Causes and Consequences of Intimate Partner Violence

Atiya Rahman
M.Sc. B.SS.

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School of Global Development
University of East Anglia

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Abstract

My thesis investigates the unexplored causes and consequences of intimate partner violence (IPV), contributing to understanding and addressing gender disparities. My first chapter examines the influence of weather shocks on IPV in the Global South. Combining data from 54 Demographic Health Surveys and weather data from 53,506 local clusters in India, Sub-Saharan Africa (SSA), and Latin America (LA), I find that in most regions, physical IPV increases with positive temperature shocks. The effects of rainfall shocks vary across regions, increasing with negative shocks in rural SSA and with positive shocks in urban LA. Some regions also show increases in emotional and sexual IPV.

My second chapter assesses the persistence of the impacts of a credit-based graduation programme on IPV, using a randomised controlled trial with two endlines at one and seven years after the programme ended in rural Bangladesh. Women in the treatment group are less likely to experience IPV compared to women in the control group in the short run, while these effects reverse in the long run. Compared to the control group, the treatment group experiences better economic conditions in the long run, yet experiences a lower reduction in IPV between endlines, perhaps due to stagnation in the economic conditions of treatment women between endlines.

The third chapter examines the impact of IPV priming on women's risk and time preferences in the same country. By randomising the order of IPV priming and preference modules in a survey, I find that IPV priming reduces risk aversion regardless of real-life IPV experiences. IPV priming further reduces (increases) impatience among women with (without) emotional IPV experiences. A theoretical model and further analysis reveal that negative emotions triggered by the priming mediate these effects. The findings of my thesis provide crucial insights for designing policies to reduce IPV and improve women's well-being

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Introduction

> *"Violence against women is perhaps the most shameful human rights violation, and it is perhaps the most pervasive. It knows no boundaries of geography, culture or wealth. As long as it continues, we cannot claim to be making real progress towards equality, development and peace."*
> — Kofi Annan, Former Secretary-General of the United Nations

While human beings sort out their coexistence with Artificial Intelligence, violence against women still looms large as a specter from a bygone century, challenging all the human achievements. One of the persistent manifestations of this farce is intimate partner violence (IPV). Despite decades of advocacy and major global agreements, such as the Convention on the Elimination of All Forms of Discrimination Against Women (CEDAW) and Sustainable Development Goal 5, IPV remains prevalent everywhere. Worldwide, more than a quarter (27%) of women aged 15-49 years who have been in a relationship experience abuse from their husbands at least once in their lifetime (WHO, 2021), with the rate being even higher in the Global South.[1] This prevalence widely varies across regions, from 16% in Southern Europe to 51% in Melanesia. Other high prevalence regions are as follows: Micronesia (41%), Polynesia (39%), South Asia (35%) and Sub-Saharan Africa (33%). Country-specific statistics show that some countries have rates of IPV above 40%, and Bangladesh is one of them (WHO, 2021). According to the latest survey on violence against women, 54% of ever-married women experienced physical and/or sexual abuse by their husbands at least once in their lifetime in Bangladesh (BBS, 2025). Improving our understanding of the driving factors and consequences of IPV is crucial for informing gender-sensitive policies that aim to improve women's well-being.

An extensive literature has documented a wide range of causes and consequences of IPV. Several factors have been identified that contribute to IPV across different contexts. IPV tends to increase with early marriage, women's education, witnessing parental violence, men's controlling behaviour, men's alcohol consumption and women's poor social networks or connections with the community (see, e.g., Gubi, Nansubuga and Wandera, 2020; Izugbara et al., 2020; Voith, Azen and Qin, 2021; Raghavan et al., 2006). Moreover, a growing body of literature examines the association between natural disasters and IPV across different countries (Boddy et al., 2024). For instance, many studies have demonstrated a positive relationship between climate shocks and IPV in Sub-Saharan Africa (SSA) (Epstein et al., 2020; Allen, Munala and Henderson, 2021; Munala et al., 2023; Ross et al., 2023) and in India (Izugbara et al., 2018; Sekhri and Storeygard, 2014; Rai, Sharma and Subramanyam, 2021; Dehingia et al., 2024). Studies on the influence

[1] IPV experience is defined as whether a woman has ever experienced physical and/or sexual violence in her lifetime.

of women's economic development on IPV are also emerging in the literature (see, e.g., Koenig et al., 2003; Panda and Agarwal, 2005; Aizer, 2010; Haushofer et al., 2019; Vyas and Watts, 2009); however, the direction of this influence is complex. Some studies find that women's economic improvement reduces the incidence of IPV (Koenig et al., 2003; Panda and Agarwal, 2005; Aizer, 2010), while others document that this triggers IPV (Vyas and Watts, 2009; Heath, 2014).

Furthermore, the consequences of IPV on women's lives have been widely studied. The physical health consequences of IPV are commonly discussed across different countries, for example, increased likelihood of suffering from chronic diseases, continued pain, increased risk of diabetes, infectious diseases, gastrointestinal disorders, and adverse pregnancy outcomes such as low birth weight, preterm birth, and intrauterine growth restriction (see, e.g., Stubbs and Szoeke, 2022; Campbell, 2002; Dillon et al., 2013; Hill et al., 2016; Maxwell et al., 2015; Sarkar, 2008). Moreover, the mental health consequences have been widely studied (see, e.g., Dokkedahl et al., 2022); for example, anxiety, depression, poor memory, and low concentration have been documented to be detrimental outcomes of IPV (see, e.g., Navarro-Mantas, de Lemus and Megías, 2021; Ishida et al., 2010; Ludermir et al., 2008). The impacts of IPV on the labour market are also severe for women, as measured by women's labour force participation, working hours, and the risk of quitting a job (see, e.g., Duvvury et al., 2013; Tolman and Wang, 2005; Brown et al., 2024; Adams et al., 2012; Rios-Avila and Javier Canavire-Bacarreza, 2017).

Still, significant evidence gaps remain on the causes and consequences of IPV. This thesis investigates some important causes and consequences of IPV that have been underexplored in the literature. It comprises three independent chapters, each with distinct core research questions, methodology, empirical findings, and discussions. In these chapters, I examine the relationship between IPV and three core dimensions: climate change, women's economic development, and women's economic preferences. My first chapter estimates the impact of weather shocks on IPV across different geographical regions, including India, Sub-Saharan Africa and Latin America, using weather data and IPV data on nationally representative samples. My second chapter utilises a randomised controlled trial (RCT) to evaluate the short- and long-term impact of a credit-based graduation programme on the incidence of IPV in Bangladesh and to test potential mediators to understand the contribution of women's economic development to reducing the incidence of IPV. For my third chapter, I conducted a survey experiment in the same country. In the experiment, women were primed to think about IPV, which allowed me to test its impact on women's risk and time preferences. My thesis ends with a short conclusion with key findings. More details on each chapter are provided below.

Chapter one: Weather shocks and IPV

In my first chapter, I examine the influence of weather shocks on the likelihood of IPV in the Global South, including India, Sub-Saharan Africa (SSA) and Latin America (LA). My study makes an important contribution to the growing literature that documents the influence of temperature or rainfall on IPV (Izugbara et al., 2018; Sekhri and Storeygard, 2014; Rai, Sharma and Subramanyam, 2021; Dehingia et al., 2024; Epstein et al., 2020; Allen, Munala and Henderson, 2021; Cools, Flatø and Kotsadam, 2020; Munala et al., 2023; Ross et al., 2023; Nguyen, 2024). Although climate change disproportionately affects the Global South (Sardinha et al., 2022), and this region has a higher prevalence of IPV, a broader picture of its effects on IPV is yet to be explored. The evidence from this chapter provides critical insights for policymakers to develop early warning systems and identify areas that require targeted interventions to mitigate the impacts of weather shocks on IPV.

To investigate spatial variation in the impact of weather shocks on IPV, I have built a novel dataset by merging historical weather data from the Climatic Research Unit (CRU) at the University of East Anglia (UEA) and IPV data from the Demographic and Health Surveys (DHSs). Specifically, I have utilised data from 54 DHSs and weather data for 53,506 local geographical clusters across India, SSA, and LA. I have also developed an innovative approach that uses a percentile-based threshold for the local distribution of monthly rainfall and temperature to define weather shocks. I further disaggregate these shocks by direction, distinguishing between positive and negative rainfall (temperature) weather shocks. Furthermore, I distinguish between rural and urban areas within each region, as these areas often vary in terms of economic opportunities and social norms around IPV. To estimate the impacts of different weather shocks (i.e., positive and negative shocks in rainfall/temperature) in rural (urban) India (SSA, LA), I use ordinary least squares (OLS) regressions and control for wives' and husbands' characteristics. I also use survey fixed effects to account for unobservable characteristics (variations) across countries and time.

My results show that the likelihood of physical IPV increases with the frequency of positive temperature in most regions, with the strongest impacts in India and LA. Interestingly, negative temperature shocks also increase physical IPV in India and LA. On the contrary, rainfall shocks influence physical IPV in different ways across different regions. Negative rainfall shocks increase physical IPV while positive rainfall shocks decrease IPV in urban India but increase IPV in urban LA. Furthermore, I observe that rainfall and temperature shocks influence emotional and sexual IPV in a way that is qualitatively similar to their impacts on physical IPV.

I further analyse the influence of weather shocks on possible intermediate outcomes, including spousal employment and women's participation in intra-household decision-making, to provide evidence on the mechanisms through which weather shocks might influence IPV. I observe that weather shocks influence spouses' employment in opposite directions in India and SSA. In rural India, the likelihood of the wife working for someone else increases with weather shocks, while the likelihood of the husbands working the entire year decreases. In rural SSA, drought increases the wife's employment under someone else while the husband's employment remains unchanged. Regarding the wife's participation in household decision-making, I observe that it increases with rainfall shocks in all regions, whereas the impact of temperature shocks on this outcome is less consistent. The influence of weather shocks on these mediating factors plausibly explains the positive impacts of weather shocks on the likelihood of IPV in these cases.

Chapter two: Graduation programme and IPV

My second chapter examines whether the reduction in IPV resulting from a graduation programme persists in the long run. Evidence shows that women's economic empowerment can influence IPV, with some studies showing a negative relationship and some reporting a positive one (Koenig et al., 2003; Panda and Agarwal, 2005; Aizer, 2010; Vyas and Watts, 2009; Heath, 2014). Assuming that improved women's economic conditions can reduce IPV, several development programmes have provided women with economic resources to reduce IPV. Some studies find that these programmes reduce IPV within one or two years after the programme ended, whereas other studies show an increase or no change in IPV with the same period of evaluation (see, e.g., Buller et al., 2018; Dalal, Dahlström and Timpka, 2013; Williams et al., 2025; Das et al., 2025). Yet, the persistence of these impacts in the long run is understudied (Angelucci and Heath, 2020). To investigate this, I evaluate the short- and long-run impacts of a credit-based graduation programme implemented by BRAC on the incidence of IPV in rural Bangladesh.

The credit-based programme provided women from ultra-poor households with a loan conditional on purchasing productive assets (mostly livestock), bi-weekly enterprise training, a weekly consumption stipend, guidance on health and social issues, savings advice, and forming a committee to mobilise resources for ultra-poor households through the community. Using a randomised controlled trial (RCT), I assess the impact of this programme on IPV with two rounds of endline survey at one and seven years after the completion of the programme. The baseline survey of this evaluation study covered 8,973 eligible women (7,042 treatment and 1,931 control) from 88 branch offices across rural areas in 11 districts of Bangladesh.[2] The first endline survey ('endline 1') revisited a random subsample of the treatment group and all women from the control group. Of these, 2,741 women (1,945 treatment and 796 control) were included in endline 1's analysis. The second endline survey ('endline 2') revisited all women from the baseline survey. Of these, 7,079 women (5,549 treatment and 1,530 control) were included in the endline 2 analysis.

My findings reveal that the treatment-control differences in IPV differ between the short and the long run. One year after the programme completion, women in the treatment group are less likely to experience physical and emotional IPV compared to women in the control group, while these differences reverse seven years after the programme completion. Women in the treatment group are more likely to experience physical and emotional IPV compared to women in the control group in the long run. The trend of IPV reveals that women in the control group experienced a strong reduction in IPV between endlines 1

[2]Each branch covers an area within approximately a 5-kilometre radius.

and 2. Women in the treatment group also experienced a reduction in IPV; however, this reduction is lower compared to that in the control group. Women's ageing might partially explain this reduction in IPV, as women in my sample became six years older between endlines 1 and 2. Furthermore, the nationwide decline in IPV is also consistent with this reduction. However, there might be other factors working behind this reduction that my study cannot identify.

Further analysis reveals that differences in the trajectory of economic outcomes (income and livestock) between treatment and control groups might explain the reversed treatment-control differences in IPV. In the short run, women in the treatment group earn more income and own more livestock compared to the control group. Husbands of women in the treatment group also earn more compared to their counterparts in the control group. In the long run, the treatment-control differences decline; however, women in the treatment group still have better economic condition compared to women in the control group. Husbands of women in the treatment group also have more income compared to their counterparts in the control group. The trend analysis reveals that the economic outcomes of women in the treatment group stagnated between endlines, while women in the control group experienced a substantial increase in their income and a moderate increase in their livestock ownership.

The changes in credit market participation of women might explain the differences in the treatment effect on economic outcomes for women over time. I observe that women in the treatment group are more likely to access loans from BRAC in both the short and the long run compared to the control group. However, the trend of credit market participation differs between treatment and control groups. Women in the treatment group reduced their access to credit from BRAC and increased their access to credit from non-BRAC NGOs, while women in the control group increased their access to credit from BRAC, and there was no change in their access to credit from non-BRAC NGOs between endlines. This suggests that women in the treatment group might have gained the skills and resources to maintain the same level of income at both endlines, while women in the control group were gradually catching up with the economic conditions of the treatment group.

My findings have two major contributions to research and policy on women's empowerment. For research, the reversed treatment effects on the incidence of IPV in the long run suggest that the timing of evaluation is important. For policy, the stagnation in economic conditions observed in the treatment group, while the control group gradually improves these outcomes, suggests that policies promoting a continuous improvement in women's economic conditions might be effective in achieving sustainable reductions in the incidence of IPV.

Chapter three: IPV and women's economic preferences

My third chapter investigates an understudied behavioural mechanism through which IPV might influence women's economic decisions. Evidence shows that women who experience IPV are more likely to have poor economic conditions (see, e.g., Duvvury et al., 2013; Brown et al., 2024; Adams et al., 2012; Tolman and Wang, 2005; Tankard, Paluck and Prentice, 2019; Shahriar and Alam, 2024). Another strand of literature demonstrates that risk preferences influence labour market participation, investment, health choices and migration (Hong, Kubik and Stein, 2004; Bonin et al., 2007; Kimball, Sahm and Shapiro, 2008; Jaeger et al., 2010; Dohmen and Falk, 2011; Becker et al., 2012; Dawson and Henley, 2015; Hsieh, Parker and van Praag, 2017; Sunde and Becker, 2018)) and time preferences influence investment in human capital (Golsteyn, Grönqvist and Lindahl, 2014; Cadena and Keys, 2015; van Huizen and Alessie, 2015; Falk et al., 2018) and savings (Horn and Kiss, 2020; Klawitter, Anderson and Gugerty, 2013; Hershey and Mowen, 2000; Jacobs-Lawson and Hershey, 2005). Drawing on both strands of literature, I explore whether the negative impacts of IPV on economic outcomes work through changing women's risk and time preferences.

To examine this mechanism, I conducted an experiment with 901 married women from low-income households by randomising the order of IPV priming and preference modules in a survey. The IPV priming module began with a detailed set of questions on IPV experiences adapted from a national survey, followed by a short video demonstrating a woman experiencing physical abuse by her husband in front of her children, and preference modules elicited women's risk and time preferences. Women in the treatment group are those who were exposed to the IPV priming module *before* the preferences module, whereas women in the control group were exposed to the IPV priming module *after* the preferences module. At the end of the priming module, I collected data on women's emotions after watching the video, particularly, to what extent this video made them angry, fearful or anxious, to test whether IPV priming changes preferences through triggering emotions.

Using a dual-self model, I develop my hypotheses. My framework distinguishes two sets of emotions (anger and fear/anxiety) that would be triggered by IPV priming. These sets of emotions are expected to have opposing effects on preferences. Anger increases individuals' risk-taking and willingness to wait for delayed rewards, whereas fear and anxiety decrease both risk tolerance and patience. As the priming could trigger any of these emotions more strongly, my first (hypothesis 1) and second (hypothesis 2) hypotheses are that IPV priming would change risk and time preferences, with the direction depending on the stronger emotion triggered by the priming. A further hypothesis (hypothesis 3) is that women with prior IPV experience would feel stronger emotions and thus experience larger changes in preferences.

My results reveal that IPV priming reduces risk aversion among women, supporting my hypothesis 1. Moreover, the reduction in risk aversion resulting from the priming does not differ between women with and without IPV experiences. In line with these findings, women with and without IPV history reported a similar level of anger, which might help explain the similar reduction in risk aversion among both groups. This finding does not support my hypothesis 3 for risk preferences, which states that the priming would have a stronger impact on preferences among women with IPV experiences.

For time preferences, the average effect of IPV priming on impatience is not statistically significant, providing no support for my hypothesis 2. However, there is heterogeneity in the impacts of IPV priming across real-life IPV experiences, supporting my hypothesis 3. The priming reduces impatience among women with experience of emotional IPV, while it increases impatience among women who never experienced emotional IPV. Moreover, the underlying emotion-based mechanism for time preferences is complex. Despite women with and without IPV experience reporting similar levels of anger, the impact of IPV priming on impatience differs across these two groups. On the other hand, women with a history of IPV reported stronger feelings of fear and anxiety compared to women without IPV experiences. These differences might explain the larger reduction in impatience among women with IPV experiences. However, this pattern does not align with the literature, which states that fear and anxiety increase impatience (Callen et al., 2014; Haushofer and Fehr, 2014; Takahashi, 2004; Lerner et al., 2015).

Furthermore, I find that the reduction in risk aversion due to IPV priming is more pronounced among younger women. This is consistent with the higher level of anger triggered by the priming, among the younger group, compared to the older group. In contrast, the disaggregated impacts of IPV priming on risk preferences by education and baseline preferences (i.e., preferences prior to the experiment) are marginally significant, and the emotional responses reported across these groups do not clearly explain these effects. Moreover, I do not find any significant disaggregated impact on time preferences by age and education groups; however, the effect on impatience among women with higher baseline preferences is marginally significant.

As I find that IPV priming influences women's risk and time preferences, IPV could have two implications for women's economic decision-making. First, while reduced risk aversion might lead to increased investment in high-return ventures, it might also result in suboptimal financial decisions, such as excessive borrowing and impulsive investments. Second, while reduced impatience might improve long-term financial planning and human capital investment, it could delay essential expenditures on health and nutrition or cause women to accept adverse situations while waiting for future improvements that may never come.

Since the observed impacts of IPV priming on preferences are driven by immediate emotional responses, these effects reflect short-run changes in preferences. Future research could investigate whether these changes are sustained and translate into real economic behaviours. My findings also have two policy implications. First, the interventions reducing IPV might also reduce emotion-based decision-making. Second, interventions that address emotional responses, such as counselling and stress management programs, might mitigate changes in preferences resulting from IPV, thus helping women make better economic decisions in high-IPV settings.

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CHAPTER 1

Weather Shocks and Intimate Partner Violence

Abstract[1]

Climate change increases the frequency of weather shocks in the form of extreme temperature or rainfall. There is growing evidence that temperature and rainfall can influence the occurrence of intimate partner violence (IPV), but no complete picture exists yet about the Global South, which is more likely to be affected by climate change and sees a higher IPV prevalence compared to other parts of the world. Combining data from 54 Demographic Health Surveys, which use standardised measures of IPV, and historical weather data of 53,506 local clusters in India, Sub-Saharan Africa (SSA), and Latin America (LA), we estimate the impact of positive and negative rainfall and temperature shocks on the occurrence of IPV. We find that in most regions the likelihood of physical IPV increases with the frequency of positive temperature shocks. The effects of rainfall shocks vary across regions: while physical IPV increases with negative shocks in rural SSA, it increases with positive shocks in urban LA. In some regions, the effects also come with an increase in emotional and sexual IPV.

1.1 Introduction

Climate change increases the frequency of weather extremes (Legg, 2021; Alexander, 2016; Mohajan, 2015). While there is growing evidence that temperature and rainfall can influence the occurrence of intimate partner violence (IPV), there is a lack of systematic evidence on the impact of weather shocks in the Global South. This is remarkable as this part of the world is more likely to be affected by climate change (Sardinha et al., 2022) and experiences high IPV prevalence (WHO, 2021). At the same time, the Global South is very diverse in terms of climatic, geographical and socio-economic characteristics. We expect this diversity to lead to important regional variation in the impacts of weather shocks on IPV. Understanding regional variation in the impact of weather shocks on IPV and potential underlying mechanisms can provide essential insights for policymakers. It enables them to develop early-warning systems and to identify areas that require targeted interventions to mitigate the impacts of weather shocks on IPV.

[1]Acknowledgements: We thank Benjamin K. Mayala of the Demographic and Health Surveys (DHS) Program, and Ian Harris and Tim Osborn from the Climate Research Unit (CRU) of UEA for advice on the geospatial matching of data from DHS and CRU.

To fill this knowledge gap, we estimate the impacts of the frequency of weather shocks on the likelihood of IPV in the Global South. For this, we use data from Demographic Health Surveys (DHS), which uses a standardised and individual measure of IPV. We use all surveys that have GPS locations of the local sampling clusters used by the DHS.[2] We link these surveys with historical weather data from the Climatic Research Unit (CRU) of the University of East Anglia (UEA), using the geo-codes of the DHS 'enumeration areas', henceforth referred to as 'clusters'. To define weather shocks, we use the 10-year distribution of weather data at each DHS cluster. For each month, we determine whether it can be defined as a weather shock, using the lowest and highest 10 percentile of a month's long-term distribution as cut-off points to define a negative and positive shock, respectively. We count the number of shocks in the preceding year to create a shocks frequency variable. We do this separately for temperature and rainfall data, giving us a total of four weather shock measures.

In the analysis, we compare three geographical regions: Sub-Saharan Africa (SSA), Latin America (LA), and India. Within each region, we further distinguish rural and urban areas, as they tend to differ on dimensions that might influence the impacts of weather shocks. For example, rural areas are more vulnerable to weather shocks (because of its dependence on agriculture), and have a lower capacity to cope with the economic impact of weather shocks (due to higher poverty levels). Attitudes towards women in rural areas might also be more conservative.

Our results show that physical IPV increases with positive temperature shocks in most regions. The effects are strongest in India and LA. We also find that IPV increases with negative temperature shocks in rural India and rural LA. The effect of rainfall shocks varies across regions. IPV increases with negative rainfall shocks in rural SSA, and positive rainfall shocks decrease IPV in urban India but increase IPV in urban LA. Looking at other forms of IPV, we observe that rainfall and temperature shocks also influence emotional and sexual IPV, and these effects vary considerably across regions. Further analysis provides support for mediating factors through which weather shocks influence physical IPV, including spousal employment and household decision-making.

Our study makes an important contribution to the growing literature that documents the influence of temperature or rainfall on IPV across diverse contexts (see, e.g., Izugbara et al., 2018; Sekhri and Storeygard, 2014; Rai, Sharma and Subramanyam, 2021; Dehingia et al., 2024; Epstein et al., 2020; Allen, Munala and Henderson, 2021; Cools, Flatø and Kotsadam, 2020; Munala et al., 2023; Ross et al., 2023; Nguyen, 2024). A systematic review by Boddy et al. (2024) concludes that IPV and natural disasters are correlated but identifies an evidence gap in understanding the nuances of this relationship. Of the 27 quantitative articles reviewed, only nine use population-based survey data, specifically the Demographic and Health Survey (DHS), National Family Health Survey, and Japan En-

[2] In the DHS surveys, a cluster is the primary sampling unit (PSU), which is usually a census enumeration area. The number of households per cluster is set by each survey design and varies across surveys.

vironmental and Children's Study—highlighting limited causal evidence and uncertainty about whether climate shocks increase the prevalence or frequency of IPV.

A substantial body of work examines Sub-Saharan Africa (SSA), generally finding a positive association between climate shocks and IPV (Epstein et al., 2020; Allen, Munala and Henderson, 2021; Cools, Flatø and Kotsadam, 2020; Munala et al., 2023; Ross et al., 2023). A notable exception is Cools, Flatø and Kotsadam (2020), a multi-country analysis of 17 SSA countries that finds no association between drought and IPV. By contrast, Munala et al. (2023) report that extreme weather events (floods and droughts) increase women's risk of IPV in Uganda, Zimbabwe, and Mozambique. Given these mixed results, Cooper et al. (2021) re-examine the drought–IPV relationship across SSA, Latin America, and South Asia, finding no association with physical IPV but evidence of increased husbands' controlling behaviour.

A number of studies investigate the impacts of climate change on IPV in India. For example, Izugbara et al. (2018) find that higher temperatures increase IPV in India, Pakistan, and Nepal, with the largest effect in India. Moreover, a one–standard-deviation rainfall shortfall relative to the long-term mean increases reports of domestic violence by 4.4% (Sekhri and Storeygard, 2014). Comparing slow-onset (drought) and rapid-onset (cyclone) disasters, Rai, Sharma and Subramanyam (2021) find that drought increases only physical IPV, whereas cyclones increase only emotional IPV. Extending this work, Dehingia et al. (2024) find that drought increases both physical and emotional IPV, while Rai, Sharma and Subramanyam (2021) detect significant effects on physical IPV only.

A limited number of studies estimate the impacts of weather shocks in Western and other contexts. In Spain, Sanz-Barbero et al. (2018) find that IPV risk rises three days after a heat wave. In Peru, Díaz and Saldarriaga (2023) show that physical IPV increases during a dry shock in the cropping season, with no effect of wet shocks. In Australia, higher maximum temperatures are associated with more police calls about domestic violence (Auliciems and DiBartolo, 1995). In the United States, violence against wives or girlfriends increases by 8% on hot days (Card and Dahl, 2011). Additionally, Nguyen (2024) examines temperature–IPV linkages using DHS surveys in 34 developing countries across Asia, Africa, and the America, although they do not analyse regional variation.

None of these studies, however, analyses regional variation in the impact of rainfall and temperature shocks in the Global South, which is our main contribution. To do so, we have developed an innovative approach that uses a standardised set of weather measures, which is sufficiently fine-grained and flexible to pick up region-specific effects. Specifically, we use monthly instead of annual deviations using long-term distributions at the local level, and we distinguish positive and negative deviations on both rainfall and temperature.

The rest of the paper is organised as follows. Section 1.2 describes data, identification strategy and outcome measures. Section 1.3 presents the main results on the impacts of weather shocks on physical IPV, robustness check of these results, impacts on other forms of IPV and impacts on mediating factors. Section 1.4 discusses the findings.

1.2 Methodology

1.2.1 Data

We use average monthly precipitation and temperature data from the Climatic Research Unit (CRU) of the University of East Anglia (UEA). CRU provides data on 0.5X0.5 decimal degrees ($\sim$ 50X50 kilometers at the equator) grid cells. Each grid cell has monthly precipitation and temperature data for the period 1901-2022. Each cell uses data from several weather stations, as weather stations are not evenly distributed across the globe. Specifically, weather data are interpolated to estimate values at the grid cells. The data is organized into a regular grid with 0.5° spacing using a method called angular distance weighting (ADW), which takes into account the distance and angular relationship between the station and the grid cell (Harris et al., 2020).

We use the Demographic and Health Surveys (DHSs), which include nationally representative samples from developing countries across different regions. The DHS is conducted multiple times in a country; however, each round uses a different nationally representative sample. While the DHS uses samples of female respondents aged 15-49 years, in some surveys, male members are also interviewed. Most of the surveys collect GPS coordinates. Importantly, since the 1990s, the DHS has had a standardised module to capture IPV.[3] We select the DHS surveys that have GPS coordinates and data on IPV. We match the clusters of these surveys with the grid cells, following two steps. First, we select the nearest grid cell for each cluster. Second, we only keep the DHS clusters that reside within the radius of their nearest grid.

Applying these procedure, we start with 74 DHS surveys that have GPS data and the standard IPV module. As our objective is to look at the influence of weather shocks on IPV across India, LA, and SSA, we only select the surveys from these regions. Of the 74 surveys, 54 cover India, LA and SSA (43 from SSA, 9 from LA and 2 from India). These 54 surveys cover 26 countries from SSA, five from LA and India. We analyse only those women who were living with a husband or partner at the time of these surveys.Table 1.A.1 shows for each selected survey, the number of DHS clusters that are matched with a weather grid cell and the number of observations under these matched clusters. The last column shows the number of matched weather grid cells. For most surveys we have a large number of matched weather grid cells, which provides sufficient variation in weather data to estimate the impact of weather shocks.

Table 1.B.1 shows descriptive statistics on the women and their husbands in the sample, disaggregated by region and whether they live in an urban or a rural area. The

[3]The DHS asks the IPV/domestic violence questions to a subsample of households that are selected for the full DHS survey. Within each of these sub-sample households, only one eligible woman is randomly chosen and interviewed in strict privacy (ICF, 2023; The DHS Program, 2014).

DHS program follows the respective country's administrative definition of rural and urban areas. While this definition is country-specific, it is mainly based on population size and administrative boundaries. We observe that the average age of women in the sample is around early 30s, and the average age of their husbands is around late 30s. The average level of education of women and their husbands is below the secondary level of education. The average number of living children they have varies between 2 and 3. Looking at IPV prevalence, the percentage of women who experienced severe physical violence in the last 12 months varies between 4% and 8%. The prevalence is highest in SSA. The prevalence of emotional IPV is higher than the prevalence of physical IPV in all regions. It is highest in SSA and LA, with prevalence rates of around 20%. The prevalence of sexual IPV ranges between 7% and 11% and is therefore higher than the prevalence of physical IPV, but lower than the prevalence of emotional IPV.

1.2.2 Identification strategy

Our identification strategy assumes that weather shocks are exogenous; specifically, shocks are not correlated with any factors that affect the occurrence of IPV. Under this assumption, we estimate the effect of weather shocks on the likelihood of IPV using the following regression.

$$y_{is} = \alpha + \sum_{n=1}^{4} X_{is}.\beta_n + N_{is}.\Theta + \gamma_p + \epsilon_{is} \tag{1.1}$$

where, y_{is} is a binary IPV indicator for woman i from survey s, taking the value of 1 if the woman experienced IPV in the previous 12 months, 0 otherwise. X_{is} is a set of annual counts of rainfall and temperature shocks. The IPV indicator is coded binary following the standard practice to collect data on IPV to minimise ambiguity and subjectivity in self-reporting (Shah and Barski, 2025), while the shock variable is measured in number to capture exposure intensity. The coefficients, βs, will capture the percentage point change in the IPV prevalence with each additional monthly (positive/negative rainfall/temperature) shock. N_{is} is a set of control variables, which includes wives' age and education, husbands' age and education and the number of living children of the wives. γ_p are survey fixed effects, each survey is unique with the combination of country and survey year. ϵ_{is} is an error term. Standard errors are clustered at the level of the DHS clusters.

Violence outcomes

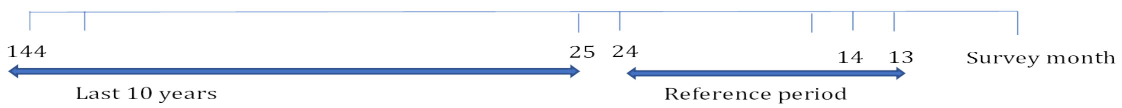
We use different categories of IPV. Physical IPV in the last 12 months is our primary dependent variable. In the DHS, physical IPV is captured using three items asking whether, in the last 12 months, the husband/partner (i) kicked or dragged her, (ii) strangled or burned her, or (iii) threatened her with a knife, gun, or other weapon. We coded physical IPV as one if she answered "yes" to any of these items (see column 1 of Table 1.B.2

). We also look at the impacts of weather shocks on other categories of IPV, including emotional and sexual IPV in the last 12 months. Emotional IPV includes humiliation, threats of harm, and insults or actions intended to make her feel bad (column 2). Sexual IPV includes being physically forced into unwanted sex, forced into unwanted sexual acts, or otherwise forced to perform sexual acts she did not want to (column 3). For each category, the indicator is equal to one if the respondent experienced at least one event.

Weather shock measure

As main explanatory variable, we use the frequency of weather shocks at each cluster in the 'reference' period of 13-24 months prior to the survey month (see Figure 1.1 for the timeline). To create this variable, we use the following two steps. First, for each month in this period and for each local cluster, we determine whether it can be classified as a weather shock. For this, we use the long-term monthly distribution at a cluster for 10 years preceding the reference period of 13-24 months prior to the survey.

Figure 1.1 Timeline to estimate the impact of weather shocks on IPV



We use a threshold for the rainfall and temperature distribution at that cluster, to define four types of weather shocks. We define a positive rainfall/temperature shock if the month-specific rainfall/temperature in the last 13-24 months prior to the survey is above the highest 10 percentile of that specific month's long-term distribution. Similarly, we define negative rainfall/temperature shocks if the month-specific rainfall/temperature in the last 13-24 months prior to the survey is below the lowest 10 percentile of that month's long-term distribution (see equation 1.2).

$$s(x_{mwc}) = \begin{cases} \text{negative} & \text{if } x_{mwc} < q_{10,mwc} \\ \text{positive} & \text{if } x_{mwc} > q_{90,mwc} \\ \text{no shock} & \text{if } q_{10,mwc} \leq x_{mwc} \leq q_{90,mwc} \end{cases} \tag{1.2}$$

where $w \in \{\text{temperature}, \text{rainfall}\}$ and x_{mwc} is the value of w in month m in the 13-24 months prior to the survey at cluster c. $q_{10,mwc}$ is the lowest 10 percentile and $q_{90,mwc}$ is the highest 10 percentile of the 10-year distribution of month m at cluster c. Note that the use of cluster-specific distributions combined with a percentile-based threshold, not only makes the shocks comparable across clusters and regions. It also reduces the correlation with possible confounding factors.

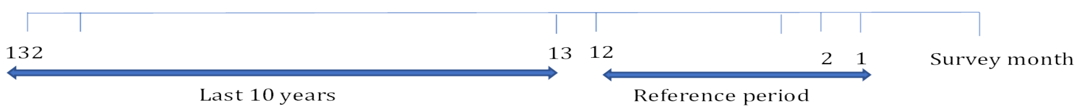
Second, after defining for each month m at cluster c whether it can be defined as a positive/negative rainfall/temperature 'shock', we count the number of shocks that each cluster experienced in the last 13-24 months prior to the survey, using equation 1.3. These count variables will be used as the main explanatory variables.

$$S^{-}_{wc} = \sum_{m=1}^{12} s(x_{wmc}) \cdot \mathbf{1}_{\{s(x_{wmc}) \text{ is `negative'}\}}, \quad S^{+}_{wc} = \sum_{m=1}^{12} s(x_{wmc}) \cdot \mathbf{1}_{\{s(x_{wmc}) \text{ is `positive'}\}} \tag{1.3}$$

Two design decisions require further discussion. First, we use the most recent 10 years to create the long-term distribution. Using a period of 10 years instead of a longer period is appropriate for the following reasons. People are more likely to focus on what happened recently. Moreover, as the average age of the wives and husbands in our sample is between 30-38 years (Table 1.B.1), weather conditions prior to the last 10-20 years would not be relevant for many of them. The recent period would also be more important, as people may adjust gradually to changes in weather patterns. Finally, as global warming has increased average temperatures, we might overestimate positive temperature shocks and underestimate negative temperature shocks if we use a longer distribution. This is indeed what we observe in the bar charts presented in Section 1.B.1, which plot the distributions of shocks using 10-year and 30-year distributions in the six sub-samples. We do not observe a similar change in the distribution of positive or negative rainfall shocks, most likely because climate change has changed rainfall patterns in different ways across different regions in the world.

Second, we use the lagged period to measure weather shocks. We do so as the domestic violence module of the DHS asks whether IPV occurred in the last 12 months, but does not provide more specific information about when the violence happened. Counting the number of shocks in the same year as the reference period used for the IPV module would make it difficult to assume that all shocks occurred *before* the violence, which we need to make causal claims. In addition, we need sufficient time for the effect of weather shocks on IPV to materialise. For example, as discussed in Section 1.3.4, weather shocks are more likely to affect spousal employment and household decision-making immediately, and these changes take time to translate into observable increases in IPV. Therefore, to examine the impacts of weather shocks on spousal employment and household decision-making (see Section 1.3.4), we measure the shocks using the earlier-mentioned method, but we adjust the reference period to reflect the time frame during which these indicators are measured, (i.e., 12 months prior to the survey month) (see Figure 1.2 for the adjusted timeline).

Figure 1.2 Timeline to estimate the impact of weather shocks on spousal employment and household decision-making



1.3 Results

1.3.1 Main effects on physical IPV

Table 1.1 presents the estimated impacts of weather shocks on the likelihood of physical IPV in each of the six regions. Columns 1 and 2 report the effects for rural and urban India. Starting with the impact of temperature shocks, we observe that positive temperature shocks strongly increase the occurrence of physical IPV. The likelihood of physical IPV increases by 0.7-0.8 percentage points with each additional monthly shock. This is a 12.5% and 20.3% increase relative to a baseline of 5.59% and 3.39% with no shocks in rural and urban India, respectively. Negative temperature shocks also increase the likelihood of IPV in rural India, but the impact is smaller than that of positive temperature shocks. Rainfall shocks do not have a strong influence on physical IPV in rural India. In urban India, however, physical IPV decreases by 0.38 percentage points with each additional positive rainfall shock.

Table 1.1 Weather shocks and physical IPV

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	-0.000928	-0.00382**	0.000431	-0.00197	0.000527	0.00831***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Negative rainfall shocks	-0.00193	0.000159	0.00351***	0.00210	0.00258	-0.00238
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Positive temperature shocks	0.00703***	0.00834***	0.00220**	0.000681	0.00852***	0.00978***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Negative temperature shocks	0.00312***	0.000621	0.000368	-0.00159	0.00549**	-0.00145
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.009	0.010	0.081	0.043	0.034	0.029
Predicted value at no shock	0.0559	0.0394	0.0682	0.0657	0.0270	0.0513

Notes: Ordinary least squares (OLS) regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Looking at the effects in Sub-Saharan Africa (columns 3 and 4), we observe that in rural SSA, physical IPV increases by 0.35 percentage points with each negative rainfall shock (5.15% increase relative to the baseline), and by 0.22 percentage points with each positive temperature shock (3.2% of the baseline). Moving to Latin American countries (columns 5 and 6), we observe that in urban LA physical IPV increases by 0.8 percentage points (16% of the baseline) with each positive rainfall shock. Heat shocks increase physical abuse by husbands in rural and urban LA by 0.85 and 0.98 percentage points (30%

and 18% of the baseline), respectively. In rural LA, IPV also increases with each negative temperature shock by 0.5 percentage points (19% of the baseline).

A comparison of the impacts of weather shocks across the six regions provides interesting insights. Positive temperature shocks increase physical IPV in nearly all regions. The effects are strongest in India and LA. Focusing on the coefficients that are statistically significant, we observe that relative to the baseline of no shocks, each additional unusually hot month increases the likelihood of IPV by 12.5%, 21.2%, 3.2%, 31.5% and 19.1% in rural and urban India, rural SSA, and rural and urban LA, respectively. The likelihood of IPV also increases with negative temperature shocks in rural India and rural LA. As to the impact of rainfall shocks, we find a positive impact of droughts on IPV in SSA but not in other regions. Furthermore, excessive rainfall increases physical IPV in urban LA, but not in India or SSA.

1.3.2 Robustness tests

To test the robustness of our findings, we conduct additional analyses, including alternative regression specifications, a placebo test and alternative measures of weather shocks. We further argue why survival bias, sampling strategy and seasonal effects in IPV reporting should not be a source of concern. Finally, we check whether our results are robust to a multiple-hypothesis test.

Logistic regression

To estimate the effects of weather shocks on the likelihood of physical IPV, we used an ordinary least squares (OLS) specification. An alternative approach is to use a logistic regression model. While OLS is easier to implement and interpret, logistic regression does not suffer from heteroscedasticity that might affect OLS estimates, tends to give more accurate predictions, and is more flexible in terms of allowing for non-linear relations. However, logistic regression with fixed effects can suffer from the incidental parameter problem, which may bias coefficient estimates (Greene, 2004). This is an important limitation of logistic regression in our case, as we combine surveys for different countries and years, and the use of fixed effects helps us to deal with differences across surveys.

In sum, both approaches have limitations in our case. It is, therefore, important to estimate a logistic regression with the same explanatory variables and compare its results with the ones we obtained with OLS. Table 1.C.1 reports the average marginal effects of weather shocks on physical IPV using a logistic regression. Comparing the marginal effects and their statistical significance with the OLS results, we observe that both lead to qualitatively similar results. In addition, Table 1.C.2 reports the differences in predicted probabilities of IPV at different frequencies of weather shocks, using the estimates generated by logistic regression. The results suggest that the marginal effects increase more or less linearly with the frequency of weather shocks. This provides further support for the marginal effects estimated with the OLS specification, which assumes that the effects are linear.

Confounders and placebo test

A possible concern is that the estimated effects of weather shocks are biased by omitted variables. We see two ways in which this could happen. First, there may be some variables that correlate with both weather shocks and the occurrence of IPV. Given how we constructed the weather shocks (see Section 1.2.2 for details), it is unlikely that our weather shock measures correlate with any confounders. To test this, we estimate the correlation between weather shocks and plausible correlates of IPV, such as demographic characteristics of wives and husbands, which we control in our main regression. Tables 1.C.3 -1.C.6 demonstrate that weather shocks are correlated with some of these characteristics. However, the coefficients are quite small, less than 0.1 in almost all cases across all types of weather shocks. Given that these shocks range from 0 to 12, these observed correlations might be negligible. We therefore test whether our main results hold without controlling for these characteristics. Table 1.C.7 shows that the impact estimates remain qualitatively similar when excluding these control variables, confirming the robustness of our main results.

