Sheikh Arsalan-ul-Haque
Ph.D. Candidate in Economics, University of Delaware
644 Lehigh Rd, Newark, DE 19711
arsalan@udel.edu — +1 302-444-1488 — Website

September 14, 2026

Professors Emily Breza, Arun Chandrasekhar, and Davide Viviano
Workshop on Development Economics and Networks for Early-Career Researchers
National Bureau of Economic Research

Dear Professors Breza, Chandrasekhar, and Viviano,

I am applying for the NBER Workshop on Development Economics and Networks for Early-Career Researchers. I am a Ph.D. candidate in Economics at the University of Delaware, and I would like to present my paper, "Unilateral Divorce and the Marriage Market: Evidence from India's Triple Talaq Ban."

This paper began as my third-year paper, and I am now developing it into my job market paper. It has benefited from comments from my readers and my dissertation proposal defense, and I am continuing to work through those comments. Presenting it at the workshop would be a useful next step. I would appreciate feedback from researchers who are less familiar with the project, both on the analysis and on how clearly I communicate what the paper contributes.

I am also interested in the opportunity to attend the lectures and discuss the other participants' research. My training so far has involved learning through a combination of coursework, my own projects, and collaboration with other researchers. I would welcome the chance to build on that experience in a small workshop where I can participate in the discussions and get to know people working on related questions.

I have included my CV, with a list of my Ph.D. courses, a statement of my research interests and motivation for attending, and my working paper. Professor Adrienne Lucas at the University of Delaware could provide a recommendation if I am shortlisted and can be contacted at alucas@udel.edu.

Thank you for considering my application.

Sincerely,

Sheikh Arsalan-ul-Haque

Sheikh Arsalan-ul-Haque

arsalan@udel.edu | Website | LinkedIn

Education

University of Delaware Newark, DE
Ph.D. Candidate in Economics *Expected May 2028*
Advisor: Adrienne Lucas. **Committee:** Sabrin Beg and Maria Rosales-Rueda. **External member:** Kate Vyborny.

University of Delaware Newark, DE
M.S. in Economics and Applied Econometrics *June 2025*

Lahore University of Management Sciences Lahore, Pakistan
B.Sc. (Honors) in Economics and Mathematics *June 2021*
GPA: 3.75/4.00; graduated with distinction.

Research Interests

Development Economics; Gender and Family Economics; Health Economics; Labor Economics.

Research in Progress

- **When Divorce Becomes Harder: Marriage Markets and Women's Outcomes** *(with Rounak Bose)*
  Examines how restrictions on divorce reshape marriage markets and women's welfare.
- **Intended and Unintended Consequences of a De Facto Inheritance Reform** *(with Sabrin Beg, Erica Field, Kate Vyborny, Suzanna Khalifa, and Jeremy Lebow)*
  Studies the effects of Punjab's land-record digitization on women's inheritance rights, property access, and household outcomes.
- **Effects of a De Facto Inheritance Reform on Child Health and Son Preference**
  Studies how Punjab's land-record digitization, which strengthened enforcement of daughters' inheritance rights, affects infant mortality, breastfeeding, and vaccination.
- **Marriage Bars, Women's Employment, and Intergenerational Outcomes** *(with Barbara Biasi and Valerie Michelman)*
  Studies how U.S. restrictions on the employment of married women shaped labor-force participation and longer-run outcomes for their children.

Publication

Beg, Sabrin, and Sheikh Arsalan-ul-Haque. 2024. "Policy Recommendations for Advancing the Education Sector in Pakistan." In *Striving to Meet the Sustainable Development Goals: Next Steps for Policymakers and Practitioners*. Asian Development Bank Institute. doi:10.56506/IEJD6853.

Research Experience

University of Delaware Newark, DE
Graduate Research Assistant *Sep 2023–Present*

- Joined as a research assistant and became a coauthor on an evaluation of Punjab's land-record digitization and its effects on women's inheritance and property rights; construct and analyze primary and secondary data using staggered difference-in-differences.
- Analyze baseline and endline data for a Randomized Control Trial intervention; cleaned and analyzed data and estimate intention-to-treat and treatment-on-the-treated effects.

Research Experience (continued)

The World Bank Remote
Short-Term Consultant *Jul 2024–Aug 2025*

- Conducted data cleaning, monitoring analysis, and high-frequency checks for the land-record digitization and inheritance-rights project.
- Contributed to proposal development, background research, and survey planning with investigators and field teams in Pakistan.

Center for Economic Research in Pakistan (CERP) Lahore, Pakistan
Research Associate *Sep 2022–Aug 2023*

- Programmed SurveyCTO instruments and coordinated survey design, field preparation, and data management for the Punjab land-digitization evaluation.
- Coordinated with research partners, implementation teams, and government stakeholders.

Lahore University of Management Sciences Lahore, Pakistan
Pre-Doctoral Research Associate *Aug 2021–Mar 2023*

- Supported field experiments on misinformation and digital literacy.
- Contributed to intervention design, survey development, and pre-analysis planning; analyzed project data and produced spatial maps in ArcGIS.

Teaching Experience

University of Delaware Newark, DE
Instructor of Record, Introduction to Macroeconomics (ECON 103) *Winter 2026*

Lahore University of Management Sciences Lahore, Pakistan
Adjunct Faculty, Development Economics (ECON 240) *Summer 2025*

Teaching Assistant, Lahore University of Management Sciences: Econometrics, Intermediate Microeconomics, Calculus II, and Introduction to Game Theory.

Technical Skills

Stata, R, Python, ArcGIS, SurveyCTO, and LaTeX.

Leadership and Service

Kaavish Lahore, Pakistan
Co-founder *Jul 2016–Present*

Founded and scaled a community-relief organization that has delivered more than 10,000 meals in underserved urban communities; led fundraising and vendor outreach.

Ph.D. Coursework

University of Delaware

Course	Title	Term
ECON 811	Microeconomic Theory I	Fall 2023
ECON 812	Macroeconomic Theory I	Fall 2023
ECON 822	Econometric Theory I	Fall 2023
ECON 813	Microeconomic Theory II	Spring 2024
ECON 814	Macroeconomic Theory II	Spring 2024
ECON 823	Econometric Theory II	Spring 2024
ECON 824	Cross-Section and Panel Data	Fall 2024
ECON 846	Topics in Development Economics	Fall 2024
ECON 881	Topics in Labor Economics	Fall 2024
ECON 825	Time-Series Econometrics	Spring 2025
ECON 871	Household Finance: Empirics	Spring 2025
ECON 880	Labor Economics	Spring 2025
ECON 870	Household Finance: Theory	Fall 2025
FSAN 817	Large Scale Machine Learning	Fall 2025

Research Interests and Motivation for Attending the Workshop

My research interests have developed around the role of family institutions in shaping women's lives. I am interested in marriage and divorce, women's work, and the decisions families make about their children. Across these topics, I keep coming back to the relationship between legal rights and the circumstances in which people can exercise them. A change in the law can alter the choices available to women, but how they respond may also depend on their families, the expectations of their communities, and the support they can draw on. Understanding how these factors fit together is what draws me to development economics and motivates my interest in this workshop.

My main dissertation project examines India's triple talaq ban & criminlization (an instantaneous form of unilateral divorce exclusively avalaible to Muslim husbands) and its implications for marriage formation and outcomes within marriage. I study whether restricting husbands' access to unilateral instant divorce changes women's experiences of marriage, including marital stability, intimate partner violence, and participation in household decisions. As I have worked on the paper, I have become especially interested in an earlier decision: whether and whom to marry when the terms of marital exit change. This has become central to how I think about the project. To understand differences in outcomes after the reform, I also need to understand whether the women entering marriage, and the matches they form, have changed.

The paper is still very much a work in progress. One of the most useful parts of developing it has been having to reconsider what I can learn from the patterns in the data and where several explanations remain possible. I am currently working through these questions, particularly the distinction between changes within marriages and changes in who enters marriage. My other work, on son preference and postnatal outcomes and on restrictions on married women's employment, reflects a related interest in how family institutions affect women and children. Moving between these projects has also made me more aware of how much household decisions depend on relationships beyond the household itself.

I would like to attend the workshop to develop a better understanding of empirical network research and learn from researchers who make these relationships central to their work. In my own research, family and community relationships often enter the explanation for why people might respond differently to the same institutional change. I want to think more carefully about what those explanations imply, what evidence would support them, and how other researchers approach these questions. The opportunity to learn through both lectures and discussion of ongoing work is particularly appealing while my dissertation is still taking shape.

The small size of the workshop is also an important part of why I want to attend. I would value the chance to discuss the parts of my research that I am still working through, hear how others have dealt with similar difficulties, and spend enough time with their work to contribute thoughtful feedback. I would bring questions grounded in my work on family law and household behavior, as well as a willingness to have my own interpretations challenged. I hope to leave with a clearer sense of my research and with a group of peers whose work I can continue to learn from as we develop our dissertations and early research agendas.

Unilateral Divorce and the Marriage Market: Evidence from India's Triple Talaq Ban

Sheikh Arsalan-ul-Haque* Rounak Bose†

PRELIMINARY WORK - DO NOT CITE

Abstract

Divorce reform can affect not only whether marriages dissolve, but also which existing marriages persist, who enters marriage, and the outcomes of marriages formed under the new law. We study India's ban and September 2018 criminalization of triple talaq, an instantaneous form of unilateral divorce historically available to Muslim husbands. Using nationally representative surveys and retrospective marriage histories, we compare changes among Muslim women with those among non-Muslim women in a difference-in-differences framework. Among marriages formed before the reform, formal divorce declines, and the remaining married population shifts toward women who married younger, have husbands at least five years older, and report less say over their husbands' earnings. These patterns are consistent with some marriages remaining intact that would otherwise have dissolved. First-marriage entry slows among women without primary completion, while the average estimate is imprecise. Our framework suggests that this decline could reflect greater reluctance among prospective husbands or fewer potential matches. Among marriages formed after the reform, divorce and emotional intimate partner violence decline without consistent socioeconomic upgrading. This pattern is consistent with a favorable behavioral response within marriage.

JEL Classification: J12, K36, J16, O12

*Department of Economics, University of Delaware
†Department of History, University of Delaware

1 Introduction

Marriage is a central economic and social institution. It shapes labor supply, savings, fertility, and the distribution of resources within the household (Becker, 1981; Greenwood et al., 2017). The rules governing marital exit can therefore shape economic decisions and relationships within marriage. Much of the literature studies reforms that expand access to divorce, with differing consequences for women.

This paper studies India's ban and September 2018 criminalization of *triple talaq*, an instantaneous and irrevocable form of unilateral divorce historically available to Muslim husbands. The reform invalidated this instantaneous route and made its pronouncement a criminal offense, without broadly rewriting Muslim family law.

The implications of divorce reform depend on whose choice changes and whether the woman wants the marriage to continue. Expanding women's access to divorce gives them greater ability to leave an unwanted marriage; reversing that change would restrict their own ability to leave. Restricting husbands' instantaneous unilateral exit instead limits their ability to end a marriage regardless of their wives' wishes, without directly changing wives' access to divorce. The two reforms therefore address different sources of vulnerability: being unable to leave an unwanted marriage and being exposed to an unwanted marital dissolution. How restricting this exit option affects women's outcomes remains an empirical question. We study both existing and newly formed marriages because these outcomes can change within a given marriage and through changes in which marriages persist or form.

The reform applied specifically to Muslim marriages, allowing us to compare changes among Muslim women with those among non-Muslim women in a difference-in-differences framework. We first examine marriages formed before the reform, asking whether divorce and the composition of the remaining married population changed differently across the two groups. We then examine whether entry into first marriage changed after the reform. Finally, we compare outcomes and characteristics of marriages formed before and after the reform among Muslims, relative to the corresponding differences among non-Muslims. To-

gether, these comparisons capture adjustment through marital exit, marriage entry, and the marriages formed under the new regime.

We implement these comparisons using rounds 4 (2015–16) and 5 (2019–21) of the National Family Health Survey (NFHS). We use the timing of first cohabitation to measure marriage formation. Following Corno et al. (2020), we reconstruct monthly entry into first marriage from retrospective histories in NFHS-5, including women who remain unmarried. The surveys also provide information on marital status, intimate partner violence, women's participation in household decisions, and the characteristics of spouses and households.

The pre-reform analysis is central to interpreting the new-marriage results. The comparison group in the new-marriage design consists of marriages formed before September 2018 but observed after the reform in NFHS-5. The pre-reform analysis shows what happened to this group, including changes in outcomes and in which marriages remained intact. The new-marriage estimates then show how marriages formed under the new legal regime differ from these earlier marriage cohorts. The marriage-hazard analysis completes the picture by asking whether the reform changed entry into first marriage.

Our results point to changes in both existing marriages and entry into marriage. Among marriages that predate the reform, formal divorce declines by 0.08 percentage points, about 21 percent of the non-Muslim pre-reform mean. Among marriages that survive, the Muslim married stock shifts toward women who married younger, have husbands at least five years older, and report less say over their husbands' earnings. Together, the decline in divorce and these compositional shifts are consistent with the reform keeping intact some pre-reform marriages that might otherwise have dissolved.

The reform appears to have slowed entry into first marriage particularly among women who did not complete primary school. Among women ages 15–19, the estimated change in the monthly probability of entering first marriage among Muslims, relative to non-Muslims, is -0.057 percentage points, equivalent to 5.3 percent of the pre-reform non-Muslim rate. This average estimate is statistically insignificant. The decline is more pronounced among

women who did not complete primary school: 0.263 percentage points, or 13.7 percent of the corresponding pre-reform non-Muslim rate. Against a benchmark of approximately 19 first marriages per 1,000 unmarried women each month, this estimate represents a differential decline of about 2.6 marriages per 1,000. For context, Corno, Hildebrandt, and Voena (2020) find that droughts reduce the annual hazard of child marriage in India by approximately 4 percent, while McGavock (2021) finds that a minimum-marriage-age reform in Ethiopia reduced marriage before age 16 by approximately 17 percent in areas where early marriage was more common. Our estimates suggest a substantial slowing of marriage entry among women without primary completion, although the average response across all women in this age range remains imprecisely estimated.

Among marriages that formed after September 2018, Muslim women are 0.4 percentage points less likely to be divorced and 4.9 percentage points less likely to report emotional intimate partner violence. The emotional-IPV estimate is sizable and statistically significant under conventional state-clustered inference, although it is less precisely estimated under wild-cluster bootstrap inference (Appendix Table D1). These improvements occur without consistent socioeconomic upgrading: primary completion rises slightly, though imprecisely estimated, while secondary completion and husbands' high-skilled employment decline. Favorable selection on these observed characteristics therefore cannot explain the improvements, and the pattern supports a direct commitment or behavioral interpretation. The increase in primary completion is consistent in direction with lower entry among women without primary schooling, although the entry analysis follows women at risk of first marriage and the composition analysis examines marriages that formed.

Taken together, the three analyses show how restricting husbands' exit can affect women in marriages that already exist and those that form after the reform. The conceptual framework (Section 2.2) helps connect these responses. For existing marriages, making exit harder can change behavior within marriage as well as which marriages remain intact. For prospective couples, the same restriction changes the terms on which both partners are willing to

marry. In the framework, women value protection against instantaneous abandonment, while husbands may be less willing to enter a marriage that is harder to leave. Marriage entry can therefore change through prospective spouses' participation decisions or through changes in available matches, including the possibility that fewer divorced men are available as potential husbands. Outcomes among marriages formed under the reform then reflect both which couples marry and what happens within their marriages. Together, these responses explain why a decline in divorce alone does not establish how women's circumstances have changed.

This paper contributes to research on divorce rights, women's outcomes, and marriage markets by examining the criminalization of *triple talaq* as a form of protection against husbands' instantaneous unilateral exit. Previous studies examine how expanded access to divorce affects violence and bargaining (Stevenson and Wolfers, 2006; Brassiolo, 2016; García-Ramos, 2021; Corradini and Buccione, 2023), as well as household saving and marriage-specific investment (Stevenson, 2007; González and Özcan, 2013; Voena, 2015). Related work examines how husbands' divorce costs shape mehr and dowry (Ambrus et al., 2010). We study a restriction on a husband's ability to end a marriage his wife may wish to continue, without directly changing her own access to divorce. The distinction matters because protecting a woman against unwanted dissolution is different from enabling her to leave an unwanted marriage. The consequences of this restriction therefore cannot be inferred simply by reversing the effects of divorce liberalization. We provide evidence on the consequences of this protection for marital survival, first-marriage entry, and women's outcomes.

