Early Marriage and Social Norms: Evidence from India's Unenforced Child Marriage Ban

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Women who marry early have lower decision-making power, less education, and poorer maternal health outcomes. While many countries have implemented laws to increase the legal age of marriage, the global number of child brides remains high, with India as the largest contributor. I analyze India's 1978 Child Marriage Restraint Act, which raised the legal age of marriage for women from 15 to 18 years. I exploit geographical variation in early marriage social norms combined with differences in year of birth to define exposure to the law and find that the ban led to a 7.8 percent decrease in the likelihood of marriage before 18, at the average norm intensity. I rule out the role of differential sex ratios, enforcement capacities or political leadership as a mechanism and instead argue that awareness of the ban combined with a perception of enforcement are driving the results. 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.

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I 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 et al. 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; Asadullah and Wahhaj 2019; Chari et al. 2017; Garcia-Hombrados 2017). 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 (Acemoglu and Jackson 2017; Aldashev, Platteau, and Wahhaj 2011; Aldashev et al. 2012). 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 (Basu 2018; Mcadams 2000a; Mcadams 2000b). 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 percent 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. 2017; Kirdar, Dayiolgu, and Koc 2012). 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 et al. (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 et al. (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 (Basu 2018; Mcadams 2000a; Schelling 1960). 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 norms and 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 II describes the Child Marriage Restraint Act of 1978. Section III outlines the identification and empirical strategy. Section IV details the data employed in this research while Section V explains the results. Section VI provides a detailed discussion of marriage market effects and mechanisms. Section VII decribes several robustness tests and Section VIII concludes.

II 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 enacted in March 1978 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.[2] 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 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.

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 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 meeting pre-determined quotas by targeting even the elderly.

[2] 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.

III Identification Strategy and Methodology

A 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 birthday to marry.[4] My sample of treated and control 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

[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.

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

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.

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:

$$M_{icm} = \beta \ \mathit{Treat}_{ic} {*} \mathit{Intensity}_m + \phi_c + \gamma_m + \alpha_z + \eta_r + \sigma_y + \epsilon_{icm} \tag{1}$$

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. 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.[5] $\mathit{Intensity}_m$ is a measure of the pre-policy average probability of marrying underage in a marriage market m. Thus, $\mathit{Intensity}_m$ 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. This is a testable assumption. I plot results from the cohort-year specific regression:

$$M_{icm} = \sum_{c=1951}^{1973} (d_{ic} {*} \mathit{Intensity}_m)\beta_c + \phi_c + \gamma_m + \eta_r + \sigma_y + \epsilon_{icm} \tag{2}$$

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

[5] It is possible to allow treatment to vary with age by allowing treatment to be a function of the months to turning 18. However, the method employed in the paper places fewer demands on the quality of the reported age data.

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.

B 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 (2017) 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.[6] 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.[7] 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;

[6] IHDS 2005.

[7] 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.

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.

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.