Second, measurement error in IPV could act as a confounder. IPV is commonly underreported (on this see, e.g., Lépine, Treibich and D'Exelle, 2020). If the underreporting was the same for all observations, the marginal effects would be unbiased. However, if underreporting was correlated with weather shocks, then the estimates of the marginal effects of weather shocks would be biased.

We, furthermore, test for potential confounders with the help of a placebo test that estimates the impacts of weather shocks that occurred *after* the reported IPV incidents (i.e., after the survey period). Specifically, we construct a set of weather shock variables for the year following each survey. We expect that these 'post-survey' weather shocks will not influence the likelihood of IPV in the 12 months prior to the survey. The results reported in Table 1.C.8 indicate that 'post-survey' shocks do not have an impact on the likelihood of IPV, with a small number of exceptions where coefficients are marginally significant. This supports the robustness of our results.

Measure of weather shocks

In our main results, we used the shocks that occurred during the lag period preceding the occurrence of IPV, assuming that it takes some time for weather shocks to influence IPV. However, both rainfall and temperature shocks might have an immediate influence on IPV. To test this, we estimate the impacts of recent weather shocks on IPV (Table 1.C.9). We define the recent weather shocks as the number of shocks that occurred in the last 12 months prior to the survey, similar to the time frame for IPV reporting. The results show that the impacts of recent rainfall shocks disappear, with the exception of urban LA. We also observe that the frequency of recent temperature shocks has a weaker influence on IPV than that of late shocks, with only survival impacts for India. These patterns suggest that weather shocks might have some immediate effects on IPV; however, the late effects might be stronger. Furthermore, we examine whether the incidence of weather shocks, instead of the frequency, influences the likelihood of IPV. The results reported in Table 1.C.10 show qualitatively similar effects of positive temperature shocks for India and LA, and qualitatively similar effects of negative rainfall shocks for SSA.

Moreover, we test whether the variations in the length of the local distribution of weather (rainfall/temperature) and cut-off points to define shocks influence the impacts of weather shocks. In our main results, we used 10 years to create local distributions of rainfall and temperature and used 10 and 90 percentiles as cut-off points to define negative and positive shocks, respectively. For robustness checks, we use 15 years or 20 years instead of 10 years to create local distributions, or use 15/85 or 20/80 percentiles instead of 10/90 percentiles as cut-off points. The changes in these dimensions might influence the frequency of shocks. For example, we observe an increase (decrease) in the frequency of positive (negative) temperature shocks with an increase in the length of the local distribution (from 10 to 30 years) (Section 1.B.1). Tables 1.C.11 and 1.C.12 report the impacts of weather shocks using these alternative definitions. These results are qualitatively similar to our main results.

Survival bias and sampling weights

We consider two potential issues that might undermine the representativeness of our sample, with possible implications for the estimates of the effects of weather shocks. The first one is survival bias. Our dataset only includes married couples and does not account for couples that divorced between the occurrence of the weather shocks and our data collection period (i.e., within the 12 months prior to the survey). If climate shocks increase the prevalence of IPV, which possibly increases the likelihood of divorce, not accounting for the couples that divorced would underestimate the effect of climate shocks on IPV. That is why our results would provide a lower bound of the effect of climate shocks on IPV. At the same time, the period we are looking at is fairly short, and annual divorce rates are low. For example, the annual divorce rate is less than 1% in India (Dommaraju, 2016) and 2-3% in SSA (UN, 2023). This suggests that survival bias is likely to be negligible. The second issue relates to the use of sampling weights. As the DHS surveys administer the IPV module on a sub-sample, sampling weights are applied to ensure representativeness. So far, we have not applied these sampling weights in the regression analyses. We re-estimate the main OLS regressions with these weights and find that most results are robust (see Table 1.C.13).

Seasonal effects in IPV reporting

Since our data covers multiple countries and survey periods, seasonal variations in weather shocks and IPV reporting could confound the results. For instance, temperature or rainfall shocks vary across months, and seasonal cycles (such as crop harvesting time) might also influence IPV reporting. To test whether our main results are robust to these seasonal cycles. We use survey-month fixed effects, which combine the survey year and the month of interview for each observation.[4] Results reported in Table 1.C.14 are qualitatively similar to our main results.

Corrections for multiple hypothesis testing

We have four types of independent variables, and we compare the impacts of weather shocks across India, SSA, and LA, in rural and urban areas. This results in a total of 12 outcomes that raise the potential risk of obtaining a few significant results by chance. When multiple hypotheses are tested simultaneously, the probability of getting at least one false positive (Type I error) increases, even if all null hypotheses are true. To reduce the probability of committing this error, several tests of multiple hypotheses have been developed (Romano and Wolf, 2010; List, Shaikh and Xu, 2016; McKenzie, 2021). We compute sharpened False Discovery Rate (FDR) q-values, which is recommended to do when the number of hypotheses (outcomes) is quite large (McKenzie, 2021). FDR q-values account for the number of outcomes that are included in the main regression, something that the standard p-values do not. Table 1.C.15 reports both p-values and q-values, which are qualitatively similar, indicating that our results are robust to adjustments for multiple hypothesis testing.

1.3.3 Other forms of IPV

In addition to physical IPV, women often experience emotional and sexual IPV. As different forms of IPV tend to be correlated (Palmer et al., 2024; Keilholtz et al., 2023; Gondolf, Heckert and Kimmel, 2002; Schumacher and Leonard, 2017; Porcerelli et al., 2006; Tesfaw and Muluneh, 2022; Meekers, Pallin and Hutchinson, 2013), we expect to find similar effects of weather shocks on emotional and sexual IPV. To estimate the effects of weather shocks on the likelihood of emotional or sexual IPV, we use the same regression approach. Table 1.2 reports the results.

Panel (A) shows the impacts of weather shocks on emotional IPV. In India, emotional IPV strongly increases with positive temperature shocks and decreases with positive rainfall shocks. In rural SSA, the likelihood of emotional IPV increases with negative rainfall shocks and decreases with positive rainfall shocks. In SSA, emotional IPV increases with negative rainfall shocks. Panel (B) reports the impacts of weather shocks on sexual IPV. Sexual IPV increases with positive temperature shocks in urban India and rural and urban LA. In urban LA, it also increases (decreases) with positive (negative) rainfall shocks.

[4] One round of DHS typically takes four to six months to complete.

Table 1.2 Weather shocks and other forms of IPV

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (A): Emotional IPV						
Positive rainfall shocks	-0.00452**	-0.00941***	-0.00540**	-0.00388	-0.00411	-0.00219
	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)
Negative rainfall shocks	0.00112	-0.000149	0.00663***	0.00572*	-0.00296	-0.000468
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.005)
Positive temperature shocks	0.00809***	0.0122***	0.000676	0.000386	0.00807	0.00568
	(0.002)	(0.003)	(0.002)	(0.002)	(0.005)	(0.007)
Negative temperature shocks	0.00238*	0.000200	-0.00486*	0.00421	-0.00455	0.00142
	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.006)
Observations	67930	25414	74368	34381	17378	12607
R^2	0.006	0.009	0.032	0.037	0.007	0.013
Predicted value at no shock	0.101	0.0892	0.208	0.196	0.173	0.205
Panel (B): Sexual IPV						
Positive rainfall shocks	-0.000225	-0.00471*	-0.00246	-0.000731	0.00438*	0.00614***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Negative rainfall shocks	0.000438	0.00112	0.00127	0.00254	-0.00159	-0.00487**
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Positive temperature shocks	0.00187	0.00863***	-0.000299	-0.000944	0.00868***	0.00600***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Negative temperature shocks	0.00823***	0.00287	-0.00245	-0.00103	0.000342	-0.000479
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	67930	25414	76486	34705	25866	30301
R^2	0.012	0.011	0.036	0.040	0.010	0.016
Predicted value at no shock	0.0920	0.0715	0.114	0.0957	0.0447	0.0527

Notes: Panel (A) reports OLS regressions with the dependent variable equal to one if emotional violence occurred in the last 12 months, zero otherwise. Emotional violence is defined as the occurrence of any type of violence listed in column (2) of Table 1.B.2 . Panel (B) reports OLS regressions with the dependent variable equal to one if sexual violence occurred in the last 12 months, zero otherwise. Sexual violence is defined as the occurrence of any type of violence listed in column (3) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1.3.4 Mechanisms

The likelihood of IPV can be directly influenced by weather shocks. For example, unusually hot months might make people short-tempered, which increases the likelihood of spousal conflict (Sanz-Barbero et al., 2018; Auliciems and DiBartolo, 1995; Card and Dahl, 2011). However, there might also be more complex mechanisms at work, which might be interesting to explore. To do so, we estimate the effect of weather shocks on two intermediate outcomes that could trigger IPV: spousal employment and women's involvement in household decisions. Inspired by two broad theories of IPV, we will argue that both outcomes could be important mediating factors. According to 'instrumental theory', men use intra-household violence to control other family members (Straus and Hotaling, 1980; Eswaran and Malhotra, 2011). When they loose the status of primary breadwinner within the household, men may resort to violence to keep control, often referred to as 'male backlash' (Macmillan and Gartner, 1999; Thoits, 1992; Haushofer et al., 2019; Chin, 2012). 'Expressive theory', in contrast, suggests that men are more likely to use violence against women to express their frustration (Dollard et al., 1939; Berkowitz, 1989), which could arise from adverse circumstances, such as economic stress (Jewkes, 2002). Moreover, the extent to which these intermediate factors would explain the impacts of weather shocks on IPV might vary across regions. For instance, in rural areas, an increase in wives' employment and involvement in intra-household decision-making might lead to a larger increase in IPV, as rural people also tend to hold more conservative attitudes towards women's work compared to those in urban areas. Moreover, these mediating effects might be more pronounced in India, where patriarchal norms are more deeply rooted in the society compared to many countries in SSA and LA.

However, the extent to which this mechanism operates likely varies across regions depending on local labor market structures and household norms. In regions where female labor force participation is already high and women have some say in household decisions, increased employment opportunities may strengthen bargaining power and reduce IPV. Conversely, in regions where gender norms strongly restrict women's decision-making or mobility, similar employment responses may heighten male backlash and lead to greater IPV incidence.

Spousal employment

The first mediator we explore is spousal employment. Evidence shows that both rainfall and temperature shocks reduce crop yields and economic output (Dell, Jones and Olken, 2012; Guiteras, 2009; Somanathan et al., 2021; Adhvaryu, Kala and Nyshadham, 2020), which might in turn reduce the income generated by the main provider (in most cases the husband). This might be accompanied by an increase in women's employment, if women need to generate more income to offset their husbands' income loss. While this increases women's economic empowerment, it risks triggering IPV (Luke and Munshi, 2011; Caridad Bueno and Henderson, 2017). Both instrumental theory and expressive theory of IPV could explain such an effect. Men might resort to violence as they feel frustrated when their role of economic provider is threatened, or they may do so to try to keep control in their household.

To look at the role of spousal employment, we estimate the effect of weather shocks on men's and women's employment (see Table 1.3).[5] We focus on the likelihood that

[5] The DHS asked women whether 'they worked in the last seven days' and if they responded 'no', they were asked whether 'they worked in the last 12 months'. For those women who responded 'yes' to any of these two questions, they were asked what their main occupation was, and they were asked whether they did this work for a family member, someone else, or whether they were self-employed. We use the last question to define whether women worked for someone else in the last 12 months. To define husbands'

the wife works for someone else, as this type of employment is often resorted to when the husband's income drops due to external shocks. For women's employment, we distinguish agricultural activities (Panel A) and indoor economic activities (see Panel B), such as domestic help, professional jobs and skilled manual jobs. In Panel C, we focus on whether the husband works the entire year, as this is what he commonly does as the main economic provider.

We observe that in rural India, weather shocks increase the likelihood that the wife works for someone else in both agriculture and indoor jobs. This is accompanied by a reduced likelihood that the husband works the entire year. We see a similar pattern for indoor jobs but a somewhat weaker pattern for agriculture jobs in urban India, as agriculture jobs are less common in urban areas. This suggests a shift towards an increased contribution of women to the household income. In rural SSA, we observe that droughts increase the likelihood that the wife works for others in indoor jobs. This does not affect whether the husband worked all year. This is not surprising as a drought most likely affects the husband's income rather than whether he works all year. We do not find any effects of weather shocks in LA, which might be due to the smaller sample for which we have employment data.

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full-time employment, we use the question on whether husbands worked for the entire year in the last 12 months.

Table 1.3 Weather shocks and spousal employment

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (A): Wife working for someone else in agriculture in the last 12 months						
Positive rainfall shocks	0.00267***	0.000514	0.000250	0.000388	-0.000862	-0.00147
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Negative rainfall shocks	0.00109	-0.0000308	0.000303	0.000213	0.00185	-0.00119
	(0.001)	(0.000)	(0.001)	(0.000)	(0.002)	(0.001)
Positive temperature shocks	0.00173***	0.00114	0.000670	0.000617	-0.000818	0.00123
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Negative temperature shocks	0.00161***	0.000375	0.00158	0.000743	-0.00282	0.0000891
	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.000)
Observations	67930	25414	68494	28590	7486	6773
R^2	0.012	0.002	0.043	0.030	0.006	0.004
Predicted value at no shock	0.000392	0.00327	0.0201	0.00363	0.0116	0.00481
Panel (B): Wife working for someone else in indoor jobs in the last 12 months						
Positive rainfall shocks	0.000285	0.00169*	-0.000628	0.00224	-0.00760*	-0.00192
	(0.000)	(0.001)	(0.001)	(0.002)	(0.003)	(0.007)
Negative rainfall shocks	-0.0000582	0.000958	0.00187**	-0.0000934	0.00256	-0.00514
	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.006)
Positive temperature shocks	0.00678***	0.00546***	0.000827	0.00110	0.00451	-0.00717
	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)	(0.008)
Negative temperature shocks	0.00105*	-0.000199	-0.000900	0.000459	0.00195	-0.00422
	(0.000)	(0.001)	(0.001)	(0.002)	(0.004)	(0.007)
Observations	67930	25414	68494	28590	7486	6773
R^2	0.005	0.003	0.026	0.051	0.057	0.044
Predicted value at no shock	0.00852	0.00579	0.0129	0.0636	0.0614	0.182
Panel (C): Husband working all year in the last 12 months						
Positive rainfall shocks	-0.00567**	0.000761	-0.0102**	0.0116***	-0.0127	-0.00615
	(0.002)	(0.002)	(0.003)	(0.003)	(0.013)	(0.015)
Negative rainfall shocks	0.00366	-0.00676*	0.00531	-0.00740*	0.0106	-0.00432
	(0.002)	(0.003)	(0.003)	(0.003)	(0.009)	(0.013)
Positive temperature shocks	-0.00771**	-0.00559	0.00264	0.0150***	-0.00360	0.0184
	(0.003)	(0.003)	(0.003)	(0.004)	(0.013)	(0.011)
Negative temperature shocks	-0.0205***	-0.00713***	-0.0125***	-0.00961*	0.00329	-0.0142
	(0.002)	(0.002)	(0.004)	(0.004)	(0.013)	(0.012)
Observations	67929	25414	68530	28613	4372	2211
R^2	0.017	0.012	0.208	0.204	0.245	0.317
Predicted value at no shock	0.771	0.905	0.540	0.761	0.519	0.550

Notes: OLS regressions with the following dependent variables. Panel (A): dependent variable equal to one if the wife worked as an employee in agriculture, zero otherwise. Panel (B): dependent variable equal to one if the wife worked as household/domestic help or was engaged in a professional/technical/managerial or skilled manual job, zero otherwise. Panel (C): dependent variable equal to one if the husband worked all year in the last 12 months during the survey period, zero otherwise. We only used the surveys where both couples were interviewed. The positive (negative) shocks variables count the number of months in the last 12 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Household decision-making

The second potential mediator is women's involvement in household decisions. Weather shocks might influence household decision-making, which in turn might influence the likelihood of IPV. We look at each in turn. First, weather shocks might have a direct or indirect influence on household decision-making. For example, joint decision-making might decrease during exceptionally hot months, as physical discomfort caused by heat might affect communication or mutual understanding. There might also be a more indirect influence. As demonstrated in the previous section, weather shocks might negatively affect husbands' employment while simultaneously increasing wives' employment. As involvement in household decision-making tends to be positively related to how much income one is able to contribute to the household (Attanasio and Lechene, 2002; Angel-Urdinola and Wodon, 2010; Hoddinott and Haddad, 1995), this might increase women's involvement in household decisions. Second, women's involvement in household decision-making might lead to an increased risk of IPV. We see at least two ways in which this could happen. Men might use violence in an instrumental way to keep control. Even where women are more involved in decisions, (a threat of) violence enables men to maintain some power, for example, by having the final say in household decisions. In addition, where spouses want different outcomes, conflict is more likely to occur where both spouses are involved in decision-making (on this see, e.g., D'Exelle and Ringdal, 2025).

Table 1.4 Weather shocks and women's involvement in household decision-making

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	-0.00220	-0.000850	-0.00589*	-0.00743*	0.00937**	0.00707*
	(0.001)	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)
Negative rainfall shocks	0.0103***	0.00639*	0.0106***	0.0139***	0.00612	0.0104***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Positive temperature shocks	0.00205	-0.00200	0.00641*	-0.000729	-0.000338	0.0000845
	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)
Negative temperature shocks	0.00868***	0.00887***	-0.0117***	-0.0218***	0.00796	0.00453
	(0.001)	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)
Observations	67930	25414	76442	34683	25755	30188
R^2	0.015	0.016	0.199	0.163	0.048	0.024
Predicted value at no shock	0.741	0.780	0.510	0.647	0.670	0.759

Notes: OLS regressions with the dependent variable equal to one if the wife reported that she makes decisions on household large purchases either alone or jointly with her husband or other family members, zero otherwise. The positive (negative) shocks variables count the number of months in the last 12 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We examine the influence of weather shocks on women's involvement in household decisions, as measured by whether women report that they are involved in decisions about large purchases. We focus on this domain instead of other decision-making domains, as it has an important economic dimension, which we expect to be influenced by weather shocks. Table 1.4 reports the results. We find that negative rainfall shocks increase the likelihood that women are involved in household decisions in nearly all regions. In LA, women's involvement in household decisions also increases with positive rainfall shocks. Looking at temperature shocks, we observe that negative temperature shocks increase women's involvement in India but decrease their involvement in SSA.

1.4 Discussion and Conclusion

This study estimates the impact of weather shocks on the likelihood of IPV in rural and urban areas of India, Sub-Saharan Africa and Latin America. We find that the likelihood of physical IPV increases with positive temperature shocks in nearly all regions. The effects are very strong, especially in India and LA. Each additional unusually hot month increases the likelihood of IPV by 12.5%, 21.2%, 3.2%, 31.5% and 19.1% in rural and urban India, rural SSA, and rural and urban LA, respectively. These results are in line with other studies that documented a positive relationship between heat and violence (Sanz-Barbero et al., 2018; Auliciems and DiBartolo, 1995; Card and Dahl, 2011). Interestingly, physical IPV also increases with the frequency of negative temperature shocks in rural India and rural LA. Colder-than-usual temperatures might disrupt livelihoods and, in turn, lead to stress and domestic conflicts. The impacts of rainfall shocks vary across regions. Physical IPV increases with negative rainfall shocks in rural SSA, but not in other regions. This is likely due to its high dependence on agriculture and high levels of poverty. Droughts increase economic stress, which again triggers IPV, in line with existing evidence (Epstein et al., 2020; Allen, Munala and Henderson, 2021; Munala et al., 2023). We also find that positive rainfall shocks decrease physical IPV in urban India, but increase it in urban LA.

In addition, we examined the impacts of weather shocks on emotional and sexual IPV, as there is growing evidence that they often correlate with physical IPV (Palmer et al., 2024; Keilholtz et al., 2023; Gondolf, Heckert and Kimmel, 2002; Schumacher and Leonard, 2017; Porcerelli et al., 2006; Tesfaw and Muluneh, 2022; Meekers, Pallin and Hutchinson, 2013). We find that the positive effect of positive temperature shocks on physical IPV comes with an increase in emotional IPV in India and an increase in sexual IPV in urban India and LA, while the positive effect of negative temperature shocks on physical IPV in rural India is accompanied by an increase in emotional and sexual IPV. Furthermore, the positive effect of negative rainfall shocks on physical IPV in rural SSA comes with an increase in emotional IPV. In addition, the positive effect of positive rainfall shocks on physical IPV in LA is accompanied by an increase in sexual IPV, while the negative effect of positive rainfall shocks on physical IPV in urban India is accompanied by a decrease in emotional and sexual IPV.

We also investigated potential mechanisms through which weather shocks could influence physical IPV, by looking at their impact on spousal employment and household decision-making. An important mechanism is a shift in spousal employment. Weather shocks might reduce husbands' income or employment opportunities, which in turn increase women's employment to offset husbands' income loss. In response, men might resort to violence as they feel frustrated when their role of economic provider is threatened, or to keep some control in their household. We find evidence in support of this mechanism in India and SSA. In India, temperature shocks reduce the likelihood that the husband works full-time, and increase women's employment. This shift is strongest in rural India, where heat makes agricultural activities more challenging, and therefore makes it attractive to increase women's off-form employment, by taking on indoor jobs, such as domestic or skilled manual jobs (see Table 1.B.3 for descriptive statistics on men and women's economic activities). In rural SSA, the likelihood that women work for others in indoor jobs increases during droughts in rural SSA, suggesting that women increase their contribution to the household income when droughts affect income obtained from agricultural activities.

Comparing these results with the effects on physical IPV documented before, we observe that the shift in employment is in line with an increased likelihood of physical IPV. In rural India, temperature shocks increase the likelihood of physical IPV and, at the same time, lead to a shift in the employment of the wife. In rural SSA, negative rainfall shocks increase the likelihood of physical IPV and increase the wife's employment by working for someone else.

Looking at the impacts on women's involvement in household decisions, we found that it increases with negative rainfall shocks in nearly all regions. The impact of temperature shocks is less clear and varies considerably across regions. In India, negative temperature shocks increase women's involvement in household decisions, whereas in SSA they decrease women's involvement. Comparing these results with the effects on physical IPV documented before, we observe that the positive effects of weather shocks on physical IPV often come with an increased involvement of women in household decision-making. This is the case in rural SSA where negative rainfall shocks increase women's involvement as well as the likelihood of physical IPV, and in urban LA where positive rainfall shocks increase both women's involvement in household decisions and the risk of physical IPV. In India, negative temperature shocks increase both physical IPV and women's involvement in household decisions.

The heterogeneity of the Global South implies that the consequences of weather shocks for intimate partner violence are unlikely to be uniform. Rather, substantial regional differences in both the magnitude and pathways of these effects should be expected. In this article, we have provided empirical insights into this variation and its underlying mechanisms. Such evidence is critical for informing policy responses, including the design of early-warning systems and the strategic targeting of prevention and support efforts toward regions and populations most vulnerable to climate-related risks.

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Appendix

1.A Survey Selection

Table 1.A.1 Summary of selected surveys

Country	DHS Year	Matched DHS clusters	Matched observations	Matched weather grid cells
India	2020	8952	46225	1132
India	2014	9718	47225	1125
Angola	2015	585	2034	194
Burkina Faso	2010	540	3508	94
Burundi	2016	506	1477	16
Cameroon	2018	366	904	105
Chad	2014	581	2349	178
Comoros	2012	212	566	5
Cote d'Ivoire	2012	334	1526	101
Egypt	2005	1278	4995	67
Egypt	2014	1725	6243	72
Ethiopia	2016	597	2597	193
Gabon	2012	318	1494	73
Gambia	2019	269	901	13
Ghana	2008	366	1018	79
Kenya	2008	381	1247	95
Kenya	2003	378	1187	79
Kenya	2014	1218	2266	145
Liberia	2019	315	1424	43
Liberia	2007	289	2196	42
Madagascar	2021	650	4128	180
Malawi	2010	814	3293	54
Malawi	2004	492	1705	53
Malawi	2015	840	3379	54
Mali	2018	328	2241	120
Mali	2012	406	2059	114

Continued on next page

Table 1.A.1 – Summary of selected surveys

Country	DHS Year	Matched DHS clusters	Matched observations	Matched weather grid cells
Mali	2006	400	1929	149
Mauritania	2020	1122	2872	117
Nigeria	2018	1349	6346	265
Nigeria	2008	877	6726	263
Nigeria	2013	876	6907	239
Rwanda	2010	488	2450	15
Rwanda	2014	468	1377	14
Rwanda	2019	478	1387	14
Rwanda	2005	449	1872	14
Sierra Leone	2019	546	2559	33
South Africa	2017	333	495	145
Tanzania	2015	542	1278	204
Tanzania	2010	414	938	167
Togo	2013	326	1751	30
Zambia	2013	719	6071	197
Zambia	2007	315	2689	146
Zambia	2018	535	4533	191
Zimbabwe	2010	390	2378	113
Zimbabwe	2015	397	2966	119
Colombia	2010	4783	26326	212
Dominican Republic	2013	518	3148	28
Dominican Republic	2007	1339	4634	27
Guatemala	2015	852	5682	43
Haiti	2012	437	2407	19
Haiti	2006	321	1270	20
Haiti	2016	447	2517	20
Haiti	2000	227	435	20
Honduras	2011	1100	10033	50
Total		53,506	262,163	7,300

Notes: For matching, we follow two steps to select all DHS surveys that fall under the weather grids. First, we select the corresponding/nearest grid cell for each cluster. We use STATA command of 'geo near' to assign the nearest weather grid for each cluster. This command computes the distances between two points based on their coordinates. Second, we only keep those DHS clusters that reside within the radius of their nearest grid. Given this radius of the grid cell (0.5X0.5 decimal degrees), we only keep those clusters with the maximum distance from the center of the nearest grid cell within $\sim$ 55.6 kilometers. Notably, one degree covers $\sim$ 111 kilometers.

1.B Descriptive Statistics

Table 1.B.1 Descriptive statistics

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
Age (wife)	32.80	33.40	30.09	30.74	31.94	33.09
Years of schooling (wife)	2.59	3.51	2.55	3.53	3.33	3.99
Age (husband)	37.29	38.16	37.11	37.96	36.99	37.54
Years of schooling (husband)	3.28	3.85	3.08	3.94	3.91	5.57
Number of living children	2.36	2.03	3.26	2.72	3.04	2.28
Physical IPV	0.06	0.04	0.08	0.07	0.06	0.07
Emotional IPV	0.11	0.09	0.20	0.20	0.18	0.22
Sexual IPV	0.11	0.08	0.11	0.10	0.07	0.07
Observations	68012	25438	77195	35066	25968	30484

Notes: Mean of the following variables: respondents' and husbands' age measured in years; their year of schooling measured by number of years of school completed; number of living children measured by how many living children the respondent has; 'Physical IPV' takes the value of 1 if any form of severe physical abuse (defined by kicked/dragged or strangled/burnt or threatened with any weapon) occurred in the last 12 months, zero otherwise; 'Emotional IPV' takes the value of 1 if any form of emotional abuse (defined by humiliated or threatened with harm or insulted or made to feel bad) occurred in the last 12 months, zero otherwise; and 'Sexual IPV' takes the value of 1 if any form of sexual abuse (defined by being forced into unwanted sex or unwanted sexual acts or performing disrespectful sexual acts) occurred in the last 12 months, zero otherwise.

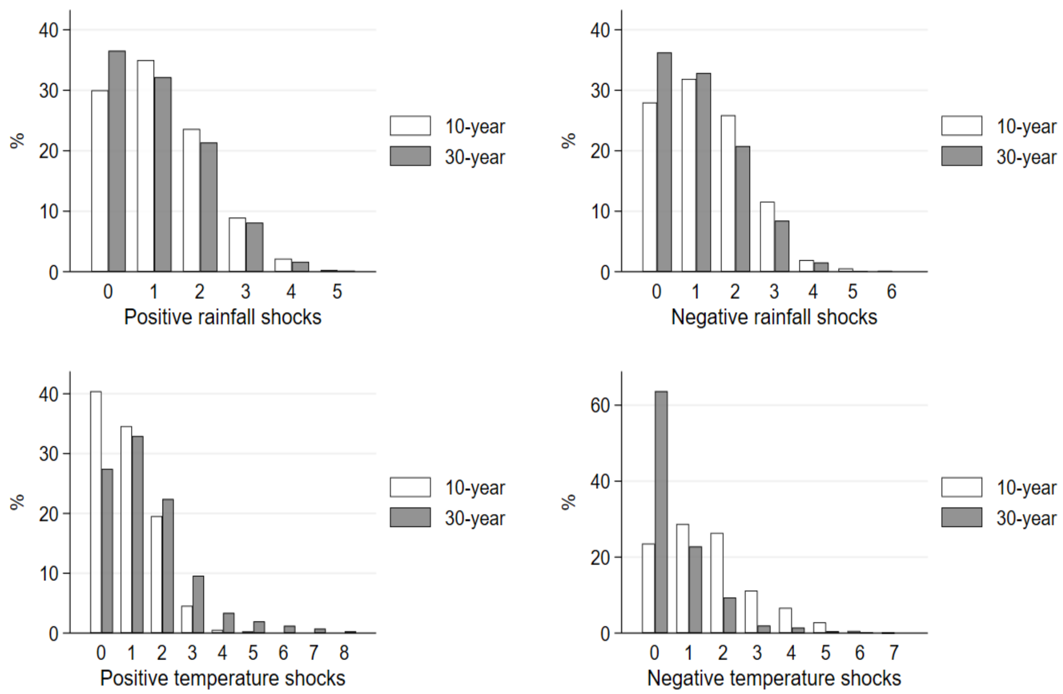
Table 1.B.2 Types of violence

(1)	(2)	(3)
Physical	Emotional	Sexual
Been kicked or dragged by husband/partner	Been humiliated by husband/partner	Been physically forced into unwanted sex by husband/partner
Been strangled or burnt by husband/partner	Been threatened with harm by husband/partner	Been forced into unwanted sexual acts by other husband/partner
Been threatened with knife/gun or other weapon by husband/partner	Been insulted or made to feel bad by husband/partner	Been physically forced to perform sexual acts respondent didn't want to

Notes: These items are from the DHS IPV module. The DHS program selects a set of items for each form of IPV to define its' prevalence at the aggregate level. Column 1 shows the items to define severe physical IPV, while columns 2 and 3 show the items for emotional and sexual IPV, respectively.

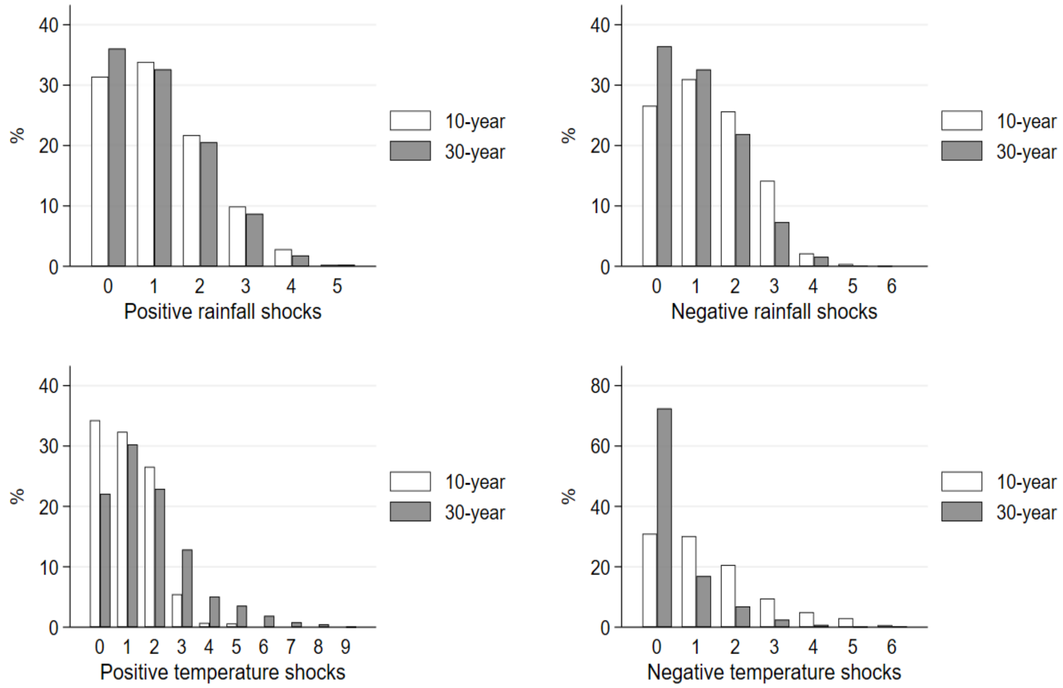
1.B.1 Shocks distribution in last 13-24 months prior to the survey

Figure 1.B.1 Shocks distribution in last 13-24 months prior to the survey at the individual level in rural India



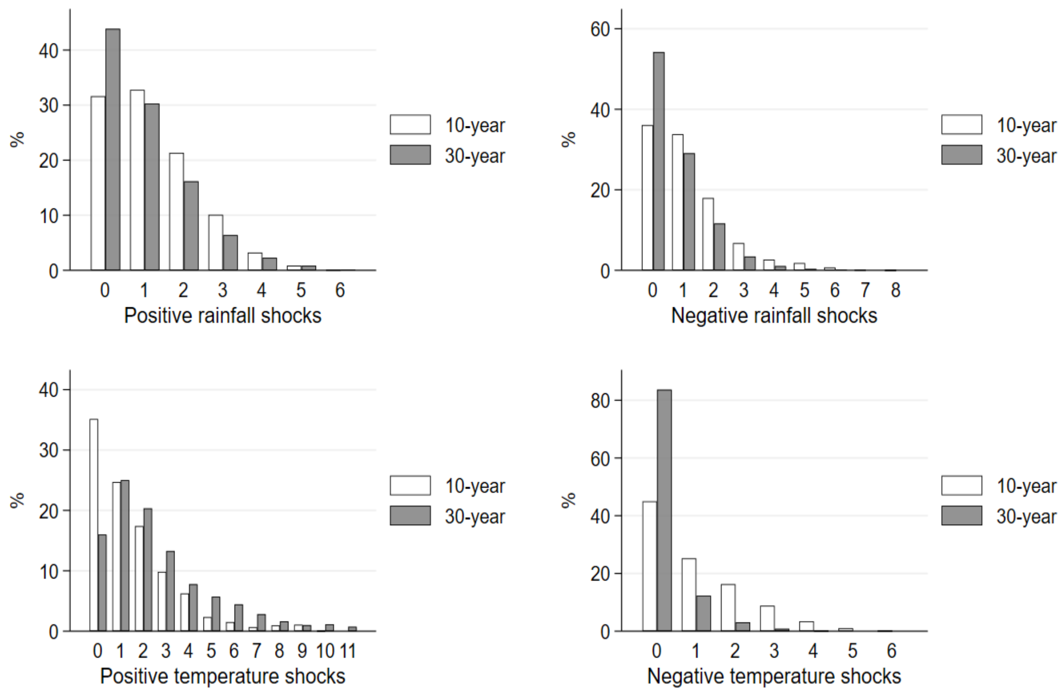
Notes: Figure shows the percentage of observation in the vertical axis and number of a specific shock in the horizontal axis across two different long-term distribution periods (10 vs 30 years). The figure on the top left illustrates positive rainfall shocks, the top right one illustrates negative rainfall shocks, the bottom left one illustrates for positive temperature shocks and the bottom right one illustrates negative temperature shocks.

Figure 1.B.2 Shocks distribution in last 13-24 months prior to the survey at the individual level in urban India



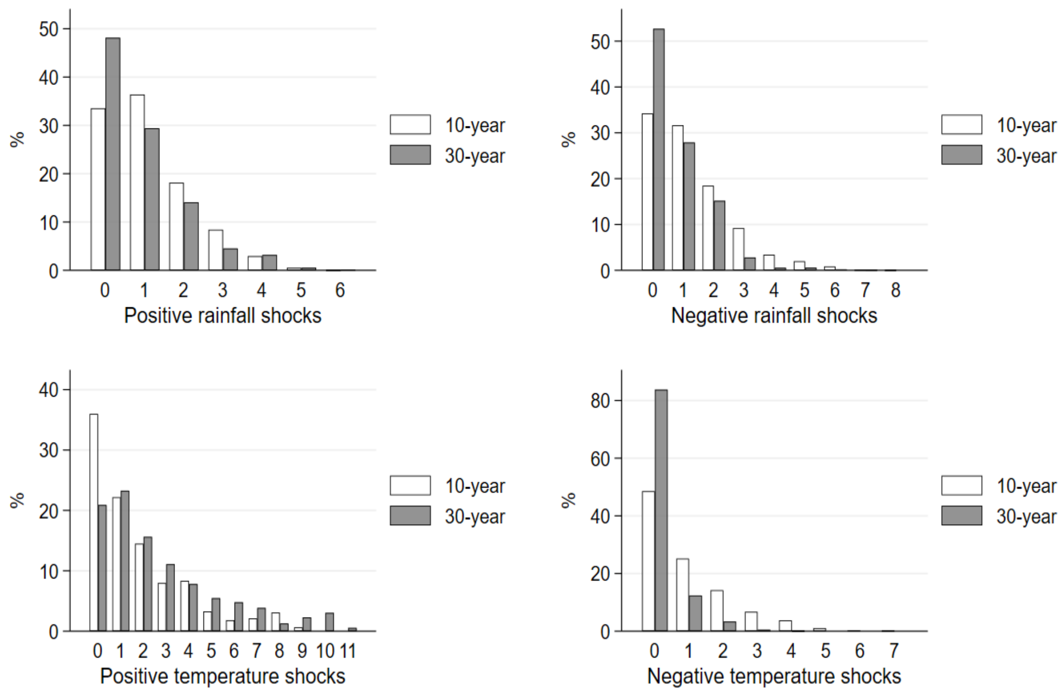
Notes: Figure shows the percentage of observation in the vertical axis and number of a specific shock in the horizontal axis across two different long-term distribution periods (10 vs 30 years). The figure on the top left illustrates positive rainfall shocks, the top right one illustrates negative rainfall shocks, the bottom left one illustrates for positive temperature shocks and the bottom right one illustrates negative temperature shocks.

Figure 1.B.3 Shocks distribution in last 13-24 months prior to the survey at the individual level in rural SSA



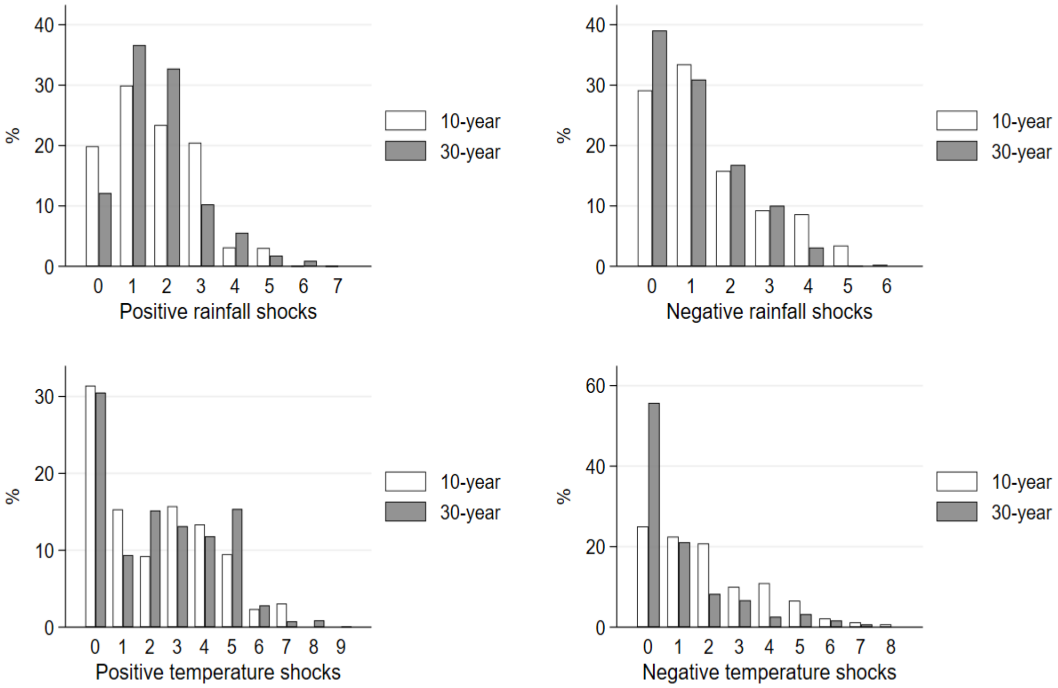
Notes: Figure shows the percentage of observation in the vertical axis and number of a specific shock in the horizontal axis across two different long-term distribution periods (10 vs 30 years). The figure on the top left illustrates positive rainfall shocks, the top right one illustrates negative rainfall shocks, the bottom left one illustrates for positive temperature shocks and the bottom right one illustrates negative temperature shocks.

