

# Show Me the Dowry: How Traditional Customs Affect Education in Rural India\*

Natasha Jha<sup>†</sup>

## Abstract

This study examines the impact of a law that increased penalties for dowry on rural households' educational attainment in India, using data from the Rural Economic and Demographic Survey. A difference-in-differences approach reveals that the law reduced dowry payments and female education, with the decline in female education being most pronounced in communities with higher dowry prevalence. This suggests that dowry payments signal adherence to tradition, and reducing female education serves as an alternative signal in the absence of dowry. Hence, while the law combats an exploitative custom, it inadvertently depletes female human capital.

**Keywords:** Dowry; Marriage payments; Education; India

**JEL Classifications:** J16, I21, I28, N35

---

\*This paper has benefited from helpful comments and suggestions from Arnab Basu, Chris Barrett, Brian Dillon, Prabhu Pingali, Nishith Prakash, and seminar participants at the New England Universities Consortium 2022 (NEUDC), the Pacific Conference for Development Economics 2022 (PacDev), the Midwest International Economic Development Conference 2022 (MIEDC), Twenty-First Summer School in International and Development Economics, and Cornell University. Previously circulated as "Dowry and Human Capital Formation: Evidence from India".

<sup>†</sup>2144 Jenkins Nanovic Halls, University of Notre Dame. njha@nd.edu. 5746317816

# 1 Introduction

Efforts to change harmful customs through policies may have unintended negative consequences. For example, more gender-equal inheritance laws in India, increase suicide rates for both men and women by engendering more conflict over household resources (Anderson and Genicot, 2015). Specifically, laws that succeed in altering family customs can result in intricate and unforeseen economic consequences. Conditional on compliance, one may predict improvements along outcomes targeted by the law, but the scope for perverse consequences is ambiguous. If legal changes influence outcomes for disadvantaged groups in a manner that does not interfere with family dynamics, it is reasonable to anticipate a reduction in harm causing customs resulting in welfare improvements. Alternatively, if the custom tackles a market failure, for example, property transfers to sons as an old age support in a virilocal exogamous society with imperfect insurance markets (Ali et al., 2014), then attempts to alter the custom tangle with family dynamics and result in increased likelihood of impacts on outcomes not targeted by the legislator. In this paper, I empirically analyze the impact of laws aimed at addressing harmful customs on outcomes for disadvantaged groups.

India's longstanding social custom of dowry is an ideal context to examine the interaction between legal changes and harmful customs.<sup>1</sup> First, prior work establishes a link between the custom of dowry and forms of severe gender disparity, such as sex-selective abortion favoring strong son preference, and gender-based violence including bride-burning and dowry related deaths (Alfano, 2017; Bhalotra et al., 2020). At the same time practicing the social custom of dowry increases the value of women at the time of marriage. Second, dowry impacts family dynamics in several ways. Economic theories on the origin of dowry discuss the mediating effect of dowry on outcomes such as female inheritance, marriage market matching, and household social status (Anderson, 2003; Botticini and Siow, 2003; Rao, 1993; Roulet, 1996). Given the wide range of outcomes, legal changes that prevent households from engaging in dowry modify the social status and position of households in complex ways that operate outside a simple bargaining model. For instance, in the absence of dowry, parents may choose to invest in com-

---

<sup>1</sup>Dowry is a practice of transferring wealth from the bride's family to the groom's family at the time of marriage. This custom dates back to 200 C.E (Anderson, 2007). Contemporary dowry transfers are substantial, typically exceeding household annual income, and remain widely prevalent in India (Chiplunkar and Weaver, 2023).

plementary measures to increase the economic value of daughters. Simultaneously, households engage in behavior to limit loss of reputation accruing to breaking the dowry custom (Akerlof, 1980). When adherence to social customs are an investment in status, and in regions where traditional customs place restrictions on the mobility and labor force participation for women (Agte and Bernhardt, 2022), do households seek alternative signals of adherence, particularly through actions that reduce female empowerment?

Theoretically, the interaction among the custom of dowry, female educational attainment and marriage market returns may result in an ambiguous relationship between dowry payments and women's educational attainment. Do households in rural India increase or decrease girls' education when faced with legal restrictions on dowry payment? In this paper, I analyze the impact of increased enforcement of anti-dowry laws on an entrenched custom - dowry payments - and its gendered implications for educational attainment in rural India. The research design employs a difference-in-differences approach, combining variation in age-based exposure with the exemption of Muslims from the scope of the 1985-86 strengthening amendment to the existing 1961 Dowry Prohibition Act. Using data from the Rural Economic and Demographic survey, I compare changes in dowry payments and years of educational attainment across Muslim and non-Muslim households, for older versus younger cohorts of girls that vary in exposure to an amendment.

This paper has three main findings. First, changes in legal enforcement capacity have the ability to impact social customs. While the amendment fails to eradicate the custom of dowry, it decreased average dowry payments by 18% in the post-amendment period. Second, attempts to modify conservative customs have unintended consequences on human capital formation. Point estimates suggest an 18% reduction in girls' educational attainment, with the strongest effects among households that are most impacted by changes in dowry payments. I find no evidence of impacts on educational attainment for boys, consistent with greater significance of dowry custom for girls. I posit that households rely on dowry payments to communicate adherence to traditional customs on the marriage market. The anti-dowry law amendment increases the cost of dowry exchange. This, in turn, prompts households to decrease their investments in girls' education as an alternate signal of adherence to traditional customs. To substantiate this claim, I document more significant reductions in educational attainment for households

in regions with higher pre-amendment dowry payments and among occupational classes where dowry customs are stronger. Additionally, I find similar trends among households practicing gender segregation during meals, serving as a proxy for adherence to traditional customs. Third, backlash from laws targeting social customs are tempered over time. When comparing regions with varying duration of exposure to anti-dowry regulations due to state-specific amendments to the federal 1961 Dowry Prohibition Act, households with longer exposure to these laws do not reduce girls' educational attainment. This suggests that extended exposure to anti-dowry laws does not negatively impact girls' education.

Prior research on the interaction between legal and cultural constraint focuses on the role of enforcement (Barfield, 2010; Parsons, 2010). When laws clash with prevailing social customs, they are difficult to enforce, thereby reducing effectiveness (Collin and Talbot, 2023). Notwithstanding successful enforcement, unintended consequences may arise on account of an increase in these activities immediately prior to implementation, as was the case with spikes in child marriage rates related to the 1929 ban on child marriage in India (Roy and Tam, 2016). The legal change I study has been shown to improve enforcement of the anti-dowry law in India (Menski, 1998). By focusing on legal systems that govern marriage customs, specifically, laws to regulate dowry practices, I provide evidence on unintended consequences despite successful enforcement, in a more recent time frame.

My study contributes to three strands in the literature. First, I provide empirical evidence on the unintended consequences accruing to the interaction of laws with social customs. The combination of group-based variation in legal exemption from the amendment, along with the widespread prevalence of dowry practice, provides a distinct opportunity to examine causal effects. Although much of the literature in this area is theoretical (Chen and Yeh, 2014; Deffains and Fluet, 2020), or based on experimental designs in areas such as tax evasion, corruption, or substance abuse (Besley et al., 2014; Musick et al., 2008), my paper shifts the focus from illegal behaviors to educational attainment as an outcome. I draw attention to the likelihood of unintended adverse outcomes, with lifetime well-being consequences, that can arise from legal changes aimed at challenging prevailing social customs.

Second, my paper expands on previous research that documents the possibility of unintended consequences from laws intended to improve women's status (Anderson and Genicot,

2015; Bhalotra et al., 2020; Calvi and Keskar, 2021). By documenting a decline in female education, my study highlights the unintended potential for pro-women laws to worsen the condition of women in dimensions not targeted by the legislator. The findings emphasize the difficulty of using laws to protect women when the underlying norms stay the same. Additionally, the findings of my paper contribute to the established literature on gender gaps in education (Hausmann et al., 2012) in developing countries by investigating how policy interventions in marriage markets may generate gender gaps in educational attainment (Vogl, 2013). By identifying an alternative pathway through which social customs impact gender gaps in education, my study provides further insight into the complex interactions between culture, policy, and education.

Third, by examining the impact of a long-standing cultural tradition on the production of human capital, I add to the growing literature on the economic impacts of culture. My contribution is distinct from prior work that more closely capture short term impacts of religious practices on outcomes such as agricultural productivity, sentencing and lending practices, and aggregate economic impacts (Campante and Yanagizawa-Drott, 2015; Mehmood et al., 2022; Montero and Yang, 2022). By examining educational attainment, my paper complements existing studies that examine economic consequences of ceremonial expenses such as *quinceañera* ceremonies in Mexico and the effect on labor supply of mothers (Suzuki, 2021). Ashraf et al. (2020) studies the mediating role of bride price, a custom common across parts of Africa, on the success of a school expansion policy in raising educational attainment. Bride price and dowry differ, as a dowry involves a transfer of wealth from the bride's family to the groom's family and is often associated with gender disparities. My study is unique in its focus on dowry and its potential impact on children's education in the Indian context.

Calvi and Keskar (2021) find that a decline in women's empowerment, and a surge in domestic violence following the 1985-86 amendment. The model used in their paper predicts an increase in investment in education for girls as a response to decrease in expected dowry. I attribute the seemingly contradictory findings on educational attainment between our papers to the social custom adherence story presented in my paper. Underlying social norms that result in establishing customs take time to evolve and respond to changes in legal frameworks. By using data from older cohorts of women my analysis capture the backlash effect which is perhaps tempered over time and does not show up for younger cohorts of women that Calvi

and Keskar (2021) study. Additionally, the loss of reputation is stronger in areas where social customs are more entrenched. My data focuses exclusively on rural areas, where traditional customs are expected to exert a more pronounced influence.<sup>2</sup>

## 2 Background

In the Indian context, dowry is closely tied with people’s representation of themselves and others. Anthropologists claim dowry as a central institution to define social prestige and status across castes (Roulet, 1996), thereby making it a widespread practice. Expectation and negotiation of dowry exchange dominates discussion regarding the arranging and establishing of a marriage. Furthermore, everything the bride brings is publicly displayed to the family and guests of the groom, making it a visible exchange.<sup>3</sup> Dowry thus becomes a public symbol of the status and prestige for households.

Existing economic models on dowry focus on bequest motives, market-clearing price for grooms, and upward social mobility to explain the origin and persistence of dowry.<sup>4</sup> The idea of dowry as a way to signal adherence to social customs is largely ignored. In contexts where prestige building through adherence to social customs is an important manner in which households build social capital, standard economic models fall short.<sup>5</sup>

Responding to the extractive nature of contemporary dowry practices, the Government of India enacted the Dowry Prohibition Act of 1961 as the primary legal means of regulating and controlling dowry. Under this act, any dowry exchange was prohibited. An important feature of this act was that Muslims were exempt from the purview of this law and subsequent amendments, since individuals to whom the Muslim Personal Law applied are not covered under personal laws in India.<sup>6</sup> This feature of the Indian legal system allows for the use of Muslim

---

<sup>2</sup>I implement the estimation strategy in Calvi and Keskar (2021) and present results in Table 19. The results of my paper remain qualitatively unchanged, however, the estimates become more imprecise.

<sup>3</sup>Contemporary dowry is comprised of costly items and cash, jewelry, trousseau, and other household goods given to the family of the groom during the wedding ceremonies (Nishimura, 1994; Roulet, 1996).

<sup>4</sup>(Anderson, 2003; Becker, 1973; Bhaskar, 2019; Botticini and Siow, 2003)

<sup>5</sup>This paper follows Putnam’s definition of social capital (1993, p. 167) as “the general forms of social organization, such as social norms and values, and social networks (especially social participation), that could facilitate coordinated actions and improve the efficiency of society.” Households value individual-level social skills that facilitate non-formal contracts or help navigate imperfect information problems. Examples of such skills could be trust (Zak and Knack (2001)) or association participation.

<sup>6</sup>The Muslim personal Law is Shariat law based on the teachings of the Koran, which prescribe retributive penalties for lawbreaking.

populations as a control group in the analysis.

Anecdotal evidence suggests that the legal provisions were insufficient and poorly executed. Recognising what was widely perceived as a “failed” law, the Government of India amended the law in 1985, and introduced stricter penal provisions and expanded enforced capacity. This paper refers to the 1985-86 amendments as the policy shock or the amendment from here on. The amendments included a more inclusive definition of dowry.<sup>7</sup> The law also increased the scope of punitive actions and fines by categorizing dowry offenses in the same class as murder.

To improve enforcement of the law, the amendment authorized state governments to appoint manpower in the form of Dowry Prohibition Officers with a responsibility to prevent dowry payments and collect evidence for prosecution of offenders. A new system of maintenance of a list of presents exchanged during the marriage was established.<sup>8</sup> The amendment also made illegal the demand for dowry, which included advertisements in matrimonial sections of newspapers. Legal changes were accompanied by expanding the Indian Penal code to include dowry death.

[Alfano \(2017\)](#) documented a phenomenal jump in reported dowry cases from 400 cases per year to 2,000 cases per year across the pre- and post-periods. Apart from increases in dowry reporting, dowry cases under litigation also saw a marked increase during the 1980s (Menksi 1998). Furthermore, the average number of dowry cases under trial at the Supreme Court in India doubled from 2 to 4 cases annually after the amendment ([Alfano, 2017](#)). These insights suggest that the amendment had high expressive value.<sup>9</sup> [Table 2](#) provides empirical evidence on the first-stage impact of the amendment on dowry payments to support this claim.

