**Appendix for “Does Interstate Conflict Affect Attitudes Towards Domestic Minorities? Evidence from India”**

**A1. Sampling Design**

The survey was conducted via telephone in the spring of 2022, with respondents selected to form a nationally representative sample of the adult population in India (with a sample size of n=7,052). The survey was conducted by the polling firm CVoter using computer assisted telephonic interviews (CATI). Due to the coronavirus pandemic, we believed telephonic interviews were preferable to face-to-face surveys to minimize risk of virus transmission. CVoter has a built a nationally representative panel of respondents from phone numbers using random digit dialing (RDD). This has been verified by analyzing the marginals on the sample and comparing to census data.

For our survey, a random sample of 7,052 from the existing large panel was selected and administered the survey. Respondents were randomly, with equal probability, allocated to one treatment group. This results in a nationally representative sample over which we can analyze data.

There were two rounds of piloting to assess understanding and attrition. The first was completed by December 2021. The second was completed in March 2022. Below is a table of the languages in which the survey was fielded in various states in India.

|  |  |
| --- | --- |
| ***Table A1. Languages in Which Survey was Fielded*** | |
| ***States*** | ***Language*** |
| Bihar | Hindi |
| Chhattisgarh | Hindi |
| Delhi | Hindi |
| Haryana | Hindi |
| Himachal Pradesh | Hindi |
| Jammu & Kashmir | Hindi |
| Jharkhand | Hindi |
| Madhya Pradesh | Hindi |
| Rajasthan | Hindi |
| UT | Hindi/Gujrati/Tamil |
| Uttar Pradesh | Hindi |
| Uttarakhand | Hindi |
| Assam | Assamese |
| NE | Assamese/Bangla/ Hindi/ English |
| West Bengal | Bangla |
| Gujarat | Gujarati |
| Goa | Hindi/Marathi |
| Karnataka | Kannada |
| Kerala | Malayalam |
| Maharashtra | Marathi |
| Orissa | Oriya |
| Punjab | Punjabi |
| Tamil Nadu | Tamil |
| Andhra Pradesh | Telugu |
| Telangana | Telugu |

**A2. Ethical Considerations**

Subjects were read a consent form, prior to beginning the survey and a debrief at the end of the survey. Only those above the age of 18 were interviewed. The study qualified for exemption under 45 CFR §46.101(b)(2).

One normative consideration worthy of discussion is whether priming respondents on conflict between India and other countries makes crisis more dangerous than it would otherwise be by broadcasting the potential for such interstate conflict. This would be inconsistent with an emerging norm to “protect the community” (Teele 2014; Johnson 2018) that exists beyond the Belmont Report’s standards of respect, benefice, and justice.

There are four reasons we do not think this risk was high with our survey and that the benefits of the survey outweigh any potential costs. First, there have been recent, high-profile crises between India and Pakistan and India and China, so we are not introducing a potential conflict to the respondents that they might not have otherwise considered. Second, we stressed the hypothetical nature of the vignette. Our treatment was not a selective exposure to information about a real historical episode or the use of news footage; rather, our vignette was written in a way which made clear the hypothetical nature of the scenario and this understanding was checked during pre-tests of the survey. Third, we used a verbal experimental treatment for precision to avoid unintended visual themes that might be associated with imagery or news footage (see Graber 1990). Fourth and finally, understanding the domestic political ramifications of interstate crises is necessary for participants and third-party actors that seek crisis de-escalation, providing an important benefit from such survey experimental work, especially if done in a non-inflammatory way such as ours.

**A3. Experiment Design**

The experimental design includes 8 vignette treatment conditions in a 2x2x2 design and one true control (respondents are randomized into 1 of 9 possible conditions). The table below shows the sources of variation and how they were applied to create 8 treatments and a control (of no vignette). We then provide the text of each experimental manipulation.

|  |  |  |  |
| --- | --- | --- | --- |
| **Table A3. Experimental Design** | | | |
| **Treatment** | **Adversary** | **India’s Action** | **Strategic Circumstances** |
| T1 (N =753) | China | Inaction | Control |
| T2 (N = 768) | Pakistan | Inaction | Control |
| T3 (N =809) | China | Inaction | Strategic Treatment |
| T4 (N = 771) | Pakistan | Inaction | Strategic Treatment |
| T5 (N = 788) | China | Counterattack | Control |
| T6 (N = 784) | Pakistan | Counterattack | Control |
| T7 (N = 794) | China | Counterattack | Strategic Treatment |
| T8 (N = 746) | Pakistan | Counterattack | Strategic Treatment |
| Control (N = 839) | N/A | N/A | N/A |

