

Communal Violence, Bargaining Power, and Reproductive Autonomy: Evidence from the 2002 Gujarat Riots*

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Abstract

This paper provides evidence that targeted communal violence persistently erodes the intra-household bargaining power and reproductive autonomy of already-married women in an affected minority community. I estimate the long-run effects of anti-Muslim riots in 2002 on Muslim women's bargaining power and reproductive autonomy using four rounds of India's National Family Health Survey (1998–2021) and a triple difference-in-differences design, comparing Muslim to non-Muslim women in Gujarat relative to other Indian states. Decision-making power over household purchases fell by 5.8 percentage points and over decisions about visiting family and friends by 5.3 percentage points, effects that persisted for nearly two decades, while the shift from concealable to male-controlled contraception indicates women lost private control over fertility. Gujarati Hindu women experienced comparable declines, and the effect is not replicated by Muslim women outside Gujarat under similar political or geographic conditions. The impact of communal violence spread far beyond the immediate deaths, diminishing the independence of women in Gujarat for decades afterward.

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1 Introduction

In the collective model of household decision-making (Chiappori, 1992; Browning and Chiappori, 1998), a woman’s autonomy within the marriage is determined by her outside options. Shocks that reduce women’s fallback utility reduce their intra-household bargaining power and lead to worse outcomes in areas they care about. An example of such a shock is the 2002 anti-Muslim riots in Gujarat, India, which disrupted Muslim economic prospects through labor market disruption, destruction of household assets, and community dislocation. Mitra and Ray (2014) show that Muslim economic prosperity historically triggered preemptive violence by the Hindu majority in India, which goes hand-in-hand with the view that communal violence functions partly to suppress minority economic advancement. This is part of a broader literature on the economic and social roots of communal violence in India (Varshney, 2003).

Previous studies on communal violence’s effect on women have focused on age at marriage and child marriage (Ghose and Pandey, 2025), political participation (Rogall and Zárate-Barrera, 2020), domestic violence through male backlash (Guarnieri and Rainer, 2021) when such episodes economically empower women, and how its effect depends heavily on the sex-ratio distortions (La Mattina, 2017), without considering the intra-household bargaining power of women who were already-married before the violence occurred. My paper contributes to the literature on conflict and women’s agency by showing that targeted communal violence diminishes intra-household bargaining power through behavioral channels rather than attitudinal change.

This paper therefore asks whether targeted communal violence erodes the intra-household bargaining power and reproductive autonomy of *already-married* women in an affected community. I study the 2002 Gujarat riots, one of the most severe episodes of communal violence in post-independence India, as a quasi-natural experiment. Using four rounds of India’s National Family Health Survey (NFHS) through 1998–2021 and a triple difference-in-differences (DDD) design, I estimate the causal effect of the riots on Muslim women’s intra-household bargaining power and reproductive autonomy across three, thirteen and seventeen years after the violence.

On February 27, 2002, the Sabarmati Express¹ carrying Hindu pilgrims was set on fire near Godhra, killing 59 people. In the immediate aftermath, Muslim mobs were blamed for the attack, triggering retaliatory anti-Muslim violence that spread across 16 of Gujarat’s 24 districts (Jaffrelot, 2003) which was unlike the previous Hindu-Muslim clashes, which had been mostly urban. Approximately 1,000–2,000 people were killed, over 200,000 Muslims were displaced, and thousands of Muslim-owned businesses were destroyed (Amnesty International, 2005; Khan, 2013). Patil (2017) traces how Hindu nationalist propaganda portrayed Muslims as racial and cultural outsiders, placing the

¹The Sabarmati Express runs between Ahmedabad and Varanasi (via Faizabad, now Ayodhya).

entire community outside the protection of the state, while [Chatterjee \(2006\)](#) documents how the violence displaced Muslim families into ghettos and subjected them to sustained economic boycotts. These effects were persistent, where Gujarati Muslims faced continued difficulty returning to businesses and public-sector employment amid ongoing boycotts more than a year after the riots ([Narula, 2003](#)). Sexual violence against Muslim women was also a systematic and well-documented feature of the riots, though official records substantially under-reported it ([Khanna, 2008](#); [Srivastava, 2011](#)).

Household decision-making autonomy declined sharply among the Gujarati Muslim women in which decision-making power on purchases fell by 5.8 percentage points and that of mobility fell by 5.3 percentage points after the riots. Gujarati Hindu women also experienced declines on several of these same dimensions, in some cases of similar or larger magnitude. I trace the effects across three survey rounds, spanning two decades, documenting the compounding long-term costs of communal violence which have been invisible to studies focused on mortality or marriage market outcomes alone. Tracing where lost decision-making authority goes reveals that power not only transfers from wife to husband but is also a disintegration of the marriage as a unit, with authority diffusing outwards to extended family members.

The riots also eroded reproductive autonomy through a specific substitution pattern. Female-controlled methods declined by 5.3 percentage points while male-controlled methods rose by 1.8 percentage points. When spousal preferences over fertility differ ([Becker, 1981](#)), women in low-autonomy settings use oral contraceptive pills specifically because they can be hidden from husbands ([Ashraf, Field, and Lee, 2014](#)). The ability to use contraception secretly is a very sensitive indicator of women's private autonomy within a marriage. I test this prediction directly, and find that the riots caused a sharp decline in female-controlled and concealable contraceptive use while male-controlled methods simultaneously increased. As a result, women lost reproductive autonomy independently of overall contraceptive access.

A further consequence of the riots was a temporary shift in women's labor supply. Employment rose by 8.0 percentage points within three years of the riots, due to an added-worker effect ([Lundberg, 1985](#)) as women entered the labor market to offset lost household income following the destruction of businesses and assets, supporting the evidence that conflict can increase women's employment as a coping mechanism ([Menon and Van der Meulen Rodgers, 2015](#)). By 2015, employment had reversed into a 4.4 percentage point suppression, since emergency-driven labor force entry did not translate into sustainable bargaining power gains. I find no detectable effects on domestic violence attitudes across any horizon, indicating that the riots reshaped bargaining power through material and behavioral channels rather than by shifting underlying gender norms.

I rule out several alternative explanations for these results. A 2010 placebo test shows no differential effect in a period with no comparable violence. The conventional

cluster-robust inference becomes unreliable since there is only one treated state. I therefore complement state-clustered standard errors with wild cluster bootstrap and randomization inference, and treat the consistency of sign and magnitude across ten outcomes, three time horizons, and multiple specifications as the primary basis for assessing the robustness of these findings. Since Gujarat is the only treated state, conventional significance tests do not apply. I therefore treat the consistency of these effects across outcomes, time horizons, and specifications, described in Section 3.4, as the basis for the causal interpretation offered here.

Previous literature has established correlational links between women’s bargaining power and household welfare outcomes, but has struggled to identify causal effects (Doss, 2013). This paper addresses this gap by providing the first evidence, consistent across outcomes, time horizons, and empirical specifications, that targeted communal violence erodes the bargaining power and reproductive autonomy of women who were already married when the violence occurred.

The remainder of this paper is organized as follows: Section 2 describes the data. Section 3 presents the empirical strategy. Section 4 reports the main results, heterogeneity analysis, and robustness checks. Section 5 concludes.

2 Data

In the following section, I describe the survey data and sample construction (Section 2.1), the primary and secondary outcome variables (Section 2.2), and the control variables used throughout the analysis (Section 2.3).

2.1 Survey Data

I use four waves of the National Family Health Survey (NFHS; IIPS and ORC Macro 2000; IIPS and Macro International 2007; IIPS and ICF 2017, 2021), which is India’s implementation of the Demographic and Health Surveys (DHS) program. The survey collects cross-sectional data using a stratified multistage cluster design, with samples representative at both the national and state levels. I restrict the analysis to currently married women aged 15–49, giving me an analysis sample of 1,254,746 women across all four rounds.² The pre-treatment baseline comes from NFHS-2 (1998–99), which was conducted approximately three years before the riots. Post-treatment rounds are from NFHS-3 (2005–06), NFHS-4 (2015–16), and NFHS-5 (2019–21), which spans over three, thirteen, and seventeen years after the riots, respectively. Figure 1 shows the survey design and the timing of the riots relative to each round.

²“Currently married” includes both formally married and cohabiting women (DHS marital status codes 1–2) and no other marital status categories are present in the harmonized sample.

I observe empowerment outcomes across four different time points and decompose the effects into short-, medium-, and long-run components. One design limitation is the absence of NFHS-1 (1992–93) in the analysis, which excluded the domestic violence module and several autonomy measures used in the later rounds, making it impossible to construct a consistent pre-treatment baseline for the primary outcomes. I therefore use NFHS-2 as the only pre-treatment period. However, I use NFHS-1 to assess the plausibility of the econometric assumptions underlying my empirical strategy, for the available variables. I discuss the results and interpretation in [Appendix A](#).

The treated population is Muslim women residing in Gujarat at the time of each survey. The Muslim population constitutes approximately 9% of the state’s population ([Office of the Registrar General and Census Commissioner, India, 2001](#)). They are concentrated mostly in the urban centers including Ahmedabad, Vadodara, and Godhra, and in rural communities across the districts where the violence was most concentrated ([Jaffrelot, 2003](#)). My analysis sample contains 315 treated women in NFHS-2 (the pre-treatment baseline), 274 in NFHS-3 (short-term post), 1,396 in NFHS-4 (medium-term post), and 2,487 in NFHS-5 (long-term post), for a total of 4,157 treated women across all post-treatment rounds in the pooled specification. Since Gujarat is the only treated state, the effective number of clusters for inference is small regardless of the number of women within it. This motivates the inference approach described in [Section 3.4](#). I address the potential concern of the riots causing permanent out-of-state displacement of Gujarati Muslim women in [Section 4.3.3](#).

2.2 Outcome Variables

I construct four categories of outcome variables: (i) decision-making autonomy, (ii) contraception method choice, (iii) employment and (iv) attitudes towards domestic violence, harmonized consistently across all four survey rounds. Full details are in [Appendix A](#).

2.2.1 Intra-household decision-making autonomy

The survey asks the respondent, an ever married woman aged 15–49, whether she has any say in regular decision making processes. These include decisions about her own health care, major household purchases, visits to family and friends, and financial expenditure. For each domain, the survey records four mutually exclusive response categories of (i) woman decides alone, (ii) jointly with husband, (iii) husband decides alone, and (iv) others decide (including in-laws or other family members). For each domain, I construct a binary indicator equal to one if the woman reports having any say in the decision, either alone or jointly with her husband and also estimate separate equations for each of the four mutually exclusive outcomes. These granular categories allow me to trace whether these women lose any decision-making power and to whom that power transfers.

2.2.2 Reproductive autonomy

I construct binary indicators for overall modern contraceptive use and for specific method categories. The key decomposition differentiates between female-controlled modern methods, i.e., oral contraceptive pills, injectable contraceptives, female sterilization, and intrauterine devices, and male-controlled methods, i.e., condoms and male sterilization. Within female-controlled methods, I further separate concealable methods (oral contraceptive pills and injectables, which can be used without their husband’s knowledge) and non-concealable methods (female sterilization and IUDs, which require clinical procedures and partner’s awareness). This decomposition follows [Ashraf, Field, and Lee \(2014\)](#), who show that concealable methods serve as a revealed-preference measure of women’s private reproductive autonomy in conditions where there is a difference in spousal preferences over fertility.

2.2.3 Employment

I define employment as a binary indicator which is equal to one if the respondent reports currently working for cash or in-kind payment at the time of the survey. This includes wage employment, self-employment, and agricultural work, and is consistent with the DHS standard definition across all rounds.³

2.2.4 Attitudes toward domestic violence

The survey asks the respondent whether a husband is justified in hitting his wife for each of four reasons: (i) she goes out without telling him, (ii) she neglects the house or children, (iii) she argues with him, and (iv) she burns the food. I construct a binary indicator for whether a woman justifies domestic violence for any of these reasons, as well as separate indicators for each subcategory. These measure stated attitudes and not incidence of domestic violence, capturing the extent to which women have internalized the norms that support marital violence, which is different from direct exposure to violence ([La Mattina, 2017](#)). I report the aggregate indicator as the primary outcome and show the results for individual subcategories in Table [A.3](#).

³There is an increase in employment among Gujarati Muslim women post-2002. This was mostly concentrated in agricultural work which rose from 34.6% to 51.9% of employed women, while employment in sales dropped from 14.1% to 5.7%, between 1998 and 2005. I do not see any other similar shift in the three other comparison groups (Table [A.2](#)). One way to interpret this is that the destruction of Muslim-owned businesses during the riots, and the boycott that followed ([Chatterjee, 2006](#)), pushed women out of commercial work and into agriculture. Occupation subcategories are not available for the full analysis sample in NFHS-4 and NFHS-5 due to a split questionnaire design.

2.2.5 Fertility preferences

Erosion of women’s bargaining power can also affect their preferences over family composition. This could either be because women’s own preferences shift under conditions of increased insecurity or because the bargaining power has shifted to the husbands. Reduced bargaining power may allow husband preferences to dominate household fertility decisions, and those preferences tend to be more son-biased than women’s (Mishra and Parasnis, 2021). I construct four measures of stated fertility preferences from the birth history module (i) the ideal number of boys, (ii) the ideal number of girls, (iii) a binary indicator for son preference (ideal boys exceeding ideal girls), and (iv) a binary indicator for whether the woman wants more children. Responses coded as ”up to God” or ”other” (code 96) are set to missing.⁴ These outcomes are analyzed as secondary outcomes in section 4 to examine whether the riots shifted stated preferences over family composition in addition to behavioral outcomes. I do not include them in the summary statistics or balance tables, since they report only the primary outcome domains.

The collective model sheds light on which domains are affected by a shock to women’s economic fallback options. Decision-making autonomy and reproductive control are the primary channels, while employment and domestic violence attitudes are secondary outcomes which I include to test the boundaries of the mechanism. The distinction between primary outcomes (decision-making autonomy, reproductive control) and secondary outcomes (employment, domestic violence attitudes) follows the collective household bargaining model (Chiappori, 1992), in which bargaining power is determined by outside options. The further decomposition into concealable and non-concealable contraceptive methods follows Ashraf, Field, and Lee (2014), who show that concealable methods serve as a private mechanism for women to act on preferences their husbands do not observe.

Table A.1 reports summary statistics for all outcome variables across all four survey rounds in the analysis sample. Tables 1 and 2 report the pre-treatment means by group (Gujarat Muslim, Gujarat Hindu, other-state Muslim, other-state Hindu) in NFHS-2 (1998–99), showing the broad comparability across groups on the primary outcome variables before the riots.

