ISER Working Paper Series

Do improved property rights decrease violence against women in India?

Sofia Amaral
Institute for Social and Economic Research
University of Essex

No. 2017-13
October 2017

INSTITUTE FOR SOCIAL
& ECONOMIC RESEARCH

University of Essex

E·S·R·C ECONOMIC & SOCIAL RESEARCH COUNCIL

www.iser.essex.ac.uk

Non-Technical Summary

Improvements in property rights are advocated to be one of the most powerful development tools used that empower individuals and improve economic conditions. In this paper, I investigate the consequences of improving in inheritance rights – a form of property rights - for women in India on violence committed against them. Violence against married women in India is prevalent across the country and the consequences of this for women's human rights, health and the well-being of their children are rampant. To prove a causal effect, I exploit the staggered implementation of a legal change in inheritance law in India that led to an increase in women's access to their share of the inheritance. This legal amendment was introduced in five different states of India starting in 1976 up to 1994 and affected women of the Hindu religious groups (i.e. Hindu, Buddhists, Jains or Sikhs). The analysis makes use of this variation as a natural experiment and a novel dataset on police-reported crimes, gender-specific mortality measures and self-reported intimate-partner violence.

I find that the aggregate rate of violence against women (including female suicides) fell. This fall is due to changes in police-reported violence as well as in female mortality. The law did not change male mortality or other forms of non-gender based crime. This suggests that there is a decrease in the prevalence of violence against women. Further, at the individual level, I find that spouses eligible for inheritance are 17 percent less likely to be victims of domestic violence. These findings are explained by an improvement in marriage market negotiations and this led to women marrying partners that consume less alcohol.

Gender equal inheritance legislation that grants equal inheritance rights across siblings is not yet available in many developing countries – in specific in about a third of the countries sampled in the Women and Business Report of the World Bank in 2012. This paper shows that legislation changes aimed at improving women's economic conditions prevent violence against women, a public health and social problem that affects 1 in 3 women globally.

Do improved property rights decrease violence against women in India?*

Sofia Amaral†

This Version: July 2017
First Version: December 2012

Abstract

This paper uses the staggered implementation of a legal change in inheritance law in India to estimate the effect of women's improved access to inheritance on violence against women. I find that the aggregate rate of violence against women (including female suicides) fell. This fall is due to changes in police-reported violence as well as in female mortality. The law did not change male mortality or other forms of non-gender based crime. Further, at the individual level I find that spouses eligible for inheritance are 17 percent less likely to be victims of domestic violence. These findings are explained by an improvement in marriage market negotiations and this led to women marrying partners that consume less alcohol.

Keywords: Domestic Violence, Property Rights, Intra-household Distribution, Marriage Market
JEL Classification: J12, J16, K42, O15

*I am thankful to Siddhartha Bandyopadhyay. I also appreciate comments received from Sanchari Roy, Emily Owens, Abhiroop Mukhopadhyay, Michael Henry, Rudra Sensarma, Facundo Albornoz, Anindya Banerjee, Ravi Kanbur, Siwan Anderson, Sonia Bhalotra, Mike Brewer, Giovanna D'Adda, André Silva, and seminar participants at the Royal Economic Society Conference 2015; University of British Columbia; Research in Economic Development Meeting, University of Ottawa; Summer School in Development Economics; Journées d'Economie Públique; and the 2013 Annual Conference on Economic Growth and Development, ISI-Delhi. I acknowledge the support of the Fundação para a Ciência e Tecnologia (grant reference: SFRH/BD/78034/2011). This work was also supported by the Economic and Social Research Council (ESRC) through the Research Centre on Micro-Social Change (MiSoC) at the University of Essex, grant number ES/L009153/1. The usual disclaimer applies.
†Contact: Institute for Social and Economic Research, University of Essex. sfmont@essex.ac.uk

1 Introduction

About a third of women globally experienced some form of violence inflicted by their partner (WHO, 2013). In addition, intimate-partners are the main perpetrators of female homicides. This makes violence committed against women a major social and public health concern that threatens economic growth and the prospects of eliminating extreme poverty. To counter this issue, several countries have passed legislative rights promoting female empowerment, economic and social equality, and control over one's body (Doepke et al., 2011; Doepke and Tertilt, 2014; Stevenson and Wolfers, 2006; Iyengar, 2009; Aizer and Dal Bó, 2009). This paper investigates the role of legislation that promotes gender equality on gender-based violence by analyzing the effects of improvements in inheritance rights in India on violence committed against women, and, on marriage conditions.

I identify the causal effect of improved inheritance rights on women's well-being by exploiting an exogenous change in inheritance legislation that affected India's Hindu religious majority group: the Hindu Succession Act (HSA). This inheritance rights reform is unique in several aspects. First, inheritance rights in developing countries such as India are a major form of income and consequently, having secure property rights is likely to increase production and investment (Banerjee et al., 2002). Second, equalizing gender rights through legislation (La Ferrara and Milazzo, 2014; Harari, 2014) is absent in most developing countries (Figure 1). In addition, the implications of equal access to inheritance rights is relevant as it is well-established that targeting income or land resources to women has implications for female empowerment, fertility, higher infant-survival rates, improved labour and political participation, and in reducing the gender wage gap (Duflo, 2011; Strauss and Thomas, 1995; Hidrobo et al., 2016; Bobonis et al., 2013). Using the timing, state and religious variation in access to inheritance, I examine the effects of an improvement in inheritance rights on three distinct measures of violence against women: aggregate police reported violence against women, gender-specific unnatural death rates and individual-level self-reports of domestic

violence.[1]

I find that violence committed against women (VAW) fell in states where inheritance rights were made equal between men and women. This result is consistent across the three different measures of VAW, and its magnitude is non-trivial as aggregate VAW fell by 36 %. To address concerns over reporting bias, I also present results using gender-specific mortality data by cause. The use of this data and methodology has not been, to the best of my knowledge, previously considered. I find that the HSA also lowered female deaths due to non-natural causes without any concomitant effects on male deaths or female deaths due to natural disasters. In addition, using individual-level data I exploit variation in the timing, state and religion of women affected by the HAS to estimate the causal impact on intimate-partner violence among wives of the head of the household. Using this measure, I find that women affected by the amendments were 17 % less likely to face intimate-partner violence. Together, these results suggest that there was a fall in violence committed against women that is unlikely to be due to a change in willingness to report (e.g. as in Iyer et al. (2012)). To address the findings of Anderson and Genicot (2014), I create an aggregate measure of violence against women that also includes female suicides. However, the results remain unchanged with improved inheritance right reducing all forms of violence committed against women.

To probe mechanisms, I investigate the role of the HSA reforms on marriage negotiations and matching quality, a previously unexplored feature in the marriage market literature (Anukriti and Dasgupta, 2017). In the Indian marriage market parents take great control over the matching process and negotiations over dowry payments (Anukriti and Dasgupta, 2017; Bloch and Rao, 2002). As a result, I posit that the effects found for violence against

[1]In the first part of the paper I use reported gender-based violence as the main dependent variable of interest. This is a measure of police-reported cases under the crime categories considered crimes against women as per the Indian Penal Code. This definition is comparable to the definition in the United Nations Declaration on the Elimination of Violence Against Women of 1993 which defines gender-based violence as all crimes involving: physical, sexual and psychological violence within the family; child sexual abuse; dowry-related violence; marital rape; female genital mutilation; rape and sexual abuse; sexual harassment in the workplace and educational institutions; trafficking in women and forced prostitution.

women are due to alterations occurring at marriage in a first instance and not necessarily due to an increase in women's assets that could lead to lower violence e.g., Hidrobo et al. (2016); Bobonis et al. (2013). Specifically, I examine whether this effect is driven by changes in husbands' behaviour or in marriage negotiations, the results suggest that women benefiting from the amendments were married to husbands who had lower alcohol consumption, a major risk factor for domestic violence (e.g., Angelucci (2008)). Further, I find that parents married their daughters closer to their "own" residence and to men of the same residential area. Together, these findings show that beneficiary women were more likely to marry higher-quality husbands, and had better outside options. I also find suggestive evidence that women's decision-making power improved as a result of the reform.

This paper makes several important contributions. First, I add to the wide literature looking at the effects of the HSA. Deininger et al. (2013) find evidence of an increase of women's likelihood of inheriting land following the introduction of the HSA amendments. Roy (2008) shows that women's exposure to the HSA reforms improves their bargaining power and autonomy. Deininger et al. (2013), Roy (2015) and Bose and Das (2016) indicate that in states where the HSA was amended, female education increased. Rosenblum (2013) finds evidence of small increases in female infant mortality, while Jain (2014) shows that in states affected by a more equal HSA son preference is reduced. I contribute to this literature by showing another positive dimension brought by the HSA, violence against women. This is an important dimension given the negative consequences it has on women and their children Aizer (2011). In addition, I also show that one of the reasons for the positive effects of the reform is due to alterations occurring at marriage. This important given that in India dowry payments are a form of pre-mortem bequest which is expected to change following a more equal gender inheritance legislation. Roy (2015) shows that following the HSA amendments, parents increased dowry payments for older cohorts of daughters while younger cohorts benefited from increased investments in education. This positive results are also in line with the findings of Heath and Tan (2014) who show that improving women's rights increased

female labour participation. Similarly, Calvi (2016) and Calvi et al. (2017) find that female empowerment, granted through the HSA, improved women's health, poverty and mortality.

Second, I make use of three distinct measures of violence against women. This is crucial to disentangle the effects of reporting and incidence of violence as in Iyer et al. (2012) and Iyengar (2009). This paper also adds to the growing literature looking at the economic and institutional causes of domestic violence (Amaral and Bhalotra, 2017; Tur-Prats, 2015; Alesina et al., 2016; Anderberg et al., 2016; Bobonis et al., 2013; Erten and Keskin, 2016) and the role played by economic conditions e.g., spouses' relative income changes, employment or income shocks in determining domestic violence (Anderberg et al., 2016; Bobonis et al., 2013; Farmer and Tiefenthaler, 1997; Aizer, 2010; Eswaran and Malhotra, 2011; Abiona and Koppensteiner, 2016). Chin (2012) also finds that increased female labour participation reduced domestic violence in India. Moreover, the author shows that there is an empowerment effect that dominates a male backlash effect. I contribute to this literature by looking at the role of inheritance, a form of non-labour income. This distinction is relevant because unlike labour-income, non-labour income has been show to improve both spouses' utility as the control over this form of income is shared by the spouses (Anderson and Eswaran, 2009). In terms of household bargaining, this distinction is relevant and predicts that improvements in inheritance rights decrease domestic violence as there is also transfer of resources to the husband (Aizer, 2010).