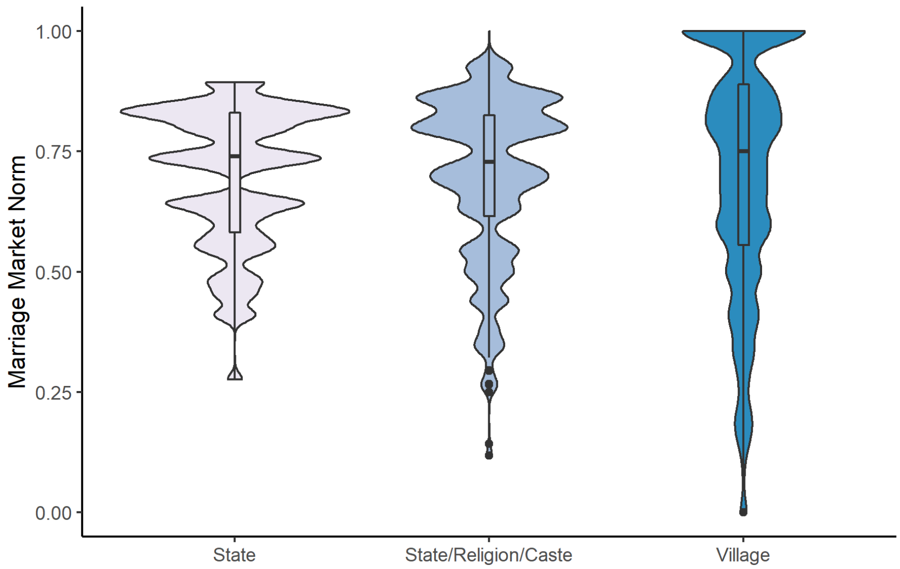


Figure 1: The violin plots display the kernel density of pre-policy age at marriage norms for three different definitions of marriage markets. A norm of 0 means all women marry at or after 18, and a norm of 1 means all women in the market marry before age 18. Each violin plot also includes a box plot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

IV Data and Descriptives

A 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.[8] I restrict my sample to women who have married only once, between the ages of 12 to 40.[9] 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 et al. (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.

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 cultural

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

[9]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.

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 6 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.

B 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.[10] 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 8 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

[10] The distribution of age at marriage by urban and rural region of residence in childhood can be found in Figure 7 in the appendix.

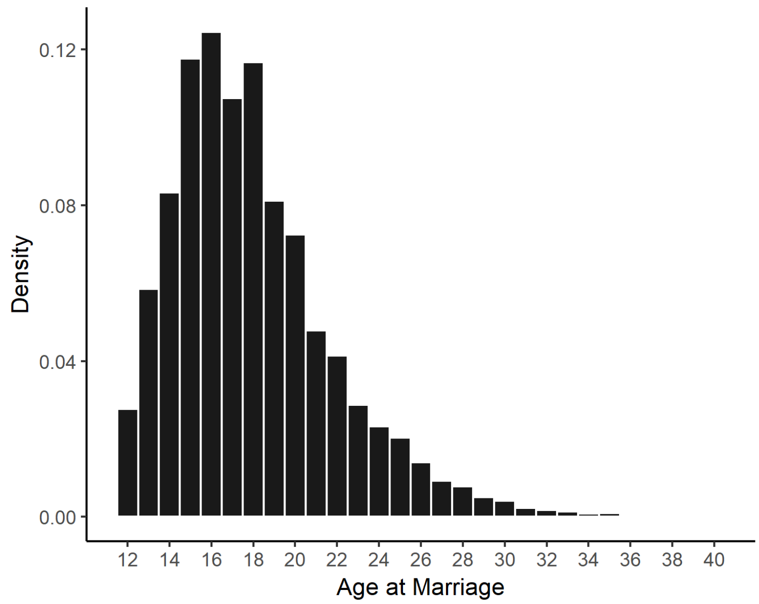
who delay marriage by one year are associated with a 1 percent greater probability of knowing their partner, on average. As shown in column (3) of the table, this result is not explained by an increase in the likelihood of marrying a blood relative. A delay in marriage by one year is also associated 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 Das Gupta 2001). Taken together, these results highlight a positive correlation between a higher age of marriage and greater agency in marriage market outcomes.

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.

V Results

A 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.

