Education, social norms, and the marriage penalty: Evidence from South Asia*

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Abstract

A growing literature attributes gender inequality in labor markets to reduced female labor supply after childbirth–the child penalty. However, if social norms constrain married women's activities outside the home, then marriage can independently reduce employment. Given that childbirth and marriage often coincide, conventional estimates of child penalties conflate these effects. This paper studies the marriage penalty in South Asia, a region with conservative gender norms and low female labor force participation, introducing a split-sample, pseudo-panel approach that separates marriage and child penalties in the absence of individual-level panel data. Marriage reduces women's labor force participation by 12 percentage points, while the marginal penalty of childbearing is small. Consistent with the influence of both opportunity costs and social norms, the marriage penalty is smaller among cohorts with higher education and less conservative gender attitudes. Patrilocality norms account for 50 percent of the marriage penalty. Furthermore, employment declines are concentrated in salaried work, not agriculture, highlighting the role of female mobility constraints in shaping employment outcomes.

Keywords: Female labor force participation, gender inequality, gender attitudes, marriage penalty

JEL codes: O10, J12, J16, J22

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1 Introduction

In nearly every country in the world, women participate in the labor force at a lower rate than men. The persistence of the traditional specialization of women in the home and men in the marketplace is one of the most consistent empirical facts in the social sciences. Much of this gap arises from the costs of child-rearing, which are disproportionately borne by women. In Asia, women spend around 5 times as much as men on household tasks (Van der Gaag et al., 2019). The sharp decline in the labor market outcomes of women relative to men around the birth of the first child – the so-called "child penalty" – is a central driver of gender inequality in the labor markets across the world (Kleven et al., 2019).

However, childbearing typically occurs concurrently with other key events of family formation, in particular marriage. Conventional estimates of the child penalty often ignore that, even in the absence of children, the act of marriage confers new responsibilities and social norms that may constrain a woman's labor supply. These marital constraints – the "marriage penalty" – may be conflated with childbearing constraints in a typical child penalty estimation, given the correlation in time between these two events.

In settings where gender roles are deeply entrenched (Jayachandran, 2020), married but childless women may be already confined to domestic responsibilities even where they are not yet constrained by the burdens of childcare. This marriage penalty may in part explain the mixed evidence globally on the impact of access to childcare on women's labor supply (Evans et al., 2021). In South Asia in particular – a region known for both conservative gender norms and low female labor force participation – the experimental evidence in favor of childcare access is weak (Nandi et al., 2020; Richardson et al., 2018), suggesting childcare responsibilities are not the main constraint to female employment. Similarly, Abraham et al. (2021) find no evidence of child penalties in India. This evidence points to the primacy of marriage over child penalties in settings where gender norms are strict.

Still, much like the child penalty, the marriage penalty may have diverse causes. Is a marriage penalty evidence of conservative social norms that proscribe married women's physical mobility and work outside the home? Or is it, instead, a reflection of the optimal household specialization between men and women, given limited outside options for women in the labor market, as in Becker (1993). In the context of South Asia, this paper answers two questions: (i) how can the marriage penalty be separated from the child penalty in the absence of individual-level panel data, and (ii) what drives the marriage penalty in South Asia. Does it represent optimal labor supply or a misallocation of talent?

To answer these questions, we use multiple rounds of the nationally representative Demographic and Health Surveys (DHS) from four South Asian countries – Bangladesh, India, Maldives, and Nepal. Separately estimating child and marriage penalties presents several challenges. First, since the timing of these major life events is endogenous, naive comparisons in the labor market outcomes between married and unmarried women or parents and the childless are contaminated by omitted variables. Recent work on child penalties has used event-study methods to solve this identification problem (Kleven et al., 2021, 2019, 2024).

However, even if a design exploits sharp changes in labor supply around marriage or childbirth for identification, these two events are correlated in time for a given individual. As such, estimates of the marriage penalty are obscured by the presence of children, and estimates of the child penalty are likewise capturing at least in part a marriage penalty. One solution is to use individual-level panel data, where a woman's employment status is observed before and after both marriage and childbirth. Here, event-study techniques readily apply, augmenting standard specifications with timing indicators for both events simultaneously, with marriage and childbirth coefficients separately identified by variation across women in the timing gap between these two events (Kleven et al., 2023).

However, access to such rich panel data is rare, particularly in developing country settings. We propose a method that allows for the separation of the marriage penalty from the child penalty in repeated cross-sectional data. Following Kleven et al. (2023), we generate pseudo-panels by matching women surveyed in the year of their marriage with younger unmarried women and older married women to construct pre- and post-marriage counterfactual employment trends. Our contribution is to restrict the pool of potential post-marriage matches to women without children (the "no child" sample) in order to isolate the marriage penalty from the child penalty. We then re-run this matching procedure on the unrestricted sample of women where children may be present (the "ignore child" sample). Comparing these two quantities yields the relative magnitude of marriage penalty alone and the combined marriage and child penalty.

We find that South Asian women reduce their labor force participation by 12 percentage points (p.p.) following marriage, even before childbearing. Among women with children, this rises just 4 p.p to 16 p.p. As such, 75 percent of the combined family formation penalty is driven by marriage itself, rather than the burden of childbearing, at least in the first five years of marriage. The largest effects are observed in India, while more muted effects are observed in Nepal, where the majority of the combined penalty is driven by children. Dynamic event-study estimates reveal flat trends in employment status leading up to the marriage date, and sharp drops in employment in the first year of marriage. These trends lend additional support to the notion that these estimates represent the causal effect of marriage. Men, in contrast, enjoy a marriage premium. This premium does not depend on the presence of children, consistent with the existing literature showing no child penalties for men (Kleven et al., 2023).

The marriage penalty may represent an optimal solution to a joint household maximization problem. If women have limited outside options in the labor market relative to their husbands, then specialization in home-based tasks might be economically efficient, even without children. However, the value of women's home production is greatly diminished without children, suggesting a role for social norms in driving the marriage penalty, particularly those that constrain women's mobility outside the home (Anukriti et al., 2020). To adjudicate these hypotheses, we use heterogeneity analyses to test the sources of the marriage penalty. Despite the fact that gender norms are more progressive in urban areas in South Asia (Asher et al., n.d.), we find no significant difference between urban and rural marriage penalties. This may in part reflect the nature of urban labor markets, where employment opportunities are concentrated *outside* the home, adding additional constraints to women's participation in the context of conservative gender attitudes (Jalota and Ho, 2024).

We then turn to testing the impact of education and social norms on marriage penalties in South Asia by interacting our post-marriage indicator with these characteristics. We find that educated women have much smaller marriage penalties, with post-secondary education erasing nearly half the baseline marriage penalty. At the same time, educated husbands exert a quantitatively similar effect. Because of positive assortative mating, spousal education levels are highly correlated, necessitating the inclusion of both interactions in a single regression. In this model, the wife's education remains significant while the coefficient on the husband's education falls to zero.

Interpreting these results through the lens of our hypotheses, we argue that a woman's education affects *both* household gender norms and her outside employment options. In contrast, her husband's education affects household norms, but does not directly affect her employment prospects. This suggests that outside options at least in part play a role in determining the marriage penalty. We then directly test the role of gender attitudes by interacting the marriage indicator with DHS measures of household gender attitudes. We find strong evidence that women in households with more liberal gender norms experience smaller marriage penalties. The effects of education and social norms appear to be independent, suggesting that both opportunity costs and social norms play a role in driving the marriage penalty.

Finally, we consider which social norms specifically bind married women's labor force participation. One such norm is patrilocality, a common practice in South Asia by which the married couple moves to the village (or household) of the groom's family upon marriage. Such migration for marriage may disrupt women's social networks and access to information, hindering job search. It may also expose women to the restrictive gender attitudes of their in-laws (Anukriti et al., 2020). Following marriage, we find a large, discrete drop in the average duration of stay in the current village, consistent with high rates of reported patrilocal migration among new brides. Controlling for this variable in our main specification reduces the effect of marriage on employment substantially, implying that patrilocality explains roughly 50 percent of the overall marriage penalty.

Recent evidence suggests that women in South Asia are constrained in their wage labor activities by social norms that curtail mobility outside the home; take-up of job offers increases significantly when identical work can be done at home (Ho et al., 2024; Jalota and Ho, 2024). We hypothesize that these norms of working outside the home drive the marriage penalty. To test this, we estimate impacts of marriage on occupational choice. Using multiple independent datasets, we show that the marriage penalty is concentrated entirely in wage work outside the home, while agricultural work—which is typically conducted on the family farm—is minimally affected. In fact, hours worked in agriculture rise, suggesting substitution to more home-friendly work activities. Our tests cannot directly identify whether this pattern of effects is driven by labor supply—household norms—or labor demand—employer discrimination. However, we note that when these sectoral results are taken together with the heterogeneous effects of household norms, this suggests, at least in part, a supply-side mechanism.

We contribute to several literatures on gender inequality in labor economics and development. Gender inequality in labor markets is a fact of life across the world. A longstanding debate in economics discusses whether this is driven by the optimal household specialization of men and women given comparative advantage (Becker, 1965, 1973), or whether other frictions—such as social norms or discrimination—drive a wedge between a woman's market productivity and her participation level (Jayachandran, 2021; Bertrand et al., 2015, 2016; Fernandez, 2013). The existence of marriage penalties could, in theory, be driven by either force, since household formation may lead to optimal specialization. We contribute to this perennial debate by providing evidence that both social norms and outside options play a role in determining women's work after marriage in a socially conservative context.

A more recent literature has attributed much of gender inequality in the labor market to the "child penalty." Child penalties have been documented in diverse settings, with magnitudes ranging from 12 to 38 percent (Kleven, 2022; Kleven et al., 2019, 2023). We show that even before childbirth, marriage itself might already affect female labor force participation in settings with deeply entrenched gender norms. Our work proposes a novel method for distinguishing between the two in cross-sectional data, providing some of the first evidence on the magnitudes of marriage penalties relative to child penalties. We argue that in western countries, child penalty likely to matter more, as childcare is the main constraint. But in developing countries, this is not necessarily true, social norms bind. Whether gender inequality is driven by child or marriage penalties presents starkly different implications for welfare and policy. Child penalty about childcare could be efficient. Marriage penalty is unlikely to be efficient and harder to address with policy. Indeed, our results on marriage penalty explain weak effects of childcare in certain contexts (Richardson et al., 2018; Nandi et al., 2020), despite average effectiveness overall (Evans et al., 2021). In addition, we provide substantially more depth about the drivers of the marriage penalty than previously explored in the literature.