The findings of this paper are in contrast with those of Anderson and Genicot (2014) who present evidence that the HSA increased male, female and the ratio of male to female suicides. The authors assume argue that the increase in suicides is explained by a rise in intimate-partner violence.[2] The different result in this paper can be explained by the following factors. First, I make use of several measures of violence against women that accurately capture the scope of household conflict and not other forms of violence that could

[2]It is unlikely that the only outside option available to women is to commit suicide. In fact, there is abundant anecdotal evidence that divorce is on the rise in India. Similarly, support services such as shelters are resources available to women.

be reflected in suicides. In fact, as shown in Figure 2, female suicides in India represent only 21% of total violence against women.[3] Therefore, the measures I consider better represent the form of violence being studied, and not necessarily other problems such as the infamous farmer suicides phenomenon in the South of India that have increased male suicides and decreased female suicides (Basu et al., 2016; Hebous and Klonner, 2014).[4] Second, when using individual-level data I restrict the sample to women who are the wives of the head of the household (47% of the main sample of women in the data). This is because the focus of this paper is on domestic violence between spouses, not other forms of inter-household violence that might affect wives, daughters-in-law and mothers in law differently. This is relevant given the fact that women who are not the spouse of the household head are likely to exhibit different household bargaining allocations (Dimri, 2017; Tur-Prats, 2015; Fernandez, 1997; Clark et al., 2010)[5]. Finally, unlike Anderson and Genicot (2014), I control for the role played by time-varying unobservable heterogeneity by including robustness tests with state-linear trends and accounting for how these vary across cohorts and state among Hindus.[6]

The remainder of this paper is organised as follows. Section 2 describes the HSA, the legislative changes introduced with the amendments, and the plausible exogeneity of the Act. Section 3 describes the data used in the analysis. Section 4 presents the empirical strategy, and section 5 presents the results. Section 6 analyses the possible mechanisms by which the change occurs. Section 7 concludes.

[3]Broken down, police-reported crimes are the large bulk of the information on violence against women available (40%). Unnatural deaths due to non-natural incidents represent 39%.

[4]Nonetheless, I repeat the estimations by also taking into account female suicides. I show that the effects of the HSA amendments on all forms of violence against women (i.e., considering police reported violence, deaths and female suicides) remains negative and significant - see Table 12.

[5]For example, Tur-Prats (2015) finds that in Spain, in areas where stem household were a social practise, contemporaneous domestic violence is lower. In contrast, Dimri (2017) uses Indian data and finds that decision-making power of daughter's-in-law, improves after the death of a father-in-law and mother-in-law. Therefore, when considering household conflict it is important to distinguish between the different relationships within the household as the predictions as to what improvements in women's outside options imply to domestic violence can be very different.

[6]Finally, it is worth mentioning that the findings of this paper have also been replicated by Mathur and Slavov (2013) and they also find evidence of a decrease in intimate-partner violence following the HSA. However, unlike in this paper, the authors do not address the channels as to why domestic violence may fall, as for example the marriage market.

2 Background: The Hindu Succession Act

In India inheritance laws are contingent on religion. The HSA governs inheritance laws of *Hindus* i.e, of Buddhists, Hindus, Jains and Sikhs[7]. The religious population of this group was 83.6% of the total population of India in the 2001 Census. This makes the HSA the most important inheritance legislation of India. It lays down succession rules of *Hindu* males and females dying intestate. The Act, passed in 1956, was primarily created to unify inheritance laws that governed *Hindus*. In addition, it aimed to reduce gender inequalities in inheritance and thus, allowed females to have full ownership and testamentary rights over all property. However, it did not address several inequalities in succession law of daughters in relation to sons.

The property of a male dying intestate under the HSA is divided into separate and joint family property (Agarwal, 1995). Separate property refers to all property that was self-acquired throughout the life-time of the individual. Joint family property consists of ancestral property i.e., property inherited from ancestral members of the family. This property, in particular in rural areas, would mostly consist of land which is often family owned.

Under the Act of 1956, daughters of a male dying intestate had rights to an equal share in separate property along with their brothers but not over joint family property. On the one hand, daughters had a right to their share of separate property and to the share of the joint family property. On the other hand, sons had the right to their share of separate property, their share of joint family property and an additional independent share on joint family property by virtue of birth (Agarwal, 1995). Sons would be given this additional share by being part of the *Hindu male coparcener*. The *Hindu male coparcener* consisted of male members of Class I, II and III heirs which were entitled to an individual share in the joint family property (Agarwal, 1995). Consequently, sons would, in addition to the amount of

[7]The act is not applicable in the state of Jammu and Kashmir and in the North-Eastern states of Arunachal Pradesh, Manipur, Meghalaya, Mizoram and Nagaland because these states are mainly ruled by customary laws (Agarwal, 1995). These states are not included in the analysis.

the daughters' share, inherit an individual share of the total inheritance because they were part of the male coparcener.

Besides this source of discrimination, a father could declare his separate property as part of the joint family property to ensure that the daughter does not get any share of this part of the property. Furthermore, a male coparcener could renounce his rights in the coparcener. This would not have any repercussions for his son which would still have his independent share of the ancestral property. However, this would exclude his daughters and Class I female heirs from this share of the property.

Overall, the HSA amendments as of 1956 created these three forms of inequalities between sons and daughters which greatly affected inheritance rights of daughters and ultimately their economic conditions. Agarwal (1995) emphasizes the implications of unequal inheritance laws and states several reports of women who claimed that the lack of land ownership greatly affected their well-being [8].

2.1 The HSA amendments as a natural experiment

In India, state governments have legislative authority and thus, are allowed to make amendments to the HSA. Kerala was the first state to amend the Act in 1976, abolishing the joint family property system among *Hindus*[9]. Andhra Pradesh amended in 1986, Tamil Nadu in 1989 and Maharashtra and Karnataka simultaneously in 1994. These states gave daughters a birth-right in the coparcenary and in this way abolished the inequalities in ancestral property division. However, daughters were only granted an individual share in the joint family property if they were unmarried at the time of the amendments.

[8] One of those descriptions points to the importance of legal rights where a women argues for individual rights over land instead of joint rights (with the husband) by saying: "For retaining the land we would be tied to the man, even if he beat us", [Agarwal (1995)- pp. 20].

[9] The amendments passed in Kerala were in line with the later amendments passed in India. However, it considered that all ancestral property was to be divided by "all surviving matrilineal joint family estates", (Agarwal, 1995)-pp.176.

These amendments where passed with considerably variation in time amongst reform states and in comparison, to the national amendment in 2005. For example, from the first amendment in Kerala to the second in there is a 10 years' gap followed by 3 and 5 years between amendment states. It can be argued that these states are more female-friendly in comparison to the northern and other central states of India and therefore, were the pioneers in these amendments. However, if these states are more female-friendly then it is reasonable for them to have inserted other female-friendly reforms near with this amendment. For instance, the introduction of gender quotas at the lower level of governance was made with considerably variation in comparison to the inheritance amendments [10]. If these states were indeed more progressive on their views of gender, then the timing of other similar female-friendly reforms should have been similar to the HSA amendments. This was not the case for state legislative changes and it was also not the case when considering reform states as a group and in comparison, to other non-reform states.

Consequently, I take the HSA amendments to be an exogenous variation as good as a natural experiment and make use of these state amendments to estimate the causal impact of improved inheritance legal rights on violence against women. This strategy has previously been followed by (Deininger et al., 2013; Roy, 2015; Bose and Das, 2016; Rosenblum, 2013; Jain, 2014; Calvi, 2016; Calvi et al., 2017). Empirically, I conduct several tests to show that this is valid. This natural experiment is a good setting to investigate the effects of gender equal inheritance legislation that is not yet available in many developing countries. About a third of the countries sampled in the Women and Business Law Report in 2012, siblings have unequal inheritance rights over movable and immovable property (Bank, 2012).

[10] Karnataka introduced female quotas in 1987 although it only passed the inheritance reform in 1994. Similarly, Kerala amended the inheritance law in 1976 and only introduced female quotas in 1991. Punjab introduced female quotas in 1991 and did not make amendments to the inheritance reform independently.

3 Data

This paper makes use of three different measures of violence against women. These are police reported crime data; unnatural deaths, and self-reported individual level data. In this section I describe each of these data separately.

Police reported Gender-based violence. The National Crime Record Bureau (NCRB) records reports the number of crimes for several types of violent, property and economic crimes[11]. The NCRB records cognizable crimes i.e., police-reported felonies which are in the first-stage of the criminal justice system and for which the police can take legal actions. In the analysis I use information on first-stage reports (FIR) to the police. This information captures reported crimes though it does not capture the effectiveness of the justice system i.e., a deterrence effect arising from punishment (prosecution, arrest, etc).

I make use of the yearly crime statistics of crimes against women for the period 1975-2004 for the 16 major states of India for which the HSA applies[12]. The dependent variable, total rate of crimes against women, is the state-year sum of the crime-categories considered under crimes against women by the Indian Penal Code. These are cruelty by husband and relatives, dowry deaths, importation of women and girls, kidnapping of women and girls, molestation, rape and sexual harassment. In addition, I also include crimes under Dowry Prohibition Act, Immoral Traffic (Prevention) Act and Indecent Representation of Women (Prevention) Act[13]. Finally, in my results where I take into account female suicides I also use state-year suicides information collected from the NCRB.

[11]Crimes are categorized under the Indian Penal Code (IPC) crimes and under the Special and Local Laws (SLL). The later refers to all punishable offenses regarding the practice of social customs that are reprehensible under special enactments.

[12]The states used in the analysis are Andhra Pradesh, Assam, Bihar, Gujarat, Haryana, Himachal Pradesh, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh and West Bengal. In 2001 the states of Chhattisgarh, Jharkhand and Uttarakhand were carved out of the states of Madhya Pradesh, Bihar and Uttar Pradesh respectively. To make the results comparable across time the data for these states was merged with the original states.

[13]By 2004, 44.5% of crimes against women were in the categories of domestic violence and dowry in particular, the former represented 38% of total incidents against women.

Mortality by gender and cause. To disentangle a reporting from an incidence effect, I investigate the effects of the HSA amendments on outcomes that are less likely to be subject to a reporting bias. Since in India, unlike in the U.S. [14], there is no information on mortality by gender for the period in consideration, I resort to the use of information on accidental deaths.

Accidental deaths or unnatural deaths are deaths that are attributed to either natural or unnatural causes[15]. These are available from the NCRB yearly publications for the state-year sample period of interest. I collected this information and calculate the rate of deaths by gender and cause (i.e. natural or unnatural causes) per 1000 female/male population.

The use of this variable is to test the effects of the HSA on outcomes for which reporting bias is lower (Sekhri and Storeygard, 2013). In addition, I also use the information on unnatural deaths due to natural events and male mortality as a placebo to test to verify the validity of the results. Reporting bias is a common problem in the empirical crime literature as this can lead to measurement error. This is a potential cause for concern if reporting behaviour is a function of social norms, gender equality and institutional quality. Nonetheless, the literature has identified that in India, reporting issues arise in non-lethal crimes as it is difficult to held a death unaccountable (Drèze and Khera, 2000). Hence, crimes such as dowry related deaths and accidental deaths would be less likely to suffer from misreporting.

Other state-level data. . In the empirical analysis I also control for several state-year socio-demographic, economic, and law and order factors. I include literacy rate, sex ratio and proportion of the population that is rural. This information was collected from the Census of 1971-2001 and interpolated for the intervening years. I also include data on per capita net

[14] As for example the measures of intimate—partner violence used in (**?**Aizer, 2010).