Our analysis also builds on research linking divorce law to marriage formation and selection. Previous work examines how changes in marital rights affect existing couples and the formation and composition of new partnerships (Rasul, 2006; Matouschek and Rasul, 2008; Chiappori et al., 2017; Reynoso, 2024). We examine these adjustments together under a restriction on husbands' exit. For existing marriages, we use characteristics fixed when the marriage formed—age at marriage and the initial spousal age gap—to study changes in the composition of marriages that remain intact. For prospective couples, we estimate

first-marriage entry among women still at risk of marrying and examine the characteristics of marriages formed under the new regime. These analyses help interpret changes in women's outcomes alongside changes in who remains married and who enters marriage.

The paper also contributes to research on family law and women's status in South Asia. Ambrus et al. (2010) show how Muslim family-law rules in Bangladesh affect marriage contracts and payments, while Calvi and Keskar (2026) show how Indian antidowry reforms change separation, bargaining, and violence. Legal and descriptive accounts have documented Muslim women's concerns about personal law and the *triple talaq* debate (Narain, 1998; Agnes, 2017; Niaz and Soman, 2015; Wommelsdorff, 2021). We provide nationally representative quasi-experimental evidence on India's post-2018 *triple talaq* reform. Unlike work on dowry law or marriage transfers, we study a husband-specific divorce right and its consequences for both pre-existing marriages and cohorts formed after the reform. The evidence shows why a decline in formal divorce alone is not a sufficient measure of women's welfare in a plural legal setting: the same legal change can alter marital survival and the composition of marriages that form.

2 Background

2.1 Triple Talaq and the 2019 Reform

Before 2019, Muslim family law in India recognized multiple forms of divorce initiated by the husband. In *Shayara Bano v. Union of India*, the Supreme Court described three forms of *talaq*: *talaq-e-ahsan*, *talaq-e-hasan*, and *talaq-e-biddat*. The first two involved revocable divorce pronounced over time, whereas *talaq-e-biddat*—commonly referred to as *triple talaq*—was understood as an instantaneous and irrevocable form of divorce based on pronouncing *talaq* three times in immediate succession. The judgment characterized *talaq-e-ahsan* as the "most proper" form of divorce and *talaq-e-hasan* as a "proper" form, while noting that *talaq-e-biddat* operated immediately and was considered irregular. (Supreme Court of India,

2017)

The practice of *triple talaq* was controversial because it gave Muslim husbands access to a unilateral and immediate form of marital exit not similarly available to wives. In August 2017, the Supreme Court declared *triple talaq* unconstitutional. That decision and the subsequent legislation were substantively distinct: the Court's ruling invalidated the practice, but it did not itself create a criminal offense or establish a statutory reporting and enforcement process. The policy regime we study began with a sequence of ordinances on September 19, 2018. Parliament later enacted the Muslim Women (Protection of Rights on Marriage) Act on July 31, 2019, and the statute states that it "shall be deemed to have come into force on the 19th day of September, 2018" (Parliament of India, 2019).

For the purposes of this paper, the key economic feature of the ordinances and Act is that they changed one specific margin of marital exit rather than rewriting Muslim family law as a whole. The Act declared pronouncements of *triple talaq* void and illegal, made them a criminal offense punishable by up to three years' imprisonment and a fine, and provided for subsistence allowance and custody-related relief for the wife (Parliament of India, 2019). It also provided that the offense would be cognizable when the police received information from the woman upon whom *talaq* was pronounced or from a person related to her by blood or marriage (Parliament of India, 2019). Thus, reporting did not depend solely on the wife herself approaching the police. By widening the set of potential reporters, the legislation plausibly increased the detectability and enforceability of violations. This criminal and procedural change—rather than the 2017 judicial invalidation alone—is why we study the ordinances and Act. In economic terms, the reform raised the cost of one husband-side exit option and weakened the threat of instantaneous unilateral dissolution.

2.2 Conceptual Framework

The reform can change the value of marriage, whether existing marriages remain observed, and which prospective matches form. We begin with prospective marriages and then apply

the framework to marriages that already existed when the law changed.

Let $r \in \{0, 1\}$ index the legal regime, where $r = 0$ is the pre-reform regime and $r = 1$ is the post-reform regime. Let F_r summarize the time, procedural, financial, and expected legal friction associated with the affected husband-side exit route. The reform raises this friction, so $F_1 > F_0$. Divorce remains available through other procedures, but the affected route is no longer instantaneous and is more costly. Below, F denotes a generic level of this friction.

Let q denote latent match-specific quality. It is fixed for a given potential couple: the reform can change the value and outcomes of their marriage, but not their underlying q. Let z collect observed individual, spouse, match, and local characteristics that can affect marriage values, outside values, or match opportunities.

2.2.1 Prospective Marriages and Entry

For a prospective couple, define the woman's and husband's gross values of marriage as

$$V_w(q, z, F) = u_w(q, z) + J_w(q, F) + B(F),$$
$$V_h(q, z, F) = u_h(q, z) + J_h(q, F) + \Omega(q, F).$$

Here, $u_j(q, z)$ is spouse j's value from the match apart from the reform-dependent terms. The function $J_j(q, F)$ captures the value of commitment, cooperation, and marriage-specific investments. The function $B(F)$ captures the woman's protection from instantaneous abandonment. Finally, $\Omega(q, F)$ is the husband's option value of retaining access to the affected future exit route.

Let $A_j(z)$ be spouse j's value outside the relevant marriage, before any exit friction. For prospective spouses it is the value of not entering; for existing spouses below it is the value available after exit. It may differ across people and matches but is held fixed as F changes.

Net surplus from marriage is

$$\Delta_j(q, z, F) = V_j(q, z, F) - A_j(z), \qquad j \in \{w, h\}.$$

A prospective spouse accepts the match when $\Delta_j(q, z, F) \geq 0$. Thus, V_j first describes the value of marriage, while Δ_j compares that value with the relevant outside value.

Let $p(q, F) \in [0, 1]$ denote the probability that the husband would use the affected exit route in the future. We suppress z from p and Ω for notational simplicity. Because a higher F reduces the value of the option only when it would be used,

$$\Omega_F(q, F) = -p(q, F) \leq 0.$$

Appendix Section G.1 provides the short option-value derivation. We assume that protection raises the woman's value of marriage, $B'(F) > 0$, and that greater commitment weakly raises each spouse's value, $J_{jF} \equiv \partial J_j(q, F)/\partial F \geq 0$. Because the outside values are fixed with respect to F,

$$\frac{\partial \Delta_w(q, z, F)}{\partial F} = J_{wF} + B'(F) > 0,$$
$$\frac{\partial \Delta_h(q, z, F)}{\partial F} = J_{hF} - p(q, F) \gtrless 0.$$

The woman's surplus rises because the reform protects her against instantaneous abandonment and may support greater cooperation. For the husband, the commitment benefit is offset by the loss of future exit flexibility. His surplus falls when $p(q, F) > J_{hF}$ and rises when the reverse is true.

Suppose both Δ_w and Δ_h increase with match quality. Let $q_w(F, z)$ and $q_h(F, z)$ be the match-quality cutoffs at which each partner is just willing to marry. Since $\partial \Delta_j/\partial q > 0$,

$$\frac{\partial q_j(F, z)}{\partial F} = -\frac{\partial \Delta_j(q, z, F)/\partial F}{\partial \Delta_j(q, z, F)/\partial q}.$$

The positive woman-side surplus effect implies $\partial q_w/\partial F < 0$: women are willing to accept

a somewhat lower-quality match. The husband-side cutoff can either rise or fall because $\partial\Delta_h/\partial F$ is ambiguous. A marriage forms only if both partners accept the match, so the relevant joint cutoff is

$$q^*(F,z) = \max\{q_w(F,z), q_h(F,z)\}.$$

At age a, the marriage hazard for women of type z can be written as

$$\lambda_{az}(F) = m_{az}(F)\left[1 - \Phi_{az}\big(q^*(F,z)\big)\right],$$

where $m_{az}(F)$ is the rate at which potential matches arrive and Φ_{az} is the distribution of offered match quality. The term in brackets is the share of offered matches that meet both participation constraints. The hazard rises if matches arrive more often or if the joint cutoff falls, and it falls if match opportunities decline or if the joint cutoff rises. Because the reform can affect each of these objects in either direction, it has no unambiguous effect on entry. Holding match opportunities fixed, however, a decline in the hazard requires the joint cutoff to rise. Because the reform lowers the woman's cutoff, any such increase in the joint cutoff must come from the husband-side cutoff. A decline can instead reflect fewer potential matches, which the empirical analysis cannot separately identify. Appendix Section G.1 derives the option-value, cutoff, and hazard comparative statics.

When a decline is caused by a higher joint cutoff, the matches that still form have higher average q within the affected type z. This implication applies only to a group for which entry declines through the cutoff channel. It does not follow from a decline caused by fewer match opportunities, nor does a higher average q mechanically imply an improvement in any particular observed characteristic in z.

2.2.2 Existing Marriages and the Surviving Stock

Once a marriage exists, however, reaching the outside option requires divorce. The husband's net value from current exit is therefore $A_h(z) - F$. He compares this value with the value of

continuing the marriage, $V_h(q, z, F)$. Define his continuation surplus as

$$\begin{aligned} S(q, z, F) &= V_h(q, z, F) - [A_h(z) - F] \\ &= \Delta_h(q, z, F) + F. \end{aligned}$$

The friction F enters positively because it lowers the value of exiting, making continuation relatively more attractive. The marriage remains observed when $S(q, z, F) \geq 0$. When $S(q, z, F) < 0$, the husband pursues divorce through an available procedure. Thus, the reform delays or discourages affected exits; it does not make divorce impossible.

Using the prospective-marriage value above,

$$\frac{\partial S(q, z, F)}{\partial F} = 1 + J_{hF} - p(q, F) \geq 0,$$

because $J_{hF} \geq 0$ and $p(q, F) \leq 1$. The first term is the direct increase in current exit friction, J_{hF} is the commitment benefit from remaining married, and $-p(q, F)$ is the loss of future exit-option value. Consequently, a higher F weakly raises continuation surplus even though it can reduce one component of the value of marriage. Existing marriages either remain under both regimes, are newly retained for longer under the post-reform regime, or exit even under the higher friction.

Assume that better matches produce greater continuation surplus, so $S_q(q, z, F) > 0$. For a given z, let $q^S(F, z)$ be the match quality at which the husband is indifferent between continuing and exiting:

$$S\big(q^S(F, z), z, F\big) = 0.$$

Marriages with $q \geq q^S(F, z)$ remain observed. The implicit-function theorem gives

$$\frac{\partial q^S(F, z)}{\partial F} = -\frac{S_F(q, z, F)}{S_q(q, z, F)} \leq 0.$$

The reform therefore lowers the minimum match quality required for an existing marriage to

remain observed. The newly retained marriages lie between the post- and pre-reform cutoffs and have lower q than marriages that would have remained observed under the old regime.

Let the average outcome among existing marriages that remain observed be

$$\bar{Y}^S(F) = \mathbb{E}\left[y(q, F, z) \mid S(q, z, F) \geq 0\right].$$

Its change has two components:

$$\frac{d\bar{Y}^S(F)}{dF} = \underbrace{\text{within-marriage effect}}_{\text{behavior changes in a given marriage}} + \underbrace{\text{survivor-selection effect}}_{\text{which existing marriages remain observed}}.$$

For characteristics fixed when the marriage formed, such as age at marriage and the initial spousal age gap, the within-marriage effect is zero. Their observed changes therefore reflect survivor selection. Outcomes that can change after marriage, such as decision-making and intimate partner violence, can reflect both behavior within surviving marriages and changes in which marriages remain observed. The framework does not assign a sign to every observed characteristic; that sign depends on how the characteristic is related to continuation surplus. Appendix Section G.2 gives the exact survivor-selection derivative.

2.2.3 New-Marriage Outcomes

Among marriages that form, the average outcome is

$$\bar{Y}^N(F) = \mathbb{E}\left[y(q, F, z) \mid q \geq q^*(F, z)\right].$$

This expectation is taken over the matches available under F, so the composition of both q and z can differ across regimes. An observed change in this average has two components:

$$\frac{d\bar{Y}^N(F)}{dF} = \underbrace{\text{within-marriage effect}}_{\text{behavior changes in a given marriage}} + \underbrace{\text{entry-selection effect}}_{\text{changing which matches form}}.$$

Once a new marriage forms, it faces the same higher husband-side exit friction as an existing marriage. What distinguishes a new-marriage cohort is that its members also entered under the post-reform participation constraints. Its outcomes therefore combine the within-marriage response to the law with entry selection.

The two components have different implications across outcomes. For a characteristic fixed when a marriage forms, the within-marriage effect is zero. A change in its cohort mean therefore reflects a change in the composition of marriages that form. Outcomes determined after entry can reflect both a within-match response to the reform and entry selection. For divorce, holding the match fixed, the higher exit friction delays or discourages exits that would otherwise have occurred through instantaneous triple talaq; entry selection may reinforce or offset this effect. Intimate partner violence and decision-making can also change through both channels, but the framework does not impose a sign on their within-marriage responses. Fixed-at-entry characteristics can nevertheless help interpret these results. If the post-reform cohort is less advantaged on observed characteristics associated with worse marital outcomes, while marital outcomes improve, observed entry selection works against rather than explains the improvement. This pattern therefore supports a favorable within-marriage response. Appendix Section G.3 provides the exact decomposition.

2.2.4 Empirical Implications

The framework yields four empirical implications:

1. *Marriage entry.* The reform lowers the woman's acceptance cutoff, while the husband's response and the change in match opportunities are ambiguous. The marriage hazard therefore has no predetermined sign. Holding match opportunities fixed, a cutoff-driven decline for a particular group must come from the husband side.

2. *Exit among existing marriages.* A higher F weakly raises continuation surplus and lowers use of the affected exit route. Divorce should therefore fall among pre-reform marriages to the extent that this route contributes to observed dissolution.

3. *Survivor selection.* The reform lowers the continuation cutoff and retains some lower-q existing marriages for longer. Fixed-at-marriage characteristics change only through survivor selection, while post-marriage outcomes can also reflect behavior within surviving unions.

4. *Outcomes among new marriages.* New-marriage outcomes combine within-match responses with entry selection. Holding the match fixed, the higher exit friction delays or discourages divorces that would otherwise have occurred through instantaneous triple talaq; entry selection may reinforce or offset this effect. The framework does not impose a sign on the within-marriage effects on intimate partner violence or decision-making. Changes in fixed-at-entry characteristics reveal observed selection; improvements that occur despite adverse observed selection support a favorable within-marriage response.

3 Empirical Strategy

We study the effects of the ban and criminalization of *triple talaq* using repeated cross-sections from the National Family Health Survey (NFHS-4, 2015–16, and NFHS-5, 2019–21). The reform can affect the stock of existing marriages, the flow into first marriage, and outcomes among newly formed marriages. We therefore use three related designs. We first study the stock of pre-reform marriages, then estimate a retrospective first-marriage hazard, and finally study outcomes among marriages formed around the reform. The cumulative ever-married analysis is reported in Appendix Table A1.

The NFHS does not report the date of marriage, so we use the timing of first cohabitation. In India, first cohabitation is a close proxy for marriage formation: premarital sex is uncommon and premarital childbearing is very rare (Edmeades et al., 2020).

3.1 Pre-Reform Married Stock

We begin with marriages formed before the reform. This design restricts the sample to women whose first cohabitation began before September 2018 and who are currently married at the time of survey. We then compare the change in the observed stock of pre-reform Muslim marriages between the pre-reform survey round (NFHS-4) and the post-reform survey round (NFHS-5) to the corresponding change among non-Muslims:

$$Y_{istw} = \beta(Muslim_i \times NFHS5_w) + \lambda_r + \lambda_t + \lambda_{s\times w} + \varepsilon_{istw}.$$

Here $NFHS5_w$ indicates observation in the post-reform survey round (NFHS-5). The religion and first-cohabitation-year fixed effects are denoted by λ_r and λ_t. A state fixed effect, λ_s, accounts for time-invariant differences across states, while a survey-round fixed effect, λ_w, accounts for nationwide differences between NFHS-4 and NFHS-5. The equation includes their full interaction, $\lambda_{s\times w}$, which allows changes across the two survey rounds to differ by state. Because this interaction contains both the state and survey-round components, we represent them jointly by $\lambda_{s\times w}$ rather than including λ_s and λ_w separately. For intimate partner violence and decision-making outcomes, we also include years-since-marriage fixed effects. Because the sample is restricted to women who are currently married at survey, these estimates describe how the observed stock of pre-reform marriages changes after the reform, rather than treatment effects on the full set of pre-reform marriages.