Figure 1.B.4 Shocks distribution in last 13-24 months prior to the survey at the individual level in urban SSA



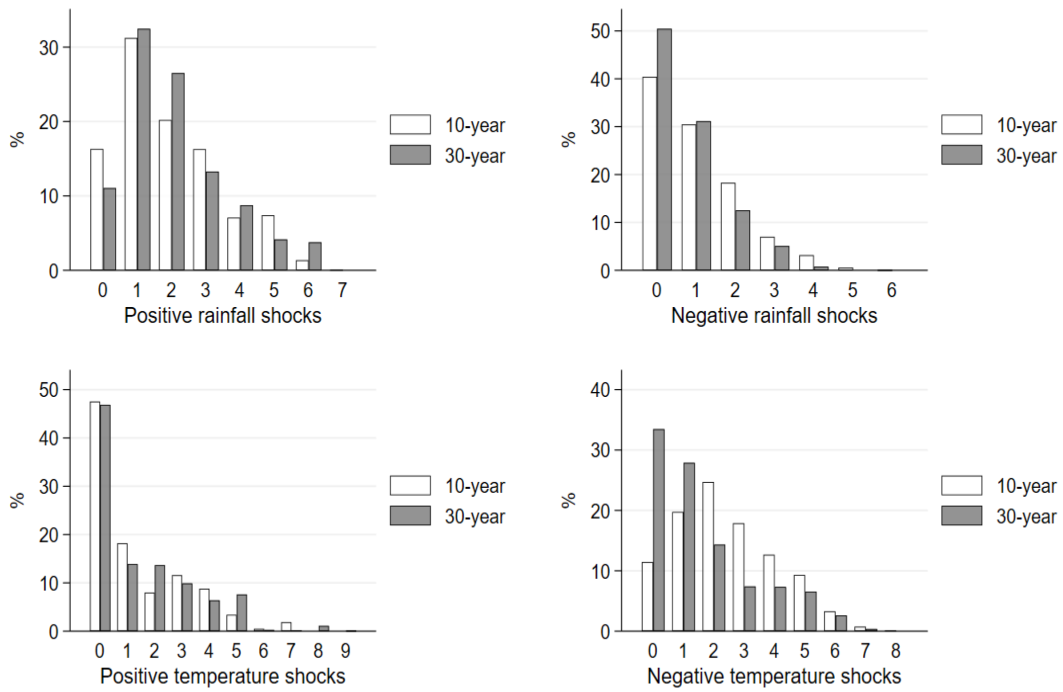
Notes: Figure shows the percentage of observation in the vertical axis and number of a specific shock in the horizontal axis across two different long-term distribution periods (10 vs 30 years). The figure on the top left illustrates positive rainfall shocks, the top right one illustrates negative rainfall shocks, the bottom left one illustrates for positive temperature shocks and the bottom right one illustrates negative temperature shocks.

Figure 1.B.5 Shocks distribution in last 13-24 months prior to the survey at the individual level in rural LA



Notes: Figure shows the percentage of observation in the vertical axis and number of a specific shock in the horizontal axis across two different long-term distribution periods (10 vs 30 years). The figure on the top left illustrates positive rainfall shocks, the top right one illustrates negative rainfall shocks, the bottom left one illustrates for positive temperature shocks and the bottom right one illustrates negative temperature shocks.

Figure 1.B.6 Shocks distribution in last 13-24 months prior to the survey at the individual level in urban LA



Notes: Figure shows the percentage of observation in the vertical axis and number of a specific shock in the horizontal axis across two different long-term distribution periods (10 vs 30 years). The figure on the top left illustrates positive rainfall shocks, the top right one illustrates negative rainfall shocks, the bottom left one illustrates for positive temperature shocks and the bottom right one illustrates negative temperature shocks.

1.B.2 Wives' and husbands' main occupation in India

Table 1.B.3 Wives' and husbands' main occupation (%) in India

	Wive working for someone else		Husbands working occasionally or seasonally	
	Rural	Urban	Rural	Urban
Professional/technical/managerial	6.26	23.49	0.97	3.98
Clerical	0.52	1.21	0.14	0.59
Sales	1.63	3.22	1.34	6.09
Agriculture - employee	34.44	21.48	35.13	20.18
Household and domestic	29.61	6.31	35.97	11.55
Services	13.59	22.01	12.31	24.92
Skilled manual	12.25	20.00	11.36	24.70
Unskilled manual	1.70	2.28	1.69	4.31
Agriculture - self employed	0.00	0.00	1.09	3.68
Total	100	100	100	1000
Observations	2523	745	18201	2364

Notes: We only report this distribution for wives who reported to work for someone else and husbands who reported to work occasionally or seasonally. The DHS asked the wives and husbands for their main economic occupation. We use the occupation groups defined by the DHS program. We report the percentage of wives (husbands) engaged in each group. The sum of all groups is 100 %.

1.C Robustness tests

1.C.1 Impacts of weather shocks using logit regressions

Table 1.C.1 Weather shocks and physical IPV using logistic regression

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	-0.000988	-0.00378**	0.000409	-0.00159	0.000846	0.00627***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Negative rainfall shocks	-0.00175	0.000281	0.00306***	0.00178	0.00280*	-0.00190
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Positive temperature shocks	0.00673***	0.00751***	0.00255**	0.000693	0.00514***	0.00708***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Negative temperature shocks	0.00296***	0.000578	0.000136	-0.00154	0.00351**	-0.000985
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Observations	67930	25414	76484	34706	25866	30301
Predicted value at no shock	0.0528	0.0363	0.0527	0.0521	0.0294	0.0468

Notes: Logit regressions with the dependent variable equal to one if physical IPV occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . We report the average marginal effects of each form of shock, setting all explanatory variables at their means. The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.2 Differences in predicted probabilities of weather shocks' effect on physical IPV

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shock (0 vs 1)	-0.000999	-0.00402**	0.000407	-0.00162	0.000830	0.00547***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Positive rainfall shock (0 vs 3)	-0.00295	-0.0111**	0.00123	-0.00471	0.00253	0.0115***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.002)
Positive rainfall shock (0 vs 6)	-0.00577	-0.0195**	0.00248	-0.00903	0.00519	0.0419***
	(0.006)	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)
Negative rainfall shock (0 vs 1)	-0.00179	0.000280	0.00296***	0.00174	0.00266*	-0.00193
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Negative rainfall shock (0 vs 3)	-0.00523	0.000845	0.00933***	0.00539	0.00843*	-0.00380
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)
Negative rainfall shock (0 vs 6)	-0.0100	0.00171	0.0201***	0.0113	0.0184	-0.0108
	(0.006)	(0.008)	(0.006)	(0.007)	(0.010)	(0.009)
Positive temperature shock (0 vs 1)	0.00645***	0.00679***	0.00244**	0.000682	0.00433***	0.00644**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Positive temperature shock (0 vs 3)	0.0216***	0.0245***	0.00762**	0.00207	0.0145***	0.0216***
	(0.005)	(0.006)	(0.003)	(0.003)	(0.003)	(0.004)
Positive temperature shock (0 vs 6)	0.0508***	0.0652***	0.0162*	0.00422	0.0340***	0.0510***
	(0.012)	(0.019)	(0.006)	(0.007)	(0.009)	(0.011)
Negative temperature shock (0 vs 1)	0.00281***	0.000571	0.000136	-0.00156	0.00317**	-0.00101
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Negative temperature shock (0 vs 3)	0.00884***	0.00174	0.000409	-0.00456	0.0102**	-0.00300
	(0.002)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)
Negative temperature shock (0 vs 6)	0.0190***	0.00355	0.000821	-0.00875	0.0227*	-0.00586
	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)	(0.007)
Observations	67930	25414	76484	34706	25866	30301
Predicted value at no shock	0.0528	0.0363	0.0527	0.0521	0.0294	0.0468

Notes: Logit regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . We calculate the differences at different frequencies of shocks (i.e., 0 vs 1, 0 vs 3, 0 vs 6). The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1.C.2 Confounders and placebo test

Table 1.C.3 Correlation between positive rainfall shocks and controls

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Wife's age (year)	0.0124***	0.0160***	-0.00238**	-0.00128	0.00429**	0.00619***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Wife's year of schooling	-0.00447	-0.00574	-0.00510*	-0.00440	-0.00153	0.00140
	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.004)
Husband's age (year)	-0.0137***	-0.0209***	0.00157**	-0.000439	-0.00159	0.000579
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Husband's year of schooling	0.0154***	0.0138***	-0.00457*	-0.000532	-0.0209***	0.00291
	(0.002)	(0.004)	(0.002)	(0.003)	(0.004)	(0.003)
Number of living children	0.0224***	0.0658***	0.00190	0.0104**	-0.0230***	-0.0575***
	(0.004)	(0.008)	(0.003)	(0.004)	(0.005)	(0.007)
Constant	1.018***	1.080***	0.553***	0.736***	2.061***	1.013***
	(0.029)	(0.050)	(0.057)	(0.057)	(0.072)	(0.064)
Observations	67930	25414	76490	34708	25867	30301
R^2	0.037	0.042	0.331	0.288	0.326	0.228

Notes: Ordinary least squares (OLS) regressions with the dependent variable the frequency of positive rainfall shocks. The shocks variable counts the number of months in the last 13-24 months prior to the survey when rainfall at the local cluster falls above the highest 10 percentile of that month's long-term distribution. Independent variables include wife's age and education, husband's age and education, number of living children. We use survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.4 Correlation between negative rainfall shocks and controls

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Wife's age (year)	-0.00268	0.00762**	0.00610***	0.0139***	0.00204	-0.000541
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Wife's year of schooling	0.0160***	0.0140***	0.0222***	0.0153***	0.00131	0.00144
	(0.002)	(0.004)	(0.002)	(0.003)	(0.004)	(0.003)
Husband's age (year)	0.00784***	-0.00314	-0.00193**	-0.00517***	-0.000690	0.000722
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Husband's year of schooling	-0.00184	-0.00444	0.0118***	0.0134***	0.0183***	0.00583**
	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)
Number of living children	-0.0241***	-0.0240**	-0.0116***	-0.0327***	-0.000336	0.00446
	(0.004)	(0.008)	(0.003)	(0.005)	(0.005)	(0.005)
Constant	0.899***	0.938***	1.700***	1.919***	1.243***	2.562***
	(0.030)	(0.050)	(0.153)	(0.130)	(0.112)	(0.121)
Observations	67930	25414	76490	34708	25867	30301
R^2	0.042	0.059	0.301	0.330	0.413	0.291

Notes: Ordinary least squares (OLS) regressions with the dependent variable the frequency of negative rainfall shocks. The shocks variable counts the number of months in the last 13-24 months prior to the survey when rainfall at the local cluster falls below the lowest 10 percentile of that month's long-term distribution. Independent variables include wife's age and education, husband's age and education, number of living children. We use survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.5 Correlation between positive temperature shocks and controls

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Wife's age (year)	-0.00621***	-0.00414	0.00671***	0.00860***	0.00618***	0.00239
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Wife's year of schooling	0.0366***	0.0217***	0.0258***	0.0107***	0.0164***	0.00423
	(0.002)	(0.004)	(0.002)	(0.003)	(0.004)	(0.003)
Husband's age (year)	0.0173***	0.0130***	-0.00144*	-0.00330***	-0.00196	-0.00127
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Husband's year of schooling	0.00698***	-0.00734*	0.0115***	0.00808**	-0.00353	-0.00380
	(0.002)	(0.004)	(0.002)	(0.003)	(0.004)	(0.003)
Number of living children	-0.0554***	-0.0499***	-0.0170***	-0.0259***	-0.0223***	-0.000723
	(0.003)	(0.007)	(0.003)	(0.005)	(0.005)	(0.006)
Constant	0.462***	0.762***	0.218***	0.301***	4.207***	0.802***
	(0.025)	(0.044)	(0.043)	(0.061)	(0.055)	(0.083)
Observations	67930	25414	76490	34708	25867	30301
R^2	0.024	0.010	0.653	0.725	0.714	0.598

Notes: Ordinary least squares (OLS) regressions with the dependent variable the frequency of positive temperature shocks. The shocks variable counts the number of months in the last 13-24 months prior to the survey when temperature at the local cluster falls above the highest 10 percentile of that month's long-term distribution. Independent variables include wife's age and education, husband's age and education, number of living children. We use survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.6 Correlation between negative temperature shocks and controls

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Wife's age (year)	-0.0169***	-0.0107***	-0.000630	-0.000896	-0.00539***	-0.00422**
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
Wife's year of schooling	-0.00920**	-0.00223	0.000110	0.00293	-0.0131***	-0.00513
	(0.003)	(0.005)	(0.002)	(0.002)	(0.004)	(0.004)
Husband's age (year)	0.00276	0.00400	0.000148	0.000456	0.00218*	0.00353***
	(0.002)	(0.003)	(0.000)	(0.001)	(0.001)	(0.001)
Husband's year of schooling	-0.0114***	0.00187	0.000309	0.00258	-0.00236	-0.00954***
	(0.003)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)
Number of living children	0.102***	0.0870***	0.00342	0.00980**	0.0156**	0.00381
	(0.005)	(0.009)	(0.002)	(0.003)	(0.005)	(0.006)
Constant	1.710***	1.228***	1.011***	1.007***	1.004***	2.044***
	(0.037)	(0.062)	(0.051)	(0.045)	(0.041)	(0.071)
Observations	67930	25414	76490	34708	25867	30301
R^2	0.029	0.023	0.600	0.623	0.673	0.506

Notes: Ordinary least squares (OLS) regressions with the dependent variable the frequency of negative temperature shocks. The shocks variable counts the number of months in the last 13-24 months prior to the survey when temperature at the local cluster falls below the lowest 10 percentile of that month's long-term distribution. Independent variables include wife's age and education, husband's age and education, number of living children. We use survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.7 Weather shocks and physical IPV without controlling for correlates of IPV

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	-0.000905	-0.00303*	0.000325	-0.00170	0.0000361	0.00690***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Negative rainfall shocks	-0.00217	0.00000680	0.00373***	0.00213	0.00266	-0.00245
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Positive temperature shocks	0.00441**	0.00719***	0.00231**	0.000425	0.00791***	0.00932***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Negative temperature shocks	0.00355***	0.000870	0.000472	-0.00134	0.00586**	-0.000946
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Observations	68012	25438	77187	35064	25967	30484
R^2	0.001	0.001	0.081	0.041	0.030	0.017
Predicted value at no shock	0.0579	0.0395	0.0677	0.0657	0.0283	0.0535

Notes: Ordinary least squares (OLS) regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include only survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.8 Post-survey weather shocks and physical IPV

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	0.00295*	0.00366*	-0.000635	0.00314	-0.00116	-0.00344
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
Negative rainfall shocks	-0.00235	-0.00178	-0.00178	-0.00104	0.00527*	0.00113
	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)
Positive temperature shocks	-0.000588	0.00183	-0.0000984	-0.000375	0.000910	-0.000462
	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
Negative temperature shocks	-0.000488	0.00165	-0.00382*	0.00212	0.000316	-0.00506
	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.023	0.024	0.086	0.046	0.045	0.033
Predicted value at no shock	0.0618	0.0329	0.0843	0.0616	0.0538	0.0920

Notes: Ordinary least squares (OLS) regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. We also control for monthly rainfall and temperature for each of the 24 months prior to the survey to avoid that post-survey shocks pick up any effects of prior-survey weather due to temporal autocorrelation. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1.C.3 Measure of weather shocks

Table 1.C.9 Recent weather shocks and physical IPV

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	-0.000787	0.000688	-0.000599	-0.00120	0.00244	0.00724^{***}
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Negative rainfall shocks	0.000441	0.00208	0.000203	0.00132	-0.00310	0.00228
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Positive temperature shocks	0.00337^{*}	0.00788^{***}	0.000112	0.00294	-0.00345^{*}	0.00110
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Negative temperature shocks	0.00403^{***}	0.00153	-0.00354^{**}	-0.00298	-0.00593^{**}	0.000537
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.009	0.010	0.081	0.043	0.033	0.027
Predicted value at no shock	0.0515	0.0309	0.0811	0.0658	0.0850	0.0545

Notes: OLS regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 12 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.10 Incidence of weather shocks and physical IPV

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shock (yes/no)	-0.00604^{*}	-0.0100^{**}	-0.000808	-0.00424	-0.0177^{**}	0.00499
	(0.003)	(0.004)	(0.002)	(0.003)	(0.006)	(0.005)
Negative rainfall shock (yes/no)	-0.00286	0.00593	0.00758^{**}	0.00149	0.0167^{***}	-0.00638
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)
Positive temperature shock (yes/no)	0.00500^{*}	0.00570	0.00535	0.00133	0.0202^{**}	0.0208^{***}
	(0.003)	(0.003)	(0.003)	(0.004)	(0.007)	(0.005)
Negative temperature shock (yes/no)	0.0131^{***}	0.00150	-0.00119	-0.00135	-0.00194	0.0104
	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.005)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.009	0.009	0.081	0.043	0.035	0.028

Notes: OLS regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables are equal to one if in any one month in the last 13-24 months prior to the survey, temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.11 Weather shocks and physical IPV, by different lengths of long-term distribution

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (A): 15 years						
Positive rainfall shocks	-0.00110	-0.00436**	-0.00104	-0.00156	-0.00306	0.00686***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Negative rainfall shocks	-0.00103	0.00110	0.00411***	0.00356*	0.00264	0.00131
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Positive temperature shocks	0.00440***	0.00389**	0.00193*	0.00174	0.00142	0.00287
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Negative temperature shocks	0.00297**	-0.0000319	0.00112	0.00151	-0.00237	-0.00508**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.009	0.009	0.081	0.043	0.033	0.028
Panel (B): 20 years						
Positive rainfall shocks	0.0000274	-0.00226	0.000316	-0.00272	0.000498	0.00360*
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Negative rainfall shocks	-0.00732***	-0.000829	0.00278*	0.000267	0.000996	0.00396
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Positive temperature shocks	0.00697***	0.00577***	0.00209*	0.00222*	0.00660**	0.00389
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Negative temperature shocks	0.00305**	0.000400	0.00208	0.000581	0.0000276	0.00224
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.010	0.010	0.081	0.043	0.033	0.027

Notes: OLS regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution- based on 15 years in panel A and 20 years in panel B.. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.C.12 Weather shocks and physical IPV, by different percentile as cut-off point

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (A): 15 percentile						
Positive rainfall shocks	-0.00147	-0.00335**	-0.000400	-0.00113	0.0000471	0.00666***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Negative rainfall shocks	-0.00337***	-0.000708	0.00330***	0.00238*	0.00181	0.000162
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Positive temperature shocks	0.00554***	0.00642***	0.000958	-0.000183	0.00619**	0.00610**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Negative temperature shocks	0.000828	-0.000155	0.00266*	-0.00146	0.00264	-0.00396*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.009	0.011	0.081	0.043	0.033	0.028
Panel (B): 20 percentile						
Positive rainfall shocks	-0.00127	-0.00206	-0.00106	-0.00170	0.00160	0.00535***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Negative rainfall shocks	-0.00488***	-0.000522	0.00347***	0.00201	0.00306*	0.000572
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Positive temperature shocks	0.00548***	0.00483***	0.000719	0.000432	0.00562**	0.00470*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Negative temperature shocks	0.000197	-0.000708	0.00228*	-0.00254	0.00192	-0.00257
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.010	0.011	0.081	0.043	0.034	0.028

Notes: Ordinary least squares (OLS) regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 15 percentile of that month's long-term distribution in panel (A) and 20 percentile of that month's long-term distribution in panel (B). Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1.C.4 Sample weights

Table 1.C.13 Weather shocks and physical IPV with sample weights

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	-0.00245	-0.00182	0.000915	-0.000147	0.00538*	0.00853***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)
Negative rainfall shocks	-0.00453*	-0.00154	0.00262*	0.00103	-0.00270	-0.00273
	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
Positive temperature shocks	0.0110***	0.0102***	0.00215*	0.00110	0.0134***	0.00869***
	(0.002)	(0.003)	(0.001)	(0.001)	(0.003)	(0.002)
Negative temperature shocks	0.00294	-0.00254	0.000695	-0.00135	0.00412	0.00169
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Observations	67930	25414	76490	34707	25866	30301
R^2	0.012	0.014	0.079	0.042	0.033	0.030
Predicted value at no shock	0.0660	0.0440	0.0658	0.0650	0.0145	0.0436

Notes: Ordinary least squares (OLS) regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1.C.5 Seasonal effects in IPV reporting

Table 1.C.14 Weather shocks and physical IPV with month of interview fixed effects

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	-0.00258*	-0.00474**	0.000839	-0.00263	0.000390	0.00707***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Negative rainfall shocks	-0.00239*	-0.00199	0.00327**	0.00273	0.00928***	-0.00149
	(0.001)	(0.002)	(0.001)	(0.001)	(0.003)	(0.002)
Positive temperature shocks	0.00597***	0.00854***	0.00168	0.000447	0.0161***	0.0184***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)	(0.003)
Negative temperature shocks	0.00420***	0.00184	-0.000272	-0.00126	0.00773**	-0.00199
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)
Observations	67930	25414	76484	34706	25866	30301
R^2	0.011	0.012	0.084	0.051	0.036	0.030
Predicted value at no shock	0.0577	0.0415	0.0694	0.0658	-0.00358	0.0423

Notes: Ordinary least squares (OLS) regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey-month fixed effects. For survey-month fixed effects, we combine the survey and the month of interview. Standard errors presented in parentheses and clustered at DHS enumeration areas.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1.C.6 Adjustments for multiple hypothesis testing

Table 1.C.15 Weather shocks and physical IPV

	India		SSA		LA	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Positive rainfall shocks	-0.000928	-0.00382*	0.000431	-0.00197	0.000527	0.00831**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
p-value	0.411	0.006	0.692	0.185	0.797	0.000
sharpened q-value	0.336	0.013	0.424	0.211	0.46	0.001
Negative rainfall shocks	-0.00193	0.000159	0.00351**	0.00210	0.00258	-0.00238
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
p-value	0.091	0.91	0.000	0.106	0.100	0.186
sharpened q-value	0.129	0.519	0.001	0.129	0.129	0.211
Positive temperature shocks	0.00703**	0.00834**	0.00220*	0.000681	0.00852**	0.00978**
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
p-value	0.000	0.000	0.009	0.524	0.000	0.000
sharpened q-value	0.001	0.001	0.016	0.424	0.001	0.001
Negative temperature shocks	0.00312**	0.000621	0.000368	-0.00159	0.00549*	-0.00145
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
p-value	0.000	0.576	0.805	0.376	0.008	0.366
sharpened q-value	0.001	0.424	0.460	0.322	0.016	0.322
Observations	67930	25414	76484	34706	25866	30301
R^2	0.009	0.010	0.081	0.043	0.033	0.027
Predicted value at no shock	0.0515	0.0309	0.0811	0.0658	0.0850	0.0545

Notes: Ordinary least squares (OLS) regressions with the dependent variable equal to one if physical violence occurred in the last 12 months, zero otherwise. Physical violence is defined as the occurrence of any type of violence listed in column (1) of Table 1.B.2 . The positive (negative) shocks variables count the number of months in the last 13-24 months prior to the survey when temperature/rainfall at the local cluster falls above (below) the highest (lowest) 10 percentile of that month's long-term distribution. Controls include women's age and education, husband's age and education, women's number of living children, and survey fixed effects. Standard errors presented in parentheses and clustered at DHS enumeration areas. p-values are reported from the main regressions, and sharpened q-values are reported as False Discovery Rate (FDR) q-values. FDR is the expected proportion of false rejections (McKenzie, 2021). * $q < 0.05$, ** $q < 0.01$, *** $q < 0.001$

CHAPTER 2

Intimate partner violence in Bangladesh: The short- and long-run impacts of a graduation programme

Abstract[1]

Assuming that women's economic empowerment can reduce intimate partner violence (IPV), several development programmes and policies have aimed to reduce IPV by supporting women's economic conditions. However, evidence on the short-run impacts of this approach is mixed, and evidence on long-run impact remains scarce. To address this gap, we evaluate a credit-based graduation programme using a randomised controlled trial (RCT) with endlines at one and seven years post-intervention. In the short run, we find that women in the treatment group are less likely to experience physical IPV than women in the control group. However, in the long run, women in the treatment group are more likely to experience IPV than the control group. This reversal is driven by the change in the economic conditions of women in treatment and control groups between the two endlines. While women in the treatment group fail to make further economic progress between both endlines, women in the control group gradually narrow the gap with women in the treatment group. These findings suggest that sustained IPV reduction requires continuous improvement in women's economic conditions.

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2.1 Introduction

Building on the hypothesis that improved economic conditions of women can reduce IPV, several development programmes, such as cash transfers, microfinance, and graduation programmes, have tried to increase women's economic empowerment in an attempt to reduce IPV. Studies evaluating the short-run effect of these programmes on the incidence of IPV show mixed results. Some report a reduction in IPV, and others document an increase or no change (see, e.g., Buller et al., 2018; Dalal, Dahlström and Timpka, 2013; Williams et al., 2025; Das et al., 2025). However, little is known about whether these impacts persist in the long run (Angelucci and Heath, 2020). To address this gap, our study explores whether women's economic empowerment through one of these development programmes affects IPV in both the short and long run.

A widely used approach to promote women's economic empowerment is the ultra-poor graduation programme. It aims to reduce poverty by enhancing women's economic empowerment. To compare the short- and long-run effects of such a programme, we evaluate the impacts of BRAC's credit-based graduation programme on IPV one and seven years after the programme ended. The credit-based graduation programme targeted women from ultra-poor households to empower them and to lift their households out of poverty. The support package of this programme included (i) a loan conditional on purchasing productive assets (mostly livestock), (ii) free bi-weekly enterprise training on these productive assets, (iii) a weekly consumption stipend, and (iv) guidance on social issues and physical health. We evaluate this programme using a randomised controlled trial (RCT) with endline surveys at one- and seven-year in rural Bangladesh.

Our results can be summarised as follows. First, we find that women in the treatment group are less likely to experience certain forms of physical and emotional IPV compared to the control group one year after the completion of the programme, but these differences reverse seven years later. In the long run, women in the treatment group are more likely to experience physical and emotional IPV compared to women in the control group. While IPV declines in both control and treatment groups between two endlines, the treatment group experiences a slower rate of decline compared to the control group, causing women in the treatment group to fall behind their counterparts in the control group.

Second, we find that in the short run, both women in the treatment group and their husbands earn significantly more than those in the control group. Women in the treatment group also own more livestock than those in the control group during the same period. In the long run, the income difference between women in the treatment and control groups disappears, while the husbands of women in the treatment group maintain a higher income compared to their counterparts in the control group. Women in the treatment group continue having more livestock than women in the control group in the long run.

Third, between both endlines, the trajectory of economic conditions of women differs between treatment and control groups. Women in the control group experience an increase in income and livestock ownership between both endlines, while these outcomes stagnate for women in the treatment group. The convergence in economic conditions over time between the treatment and control groups might be linked to differences in credit market participation between the two groups. In both the short and long run, the treatment group is more likely to participate in other credit programmes, while the treatment-control difference in credit market participation declines sharply over time.

Our study is closely related to the growing literature on the impact of development programmes that support women's economic empowerment in order to reduce IPV. This literature covers three types of development programmes: cash transfers, microfinance and graduation programmes. A systematic review (Buller et al., 2018) finds that most cash transfer programmes achieve IPV reduction; however, these are short-run evaluations. One of these studies investigates the long-run impacts in Mexico. Using a pseudo-panel of comparable groups of beneficiary and non-beneficiary couples from three nationally representative surveys, they find that the impact of a conditional cash transfer programme on the incidence of IPV disappeared between nine and thirteen years after the programme ended (Bobonis, Castro and Morales, 2015).[2] Similarly, the evidence on the impact of microfinance on the incidence of IPV is mixed. Some studies document a positive relationship (Dalal, Dahlström and Timpka, 2013), others find a negative relationship, and many report no significant impact (Allan-Blitz, Olson and Tran, 2023; Williams et al., 2025). Most studies estimated impacts on IPV within two years of programme completion.[3]

Compared to the literature on the impacts of cash transfers and microfinance on the incidence of IPV, evidence on the impacts of graduation programmes, which combine asset transfers, training, and support, on the incidence of IPV is limited.[4] The first contribution of our paper is to provide evidence on the time-variant trajectory of the impact of a credit-based graduation programme on the incidence of IPV by estimating the impact one and seven years after the completion of the programme. This evidence will further help explain whether the timing of the evaluation accounts for the mixed impacts of such development programmes on IPV in prior evaluation studies.

[2]Several studies have investigated the impacts of cash transfer programmes targeting women on economic and health outcomes across different contexts (see, e.g., Haushofer and Shapiro, 2016; Gobin, Santos and Toth, 2017; Fafchamps et al., 2011; Bouguen and Dillon, 2024; Nawaz and Iqbal, 2021; Blattman et al., 2016; Kipchumba et al., 2024; Bossuroy et al., 2022; Sabates and Devereux, 2015; Burchi and Strupat, 2018). These studies have shown that cash transfer programmes for women can have positive impacts, including poverty reduction, an increase in consumption, positive human capital investment, asset accumulation, improved access to health services and enhanced women's decision-making power.

[3]The literature on the impacts of microfinance on economic outcomes and women's empowerment documents mixed evidence across different countries. Some studies find that its impacts on income, health outcomes, education and women's empowerment are also not significant (Banerjee, Karlan and Zinman, 2015; Banerjee et al., 2015*b*), while other studies find significant impacts on income, consumption and women's empowerment (Khandker, 1998; Pitt, Khandker and Cartwright, 2003; Shah and Butt, 2011; Al-shami et al., 2021).

[4]It is worth noting that several studies assessed the impacts of graduation programmes on economic outcomes. Longitudinal evidence suggests that such programmes can produce sustained economic benefits. Several studies, such as Bandiera et al. (2017), Banerjee, Duflo and Sharma (2021), Blattman, Fiala and Martinez (2020), found that the impact on economic outcomes (i.e., earnings, assets) persisted about nine to ten years after programme completion in different contexts.

Another strand of literature suggests that the influence of women's economic empowerment on the incidence of IPV is mixed. Some studies find that improvements in women's economic conditions reduce the likelihood of IPV (Koenig et al., 2003; Panda and Agarwal, 2005; Aizer, 2010), while others document backlash effects where women's economic independence triggers IPV (Vyas and Watts, 2009; Heath, 2014). The second contribution of our paper is to expand this literature by exploring whether a credit-based graduation programme affects both IPV and the economic conditions of women, providing suggestive evidence on potential mechanisms through which the programme might influence the incidence of IPV.

The rest of the paper is structured as follows. Section 2.2 describes the research design, including the intervention, sample, data, and identification strategy. Section 2.3 reports the balancing test, estimates of the programme's impact on the incidence of IPV and robustness checks. Section 2.4 examines potential mechanisms, and Section 2.5 concludes the paper.

2.2 Research Design

This section describes our research design. We begin with an overview of the credit-based graduation programme. Next, we present randomisation, sample and survey timeline, followed by measures of outcomes and other indicators. Finally, we present our identification strategy to estimate the short- and long-run impacts of the programme.

2.2.1 The Intervention

BRAC's Ultra-Poor Graduation (UPG) programme, which originated in Bangladesh in 2002, aims to reduce poverty by targeting women from ultra-poor households. The UPG programme provides ultra-poor women with livelihood support, enterprise training and consumption allowances to engage them in income-generating activities and support their households to come out of poverty. The intervention has been replicated worldwide in partnership with governments, NGOs and multilateral institutions across different contexts in more than 50 countries (Banerjee et al., 2015*a*), reaching more than 14 million people from 3.1 million ultra-poor households in these countries as of 2024.[5] Since its inception, the programme has undergone several context-specific iterations, but its core objective has not changed.

Our research focuses on BRAC's *credit-based* graduation programme. In 2007, BRAC started implementing this credit-based model to target the marginally better-off ultra-poor households.[6] The programme followed a rigorous multi-stage process to identify the

[5] https://www.brac.net/program/ultra-poor-graduation/

[6] They are referred to as marginally better-off ultra-poor to distinguish them from a more vulnerable group of beneficiaries who had fewer productive assets and less land ownership. Notably, the more vulnerable group received a different support package.

beneficiary households (Bandiera et al., 2017; Rahman, Bhattacharjee and Das, 2021). The programme staff selected the poorest villages, and in selected villages, they conducted a participatory wealth ranking (PWR) exercise, where the households of a community are ranked into several wealth groups (very poor, poor, middle-class, non-poor). Then, the staff conducted a short survey with the main female members of the households from the bottom three wealth ranks to gather information on selection criteria.

Using this survey, the staff assessed the eligibility of these households. To be eligible for the programme support, the households had to meet at least three out of five selection criteria, including income level, land ownership, child labour status and food security (see details in Appendix, Section 2.A). The households that met three out of these selection criteria and received approval by senior-level BRAC staff were finally selected. The main female members from the selected households were offered the loan (conditional on purchasing productive assets) and other grant support, including enterprise development training, supplementary assets, coaching, consumption allowance, guidance on social and health issues, savings advice and community resource mobilisation (see details in Appendix, Section 2.B).

2.2.2 Data

To estimate the impact of the programme, we use an RCT, conducted by the BRAC Institute of Governance and Development (BIGD), BRAC University.[7] Randomisation was done at the branch office level, stratified by districts.[8] The evaluation covers 88 branch offices (66 treatment and 22 control branches) in 11 districts of Bangladesh. From each district, we have six treatment and two control branches. After the selection of eligible households following a rigorous process described in Section 2.2.1, a baseline survey was conducted in April-July 2016, covering 8,973 eligible women (7,042 treatment and 1,931 control) (Figure 2.1). Thereafter, BRAC implemented the intervention in the treatment branches until December 2017.

The first endline survey (hereafter referred to as 'endline 1') was conducted in November 2018-January 2019, a year after the completion of the programme, in which all women in the control group (1,931) and a random subsample of two-thirds of the women in the treatment group were re-interviewed (4,666). Out of them, we could include 2,741 women (1,945 treatment and 796 control) in our analysis. To examine the long-term impact of the programme, we conducted another survey in June-September 2024, seven years after the completion of the programme. In this survey (referred to as 'endline 2'), we revisited all women who were interviewed at the baseline. Among them, we could use 7,079 women (5,549 treatment and 1,530 control) for our analysis for this round.

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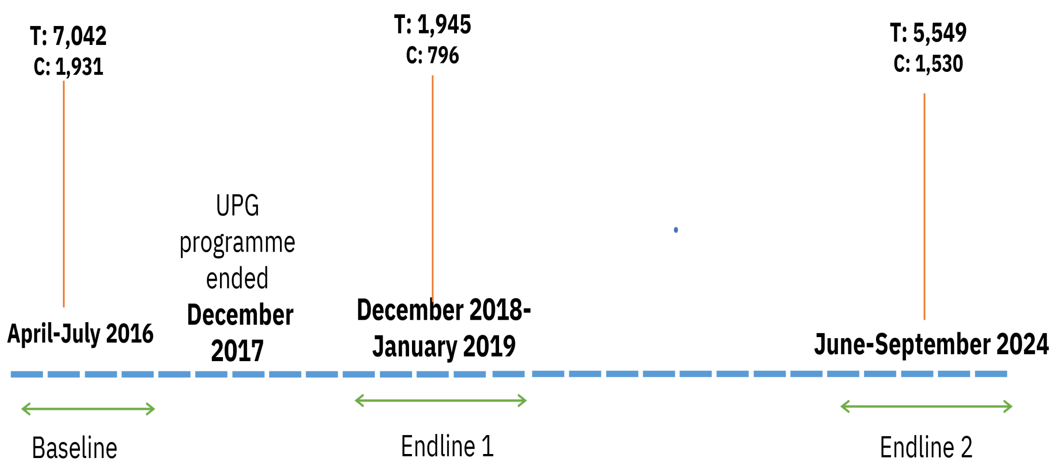
[7] A team from the 'Economic Cluster' of BRAC's Research and Evaluation Division (RED) designed this study and conducted the baseline survey in 2016. In 2019, this cluster was merged with BRAC Institute of Governance and Development (BIGD), and the project's subsequent activities were carried out under BIGD (Rahman, Bhattacharjee and Das, 2021).

[8] Each branch serves an area within approximately a 5-kilometre radius.

Several factors reduced our sample size in both endlines, including death of respondents, untraceable households, absence of respondents, random exclusion from the sample, marital status (unmarried/separated/widow/not living with husband) and being interviewed by male enumerators. Panel B in column 1 of Table 2.C.3 shows the distribution of these reasons by both endlines. The first reason for sample exclusion from endline 1 is random sub-sampling. We randomly selected two-thirds of women in the treatment group for endline 1 due to budget constraints, leading to the exclusion of the remaining treated households from the analysis.

The second reason is marital status. As we asked questions from the IPV module only to women who were married at the time of the interview, women who were not married are excluded from the analysis. The third reason is the gender of the enumerator. In endline 1, a random half of the sample was interviewed by female enumerators, and the other half by male enumerators. This was done to test for reporting bias in IPV responses. As a comparison between responses collected by male and female enumerators confirmed that IPV is under-reported if the data is collected by male enumerators (Das et al., 2025), we exclude IPV data collected by male enumerators from our analysis. Additional reasons for attrition include respondents being replaced due to death, absence of respondents, untraceable households, etc. This results in a final sample of 2,741 observations for endline 1. Similarly, for endline 2, we could use 7,079 women for our analysis because of the condition of being married for the IPV module, death and other attrition reasons.

Figure 2.1 Sample size across survey rounds



Notes: Table 2.C.1 presents disaggregated statistics by treatment and control groups, and Table 2.C.3 shows the distribution of inclusion and exclusion reasons for analysis in each round of the endline survey.

Our primary outcomes measure whether women experience different forms of physical and emotional IPV.[9] While we did not collect data on the incidence of IPV at baseline,

[9]Our primary outcomes are physical and emotional IPV, which are common forms of IPV in Bangladesh. A national survey conducted in 2024 reports that 29% of ever-married women reported experiencing any sexual IPV in their lifetime, while 47% and 33% reported experiencing physical abuse and emotional abuse, respectively (BBS, 2025).

in endline 1 and endline 2 we collected detailed information on women's experiences of different forms of IPV in the 12 months preceding the survey.[10] We interviewed only the main female member of the sample households in all rounds of the survey (i.e., baseline, endline 1 and endline 2).[11] In addition to IPV experiences, the surveys collected detailed information from these women about the demographic and socioeconomic characteristics of the household members, including the composition of the household, occupation, the labour supply and income of each member, productive assets and financial market participation. The gender of enumerators collecting data on these variables varied across survey rounds (Table 2.C.2). At baseline and endline 1, other variables were collected by the male enumerators for the entire sample, even in cases where female enumerators collected data on the incidence of IPV (in endline 1).[12] In contrast, in endline 2, both IPV and other variables were collected by female enumerators for the entire sample.

2.2.3 Identification Strategy

To develop our hypothesis on the impact of a credit-based graduation programme on the incidence of IPV, we draw on the mixed empirical evidence on the relationship between women's economic empowerment and IPV. Women's economic empowerment could reduce IPV by increasing women's bargaining power (Koenig et al., 2003; Panda and Agarwal, 2005; Aizer, 2010). Alternatively, it could increase IPV if husbands use violence to reassert control when threatened by women's growing economic independence (Vyas and Watts, 2009; Heath, 2014). Given that the graduation programme's economic benefits appear to persist, but the mechanisms linking women's economic empowerment to IPV might go in the opposite direction.[13] The long-run effects of the programme of our interest, the credit-based graduation programme, on IPV can go in either direction: (1) a sustained reduction in IPV, if the bargaining power mechanism works, or (2) no effect or an increase in IPV, if backlash effects emerge over time. Therefore, we hypothesise that the programme influences the incidence of IPV in the long run, without specifying the direction of change. To test this hypothesis, we estimate the average treatment effects on the incidence of IPV

[10] In our pre-analysis plan, we registered three levels of empowerment outcomes: individual, household and community levels (see `https://www.socialscienceregistry.org/trials/13845`. In this paper, we only focus on IPV.

[11] As the graduation programme aimed to reduce poverty through the main female member of the ultra-poor households, we selected the main female members as our respondents.

[12] Half of the random sample was interviewed by the female enumerators to collect data on the incidence of IPV in endline 1. For our endline 1 analysis, we will only use those observations which were interviewed by the female enumerators.

[13] Bandiera et al. (2017) find that a different model of graduation programme had positive impacts on women's income and asset accumulation, which persisted seven years after the programme completion in Bangladesh. They evaluated a grant-based model providing livelihood support as grants rather than credit, while other components of the graduation approach (training, consumption allowance, savings advice, guidance on health and social issues and forming a committee to mobilise resources for ultra-poor households through the community) remain the same for both grant- and credit-based graduation programmes.

and the mediating factors in the short (long) run, using the following regression.

$$z_{ibd} = \beta_0 + T_{bd}.\beta_1 + \chi_{ibd}.\Theta + \gamma_d + \epsilon_{ibd} \tag{2.1}$$

where z_{ibd} is the set of outcomes of interest, including IPV experiences in the last 12 months, women's income, husbands' income, women's credit market participation and women's livestock ownership. We use these outcomes as measured by endline 1 and endline 2 for woman i in branch b in district d to estimate short-run and long-run impacts, respectively. T_{bd} is an indicator variable taking the value one if the branch b was assigned to treatment and zero otherwise. χ_{ibd} is a vector of covariates (i.e., respondents' age, household income, financial assets, livestock ownership, land ownership and food security at baseline). γ_d are district fixed effects; ϵ_{ied} is the error term. We cluster standard errors at the branch level to deal with non-independence within each branch. β_1 is the treatment effect in the short (long) term.

To estimate the difference between short- and long-run impacts, we use a difference-in-difference estimator, using the following equation:

$$z_{ibd} = \beta_0 + Y_t.\beta_1 + T_{bd}.\beta_2 + T_{bd}.Y_t.\beta_3 + \chi_{ibd}.\Theta + \gamma_d + \epsilon_{ibd} \tag{2.2}$$

where z_{ibd} is the same set of outcomes and Y_t is a binary variable equal to one for endline 2 and zero for endline 1. T_{bd} is an indicator variable, which is equal to one if the branch b was assigned to the treatment and zero otherwise. χ_{ibd} is a vector of controls, which includes respondents' age, household income, financial assets, livestock ownership, land ownership and food security at baseline. γ_d are district fixed effects; ϵ_{ied} is the error term. We cluster standard errors at the branch level to deal with non-independence within each branch. β_3 estimates the average difference between short- and long-run treatment effects.

2.3 Results

2.3.1 Randomisation checks

Random assignment of treatment in an RCT is essential to identify the impact of the program. If the assignment correlates with other factors, post-intervention differences between treatment and control may be influenced by these factors rather than reflecting the program's causal effect. To test whether we can assume that assignment to treatment was random, we conduct multiple balance tests. For each set of balance tests, we report the mean of the treatment and control groups. We estimate the difference between these groups using district fixed effects to control for unobservable characteristics of districts.

