Online Appendix

Table of Contents

---

[0]Note that the Supplementary Materials uploaded on the CUP site include appendices A to C. The full Online Appendix including appendices D to H can be found on Dataverse at https://doi.org/10.7910/DVN/F8SRTA.

A   Summary Statistics and Balance

Table A1 shows that randomization was effectively implemented, as participants are balanced across treatments on observable characteristics.

Table A1: **Balance Across Treatments**

	(1) Baseline Treatment		(2) Exclusive Treatment		(3) Inclusive Treatment		(4) T-test Difference	
Variable	N	Mean	N	Mean	N	Mean	(1)-(2)	(1)-(3)
% Female (vs. Male)	532	0.52 (0.02)	526	0.53 (0.02)	534	0.55 (0.02)	-0.02	-0.04
Age (avg.)	532	33.27 (0.42)	526	33.24 (0.46)	534	33.59 (0.44)	0.03	-0.32
% Muslim (vs. Hindu)	532	0.48 (0.02)	526	0.44 (0.02)	534	0.49 (0.02)	0.04	-0.01
% Delhi	532	0.32 (0.02)	526	0.33 (0.02)	534	0.34 (0.02)	-0.02	-0.02
% Uttar Pradesh	532	0.24 (0.02)	526	0.23 (0.02)	534	0.21 (0.02)	0.00	0.03
% Gujarat	532	0.14 (0.01)	526	0.16 (0.02)	534	0.13 (0.01)	-0.02	0.01
% Maharashtra	532	0.31 (0.02)	526	0.27 (0.02)	534	0.32 (0.02)	0.04	-0.01
% Lower Caste	532	0.43 (0.02)	526	0.39 (0.02)	534	0.42 (0.02)	0.04	0.01
% Resident Large City	532	0.78 (0.02)	526	0.77 (0.02)	534	0.81 (0.02)	0.01	-0.03
% 12th Grade or Higher	532	0.84 (0.02)	526	0.84 (0.02)	534	0.86 (0.01)	-0.00	-0.02
% Voted BJP	410	0.60 (0.02)	401	0.65 (0.02)	405	0.66 (0.02)	-0.05	-0.06
% Homogeneous Group	532	0.40 (0.02)	526	0.44 (0.02)	534	0.36 (0.02)	-0.04	0.04
% Hindu Majority	532	0.42 (0.02)	526	0.37 (0.02)	534	0.44 (0.02)	0.05	-0.02
% Muslim Majority	532	0.18 (0.02)	526	0.19 (0.02)	534	0.19 (0.02)	-0.01	-0.01
Group Consciousness Index (avg.)	532	0.73 (0.01)	526	0.75 (0.01)	534	0.74 (0.01)	-0.01	-0.00

Notes: Standard errors in parentheses. T-test values represent the differences in the means across groups.
* indicates $p < 0.05$.

B Full Regression Output

This Section presents the full regression output in table format for all results presented in the main paper.

Table B2: **Treatment Effects on Supply for Leadership**
(Full Regression Output for Figure 4 of the Main Article)

	Dependent variable:					
	Willingness to Lead (1-4)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	*Overall*	*Muslims*	*Muslims*	*Hindus*	*Hindus*
Exclusive	0.044	0.038	0.062	0.051	0.026	0.018
	(0.039)	(0.039)	(0.055)	(0.055)	(0.055)	(0.055)
Inclusive	0.074*	0.073*	0.163***	0.160***	−0.011	−0.015
	(0.039)	(0.039)	(0.053)	(0.053)	(0.056)	(0.056)
Age		0.004**		0.001		0.008***
		(0.002)		(0.002)		(0.002)
Female		−0.002		0.032		−0.026
		(0.033)		(0.048)		(0.047)
Education High		−0.060		−0.064		−0.054
		(0.045)		(0.060)		(0.068)
Uttar Pradesh		−0.020		−0.033		−0.010
		(0.043)		(0.060)		(0.064)
Gujarat		−0.073		0.119		−0.151**
		(0.051)		(0.098)		(0.067)
Maharashtra		−0.047		−0.034		−0.047
		(0.040)		(0.053)		(0.061)
Hindu Majority		−0.012		−0.015		−0.012
		(0.035)		(0.050)		(0.049)
Muslim Majority		−0.054		−0.063		−0.040
		(0.044)		(0.059)		(0.065)
Group Consciousness		0.328***		0.226*		0.475***
		(0.090)		(0.126)		(0.133)
Survey Round		0.042		0.095		−0.038
		(0.047)		(0.059)		(0.078)
Observations	1,592	1,592	753	753	839	839
Adjusted R^2	0.001	0.014	0.010	0.013	−0.002	0.023

Note: *p<0.1; **p<0.05; ***p<0.01

Table B3: **Pro-Hindu Bias among Hindu Respondents in Hindu Majority Groups**
(Full Regression Output for Figure 5 of the Main Article)

	Dependent variable:
	Pro-Hindu Bias (-2 to 2)
Exclusive	−0.126
	(0.153)
Inclusive	0.126
	(0.147)
Observations	367
Adjusted R^2	0.002
Note:	*p<0.1; **p<0.05; ***p<0.01

Table B4: **Group Composition and Willingness to Lead Among Muslims**
(Full Regression Output for Figure 6 of the Main Article)

	Dependent variable:	
	Willingness to Lead (1-4)	
	(1)	(2)
Exclusive	0.088	0.088
	(0.084)	(0.083)
Inclusive	0.052	0.052
	(0.084)	(0.084)
Mixed Groups	−0.082	
	(0.076)	
Exclusive*Mixed	−0.046	
	(0.110)	
Inclusive*Mixed	0.183*	
	(0.108)	
Hindu Majority		0.023
		(0.085)
Muslim Majority		−0.264***
		(0.100)
Exclusive*Hindu Majority		−0.210*
		(0.123)
Inclusive*Hindu Majority		0.085
		(0.119)
Exclusive*Muslim Majority		0.234
		(0.144)
Inclusive*Muslim Majority		0.351**
		(0.141)
Observations	753	753
Adjusted R^2	0.013	0.023
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table B5: **Perceived Centrality**

(Full Regression Output for Figure 2 of the Main Article)

	Dependent variable:											
	Muslim-Hindu Contributions			Muslim Contributions			Hindu Contributions			Self-assessed Prototypicality		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Overall	*Hindus*	*Muslims*	*Overall*	*Hindus*	*Muslims*	*Overall*	*Hindus*	*Muslims*	*Overall*	*Hindus*	*Muslims*
Exclusive	−0.375***	−0.540***	−0.047	0.040	0.028	0.133	0.415***	0.569***	0.180**	0.016	0.038	0.008
	(0.107)	(0.124)	(0.144)	(0.060)	(0.060)	(0.087)	(0.069)	(0.096)	(0.090)	(0.053)	(0.075)	(0.072)
Inclusive	0.224**	0.077	0.354**	0.347***	0.280***	0.403***	0.124*	0.203**	0.048	0.025	0.034	0.012
	(0.107)	(0.126)	(0.140)	(0.060)	(0.061)	(0.085)	(0.069)	(0.097)	(0.088)	(0.052)	(0.077)	(0.070)
Observations	1,592	839	753	1,592	839	753	1,592	839	753	1,592	839	753
Adjusted R^2	0.018	0.032	0.010	0.023	0.027	0.028	0.022	0.040	0.003	−0.001	−0.002	−0.003

Note: *p<0.1; **p<0.05; ***p<0.01

Table B6: **Perceived Entitlement**
(Full Regression Output for Figure 3 of the Main Article)

	Dependent variable:											
	Muslim MLA: Qualified			Muslim MLA: Deserving			Self: Qualified			Self: Deserving		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Overall	*Muslims*	*Hindus*	*Overall*	*Muslims*	*Hindus*	*Overall*	*Muslims*	*Hindus*	*Overall*	*Muslims*	*Hindus*
Exclusive	−0.014	0.157	−0.085	0.002	0.047	0.052	−0.052	−0.068	−0.027	−0.049	−0.019	−0.067
	(0.082)	(0.099)	(0.120)	(0.081)	(0.097)	(0.117)	(0.055)	(0.073)	(0.081)	(0.058)	(0.080)	(0.083)
Inclusive	−0.014	0.229**	−0.215*	0.010	0.153	−0.086	−0.003	−0.022	0.011	0.040	0.110	−0.029
	(0.084)	(0.097)	(0.125)	(0.082)	(0.095)	(0.122)	(0.055)	(0.071)	(0.083)	(0.058)	(0.077)	(0.084)
Observations	829	386	443	829	386	443	1,592	753	839	1,592	753	839
Adjusted R^2	−0.002	0.010	0.002	−0.002	0.002	−0.001	−0.001	−0.001	−0.002	0.0002	0.002	−0.002

Note: *p<0.1; **p<0.05; ***p<0.01

Table B7: **Treatment Effects on Willingness to Seek Out Information**
(Full Regression Output for Figure 7 of the Main Article)

	Dependent variable:					
	Willingness to Seek Out Information					
	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	*Overall*	*Muslims*	*Muslims*	*Hindus*	*Hindus*
Exclusive	−0.016	−0.017	−0.039*	−0.041**	0.008	0.005
	(0.018)	(0.018)	(0.021)	(0.020)	(0.028)	(0.028)
Inclusive	0.019	0.015	0.009	0.004	0.028	0.023
	(0.018)	(0.018)	(0.020)	(0.020)	(0.029)	(0.028)
Female		0.033**		0.043**		0.037
		(0.015)		(0.018)		(0.024)
Uttar Pradesh		−0.070***		−0.028		−0.091***
		(0.020)		(0.022)		(0.033)
Gujarat		−0.073***		−0.056		−0.062*
		(0.023)		(0.037)		(0.034)
Maharashtra		−0.070***		−0.042**		−0.092***
		(0.018)		(0.020)		(0.031)
Age		0.004***		0.003***		0.004***
		(0.001)		(0.001)		(0.001)
Education High		0.004		−0.015		0.038
		(0.020)		(0.022)		(0.035)
Hindu Majority		0.003		0.011		−0.0004
		(0.016)		(0.019)		(0.025)
Muslim Majority		−0.025		−0.0003		−0.056*
		(0.020)		(0.022)		(0.033)
Observations	1,592	1,592	753	753	839	839
Adjusted R^2	0.001	0.042	0.005	0.044	−0.001	0.030

