Essays on Household Responses to Policy and Climate Shocks in Low-Income Settings

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Abstract

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This dissertation examines how public policies and environmental shocks shape health and economic outcomes for vulnerable populations, with a focus on reproductive autonomy, gender-based violence, and rural livelihoods.

Maternal and Child Health (MCH) programs promote safe motherhood by linking financial incentives for institutional delivery with health-worker outreach and family-planning education. In Chapter 1, I study how MCH programs influence reproductive behavior through pre-conception margin, by affecting contraceptive use and fertility, and post-conception margin by affecting whether unintended pregnancies continue to birth. Using a difference-in-differences design exploiting India's national MCH program, I find that it increased the probability of modern contraceptive use by 7 percentage points and reduced fertility among older women. In contrast, the program increased unintended pregnancies ending in births among younger women by 4 percentage points. Evidence from a later-phase reform of the program that expanded health-worker incentives shows that greater contact with community health workers contributed to this rise. These findings reveal an unintended consequence of maternal health interventions: by simultaneously promoting planned and protected pregnancy, they can inadvertently constrain

women's reproductive autonomy.

Chapter 2 turns to a different dimension of women's vulnerability, examining the causal relationship between anomalously hot days and intimate partner violence (IPV) in Peru, highlighting the intersection between environmental stressors and gender-based violence. The findings demonstrate that anomalously hot days increase IPV, with effects concentrated in urban settings. Two mechanisms help explain this pattern. First, hot days deteriorate the psychological well-being of male partners in the most densely urban provinces, raising the likelihood of violent behavior. Second, hot days increase community crime rates in urban areas, creating an environment where IPV becomes more likely. Together, these results emphasize the economic and social consequences of temperature anomalies and underscore the need for targeted interventions to mitigate IPV risks, particularly in urban populations.

Chapter 3 broadens the lens to rural economic welfare, studying Indonesian farmers' responses to temperature and rainfall shocks, defined as deviations from local seasonal means. Coauthored with Hyelim Son and XingXia, this chapter exploits within-subdistrict variation over time to find that both unusually high and low temperatures reduce rural household expenditure, while low rainfall does not. Households buffer these shocks by drawing down liquid assets and increasing agricultural work hours. Low rainfall shifts hours from non-agricultural to agricultural work, whereas temperature shocks increase agricultural hours without reducing non-agricultural work. Farmers also adjust cultivated land area and crop mix. These results suggest that farmers intensify farming labor to mitigate weather-related losses, challenging the view that rural labor readily reallocates to non-agriculture during agricultural downturns, and indicate greater vulnerability to temperature variations than to rainfall.

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DEDICATION

To my family and Professor Kyongwook Choi, whose support and encouragement made this journey possible.

Chapter 1

UNINTENDED CONSEQUENCES: MATERNAL HEALTH PROGRAMS AND THE CONTINUATION OF UNINTENDED PREGNANCIES

1.1 Introduction

An unintended pregnancy ending in a live birth, often referred to as an unplanned birth, is a substantial public health and development concern. Between 2015 and 2019, an estimated 121 million unintended pregnancies occurred annually worldwide, and nearly 40 million of these were carried to term (Bearak et al., 2020). The burden is particularly high in low-income settings such as India, which accounted for over 14 percent of all unintended pregnancies during this period, with roughly one-third ending in live births (Mcfarlane et al., 2022). These births are associated with lower utilization of maternal health services, worse maternal mental health, and poorer health and developmental outcomes for children (Wado et al., 2013; Bahk et al., 2015; Nelson et al., 2022; Singh et al., 2012; Marston and Cleland, 2003; Shapiro-Mendoza et al., 2006; Foster et al., 2018). Considering their wide-ranging impact on maternal and child well-being, understanding the determinants of unplanned births is essential for designing policies that improve maternal and child health.

Maternal and Child Health (MCH) programs are a central policy instrument for improving outcomes around childbirth and advancing reproductive goals. These initiatives combine messaging on safe motherhood and institutional delivery with financial incentives and outreach by community health workers (Neelsen et al., 2021). Their typical activities emphasize family planning, antenatal care, skilled attendance at delivery, and postnatal follow-up, often framed around the importance of a planned and protected pregnancy. Community health workers also provide counseling on nutrition, birth preparedness, and birth spacing, and encourage women to seek timely care during pregnancy (Gebremedhin et al., 2022).

Maternal health programs create an inherent tension in predicting their effects on fertility. By expanding access to contraception, they can reduce pregnancies. At the same time, they may increase the likelihood that unintended pregnancies are carried to term by influencing fertility preferences and altering the salience of pregnancy. One possible channel underlying this second effect operates through perceived fertility preferences within households. Family-planning counseling provided by health workers can shift women's preferences toward limiting childbearing without changing their perceptions of their husbands' preferences, increasing perceived preference discordance. In male-dominated contexts, where women's reproductive autonomy is constrained, pregnancy outcomes tend to align more closely with husbands' preferences (Komura, 2013; Doepke and Tertilt, 2018; Mishra and Parasnis, 2021; Thomson, 1997). As a result, when women perceive such discordance, they may be more likely to defer to what they believe their husbands prefer, increasing the likelihood that they carry unintended pregnancies to term. A second channel operates through norms surrounding pregnancy continuation. Repeated contact with health workers and sustained messaging about safe pregnancy and delivery may increase the salience of pregnancy by heightening its cognitive, emotional, and social importance, thereby discouraging abortion even when pregnancies are unintended.

Despite these opposing forces, little is known about how such interventions affect overall fertility and the continuation of unintended pregnancies. Existing studies of MCH programs have focused primarily on service utilization and maternal and infant mortality (Lim et al., 2010; Powell-Jackson et al., 2015) or on aggregate fertility levels (Nandi and Laxminarayan, 2016). This paper addresses this gap by studying India's nationwide maternal and child health program, the Janani Suraksha Yojana (JSY), launched in 2005 to foster safe motherhood. JSY promotes both planned and protected pregnancies through two main components: financial incentives that lower the cost of institutional delivery and sustained engagement with community health workers (Accredited Social Health Activists, or ASHAs). These features create conditions that can shape not only the timing and number of pregnancies through contraceptive use but also the likelihood that unintended ones are carried to term.

I estimate the causal effect of JSY on these reproductive behaviors using a difference-in-differences (DiD) strategy that leverages variation in program eligibility across Indian states. Indian states were classified as either Low-Performing States (LPS) or High-Performing States (HPS) based on baseline institutional delivery rates.[1] In LPS, where institutional delivery rates were historically low, JSY benefits were universally available to all pregnant women for every delivery. In HPS, eligibility was restricted to women below the poverty line, Scheduled Castes or Tribes, and those with up to two live births. The DiD design compares changes in outcomes between two groups with similar socioeconomic characteristics: ineligible women in HPS (those above the poverty line and not from Scheduled Castes or Tribes) serve as the control group, while eligible women with the same characteristics in LPS form the treatment group.

The analysis yields two main findings. First, JSY increased the likelihood of using modern contraceptive methods among all women and reduced fertility among older women. Specifically, the likelihood of using a modern contraceptive method rose by 6–7 percentage points overall, and among women aged 30–40, the number of births over the past five years fell by about 0.10, a decline of nearly 28 percent relative to baseline. This pattern suggests that improved access to family planning services through health programs enabled women to lower their total fertility. Second, the program increased the likelihood of unplanned births, with the effect concentrated among younger women aged 19-29: the probability of an unplanned birth rose by about 4 percentage points, or 20 percent relative to baseline. These results indicate that MCH programs can influence reproductive decisions along both the pre-conception margin, by affecting contraceptive use and fertility

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[1] LPS include Uttar Pradesh, Uttarakhand, Bihar, Jharkhand, Madhya Pradesh, Chhattisgarh, Assam, Rajasthan, Odisha, and Jammu and Kashmir.

levels, and the post-conception margin, by shaping whether unintended pregnancies are carried to term.

To investigate a key channel underlying the increase in unplanned births, I exploit a later-phase reform of the program that expanded ASHA incentives in High-Performing States.[2] Using a regression discontinuity design, I first show that the reform sharply increased women's interactions with community health workers. Building on this evidence, I then estimate a second difference-in-differences model to assess how these expanded interactions affected reproductive behavior. The reform raised the likelihood of an unplanned birth among younger women by 5 percentage points, about 15 percent relative to baseline, with no significant effect among older women. These findings suggest that increased engagement with community health workers, driven by their new cash incentives, contributed to the rise in unplanned births.

I provide suggestive evidence for two behavioral mechanisms through which greater engagement with community health workers contributed to the rise in unplanned births. First, I present evidence consistent with the perceived discordance mechanism discussed above. Using panel data from the India Human Development Survey, I examine how exposure to community health workers affected women's fertility preferences and their perceptions of spousal alignment. Exposure to ASHAs increased the likelihood that women reported not wanting additional children. More importantly, it also raised the share of women who perceived that they did not want more children whereas their husbands did. This widening perception gap suggests that greater engagement with health workers reshaped women's own fertility preferences and heightened perceived discordance within couples. In male-dominated contexts, such belief gaps can translate into higher rates of unplanned births.

Second, more engagement with health workers leads to lower acceptance of abortion. This finding aligns with previous literature showing that community health workers of-

[2]Four years after JSY began, the program was reformed to extend cash incentives to ASHAs in High-Performing States, where previously only mothers received payments.

ten promote pronatalist norms and discourage abortion (Nandagiri, 2019; Glenton et al., 2017; Gupta et al., 2017; Javadekar et al., 2025). They may do so through repeated counseling, financial incentives that pay ASHAs for each institutional delivery and thereby encourage them to promote delivery over abortion, or fears that abortion intentions disclosed to ASHAs will not remain confidential in village settings, making abortion appear less acceptable or feasible. When abortion is perceived as less acceptable within households and communities, unintended pregnancies are more likely to be carried to term, contributing to the rise in unplanned births.

Taken together, the findings demonstrate that MCH programs affect not only whether and when to conceive but also the likelihood that unintended pregnancies result in live births. While these programs aim to improve maternal and neonatal health, their incentive structures and delivery mechanisms can generate unintended effects on post-conception behavior. Understanding these broader impacts is essential for designing MCH interventions that support both health improvements and reproductive autonomy.

This paper contributes to four strands of literature at the intersection of maternal health policy, fertility behavior, community health workers, and incentive design in public service delivery.

First, it expands the literature on the effects of maternal and child health (MCH) programs, which has largely focused on service uptake and clinical outcomes such as institutional delivery rates and maternal or neonatal mortality (e.g., Lim et al., 2010; Powell-Jackson et al., 2015). Recent work has begun to examine behavioral outcomes such as sex-selective abortion (e.g., Javadekar et al., 2025). By further shifting attention to whether unintended pregnancies are carried to term, this paper highlights that MCH interventions can influence a broader set of reproductive intentions and birth composition. These outcomes are central to maternal well-being but are often overlooked in program evaluations.

Second, the paper contributes to research on the determinants of unintended fertility. Much of this work emphasizes pre-conception constraints, such as limited access to contraception or safe abortion services (e.g., Bongaarts, 1990; Bailey, 2010; Miller, 2010). I show that maternal health programs can also shape post-conception behavior. By expanding interactions with health workers who promote institutional delivery and safe motherhood, such programs may increase the likelihood that unintended pregnancies are carried to term. This mechanism helps explain how unplanned births may rise even in settings where contraceptive access is improving.

Third, this study adds to the literature on community health workers (CHWs), which has emphasized their role in expanding access to care and improving health outcomes (e.g., Björkman and Svensson, 2009; Dupas, 2011). I provide new evidence that CHWs can also influence fertility preferences and attitudes. Exposure to India's community health workers influences women's own fertility preferences without altering how they perceive their husbands' preferences, leading to greater reports of discordance within couples. It also affects abortion attitudes, particularly reducing acceptance of abortion among men. Together, these findings suggest household- and community-level channels through which CHWs may influence reproductive decisions beyond the delivery of clinical services.

Finally, this paper contributes to the literature on incentive design in public service delivery. Prior work has shown that performance-based pay can increase provider effort and improve the delivery of targeted services. For example, Gertler and Vermeersch (2013) and Singh and Masters (2017) show that provider incentives can improve child health and service delivery. Moreover, Mendelson et al. (2017) and Gadsden et al. (2021) find in systematic reviews that pay-for-performance programs often raise health service use and quality. I extend this literature by showing that incentives tied to institutional delivery affected not only service uptake but also behavioral outcomes. Specifically, the expansion of ASHA incentives increased their engagement with pregnant women, which in turn influenced decisions about whether to continue unintended pregnancies. This suggests that pay-for-performance programs can affect sensitive outcomes such as reproductive autonomy, with implications for how such incentives are designed and targeted.

The rest of the paper proceeds as follows. Section 3.2 provides institutional background on the JSY program. Section 1.3 outlines the conceptual motivation for how maternal and child health (MCH) programs can influence fertility behavior along both pre- and post-conception margins. Section 1.4 describes the data, variable construction, and empirical strategy. Section 1.5 presents the main results on overall fertility and unplanned births. Section 1.6 validates the empirical design using pre-trend tests. Section 1.7 examines mechanisms operating through exposure to community health workers, while Section 1.8 investigates the underlying behavioral responses, focusing on changes in perceived fertility preferences and abortion attitudes. Section 1.9 presents robustness checks addressing potential selection bias. Section 3.5 concludes.

1.2 Janani Suraksha Yojana: a Safe Motherhood Initiative

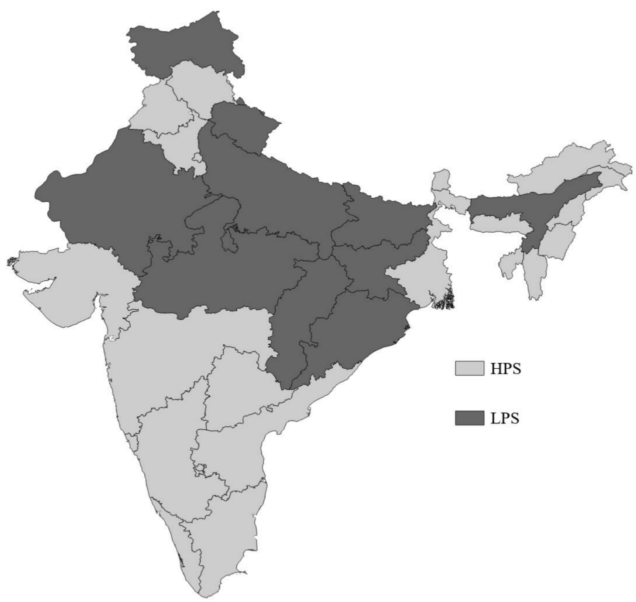
The Janani Suraksha Yojana (JSY) is a conditional cash transfer program launched by the Government of India in 2005 with the primary objective of reducing maternal and neonatal mortality by encouraging institutional deliveries. The program operates under the National Health Mission (NHM) and targets economically vulnerable pregnant women, aiming to increase access to healthcare facilities and reduce home births, which are often associated with higher risks of maternal and neonatal complications.

A key feature of JSY is its cash incentive structure, designed to promote institutional deliveries. Under the scheme, pregnant women receive monetary support when they give birth in a health facility, reducing financial barriers to accessing medical care. Additionally, Accredited Social Health Activists (ASHAs), who are trained community health workers, are compensated for assisting expectant mothers. ASHAs provide guidance on maternal health, facilitate transportation to health facilities, and help with registration and documentation required to access JSY benefits. By offering incentives to both mothers and ASHAs, the program aims to promote broader outreach and greater uptake of maternal healthcare services.

The program distinguishes between Low-Performing States (LPS) and High-Performing

States (HPS) based on institutional delivery rates. In LPS, where institutional delivery rates were initially low, all pregnant women are eligible for JSY benefits. In contrast, in HPS, where institutional deliveries were already relatively high, eligibility is limited to women classified as Below the Poverty Line (BPL) or belonging to Scheduled Castes (SC) or Scheduled Tribes (ST), and only for up to two live births. Figure 1.1 shows how Indian states are classified as Low- or High-Performing under the JSY program.

Figure 1.1: Low- and High-Performing States under the JSY Program



The financial incentives provided under JSY have changed over time. In 2005, mothers in low-performing states (LPS) received 1,400 Rps (about $32 USD, or 1,000 Rps /$23 USD in urban areas) for giving birth in a health facility, and ASHAs were paid 600 Rps (around $14 USD) for each delivery they supported. In high-performing states (HPS), mothers received 700 Rps ($16 USD; 600 Rps/$14USD in urban areas), and ASHAs did not receive any payment initially. From April 2009, the program expanded to offer ASHAs in HPS a payment of 200Rps (about $5 USD) per delivery while keeping the same payments for mothers. To give a sense of scale, the 1,400 Rps payment to mothers in 2005 amounted to about 51–52 percent of India's average monthly per-capita income (the annual income was approximately $740 USD in 2005, World Bank 2023), making it a significant amount for low-income households. Similarly, the 600Rps payment to ASHAs was a meaningful performance-based incentive in areas where other earning opportunities were limited.[3] Table 1.1 summarizes how JSY incentives varied over time and by state classification.

Table 1.1: Variation in JSY Incentive Structure

State Type	Recipient	2005–2009 Payment (Rps)	From April 2009 Payment (Rps)
LPS	Mother	1,400 (Rural) 1,000 (Urban)	1,400 (Rural) 1,000 (Urban)
	ASHA	600 per delivery	600 per delivery
HPS	Mother	700 (Rural) 600 (Urban)	700 (Rural) 600 (Urban)
	ASHA	None	200 per delivery

1.3 Conceptual Motivation

Following Ananat et al. (2009), reproductive behavior can be viewed as involving two related but distinct choices: whether to conceive and whether to continue a pregnancy once it has begun. Together, these choices determine both overall fertility and the incidence of unplanned births.

Both decisions are shaped by financial, informational, and social constraints. The decision to conceive depends on access to contraception and family-planning counseling, which can reduce fertility (Ashraf et al., 2014; Dupas, 2011), as well as on the expected costs of childbearing. The decision to continue a pregnancy reflects the financial and so-

[3] Based on a 2017 survey of ASHAs in Karnataka, Shet et al. (2018) report that most earned only about Rps 1,200–2,000 per month from activity-based incentives, without any fixed salary, and many considered the pay scale unjust relative to their workload. While incomes varied somewhat across states, these figures highlight how modest ASHAs' earnings were in practice.

cial costs of delivery, the cost and availability of abortion, interactions with health workers, and prevailing social norms surrounding termination (Ananat et al., 2009; Valente, 2014; Antón et al., 2018).

India's Janani Suraksha Yojana (JSY) provides a useful setting to examine how a maternal and child health (MCH) intervention influences these decisions. JSY is a nationwide safe-motherhood program that provides conditional cash transfers to promote institutional delivery and mobilizes community health workers (ASHAs) to support women through pregnancy and childbirth. While the program's explicit goal is to reduce maternal and neonatal mortality, its broader emphasis on safe motherhood, encouraging pregnancies that are both planned and protected, may also shape reproductive behavior in unintended ways.

Planned Pregnancy Emphasizing planned pregnancy within JSY may affect fertility behavior through both contraceptive use and fertility preferences. Family-planning counseling provided by ASHAs increases awareness and access to modern contraceptive methods and promotes the benefits of smaller families and adequate birth spacing (Ministry of Health and Family Welfare, Government of India, 2006). These activities could lower informational and access barriers to contraception, thereby reducing fertility. At the same time, repeated exposure to messages emphasizing planning and control over childbearing may shift household fertility preferences toward smaller family sizes and reduced additional childbearing. When these changes are not matched by what women perceive to be their husbands' preferences, a perception gap may emerge within couples. In male-dominated settings, where men hold greater influence over pregnancy outcomes (Komura, 2013; Doepke and Tertilt, 2018; Mishra and Parasnis, 2021; Thomson, 1997), women facing such perceived discordance may be more likely to defer to what they believe their husbands prefer. As a result, even when pregnancy continuation behavior itself does not change, a larger share of births is perceived as unintended from women's perspective, leading to an increase in unplanned births.

Protected Pregnancy Emphasizing protected pregnancy may plausibly increase the likelihood that unintended pregnancies are carried to term. By combining incentives for institutional delivery with performance-based payments that encourage ASHAs to maintain regular contact with pregnant women and promote safe motherhood, the program could lower the perceived financial and medical risks of childbirth and, in doing so, frame pregnancy as something to be safeguarded and successfully completed under supervision. This framing may sensitize pregnancy by heightening its emotional, moral, and social salience, and could make termination appear less acceptable, leading to an increase in the number of unintended pregnancies to be carried to term.

Figure 1.2 illustrates the pathways through which JSY may influence reproductive outcomes, highlighting how its emphasis on planned and protected pregnancy links program design to changes in fertility and unplanned births.

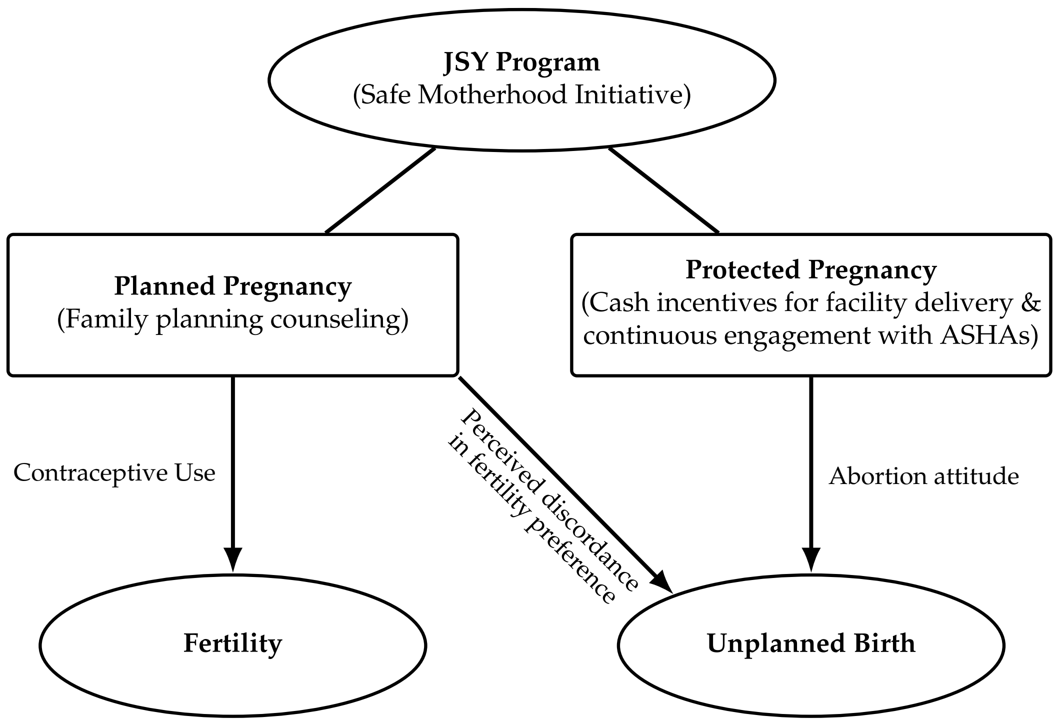
Taken together, this study examines three primary research questions:

- **Hypothesis 1:** How does fertility respond to the availability of maternal health services under the JSY program?
- **Hypothesis 2:** Does the provision of maternal health services under the JSY program lead to an increase in unplanned births?
- **Hypothesis 3:** If so, is this effect primarily driven by health worker engagement?

1.4 Data and Empirical Strategy

The primary data source is the National Family Health Survey (NFHS) of India, specifically waves 2 (1998–99), 3 (2005–06), and 4 (2015–16). The NFHS is a nationally representative household survey that provides detailed information on fertility, reproductive health, and maternal and child health outcomes. These three survey rounds span both the periods before and after the introduction of India's Janani Suraksha Yojana (JSY) program in 2005, allowing for an examination of changes in reproductive behavior associated with this program.

Figure 1.2: Conceptual Framework Linking JSY Components to Reproductive Decisions



The analysis focuses on rural areas, which were the primary target of JSY and received higher cash incentives for both mothers and Accredited Social Health Activists (ASHAs). The main sample for the H1 fertility analysis consists of women aged 19 to 40, capturing the key childbearing years, who live in rural areas, are above the poverty threshold, and do not belong to Scheduled Castes or Scheduled Tribes[4].

For the H2 unplanned birth analysis, I focus on women directly eligible for JSY benefits. Because the program provides cash transfers only to those who give birth, exposure can be observed only among women with at least one live birth during the post-treatment period. Limiting the analysis to women with recent births ensures the sample includes those for whom the program was operationally relevant, allowing for a more precise esti-

[4]In NFHS-2, where data on Below Poverty Line (BPL) card ownership are unavailable, the bottom 20% of the household wealth index is used as a proxy for BPL status.

mate of JSY's impact.[5] To capture this, I define a recent birth cohort as women who gave birth within the five years preceding the survey. This sample, like that used in the H1 fertility analysis, is restricted to rural women who are above the poverty line and not from Scheduled Castes or Tribes.

For both the H1 (fertility) and H2 (unplanned births) analyses, I implement a difference in differences (DiD) design that exploits variation in program eligibility across states and socio-economic groups, along with the timing of the program's launch. The treatment group consists of women in LPS who are not eligible for JSY in HPS. Specifically, these are women who are above the poverty line and do not belong to Scheduled Castes (SC) or Scheduled Tribes (ST) groups as discussed in Section 3.2. The control group includes women with the same socio-economic characteristics residing in HPS.

I estimate the following equation to estimate the impact of the JSY program on fertility:

$$Y_{isy} = \alpha_0 + \alpha_1 \text{Treat}_{is} \times \text{Post}_y + X'_{iy}\boldsymbol{\alpha} + \zeta_y + \lambda_s + \epsilon_{isy} \tag{1.1}$$

where the dependent variable Y_{isy} is Num_Birth_{isy}, defined as the number of births a woman i in state s had in the five years preceding the survey interview in year y,[6] or $Contracept_{isy}$, a binary indicator for modern contraceptive use that equals one if a woman i reports using a modern method of contraception in survey year y, and zero otherwise.

The key independent variable, $\text{Treat}_{is} \times \text{Post}_y$, is a dummy indicating whether the woman resides in LPS and whether the outcome is defined over a period after the launch of the JSY program. The vector X_{iy} includes woman i's age, years of education, partner's education level, number of children over age five, religion (indicators for Muslim and

[5] Potential selection bias from conditioning on recent births is addressed in the robustness checks section, where I examine maternal compositional changes after treatment and also replicate the main results using a broader sample that includes all women, regardless of birth status.

[6] For NFHS-3 (2005–06), special care is needed because the survey period itself overlaps with the program's launch in April 2005, although most fertility behavior it records predates JSY. To isolate pre-JSY fertility behavior, births resulting from conceptions after April 2005 are excluded. Assuming a nine-month gestation period, this window includes births conceived between April 2000 and March 2005.

Hindu), household head's age, and an indicator for whether the household is female-headed. Fixed effects for year of interview (ζ_y) and state (λ_s) are included. Standard errors are clustered at the state level, given that the JSY program was implemented at the state level.

Then, I estimate the following specification to test Hypothesis 2 using the recent-birth cohort:

$$Unplanned\_bir_{ibsty} = \beta_0 + \beta_1 \text{Treat}_{is} \times \text{Post}_t + X'_{iy}\boldsymbol{\beta} + \theta_b + \eta_t + \lambda_s + \zeta_y + u_{ibsty} \tag{1.2}$$

Here, $Unplanned\_bir_{ibsty}$ is a binary indicator for whether woman i's most recent birth was unplanned, where b denotes the birth order and t the year of birth. A birth is defined as unplanned if its birth order exceeds the woman's reported ideal number of children. The ideal number is based on her response to the question: *"If you could go back to the time you did not have any children and could choose exactly the number of children to have in your whole life, how many would that be?"* If the birth order of the most recent child exceeds this stated ideal, the indicator is set to one; otherwise, it is coded as zero.[7]

$\text{Treat}_{is} \times \text{Post}_t$ equals one for women in the treatment group whose most recent birth was conceived after the launch of the JSY program in 2005. The regression includes fixed effects for birth order (θ_b), birth year (η_t), state (λ_s), and interview year (ζ_y). Standard errors are clustered at the state level.

Appendix Table C.II.1 presents summary statistics disaggregated by treatment and control groups in the pre- and post-JSY periods. Panel A reports statistics for the H1 fertility analysis sample, and Panel B for the H2 unplanned birth analysis sample, which is limited to women who gave birth within the past five years.

[7]In a robustness check using the full sample of women, including those who have never given birth, women with no births during this period are also coded as zero, consistent with defining unplanned birth conditional on having given birth.

1.5 Main Results

1.5.1 Results for H1 Fertility Analysis

Table 1.2 reports estimates from Equation 1.1. Column (1) presents results for all women aged 19 to 40, while Columns (2) and (3) report separate estimates for younger women aged 19 to 29 and older women aged 30 to 40.

Table 1.2: The Impact of JSY on the Number of Births

	Number of births over the past 5 years		
	(1)	(2)	(3)
	All women	Young women (19-29)	Older women (30-40)
Treat × Post	-0.034	0.007	-0.100***
	(0.029)	(0.048)	(0.029)
Control mean (pre)	0.666	0.975	0.351
Observations	140832	70821	70011
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children over age five, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for year of interview and state. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

The results suggest that access to maternal and child health (MCH) services under JSY had different effects across age groups. Among older women, program exposure is associated with a reduction of 0.10 births over the past five years, significant at the 1 percent level. This decline corresponds to a 28 percent reduction relative to the pre-treatment mean in high-performing states (HPS). In contrast, there is no statistically significant change in fertility among younger women.

The observed fertility decline among older women is consistent with the intended role of JSY health workers in promoting family planning. As discussed in Section 1.3, ASHAs, while primarily tasked with encouraging institutional delivery, also distribute contraceptives and provide counseling on birth spacing and fertility limitation (Saxena et al., 2018, 2021; Moughalian et al., 2024). These responsibilities position them to reduce informational and access barriers to contraception, particularly for women who have completed or nearly completed their desired fertility.

If this mechanism is at play, we would expect to see an increase in contraceptive use following JSY exposure, particularly among older women who exhibit fertility reductions. To test this, I estimate JSY's effect on current use of modern contraceptive methods, using the same difference-in-differences strategy as in the fertility analysis (Equation 1.1).[8]

Table 1.3 presents results separately by age group. Among older women, JSY exposure leads to a significant increase in modern contraceptive use. This pattern supports the interpretation that health worker engagement under JSY enabled women nearing their desired family size to adopt fertility-limiting methods, contributing to the observed decline in recent births.

Among younger women, contraceptive use also increases, yet fertility remains unchanged. One explanation is that younger women primarily adopt contraception to space rather than to limit childbearing (Westoff and Koffman, 2010; Timæus and Moultrie, 2020). Another possibility is that JSY influenced post-conception decisions by heightening the salience of pregnancy. These changes may have increased the likelihood that unintended pregnancies were carried to term, offsetting any fertility-reducing effects of contraception. The next section investigates this channel by examining whether JSY affects the probability that unintended pregnancies result in live births, especially among younger women.

[8]This analysis uses data from NFHS waves 2 and 4. Wave 3 is excluded because its interview period overlaps with the rollout of JSY, making it difficult to consistently classify treatment status for contemporaneous outcomes, unlike in the fertility analysis based on retrospective birth histories.

Table 1.3: The Impact of JSY on Modern Contraceptive Use

	Currently using modern contraceptive method		
	(1)	(2)	(3)
	All women	Younger (19-29)	Older (30-40)
Treat × Post	0.063**	0.070**	0.069*
	(0.031)	(0.032)	(0.036)
Control mean (pre)	0.505	0.375	0.644
Observations	122002	61151	60851
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for year of interview and state. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

1.5.2 Results for H2 Unplanned Births Analysis

Table 1.4 reports the estimated effects of JSY on unplanned births. Columns (1)–(3) compare the post-program period to the pre-program period using a single post-treatment indicator. Columns (4)–(6) divide the post-program period into an early diffusion phase (2005–2008), before the 2009 reform that introduced ASHA cash incentives in HPS, and a later diffusion phase (2009–2016). Thus, the key variables of interest are $Treat \times Post$, $Treat \times Post(2005–2008)$, and $Treat \times Post(2009–2016)$.[9]

The estimates indicate that JSY increased unplanned births. In Column (1), program exposure raises the probability of having an unplanned birth by 3.2 percentage points, significant at the 10 percent level. Column (4) shows that exposure during the early diffusion period increases this probability by 4.3 percentage points (significant at the 1 percent level), while the estimate for the later period is 3.2 percentage points (significant at the 10

[9]Note that ASHA incentives were part of JSY from the start in LPS, and the 2009 reform mainly extended them to HPS. Accordingly, there is no strong expectation that late-period effects should be larger for the treatment group defined in LPS.

percent level).

The heterogeneity analysis by age indicates that this increase is concentrated among younger women. Columns (2) and (3) report that program exposure raises the probability of an unplanned birth among younger women by 4.2 percentage points, significant at the 1 percent level, with no statistically significant effect observed among older women. In Columns (5) and (6), both the early and later diffusion phases of the program significantly increase unplanned births among younger women, but not among older women.

Table 1.4: The Impact of JSY on Unplanned Births

	Unplanned birth					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Younger	Older	All	Younger	Older
Treat × Post	0.032*	0.042***	0.020			
	(0.017)	(0.015)	(0.029)			
Treat × Post (2005-2008)				0.043***	0.056***	-0.001
				(0.015)	(0.016)	(0.032)
Treat × Post (2009-2016)				0.032*	0.042**	0.021
				(0.017)	(0.015)	(0.030)
Control mean (pre)	0.278	0.205	0.507	0.278	0.205	0.507
Observations	63213	46465	16748	63213	46465	16748
Fixed effects	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Note: The dependent variable is an indicator for whether a woman's most recent birth in the past five years was unplanned. The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for birth year, birth order, interview year, and state. Standard errors are clustered at the state level and reported in parentheses. Significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

The rise in unplanned births among younger women, together with the null effect on fertility in Section 1.5.1, helps explain why fertility did not decline in this group despite increased contraceptive use. These results are consistent with more unintended pregnancies resulting in births. Among older women, fertility declined and unplanned births did not rise, consistent with improved fertility control in this group.

A potential concern is that this analysis conditions on having given birth in the past five years, which could be problematic if the composition of younger women who give birth changes in treated states. Differences at the extensive margin are addressed in Table 1.2, which shows no change in the number of births among younger mothers. However, there may still be changes in the composition of mothers in treated areas. This concern is partially addressed in Section 1.9 through a robustness check that examines shifts in maternal characteristics, following Bossavie et al. (2023), and by replicating the analysis using the full sample of ever-married women, including those who have never given birth.

In sum, these results suggest that maternal and child health (MCH) services under JSY affected fertility behavior differently by age, increasing unplanned births among younger women while improving fertility control among older women. In the next section, I test for pre-trends to assess the validity of the difference-in-differences design. I then isolate the health worker channel to better understand the mechanism underlying these effects.

1.6 Validation of Empirical Design

The empirical strategy in this paper relies on the Difference-in-Differences (DiD) approach. A key identifying assumption is that, in the absence of the program, the treatment and control groups would have followed parallel trends. In this section, I assess this assumption by examining whether the number of births and the probability of unplanned births evolved similarly between treatment and control groups in the pre-treatment period.

