# Symbolic Policies Cause Economic Harm*

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**Abstract**

Ethnonationalist governments adopt symbolic policies that challenge the status of ethnic and religious minorities as equal members of the nation. We propose that exposure to such policies reduces the cognitive bandwidth of marginalised groups, leading to suboptimal economic decisions. We test this hypothesis in India, where the Muslim minority population has faced exclusionary policies. About 1,800 gig workers performed data entry and information processing tasks in an online field experiment. In the course of completing their tasks, workers were randomly exposed to social media content referencing exclusionary policies. The policies varied in whether they were primarily symbolic or whether they were also likely to cause direct material harm to affected Muslim participants. We measured decision making by allowing workers to choose between two payment contracts—one of which would objectively lead to higher earnings for a given worker. Despite no change in productivity, treated Muslim participants were significantly more likely to select economically suboptimal ('wrong') contracts, leading to lower earnings. We explore additional data, including cognitive tests and categorisations of mistakes, that suggest that wrong contract choice is driven by increased cognitive load. Our findings demonstrate how even purely symbolic exclusionary policies can have tangible economic consequences, potentially exacerbating marginalisation of minority groups through cognitive and psychological channels.

*Keywords:* Ethnonationalism, Exclusionary policies, Cognitive bandwidth, Decision-making, Identity politics, Minority marginalisation, Behavioural economics, Social psychology, Religious minorities. *JEL Codes*: D91, J15, D83, Z13, J71, P16, Z12.

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# 1 Introduction

Government policies do more than allocate resources. They also serve symbolic functions, shaping how citizens understand collective identity and their place within the national community. Public monuments, place names, language laws and school curricula can all signal which groups are recognised as legitimate members of the nation and which are cast as outsiders. In Israel, the 2018 'Nation-State Law' enshrines Jewish national symbols and downgrades Arabic from its status as an official language (Barak-Corren et al., 2022). In the United States, the second Trump administration has sought to restore symbols of the Confederacy in public spaces (Ankori-Karlinsky, 2026). In India, cities associated with Muslim rulers have been re-named, and history textbooks revised to reduce references to Muslim contributions to Indian history (Haas and Lindstam, 2024; Jaffrelot, 2021). In this article, we argue that such *symbolic* policies generate material consequences even in the absence of direct changes to individuals' economic opportunities or incomes.

Individuals derive part of their identity and self-worth from group membership (Akerlof and Kranton, 2000; Shayo, 2009; Tajfel and Turner, 1986). By signalling that certain groups are outsiders to the national community, exclusionary symbolic policies may generate feelings of insecurity and non-belonging that become a source of concern and preoccupation, consuming mental resources. Meanwhile, economic decision-making requires cognitive resources (Enke, 2024). When attention and working memory are occupied by worry, preoccupation, or unresolved threats, fewer resources remain available for careful reasoning, increasing the likelihood of costly mistakes (Sweller, 1988; Kahneman, 2011). For instance, cognitive load can increase risk aversion, impatience, and costly decision errors, with larger effects when decisions are more complex (Deck and Jahedi, 2015; Gabaix and Graeber, 2024).[1] Consistent with this, reducing financial worries improves worker productivity and reduces mistakes, particularly among poorer workers (Kaur et al., 2025).[2] We argue that exclusionary policies, even when primarily symbolic, impose a similar cognitive burden, consuming mental resources that would otherwise support economic decision-making.

We conduct an experiment in India to examine the cognitive and economic consequences of symbolic exclusionary policies. India provides a particularly relevant setting because recent political developments have increasingly linked national belonging to majoritarian conceptions of identity.

[1] We refer to cognitive load loosely as the set of demands on finite working memory.

[2] Cognitive demands also shape behaviour in other high-stakes settings, such as policing (Dube et al., 2025).

Under the Bharatiya Janata Party (BJP), state policy has become increasingly aligned with Hindu majoritarian ideology, particularly in relation to India's large Muslim minority. Since 2014, a series of controversial policies have signalled that Muslims are not fully recognised as members of the national community. India, however, is not unique. Similar dynamics have emerged globally, from anti-immigrant rhetoric in Europe and the U.S. to exclusionary nationalisms in Turkey and China (Simonsen and Bonikowski, 2020; Eichengreen, 2018; Mukand and Rodrik, 2018). By focusing on India, our study offers insights into a broader global trend in which ethnic and religious minorities are increasingly cast as outsiders within the nations they call home.