### 3 Conceptual Framework

This section provides a conceptual framework that incorporates signaling in a theory of human capital formation to explain the decline in female educational attainment. Dowry and educa-

---

<sup>7</sup>The definition of dowry was expanded to include “any property of valuable security given or agreed to be given either directly or indirectly, by i) one party to a marriage to another party to a marriage, ii) by parents of either party to a marriage or by any other person to either party to the marriage or any other person” (GOI 1986).

<sup>8</sup>The list of presents includes a description, estimated value of gifts, presenting individual and relationship to the couple.

<sup>9</sup>Expressive value is derived from the very act of passing a particular law that serves as a signal regarding a social custom.

tion are first discussed as inputs into the human capital formation production function. The discussion is then expanded to accommodate the potential of dowry and education to serve as signals to communicate adherence to traditional social customs.

### 3.1 Human Capital Production Function Model

The economic theory behind a human capital production function considers dowry and education as inputs into a production function. The returns on either of these inputs can be thought of as higher lifetime utility either through higher wages, matching with high quality spouses,<sup>10</sup> or improvements in bargaining power (Calvi and Keskar, 2021).

The documented decline in female education is inconsistent with dowry and education as substitutes since a decline in dowry payments would show up as increases in educational attainment. Yet, this paper finds scarce evidence to support gains in educational attainment across multiple specifications and subsamples.

Alternatively, a strong price effect may obscure the substitute relationship between dowry and educational attainment. Under this scenario, a decrease in dowry payments would lead to increases in education demand, thereby making education more expensive. If the reduction in educational attainment is due to increases in the price of education rather than the reduction in dowry, then the results do align with a substitute story. However, this scenario is ruled out, since for the poorest occupation type where dowry is least important, the evidence documents a gain in educational attainment (Table 6).

Taken together this evidence suggests, that if viewed from the human capital production approach, dowry and education are complements, particularly for women. This result is consistent with prior studies in the South Asian context, which finds more educated brides trade off higher dowries and female education serves as a negative signal in the presence of gendered attitudes (Buchmann et al., 2023).<sup>11</sup> The presence of gendered social attitudes motivates a discussion on the signaling potential of dowry and education.

---

<sup>10</sup>High quality spouses are measured by either higher spousal income (Vogl, 2013) or greater upward social mobility (Anderson, 2003).

<sup>11</sup>Educated girls are perceived as more rebellious and less likely to be controlled (Karim et al., 2022).



## 3.2 Signaling Model

In the presence of unequal gender norms, along with being inputs in the human capital production function, education and dowry investments may also serve as signals to communicate adherence to traditional customs. This paper modifies the signaling model presented in [Buchmann et al. \(2023\)](#) to consider dowry payments as the primary means for signaling unobservable bride attributes.<sup>12</sup> In the Indian context, bride characteristics (adherence to traditional customs) are not fully observable to grooms given the widespread prevalence of arranged marriages ([Vogl, 2013](#)). If traditional custom adherence is valued in the marriage market, then greater engagement with dowry payments, as a long-standing social custom, are a potential way to signal a valued but hidden characteristic of the bride.

The amendment increases the cost of engaging in dowry payments, thereby making dowry payments a less preferred way to signal. Given the potential of lower female education as a viable signal, the amendment induces households to signal adherence to traditional customs by reducing female education. Prior literature suggests age at marriage as an alternate channel for signaling. Although this is theoretically possible, changes in age at marriage are more likely to be a preferred strategy when households are able to use dowry payments to compensate for changes in age at marriage. In such a scenario, women can choose to marry late and increase schooling, conditional on paying a higher dowry. However, in the absence of large magnitudes of dowry payments, as is the case post-amendment, adjustments along the education channel are a more credible signal of traditional customs.

In a perfect experiment, the above conceptual framework can be tested using exogenous variation in the value placed by grooms on the social custom adherence by brides. Unfortunately, such an experiment is impossible, and moreover, secondary data to measure valuation of social custom adherence is unavailable. Instead, the paper examines how family-level adherence to observable traditional custom mediates the impact on female education in the post-reform period. This approach, though imperfect, provides valuable insights assuming that unobservable bride attributes that measure traditional custom adherence are correlated with family level traditional custom adherence.

---

<sup>12</sup>Education, as a means to signal, is presumably more costly given the potential lifetime labor market impacts. However, it is important to acknowledge that, in the presence of gender unequal norms, lower female education can serve as a signal to communicate greater adherence.

Measures of family-level traditional custom adherence available in the data are importance of dowry payments, intensity of engagement with dowry custom, and adherence to a patriarchal practice. Combining insights from the framework with available data generates the following testable predictions. The first, reduction in education is expected to be more pronounced for girls within households most impacted by changes in dowry payments. Second, variation in importance of dowry payments is expected to mediate the impact on female education. Third, households that engage in other traditional social customs such as patriarchal norms are more likely to switch to using education as a signal.

## 4 Data

The data used in the analysis are part of the ARIS-REDS data (hereafter, REDS data) collected by the National Council of Applied Economic Research in Delhi, India. The analysis uses data from the 1999 survey round, which is a detailed survey of rural households, spanning 17 major states in India. The REDS data are a nationally representative rural sample of Indian households and contain detailed retrospective information on individual characteristics of all household members provided by the household head.

The REDS dataset is unique in that it records the monetary value of dowry payments. Nearly 79% of the sample report a dowry payment (Table 1), providing evidence on the widespread and persistent nature of this social custom. Summary statistics reveal that average dowry payments are of large magnitude and exceed the annual household income. Along with dowry payments, the data records educational attainment and demographic characteristics at the household level. This allows the empirical analysis on a matched dowry and educational attainment for the individual, as well as the spouse, which is conducive to examining the first-stage impact of the amendment cleanly.

The recall nature of dowry payments, as well as its illegal nature after 1960, raises concerns about data quality. Dowry payments are a large expense, and households are likely to remember these large financial transactions accurately (Chiplunkar and Weaver, 2023). The law did not penalize retrospectively, and neither did the penalty vary by magnitude of dowry. These institutional features address concerns around reporting bias to some extent. This paper takes these concerns seriously and presents evidence on reporting bias in Appendix section B. The

evidence does not suggest changes in the variance of reporting relative to the amendment date across treatment (Non-Muslim) and control groups (Muslims).

The empirical investigation focuses on sons and daughters of the household head, thereby eliminating concerns about confounding intergenerational impacts. All birth cohorts until 1994 are included in the sample, based on potential school enrollment in 1999. The final sample contains 21,095 observations, comprising of 9,797 daughters and 11,298 sons. Sibling links are identified using birth rosters that contain detailed information on birth order, year of marriage, and gender. Information on household landholding, occupation, and engagement with social customs is obtained from the household roster. Table 1 presents descriptive statistics. The sample contains around 7.5% individuals following Islam— an important variation for the empirical strategy.

Table 1: Descriptive statistics for REDS 99 dataset

Variable	(1) Mean	(2) SD	(3) Pre	(4) Post
Number of households	7,002			
Family size	6.76	3.66		
Number of boys	2.08	1.32		
Number of girls	1.68	1.42		
Muslim	7.5%			
Years of education of household head	5.12	4.70		
Income (2015 Rs)	101,357	151,932		
Number of marriages	11,063			
Marriages with dowry	79%			
Dowry amount (Rs)	136,127	287,133	1,54,894	79,673
Observations	21,095			

Notes: Columns (3) and (4) split reported dowry payments across pre- and post-periods only for married individuals.

## 5 Empirical Strategy

This paper aims to causally identify the impact of the amendment along two fronts- changes in dowry payments as a measure of social customs and changes in educational attainment as a measure of human capital formation. A cross-sectional comparison of children by birth parity across households is unable to credibly identify impacts due to presence of unobservables impacting both exposure to dowry payments and educational attainment. Comparison of children within households across time is also insufficient, as educational attainment presumably increases mechanically as a function of time. To address these identification concerns, the em-

irical strategy exploits both within, as well as across birth cohort variation, in exposure to the amendment. The following sections discuss details on challenges to identification, treatment assignment, and empirical equations.

## 5.1 Identification

The identification strategy relies on the appropriateness of using Non-Muslim and Muslim groups as treatment and control, respectively. There are four conditions that should be satisfied for the estimation strategy to identify causal estimates. First, the definition of Muslim and Non-Muslim groups should be stable through the time period of analysis. Within India, religious conversions are rare and intermarriages comprise only 1% of the sample. Thus, identification of family religion is equivalent to individual-level exposure to the amendment.

Second, Muslim dowry practices should not have been affected by the amendment. Figure 3 provides supporting evidence that the amendment impacted Non-Muslim dowries only. Payments declined for Non-Muslim households, with no discernible trend for Muslim households.

Third, educational attainment across treatment and control groups should trend in a similar manner prior to the amendment. The parallel trend assumption is verified through an empirical test complemented with institutional knowledge (Roth, 2022). Figure 8 suggests no significant differences across treatment and control groups in the pre-period.<sup>13</sup> Additionally, a robustness check to account for the possibility of different states trending differently over the study period does not affect the findings (Table 13).

India's constitutionally mandated secular directive deems unconstitutional any religion-based targeting for educational policy. Thus, the institutional context rules out differential educational policy based on religion. Furthermore, across states, time invariant differences in educational policy accounted for using state-level fixed effects.

Fourth, presence of contemporaneous policies that changed schooling costs over the time period of the study have the potential to bias estimates. The District Primary Education Program (DPEP) is the only policy with a potential overlap.<sup>14</sup> However, the DPEP was launched

---

<sup>13</sup>A joint test of significance for all pre-coefficients reveals an F-stat of 8.10 and 9.23 for men and women, respectively. This suggests that pre-amendment differences were jointly significant; a visual inspection of coefficients provides reassurances for the parallel trends assumption

<sup>14</sup>The DPEP was among the first policy initiatives launched by the Government of India to achieve universal primary education.

in 1994, and rolled out in a staggered manner targeting areas of low levels of mothers (Sunder, 2020). All the insights described here, when combined, provide support for the empirical strategy.

## 5.2 Treatment Assignment

Individuals vary in their exposure to the amendment based on household religion and if they were already likely to be married when the amendment was enacted. Treatment assignment within birth year cohorts is based on household religion. Individuals belonging to Non-Muslim households are considered treated. Across cohort exposure is determined by using a reference age at marriage interval and the birth year of individual.

Birth year cohorts older than the reference age at marriage interval at the time of amendment are considered pre-cohorts. Post-cohorts consist of birth year cohorts that are below the reference age at marriage interval at the time of amendment. The use of birth year in defining cohort exposure is deliberate and done to avoid endogeneity concerns associated with year of marriage.<sup>15</sup> The birth cohorts in between the pre and post-assignment are partially treated and dropped to obtain clean estimates. Figure 1 details this process for men.

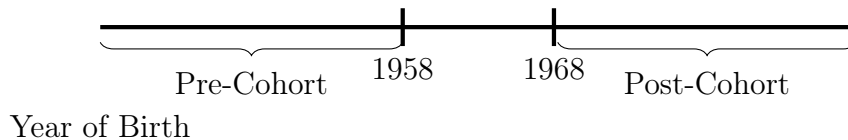


Figure 1: *Cohort Assignment for men with reference age at marriage interval [16,26]. If year of birth is less than the upper limit on the age of marriage interval, then the individual is assigned to the pre-cohort. If the year of birth is greater than the lower limit on the marriage interval, then the individual is considered as treated.*

The reference age at marriage interval is determined using the data sample. Figure 4 plots distributions of age at marriage by gender. On average, women marry earlier with lower variance in the distribution, whereas men typically marry later and experience greater variability in the age at marriage. The reference age at marriage incorporates these key differences and is defined as the median age plus or minus 1 standard deviation (SD). As per the definition, the interval of marriage for men is between 16-26 years, and for women, it is 15-21 years. A robustness

---

<sup>15</sup>Prior literature has used age of menarche as an instrument for marriageable age for girls (Field and Ambrus, 2008); however, the REDS survey does not collect information on the age of menarche. Moreover, age of menarche is not a valid instrument for boys.

check using the legal age of marriage to define across cohort exposure (columns 4 through 6 of Table 13) presents comparable estimates.

### 5.3 Empirical Equation

The exposure of an individual is jointly determined by their religion as well as the age at the time of the amendment. Based on this definition, the exposure provides the necessary spatial and temporal variations required to identify the causal impact of the policy. The first stage equation is

$$y_{icfst} = c_1 + \beta_{1ct} + \beta_2 Post_{ict} * NonMuslim_{fs} + \beta_3 NonMuslim_{fs} + X'_{ifs} \gamma + \alpha_s + \varepsilon_{icfst} \quad (1)$$

First-stage impacts on dowry payments are examined along two dimensions: if any dowry is paid and the dowry amount paid. To study the extensive margin,  $y_{icfst}$  is an indicator equal to 1 if any dowry was paid by individual  $i$ , born in cohort  $c$ , belonging to family  $f$ , in state  $s$ , and married in year  $t$ . To study the intensive margin,  $y_{icfst}$  captures the monetary amount of dowry<sup>16</sup> for individual  $i$ , born in cohort  $c$ , belonging to family  $f$ , and state  $s$ , and married in year  $t$ .  $Post_t$  is an indicator equal to 1 if the marriage occurred in the post-amendment period and  $NonMuslim_{fs}$  identifies the religion of the household  $f$  and state  $s$  to which the individual belongs.