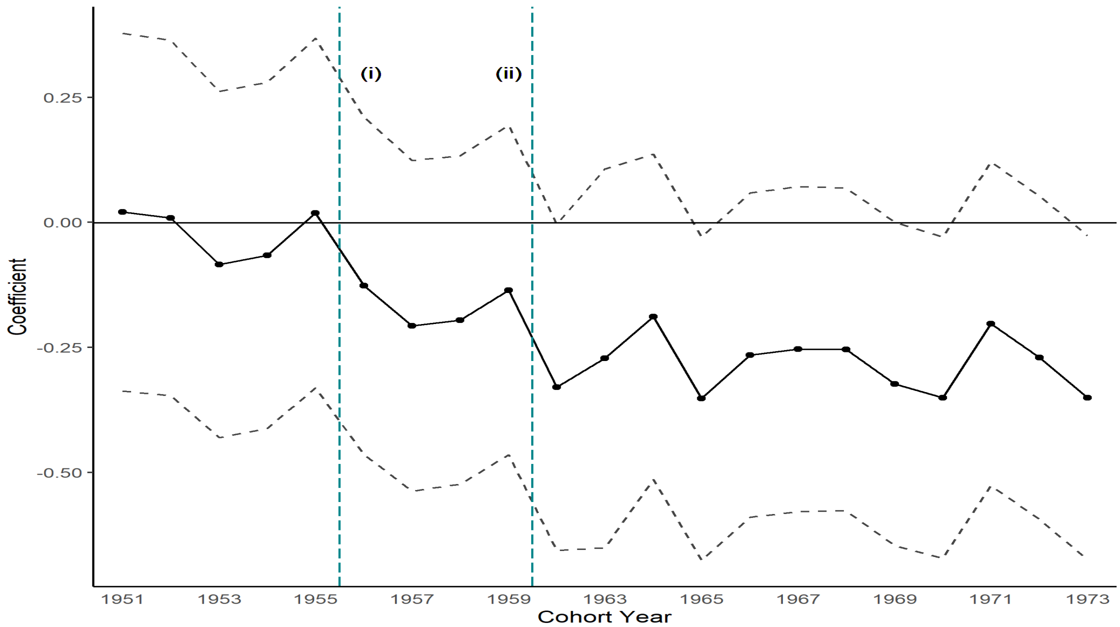
As expected, the estimates fluctuate around 0 for women born before 1956; the policy did not affect women who were not exposed to it. The decrease in the coefficient estimates begins for cohorts born in 1957, eventually stabilizing for women born after 1960. The earlier break from the trend is not surprising 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.

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

(a) Marriage Market: State



(b) Marriage Market: State/Religion/Caste

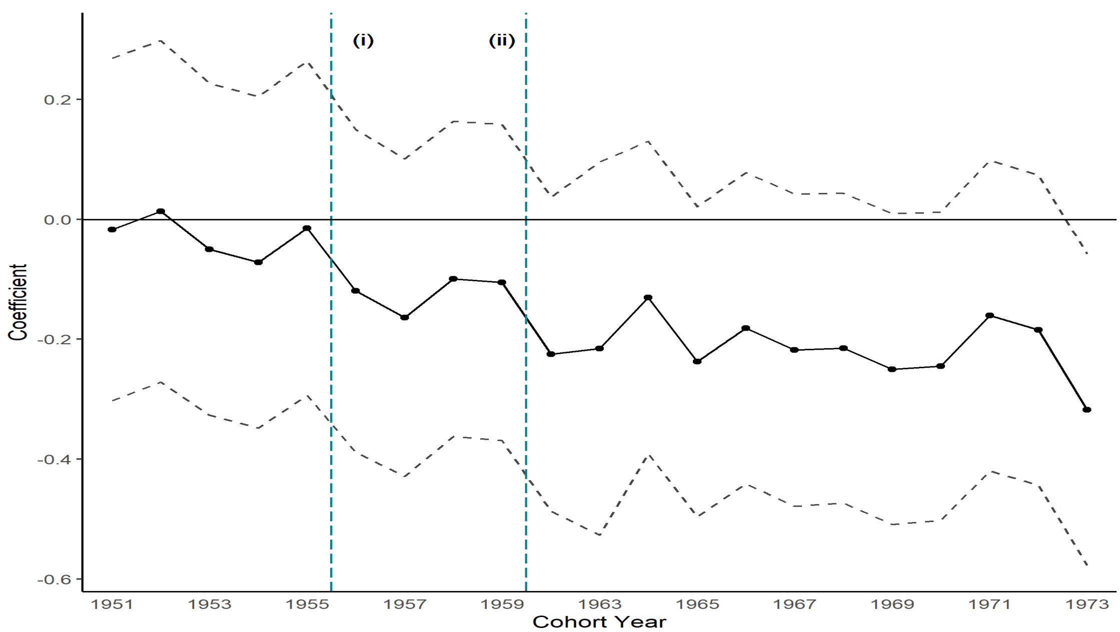


Figure 3: The figure plots the interaction coefficients of a cohort dummy (or year of birth) and the marriage market pre-policy probability of teen marriage. In the **top panel**, a marriage market is defined at the state level and in the **bottom panel**, a marriage market is defined using a combination 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.