[15] Accidental deaths attributable to natural events are due to lightning, heat strokes, floods, earthquake, landslides, cold exposure, cyclone, avalanche, starvation, epidemic, torrential rains and others. Accidental deaths attributable to non-natural events are sub-categorized into deaths due to air-crashes, collapse of structures, drowning, explosion, falls, fire, sudden deaths, poisoning, traffic and miscellaneous.

state domestic product collected from various publications from the Reserve Bank of India. Law and order effectiveness is controlled by including strength of police force measured by the number of total police force (both civil and armed) per 100,000 inhabitants, collected from the yearly publications of the NCRB. Further, in India, law and order decision-making are decentralized from the central government. Consequently, determination of the level of policing and the level of expenditures in law and order are subject to some level of discretion and vary across the states and according to the preferences of the policy-makers. Moreover, Iyer et al. (2012), Clots-Figueras (2011) and Brule (2012) show that political factors are an important determinant in influencing reporting behaviour, legislation changes and on legislation implementation. Thus, to control for the effect that political identity might have on criminal behaviour and reporting behaviour, I include a dummy variable for female Chief Ministers in office and I also control for the proportion of seats held by women in the Legislative Assembly to account for the fact that women from lower castes are more likely to pass laws that increase gender legal balance (Clots-Figueras, 2011). This information is collected from several Statistical Reports of Legislative Assemblies Elections.

Table 1 presents the descriptive statistics for the variables used in the police-reported analysis. The mean crime rate against women is 1.6 crimes per 10,000 women; reform states have higher incidence of crimes against women (Figure 1). On average, reform states have higher income per capita, higher literacy rates and lower rural population but, they have a smaller police force, lower robbery rates and higher riot rates when compared to non-reform states. Before the HSA amendments, reform states were different in observables; HSA states had higher rural population, police strength, income per capita and female political representation. Gender-based violence was not different between HSA and non-HSA amendment states prior to the legal changes. Accidental deaths for both genders due to non-natural events were lower in HSA amendment states. The empirical analysis controls for these observable differences.

Individual-level self-reported intimate-partner violence. In order to ensure that the

results are not driven by the level of aggregation of the data or measurement concerns, I also use data on self-reported intimate-partner violence. In this case, the dependent variable used is the life-time incidence of violence collected from the National Family Health Survey (NFHS) rounds 2 and 3 of (1998-99 and 2005-06). These are the only surveys available for India which contain information on domestic violence. The NFHS is a comprehensive survey which comprises detailed health and socio-economic information of women. The surveys have different sets of questions regarding domestic violence. The NFHS-2, asked "Since you completed 15 years of age, have you been beaten or mistreated physically by any person?" and "Who has beaten you or mistreated you physically?". I identify through these questions the incidence of IPV for women interviewed in the NFHS-2. The NFHS-3 asked "(Does/did) your (last) husband ever do any of the following things to you?" listing nine different types of physical and sexual violence[16]. The binary variable was obtained by attributing the value 1 if a woman responds affirmatively to any of these types of violence. Then, I combined this information with the information provided by the NFHS-2 to obtain a similar measure of domestic violence across surveys. These questions are in line with the practice advised by international organizations (WHO, 2013) and allow to obtain a reliable measure of IPV.

However, this variable is also a reported measure, and can be a potential issue to the identification strategy due to measurement error if the misreporting is correlated with the treatment definition. The lack of an ideal measure of violence against women is a common problem in the crime literature as the true incidence is not observable. Regardless of whether the main variable is a police-reported, self-reported or even hospitalization measures would be subject to some form of measurement error (Aizer, 2010). For this reason, in addition to using three different measures of violence against women, I also employ a wide range of sample selection criteria and robustness tests to reduce any potential source of bias deriving

[16]These are: slap; twist arm or pull hair, push, shake, or throw something at the respondent; punch with his fist or with something that could hurt the respondent; kick, drag or beat the respondent up; try to choke or burn on purpose; threaten or attack with a knife, gun, or any other weapon; physically force the respondent to have sexual intercourse with him even when the respondent did not want to and force to perform any sexual acts the respondent did not want to.

from misreporting. This is the main difference with respect to the results presented in Anderson and Genicot (2014).

First, in order to limit the confounding factors related to the nature of the IPV measure used, I restrict the sample to women who were surveyed for the domestic violence questionnaires in the NFHS and also who are currently married, and were selected and interviewed without interruptions from any adult member of the household for the domestic violence section of the questionnaires. In addition, I keep only women who are wives of the head of the household to reduce intra-household dynamics that could affect the dependent variable through this link. Further, the sample only contains individual level information on women from which there are no missing variables from all the explanatory variables used in the estimations. Women married after 2005 are excluded from the analysis as the HSA was amended at the national level in 2005. Finally, as in the previous section, the sample includes women from the 16 major states of India[17]. The sample after these restrictions contains 40,884 women born between 1949-1991 and married between 1962- 2004. I estimate the main model using a linear probability model[18].

Second, in my specifications I also include a vector of women, couple and household control variables which could otherwise capture some of the effects of the amendments and influence the incidence of domestic violence. These control variables include age and educational gap to capture the relative bargaining power of spouses. Further, I include household size as exposure towards violence may also depend on whether other family members tolerate or encourage this practice, a rural household dummy and son preference as a measure of perceptions of gender-roles and social norms of the wife. In addition, I add a dummy for whether the household has a television to control for exposure to media devices as this may alter the behaviour of criminals, women's attitudes towards domestic violence, female autonomy and fertility choices (Jensen and Oster, 2009; Card and Dahl, 2011; Ferrara et al.,

[17]This sample represents approximately 82% of the wives sampled by the pooled surveys.
[18]Estimates with a logit model are consistent with the main findings.

2012). Table 7 presents descriptive statistics. Women in states where the HSA was amended are more likely to live in urban areas, have lower son preference, age and education gap and are more likely to be exposed to television content. In the sample, 45% of women have been subject to some form of domestic violence, and this is lower in HSA states when compared to non-HSA states with means of 39% and 49%, respectively.

4 Identification strategy

Empirical Problem. In estimating the effect of improved access to inheritance on violence against women it is important to ascertain two things: i) establish causality and ii) distinguish between reported violence and incidence. Regarding the first, the use of a double differences specifications allows to disentangle a potential correlation between a gender-progressive legislation and violence against women. This is relevant as it could be that the two factors in analysis are affected by unobservables that mask the relationship and hence, provide a misleading estimate of the effect of improved inheritance rights and crime.

Second, observed information on violence depends on both incidence and the probability of reporting a crime or social desirability bias in responses to enumerators. Thus, the effect of the amendments could influence both factors. However, if reporting behaviour differs across states and in time and between reform and non-reform states, this could contaminate the results. I address this issue by performing several robustness tests on low reporting bias crimes, other types of crimes which can be taught as control categories. Altogether, the results disentangle a reporting effect from an incidence effect.

Empirical Strategy. As explained before, this paper considers three different measures of violence against women. Since these measures are observed at the state and separately at the individual level I present two different reduced-form equations using similar double differences empirical strategies. I describe each of these separately below.

State- level data. I use the staggered implementation of the HSA amendments to analyse the effect of inheritance rights on violence against women using a difference-in-differences estimator. The main assumption required to analyse the effect of the state amendments on violence against women is that in the absence of the legal amendments, the change in reported violence against women in reforming states would have been the same as in non-reforming states only differing by a linear trend. I employ a double difference estimation strategy by estimating the following equation:

$$y_{st} = \lambda_t + \alpha_s + \delta_1 \text{HSA}_{st} + \beta' X_{st} + \epsilon_{st} \tag{1}$$

where y_{st} is the natural logarithm of total reported violence against women per 1000 female population occurring in state s in year t. The estimation includes year-fixed effects to account for common country-wise year changes, λ_t. In addition, it also controls for state-specific unobserved time-invariant characteristics that could affect the incidence of violence by including state-fixed effects, α_s. HSA_{st} is a dummy variable that is equal to one if the state s at time t amended the HSA and takes the value zero otherwise. Thus, δ_1 is the coefficient of interest which captures the effect of the amendments on reported violence against women. I also include a vector of several time and state-specific time-varying socio-economic and demographic variables. To mitigate potential serial correlation problems in the error term and heteroskedasticity problems, I use two different methods to obtain standard-errors. First, I use robust standard errors clustered at the state-level (Bertrand et al., 2004) and second, I provide p-values of estimates obtained using wild bootstrap standard-errors that account for the reduced number of clusters (Cameron et al., 2008).

The common time-effects assumption underlying a difference-in-difference estimation holds if any difference that is to exist between reform and non-reform states is captured by λ_t and α_s. This assumption is advantageous as it controls for unobserved time-invariant state-level heterogeneity but, it does not control for time-varying unobserved state-level het-

erogeneity. This is important as it could be that states that had lower rates of violence against women in the first place, could have also been those who first passed the law. This could invalidate the required assumption under the difference-in-differences. Nonetheless, even though the assumption is not directly testable I include several robustness checks and ensure that our results are not driven by unaccounted time-varying unobserved state-level heterogeneity. Namely, I include state-specific linear trends and perform several placebo tests to understand if there are any confounding effects that would exhibit time-varying unobserved state-level heterogeneity if it exists.

Individual level data. The empirical strategy used compares the probability of IPV of women who were unmarried relative to the reform years in reform states with those who were already married relative to the reform year in reform states in relation to women in non-reform states. Women who were not yet married in reform states would be those who would benefit from the HSA amendments. Women in reform states who were already married were legally the same as women in non-reform states. Hence, the effect of the reform on incidence of domestic violence for this group of women should not differ from those in non-reform states. I estimate the following equation:

$$y_{isk} = \alpha_s + \lambda_k + \delta_2 HSA_{is} + \beta' X_i + \zeta_{isk} \tag{2}$$

where y_{isk} is the incidence of domestic violence for women i in state s born in year k. The dependent variable is a dummy that reflects the incidence of violence committed by the husband throughout the length of the marriage until the survey year. I include state-fixed effects for similar reasons to those in section 4. I also include a vector of control variables X_i, birth-year fixed effects to control for cohort country-wise specific changes. The term HSA_{is} is a dummy variable that takes the value 1 if a woman living in a reform state s, married in a year after the reform, relative to the reform year in her state. This group of women had the full benefits of the reform and therefore, constitutes the treatment group in the reform

states. Thus, δ_2 is the coefficient of interest. The identification strategy assumption is that women in reform states who married after the amendments would not differ from women in non-reform states in linear trends, in the absence of the amendments[19].

Identification Concerns. There are two issues that may call into question the validity of (1) and (2). The first pertains to the presence of female members of State Assemblies and second, the introduction of gender political quotas in villages in India. Clots-Figueras (2011), shows that having lower caste women in State Assemblies increases non-development expenditure. In addition, states with more low caste female representatives at the state-level are more likely to pass women-friendly laws such as the inheritance law. Thus, it could be that lower caste women in reform states are more effective at influencing policy decisions that affect domestic violence through for example, increases in expenditure in policing that could have a deterrence effect on husbands. Yet, it is not clear whether the increase in non-development expenditures would directly affect violence against women as opposed to an increase in any other form of non-development expenditure that is not related to violence against women. In any case, this remains a potential problem for the estimations. I include in the robustness tests of the estimates state-specific birth cohort linear trends in order to mitigate omitted variable bias problems. Finally, as per the previous section I separately estimate all results for the full sample of states without Kerala and simultaneously without Karnataka and Maharashtra.