2.3 Control Variables

All regressions control for individual-level demographic characteristics including age (in years), age squared, number of children ever born, educational attainment (four categories), sex of household head, husband’s educational attainment, and urban or rural residence.

⁴This non-response is not random. Gujarati Muslim women became significantly more likely to give a non-numeric response after the riots, especially in the short term. I construct a separate binary indicator for this pattern and report it as an outcome in its own right (Section 4.1.5). Stated fertility preferences carry known measurement error more broadly (Bankole and Westoff, 1998), that supports treating this non-response pattern as substantive rather than as missing data to be discarded.

These controls are available and consistently coded across all four survey rounds. I discuss the choice of controls, including the exclusion of income and wealth controls, in Section 3. I apply survey sampling weights in all specifications to account for the stratified sampling design of the survey.

3 Empirical Analysis

In the following section, I discuss the main identification strategy, the identification assumptions and inference.

3.1 Primary Identification Strategy

I use a pooled triple difference-in-differences (DDD) design (Gruber, 1994; Olden and Møen, 2022) to estimate the causal effect of the 2002 Gujarat riots on the intra-household bargaining power and reproductive autonomy of already-married Muslim women in Gujarat. The violence spread in 16 out of 24 districts of Gujarat (Jaffrelot, 2003) instead of in isolated pockets. As a result, the whole state of Gujarat experienced the broad economic and social consequences of the riots in 2002, and Muslim women nationally may have experienced differential impacts unrelated to the direct effects of the riots. I use state boundary as the source of identification instead of within-Gujarat district-level variation in riot intensity, since Jha (2014) shows that local historical and electoral factors shape riot intensity within Gujarat.

The triple-difference design addresses these issues simultaneously and compares the riot-specific impacts on Muslim women with the (i) Hindu women in Gujarat, (ii) Muslim women in other Indian states, and (iii) Hindu women in other Indian states. A standard difference-in-differences comparing Gujarat to other states would have combined the riot-specific effects on the targeted minority with any Gujarat-wide shock or nation-wide Muslim specific shock.

The main estimating equation is:

$$Y_{ist} = \beta (\text{Gujarat}_s \times \text{Muslim}_i \times \text{Post}_t) + \gamma X_{ist} + \alpha_{s,r(i)} + \delta_{t,r(i)} + \phi_{st} + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is the outcome of interest for an individual woman i residing in state s in survey round t , where $r(i) \in \{\text{Muslim, non-Muslim}\}$, denoting the religion group of woman i . Post_t is a binary indicator, equal to one for all post-riot rounds (NFHS-3 in 2005–06, NFHS-4 in 2015–16, and NFHS-5 in 2019–21), and equal to zero for the pre-treatment baseline round (NFHS-2 in 1998–99). Gujarat_s is a binary indicator equal to one if the woman i resides in Gujarat and zero otherwise. Muslim_i is a binary indicator equal to

one if the woman i identifies as Muslim and zero for all other religious groups (Hindu, Christian, Sikh, and others).^{5,6}

X_{ist} is a vector of individual and household-level controls which includes age (in years), age squared, number of children ever born, educational attainment,⁷ sex of the household head, husband’s educational attainment,⁸ and urban or rural residence. These controls absorb the individual-level heterogeneity that could influence the treatment effect if the demographic composition of the Gujarati Muslim population changed differentially after the riots.^{9,10} These variables are consistently available and are comparably coded across all three survey rounds. I do not include controls that are available only in later survey rounds, since their inclusion would make the estimated effects incomparable across the short-, medium-, and long-term horizons. I exclude income and wealth controls because these are potential mediators of the treatment effect as the riots directly destroyed economic assets of Muslims. ε_{ist} is an idiosyncratic error term.

The coefficient of interest, β , captures the differential post-riot change in the outcomes for Muslim women in Gujarat, relative to the change experienced by non-Muslim women in Gujarat, Muslim women in other states, and non-Muslim women in other states. This estimator is valid under the identifying assumption that, in the absence of the riots, Muslim women in Gujarat would have followed the same trend as non-Muslim women in Gujarat and Muslim women in other states.

The treatment group consists of Muslim women who are currently married and are residing in Gujarat at the time of each survey. There are 315 women in NFHS-2, 274 in

⁵The state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects jointly absorb all main effects and all three two-way interactions (Gujarat $\times$ Muslim, Muslim $\times$ Post, and Gujarat $\times$ Post), so β is identified entirely from the triple interaction.

⁶Gujarat experienced its own trajectory of political and economic change over the study period, shifts in the state’s industrial policy, and broader economic growth, which is independent of the 2002 riots. Without ϕ_{st} any such state-specific trend common to both Muslim and non-Muslim women in Gujarat would be incorrectly attributed to the Gujarat $\times$ Muslim $\times$ Post interaction. This term absorbs shocks common to all Gujarati women regardless of religion, but it cannot, by construction, distinguish a single shock that affected Muslim and Hindu women with different intensity from two distinct shocks operating through separate channels.

⁷I code educational attainment into four categories following the DHS standard: no education, primary, secondary, and higher education, corresponding to values 0–3 of the DHS variable $v106$.

⁸The NFHS women’s questionnaire (Individual Recode) is administered only to women, so there is no possibility of male respondents contaminating the sample.

⁹For example, if displacement led to selective out-migration of younger or more educated women, the post-riot Gujarati Muslim sample would be very different from the pre-riot baseline which can bias the triple-difference estimate.

¹⁰A pre-treatment balance test (Appendix Table A.4) shows that four of six covariates (children ever born, urban residence, and both respondents’ and husbands’ education) differ significantly across the four *state $\times$ religion* cells beyond what an additive Gujarat effect and Muslim effect would predict, motivating the inclusion of these covariates in my preferred specification rather than relying on unconditional parallel trends. As a robustness check, Appendix Table A.5 reports the pooled triple-DiD estimates without these controls. The sign and statistical significance of the primary decision-making and contraception findings are unchanged; the purchase and visit autonomy coefficients remain negative and significant at conventional levels without controls, as do the contraception and concealable-method estimates.

NFHS-3, 1,396 in NFHS-4, and 2,487 in NFHS-5.¹¹ The pooled specification uses all four rounds simultaneously, with a total analysis sample of 1,254,746 observations. The treated group contributes 4,472 observations in total across all rounds, of which 4,157 are in the three post-treatment rounds. Survey sampling weights are applied in all specifications to account for the stratified sampling design.

3.2 Temporal Dynamics

While the pooled specification in Section 3.1 estimates an average effect across all post-riot rounds, it limits the treatment effect to be same at each time-period. On the other hand, the pairwise specifications compare the pre-riot baseline separately to each post-treatment round, allowing the effects to vary in sign and magnitude over time. The short-term specification (NFHS-2 vs. NFHS-3) asks whether effects emerged within three years of the riots. The medium-term specification (NFHS-2 vs. NFHS-4) examines the persistence of these effects at thirteen years. The long-term specification (NFHS-2 vs. NFHS-5) tests whether the effects survived approximately seventeen to nineteen years after the riots. In each pairwise specification, Post_t is redefined as an indicator for the later round only, whereas state $\times$ religion and round $\times$ religion fixed effects are absorbed as fully interacted terms, consistent with the pooled specification. Survey sampling weights are applied throughout.

3.3 Identification Assumptions and Threats

The parallel trends assumption is untestable and I cannot observe the counterfactual trajectory of Gujarati Muslim women in the absence of the riots. I therefore rely on three indirect tests that are consistent with its validity. I find that the 2010 time placebo shows no differential pre-existing trend. I further use NFHS-1 survey round for available outcomes, the results of which are reported in the Appendix A.

Olden and Møen (2022) show that the triple difference design requires only one parallel trend assumption on the differential between groups, instead of two separate assumptions. The Gujarati Muslim women were already worse off on several dimensions compared to Muslim women in other Indian states before the riots, with lower purchase autonomy (0.755 vs 0.486) and lower visit autonomy (0.649 vs 0.427) (Table 1 and 2).

I test this assumption in multiple ways. First, I replace the treatment date with a fake treatment date of 2010, which is a year between NFHS-3 and NFHS-4 when no comparable violence has occurred in Gujarat. If pre-existing differential trends were

¹¹There are approximately 3,500–23,000 non-Muslim women in each round who share the Gujarat-specific economic and social environment, approximately 10,000–90,000 Muslim women in other Indian states per round, who share the same religious identity and face comparable national-level trends; and finally approximately 170,000–430,000 non-Muslim women in other Indian states per round, who serve as the broad baseline.

driving the results, the placebo coefficient should be very large. If the parallel trends assumption holds, it should be near zero. Second, I examine whether Gujarat experienced a broader state-wide shock affecting all women irrespective of religion, by conducting a standard difference-in-differences comparing Hindu women in Gujarat to Hindu women elsewhere in India before and after 2002. I discuss the results of this comparison, and their implications for interpreting the main effects, in Section 4.3.

Another concern can be that the post-2002 Gujarati Muslim survey respondents may not be representative of the pre-riot population, since the riots displaced over 200,000 Muslims (Srivastava, 2011; Badigar, 2012). If the women who left were very different from those who stayed, the decline in autonomy could partly reflect who remained in Gujarat rather than the actual causal effect. There are two reasons why this is not the main driver of the results. First, Kaur (2007) documents that the most displaced Gujarati Muslims returned home within one to two years, so the medium- and long-run estimates are less affected by this. Second, if the main explanation was the change in composition, the effects should fade over time as displaced women returned and reintegrated into society.

NFHS-1 (1992-93) could also be used as a second pre-treatment period, but it is not a reliable pre-treatment baseline for this analysis. The survey fieldwork was conducted in the immediate aftermath of the 1992–93 Babri Masjid demolition and subsequent communal riots, which produced serious anti-Muslim violence across India including in Gujarat specifically.¹²

A natural robustness check in settings with a single treated unit is the synthetic control method (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010). Ghose and Pandey (2025) implement this approach for the same setting, since age at marriage is retrospectively recorded in a single survey round, giving them approximately twelve pre-treatment years of annual data. My setting does not admit the same approach since my primary outcomes are measured only at the time of each survey interview, with two pre-treatment survey rounds for employment and contraception and only one for decision-making. With fewer pre-treatment periods than donor states, the synthetic control optimization is under-determined and will fit pre-treatment data mechanically. This is an overfitting problem that invalidates inference based on the standard placebo permutation test (Abadie, Diamond, and Hainmueller, 2010).

I therefore rely on randomization inference, wild cluster bootstrap, the 2010 time placebo, and male outcomes falsification as my primary identification robustness checks.¹³

¹²The Babri Masjid mosque in Ayodhya was demolished by Hindu nationalist mobs on December 6, 1992, triggering widespread communal riots across India. Gujarat experienced significant anti-Muslim violence in December 1992 and January 1993, with Surat and Ahmedabad among the worst-affected cities (Jaffrelot, 2003). I report the pre-trends test in Appendix A, where I discuss the interpretation of the results in the context of the triple-difference design.

¹³I have also explored whether the NFHS contraceptive calendar could support a synthetic control for the contraception outcome specifically, but it was not feasible given the available data. The DHS calendar records month-by-month contraceptive use going back five years from the interview date, which could

3.4 Inference with a Single Treated Unit

I cluster the standard errors at the state level throughout the main specification, which accounts for any arbitrary serial correlation and heteroskedasticity within the states over time. With 27 states in the analysis sample, this yields 26-27 clusters. However, with only one treated state, conventional inference is unreliable by construction, and I address this using randomization inference which asks whether Gujarat’s estimated effect is unusual compared to a permutation distribution of placebo assignments. [MacKinnon and Webb \(2018\)](#) show that the wild cluster bootstrap fails in this setting for the same reason, and further recommend randomization inference instead. Randomization inference asks if Gujarat were not special, what would the estimated effect look like if I assigned the treatment to other states at random? I permute the Gujarat indicator across the 26 comparison states, re-estimate the triple-difference coefficient for each permutation, and record what share of these placebo estimates exceed the observed coefficient in absolute values. This permutation distribution serves as the reference against which I assess the observed estimate, using 1,000 replications via the `ritest` command ([Hef, 2017](#)).

One mechanical constraint of this approach is that with only 26 comparison states, the smallest possible p-value is $1/26 \approx 0.038$. Gujarat can only occupy one position in the permutation distribution regardless of how large the true effect is. This implies that the conventional significance thresholds are unreachable by construction, and not because the evidence is weak. I therefore report both wild cluster bootstrap and randomization inference p-values alongside the main results, and treat the consistency of sign and magnitude across all the outcomes and time horizons as the primary basis for assessing the robustness of the findings in place of conventional significance thresholds.¹⁴

To address concerns about multiple hypothesis testing across outcome families, I implement the Benjamini-Krieger-Yekutieli sharpened false discovery rate correction, described in Section 4.1.

4 Results

In this section, I present the paper’s main findings. Section 4.1 reports the pooled triple-difference estimates for decision-making autonomy, reproductive autonomy, attitudes

provide a retrospective pre-treatment time series at the state level. However, NFHS-2 (1998–99) did not collect the calendar module. The survey codebook documents “Calendar: None” and all respondents show no calendar entries in the data. The calendar is available only from NFHS-3 (2005–06) onward, and the NFHS-3 calendar extends back only to approximately January 2001, yielding at most thirteen pre-treatment monthly observations before the February 2002 riots. With twenty-five donor states and fewer pre-treatment periods than donor units, the synthetic control optimization is under-determined and will fit pre-treatment data mechanically by construction, making permutation-based inference uninformative.

¹⁴The findings in this paper should be read as descriptively robust (consistent in sign, magnitude, and timing across ten outcomes and multiple specifications) and not as meeting conventional statistical significance thresholds, which the single-treated-state design cannot achieve by construction.

towards domestic violence, employment, and fertility preferences. Section 4.2 decomposes these effects into short-, medium-, and long-run horizons. Section 4.3 presents robustness checks including a Gujarat-wide shock, pre-existing trends, sample selection from displacement, geographic spillover, and political environment.

