



# Domestic violence and workfare: An evaluation of India's MGNREGS

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## ABSTRACT

Economic shocks are commonly linked with domestic violence. This paper looks at how India's workfare program mediates the effect of income shocks on domestic violence. The Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS) guarantees 100 days of employment to rural households and acts as a form of insurance. Using the phased implementation of MGNREGS across districts in India from 2006 to 2008, I employ a difference-in-differences strategy to show that the introduction of the MGNREGS mitigates the effect of adverse rainfall shocks on officially reported domestic violence crimes at the district level by 8 to 22 percent. Using complementary household data from the India Human Development Survey, I explore the mediating effect of the MGNREGS on rainfall shocks and possible increases in women's empowerment. There are positive effects of participating in the scheme on women's freedom of mobility but inconclusive evidence on women's say in household decisions.

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## 1. Introduction

Domestic violence comprises the largest share of all reported crimes against women in India (NCRB, 2016). Almost one-fourth of Indian women say they have experienced it in the past year alone and 50 percent think it is justified under some circumstances (NFHS-4, 2017). There are several theories to explain the prevalence of domestic violence: evolutionary theories, social and family cultures of violence and feminist theories (Eswaran & Malhotra, 2011; Anderson, 1997; Alesina, Brioschi, & Ferrara, 2016). Despite being intrinsically pathological in nature, abuse also has economic underpinnings (Farmer & Tiefenthaler, 1997; Tauchen, Witte, & Long, 1991). Economic stress is considered an important trigger for mental illness and aberrant behavior (Rakovec-Felser, 2014; Linn, Sandifer, & Stein, 1985). Recent cross-country findings suggest that job losses experienced by men especially, are associated with higher incidences of intimate-partner violence (Bhalotra, Kambhampati, Rawlings, & Siddique, 2020).

A natural research question then arises whether policies that alleviate economic stress also reduce domestic violence. In rural India, where much of the population is still dependent on rain-fed agriculture, low rainfall is a form of productivity and income shock (Jayachandran, 2006). In this context, I examine the role of India's rural workfare program in mediating the impact of adverse

rainfall shocks on domestic violence. This paper is motivated by two strands of prior work. The first is research showing that the effect of shocks is more acute on marginalised members of the household, such as women or the elderly (Bhalotra, Kambhampati, Rawlings, & Siddique, 2018; Miguel, 2005; Sekhri & Storeygard, 2014). Sekhri and Storeygard (2014) show that dry rainfall shocks significantly increase the number of officially reported incidences of dowry deaths and domestic violence.<sup>1</sup> According to the authors, dowry payments behave as consumption smoothing mechanisms during weather shocks and domestic violence initiated to extract such payments may escalate ultimately into dowry deaths. The second motivating strand includes work looking at the insurance role of India's rural employment guarantee program. The program has been shown to mediate violent conflict (Fetzer, 2020), allow households to diversify into riskier crop choices (Gehrke, 2017), reduce female and overall infant mortality (Merfeld & Saha, 2018; Banerjee & Maharaj, 2020) and influence investments in human capital (Foster & Gehrke, 2017).

The Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA) is a national 'right to work' act that guarantees

<sup>1</sup> A dowry death is the death of a woman due to disputes related to the cash and assets given to the groom and his family by the bride's family. According to Section B of the Indian Penal Code, if a woman dies under suspicious circumstances within 7 years of marriage and there exists evidence to support that she was harassed by her husband or his family about dowry payment, the husband is held responsible for her death.

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100 days of unskilled manual work to all rural households and mandates that one-third of beneficiaries (workers) should be women.<sup>2</sup> It was first introduced in 2006 for the 200 most 'backward' districts in India and then extended to the rest of India over two phases in 2007 and 2008. The program is of particular importance during the agricultural lean season and for members of historically disadvantaged castes (Klonner & Oldiges, 2014). Research in specific parts of India have found that the MGNREGS increases household food security and savings, and reduces the incidence of depression (Ravi & Engler, 2015).

In this paper, I conduct two sets of analyses at the district and household level to examine how the effect of adverse income shocks, proxied by rainfall, changes due to the MGNREGS. For the district-level analysis I use data on officially reported crimes of domestic violence from the National Crime Records Bureau. The staggered implementation of the scheme allows me to employ a difference-in-differences empirical strategy across districts and phases. By interacting the rainfall shock with a treatment indicator of whether the MGNREGS is available in the district, I find that dry shocks have a positive and significant effect on officially reported domestic violence and the introduction of the scheme reduces this by 8–22 percent. The mediation effects are significant only for dry shocks occurring in the crucial agricultural months and not during the rest of the year, lending support to the insurance mechanism. Identification is threatened if the assumption of parallel trends does not hold for districts in the three phases and to address this, I conduct placebo tests, and robustness checks on other crimes against women.<sup>3</sup>

To explore whether the same relationship holds at the household level and other possible mechanisms, I use household survey data from the India Human Development Survey (IHDS). The survey asks female respondents whether domestic violence is usual in their communities. While the survey is administered to individual women, the response may or may not pertain to their own experience and respondents may be giving their impression of community prevalence of domestic violence. To address this limitation, I show that the association between participation in the MGNREGS and the *deviation* of individual responses from village averages also holds. The first round of the IHDS was conducted in 2004–05 before the MGNREGS was implemented and the second round in 2011–12 when the scheme covered the whole country. Since participation in the MGNREGS is available only in the later round of surveys, I first present cross-sectional estimates from 2011–12 and show that women who have ever worked in the scheme have a 4 percent lower likelihood of reporting that domestic violence is usual in the community. To causally identify the effect of participating in the scheme, I use the *difference* between the prevailing MGNREGS wage rate and women's agricultural wages as an excluded instrument. The instrument is valid if the wage-differential influences women's participation in the scheme but does not affect their individual reporting of domestic violence, conditional on covariates. A more detailed discussion of the identification strategy and the exclusion restriction is given in Section 4.

The IHDS is a panel dataset but because the MGNREGS was not rolled out in any district in the first survey round (2004–05) and coverage already expanded to the whole country in the second round (2011–12), I do not have time-varying participation in the scheme. However, I exploit the panel structure of the data by

interacting MGNREGS participation in 2011–12 with time-variant rainfall shocks. Although I cannot estimate the direct effect of MGNREGS participation, I can estimate how participation interacts with rainfall shocks to affect domestic violence reporting, while partialling out individual fixed effects. The estimations show that dry rainfall shocks have a positive and significant effect on reporting that domestic violence is usual and this is reduced with participation in the workfare scheme. These findings are consistent with the district-level estimations using officially reported data and support the insurance mechanism of MGNREGS. Estimates of the program's effect on women's mobility are positive and significant but I find limited evidence on improvements in women's say in household decisions.

There is substantial research highlighting the economic underpinnings of domestic violence and its instrumental motives. Bloch and Rao (2002) illustrate how domestic violence is used as an extortion mechanism to appropriate resources from the wife's natal family. In South India, Srinivasan and Bedi (2007) find that higher initial payments of dowry tend to lower marital violence via their effect on household resources and more recently, Menon (2019) finds a positive relationship between the price of gold (and thus, lower dowry endowments) and domestic violence. This paper is closely aligned to an emerging body of work on the effect of policy instruments such as cash transfers and workfare on intimate-partner violence (Hidrobo, Peterman, & Heise, 2016; Haushofer, Ringdal, Shapiro, & Wang, 2019; Buller et al., 2018; Merfeld, 2020; Ramos, 2016). In a context where extreme weather events are becoming more common, research on the role of social protection policy and its impact on women's safety is even more salient.

The first contribution of this paper is to assess the effect of workfare and specifically the MGNREGS, on proximate outcomes such as gender relations within the household. There exist several evaluation studies of the program on immediate and secondary labor market outcomes (Imbert & Papp, 2015; Azam, 2012; Merfeld, 2019; Merfeld, 2020), and child labour and education outcomes (Afridi, Mukhopadhyay, & Sahoo, 2016; Shah & Steinberg, 2015). However, there is a limited but growing literature on its effect on domestic violence and women's decision-making power. Exceptions include recent work by Field, Pande, Rigol, Schaner, and Moore (2016) who look at the effects of MGNREGS payments into the woman's own bank account and by Tagat (2020) on intra-household decision making. This paper is also closely related to previous work by Amaral, Bandyopadhyay, and Sensarma (2015) who look at the direct effects of the MGNREGS on official crimes against women at the district level by comparing districts in only Phase I and III.<sup>4</sup> My paper departs from theirs by focusing on the insurance function of the MGNREGS and not its direct impact which may occur via multiple mechanisms, possibly even in opposing directions. By using different datasets at the district and household level, I consider alternative channels of causation, such as increased empowerment, and find most support for the insurance role.