3.2 Marriage Entry

Following Corno et al. (2020), we use retrospective first-cohabitation histories to construct a discrete-time first-marriage hazard. Using NFHS-5, we organize the risk set as a person-month panel. A woman contributes one observation for each month beginning with the month of her twelfth birthday and exits in the month of first cohabitation, the month immediately before her thirty-first birthday, or her interview month, whichever occurs first. The outcome

equals one in the month of first cohabitation and zero in all earlier months at risk; women who have not married by the end of their observation window are right-censored.

Let H_{iamsb} denote the first-marriage indicator for woman i, at attained age a, in calendar month m, state s, and birth year b. We estimate

$$H_{iamsb} = \beta(Muslim_i \times Post_m) + \lambda_r + \lambda_{s\times b} + \lambda_a + \lambda_{\text{month}(m)} + \varepsilon_{iamsb},$$

where $Post_m$ equals one from September 2018 onward. State-by-birth-year fixed effects $[\lambda_{s\times b}]$ absorb state and birth-year components while allowing marriage timing to vary across state-specific birth cohorts. Religion $[\lambda_r]$ and attained-age $[\lambda_a]$ fixed effects are included separately. Calendar-month-of-year fixed effects $[\lambda_{\text{month}(m)}]$ account for seasonal patterns in marriage formation. The coefficient β is the Muslim–non-Muslim differential change in the monthly first-marriage hazard after the reform. The specification uses NFHS sampling weights and clusters standard errors by state.

3.3 Outcomes among New Marriages

We next study marriages formed around the reform. The pre-reform married-stock results above matter for interpreting this specification because earlier Muslim marriage cohorts observed in NFHS-5 are not simply old-regime marriages: they were formed before the reform but are observed under the new legal regime, and the currently married sample may include unions that would have dissolved before the ban. Changes in this comparison group can therefore affect the new-marriage-cohort estimates. To alleviate this concern, Appendix Tables A3–A7 re-estimate the new-marriage-cohort specification using only pre-reform marriages that were observed before the reform in NFHS-4 as the pre-period comparison group. With this caveat in mind, our main design compares Muslim women whose first cohabitation began in September 2018 or later to earlier Muslim marriage cohorts, using non-Muslim

marriage cohorts as the comparison group:

$$Y_{istw} = \beta(Muslim_i \times Post_t) + \lambda_r + \lambda_{s\times t} + \lambda_w + \varepsilon_{istw}.$$

Here $Post_t$ equals one if first cohabitation began in September 2018 or later. The specification includes religion fixed effects [λ_r]. State fixed effects [λ_s] account for time-invariant differences across states, while first-cohabitation-year fixed effects [λ_t] account for common differences across marriage cohorts. State-by-first-cohabitation-year fixed effects [$\lambda_{s\times t}$] further allow these cohort differences to vary by state. Because the full set of state-by-first-cohabitation-year fixed effects incorporates both the state and marriage-cohort components, we represent them jointly by $\lambda_{s\times t}$. Survey-wave fixed effects [λ_w] are included separately to account for common differences between NFHS-4 and NFHS-5. The coefficient of interest, β, captures whether Muslim marriage cohorts formed under the post-reform regime differ from the corresponding change among non-Muslims. We use this design for outcomes defined on marriages formed around the reform, including divorce, intimate partner violence, household decision-making, and spouse and match characteristics. For divorce, intimate partner violence, and decision-making outcomes, we additionally include years-since-marriage fixed effects so that exposed Muslim cohorts are compared to non-Muslim cohorts at the same marital duration. Because NFHS-4 does not report year of first cohabitation for divorced women, the divorce regressions are estimated in NFHS-5 only.

Across all designs, regressions use the appropriate NFHS survey weights and standard errors clustered at the state level. The appendix reports wild-cluster bootstrap p-values and event-study or permutation-based exercises, where applicable, for the survey-round and new-marriage-cohort designs.

4 Data

Our main data source is the National Family Health Survey (NFHS), India's Demographic and Health Survey. We use NFHS-4 (2015–16) and NFHS-5 (2019–21), which provide nationally representative information on women's marriage histories, education, fertility, household characteristics, intimate partner violence, and women's decision-making within the household.

The emotional, physical, and sexual IPV outcomes are indicators equal to one if the respondent reports ever having experienced the corresponding form of intimate partner violence.

Our main analyses use three related samples. The pre-reform married-stock analysis restricts the sample to currently married women whose first cohabitation occurred between January 2012 and August 2018. The first-marriage hazard uses all NFHS-5 women with retrospective marriage information, including women who remain unmarried and are therefore right-censored. The new-marriage analysis uses ever-married women for whom the timing of first cohabitation is observed and whose first cohabitation occurred in January 2012 or later, with September 2018 separating the pre- and post-reform cohorts. Throughout, we use the timing of first cohabitation as our measure of marriage formation. Appendix Table A1 reports the cumulative ever-married analysis in the unrestricted full sample.

Marriage timing for formerly married women is unavailable in NFHS-4. The pre-reform divorce analysis therefore does not impose the January 2012 lower bound: it includes all ever-married women in NFHS-4 and ever-married women whose first cohabitation occurred before September 2018 in NFHS-5; formerly married women in NFHS-4 are assigned a common unknown-marriage-year category. The new-marriage divorce analysis, for which marriage-cohort timing is required, is estimated using NFHS-5 only.

Because these outcomes come from different NFHS modules, estimation samples vary across specifications. In Appendix Table F1, we examine whether selection into these modules differs by treatment status and find no evidence that it does.

5 Results

The results are organized around the three empirical exercises in Section 3. We first study marital exit among pre-reform marriages and then the stock of those marriages observed among currently married women. We next estimate the first-marriage hazard directly using retrospective monthly histories, and then turn to outcomes among marriages formed around the reform.

5.1 Pre-Reform Marriages

Among marriages formed before the reform, we ask two questions. First, did formal marital exit change after the reform? Second, did the composition of pre-reform marriages that remained intact change? The first analysis includes all available ever-married women whose marriages predate the reform. The second restricts attention to currently married women whose first cohabitation began between January 2012 and August 2018 and examines characteristics of those marriages observed across NFHS rounds.

Table 1 reports the results for marital exit. Formal divorce among pre-reform Muslim marriages declines by 0.082 percentage points relative to the corresponding change among non-Muslims, approximately 21 percent of the non-Muslim NFHS-4 mean. The estimate for divorce or separation is smaller and statistically insignificant. The evidence therefore suggests that the reform reduced formal divorce, while the estimate for the broader dissolution measure is imprecise. Because NFHS-4 does not report marriage timing for formerly married women, the analysis includes all ever-married NFHS-4 women and assigns those with unavailable marriage years to a common fixed-effect category.

Table 1: Pre-Reform Marriages: Divorce Across Survey Waves

	Divorced	Divorced or Separated
Muslim × NFHS5$_w$	-0.082*	-0.060
	(0.042)	(0.080)
Non-Muslim (NFHS-4), percent	0.389	1.358
Non-Muslim (NFHS-5), percent	0.415	1.470
Muslim (NFHS-4), percent	0.787	1.752
Observations	1,043,170	1,043,170

Notes: The NFHS-4 sample includes all ever-married women, while the NFHS-5 sample includes ever-married women whose first cohabitation began before September 2018. Binary outcomes are multiplied by 100, so coefficients and standard errors are in percentage points. Specifications include religion, marriage-year, state, and state × survey-wave fixed effects. NFHS-4 women with unavailable marriage years share a common unknown-marriage-year fixed-effect category. Regressions use NFHS survey weights, and standard errors are clustered at the state level.

Panel A of Table 2 examines characteristics determined when the marriage formed. Relative to non-Muslims, the pre-reform Muslim marriages observed after the reform shift toward women who married 0.30 years younger. These marriages are also 5.7 percentage points more likely to involve a husband at least five years older, an increase of 13.8 percent relative to the non-Muslim NFHS-4 mean. The estimate for a husband at least ten years older is positive but less precise. Because age at marriage and the initial spousal age gap cannot change within an existing marriage, these estimates show that the composition of the surviving stock changed: relative to non-Muslims, the Muslim marriages still intact and observed after the reform increasingly consist of marriages formed at younger ages and with larger spousal age gaps. Together with the decline in divorce, the pattern is consistent with the reform keeping intact some marriages that would otherwise have dissolved. These estimates remain visible under conservative wild-cluster bootstrap inference (Appendix Table D2).

Table 2: Pre-Reform Marriages Observed after the Reform

Panel A. Characteristics Fixed at Marriage Formation

	(1) Age at First Cohabitation	(2) Age Gap $\geq$ 5 Years	(3) Age Gap $\geq$ 10 Years
Muslim $\times$ NFHS5$_w$	-0.300***	0.057***	0.019*
	(0.096)	(0.016)	(0.011)
Non-Muslim (NFHS-4)	20.387	0.414	0.087
Non-Muslim (NFHS-5)	20.221	0.429	0.099
Muslim (NFHS-4)	20.130	0.420	0.069
Observations	202,666	32,509	32,509

Panel B. Decision-Making in Continuing Marriages

	(1) Healthcare	(2) Large Purchases	(3) Relatives	(4) Husband's Money	(5) Any Decision
Muslim $\times$ NFHS5$_w$	-0.019	-0.024	-0.032	-0.052***	-0.003
	(0.018)	(0.018)	(0.022)	(0.017)	(0.015)
Non-Muslim (NFHS-4)	0.694	0.653	0.683	0.641	0.813
Non-Muslim (NFHS-5)	0.790	0.762	0.786	0.754	0.884
Muslim (NFHS-4)	0.687	0.643	0.672	0.619	0.789
Observations	31,441	31,441	31,441	31,441	31,441

Notes: Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Both panels restrict the sample to currently married women whose first cohabitation began before September 2018. Panel A reports characteristics fixed when the marriage formed; Panel B reports current household decision-making outcomes. Each column reports the coefficient on $Muslim_i \times Post_w$, where $Post_w$ indicates observation in NFHS-5. Outcomes are reported in their original units; the text expresses binary-outcome coefficients in percentage points where stated. Panel A specifications include religion, marriage-year cohort, state, and state $\times$ survey-wave fixed effects. Panel B specifications additionally include years-since-marriage fixed effects. Regressions use the appropriate NFHS survey weights, and standard errors are clustered at the state level.

Women in pre-reform Muslim marriages observed after the reform are 5.2 percentage points less likely to report having a say over the use of their husbands' earnings, an 8.1 percent decline relative to the non-Muslim NFHS-4 mean. The remaining decision-making estimates are generally negative but imprecise, while the indicator for participation in any decision is close to zero. Unlike age at marriage and the initial age gap, decision-making can change after a marriage forms. The decline may therefore reflect changes within continuing marriages, selective retention of marriages in which women have less say, or both.

The surviving pre-reform Muslim married stock also shifts toward women who are 5.3 percentage points more likely not to be working. Secondary completion is 1.4 percent-

age points lower in the baseline specification and remains negative across the alternative specifications, although the estimate attenuates and becomes imprecise. Together with the lower age at marriage and having older husbands, these patterns describe a surviving stock with weaker labor-market attachment and suggestively lower educational attainment. The intimate-partner-violence estimates are small, statistically insignificant, and mixed in sign. Appendix Tables B7–B10 report the complete results under the baseline and alternative fixed-effect specifications.

5.2 Marriage Entry

The retrospective hazard estimates measure the monthly probability that an at-risk woman enters her first marriage. Table 3 reports the average Muslim–non-Muslim differential change after September 2018. All coefficients, standard errors, and means in the hazard tables are multiplied by 100 and are therefore expressed in percentage points. The estimate for ages 12–30 is negative, at -0.043 percentage points (standard error 0.029), but imprecise. The age-specific estimates are similarly imprecise.

Table 3: Monthly First-Marriage Hazard around the Triple Talaq Reform

	Ages 12–30	Ages 12–17	Ages 15–19	Ages 20–24	Ages 25–30
Muslim × Post	-0.043	-0.018	-0.057	-0.011	0.044
	(0.029)	(0.023)	(0.051)	(0.073)	(0.154)
Non-Muslim pre-reform mean	0.859	0.506	1.081	1.876	2.003
Non-Muslim post-reform mean	0.737	0.188	0.496	1.325	1.759
Muslim pre-reform mean	0.859	0.529	1.126	1.884	1.785
Person-month observations	65,669,063	45,385,706	28,848,730	9,334,995	2,370,078

Notes: The dependent variable equals one in the month of first cohabitation and zero in prior months at risk. Each column is a weighted linear probability model using NFHS-5 women and the specification described in Section 3. Post begins in September 2018. Standard errors, clustered by state, are in parentheses. Coefficients, standard errors, and reported means are multiplied by 100 and are therefore expressed in percentage points.

Our interviews with civil-society leaders from two prominent women's-rights organizations, Bharatiya Muslim Mahila Andolan and the Bebaak Collective, motivate the primary-

completion heterogeneity in Table 4. Both organizations work closely with Muslim women and provided testimony in the Supreme Court proceedings concerning this issue. Drawing on their casework, the leaders we interviewed described women who sought assistance after their husbands used this practice as often coming from economically and educationally disadvantaged households.

We use primary completion because it is generally determined by these ages and changes little over this age range. We treat ages 15–19, when women are at meaningful risk of entering first marriage, as the focal specification.

Table 4: Monthly First-Marriage Hazard by Primary Completion

	Ages 12–30	Ages 15–19	Ages 20–24
Post	-0.008	0.033	0.445*
	(0.118)	(0.170)	(0.236)
Muslim × Post	-0.347***	-0.263***	-0.575***
	(0.096)	(0.096)	(0.208)
Primary completed	-0.577***	-0.891***	-0.728***
	(0.027)	(0.041)	(0.079)
Muslim × Primary completed	0.203***	0.313***	0.438***
	(0.044)	(0.067)	(0.125)
Post × Primary completed	-0.214*	-0.014	-0.509**
	(0.109)	(0.127)	(0.227)
Muslim × Post × Primary completed	0.295***	0.186*	0.538***
	(0.097)	(0.103)	(0.186)
Non-Muslim, no-primary pre-reform mean	1.323	1.917	2.633
Non-Muslim, no-primary post-reform mean	1.483	1.378	2.576
Muslim, no-primary pre-reform mean	1.179	1.651	2.211
Person-month observations	65,655,171	28,842,699	9,333,144

Notes: Each column estimates the fully interacted Muslim, post-reform, and primary-completion specification described in Section 3. The Muslim × Post coefficient is the Muslim–non-Muslim post-reform differential among women without primary completion. For primary-completed women, the corresponding differential is the sum of that coefficient and the triple interaction; the triple interaction tests equality between the two differentials. The reported means correspond to the no-primary-completion reference group. The dependent variable equals one in the month of first cohabitation and zero in prior months at risk. Post begins in September 2018. Standard errors, clustered by state, are in parentheses. Coefficients, standard errors, and reported means are multiplied by 100 and are therefore expressed in percentage points. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Among women ages 15–19 who did not complete primary school, the Muslim–non-Muslim

differential change in the monthly first-marriage hazard is −0.263 percentage points, or 13.7 percent of the pre-reform non-Muslim mean for this group. This estimate compares a 0.230-percentage-point decline among Muslim women without primary completion ($p = 0.049$) with a 0.033-percentage-point increase among non-Muslim women without primary completion ($p = 0.849$).

Among women ages 15–19 who completed primary school, the Muslim hazard declines by 0.058 percentage points ($p = 0.283$), while the non-Muslim hazard increases by 0.018 percentage points ($p = 0.726$). These changes produce a much smaller Muslim–non-Muslim differential of −0.077 percentage points for primary-completed women. The differential is therefore 0.186 percentage points less negative among primary-completed women than among women without primary completion; the test of equality between the two differentials has a p-value of 0.079.

The pooled ages 12–30 specification shows a similar pattern. At ages 20–24, however, the negative differential partly reflects an increase in the non-Muslim hazard, while the Muslim change is negative but imprecise. We therefore view the ages 15–19 result as the clearest evidence that marriage entry slowed among Muslim women without primary completion.

Section 2.2 models marriage as forming only when both prospective spouses accept the match, so the operative threshold is the stricter of their two minimum-quality thresholds. In the model, greater protection against instantaneous unilateral divorce lowers the minimum match quality a woman is willing to accept—a force that, by itself, would increase marriage entry. If the decline among women without primary completion operates through acceptance decisions, holding the number and quality of potential matches fixed, it must therefore reflect prospective husbands requiring a higher-quality match. Alternatively, the hazard could fall because these women receive fewer potential matches. We cannot distinguish between these two channels.

