THREE ESSAYS ON GENDER IN SOUTH ASIA

by

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DISSERTATION ABSTRACT

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This dissertation explores distinct facets of the association between social institutions and family formation, as it pertains to women in South Asia. In Chapter II, I analyze an increase in the legal age of marriage for women in India and find the policy led to a permanent decrease in teen marriage, despite a lack of widespread enforcement. In communities where the national policy represents a tightening of standards over current norms, the law can benefit young women. In Chapter III, I find that decreases in income lead to increases in crimes in the name of honor. The results indicate that honor killings, primarily believed to be culturally or religiously motivated, are partially explained by shocks to income–short periods of economic distress can increase the risk of violence against vulnerable members of the population. Finally, in Chapter IV, I provide evidence for co-dependencies in fertility behavior in joint household residences. The results highlight the existence of fertility rivalry as a means to improve social status.

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CHAPTER I

INTRODUCTION

Family composition is an immensely important determinant of economic outcomes such as labor market participation, savings, consumption, and fertility decisions. However, the incentive structures and social norms central to the establishment of the family may also promote the harmful customs that developing countries are rigorously investing to eradicate, such as early marriage and childbirth or female infanticide. In the three essays of this dissertation, I examine the interplay between social norms and family in the distinct contexts of early marriage, honor killings, and joint household structures.

In the first essay, I study the effect of a 1978 child marriage ban in India which raised the legal age of marriage for women from 15 to 18 years. Prior to 1978, approximately 26 percent of women married under the age of 15, while 72 percent married before 18. I exploit variation in a woman's year of birth and local child marriage social norms to define exposure to the policy and find that the ban led to a 7.8 percentage point decrease in the probability of marrying under 18, at the average intensity.

In my exploration of the mechanisms for such an effect, I show that differential sex ratios, enforcement capacities, political leadership, or agriculture income shocks do not explain the results and instead argue that a perception of enforcement drives individual behavior. The results suggest that national policies targeting local customs can become focal points (people mutually believe the law represents others' behavior) even without stringent enforcement, leading to convergence towards the new law. The research makes a significant contribution to

our understanding of policy implementation by highlighting the non-sanctionary role of law in affecting the behavior of individuals.

In the second essay, I investigate a unique form of violence against women where the primary justification for the crime is the protection of "honor" as a social construct. Theoretically, women who (allegedly or not) participate in pre-marital relationships, and their female relatives, are often no longer considered 'marriageable' by society and must then be financially supported by their natal family. In periods of economic distress and poverty, natal and extended families may be more sensitive to accepting this responsibility, instead resorting to violence.

To explore this hypothesis, I systematically collect and construct a data set of honor killings from newspaper reports. I take advantage of detailed remote sensing rainfall data (as measured by satellites) to proxy for income changes - cotton farming is a significant contributor to rural household income in Pakistan, and increases in precipitation over the average decrease cotton production. Consistent with the hypothesis that income shocks influence honor killings, I find that a standard deviation increase in contemporaneous rainfall leads to a 6.3 percent increase in reported honor killings. The results indicate that honor killings, primarily thought of as culturally or religiously motivated, are partially explained by shocks to income–short periods of economic distress can increase the risk of violence against vulnerable members of the population. Whereas implementing laws to restrict practices embedded in local cultures are met with much push back in the country, evidence suggesting that poverty or income variation is a contributing factor makes it easier to develop socially acceptable policies to restrict honor killings.

In the third essay of this dissertation, I explore the social tradition of patrilocality in India to estimate the effect of joint household residence on competitive fertility behavior. Patrilocality implies that it is usually one or more adult sons who co-reside with their parents, wives, and children while adult daughters move out upon marriage. A joint residence is very common; approximately 20 percent of households in India have at least one co-residing daughter-in-law.

I examine the hypothesis that the social status of women in joint Indian households is determined by both their husbands' relative age and the gender of their children. Theoretically, women who marry the elder co-resident son (first rank) should have greater empowerment due to being the elder daughter-in-law, compared to women who marry the younger co-resident son (second rank). I provide evidence for this hypothesis and show that first ranking women increase birth spacing by 9.1 percent on average upon the entrance of the second rank. On the other hand, women who marry the younger co-residing son cannot improve their autonomy using rank and instead have a greater incentive to birth a son. I show that on average, second ranking women reduce their marriage-to-first-birth interval by approximately 4.8 percent if the first ranking woman in the household already has a son. The results provide evidence for fertility rivalry within a household.

CHAPTER II

EARLY MARRIAGE AND SOCIAL NORMS: EVIDENCE FROM INDIA'S UNENFORCED CHILD MARRIAGE BAN

2.1. Introduction

Social norms shape and inform the behavior of individuals. Norms can solve coordination problems by providing 'reference points' or salient patterns of beliefs to which individuals converge (Schelling 1960). Social norms can also offer crucial signals of membership. For example, Carvalho (2013) suggests veiling among Muslim women offers a commitment device to both reduce deviations from religious behavior and signal social identity. Often, customs may also foster communal cooperation in ensuring punishment of transgressors of the norm. Murder for 'honor' is the socially accepted and practiced punishment for young couples in tribal Pakistan, who digress from local customs of arranged marriage by marrying of their own choosing. Thus, social norms can be oppressive if the conventions disadvantage at least some members of the community (Platteau and Wahhaj 2014).

A prominent example of a social norm which results in harmful impacts is that of early marriage. Previous studies have determined the correlation of the early marriage of women with higher levels of intimate partner violence, but lower levels of education and economic empowerment (Yount et al. 2016; Yount, Crandall, and Cheong 2018). Recent literature has also observed a causal effect of early marriage on female schooling, infant mortality rates, gender based norms, and inter-generational health and education outcomes (Field and Ambrus 2008; Sekhri and Debnath 2014; Chari et al. 2017; Garcia-Hombrados 2017;

Asadullah and Wahhaj 2019). Despite documentation of the harmful effects of early marriage on young women and their children, the custom has not been successfully eradicated. Approximately 650 million women and girls today married before their 18th birthday, with India as the largest contributor to this number (UNICEF 2018).

Governments often use legal sanctions to curtail participation in harmful customs. However, the success of legislation in reducing participation is highly dependent on the degree of enforcement of the law, as well as the strength of existing local norms. If local authorities or traditional leaders have a vested interest in maintaining community norms, the cost of enforcing statutory laws may be too high for governments. Likewise, the cost of deviation from the norm may be too high for individuals whose own preferences differ from local customs (Aldashev, Platteau, and Wahhaj 2011; Aldashev et al. 2012; Acemoglu and Jackson 2017). Furthermore, laws which conflict with local norms may simply be ignored. For example, dowry payment practices are prevalent in both India and Pakistan, despite legislation disallowing large transfers at marriage.

However, lack of enforcement does not decidedly make legislation a 'dead letter'. In fact, unenforced laws can still affect customs through their expressive role (Mcadams 2000b, 2000a; Basu 2018). That is, the law can act as a focal point and replace existing norms as long as it changes expectations of individuals about the behavior of others (Mcadams 2000b). For individuals who's own preferences differ from the local custom, even unenforced laws can offer a valid reason to change behavior.

In this paper, I study the effectiveness of India's 1978 child marriage ban which raised the legal age of marriage for women from 15 to 18. I find

evidence suggesting individuals exposed to the policy change were induced to delay marriage. There is little documentation of enforcement of the ban, although evidence suggests individuals were aware of the law. I argue that despite a lack of observable enforcement, the policy change provided a signal of the behavior of others. A high *perception* of enforcement has the potential to further highlight and strengthen this signal. Thus, if expectations of the social norm in the arranged marriage market are correlated with parents' own decisions, these signals, by updating available information, can affect behavior.

In October 1978, the Indian government passed the Child Marriage Restraint (Amendment) Act (CMRA) , raising the legal age of marriage for women from 15 to 18 and 18 to 21 years for men. The CMRA amendment was a national policy, ruling out the use of geographical or time variation to identify the effect of the ban. Instead, I use a quasi-experimental approach where my identification strategy relies on the fact that an individual's exposure to the policy was a function of her exogenous age when the policy was enacted and the strength of early marriage norms in her marriage market. Thus, I define treated cohorts as individuals younger than 15 (these women must now wait three extra years to legally marry) and control cohorts as individuals older than 18 in 1978 (these women could legally marry before and after the ban). I exploit the intensity of early marriage norms in the individual's marriage market, wherein norms are defined as the pre-policy probability of marrying before 18. Marriage markets are defined as a combination of an individual's state, religion and caste at birth. Substantial variations existed in the prevalence of pre-policy child marriage across marriage markets indicating differences in early marriage traditions and stigma, which would potentially impact the effectiveness of the law across markets.

Using the 1998 and 2005 survey years from the Demographic and Health Survey (DHS), I show that the early marriage ban led to a 7.8 and 5.1 percentage point decrease at the average norm intensity, in the likelihood that a woman is married before the ages 18 and 15, respectively. Interestingly, I do not find that the policy significantly changed the age of marriage for men, or the quality of marital matches for treated women. Moreover, I do not find evidence for differential sex ratios, political alignment, agriculture income shocks or enforcement capacities (as measured by police presence) as driving the results.

This paper makes contributions to three broad strands of existing literature. First, the research contributes to the vast literature studying the effectiveness of policy measures in reducing early marriage. Evidence suggests conditional cash transfers, compulsory education, and empowerment programs all have positive impacts on reducing the likelihood of underage marriage of girls (Buchmann et al. 2018; Kirdar, Dayiolgu, and Koc 2018). García-Hombrados (2017) shows that women who are exposed to a legal age of marriage of 18 in Ethiopia delay cohabitation and have lower infant mortality rates. Within the Indian context, Hatekar, Mathur, and Rege (2007) illustrate that the earlier 1929 Child Marriage Restraint Act which implemented a minimum legal age of 14 for girls in the country, increased the average age of marriage. Blank, Charles and Sallee (2009) demonstrate that while minimum age of marriage laws lower the incidence of early marriage in the United States, variation in these laws by state leave room for systematic misrepresentation of age in official records, as well as migration to other states to circumvent the law. In contrast to earlier work, my paper studies an increase in the legal age of marriage within a context where there were minimal external sanctions. I show that the law nonetheless affected the behavior

of individuals. The results have important implications for the role of governments in affecting the acceptability and practice of harmful customs through the non-sanctioning arm of the law.

Second, the paper contributes broadly to the literature studying the interaction of laws with social norms, and in particular, the expressive influence of the law (Schelling 1960; Mcadams 2000a; Basu 2018). Previous theoretical work on the expressive role of the law suggests that by changing the expectations of individuals in a coordinated manner, statutory laws can remove inefficient or harmful local customs (Aldashev, Platteau, and Wahhaj 2011; Platteau and Wahhaj 2014). Experimental work provides support to the theory. For example, Vogt et al. (2016) show that the provision of video based information on female genital cutting significantly improves attitudes towards uncut girls in Sudan. Similarly, Amirapu, Asadullah and Wahhaj (2019) randomly provide video-based information on a recent child marriage law in Bangladesh. The authors find that the provision of information about the law is sufficient to change individuals' own belief and attitudes about the appropriate legal age of marriage, although it does not change expectations of the behavior of others. Chen and Yeh (2014) show that obscenity laws in the US have heterogenous effects on own sexual attitudes, depending on an individual's religiosity. In contrast, this paper supports earlier research by providing suggestive policy-based evidence for the expressive role of the law.

More broadly, the research relates to earlier economics and psychology literature studying the effects of reducing information gaps or improving knowledge of community norms. For example, the provision of information about labor market aspirations of female peers in Saudi Arabia improves own aspirations to

participate in the labor market (Aloud et al. 2020). Schultz et al. (2007) find that providing households with information about community and own electricity consumption leads to convergence; households consuming more than the average were now likely to decrease consumption but households consuming less electricity were induced to increase consumption.

To provide support for my identification strategy, I show that the probability of marrying before 18 does not differ systematically for treated and control cohorts across marriage markets, before announcement of the ban. By including state and time-varying fixed effects, I rule out confounding effects which may be correlated with early marriage. As a robustness check, I also allow for differential time trends by religion and caste and use different definitions of marriage markets. Finally, I rule out migration across marriage markets as a confounding factor that might bias the estimates.

The remainder of the paper is organized as follows. Section 2.2. describes the Child Marriage Restraint Act of 1978. Section 2.3. outlines the identification and empirical strategy. Section 2.4. details the data employed in this research while Section 2.5. explains the results. Section 2.6. provides a detailed discussion of marriage market effects and mechanisms. Section 2.7. describes several robustness tests and Section 2.8. concludes.

2.2. Child Marriage Restraint Act (Amendment) of 1978

During the 1970s, population control agendas were part of the regular discourse in international organizations, such as the United Nations and the World Bank. In 1974, the Indian Ministry of Health and Family Planning issued a mass announcement predicting a 15 percent decrease in birth rates if the minimum age of marriage for women were increased to 18 years (Dandekar 1974). This was

followed by then Indian Prime Minister Indira Gandhi, leader of the Congress National Party, announcing the government would consider raising the minimum legal age of marriage in the country (Bhatia and Tambe 2014). Conversation around the proposed Child Marriage Restraint Act (Amendment) soon died down in the 1976-77 Emergency Rule during which stricter population control policies were adopted instead (including forced sterilizations), which would later contribute to the fall of the Congress Party in the 1977 General Election.

The amendment to raise the legal age of marriage was finally brought to parliament in 1978, under the Janata Party leadership. The Janata Party was keen to adopt policies which provided a different path to population control than the one chosen by its predecessors, and the amendment was brought to Congress in March 1976 with the support of demographers but without much debate from civil society (Bhatia and Tambe 2014). This Child Marriage Restraint (Amendment) Act of 1978 raised the legal age of marriage for women from 15 to 18 years, and from 18 to 21 years for men. The law was enacted on 1st October 1978 and was applicable to all regions of the country.

Although the minimum age of marriage was raised by three years, the amendment did not increase the severity of punishment of convicted offenders, nor were marriages involving underage brides or grooms considered invalid. As before, punishment of offenders was restricted to a maximum of three months of jail time, a 1000 Rupee fine, or both.[1] In the case of an offense, the guardian of the underage bride or groom, the priest who officiates the wedding, and any groom above 21 years of age are punishable under the law. However, the law did

[1]In most cases, this fine would be much less than the cost of arranging the marriage of a daughter, or paying her dowry. Thus, the severity of the punishment is not credibly binding for parents of young girls.

make offenses partly cognizable, allowing law enforcement to investigate complaints made by the public within a window of a year of the marriage (Mahmood 1980).

Evidence of enforcement is minimal. With the exception of Gujarat, states only recently have begun to make marriage registration mandatory in India. Without the compulsory registration of marriages, which requires proof of age for both the bride and groom, there is little the government could do to effectively track non-abiders of the law.

There are two reasons why the CMRA had potential to influence behavior despite a lack of enforcement. First, the 1992 Demographic and Health Survey (DHS) survey indicates individuals were aware of the legal age of marriage. When asked what the legal age of marriage for women in the country is, approximately 56 percent correctly answered 18. Approximately 70 percent of respondents from urban areas had correct knowledge of the legal age of marriage. While 56 percent would seem low at first glance, it is important to bear in mind that during this period the government relied on non-televised methods of communication and information dissemination. These statistics, although measured several years after the policy change, provide suggestive evidence that individuals were aware of the law.[2]

Second, the late 1970s were characterized by active governance, with the initiation of massive and often, unpleasant, social projects. The announcement of the CMRA, and the connection drawn between the child marriage ban and population growth reduction was made during this period. It is quite possible that the perceived enforcement of the ban was high, simply because the government

[2]It is likely that over time awareness of the policy grew, in which case 56 percent would be an overestimate. However, it is also possible that awareness of the policy change decreased over time due to a lack of enforcement, suggesting the number is underestimating awareness. A priori, it is unclear which direction of bias to expect.

had proven its intention and capacity for stringent enforcement; a high perception of enforcement had the potential to further highlight, and make credible, the early marriage ban. An example of such stringent enforcement is the mass sterilization campaign which was aimed at reducing population growth in the country. Thousands of men across India were forcibly sterilized, with local officials often meeting pre-determined quotas by targeting even the elderly.

2.3. Identification Strategy and Methodology

2.3.1. Empirical Strategy. The primary identification challenge is that the CMRA amendment was a national policy affecting all Indian states - apart from Jammu and Kashmir - at once, ruling out the use of geographical or time variation in implementation to estimate the effect of the ban. Instead, I exploit variation in pre-policy social norms across marriage markets. Thus, an individual's exposure to the policy is defined as a combination of her marriage market norms and date of birth.

Women born before October 1960 were older than 18 when the policy was enacted and could thus legally marry; these women should not be affected by the law. Women born after October 1963 were younger than 15 years when the policy was passed and would now have to wait three extra years to legally marry.[3] I drop women between the ages of 15 and 18 (when the policy was enacted) from my sample as they are only partially treated. They could legally marry under the old law but any unmarried individuals in these cohorts must now wait until their 18th

[3]Some prior studies have used the age of cohabitation rather than the age of marriage. I use the age of marriage because the CMRA amendment does not differentiate between women who marry but have not consummated the marriage, and women who marry and have consummated the marriage. The act of the marriage is illegal, rather than cohabitation or engagement, and marriage generally predates cohabitation in India. As a robustness I show that the age at cohabitation displays similar patterns.

birthday to marry.[4] My sample of control and treated women includes individuals born between 1950-1960 and 1963-1973, respectively.

Marriage market norms at birth are used as a second dimension of variation in the intensity of an individual's exposure to the policy. Marriage markets are defined as a combination of state of residence, religion, and caste. Marriage market norms, such as acceptable ages of marriage or the stigma of marrying late, can vary widely across regions and cultures and can impact the intensity with which a woman is exposed to the policy. For example, Maertens (2013) shows that perceptions about the behavior of others, or the acceptable age of marriage for girls, constrains the decisions of parents to invest in their daughter's education (a substitute for marriage). However, social norms are not directly observable to the econometrician. I appeal to stationarity and estimate social norms as the pre-policy probability of marrying under 18 in a marriage market, allowing norms to vary by religion and across caste. Thus, my measure of social norms captures the pre-policy average behavior of women in a marriage market.

An increase in the legal age of marriage can lower the cost of deviation for households whose preferences do not align with local norms, allowing them to delay the marriage of their daughters. Provision of information and signals of the behavior of others can lead to delayed marriage if households do not want to deviate too far from the local norm. However, announcement of the ban could also induce households to marry their daughters off early, in expectation of future enforcement. Theoretically, it is not clear a priori if we should expect respondents in the highest intensity child marriage regions to reduce early marriage.

[4]My results are robust to the inclusion of women of ages 15 to 18 at policy enactment.

I use a quasi-experimental design to compare outcomes across cohorts in marriage markets with varying pre-policy age of marriage norms. To exploit variation in exposure to the policy, I run the following regression:

$$M_{icm} = \beta \ \textit{Treat}_{ic}\textit{*Intensity}_m + \phi_c + \gamma_m + \alpha_z + \eta_r + \sigma_y + \epsilon_{icm} \tag{2.1}$$

where M_{icm} is a dummy variable equal to 1 if a woman i, born in cohort c and marriage market m, married before 18, and 0 otherwise. I also examine the effect of the policy on the probability of marriage before 15. $\textit{Treat}_{ic}$ is a dummy equal to 1 if a woman was less than 15, and 0 if she was older than 18 when the policy was enacted. $\textit{Intensity}_m$ is a measure of the pre-policy average probability of marrying underage in a marriage market m. Thus, $\textit{Intensity}_m$ varies between 0 and 1, where 1 indicates that within a marriage market m, all women marrying before the ban were younger than 18. α_z, η_r, and σ_y are caste, religion, and survey year fixed effects. ϕ_c and γ_m are cohort and marriage market fixed effects.

The primary variable of interest is *Treat*Intensity*. If the increase in the legal age of marriage caused a decline in the probability of early marriage in the highest child marriage regions, I would expect β to have a negative sign.

My identification assumption is that in the absence of the policy, there would not have been a systematic difference in the change in the probability of early marriage across marriage markets. In other words, there should not be a differential change across marriage markets for women not exposed to the policy. To test this assumption, I plot results from the cohort-year specific regression:

$$M_{icm} = \sum_{c=1951}^{1973} (d_{ic}\textit{*Intensity}_m)\beta_c + \phi_c + \gamma_m + \eta_r + \sigma_y + \epsilon_{icm} \tag{2.2}$$

where M_{icm} is an indicator variable equal to 1 if a woman i born in the year c and marriage market m is married under 18, and 0 otherwise. d_{ic} is a cohort dummy (year of birth instead of month-year of birth) for individual i, ϕ_c are cohort fixed effects, and γ_m are marriage market fixed effects. I also include religion and survey year fixed effects, η_r and σ_y respectively. Individuals born in the year 1950 form my control group and the dummy is omitted from the regression. Thus, I can interpret each coefficient β_c as the estimated impact of the policy on a cohort. Because the policy did not affect the marriage decisions of women who were already older than 18, I should expect the coefficient estimates β_c for c $\leq$ 1959 to be 0 and to begin decreasing for c $\geq$ 1960.

2.3.2. Marriage Markets in India. To fully understand this identification strategy, a discussion of marriage markets in India is merited. Prior literature has studied behavior in marriage markets at various geographical levels. For example, Foster (2002) defines marriage markets at the village level when studying the role of marriage market selection on human capital formation in Bangladesh. In contrast, Beauchamp, Calvi, and Fulford (2016) suggest the closest approximation of a marriage market in India is at the district, religion and caste level. There are trade-offs to defining marriage markets too broadly or narrowly. Most women in India migrate to their husband's home at marriage, so using the village of residence may lead to biased results if women differentially select into marriage markets with different social norms, after the ban. The likelihood of early marriage may also be affected by differential sex ratios within a market, potentially biasing my estimates. This confound is especially problematic the smaller the geographical marriage market; any effect of too few marriageable men or women is likely washed out at the district or state level.

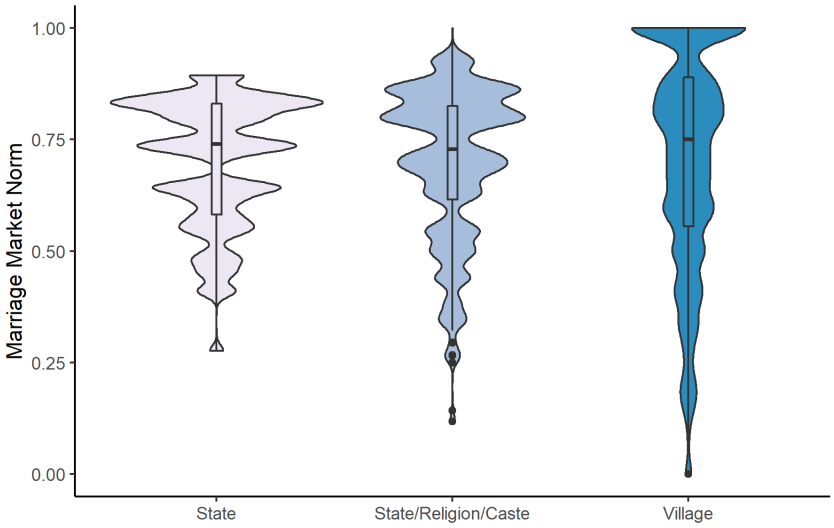
Keeping these trade-offs in mind, I define a marriage market as a combination of an individual's state of residence, religion and caste; there are 130 unique markets in the sample data. Ideally, I would know each individual's residence of birth, but the data only report current residence. Nonetheless, approximately 91 percent of women in India remain in their state of birth after marriage, and 95 percent marry within their caste.[5] Thus, an identification assumption in the analysis is that the current marriage market is a good proxy for the marriage market at birth. On average, women in India migrate 3.6 hours from their natal village upon marriage.[6] Thus, most migration occurs within the marriage market, reducing concerns of selection bias. As a robustness test, I show that migration upon marriage is not biasing my results.

To confirm that the results are not being driven by my choice of marriage markets, I separately define marriage market norms at the state and village level and rerun my analysis; the results are robust to other definitions of marriage markets. In Figure 1, I display the kernel density of pre-policy early marriage norms across the different definitions of marriage markets. Not surprisingly, norms defined at a smaller geographical scale have greater variation in pre-policy norms, and a smoother density curve. The national average pre-policy early marriage norm is similar across the various definitions of marriage markets. Finally, the DHS does not include district level information. Using the IHDS, I confirm the results are similar if marriage markets are defined at the district, religion and caste level.

[5]IHDS 2005.

[6]IHDS 2005. The survey question asks the amount of time it took the woman to travel to her natal home upon marriage. The mode of travel or distance is not specified. For women residing in urban regions, the average travel time is 4.2 hours. The corresponding number for rural women is 3.1 hours.

Figure 1. Kernel Density of Norms Across Markets



Note: The violin plots display the kernel density of pre-policy age at marriage norms for three different definitions of marriage markets. A norm of 0 means all women marry at or after 18, and a norm of 1 means all women in the market marry before age 18. Each violin plot also includes a box plot displaying the mean, inter-quartile range and standard deviation of pre-policy age at marriage norms.

Table 1. Characteristics of Marriage Markets

Dependent Variable	State	State/Religion/Caste	Village
Respondent Education	-7.00^{***}	-5.72^{***}	-5.863^{***}
	(1.465)	(0.998)	(0.296)
Partner Education	-3.56^{***}	-3.86^{***}	-4.13^{***}
	(1.01)	(0.889)	(0.271)
Number of Children	2.66^{**}	2.91^{***}	1.69^{***}
	(0.891)	(0.518)	(0.089)
Currently Working = 1	0.265^{*}	0.285^{***}	0.219^{***}
	(0.145)	(0.094)	(0.023)
Survey FE	X	X	X
Cohort FE	X	X	X
Religion/Caste FE	X	X	X
Observations	77,601	74,081	28,846

Note: Each coefficient comes from a separate regression where I regress the intensity of early marriage on various dependent variables. Intensity is measured as the pre-policy probability of marriage under 18. In all specifications, standard errors are clustered at the marriage market level. *p<0.1; **p<0.05; ***p<0.01

To provide a better understanding of marriage market norms, I present correlates of early marriage norms with socio-economic covariates in Table 1. Each estimate is extracted from a regression of the dependent variable on early marriage intensity, estimated separately for marriage markets defined at the state, state-religion-caste, and village level. It is apparent that marriage markets with stronger early marriage norms are associated with significantly lower levels of female and male education, as well as higher fertility. Women in markets with stronger early marriage norms are also significantly more likely to participate in the labor force, particularly in the agricultural sector. These results are expected; arranged marriage customs incentivize marriage as a substitute for education, because dowry payments are often lower for young girls and marriage of daughters reduces the financial burden of low income families.