The coefficient of interest is  $\beta_2$ , which captures the impact of the amendment on difference in dowry payments for Non-Muslim girls in the pre versus post-period, as compared to differences in dowry payment for Muslim girls.  $\beta_{1ct}$  is the cohort of birth fixed effects to absorb year-specific shocks.  $X'_{ifs}$  are individual family-level controls that include total number of household members, reported household income, caste of individual, and birth order.  $\alpha_s$  are state fixed effects to control for time-invariant state characteristics. To study the impact on dowry payments, the analysis sample is restricted to married girls. Results are reported in columns 1 (intensive) and 2 (extensive) of Table 2.

To estimate the impact of change in dowry practices on educational attainment, the following difference-in-differences framework is employed

---

<sup>16</sup>Dowry amounts are recorded in rupees and deflated to ease comparison across years.

$$y_{icfs} = c_1 + \beta_{1ct} + \beta_2 Post_{ic} * NonMuslim_{fs} + \beta_3 NonMuslim_{fs} + X'_{ifs} \gamma + \alpha_s + \alpha_g + \varepsilon_{icfs} \quad (2)$$

$y_{icfs}$  is the years of education for individual  $i$ , born in cohort  $c$ , and belonging to family  $f$ , in state  $s$ . Equation 2 refers to the main estimation equation, in which  $\beta_{1ct}$  is a cohort of birth fixed effect equivalent to a year fixed effect and controls for year-specific unobservables.  $Post_{ic}$  is a dummy equaling 1 when individual  $i$  belonged to the Post cohort based, on the reference age at marriage definition.  $NonMuslim_{fs}$  is a dummy equaling 1 if family  $f$  in state  $s$  followed any religion except Islam. Finally,  $X_{ifs}$  is a vector of household co-variate, including total household members and caste group fixed effects, as well as birth order fixed effects. Caste group fixed effects are included to account for differences in household characteristics by caste group.  $\alpha_s$  includes state-fixed effects to absorb time-invariant unobservable differences in educational policies across states and  $\alpha_g$  are gender fixed effects.

The coefficient  $\beta_2$  is the main coefficient of interest and provides the impact of the policy on the treated or Non-Muslim religious groups. The preferred specification uses birth order fixed effects to interpret coefficients as the effect of changes in dowry practices on the educational attainment of the average child in the cohort. Column (3) in Table 3 presents the results from estimating equation 2 for the sample of females. Column (3) in Table 16 reports estimated coefficients, using equation 2 on a sample of males.

## 6 Results

Figure 2 provides graphical evidence on the impact of the amendment on dowry payment practice, using a local polynomial regression of dowry amounts on a time trend. This figure documents a decline in dowry payments over time, with steeper declines in the post-amendment period. The steeper post amendment slope is interpreted to indicate a greater reduction in dowry amounts in the post-amendment period.

Graphical evidence on the first-stage impact of the amendment on dowry practice is augmented with an empirical test. Changes in dowry payment practices are examined along the intensive and extensive margins in columns 1 and 2, respectively of Table 2. Findings indicate

a decline of 18% in the log dowry amount paid. The estimates on the extensive margin, a measure of whether any dowry was exchanged, shows no significant difference in the post-period for Non Muslim marriages. Given that social customs are slow to adjust and dowry is a widely prevalent phenomena India, these results suggest that the amendment had expressive value, in that it impacted the average dowry amounts and was successful in communicating the changing institutional stance on the practice of dowry. Furthermore, these results are in line with previous literature that examines the first-stage impact of this amendment on dowry practices (Calvi and Keskar, 2021).

Raw data on educational attainment and dowry amounts, plotted in Figure 6, suggest a positive correlation between educational attainment and dowry for both men and women. Higher educated men receive more dowry while families pay higher dowries to marry off more educated females. However, the relationship between dowry and educational attainment is stronger for women, with a noticeably steeper slope.

Table 3 finds evidence to support a decline in educational attainment for girls due to the amendment. Using equation 2 for estimation, column (3) of Table 3 reports coefficients on *NonMuslim*, as well as the interaction term *PostXNonMuslim*. The coefficient of interest is the interaction term *PostXNonMuslim*, which represents the change in years of education for Non-Muslim children belonging to cohorts exposed to the amendment, as compared to exposed Muslim cohorts.

The estimation result suggests a negative and significant reduction in the years of education for Non-Muslims girls in the exposed cohorts across variations in specifications. On average, Non-Muslim girls attain 0.65 fewer years of education, which represents a 18% reduction over the mean (column (3) in Table 3). The second-stage result suggests that dowry and educational attainment are complements in the human capital production function for females in the sample.

Moreover, this result is consistent across the parsimonious specification (column (3) in Table 3), which controls for household and birth order fixed effects only to the preferred specification, as in equation 2 (column (3) in Table 3). The effect size reduces from a drop of 22% to a decline of 18% in educational attainment with the inclusion of greater number of controls. However, the result consistently is a negative and significant decline in educational attainment for females.

Figure 7 examines spousal gaps in education across pre and post-amendment periods. Con-



sistent with the decline in years of education for females, Figure 7 suggests that the spousal education gap increases between spouses, specifically by 2-3 years. For males, the education gap distribution shifts rightward across all levels, with greatest changes between 2-3 years. This evidence is interpreted to suggest that declines in female education carry forward to the marriage market and lead to increased spousal education gaps.

## 7 Mechanisms

### 7.1 Dowry and Education as Complements

To link the decline in education to changes in dowry payments, differences in gender-based exposure to the amendment are utilized. The rotating capital fund view of dowry payments considers dowry as a flow of resources (Rajaraman, 1983). For the bride’s family, this is an outflow of resources, whereas for the groom’s family, dowry is an inflow of resources. A crucial element is the differences in fungibility of this flow across the bride’s and groom’s families. For the bride’s family, dowry is an expenditure outlay, whereas for the groom’s family, dowry is received as a sum of valuable goods and cash. Given differences in fungibility of dowry by gender, changes in dowry payments are more likely to affect the family of the bride.

Using plausibly exogenous variation in gender of the first-born (Anukriti et al., 2022), with the rotating capital fund view of dowry payments, allows for a causal analysis linking changes in dowry payments to educational attainment impacts. Families with firstborn girls exposed to the amendment should see a differential impact on educational attainment, as compared to families with firstborn boy exposure. To test this hypothesis, the following triple difference estimation strategy is implemented

$$\begin{aligned}
y_{icfs} = & c_1 + \beta_{1ct} + \beta_2 Post_{ic} * NonMuslim_{fs} + \beta_3 FBFemale_{ifs} + \beta_4 NonMuslim_{fs} * FBFemale_{ifs} \\
& + \beta_5 Post_{ic} * FBFemale_{ifs} + \beta_6 Post_{ic} * NonMuslim_{fs} * FBFemale_{ifs} \\
& + X'_{ifs} \gamma + \alpha_s + \alpha_g + \varepsilon_{ifsc}
\end{aligned} \tag{3}$$

$FBFemale_{ifs}$  is an indicator equaling 1 when the gender of the firstborn individual  $i$  in family  $f$  in state  $s$  is female. The test statistic  $\beta_2 Post_{ic} * NonMuslim_{fs} + \beta_6 Post_{ic} * NonMuslim_{fs} *$

$FBFemale_{ifs}$  in equation 3 represents the overall effect of having a female firstborn exposed to the amendment, as compared to a male firstborn. Estimation results for males, females, and pooled samples are reported in columns 1 through 3 of Table 4, along with the test statistic of interest.

Findings in Table 4 suggest that the overall effect of having a female firstborn exposed to the amendment led to a significant reduction in the total years of education for females in the sample by 0.68 years. No such change is reported for the males or for the overall sample. Additionally, for males belonging to households where the firstborn female is exposed to the amendment, there is a significant reduction in years  $-(1.08)$  of educational attainment as compared to males belonging to households in which, the firstborn male is exposed to the amendment. Taken together, these results suggest that the amendment led to overall cutbacks in education for females and that these effects were driven by changes in dowry payments.<sup>17</sup>

## 7.2 Dowry and Education as Signals of Traditional Adherence

For dowry and education to serve as signals, a decline in dowry payments should reduce education for females, and this impact should be more pronounced within households with greater reliance on dowry payments, within communities with stricter dowry payment customs, and for households with greater adherence to other traditional customs.

Pre-reform dowry payments are used as a measure of household reliance on dowry payments, and changes in educational attainment are examined for households above or below pre-reform median dowry payments. Greater reduction in educational attainment for households with higher reliance on dowry payments provides evidence on the strength of association between education and dowry.

The empirical test implements equation 2 on subsamples of median dowry payments above and below baseline. To tag households as above the baseline in median dowry payments, states are first divided into regions corresponding to similar measures of social norms (Carranza, 2014). A region-specific, pre-reform median dowry payment is calculated. Households are then tagged as above or below this region-specific measure of dowry payments. The sample is split into 4,218 households belonging to above median and 2,471 households belonging to below median.

---

<sup>17</sup>Additional analysis built on a more simplistic view of the rotating capital fund is presented in the Appendix. Results align with the findings reported in this section.

Table 5 reports the results of the estimation across males, females, and a pooled sample of the data (columns 1, 2, and 3 respectively).

Females in households with greater pre-reform dowry payments experience a greater intensity in reduction in educational attainment as compared to the overall sample. Years of education declines by 0.85 years for treated females in the post-reform period; this represents a 18% decline. There are no effects on educational attainment for males in either type of household and no effects on females in below median payment households.

Anthropological insights into the relative prevalence and importance of dowry (Madan, 1975) are utilized to identify communities with stricter dowry payment customs. Consistent with papers (Nishimura, 1994; Roulet, 1996) that provide qualitative evidence on the low prevalence of dowry among daily wage-earners and a high prevalence of dowry among salaried and wage earning occupation classes, households are categorized into “Agricultural Wage Earners,” “Self-employed farming,” and “Non-farm salary & wage” based on the occupation of the head of the household.<sup>18</sup> “Agricultural Wage Earners” are the poorest income group in the sample. Estimates from a subsample analysis for each occupation category, using equation equation 2 are presented in Table 6.

Occupational classes most invested in dowry payments (Nishimura, 1994) experience the largest decreases in years of education for both male as well as female children (Table 6). Children belonging to non-farm salary and wage earners have a 4.85 and 2.35 decline in years of education for both males and females, respectively. There are no significant changes in educational attainment for self-employed farming households. Consistent with lower prevalence of dowry payments for agricultural wage earners, Table 6 finds gains in educational attainment for treated females in the post-reform period for this occupation group. The results suggest that the impact on educational attainment for agricultural wage-earning households versus non-farm salary and wage earning households differs in a manner consistent with dowry payment custom.

To identify households with greater adherence to traditional customs, household proscription to a patriarchal norm— gender segregation during meals - is used as a proxy. Households are classified as “traditional” and “non traditional,” based on whether there is mixing by genders during meal time. “Traditional” households in the sample are those where men and women

---

<sup>18</sup>“Self-employed farming” includes self employed farming and agricultural family worker. “Non-farm salary & wage” includes categories of self-employed, non-farming salary , non-agricultural wages and non-agricultural family worker.

consume meals separately and men consume meals first, suggesting the presence of gendered social attitudes in these households (Ibnouf, 2009; Neogy, 2010). In the absence of survey data on gender role attitudes, gender segregation in meal sharing is the closest proxy to traditional social custom that are often expressed as unequal gender norms. The specification in equation 2 is used to estimate coefficients for subsamples of “Traditional” and “Non traditional” households. Results are reported in Table 7 for males, females, and overall samples (columns 1 and 4, 2 and 5, and 3 and 6) for males, females, and overall samples, respectively). The results are at best suggestive evidence on greater reduction in educational attainment for females in more traditional households. The coefficient of interest is negative and higher for traditional, as compared to nontraditional households. Both coefficients are imprecisely estimated, which is unsurprising given the blunt and noisy method of capturing traditional behavior. Given the data constraints, this result is interpreted as correlational evidence that households with stronger traditional customs are more likely to consider using lower education to signal in the absence of dowry payments.

### 7.3 Competing Mechanisms

This section explores competing mechanisms that have the potential to confound the documented complementary relationship between dowry and education. Empirical evidence is presented to rule out price effect and differences in bequest ability as potential mechanisms.

A price effect, whereby dowry and education are substitutes and the amendment increases the demand and consequently the price of education, is unlikely to be an alternate explanation for the observed decline in educational attainment. Under this scenario an increase in prices is most likely to hurt the poorest households, and furthermore, the effect on educational attainment should not vary by gender.

The analysis finds differential effects on education by gender through all specifications. Additionally, Table 6 suggests that, for the poorest households in the sample, agricultural wage earners (columns 7 through 9 ), females experienced gains in educational attainment. This result emphasizes that for the poorest group for whom dowry practices are least important, female education increases substantially. This result lends support to the use of dowry payments to signal traditional adherence rather than the presence of a price effect.

In the absence of dowry payments, perhaps, parents plan for increases in bequests, a more costly form of wealth transfer at the expense of educational attainment. Under this scenario, the relationship between education and dowry is expected to be mediated by a measure of bequest ability. For households more likely to use bequests, the reduction in educational attainment should be higher and more likely to impact females.