The second contribution of this paper is to a broad literature on the relationship between women's empowerment and domestic violence.<sup>5</sup> There are four channels via which this relationship manifests itself: bargaining, exposure to the spouse, backlash and reporting effects. The household bargaining model of violence predicts lower levels of violence if the woman's outside option captured by

<sup>2</sup> The NREGA was later renamed as the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA) in 2009 and I use this henceforth. To refer to the corresponding scheme and employment program undertaken within the act, I use MGNREGS.

<sup>3</sup> The National Crime Records Bureau classifies crimes against women into the following major groups: rape, kidnapping and abduction of women, dowry deaths, assault, insult to modesty of women, cruelty by husband or his relatives, importation of girls from a foreign country and abetment of suicide of women.

<sup>4</sup> More recent work by Amaral and co-authors also looks at the reporting effects of all-women police stations on gender-based violence (Amaral, Nishith, & Bhalotra, 2018).

<sup>5</sup> A comprehensive overview of women's empowerment and its relation to economic outcomes is given by Duflo (2012). For an Africa-specific examination of the link between intimate-partner violence and its link with both ancient cultural traditions and current economic roles, refer to Alesina et al. (2016).

earnings or property rights improves (Aizer, 1847; Panda & Agarwal, 2005; Mathur & Slavov, 2016).<sup>6</sup> On the other hand, Lenze and Klasen (2017) do not find any significant effect of female employment on domestic violence in Jordan. Eswaran and Malhotra (2011) examine the relationship in the opposite direction, with domestic violence being used by the husband to control his wife's autonomy and increase his own say within the household. The exposure reduction channel relies on the idea that reduced exposure to the spouse when the women is employed, implies reduced occasion for abuse (Chin, 2012).

Studies positing the backlash effect of female empowerment find that relative increases in women's earning abilities challenge the norms of male dominance within the household (Bhalotra et al., 2018). This result of greater marital violence is found in several developing country contexts, e.g. by Bulte and Lensink (2019) in Vietnam, by Luke and Munshi (2011) among certain castes in tea-growing estates of India and by Blanco and Villa (2008) in Mexico. The other channel via which the empowerment of women may affect domestic violence is through reporting effects. Increased political representation of women leads to greater officially reported crimes as women are more willing to file complaints with the police (Iyer, Mani, Mishra, & Topalova, 2012). My findings run counter to both the exposure and backlash channel. While I cannot completely rule out reporting effects and find some improvements in empowerment, they do not seem to be driving the main results. Instead, the evidence suggests that India's workfare program attenuates economic stress which is a potential trigger for domestic abuse.

The rest of the paper is organised as follows: Section 2 provides greater detail on the implementation of the MGNREGA and some context of domestic violence in India. The datasets used for my empirical analysis are described in Section 3. Section 4 describes my empirical strategy and results using district and household level data separately. The last section concludes my paper and discusses some policy implications for the Indian context. In the Appendix, I present some tables as supporting evidence and additional results on the risk mitigating role of the MGNREGS.

## 2. Background

### 2.1. Mahatma Gandhi National Rural Employment Guarantee Act

The MGNREGA is a national 'right to work' law passed in 2005 and implemented in three phases at the district level between 2006 to 2008. It guarantees 100 days of unskilled manual work to all rural households and mandates that one-third of beneficiaries (workers) should be women. Although households are free to allocate labor days among any of its adult members, the MGNREGS has consistently exceeded this target.<sup>7</sup> The MGNREGA, despite originally not having female empowerment as its primary objective, is often hailed as a women's act (Narayanan & Das, 2014; Pankaj & Tankha, 2010). Since wages for women are usually lower than that for men, wage equality within MGNREGS yields greater gains for women (Khera & Nayak, 2009).

The provision of work under the MGNREGS is supposed to be demand driven. Adult members have to apply for a job card at the Gram Panchayat (village council) and after verification, are guaranteed employment within 15 days of application. Particularly relevant for female workers is that the work-site has to be located

within 5 km radius of their residence and childcare facilities are supposed to be provided for sites with more than five children under 6 years of age.<sup>8</sup> Despite encouraging legal provisions at the national level, implementation of and participation in the MGNREGS varies across states. According to IHDS data, the share of women having ever participated in MGNREGS ranges from 48 percent in the South to only 11 percent in Western India. Irregularities regarding delays in payment, inadequate work-site facilities, leakages and varying levels of political support are serious hurdles for the scheme in some states (Bhatia et al., 2006). Field studies indicate positive perceptions towards the act and its implications for women's empowerment (Khera & Nayak, 2009; Pankaj & Tankha, 2010) and secondary data have shown that women's participation in the MGNREGS exceeds that in other forms of wage labor (Dasgupta et al., 2011). However, there are a limited number of studies that estimate the effect on domestic violence with large-scale survey data.

Approximately 25 percent of the IHDS sample of rural women report to have ever participated in the MGNREGS. These participants comprise an equal share of women who were already working in 2004–05 when the program was not available (51 percent, Table 1, Column 2) and who opted into the MGNREGS from unemployment or unpaid labor. Since women substituted into the MGNREGS from other paid employment, there is a possible revealed preference for employment conditions under the MGNREGS. To be considered as working for pay, the woman must have spent at least 240 h (30 days) in the past year either as a salaried worker, agricultural wage laborer or a non-agricultural wage laborer (IHDS user guide, 2004–05). There is no minimum hour criterion to be considered a participant in the MGNREGS. Further, female participants in the MGNREGS are not necessarily employed continuously: among the women who reported having ever worked in the program, only 67 percent continued to work in 2011–12, in MGNREGS or elsewhere (Table 1, Column 3). This picture is coherent with the MGNREGS's role as 'employment of the last resort' as households are using it in times of need and it is weaker households, such as members of historically disadvantaged castes or those who have experienced household shocks, who are more likely to opt into the program (Dasgupta et al., 2011; Wray, 2006). It also illustrates the transitory nature of women's work in rural India.

In addition to changes on the extensive margin, MGNREGS is also associated with changes on the intensive margin, i.e. number of hours worked. While the overall reduction in total labor hours for working women between 2004–05 and 2011–12 is by 6 labor days (4 percent), the reduction for MGNREGS participants is much more, by 30 labor days (20 percent). Women who did not work in the MGNREGS, in fact, increased their work hours (Table 2, top panel). This effect is present for men as well, but the percent decline in labor hours is by slightly less (Table 2, bottom panel). The overall declining trend of female labor force participation in India has been much commented upon (Chatterjee, Murgai, & Rama, 2015) and despite not claiming causality, the MGNREGS have been ascribed a potential role in the literature (Andres, 2017). A substantial portion of the withdrawal from the labor force is by rural women and "there is a high probability that with increasing rural affluence they moved out of low paying self-employment activities choosing either to study or devote themselves to domestic duties. . . MGNREG appears to have driven up private sector remunerations for men and women. This increase in incomes has possibly led to large withdrawals of women from the labor force." (Rangarajan, 2018).

<sup>6</sup> More evidence on the effects of employment on women's household bargaining power can be found in Getahun and Villanger (2017).

<sup>7</sup> In the past 5 years alone, more than 50% of the total person-days generated by MGNREGS have been for women. [http://mnregaweb4.nic.in/netnrega/all\\_lvl\\_details\\_dashboard\\_new.aspx](http://mnregaweb4.nic.in/netnrega/all_lvl_details_dashboard_new.aspx). [http://nrega.nic.in/Nrega\\_guidelinesEng.pdf](http://nrega.nic.in/Nrega_guidelinesEng.pdf) Accessed October 19, 2018

<sup>8</sup> In case work-sites are beyond 5 km of their home, the state must pay conveyance charges to the beneficiary. [http://planningcommission.gov.in/reports/genrep/mgnar-ega\\_guidelines\\_2012.pdf](http://planningcommission.gov.in/reports/genrep/mgnar-ega_guidelines_2012.pdf) Accessed November 6, 2018

**Table 1**

Share of women working for pay across survey rounds by whether they have ever participated in the MGNREGS.

Ever participated in MGNREGS	Share working for pay (%)	
	2004–05	2011–12
(1)	(2)	(3)
No	22.24	23.16
Yes	50.62	66.87
Overall %	25.1	34.23

Source: IHDS 2004–05 and 2011–12, Note: Data are weighted by IHDS 2004–05 survey weights. IHDS 2004–05 records individuals as being in paid employment if they reported working for a minimum of 240 h (30 days) in the past year, as per the IHDS 2004–05 definition. The same definition is used for IHDS 2011–12 for consistency and is inclusive of work in MGNREGS, if over 240 h. Women having ‘ever’ participated in MGNREGS does not depend on a minimum hour criterion and this variable is from the 2011–12 survey round.