1.6.1 Test for Parallel Pre-Trends in Fertility Analysis (H1)

I begin by testing for parallel pre-trends in fertility outcomes. Following Griffith and Noonen (2022), I estimate a regression in Equation 1.3 using data from the pre-treatment period. The dependent variable is the number of births a woman reports in the five years prior to the interview. Year_y denotes the year of interview and accounts for common time trends across states. The interaction term between treatment status and interview year ($\text{Treat}_{is} \times \text{Year}_y$) captures any differential trends between the treated and control groups before the program. If the parallel trend holds, the interaction should be statistically insignificant ($\tilde{\alpha}_2 = 0$). The estimating equation is as follows:

$$Num\_Births_{isy} = \tilde{\alpha}_0 + \tilde{\alpha}_1 \text{Year}_y + \tilde{\alpha}_2 \text{Treat}_{is} \times \text{Year}_y + X'_{iy}\tilde{\boldsymbol{\alpha}} + \lambda_s + \epsilon_{isy}, \tag{1.3}$$

In Appendix Table A.II.4, estimates of Equation 1.3 indicate that the null of parallel pretrends is rejected at the 10% level across specifications.

The rejection of parallel pretrends appears driven by Uttar Pradesh (UP). As India's most populous state, UP exhibited fertility rates substantially above the national average in the pre-JSY period. The state's total fertility rate stood at 4.0 in NFHS-2 (1998–99) and 3.8 in NFHS-3 (2005–06)—the highest or near-highest among major states (Halli et al., 2019; Ministry of Finance, 2025). This elevated baseline, combined with UP's substantial weight in the sample, could have made the linear pre-trend specification particularly sensitive to this state's trajectory. To assess robustness, I re-estimate Equation 1.3 excluding UP. The pre-trend test no longer rejects at conventional significance levels for the remaining states (Appendix Table A.II.5).

As an additional robustness check for violations of the parallel trends assumption, I implement the sensitivity check proposed by Kahn-Lang and Lang (2020), following the approach of Griffith and Noonen (2022). The method allows for differential linear trends between treatment and control groups in the pre-period and examines whether a treatment effect remains after extrapolating these trends into the post-period. Specifically, I estimate:

$$Num\_Births_{isy} = \breve{\alpha}_0 + \breve{\alpha}_1 \text{Year}_y + \breve{\alpha}_2 \text{Treat}_{is} \times \text{Year}_y + \breve{\alpha}_3 \text{Treat}_{is} \times \text{Post}_y + X'_{iy}\breve{\boldsymbol{\alpha}} + \lambda_s + \epsilon_{isy} \tag{1.4}$$

In this specification, $(\breve{\alpha}_1 + \breve{\alpha}_2)$ captures the trends in treated states. This trend is then extended into the post-treatment period to estimate what would have happened in treated states had the program not been implemented. The coefficient $\breve{\alpha}_3$ measures how much actual outcomes in treated states differ from this projected trend. A statistically significant $\breve{\alpha}_3$ indicates that the treatment effect still exist.

The results from the sensitivity analysis in Table 1.5 remain qualitatively consistent with the main DiD estimates reported in Table 1.2. Once differential pre-treatment trends are extrapolated into the post-period, the estimated effect of JSY on fertility among older women remains large and statistically significant. Specifically, the magnitude increases from a 0.10 to a 0.372 reduction in births, suggesting that the original estimate is not driven by modest trend differences. In contrast, for younger women, the treatment effect remains close to zero and statistically insignificant across both specifications. These findings reinforce the interpretation that JSY reduced fertility among older women, while having no measurable effect on younger women's childbearing.

1.6.2 Test for Parallel Pre-Trends in Unplanned Birth Analysis (H2)

Next, I test for pre-treatment trends in unplanned births using the specification in Equation 1.5 on the pre-treatment sample. In this regression, the time variable is the year of birth, and treatment status is interacted with birth year to assess whether treated and control groups exhibited differential trends prior to JSY. The model includes standard controls, along with fixed effects for birth order (θ_b), state (λ_s), and interview year (ζ_y), as in the main analysis.

The estimating equation is:

$$Unplanned\_bir_{ibsty} = \tilde{\beta}_0 + \tilde{\beta}_1 \text{Birth\_year}_t + \tilde{\beta}_2 (\text{Treat}_s \times \text{Birth\_year}_t) + X'_{iy}\tilde{\boldsymbol{\beta}} + \theta_b + \zeta_y + \lambda_s + u_{ibsty} \tag{1.5}$$

Table 1.6 reports the estimates. Across all groups, the interaction terms are statistically insignificant, consistent with the parallel trends assumption in unplanned birth outcomes

Table 1.5: Regressions with Non-Parallel Trends: Following Kahn-Lang and Lang

	Number of births over the past 5 years		
	(1)	(2)	(3)
	All women	Younger (19-29)	Older (30-40)
Year of Interview	-0.007***	-0.010***	-0.005***
	(0.001)	(0.002)	(0.001)
Year of Interview × Treat	0.011**	0.003	0.019***
	(0.004)	(0.006)	(0.003)
Treat × Post	-0.184***	-0.019	-0.372***
	(0.053)	(0.075)	(0.040)
Control mean (pre)	0.666	0.975	0.351
Observations	140832	70821	70011
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children over age five, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include state fixed effects. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

during the pre-treatment period.

Event-Study for Nonlinear Pre-Trends. To complement the linear trend test, I also examine potential non-linear pre-treatment dynamics. I estimate an event-study specification restricted to pre-treatment birth years, following Autor (2003), Borusyak et al. (2024), Bossavie et al. (2023), and Salemi (2021). The model interacts treatment status with year-of-birth indicators, using 2004 as the omitted reference year. If trends were parallel, the coefficients on pre-treatment interactions (1994–2003) should be small and statistically insignificant.

The estimating equation is:

$$Unplanned\_bir_{ibsty} = \breve{\beta}_0 + \sum_{\tau \neq 2004} \breve{\beta}_\tau(\text{Treat}_{is} \times \text{Birth\_year}_{t=\tau}) + X'_{it}\breve{\boldsymbol{\beta}} + \theta_b + \lambda_s + \zeta_y + u_{ibsty} \tag{1.6}$$

where $\breve{\beta}_\tau$ captures the difference in outcomes between treated and control groups for each birth cohort τ relative to 2004.

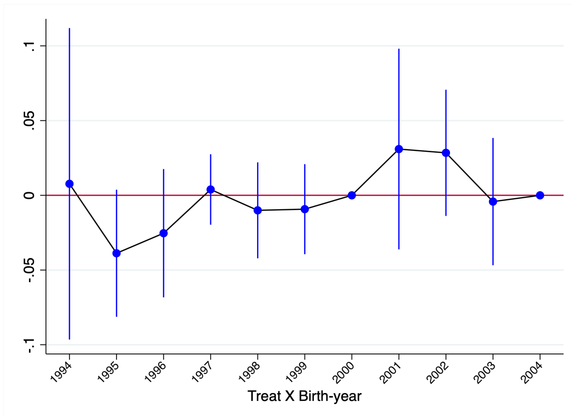
Table 1.6: Pre-Treatment Trends in Unplanned Births

	Unplanned birth		
	(1)	(2)	(3)
	All	Younger	Older
Year of Birth	0.001	-0.003	0.010
	(0.003)	(0.003)	(0.007)
Year of Birth × Treat	0.003	0.002	0.004
	(0.003)	(0.002)	(0.005)
Control mean	0.278	0.205	0.507
Observations	19026	14161	4865
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

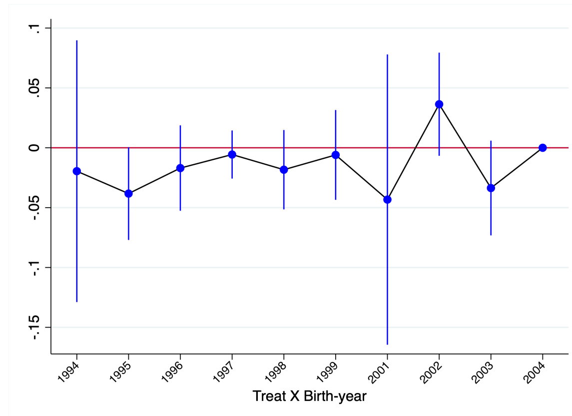
Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for birth order, interview year, and state. Standard errors are clustered at the state level and reported in parentheses. Significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

Panels A–C of Figure 1.3 plot the estimated coefficients with 90% confidence intervals separately for all women, younger women, and older women. Pre-treatment estimates are small in magnitude and statistically indistinguishable from zero across most birth-year cohorts, providing visual support for the parallel trends assumption.

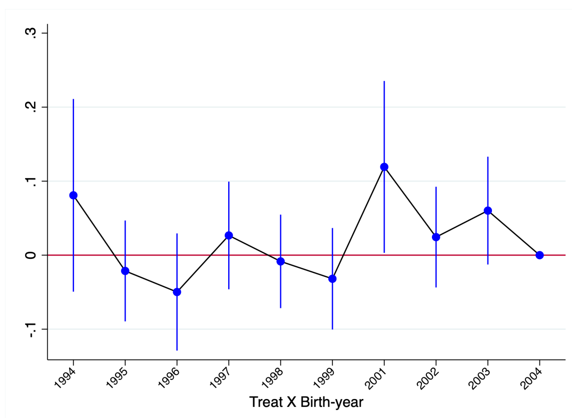
Figure 1.3: Event-study for pre-treatment trends



Panel A: PTA - All women



Panel B: PTA - Younger women



Panel C: PTA - Older women

To formally test this, I follow Borusyak et al. (2024) and Salemi (2021) in assessing whether the pre-treatment coefficients are jointly significant. Appendix Table A.II.6 reports the corresponding F-statistics and *p*-values. The results fail to reject parallel pre-trends for the full sample and the younger cohort. However, the test performs less well in the older cohort, likely due to smaller sample size and limited variation across pre-policy cohorts in that subgroup.

1.7 Mechanism Analysis: Health Worker Channel

This section studies whether increased engagement with health workers can account for the rise in unplanned births among young women. I leverage a 2009 reform of JSY that introduced performance-based incentives for ASHAs in HPS. The reform provides quasi-experimental variation in health-worker engagement to isolate its causal effect on reproductive outcomes.

1.7.1 Did the Reform Increase Exposure to ASHAs?

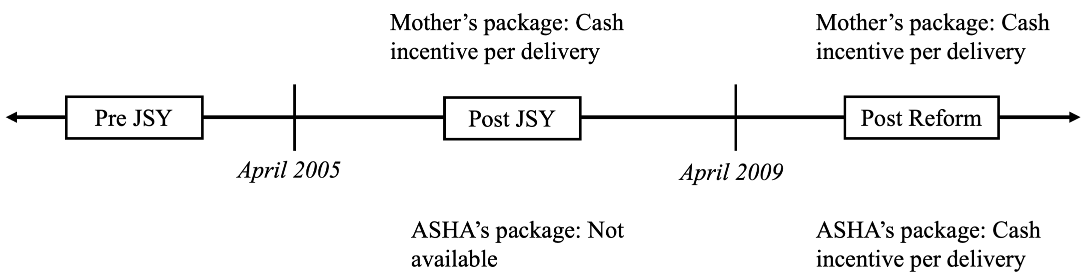
Figure 1.4 presents the timeline of incentive changes under JSY in HPS and LPS. As shown in the figure, cash incentives to mothers were introduced in April 2005, while performance-based incentives for ASHAs were extended to HPS only in April 2009. Before this reform, ASHAs in LPS were already incentivized, but those in HPS were not. After 2009, ASHAs in HPS became eligible for payments per institutional delivery, likely increasing their interactions with pregnant women.

To verify that the 2009 reform increased interactions with health workers among younger women, I use data from the District Level Household and Facility Survey (DLHS) round 4 (2012–13). This survey includes births spanning both the period immediately after JSY's initial launch and the years following the 2009 reform, providing a natural setting to assess changes in exposure to health workers.

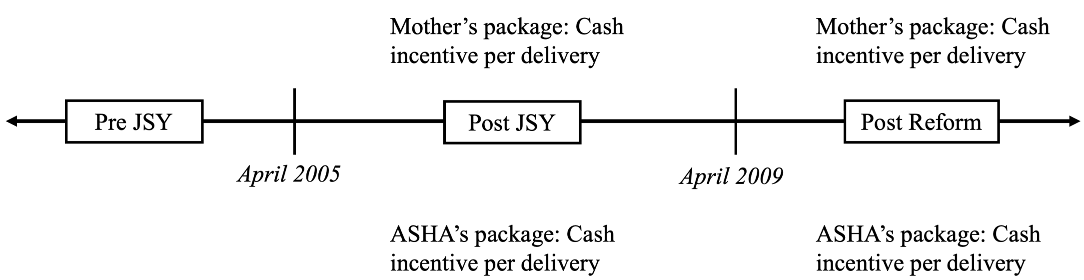
DLHS-4 collects retrospective information on ASHA interactions related to pregnancy, delivery, and postnatal care. I define a woman as exposed to an ASHA if she reports any of the following[10]: (i) receiving antenatal care from an ASHA, (ii) being assisted by an ASHA during facility delivery, (iii) receiving transport support from an ASHA, or (iv) being informed by an ASHA about danger signs related to diarrhea or pneumonia. This composite measure captures a broad range of ASHA engagement within the maternal and

[10] While the measure is described in terms of ASHA workers, it also includes interactions with Anganwadi workers, consistent with JSY program guidelines that permit either type of frontline worker to provide maternal and child health services.

Figure 1.4: Timeline of Incentive Changes under JSY



Panel A: High Performing States



Panel B: Low Performing States

child health services. The sample is restricted to younger women aged 19 to 29 who meet JSY eligibility criteria in HPS. Eligibility in HPS is defined as being below the poverty line or belonging to a Scheduled Caste or Tribe, and having no more than two previous live births. Appendix Table A.II.2 presents summary statistics for this sample.

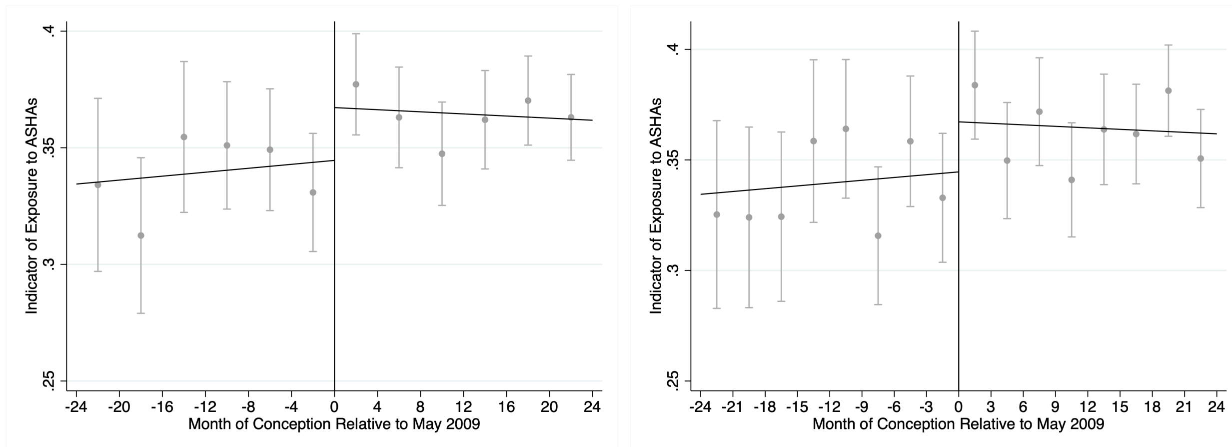
Appendix Figure A.III.1 presents average ASHA exposure by estimated conception cohort, using four-month intervals (Panel A) and three-month intervals (Panel B). Conception dates are calculated as nine months prior to the reported birth month to approximate gestational timing. All panels in Appendix Figure A.III.1 shows a clear increase in ASHA exposure beginning in May 2009, coinciding closely with the April 2009 policy implementation. In Panel A, Between January 2007 and April 2009, average exposure fluctuated between 31.2% and 35.5%. After the reform, it rose to between 33.4% and 38.4%, with a notable increase from 33.1% in Jan 2009 to 38.4% in May 2009. A similar upward shift is evident in Panel B. Based on this visual evidence, I define May 2009 as the empirical cutoff in the analysis.

Figure 1.5 illustrates this discontinuity graphically. Panel A divides the sample into four-month bins on each side of the cutoff, while Panel B uses three-month bins. Each dot represents the average exposure within a bin, the solid lines show local linear fits estimated separately before and after the cutoff, and the vertical bars indicate 90 percent confidence intervals. Both panels reveal a visible upward shift in exposure at the May 2009 threshold.

To estimate the effect of the reform on ASHA exposure, I implement a sharp regression discontinuity (RD) design centered at the May 2009 conception threshold, following the approach in Sun and Zhao (2016). The running variable is the month of conception, and the outcome is the ASHA exposure indicator. The sharp RD framework is appropriate because the policy was implemented simultaneously across all HPS around the threshold.

The RD parameter is defined as:

Figure 1.5: ASHA Exposure around the 2009 Reform



Panel A: 4-Month Bins around Cutoff

Panel B: 3-Month Bins around Cutoff

$$\tau_{RD} = \lim_{c \uparrow l^*} E[exposure_i \mid c_i = l^*] - \lim_{c \downarrow l^*} E[exposure_i \mid c_i = l^*] \tag{1.7}$$

where c_i denotes the conception date and l^* represents the reform threshold in May 2009. I estimate this parameter using local linear regressions, implementing multiple specifications that vary by bandwidth selection (MSERD and CERRD)[11] and kernel function (uniform and triangular), as recommended by Calonico et al. (2014) and Cattaneo et al. (2020). These alternative specifications allow for sensitivity checks around the choice of smoothing and weighting.

As an additional sensitivity check, I estimate a pooled OLS specification with an RD

[11]The mean squared error–optimal selector (MSERD) minimizes mean squared error and is well suited for point estimation. The coverage error–optimal selector (CERRD) is designed for robust bias-corrected confidence intervals, delivering faster coverage error decay rates (Calonico et al., 2017).

structure using the full sample, without restricting the bandwidth:

$$\text{Exposure}_i = \beta_0 + \beta_1 \mathbb{1}(c_i \geq l^*) + f(c_i - l^*) + \mathbb{1}(c_i \geq l^*) \times f(c_i - l^*) + \varepsilon_i,$$

where $\mathbb{1}(c_i \geq l^*)$ is an indicator for conception at or after the May 2009 threshold, and $f(c_i - l^*)$ denotes a linear polynomial in the number of months relative to the cutoff.

Table 1.7 reports estimates from the regression discontinuity (RD) analysis. Columns (1) through (4) present RD estimates using local linear regressions with different bandwidth selection methods and kernel functions. Specifically, Columns (1) and (2) use the MSERD criterion with uniform and triangular kernels, while Columns (3) and (4) use the CERRD criterion. Column (5) reports a pooled OLS estimate using the full sample as a robustness check. All standard errors are clustered by month of conception.

Across all RD specifications, the results consistently indicate a significant increase in ASHA exposure following the 2009 reform. The estimated effects range from 5.0 to 7.2 percentage points and are statistically significant at the 1 percent level. The OLS estimate in Column (5) also shows a significant increase of 4.0 percentage points, significant at the 5 percent level, providing further support for the main findings.

1.7.2 Effect of Health-Worker Engagement on Unplanned Births

To explore whether greater health worker engagement, as shown in the previous section, contributes to unplanned births, I use data from the second round (2011–12) of the India Human Development Survey (IHDS). IHDS-II is a nationally representative panel survey of over 42,000 households across India, collecting detailed household- and individual-level information on health, education, economic conditions, and fertility behavior, including ideal family size and gender preferences, similar to the NFHS.

I construct a birth-level sample of rural births conceived in or after January 2005, as these conceptions could plausibly have been influenced by the program. The unit of observation is a birth, and the dataset includes multiple births per woman. Following the

Table 1.7: The Impact of the 2009 Reform on Health Worker Access

	Exposure to ASHAs (d)				
	(1)	(2)	(3)	(4)	(5)
RD estimate	0.050***	0.067***	0.063***	0.072***	
	(0.015)	(0.013)	(0.022)	(0.011)	
OLS estimate					0.040**
					(0.012)
Bandwidth (months)	8	8	7	7	
BW selection	MSERD	MSERD	CERRD	CERRD	
Kernel	Uniform	Trinangular	Uniform	Triangular	
Observations	4541	4541	3927	3927	21500

Note: The sample includes younger rural women who are either from Scheduled Castes or Tribes (SC/ST) or below the poverty line (BPL), and who had a pregnancy in the past five years. It is further restricted to those with fewer than three births, reflecting JSY eligibility criteria in high-performing states (HPS). Standard errors, clustered at the month of conception level, are reported in parentheses. Significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

approach used in the main analysis, a birth is defined as unplanned if its birth order exceeds the mother's stated ideal number of children.

Panel C of Appendix Table C.II.1 provides summary statistics for the sample used in the health worker channel analysis (H3). The treatment group consists of births from rural women residing in HPS, while the control group includes those in LPS.

To identify this effect, I implement a secondary difference-in-differences (DiD) strategy. The treatment group consists of JSY-eligible women in HPS who gained exposure to incentivized ASHAs after April 2009. The control group includes comparable women in LPS, who were exposed to incentivized ASHAs both before and after the reform. Because the only policy change affecting the treatment group was the introduction of ASHA incentives, this comparison isolates the incentive-driven effect of health workers.

The estimating equation is as follows:

$$Unplanned\_Bir_{cbmst} = \gamma_0 + \gamma_1 \mathrm{HPS}_{mst} \times \mathrm{Post}_t + X'_{cbhst}\gamma + \eta_t + \theta_b + \mu_m + \omega_{cbmst} \quad (1.8)$$

where $Unplanned\_Bir_{cbhst}$ is an indicator for whether birth c of birth order b from mother m in state s at time t was unplanned. The interaction term $\text{HPS}_{mst} \times \text{Post}_t$ captures exposure to incentivized ASHAs. The specification includes fixed effects for birth year (η_t), birth order (θ_b), and mother (μ_m). Standard errors are clustered at the state level.

The results show that increased engagement with health workers through performance-based incentives increased the likelihood of unplanned births, with effects concentrated among younger women. Table 1.8 reports the estimates from regression 1.8. Column (1) shows that the likelihood of an unplanned birth significantly rises by 4.1 percentage points following the introduction of ASHA incentives. Columns (2) and (3) show that this effect is driven by younger women, among whom the probability increases by 5 percentage points (significant at the 5% level). No statistically significant effect is observed for older women.

These results closely mirror the main findings in Table 1.4, where the overall JSY program also increased unplanned births, particularly among younger women. Taken together with the evidence in Section 1.7.1 that the new incentives expanded women's access to ASHAs, these results suggest that increased health worker engagement is an important channel through which the JSY program affected unplanned births. The following section provides suggestive evidence on the behavioral responses to health worker engagement that may underlie this channel.

1.8 Behavioral Responses Underlying the Health Worker Channel

This section explores potential mechanisms through which access to health workers may have contributed to the observed increase in unplanned births. Specifically, I test whether increased exposure to health workers affected (i) women's perceived alignment of fertility preferences within couples and (ii) attitudes toward abortion.

Table 1.8: The Impact of Health Worker Access on Unplanned Births

	Unplanned birth		
	(1)	(2)	(3)
	All	Younger	Older
HPS × Post	0.041**	0.050**	0.010
	(0.018)	(0.019)	(0.045)
Control mean (pre)	0.329	0.120	0.566
Observations	4726	3192	1532
Fixed effects	✓	✓	✓

Note: The dependent variable is an indicator for whether the birth was unplanned. The data come from IHDS-II (2011–12). The sample is restricted to births to rural women from Scheduled Castes or Tribes or those below the poverty line. All regressions include fixed effects for birth year, birth order, state, and mother. Standard errors, clustered at the state level, are reported in parentheses. Significance is indicated as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

1.8.1 Perceived Discordance in Fertility Preferences

This subsection investigates whether increased access to community health workers heightened women's perception of discordance in fertility preferences within their households. Perceived discordance arises when a woman believes that her own fertility intentions differ from those of her husband, specifically when she wants to stop childbearing but thinks her husband wants more children. In male-dominated contexts, including India, reproductive outcomes tend to align with men's preferences (Komura, 2013; Doepke and Tertilt, 2018; Mishra and Parasnis, 2021; Thomson, 1997). As a result, women who perceive such misalignment may be unable to act on their own fertility intentions, resulting in outcomes that reflect their husbands' preferences rather than their own.

This mechanism is particularly relevant in the context of JSY, which relies on ASHAs to bridge the public health system and households. While ASHAs are primarily tasked with promoting institutional delivery, they also counsel women on family planning and birth spacing (Ministry of Health and Family Welfare, Government of India, 2006). Through repeated contact, these interactions may lead women to update their own fertility preferences and become more inclined to stop childbearing, while continuing to believe that their husbands still want additional children.

Such asymmetric belief updating can increase the share of women who perceive that they do not want more children whereas their husbands do, reinforcing perceived discordance within couples. When women have limited autonomy over reproductive decisions, this belief gap can constrain their ability to act on their own intentions once conception occurs, increasing the likelihood that unintended pregnancies are carried to term.

To empirically assess this mechanism, I use panel data from rounds 1 and 2 of the India Human Development Survey (IHDS). This analysis is conducted at the woman level, using the subset of women from the IHDS round 2 birth sample (used in the analysis of the H3 health worker channel) who were also surveyed in round 1. I restrict the sample to women who were interviewed after April 2005 in IHDS1, ensuring that all observations were collected after the launch of JSY, and again in IHDS2 (2011–12), after the program's expansion in 2009. This panel allows me to examine within-woman changes in fertility preferences during a period of increasing access to health workers.

Both rounds of IHDS include questions on fertility intentions, asking whether the woman wants to have more children and whether she believes her husband wants more children than they currently have. These questions make it possible to see how women's fertility preferences changed and how their views of their husbands' preferences shifted as health worker access expanded. Based on these responses, I create three binary variables: whether the woman wants more children (*Wife_wants*), whether she thinks her husband wants more (*Husband_wants*), and whether she thinks her husband wants more while she does not (*Husband_wants_more*), capturing perceived discordance in fertility preferences.[12]

[12] Appendix Table A.II.3 provides an overview of the datasets and main outcome variables used in the analyses.

The empirical specification is as follows:

$$y_{mst} = \delta_0 + \delta_1 \text{HPS}_{ms} \times \text{Post}_t + X'_{mst}\delta + \lambda_s + \mu_m + \epsilon_{mst} \quad (1.9)$$

where y_{mst} denotes outcomes for individual m in state s at time t, including the woman's desire for more children, her perceived husband's desire, and the perceived-discordance indicator *husb_want_more*. Post_t equals one for interviews conducted after April 2009, when ASHA incentives were expanded to High-Performing States (HPS). HPS_{ms} equals one if the respondent resides in an HPS. The interaction term captures the differential change in outcomes in HPS following the reform. The model includes woman and state fixed effects, and standard errors are clustered at the state level.

Table 1.9 reports the estimated effects of health-worker access on perceived fertility preferences. Among younger women (Panel B), exposure to the JSY reform leads to a significant decline in women's own stated desire for more children, by 19.6 percentage points (significant at the 10% level), and a significant rise of 7.3 percentage points in the share of women who report that they do not want more children but believe their husbands do. No comparable effects are observed among older women (Panel C), further underscoring that the effects of JSY on unplanned births are concentrated among younger women.

These results are consistent with the perceived-discordance mechanism. Exposure to ASHAs appears to have shifted women's own fertility preferences without changing how they perceive their husbands' preferences, thereby widening the perceived gap within couples. In male-dominated contexts, such belief gaps can constrain women's ability to act on their own reproductive intentions and help explain the rise in unplanned births among younger women documented in Section 1.7.

1.8.2 Attitudes Toward Abortion

Another potential mechanism linking increased access to health workers with higher unplanned fertility is a shift in abortion-related attitudes. This channel may operate through

Table 1.9: The Impact of Health Worker Access on Perceived Discordance in Fertility Preferences

	Husband–Wife desire for more children		
	(1)	(2)	(3)
	Wife wants	Husband wants	Husband wants more
Panel A: All women			
HPS × Post	-0.169*	-0.115	0.027
	(0.086)	(0.085)	(0.036)
Control mean (pre)	0.551	0.564	0.037
Observations	690	690	690
Panel B: Younger women			
HPS × Post	-0.196*	-0.082	0.073*
	(0.112)	(0.112)	(0.040)
Control mean (pre)	0.725	0.754	0.049
Observations	276	276	276
Panel C: Older women			
HPS × Post	-0.124	-0.121	-0.023
	(0.105)	(0.115)	(0.035)
Control mean (pre)	0.459	0.462	0.030
Observations	414	414	414
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The dependent variable in Columns (1) and (2) indicates whether the wife or her husband (as perceived by the wife) reports a desire for more children. Column (3) is an indicator for perceived spousal discordance in fertility preferences, equal to one if the wife reports not wanting more children but believes her husband does. The data come from the balanced panel of IHDS-I (2005–06) and IHDS-II (2011–12). Panel A includes the sample of rural women from Scheduled Castes or Tribes, or those below the poverty line. Panel B restricts the sample to younger women aged 19–29, and Panel C to older women aged 30–40. All regressions control for the number of children, an indicator for current pregnancy, the woman's age and education, and her husband's education. State and individual fixed effects are included. Standard errors, clustered at the state level, are reported in parentheses. Significance is indicated as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

several pathways. First, repeated engagement with community health workers may increase the perceived moral or social cost of abortion. Studies show that health workers often hold conservative views on abortion and can influence perceptions through sustained interactions that reinforce stigma, in India (Nandagiri, 2019; Sunil, 2022) as well as in other low- and middle-income countries (Glenton et al., 2017).

Even when they do not personally hold such conservative views, the performance-based incentive structure of JSY may create financial motives for ASHAs to discourage abortion. According to Javadekar et al. (2025), ASHAs who receive payments conditional on institutional delivery may dissuade households from terminating pregnancies.

Additionally, qualitative evidence from India documents women's concerns about maintaining confidentiality regarding abortion when interacting with community health workers (Gupta et al., 2017). Under JSY, ASHAs are expected to know about women's pregnancies. The very fact that health workers are aware of a pregnancy may make households reluctant to consider abortion, as the decision may no longer remain private.

To examine whether abortion attitudes shifted following the JSY reform, I use data from the World Values Survey (WVS) waves 4 (2001), 5 (2006), and 6 (2012). The WVS includes a question on abortion attitudes: *"How justifiable do you consider abortion to be?"* with responses ranging from 1 (Never justifiable) to 10 (Always justifiable). Based on this question, I construct a standardized measure reflecting respondents' attitudes toward abortion. The sample includes both men and women and is not restricted by age, as spousal age gaps are common in India, and broader shifts in abortion attitudes may influence fertility outcomes among younger women.

I exploit the phased rollout of JSY, which introduced cash incentives to mothers beginning in 2005 and expanded payments to health workers in 2009 in high-performing states (HPS). To estimate changes in abortion attitudes associated with program exposure, I implement a triple-differences (DDD) strategy that compares outcomes across both states (HPS versus LPS) and three time periods: before JSY (pre-2005), during the initial rollout (2005–2008), and after the expansion of ASHA incentives (2009 onward).

The estimating equation is as follows:

$$
\begin{aligned}
Z\_JustAbortion_{ist} = \gamma_0 + \gamma_1 \text{HPS}_{is} \times \text{Post (2005-2008)}_t + \gamma_2 \text{HPS}_{is} \times \text{Post (2009-)}_t \\
+ X_{ist} + \delta_t + \lambda_s + \epsilon_{ist} \qquad (1.10)
\end{aligned}
$$

In this equation, $Z\_JustAbortion_{ist}$ is the standardized measure of abortion justification for individual i in state s at time t. The variable HPS_{is} is an indicator for whether the individual resides in a high-performing state. Post $(2005\text{-}2008)_t$ is an indicator for the early JSY diffusion period (2005-2008), while Post $(2009\text{–})_t$ represents the later diffusion phase following the 2009 reform. The term X_{ist} represents a vector of individual-level control variables. δ_t and λ_s denote year and state fixed effects, respectively. Standard errors are clustered at the state level and denoted by ϵ_{ist}. The coefficient of interest, γ_2, captures the change in attitudes toward abortion associated with increased access to health workers during the later diffusion period in high-performing states.

Table 1.10 presents the estimated effects of JSY on attitudes toward abortion. The results indicate that increased exposure to health workers following the 2009 reform is consistently associated with more conservative views on abortion. The effect is particularly pronounced among men. The estimates suggest that the reform reduced abortion acceptability by 0.43 standard deviations among men, with smaller and statistically insignificant effects for women. In contrast, the earlier phase of the program (2005–2008), which introduced cash incentives for mothers but did not directly incentivize health workers, shows no significant association with abortion attitudes.

This pattern implies that the messaging and interactions facilitated by ASHAs may have diffused through households and communities, shaping broader social norms around abortion. The results further suggest that when abortion is framed as morally or socially unacceptable, men may internalize these messages more strongly in patriarchal settings. Since men often hold greater influence over fertility decisions in such settings, such shifts in male attitudes could help explain the rise in unplanned births documented in Sec-

Table 1.10: The Impact of JSY on Abortion Attitudes Among Women and Men

	Abortion Acceptability (z-score)		
	(1)	(2)	(3)
	All	Women	Men
HPS × Post (2005-2008)	0.277	0.006	0.421
	(0.570)	(0.512)	(0.595)
HPS × Post (2009-)	-0.377	-0.325	-0.425*
	(0.236)	(0.289)	(0.220)
HPS	0.189	0.165	0.213
	(0.205)	(0.220)	(0.206)
Control mean (pre)	-0.000	-0.000	0.000
Observations	4161	1901	2260
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The dependent variable is a standardized index measuring attitudes toward abortion. The data come from the World Values Survey (WVS), waves 4 (2001), 5 (2006), and 6 (2012). The sample includes rural women and men from lower socioeconomic backgrounds, defined based on indicators of lower caste status, low income, and residence in areas with fewer than 10,000 residents. Column (2) restricts the sample to women, and Column (3) to men. All regressions control for age, education, and number of children. Interview year and state fixed effects are included. Standard errors, clustered at the state level, are reported in parentheses. Significance is indicated as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

tion 1.7.

These findings should be interpreted with caution because of limitations in the WVS data. The sample is not restricted to individuals who recently experienced a pregnancy or birth, which may weaken the link between stated attitudes and actual fertility behavior. As a result, the estimates may capture attitudes at a broader level rather than directly reflecting behavioral responses to the program.

1.9 Robustness: Testing for Selection Bias

A potential concern with the main analysis in Table 1.4 is that it conditions on women who gave birth in the past five years. If JSY influenced fertility behavior, this restriction may introduce selection bias. Specifically, if the program affected who gives birth, comparisons limited to ever-mothers may conflate treatment effects with shifts in the composition of mothers.

As discussed in Section 1.5.2, this concern is partly mitigated by the earlier finding in Table 1.2, which shows no statistically significant change in overall fertility among younger women—the group where the increase in unplanned births occurred. This suggests that the rise in unplanned births is unlikely to be driven by an increase in the number of births. Nonetheless, it remains important to assess whether changes in the composition of women who give birth, that is, selection on the intensive margin, could bias the estimates.