We recruited a sample of 1801 online workers in India to complete basic data processing tasks, including tagging and labelling factual news content posted on Twitter.[3] The news articles referred to recent policies, such as renaming of cities associated with Muslim rulers, the construction of Hindu nationalist monuments, and revisions to school textbooks removing Muslim historical figures. We randomly varied whether participants saw tweets about policies that excluded minorities or those that were neutral.[4] We measured participants' productivity on the tasks as well as the quality of their economic decisions. Participants were offered the choice between two payment contracts: a piece-rate contract paying a fixed amount for each correct transcription regardless of others' performance, and a tournament contract offering a larger reward to top performers and nothing otherwise. We define a decision mistake as the failure to choose the contract that maximised earnings and examine whether exposure to exclusionary policies increases the likelihood of such mistakes.

We find that exposure to symbolic exclusionary policies worsens economic decision-making among Muslim participants, making them more likely to choose the wrong payment contract and leave money on the table. This is not because they perform worse on the main task. Productivity is largely unchanged, yet treated Muslims earn less than those in the control group. The effects are similar across policies that are primarily symbolic and policies that also have potential material implications. This suggests that the symbolic component of exclusion, rather than the prospect of direct material consequences, is sufficient to generate economic harm. Far from being merely

[3] Although the platform was rebranded as X in 2023, we use the term "Twitter" throughout the paper for clarity and consistency with the existing academic literature.

[4] Participants were exposed only to real policy discussions rather than hateful or abusive content. Appendix B provides a detailed discussion of the study's ethical considerations.

expressive, such policies can reduce earnings by increasing costly decision mistakes.[5]

We next examine the mechanisms underlying these effects. Consistent with our argument, exposure to exclusionary messages reduces feelings of belonging and recognition while increasing stress and concerns about discrimination among Muslim participants. It also impairs performance on cognitive tasks. The evidence points to cognitive burden rather than changes in beliefs or preferences. Treatment does not affect beliefs about which contract is optimal or beliefs about relative performance. Moreover, it increases both types of mistakes: choosing the tournament when the piece-rate contract is optimal and choosing the piece-rate contract when the tournament is optimal. This pattern is inconsistent with increased risk aversion, overconfidence, or directional shifts in preferences. We also find suggestive evidence that insecurity is a plausible pathway. The treatment causally increases feelings of insecurity and exclusion, while in the control group, participants reporting greater insecurity are more likely to make contract-choice mistakes, even after controlling for performance, beliefs, and pre-treatment covariates. Although this does not constitute a causal mediation analysis, the pattern is consistent with the view that heightened insecurity and diminished belonging consume cognitive resources and thereby contribute to poorer economic decision-making.

Although our pre-registered hypotheses focus on Muslim participants, we also explore treatment effects among Hindus. In contrast to Muslims, Hindus do not experience increased insecurity, reduced cognitive performance, or higher rates of contract-choice mistakes following exposure to the treatment. This pattern is consistent with our interpretation that the effects operate through threats to belonging and recognition.[6]

We contribute to several strands of the literature. First, we contribute to a nascent but growing literature showing that symbolic policies communicate information about status, power, and social order, helping coordinate collective narratives and norms even in the absence of material redistribution (Mendelberg, 2022; Rozenas and Vlasenko, 2022). Recent work shows that such interventions shape political attitudes, trigger backlash, and influence political behaviour through

[5]Our results are robust to placebo and re-randomisation exercises and remain qualitatively similar in a separate in-person experiment conducted in West Bengal.

[6]Some Hindus nevertheless earn less following treatment exposure, not because they make more mistakes, but because a subset of Hindus, likely to support the treatment policies, reduces effort on the productivity task. These exploratory findings suggest that exclusionary policies may affect majority and minority groups through distinct channels.

historical narratives (De Juan et al., 2025; Haas and Lindstam, 2024), commemorative status (Rahnama, 2025; Turkoglu et al., 2023), museum exhibitions (Balcells et al., 2022; Balcells and Voytas, 2025), and place names (Lindstam and Haas, 2026; Villamil and Balcells, 2021). Beyond shaping political attitudes and behaviour, exclusionary symbolic policies can impose substantial economic and social costs on targeted groups. The French headscarf ban disrupted Muslim girls' educational trajectories by increasing perceptions of discrimination (Abdelgadir and Fouka, 2020); Denmark's "ghetto laws" reduced assimilation efforts and income (Foged et al., 2025; Govind et al., 2024); and a symbolic change in French citizenship law reduced national identification among second-generation immigrants (Baudin et al., 2025). Confederate monuments reduced the Black population share in the U.S. South through out-migration, lowered Black respondents' willingness to accept jobs in depicted locations, and altered racial violence patterns by shaping shared beliefs about social order (Ferlenga, 2025; Ankori-Karlinsky, 2026).