Note: *p<0.1; **p<0.05; ***p<0.01

Table B8: **Treatment Effects on Politician Evaluations**
(Full Regression Output for Figure 8 of the Main Article)

						Dependent variable:						
						Politician Evaluation						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Overall	_Overall_	_Overall_	_Overall_	_Muslims_	_Muslims_	_Muslims_	_Muslims_	_Hindus_	_Hindus_	_Hindus_	_Hindus_
	Muslim MLA	Muslim MLA	Hindu MLA	Hindu MLA	Muslim MLA	Muslim MLA	Hindu MLA	Hindu MLA	Muslim MLA	Muslim MLA	Hindu MLA	Hindu MLA
Exclusive	−0.042	−0.021	−0.030	−0.060	0.049	0.044	−0.091	−0.103	−0.048	0.001	0.011	−0.020
	(0.061)	(0.056)	(0.065)	(0.061)	(0.072)	(0.064)	(0.099)	(0.093)	(0.086)	(0.082)	(0.081)	(0.078)
Inclusive	−0.011	0.002	0.069	0.033	0.156**	0.117*	0.026	0.034	−0.139	−0.065	0.079	0.029
	(0.062)	(0.057)	(0.063)	(0.059)	(0.071)	(0.063)	(0.095)	(0.089)	(0.090)	(0.085)	(0.080)	(0.077)
Female		0.206***		0.304***		0.272***		0.395***		0.208***		0.240***
		(0.047)		(0.052)		(0.055)		(0.080)		(0.067)		(0.066)
Age		0.024***		0.017***		0.020***		0.014***		0.020***		0.014***
		(0.002)		(0.003)		(0.003)		(0.004)		(0.003)		(0.004)
Education High		−0.141**		−0.113		−0.059		0.004		−0.155		−0.235**
		(0.062)		(0.071)		(0.068)		(0.102)		(0.095)		(0.096)
Hindu Majority		0.030		−0.020		−0.023		0.027		0.089		−0.037
		(0.051)		(0.055)		(0.059)		(0.084)		(0.073)		(0.070)
Muslim Majority		−0.085		0.021		0.014		0.025		−0.220**		−0.009
		(0.062)		(0.070)		(0.070)		(0.100)		(0.093)		(0.095)
Observations	829	829	763	763	386	386	367	367	443	443	396	396
Adjusted R²	−0.002	0.169	0.001	0.128	0.008	0.226	−0.001	0.124	0.001	0.133	−0.002	0.096

Note: *p<0.1; **p<0.05; ***p<0.01

A8

C Additional Results and Robustness

This Section presents results from additional analyses and robustness tests.

C.1 Additional Results

Table C9 reports results from a regression in which we interact treatment assignment with respondent religion; we find that differences in treatment effects for Hindus and Muslims are statistically distinguishable from zero.

Table C9: **Effect of Historical Representations on Willingness to Lead**

	Dependent variable:	
	Willingness to Lead (1-4)	
	(1)	(2)
Exclusive*Muslim	0.036	0.032
	(0.078)	(0.078)
Inclusive*Muslim	0.174**	0.171**
	(0.077)	(0.077)
Exclusive	−0.011	−0.012
	(0.120)	(0.120)
Inclusive	−0.185	−0.181
	(0.121)	(0.121)
Muslim	−0.038	−0.100*
	(0.055)	(0.056)
Female		−0.003
		(0.033)
Age		0.004***
		(0.002)
Education High		−0.061
		(0.045)
Uttar Pradesh		−0.025
		(0.043)
Gujarat		−0.084
		(0.053)
Maharashtra		−0.046
		(0.040)
Group Consciousness		0.351***
		(0.092)
Survey Round		0.040
		(0.047)
Hindu Majority		−0.013
		(0.035)
Muslim Majority		−0.052
		(0.044)
Observations	1,592	1,592
Adjusted R^2	0.003	0.016

Note: *p<0.1; **p<0.05; ***p<0.01

Figure C1 shows how the inclusive and exclusive treatment affect evaluations of the Hindu and Muslim MLA (politician) for each separate dimension among Muslim respondents. We find that inclusive history positively affects perceptions that the Muslim MLA is qualified, deserving, representative of his constituencies and popular.

Figure C1: **Treatment Effects of Historical Representations on Perceptions of Hindu and Muslim MLAs among Muslims**
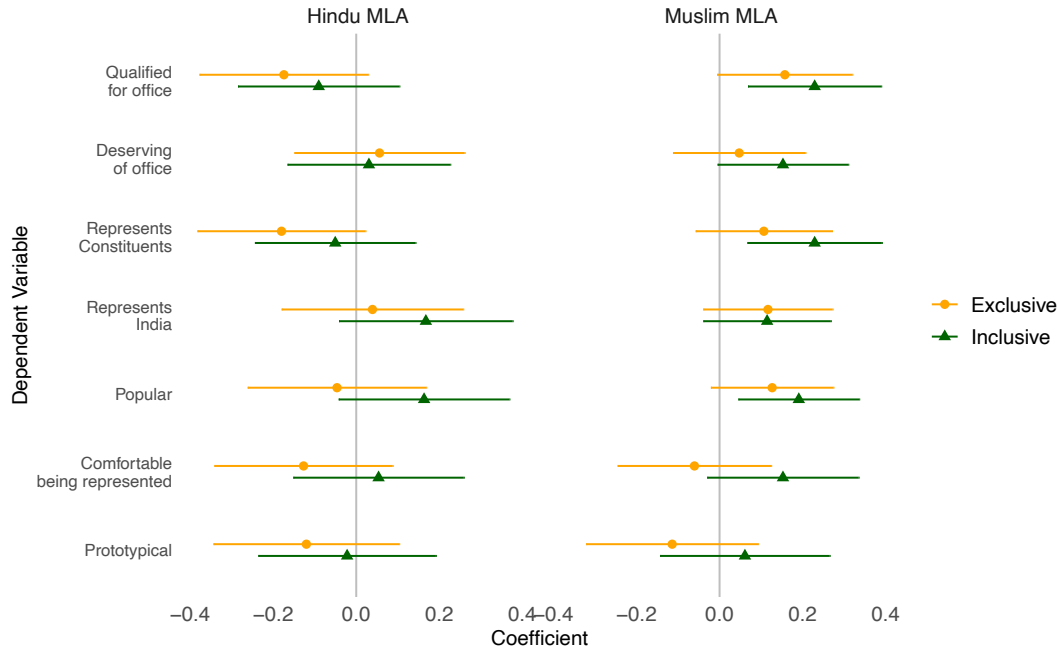
Notes: Coefficients are based on linear regression models excluding controls (bright colours) and including sociodemographic controls (dim colours). 95% (thin line) and 90% (thick line) confidence intervals are shown. The outcome variable ranges from 1-5. Full regression output can be found in Tables 2 and 3 of the Supplementary Material on Dataverse.

Figure C2 shows effects of our treatments on the perceived entitlement to lead of Hindu MLAs. As expected, we see no significant results among Hindus nor Muslims.

Figure C2: **Treatment Effects on the Perceived Entitlement of Hindu MLA**
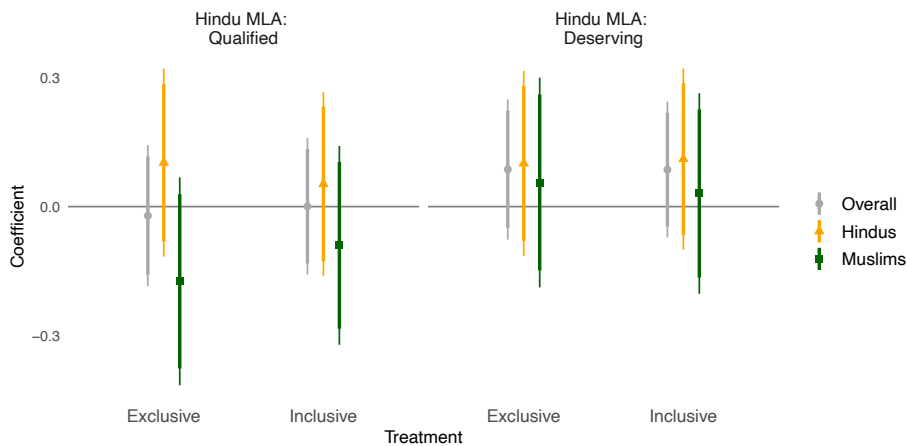
Notes: This figure displays the effects of our treatments on the perceived entitlement of Hindu MLAs. Full regression output can be found in Table 1 of the Supplementary Material on Dataverse.

Figure C3 shows no difference in effect sizes of the inclusive treatment on pro-Hindu bias among Hindus with high or low levels of group consciousness.