B 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 (4) and (5), I interact these controls with a *Treated* dummy variable to allow their impact to vary across pre and post-policy cohorts. As a robustness check, column (3) 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 (2), the results suggest that women in marriage markets with high intensity child marriage norms were 11.3 percent 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 (2) suggests that women exposed to high intensity early marriage norms are 7.4 percent 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 (5) in Panel B, although the point estimate in column (6) is almost identical to the coefficient in my preferred specification in column (2).

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 percent decrease in the likelihood of marriage under 18 and 15, respectively. The results can also be interpreted in terms of percentile 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 percent decrease in the likelihood of marrying before 18 and 15 respectively. In column (5) of panel (A), the point estimate increases with the inclusion of *state* $\times$ *treated*, *religion* $\times$ *treated*, and *caste* $\times$ *treated* controls; a move from the 10th to the 90th percentile of early marriage intensity translates to a 15.7 percent decrease in the probability of marrying under 18 for women exposed to the ban.

Table 4: Effect of Policy on Early Marriage

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent Variable			*1(= Marriage Before 18)*		
Treated*Intensity	-0.113^{***}	-0.113^{***}	-0.052^{*}	-0.112^{**}	-0.351^{***}
	(0.031)	(0.031)	(0.029)	(0.044)	(0.056)
Mean Dependent Variable	0.52	0.52	0.52	0.52	0.52
Observations	74,215	74,215	74,215	74,215	74,215
R^2	0.162	0.162	0.163	0.163	0.164
Adjusted R^2	0.158	0.158	0.159	0.159	0.159
Panel B: Dependent Variable			*1(= Marriage Before 15)*		
Treated*Intensity	-0.074^{***}	-0.074^{***}	0.0002	-0.035	-0.069
	(0.019)	(0.019)	(0.019)	(0.028)	(0.052)
Mean Dependent Variable	0.17	0.17	0.17	0.17	0.17
Observations	74,215	74,215	74,215	74,215	74,215
R^2	0.085	0.085	0.087	0.086	0.086
Adjusted R^2	0.081	0.081	0.082	0.081	0.081
Cohort FE	X	X	X	X	X
Marriage Market FE	X	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

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 9 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 percent 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.

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

Notes: 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

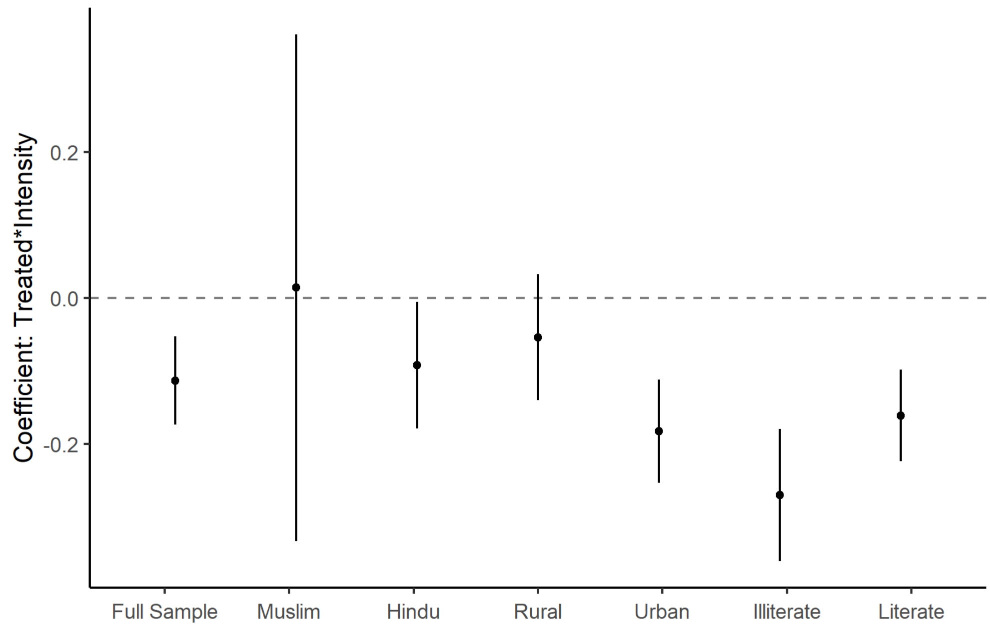
An important caveat applies to these interpretations. For the main specifications in Table 4, the probability of marriage under 15 and 18 are predetermined variables. That is, the choice of marrying under 15 or 18 is made by the control group before the policy change. Thus, the behavior of treated women 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 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 percent 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 find that the results are driven by Hindu women. Coefficient estimates for Muslim women are insignificant, possibly because of a much smaller sample size.