4.1 Main Results: Pooled Triple-DiD

There are sharp and persistent effects on two distinct dimensions of Muslim women’s bargaining power and reproductive autonomy within the household. Figure 2 presents the pooled triple-DiD estimates for all primary outcomes. Muslim women in Gujarat experience a 5.8 percentage point ($p < 0.01$) decline in their say about major household purchases, a 3.8 percentage point ($p < 0.01$) decline in decisions about how money will be spent and a 5.3 percentage point ($p < 0.01$) decline in decisions about visiting family and friends after the riots (Table A.6). Muslim women who could jointly make everyday decisions with their partners prior to the riots witnessed that power shift to members outside the conjugal unit. I observe a similar erosion of bargaining power in reproductive autonomy. The use of oral contraceptive pills among these women declined by 2.9 percentage points ($p < 0.01$), alongside a broader 5.3 percentage point decline in female-controlled modern contraception ($p < 0.05$).

4.1.1 Intra-household decision-making autonomy

The household decision-making autonomy declined sharply and broadly across all domains. The effects were most severe in the domains where women’s economic outside options are most directly affected. With pre-riot baseline means of 0.755, 0.649 and 0.936, respectively, Muslim women in Gujarat experienced a 5.8 percentage point decline in purchase decisions, a 5.3 percentage point decline in visit decisions and a 3.8 percentage point decline in money use decisions. Decisions on health show smaller and imprecisely estimated negative effects (Table A.7). After the riots, the decision-making authority shifted to the members outside of the conjugal unit, such as in-laws or extended family members (Figure 3).^{15,16}

¹⁵For purchase decisions, joint decision-making declined by 5.1 percentage points, of which 3.3 pp shifted to husband-alone and 2.5 pp to others. For visit decisions, the shift toward others is even larger, joint decision-making fell by 4.2 pp while the others category rose by 3.9 pp. Healthcare decisions show a weaker pattern, with only the others category significantly increasing (3.5 pp). Involvement of the extended family members restricts rather than supports women’s autonomy, as evidence from rural India shows that co-residence with mothers-in-law significantly reduces daughters-in-law’s mobility and access to social networks (Anukriti et al., 2020).

¹⁶For money decisions, joint decision-making declined by 3.8 percentage points where the authority diffused to other members of the family by 3.8 percentage points, following the same pattern as the purchase and visit domains. The woman-alone category shows no significant change.

4.1.2 Reproductive autonomy

Muslim women lost their reproductive autonomy, through a shift from concealable methods to male-controlled methods. Overall modern contraceptive use declined by 3.7 percentage points ($p < 0.05$), and this effect was driven predominantly by a decline in female-controlled methods. Female-controlled methods declined by 5.3 percentage points ($p < 0.01$) while male-controlled methods (condoms and male sterilization) increased by 1.8 percentage points ($p < 0.05$) (Table A.8). The decline in female-controlled contraception is concentrated mostly in concealable methods. Oral contraceptive pill use declined by 2.9 percentage points ($p < 0.01$), while non-concealable female methods (IUD, female sterilization) show a marginal decline (-2.4 pp, $p=0.086$) (Figure 4). This is because women with some private autonomy use pills secretly. When husbands gained dominance after the riots, obtaining and concealing pills became difficult, since husbands monitor finances more closely, women lose independent income to purchase pills, or social surveillance increases. As Ashraf, Field, and Lee (2014) demonstrate, when there are differences in spousal preferences over fertility, women who have very little to no autonomy use oral contraceptive pills specifically because they can be concealed from their husbands. This decline in concealable methods among Gujarati Muslim women after the riots shows the loss of private reproductive control.

The decline in female-controlled and concealable contraceptive use could reflect disrupted access to contraceptive products rather than a change in intra-household bargaining power. If the riots destroyed pharmacies and family planning outreach infrastructure in Gujarat Muslim communities, pill availability could have declined independently, without any shift in spousal decision-making. There are three arguments against this interpretation. First, if physical access were the sole reason behind this change, all modern contraceptive methods should have declined proportionally since the same supply chains and health facilities distribute both pills and condoms. Instead, female-controlled concealable methods declined sharply while male-controlled methods simultaneously increased. This substitution pattern shows a shift in who controls the fertility decisions within the household. Second, the decision-making autonomy declines in purchases and visits involve no contraceptive products, yet they move together with the contraception pattern in sign, magnitude, and timing. Third, male employment and literacy among Gujarat Muslim men remained comparable to or above comparison groups across all three post-treatment rounds (Table A.9). This rules out the broader community-level infrastructure collapse that a supply disruption story would require. Together, the substitution from female-concealable to male-controlled methods, combined with the decision-making evidence, confirms that women were losing private reproductive autonomy within marriage rather than losing access to contraceptive products.

4.1.3 Attitudes towards domestic violence

I find no significant effects on attitudes towards domestic violence (+0.90 pp, not significant). This null result holds across all four individual justifications for domestic violence reported in Table A.3, not only the aggregate indicator. Given the power constraints associated with the single-treated-state design, I interpret this as suggestive rather than definitive evidence that the riots did not shift the stated gender norms.

4.1.4 Employment and Literacy

The collective model predicts that riots took away Muslim women’s outside economic options but did not change their stated beliefs about the gender norms within the marriage or their access to formal employment (+2.5 pp, not significant). The decline in literacy (−2.4 pp, $p < 0.01$ under conventional clustering) does not survive wild cluster bootstrap ($p=0.452$) or randomization inference ($p=0.644$), despite being significant under conventional clustering. This pattern of results shows that the riots affected women through behavioral channels and not attitudinal ones.

4.1.5 Fertility Preferences

I report triple-difference estimates for (i) the ideal number of boys, (ii) the ideal number of girls, (iii) a binary indicator for son preference (ideal boys exceeding ideal girls), and (iv) a binary indicator for wanting more children. None of the four outcomes show a statistically significant effect.

A related pattern emerges in how women respond to the fertility preference questions themselves. I construct a binary indicator, *uptogod*, equal to one if a woman gives a non-numeric response (“up to God or other”) rather than a specific ideal number of children. Gujarati Muslim women became significantly more likely to give such a response after the riots (2.9 percentage points, $p < 0.01$).

To address concerns about multiple hypothesis testing, I report Anderson (2008) sharpened q-values under both within-family and pooled corrections in Table A.11. The purchase, visit and money autonomy results survive correction at the one percent level within-family, any-contraception survives at the one-percent level under both approaches, and modern contraception survives the within-family correction at the five percent level.

4.2 Temporal Dynamics

The pairwise comparisons show distinct temporal patterns with an immediate and persistent decline in decision-making authority, a gradual erosion of reproductive autonomy, and a brief employment reversal (Table A.12 - A.14).

Purchase and money decisions follow a non-monotonic pattern with purchases declining sharply in the short-term (-8.5 pp, $p < 0.01$) and long-term (-7.9 pp, $p < 0.01$), but become statistically insignificant at the medium-term horizon (-1.1 pp), and money decisions follow an identical pattern (-5.9 pp short-term, $p < 0.05$; -1.5 pp medium-term (not significant); -2.7 pp long-term, $p < 0.05$). The medium-term attenuation coincides with weaker randomization inference evidence where the RI p -value for purchases rises from 0.203 (short-term) to 0.817 (medium-term) before falling again to 0.377 (long-term).¹⁷ Visit decisions are significant at all three horizons (-7.9 pp, -3.3 pp, and -4.4 pp for 2005, 2015 and 2019, respectively, all $p < 0.10$). Health decisions show no effect in 2005 but a significant decline emerges only by the long-term horizon (-3.8 pp, $p < 0.01$) showing a slower decline than others.

The decline in female-concealable contraceptive use was gradual where the effect is small (-0.8 pp) in 2005, but the decline grows and becomes significant by the 2015 (-4.4 pp, $p < 0.01$) and 2019 (-3.6 pp, $p < 0.01$), explaining that household equilibrium adjusts over time rather than shifting abruptly. Employment shows a sharp reversal with a significant short-run spike of 8.0 percentage points ($p < 0.05$), since the households lost income, with an employment suppression by 2015 (-4.4 pp) and eventually returning to a null effect by the long-term horizon ($+1.8$ pp, not significant). Unlike the gradual erosion in contraceptive autonomy, the emergency-driven labor force entry did not translate into durable economic gains.

Literacy shows a significant short-term decline (-3.1 pp, $p < 0.01$) that is null at both 2015 (-1.4 pp) and 2019 (-1.9 pp, not significant at 5% level). This is due to a composition change in the surveyed sample and not because of a lasting effect on women's reading ability.

The preference for a son spiked sharply ($+0.045$, $p < 0.01$) in the short-run, but declined by 2015. This confirms that changes in bargaining power, not beliefs, drive the long-run outcomes. The pairwise specifications reveal a sharp short-run pattern, suggesting no persistent average shift in stated fertility preferences over the full two-decade period. Within three years of the riots, Gujarati Muslim women reported significantly higher ideal numbers of both boys ($+0.164$, $p < 0.01$) and girls ($+0.113$, $p < 0.01$), with the increase in ideal boys substantially larger, producing a significant increase in the probability of son preference ($+0.045$, $p < 0.01$). By 2015, all three effects had fully dissipated to near zero and remain insignificant through 2019 showing no persistent changes in the stated fertility preferences.¹⁸

¹⁷A temporary loss of significance at one horizon, without a change in sign, is not evidence against a persistent effect that is significant at both the near- and long-term horizons.

¹⁸As an additional test of the bargaining power mechanism, I examine whether sex-composition-driven stopping behavior changed for Gujarati Muslim women after 2002, following the parity progression approach standard in the son preference literature (Clark, 2000; Arnold, Choe, and Roy, 1998). Using the NFHS birth histories reshaped to the birth level, I constructed indicators for whether a woman stopped childbearing within three and five years of a given birth, conditional on the sex of her existing

However, "uptogod" shows a persistent effect where the pairwise estimates are significant at all three horizons (2005: 12.2 pp, $p < 0.01$; 2015: 5.4 pp, $p < 0.05$; 2019: 9.6 pp, $p < 0.01$). Hence, women's willingness to state a specific fertility preference was severely and persistently affected by the violence.¹⁹

There are two mechanisms explaining this pattern. First, the post-riot insecurity may have triggered a precautionary demand for larger families with a stronger tilt toward sons since they represent greater economic and protective value in contexts of minority vulnerability. Second, the short-run erosion of women's bargaining power documented in Section 4.1 may have temporarily allowed husband preferences over fertility, which are more son-biased, to dominate household reproductive decision-making.²⁰ The full recovery of stated preferences by 2015, while behavioral outcomes such as contraceptive method choice and decision-making autonomy remain depressed, suggests that stated preferences are more malleable and recover faster than the structural household equilibrium.

4.3 Robustness Checks

I use several checks to ensure that my main results reflect factors other than the specific targeting of Muslim women. These collectively establish that the main effects cannot be attributed to pre-existing trends, geographic spillover, political environment, displacement, or community-level collapse, though I find that Gujarati Hindu women also experienced declines on several dimensions. Two additional checks show that the results are not an artifact of the clustering choice or the inference method, with PSU-level clustering and randomization inference both being consistent with the main findings.

4.3.1 Treatment Effect Heterogeneity by Religious Group

To test whether the effects reflect a Gujarat-wide shock or a shock specific to targeted minorities, I estimate a standard difference-in-differences comparing Hindu women in Gujarat to Hindu women elsewhere in India before and after 2002. Gujarati Hindu women also show significant declines in decision-making and reproductive autonomy, along with increased acceptance of domestic violence (Table 3).

children, restricting to parity 1 and parity 2 births where son-preference stopping decisions are most active. The descriptive evidence does not support a consistent post-2002 strengthening of son-preference stopping behavior among Gujarati Muslim women: the estimated change in the son-daughter stopping gap ranges from -0.016 to $+0.060$ across parity transitions and window lengths, switching sign depending on specification and showing no stable pattern distinguishing Gujarati Muslim women from the comparison groups. Given the moderate cell sizes for this subgroup (700 to 1,000 births per parity-sex cell) and this sensitivity to window choice, I do not interpret this as informative either for or against the hypothesis, and do not pursue a formal regression specification on this outcome.

¹⁹Since women who give a non-numeric response are excluded from the `ideal_boys` and `ideal_girls` regressions at every time-horizon, those estimates are drawn from a sample that persistently under-represents women whose stated preferences were most acutely disrupted.

²⁰These mechanisms are not mutually exclusive, and I cannot distinguish between them with the available data.

This pattern suggests that the estimated disruption to Hindu households is too large to be explained entirely by the targeted destruction of Muslim livelihoods and communities. The magnitude of the Hindu coefficients is comparable to or larger than the Muslim triple-difference estimates in Table A.6 on several outcomes, including purchase decisions, visit decisions, money decisions, health decisions, and modern contraception.²¹

There are two possible explanations for this pattern. First, Hindu husbands' participation in or proximity to the violence, and subsequent legal and social exposure, may have affected Hindu households through channels different from the direct destruction of Muslim-owned businesses and community infrastructure (Jaffrelot, 2003). Second, Gujarat may have experienced a broader post-2002 disruption to household dynamics that is unrelated to violence-participation specifically. Without household-level data on exposure to or participation in the violence, I cannot distinguish between these explanations or determine their relative contribution to the results in Table 3.

4.3.2 Time placebo

To rule out the pre-existing differential trends, I replace the true post-2002 indicator with a fake treatment date of 2010 (Table 4).²²

$$Y_{ist} = \beta_1^F (\text{Gujarat}_s \times \text{Muslim}_i \times \text{FakePost}_t) + \gamma X_{ist} + \alpha_{s,r(i)} + \delta_{t,r(i)} + \phi_{st} + \varepsilon_{ist} \quad (2)$$

For purchase decisions, visit decisions, literacy and modern contraception, the placebo coefficients are small and not significant. For money decisions, health decisions, any contraception, traditional contraception, employment, and domestic violence attitudes, the placebo coefficients are significant.²³

4.3.3 Sample Selection from Displacement

As discussed in Section 3, a potential concern is that post-2002 Gujarati Muslim survey respondents comprise a selected sample of non-migrants, due to the riots potentially causing permanent out-of-state displacement of Muslim women. If the displaced women were selected from households with higher baseline autonomy, my estimates could be

²¹Part of this asymmetry is mechanical, since Table 3 is a standard two-way difference-in-differences that does not net out state-level trends common to both religious groups, unlike the triple-difference specification used for the main results.

²²2010 is a year between NFHS-3 and NFHS-4 during which no comparable communal violence occurred in Gujarat.

²³Since NFHS-2 is the only pre-treatment round, this exercise pools 1998 with 2005 in the pre-period, so it cannot fully distinguish a persistent true effect from a pre-existing trend for these outcomes. I read the significant placebo coefficients as a limitation of this test rather than as evidence against the main results specifically for purchase decisions, visit decisions, and modern contraception, where the placebo is clean.

biased away from zero through a compositional channel, and not the true causal effect.