Because the overall hazard estimate is statistically insignificant, this cutoff interpretation is specific to women without primary completion and does not imply that the joint cutoff

rose for women as a whole.

5.3 New Marriages

The third exercise studies outcomes among marriages formed around the reform, conditional on a marriage having formed. Here, $Post_t$ equals one when first cohabitation began in September 2018 or later. The pre-reform-marriage analysis remains relevant because the earlier Muslim cohorts used as the comparison group were formed before the reform but include those that were observed after it. The estimates below compare Muslim marriages formed under the new legal regime with these earlier cohorts, relative to the corresponding cohort comparison among non-Muslims.

5.3.1 Marital Outcomes

Table 5 combines results on marital exit and intimate partner violence. Post-reform Muslim marriage cohorts are 0.4 percentage points less likely to be divorced, an 80 percent decline relative to the non-Muslim pre-reform mean of 0.5 percent. The estimate for divorce or separation is also negative, at 0.3 percentage points, but is less precise.

Panel B reports intimate partner violence. The clearest estimate is for emotional IPV: the coefficient is -4.9 percentage points, a 45.8 percent decline relative to the non-Muslim pre-reform mean of 10.7 percent. The estimates for the women's own attitudes toward wife beating, husbands' controlling behavior, and physical IPV are also negative but imprecise under conventional clustered inference; sexual IPV is positive and imprecisely estimated. The emotional-IPV estimate is less stable under wild-cluster bootstrap inference (Appendix Section D).

Figure 1 reports the event studies for divorce and emotional IPV. The divorce differential is flat and statistically indistinguishable from zero across the pre-reform bins and turns negative for cohorts formed at and after the reform. The break at the reform, rather than a gradual decline as cohorts become more recent, indicates that lower divorce is not simply due

to post-reform marriages having had less time to dissolve; the specification also compares women at the same years since marriage. The emotional-IPV differential also turns negative at and after the reform.

The any-decision indicator—participation in at least one of four household decisions—increases by 4.2 percentage points, or 4.9 percent relative to the non-Muslim pre-reform mean of 85.6 percent. We treat this estimate cautiously: the component margins are imprecise, its pre-reform event-study differential is not flat, and women's say over their husbands' earnings declines among pre-reform marriages observed after the reform, potentially worsening the comparison group. The estimate is similar when the comparison group is restricted to pre-reform marriages observed before the reform (Appendix Table A5); complete estimates and the decision-making event study appear in Appendix Table B3 and Appendix Figure E3.

Table 5: Outcomes among New Marriages: Marital Exit and Intimate Partner Violence

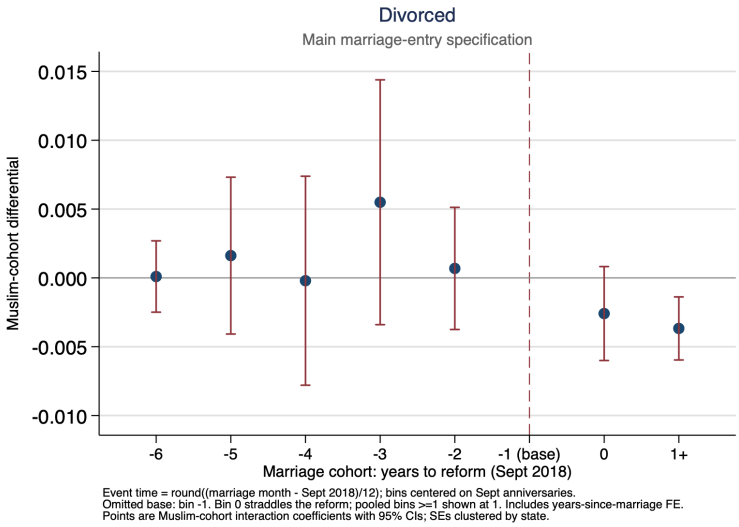
Panel A. Marital Exit

	(1) Divorced	(2) Divorced or Separated
Muslim × Post (Month)	-0.004***	-0.003
	(0.001)	(0.002)
Non-Muslim (Pre)	0.005	0.014
Non-Muslim (Post)	0.002	0.005
Muslim (Pre)	0.009	0.017
Observations	156,651	156,651

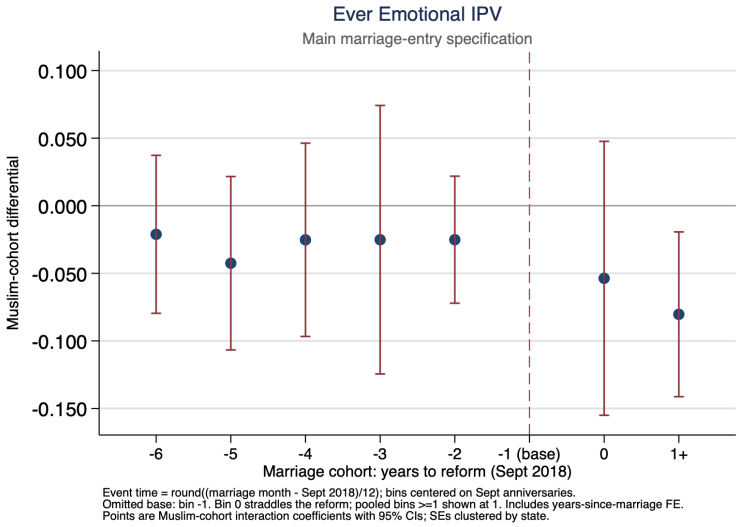
Panel B. Intimate Partner Violence

	(1) Wife Beating Justified	(2) Husband Controlling	(3) Emotional IPV	(4) Physical IPV	(5) Sexual IPV
Muslim × Post (Month)	-0.077	-0.122	-0.049**	-0.016	0.026
	(0.048)	(0.092)	(0.020)	(0.042)	(0.016)
Non-Muslim (Pre)	0.378	0.470	0.107	0.205	0.048
Non-Muslim (Post)	0.348	0.473	0.065	0.114	0.025
Muslim (Pre)	0.412	0.475	0.107	0.206	0.056
Observations	37,068	25,687	25,702	25,702	25,702

Standard errors in parentheses. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Notes: Coefficients follow Section 3. Panel A uses NFHS-5; Panel B uses the relevant NFHS modules and includes years-since-marriage fixed effects. All regressions use first cohabitation, survey weights, and state-clustered standard errors. The module-selection check is in Appendix Table F1.



(a) Divorce



(b) Emotional intimate partner violence

Figure 1: Event Studies for New-Marriage Outcomes

5.3.2 Composition and Match Characteristics

The composition results are central to interpreting the conditional new-marriage outcomes. Table 6 shows that post-reform Muslim marriage cohorts are 2.7 percentage points less likely to have completed secondary school and 3.8 percentage points more likely to belong to a lower-caste or tribe group. Husbands of these women are 6.0 percentage points less likely

to hold high-skilled occupations. Taken together, these estimates provide no evidence of socioeconomic upgrading and, if anything, point toward greater socioeconomic disadvantage. To the extent that these characteristics are associated with worse marital outcomes, observed selection works against the lower divorce and emotional-IPV estimates and the suggestive improvement in decision-making. Favorable selection on these observed characteristics therefore cannot explain the improvements. Instead, the pattern supports an interpretation in which greater commitment or behavioral change improved outcomes within the marriages that formed.

Table 6: Composition of New Marriages

Panel A. Women's Characteristics

	(1) Years of Education	(2) Primary Completed	(3) Secondary Completed	(4) Lower Caste/Tribe
Muslim × Post (Month)	-0.196	0.008	-0.027*	0.038**
	(0.120)	(0.006)	(0.013)	(0.016)
Non-Muslim (Pre)	9.489	0.857	0.391	0.791
Non-Muslim (Post)	10.600	0.918	0.502	0.813
Muslim (Pre)	7.937	0.785	0.254	0.490
Observations	233,004	233,004	227,582	231,910

Panel B. Spouse and Match Characteristics

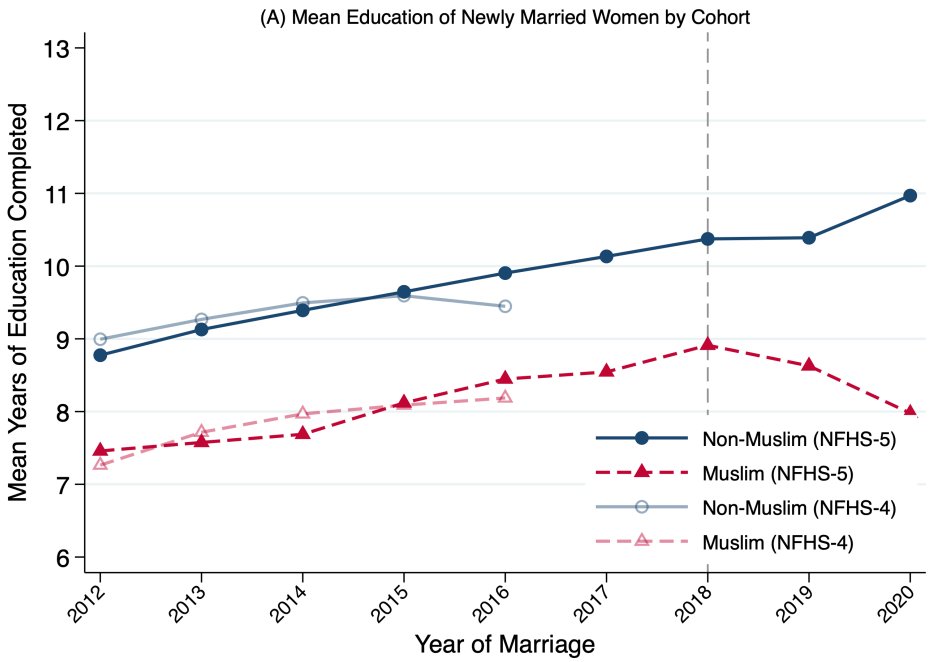
	(1) Not Working	(2) Husband High-Skilled	(3) Spouse: Primary	(4) Spouse: Secondary
Muslim × Post (Month)	-0.023	-0.060**	0.017	-0.074
	(0.019)	(0.029)	(0.033)	(0.052)
Non-Muslim (Pre)	0.843	0.162	0.883	0.396
Non-Muslim (Post)	0.876	0.182	0.925	0.501
Muslim (Pre)	0.917	0.136	0.760	0.237
Observations	37,102	34,641	36,966	36,966

Standard errors in parentheses. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.
Notes: Coefficients are from the specification in Section 3. Panel A reports women's education, caste, and work-related characteristics; Panel B reports spouse and match characteristics. Secondary completion is restricted to women age 18 or older, and the high-skilled occupation measure is defined among husbands who report working. All regressions use first cohabitation, the appropriate survey weights, and state-clustered standard errors.

Because the regression estimate for years of education is less stable, Figure 2 provides complementary descriptive evidence on the changing composition of newly formed cohorts. It plots mean years of education among newly married women by marriage cohort and shows a relative decline for Muslim women around the reform cutoff.

Taken together, the new-marriage results show changes in both outcomes and the ob-

Figure 2: Mean Years of Education among Newly Married Women by Marriage Cohort



Notes: The figure plots average years of education among newly married women by first-cohabitation cohort, separately for Muslim and non-Muslim women. First cohabitation is used as the measure of marriage formation. The vertical line indicates the post-reform cutoff. The figure is descriptive.

servable composition of marriages formed under the new regime.

6 Robustness

This section summarizes a range of robustness checks, all reported in the appendix. We assess whether the results are sensitive to the composition of the pre-period comparison group, alternative fixed effects, placebo reform timing, inference with a limited number of state clusters, pre-trends, and differential selection into survey modules. Taken together, these exercises support the main interpretation, while also indicating that some estimates are less precisely estimated under more conservative inference procedures.

First, for the new-marriage-cohort analysis, we address the concern that earlier Muslim marriage cohorts in the baseline comparison group were formed before the reform but observed after it. Appendix Tables A3–A7 construct a clean comparison group using only pre-reform marriages observed before the reform. Among the outcomes that can be re-estimated in this design, the main patterns are similar to the baseline results: emotional intimate partner violence declines, women's participation in any household decision increases, women are less likely to have completed secondary education, and husbands are less likely to work in high-skilled occupations. The divorce outcome cannot be estimated in this exercise because the pre-reform survey does not report the timing of first cohabitation for divorced women. This comparison therefore cannot validate the divorce estimate directly, but it shows that the main marital-quality and composition findings are not driven by using pre-reform marriages observed after the reform as part of the comparison group.

Second, we examine sensitivity to alternative fixed effects in both the new-marriage and pre-reform marriage analyses. Appendix Tables B1–B5 re-estimate the new-marriage specifications, while Appendix Tables B6–B10 repeat the exercise for the pre-reform marriage analyses. Across the two exercises, we vary marriage-year versus birth-year controls, allow cohort effects to differ by state, and use district fixed effects. The main new-marriage

patterns are broadly similar across specifications: the decline in divorce remains stable, as do the cohort-composition changes in women's and spouses' observable characteristics. In the pre-reform married stock, the lower age at first marriage, higher probability that the husband is at least five years older, and reduced say over husbands' earnings remain visible across the alternative cohort and geographic controls, while the IPV estimates remain small and mixed.

Third, for the main new-marriage-cohort analysis only, we assess whether the results could be driven by spurious reform timing. This exercise is applicable to that design because treatment status is determined by the month of first cohabitation, providing a set of marriage months over which placebo reform timing can be assigned. It is not applied to the pre-reform married-stock analyses, where the post indicator is defined by survey round since only two survey rounds are available. Following Kuka et al. (2020), we randomly assign the observed number of post-reform marriage months as placebo-treated months, drawn without replacement, and then re-estimate the baseline monthly difference-in-differences specification. The remaining marriage months serve as the placebo pre-period. In every iteration, we keep the estimation sample, controls, and fixed effects—including years-since-marriage fixed effects where used in the baseline specification—unchanged. We repeat this procedure 1,000 times and compare the baseline estimate to the resulting distribution of placebo estimates. The randomization-inference p-value is the finite-simulation-corrected share of placebo estimates whose absolute value is at least as large as the baseline estimate. Appendix Figures C1–C5 report this exercise for selected outcomes from the marital-exit, marital-quality, decision-making, education, and spouse and household-composition parts of the main new-marriage-cohort analysis. The decline in divorce, the decline in emotional intimate partner violence, and the increase in women's participation in any household decision lie in the tails of their placebo distributions, as do several of the education and composition estimates.

Fourth, Appendix Section D reports wild-cluster bootstrap p-values clustered at the state level. In the monthly new-marriage specification, the decline in divorce remains strongly

significant, while the increase in women's participation in any decision and the increase in belonging to a lower-caste group remain significant at the 10 percent level. The estimates for emotional intimate partner violence, women's secondary completion, and husbands' high-skilled employment are less precisely estimated. In the pre-reform married stock, the lower age at first marriage, the higher probability that the husband is at least five years older, reduced say over the husband's earnings, the women not-working and women's-education margins remain visible. The spouse-schooling estimates and the IPV block are null.

Finally, Appendix Table F1 examines whether selection into the domestic violence and spouse-characteristics modules differs by treatment status. This is important because several of the outcomes used in the paper are observed only in subsamples. We find no evidence of differential post-reform selection for Muslims into either module. This makes it less likely that the main findings are driven by compositional changes in survey observability rather than by changes in outcomes themselves.

Overall, the appendix results suggest that the core patterns in the paper are not artifacts of a single specification, comparison group, or inference procedure. The strongest findings—reduced divorce and changes in the composition of new and surviving marriages—remain visible across a range of alternative exercises, even though some secondary outcomes are less precisely estimated under more conservative inference. Taken together, these checks reinforce the paper's central interpretation: restricting unilateral divorce changes outcomes among marriages formed under the new regime while also locking relatively weak pre-reform marriages into the surviving married stock.

7 Conclusion

This paper studies how banning and criminalizing husbands' unilateral divorce affected marital survival, entry into first marriage, and outcomes among marriages formed under the new legal regime. Using nationally representative repeated cross-sectional survey data and

retrospective marriage histories, we compare Muslim women with non-Muslim women across three margins: pre-reform marriages, first-marriage entry, and marriages formed after the reform.

Among marriages formed before the reform, formal divorce declines and the surviving Muslim married stock shifts toward women who married younger, have older husbands, and report less say over their husbands' earnings. Together, these findings are consistent with the reform keeping intact some marriages that might otherwise have dissolved.