**Table 2**

Total annual hours worked across survey rounds by gender and participation in the MGNREGS in 2011–12.

Participating in MGNREGS 2011–12	Hours Worked		Percent Difference (%) $H_0: w_0 - w_1 = 0$ (p-value)
	2004–05 ( $w_0$ ) (1)	2011–12 ( $w_1$ ) (2)	
<i>Women</i>			
No	1190	1237	3.9 (0.000)
Yes	1220	978	–19.8 (0.000)
Overall Average	1198	1148	–4.1 (0.006)
<i>Men</i>			
No	1722	1769	2.7 (0.003)
Yes	1615	1330	–17.6 (0.000)
Overall Average	1692	1661	–1.8 (0.016)

Source: IHDS 2004–05 and 2011–12, Note: Data are weighted by IHDS 2004–05 survey weights. The sample includes women and their spouses who have worked a minimum of 240 h (30 days) in the reporting year, as per the IHDS 2004–05 definition. Participation in MGNREGS does not depend on a minimum hour criterion.

Despite households being eligible for 100 days of employment, the average number of days worked in the MGNREGS ranges from 14, 50 and 54 days in households where only the husband, only the wife, and where both spouses work in the program, respectively (Appendix Table 12). Appendix Table 12 shows an illustrative multinomial logit estimation to identify some predictors of participation and *who* among the household participates. The greater the MGNREGS wages are compared to male agricultural wages, the more unlikely it is that only the wife will work (Column 1). Conversely, if the MGNREGS wage gain for women is higher, it is unlikely that only the husband will work (Column 2). Higher education, assets and number of adults in the household all predictably, reduce the likelihood of participation. This is to a large extent due to rationing of work but also because of the MGNREGS’ role of providing flexible employment when needed, i.e. during the lean agricultural season. The most common responses when asked why the full 100 days are not used are: that there was not enough work, followed by the respondent being not interested and low wages. Across reasons, the average number of days worked by the woman in the scheme is consistently higher than her spouse, implying that households allocate labor such that the wage gains are highest.

MGNREGA was implemented in a phased manner, covering 200 districts in Phase 1 implemented on February 2, 2006, and was extended to 130 additional districts between 2007 and 2008. All remaining districts with any rural populations were included on

April 1, 2008.<sup>9</sup> The districts in Phase 1 were among the least developed in the country with little presence of government institutions. The classification of these districts as ‘backward’ was based on the population share of Scheduled Castes (SC) and Scheduled Tribes (ST) according to the 1991 census, the level of agricultural wages in 1996–97 and output per worker in 1990–93 (Planning Commission, 2003). However, the ranking of districts based on these criteria was adjusted such that Phase 1 districts were distributed across different states.<sup>10</sup> According to the Planning Commission (2003), the presence of local institutions is weaker in backward districts implying that the implementation of MGNREGA would be more difficult there than in the rest of the country.

## 2.2. Domestic violence in India

The neglect of Indian girl children in their natal homes has been extensively documented (Sen, 1992; Jayachandran & Kuziemko, 2011; Oster, 2009). My research examines relations in the woman’s marital home where she may be actively subject to abuse. More than 50% of Indian women think that it is justified for a husband to beat his wife under some circumstances (NFHS-4, 2017). The circumstances range from the wife disrespecting her in-laws (37%), neglecting housework or the children (33%), arguing with him (29%), going out without telling him (26%), being unfaithful (23%), not cooking properly (19%) or refusing sexual intercourse (13%). These estimates have not changed since the last NFHS survey in 2005–06. While there is much variation across states with respect to attitudes towards domestic violence being justified, ranging from 7% in Sikkim to above 80% in Manipur, Andhra Pradesh and Telangana, they are nationally pervasive. Domestic violence makes up the largest share (32.6%) of all crimes against women (NCRB, 2016).<sup>11</sup>

## 3. Data

This paper uses multiple data sources to empirically test the relationship between the MGNREGS and domestic violence. The main dependent variable for the district level analysis is registered, that is, officially reported cases of domestic violence from the National Crime Records Bureau (NCRB) and is denoted as ‘cruelty by the husband or his relatives’.<sup>12</sup> The NCRB crime data are available annually at the district level which include urban as well as rural areas. The districts which are entirely urban and those crimes that are committed on Indian Railways premises have been omitted. My analysis pertains to the 2001–13 period.

To capture rainfall shocks, I use data from the Global Precipitation Climatology Centre provided by the Earth System Research Laboratory’s Physical Sciences Division. This dataset has been widely used in economics research (e.g. by Miguel, Satyanath, & Sergenti (2004)) and covers the full period of interest (2000–13) on a monthly basis (GPCC Full Data Monthly Product - V7). It contains gridded measurements of global land-surface precipitation based on approximately 80,000 stations world-wide with a spatial resolution of  $0.5^\circ \times 0.5^\circ$  latitude by longitude.<sup>13</sup> Each coordinate is matched to the Indian administrative district that it falls within and the average of all such points gives the overall average annual

<sup>9</sup> Out of the 200 districts in Phase 1, 119 are in just seven states: Bihar, Chattisgarh, Jharkhand, Madhya Pradesh, Orissa, Rajasthan and Uttar Pradesh.

<sup>11</sup> Since the official NCRB crime category for domestic violence also includes cruelty by other family members, I use this term over intimate-partner violence.

<sup>12</sup> The data I use are of crimes as reported to police stations. My analysis does not include information on whether the cases have moved to judicial proceedings or any prosecution. These data can be accessed from the online version of Crimes in India (2015) or from India’s Open Government Data (OGD) Platform. <https://data.gov.in> Accessed November 6, 2018.

<sup>13</sup> <https://www.esrl.noaa.gov/> Accessed November 6, 2018.

<sup>9</sup> <http://www.nrega.nic.in/netnrega/forum/2-MGNREGA.pdf>

rainfall for that district. Rainfall is measured in millimetres and deviations are calculated from a long term mean over the period 1981 to 2010 and standardised using the district specific standard deviation in the sample period.

In my estimations, I condition on district characteristics such as total population, number of persons literate and employed, and share of SC/ST populations from the Indian Census 2001. These variables are available only in 2001 and are not time-varying, so I interact them with a linear year-trend to capture how the effects of these baseline values on officially reported crimes change over time. Over the sample period, many new districts have been created by splitting up existing districts or merging different parts of several districts. Between 2001 and the next Census conducted in 2011, 47 new districts were created leading to a total of 640.<sup>14</sup> To maintain consistency across years and data sources, I consolidate new districts with their 'parent' districts as long as they were included in the same phase of MGNREGA implementation. If not, these districts are excluded.

District level summary statistics for the period before MGNREGA implementation, i.e. 2005 and earlier, are presented in Table 3. The officially reported incidence of domestic violence is slightly higher in Phase 2 and 3 districts relative to districts in Phase 1, but the levels are comparable. This pattern is not consistent across all other crimes against women. Dowry deaths, rape and assault crimes are reported higher in Phase 1 districts whereas kidnapping and insults to the modesty of women are highest in Phase 3 districts. The differences in levels can partly be attributed to differences in rates of reporting but it is not possible to discern the relative magnitudes of under-reporting across the districts and different types of crimes against women.

I use the India Human Development Survey (IHDS) as my main data source for the household analysis. The IHDS is a nationally representative panel survey of 41,554 households across villages and urban neighbourhoods in India (Desai et al., 2008; Desai & Vanneman, 2015). The first round of surveys were completed in 2004–05 and the second in 2011–12. The survey covers multiple topics ranging from issues in health, education, household employment and consumption. This paper uses data from the IHDS module on ever-married women (EW) aged 15–49 years and restricts attention to only rural areas and those women present in both rounds of survey with non-missing data bringing the overall sample size to around 13,000 women. Corresponding household and village level data have been included from separate modules. Summary statistics are presented in Table 4. The average female respondent in the 2011–12 survey round is 40 years old with primary level schooling, was likely to have been married before age 18, is from a disadvantaged caste and has 3 children.

The variables of interest are indicators of domestic violence equal to 1 if the respondent says it is 'usual in the community for the husband to beat his wife' in the following circumstances and 0 otherwise: she goes out without his permission, her natal family does not give expected money (or dowry), she neglects the house or her children, she does not cook properly and if she disrespects elders in the family.<sup>15</sup> The experience of domestic violence is a sensitive issue and there are many different survey methodologies to elicit this information - e.g. the list method, direct questioning or asking about the experiences of others. To avoid endangering the respondent, the IHDS asks whether spousal violence is 'usual' in the community rather than about the respondent's own experience of it (Desai et al., 2008). This has some limitations but it

also avoids the problem of under-reporting that is associated with answering about one's own experiences. Around 73 percent of women report that it is usual for the husband to beat his wife in at least one circumstance (Table 4).