These concerns are not likely to account for the main results for four reasons. First, Gujarat’s Muslim population grew by 27.3 percent between the 2001 and 2011 Censuses (Office of the Registrar General and Census Commissioner, India, 2001; Office of the Registrar General and Census Commissioner, 2011). This exceeds the national Muslim population growth rate of 24.6 percent over the same period and is inconsistent with large-scale permanent depopulation of the Gujarati Muslim community following the riots.²⁴ Second, Ghose and Pandey (2025) document using 2001 Census migration data that interstate migration rates among Gujarati marriage migrants²⁵ are the lowest in India at 19 percent. Since marriage migrants represent women with the strongest ties to their origin states and thus the greatest capacity for return migration, their low interstate migration rate provides an upper bound on the likelihood of permanent post-riot out-of-state displacement. This suggests that permanent out-of-state relocation was limited even among those who were initially displaced. Third, while more economically resourced households may have a greater capacity for permanent relocation, women with stronger community ties, who may also exhibit higher autonomy, have stronger incentives to remain near home. Fourth, any differential sample selection would be expected to attenuate over time as the displaced populations reintegrated. The long-run stability of the purchase and visit autonomy effects across the three post-treatment rounds is consistent with a persistent causal effect on the women who remained in Gujarat.²⁶

4.3.4 Neighboring States and Political Environment

The effects did not diffuse into the states bordering Gujarat (Rajasthan, Madhya Pradesh, Maharashtra, and Goa). I test whether Muslim women have experienced the spillover threat effects from the proximity of the violence. Across all ten primary outcomes, the neighboring state coefficients are small and statistically indistinguishable from zero, with the only exception of money autonomy (-0.054 , $p < 0.10$) (Table A.15).

I also exclude Gujarat entirely and test whether Muslim women in right-wing ruled states experienced differential post-2002 trends relative to Muslim women in Congress and Left-ruled states, using time-varying political alignment coded from actual state

²⁴The Muslim share of Gujarat’s surveyed population also did not decline over this period, from 8.19% in NFHS-2 (1998–99) to 9.70% in NFHS-5 (2019–21).

²⁵Marriage migrants refers to women who moved to a different state specifically for marriage.

²⁶An alternative approach to address displacement concerns is to restrict the analysis to women who have always lived in their current place of residence, who by definition did not leave after the riots. The variable v104 identifies these women across all four rounds. However, the always-resident Gujarat Muslim treated cell contains only 115 pre-treatment observations and 36 short-run post-treatment observations. Estimates from this subsample are unstable and sign-reversals across outcomes can be considered as small-sample noise and not a true treatment effect.

governments at each survey round.^{27,28} The results show no coherent pattern across the three control group comparisons. The coefficients vary in sign and significance across panels, and no outcome replicates the consistent direction and magnitude of the Gujarat effects. The large negative effects on purchase and visit autonomy and concealable contraception that characterize the main results are not present in BJP states outside Gujarat (Table A.16). This suggests that a generic Hindu nationalist political environment is not the mechanism driving the main findings, though I find a narrower and more tentative pattern on employment and purchase autonomy specifically.²⁹

4.3.5 Male Employment and Literacy

If the riots destroyed Muslim-owned businesses, disrupted schooling, and degraded public services in Gujarat Muslim communities broadly, male employment and literacy would also be expected to decline relative to comparison groups. There is no decline in male outcomes among Gujarat Muslims across nearly two decades of post-treatment observations (Table A.9). Gujarat Muslim men show employment rates of 100, 94, and 93 percent in 2005, 2015, and 2019, respectively. These are comparable to Gujarat Hindu men and above other-state Muslim men at each horizon. Literacy rates follow the same pattern. The effects on women’s decision-making autonomy, reproductive control, and employment reflect specifically gendered channels and not a community-wide economic shock that mechanically dragged women’s outcomes downward.³⁰

²⁷Governing party is a first-order determinant of whether communal violence occurs and how severe it is in India, with Congress-held states historically less prone to Hindu-Muslim rioting due to electoral dependence on Muslim voters (Wilkinson, 2006; Nellis, Weaver, and Rosenzweig, 2016). This is my motivation behind testing whether party control also shaped the post-riot trajectory of women’s outcomes, independent of the Gujarat-specific effect.

²⁸BJP-ruled (right-wing) states by round: Round 1 (1998–99): Himachal Pradesh, Goa, Rajasthan, Madhya Pradesh, and Delhi; Round 2 (2005–06): Himachal Pradesh, Goa, Madhya Pradesh, and Rajasthan; Round 3 (2015–16): Goa, Madhya Pradesh, Rajasthan, Maharashtra, Haryana, and Assam; Round 4 (2019–21): Himachal Pradesh, Goa, Madhya Pradesh, Maharashtra, Haryana, Karnataka, Assam, and Uttar Pradesh. Congress-ruled states include Andhra Pradesh, Karnataka, Kerala, and Maharashtra in early rounds, with Kerala alternating between Congress and Left Front governments across rounds. Left Front states comprise West Bengal and Tripura in rounds 1–3 and Kerala in rounds 2 and 4. Rajasthan is coded as Congress in round 4 following the Congress victory in December 2018; Himachal Pradesh is coded as Congress in rounds 3 and 4 following the Congress victory in December 2012 and 2017, respectively.

²⁹Beyond ruling out a generic BJP effect, my analysis has a suggestive finding with implications which are beyond the specific context of the 2002 violence. Muslim women in BJP-ruled states experienced systematically lower employment and purchase autonomy after 2002 compared to the Muslim women in Congress and Left-ruled states across all three control group comparisons, which is consistent with a broader structural relationship between Hindu nationalist governance and Muslim women’s intra-household bargaining power and reproductive autonomy. This finding should be interpreted cautiously. The time-varying political alignment analysis cannot disentangle the direct effect of BJP governance from selection into BJP governance, or from the possibility that the post-2002 national political climate had differential effects on Muslim women across all states regardless of state-level governance.

³⁰The NFHS men’s individual recode is not available for NFHS-2 (1998–99), which was conducted before the men’s questionnaire was introduced in NFHS-3. Because no pre-treatment baseline exists for men, these comparisons are purely descriptive and cannot be used to establish a causal effect of the riots

4.3.6 Inference Robustness: Wild Cluster Bootstrap and Randomization Inference

The wild cluster bootstrap (WCB) p-values and randomization inference (RI) p-values with the main pooled coefficients for all the primary outcomes do not cross the conventional $p < 0.05$ threshold. However, this may reflect the mechanical constraints of both approaches in a single treated unit setting and not the weakness of the evidence, as discussed in Section 3.4. The lowest WCB p-values are for purchase autonomy ($p = 0.25$) and visit autonomy ($p = 0.36$), and the lowest RI p-value is for purchase autonomy at the short-term horizon ($p = 0.20$).

The WCB is known to be unreliable with a single treated cluster (MacKinnon and Webb, 2018), and the RI permutation distribution is discrete with a minimum achievable p-value of $1/26 \approx 0.038$.³¹ I therefore do not interpret the failure to cross a conventional threshold under WCB or RI as evidence against either the main findings or the placebo tests specifically. Instead, the purchase and visit autonomy effects are the largest in magnitude among all outcomes and rank lowest in both WCB and RI p-values consistently across all four specifications. The signs of all primary decision-making and contraception effects are negative across all three time horizons, and the placebo tests give precisely estimated zeros under conventional clustering throughout.

4.3.7 Alternative Clustering

All main specifications cluster standard errors at the state level. As a robustness check, I re-estimate the pooled specification with standard errors clustered at the primary sampling unit (PSU) level with 28,000 clusters (Table A.18). The outcomes that are significant under state clustering remain significant, and those that are not remain null, showing that the pattern remains unchanged. Hence, the main findings are independent of the state-level clustering choice.

5 Conclusion

This paper asks whether targeted communal violence erodes the intra-household bargaining power and reproductive autonomy of already-married women in affected minority

on male outcomes. The Gujarat Muslim male cell contains 85 observations in NFHS-3, 263 in NFHS-4, and 339 in NFHS-5. The comparison serves as contextual evidence against the general infrastructure hypothesis rather than as a formal identification test.

³¹"precisely estimated" and "statistically significant by WCB or RI standards" are distinct claims. A conventional standard error close to zero indicates the estimator has low sampling variability under standard asymptotic theory, while WCB and RI are stricter about how the estimate compares to a small-sample permutation or bootstrap distribution. With only 26–27 clusters, this stricter standard is harder to satisfy for any estimate, including the placebo coefficients discussed above; the same gap between conventional and WCB/RI p-values appears for my main effects in Table A.17, not only for the placebo tests.

communities. The 2002 Gujarat riots produced a significant and long-lasting erosion of women's bargaining power and autonomy within marriage in the affected state, most persistently among Muslim women. Gujarati Muslim women experienced a 5.8 percentage point decline in purchase decision-making and a 5.3 percentage point decline in decisions about visiting family and friends. The erosion of decision-making authority was not only a simple transfer from wife to husband but also a fragmentation of the conjugal unit, with authority diffusing outward to the extended family and in-laws. In the reproductive domain, the decline in modern contraceptive use was driven by a decline in concealable methods (oral contraceptive pills) while male-controlled methods rose. As a result, Muslim women lost their ability to manage fertility. I find no detectable effects on domestic violence attitudes across any horizon, though I find a short-run employment spike that reversed into suppression by 2015, confirming the collective model's prediction that the bargaining position is determined by resources and outside options rather than by beliefs alone.

The riots compressed women's economic outside options by destroying assets, severing community social capital, and intensifying protective restrictions on mobility, but did not shift the gender norms. These effects persisted for nearly two decades and survived economic and demographic change showing that loss of intra-household bargaining power after a severe shock does not automatically return.

Several limitations point to directions for future research. The cross-sectional structure of the NFHS prevents me from tracking individual women over time. The analysis is also limited to currently married women, and the consequences for never-married, divorced, or widowed women are not captured. Future research can explore the inter-generational transmission of the bargaining power shock. Women who experienced the most severe erosion of autonomy in the post-riot period were also making decisions about children's nutrition, healthcare, and schooling during this time. If reduced bargaining power constrained women's ability to invest in their children's human capital, the consequences of the 2002 riots may extend beyond the women directly affected to the next generation.

The 2002 Gujarat riots killed approximately 1,000 to 2,000 people within the span of a few weeks. This paper documents its effects on the women who survived, and on the women who came of age and married in the two decades that followed. These effects were measured not in weeks but in years, in the silent erosion of the ability to decide where to go, how to spend money, and whether to use a contraceptive pill. The riots diminished the bargaining power and shaped the health and schooling investments mothers could make for their children, extending the impact to a generation that was not yet born in 2002. These results show that the costs of communal violence extend beyond immediate deaths and destruction of property, and reduce women's welfare along multiple dimensions that are often overlooked.

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Figures and Tables

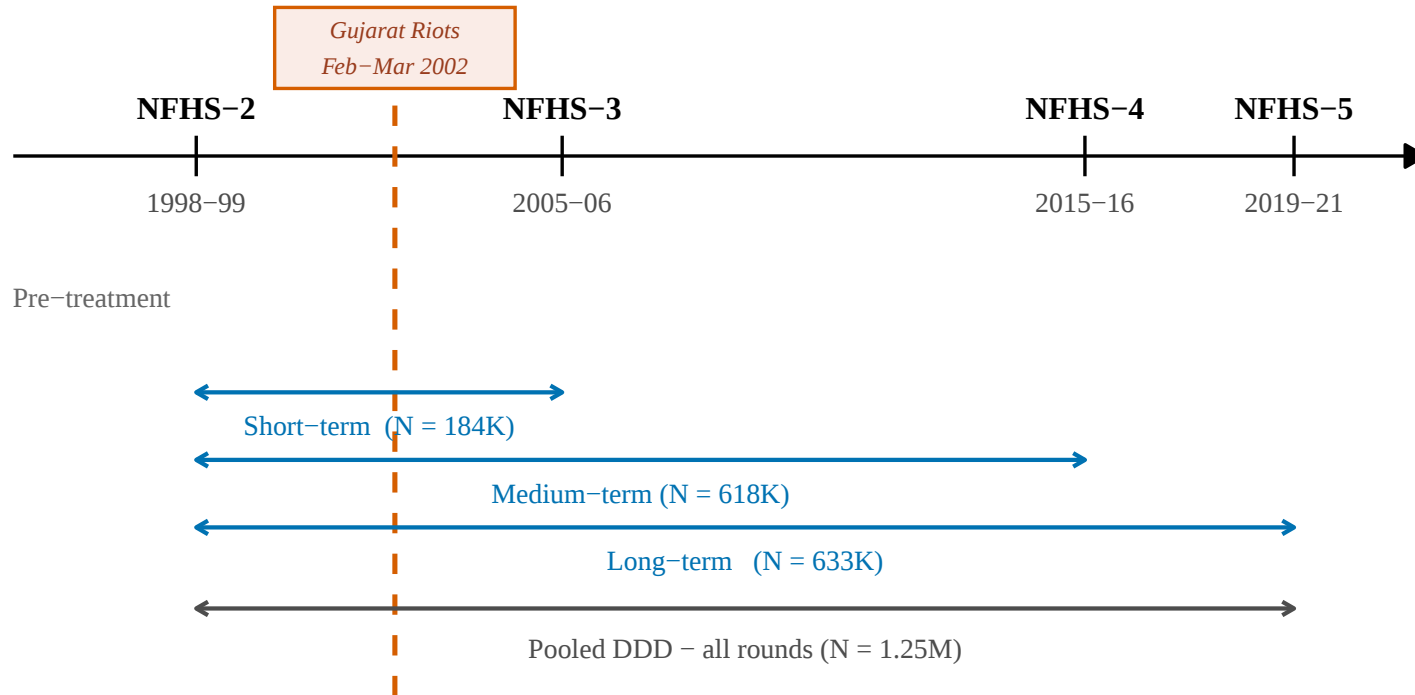


Figure 1: Study Design and Survey Timeline

Notes: The figure illustrates the triple difference-in-differences identification strategy. The 2002 Gujarat riots serve as the quasi-natural experiment. NFHS-2 (1998-99) provides the pre-treatment baseline. Short-, medium-, and long-term specifications compare NFHS-2 to each subsequent round; the pooled specification uses all four rounds. Sample sizes reflect women currently married at time of survey