We also contribute to a nascent literature on *which* norms underlie persistent gender inequality. Recent work has identified the critical role of patrilocality customs in driving adverse outcomes for women (Khalil and Mookerjee, 2019; Anukriti et al., 2020). In addition to patrilocality, restrictions on female mobility have emerged as a key determinant of women's labor force participation in socially conservative contexts. This is both because of limited physical safety outside the home (Amaral et al., 2023; Field and Vyborny, 2022), as well as because of pervasive “female seclusion” norms that prevent women from enganging in market work (Bernhardt and Agte, 2023; Jalota and Ho, 2024; Ho et al., 2024). We contribute to this literature by providing new evidence that women’s reduction in labor supply upon marriage is driven by both patrilocality and restricted mobility for married women.

2 Data and Empirical Strategy

2.1 Data

We use data from the Demographic and Health Survey (DHS) from Bangladesh, India, Maldives, and Nepal in the analysis (Table A1).[1] The surveys were conducted approximately every 5 years starting in the 1990s. These nationally representative surveys form a repeated cross section of reproductive age women between the ages of 15 and 45. The more recent surveys also include data on the men in the households, so there are fewer rounds of data for men. The final sample covers 1,780,854 women and 389,313 men residing in around 650,000 households in all level 1 administrative areas of the 4 countries.

The surveys include age, education, urban residence, employment status, and marital status. For women, the surveys include age at cohabitation, which we use as the age at marriage, and birth history, which includes the timing of the first birth. Importantly, these surveys contain complete household rosters with data on employment status for *both* married and unmarried women, which will be essential to the pseudo-panel approach below.

More recent rounds of the DHS also include questions on decision making and attitude toward domestic violence. The decision making questions include whether or not women are involved in decisions on their own health care, purchases, and visits to family. The decision making index is the sum of all the decision making items in which the woman is involved individually or jointly. A higher index suggests higher decision making power for women. The questions on attitude toward domestic violence include whether it would be justified for a husband to beat his wife if she goes out without telling her husband, neglects the children, argues with her husband, refuses sex, and burns the food. The index for the attitude toward

[1] Other countries in South Asia are excluded because the DHS data lack sufficient information on employment status for both married and unmarried women.

domestic violence is the sum of all the items with which the woman agrees. A higher index suggests a higher acceptance of domestic violence.

We supplement the DHS with data from the Indian Human Development Survey (IHDS), a nationally representative panel survey of 41,554 households in 1,503 villages and 971 urban neighborhoods in India. The first wave of the IHDS was conducted in 2004-5 and the second round was in 2011-12. The analysis treats the two waves of the IHDS as repeated cross sections and the cohorts were constructed in the same way as the DHS using information on age, education, urban residence, employment status, and timing of marriage. Additionally, the IHDS provides occupation-specific information, which includes salaried employment, farm work, and total labor supply.

2.2 Empirical strategy

2.2.1 Estimation

We use an event study approach based on sharp changes in women's labor market outcomes observed around the time of marriage, $t = 0$, for individuals at age a observed in calendar year y. Ideally, the estimation uses individual-level panel data, in which case relative time indicators for marriage and childbirth can be included simultaneously, along with individual and year fixed effects, in a single event-study regression where labor market participation is the outcome Kleven et al. (2023).

However, in the absence of such data, following Kleven (2022), cross sectional data can be used to create pseudo-panel data. For married individuals, we observe the age of marriage, and therefore their location the post-marriage event space, $t \geq 0$. For unmarried individuals, we cannot observe their timing of marriage, and therefore only observe that they are in the pre-marriage event space, $t < 0$, but not their precise location in this space. The pseudo-panel approach matches married women to unmarried women with the same demographic characteristics to form a pre-marriage counterfactual estimate of the outcome, transforming the cross sectional data into a pseudo-panel across the pre and post-marriage event spaces.

Specifically, woman i is observed in the year of her marriage $t = 0$ in calendar year y at age a, with demographic characteristics X_i. She is matched to a surrogate observation in the pre-marriage period, an unmarried woman j observed in year $y - n$ with age $a - n$ and the same observed characteristics $X_i = X_j$. Then, for each n up to 5, the matches at $t - n$ are used to construct the pre-marriage counterfactual at that period. To maximize the sample, we match on a parsimonious set of characteristics, including the level of education and rural or urban residence.

A similar procedure is then applied to women k in the post marriage event space, who are observed in y at $t + n$ for n from 1 to 5. For these post-marriage matches, we additionally require that the age at marriage is the same as index woman i. Accordingly, a pseudo-panel is constructed for woman i to generate counterfactual employment levels for 5 years before and after marriage. Furthermore, woman i represents all of the women with X_i observed at $t = 0$ in y, who enter her cohort as "reference women" and are matched with the same counterfactual women j and k. Well call these groups, indexed c, "marriage cohorts."

These marriage cohorts are then collapsed at the event year-cohort level to obtain average labor market outcomes for each cohort for five years pre- and post-marriage, as well as cohort characteristics. The procedure is then repeated on the sample of men. A marriage cohort c, then, is a three-element vector, defined by an age of marriage, a rural-urban indicator, and an education level. Within each cohort, age a and birth year (birth cohort) b vary with event-time t by construction, given that the pre- and post-marriage components of the marriage cohorts are formed using ages relative to the reference woman.

For marriage cohort c at event time t, we estimate the following regression specification separately for each gender g:

$$Y_{ct}^{g} = \alpha^{g} + \beta^{g} D_{ct} + \delta_{a}^{g} + \gamma_{b}^{g} + \nu_{it}^{g} \tag{1}$$

Where Y_{ct}^{g} is the outcome of interest, the cohort average employment status. D_{ct} is an indicator for post-marriage event periods, and β^{g} is the estimate of the marriage penalty or premium. In our main estimates, we present D_{ct} as a collapsed indicator for all post-marriage event periods, giving the event time-averaged treatment effect. However, we also present event-study specifications where we include leads and lags of the event year ($t - 5$ up to $t + 5$), allowing for dynamic pre- and post-marriage trends in employment.

In a marriage single cohort, the pre- and post-marriage indicators are collinear with age and/or birth cohort fixed effects δ_a, and γ_b since the marriage cohort matches were selected by their age relative to the reference woman. However, when many marriage cohorts are stacked within a given survey round, then age fixed effects can be included in the regression, since age of marriage varies across marriage cohorts. That is, within a given age there is still variation across marriage cohorts in the location in event-time.

However, age and birth cohort remain collinear for all marriage cohorts in a given survey round (country-year). However, when multiple survey rounds per country are stacked, birth cohort fixed effects γ_b^g can be included as well as the age effects. Alternatively, to take into account multiple surveys across countries, country and survey year fixed effects could also be included. Note, however, that in cross-country pooled sample, country-year, age, and birth cohort fixed effects are collinear, and so including any two of these three yields equivalent estimates.[2]

The sample size in each country varies, so the analysis is weighted by marriage cohort size so that it is representative at the individual level, and provides identical coefficient estimates to estimation on the microdata in a "stacked" model. Standard errors are clustered at the marriage cohort level.

We also consider several heterogeneity analyses to investigate the mechanisms underlying the marriage penalty. To explore the role of urban residence, the same analysis as in equation 1 is conducted on the urban and rural samples separately. To explore the role of education, the analysis includes an interaction term between women's higher education and the post-marriage indicator. A separate regression is run to analyze the role of husband's education by including an interaction term between husband's higher education and the post-marriage indicator. Higher education (or husband's higher education) takes the value one if women (or their husbands) have more than secondary education. Finally, we explore the role of social norms similarly, interacting the post-marriage indicator with a marriage cohort-averaged gender attitudes index.

2.2.2 Identification of the marriage penalty

Marriage and childbirth are correlated in time across individuals. A naive pseudo-panel event-study regression would likely conflate these two effects, particularly as the share of women with children rises with t. We therefore propose a split-sample approach to separate

[2] In practice, we include birth-cohort and country-year fixed effects.

the marriage penalty from the child penalty. The first case, the "no child sample", restricts the sample to women and men without children 5 years after marriage to explore gender norms. The second case, "the ignore child sample", matches women and men in the post marriage event space without considering the presence of children. This case combines the marriage and child penalties (or premium).

Our analysis relies on two identification assumptions to interpret β as the causal effect of marriage on employment. First, the precise timing of marriage must be uncorrelated with other shocks that plausibly influence labor supply. Our split sample approach, by construction, rules out childbirth as such a confounder. However, other confounders, such as relocation in the context of patrilocality, might also be correlated with marriage.

Our split-sample approach also generates a second identification assumption: that the women in the “no child” sample are not diferentially selected in their propensity to work. This assumption is more challenging to satisfy. Specifically, women who remain childless up to $t = 5$, particularly in a context of strong patriarchal norms, are likely to be those with a higher unobserved propensity to work. As such, this introduces a time-varying bias in the estimates, since this problem becomes increasingly severe as t rises: married women without children many years after marriage are particularly selected. However, since these women likely have a higher unobserved propensity to work, the results as we move toward the end of the event window probably represent a lower bound on the true marriage penalty.

2.2.3 Summary statistics

Table A2 presents marriage cohort-level summary statistics for the women in the no child and ignore child samples. About 20 percent of women in the sample are working. The average age of the cohort is 20 at the time of survey, almost 40 percent reside in urban areas, almost a third have more than secondary education. The average age at marriage is 21 and women marry men who are on average 3 years older than them. On average, women participate in 0.3 decisions out of a maximum of 5. Women agree to an average of 0.2 statements out of 5 on when it would be justified for a man to beat his wife. These characteristics are similar across the two samples.[3]

[3] Table A4 presents marriage cohort-level summary statistics for the women in the IHDS. Women's characteristics are similar across the no child and ignore child samples. About a third of women in the IHDS sample are

Table A3 presents the summary statistics for the men in each sample. The size of the male sample is 75 to 80 percent of the female sample, since women of childbearing age are over-sampled in the DHS. About 53 percent of the men in the sample are working. The average age of the cohort is 21, a year older than the women's average age. About a quarter of men have more than secondary education, slightly lower than the women's share.

3 Results

3.1 Main results

The results of the main marriage penalty estimation are in Table 1. The model collapses the yearly event study indicators into pre- and post-marriage periods, with the full set of birth cohort and country-year fixed effects. The post-marriage treatment indicator is then interacted with a binary variable indicating whether the cohorts exclude women with children (the "no child sample") or allow for childbirth (the "ignore child sample").

Columns (3) and (4) show the results for women. On average, marriage reduces labor force participation by 12.8 percentage points across South Asia across all women in the five years after marriage. For women without children, which reflects the true marriage penalty, the effect is 11.5 p.p. Figure 1 plots the penalties by country, with estimates provided in Appendix Table A5. The estimates reveal negative marriage penalties for every country in the sample, which are statistically significant for 3 out of 4 countries. The penalty is largest in India, at 11.8 p.p., and lowest in Nepal, at just 2.3 p.p., where it is not significant.