Another reason for preferring the IHDS over other data sources, such as the NFHS, is its detailed information on women's participation in economic activities and employment status - 34 percent of women report working for pay in the 2011–12 round. The main explanatory variables for the analysis using household-level data are: whether the woman ever worked in the MGNREG scheme (25 percent of the sample), the number of days worked by her and by other members in the household. When the first round of IHDS was completed in 2004–05, none of the households in the sample had access to MGNREG schemes (which were rolled out in 2006). In the second round of surveys in 2011–12, MGNREG schemes had been rolled out across the whole country. Some households opted in while others did not. Since MGNREGS related variables are only available in the second round, I first use cross-sectional data from 2011–12 to estimate the relationship between participation and domestic violence and the mechanisms that drive it. Next, to examine its role in mediating rainfall shocks, I exploit the panel structure by interacting the 2011–12 MGNREGS participation with time-varying rainfall shocks. This partials out the effect of individual fixed unobservables from the relationship between rainfall and domestic violence, and the interaction effect captures how participation mitigates the impact of shocks, similar to the district level estimations using officially reported data. The next section discusses my identification strategy in more detail.

## 4. Empirical strategy and results

### 4.1. District level identification

I use district level data to test whether the MGNREGS mediates the relationship between officially reported domestic violence crimes and rainfall shocks, thereby acting as a form of insurance. Following Wooldridge (2010), Sekhri and Storeygard (2014) and Fetzer (2020), I use a Poisson specification in a panel setting, as crimes reported are count data, as well as Within estimators using linear specifications. The conditional log mean of the dependent variable in the Poisson specification is:

$$\log E(C_{dt}) = \alpha R_{dt} + \beta MGNREGS_{dt} + \gamma (R_{dt} \times MGNREGS_{dt}) + \mathbf{X}'_{dt} \delta + \theta_d + \kappa_t + \psi_{dt}, \quad (1)$$

where  $C_{dt}$  is the number of officially reported crimes against women,  $R_{dt}$  is a rainfall shock and  $MGNREGS_{dt}$ , an indicator of whether the MGNREG scheme is available in district  $d$  and year  $t$ . The coefficient of interest is  $\gamma$ , which captures the change in the effect of shocks induced by the introduction of MGNREGS. I standardize the district level rainfall variable by calculating the deviation from the long-run mean and divide by the district specific standard deviation in the sample period. I use a linear spline at  $-1$ , i.e. one standard deviation below the long run average, to separately estimate the slope coefficient of dry and wet rainfall realizations. This is a piecewise regression which allows the slope of the regression to change at a specific point, equal to 1 standard deviation below the mean in my estimation.<sup>16</sup> The estimations are not sensitive to this specification, and using a 'drought' indicator equal to 1 if rainfall is in the lowest quintile of the district distribution and 0 otherwise, does not change my main result. The vector  $\mathbf{X}'_{dt}$

<sup>14</sup> As of 2018, there are 712 districts in total.

<sup>15</sup> The circumstance of an extramarital affair is also listed among the questions but has been omitted since there is limited variation in responses. The question on respecting elders was not included in IHDS 2004–05 so has been omitted from panel estimations. These situations are similar to those used by the NFHS as well.

<sup>16</sup> A similar specification is used by Sekhri and Storeygard (2014). I use Stata's `mkspline` command to split the standardized rainfall score and generate two continuous variables: a dry shock is equal to the rainfall score below  $-1$ , and wet rainfall is equal to the score above  $-1$ .

**Table 3**  
District level summary statistics by MGNREGS phase (before implementation)

	Phase 1 (1)	Phase 2 (2)	Phase 3 (3)	Total (4)
	Mean (standard deviation)			
<i>Crime</i>				
Domestic violence	76.13 (124.36)	78.43 (106.55)	88.91 (116.37)	82.34 (117.19)
Dowry death	13.18 (17.22)	10.99 (12.62)	9.48 (12.05)	11.05 (14.19)
Rape	32.42 (30.33)	27.08 (28.36)	23.13 (23.70)	27.11 (27.40)
Kidnap	19.91 (23.12)	21.12 (23.35)	24.65 (29.96)	22.29 (26.52)
Assault	58.02 (59.58)	51.65 (57.82)	52.01 (65.16)	53.94 (61.82)
Insult to modesty	11.51 (30.55)	14.13 (31.64)	18.98 (59.03)	15.42 (45.80)
<i>Rainfall</i>				
Annual rainfall (mm.)	1273.45 (685.62)	1481.05 (966.22)	1249.91 (916.67)	1308.38 (862.75)
Standardized rainfall	-0.21 (0.89)	-0.25 (0.86)	-0.28 (0.90)	-0.25 (0.89)
<i>Demographic</i>				
Population ('000s)	1794.17 (1297.96)	1682.87 (1406.17)	1698.14 (1424.09)	1726.99 (1379.45)
Persons literate ('000s)	855.17 (709.24)	872.39 (868.56)	999.49 (897.40)	923.36 (834.94)
Persons employed ('000s)	729.55 (500.68)	648.64 (531.20)	660.19 (573.67)	680.91 (541.83)
Scheduled Caste (%)	15.43 (8.66)	15.73 (8.46)	14.68 (8.35)	15.16 (8.49)
Scheduled Tribe (%)	22.97 (25.88)	16.82 (26.77)	13.51 (26.24)	17.69 (26.55)

Source: District level crime data are from the NCRB and rainfall data are from ESRL for the period 2001–05. Demographic data are from Census 2001. Note: Rainfall is standardized by subtracting the long-run average annual rainfall from the realized rainfall in the given year for a district and dividing by the district-specific standard deviation for the sample period.

**Table 4**  
Female respondents' characteristics in 2011–12.

Variable	Mean	Std. Dev.	Min	Max	N
<i>Women's characteristics</i>					
Age (in years)	39.37	8.40	20	70	13218
Child marriage	0.64	0.48	0	1	13218
Number children alive	3.22	1.56	0	13	13218
Education (Completed Years)	2.96	3.97	0	16	13218
Spouse education (Completed Years)	5.45	4.70	0	16	13218
Domestic violence (DV) is usual	0.73	0.45	0	1	13218
DV deviation from PSU mean	0.08	0.43	-1	1	13218
Working for pay	0.34	0.47	0	1	13218
Ever worked in MGNREGS	0.25	0.44	0	1	13218
Days worked in MGNREGS	37.30	27.76	1	100	4365
<i>Household characteristics</i>					
Household asset index (0–30)	12.36	5.33	0	29	13218
Brahmin	0.04	0.19	0	1	13218
Forward caste	0.14	0.34	0	1	13218
Other Backward Castes (OBC)	0.39	0.49	0	1	13218
Dalit	0.25	0.43	0	1	13218
Adivasi	0.08	0.28	0	1	13218
Muslim	0.09	0.29	0	1	13218
Christian, Sikh, Jain	0.02	0.12	0	1	13218
HH has MGNREGS job card	0.18	0.39	0	1	13218
Number of MGNREGS days worked by HH	49.06	31.37	1	100	5209
<i>Village characteristics</i>					
Male agricultural wage (Rs.)	158.33	67.92	19	500	13218
Female agricultural wage (Rs.)	122.22	53.06	20	425	13218
MGNREGS daily wage rate (Rs.)	123.65	19.56	40	300	13218

Source: Data are from IHDS 2011–12 and weighted using sample weights. Note: Domestic violence indicator refers to the woman responding it is usual in the community for at least one of the circumstances asked. 'DV deviation from PSU mean' is the sum of circumstance in which domestic violence is usual reported by the woman minus the village average. A woman is considered to be working for pay if she reports being in paid employed for a minimum of 240 h (30 days). Number of days worked in the MGNREGS for the woman and the household is conditional on participation in the scheme during the survey year.

includes district level covariates from the Census 2001: population, share of Scheduled Tribes and Scheduled Castes, persons literate and employed, all interacted with a linear time trend. The terms  $\theta_d$  and  $\kappa_t$  are the district and year fixed effects respectively and  $\psi_{dt}$  is the idiosyncratic error term.