To examine this hypothesis, landholding owning status of households is used as a proxy for bequest ability. Households are classified into marginal and non-marginal landholding categories based on reported total landholding size of the households.<sup>19</sup> Using the estimation strategy in equation 2, impact on educational attainment is estimated by subsamples based on landholding size (Table 8).

Table 8 suggests no difference in educational attainment across households of low or high bequest ability. The estimated coefficient of interest for females is remarkably similar for marginal as well as non-marginal households. Further, there are no differences in educational attainment for males and females within bequest ability. These results help rule out an offsetting between education and bequest. This result is unsurprising, given the extreme aversion to increasing female bequests in the Indian context (Roy, 2015).

## 8 Secondary Outcomes

### 8.1 Age at Marriage

Impacts of the amendment on age at marriage are examined for documenting effects on the marriage market. Changes in age at marriage are an alternate margin of adjustment by households. Additionally, large shifts in age at marriage has implications for treatment assignment considered in this paper.

Shifts in the distribution of age at marriage across pre- and post-periods are documented in Figure 5. The age at marriage distribution shifts right in the post-period across genders. The graphical evidence is consistent with an increase in age at marriage in the post-amendment period. To examine these changes empirically, equation 1 is estimated for the sample of males, females, and a pooled sample. Estimation results are presented in Table 9.

---

<sup>19</sup>The census definition of marginal landholding as a landholding size less than 2 hectares (or 5 acres) of land is followed.

Results reported in Table 9 corroborate the graphical evidence. Age at marriage increases by around 4% for both genders in the post-period.<sup>20</sup> Similar gains in age at marriage suggest that matches in the post-period take longer in the absence of dowry payments. However, the increase in age at marriage for females is puzzling, given the reduction in educational attainment. To explore this result further, the analysis is performed by birth orders.

Testing for heterogeneous impacts on age at marriage by birth order provides insights into household strategy, as well household learning with respect to the matching process in the absence of dowry. Equation 1 is estimated for subsamples of firstborn, second born and higher order births ( $\geq 3$ ) for males and females. Results are reported in columns 1 through 3 for males and columns 4 through 6 for females in Table 10. The overall increase in age at marriage can be attributed to younger siblings within households. For both genders, later born children display significantly greater age at marriage. For males, higher order male children marry later, while there is correlational evidence to support firstborn and second born males rush marriages. For females, second born females delay marriages, with higher order births also showing a rise in the age at marriage, although the coefficient is estimated imprecisely. These results may be interpreted to suggest a greater time to match in the absence of dowry payments.

## 8.2 Variation in enforcement capabilities

To provide insights on the interaction between enforcement capacity and social customs, impacts on educational attainment are examined relative to spatial variation in enforcement of the amendment. Sub-national differences in institutional capacity, gender attitudes, and existing state-level policies create a reasonable variation in the enforcement of the amendment. In the absence of direct institutional measures of enforcement, the analysis uses two complementary proxies.

The first proxy is the presence of prior state-level legal procedures targeting dowry payments. Between the years 1975 and 1976, the states of Bihar, Haryana, Himachal Pradesh, and Punjab implemented independent state amendments to the Dowry Prohibition Act (1961). This paper argues that the 1985 amendment had a lesser impact in terms of changes in enforcement in

---

<sup>20</sup>This increase is moderate and lies within the interval of age at marriage, and thus, does not pose threats to identification.

states with preexisting amendments.<sup>21</sup> Thus, the change in enforcement was higher in states with no preexisting amendments.

States are classified as “Early State-Level Amendment” and “No State-Level Amendment,” based on whether there was a state-level amendment effective at the time of the 1985 amendment to the Anti-Dowry law. Equation 2 is estimated for the two sub samples of states, and estimates on years of educational attainment are reported in Table 11. The findings suggest that the decline in female education occurred primarily in states with no preexisting state-level amendments or in the subsample of states with the highest change in enforcement. The results do not support any significant changes in educational attainment for either gender in states with preexisting state-level amendments or for males in no amendment states. This result is interpreted to suggest that changes in enforcement directed at a long-standing cultural tradition led to decline in educational attainment for females.

The second proxy utilizes a within state variation in enforcement in the form of changes in the magnitude of dowry payments across pre and post-amendment at the district level. Districts that experienced a magnitude of shift in dowry payments greater than the median district in the state are classified as “Above Median Exposure,” and similarly for “Below Median Exposure” districts. Under the assumption that changes in enforcement brought about shifts in dowry payments, for districts with larger than median shift in dowry payments, enforcement is higher. To test the hypothesis relating greater enforcement to larger declines in female educational attainment, equation 2 is estimated for the two subsamples and results are reported in Table 12.

Estimates in Table 12 suggest a significant decline in educational attainment for females in districts with above median exposure or greater enforcement of about 0.95 years. For districts with below median exposure and for males across both types of districts, there are no such effects. This result is interpreted to provide complementary evidence on the interaction of enforcement with social customs. Areas with higher enforcement are those that experienced a significant decline in female educational attainment. These results provide further support for the use of dowry and education as signals by substantiating a backlash against laws that conflict with social customs.

---

<sup>21</sup>(Alfano, 2017) used a similar argument to examine birth rates by gender.

## 9 Robustness and Placebo

Robustness checks are implemented to address four broad concerns. The first is treatment assignment; the second is the possibility of states trending differently over the study period. The third check accounts for within-state correlation in idiosyncratic shocks by using different levels of clustering. Finally, an alternate estimator is implemented to test the robustness of the main result (De Chaisemartin and d’Haultfoeuille, 2020). Along with robustness checks, a placebo test is implemented. Additionally, the parallel trend assumption is tested, which is necessary for identification.

To address endogeneity concerns related to treatment assignment, the legal age of marriage is considered to assign across cohort exposure. Columns 4 through 6 in Table 13 present the results of estimation of regression equation 2. The estimates corroborate a negative and significant reduction in years of education for females and are consistent with the exposure definition used in this paper. Despite the consistent results, the paper prefers using an age of marriage interval since legal marriage age is hard to enforce and descriptive evidence suggests that the legal age of marriage is often ignored in household decision-making.

The possibility that different states were trending differently over time in a manner that impacted educational attainment would reduce the credibility of the estimates. Prior literature that studies the pro-women inheritance right reforms in India document impacts on a range of fertility, preference for sons, educational attainment, and dowry (Bhalotra and Cochrane, 2010; Roy, 2015). The amendment studied in these studies was staggered across states and years and impacted states in the sample.<sup>22</sup> To account for difference in time paths across states, state time trends are included as controls in the main specification. Columns 1 through 3 in Table 13 present the estimation results for the entire sample. No qualitative changes in results are observed at the overall level; rather, the coefficient on the interaction term is of a larger magnitude, suggesting that the experience of states over the study period was meaningful. The inclusion of state time trends is also important to account for states that had preexisting amendments at the time the 1985 amendment was enacted.

Within the context of the study, there is potential for within-state correlation in errors,

---

<sup>22</sup>Kerala was one of the first states to amend the Hindu Succession Act (1976), followed by Andhra-Pradesh (1986), Tamil Nadu (1989), Maharashtra (1994), and lastly by Karnataka (1994).



since each state has autonomy in designing educational policies and infrastructure spending. Such a set-up raises concern about the presence of within-state correlation in error terms when modeling educational attainment of individuals. To account for this intra-cluster correlation, the analysis clusters standard errors at the state level (Table 13, columns 7 through 9), as well as at the state-year level (Table 14) for females.<sup>23</sup> The negative and significant impact on female education is robust to different levels of clustering of standard errors. Taken together, the results in Tables 13 and 14 indicate that decline in female education is a strong result, even when accounting for unobserved shocks within states and across state years.

To account for heterogeneous treatment effect across states and time periods, an alternate estimator is used. Table 15 presents estimation results on educational attainment using the estimator proposed by De Chaisemartin and d’Haultfoeuille (2020), which is robust to heterogeneous and dynamic treatment effects. The decline in educational attainment is robust to the use of an alternate estimator and provides further confidence in the estimated effects.

Investment in boys’ education indirectly influences the dowry that the boy receives, from whom the boy is likely to marry eventually. If the dowry is a small component of the lifetime returns to education for men, then changes in dowry payments are unlikely to influence educational attainment for men. Furthermore, men are less likely to be affected by the suggestive gender unequal customs. Thus, examining the impact of the amendment on the educational attainment of boys is a close placebo test.

Table 16 reports the results from estimation of equation (2) on the educational attainment of males as a placebo outcome. On average, educational attainment for men should not be impacted by the dowry amendment. Across specifications, the results are consistent and suggest no changes in educational attainment for non-Muslim males in the post-period.

The empirical strategy requires that the outcome trends similarly across treatment and control groups in the pre-period to identify causal estimates. To provide empirical evidence on parallel trends assumption, the following test is set up. Using a similar empirical equation as in equation 2, the interaction on year of birth with treatment status for the untreated cohorts is examined over the pre-period. Figure 8 plot the resulting interaction coefficients from this estimation for males (panel (a)) and females (panel (b)), respectively. Graphical

---

<sup>23</sup>Estimation results for males and the entire sample are presented in Appendix Table 20, and Table 21 respectively

evidence supports the conclusion that for both males and females, the majority of the cohorts did not differ significantly in their educational attainment across treatment and control in the pre-period. This empirical evidence is combined with the institutional knowledge on state-level educational policy to claim support for the parallel trends assumption.

## 10 Discussion

My paper attempts to disentangle the complex interaction between human capital production, culture, and formal institutions by exploiting an institutional change in India. By drawing attention to the consequences of altering customs, I provide an empirical setting to study the interaction of laws with social customs. I document an adverse impact on educational attainment for women, with larger declines in years of education consistent with larger shifts in dowry payments. To explain these results, I highlight the role of dowry payments and female educational attainment as signals to communicate adherence to traditional customs. I also find that changes in enforcement capacity are negatively related to declines in educational attainment for women.

The findings linking increases in enforcement capacity with stronger declines in educational attainment suggest the presence of a backlash effect. However, this result hinges on the appropriateness of using magnitude of changes in dowry payments as a proxy for enforcement capacity. The use of this specific proxy may be misleading if changes in dowry payments are correlated with strength of social customs. If areas with weaker social custom of dowry experienced larger changes in dowry payments, then the measure of enforcement captures strength of the custom. The presence of state fixed effects in the estimation alleviates these concerns insofar as time-invariant social customs. Thus, the estimates are conditional on controlling for state-specific social customs. An additional concern that the analysis is unable to comprehensively rule out is the possibility of redistribution of public resources to support amendment implementation. While there is no anecdotal evidence to suggest a state-level redistribution across public services to support implementation of the amendment, in the absence of fiscal expense data, crowding out remains a possibility.

The increase in age at marriage as a result of the amendment is noteworthy, considering the dowry amendment law did not directly target age at marriage. This suggests the potential

of spillover impacts on age at marriage from policies that affect marriage markets. While most countries have legal age at marriage laws, such laws often go unenforced in developing countries (Collin and Talbot, 2023). With enforcement rates being stagnant over time, the results of this paper suggest that laws targeting the marriage market on alternate dimensions might be a viable policy alternative.

Decline in educational attainment along with increase in age at marriage for females is a puzzling result. Impacts on age at marriage by birth order provide insights in the form of learning effects by families. Households delay marriages presumably with higher time to match in the absence of dowry payments. This effect is strongest for higher birth order children who are more likely among siblings to be impacted by parental learning based on elder sibling marriages. Data limitations prevent an examination of investment in non-school skill formation by females to increase bridal value. Such questions are beyond the scope of this paper but provide interesting avenues for further research.

The findings of this paper should be interpreted with certain caveats given the data limitations. First, to rule out price effects the narrative assumes that the price of schooling responds to increased demands in education. However, in the absence of measures of schooling costs, this assumption is not testable. Second, land ownership is considered to be a proxy for bequest ability, with greater landholding being more conducive to passing on land as inheritance. This assumption is not directly testable in the data, but rests on higher costs of transfer based on the size of the landholding. In the absence of appropriate measures of traditional adherence, such as gender attitudes, the estimated coefficients are imprecise. The analysis can be strengthened with supplementary information on social capital to examine heterogeneity in results relative to levels of social capital. In the absence of spousal age, the inferences drawn from examining age at marriage are incomplete, and thus, do not allow comment on the marriage market matching process.

Though the context of this study is specific, the broader takeaway is to acknowledge that social customs do respond to expansions in enforcement capacity. However, gender-specific customs such as dowry, when challenged, have the potential to direct adverse changes. The results of this paper challenge prior beliefs on the relationship between different components of human capital formation and speaks to interactions between child-specific and general investment in

human capital in an environment unequal for genders.