The marginal effect of accounting for childbearing in the regression specification is small (column 4). The marriage penalty among women with or without children—what we call the "combined family formation penalty"– is 14 p.p. Meanwhile, the marriage penalty for childless women is 11.5 p.p., suggesting that childbearing explains less than 20 percent of the combined family formation penalty. However, in Figure 1, the gaps between the ignore child and no child samples vary widely across countries. They are smallest in India and Bangladesh—the large countries that dominate the aggregate sample—where the marriage penalty explains 82 and 89 percent of the combined family formation penalty respectively. Instead, in Nepal

working, slightly higher than the DHS sample. The average age at marriage in the IHDS sample is 22, a year older than the DHS sample.

Table 1: Marriage penalties and premiums

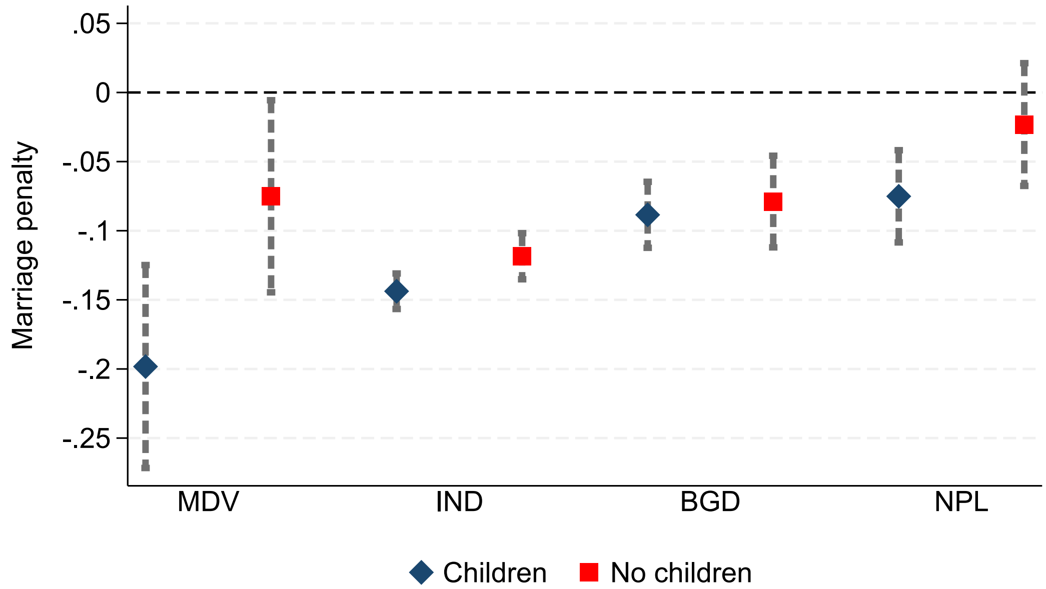
Dependent variable	Working			
Sample	Male		Female	
	(1)	(2)	(3)	(4)
Post-marriage	0.233***		-0.128***	
	(0.013)		(0.005)	
Post-marriage × No child sample		0.129***		-0.115***
		(0.009)		(0.008)
Post-marriage × Ignore child sample		0.266***		-0.140***
		(0.016)		(0.006)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	15414	15414	23447	23447
R^2	0.642	0.650	0.263	0.272

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and Maldives the marriage penalty explains 31 and 38 percent, respectively. As expected, the estimates for the "ignore child" sample are larger than the no child sample for all countries. Still, the relatively small gap in these estimates implies that the reduction in female labor force participation around the time of marriage is driven specifically by employment restrictions placed on married women rather than their childcare burdens.

In contrast, men enjoy a marriage premium in South Asia. Table 1, columns (1) and (2), shows the estimates for the men's sample. The estimated premium for men is 23.3 p.p. in aggregate, 12.9 p.p. for the no child sample and 26.6 for the ignore child sample, so that marriage explains roughly half of the total male premium. Annex Table A6 estimates these premiums by country. The no child, pure marriage estimates are remarkably similar across countries. In contrast, the combined family formation premium is nearly twice as large in India as in any other South Asian countries. These positive impacts of both marriage and children are consistent with the extensive literature on the marriage earnings premium in the US (Hersch and Stratton, 2000). As with women, the difference in the estimates between the no-child and ignore-child samples is small. Taken together, the findings in Table 1 demonstrate a marriage penalty for women and marriage premium for men in South Asia. For women, these effects

Figure 1: Country-specific penalties



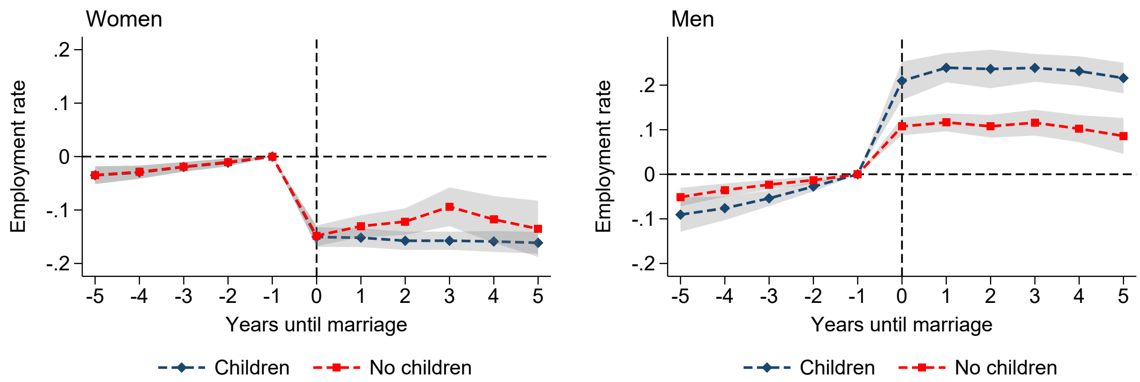
Note: Figure shows coefficient estimates and 95% confidence intervals from country-specific marriage penalty regressions for the sample of female cohorts. Standard errors are clustered at the cohort level. "Children" is the "ignore child" sample.

are driven primarily by the act of marriage itself, rather than the time and financial costs associated with child-rearing.

3.2 Event-studies

Figure 2 plots event-study coefficients on indicators for leads and lags of the marriage year, relative to $t = -1$, by matching sample, for men and women. For each set of estimates, the pre-marriage event coefficients show a mild upward trend in labor force participation for both men and women prior to marriage, with no evidence of anticipation effects. For women, the reduction in labor force participation is observed immediately, with a $t = 0$ reduction of 15 p.p. in both the "ignore child" and "no child" samples, suggesting no additional child penalty. Dynamic point estimates are available in Table A7. As expected, the no-child dynamic estimates in Figure 2 are almost always above the ignore child sample estimates. Still, the confidence intervals typically overlap, and the magnitudes in the differences are very small. For both samples, the effects persist 5 years after marriage, at 16.1 p.p. for the combined family formation penalty and 13.5 for women without children.

Figure 2: Marriage event study



Note: Figure shows coefficient estimates and 95% confidence intervals from pooled event-study marriage penalty regressions for the sample of female cohorts. Standard errors are clustered at the cohort level. "Children" is the "ignore child" sample.

Figure 2 also shows the dynamic male marriage employment premium, relative to $t = 0$. As with women, the pre-trends indicate a slight increase in employment up to the date of marriage. However, after marriage, male employment jumps by across South Asia 21 p.p. in the combined effect and 11.6 p.p. among men without children. The male marriage premium is sustained throughout the 5-year window and does not vary substantially over time.

Figures A1 and A2 plot the event-study estimates for women and men, respectively, by country, with corresponding estimates for women in Tables A8 and A9. Lower labor force participation among married women without children persists up to 5 years after marriage in India and Maldives. In contrast, female employment rates appear to revert back to pre-marriage levels by $t = 5$ in the no-child sample for Bangladesh and Nepal. Still, it is important to recall that the event-study estimates in the no-child sample are likely affected by time-varying selection bias. In particular, the pool of eligible women gets increasingly selected as time passes, since the set of married women with no children at $t = 5$ are likely to have the highest propensity to participate in the labor market, even after matching on our set of observables. As such, we are increasingly likely to under-estimate the true dynamic marriage penalty effects as t increases. Across all countries, employment declines are more persistent for the ignore child sample, where this problem does not exist.[4] Lastly, in addition to this potential source of bias, the effects become less precisely estimated as t rises, since the pool of possible matches shrinks. For men, the marriage premium persists in all countries except Maldives, where they are noisily estimated.

4 Determinants of the marriage penalty

In this section, we explore the relative importance of opportunity costs and social norms in driving the marriage penalty. This reflects to competing theories about the drives of labor specialization within the household. The marriage penalty might be the optimal choice of a joint household optimization problem in which men and women face different outside options and returns to labor market activity. But could also be due to social norms, which drive a wedge between optimal labor supply and realized level for women. Specifically, we argue social norms about women's mobility outside the home could constrain their labor supply after marriage. These theories have starkly different welfare implications. – first is efficient second suggests misallocation.,

Specifically, we test whether marriage penalties are lower for (i) for better-educated couples, (ii) for couples with more liberal gender attitudes, and (iii) for occupations that require less work outside the home. To test hypotheses (i) and (ii), we interact fixed marriage cohort-specific characteristics with the post-marriage indicator to estimate heterogeneous responses of employment to marriage. To test (iii), we use data from the Indian Human Development Survey (IHDS), which provides detailed occupation-specific labor supply data, to estimate marriage penalty impacts on different categories of work.

4.1 Education

Education interacts with other human capital domains and affects a range of labor market outcomes (Heath and Jayachandran, 2016). Education is likely to be a critical determinant of marriage penalty for two reasons. First, positive returns to female education in South Asia (Psacharopoulos and Patrinos, 2018) imply that more educated wives have a greater oppor-

[4]This could of course also be due to a more persistent child penalty as well. We cannot separately identify these two effects.

tunity cost of non-participation. Second, education levels are related to women's bargaining power and the loosening of constraints to the employment and mobility of married women.

In a unitary household model, relative prices wages labor supply through both own and cross-substitution effects, as well as income effects. So a woman should respond to her own outside options positively and her husband's options negatively. Instead, in the collective household model would predict only own-wage substitution effects (plus weaker income effects), as well as intra-household bargaining power effects (Chiappori, 1992). Women respond more to their own outside options because the bargaining effect reinforces the own-price effect, while the cross-effect of the husband is eliminated.

For simplicity, we measure outside work options using education levels, which are constructed as a binary indicator for whether the wife or husband has attained any post-secondary education.[5] Table 2 begins by including the interaction with the woman's higher education indicator in the ignore child sample (column 1-2) and the no child sample (column 4-5). Women with education levels beyond secondary school are significantly less affected by the marriage penalty on average in South Asia. Indeed, the marriage penalty is 14.9 p.p. for women without higher education, and just 7.9 p.p. for women with post-secondary schooling (column 5), a difference that is significant at the 1% level. In aggregate, higher education offsets 53% of the pure child penalty and 46% of the combined marriage and child penalty. This implies that even among women with children, the employment penalty can be partially offset with greater education.