Another issue to address is the effect of female political representation in villages. Iyer et al. (2012) explore the effect of the *Gram Panchayatt* reform that introduced female quotas in rural village councils on crimes against women. Hence, one might be concerned that the plausible exogenous reform of the HSA may in fact be endogenous or correlated with female-

[19]This specification is considerably different from the approach taken in Anderson and Genicot (2014) and follows the literature on difference-in-differences with a state-time variation in the policy implementation, by accounting for unobserved confounding factors with the inclusion of time-varying fixed-effects and state-specific time-factor trends (Wolfers, 2006). The findings presented in this paper are also corroborated in the articles I became aware since writing the latest version of this paper (Heath and Tan, 2014) and (Mathur and Slavov, 2013).

friendly reforms as the 73^{rd} amendment for political representation of women. However, out of the five states that amended the inheritance law only the states of Karnataka and Maharashtra did so after the introduction of female political quotas at the lower level of governance, i.e. 7 and 2 years before, respectively. As a result, for these 2 states the amendments could have been endogenous to female representation. In order to circumvent this problem I estimate equation 5.1. with and without these states. The results do not change.

Finally, state policies affecting law and order that are correlated with the passage of the reform would invalidate the estimation strategy by omitted variable bias. Even though I do not find evidence of this mechanism or of these policies, I cannot discard this. However, the inclusion of state-specific linear trends would mitigate the issues raised above[20].

5 Results

Police reported Gender-based violence. Figure 3 presents the unconditional estimates of the effect of the yearly effect of the legislative amendments on gender-based violence. Prior to the amendments, the coefficient is stable around zero showing that there was no previous correlation between gender-based violence in states that later amended the HSA. After the HSA amendments, the estimates fall and stabilise after 5 years of the amendment.

In Table 2 I report the coefficient of interest, δ_1, in (1) which captures the effect of the amendments on reported total violence committed against women. Across all specifications, the effect is negative. Column (1) presents the unconditional estimations which points to a significant negative effect of the amendments on total crimes against women. The coefficient of about -1.3 which suggests that total reported crimes decrease by approximately

[20]One other potential problem could be post-marriage migration. However, as mentioned in Roy (2015) and in Drèze and Khera, (2000), post-marriage migration is within-state migration and hence, I discard this possibility as a concern for the identification strategy.

73%. When including socio-economic time-varying covariates in column (2), the magnitude increases slightly effect. Adding the different control variables (i.e., police force and literacy rates) does not change the direction of the effect. When using the full set of control variables, the coefficient is about -1.5, which amounts to a decrease of 77% in total crimes committed against women in reform states.

However, the difference-in-differences estimator does not control for time-varying unobserved state-heterogeneity. Thus, the effect presented in columns 1-6 could be due to unobserved differences across states that vary across time. One possible way to mitigate this problem is to allow for state-specific time trends. When accounting for state-linear trends in columns 7 and 8, the coefficient of the post-HSA amendments is much smaller in magnitude then in previous estimations. The coefficient is of about -0.44 and would amount to a decrease of about 36%. The magnitude of the effect is reduced but the significance remains at 10% significance level. Due to the low number of clusters in the sample (i.e. 16 states) I also include p-values obtained using wild-bootstrap standard-errors clustered as advised in Cameron et al. (2008). The significance of the results obtained is consistent with those presented in the main table of results. This is particularly valid for the preferred specification, column 8, which accounts for state-varying controls, state unobserved fixed effects, year dummies and cluster-level trends.

There are two issues that need addressing on what regards the validity of the identification. First, I cannot identify violence reported by religious groups. As mentioned in section 3 the reform would have benefits for the *Hindu* women but not others. Thus, even though *Hindu* religious population is 83.6% per the Census 2001, I cannot rule out the identification problems that arise from this. Second, the existence of the marital status requirement in the state-level amendments is not observable in this data. Yet, it is plausible to assume that reporting is a function of individual and couple characteristics which are not observed in this data. Age, experience in marriage etc, are likely to influence a woman's decision to report a crime to the police. Thus, I assess these problems with the use of individual-level data.

Female and Male Unnatural Deaths. To ensure that the estimated effects are not driven by changes in reporting behaviour or misreporting I analyse the effect of the amendments on gender specific crimes that are less likely to be subject to misreporting. I estimate the effect of the amendments on female and male accidental deaths. If accidental deaths are random events (i.e., not subject to changes in inheritance rights or changes in reporting behaviour) then the effect of the amendments should be negligible. Thus, I expect that there is no effect of the HSA on accidental deaths due to random events. However, accidental deaths can disguise violent crimes against women[21]. If so these should decrease as well if the effect is indeed due to a change in incidence. If the effect of the HSA amendments on accidental deaths is positive then this could be reflecting a reporting effect that could reflect loss of welfare of women. Moreover, accidental deaths are divided into deaths due to natural causes and other non-natural causes. I expect that both female and male accidental deaths due to natural causes not be affected by the amendments as these are random. However, if there is a change in reporting behaviour this could have changed the reporting of accidental deaths due to other causes. I test this hypothesis using the NCRB yearly publications on accidental deaths of females and males. I use this information for the same state-year sample as in the previous section.

Figures 4 and 5 present the unconditional estimations on both female and male accidental deaths for the dependent variables considered. The trend in natural accidental deaths is not correlated with the pre-post HSA for both genders. For females, accidental deaths post-HSA decrease and the variation in total accidental deaths is completely explained by the variation in accidental deaths due to non-natural events.

Table 4 and 5 present the results of conditional estimations. Table 4 presents the results for female accidental death rates and Table 5 for male accidental deaths. Results suggest that the amendments decreased total accidental death rates of females and this result is

[21] Unnatural deaths of women are often used strategically by in-laws to destroy evidence and hide potential effects from torture or violence used against women (Singh et al., 2003).

entirely driven by a decrease in accidental deaths due to non-natural events. These results point to the suggested hypothesis that women's welfare in HSA amendment states increased. The results for accidental deaths of males and do not show conclusive evidence that the amendments had an effect on males. The negative effect of the amendments on accidental deaths of males is not consistent (Table 5). As expected, I do not find any effect on random accidental deaths for both females and males.

Individual Level self-reported intimate-partner violence. The impact of the legal amendments is shown in Tables 8 and 9. In Table 8 I present the results for the full sample of women and in Panel A I include all states of the sample. The estimated effect points to a decrease in the probability of facing domestic violence in the marriage by 6.7 percentage points (column 1). This amounts to a decrease in the probability of facing domestic violence by 17% for beneficiary women in comparison to non-beneficiaries. Further across specifications (columns 2 and 3 of Panel A) the estimated coefficient remains negative and significantly different from zero at the 1% level. The inclusion of state-cohort linear trends increases magnitude of the effect to about 9.9 percentage points which remains about the same after including state-*Hindu* specific effects and *Hindu*-specific linear trends (column 3). Moreover, the exclusion of Kerala (Panel B) and the exclusion of Karnataka and Maharashtra (Panel C) do not affect the direction of the effect and the magnitude remains stable.

In Table 9, I repeat the estimations restricting the sample to *Hindu* women (columns 1-7) and Muslim (columns 8-10). The majority of the population in India and within the states is *Hindu* and the reform should only have an effect for this religious group. Hence, I expect the effect on *Hindu* women to be similar to the estimated effect in Table 8. Women affected by the HSA are 6.5 percentage points less likely to be a victim of domestic violence. This effect is also significant and similar to the coefficient found in Table 8. Additional robustness tests, columns 4-5 and 6-7 for the exclusion of Kerala and Karnataka and Maharashtra show that the estimates remain negative and significant.

Further, I test for the effect on non-beneficiary women (i.e. Muslim women) and expect to find no effects due to the HSA amendments as these rules out the hypothesis that HSA states have time-variant unobservable factors that differently affect women in these states. The failure to reject this hypothesis may cause a concern on the validity of the identification strategy. Columns 8-9 of Table 9 show that there is no differential effect among Muslim women in HSA states in comparison to non-HSA Muslim women. The effect is negative across all specifications and it is not significant. This effect rules out a contagion effect across religions and reassures the validity of the difference-in-differences strategy.

Women who married after the amendments have lower probability of facing marital violence. These results are consistent with the findings of Panda and Agarwal (2005) and thus emphasise the role of property ownership which in this case is likely to take the form of inheritance on women's welfare. However, this result can be due to an empowerment effect or instead, a result of a marriage market effect which may not necessarily be in favour of women. In the following section, I identify the possible mechanisms by considering first the effect on the husband's behaviour and second on the marriage market.

Addressing Anderson and Genicot (2014). In Table 12, I present results of (1) where the main outcome variable is the sum of all forms of violence against women per capita. This includes the two measures used in this paper and the female suicides information used by Anderson and Genicot (2014). The idea is to understand the difference in results between this paper and Anderson and Genicot (2014). Across specifications, I find that in states amending the HSA violence against women decreased. While the magnitude of this aggregate measure decreased, the direction of the effect is negative. This is consistent with the fact that suicides are only about 20% of total violence against women and with the fact that the positive effects found in Anderson and Genicot (2014) are smaller in magnitude when compared to those of the remaining forms of violence.

5.1 Robustness

In order to circumvent some of the problems raised in section 4 I conduct a series of robustness tests. First, I repeat the estimation of (1.) as in columns 6 and 7 of Table 2 excluding the state of Kerala. This state is peculiar in respect to several development outcomes. Also, this state was the first to introduce the reforms in 1976 which implies that there is a limited number of pre-reform observations for this state. Excluding Kerala from the sample does not change the results (columns 9 and 10 in Table 2). Second, in the states of Karnataka and Maharashtra the timing of the legislative amendment could be endogenous. I repeat the estimations removing these two states (columns 11 and 12- Table 2) and the coefficients of the effect of the amendments remain negative and significant after excluding these states from the analysis. The effect of the amendments for the states of Andhra Pradesh, Kerala and Tamil Nadu point to a decrease of about 59% in violence against women.

In addition, I estimate (1) using as main variables of interest pre-reform years in reform states as well as a post-reform effect. Results are presented in Table 3. In columns 1-3 the effect on 3 and 4 years pre-reform is not significant. In addition, for the preferred estimation in column 3 the pre-reform coefficients are also smaller in magnitude and significant. Finally, I also test the effect on crimes less likely to be affected by the HSA i.e., non-gender specific crimes. In Table 6 I report the results for robbery and riots per 100,000 total population. The amendments do not have any effect on these crime categories.

Taken together, the legal amendments influenced reported crimes committed against women without any concomitant effect on crimes that would not have been directly affected by the Act. In addition, deaths that are less likely to be affected by reporting behaviour also decreased. I take this as suggestive evidence that there was a decrease in incidence of violence against women rather than a decrease in reporting behaviour.