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. Illiterate refers to women who have zero years of schooling. Literate women have atleast one year of schooling. *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. All regressions include survey, cohort, marriage market, and caste fixed effects. 95% confidence intervals are displayed.

VI Discussion

A 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.

Table 6: Effect of Policy on Men and Match Outcomes

	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

Notes: 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 results are reported in columns (1) and (2) of Table 6.[11] 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 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.

B 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.[12] I test whether the likelihood of cohabitation or *gauna* before 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.

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

[12] 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 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 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.

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 *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.

C 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 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 10 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.

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

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 un-affordable. 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 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.

VII 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.

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

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.[13] 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

[13] 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.

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 percent 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 marriage norms. However, specifications at the village level are also more likely to capture the effects of changing adult sex ratios that 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.

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

	(DHS)	(IHDS)					(DHS)
Marriage Market	State (1)	State (2)	State-Religion-Caste (3)	District (4)	District-Religion-Caste (5)	Village (6)	Village (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

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. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

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

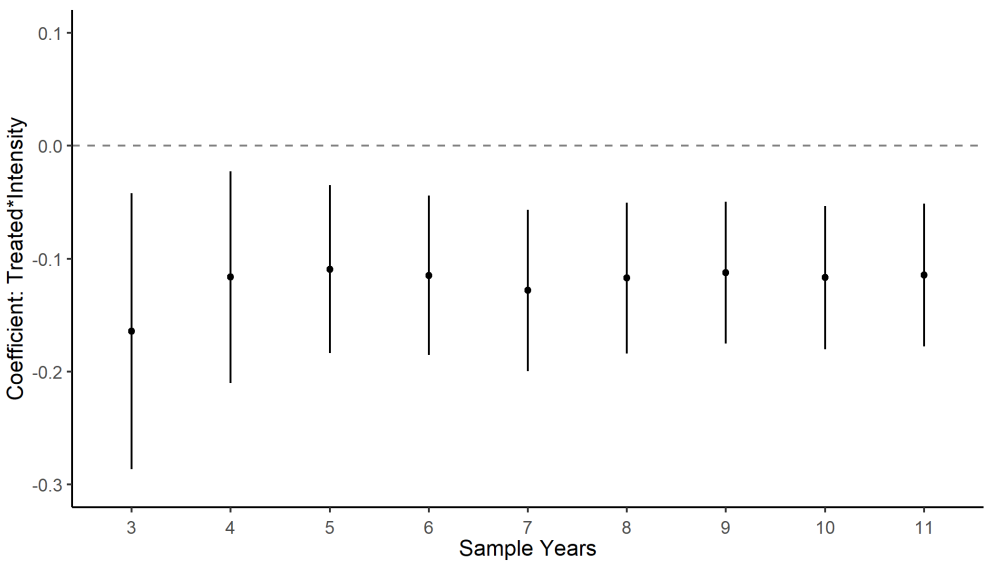
Notes: The sample includes women who marry once between the ages 12 to 40. The sample used in columns (1), (3), and (5) consists of women who have not migrated (women who have lived in the place of residence since before marriage). Columns (2), (4), and (6) use samples of those women who have migrated (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

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.

In Table 13 in the appendix, I show that the main results are robust to the inclusion of state by year of birth sex ratios. In Table 14, 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



Notes: 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.

VIII 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 percent decrease in the likelihood of marriage before 18 at the average norm intensity, with a 7.4 percent 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.