There are two threats to identification for the specification in Eq. 1: the assumption of parallel trends for districts in the three phases not holding and measurement error in the dependent variable.<sup>17</sup>

In order to deal with the assumption of parallel trends, I perform some robustness checks. First, I conduct a placebo test by bringing forward, i.e. hypothetically implementing the MGNREGS earlier than the actual year of implementation by one and two years, and testing the 'hypothetical' effect on reported crimes of domestic violence. If the findings are driven purely by different trends in districts according to their assigned phase, then the effect should be present before MGNREGS implementation as well, i.e. prior to 2006. Recent advances in econometrics show that the difference-in-differences estimator with two-way fixed-effects and staggered treatment, such as the introduction of MGNREGS in 3 phases with district and year fixed-effects used in this paper, is actually a weighted average of all possible  $2 \times 2$  estimates (Goodman-Bacon, 2021; Sun & Abraham, 2020; Callaway & Sant'Anna, 2020). Districts in each phase are compared to those in the other two phases, before and after treatment, leading to a total of 6 estimates. The weights on the estimates depend on group sizes and variance in treatment such that for the two-way estimator to be unbiased, we would also require treatment effects to be constant over time. While a full Bacon-decomposition of the two-way fixed effects estimator is out of scope of this paper, I conduct an event study with the full set of leads and lags to implementation of the MGNREGS (Cunningham, 2021) to show that trends in the effect of dry weather shocks were similar across districts in the three phases.

Second, I test whether the mediating effect of MGNREGS on adverse dry shocks is present even for other crimes against women, such as rape, kidnapping, etc. My argument is that the implementation of MGNREGS affects domestic violence by acting as a form of insurance for the household. If instead, the effect is via improvements in law and order or different trends in districts, then it should be present for other crimes against women as well.

Domestic violence is known to be under-reported in India. In case the degree of under-reporting is uncorrelated with the explanatory variables, the estimated coefficients will be unbiased and consistent but the standard errors will be inflated, which strengthens the significant findings of this paper. However, the coefficients may be inconsistent if under-reporting of crimes is systematically correlated with adverse rainfall shocks and further, that this correlation changes once MGNREGS are introduced in the district. If dry shocks and the reporting of domestic violence are negatively correlated, the positive coefficient on dry shocks and the subsequent mediation effect of MGNREGS are underestimated. If they are positively correlated, the increase in crimes during years with negative rainfall shocks could be a pure reporting effect that for some reason reduces once the workfare program is introduced. Unless MGNREGS also brings about a deterioration in

local law and order or increases social costs which make women reluctant to report such crimes, this should not be the case. Khanna and Zimmermann (2017), in fact, find greater information sharing and support between civilians and the police after MGNREGS were implemented which makes this unlikely. Further, anecdotal and descriptive findings using IHDS survey data suggest that women's autonomy (Khera & Nayak, 2009) and freedom of mobility increases with participation in the MGNREGS, so the costs of reporting domestic violence crimes may be lower post-implementation. If this is the case, the attenuation effect of the scheme that I find is likely underestimated.<sup>18</sup>

#### 4.2. District level results and robustness checks

The main results are presented in Table 5. Column 1 estimates the effect of a drought, defined as annual rainfall in the lowest quintile of the district rainfall distribution, as increasing reported domestic violence by 0.140 log points or by 15 percent. This increase is mitigated once the MGNREGS is introduced by 0.078 log points, or a decrease of 8 percent. Column 2 finds that the direction and magnitude of the results are stable after controlling for trends in baseline district characteristics though the significance is slightly weaker. Column 3 also uses a Poisson specification but the effect of the rainfall shock is separately estimated for dry and wet shocks. Dry shocks, defined as a continuous variable equal to the standardized rainfall score less than  $-1$ , increases reported incidences of domestic violence by 0.118 log points. This is equivalent to an increase of 12 percent in the average number of domestic violence crimes reported. The implementation of MGNREGS mutes this effect by 0.244 log points or equivalently by 22 percent. Using a within-estimator in Column 4, I find that a dry shock increases the number of domestic violence crimes reported by 14, i.e. by 12 percent of the average number of crimes in a year. The MGNREGS' mediating effect on dry shocks once it is implemented in the district, is to reduce the reported incidence by 42, i.e. 37 percent of the average. The net effect of a dry shock after MGNREGS is the sum of both or  $-25$  percent. These results hold in the Column 4 estimation of the same model excluding the trend in baseline employment levels, which may suffer from post-treatment bias.

I find that the result is stronger for more extreme dry shocks, defined by 1.5 standard deviation below the long-term mean or lower, that occur during the key months of June, July, August and September (which are crucial for the kharif crop) but not so for wet shocks and for shocks occurring during other seasons (Table 5, Column 5). Kharif is India's main agricultural season that begins with the onset of the monsoons.<sup>19</sup> These results hold while controlling for district and year fixed effects, and time trends of the total district population, SC and ST shares, literate and employed persons. It is interesting to note that the introduction of MGNREGS does not have a significant effect during wet shocks. This could suggest that the program becomes more relevant during droughts. In absolute terms, the magnitude of the negative coefficients on wet shocks is much smaller than on dry shocks - consistent with findings of Sekhri and Storeygard (2014). While the direct impact of rainfall shocks and the mediation effect of MGNREGS are consistent across model specifications, the direct effect of the program is not. However, it has a significant negative sign in the preferred specification with the full set of controls (Column 3). Substantial evidence exists on gains in wages and output (Imbert & Papp, 2015; Azam, 2012; Cook & Shah, 2020), and women's empowerment due to the MGNREGS. These could be other important channels through which

<sup>17</sup> I thank an anonymous referee for raising a question about the Stable Unit Treatment Value Assumption (SUTVA) which would be violated if individuals systematically migrated across districts to get access to the MGNREGS before their designated phase. Firstly, migration in India is historically low, particularly rural-rural migration unless among women for purposes of marriage (Munshi & Rosenzweig, 2016). Secondly, households have to register for the MGNREGS with the Gram Panchayats (village councils or GP) of their residence. The workfare scheme is not portable across locations and benefits are restricted to the village in which the household resides according to documents submitted to and verified by the GP at the time of application.

<sup>18</sup> I thank an anonymous referee for this insight.

<sup>19</sup> More than 60% of agriculture in India is rainfed. <http://www.worldbank.org/en/news/feature/2012/05/17/india-agriculture-issues-priorities>.

**Table 5**  
Effect of rainfall shocks and MGNREGS on reported domestic violence crimes.

	Dependent variable: Reported domestic violence crimes (mean = 121)				
	Poisson (1)	Poisson (2)	Poisson (3)	Within (4)	Within (5)
Drought indicator	0.140*** (0.034)	0.107*** (0.024)			
MGNREGS × Drought indicator	-0.078** (0.035)	-0.060* (0.033)			
Dry shock			0.118* (0.060)	11.134* (6.159)	
MGNREGS × Dry shock			-0.244*** (0.077)	-43.120*** (13.688)	
Wet shock			-0.035** (0.017)	-4.073** (2.004)	
MGNREGS × Wet shock			0.006 (0.019)	1.518 (2.789)	
MGNREGS	0.008 (0.043)	0.021 (0.039)	-0.239** (0.104)	-46.693*** (16.893)	-0.870* (0.494)
JJAS dry shock					0.239** (0.110)
MGNREGS × JJAS dry shock					-0.633*** (0.143)
JJAS wet shock					-0.019 (0.015)
MGNREGS × JJAS wet shock					0.005 (0.017)
JFM, AM, OND shocks	No	No	No	No	Yes
MGNREGS × JFM, AM, OND shocks	No	No	No	No	Yes
District controls	No	Yes	Yes	Excl. emp	Yes
Number of Obs	6577	6577	6577	6577	6577
Number of Districts	507	507	507	507	507
R-Squared				0.229	
Chi-squared	552	729	844		1101

Source: Data are from NCRB 2001–13 on officially reported crimes of domestic violence. Note: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors, given below coefficients in parentheses are clustered at the district level. Drought is an indicator equal to 1 if the annual rainfall is below the 20th percentile of the district-specific rainfall distribution and 0 otherwise. Dry and wet shocks are continuous variables equal to the standardized rainfall score below and above -1, respectively. Column 5 uses a -1.5 as the threshold or 'knot' to distinguish dry and wet shocks for separate seasons. JJAS refers to June, July, August and September, which are crucial for the Kharif crop. Wet and Dry shocks during all other seasons are (i) January, February and March (JFM) (ii) April and May and (AM) (iii) October, November and December(OND). District controls include interactions of 2001 levels of population, SC share, ST share, literacy and employment with a linear time trend. All specifications include district and year fixed effects.

the MGNREGS affects domestic violence, in addition to its risk-alleviating role. I examine changes in empowerment using household survey data in the following sections.

In Table 6, I use a placebo test by hypothetically bringing forward the implementation of MGNREGS by one (Columns 1 and 2) and two years (Columns 3 and 4) and restrict attention to 2005 and before period such that none of the districts actually had the program. The placebo thus, assigns districts to the program earlier than the actual year of roll-out. If the main findings are driven by different trends, the districts which got the program earlier should still show greater decreases in domestic violence crimes than those that got it later, even when the program itself was not implemented. However, this is not the case. While dry shocks still lead to an increase in reported domestic violence which is consistent with earlier results, the placebo interaction does not have a same significant effect. In fact, Table 6 finds a weakly significant but positive effect of the placebo interacted with the dry shock (Column 2) and a stronger positive effect of the wet shock interaction with the placebo (Columns 3 and 4). These findings are counter to those presented in Table 5.