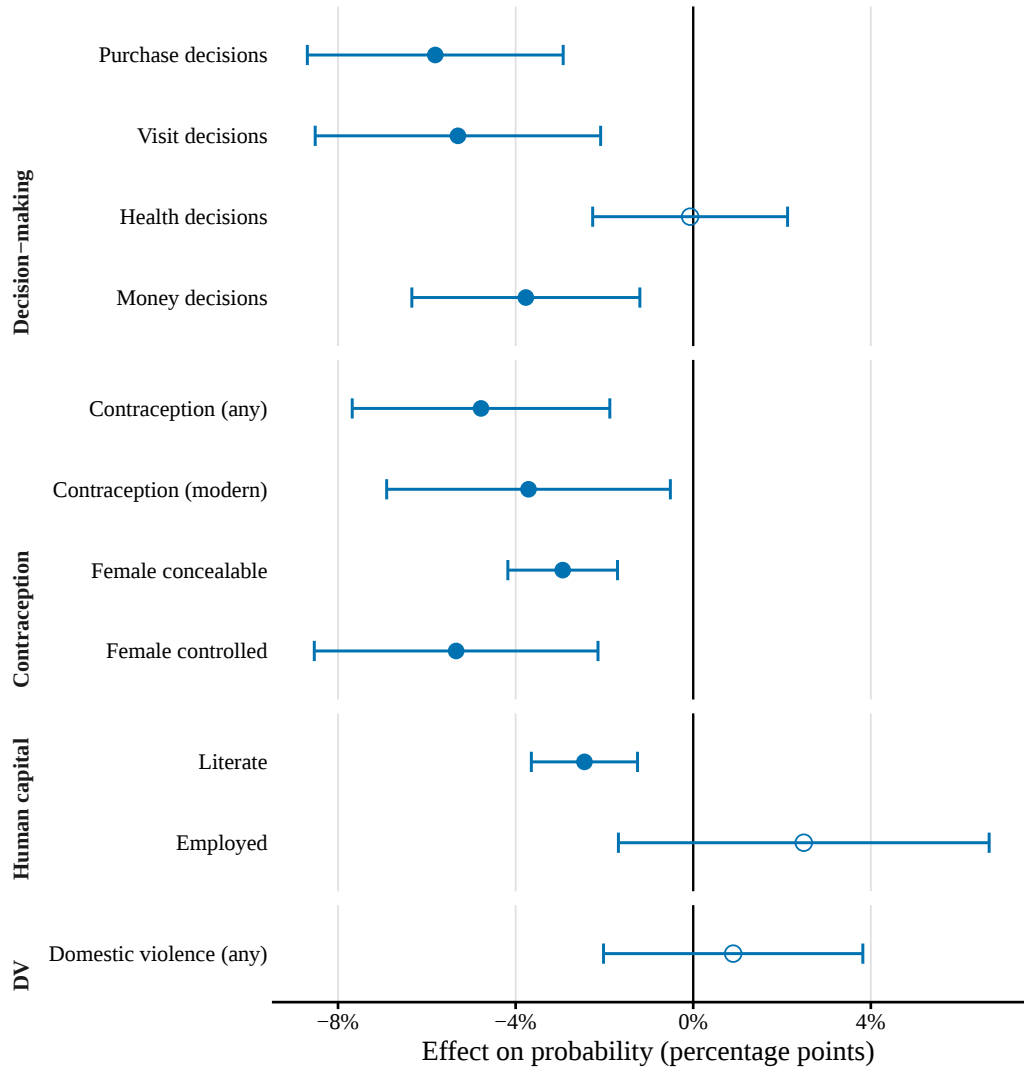


Figure 2: Effects on Women's Autonomy: Primary Outcomes

Notes: Purchase and visit decisions measure whether the respondent has any say (alone or jointly) in major household purchases and in visits to family or friends, respectively. Health and money decisions measure the same for her own healthcare and for household financial expenditure. Female concealable and female controlled contraception refer to methods usable without a husband's knowledge (oral pills, injectables) versus the broader set of female-managed modern methods (adding IUDs and sterilization). Literate, employed, and domestic violence (any) are binary indicators for literacy, current employment for pay, and whether the respondent justifies domestic violence for at least one of four stated reasons. Pooled triple-DiD estimates from Equation (1). Each row reports the coefficient on $\text{Gujarat} \times \text{Muslim} \times \text{Post}$ for the indicated outcome. Filled circles indicate significance at the 5% level; open circles indicate insignificance. Confidence intervals are based on standard errors clustered at the state level.

N = 1,254,746.

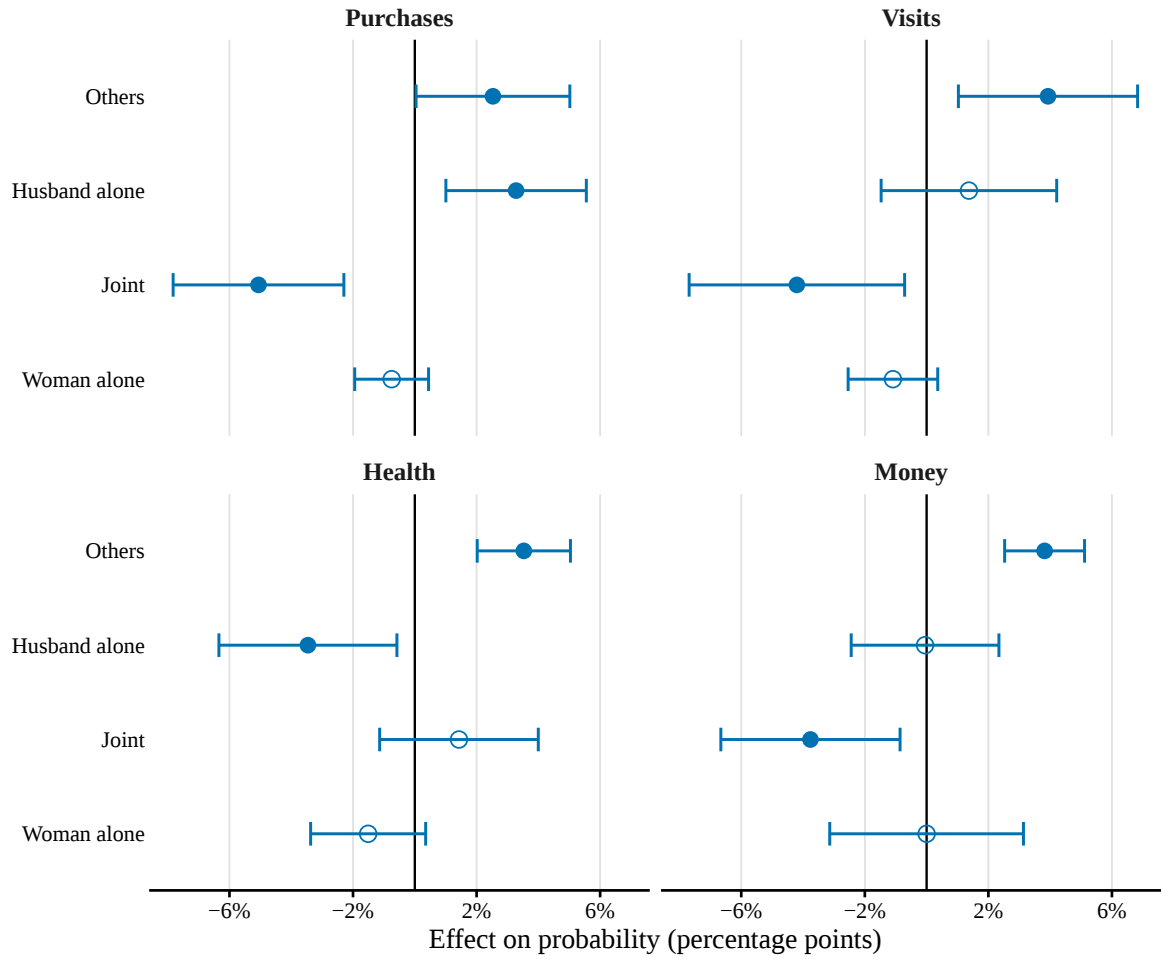


Figure 3: Decision-Making Authority: Who Decides by Domain

Notes: Purchases and visits refer to decisions over major household expenditures and visits to family or friends outside the household; health and money refer to decisions over the respondent's own healthcare and general household financial expenditure. 'Others' refers to authority held by extended family members such as in-laws, rather than the respondent or her husband. Pooled triple-DiD estimates from Equation (1). Each panel reports effects on the probability that each decision type is made by the woman alone, jointly with her husband, by the husband alone, or by others. Filled circles indicate significance at the 5% level. Confidence intervals based on state-level clustered standard errors.

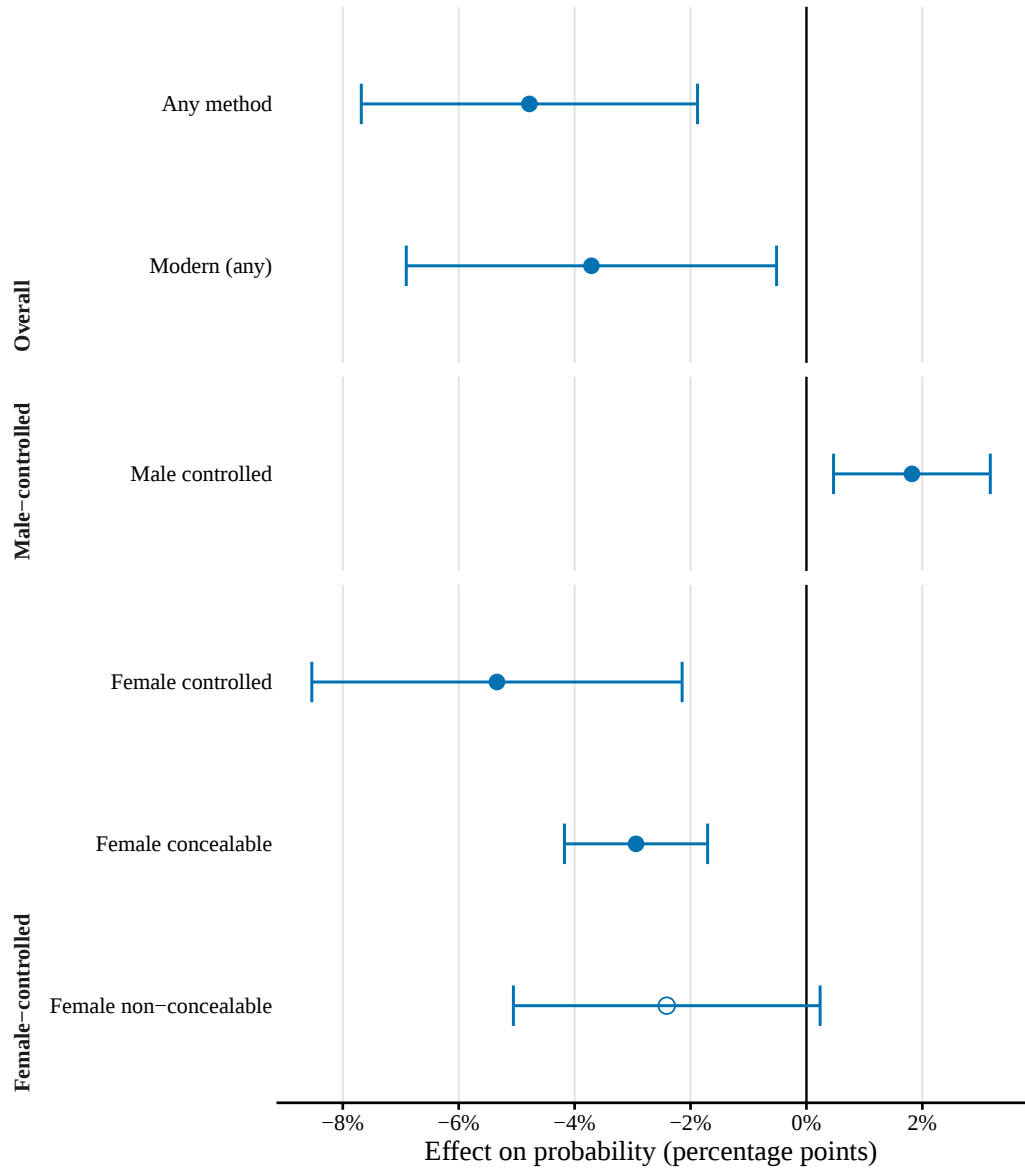


Figure 4: Contraceptive Use: Decomposition by Method Type

Notes: Male-controlled methods comprise condoms and male sterilization. Female-controlled methods comprise oral pills, injectables, IUDs, and female sterilization; female concealable methods (pills and injectables) can be used without a husband's knowledge, while female non-concealable methods (IUDs, sterilization) require clinical procedures and typically partner awareness. Pooled triple-DiD estimates from Equation (1). Outcomes are decomposed by who controls the method: male-controlled (condom), female-controlled (pill, IUD, sterilization), and female concealable (injectable, implant). Filled circles indicate significance at the 5% level. Confidence intervals based on state-level clustered standard errors.

N = 357,244.