Appendix

Table 12: 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

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

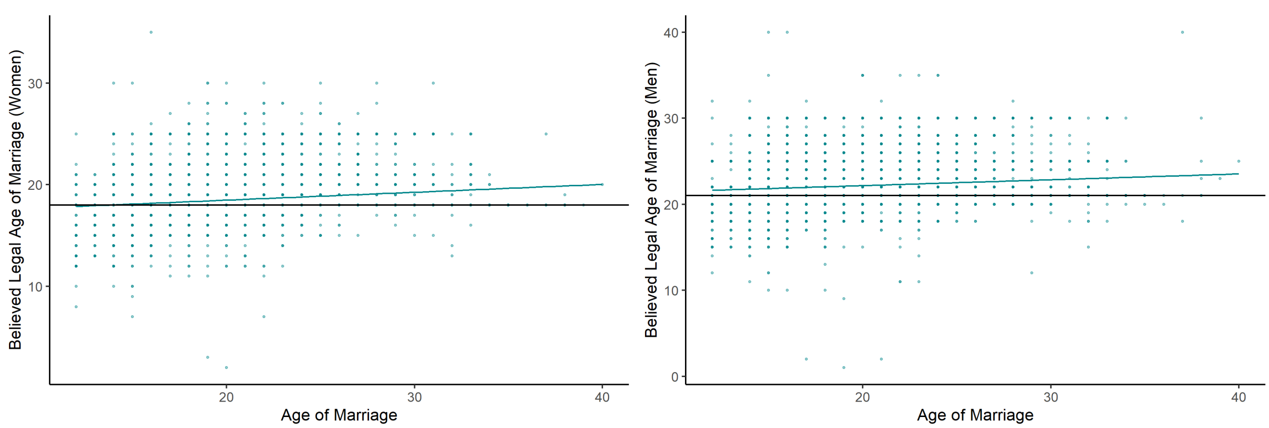


Figure 6: Data is accessed from the 1992 DHS survey. The figure plots own reported age of marriage against the believed legal age of marriage for men and women separately. The horizontal line represents the true legal age of marriage, which is 18 for women and 21 for men.

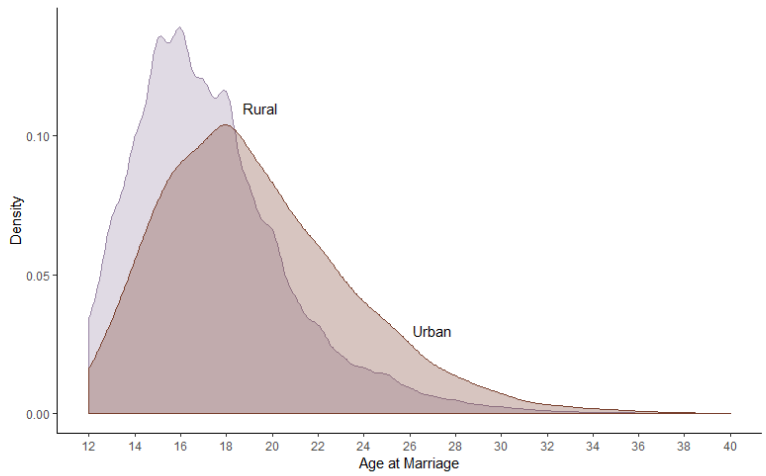


Figure 7: This figure shows the distribution of age at marriage by the respondent's region of residence in childhood. The sample includes women who marry once, between the ages 12 to 40. Data is accessed for India from the 1998 and 2005 Demographic and Health Survey.