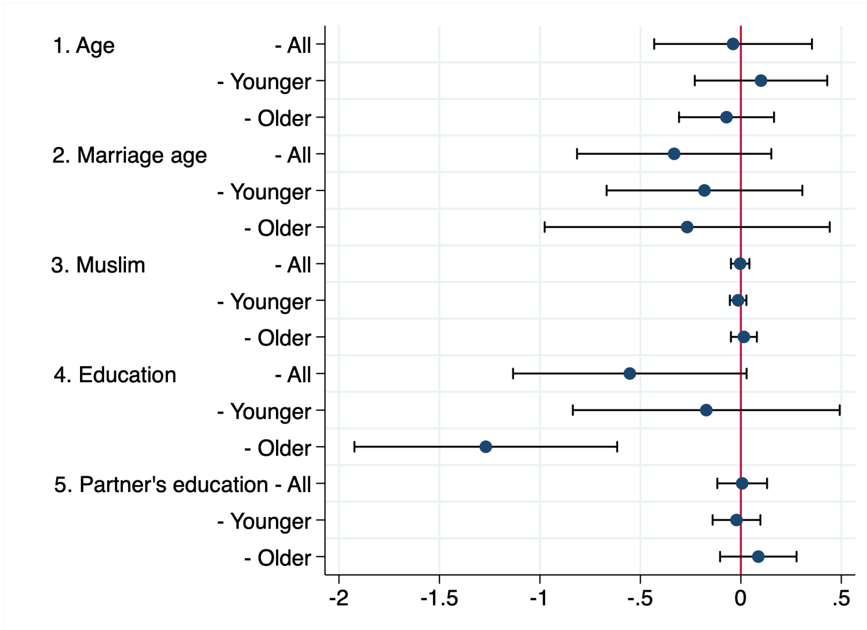
As a first robustness check, I examine changes in observable maternal characteristics. I follow the approach in Bossavie et al. (2023), estimating DiD regressions for five key covariates: age, age at marriage, religion, education, and partner's education. This strategy tests whether the observable profile of mothers changed across treatment and control groups after JSY implementation.[13]

Figure 1.6 plots the estimated effects of JSY on key maternal characteristics using a DiD specification. Across most outcomes—age, age at marriage, religion, education, and partner's education—estimates are small and statistically insignificant. Notably, point estimates are near zero for religion and partner's education across all age groups. Education shows a slight decline among older women, but this group had largely completed schooling well before JSY was introduced, making it unlikely that the program directly affected their education. By contrast, no such shifts are evident among younger women, for whom education levels remain stable. Overall, there is no evidence of systematic changes in observable characteristics among younger women, suggesting that selection on observables is unlikely to bias the main results.

However, changes in unobserved characteristics may still bias the estimates. To ad-

[13]The premise is based on the assumption articulated by Altonji et al. (2005) and Oster (2019), that changes in unobservable characteristics are likely to be correlated with changes in observable ones.

Figure 1.6: Compositional changes in mothers, post JSY



Note: This figure plots estimates of the effect of JSY exposure on maternal characteristics, by age group. Each coefficient corresponds to the interaction between treatment status and the post-treatment period. All regressions include birth year, birth order, interview year, and state fixed effects as in the main analysis. Standard errors are clustered at the state level.

dress this concern, I conduct a second robustness check using the full sample of ever-married women. Specifically, I replicate Equation 1.2 on all ever-married women, regardless of whether they gave birth in the past five years. The dependent variable is an indicator for whether the respondent experienced an unplanned birth during that period, and women who did not give birth are coded as having zero unplanned births. This approach avoids conditioning on childbirth and allows estimation of the average treatment effect across the entire population of ever-married women.[14]

One complication arises with the use of NFHS Wave 3 (2005–06), which was fielded

[14]As in the main analysis, these regressions include birth order fixed effects. For women with no live births (i.e., never-mothers), birth order is coded as zero so that these cases are consistently incorporated in the fixed effect structure.

shortly after JSY was introduced. In the birth-only sample, exposure can be assigned based on the timing of the birth. However, for women who did not give birth, treatment status cannot be clearly assigned, because although their date of interview falls after the launch of JSY, most of the five-year recall period still falls in the pre-JSY era. As a result, it is difficult to consistently define pre- and post-treatment periods for these women.

To ensure a clean comparison, I restrict the sample to NFHS Wave 2 (1998–99), which predates JSY, and Wave 4 (2015–16), where the entire five-year exposure window falls within the post-JSY period.

Table 1.11 reports the results. The estimates confirm the robustness of the main findings in Table 1.4. For the full sample in Column (1), the estimated effect is small and statistically indistinguishable from zero. In contrast, Column (2) shows that JSY significantly increases the likelihood of unplanned births among younger women. These results underscore that the findings are not an artifact of conditioning on childbirth.

Table 1.11: The Impact of JSY on Unplanned Births: Results from Full Sample

	Unplanned birth			
	(1)	(2)	(3)	(4)
			Younger	Younger
	All	Younger	& Early marriage	& More daughters
Treat × Post	-0.008	0.031***	0.044**	0.057***
	(0.009)	(0.011)	(0.021)	(0.018)
Control mean (pre)	0.139	0.149	0.243	0.252
Observations	122002	61150	11093	17465
Fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Note: The dependent variable is an indicator for whether a woman's most recent birth in the past five years was unplanned. The sample includes all ever-married women from NFHS Waves 2 (1998–99) and 4 (2015–16), including both ever-mothers and never-mothers. All models include fixed effects for interview year, state, and birth order (with never-mothers and childless women coded as birth order 0). Column (1) reports estimates for the full sample; column (2) restricts to younger women; column (3) further restricts to younger women who married before age 16; and column (4) restricts to younger women whose daughters outnumber sons. Standard errors are clustered at the state level and reported in parentheses. Significance is indicated as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

To probe this further, Columns (3) and (4) restrict the sample to younger women with plausibly lower intrahousehold bargaining power—those who married before age 16 and those with more daughters than sons.[15] The estimated effects in these subgroups are larger than those in Column (2), suggesting that JSY had an especially pronounced impact where women's ability to assert their fertility preferences was most limited. This pattern directly aligns with the mechanism in Section 1.8.1, which documents that the JSY program increased perceived discordance in fertility preferences between husbands and wives. When such perceived discordance arises, women with weaker bargaining power are less able to act on their own fertility intentions and are more likely to defer to what they believe their husbands prefer, increasing the likelihood that unintended pregnancies are carried to term. The stronger effects observed in these mechanism-relevant subgroups therefore provide additional evidence that the main results operate through spousal preference perceptions rather than by conditioning on childbirth.

1.10 Conclusion

This paper documents an unintended consequence of expanding maternal health services on unplanned fertility, using evidence from India's Janani Suraksha Yojana (JSY). While the program reduced fertility among older women, it simultaneously increased unplanned births among younger women in rural areas. Mechanism analysis shows that improved access to community health workers in the program is a key channel. These interactions make women more likely to believe that they wish to stop childbearing while their husbands do not. In male-dominated settings, this perceived discordance can limit women's ability to act on their reproductive intentions. They also shift attitudes against abortion, increasing the likelihood that unintended pregnancies are carried to term. These findings show that health system interventions that expand institutional deliveries and family planning can unintentionally change reproductive behavior.

[15] Evidence indicates that early marriage and having fewer sons reduce women's bargaining power within households (Li and Wu, 2011; Tauseef and Sufian, 2024).

These findings have important implications for policy design. Maternal and child health interventions aim to improve maternal survival by promoting institutional delivery and antenatal care. However, their incentive structure can unintentionally increase unplanned births. Shifting from payment-per-delivery incentives for health workers to incentive designs that also support women's reproductive autonomy may help limit these unintended effects while maintaining health gains. In addition, providing services such as maternal mental health care and early childhood support may help mitigate the negative impacts of unplanned births.

Future research should aim to disentangle the separate effects of conditional cash transfers to mothers and improved access to health workers on fertility intentions and unplanned births. It should also examine longer-term consequences of unplanned births, including maternal mental health, child health, and human capital accumulation. This would help clarify the full welfare implications of maternal health interventions in low-income settings.

Chapter 2

HOT WEATHER, HOT TEMPERS: THE IMPACT OF TEMPERATURE ANOMALIES ON INTIMATE PARTNER VIOLENCE IN PERU

2.1 Introduction

As awareness of climate change continues to grow, the influence of high temperatures on human societies is becoming increasingly pronounced. An expanding body of research underscores the impacts of such climate shocks on various aspects of human life, including income, health, and education (Carleton and Hsiang, 2016; Wu et al., 2016). These impacts also extend to agricultural and labor productivity (Deschênes and Greenstone, 2007; Somanathan et al., 2021) and even crime and human conflict (Ranson, 2014; Hsiang et al., 2013).

The negative effects of extreme heat are particularly severe in low-income countries (IPCC, 2014; Hallegatte et al., 2016). This vulnerability is due to limited infrastructure to mitigate the effects of extreme heat and a lack of access to cooling appliances. Furthermore, while social protection programs in low-income countries often assist victims of natural disasters, support specifically for victims of extreme heat is scarce, further increasing their vulnerability (Baez et al., 2017).

The disproportionate impact of temperature anomalies also arises within households, exposing women to heightened risks of intimate partner violence. Increased stress and tension from anomalously hot days can exacerbate conflicts, particularly in households where social norms and gender roles create a significant gender gap between men and women. Economic pressures related to temperature anomalies, such as reduced agricultural and labor productivity, can lead to greater financial strain on families, further intensifying these conflicts. Understanding how temperature anomalies contribute to intimate partner violence is essential for addressing the specific vulnerabilities that women face during these climate events.

This paper examines the causal relationship between temperature anomalies and intimate partner violence (IPV) in Peru. I focus on the immediate behavioral response to high temperature shocks and explore the pathways that may mediate this relationship. The analysis draws on administrative data from Peru's national domestic violence helpline (Línea 100), measured as the number of calls per 10,000 residents in each province. Temperature anomalies are measured as the monthly count of anomalously hot days, where a hot day is one on which the daily maximum temperature exceeds the province-specific calendar-month long-run mean (1985–2014) by at least 3°C (details in Section 2.3). I further explore heterogeneity across urban and rural settings, classifying a province as urban if a majority of residents live in urbanized areas according to the national census.

My empirical approach relies on matching helpline call volumes to weather conditions at the province–month level. I construct a panel dataset combining geo-coded temperature data with IPV helpline calls from 2015 to 2022, and I apply regression models with province-by-month and year-by-month fixed effects that exploit variation in weather conditions within a particular location and calendar month while absorbing time-invariant seasonal patterns at the province level and common national shocks. Such variation is considered essentially random (Dell et al., 2014; Hsiang, 2016), because, for example, I am comparing IPV occurrences in a relatively warm January in a specific province with outcomes in a relatively cool January in that same province.

I find that anomalously hot days lead to a prompt rise in intimate partner violence in Peru. The effect is concentrated in urban areas: in urban provinces, the total effect of anomalously hot days on IPV helpline calls is 0.031 calls per 10,000 people, representing a 1.7 percent increase relative to the monthly mean of 1.801 calls per 10,000, and this estimate is robust across all four fixed-effect specifications. In rural provinces, the estimated effect is small and statistically insignificant.

Section 2.2 develops a conceptual framework that outlines four potential pathways through which exposure to temperature anomalies could influence IPV—income and intra-household bargaining, partner co-location and exposure, psychological stress, and community violence—and discusses why these channels are expected to operate more strongly in urban settings. The empirical investigation of these channels identifies psychological stress among male partners and community violence as the primary drivers of the urban effect, while the co-location channel is ruled out by the mobility evidence and the income channel operates through urban women's labor supply response rather than aggregate income changes.

This study makes a primary contribution by using administrative helpline call data, which provide a more precise and timely measure of IPV incidents. Prior work largely relies on Demographic and Health Surveys (DHS), which record whether IPV occurred at any point in the past 12 months but not when within that period. This design makes it impossible to link incidents to specific weather shocks, and the retrospective format can also introduce recall bias, as respondents may disproportionately emphasize recent events. Moreover, survey responses depend on individuals' willingness to disclose sensitive experiences, which may vary systematically with stigma or social norms. In contrast, monthly helpline calls capture IPV as it is reported, enabling closer alignment with short-term temperature shocks and reducing concerns about recall and disclosure bias.

This study also contributes by providing new evidence on the short-term impacts of temperature anomalies on intimate partner violence in a low-income country setting. While much prior work has examined rainfall shocks and long-term income channels (Sekhri and Storeygard, 2014; Abiona and Koppensteiner, 2021; Díaz and Saldarriaga, 2023; Zhu et al., 2023), only a small body of research addresses temperature shocks (Sekhri and Storeygard, 2014; Abiona and Koppensteiner, 2021; Díaz and Saldarriaga, 2023; Bollman et al., 2023), and these studies typically rely on annually averaged temperatures to evaluate cumulative effects on IPV. Such long-term approaches may obscure the acute impacts of temperature anomalies, as lower-temperature days reduce the overall average. Recent research on short-term fluctuations has largely focused on high-income contexts such as the United States (Henke and chi Hsu, 2020). By leveraging daily temperature variation and monthly IPV helpline call data in Peru, this paper provides novel evidence on how acute temperature shocks affect IPV in a setting where household vulnerability and institutional capacity differ sharply from high-income countries.

Additionally, this study contributes by identifying a particularly vulnerable subgroup with heightened exposure to temperature risks: the urban population. To the best of my knowledge, it is the first to establish a causal relationship between urban temperature risks and IPV in low-income countries. While previous studies, such as those by Bollman et al. (2023) and Díaz and Saldarriaga (2023), have primarily focused on rural areas and linked increases in IPV to poverty exacerbated by climate change, their findings may not fully capture the experiences of the entire population. By including data from all provinces and considering both rural and urban areas in Peru, this research reveals how temperature risks differentially impact IPV in these settings.

The remainder of the paper is organized as follows. Section 2.2 presents the conceptual framework. Section 2.3 outlines the data sources and variable construction. Sections 2.4 present the main results and heterogeneity analysis. Section 2.5 examines potential mechanisms and provides supporting evidence. Section 2.6 reports robustness checks. Section 2.7 concludes.

2.2 Conceptual Framework: Pathways from Heat to IPV

To guide the empirical analysis, this section outlines the pathways through which acute temperature shocks may translate into intimate partner violence and discusses why these channels are expected to operate more strongly in urban settings. I consider four potential mechanisms: income and intra-household bargaining, partner co-location and exposure, psychological stress, and community violence.

The first pathway operates through income and intra-household bargaining. A large literature documents that negative income shocks shift bargaining power within the household, increasing women's vulnerability to abuse (Aizer, 2010; Anderberg et al., 2016).

Extreme heat suppresses labor supply and earnings through reduced physical work capacity and absenteeism (Somanathan et al., 2021; Graff Zivin and Neidell, 2014). Crucially, whether this translates into a shift in intra-household bargaining power depends not on whether total household income falls, but on whether the shock falls asymmetrically across partners. A large literature establishes that it is the identity of who earns income rather than its household-level total that determines bargaining power (Lundberg and Pollak, 1993; Duflo, 2003): income controlled by women raises their outside options and reduces abuse risk (Aizer, 2010), while income losses concentrated among women weaken their relative position (Anderberg et al., 2016).

The income channel posits that anomalously hot days reduce household resources, generating financial stress that increases conflict and violence within the household (Fox et al., 2002; Schuler et al., 2011; Díaz and Saldarriaga, 2023). Under intra-household bargaining models, a deterioration in women's earnings relative to men's weakens women's outside options and raises their vulnerability to IPV (Heath, 2014; Angelucci and Heath, 2020). I test this channel at two levels: aggregate household income and individual labor market outcomes by gender.

In urban Peru, there are reasons to expect the burden to fall disproportionately on women. Urban women's employment is concentrated in occupations where earnings depend on daily physical presence and market transactions—including retail trade, domestic service, market vending, and food preparation (Instituto Nacional de Estadística e Informática, 2019; Beltrán et al., 2025).[1] On days when extreme heat reduces worker productivity and suppresses economic activity, earnings in these occupations fall directly and immediately (Somanathan et al., 2021; Graff Zivin and Neidell, 2014). Urban men, by contrast, are more likely to hold formal salaried positions in construction, manufacturing, or transport, where fixed contracts provide insulation from short-run weather variation (In-

[1] In urban Peru in 2018, 50.7 percent of employed women worked in services and 29.6 percent in commerce, together accounting for 80.3 percent of female urban employment. By contrast, construction and transport accounted for just 2.8 percent of female urban employment, compared with 28.7 percent among urban men (Instituto Nacional de Estadística e Informática, 2019).

stituto Nacional de Estadística e Informática, 2019; Beltrán et al., 2025; Instituto Nacional de Estadística e Informática, 2018).[2] This occupational gender segregation is sharper in cities than in rural areas, where both men and women are predominantly engaged in subsistence agriculture (Instituto Nacional de Estadística e Informática, 2019; Beltrán et al., 2025).[3] As a result, the same temperature anomaly is predicted to cause a larger and more consistent decline in women's earnings, narrowing their relative economic position and weakening their bargaining power—a pattern the empirical results are consistent with.

The second channel concerns partner co-location and exposure. Research on the COVID-19 pandemic demonstrates that stay-at-home orders increased the amount of time partners spent together, raising IPV risk through greater proximity and reduced opportunities to escape a violent home environment (Leslie and Wilson, 2020; Bullinger et al., 2021; Agüero, 2021). By analogy, one might expect extreme heat to keep individuals indoors, thereby increasing co-location and the likelihood of conflict. Whether this mechanism operates in practice, however, depends critically on the availability of residential cooling. In settings where air conditioning is rare, indoor environments may become less tolerable than outdoor spaces during a heat shock, inducing households to seek relief outdoors rather than retreating inside. Peru is classified among countries at high risk from inadequate access to cooling (Sustainable Energy for All, 2025), suggesting that the co-location mechanism may not hold in this context. Consistent with this argument, the empirical evidence indicates that hot days increase time spent outdoors and reduce residential dwell-time, which lowers rather than raises partners' exposure to each other, providing little support for co-location as a driver of the observed increase in IPV calls.

It bears noting, however, that the observed increase in outdoor mobility may reflect

[2] In 2017, 58.0 percent of workers in transport and communications and 56.6 percent in manufacturing held formal employment contracts, compared with 24.4 percent in hotels and restaurants and 47.3 percent in commerce—the sectors where urban women are predominantly employed (Instituto Nacional de Estadística e Informática, 2018).

[3] In rural Peru in 2018, 70.0 percent of employed women and 78.1 percent of employed men worked in agriculture (Instituto Nacional de Estadística e Informática, 2019), reflecting the similar exposure to weather risk assumed in the text.

not only heat-avoidance behavior but also increased social activity on warm days. To the extent that warm-weather socializing facilitates alcohol consumption, which is positively associated with IPV (Angelucci, 2008; Foran and O'Leary, 2008; Cohen and Gonzalez, 2024; Díaz and Saldarriaga, 2023), this behavioral pathway may represent an additional channel linking heat exposure to IPV risk. The empirical evidence does not find a significant effect of extreme heat days on alcohol consumption, suggesting that this social activity pathway does not operate as a meaningful mediator in the current context.

The third pathway operates through the psychological effects of heat. Experimental evidence in psychology shows that rising ambient temperatures suppress serotonin levels, worsening mood and psychological wellbeing, which in turn elevates aggressive behaviors (Anderson, 2001; Pietrini et al., 2000; Moore et al., 2002). Consistent with this, economics literature shows that elevated mental distress increases the propensity for conflict and violence (Card and Dahl, 2011; Cardazzi et al., 2024; Marcotte and Markowitz, 2011). Thus, heat exposure is predicted to increase IPV through its deteriorating effect on psychological wellbeing. Urban residents tend to face greater thermal stress than their rural counterparts due to the urban heat island effect (Manoli et al., 2019), limited access to green space (Cano et al., 2025), and greater reliance on indoor work environments that constrain the outdoor behavioral adaptation documented in the mobility channel above, making this channel more likely to operate there.

The fourth potential pathway runs through community-level violence and social norms. Prior research documents that extreme heat is associated with increased street crime and interpersonal violence (Ranson, 2014; Hsiang et al., 2013), and exposure to such violence may lower the perceived social cost of household aggression as community violence normalizes violent behavior over time (Mattina, 2017; Gutierrez and Gallegos, 2016). The concentration of economic activity and social interaction in urban settings suggests this channel may be more pronounced in densely populated areas, a prediction examined empirically in Section 2.5.4.

Of the four channels, the income and bargaining power pathway and the psychological stress pathway are predicted to operate most strongly in urban Peru, where occupational gender segregation may magnify the asymmetric income shock and where the urban environment may amplify thermal stress and constrain behavioral adaptation. The community violence channel may also be more active in urban settings, where the concentration of social activity and higher baseline crime rates create greater scope for spillovers from community to household violence. Section 2.5 presents evidence on each channel and evaluates which mechanisms are supported in the Peruvian context.

2.3 Data and Variables

2.3.1 Intimate Partner Violence

Information on IPV comes from the Ministry of Women and Vulnerable Populations (Ministerio de la Mujer y Poblaciones Vulnerables, MIMP). MIMP operates domestic violence helpline called Línea 100. Línea 100 is a free 24-hour service, specialized in free telephone assistance nationwide in Peru. It provides information, guidance, counseling and emotional support in multiple languages for people affected by acts of violence against women and members of the family group or who know of a case (https://www.gob.pe). In addition to counseling, Línea 100 staff can refer cases to specialized services such as emergency centers for women (Centros de Emergencia Mujer, CEM), and in severe cases they can coordinate with the police or legal services. Thus, the helpline data likely reflect a wide range of IPV incidents, from those seeking emotional support to those requiring legal or protective intervention. Information on calls is publicly available on the website of the MIMP and is updated monthly.

I use the monthly volume of helpline calls for each province from 2015 to 2022.[4][5] The primary outcome of interest is the monthly number of helpline calls per 10,000 people for each province. MIMP provides the total number of calls for each province, and I divide

[4]The years 2020 and 2021 are excluded due to the significant impact of COVID-19.

[5]Peru is administratively divided into 25 regions (departments), which are further subdivided into 196 provinces and 1,869 districts. This study primarily uses data at the provincial level.

the number of calls by the total population of the province, with population data obtained from the 2007 Peruvian Census.

2.3.2 *Weather*

My primary weather data source is the ERA5 reanalysis dataset from the European Centre for Medium-Range Weather Forecasts, which provides hourly global temperature and precipitation data at a spatial resolution of 0.25×0.25 degrees (approximately 27.5×27.5 kilometers at the equator) covering the period from 1983 to 2022.

I construct province-level daily climate variables by matching each district to its nearest ERA5 grid cell and aggregating to the province level using district population weights. For each province p on date t, the population-weighted daily maximum temperature is

$$\text{Temp}_{p,t} = \frac{\sum_{d \in p} \text{pop}_d \cdot \text{Temp}_{d,t}}{\sum_{d \in p} \text{pop}_d},$$

where d indexes districts within province p and pop_d denotes district population from the 2007 Peruvian Census.[6] This approach ensures that measured temperature exposure reflects the conditions experienced by the resident population rather than an unweighted spatial average.

I define extreme heat and cold days relative to province- and calendar-month-specific long-run means computed over the baseline period from 1983 to 2012, which predates the sample period and ensures the thresholds are not contaminated by the variation being studied. For each province p and calendar month m, the long-run mean maximum temperature $\bar{T}_{p,m}$ is the average daily maximum temperature observed in month m across all years in the baseline. A day is classified as an extreme heat (cold) day if its population-weighted daily maximum temperature in province p exceeds (falls below) $\bar{T}_{p,m}$ by at least

[6]For the 74 districts established after the 2007 Census, I use 2017 Census population as a fallback.

3°C. My main temperature variables, *Num High Days* and *Num Low Days*, count the number of extreme heat and cold days, respectively, in each province $\times$ year $\times$ month cell.

Province- and calendar-month-specific thresholds are necessary given Peru's substantial geographic heterogeneity in temperature. In any given month, long-run mean maximum temperatures span from below 10°C in Andean highland provinces (for example, Junín at 11.0°C in January and Puno at 13.2°C) to above 30°C in coastal and jungle provinces such as Piura (32.2°C) and Loreto (30.3°C), with a cross-province standard deviation of 6.6°C. A single national threshold would define "extreme" relative to a benchmark that is climatologically irrelevant for most individual provinces.

The choice of a 3°C deviation is grounded in the local variability of daily temperatures. The within-province $\times$ calendar-month standard deviation of daily maximum temperature averages 1.56°C across all province-month cells over the baseline period, so a $\pm$3°C threshold corresponds to approximately 2 standard deviations above (or below) the local norm, capturing roughly the top 2–3 percent of the local daily temperature distribution. I verify robustness to alternative thresholds of $\pm$2°C and $\pm$4°C in the Appendix and find consistent results.

For the exposure channel analysis, which uses daily Google mobility data (Bollman et al., 2023), I employ the daily binary indicator directly (equal to one if the population-weighted daily maximum temperature in the province exceeds $\bar{T}_{p,m}$ by at least 3°C, and zero otherwise), rather than the monthly count.

Additionally, I explore an alternative nonlinear specification following Mullins and White, 2019. I group daily maximum temperatures for each province into 5°C-wide bins ranging from $<$ 10°C to $>$ 30°C, and count the number of days each month falling into each bin.

I also construct monthly rainfall controls from the same ERA5 dataset using the same population-weighted aggregation procedure. For each province and month, I sum daily rainfall to obtain the monthly total. I compute province- and calendar-month-specific z-scores relative to the 1983–2012 baseline mean and standard deviation, and define binary indicators for high and low rainfall shocks as months in which the z-score exceeds 1 or falls below -1, respectively. These rainfall shock indicators are included as controls in all regressions to isolate the effects of temperature from concurrent precipitation shocks.

Summary statistics for IPV helpline calls and each weather variable are reported in Appendix Table B.I.1. Panel A of Figure 2.1 illustrates the average number of extreme heat days per month from 2015 to 2022, defined as days on which the population-weighted daily maximum temperature exceeds the province- and calendar-month-specific long-run mean by at least 3°C, while Panel B depicts the average monthly number of helpline calls per 10,000 residents across all provinces over the sample period. Triangles on each map represent metropolitan areas with populations exceeding 300,000. Panel B shows that helpline calls are not always most frequent in urban areas. This suggests that helpline data capture IPV occurrences beyond metropolitan areas, making it a suitable measure for analyzing the impact of heat waves on IPV across different regions rather than being limited to urban settings.

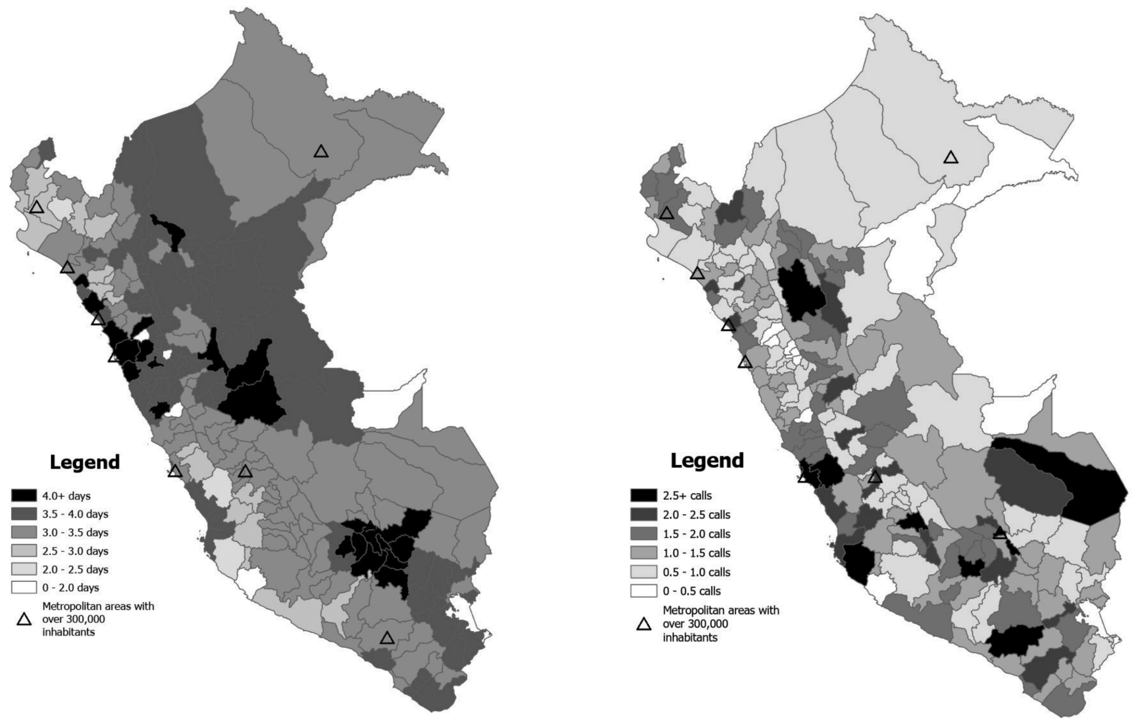
2.3.3 Channel Outcome Variables

Income

For the income channel, I utilize the Peru National Household Survey (ENAHO), a longitudinal panel survey conducted annually from 2016 to 2018 covering a representative sample of the national population.[7] ENAHO provides detailed information on employment, income, expenditures, and household demographics. Given the study's focus on short-term effects of heat shocks, ENAHO is well-suited to this analysis: income components are reported for the previous month (or shorter recall periods), making them directly comparable to monthly temperature variation.

At the individual level, I construct two labor income measures from ENAHO. *Total*

[7] Further details are available at: https://ghdx.healthdata.org/record/peru-national-household-survey-2016



Panel A: Heat Wave Map

Panel B: IPV Map

Figure 2.1: Heat Waves and IPV Maps

labor income is the sum of gross earnings from the respondent's main and secondary occupations, covering both wage employment and self-employment, expressed as a monthly figure. *Net labor income* is the sum of net earnings after mandatory deductions from all occupations: main wage employment, main self-employment, secondary wage employment, and secondary self-employment. I apply a $\log(x+1)$ transformation to each measure to accommodate zero values.

At the household level, I use three additional measures from ENAHO: log total household income (*Log net income*), log total household expenditure (*Log expenditure*), and non-labor income. Monthly values are obtained by dividing annual totals by twelve, and I apply a log transformation with a floor of one to handle small and zero values.

Because weather shocks are expected to affect agricultural households more strongly, I classify households into farm and non-farm groups. A household is coded as a farm household if at least one member is engaged in agriculture or fishery in any survey wave, either as a self-employed producer or as a wage laborer. All other households are classified as non-farm.

Exposure

To explore the exposure channel, I use two complementary data sources. First, I use province-level daily mobility data provided by Google, following Bollman et al., 2023. Google's Community Mobility Reports record percentage changes in visits to and time spent in five categorized place types relative to a pre-COVID baseline period: (i) retail and recreation, (ii) parks, (iii) transit stations, (iv) workplaces, and (v) residential areas.[8] I use data from October 16, 2021 to October 15, 2022, focusing on this later period to avoid distortions in mobility patterns caused by COVID-19-related restrictions during 2020 and early 2021. For province-date cells with missing values, which arise when the number of visitors is insufficient to preserve anonymity, I impute values as the midpoint between the maximum negative and minimum positive observed values for that province.

Second, I use ENAHO individual-level data to construct labor supply measures that capture time allocation more directly. *Worked last week* is a binary indicator equal to one if the individual worked during the reference week. *Wage worker* is a binary indicator equal to one if the individual is employed as a wage or salary employee and zero otherwise. *Hours worked* measures total hours in the reference week, computed as the sum of hours in the primary and secondary occupations, with zero assigned for non-workers. Together, these measures capture both the extensive and intensive margins of labor supply, which determine the time individuals spend outside the home and thus their exposure to ambi-

[8] Further details are available at: https://support.google.com/covid19-mobility/answer/9824897?hl=en&ref_topic=9822927&sjid=12464980794855916007-NC

ent temperature conditions.

Mental Health

To explore the mental health channel, I leverage the Peru Continuous Demographic and Health Survey (DHS) from 2013 to 2017. The DHS is a repeated cross-section survey conducted annually by the Peruvian National Bureau of Statistics, covering health outcomes and socio-demographic characteristics of individuals.

The DHS includes the Patient Health Questionnaire-9 (PHQ-9), a widely used instrument for screening and measuring mental distress. The PHQ-9 asks respondents to rate the frequency over the past two weeks of nine symptoms: (1) little interest or pleasure in activities, (2) feeling depressed or hopeless, (3) sleep difficulties, (4) fatigue or low energy, (5) appetite disturbance, (6) feelings of worthlessness or failure, (7) difficulty concentrating, (8) psychomotor disturbance, and (9) suicidal ideation. I construct a mental distress index for male spouses by standardizing each of the nine items to z-scores, averaging the nine standardized scores, and then standardizing the resulting average. The final index is a z-score with mean zero and standard deviation one, where higher values indicate greater mental distress.

Community Violence

Data on community violence are obtained from the Sistema de Denuncias de la Policía Nacional del Perú (SIDPOL), which contains police-reported crime records at the district level from 2018 to 2024. I aggregate these records to the provincial level by month and compute crime rates per 10,000 population. I construct five crime-rate outcomes. The broadest measure, *All Crimes*, counts all reported offenses. *General Safety Offenses* is a composite of three subcategories examined separately: *Public-Safety Crimes* (*delitos contra la seguridad pública*), *Crimes against Health and Body* (*delitos contra la vida, el cuerpo y la salud*), and *Crimes against Liberty* (*delitos contra la libertad*).

To assess whether temperature affects the propensity to report rather than the incidence of violence, I construct two additional outcomes from SIDPOL that have no theoretical link to heat-driven aggression: *Administrative Offenses* (*delitos contra la administración pública*), which counts crimes against public administration, and *Fraud* (*estafa* and *extorsión*), which counts reports of fraud and extortion. If heat shocks do not affect these non-violent categories while IPV-related calls increase, this supports the interpretation that the estimated effects reflect changes in violence incidence rather than changes in reporting behavior more generally.

Sub-samples: Urbanization

To estimate heterogeneous effects of heat waves by degree of urbanization, I use data from the 2007 Peruvian Census, which reports the share of the population living in urban areas for each province. I construct a binary variable, *urban*, equal to one for provinces with an urbanization rate above 34.3 percent (the sample median) and zero otherwise. For the province of Callao, where the 2007 Census did not report urbanization, I impute the value using data from the 2017 Census. Based on this classification, among the 190 provinces in my sample, 95 are categorized as urban and 95 as rural.[9]

2.4 Main Results

I examine the effect of anomalously hot days on IPV helpline calls across Peruvian provinces, using within-province variation in the monthly count of temperature anomalies as the identifying source of variation. I estimate the following baseline specification:

$$\begin{aligned}\text{IPV}_{p,m,y} = \alpha &+ \beta_1\, \mathit{HighTemp}_{p,m,y} + \beta_2\, \mathit{LowTemp}_{p,m,y} \\ &+ \gamma_1\, \text{HighRain}_{p,m,y} + \gamma_2\, \text{LowRain}_{p,m,y} + \mu_{m,y} + \delta_{pm} + \varepsilon_{p,m,y} \end{aligned} \tag{2.1}$$

[9] In a complementary analysis, I also use a continuous measure of urbanization and an alternative binary threshold at the 75th percentile (58.4 percent) to assess the robustness of the heterogeneity results.

where $\text{IPV}_{p,m,y}$ is the monthly number of helpline calls per 10,000 population in province p, month m, and year y. $HighTemp_{p,m,y}$ ($LowTemp_{p,m,y}$) is the high-temp (low-temp) shock, counting the number of days in the month on which the population-weighted daily maximum temperature exceeds (falls below) the province-specific calendar-month long-run mean by at least 3°C. $\text{HighRain}_{p,m,y}$ and $\text{LowRain}_{p,m,y}$ are binary indicators for high and low rainfall shocks, respectively. $\mu_{m,y}$ denotes year-by-month fixed effects and δ_{pm} denotes province-by-month fixed effects. Standard errors are clustered at the province level.

Table 2.1 reports estimates of β_1 and β_2 from four specifications of increasing stringency. Column (1) includes separate year, month, and province fixed effects. Column (2) replaces these with year-by-month and province fixed effects, absorbing all common national shocks at the monthly level. Column (3) adds province-by-year fixed effects. Column (4), the preferred specification, uses province-by-month fixed effects as the most stringent alternative. All specifications include controls for high and low rainfall shocks.