Husbands' higher education may also help mitigate a women's marriage penalty by liberalizing household norms around women's work. At the same time, more educated men earn more, strengthening the pull of the income effect (and relative price effect, in a unitary model) that draws married women out of the labor market. Table 2, columns (2) and (5), tests the role of husband's education by interacting the post-marriage indicator with the cohort share of husbands with higher education. The magnitudes of this interaction effect are similar to the results in columns (1) and (4). Cohorts with more educated husbands have significantly smaller marriage penalties.

However, the regressions in columns (1)-(2) and (4)-(5) of Table 2 ignore a critical source

[5]The latter of these is the marriage cohort averaged share of women married to husbands with post-secondary education.

Table 2: Marriage penalty by education

Dependent variable	Working					
Sample	Child			No child		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-marriage	-0.162***	-0.178***	-0.171***	-0.140***	-0.149***	-0.139***
	(0.007)	(0.008)	(0.009)	(0.009)	(0.011)	(0.011)
Post-marriage × Higher education	0.052***		0.054**	0.060***		0.082***
	(0.010)		(0.021)	(0.018)		(0.030)
Post-marriage × Husband higher education		0.082***	-0.001		0.071**	-0.036
		(0.018)	(0.037)		(0.028)	(0.046)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13134	12886	12886	10313	9820	9820
R^2	0.339	0.343	0.348	0.228	0.229	0.232

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. Husband education is the share of women in the pseudo-cohort whose husband has any post-secondary education. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

of omitted variable bias. In particular, the marriage market in South Asia, as in other contexts, is characterized by high levels of positive assortative mating, where women with more education tend to marry men with more education (Becker, 1993). Indeed, about 70 percent of women with more than secondary schooling are married to men with the same level of education. As such, the educational attainment of wives is highly correlated with that of their husbands', suggesting that neither analysis alone appropriately accounts for these correlations. Estimating the independent effects is important, since wives' education affects both her labor market returns and household norms, while husband's education mostly affects the latter and not the former. Including interactions with education levels for both husbands and wives in the same regression allows us to estimate independent effects of husbands' and wives' outside options.

Columns (3) and (6) present the results of a model containing interactions between the post-marriage indicator and both the husband and wife's education, for the ignore child and no child samples. After accounting for the male education interaction, a woman's attainment of post-secondary education now offsets 60% of the baseline marriage employment penalty (column 6). The coefficient on the interaction with husband's education is -0.036, suggesting that in cohorts where the men are highly educated, the marriage penalty is 3.6 p.p. *larger*. However, this coefficient is not significantly different from zero in the regional sample or in any of the individual countries. Similar results obtain for the combined marriage and child penalty sample in column (3), where the male education interaction is insignificant, while the female interaction coefficient remains positive and significant.

The results suggest collective model is a better fit for the data. In the unitary model, income and relative price effects reinforcement, predicting a positive own education effect and a negative husband effect, the latter of which we do not observe in the data. In the collective model, husbands wage has a limited effect on a wife's labor supply because there are no relative price effects and the income effect is weakened via the sharing rule.

However, the husband's education is likely to be correlated with more liberal social norms. The lack of a positive coefficient on husband's education in Table 2 after accounting for positive assortative mating might suggest that a woman's opportunity cost of not working matter more than her husband's gender attitudes in determining the marriage penalty. However, it is important to note that the male education effect can theoretically go in both directions—that is, income effects push the coefficient downward while social norms push it up, so that the results in Table 2 do not rule out the importance of gender norms. In fact, these offsetting effects could well explain the zero effect of husband's ed in the context of a collective model with social norms considerations. As such, we need to model the role of gender attitudes directly.

4.2 Gender attitudes

Women's labor supply in South Asia is constrained by household norms around female employment (Heath et al., 2024; Jayachandran, 2021; Bussolo et al., 2024). In conservative contexts, working outside the home, even without the burden of childcare, violates social norms that control women's mobility. These norms bind more strongly when women are married and become subject to pressure from the husband and his family (Anukriti et al., 2020). This dynamic may in part explain the prevalence of a marriage penalty in South Asia. If so, we should expect to see larger marriage penalties among marriage cohorts with more restrictive gender attitudes on average.

To test the role of gender attitudes, we would ideally use survey measures eliciting the husband's approval of women working outside of the home. However, the DHS does not contain survey questions on men's attitudes specifically around the labor market participation of their wives. Instead, the DHS contains information on other gender attitudes, including with respect to domestic violence, as well as measures of household decision-making roles. Using the DHS women's module, we compute two measures of gender attitudes: (i) the average agreement in the cohort with five survey questions featuring scenarios under which domestic violence toward a might could be "justified," and (ii) the average agreement in the cohort with five questions around decision-making on household income, expenditures, and child-rearing. For the "attitudes toward violence" index, larger numbers indicate a larger share of female respondents in the cohort agreed with justifications for wife-beating, while for the "decision-making" index, larger numbers indicate that the wife participates in more household decisions. These are highly imperfect proxies for the husband's gender attitudes toward work, since they are taken from wives' responses and do not directly concern work. Still, they may capture meaningful variation across marriage cohorts in the permissiveness of gender attitudes. For each of these variables, we collapse the average responses at the marriage cohort-event time level and then take the mean within marriage cohorts to get a marriage cohort-specific, time-invariant measure of gender attitudes.

Table 3: Marriage penalties for women by household gender attitudes

Dependent variable	Working			
Sample	Child		No child	
	(1)	(2)	(3)	(4)
Post-marriage	-0.162***	-0.131***	-0.140***	-0.108***
	(0.009)	(0.007)	(0.011)	(0.010)
Post-marriage × Decisionmaking	0.027***		0.041***	
	(0.006)		(0.010)	
Post-marriage × Attitude towards violence		-0.056***		-0.036*
		(0.015)		(0.020)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	10896	10896	8818	8818
R^2	0.332	0.331	0.229	0.227

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Decision-making is measured as the sum of indicator variables for 5 DHS questions on whether the respondent is involved household decisions, averaged by cohort, with larger values indicating greater involvement. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3 contains the results for decision-making and domestic violence. In the all South

Asia sample, the marriage penalty when the woman has no decision-making agency is 14.0 p.p (column 3). However, as agency rises, the penalty falls. A one standard deviation increase in decision-making power reduces the marriage penalty by 4.6 p.p, significant at the 1% level. Similar, though slightly weaker, results obtain in column (1) for the combined family formation penalty. This is likely because child penalties are driven by other factors, for example, access to affordable childcare, that do not influence marriage penalties and are potentially unrelated to social norms.

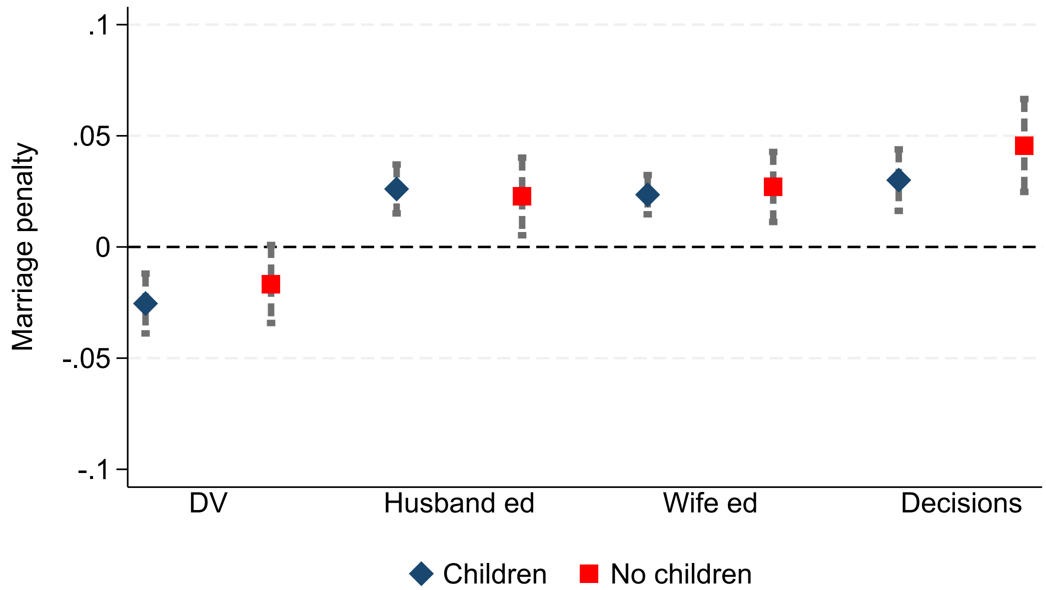
The results for violence in columns (2) and (4) go in the same direction, though are only significant at 10% for the marriage penalty estimate in (4). On average, among households where women do not agree with any domestic violence justifications, the marriage penalty is 10.8 p.p., rising by 3.6 p.p. for every additional justification agreed with. Slightly stronger results obtain for the combined marriage and child penalty in column (2).

The preceding analysis does not separate the effects of education and social attitudes, which are likely to be highly correlated, at least within countries. Appendix Table A10 includes interactions between the post-marriage indicator and *both* the female higher education dummy and the cohort decision-making index. Column (1) of both Panels A and B, the regional specification, reveals that both interaction terms remain strong and significant, and nearly unchanged in magnitude from the single-interaction specifications. This suggests independent effects of outside options and household constraints in shaping the marriage penalty. Finally, Figure 3 summarizes all of the heterogeneous effects tested in Section 4, emphasizing the roles of female educational attainment and social norms in shaping the marriage penalty.

5 Mechanisms: which norms matter?

What social norms drive the impact? This is a challenging question, since most household survey with labor market information do not contain detailed data on adherence to specific gender attitudes or social norms. In the South Asian context, two critical social norms that might drive marriage penalties are patrilocality and physical mobility. In this section, we test the relative importance of each of these explanations.

Figure 3: Heterogeneous penalties



Note: Figure shows interaction coefficient estimates and 95% confidence intervals obtained by estimating the baseline marriage penalty regression specification, interacting the post-marriage indicator with a given cohort characteristic, separately for the no child and ignore child samples. Domestic violence is measured as the sum of agreement indicator variables for 5 DHS questions on the acceptability of domestic violence, averaged by cohort. Decision-making is measured as the sum of indicator variables for 5 DHS questions on whether the female respondent is involved in decision making individually or jointly, averaged by cohort. Husband education is the share of women in the cohort whose husband has any post-secondary education. Women's education is an indicator variable for any post-secondary education for the cohort.