## References

- Agte, P. and A. Bernhardt (2022). The economics of purity norms: Caste, status, and women’s work in India.
- Akerlof, G. A. (1980). A theory of social custom, of which unemployment may be one consequence. *The quarterly journal of economics* 94(4), 749–775.
- Alfano, M. (2017). Daughters, dowries, deliveries: The effect of marital payments on fertility choices in India. *Journal of Development Economics* 125, 89–104.
- Ali, D. A., K. Deininger, and M. Goldstein (2014). Environmental and gender impacts of land tenure regularization in Africa: Pilot evidence from Rwanda. *Journal of Development Economics* 110, 262–275.
- Anderson, S. (2003). Why dowry payments declined with modernization in Europe but are rising in India. *Journal of Political Economy* 111(2), 269–310.
- Anderson, S. (2007). The economics of dowry and brideprice. *Journal of Economic Perspectives* 21(4), 151–174.
- Anderson, S. and G. Genicot (2015). Suicide and property rights in India. *Journal of Development Economics* 114, 64–78.
- Anukriti, S., S. Kwon, and N. Prakash (2022). Saving for dowry: Evidence from rural India. *Journal of Development Economics* 154, 102750.
- Ashraf, N., N. Bau, N. Nunn, and A. Voena (2020). Bride price and female education. *Journal of Political Economy* 128(2), 591–641.
- Barfield, T. (2010). *Afghanistan: A Cultural and Political History*. Princeton University Press, Princeton, NJ.
- Becker, G. S. (1973). A theory of marriage: Part I. *Journal of Political Economy* 81(4), 813–846.
- Besley, T., A. Jensen, and T. Persson (2014). Norms, enforcement, and tax evasion. *The Review of Economics and Statistics*, 1–28.
- Bhalotra, S., R. Brulé, and S. Roy (2020). Women’s inheritance rights reform and the preference for sons in India. *Journal of Development Economics* 146, 102275.
- Bhalotra, S., A. Chakravarty, and S. Gulesci (2020). The price of gold: Dowry and death in India. *Journal of Development Economics* 143, 102413.
- Bhalotra, S. R. and T. Cochrane (2010). Where have all the young girls gone? Identification of sex selection in India. *IZA Discussion Paper No. 5381, Institute of Labor Economics (IZA), Bonn*.
- Bhaskar, V. (2019). The demographic transition and the position of women: A marriage market perspective. *The Economic Journal* 129(624), 2999–3024.
- Botticini, M. and A. Siow (2003). Why dowries? *American Economic Review* 93(4), 1385–1398.
- Buchmann, N., E. Field, R. Glennerster, S. Nazneen, and X. Y. Wang (2023, October). A signal to end child marriage: Theory and experimental evidence from Bangladesh. *American Economic Review* 113(10), 2645–88.
- Calvi, R. and A. Keskar (2021). ‘Til Dowry Do Us Part: Bargaining and Violence in Indian Families. *CEPR Discussion Paper No. DP15696*.
- Campante, F. and D. Yanagizawa-Drott (2015). Does religion affect economic growth and happiness? Evidence from Ramadan. *The Quarterly Journal of Economics* 130(2), 615–658.
- Carranza, E. (2014). Soil endowments, female labor force participation, and the demographic deficit of women in India. *American Economic Journal: Applied Economics* 6(4), 197–225.

- Chen, D. L. and S. Yeh (2014). The construction of morals. *Journal of Economic Behavior & Organization* 104, 84–105.
- Chiplunkar, G. and J. Weaver (2023). Marriage markets and the rise of dowry in India. *Journal of Development Economics* 164, 103115.
- Collin, M. and T. Talbot (2023). Are age-of-marriage laws enforced? Evidence from developing countries. *Journal of Development Economics* 160, 102950.
- De Chaisemartin, C. and X. d’Haultfoeuille (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review* 110(9), 2964–96.
- Deffains, B. and C. Fluet (2020). Social norms and legal design. *The Journal of Law, Economics, and Organization* 36(1), 139–169.
- Field, E. and A. Ambrus (2008). Early marriage, age of menarche, and female schooling attainment in bangladesh. *Journal of Political Economy* 116(5), 881–930.
- Hausmann, R., L. D. Tyson, S. Zahidi, et al. (2012). The global gender gap report 2012. World Economic Forum Geneva.
- Ibnouf, F. O. (2009). The role of women in providing and improving household food security in Sudan: Implications for reducing hunger and malnutrition. *Journal of International Women’s Studies* 10(4), 144–167.
- Jayachandran, S. and I. Kuziemko (2011). Why do mothers breastfeed girls less than boys? evidence and implications for child health in India. *The Quarterly Journal of Economics* 126(3), 1485–1538.
- Karim, S., C. Kwong, M. Shrivastava, and J. P. Tamvada (2022). My mother-in-law does not like it: resources, social norms, and entrepreneurial intentions of women in an emerging economy. *Small Business Economics*, 1–23.
- Madan, T. N. (1975). Structural implications of marriage in north India: Wife-givers and wife-takers among the pandits of Kashmir. *Contributions to Indian sociology* 9(2), 217–243.
- Mehmood, S., A. Seror, and D. L. Chen (2022). Religious Rituals: Evidence from Ramadan.
- Menski, W. F. (1998). Legal strategies for curbing the dowry problem.
- Montero, E. and D. Yang (2022). Religious festivals and economic development: Evidence from the timing of mexican saint day festivals. *American Economic Review* 112(10), 3176–3214.
- Musick, K., J. A. Seltzer, and C. R. Schwartz (2008). Neighborhood norms and substance use among teens. *Social Science Research* 37(1), 138–155.
- Neogy, S. (2010). Gender inequality, mothers’ health, and unequal distribution of food: Experience from a CARE project in India. *Gender & Development* 18(3), 479–489.
- Nishimura, Y. (1994). Marriage payments among the Nagarattars in South India. *Contributions to Indian Sociology* 28(2), 243–272.
- Parsons, T. (2010). *The rule of empires: Those who built them, those who endured them, and why they always fall*. Oxford University Press.
- Rajaraman, I. (1983). Economics of bride-price and dowry. *Economic and Political Weekly*, 275–279.
- Rao, V. (1993). The rising price of husbands: A hedonic analysis of dowry increases in rural India. *Journal of Political Economy* 101(4), 666–677.
- Roth, J. (2022). Pretest with caution: Event-study estimates after testing for parallel trends. *American Economic Review: Insights* 4(3), 305–22.
- Roulet, M. (1996). Dowry and prestige in North India. *Contributions to Indian Sociology* 30(1), 89–107.
- Roy, S. (2015). Empowering women? Inheritance rights, female education and dowry payments in India. *Journal of Development Economics* 114, 233–251.

- Roy, S. and H. F. Tam (2016). Princely states and gender differential in human capital investment. *Working Paper*.
- Sunder, N. (2020). Parents' schooling and intergenerational human capital: Evidence from India. *Unpublished Manuscript 24*.
- Suzuki, M. (2021). Affording Expensive Ceremonies: Evidence from Quinceañeras in Mexico.
- Vogl, T. S. (2013). Marriage institutions and sibling competition: Evidence from south asia. *The Quarterly Journal of Economics* 128(3), 1017–1072.
- Yamaguchi, K. (1989). A formal theory for male-preferring stopping rules of childbearing: Sex differences in birth order and in the number of siblings. *Demography* 26(3), 451–465.
- Zak, P. J. and S. Knack (2001). Trust and growth. *The Economic Journal* 111(470), 295–321.

# Figures & Tables

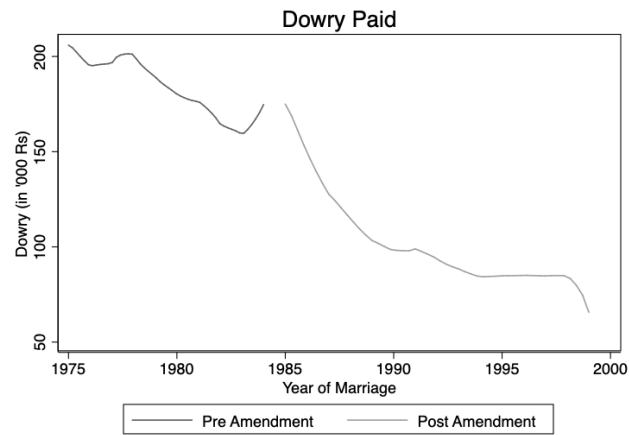


Figure 2: Trend in Dowry Amounts Pre and Post Amendment

The figure plots estimates from a kernel-weighted local polynomial regression of dowry paid amounts on year of marriage. Bandwidth is 5. Dowry paid is defined as net dowry paid. All dowry amounts are deflated using the year 2015 as baseline. Sample comprises all marriages since 1975 and includes of 10,014 observations. Source: REDS 1999

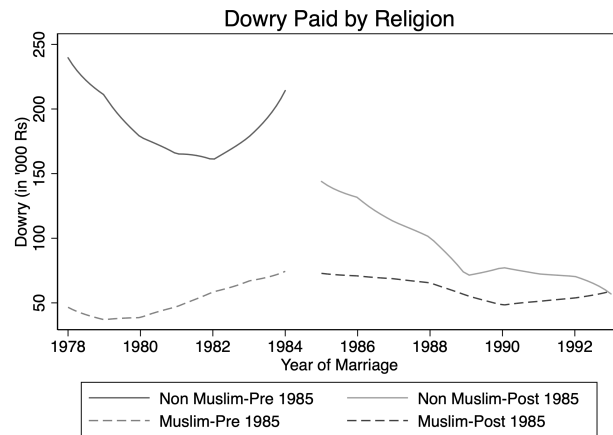


Figure 3: Trend in Dowry Amounts by Religion Pre and Post Amendment

The figure plots estimates from a kernel-weighted local polynomial regression of dowry paid amounts on year of marriage. Bandwidth is 2. Dowry amounts are in 2015 prices. Source: REDS 1999.

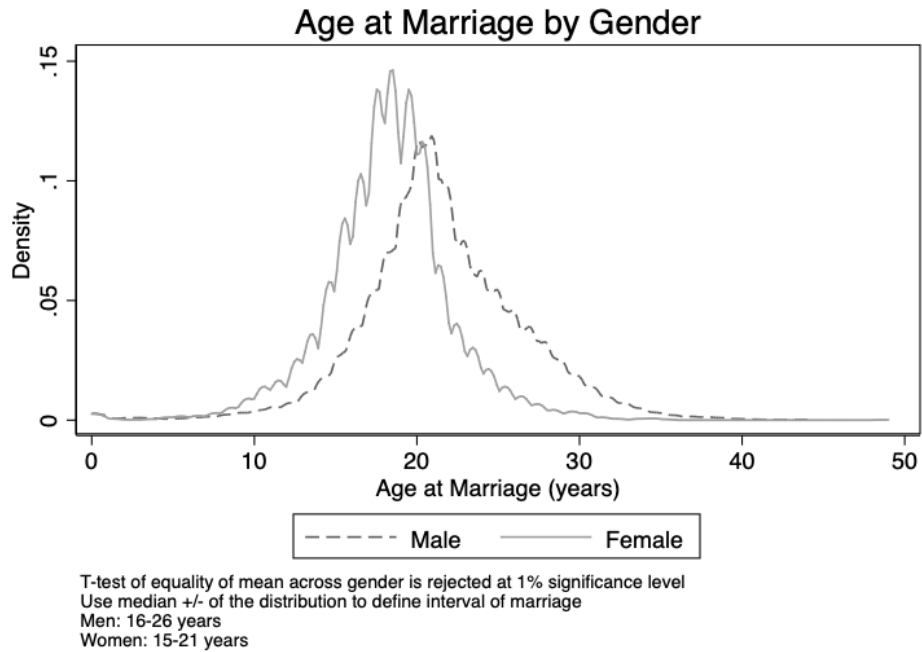


Figure 4: Marriageable age intervals

The figure plots the probability density for age at marriage using an Epanechnikov kernel function with optimal bandwidths. The sample comprises the entire married population and contains 11,054 observations over the time period 1960-1999. Source: REDS 1999.

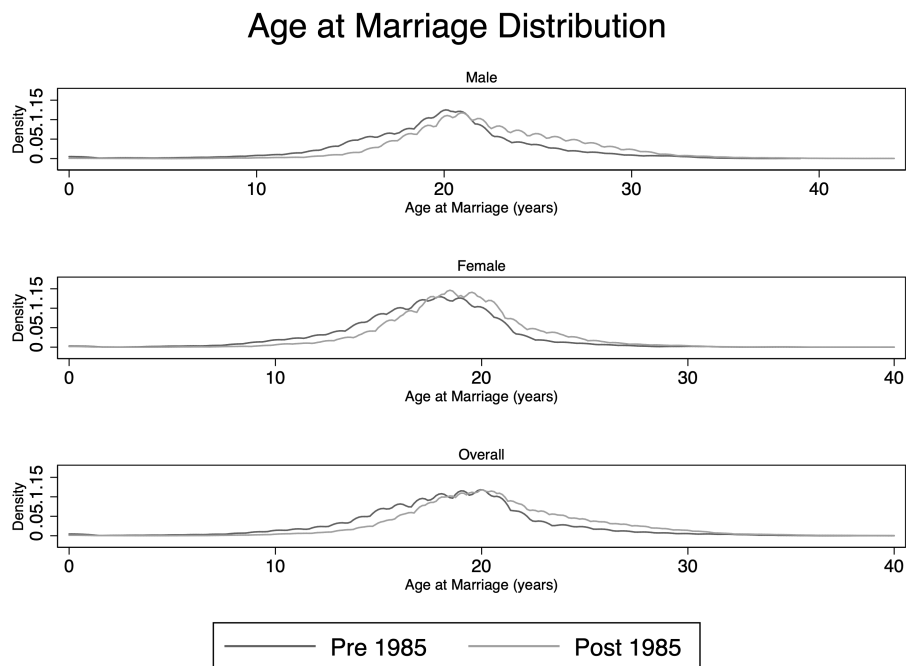


Figure 5: Age at Marriage Distribution by Gender and Time Period

The figure plots shifts in the probability density for age at marriage by gender across pre and post amendment periods. Probability densities are calculated using an Epanechnikov kernel function with optimal bandwidths. The sample comprises the entire married population and contains 11,054 observations over the time period 1960-1999. Source: REDS 1999.