Table 1: Pre-Treatment Means by Group, 1998–99 (NFHS-2): Full Decomposition (Part A)

	Guj. Muslim (1)	Guj. Hindu (2)	Other Muslim (3)	Other Hindu (4)	Diff. (1)–(3) (5)	Full Sample (6)
<i>Panel A: Human capital</i>						
Literate	0.331	0.171	0.204	0.169	0.127***	0.174
Employed	0.251	0.510	0.194	0.393	0.057	0.374
<i>Panel B: Decision-making autonomy (any say)</i>						
Purchase decisions	0.755	0.734	0.472	0.522	0.283***	0.526
Visit decisions	0.649	0.651	0.428	0.479	0.222***	0.481
Health decisions	0.717	0.714	0.498	0.506	0.219***	0.516
Money decisions	0.936	0.841	0.735	0.683	0.201***	0.696
<i>Panel B2: Decision-making decomposition</i>						
<i>Purchase decisions</i>						
Woman alone	0.086	0.071	0.116	0.108	-0.031	0.107
Jointly with husband	0.669	0.663	0.356	0.414	0.313***	0.420
Husband alone	0.147	0.169	0.388	0.341	-0.241***	0.339
Others	0.098	0.097	0.140	0.137	-0.041**	0.135
<i>Visit decisions</i>						
Woman alone	0.190	0.165	0.131	0.139	0.059***	0.139
Jointly with husband	0.460	0.486	0.297	0.340	0.163***	0.342
Husband alone	0.296	0.287	0.445	0.399	-0.149***	0.399
Others	0.054	0.062	0.128	0.122	-0.073***	0.120
<i>Health decisions</i>						
Woman alone	0.374	0.333	0.294	0.276	0.079**	0.281
Jointly with husband	0.343	0.381	0.204	0.231	0.139***	0.235
Husband alone	0.255	0.234	0.406	0.400	-0.151***	0.393
Others	0.029	0.052	0.096	0.093	-0.067***	0.092
<i>Money decisions</i>						
Woman alone	0.515	0.425	0.537	0.400	-0.022	0.412
Jointly with husband	0.421	0.416	0.198	0.283	0.224***	0.284
Husband alone	0.048	0.126	0.238	0.285	-0.191***	0.272
Others	0.016	0.033	0.027	0.032	-0.011	0.031
<i>Panel C: Contraception</i>						
Modern contraception (any)	0.495	0.500	0.276	0.414	0.219***	0.402
Female-controlled (any)	0.422	0.448	0.230	0.368	0.192***	0.355
Concealable methods	0.044	0.011	0.038	0.017	0.006	0.020
Non-concealable methods	0.378	0.437	0.192	0.351	0.186***	0.335
Male-controlled (any)	0.073	0.052	0.046	0.046	0.027*	0.047
Observations	315	3530	10460	75915		90220

Notes: Means from NFHS-2 (1998–99), restricted to currently married women aged 15–49. Survey sampling weights applied throughout. Column (5) reports the difference between columns (1) and (3); stars from an unweighted two-sample t – test : * < 0.10 , ** < 0.05 , *** < 0.01 . Decision-making decomposition categories in Panel B2 are mutually exclusive and exhaustive within each domain and sum to one. Concealable methods comprise oral contraceptive pills and injectables. Non-concealable female methods comprise IUDs and female sterilization. Male-controlled methods comprise condoms and male sterilization. Continued in Table A1b.

Table 2: Pre-Treatment Means by Group, 1998–99 (NFHS-2): Full Decomposition (Part B)

	Guj. Muslim (1)	Guj. Hindu (2)	Other Muslim (3)	Other Hindu (4)	Diff. (1)–(3) (5)	Full Sample (6)
<i>Panel D: Domestic violence attitudes</i>						
Justifies DV (any)	0.238	0.296	0.510	0.518	-0.272***	0.506
Goes out without telling	0.177	0.217	0.390	0.373	-0.213***	0.367
Neglects children/house	0.186	0.229	0.396	0.413	-0.210***	0.402
Argues with husband	0.122	0.154	0.345	0.351	-0.223***	0.340
Burns the food	0.081	0.119	0.239	0.256	-0.158***	0.247
<i>Panel E: Demographics</i>						
Age	31.437	31.984	30.244	31.064	1.193*	31.007
Children ever born	3.028	2.847	3.528	2.920	-0.499***	2.991
Observations	315	3530	10460	75915		90220

Notes: Continuation of Table A1a. Means from NFHS-2 (1998–99), restricted to currently married women aged 15–49. Survey sampling weights applied throughout. Column (5) reports the difference between columns (1) and (3); stars from an unweighted two-sample *t*-test : * < 0.10, ** < 0.05, *** < 0.01.

Table 3: Treatment Effect Heterogeneity by Religious Group

	DV Any	DV None	Literate	Employed	Contra. Any	Contra. Mod.	Contra. Trad.	Health	Purchases	Visits	Money
Gujarat $\times$ Post	0.103** (0.044)	-0.103** (0.044)	0.003 (0.006)	-0.039* (0.021)	-0.102*** (0.017)	-0.104*** (0.007)	0.002 (0.012)	-0.190*** (0.022)	-0.199*** (0.026)	-0.106*** (0.022)	-0.168*** (0.016)
Pre-treatment Mean	0.301	0.699	0.170	0.517	0.553	0.501	0.053	0.712	0.732	0.650	0.841
Sample Mean	0.422	0.578	0.521	0.325	0.565	0.489	0.076	0.680	0.658	0.673	0.735
Observations	267,645	267,645	246,892	270,681	270,688	270,688	270,688	259,189	259,182	259,168	202,137

Notes: Standard two-way difference-in-differences, restricted to Hindu women only. Compares Gujarati Hindu women to Hindu women elsewhere in India, before and after 2002. Fixed effects: state, round, and state $\times$ round. Standard errors clustered at state level. If the main triple-difference results reflect targeting of Muslims specifically rather than a generic Gujarat-wide shock, Gujarat $\times$ Post should show a different pattern here than the Muslim-specific results in the main analysis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Placebo Test: Fake Treatment Year (Gujarat $\times$ Muslim $\times$ Post-2010)

	(1) DV Any	(2) Literate	(3) Employed	(4) Contra. Any	(5) Contra. Mod.	(6) Contra. Trad.	(7) Health	(8) Purchases	(9) Visits	(10) Money
triple_fake	0.018* (0.009)	0.005 (0.007)	-0.066*** (0.011)	-0.051*** (0.008)	-0.018 (0.013)	-0.034*** (0.008)	-0.020** (0.010)	0.011 (0.014)	0.010 (0.011)	0.027** (0.011)
Observations	351,869	325,643	357,230	357,244	357,244	357,244	341,548	341,540	341,516	267,467
R-squared	0.143	0.786	0.153	0.201	0.189	0.070	0.105	0.116	0.129	0.045

Notes: Fake treatment assigned to 2010 using all four survey rounds (NFHS-2 through NFHS-5). The triple interaction Gujarat $\times$ Muslim $\times$ Post-2010 tests whether the main results reflect the 2002 riots rather than a spurious post-2010 trend. Fixed effects: state $\times$ Muslim, round $\times$ Muslim, and state $\times$ round. Standard errors clustered at the state level. * < 0.10 , ** < 0.05 , *** < 0.01 .

A Appendix

Table A.1: Summary Statistics

	Mean	SD	Min	Max
<i>Panel A: Human capital</i>				
Literate	0.600	0.490	0	1
Employed	0.313	0.464	0	1
<i>Panel B: Decision-making autonomy</i>				
Purchase decisions (any)	0.667	0.471	0	1
Visit decisions (any)	0.683	0.465	0	1
Health decisions (any)	0.691	0.462	0	1
Money decisions (any)	0.739	0.439	0	1
<i>Panel C: Contraception</i>				
Any method	0.560	0.496	0	1
Modern methods	0.475	0.499	0	1
Female concealable	0.045	0.208	0	1
Female controlled	0.398	0.489	0	1
Male controlled	0.073	0.261	0	1
<i>Panel D: Domestic violence</i>				
Justifies DV (any)	0.431	0.495	0	1
<i>Panel E: Demographics</i>				
Age	33.324	8.539	15	49
Children ever born	2.481	1.679	0	18
Observations	1,254,746			

Notes: Summary statistics computed from all four NFHS rounds (1998–99, 2005–06, 2015–16, 2019–21), restricted to currently married women aged 15–49 in the analysis sample. All variables are binary indicators except age (years) and children ever born (count).

Table A.2: Occupation Type of Employed Women by Group: NFHS-2 (1998) and NFHS-3 (2005)

	Gujarat Muslim	Gujarat Hindu	Other Muslim	Other Hindu
<i>Panel A: NFHS-2 (1998–99, pre-riots)</i>				
Professional/technical/managerial	0.0%	4.9%	4.9%	7.3%
Clerical	1.2%	1.5%	0.7%	2.3%
Sales	14.5%	3.7%	3.8%	4.7%
Agricultural	33.7%	70.7%	49.5%	62.2%
Services/household and domestic	12.0%	4.7%	3.6%	4.3%
Manual (skilled and unskilled)	38.6%	14.5%	37.5%	19.2%
<i>N employed</i>	83	1855	2485	28699
<i>Panel B: NFHS-3 (2005–06, post-riots)</i>				
Professional/technical/managerial	1.9%	5.8%	5.6%	8.4%
Clerical	0.0%	0.7%	1.1%	3.0%
Sales	5.6%	3.5%	5.2%	6.4%
Agricultural	53.3%	61.3%	39.6%	53.0%
Services/household and domestic	8.4%	7.6%	9.5%	9.4%
Manual (skilled and unskilled)	30.8%	21.1%	39.0%	19.9%
<i>N employed</i>	107	1443	3115	34234

Notes: Shares are column percentages among employed women (*occ_type* > 0) within each group and round. Occupation type is harmonized from V717 across rounds. NFHS-4 (2015–16) and NFHS-5 (2019–21) are excluded from this table because the employment module was administered to only 15–18% of the analysis sample in those rounds due to a split questionnaire design, making the subsample non-representative of the full analysis sample. Agricultural includes both self-employed cultivators and agricultural labourers. Manual includes both skilled and unskilled workers. Services/household includes domestic workers, cooks, cleaners, and similar occupations.

Table A.3: Pooled DDD: Domestic Violence Attitude Subcategories

	Justifies DV if wife. . .					Does not justify DV
	Any	Goes out	Neglects	Argues	Burns food	
Gujarat $\times$ Muslim $\times$ Post	0.009 (0.015)	0.011 (0.012)	-0.015 (0.014)	-0.029* (0.015)	-0.015 (0.011)	-0.009 (0.015)
Outcome mean	0.431	0.270	0.328	0.279	0.184	0.569
Observations	351,869	355,255	355,408	354,523	355,161	351,869

Notes: Pooled triple-DiD estimates. Each column is a separate binary outcome equal to 1 if the respondent justifies domestic violence for the stated reason. *Goes out*: wife goes out without permission. *Neglects*: wife neglects house or children. *Argues*: wife argues with husband. *Burns food*: wife burns food. *Any*: justifies DV for any of the above reasons. *Does not justify DV*: inverse composite (justifies for none). Fixed effects: state $\times$ Muslim, round $\times$ Muslim, and state $\times$ round. Standard errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Pre-Treatment Covariate Balance Across State $\times$ Religion Cells (NFHS-2)

	(1) Age	(2) Children Ever Born	(3) Urban	(4) Secondary+ Educ.	(5) Husband Secondary+ Educ.	(6) Female HH Head
Gujarat $\times$ Muslim	0.273 (0.280)	-0.427*** (0.084)	-0.279*** (0.068)	0.082** (0.033)	0.167*** (0.040)	-0.013 (0.016)
Observations	90,220	90,220	90,220	90,197	90,003	90,220

Notes: Each column reports the coefficient on Gujarat $\times$ Muslim from a separate regression of the indicated covariate on Gujarat, Muslim, and their interaction, restricted to NFHS-2 (1998–99, pre-treatment) and weighted by survey weights. The interaction coefficient tests whether Gujarati Muslim women differ from an additive combination of a Gujarat effect and a Muslim effect on each covariate. Secondary+ Education is a binary indicator for secondary schooling or higher. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Triple-DiD Estimates Without Covariate Controls

	(1) Purchase	(2) Visit	(3) Health	(4) Money	(5) Contra Any	(6) Female Ctrl	(7) Concealable	(8) Male Ctrl	(9) Employed	(10) Literate	(11) DV Any
Gujarat $\times$ Post $\times$ Muslim	-0.073*** (0.016)	-0.065*** (0.017)	-0.011 (0.012)	-0.039*** (0.013)	-0.064*** (0.014)	-0.063*** (0.015)	-0.033*** (0.007)	0.013** (0.005)	0.052*** (0.017)	-0.050*** (0.015)	0.025* (0.014)
Pre-treatment Mean	0.755	0.649	0.717	0.936	0.542	0.422	0.044	0.073	0.251	0.331	0.238
Observations	341,540	341,516	341,548	267,467	357,244	357,244	357,244	357,244	357,230	325,643	351,869

Notes: Pooled triple-DiD coefficient on Gujarat $\times$ Muslim $\times$ Post for each outcome, estimated without individual-level controls. All specifications include state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects and survey weights. Standard errors clustered at the state level in parentheses.

Table A.6: Pooled DDD (All Rounds): Primary Outcomes

	(1) DV Any	(2) Literate	(3) Employed	(4) Contra Any	(5) Contra Mod	(6) Contra Trad	(7) Health	(8) Purchases	(9) Visits	(10) Money
Gujarat $\times$ Post $\times$ Muslim	0.009 (0.015)	-0.024*** (0.006)	0.025 (0.021)	-0.048*** (0.015)	-0.037** (0.016)	-0.011* (0.006)	-0.001 (0.011)	-0.058*** (0.015)	-0.053*** (0.016)	-0.038*** (0.013)
Pre-treatment Mean	0.238	0.331	0.251	0.542	0.495	0.047	0.717	0.755	0.649	0.936
Observations	351,869	325,643	357,230	357,244	357,244	357,244	341,548	341,540	341,516	267,467
R-squared	0.143	0.786	0.153	0.201	0.189	0.070	0.105	0.116	0.129	0.045

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions include state $\times$ Muslim, round $\times$ Muslim, and state $\times$ round fixed effects and control for age, age², children ever born, education, household head sex, husband education, and urban/rural residence. Survey weights applied.

Table A.7: Decision-Making Subcategories: Who Decides (Pooled DDD)

	(1) Woman Alone	(2) Jointly	(3) Husband Alone	(4) Others
<i>Panel A: Healthcare decisions</i>				
Gujarat $\times$ Post $\times$ Muslim	-0.015 (0.009)	0.014 (0.013)	-0.035** (0.015)	0.035*** (0.008)
Pre-treatment Mean	0.374	0.343	0.255	0.029
Observations	341,548	341,548	341,548	341,548
R-squared	0.106	0.170	0.066	0.090
<i>Panel B: Purchase decisions</i>				
Gujarat $\times$ Post $\times$ Muslim	-0.007 (0.006)	-0.051*** (0.014)	0.033*** (0.012)	0.025* (0.013)
Pre-treatment Mean	0.086	0.669	0.147	0.098
Observations	341,540	341,540	341,540	341,540
R-squared	0.066	0.108	0.062	0.136
<i>Panel C: Visits outside home</i>				
Gujarat $\times$ Post $\times$ Muslim	-0.011 (0.007)	-0.042** (0.018)	0.014 (0.015)	0.039** (0.015)
Pre-treatment Mean	0.190	0.460	0.296	0.054
Observations	341,516	341,516	341,516	341,516
R-squared	0.066	0.125	0.073	0.133
<i>Panel D: Money use decisions</i>				
Gujarat $\times$ Post $\times$ Muslim	-0.000 (0.016)	-0.038** (0.015)	-0.001 (0.012)	0.038*** (0.007)
Pre-treatment Mean	0.515	0.421	0.048	0.016
Observations	267,467	267,467	267,467	267,467
R-squared	0.141	0.079	0.030	0.079

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions absorb state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects, weighted by survey weights. Treatment group: Gujarat Muslim women (N=4,472). Each panel reports the DDD coefficient for who makes decisions in that domain: woman alone, jointly with husband, husband alone, or someone else. Panels share a common column structure.