The results in Table 1 indicate that marriage markets with stronger early marriage norms have lower socio-economic development compared to markets with weaker early marriage traditions. Any pre-existing marriage market trends or social programs which differentially target high intensity early marriage regions can potentially bias the estimated effect of the child marriage ban. In additional specifications, I control for differential time trends, as well as potential competing mechanisms.

2.4. Data and Descriptives

2.4.1. Data. This study employs data from the Demographic and Health Survey (DHS) of 1998 and 2005. The DHS is a nationally representative survey of households across 26 states and union territories. All women in the household between the ages of 15 to 49, as well as any female visitors, are administered a separate questionnaire to obtain information on birth history,

fertility, and marriage.[7] I restrict my sample to women who have married only once, between the ages of 12 to 40.[8] Approximately 99.2 percent of women from the full sample marry within this age interval.

An advantage of using the DHS data is that the survey includes both year of marriage and the month and year of birth. I exploit this information to define a cohort as those women born in the same month and year combination. Thus, my estimation strategy relies upon comparing the effect of the policy across birth cohorts. I restrict my analysis to 10 years of treated and control cohorts so that the final sample consists of 78,718 women.

For complementary analysis, I access several other data sets. I employ the 1992 DHS survey to access information on legal age of marriage knowledge. I utilize the 2005 India Human Development Survey (IHDS) to obtain correlations of age of marriage with partner and marriage characteristics. To explore possible mechanisms, I obtain police and population statistics from the 1981 Indian Census.

The advantage of using survey data instead of government records is that self-reported data is less likely to misrepresent the practice of early marriage in India. As shown by Blank, Charles, and Sallee (2009), individuals have an incentive to misrepresent their age in administrative records. Political pressure can also incentivize local governments to underestimate child marriage in official records.

[7]For the DHS 1998 survey, only ever-married women are selected for the interview.

[8]In certain cases, the marriage ceremony of a young bride may be performed but cohabitation may take place several years afterward. The DHS does not classify these women as married until the *gauna* ceremony, a ceremony associated with the consummation of marriage, has been performed. Thus, these women will be classified as "unmarried" in my sample. This should not affect my results because only approximately 1 percent of the full sample includes girls who are married but gauna has not been performed.

There are three potential limitations with using retrospective birth and marriage data which must be addressed. First, although the DHS interviewers probe respondents when recording birth and marriage dates, there is still a likelihood of misreporting or recall bias. As long as the misreporting is approximately random, this should not affect the interpretation of my results. Second, there exists a tendency for women to report the same month of birth as the month of marriage, which may be a result of systematic misreporting or a predisposition to marry close to their month of birth (Collin and Talbot 2017). I explore the possibility of correlation between the month of birth and month of marriage using the DHS 2005 survey, and do not find evidence for systematic misreporting of this kind in my data. Third, it is possible that respondents use the existing legal age of marriage as an "anchor" for reported age of marriage. Using the 1992 DHS survey, I find that the correlation coefficient between the respondent's reported age of marriage and belief of legal age of marriage is 0.169 for women and 0.09 for men. Figure A.7 in the appendix suggests correlation between own age of marriage and knowledge of the legal age of marriage is only weakly positive.

The main outcome of interest in this research is a woman's probability of marrying early. I measure underage marriage as an indicator variable equal to 1 if a woman is married before 18, and 0 otherwise. Separately, I also test for the effects of the policy on the likelihood of marrying under 15, measured by an indicator variable equal to 1 if a woman is married before 15, and 0 otherwise. An alternative would be to consider age of marriage instead of the probability of underage marriage. However, age of marriage averages capture changes in marriage

at different age groups and do not allow me to separate the age margins that should be most affected by the policy.

2.4.2. Descriptive Statistics. Table 2 provides summary statistics for the full sample employed in the study. The table highlights the high levels of underage marriages in the sample; 18 percent of women married between the ages 12 and 15 while 52 percent of women married before 18. While the average age of marriage for a woman is 17.97, there is substantial variation in this number. This is apparent in Figure 2, which depicts the age of marriage distribution for the sample.[9] The fact that there is a positive density for age at marriage earlier than 15 indicates that the previous minimum legal age of marriage of 15 years was not strictly enforced. Figure A.9 in the appendix depicts the pre-policy geographical variation in early marriage across states. As expected, the level difference shows the prevalence of marriage before 18 is higher than the probability of marriage under 15.

Finally, Table 2 reports that approximately 78 percent of the sample includes Hindu women. The second most common religious denomination is Muslims. Women in the sample have approximately 4 years of education and 3.52 births. Partners have 6.78 years of education and tend to be about 6 years older than their wives.

In Table 3, I regress several marriage market characteristics on age of marriage using IHDS data for women who married between the ages 12 and 40. The results suggest that women who delay marriage by one year are associated with a 1 percent greater probability of knowing their partner, on average. As shown in column (3) of the table, this result is not explained by an increase in

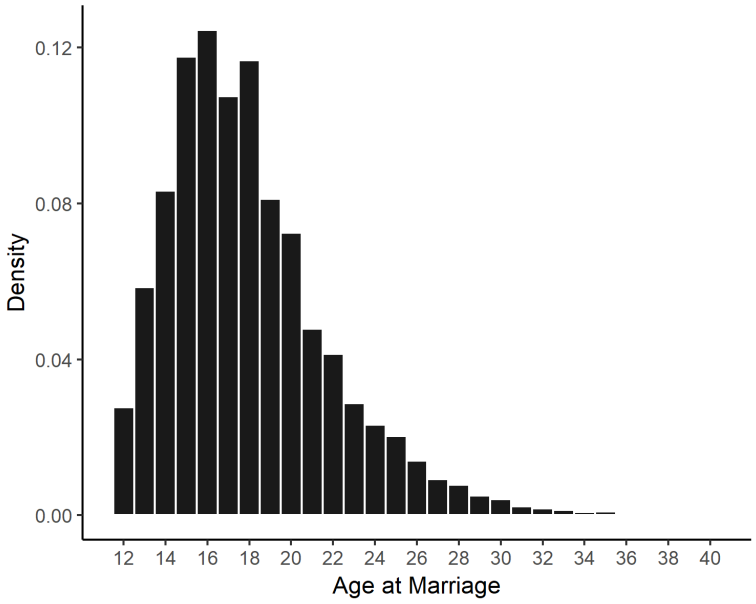
[9]The distribution of age at marriage by urban and rural region of residence in childhood can be found in Figure A.8 in the appendix.

Table 2. Summary Statistics For Full Sample

Statistic	Mean	St. Dev.	Min	Max	N
Woman Characteristics					
Current age	36.24	6.46	25	49	78,718
Age at marriage	18.03	3.89	12	40	78,718
Married Before 15 = 1	0.16	0.37	0	1	78,718
Married Before 18 = 1	0.51	0.50	0	1	78,718
Education in years	4.47	5.09	0	23	78,718
Hindu = 1	0.78	0.42	0	1	78,718
Childhood residence (Urban = 1)	0.31	0.46	0	1	78,718
Current residence (Urban = 1)	0.40	0.49	0	1	78,718
Number of children	3.55	1.99	0	16	78,718
Currently working = 1	0.37	0.48	0	1	78,713
Partner Characteristics					
Partner's age	42.05	7.97	19	95	78,718
Partner's education in years	6.91	5.26	0	30	78,718

Note: The table presents summary statistics for selected woman and partner characteristics. The sample includes women who married once between the ages of 12 to 40. Data is accessed from the 1998 and 2005 Demographic and Health Survey (DHS).

Figure 2. Age at Marriage Distribution



Note: The figure depicts the age of marriage distribution for the sample of women who married once, between the ages of 12 to 40. Data is accessed from the 1998 and 2005 DHS surveys.

the likelihood of marrying a blood relative. A delay in marriage by one year is also associated with a 1.8 percent increase in the probability that a woman has atleast some say in choosing her partner. Finally, delayed marriage is associated with a significantly shorter migration upon marriage. A shorter distance to their natal home allows young women to maintain an emotional and material support system after marriage, obtain health care after childbirth, and deter their husband or in-laws from mistreating them (Bloom, Wypij, and Gupta 2001). Taken together, these results highlight a positive correlation between a higher age of marriage and greater agency in marriage market outcomes.

Table 3. Age of Marriage and Marital Market Correlations (IHDS 2005)

	Marriage Characteristics			
	Previously Known	Say in Decision	Blood Relation	Distance: Natal Home
	(1)	(2)	(3)	(4)
Age of marriage	0.010***	0.018***	−0.003	−0.054*
	(0.002)	(0.003)	(0.002)	(0.032)
Education (in years)	0.010***	0.022***	−0.005***	0.065***
	(0.002)	(0.002)	(0.001)	(0.018)
Mean Dependent Variable	0.410	0.518	0.141	3.60
District FE	*X*	*X*	*X*	*X*
Religion/Caste FE	*X*	*X*	*X*	*X*
Year of Birth FE	*X*	*X*	*X*	*X*
Observations	7,621	5,312	5,789	7,632
R^2	0.179	0.276	0.135	0.086
Adjusted R^2	0.140	0.225	0.081	0.042

Note: Data is accessed from the 2005 IHDS survey. The sample includes women who married once, between the ages 12 to 40. *Previously Known* and *Say in Decision* are indicator variables equal to 1 if a respondent knew her husband for longer than a day prior to marriage, and if she had some say in choosing her husband, respectively. *Blood Relation* is an indicator variable equal to 1 if the respondent is related to her husband by blood. *Distance to Natal Home* describes the number of hours it took her respondent to visit her natal home from her marital home. *Age at marriage* and *Years of Education* are the respondent's age at marriage and education (in years), respectively. Standard errors are clustered at the district level. *p<0.1; **p<0.05; ***p<0.01

2.5. Results

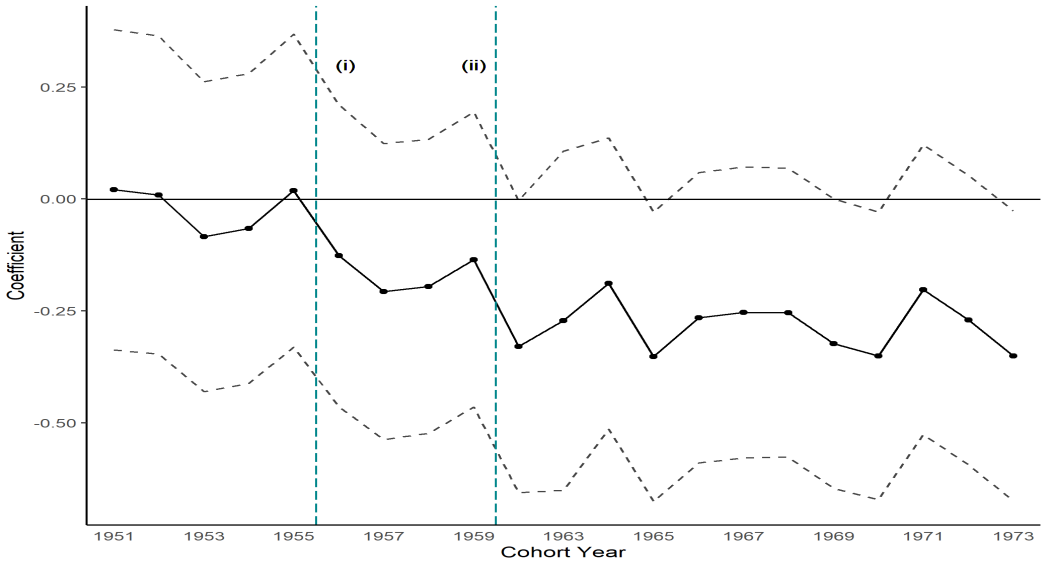
2.5.1. Cohort Analysis. Before presenting the main results, I estimate equation (2) to visualize the cohort level effects of the policy change, and to test my identification assumption that there should not be a systematic difference in the probability of early marriage across marriage markets, for women not exposed to the policy. I plot the coefficient estimates β_c for *birth year dummies* $\times$ *intensity* in Figure 3, with marriage markets defined at both the state, and state-religion-caste level, respectively. Women born in 1950 form the reference group and this cohort dummy is omitted from the regression. Thus, each estimated coefficient is interpreted as the cohort specific impact of the early marriage ban.

As expected, the estimates fluctuate around 0 for women born before 1956; the policy did not affect women who were not exposed to it. The decrease in the coefficient estimates begins for cohorts born in 1957, eventually stabilizing for women born after 1960. The earlier break from the trend is not surprising as it coincides with the Ministry of Health's announcement in 1974 and the policy introduction in Congress in 1976. The figure indicates the policy began to have an effect at announcement, implying that my results will be underestimates. The cohort specific results also provide visual evidence that the identification assumption is reasonable, and the policy had a permanent effect on the probability of early marriage for treated women.

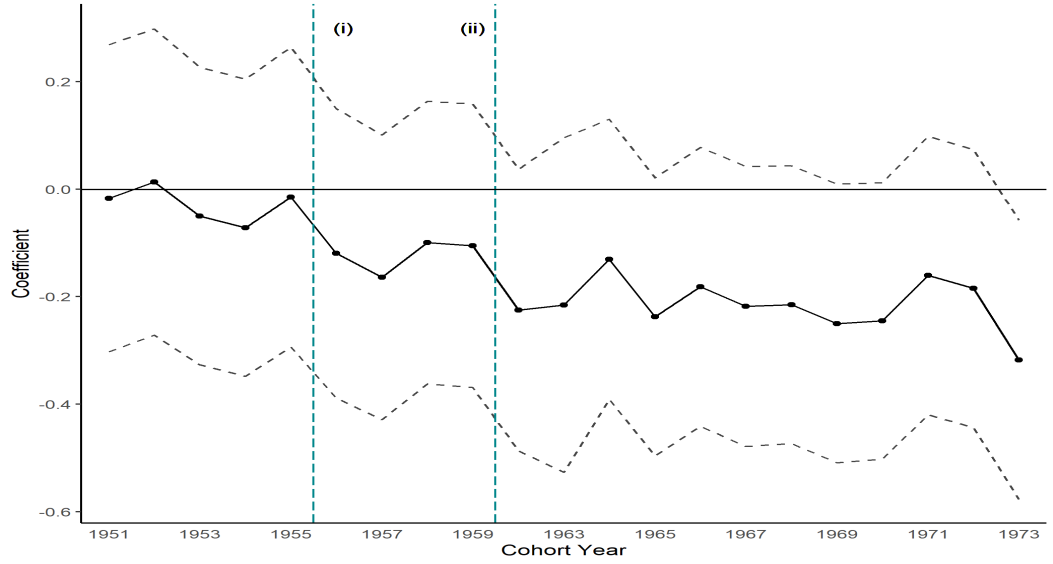
2.5.2. Policy Effect for Women. Table 4 presents results from equation (1). I separately analyze the effects of exposure to the policy on the probability of marriage under 15 and 18 years. Specifications include controls for religious membership, state of residence, and caste. In columns (3) and (4), I interact these controls with a *Treated* dummy variable to allow their impact

Figure 3. Results: Cohort Specific Policy Impact

(a) Marriage Market: State



(b) Marriage Market: State/Religion/Caste



Note: The figure plots the interaction coefficients of a cohort dummy (or year of birth) and the marriage market pre-policy probability of teen marriage. In the **top panel**, a marriage market is defined at the state level and in the **bottom panel**, a marriage market is defined using a combination of state, religion and caste. The dotted line (i) indicates the announcement by the Ministry of Health, while the dotted line (ii) indicates the passage of the policy. The three year gap between 1960 and 1963 on the x-axis indicates the dropped cohorts aged between 15-18 at policy passage. 95% confidence intervals are displayed.

to vary across pre and post-policy cohorts. As a robustness check, column (2) includes controls for state specific time trends to rule out the influence of pre-existing geographical trends that might confound the estimates. Standard errors are clustered at the marriage market level in all specifications.

Panel A shows results for the effect of the policy on the probability that a woman is married before attaining the age of 18. Across all specifications, the estimated coefficients are negative and significant. In column (1), the results suggest that women in marriage markets with high intensity child marriage norms were 11.3 percentage points less likely to marry under 18 after the policy, relative to women in marriage markets with low intensity child marriage norms. This pattern is robust to the inclusion of *state* $\times$ *treated*, *religion* $\times$ *treated*, and *caste* $\times$ *treated* fixed effects, as well as the demanding state specific time trends.

The results in Panel B indicate that a portion of the decrease in teen marriage is explained by a decline in the likelihood of marrying under 15. Column (1) suggests that women exposed to high intensity early marriage norms are 7.4 percentage points less likely to marry under 15 after the ban, compared to women exposed to low intensity early marriage norms. However, these results are not robust to the inclusion of state specific time trends or *state* $\times$ *treated* fixed effects, as is apparent from columns (3) to (4) in Panel B, although the point estimate in column (4) is almost identical to the coefficient in my preferred specification in column (1).

There are several ways to interpret the magnitude of the estimates in Table 4. At the average early marriage norm intensity, 0.69, the effect of treatment is a 7.8 and 5.1 percentage point decrease in the likelihood of marriage under 18 and 15, respectively. The results can also be interpreted in terms of percentile

Table 4. Effect of Policy on Early Marriage

	(1)	(2)	(3)	(4)
Panel A: Dependent Variable	*1(= Marriage Before 18)*			
Treated*Intensity	−0.113***	−0.052*	−0.112**	−0.351***
	(0.031)	(0.029)	(0.044)	(0.056)
Mean Dependent Variable	0.52	0.52	0.52	0.52
Observations	74,215	74,215	74,215	74,215
R^2	0.162	0.163	0.163	0.164
Adjusted R^2	0.158	0.159	0.159	0.159
Panel B: Dependent Variable	*1(= Marriage Before 15)*			
Treated*Intensity	−0.074***	0.0002	−0.035	−0.069
	(0.019)	(0.019)	(0.028)	(0.052)
Mean Dependent Variable	0.17	0.17	0.17	0.17
Observations	74,215	74,215	74,215	74,215
R^2	0.085	0.087	0.086	0.086
Adjusted R^2	0.081	0.082	0.081	0.081
Cohort FE	X	X	X	X
Marriage Market FE	X	X	X	X
Religion/Caste FE	X	X	X	X
State Specific Time Trend		X		
State*Treated			X	X
Religion*Treated				X
Caste*Treated				X

Notes: The sample includes women who marry once between the ages 12 to 40. The outcome variables are the probability of marrying under 18 and 15, respectively. *Treated* is a dummy equal to 1 if a woman was less than 15, and 0 if she was greater than 18 in October 1978. *Intensity* is measured as the pre-policy average probability of marrying under 18 in a marriage market. The average Intensity is 0.69. Marriage markets are defined as a combination of state, religion, and caste; there are 130 unique markets. All regressions include survey fixed effects. Standard errors are clustered at the marriage market level, and results are robust to clustering at the cohort level. *p<0.1; **p<0.05; ***p<0.01

changes. A move from the 10th to the 90th percentile of social norm intensity after the policy is associated with a 5.1 and a 3.3 percentage point decrease in the likelihood of marrying before 18 and 15 respectively. In column (4) of panel (A), the point estimate increases with the inclusion of *state* × *treated*, *religion* ×

treated, and *caste* × *treated* controls; a move from the 10th to the 90th percentile of early marriage intensity translates to a 15.7 percentage point decrease in the probability of marrying under 18 for women exposed to the ban.

Next, I rerun equation (1) by separate age of marriage groups. Assuming that the probability of entering into marriage is not different for treated women across low and high intensity marriage markets after the policy, if women are less likely to be married before 18, then we should expect to see an increase in the probability of marrying at more mature age groups. In India, approximately 98.1 percent of women above the age of 30 have married atleast once, suggesting it is unlikely that the policy would incentivize women to remain unmarried. Nonetheless, in Figure A.10 in the appendix, I plot *Year of Birth*Intensity* coefficients from a regression with the probability of marrying as the outcome variable. I do not find a significant change across marriage markets in the probability of entering marriage for cohorts exposed to the policy, suggesting the assumption is valid.

The results for other age groups are presented in Table 5. The outcome variables are indicators equal to 1 if a woman marries at the ages 18-20, 21-23, and 24-26, and 0 otherwise. Column (1) suggests the new legal age of marriage significantly increased the probability of marriage at the ages 18-20 by 7.4 percentage points at the average norm intensity, for treated women in high intensity marriage markets relative to low intensity markets. These results are robust to the inclusion of state specific time trends. I do not find significant changes in the likelihood of marriage at the ages 21-26. Taken together, the results indicate that the policy successfully caused a shift away from teen marriage towards marriage at the ages 18-20, on average.

An important caveat applies to these interpretations. For the main specifications in Table 4, the probability of marriage under 15 and 18 are predetermined variables. That is, the choice of marrying under 15 or 18 is made by the control group before the policy change. Thus, the behavior of treated women in response to the policy should not affect the decisions of the control group. However, the farther the age group from 18, the greater the likelihood of treatment group responses affecting the outcome of the control group through equilibrium changes, and the greater the likelihood of SUTVA (stable unit treatment value assumption) violations. For example, if treated women delay marriage to the ages 18-20 on average, women who are in this age group might be forced to delay marriage or marry at the same age but to a lower quality partner. This potential

Table 5. Effect of Policy on Marriage at Different Age Groups

	1(= Marriage at 18/19/20)		1(= Marriage at 21/22/23)		1(= Marriage at 24/25/26)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Intensity	0.107***	0.064*	0.034	0.039	−0.016	−0.026
	(0.027)	(0.036)	(0.022)	(0.035)	(0.017)	(0.018)
Cohort FE	X	X	X	X	X	X
Marriage Market FE	X	X	X	X	X	X
Religion/Caste FE	X	X	X	X	X	X
State Time Trend		X		X		X
Mean Dependent Variable	0.27	0.27	0.12	0.12	0.06	0.06
Observations	74,215	74,215	74,215	74,215	74,215	74,215
R^2	0.028	0.029	0.057	0.058	0.055	0.056
Adjusted R^2	0.023	0.024	0.053	0.053	0.050	0.051

Note: The dependent variables are binary variables equal to 1 if a woman married at the specified ages, and 0 otherwise. *Treated* is a dummy equal to 1 if a woman was less than 15, and 0 if she was greater than 18 in October 1978. *Intensity* is measured as the pre-policy average probability of marrying under 18 in a marriage market. Marriage markets are defined as a combination of state, religion, and caste. Standard errors are clustered at the marriage market level, and results are robust to clustering at the cohort level. All regressions include survey fixed effects. *p<0.1; **p<0.05; ***p<0.01

bias at higher age groups is difficult to sign, and depends on the behavior of the control group to marry early or at a mature age, in response to the choice by treated women.

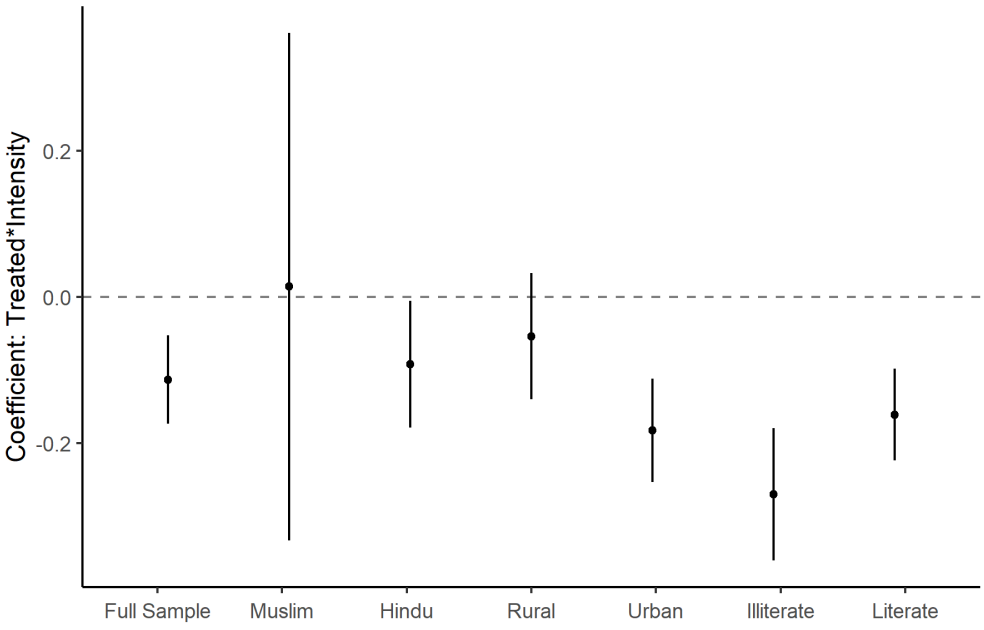
Finally, to determine which subgroups are driving the results, I conduct heterogeneity analysis by literacy, religion, and region of residence. Figure 4 displays coefficients and 95 percent confidence intervals for the *Treated*Intensity* variables in the separate sub sample regressions. I categorize women as illiterate if they have zero years of schooling, and literate if they have atleast one year of schooling. Marriage and education are substitutes, so I cannot use the years of education of a woman to distinguish the effect of the policy. However, the youngest woman in my sample would be 5 years old when the ban was enforced, and the decision to obtain atleast a year of schooling should have already been determined prior to policy enforcement. Figure 4 shows the main results are driven by both literate and illiterate women, although the magnitude of the effect is larger for women who have zero years of education. To understand the relative magnitude of the effects, a move from the 10th to the 90th percentile of early marriage norm intensity is associated with a 7.7 and 9.4 percentage point decrease in the likelihood of marriage under 18 for literate and illiterate women after the policy, respectively.

Interestingly, the results in Figure 4 also indicate that women from urban areas were more likely to respond to the policy. Point estimates for rural women are small and insignificant. These differences are potentially driven by variation in awareness of the policy. According to the 1992 Demographic and Health Survey (DHS), approximately 70 percent of respondents residing in urban areas had correct knowledge of the legal age of marriage for women. In contrast, only 46

percent of women residing in rural areas correctly stated the legal age of marriage for women. Although the survey was conducted several years after enactment of the law, these differences provide suggestive evidence that the results are driven by those groups of women who were more aware of the early marriage ban. Finally, I

Figure 4. Heterogeneity: Religion, Literacy, Region of Birth

(Probability of Marrying Under 18)



Note: The figure presents coefficients and standard errors from several subgroup analysis for the *Treated*Intensity* variable, where the outcome is a binary variable equal to 1 if a woman is married before the age 18. *Treated* is a dummy equal to 1 if a woman was less than 15, and 0 if she was greater than 18 in October 1978. *Intensity* the pre-policy probability of marrying under 18 for a marriage market, where marriage markets are defined using a combination of state, religion and caste. Illiterate refers to women who have zero years of schooling. Literate women have atleast one year of schooling. All regressions include survey, cohort, marriage market, and caste fixed effects. 95% confidence intervals are displayed.

find that the results are driven by Hindu women. Coefficient estimates for Muslim women are insignificant, possibly because of a much smaller sample size.

