975

**TABLE D4 – SAMPLE DISTRIBUTION BY PROVINCE (LFS 2017–21)**

	(1) Total	(2) Rural	(3) Urban	(4) Male	(5) Female
<i>Provinces</i>					
Central	9.37 (6,232,859)	12.43 (4,733,690)	5.27 (1,499,169)	9.44 (3,112,886)	9.30 (3,119,973)
Copperbelt	15.10 (10,045,035)	4.48 (1,705,719)	29.34 (8,339,316)	15.17 (5,000,926)	15.04 (5,044,109)
Eastern	12.07 (8,030,085)	18.50 (7,045,261)	3.46 (984,824)	12.16 (4,010,410)	11.98 (4,019,675)
Luapula	7.12 (4,734,165)	9.34 (3,556,011)	4.14 (1,178,153)	7.07 (2,331,595)	7.16 (2,402,570)
Lusaka	19.12 (12,721,194)	4.55 (1,732,659)	38.66 (10,988,536)	19.06 (6,283,431)	19.19 (6,437,763)
Muchinga	5.85 (3,893,256)	7.37 (2,807,649)	3.82 (1,085,607)	5.95 (1,960,894)	5.76 (1,932,361)
Northern	8.09 (5,381,227)	11.71 (4,460,566)	3.24 (920,661)	8.19 (2,699,289)	7.99 (2,681,937)
North Western	4.91 (3,266,569)	6.01 (2,290,450)	3.43 (976,119)	4.88 (1,610,158)	4.94 (1,656,410)
Southern	12.19 (8,111,312)	16.01 (6,097,776)	7.08 (2,013,536)	12.09 (3,984,434)	12.30 (4,126,878)
Western	6.17 (4,101,879)	9.61 (3,662,525)	1.55 (439,354)	5.99 (1,974,539)	6.34 (2,127,340)
Observations	160,701	104,016	56,685	79,608	81,093

**TABLE D5 – HOUSEHOLDS BENEFICIARY OF SOCIAL PROTECTION PROGRAMS  
(LFS 2017–21)**

	All sample			Only SP recipients		
	(1) Total	(2) Rural	(3) Urban	(4) Total	(5) Rural	(6) Urban
Total number of HH member from SP db	0.10 (1,310,604)	0.16 (1,097,533)	0.04 (213,071)	1.26 (15,776,484)	1.27 (1,097,533)	1.22 (213,071)
Someone in the HH is beneficiary of at least one social benefit programme	0.08 (1,041,199)	0.13 (866,329)	0.03 (174,870)	1.00 (12,533,499)	1.00 (866,329)	1.00 (174,870)
Social protection type HH is beneficiary of Social Cash Transfer programme	0.03 (381,794)	0.05 (314,574)	0.01 (67,220)	0.37 (4,595,871)	0.36 (314,574)	0.38 (67,220)
HH is beneficiary of Public Welfare assistance	0.00 (32,804)	0.00 (23,596)	0.00 (9,208)	0.03 (394,885)	0.03 (23,596)	0.05 (9,208)
HH is beneficiary of Fertiliser Input Support programme	0.04 (563,917)	0.07 (494,081)	0.01 (69,835)	0.54 (6,788,182)	0.57 (494,081)	0.40 (69,835)
HH is beneficiary of Food Security Pack	0.00 (49,075)	0.01 (39,117)	0.00 (9,958)	0.05 (590,739)	0.05 (39,117)	0.06 (9,958)
HH is beneficiary of School Feeding Program	0.02 (201,948)	0.03 (177,129)	0.00 (24,818)	0.19 (2,430,958)	0.20 (177,129)	0.14 (24,818)
HH is beneficiary of Women Empowerment programme	0.00 (43,121)	0.00 (27,038)	0.00 (16,083)	0.04 (519,074)	0.03 (27,038)	0.09 (16,083)
HH is beneficiary of OVC bursary	0.00 (37,946)	0.00 (21,998)	0.00 (15,947)	0.04 (456,775)	0.03 (21,998)	0.09 (15,947)
Observations	29,528	18,220	11,308	2,903	2,438	465