Panel A reports the average effect across all 190 provinces. The coefficient on the extreme heat shock is positive and statistically significant in columns (1) and (2), at 0.017 and 0.016 calls per 10,000 population, respectively. The estimate attenuates and becomes statistically insignificant in columns (3) and (4), indicating that the average effect masks heterogeneity across provinces that the stricter fixed effects absorb. The coefficient on the cold shock is positive but statistically insignificant across columns (2) through (4).

To examine heterogeneity by urbanization, I augment Eq. 2.1 with an interaction term:

$$\begin{aligned}\text{IPV}_{p,m,y} = \alpha &+ \beta_1\, HighTemp_{p,m,y} + \beta_2\,(HighTemp_{p,m,y} \times \text{Urban}_p)\\ &+ \beta_3\, LowTemp_{p,m,y} + \beta_4\,(LowTemp_{p,m,y} \times \text{Urban}_p)\\ &+ \mathbf{W}'_{p,m,y}\gamma + \mu_{m,y} + \delta_{pm} + \varepsilon_{p,m,y} \qquad (2.2)\end{aligned}$$

where Urban_p is a binary indicator equal to one for provinces with a 2007 Census urbanization rate above the sample median of 34.3 percent, and $\mathbf{W}_{p,m,y}$ is a vector of rainfall shock controls and their interactions with Urban_p. The total effect of an extreme heat day in urban provinces is $\beta_1 + \beta_2$.

Panel B reports estimates of Eq. 2.2. The interaction term β_2 is positive and statistically significant across all four specifications, with point estimates ranging from 0.020 to 0.034. The total urban effect $\beta_1 + \beta_2$ ranges from 0.019 to 0.031 calls per 10,000 people and is statistically significant at the 5 percent level in all specifications (p-values between 0.002 and 0.013). In the preferred specification of column (4), the urban total effect of 0.031 calls per 10,000 implies a 1.7 percent increase relative to the monthly mean of 1.801 calls per 10,000. The stability of this estimate across all four fixed-effect structures indicates that the finding is robust to the choice of specification and is not driven by unobserved province-level trends. Appendix Table B.I.2 further confirms that the urban effect is robust to alternative temperature anomaly definitions, including stricter and looser deviation thresholds and percentile-based measures. By contrast, the base coefficient β_1 in Panel B, the estimated effect for rural provinces, is small and statistically indistinguishable from zero across all specifications.

These results indicate that extreme heat raises IPV helpline calls primarily in urban provinces. Section 2.5 examines the mechanisms behind this urban concentration in detail.

2.5 Mechanism

In this section, I examine four potential channels through which exposure to temperature anomalies can affect IPV: income, exposure, mental health, and community violence. To identify the pathways that mediate the observed increase in IPV in Peru, I estimate the effects of anomalously hot days on these factors while ruling out alternative explanations. The results indicate that aggregate household income is unaffected by temperature anomalies and that hot days increase outdoor activity rather than residential colocation, providing little support for these as primary operative channels; the gendered withdrawal of urban women from formal wage employment, however, provides sugges-

Table 2.1: Temperature Shocks and IPV Helpline Calls

	IPV calls			
	(1)	(2)	(3)	(4)
Panel A: All				
Anomalously hot days (MaxT $\geq$ LR+3°C)	0.017***	0.016**	0.006	0.013
	(0.006)	(0.007)	(0.004)	(0.009)
Anomalously cold days (MaxT $\leq$ LR$-$3°C)	0.018*	0.007	0.005	0.001
	(0.010)	(0.011)	(0.009)	(0.012)
High rainfall shock (z-score $>$ 1)	0.038	-0.008	-0.020	0.006
	(0.026)	(0.028)	(0.024)	(0.029)
Low rainfall shock (z-score < -1)	-0.044*	0.005	0.004	-0.009
	(0.023)	(0.025)	(0.023)	(0.027)
Panel B: Urban Heterogeneity				
Anomalously hot days (MaxT $\geq$ LR+3°C)	0.008	0.001	-0.006	-0.003
	(0.006)	(0.007)	(0.005)	(0.009)
High-temp shock (+3°C) $\times$ Urban	0.020*	0.029***	0.024***	0.034**
	(0.011)	(0.010)	(0.007)	(0.013)
Anomalously cold days (MaxT $\leq$ LR$-$3°C)	0.006	-0.007	-0.006	-0.020
	(0.012)	(0.013)	(0.013)	(0.014)
Low-temp shock ($-$3°C) $\times$ Urban	0.022	0.025	0.021	0.040*
	(0.018)	(0.018)	(0.016)	(0.021)
High-temp effect, urban	0.028	0.031	0.019	0.031
P-value	[0.006]	[0.002]	[0.003]	[0.013]
Mean Y	1.801	1.801	1.801	1.801
Observations	18240	18240	18240	18240
Year FE	✓			
Month FE	✓			
Year $\times$ Month FE		✓	✓	✓
Province FE	✓	✓		
Province $\times$ Month FE				✓
Province $\times$ Year FE			✓	

Note: The dependent variable is the monthly number of IPV helpline calls per 10,000 population. Anomalously hot (cold) days are defined as days per month where the daily maximum temperature exceeds (falls below) the province- and calendar-month-specific long-run mean (1985–2014) by more than the indicated threshold. Panel B includes controls for high and low rainfall shocks; the corresponding coefficients are not reported for brevity. The urban total effect is the sum of the base coefficient and the interaction term. Standard errors clustered at the province level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

tive evidence for a co-presence effect at the margin. The analysis identifies two plausible primary pathways: deteriorating mental health among male spouses in the most densely urban provinces, and increased community violence in urban areas. These mechanisms may operate simultaneously and interact in complex ways, with some reinforcing each other and others offsetting their effects (Díaz and Saldarriaga, 2023; Roy et al., 2019).

2.5.1 Income Channel

I examine the income channel using ENAHO panel data from 2016 to 2018, first at the household level and then at the individual level disaggregated by gender. I begin by testing whether unusually hot days reduce household-level income, expenditure, or non-labor income. The specification is:

$$\begin{aligned} Y_{hpt} = \alpha &+ \beta_1 HighTemp_{pt} + \beta_2 (HighTemp_{pt} \times Urban_p) \\ &+ \gamma_1 LowTemp_{pt} + \gamma_2 (LowTemp_{pt} \times Urban_p) + \psi_h + \mu_{my} + \delta_{pm} + \epsilon_{hpt} \end{aligned} \tag{2.3}$$

where Y_{hpt} is log net income, log total expenditure, or log non-labor income for household h in province p at time t. $HighTemp_{pt}$ counts the number of days in the 30 days prior to the interview date on which the daily maximum temperature exceeds the province-specific calendar-month long-run mean by at least 3°C, and $LowTemp_{pt}$ is the symmetric cold-shock measure. The coefficients β_1 and $\beta_1 + \beta_2$ capture the effect of warm anomalies on rural and urban households, respectively. The model includes household, month-by-year, and province-by-month fixed effects, with standard errors clustered at the province level.

Table 2.2 reports the estimates. For each outcome I present two columns: the full sample and a subsample restricted to farm households, which face more direct weather exposure through agricultural activity. The coefficients on the high-temperature shock are small in magnitude and statistically insignificant across all six columns. The urban total effect ($\hat{\beta}_1 + \hat{\beta}_2$) is close to zero throughout, ranging from -0.001 ($p = 0.839$) for log net income among farm households to 0.003 ($p = 0.221$) for log expenditure among farm households. Hot days do not meaningfully reduce aggregate household resources in either urban or rural settings.

Table 2.2: Temperature Shocks and Household Income

	Log net income		Log expenditure		Non-labor income	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Farm	All	Farm	All	Farm
High-temp shock (+3°C)	0.000	-0.003	0.001	0.003	0.008	0.007
	(0.005)	(0.007)	(0.004)	(0.005)	(0.016)	(0.015)
High-temp shock (+3°C) × Urban	0.001	0.002	-0.001	0.001	-0.005	-0.007
	(0.006)	(0.008)	(0.005)	(0.005)	(0.016)	(0.019)
Low-temp shock (−3°C)	0.012	0.013	-0.008	-0.012	-0.032	-0.024
	(0.014)	(0.014)	(0.009)	(0.009)	(0.035)	(0.033)
Low-temp shock (−3°C) × Urban	-0.018	-0.023	0.004	0.005	0.017	0.021
	(0.015)	(0.016)	(0.010)	(0.011)	(0.040)	(0.042)
High-temp effect, urban	0.001	-0.001	0.001	0.003	0.003	0.001
P-value	[0.647]	[0.839]	[0.699]	[0.221]	[0.703]	[0.958]
Mean Y	7.633	7.065	7.512	7.029	2.402	3.042
Observations	31091	15111	31091	15111	31091	15111
Household FE	✓	✓	✓	✓	✓	✓
Year × Month FE	✓	✓	✓	✓	✓	✓
Province × Month FE	✓	✓	✓	✓	✓	✓

Note: The outcome variables are the logs of monthly household net income, monthly household total expenditure, and monthly household non-labor income, all deflated. Odd-numbered columns use the full sample; even-numbered columns restrict to farm households. All regressions include interactions between temperature shocks and urban residence indicators. High- and low-rainfall shock controls and their interactions are included in all regressions but omitted from the table for brevity. *High-temp effect, urban* reports the marginal effect of high-temperature shocks in urban areas. Regressions are weighted using household sampling weights and include household, month-by-year, and province-by-month fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Aggregate household income may, however, mask asymmetric earnings adjustments between spouses. Under household bargaining models, what matters for IPV risk is not the level of total income but the identity of who earns it: income losses concentrated among women weaken their relative bargaining position (Angelucci and Heath, 2020). I therefore turn to individual-level ENAHO data and estimate a triple-interaction specification that allows the effects of warm anomalies to vary by urbanization and gender:

$$
\begin{aligned}
Y_{ipt} = \alpha &+ \beta_1 HighTemp_{pt} + \beta_2 (HighTemp_{pt} \times Urban_p) + \beta_3 (HighTemp_{pt} \times Female_i) \\
&+ \beta_4 (HighTemp_{pt} \times Urban_p \times Female_i) + \gamma LowTemp_{pt} + \psi_i + \mu_{my} + \delta_{pm} + \epsilon_{ipt}
\end{aligned}
\tag{2.4}
$$

where i indexes individuals and γ collects the corresponding cold-shock interactions entered symmetrically. The female–male differential in urban areas is identified by $\hat{\beta}_3 + \hat{\beta}_4$. The model includes individual, month-by-year, and province-by-month fixed effects.

Table 2.3 reports estimates for three labor market outcomes: log labor income (column 1), an indicator for having worked in the past week (column 2), and an indicator for wage employment (column 3). In column (1), the high-temperature shock coefficient for urban women relative to urban men ($\hat{\beta}_3 + \hat{\beta}_4$) is -0.018 ($p = 0.152$), negative but statistically indistinguishable from zero. The corresponding rural differential is 0.016 and also insignificant ($p = 0.446$). The employment outcomes in columns (2) and (3) reveal a clearer pattern. The female–male differential in the probability of having worked is -0.005 ($p = 0.031$) in urban areas, and the corresponding differential in wage employment is -0.006 ($p = 0.001$). Neither differential is significant in rural areas. These estimates indicate that urban women withdraw from formal wage employment on unusually hot days while urban men do not; the absence of a significant income decline in column (1) suggests that the labor supply reduction occurs primarily at the extensive margin without a corresponding wage penalty. Appendix Table B.I.4 confirms these findings under the alternative province-by-year fixed-effects specification.

Taken together, aggregate household income, expenditure, and non-labor income are unaffected by high-temperature shocks, and the individual-level labor income differential for urban women relative to urban men is negative but statistically insignificant. The one robust finding is that urban women reduce formal wage employment on anomalously

Table 2.3: Temperature Shocks and Gender Gaps in Labor Outcomes

	(1) Log labor income	(2) Worked	(3) Wage employment
High-temp shock (+3°C)	-0.035**	0.004	-0.004
	(0.016)	(0.003)	(0.003)
High-temp shock (+3°C) × Urban	0.013	-0.003	0.002
	(0.019)	(0.003)	(0.003)
High-temp shock (+3°C) × Female	0.016	-0.002	0.001
	(0.021)	(0.003)	(0.004)
High-temp shock (+3°C) × Urban × Female	-0.034	-0.004	-0.007*
	(0.024)	(0.004)	(0.004)
Low-temp shock (−3°C)	0.076*	-0.005	0.005
	(0.044)	(0.007)	(0.006)
Low-temp shock (−3°C) × Urban	-0.057	0.010	-0.006
	(0.045)	(0.008)	(0.006)
Low-temp shock (−3°C) × Female	-0.030	0.000	0.003
	(0.039)	(0.007)	(0.005)
Low-temp shock (−3°C) × Urban × Female	0.004	-0.006	-0.008
	(0.047)	(0.009)	(0.007)
Female–male differential (urban)	-0.018	-0.005	-0.006
P-value	[0.152]	[0.031]	[0.001]
Female–male differential (rural)	0.016	-0.002	0.001
P-value	[0.446]	[0.539]	[0.730]
Mean Y	2.388	0.675	0.300
Observations	80374	80264	80264
Individual FE	✓	✓	✓
Year × Month FE	✓	✓	✓
Province × Month FE	✓	✓	✓

Note: The outcome variables are log labor income, an indicator for having worked in the last week, and an indicator for wage employment. All regressions include fully interacted temperature shock variables with urban residence and female indicators. Rainfall shock controls and their interactions are included in all regressions but omitted from the table for brevity. *Female–male differential* reports the marginal effect of high-temperature shocks for females relative to males in urban and rural areas, respectively. Regressions are weighted using individual sampling weights and include individual, month-by-year, and province-by-month fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

hot days while urban men do not. Intra-household bargaining models predict that bargaining power responds to permanent income and long-run earnings capacity rather than transitory fluctuations (Aizer, 2010), so a short-lived reduction in women's wage employment on hot days is unlikely to shift the outside options that determine women's leverage within the relationship. The withdrawal of urban women from formal wage employment is therefore relevant not as an income channel but as a labor supply response that may affect the time partners spend together, a possibility taken up in the following section.

2.5.2 Exposure Channel

A second potential pathway operates through changes in partner co-presence. If warm anomalies alter the time either partner spends at home, intra-household exposure changes in ways that may raise IPV risk (Leslie and Wilson, 2020; Bullinger et al., 2021; Agüero, 2021). I test this prediction by examining how anomalously hot days affect mobility patterns across five location categories using province-level data from Google Community Mobility Reports, following Bollman et al. (2023). The specification is:

$$\begin{aligned} Y_{pd} = \alpha &+ \beta_1\, \mathit{HotDay}_{pd} + \beta_2\, \mathit{ColdDay}_{pd} + \gamma_1\, \mathrm{HighRain}_{pd} + \gamma_2\, \mathrm{LowRain}_{pd} \\ &+ \psi_{my} + \mu_d + \delta_{pm} + \varepsilon_{pd} \end{aligned} \tag{2.5}$$

where Y_{pd} is the percentage change in visits to a given location category in province p on day d, relative to a pre-pandemic baseline. HotDay_{pd} ($\mathit{ColdDay}_{pd}$) is a binary indicator equal to one if the daily maximum temperature in province p on day d exceeds (falls below) the province-specific calendar-month long-run mean over 1983–2012 by at least 3°C. $\mathrm{HighRain}_{pd}$ ($\mathrm{LowRain}_{pd}$) is a binary indicator for a daily rainfall z-score above (below) one standard deviation from the province-month baseline. ψ_{my} denotes year-by-month fixed effects, μ_d day-of-week fixed effects, and δ_{pm} province-by-month fixed effects. Standard errors are clustered at the province level. The sample covers October 2021 to October

2022, the period for which Google Mobility data are available for Peru. To examine heterogeneity by urbanization, I augment Eq. (2.5) with interactions between the temperature variables and the urban indicator Urban_p defined in Section 2.3.3.

Table 2.4 runs counter to the co-location prediction: on anomalously hot days, residents move toward outdoor spaces rather than remaining at home — a pattern consistent with Peru's limited access to residential cooling, which leaves indoor environments less tolerable than shaded outdoor spaces during heat anomalies (Sustainable Energy for All, 2025).[10] Panel A shows that hot days significantly increase park visits by 2.456 percentage points across all provinces while leaving residential time statistically unchanged; cold days exhibit the opposite pattern, with park visits declining significantly and residential time rising, consistent with Bollman et al. (2023).[11] Panel B shows that the increase in outdoor activity is concentrated in urban areas, with park visits rising by 2.602 percentage points (significant at the 1 percent level), while residential time, workplace visits, and transit use show no statistically significant urban response. Anomalously hot days thus push residents outdoors rather than keeping them at home, ruling out the co-location hypothesis as a driver of the observed increase in IPV calls.

However, the increase in outdoor activity raises a related concern: more time spent in parks and social settings may raise alcohol consumption, which could lower inhibitory control and elevate violence risk (Cohen and Gonzalez, 2024). I test this using individual-level ENAHO data on male alcohol use. The outcome variable is a binary indicator equal to one if the respondent consumed any alcohol in the past 30 days. The treatment variable is $HighTemp_{pt}$, the count of hot days in the 30 days prior to the interview date, defined as in Section 2.5.1. The specification includes district, year-by-month, and province-by-month fixed effects, with household sampling weights applied.

[10]This pattern is consistent with evidence from Singapore, where hotter days increase mobility to air-conditioned destinations — offices, shopping centers, and schools — particularly among households without home cooling (Fesselmeyer et al., 2024).

[11]Cold days also significantly reduce workplace visits by 0.778 percentage points, transit station visits by 1.298 percentage points, and retail area visits by 1.744 percentage points.

Table 2.5 reports the estimates. Column (1) uses all male respondents; column (2) restricts to ever-drinkers to account for selection into abstinence. In both columns, the high-temperature shock coefficient is small and statistically indistinguishable from zero; the total urban effect is 0.001 ($p = 0.781$) in column (1) and 0.000 ($p = 0.999$) in column (2). Warm anomalies do not significantly affect male alcohol consumption in either urban or rural provinces, ruling out the outdoor socializing–alcohol pathway as a mediator.

To further explore, I next examine whether anomalously hot days reduce labor supply among men and women, which would increase the time partners spend together at home and thereby raise IPV risk through greater exposure to a potentially abusive partner (Díaz and Saldarriaga, 2023). I estimate the triple-interaction specification in Eq. (2.4) across five labor supply outcomes: whether the respondent worked in the past week, whether they held a wage-employment position, total hours worked, primary-job hours, and secondary-job hours. The specification allows the effect of warm anomalies to differ by gender and urbanization, recovering separate estimates for urban men, urban women, and rural women.

Table 2.6 reports the estimates, where columns (1) and (2) report the same outcomes as columns (2) and (3) of Table 2.3 — the probability of having worked and wage employment. Among urban women, the high-temperature shock reduces the probability of having worked (-0.004, $p = 0.065$, column 1) and wage employment (-0.007, $p < 0.001$, column 2), as well as primary-job hours (-0.127, $p = 0.028$, column 4). Among urban men, the high-temperature shock significantly increases primary-job hours ($+0.189$, $p = 0.048$, column 4) and reduces secondary-job hours (-0.114, $p = 0.014$, column 5), suggesting a reallocation between job types rather than a withdrawal from work. Neither pattern extends to rural areas.

These findings support the exposure mechanism primarily through the labor supply response of urban women. Reduced formal employment and primary-job hours bring urban women home on anomalously hot days, raising their exposure to a potentially abusive partner (Díaz and Saldarriaga, 2023). The pattern for urban men — increased

Table 2.4: Temperature Shocks and Mobility

	(1) Park	(2) Workplace	(3) Transit	(4) Residential	(5) Retail
Panel A: All					
Hot day (+3°C)	2.456***	-0.234	0.256	-0.157	-0.208
	(0.576)	(0.563)	(0.274)	(0.117)	(0.588)
Cold day (−3°C)	-3.984***	-0.778*	-1.298***	0.641***	-1.744***
	(0.530)	(0.456)	(0.346)	(0.169)	(0.536)
High rainfall shock	-1.874***	-0.318	-0.234	0.094*	-0.249
	(0.359)	(0.217)	(0.161)	(0.050)	(0.266)
Low rainfall shock	1.706***	0.355	-0.138	-0.099*	-0.381
	(0.442)	(0.380)	(0.284)	(0.051)	(0.285)
Panel B: Urban Heterogeneity					
Hot day (+3°C)	1.817	-0.219	0.040	-0.160	1.132
	(1.704)	(1.508)	(0.391)	(0.216)	(1.229)
Hot day (+3°C) × Urban	0.785	-0.036	0.269	0.013	-1.740
	(1.801)	(1.629)	(0.496)	(0.253)	(1.378)
Cold day (−3°C)	-2.961***	-1.090	-0.747*	0.196	0.518
	(0.795)	(1.009)	(0.431)	(0.186)	(0.442)
Cold day (−3°C) × Urban	-1.216	0.409	-0.679	0.545**	-2.785***
	(1.001)	(1.106)	(0.594)	(0.268)	(0.769)
High-temp effect, urban	2.602	-0.255	0.309	-0.147	-0.608
P-value	[0.000]	[0.667]	[0.343]	[0.276]	[0.357]
Mean Y	23.603	28.188	-7.634	5.129	12.101
Observations	37466	37466	37466	37466	37466
Year × Month FE	✓	✓	✓	✓	✓
Province × Month FE	✓	✓	✓	✓	✓
Day-of-week FE	✓	✓	✓	✓	✓

Note: The outcome variables are percentage changes in visits to parks, workplaces, transit stations, residential areas, and retail and recreation locations relative to a pre-pandemic baseline. All regressions include rainfall controls. Panel B additionally includes interactions between the hot-day and cold-day variables and the urban indicator. *High-temp effect, urban* reports the marginal effect of hot days in urban provinces. Regressions include year-by-month, day-of-week, and province-by-month fixed effects. Standard errors clustered at the province level are reported in parentheses. The sample covers October 2021 to October 2022. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.5: Temperature Shocks and Alcohol Consumption

	Any Alcohol Use in Past 30 Days	
	(1)	(2)
	All Males	Among Ever-Drinkers
High-temp shock (+3°C)	0.005	0.004
	(0.004)	(0.004)
High-temp shock (+3°C) × Urban	-0.004	-0.004
	(0.004)	(0.004)
Low-temp shock (−3°C)	-0.007	-0.009
	(0.006)	(0.006)
Low-temp shock (−3°C) × Urban	0.012	0.013
	(0.008)	(0.008)
High-temp effect, urban	0.001	0.000
P-value	[0.781]	[0.999]
Mean Y	0.446	0.466
Observations	58260	55483
District FE	✓	✓
Year × Month FE	✓	✓
Province × Month FE	✓	✓

The outcome variable is an indicator for any alcohol consumption in the past 30 days. All regressions include interactions between temperature shock variables and the urban indicator. Rainfall shock controls and their interactions with the urban indicator are included in all regressions but omitted from the table for brevity. All regressions additionally control for age, cohabitation status, refrigerator ownership, electricity access, education level, and household size. *High-temp effect, urban* reports the marginal effect of high-temperature shocks in urban areas. Regressions are weighted using household sampling weights and include district, year-by-month, and province-by-month fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

primary-job hours alongside reduced secondary-job hours — reflects a shift between job types and does not clearly imply greater time at home. The withdrawal of urban women from formal employment also links directly to the income channel in Section 2.5.1, since formal jobs typically yield higher and more stable earnings than informal alternatives. Appendix Table B.I.7 confirms these findings under the alternative province-by-year fixed-effects specification.

The exposure channel yields mixed evidence. The aggregate mobility evidence shows that hot days increase time in outdoor spaces, particularly parks, arguing against a general co-location mechanism; warm anomalies also have no significant effect on male alco-

Table 2.6: Temperature Shocks and Labor Supply

	(1) Worked last week	(2) Wage worker	(3) Hours worked (total)	(4) Hours worked (primary)	(5) Hours worked (secondary)
High-temp shock (+3°C)	0.004	-0.004	0.027	0.018	0.009
	(0.003)	(0.003)	(0.180)	(0.165)	(0.056)
High-temp shock (+3°C) × Urban	-0.003	0.002	0.048	0.176	-0.128*
	(0.003)	(0.003)	(0.200)	(0.192)	(0.073)
High-temp shock (+3°C) × Female	-0.002	0.001	0.055	-0.039	0.094
	(0.003)	(0.004)	(0.170)	(0.145)	(0.070)
High-temp shock (+3°C) × Urban × Female	-0.004	-0.007*	-0.247	-0.294	0.047
	(0.004)	(0.004)	(0.204)	(0.187)	(0.082)
Low-temp shock (−3°C)	-0.005	0.005	0.129	0.239	-0.109
	(0.007)	(0.006)	(0.353)	(0.371)	(0.206)
Low-temp shock (−3°C) × Urban	0.010	-0.006	0.149	0.120	0.029
	(0.008)	(0.006)	(0.386)	(0.421)	(0.225)
Low-temp shock (−3°C) × Female	0.000	0.003	-0.236	-0.069	-0.167
	(0.007)	(0.005)	(0.327)	(0.321)	(0.172)
Low-temp shock (+3°C) × Urban × Female	-0.006	-0.008	-0.112	-0.364	0.251
	(0.009)	(0.007)	(0.402)	(0.399)	(0.192)
High-temp effect, urban female	-0.004	-0.007	-0.117	-0.139	0.022
P-value	[0.055]	[0.000]	[0.065]	[0.019]	[0.461]
High-temp effect, urban male	0.001	-0.002	0.075	0.194	-0.119
P-value	[0.424]	[0.233]	[0.346]	[0.038]	[0.010]
High-temp effect, rural female	0.002	-0.003	0.082	-0.021	0.103
P-value	[0.649]	[0.140]	[0.670]	[0.896]	[0.097]
Mean Y	0.675	0.300	29.260	26.857	2.403
Observations	80264	80264	80264	80264	80264
Individual FE	✓	✓	✓	✓	✓
Year × Month FE	✓	✓	✓	✓	✓
Province × Month FE	✓	✓	✓	✓	✓

Note: The outcome variables are indicators for having worked in the last week and wage employment, hours worked across all jobs, hours worked in the primary job, and hours worked in secondary jobs. All regressions include fully interacted temperature shock variables with urban residence and female indicators. Rainfall shock controls and their interactions are included in all regressions but omitted from the table for brevity. *High-temp effect, urban female* (*urban male*, *rural female*) reports the total effect of the high-temperature shock for urban women (urban men, rural women), computed as the sum of the relevant coefficients from the triple-interaction specification. Regressions are weighted using individual sampling weights and include individual, month-by-year, and province-by-month fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

hol consumption, ruling out the outdoor socializing–alcohol pathway. The labor supply evidence, however, points toward a gendered asymmetry: urban women reduce formal wage employment and primary-job hours on anomalously hot days, increasing their time at home, while urban men reallocate between primary and secondary employment without a clear increase in home presence. The exposure channel therefore provides only partial support for the co-presence hypothesis: hot days push residents outdoors, but the gendered withdrawal of urban women from wage employment suggests that heat-induced changes in time use may nonetheless create conditions for intra-household conflict at the margin.

2.5.3 Psychological Stress and Mental Health

A third pathway connects hot days to IPV through the deterioration of male psychological well-being, which prior research links to elevated aggression and a heightened propensity for conflict and violence (Anderson, 2001; Card and Dahl, 2011; Cardazzi et al., 2024). I test this pathway using individual-level data from the Peruvian DHS (2013–2017), restricting the sample to male respondents. The outcome is the Mental Distress Index, a standardized composite of PHQ-9 items described in Section 2.3.3. I estimate the following specification:

$$\begin{aligned} MentalDistress_{ipt} = \alpha &+ \beta_1\, HighTemp_{pt} + \beta_2\, HighTemp_{pt} \times Urban_p \\ &+ \beta_3\, LowTemp_{pt} + \beta_4\, LowTemp_{pt} \times Urban_p + \gamma \mathbf{X}_{ipt} + \psi_{my} + \delta_{pm} + \lambda_d + \varepsilon_{ipt} \end{aligned} \tag{2.6}$$

where $HighTemp_{pt}$ ($LowTemp_{pt}$) is the count of anomalously hot (cold) days in province p in the 30 days prior to the interview date, $\mathbf{X}_{ipt}$ is a vector of individual and household controls, ψ_{my} denotes year-by-month fixed effects, δ_{pm} province-by-month fixed effects, and λ_d district fixed effects. Rainfall shock controls and their interactions with $Urban_p$ are included in all regressions but omitted from the equation for brevity. Standard errors are clustered at the province level.

Table 2.7 shows that anomalously hot days raise male psychological distress, with the effect concentrated in the most densely urban provinces. Column (1) uses the binary urban indicator and yields a marginally significant positive effect, while column (2) replaces it with a high-urban indicator equal to one for provinces with an urbanization rate above 92.3 percent — the 75th percentile of the DHS distribution — and produces a stronger and more statistically robust result, suggesting that the effect intensifies with urbanization density. The total effect in high-urban provinces is 0.016 standard deviations ($p < 0.001$), indicating that anomalously hot days meaningfully deteriorate the psychological well-being of male partners in the most densely urbanized settings. This urban concentration is consistent with the main IPV results and supports the interpretation that heat-induced psychological stress among male partners is a plausible mechanism linking temperature anomalies to intimate partner violence in urban areas.

Figure 2.2 illustrates why the effect is stronger at higher urbanization thresholds. The figure plots the estimated effect of anomalously hot days on mental distress across five urbanization quintiles. The coefficient is negative or near zero in the three least urbanized quintiles and rises steadily in the upper two, reaching its largest value in the most urbanized group. This gradient explains the weaker result in column (1): the binary urban indicator averages over the full distribution, diluting an effect concentrated at the very top. A limitation of the mental distress results is therefore that the effect is detectable only at a higher urbanization threshold than the binary urban indicator used in the main IPV analysis. Appendix Figure B.II.1 provides direct evidence that the same concentration holds for the main IPV outcome. The figure plots the effect of anomalously hot days on IPV calls across five urbanization quintiles using the preferred province-by-month fixed-effects specification. The coefficient is positive but statistically indistinguishable from zero in quintiles one through four, then rises sharply in the most urbanized quintile with a confidence interval entirely above zero. This parallel gradient in both the IPV and mental distress outcomes across the urbanization distribution supports the interpretation that psychological stress is a plausible mechanism linking heat anomalies to intimate partner violence in the most densely urban provinces.

Appendix Table B.I.8 confirms these results under the alternative province-by-year fixed-effects specification, where the high-urban effect is 0.018 standard deviations ($p = 0.001$). Taken together, the evidence indicates that anomalously hot days deteriorate the psychological well-being of male partners in the most densely urban provinces, consistent

Table 2.7: Temperature Shocks and Mental Distress Index

	Mental Distress Index	
	(1)	(2)
High-temp shock (+3°C)	0.005	0.003
	(0.006)	(0.004)
High-temp shock (+3°C) × Urban	0.002	
	(0.007)	
High-temp shock (+3°C) × High Urban		0.013**
		(0.005)
Low-temp shock (−3°C)	-0.012	-0.001
	(0.013)	(0.007)
Low-temp shock (−3°C) × Urban	0.017	
	(0.015)	
Low-temp shock (−3°C) × High Urban		0.023
		(0.024)
High-temp effect, urban	0.007	0.016
P-value	[0.076]	[0.000]
Mean Y	-0.175	-0.175
Observations	58260	58260
District FE	✓	✓
Year × Month FE	✓	✓
Province × Month FE	✓	✓

Note: The outcome variable is the mental distress index. Column (1) includes interactions between temperature shock variables and the urban indicator, while Column (2) includes interactions between temperature shock variables and the high-urban indicator. Rainfall shock controls and their interactions with the urban indicator are included in all regressions but omitted from the table for brevity. All regressions additionally control for age, cohabitation status, refrigerator ownership, electricity access, education level, and household size. *High-temp effect, urban* reports the marginal effect of high-temperature shocks in urban or high-urban areas, respectively. Regressions are weighted using household sampling weights and include district, year-by-month, and province-by-month fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

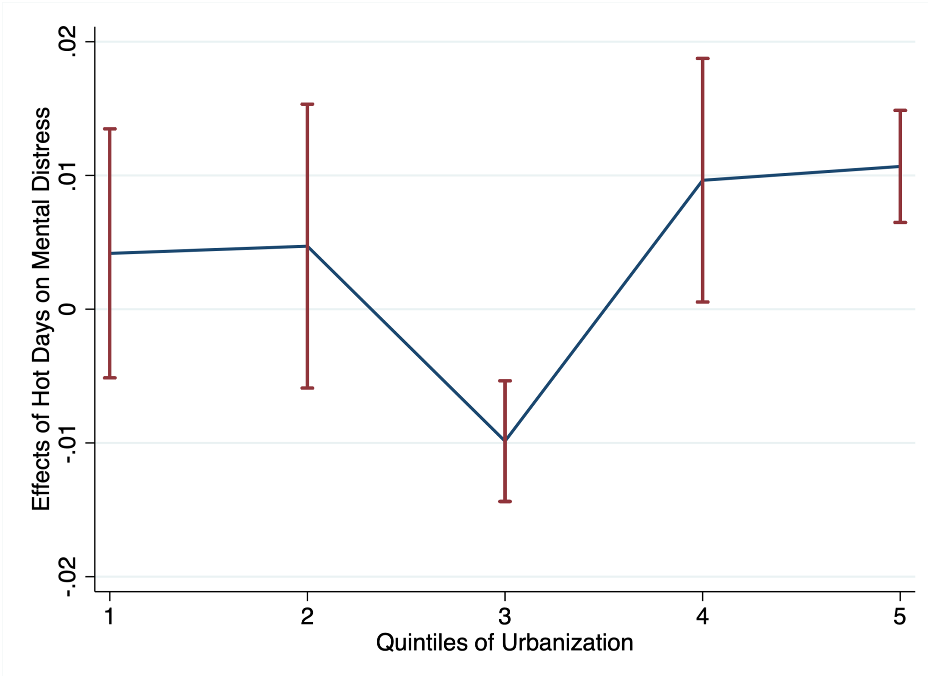


Figure 2.2: Effect of Anomalously Hot Days on Mental Distress by Urbanization Quintile

with a heat-induced stress pathway that contributes to the observed increase in intimate partner violence.

2.5.4 Community Violence

Prior research documents that high temperatures cause street crime and interpersonal violence (Ranson, 2014; Hsiang et al., 2013). These spikes in community violence may make violent behavior more visible and seemingly more acceptable (Wu, 2025), which prior research links to higher IPV risk (Mattina, 2017; Gutierrez and Gallegos, 2016; Noe and Rieackmann, 2013; Hossain et al., 2024; Østby et al., 2019a).

I test this channel by estimating the relationship between anomalously hot days and community crimes using province-level monthly data from SIDPOL police records (2018–2024). I examine five crime categories: all crimes, general safety offenses, and three sub-categories that together comprise general safety offenses — public-safety crimes, crimes against health and body, and crimes against liberty. The specification is:

$$
\begin{aligned}
Crime_{pt} = \alpha &+ \beta_1\, HighTemp_{pt} + \beta_2\, HighTemp_{pt} \times Urban_p \\
&+ \beta_3\, LowTemp_{pt} + \beta_4\, LowTemp_{pt} \times Urban_p + \psi_{my} + \delta_{pm} + \varepsilon_{pt}
\end{aligned} \tag{2.7}
$$

where $Crime_{pt}$ is the monthly number of crime reports per 10,000 population in province p at time t, $HighTemp_{pt}$ ($LowTemp_{pt}$) is the count of anomalously hot (cold) days in the month as defined in Section 2.3.2, $Urban_p$ is the binary urban indicator defined in Section 2.3.3, ψ_{my} denotes year-by-month fixed effects, and δ_{pm} province-by-calendar-month fixed effects. Rainfall shock controls and their interactions with $Urban_p$ are included in all regressions but omitted from the equation for brevity. Standard errors are clustered at the province level.