6 Channels

6.1 The effect on marriage negotiations

I consider the effect on the marriage market to assess whether parents' marital decisions regarding their daughters also changed because of the HSA amendments. I look at this mechanism by analysing the effect of the amendments on marriage negotiations. My hypothesis is that a reduction in domestic violence is a consequence of better marriage market negotiations that consequently, improved women's outside options.

I use the Indian Human Development Survey (IHDS) of 2004, a representative survey of women at the state-level that contains several indicators of marriage practices in India. I estimate a similar equation to (2), on several outcomes related to marriage negotiations. I analyse the effect of a woman being married post-HSA changes on i) proximity of current residence to natal village/town; ii) husband being from the same natal village/town and iii) involvement of brides in the marriage negotiation. The first two measures are capturing the changes in marriage negotiations directly. There are several ways through which these could have changed following the HSA. First, I expect that if parents improved the marriage allocations of the daughter (as per the previous result), they would also be more likely to marry their daughter to a husband whom they were previously acquainted with. Second, if parents bequeathed their daughters with a share in ancestral property (e.g., immovable property as land) it is also more likely that they marry the daughters closer to where they can still exert control, monitor or share the property. Finally, I also investigate the effect on women's agency by looking at whether women have any say in the choice of husband.

Table 11 presents the results. Women married after the HSA amendments in reform states are less distant from the natal village by about 0.3-0.6 hours i.e., 18-36 minutes. Further, women have a higher probability of marrying someone from their natal residence by about 5 percentage points. These results point to an improvement in women's outside

option, (or even security if parents can protect their daughters from domestic violence). Roy (2015) shows that older cohorts who were beneficiary of the HSA changes had higher dowries which were intentionally to compensate daughters for disinheriting them. The findings this study are in line with the authors' conclusions and with previous research that show that dowry payments under certain conditions are not necessarily harmful to women (Suran et al., 2004; Ambrus et al., 2010).

Finally, I do not find an effect on women being more involved in marriage negotiations. Taken together the effects on decision-making in Tables 10 and 11 show weak results on women's empowerment, measured by having any say in decisions that affect women's utility.

6.2 The effect on the husband's behaviour

Women affected by the HSA amendments increased their premium in the marriage market and therefore were more likely to marry a higher quality groom. I examine the previous section mechanism by studying the effect of the HSA legislative changes on men who married eligible women. To measure husband's quality, I take a measure of behaviour which has been identified as a major risk factor of IPV, alcohol consumption. Angelucci (2008) shows that an increase in income transfers to women decreases husband's alcohol consumption and associated aggressive behaviour. Further, the literature has found a positive relationship between alcohol consumption and aggressive behaviour (Mehlum et al., 2006; Corman and Mocan, 2013). In particular, Wang (2013) finds that transferring property rights to women decreased household consumption of alcohol. I use this measure of husbands' behaviour to analyse a plausible mechanism through which domestic violence decreased post-HSA amendments. I expect that if domestic violence decreased through a change in husbands' behaviour, then alcohol consumption of a husband of a woman married post-HSA amendments should decrease.

I use the men's questionnaire of the NFHS-III[22] to collect information on husbands. I then combine men's survey with the women's data, considered in the previous section. The sample consists of information on 18,577 *Hindu* couples. The main dependent variable is a binary variable for alcohol consumption. In addition, I also analyse the effect on the sample of drinkers using a binary variable for the frequency of alcohol consumption that takes the value 1 if the self-reported consumption is "daily" and 0 if "weekly or less often". I use a similar empirical specification to the one employed in the previous section to model alcohol consumption as a function of the post-HSA effect in reform states in comparison to non-reform states. As before, all estimations include state-fixed effects, husband's birth-year fixed effects and other individual, marriage and household specific control variables. Results are presented in Table 10.

The probability of being an alcohol consumer among men who married eligible women after the HSA legislative changes decreases by about 4.3 percentage points. This effect is significant and the inclusion of state-specific cohort trends does not affect the magnitude of the effect and the coefficient remains significant at a conventional level. Moreover, among consumers, the frequency of consumption decreases by about 3 percentage points. This result is consistent with the initial hypothesis that women married higher quality husbands who have lower probability of engaging in addictive behaviours that are highly correlated with the use of violence.

In addition, I inspect the effect on decision-making. I test for the effect of being an HSA beneficiary on women having any say in household financial decisions i.e., either jointly with the husband or solely on their own. I find suggestive evidence of an increase in decision-making by about 2 percentage points. This result is no longer significant when I include state-cohort linear trends but, the coefficient is stable.

[22]Men's questionnaires were not conducted in previous survey rounds.

7 Conclusion

This paper shows empirically that female inheritance legislation has important implications for women's well-being. It establishes the relation between inheritance rights of women and violence against them. The results contribute to a growing literature on the effects of women's economic conditions on violence against them and on the importance of policies targeted specifically to women.

Using the staggered implementation of legal amendments in the major inheritance law in India, the Hindu Succession Act, this paper analyses the effect of improved legislation that improved property rights of women on violence against them, using both police-reported aggregated violence and self-reported intimate partner violence. I show that the amendments reduced gender-based violence. In addition, I do not find a causal effect on non-gender based police-reported crimes and on crimes less subject to reporting behaviour changes. Thus, the Act triggered a decrease in crimes committed against women rather than a change in reporting behaviour.

In order to overcome some of the identification issues that arise when looking at reported violence, I estimate the effect of self-reported incidence of intimate partner violence. I find that domestic violence decreased for women who would be beneficiaries of the amendments without any effect found for women who would not be legally eligible. Finally, I estimate the effect on husband's behaviour that is closely related to the aggressive behaviour and on marriage practices. I find that violence decreased through a reduction in the probability of marrying an alcohol consumer and on the frequency of alcohol consumption. Further, post-marriage residence of beneficiary women is closer to the natal family and the husbands are more likely to be from the pre-marriage residential area. This provides evidence that marital outcomes of women improved by both marrying better husbands and improved outside options. I do not find evidence that eligible women had higher decision-making power over financial issues within the household or that they were more involved in marriage ne-

gotiations.

In terms of policy analysis, these results highlight the importance of legal conditions in economic development and on the effects of well-defined property rights from a welfare perspective. The 2005 amendments of the HSA Act in India, guarantee that daughters in all other states governed by *Hindu* law are now legally entitled to inheritance rights at par with sons regardless of their marital status. The implications of this are yet to be fully understood, this paper suggests though that this might improve women's security. Future research should aim to analyse the overall effect of the amendments addressing whether the reforms are Pareto improving or to the contrary they may benefit some women, but may also trigger negative externalities within families or across religions by lowering welfare in groups that did not benefit from the reform.

Compliance with Ethical Standards: Funding: This study was funded by the Fundação para a Ciência e Tecnologia (grant reference: SFRH/ BD/78034/2011) and the Economic and Social Research Council (ESRC) through the Research Centre on Micro-Social Change (MiSoC) at the University of Essex, grant number ES/L009153/1. The usual disclaimer applies. The author declares that there is no conflict of interest.

References

Abiona, O., Koppensteiner, M. F., 2016. The impact of household shocks on domestic violence: Evidence from Tanzania. University of Leicester Working Paper No.16/14.

Agarwal, B., 1995. A field of one's own. Cambridge Books.

Aizer, A., 2010. The gender wage gap and domestic violence. American Economic Review 100 (4), 1847–59.

Aizer, A., 2011. Poverty, violence, and health: The impact of domestic violence during pregnancy on newborn health. Journal of Human Resources 46 (3).

Aizer, A., Dal Bó, P., 2009. Love, hate and murder: Commitment devices in violent relationships. Journal of public Economics 93 (3), 412–428.

Alesina, A., Brioschi, B., Ferrara, E. L., 2016. Violence against women: A cross-cultural analysis for africa. NBER Working Paper w21901.

Amaral, S., Bhalotra, S., 2017. Sex ratio and violence against women: The long-run effects of sex-selection. mimeo.

Ambrus, A., Field, E., Torero, M., 2010. Muslim family law, prenuptial agreements, and the emergence of dowry in bangladesh. The Quarterly Journal of Economics 125 (3), 1349–1397.

Anderberg, D., Rainer, H., Wadsworth, J., Wilson, T., 2016. Unemployment and domestic violence: theory and evidence. The Economic Journal 126 (597), 1947–1979.

Anderson, S., Eswaran, M., 2009. What determines female autonomy? evidence from bangladesh. Journal of Development Economics 90 (2), 179–191.

Anderson, S., Genicot, G., 2014. Suicide and property rights in india. Tech. rep., National Bureau of Economic Research.

Angelucci, M., 2008. Love on the rocks: Alcohol abuse and domestic violence in rural mexico. The B.E. Journal of Economic Analaysis and Policy 8 (1).

Anukriti, S., Dasgupta, S., 2017. Marriage markets in developing countries. forthcoming in Oxford Handbook of Women and the Economy.

Banerjee, A. V., Gertler, P. J., Ghatak, M., 2002. Empowerment and efficiency: tenancy reform in west bengal. Journal of political economy 110 (2), 239–280.

Bank, W., 2012. Women, business and the law. Tech. rep.

Basu, D., Das, D., Misra, K., 2016. Farmer suicides in india. Economic & Political Weekly 51 (21), 61.

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in-differences estimates? The Quarterly Journal of Economics 119 (1), 249–275.

Bloch, F., Rao, V., 2002. Terror as a bargaining instrument: A case study of dowry violence in rural india. The American Economic Review 92 (4), pp. 1029–1043.

Bobonis, G. J., Gonzalez-Brenes, M., Castro, R., 2013. Public transfers and domestic violence: The roles of private information and spousal control. American Economic Journal: Economic Policy 5 (1), 179–205.

Bose, N., Das, S., 2016. Women's inheritance rights, household allocation and gender bias.

Brule, R., 2012. Gender equity and inheritance reform: evidence from rural india. mimeo.

Calvi, R., 2016. Why are older women missing in india? the age profile of bargaining power and poverty. Unpublished Manuscript 45, 58–67.

Calvi, R., Lewbel, A., Tommasi, D., 2017. Late with mismeasured or misspecified treatment: An application to women's empowerment in india.

Cameron, A. C., Gelbach, J. B., Miller, D. L., August 2008. Bootstrap-Based Improvements for Inference with Clustered Errors. The Review of Economics and Statistics 90 (3), 414–427.

Card, D., Dahl, G. B., 2011. Family violence and football: The effect of unexpected emotional cues on violent behavior*. The Quarterly Journal of Economics.

Chin, Y.-M., 2012. Male backlash, bargaining, or exposure reduction?: women's working status and physical spousal violence in india. Journal of Population Economics 25 (1), 175–200.

Clark, C. J., Silverman, J. G., Shahrouri, M., Everson-Rose, S., Groce, N., 2010. The role of the extended family in women's risk of intimate partner violence in jordan. Social Science & Medicine 70 (1), 144–151.

Clots-Figueras, I., 2011. Women in politics: Evidence from the indian states. Journal of Public Economics 95 (7-8), 664–690.