**TABLE D6 – SOCIAL PROTECTION COVERAGE BY DEMOGRAPHIC CHARACTERISTICS (LFS 2017–21)**

	(1) Total	(2) Non-SP recipient	(3) SP Recipient
Household size	4.93 (61,737,754)	4.93 (56,616,509)	4.85 (5,121,245)
<i>Vulnerable population:</i>			
Member above age 60	0.15 (1,823,565)	0.13 (1,518,633)	0.29 (304,933)
Member below 12	0.59 (7,440,706)	0.62 (7,156,866)	0.27 (283,840)
<i>Provinces</i>			
Central	0.09 (1,090,006)	0.09 (1,036,256)	0.05 (53,750)
Copperbelt	0.15 (1,938,217)	0.16 (1,856,829)	0.08 (81,388)
Eastern	0.12 (1,526,832)	0.11 (1,279,477)	0.23 (247,355)
Luapula	0.07 (913,097)	0.07 (799,894)	0.11 (113,203)
Lusaka	0.21 (2,646,555)	0.23 (2,609,607)	0.03 (36,947)
Muchinga	0.05 (653,573)	0.05 (560,837)	0.09 (92,735)
Northern	0.07 (887,858)	0.07 (764,528)	0.12 (123,330)
North Western	0.05 (592,802)	0.05 (526,476)	0.06 (66,325)
Southern	0.13 (1,577,046)	0.12 (1,406,741)	0.16 (170,305)
Western	0.06 (703,004)	0.06 (632,344)	0.07 (70,660)
Observations	29,529	26,590	2,939

**TABLE D7 – FOOD CONSUMPTION AND SOCIAL PROGRAMMES, 2022**

	Log HH food consumption pc		
	(1)	(2)	(3)
Past month rain below p10	-0.0930 (0.0927)	-0.0790 (0.0911)	-0.0772 (0.0880)
Social Cash Transfer × past month rain below p10	0.1882 (0.1461)		
Keeping Girls in School × past month rain below p10		0.0670 (0.2919)	
School Feeding Program × past month rain below p10			-0.3831 (0.2751)
<i>Fixed effects</i>			
District	Yes	Yes	Yes
Year	Yes	Yes	Yes
Month	Yes	Yes	Yes
<i>Fit statistics</i>			
Observations	8,028	8,028	8,028
R <sup>2</sup>	0.27946	0.28009	0.28086
Within R <sup>2</sup>	0.08837	0.08916	0.09013

Notes: this table shows the results of experiencing an extreme weather event in the previous growing season according to Equation 4.4. The dependent variable is the logarithm of the household food consumption per capita. It includes the food consumption produced by the household. This table shows the potential mitigation effects of social protection policies. Standard errors are clustered at the district level. All regressions include district and year-quarter fixed effects. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE D8 – THE EFFECTS OF CLIMATE SHOCKS ON HOUSEHOLD INCOME PER CAPITA**

	All HH			Female-headed HH		
	Any social benefit (1)	Social Cash Transfer (2)	Input Fertilizer (3)	Any social benefit (4)	Social Cash Transfer (5)	Input Fertilizer (6)
Precipitation below p10 (past quarter)	0.022 (0.022)	0.024 (0.022)	0.023 (0.022)	0.010 (0.030)	0.012 (0.030)	0.011 (0.030)
Precipitation above p90 (past quarter)	-0.099*** (0.029)	-0.097*** (0.030)	-0.095*** (0.029)	0.044 (0.035)	0.047 (0.035)	0.048 (0.035)
Temperature above p90 (past quarter)	-0.055 (0.040)	-0.055 (0.040)	-0.054 (0.040)	0.035 (0.038)	0.036 (0.038)	0.036 (0.038)
HH receiving SP × precipitation below p10	0.028 (0.032)	0.017 (0.032)	0.034 (0.037)	-0.008 (0.039)	0.011 (0.042)	0.027 (0.052)
HH receiving SP × precipitation above p90	0.114*** (0.022)	0.214*** (0.042)	0.075*** (0.026)	0.111*** (0.034)	0.175*** (0.042)	0.139** (0.058)
HH receiving SP × temperature above p90	0.047 (0.031)	0.122*** (0.034)	0.060 (0.036)	0.029 (0.042)	0.070 (0.060)	0.060 (0.071)
<i>Fit statistics</i>						
Observations	93,165	93,165	93,165	24,142	24,142	24,142
R-squared	0.224	0.223	0.224	0.212	0.212	0.211