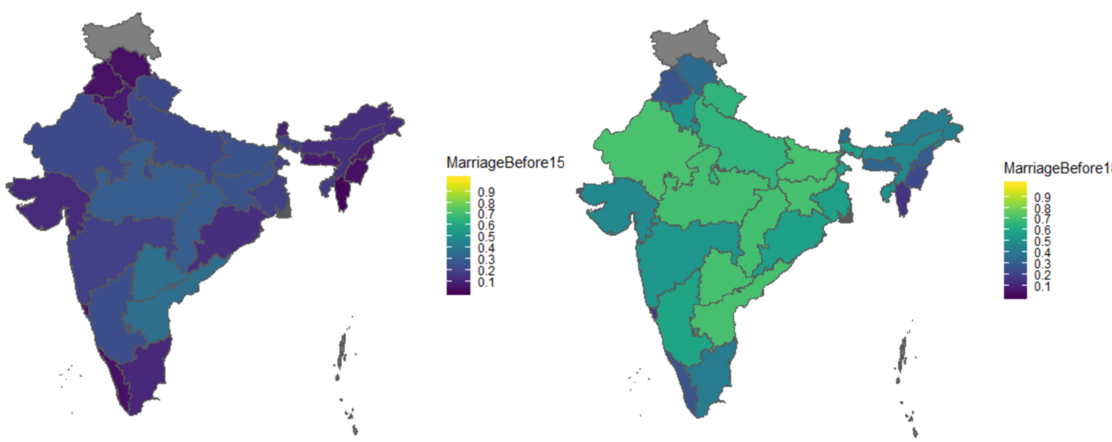
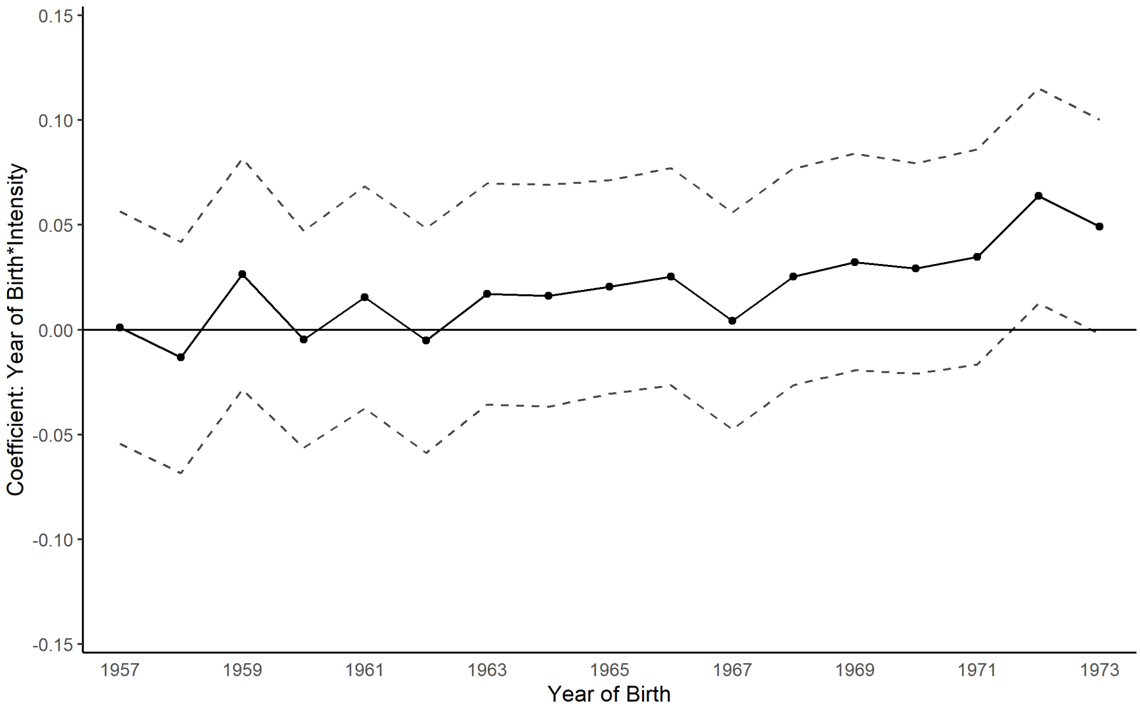


Figure 8: The maps show the geographical variation in the pre-policy probability of marriage before 15 and 18, respectively, by state.

Figure 9: 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*. A marriage market is defined at the state-religion-caste level. Data is accessed from the 2005 DHS survey for India.