In Fig. 1, I show the results of an event study by regressing the full set of indicators of the 'time to MGNREGS implementation' interacted with dry and wet rain shocks on domestic violence crimes. The district-panel length runs from 2001–2013 so Phase 3 districts which get the program in 2008 are 7 years from treatment in 2001, 6 years from treatment in 2002 and so on, until they are 5 years into treatment in 2013. Years before and after implementation are shown on the x-axis with negative and positive values, respectively. Similarly, for Phase 1 (2) districts, the time to

MGNREGS implementation is equal to -5 (-6) years in 2001 and equal to 7 (6) years in 2013 as they have been exposed to the program for that long. Fig. 1 (top) presents the estimated coefficients of all the interactions of 'time to MGNREGS' with the dry shock using the year of treatment as the reference category. The coefficient plots show that while there is no clear trend before MGNREGS, there is a significant downward trend after implementation. The baseline value (=0) shows the direct positive effect of dry shocks. The results for the interactions with wet shock are less conclusive (Fig. 1, bottom). While the coefficients are mostly negative and significant before MGNREGS implementation, there seems to be no clear trend either before or after implementation. Similar results hold for the estimation with district controls.

Further, in case the assumption of parallel trends fails and the effect found in Table 5 is actually driven by different trends of gender-based crimes that coincide with the implementation of MGNREGS, it should also hold for other crimes against women. Table 7 checks this and finds that it is not the case for any of the other crimes. Dry shocks do not significantly increase other reported crimes against women. This finding (or lack thereof) provides support for the insurance mechanism which would not be present for other crimes, except possibly for dowry deaths, but would affect domestic violence. However, Table 7 finds significantly negative direct effects of MGNREGS on assaults and insult to the modesty of women, which are further strengthened during dry shocks. Kidnapping crimes see a significant attenuation effect of the program but the direct effects are not significant. These findings may be due to improvements in income from the workfare program which breaks the link between crime and poverty. The



**Table 6**  
 Placebo test of effect of MGNREGS and rainfall shocks on domestic violence

	Dependent variable: Reported domestic violence crimes (mean = 121)			
	Within (1)	Poisson (2)	Within (3)	Poisson (4)
Dry shock	10.017*** (3.360)	0.102** (0.043)	12.336*** (3.645)	0.134*** (0.048)
Wet Shock	-5.132*** (1.593)	-0.049*** (0.016)	-6.018*** (1.584)	-0.061*** (0.017)
Placebo1 (1 yr earlier)	16.535 (17.673)	0.538* (0.326)		
Placebo1 × Dry shock	14.469 (13.777)	0.484* (0.291)		
Placebo1 × Wet shock	5.671 (3.961)	0.028 (0.046)		
Placebo2 (2 yrs earlier)			-13.672 (18.996)	-0.102 (0.196)
Placebo2 × Dry shock			-12.810 (17.440)	-0.102 (0.178)
Placebo2 × Wet shock			7.621** (2.967)	0.076** (0.031)
Number of Obs	2470	2470	2470	2470
Number of Districts	494	494	494	494
R-Squared	0.066		0.067	
Chi-Squared		91		89.8

Source: Data are from NCRB 2001–13 on officially reported crimes of domestic violence. Note: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors, given below coefficients in parentheses are clustered at the district level. Dry and wet shocks are continuous variables equal to the standardized rainfall score below and above -1, respectively. All specifications include district and year fixed effects and interactions of 2001 levels of population, SC share, ST share, literacy and employment with a linear time trend.

positive signs for the interaction term of the program with wet shocks is inconsistent with the findings for domestic violence, suggesting that other mechanisms may be at play.

The MGNREGS were rolled out such that ‘backward’ districts received the program earlier in Phase 1. They may not be very comparable to the districts in the later two phases. As a robustness check, I restrict my estimations to only Phase 2 and 3 and show that the findings do not change (Appendix Table 14). The coefficients on the interaction term are marginally higher.

### 4.3. Household level identification

To examine the relationship between domestic violence and workfare, and its possible channels with household survey data, I use the following specification:

$$v_{id} = \alpha + \beta w_{id} + \mathbf{x}'_{id}\gamma + \eta_d + \epsilon_{id}, \tag{2}$$

where  $v_{id}$  refers to whether the ever-married woman  $i$  residing in district  $d$  says it is usual for a husband to beat his wife in the community. The key variable of interest is MGNREGS participation,  $w_{id}$ , which refers to either an indicator of whether the woman ever participated in the program or the number of days worked therein during the current year. The matrix of covariates  $\mathbf{x}'_{id}$ , includes respondent and spouse characteristics such as age, education, and household attributes such as caste, religion and asset score. District specific effects are included in  $\eta_d$  and  $\epsilon_{id}$  is the idiosyncratic error term. This estimation uses cross-section data from the IHDS 2011–12.

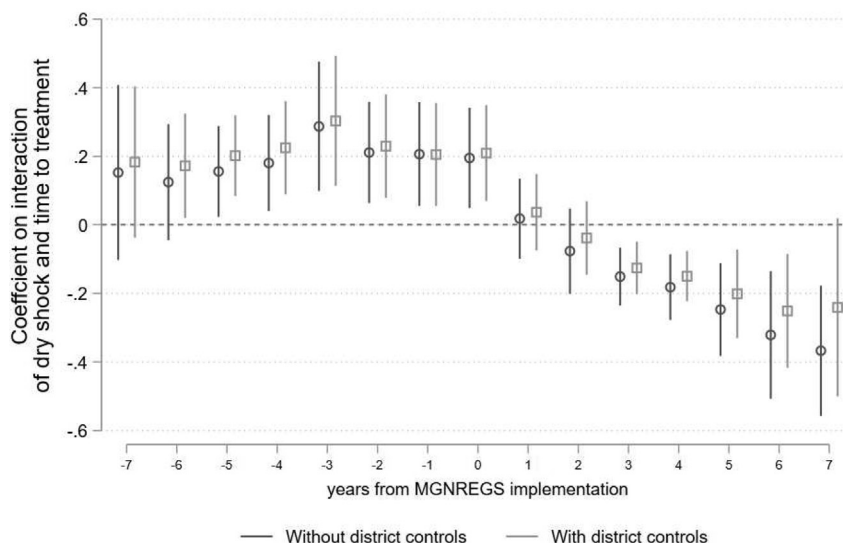
I use the same specification (Eq. 2) to test possible channels through which MGNREGS participation might affect domestic violence. To explore the insurance channel further, I use the number of days worked by other members of the household, *without* the woman herself working in the scheme, in place of the explanatory variable  $w_{id}$ . If the effect of the MGNREGS operates via its role in alleviating risk, it should hold regardless of which household member participates in it. I also use self-reported measures of ‘say’ in household decisions and the woman’s freedom of mobility as dependent variables to test whether participation in MGNREGS impacts domestic violence by increasing women’s bargaining

power within the household. These include indicators of whether the woman can visit a health center, a friend or relative, and the grocery store alone and whether she has the most say in decisions related to cooking, large purchases, number of children to have, what to do when a child is ill and about her child’s wedding.

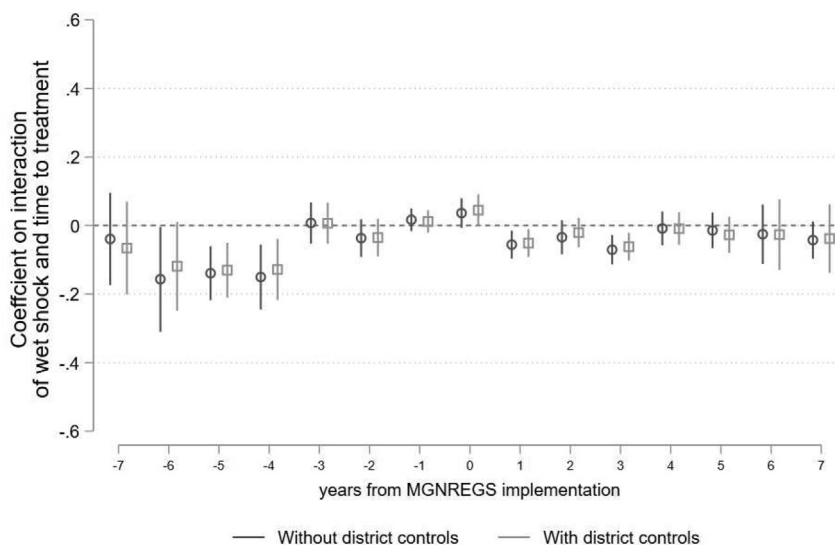
A limitation of the IHDS data, mentioned earlier, is that the respondent is asked whether it is usual in the community for the husband to hit his wife in specific circumstances. This information pertains not only to the individual’s experience but to the whole community. Thus, it is possible that village level unobservables are driving both reports of domestic violence being usual, as well as MGNREGS participation. As a robustness check, I use the *deviation* of the individual’s report of domestic violence from the village mean as the dependent variable and show that the association with participation in the MGNREGS still holds.