The average change in first-marriage entry is negative but imprecise, with the decline concentrated among women without primary completion. In the conceptual framework, holding the availability and quality of potential matches fixed, this subgroup decline can only arise from prospective husbands becoming more selective because the reform lowers women's acceptance threshold. It can instead reflect fewer potential matches. Among marriages that form under the new regime, divorce and emotional intimate partner violence decline. The composition estimates show less secondary schooling, greater representation of lower-caste or tribe groups, and fewer husbands in high-skilled occupations. Taken together, these estimates provide no evidence of socioeconomic upgrading and, if anything, point toward greater socioeconomic disadvantage. Favorable selection on these observed characteristics therefore cannot explain the improvements. The pattern instead supports an interpretation based on greater commitment or behavioral change within the marriages that formed.

Taken together, the findings show that banning and criminalizing husbands' unilateral divorce operates differently across marriage cohorts. It can protect women and increase durability among marriages formed under the new regime, while also keeping relatively fragile pre-reform marriages intact and raising the terms on which some prospective husbands are willing to marry. A decline in divorce therefore has different implications depending on when a marriage formed: it may represent greater durability among new marriages or the retention of weaker marriages formed under the old regime.

A Additional Empirical Details

A.1 Full-Sample Ever-Married Status

For completeness, Table A1 estimates the survey-wave specification in the unrestricted full sample of women. The outcome indicates whether a woman has ever been married by her survey date, and the treatment variable is $Muslim_i \times Post_w$, where $Post_w$ indicates observation in NFHS-5. The specification includes religion, survey-wave, state, state $\times$ survey-wave, and age fixed effects. The estimate implies a 0.9 percentage point increase in age-adjusted ever-married prevalence. This outcome is cumulative: divorced, separated, and widowed women remain ever married. It is therefore not mechanically affected by the decline in divorce, but it is a survey-date stock comparison rather than a direct measure of marriage entry.

Table A1: Ever-Married Status in the Full Sample

	Ever Married by Survey Date
Muslim × NFHS5$_w$	0.902***
	(0.317)
Non-Muslim (NFHS-4), percent	77.873
Non-Muslim (NFHS-5), percent	76.614
Muslim (NFHS-4), percent	73.505
Observations	1423801

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The underlying binary outcome equals one if the respondent is currently or formerly married by the survey date and is multiplied by 100 before estimation. The coefficient and standard error are therefore reported in percentage points. The table reports the coefficient on $Muslim_i \times Post_w$, where $Post_w$ indicates observation in NFHS-5 (2019–21). The specification includes religion, state, survey-wave, state × survey-wave, and age fixed effects. The regression uses NFHS survey weights, with standard errors clustered at the state level.

A.2 Full-Sample Marital-Status Decomposition

The ever-married outcome is cumulative: currently married, widowed, divorced, and separated women are all coded as ever married. Table A2 adds never-married women and decomposes the mutually exclusive current-status categories in the unrestricted full sample. The binary outcomes are multiplied by 100 before estimation, so all coefficients and standard errors are reported in percentage points. The never-married coefficient is the negative of the ever-married coefficient. The divorce-or-separation coefficient equals the sum of the separate divorce and separation coefficients; the small positive separation estimate partly offsets the negative divorce estimate, while adding sampling variation.

Table A2: Marital-Status Outcomes in the Full Sample

	Never Married	Currently Married	Widowed	Divorced	Separated	Divorced/Separated
Muslim × NFHS5$_w$	-0.902***	1.193***	-0.239*	-0.068**	0.015	-0.052
	(0.317)	(0.330)	(0.140)	(0.032)	(0.058)	(0.057)
Non-Muslim (NFHS-4), percent	22.127	73.614	3.202	0.303	0.755	1.058
Non-Muslim (NFHS-5), percent	23.386	72.260	3.262	0.309	0.783	1.092
Muslim (NFHS-4), percent	26.495	69.787	2.431	0.578	0.709	1.288
Observations	1423801	1423801	1423801	1423801	1423801	1423801

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The binary outcomes are multiplied by 100 before estimation, so coefficients and standard errors are reported in percentage points. Each column reports the coefficient on $Muslim_i \times Post_w$, where $Post_w$ indicates observation in NFHS-5 (2019–21). Never married, currently married, widowed, divorced, and separated are mutually exclusive categories; currently married includes women who report being married or living with a partner, and divorced or separated combines the latter two categories. All specifications use the unrestricted full sample and include religion, state, survey-wave, state × survey-wave, and age fixed effects. Regressions use NFHS survey weights, with standard errors clustered at the state level.

A.3 Clean NFHS-4 Pre-Period Comparison

As a robustness check on the new-marriage design, we remove marriages formed before the reform but observed in NFHS-5 from the comparison group. The clean pre-period consists only of NFHS-4 respondents whose first cohabitation occurred between January 2012 and August 2018, while the post-period consists of NFHS-5 respondents whose first cohabitation occurred in September 2018 or later. Each table reports the coefficient on $Muslim_i \times Post_t$. All specifications include religion, state, marriage-year, state × marriage-year, and survey-wave fixed effects. The intimate-partner-violence and decision-making specifications additionally include years-since-marriage fixed effects, matching the corresponding main specifications. Regressions use the appropriate NFHS weights, and standard errors are clustered at the state level.

Table A3: Clean NFHS-4 Pre-Period Comparison: Marriage Outcomes

	(1)	(2)	(3)
	Age at First Cohabitation	Age Gap $\geq$ 5 Years	Age Gap $\geq$ 10 Years
Muslim × Post	-0.013	0.038	0.028
	(0.155)	(0.042)	(0.017)
Non-Muslim (Pre)	20.387	0.414	0.087
Non-Muslim (Post)	21.262	0.374	0.070
Muslim (Pre)	20.130	0.420	0.069
Observations	104,139	17,525	17,525

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Clean NFHS-4 Pre-Period Comparison: Intimate Partner Violence

	(1)	(2)	(3)	(4)	(5)
	Wife Beating Justified	Husband Controlling	Emotional IPV	Physical IPV	Sexual IPV
Muslim × Post	-0.080*	-0.110	-0.057**	-0.010	0.044***
	(0.044)	(0.109)	(0.024)	(0.034)	(0.015)
Non-Muslim (Pre)	0.407	0.490	0.084	0.159	0.044
Non-Muslim (Post)	0.348	0.473	0.065	0.114	0.025
Muslim (Pre)	0.450	0.475	0.083	0.144	0.035
Observations	17,555	11,161	11,167	11,167	11,167

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Clean NFHS-4 Pre-Period Comparison: Decision-Making

	(1)	(2)	(3)	(4)	(5)
	Healthcare	Large Purchases	Relatives	Husband's Money	Any Decision
Muslim × Post	0.004	-0.015	-0.006	-0.031	0.043*
	(0.022)	(0.041)	(0.026)	(0.049)	(0.021)
Non-Muslim (Pre)	0.694	0.653	0.683	0.641	0.813
Non-Muslim (Post)	0.729	0.699	0.734	0.694	0.845
Muslim (Pre)	0.687	0.643	0.672	0.619	0.789
Observations	16,803	16,803	16,803	16,803	16,803

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Clean NFHS-4 Pre-Period Comparison: Women's Characteristics

	(1)	(2)	(3)	(4)
	Years of Education	Primary Completed	Secondary Completed	Belong to a Lower Caste
Muslim × Post	-0.181	0.014**	-0.037***	0.028
	(0.113)	(0.006)	(0.013)	(0.018)
Non-Muslim (Pre)	9.320	0.852	0.380	0.784
Non-Muslim (Post)	10.600	0.918	0.502	0.813
Muslim (Pre)	7.755	0.772	0.253	0.501
Observations	103,934	103,934	99,392	103,627

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Clean NFHS-4 Pre-Period Comparison: Spouse Characteristics

	(1)	(2)	(3)	(4)
	Not Working	Husband High-Skilled	Spouse: Primary	Spouse: Secondary
Muslim × Post	0.007	-0.066*	0.006	-0.058
	(0.017)	(0.037)	(0.037)	(0.055)
Non-Muslim (Pre)	0.888	0.176	0.890	0.401
Non-Muslim (Post)	0.876	0.182	0.925	0.501
Muslim (Pre)	0.929	0.152	0.781	0.248
Observations	17,576	16,259	17,517	17,517

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.4 Spousal Age Gaps in New Marriages

For completeness, we also report spousal age-gap outcomes for newly formed marriages. In contrast to the pre-reform married stock, where the probability that the husband is at least five years older rises sharply, there is no comparable change in spousal age gaps among post-reform Muslim marriage cohorts.

Table A8: Marriage-Market Composition: Spouse and Match Characteristics (Age Gap)

	(1)	(2)
	Age Gap $\geq$ 5 Years	Age Gap $\geq$ 10 Years
Muslim $\times$ Post (Month)	0.006	0.009
	(0.039)	(0.013)
Non-Muslim (Pre)	0.423	0.094
Non-Muslim (Post)	0.374	0.070
Muslim (Pre)	0.463	0.089
Observations	36,560	36,560

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each column reports the coefficient on $Muslim_i \times Post_t$ from the difference-in-differences specification described in Section 3. Outcomes are indicators for whether the husband is at least 5 years older or at least 10 years older than the wife, respectively. These outcomes are drawn from the spouse-characteristics module. We use first cohabitation as our measure of marriage formation. All specifications include religion, marriage-year cohort, state, state $\times$ marriage-year cohort, and survey-wave fixed effects. Regressions use the appropriate NFHS survey weights. Standard errors are clustered at the state level.

A.5 Post-Reform Marriage Formation

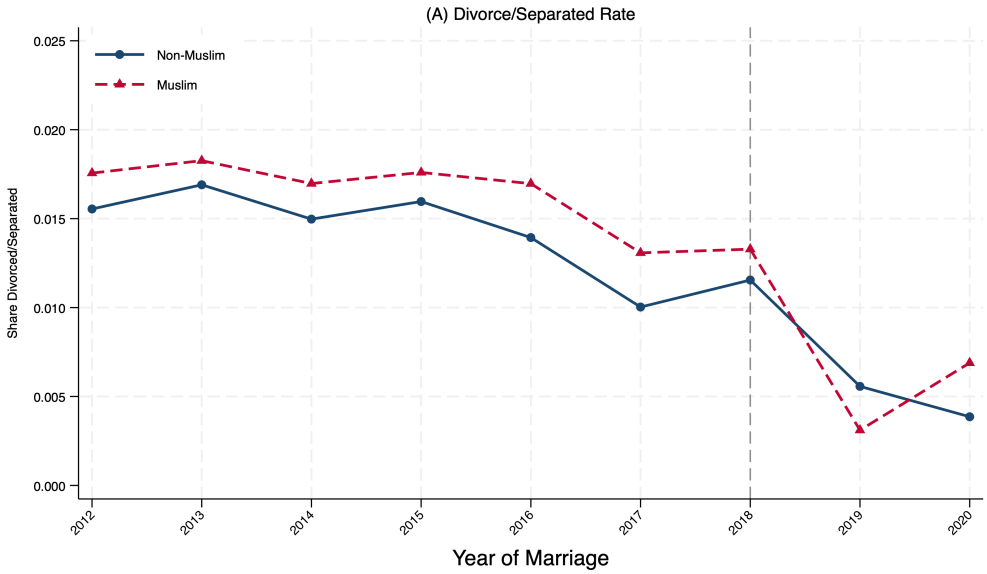
Table A9: Post-Reform Marriage Formation in the Full Sample

	(1)
	Married in September 2018 or Later
Muslim × NFHS5$_w$	0.531***
	(0.119)
Religion FE	Yes
Survey Wave FE	Yes
State FE	Yes
State × Survey Wave FE	Yes
Age FE	Yes
Observations	1,423,801
R^2	0.050

Notes: The underlying binary outcome equals one if the respondent is ever married and reports first cohabitation on or after September 2018; never-married women are coded as zero. It is multiplied by 100 before estimation, so the coefficient and standard error are reported in percentage points. The table uses the unrestricted full sample and reports the coefficient on $Muslim_i \times Post_w$, where $Post_w$ indicates observation in NFHS-5 (2019–21). The specification includes religion, state, survey-wave, state × survey-wave, and age fixed effects. The regression uses NFHS survey weights, with standard errors clustered at the state level. Because NFHS-4 was fielded in 2015–16, marriages formed on or after September 2018 are observed only in NFHS-5, so this table is interpreted as supporting evidence on post-reform marriage formation rather than an independent pre/post design.

A.6 Descriptive Trends

Figure A1: Divorce or Separation by Marriage Cohort



Notes: The figure plots the divorce-or-separation rate by first-cohabitation cohort, separately for Muslim and non-Muslim women. First cohabitation is used as the measure of marriage formation. The vertical line indicates the post-reform cutoff. The figure is descriptive.

A.7 NFHS Round Balance

	(1)	(2)
	Age	Muslim
Post_w	0.525***	-0.006
	(0.082)	(0.005)
Muslim	-1.313***	
	(0.128)	
Post_w × Muslim	-0.009	
	(0.070)	
Observations	1,423,801	1,423,801
State FE	Yes	Yes

Standard errors clustered at the state level.

Column 1 tests whether the Muslim–non-Muslim age gap changes across NFHS rounds.

Column 2 tests whether the weighted Muslim share changes across NFHS rounds within states.

B Alternative Fixed Effects

B.1 New-Marriage Specifications

The following tables apply the alternative-fixed-effects exercise to the marriage-cohort design for marriages formed around the reform.

Table B1: Marriage Outcomes: Alternative Fixed Effects

	Divorced					Divorced or Separated					Age at First Cohabitation					Husband Older than 5 Years					Husband Older than 10 Years				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Muslim × Post	-0.004***	-0.004***	-0.004***	-0.004***	-0.004***	-0.003	-0.003	-0.003	-0.003	-0.003*	0.189	0.153	0.143	-0.088	-0.106	-0.006	0.006		-0.002	0.027	0.001	0.009		0.002	0.009
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.129)	(0.126)	(0.126)	(0.066)	(0.068)	(0.036)	(0.039)		(0.033)	(0.041)	(0.011)	(0.013)		(0.012)	(0.011)
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes
Marriage-Year FE	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No	Yes	Yes	–	No	No	Yes	Yes	–	No	No
Birth-Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes	No	No	No	Yes	Yes	No	No	–	Yes	Yes	No	No	–	Yes	Yes
State FE	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes
State × Marriage-Year FE	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	–	No	No	No	Yes	–	No	No
State × Birth-Year FE	No	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	No	No	–	No	Yes	No	No	–	No	Yes
District FE	No	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	–	No	No	No	No	–	No	No
Observations	156,652	156,651	156,652	156,651	156,556	156,652	156,651	156,652	156,651	156,556	233,243	233,242	233,243	233,242	233,133	36,562	36,560	.	36,559	36,424	36,562	36,560	.	36,559	36,424

Table B2: Intimate Partner Violence Outcomes: Alternative Fixed Effects

	Wife Beating Justified				Controlling Behavior				Emotional IPV				Physical IPV				Sexual IPV			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Muslim × Post	-0.080*	-0.077	-0.078*	-0.086*	-0.111	-0.122	-0.104	-0.103	-0.047**	-0.049**	-0.042*	-0.041*	-0.005	-0.016	0.001	-0.010	0.021	0.026	0.019	0.019
	(0.041)	(0.048)	(0.041)	(0.048)	(0.081)	(0.092)	(0.079)	(0.079)	(0.022)	(0.020)	(0.022)	(0.023)	(0.042)	(0.042)	(0.046)	(0.049)	(0.018)	(0.016)	(0.020)	(0.020)
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No
Birth-Year FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Marriage-Year FE	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
State × Birth-Year FE	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Observations	37,069	37,068	37,066	36,938	25,689	25,687	25,685	25,542	25,704	25,702	25,700	25,556	25,704	25,702	25,700	25,556	25,704	25,702	25,700	25,556

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B3: Decision-Making Outcomes: Alternative Fixed Effects

	Own Health Care				Large Purchases				Visits to Relatives				Husband's Money				Any Decision			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Muslim × Post	0.001	0.012	-0.007	0.015	-0.014	-0.005	-0.019	-0.007	0.004	0.015	-0.006	0.002	-0.018	-0.001	-0.020	-0.006	0.030	0.042*	0.024	0.035
	(0.024)	(0.021)	(0.024)	(0.021)	(0.045)	(0.040)	(0.044)	(0.037)	(0.033)	(0.022)	(0.035)	(0.030)	(0.060)	(0.050)	(0.063)	(0.055)	(0.026)	(0.022)	(0.027)	(0.024)
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No
Birth-Year FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Marriage-Year FE	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
State × Birth-Year FE	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Observations	35,331	35,329	35,328	35,194	35,331	35,329	35,328	35,194	35,331	35,329	35,328	35,194	35,331	35,329	35,328	35,194	35,331	35,329	35,328	35,194