While this literature establishes that symbolic policies can shape beliefs, identification, migration, and behaviour, we highlight a distinct mechanism through which they can generate economic harm. We show that exclusionary symbolic policies impose *immediate* material costs by impairing economic decision-making. Importantly, this mechanism does not require meaningful updating of political beliefs or expectations. After many years in power, the unveiling of another symbolic monument or the renaming of a city may convey little new information about who holds power in society. Yet such policies can still make concerns about belonging, recognition, and discrimination more salient among targeted groups. These concerns consume cognitive resources, leaving fewer resources available for careful decision-making and increasing the likelihood of costly errors. In doing so, we identify a novel channel through which symbolic policies generate economic harm: by taxing cognitive bandwidth and impairing decision quality.

Second, we contribute to a growing literature examining how psychological burdens shape economic behaviour. This literature argues that poverty-related constraints impair cognition and decision-making, with important consequences for economic behaviour (Mani et al., 2013; Carvalho et al., 2016; Lichand and Mani, 2020).[7] However, the existing literature has focused almost exclu-

[7]More generally, financial concerns have been shown to affect psychological well-being (Mullainathan and Shafir, 2013; Chemin et al., 2013; Haushofer and Shapiro, 2016, 2018; Green et al., 2016) as well as decision-making and preferences, such as time discounting (Shah et al., 2015; Ong et al., 2019; Fehr et al., 2022; Bartos et al., 2018), and worker productivity (Kaur et al., 2025).

sively on economic constraints such as the paucity of financial resources or time, largely ignoring the impact of identity-related concerns. The reviews by Ridley et al. (2020); Schilbach et al. (2016); Mullainathan and Shafir (2013); Haushofer and Fehr (2014) and Kremer et al. (2019), for instance, make no mention of exclusion or identity. We fill this gap by examining whether identity threats, such as those posed by exclusionary policies, impose cognitive burdens similar to those traditionally associated with poverty, thereby affecting economic behaviour and decision-making.[8]

Third, our findings complement the discrimination and stereotype threat literature. A large body of research shows that stereotypes, prejudice, perceived discrimination, and identity salience shape economic outcomes for minority groups. Research on stereotype threat documents how making a negative stereotype salient can impair performance in stereotype-relevant domains (Steele and Aronson, 1995; Coffman, 2014). These effects influence educational achievement (Carlana, 2019), labour market outcomes (Glover et al., 2017), health care (Alsan and Wanamaker, 2018), and income mobility (Chetty et al., 2020). Related work shows that making social identity salient can alter expectations about fair treatment and evaluation (Hoff and Pandey, 2014). More broadly, recent research has examined the psychological and emotional consequences of discrimination, including trauma, stress, and emotional disturbance (Ang, 2021).

Our work studies a different mechanism. Rather than interpersonal discrimination or identity salience, we focus on exclusionary state policies that signal who belongs within the national community. We examine whether exposure to such policies, in the ordinary media environments where citizens routinely encounter political information, imposes cognitive costs that affect economic decision-making. In doing so, we broaden the study of identity-related economic costs beyond interpersonal interactions to include the consequences of symbolic exclusion.[9]

Our mechanism also differs from stereotype threat in an important respect. Stereotype threat arises from anxiety about confirming a negative stereotype regarding group competence and typically reduces performance on the stereotype-relevant task itself. By contrast, the policies we study do not question Muslim participants' competence; they question their standing as full members of the nation. The relevant threat therefore concerns belonging rather than ability. Consistent

[8]More broadly, we identify identity-related concerns as a previously overlooked source of cognitive burden, extending a literature that has traditionally focused on poverty, financial scarcity, and psychological poverty traps (Dean et al., 2017).

[9]Relatedly, a growing literature studies the political and social consequences of social media and online platforms; see Zhuravskaya et al. (2020) for a review and Müller and Schwarz (2021); Enikolopov et al. (2020) for examples.

with this distinction, we observe no decline in task productivity. Instead, the effects appear in a separate economic decision, suggesting a broader reduction in cognitive bandwidth rather than a task-specific performance effect.