The specifications using household survey data (Eq. 2) can be estimated using a linear probability model or a probit model. However, there is a problem of endogeneity in the form of unobservables at the household or individual level that may drive the relationship between MGNREGS participation and reporting that domestic violence is usual. To address this, I use the *difference* between the MGNREGS wage and women’s agricultural wages as an instrument for participation in the program. This wage-differential is determined at a higher level, i.e. the village and is not directly chosen by the household but influences participation. The relevance criterion of the wage differential in determining who within the household participates was discussed earlier (Appendix Table 12). However, the exclusion restriction is at best, an assumption. Since the IHDS sample households are followed over two survey rounds in the same location, they have not systematically moved to other districts/villages to benefit from a higher wage differential. However, villages with higher wage gaps between MGNREGS and agriculture may be systematically different from other villages and this may drive the relationship between participation in MGNREGS and reporting that domestic violence is usual.<sup>20</sup> To address this concern, I control

<sup>20</sup> Merfeld (2019) shows that the implementation of MGNREGS brings about wage and employment changes in casual labour markets.



Note: The year of MGNREGS treatment is the baseline (=0) which captures the direct effect of dry shock. Results from Poisson estimation.



Note: The year of MGNREGS treatment is the baseline (=0) which captures the direct effect of wet shock. Results from Poisson estimation.

**Fig. 1.** Event study: Coefficient estimates of dry (top) and wet (bottom) shocks interacted with ‘time to MGNREGS’ indicators using a Poisson regression. Both estimations include district and year fixed effects and the district controls are interactions of 2001 levels of population, SC share, ST share, and literacy with a linear time trend.

for the average agricultural wage (including both men and women) in the village which may independently have effects on labour supply, household income and domestic violence. I show the results of instrumentation using both a control function approach and 2 Stage Least Squares (2SLS). The control-function approach allows for a regression based Hausman-test and is considered more appropriate for non-linear models, such as in Eq. 2 with a continuous endogenous variable (number of days worked in the MGNREGS) and a non-linear second stage (binary indicator whether domestic violence as usual) (Wooldridge, 2015; Rivers & Vuong, 1988).

My last set of results pertain to whether the same mediating relationship with rainfall shocks, found with district level data, holds at the individual level. I exploit the panel structure of the data using both survey rounds 2004–05 and 2011–12 with the following specification:

$$v_{idr} = \alpha + \beta(w_{id} \times R_{dr}) + \gamma R_{dr} + \delta x_{idr} + \eta_i + v_r + \epsilon_{idr}, \tag{3}$$

where as before,  $v_{idr}$  refers to whether the woman  $i$  in district  $d$  says it is usual for a husband to beat his wife in the community. The variation across survey rounds in 2004–05 and 2011–12 is captured with the additional subscript  $r$ . Since information on MGNREGS participation,  $w_{id}$  is only available in the 2011–12 survey round, I cannot partial out individual fixed effects and estimate the direct effect of participation. However, time variation in district level rainfall shocks ( $R_{dr}$ ) allows me to estimate how the effect of rainfall changes for those who participate in the program through  $\beta$ , the coefficient on the interaction term. While I cannot account for other time-varying unobservables that may drive domestic violence, I include household assets as controls ( $x_{idr}$ ) and individual and round specific effects (by  $\eta_i$  and  $v_r$ , respectively). The idiosyncratic error term is  $\epsilon_{idr}$ . I estimate Eq. 3 using the conditional logit estimator to account for the binary dependent variable.

**Table 7**  
Effect of rainfall shocks and MGNREGS on other reported crimes - Within estimations.

	Dependent variable: Officially reported crimes against women				
	Dowry Death (1)	Rape (2)	Kidnap (3)	Assault (4)	Insult to Modesty (5)
Dry shock	-0.234 (0.411)	-0.287 (1.134)	1.433 (1.653)	3.335 (2.174)	0.906 (1.224)
MGNREGS	-0.082 (0.906)	-1.072 (2.469)	-5.288 (3.868)	-11.090** (5.291)	-6.197* (3.401)
MGNREGS × Dry shock	-0.221 (0.591)	-1.751 (1.899)	-5.014* (2.831)	-9.610** (3.904)	-4.251** (1.965)
Wet shock	-0.155 (0.148)	-0.891*** (0.339)	0.374 (0.450)	-0.784 (0.798)	-0.094 (0.562)
MGNREGS × Wet shock	0.250 (0.199)	2.350*** (0.468)	0.429 (0.721)	3.647*** (1.152)	1.018 (1.005)
Dependent variable mean	12.96	34.36	38.23	66.50	14.80
Number of Obs	6642	6642	6642	6642	6642
Number of Districts	512	512	512	512	512
R-Squared	0.093	0.180	0.518	0.189	0.048

Source: Data are from NCRB 2001–13 on officially reported crimes of domestic violence. Note: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors, given below coefficients in parentheses are clustered at the district level. Dry and wet shocks are continuous variables equal to the standardized rainfall score below and above -1, respectively. All specifications include district and year fixed effects and interactions of 2001 levels of population, SC share, ST share, literacy and employment with a linear time trend.

#### 4.4. Household level results - mechanisms

In this section, I explore which mechanisms explain the relationship between the workfare program and domestic violence: reduction in exposure to the spouse, backlash, increases in women’s empowerment, reporting effects or the risk-alleviation channel.

According to IHDS data presented earlier in Table 2, women who worked in the MGNREGS and in the previous survey round, reduced their total hours of work by approximately 240 h. This and the fact that most cases of domestic violence occur outside of working hours and at night (Catalano, 2006) dismisses the explanation that the relationship between MGNREGS and domestic violence operates by reducing the wife’s exposure to the spouse. This explanation might be more appropriate for programs involving work-related migration.

Table 8 shows the correlation between working in the MGNREG scheme and reporting that domestic violence is usual in the community while controlling for individual and household attributes such as age, years of education, spouse characteristics, asset levels, caste, religion and district specific effects. Column 1 shows that having ever worked in MGNREGS is associated with a 4 percent lower likelihood of reporting domestic violence as usual (in at least one circumstance).<sup>21</sup> Column 2 finds that even if the respondent herself does not work in the MGNREGS, an additional day worked by other members of the household still has a 0.1 percent significantly lower likelihood of reporting domestic violence as usual. Columns 3 and 4 find negative and significant relationships between respondents’ deviations from the village mean of circumstances in which domestic violence is usual and participation in MGNREGS. The negative correlation between participation and domestic violence contradicts the backlash channel of causation since the effect is in the reverse direction.

To identify the causal effect of participation, Table 9 uses the village level difference between the MGNREGS wage and women’s agriculture wages as an excluded instrument in a 2SLS (Columns 1–6) and control function approach (Column 7). The 2SLS estimations indicate a negative and significant effect of days worked in

the MGNREGS on reporting domestic violence as usual in the community for least one circumstance (Column 1). This effect is stronger and significant for the circumstances of inadequate dowry and when the wife neglects household duties (Column 3 and 4). The control-function estimation shows that the same significant negative relationship found earlier still holds despite the non-linear second-step (Column 7). The regression based Hausman test by including the residuals from the first-stage suggests that there is indeed a problem of endogeneity that warrants the use of an instrument. The first-stage regression (Column 7) is common for the preceding estimations with the a Kleibergen-Paap F-statistic of 16.39.10.

Next, I examine whether the relationship between MGNREGS and domestic violence is driven by changes in women’s empowerment and agency in the household. The IHDS provides self-reported data by the respondent (EW) on freedom of mobility and who has the ‘most say’ in household issues which includes matters of cooking, large purchases, number of children to have, what to do when the child is ill and the child’s wedding. The variables related to her mobility include her freedom to go alone to visit the health centre, friends and relatives and to buy groceries. Using the specification in Eq. 2 where the dependent variables are now indicators of freedom of mobility, I find that there are strongly significant and positive effects of women’s participation in the program (Columns 1,3 and 5). The correlations do not hold, however, when other members of the household participate, excluding the woman herself (Columns 2,4 and 6). The encouraging improvements in the woman’s mobility, especially when she herself works in the MGNREGS suggest that empowerment may be an important channel for the direct effect of the MGNREGS on domestic violence. As women work outside their homes, they may be viewed as productive assets or be more in control over their own welfare.<sup>22</sup> However, these results are not robust to causal identification and there are limited changes in women’s ‘say’ in household decisions (Appendix Table 13). The slow moving nature of social norms has been pointed out by Field et al. (2016) and in a patriarchal context, changes in household decision making may be hard to detect in self-reported data. Another limitation is that the IHDS does not have an option for responding that decisions are jointly made - a clear improvement over others having sole control

<sup>21</sup> Alcohol abuse is known to be high risk factor for domestic violence (Jewkes, 2002). Due to its sensitive nature, the IHDS has many missing observations for the question on alcohol consumption and the sample size decreases when it is included. However, the size and significance of the marginal effect (not shown) does not change upon inclusion.