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B4: Education and Spouse Education Outcomes: Alternative Fixed Effects

	Husband More Educated					Years of Education					Completed Primary					Spouse: Primary					Spouse: Secondary					Completed Secondary				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Muslim × Post	-0.028	-0.035		-0.034	-0.038	-0.204	-0.196	-0.152	-0.265***	-0.270***	0.011	0.008	0.014*	0.009	0.006	0.029	0.017		0.024	0.017	-0.081*	-0.074		-0.091**	-0.093**	-0.026*	-0.027*	-0.022*	-0.034***	-0.030**
	(0.037)	(0.037)		(0.033)	(0.031)	(0.121)	(0.120)	(0.115)	(0.097)	(0.085)	(0.009)	(0.006)	(0.008)	(0.009)	(0.010)	(0.036)	(0.033)		(0.034)	(0.031)	(0.047)	(0.052)		(0.037)	(0.035)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)
Religion FE	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	–	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No	Yes	Yes	–	No	No	Yes	Yes	–	No	No	Yes	Yes	Yes	No	No
Birth-Year FE	No	No	–	Yes	Yes	No	No	No	Yes	Yes	No	No	No	Yes	Yes	No	No	–	Yes	Yes	No	No	–	Yes	Yes	No	No	No	Yes	Yes
State FE	Yes	Yes	–	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	No	Yes	Yes
Round FE	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Marriage-Year FE	No	Yes	–	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	–	No	No	No	Yes	–	No	No	No	Yes	No	No	No
State × Birth-Year FE	No	No	–	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	No	No	–	No	Yes	No	No	–	No	Yes	No	No	No	No	Yes
District FE	No	No	–	No	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	–	No	No	No	No	–	No	No	No	No	Yes	No	No
Observations	36,929	36,927	.	36,926	36,796	233,005	233,004	233,005	233,004	232,895	233,005	233,004	233,005	233,004	232,895	36,967	36,966	.	36,964	36,834	36,967	36,966	.	36,964	36,834	227,582	227,582	227,582	227,581	227,480

Table B5: Composition Outcomes: Alternative Fixed Effects

	Not Working					Husband High-Skilled					Lower Caste / Tribe				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Muslim × Post	-0.021	-0.023		-0.021	-0.010	-0.049*	-0.060**		-0.056**	-0.062**	0.038**	0.038**	0.032**	0.042***	0.044**
	(0.021)	(0.019)		(0.021)	(0.021)	(0.027)	(0.029)		(0.022)	(0.023)	(0.016)	(0.016)	(0.015)	(0.014)	(0.016)
Religion FE	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	–	No	No	Yes	Yes	–	No	No	Yes	Yes	Yes	No	No
Birth-Year FE	No	No	–	Yes	Yes	No	No	–	Yes	Yes	No	No	No	Yes	Yes
State FE	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	No	Yes	Yes
Round FE	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Marriage-Year FE	No	Yes	–	No	No	No	Yes	–	No	No	No	Yes	No	No	No
State × Birth-Year FE	No	No	–	No	Yes	No	No	–	No	Yes	No	No	No	No	Yes
District FE	No	No	–	No	No	No	No	–	No	No	No	No	Yes	No	No
Observations	37,103	37,102	.	37,100	36,972	34,642	34,641	.	34,639	34,508	231,911	231,910	231,911	231,910	231,801

B.2 Pre-Reform Married-Stock Specifications

These tables report the baseline and alternative-fixed-effects specifications for the survey-round design of the pre-reform married stock. The sample is restricted to currently married women whose first cohabitation began between January 2012 and August 2018. Each column reports the coefficient on $Muslim_i \times Post_w$, where $Post_w$ indicates observation in NFHS-5. Column (2) is the baseline specification reported in the main text; it includes religion, marriage-year, state, survey-wave, and state × survey-wave fixed effects. Column (1) omits the state × survey-wave interaction; column (3) replaces state with district fixed effects where applicable; column (4) replaces marriage-year with birth-year fixed effects; and column (5) adds state × birth-year fixed effects. The IPV and decision-making tables omit the district specification, so their columns (3) and (4) are the birth-year and state × birth-year variants. These two outcome families also include years-since-marriage fixed effects throughout. All regressions use the appropriate NFHS survey weights, with standard errors clustered at the state level.

Table B6: Pre-Reform Married Stock: Age at First Marriage and Age Gaps—Baseline and Alternative Fixed Effects

	Age at First Cohabitation					Husband Older than 5 Years					Husband Older than 10 Years				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Muslim × NFHS5$_w$	-0.269**	-0.300***	-0.200**	-0.139***	-0.125***	0.045**	0.057***		0.040**	0.045**	0.014	0.019*		0.013	0.016
	(0.110)	(0.096)	(0.089)	(0.035)	(0.040)	(0.017)	(0.016)		(0.017)	(0.018)	(0.011)	(0.011)		(0.012)	(0.013)
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes
Marriage-Year FE	Yes	Yes	Yes	No	No	Yes	Yes	–	No	No	Yes	Yes	–	No	No
Birth-Year FE	No	No	No	Yes	Yes	No	No	–	Yes	Yes	No	No	–	Yes	Yes
State FE	Yes	Yes	No	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes
Survey-Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes
State × Survey-Round FE	No	Yes	No	No	No	No	Yes	–	No	No	No	Yes	–	No	No
State × Birth-Year FE	No	No	No	No	Yes	No	No	–	No	Yes	No	No	–	No	Yes
District FE	No	No	Yes	No	No	No	No	–	No	No	No	No	–	No	No
Observations	202,666	202,666	202,666	202,665	202,541	32,509	32,509	.	32,506	32,366	32,509	32,509	.	32,506	32,366

Table B7: Pre-Reform Married Stock: Intimate Partner Violence and Related Outcomes—Baseline and Alternative Fixed Effects

	Wife Beating Justified				Controlling Behavior				Emotional IPV				Physical IPV				Sexual IPV			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Muslim × NFHS5$_w$	-0.011	0.001	-0.007	-0.008	0.016	0.018	0.015	0.010	-0.005	-0.011	-0.007	-0.003	0.010	0.001	0.009	0.009	0.020	0.022	0.023	0.024
	(0.024)	(0.025)	(0.021)	(0.019)	(0.035)	(0.032)	(0.034)	(0.036)	(0.016)	(0.015)	(0.015)	(0.014)	(0.016)	(0.016)	(0.013)	(0.013)	(0.019)	(0.021)	(0.020)	(0.019)
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No
Birth-Year FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Survey-Round FE	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
State × Birth-Year FE	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Years-Since-Marriage FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,498	32,498	32,495	32,358	22,891	22,891	22,888	22,733	22,902	22,902	22,899	22,744	22,902	22,902	22,899	22,744	22,902	22,902	22,899	22,744

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B8: Pre-Reform Married Stock: Women's Decision-Making—Baseline and Alternative Fixed Effects

	Own Health Care				Large Purchases				Visits to Relatives				Husband's Money				Any Decision			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Muslim × NFHS5$_w$	-0.017	-0.019	-0.022	-0.027	-0.020	-0.024	-0.028	-0.027	-0.035	-0.032	-0.044	-0.043*	-0.050**	-0.052***	-0.052***	-0.052***	-0.005	-0.003	-0.011	-0.013
	(0.020)	(0.018)	(0.018)	(0.018)	(0.020)	(0.018)	(0.017)	(0.017)	(0.028)	(0.022)	(0.026)	(0.025)	(0.021)	(0.017)	(0.018)	(0.019)	(0.016)	(0.015)	(0.015)	(0.016)
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No
Birth-Year FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Round FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Survey-Round FE	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
State × Birth-Year FE	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Years-Since-Marriage FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31,441	31,441	31,438	31,299	31,441	31,441	31,438	31,299	31,441	31,441	31,438	31,299	31,441	31,441	31,438	31,299	31,441	31,441	31,438	31,299

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B9: Pre-Reform Married Stock: Education and Spouse Education—Baseline and Alternative Fixed Effects

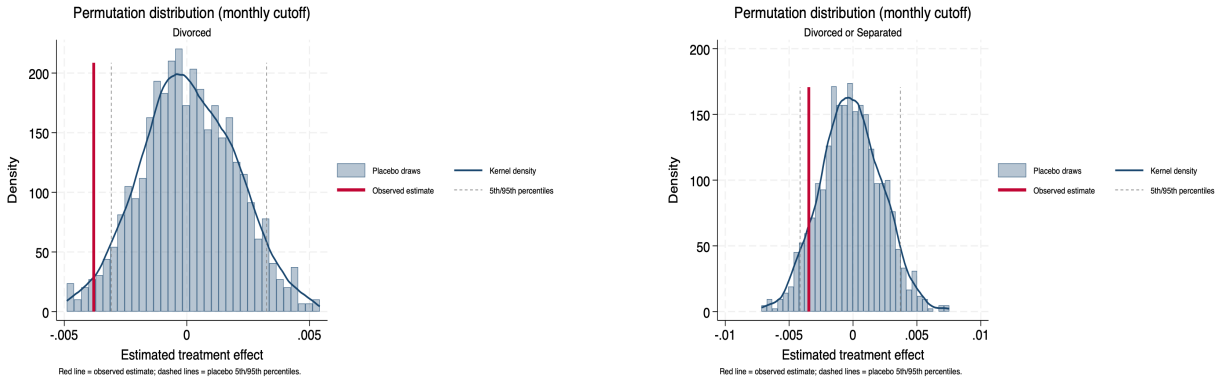
	Husband More Educated					Years of Education					Completed Primary					Spouse: Primary					Spouse: Secondary					Completed Secondary				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Muslim × NFHS5$_w$	0.027	0.022		0.025	0.025	0.024	0.005	0.073	0.046	0.025	0.011**	0.010**	0.011**	0.010*	0.007	-0.005	-0.008		-0.004	-0.004	0.011	0.009		0.019	0.021	-0.010	-0.014*	-0.005	-0.007	-0.005
	(0.026)	(0.030)		(0.025)	(0.026)	(0.066)	(0.069)	(0.066)	(0.084)	(0.080)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.014)	(0.013)		(0.015)	(0.013)	(0.017)	(0.021)		(0.016)	(0.016)	(0.007)	(0.008)	(0.007)	(0.010)	(0.011)
Religion FE	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	–	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No	Yes	Yes	–	No	No	Yes	Yes	–	No	No	Yes	Yes	Yes	No	No
Birth-Year FE	No	No	–	Yes	Yes	No	No	No	Yes	Yes	No	No	No	Yes	Yes	No	No	–	Yes	Yes	No	No	–	Yes	Yes	No	No	No	Yes	Yes
State FE	Yes	Yes	–	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	No	Yes	Yes
Survey-Round FE	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Survey-Round FE	No	Yes	–	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	–	No	No	No	Yes	–	No	No	No	Yes	No	No	No
State × Birth-Year FE	No	No	–	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes	No	No	–	No	Yes	No	No	–	No	Yes	No	No	No	No	Yes
District FE	No	No	–	No	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	–	No	No	No	No	–	No	No	No	No	Yes	No	No
Observations	32,384	32,384	.	32,381	32,242	202,443	202,443	202,443	202,442	202,318	202,443	202,443	202,443	202,442	202,318	32,421	32,421	.	32,418	32,279	32,421	32,421	.	32,418	32,279	198,419	198,419	198,419	198,418	198,309

Table B10: Pre-Reform Married Stock: Composition Outcomes—Baseline and Alternative Fixed Effects

	Not Working					Husband High-Skilled					Lower Caste / Tribe				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Muslim × NFHS5$_w$	0.052***	0.053***		0.051***	0.051***	0.007	-0.002		0.009	0.013	-0.013	-0.017	-0.012	-0.014	-0.012
	(0.014)	(0.016)		(0.014)	(0.015)	(0.024)	(0.026)		(0.023)	(0.024)	(0.014)	(0.015)	(0.015)	(0.015)	(0.014)
Religion FE	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	–	No	No	Yes	Yes	–	No	No	Yes	Yes	Yes	No	No
Birth-Year FE	No	No	–	Yes	Yes	No	No	–	Yes	Yes	No	No	No	Yes	Yes
State FE	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	No	Yes	Yes
Survey-Round FE	Yes	Yes	–	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Survey-Round FE	No	Yes	–	No	No	No	Yes	–	No	No	No	Yes	No	No	No
State × Birth-Year FE	No	No	–	No	Yes	No	No	–	No	Yes	No	No	No	No	Yes
District FE	No	No	–	No	No	No	No	–	No	No	No	No	Yes	No	No
Observations	32,528	32,528	.	32,525	32,387	30,448	30,448	.	30,445	30,305	201,464	201,464	201,464	201,463	201,338

C Permutation Tests

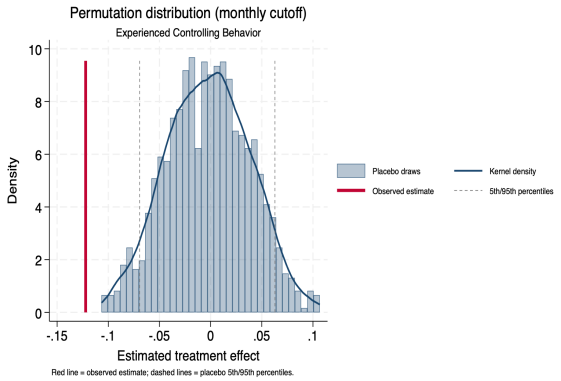
The permutation-based randomization-inference exercise applies only to the main monthly new-marriage-cohort analysis. In that design, treatment status is defined by the month of first cohabitation, so placebo reform timing can be assigned across marriage months while holding fixed the number of post-reform months. The exercise is not applied to the pre-reform married-stock or full-sample ever-married analyses, for which the post indicator is defined by NFHS survey round and only two rounds are available. The figures below report selected outcomes from the marital-exit block (divorce and divorce or separation), the marital-quality block (wife-beating attitudes, husbands' controlling behavior, and emotional IPV), decision-making (any decision), women's education and composition, and spouse and household composition.



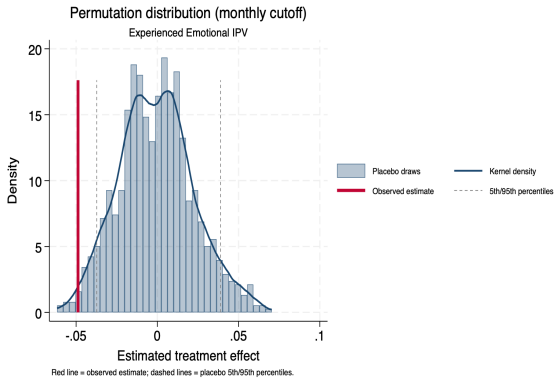
(a) Divorced

(b) Divorced or Separated

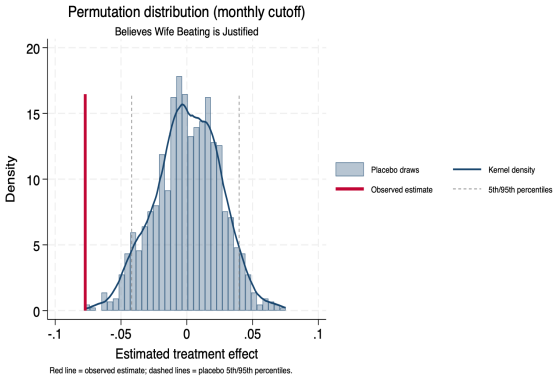
Figure C1: Permutation Tests: Marriage Outcomes



(a) Husband Controlling

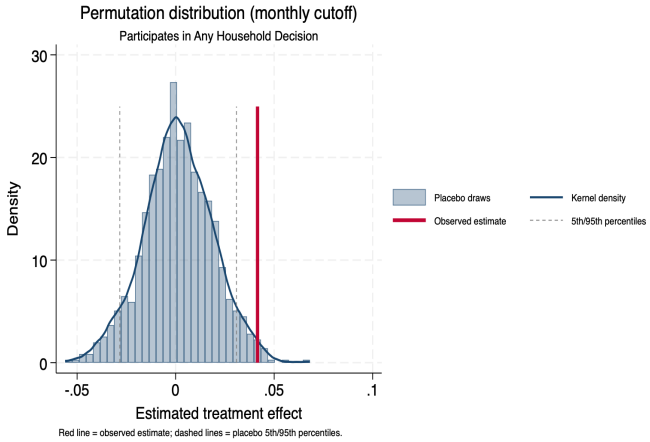


(b) Emotional IPV



(c) Wife Beating Justified in Any Situation

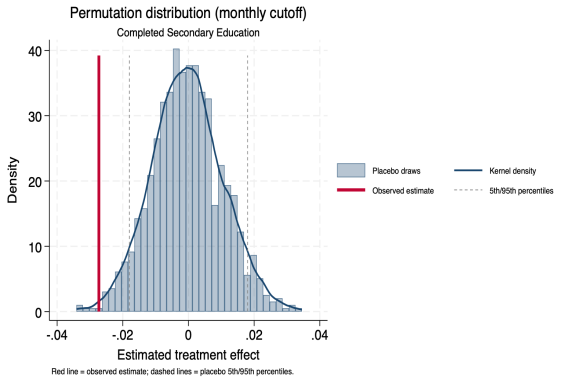
Figure C2: Permutation Tests: IPV Outcomes