First, we test whether the treatment and control groups were similar at baseline. As shown in Table 2.C.4 , there are no significant differences in terms of respondents' (wives) demographic characteristics and their households' economic characteristics. One exception is cultivable land ownership, for which the difference is significant. However, the joint orthogonality test (p-value = 0.53) confirms that baseline characteristics do not jointly predict treatment assignment.[14]

Second, as mentioned earlier, two-thirds of the treatment group were randomly selected for the endline 1 survey. To check the validity of this random selection, we compare their baseline characteristics with those of the remaining one-third of women in the treatment group. Table 2.C.5 shows no significant differences for most of the indicators, with two exceptions where the differences are significant. However, the joint significance test is marginally significant, supporting the randomness of selection for endline 1.

Third, we compare the baseline characteristics of the selected treatment group for endline 1 with those of the control group. Table 2.C.6 confirms that these groups were similar at baseline. Fourth, we test whether baseline characteristics are balanced within the samples used for our analysis. We do this separately for endlines 1 and 2. Tables 2.C.7 and 2.C.8 show that treatment and control groups remain balanced for most baseline characteristics. The ones that are not balanced are included as controls in the regressions.

2.3.2 Impacts on the incidence of IPV

In this section, we estimate the programme's impacts on different forms of physical and emotional IPV. The results are reported in Tables 2.1 and 2.2 . Panels A and B of each table estimate the impact at one and seven years after the completion of the programme, respectively, while Panel C estimates the difference in the short- and long-run impacts.

The results show that women in the treatment group are less likely to experience only one out of four forms of physical IPV compared to women in the control group in the short run. Particularly, the treatment group is 4 percentage points less likely to experience physical abuse for dowry which is 50% lower than the average prevalence in the control group (column 4 in Panel A of Table 2.1).[15] In contrast, in the long run, the results reverse. Women in the treatment group are about 1-4 pp more likely to experience several forms of physical IPV, including physical hurt, threatened with hurt, slapped and abuse for dowry ('Treatment' coefficients in Panel B of Table 2.1), which is 18%-38% relative to the control mean. Column 5 in Panel B reports that women in the treatment group are 5 pp more likely to experience at least one form of IPV, which is 22% relative to the control mean of 0.22.

[14] A joint orthogonality test obtained by regressing all pre-intervention characteristics on the treatment variable can be an alternative to conducting a t-test for each characteristic (McKenzie, 2015). The former approach is better than the latter one because with the latter approach, the difference between the treatment and control groups might be significant for some baseline characteristics by chance, while the former one assesses whether all selected baseline characteristics can jointly influence the likelihood of being treated.

[15] Dowry-related abuse refers to violence perpetrated when a husband or in-laws demand additional payments from the wife's family beyond the initial dowry. This is a common form of IPV in South Asia (Ambrus, Field and Torero, 2010). Husband and in-laws put the wife under pressure through using violence as a means of forcing her parental family to provide additional dowry.

'Year' coefficients in Panel C show a significant reduction in all forms of physical IPV between the endlines 1 and 2 in both control and treatment groups. Between endlines 1 and 2, the experience of any form of physical IPV reduces by 29 pp in the control, which is 56% of the mean of the control group at endline 1. Different forms of physical IPV significantly reduced by 18-20 pp in the control group. One exception is the prevalence of abuse for dowry, which did not reduce significantly in the control group between the two endlines. However, the difference between treatment and control is marginally significant in the long run for this indicator, whereas it is highly significant for other forms of physical IPV (see 'Treatment' coefficients in Panel B). This pattern suggests that the treatment-control difference is large for those forms of violence that have declined drastically between the two endlines. Moreover, taking the sum of coefficients of 'Year' and 'Treatment X Year', we find that women in the treatment group experienced slower reductions in IPV over time than the control group. However, the interaction term ('Treatment X Year') is not significant, indicating that the treatment-control difference in physical IPV prevalence does not differ between the two endlines.

Table 2.1 Impacts on physical IPV

	(1) Physically hurt	(2) Threaten with hurt	(3) Slap	(4) Abuse for dowry	(5) Any physical IPV
Panel (A): Endline 1					
Treatment (Yes/No)	-0.0237	-0.0270	-0.0249	-0.0422**	-0.0327
	(0.037)	(0.035)	(0.041)	(0.021)	(0.040)
Observations	2741	2741	2741	2733	2741
Mean of control group at endline 1	0.279	0.294	0.391	0.0830	0.514
Panel (B): Endline 2					
Treatment (Yes/No)	0.0307***	0.0365***	0.0323**	0.00940*	0.0488***
	(0.009)	(0.013)	(0.015)	(0.005)	(0.014)
Observations	7069	7071	7069	7063	7077
Mean of control group at endline 2	0.0785	0.111	0.161	0.0366	0.224
Panel (C): Endline 1 vs Endline 2					
Year (Endline 2=1 Endline 1=0)	-0.200***	-0.183***	-0.231***	-0.0455	-0.291***
	(0.045)	(0.043)	(0.046)	(0.030)	(0.042)
Treatment (Yes/No)	-0.0245	-0.0211	-0.0300	-0.0351	-0.0271
	(0.044)	(0.046)	(0.048)	(0.029)	(0.044)
Treatment X Year	0.0550	0.0555	0.0635	0.0416	0.0736
	(0.052)	(0.055)	(0.052)	(0.032)	(0.052)
Observations	9810	9812	9810	9796	9818

Notes: Ordinary least squares (OLS) regressions with different types of physical abuse (columns 1-4), and each type takes the value of one if the respondent experienced that form of physical IPV in the last 12 months prior to the survey and zero otherwise. Any physical IPV (column 5) equals to one if the woman experienced any form of physical IPV and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as the respondent's age, the respondent's income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects, and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.2 reports the treatment effects on the incidence of emotional IPV. In the short run ('Treatment' coefficients in Panel A), women in the treatment group are less likely to experience two out of three forms of emotional IPV, intimidation and mental torture, by 11 and 8 pp, respectively, compared to the control group, which are about 30% and 39% lower relative to the means of the control group. In the long run ('Treatment' coefficients in Panel B), women in the treatment group are more likely to experience two forms of emotional IPV, insult and mental torture, by 4 and 3 pp, which are 18% and 35% relative to the means of the control group. However, the proportion of experiencing at least one form of emotional IPV does not significantly differ between treatment and control.

Looking at the change in IPV between endlines, we observe that the control group experienced a significant reduction in two out of three forms of emotional IPV between endlines by 11-35 pp ('Year' coefficients in Panel C). Experiencing at least one form of IPV reduced by 28 pp between endlines in the control group, which is 33% of the prevalence among the control group at endline 1. Moreover, the short- and long-run treatment effects do not differ, with the non-significant interaction term ('Treatment X Year') across all forms of emotional IPV, except mental torture. For mental torture, the long-run treatment effect is marginally larger than the short-run treatment effect by 10 pp.

Table 2.2 Impacts on emotional IPV

	(1) Insult	(2) Intimidate	(3) Mental torture	(4) Any emotional abuse
Panel (A): Endline 1				
Treatment (Yes/No)	0.0415	-0.113***	-0.0782**	0.00719
	(0.059)	(0.041)	(0.039)	(0.051)
Observations	2741	2740	2736	2741
Mean of control group at endline 1	0.480	0.370	0.201	0.592
Panel (B): Endline 2				
Treatment (Yes/No)	0.0390**	0.00473	0.0294**	0.0158
	(0.018)	(0.018)	(0.012)	(0.020)
Observations	7066	7067	7059	7075
Mean of control group at endline 2	0.213	0.352	0.0832	0.473
Panel (C): Endline 1 vs Endline 2				
Year (Endline 2=1 Endline 1=0)	-0.267***	-0.0194	-0.119**	-0.119**
	(0.054)	(0.051)	(0.050)	(0.054)
Treatment (Yes/No)	0.0375	-0.101*	-0.0724	0.0134
	(0.064)	(0.053)	(0.048)	(0.059)
Treatment X Year	0.00313	0.1000	0.0984*	-0.000420
	(0.066)	(0.063)	(0.054)	(0.065)
Observations	9807	9807	9795	9816

Notes: Ordinary least squares (OLS) with different types of emotional abuse (columns 1-3), and each type takes the value of one if the respondent experienced that form of emotional IPV in the last 12 months prior to the survey and zero otherwise. Any emotional IPV (column 4) equals to one if the woman experienced any form of emotional IPV and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as the respondent's age, the respondent's income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects, and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note that in both Tables 2.1 and 2.2 , we observe a very strong decline in IPV between endlines 1 and 2, which is substantially larger than the long-run treatment-control differences for both physical and emotional IPV. This raises concerns about whether the relatively small treatment-control differences are meaningful given the large general decline in IPV. We investigate potential explanations for this drastic reduction in Section 2.D in the appendix. We argue that wives' ageing over the six years between endlines 1 and 2 partly explains the decline, consistent with prior studies showing that older women are less likely to experience IPV than younger women. Our disaggregated analysis by the wives' age at baseline reveals an important pattern. We find that the treatment effect on IPV is strongest among those groups of women who experienced the largest decline in IPV between both endlines. Specifically, women aged 31-38 years in the control group experienced the largest decline in IPV between the two endlines, while women in this same age group in the treatment group reported significantly higher physical IPV than the control group in the long run. And, this is the largest and most significant treatment-control gap across all age groups. This demonstrates that women in the treatment group benefited less from the age-related reduction in IPV than women in the control group, indicating the programme slowed down the decline in IPV among women in the treatment group in the long run. Furthermore, the literature on IPV trends in Bangladesh suggests that national-level initiatives (such as awareness campaigns and providing legal support), economic growth, and changing social norms might also have contributed to this reduction. Other factors are also likely to drive this general decline that we cannot fully identify within our study.

2.3.3 Robustness checks

In this section, we conduct robustness tests for our main findings. We test whether using a balanced sample of both endlines affects our results due to the smaller sample and examine whether post-intervention exposure to other credit programmes confounds our estimates. We also estimate local average treatment effects (LATE), accounting for partial compliance in the treatment group and contamination in the control group.

Balanced sample

In the results presented so far, we used an unbalanced sample for each round of endline, meaning that for a given round of the endline survey, we used those women who were included in the respective round's analysis but were not necessarily successfully interviewed in the other round of the endline survey. As mentioned in Section 2.2.2, we have a smaller number of observations for endline 1's analysis due to several reasons, resulting in a smaller number of observations if we create a sample with only those women who are included in the analysis of both endlines. In this section, we examine the treatment effects using the balanced sample to test whether our estimated treatment effects are large enough to be detected with a smaller sample in the long run. Tables 2.E.1 and 2.E.2 report the results. We observe that the direction of the treatment-control difference in both physical and emotional IPV in the long run is similar to what we have found using the unbalanced sample in Section 2.3.2, but we lose significance for most forms of IPV in endline 2, perhaps due to reduced sample size and consequent loss of statistical power.

Controlling for participation in other credit programmes

Given that credit is a core component of the credit-based graduation programme, differential post-intervention exposure to other credit programmes between the treatment and control groups could bias our observed treatment effects. If the control group is more likely to participate in such programmes compared to the treatment group during endlines, our estimates would be underestimated. In contrast, if the treatment group has more access to such credits, our estimates would overstate the effects of the credit-based graduation programme, as they would include the effects of additional credit resources. Section 2.E.2 documents credit trajectories of treatment and control groups from baseline to endline 2.[16] Later, in Section 2.4.3, we show whether there is a difference in participation in other credit programme between the treatment and control groups. Since both groups could access other credit programmes, we control for such participation to distinguish the impacts of the graduation programme from exposure to other credit programmes. Table 2.E.3 and 2.E.4 report the results, which remain qualitatively similar to our main findings.

Local average treatment effects (LATE) estimates

In endline 1, we found that about 23% of women in the treatment group did not participate in the programme, and none of the women in the control group received any support from the UPG programme. In endline 2, we found that about 11% of women in the control group received support from the UPG programme.[18] This imperfect compliance in the treatment group and contamination in the control group could bias the impact estimates. The observed positive difference in incidence of IPV between women in the treatment and control groups could have been larger in the long run if there was no such bias. To mitigate this bias, we use the instrumental variable (IV) approach to estimate local average treatment effects (LATE) on our primary outcomes, the incidence of IPV. We use treatment assignment as an IV for actual participation in the programme (see the estimating equations in Section 2.E.3 in the appendix). This allows us to estimate the effect for those who are actually affected by the programme's intervention. Tables 2.E.5 and 2.E.6 present results that are qualitatively similar to our main findings.

2.4 Mechanisms

Our main results show that women in the treatment group experience a lower incidence of IPV compared to women in the control group, but this reverses in the long run. This section examines potential intermediate outcomes that could explain the treatment-control difference in the incidence of IPV. Since the programme provides economic support to improve the economic conditions of women and households, differences in the economic conditions of spouses between the treatment and control groups might mediate the impact of the programme on the incidence of IPV.

[16]In rural Bangladesh, many local and international NGOs, including BRAC, have been offering microfinance services to underprivileged individuals.[17] From the poorest households, these programmes mainly target women as their primary clients. However, loans are only provided if the staff is confident enough, after assessing the clients' economic conditions, that the clients would be able to repay the loans. Although these programmes target low-income individuals, the selection criteria and loan sizes vary across schemes. As a result, our sample women face challenges accessing microfinance, and even when they do, they often struggle to translate credit into improved livelihoods. On the contrary, along with credit, the credit-based graduation programme provides additional grant support in the form of enterprise training, consumption allowance, and input assets that are not offered by other credit programmes.

[18]Five years after the programme completion, the control group was allowed to receive the support from the UPG programme if they were still eligible.

Building on the existing literature on women's economic empowerment and IPV documented in Section 2.2.3, we assume that women's economic empowerment could influence IPV. Women's income has a complex relationship with IPV. For example, improved economic conditions of women might reduce IPV by increasing their bargaining power (Koenig et al., 2003; Panda and Agarwal, 2005; Aizer, 2010). Conversely, improved economic conditions might threaten husbands' authority over the household, resulting in increased controlling behaviour of husbands and IPV (Vyas and Watts, 2009; Heath, 2014). Similarly, changes in husbands' income could affect IPV in two pathways. Higher income might increase men's dominance and control over their wives, or it might reduce household economic stress, thereby decreasing IPV (Bhalotra et al., 2021).

To test these mechanisms, we estimate treatment effects on mediating factors: women's and their husbands' income, and women's livestock ownership. We will interpret these mechanisms drawing on two theories of IPV, 'instrumental' (Macmillan and Gartner, 1999; Thoits, 1992; Haushofer et al., 2019; Chin, 2012), which emphasises strategic use of violence to maintain control, and 'expressive' (Dollard et al., 1939; Berkowitz, 1989), which links violence to stress and frustration.

First, we will test whether treatment effects on women's income explain this reversal in the treatment effects on IPV in the long run. The treatment effect on women's income can explain this pattern regardless of direction. If the programme increases women's income relative to the control group in the long run, this explains the positive treatment effect on the incidence of IPV through male backlash following instrumental theory. Husbands may use violence to reassert control when their wives' earnings threaten their authority as primary earners. Conversely, if the programme's initial effect on women's income fades such that treated women earn the same or less than the control group in the long run, this predicts increased IPV through frustration following expressive theory. Husbands may experience frustration when their wives' earnings fall short of expectations created by programme participation, or they may perceive their wives as economically unsuccessful and less empowered. We will also estimate the treatment effect on husbands' income, which may also influence IPV. If the programme generates positive spillovers on husbands' income (for example, through reallocation of household labour or resources) while wives do not gain economically from participating in the programme, this might weaken women's position in the household, resulting in IPV. Alternatively, if husbands' income remains stagnant while their wives participate in income-generating activities, this divergence from traditional gender roles may provoke backlash, or husbands may perceive a loss of breadwinner status.

Second, we will examine the treatment effect on women's livestock ownership. If the programme increases women's livestock ownership relative to the control group, husbands may use violence to gain control over these assets (instrumental theory). If treatment effects on assets diminish such that treated women own fewer or similar assets as the control group, this may signal economic failure or disempowerment, potentially triggering violence (expressive theory). Third, we will examine participation in other credit programmes across the treatment and control groups at both endlines to explain the programme's impacts on income and livestock ownership, as credit market participation might be one of the mechanisms which influence these economic outcomes.[19]

2.4.1 Women and their husbands' income

Table 2.3 reports the treatment effect on spouses' income. In the short run ('Treatment' coefficients in Panel A), women in the treatment group earn more compared to the control group, which is almost double the average income of control wives (column 1). Husbands of women in the treatment group also earn more than husbands of women in the control group; however, the treatment-control difference is only 12% relative to the control group (column 2). This suggests that economic gain for women is larger than that for their husbands, one year after the programme completion. Despite this larger economic gain for women, we still observed reductions in several forms of physical and emotional IPV in the treatment group compared to the control group, suggesting that there is no backlash effect from women's rising income. Rather, the increasing income might reduce their IPV exposure by improving their bargaining power.

In the long run ('Treatment' coefficients in Panel B), the treatment-control difference in women's income becomes statistically insignificant, though the treatment group maintains a marginally higher income. Husbands of women in the treatment group continue to earn more than husbands of women in the control group; however, the treatment-control difference decreases from 12% in the short run to 8% in the long run relative to the mean of the control group. Furthermore, from Panel (C), we observe contrasting income trajectories between the two groups. The income of women in the control group experiences a significant increase between endlines 1 and 2 (50%) ('Year' coefficients in Panel C), while the income of women in the treatment group becomes stagnant between both endlines (indicated by the sum of 'Year and 'Treatment X Year' coefficients). This substantial increase in income among women in the control group corresponds with their larger IPV reduction, while the income stagnation among women in the treatment group corresponds with their slower IPV reduction rate compared to the control group. These patterns support the hypothesis that women's rising income drives a reduction in IPV.

The disappearance of treatment effects on women's income in the long run might have increased IPV in two ways. First, women's initially higher earnings in the short run might have raised their husbands' expectations of their economic contributions. When women's economic gain disappeared in the long run, their husbands might have responded with frustration and violence, consistent with the expressive theory of IPV (Dollard et al., 1939; Berkowitz, 1989) and empirical evidence showing that frustration arising from unexpected

[19]As mentioned earlier in Section 2.2.2, the gender of enumerators who collected data on the mediating factors was not the same for both endlines. The details on its plausible effects on our estimation of the impacts on these mediating factors are provided in Section 2.F in the Appendix.

events might trigger IPV (Card and Dahl, 2011; Santaularia et al., 2022). Second, women's income stagnation in contrast to their husbands' sustained income gain might have led husbands to perceive their wives as economic failures, triggering an increase in IPV.

Table 2.3 Impacts on income

	(1) Women's income	(2) Husband's income
Panel (A): Endline 1		
Treatment (Yes/No)	13955.7***	13724.2***
	(2381.580)	(4989.055)
Observations	2741	2735
Mean of control group at endline 1	11447.4	117217.4
Panel (B): Endline 2		
Treatment (Yes/No)	1092.6	8884.6**
	(1738.240)	(4462.761)
Observations	7079	7079
Mean of control group at endline 2	22121.0	114342.3
Panel (C): Endline 1 vs Endline 2		
Year (Endline 2=1 Endline 1=0)	10456.3***	-3096.8
	(2329.692)	(7036.230)
Treatment (Yes/No)	12902.2***	12927.9*
	(2822.515)	(7174.508)
Treatment X Year	-11479.9***	-3622.0
	(3458.578)	(8185.187)
Observations	9820	9814

Notes: Ordinary least squares (OLS) with the following outcomes: Income measured by the annual income in Bangladeshi currency (BDT) at 2024's constant prices, with endline 1's incomes being inflated using the general consumer price index. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as respondents' age and income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects, and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.4.2 Livestock ownership

The second mediator is women's livestock ownership. Table 2.4 reports the estimated treatment effects on women's livestock ownership. In the short run ('Treatment' coefficients in Panel A), women in the treatment group own substantially more livestock than the control group. Ownership of cows exceeds double the control mean, while goat and poultry ownership are 38% and 54% higher relative to the control mean. These patterns explain the short-run reduction in IPV among the treatment group compared to the control group, perhaps by strengthening women's economic position within households.

The long-run effects ('Treatment' coefficients in Panel B) show declines in treatment effects. The treatment-control difference in women's ownership of cows and poultry falls to 54% and 20% relative to the control mean, while the difference in goat ownership disappears completely. Furthermore, 'Year' coefficients in Panel C shows that livestock ownership of women in the control group (goat and poultry) increases between the endlines, 82% for the number of goat and 19% for the number of poultry, whereas livestock ownership of women in the treatment group stagnates (indicated by the sum of 'Year and 'Treatment X Year' coefficients). These patterns align with the patterns of women's income documented in Section 2.4.1 and reinforce the hypothesis that rising economic outcomes (livestock ownership and income) for women in the control group might significantly reduce IPV between the two endlines, while stagnation in economic outcomes for women in the treatment group between the two endlines might trigger their husbands' frustrations, resulting in a lower reduction in the incidence of IPV compared to the control group.

Table 2.4 Impacts on number of livestock owned by the women

	(1) Cow	(2) Goat	(3) Poultry
Panel (A): Endline 1			
Treatment (Yes/No)	0.318***	0.105**	2.235***
	(0.046)	(0.049)	(0.447)
Observations	2741	2741	2741
Mean of control group at endline 1	0.170	0.276	4.167
Panel (B): Endline 2			
Treatment (Yes/No)	0.152***	-0.00965	1.207***
	(0.032)	(0.058)	(0.304)
Observations	7079	7079	7079
Mean of control group at endline 2	0.251	0.514	4.918
Panel (C): Endline 1 vs Endline 2			
Year (Endline 2=1 Endline 1=0)	0.0737	0.227***	0.789***
	(0.049)	(0.073)	(0.243)
Treatment (Yes/No)	0.306***	0.0858	2.266***
	(0.060)	(0.080)	(0.461)
Treatment X Year	-0.150**	-0.0880	-1.071***
	(0.066)	(0.087)	(0.374)
Observations	9820	9820	9820

Notes: Ordinary least squares (OLS) with the following outcomes: the number of cow/goat/poultry owned by the wife (solely or jointly with husband and/or family members). Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as respondents' age and income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects, and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.4.3 Participation in other credit programmes

We further explore whether participation in other credit programmes might explain the differential trajectory of the economic conditions of women between the treatment and control groups. Table 2.5 reports the impacts of the credit-based graduation programme on the post-intervention participation in other credit programmes, particularly operated by BRAC and non-BRAC NGOs. The treatment group is 43 percentage points more likely to take the loan from BRAC in the short run compared to the control group, which is 3 times the average uptake in the control group ('Treatment' coefficients in Panel A). The number of loans and the total value of loans from BRAC are significantly higher among the treatment group compared to the control group. In contrast, the treatment group is 9 pp less likely to take any loan from non-BRAC NGOs compared to the control group in the short run. The number of loans taken from non-BRAC NGOs is lower in the treatment group compared to the control.

In the long run ('Treatment' coefficients in Panel B), the treatment group is marginally more likely to take any loan from BRAC compared to the control group, with a significant treatment-control difference of 5 pp. The treatment-control differences in terms of the number and amount of loans are also lower and weaker in the long run compared to those in the short run. Similarly, the treatment-control differences are no longer significant in terms of loan uptake, number of loans and amount of loans from non-BRAC NGOs in the long run.

'Year' coefficients in Panel C report that loan uptake from BRAC significantly increases in the control group between endlines 1 and 2, whereas the between-endlines change in credit uptake from non-BRAC NGOs is not significant. This increased access to credit from BRAC in the control group might explain their substantial improvements in the economic conditions observed during the same period. Furthermore, the long-run treatment effect on loan uptake from BRAC is significantly smaller than that in the short run (see 'Treatment X Year' coefficients in columns 1-3 of Panel C), while the short-run treatment effect is smaller than the long-run effect on loan uptake from non-BRAC NGOs (see 'Treatment X Year' coefficients in columns 4-6 of Panel C).

These findings suggest a shift in credit market participation for both treatment and control groups. The treatment group is shifting from BRAC to the non-BRAC credit programmes, while the control group is shifting from the non-BRAC to BRAC credit programmes. Notably, these patterns of credit market participation align with the difference in the trajectories of the economic conditions observed between the two groups. The control group's increasing access to BRAC loans between the two endlines corresponds with their rising income and livestock ownership, while reduced engagement with BRAC credit among the treatment group might explain the stagnation in their economic conditions. This suggests that women in the treatment group could maintain the same level of income and livestock ownership despite their reduced borrowing from BRAC, while women in the control group, with their gradual increase in credit access with BRAC, required six years (gap between the two endlines) to reach the income level at which the economic conditions of women in the treatment group stagnated.

Table 2.5 Impacts on credit taken from NGOs

	(1) Loan taken from BRAC	(2) Loan from BRAC (num)	(3) Total loan from BRAC (BDT)	(4) Loan taken from non-BRAC NGO	(5) Loan from from non-BRAC NGO (num)	(6) Total loan from non-BRAC) NGO (BDT)
Panel (A): Endline 1						
Treatment (Yes/No)	0.433***	1.455***	15898.4***	-0.0858**	-0.248**	-1268.6
	(0.030)	(0.074)	(2000.125)	(0.039)	(0.122)	(2515.955)
Observations	2741	2741	2741	2741	2741	2741
Mean of control group at endline 1	0.143	0.217	6867.9	0.378	0.781	17637.3
Panel (B): Endline 2						
Treatment (Yes/No)	0.0529*	0.0629*	4536.1**	0.0392	0.0477	3541.6
	(0.027)	(0.037)	(1978.336)	(0.032)	(0.068)	(2896.486)
Observations	7079	7079	7079	7079	7079	7079
Mean of control group at endline 2	0.222	0.288	14081.4	0.393	0.683	24572.9
Panel (C): Endline 1 vs Endline 2						
Year (Endline 2=1 Endline 1=0)	0.0782***	0.0727*	7498.7***	0.0153	-0.0928	7352.5**
	(0.029)	(0.039)	(1808.652)	(0.039)	(0.139)	(3183.723)
Treatment (Yes/No)	0.433***	1.463***	15875.3***	-0.0803**	-0.240*	-599.6
	(0.029)	(0.065)	(2194.315)	(0.040)	(0.137)	(2671.383)
Treatment X Year	-0.381***	-1.405***	-11358.9***	0.118***	0.285*	3920.4
	(0.034)	(0.066)	(2190.186)	(0.043)	(0.146)	(3512.904)
Observations	9820	9820	9820	9820	9820	9820

Notes: Ordinary least squares (OLS) with the following outcomes: (1) whether women took any loan from BRAC in the last 18 months prior to the survey, (2) number of loans taken from BRAC, (3) amount of loans taken from BRAC, (4) whether women took any loan from non-BRAC NGOs, (5) number of loans taken from non-BRAC NGOs and (6) amount of loans taken from non-BRAC NGOs. Loan size is reported at 2024's constant prices by inflating endline 1's loan using CPI. Notably, women in the treatment group received the loan from the UPG programme about two years prior to the endline 1, so loan from the UPG programme is not counted for this analysis. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as the respondent's age and income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects, and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.5 Discussion and Conclusion

In this paper, we examined whether the impacts of a credit-based graduation programme on the incidence of IPV are sustained in the long run by conducting an evaluation at one and seven years after the programme was completed in rural Bangladesh. We interpret our findings as follows. First, one year after the completion of the programme, women in the treatment group were significantly less likely to experience physical and emotional IPV compared to women in the control group. This reduction is accompanied by improved economic conditions for both women and their husbands. Women in the treatment group earned nearly double the income of women in the control group, owned significantly more livestock (almost double the number of cows, 38% more goats and 54% more poultry relative to the average ownership among the control group), and saw an increase in their husbands' income.

Second, seven years after the completion of the programme, we observe a drastic reversal in treatment effects on the incidence of IPV. Women in the treatment group were more likely to experience both physical and emotional IPV than women in the control group. This is accompanied by an important difference in the trajectory of economic conditions between the treatment and control groups. Income of women in the control group grew by 50% between the two endlines, and their livestock ownership increased by 82% for the number of goat and 19% for the number of poultry. In contrast, the income and livestock ownership of women in the treatment group stagnated between the two endlines. However, women in the treatment group maintained higher levels of income and livestock in absolute terms than women in the control group, and their husbands also earned more compared to their counterparts in the control group in both the short and long run.

Several mechanisms could plausibly explain the differential change in the economic conditions between treatment and control groups, and credit market participation could be one of these mechanisms. Women in the treatment group reduced borrowing from BRAC while increasing borrowing from non-BRAC microcredit institutions between endlines 1 and 2. Women in the control group steadily increased borrowing from BRAC's Microfinance programme between endlines. Expanding credit market participation of women in the control group might lead to an increase in income and livestock ownership, while reduced borrowing of women in the treatment group from BRAC might lead to a stagnation in their income and livestock ownership between endlines 1 and 2. However, this cannot fully explain the pattern for the following reasons. First, women in the treatment group still borrowed significantly more from BRAC than women in the control group at endline 2. The control group converged with the treatment group over time but did not catch up entirely in terms of participating in the credit market. Second, women in the treatment group maintained the same level of income in both endlines despite reduced borrowing from BRAC seven years after the completion of the programme. The challenge is that their income and livestock ownership stopped growing in the long run. This pattern suggests that the programme may have equipped women with the skills and resources to sustain existing income-generating activities without relying heavily on credit. However, these capabilities and resources might be insufficient for a continued improvement in economic conditions.

The puzzle is why women in the treatment group experience a smaller reduction in the incidence of IPV compared to women in the control group between endlines 1 and 2, despite having improved economic conditions in absolute terms. We tentatively propose two possible propositions drawing on the 'expressive' and 'instrumental' theories of IPV, though our data cannot directly test either. First, the husbands of women in the treatment group may have become frustrated by the income stagnation of their wives, particularly as it contrasts sharply with the strong income increase the same women experienced in the short run. This supports the 'expressive' theory of IPV, which states that violence happens when husbands feel frustrated (Dollard et al., 1939; Berkowitz, 1989). Second, income stagnation may have weakened the bargaining power of women in the treatment group. Husbands may have interpreted this stagnation as economic failure that may have diminished their wives' influence over household decisions, which is consistent with the 'instrumental theory' of IPV, stating that husbands are more likely to use violence to control women if wives have less bargaining power (Straus and Hotaling, 1980; Eswaran and Malhotra, 2011). Both mechanisms could have increased the vulnerability of women in the treatment group to IPV relative to women in the control group. Note that husbands of women in the treatment group could maintain a higher income compared to their counterparts in the control group in both the short and long run, reinforcing these two dynamics and helping explain the higher incidence of IPV among the treatment group compared to the control group. Notably, women in both the treatment and control groups experienced a reduction in IPV between endlines 1 and 2; however, women in the treatment group experienced a lower reduction compared to women in the control group.

Our findings have important implications for both research and policy on women's empowerment at the household level. For research, examining the trajectory of the programme's impact on the incidence of IPV sheds light on why the evidence on the impacts of development programmes, targeting women's economic empowerment, on the incidence of IPV is mixed. Although results vary, many evaluation studies that were conducted within one or two years post-programme find that such programmes reduce the incidence of IPV, whereas long-term evaluation studies reveal different patterns. For example, Bobonis, Castro and Morales (2015) find null long-run impacts of a cash transfer programme on the incidence of IPV. Our results reveal that the programme's effects on IPV differ between the short- and long-run. This evidence suggests that the mixed findings in existing literature might stem from differences in the timing of the evaluation.

For policy, our analysis of why the change in the incidence of IPV over time differ between women in the treatment and control groups reveals that although women in the treatment group maintain better economic conditions in absolute terms compared to women in the control group in the long run, their economic conditions stagnate between the two endlines, while women in the control group experience gradual economic improvement, slowly converging with the treatment group. This pattern suggests that promoting continuous improvements in the economic conditions of women might be an effective strategy for achieving sustainable reductions in the incidence of IPV in male-dominated settings where women earn substantially less than their husbands.

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Appendix

2.A Selection criteria of the programme

The selection criteria of the credit-based graduation programme are as follows:

(i) have to be dependent on seasonal or irregular income;

(ii) need to have $\leq$30 decimal land;

(iii) have not been able to use loans successfully in the past for productive assets;

(iv) must have school-age children (6-14 years) who work to assist in income generation for the household;

(v) failed to consume meat or egg for any meals in the past three days.

Each beneficiary household must satisfy at least three out of the five criteria listed above to be eligible for the conditional loan and other forms of support.

The programme also follows two exclusion criteria before checking the eligibility criteria listed above. First, the programme excludes those households which do not have adult females who are capable of working because the programme targets women. Second, the programme excludes those households which are already receiving support from the Government/NGO or are active borrowers of any microfinance programme to prevent duplication.

2.B Support package of the programme

The main female member of the selected household (referred to as the beneficiary) received the support package. The components of the support package are as follows:

1. Enterprise Development Training

   Each beneficiary received training from BRAC on the productive asset that they selected. Once enterprise/asset selection was finalised, participants received a three-day-long classroom training. After the asset selection, they received another seven days of refresher training. This training aims to help beneficiaries learn how to maintain and care for their asset purchased through loans. The programme beneficiaries could select either of the following assets: livestock rearing, poultry rearing, or agriculture. The speciality on which the beneficiary was trained was done in consultation with BRAC staff since the geographical setup and surroundings of participant households, their prior experience in managing an enterprise, as well as the physical capability of the women, needed to be taken into account.

2. Conditional loan

   Upon completion of the enterprise training, beneficiaries received a conditional loan of a maximum of BDT 15,000, which is about 86% of the annual per capita income of selected ultra-poor households, for purchasing productive assets.[20] The loan was typically disbursed within 21 days of training. Beneficiaries were allowed a two-month grace period for repaying the loan, and the loan must be entirely repaid within one year or 11.5 instalments. After successfully repaying the loan, beneficiaries were encouraged to take regular loans from BRAC's Microfinance programme.

3. Free supporting assets or Input

   Each beneficiary received additional assets for free to support their enterprise with a maximum value of BDT 1000. These additional assets are referred to as input.[21]

4. Coaching

   The beneficiaries also received bi-weekly coaching through one-to-one home visits and group visits.[22] These visits covered asset monitoring, checking household well-being, training on financial management and asset diversification, and discussion on social issues (e.g., child marriage, dowry, human trafficking). The module on dowry and domestic violence includes advice on the consequences of these issues and potential strategies to tackle them. It also includes a reminder of the legal ramifications of these practices.[23]

5. Consumption Allowance

   Each beneficiary received a consumption allowance of BDT 210 (in cash) per week to support their daily expenses during the programme cycle.[24]

6. Guidance on social and health issues

   For severe morbidity, the programme beneficiaries and their household members received financial assistance. In the case of mild illnesses, beneficiaries were referred to nearby health centers for treatment. Moreover, the programme staff check on potential social issues (e.g., school drop-outs, intra-household conflicts, etc.)

[20]The interest charged (20% declining method/ the flat rate is 8% if all instalments are paid on time) on this conditional loan is lower than the traditional microfinance interest rate (27% declining method/ the flat rate is 12%) (Rahman, Bhattacharjee and Das, 2021).

[21]If someone received a cow, she got poultry, medicine for the cow and tree seedlings as inputs. For small enterprises (i.e., land cultivation), the ceiling for input was BDT 2,150.

[22]The participants received 12 home visits and 12 group visits during the programme period.

[23]1-5 years jail sentence and/or 50,000 BDT fine as per Bangladeshi law for Dowry. 6-month jail sentence and 10,000 BDT fine for domestic abuse, with more stringency for repeated offenders.

[24]The allowance amount is 15% of the baseline income of our sampled households. The duration of the allowance varies across asset categories—24 weeks for small enterprises; 36 weeks for livestock, and 52 weeks for agriculture/nursery. The amount of allowance (Tk. 30 per day) is calculated as follows. Ultra-poor households targeted by the credit-based model generally consist of 4 members (as per the baseline survey of this study) According to the Household Income and Expenditure Survey (HIES) 2016 (BBS 2019), per capita per day rice consumption in rural areas is about 433 gram. Hence, each ultra-poor household requires about 1700 gram of rice per day. BRAC decided to help them purchase around 1 kg of rice per day through the allowance. The price of this amount of rice is Tk. 27. Hence, BRAC provided consumption allowance of Tk. 30 per day.

and if any issue was identified, they referred the household to the relevant department/programme of Government or NGO for support.

7. Savings Advice

   Each beneficiary was encouraged to save with the programme according to their ability. The maximum ceiling of savings was BDT 150/month or BDT 30/week with a 7.5% annual interest rate. A general savings account was opened in the beneficiary's name with BRAC's Microfinance programme.

8. Community Resource Mobilization

   Community resource is mobilized through forming a committee called the Village Social Solidarity Committee (VSSC). For community resource mobilization, BRAC formed a committee in each programme village including well-connected and powerful people.[25] This committee is responsible for helping the programme beneficiaries to solve several problems, for example, mobilising resources to repair the houses, install latrines, survive during unprecedented times (i.e., natural disasters, road accidents); and help to enrol children in school.

[25] The aims of forming committees are to maintain or strengthen customary systems of social support for the poorest, while also providing some more systematic, community-level protection against the social and environmental risks faced by the ultra-poor.

2.C Sample description

Table 2.C.1 Sample size across treatment status, survey rounds and analysis status

	(1)	(2)	(3)
Group	Baseline	Endline 1 analysis	Endline 2 analysis
Treatment	7,042	1,945	5,549
Control	1,931	796	1,530
Total	8,973	2,741	7,079

Notes: Columns 2 and 3 report the number of women who were eligible and interviewed with the IPV module in each of the respective rounds. The reasons for exclusion from analysis by rounds of endline are reported in Table 2.C.3 . The last column (4) reports the balanced panel, which includes the common sample from columns 2 and 3. Balanced panels are those women who were (i) successfully interviewed in both endlines, (ii) the same respondents at the baseline and both endlines, (iii) interviewed by female enumerators in endline 1 and (iv) eligible for IPV (i.e., married) in both rounds.

Table 2.C.2 Survey modules and gender of enumerator by survey rounds

(1)	(2)	(3)	(4)	(5)	(6)
SL	Module	Explanation	Baseline (2016)	Endline 1 (2019)	Endline 2 (2024)
1	Household demographic characteristics	Age, gender, marital status, education	Yes, collected by male	Yes, collected by male	Yes, collected by female
2	Household members' employment	Employability, working hours, income in last one year	Yes, collected by male	Yes, collected by male	Yes, collected by female
3	Business (productive) asset accumulation	Number, value, ownership (owned by whom)	Yes, collected by male	Yes, collected by male	Yes, collected by female
4	Loan taken from BRAC	Amount and year	Yes, collected by male	Yes, collected by male	Yes, collected by female

Continued on next page

Table 2.C.2 – Survey modules and gender of enumerator by survey rounds

SL	Module	Explanation	Baseline (2016)	Endline 1 (2019)	Endline 2 (2024)
5	Loan taken from non-BRAC sources	Amount and year	Yes, collected by male	Yes, collected by male	Yes, collected by female
6	Intimate partner violence	Physical and emotional abuse	No	Yes, collected by female	Yes, collected by female

Notes: Columns 4-6 show whether the respective module was included in the respective survey and if yes, who collected the information. Notably, only the wife was interviewed. For the questions on the husband's income and asset accumulation, the wife was requested to seek assistance from her husband when needed.

Table 2.C.3 Sample inclusion and exclusion for analysis

	(1)	(2)
	Endline 1 Frequency (%)	Endline 2 Frequency (%)
Panel (A): Inclusion		
Analysis sample	2741	7079
	(31%)	(79%)
Panel (B): Exclusion reasons		
Random exclusion	2398	0
	(27%)	(0%)
Marital status	410	691
	(5%)	(8%)
Interviewed by male enumerator	2779	0
	(31%)	(0%)
Dead respondents	21	97
	(0%)	(1%)
Other attrition reasons	624	1106
	(7%)	(12%)
Total	8,973	8,973
	(100%)	(100%)

Notes: Number of observations are reported for inclusion and exclusion reasons. 'Random exclusion' are those treatment households which were randomly excluded from endline 1 survey. 'Marital status' includes those women who were excluded from the analysis because they were not married in the respective round of survey. 'Interviewed by male enumerator' are those interviews which were done by male enumerators in endline 1 survey. 'Replaced respondents' are those interviews where the baseline respondent was dead and the adult main female member was interviewed from the household. Percentages are in parentheses. 'Other attrition reasons' includes those interviews which could not be conducted because of failure to identify household, sickness of the respondent and other reasons.