2.6. Discussion

2.6.1. Marriage Market Effects. The early marriage results for women are best understood in the context of broader changes at the marriage market level. The 1978 CMRA also increased the legal age of marriage for men from 18 to 21. Thus, it is crucial to understand the overall effect of the policy on the behavior of both men and women to account for any feedback effects while interpreting the results.

Using the 2005 DHS survey, I employ a similar methodology to test for possible policy impacts on the probability of underage marriage for men. The sample is restricted to men who married once, between the ages 12 to 60. Men older than 21 when the policy was enacted form the control group, while men younger than 18 form the treatment group.

The results are reported in columns (1) and (2) of Table 6.[10] The policy did not have a significant impact on the probability of marriage under 18 or 21 for men. I offer two explanations for these results. First, it is possible that because the focus of the legislation was on reducing underage marriage for girls, and thereby reducing population growth in the country, the increase in the legal age of marriage for men went relatively unnoticed. According to the 1992 DHS survey, only 33.6 percent of respondents correctly stated 21 as the legal age of marriage for men. This low number suggests that measures to increase the legal age for men were most likely not salient enough to affect behavior. Second, social norms are not expected to have symmetrical and significant effects on the age of marriage

[10]The pre-policy average age of marriage for men in India was approximately 24.35.

decisions for both men and women. For example, Maertens (2013) points out that norms regarding the ideal age of marriage in India are binding for girls but not for boys. A policy which changes behavior by affecting perceptions of the norm, but with minimal enforcement, should then not significantly affect the age of marriage choices of men.

The early marriage ban, by changing the age of marriage for women, had the potential to also affect *whom* the women married, on average. The IHDS survey includes numerous questions about the marital history of women. I take advantage of this information to test whether the policy changed marriage patterns other than the likelihood of early marriage. The results are presented in columns (3) to (6) of Table 6. I do not find significant differences in the probability of marrying a partner known for less time or who lives further away from a woman's natal family, indicating a potential expansion in the search for marital partners. I also do not find significant changes for treated women having more autonomy in whom to marry or marrying someone completely unknown to them. Based on these set of indicators, it does not seem as though the policy is associated with treated women in marriage markets with strong child marriage norms marrying men they would not have married otherwise, relative to women in markets with weaker child marriage norms.

2.6.2. Secondary Outcomes. Since the policy change led to a decrease in the early marriage of women, I should also expect to see changes in the probability of early cohabitation, as marriage in India is closely followed by cohabitation.[11] I test whether the likelihood of cohabitation or *gauna* before

[11]In my data, I do not see any women who cohabit with their husband prior to marriage. In fact, most women cohabit at the same age as marriage; only approximately 6 percent cohabit more than a year after marriage.

Table 6. Policy Correlations with Match Search

	Men: DHS (2005)		Women: IHDS (2005)			
	1(=Marriage Before 18)	1(=Marriage Before 21)	1(=Husband Unknown)	1(=Say in Choosing Husband)	1(=Husband From Village)	Distance from Natal Home
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Intensity	0.012	−0.084	0.051	−0.033	0.061	−0.255
	(0.031)	(0.085)	(0.038)	(0.056)	(0.038)	(0.447)
Religion/Caste Fixed Effects	X	X	X	X	X	X
Marriage Market Fixed Effects	X	X	X	X	X	X
Cohort Fixed Effects	X	X	X	X	X	X
Mean Dependent Variable	0.09	0.30	0.41	0.46	0.16	3.65
Observations	13,440	13,440	5,079	3,325	5,075	5,024
R^2	0.044	0.098	0.447	0.542	0.224	0.221
Adjusted R^2	0.041	0.096	0.368	0.440	0.113	0.108

Note: In columns (1) and (2), data is accessed from the DHS 2005 survey and marriage markets are defined at the state-religion-caste level. In columns (3)-(6), data is accessed from the IHDS 2005 survey, and marriage markets are defined at the district-religion-level. In columns (1) and (2), *Treated* is a dummy equal to 1 if a man was younger than 18 and 0 if a man was older than 21 when the legal age of marriage was increased; *Intensity* is the pre-policy probability of marrying under 21 in a marriage market. In columns (3)-(6), *Treated* is a dummy equal to 1 if a woman was younger than 15 and 0 if she was older than 18 when the legal age of marriage was increased; *Intensity* is the pre-policy probability of marrying under 18 in a marriage market. Standard errors are clustered at the marriage market level in all regressions. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

the age 15 and 18 is affected by the policy. In columns (1) and (2) of Table 7, I show that in fact, the probability of cohabitation before 15 and 18 follows similar patterns as that of marriage. The results are robust to the inclusion of state specific time trends.

In column (3) of Table 7, I find significant evidence for a decrease in average age gaps between partners. This is not surprising; if women are induced to delay marriage but men are not likely to change their behavior in response to the policy, age gaps should decrease. On average, the age gap between partners decreases by 7.7 months in high intensity regions relative to low intensity marriage markets. Further analysis also suggests a lower likelihood of giving birth under the age of 18 for women who delay marriage, although the point estimate is insignificant.

Next, I consider the differential impact of the policy on fertility and female empowerment. The results in column (5) suggest that treated women in high intensity marriage markets are associated with 0.4 fewer children on average, relative to low intensity regions. The average fertility in the sample is 3.51; the coefficient translates to an 11 percent drop in fertility in the highest intensity regions from the mean.

Columns (6) and (7) display results for the policy impact on two measures of female empowerment within a marriage. In column (6), the outcome variable is an indicator equal to 1 if a woman has some say on how to spend her own income. In column (7), the dependent variable is a normalized index of separate measures of decision making power over obtaining health care, making large household purchases, and visiting natal family. Higher values represent greater decision making power. Note that the two variables measure *control*, as opposed to

Table 7. Effect of Policy on Secondary Outcomes

	IHDS		DHS				
	1(=Cohabitation Before 15)	1(=Cohabitation Before 18)	Age Gap With Partner	1(=First Birth Before 18)	Fertility	1(=Say Over Own Income)	Decision-Making (Index)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated*Intensity	−0.075***	−0.412**	−0.642**	−0.010	−0.403*	0.059**	0.110
	(0.025)	(0.042)	(0.308)	(0.035)	(0.209)	(0.028)	(0.071)
Religion/Caste FE	X	X	X	X	X	X	X
Cohort FE	X	X	X	X	X	X	X
Marriage Market FE	X	X	X	X	X	X	X
Mean Dependent Variable	0.07	0.36	6.05	0.33	3.51	0.79	0
Observations	5,089	5,089	71,928	19,384	20,071	20,071	20,071
R^2	0.203	0.334	0.075	0.128	0.269	0.114	0.145
Adjusted R^2	0.088	0.238	0.071	0.111	0.255	0.097	0.128

Note: *Intensity* is measured as the pre-policy probability of marrying under 18 in a marriage market. The dependent variable *1(=Some Say Over Own Income)* is equal to 1 if a woman has some say over the use of her own income. The dependent variable *Decision-making power* is equal to an average of individual z-scores calculated using the mean and standard deviations of separate variables capturing a respondent's decision making power in accessing health care, making large household purchases, or visiting natal family. Higher values indicate greater decision making power.

access to resources. Control presupposes access to resources. The results suggest the policy change was associated with a 5.9 percent increase in control over own income in marriage markets where women delayed marriage. However, there is no significant change in the decision making power index.

2.6.3. Mechanisms. I argue that because evidence for enforcement of the ban is minimal, the policy likely impacted the probability of early marriage through a high perception of enforcement and awareness of the policy change. Unfortunately, I cannot directly test for this mechanism. Instead, I rule out potential competing explanations for drivers of the results.

In Table 8, I test for possible mechanisms that may explain the results presented in the paper. I start by ruling out the possibility that the results are driven by variation in policy enforcement. In columns (1) and (5), I include the proportion of seats won by the Janata party in the 1977 General Election, interacted with treatment (*Treated*Janata Seats*). The variable captures any differences in political leadership which might have affected enforcement or advertisement of the ban. In columns (2) and (6), I include the number of police officers in 1978 by state, weighted by population, and interacted with treatment (*Treated*Police*). The presence of police officers should capture the capacity for policy enforcement across regions. Throughout the various specifications, the presence of police and the region's political alignment does not significantly affect the likelihood of treated women marrying before 15 or 18. Moreover, the coefficients for the variable of interest *Treated*Intensity* are stable, suggesting that enforcement variation does not explain the estimated response to the policy.

In columns (3) and (7) of Table 8, I include the pre-policy average years of education obtained by women in a marriage market. Average education should

Table 8. Mechanisms: Policy Effect with Socio-Economic Controls

	1(=Marriage Before 15)				1(=Marriage Before 18)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated*Intensity	−0.078***	−0.074***	−0.124***	−0.149***	−0.116***	−0.106***	−0.240***	−0.246***
	(0.023)	(0.019)	(0.022)	(0.028)	(0.035)	(0.032)	(0.044)	(0.051)
Treated*Janata Seats	0.008			0.015	0.004			0.009
	(0.011)			(0.011)	(0.018)			(0.019)
Treated*Police		0.023		0.039**		0.016		0.033*
		(0.015)		(0.016)		(0.015)		(0.019)
Treated*Average Educ			−0.007***	−0.008***			−0.017***	−0.017***
			(0.002)	(0.002)			(0.005)	(0.005)
Cohort FE	X	X	X	X	X	X	X	X
Marriage Market FE	X	X	X	X	X	X	X	X
Religion/Caste FE	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X
Observations	74,081	71,624	74,081	71,624	74,081	71,624	74,081	71,624
R^2	0.085	0.086	0.085	0.086	0.162	0.164	0.162	0.165
Adjusted R^2	0.080	0.080	0.080	0.081	0.157	0.159	0.157	0.160

Notes: Marriage markets are defined using a combination of state of residence, religion, and caste. All regressions include survey fixed effects. Standard errors are clustered at the marriage market level. *p<0.1; **p<0.05; ***p<0.01

capture differences in socio-economic development as well as returns to marriage across marriage markets. The results are robust to the inclusion of these variables, suggesting that the early marriage estimates are not driven by differences in education. As indicated in columns (4) and (8), the coefficients are also robust to the simultaneous inclusion of all variables.

It is possible that the decrease in the probability of early marriage is caused not by the increase in the minimum legal age of marriage, but by some other policy change. The late 1970s in India were a period of drastic reforms and policy changes, including the 1976-1977 Emergency Rule and the 1977 General Election. A possible confounding factor is the forced male sterilization campaign in the country which may have caused changes in the marriage market and affected women's age of marriage. I plot the density of male and female sterilizations in my sample, by year, and include the results in Figure A.11 in the appendix. At first glance, it seems there is an increase in the density of sterilizations for men in 1976, the beginning of the Emergency period. However, only about 2 percent of the men in my sample are sterilized so the changes in density are driven by very small

differences in observations. To test whether the sterilization campaign is driving the estimated results, I control for whether a woman or her partner are sterilized in my main regression, and find that the results are robust to the inclusion.

Finally, differential aggregate economic conditions, such as droughts, could affect the timing of child marriage in India (Corno, Hildebrandt, and Voena 2020). Specifically, households facing negative agriculture income shocks have an incentive to delay the early marriage of their daughters because dowry payments become unaffordable. It is possible that households in regions facing negative shocks in 1978 are more likely to respond to the policy by delaying marriage, especially if their local early marriage norms are already relatively weak. I explore this potential explanation next.

Following Corno, Hildebrandt, and Voena (2020), I construct a measure *Drought* equal to 1 for districts receiving yearly rainfall in 1978 below the 15th percentile long run rainfall in that district. Data is accessed from the University of Delaware Terrestrial Precipitation V 3.01. Long run rainfall is measured for the years 1930 to 1975. Separately for marriage markets measured at the district, and district/religion/caste level, I explore whether district level droughts differentially affect the early marriage decision of treated women. Table 9 summarizes the results. For all specifications, the decision of marrying under 15 for treated women is unaffected by agriculture income shocks.

I obtain mixed results for the probability of marriage under 18 for marriage markets defined at the district level. In columns (6) - (8) the results indicate treated women are significantly less likely to marry under 18 if they are exposed to a drought in their district in 1978. However, the response to the policy is not muted or aggravated by the drought. This is clear from the insignificant triple

Table 9. Effect of Policy on Early Marriage by Income Shocks (IHDS 2005)

	District				District/Religion/Caste			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent Variable				**1(= Marriage Before 18)**				
Treated*Intensity	−0.336***	−0.359***	−0.361***	−0.358***	−0.460***	−0.499***	−0.491***	−0.498***
	(0.042)	(0.048)	(0.045)	(0.047)	(0.041)	(0.043)	(0.042)	(0.045)
Treated*Drought(1978)		−0.035	−0.077*	−0.021		−0.233***	−0.274***	−0.222***
		(0.049)	(0.045)	(0.089)		(0.063)	(0.069)	(0.081)
Treated*Drought(1977)			0.153***				0.103	
			(0.058)				(0.074)	
Treated*Drought(1976)			0.141***				0.096*	
			(0.049)				(0.053)	
Treated*Intensity*Drought(1978)				−0.029				−0.03
				(0.212)				(0.142)
Panel B: Dependent Variable				**1(= Marriage Before 15)**				
Treated*Intensity	−0.087***	−0.101***	−0.100***	−0.102***	−0.106***	−0.139***	−0.142***	−0.143***
	(0.030)	(0.033)	(0.033)	(0.033)	(0.028)	(0.033)	(0.033)	(0.033)
Treated*Drought(1978)		0.021	0.014	0.007		0.031	0.036	−0.011
		(0.040)	(0.039)	(0.06)		(0.057)	(0.059)	(0.045)
Treated*Drought(1977)			−0.088				−0.106	
			(0.064)				(0.081)	
Treated*Drought(1976)			0.028				−0.003	
			(0.037)				(0.045)	
Treated*Intensity*Drought(1978)				0.028				0.114
				(0.165)				(0.147)
Cohort FE	X	X	X	X	X	X	X	X
Marriage Market FE	X	X	X	X	X	X	X	X
Religion/Caste FE	X	X	X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X
Observations	7,018	5,649	5,649	5,649	5,074	4,008	4,008	4,008
R^2	0.269	0.274	0.276	0.274	0.352	0.360	0.360	0.359
Adjusted R^2	0.214	0.209	0.210	0.209	0.254	0.247	0.247	0.246

Note: The dependent variable is a dummy equal to 1 if an individual is married before 18 and 15 years, respectively. *Intensity* captures the pre-policy early marriage norms in a marriage market. *Drought* is measured as a dummy equal to 1 if a district's yearly rainfall was below the long run (1930-1975) 15th percentile rainfall for that district. Standard errors are clustered at the marriage market level. *p<0.1; **p<0.05; ***p<0.01

difference coefficient *Treated*Intensity*Drought(1978)* in columns (4) and (8). In all specifications, my variable of interest *Treated*Intensity* remains significant and stable in magnitude. Taken together, the results suggest that the estimated response to the policy is not explained by broader agriculture economic shocks.

2.7. Robustness

In this section, I review various robustness checks.

As mentioned, there are several approaches to defining marriage markets in India. In the context of this paper, the choice between these different marriage

market definitions will affect the variation in my social norm intensity measure, as well as the extent to which differential adult sex ratios across markets could theoretically bias my estimates. While the DHS data used in my main analysis provides a much larger sample size per cohort and goes further back in time, I do not have information on a woman's district of birth or residence.

As a robustness, I use the IHDS 2005 data to redefine marriage markets at the district and district-religion-caste level and rerun my analysis.[12] I also replicate the main results at the state and village marriage market level using both the DHS and IHDS data.

Table 10 displays the results for the probability of marriage before 15 and 18. All specifications include controls for cohort, marriage market, religion, and caste fixed effects. Several interesting conclusions can be made from the table. First, the results suggest there exists a very similar pattern across specifications and the two data sets; women exposed to the ban in markets with strong early marriage norms were significantly less likely to marry under 18 and 15. For example, in column (5) norms are defined at the district-religion-caste level and the results suggest that a move from the 10th to the 90th percentile in social norm intensity is associated with a 46.1 and 10.7 percentage point decline in the likelihood of marriage before 18 and 15, respectively. Thus, the main results are robust to the use of other definitions of marriage markets.

Second, for both data sets, the coefficient sizes increase as the marriage market (and therefore, social norm) is defined at a smaller geographical zone. One explanation for this pattern could be that social norms measured at a narrower geographical scale are more accurate measures of the exposure of women to early

[12] A concern with using the IHDS is that I do not have the sample size to drop those marriage markets with fewer than 5 observations pre-policy.

marriage norms. However, specifications at the village level are also more likely to capture small sample bias and the effects of changing adult sex ratios which might be correlated with early marriage norms, biasing the estimates.

A potential confounding factor for identification would be if households chose their marriage market exposure as a result of the policy. I estimate social norms using geographical residence at the time of the survey, instead of residence before marriage. If households select into markets with similar early marriage norms, my estimates should remain unbiased. However, if households select into markets with different norms as a result of the policy change, then my norm *Intensity* variable becomes endogenous, potentially biasing the estimated coefficients. To understand the extent of the potential bias, I proxy for migration using the number of years a woman has stayed in her current place of residence. I separate the sample by women who have remained in the region after marriage (women who have not migrated to markets with potentially different norms) and women who have migrated since marriage. The results in Table 11 suggest that across specifications with varying marriage markets, the estimated effect of the policy on the probability of marriage before 18 is stable across the two samples. That is, the main results are not driven by women selecting into markets with different norms. In fact, households likely marry their daughters into families with similar early marriage norms and traditions.

To ensure the results are not driven by shocks in specific marriage markets, I drop each individual marriage market at a time and rerun my results for markets defined at the state and state/religion/caste level. Figure 11 in the appendix displays the distribution of coefficients estimated from the regressions, separately for state and state/religion/caste level. The estimates are stable and robust.

Table 10. Robustness: Marriage Markets Redefined (IHDS 2005 and DHS)

	(DHS)	(IHDS)					(DHS)
Marriage Market	State	State	State-Religion-Caste	District	District-Religion-Caste	Village	Village
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Dependent Variable				*1(= Marriage Before 18)*			
Treated*Intensity	−0.152***	−0.043	−0.176***	−0.336***	−0.461***	−0.567***	−0.290***
	(0.048)	(0.070)	(0.048)	(0.042)	(0.041)	(0.041)	(0.020)
Mean Dependent Variable	0.51	0.39	0.37	0.37	0.37	0.37	0.57
Observations	77,740	6,987	7,582	7,033	5,089	3,381	28,066
R^2	0.143	0.171	0.207	0.268	0.351	0.459	0.289
Adjusted R^2	0.140	0.133	0.163	0.216	0.258	0.305	0.241
Panel B: Dependent Variable				*1(= Marriage Before 15)*			
Treated*Intensity	−0.095***	−0.068	−0.028	−0.087***	−0.107***	−0.114***	−0.118***
	(0.031)	(0.103)	(0.023)	(0.030)	(0.027)	(0.029)	(0.017)
Mean Dependent Variable	0.16	0.09	0.08	0.08	0.08	0.08	0.19
Observations	77,740	6,987	7,582	7,033	5,089	3,381	28,066
R^2	0.074	0.090	0.108	0.157	0.245	0.336	0.223
Adjusted R^2	0.070	0.049	0.059	0.097	0.136	0.146	0.171
Marriage Markets	25	13	190	336	1266	1670	1504
Cohort FE	X	X	X	X	X	X	X
Marriage Market FE	X	X	X	X	X	X	X
Religion/Caste FE	X	X	X	X	X	X	X
Survey FE	X						X

Note: The sample includes women who marry once between the ages 12 to 40. The outcome variables are the probability of marrying under 18 and 15, respectively. *Treated* is a dummy equal to 1 if a woman was less than 15, and 0 if she was greater than 18 in October 1978. *Intensity* is measured as the pre-policy average probability of marrying under 18 in a marriage market. *p<0.1; **p<0.05; ***p<0.01

Table 11. Robustness: Migration Effects (DHS)

	1(= Marriage Before 18)					
	State		State-Religion-Caste		Village	
	Not Migrated	Migrated	Not Migrated	Migrated	Not Migrated	Migrated
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Intensity	−0.238***	−0.104**	−0.190***	−0.074**	−0.302***	−0.274***
	(0.068)	(0.047)	(0.047)	(0.032)	(0.044)	(0.023)
Cohort FE	X	X	X	X	X	X
Marriage Market FE	X	X	X	X	X	X
Mean Dependent Variable	0.49	0.52	0.50	0.53	0.57	0.57
Observations	26,121	51,591	24,669	49,522	8,383	19,680
R^2	0.143	0.148	0.165	0.167	0.383	0.319
Adjusted R^2	0.134	0.144	0.152	0.161	0.226	0.252

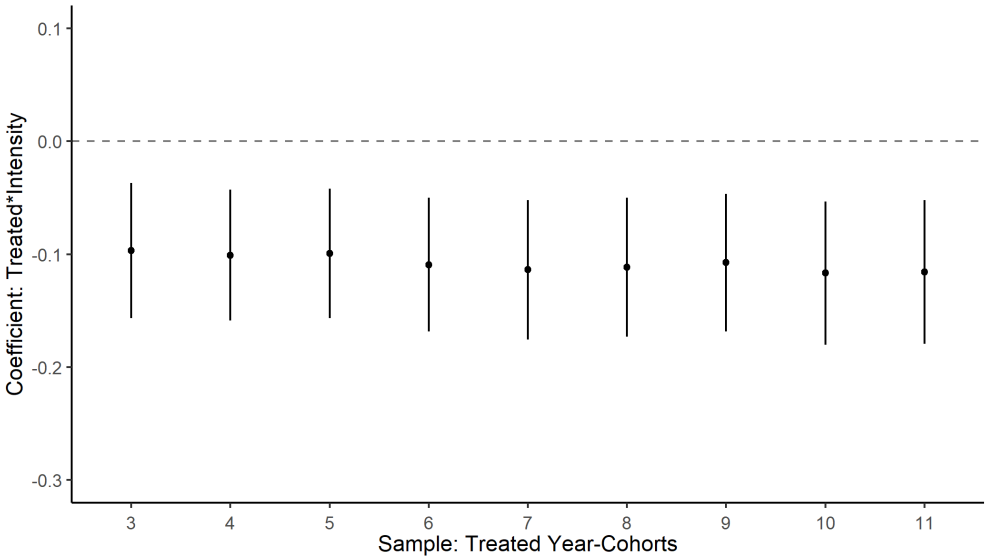
Note: The sample includes women who marry once between the ages 12 to 40. The sample used in columns (1), (3), and (5) consists of women who have not migrated (women who have lived in the place of residence since before marriage). Columns (2), (4), and (6) use samples of those women who have migrated (women who have lived in the place of residence less than the length of their marriage). The outcome variable is the probability of marrying under 18. *Treated* is a dummy equal to 1 if a woman was less than 15, and 0 if she was greater than 18 in October 1978. *Intensity* is measured as the pre-policy average probability of marrying under 18 in a marriage market. All regressions include survey, religion, and caste fixed effects. *p<0.1; **p<0.05; ***p<0.01

In Table A.29 in the appendix, I show that the main results are robust to the inclusion of state by year of birth sex ratios. In Table A.30, I rerun my analysis using hazard and logit models. For the hazard model specification in columns (1) and (2), the data is reconstructed to create a panel for each woman by age. In all of the specifications, the results indicate that women in the highest early marriage districts were at significantly lower risk to marry before age 15 and 18 after the policy.

Finally, my analysis restricts the data sample to a 20 year window around the policy change, with 10 years of treated and control cohorts. As an additional robustness test, I show that the results are robust to the use of different sample windows. In Figure 5, I plot the coefficients for *Treated*Intensity* from separate regressions which vary the sample time interval. The figure displays 95 percent

confidence intervals. As expected, I find similar results across regressions. Although the magnitude of the effect increases as the sample size decreases, the coefficient estimates are not significantly different across samples suggesting the results are not sensitive to the sample window used in the analysis.

Figure 5. Robustness: Varying Sample Time Intervals



Note: I rerun my analysis of the policy effect on the probability of marriage under 18. I plot the coefficients for *Treated* and *Treated*Intensity* for different sample windows around the enforcement of the ban, from 6 to 22 years. The paper uses a window of 20 years of cohorts around the policy (10 years of treated and 10 years of control cohorts). 95% confidence intervals are displayed.

2.8. Conclusion

In conclusion, this research studies the national CMRA amendment of 1978 which increased the legal age of marriage for women from 15 to 18 years. I find that the policy change led to a 7.8 percentage point decrease in the likelihood of marriage before 18 at the average norm intensity, with a 7.4 percentage point

corresponding increase in the likelihood of marriage at the ages 18 to 20. The effects are driven by both literate and illiterate women residing in urban areas. I find no effect of the policy on the likelihood of early marriage for men.

I argue that the policy significantly changed the behavior of treated women despite little evidence for enforcement. I provide evidence that differential sex ratios, enforcement capacities or political leadership do not explain the results. Finally, the estimates are robust to the inclusion of multiple fixed effects, state specific time trends, and varying definitions of marriage markets.

As India considers another increase in the legal age of marriage for women from 18 to 21 years, the importance of understanding the impact of previous changes in the legal minimum age of marriage is further underlined. The results offer interesting insights into the interplay between policy, norms, and harmful customs. The research provides suggestive evidence for the expressive and non-sanctionary function of the law, an often overlooked aspect of policy implementation and design. The results by no means suggest that the mere announcement of a policy change is always sufficient for affecting behavior. Instead, a high perception of enforcement cultivated through active governance and policy awareness can provide credibility to initial changes in legislation which remain unenforced.

CHAPTER III

IN THE NAME OF HONOR? EVALUATING THE IMPACT OF WEATHER VARIABILITY ON "HONOR" KILLINGS IN PAKISTAN

3.1. Introduction

An 'honor' killing is broadly defined as the murder of an individual for bringing shame or dishonor to a family or community. The act of punishing or murdering the individual who brings shame is then a tool to regain lost honor from the viewpoint of the village or community. Every year, approximately 5,000 women are murdered globally in the name of 'honor' (Mann 2004). Honor based violence is prevalent in South Asia, the Middle East, Africa, and parts of Europe (United Nations 2002).