**Before All Vignettes:** “We would like your opinion on the following hypothetical scenario.”

**Treatment Arms**

**Adversary:**

Crisis vignette involves **Pakistan** or **China**

**Altered Strategic Circumstances (Inserted in below vignette):**

[Note: Given our expectations that most respondents will believe India is more powerful than

Pakistan and equivalently or less powerful than China, we have truncated discussion of the possible treatment to exclude altered strategic circumstances that favor China further or disfavor Pakistan further than the control condition.]

Altered Strategic Circumstances condition: Experts say **China's/Pakistan’s** forces have been

training **far away from/near** the border and might be less/more prepared than India for war.

**Control:** [No text]

**Inaction**

India claims **Pakistan’s/China’s** military forces attacked, killed 20 Indian soldiers, and injured many more in clashes in **Kashmir/Ladakh**. [**Altered Strategic Circumstances/Control**] Prime Minister Modi **chooses not to counterattack immediately, but announces that India will give** **a “befitting reply” at the time and place of its choosing**. How much do you support Modi’s decision to **take no further action at this time**?

**Counterattack**

India claims **Pakistan’s/China’s** military forces attacked, killed 20 Indian soldiers, and injured many more in clashes in **Kashmir/Ladakh**. [**Altered Strategic Circumstances/Control**] Prime Minister Modi orders a counterattack, killing 10 Chinese soldiers but losing 5 more Indian soldiers. How much do you **support Modi’s decision to counterattack**?

**Example vignette (T4 Pakistan/altered strategic circumstances treatment/“befitting reply”)**

India claims Pakistan's military forces attacked, killed 20 Indian soldiers, and injured many more in clashes in Kashmir. Experts say Pakistan's forces have been training near the border and might be more prepared than India for war. Prime Minister Modi chooses not to counterattack immediately, but announces that India will give a “befitting reply” at the time and place of its choosing.

**A4. Descriptive Statistics**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| **Variable** | **N** | **Mean** | **St. Dev.** | **Min.** | **Max.** |
| Muslim | 7,052 | 0.132 | 0.338 | 0 | 1 |
| Female | 7,052 | 1.446 | 0.497 | 1 | 2 |
| Rural | 7,052 | 0.712 | 0.453 | 0 | 1 |
| Age | 7,052 | 38.41 | 13.325 | 18 | 102 |
| BJP Supporter | 7,052 | 0.688 | 0.463 | 0 | 1 |

**A5. Measures of Tolerance**

In our survey, we measured tolerance towards the Muslim minority using two measures. The first asked respondents, “Would you be willing to have a Muslim as a neighbor?” This was measured on a yes/no scale, with respondents also provided the option not to respond (“can’t say”). Measuring tolerance in this manner is widely used in survey research in various contexts beyond India, and was also locally resonant.

The second survey question we use to measure tolerance was, “Do you believe the Muslim population is India in growing too rapidly?”This question too was measured on a yes/no scale. We chose this question primarily for its local salience. As *The Wire notes,* “In the right-wing political parlance, Muslims are labelled as a community that is devoted to doubling its population numbers and occupying land illegally. Such allegations have resulted in the coinage of slurs and terms such as ‘population jihad’, implying a type of coordinated attempt by Muslims to become the majority community in India” (Aswani 2023). More than a year after the survey was fielded, Prime Minister Modi alleged during a campaign event in 2024 that the opposition parties sought to redistribute “wealth to those who have more children, to infiltrators,” both descriptions which were widely taken to refer to India’s Muslim population (Travelli and Raj 2024). As such, we believe that those who answered in the affirmative would be less tolerant of the Muslim minority than those who did not.

This measure has also been used in previous surveys in other contexts. For example, it was used in a 1993 survey of Americans done by Zogby. The language in that question, whether the respondent agreed or disagreed with the proposition, the “Muslim population in the United States is growing too rapidly” is closest to our survey question. He found that only 1/5 Americans (after the first World Trade Center attack) said they agreed, so we had reason to believe the number wouldn't be overwhelmingly high in India since America was not an especially tolerant place when this survey was done. Variants of this measure have been used as a proxy for Islamophobia in the United States in subsequent decades (The Bridge Initiative 2015). More recent psychological work has also used statements asserting the presence of rapid population growth as a prime which appears to be associated with anger and anxiety in conservative respondents (see Bai 2020).