Notes: this table shows the results of the impact of experiencing at least one month of an extreme weather event in the past quarter on incomes according to Equation 4.5. Controls include gender, age group, education, rural residence. Standard errors are clustered at the district level. All regressions include district and year-quarter fixed effects. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

**TABLE D9 – LIKELIHOOD OF MIGRATION IN THE PAST 12 MONTHS - P10 & P90  
(LFS 2017-2021)**

	Migration	Migration due to labour
	(1)	(2)
<i>Panel A. Baseline results</i>		
Precipitation Below P10 (Past year)	-0.001 (0.003)	-0.000 (0.001)
Precipitation Above P90 (Past year)	0.006** (0.002)	0.002 (0.001)
Temperature Above P90 (Past year)	0.000 (0.004)	0.001 (0.001)
Observations	99,124	99,124
R-squared	0.029	0.010
<i>Panel B. Interaction with female</i>		
Precipitation Below P10 (Past year)	-0.001 (0.003)	-0.000 (0.001)
Precipitation Above P90 (Past year)	0.006** (0.002)	0.004*** (0.001)
Temperature Above P90 (Past year)	0.001 (0.004)	0.003** (0.001)
Female x Precipitation Below P10 (Past year)	0.000 (0.001)	-0.001 (0.001)
Female x Precipitation Above P90 (Past year)	0.000 (0.001)	-0.004*** (0.001)
Female x Temperature Above P90 (Past year)	-0.002 (0.001)	-0.004*** (0.001)
Year-quarter FE	Yes	Yes
District FE	Yes	Yes
Controls	Yes	Yes
Observations	99,124	99,124
R-squared	0.029	0.011

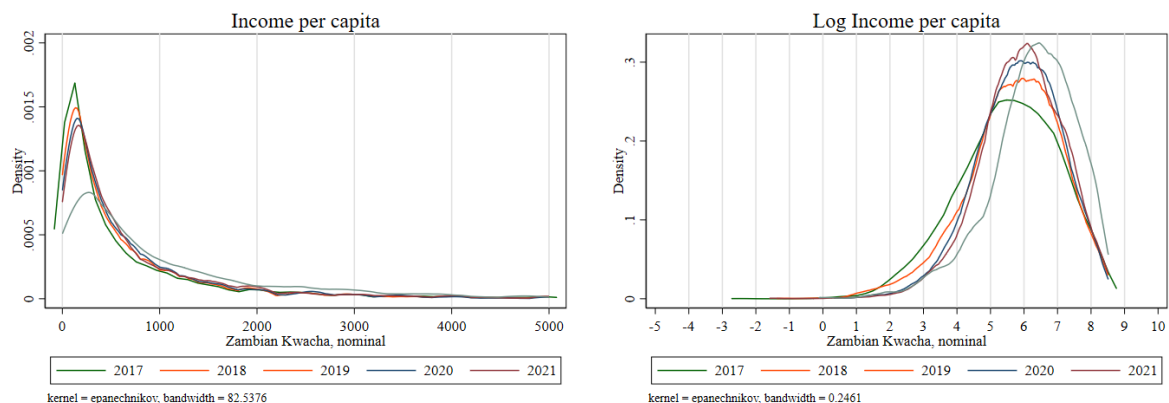
Notes: Standard errors are clustered at the District level. Controls include gender, age group, education, rural residence. All regressions include District and fixed effects. Significance levels at the 1, 5, and 10 percent levels denoted by \*\*\*, \*\*, and \*, respectively.