While economists have studied the causes and consequences of various forms of violence against women in great detail, honor based crimes remain unexplored. However, the need to analyze these crimes separately is underlined by several prominent distinctions which can be drawn between honor killings and other forms of domestic violence. First, the victims of these crimes are generally young women. Second, perpetrators tend to be male members of her natal family. In my data, approximately 70 percent of victims are women, while approximately 43 percent of perpetrators are male members of her natal family. In contrast, the perpetrator in other forms of domestic violence against women tends to be a husband or intimate partner. Finally, perpetrators often act on behalf of the larger family or community. The community provides either explicit or implicit approval and rewards the perpetrator by restoring the honor lost (Terman 2010). In some

cases, honor killings are ordered by local *jirgas*.[1] This explains why perpetrators often display pride in their actions and honor based crimes are less likely to be hidden; honor crimes are not viewed as "shameful" (Laghari 2016).

This research aims to reduce the existing gap in the literature. First, I systematically collect a database of honor killings in Pakistan. I access 11 online and print newspapers to construct a data set spanning 41 months from April 2014 to August 2017, covering 94 districts (representing approximately 91 percent of the population). The data collection methodology is similar to much of the conflict literature which uses newspaper and media reports to build databases for conflict (ACLED 2017). Leaning on earlier anthropological literature, I discuss in detail the cultural and economic drivers of honor based crimes. Second, I use plausibly exogenous local rainfall variation to identify the impact of income shocks on honor killings in Pakistan. Using a Poisson model, I find that an increase in rainfall above the long run mean in the previous month results in a significant increase in honor killing incidents in cotton producing districts.

I argue that the results provide suggestive evidence for the income shock theory; rainfall shocks above the long run average reduce cotton production. I do not find evidence for rainfall shocks affecting other major crops grown in the country such as wheat, barley, rice, sugarcane, or tobacco. Cotton production contributes to approximately 2 percent of Pakistan's GDP; the country is the fourth largest producer of cotton globally (Shaw 1990).[2] I show that rainfall shocks occurring specifically in cotton harvesting months, are driving my results. Taken

[1]Local courts held by male members of tribes or communities to determine punishments and settle disputes.

[2]This number only includes raw cotton. The contribution of cotton products and textiles to Pakistan's GDP is much greater.

together, the results provide suggestive evidence for the role of economic shocks, and specifically agricultural income shocks, in explaining violence against women for 'honor'.

This research contributes to two main strands of literature. First, the study furthers our understanding of the social costs to women of weather variability in agriculturally dependent countries. This study is similar to previous research evaluating the effects of extreme weather on excess female mortality (Rose 1999; Sivadasan and Xu 2019), dowry related deaths (Sekhri and Storeygard 2014), and witch killings (Miguel 2005; Oster 2004). Sekhri and Storeygard (2014) suggest that dowry killings are driven by the consumption smoothing hypothesis; dowry deaths provide households with access to income during periods of poverty. In contrast, Miguel (2005) finds evidence that households near subsistence, when encountering negative income shocks, kill unproductive elderly women to protect other members from facing decreased consumption. My work complements that of Miguel (2005); I find suggestive evidence that periods of economic stress, brought on by shocks to income, play a role in increasing the murder of young women who may no longer be 'marriageable', in the name of honor.

More broadly, this research contributes to earlier work on poverty and crime. The literature includes research on the effect of weather or temperature variation on crime (Blakeslee and Fishman 2017; Wilson 2020), as well as the impact of income shocks on domestic violence or violent crime (Abiona, Foureaux Koppensteiner, and Koppensteiner 2016; Bignon, Caroli, and Galbiati 2017; Carr and Packham 2018). My research differs from earlier work as it provides the first insight into the role of economic shocks on honor killings, a distinct yet previously unexplored type of crime.

A limitation of the research is that I only observe reported incidents, which are dependent on both the occurrence of a crime and the reporting of the incident by a newspaper in my sample. Thus, I estimate the impact of shocks on observed crimes. However, I show that rainfall shocks above the long run average are not significantly associated with changes in the average number of times the same incident is reported across newspapers. I interpret this as suggestive evidence that shocks do not affect the likelihood of reporting honor killings and therefore, the estimated effect of rainfall shocks on increased honor killings are not biased. I discuss potential misreporting issues in detail after presenting the results.

Next, I provide a brief background on honor based killings. In Section 3.3., I summarize the data collection methodology. Section 3.4. briefly describes honor killings and Section 3.5. includes the empirical methodology and main results. I discuss the potential misreporting issues and mechanisms in Sections 3.6 and 3.7. Section 3.8. concludes.

3.2. Background on Honor Killings

There are several interpretations of honor as a social construct of local cultures, norms, or traditions. The most common interpretation is that of honor preserved by men through the sexual modesty of women. Men in traditionally tribal or patriarchal societies must uphold honor, viewed as reputation or moral standing, for themselves and their families by controlling or restricting the sexual freedoms of women (Awwad 2014). In this sense, honor is fluid; it can be lost but it can also be regained.

Honor based crimes thus arise as a tool to punish the transgressing individuals in order to regain honor. In fact, there is no requirement of proof that a 'shameful' act has occurred in order to justify an honor killing. The mere rumor,

gossip, or publicity of alleged sexual misconduct is enough to bring dishonor (Ruane 2000). In Pakistan's context, where the 'purity' of a woman is crucial to ensure a successful arranged marriage, sexual misconduct might include anything from coming home late, or having an affair, to marrying someone of her own choosing. Which actions constitute as behavior that is dishonorable will vary across cultures and societies.

Honor killing beliefs are strong in many parts of rural Pakistan. Laghari (2016) interviews perpetrators of honor killings in Sindh, Pakistan, quoting "Women are our honor and if somehow they are misguided, men shall teach them by force" (181). Perpetrators display pride in their actions, simultaneously recognizing the approval and protection provided to them by their community. Laghari (2016, 182) quotes a man who killed his cousin for honor:

> "After this honour killing ... you cannot imagine how people felt about me. They appreciated and praised me not only in my presence but also in my absence. And I feel that due to this honorable act, my respect and value has increased in the society. But... Oh God! If I had not killed their sister, believe me, the world would have appeared a kind of hell to me. Those who have not killed their family's women over such allegations are considered cowards and their value is less than a dog or a cat. The system, the culture, the tradition is so dangerous. You cannot imagine the insulting remarks people pass about those who don't kill. People laugh at them. They spit behind them. They are considered dishonored to the extent that people do not want to talk to them and cannot eat with them by saying that eating with these besmirched men is a curse. Their names become obscenities."

The quote highlights that men or families unwilling to murder individuals who have dishonored their household are often ostracized and abused by the community themselves. Those who do commit the crime often have society's sympathy. According to a 2014 poll conducted by Pew Research Center, only 46 percent of

the population agrees that honor killing is never a justified response to pre-marital affairs.

It is worth mentioning that the labeling of these specific crimes as honor based is not without its complexities and nuances. For example, earlier anthropological research has pointed out that the term "honor killing" is often reserved to describe crimes against women only within certain cultures and religions (Grewal 2013). However, similar crimes occurring in the US, for example, are labeled as domestic violence. The use of this terminology, and the labeling of these crimes as cultural or private in nature, also make local authorities and governments reluctant to charge perpetrators, potentially making it more difficult for victims to seek justice.

Moreover, the labeling of these crimes as honor based underlines the cultural driving factors, but leaves little room for alternative explanations and causes. In fact, there may be economic motivations behind honor killings (Lari 2011). These may include murdering a man or woman to remove them as a legal heir. If a woman marries by choice, her natal family can no longer marry her to an extended family member to keep any land in the family. To avoid future division of property, her natal family may kill both her and her husband. Furthermore, if a woman has an alleged affair (or there is a rumor of her involvement in an affair), she may no longer be marriageable; her family may murder her to avoid having to provide for her financial support, especially during periods of poverty. Lari (2011) also points out there is an increase in incidents during the crop ripening period when communities are affluent, arguing that this increased wealth creates incentives for exploitation and the murder of individuals under false honor based

pretexts. A female survivor, accused of dishonoring her family, explains how honor crimes can be economically motivated (Laghari 2016, 196):

> "My brother and brother-in-law had killed a young boy of about eighteen years of age. In fact, they had borrowed a handsome amount of money from him. On that day, my brother-in-law phoned the boy and told him to come and collect his money. When the boy arrived on his bike, they took him to their *otaque* and killed him. Then they came to kill me to give the impression that the killings were a karo-kari [honor killing]. God knows the truth; I do not know who the unlucky child was."

Often, village leaders or *vaderas*, have an incentive in propagating crimes of honor (Laghari 2016; Lari 2011). The *vadera* holds local courts to arbitrate the crime, and takes a portion of the compensation payment for himself. Thus, it is also in the local leaders economic interests to keep honor killings outside of the official law enforcement system.

When honor killing cases do get reported to local authorities, they are often treated more leniently by both official authorities and the legislation, compared to murders. This is partly because honor crimes are viewed as private family matters rather than crimes against the state, reducing chances of cases being pursued in courts. Previously, the Pakistani legal system allowed perpetrators to seek legal pardon for their crime from other family members. So even if cases reached the court, sentences were rarely made. This loophole was closed by the Criminal Law (Amendment) Act of 2016. However, the effectiveness of stricter legislation is raised into question because of the unwillingness or ineptness of local law enforcement to correctly categorize or pursue honor killing cases.

Given the difficulty of reporting honor based crimes to local authorities, and the fact that most of these crimes are mis-categorized as murders or domestic violence, data from official authorities is minimal and unreliable. I calculate

a correlation coefficient of 0.26 with district-year level data collected from the Punjab police (36 districts only). My data indicate that Punjab police reports underestimate atleast 17 percent of district-year honor killing incidents, compared to my sample.

As data from official authorities is often miscoded and unreliable, any estimates that have been made widely available have been compiled using reporting from newspapers. Available numbers come from the Human Rights Commission in Pakistan and the Aurat Foundation, an organization lobbying for increased women's rights. However, these organizations provide only broad estimates each year, making it difficult to conduct robust scientific analysis. Thus, my first task is to construct a database of honor killings.

3.3. Data

3.3.1. Honor Killing Data Collection. I collect data on honor killing incidents from 11 English and Urdu newspapers for the 41 months spanning April 2014 to August 2017. Approximately half of the news sources were accessed online. The remaining archives were accessed from a local library in Lahore, Pakistan. I focus on local rather than international sources because local media is less prone to reporting bias, and is less likely to focus on "sensational" honor killings (Bruijne and Veen 2017; Ortiz et al. 2005).

To collect the data, a team of research assistants read through each of the 11 newspapers for the specified time period, and scanned any articles related specifically to honor killings, or broadly to violence borne out of sexual or marital freedom. I then sift through the scanned articles to determine which articles describe honor killings. An example of an article included in my data is provided in Figure B.1 in the appendix. For articles which are considered relevant, I code

the information provided. Articles provide varying degrees of information, with many even listing the victim's full name and village of residence. The detail of information often provided suggests it is unlikely that reports would be completely falsified.

The decision of which articles to include in my honor killing data set requires a degree of subjectivity. Recognizing this, I provide a quality number to each article to indicate possible reporter and coder subjectivity. Quality 1 incidents are those for which the article clearly states the incident is an honor killing and provides a reason for the crime. Quality 2 incidents are those for which the article clearly states the crime is an honor killing but does not provide any reason for why the crime was committed. Quality 3 incidents are those for which the article does not explicitly state that the incident was related to honor, but the reasons provided in the report suggest that it was. While the second category allows for reporter subjectivity, the third category accounts for coder subjectivity. Approximately 72 percent of the incidents in my data are categorized as either Quality 1 or Quality 2. This coding process allows me to conduct a robustness test later where I drop articles involving a higher degree of subjectivity.

Finally, each incident coded in the database is linked to its scanned article which is maintained separately, allowing for reproducibility of the data set, improvements and expansions in the future, as well as further analysis.

3.3.2. Rainfall. I calculate district-month rainfall averages for my sample period (April 2014 - August 2017) from the Global Precipitation Measurement-v6 (GPM) satellite data. The GPM data combines satellite precipitation measures with information from land-based weather stations to improve accuracy and reduce spatial auto-correlation (Huffman et al. 2019).

The data also does better than most satellite measures in high elevation regions where previously satellites have underestimated precipitation (Sun et al. 2018). Measurements are collected every half hour at 0.1 arc degree grid cells, approximately equal to 115 km^2. The GPM data is also less sensitive to the potential endogeneity of weather station placement. In Figure B.2 in the appendix, I show the distribution of monthly rainfall for my sample.

I separately estimate monthly long run (1970-2010) average rainfall (in mm) from the University of Delaware Air Temperature and Precipitation database. This data set interpolates precipitation at the 0.5 arc degree resolution, using weather station measurements from various sources (Willmott and Matsuura 2009).

3.3.3. Other Data. I also access daily temperature satellite data from ERA5, Copernicus Climate Change Service, which I then aggregate to the district-month level. The ERA5 data calculates daily temperatures from hourly estimates and is available from 1979 to the present.

For agricultural production information, district and year level data are obtained from the Agricultural Statistics, Pakistan Bureau of Statistics. The data spans the years 1981 to 2008, and includes production information for major crops grown in the country. I obtain district level data on socio-economic indicators separately from the 2017 Demographic and Health Survey (DHS) and the 2014 Pakistan Social and Living Standard Measurement (PSLM) survey. The data include information on domestic violence, fertility, labor force participation, and health.

Finally, I collect district-year level statistics on the number of marriages and divorces for my sample period 2014-2017 from the Punjab Development

Statistics. While I am only able to collect this information for districts in Punjab, the data allow me to run simple correlations with honor killing incidents.

3.4. Honor Killing Descriptive Statistics

3.4.1. Summary Statistics of Incidents. For the study period, I compile 1290 unique honor killing incidents.[3] Summary statistics for the incidents are provided in Table 12. On average, each incident involves 1.38 deaths and 0.13 assaults. Approximately 70 percent of the victims killed and 53 percent of the victims assaulted are women. As incidents often involve more than one victim, I define the Primary Victim as the person who is closest to the perpetrator in relation.[4] This definition allows me to take an agnostic view of the predominant gender of victims in honor killing incidents. This is important because many of the incidents involve killing a second victim who is accused of aiding or being in a relationship with the primary victim. Table 12 shows that in 81 percent of the incidents, the Primary Victim is a female with an average age of 25.[5]

On average, an incident involves 1.65 perpetrators. As shown in Table 13, in approximately 43 percent of the reported incidents, the perpetrator was a male member of the natal family such as a father, brother, or son.[6] Approximately 19 percent of the perpetrators are partners and 14 percent are members of the extended family, including a victim's cousin, grandfather, nephew, uncle, step-son,

[3]I collect a total of 2148 reports, but drop repeated reporting for the same incident across newspapers. On average, an incident appears 1.65 times in my data.

[4]For example, if a man and a woman are killed for honor by the father of the man, then the man would be the Primary Victim.

[5]Incidents are not necessarily similar if the primary victim is male. For example, perpetrators are members of the victim's natal family in only 10.5 percent of incidents involving a male primary victim.

[6]There are only 17 incidents in my sample which involve a female natal family member such as the victim's mother or sister.

Table 12. Honor Killing Incident Summary Statistics

Statistic	Mean	St. Dev.	Min	Max	N
Total Killed	1.38	0.66	0	6	1,290
Prop. Killed: Women	0.70	0.39	0	1	1,283
Total Assaulted	0.13	0.55	0	10	1,290
Prop. Assaulted: Women	0.53	0.48	0	1	1,290
Female Primary Victim =1	0.81	0.39	0	1	1,262
Primary Victim Age	25.07	8.43	13	65	661
Perpetrators	1.65	1.63	1	25	1,147
FIR	0.62	0.49	0	1	1,290
Police	0.93	0.26	0	1	1,290
Rural	0.62	0.49	0	1	479

Note: Data spans the 41 months from April 2014 to August 2017. The `Primary Victim` is identified as the person closest in relation to the perpetrator. `Police` is an indicator variable equal to 1 if the article includes any mention of the police, such as a statement or an indication that the police collected the body of the victim from the crime scene. `FIR/Arrest` is an indicator variable equal to 1 if the article mentions an arrest was made or a first incident report (FIR) was filed.

or an unnamed relative. While these are the most common categories, perpetrators can also be in-laws or unrelated members of the community, such as a neighbor.

For each incident, I create an indicator variable *Police* which signifies that local law enforcement was aware of the crime. For example, the article includes a statement from the police, mentions the police took the body of the victim or that the police is investigating. I separately create an indicator variable *FIR/Arrest* signifying whether any action was taken by the police, such as writing up a first incident report (FIR), or making an arrest during investigations. As shown in Table 12, the police had knowledge of the incident according to 93 percent of the reports. However, some action was reportedly taken in only 62 percent of

Table 13. Relation of the Perpetrator

Statistic	Frequency	Percentage
Natal family	672	42.55
Partner	295	18.68
Extended family	222	14.05
In-law	206	13.04
No relation	130	8.23
Unknown	42	2.66
Other	12	0.76
	1,579	100

Note: `Natal` family members include the mother, father, son, or brother. `Extended family` members include the uncle, cousin, grandfather, step-son, nephew, or an unknown relative. `In laws` include the mother-in-law, father-in-law, brother-in-law or uncle-in-law. `Partner` includes the husband, and in 4 cases, the wife. `Other` includes the fiance, ex-fiance, ex-boyfriend, or ex-husband.

the incidents.[7] Recognizing that these numbers are not definitive, they are still indicative of the hesitancy of family members to register crimes as well as the reluctance of the police to pursue perpetrators. In many cases, even if a case is registered or an arrest is made, perpetrators are often let go or pardoned by family members.

I also collect information on the reasons stated for the honor killing, displayed in Table 14[8]. The table shows that approximately half of all incidents occur because the victim had an alleged affair, while in 22 percent of the incidents the victim either married by choice or wanted to marry by choice. In 16 percent of the incidents, the victim was accused of having a questionable character which

[7]Conditional on the police having knowledge of the incident, an FIR was written up or an arrest was made for 65 percent of the incidents.

[8]The reasons stated are quoted by the reporter either directly from the police or the perpetrator. In certain cases, the reporter states the reasons behind an honor killing incident without naming a specific source.

could be because she came home late, was texting or calling a man unrelated to her. As discussed earlier, most honor killings are meant to punish the victim for displaying sexual/marital freedom, or in some cases, merely voicing the desire to display sexual/marital freedom.

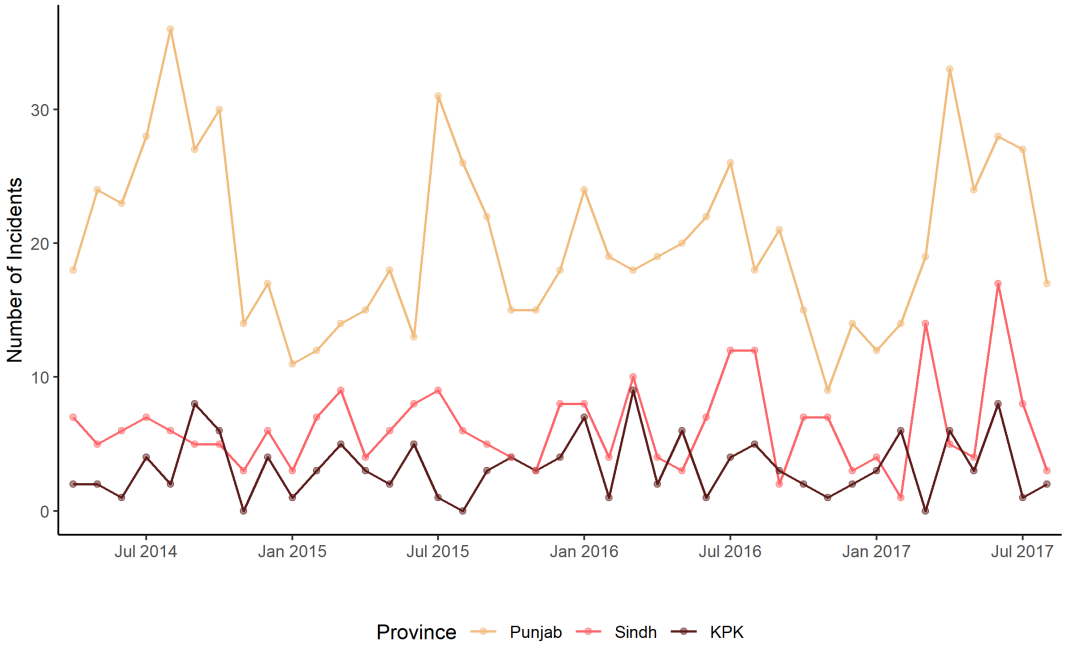
In Figure 6, I plot reported honor killing trends for the three most populated provinces in Pakistan. The graph makes clear the temporal and geographical variation in honor killing incidents. Figure B.3 in the appendix maps this geographical variation in honor killing incidents across the country. There are several districts for which I do not see any honor killings. While this may be because some of the districts are desert areas or otherwise have very low populations, it could simply be because of a lack of media reporting for those regions.

Table 14. Reasons Stated for Reported Incidents

Statistic	Frequency	Percentage
Alleged Affair	587	47.80
Marriage of Choice	264	21.50
Questionable Character	200	16.29
Honor	68	5.54
Elopement	62	5.05
Divorce/Other Dispute	28	2.28
Assault	9	0.73
Domestic dispute	7	0.57
Rejected proposal	3	0.24
	1,228	100

Note: `Marriage of Choice` includes marrying someone by choice or having the desire to marry someone by choice. `Questionable Character` includes talking to someone on the phone, going out late at night, or otherwise displaying 'immoral' behavior. `Honor` includes articles which include family or police statements suggesting the crime was for honor, without further explanation.

Figure 6. Reported Honor Killing Trends by Province



Note: Data spans the months from April 2014 to August 2017. The figure shows trends for monthly raw incident counts by province.

I display the method used to commit the honor killing in Figure B.4 of the appendix. Previous qualitative literature has suggested that the murder of victims for honor tends to be purposefully prolonged and 'gruesome' to caution other women in the family or village from transgressions. Interestingly, I do not find evidence for this claim; most victims in my data were shot dead. I have very few incidents where the victim was stoned or burned to death, for example. While it is difficult to categorize certain deaths as being more 'gruesome' than others, and it is also possible that victims are tortured before being shot, these cases are few in my data.

3.4.2. Socio-Economic Correlations. Do honor killings generally occur in districts where there exists a higher prevalence of violence against women? In Table 15, I summarize correlations of honor killing incidents with self-reported

violence against women from the 2017 DHS. Several interesting points can be made here. First, districts which have higher incidents are associated with a significantly greater prevalence of emotional violence against women, but lower levels of controlling behavior. Emotional violence refers to ever being humiliated or insulted by a partner. Controlling behavior includes displays of jealousy, attempts to track or restrict movements, and accusations of unfaithfulness. This is possible if greater controlling behavior results in fewer displays of sexual freedom in the first place.

Reported honor killings do not seem to be significantly correlated with higher prevalence of violence from a woman's natal family. This could be because the sample of women who are interviewed for the domestic violence module include married women above the age of 15. It is possible that these women are not representative of unmarried women who select into marriage of their choice; approximately 80 percent of the respondents in the DHS survey already had some say in whom they married. Finally, the variable *Bad Name to Family* measures the proportion of women in a district facing domestic abuse who do not seek help because of a concern of bringing a bad name to the family. This is the closest measure of concerns of bringing shame, or losing honor, allowed by the data. Interestingly, the variable is significantly positively correlated with reported incidents as displayed in column (1). Conditional on province fixed effects, districts where women are concerned about bringing a bad name to their family are associated with approximately 9 more reported incidents. Conditional on prevalent domestic violence, the estimate is no longer significant.

Table 15. Correlations: Reported Incidents with Domestic Violence (Without Lahore)

	Incidents			
	(1)	(2)	(3)	(4)
Bad Name to Family = 1	9.135*		6.582	4.369
	(5.383)		(5.441)	(5.826)
Blood Relation to Partner = 1	−8.180		−15.108	−23.639
	(15.164)		(15.616)	(16.645)
Partner Choice = 1	−0.451		7.142	9.722
	(10.780)		(11.224)	(12.480)
Domestic Violence				
Controlling Behavior		−37.997*	−35.557*	−48.845**
		(19.830)	(19.897)	(21.686)
Physical Violence		0.537	−9.717	−19.053
		(32.417)	(33.322)	(35.385)
Emotional Violence		34.534	36.049	41.721*
		(21.236)	(21.667)	(24.205)
Violence from Natal Family		−53.585	−54.572	−56.071
		(53.205)	(54.401)	(63.879)
Population	0.000***	0.00001***	0.00001***	0.00001***
	(0.000)	(0.000)	(0.000)	(0.000)
Province Fixed Effects	✓	✓	✓	✓
Socio-economic Controls				✓
Observations	72	71	71	71
R^2	0.477	0.496	0.521	0.606
Adjusted R^2	0.401	0.411	0.411	0.425

Note: The dependent variable is the number of incidents per district during the period April 2014 to August 2017. Lahore is excluded from the sample. The mean of the dependent variable is 14.2 incidents. The independent variables are selected from the Demographic and Health Survey 2017, and are district level averages for female respondents between the ages 15-49. `Bad Name to Family` indicates the percentage of respondents facing domestic violence who did not seek help from fear of bringing a bad name or shaming her family. `Blood Relation to Partner` and `Partner Choice` indicate the percentage of respondents in a district who are related to their partner by blood and who had some say in choosing their partner, respectively. `Controlling Behavior` includes displays of jealousy, attempts to track or restrict movements, and accusations of unfaithfulness. `Physical Violence` refers to the proportion of women who have been punched, kicked or beaten by their partner. `Emotional Violence` refers to being humiliated, made to feel bad or insulted by a partner. `Socioeconomic Controls` include average wealth in the district, population, average probability of working before and after marriage, fertility, average age of cohabitation, education, partner's education, and average probability of phone and bank account ownership. `Population` is measured using the 2017 Population Census from the Pakistan Bureau of Statistics. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

To understand other correlates of honor killings, I test for significant associations of incidents with a number of socio-economic indicators. I access data from the 2017 Demographic and Health Survey and the 2014 Pakistan Social and Living Standard Measurement Survey. In Table B.2 of the appendix, I summarize socio-economic correlates with honor killing incidents pooled at the district level. I do not find significant associations of incidents with measures of household wealth, female labor force participation before and after marriage, education or fertility.