Finally, in our choice of measures, we sought to ensure that our dependent and independent variables were not too close to one another, so that our outcomes did not merely reflect our primes back to us. If we were to ask about Muslims and terrorism, for instance, we were concerned that a prime about a terrorist attack in Kashmir would be too close to our outcome of interest measure. As such, we chose instead measures that have been used to capture Islamophobia or interethnic intolerance in other contexts. If these measures truly were far removed from the instrument, we might expect no effect on either. Instead, we find a modest but measurable increase in stated tolerance in one measure.

**A6. Balance Tests**

**Table A6. Difference in Means (with T-test P-values) between**

**Covariate Mean for Respondents in Survey Version 1 and \_\_\_\_\_\_\_.**

|  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| **Covariates** | **Min, Max** | **Version 2** | **Version 3** | **Version 4** | **Version 5** | **Version 6** | **Version 7** | **Version 8** | **Version 9** |
| ***Muslim*** | (0, 1) | -0.001 | -0.002 | 0.006 | 0.016 | -0.008 | 0.003 | -0.011 | -0.018 |
| ***Female*** | (1, 2) | -0.013 | -0.034 | -0.032 | -0.040 | -0.036 | 0.032 | -0.035 | -0.046\* |
| ***Age*** | (18, 102) | 0.316 | -0.060 | -0.700 | 1.033 | 0.391 | 0.041 | 0.073 | -0.208 |
| ***Rural*** | (0, 1) | -0.019 | 0.029 | -0.008 | -0.029 | -0.013 | 0.025 | 0.001 | -0.023 |
| ***BJP Supporter*** | (0, 1) | 0.004 | 0.001 | -0.011 | -0.028 | 0.016 | -0.012 | 0.019 | -0.001 |
| ***High income*** | (0, 1) | -0.036 | -0.043\* | -0.030 | -0.012 | -0.003 | -0.038 | 0.002 | -0.013 |
| ***High education*** | (0, 1) | -0.059\*\* | -0.030 | -0.009 | -0.024 | -0.021 | -0.001 | -0.003 | 0.007 |
| \* p<0.1, \*\* p<0.05 | | | | | | | | | |

In 56 t-tests, we find 2 variables where the difference in means exceeds the p<0.10 level and 1 additional variable where the difference in means exceeds the p<0.05 level, which is consistent with expectations. Given that this is what we would expect by chance, we can be confident that the randomization succeeded in achieving balance on demographic and partisan covariates.

Given the presence of multiple treatment conditions, we fit a multinomial logistic regression model to the following equation:

The test for joint orthogonality for the model as a whole fails to reject the null (p=0.32).[[1]](#footnote-1) This further contributes to our confidence that randomization succeeded in achieving balance on covariates.

**A7. Main Experimental Results**

***Unwillingness to Have a Muslim Neighbor***

Of non-Muslim respondents who were exposed to crisis vignettes involving China *or* Pakistan, 38.8% stated they were unwilling to have a Muslim neighbor, 4.7 percentage points (p<0.05) less than the 43.5% of respondents in the control condition that expressed this discriminatory view.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Difference-in-Means in Respondent Unwillingness to have Muslim Neighbor, by treatment**  (Scale of 0-1, 0 being “yes” or “can’t say” and 1 being “no” on willingness to have Muslim neighbor)[[2]](#footnote-2) | | | | |
|  | **All crisis vignettes** | **Control** | **Difference-in-Means (SE)** | **P-value** |
| **Unwillingness to have a Muslim neighbor** | 0.388  N=5407 | 0.435  N=715 | -0.047 (0.020) | 0.018 |

36.7% of respondents that received a crisis vignette involving Pakistan said they would not be willing to have a Muslim neighbor compared to 41.4% of respondents receiving either China vignettes or the control condition, a difference in means of 4.7 percentage points (p<0.001 from a two-sided t-test permitting unequal variances).

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Difference-in-Means in Respondent Willingness to have Muslim Neighbor, by treatment**  (Scale of 0-1, 0 being “yes” or “can’t say” and 1 being “no” on willingness to have Muslim neighbor) | | | | |
|  | **All Pakistan vignettes** | **All other treatments** | **Difference-in-Means (SE)** | **P-value** |
| **Unwillingness to have a Muslim neighbor** | 0.367  N=2659 | 0.414  N=3463 | -0.047 (0.013) | 0.0002 |

The difference-in-means for unwillingness to have a Muslim neighbor is 4.2 percentage points (p<0.01) between Pakistan and China vignette conditions, excluding the control condition.