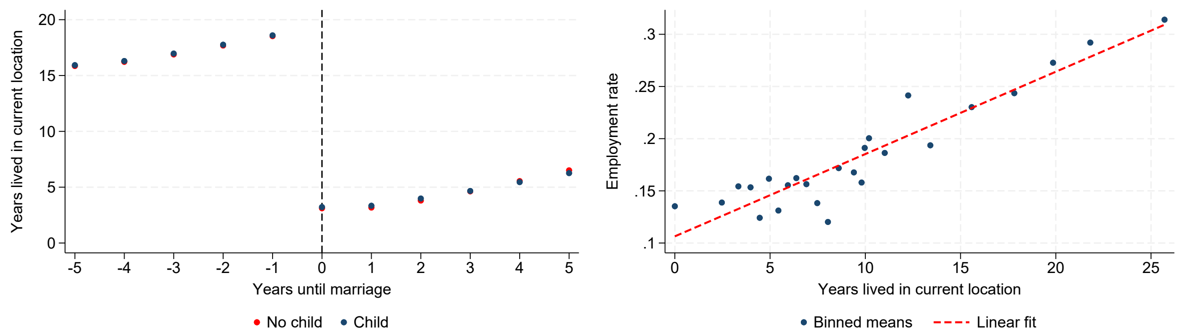
5.1 Patrilocality

Patrilocality is a social practice in which the married couple resides with or near the groom's family. This practice is common among most of South Asia's ethno-cultural groups. In India 70 percent of married women co-reside with parents in law following marriage (Jayaraman and Khan, 2023). Patrilocality has been show to reduce women's household agency, mobility, and social connections outside the home (Anukriti et al., 2020; Khalil and Mookerjee, 2019), though impacts on labor supply have been more mixed (Khanna and Pandey, 2024; Landmann et al., 2018; Jayaraman and Khan, 2023).

Patrilocality may explain the marriage penalty in two ways. First, when a woman marries, she may move to a new location. In the process, she loses social networks and information that might help in reducing job search costs. Second, if she moves in with her in-laws, she also comes under new obligations and restrictions, perhaps to care for parents in law. The impact of marriage may just be the impact of moving, or assuming the expectations of a new household.

Figure 4: Patrilocality, migration for marriage, and work

(a) Duration of stay and marriage (b) Duration of stay and employment



Note: Panel (a) shows average years lived in the location in which a respondent was sampled in the DHS by years before and after marriage. Panel (b) shows cohort-year level correlation between average years and the employment rate, controlling for birth year and age fixed effects. All estimates are weighted by cohort size.

The analysis confirms that patrilocality induces a discrete shift in location at marriage. Figure 4, panel (a), plots the average number of years a female DHS respondent lived in her current location by event-time. The results reveal a very large drop in the average duration in the current location—equivalent to nearly 15 years—occurring exactly at the time of marriage.[6] In panel (b), we relate average duration in a woman's current place to the employment rate, revealing a strong positive correlation, conditional on age and cohort fixed effects, likely due to the labor market networks and information that accrue to long-tenured individuals. Due to patrilocality, migration for marriage induces a distinct drop in tenure that may have implications for employment. We can therefore think of migration for marriage as an alternative treatment that is strongly correlated in time with marriage, though not collinear since some women do not migrate for marriage, while others migrate in the years before or after. The problem is that migration can disrupt employment for reasons that have little to do with

[6]These estimates are unconditional averages, but coefficients from regressions with country, year, birth year, and age fixed effects reveal similar patterns, which are statistically significant.

household social norms. Since this migration is driven by the patrilocality norm, it is part of a broader family of norm-based mechanisms. However, it does not concern household norms around women's work, per se.

Table 4: Marriage penalties for women by migration

Dependent variable	Working					
Sample	Child			No child		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-marriage	-0.140***	-0.050***	-0.160***	-0.115***	-0.055**	-0.086***
	(0.006)	(0.019)	(0.014)	(0.008)	(0.027)	(0.024)
Duration in current place (years)		0.006***			0.004**	
		(0.001)			(0.002)	
Post-marriage × Mother-in-law			0.034			-0.048
			(0.021)			(0.036)
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Years						
Observations	13134	8462	11037	10313	6692	8862
R^2	0.334	0.334	0.350	0.223	0.217	0.242

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Moved for marriage is measured as the cohort share of women who migrated within t years of marriage. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To isolate the marriage penalty net of patrilocality effects—that is, holding location fixed—we control for the number of years in the current location in Table 4. Columns (1) and (4) reprint the main family formation and marriage penalty estimates, respectively, for reference. In both samples, the inclusion of controls for duration reduces the penalty estimate to 5-6 p.p. in columns (2) and (5). In both specifications, the impact of current place is positive, reflecting the relationship observed in Figure 2, panel (b): a one-year increase in duration is associated with an increase in employment of 0.4-0.6 p.p. Comparing estimates of the marriage penalty between columns (4) and (5) implies that patrilocality explains roughly half (52%) of the marriage penalty.

The patrilocality effect might be driven either by the labor market disruption of a move, or by the "mother-in-law effect." The latter refers to the potentially negative impact of. To test for mother-in-law effects, we interact the post-marriage indicator with a variable measuring the share of the cohort that lives with the mother-in-law. The results are in columns (3) and

(6) of Table 4. In both samples, the interaction is not statistically significant. However, the magnitude is reasonably large in the no child sample in column (6): increasing the probability of living with the mother in law from 0 to 1 magnifies the marriage penalty by 4.8 p.p. Though not significant, the point estimate is relatively large and in the expected direction.

5.2 Occupational choice

The occupational choices of married women provide a potentially informative signal about the types of gender norms that drive the marriage penalty. In particular, recent evidence suggests that a key constraint to women's work in South Asia are norms that restrict female mobility. As such, occupations that require entering into formal labor arrangements at a place of work outside the home are likely to be most strongly curtailed after marriage. This is consistent with recent experimental evidence suggesting that women's take-up of otherwise identical employment opportunities in Mumbai rises significantly if this work can be done from home, while only 28% of women report being allowed to work outside the home at all (Ho et al., 2024; Jalota and Ho, 2024). The social stigma is not against women working per se, but against them working in an environment where they may be forced to interact with men that they are not related to (cite Alice evans). These concepts of female seclusion have roots in the purity norms of the caste system (Bernhardt and Agte, 2023), but extend to both Hindu and Muslim communities in South Asia.

We test for the role of female seclusion norms by dividing occupations into four categories: manual, services (which includes sales, professional/technical, and clerical work), agriculture, and domestic work. The first two take place outside the home, while the latter two are more likely to be done inside the home.[7] We then re-calculate the main outcome variable for each of these occupation categories, so that the outcome is the probability of employment in a given category.

The results of the marriage penalty regressions by occupation are in Table 5. Estimates reveal that in the no child sample (Panel A), the marriage penalty effects are concentrated entirely in manual labor occupations (1) and services (2), both of which generally involve em-

[7] For instance, much of agriculture refers to work on the family farm, which is at home by definition. Domestic work, in contrast, refers to services work of a household nature done in a private home. While this is technically not work done at home, it has a more private character, requires less formal workplace arrangements, women are less likely to encounter male co-workers, etc. Domestic work is therefore more likely to be socially acceptable.

Table 5: Marriage penalties by occupation

Occupation	Manual	Services	Agri	Domestic
	(1)	(2)	(3)	(4)
Panel A: No child				
Post-marriage	-0.026***	-0.074***	0.002	0.005
	(0.004)	(0.007)	(0.008)	(0.004)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.040	0.085	0.091	0.028
Observations	8330	8330	8330	8330
R^2	0.103	0.197	0.303	0.285
Panel B: Child				
Post-marriage	-0.032***	-0.089***	-0.013**	-0.000
	(0.003)	(0.006)	(0.005)	(0.003)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Mean Dep Var	0.037	0.077	0.084	0.026
Observations	10618	10618	10618	10618
R^2	0.139	0.247	0.455	0.328

Note: Standard errors in parentheses clustered at the cohort level. Sample is all female cohorts. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ployment outside the home. The employment rate in these two categories falls dramatically, by 65 and 87%, respectively, relative to the pre-marriage mean participation rate. Instead, there are no significant employment penalties in agriculture (3) or domestic work (4), occupations that can be done from home or that require less formal workplace arrangements. Results are similar in the "ignore child" sample (Panel B), though the declines are generally slightly larger, consistent with a child marriage penalty.

These occupational categories are somewhat coarse, and do not contain any information responses on the intensive margin (i.e., work hours). To investigate these effects in greater detail, we leverage survey data from India (IHDS) on sector of work and labor supply. The benefit of the IHDS is that it explicitly identifies *salaried* work, which corresponds more closely to work done outside the home. Reconstructing our marriage cohorts identically as in the DHS, we estimate the impact of marriage on work in salaried employment versus farm work, as well as on total labor supply.

Table 6: Marriage penalties by type of work: India

Margin	Working			Work hours	
Sector	All	Farm	Salaried	All	Farm
	(1)	(2)	(3)	(4)	(5)
Post-marriage × No child sample	-0.069**	-0.015	-0.060***	-106.000***	22.542*
	(0.032)	(0.023)	(0.016)	(22.597)	(11.527)
Post-marriage × Ignore child sample	-0.069**	-0.009	-0.068***	-133.726***	26.738***
	(0.027)	(0.021)	(0.012)	(16.708)	(10.294)
Cohort FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	3917	3917	3917	3917	3917
R^2	0.065	0.081	0.086	0.148	0.055

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6 contains the results. Column (1) replicates the main marriage penalty findings, revealing a 6.9 p.p. drop in employment following marriage, an effect identical for both samples, suggesting no independent child penalty. This drop is driven entirely by reductions in salaried work (column 3), with no significant change in farm work, again reinforcing the notion. On the intensive margin, overall labor supply falls dramatically (column 4), by 106 hours per year on a pre-marriage baseline of 200 hours.[8] However, agricultural labor supply actually rises by 22.5 hours per year, suggesting substitution between work done at home and outside, even as total labor supply falls. This reallocation of labor is consistent with stigma around the types of jobs married women can do, rather than whether they can work at all. Event study plots in Figure A4 show minimal pre-trends, and that these shifts are sustained up to five years after marriage.

[8]Note that these numbers are very low because they include all the zeroes.

6 Robustness

Time-varying selection bias: The most important potential threat to our identification approach is the issue of time-varying selection bias. Married women who remain childless after $t + k$ years of marriage are likely to be selected on unobserved traits. As such, the matched post-marriage comparison group in the no child sample may exhibit selection bias, and this bias is likely to compound as time passes, since a married woman who is childless at $t = 5$ is likely to be more selected than one who is childless at $t = 1$. However, it is most plausible that women who delay childbearing further are likely to have greater unobserved propensity to work, pushing up their employment levels. This suggests that our no-child results are, if anything, under-estimated particularly toward the end of the pseudo-panel. To address this concern, we consider whether certain demographic characteristics are balanced across the post-marriage periods, or whether they exhibit a differential trend in the child vs. no child samples. Figure A3 plots cohort average post-marriage demographics by event-time and sample. While there are some mild trends in how demographics evolve by cohort-time, there doesn't seem to be differential trends between the two matching samples of the type that would systematically explain our results. Regression-based estimates in Table A14 confirm that trends in demographics are generally not significantly different between the two samples.