Figure C3: **Demand for Muslim Leaders among Hindu Respondents with High and Low Levels of Group Consciousness**
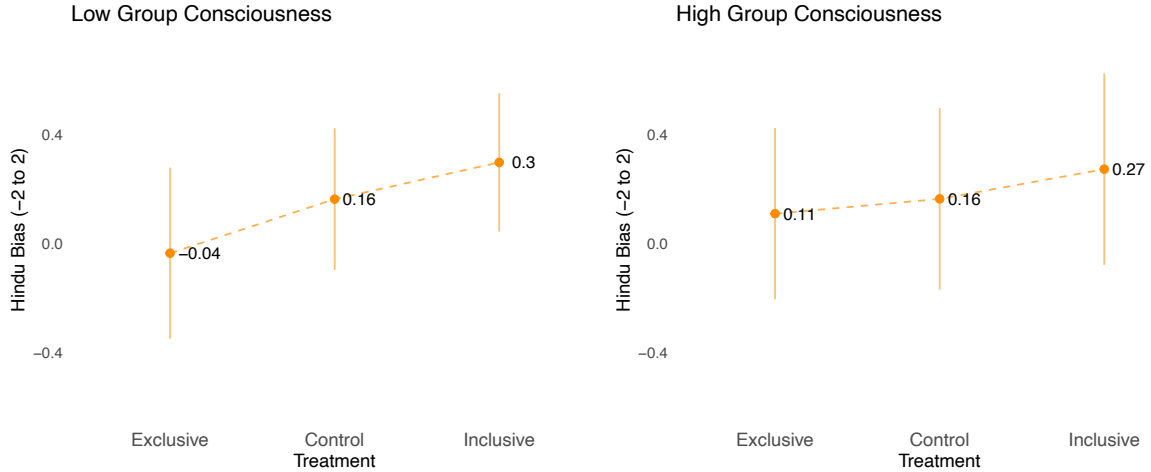
Notes: This figure shows expected pro-Hindu bias in the different treatment conditions among Hindus with low (left-panel) and high (right-panel) group consciousness. Above and below median values are used. 95% confidence intervals are shown. Full regression output can be found in Table 4 of the Supplementary Material on Dataverse.

Seeking Out Political Information: We were able to use individuals' randomly generated survey ID codes to track whether respondents visited the website with political information, as they were required to provide this code to enter. Although quite a few respondents visited this website (13%), we find no evidence that our experiment treatments affected their probability of doing so. Table C10 shows that the history treatments did not affect whether or not individuals actually visited the website with information on how to become politically engaged.

Table C10: **Effects of Historical Representations on Behavioral Measure of Willingness to Seek out Information on Citizenship Participation**

	Dependent variable:	
	Visited Website	
	(1)	(2)
	Muslims	*Hindus*
Exclusive	0.010	0.035
	(0.026)	(0.032)
Inclusive	−0.036	−0.005
	(0.025)	(0.033)
Observations	753	839
Adjusted R^2	0.002	−0.0002
Note:	*p<0.1; **p<0.05; ***p<0.01	

C.2 Treatment effects with distributions

Figure C4 displays treatment effects of historical representations on Muslim participants' willingness to lead (top left panel), Hindu participants' willingness to lead (top right panel), Hindu participants' pro-Hindu bias (bottom left panel) and Hindu participants' evaluations of the qualification of the Muslim MLA (bottom right panel). Individuals are plotted, along with means 95% confidence intervals. The results show that the lack of an exclusive treatment effect does not appear to be due to an increased number of respondents at high and low values "cancelling each other out," but rather to little movement across outcome categories relative to the baseline condition.

Figure C4: **Treatment Effects (with Distributions)**
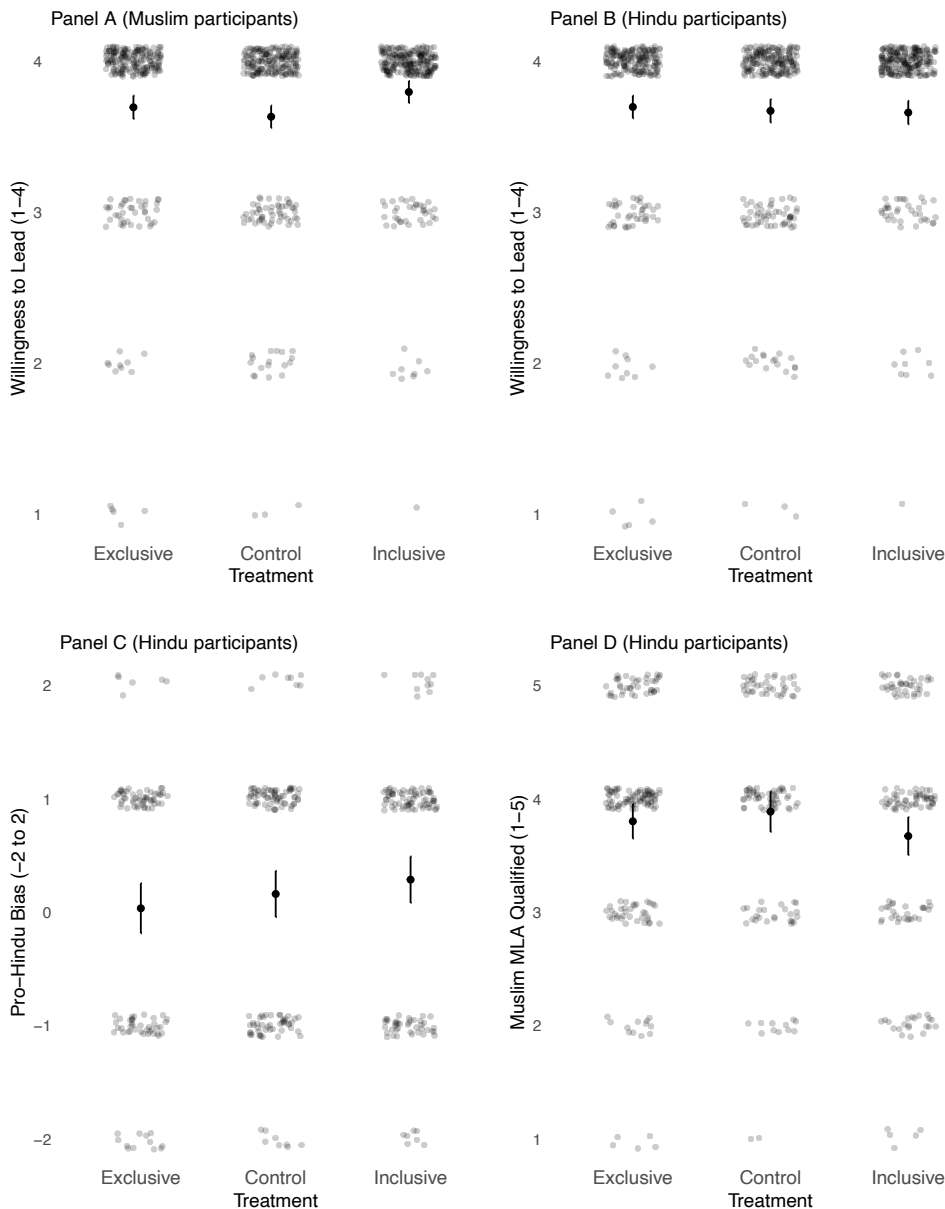
Notes: Treatment effects of historical representations on Muslim participants' willingness to lead (Panel A), Hindu participants' willingness to lead (Panel B), Hindu participants' pro-Hindu bias (Panel C) and Hindu participants' evaluations of the qualification of Muslim politicians (panel D). Full regression output can be found in Appendix Tables B2 and B3.

C.3 Analysis of open-ended responses

Figure C5 and Table C11 show results from more systematic analyses of our open-ended responses discussed at the end of section 6.3.1. As shown in Figure C5, the most frequently mentioned words in the open-ended responses relate to the leadership role (e.g., "representative", "leadership", or references to leading a "group") or specific leadership qualities (e.g., "good", "best", "know"). Appendix Table C11 shows that when asked why they wanted to volunteer for the role, 16% of respondents explicitly mentioned the word "leader" or "leadership" in their responses. This is noteworthy as we did not ourselves refer to the word "leadership" in the description of the task. Another 17% mentioned the word "represent". Considerations of suitability were present in many responses. Some elaborated on specific skills making them particularly good or qualified at managing groups, while others elaborated on specific leadership responsibilities they had had in the past. Overall, 19% of respondents mentioned the word "good" in their responses. Another 5% mentioned the word "qualified". Respondents therefore do not appear to view the role as inconsequential but clearly felt that the role of group representative in our lab-like set-up reflected an actual leadership position.

Figure C5: **20 Most Frequently Mentioned Words**
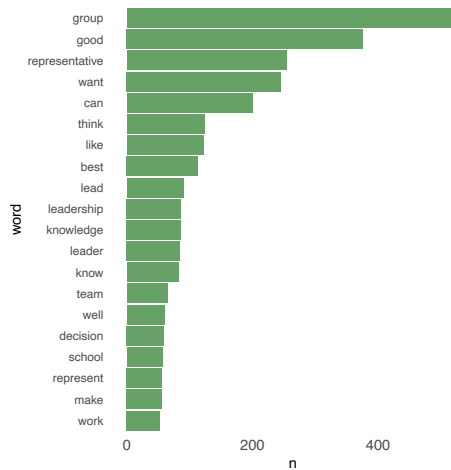
When elaborating on their reasons to volunteer for the group representative role, few respondents mentioned their specific skills in sociology or geography and few referred to strategic incentives to volunteer related to potential earnings. This suggests that respondents appeared to have thought of this leadership position in more general, abstract terms. We therefore do not expect the greater desire among Muslims to take on this role under the inclusive treatment condition to be related to a greater sense of confidence in their subject skills, or even in strategic considerations regarding earnings, but simply a greater sense of "having the right" to take a decision on behalf of Hindus. Note that this interpretation is consistent with our empirical findings presented in Appendix Table C16.

Table C11: **Percentage of Respondents Who Mentioned Words Related to Leadership, Suitability and Alternative Perceptions of the Role**

Word	Percentage
Represent	17
Lead	16
Good	19
Quali	5
Sociology	2
Geography	3
Earn	2

C.4 Alternative explanations

Null Effect on Prototypicality In the context we study, we expected that historical narratives could shape both the perceived historical contributions of different religious groups to the nation and the perceived "prototypical" member of that nation. Our explanation in Section 6.2 for why we observe support for the former expectation but not the latter centers on measurement: while our question on contributions concerned *groups*, our questions on perceived prototypicality focused on the *individual* respondent; participants were asked to what extent they felt a) they could be described as a typical Indian and b) similar to other Indians. We speculate: "Explicit self-assessments might be more difficult to shift in the short-term, and might not fully capture the idea of perceived group-level prototypicality; the personal nature of the questions also may have provoked unexpected responses."