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| --- | --- | --- | --- | --- |
| **Difference-in-Means in Respondent Willingness to have Muslim Neighbor, by treatment**  (Scale of 0-1, 0 being “yes” or “can’t say” and 1 being “no” on willingness to have Muslim neighbor) | | | | |
|  | **All Pakistan vignettes** | **All China**  **vignettes** | **Difference-in-Means (SE)** | **P-value** |
| **Unwillingness to have a Muslim neighbor** | 0.367  N=2659 | 0.409  N=2748 | -0.042 (0.013) | 0.002 |

***Attitudes Towards Muslim Population Growth***

Attitudes toward Muslim population growth were indistinguishable between crisis vignette and control conditions.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Difference-in-Means in Respondent Belief that Muslim Population Growth is Too Rapid, by treatment**  (Scale of 0-1, 0 being “no” or “can’t say” and 1 being “yes” on belief that Muslim population growth is too rapid)[[3]](#footnote-3) | | | | |
|  | **All crisis vignettes** | **Control** | **Difference-in-Means (SE)** | **P-value** |
| **Muslim population growth is too rapid** | 0.808  N=5407 | 0.797  N=715 | 0.012 (0.016) | 0.47 |

Attitudes toward Muslim population growth were indistinguishable between Pakistan crisis vignettes and all other conditions.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Difference-in-Means in Respondent Belief that Muslim Population Growth is Too Rapid, by treatment**  (Scale of 0-1, 0 being “no” or “can’t say” and 1 being “yes” on belief that Muslim population growth is too rapid) | | | | |
|  | **All Pakistan vignettes** | **All other conditions** | **Difference-in-Means (SE)** | **P-value** |
| **Muslim population growth is too rapid** | 0.812  N=2659 | 0.804  N=3463 | 0.009 (0.010) | 0.39 |

Attitudes toward Muslim population growth were indistinguishable between Pakistan crisis vignettes and China crisis vignettes (excluding the control condition).

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Difference-in-Means in Respondent Belief that Muslim Population Growth is Too Rapid, by treatment**  (Scale of 0-1, 0 being “no” or “can’t say” and 1 being “yes” on belief that Muslim population growth is too rapid) | | | | |
|  | **All Pakistan vignettes** | **All China vignettes** | **Difference-in-Means (SE)** | **P-value** |
| **Muslim population growth is too rapid** | 0.812  N=2659 | 0.805  N=2748 | 0.007 (0.011) | 0.51 |

***Willingness to Have a Hindu Neighbor (For Muslim respondents)***

While the sample size is small, Muslim respondents expressed greater willingness to have a Hindu neighbor if they received information about a crisis with Pakistan.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Difference-in-Means in Respondent Willingness to have Hindu Neighbor (for Muslims), by treatment**  (Scale of 0-1, 0 being “yes” or “can’t say” and 1 being “no” on willingness to have Muslim neighbor) | | | | |
|  | **All Pakistan vignettes** | **All other treatments** | **Difference-in-Means (SE)** | **P-value** |
| **Unwillingness to have a Muslim neighbor** | 0.015  N=410 | 0.038  N=520 | -0.024 (0.011) | 0.022 |

**A8. Sub-Group Tests**

We tested for subgroup effects for Modi supporters and non-supporters, BJP supporters and non-supporters, high- and low-income respondents, residents of states with higher-than-average Muslim populations, old and young, residents of northern versus southern states, gender, and higher versus lower than median education. None of these interaction effects reached conventional levels of significance (p<0.10).

We ran a series of OLS regressions with the dichotomous dependent variable of unwillingness to have a Muslim neighbor on a treatment variable that took a value of 1 for all Pakistan vignettes and 0 otherwise, a dummy variable of the sub-group, and the interaction term of the treatment and sub-group dummy.