Weights: Selecting cohort weights presents a non-trivial problem. Two options are available—to weight by the sample size of the reference woman's cohort, or to weight by the sample size of each individual cohort-time cell. Throughout the paper, we choose the former, since it avoids the issue of differential weighting of event-time periods simply because more matches are available in a given period. However, this weighting choice might affect the results. Table A12, Panel A, considers the impact of using cell size weights, and finds the results unchanged. Indeed, even in Panel B when weights are abandoned entirely—treating each cohort identically regardless of the number of women it is comprised of—the results are remarkably similar.

Balanced panel: The composition of cohorts in the sample might change over time, since many cohorts do not have a fully balanced panel of matched observations before and after marriage. if this drop-out occurs in a non-random fashion, it might bias the aggregate results.

To account for this possibility, we re-estimate the main effects in Table A13 using a fully balanced panel (i.e., only cohorts with matches in all periods from $t = -5$ to $t = 5$). The results are unchanged.

Urban versus rural effects: Urban areas, with dynamic, service-oriented economies, may present with more and higher-earning employment opportunities (Petrongolo and Ronchi, 2020). Better job prospects imply a larger opportunity cost of keeping women out of labor market, while urban areas in South Asia may also have less conservative gender norms (Asher et al., n.d.). At the same time, women's baseline employment levels are significantly lower in urban than rural South Asia for a variety of reasons (Klasen and Pieters, 2015), including income effects, sectoral composition, and the predominance of outside-the-home work arrangements. Therefore, the relative size of the marriage penalty in rural and urban areas is theoretically ambiguous. Table A11 tests this question empirically by splitting the sample between urban and rural cohorts and re-estimating the main penalty regressions. Both male premiums and female penalties are quantitatively similar across locations.

7 Conclusion

Recent studies have shown how child penalties affect women's labor market outcomes. However, a marriage penalty may emerge even before child-rearing, either because of optimal household task specialization or social norms that prevent married women from working outside the home. In the context of South Asia, We show that the marriage penalty for women is substantially larger than the child penalty. This suggests that it is not the burdens of child-rearing, per se, that inhibit female labor force participation in South Asia. Instead, low female labor supply in the region emerges after marriage and before children. This result reconciles mixed findings in the literature on access to childcare, since access is not the binding constraint to participation in South Asia.

We also explore the drivers of marriage penalties for women in South Asia in order to test whether such outcomes are a natural result of optimal household specialization, or are driven by conservative gender norms that constrain the labor supply of married women. Our findings are consistent with both explanations. We find that a woman's education – and not that of her husband – substantially reduces the marriage penalty. Since greater female education has the potential to both change gender norms *and* improve women's opportunities in the labor market, it is hard to disentangle these two mechanisms with this test alone. Still, we note that husband's education, which should affect household norms but not women's labor market outcomes, does not affect the marriage penalty. Regardless of the mechanism, the results clearly show that increasing education for women has the potential to mitigate the marriage penalty, potentially helping them realize their labor market potential.

Finally, we test for gender norms as a driver of the marriage penalty, and therefore a source of potential labor market misallocation in South Asia. Norms around household gender roles, as measured by women's decision-making power and attitudes toward domestic violence, are highly predictive of the extent of marriage penalties across South Asia. In addition, when both norms and female education are included as mechanisms, they remain strong independent predictors of the marriage penalty, suggesting that both labor market opportunities and social norms play a role in driving the marriage penalty. Policies that promote gender equality within the household and a shift in social norms may also have the potential to mitigate women's marriage penalty.

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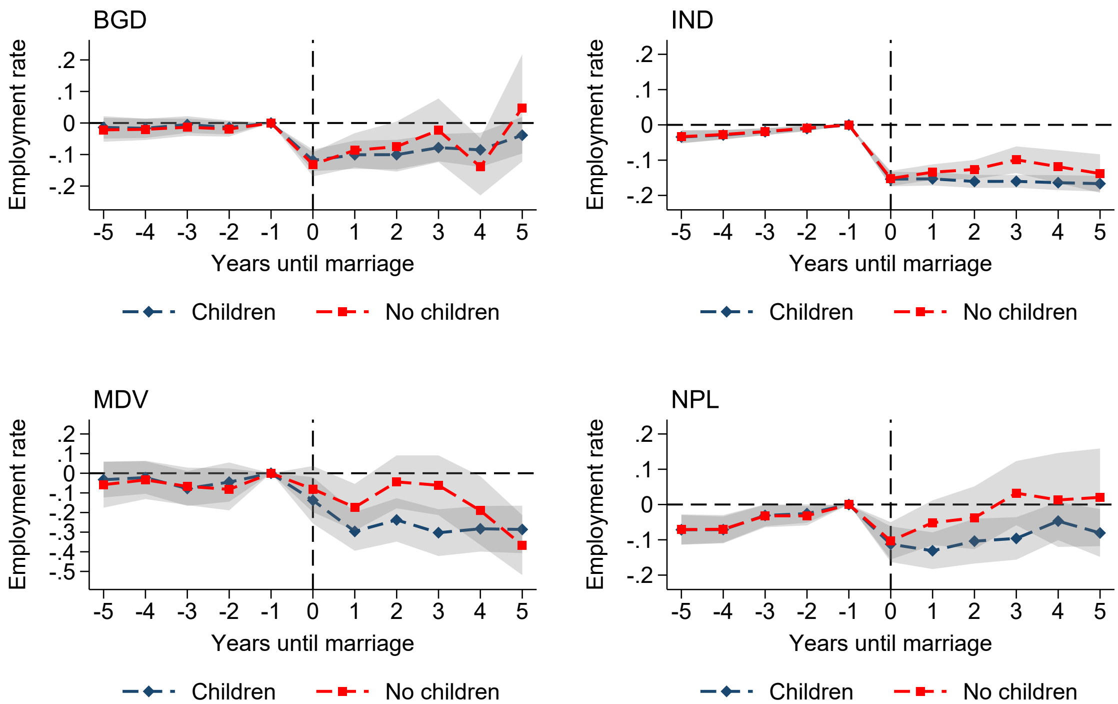
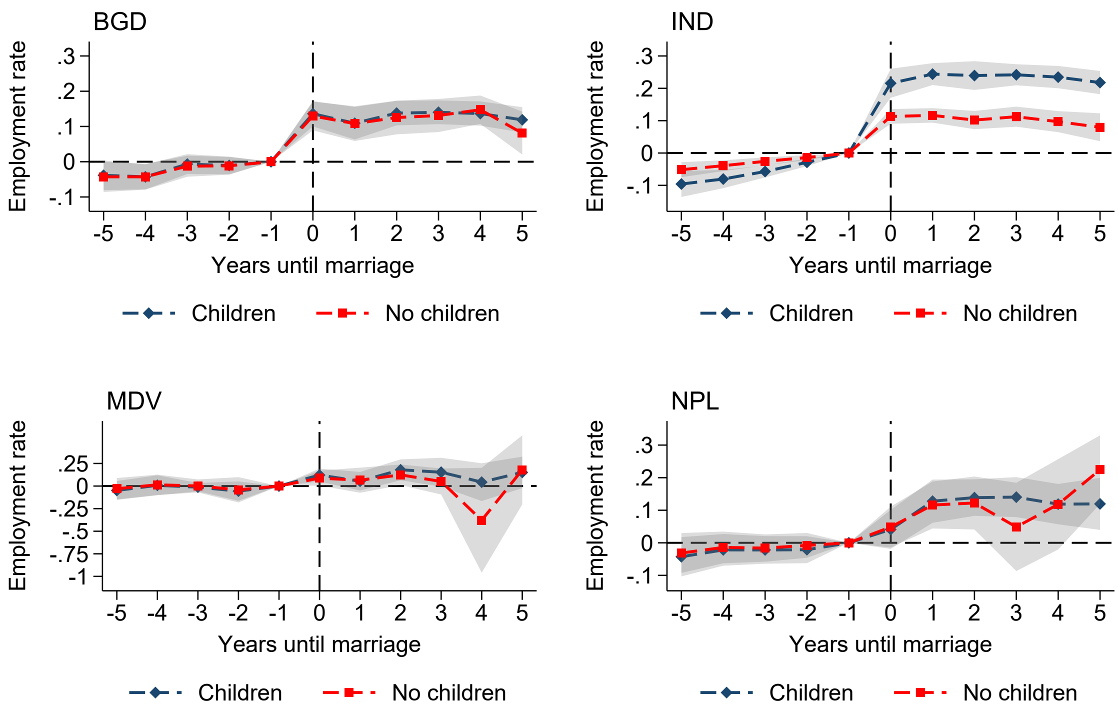
A Appendix

Figure A1: Country-specific female event study



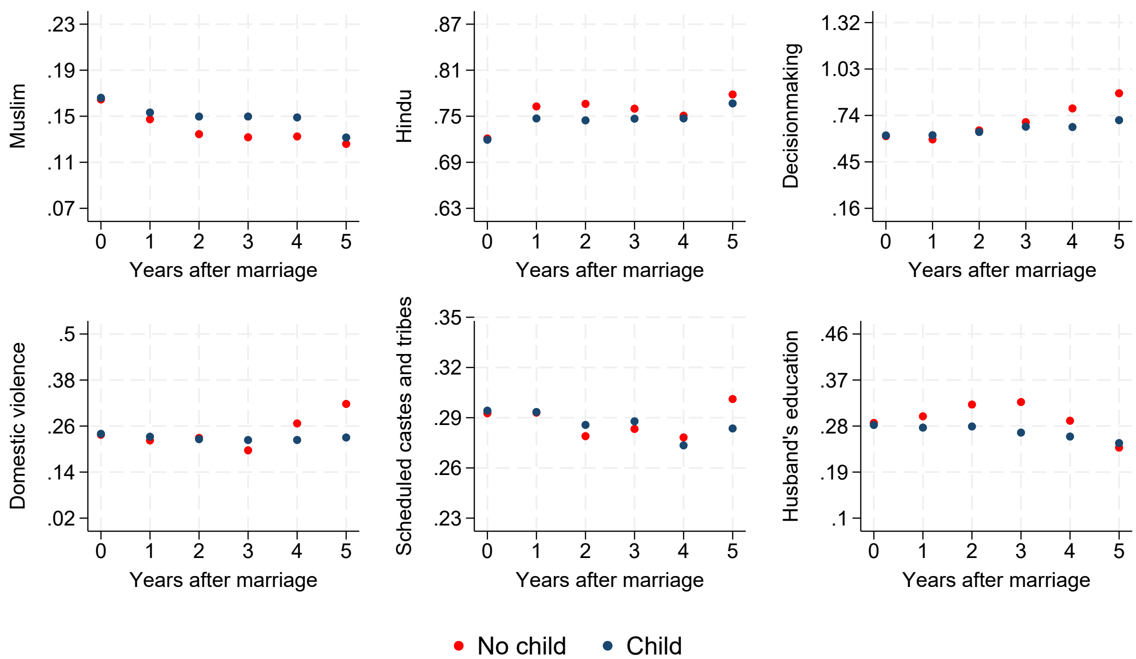
Note: Figure shows coefficient estimates and 95% confidence intervals from country-wise event-study marriage penalty regressions for the sample of female cohorts. Standard errors are clustered at the cohort level. "Children" is the "ignore child" sample.