# 2 Conceptual Framework: Symbolic Policies and Cognitive Load

## 2.1 What are Symbolic Policies?

Public policies often contain both material and symbolic components (Boussaguet and Faucher, 2025; Rozenas and Vlasenko, 2022; Tallent et al., 2026). A policy's material dimension refers to the extent to which it allocates resources, imposes costs, or regulates behaviour in ways that produce tangible and measurable effects. Examples of such policies include tax reforms, welfare benefits, or labour market regulations that directly affect individuals' incomes and opportunities. By contrast, a policy's symbolic dimension concerns the meanings it conveys to the public (Tallent et al., 2026). Public policies have an important communicative role that shapes how citizens interpret social categories, collective identities and political authority (Boussaguet and Faucher, 2025). While most policies combine material and symbolic elements to varying degrees, some function primarily as signals intended to convey particular values or interpretations of social reality. Examples include the adoptions of national symbols, the renaming of cities and streets, or the establishment of commemorative practices.

Building on recent work by Tallent et al. (2026, 4), we define symbolic policies as those that "convey meaningful messages to the public while having minimal material impact". Earlier scholarship often treated such policies as mere "spectacle" and, by definition, as lacking tangible effects (Edelman, 1988; Mazur, 2010; Suárez, 2014). As Boussaguet and Faucher (2025, 12) highlight, such policies were frequently viewed as lacking any real output as they "incur no administrative or financial costs" and "do not influence the general allocation of resources". Consistently, Rahnama and Williamson (2025) show that many citizens view symbolic policies as distractions, unrelated to contemporary social justice outcomes. However, such perspectives overlook the fact that symbolic policies can have powerful downstream effects. Once interpreted by audiences, the messages embedded in policies can trigger reactions whose intensity depends on how strongly they resonate with individuals emotions, concerns and identities (Tallent et al., 2026). Although these policies may

not be designed to produce *direct* material consequences, they can shape perceptions of inclusion and exclusion, and thereby potentially influence attitudes and behaviour.

## 2.2 Exclusionary Symbolic Policies in India

India provides a useful context for studying the individual-level consequences of exposure to exclusionary symbolic policies. Since 2014, the Bharatiya Janata Party (BJP) has been the governing political party and has promoted an ideology commonly referred to as *Hindutva*. This ideology emphasises India's identity as a Hindu nation and draws partly on early twentieth-century forms of ethnic nationalism (Jaffrelot, 1999, 2021). Many of the policies introduced during the BJP's period in office reflect this conception of national identity. Examples include the Citizenship Amendment Act (CAA), which offers an expedited path to citizenship for undocumented migrants from Afghanistan, Bangladesh, and Pakistan who belong to specified non-Muslim religious communities (Hindus, Sikhs, Buddhists, Jains, Parsis, and Christians), while excluding Muslims[10]; restrictions on Muslim attire in some states; and the renaming of public spaces and the construction of monuments honouring Hindu figures (Nussbaum, 2007; Jaffrelot, 2017; Roy, 2024).

We classify exclusionary policies in contemporary India into two broad categories, which we term *Strongly Symbolic* and *Weakly Symbolic* policies. This distinction builds on our definition of symbolic policies as those that rely primarily on their meaning-making function rather than their immediate material impact. Strongly Symbolic policies do not cause direct or immediate material harm but operate through cultural and psychological exclusion. They include renaming cities and public institutions associated with India's Muslim heritage to Hindu names, and removing references to Muslim rulers such as the Mughals from school textbooks. These measures reshape representations of national history and culture in ways that elevate Hindu heritage while reducing the visibility of Muslim contributions. Although they do not directly impose large material costs, they communicate messages about who belongs to the nation and can influence how individuals perceive their own status. Repeated exposure to such signals may generate uncertainty and psychological strain.

[10]The CAA, together with the associated National Register of Citizens (NRC), provoked widespread protests across India in 2019–2020, among the largest mobilisations in the country in recent decades. Critics argued that, by making religion a criterion for citizenship for the first time, the CAA in combination with a nationwide NRC could be used to deny citizenship to Muslims unable to document their status, while offering excluded non-Muslims a route to reclaim it.