Finally, using the compiled data set on reported honor killings, I construct a panel spanning 41 months for 75 districts which have at least one honor killing incident for the sample period. There are 0.33 reported honor killing incidents per district-month, on average. This translates to roughly 1 incident (or 1.38 deaths) in a district every three months.

3.5. Weather Shocks and Honor Killings

I begin my analysis by exploiting plausibly exogenous local rainfall deviations in a district-month from the long run average (1970-2010) to explain variation in honor killing incidents. The outcome variable is a count of incidents in a district and month, therefore I employ a Poisson regression model to estimate the equation:

$$Incidents_{dt} = \beta_1 WetShock_{dt} + \beta_2 DryShock_{dt} + \beta_3 X_{dt} + f(Lags) + \alpha_d + \gamma_y + \eta_{dy} + \epsilon_{dt} \tag{3.1}$$

where $Incidents_{dt}$ is the number of honor killing incidents in district d and month-year t, and $WetShock_{dt}$ is the absolute deviation of rainfall above the long run average, in mm. Similarly, $DryShock_{dt}$ is the absolute deviation of rainfall below the long run average, in mm. X_{dt} includes district by month-year controls for temperature. *f(Lags)* is a function of monthly lags of wet and dry shocks, and α_d,

γ_y, and η_{dy} are district, year, and district by year fixed effects. Following Sekhri and Storeygard (2014), and supported by Figure B.5 in the appendix, I use a linear spline with a fixed knot at zero to distinguish between the separate effects of wet and dry shocks. Note that rainfall is measured with error, which should lead to attenuation bias in my estimates.

I want to isolate the effect of variation in rainfall within a district and month-year, from the long run average district-month rainfall. Inclusion of district fixed effects purges any time-invariant unobserved characteristics common within a district, such as geography. The inclusion of year fixed effects will control for any time-varying shocks common to all districts in the sample. Finally, to allow for spatial correlation in the error term ϵ_{dt}, standard errors are clustered at the district level.

In Table 16, I present results from the estimation of equation (1). In column (3), I find that a wet shock in the contemporaneous month significantly increases reported honor killing incidents. I do not find a significant effect of dry shocks on honor killings. The within district-month standard deviation of rainfall is 23.6 mm. Thus, a one standard deviation increase in rainfall in the current month results in a 2.63 percent increase in reported incidents, on average. In column (4), I lose precision for my contemporaneous rainfall shock with the inclusion of month fixed effects, and the point estimate decreases from 0.0011 to 0.0003, although the coefficients are not significantly different. The inclusion of month fixed effects

effects removes considerable variation from my rainfall shock measures; note that about 15 percent of the variation in *WetShock* is explained by month fixed effects, and including district and district*year effects explains 25 percent of the variation.

Table 16. Effect of Rainfall Shocks on Honor Killing Incidents (Poisson)

	Incidents					
	(1)	(2)	(3)	(4)	(5)	(6)
Wet Shock (t + 4)					-0.0002	-0.0003
					(0.0008)	(0.0006)
Dry Shock (t + 4)					-0.0099	-0.0044
					(0.0065)	(0.0059)
Wet Shock (t + 3)					-0.0005	0.00001
					(0.0008)	(0.0006)
Dry Shock (t + 3)					-0.0059	-0.0021
					(0.0065)	(0.0062)
Wet Shock (t + 2)					0.0007	0.0008
					(0.0007)	(0.0005)
Dry Shock (t + 2)					-0.0046	-0.0061
					(0.0071)	(0.0065)
Wet Shock (t + 1)					0.0009	0.0009*
					(0.0007)	(0.0005)
Dry Shock (t + 1)					-0.0002	-0.0031
					(0.0070)	(0.0065)
Wet Shock	0.0012***	0.0011**	0.0011**	0.0003	0.0011	0.0018***
	(0.0004)	(0.0004)	(0.0005)	(0.0006)	(0.0007)	(0.0005)
Dry Shock	0.0045	0.0024	0.0016	0.0039	-0.0005	0.0018
	(0.0062)	(0.0070)	(0.0076)	(0.0059)	(0.0076)	(0.0074)
Wet Shock (t - 1)		0.0007	0.0007	0.0003	0.0011	0.0015**
		(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0007)
Dry Shock (t - 1)		-0.0013	-0.0020	0.0022	-0.0013	-0.0001
		(0.0056)	(0.0060)	(0.0063)	(0.0066)	(0.0062)
Wet Shock (t - 2)		-0.0006	-0.0006	-0.0010**	-0.0003	0.00001
		(0.0004)	(0.0005)	(0.0005)	(0.0006)	(0.0005)
Dry Shock (t - 2)		-0.0108	-0.0122	-0.0061	-0.0130	-0.0168**
		(0.0076)	(0.0082)	(0.0078)	(0.0088)	(0.0085)
Wet Shock (t - 3)		0.0005	0.0005	0.0004	0.0014**	0.0011**
		(0.0004)	(0.0004)	(0.0005)	(0.0007)	(0.0005)
Dry Shock (t - 3)		0.0011	0.00003	0.0062	-0.0009	-0.0027
		(0.0068)	(0.0074)	(0.0080)	(0.0117)	(0.0101)
Temperature	✓	✓	✓	✓	✓	✓
District	✓	✓	✓	✓	✓	✓
Year		✓	✓	✓	✓	✓
District*Year			✓	✓	✓	✓
Month				✓	✓	
Observations	3,075	2,850	2,130	2,130	1,930	1,930
AIC	3,972.30	3,711.00	3,814.41	3,802.51	3,413.76	3,413.88

Note: The table presents results from Poisson regressions where the dependent variable is the number of honor killing incidents in a district and month-year. `WetShock` and `DryShock` are the positive and negative absolute deviations of rainfall from the long run mean rainfall in a district-month (in mm), respectively. The sample covers 75 districts for 41 months. Clustered (district) standard-errors in parenthesis. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

In column (6), I include leads for both wet and dry shocks as a robustness check. A wet shock two months in the future is marginally significant in explaining reported incidents. This suggests the use of a stricter probability for a Type I error for inference purposes. Using a stricter cutoff to interpret column (6), a wet shock in both the current and previous month significantly increases reported incidents by approximately 8.1 percent. Finally, I estimate equation (1) using a Negative Binomial model; the results are qualitatively similar and are presented in Table B.3.

It is improbable that a 1 mm positive rainfall shock would be harmful or even have symmetric effects across the different agroclimatic zones in the country. To make shocks comparable across regions, I normalize rainfall shocks with the standard deviation of long run rainfall in a district and month. I divide shocks into bins of 0.75 standard deviations, following Sekhri and Storeygard (2014); the reference category is the bin centered on the mean. In Table B.4, I show that small wet shocks in the previous month do not seem to significantly affect honor killing incidents. Shocks between 1.24 to 1.875 standard deviations significantly increase reported incidents.

Next, I redefine wet and dry rainfall shocks using the methodology employed in earlier literature (Jayachandran 2006; Kaur 2019). A wet shock is an indicator equal to 1 for positive rainfall shocks above the 80th percentile for a district and month. A dry shock is an indicator equal to 1 for negative rainfall below the 20th percentile for a district and month. This definition captures the non-linearity of rainfall shocks. In Table 17, I show that extreme wet shocks in the current month are associated with a significant increase in reported honor killing incidents. Specifically, in column (3), an extreme wet shock in the current month

Table 17. Effect of Extreme Rainfall Shocks on Honor Killing Incidents (Poisson)

	Incidents				
	(1)	(2)	(3)	(4)	(5)
Wet Shock (t + 4)				-0.1261	-0.0237
				(0.0979)	(0.1278)
Dry Shock (t + 4)				0.2355	0.1063
				(0.2616)	(0.2638)
Wet Shock (t + 3)				-0.0926	-0.0595
				(0.1198)	(0.1527)
Dry Shock (t + 3)				0.3147	0.2534
				(0.4181)	(0.4104)
Wet Shock (t + 2)				0.0229	0.0514
				(0.1129)	(0.1361)
Dry Shock (t + 2)				0.0011	0.0087
				(0.2261)	(0.2404)
Wet Shock (t + 1)				0.2090*	0.0908
				(0.1157)	(0.1193)
Dry Shock (t + 1)				0.0413	0.0715
				(0.3472)	(0.3368)
Wet Shock	0.2768***	0.3151***	0.2833***	0.2713**	0.2117*
	(0.0922)	(0.0959)	(0.1062)	(0.1076)	(0.1247)
Dry Shock	0.2241	0.1774	0.0372	0.0632	0.0644
	(0.1865)	(0.1986)	(0.2123)	(0.2488)	(0.2618)
Wet Shock (t - 1)		0.0215	-0.0213	-0.0871	-0.0719
		(0.0910)	(0.0996)	(0.1073)	(0.1502)
Dry Shock (t - 1)		-0.2194	-0.3706	-0.3799	-0.4049
		(0.2707)	(0.2651)	(0.2994)	(0.2869)
Wet Shock (t - 2)		-0.0119	-0.0294	0.0300	-0.1284
		(0.0923)	(0.0992)	(0.0974)	(0.1127)
Dry Shock (t - 2)		-0.2547	-0.3717	-0.2749	-0.2504
		(0.2478)	(0.2701)	(0.3104)	(0.3284)
Wet Shock (t - 3)		0.1059*	0.0591	0.1292	0.0510
		(0.0614)	(0.0739)	(0.0826)	(0.1064)
Dry Shock (t - 3)		-0.1008	-0.2655	-0.2076	-0.2208
		(0.2187)	(0.2183)	(0.2738)	(0.3038)
Temperature	✓	✓	✓	✓	✓
District	✓	✓	✓	✓	✓
Year		✓	✓	✓	
District*Year			✓	✓	✓
Month					✓
Observations	3,075	2,850	2,130	1,930	1,930
AIC	3,970.75	3,706.97	3,813.42	3,418.20	3,410.80

Note: The table presents results from a Poisson estimation where the dependent variable is the number of honor killing incidents in a district and month-year. `WetShock` and `DryShock` are indicator variables equal to 1 if rainfall was above the 80 percentile or below the 20th percentile for a district-month, respectively. Leads and lags are defined similarly. Clustered (district) standard-errors in parenthesis. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

leads to a 32 percent increase in reported killings. I include leads in columns (5) and (6) as a robustness check. As before, a marginally significant wet shock in the future in column (5) suggests a stricter Type I error in interpreting the results.

As mentioned earlier, the decision of which articles to include in the data set requires a degree of subjectivity. Recognizing this, I give each article a quality index to allow for researcher subjectivity. As a robustness test, I drop articles which do not specifically use the terms "honor" or "honor killing" to describe the incident. I rerun my results on the remaining sample. Table 18 shows the results are robust to the use of the smaller sample of only Quality 1 or Quality 2 incidents.

3.6. Income Shock Theory

So far, I have shown that positive rainfall shocks do affect reported incidents. To test the hypothesis that the estimated results are driven by shocks to income, I explore the effect of wet and dry rainfall shocks on agricultural production. The agricultural data is at the district-year level so I aggregate monthly wet and dry shocks to obtain measures of seasonal rainfall variation. In Table 19, I show that cotton production is very sensitive to even small rainfall shocks above the long run average, while there is no significant effect on other major crops. The point estimates for cotton are significant and increasing for every standard deviation increase in wet shocks.

Cotton is tolerant to small dry shocks because its deep vertical roots allow the plant to access groundwater. However, the crop is sensitive to even small amounts of rainfall, especially at the ball formation and flowering stage.[9]

[9]https://www.reuters.com/article/us-pakistan-cotton-climate-change/climate-damage-to-pakistans-cotton-crop-ripples-through-economy-idUSKBN1YM0OU
https://tribune.com.pk/story/1497149/monsoon-rains-damage-10-15-cotton-onion-crops-sindh

In Table B.5 in the appendix, I show that the effect of wet shocks on cotton are driven by the months July to January, which are broadly the ball formation and harvesting months for cotton crops in Pakistan.
Crops which obtain too much moisture can potentially become moldy, diseased, or otherwise unusable, severely reducing yields.[10] Ginning companies will refuse to purchase raw cotton which holds moisture, further reducing incomes for farmers.

Cotton accounts for approximately 39 percent of total income for landowners, and 45 percent of total income for sharecroppers in Pakistan (Orden et al. 2006). Thus, even a small shock can account for a large portion of annual income. In my sample, approximately 62 percent of the reported incidents occurred in rural areas, suggesting my sample consists of families who are likely linked to the agriculture sector.[11] Figure B.6 in the appendix displays the variation in cotton production across the country.

To test whether my results are driven by districts producing cotton, I run the following regression:

$$Incidents_{dt} = \beta_1 WetShock_{dt} * Cotton_d + \beta_2 DryShock_{dt} * Cotton_d + f(Lags_{dt} * Cotton_d) + \beta_3 X_{dt} + \alpha_d + \gamma_y + \eta_{dy} + \epsilon_{dt}$$

where $Cotton_{dt}$ is a dummy equal to 1 if district d has sown cotton on some positive area of land over the years 2000 to 2008, and 0 otherwise.[12] $WetShock_{dt}$ and $DryShock_{dt}$ are measured as before. α_d, γ_y, and η_{dy} are district, year, and district by year fixed effects, respectively.

[10] https://tribune.com.pk/story/593860/heavy-rains-batter-cotton-crop/

[11] Based on data entry of a small sample of 479 incidents. Remaining data entry to be completed.

[12] I also reproduce the results by defining Cotton as a dummy equal to 1 if a district has produced some positive amount of cotton (instead of area cultivated).

Table 18. Robustness-Quality 1 and 2 Articles Only

	Incidents			
	(1)	(2)	(3)	(4)
Wet Shock (t + 2)				0.0013*
				(0.0008)
Dry Shock (t + 2)				-0.0076
				(0.0101)
Wet Shock (t + 1)				0.0009
				(0.0007)
Dry Shock (t + 1)				-0.0012
				(0.0080)
Wet Shock	0.0016***	0.0015**	0.0017***	0.0018**
	(0.0004)	(0.0006)	(0.0007)	(0.0009)
Dry Shock	0.0012	0.0019	0.0018	0.0022
	(0.0075)	(0.0073)	(0.0079)	(0.0072)
Wet Shock (t - 1)		0.0009	0.0010	0.0014
		(0.0007)	(0.0008)	(0.0010)
Dry Shock (t - 1)		-0.0028	-0.0036	-0.0062
		(0.0082)	(0.0087)	(0.0097)
Wet Shock (t - 2)		-0.0004	-0.0003	-0.0005
		(0.0006)	(0.0007)	(0.0006)
Dry Shock (t - 2)		-0.0058	-0.0069	-0.0069
		(0.0078)	(0.0084)	(0.0087)
Wet Shock (t - 3)		0.0003	0.0004	0.0010
		(0.0005)	(0.0005)	(0.0006)
Dry Shock (t - 3)		-0.0010	-0.0020	0.0038
		(0.0078)	(0.0085)	(0.0093)
Temperature	✓	✓	✓	✓
District	✓	✓	✓	✓
Year		✓	✓	
District*Year			✓	✓
Month				✓
Observations	2,993	2,774	1,890	1,782
AIC	3,264.03	3,043.74	3,122.81	2,932.18

Note: The sample spans the months from April 2014 to August 2017, and includes only Quality 1 and Quality 2 articles. The dependent variable is the number of honor killing incidents in a district and month-year. `WetShock` and `DryShock` are the positive and negative absolute deviations of rainfall from the long run mean rainfall in a district-month (in mm), respectively. Standard errors in parenthesis, with clustering at the district level. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

Table 19. Effect of Seasonal Rainfall Shock Severity on Crop Production

	Cotton	Sugarcane	Rice	Wheat
	(1)	(2)	(3)	(4)
Wet Shock > 4 stdev	−43,917.4**	−84,031.6	−2,182.0	−21,380.1
	(17,326.3)	(106,403.4)	(7,823.8)	(21,482.7)
Wet Shock (3, 4] stdev above	−48,305.0**	−35,972.4	−10,933.6	53,900.0
	(22,017.2)	(110,575.7)	(8,232.6)	(56,024.1)
Wet Shock (2, 3] stdev above	−30,408.0**	39,486.2	1,137.3	−2,567.1
	(13,433.2)	(66,876.0)	(7,324.5)	(8,180.8)
Wet Shock (1, 2] stdev above	−33,358.8***	24,660.7	−1,239.8	2,689.3
	(11,148.1)	(40,509.1)	(3,112.3)	(5,254.2)
Dry Shock (1, 2] stdev below	31,351.6***	43,600.9	3,521.8	−19,968.8
	(11,841.4)	(33,435.8)	(4,168.1)	(23,618.9)
Dry Shock (2, 3] stdev above	8,527.5	−22,411.4	−2,713.0	−1,313.0
	(17,990.6)	(63,205.6)	(2,376.3)	(6,528.2)
Dry Shock > 3 stdev below	16,577.0	49,708.9	−967.9	−4,647.2
	(31,177.2)	(83,337.2)	(3,708.6)	(7,480.3)
District/Season-Year FE	✓	✓	✓	✓
Observations	546	449	547	475
R^2	0.970	0.964	0.945	0.966
Adjusted R^2	0.964	0.955	0.934	0.958

Note: The table presents OLS regression results where the dependent variable is the production of cotton, sugarcane, rice, and wheat for a district-year, in tonnes. `WetShock` and `DryShock` are indicator variables for standardized deviations of rainfall from the long run average, calculated at the district-season level. Bins are divided into 1 standard deviations each. Clustered (district) standard-errors in parenthesis. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

The results are presented in Table 20. In column (2), the table shows that a 1 standard deviation increase in rainfall above the long run average in the previous month, in districts which cultivate cotton, significantly increases reported honor killing incidents by approximately 6.3%. In columns (3) and (4), I include Month and Month-Year fixed effects respectively, to control for seasonality effects. The results are robust to the inclusion, although the point estimate decreases slightly. In column (4), a wet shock above the long run average in a cotton producing district now increases reported incidents by 6.08%. The results are consistent with the income shock hypothesis; positive rainfall shocks lead to a fall in cotton production and a significant increase in reported honor killings.

Next, I test whether the results are indeed driven by rainfall shocks in months that align with periods of cotton harvesting, I interact rainfall shocks with a dummy *Harvest* which equals 1 for the major cotton harvesting months in the country, September to December. In column (5) of Table B.6, the results indicate that a positive rainfall shock in the previous month increases reported incidents by 5.3%, for shocks occurring in harvesting months compared to non-cotton harvesting months. All regressions control for district and month-year temperature variation.

The estimated effects in this research are comparable in magnitude to those presented in earlier literature similar to this paper. Specifically, Sekhri and Storeygard (2014) estimate a 7.8 percent increase in dowry related deaths from a 1 standard deviation decrease in rainfall. The effect is smaller than those estimated by Miguel (2005), who finds that droughts cause a doubling of witch killings in rural Tanzania. However, Miguel (2005) focuses on extreme weather events; I find effects of comparable magnitudes when I estimate the effect of only extreme wet

Table 20. Results: Interaction with Cotton Production

	Incidents			
	(1)	(2)	(3)	(4)
Wet Shock	0.0009	0.0008	0.0003	0.00002
	(0.0007)	(0.0007)	(0.0008)	(0.0009)
Wet Shock*Cotton	0.00009	0.0004	0.0001	0.00005
	(0.0008)	(0.0010)	(0.0010)	(0.0010)
Dry Shock	-0.0352	-0.0399	-0.0343	-0.0353
	(0.0261)	(0.0295)	(0.0297)	(0.0282)
Dry Shock*Cotton	0.0379	0.0426	0.0397	0.0373
	(0.0270)	(0.0301)	(0.0309)	(0.0289)
Wet Shock (t - 1)	-0.0012	-0.0012	-0.0012	-0.0004
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Wet Shock(t-1)*Cotton	0.0025***	0.0026**	0.0021*	0.0025**
	(0.0010)	(0.0011)	(0.0012)	(0.0012)
Dry Shock (t - 1)	-0.0281*	-0.0297*	-0.0254	-0.0198
	(0.0144)	(0.0174)	(0.0181)	(0.0193)
Dry Shock(t-1)*Cotton	0.0305*	0.0311	0.0301	0.0261
	(0.0163)	(0.0190)	(0.0192)	(0.0203)
Wet Shock (t - 2)	-0.0009	-0.0011	-0.0012*	-0.0006
	(0.0006)	(0.0007)	(0.0007)	(0.0008)
Wet Shock(t-2)*Cotton	0.0002	0.0003	0.00002	0.0003
	(0.0008)	(0.0010)	(0.0011)	(0.0011)
Dry Shock (t - 2)	-0.0284	-0.0317	-0.0150	-0.0160
	(0.0189)	(0.0213)	(0.0205)	(0.0213)
Dry Shock (t - 2)*Cotton	0.0220	0.0232	0.0131	0.0078
	(0.0197)	(0.0226)	(0.0217)	(0.0223)
Temperature	✓	✓	✓	✓
District	✓	✓	✓	✓
Year	✓	✓		
District*Year		✓	✓	✓
Month			✓	
Month-Year				✓
Observations	2,730	2,079	2,079	2,079
AIC	3,626.78	3,723.76	3,715.27	3,729.74

Note: The dependent variable is the number of honor killing incidents in a district and month-year. Cotton is a dummy equal to 1 if a district produced some positive amount of cotton over the years 2000 to 2008. Standard errors in parenthesis, with clustering at the district level. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

shocks (floods) on reported honor killings. The fact that I find similar effect sizes despite the use of data collected from media reports is reassuring.

Earlier work finds that dry shocks or droughts are associated with negative income shocks, and increased crime. In contrast, my research finds the opposite; higher than average rainfall is associated with a fall in cotton production and likely, agricultural income. I do not find robust evidence for dry shocks affecting reported crimes. This difference merits further discussion. First, my sample time period has few dry shocks, as displayed in Figure B.5. It is likely that extreme dry shocks are detrimental to agricultural production and also increase honor killing incidents through the same channel. However, I am not powered to detect or rule out such an effect with the current sample. Second, Pakistan has one of the largest irrigation systems in the world.[13] It is possible that small dry shocks are mitigated with the support of the country's irrigation network, while crops have little protection from harsh and unexpected rains.

3.7. Discussion

3.7.1. Potential Misreporting. A major concern with the use of newspaper reports, and with working with statistics on violence against women in general, is that of misreporting. In this particular context, reporting is a function of both the crime having occurred and the crime being reported by the media. While the goal is to understand the effect of rainfall shocks on occurrence of honor killings, I only observe the combined effect on occurrence and reporting. The estimates will be biased if the probability of the media reporting an incident is correlated with rainfall shocks. To test for reporting bias, I estimate the Poisson

[13]Pakistan has the fourth highest amount of irrigated land in the world. CIA World Factbook, 2012.

regression:

$$Repeats_{dt} = \beta_1 WetShock_{dt} + \beta_2 DryShock_{dt} + f(Lags) + \alpha_d + \gamma_y + \eta_{dy} + \epsilon_{dt} \quad (3.2)$$

where $Repeats_{dt}$ is the average number of times an incident in district d and month-year t is reported across newspapers. $WetShock_{dt}$, $DryShock_{dt}$, and *f(Lags)* are defined as before. α_d, γ_y, and η_{dy} are district, year, and district by year fixed effects. Note that the sample is no longer balanced because I am restricting the data to district-month pairs with atleast one incident.

I use the average number of times an incident is reported across newspapers as a noisy measure of the likelihood of reporting honor killings.[14] In Table B.7, I do not find evidence that conditional on the occurrence of an incident, wet rainfall shocks lead to changes in the likelihood of reporting. The effect of contemporaneous and lagged wet shocks are insignificant, even after controlling for district level variation. Ofcourse, this is not conclusive evidence; it is still possible that media reporting is biased in an unobservable manner. Underreporting by the media will imply the reported estimates in Table 20 are unbiased lower bounds.

3.7.2. Selection Into Marriage. The most common reported reasons for honor killings are alleged affairs or marrying by choice. To accurately estimate the effect of rainfall shocks on honor killings, I need to rule out the possibility that shocks induce young men or women to elope or participate in affairs. I do not have the data to reject this possibility. Instead, I test whether yearly wet and dry rainfall shocks significantly affect the number of marriages or divorces in a district, using a subset of data from Punjab. Table B.8 displays the results; I do not find a significant impact of rainfall shocks on marriage or divorce. However,

[14]The average number of times an incident is repeated across newspapers is 1.54.

this is merely suggestive evidence and should be interpreted with caution because the data sample is very limited.

3.8. Conclusion

This research examines the effects of rainfall shocks on reported honor killing incidents in Pakistan. The research suggests that negative income shocks cause a significant increase in the murder of young men and women in the name of 'honor'. Higher than average rainfall leads to a fall in the production of cotton, a crucial cash crop in Pakistan, and an increase in reported incidents by approximately 6.3 percent. A possible explanation for this result is that women who participate in pre-marital relationships (allegedly or not) are no longer marriageable, implying that the woman would need to be financially supported by her natal family for her remaining years. In periods of economic distress, families may be more sensitive to accepting this responsibility.

The results in this paper by no means imply that economic factors are the only explanation behind honor based crime. It is highly likely that perpetrators genuinely believe their actions are motivated by the need to protect their honor, which is why honor killings still occur in periods of relatively low economic distress. It is also likely that non-agricultural income shocks, or changes in land rights, explain increases in crimes under the guise of honor. Nonetheless, the results presented are helpful in explaining a portion of the variation in honor killings.

Recognizing the limitations of the data collection process and the potential for misreporting, I cautiously summarize some policy implications from my research. First, data collection on honor based crimes must be improved. This would entail training local law enforcement and authorities to improve

investigation and reporting of honor based crimes. While the government has taken the positive step of disallowing the pardoning of perpetrators of honor killings, the potential impact of this change will be muted if local authorities miscategorize the crimes. Second, increased access to improved weather insurance may reduce the economic distress faced by households from damaged cotton crops, thereby reducing the indirect risk to women of income shocks.

CHAPTER IV

COMPETITION IN SIBLING FERTILITY? AN ANALYSIS OF SOUTH ASIAN JOINT HOUSEHOLD

4.1. Introduction

Economists have documented that in almost every sphere of a woman's life, she has fewer opportunities and lower autonomy than her male counterparts. These gaps are particularly apparent in developing countries. In India, only 26 percent of women participate in the labor force compared to 78 percent of men. Approximately 1 in 3 women in the country have faced a less severe form of intimate partner violence, and only 11 percent have complete decision-making power over their health care. Low levels of female empowerment are further linked to poorer child and maternal health outcomes and less say over fertility decisions.