An alternative explanation for why we do not observe a treatment effect as regards an individual's self-assessment of whether they constitute a "typical" Indian is that when thinking about what makes a typical Indian, respondents do not only weigh their religious (in our study, Hindu/Muslim) identity, but also other identity categories (wealth, education, and so on) as well. If, for instance, an individual thinks that their gender is relevant to conceptions of whether they are a "typical" Indian but their religion is not, then we might not observe any treatment effect *even if* our treatments alter individuals' perceptions of their religious group's place in the nation.

While this explanation is very plausible, we nevertheless believe that it is unlikely to fully explain the lack of an observed treatment effect on subjects' self-assessed prototypicality. Most notably, while the degree to which individuals weigh their religious identity may vary, we expect that the high salience of religion in India – and its frequent invocation in politics generally and specifically in debates about who is a true Indian and loyal to the Indian nation – means that it will be weighed at least to some degree by most, and heavily by at least a portion, of our respondent pool. Indeed, when asked at the end of the experiment whether following "Hindu customs and traditions" was important to being a "true Indian", 26% of respondents said it was "very important" and 37% said it was "extremely important" (results are consistent across both Hindu and Muslim respondents and remain unchanged if we limit analysis to the control group only). Our points #1-3 below further indicate that participants highly weighed their religious identity, relying on a pre-treatment identity measure. It is also worth noting that we do not find a treatment effect on *either* of our prototypicality measures – not only self-assessed proximity to the typical Indian but also self-assessed similarity to other Indians. Were the alternative explanation fully responsible for these null results, it would thus have to be the case that respondents placed little weight on their religious identity when thinking both about what makes a typical Indian and what determines their similarity to other Indians. However, as shown in Figure C6 and tables below, respondents did not appear to treat these two measures entirely interchangeably.

That being said, we can investigate if, as the alternative explanation would imply, our treatment has a lesser effect on the self-assessed prototypicality of those who weigh their religious identity comparatively less. We take three approaches:

First, if the alternative explanation is correct, then those who possess characteristics other than religion that they believe make them prototypical Indians should be less responsive to treatments altering the perceived place in the nation of their religious group.

The left panel of Figure C6 displays the association between different demographic variables, elicited pre-treatment, and the degree to which individuals in the control group only said they felt like a) a typical Indian and b) similar to other Indians. While we observe some relationships – older respondents, those from large cities, and those from lower caste background tend to assess themselves as more prototypical – results are sensitive to the set of variables included and to which measure of prototypicality we use, and are of extremely small magnitude when compared to measures capturing religious identification (see right panel of

A14

Figure C6: **Demographic Correlates of Prototypicality (Control Group Only)**
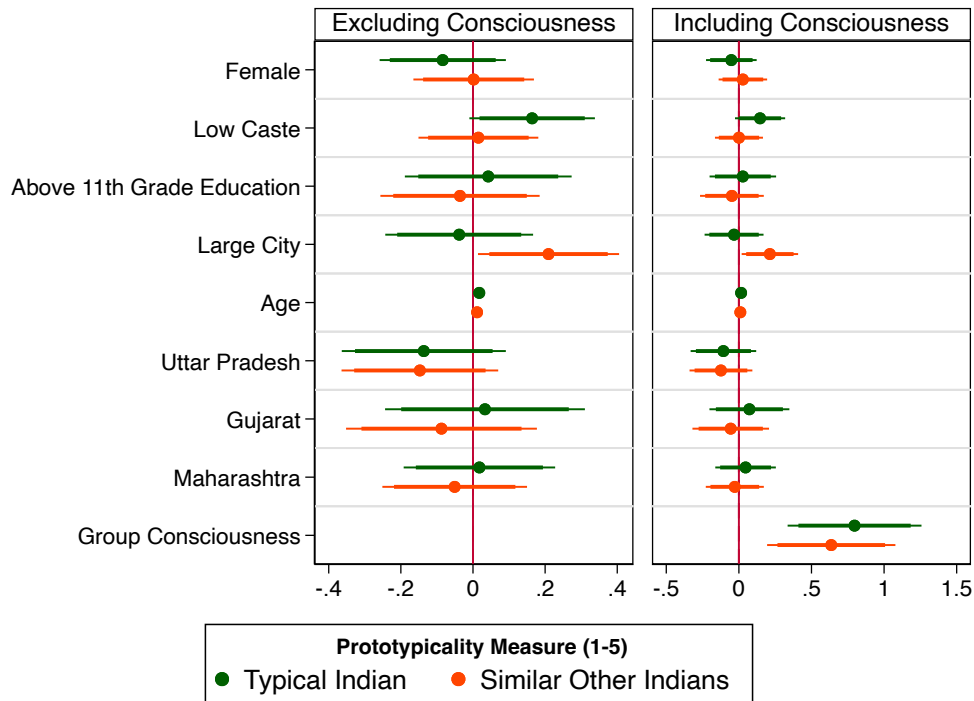
Notes: This figure displays demographic correlates of self-assessed protoypicality for respondents in the control group, excluding (left panel) and including (right panel) a measure for religious group consciousness and separately for each measure of prototypicality: perceived proximity to the "typical" Indian (green) and similarity to other Indians (orange). 95% (thin line) and 90% (thick line) confidence intervals are shown. Delhi is the reference category for state of residence. Full regression output can be found in Table 8 of the Supplementary Material on Dataverse.

Figure C6). More to the point of the exercise, we do not find consistent evidence that the identity categories associated with prototypicality (age, urban residence, caste background), however weakly, interact with our treatment assignment to affect self-assessed prototypicality for either Hindu or Muslim respondents (see Appendix Tables C12 and C13).

Second, if the alternative explanation is correct, we should observe greater treatment effects for those who, pre-treatment, indicated a high degree of religious group consciousness. As reported in Appendix Table C18, we find little evidence that group consciousness meaningfully interacts with treatment assignment when considering our main outcome of interest: willingness to lead. Appendix Table C14 conducts the same exercise but considering Hindu and Muslim respondents' prototypicality: separately for each of our two measures of prototypicality, and averaging across these two measures. As Appendix Table C14 shows, group consciousness does not interact meaningfully with treatment assignment for any of our prototypicality measures for either Muslim or Hindu respondents.

In sum, while plausible, we find little support for this alternative explanation regarding the driver of our null findings on self-assessed prototypicality measures. Our investigation suggests that people highly weigh their religious identity, and that their relative ranking of identity categories does not appear to interact meaningfully with their treatment assignment to affect self-assessed prototypicality.

As a final exercise, we can see whether treatment effects differ for respondents who appear to attach greater weight to religion *relative* to other identity categories. Although we did not explicitly ask respondents to reflect on this question for fear of priming them as to the study's

Table C12: **Demographic Predictors of Prototypicality and Treatment (Muslims Only)**

	Dependent variable:					
	Measure of Prototypicality					
	Typical	Similar	Typical	Similar	Typical	Similar
	(1)	(2)	(3)	(4)	(5)	(6)
Exclusive Trt	-0.02	0.02	-0.23	0.17	-0.20	0.05
	(0.12)	(0.12)	(0.30)	(0.29)	(0.18)	(0.17)
Inclusive Trt	-0.09	0.01	-0.38	0.09	-0.11	0.10
	(0.12)	(0.12)	(0.30)	(0.29)	(0.19)	(0.18)
Low Caste	0.24**	0.15				
	(0.12)	(0.11)				
Low Caste X Excl	0.04	0.01				
	(0.17)	(0.16)				
Low Caste X Incl	0.18	-0.04				
	(0.16)	(0.16)				
Age			0.01*	0.01**		
			(0.01)	(0.01)		
Age X Excl			0.01	-0.00		
			(0.01)	(0.01)		
Age X Incl			0.01	-0.00		
			(0.01)	(0.01)		
Reside Large City					-0.11	0.23*
					(0.14)	(0.13)
City X Excl					0.25	-0.03
					(0.20)	(0.19)
City X Incl					0.17	-0.14
					(0.21)	(0.20)
N	753	753	753	753	753	753
Adjusted R^2	0.02	-0.00	0.03	0.01	-0.00	0.00

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

Notes: This table displays results of an interaction between demographics shown to predict perceived prototypicality (see Figure C6) with treatment assignment. We display results for Muslim respondents only and separately for each of our prototypicality measures: perceived proximity to the "typical" Indian (columns 1, 3, and 5) and similarity to other Indians (columns 2, 4, and 6).

focus on religion, we *did* ask them pre-treatment to reflect on whether it would be easier or harder to get along with a new neighbor of a particular religious (Hindu, Muslim, or Christian – answers for the first two groups constitute our measure of group consciousness referenced above), wealth (poor or rich), caste (Dalit), or political party (BJP or Congress) background.

Appendix Figure C7 displays the percentage of respondents saying that it would be either 1) neither easier nor harder, 2) easier or harder, or 3) a lot easier or harder, to get along with a new neighbor of each identity category. Consistent with participants weighing salient (Hindu and Muslim) religions more than other identity categories, we observe that it is Muslims and Hindus that are both least likely to engender neutral responses ("neither easier nor harder") and most likely to engender extreme responses ("a lot easier or a lot harder"). However, consistent with the findings in points #1 and #2 above, we do not observe that an individual's relative identity import ranking interacts meaningfully with their treatment assignment to affect their self-assessed prototypicality. For instance, we find no differences

Table C13: **Demographic Predictors of Prototypicality and Treatment (Hindus Only)**

	Dependent variable:					
	Measure of Prototypicality					
	Typical	Similar	Typical	Similar	Typical	Similar
	(1)	(2)	(3)	(4)	(5)	(6)
Exclusive Trt	0.06	-0.02	0.59*	0.30	0.01	0.34*
	(0.11)	(0.10)	(0.30)	(0.28)	(0.19)	(0.17)
Inclusive Trt	0.03	0.09	0.13	0.18	-0.15	0.12
	(0.11)	(0.10)	(0.32)	(0.29)	(0.20)	(0.18)
Low Caste	0.02	-0.18				
	(0.14)	(0.13)				
Low Caste X Excl	-0.07	0.20				
	(0.20)	(0.18)				
Low Caste X Incl	-0.18	-0.05				
	(0.20)	(0.18)				
Age			0.02***	0.01*		
			(0.01)	(0.01)		
Age X Excl			-0.02*	-0.01		
			(0.01)	(0.01)		
Age X Incl			-0.00	-0.00		
			(0.01)	(0.01)		
Reside Large City					0.09	0.28**
					(0.16)	(0.14)
City X Excl					0.03	-0.39*
					(0.22)	(0.20)
City X Incl					0.16	-0.04
					(0.22)	(0.20)
N	839	839	839	839	839	839
Adjusted R^2	-0.00	0.00	0.01	0.00	-0.00	0.00

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

Notes: This table displays results of an interaction between demographics shown to predict perceived prototypicality (see Figure C6) with treatment assignment. We display results for Hindu respondents only and separately for each of our prototypicality measures: perceived proximity to the "typical" Indian (columns 1, 3, and 5) and similarity to other Indians (columns 2, 4, and 6).

between individuals who were more or equally likely to give an extreme response for Hindus or Muslims ("a lot easier or a lot harder") than for other identity categories, as compared with individuals who were not more likely to give such a response for these religious identities (see Appendix Table C15).