## Education and Dowry

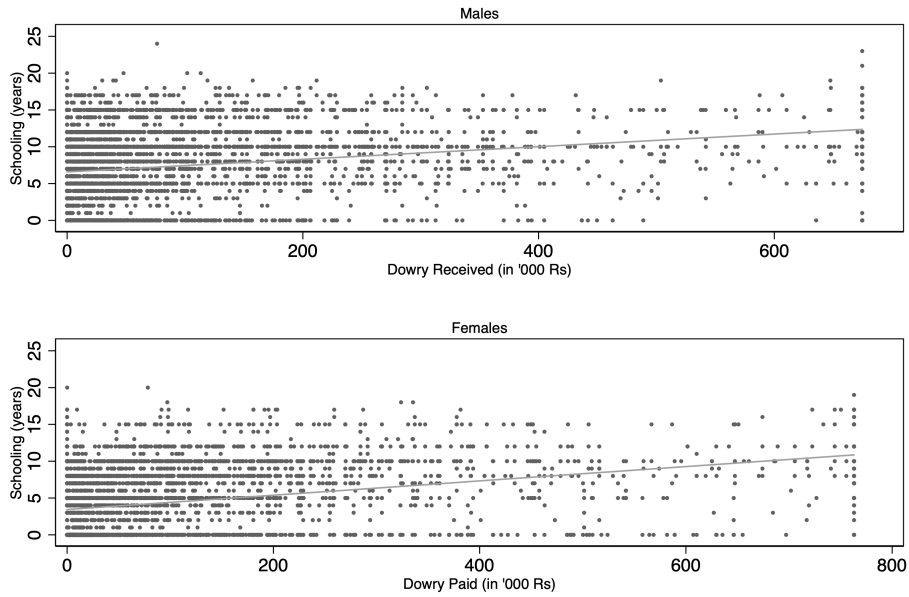


Figure 6: Correlation: Education and Dowry

The figure combines a scatter plot and a linear regression of years of education on dowry payments. The sample comprises the entire married population and contains 11,054 observations over the time period 1960-1999. Outliers are capped at the 99th percentile level. Dowry amounts are in 2015 prices. For males, the correlation coefficient is 0.214, and for females, it is 0.341.

Source: REDS 1999.

## Education Gap

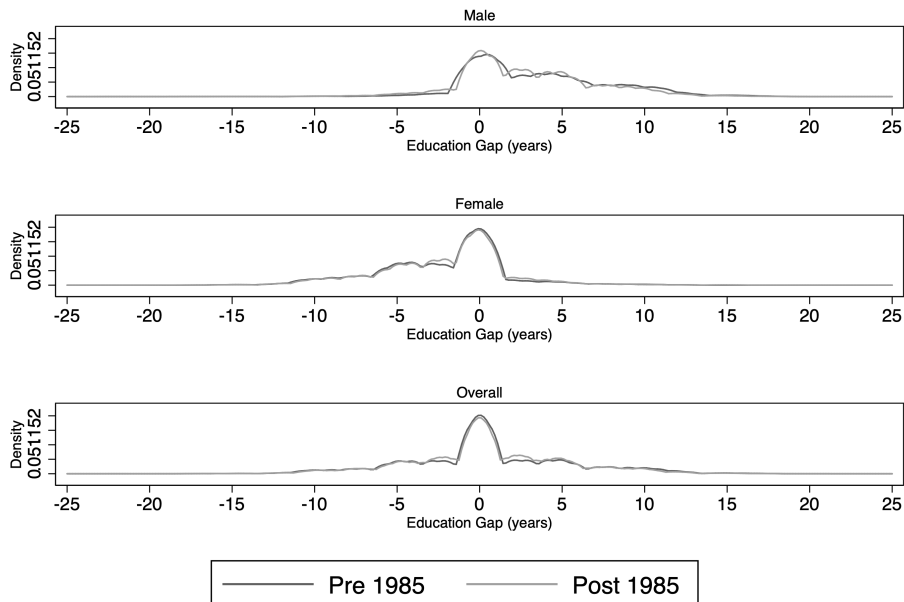
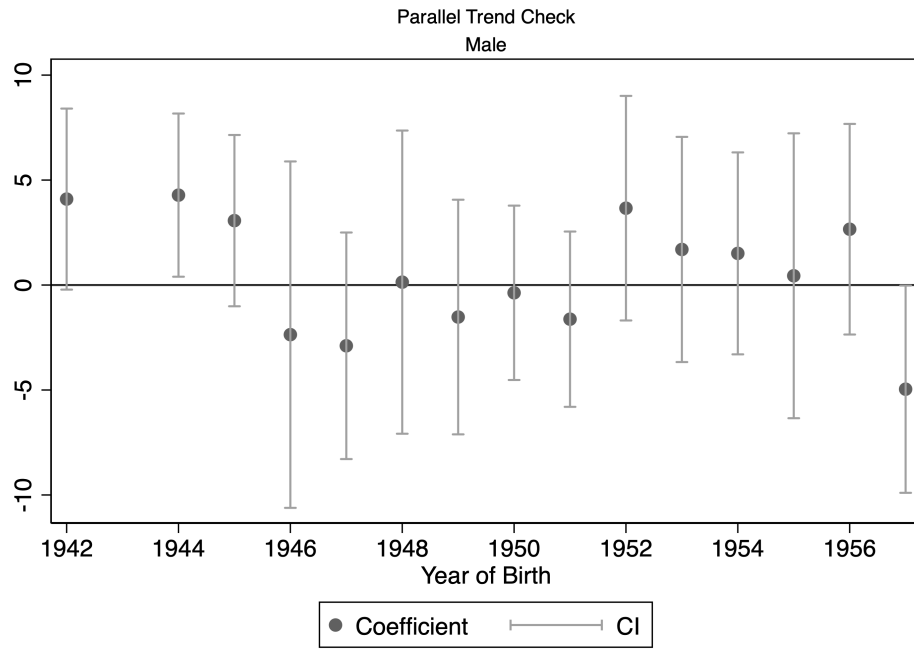
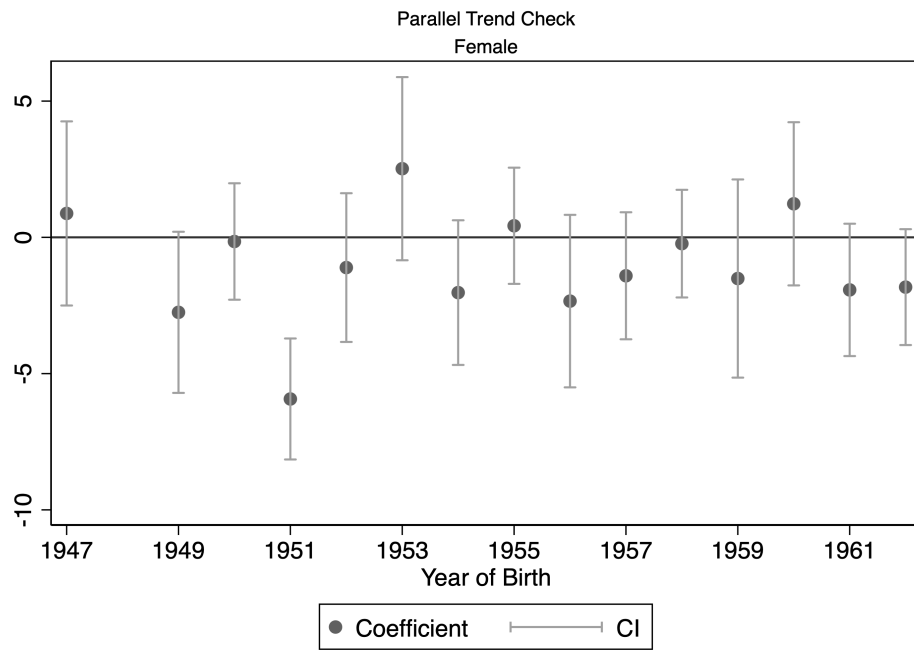


Figure 7: Shifts in Spousal Education Gap Pre and Post Amendment

The figure plots shifts in the probability density for the difference in spousal education by gender across pre and post amendment periods. Probability densities are calculated using an Epanechnikov kernel function with optimal bandwidths. The sample comprises the entire married population and contains 11,054 observations over the time period 1960-1999. Source: REDS 1999.



(a)



(b)

Figure 8: Parallel Trends

The figures plots the regression coefficient on the interaction between individual birth cohort with religion using specification 2 for all pre-exposure male cohorts in panel (a) and female cohorts in panel (b). 95% confidence intervals are reported. The sample comprises all pre-exposure males and females comprising 999 observations and 1,496 observations respectively. Source: REDS 1999.

Table 2: First Stage Impact

	Female	
	Log Dowry Amount (Rs) (1)	Dowry Paid (=1) (2)
Post X Non-Muslim	-0.18 (0.06)	0.02 (0.02)
Non-Muslim (=1)	-0.09 (0.15)	-0.09 (0.04)
Control mean	11.23	.75
Household control	X	X
Birth order fixed effect	X	X
Observations	4,198	5,446
$R^2$	0.38	0.25

Notes: This table reports results from estimating equation (1) on the sample of all married women in the son and daughter module of the 1999 wave of the REDS data. The outcome variable *Log Dowry Amount (Rs)* is used to capture intensive margin changes and is measured as the log of deflated dowry amount in rupees. The outcome variable *Dowry Paid = 1* is an indicator to capture extensive margin changes and measures if any positive dowry amount was paid. The variable *Post* is an indicator equal to 1 if the marriage occurs after 1985. *Non-Muslim* is an indicator to identify if the female belongs to a non-muslim household. Household controls include reported income in 1999, caste and total number of household members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 3: Impact on Years of Schooling for Females

	Female School-Yrs		
	(1)	(2)	(3)
Post X Non-Muslim	-0.82 (0.34) [0.02]	-0.72 (0.34) [0.05]	-0.65 (0.34) [0.07]
Non-Muslim (=1)	1.66 (0.31)	1.11 (0.32)	2.37 (0.39)
Control mean	3.6	3.6	3.6
Household control	X	X	X
Birth order fixed effect	X	X	X
Upper caste fixed effect		X	
All caste fixed effect			X
Effect Size	-22%	-19%	-18%
Observations	9,797	9,797	9,797
$R^2$	0.26	0.28	0.29

Notes: This table reports results from estimating equation (2) on the sample of all females in the son and daughter module of the 1999 wave of the REDS data. The outcome variable *Female School-Yrs* measures reported years of schooling. The variable *Post* is an indicator equal to 1 if the female belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the female belongs to a non-muslim household. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses. Brackets report p-values based on a t-test against the null for the coefficient of interest, using wild bootstrap heteroskedasticity robust errors.

Table 4: Heterogeneity by Firstborn Gender

	School-Yrs		
	Male (1)	Female (2)	ALL (3)
Post X Non-Muslim X FB Female	-1.08 (0.58)	-0.11 (0.35)	-0.08 (0.28)
Post X FB Female	0.97 (0.32)	-0.00 (0.28)	0.64 (0.21)
Non-Muslim X FB Female	0.47 (0.48)	0.29 (0.20)	-0.27 (0.18)
Post X Non-Muslim	0.34 (0.69)	-0.57 (0.38)	-0.19 (0.39)
Non-Muslim (=1)	0.80 (0.69)	2.19 (0.41)	1.66 (0.40)
Control mean	6.91	3.6	4.92
Gender fixed effect			X
Household control	X	X	X
Post X Non-Muslim + Post X Non-Muslim X FB Female	-.73	-.68	-.27
P-value	.38	.07	.5
Observations	11,298	9,797	21,095
R <sup>2</sup>	0.28	0.29	0.29

Notes: This table reports results from estimating equation (3) on the sample of all males and females in the son and daughter module of the 1999 wave of the REDS data. Columns (1), (2), and (3) report estimation results on the sub-sample of males, females, and the pooled, respectively. The outcome variable *School-Yrs* measures reported years of schooling. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the individual belongs to a non-Muslim household. The variable *FB Female* is an indicator equal to 1 if the individual belongs to a household with a firstborn female. The test statistic  $Post_{ic} * NonMuslim + Post * NonMuslim * FBFemale$  represents the overall effect on educational attainment of having a female firstborn exposed to the amendment, as compared to a male firstborn. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 5: Heterogeneity in Educational Attainment by Baseline Dowry Payments

	Above Median Payment			Below Median Payment		
	Male (1)	Female (2)	ALL (3)	Male (4)	Female (5)	ALL (6)
Post X Non-Muslim	-0.17 (0.83)	-0.85 (0.42)	-0.48 (0.46)	1.43 (1.29)	-1.05 (0.68)	0.12 (0.66)
Non-Muslim (=1)	0.92 (0.86)	2.43 (0.48)	1.65 (0.49)	-0.07 (1.25)	2.86 (0.77)	1.49 (0.70)
Control mean	7.64	4.55	5.84	5.62	2.17	3.46
Gender fixed effect			X			X
Household control	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X
Observations	6,857	5,862	12,719	4,441	3,935	8,376
R <sup>2</sup>	0.30	0.31	0.30	0.26	0.22	0.26