Researchers and policymakers continue debating which interventions and policies are successful in improving female autonomy. For example, legislation of equal inheritance rights for women in India encouraged female foeticide and did not increase women's likelihood of inheriting property, although investment in female education and dowry increased (Bhalotra, Brule, and Roy 2020; Roy 2015). Evidence suggests that women themselves also invest in methods to improve their autonomy after marriage. A prominent example is an effort to birth a son, as male offspring provide old age security, a claim to the household property, and improve short-term decision-making power (Bloom, Wypij, and Gupta 2001; Lambert and Rossi 2016; Zimmermann 2018).

Yet, female autonomy is also dependent on the presence of other women in the household. This fact is especially notable in joint or extended residences which are characterized by adult sons co-residing with their parents, wives, and

children. Gupta (1995) notes that a woman's autonomy changes during her life cycle; a young bride is subservient not only to the male members of the household, but to older women as well. Thus, Indian joint households follow gender as well as age hierarchy. A woman's status rises with the birth of a son, and with age.

In this research, I explore the hypothesis that the social status of women in joint Indian households is determined by both their husband's relative age, as well as the gender of their children. Theoretically, women who marry the elder co-resident son (first rank) should have greater empowerment as a result of being the elder daughter-in-law, compared to women who marry the younger co-resident son (second rank). Upon the entrance of the second rank, first rank women are no longer at the bottom of the age hierarchy and should therefore have less pressure to bear sons as a means to improve their position in the household. Second ranking women cannot compete across ranks without exiting the household, which can come at a significant economic cost. Thus, the incentive to bear a son to improve their status in the household should be higher for younger co-resident daughter-in-laws.

Focusing on households with only two co-residing sons, I utilize data from the Demographic and Health Survey (DHS) to test this hypothesis. First, I show that second ranking women are significantly less likely to have any say in decisions relating to their own healthcare, large household purchases, visits to their natal family, and disposal of their partner's income, compared to first ranking women. Women married to the elder son are effectively higher in the age hierarchy upon the entrance of their younger sister-in-law, allowing them to exhibit greater mobility and decision making power. Exploiting the entrance of the younger son's wife into the household, I show that first ranking women increase birth spacing by

9.1 percent on average, even if they have not birthed a son. This increase is not explained by changes in early childhood investments, such as breastfeeding. On the other hand, women who marry the younger co-residing son cannot improve their autonomy using rank, and instead have a greater incentive to birth a son. On average, second ranking women reduce their marriage to first birth interval by approximately 4.8 percent, if the first ranking woman already has a son. Taken together, the results provide evidence for co-dependencies of female autonomy in joint households.

To my knowledge, there is no quantitative study analyzing the possible co-dependency of fertility behavior in joint patrilocal households. Coffey, Khera, and Spears (2016) study the effect of a woman's rank within joint households in India and find that women married to the younger brother have lower empowerment and higher child mortality. In contrast, my paper focuses on the impact of the social status of women within a household on their fertility behavior. Most closely related to my paper, Rossi (2019) studies polygamous households in Senegal and finds that first wives lengthen birth spacing with the entrance of a competitor, but this effect is smaller if the co-wife has a longer reproductive period. The results suggest the existence of a natural reduction in total fertility in polygamous households (the husband must now spend time with each wife), as well as a competition effect which pushes fertility upwards.

This research relates to the literature studying competition within joint households but differs for several reasons. First, I study fertility behavior among women who live in the same household, but are not co-wives. Thus, my setting does not imply a natural reduction in fertility as husbands split time between wives. Also, women in polygamous households are competing for resources

provided by the partner. In contrast, while some resources in joint households are considered pooled, the women in my sample should theoretically have access to separate assets or income from their individual husbands. Thus, that the results of fertility rivalry from earlier work should naturally extend to a joint family household are not a priori clear.

The remaining paper is structured as follows. In Section 4.2., I provide a background of joint household structures in India and describe the data. Section 4.3. provides the empirical methodology, while Section 4.4. discusses the results. Section 4.5. concludes.

4.2. Context and Data

4.2.1. Context. There are two characteristics of South Asian culture that are relevant to our understanding of the context of this research-patrilocality and joint family household structures.

The institution of patrilocality refers to a kinship system whereby women join their husband's household upon marriage. This tradition explains why the most common reason for migration of Indian women is marriage; on average, a woman migrates 3.6 hours away from her natal home once she is married.[1] Patrilocality is strongly associated with family lineage, land, and property being passed down through male heirs. Productive assets such as agricultural land require the physical presence of sons, who must gradually take over control and management of the resources from the patriarch.

Patrilocality implies that it is usually one or more adult sons who reside with their parents, their wives and children. While nuclear households entail a couple and their children co-residing, extended or joint households can involve

[1]India Health and Development Survey 2005.

other couples, grandparents, or in-laws who live in the same residence. Adult daughters move out upon marriage. Table 21 shows that approximately 20 percent of households in India have atleast one co-residing daughter-in-law. The corresponding number is 35 and 9 percent for Pakistan and Bangladesh, respectively.

Prior literature suggests there exist both benefits and disadvantages for women residing in joint versus nuclear households. Using South Asian household data, Khalil and Mookerjee (2019) find that women residing in joint households are significantly less likely to face domestic abuse compared to women living in nuclear residences. With more members in the household, the chances of someone intervening to impede physical abuse are greater. However, women in joint households are less likely to make decisions concerning large household purchases, health care, fertility, or visits to their natal family (Khalil and Mookerjee 2019). They must defer to elders, including their mother-in-law, who is often in control of their mobility and fertility decisions (Anukriti et al. 2020).

The data suggests interesting fertility characteristics of joint households. Figures C.7 and C.8 in the appendix display the total and average fertility in India by the number of co-residing sons. As is expected, the total fertility increases as the number of co-residing sons increases from one to five. However, the average fertility remains similar across household size. Within households, the rank of the son is positively correlated to fertility. Higher ranking wives tend to have greater fertility, on average. This is apparent in Figure C.9 in the appendix. In fact, Table C.9 indicates that second ranking women tend to have approximately one child less than their first ranking counterparts.

Table 21. Percentage of Co-Residing Daughter-in-Laws by Country

Daughter-In-Laws	India (%)	Pakistan (%)	Bangladesh (%)
0	79.77	66.75	91.17
1	17.45	23.45	7.79
2	2.38	7.24	0.90
≥ 3	0.39	2.56	0.14
Total (%)	100	100	100
Households	612,102	21,192	45,698

Note: Daughter-in-laws are defined using the relationship to the household head. The table displays the proportion of households in the sample with the number of co-residing daughter-in-laws. Data is accessed from the Indian Demographic and Health Survey years 1992, 1998, 2005, and 2015.

4.2.2. Data. Demographic and Health Survey: For the main analysis in this paper, I employ the 1992, 1998, 2005 and 2015 Demographic and Health Surveys (DHS) for India. The nationally representative survey samples all women in the household aged 15-49. The DHS contains data on a woman's complete birth history, fertility preferences, and decision making power. The advantage of the DHS is that each woman in the eligible age group is interviewed, allowing me to compare women within a household, using household fixed effects.[2] However, the survey does not allow me to track women who split away from their patrilocal residence.

India Human Development Survey: To obtain information on household formation, I access the India Human Development Survey (IHDS) for the years 2005 and 2012. The IHDS is a nationally representative survey of 42,152 households which are followed and 82 percent of which are successfully re-

[2]Only ever-married women are surveyed in the 1992 and 1998 DHS.

interviewed in 2012. The advantage of the IHDS data is that most households are observed twice, allowing me to track households which restructure their residence type by splitting. However, only one woman per residence is interviewed in detail for birth history, marriage and empowerment variables. Thus, I cannot compare women within a household, as with the DHS.

The DHS defines a household as a collection of individuals who eat from the same pot. To construct a sample of joint family structures, households with a co-resident daughter-in-law, as defined by her relationship to the head of the household, are identified. The sample is then further reduced to only those households where two daughter-in-laws reside; this is the primary study sample. Each woman is ranked in order of the age of her husband; wives of the older son are ranked as first order, while wives of the younger son are given a second order rank. Note that there are other residence structures that can be categorized as joint households with two co-residing sister-in-laws. For example, the older brother may be the head of household with a co-residing younger brother and sister-in-law. These cases are fewer in the data and are not included in my sample.[3]

There are several outcomes studied in this paper. To determine the effect of rank on female empowerment in the household, I separately study decision making power and domestic violence patterns. To understand changes in fertility behavior, the outcome variable used in the main specifications is birth spacing. I explain the construction of these variables in detail below.

Fertility: The main outcome of interest is birth spacing. Birth spacing is measured in months. For the first child, the marriage to birth interval is used to measure spacing.

[3]Only 0.9 percent of women are categorized as the sister-in-law of the head of the household.

Decision Making Power: The DHS asks each interviewed woman whom in the household usually makes decisions concerning her healthcare, major household purchases, visits to her natal family, and how to spend her husband's income. The responses are categorized as the respondent alone, her husband alone, the two jointly, another household member alone, or the respondent with another household member jointly making the decision. I create separate indicators equal to 1 if a woman has atleast some decision making power in each of these categories, and 0 otherwise.

Domestic Violence: The DHS survey asks respondents if they believe it is acceptable for a husband to beat his wife if she goes out without telling him, neglects the children, argues, refuses sexual relations, or cooks poorly. I code these questions as separate indicator variables equal to 1 if the respondent finds the physical abuse acceptable, and 0 otherwise.

4.3. Methodology and Identification

4.3.1. Empirical Methodology. To understand fertility co-dependencies, I delineate three testable hypotheses. First, I test whether a higher rank is associated with improved empowerment outcomes. Second, I measure the fertility response of the first rank to the entrance of the second rank into the shared residence. Lastly, I estimate the fertility response of the second rank upon entrance, if the first rank already has a son.

To test for differences in empowerment outcomes, I run the OLS regression

$$Y_{ih} = \alpha \ \textit{Second Rank}_{ih} + \beta X_i + \gamma_h + \phi_r + \epsilon_{ih} \tag{4.1}$$

where Y_{ih} includes measures of decision making power and domestic violence incidence for a woman i, residing in household h. *Second Rank* is an indicator variable equal to 1 if a woman is married to the younger co-resident son, and

0 if she is married to the elder co-resident son. X_i is a vector of woman-level characteristics including education (in years), partner's education, and age at marriage. γ_h are household fixed effects, ϕ_r are religion fixed effects, and ϵ_{ih} captures any idiosyncratic variation.

The household fixed effects γ_h control for those characteristics within a household which might affect the outcomes of both daughter-in-laws. This might include shared household wealth, geography, household head characteristics, presence of a mother-in-law, disease environment, and the availability of labor market opportunities. Conditional on controlling for household level variation, I interpret α as the difference in empowerment outcomes Y_{ih}, that can be attributed to a lower ranking. If a higher rank is correlated with greater social status or empowerment, I should expect α to be negative.

To measure the fertility response of the first rank upon the entrance of the second rank into the household, I estimate the equation

$$\textit{Birth Interval}_{i,j,m} = \alpha \textit{After}_{i,j} + \beta \textit{No Sons}_{i,j} + \delta \textit{After*No Sons}_{i,j} + \theta X_{i,j} + \gamma_m + \lambda_b + \epsilon_{i,j,m} \quad (4.2)$$

where *Birth Interval* is measured in months, for child i born after child j to mother m. For the first child, I substitute birth interval with the marriage to first birth interval. *After* is a dummy equal to 1 if child i was born after the second ranking daughter-in-law was married, and 0 otherwise. *No Sons* is a dummy equal to 1 if the mother does not have any sons when child i is born, and 0 if she already has at least one son. The vector X includes the age and age squared of the mother at the birth of child i. γ_m and λ_b are mother and birth order fixed effects. Lastly, $\epsilon_{i,j,m}$ captures any idiosyncratic variation. If the entrance of the second ranking

woman is associated with reduced pressure on the first rank to bear children, I would expect α and δ to be positive.

Finally, to estimate the fertility response of the second rank I run the regression

$$\textit{Birth Interval}_m = \alpha \textit{Number of Sons}_m + \beta X_m + \mu_s + \phi_r + \upsilon_z + \epsilon_m \tag{4.3}$$

where *Birth Interval* is measured as the number of months from marriage to first birth for a second ranking woman m. *Number of Sons* is the number of sons already born to the first rank woman, at the time of entrance of the second daughter-in-law. I also use other measures of the first rank's fertility, such as a dummy equal to 1 if she has a son, and a measure for the proportion of sons. The vector X includes the age, age squared, education, and partner's education for the second rank woman m. μ_s, ϕ_r, and υ_z are state, religion, and survey fixed effects. As before, ϵ_m captures any idiosyncratic variation.

4.3.2. Identification Strategy. My identification strategy exploits the rank of each daughter-in-law, defined using the age of her husband, as well as the timing of the entrance of the second ranking daughter-in-law into the residence.

A major identifying assumption necessary for causal inference is that high and low ranking women are not significantly different prior to marriage. In other words, women should not be selecting into marriage based on the birth order of potential matches. In their research, Coffey, Khera, and Spears (2016) experimentally vary potential partner characteristics using hypothetical matrimonial ads in India to show that arranged marriages do not respond to the birth ranking of grooms. While I cannot offer similar causal evidence against selection into rank, in Table 22 I provide a summary of baseline characteristics for first and second rank women.

As expected, the baseline characteristics for a respondent's current age and partner's current age vary significantly across rank. Higher ranking women and their partners tend to be approximately five years older than lower ranking women and their husbands. Height- a partial measure of early health-, religion and partner's education are not significantly different across ranks. However, the statistics do suggest that second rank daughter-in-laws tend to have half a year more education and marry three months later than their elder sister-in-laws, on average. To ensure that the results are not driven by differences in age at marriage or education, I will control for both in the estimated regressions.

Table 22. Selection into Rank: Characteristics by Rank of Daughter-in-law

Statistic	Rank 1	Rank 2	Difference
Current Age	30.43	25.35	-5.08***
	(6.28)	(5.12)	
Partner's Current Age	35.32	29.63	-5.69***
	(6.43)	(5.50)	
Education (Years)	6.52	7.08	0.56***
	(5.19)	(5.18)	
Partner's Education (Years)	9.11	9.02	-0.09
	(4.52)	(4.52)	
Age at Marriage	18.45	18.64	0.19**
	(3.56)	(3.53)	
Height (cm)	1527	1528	1
	(58.58)	(61.11)	
Hindu = 1	0.79	0.79	0
	(0.41)	(0.41)	
N	4327	4327	-

Note: Data is accessed from the DHS 2015, 2005, 1998 and 1992 survey years for India. *Rank 1* refers to the wife of the elder co-resident son. *Rank 2* refers to the wife of the younger co-resident son. For differences: *p<0.1; **p<0.05; ***p<0.01

4.4. Results

In Table 23, I show that a higher rank is associated with higher decision making power. The table shows that after controlling for differences in own and partner's education, as well as age at marriage, women who marry the younger co-resident son have less decision making say over large household purchases, visits to health-care facilities, visits to their natal family, and spending over their partner's income. In column (3), the results suggest that marrying the second ranking co-resident son is associated with a 2.4 percent lower likelihood of visiting natal family. The estimate translates to a 5.7 percent magnitude reduction relative to the mean outcome. Similarly, women marrying the younger co-resident son are 3.7 percent less likely to have a say in accessing health-care, relative to the average. The results are statistically significant for all outcomes.

Interestingly, a lower rank is not associated with a higher likelihood of facing domestic violence. In Table 24, I show that women who marry the younger co-resident son are not significantly more likely to be beaten if they go out, neglect childcare, argue, refuse sexual relations, or cook food poorly. These results are in line with earlier research suggesting women in joint household residences are less likely to face domestic abuse because of the presence of potential mediators.

Next, in Table 25 I show the main results for the fertility response of the first rank upon the entrance of the second rank into the residence. The results in column (1) suggest that first ranking women increase birth spacing by 3.25 months with the entrance of the second daughter-in-law in the household, conditional on the birth order of the child and age of the mother. In column (2), I also include

Table 23. Effect of Rank on Empowerment/Decision-Making

	Atleast Some Decision-Making Power On:			
	1(=Healthcare Access)	1(=Large Purchases)	1(=Family visits)	1(=Partner's Income)
	(1)	(2)	(3)	(4)
No Controls				
1(= Second Rank)	−0.014	−0.014	−0.021**	−0.039***
	(0.008)	(0.009)	(0.008)	(0.015)
With Controls				
1(= Second Rank)	−0.017**	−0.017*	−0.024***	−0.043***
	(0.009)	(0.009)	(0.009)	(0.015)
Education (years)	0.005**	0.005**	0.004*	0.006*
	(0.002)	(0.002)	(0.002)	(0.004)
Partner's Education	−0.0004	−0.001	−0.0003	−0.001
	(0.001)	(0.001)	(0.001)	(0.002)
Age at Marriage	0.001	0.002	0.002	−0.005
	(0.003)	(0.003)	(0.002)	(0.004)
Religion FE	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Mean Outcome	0.45	0.39	0.42	0.61
Observations	12,084	12,084	12,084	6,041
R^2	0.780	0.763	0.780	0.714
Adjusted R^2	0.561	0.527	0.561	0.396

Note: The sample is restricted to households with two co-residing daughter-in-laws. *Second Rank* is an indicator equal to 1 if the respondent is married to the younger brother in the household. *Healthcare Access*, *Large Purchases*, *Family Visits*, and *Partner's Income* are indicator variables equal to 1 if the respondent has some say in the usual decision making over accessing healthcare for herself, making large household purchases, visiting relatives or family, and spending partner's earned money, respectively. *p<0.1; **p<0.05; ***p<0.01

Table 24. Effect of Rank on Empowerment

	Beating Justified if Wife:				
	1(=Goes Out)	1(=Neglects Childcare)	1(=Argues)	1(=Refuses Sex)	1(=Cooks Poorly)
	(1)	(2)	(3)	(4)	(5)
No Controls					
1(= Second Rank)	−0.004	0.002	0.005	0.007	−0.004
	(0.012)	(0.013)	(0.013)	(0.010)	(0.011)
With Controls					
1(= Second Rank)	0.003	0.008	0.011	0.012	0.001
	(0.012)	(0.013)	(0.013)	(0.010)	(0.012)
Education (years)	−0.007**	−0.005*	−0.007**	−0.006**	−0.005*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Partner's Education	−0.002	−0.001	−0.001	0.0001	0.0003
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Age at Marriage	−0.001	−0.003	−0.0004	0.002	−0.0001
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Religion FE	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓
Mean Outcome	0.21	0.26	0.23	0.11	0.16
Observations	5,522	5,522	5,522	5,522	5,522
R^2	0.713	0.707	0.693	0.672	0.688
Adjusted R^2	0.415	0.404	0.376	0.333	0.365

Note: The sample is restricted to households with two co-residing daughter-in-laws. *Second Rank* is an indicator equal to 1 if the respondent is married to the younger brother in the household. *p<0.1; **p<0.05; ***p<0.01

include mother fixed effects to control for variation at the individual level which remains fixed across births, such as education. While the coefficient size decreases significantly with the inclusion of mother fixed effects, the results still indicate that first ranking women significantly increase spacing by 1.1 months on average, after the entrance of the second rank. The coefficient translates to a 3.6 percent increase in birth spacing, relative to the average birth interval of 30.76 months.

Next, I test whether first ranking women change their birth spacing behavior, if they already have sons. In columns (3) and (4) of Table 25, I include a control for whether a woman has any sons from previous births interacted with *After*. The results in column (4) suggest that women are likely to increase birth spacing by 4.95 months on average after the entrance of the second rank, if they do not already have sons. The coefficient translates to an approximately 16 percent increase from the average birth interval. In columns (2) and (4) of Table 26, I show that the change in birth intervals is not explained by a significant change in early childhood investments, as measured by the number of months a child is breastfed. Taken together, the results provide suggestive evidence that first ranking daughter-in-laws increase birth spacing after the entrance of the second rank if they do not already have sons, but this increase is not accompanied by a change in health investments for their children.

Finally, to estimate the fertility response of the second rank, I run regression equation (3). The sample is restricted to second ranking daughter-in-laws, and the outcome of interest is a woman's marriage to first birth interval. The results in Table 27 suggest that second ranking women do change their first birth interval conditional on the fertility of the first daughter-in-law. Yet, what matters is the number of sons; each additional son born to the first rank is associated with

Table 25. Birth Spacing for First Rank Daughter-in-Laws

	Birth Interval (months)			
	(1)	(2)	(3)	(4)
After	3.248***	1.100*	1.993***	−0.956
	(0.490)	(0.638)	(0.588)	(0.727)
Age	1.601***	16.547***	1.594***	16.466***
	(0.169)	(0.657)	(0.165)	(0.653)
Age^2	0.023***	−0.166***	0.023***	−0.165***
	(0.003)	(0.012)	(0.003)	(0.012)
No Sons			−1.606***	−2.125***
			(0.603)	(0.708)
After*No Sons			2.483***	4.953***
			(0.874)	(1.014)
DHS Cluster FE	✓		✓	
Survey FE	✓		✓	
Birth Order FE	✓	✓	✓	✓
Mother FE		✓		✓
Mean Outcome	30.76	30.76	30.76	30.76
Observations	16,108	16,108	16,108	16,108
R^2	0.389	0.630	0.389	0.632
Adjusted R^2	0.224	0.411	0.225	0.413

Note: *After* is an indicator equal to 1 if the child is born after the entrance of the second daughter-in-law, and 0 otherwise. *No Sons* is an indicator equal to 1 if the first rank does not have any sons prior to the birth, and 0 if she already has at least one son. Result is robust if sample is restricted to only those households where both daughter-in-laws have resided in the residence since marriage (no migration in or out). For columns (1) and (3), clustering is at the PSU level and for columns (2) and (4), clustering is at the mother level. *p<0.1; **p<0.05; ***p<0.01

Table 26. Early Investment Changes

	Breastfeeding (months)			
	(1)	(2)	(3)	(4)
After	−4.709***	−2.492	−5.457***	−3.096
	(1.070)	(2.575)	(1.444)	(3.318)
Age	−0.092	−5.114	−0.095	−4.995
	(0.076)	(4.011)	(0.076)	(4.020)
Age^2	−0.004***	−0.017	−0.004***	−0.019
	(0.001)	(0.074)	(0.001)	(0.074)
1(=Male)	0.436	−0.448	0.462	0.148
	(0.737)	(1.348)	(0.756)	(1.608)
No Sons			−1.237	−2.794
			(1.951)	(4.525)
After*No Sons			1.465	0.966
			(2.000)	(4.046)
SU Cluster FE	✓		✓	
Survey FE	✓		✓	
Birth Order FE	✓	✓	✓	✓
Mother FE		✓		✓
Mean Outcome	15.24	15.24	15.24	15.24
Observations	3,592	3,592	3,592	3,592
R^2	0.680	0.917	0.681	0.918
Adjusted R^2	0.243	0.528	0.243	0.529

Note: The sample is restricted to the first ranking woman, within households with two co-residing daughter-in-laws. Data is accessed from the Indian DHS survey years 1992, 1998, 2005, 2015. Result is robust if sample is restricted to only those households where both daughter-in-laws have resided in the residence since marriage (no migration in or out). For columns (1) and (3), clustering is at the PSU level and for columns (2) and (4), clustering is at the mother level. *p<0.1; **p<0.05; ***p<0.01

Table 27. Second Rank: Marriage to First Birth Interval

	Marriage to First Birth (months)					
	(1)	(2)	(3)	(4)	(5)	(6)
Rank 1: Number of Kids	−1.262***					
	(0.382)					
Rank 1: Prop. Sons		−1.251				
		(1.964)				
Rank 1: No Son			3.575***	1.720		
			(1.254)	(1.226)		
Rank 1: Number of Sons					−1.851***	−1.039*
					(0.572)	(0.539)
Age at Marriage	−1.333***	−1.158***	−1.345***	−2.349***	−1.362***	−2.352***
	(0.246)	(0.300)	(0.242)	(0.307)	(0.242)	(0.306)
Education	−0.299*	−0.113	−0.291*	−0.140	−0.295*	−0.140
	(0.168)	(0.206)	(0.169)	(0.175)	(0.169)	(0.175)
Partner's Education	0.197	0.072	0.193	0.077	0.189	0.073
	(0.180)	(0.198)	(0.180)	(0.172)	(0.178)	(0.171)
PSU Cluster FE	✓	✓	✓	✓	✓	✓
Survey FE	✓	✓	✓	✓	✓	✓
Religion/Caste FE	✓	✓	✓	✓	✓	✓
Mother Birth-Year FE				✓		✓
Mean Dep.	22.05	22.05	22.05	22.05	22.05	22.05
Mean Indep.	1.46	0.54	0.47	0.47	0.77	0.77
Observations	3,112	2,175	3,112	3,112	3,112	3,112
R^2	0.593	0.620	0.592	0.647	0.593	0.648
Adjusted R^2	0.169	0.157	0.169	0.259	0.169	0.259

Note: The dependent variable is the marriage to first birth interval, measured in the number of months. *No Sons* is an indicator equal to 1 if the first rank does not have any sons prior to the birth, and 0 if she already has at least one son. *Prop. Sons* is the proportion of children of first rank who are male. *p<0.1; **p<0.05; ***p<0.01

1.04 fewer months to the birth of the second rank's child on average. This estimate from column (6) corresponds to a 4.7 percent decrease at the average marriage to first birth interval. Interestingly, the proportion of children that are male or the total number of kids by the first rank do not have a significant association with the first birth interval of the second rank.

4.5. Conclusion

A major policy goal in developing countries is the achievement of improved female empowerment; higher autonomy for women in the household is linked to greater labor force participation, better child nutrition, and more say in fertility and financial decisions. In the past, economists have focused on policy interventions which improve the bargaining power of women relative to men in the household with continued debate on what "works". Prior research has also shown that women themselves invest in methods to improve their own autonomy. A prominent example is the effort to birth a son, as male offspring provide old age security and a claim to household property.

In this research, I suggest that female autonomy in joint Indian households is determined by both a woman's social status relative to other women within the household, and her ability to bear sons. I show that first ranking women increase birth spacing by 9.1 percent on average, upon the entrance of a younger sister-in-law. On the other hand, second ranking women reduce their marriage to first birth interval by approximately 4.8 percent if the first rank already has a son. The results provide evidence for fertility co-dependencies within joint residence households, and have significant implications for policy targeting and effectiveness in developing countries.