Table 2.8 shows that anomalously hot days significantly increase community crime in urban but not rural provinces. The effect is not confined to a single crime type: the interaction between the high-temperature shock and urban status is significant at the 1 percent level for all crimes and at the 5 percent level for the general safety offenses composite, with significant effects across all three subcategories — public-safety crimes, crimes against health and body, and crimes against liberty. The total urban effects on all crimes and general safety offenses are 0.055 ($p = 0.074$) and 0.034 ($p = 0.092$), respectively, and Appendix Table B.I.9 confirms the pattern under province-by-year fixed effects. This urban concentration mirrors the gradient in the main IPV results and supports the interpretation that heat-induced increases in community violence contribute to intimate partner violence through the normalization of aggression in densely urban settings.

While these results are consistent with the interpretation that community violence may mediate the effect of anomalously hot days on IPV, some caveats remain. It is possible that heat simultaneously increases both community crime and IPV, and that the observed comovement reflects a common response to the same shock rather than a causal pathway from community violence to household abuse. Moreover, SIDPOL records capture reported crimes rather than the full extent of violent interactions, and may understate the true magnitude of heat-induced community unrest. In light of these limitations, the evidence is best interpreted as suggestive of a community violence channel rather than as definitive causal evidence.

Table 2.8: Temperature Shocks and Community Violence

	(1) All Crimes	(2) General safety offenses	(3) Public-safety Crimes	(4) Crimes against health and body	(5) Crimes against liberty
Panel A: All					
High-temp shock (+3°C)	-0.023	0.007	0.010	-0.002	-0.001
	(0.019)	(0.012)	(0.009)	(0.003)	(0.002)
Low-temp shock (−3°C)	-0.034	-0.013	0.001	-0.011	-0.004
	(0.030)	(0.018)	(0.014)	(0.007)	(0.004)
High rainfall shock	0.047	-0.002	0.008	-0.012	0.002
	(0.063)	(0.036)	(0.027)	(0.016)	(0.009)
Low rainfall shock	0.067	0.043	0.044	-0.002	0.002
	(0.087)	(0.040)	(0.030)	(0.015)	(0.011)
Panel B: Urban Heterogeneity					
High-temp shock (+3°C)	-0.099***	-0.020*	-0.008	-0.007*	-0.006***
	(0.023)	(0.011)	(0.008)	(0.004)	(0.002)
High-temp shock (+3°C) × Urban	0.155***	0.054**	0.036**	0.010*	0.009***
	(0.037)	(0.022)	(0.017)	(0.005)	(0.003)
Low-temp shock (−3°C)	0.042	0.002	0.002	-0.004	0.005
	(0.031)	(0.020)	(0.009)	(0.010)	(0.005)
Low-temp shock (−3°C) × Urban	-0.134***	-0.027	0.001	-0.012	-0.016**
	(0.049)	(0.029)	(0.021)	(0.012)	(0.007)
High-temp effect, urban	0.055	0.034	0.028	0.003	0.003
P-value	[0.074]	[0.092]	[0.089]	[0.462]	[0.163]
Mean Y	5.447	1.973	0.710	0.740	0.523
Observations	16464	16464	16464	16464	16464
Year × Month FE	✓	✓	✓	✓	✓
Province × Month FE	✓	✓	✓	✓	✓

Note: The outcome variables are the monthly number of all crime reports, general safety offense reports, public-safety crime reports, crime reports against health and body, and crime reports against liberty, all measured per 10,000 population. All regressions include rainfall shock controls. Panel B additionally includes interactions between temperature shock and rainfall shock variables and the urban indicator. Regressions include year-by-month and province-by-month fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

2.6 Robustness Checks

2.6.1 Reporting Behavior

A potential concern with using IPV helpline calls as the outcome variable is that the estimated effects may partly reflect changes in reporting behavior rather than changes in the underlying incidence of partner violence (Carias et al., 2024). If anomalously hot days raise the general propensity to report to authorities independently of any change in violence incidence, the main estimates would be upward biased.

I test this interpretation by examining whether hot days increase reports of non-violent offense categories that are unrelated to heat-induced aggression or public safety-related violence. Fraud and administrative offenses involve no element of physical violence and are thus unlikely to respond to the behavioral mechanisms linking heat to IPV. If hot days raise the general propensity to contact authorities, reports of these offense types should rise alongside IPV calls; if instead the main results reflect genuine changes in violence incidence, these offense categories should be unresponsive to temperature anomalies.

I estimate the effect of anomalously hot days on reports of administrative offenses and fraud using province-level monthly SIDPOL police records (2018–2024), following the same specification as Equation 2.1 with province-by-month and year-by-month fixed effects and standard errors clustered at the province level. Table 2.9 reports the estimates. Administrative offense reports are statistically indistinguishable from zero in both rural and urban provinces, with a statistically insignificant total urban effect. Fraud reports also show no statistically significant urban response; while rural provinces exhibit a significant decline in fraud reports on hot days, the total urban effect is statistically insignificant.

The absence of any significant increase in reports of administrative offenses or fraud on anomalously hot days supports the interpretation that the main results capture genuine changes in IPV incidence rather than a general shift in the propensity to contact authorities. Appendix Table B.I.10 confirms these null findings under province-by-year fixed effects.

Table 2.9: Temperature Shocks and Other Crime Reports

	(1) Administrative Offense Reports	(2) Fraud Reports
Panel A: All		
High-temp shock (+3°C)	-0.001	-0.016***
	(0.001)	(0.004)
Low-temp shock (−3°C)	0.001	0.012**
	(0.002)	(0.006)
High rainfall shock	-0.005	0.000
	(0.008)	(0.013)
Low rainfall shock	-0.003	0.017
	(0.007)	(0.011)
Panel B: Urban Heterogeneity		
High-temp shock (+3°C)	-0.001	-0.028***
	(0.001)	(0.004)
High-temp shock (+3°C) × Urban	0.000	0.024***
	(0.002)	(0.005)
Low-temp shock (−3°C)	0.006***	0.014***
	(0.002)	(0.005)
Low-temp shock (−3°C) × Urban	-0.009**	-0.002
	(0.004)	(0.008)
High-temp effect, urban	-0.001	-0.005
P-value	[0.616]	[0.335]
Mean Y	0.176	0.337
Observations	16464	16464
Year × Month FE	✓	✓
Province × Month FE	✓	✓

Note: The outcome variables are the monthly number of administrative offense reports per 10,000 population (column 1) and fraud reports per 10,000 population (column 2), drawn from SIDPOL police records (2018–2024). All regressions include rainfall shock controls. Panel B additionally includes interactions between temperature shock and rainfall shock variables and the urban indicator. *High-temp effect, urban* reports the total effect of high-temperature shocks in urban provinces, computed as the sum of the relevant base coefficient and the interaction term. Regressions include year-by-month and province-by-month fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

2.6.2 *Falsification Test*

To ensure that the relationship between temperature anomalies and IPV calls is not driven by underlying trends or systematic factors, I conduct a falsification test. This involves assessing the effect of future temperature shocks on IPV calls to verify that the observed impact is due to exogenous weather shocks rather than pre-existing trends.

Specifically, I estimate a modified version of Eq. 2.2, replacing the current month's temperature anomaly variable with anomalously hot days occurring 4, 5, 6, and 7 months after the reference month. This approach tests whether future weather events, which should not influence current IPV calls, show any significant effects.

The results of this falsification test are presented in Table 2.10. Column (1) reproduces the preferred specification and confirms that current-month temperature anomalies significantly increase IPV calls in urban areas. In columns (2) through (5), I replace the current month's temperature shock with shocks occurring 4, 5, 6, and 7 months later, respectively. In each of these placebo specifications, the urban interaction term and the total urban effect are statistically indistinguishable from zero.

These findings suggest that households do not anticipate and respond to future temperature anomalies, and that the temperature shock variable is not merely capturing unobserved factors influencing IPV trends. The absence of significant effects in the falsification test therefore provides supporting evidence that the observed relationship between temperature anomalies and IPV calls in the main analysis reflects a genuine causal effect.

2.7 Conclusion

This paper examines the causal relationship between temperature anomalies and the incidence of intimate partner violence. Unlike existing studies, it focuses on short-term, acute weather events (specifically, anomalously hot days) and identifies a particularly vulnerable group: urban populations. The findings demonstrate that anomalously hot days significantly increase IPV helpline calls in urban but not rural provinces, and this

Table 2.10: Falsification Test: Placebo Temperature Shocks

	IPV calls				
	(1)	(2)	(3)	(4)	(5)
	Actual	+4 Months	+5 Months	+6 Months	+7 Months
High-temp shock (+3°C)	-0.002	0.010	0.021**	0.013	0.015*
	(0.010)	(0.008)	(0.010)	(0.008)	(0.008)
High-temp shock (+3°C) × Urban	0.028*	0.005	-0.010	-0.003	-0.015
	(0.014)	(0.014)	(0.014)	(0.012)	(0.012)
Low-temp shock (−3°C)	-0.025	-0.020	-0.024*	-0.021*	-0.029**
	(0.016)	(0.017)	(0.013)	(0.012)	(0.014)
Low-temp shock (−3°C) × Urban	0.048**	0.059***	0.045**	0.039*	0.045**
	(0.023)	(0.023)	(0.020)	(0.020)	(0.022)
High-temp effect, urban	0.026	0.015	0.010	0.010	0.000
P-value	[0.047]	[0.272]	[0.381]	[0.383]	[0.962]
Observations	16,910	16,910	16,910	16,910	16,910
Mean dep. var.	1.737	1.737	1.737	1.737	1.737
Year × Month FE	✓	✓	✓	✓	✓
Province × Month FE	✓	✓	✓	✓	✓

Note: The dependent variable is the monthly number of IPV helpline calls per 10,000 population. Column (1) reports the main result using actual temperature anomaly shocks. Columns (2)–(5) replace the actual shocks with placebo shocks constructed by shifting the temperature variable forward by 4, 5, 6, and 7 months, respectively. Urban provinces are those with an urbanization rate above the sample median (34.3 percent) based on the 2007 Peruvian Census. *High-temp effect, urban* reports the total effect in urban provinces, computed as the sum of the base coefficient and the interaction term. All specifications use date and province-by-month fixed effects and control for high and low rainfall shocks and their interactions with urban status; the corresponding coefficients are not reported for brevity. Standard errors clustered at the province level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

result is robust across all four fixed-effect specifications and to alternative temperature anomaly definitions. I examine four potential mechanisms (labor income, partner co-presence, male psychological well-being, and community violence) and find that the evidence is most consistent with the psychological stress and community violence channels. Male residents in the most densely urban provinces experience meaningful deterioration in psychological well-being on anomalously hot days, and hot days concurrently increase community crime rates across multiple offense categories in urban areas, creating an environment that elevates IPV risk. The gendered withdrawal of urban women from formal wage employment on anomalously hot days provides additional suggestive evidence for a co-presence pathway at the margin.

This paper has a couple of limitations. First, using helpline call data may present certain challenges. If temperature shocks lead to behavioral changes, such as a reduced likelihood of seeking third-party help during IPV incidents, then using helpline call data might introduce bias or only capture the patterns of individuals who actively seek help. Nevertheless, this would likely result in an underestimation of the true impact, suggesting that the actual effect could be even greater in the same direction. Additionally, using helpline call data effectively captures instances that are severe enough to require immediate assistance, thereby reflecting the most critical cases of domestic violence. To address the concern that anomalously hot days raise the general propensity to contact authorities, I show that reports of fraud and administrative offenses, which are unlikely to be driven by heat-induced emotional arousal, do not respond significantly to temperature anomalies, supporting the interpretation that the main results reflect genuine changes in IPV incidence rather than a shift in reporting behavior.

Second, this study cannot fully disentangle the specific pathways through which temperature anomalies influence IPV because of potential violations of exclusion restrictions. Exclusion restrictions require that the identified pathways, such as mental well-being and community violence, are not influenced by other unobserved factors that simultaneously affect IPV. In this case, temperature anomalies may trigger broader changes which could interact with multiple pathways, making it challenging to isolate the independent effect of each mechanism. While my findings provide suggestive evidence for certain pathways, the violation of exclusion restrictions limits the ability to draw definitive causal conclusions about their roles.

This paper highlights several policy implications. First, it emphasizes the importance of targeted heat mitigation strategies in urban areas, including infrastructure improvements such as expanding green spaces and adopting cool roofs to reduce heat exposure. Addressing the mental health impacts of temperature anomalies is equally crucial. Expanding access to mental health services, such as tele-health initiatives and mobile mental health units, can help mitigate the link between anomalously hot days and violence.

Public awareness campaigns can further educate residents on coping strategies for heat-induced stress. Additionally, this study underscores the need to address community violence during periods of extreme heat, as rising violent crimes can reinforce harmful attitudes toward IPV. Policies should focus on strengthening community-based violence prevention programs and challenging norms that justify violence during periods of elevated temperatures.

Future research should further explore the psychological dimensions associated with temperature anomalies and their impact on behavior. Conducting short-term longitudinal surveys could offer valuable insights into how stress and violence patterns evolve in response to temperature fluctuations over days, weeks, or months. Additionally, investigating the effectiveness of psychological strategies in mitigating stress and reducing domestic violence is a promising area for study. This approach can provide a deeper understanding of how urban stressors interact with climate variables over time, further informing policy and intervention strategies.

Chapter 3

FARMERS' RESPONSES TO TEMPERATURE AND RAINFALL SHOCKS IN INDONESIA

3.1 Introduction

Smallholder farmers in low- and middle-income countries (LMICs) comprise a substantial portion of the global poor (World Bank, 2016). They are heavily dependent on agriculture for both subsistence and income, which makes them particularly vulnerable to climate change. In tropical countries such as Indonesia, climate change is projected to bring greater temperature and rainfall variability, characterized by a higher frequency of large deviations from seasonal norms, alongside a gradual rise in mean temperatures (Olonscheck et al., 2021; Meehl et al., 2007).[1] Evidence from climate science suggests that such variability may pose a greater risk to agro-ecosystems than warming alone, particularly in tropical regions where temperatures were historically stable year-round (Katz and Brown, 1992; Vasseur et al., 2014).[2]

On the other hand, the economics literature has primarily focused on the effects of mean warming. For instance, motivated by evidence that temperatures above 32°C hinder the growth of staple crops such as corn, recent studies in LMICs have focused on farmers' responses to temperatures exceeding this threshold (Jessoe et al., 2018; Aragón et al., 2021; Cui and Tang, 2024; Tabe-Ojong et al., 2024). This focus overlooks another channel through which climate change may affect farmers: large deviations in weather

[1]Moreover, these challenges are unique to the tropics, as temperature variability is projected to decline in high latitudes while rising in tropical regions (Olonscheck et al., 2021).

[2]Specifically, these studies show that temperature and rainfall variability can reduce crop yields and constrain economic growth independently of changes in mean temperature, with particularly large effects on low-latitude countries (Wheeler et al., 2000; Rowhani et al., 2011; Ray et al., 2015; Kotz et al., 2021).

from seasonal norms may disrupt agricultural production even when temperatures remain below 32 °C, as shown by Jagnani et al. (2021).

This paper examines the mitigation strategies employed by Indonesian farmers in response to temperature and rainfall shocks. We analyze multiple margins of adjustment, including the use of buffer stocks, labor allocation across sectors, land input, crop mix, and remittances. Our analysis combines individual- and household-level panel data from the Indonesian Family Life Survey (IFLS) with high-resolution meteorological data. We use time and household fixed effects to isolate plausibly random variations in temperature and precipitation within sub-districts (kecamatan) over time, which allows us to estimate farmers' short-run adaptive responses to realized weather shocks (Deschênes and Greenstone, 2007; Schlenker and Roberts, 2009). Our weather shocks are defined using deviations from the location- and season-specific long-run mean to capture localized temperature and rainfall variability.[3]

We begin with the impact on expenditure and find that both high- and low-temperature shocks are associated with substantial declines in rural household expenditure. This result is notable given that monsoon-season daily average temperature ranges from 15°C to 31.9°C for our sample period, with 99% of observations between 18°C and 29°C (Figure 3.1). Within this range, rice, corn, and cassava yields typically increase with temperature in the absence of pests and disease (Schlenker and Roberts, 2009; Welch et al., 2010). Nonetheless, our findings align with the agronomy literature showing that, in the humid tropics, temperature deviations in either direction can depress crop yields by promoting the proliferation of weeds, pests, and plant diseases such as rice blast. We review this literature in Section 3.2.2.

[3]We define high- and low- temperature shocks based on the share of growing-season days with temperatures deviating from the long-run month-specific means. Similarly, we define rainfall shocks using deviations from a sub-district's long-run monsoon rainfall. Section 3.3.2 describes the construction of these shocks. Since rice, the staple crop, is primarily cultivated during the monsoon season, we define the growing season as the monsoon season throughout the paper. To account for regional variation in monsoon timing across Indonesia, we adopt the province-specific monsoon calendars provided in Maccini and Yang (2009a).

We also find that high monsoon-season rainfall is associated with moderate increases in household expenditure, while the effect of low rainfall is small and statistically insignificant. The positive impact of high rainfall is consistent with prior work showing the beneficial impact of rainfall on agricultural income (Paxson, 1992; Jayachandran, 2006; Adhvaryu et al., 2013). However, the negligible effect of low rainfall departs from these earlier studies and suggests that our sample households, likely aided by irrigation technologies, have sufficient strategies to mitigate the adverse effects of rainfall deficits. This interpretation aligns with recent evidence that irrigation makes agricultural production less sensitive to rainfall shortages (Sarsons, 2015; Colmer, 2021; Gatti et al., 2021; Ray et al., 2015). Our findings echo Colmer (2021)'s observation that "temperature is a more important driver of agricultural productivity than rainfall," even though much of the traditional economics literature has focused primarily on rainfall.

In line with prior studies on buffer stocks (Rosenzweig and Wolpin, 1993; Beegle et al., 2006; Aragón et al., 2021), we further find that households respond to adverse agricultural shocks by depleting liquid assets, particularly jewelry. Low-temperature shocks, low rainfall, and to some extent high-temperature shocks, are all associated with declines in jewelry holdings, while high-temperature shocks are also associated with small (albeit statistically insignificant) reductions in household savings. Our findings are particularly relevant in contexts with limited asset ownership. In our sample, 50% of rural households own jewelry, while only 20% have savings.

For asset-poor households, labor reallocation between sectors may be one of the few available margins of adjustment to weather shocks. Under the classic agricultural household model, adverse agricultural productivity shocks should push workers into non-agricultural activities, while favorable shocks pull them into agriculture (Singh et al., 1986; Jessoe et al., 2018). While some studies find evidence consistent with these predictions,[4] the broader literature presents mixed evidence. This variation occurs because

[4]Jessoe et al. (2018) and Huang et al. (2020) document reductions in agricultural employment following periods of extreme heat exceeding 32°C.

the impact of weather shocks on sectoral labor allocation depends on the labor intensity of agricultural practices and the structure of the local economy. First, transition to non-agricultural employment hinges on the capacity of local manufacturing and service sectors to absorb workers displaced from agricultural (Colmer, 2021; Liu et al., 2023; Huang et al., 2020).[5] Local agricultural productivity shocks can even have a direct impact on local non-agricultural employment through local demand effects, with negative shocks reducing and positive shocks increasing non-agricultural labor demand.[6]

More importantly, adverse weather shocks may necessitate greater farm labor input rather than reducing it. Several studies find that high temperatures can increase farm labor, as households increase work intensity to offset agricultural productivity losses. In Sub-Saharan Africa, for example, higher temperatures increase the need for weeding and pesticide application, both of which are labor-intensive in LMICs (Jagnani et al., 2021; Tabe-Ojong et al., 2024). Furthermore, Aragón et al. (2021) find that smallholder farmers in Peru respond to extreme heat by expanding planting areas, which also requires additional labor input.

We find evidence consistent with the last mechanism – that adverse weather shocks can paradoxically increase the labor intensity of agricultural production, which diverges from earlier studies that report a reduction in agricultural labor input following adverse temperature shocks (Jessoe et al., 2018; Huang et al., 2020; Albert et al., 2021).

First, although both high- and low-temperature shocks are adverse productivity shocks that reduce household expenditure, neither leads to a reduction in farm labor input. High-

[5]In contexts where such capacity is limited, as is often the case in rural areas, households may instead respond by resorting to migration, both in the form of domestic migration (Bohra-Mishra et al., 2014; Baronchelli and Ricciuti, 2022) and international immigration (Cai et al., 2016; Jessoe et al., 2018; Zhu et al., 2024; Ibáñez et al., 2024).

[6]Jessoe et al. (2018) and Liu et al. (2023) present evidence that a decline in agricultural productivity can lead to contractions in the local economy, thereby reducing demand for non-agricultural goods and services and lowering labor demand in non-tradable sectors. Conversely, Emerick (2018) shows that positive agricultural productivity shocks, such as high rainfall, can reduce agricultural employment and boost non-agricultural employment, as higher agricultural income increases demand for local non-tradable sectors such as construction and education.

temperature shocks substantially increase agricultural work hours while having minimal effect on non-agricultural hours. Similarly, low-temperature shocks substantially increase agricultural work hours among adults in rice-farming households, although their effects on the average rural adult are small but still positive. These results support the hypothesis that adverse temperature conditions heighten the need for labor-intensive farm activities, such as expanding planting areas, pest control, and weeding. To provide further support for these mechanisms, we show that high-temperature shocks, and to some extent low-temperature shocks, are associated with increased planting areas and a higher likelihood of crop loss among rice-farming households — particularly losses related to plant disease. These results are consistent with existing research in agronomy showing that, in the humid tropics, both high- and low-temperature shocks increase the need for pest control and weeding. We review this literature in Section 3.2.2.

Second, in response to low rainfall, farm households increase their agricultural labor hours while reducing non-agricultural work. These effects are particularly pronounced in villages with access to technical irrigation systems, suggesting that households with access to irrigation canals cope with low rainfall by increasing irrigation-related labor. These findings align with previous research showing that irrigation practices in Indonesia remain highly labor-intensive due to limited mechanization and infrastructure (Sato and Uphoff, 2007; Salman et al., 2022).

Finally, high rainfall has a near-zero impact on total work hours but reduces rural adults' work participation, with small but statistically significant effect sizes. This decline is driven mainly by a lower likelihood of participating in agricultural work, with no compensating increase in non-agricultural activities. These results align with Emerick (2018)'s finding of reduced agricultural employment following high rain. However, unlike Emerick (2018), we find no evidence of labor reallocation toward the non-agricultural sector, suggesting limited scope for local demand effects in our context.

We also find some evidence of adjustments in land use and crop mix. Importantly, the strategies used to manage temperature shocks differ substantially from those adopted to cope with low rainfall. For instance, rice-farming households increase their cultivated area in response to high- and low-temperature shocks, yet reduce it when rainfall is scarce. We discuss these results in Section 3.4.3.

We contribute to the literature on farmers' adjustment and mitigation strategies to weather shocks in LMICs. Prior work consistently demonstrates that extreme heat reduce crop yields (Schlenker and Roberts, 2009; Welch et al., 2010; Lobell et al., 2011), while insufficient rainfall further compromises agricultural productivity (Levine and Yang, 2014a; Hatfield and Prueger, 2015). Common coping mechanisms include adjustment in consumption (Skoufias et al., 2012), labor reallocation across sectors (Colmer, 2021), assets sales (Rosenzweig and Wolpin, 1993), and adjustments in land use (Aragón et al., 2021), crop mix (Aragón et al., 2021; Cui and Tang, 2024), and productive inputs such as fertilizer and machinery (Chen and Gong, 2021).

While our findings on consumption, asset sales, land use, and crop mix are consistent with this literature, we add nuance to the understanding of labor reallocation. We show that adverse weather shocks can paradoxically increase farmers' labor hours in agriculture even as they lower agricultural productivity. We provide suggestive evidence of a damage-control mechanism: our results are consistent with the hypothesis that farmers spend more time on irrigation, expanding cultivated area, and pest and disease management following adverse weather shocks. However, these adjustments in labor allocation and farmland use are insufficient to offset weather-induced losses, leading to liquid asset draw-downs and adjustments in household consumption.

We also show that Indonesian farmers deploy different coping strategies for temperature shocks versus low rainfall, particularly in land-use adjustments. Our results suggest that Indonesian farmers are better positioned to cope with rainfall shocks than temperature shocks. One possible explanation is the lack of technologies to mitigate the effects of temperature shocks, in contrast to the widespread use of irrigation systems to manage low rainfall. To this end, our work also underscores the value of analyzing temperature and rainfall jointly when assessing climate impacts on agriculture. Notably, prior work of rainfall shocks and labor reallocation often omit temperature altogether (Agamile et al., 2021; Afridi et al., 2022) and either do not discuss irrigation availability (Afridi et al., 2022) or focus on contexts without irrigation (Agamile et al., 2021).

Our work also highlights the unique challenges facing countries in the humid tropics. In tropical settings such as Indonesia, temperatures have historically been stable throughout the year. Climate change is projected to introduce more frequent deviations from seasonal norms along with a gradual increase in mean temperature. This increase in climate variability poses a greater risk to the agro-ecosystem and agricultural productivity than warming alone (Olonscheck et al., 2021; Vasseur et al., 2014). Prior research has predominantly focused on warming and extreme heat, paying little attention to the impact of lower-than-normal temperatures.[7] We show that temperature deviations from seasonal norms, in either direction, can disrupt agricultural production, a finding that may be especially salient in the humid tropics due to pest-weed-disease complexes. These findings underscore the vulnerability of tropical regions, where many of the agriculturally based LMICs are located, and reinforce the need to deepen our understanding of the full range of climate change impacts (Thornton et al., 2014).

The remainder of the paper is organized as follows. Section 3.2 provides background on Indonesia's agricultural systems, including the unique challenges faced by the humid tropics. Section 3.3 describes the data and the empirical strategy, including the construction of temperature and rainfall shocks. Section 3.4 presents the results. Section 3.5 concludes.

[7]To the best of our knowledge, only two prior papers explicitly study the effects of lower-than-normal temperatures. Hongoli and Hahn (2023) show that cold days (below 15°C) in Tanzania reduce agricultural productivity and early-life health. Cai et al. (2018) study manufacturing workers in Xiamen, a subtropical city in China with relatively stable temperatures, and find that worker productivity peaks at 75-80°F (close to the city's 78°F average), with both higher and lower temperatures reducing productivity.

3.2 Background: Agriculture in Indonesia

Agriculture plays a central role in Indonesia's economy, employing 33% of the labor force (World Bank, 2022) and supporting the livelihoods of 28 million households (BPS, 2023). Some 93% of Indonesia's farms are smallholder farms owned and operated by a single household (FAO, 2018). They are typically characterized by highly labor intensive farming practices and low mechanization rate (Sato and Uphoff, 2007). Paman et al. (2014) find that the per-hectare mechanization rate is much lower in Indonesia than in countries with similar income levels such as the Philippines and India.

3.2.1 Major Crops and Their Climatic Requirements

Rice is the primary staple food crop and the most common crop grown by smallholder farmers, followed by other staples such as cassava, corn, and soybean, while some also grow cash crops such as rubber and coffee for export (FAO, 2018). Among the farm households in our sample, 41% grew rice, 14% grew cassava, 14% for corn, 4% grew soybeans, 7% grew rubber, and 6% planted coffee (Appendix Figure C.I.4). Oil palm was not a common crop in the IFLS-surveyed villages during our sample period (2000-2014), albeit an important cash crop in other regions of Indonesia.[8]

Rice is mainly grown during the monsoon season as it demands abundant and well-distributed water.[9] Optimal growth occurs at daytime temperatures of 29-30°C and night-

[8]The IFLS asked all farming households to list the crops produced on their household farm for the market or home consumption. Oil palm was not included as a potential crop type in the IFLS database. Soil suitability is an important consideration for oil palm productivity. Before 2016, oil palm production in Indonesia was concentrated in Central Sumatra, with production also in West Kalimantan, and Central and West Sulawesi (Gehrke and Kubitza, 2021), while the majority of IFLS villages are outside of these areas.

[9]Monsoon season typically extends from November to April, lasting six months, albeit with considerable regional variations. Throughout the paper, we adopt province-specific monsoon period definitions from Maccini and Yang (2009a) to account for variations in monsoon timing and duration across provinces. Certain lowland rice areas produce two rice crops per year. The initial and primary crop is typically planted at the monsoon's onset in November and harvested in January. Subsequently, a second crop is planted near the end of the monsoon in February and harvested by the advent of the dry season in May (FAO, 2005).

time temperatures up to 25°C, while temperatures above 35°C are detrimental to rice growth (Wassmann et al., 2009; Welch et al., 2010).

Corn (maize) also requires abundant and well-distributed water and grows best at daytime temperatures of 26-33°C and nighttime temperatures of 17-23°C (Waqas et al., 2021). It is typically grown during the monsoon season in rain-fed agriculture systems and, in contrast, during the dry season in rotation with rice in irrigated areas (FAO, 2005).

Cassava often thrives on marginal lands less suitable for rice or corn. It grows best at 28-30°C, tolerates heat up to 40°C, and withstands moderate drought once established. However, it is somewhat susceptible to pests and diseases (Jarvis et al., 2012; Pushpalatha and Gangadharan, 2020). Planting typically coincides with the monsoon season, though it can be planted year-round.

Soybean is typically grown as a dry-season crop in rotation with rice (FAO, 2005). It flourishes at 25-30°C in well-drained soils but is highly sensitive to water-logging; as a result, excessive rain during pod filling can markedly reduce yields (Gangana Gowdra et al., 2025).

Rubber, a perennial crop cultivated for its latex, is often a diversification crop planted on degraded uplands or non-irrigated plots less suited for rice cultivation. Rubber trees can tolerate a wide range of temperatures; however, latex production is highest in humid environments with mean temperature of 25-28°C (Nguyen and Dang, 2016; Prasada et al., 2021).

About 90% of Indonesia's coffee production consists of Robusta, which grows best at 24-30°C and is sensitive to both high and low temperature extremes. The remaining Arabica is cultivated at higher altitudes with optimal temperatures of 15-24°C (Ramadhillah and Masjud, 2024; Kath et al., 2021).

3.2.2 Temperature and Crop Yield in the Humid Tropics

Although Indonesia is a tropical country with generally warm and stable temperatures year-round, deviations from normal temperatures, both lower and higher, can significantly reduce crop yields. As Elad and Pertot (2014) note, "Although the increase in temperatures predicted in the tropics will be relatively small compared with that predicted for temperate climates, the strongest consequences of global warming are expected to be observed in the tropics, because tropical species have a narrow temperature growth range and are therefore relatively sensitive to changes in temperature."

To begin with, for most of the crops discussed above, yields increase with temperature up to about 30-32°C (Schlenker and Roberts, 2009; Welch et al., 2010). Consequently, when daytime temperatures are below 30°C, lower temperatures can slow plant growth. Conversely, when temperatures exceed 32°C, further increases also tend to reduce plant growth (Welch et al., 2010).

Moreover, temperature deviations in either direction may also exacerbate pest and disease pressures, leading to substantial agricultural productivity losses. High temperatures, as well as low rainfall, can exacerbate parasitic weed infestation (Rodenburg et al., 2011; Ramesh et al., 2017), while cooler-than-normal conditions in the humid tropics can promote insect pest proliferation (Deutsch et al., 2018; Skendžić et al., 2021) and weaken plants' natural defenses against fungal and other diseases (Chakraborty et al., 2000; Suzuki et al., 2014). Rice production is particularly vulnerable to cooler-than-normal temperatures in the humid tropics. Rice blast fungus, the most serious disease threat to rice, thrives in humid environments with night temperatures around 22°C (Bevitori and Ghini, 2014; Asibi et al., 2019). In the humid tropics, low-temperature shocks exacerbates both the incidence and the severity of rice blast disease (Luo et al., 1995).

Consistent with these patterns, data from Indonesia's national crop insurance scheme show that between 2015 and 2018, pests and diseases accounted for 54% of rice crop loss claims, compared with 33% from flooding and 13% from drought (Shynkarenko et al., 2019).

3.3 Data and Methodology

3.3.1 Household Data

Our analysis combines individual and household panel data with high-resolution meteorological data. Individual and household panel data are extracted from the Indonesian Family Life Survey (IFLS), a longitudinal study that covers 13 of Indonesia's 27 provinces and represents approximately 83% of the national population (Strauss et al., 2009). We use three rounds of the IFLS, waves 3, 4, and 5, to form a household panel. These three waves were conducted in years 2000, 2007, and 2015, respectively.[10]

Sample Restrictions We restrict the sample to rural households where the household head is of working age (15 to 65 years). For some analysis, we also examine subsamples of farm households and rice-farming households. Following Christian et al. (2019a), we classify a household as a farm household if any member reported working in the agricultural sector or owning a farm business in any survey wave. We classify rice-farming households as farm households that grow rice in any wave. Among rural households in our sample, 86.5% are farm households and 51.6% are rice-farming households. For individual-level regressions, we use data on all individuals aged 15 and above in these household types (rural, farm, or rice-farming).

The average household head in our sample is around 46 years old, with 6 years of schooling. Around 79% of individuals worked in the past week, with 47% working for at least one hour in the agricultural sector. The average individual worked 31 hours in the past week, with 14 hours in agriculture and 17 hours in non-agricultural sectors (unconditional on working). Appendix Table C.II.1 reports sample characteristics.

Construction of Outcome Variables Household expenditure is constructed using the IFLS household consumption module and includes expenditures on purchased goods and

[10]We excluded IFLS wave 1 (conducted in 1993/94) and 2 (conducted in 1997) from this analysis to avoid the influence of the 1997 Asian Financial Crisis.

services (such as food, non-food items, and schooling costs) for all household members.[11] IFLS also asked for the value of jewelry and savings owned by each household in each wave, which we use to construct asset-related outcomes. The value of livestock is also available in waves 4 and 5 of the IFLS. Additionally, IFLS asked about the amount of transfers household members received in the past 12 months from family members who do not live in the same household (including spouse) and from friends and neighbors. We use the former to construct our measure of remittances. All consumption, assets, and transfer items are inflation-adjusted using 2010 as the base year.

IFLS asked farm households for the list of crops produced on the household farm (either for the market or for home consumption) and the total area of land cultivated by the household. However, IFLS did not ask for the quantities of each crop produced or the area of land allocated to each crop. Thus, our measure of crop mix is a series of indicators of whether a certain crop is cultivated by the household.

We construct three measures of individual labor supply to measure labor allocation between agricultural and non-agricultural activities: total hours worked, hours of work in the agricultural sector, and hours in the non-agricultural sector. All three variables are calculated as the sum of hours across primary and secondary jobs over the past week.[12]

3.3.2 *Weather Data and the Construction of Weather Shocks*

Weather shocks are defined at the sub-district (kecamatan) level using temperature and rainfall data from the ERA5-Land database maintained by the European Center for Medium-Range Weather Forecasts. This database provides daily average temperature and monthly cumulative rainfall at a spatial resolution of $0.1^\circ \times 0.1^\circ$, which corresponds to approx-

[11] Consumption items were reported over varying recall periods (weekly, monthly, or yearly) in the IFLS. We convert each item to a monthly equivalent before aggregating to total expenditure.