## 2. Figures

**FIGURE D1 – DISTRIBUTION OF INCOME PER CAPITA (LFS 2017–21)**

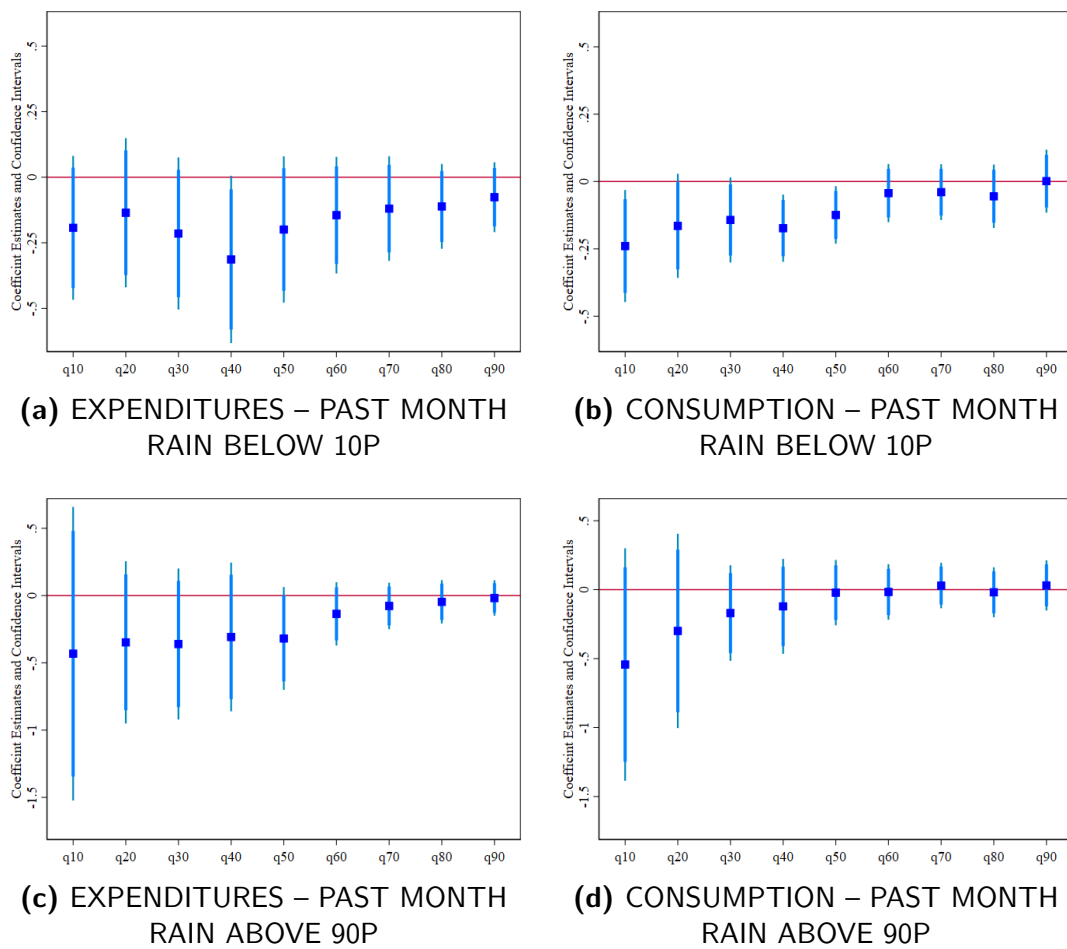
**(a) AUGUST-OCTOBER**

**(b) NOVEMBER-JANUARY**



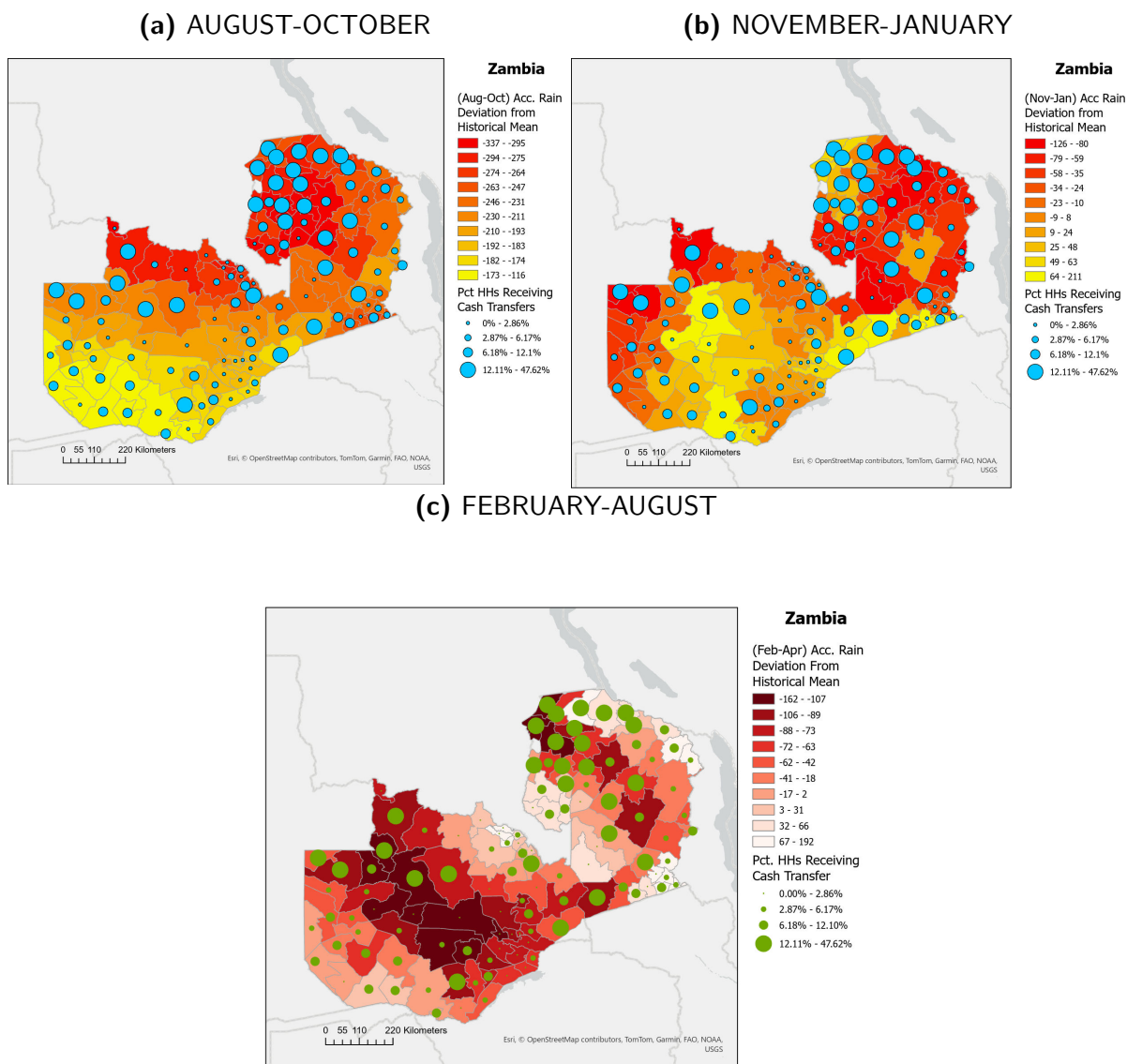
*Notes:* This figure shows the distribution of household income per capita based on the Labour Force Surveys (LFS) 2017–2021. Panel (a) plots the kernel density of income per capita for interviews conducted between August and October; panel (b) plots the corresponding density of log income per capita for interviews conducted between November and January. Income is expressed in real Zambian kwacha.

**FIGURE D2 – UNCONDITIONAL QUANTILES ANALYSIS: IMPACT OF WEATHER SHOCKS ON HH FOOD CONSUMPTION VALUE PER CAPITA**



*Notes:* the figure displays estimated coefficients of Equation 4.4 for the main climate shocks measures on the food consumption value per capita per decile of the consumption distribution. The blue dots are the estimated coefficients and the red bars are the 90% confidence interval.

**FIGURE D3 – PRECIPITATION AND SHARE OF SOCIAL CASH TRANSFER BENEFICIARIES**



Notes: This figure presents each district's percentage of households receiving cash transfer in the LMCS 2022 over the district's precipitation deviation from the historical mean for three different quarters before the survey.

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