CHAPTER V

CONCLUSION

Family formation and composition are significant determinants of female employment, autonomy, and fertility choices. Yet, family formation decisions are often constrained by prevailing social norms and traditions. For example, norms can mediate or restrict when to marry, disincentivize deviations from arranged marriage customs, and dictate household structures. In my dissertation, I highlight the importance of social norms for family formation and composition in three distinct South Asian contexts, with a focus on the consequences to women.

In Chapter II of the dissertation, I study the interaction of early marriage social norms with statutory law. I show that a three year increase in the legal age of marriage for women led to a significant decrease in early marriage rates. Moreover, women who delay marriage are not worse off in terms of their decision making power after marriage or during the marriage matching process. The study contributes to our understanding of the role of legislation and the unique power of governments to affect practice of customs through signals of what is approved behavior.

In Chapter III, I analyze honor killings - a specific form of violence against women which arises from the skewed incentives placed by arranged marriage markets on conserving the sexual modesty of women. Crossing these social boundaries can be costly for families who must bear the financial responsibility of any unmarried women in the family. While much of public discourse has labeled the crimes as culturally or religiously motivated, this research provides the first empirical evidence that honor killings are partly motivated by agricultural

income shocks. The results offer a significant contribution to public and academic discourse on honor killings.

Finally, in Chapter IV, I study the tradition of patrilocality which often results in multiple sons co-residing with their parents and natal families. I hypothesize that a woman's status is jointly determined by her husband's relative age and the gender of her children. I show that older women have greater empowerment and a higher share of total household fertility. Younger women in the household therefore reduce birth spacing, in an effort to birth a son as a means to improving their relative autonomy. This research highlights possible fertility rivalry among co-residing women in joint household residences.

Policymakers in South Asia have made numerous attempts to reduce gender gaps both within and outside the household, with varying success. A contributing factor to persistent gender gaps are the norms which shape the environment and context for policy interventions. This dissertation contributes to the necessary step for improving policy effectiveness-an enhanced understanding of how norms can affect decisions at the household level.

APPENDIX A

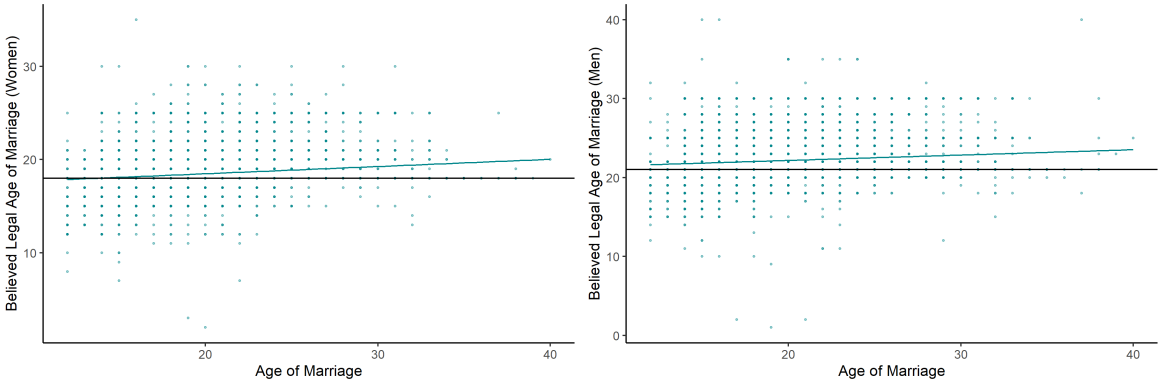
APPENDIX FOR CHAPTER II

Table A.28. Data Sources

Data	Source	Description
Female age of marriage	Demographic and Health Survey (DHS)	1998, 2005
	India Human Development Survey	2005
Male age of marriage	Demographic and Health Survey (DHS)	2005
Knowledge of age of marriage	Demographic and Health Survey (DHS)	1992
Political leadership (Janata Seats)	Statistical Report, General Elections 1977	State level
Police	Census of India	State level
Population	Census of India	State level
Rainfall/Drought	University of Delaware, Terrestrial Precipitation (V 3.01)	District level
Sex Ratio	Census of India	State level

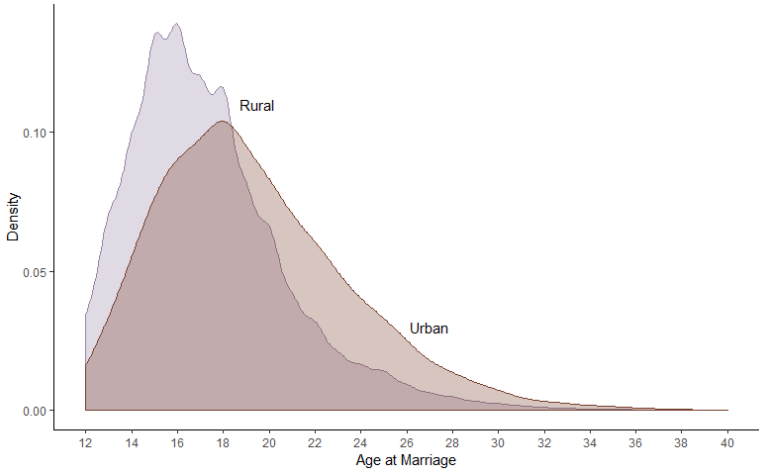
Note: The table reports the different sources of data employed in the study, with descriptions of usage.

Figure A.7. Reported Legal Age of Marriage



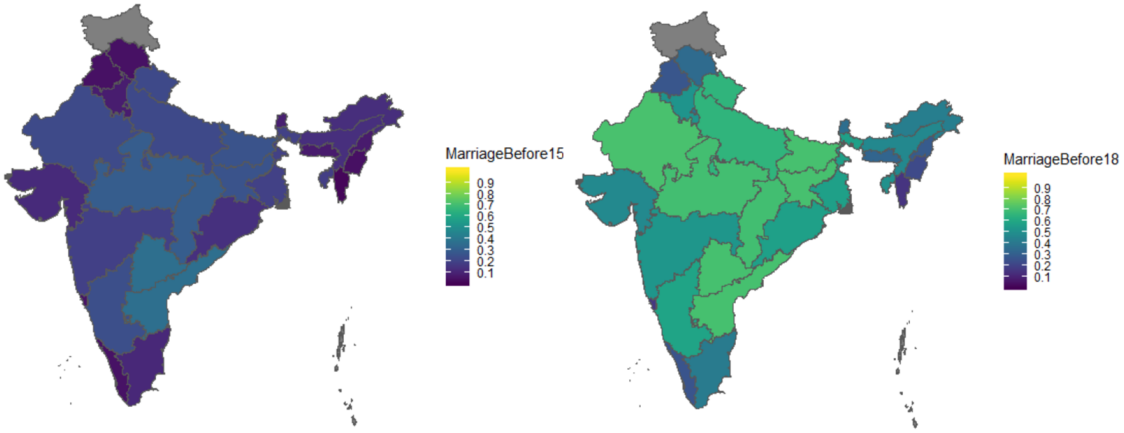
Note: Data is accessed from the 1992 DHS survey. The figure plots own reported age of marriage against the believed legal age of marriage for men and women separately. The horizontal line represents the true legal age of marriage, which is 18 for women and 21 for men.

Figure A.8. Age of Marriage Distribution by Area



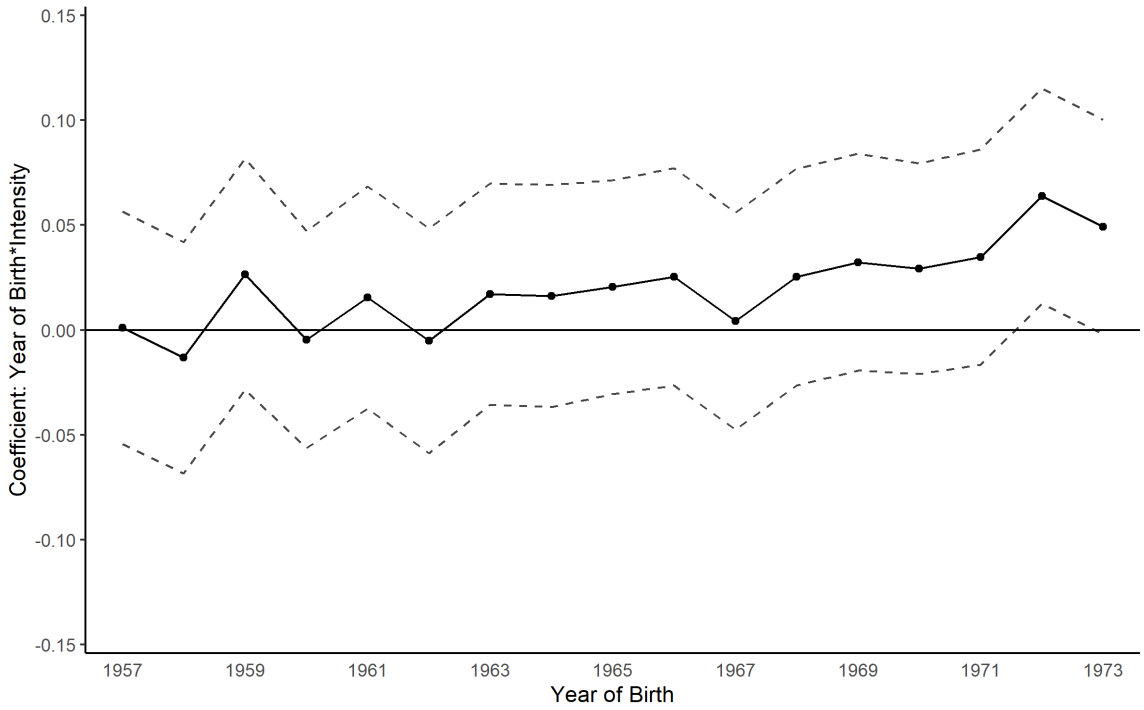
Note: This figure shows the distribution of age at marriage by the respondent's region of residence in childhood. The sample includes women who marry once, between the ages 12 to 40. Data is accessed for India from the 1998 and 2005 Demographic and Health Survey.

Figure A.9. Variation in Early Marriage by State



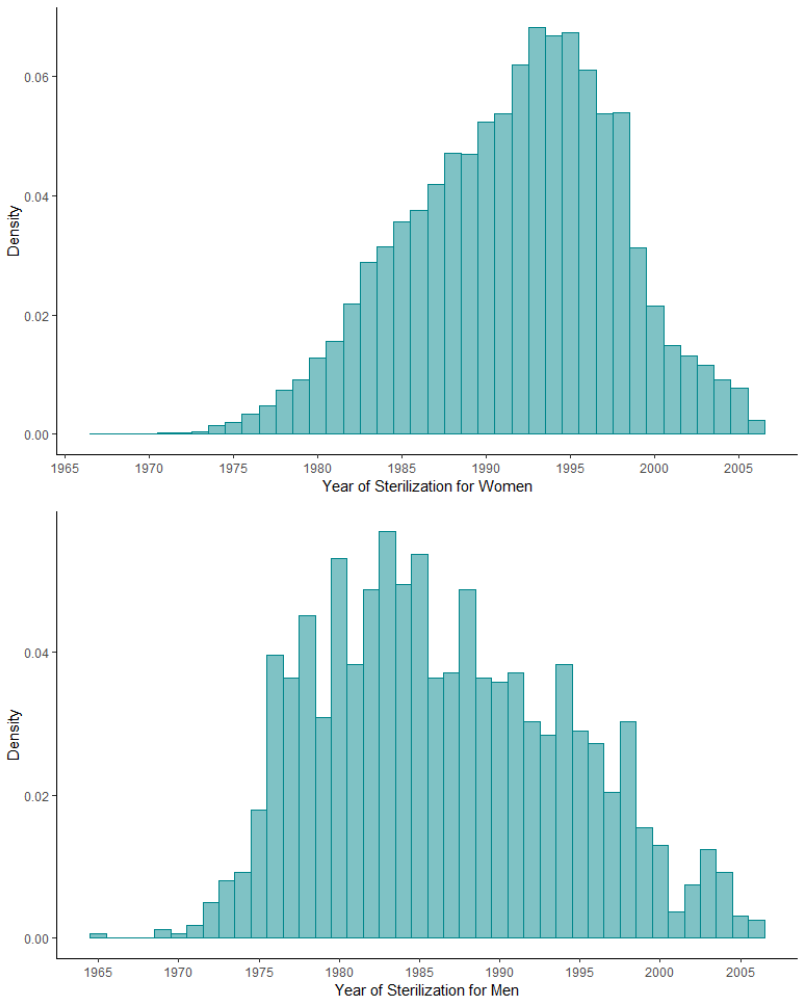
Note: The maps show the geographical variation in the pre-policy probability of marriage before 15 and 18, respectively, by state.

Figure A.10. Probability of Marriage by Cohort and Marriage Market Norms



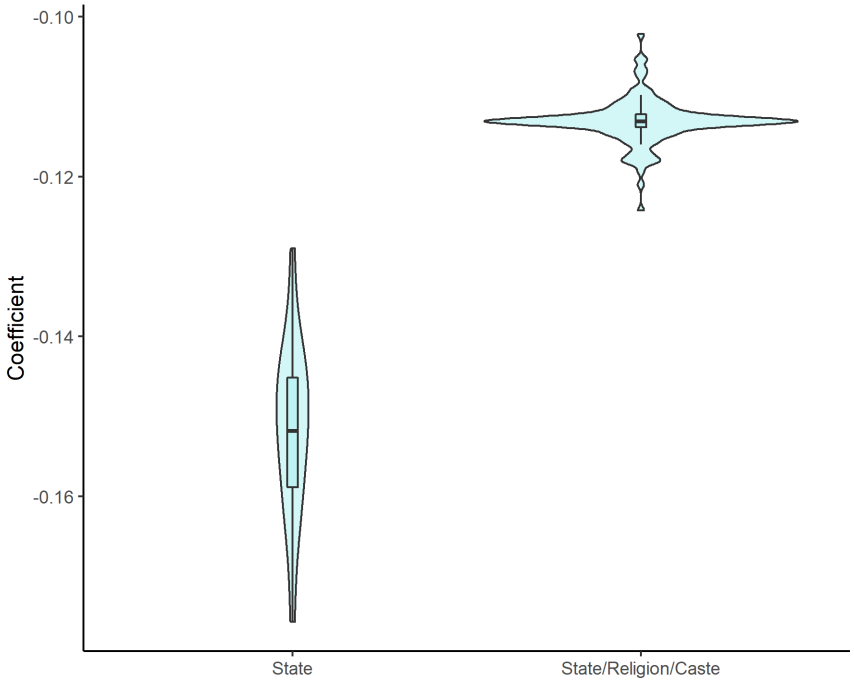
Note: The figure plots the interaction coefficients of a cohort dummy and the marriage market probability of entering marriage, *Year of Birth*Intensity*, from a regression where the dependent variable is equal to 1 if an individual is married and 0 otherwise. A marriage market is defined at the state-religion-caste level. Data is accessed from the 2005 DHS survey for India.

Figure A.11. Distribution of Sterilization by Year



Note: The figure plots the year of sterilization for the sample of women (top panel) and men (bottom panel) in my data who are sterilized.

Figure A.12. Coefficient Robustness to Exclusion of Markets



Note: The violin plot displays the distribution of coefficients for the variable *Treated*Intensity* estimated by rerunning the analysis after dropping each marriage market at a time. Results for state and state/religion/caste level marriage markets are shown separately. Data is accessed from the DHS.

Table A.29. Robustness: Inclusion of Sex Ratios

	1(=Marriage Before 15)	1(=Marriage Before 18)
	(1)	(2)
Treated*Intensity	-0.082^{***}	-0.102^{***}
	(0.021)	(0.036)
Sex Ratio	0.00001	0.0001^{**}
	(0.00001)	(0.00003)
Cohort FE	X	X
Marriage Market FE	X	X
Religion/Caste FE	X	X
Observations	68,836	68,836
R^2	0.085	0.166
Adjusted R^2	0.080	0.162

Note: The variable *Sex Ratio* measures the number of females per 1000 males in the state and year of birth for each individual. Data for sex ratios was accessed from the Indian Census and linearly interpolated wherever necessary. All regressions include survey fixed effects. Standard errors are clustered at the marriage market level. *p<0.1; **p<0.05; ***p<0.01

Table A.30. Robustness: Hazard and Logit Models

	Hazard		Logit	
	1(= Married Before 15)	1(= Married Before 18)	1(= Married Before 15)	1(= Married Before 18)
	(1)	(2)	(3)	(4)
Treated*Intensity	-0.011^{**}	-0.064^{***}	-3.184^{***}	-3.011^{***}
	(0.005)	(0.010)	(0.784)	(0.363)
District FE	X	X	X	X
Age FE	X	X		
Cohort FE	X	X	X	X
Religion/Caste FE	X	X	X	X
Mean Dependent Variable	0.013	0.052	0.082	0.372
Observations	38,580	38,580	7,033	7,033
R^2	0.051	0.097		
Adjusted R^2	0.046	0.092		
Log Likelihood			-1,504.721	-3,606.639
Akaike Inf. Crit.			3,947.442	8,151.278

Note: Data is accessed from the IHDS 2005 survey. Marriage markets are defined at the district level. *Intensity* is the pre-policy early marriage norm in a marriage market. *Treated* is a binary variable equal to 1 for individuals younger than 15 and 0 for individuals older than 18 when the policy was enacted. The sample is panelized for each individual. *p<0.1; **p<0.05; ***p<0.01

APPENDIX B

APPENDIX FOR CHAPTER III

Figure B.1. Examples of Honor Killings

Qandeel Baloch smothered to death by brother

MULTAN (Online): Social media fame girl Qandeel Baloch was smothered to death allegedly by her brother in Multan on Saturday.

Qandeel Baloch, originally Fauzia Saeed, who had been spending days with her parents after she returned home for Eid, was asleep at her parent's home inGreen Town in Multan, when her brother, identified as Waseem entered the house and smothered her to death before fleeing, police said.

Senior police officer Sultan Azam said that the suspect had fled the city after committing the crime.

The father of Qandeel Baloch told the police that video clips may have enraged his son and he killed her. He said that she was tearing their honour into pieces in TV programmes.

Qandeel had also sought security from the interior ministry some days but the interior ministry said that the reasons she furnished for security did not warranty security.

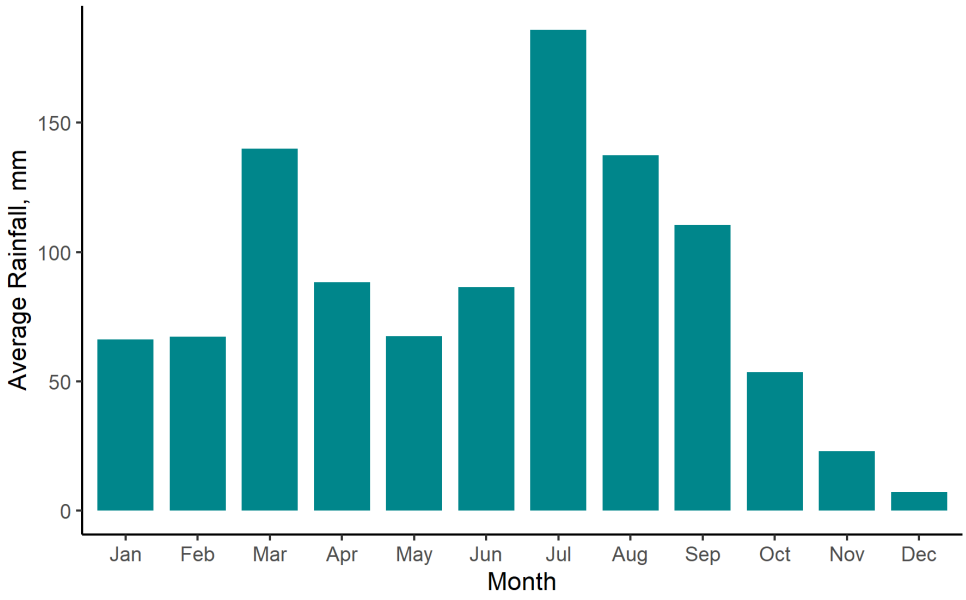
Qandeel had also demanded that action be initiated against those who had made public documents related to her identity public on social media. She also said that her life was under threat and she was receiving threatening telephonic calls from unknown persons.

Man strangles wife for 'honour'

TOBA TEK SINGH: A man allegedly killed his wife for honour at Chak Akbar Sahu on Monday. Pervez Ahmad doubted that his 20-year-old wife Bushra had developed relations with a youth. On the day of the incident, Pervez allegedly strangled his wife. The police have shifted the body to the Arrouti Rural Health Centre for autopsy. The police are conducting raids to arrest the accused. —PTR

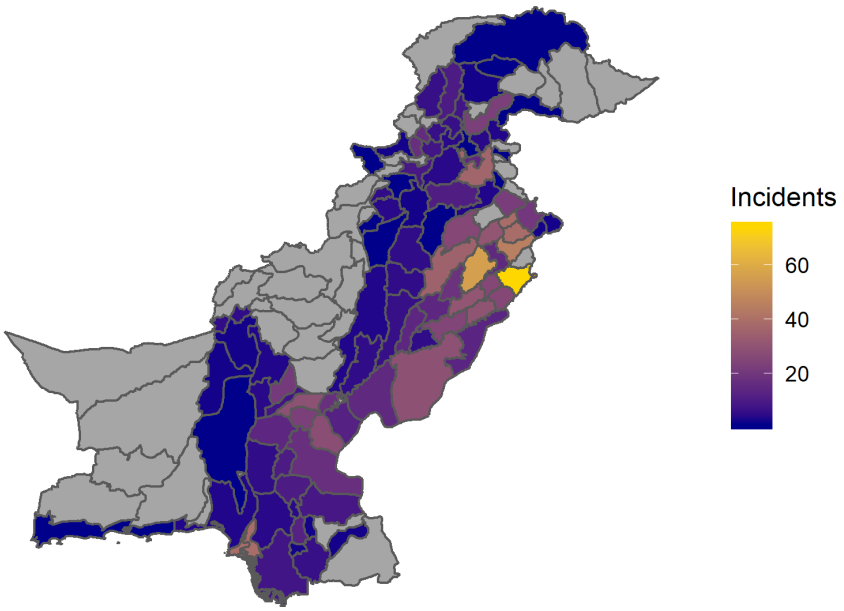
Note: The figure includes two examples of honor killing reports included in the data set.

Figure B.2. Average monthly rainfall (2014-2017)



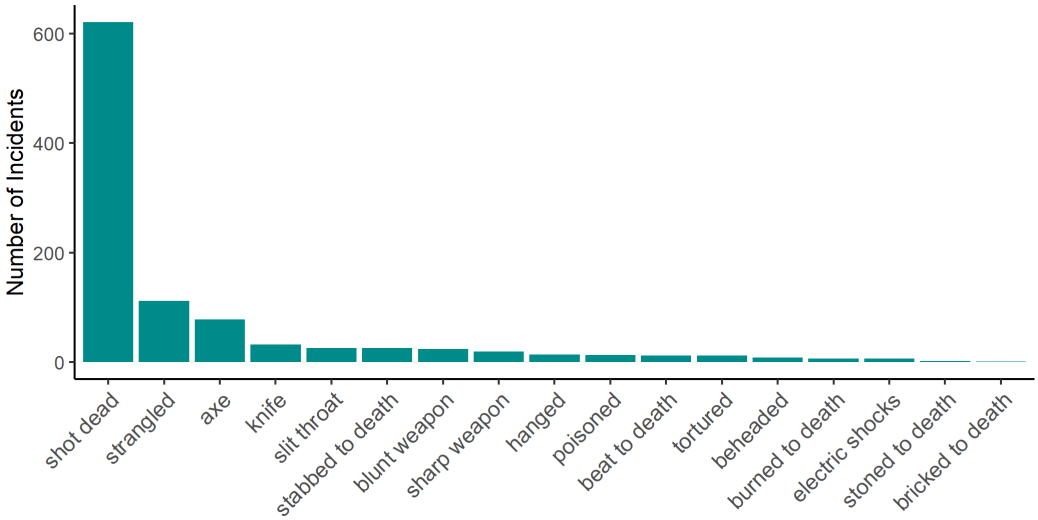
Note: Average monthly rainfall, in mm, for districts in Pakistan for my sample period April 2014 to August 2017. Data is accessed from the Global Precipitation Measurement v6 (GPM) dataset.

Figure B.3. Reported Honor Killing Incidents by District



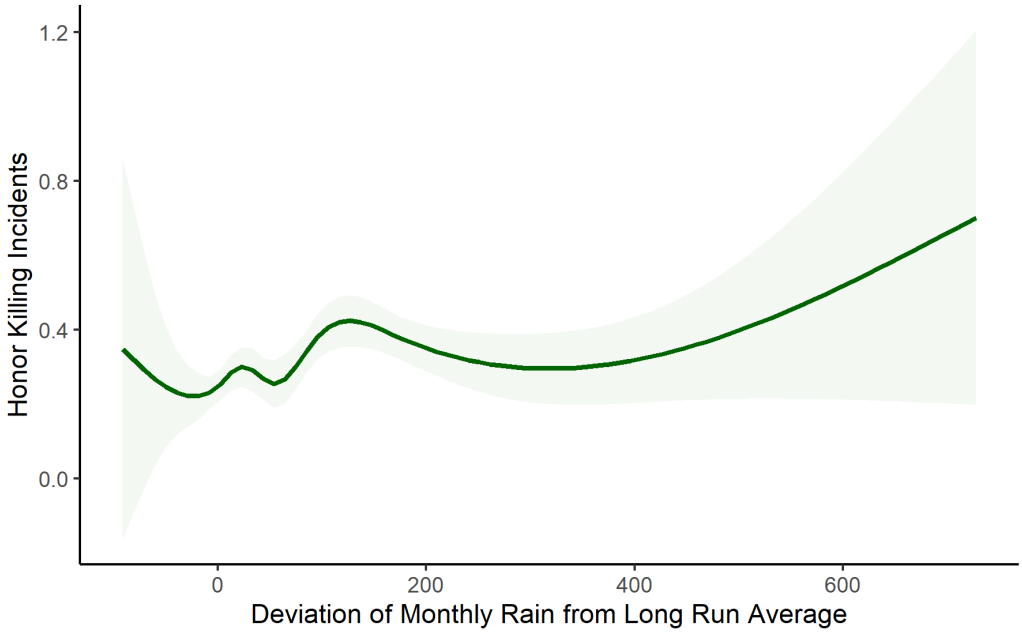
Note: Data spans the months April 2014 to August 2017. The figure shows total incidents in the sample, per district.