[12] We cap weekly hours at 168 (24 hours per day for 7 days). In the raw data, a small percentage of individuals reported working more than 168 hours per week in their primary job: around 0.07% in 2000, 0.04% in 2007, and none in 2014. Observations with reported hours above 168 and below 990 were recoded to 168 (values above 990 represent missing values).

imately an 11 $\times$ 11 kilometer area near the equator. Grid-level data are converted to sub-district level by taking an inverse distance-weighted average of the readings of the five nearest grids to each sub-district centroid.[13]

Construction of Temperature Shocks

Following established approaches in both agronomy and economics (Deschênes and Greenstone, 2007; Schlenker and Roberts, 2009; Aragón et al., 2021), we model agricultural productivity as a function of cumulative exposure to temperature shocks and rainfall during the growing season, defined as the monsoon season in our context.

Temperature shocks are measured as the percentage of monsoon days with daily average temperatures exceeding (high-temperature shocks) or falling below (low-temperature shocks) thresholds. While previous studies emphasize that harmful temperature thresholds should be context-specific, depending on factors such as crop type and agricultural technology, they typically apply a single national threshold. Schlenker and Roberts (2009) find that temperatures above 29°C harm US corn yields (30°C for soybeans, 32°C for cotton), while Aragón et al. (2021) find temperatures above 33°C damage Peruvian corn.

Our approach diverges by defining location- and month-specific thresholds based on historical monthly means, calculated for each sub-district and month using data from 1973 to 1999 (a period that predates our household panel). This decision is based on several compelling reasons. Due to Indonesia's proximity to the equator, intra-annual temperature variation within a given location is limited. However, there is substantial cross-sectional variation in temperature across locations in any given month, driven primarily by differences in elevation. For example, the long-run monthly mean temperature in Central Jakarta ranges from 25.3°C to 26.6°C throughout the year, whereas most areas in Bandung City experience means between 21°C to 23°C, and West Bandung district sees even lower means, between 19.5°C and 22.5°C. Appendix Figures C.I.1, C.I.2 and C.I.3

[13]Out of the 2,105 IFLS sub-districts, 25 were dropped due to substantial boundary changes over time. After restricting to rural areas, the final household-level sample includes 1,250 sub-districts.

further illustrate the substantial cross-location variations in temperature across Indonesia and the limited intra-annual dispersion in temperature within each location. These geographic differences in climate and elevation, in turn, give rise to wide variations in crop composition and agricultural practices in Indonesia.

In our main specification, the high-temperature threshold is 1°C above the historical month-specific mean, and the low-temperature threshold is 0.7°C below the mean. These thresholds are chosen so that around 10% of the household sample experienced each type of shock. For robustness, we also conduct our analyses using alternative temperature thresholds ranging from 0.7°C to 1.2°C above or below the historical monthly mean. We obtained qualitatively similar results under these alternative thresholds (columns 3-4 of Appendix Tables C.II.6 and C.II.7).

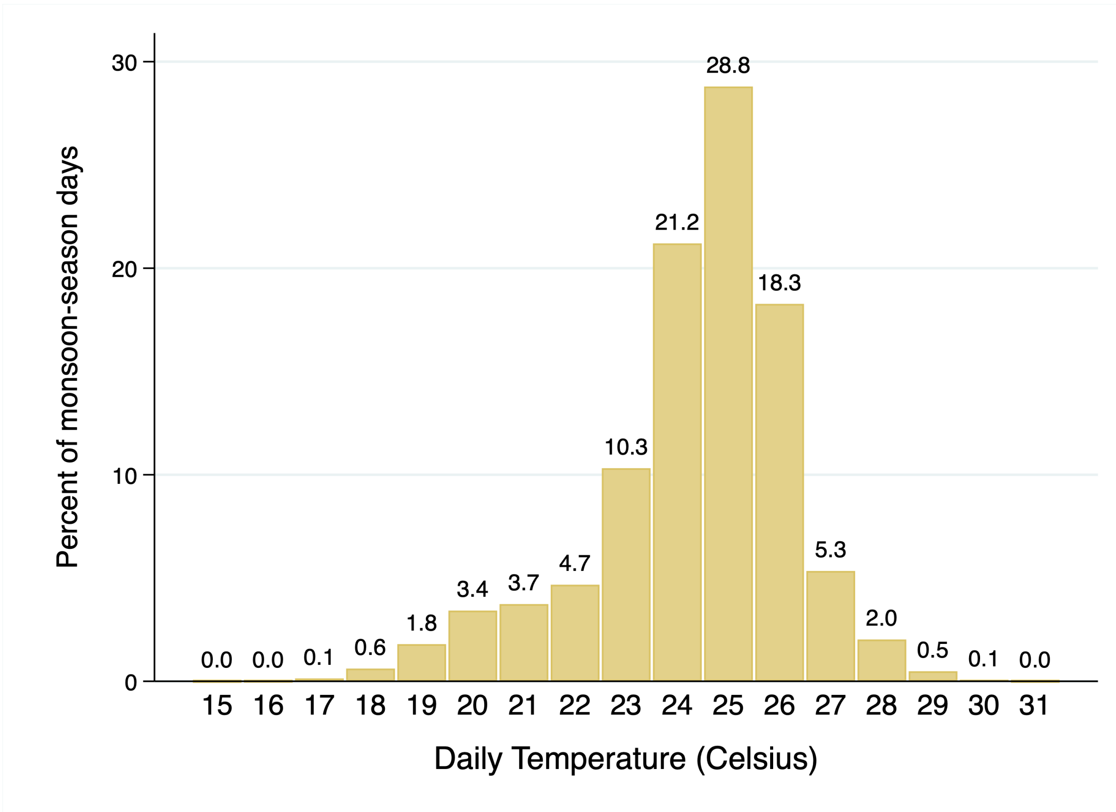
Our high-temperature shock variable measures the percentage of monsoon-season days with temperatures at least 1°C above the sub-district's month-specific long-run mean, while low-temperature shock measures the percentage at least 0.7°C below the mean. As such, these shocks also capture temperature variability by measuring the frequency of large deviations from seasonal norms.

The majority of "high-temperature days" in our sample are moderate in temperature compared to the high-temperature days typically evaluated in the literature. In fact, for the monsoon periods in our sample, average daily temperatures fall between 15°C and 31.9°C, with only 0.05% of household-monsoon-days above 30°C (86°F) and 2.6% below 20°C (68°F). Figure 3.1 presents the distribution of daily average temperature during the monsoon seasons relevant to our analysis. Additionally, the long-run monthly mean temperatures for our geographic sample range from 17.6°C to 28.4°C, with the inter-quartile range being 23.7°C to 25.8°C (see Appendix Figure C.I.1 for the distribution).

Construction of Rainfall Shocks

High- and low-rainfall shocks are defined based on the cumulative rainfall during the most recent completed monsoon season. First, for each year from 1973 to 2015, we cal-

Figure 3.1: Distribution of daily average temperature during monsoon season



Notes: Density of daily average temperature during the last completed monsoon season. The unit of observation is household-day. Sample includes only rural IFLS households. Label on top of each bar shows the percentage of household-day observations with daily average temperature falling in the bin.

culate cumulative monsoon-season rainfall for each sub-district by aggregating monthly rainfall over the monsoon period, using the province-specific monsoon period definitions provided in Maccini and Yang (2009a) that account for variations in monsoon timing across provinces.[14] We then use historical data from 1973 to 1999 to calculate the long-run mean and standard deviation (SD) of monsoon rainfall for each sub-district. The monsoon rainfall for each sample year is then standardized using the long-term mean and

[14]The average monsoon period across Indonesian provinces is approximately 6 months, with the monsoon season generally ranging from November to April.

SD. For our main specification, high-rainfall shock equals one when standardized monsoon rainfall exceeds 1 SD, while low-rainfall shock equals one when it falls below -1 SD. For robustness, we also conduct our analyses using alternative rainfall shock thresholds ranging from 0.9 SD to 1.1 SD. We obtained qualitatively similar results under these alternative thresholds (columns 5-6 of Appendix Tables C.II.6 and C.II.7). We also construct a standardized z-score for the cumulative rainfall of the most recent dry season and include it as a control variable in all regressions.

Finally, since the IFLS surveys took place over the course of several months, we match each respondent in the IFLS to the rainfall shocks corresponding to the most recent completed monsoon season. That is, individuals interviewed during a on-going monsoon season are matched to the rainfall and temperature shocks of the previous monsoon to dry season cycle.

3.3.3 Estimating Equation

Our main estimating equation is as follows:

$$y_{ikt} = \alpha + \beta_1 Weather_{kt} + Time_t + \rho_i + \epsilon_{i,k,t} \tag{3.1}$$

where $Weather_{k,t}$ is a vector of climate shocks defined at the sub-district level. $Time_t$ denotes time fixed effects, including year fixed effects to control for national-level changes and quarter-of-the-year fixed effects to account for potential seasonality in the outcome of interest. ρ_i represents household fixed effects when y_{ikt} is measured at the household level and individual fixed effects when y_{ikt} is measured at the individual level.

Following recent literature (e.g., Emerick (2018)), we control for the standardized z-score of dry-season rainfall in all regressions. Our main specification does not control for dry-season temperature shocks, as the primary crops are grown during the monsoon season. Nevertheless, our results are robust to including dry-season temperature shocks (columns 2 of Appendix Tables C.II.6 and C.II.7).

Given the strong spatial correlation in weather shocks, we report Conley (1999) standard errors that permit arbitrary spatial correlation within a 100-km radius of each subdistrict's centroid and temporal correlation across the three survey waves.[15]

3.4 Results

3.4.1 Impact on Household Expenditure

We begin by estimating the impact of temperature and rainfall shocks on rural household expenditure. Table 3.1 presents the results. We find that high- and low-temperature shocks significantly reduce household expenditure. High rainfall significantly increases expenditure, while the effect of low rainfall is negative but statistically insignificant.

Specifically, our estimates imply that, for the average rural household, sustained high temperatures over the entire monsoon season would reduce expenditure by 33.1% (column 1), statistically significant at the 10% level (column 1). The average sample household experienced high temperatures for 14.9% of the monsoon season, the implied average exposure would reduce household expenditure by about 5%.[16] Similarly, a full monsoon season of low temperatures is associated with a 59.5% decline in household expenditure, statistically significant at the 10% level. Given that households experienced low temperatures for 9.9% of the monsoon season on average, this corresponds to an estimated 6% reduction in expenditure.

High monsoon-season rainfall is associated with an 8.3% increase in rural household expenditure. This result aligns with prior studies showing that increased rainfall during the growing season boosts agricultural productivity and household income (Paxson, 1992; Jayachandran, 2006; Adhvaryu et al., 2013). On average, 10.9% of the sample households experienced high monsoon-season rainfall. This average exposure translates to a 0.9%

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[15]We follow the implementation methods developed by Hsiang (2010) and refined by Fetzer (2020).

[16]In comparison, Cui and Tang (2024) find that extreme heat during the growing season has no impact on farmer's consumption in Northeast China as farmers there are able to fully smooth consumption by utilizing buffer stocks.

increase in expenditure.

Table 3.1: Weather shocks and household expenditure

	Log real household expenditure		
	(1)	(2)	(3)
	All HH	Farm HH	Rice-farm HH
High-temp shock (Monsoon season)	-0.331*	-0.409**	-0.566**
	(0.184)	(0.186)	(0.223)
Low-temp shock (Monsoon season)	-0.595*	-0.655**	-0.834***
	(0.307)	(0.307)	(0.319)
High-rain shock (Monsoon season)	0.083***	0.096***	0.095**
	(0.032)	(0.032)	(0.039)
Low-rain shock (Monsoon season)	-0.023	-0.015	-0.008
	(0.036)	(0.038)	(0.042)
Mean Y	14.297	14.276	14.252
Observations	12647	10940	6530
Household FE	Yes	Yes	Yes

Note: Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. We calculate the panel version of Conley standard errors using the Stata routines developed by Hsiang (2010) and Fetzer (2020), allowing for spatial correlation within a 100 km radius of subdistrict centroids. The regressions control for the rainfall z-score during the dry season and include fixed effects for households, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

Consistent with our hypothesis that temperature and rainfall shocks affect rural households through their effects on agricultural productivity, we find that the impacts of temperature and high-rainfall shocks are generally larger in magnitude for farm and rice-farming households (columns 2 and 3) than for the full sample (column 1), although the differences across these samples are not statistically significant.

Finally, low monsoon-season rainfall is associated with small and statistically insignificant reductions (2.3%) in rural household expenditure. While earlier literature finds that low rainfall reduces corp yields, agricultural income, and household expenditure (Paxson, 1992; Jensen, 2000; Skoufias et al., 2012), Colmer (2021) shows that much of this earlier literature over-estimated the impact of rainfall by not controlling for temperature. Moreover, Colmer (2021) shows that in areas of India with full irrigation coverage, low rainfall does not have any impact on crop yields, while the impact of temperature cannot be mitigated by irrigation. Gatti et al. (2021) also find that, in Indonesia, the presence of irrigation infrastructure mitigates the impact of rainfall on rice production. Among our sample households, 89% had access to some type of irrigation and 46% had access to technical irrigation.[17] Access to irrigation likely helped households mitigate some negative impacts of low rainfall. In subsequent sections, we examine additional coping strategies for low-rainfall shocks, including selling buffer stocks and adjusting crop mix.

Taken together, these estimates suggest that temperature shocks are associated with larger adjustments in household welfare than rainfall shocks. This is consistent with Colmer (2021)'s conclusion that unexpected temperatures are more difficult for farmers to manage than unexpected rainfall. Similarly, a more recent study by Stainier et al. (2025) also shows that extreme temperatures affect rural households' nutritional intake, whereas rainfall shocks do not have comparable effects.

3.4.2 Impact on Labor Allocation

Next, we examine the impact on individuals' labor supply and allocation of labor between agricultural and non-agricultural sectors. Table 3.2 presents the effects on hours of work in the past week, where the outcome variable is total hours of work in Panel A, hours in the agricultural sector in Panel B, and hours in the non-agricultural sector in Panel C.[18]

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[17] Data on irrigation availability are drawn from the IFLS community survey. Among the 7,734 households residing in villages covered by the community survey, 46% lived in villages with access to technical irrigation, 43% had access to semi-technical or simple irrigation (e.g., tube or well pumps), and the remaining 11% had no access to irrigation.

[18] For these regressions, the sample includes all individuals from rural households whose age was between 15 and 65 as of IFLS wave 3 (i.e., year 2000). For individuals not engaged in either agricultural or non-agricultural work during the past week, the corresponding hours of work were set to zero. As such, estimates in Table 3.2 represent the combined impacts on both the extensive margin of work participation and the intensive margin of work hours. Appendix Table C.II.2 presents the impact on the extensive mar-

Table 3.2: Weather shocks and hours of work in the past week

	(1) All HH	(2) Farm HH	(3) Rice-farm HH	(4) Farm males	(5) Farm females	(6) Rice-farm males	(7) Rice-farm females
Panel A	Outcome var. = Hours of work in the past week						
High-temp shock	11.239**	11.186**	19.988***	14.385**	8.607	22.089***	17.940**
	(4.518)	(5.029)	(6.851)	(5.736)	(5.927)	(8.324)	(7.729)
Low-temp shock	6.923	3.676	17.759*	6.026	2.241	13.071	21.532
	(7.787)	(8.027)	(10.174)	(9.320)	(10.413)	(12.928)	(14.823)
High-rain shock	-0.713	-0.500	-0.103	-0.771	-0.377	-0.415	0.013
	(0.794)	(0.853)	(1.088)	(1.134)	(1.086)	(1.429)	(1.375)
Low-rain shock	0.578	0.962	1.492	0.658	1.170	1.573	1.334
	(1.001)	(1.119)	(1.334)	(1.549)	(1.319)	(1.752)	(1.545)
Mean Y	30.972	30.934	31.079	39.396	23.181	39.326	23.509
Panel B	Outcome var. = Hours of work in the agricultural sector						
High-temp shock	10.524**	11.627**	22.215***	15.042***	8.676	26.816***	18.157**
	(4.515)	(4.890)	(6.275)	(5.605)	(5.393)	(6.902)	(7.060)
Low-temp shock	1.284	0.585	16.441*	2.351	-1.083	16.513	16.362
	(6.821)	(7.389)	(9.403)	(9.373)	(8.575)	(11.669)	(12.038)
High-rain shock	-0.317	-0.333	-0.725	-1.103	0.236	-1.804*	0.021
	(0.718)	(0.792)	(1.064)	(0.884)	(0.941)	(1.081)	(1.259)
Low-rain shock	1.716*	2.139**	2.749**	2.237*	2.045*	2.476	2.914**
	(0.953)	(1.055)	(1.301)	(1.358)	(1.126)	(1.555)	(1.480)
Mean Y	13.800	16.005	18.192	21.931	10.576	24.244	12.637
Panel C	Outcome var. = Hours of work in the non-agricultural sector						
High-temp shock	0.831	-0.312	-2.016	-0.372	-0.069	-4.254	-0.217
	(2.682)	(2.669)	(3.311)	(4.120)	(3.301)	(5.866)	(3.649)
Low-temp shock	5.842	3.328	1.731	4.202	3.324	-2.519	5.170
	(6.111)	(6.320)	(6.759)	(7.474)	(7.712)	(10.090)	(9.173)
High-rain shock	-0.412	-0.187	0.590	0.288	-0.613	1.316	-0.007
	(0.722)	(0.711)	(0.836)	(0.858)	(0.923)	(1.116)	(1.120)
Low-rain shock	-1.159*	-1.201*	-1.296*	-1.632	-0.875	-0.988	-1.580*
	(0.690)	(0.728)	(0.772)	(1.268)	(0.881)	(1.317)	(0.943)
Mean Y	17.177	14.936	12.896	17.479	12.606	15.100	10.872
Observations	28600	24573	14748	11438	13128	6877	7868
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for individuals, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01, *** p<.01

We find that high temperatures significantly increase agricultural work hours while having an negligible impact on non-agricultural work hours. A full monsoon season of high temperatures is associated with a 10.5-hour increase per week in agricultural work among rural adults (statistically significant; column 1, Panel B). Given that the average household experienced high temperatures for 14.9% of the monsoon season, this average exposure implies an increase in agricultural work by 1.56 hours per week, a 11% rise relative to the 13.8-hour weekly average. For adults in rice-farming households, the impacts are twice as large, with an entire monsoon season of high temperatures increasing agricultural work by 22.2 hours per week from a basis of 18.2 hours (column 3 of Panel B).[19] In contrast, the impacts on non-agricultural work hours are close to zero and statistically insignificant (Panel C).

Low-temperature shocks have small, positive, but statistically insignificant impacts on the average rural adult's total work hours, with similarly small, positive impacts on both agricultural and non-agricultural work hours. However, among adults in rice-farming households, the impacts on agricultural hours are large and statistically significant at the 10% level (column 3, Panel B). A full monsoon season of low temperatures increases agricultural work hours by 16.4 hours per week (from a basis of 18.2 hours) for these adults, while leaving non-agricultural work hours essentially unchanged.

Together, these results suggest that high-temperature shocks increase agricultural labor inputs, while low-temperature shocks also increase agricultural labor inputs among rice-farming households.

We propose two primary explanations for this phenomenon. First, to mitigate temperature-induced productivity losses, farmers may expand planting areas, which in turn requires more farm labor. We show in Table 3.4, column 2, that high-temperature shocks, and to a lesser extent low-temperature shocks, are associated with substantial increases in cul-

gin of work participation, using indicators of work or sectoral participation as outcome variables. The impacts on work participation are consistent with the direction of impacts shown in Table 3.2.

[19]The impacts are also larger on males than on females, a finding consistent with males working longer hours than females on average (columns 4-7 of Panel B).

tivated land area among rice-farming households. We discuss these results in detail in Section 3.4.3. This channel is consistent with Aragón et al. (2021)'s finding that smallholder farmers in Peru respond to extreme heat by expanding planting areas and increasing within-household farm labor.

Second, as we reviewed in Section 3.2.2, in the humid tropics both high- and low-temperature shocks can increase the need for pest and weed control, while low-temperature shocks also intensify the need for fungus disease control on rice farms. These activities are labor intensive in LMICs (Sato and Uphoff, 2007). This explanation aligns with Jagnani et al. (2021)'s finding that higher temperatures increase weeding and pesticide application and Tabe-Ojong et al. (2024)'s finding that such activities are associated with higher farm labor. Although IFLS does not contain data on pesticide usage or time spent on pest or disease control, we indirectly test this mechanism by examining the impact of weather shocks on the incidence of crop loss and disease-related crop loss in the past 12 months (columns 3-6 of Table 3.4). We find that high-temperature shocks significantly increase the likelihood of crop loss for all farm households. Moreover, consistent with the agronomy literature linking low-temperature shocks and rice-blast disease, we find that low-temperature shocks have large, positive, albeit statistically insignificant, impacts on the likelihood of disease-related crop loss for rice-farming households.[20]

In addition to these two explanations, several other mechanisms may be at play. Adverse weather shocks that reduce agricultural productivity may lead to higher work hours if the income effect of lower hourly productivity outweighs the substitution effect, a situation particularly likely in LMICs with incomplete financial markets (as illustrated in

[20]These estimates likely represent a lower bound of the effects on crop loss, as households may have already changed their planting area or crop mix (e.g., planting a smaller land area or more weather-resilient crops) in response to the weather shocks, responses that would have reduced the likelihood of crop loss. Estimates in Table 3.4 also show that both high- and low-*rainfall* shocks reduce the likelihood of crop loss. The observed reduction in crop loss associated with low rainfall is likely partially attributable to a decrease in the total cultivated land area (columns 1-2), which in turn results in fewer harvests susceptible to damage. Similar patterns emerge with crop loss from disease: farmers reduce their planting areas in response to low temperatures, which limits the amount of realized crop loss associated with low-temperature shocks.

Jayachandran (2006)'s conceptual framework). Meanwhile, lower agricultural productivity may also depress demand for the outputs of the local non-agricultural sector, thus reducing non-agricultural labor demand. Together, these forces imply higher agricultural work hours and lower (or unchanged) non-agricultural work hours. Additionally, temperature shocks may impair workers' health and productivity (Dell et al., 2012; Cai et al., 2018; Lai et al., 2023), necessitating more hours to complete the same workload.

Turning to the impact of rainfall, low-rainfall shocks have small, statistically insignificant effects on overall work hours. However, they modestly increase agricultural work hours and reduce non-agricultural hours, with both effects statistically significant at the 10% level. For the average rural household, low monsoon-season rainfall is associated with 1.7 more hours of agricultural work and 1.2 fewer hours of non-agricultural work per week. The magnitudes are slightly larger for farm households, and the impacts are similar for male and female farm household members.

These results suggest that low rainfall increases agricultural labor inputs and reduces households' time spent in non-agricultural work. We hypothesize that these results reflect a greater need for intensive irrigation following low rainfall, which is labor-intensive in Indonesia (Sato and Uphoff, 2007; Salman et al., 2022). To test this mechanism, we analyze the subsample of villages with access to technical irrigation, which typically means irrigation canals that enable rice cultivation even under low-rainfall conditions (Tirtalistyani et al., 2022; Sato and Uphoff, 2007). Consistent with this mechanism, we find that the increase in agricultural hours and the decline in non-agricultural hours following low rainfall are concentrated in villages with technical irrigation; by contrast, the response of work hours to low-rainfall shocks is markedly attenuated in villages without technical irrigation (Appendix Table C.II.3).[21] These results suggest that although irrigation can

[21]Even in villages with access to irrigation canals, irrigation requires substantial labor due to limited mechanization and inadequate infrastructure (Sato and Uphoff, 2007; Salman et al., 2022). The analysis in Appendix Table C.II.3 is restricted to villages that participated in the IFLS community survey, which included questions of the type of irrigation available at the village level. We concede that the results in Appendix Table C.II.3 may not be fully attributable to irrigation alone as villages with access to irrigation may be very different from those without (e.g., lower altitude, closer to major waterways, or wealthier

provide insurance against rainfall shocks (Mazur, 2023; Gatti et al., 2021; Blakeslee et al., 2020), it can divert labor from other activities in countries with limited mechanization such as Indonesia.

High rainfall shocks have an almost negligible effect on hours of work. However, they reduce work participation at the extensive margin. Estimates in Appendix Table C.II.2 show that high rainfall lowers rural adults' probability of work participation by 2.3 percentage points (pp) relative to a baseline of 78.2%. The decline is 2 pp for men from farm households and 3.4 pp for women, and is driven primarily by reduced participation in agricultural work, with little to no effect on non-agricultural work participation. These results are consistent with Emerick (2018), who finds that high rainfall reduces agricultural employment in India. However, unlike in Emerick (2018), we do not observe a compensating reallocation of labor toward non-agricultural sectors in Indonesia.

Notably, neither temperature nor rainfall shocks lead to statistically meaningful increases in non-agricultural work hours. This finding stands in contrast with prior work that finds farmers shifting from agricultural to non-agricultural work during periods of low agricultural productivity. For example, Colmer (2021) and Huang et al. (2020) find labor reallocation in response to heat, Agamile et al. (2021) and Afridi et al. (2022) in response to low rainfall, Albert et al. (2021) in response to dryness (a combination of heat and low rainfall).

One potential reason for these differing findings is the high labor intensity of farming practices in Indonesia (Sato and Uphoff, 2007). Indonesia has a lower per-hectare mechanization rate compared to most South Asian and Latin American countries, making tasks such as weeding and irrigation particularly labor-intensive (Pingali, 2007; Paman et al., 2014). More importantly, previous research highlights that labor reallocation depends critically on the capacity of the local non-tradable sector to absorb excess agricultural workers (Blakeslee et al., 2020; Colmer, 2021). Yet the manufacturing and mining indus-

(Blakeslee et al., 2020)).

tries in Indonesia have been in relative decline since the Asian financial crisis of the late 1990s (Arifin et al., 2019). This weakness in the tradable sector likely constrained its ability to absorb excess labor from agriculture.[22]

3.4.3 Impact on Liquid Assets

The theory of optimal saving posits that rural households with limited access to formal insurance or credit use liquid assets, such as jewelry and livestock, as buffer stocks to insulate their consumption from income shocks (Rosenzweig and Wolpin, 1993). We examine the impact of weather shocks on three forms of liquid assets: jewelry, livestock, and formal savings. In our sample, jewelry is the most widely held asset (owned by 50.4% of rural households and 49.8% of farm households), followed by livestock (37.7% of rural households and 39.2% of farm households). Poultry is the most common type of livestock, owned by 29% of households, while 8% of households own non-poultry livestock, with cattle being the most prevalent. Formal savings are less common, owned by only 20.5% of rural households and 19.4% of farm households.

Table 3.3 reports the regression results, with Panel A presenting extensive-margin outcomes (asset ownership) and Panel B presenting intensive-margin outcomes (asset values). The results suggest that households use jewelry holdings and sales to smooth consumption. Adverse weather shocks — high temperature, low temperature, and low rainfall — are all associated with reductions in jewelry ownership, although the impact of high-temperature shocks are not statistically significant. The impacts are particularly pronounced among rice farming households.

[22] An alternative explanation for limited labor reallocation is skill mismatch. Temporarily displaced agricultural workers may lack the skills for non-agricultural employment. We conduct heterogeneity tests by adult's educational attainment and find that both highly and less educated adults were equally unlikely to shift into non-agricultural sectors, suggesting that skill mismatch is not the primary constraint.

Table 3.3: Weather shocks and asset related outcomes

	(1) All HH	(2) Farm HH	(3) Rice-farm HH	(4) All HH	(5) Farm HH	(6) Rice-farm HH	(7) All HH	(8) Farm HH	(9) Rice-farm HH
Panel A	Outcome = Own jewelry (d)			Outcome = Own livestock (d)			Outcome = Own saving (d)		
High-temp shock	-0.068	-0.091	-0.126	0.416**	0.414***	0.281	-0.033	-0.031	-0.035
	(0.106)	(0.105)	(0.126)	(0.163)	(0.154)	(0.199)	(0.074)	(0.071)	(0.099)
Low-temp shock	-0.470**	-0.507***	-0.761***	0.071	0.125	-0.129	0.134	0.132	0.131
	(0.197)	(0.193)	(0.247)	(0.276)	(0.295)	(0.420)	(0.134)	(0.121)	(0.159)
High-rain shock	0.037	0.037	0.046	0.001	-0.006	-0.061	0.008	0.006	0.008
	(0.025)	(0.026)	(0.031)	(0.037)	(0.039)	(0.051)	(0.017)	(0.019)	(0.018)
Low-rain shock	-0.051*	-0.044	-0.084**	-0.146***	-0.136***	-0.139***	0.013	-0.009	0.004
	(0.030)	(0.030)	(0.038)	(0.028)	(0.028)	(0.041)	(0.019)	(0.021)	(0.024)
Mean Y	0.504	0.498	0.497	0.366	0.380	0.393	0.205	0.194	0.179
Panel B	Outcome = Log value of jewelry			Outcome = Log value of livestock			Outcome = Log value of saving		
High-temp shock	-1.349	-1.663	-2.274	5.786***	5.876***	4.677*	-0.809	-0.727	-0.642
	(1.441)	(1.415)	(1.725)	(2.093)	(2.003)	(2.579)	(1.054)	(1.011)	(1.414)
Low-temp shock	-6.238**	-6.656***	-9.495***	1.317	1.962	-0.339	1.650	1.634	2.217
	(2.675)	(2.564)	(3.362)	(3.564)	(3.778)	(5.349)	(1.891)	(1.709)	(2.287)
High-rain shock	0.523	0.546	0.694	0.152	0.051	-0.601	0.120	0.102	0.145
	(0.362)	(0.369)	(0.443)	(0.459)	(0.496)	(0.640)	(0.233)	(0.252)	(0.260)
Low-rain shock	-0.642	-0.546	-1.077**	-1.913***	-1.819***	-1.858***	0.231	-0.095	0.040
	(0.399)	(0.396)	(0.502)	(0.372)	(0.375)	(0.534)	(0.274)	(0.289)	(0.344)
Mean Y	6.928	6.833	6.818	4.538	4.716	4.891	2.938	2.770	2.567
Observations	12647	10940	6530	8840	7614	4466	12647	10940	6530
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for households, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

For livestock, prior work shows mixed responses: households may sell small stocks such as poultry and sheep to buffer against drought-related income shocks (Fafchamps et al., 1998), but may also retain livestock, a productive asset, when facing drought or temperature shocks (Kazianga and Udry, 2006; Cui and Tang, 2024). We find that low rainfall substantially reduces livestock in our setting. Further analyses show that this is driven by a large reduction in poultry ownership and a smaller reduction in non-poultry livestock (Table C.II.5), consistent with the use of easily liquidated assets (jewelry, poultry) to buffer against shocks. By contrast, high-temperature shocks are associated with large increases in livestock ownership, driven primarily by an increase in non-poultry animals (such as cattle), while low-temperature shocks have small, statistically insignificant impacts. These results are consistent with qualitative evidence that livestock rearing is particularly vulnerable to low rainfall as drought threatens fodder supplies (e.g., grass and corn), while in high-temperature periods livestock rearing can serve as an effective income-generating strategy; large livestock such as cattle can also provide draft power and manure for organic fertilizer (Watts et al., 2024).

Taken together, the results show that households manage weather-related income risks by shifting their asset composition. Easily liquidated assets such as jewelry and poultry are drawn down after adverse shocks, whereas ownership of larger, productive livestock tends to be maintained or even expanded. Overall, the evidence suggests a nuanced asset management strategy by rural households in response to weather shocks.

3.4.4 Other Responses to Weather Shocks

Other than labor reallocation and sales of liquid assets, several other mechanisms could serve to buffer household consumption from weather-induced income fluctuations.

Land Area Under Cultivation

We present the impacts of weather shocks on the total cultivated land area in columns 1 and 2 of Table 3.4. The results show that that rice-farming households expand their cultivation area in response to both high- and low-temperature shocks, while all farm households reduce their cultivated area when monsoon rainfall is low. Specifically, high-temperature shocks have a statistically significant impact on rice-farming households: the average high-temperature dosage of 14.9% of the monsoon season corresponds to a 32.1% increase in cultivated land area. The impacts of low-temperature shocks, although statistically insignificant, are comparable in magnitude: the average low-temperature dosage of 9.9% of the monsoon season corresponds to a 19.8% increase in cultivated land area among rice-farming households. In contrast, low monsoon rainfall is associated with a 31.7% decline in cultivated area among all farm households. Overall, these sizable effects suggest that adjustments in cultivated land area constitute an important channel through which Indonesian farmers respond to weather shocks.

Table 3.4: Weather shocks, farmland cultivated, and crop loss in the past year

	Log farmland cultivated (m^2)		Any crop loss		Any crop loss from disease	
	(1)	(2)	(3)	(4)	(5)	(6)
	Farm HH	Rice-farm HH	Farm HH	Rice-farm HH	Farm HH	Rice-farm HH
High-temp shock	0.974	2.153**	0.303***	0.476***	0.048	0.099**
	(0.703)	(0.882)	(0.101)	(0.130)	(0.031)	(0.042)
Low-temp shock	-0.214	2.001	0.147	0.262	0.059	0.182
	(1.311)	(1.631)	(0.231)	(0.340)	(0.089)	(0.126)
High-rain shock	0.123	0.126	-0.090***	-0.081***	-0.002	-0.000
	(0.154)	(0.216)	(0.031)	(0.030)	(0.008)	(0.009)
Low-rain shock	-0.317**	-0.374*	-0.080***	-0.132***	-0.010*	-0.021***
	(0.155)	(0.201)	(0.020)	(0.028)	(0.005)	(0.007)
Mean Y	5.076	6.537	0.137	0.178	0.016	0.019
Observations	10940	6530	7614	4466	7614	4466
Household FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for households, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

Crop Mix

Recent economics literature finds that smallholder farmers diversify their crops in response to extreme heat (Aragón et al., 2021; Cui and Tang, 2024) and adjust crop mix in response to long-run climate change (Cui and Zhong, 2024). Qualitative studies on Indonesia similarly document crop diversification as a common strategy farmers use to stabilize income amid climate uncertainty (Yusriadi et al., 2024).

We present the impact of temperature and rainfall shocks on crop mix in Table 3.5, where the outcome variable indicates whether each crop was planted on the household farm. Because the IFLS does not include data on the quantity produced or land area allocated to each crop, our outcome measure captures only the extensive margin of crop diversification, rather than the intensive margin of crop mix.

The upper panel of Table 3.5 reports the effects on the three most common crops: rice, corn, and cassava. High-temperature shocks significantly increase the likelihood of rice cultivation, while low-temperature shocks also have a sizable, though statistically insignificant, positive effect. Given that more than 40% of households grow rice by default and that nearly all sample days fall within the temperature range suitable for rice growth, the estimates in Table 3.5 suggest that households treat rice as a default diversification crop to buffer against temperature deviations.

High-temperature shocks have a moderately positive, though statistically insignificant, effect on rice-farming households' likelihood of growing corn (column 4), but do not affect the incidence of cassava cultivation. In contrast, low-temperature shocks substantially reduce corn cultivation and have a moderately negative, albeit statistically insignificant, effect on cassava. These results are consistent with corn and cassava having higher heat requirements than rice, and thus losing their comparative advantage during cool periods.