Table 2.C.4 Balancing test

	Treatment	Control	Difference
Per capita monthly income (BDT at 2024 constant prices)	2773.2	2758.6	38.41
			(71.074)
Wife's age (year)	32.03	31.67	0.642
			(0.395)
Wife's year of schooling	3.243	3.243	-0.0417
			(0.142)
Wife's income (BDT at 2024 constant prices)	13260.6	11619.2	1271.5
			(919.531)
Husband's income (BDT at 2024 constant prices)	105253.6	104436.4	2350.2
			(2736.225)
Homestead land (decimal)	4.014	3.904	0.190
			(0.336)
Cultivable land (decimal)	2.426	1.736	0.897**
			(0.409)
Total savings (BDT)	1153.6	1441.9	-194.2
			(238.919)
Having outstanding loan (yes=1 no=0)	0.388	0.408	0.00235
			(0.032)
Having outstanding lending (yes=1 no=0)	0.0244	0.0202	0.00522
			(0.006)
Able to afford two meals (yes=1 no=0)	0.855	0.850	-0.00793
			(0.037)
Having cow (yes=1 no=0)	0.0822	0.0818	0.00151
			(0.011)
Having goat (yes=1 no=0)	0.127	0.131	-0.00901
			(0.016)
Having poultry (yes=1 no=0)	0.805	1.002	-0.134
			(0.288)
Total productive asset value (BDT)	4787.6	4578.1	335.9
			(308.819)
P-value of joint orthogonality test	0.5302		
Number of observations	7042	1931	8973

Notes: Columns 2 and 3 report the means across control and treatment groups. Column 3 reports OLS estimates of baseline characteristics on treatment status (treatment=1, control=0) controlling district fixed effects and clustering at the branch level, reflecting the difference between treatment and control groups. BDT stands for the currency of Bangladesh (Taka). All asset information (i.e., land, savings, productive asset) are measured at the household level. The p-values of joint orthogonality tests are obtained by regressing the treatment variable on all characteristics (mentioned in the first column of the table) controlling district fixed effects and clustering at the branch level. Standard errors are in parentheses in column 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.C.5 Balancing test for selected women in the treatment group for endline 1

	Selected	Not selected	Difference
Per capita monthly income (BDT at 2024 constant prices)	2756.2	2806.5	-43.25
			(47.425)
Wife's age (year)	32.10	31.90	0.0706
			(0.288)
Wife's year of schooling	3.281	3.168	0.137
			(0.103)
Wife's income (BDT at 2024 constant prices)	13218.7	13342.9	137.9
			(793.647)
Husband's income (BDT at 2024 constant prices)	105266.5	105228.2	-368.8
			(1572.415)
Homestead land (decimal)	4.080	3.884	0.117
			(0.205)
Cultivable land (decimal)	2.409	2.460	-0.200
			(0.464)
Total savings (BDT)	1168.0	1125.2	0.741
			(161.269)
Having outstanding loan (yes=1 no=0)	0.405	0.354	0.0429***
			(0.015)
Having outstanding lending (yes=1 no=0)	0.0251	0.0231	0.00151
			(0.005)
Able to afford two meals (yes=1 no=0)	0.852	0.862	-0.00767
			(0.016)
Having cow (yes=1 no=0)	0.0810	0.0846	-0.00352
			(0.008)
Having goat (yes=1 no=0)	0.131	0.119	0.0144*
			(0.008)
Having poultry (yes=1 no=0)	0.867	0.684	0.181
			(0.151)
Total productive asset value (BDT)	4889.9	4586.8	233.5
			(211.548)
P-value of joint orthogonality test	0.0851		
Number of observations	4666	2376	7042

Notes: Columns 2 and 3 report the means across control and treatment groups. Column 3 reports OLS estimates of baseline characteristics on treatment status (treatment=1, control=0) controlling district fixed effects and clustering at the branch level, reflecting the difference between treatment and control groups. BDT stands for the currency of Bangladesh (Taka). All asset information (i.e., land, savings, productive asset) are measured at the household level. The p-values of joint orthogonality tests are obtained by regressing the treatment variable on all characteristics (mentioned in the first column of the table) controlling district fixed effects and clustering at the branch level. Standard errors are in parentheses in column 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.C.6 Balancing test for endline 1 sample

	Treatment	Control	Difference
Per capita monthly income (BDT at 2024 constant prices)	2773.2	2758.6	26.00
			(74.653)
Wife's age (year)	32.03	31.67	0.684*
			(0.409)
Wife's year of schooling	3.243	3.243	-0.00686
			(0.140)
Wife's income (BDT at 2024 constant prices)	13260.6	11619.2	1281.6
			(907.019)
Husband's income (BDT at 2024 constant prices)	105253.6	104436.4	2198.1
			(2779.480)
Homestead land (decimal)	4.014	3.904	0.244
			(0.332)
Cultivable land (decimal)	2.426	1.736	0.838**
			(0.403)
Total savings (BDT)	1153.6	1441.9	-196.6
			(238.500)
Having outstanding loan (yes=1 no=0)	0.388	0.408	0.0178
			(0.031)
Having outstanding lending (yes=1 no=0)	0.0244	0.0202	0.00593
			(0.005)
Able to afford two meals (yes=1 no=0)	0.855	0.850	-0.0107
			(0.036)
Having cow (yes=1 no=0)	0.0822	0.0818	0.000651
			(0.010)
Having goat (yes=1 no=0)	0.127	0.131	-0.00411
			(0.015)
Having poultry (yes=1 no=0)	0.805	1.002	-0.0669
			(0.292)
Total productive asset value (BDT)	4787.6	4578.1	422.0
			(309.226)
P-value of joint orthogonality test	0.3554		
Number of observations	4666	1931	6597

Notes: Columns 2 and 3 report the means across control and treatment groups. Column 3 reports OLS estimates of baseline characteristics on treatment status (treatment=1, control=0) controlling district fixed effects and clustering at the branch level, reflecting the difference between treatment and control groups. BDT stands for the currency of Bangladesh (Taka). All asset information (i.e., land, savings, productive asset) are measured at the household level. The p-values of joint orthogonality tests are obtained by regressing the treatment variable on all characteristics (mentioned in the first column of the table) controlling district fixed effects and clustering at the branch level. Standard errors are in parentheses in column 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.C.7 Balancing test across treatment and control using analysed sample: Endline 1

	Treatment	Control	Difference
Per capita monthly income (BDT at 2024 constant prices)	2766.4	2758.6	27.78
			(95.910)
Wife's age (year)	32.06	31.67	1.021*
			(0.565)
Wife's year of schooling	3.258	3.243	-0.0383
			(0.181)
Wife's income (BDT at 2024 constant prices)	13243.9	11619.2	776.9
			(1097.558)
Husband's income (BDT at 2024 constant prices)	105258.7	104436.4	2905.0
			(3085.002)
Homestead land (decimal)	4.040	3.904	0.397
			(0.345)
Cultivable land (decimal)	2.419	1.736	0.670
			(0.445)
Total savings (BDT)	1159.3	1441.9	-319.6
			(392.691)
Having outstanding loan (yes=1 no=0)	0.395	0.408	0.0219
			(0.038)
Having outstanding lending (yes=1 no=0)	0.0247	0.0202	0.0101
			(0.006)
Able to afford two meals (yes=1 no=0)	0.854	0.850	-0.0227
			(0.037)
Having cow (yes=1 no=0)	0.0817	0.0818	-0.00441
			(0.014)
Having goat (yes=1 no=0)	0.128	0.131	0.000599
			(0.016)
Having poultry (yes=1 no=0)	0.830	1.002	-0.719
			(0.601)
Total productive asset value (BDT)	4828.4	4578.1	329.2
			(396.335)
P-value of joint orthogonality test	0.0186		
Number of observations	1945	796	2741

Notes: Columns 2 and 3 report the means across control and treatment groups. Column 3 reports OLS estimates of baseline characteristics on treatment status (treatment=1, control=0) controlling district fixed effects and clustering at the branch level, reflecting the difference between treatment and control groups. BDT stands for the currency of Bangladesh (Taka). All asset information (i.e., land, savings, productive asset) are measured at the household level. The p-values of joint orthogonality tests are obtained by regressing the treatment variable on all characteristics (mentioned in the first column of the table) controlling district fixed effects and clustering at the branch level. Standard errors are in parentheses in column 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.C.8 Balancing test across treatment and control using analysed sample: Endline 2

	Treatment	Control	Difference
Per capita monthly income (BDT at 2024 constant prices)	2766.4	2758.6	29.09
			(77.263)
Wife's age (year)	32.06	31.67	0.684*
			(0.394)
Wife's year of schooling	3.258	3.243	-0.0158
			(0.151)
Wife's income (BDT at 2024 constant prices)	13243.9	11619.2	955.6
			(968.130)
Husband's income (BDT at 2024 constant prices)	105258.7	104436.4	2073.6
			(2754.512)
Homestead land (decimal)	4.040	3.904	0.254
			(0.343)
Cultivable land (decimal)	2.419	1.736	0.876*
			(0.477)
Total savings (BDT)	1159.3	1441.9	-272.1
			(261.086)
Having outstanding loan (yes=1 no=0)	0.395	0.408	-0.00447
			(0.033)
Having outstanding lending (yes=1 no=0)	0.0247	0.0202	0.00593
			(0.006)
Able to afford two meals (yes=1 no=0)	0.854	0.850	0.00149
			(0.034)
Having cow (yes=1 no=0)	0.0817	0.0818	0.00135
			(0.012)
Having goat (yes=1 no=0)	0.128	0.131	-0.0119
			(0.017)
Having poultry (yes=1 no=0)	0.830	1.002	-0.316
			(0.339)
Total productive asset value (BDT)	4828.4	4578.1	222.9
			(326.017)
P-value of joint orthogonality test	0.2561		
Number of observations	5549	1530	7079

Notes: Columns 2 and 3 report the means across control and treatment groups. Column 3 reports OLS estimates of baseline characteristics on treatment status (treatment=1, control=0) controlling district fixed effects and clustering at the branch level, reflecting the difference between treatment and control groups. BDT stands for the currency of Bangladesh (Taka). All asset information (i.e., land, savings, productive asset) are measured at the household level. The p-values of joint orthogonality tests are obtained by regressing the treatment variable on all characteristics (mentioned in the first column of the table) controlling district fixed effects and clustering at the branch level. Standard errors are in parentheses in column 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.D Reduction in IPV between endlines 1 and 2

This section examines why IPV reduces among the control group from endline 1 to endline 2. First, we investigate whether the decline happens because of wives' ageing, since older women are generally less likely to experience IPV compared to younger women (Garg et al., 2021; Wado et al., 2021; Sardinha et al., 2024). Second, we reflect on whether national-level initiatives or measures contributed to reducing IPV over this period.

2.D.1 The role of wife's age in IPV reduction

The results in Tables 2.1 and 2.2 show that the treatment effects are smaller than the decline in IPV observed in the control group between endlines 1 and 2. For physical IPV, the decline in IPV between both endlines is nearly six times the treatment effect; for emotional IPV, it is about four times, specifically for mental torture, where the treatment effect is largest relative to the control mean. To explore the drivers of this significant decline in IPV in the control group, we examine whether the ageing of the wives contributed to it, given the six-year gap between endlines 1 and 2. Prior studies across developing countries find that IPV prevalence is highest among adolescent women and declines with age (Garg et al., 2021; Wado et al., 2021; Sardinha et al., 2024). A similar pattern is observed in Bangladesh.[26]

To understand the role of age in IPV reduction in our sample, we examine whether the declines in IPV in the control group vary by wives' baseline age. We assess endline-specific IPV prevalences and differences between the two endlines by baseline age groups. Column 1 of Table 2.D.1 reports the mean IPV prevalence in endlines 1 and 2 and the difference between endlines 1 and 2 for the control group. Columns 2–5 report the same statistics by wives' baseline age groups. Panels A and B present physical and emotional IPV, respectively.

For physical IPV ('Mean (Endline 1)' in Panel A), prevalence shows a clear age gradient of IPV experience. In endline 1, prevalence reduces with the wife's age at baseline. Relative to women aged 15-25 at baseline, it is lower by about 12 percentage points (pp) among those aged 26-30 and 31-38, and by about 15 pp among those aged ≥ 39. In proportional terms, prevalence among the ≥ 39 group is roughly 40% lower than among the 16-25 group. In endline 2 ('Mean (Endline 2)), the pattern is similar, but absolute differences across age groups are smaller (about 3–6 pp) compared with endline 1 (about 12–17 pp). Comparing the same age group across endlines, the decline in physical IPV from endline 1 to endline 2 ranges from 51% to 62% ('Year' in Panel A). For the youngest group, the reduction is 54%, rising to 58% and 62% for the 26-30 and 31-38 groups, respectively, and then lowering for the oldest group.

For emotional IPV ('Mean (Endline 1)' in Panel B), results are similar. In endline 1, older groups have a lower prevalence than younger groups by about 12–17 pp; in endline 2 ('Mean (Endline 2)'), these absolute differences reduce to about 3–6 pp. The endline-to-endline decline varies between 29% and 35%, with the two oldest groups showing, respectively, the lowest and the highest reduction rates ('Year' in Panel B). Together, these patterns suggest that wives ageing six years between the two endlines might partly

[26] Stake et al. (2020), using DHS data, reports that women younger than 30 experience significantly higher IPV than those aged 35 and above (odds ratio = 1.53). Likewise, Hossain, Kadir and Kabir (2025), using MICS data, finds that women aged 35–39 have 65% higher odds of domestic violence than those aged 45–49 (odds ratio = 1.65), and women aged 40–44 have 30% higher odds (odds ratio = 1.304). A recent national survey on violence against women reports a 14 percentage points gap in the prevalence of any IPV between women aged 15–19 and those aged 45–49 (BBS, 2025).

explain the IPV decline between endlines 1 and 2. However, age alone cannot account for the full reduction observed in the control group.

Table 2.D.1 IPV prevalence among control group, by baseline age groups of wives and by endlines

	(1) All	(2) 15-25 years	(3) 26-30 years	(4) 31-38 years	(5) 39+ years
Panel (A): Physical IPV					
Mean (Endline 1)	0.514	0.582	0.604	0.487	0.325
	(796)	(239)	(207)	(193)	(157)
Mean (Endline 2)	0.224	0.265	0.254	0.186	0.159
	(1530)	(452)	(429)	(354)	(295)
Year (Endline 2=1 Endline 1=0)	-0.290***	-0.316***	-0.350***	-0.301***	-0.166***
	(0.020)	(0.037)	(0.038)	(0.039)	(0.040)
Observations	2326	691	636	547	452
Panel (B): Emotional IPV					
Mean (Endline 1)	0.834	0.891	0.855	0.788	0.777
	(796)	(239)	(207)	(193)	(157)
Mean (Endline 2)	0.559	0.591	0.566	0.559	0.502
	(1530)	(452)	(429)	(354)	(295)
Year (Endline 2=1 Endline 1=0)	-0.275***	-0.301***	-0.289***	-0.228***	-0.275***
	(0.020)	(0.035)	(0.038)	(0.042)	(0.047)
Observations	2326	691	636	547	452

Notes: Panel (A): Physical IPV prevalence in the control group, taking the value of one if the wife experienced any form of physical IPV in the last 12 months prior to the survey and zero otherwise. Panel (B): Emotional IPV prevalence in the control group, taking the value of one if the wife experienced any form of emotional IPV in the last 12 months prior to the survey and zero otherwise. The number of observations is in parentheses for means, and standard errors are in parentheses for differences. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next, we examine the treatment effects on the incidence of physical and emotional IPV by baseline age groups of wives. Table 2.D.2 reports the results. We observe that among all age groups, there is no significant difference between the treatment and control groups in endline 1 (‘Treatment’ coefficients in Panel A). Among women aged between 16-25 and 26-30 years at baseline, we observe a marginally higher physical IPV prevalence among the treatment group compared to their counterparts in the control group in endline 2 (‘Treatment’ coefficients in Panel B). Women in the treatment group aged 16-25 and 26-30 years old are more likely to experience IPV by 18% and 19% relative to the mean among their counterparts in the control group, respectively. Among women aged 31-38 years at baseline, women in the treatment group are significantly more likely to experience physical IPV by 7.4 pp (40% relative to the control mean). Among women aged 31–38, the decline in IPV between the two endlines is larger than for other age groups; however, women in the treatment group in this age group report higher IPV than the control group, indicating that women in the treatment group do not benefit from the age-related decline in IPV.

Table 2.D.2 Impacts on any physical IPV, by baseline age groups of wives

	(1) 15-25 years	(2) 26-30 years	(3) 31-38 years	(4) 39+ years
Panel (A): Endline 1				
Treatment (Yes/No)	-0.0456	-0.0483	-0.0234	-0.0282
	(0.060)	(0.056)	(0.044)	(0.049)
Observations	768	694	689	589
Mean in control group	0.582	0.604	0.487	0.325
Panel (B): Endline 2				
Treatment (Yes/No)	0.0478*	0.0486*	0.0739***	0.0329
	(0.025)	(0.025)	(0.024)	(0.024)
Observations	2049	1802	1746	1479
Mean in control group	0.265	0.254	0.186	0.159
Panel (C): Endline 1 vs Endline 2				
Year (Endline 2=1 Endline 1=0)	-0.319***	-0.342***	-0.309***	-0.164***
	(0.063)	(0.059)	(0.049)	(0.052)
Treatment (Yes/No)	-0.0282	-0.0440	-0.00157	-0.0153
	(0.063)	(0.057)	(0.053)	(0.050)
Treatment X Year	0.0721	0.0881	0.0738	0.0443
	(0.073)	(0.070)	(0.062)	(0.061)
Observations	2817	2496	2435	2068
Mean in control group	0.265	0.254	0.186	0.159

Notes: Ordinary least squares (OLS) with different types of physical abuse by baseline age groups, and each type takes the value of one if the respondent experienced that form of physical IPV in the last 12 months prior to the survey and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as respondents' income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.D.3 reports the treatment effects on the incidence of emotional IPV across baseline age groups of the wives. Among all age groups, we observe that there is no significant difference in emotional IPV between the treatment and control groups in endlines 1 and 2 (Panels A and B). The change in IPV between the two endlines also does not significantly differ between the treatment and control groups (see coefficients of 'Treatment × Year' in Panel C). The treatment effect on experiencing at least one form of emotional IPV is weak for the entire sample; therefore, age-disaggregated treatment effects on emotional IPV are also not significant.

Table 2.D.3 Impacts on any emotional IPV by baseline age groups of wives

	(1) 15-25 years	(2) 26-30 years	(3) 31-38 years	(4) 39+ years
Panel (A): Endline 1				
Treatment (Yes/No)	-0.0386	0.0323	0.0582	-0.0105
	(0.034)	(0.037)	(0.043)	(0.047)
Observations	768	694	689	589
Mean in control group	0.582	0.604	0.487	0.325
Panel (B): Endline 2				
Treatment (Yes/No)	-0.0120	0.0249	0.0103	0.0169
	(0.031)	(0.027)	(0.032)	(0.036)
Observations	2049	1802	1746	1477
Mean in control group	0.265	0.254	0.186	0.159
Panel (C): Endline 1 vs Endline 2				
Year (Endline 2=1 Endline 1=0)	-0.300***	-0.285***	-0.225***	-0.278***
	(0.044)	(0.056)	(0.063)	(0.050)
Treatment (Yes/No)	-0.0321	0.0375	0.0576	-0.000668
	(0.039)	(0.042)	(0.049)	(0.058)
Treatment X Year	0.0164	-0.0127	-0.0486	0.0139
	(0.052)	(0.061)	(0.069)	(0.061)
Observations	2817	2496	2435	2066
Mean in control group	0.265	0.254	0.186	0.159

Notes: Ordinary least squares (OLS) with different types of emotional abuse by baseline age groups, and each type takes the value of one if the respondent experienced that form of physical IPV in the last 12 months prior to the survey and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as respondents' income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects, and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.D.2 National trends of IPV prevalence

Our results show substantial declines in IPV in both treatment and control groups between the two endlines. While estimated treatment effects on the incidence of IPV remain significant in the long run, the broader national decline in IPV provides context for interpreting these estimates. National trends may partly explain these declines. We explore three potential avenues to explain these declines: (i) documented trends in IPV prevalence, (ii) national policy initiatives, and (iii) socioeconomic changes identified in prior research.

First, according to national statistics, in general, Bangladesh has been experiencing a downward trend in IPV over the past decade. Recent statistics from the national survey on Violence against women reveal that IPV prevalence declines from 55% to 41% between 2015 and 2024 (BBS, 2025), with a reduction of 25% over a decade.[27]Although

[27]Any form of violence (measured by using the UN standard framework) experienced in the last 12

our indicators do not perfectly match the questionnaire used in this national survey, both their survey and our survey confirm that the IPV prevalence is reducing over time, perhaps because of awareness raising or prevention plans for violence against women at the national level.

Second, the National Action Plan to Prevent Violence against Women and Children (NAPPVWC) was formulated in 2018 with an aim to develop a society free from violence against women and children by 2030 (MoWCA, 2018). The policy and programme uptake between 2020 and 2024 might lead to stronger change in violence against women compared to the early stage (i.e., 2018-2019). This plan comprises several activities under a set of broad themes, including legal and justice, policing and legal support, health, awareness and education, social support, disaster, industry and workplace, land and housing, and data monitoring (MoWCA, 2020). We have limited information on the progress or an evaluation of this comprehensive plan (see the last column of the table). The trend of decrease in IPV from the national statistics should capture the influence of this plan, if there is any. Without district-level implementation data on NAPPVWC, we cannot assess its specific contribution to IPV reduction in our sample areas.

Third, prior studies document and explain Bangladesh's decline in IPV. Drawing on a longitudinal qualitative study in six rural villages (2011–2013; 74 life histories), Schuler and Nazneen (2018) identify four mechanisms consistent with the national decline in IPV past-year by 11 percentage points(from 36% in 2002 to 25% in 2014 reported by the national surveys on Bangladesh). These mechanisms include: (i) women's earnings have risen, and men rely more on them; (ii) greater economic security gives women more credible exit options and leverage; (iii) local actors and NGOs intervene more often; and (iv) norms have shifted—empowerment is more accepted, so backlash is lower. Complementary qualitative evidence in rural Bangladesh highlights women's economic solvency and microfinance, mass-media/legal awareness, and the shift from extended to nuclear households as plausible drivers of reduced IPV (Sony, 2023). However, the causal impact of microfinance on the incidence of IPV is mixed: some studies find higher IPV among certain subgroups of microfinance participants (Dalal, Dahlström and Timpka, 2013), whereas a meta-analysis reports reductions in psychological/sexual IPV and controlling behaviours (Allan-Blitz, Olson and Tran, 2023), and a recent systematic review across LMICs emphasises mixed effects of microfinance on the incidence of IPV (Williams et al., 2025). Finally, delaying marriage through schooling appears to be effective in reducing IPV. Particularly, in Bangladesh, female secondary stipend reduced IPV for eligible rural cohorts by delaying marriage and improving partner quality (Sara and Priyanka, 2023). Consistent with this channel, higher secondary enrollment rate among girls increases from 37% to 53% from 2019 to 2023 (BANBEIS, 2024), while child marriage rate[28] has declined from 65% to 50% between 2011 and 2022 (NIPORT and ICF, 2023). Although these refer-

months among ever-married women.

[28]The proportion of girls aged 20-24 years getting married before 18

enced studies do not cover 2019–2024 specifically, the mechanisms they identify plausibly account for part of the observed decline in IPV in our control group over this period; however, we can not test these mechanisms directly.

2.E Robustness checks

2.E.1 Impacts on the incidence of IPV using balanced sample

Table 2.E.1 Impacts on physical IPV using balanced sample

	(1) Physically hurt	(2) Threaten with hurt	(3) Slap	(4) Abuse for dowry
Panel (A): Endline 1				
Treatment (Yes/No)	-0.0328	-0.0351	-0.0240	-0.0381**
	(0.040)	(0.033)	(0.044)	(0.018)
Observations	2353	2353	2353	2345
Mean of control group at endline 1	0.287	0.296	0.402	0.0782
Panel (B): Endline 2				
Treatment (Yes/No)	0.0164	0.0436***	0.0209	0.00868
	(0.012)	(0.015)	(0.019)	(0.008)
Observations	2352	2352	2351	2349
Mean of control group at endline 2	0.0841	0.103	0.167	0.0413
Panel (C): Endline 1 vs Endline 2				
Year (Endline 2=1 Endline 1=0)	-0.203***	-0.193***	-0.236***	-0.0369
	(0.049)	(0.042)	(0.046)	(0.025)
Treatment (Yes/No)	-0.0332	-0.0292	-0.0281	-0.0328
	(0.044)	(0.039)	(0.048)	(0.022)
Treatment X Year	0.0501	0.0670	0.0534	0.0361
	(0.056)	(0.055)	(0.052)	(0.028)
Observations	4705	4705	4704	4694

Notes: Ordinary least squares (OLS) regressions with different types of physical abuse using 'balanced' sample and each type takes the value of one if the respondent experienced that form of physical IPV in the last 12 months prior to the survey and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as wife's age (i.e., respondent's age), respondent's income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.E.2 Impacts on emotional IPV using balanced sample

	(1) Insult	(2) Intimidate	(3) Mental torture	(4) Any emotional abuse
Panel (A): Endline 1				
Treatment (Yes/No)	0.0371	-0.108**	-0.0693	0.0155
	(0.061)	(0.042)	(0.042)	(0.053)
Observations	2353	2352	2348	2353
Mean of control group at endline 1	0.477	0.367	0.196	0.585
Panel (B): Endline 2				
Treatment (Yes/No)	0.0363	-0.0400	0.0195	-0.0236
	(0.028)	(0.027)	(0.015)	(0.033)
Observations	2347	2350	2348	2353
Mean of control group at endline 2	0.214	0.375	0.0855	0.496
Panel (C): Endline 1 vs Endline 2				
Year (Endline 2=1 Endline 1=0)	-0.264***	0.00732	-0.111**	-0.0884*
	(0.054)	(0.050)	(0.051)	(0.053)
Treatment (Yes/No)	0.0341	-0.103**	-0.0655	0.0166
	(0.062)	(0.048)	(0.045)	(0.056)
Treatment X Year	0.00567	0.0582	0.0813	-0.0413
	(0.067)	(0.061)	(0.054)	(0.065)
Observations	4700	4702	4696	4706

Notes: Ordinary least squares (OLS) with different types of emotional abuse abuse using ‘balanced’ sample and each type takes the value of one if the respondent experienced that form of emotional IPV in the last 12 months prior to the survey and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as wife’s age (i.e., respondent’s age), respondent’s income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

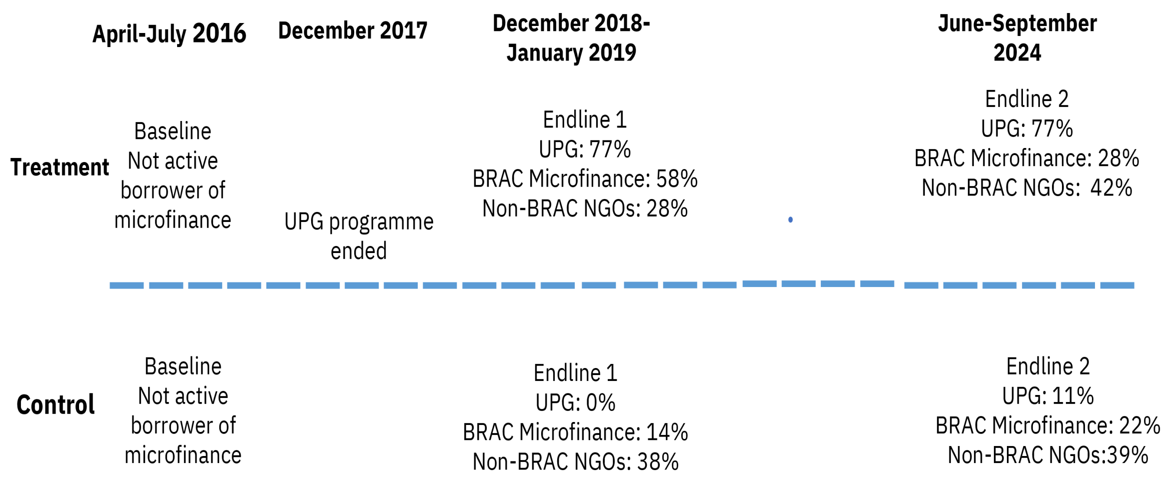
2.E.2 Credit Journey

Figure 2.E.1 illustrates the credit trajectories of treatment and control groups from baseline to endline 2. At baseline (April-July 2016), none from the treatment and the control groups were active borrowers of any microfinance programme. However, one of the selection criteria was failure to utilise a loan in the past (see section 2.A in the appendix), indicating that they might have had a history of taking loans, but they did not have the capacity to use the loan for a productive purpose and repay the loan. After completing the baseline survey, the UPG programme disbursed the loan, and the beneficiaries repaid the loan by August 2017. From September to December 2017, the programme completed other activities, such as monitoring the progress of the beneficiaries, forming the community support group and most importantly, sharing the list of beneficiaries with the BRAC Microfinance team as potential clients.

In the endline 1 survey, we found that 77% of women in the treatment group received a support package from the UPG programme, while none of the control group members received a support package. 58% and 28% of the treatment group received loans from

BRAC Microfinance and non-BRAC NGOs, respectively, while these uptakes were 14% and 38% among the control group in the last 18 months prior to the survey. This sharp increase in credit take-up with BRAC and decrease with non-BRAC NGOs among the treatment group might occur because of the UPG team sharing a list of beneficiaries with BRAC Microfinance, identifying them as potential clients or a strong institutional network with BRAC because of participation in the UPG programme. In the endline 2 survey, we found that the uptake of the control group with BRAC Microfinance increased (22%), while the uptake reduced among the treatment group (28%). Treatment group increased borrowing from non-BRAC NGOs (42%) while this uptake among the control group remained the same between endlines 1 and 2 (39%). Moreover, 11% of the control group received support from the UPG programme between endlines 1 and 2 because five years after the programme ended, the control group was allowed to receive the support from the UPG programme if they were still eligible.

Figure 2.E.1 Trend of credit market participation across treatment and control groups



Notes: Proportions of treatment and control groups participating in BRAC's UPG programme, BRAC's credit programme and non-BRAC NGOs' credit programmes across different time periods.

Table 2.E.3 Impacts on physical IPV controlling for other credit programme participation

	(1) Physically hurt	(2) Threaten with hurt	(3) Slap	(4) Abuse for dowry
Panel (A): Endline 1				
Treatment (Yes/No)	-0.00135	-0.0174	-0.0252	-0.0371*
	(0.036)	(0.036)	(0.042)	(0.021)
Observations	2741	2741	2741	2733
Mean of control group at endline 1	0.279	0.294	0.391	0.0830
Panel (B): Endline 2				
Treatment (Yes/No)	0.0311***	0.0380***	0.0328**	0.00910*
	(0.008)	(0.013)	(0.015)	(0.005)
Observations	7069	7071	7069	7063
Mean of control group at endline 2	0.0785	0.111	0.161	0.0366
Panel (C): Endline 1 vs Endline 2				
Year (Endline 2=1 Endline 1=0)	-0.196***	-0.180***	-0.229***	-0.0447
	(0.045)	(0.043)	(0.046)	(0.030)
Treatment (Yes/No)	-0.0122	-0.0115	-0.0256	-0.0346
	(0.044)	(0.045)	(0.048)	(0.029)
Treatment X Year	0.0439	0.0475	0.0598	0.0413
	(0.051)	(0.054)	(0.052)	(0.033)
Observations	9810	9812	9810	9796

Notes: Ordinary least squares (OLS) with different types of physical abuse (columns 1-4) and each type takes the value of one if the respondent experienced that form of physical IPV in the last 12 months prior to the survey and zero otherwise. Any physical IPV (column 5) equals to one if the woman experienced any form of emotional IPV and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as respondents' age and income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects and credit market participation in the respective round of the survey. Standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.E.4 Impacts on emotional IPV controlling for other credit programme participation

	(1) Insult	(2) Intimidate	(3) Mental torture	(4) Any emotional abuse
Panel (A): Endline 1				
Treatment (Yes/No)	0.0476	-0.107**	-0.0716*	0.0151
	(0.059)	(0.041)	(0.038)	(0.051)
Observations	2741	2740	2736	2741
Mean of control group at endline 1	0.480	0.370	0.201	0.592
Panel (B): Endline 2				
Treatment (Yes/No)	0.0405**	0.00453	0.0300**	0.0155
	(0.017)	(0.017)	(0.012)	(0.020)
Observations	7066	7067	7059	7075
Mean of control group at endline 2	0.213	0.352	0.0832	0.473
Panel (C): Endline 1 vs Endline 2				
Year (Endline 2=1 Endline 1=0)	-0.264***	-0.0171	-0.117**	-0.116**
	(0.054)	(0.052)	(0.050)	(0.054)
Treatment (Yes/No)	0.0515	-0.102*	-0.0661	0.0188
	(0.063)	(0.053)	(0.048)	(0.058)
Treatment X Year	-0.00959	0.102	0.0931*	-0.00560
	(0.066)	(0.063)	(0.054)	(0.065)
Observations	9807	9807	9795	9816

Notes: Ordinary least squares (OLS) with different types of emotional abuse (columns 1-3) and each type takes the value of one if the respondent experienced that form of emotional IPV in the last 12 months prior to the survey and zero otherwise. Any emotional IPV (column 4) equals to one if the woman experienced any form of emotional IPV and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.1. Panel (C) uses equation 2.2 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment takes the value of one if the wife was offered the intervention at baseline and zero otherwise. Controls include baseline characteristics, such as respondents' age and income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects and credit market participation in respective round of survey. Standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.E.3 LATE impacts

All women in the treatment group were offered the programme; however, not everyone participated in the programme. Due to imperfect compliance, we use an instrumental variable (IV) approach to estimate the local average treatment effect (LATE). For LATE estimation, we first regress programme participation on being assigned to treatment status (randomly assigned to the women at baseline). Thus, our first stage regression is presented below.

$$C_{ibd} = \beta_0 + T_{bd}.\beta_1 + \chi_{ibd}.\Theta + \gamma_d + \epsilon_{ibd} \tag{2.3}$$

where C_{ibd} is whether woman i in branch b in district d complied to the programme. T_{bd} is an indicator variable taking the value one if the branch b was assigned to treatment and zero otherwise. χ_{ibd} is a vector of covariates (i.e., respondents' age, household income, financial assets, livestock ownership, land ownership and food security). γ_d are district fixed effects; ϵ_{ied} is the error term. Because randomisation occurred at the branch level, we cluster standard errors at the branch level.

For our second stage regression, we regress our outcome variables on predicted compliance to estimate the LATE impacts of the programme in the short (long) run.

$$z_{ibd} = \beta_0 + \hat{C_{ibd}}.\beta_1 + \chi_{ibd}.\Theta + \gamma_d + \epsilon_{ibd} \tag{2.4}$$

To estimate the difference between short- and long-run impacts using LATE estimation, we use the following equation.

$$z_{ibd} = \beta_0 + Y_t.\beta_1 + \hat{C_{ibd}}.\beta_2 + C_{ibd}.Y_t.\beta_3 + \chi_{ibd}.\Theta + \gamma_d + \epsilon_{ibd} \tag{2.5}$$

Table 2.E.5 LATE impacts on physical IPV

	(1) Physically hurt	(2) Threaten with hurt	(3) Slap	(4) Abuse for dowry	(5) Any physical IPV
Panel (A): Endline 1					
Treatment received	-0.0308	-0.0350	-0.0323	-0.0548**	-0.0424
	(0.047)	(0.044)	(0.052)	(0.026)	(0.051)
Observations	2741	2741	2741	2733	2741
Mean of control group at endline 1	0.279	0.294	0.391	0.0830	0.514
Panel (B): Endline 2					
Treatment received	0.0481***	0.0573***	0.0506**	0.0147*	0.0765***
	(0.013)	(0.021)	(0.024)	(0.008)	(0.022)
Observations	7069	7071	7069	7063	7077
Mean of control group at endline 2	0.0785	0.111	0.161	0.0366	0.224
Panel (C): Endline 1 vs Endline 2					
Year (Endline 2=1 Endline 1=0)	-0.167***	-0.123***	-0.182***	-0.0474*	-0.212***
	(0.040)	(0.039)	(0.047)	(0.027)	(0.043)
Treatment received	0.0104	0.0462	0.0228	-0.0459	0.0627
	(0.053)	(0.057)	(0.071)	(0.033)	(0.063)
Treatment received X Year	0.0135	-0.0340	-0.00523	0.0563	-0.0454
	(0.055)	(0.057)	(0.070)	(0.035)	(0.064)
Observations	9810	9812	9810	9796	9818

Notes: Ordinary least squares (OLS) with different types of physical abuse (columns 1-4) and each type takes the value of one if the respondent experienced that form of physical IPV in the last 12 months prior to the survey and zero otherwise. Any physical IPV (column 5) equals to one if the woman experienced any form of emotional IPV and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.4. Panel (C) uses equation 2.5 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment received takes the value of one if the wife received the intervention and zero otherwise. Controls include baseline characteristics, such as respondents' age and income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.E.6 LATE impacts on emotional IPV

	(1) Insult	(2) Intimidate	(3) Mental torture	(4) Any emotional abuse
Panel (A): Endline 1				
Treatment received	0.0539	-0.147***	-0.102**	0.00933
	(0.076)	(0.053)	(0.049)	(0.066)
Observations	2741	2740	2736	2741
Mean of control group at endline 1	0.480	0.370	0.201	0.592
Panel (B): Endline 2				
Treatment received	0.0611**	0.00742	0.0460**	0.0247
	(0.030)	(0.027)	(0.020)	(0.031)
Observations	7066	7067	7059	7075
Mean of control group at endline 2	0.213	0.352	0.0832	0.473
Panel (C): Endline 1 vs Endline 2				
Year (Endline 2=1 Endline 1=0)	-0.185***	-0.00472	-0.0827*	-0.0919*
	(0.065)	(0.044)	(0.047)	(0.055)
Treatment received	0.148	-0.114*	-0.0480	0.0496
	(0.098)	(0.067)	(0.062)	(0.086)
Treatment received X Year	-0.140	0.103	0.0626	-0.0475
	(0.097)	(0.067)	(0.063)	(0.083)
Observations	9807	9807	9795	9816

Notes: Ordinary least squares (OLS) with different types of emotional abuse (columns 1-3), and each type takes the value of one if the respondent experienced that form of emotional IPV in the last 12 months prior to the survey, and zero otherwise. Any emotional IPV (column 4) equals to one if the woman experienced any form of emotional IPV and zero otherwise. Panels (A) and (B) report short- and long-run impacts using equation 2.4. Panel (C) uses equation 2.5 where year takes the value of one for endline 2 and zero for endline 1. In all regressions, Treatment received takes the value of one if the wife received the intervention and zero otherwise. Controls include baseline characteristics, such as respondents' age and income, household income, financial assets, livestock ownership, land ownership and food security. We also use district fixed effects, and standard errors are in parentheses and clustered at the branch level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.F Enumerator gender difference across endlines

As mentioned earlier, there is a difference in the gender of enumerators across the two endlines, with male enumerators collecting data on mediators in endline 1 and female enumerators in endline 2. This raises the possibility that the observed difference in programme impacts between the short and long run may be partly attributable to enumerator gender, rather than reflecting a true fading of the intervention's effects. Enumerator gender is constant across treatment and control groups within each endline, so within-endline treatment–control comparisons are not confounded by interviewer variation.

The short-run increase in women's income could partly reflect reporting differences linked to enumerator gender. One possibility is that women in the treatment group might have gained confidence and empowerment through the programme and thereby felt more willing to disclose their income to male enumerators in endline 1, while women in the control group might have felt less comfortable doing so. However, we cannot test these mechanisms with our data. Without within-round variation in enumerator gender, we cannot determine whether changes by endline 2 reflect comfort with interviewer gender or true treatment-control gap. These mechanisms are untestable in our data. If empowered women were indeed more confident, they would have been equally willing to report their income to female enumerators. Alternatively, women in the treatment group might have concealed their increased income from male enumerators at endline 1 due to social pressure or fear of judgment. However, if this were the case, we would expect to observe a smaller treatment effect in the short run, which is not supported by our findings. Testing these mechanisms is not possible because enumerator gender does not vary within rounds for mediators.

Existing evidence on interviewer gender effects supports caution in this interpretation. While substantial effects have been documented for highly sensitive issues, such as IPV, sexual behaviour, fertility decisions, or beliefs and experiences related to sensitive gender norms (see, e.g., Haber et al., 2018; Harling et al., 2019; Kadam et al., 2025), the effect on income reporting remains limited. Thus, while reporting biases due to the enumerator's gender cannot be ruled out, any effect on income might be smaller than for more sensitive topics such as IPV.

Since the enumerator's gender was consistent within each round of our study, any reporting bias would affect the treatment and control groups equally within each round. Moreover, we observe a similar pattern of declining treatment effects over time for other mediators, such as livestock ownership and labour supply (i.e., working hours). These variables are arguably less sensitive to enumerator gender, which suggests that lower treatment effects on these variables in the long run than in the short run might reflect real changes rather than reporting bias.

CHAPTER 3

Intimate Partner Violence and Women's Risk and Time Preferences

Abstract[1]

Globally, more than a quarter of ever-partnered women aged 15-49 years experience intimate partner violence (IPV) in their lifetime (WHO, 2021). IPV, beyond physical and psychological harm, affects women's economic decisions. An underexplored mechanism works via the potential influence of IPV on women's risk and time preferences, which shape critical economic decisions such as savings, labour market participation, and investments. To examine this channel, we conduct an experiment with 901 married women in Bangladesh, priming IPV through a video showing a woman experiencing physical abuse by her husband in front of her children. Randomising the order of the IPV priming module and the preference elicitation module in the private interviews, we identify the effect of IPV priming on women's risk and time preferences. We find that IPV priming reduces risk aversion among women, regardless of whether they experienced IPV. Among women who experienced emotional IPV, IPV priming also decreases impatience, and it increases impatience among women who never experience emotional IPV. Guided by a theoretical model and supported by further analysis, we demonstrate that these effects work via negative emotions, including anger, fear and anxiety, triggered by the IPV priming.

3.1 Introduction

More than one-fourth (27%) of women aged 15-49 years who have been in a relationship experience intimate partner violence (IPV) at least once in their lifetime (WHO, 2021).[2] Regional analysis of the prevalence of IPV shows that it ranges from 16% (Southern Europe) to 51% (Melanesia). The other regions with high rates of IPV include Micronesia (41%), Polynesia (39%), South Asia (35%) and Sub-Saharan Africa (33%) (WHO, 2021). In some countries, this rate exceeds 40%, and Bangladesh is one of them. The recent

[1]Acknowledgements: We thank BRAC Institute of Governance and Development (BIGD) and BRAC, Bangladesh, for invaluable support with the fieldwork. We acknowledge funding from BRAC and the German Development Bank (KfW) Climate Bridge Fund. We also thank Pieter Serneels for useful comments on earlier drafts, and participants at workshops at the University of East Anglia and Royal Holloway University of London. The study obtained ethical approval from the ethics committee of the University of East Anglia (ETH2223-1657) on 11 May 2023. For the pre-analysis plan, see `https://www.socialscienceregistry.org/trials/11488`

[2]IPV experience is defined as whether a woman has ever experienced physical and/or sexual violence in her lifetime.

survey on violence against women conducted in 2024 in Bangladesh reveals that more than half (54%) of ever-married women aged 15 and above experience physical and/or sexual IPV at least once in their lifetime (BBS, 2025).