Figure B.4. Reported Incidents by Method of Crime



Note: The figure displays the sample of incidents by the method used to commit the murder.

Figure B.5. Incidents Against Rainfall Shock Deviations



Note: Honor killing incidents against deviations of district-time rainfall shocks (in mm) from the long run average (1970-2010).

Table B.1. Correlations: Reported Incidents with Domestic Violence (With Lahore)

	Incidents			
	(1)	(2)	(3)	(4)
Bad Name to Family = 1	8.211		4.594	3.647
	(7.353)		(6.808)	(7.757)
Blood Relation to Partner = 1	−1.986		−19.137	−24.922
	(20.688)		(19.558)	(22.165)
Partner Choice = 1	−7.861		9.236	16.998
	(14.666)		(14.064)	(16.547)
Domestic Violence				
Controlling Behavior		−49.460**	−47.043*	−61.107**
		(24.503)	(24.821)	(28.762)
Physical Violence		−2.973	−13.097	−25.823
		(40.246)	(41.767)	(47.102)
Emotional Violence		37.563	40.489	47.700
		(26.361)	(27.146)	(32.209)
Violence from Natal Family		−1.648	−7.228	14.437
		(65.135)	(67.437)	(83.729)
Population	0.00001***	0.00001***	0.00001***	0.00001***
	(0.000)	(0.000)	(0.000)	(0.000)
Province Fixed Effects	✓	✓	✓	✓
Socio-economic Controls				✓
Observations	73	72	72	72
R^2	0.567	0.665	0.675	0.697
Adjusted R^2	0.505	0.610	0.602	0.562

Note: The dependent variable is the number of incidents per district during the period April 2014 to August 2017. The mean of the dependent variable is 14.2 incidents. The independent variables are selected from the Demographic and Health Survey 2017, and are district level averages for female respondents between the ages 15-49. `Bad Name to Family` indicates the percentage of respondents facing domestic violence who did not seek help from fear of bringing a bad name or shaming her family. `Blood Relation to Partner` and `Partner Choice` indicate the percentage of respondents in a district who are related to their partner by blood and who had some say in choosing their partner, respectively. `Controlling Behavior` includes displays of jealousy, attempts to track or restrict movements, and accusations of unfaithfulness. `Physical Violence` refers to the proportion of women who have been punched, kicked or beaten by their partner. `Emotional Violence` refers to being humiliated, made to feel bad or insulted by a partner. `Socioeconomic Controls` include average wealth in the district, population, average probability of working before and after marriage, fertility, average age of cohabitation, education, partner's education, and average probability of phone and bank account ownership. `Population` is measured using the 2017 Population Census from the Pakistan Bureau of Statistics. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

Table B.2. Correlations: Reported Incidents with Socio-Economic Indicators

	Incidents		
	(1)	(2)	(3)
Economic Correlates			
Wealth Index (0-7)	0.855		2.459
	(3.803)		(4.816)
Own Bank Account = 1	21.429		14.005
	(64.578)		(68.723)
Own Phone = 1	−13.238		−14.238
	(19.954)		(27.389)
Worked Before Marriage = 1	−21.627		−17.929
	(33.483)		(37.117)
Worked After Marriage = 1	7.800		2.419
	(32.872)		(36.320)
Social Correlates			
Education (years)		1.294	1.709
		(2.143)	(2.402)
Partner Education (years)		−0.641	−1.197
		(1.934)	(2.301)
Children		−0.699	−1.936
		(6.313)	(7.038)
Age at Cohabitation		−2.417	−2.637
		(2.144)	(2.325)
Population	0.00001***	0.00001***	0.00001***
	(0.000)	(0.000)	(0.000)
Province FE	✓	✓	✓
Observations	73	73	73
R^2	0.564	0.566	0.575
Adjusted R^2	0.485	0.496	0.464

Note: Dependent variable is the number of incidents per district during the period April 2014 to August 2017. The mean of the dependent variable is 14.23 incidents. The independent variables are selected from the Demographic and Health Survey 2017, and are district level averages for female respondents between the ages 15-49. `Own Phone` and `Own Bank Account` indicate the percentage of respondents in a district who own a mobile phone and bank account, respectively. `Population` is measured using the 2017 Population Census from the Pakistan Bureau of Statistics. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

Table B.3. Effect of Rainfall Shocks on Honor Killing Incidents (Negative Binomial)

	Incidents					
	(1)	(2)	(3)	(4)	(5)	(6)
Wet Shock (t + 4)					-0.0002	-0.0003
					(0.0008)	(0.0006)
Dry Shock (t + 4)					-0.0099	-0.0045
					(0.0065)	(0.0060)
Wet Shock (t + 3)					-0.0005	0.00001
					(0.0008)	(0.0006)
Dry Shock (t + 3)					-0.0059	-0.0023
					(0.0065)	(0.0062)
Wet Shock (t + 2)					0.0007	0.0008
					(0.0007)	(0.0005)
Dry Shock (t + 2)					-0.0045	-0.0060
					(0.0072)	(0.0066)
Wet Shock (t + 1)					0.0009	0.0009*
					(0.0007)	(0.0005)
Dry Shock (t + 1)					0.0002	-0.0025
					(0.0071)	(0.0066)
Wet Shock	0.0013***	0.0011**	0.0012**	0.0003	0.0011	0.0018***
	(0.0004)	(0.0005)	(0.0005)	(0.0006)	(0.0007)	(0.0005)
Dry Shock	0.0030	0.0009	0.0006	0.0031	-0.0008	0.0011
	(0.0060)	(0.0066)	(0.0074)	(0.0060)	(0.0077)	(0.0076)
Wet Shock (t - 1)		0.0006	0.0007	0.0003	0.0011	0.0015**
		(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0007)
Dry Shock (t - 1)		-0.0006	-0.0014	0.0023	-0.0012	0.0001
		(0.0056)	(0.0061)	(0.0062)	(0.0065)	(0.0062)
Wet Shock (t - 2)		-0.0006	-0.0006	-0.0010*	-0.0003	0.000013
		(0.0005)	(0.0005)	(0.0005)	(0.0006)	(0.0005)
Dry Shock (t - 2)		-0.0119*	-0.0127	-0.0063	-0.0130	-0.0169**
		(0.0071)	(0.0078)	(0.0077)	(0.0088)	(0.0083)
Wet Shock (t - 3)		0.0005	0.0005	0.0004	0.0013**	0.0011**
		(0.0004)	(0.0004)	(0.0005)	(0.0007)	(0.0005)
Dry Shock (t - 3)		0.0007	-0.0007	0.0054	-0.0010	-0.0029
		(0.0063)	(0.0072)	(0.0080)	(0.0116)	(0.0098)
Temperature	✓	✓	✓	✓	✓	✓
District	✓	✓	✓	✓	✓	✓
Year		✓	✓	✓	✓	✓
District*Year			✓	✓	✓	✓
Month				✓	✓	
Observations	3,075	2,850	2,130	2,130	1,930	1,930
AIC	3,935.25	3,680.30	3,808.93	3,801.41	3,415.40	3,414.50

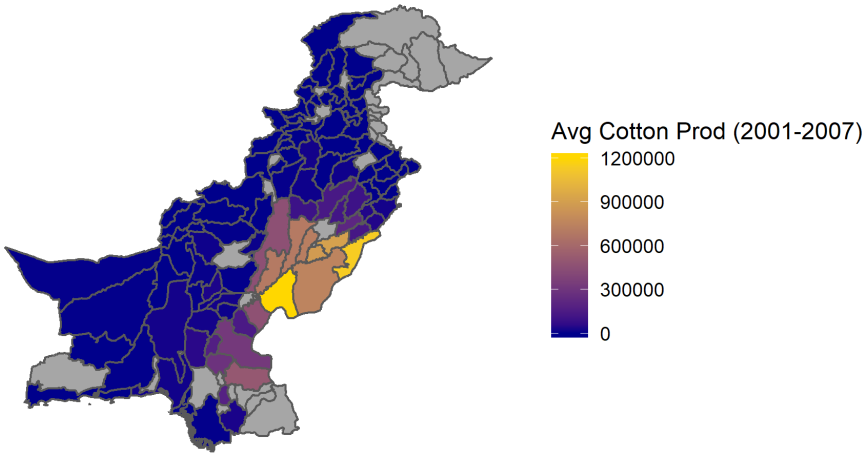
Note: The table presents results from Negative Binomial regressions where the dependent variable is the number of honor killing incidents in a district and month-year. `WetShock` and `DryShock` are the positive and negative absolute deviations of rainfall from the long run mean rainfall in a district-month (in mm), respectively. The sample covers 75 districts for 41 months. Clustered (district) standard-errors in parenthesis. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

Table B.4. Severity of lagged rainfall shocks on honor killings

	Incidents	
	(1)	(2)
Wet Shock (0.375, 1.125]	0.20648	0.17316
	(0.16301)	(0.17515)
Wet Shock (1.125, 1.875]	0.26637*	0.18417
	(0.13620)	(0.14037)
Wet Shock (1.875, 2.625]	0.13269	0.03316
	(0.16920)	(0.18555)
Wet Shock (2.625, 3.375]	0.17282	0.09137
	(0.17467)	(0.18056)
Wet Shock (3.375, 4.125]	0.12863	0.08808
	(0.22822)	(0.25389)
Wet Shock (> 4.125)	0.16808	0.09370
	(0.14784)	(0.15776)
Dry Shock (<= -0.375)	0.09420	0.01373
	(0.12779)	(0.13725)
District	✓	✓
Year	✓	✓
District*Year		✓
Observations	3,000	2,252
Adj-pseudo R^2	0.17072	0.0331
AIC	3,909.65	4,012.97

Note: Results from a Poisson estimation where the dependent variable is the number of honor killing incidents in a district and month-year. `WetShock` and `DryShock` are separated into bins of 0.75 standard deviations; the variables are indicator variables. The reference category is 0.375 standard deviations around 0, the mean. Clustered (district) standard-errors in parenthesis. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

Figure B.6. Average Cotton Production (in tonnes)



Note: Data for raw cotton production spans the years 2001 to 2007, where production is measured in tonnes. Data accessed from the Pakistan Agriculture Statistics, Pakistan Bureau of Statistics.

Table B.5. Effect of Monthly Rainfall Shocks on Cotton Production

	Cotton Production (tonnes)
January Wet Shock	−93.581***
	(32.269)
Dry Shock	125.855***
	(48.450)
February Wet Shock	5.985
	(6.085)
Dry Shock	174.216**
	(79.298)
March Wet Shock	14.787
	(10.410)
Dry Shock	−243.843***
	(91.810)
April Wet Shock	−14.221
	(16.055)
Dry Shock	−362.689
	(254.867)
May Wet Shock	−86.745**
	(35.401)
Dry Shock	565.142*
	(294.735)
June Wet Shock	12.193**
	(6.172)
Dry Shock	−154.410
	(405.604)
July Wet Shock	−12.524*
	(6.475)
Dry Shock	238.829*
	(137.069)
August Wet Shock	−44.081**
	(18.629)
Dry Shock	181.521
	(116.877)
September Wet Shock	−11.548
	(21.085)
Dry Shock	76.802
	(417.073)
October Wet Shock	−60.221**
	(28.684)
Dry Shock	85.015
	(118.549)
November Wet Shock	−10.681
	(28.099)
Dry Shock	−53.549
	(61.137)
December Wet Shock	−39.523*
	(22.118)
Dry Shock	−41.655
	(65.945)
Observations	6,552
Adjusted R^2	0.967

Note: The table presents results from a OLS regression where the dependent variable is cotton production (in tonnes). `Wet` and `Dry Shocks` are measured as the positive and negative deviations of rainfall from the long run district-month rain. The estimation includes district and year fixed effects. Standard errors are clustered at the district level. *p<0.1; **p<0.05; ***p<0.01

Table B.6. Results: Interaction with Cotton Harvest Period

	Incidents					
	(1)	(2)	(3)	(4)	(5)	(6)
Wet Shock (t + 1)					0.0004	0.0004
					(0.0007)	(0.0007)
Dry Shock (t + 1)					0.0043	0.0043
					(0.0065)	(0.0065)
Wet Shock	0.0012*	0.0004	0.0004	-0.0004	0.0007	0.0007
	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0008)	(0.0008)
Wet Shock*Harvest	-0.0002	0.0001	0.0001	0.0008	0.0002	0.0002
	(0.0010)	(0.0012)	(0.0012)	(0.0014)	(0.0012)	(0.0012)
Dry Shock	0.0091	0.0096*	0.0096*	0.0069	0.0081	0.0081
	(0.0062)	(0.0052)	(0.0052)	(0.0064)	(0.0059)	(0.0059)
Dry Shock*Harvest	-0.0341*	-0.0356	-0.0356	-0.0310	-0.0363	-0.0363
	(0.0193)	(0.0250)	(0.0250)	(0.0240)	(0.0262)	(0.0263)
Wet Shock (t - 1)	-0.00007	-0.0007	-0.0007	-0.00004	-0.0005	-0.0005
	(0.0006)	(0.0007)	(0.0007)	(0.0008)	(0.0007)	(0.0007)
Wet Shock(t-1)*Harvest	0.0013	0.0020*	0.0020*	0.0018	0.0022*	0.0022*
	(0.0010)	(0.0012)	(0.0012)	(0.0014)	(0.0012)	(0.0012)
Dry Shock (t - 1)	0.0017	0.0063	0.0063	0.0074	0.0036	0.0036
	(0.0064)	(0.0069)	(0.0069)	(0.0072)	(0.0068)	(0.0068)
Dry Shock(t-1)*Harvest	-0.0080	-0.0102	-0.0102	-0.0117	-0.0068	-0.0068
	(0.0132)	(0.0136)	(0.0137)	(0.0138)	(0.0141)	(0.0141)
Wet Shock (t - 2)	-0.0006	-0.0012	-0.0012	-0.0003	-0.0003	-0.0003
	(0.0007)	(0.0008)	(0.0008)	(0.0010)	(0.0008)	(0.0008)
Wet Shock(t-2)*Harvest	-0.0002	-0.0002	-0.0002	-0.0011	-0.0009	-0.0009
	(0.0007)	(0.0011)	(0.0011)	(0.0014)	(0.0011)	(0.0011)
Dry Shock (t - 2)	-0.0138	-0.00009	-0.00009	-0.0024	0.0008	0.0008
	(0.0109)	(0.0114)	(0.0115)	(0.0130)	(0.0117)	(0.0118)
Dry Shock (t - 2)*Harvest	0.0005	-0.0104	-0.0104	-0.0084	-0.0108	-0.0108
	(0.0164)	(0.0171)	(0.0171)	(0.0173)	(0.0176)	(0.0176)
Wet Shock (t - 3)	0.0013**	0.0003	0.0003	0.0008	0.0002	0.0002
	(0.0006)	(0.0007)	(0.0007)	(0.0010)	(0.0007)	(0.0007)
Wet Shock(t-3)*Harvest	-0.0009	0.0003	0.0003	-0.0006	0.0006	0.0006
	(0.0008)	(0.0010)	(0.0010)	(0.0013)	(0.0011)	(0.0011)
Dry Shock (t - 3)	-0.0006	0.0005	0.0005	-0.0016	-0.0013	-0.0013
	(0.0073)	(0.0079)	(0.0079)	(0.0087)	(0.0083)	(0.0084)
Dry Shock (t - 3)*Harvest	0.0031	0.0140	0.0140	0.0163	0.0166	0.0166
	(0.0159)	(0.0143)	(0.0143)	(0.0135)	(0.0140)	(0.0140)
Temperature	✓	✓	✓	✓	✓	✓
District	✓	✓	✓	✓	✓	✓
Year	✓		✓			✓
District*Year	✓	✓	✓	✓	✓	✓
Month		✓	✓		✓	✓
Month-Year				✓		
Observations	2,130	2,130	2,130	2,130	2,085	2,085
AIC	3,811.50	3,795.28	3,801.28	3,818.78	3,724.87	3,730.87

Note: The dependent variable, and wet and dry shocks are measured as before. Harvest is a dummy equal to 1 for the months September to December. Standard errors in parenthesis, with clustering at the district level. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

Table B.7. Reporting Bias: Effect of Rainfall Shocks on Repeated Reporting

	Number of Times Repeated		
	(1)	(2)	(3)
Wet Shock (t + 3)			-0.0012
			(0.0012)
Dry Shock (t + 3)			-0.0048
			(0.0091)
Wet Shock (t + 2)			-0.000009
			(0.0005)
Dry Shock (t + 2)			-0.0035
			(0.0051)
Wet Shock (t + 1)			-0.00005
			(0.0008)
Dry Shock (t + 1)			-0.0031
			(0.0040)
Wet Shock	-0.0003	-0.0004	-0.0008
	(0.0003)	(0.0004)	(0.0005)
Dry Shock	-0.0067*	-0.0051	-0.0031
	(0.0034)	(0.0036)	(0.0043)
Wet Shock (t - 1)	0.0001	-0.0001	-0.0002
	(0.0002)	(0.0003)	(0.0005)
Dry Shock (t - 1)	-0.0020	-0.0006	0.0017
	(0.0052)	(0.0058)	(0.0067)
Wet Shock (t - 2)	0.0001	-0.0001	-0.0008
	(0.0005)	(0.0007)	(0.0006)
Dry Shock (t - 2)	-0.0019	-0.0024	-0.0043
	(0.0052)	(0.0059)	(0.0056)
Wet Shock (t - 3)	-0.00004	0.0002	0.00008
	(0.0004)	(0.0004)	(0.0006)
Dry Shock (t - 3)	-0.0001	0.0001	0.0029
	(0.0036)	(0.0037)	(0.0054)
Temperature	✓	✓	✓
District	✓	✓	✓
District*Year	✓	✓	✓
Year	✓		
Month		✓	
Month-Year			✓
Observations	634	634	578
AIC	2,183.44	2,192.57	2,071.56

Note: The dependent variable is the average number of times an incident is reported across newspapers, for a district-month. The sample is restricted to district-month combinations with a positive number of incidents. Standard errors are clustered at the district level. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

Table B.8. Effect of Rainfall Shocks on Marriage and Divorce Patterns in Punjab

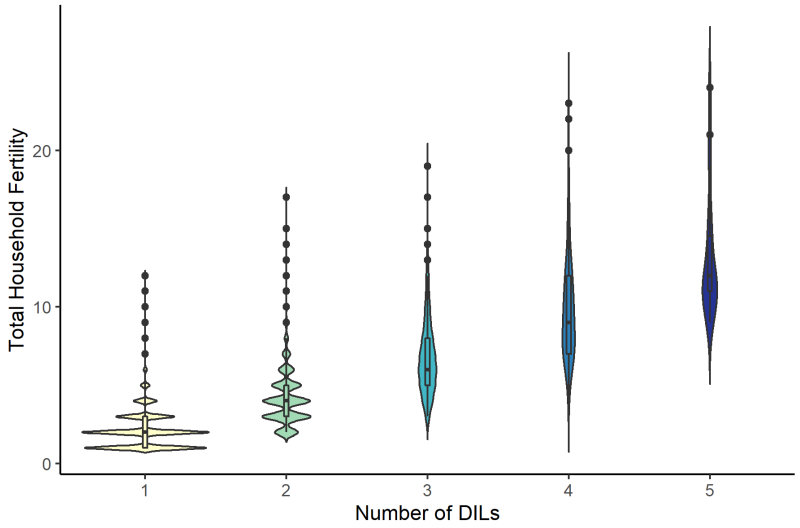
	Marriages		Divorces	
	(1)	(2)	(3)	(4)
Total Wet Shock	−13.355	−145.342	0.371	−31.521*
	(65.100)	(109.499)	(11.407)	(17.815)
Total Dry Shock	−1,334.015	−741.934	−283.083	−188.235
	(1,828.895)	(1,833.727)	(321.472)	(302.413)
Estimated Population	117.677	222.602**	24.799	56.944**
	(82.210)	(107.165)	(15.927)	(20.684)
District FE	✓	✓	✓	✓
Year FE		✓		✓
Observations	60	60	58	58
R^2	0.774	0.791	0.855	0.879
Adjusted R^2	0.506	0.527	0.681	0.723

Note: The table presents results from OLS regressions for the number of marriages and divorces in a district-year. The sample is limited to just the Punjab province. `Marriages` and `Divorces` are the number of registered marriages and divorces in a district and year. `Total Wet Shock` is defined as the sum of monthly wet shocks (in mm) for the respective year. `Total Dry Shock` is defined likewise for dry shocks. Monthly shocks are defined as above (absolute deviation from the long run average). Standard errors in parenthesis. Signif Codes: ***: 0.01, **: 0.05, *: 0.1

APPENDIX C

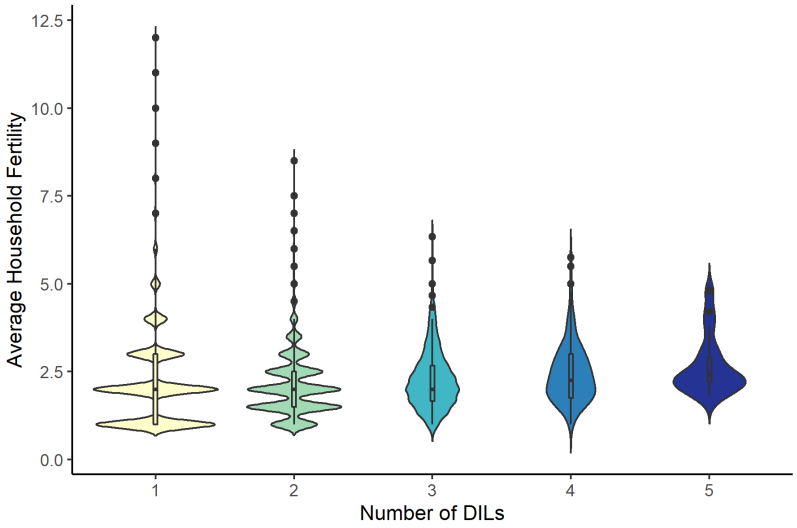
APPENDIX FOR CHAPTER IV

Figure C.7. India Total Fertility by Co-Resident Sons



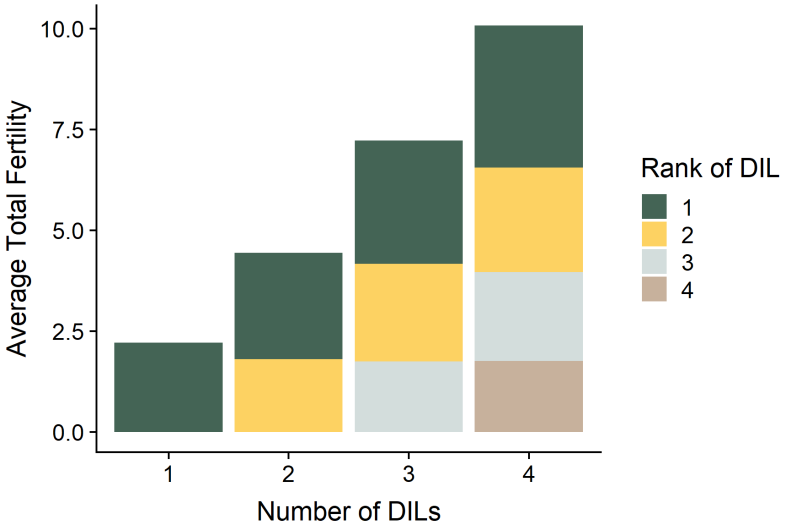
Note: Data is accessed from the 2015, 2005, and 1998 India DHS. The sample consists of households with a maximum of 5 daughter-in-laws co-residing. The figure displays the distribution of total fertility for households with varying numbers of daughter-in-laws (DILs).

Figure C.8. India Average Fertility by Co-Resident Sons

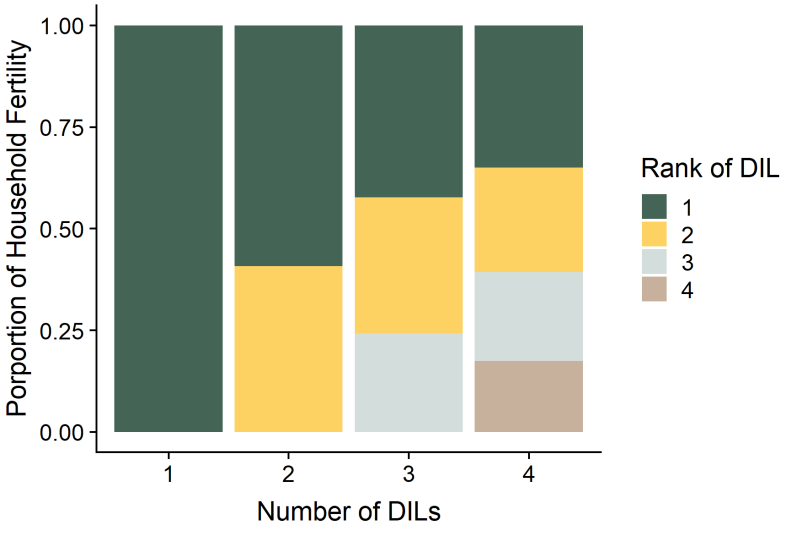


Note: Data is accessed from the 2015, 2005, and 1998 India DHS. The sample consists of households with a maximum of 5 daughter-in-laws co-residing. The figure displays the distribution of total fertility for households with varying numbers of daughter-in-laws (DILs).

Figure C.9. Proportion of Total Fertility by Co-residing Sons



(a)



(b)

Note: In panel (a), the figure displays the average total household fertility for residences with different numbers of co-residing sons and their partners. The shading represents the total average fertility that is attributed to the differently ranked sons. In panel (b), the figure depicts the same pattern but now the y-axis represent the proportion of household fertility that can be attributed to differently ranked sons. Data is accessed from the DHS 2015, 2005 and 1998 years, for India.

Table C.9. Fertility Differences Within Household

	Number of Children	
	(1)	(2)
Second Rank = 1	-0.807^{***}	-0.545^{***}
	(0.046)	(0.058)
Education (years)	-0.055^{***}	-0.046^{***}
	(0.012)	(0.016)
Partner's Education	-0.005	-0.007
	(0.005)	(0.008)
State/Survey FE	✓	✓
Household FE	✓	✓
Observations	4,175	1,844
R^2	0.696	0.739
Adjusted R^2	0.385	0.467

Note: The sample is restricted to India only. Data is accessed from the DHS 2015, 2005, and 1998 survey years. `Second Rank = 1` if the respondent is the wife of the younger co-resident son. Column (1) includes the full sample. Column (2) includes a sample of households where both women have completed their fertility. Standard errors are clustered at the DHS cluster (village) level. *p<0.1; **p<0.05; ***p<0.01

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