Corman, H., Mocan, N. H., 2013. Alcohol consumption, deterrence and crime in new york city. Tech. rep., National Bureau of Economic Research.

Deininger, K., Goyal, A., Nagarajan, H., 2013. Women's inheritance rights and intergenerational transmission of resources in india. Journal of Human Resources 48 (1), 114–141.

Dimri, A., 2017. Patrilocality in india: Beyond the mother-in-law, daughter-in-law drama? mimeo.

Doepke, M., Tertilt, M., 2014. Does female empowerment promote economic development? Tech. rep., National Bureau of Economic Research.

Doepke, M., Tertilt, M., Voena, A., 2011. The economics and politics of womens rights. NBER Working Papers 11-3, National Bureau of Economic Research.

Duflo, E., 2011. Women's empowerment and economic development.

Erten, B., Keskin, P., 2016. For better or for worse?: Education and the prevalence of domestic violence in turkey. American Economic Journal: Applied Economics.

Eswaran, M., Malhotra, N., 2011. Domestic violence and women's autonomy in developing countries: theory and evidence. Canadian Journal of Economics 44 (4), 1222–1263.

Farmer, A., Tiefenthaler, J., 1997. An economic analysis of domestic violence. Review of Social Economy 55 (3), 337–358.

Fernandez, M., 1997. Domestic violence by extended family members in india: Interplay of gender and generation. Journal of Interpersonal Violence 12 (3), 433–455.

Ferrara, L., Chong, Duryea, 2012. Soap operas and fertility: Evidence from brazil. American Economic Journal: Applied Economics 4(4), 1–31.

Harari, M., 2014. Women's inheritance rights and bargaining power: Evidence from kenya. Department of Economics of MIT, 1–47.

Heath, R., Tan, X., 2014. Intrahousehold bargaining, female autonomy, and labor supply: Theory and evidence from india. mimeo.

Hebous, S., Klonner, S., 2014. Economic distress and farmer suicides in india: an econometric investigation. Tech. rep., Discussion Paper Series, University of Heidelberg, Department of Economics.

Hidrobo, M., Fernald, L., 2013. Cash transfers and domestic violence. Journal of Health Economics 32 (1), 304–319.

Hidrobo, M., Peterman, A., Heise, L., 2016. The effect of cash, vouchers, and food transfers on intimate partner violence: evidence from a randomized experiment in northern ecuador. American Economic Journal: Applied Economics 8 (3), 284–303.

Iyengar, R., 2009. Does the certainty of arrest reduce domestic violence? evidence from mandatory and recommended arrest laws. Journal of Public Economics 93 (1-2), 85–98.

Iyer, L., Mani, A., Mishra, P., Topalova, P., 2012. The power of political voice: Women's political representation and crime in india. American Economic Journal: Applied Economics 4 (4), 165–93.

Jain, T., 2014. Where there is a will fertility behavior and sex bias in large families. Journal of Human Resources 49 (2), 393–423.

Jensen, R., Oster, E., 2009. The power of tv: Cable television and women's status in india. The Quarterly Journal of Economics 124, 1057–1094.

La Ferrara, E., Milazzo, A., 2014. Customary norms, inheritance, and human capital: evidence from a reform of the matrilineal system in ghana.

Mathur, A., Slavov, S., 2013. Escaping domestic violence: Empowering women through employment, earnings and wealth in india. Tech. rep., AEI Economic Policy Working Paper Series 2013-03.

Mehlum, H., Miguel, E. A., Torvik, R., 2006. Poverty and crime in 19th century germany. Journal of Urban Economics 59 (3), 370–388.
URL http://EconPapers.repec.org/RePEc:eee:juecon:v:59:y:2006:i:3:p:370-388

Panda, P., Agarwal, B., 2005. Marital violence, human development and women's property status in india. World Development 33 (5), 823–850.

Rosenblum, D., 2013. Unintended consequences of women's inheritance rights on female mortality in india. Economic Development and Cultural Change, forthcoming.

Roy, S., 2008. Female empowerment through inheritance rights: evidence from india. London School of Economics, London.

Roy, S., 2015. Empowering women? inheritance rights, female education and dowry payments in india. Journal of Development Economics 114, 233–251.

Sekhri, S., Storeygard, A., March 2013. Dowry deaths: Consumption smoothing in response to climate variability in india. mimeo.

Singh, D., Dewan, I., Pandey, A. N., Tyagi, S., 2003. Spectrum of unnatural fatalities in the chandigarh zone of north-west india – a 25 year autopsy study from a tertiary care hospital. Journal of Clinical Forensic Medicine 10 (3), 145 – 152.

Stevenson, B., Wolfers, J., 2006. Bargaining in the shadow of the law: Divorce laws and family distress. The Quarterly Journal of Economics 121 (1), 267–288.

Strauss, J., Thomas, D., 1995. Human resources: Empirical modeling of household and family decisions. Handbook of development economics 3, 1883–2023.

Suran, L., Amin, S., Huq, L., Chowdury, K., 2004. Does dowry improve life for brides? a test of the bequest theory of dowry in rural bangladesh. Policy Research Division Working Papers No. 195.

Tur-Prats, A., 2015. Family types and intimate-partner violence: A historical perspective. Working Paper.

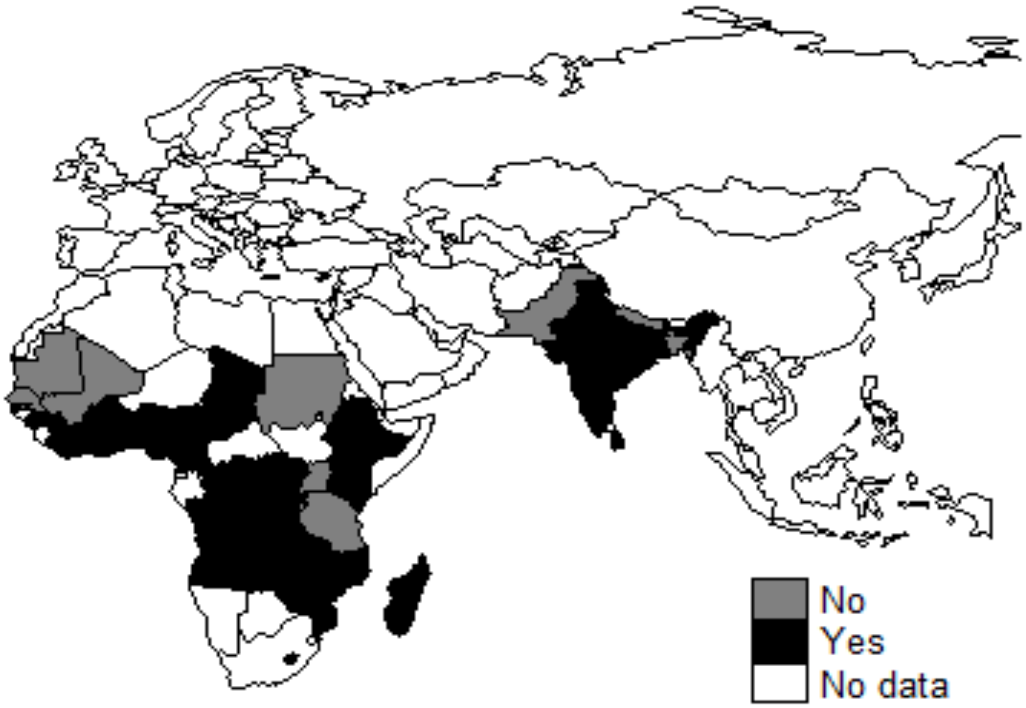
Wang, S.-Y., 2013. Property rights and intra-household bargaining. *forthcoming* Journal of Development Economics.

WHO, 2013. Global and regional estimates of violence against women prevalence and health effects of intimate partner violence and non-partner sexual violence.

Wolfers, J., 2006. Did unilateral divorce laws raise divorce rates? a reconciliation and new results. American Economic Review 96 (5), 1802–1820.

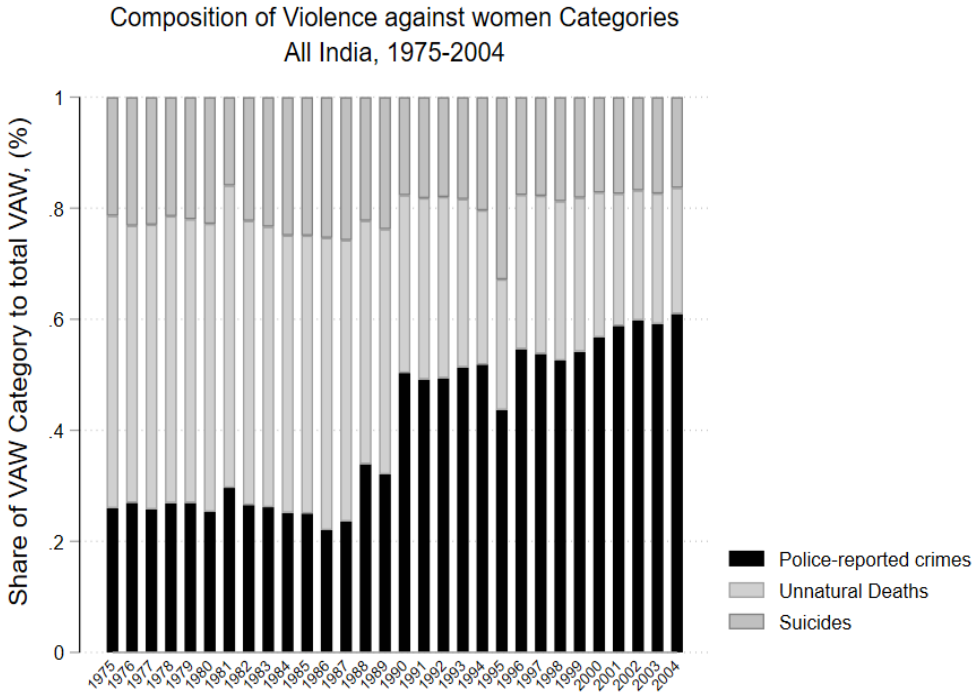
8 Appendix

Figure 1: Siblings inheritance equal rights



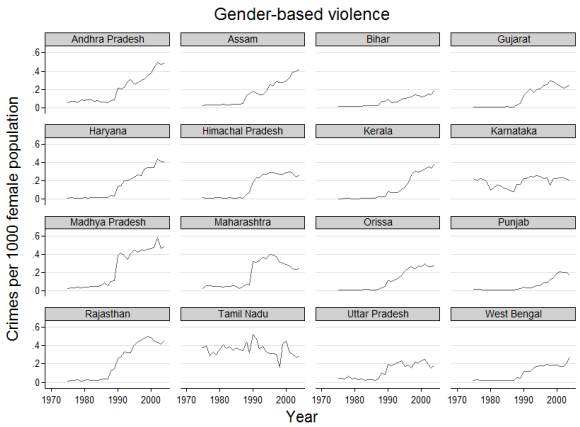
Notes: The map identifies countries that have equality in legislation between sons and daughters over moveable and immoveable inherited property. Source: Women, Business and the Law 2012 database, World Bank.