In addition to the health consequences of IPV, its economic consequences are well-documented.[3] Women who experience IPV are more likely to have poor economic outcomes, as indicated by low labour force participation (Duvvury et al., 2013), job instability (Brown et al., 2024; Adams et al., 2012), reduced working hours (Tolman and Wang, 2005), and poor savings (Tankard, Paluck and Prentice, 2019; Shahriar and Alam, 2024). At the same time, a growing body of studies shows that attitudes towards risk and time are important predictors of economic behaviour. Specifically, risk preferences influence labour force participation, health choices, investment decisions and migration (Hong, Kubik and Stein, 2004; Bonin et al., 2007; Kimball, Sahm and Shapiro, 2008; Jaeger et al., 2010; Dohmen and Falk, 2011; Becker et al., 2012; Dawson and Henley, 2015; Hsieh, Parker and van Praag, 2017; Sunde and Becker, 2018)[4] and greater patience leads to higher investment in human capital (Golsteyn, Grönqvist and Lindahl, 2014; Cadena and Keys, 2015; van Huizen and Alessie, 2015; Falk et al., 2018) and larger savings (Horn and Kiss, 2020; Klawitter, Anderson and Gugerty, 2013; Hershey and Mowen, 2000; Jacobs-Lawson and Hershey, 2005).[5] Bringing both strands of literature together, we explore whether the negative economic impacts of IPV work via a change in women's risk and time preferences. If IPV influences economic preferences, which, in turn, worsen the economic conditions of women, this would be relevant for policymakers to design interventions, such as counselling to manage trauma, stress management trainings, psycho-social education about causes and consequences of IPV and cognitive therapy, to mitigate the impact of IPV on preferences.

To investigate this behavioural mechanism, we use a survey experiment with a sample of 901 currently married women from low-income households in Bangladesh, in which we include an 'IPV priming module'.[6] This module starts with a detailed set of questions on

[3] Women who experience IPV are more likely to suffer from chronic diseases, continued pain, increased risk of diabetes, infectious diseases, gastrointestinal disorders, and adverse pregnancy outcomes such as low birth weight, preterm birth, and intrauterine growth restriction (see, e.g., Stubbs and Szoeke, 2022; Campbell, 2002; Dillon et al., 2013; Hill et al., 2016).

[4] For instance, more risk-averse individuals are less likely to be self-employed and invest in stocks, and countries with higher aggregate risk aversion tend to have less productive labour forces (Dohmen and Falk, 2011; Falk et al., 2018).

[5] Both risk and time preferences also influence health choices. For example, greater risk aversion is less likely to be associated with smoking, heavy drinking, overweight/obesity, and seat-belt non-use (Barsky et al., 1997; Anderson and Mellor, 2008) and greater patience leads to better health outcomes, such as less smoking and lower obesity (Grossman, 2017; Komlos, Smith and Bogin, 2004; Smith, Bogin and Bishai, 2005; Ikeda, Kang and Ohtake, 2010).

[6] 'Priming' refers to a method that activates the thoughts or emotions associated with real-world events, which might influence a wide range of psychological and behavioural outcomes, including emotions, preferences and beliefs. In psychological and behavioural economics literature, 'priming' is drawing wider attention as an experimental technique to assess the impacts of contextual factors on preferences (see, e.g., Callen et al., 2014; Notaro, Mariel and Hadjichristidis, 2024; Anderberg et al., 2025).

IPV experiences, followed by a short video demonstrating a woman experiencing physical abuse by her husband in front of her children. We also use a 'preferences module', which elicits women's risk or time preferences. Randomising the order of the IPV priming module and the preference elicitation module at the individual level, we identify the effect of IPV priming on women's risk and time preferences. We also collected data on the emotional responses after watching the IPV video.

Our hypotheses are guided by the dual self model, which states that an individual's emotional state shapes one's economic preferences. The experience of IPV often comes with strong negative emotions, such as anger, fear, anxiety, depression and stress, which in turn might influence women's attitudes towards risk and time.[7] Our IPV priming is designed to put women in a mental state of thinking about IPV, regardless of whether they experienced IPV in real life and to trigger these negative emotional responses. Among these several negative emotions, anger, fear and anxiety might be core emotional responses to IPV, and evidence shows that these emotions might have a different influence on preferences. Therefore, our framework distinguishes two sets of emotions, anger and fear/anxiety, which tend to have opposite effects on preferences. Anger might increase women's willingness to take risks and willingness to wait for delayed reward, while fear and anxiety might reduce risk tolerance and patience (Carver and Harmon-Jones, 2009; Angus et al., 2015; Lerner and Keltner, 2001; Lerner et al., 2015; Lerner and Tiedens, 2006; Raghunathan and Pham, 1999; Song et al., 2021; Loewenstein, 2000; Callen et al., 2014; Haushofer and Fehr, 2014; Takahashi, 2004; Lerner et al., 2015). Given the opposite effects of these emotions on preferences, our first two hypotheses state that IPV-priming will change risk aversion and impatience. Our third hypothesis is that women with prior experience of IPV will experience greater changes in preferences.

Our results are summarised as follows. First, we find that IPV priming reduces risk aversion by 28% relative to the control mean, and this effect is not influenced by their prior experience of emotional or physical IPV. Second, we find that impatience decreases among women with emotional IPV, while it increases among women without these experiences. Third, we find that anger is more strongly triggered than fear and anxiety. This pattern explains the reduction in risk aversion, as outlined in our framework. Looking at the differences in emotional responses by IPV experiences, we observe that women with and without IPV experiences report a stronger feeling of anger than fear and anxiety, and the level of feeling of anger does not differ between these two groups. Moreover, women with IPV experiences reported stronger feelings of fear and anxiety compared to those without IPV experiences.

The similarity in reported anger might explain why both groups of women experience

[7]Women exposed to IPV experience a wide range of emotions, including sadness (Houry, Kaslow and Thompson, 2005), self-worthlessness (Houry, Kaslow and Thompson, 2005), fear (Anderson and Saunders, 2003), anxiety (Mapayi et al., 2013; Ahmadabadi et al., 2020; Lang, Kennedy and Stein, 2002; Stefania et al., 2023), anger (Kuijpers, van der Knaap and Winkel, 2012; Slep et al., 2021; Swan et al., 2005; Hamberger and Guse, 2002), depression (Mechanic, Weaver and Resick, 2008; Mapayi et al., 2013; Ahmadabadi et al., 2020), post-traumatic stress (Golding, 1999; Shoener, 2008).

a similar reduction in risk aversion resulting from the priming. However, anger triggered by the priming does not appear to mediate the differential impacts of the priming on time preferences across IPV experiences. Rather, the differences in reported fear and anxiety between women with and without IPV experiences might explain the differential impact on impatience across women with and without emotional IPV experiences.

Our paper is most closely related to the literature that demonstrates the impacts of conflict, such as civil wars, ethnic cleansing, post-election crisis and drug war, can change risk and time preferences (see e.g., Voors et al., 2012; Imas, Kuhn and Mironova, 2015).[8] These studies find that people exposed to violence are more likely to be risk-averse in different contexts (Callen et al., 2014; Fatas et al., 2021; Moya, 2018; Jakiela and Ozier, 2019; Nasir, Rockmore and Tan, 2020; Brown et al., 2019). An exception is Voors et al. (2012), who find that exposure to violence increases risk-taking.[9] Our finding that IPV priming reduces risk aversion is in line with this evidence from Voors et al. (2012). Furthermore, the literature on time preferences suggests that people exposed to violence value the present more (Voors et al., 2012; Imas, Kuhn and Mironova, 2015).

Our study makes two major contributions to the literature. First, our study contributes to this strand of literature by examining the impact of the IPV priming on women's risk and time preferences that, to the best of our knowledge, have not been analysed together in the context of IPV. In contrast to existing work, which largely focuses on community-level violence, we find that exposure to an IPV-priming module reduces both risk aversion and impatience, suggesting that the impact of violence on preferences varies by the type of violence. Second, we investigate emotional responses as a potential mechanism through which IPV priming might change preferences. This mechanism has not been tested before in this context.

The rest of the paper is structured as follows. Section 3.2 describes the design. Section 3.3 describes the data, and Section 3.4 presents the main effects. Section 3.5 reports some robustness checks, while Section 3.6 examines the heterogeneity of the effects. Section 3.7 discusses the role of emotions as a mechanism. Finally, Section 3.8 discusses our findings and concludes the paper.

3.2 Design

In this section, we present the survey experiment that allows us to identify the impact of IPV priming on women's risk and time preferences, followed by the hypotheses and the identification strategy.

[8] It is worth noting that other exogenous shocks, such as economic distress and natural calamities, can influence risk and time preferences (see e.g., Schildberg-Hörisch, 2018; Haushofer and Fehr, 2014; Voors et al., 2012; Imas, Kuhn and Mironova, 2015).

[9] Rockmore and Barrett (2022) highlight the disaggregated impacts of violence by the nature of violence on risk-taking. They find that experiencing violence makes people risk-lovers, while witnessing or perpetrating violence leads to risk aversion.

3.2.1 Structure of the experiment

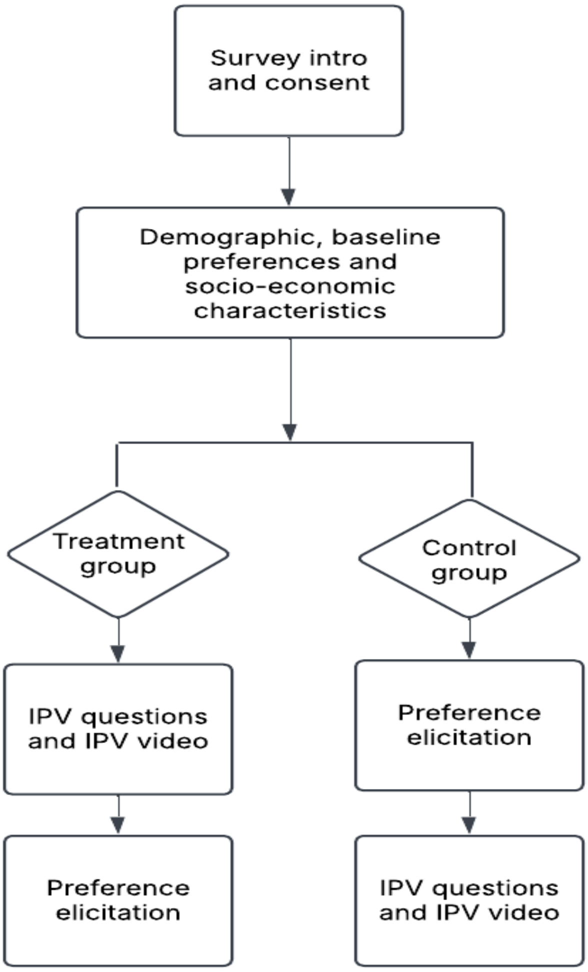
Figure 3.1 presents the structure of the survey experiment. Each interview started with a short intro and a request to give consent to participate in the study, followed by a module that captures demographic characteristics and the employment status of each household member.[10] Next, we elicited women's risk and time preferences for the first time, referred to as baseline preferences. Then, we asked questions about housing (housing structure, sanitation and drinking sources), household asset ownership and climate change (questions on how climate change is affecting the household and their coping mechanisms).[11]

Next, we implemented the IPV priming module, and we elicited women's preferences for a second time. The order of the priming and preferences modules was randomised at the individual level. We randomly assigned women to one of two experimental arms. Women who went through the IPV module before the preferences module are referred to as the 'treatment group', while women who first went through the preferences module before the IPV priming module are referred to as the 'control group'.

[10]Our study was registered with 'The American Economic Association's registry for randomised controlled trials' in June 2023. The pre-analysis plan can be accessed here: `https://www.socialscienceregistry.org/trials/11488`. In our pre-analysis plan, we outlined four sets of preferences, including risk, time, social and intra-household. This paper focuses on risk and time preferences, while the remaining preferences – for which we used a different sample – are analysed in a separate paper.

[11]As mentioned earlier, our experiment was embedded in the baseline survey of BRAC's climate change programme. The climate change module was designed to assess the exposure of these households to climate change and their coping mechanism prior to the BRAC's intervention.

Figure 3.1 Structure of the experiment



The 'IPV priming' module began with a detailed set of questions on IPV experiences, adapted from a national survey on violence against women in Bangladesh (BBS, 2016). After these questions, the respondent watched a short video on the enumerator's tablet that demonstrates a woman experiencing physical abuse by her husband in front of her children.[12] Particularly, we asked women whether they experienced different forms of (physical/emotional/sexual) IPV in their lifetime and in the last 12 months. This information helps identify groups of women who experienced IPV in real-life and who did not (for more details, see the different types of IPV in Table 3.A.1).[13]

The video was developed by the WE CAN Bangladesh, a local organisation that aims to make women aware of IPV and available support to fight against violence against women

[12]We showed the video after completing the questions on past IPV experience, as showing it beforehand could have biased women's reporting of IPV.

[13]The disaggregated analysis of the impacts by IPV experiences includes physical and emotional IPV because the prevalence of these forms of IPV is higher than that of sexual IPV, in Bangladesh. According to the national survey conducted in 2024, 47% and 33% reported experiencing physical abuse and emotional abuse, respectively, while 29% of ever-married women reported experiencing any sexual IPV in their lifetime (BBS, 2025). Similarly, in our sample, 25% of respondents ever experienced sexual IPV, whereas this rate is higher for physical and emotional IPV (52% and 47%).

in Bangladesh.[14] After showing the video, participants were asked a set of debriefing questions to capture to what extent the video made them angry, fearful or anxious.

To limit the duration of the survey, each woman was randomly assigned to only one of the preference modules: either the risk preferences module or the time preferences module. This created two random subsamples, each having both treatment and control groups with individual-level randomisation.[15]

3.2.2 Preference elicitation

We elicited risk preferences in two ways. First, women were asked to report their willingness to take risks on a scale of 0-10, with 10 referring to the highest willingness (hereafter referred to as a scale measure). We inverted the scale of the responses to this question to measure risk aversion, where 10 refers to the highest risk aversion. Second, we also asked a series of hypothetical questions, following a staircase method (hereafter referred to as a staircase measure). Specifically, women were asked to make five subsequent choices between a lottery and a sure payment that slightly exceeds the expected value of the lottery. The sure amount changes in each question, depending on the choices made in the previous questions, following a so-called titration procedure (for details, see the questions, R1 and R2, presented in Table 3.B.1 and amounts presented in Figure 3.B.1). We calculated an interval for the risk aversion parameter of each woman, using the constant relative risk aversion (CRRA) utility function (see details in Section 3.B.1).

Time preferences were elicited in the same way. First, women were asked to assess their willingness to wait for a return in the future on a scale of 0-10, with 10 referring to the highest patience. We inverted the scale of the responses to this question to measure women's impatience, where 10 refers to the highest impatience level. Second, we also asked a series of hypothetical questions following a staircase method. Specifically, we asked them to make five subsequent choices between an amount in the present and a slightly higher amount in the future. The amount in the future changes in each question, depending on the choices made in the previous questions (for details, see the questions, T1 and T2, presented in Table 3.B.1 and amounts presented in Figure 3.B.2). We calculated an interval for the discount rate of each woman using the exponential discounting method, following D'Exelle, Van Campenhout and Lecoutere (2012) (see details in Section 3.B.2).

We elicited preferences using the scale measure twice: first, at the beginning of the survey to elicit baseline preferences and second, after randomisation ('before' the IPV priming module for the control group and 'after' it for the treatment group) to estimate

[14] The video is available here `https://www.youtube.com/watch?v=pGvY3e6t-vE&t=1s&ab_channel=wecancampaign`.

[15] We first generated a set of random numbers, and assigned them randomly and equally to four groups: treatment and control groups within each of the subsamples. These random numbers were then randomly distributed to the enumerators, who then assigned these numbers to the women being interviewed. Enumerators were unaware of the treatment status assigned to each number. They only knew the random numbers. Based on the assigned number, the relevant modules automatically showed up on the survey device.

the impact of IPV priming. After randomisation, we also used the staircase measure.

3.2.3 Implementation

We conducted this survey experiment with currently married (living with their husbands) women from poor households in Bangladesh. In this country, the IPV rate is quite high at 54% (BBS, 2025).[16] In June-July, 2023, we did this experiment in collaboration with our local partners - BRAC Institute of Governance and Development (BIGD) and BRAC.[17] This experiment was embedded with a baseline survey on BRAC's climate change programme. BRAC designed a comprehensive climate change resilience programme consisting of four interventions in rural and urban slums. BRAC identified those districts which are highly affected by climate change. Afterwards, from these selected districts, eligible individuals were selected for the interventions based on specific selection criteria.[18] After BRAC's selection, BIGD randomly drew a sample of respondents to conduct the baseline survey prior to offering any support package from BRAC. This pool of eligible individuals includes both women and men, depending on the type of programme (see 'Target' column of Table 3.C.1). However, our experiment was conducted only with the currently married women (living with their husbands) from this pool.[19]

3.2.4 Hypotheses

To develop hypotheses on the impact of IPV priming on women's risk and time preferences, we draw on the 'dual self' model developed by Fudenberg and Levine (2006). This model assumes that preferences depend on one's emotional state and hence underpins the main mechanism through which we expect women's risk and time preferences to be influenced by IPV priming. The dual self model assumes that individual economic preferences depend on one's emotional state. In the 'cool self' state, one has baseline preferences in the absence of emotions, while in the 'hot self' state, emotions might alter preferences. Formally, we assume a woman has the following aggregate utility function:

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[16] In 2024, 54% of ever-married women aged 15 and above experienced physical and/or sexual IPV at least once in their lifetime. 69.6% of ever-married women experienced one or more forms of violence (i.e., physical violence, sexual violence, economic violence, emotional violence, and controlling behaviour) by their husbands at least once in their lifetime, and 54.7% experienced violence during the last 12 months. The latest Bangladesh Violence Against Women survey was conducted in 2024.

[17] BIGD is a social science research and academic institution in Bangladesh with expertise in the field of gender, economics, environment and climate change, governance and politics. BRAC, a large international organisation, has been implementing several development programmes in Bangladesh to tackle development issues, including poverty, climate migration, and violence.

[18] The beneficiary selection criteria vary across these programmes (see Table 3.C.1 in the appendix); however, the basic participant selection criteria are similar across programmes. These criteria include (i) economic vulnerability, (ii) climate change returnee migrants at risk of replacement or climate change vulnerable migrants, and (iii) the COVID-19-induced new poor who fell below the poverty line because of this pandemic.

[19] BRAC selected one eligible member from one household to avoid support duplication.

$$U(a; m, \beta) = \big(1 - \beta(m)\big) \cdot u^{\text{cool}}(a) + \beta(m) \cdot u^{\text{hot}}(a; m)$$

with u^{cool} being the utility in the 'cool self' state and u^{hot} being the utility in the 'hot self' state, m being the type of emotion, and $\beta(m) \in [0, 1]$ being the weight given to the hot self utility function. $a \in [0, 1]$ is the woman's decision variable, which a woman uses to maximise their utility U.

We assume that u^{cool} and u^{hot} differ in terms of risk preferences, as captured by the curvature of the utility function (with stronger concavity for higher risk aversion), and in terms of time preferences, the utility functions can be thought to capture the aggregate utility across different time periods (with a larger discount rate for higher impatience). Patient individuals give greater weights to future periods, while impatient individuals give lower weights to them.

How u^{cool} and u^{hot} are exactly compared depends on the emotion that is triggered by the IPV priming. We expect IPV priming to trigger three emotions. First, it may make women feel deep *anger* due to helplessness, repeated injustice, and the lack of support from their community, the legal system, or even their own family to prevent IPV. Second, it may also induce *anxiety* about their own and their children's future. Third, it can trigger *fear* of physical insecurity, retaliation by the perpetrator, and financial instability.

While these three emotions increase the weight given to the hot state, their impact on u^{hot} might vary. Specifically, evidence from existing studies shows that emotions influence risk aversion and impatience, but that the direction of the effect depends on the type of emotion. Specifically, we expect that the effect of anger is opposite to the effect of fear and anxiety, because anger motivates people to take actions, while fear and anxiety motivate people to avoid situations (Carver and Harmon-Jones, 2009; Angus et al., 2015). Therefore, we outline two scenarios: anger-driven and fear/anxiety-driven changes in risk aversion and impatience.

Regarding risk preferences, anger tends to make individuals less risk-averse and more willing to accept uncertain outcomes, whereas fear or anxiety increases risk aversion and supports a preference for a safer option (Lerner and Keltner, 2001; Lerner et al., 2015; Lerner and Tiedens, 2006; Raghunathan and Pham, 1999; Carver and Harmon-Jones, 2009; Angus et al., 2015; Wake, Wormwood and Satpute, 2020; Callen et al., 2014). In our model, the type of emotion, m, influences the curvature of the utility function of risk preferences. If the priming triggers anger, the utility function in the 'hot self' state will be less concave than that in the 'cool self' state. In contrast, if the priming triggers fear and anxiety, the concavity of the utility curve will be larger in the 'hot self' state compared to that in the 'cool self' state.

A similar pattern holds for time preferences. Regarding time preferences, existing studies show that when individuals feel anger, they become less impatient and more willing to wait for future gains (i.e., delayed reward) (Lerner and Tiedens, 2006; Song et al., 2021; Lerner et al., 2015; Loewenstein, 2000). Conversely, when individuals experience fear or anxiety, they become more impatient and prefer more immediate options (Callen et al., 2014; Haushofer and Fehr, 2014; Takahashi, 2004; Lerner et al., 2015). In our model, the type of emotion, m, influences the discount weights assigned to future periods for time preferences. If anger is triggered by the priming, lower discount weights will be given to future periods in the 'hot self' state than those in the 'cool self' state. If the priming triggers fear and anxiety, higher discount weights will be given to the future periods in the 'hot self' state than in the 'cool self' state.

The ultimate effect of an emotion on risk and time preferences depends on two elements: (i) how much the emotion changes the utility of the hot self state (in terms of its concavity and inherent discount rate), and (ii) how much the emotion changes the weight given to the hot self state. As we do not know how much the emotion will change these two elements, we cannot predict the exact size of the effect. Consequently, as IPV priming triggers both anger and anxiety/fear, which both have opposing effects, and we do not know which effect is strongest, the net effect of IPV priming on risk or time preferences cannot be predicted. Accordingly, our hypotheses specify only that preferences will change, without indicating whether risk aversion or impatience will increase or decrease. Our first two hypotheses are that IPV priming will affect risk aversion (hypothesis 1) and impatience (hypothesis 2) among women.

We also expect the effects of the priming would vary with women's real-life IPV experiences. Two mechanisms are plausible. Women who have experienced IPV might react more strongly because the IPV priming module acts as a trauma reminder. Psychological studies on trauma recollection provide evidence supporting this mechanism.[20] Alternatively, due to repeated exposure to IPV, women with a prior history of IPV might be habituated to IPV, resulting in weaker emotional reactions among them. We expect that the IPV-priming module we used will act as a trauma reminder for women with prior IPV experiences. We further assume that the latter mechanism, being habituated to IPV, might reduce the effect of trauma; however, it might not fully nullify this effect. Therefore, our hypothesis 3 is that women with prior IPV experiences will show stronger emotional reactions and, in turn, will have larger changes in risk aversion and impatience compared to women without such experiences.

[20]Particularly, several studies show significant differences in physiological and psychological reactions to trauma reminder (e.g., script-driven imagery) between people with post-traumatic stress disorder (PTSD) and those without PTSD. For instance, when trauma is cued with short, personalised scripts or images, adults with PTSD show larger autonomic responses, especially faster increases in heart rate, compared to trauma-exposed controls without PTSD (see (Lindauer et al., 2006; Pitman et al., 1987; Pole, 2007)). Studies on brain scans during trauma recall show similar evidence. Among the PTSD group, systems that detect threat are more active, and systems that control emotions are less active, so trauma recall tends to produce stronger emotional reactions among people with PTSD compared to people without PTSD (Sartory et al., 2013). Furthermore, evidence on self-reported emotional response to trauma recollection also shows a similar pattern. During trauma scripts, PTSD group reports more fear/anxiety than non-PTSD controls (see Shin et al., 1999).

3.2.5 Identification Strategy

To estimate the impact of the IPV priming module on women's risk and time preferences (hypotheses 1 and 2), we use the following equation, which will be estimated with Ordinary Least Squares (OLS):

$$y_i = \alpha + X_i.\beta + M_i.\Theta + \gamma_p + \lambda_e + \epsilon_i \tag{3.1}$$

where y_i is the (risk/time) preference of woman i. X_i is the treatment variable, which is a binary variable taking a value of 1 for treatment and 0 for control. M_i is a set of control variables that includes the woman's and her husband's characteristics and their household wealth. γ_p is the BRAC programme fixed effects. Since BRAC selected these women to provide different support packages after this survey to make them climate change resilient, we use programme (i.e., support package) fixed effects to control for unobservable factors of these selected women. λ_e denotes the enumerator fixed effects. We use the enumerator fixed effects to control for enumerators' unobservable characteristics. ϵ_i is the error term.

Since the staircase measures of preferences take ranges instead of continuous values, we use 'interval regression' to estimate the impact on these outcomes. The key reason is that the interval regression deals with all types of censoring (D'Exelle, Van Campenhout and Lecoutere, 2012), while OLS would provide inconsistent estimates for a censored variable.[21]

Further, to estimate whether the impacts of the IPV priming module vary across real-life IPV experiences (hypothesis 3), we examine these impacts among those who experienced IPV and those who did not. We test this hypothesis for two forms of IPV, emotional and physical. We run the following regression to test the heterogeneity of treatment effects across IPV experience.

$$y_i = \alpha + X_i.\beta_1 + I_i.\beta_2 + X_i.I_i.\beta_3 + M_i.\Theta + \gamma_p + \lambda_e + \epsilon_i \tag{3.2}$$

where I_i is IPV experience. It takes the value of one if the woman experienced IPV ever in their lifetime and zero otherwise. The sum of β_1 and β_3 from equation 3.2 captures the impact of the priming of IPV among those women who ever experienced IPV. Meanwhile, β_1 captures the impact of the priming of IPV among those women who never experience IPV. We distinguish this analysis across emotional and physical IPV experiences. We also follow the same equation to do heterogeneity analysis by age and education groups.

[21] If one of the bounds of the interval is missing, interval regression interprets them as censored data. For instance, our 1st interval for the measure of time preferences is greater than 6.57 Table 3.B.2, so interval regression treats it as right-censored data. And, our 32nd interval is lower than 0.25, which is treated as left-censored data in the interval regression.

3.3 Sample

We interviewed a sample of 901 women, of whom 442 were randomly assigned to the risk preference module and 459 to the time preference module.[22] The risk subsample includes 226 treatment and 216 control women, whereas the time subsample consists of 231 treatment and 228 control women. The data collection was conducted from June to July 2023. Our IPV priming module included detailed questions about IPV experiences, which are sensitive in nature. Consequently, we followed strict ethical protocols when asking these questions (see details in Section 3.D).

A common concern in an experimental setting is the potential for an experimental demand effect, where respondents might report their behaviour based on what they think the researchers want. For instance, in our case, if the IPV priming module had provided a message or instructed respondents on how to respond to the preferences module, it would have created experimental demand effects. Our study mitigates such effects in two ways. First, the content of the IPV priming module has no connection with risk or time preferences. Second, since this experiment was part of a baseline survey, we could frame it as such without revealing the objective of our experiment to respondents and enumerators. Furthermore, without knowing the hypotheses, it would be difficult for both respondents and enumerators to predict the expected direction of change in preferences.

Table 3.E.1 shows some descriptive statistics of our sample. The average age of women in the sample is 42 years, whereas the average age of their husbands is 49.5 years. Both spouses have about 3 years of schooling, and the marriage length is about 25 years on average. The average cultivable land ownership is 7.39 decimals (which is equivalent to 0.0739 acres). For livestock, 47% of households own poultry, 26% own cows, and 18% own goats. The ownership rates for other business assets are lower, with 12% owning a van or other vehicles, 18% owning a sewing machine, and 15% owning fishing nets. Notably, business assets are those which are used for income-generating activities.

For descriptive statistics, we categorise IPV experiences as follows: (i) 'Recent IPV' group includes those women who experienced IPV in the last 12 months prior to the survey, (ii) 'Distant IPV' are those women who experienced IPV but not recently (i.e., with distant IPV experience) and (iii) 'Never IPV' are those women who never experienced IPV in their lifetime. Looking at IPV experience among our sample, we find that 47% of women reported experience of emotional IPV (28.97% recent and 18.09% distant), 52% of women reported physical IPV (19.64% recent and 32.19% distant), and 25% of women reported sexual IPV experiences (13.65% recent and 11.88% distant). Furthermore, about half of the women we surveyed have ever experienced IPV in their lifetime.

[22]Since this experiment was conducted with only currently married women (living with their husbands) from the baseline sample, we could not identify in advance how many women would be eligible. Therefore, we generated random numbers for the entire baseline sample and assigned them equally across all groups. However, only married women were interviewed for this experiment. The subset of random numbers belonging to married women did not split perfectly evenly across groups, resulting in module sample sizes that differ by 4–5% by chance.

Next, we test whether treatment and control groups are balanced for each of the two subsamples. Section 3.F shows all balancing tests between treatment and control groups. Specifically, we check whether the treatment and control groups are homogeneous in terms of demographic and economic characteristics. Tables 3.F.1 and 3.F.2 report the results for risk and time subsamples, respectively. Columns 1 and 2 show the average values for control and treatment groups, and column 3 reports the difference between these groups. Since we use a long list of characteristics for the balancing test between the treatment and control groups, the difference between the treatment and control groups might be significant for some of these variables by chance. However, a joint orthogonality test obtained by regressing all pre-experiment characteristics on the treatment variable can be an alternative to conducting a t-test for each characteristic (McKenzie, 2015).[23] The p-value of a joint orthogonality test (results reported in Tables 3.F.1 and 3.F.2) confirm that women's demographic and economic characteristics are not systematically correlated with treatment status for both risk and time subsamples.

Furthermore, we examine whether the order of the preferences and IPV priming modules influences the reporting of IPV experiences. To assess the disaggregated treatment effects by IPV experiences, the reporting of IPV should be similar between treatment and control groups to have unbiased estimates of heterogeneity analysis because a non-random measurement error leads to the issue of endogeneity (Bound, Brown and Mathiowetz, 2001). Our concern was that there might be an order effect on IPV reporting. Therefore, we test whether the reporting of each form of IPV differs between the treatment and control groups, across each subsample.

Table 3.F.3 shows no significant differences in the reporting of any form of IPV between treatment and control groups across both risk and time subsamples, indicating that the IPV reporting does not differ by whether preferences are elicited before or after asking about real-life IPV experiences. The p-values of the joint orthogonality test are large enough to confirm that IPV reporting of treatment and control groups is similar on average for both subsamples. These results also confirm that the proportions of women in different groups of IPV (recent/distant/never) are similar in the treatment and control groups.

[23] Pre-experimental characteristics are those indicators that have been collected before administering both IPV and preferences modules.

3.4 Impact estimates

Table 3.1 shows the impact of IPV priming on women's risk and time preferences.[24] Starting with the aggregate impact on risk preferences (Panel A), we observe that IPV priming decreases risk aversion; however, the impact is statistically significant only for the scale measure of risk aversion (Column 1). The priming reduces risk aversion by 0.757 points (on a scale of 0-10 with higher scores indicating greater risk aversion), which is about 28% relative to the control mean of 2.696 points (on the same scale). The impact on the staircase measure of risk aversion is also negative; however, it is not statistically significant. In terms of time preferences, we observe no significant impact of the IPV priming module on impatience, irrespective of the measurement approach.

Panel B of Table 3.1 shows results for emotional IPV groups, while Panel C reports results for physical IPV groups. The disaggregated analysis on risk preferences among emotional IPV groups shows that the priming decreases risk aversion among both groups of women, those who have ever and those who never experienced emotional IPV, by 0.813 and 0.707 points (on a scale of 0-10), respectively. However, the difference between impacts among women with prior experience of emotional IPV and those who never experienced emotional IPV is larger but not significant. On the other hand, the disaggregated analysis on time preferences (reported in columns 3 and 4 of Panel B) shows that IPV priming has opposite effects on time preferences across these groups. In particular, IPV priming reduces impatience among women with prior emotional IPV experience by 1.424 points (on a scale of 0-10, with higher scores indicating greater impatience), while it increases impatience among those who never experienced emotional IPV by 1.236 (on the same scale). The difference between these impacts is statistically significant. Among physical IPV groups (Panel C), we observe similar patterns for risk preferences, with the priming reducing risk aversion across both groups. However, unlike emotional IPV groups, there is no significant impact on impatience among physical IPV groups, indicating that IPV priming influences time preferences differently depending on the form of IPV experienced.

[24]It is important to note that we have non-response cases across all measures, resulting in fewer observations in this table compared to those presented in Tables 3.F.1 and 3.F.2 . Furthermore, there are more non-response cases for the staircase measure (Column 2 of Table 3.1) than for the scale measure of risk aversion (Column 1). Additionally, we exclude those observations which fall into the last two combinations of risk mapping (see Table 3.B.1) to exclude negative risk aversion parameters from analysis. As the sure amount is higher than the highest value of the lottery for the upper (lower) bound of the 31st (32nd) combination, the risk aversion parameter becomes negative using the constant relative risk aversion (CRRA) utility function (equation 3.6). If someone is extremely risk-loving, possibly he/she could choose a lottery even when a sure amount is higher than the winning amount of the lottery, resulting in a negative risk aversion parameter. Although mathematically conceivable, such choices are logically inconsistent with rational behaviour. Consequently, we exclude those respondents who selected these combinations from the analysis.

Table 3.1 Impact of IPV priming on women's preferences

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Panel (A): Aggregate impact				
Impact	-0.757***	-0.270	0.0381	-0.0963
	(0.281)	(0.412)	(0.272)	(0.495)
Mean of control	2.696	2.855	2.978	4.388
Panel (B): Disaggregate impact by prior emotional IPV				
Impact (Ever)	-0.813**	-0.214	-0.600	-1.424**
	(0.413)	(0.587)	(0.377)	(0.704)
Impact (Never)	-0.707*	-0.453	0.662*	1.236*
	(0.388)	(0.614)	(0.377)	(0.678)
Impact diff (Ever vs Never)	-0.107	0.238	-1.263**	-2.660***
	(0.571)	(0.866)	(0.525)	(0.979)
Panel (C): Disaggregate impact by prior physical IPV				
Impact (Ever)	-0.813**	0.143	-0.241	-0.799
	(0.393)	(0.548)	(0.379)	(0.694)
Impact (Never)	-0.694*	-0.782	0.361	0.695
	(0.410)	(0.663)	(0.395)	(0.727)
Impact diff (Ever vs Never)	-0.119	0.925	-0.602	-1.493
	(0.574)	(0.878)	(0.550)	(1.019)
Observations	436	390	455	455

Notes: Panel A: the impact of the IPV priming module on average; Panel B (C): the impacts among women with ever experience of emotional (physical) IPV, women who never experienced emotional (physical) IPV and the difference in the impacts of these two groups. The original OLS regressions for panels B and C are reported in Table 3.G.1 . It is worth noting that 47% of women reported that they had ever experienced emotional IPV, while the rate is 52% for physical IPV. Columns 1 and 3: Ordinary Least Squares (OLS) regressions with the following preference outcomes: risk aversion and impatience measured on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks) and impatience (i.e., lowest willingness to wait for future), respectively. Columns 2 and 4: Interval regression with the latent dependent variable that measures risk aversion in terms of risk aversion parameter and impatience in terms of discount rate. Table 3.B.1 in Section 3.B includes a detailed explanation of these outcomes. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.5 Robustness checks

This section presents the robustness tests for our results. We conduct a sensitivity test for baseline preferences, a check for time-related bias in preference elicitation and use an alternative regression specification. We also argue why potential measurement error in IPV reporting and timing of IPV experiences in real life should not create any bias in the disaggregated impacts across women's real-life IPV experiences.

3.5.1 Sensitivity to baseline preferences

A potential concern in experiments is that pre-existing differences in outcomes can influence the impact of the treatment on these outcomes. In our case, if the treatment and control groups have different preferences prior to the experiment (i.e., baseline preferences), this difference would bias our results. The observed effect of treatment on preferences could be attributed to baseline differences rather than the priming itself. We observe small differences in baseline preferences between the treatment and control groups (see Tables 3.F.1 and 3.F.2). As these differences are not statistically significant, they are less likely to influence our estimates. However, we control for baseline preferences to improve the precision of the estimates by reducing unexplained variation in our outcomes. To do this, we conduct an analysis of covariance (ANCOVA), which controls for baseline preferences, adjusting for differences (even the smallest ones) between groups (Table 3.2 reports the results).

We observe that IPV priming decreases risk aversion even after controlling for baseline risk preferences, which is consistent with the main findings reported in Panel A of Table 3.1 . Consistently, we do not find any significant impact on impatience after controlling for baseline time preferences. These results confirm that our results are robust to the baseline preferences.[25]

Table 3.2 Impact of IPV priming on women's preferences controlling for baseline preferences

	(1) Risk aversion (scale)	(2) Impatience (scale)
Impact	-0.445**	0.0929
	(0.193)	(0.183)
Observations	434	451
Mean of control	2.696	2.978

Notes: OLS regressions with the following preference outcomes: risk aversion and impatience measured on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks) and impatience (i.e., lowest willingness to wait for future). Table 3.B.1 in Section 3.B includes a detailed explanation of these outcomes. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. We also control for baseline preferences. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[25] We elicited baseline preferences only using the scale measures. Therefore, we could not conduct ANCOVA for the staircase measures.

3.5.2 Ordered logit model

We further assess the robustness of our findings by using an ordered logit regression as an alternative to the linear (OLS) model. This approach accounts for the ordinal nature of the scale measures of preferences. As the ordered logit requires ordinal dependent variables, for the staircase measure, we construct an ordinal version of risk aversion and impatience using the inverted linear ranks of all combinations presented in Figures 3.B.1 and 3.B.2 for risk and time preferences, respectively. For risk preferences, the ranks represent risk-taking attitudes, with 1 indicating the lowest risk-taking and 32 indicating the highest risk-taking. We invert these ranks to obtain a measure of risk aversion. For time preferences, the ranks represent patience, with 1 indicating the lowest patience and 32 indicating the highest patience. We invert these ranks to obtain a measure of impatience.

Table 3.3 Impact of IPV priming module on women's preferences (ordered logit)

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Impact	-0.554***	-0.171	0.00518	0.00896
	(0.201)	(0.225)	(0.220)	(0.193)
Observations	436	420	455	455

Notes: Ordered logit regression with the following preference outcomes: Columns 1 and 3: risk aversion and impatience measured on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks) and impatience (i.e., lowest willingness to wait for future), respectively. Columns 2 and 4: risk aversion and impatience measured by inverted ranks in the tree of risk (Figure 3.B.1) and time (Figure 3.B.2). Log-odds (coefficients from ordered logit regressions) are reported. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.5.3 Underreporting of IPV

A potential concern in interpreting the impact of IPV priming by IPV experience is reporting bias. IPV is often underreported due to stigma, fear of repercussion, social norms, or lack of trust in institutional support systems (Christopher et al., 2022; Lépine, Treibich and D'Exelle, 2020; Cullen, 2020). If a subset of women categorised as 'never experienced IPV' in our sample had in fact experienced IPV but chose not to report it, our estimated differences in impacts on preferences by real-life IPV experiences, reported in Panels B and C of Table 3.1 would have presented a lower bound of the true differences. For risk aversion, where we observe similar effects of IPV priming across women with and without IPV experiences, the true effect would be higher than the observed effect for women who ever experienced IPV, whereas for women who report never experiencing IPV, the true effect would be lower than the observed effect if IPV is underreported. Consequently, the true between-group difference in risk aversion would be higher than the observed difference. For time preferences, where IPV priming increases impatience among those who ever experienced and decreases it among those who never experienced, underreporting would make the true effects higher for both observed estimates. Accordingly, in the presence of underreporting, our observed between-group differences in impacts on preferences would represent a lower bound on the true differences.

3.5.4 Timing of IPV experiences

The timing of IPV experience might influence how women would react to the priming, which, in turn, might change the impacts on preferences. To assess this, we examine disaggregated impacts of IPV priming across three groups: 'Recent', 'Distant' and 'Never'.[26] Table 3.G.2 shows the disaggregated results.[27] We find that the priming decreases risk aversion among women who either recently experienced or never experienced emotional IPV, with a stronger effect observed among the recently experienced group. In contrast, the impact is not statistically significant among women with distant IPV experience. Consistently, we observe impacts on time preferences for the former two groups, but in opposite directions. The priming decreases impatience among those who have recent experiences of IPV and increases impatience among those who never experienced IPV. The difference between these impacts is statistically significant. These results suggest that the priming has weaker impacts on preferences among women with distant IPV experience compared to women with recent IPV experience and without IPV experience. The absence of significant effects of IPV priming among the former group of women might reflect the effects of cognitive adaptation or the normalisation of IPV over time (Nicholson and Lutz, 2017; Dare, Guadagno and Nicole Muscanell, 2013).

3.6 Heterogeneity

The impact of IPV priming may vary by women's demographic characteristics, age, education and level of baseline preferences. These characteristics shape how women perceive IPV and, in turn, how they would respond to the priming. Older and less educated women tend to hold more accepting attitudes towards IPV (Gunarathne et al., 2024; Wang, 2016), which might influence the impacts of IPV priming. Baseline preferences capture women's initial preferences that might influence the changes in preferences resulting from the priming. To examine disaggregated impacts across these factors, we first disaggregate the effects by women's age (younger versus older) and second, by their level of education (more versus less years of schooling). We also examine whether the impacts

[26]'Recent IPV' group includes those women who experienced IPV in the last 12 months prior to the survey, 'Distant IPV' are those women who experienced IPV but not recently and 'Never IPV' are those women who never experienced IPV in their lifetime.

[27]Table 3.E.1 shows the distribution of our sample across the three groups of emotional (physical) IPV experience. About half of our sample never experienced emotional or physical IPV.

vary across baseline preferences.[28]

3.6.1 Age and level of education

The impact of the IPV priming module may vary across women's demographic characteristics, particularly age and level of education. Panels A and B of Table 3.4 report the disaggregated results by age and level of education, respectively. Both age and education groups are defined using the sample's median age and years of schooling, respectively. Women who are older than 40 years are classified as 'older', and those who are 40 or less than 40 years old as 'younger', while women with more than three years of education are categorised as 'more educated', and those with three or less than three years as 'less educated'. The results show that IPV priming decreases risk aversion among younger women, and among both more and less educated women. However, we do not observe any significant impacts on impatience across these demographic groups.