Notes: This table reports results from estimating equation (2) on the sample of all males and females in the son and daughter module of the 1999 wave of the REDS data. The estimation is performed separately for individuals belonging to households tagged as above and below baseline dowry payments. Columns (1), (2), and (3) report estimation results for males, females, and the pooled sample belonging to above median dowry paying households. Columns (4), (5), and (6) report estimation results for males, females, and the pooled sample belonging to below median dowry paying households. Households are classified as above (below) median dowry payments if they reside in states where the state-level, pre-1985 median dowry payment is higher (lower) than the region-specific, pre-1985 median dowry payment. States are divided into regions based on similarity of social norms. The outcome measures reported years of schooling. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the individual belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 6: Heterogeneity in Educational Attainment by Occupation Type

	Self-employed farming			Non-farm salary and wage			Agricultural wage earners		
	M (1)	F (2)	ALL (3)	M (4)	F (5)	ALL (6)	M (7)	F (8)	ALL (9)
Post X Non-Muslim	1.68 (1.04)	-0.31 (0.52)	0.90 (0.63)	-4.85 (1.09)	-2.35 (0.83)	-2.68 (0.92)	2.81 (2.72)	2.63 (1.09)	2.85 (1.15)
Non-Muslim (=1)	-0.41 (1.07)	2.26 (0.64)	0.54 (0.66)	5.76 (1.19)	3.74 (0.89)	3.78 (0.97)	-3.08 (2.76)	-0.81 (1.29)	-2.03 (1.20)
Control mean	6.5	3.33	4.49	6.21	3.07	4.21	3.93	2.09	2.71
Gender fixed effect			X			X			X
Household control	X	X	X	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X	X	X	X
Observations	5,651	4,872	10,523	2,774	2,362	5,136	1,526	1,427	2,953
$R^2$	0.27	0.27	0.28	0.43	0.41	0.40	0.35	0.32	0.32

Notes: This table reports results from estimating equation (2) on the sample of all males and females in the son and daughter module of the 1999 wave of the REDS data. The estimation is performed separately for individuals belonging to households with different occupation types. Households are clubbed under three broad occupation types based on the reported occupation of household heads. "Self-employed farming" includes occupations reported as self-employed farming and agricultural family workers. "Non-farm salary & wage" are households with self-employment on non-farm activities, such as salaried, non-agricultural wage earners and non-agricultural family workers. "Agricultural Wage Earners" are households with the head of the household engaged in agricultural activities in exchange for wage on land that is not self-owned. Columns (1), (2), and (3) report estimation results on years of education for the sample of male, female and pooled individuals belonging to "Self-employed farming" households. Columns (4), (5), and (6) report estimation results on years of education for the sample of male, female, and pooled individuals belonging to "Non-farming salary & wage" households. Columns (7), (8), and (9) report estimation results on years of education for the sample of male, female, and pooled individuals belonging to "Agricultural Wage Earners" households. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify whether the individual belongs to a non-Muslim household. Household controls include reported income in 1999, caste and total number of members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 7: Heterogeneity in Educational Attainment by Traditional Norms

	Traditional			Non-traditional		
	Male (1)	Female (2)	ALL (3)	Male (4)	Female (5)	ALL (6)
Post X Non-Muslim	2.47 (2.02)	-1.13 (0.87)	0.04 (0.91)	-0.67 (0.95)	-0.82 (0.53)	0.04 (0.91)
Non-Muslim (=1)	-1.23 (2.03)	2.75 (0.95)	1.45 (0.92)	1.01 (1.05)	2.81 (0.69)	1.45 (0.92)
Control mean	6.95	3.94	5.07	5.89	3.07	4.14
Gender fixed effect			X			X
Household control	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X
Observations	4,396	3,993	8,389	4,337	3,796	8,389
$R^2$	0.35	0.31	0.33	0.29	0.31	0.33

Notes: This table reports results from estimating equation (2) on the sample of all males and females in the son and daughter module of the 1999 wave of the REDS data. The estimation is performed separately for individuals belonging to households classified as "Traditional", using adherence to gender unequal social norms. Households are tagged as "Traditional" if at least one member reported gender segregation while eating meals. "Non traditional" households are those where no member reports gender segregation while eating meals. Columns (1), (2), and (3) report estimation results on years of education for the sample of male, female, and pooled individuals belonging to "Traditional" households. Columns (4),(5), and (6) report estimation results on years of education for the sample of male, female, and pooled individuals belonging to "Non traditional" households. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the individual belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 8: Heterogeneity in Educational Attainment by Bequest Ability

	Marginal			Non-marginal		
	Male (1)	Female (2)	ALL (3)	Male (4)	Female (5)	ALL (6)
Post X Non-Muslim	-0.43 (0.71)	-0.50 (0.38)	-0.36 (0.37)	1.00 (1.24)	-0.49 (0.89)	0.91 (0.96)
Non-Muslim (=1)	1.87 (0.73)	2.91 (0.46)	2.28 (0.42)	-1.22 (1.21)	0.95 (0.85)	-0.76 (0.92)
Control mean	6.82	3.46	4.8	7.34	4.21	5.46
Gender fixed effect			X			X
Household control	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X
Observations	8,239	7,063	15,302	3,059	2,734	5,793
$R^2$	0.26	0.29	0.28	0.37	0.33	0.34

Notes: This table reports results from estimating equation (2) on the sample of all males and females in the son and daughter module of the 1999 wave of the REDS data. The estimation is performed separately for individuals belonging to households based on landholding size. Marginal households have land ownership less than 2.5 acres, whereas “Non-marginal” households are households with greater than 2.5 acres of land. Columns (1), (2), and (3) report estimation results on years of education for the sample of male, female, and pooled individuals belonging to “Marginal” households. Columns (4), (5), and (6) report estimation results on years of education for the sample of male, female, and pooled individuals belonging to “Non-marginal” households. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the individual belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 9: Age at Marriage

	Age at Marriage		
	Male (1)	Female (2)	ALL (3)
Post X Non-Muslim	0.81 (0.40)	0.69 (0.41)	0.63 (0.30)
Non-Muslim (=1)	0.89 (0.58)	1.96 (0.53)	1.66 (0.40)
Post (=1)	1.70 (0.39)	1.14 (0.40)	1.57 (0.29)
Control mean	19.8	17.19	18.47
Gender fixed effect			X
Household control	X	X	X
Birth order fixed effect	X	X	X
Observations	5,600	5,434	11,034
$R^2$	0.34	0.24	0.36

Notes: This table reports results from estimating equation (1) on the restricted sample of all married males and females in the son and daughter module of the 1999 wave of the REDS data. Columns (1), (2), and (3) report estimation results for the sample of male, female, and pooled individuals. The outcome variable is the reported age at marriage in years. The variable *Post* is an indicator equal to 1 if the marriage occurs after 1985. *Non-Muslim* is an indicator to identify if the female belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 10: Birth Order Effects

	Age at Marriage					
	Male		Male	Female		Female
	(1)	(2)	(3)	(4)	(5)	(6)
	Firstborn	Second born	Higher order	Firstborn	Second born	Higher order
Post X Non-Muslim	-0.05 (0.70)	-0.14 (0.83)	2.13 (0.61)	-0.43 (1.01)	1.62 (0.69)	0.81 (0.52)
Non-Muslim (=1)	2.56 (1.10)	1.51 (0.89)	-1.07 (0.83)	3.23 (1.22)	0.83 (0.91)	1.86 (0.59)
Post (=1)	1.77 (0.67)	2.66 (0.80)	0.85 (0.57)	2.41 (0.98)	0.23 (0.66)	1.19 (0.50)
Control mean	20.65	19.89	19.06	17.61	17.42	16.88
Household control	X	X	X	X	X	X
Observations	1,909	1,335	2,373	1422	1255	2769
$R^2$	0.23	0.35	0.35	0.17	0.23	0.24

Notes: This table reports results from estimating equation (1) on the restricted sample of all married males and females in the son and daughter module of the 1999 wave of the REDS data. The analysis is done separately for firstborn, second born, and higher order birth parity. Columns (1), (2), and (3) report estimation results for the sample of males by firstborn, second-born, and higher order births. Columns (4), (5), and (6) report estimation results for the sample of females by firstborn, second born, and higher order births. The outcome variable is the reported age at marriage in years. The variable *Post* is an indicator equal to 1 if the marriage occurs after 1985. *Non-Muslim* is an indicator to identify if the female belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 11: Heterogeneity by Enforcement Potential

	Early-State Level Amendment			No-State Level Amendment		
	Male (1)	Female (2)	ALL (3)	Male (4)	Female (5)	ALL (6)
Post X Non-Muslim	0.02 (1.15)	0.11 (0.69)	0.08 (0.73)	0.24 (0.82)	-0.92 (0.39)	-0.28 (0.43)
Non-Muslim (=1)	0.73 (1.89)	2.58 (1.17)	1.67 (1.19)	0.74 (0.81)	2.49 (0.43)	1.56 (0.44)
Control mean	6.94	3.72	5.33	6.91	3.58	4.87
Gender fixed effect			X			X
Household control	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X
Observations	1,618	1,253	2,871	9,680	8,544	18,224
$R^2$	0.40	0.39	0.38	0.27	0.28	0.28

Notes: This table reports results from estimating equation (2) on the sample of all males and females in the son and daughter module of the 1999 wave of the REDS data. The estimation is performed separately for individuals residing in states with preexisting state-level amendments pertaining to the dowry act, such states are termed as the “Early State Level Amendment”. The states of Bihar, Haryana, Himachal Pradesh, and Punjab form the “Early State-Level Amendment” group, with state-level amendments in 1975. States with no preexisting state-level amendments pertaining to the dowry act are termed “No State-Level Amendment.” Columns (1), (2), and (3) report estimation results for the sample of males, females, and pooled samples in “Early State-Level Amendment” states. Columns (4), (5), and (6) report estimation results for the sample of males, females, and pooled samples in “No State Level Amendment” states. The outcome variable is reported years of education. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the individual belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 12: Heterogeneity by Enforcement Exposure

	Above Median Exposure			Below Median Exposure		
	Male (1)	Female (2)	ALL (3)	Male (4)	Female (5)	ALL (6)
Post X Non-Muslim	0.29 (1.04)	-0.95 (0.57)	-0.39 (0.58)	0.01 (0.95)	0.51 (0.53)	0.42 (0.51)
Non-Muslim (=1)	1.52 (0.98)	3.25 (0.64)	2.38 (0.59)	-0.04 (1.04)	1.86 (0.60)	0.74 (0.57)
Control mean	7.92	4.43	5.84	6.17	2.93	4.21
Gender fixed effect			X			X
Household control	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X
Observations	3,152	2,888	6,040	3,607	3,362	6,969
R <sup>2</sup>	0.18	0.29	0.26	0.16	0.27	0.24

Notes: This table reports results from estimating equation (2) on the sample of all males and females in the son and daughter module of the 1999 wave of the REDS data. Households in districts that experienced a change in dowry payments across pre-and-post periods which was greater than the median change in the state are classified as “Above Median Exposure.” Households in districts that experienced a change in dowry payments across pre-and-post periods, which was greater than the median change in the state are classified as “Below Median Exposure.” The estimation is performed separately for individuals in districts tagged as “Above Median Exposure” and “Below Median Exposure.” Columns (1), (2), and (3) report estimation results for males, females, and the pooled sample, belonging to households in “Above Median Exposure” districts. Columns (4), (5) and (6) report estimation results for males, females, and the pooled sample belonging to households in “Below Median Exposure” districts. The outcome measures reported years of schooling. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the individual belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses.

Table 13: Robustness

	State Time Varying			Legal Age			Clustering		
	M (1)	F (2)	ALL (3)	M (4)	F (5)	ALL (6)	M (7)	F (8)	ALL (9)
Post X Non-Muslim	-0.12 (0.70)	-0.87 (0.37)	-0.37 (0.38)				0.17 (0.60)	-0.65 (0.37)	-0.16 (0.39)
Post_Legal X Non-Muslim				-0.17 (0.88)	-0.74 (0.43)	-0.37 (0.45)			
Non-Muslim (=1)	1.02 (0.71)	2.33 (0.42)	1.55 (0.41)	1.20 (0.88)	2.51 (0.46)	1.77 (0.47)	0.85 (0.64)	2.37 (0.36)	1.53 (0.40)
Control mean	6.91	3.6	4.92	7.16	3.57	4.71	6.91	3.6	4.92
Gender fixed effect			X			X			X
Household control	X	X	X	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X	X	X	X
Observations	11,298	9,797	21,095	12,238	9,984	22,222	11,298	9,797	21,095
R <sup>2</sup>	0.29	0.30	0.29	0.27	0.28	0.28	0.28	0.29	0.29

Notes: This table reports results from estimating variants of equation (2) on the sample of all females in the son and daughter module of the 1999 wave of the REDS data. The outcome variable *Female School-Yrs* measures reported years of schooling. Columns (1), (2), and (3) report estimates for the sample of males, females, and the pooled sample, respectively while controlling for state time-varying trends in equation (2). Columns (4), (5), and (6) report estimates for the sample of males, females, and the pooled sample respectively using equation (2) with cohort exposure assigned using the legal age of marriage. Columns (7), (8), and (9) report estimates for the sample of males, females, and the pooled sample, respectively, using equation (2) with state-level clustered standard errors in parentheses. The variable *Post* is an indicator equal to 1 if the female belongs to the post-cohort based on the reference age at marriage. The variable *Post Legal* is an indicator equal to 1 if the female belongs to the post-cohort based on the legal age at marriage. *Non-Muslim* is an indicator to identify whether the female belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses for columns (1) through (6).