We are not interested in whether the different sub-groups have distinct (non-experimental) means but rather only if they have distinct heterogenous treatment effects from exposure to the Pakistan vignettes. The key test statistic then is the significance of the coefficient on the interaction term itself. The table below shows the p-values for those 8 interaction terms across the 8 regressions.

|  |  |
| --- | --- |
| **Interaction Term (Pakistan Treatment \* \_\_\_\_)** | **P-value** |
| Modi Supporter | 0.466 |
| BJP Supporter | 0.222 |
| High Income (Rs. 10,000/month family income and above) | 0.273 |
| Residents of States with Above Average Muslim Populations | 0.970 |
| Old (40+ years) | 0.823 |
| Resident of Northern (vs. Southern) State | 0.802 |
| Gender | 0.246 |
| Higher Education (Higher Secondary and Above) | 0.357 |

**A9. Additional Outcome Measure: Reputational Spillover**

In our experiment, we included an additional outcome after the experimental treatments which measured support for Prime Minister Modi. We detail that finding here.

In situations of linked outgroup, it is possible that national leaders will gain reputational benefits from interstate crises that spillover to domestic contexts. The literature on leader reputation in international relations has focused almost exclusively on leader foreign policy statements and behavior (see Lupton 2020 for a recent review). Newer scholarship proposes instead that reputations for resolve can be viewed “at least partially as a leader-level characteristic that persists across domestic and international situations” (Goldfein et al. 2023, 610). While that work shows that domestic acts can have international reputational benefits, we seek to study whether international acts spill over into domestic domains: specifically, whether they affect confidence in a leader to protect against a linked outgroup.

Leaders may garner reputational advantages in confronting external adversaries that redound to their benefit in how citizens perceive them dealing with domestic adversaries. Specifically, since Indian Muslims are perceived as linked to Pakistan, we expect a leader’s external behavior in a crisis with Pakistan will enhance the reputation of that leader in how they deal with Muslim extremists in India. Hence, we hypothesized:

**H:** Respondents that receive information about Prime Minister Modi escalating a hypothetical conflict with Pakistan will express greater confidence in Modi’s willingness to protect Hindus from Muslim extremists than those exposed to an India-China crisis vignette or those exposed to a vignette where Prime Minister Modi de-escalates a hypothetical India-Pakistan crisis.

We found little evidence that an Indian leader that ordered escalation in a crisis with Pakistan—who should ostensibly gain reputational advantages from standing firm in a crisis rather than inaction—gained domestic reputational benefits in the process. Respondents were equally likely to express confidence that Modi would work to protect Hindus from Muslim extremists in India if they received a crisis vignette where Modi ordered a counterattack on Pakistan as they were in other treatment arms (see Figure A1 below).

While we measured respondent belief that Modi could protect Hindus from Muslim extremists after a confrontation with Pakistan as a direct measure of reputational consequences toward an India-based group that might be perceptually linked to Pakistan, we also included other reputational measures. Similar null findings were present for tests of whether Modi gained general reputational advantages from escalatory behavior, as in attitudes towards his willingness to defend against criminals. Respondents were no more confident that Modi would work to defend common people against criminals when exposed to vignettes where Modi ordered a counterattack on Pakistan or China than they were when exposed to vignettes were Modi opted for inaction.

As a known personality, respondents may have been less likely to alter their attitudes toward Prime Minister Modi than they might have been for a comparatively less well-known figure or one with a more dovish reputation. On Bayesian or other grounds, Modi may be established as a “hawk” in the minds of the Indian public so that his actions in a crisis vignette would not meaningfully alter that reputation. As such, our findings on reputational spillover may only apply to perceived “hawks,” who benefit from other public opinion advantages according to prior research (Mattes and Weeks 2019). Here, too, while Modi may be an outlier in this regard, he is hardly alone with many elected leaders perceived as hawks regarding domestic and external out-groups, including Benyamin Netanyahu and Donald Trump in their earlier careers. This suggests the need for future research to establish the external generalizability of these findings in other leadership and situational contexts.

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1. McKenzie (2015) recommends this approach. [↑](#footnote-ref-1)
2. These results are robust to how respondents who “can’t say” are handled. Dropping “can’t say” or testing which respondents affirmatively say they would be willing to have a Muslim neighbor (“yes”) both result in a similar finding of less intolerance/more tolerance for those that receive vignettes vs. control, Pakistan vignettes vs. all other conditions, and Pakistan vignettes vs. China vignettes. [↑](#footnote-ref-2)
3. These results do not depend on how respondents who “can’t say” are handled. Alternatively, tests on which respondents assess Muslim population growth is not too rapid find a similar set of null findings. [↑](#footnote-ref-3)