Figure 2: State-level measures of violence committed against women in India



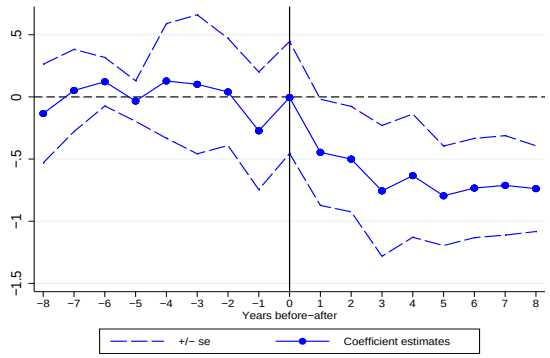
Notes: The rate of crimes against women is the state-year sum of the crime-categories considered under crimes against women by the Indian Penal Code.

Figure 3: Crimes against women across states between 1975-2004



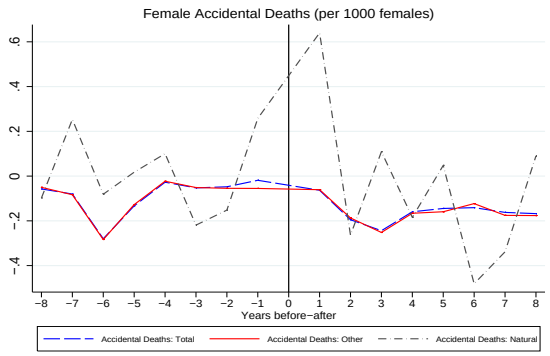
Notes: The rate of crimes against women is the state-year sum of the crime-categories considered under crimes against women by the Indian Penal Code.

Figure 4: Gender-based violence event-study



Notes: Estimated coefficients (vertical axis) and standard errors of the pre and post effect of the HSA (horizontal axis) on gender-based violence. Robust standard errors are clustered at the state level. The total rate of crimes against women is the state-year sum of the crime-categories considered under crimes against women by the Indian Penal Code.

Figure 5: Accidental deaths event-study



Notes: Estimated coefficients (vertical axis) and standard errors of the pre and post effect of the HSA (horizontal axis) on female accidental deaths. Robust standard errors are clustered at the state level. Accidental deaths attributable to natural events those that are due to lightning, heat strokes, floods, earthquake, landslides, cold exposure, cyclone, avalanche, starvation, epidemic, torrential rains and others. Accidental deaths attributable to non-natural events are sub-categorized into deaths due to air-crashes, collapse of structures, drowning, explosion, falls, fire, sudden deaths, poisoning, traffic and miscellaneous.

Figure 6: Accidental deaths event-study

Notes: Estimated coefficients (vertical axis) and standard errors of the pre and post effect of the HSA (horizontal axis) on male accidental deaths. Robust standard errors are clustered at the state level. Accidental deaths attributable to natural events those that are due to lightning, heat strokes, floods, earthquake, landslides, cold exposure, cyclone, avalanche, starvation, epidemic, torrential rains and others. Accidental deaths attributable to non-natural events are sub-categorized into deaths due to air-crashes, collapse of structures, drowning, explosion, falls, fire, sudden deaths, poisoning, traffic and miscellaneous.

Table 1: State Descriptive Statistics

	Total[a]	Accidents (F)[a]	Other (F)[a]	Natural (F)[a]	Accidents (M)[a]	Other (M)[a]	Natural (M)[a]	Robbery[b]
All-N	480	480	480	480	480	480	480	480
All- Mean	0.160	0.127	0.119	0.007	0.280	0.264	0.016	0.022
All- SD	0.142	0.083	0.078	0.020	0.146	0.137	0.022	0.018
HSA-N	150	150	150	150	150	150	150	150
HSA- Mean	0.210	0.172	0.165	0.007	0.371	0.357	0.014	0.017
HSA-SD	0.139	0.099	0.092	0.018	0.165	0.153	0.026	0.014
Non-HSA-N	330	330	330	330	330	330	330	330
Non-HSA- Mean	0.137	0.106	0.099	0.007	0.239	0.222	0.017	0.025
Non-HSA- SD	0.137	0.066	0.060	0.020	0.114	0.105	0.021	0.019
Diff.[c]	0.073†	-0.065†	0.066†	0.00	-0.280†	0.135†	-0.003	-0.008†
Diff.- SE	0.014	0.008	0.007	0.020	0.007	0.012	0.002	0.002
Pre-HSA Diff.[d]	-0.017	-0.088†	-0.089†	0.001	-0.0866†	-0.095†	0.008†	0.000
Pre-HSA Diff.-SE	0.019	0.011	0.010	0.003	0.019	0.018	0.003	0.002
	Riots[b]	Income per cap.	Police Force[b]	Rural Population	Women Seats	Sex ratio	Gender CM	Literacy
All-N	464	480	480	480	480	480	480	480
All- Mean	0.115	0.790	0.353	0.763	0.046	0.934	0.075	0.538
All- SD	0.092	0.774	0.347	0.091	0.026	0.045	0.264	0.151
HSA-N	145	150	150	150	150	150	150	150
HSA- Mean	0.127	0.904	0.179	0.692	0.043	0.974	0.060	0.625
HSA-SD	0.066	0.838	0.222	0.068	0.031	0.034	0.238	0.158
Non-HSA-N	319	330	330	330	330	330	330	330
Non-HSA- Mean	0.110	0.738	0.431	0.796	0.047	0.916	0.082	0.498
Non-HSA- SD	0.102	0.739	0.365	0.081	0.023	0.038	0.275	0.130
Diff.[c]	0.017*	0.166‡	-0.438†	-0.104†	-0.004	0.058†	-0.022	0.127†
Diff.- SE	0.009	0.076	0.073	0.008	0.003	0.004	0.025	0.014
Pre-HSA Diff.[d]	-0.006	0.553†	0.166†	0.082†	0.019†	-0.024	0.069*	0.027
Pre-HSA Diff. -SE	0.013	0.101	0.046	0.012	0.003	0.006	0.035	0.02

Notes: HSA and Non-HSA stand for states which have Hindu Succession Act Amendments and those that do not have, respectively. Accidents are total accidental deaths, Natural is accidental deaths due to natural causes and Other are accidental deaths due to other causes. Gender CM is a gender dummy for the state Chief Minister. a) Rates per 1000 gender-specific population. b) Rates per 100,000 total population. c) The difference in means between reform and non-reform states is an hypothesis test of the difference in means is equal to zero. d) Means difference test between reform and non-reform states for pre-HSA years. Significance values at the 10% is marked with $*$, at 5% with ‡ and 1% with †.

Table 2: Difference-in differences estimations on gender-based violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Mean of Dep. Var.						0.16					
Post-law	-1.436***	-1.438***	-1.449***	-1.451***	-1.452***	-0.437*	-0.439*	-1.136***	-0.477*	-1.650***	-0.887***
	(0.252)	(0.249)	(0.269)	(0.268)	(0.258)	(0.215)	(0.236)	(0.243)	(0.266)	(0.343)	(0.221)
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.07]	[0.09]	[0.01]	[0.11]	[0.2]	[0.01]
Controls	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Police Force	No	Yes	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Literacy Rate	No	No	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Political Controls	No	No	No	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Linear Trends	No	No	No	No	No	Yes	Yes	No	Yes	No	Yes
N	480	480	480	480	480	480	480	450	450	420	420
No. of States	16	16	16	16	16	16	16	15	15	14	14
Adj. R^2	0.88	0.88	0.88	0.88	0.88	0.95	0.95	0.91	0.95	0.91	0.96

Notes: Dependent variable is the natural logarithm of the ratio of the incidence of total reported crimes per state per female population (measured in thousands). Robust standard errors are clustered at the state-level are in curved brackets. P-values obtained using wild bootstrap standard-errors are in squared brackets. Coefficients significant at the 10%, 5% and 1% level are marked with $*$,$**$,$***$. Controls include female-male ratio, literacy rate, proportion of rural population and per capita net state domestic product. Strength of police force is per hundred thousand of population. Political controls include the proportion of seats held by women in the state legislature and a gender of the Chief Minister dummy. Columns 9 and 10 exclude Kerala and 11 and 12 exclude Karnataka and Maharashtra.

Table 3: Difference-in differences estimations on gender-based violence

	(1)	(2)	(3)
Post-law	-1.363***	-1.518***	-0.413*
	(0.382)	(0.280)	(0.211)
-3 years	-0.305	-0.408	0.083
	(0.305)	(0.283)	(0.169)
-4 years	-0.230	-0.352	0.074
	(0.326)	(0.315)	(0.183)
Controls	No	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
State Trends	No	No	Yes
F-test$_{(-3,-4)}$	1.11	2.61	0.12
p-value$_{(-3,-4)}$	0.354	0.107	0.88
N	480	480	480
No. of states	16	16	16
Adj. R^2	0.86	0.88	0.95

Notes: Dependent variable is the natural logarithm of the ratio of the incidence of total reported crimes per state per female population (measured in thousands). Robust standard errors are clustered at the state-level. Coefficients significant at the 10%, 5% and 1% level are marked with $*$,$**$,$***$. Controls include female-male ratio, literacy rate, proportion of rural population and per capita net state domestic product. Strength of police force is per hundred thousand of population. Political controls include the proportion of seats held by women in the state legislature and a gender of the Chief Minister dummy.

Table 4: Difference-in differences estimations: Female Accidental Deaths

	(1)	(2)	(3)	(4)	(5)
	Total Accidental Deaths				
Post-HSA	-0.011	-0.178**	-0.124***	-0.118**	-0.117
	(0.115)	(0.0735)	(0.0330)	(0.0521)	(0.0715)
Adj. R^2	0.07	0.21	0.41	0.41	0.42
	Other Causes Accidental Deaths				
Post-HSA	0.020	-0.164**	-0.113***	-0.104*	-0.139*
	(0.121)	(0.0743)	(0.0384)	(0.0513)	(0.0747)
Adj. R^2	0.04	0.25	0.50	0.48	0.50
	Natural Causes Accidental Deaths				
Post-HSA	0.294	0.111	-0.032	-0.075	0.392
	(0.228)	(0.184)	(0.286)	(0.287)	(0.335)
Adj. R^2	0.34	0.35	0.43	0.44	0.46
Controls	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
State Linear Trends	No	No	Yes	Yes	Yes
N	480	480	480	450	420
No. of States	16	16	16	15	14

Notes: Dependent variables are the natural logarithm of the rate of accidental deaths by per female population (measured in thousands). Accidental deaths attributable to natural events those that are due to lightning, heat strokes, floods, earthquake, landslides, cold exposure, cyclone, avalanche, starvation, epidemic, torrential rains and others. Accidental deaths attributable to non-natural events are sub-categorized into deaths due to air-crashes, collapse of structures, drowning, explosion, falls, fire, sudden deaths, poisoning, traffic and miscellaneous. Robust standard errors are clustered at the state-level. Coefficients significant at the 10%, 5% and 1% level are marked with $*$,$**$,$***$. Controls include female-male ratio, literacy rate, proportion of rural population and per capita net state domestic product and strength of police force per hundred thousand of population.