Rainfall shocks have negligible effects on the likelihood that farmers plant rice, corn, or cassava, likely reflecting the widespread availability of irrigation. However, rainfall

Table 3.5: Weather shocks and crop types

	(1) Farm HH	(2) Rice-farm HH	(3) Farm HH	(4) Rice-farm HH	(5) Farm HH	(6) Rice-farm HH
	Outcome var.= Rice		Outcome var.= Corn		Outcome var.= Cassava	
High-temp shock	0.292***	0.480***	0.018	0.132	0.053	0.003
	(0.092)	(0.147)	(0.095)	(0.137)	(0.093)	(0.128)
Low-temp shock	0.336	0.594*	-0.480***	-0.465*	-0.097	-0.276
	(0.217)	(0.348)	(0.180)	(0.251)	(0.187)	(0.248)
High-rain shock	-0.012	-0.014	0.005	0.003	-0.010	0.011
	(0.020)	(0.033)	(0.013)	(0.021)	(0.016)	(0.023)
Low-rain shock	-0.013	-0.023	-0.012	-0.011	0.002	0.018
	(0.021)	(0.035)	(0.025)	(0.035)	(0.024)	(0.029)
Mean Y	0.410	0.707	0.136	0.178	0.138	0.161
	Outcome var.= Soybean		Outcome var.= Rubber		Outcome var.= Coffee	
High-temp shock	-0.048	-0.102	-0.061	-0.075	-0.103***	-0.091*
	(0.056)	(0.091)	(0.048)	(0.059)	(0.039)	(0.053)
Low-temp shock	-0.126	-0.223	0.184	0.269*	-0.108	-0.078
	(0.104)	(0.168)	(0.126)	(0.155)	(0.088)	(0.107)
High-rain shock	0.006	0.014	-0.002	-0.005	0.008	0.007
	(0.010)	(0.016)	(0.010)	(0.014)	(0.007)	(0.009)
Low-rain shock	0.041**	0.071***	-0.002	0.000	0.023*	0.016
	(0.017)	(0.026)	(0.013)	(0.015)	(0.012)	(0.014)
Mean Y	0.038	0.061	0.065	0.069	0.057	0.043
Observations	10940	6530	10940	6530	10940	6530
Household FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: The outcome variables are binary indicators denoting whether the household farm produced each crop. Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for households, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

shocks may still affect the intensive margin, namely, the quantity of these crops planted. Although our data do not allow us to test this hypothesis directly, adjustments along the intensive margin are plausible given our finding that low rainfall reduces the total area under cultivation.

Turning to soybean (lower panel of Table 3.5), low rainfall significantly increases the likelihood of it being cultivated, which is consistent with soybean's role as a secondary crop grown during the dry season in rotation with rice (see Section 3.2.1). Since soybean thrives in well-drained soil, it serves as an ideal dry-season crop that compensates for reduced rice yields in years with low monsoon rainfall. For the two common cash crops, rubber and coffee, low-temperature shocks increase the likelihood of rubber, or latex, production among rice-farming households (column 4). There is also some evidence that high-temperature shocks reduce the probability of coffee cultivation (columns 4), which is consistent with prior studies showing that both Arabica and Robusta coffee require relatively narrow temperature ranges for optimal cultivation (Ramadhillah and Masjud, 2024; Kath et al., 2021) .

Taken together, these results are consistent with farmers' increasing crop diversification in response to weather shocks, while also taking into account the comparative advantage of each crop under different weather conditions.

Remittances and Informal Transfers

Given that remittances and informal transfers often serve an insurance function by cushioning the effects of adverse income shocks in LMICs (Combes and Ebeke, 2011), we also examine how weather shocks affect these transfers in our context. Results are presented in Appendix Table C.II.4. On average, 48.3% of rural households in our sample received remittances from family members living elsewhere, while 52.6% received informal transfers from friends and neighbors.[23] We find that low-temperature shocks substantially increase

[23]The IFLS surveys recorded the amount of transfers each individual received from (i) family members, such as spouses and relatives, not residing in the same household, and (ii) friends or neighbors, over

the likelihood of receiving remittances and transfers, as well as the amount received. Low rainfall also increases the likelihood and amount of financial assistance from friends and neighbors received by rice-farming households. The impact of high-temperature and high-rainfall shocks are relatively small in magnitude.

Other than remittances, seasonal or long-term migration may be another common response to agricultural productivity shocks. However, we cannot study the impact on migration in our context because the IFLS surveys do not provide sufficient information on migration. Although we can track individuals' location across survey waves to infer long-term migration, we do not observe the timing of such movements. Data on short-term seasonal migration is available only in Wave 3. Based on this data, around 11% of rural individuals conducted seasonal migration lasting more than 2 months.

The patterns suggest that informal transfers function as a complementary insurance channel. However, the limited migration data restricts our ability to fully assess the broader landscape of household mobility as a coping mechanism.

3.5 Conclusion

This paper examines how rural households in Indonesia adjust to short-term temperature and rainfall shocks, focusing on labor allocation, asset sales, land use, and crop choice. Combining nationally representative household panel with high-resolution meteorological data, we find that temperature deviations — both above and below seasonal norms — substantially reduce household expenditure. By contrast, low rainfall has a smaller and statistically insignificant effect on household expenditure, likely reflecting widespread access to irrigation.

Households respond to these adverse shocks by drawing down liquid assets and intensifying agricultural labor. Rather than reallocating labor to non-agricultural sec-

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the past year. We aggregate these transfers across household members to derive the total amount each household received from each source category. Most remittances in our sample originated from non-spousal relatives.

tors, farmers increase agricultural hours in response to adverse temperature and rainfall shocks, suggesting that adverse weather conditions often heighten, rather than reduce, agricultural labor demand in Indonesia. This response is especially pronounced among rice-farming households and in villages with technical irrigation systems, likely due to the labor intensity of irrigation and other agricultural activities such as weeding and pest control. At the same time, temperature shocks increase land area under cultivation, whereas low rainfall reduces it.

Our findings suggest that the adaptive capacity of Indonesian farmers is constrained by structural factors such as limited mechanization and weak non-agricultural sectors. The contrast between households' relative resilience to rainfall shocks and their vulnerability to temperature fluctuations highlights an important policy gap. While irrigation mitigates rainfall deficits, comparable technologies or institutions for managing temperature stress — such as improved crop varieties, early pest and disease detection systems, and better access to best agricultural practices — remain underdeveloped (Knox et al., 2012).

More broadly, our results underscore that climate change in the humid tropics is not only about extreme heat or drought, but also about climate variability around historically stable conditions. Temperature deviations in either direction disrupt production and livelihoods, underscoring the need for climate policies that address variability as much as mean change.

Finally, although the panel data methods and fixed effects approach allows us to isolate the impact of weather shocks from other time-varying shocks to agricultural productivity, they also restrict our focus to farmers' short-run ex post responses to realized weather shocks. However, adaptation to climate change involves both ex post responses to realized weather shocks and ex ante adaptive investments taken in anticipation of potential weather shocks (Carleton et al., 2024). As past studies have shown, farmers' short-term responses to realized weather shocks may be quite different from their adaptive behavior to long-run trends (Colmer, 2021; Liu et al., 2023). Future research, as well as policy interventions, should focus on strengthening both ex post coping mechanisms and ex ante adaptive investments, as farmers' ability to cope with climate variability will depend on both. Expanding access to crop insurance, promoting climate-resilient and pest-tolerant crop varieties, and investing in mechanization and rural infrastructure may reduce the labor burden of climate adaptation. Complementary investments in non-farm employment opportunities could also broaden rural households' capacity to diversify income sources.

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Appendix A

CHAPTER 1

Appendix A.I. Further Robustness Checks

A.I.1 Testing JSY Effects Among APL Women with Similar Characteristics to BPL

A potential concern in linking the main finding that JSY increased unplanned births to the mechanism analysis involving access to community health workers is that the two results are estimated on different samples. The main analysis focuses on women above the poverty line and not from Scheduled Castes or Tribes, while the mechanism analysis uses data on births to women who are either below the poverty line or from SC or ST groups. If these populations differ systematically in how they interact with health workers or respond to JSY incentives, the interpretation that increased access to ASHAs explains the rise in unplanned births may be weakened.

To bridge the sample difference between the main and mechanism analyses, I restrict the main analysis sample to above-poverty-line (APL) women in the bottom two quintiles of the wealth index, representing the poorest segment of the APL population. Although these women are technically above the poverty line, their socioeconomic characteristics more closely resemble those of below-poverty-line (BPL) women included in the mechanism analysis. This subgroup is more likely to have comparable exposure to ASHAs and to face similar constraints in exercising fertility preferences.

Table A.I.1 presents the results from this subsample analysis. The estimated effect of JSY on unplanned births remains positive and statistically significant among poorer APL women, with magnitudes similar to those in the main specification. The effect is again concentrated among younger women, significant at the 1% level. These results show that JSY increased unplanned births even among women whose socioeconomic status is closer to that of the BPL and SC/ST populations used in the mechanism analysis. While this test does not directly evaluate the mechanism, it supports the use of the BPL/SCST sample to examine how increased access to community health workers may have contributed to the observed rise in unplanned births.

Table A.I.1: Impact of JSY on Unplanned Births Among Poorer APL Women

	Unplanned births					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Younger	Older	All	Younger	Older
Treat × Post	0.040*	0.049***	0.022			
	(0.020)	(0.018)	(0.046)			
Treat × Post (2005-2008)				0.026	0.054**	-0.067
				(0.023)	(0.022)	(0.061)
Treat × Post (2009-2016)				0.041*	0.049**	0.028
				(0.022)	(0.019)	(0.047)
Control mean (pre)	0.278	0.205	0.507	0.278	0.205	0.507
Observations	25295	17923	7370	25295	17923	7370
Fixed effects	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Note: The dependent variable is an indicator for whether a woman's most recent birth in the past five years was unplanned. The data come from the NFHS of India, waves 2–4. The sample is restricted to the poorest subgroup within the main sample—rural women from higher castes and above the poverty line. Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for birth year, birth order, interview year, and state. Standard errors are clustered at the state level and reported in parentheses. Significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

A.I.2. Placebo Test: Ineligible Births Above Second Parity

To assess the internal validity of the mechanism analysis, I conduct a placebo test using births that were not eligible for JSY benefits. Specifically, I restrict the sample to third and higher-order births among rural women below the poverty line or from Scheduled Castes or Tribes. These higher-parity births were excluded from the main mechanism analysis in Section 1.7, which focused on JSY-eligible first and second births, in line with the program's parity-based eligibility rule in high-performing states (HPS). Since JSY incentives do not apply beyond the second birth, these women should not have experienced increased engagement with ASHAs following the 2009 reform. Although the sample size is limited, this exercise provides a useful robustness check.

Table A.I.2 reports the results. The interaction term between HPS status and the post-reform period is statistically insignificant in all columns, with point estimates close to zero. Among younger women, the coefficient is positive but not statistically distinguishable from zero, while the estimates for the full sample and older women are slightly negative and also insignificant.

Table A.I.2: The Impact of Health Worker Access on Unplanned Births Beyond Second Parity

	Unplanned birth		
	(1)	(2)	(3)
	All	Younger	Older
HPS $\times$ Post	-0.012	0.114	-0.031
	(0.108)	(0.137)	(0.190)
Control mean (pre)	0.728	0.651	0.796
Observations	177	103	70
Fixed effects	✓	✓	✓

Note: The dependent variable is an indicator for whether the birth was unplanned. The data come from IHDS-II (2011–12). The sample is restricted to births to rural women from Scheduled Castes or Tribes or those below the poverty line. All regressions include fixed effects for birth year, birth order, state, and mother. Standard errors, clustered at the state level, are reported in parentheses. Significance is indicated as follows: * p<0.1, ** p<0.05, *** p<0.01.

These null results lend support to the interpretation that the rise in unplanned births observed in the main analysis is specific to JSY-eligible women and is not driven by broader trends affecting all women. The placebo test helps rule out alternative explanations and reinforces the claim that increased health worker engagement contributed to the rise in unplanned births among younger women.

Appendix A.II. Tables

Table A.II.1: Summary Statistics

	Treatment			Control			
Variable	N	Mean	SD	N	Mean	SD	Diff
A. Fertility analysis (H1)							
Age (years)	18426	29.4	5.83	20453	30.1	5.76	–0.702***
Number of births in past 5 years	18426	.963	.876	20453	.727	.812	0.236***
Number of children	18426	2.84	1.53	20453	2.38	1.2	0.464***
Number of children above age 5	18426	1.88	1.69	20453	1.63	1.43	0.253***
Birth order of the last child	18426	3.21	1.78	20453	2.57	1.34	0.638***
Household head - Hindu	18426	.82	.384	20453	.779	.415	0.041
Household head - Muslim	18426	.165	.371	20453	.111	.315	0.053
Years of schooling (imp)	18426	3.13	4.2	20453	5.21	4.54	–2.075***
Any contraceptive use	18426	.451	.498	20453	.645	.479	–0.194***
Modern contraceptive use	18426	.375	.484	20453	.567	.495	–0.192***
B. Unplanned births analysis (H2)							
Age (years)	10137	26.7	4.93	8890	26.5	4.6	0.287
Number of children	10137	2.7	1.59	8890	2.17	1.23	0.525***
Household head - Hindu	10137	.793	.405	8890	.753	.431	0.040
Household head - Muslim	10137	.195	.396	8890	.132	.338	0.063
Years of schooling (imp)	10137	3.2	4.24	8890	5.85	4.66	–2.656***
Unplanned birth	10137	.357	.479	8890	.278	.448	0.079***
Birth order of the last child	10137	3.04	1.86	8890	2.34	1.37	0.708***
C. Mechanism analysis (H3)							
Age (years)	2102	28.1	4.41	2870	29.2	4.83	–1.164***
No. children	2102	2.64	1.1	2870	3.27	1.48	–0.636***
Below poverty line	2102	.525	.499	2870	.675	.469	–0.149**
Scheduled caste/tribe	2102	.74	.439	2870	.651	.477	0.089
Woman's years of education	2102	5.09	4.29	2870	2.6	3.69	2.498***
Unplanned Birth	2102	.223	.416	2870	.329	.47	–0.105***

Note: Panel A includes the sample for the fertility analysis (H1), and Panel B includes the sample for the unplanned births analysis (H2). In both panels, the treatment group consists of rural women in low-performing states (LPS) who are above the poverty line and do not belong to Scheduled Castes or Tribes (SC/ST). Panel B further restricts the sample to women who had a birth within the past five years. Panel C presents the sample for the mechanism analysis (H3), focusing on the health worker channel. This sample includes births from rural women who are either below the poverty line or belong to SC/ST groups. The treatment group in Panel C consists of births from rural women residing in high-performing states (HPS).

Table A.II.2: Summary Statistics for Regression Discontinuity Sample from DLHS-4

Variable	N	Mean	SD
Total live births	21315	1.74	.897
Mother's age in completed years	21614	24.3	2.82
Years of Education	21307	6.47	4.6
Has fridge	21614	.176	.38
Own house	21614	.939	.238
ASHA encouraged facility delivery	21614	.225	.417
Any ASHA engagement	21614	.354	.478

The sample is from DLHS-4 (2012–13) and includes JSY-eligible women aged 19–40 residing in high-performing states (HPS), with no more than two previous live births. The sample includes births conceived before and after the 2009 reform, and is used in the health worker channel analysis (H3)

Table A.II.3: Summary of Analyses, Data, and Empirical Strategies

Analysis	**Main Outcome(s)**	**Data Source**	**Empirical Strategy**
H1 Fertility and Contraceptive Use	Number of Births; Contraceptive use	NFHS Rounds 2–4	Difference-in-Differences (LPS vs. HPS)
H2 Unplanned Births	Indicator for unplanned birth	NFHS Rounds 2–4	Difference-in-Differences (LPS vs. HPS)
H3 Health Worker Channel	Unplanned births; ASHA contact	IHDS (2011–12); DLHS 4	DiD exploiting 2009 reform; Regression Discontinuity
Perceived Discordance Mechanism	Perceived fertility preferences	IHDS Panel (2005–06, 2011–12)	DiD exploiting 2009 reform
Abortion Attitude Mechanism	Standardized (z-score) abortion attitude	WVS Waves 4–6 (2001–2012)	Triple Differences

Table A.II.4: Pre-Treatment Trends in Fertility

	Number of births over the past 5 years		
	(1)	(2)	(3)
	All women	Younger (19-29)	Older (30-40)
Year of Interview	-0.005*	-0.018***	0.008***
	(0.003)	(0.004)	(0.003)
Year of Interview × Treat	0.009*	0.011*	0.007*
	(0.005)	(0.006)	(0.004)
Control mean (pre)	0.666	0.975	0.351
Observations	43997	23299	20697
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children over age five, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include state fixed effects. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.II.5: Pre-Treatment Trends in Fertility (Excluding Uttar Pradesh)

	Number of births over the past 5 years		
	(1)	(2)	(3)
	All women	Younger (19-29)	Older (30-40)
Year of Interview	-0.005**	-0.018***	0.007***
	(0.003)	(0.004)	(0.003)
Year of Interview × Treat	0.005	0.008	0.004
	(0.005)	(0.008)	(0.004)
Control mean (pre)	0.666	0.975	0.351
Observations	38156	19962	18193
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16), excluding Uttar Pradesh. Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children over age five, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include state fixed effects. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

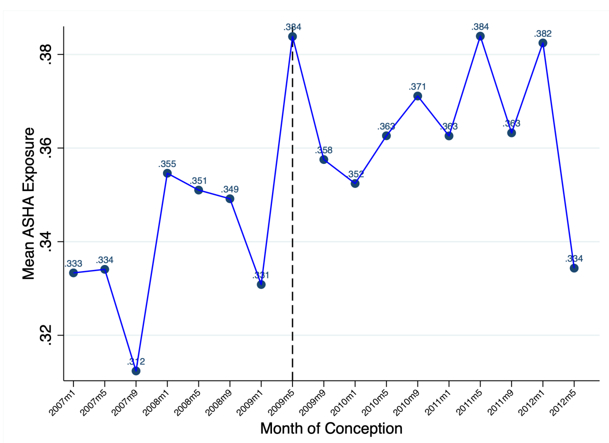
Table A.II.6: F-test Results of Pre-Trends in Event Study Specifications

	(1)	(2)
	F-value	P-value
Unplanned birth (All)	1.3697	.2483
Unplanned birth (Younger)	.8905	.5462
Unplanned birth (Older)	4.591	.0009

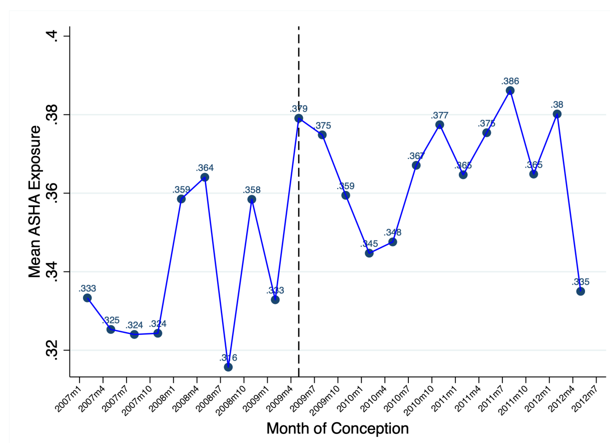
Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include state fixed effects for birth order, interview year, and state. Standard errors are clustered at the state level and reported in parentheses.

Appendix A.III. Figures

Figure A.III.1: ASHA Exposure by Conception Cohort



Panel A: 4-Month Intervals



Panel B: 3-Month Intervals

Appendix B

CHAPTER 2

B.I. Appendix Tables

Table B.I.1: Summary Statistics

	count	mean	sd	min	max
IPV helpline calls (per 10,000)	18240	1.801	1.780	0	17.670
Anomalously hot days (MaxT $\geq$ LR+3°C)	18240	1.714	3.217	0	27.000
Anomalously cold days (MaxT $\leq$ LR$-$3°C)	18240	0.733	1.257	0	12.000
Days with MaxT $<$ 10°C	18240	0.841	2.631	0	30.000
Days with MaxT 10–15°C	18240	9.768	11.642	0	31.000
Days with MaxT 15–20°C	18240	7.731	10.090	0	31.000
Days with MaxT 20–25°C	18240	5.637	9.907	0	31.000
Days with MaxT 25–30°C	18240	4.103	8.167	0	31.000
Days with MaxT $>$ 30°C	18240	2.359	6.293	0	31.000
High rainfall shock (z-score $>$ 1)	18240	0.157	0.364	0	1.000
Low rainfall shock (z-score < -1)	18240	0.171	0.377	0	1.000

Table B.I.2: Effect of Temperature Anomalies on IPV Calls: Alternative Anomaly Definitions

	IPV calls					
	(1)	(2)	(3)	(4)	(5)	(6)
	Max T $\geq$ LR+3°C	Max T $\geq$ LR+4°C	Max T $\geq$ LR+2°C	Mean T $\geq$ LR+3°C	Max T $\geq$ p90	Max T $\geq$ p95
High-temp shock ($+3°C$)	-0.001	0.008	-0.005	0.002	-0.002	0.000
	(0.009)	(0.019)	(0.006)	(0.028)	(0.005)	(0.007)
High-temp shock ($+3°C$) $\times$ Urban	0.029**	0.052*	0.012*	0.065	0.007	0.014
	(0.013)	(0.029)	(0.007)	(0.042)	(0.007)	(0.010)
Low-temp shock ($-3°C$)	-0.024*	-0.064***	-0.012	-0.099**	-0.009	-0.002
	(0.014)	(0.023)	(0.008)	(0.046)	(0.007)	(0.012)
High-temp effect, urban	0.028	0.060	0.006	0.067	0.005	0.014
P-value	[0.023]	[0.017]	[0.310]	[0.048]	[0.430]	[0.126]
Observations	18,240	18,240	18,240	18,240	18,240	18,240
Mean dep. var.	1.801	1.801	1.801	1.801	1.801	1.801
Year $\times$ Month FE	✓	✓	✓	✓	✓	✓
Province $\times$ Month FE	✓	✓	✓	✓	✓	✓

Note: The dependent variable is the monthly number of IPV helpline calls per 10,000 population. Anomalously hot (cold) days are defined as days per month where the daily maximum temperature exceeds (falls below) the province- and calendar-month-specific long-run mean (1985–2014) by more than the indicated threshold. The p90 and p95 specifications use the province- and calendar-month-specific 90th and 95th percentiles of the baseline distribution as the threshold, respectively. Urban provinces are those with an urbanization rate above the sample median (34.3 percent) based on the 2007 Peruvian Census. *High-temp effect, urban* reports the total effect in urban provinces, computed as the sum of the base coefficient and the interaction term. All specifications control for high and low rainfall shocks and their interactions with urban status; the corresponding coefficients are not reported for brevity. Standard errors clustered at the province level are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.I.3: Temperature Shocks and Household Income

	Log net income		Log expenditure		Non-labor income	
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Farm	All	Farm	All	Farm
High-temp shock (+3°C)	0.004	0.005	0.001	0.003	-1.459	-2.797
	(0.006)	(0.007)	(0.005)	(0.005)	(4.733)	(3.913)
High-temp shock (+3°C) × Urban	-0.002	-0.006	-0.001	-0.005	8.684	1.582
	(0.007)	(0.008)	(0.005)	(0.006)	(5.285)	(5.667)
Low-temp shock (−3°C)	0.007	0.006	-0.011	-0.009	-2.671	-2.124
	(0.012)	(0.012)	(0.008)	(0.009)	(6.555)	(6.573)
Low-temp shock (−3°C) × Urban	-0.006	-0.019	0.011	0.003	0.341	7.928
	(0.013)	(0.015)	(0.008)	(0.010)	(9.569)	(9.441)
High-temp effect, urban	0.002	-0.001	-0.000	-0.002	7.225	-1.215
P-value	[0.430]	[0.888]	[0.991]	[0.618]	[0.052]	[0.804]
Mean Y	7.632	7.070	7.512	7.034	890.506	401.430
Observations	31135	15219	31135	15219	31135	15219
Household FE	✓	✓	✓	✓	✓	✓
Year × Month FE	✓	✓	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓	✓	✓

The outcome variables are the logs of monthly household net income, monthly household total expenditure, and monthly household non-labor income, all deflated. Odd-numbered columns use the full sample; even-numbered columns restrict to farm households. All regressions include interactions between temperature shocks and urban residence indicators. High- and low-rainfall shock controls and their interactions are included in all regressions but omitted from the table for brevity. *High-temp effect, urban* reports the marginal effect of high-temperature shocks in urban areas. Regressions are weighted using household sampling weights and include household, month-by-year, and province-by-year fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.I.4: Temperature Shocks and Gender Gaps in Labor Outcomes

	(1) Log labor income	(2) Worked	(3) Wage employment
High-temp shock (+3°C)	-0.009	0.005	-0.002
	(0.021)	(0.004)	(0.003)
High-temp shock (+3°C) × Urban	0.009	-0.005	0.002
	(0.024)	(0.004)	(0.004)
High-temp shock (+3°C) × Female	0.016	-0.001	0.001
	(0.020)	(0.003)	(0.004)
High-temp shock (+3°C) × Urban × Female	-0.038	-0.005	-0.007*
	(0.024)	(0.004)	(0.004)
Low-temp shock (−3°C)	0.061	0.000	0.003
	(0.039)	(0.006)	(0.005)
Low-temp shock (−3°C) × Urban	-0.048	0.005	-0.004
	(0.043)	(0.007)	(0.006)
Low-temp shock (−3°C) × Female	-0.032	0.000	0.003
	(0.038)	(0.007)	(0.005)
Low-temp shock (−3°C) × Urban × Female	0.008	-0.006	-0.007
	(0.045)	(0.009)	(0.007)
Female–male differential (urban)	-0.022	-0.005	-0.006
P-value	[0.087]	[0.029]	[0.000]
Female–male differential (rural)	0.016	-0.001	0.001
P-value	[0.421]	[0.789]	[0.746]
Mean Y	2.388	0.675	0.300
Observations	80400	80288	80288
Individual FE	✓	✓	✓
Year × Month FE	✓	✓	✓
Province × Year FE	✓	✓	✓

Note: The outcome variables are log labor income, an indicator for having worked in the last week, and an indicator for wage employment. All regressions include fully interacted temperature shock variables with urban residence and female indicators. Rainfall shock controls and their interactions are included in all regressions but omitted from the table for brevity. *Female–male differential* reports the marginal effect of high-temperature shocks for females relative to males in urban and rural areas, respectively. Regressions are weighted using individual sampling weights and include individual, month-by-year, and province-by-year fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.I.5: Temperature Shocks and Mobility

	(1) Park	(2) Workplace	(3) Transit	(4) Residential	(5) Retail
Panel A: All					
Hot day (+3°C)	4.174***	-0.847	1.120	-0.288**	-0.377
	(1.112)	(0.818)	(0.740)	(0.115)	(0.816)
Cold day (−3°C)	-4.513***	-1.063**	-1.359***	0.728***	-2.001***
	(0.651)	(0.487)	(0.451)	(0.171)	(0.481)
High rainfall shock	-2.296***	0.146	-0.277	0.097**	-0.236
	(0.474)	(0.350)	(0.231)	(0.049)	(0.308)
Low rainfall shock	1.332*	1.358**	-0.352	-0.063	-0.583
	(0.693)	(0.527)	(0.436)	(0.067)	(0.442)
Panel B: Urban Heterogeneity					
Hot day (+3°C)	3.341	-5.281***	-0.683	-0.117	-0.911
	(2.077)	(1.421)	(0.453)	(0.245)	(1.498)
Hot day (+3°C) × Urban	1.028	5.558***	2.257**	-0.208	0.650
	(2.481)	(1.664)	(1.094)	(0.275)	(1.720)
Cold day (−3°C)	-4.598***	-0.392	-0.480	0.203	1.075**
	(1.517)	(1.496)	(0.388)	(0.225)	(0.503)
Cold day (−3°C) × Urban	0.166	-0.748	-1.053	0.639**	-3.771***
	(1.706)	(1.620)	(0.652)	(0.291)	(0.777)
High-temp effect, urban	4.370	0.278	1.574	-0.325	-0.261
P-value	[0.001]	[0.749]	[0.093]	[0.012]	[0.777]
Mean Y	23.602	28.188	-7.634	5.128	12.099
Observations	37468	37468	37468	37468	37468
Year × Month FE	✓	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓	✓
Day-of-week FE	✓	✓	✓	✓	✓

Note: The outcome variables are percentage changes in visits to parks, workplaces, transit stations, residential areas, and retail and recreation locations relative to a pre-pandemic baseline. All regressions include rainfall controls. Panel B additionally includes interactions between the hot-day and cold-day variables and the urban indicator. *High-temp effect, urban* reports the marginal effect of hot days in urban provinces. Regressions include year-by-month, day-of-week, and province-by-year fixed effects. Standard errors clustered at the province level are reported in parentheses. The sample covers October 2021 to October 2022. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.I.6: Temperature Shocks and Alcohol Consumption

	Any Alcohol Use in Past 30 Days	
	(1)	(2)
	All Males	Among Ever-Drinkers
High-temp shock (+3°C)	0.002	0.002
	(0.004)	(0.004)
High-temp shock (+3°C) × Urban	-0.002	-0.002
	(0.004)	(0.004)
Low-temp shock (−3°C)	0.001	-0.001
	(0.007)	(0.007)
Low-temp shock (−3°C) × Urban	-0.001	0.000
	(0.008)	(0.008)
High-temp effect, urban	0.000	-0.000
P-value	[0.904]	[0.924]
Mean Y	0.446	0.466
Observations	58271	55493
District FE	✓	✓
Year × Month FE	✓	✓
Province × Year FE	✓	✓

The outcome variable is an indicator for any alcohol consumption in the past 30 days. All regressions include interactions between temperature shock variables and the urban indicator. Rainfall shock controls and their interactions with the urban indicator are included in all regressions but omitted from the table for brevity. All regressions additionally control for age, cohabitation status, refrigerator ownership, electricity access, education level, and household size. *High-temp effect, urban* reports the marginal effect of high-temperature shocks in urban areas. Regressions are weighted using household sampling weights and include district, year-by-month, and province-by-year fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.I.7: Temperature Shocks and Labor Supply

	(1) Worked last week	(2) Wage worker	(3) Hours worked (total)	(4) Hours worked (primary)	(5) Hours worked (secondary)
High-temp shock (+3°C)	0.005	-0.002	0.103	0.006	0.097
	(0.004)	(0.003)	(0.190)	(0.178)	(0.077)
High-temp shock (+3°C) × Urban	-0.005	0.002	-0.052	0.152	-0.204**
	(0.004)	(0.004)	(0.214)	(0.209)	(0.091)
High-temp shock (+3°C) × Female	-0.001	0.001	0.087	-0.014	0.101
	(0.003)	(0.004)	(0.168)	(0.144)	(0.069)
High-temp shock (+3°C) × Urban × Female	-0.005	-0.007*	-0.282	-0.316*	0.035
	(0.004)	(0.004)	(0.206)	(0.189)	(0.081)
Low-temp shock (−3°C)	0.000	0.003	0.196	0.148	0.048
	(0.006)	(0.005)	(0.305)	(0.289)	(0.200)
Low-temp shock (−3°C) × Urban	0.005	-0.004	-0.055	0.039	-0.094
	(0.007)	(0.006)	(0.347)	(0.337)	(0.209)
Low-temp shock (−3°C) × Female	0.000	0.003	-0.209	0.011	-0.220
	(0.007)	(0.005)	(0.322)	(0.308)	(0.167)
Low-temp shock (+3°C) × Urban × Female	-0.006	-0.007	-0.107	-0.415	0.308*
	(0.009)	(0.007)	(0.396)	(0.385)	(0.186)
High-temp effect, urban female	-0.006	-0.005	-0.144	-0.173	0.029
P-value	[0.005]	[0.000]	[0.039]	[0.007]	[0.374]
High-temp effect, urban male	-0.000	0.001	0.050	0.158	-0.107
P-value	[0.792]	[0.687]	[0.610]	[0.153]	[0.018]
High-temp effect, rural female	0.004	-0.000	0.190	-0.008	0.198
P-value	[0.391]	[0.840]	[0.300]	[0.961]	[0.019]
Mean Y	0.675	0.300	29.260	26.858	2.402
Observations	80288	80288	80288	80288	80288
Individual FE	✓	✓	✓	✓	✓
Year × Month FE	✓	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓	✓

Note: The outcome variables are indicators for having worked in the last week and wage employment, hours worked across all jobs, hours worked in the primary job, and hours worked in secondary jobs. All regressions include fully interacted temperature shock variables with urban residence and female indicators. Rainfall shock controls and their interactions are included in all regressions but omitted from the table for brevity. *High-temp effect, urban female* (*urban male*, *rural female*) reports the total effect of the high-temperature shock for urban women (urban men, rural women), computed as the sum of the relevant coefficients from the triple-interaction specification. Regressions are weighted using individual sampling weights and include individual, month-by-year, and province-by-year fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.I.8: Temperature Shocks and Mental Distress Index

	Mental Distress Index	
	(1)	(2)
High-temp shock (+3°C)	-0.007	-0.002
	(0.008)	(0.003)
High-temp shock (+3°C) × Urban	0.012	
	(0.008)	
High-temp shock (+3°C) × High Urban		0.020***
		(0.006)
Low-temp shock (−3°C)	0.012	0.008
	(0.014)	(0.007)
Low-temp shock (−3°C) × Urban	-0.006	
	(0.017)	
Low-temp shock (−3°C) × High Urban		-0.019
		(0.016)
High-temp effect, urban	0.005	0.018
P-value	[0.149]	[0.001]
Mean Y	-0.175	-0.175
Observations	58271	58271
District FE	✓	✓
Year × Month FE	✓	✓
Province × Year FE	✓	✓

Note: The outcome variable is the mental distress index. Column (1) includes interactions between temperature shock variables and the urban indicator, while Column (2) includes interactions between temperature shock variables and the high-urban indicator. Rainfall shock controls and their interactions with the urban indicator are included in all regressions but omitted from the table for brevity. All regressions additionally control for age, cohabitation status, refrigerator ownership, electricity access, education level, and household size. *High-temp effect, urban* reports the marginal effect of high-temperature shocks in urban or high-urban areas, respectively. Regressions are weighted using household sampling weights and include district, year-by-month, and province-by-year fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.I.9: Temperature Shocks and Community Violence

	(1) All Crimes	(2) General safety offenses	(3) Public-safety Crimes	(4) Crimes against health and body	(5) Crimes against liberty
Panel A: All					
High-temp shock (+3°C)	0.008	0.009**	0.005**	0.001	0.003**
	(0.006)	(0.004)	(0.002)	(0.002)	(0.001)
Low-temp shock ($-$3°C)	-0.003	-0.006	-0.003	-0.004	-0.000
	(0.014)	(0.010)	(0.005)	(0.006)	(0.003)
High rainfall shock	-0.005	0.003	0.010	-0.014	0.007
	(0.044)	(0.022)	(0.014)	(0.013)	(0.010)
Low rainfall shock	0.012	0.018	0.018	0.001	-0.001
	(0.051)	(0.026)	(0.018)	(0.012)	(0.009)
Panel B: Urban Heterogeneity					
High-temp shock (+3°C)	-0.005	0.006*	0.004**	0.001	0.001
	(0.008)	(0.003)	(0.002)	(0.002)	(0.002)
High-temp shock (+3°C) × Urban	0.026**	0.007	0.001	0.002	0.004**
	(0.011)	(0.006)	(0.003)	(0.003)	(0.002)
Low-temp shock ($-$3°C)	0.031**	0.001	-0.000	-0.004	0.005
	(0.013)	(0.013)	(0.005)	(0.008)	(0.004)
Low-temp shock ($-$3°C) × Urban	-0.063**	-0.013	-0.004	0.001	-0.010*
	(0.026)	(0.017)	(0.009)	(0.011)	(0.005)
High-temp effect, urban	0.020	0.012	0.005	0.002	0.005
P-value	[0.012]	[0.032]	[0.139]	[0.366]	[0.002]
Mean Y	5.447	1.973	0.710	0.740	0.523
Observations	16464	16464	16464	16464	16464
Year × Month FE	✓	✓	✓	✓	✓
Province × Year FE	✓	✓	✓	✓	✓