Table A.8: Contraception Method Choice (Pooled)

	(1) Any User	(2) Male Control	(3) Female Control	(4) Concealable	(5) Non-Conceal	(6) Traditional
Gujarat $\times$ Muslim $\times$ After2002 (DDD)	-0.046*** (0.015)	0.018** (0.007)	-0.053*** (0.016)	-0.029*** (0.006)	-0.024* (0.013)	-0.011* (0.005)
Pre-treatment Mean	0.542	0.073	0.422	0.044	0.378	0.047
Observations	357,244	357,244	357,244	357,244	357,244	357,244
R-squared	0.199	0.073	0.210	0.089	0.234	0.070

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions absorb state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects, weighted by survey weights. Treatment group: Gujarat Muslim women (N=4,472). Dependent variables: (1) uses any method; (2) male-controlled (condom/vasectomy); (3) female-controlled modern; (4) concealable (pill); (5) non-concealable (IUD/sterilization); (6) traditional.

Table A.9: Male Employment and Literacy by Group: Post-Treatment Rounds

Group	Currently Employed (%)			Literate (%)		
	2005–06	2015–16	2019–21	2005–06	2015–16	2019–21
<i>Panel A: Means (weighted)</i>						
Gujarat Muslim	100.0	93.9	92.6	91.7	86.1	81.0
Gujarat Hindu	99.0	93.7	93.3	78.6	85.7	82.9
Other-State Muslim	97.3	91.1	91.6	67.4	73.6	73.4
Other-State Hindu	97.0	90.9	90.3	78.4	81.8	81.3
<i>Panel B: Sample sizes (unweighted)</i>						
Gujarat Muslim	85	263	339	84	259	337
Gujarat Hindu	791	3267	2794	786	3237	2789
Other-State Muslim	4930	7942	6253	4868	7891	6213
Other-State Hindu	34755	50619	45894	34624	50338	45654

Notes: Means for currently married men aged 15–49 from the NFHS men’s individual recode files. No men’s recode is available for NFHS-2 (1998–99), so all three rounds shown are post-treatment. Employment is coded 1 if the respondent reports currently working (mv714). Literacy is coded 1 if the respondent can read at all (mv155 ∈ {1,2}). Gujarat Muslim men show employment and literacy rates comparable to or above all comparison groups across all three post-treatment rounds, inconsistent with a general community-level economic or infrastructure collapse. The documented effects on women’s outcomes therefore reflect specifically gendered rather than general economic channels. Means are survey-weighted; sample sizes are unweighted.

Table A.10: Fertility Preferences: Pooled DDD

	(1) Ideal Boys	(2) Ideal Girls	(3) Son Preference	(4) Wants More	(5) Up to God
Gujarat $\times$ Post $\times$ Muslim	0.032 (0.039)	0.002 (0.026)	0.005 (0.014)	0.004 (0.015)	0.091*** (0.019)
Pre-treatment Mean	1.176	0.812	0.308	0.282	0.055
Observations	348,782	348,789	348,781	348,967	357,157
R-squared	0.277	0.150	0.125	0.418	0.081

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions absorb state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects, weighted by survey weights. Treatment group: Gujarat Muslim women (N=4,472). Ideal boys and ideal girls are continuous outcomes (numeric responses only; code 96 = other/up to God excluded). Son preference is a binary indicator equal to one if ideal boys exceeds ideal girls. Wants more is a binary indicator equal to one if the woman desires another child. Up to God is a binary indicator equal to one if the respondent gave a non-numeric response (code 96 or 99) instead of a specific ideal number of children.

Table A.11: Multiple Testing Correction: Anderson Sharpened q -Values

	p -value	Family	q -value (within-family)	q -value (all 10 pooled)
<i>Panel A: Decision-making autonomy</i>				
Health decisions	0.947	Family 1	0.947	0.947
Purchase decisions	0.001***	Family 1	0.002***	0.003***
Visit decisions	0.003***	Family 1	0.007***	0.008***
Money decisions	0.008***	Family 1	0.010**	0.015**
<i>Panel B: Contraception</i>				
Modern contraception (any)	0.003***	Family 2	0.010***	0.008***
Modern contraception	0.031**	Family 2	0.046**	0.052*
Traditional contraception	0.083*	Family 2	0.083*	0.118
<i>Panel C: Secondary outcomes</i>				
Literate	0.000***	—	—	0.003***
Employed	0.254	—	—	0.318
Justifies DV (any)	0.553	—	—	0.614

Notes: p -values are from the pooled triple-difference specification (Gujarat $\times$ Muslim $\times$ Post coefficient), clustered at the state level with 26 degrees of freedom. q -values implement the Benjamini-Krieger-Yekutieli sharpened false discovery rate correction (Anderson, 2008) at $\alpha = 0.05$. Within-family correction applies separately to the decision-making family (Family 1, 4 outcomes) and the contraception family (Family 2, 3 outcomes), consistent with the theoretical grouping in Section 2. The all-10 pooled correction applies a single correction across all primary outcomes excluding `dv_none`. Secondary outcomes in Panel C are not included in the within-family correction given their role as falsification checks; pooled q -values are reported for completeness. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Short-term Effects (1998 vs. 2005): Primary Outcomes

	(1) DV Any	(2) Literate	(3) Employed	(4) Contra Any	(5) Contra Mod	(6) Contra Trad	(7) Health	(8) Purchases	(9) Visits	(10) Money
Gujarat $\times$ Post $\times$ Muslim	-0.007 (0.022)	-0.031*** (0.007)	0.080** (0.031)	-0.016 (0.020)	-0.031** (0.015)	0.016* (0.008)	0.026* (0.015)	-0.085*** (0.017)	-0.079*** (0.024)	-0.059** (0.021)
Pre-treatment Mean	0.238	0.331	0.251	0.542	0.495	0.047	0.717	0.755	0.649	0.936
Observations	180,432	153,213	183,690	183,704	183,704	183,704	177,827	177,819	177,795	107,515
R-squared	0.142	0.794	0.190	0.226	0.214	0.076	0.083	0.079	0.098	0.065

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions absorb state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects and control for age, age², children ever born, education, household head sex, husband education, and urban/rural residence.

Table A.13: Medium-term Effects (1998 vs. 2015): Primary Outcomes

	(1) DV Any	(2) Literate	(3) Employed	(4) Contra Any	(5) Contra Mod	(6) Contra Trad	(7) Health	(8) Purchases	(9) Visits	(10) Money
Gujarat $\times$ Post $\times$ Muslim	0.010 (0.014)	-0.014 (0.008)	-0.044* (0.022)	-0.043*** (0.015)	-0.033* (0.017)	-0.009* (0.005)	0.012 (0.012)	-0.011 (0.019)	-0.033* (0.018)	-0.015 (0.009)
Pre-treatment Mean	0.238	0.331	0.251	0.542	0.495	0.047	0.717	0.755	0.649	0.936
Observations	179,340	151,497	181,899	181,913	181,913	181,913	176,781	176,777	176,751	106,419
R-squared	0.151	0.794	0.171	0.203	0.191	0.065	0.108	0.099	0.125	0.034

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions absorb state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects and control for age, age², children ever born, education, household head sex, husband education, and urban/rural residence.

Table A.14: Long-term Effects (1998 vs. 2019): Primary Outcomes

	(1) DV Any	(2) Literate	(3) Employed	(4) Contra Any	(5) Contra Mod	(6) Contra Trad	(7) Health	(8) Purchases	(9) Visits	(10) Money
Gujarat $\times$ Post $\times$ Muslim	0.023* (0.014)	-0.019* (0.011)	0.018 (0.018)	-0.085*** (0.013)	-0.043** (0.020)	-0.042*** (0.011)	-0.038*** (0.012)	-0.079*** (0.015)	-0.044*** (0.014)	-0.027** (0.013)
Pre-treatment Mean	0.238	0.331	0.251	0.542	0.495	0.047	0.717	0.755	0.649	0.936
Observations	169,653	141,203	171,573	171,587	171,587	171,587	166,880	166,876	166,850	96,651
R-squared	0.178	0.735	0.172	0.217	0.195	0.068	0.147	0.131	0.163	0.044

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions absorb state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects and control for age, age², children ever born, education, household head sex, husband education, and urban/rural residence.

Table A.15: Geographic Spillover: Muslim Women in States Adjacent to Gujarat

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	DV Any	Literate	Employed	Contra. Any	Contra. Mod.	Contra. Trad.	Health	Purchases	Visits	Money
Neighbor $\times$ Muslim $\times$ Post	0.029 (0.029)	-0.027 (0.027)	0.028 (0.029)	0.011 (0.020)	0.009 (0.021)	0.002 (0.008)	-0.013 (0.017)	-0.022 (0.021)	0.005 (0.026)	-0.038* (0.022)
Pre-treatment Mean	0.591	0.232	0.246	0.391	0.381	0.011	0.451	0.466	0.395	0.744
Sample Mean	0.436	0.531	0.309	0.541	0.463	0.079	0.689	0.665	0.680	0.739
Observations	336,499	311,733	341,765	341,779	341,779	341,779	326,640	326,632	326,610	255,531
Clusters	26	26	26	26	26	26	26	26	26	26

Notes: Pooled triple-DiD estimates excluding Gujarat. Treatment group: Muslim women in states geographically adjacent to Gujarat (Rajasthan, Madhya Pradesh, Maharashtra, Goa). Control group: Muslim women in all other Indian states. Fixed effects: state $\times$ Muslim, round $\times$ Muslim, and state $\times$ round. Standard errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

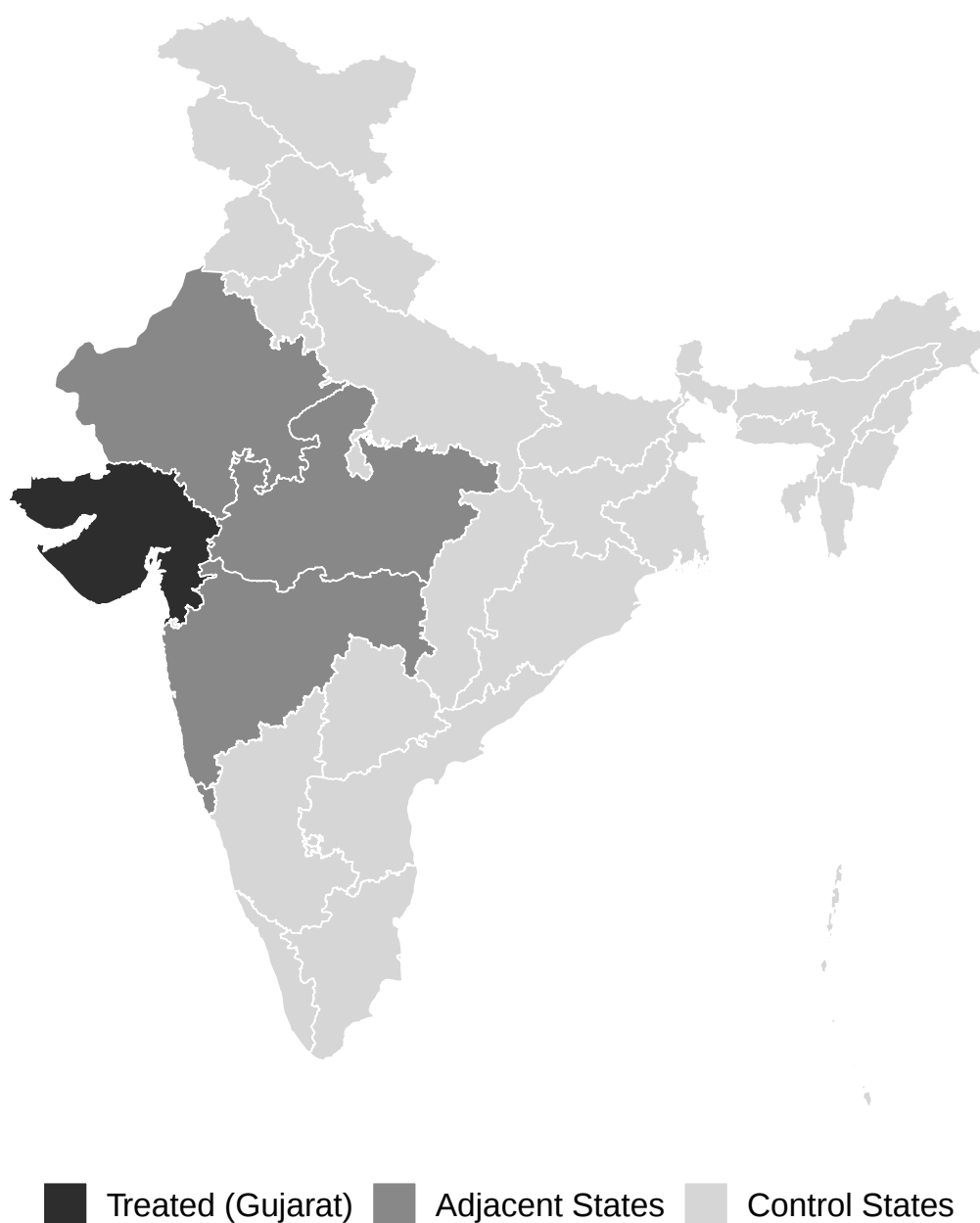


Figure A.1: Geographic Distribution of Treatment and Spillover States

Notes: Gujarat (dark) is the treated state. Adjacent states (medium gray) Rajasthan, Madhya Pradesh, Maharashtra, and Goa share a land border with Gujarat and are used in the geographic spillover robustness check (Table [A.15](#)). All remaining states serve as controls.