Table C14: **Group Consciousness, Treatment, & Prototypicality**

	Dependent variable:					
	Measure of Prototypicality					
	Average	Average	Typical	Typical	Similar	Similar
	(1)	(2)	(3)	(4)	(5)	(6)
Exclusive Trt	-0.21	-0.26	-0.23	-0.15	-0.18	-0.37
	(0.31)	(0.34)	(0.37)	(0.40)	(0.34)	(0.38)
Inclusive Trt	-0.16	-0.06	-0.33	0.12	0.00	-0.23
	(0.31)	(0.31)	(0.37)	(0.37)	(0.34)	(0.35)
Religious Group Consciousness	1.21***	0.16	1.10***	0.44	1.33***	-0.13
	(0.30)	(0.28)	(0.37)	(0.33)	(0.33)	(0.32)
Consciousness X Excl	0.31	0.33	0.34	0.17	0.27	0.49
	(0.43)	(0.42)	(0.52)	(0.50)	(0.47)	(0.47)
Consciousness X Incl	0.26	0.09	0.42	-0.11	0.10	0.29
	(0.43)	(0.39)	(0.52)	(0.46)	(0.47)	(0.44)
N	839	753	839	753	839	753
Adjusted R^2	0.07	-0.00	0.04	0.00	0.06	-0.00
Sample	Hindus	Muslims	Hindus	Muslims	Hindus	Muslims

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

Notes: This table displays results of an interaction between religious group consciousness and treatment assignment. We display results separately for Hindu (columns 1, 3, and 5) and Muslim (columns 2, 4, and 6) respondents and separately for each of our prototypicality measures: perceived proximity to the "typical" Indian (columns 3 and 4), similarity to other Indians (columns 5 and 6), and an average of the two (columns 1 and 2).

Table C15: **Relative Importance of Religion, Treatment, & Prototypicality**

	Dependent variable:					
	Measure of Prototypicality					
	Average	Average	Typical	Typical	Similar	Similar
	(1)	(2)	(3)	(4)	(5)	(6)
Exclusive Trt	-0.01	-0.07	-0.05	-0.11	0.04	-0.03
	(0.10)	(0.11)	(0.12)	(0.12)	(0.11)	(0.12)
Inclusive Trt	-0.01	0.06	-0.09	0.08	0.08	0.04
	(0.10)	(0.10)	(0.12)	(0.12)	(0.11)	(0.11)
Prioritizes Religious Identity	0.07	-0.06	0.07	-0.07	0.07	-0.06
	(0.11)	(0.10)	(0.13)	(0.12)	(0.12)	(0.11)
Prioritizes X Excl	0.09	0.14	0.18	0.19	0.01	0.09
	(0.15)	(0.14)	(0.18)	(0.17)	(0.17)	(0.16)
Prioritizes X Incl	0.08	-0.09	0.14	-0.10	0.01	-0.09
	(0.15)	(0.14)	(0.19)	(0.17)	(0.17)	(0.16)
N	839	753	839	753	839	753
Adjusted R^2	-0.00	-0.00	0.00	-0.00	-0.00	-0.00
Sample	Hindus	Muslims	Hindus	Muslims	Hindus	Muslims

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

Notes: This table displays results of an interaction between the importance an individual placed on religion *as compared with* other identity categories and treatment assignment. We display results separately for Hindu (columns 1, 3, and 5) and Muslim (columns 2, 4, and 6) respondents and separately for each of our prototypicality measures: perceived proximity to the "typical" Indian (columns 3 and 4), similarity to other Indians (columns 5 and 6), and an average of the two (columns 1 and 2). We count an individual as prioritizing religion comparatively more if they were equally or more likely to provide an "extreme" response ("a lot easier or a lot harder") to the question of whether it would be easier or harder to have a neighbor of Hindu/Muslim identity, as compared with *every other* category (wealth, caste, or political party) in the neighbor question. If a respondent provided a more extreme response to any single alternative identity category, they were thus coded as not prioritizing their religious identity more than other identities. In total, approximately 49% of our sample was coded as prioritizing religion over other identity categories.

Figure C7: **Relative Import Given to Identity Categories**
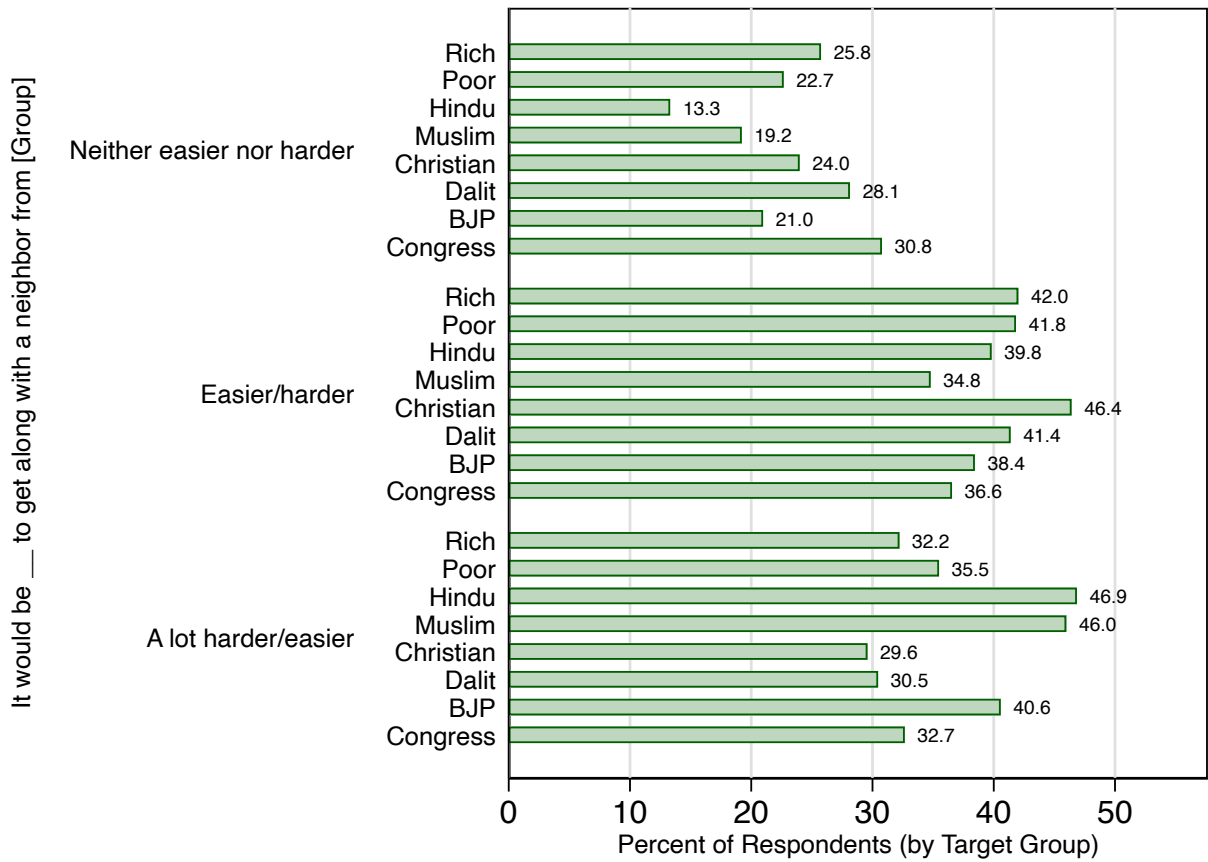
Notes: This figure displays the percentage of respondents saying that it would be either 1) neither easier nor harder, 2) easier or harder, or 3) a lot easier or harder, to get along with a new neighbor of each identity category.

Overall Engagement and Performance as Alternative Explanations We evaluate alternative explanations for one of our central findings that inclusive renderings of history increase marginalized group members' willingness to take on leadership positions. Two possible alternative mechanisms come to mind. First, inclusive renderings of history may simply improve Muslim respondents' overall experience with – and engagement in – the survey. Second, Muslim respondents may score better in the inclusive history exercise because they are more familiar with the material and/or find the correct responses to be more consistent with their worldview. If this were the case, Muslim minorities may be more likely to volunteer because they believe more strongly in their performance in the exercises. Table C16 shows how the history treatments affect firstly overall engagement with the study measured through an item asking respondents to what extent they were involved with the study, and secondly respondents' score in the history exercises. The findings reveal that the inclusive history treatment does not increase Muslim respondents' overall engagement with the study. Muslim respondents also did not score better in the inclusive history exercises. In light of this evidence, it seems unlikely that engagement or performance could better explain why minorities volunteer more when exposed to inclusive history.