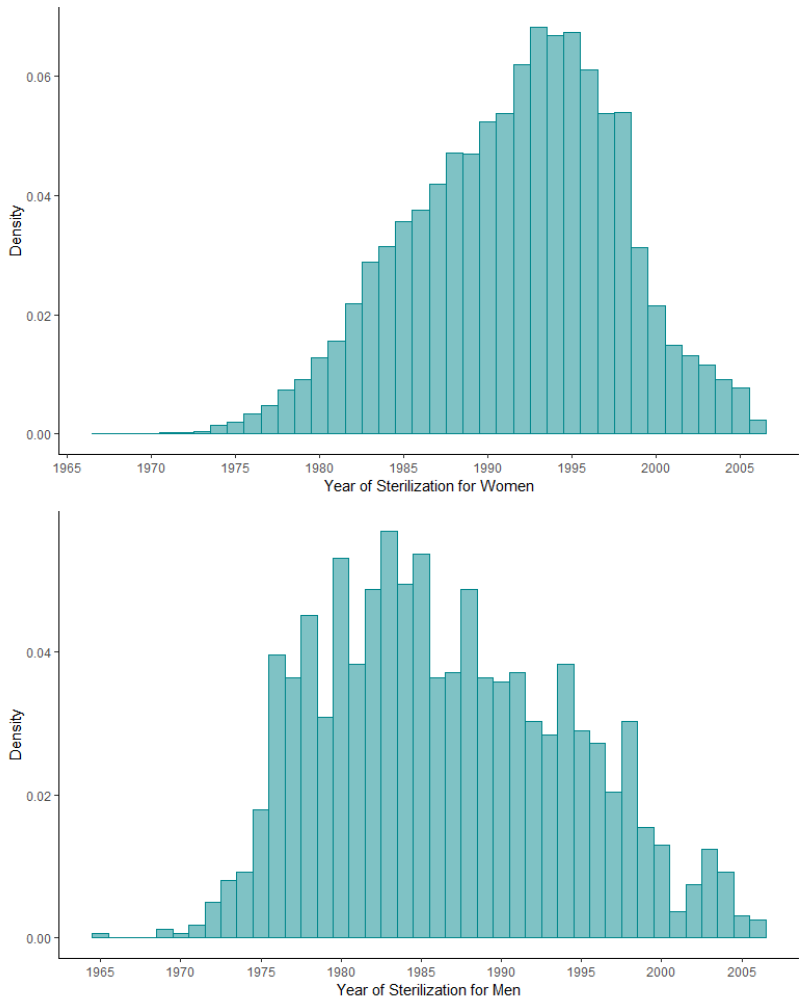
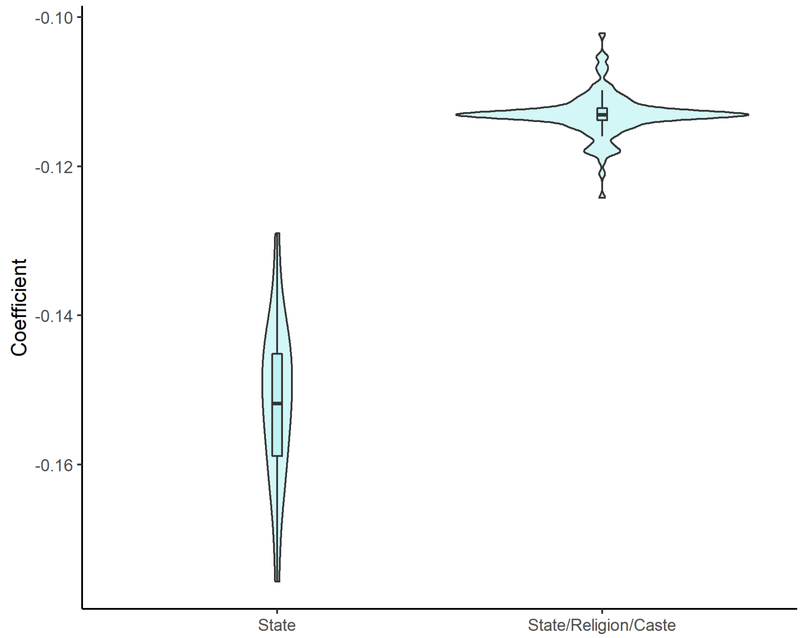


Figure 10: The figure plots the year of sterilization for the sample of women (top panel) and men (bottom panel) in my data who are sterilized.

Figure 11: Coefficient Robustness to Exclusion of Markets



Note: The figure 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 in the figure. Data is accessed from the DHS.

Table 13: 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

Notes: 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 14: 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

Notes: 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. *p<0.1; **p<0.05; ***p<0.01

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