<sup>22</sup> I thank an anonymous referee for this insight.

**Table 8**  
Correlation between MGNREGS participation and domestic violence - OLS estimations.

	Dependent variable: Whether domestic violence is usual in community as			
	Woman's binary response		Deviation from village mean	
	(1)	(2)	(3)	(4)
MGNREGS (W ever worked)	-0.036** (0.016)		-0.045*** (0.015)	
MGNREGS (HH days worked)		-0.001** (0.001)		-0.001** (0.000)
Spouse education (years)	-0.005*** (0.001)	-0.005*** (0.001)	-0.004*** (0.001)	-0.004*** (0.001)
Dependent variable mean	0.73	0.73	0.04	0.04
R-squared	0.060	0.061	0.056	0.055
Number of obs.	15711	13661	15711	13661

Source: Data are from IHDS 2011–12. Note: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors, given below coefficients in parentheses, are clustered at the village (primary sampling unit or PSU) level for all estimations. All specifications include the woman's (W) and household (HH) characteristics including age, years of completed education, assets, and fixed effects for district, caste and religion. Columns 2 and 4 are estimated on a restricted sample of observations where the woman, herself, did not work in the MGNREGS during the survey year.

**Table 9**  
Effect of MGNREGS participation on domestic violence - 2SLS and control function estimation.

Dependent variable:	Circumstances in which woman reports that domestic violence is usual:							First-Stage MGNREGS days (8)
	Any (1)	Leaves (2)	Dowry (3)	Neglects 2SLS (4)	Cooks (5)	Disrespects (6)	Any Control Function (7)	
MGNREGS (W days worked)	-0.017* (0.009)	-0.015 (0.009)	-0.033*** (0.012)	-0.022** (0.010)	-0.005 (0.009)	-0.011 (0.010)	-0.017* (0.010)	
Residuals							0.017* (0.010)	
$W \text{ Wage}_{NREGS} - \text{Wage}_{Agri}$								-0.043*** (0.011)
Dependent variable mean	0.68	0.55	0.34	0.45	0.34	0.50	0.68	5.05
Kleibergen-Paap F-statistic								16.394
Number of obs.	13404	13404	13404	13404	13404	13404	13160	13404

Source: Data are from IHDS 2011–12. Note: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors, given below coefficients in parentheses, are clustered at PSU level for IV estimations and cluster bootstrapped at PSU level for the control function estimation. All specifications include the woman's (W) and household characteristics including age, years of completed education, assets, average village level agricultural wage, district, caste and religion dummies. Column (8) refers to the First-Stage OLS estimation with number of days participated in MGNREGS as the dependent variable.

**Table 10**  
Effect of MGNREGS participation on freedom of mobility - OLS estimations

	Dependent variable: Able to visit alone to:					
	Health Center		Friend or relative		Grocery Shop	
	(1)	(2)	(3)	(4)	(5)	(6)
MGNREGS (W worked)	0.043*** (0.015)			0.041*** (0.014)		0.055*** (0.013)
MGNREGS (HH worked)		0.013 (0.025)			0.031 (0.020)	0.007 (0.022)
Number of obs.	13036	11208	13036	11208	13036	11208
R-squared	0.043	0.043	0.046	0.045	0.041	0.039

Source: Data are from IHDS 2011–12. Note: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors, given below coefficients in parentheses, are clustered at PSU level. All specifications include respondent (W) and household (HH) characteristics including age, years of completed education, assets, average village level agricultural wage, district, caste and religion dummies. Columns 2,4 and 6 are estimated on a restricted sample of observations where the woman, herself, did not work in the MGNREGS during the survey year.

(Annan, Donald, Goldstein, Martinez, & Koolwal, 2021; Bussolo, Sarma, & Williams, 2021).

Lastly, Table 11 explores the risk-alleviating channel and whether the district-level results also hold for household data. To account for the binary dependent variable in a panel setting, I use conditional logit estimations to examine the effect of rainfall shocks on women's reporting that domestic violence is usual in the community, and whether MGNREGS participation mitigates it (Mátyás & Sevestre (2013), Chapter 7). Higher levels of rainfall, measured by a standardized score, reduce the reporting of domestic violence as usual (Table 11). This is further lessened with

participation in MGNREGS by the individual women (Column 1), and by others in the household even when she doesn't participate herself (Column 3). These results hold regardless of the rainfall specification: dry and wet shocks, defined by a linear spline around -1, continue to have a positive and negative associations with domestic violence, respectively. The interaction of the dry shock with days worked in the MGNREGS is weakly significant and participation has no effect on wet shocks (Columns 2). All of these estimations use rainfall shocks in the year prior to the survey and are not significant for current shocks. One reason this may be is that the IHDS surveys are held throughout the year and there

**Table 11**  
MGNREGS participation, rainfall shocks and domestic violence - conditional logit estimations using panel data

Dependent variable - Domestic violence is usual in any circumstance (mean = 0.5)			
	(1)	(2)	(3)
Standardized rain score <sub>t-1</sub>	-0.069*** (0.024)		-0.049* (0.026)
MGNREGS <sub>t</sub> (W days worked) × Rain score <sub>t-1</sub>	-0.003** (0.001)		
MGNREGS <sub>t</sub> (HH days worked) × Rain score <sub>t-1</sub>			-0.007** (0.003)
Dry shock <sub>t-1</sub>		0.367*** (0.117)	
MGNREGS <sub>t</sub> (W days worked) × Dry shock <sub>t-1</sub>		-0.023* (0.012)	
Wet shock <sub>t-1</sub>		-0.117*** (0.027)	
MGNREGS <sub>t</sub> (W days worked) × Wet shock <sub>t-1</sub>		-0.002 (0.001)	
Chi-squared	1309.409	1542.166	716.204
Number of obs.	15564	15564	13528
Number of individuals	7782	7782	6764

Source: Data are from IHDS 2004–05 and 2011–12. Note: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Standard errors, given below coefficients in parentheses, are bootstrapped for all estimations. All specifications control for HH assets, individual effects and survey round.

may be a rainfall shock in the current year occurring in a month after the household interview. The data would therefore not capture its effects.

## 5. Discussion

This paper examines the role of policy in mediating the effect of income shocks on domestic violence. Looking at India's rural employment guarantee program, I find that the introduction of the MGNREGS reduces the effect of a dry rainfall shock on reported crimes of domestic at the district level by 8–22 percent. At the individual level, participation in the program is associated with a 4 percent lower likelihood of reporting that it is usual for a husband to beat his wife under some circumstances. The main mechanisms supported by my findings is that the workfare program reduces economic stress which is considered a trigger for abuse. There are improvements in indicators of women's empowerment such as freedom of mobility, suggesting that higher agency may be a complementary mechanism but limited effects on say in household decisions.

This paper is related to the ongoing debate about alternative instruments of social insurance in India.<sup>23</sup> While a lot of academic work on the MGNREGS has been on its labor market impacts, changes in proximate spheres such as the household should also be considered. Possible effects along the gender dimension need to be kept in mind when piloting other social protection instruments. This research also highlights the discrepancy between women's participation in the overall labor force (approximately, 25 percent) to their share of person-days generated in MGNREGS (more than 50 percent).<sup>24</sup> A better understanding of why work in MGNREGS is attractive and how these conditions - flexibility, proximity to residence and childcare facilities - can be replicated elsewhere is needed.

Changing attitudes and beliefs about gender roles is a slow albeit necessary process (Jayachandran, 2019), but realigning economic incentives to achieve better outcomes for women may be feasible in the short run. One unanswered question is whether

the effects of such programs are persistent or taper off once benefits are withdrawn.

## Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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## Appendix A. Supplementary data

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<sup>23</sup> For context, the Economic Survey 2018 dedicated an entire chapter to the 'radical' option of a universal basic income. <https://www.indiabudget.gov.in/es2016-17/echap09.pdf> Accessed January 24, 2019.

<sup>24</sup> World Development Indicators, 2017 and <http://mnregaweb4.nic.in> Accessed January 24, 2019.

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