Note: The outcome variables are the monthly number of all crime reports, general safety offense reports, public-safety crime reports, crime reports against health and body, and crime reports against liberty, all measured per 10,000 population. *High-temp effect, urban* reports the total effect of high-temperature shocks in urban provinces, computed as the sum of the relevant base coefficient and the interaction term. Regressions include year-by-month and province-by-year fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B.I.10: Temperature Shocks and Other Crime Reports

	(1) Administrative Offense Reports	(2) Fraud Reports
Panel A: All		
High-temp shock (+3°C)	0.002**	-0.002**
	(0.001)	(0.001)
Low-temp shock (-3°C)	0.001	-0.000
	(0.002)	(0.002)
High rainfall shock	-0.006	0.004
	(0.006)	(0.007)
Low rainfall shock	-0.009	-0.003
	(0.006)	(0.006)
Panel B: Urban Heterogeneity		
High-temp shock (+3°C)	0.002***	-0.003***
	(0.001)	(0.001)
High-temp shock (+3°C) × Urban	-0.001	0.003
	(0.002)	(0.002)
Low-temp shock (-3°C)	0.003**	0.000
	(0.002)	(0.003)
Low-temp shock (-3°C) × Urban	-0.004	-0.001
	(0.003)	(0.004)
High-temp effect, urban	0.002	-0.001
P-value	[0.213]	[0.637]
Mean Y	0.176	0.337
Observations	16464	16464
Year × Month FE	✓	✓
Province × Year FE	✓	✓

Note: The outcome variables are the monthly number of administrative offense reports per 10,000 population (column 1) and fraud reports per 10,000 population (column 2), drawn from SIDPOL police records (2018–2024). The treatment variable is the count of anomalously hot (cold) days in the month, defined as days on which the daily maximum temperature exceeds (falls below) the province-specific calendar-month long-run mean by more than 3°C. All regressions include rainfall shock controls. Panel B additionally includes interactions between temperature shock and rainfall shock variables and the urban indicator. *High-temp effect, urban* reports the total effect of high-temperature shocks in urban provinces, computed as the sum of the relevant base coefficient and the interaction term. Regressions include year-by-month and province-by-year fixed effects. Standard errors clustered at the province level are reported in parentheses. Statistical significance is indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B.II. Appendix Figures

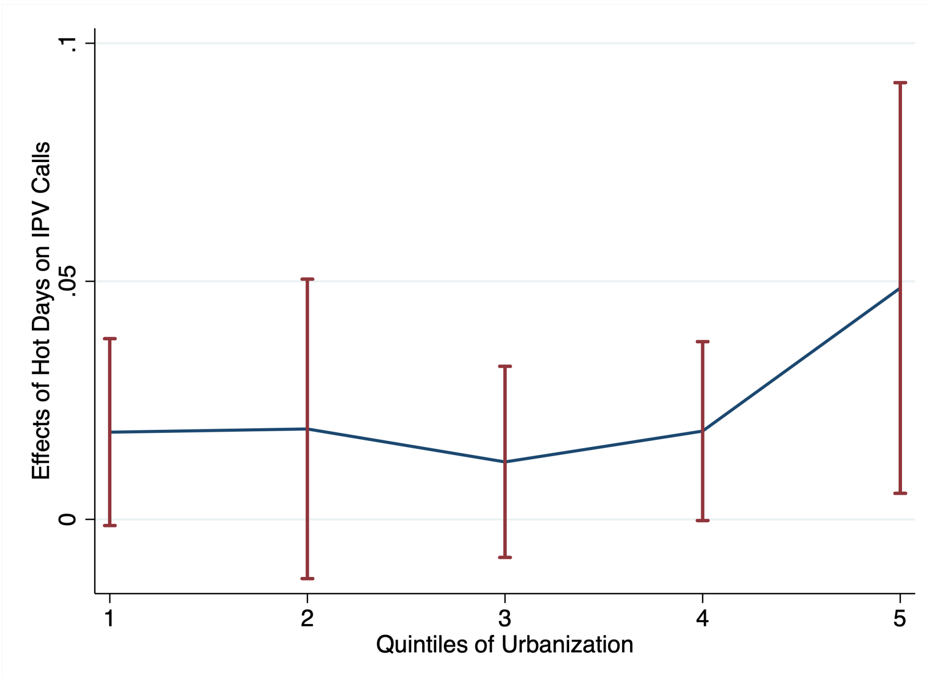


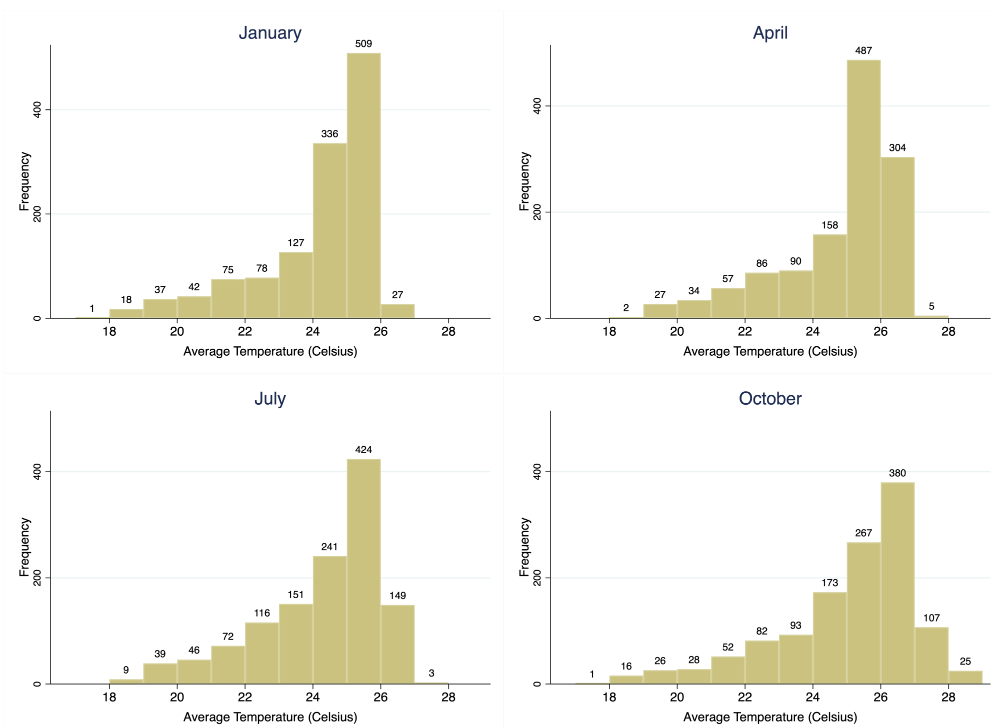
Figure B.II.1: Effect of Anomalously Hot Days on IPV Calls by Urbanization Quintile

Appendix C

CHAPTER 3

C.I. Appendix Figures

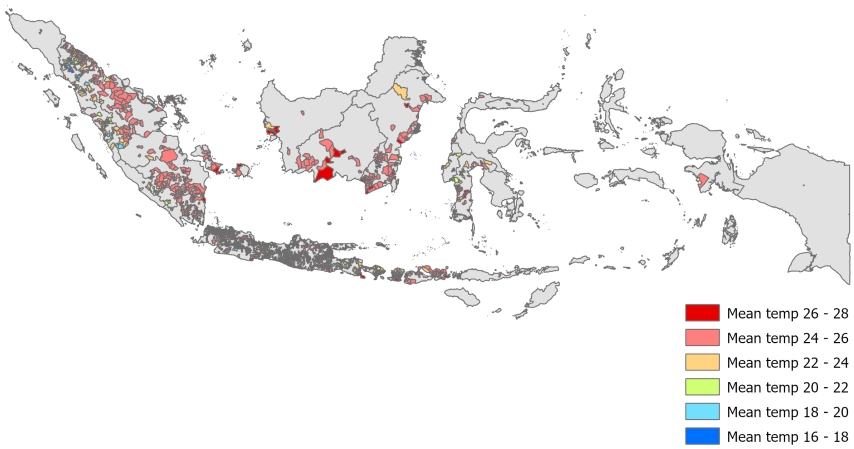
Figure C.I.1: Distribution of long-run monthly mean temperature in January, April, July, and October



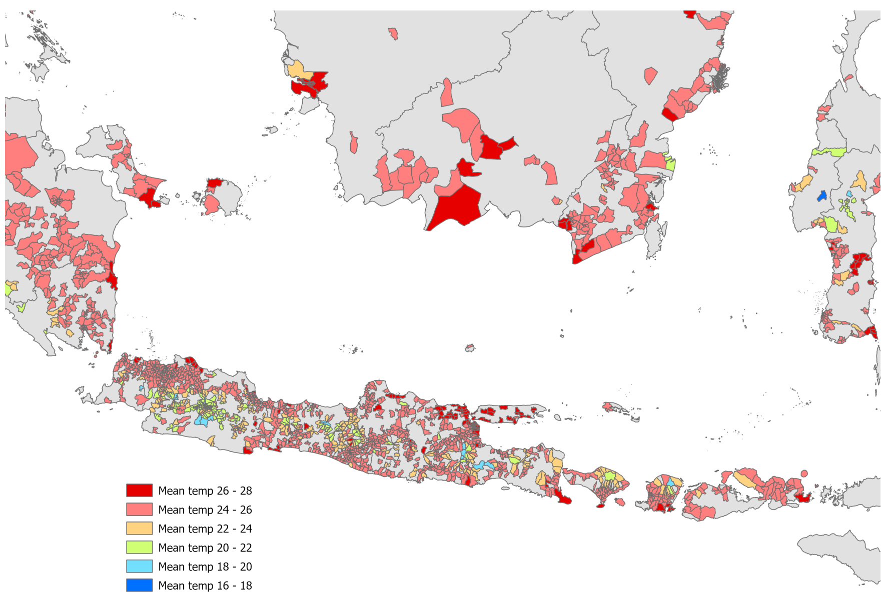
Notes: Long-run monthly mean temperatures are calculated using temperature data from 1973 to 1999. Unit of observation is kecamatan (sub-district). Sample includes only the 1,250 rural kecamatans surveyed in the IFLS. Label on top of each bar shows the number of kecamatans with long-run monthly mean temperature falling in the bin.

Figure C.I.2: **January** long-run mean temperatures across IFLS sub-districts

ALL IFLS sub-districts



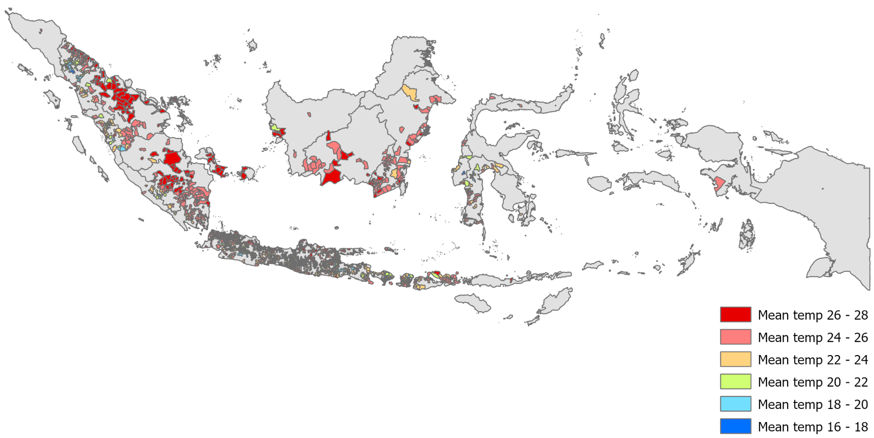
Detailed View



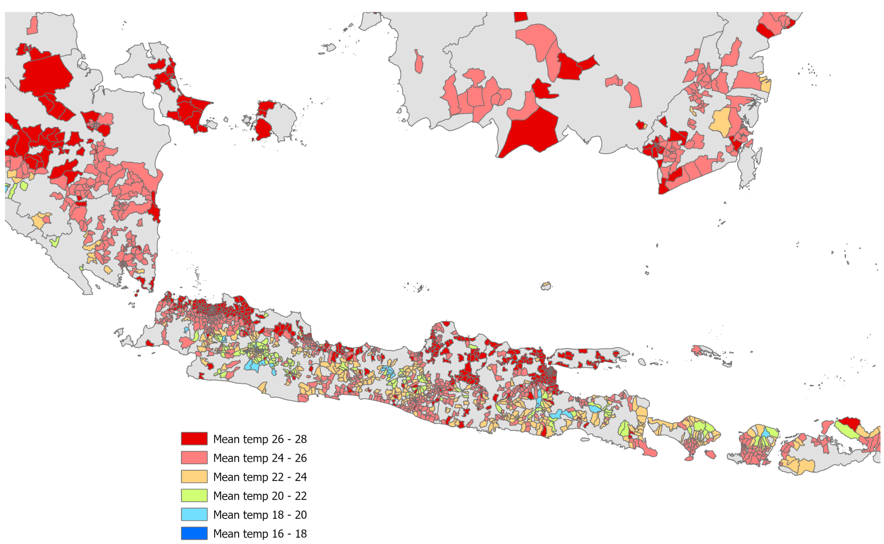
Notes: The colored areas indicate sub-districts (kecamatan) included in the IFLS. Long-run mean January temperatures are calculated using January temperature data from 1973 to 1999.

Figure C.I.3: **July** long-run mean temperatures across IFLS sub-districts

ALL IFLS sub-districts



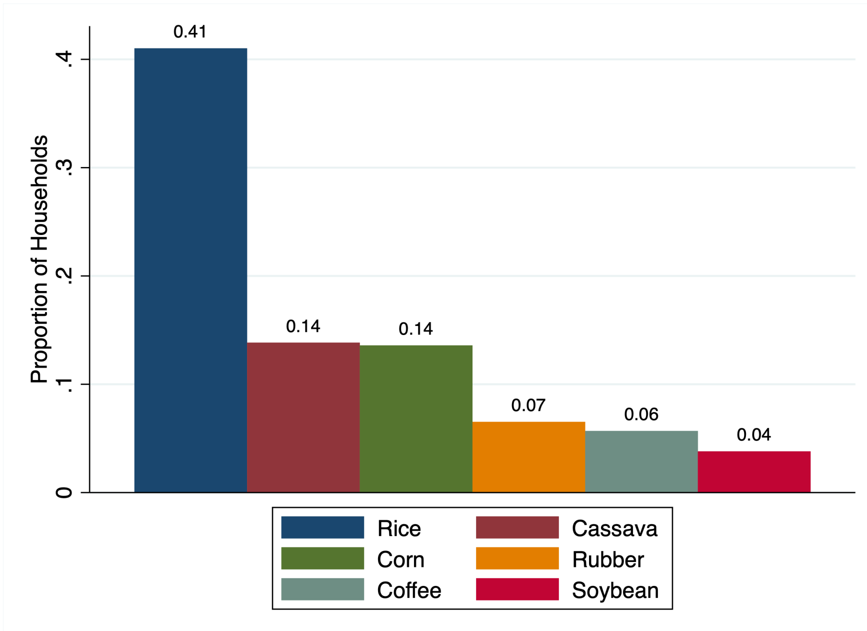
Detailed View



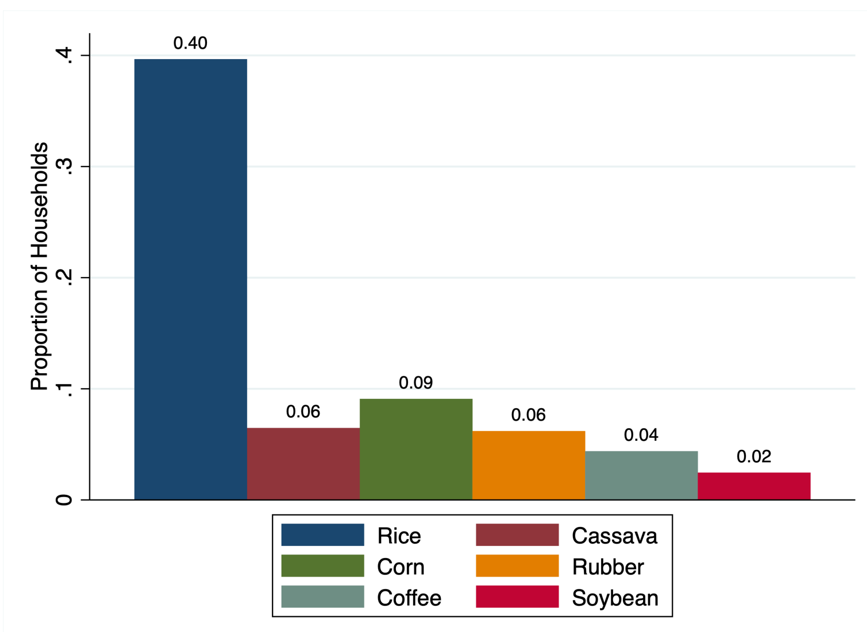
Notes: The colored areas indicate sub-districts (kecamatan) included in the IFLS. Long-run mean July temperatures are calculated using July temperature data from 1973 to 1999.

Figure C.I.4: Share of farm households producing each crop

Panel A: Share of farm households producing each crop



Panel B: Share of farm households producing each crop as primary crop



Notes: Bars show the average proportion of rural farm households growing each type of crop across the three IFLS waves used in our study.

C.II. Appendix Tables

Table C.II.1: Summary Statistics

Variable	Mean	SD	Obs
A. Household characteristic			
Age of household head	45.7	12.7	12647
Head's years of schooling	6.07	4.36	12647
Ethnicity			
Javanese ethnicity	.419	.493	12647
Sundanese ethnicity	.11	.313	12647
Betawi ethnicity	.019	.135	12647
Minangkabau ethnicity	.052	.221	12647
Balinese ethnicity	.057	.232	12647
Farm households	.865	.342	12647
Rice-farming household	.516	.5	12647
Crop Type			
Cassava cultivated	.119	.324	12647
Corn cultivated	.119	.323	12647
Soybean cultivated	.034	.181	12647
Rubber cultivated	.053	.223	12647
Coffee cultivated	.05	.217	12647
B. Individual characteristics			
Worked during past week	.789	.408	28600
Worked in agri sector during past week	.472	.499	28600
Worked in non-agri sector during past week	.318	.466	28600
Hours worked during past week	31.3	28.2	28600
Hours worked in agri sector during past week	14.2	21.6	28600
Hours worked in non-agri sector during past week	17	26.6	28600
C. Weather shocks			
Percent of days with high temperature in the last Monsoon season	.149	.141	12647
Percent of days with low temperature in the last Monsoon season	.099	.067	12647
High-rain shock during last monsoon season	.109	.312	12647
Low-rain shock during last monsoon season	.189	.392	12647

Panel A presents summary statistics for our sample households — households that resided in rural areas in IFLS 3 (i.e., year 2000) with a household head aged 15–65 as of 2000. Panel B presents summary statistics for our sample individuals — individuals aged 15-65 and lived in rural areas as of 2000. Panel C provides summary statistics on weather shocks experienced by the sample households.

Table C.II.2: Weather shocks and working status

	(1) All HH	(2) Farm HH	(3) Rice-farm HH	(4) Farm males	(5) Farm females	(6) Rice-farm males	(7) Rice-farm females
Panel A	Outcome var. = Worked in the past week						
High-temp shock	0.118**	0.091	0.177**	0.047	0.131	0.065	0.272**
	(0.057)	(0.058)	(0.069)	(0.037)	(0.100)	(0.048)	(0.115)
Low-temp shock	0.186*	0.170	0.276***	0.223***	0.128	0.346***	0.213
	(0.113)	(0.114)	(0.103)	(0.073)	(0.185)	(0.082)	(0.175)
High-rain shock	-0.023**	-0.027***	-0.025*	-0.020***	-0.034**	-0.014	-0.035
	(0.011)	(0.010)	(0.014)	(0.008)	(0.017)	(0.010)	(0.022)
Low-rain shock	-0.031**	-0.025*	-0.027	-0.011	-0.036	-0.003	-0.047
	(0.014)	(0.015)	(0.019)	(0.011)	(0.023)	(0.014)	(0.030)
Mean Y	0.782	0.796	0.812	0.928	0.676	0.935	0.699
Panel B	Outcome var. = Worked in the agri sector during the past week						
High-temp shock	0.087	0.097	0.241**	0.039	0.149	0.179*	0.297**
	(0.075)	(0.082)	(0.099)	(0.071)	(0.119)	(0.095)	(0.132)
Low-temp shock	0.112	0.135	0.395**	0.250	0.038	0.498***	0.314
	(0.142)	(0.163)	(0.191)	(0.161)	(0.206)	(0.193)	(0.230)
High-rain shock	-0.021	-0.025*	-0.029	-0.030**	-0.021	-0.041**	-0.021
	(0.013)	(0.014)	(0.018)	(0.014)	(0.020)	(0.018)	(0.026)
Low-rain shock	-0.015	-0.016	-0.016	0.012	-0.038	0.010	-0.038
	(0.014)	(0.017)	(0.022)	(0.017)	(0.023)	(0.022)	(0.031)
Mean Y	0.459	0.533	0.604	0.661	0.415	0.733	0.486
Panel C	Outcome var. = Worked in the non-agri sector during the past week						
High-temp shock	0.031	-0.006	-0.064	0.143	0.008	-0.019	-0.114
	(0.053)	(0.053)	(0.064)	(0.127)	(0.068)	(0.071)	(0.085)
Low-temp shock	0.074	0.035	-0.119	0.230	-0.027	0.089	-0.152
	(0.116)	(0.131)	(0.152)	(0.306)	(0.154)	(0.156)	(0.196)
High-rain shock	-0.002	-0.002	0.004	0.009	0.010	-0.013	0.026
	(0.013)	(0.014)	(0.014)	(0.028)	(0.016)	(0.017)	(0.019)
Low-rain shock	-0.016	-0.009	-0.010	-0.050*	-0.023	0.003	-0.013
	(0.011)	(0.012)	(0.013)	(0.030)	(0.016)	(0.016)	(0.019)
Mean Y	0.323	0.263	0.207	0.693	0.266	0.261	0.202
Observations	28600	24573	14748	3462	11438	13128	6877
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for individuals, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01, *** p<.01

Table C.II.3: Heterogeneous effects by irrigation status - Labor market outcomes

	(1) All HH	(2) Farm HH	(3) Rice-farm HH	(4) All HH	(5) Farm HH	(6) Rice-farm HH	(7) All HH	(8) Farm HH	(9) Rice-farm HH
	Outcome = Hours worked			Outcome = Agri. hours worked			Outcome = Nonagri. hours worked		
Panel A: Villages with technical irrigation									
High-temp shock	25.183***	27.304***	46.329***	12.105	13.453	25.469**	13.712**	14.530**	22.143***
	(8.064)	(8.525)	(9.173)	(8.365)	(9.058)	(9.912)	(5.958)	(6.301)	(7.983)
Low-temp shock	54.817***	57.739***	63.573***	18.619*	20.097*	25.024**	37.026***	38.585***	40.254***
	(13.199)	(14.297)	(12.909)	(10.170)	(11.227)	(10.255)	(13.600)	(14.109)	(12.203)
High-rain shock	-0.508	0.710	1.919	-1.025	-0.967	-2.003	0.394	1.545	3.776**
	(1.282)	(1.260)	(1.707)	(1.262)	(1.359)	(1.497)	(1.433)	(1.387)	(1.834)
Low-rain shock	2.403	1.159	-0.727	5.127**	5.770**	5.544**	-2.876	-4.777***	-6.593***
	(2.168)	(2.231)	(2.453)	(2.163)	(2.417)	(2.732)	(1.859)	(1.837)	(2.314)
Mean Y	30.555	30.498	30.854	11.512	13.212	15.209	19.056	17.301	15.668
Observations	6693	5814	3658	6693	5814	3658	6693	5814	3658
Panel B: Villages without technical irrigation									
High-temp shock	7.544	9.583	16.026*	12.622*	12.913*	19.823**	-5.083	-3.336	-3.806
	(7.663)	(8.074)	(9.737)	(7.178)	(7.432)	(8.090)	(3.186)	(3.398)	(4.531)
Low-temp shock	-13.971	-13.129	0.587	-5.795	-6.268	9.649	-8.120	-6.805	-8.994
	(9.853)	(10.173)	(12.936)	(9.934)	(10.320)	(11.869)	(6.974)	(7.005)	(8.451)
High-rain shock	-1.139	-1.488	-1.425	0.162	0.153	-0.518	-1.306	-1.646*	-0.916
	(1.096)	(1.179)	(1.381)	(1.097)	(1.177)	(1.438)	(0.878)	(0.865)	(0.886)
Low-rain shock	0.243	0.283	1.237	1.087	1.337	2.712*	-0.837	-1.046	-1.465
	(1.514)	(1.623)	(1.881)	(1.320)	(1.390)	(1.646)	(0.786)	(0.837)	(0.943)
Mean Y	30.554	30.426	30.727	16.019	17.462	19.688	14.540	12.969	11.045
Observations	13971	12788	8274	13971	12788	8274	13971	12788	8274
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The sample in Panel A includes villages that participated in the IFLS Wave 3 Community Survey and reported having technical irrigation systems, while Panel B includes those that reported having none. Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for individuals, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

Table C.II.4: Weather shocks and remittances

	(1) All HH	(2) Farm HH	(3) Rice-farm HH	(4) All HH	(5) Farm HH	(6) Rice-farm HH
Panel A	Outcome = Received remittances			Outcome = Received assistance from friends & neighbors		
High-temp shock	-0.044	0.012	0.150	-0.249	-0.237	-0.123
	(0.117)	(0.121)	(0.119)	(0.163)	(0.171)	(0.184)
Low-temp shock	0.523**	0.567**	0.615**	0.665**	0.797***	1.084***
	(0.257)	(0.258)	(0.295)	(0.275)	(0.279)	(0.340)
High-rain shock	-0.024	-0.017	-0.030	-0.017	-0.017	-0.030
	(0.023)	(0.025)	(0.024)	(0.030)	(0.033)	(0.031)
Low-rain shock	0.051	0.046	0.038	0.046	0.062	0.070*
	(0.038)	(0.042)	(0.039)	(0.041)	(0.044)	(0.042)
Mean Y	0.483	0.487	0.506	0.526	0.533	0.549
Observations	12636	10933	6526	12636	10933	6526
Panel B	Outcome = Log remittance amount			Outcome = Log amount received from friends & neighbors		
High-temp shock	0.293	1.063	2.648	-2.558	-2.559	-1.765
	(1.436)	(1.519)	(1.673)	(1.978)	(2.065)	(2.302)
Low-temp shock	6.209**	6.951**	7.241**	7.293**	8.639**	11.610***
	(3.088)	(3.022)	(3.532)	(3.358)	(3.398)	(4.355)
High-rain shock	-0.269	-0.213	-0.189	-0.106	-0.100	-0.077
	(0.287)	(0.314)	(0.325)	(0.403)	(0.434)	(0.413)
Low-rain shock	0.602	0.551	0.446	0.646	0.941*	1.135**
	(0.456)	(0.506)	(0.493)	(0.534)	(0.553)	(0.520)
Mean Y	5.844	5.842	6.042	5.953	6.004	6.156
Observations	12636	10933	6526	12636	10933	6526
Household FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for households, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

Table C.II.5: Weather shocks and livestock – poultry v.s. other livestock

	(1) All HH	(2) Farm HH	(3) Rice-farm HH	(4) All HH	(5) Farm HH	(6) Rice-farm HH
Panel A	Outcome = Own poultry (d)			Outcome = Own other livestock (d)		
High-temp shock	0.212	0.190	-0.077	0.208***	0.224***	0.342***
	(0.165)	(0.169)	(0.205)	(0.078)	(0.084)	(0.103)
Low-temp shock	-0.425	-0.401	-0.987**	0.025	-0.019	-0.057
	(0.286)	(0.324)	(0.459)	(0.144)	(0.159)	(0.180)
High-rain shock	-0.023	-0.027	-0.096**	-0.003	-0.010	-0.005
	(0.030)	(0.031)	(0.038)	(0.023)	(0.026)	(0.026)
Low-rain shock	-0.109***	-0.091***	-0.094**	-0.058***	-0.066***	-0.082***
	(0.030)	(0.031)	(0.038)	(0.018)	(0.021)	(0.027)
Mean Y	0.288	0.299	0.308	0.079	0.087	0.093
Panel B	Outcome = Log value of poultry			Outcome = Log value of other livestock		
High-temp shock	2.734	2.530	-0.436	2.985***	3.163***	4.974***
	(1.997)	(2.046)	(2.479)	(1.118)	(1.208)	(1.502)
Low-temp shock	-5.231	-5.018	-11.160**	0.220	-0.558	-0.890
	(3.415)	(3.850)	(5.436)	(2.091)	(2.306)	(2.680)
High-rain shock	-0.214	-0.261	-1.099**	0.032	-0.069	0.039
	(0.364)	(0.380)	(0.471)	(0.317)	(0.356)	(0.371)
Low-rain shock	-1.413***	-1.204***	-1.241***	-0.775***	-0.865***	-1.063***
	(0.367)	(0.381)	(0.467)	(0.257)	(0.295)	(0.394)
Mean Y	0.288	0.299	0.308	0.079	0.087	0.093
Observations	8840	7614	4466	8840	7614	4466
Household FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for households, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

Table C.II.6: Alternative specifications - Effects on household expenditure and jewelry

	(1) Original specification	(2) With dry season temp	(3) Temp: $\pm 0.7°C$	(4) Temp:$+1.2°C$ & $-1°C$	(5) Rain: $\pm 0.9SD$	(6) Rain: $\pm 1.1SD$
			Panel A: Log real household expenditure			
High-temp	-0.331*	-0.331*	-0.342**	-0.268	-0.289	-0.338*
	(0.184)	(0.183)	(0.166)	(0.181)	(0.191)	(0.183)
Low-temp	-0.595*	-0.545*	-0.628**	-1.004**	-0.602**	-0.580*
	(0.307)	(0.304)	(0.308)	(0.463)	(0.306)	(0.307)
High-rain	0.083***	0.081**	0.078**	0.083***	0.078**	0.101***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.031)	(0.034)
Low-rain	-0.023	-0.023	-0.019	-0.030	-0.043	-0.015
	(0.036)	(0.036)	(0.037)	(0.035)	(0.036)	(0.034)
Mean Y	14.297	14.297	14.297	14.297	14.297	14.297
			Panel B: Own jewelry (dummy variable)			
High-temp	-0.068	-0.068	-0.056	-0.099	-0.018	-0.087
	(0.106)	(0.104)	(0.101)	(0.123)	(0.098)	(0.102)
Low-temp	-0.470**	-0.472**	-0.460**	-0.935***	-0.457**	-0.460**
	(0.197)	(0.194)	(0.202)	(0.292)	(0.196)	(0.199)
High-rain	0.037	0.041	0.036	0.035	0.011	0.043*
	(0.025)	(0.025)	(0.025)	(0.025)	(0.023)	(0.026)
Low-rain	-0.051*	-0.052*	-0.052	-0.043	-0.087***	-0.039
	(0.030)	(0.030)	(0.032)	(0.029)	(0.029)	(0.025)
Mean Y	0.504	0.504	0.504	0.504	0.504	0.504
			Panel C: Log value of jewelry			
High-temp	-1.349	-1.348	-1.179	-1.778	-0.697	-1.547
	(1.441)	(1.424)	(1.359)	(1.682)	(1.344)	(1.370)
Low-temp	-6.238**	-6.273**	-6.118**	-12.960***	-6.080**	-6.101**
	(2.675)	(2.640)	(2.734)	(3.913)	(2.656)	(2.692)
High-rain	0.523	0.562	0.506	0.504	0.187	0.618
	(0.362)	(0.369)	(0.362)	(0.364)	(0.331)	(0.380)
Low-rain	-0.642	-0.648	-0.652	-0.555	-1.110***	-0.517
	(0.399)	(0.397)	(0.417)	(0.380)	(0.391)	(0.325)
Mean Y	6.928	6.928	6.928	6.928	6.928	6.928
Observations	12647	12647	12647	12647	12647	12647
HH FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample includes all rural households. Each column is a different specification: (1) original specification; (2) adds controls for dry-season temperature shocks; (3) replaces temperature shocks with thresholds of $\pm 0.7°C$; (4) uses thresholds of $+1.2°C$ and $-1.0°C$; (5) defines rainfall shocks using a ± 0.9 SD threshold; and (6) defines rainfall shocks using a ± 1.1 SD threshold. Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for households, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

Table C.II.7: Alternative specifications - Effects on hours of work in the past week

	(1) Original specification	(2) With dry season temp	(3) Temp: $\pm 0.7°C$	(4) Temp: $+1.2°C$ & $-1°C$	(5) Rain: $\pm 0.9SD$	(6) Rain: $\pm 1.1SD$
	Panel A: Hours of work in the past week					
High-temp	11.239**	11.227**	8.186**	11.918**	10.132**	11.410***
	(4.518)	(4.502)	(4.028)	(4.815)	(4.558)	(4.423)
Low-temp	6.923	6.630	4.011	8.796	7.100	6.659
	(7.787)	(8.063)	(7.237)	(11.150)	(7.663)	(7.765)
High-rain	-0.713	-0.706	-0.633	-0.739	-0.989	-0.090
	(0.794)	(0.862)	(0.813)	(0.778)	(0.655)	(0.777)
Low-rain	0.578	0.578	0.854	0.588	1.044	0.677
	(1.001)	(1.004)	(1.004)	(0.964)	(0.921)	(0.882)
Mean Y	30.972	30.972	30.972	30.972	30.972	30.972
	Panel B: Hours of work in the agricultural sector					
High-temp	10.524**	10.491**	9.219**	10.678**	10.538**	11.075**
	(4.515)	(4.403)	(4.094)	(4.831)	(4.490)	(4.515)
Low-temp	1.284	0.634	0.343	-2.626	1.441	0.991
	(6.821)	(6.729)	(6.900)	(11.225)	(6.652)	(6.751)
High-rain	-0.317	0.012	-0.202	-0.358	-0.926	0.026
	(0.718)	(0.785)	(0.737)	(0.715)	(0.675)	(0.568)
Low-rain	1.716*	1.683*	1.785*	1.834**	1.490*	1.518*
	(0.953)	(0.979)	(0.948)	(0.894)	(0.820)	(0.799)
Mean Y	13.800	13.800	13.800	13.800	13.800	13.800
	Panel C: Hours of work in the non-agricultural sector					
High-temp	0.831	0.850	-0.941	1.322	-0.301	0.447
	(2.682)	(2.610)	(2.344)	(3.088)	(2.594)	(2.590)
Low-temp	5.842	6.171	3.851	11.532	5.867	5.870
	(6.111)	(6.098)	(6.193)	(7.372)	(6.116)	(6.211)
High-rain	-0.412	-0.731	-0.447	-0.397	-0.079	-0.134
	(0.722)	(0.702)	(0.723)	(0.726)	(0.630)	(0.678)
Low-rain	-1.159*	-1.126*	-0.950	-1.267*	-0.463	-0.860
	(0.690)	(0.684)	(0.696)	(0.671)	(0.666)	(0.625)
Mean Y	17.177	17.177	17.177	17.177	17.177	17.177
Observations	28600	28600	28600	28600	28600	28600
Indi FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Sample includes all rural households. Each column is a different specification: (1) original specification; (2) adds controls for dry-season temperature shocks; (3) replaces temperature shocks with thresholds of $\pm 0.7°C$; (4) uses thresholds of $+1.2°C$ and $-1.0°C$; (5) defines rainfall shocks using a ±0.9 SD threshold; and (6) defines rainfall shocks using a ±1.1 SD threshold. Standard errors are adjusted for spatial dependence as outlined in Conley (1999), with values reported in parentheses. The regressions control for the rainfall z-score during the dry season and include fixed effects for households, years, and quarters of the year. Significance is indicated at * p<.1, ** p<.05, *** p<.01

VITA

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