Figure A2: Country-specific male event study



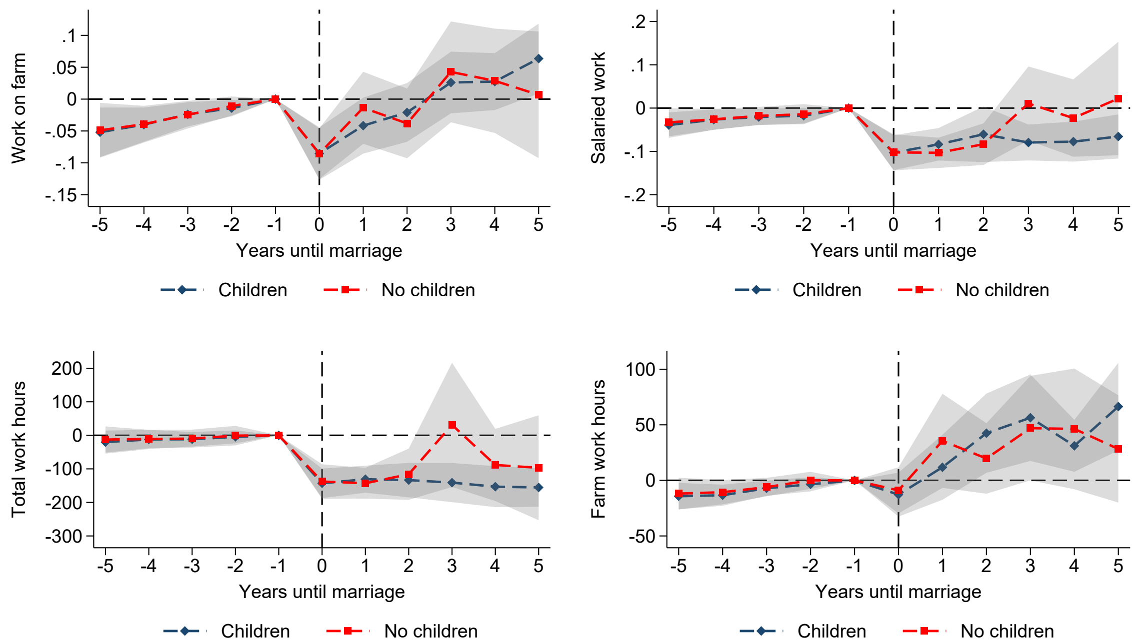
Note: Figure shows coefficient estimates and 95% confidence intervals from country-wise event-study marriage penalty regressions for the sample of male cohorts. Standard errors are clustered at the cohort level. “Children” is the “ignore child” sample.

Figure A3: Demographics after marriage



Note: Figure shows average cohort demographics and spousal characteristics for $t \geq 0$. Husband's education is measured as the share of women married to a husband with post-secondary education. Muslim, Hindu, and Scheduled caste/tribe are measured as the share of women in these demographic groups. "Child" is the "ignore child" sample.

Figure A4: India event study: Type of work and labor supply



Note: Figure shows coefficient estimates and 95% confidence intervals from India-only event-study marriage penalty regressions for the sample of female cohorts. Standard errors are clustered at the cohort level. “Children” is the “ignore child” sample.

Table A1: DHS datasets used in the analysis

Country	Women	Sample size	Men	Sample size
Bangladesh	1993	11863	1993	8157
	1999	13527	1999	9028
	2004	15338	2004	20030
	2007	13769	2007	8183
	2014	20336	2014	6790
	2017	24439	2017	27602
India	1993	84558		
	1998	84862		
	2005	118514	2005	73465
	2015	670738	2015	110478
	2019	692985	2019	100275
Maldives	2009	6558	2009	1645
	2016	7074	2016	4132
Nepal	1996	903		
	2001	909	2001	2208
	2006	2970	2006	4333
	2011	3832	2011	4080
	2016	3607	2016	4043
	2022	4072	2022	4864

Note: List of DHS surveys used in the analysis. The Bangladesh DHS samples ever married women. To match women to surrogate observations in the pre-marriage event space, the household roster is used. To construct a sample of men, the household roster is used. Married men in the household roster are matched to their wives in the ever married women dataset. These men are then matched to surrogate observations in the pre-marriage event space using the household roster.

Table A2: Summary statistics: Women

Panel A. No child sample	Mean	SD	Obs
Working	0.195	0.129	8,922
Birth year	1,996.929	4.731	9,984
Age	19.512	3.626	9,984
Urban	0.377	0.485	9,984
Less than primary education	0.030	0.148	8,897
Primary education	0.018	0.100	8,897
Secondary education	0.125	0.143	8,897
More than secondary education	0.276	0.447	8,897
Decision making index	0.314	0.312	9,874
Attitude toward domestic violence index	0.187	0.191	9,874
Age gap with partner	3.051	1.545	4,859
Age at marriage	21.221	3.098	4535
Panel B. Ignore child sample	**Mean**	**SD**	**Obs**
Working	0.194	0.124	11,065
Birth year	1,996.881	4.781	11,944
Age	19.555	3.680	11,944
Urban	0.377	0.485	11,944
Less than primary education	0.031	0.149	10,776
Primary education	0.018	0.100	10,776
Secondary education	0.125	0.142	10,776
More than secondary education	0.276	0.447	10,776
Decision making index	0.330	0.336	11,763
Attitude toward domestic violence index	0.188	0.192	11,763
Age gap with partner	3.062	1.507	6,760
Age at marriage	21.274	3.178	6,228

Note: Data on cohorts of women from Bangladesh, India, Maldives, Nepal DHS. Decision making index based on the sum of decisions in which women are involved individually or jointly. Attitude toward domestic violence index based on the sum of statements respondents believe violence is justified.

Table A3: Summary statistics: Men

Panel A. No child sample	Mean	SD	Obs
Working	0.530	0.261	7,540
Birth year	1,993.742	6.138	7,540
Age	20.958	4.251	7,540
Urban	0.379	0.485	7,540
Less than primary education	0.033	0.148	6,481
Primary education	0.021	0.101	6,481
Secondary education	0.130	0.138	6,481
More than secondary education	0.234	0.424	6,481
Panel B. Ignore child sample	Mean	SD	Obs
Working	0.541	0.265	8,792
Birth year	1,993.519	6.277	8,792
Age	21.155	4.394	8,792
Urban	0.373	0.484	8,792
Less than primary education	0.035	0.154	7,446
Primary education	0.021	0.103	7,446
Secondary education	0.128	0.137	7,446
More than secondary education	0.232	0.422	7,446

Note: Data on cohorts of men from Bangladesh, India, Maldives, Nepal DHS.

Table A4: Summary statistics: Women (IHDS)

Panel A. No child sample	Mean	SD	Obs
Working	0.313	0.284	1,745
Birth year	1987.120	5.275	1,745
Age	21.086	4.218	1,745
Urban	0.457	0.498	1,745
Less than primary education	0.059	0.187	1,745
Primary education	0.070	0.211	1,745
Secondary education	0.118	0.225	1,745
More than secondary education	0.349	0.477	1,745
Age gap with partner	4.722	2.628	705
Age at marriage	21.575	3.263	708
Hours worked	179.397	313.579	1,745
Hours worked: farm	46.839	108.499	1,745
Hours worked: non-farm	21.042	99.788	1,745

Panel B. Ignore child sample	Mean	SD	Obs
Working	0.312	0.273	2,188
Birth year	1986.375	5.437	2,188
Age	21.785	4.346	2,188
Urban	0.451	0.498	2,188
Less than primary education	0.057	0.181	2,188
Primary education	0.074	0.217	2,188
Secondary education	0.115	0.210	2,188
More than secondary education	0.347	0.476	2,188
Age gap with partner	4.705	2.332	1,074
Age at marriage	21.670	3.198	1,077
Hours worked	169.098	281.248	2,188
Hours worked: farm	51.517	118.111	2,188
Hours worked: non-farm	22.887	86.974	2,188

Note: Data on cohorts of women from the India Human Development Survey (IHDS). The two waves are treated as repeated cross sections.

Table A5: Country-specific marriage penalties for women

Dependent variable	Working			
Sample	BGD	IND	MDV	NPL
	(1)	(2)	(3)	(4)
Post-marriage × No child sample	-0.079***	-0.118***	-0.075**	-0.023
	(0.017)	(0.009)	(0.035)	(0.023)
Post-marriage × Ignore child sample	-0.089***	-0.144***	-0.198***	-0.075***
	(0.012)	(0.007)	(0.037)	(0.017)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	4346	13484	1021	4596
R^2	0.189	0.174	0.250	0.218

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Country-specific marriage premiums for men

Dependent variable	Working			
Sample	BGD	IND	MDV	NPL
	(1)	(2)	(3)	(4)
Post-marriage × No child sample	0.142***	0.129***	0.091*	0.094***
	(0.017)	(0.011)	(0.053)	(0.022)
Post-marriage × Ignore child sample	0.147***	0.272***	0.126***	0.119***
	(0.016)	(0.017)	(0.036)	(0.018)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	2405	9522	325	3162
R^2	0.353	0.663	0.460	0.316

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7: Marriage penalty: event-study estimates for men and women