[28]Importantly, our treatment and control groups are balanced in terms of age, education and baseline preferences (Tables 3.F.1 and 3.F.2).

Table 3.4 Impact of IPV priming on women's preferences, by women's demographic characteristics

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Panel (A): Age group				
Impact(Older)	-0.518	0.0597	0.254	-0.340
	(0.418)	(0.562)	(0.346)	(0.671)
Impact(Younger)	-1.009***	-0.574	-0.218	0.215
	(0.384)	(0.623)	(0.406)	(0.754)
Impact diff (Older vs Younger)	0.491	0.634	0.472	-0.556
	(0.573)	(0.864)	(0.524)	(1.021)
Observations	436	390	455	455
Panel (B): Education group				
Impact (More educated)	-0.686*	0.00545	-0.0450	0.705
	(0.395)	(0.554)	(0.385)	(0.712)
Impact (Less educated)	-0.799*	-0.563	0.118	-0.710
	(0.420)	(0.558)	(0.369)	(0.680)
Impact diff (More vs Less)	0.113	0.569	-0.163	1.415
	(0.598)	(0.742)	(0.522)	(0.976)
Observations	436	390	455	455

Notes: Panel (A): We report the impacts among older women, younger women and the difference in the impacts of these two groups. The original OLS regressions for this panel are reported in panel (A) of Table 3.H.1 . Impact (Older) reports the impact among older women (aged above 40 years old), which is the sum of coefficients of 'Treatment' and 'Treatment X Older' from the same panel. Impact (Younger) reports the impact among younger women (aged 40 or less than 40 years old), which is the coefficient of 'Treatment' from the same panel. Impact diff reports the difference in impacts for older and younger, which is the coefficient of 'Treatment X Older' from the same panel. Notably, we create age groups using median age of our respondents (i.e., 40 years) as the cut-off point. Panel (B): We report the impacts among more educated women, less educated women and the difference in the impacts of these two groups. The original OLS regressions for this panel are reported in panel (B) of Table 3.H.1 . Impact (More educated) reports the impact among more educated women (having more than 3 years of schooling), which is the sum of coefficients of 'Treatment' and 'Treatment X More educated' from the same panel. Impact (Less educated) reports the impact among less educated women (having 3 or less than 3 years of schooling), which is the coefficient of 'Treatment' from the same panel. Impact diff reports the difference in impacts for more and less educated, which is the coefficient of 'Treatment X More educated' from the same panel. Notably, we create education groups using median years of schooling of our respondents (i.e., 3 years) as the cut-off point. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and enumerator fixed effects. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.6.2 Baseline preferences

Women with different baseline preferences may respond to the priming in different ways. To examine this, we disaggregate the impacts on risk (time) preferences across baseline risk (time) preferences. Almost half of our respondents reported baseline risk aversion (impatience) greater than zero. Women with baseline preference of more than zero are categorised as 'higher baseline preference', and others as 'lower baseline preference'. Table 3.5 shows that the reduction in risk aversion of women with higher risk aversion at baseline is stronger than that for women with lower risk aversion at baseline, particularly when risk aversion is measured using the staircase. Regarding time preferences, we find that the priming makes women even more impatient compared to those with lower baseline impatience.

Table 3.5 Impact of the priming of IPV on women's preference by baseline preferences

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Impact (Higher baseline)	-0.559	-0.902*	0.631*	-0.393
	(0.386)	(0.494)	(0.353)	(0.678)
Impact (Lower baseline)	-0.253	0.503	-0.271	0.318
	(0.287)	(0.706)	(0.299)	(0.755)
Impact diff (Higher vs Lower)	-0.306	-1.405	0.902*	-0.780
	(0.483)	(0.874)	(0.469)	(1.019)
Observations	434	388	451	451

Notes: We report the impacts among women who had a level of risk aversion (impatience) more than zero and who had zero risk aversion (impatience) on a scale of 0-10 at baseline, and the difference in the impacts of these two groups. The original OLS regressions are reported in Table 3.H.2 . Impact (Higher baseline preference) reports the impact among women who had a level of risk aversion (impatience) more than zero, which is the sum of the coefficients of 'Treatment' and 'Treatment X Higher baseline preference'. Impact (Lower baseline preference) reports the impact among women who had zero risk aversion (impatience) at baseline, which is the coefficient of 'Treatment'. Impact diff reports the difference in impacts for higher and lower preference groups, which is the coefficient of 'Treatment X Higher baseline preference'. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.7 Mechanisms: the role of emotions

This section first examines i) whether the effects of IPV priming on risk and time preferences are mediated by emotions, as assumed in our theoretical model, and if yes, ii) which emotions are most strongly triggered by IPV priming. Second, we examine whether emotional responses to IPV priming vary by women's IPV experience. This tests the mechanism component of our hypothesis 3, which predicts that women with prior IPV experiences would show stronger emotional responses to the priming and, in turn, would lead to larger changes in their preferences. Furthermore, since we find that the effects of IPV priming on preferences vary by women's age, education, and baseline preferences, we examine whether emotional responses to the priming also vary by these characteristics.

3.7.1 Emotions triggered by the video

In Section 3.2.4, our model assumes that IPV priming triggers negative emotions that influence women's preferences. To test the emotions as mediating factors, we look at the distribution of self-reported emotional responses to the priming video. Following the video, we asked the women three questions to assess the extent to which the video triggered the feelings of anger, fear and anxiety. We asked three questions: whether this video made you (i) angry, (ii) fearful and (iii) anxious. There were four options to respond: not at all, a little bit, a lot and extremely. Figure 3.I.1 shows this distribution. The distribution illustrates that the priming triggered all three emotions, but to different degrees.

About 30% of the women reported feeling no anger or anxiety, whereas about 43% reported no fear in response to the video. Nevertheless, most of the women reported feeling some level of emotional response after watching the video. Among the two highest intensity responses (extremely or a lot), anger was reported as a stronger reaction to the video than fear and anxiety. A higher percentage of women reported feeling anger (a lot or extremely) after watching the video compared to fear and anxiety. This distribution confirms that priming triggers negative emotions, and anger is the most strongly triggered by IPV priming than fear and anxiety. As anger is more likely to make women more risk-seeking and patient, the stronger activation of anger by the priming might mediate the negative impacts of IPV priming on risk aversion and impatience. We further check whether emotions triggered by the video are similar among women who were primed and those who were not. Table 3.J.1 shows that none of the emotions triggered by the video differs between treatment and control groups. This confirms that emotions did not influence the preferences of the control group, as preferences were elicited before the IPV priming for this group and emotions were triggered after watching the priming video.

3.7.2 Emotions triggered by the video, by IPV groups

To examine whether these emotions triggered by the video mediate the impact of the IPV priming module on preferences across IPV experience groups, we compare self-reported emotional responses – particularly, anger, fear and anxiety – across IPV groups (i.e., 'Ever' and 'Never' experienced IPV) in the pooled sample. We hypothesise that women with prior experience of IPV might feel stronger emotions after watching the video, which, in turn, might trigger stronger impacts on preferences.

Table 3.6 reports the mean differences in feelings of anger, fear and anxiety after watching the video among IPV groups. The findings show that the level of feeling of anger is 2.66 (on a scale of 1-4) among women who never experienced emotional IPV, while the levels of feeling of fear and anxiety are 2.11 and 2.31 (on the same scale). Similarly, among women who ever experienced emotional IPV, the level of feeling of anger (2.72) triggered by the priming is higher than that of fear and anxiety (2.27 and 2.51). These patterns are similar for physical IPV. These results show that the feeling of anger was stronger than that of fear and anxiety across both IPV groups. Furthermore, the results indicate that women with and without prior experience of IPV were similar in terms of reporting anger after watching the video. We further observe that women with ever experiences of emotional IPV reported significantly higher feelings of fear and anxiety compared to those women without such experiences.

These results suggest that the video triggered anger at the same level among both groups, which, in turn, might reduce risk aversion similarly for these groups as previously documented in Table 3.1 . This finding is consistent with other studies that have documented how anger reduces risk aversion in laboratory settings (Lerner and Keltner, 2001; Lerner et al., 2015; Lerner and Tiedens, 2006; Raghunathan and Pham, 1999). For time preferences, however, anger does not seem to be the driving emotion. Instead, the priming triggers stronger fear and anxiety among women with IPV experiences compared to those without such experiences, which, in turn, might lead to a larger reduction in impatience among women with IPV experiences. Notably, this pattern is not consistent with the existing literature, which states that fear and anxiety make an individual more impatient (Callen et al., 2014; Haushofer and Fehr, 2014; Takahashi, 2004; Lerner et al., 2015).

Table 3.6 Emotions after watching video across IPV groups

	(1) Never	(2) Ever	(3) Difference (1)-(2)
Panel (A): Emotional IPV			
Anger	2.66	2.72	-0.06
	(1.23)	(1.27)	(0.08)
Fear	2.11	2.27	-0.16**
	(1.14)	(1.23)	(0.08)
Anxiety	2.31	2.51	-0.20***
	(1.10)	(1.15)	(0.08)
N	474	424	898
Panel (B): Physical IPV			
Anger	2.68	2.70	-0.02
	(1.25)	(1.25)	(0.08)
Fear	2.13	2.23	-0.09
	(1.17)	(1.20)	(0.08)
Anxiety	2.33	2.46	-0.13*
	(1.13)	(1.13)	(0.08)
N	431	467	898

Notes: Columns 1 and 2: Mean of emotions across IPV-experienced groups. 'Anger' ('fear'/'anxiety) takes the value on a scale of 1-4 with 1 referring to feeling no anger ('fear'/'anxiety) at all and 4 referring to feeling angry ('fear'/'anxiety) extremely. We categorise emotional (physical) IPV experiences into two groups: (i) 'Ever' are those women who ever experienced emotional (physical) IPV in their lifetime and 'Never' are those women who never experienced emotional (physical) IPV. Column 3 reports the difference in impacts among 'Ever' and 'Never' groups. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.7.3 Emotions triggered by the video, by age, education levels and baseline preferences

To understand whether emotional responses mediate the impact of the IPV priming module in the same way for all women or differently based on their characteristics, we examine how the emotions triggered by the video vary by women's age, level of education, and baseline preferences. Table 3.J.2 shows that the video triggers stronger emotions among younger and more educated women compared to their counterparts. This might explain the reduction in risk aversion among these groups (shown in Table 3.4). Older and less educated women are less likely to report weak emotions, which also explains the absence of impacts on the preferences of older women. However, this does not explain the significant impact on risk aversion among less educated women. Prior studies suggest that older and less educated women are more likely to hold positive attitudes towards IPV (Gunarathne et al., 2024; Wang, 2016), which might drive their weaker emotional responses to the video.

Table 3.J.3 further shows that women with lower baseline preferences report stronger emotional responses—particularly anger and fear—compared to women with higher baseline preferences. However, despite these stronger emotional reactions, IPV priming does not generate significant changes in preferences among this group (Table 3.5), suggesting that stronger emotional responses do not necessarily translate into larger preference changes for this group.

3.8 Discussion and conclusion

We discuss the results and connect them to the framework that we used to develop the hypotheses. In the framework, we distinguished two sets of emotions (anger and fear/anxiety) that would be triggered by IPV priming, each of them having opposite effects on time and risk preferences. As we could not predict which emotions would dominate, our first two hypotheses stated that IPV priming would change risk aversion and impatience, *without specifying the direction of change.* Our hypothesis 3 proposed that changes in preferences resulting from IPV priming would be stronger among women with prior IPV experiences compared to those without such experience.

We interpret our findings as follows. First, consistent with hypothesis 1, IPV priming influences women's risk preferences. Specifically, it *reduces* risk aversion, which is consistent with the identified effects on emotions. On average, anger was more strongly triggered by IPV priming than fear or anxiety. Following our model, this may explain why the net effect resulted in a reduction in risk aversion. The effect on risk aversion, however, does not differ between women who experienced and women who did not experience emotional or physical IPV. In other words, we are unable to confirm our hypothesis 3 in the context of risk preferences. We also observed that anger triggered by the priming video does not differ between women with and without a prior history of emotional or physical IPV experiences. Both groups of women are equally angry after watching the video. This may explain why the effect of IPV priming on women's risk preferences does not vary with women's IPV experience.

Second, we failed to find support for hypothesis 2, as we did not find an effect of IPV priming on women's time preferences on average. However, disaggregating the analysis by IPV experience, we did find important effects. Particularly, IPV priming *reduces* impatience among women with real-life emotional IPV experience and increases impatience among those without such experience. These results are consistent with our hypothesis 3, which predicted that IPV priming would have a stronger impact on time preferences among women with a history of IPV. On the contrary, we found no significant effect of IPV priming on impatience across physical IPV experience groups.

The underlying mechanism of the impacts on time preferences is less straightforward. The similarity in reported anger between women with and without IPV experiences does not explain the differential impacts on impatience. Instead, differences in reported fear and anxiety might explain the difference in impact on impatience between these two groups of women. Specifically, women with IPV experiences reported stronger feelings of fear and anxiety compared to women without IPV experiences, which, in turn, might drive a larger reduction in impatience among women with IPV experiences. Notably, this pattern does not align with the existing literature, which argues that fear and anxiety make an individual more impatient (Callen et al., 2014; Haushofer and Fehr, 2014; Takahashi, 2004; Lerner et al., 2015). Fear and anxiety generally prompt people to avoid uncertainty or escape from danger and make people pessimistic (Lerner and Tiedens, 2006; Carver and Harmon-Jones, 2009; Angus et al., 2015). Accordingly, existing studies show that these emotions make individuals more present-biased Callen et al. (2014); Haushofer and Fehr (2014); Takahashi (2004); Lerner et al. (2015). In contrast, we observe that stronger feelings of fear and anxiety might result in a larger reduction in impatience. One plausible explanation could be that women with IPV experiences might become fearful and anxious about their unpleasant or dangerous present after watching the video, leading to their willingness to escape from the present by focusing more on the future.

Third, beyond our hypotheses, we also examined the disaggregated impacts of IPV priming by age, education and baseline preferences. We observed that the impact on risk preferences is stronger among younger women, with a significant reduction in risk aversion. Consistently, younger women reported stronger emotional feelings, with the highest levels of anger reported after watching the priming video. This aligns with a significant reduction in risk aversion. It is plausible that younger women may be more likely to report stronger emotions, whereas older women may be more accepting of IPV. In contrast, we did not find any significant impact on time preferences by age group.

Furthermore, the disaggregated impacts on risk aversion across women's education and baseline preferences are less conclusive, with the effects being marginally significant. We also found no significant impacts on time preferences across any of the groups defined by education or baseline preferences. We could not find any significant impacts on preferences across education, perhaps because our sampled women are not highly educated, with a median of only 3 years of schooling (Table 3.E.1). The disaggregated impacts by education might have been more pronounced with a broader range of educational variation.

To distinguish emotion-driven changes in preferences from pre-existing differences in preferences by IPV experience, we examined the correlation between IPV experience and baseline preferences. This assesses whether preferences are correlated with IPV experiences without any priming, or preferences only change when women are primed. We did not find a significant correlation between IPV experience and baseline preferences

(Table 3.K.1). At the risk of reading too much into a correlational analysis, it seems to suggest that past IPV experience does not directly influence women's preferences. It looks like preferences are only influenced when emotions are triggered. Our study captures these immediate emotional responses to IPV priming, providing evidence of changes in preference in the short run. Future research could extend this work by examining whether such effects persist over time or translate into real economic behaviours (e.g., saving, borrowing, labour market decisions).

Given that we find that IPV priming influences women's economic preferences, IPV could have important implications for women's economic activities. First, a reduction in risk aversion triggered by IPV priming may have important implications for a range of economic activities. Although reduced risk aversion could lead to beneficial outcomes, such as increased investment in high-return ventures or entrepreneurial activities, emotion-driven risk-taking may also result in suboptimal financial decisions, such as excessive borrowing from informal sources, impulsive investments without proper risk assessment and taking up employment opportunities with higher income volatility.

Second, the observed reduction in impatience might have several implications for economic behaviour. Lower impatience generally leads to increased savings accumulation, better long-term financial planning, and greater investment in human capital; however, an emotion-driven decrease in impatience may also result in poor consumption decisions, such as delaying essential current expenditures on health and nutrition to save for the future. In addition, greater patience can make women accept adverse situations, waiting for future improvements that may never come.

A final note is required on the insights that our results provide for policy, and in particular for the design of interventions addressing the effects of IPV. Our findings have two policy implications. First, our evidence on the effects of IPV priming on economic preferences suggests that policies that reduce IPV might also reduce emotion-driven decision-making episodes, potentially improving women's long-term economic outcomes. Second, changes in preferences are driven by emotional responses to IPV-related content, suggesting that the priming only works for a short period and that volatility in preferences is not beneficial. Thus, interventions that stabilise emotions, such as counselling services, peer support groups, or stress management resources, might mitigate the impacts of IPV on preferences, thereby helping women make better economic decisions in settings where the IPV rate is high.

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Appendix

3.A Real-life IPV experiences

Table 3.A.1 Types of violence perpetrated by the husband in the last 12 months

(1)	(2)	(3)
Emotional	Physical	Sexual
Been humiliated	Been slapped, punched or injured	Been physically forced into unwanted sex
Been humiliated in front of other people	Been pushed or shoved or pulled hair	Been forced into unwanted sexual acts in fear of future torture or any kind of harm
Been scared or intimidated	Been burnt with hot things	Been physically forced to perform sexual acts, the respondent didn't want to
Been verbally threatened to hurt	Been thrown with acid intentionally	Other kind of sexual abuse (making frequent and persistent attempts at sexual contact, using alcohol or drugs to loosen your inhibitions, threatening your job, home, family, or reputation, etc.)
Been tortured for socialising	Been thrown with hot water/oil/ milk/peas etc. intentionally	
Been threatened with marrying other women	Been kicked, dragged or beaten up	
Been threatened with divorce	Been suffocated or choked by hand	
Been tortured for keeping relation or for communicating with your parental relatives	Been burnt	
	Been threatened with knife/gun or other weapon	

3.B Outcomes

Table 3.B.1 Outcomes

Theme	Ques
	Risk preferences
R1: Risk-aversion (scale)	Please tell me, in general, how willing or unwilling you are to take risks, using a scale from 0 to 10, where 0 means you are "completely unwilling to take risks" and 10 means you are "very willing to take risks." You can also use any number between 0 and 10 to indicate where you fall on the scale, using 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10. In analysis, we invert the responses to this question to measure risk-aversion. For instance, a response of 0, indicating the lowest willingness to take risks, is coded as 10, representing the highest level of risk-aversion.
R2: Risk-aversion (staircase)	Please imagine the following situation: You can choose between a sure payment of a particular amount of money, OR a draw, where you would have an equal chance of getting BDT 1200 or getting nothing. We will present to you some different situations. What would you prefer: A draw with a 50-percent chance of receiving BDT 1200 and the same 50-percent chance of receiving nothing, or the amount of BDT 640 as a sure. Next, if 50-percent is chosen, would you prefer the 50/50 chance or the amount of BDT 960 as a sure payment? and if sure is chosen, would you prefer the 50/50 chance or the amount of BDT 320 as a sure payment? This procedure continues as illustrated by Figure 3.B.1
	Time preferences
T1: Impatience (scale)	How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future? Please indicate your answer on a scale from 0 to 10. A 0 means "completely unwilling to do so," and a 10 means "very willing to do so." You can also use any number between 0 and 10 to indicate where you fall on the scale, using 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, or 10. In analysis, wee invert the responses to this question to measure impatience. For example, a response of 0, indicating the lowest willingness to wait for future, is coded as 10, representing the highest level of impatience.
T2: Impatience (staircase)	Suppose you were given the choice between receiving a payment today or a payment in 12 months. We will now present to you five situations. The payment today is the same in each of these situations. The payment in 12 months is different in every situation. For each of these situations, we would like to know which you would choose. Please assume there is no inflation, i.e. future prices are the same as today's prices. Would you rather receive BDT 400 today or BDT 615 in 12 months? Next, if 'today' is chosen, would you rather receive BDT 400 today or BDT 740 in 12 months? and if 'future' is chosen, would you rather receive BDT 400 today or BDT 502 in 12 months? This procedure continues as illustrated by Figure 3.B.2 .

3.B.1 Staircase measure of risk preferences

The staircase measure follows a hypothetical staircase method, which consists of a 'titration procedure' with five subsequent decisions.[29] Each individual had to respond to five interdependent questions. Each subsequent question depends on the respondent's response to the previous question, except the first one. All respondents first decided between the lottery versus a sure payment that slightly exceeds the expected value of the lottery. In the second decision (and all subsequent decisions), the lottery remained the same. If the participant had chosen the sure option in the first question, the sure option in the subsequent decision was smaller. If the participant had opted for the lottery, the sure payment increased. In the same approach, the sure option was increased or decreased in the third decision when the lottery or the sure payment was preferred in the second decision, respectively. This process was repeated five times.

With respondents making five decisions, there are 32 possible combinations of decisions (Figure 3.B.1).[30] For each combination, we calculate a specific interval because we can not calculate the exact point where the respondent was indifferent between a sure amount and a lottery (referred to as an indifference point). We can only observe that the respondent chose one option over the other (i.e., choosing A over B or choosing B over A). We set option A equal to option B to calculate the risk aversion parameter for these decisions, and conclude that the respondent's indifference point should be greater than or less than these parameters. In this way, we create a total of 32 unique intervals.[31]

At each of these decisions, we set the expected utility (U) of the lottery equal to the utility of a sure amount.

$$\texttt{prob\_high} \cdot U(x_{\text{high}}) + (1 - \texttt{prob\_high}) \cdot U(x_{\text{low}}) = U(x_{\text{safe}}) \tag{3.3}$$

where x is the sure or lottery amount and *prob_high* is the probability of winning the lottery.

To calculate the utility, we use the constant relative risk aversion (CRRA) utility function as follows.

$$U(x) = \begin{cases} \frac{x^{1-\rho}}{1-\rho}, & \text{if } \rho \neq 1 \\ \ln(x), & \text{if } \rho = 1 \end{cases} \tag{3.4}$$

where *rho* is risk aversion parameter.

Given that `prob_high` is 0.5, x_{high} is 1200 and x_{low} is zero for each lottery, at each decision, rho (ρ) is driven by using the following steps,

[29] This 'titration procedure' eliminates the issue of 'multiple switching'.

[30] Notably, each combination starts with the same decision, consisting of two choices: lottery of getting 1200 BDT or nothing with 50% chance and a sure amount of 624 BDT.

[31] We have two choices (i.e., lottery or sure amount) and five subsequent decisions, which result in 2^5=32 combinations.

$$0.5 \cdot U(1200) + 0.5 \cdot U(0) = U(x_{\text{safe}}) \tag{3.5}$$

$$0.5 \cdot \frac{1200^{1-\rho}}{1-\rho} = \frac{x_{\text{safe}}^{1-\rho}}{1-\rho} \tag{3.6}$$

Multiply both sides by $1 - \rho$,

$$0.5 \cdot 1200^{1-\rho} = x_{\text{safe}}^{1-\rho} \tag{3.7}$$

Taking the natural logarithm of both sides,

$$\ln\left(0.5 \cdot 1200^{1-\rho}\right) = \ln\left(x_{\text{safe}}^{1-\rho}\right) \tag{3.8}$$

$$\ln(0.5) + (1-\rho)\ln(1200) = (1-\rho)\ln(x_{\text{safe}}) \tag{3.9}$$

$$(1-\rho)\left[\ln(x_{\text{safe}}) - \ln(1200)\right] = \ln(0.5) \tag{3.10}$$

Solving for ρ,

$$\rho = 1 - \frac{\ln(0.5)}{\ln(1200) - \ln(x_{\text{safe}})} \tag{3.11}$$

For each combination, the upper bound of ρ is calculated using the value of the last sure amount that the respondent accepted (i.e., chose B over A) and the lower bound is calculated using the value of the last sure amount which the respondent rejected (i.e., chose A over B) (see Table 3.B.2).[32] For instance, for the first combination, there is no point where B is accepted, so there is no upper bound. For this combination, the interval of the risk aversion parameter is greater than 1.2, indicating stronger risk aversion.[33] On the contrary, there is no point where B is rejected for the last combination (i.e., 32nd combination), so there is no lower bound. For this combination, the interval is lower than -20.14, indicating a strong preference for risk. Next, for the second combination, the last value of B accepted is 80 BDT, and the last value of B rejected is 40 BDT, and these values correspond to the risk aversion between 1.20 and 1.26.[34] The same procedure

[32] As the sure amount varies in each decision, B is the driving factor of each decision-making (i.e., last accepted or rejected B).

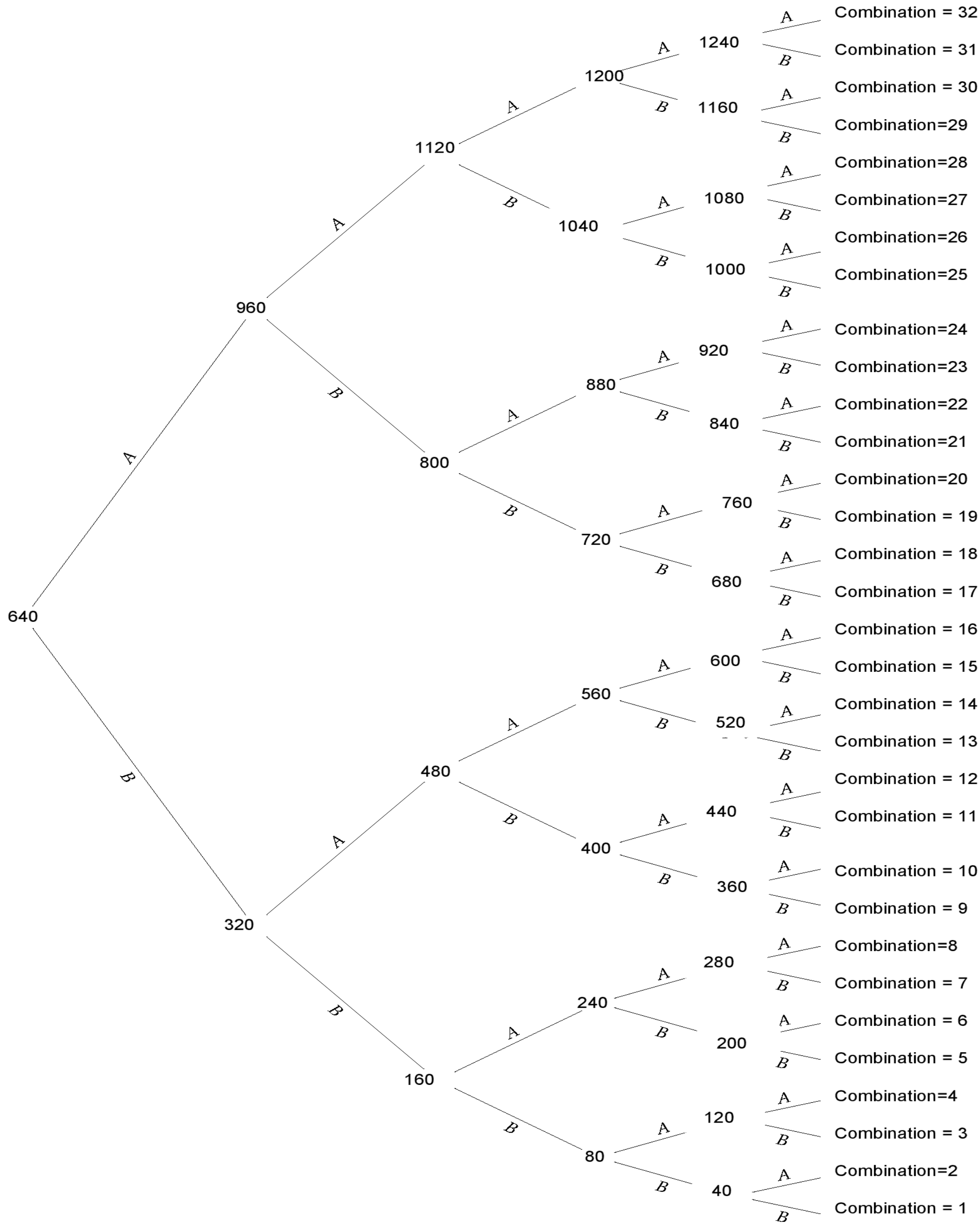
[33] The risk aversion parameter range is as follows:

$$\text{risk aversion parameter, } \rho : \begin{cases} \rho > 0 & \text{risk-averse} \\ \rho = 0 & \text{Risk neutral} \\ \rho < 0 & \text{Risk loving} \end{cases}$$

[34] The lower (upper) bound of the combination of 31 (30) can not be calculated because of the same value of sure amount and lottery. In our sample, only four respondents fall into these combinations.

is followed for the remaining combinations. Following this approach, we assigned each respondent an interval of risk aversion parameter.

Figure 3.B.1 Mapping of combinations of decisions: Risk preferences



Notes: In the staircase method, each respondent made five interdependent decisions. In each decision, the respondent was asked to choose between a fixed lottery (50% chances of winning 1200 BDT and 50% of winning nothing) and a sure amount. The figure illustrates the staircase procedure. The values shown in the tree represent the sure amounts at each decision. First, each respondent was asked whether they would prefer to choose a sure payment of 640 BDT or a lottery of getting 1200 BDT or nothing. In case the respondent chose lottery (A), in the second question, the sure amount was adjusted upwards to 960 BDT. If, on the other hand, the respondent chose the sure amount (B), the corresponding payment was adjusted down to 320 BDT. The tree follows the same logic for further steps (Falk et al., 2023). The combinations at the end of each branch represent a linear ranking of risk preferences, particularly risk-taking attitudes.

Table 3.B.2 Risk-aversion parameter intervals of decision combinations

Combination	Sequence	High value lottery (A)	Low value lottery	Last rejected sure amount (B)	Last accepted certain amount (B)	Lower bound (rho)	Upper (bound (rho)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1	BBBBB	1200	0		40		1.20
2	BBBBA	1200	0	40	80	1.20	1.26
3	BBBAB	1200	0	80	120	1.26	1.30
4	BBBAA	1200	0	120	160	1.30	1.34
5	BBABB	1200	0	160	200	1.34	1.39
6	BBABA	1200	0	200	240	1.39	1.43
7	BAAAB	1200	0	240	280	1.43	1.48
8	BBAAA	1200	0	280	320	1.48	1.52
9	BABBB	1200	0	320	360	1.52	1.58
10	BABBA	1200	0	360	400	1.58	1.63
11	BABAB	1200	0	400	440	1.63	1.69
12	BABAA	1200	0	440	480	1.69	1.76
13	BAABB	1200	0	480	520	1.76	1.83
14	BAABA	1200	0	520	560	1.83	1.91
15	BAAAB	1200	0	560	600	1.91	2.00
16	BAAAA	1200	0	600	640	2.00	2.10
17	ABBBB	1200	0	640	680	2.10	2.22
18	ABBBA	1200	0	680	720	2.22	2.36
19	ABBAB	1200	0	720	760	2.36	2.52
20	ABBAA	1200	0	760	800	2.52	2.71
21	ABABB	1200	0	800	840	2.71	2.94
22	ABABA	1200	0	840	880	2.94	3.23
24	ABAAA	1200	0	920	960	3.61	4.11
25	AABBB	1200	0	960	1000	4.11	4.80
26	AABBA	1200	0	1000	1040	4.80	5.84
27	AABAB	1200	0	1040	1080	5.84	7.58
28	AABAA	1200	0	1080	1120	7.58	11.05
29	AAABB	1200	0	1120	1160	11.05	21.45
30	AAABA	1200	0	1160	1200	21.45	
31	AAAAB	1200	0	1200	1240		-20.14
32	AAAAA	1200	0	1240		-20.14	

Notes: Column 1: combination of decisions drawn from Figure 3.B.1 ; column 2: sequence of high value of the lottery (A) and sure amount (B) from Figure 3.B.1 ; columns 3 and 4: high and low values of the lottery which are fixed; column 5: last rejected lottery (B) in the sequence; column 6: last accepted lottery (B) in the sequence; column 7: calculated lower bound of risk aversion parameter using equation 3.11; column 7: calculated upper bound of the monthly discount rate using the same formula.

3.B.2 Staircase measure of time preferences

The staircase measure of time preferences follows a hypothetical staircase method, which consists of a 'titration procedure' with five subsequent decisions. Each individual had to respond to five interdependent questions. Each question depends on the respondent's response to the previous question, except the first one. All respondents first decided between the present gain versus a future gain that slightly exceeds the value of the present gain.[35] In the second decision (and all subsequent decisions), the present gain remained the same. If the participant had chosen the future gain in the first question, the future gain in the subsequent decision was smaller. If the participant had opted for the present gain, the future gain increased. In the same approach, the future gain was increased or decreased in the third decision when the present or the future gain was preferred in the second decision, respectively. This process was repeated five times.

With respondents making five decisions, there are 32 possible combinations of decisions (Figure 3.B.2).[36] Similar to the staircase measure for risk aversion (section 3.B.1), we calculate a specific interval of monthly discount rates for each combination.[37]

At each decision, we set the present value of the future payment to the present payment as follows.

$$A = \frac{B}{(1+r)^t} \tag{3.12}$$

where r is monthly discount rate; B is future payment; A is present payment and t is time (i.e., 12 months)

Solving for discount rate, r,

$$r = \left(\frac{B}{A}\right)^{\frac{1}{t}} - 1 \tag{3.13}$$

For each combination, the upper bound is calculated using the value of the last future payment that the respondent accepted (i.e., chose B) and the lower bound is calculated using the value of the last future payment which the respondent rejected (i.e., chose A) (see Table 3.B.3).[38] For instance, for the first combination, there is no point where B is accepted, so there is no upper bound. For this combination, the interval of the discount rate is greater than 3.63%, indicating higher impatience. On the contrary, there is no

[35] We use the choice options of present and future payments instead of near future versus distant future because this reflects the real-world intertemporal trade-offs, and it is also easier for the respondent to compare between the choices.

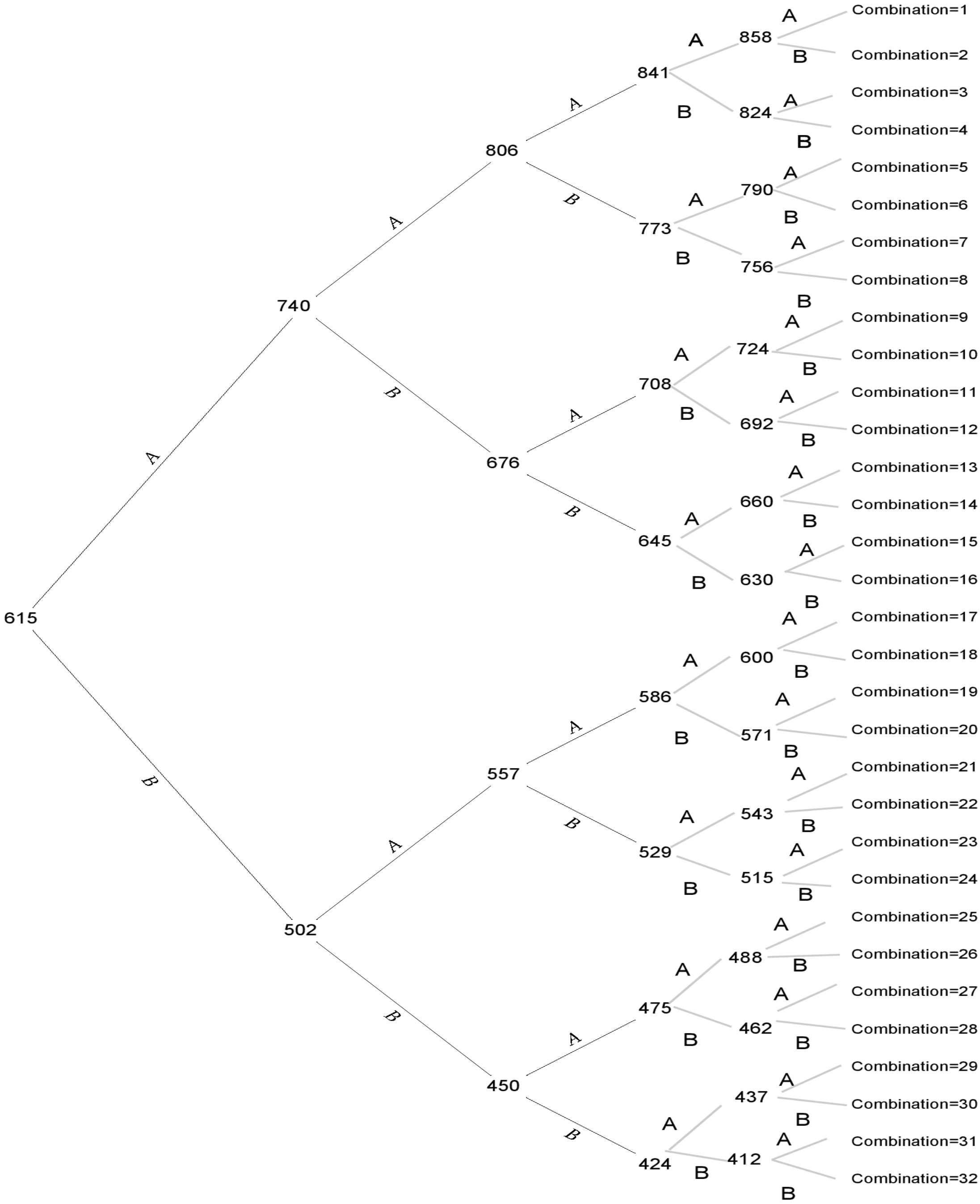
[36] Notably, each combination starts with the same decision, consisting of two choices: the present payment of 400 BDT and the future payment of 600 BDT.

[37] For all decisions in our approach, the respondents always choose between today vs the next 12 months, while the hyperbolic discounting approach requires more than one combination of time periods. Therefore, we use an exponential discounting approach.

[38] As the future payment varies in each decision, B is the driving factor for each decision-making (i.e., last accepted or rejected B).

point where B is rejected for the last combination (i.e., 32nd combination), so there is no lower bound. For this combination, the interval is lower than 0.24%, showing lower impatience. Next, for the second combination, the last value of B accepted is 858 BDT and the last value of B rejected is 841 BDT, and these values correspond to the discount rate between 3.58% to 3.63%. The same procedure is followed for the remaining combinations. Following this approach, we assigned each respondent an interval of discount rate.

Figure 3.B.2 Mapping of combinations of decisions: Time preferences



Notes: In the staircase method, each respondent made five interdependent decisions. In each decision, the respondent was asked to choose between a fixed payment of 400 BDT today and an amount in 12 months from now. The figure shows the staircase procedure. The values shown in the tree represent the payment in 12 months at each decision. First, each respondent was asked whether they would prefer to receive 400 BDT today or 615 BDT in 12 months from now. In case the respondent chose the payment today (A), in the second question, the payment in 12 months was adjusted upwards to 740 BDT. If, on the other hand, the respondent chose the payment in 12 months (B), the corresponding payment was adjusted down to 502 BDT. The tree follows the same logic for further steps (Falk et al., 2023). The combinations at the end of each branch represent a linear ranking of time preferences, particularly patience.

Table 3.B.3 Discount rate intervals of decision combinations

Combination	Sequence	Today's payment (A)	Last rejected future's payment (B)	Last accepted future's payment (B)	Lower bound (discount)	Upper bound (discount)
(1)	(2)	(3)	(4)	(5)	(6)	(7)
1	AAAAA	400	858		6.57	
2	AAAAB	400	841	858	6.39	6.57
3	AAABA	400	824	841	6.21	6.39
4	AAABB	400	806	824	6.01	6.21
5	AABAA	400	790	806	5.84	6.01
6	AABAB	400	773	790	5.64	5.84
7	AABBA	400	756	773	5.45	5.64
8	AABBB	400	740	756	5.26	5.45
9	ABAAA	400	724	740	5.07	5.26
10	ABAAB	400	708	724	4.87	5.07
11	ABABA	400	692	708	4.67	4.87
12	ABABB	400	676	692	4.47	4.67
13	ABBAA	400	660	676	4.26	4.47
14	ABBAB	400	645	660	4.06	4.26
15	ABBBA	400	630	645	3.86	4.06
16	ABBBB	400	615	630	3.65	3.86
17	BAAAA	400	600	615	3.44	3.65
18	BAAAB	400	586	600	3.23	3.44
19	BAABA	400	571	586	3.01	3.23
20	BAABB	400	557	571	2.80	3.01
21	BABAA	400	543	557	2.58	2.80
22	BABAB	400	529	543	2.36	2.58
23	BABBA	400	515	529	2.13	2.36
24	BABBB	400	502	515	1.91	2.13
25	BBAAA	400	488	502	1.67	1.91
26	BAAAB	400	475	488	1.44	1.67
27	BBABA	400	462	475	1.21	1.44
28	BBABB	400	450	462	0.99	1.21
29	BBBAA	400	437	450	0.74	0.99
30	BBBAB	400	424	437	0.49	0.74
31	BBBBA	400	412	424	0.25	0.49
32	BBBBB	400		412		0.25

Notes: Column 1: combination of decisions drawn from Figure 3.B.2 ; column 2: sequence of today's payment (A) and future's payment (B) from Figure 3.B.2 ; column 3: today's payment which is fixed; column 4: last rejected future's payment (B) in the sequence; column 5: last accepted future's payment (B) in the sequence; column 6: calculated lower bound of the monthly discount rate using equation 3.13; column 7: calculated upper bound of the monthly discount rate using the same formula.