Table 14: Robustness-Impact on Years of Schooling for Females

	Female School-Yrs		
	(1)	(2)	(3)
Post X Non-Muslim	-0.82 (0.36) [0.03]	-0.72 (0.36) [0.05]	-0.65 (0.34) [0.05]
Non-Muslim (=1)	1.66 (0.33)	1.11 (0.34)	2.37 (0.39)
Control mean	3.6	3.6	3.6
Household control	X	X	X
Birth order Fixed Effect	X	X	X
Upper caste fixed effect		X	
All caste fixed effect			X
Effect size	-22%	-19%	-18%
Observations	9,797	9,797	9,797
$R^2$	0.26	0.28	0.29

Notes: This table reports results from estimating equation (2) on the sample of all females in the son and daughter module of the 1999 wave of the REDS data. The outcome variable *Female School-Yrs* measures reported years of schooling. The variable *Post* is an indicator equal to 1 if the female belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify whether the female belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Standard errors in parentheses are clustered by state year. Brackets reports p-values based on a t-test against the null for the coefficient of interest, using wild-cluster bootstrapped errors by state year..

Table 15: Difference-in-differences Estimator using de Chaisemartin and D’Haultfoeuille (2021)

	Estimate	SE	LB CI	UB CI	N	Switchers
School-Yrs	-.6994971	.3186149	-1.323982	-.075012	6118	4661

Notes: This table reports results using the difference-in-differences Estimator proposed in de Chaisemartin and D’Haultfoeuille (2021). The sample comprises all females in the son and daughter module of the 1999 wave of the REDS data. The outcome variable *School-Yrs* measures the reported years of schooling.

Table 16: Placebo-Impact on Years of Schooling for Male

	Male School-Yrs		
	(1)	(2)	(3)
Post X Non-Muslim	-0.08 (0.67) [0.88]	0.08 (0.67) [0.89]	0.17 (0.68) [0.77]
Non-Muslim (=1)	1.01 (0.66)	0.43 (0.66)	0.85 (0.69)
Control mean	6.91	6.91	6.91
Household control	X	X	X
Birth order fixed effect	X	X	X
Upper caste fixed effect		X	
All caste fixed effect			X
Effect size	-1.2%	1.1%	2%
Observations	11,298	11,298	11,298
$R^2$	0.26	0.27	0.28

Notes: This table reports results from estimating equation (2) on the sample of all males in the son and daughter module of the 1999 wave of the REDS data. The outcome variable *Male School-Yrs* measures reported years of schooling. The variable *Post* is an indicator equal to 1 if the male belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify whether the male belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses. Brackets reports p-values based on a t-test against the null for the coefficient of interest, using wild bootstrap heteroskedasticity robust errors.

# A Supplementary Analysis

## A.1 Rotating Capital Fund-Net Payers and Net Receivers

Using an identical regression specification as in equation 2, the impact of the amendment on educational attainment is estimated for Net Payer and Net Receiver households. Table 17 reports the estimated coefficients on the coefficient of interest *PostXNon-Muslim* and *Non-Muslim*. Consistent with the evidence presented in Table 4, Net Payer households are the main drivers of the reduction in educational attainment with 1.34 years of lower education for females in Net Payer households. This is a decline of 25% over the mean, as compared to 18% decline for the entire sample. No significant differences in educational attainment for Net Receiver households across females or males is recorded.

Though this result reinforces the impacts of the amendment on educational attainment through changes in dowry practice, it should not be viewed in isolation. Classifying households as Net Payers (Net Receivers), based on the relative number of females (males), raises endogeneity concerns given the literature documenting gender-based fertility-stopping rules practiced in developing countries (Jayachandran and Kuziemko, 2011; Yamaguchi, 1989). Households with a larger number of girls are more likely to have stronger son preferences, and consequently, practice stronger traditional social norms. The estimation strategy is able to provide causal evidence, even in the presence of gender-based fertility rules assuming traditional social norms practiced by households are sticky and unlikely to vary over time. Furthermore, the estimation equation controls for the total number of household members, thereby mitigating concerns around differences in family size driving results.

Table 17: Impact on Years of Schooling : Net Payers and Receivers

	Net Payers			Net Receivers		
	Male (1)	Female (2)	ALL (3)	Male (4)	Female (5)	ALL (6)
Post X Non-Muslim	-1.94 (2.36)	-1.34 (0.47)	-1.42 (0.49)	0.41 (0.72)	-0.16 (0.47)	0.37 (0.48)
Non-Muslim (=1)	3.28 (2.48)	2.93 (0.56)	2.81 (0.54)	0.60 (0.72)	1.97 (0.53)	0.88 (0.49)
Control mean	4.26	5.32	4.92	4.26	5.32	4.92
Gender fixed effect			X			X
Household control	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X
Observations	1,957	5,275	7,232	9,341	4,522	13,863
R <sup>2</sup>	0.31	0.30	0.30	0.28	0.29	0.29

Notes: This table reports results from estimating equation (2) on the sample of all males and females in the son and daughter module of the 1999 wave of the REDS data. The estimation is performed separately for individuals belonging to households classified as “Net Payers” and “Net Receivers.” Net Payer (Net Receiver) status is assigned based on whether the household has a greater number of female children (male children). Columns (1), (2), and (3) report estimation results on years of education for the sample of male, female, and the pooled individuals belonging to “Net Payers” households. Columns (4), (5), and (6) report estimation results on years of education for the sample of male, female, and the pooled individuals belonging to “Net Receivers” households. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify whether the individual belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses. \* p < 0.10 , \*\* p < 0.05 , \*\*\* p < 0.01 .

Net Payer households are more likely to have female household heads, more educated fathers, and fewer

numbers of household members. Differences in household size is likely driven by virilocal marriage practices. Controls for household size are used in the analysis.

Table 18: Balance Table : Net Payers and Net Reciever Households

Variable	(1) Net Receivers	(2) Net Payers	(3) Difference
Household head male (=1)	0.939 (0.240)	0.926 (0.262)	-0.013 (0.007)
Household head marital status	1.103 (0.304)	1.105 (0.306)	0.001 (0.008)
Household income at time of survey ('000 Rs)	102.900 (162.288)	97.856 (124.621)	-5.043 (3.572)
Fathers years of education	5.055 (4.696)	5.283 (4.737)	0.229 (0.127)
Mothers years of education	2.596 (3.807)	2.629 (3.859)	0.033 (0.103)
Total number of household members	6.350 (3.513)	5.717 (2.727)	-0.633 (0.078)
Observations	4,895	2,104	6,999

Notes: Data consists of all households in the 1999 wave of the REDS data. Standard errors are reported in parentheses..

## A.2 Robustness

The estimation strategy used in Calvi and Keskar (2021) is implemented. Two empirical specifications are considered with results reported in Table 19. The first specification retains cohort-level exposure assignment relative to the age of marriage interval, but replaces cohort of birth fixed effects with a post indicator. The second specification alters the exposure assignment to consider all cohorts born post 1985 to be exposed to the amendment. Under both specifications the results of this paper remain qualitatively unchanged, and the coefficient of interest retains a negative magnitude, although it is now imprecisely estimated.

Table 19: Impact on Years of Schooling

	School-Yrs					
	Male (1)	Female (2)	ALL (3)	Male (4)	Female (5)	ALL (6)
Post X Non-Muslim	0.42 (0.72)	-0.43 (0.33)	0.11 (0.38)			
Non-Muslim (=1)	1.06 (0.73)	2.20 (0.40)	1.54 (0.40)	1.02 (0.30)	1.86 (0.30)	1.39 (0.22)
Post (=1)	-0.56 (0.70)	1.80 (0.32)	0.64 (0.37)			
Born after 1985 (=1)				-3.23 (0.25)	-1.03 (0.24)	-2.33 (0.18)
Born after 1985 X Non-Muslim				-0.35 (0.26)	-0.39 (0.25)	-0.27 (0.19)
Control mean	6.91	3.6	4.92	7.22	4.66	6.10
Gender fixed effect			X			X
Household control	X	X	X	X	X	X
Birth order fixed effect	X	X	X	X	X	X
Observations	11,298	9,797	21,095	13,624	10,934	24,558
$R^2$	0.10	0.20	0.16	0.20	0.22	0.22

Notes: This table uses the sample of all females and males belonging to the sons and daughters module of the 1999 wave of REDS data. Columns (1), (2), and (3) report results from estimating equation (2) by swapping out cohort fixed effects with an indicator for post-period, for the sample of males, females, and the pooled sample, respectively. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the individual belongs to a non-Muslim household. Columns (4),(5), and (6) report results for the sample of males, females, and the pooled sample from replacing *Post* in equation (2) with a dummy *Born after 1985* which equals 1 if the individual was born after 1985. Household controls include reported income in 1999, caste, and total number of members. All regressions control for state fixed effects. Robust standard errors are reported in parentheses. \* p < 0.10 , \*\* p < 0.05 , \*\*\* p < 0.01 .

Table 20: Robustness - Impact on Years of Schooling for Male

	Male School-Yrs		
	(1)	(2)	(3)
Post X Non-Muslim	-0.08 (0.64) [0.90]	0.08 (0.64) [0.91]	0.17 (0.64) [0.80]
Non-Muslim (=1)	1.01 (0.62)	0.43 (0.62)	0.85 (0.68)
Control mean	6.91	6.91	6.91
Household control	X	X	X
Birth order fixed effect	X	X	X
Upper caste fixed effect		X	
ALL caste fixed effect			X
Effect size	-1.2%	1.1%	2%
Observations	11,298	11,298	11,298
$R^2$	0.26	0.27	0.28

Notes: This table reports results from estimating equation (2) on the sample of all males in the son and daughter module of the 1999 wave of the REDS data. The outcome variable *Male School-Yrs* measures reported years of schooling. The variable *Post* is an indicator equal to 1 if the male belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the male belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Standard errors in parentheses are clustered by state year. Brackets reports p-values based on a t-test against the null for the coefficient of interest, using wild-cluster bootstrapped errors by state year.

Table 21: Robustness-Impact on Years of Schooling for All

	School-Yrs		
	(1)	(2)	(3)
Post X Non-Muslim	-0.35 (0.35) [0.33]	-0.22 (0.35) [0.54]	-0.16 (0.35) [0.66]
Non-Muslim (=1)	1.26 (0.33)	0.69 (0.33)	1.53 (0.38)
Control mean	4.92	4.92	4.92
Household control	X	X	X
Birth order fixed effect	X	X	X
Upper caste fixed effect		X	
All caste fixed effect			X
Observations	21,095	21,095	21,095
$R^2$	0.27	0.28	0.29

Notes: This table reports results from estimating equation (2) on the sample of all children in the son and daughter module of the 1999 wave of the REDS data. The outcome variable *School-Yrs* measures reported years of schooling. The variable *Post* is an indicator equal to 1 if the individual belongs to the post-cohort based on the reference age at marriage. *Non-Muslim* is an indicator to identify if the individual belongs to a non-Muslim household. Household controls include reported income in 1999, caste, and total number of household members. All regressions control for state fixed effects. Standard errors in parentheses are clustered by state year. Brackets reports p-values based on a t-test against the null for the coefficient of interest, using wild-cluster bootstrapped errors by state year.



Figure 9: The figure plots the mean and 2 standard deviations of a measure of dispersion of the reported dowry payments across years. The measure of dispersion captures the standard deviation of the error term from a regression of dowry payments on co-variables to control for recall period and individual-level characteristics. The sample comprises all married women in panel (a), all Muslim married women in panel (b), and all non-Muslim married women in panel (c) over the time period 1970-1999. Source: REDS 99.

## B Dowry Reporting

Despite the law having no provisions for retrospective penalty associated with dowry payments, households may systematically under-report dowry for marriages in the post-period. To test for underreporting variance in reporting amounts are explored. Dowry payments are modeled using the following equation :

$$y_{it} = \Delta_i + \Delta_t + \Delta_{it}$$

$\Delta_t$  captures the recall bias,  $\Delta_i$  are family characteristics and comprise the observable components of reported dowry  $y_{it}$ . The observable components are parcelled out, and a measure of dispersion for the error terms is calculated for the overall, treatment, and control subsamples. The empirical test checks association between variance in dowry reporting with treatment status. If the variance in dowry reporting is associated with treatment then there is evidence of systematic misreporting, which is problematic for the first-stage estimation. To test for systematic misreporting the distribution of the variance of errors across years for the control and treatment populations are created (panel (a) & (b) in Figure 9). A visual inspection of the graphs suggests that the distribution of errors remains fairly consistent across treatment and control populations. Additionally, the error distribution does not seem to vary across years, and thus, is presumably uncorrelated with the timing of the amendment. These insights are interpreted to suggest that the amendment timing is not salient for households while recalling dowry payments.