Dependent variable	Working			
Gender	Male		Female	
Sample	Child	No Child	Child	No Child
	(1)	(2)	(3)	(4)
Year $t-5$	-0.091***	-0.051***	-0.035***	-0.035***
	(0.020)	(0.010)	(0.008)	(0.009)
Year $t-4$	-0.076***	-0.035***	-0.030***	-0.029***
	(0.014)	(0.008)	(0.006)	(0.006)
Year $t-3$	-0.054***	-0.023***	-0.019***	-0.019***
	(0.009)	(0.006)	(0.005)	(0.005)
Year $t-2$	-0.028***	-0.013***	-0.011***	-0.010***
	(0.006)	(0.004)	(0.004)	(0.004)
Year $t=0$	0.210***	0.107***	-0.150***	-0.148***
	(0.022)	(0.010)	(0.009)	(0.010)
Year $t+1$	0.239***	0.116***	-0.152***	-0.130***
	(0.017)	(0.010)	(0.009)	(0.010)
Year $t+2$	0.236***	0.108***	-0.158***	-0.122***
	(0.022)	(0.013)	(0.009)	(0.013)
Year $t+3$	0.239***	0.116***	-0.157***	-0.094***
	(0.016)	(0.015)	(0.009)	(0.018)
Year $t+4$	0.231***	0.102***	-0.159***	-0.118***
	(0.017)	(0.016)	(0.010)	(0.022)
Year $t+5$	0.216***	0.086***	-0.161***	-0.135***
	(0.018)	(0.020)	(0.010)	(0.027)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	8146	7261	13130	10311
R^2	0.673	0.460	0.335	0.227

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. $t=0$ is the time of marriage, $t-i$ is the pre-marriage space, $t+i$ is the post-marriage space. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A8: Marriage penalty: event-study estimates for female ignore child sample

Dependent variable	Working			
Sample	BGD	IND	MDV	NPL
	(1)	(2)	(3)	(4)
Year $t-5$	-0.014	-0.034***	-0.033	-0.071***
	(0.018)	(0.009)	(0.046)	(0.021)
Year $t-4$	-0.017	-0.029***	-0.022	-0.071***
	(0.016)	(0.007)	(0.042)	(0.019)
Year $t-3$	-0.005	-0.019***	-0.078*	-0.031**
	(0.014)	(0.005)	(0.045)	(0.016)
Year $t-2$	-0.014	-0.010**	-0.045	-0.026*
	(0.011)	(0.004)	(0.051)	(0.013)
Year $t=0$	-0.118***	-0.154***	-0.138**	-0.112***
	(0.017)	(0.010)	(0.060)	(0.026)
Year $t+1$	-0.101***	-0.153***	-0.296***	-0.131***
	(0.023)	(0.010)	(0.050)	(0.027)
Year $t+2$	-0.100***	-0.160***	-0.238***	-0.104***
	(0.024)	(0.009)	(0.056)	(0.032)
Year $t+3$	-0.078***	-0.160***	-0.303***	-0.096***
	(0.022)	(0.009)	(0.061)	(0.031)
Year $t+4$	-0.085***	-0.164***	-0.283***	-0.047*
	(0.028)	(0.011)	(0.059)	(0.028)
Year $t+5$	-0.039	-0.167***	-0.286***	-0.081**
	(0.030)	(0.011)	(0.062)	(0.035)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	2454	7506	577	2585
R^2	0.205	0.223	0.233	0.207

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. BGD - Bangladesh, IND - India, MDV - Maldives, NPL - Nepal. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. $t = 0$ is the time of marriage, $t-i$ is the pre-marriage space, $t+i$ is the post-marriage space. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: Marriage penalty: event-study estimates for female no child sample

Dependent variable	Working			
Sample	BGD	IND	MDV	NPL
	(1)	(2)	(3)	(4)
Year $t-5$	-0.022	-0.033***	-0.058	-0.071***
	(0.019)	(0.009)	(0.060)	(0.022)
Year $t-4$	-0.020	-0.027***	-0.034	-0.070***
	(0.017)	(0.007)	(0.050)	(0.020)
Year $t-3$	-0.013	-0.019***	-0.067	-0.032*
	(0.014)	(0.005)	(0.049)	(0.017)
Year $t-2$	-0.019	-0.009**	-0.082	-0.032**
	(0.012)	(0.004)	(0.054)	(0.014)
Year $t=0$	-0.131***	-0.152***	-0.081	-0.103***
	(0.019)	(0.011)	(0.060)	(0.027)
Year $t+1$	-0.087***	-0.134***	-0.173***	-0.052
	(0.028)	(0.011)	(0.061)	(0.032)
Year $t+2$	-0.075*	-0.126***	-0.044	-0.038
	(0.040)	(0.014)	(0.069)	(0.045)
Year $t+3$	-0.023	-0.099***	-0.061	0.032
	(0.051)	(0.019)	(0.078)	(0.046)
Year $t+4$	-0.139***	-0.119***	-0.190**	0.013
	(0.046)	(0.024)	(0.089)	(0.068)
Year $t+5$	0.048	-0.138***	-0.366***	0.020
	(0.087)	(0.028)	(0.078)	(0.071)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	1887	5972	439	2006
R^2	0.191	0.146	0.334	0.246

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. BGD - Bangladesh, IND - India, MDV - Maldives, NPL - Nepal. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. $t=0$ is the time of marriage, $t-i$ is the pre-marriage space, $t+i$ is the post-marriage space. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A10: Marriage penalties for women by housing bargaining and education

Dependent variable	Working			
Sample	Child		No child	
	(1)	(2)	(3)	(4)
Post-marriage	-0.181***	-0.155***	-0.162***	-0.137***
	(0.009)	(0.009)	(0.012)	(0.011)
Post-marriage × Higher education	0.055***	0.054***	0.062***	0.066***
	(0.010)	(0.011)	(0.019)	(0.019)
Post-marriage × Decisionmaking	0.024***		0.037***	
	(0.006)		(0.010)	
Post-marriage × Attitude towards violence		-0.040***		-0.017
		(0.016)		(0.019)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	10896	10896	8818	8818
R^2	0.337	0.336	0.234	0.232

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Decisionmaking is measured as the sum of indicator variables for 5 DHS questions on whether the respondent is involved household decisions, averaged by cohort, with larger values indicating greater involvement. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A11: Marriage penalties: urban and rural

Dependent variable	Working			
Sample	Male		Female	
	(1)	(2)	(3)	(4)
Panel A: Urban				
Post-marriage	0.286***		-0.154***	
	(0.024)		(0.008)	
Post-marriage × No child sample		0.162***		-0.132***
		(0.016)		(0.013)
Post-marriage × Ignore child sample		0.330***		-0.171***
		(0.028)		(0.010)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	6381	6381	10760	10760
R^2	0.654	0.665	0.254	0.268
Panel B: Rural				
Post-marriage	0.204***		-0.117***	
	(0.016)		(0.006)	
Post-marriage × No child sample		0.112***		-0.106***
		(0.011)		(0.009)
Post-marriage × Ignore child sample		0.231***		-0.127***
		(0.019)		(0.007)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	9033	9033	12687	12687
R^2	0.651	0.657	0.325	0.337

Note: Standard errors in parentheses clustered at the cohort level. Sample is all female cohorts. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A12: Marriage penalties: robustness to cell size weights

Dependent variable	Working			
Sample	Male		Female	
	(1)	(2)	(3)	(4)
Panel A: Cell size weights				
Post-marriage	0.234***		-0.149***	
	(0.012)		(0.005)	
Post-marriage × No child sample		0.140***		-0.113***
		(0.008)		(0.007)
Post-marriage × Ignore child sample		0.256***		-0.161***
		(0.013)		(0.006)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	15827	15827	23441	23441
R^2	0.608	0.612	0.576	0.577
Panel A: Unweighted				
Post-marriage	0.130***		-0.134***	
	(0.005)		(0.005)	
Post-marriage × No child sample		0.124***		-0.111***
		(0.007)		(0.007)
Post-marriage × Ignore child sample		0.134***		-0.149***
		(0.006)		(0.006)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	16163	16163	23465	23465
R^2	0.358	0.358	0.270	0.275

Note: Standard errors in parentheses clustered at the cohort level. Weighting specification is given in panel header. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13: Marriage penalties and premiums: balanced panel

Dependent variable	Working			
Sample	Male		Female	
	(1)	(2)	(3)	(4)
Post-marriage	0.248***		-0.130***	
	(0.015)		(0.006)	
Post-marriage × No child sample		0.137***		-0.112***
		(0.017)		(0.010)
Post-marriage × Ignore child sample		0.264***		-0.144***
		(0.017)		(0.007)
Cohort FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Country × Year FE	Yes	Yes	Yes	Yes
Observations	6171	6171	8763	8763
R^2	0.694	0.698	0.185	0.200

Note: Standard errors in parentheses clustered at the cohort level. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A14: Post-marriage balance

Dependent variable	Muslim	Hindu	Decision	Violence	STSC	Spouse Ed
	(1)	(2)	(3)	(4)	(5)	(6)
Year $t+1$	-0.013**	0.028***	0.001	-0.008	-0.001	-0.005
	(0.006)	(0.007)	(0.020)	(0.011)	(0.007)	(0.011)
Year $t+2$	-0.016***	0.025***	0.021	-0.014	-0.009	-0.003
	(0.005)	(0.007)	(0.019)	(0.012)	(0.007)	(0.012)
Year $t+3$	-0.016***	0.027***	0.055**	-0.016	-0.006	-0.015
	(0.006)	(0.007)	(0.021)	(0.014)	(0.008)	(0.012)
Year $t+4$	-0.017***	0.028***	0.052**	-0.016	-0.021***	-0.023*
	(0.006)	(0.007)	(0.021)	(0.013)	(0.007)	(0.012)
Year $t+5$	-0.034***	0.047***	0.095***	-0.010	-0.011	-0.035***
	(0.006)	(0.007)	(0.022)	(0.014)	(0.008)	(0.012)
Year $t+1$ × No child sample	-0.004	0.014	-0.021	-0.007	0.001	0.018
	(0.008)	(0.011)	(0.028)	(0.016)	(0.010)	(0.017)
Year $t+2$ × No child sample	-0.014	0.020*	0.015	0.006	-0.005	0.039*
	(0.009)	(0.011)	(0.034)	(0.020)	(0.012)	(0.023)
Year $t+3$ × No child sample	-0.017	0.011	0.033	-0.024	-0.003	0.056**
	(0.010)	(0.013)	(0.044)	(0.023)	(0.016)	(0.025)
Year $t+4$ × No child sample	-0.015	0.002	0.121**	0.046	0.006	0.027
	(0.013)	(0.017)	(0.050)	(0.029)	(0.018)	(0.029)
Year $t+5$ × No child sample	-0.004	0.010	0.173**	0.090**	0.019	-0.013
	(0.014)	(0.018)	(0.068)	(0.045)	(0.021)	(0.030)
Observations	11614	11614	8588	8588	4490	12273
R^2	0.004	0.004	0.004	0.001	0.003	0.004

Note: Standard errors in parentheses clustered at the cohort level. Muslim, Hindu, and STSC are the share of women in each post-marriage year who are Muslim, Hindu, and Scheduled Tribes and Castes, respectively. Spouse Ed is the share of women married to husbands with post-secondary education. Decision and violence are the average decisionmaking and domestic violence indices. All estimates weighted by cohort size. No child sample refers matching post-marriage observations in the sample without children. Ignore child sample refers matching post-marriage observations in the full sample. Cohort FE refer to birth cohort. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.