Table C16: **The Effect of Historical Representations on Reported Engagement in the Study and Score in the History Exercises**

	Dependent variable:			
	Engagement		Score	
	Muslims	*Hindus*	*Muslims*	*Hindus*
	(1)	(2)	(3)	(4)
Exclusive	0.082	−0.001	−0.213***	−0.058
	(0.066)	(0.080)	(0.072)	(0.061)
Inclusive	0.011	−0.044	−0.401***	−0.211***
	(0.065)	(0.081)	(0.070)	(0.062)
Observations	753	839	753	839
Adjusted R^2	−0.0003	−0.002	0.039	0.012

Note: *p<0.1; **p<0.05; ***p<0.01

Demand Effects as an Alternative Explanation Another alternative explanation that comes to mind are potential demand effects. Could participants be inferring that the authors of the study hold the view advanced by the narrative they are presented with, and therefore are more likely to give the answers they think the study's authors want to hear? This effect could be exacerbated by the provision of incentives for "correct" answers among respondents who act strategically. When designing the textbook exercises, we took great care to explain that the treatment material (textbook excerpts) was taken from real, state-sponsored textbooks. We explicitly state that "All lessons come from real textbooks, approved by government educational boards in India". When asking participants to answer comprehension questions based on the material, we were also careful to not insinuate that a correct comprehension quiz question is subject to the researchers' beliefs, but should correspond to the state-sponsored narrative. We write: "Please answer the following questions according to what was described in the lesson". Thus, we took special effort to ensure that participants did not feel that we as researchers were pushing a particular narrative. We believe this should reduce the risk of possible demand effects.

Relatedly, we took great care thinking about how to ensure that the goal of the study did

not appear obvious to respondents. The narratives were embedded in a larger experimental set-up in which we believe our reasons to show respondents historical narratives were obfuscated. We introduce our participants to a broader theme – "back to school" – and asked participants to engage with several textbook exercises on different topics (history, geography and sociology). We believe the design choice to embed the historical narratives in a larger set-up – in which they are not presented as the central focus of the exercise – substantially reduced concerns about demand-effects.

When designing our measure capturing respondents' willingness to become group representative, we took great care to ensure that it would be clear to all respondents that the role of the group representative would *not be* to answer comprehension questions for the group, but rather to select which topic – geography or sociology – should count for the group's payment from the upcoming exercises. According to the alternative explanation, participants may infer from the treatment texts whether the researchers hold a more inclusive/exclusive view and, given that correct comprehension responses are incentivized, it becomes more important for participants in the "group" exercises to delegate the answering to those participants who are most likely to be able to give the answers the researchers want to hear. A Muslim participant is therefore thought to be more likely to be able to provide answers the researchers want to hear under the inclusive treatment condition, and a Hindu participant should be more able to provide such answers under the exclusive condition. This could indeed be the case if the role of the group representative were to respond to history questions in the name of the group. However, it is less clear why exposure to the inclusive narrative would make participants feel that Muslims are better able to determine whether the researchers prefer the group to select geography or sociology as the topic which will count for the group's payment. As Blair, Coppock and Moor (2020) highlight in their work on sensitivity bias, social desirability is unlikely to be a meaningful source of error unless respondents perceive a particular answer to be socially desirable by the social referent (i.e., the researchers). We therefore believe that our theorized mechanism – that Muslim minorities exposed to the inclusive historical narrative should come to feel more entitled to take decisions on behalf of Hindus and should therefore become more likely to volunteer for decision-making roles (whatever the decision may concern) – is a more plausible explanation.

Although we do not expect it to be clear to respondents what the socially desirable answer is when deciding between school subjects, we concede that respondents in the inclusive history treatment arm may perceive the researchers as holding more inclusive views, and thereby imagine that there may be some undefined advantage in Muslims taking on the role of group representative. Nonetheless, when considering the possible observable implications of this strategic behavior, we find little support consistent with this alternative explanation:

1. First, if this demand effect indeed played a significant role, we would expect to observe an effect of the inclusive and exclusive history treatments on Hindus' willingness to volunteer. As described in our hypotheses section and pre-analysis plan, we do not expect the historical narratives to affect members of the Hindu community's sense of entitlement to lead – our supply-side expectations apply only to members of the Muslim community. If the type of demand effect played a significant role, we would expect Hindus in the inclusive treatment to become less willing to volunteer (increasing the chances that a Muslim is selected), and Hindus in the exclusive treatment to become more willing to volunteer. We do not observe these outcomes, however.

2. Second, the alternative interpretation also implies that Hindus should show less/more pro-Hindu bias in the inclusive/exclusive treatments in the exercise where they are asked to rank their fellow group members (demand-side channel). However, this is not the case. Although we also expected – for different reasons – to observe these demand-side channel effects, it is important to note that according to our theory, the

demand- and supply-side channels can function independently of each other: Hindus not responding to the inclusive treatment, which we attribute to their resistance to inclusive narratives, does not mean that Muslims' responses to the inclusive treatment are not attributable to our theorized mechanism. While it thus seems entirely plausible to us under our theory that the inclusive treatment may produce these different responses among Hindus and Muslims, it is less clear why the treatment should produce demand effects among Muslims, but not among Hindus.

3. Third, were this alternative interpretation correct, we would also expect to observe more consistent effects of the exclusive history treatment. We proposed that the exclusive history treatment may not have as important an effect as the inclusive treatment, due to the current status quo in India and the salience of exclusionary historical narratives. If, however, results could be explained by strategic behavior due to demand effects, we would expect to see larger effects in the exclusive treatment condition.

If the alternative explanation were to be true, one possible implication could be that the effect of the history treatments should be largest among those respondents more likely to be sensitive to demand effects. Although it is not entirely clear who these respondents might be, we explore whether the effects of the historical representations on participants' willingness to volunteer is conditional on their score in the comprehension questions following the history excerpts. Assuming (as the strategic argument implies) that a high score reflects not only high attention/comprehension, but also strategic behavior/sensitivity to demand effects, the alternative interpretation would suggest that the positive effect of the inclusive treatment observed on Muslims' willingness to volunteer should increase as a function of respondents' score on the comprehension question. Figure C8 shows the conditional effect of the inclusive history treatment on Muslim respondents willingness to lead, given different levels of their comprehension question scores (which range from 1-4). As can be observed in the Figure, treatment effects are significant among respondents who answer correctly to 3 or 4 questions. This should be expected as those with only few points are unlikely to have paid much attention to the treatment texts. However, the positive effect does not increase among respondents with the highest score (respondents who are likely to be sensitive to demand effects) as compared to those with more average scores, but rather reaches a plateau. Put differently, once subjects have surpassed a certain "attention threshold", the effect of the inclusive treatment appears independent of whether the participant received higher scores in the textbook exercise, contrary to what the strategic incentives argument would suggest.

Another potential implication of the strategic argument put forth could be that we should observe evidence of strategic motivations in the open-ended questions in which respondents elaborate on their reasons for volunteering. However, when analysing respondents' open-ended responses on why or why not they volunteered as group representative, we find little evidence in favor of the strategic incentives mechanism. We have added a new analysis of open-ended responses in Section C.3 of the Appendix. As can be observed in Table C11, very few respondents mention strategic considerations – such as their abilities in sociology or geography, or considerations regarding earnings – in these responses. Rather, most respondents choose to elaborate on what makes them good leaders, in more general terms.

Finally, while a large and robust body of work shows the existence of experimenter demand effects in experimental settings, a smaller but growing line of research suggests that while the primary source of demand effects in laboratory experiments tends to be subtle cues offered by researchers in their direct interactions with experimental participants, online survey experiments on the other hand, with their depersonalized interactions between researchers and participants, tend to be considered least-likely cases for the presence of such demand effects (McDermott 2002; Siah 2005). Consistently, studies suggest that experimenter demand appears to play a more limited role in online settings (Chandler et al. 2015; White et al. 2018).

Figure C8: **Conditional Effects of The Inclusive History Treatment on Muslim Respondents' Willingness to Lead Given Comprehension Scores**
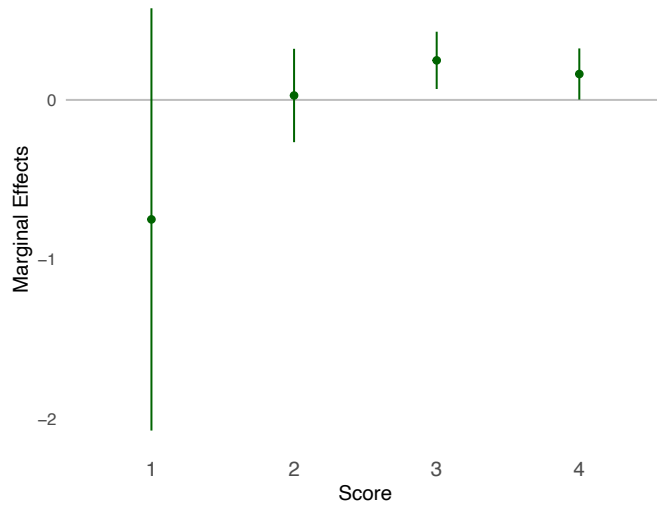
Notes: This figure displays the marginal effects of the inclusive history treatment on Muslim respondents' willingness to lead given different scores on the comprehension questions. N=511. The large confidence interval for score category 1 is due to the fact that very few respondents (N=5) received such a low score. Full regression output can be found in Table 5 of the Supplementary Material on Dataverse.

Recent work on the prevalence of demand effects in online survey experiments suggests that research participants generally show a limited ability (or willingness) to adjust their behavior to align with researcher expectations. Mummolo and Peterson (2019) find that even when participants are assigned information about experimenter intent and when financial incentives to respond in line with researcher expectations are provided, participants fail to adjust their behavior to meet these expectations. The authors conclude that online survey experimental designs are often robust to demand effects and that efforts to obfuscate the aim of experimental studies may be unnecessary. While it would be unwise to categorically dismiss the risk of experimenter demand effects – for which we did take several steps to obfuscate the aim of our study – these more recent findings indicate that the risk of demand effects in online experiments such as ours may be smaller than in more traditional settings in the lab.