Table A.16: Political Environment and Muslim Women's Autonomy (Excl. Gujarat)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	DV Any	Literate	Employed	Contra. Any	Contra. Mod.	Contra. Trad.	Health	Purchases	Visits	Money
<i>Panel A: BJP states vs. all non-BJP states</i>										
BJP $\times$ Muslim $\times$ Post	0.017 (0.016)	-0.003 (0.014)	-0.008 (0.023)	0.002 (0.012)	0.010 (0.018)	-0.008 (0.013)	-0.002 (0.011)	-0.008 (0.014)	-0.003 (0.012)	0.014 (0.015)
Pre-treatment Mean	0.465	0.171	0.263	0.343	0.326	0.017	0.443	0.466	0.393	0.779
Sample Mean	0.436	0.531	0.309	0.541	0.463	0.079	0.689	0.665	0.680	0.739
Observations	336,499	311,733	341,765	341,779	341,779	341,779	326,640	326,632	326,610	255,531
Clusters	26	26	26	26	26	26	26	26	26	26
<i>Panel B: BJP states vs. Congress states</i>										
BJP $\times$ Muslim $\times$ Post	0.045** (0.019)	-0.029* (0.015)	-0.076*** (0.011)	-0.020 (0.028)	0.004 (0.034)	-0.025** (0.010)	0.005 (0.017)	0.029 (0.029)	0.026 (0.019)	0.022 (0.024)
Pre-treatment Mean	0.465	0.171	0.263	0.343	0.326	0.017	0.443	0.466	0.393	0.779
Sample Mean	0.410	0.521	0.334	0.578	0.511	0.068	0.682	0.659	0.671	0.731
Observations	145,451	132,342	147,493	147,496	147,496	147,496	141,158	141,149	141,147	109,089
Clusters	15	15	15	15	15	15	15	15	15	15
<i>Panel C: BJP states vs. Left Front states</i>										
BJP $\times$ Muslim $\times$ Post	-0.064 (0.065)	-0.013 (0.018)	-0.105*** (0.032)	0.041 (0.027)	0.043 (0.025)	-0.002 (0.007)	-0.028* (0.013)	-0.051** (0.018)	-0.042 (0.025)	-0.057*** (0.009)
Pre-treatment Mean	0.465	0.171	0.263	0.343	0.326	0.017	0.443	0.466	0.393	0.779
Sample Mean	0.353	0.550	0.314	0.591	0.506	0.085	0.681	0.659	0.677	0.731
Observations	114,596	107,205	115,759	115,764	115,764	115,764	110,661	110,657	110,655	88,406
Clusters	13	13	13	13	13	13	13	13	13	13

Notes: Time-varying triple-DiD estimates excluding Gujarat. Treatment: Muslim women in BJP-ruled states at time of survey. BJP status varies by round based on actual state government: Round 1 (1998–99): HP, Goa, Rajasthan, MP, Delhi; Round 2 (2005–06): HP, Goa, MP, Rajasthan; Round 3 (2015–16): HP, Goa, MP, Rajasthan, Maharashtra, Haryana, Assam; Round 4 (2019–21): HP, Goa, MP, Maharashtra, Haryana, Karnataka, Assam, UP. Panel A compares BJP states to all non-BJP states. Panel B restricts to BJP and Congress states. Panel C restricts to BJP and Left Front states (West Bengal and Tripura rounds 1–3; Kerala rounds 2 and 4). Fixed effects: state $\times$ Muslim, round $\times$ Muslim, and state $\times$ round. Pre-treatment mean reflects each state's political alignment as of the pre-treatment round (1998–99). Standard errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Inference Robustness: Conventional, Wild Cluster Bootstrap, and Randomization Inference

	Short-term				Medium-term				Long-term				Pooled			
	Coef.	State p	WCB p	RI p	Coef.	State p	WCB p	RI p	Coef.	State p	WCB p	RI p	Coef.	State p	WCB p	RI p
<i>Panel A: Decision-making autonomy</i>																
Purchase decisions	-0.085	0.000	0.454	0.203	-0.011	0.569	0.663	0.817	-0.079	0.000	0.465	0.377	-0.058	0.001	0.416	0.172
Visit decisions	-0.079	0.003	0.459	0.377	-0.033	0.072	0.474	0.527	-0.044	0.005	0.456	0.590	-0.053	0.003	0.453	0.493
Health decisions	0.026	0.091	0.424	1.000	0.012	0.359	0.483	0.922	-0.038	0.003	0.440	0.588	-0.001	0.947	0.971	0.916
Money decisions	-0.059	0.011	0.503	0.522	-0.015	0.127	0.473	0.899	-0.027	0.042	0.501	0.687	-0.038	0.008	0.510	0.693
<i>Panel B: Contraception</i>																
Contraception (any)	-0.016	0.436	0.576	0.674	-0.043	0.009	0.437	0.485	-0.085	0.000	0.450	0.378	-0.048	0.003	0.438	0.334
Contraception (modern)	-0.031	0.041	0.407	0.545	-0.033	0.062	0.448	0.328	-0.043	0.041	0.454	0.685	-0.037	0.031	0.398	0.483
Female concealable	-0.008	0.222	0.514	0.665	-0.044	0.000	0.476	0.300	-0.036	0.001	0.471	0.538	-0.029	0.000	0.390	0.305
Female controlled	-0.040	0.001	0.403	0.402	-0.061	0.006	0.470	0.298	-0.052	0.020	0.457	0.482	-0.053	0.003	0.347	0.380
<i>Panel C: Human capital</i>																
Literate	-0.031	0.000	0.412	0.667	-0.014	0.104	0.386	0.601	-0.019	0.094	0.451	0.695	-0.024	0.000	0.452	0.644
Employed	0.080	0.016	0.430	0.229	-0.044	0.061	0.416	0.849	0.018	0.349	0.532	0.731	0.025	0.254	0.650	0.434
<i>Panel D: Domestic violence</i>																
Domestic violence (any)	-0.007	0.733	0.759	0.849	0.010	0.486	0.576	0.862	0.023	0.099	0.449	0.815	0.009	0.553	0.618	0.745

Notes: Each row reports the triple-DiD coefficient and three inference p-values across four time horizons. State p : two-sided p-value from the main regression with standard errors clustered at the state level (26–27 clusters). WCB p : wild cluster bootstrap p-value (MacKinnon and Webb, 2018), using state $\times$ religion $\times$ round subclusters, Webb weights, and 10,000 replications. RI p : randomization inference p-value (Hek, 2017), permuting the Gujarat treatment indicator across 26 comparison states using 1,000 replications. With a single treated state, the minimum achievable RI p-value is $1/26 \approx 0.038$. The gap between State p and WCB p reflects the small-cluster correction: with 26–27 state clusters the asymptotic normal approximation overstates precision, and the WCB accounts for this. Short-term: 1998 vs. 2005. Medium-term: 1998 vs. 2015. Long-term: 1998 vs. 2019. Pooled: all rounds.

Table A.18: Robustness: PSU-Level Clustering – Primary Outcomes (Pooled)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	DV Any	Literate	Employed	Contra Any	Contra Mod	Contra Trad	Health	Purchases	Visits	Money
Gujarat $\times$ Post $\times$ Muslim	0.009 (0.048)	-0.024 (0.031)	0.025 (0.046)	-0.048 (0.047)	-0.037 (0.042)	-0.011 (0.021)	-0.001 (0.076)	-0.058 (0.044)	-0.053 (0.049)	-0.038 (0.046)
Pre-treatment Mean	0.238	0.331	0.251	0.542	0.495	0.047	0.717	0.755	0.649	0.936
Observations	351,869	325,643	357,230	357,244	357,244	357,244	341,548	341,540	341,516	267,467
R-squared	0.143	0.786	0.153	0.201	0.189	0.070	0.105	0.116	0.129	0.045

Notes: Standard errors clustered at the PSU level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions absorb state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects, weighted by survey weights. All regressions control for age, age², children ever born, education, household head sex, husband education, and urban/rural residence. Survey weights applied.

Pre-Trend Evidence Using NFHS-1 (1992–93)

Table A.19 reports pre-trend estimates using NFHS-1 (1992–93) as a second pre-treatment period, stacking it with NFHS-2 (1998–99) and estimating the triple-difference coefficient on $\text{Gujarat} \times \text{Muslim} \times \text{Post}(1998)$. This tests whether Gujarati Muslim women were already on a differential trajectory relative to comparison groups before the 2002 riots.

Three features of this design require clarification before interpreting the results. First, the decision-making autonomy module (s511) was not collected in NFHS-1, so the primary outcomes of this paper (purchase autonomy, visit autonomy, health decisions, and money decisions) cannot be tested here. The pre-trend evidence is hence limited to employment, modern contraception, concealable contraception, and literacy. Second, the Babri Masjid demolition in December 1992 and subsequent communal riots were a national event affecting Muslim communities across India. Since the triple-difference design absorbs national Muslim-specific shocks via $\text{round} \times \text{religion}$ fixed effects, $\hat{\beta}_1$ identifies only the differential Gujarat-specific pre-trend for Muslim women and not the nationwide Babri Masjid effect. Third, NFHS-1 fieldwork overlapped directly with the 1992–93 violence, meaning the 1992–93 data itself may already reflect a post-Babri Masjid shock rather than a clean pre-treatment baseline.

The results in Table A.19 show significant pre-trends for employment (-0.109 , $p < 0.05$), literacy (-0.086 , $p < 0.01$), and concealable contraception ($+0.042$, $p < 0.01$), while contraception any is not significant (-0.034 , $p = 0.506$). Two aspects of this pattern are worth noting. The employment and literacy pre-trends are negative and significant, consistent with Gujarat-specific economic discrimination against Muslims that may have predated 2002. This possibility is also documented by Chatterjee (2006) for the post-Babri Masjid period. If anything, this suggests that Gujarati Muslim women were already on a worse economic trajectory before the riots, which would make the post-2002 estimates in the main analysis conservative rather than inflated. The concealable contraception pre-trend is positive and highly significant, meaning Gujarati Muslim women were using *more* pill-based contraception relative to comparison groups between 1992 and 1998, which runs in the opposite direction to the main post-2002 finding of a sharp decline. This reversal strengthens rather than undermines the main result: the riots interrupted a positive pre-trend in reproductive autonomy.

Given these complications, the parallel trends assumption in the main analysis rests primarily on the 2010 time placebo reported in Section 4.3, which tests for differential trends between NFHS-3 (2005) and NFHS-4 (2015) in the absence of comparable violence and finds precisely estimated zeros across all primary outcomes. The pre-trend evidence here is reported for transparency rather than as the primary identification test.

Table A.19: Pre-Trend Test: NFHS-1 (1992–93) vs. NFHS-2 (1998–99)

	(1) Employed	(2) Contra Any	(3) Concealable	(4) Literate
Gujarat $\times$ Muslim $\times$ Post(1998)	-0.116** (0.045)	-0.035 (0.053)	0.040*** (0.005)	-0.098*** (0.014)
Observations	174,252	174,266	174,266	142,175
R-squared	0.213	0.229	0.031	0.807

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions absorb state $\times$ religion, round $\times$ religion, and state $\times$ round fixed effects and control for age, age², children ever born, education, household head sex, husband education, and urban/rural residence. Survey weights applied. The Babri Masjid demolition (December 1992) was a national event; because the triple-difference design absorbs national Muslim-specific shocks via round $\times$ religion fixed effects, $\hat{\beta}_1$ identifies only the differential Gujarat-specific pre-trend for Muslim women. Decision-making autonomy outcomes are not available in NFHS-1 and cannot be tested here.

Data Harmonization Across NFHS Rounds

The analysis pools together four cross-sectional rounds of the NFHS (1998–99, 2005–06, 2015–16, and 2019–21). While all four rounds are part of the DHS program, the questionnaire structure, variable naming conventions, and the response coding differ heavily across the rounds, which I carefully harmonize before pooling.

State codes

Gujarat is coded as state 6 in NFHS-2, state 24 in NFHS-3, state 11 in NFHS-4, and state 24 in NFHS-5. All rounds are mapped to a common harmonized state code (Gujarat = 6) based on the NFHS-2 coding scheme. The states created after NFHS-2 by the reorganization of Indian states are merged back into their predecessor units, such as, Jharkhand into Bihar, Chhattisgarh into Madhya Pradesh, Uttarakhand into Uttar Pradesh, Telangana into Andhra Pradesh, and Puducherry into Tamil Nadu. This gives me 27 harmonized state units which are consistently identified across all four rounds.

Decision-making variables

NFHS-2 uses module *s511* with response categories coded 1 (respondent), 2 (husband), 3 (jointly), 4 (others), and 5 (jointly with others). NFHS-3 through NFHS-5 use module *v743* with response categories coded as 1 (respondent alone), 2 (respondent and husband), 3 (respondent and other person), 4 (husband alone), 5 (someone else), and 6 (other). The primary binary outcome of whether a woman has any say in the decision is harmonized such that in NFHS-2, the codes 1, 3, and 5 indicate any say whereas, in later rounds, codes 1, 2, and 3 indicate any say. The granular decomposition (woman alone, jointly with husband, husband alone, others) maps NFHS-2 codes 1, 3 and 5 together, 2, and 4

respectively to the equivalent categories in the surveys after NFHS-3. I verify that the four mutually exclusive categories sum to one within each round.

Literacy

NFHS-2 uses a binary indicator (*s119* with 0 = cannot read, 1 = can read). NFHS-3 through NFHS-5 use *v155* with values 0 (cannot read), 1 (can read parts of a sentence), 2 (can read whole sentence), 3 (blind/visually impaired), and 4 (no card available). The harmonized binary literacy variable equals one if the respondent can read at all (codes 1 or 2 in later rounds; code 1 in NFHS-2), and is set to missing for codes 3 and 4.

Contraception

The main modern method codes (pill = 1, IUD = 2, condom = 5, female sterilization = 6, male sterilization = 7) are consistently available across all four rounds and form the harmonized contraception analysis. However, several methods introduced only from NFHS-3 onward (injectables (3), diaphragm (4), implants (11), female condom (14), and foam/jelly (15)) are included in the modern contraception indicator for rounds 2–4 but not round 1. Emergency contraception (code 16) is available only in NFHS-5. The granular method-type decomposition (male-controlled, female-controlled concealable, female-controlled non-concealable, traditional) uses only the harmonized codes available across all rounds (1, 2, 5, 6, 7, 8, 9), to ensure comparability.

Cluster identifiers

NFHS-3 uses *s021* rather than *v021* for the primary sampling unit identifier. This is mapped to the common `cluster_id_raw` variable used for clustering standard errors.

Sample sizes

The final pooled analysis sample consists of 1,254,746 currently married women aged 15–49, comprising 90,303 observations from NFHS-2, 93,724 from NFHS-3, 527,889 from NFHS-4, and 542,830 from NFHS-5. The treatment group of Gujarati Muslim women in the analysis sample comprises 315 observations in NFHS-2, 274 in NFHS-3, 1,396 in NFHS-4, and 2,487 in NFHS-5, for a total of 4,472 treated observations across all post-treatment rounds.