Table 5: Difference-in differences estimations: Male Accidental Deaths

	(1)	(2)	(3)	(4)	(5)
	Total Accidental Deaths				
Post-HSA	0.06	-0.130*	-0.076***	-0.083	-0.062
	(0.100)	(0.077)	(0.022)	(0.052)	(0.054)
Adj. R^2	0.431	0.573	0.748	0.748	0.753
	Other Causes Accidental Deaths				
Post-HSA	0.069	-0.130	-0.079***	-0.082	-0.090
	(0.107)	(0.075)	(0.021)	(0.048)	(0.059)
Adj. R^2	0.381	0.553	0.754	0.746	0.758
	Natural Causes Accidental Deaths				
Post-HSA	0.356	0.132	0.129	0.115	0.385
	(0.213)	(0.156)	(0.229)	(0.229)	(0.358)
Adj. R^2	0.490	0.505	0.574	0.570	0.605
Controls	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
State Linear Trends	No	No	Yes	Yes	Yes
N	480	480	480	450	420
No. of states	16	16	16	15	14

Notes: Dependent variables are the natural logarithm of the rate of accidental deaths by per male population (measured in thousands). Accidental deaths attributable to natural events those that are due to lightning, heat strokes, floods, earthquake, landslides, cold exposure, cyclone, avalanche, starvation, epidemic, torrential rains and others. Accidental deaths attributable to non-natural events are sub-categorized into deaths due to air-crashes, collapse of structures, drowning, explosion, falls, fire, sudden deaths, poisoning, traffic and miscellaneous. Robust standard errors are clustered at the state-level. Coefficients significant at the 10%, 5% and 1% level are marked with $*$,$**$,$***$. Controls include female-male ratio, literacy rate, proportion of rural population and per capita net state domestic product and strength of police force per hundred thousand of population.

Table 6: Difference-in difference estimations on gender neutral crimes

	(1)	(2)	(3)	(4)	(5)
	Robery per 100,000 population				
Post-HSA	0.338	0.130	0.298	0.381*	0.099
	(0.242)	(0.278)	(0.181)	(0.198)	(0.145)
N	480	480	480	450	420
Adj. R^2	0.05	0.11	0.46	0.44	0.46
	Riot per 100,000 population				
Post-HSA	0.242	0.008	0.094	0.123	-0.042
	(0.232)	(0.264)	(0.137)	(0.196)	(0.109)
Controls	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
State Linear Trends	No	No	Yes	Yes	Yes
N	462	462	462	433	404
Adj. R^2	0.23	0.33	0.65	0.65	0.65
Number of States	16	16	16	15	14

Notes: Dependent variables are in logarithms. Coefficients significant at the 10%, 5% and 1% level are marked with $*$,$**$,$***$.Controls include female-male ratio, literacy rate, proportion of rural population and per capita net state domestic product and strength of police force per hundred thousand of population.

Table 7: Descriptive Statistics - Intimate partner violence

	All	HSA^a	Non-HSA^b	Diff.$^{(b-a)}$
Intimate-Partner Violence	0.452	0.390	0.485	0.095
	(0.067)	(0.054)	(0.040)	(0.067)
Hindu	0.852	0.840	0.858	0.019
	(0.024)	(0.041)	(0.030)	(0.051)
Muslim	0.120	0.106	0.128	0.022
	(0.021)	(0.035)	(0.026)	(0.044)
Rural	0.574	0.469	0.630	0.161***
	(0.025)	(0.043)	(0.032)	(0.054)
Son Pref.	0.294	0.120	0.386	0.266***
	(0.027)	(0.046)	(0.034)	(0.057)
Household size	4.930	4.492	5.163	0.671***
	(0.108)	(0.181)	(0.135)	(0.226)
Spousal education gap	2.087	1.385	2.460	1.075**
	(0.200)	(0.335)	(0.250)	(0.418)
Spousal age gap	6.067	6.659	5.754	-0.906
	(0.296)	(0.495)	(0.369)	(0.618)
Television	0.488	0.600	0.429	-0.171**
	(0.035)	(0.058)	(0.043)	(0.072)
N	40,884	14,160	26,724	

Notes: This table presents the means and standard errors clustered at the state-level, in parenthesis. The Diff. is the means test difference between Non-HSA and HSA states. Significance levels at the 5% and 1% level are denoted by ** and ***, respectively.

Table 8: Difference-in- differences estimations on intimate partner violence

	(1)	(2)	(3)
Mean of dep. var.		0.452	
	Panel A		
Post-HSA	-0.067***	-0.099***	-0.098***
	(0.013)	(0.012)	(0.011)
N	40,884	40,884	40,884
No. of States	16	16	16
Adj.R^2	0.12	0.12	0.12
	Panel B		
Post-HSA	-0.065***	-0.097***	-0.096***
	(0.013)	(0.012)	(0.012)
N	39,790	39,790	39,790
No. of States	15	15	15
Adj.R^2	0.11	0.11	0.11
	Panel C		
Post-HSA	-0.055***	-0.092***	-0.091***
	(0.010)	(0.015)	(0.014)
N	34,552	34,552	34,552
No. of States	14	14	14
Adj.R^2	0.11	0.11	0.11
State FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
State-Cohort Linear Trends	No	Yes	Yes
Hindu*State	No	No	Yes
Hindu*Cohort	No	No	Yes

Notes: Using the sample of wives of the pooled NFHS. The dependent variable is a stock of domestic violence dummy that equals to 1 if the wife was ever subject to violence committed by the husband and 0 otherwise. Control variables include a *Hindu* dummy, a rural household dummy, son preference of the wife, household size, spousal education and age gap and a dummy for television ownership. The explanatory variable of interest is a dummy that takes the value 1 if a woman resident in a reform state married after the reform relative to the reform year in her state. Robust standard-errors are clustered at the state-level. Significance levels at the 10%, 5% and 1% level are denoted with $*$,$**$,$***$.

Table 9: Difference-in- differences estimations on intimate partner violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mean of Dep. Variable				0.448					0.504	
Post-HSA	-0.094***	-0.065***	-0.099***	-0.064***	-0.099***	-0.057***	-0.057***	-0.077	-0.071	-0.040
	(0.017)	(0.016)	(0.015)	(0.016)	(0.015)	(0.017)	(0.017)	(0.044)	(0.050)	(0.048)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort Linear Trends	No	No	Yes	No	Yes	No	Yes	Yes	Yes	Yes
N	34,824	34,824	34,824	34,139	34,139	29,350	29,350	4,916	4,716	4,213
No. of States	16	16	16	15	15	14	14	16	15	14
Adj. R^2	0.07	0.12	0.12	0.12	0.12	0.12	0.12	0.10	0.09	0.10
Religious Sample	*Hindu*	*Hindu*	*Hindu*	*Hindu*	*Hindu*	*Hindu*	*Hindu*	Muslim	Muslim	Muslim

Notes: Using the sample of *Hindu* and Muslim wives of the pooled NFHS, respectively. The dependent variable is a stock of domestic violence dummy that equals to 1 if the wife was ever subject to violence committed by the husband and 0 otherwise. Control variables include a rural household dummy, son preference of the woman, household size, spousal education and age gap and a dummy for television ownership. The explanatory variable of interest is a dummy that takes the value 1 if a woman resident in a reform state, married after the reform relative to the reform year in her state. Robust standard-errors are clustered at the state-level. Significance levels at the 10%, 5% and 1% level are denoted with $*$,$**$,$***$.

Table 10: Difference-in-differences estimates for *Hindu* husbands

	(1)	(2)	(1)	(2)	(1)	(2)
	Alcohol consumer		Frequency		Decision	
Mean of dep. var.	0.406		0.117		0.844	
Post-HSA	-0.043***	-0.044*	-0.033**	-0.050***	0.024*	0.022
	(0.010)	(0.024)	(0.013)	(0.009)	(0.014)	(0.021)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort Linear Trends	No	Yes	No	Yes	No	Yes
N	18,577	18,577	8,400	8,400	14,301	14,301
Adj. R^2	0.06	0.06	0.04	0.04	0.06	0.06

Notes: Using the sample of *Hindu* husbands of the NFHS-III. The dependent variables are (in order of appearance): a) a binary variable for alcohol consumers; b) a binary variable for frequent alcohol consumers and, c) a binary variable that takes the value 1 if the wife has any role in decision-making. Control variables include the respondent's education in groups (the omitted category is no education), urban household dummy, household size, age and age squared. The specification on Decision contains spouse age gap instead of husband's age and age squared.

Table 11: Difference-in-differences for *Hindu* women

	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Dep. Variable		Distance			Husband from Natal Village			Involved in Marriage Negotiation	
Mean of Dep. Variable		3.397			0.112			0.424	
Post-HSA	-0.279**	-0.561***	-0.294**	0.050**	0.047*	0.050**	0.002	-0.017	0.002
	(0.101)	(0.076)	(0.100)	(0.023)	(0.025)	(0.022)	(0.029)	(0.033)	(0.029)
Controls	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort Linear Trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	15,061	15,061	15,061	15,061	15,061	15,061	15,061	15,061	15,061
Adj. R^2	0.02	0.01	0.02	0.05	0.05	0.05	0.17	0.17	0.17

Notes: Using the sample of *Hindu* wives of the IHDS. The dependent variables are (in order of appearance): a) distance from the the post-marriage household to the natal house (in hours); b) a binary variable that takes the value 1 if the respondent grew up in the same village as the current husband and, c) a binary variable to whether the respondent had any say in the choice of husband. Control variables include women's education (in years), urban household dummy, household size, a dummy variable for land ownership of the household and son preference of the woman.

Table 12: Effect of HSA on all forms of violence committed against women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rate per Capita	All VAW (Police-reported, unnatural deaths and female suicides)							
Post-law	-0.441**	-0.441**	-0.459***	-0.409**	-0.462***	-0.462***	-0.169**	-0.163*
	(0.154)	(0.154)	(0.153)	(0.146)	(0.121)	(0.123)	(0.0727)	(0.0882)
N	480	480	480	480	480	480	480	480
Adj. Rsq	0.800	0.800	0.804	0.805	0.830	0.829	0.906	0.908
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Force	No	No	Yes	No	Yes	Yes	No	Yes
Literacy Rate	No	No	No	Yes	Yes	Yes	No	Yes
Political Controls	No	No	No	No	No	Yes	No	Yes
Year Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Linear Trends	No	No	No	No	No	No	Yes	Yes

Notes: Dependent variable is the natural logarithm of all forms of violence against women per total population. These include incidence of total reported crimes against women as in Table 2 plus female unnatuural deaths due to other causes as in Table 4 and female suicides as in Anderson and Genicot (2014). Robust standard errors are clustered at the state-level. Coefficients significant at the 10%, 5% and 1% level are marked with ∗,∗∗,∗ ∗ ∗. Controls include female-male ratio, literacy rate, proportion of rural population and per capita net state domestic product. Strength of police force is per hundred thousand of population. Political controls include the proportion of seats held by women in the state legislature and a gender of the Chief Minister dummy.