Interpretations of the heterogeneous treatment effects of the inclusive treatment effect on evaluations of the Muslim MLAs We believe our demand-side measure has merit, as it captures respondents' evaluations of real-world politicians, and thereby complements our "lab-like" behavioral measures. Notably, we find that Muslim respondents who are exposed to the inclusive history treatment are more likely to rate the Muslim MLA positively than other Muslim respondents who are asked about the same MLA. We believe this finding is important, can tell us something about the evaluation of real-world political leaders, and is unlikely to be explained by changes in beliefs about the MLA's constituents.

However, according to our pre-registered hypothesis related to the demand-side channel, we expected both Hindus and Muslims (i.e., "greater society") to evaluate the Muslim politician more positively when exposed to the inclusive treatment. The finding that only Muslims do so therefore provides only partial evidence in favor of this hypothesis. Since we did not expect to observe any difference across Hindu and Muslim respondents, we did not develop and pre-register clear expectations as to why their responses might differ. After analyzing the data, we speculate that Hindus' attitudes towards Muslims may be more difficult to shift (see Section 7).

An alternative explanation is that the inclusive treatment may be affecting how respondents imagine the composition of the MLA's constituents. If this were the case, respondents may view the MLA as more representative of his constituents not because respondents view the MLA as having a greater claim to national identity, but because respondents imagine the MLA's constituents to be largely composed of Muslims. Although we cannot fully dismiss this alternative explanation, it is unclear why the treatment should affect how respondents imagine the MLA's constituents. Were this the case, we would also expect to see the same positive effect of the inclusive history treatment among Hindus.

Additional analyses on perceived centrality   Could the results on perceived centrality reflect reading comprehension or faithful reporting of what was contained in the treatment text, rather than an increase in salience of Hindu/Muslim contributions? Figure C9 shows additional results on the perceived centrality of different groups. Certain words (individuals, groups, items) coded as "Hindu" for our outcome measure capturing "perceived Hindu centrality" were not explicitly mentioned in the exclusive history treatment text. Although many groups and individuals coded as "Hindu" – such as "Aryans" and "Maharana Pratap" – were explicitly mentioned in the text, others such as "V.D. Savarkar" and "Raja Ram Mohan Roy" were not. We create a dummy variable capturing whether respondents selected groups or individuals coded as "Hindu" that *were not explicitly mentioned in the text* and examine whether respondents were more likely to mention these groups or individuals when exposed to the exclusive history treatment. As shown in the left-hand panel of Figure C9, Hindu respondents in the exclusive treatment condition were indeed more likely to mention these groups. We believe this indicates that an increase in salience of Hindu groups and individuals (Hindu "heroes"), rather than faithful reporting of groups mentioned in the text, is more likely to explain our findings.

Certain groups explicitly mentioned in the inclusive history treatment text were not coded as "Muslim" for our outcome measure capturing "perceived Muslim Centrality". This relates primarily to groups mentioned in the time period "ancient India", prior to the arrival of Islam to India, such as "Aryans" and "Dasyus". The text elaborates on these groups to show the plural and heterogeneous nature of ancient India. If respondents were simply reporting groups mentioned in the text they read, they should be more likely to mention these non-Muslim groups as well. We create a dummy variable, capturing whether respondents selected these non-Muslim groups included in the inclusive history text, and examine whether respondents in the inclusive history treatment group were more likely to mention these groups, than respondents in the control group. As shown in the right-hand panel of Figure C9, this is not the case. Neither Hindus nor Muslims are more likely to report that these groups contributed to India in the inclusive treatment group. We believe this shows that an increase in salience of Muslim groups and individuals (Muslim "heroes"), rather than faithful reporting of groups mentioned in the text, is more likely to explain our findings.

High baseline level of accepting information on how to become politically involved
Could the high baseline levels of accepting information on how to become politically involved suggest that there is a broader issue of social desirability bias in our sample? We do not believe that the high baseline levels necessarily indicate high levels of social desirability in the sample, but rather reveals that saying "yes" to this question is not costly. We base this on the following reasoning:

- People often indicate a willingness to partake in an activity or receive information, but they do not always see that intention through. In part, this is because "stated" preferences are less costly than "revealed" preferences (Kahneman and Tversky 1979). In part, this could be because people tend to like to keep their options open and feel reluctant to "close doors" (Shin and Ariely 2004). For instance, if we were to host

Figure C9: **Treatment Effects on Alternative Measures of Perceived Contributions**

Notes: This figure displays the effects of our treatments on (1) a dummy indicating whether respondents selected Hindu words not explicitly mentioned in the exclusive treatment text and (2) a dummy indicating whether respondents selected non-Muslim words included in the inclusive history treatment. Full regression output can be found in Table 6 of the Supplementary Material on Dataverse.

an event providing information to marginalized groups on how to become politically engaged and asked those interested to sign up beforehand, it is likely that quite a few people who signed up would not actually end up showing up to the event. When signing up, many of these people would probably be thinking "perhaps I might want to further down the line" or "better keep that option open". In this sense, even though saying "no" is also easy, doing so would imply foregoing an opportunity. In this sense, saying "no" is not just as easy as saying "yes". It is therefore not surprising to us that many respondents answered "yes" to this question. Indeed, this highlights the value of our incentivized experimental game, which by design makes individuals' decisions costlier.

- We assessed the risk of social desirability bias in our sample by comparing it to other studies on India, concluding that our sample appears similar to other online samples in India recruited through a variety of online platforms such as Facebook, MTurk and Qualtrics. In particular, we added an analysis comparing respondents' pre-treatment stated political interest (using a more traditional, 4-point scale measure that mirrors measures in other studies) with stated political interest among sampled respondents in two other recent studies on India (one by Boas, Christenson and Glick (2020) and one by Guess et al. (2020)). Political interest – measured through this more traditional scale — is not higher in our sample, than in comparable samples. This lends further support to our interpretation that the high proportion of respondents who answered "yes" to this question is likely to be due to the design of the measure, and not due to our sample being particularly prone to giving socially desirable answers.

- While we agree that this type of social desirability is likely to affect behavior in settings in which the host/experiment interacts directly with the participant/subject, it appears much less likely in an anonymous online setting (recent research also supports this notion, see Mummolo and Peterson (2019)). Our preferred interpretation – that respondents with some interest in becoming politically engaged preferred to keep their

options open – appears more plausible to us.

Finally, even if some respondents did answer "yes" to this question because they believe it is the socially desirable answer, this is unlikely to account for our findings on the role of historical narratives on willingness to receive political information. For instance, if demand effects accounted for our findings, and the treatments reveal to respondents what they believe the researchers want to hear, we would expect Hindus to also respond to our treatments by choosing the more socially desirable answer (e.g., show a greater willingness to receive political information under the exclusive treatment condition). However, this is not the case. In sum, although we cannot fully erase this concern, we *do not believe* that the high proportion of respondents answering "yes" to this item can be taken as evidence that (1) there is a problem of social desirability bias in our sample (above and beyond what is normal in surveys of this kind) and (2) demand effects explain our findings on the role of historical narratives in shaping respondents willingness to receive information on how to become politically involved.

C.5 Additional Empirical Expectations

Tables C17-C18 evaluate some of the empirical expectations outlined in Section G.7.[1] We do not observe that treatment effects differ according to respondents' gender or caste. We also observe that exclusive historical representations increase Muslim respondents' willingness to lead among BJP supporters (e.g., among Muslims with low levels of group consciousness).

Table C17: **Heterogeneous Effects Given Gender and Caste Identity**

	Dependent variable:			
	Willingness to Lead			
	Muslims	*Hindus*	*Muslims*	*Hindus*
	(1)	(2)	(3)	(4)
Exclusive	−0.160	0.261	0.028	0.005
	(0.281)	(0.284)	(0.079)	(0.066)
Inclusive	0.149	0.313	0.166**	−0.036
	(0.274)	(0.292)	(0.080)	(0.067)
Woman	0.009	0.089		
	(0.075)	(0.079)		
Exclusive*Woman	0.088	−0.093		
	(0.109)	(0.110)		
Inclusive*Woman	0.006	−0.128		
	(0.106)	(0.113)		
Low Caste			0.007	−0.151*
			(0.076)	(0.084)
Exclusive*Low Caste			0.068	0.051
			(0.109)	(0.119)
Inclusive*Low Caste			−0.005	0.056
			(0.107)	(0.123)
Observations	753	839	753	839
Adjusted R^2	0.008	−0.004	0.007	0.002

Note: *p<0.1; **p<0.05; ***p<0.01

---

[1]Due to space limitations and because our empirical expectations were exploratory in nature, we do not present analyses for every stated expectation. However, these analyses are available upon request.

Table C18: **Heterogeneous Effects Given BJP Support and Levels of Group Consciousness**

	Dependent variable:			
	Willingness to Lead			
	Muslims	*Hindus*	*Muslims*	*Hindus*
	(1)	(2)	(3)	(4)
Exclusive	−0.009	0.240*	0.099	0.170
	(0.076)	(0.127)	(0.258)	(0.231)
Inclusive	0.131*	0.211	0.272	−0.117
	(0.075)	(0.134)	(0.237)	(0.231)
BJP Vote	−0.117	0.106		
	(0.079)	(0.095)		
Exclusive*BJP Vote	0.217*	−0.223		
	(0.115)	(0.141)		
Inclusive*BJP Vote	0.095	−0.194		
	(0.111)	(0.148)		
Group Consciousness			0.299	0.529**
			(0.215)	(0.228)
Exclusive*Group Consciousness			−0.051	−0.221
			(0.318)	(0.323)
Inclusive*Group Consciousness			−0.136	0.142
			(0.296)	(0.324)
Observations	608	608	753	839
Adjusted R^2	0.013	−0.001	0.011	0.013

Note: *p<0.1; **p<0.05; ***p<0.01

Table C19 shows that the inclusive history treatment positively affects Muslim respondents sense of being deserving to lead among those with high levels of group consciousness.