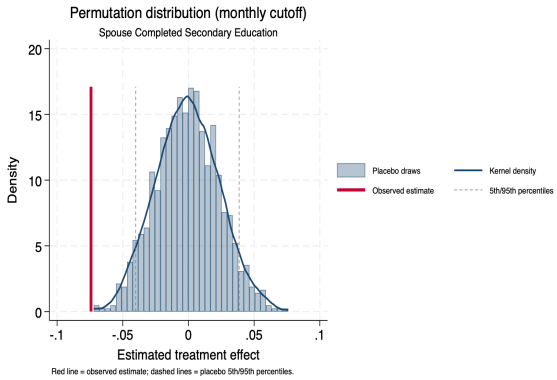
(a) Any Decision

Figure C3: Permutation Tests: Decision-Making Outcomes

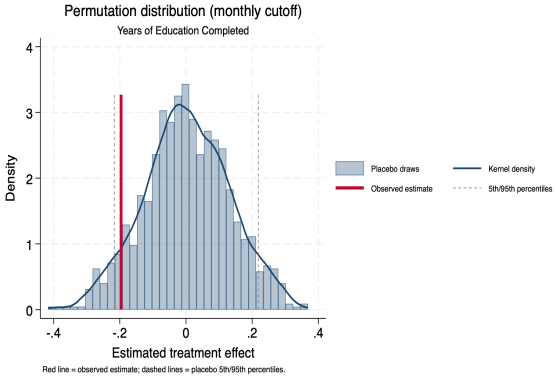
Figure C4: Permutation Tests: Education Outcomes



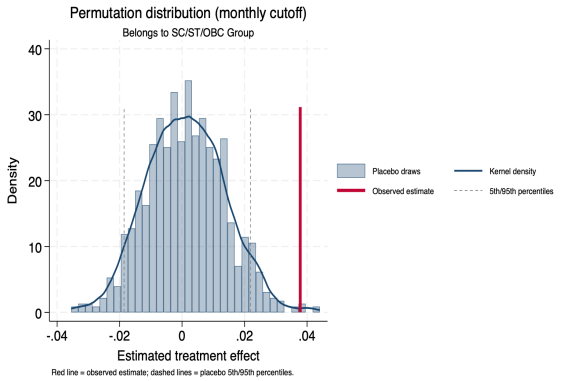
(a) Secondary Completed



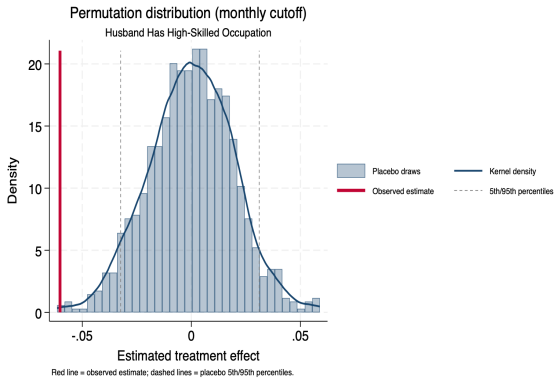
(b) Spouse Completed Secondary Education



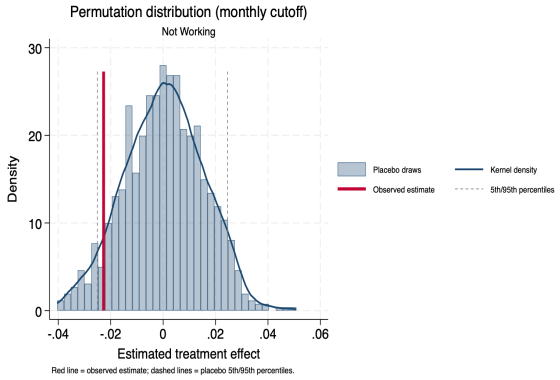
(c) Years of Education Completed



(a) Lower Caste / Tribe



(b) Husband Occupation High Skilled



(c) Women Not Working

Figure C5: Permutation Tests: Additional Composition Outcomes

D Wild-Cluster Bootstrap Inference

Table D1: Wild-Cluster Bootstrap Inference: Main New-Marriage Specification (Monthly Cutoff)

	Coef.	Conventional p	Wild-bootstrap p
Divorce and Marriage			
Divorced	-0.004	0.006	0.000
Divorced or Separated	-0.003	0.148	0.150
Age at First Cohabitation	0.153	0.231	0.342
Age Gap $\geq$ 5 Years	0.006	0.879	0.915
Age Gap $\geq$ 10 Years	0.009	0.480	0.546
Intimate Partner Violence			
Wife Beating Justified	-0.077	0.114	0.223
Husband Controlling	-0.122	0.195	0.335
Emotional IPV	-0.049	0.019	0.158
Physical IPV	-0.016	0.711	0.785
Sexual IPV	0.026	0.113	0.154
Women's Own Characteristics			
Years of Education	-0.196	0.112	0.327
Primary Completed	0.008	0.216	0.150
Secondary Completed	-0.027	0.051	0.204
Lower Caste/Tribe	0.038	0.027	0.081
Not Working	-0.023	0.239	0.215
Spouse Characteristics			
Husband High-Skilled	-0.060	0.046	0.138
Spouse: Primary	0.017	0.613	0.781
Spouse: Secondary	-0.074	0.161	0.381
Decision-Making			
Healthcare	0.012	0.563	0.546
Large Purchases	-0.005	0.912	0.935
Relatives	0.015	0.486	0.569
Husband's Money	-0.001	0.990	0.985
Any Decision	0.042	0.069	0.065

Notes: Each row is a separate DiD regression of the outcome on Muslim $\times$ Post, post defined by marriage on/after September 2018. IPV, decision-making, and divorce outcomes add years-since-marriage fixed effects; marriage, own, and spouse characteristics use the base marriage-entry fixed effects (religion, marriage-year, state, state $\times$ marriage-year, survey round). Divorce outcomes use NFHS-5 only. Wild-cluster-bootstrap p-values use 500 replications, seed 12345, clustered on 35 states.

Table D2: Wild-Cluster Bootstrap Inference: Survey-Year Pre-Reform Marriages (Monthly Cutoff)

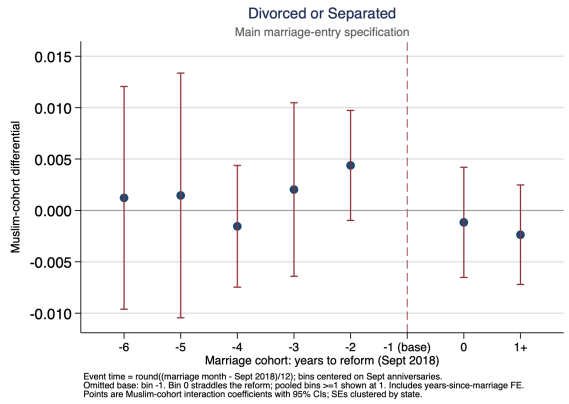
	Coef.	Conventional p	Wild-bootstrap p
Marriage			
Age at First Cohabitation	-0.300	0.004	0.035
Age Gap $\geq$ 5 Years	0.057	0.001	0.008
Age Gap $\geq$ 10 Years	0.019	0.095	0.108
Intimate Partner Violence			
Wife Beating Justified	0.001	0.957	0.896
Husband Controlling	0.018	0.585	0.596
Emotional IPV	-0.011	0.453	0.550
Physical IPV	0.001	0.958	0.954
Sexual IPV	0.022	0.295	0.423
Women's Own Characteristics			
Years of Education	0.005	0.937	0.919
Primary Completed	0.010	0.038	0.054
Secondary Completed	-0.014	0.070	0.058
Lower Caste/Tribe	-0.017	0.266	0.277
Not Working	0.053	0.002	0.000
Spouse Characteristics			
Husband High-Skilled	-0.002	0.937	0.892
Spouse: Primary	-0.008	0.525	0.519
Spouse: Secondary	0.009	0.686	0.731
Decision-Making			
Healthcare	-0.019	0.323	0.254
Large Purchases	-0.024	0.189	0.165
Relatives	-0.032	0.163	0.162
Husband's Money	-0.052	0.004	0.004
Any Decision	-0.003	0.862	0.838

Notes: Each row is a separate DiD regression of the outcome on Muslim $\times$ NFHS5$_w$, on the stock of pre-reform marriages (formed before September 2018, currently married), where $NFHS5_w = 1$ for observations in NFHS-5. IPV and decision-making outcomes add years-since-marriage fixed effects; marriage, own, and spouse characteristics use the base survey-year fixed effects (religion, marriage-year, state, state $\times$ survey wave). Wild-cluster-bootstrap p-values use 500 replications, seed 12345, clustered on 35 states.

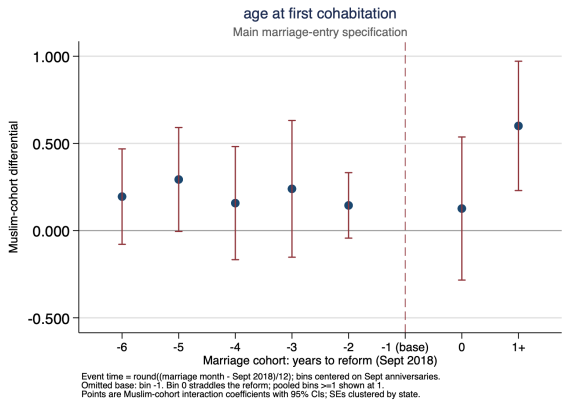
For the unrestricted full-sample ever-married specification reported in Table A1, the wild-cluster-bootstrap p-value for the coefficient on $Muslim_i \times Post_w$ is 0.038 . The bootstrap uses the same specification and survey weights as the main result, with standard errors clustered at the state level and 500 replications using seed 12345.

E Event-Study Estimates

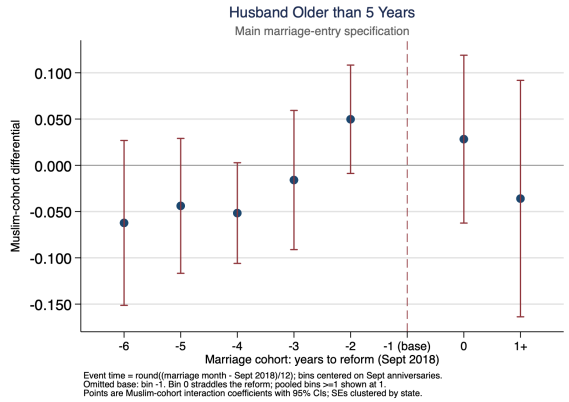
The appendix figures below use the whole-pre main new-marriage-cohort specification and the rounded event-time bins centered on the September 2018 ordinance. The displayed event-time range begins at −6; clean pre-reform estimates are reported separately in the robustness tables and are not used here.



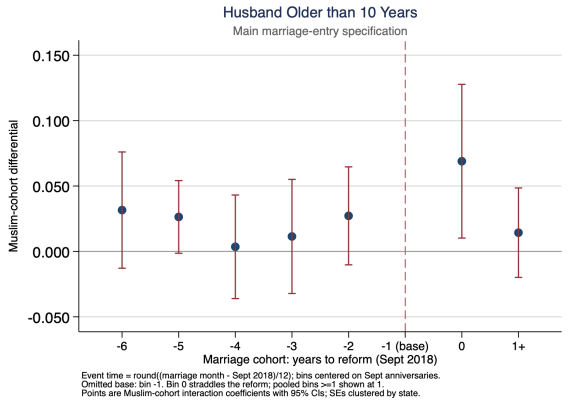
(a) Divorced or separated



(b) Age at first cohabitation

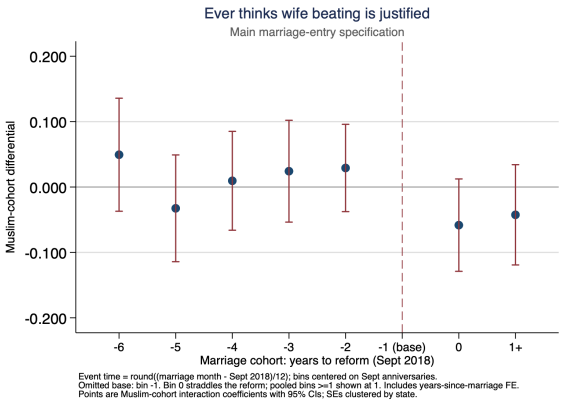


(c) Husband age gap ≥ 5 years

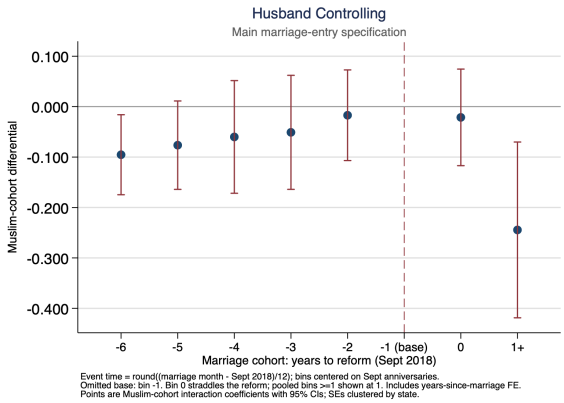


(d) Husband age gap ≥ 10 years

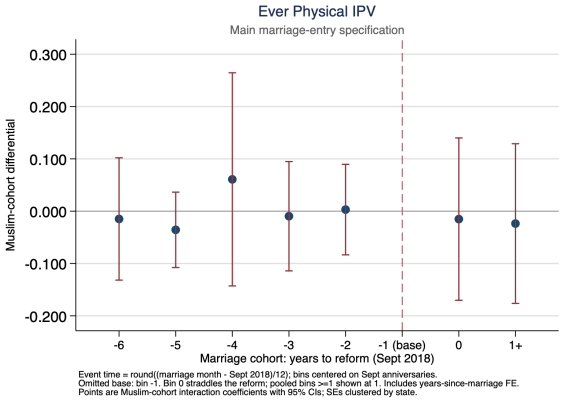
Figure E1: Whole-pre rounded event studies: marriage outcomes.



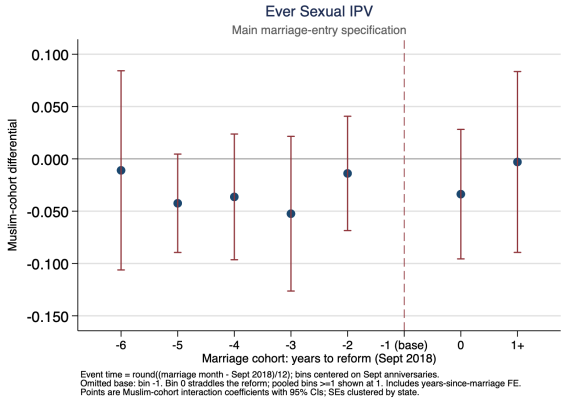
(a) Wife-beating beliefs



(b) Husband controlling

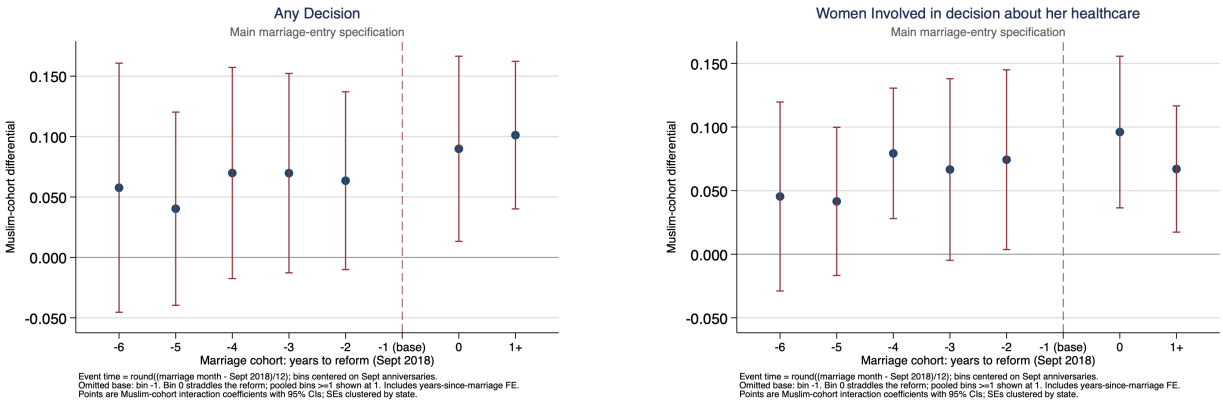


(c) Physical IPV



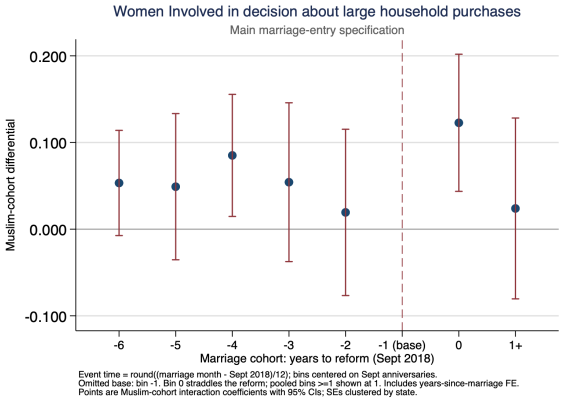
(d) Sexual IPV

Figure E2: Whole-pre rounded event studies: IPV outcomes. Emotional IPV is shown in the main text.