3.C Sample selection

Table 3.C.1 BRAC Programme

Programme	Target	Selection criteria	Support
Ultra-poor graduation programme (UPG)	Ultra-poor Women	i. Climate induced migrants ii. People living in climatic hotspots, iii. People who are economically impacted by COVID-19. iv. The participants' age will be above 50 years	i. one-time support through grants (with an interest-free soft loan by BRAC) to avail quality assets and grants climate-friendly inputs (such as flood/ saline tolerant seed for homestead farming, fodder, vaccine, tin for the roof, and clean cooking stove) for farm and non-farm enterprise development, ii. Classroom training on the enterprise/ transferred assets to provide basic knowledge and skills on asset management and income diversification, iii.Savings encouragement training; iv. Training on Household Financial Planning; v. Connecting them to financial services (i.e., Microfinance) at the end of the intervention; vi. Hands-on training on livelihoods and lives; vii. Climate awareness; viii. Visits to monitor and raise awareness on social issues; ix. community mobilization (forming committee); x. Linking to Government Social Safety Net Programme (SSNP) if applicable.
Urban Development Programme (UDP)	Household head	i. New poor who are also climate migrants living in urban low-income settlements or new poor living in climate vulnerable urban settlements. ii. People who are economically impacted by COVID-19. (Emerging "New Poor" who were vulnerable non-poor (income between median and poverty line) before the COVID-19 and have now fallen below the poverty line due to complete or partial loss of income). Pre COVID-19 per capita income of BDT 8,000 (USD 3.2 per day) or more; and current (March 2021) monthly per capita income of BDT 4,800 (USD 1.9 per day) or below. iii. Living for at least 4 months in the selected intervention areas. iv. At least one active member in the family aged 18-60 years. v. Current dependency on insecure/ irregular earning sources and/or savings.	i. Raise social awareness among the selected communities through distribution of leaflets/posters, ii. Financial literacy, Entrepreneurship Development and Business Management (EDBM) training, iii. Selected vulnerable youth are connected, based on demand-supply analysis, with BRAC Skills Development Programme (SDP) or other technical service providers for apprenticeship/ skills-training with stipend, iv. Facilitate enhanced access of the participants to skills training from relevant Government-organized non-governmental organization (GO-NGO) entities, v. Provide business support (in kind) to the selected participants, vi. Create Market linkages through networking support, vii. Provide support for job placement for vulnerable youth in agreement with selected private stakeholders, viii. Facilitate enhanced access of the participants to financial services from GO-NGO, ix. Arrange social safety net forum, x. Sensitise local community on social safety net services, xi. Facilitate advocacy dialogue between city authority and selected community, xii. Advocacy with city authority and concerned government agencies to incorporate climate vulnerable and new poor eligible participants in the social safety net schemes, xiii. Provide short-term assistance in meeting nutritional needs of children in the most vulnerable most vulnerable families, xiv. Establish WASH facilities in selected communities based on need assessment
Migration programme	Women, men and youth returnee migrants	i. Climate induced migrants, ii. Reverse migrants, iii. People living in climate hotspots primarily in city corporation and pourashava, iv. Emerging "new poor" who were vulnerable non-poor (income between median and poverty line) before the COVID-19 and have now fallen below the poverty line due to complete or partial loss of income, v. People who are economically impacted by COVID-19.	i. Selected migrants will have access to tailor-made socio-economic reintegration and economic recovery plans, ii. Support to develop business plans for target women, men, and youth returnee migrants, iii. Entrepreneurship development training, iv. Financial literacy training, v. Provide a one-time financial grant for income generation, vi. Access to psychosocial services (PSS), vii. Access to health, trauma, and psychiatric health treatment, viii. Sensitisation workshops/ meeting for government and private service providers at the union, upazila, and district level on climate change, resilience building and better financial services for returnee reintegration, ix. Access government Social Safety Net Programmes. (SSNP), x. Different services, such as Probashi Kallyan Bank Loans, Migrants Children Scholarships, and legal support, ADR, provided by LGIs, xi. Access to various services of housing, education, relief, or health support of the public and private sector, xii. Are connected to microfinance institutions (MFI) or banks to acquire further financial assistance.
Climate Change Programme (CCP)	Climate induced migrant Farmers	i. Climate induced migrants, ii. People living in climatic hotspots, primarily in Satkhira and Sirajganj pourashavas and Khulna City Corporation, iii. Emerging "New Poor" who were vulnerable non-poor (income between median and poverty line) before the COVID-19 and have now fallen below the poverty line due to complete or partial loss of income, iv. People who are economically impacted by COVID-19.	i. Households level action plan developed for 6,000 targeted farmers in the project area, ii. Farmers engaging with dairy, crop and vegetable farming will be supported with need-based grants and training owing to avail quality inputs for increasing their farming production, iii. Daylong capacity building sessions for farmers providing the inputs support with follow-up and refresher courses, iv. At least 6 types of IEC/BCC materials will be developed, v. Daylong training sessions to engaging the farmers with the local relevant government departments, vi. Daylong refresher training for targeted farmers and relevant local government department, vii. Household visits for improving farmer's knowledge and practices, viii. ToT development for engaging BRAC social enterprises (Seed and Agro Enterprise, Dairy and Food Enterprise and Artificial Insemination Enterprise) and relevant government departments at the local level, ix. Daylong training sessions for targeted farmers for improving their product marketing capacity, x. Daylong networking sessions with market drivers at the local level for creating and strengthening access to a local market for the targeted farmers.

3.D Ethical protocols

We hired female enumerators for data collection, as we collected data from women on a sensitive issue, IPV. We provided extensive training to them on how to collect data on IPV experiences. We followed the ethical and safety guidelines recommended by the World Health Organization (WHO). For example,

1. we framed this study as a baseline survey for the climate-change interventions evaluation instead of as a study about IPV;

2. women were asked for consent. The interviewers read out an information sheet covering non-technical information on the study objectives, a description of the topics covered in the questionnaire, the expected interview duration, and the researchers' contact information. The respondents were told that some topics might be sensitive, and they would be given the option not to answer questions about these topics. If the respondent agreed to give their consent, they were requested to sign on the data-collection tool (i.e., tablet);

3. interviews were conducted in a safe place outside the house, where the participants felt comfortable talking to another woman;

4. during the training, the interviewers received strict instructions to maintain confidentiality and no interviewers worked in their community.

To minimise the risk of any repercussions from the husband, family members or wider community on the respondent, one of the major responsibilities of the interviewers is to ensure that the data collection is done alone. If anyone comes, they should change the topic and discuss dummy issues (i.e., food consumption). Moreover, the IPV priming was implemented at the end of the survey[39]; therefore, the interviewers were able to build rapport and a better connection with the participants, which helped them discuss sensitive issues. Furthermore, the interviewers and data management team of BIGD were prepared to take essential steps to tackle any repercussions. For instance, the interviewers assessed the risk of eliciting this module based on the participants' health, facial expression and surrounding environment, and they had the freedom not to move forward with this module if it seemed risky for the respective participant. Furthermore, if either of the interviewers or participants felt uncomfortable discussing this issue, they stopped the interview at that moment.

[39] For the control group, the IPV priming was implemented at the end and for the treatment group, the IPV priming was the second last module.

3.E Descriptive statistics

Table 3.E.1 Sample characteristics

	Mean	SD	Observations
Respondent's age	42.51	11.67	901
Respondent's education	3.51	3.48	901
Employed respondent	0.68	0.47	901
Husband's age	50.23	14.21	895
Husband's education	3.47	3.68	901
Employed husband	0.89	0.32	901
Marriage length	25.22	12.75	901
Number of sons	0.80	0.79	901
Number of daughters	0.58	0.78	901
Cultivable land (decimal)	7.39	26.52	901
Pond (decimal)	1.07	11.63	901
Cow (yes/no)	0.26	0.44	901
Goat (yes/no)	0.18	0.38	901
Sheep (yes/no)	0.01	0.12	901
Poultry/pigeon (yes/no)	0.47	0.50	901
Duck/Turkey (yes/no)	0.07	0.26	901
Van/other vehicle (yes/no)	0.12	0.33	901
Sewing machine (yes/no)	0.18	0.38	901
Fishing net (yes/no)	0.15	0.36	901
Household savings (BDT)	9447.85	34250.73	901
Male headed household (yes/no)	0.80	0.40	901
Baseline risk-aversion (scale)	2.43	2.98	439
Baseline impatience (scale)	3.08	3.86	455
Anger	0.62	0.48	898
Fear	0.42	0.49	898
Anxiety	0.49	0.50	898
Recent emotional IPV	28.97	45.39	901
Distant emotional IPV	18.09	38.52	901
Never emotional IPV	52.94	49.94	901
Recent physical IPV	19.64	39.75	901
Distant physical IPV	32.19	46.75	901
Never physical IPV	48.17	49.99	901
Recent sexual IPV	13.65	34.35	901
Distant sexual IPV	11.88	32.37	901
Never sexual IPV	74.47	43.63	901

Notes: SD stands for standard deviation and N stands for number of observations. We measure 'risk aversion', 'Impatience' on a scale of 0-10, with 10 referring to the highest risk aversion and impatience. These questions were asked to the relevant subsample; for example, the risk subsample was asked the risk aversion question, and the time subsample was asked about impatience. 'Anger', 'Fear' and 'Anxiety' measure the mean proportion of women who reported anger, fear, and anxiety after watching the video. Anger (fear/anxiety) takes the value of one if woman reported that the video made her angry (fearful/anxious) a lot or extremely, and zero otherwise (not at all, a little bit). The indicators capturing IPV experiences are expressed in percentages. 'Recent emotional (physical/sexual) IPV' measures the % of the respondents who reported to experience emotional (physical/sexual) IPV in the last 12 months, 'Distant emotional (physical/sexual)' measures the % of the respondents who reported to experience emotional (physical/sexual) IPV ever in their lifetime but not in the last 12 months; and 'Never emotional (physical/sexual)' measures the % of the respondents who never experienced emotional (physical/sexual) IPV.

3.F Balancing tests

Table 3.F.1 Balance Table: Risk groups

	(1) Control	(2) Treatment	(3) Difference (1)-(2)
Respondent's age	41.95	42.55	-0.60
	(12.07)	(11.45)	(1.12)
Respondent's education	3.51	3.75	-0.24
	(3.56)	(3.52)	(0.34)
Employed respondent	0.69	0.67	0.02
	(0.47)	(0.47)	(0.04)
Husband's age	49.13	50.30	-1.17
	(14.18)	(13.78)	(1.34)
Husband's education	3.77	3.50	0.26
	(3.71)	(3.71)	(0.35)
Employed husband	0.93	0.85	0.08**
	(0.26)	(0.36)	(0.03)
Marriage length	24.54	24.91	-0.37
	(12.66)	(12.35)	(1.19)
Number of sons	0.86	0.79	0.06
	(0.81)	(0.76)	(0.08)
Number of daughters	0.64	0.61	0.04
	(0.81)	(0.80)	(0.08)
Cultivable land (decimal)	9.75	7.84	1.90
	(33.94)	(29.35)	(3.01)
Pond (decimal)	0.53	1.16	-0.63
	(2.20)	(6.23)	(0.45)
Cow (yes/no)	0.34	0.24	0.09**
	(0.47)	(0.43)	(0.04)
Goat (yes/no)	0.20	0.19	0.01
	(0.40)	(0.39)	(0.04)
Sheep (yes/no)	0.02	0.02	0.00
	(0.14)	(0.13)	(0.01)
Poultry/pigeon (yes/no)	0.49	0.48	0.01
	(0.50)	(0.50)	(0.05)
Duck/Turkey (yes/no)	0.10	0.09	0.01
	(0.30)	(0.28)	(0.03)
Van/other vehicle (yes/no)	0.15	0.12	0.03
	(0.36)	(0.33)	(0.03)
Sewing machine (yes/no)	0.17	0.19	-0.02
	(0.38)	(0.39)	(0.04)
Fishing net (yes/no)	0.14	0.13	0.02
	(0.35)	(0.34)	(0.03)
Household savings (BDT)	8579.67	10980.98	-2401.31
	(23159.93)	(39984.89)	(3126.61)
Male headed household (yes/no)	0.86	0.80	0.06*
	(0.35)	(0.40)	(0.04)
Baseline risk-aversion (scale)	2.51	2.34	0.17
	(2.98)	(2.98)	(0.28)
P-value of joint orthogonality test	0.6395		
N	216	226	442

Notes: Columns 1 and 2 report the descriptive statistics across control and treatment groups of 'risk' subsample, and column 3 reports the difference between columns 1 and 2. The productive asset ownership (cow, goat, sheep and others) takes a value of 1 if the household has the asset, 0 otherwise. Household savings are measured in Taka, Bangladeshi currency (BDT). risk aversion captures the self-assessed behaviour on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks). The p-values of joint orthogonality tests are obtained by regressing the treatment variable on women, their husbands and household characteristics (mentioned in the first column of the table). Standard deviations are in parentheses in columns 1 and 2 and standard errors are in parentheses in column 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.F.2 Balance Table: Time groups

	(1) Control	(2) Treatment	(3) Difference (1)-(2)
Respondent's age	42.23	43.26	-1.02
	(11.98)	(11.22)	(1.08)
Respondent's education	3.29	3.48	-0.20
	(3.35)	(3.48)	(0.32)
Employed respondent	0.64	0.72	-0.08*
	(0.48)	(0.45)	(0.04)
Husband's age	50.04	51.38	-1.34
	(14.54)	(14.33)	(1.35)
Husband's education	3.29	3.35	-0.06
	(3.60)	(3.69)	(0.34)
Employed husband	0.90	0.87	0.03
	(0.30)	(0.33)	(0.03)
Marriage length	25.09	26.29	-1.19
	(13.03)	(12.95)	(1.21)
Number of sons	0.81	0.76	0.05
	(0.80)	(0.80)	(0.07)
Number of daughters	0.57	0.52	0.05
	(0.77)	(0.73)	(0.07)
Cultivable land (decimal)	6.32	5.82	0.50
	(21.69)	(18.96)	(1.90)
Pond (decimal)	0.48	2.07	-1.59
	(2.26)	(21.92)	(1.46)
Cow (yes/no)	0.22	0.23	-0.01
	(0.41)	(0.42)	(0.04)
Goat (yes/no)	0.15	0.17	-0.02
	(0.36)	(0.38)	(0.03)
Sheep (yes/no)	0.02	0.00	0.01
	(0.13)	(0.07)	(0.01)
Poultry/pigeon (yes/no)	0.42	0.49	-0.08*
	(0.49)	(0.50)	(0.05)
Duck/Turkey (yes/no)	0.05	0.05	0.01
	(0.22)	(0.21)	(0.02)
Van/other vehicle (yes/no)	0.12	0.10	0.01
	(0.32)	(0.31)	(0.03)
Sewing machine (yes/no)	0.19	0.16	0.04
	(0.40)	(0.36)	(0.04)
Fishing net (yes/no)	0.16	0.17	-0.01
	(0.37)	(0.38)	(0.03)
Household savings (BDT)	5683.97	12474.71	-6790.74**
	(16164.95)	(47337.57)	(3310.55)
Male headed household (yes/no)	0.76	0.77	-0.02
	(0.43)	(0.42)	(0.04)
Baseline impatience (scale)	3.05	3.11	-0.06
	(3.84)	(3.89)	(0.36)
P-value of joint orthogonality test	0.8491		
Number of observations	228	231	459

Notes: Columns 1 and 2 report the descriptive statistics across control and treatment groups of 'time' subsample, and column 3 reports the difference between columns 1 and 2. The productive asset ownership (cow, goat and others) takes a value of 1 if the household has the asset, 0 otherwise. Household savings are measured in Taka, Bangladeshi currency (BDT). Impatience captures the self-assessed impatience level on a scale of 0-10, with 10 referring to the highest impatience (i.e., lowest willingness to wait for future). The p-values of joint orthogonality tests are obtained by regressing the treatment variable on women, their husbands and household characteristics (mentioned in the first column of the table). Standard deviations are in parentheses in columns 1 and 2 and standard errors are in parentheses in column 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.F.3 Balance table on IPV reporting across treatment and control

	(1) Control	(2) Treatment	(3) Difference (1)-(2)
Panel (A): Risk sub-sample			
Recent emotional IPV	0.24	0.30	-0.06
	(0.43)	(0.46)	(0.04)
Distant emotional IPV	0.18	0.19	-0.01
	(0.38)	(0.39)	(0.04)
Never emotional IPV	0.58	0.51	0.07
	(0.49)	(0.50)	(0.05)
Recent physical IPV	0.16	0.23	-0.07*
	(0.37)	(0.42)	(0.04)
Distant physical IPV	0.36	0.30	0.06
	(0.48)	(0.46)	(0.04)
Never physical IPV	0.49	0.47	0.01
	(0.50)	(0.50)	(0.05)
Recent sexual IPV	0.12	0.16	-0.04
	(0.32)	(0.37)	(0.03)
Distant sexual IPV	0.08	0.13	-0.04
	(0.28)	(0.34)	(0.03)
Never sexual IPV	0.80	0.71	0.09**
	(0.40)	(0.45)	(0.04)
P-value of joint orthogonality test	0.1582		
N	216	226	442
Panel (B): Time sub-sample			
Recent emotional IPV	0.32	0.30	0.02
	(0.47)	(0.46)	(0.04)
Distant emotional IPV	0.17	0.19	-0.02
	(0.38)	(0.39)	(0.04)
Never emotional IPV	0.51	0.51	0.00
	(0.50)	(0.50)	(0.05)
Recent physical IPV	0.22	0.18	0.04
	(0.41)	(0.39)	(0.04)
Distant physical IPV	0.32	0.32	-0.00
	(0.47)	(0.47)	(0.04)
Never physical IPV	0.46	0.50	-0.04
	(0.50)	(0.50)	(0.05)
Recent sexual IPV	0.13	0.14	-0.02
	(0.33)	(0.35)	(0.03)
Distant sexual IPV	0.11	0.15	-0.03
	(0.32)	(0.36)	(0.03)
Never sexual IPV	0.76	0.71	0.05
	(0.43)	(0.45)	(0.04)
P-value of joint orthogonality test	0.6951		
N	229	229	458

Notes: Columns 1 and 2 report the mean proportion of different forms of IPV experiences among control and treatment groups. 'Recent' equal to one if the respondent experienced emotional (physical/sexual) IPV in the last 12 months and zero otherwise; 'Distant' equal to one if the respondent experienced emotional (physical/sexual) in their lifetime but not in the last 12 months and zero otherwise; and 'Never' equal to one if the respondent never experienced emotional (physical/sexual) in their lifetime and zero otherwise Column 3 reports the difference between columns 1 and 2. The p-values of joint orthogonality tests are obtained by regressing the treatment variable on IPV indicators (mentioned in the first column of the table). Standard deviations are in parentheses in columns 1 and 2 and standard errors are in parentheses in column 3.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.G Impacts of the IPV priming by prior experience of IPV

Table 3.G.1 Impact of the priming of IPV on women's preferences by lifetime IPV experience

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Panel (A): Emotional IPV				
Treatment	-0.707*	-0.453	0.662*	1.236*
	(0.388)	(0.614)	(0.377)	(0.678)
Emotional (ever)	0.0288	0.642	0.581	0.519
	(0.408)	(0.641)	(0.378)	(0.717)
Treatment X Emotional (ever)	-0.107	0.238	-1.263**	-2.660***
	(0.571)	(0.866)	(0.525)	(0.979)
Observations	436	390	455	455
Panel (B): Physical IPV				
Treatment	-0.694*	-0.782	0.361	0.695
	(0.410)	(0.663)	(0.395)	(0.727)
Physical (ever)	-0.282	0.466	0.481	0.226
	(0.434)	(0.653)	(0.384)	(0.730)
Treatment X Physical (ever)	-0.119	0.925	-0.602	-1.493
	(0.574)	(0.878)	(0.550)	(1.019)
Observations	436	390	455	455

Notes: Columns 1 and 3: OLS regressions with the following preference outcomes: risk aversion and impatience measured on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks) and impatience (i.e., lowest willingness to wait for future), respectively. Columns 2 and 4: Interval regression with the latent dependent variable that measures risk aversion in terms of risk aversion parameter and impatience in terms of discount rate. Table 3.B.1 in section 3.B includes a detailed explanation of these outcomes. The explanatory variables are as follows: 'Treatment' equal to one if the respondent was primed and zero otherwise; 'Ever' equal to one if the respondent ever experienced emotional (physical) IPV in their lifetime and zero otherwise (i.e., never experienced IPV women); and 'Treatment X Ever' is the interaction term of these two indicators. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.G.2 Impact of the IPV priming module on women's preferences by IPV experience

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Panel (A): Emotional IPV				
Impact(Recent)	-1.218**	-0.638	-0.918*	-1.191
	(0.517)	(0.812)	(0.473)	(0.856)
Impact (Distant)	-0.198	0.325	-0.0701	-1.860
	(0.700)	(0.863)	(0.648)	(1.211)
Impact(Never)	-0.714*	-0.419	0.663*	1.228*
	(0.390)	(0.611)	(0.379)	(0.679)
Impact diff (Recent vs Distant)	-1.020	-0.963	-0.847	0.669
	(0.872)	(1.228)	(0.813)	(1.473)
Impact diff (Recent vs Never)	-0.504	-0.219	-1.581***	-2.419**
	(0.634)	(0.996)	(0.602)	(1.092)
Impact diff (Distant vs Never)	0.516	0.744	-0.733	-3.088**
	(0.826)	(1.118)	(0.738)	(1.391)
Observations	436	390	455	455
Panel (B): Physical IPV				
Impact(Recent)	-1.216*	-0.822	-0.742	-0.763
	(0.641)	(1.114)	(0.629)	(1.170)
Impact (Distant)	-0.597	0.328	0.0464	-0.790
	(0.520)	(0.609)	(0.497)	(0.893)
Impact(Never)	-0.684*	-0.758	0.356	0.699
	(0.411)	(0.648)	(0.397)	(0.726)
Impact diff (Recent vs Distant)	-0.619	-1.150	-0.788	0.0266
	(0.828)	(1.307)	(0.828)	(1.505)
Impact diff (Recent vs Never)	-0.532	-0.0643	-1.098	-1.462
	(0.772)	(1.265)	(0.740)	(1.372)
Impact diff (Distant vs Never)	0.0869	1.086	-0.310	-1.489
	(0.663)	(0.935)	(0.640)	(1.172)
Observations	436	390	455	455

Notes: We report the impacts among women who ever experienced IPV, women who ever but not recently experienced IPV, women who never experienced IPV and the differences in the impacts of these three groups. The original OLS regressions are reported in Table 3.G.3 . Impact (Recent) reports the impact among women who experienced IPV in the last 12 months, which is the sum of coefficients of 'Treatment' and 'Treatment X Recent'. Impact (Distant) reports the impact among women who ever experienced IPV in their lifetime but not in the last 12 months, which is the coefficient of 'Treatment' and 'Treatment X Distant'. Impact (Never) reports the impact among women who never experienced emotional (physical) IPV in their lifetime, which is the coefficient of 'Treatment'. Impact diff (Recent vs Distant) reports the difference in columns 1 and 2, which is the difference between the coefficients of 'Treatment X Recent' and 'Treatment X Distant'. Impact diff (Recent vs Never) reports the difference in columns 1 and 3, which is the coefficient of 'Treatment X Recent'. Impact diff (Distant vs Never) reports the difference in columns 2 and 3, which is the coefficient of 'Treatment X Ever but not recent'. Panels (A) and (B) report the impacts disaggregated by emotional and physical IPV experiences, respectively. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.G.3 Impact of the priming of IPV on women's preferences by IPV experience

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Panel (A): Emotional IPV				
Treatment	-0.714*	-0.419	0.663*	1.228*
	(0.390)	(0.611)	(0.379)	(0.679)
Recent	0.179	1.334*	0.618	0.291
	(0.498)	(0.799)	(0.415)	(0.810)
Distant	-0.191	-0.258	0.507	0.937
	(0.553)	(0.783)	(0.582)	(1.047)
Treatment X Recent	-0.504	-0.219	-1.581***	-2.419**
	(0.634)	(0.996)	(0.602)	(1.092)
Treatment X Distant	0.516	0.744	-0.733	-3.088**
	(0.826)	(1.118)	(0.738)	(1.391)
Observations	436	390	455	455
Panel (B): Physical IPV				
Treatment	-0.684*	-0.758	0.356	0.699
	(0.411)	(0.648)	(0.397)	(0.726)
Recent	-0.00429	2.179**	0.392	0.533
	(0.618)	(1.105)	(0.482)	(0.955)
Distant	-0.387	-0.275	0.526	0.0325
	(0.472)	(0.646)	(0.439)	(0.842)
Treatment X Recent	-0.532	-0.0643	-1.098	-1.462
	(0.772)	(1.265)	(0.740)	(1.372)
Treatment X Distant	0.0869	1.086	-0.310	-1.489
	(0.663)	(0.935)	(0.640)	(1.172)
Observations	436	390	455	455

Notes: Columns 1 and 3: OLS regressions with the following preference outcomes: risk aversion and impatience measured on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks) and impatience (i.e., lowest willingness to wait for future), respectively. Columns 2 and 4: Interval regression with the latent dependent variable that measures risk aversion in terms of risk aversion parameter and impatience in terms of discount rate. Table 3.B.1 in section 3.B includes a detailed explanation of these outcomes. The explanatory variables are as follows: 'Treatment' equal to one if the respondent was primed and zero otherwise; 'Recent' equal to one if the respondent experienced emotional (physical) IPV in the last 12 months and zero otherwise; 'Distant' equal to one if the respondent experienced emotional (physical) in their lifetime but not in the last 12 months;'Treatment X Recent' is the interaction term of these 'Treatment' and 'Recent' indicators and 'Treatment X Distant' is the interaction term of 'Treatment' and 'Distant' indicators. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.H Main regressions of the heterogeneity analysis

Table 3.H.1 Impact of the priming of IPV on women's preferences by women's demographic characteristics

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Panel (A): Age groups				
Treatment	-1.009***	-0.574	-0.218	0.215
	(0.384)	(0.623)	(0.406)	(0.754)
Older	-0.543	-0.486	0.616	1.037
	(0.574)	(0.883)	(0.464)	(0.996)
Treatment X Older	0.491	0.634	0.472	-0.556
	(0.573)	(0.864)	(0.524)	(1.021)
Observations	436	390	455	455
Panel (B): Education groups				
Treatment	-0.799*	-0.563	0.118	-0.710
	(0.420)	(0.558)	(0.369)	(0.680)
More educated	-1.163**	-0.287	-0.0139	-0.627
	(0.458)	(0.731)	(0.401)	(0.808)
Treatment X More educated	0.113	0.569	-0.163	1.415
	(0.598)	(0.742)	(0.522)	(0.976)
Observations	436	390	455	455

Notes: Columns 1 and 3: OLS regressions with the following preference outcomes: risk aversion and impatience measured on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks) and impatience (i.e., lowest willingness to wait for future), respectively. Columns 2 and 4: Interval regression with the latent dependent variable that measures risk aversion in terms of risk aversion parameter and impatience in terms of discount rate. Table 3.B.1 in section 3.B includes a detailed explanation of these outcomes. Panel (A): The explanatory variables are as follows: 'Treatment' equal to one if the respondent was primed and zero otherwise; 'Older' equal to one if the respondent was above 40 years old and zero if the respondent was 40 or below 40 years old; and 'Treatment X Older' is the interaction term of these two indicators. Panel (B): The explanatory variables are as follows: 'Treatment' equal to one if the respondent was primed and zero otherwise; 'More educated' equal to one if the respondent had more than 3 years of schooling and zero if the respondent had 3 or less than 3 years of schooling; and 'Treatment X Older' is the interaction term of these two indicators. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. Robust standard errors are in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

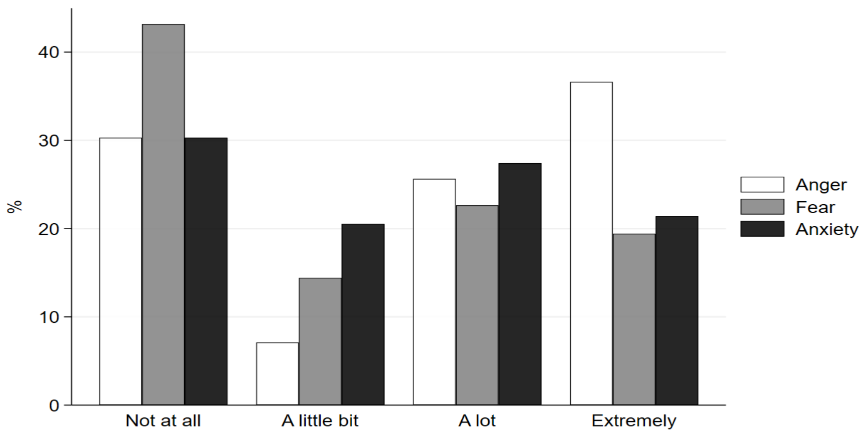
Table 3.H.2 Impact of the priming of IPV on women's preferences by baseline preference

	(1) Risk aversion (scale)	(2) Risk aversion (stair)	(3) Impatience (scale)	(4) Impatience (stair)
Treatment	-0.253	0.503	-0.271	0.318
	(0.287)	(0.706)	(0.299)	(0.755)
Higher baseline preference	3.743***	0.0184	3.132***	1.696**
	(0.356)	(0.707)	(0.402)	(0.863)
Treatment X Higher	-0.306	-1.405	0.902*	-0.780
	(0.483)	(0.874)	(0.469)	(1.019)
Observations	434	388	451	451

Notes: Columns 1 and 3: OLS regressions with the following preference outcomes: risk aversion and impatience measured on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks) and impatience (i.e., lowest willingness to wait for future), respectively. Columns 2 and 4: Interval regression with the latent dependent variable that measures risk aversion in terms of risk aversion parameter and impatience in terms of discount rate. Table 3.B.1 in section 3.B includes a detailed explanation of these outcomes. The explanatory variables are as follows: 'Treatment' equal to one if the respondent was primed and zero otherwise; 'Higher baseline preference' equal to one if the respondent had level of risk aversion (impatience) more than zero at baseline and zero otherwise; and 'Treatment X Higher baseline preference' is the interaction term of these two indicators. In all regressions, controls include women's age, education, length of marriage, employment status, husband's characteristics, number of sons and daughters and household's productive assets, savings, land ownership and gender of household head, BRAC programme fixed effects and the enumerator fixed effects. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.I Distribution of self-reported emotions

Figure 3.I.1 Distribution of self-reported emotional responses after watching the video



3.J Emotions triggered by the video, by different factors

Table 3.J.1 Emotions triggered by the video, by treatment status

	(1) Control	(2) Treatment	(3) Difference (1)-(2)
Anger	2.75	2.62	0.13
	(1.25)	(1.24)	(0.08)
Fear	2.23	2.14	0.08
	(1.21)	(1.16)	(0.08)
Anxiety	2.43	2.37	0.07
	(1.14)	(1.12)	(0.08)
N	442	456	898

Notes: Columns 1 and 2 report the mean of feelings of emotions (measured on a scale of 0-4) after watching the video among control and treatment groups. Column 3 reports the difference between columns 1 and 2. Standard deviations are in parentheses in columns 1 and 2 and standard errors are in parentheses in column 3.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.J.2 Emotions after watching video across age and level of education

	(1) Younger	(2) Older	(3) Difference (1)-(2)
Panel (A): Age group			
Anger	3.07	2.34	0.73***
	(1.11)	(1.27)	(0.08)
Fear	2.36	2.02	0.34***
	(1.21)	(1.15)	(0.08)
Anxiety	2.54	2.28	0.26***
	(1.10)	(1.14)	(0.08)
N	426	472	898
Panel (B): Education groups	Less educated	More educated	Difference (1)-(2)
Anger	2.43	2.97	-0.54***
	(1.28)	(1.15)	(0.08)
Fear	2.12	2.25	-0.13*
	(1.20)	(1.17)	(0.08)
Anxiety	2.30	2.52	-0.22***
	(1.15)	(1.10)	(0.08)
N	469	429	898

Notes: Mean of emotions across age and education groups. 'Anger' ('fear'/'anxiety) takes the value on a scale of 1-4 with 1 referring to feeling no anger ('fear'/'anxiety) at all and 4 referring to feeling angry ('fear'/'anxiety) extremely. Women with higher baseline preferences are those who reported baseline preferences more than zero and women with lower baseline preferences are those who reported baseline preferences greater than zero. Standard deviations are in parentheses in columns 1 and 2 and standard errors are in parentheses in column 3.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.J.3 Emotions after watching video across baseline preferences

	(1) Lower preference	(2) Higher preference	(3) Difference (1)-(2)
Panel (A): Risk sub-sample			
Anger	2.75	2.52	0.24**
	(1.23)	(1.31)	(0.12)
Fear	2.10	2.18	-0.08
	(1.17)	(1.20)	(0.11)
Anxiety	2.42	2.39	0.03
	(1.11)	(1.19)	(0.11)
N	220	219	439
Panel (A): Time sub-sample			
Anger	2.93	2.58	0.35***
	(1.21)	(1.22)	(0.11)
Fear	2.34	2.12	0.22**
	(1.23)	(1.15)	(0.11)
Anxiety	2.49	2.33	0.16
	(1.13)	(1.10)	(0.11)
N	216	236	452

Notes: Mean of emotions across age and education groups. 'Anger' ('fear'/'anxiety) takes the value on a scale of 1-4 with 1 referring to feeling no anger ('fear'/'anxiety) at all and 4 referring to feeling angry ('fear'/'anxiety) extremely. Panel (A): Older groups women are those who are 40 or above 40 years old and younger women are those who are below 40 years old (median age of our sample is 40 years). Panel (B): Women with >Class 3 are those who completed more than class 3 (median education) and women with $\leq Class3$ are those who completed class 3 or less (median education of our sample is class 3). Standard deviations are in parentheses in columns 1 and 2 and standard errors are in parentheses in column 3.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.K Correlation between IPV experiences and baseline preferences

Table 3.K.1 Correlation between IPV experiences and baseline preferences

	(1)	(2)	(3)	(4)
	Emotional IPV		Physical IPV	
	Risk-aversion (scale)	Impatience (scale)	Risk-aversion (scale)	Impatience (scale)
Recent IPV	-0.165	0.00170	-0.480	-0.391
	(0.339)	(0.455)	(0.385)	(0.515)
Ever but not recent IPV	0.144	-0.424	-0.247	-0.327
	(0.399)	(0.475)	(0.338)	(0.433)
Constant	2.747^{**}	5.568^{***}	3.047^{**}	5.777^{***}
	(1.223)	(1.552)	(1.200)	(1.573)
Observations	435	453	435	453

Notes: Dependent variables: risk aversion and impatience measured on a scale of 0-10, with 10 referring to the highest risk aversion (i.e., lowest willingness to take risks) and impatience (i.e., lowest willingness to wait for the future). Explanatory variables: 'Recent' equal to one if the woman experienced emotional (physical) IPV in the last 12 months and zero otherwise; 'Distant' equal to 1 if the woman experienced emotional (physical) IPV in their lifetime but not in the last 12 months. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Conclusion

My thesis investigates unexplored causes and consequences of IPV in the Global South. *My first chapter* analyses the influence of weather shocks on IPV in the Global South and investigates potential mechanisms. Prior studies examine the influence of climate change on IPV across different contexts (Epstein et al., 2020; Allen, Munala and Henderson, 2021; Munala et al., 2023; Ross et al., 2023; Izugbara et al., 2018; Sekhri and Storeygard, 2014; Rai, Sharma and Subramanyam, 2021; Dehingia et al., 2024). However, a comprehensive picture of the effects of climate change on IPV in the Global South remains unexplored. Using a dataset which combines historical weather data from the Climatic Research Unit of the University of East Anglia with nationally representative Demographic and Health Surveys (DHS), I assess the impact of rainfall and temperature shocks on physical, emotional, and sexual IPV across India, Sub-Saharan Africa (SSA), and Latin America (LA). My findings indicate that physical IPV increases with the frequency of positive temperature shocks, whereas the effects of rainfall shocks vary by region. Negative rainfall shocks in rural SSA lead to higher physical IPV, while positive shocks in urban LA have similar effects. In some regions, these shocks also contribute to increased emotional and sexual IPV.

Furthermore, I examine the impacts of weather shocks on intermediate outcomes, including spouses' employment and participation in intra-household decision-making. The results reveal that the likelihood of the wife working for someone else increases and the likelihood of the husbands working the entire year decreases with weather shocks in rural India, while in rural SSA, drought increases the wife's employment under someone else, while the husband's employment remains unchanged. These patterns might explain an increase in IPV during weather shocks in these regions. These findings call for early-warning systems and targeted interventions to mitigate the impact of weather shocks on IPV.

My second chapter examines the impacts of a credit-based graduation programme on the incidence of physical and emotional IPV in rural Bangladesh. Using an RCT with two-round endline surveys at one and seven years after the programme ended, I estimate both short- and long-run impacts of this programme to test whether the reductions in IPV resulting from this programme persist. I find that in the short run, women in the treatment group are less likely to experience physical and emotional IPV compared to the control group, while in the long run, women in the treatment group are more likely to experience physical and emotional IPV compared to women in the control group. Furthermore, the trend of IPV reveals that the control group experienced a large reduction in IPV between endlines 1 and 2. The treatment group also experienced reductions in IPV between endlines; however, the reduction rate of IPV for them is lower compared to that for the control group.

I also explore the potential mechanisms through which the programme might change the incidence of IPV. In the short run, the income and livestock assets of women in the treatment group are significantly higher compared to those of women in the control group, and the income of husbands of women in the treatment group is also higher than that of their counterparts in the control group. In the long run, treatment-control differences in these economic outcomes decline; however, the treatment group has better economic outcomes than the control group in absolute terms. Declines in treatment effects on economic outcomes might stem from stagnation in economic outcomes among the treatment group and an increase among the control group between endlines 1 and 2. The changes in economic outcomes might be linked to changes in credit market participation. I observe that the treatment group is more likely to borrow from BRAC compared to the control group in both the short and long run; however, the treatment effect declines in the long run. This pattern reflects the differential treatment-control differences in economic outcomes between endlines 1 and 2. Furthermore, the participation trend in the credit market between endlines reveals that women in the treatment group reduced borrowing from BRAC microfinance programme and increased their borrowing from non-BRAC NGOs, while women in the control group increased their borrowing from BRAC, and there was no change in their borrowing from non-BRAC NGOs between endlines. This pattern also helps explain the differences in the trajectory of economic outcomes between treatment and control groups.

These findings have both research and policy implications. First, the timing of the evaluation of the programme is more likely to influence the impacts on the incidence of IPV. Second, continuous improvements in the economic conditions of women might be a more influential driver in reducing IPV in contexts where women earn significantly less than their husbands. These findings suggest that policies promoting this driver might be an effective approach to achieving sustainable reductions in the incidence of IPV.

My third chapter examines the channel through which IPV might influence the economic decision-making of women. Particularly, I estimate the impacts of IPV priming on women's economic preferences. I conducted an experiment with 901 married women from rural Bangladesh. I employed an 'IPV priming module' consisting of standardised questions on IPV experiences and a short video, and I used a 'preferences module' to elicit economic preferences. In a survey, I randomised the order of these two modules to capture the impacts of IPV priming on women's risk and time preferences at the individual level. Women who were exposed to the IPV priming module *before* the preference module are referred to as treatment, whereas women in the control group are those who were exposed to the priming module *after* the preference module.

I estimate the impacts of IPV priming on risk and time preferences on average and distinguish these impacts by women's real-life IPV experiences. My results show that IPV priming reduces risk aversion, and women reported anger as a stronger emotional response to the priming compared to fear and anxiety. However, both women with and without IPV experiences in real life experience a similar level of reduction in risk aversion resulting from the priming, and there is no difference in their reporting of feelings of anger. This pattern is consistent with the existing literature showing that angry individuals tend to be less risk-averse (see, e.g., Lerner and Keltner, 2001; Lerner et al., 2015; Lerner and Tiedens, 2006; Carver and Harmon-Jones, 2009; Angus et al., 2015; Callen et al., 2014).

For time preferences, I do not observe any change in impatience resulting from the priming on average. This impact varies across women's real-life IPV experiences. Specifically, I find that the priming reduces impatience among women with emotional IPV experiences, while it increases among women without emotional IPV experiences in real life. As women with IPV experiences reported being more fearful and anxious than women without IPV experiences, the differences in impact between women with and without IPV experiences might be driven by fear and anxiety, rather than anger. This pattern does not align with the evidence from other studies, which suggests that fear and anxiety increase impatience (Callen et al., 2014; Haushofer and Fehr, 2014; Takahashi, 2004; Lerner et al., 2015).

I further distinguish the impacts of IPV priming by women's age, education and baseline preferences (i.e., preferences prior to the experiment), which tend to influence the impacts. I observe that the reduction in risk aversion due to IPV priming is more pronounced among younger women, which is consistent with the younger group reporting a stronger feeling of anger compared to the older group. Furthermore, the impacts on risk preferences marginally vary across women's education and baseline preferences, and the underlying emotion-based mechanism does not provide a clear explanation. Moreover, the impacts on time preferences do not vary by age and education, and marginally vary across baseline preferences.

Since my findings reveal that IPV priming changes women's economic preferences, IPV could have implications for women's economic decision-making. A reduction in risk aversion might be beneficial, such as increased investment, but at the same time, it could also be detrimental, leading to excessive borrowing and impulsive investments. Similarly, decreased impatience might improve long-term financial planning and human investment, but it could delay essential expenditures on health and nutrition or cause women to accept adverse situations while waiting for speculative future improvements. Furthermore, my findings also have two policy implications. First, the interventions reducing IPV might also reduce emotion-based decision-making. Second, interventions that address emotional responses, such as counselling and stress management programmes, might mitigate IPV-induced changes in preferences, thereby helping women make better economic decisions in high-IPV settings.

To conclude, my thesis contributes to the literature on the drivers of IPV by examining the impacts of climate change and a poverty reduction initiative on IPV. My thesis also contributes to the literature on the behavioural implications of IPV. IPV, as an important indicator of gender inequality, is a threat to human capital and economic progress.

Therefore, a broader understanding of its causes and consequences is crucial for improving the well-being of women, thereby promoting social and economic development. I think that the findings of my thesis will be useful for academicians, development practitioners and policymakers in enhancing their understanding of IPV, and these findings will contribute to the evidence and policies that are required to reduce gender disparities within the household. At the end of this thesis, I would like to take the opportunity to affirm my belief that inspired me to work on this issue: gender equality can never be achieved unless men and women share an equal position at home.

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