Table C19: **The Conditional Effect of the Treatments Given Group Consciousness on Feelings of Being Qualified and Worthy to Lead**

	Dependent variable:			
	Qualified		Deserving	
	(1)	(2)	(3)	(4)
	Muslims	*Hindus*	*Muslims*	*Hindus*
Exclusive*Group Consciousness	0.058	0.473	−0.151	0.394
	(0.423)	(0.468)	(0.463)	(0.478)
Inclusive*Group Consciousness	−0.078	0.092	0.869**	−0.239
	(0.394)	(0.471)	(0.431)	(0.480)
Exclusive	−0.118	−0.385	0.102	−0.372
	(0.343)	(0.335)	(0.375)	(0.342)
Inclusive	0.041	−0.067	−0.565	0.122
	(0.316)	(0.335)	(0.346)	(0.342)
Group Consciousness	0.326	1.088***	−0.011	1.170***
	(0.286)	(0.331)	(0.313)	(0.338)
Observations	753	839	753	839
Adjusted R^2	−0.001	0.046	0.009	0.042

*Note:* *p<0.1; **p<0.05; ***p<0.01

C.6 The role of pre-treatment history education

In order to investigate possible pre-treatment effects of real-world exposure to different historical narratives and how they might interact with our experimental treatments, we sampled states with different histories and timings of textbook reforms (see Section 4.1). We calculate the years of education under BJP state governments as a proxy for exposure to an exclusive historical narrative, as it is against these governments that accusations of rewriting history have most recently and often been levied.[2,3] While Gujarat shifted to BJP state-level government control in 1995 and has not changed since, periods of control under BJP and its allies differed in Maharashtra (1995-1999, 2014-present), Uttar Pradesh (1991-1993, 1997-2002, 2017-present), and Delhi (1993-1998). Consistently, as Figure C10 illustrates, estimated years of education under BJP state governments are comparatively higher for respondents from Gujarat.

---

[2]Specifically, we calculate years of education for which BJP and/or BJP allies held a plurality of the state legislature and the chief minister position, using data from `https://lokdhaba.ashoka.edu.in/` (accessed October 11, 2021), as well as participants' stated ages and educational attainments, and assuming that they began formal state schooling at the age of six.

[3]As noted in Appendix Section E.3, it is widely acknowledged that India today is a one-party dominant system with BJP and its partners at the helm (Chhibber and Verma 2019). Thus, while we might expect some variation in degree across states, most generally we would presume some level of exposure to the Hindutva narrative across most if not all states, and certainly across all four states in our sample – which, as shown in this Section, have all experienced periods of BJP or like-minded party control at the state level. This exercise is thus meant to proxy for differences in degree of exposure, then, with the presumption that most if not all respondents have experienced some minimum amount of exposure.

Figure C10: **Years of BJP Education, Overall and by State**
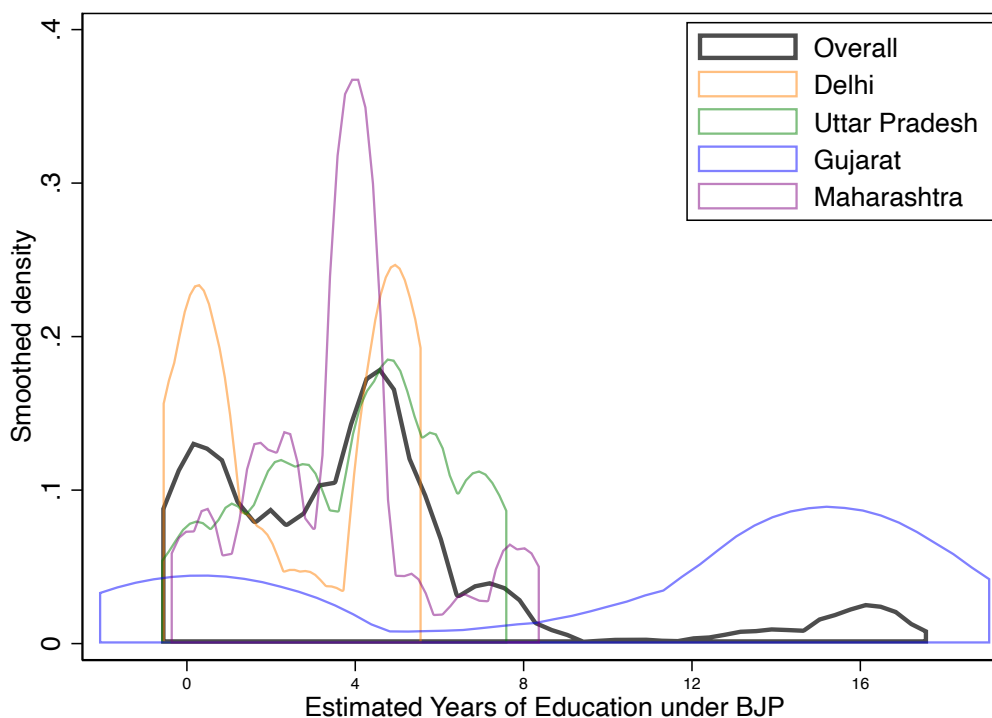
Notes: This graph displays the densities of participants' estimated years of education under BJP and BJP-allied state governments, overall and by state.

Figure C11 plots treatment effects on Muslims' (left panel) and Hindus' (right panel) willingness to lead, over participants' binned years of estimated exposure to an exclusive historical narrative (as proxied by estimated years of education under a BJP or BJP-allied state government). We observe that the positive effect of the *inclusive* treatment on Muslims' supply of leadership is particularly strong for those Muslim participants with fewer years of education under the BJP. We do not observe a relationship between years of education under the BJP and effects of either the *exclusive* treatment on Muslims' willingness to lead, or the *inclusive* treatment on Hindus' willingness to lead. Lastly, we observe that the *exclusive* treatment has a positive effect on the supply of leadership among Hindus with more years of education under BJP governments.

How should we interpret these results? First, it could be that participants are (at least in the short term) primarily receptive to teachings from versions of history with which they are familiar, so long as that history *contains a positive bias toward their social group*. Such an interpretation could explain why Muslims (Hindus) with lesser (greater) exposure to an exclusive history in the real world increase their supply of leadership comparatively more in response to our *inclusive* (*exclusive*) experimental treatments. It could also explain why Hindu and Muslim respondents register little change in response to treatments that are inconsistent with their real world exposure, or which portray their social group with a comparatively more negative bias. One important implication would be that an exclusive historical telling can decrease receptivity to alternative, inclusive historical narratives *even among minority groups that would seemingly benefit from them.*

A second alternative interpretation is that Muslims with lesser exposure to an exclusive narrative will be more receptive to an alternative (inclusive) narrative that displays their social group positively because they are less likely to have a strong prior as to which narrative is correct. While possible, we think it more likely that educated Muslims with little exposure to an *exclusive* narrative received a comparatively more *inclusive* narrative, as opposed to no

Figure C11: **Treatment Effects by Years of BJP Education, Hindus and Muslims**
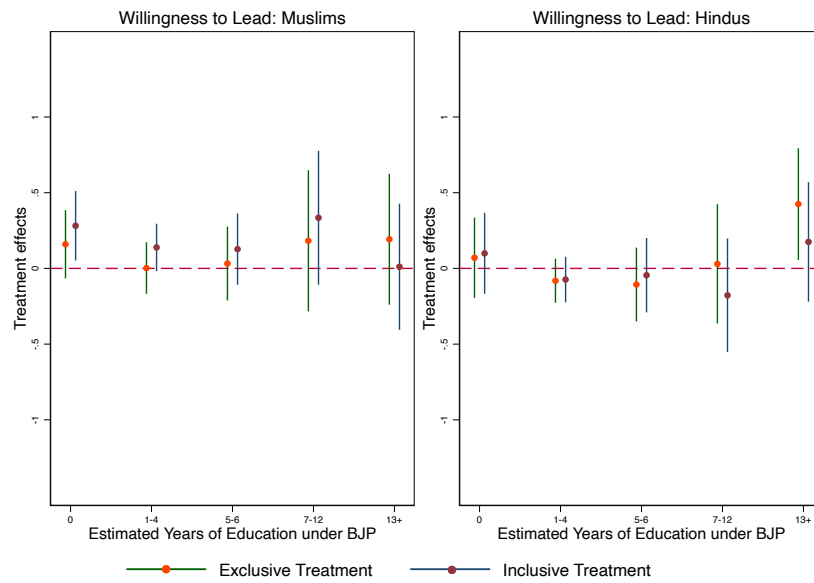
Notes: This graph displays treatment effects on willingness to lead for Muslims (left panel) and Hindus (right panel), by the binned number of estimated years of education under BJP or BJP-allied state government. Regressions include controls for age, state, years of total education, and whether the participant attended a public (government) school. Full regression output can be found in Tables 9 (Muslim respondents) and 10 (Hindu respondents) of the Supplementary Material on Dataverse.

narrative at all; this interpretation would also not explain the positive effect of the *exclusive* treatment on supply of leadership among Hindus with high exposure to BJP-led education.

Third, it is possible that *all* participants are familiar with the *exclusive* historical narrative, conveyed through textbooks or some other medium, which reduces the impact of the *exclusive* treatment except for those who most fervently believe the material due to very high levels of exposure and thus reject all alternatives (including the politically neutral *baseline* narrative). In contrast, an *inclusive* narrative might have an impact among those with the greatest reason to be receptive to its teachings (in our study, Muslims).

Fourth, differences across years of exposure to BJP education could be driven by other factors. Although we control for a respondent's age, years of total education, state, and type of education, there may be other, unaccounted-for differences between those receiving more or less BJP or BJP-allied education that drive results. We also have comparatively few participants with a high number of years of BJP schooling, and thus can be less certain of estimates for those at higher levels of exposure. We treat the evidence presented above as largely speculative, and we intend to conduct a follow-up observational study to evaluate the impact of textbook reforms in India on identity.

Despite these latter caveats, we note that the evidence discussed in this section suggests that real world historical narratives *can matter* and interact meaningfully with experimental stimuli, in particular by shaping individuals' perceptions about which historical narratives are valid and which are not.