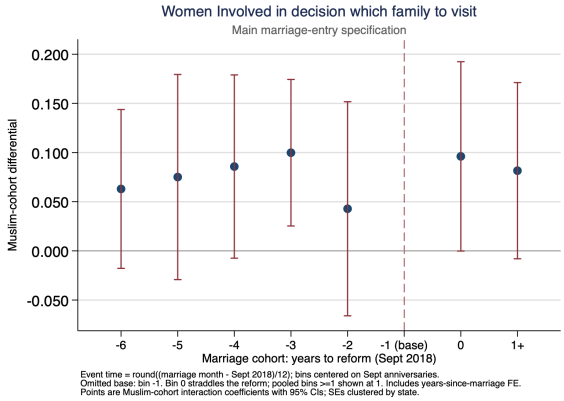


(a) Any decision

(b) Healthcare

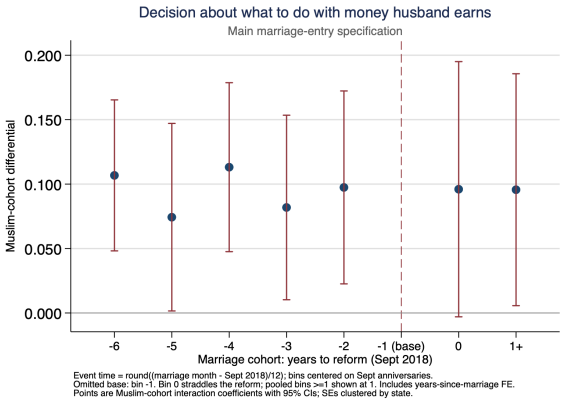


(c) Large purchases



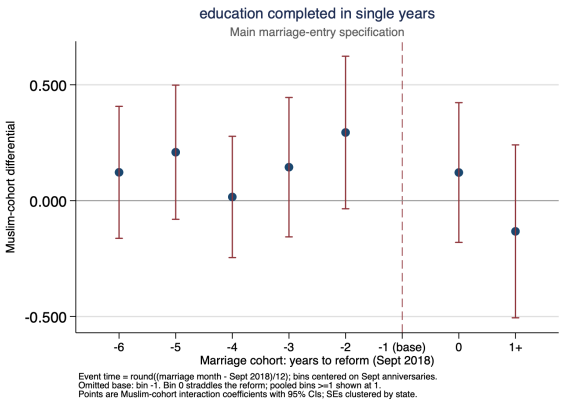
(d) Relatives

Figure E3: Whole-pre rounded event studies: decision-making outcomes.

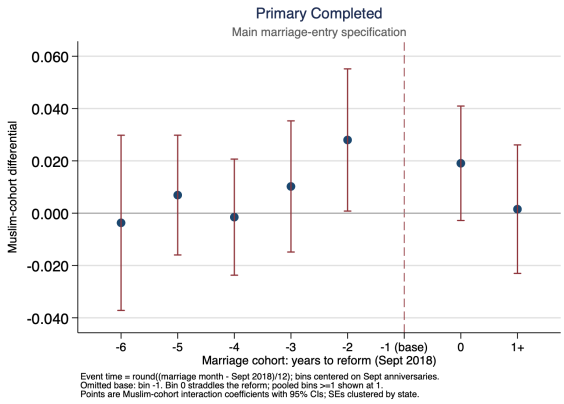


(a) Husband's money

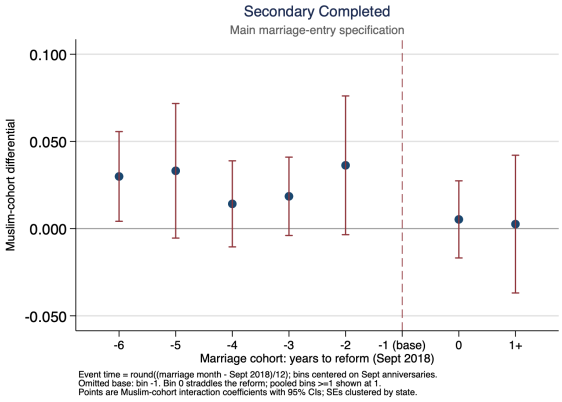
Figure E4: Whole-pre rounded event study: decision about husband's money.



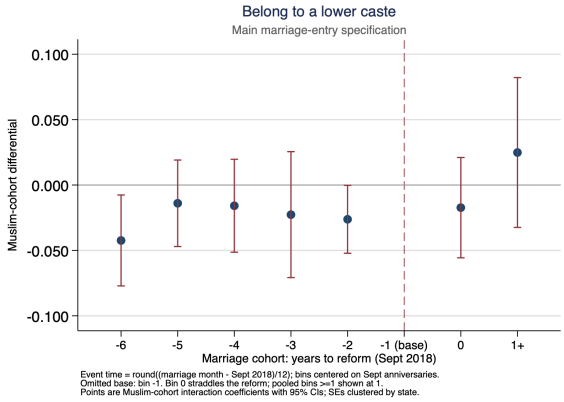
(a) Years of education



(b) Primary completed

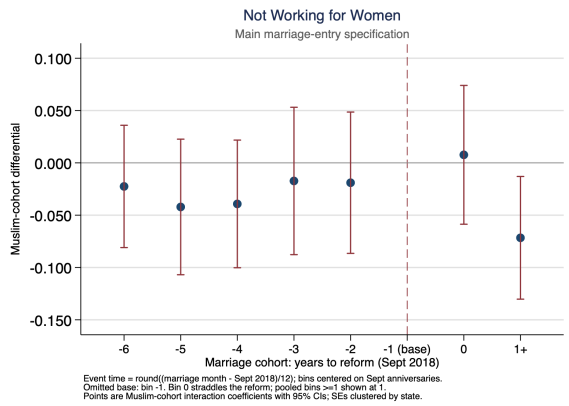


(c) Secondary completed



(d) Lower caste/tribe

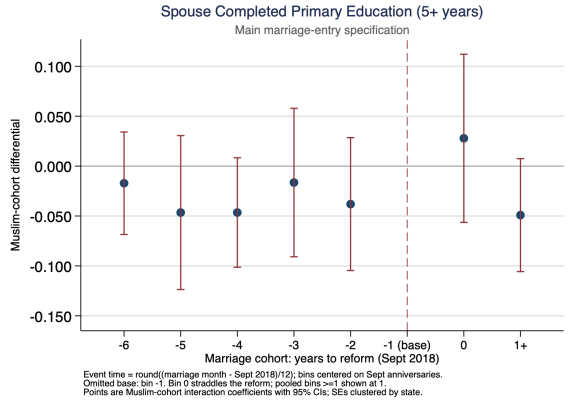
Figure E5: Whole-pre rounded event studies: women's characteristics.



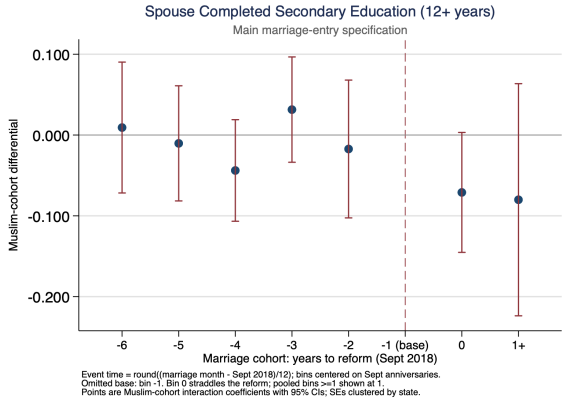
(a) Not working



(b) Husband high-skilled



(c) Spouse primary completed



(d) Spouse secondary completed

Figure E6: Whole-pre rounded event studies: spouse and match characteristics.

F Selection into Survey Modules

Table F1: Selection into Survey Modules

	(1) Asked husband's questionnaire	(2) Observed spouse education module	(3) Selected for domestic violence module	(4) Observed in domestic violence module — Selected sample	(5) Observed in domestic violence module — Full sample
Muslim × Post	0.007	0.008	-0.016	0.002	0.003
	(0.010)	(0.010)	(0.018)	(0.012)	(0.007)
Religion FE	Yes	Yes	Yes	Yes	Yes
Marriage-Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Round FE	Yes	Yes	Yes	Yes	Yes
State × Marriage-Year FE	Yes	Yes	Yes	Yes	Yes
Observations	233,242	233,242	233,242	125,564	233,242

Standard errors in parentheses

* p|0.10, ** p|0.05, *** p|0.01

G Conceptual Framework Derivations

G.1 Bilateral Participation and Marriage Entry

Let G denote the husband's uncertain future gross gain from using the affected exit route rather than continuing the marriage, before paying the legal friction. We hold the conditional distribution of G fixed as F changes and assume that it has no mass point at $G = F$. Other reform-induced changes in the value of marriage are captured by $J_h(q, F)$. For any scalar x, let $(x)_+ = \max\{x, 0\}$. The husband's option value is

$$\Omega(q, F) = \mathbb{E}\left[(G - F)_+ \mid q\right].$$

The option is used when $G > F$. Defining $p(q, F) = \Pr(G > F \mid q)$ and differentiating under the expectation gives

$$\frac{\partial \Omega(q, F)}{\partial F} = -\Pr(G > F \mid q) = -p(q, F) \leq 0.$$

Thus, a one-unit increase in F reduces the option payoff by one unit only in the future states in which the option would be used. The distribution of G can vary with q; no parametric relationship between the two is required.

Suppose each partner's net value of marriage is increasing in match quality, and let $q_j(F, z)$ solve $\Delta_j(q_j, z, F) = 0$ for $j \in \{w, h\}$. The implicit-function theorem gives

$$\frac{\partial q_j(F, z)}{\partial F} = -\frac{\partial \Delta_j(q, z, F)/\partial F}{\partial \Delta_j(q, z, F)/\partial q}.$$

If protection and commitment raise the woman's value of marriage, then $\partial \Delta_w / \partial F > 0$ and her cutoff falls. For husbands,

$$\frac{\partial \Delta_h}{\partial F} = \frac{\partial J_h}{\partial F} - p(q, F),$$

so the sign depends on whether the commitment benefit exceeds the lost option value of

low-cost exit. Since the effective cutoff is $q^*(F,z) = \max\{q_w(F,z), q_h(F,z)\}$, either response can determine the entry effect.

With $m_{az}(F)$ denoting the arrival rate of potential matches and Φ_{az} and ϕ_{az} denoting the distribution and density of offered match quality, the age-specific marriage hazard is

$$\lambda_{az}(F) = m_{az}(F)\left[1 - \Phi_{az}\big(q^*(F,z)\big)\right].$$

Its derivative is

$$\frac{d\lambda_{az}(F)}{dF} = \frac{\partial m_{az}(F)}{\partial F}\left[1 - \Phi_{az}\big(q^*(F,z)\big)\right] - m_{az}(F)\phi_{az}\big(q^*(F,z)\big)\frac{\partial q^*(F,z)}{\partial F}.$$

The sign is therefore ambiguous even if match opportunities are unchanged, and it can differ across types z or ages a.

For a fixed age a, type z, and distribution of offered matches, let

$$\bar{q}^N_{az}(F) = \mathbb{E}\left[q \mid q \geq q^*(F,z)\right].$$

Differentiating with respect to the joint cutoff gives

$$\frac{\partial \bar{q}^N_{az}(F)}{\partial q^*} = \frac{\phi_{az}(q^*)}{1 - \Phi_{az}(q^*)}\left[\bar{q}^N_{az}(F) - q^*(F,z)\right] \geq 0.$$

Therefore, a cutoff-driven decline in entry raises average q among the marriages formed within that type. A decline caused by a lower arrival rate does not imply this change.

G.2 Exit and Survivor Selection

For an existing marriage, continuation surplus is

$$S(q,z,F) = V_h(q,z,F) - \left[A_h(z) - F\right].$$

Using $V_h(q,z,F) = u_h(q,z) + J_h(q,F) + \Omega(q,F)$ and $\Omega_F(q,F) = -p(q,F)$,

$$S_F(q,z,F) = 1 + J_{hF} - p(q,F) \geq 0.$$

Assume $S_q(q,z,F) > 0$ and let $q^S(F,z)$ solve $S(q^S,z,F) = 0$. The implicit-function theorem gives

$$\frac{\partial q^S(F,z)}{\partial F} = -\frac{S_F(q,z,F)}{S_q(q,z,F)} \leq 0.$$

Hence, the reform weakly lowers the match-quality cutoff for remaining in the observed married stock.

For a fixed type z, let Ψ_z and ψ_z denote the distribution and density of q among existing marriages, and write $c(F,z) = q^S(F,z)$. The average observed outcome among surviving marriages of type z is

$$\bar{Y}^S(F,z) = \frac{\displaystyle\int_{c(F,z)}^{\infty} y(q,F,z)\psi_z(q)\,dq}{1 - \Psi_z(c(F,z))}.$$

Differentiating gives

$$\begin{aligned}\frac{d\bar{Y}^S(F,z)}{dF} &= \mathbb{E}\left[y_F(q,F,z) \mid q \geq c(F,z)\right] \\ &\quad + \frac{\psi_z(c)}{1-\Psi_z(c)}\frac{\partial c(F,z)}{\partial F}\left[\bar{Y}^S(F,z) - y(c,F,z)\right].\end{aligned}$$

The first term is the within-marriage effect. The second is the survivor-selection effect. Since $c_F \leq 0$, the selection term is negative for an outcome that is lower among the newly retained marriages at the cutoff than among marriages already in the surviving stock. For a characteristic fixed when the marriage formed, $y_F(q,F,z) = 0$, so its change conditional on z is entirely compositional. The unconditional mean in the main text also permits differential survival to change the distribution of z; that reweighting is part of survivor selection.

G.3 Outcomes among New Marriages

Let P_r^N denote the joint distribution of (q, z) among marriages formed under regime r, and write $y_r(q, z) = y(q, F_r, z)$. The average outcome in the new-marriage cohort is

$$\bar{Y}_r^N = \int y_r(q, z)\, dP_r^N(q, z).$$

Adding and subtracting the outcome under the post-reform law evaluated over the pre-reform distribution gives the exact decomposition

$$\bar{Y}_1^N - \bar{Y}_0^N = \underbrace{\int [y_1(q, z) - y_0(q, z)]\, dP_0^N(q, z)}_{\text{within-marriage effect}} + \underbrace{\left[\int y_1(q, z) dP_1^N(q, z) - \int y_1(q, z) dP_0^N(q, z)\right]}_{\text{entry-selection effect}}.$$

The first term holds the pre-reform set of matches fixed and changes the legal regime. The second holds post-reform outcomes fixed and changes the joint distribution of latent match quality and observed characteristics among the marriages that form. For a characteristic fixed at entry, the first term is zero. For divorce, intimate partner violence, and decision-making, both terms can contribute. Holding the match fixed, the higher exit friction delays or discourages divorces that would otherwise have occurred through instantaneous triple talaq; entry selection may reinforce or offset this effect. The framework does not impose a sign on the within-marriage effects on intimate partner violence or decision-making.

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