Weakly Symbolic policies combine symbolic messaging with the potential for direct material consequences. These policies convey exclusionary messages similar to those classified as Strongly Symbolic, but they also create the possibility of tangible harm for minority communities. In this sense, their effects are not purely symbolic, although their primary function remains communicative rather than redistributive.[11] An example is the National Register of Citizens (NRC), which places the burden of proving citizenship on individuals and classifies those unable to do so as "illegal". Nearly two million people were excluded from the 2018 Assam citizenship draft, and detention centres were constructed for those deemed non-citizens.[12][13] Other examples include restrictions on cow slaughter, anti-conversion laws and hijab bans in school – policies which both send strong exclusionary messages while at the same time restricting the behaviour among subgroups of religious minorities. In sum, in our framework, both Strongly Symbolic and Weakly Symbolic policies operate primarily through their symbolic dimension, while differing in whether they also carry direct material consequences.

Figure 1 provides a snapshot of perceptions of belonging and discrimination among Hindus and Muslims in India using representative data from Pew Research Center (N = 29,999).[14] The Figure shows responses to three questions, and reveals clear differences between religious groups. A significantly higher proportion of Muslims report perceiving discrimination against Muslims or having experienced discrimination in the past year, compared with Hindus, while they are less likely to agree with the statement "It is important to be Hindu to be a true Indian". These patterns highlight variation in perceived belonging and the potential salience of identity-linked exclusion among Muslim respondents.

[11] We do not classify these policies as material, despite their potential for material harm. This is because any material consequences tend to be limited in scope – either affecting a relatively small subset of the population or remaining geographically restricted. For example, cow slaughter regulations primarily affect a small group of workers in the meat industry, while the National Register of Citizens (NRC) has so far been implemented only in Assam. As such, these policies continue to operate primarily through their symbolic dimension, albeit to a lesser extent than those classified as Strongly Symbolic.

[12] For an account of the detention camps, see Siddique (2020).

[13] The Citizenship Amendment Act (CAA) interacts with the NRC by offering a path to citizenship for non-Muslim groups who might otherwise be excluded. The United Nations has described the CAA as "fundamentally discriminatory". UN News (2019). India's new citizenship law is 'fundamentally discriminatory': UN human rights office. 13 December. Available at: `https://news.un.org/en/story/2019/12/1053511` (Accessed 25 May 2025).

[14] https://www.pewresearch.org/dataset/india-survey-dataset/

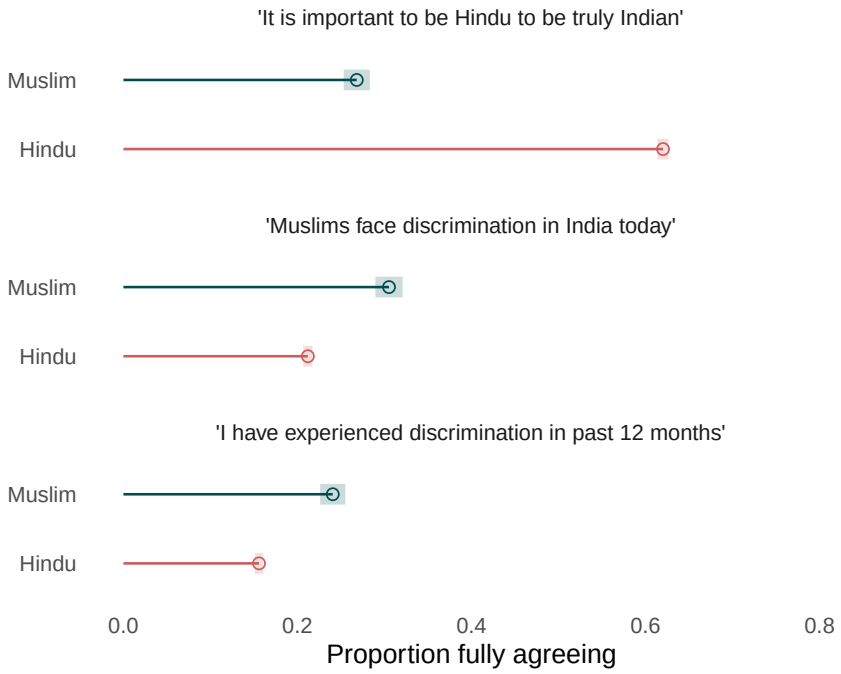



Figure 1: **Differences in Belonging across Hindus and Muslims**

*Note*: This figure displays the proportion of Hindu and Muslim respondents who agree with statements about Indian belonging and concerns about discrimination in India. Shaded boxes represent 95% confidence intervals. N = 29,999. The data come from a nationally representative sample collected by the Pew Research Center.

## 2.3 How do Symbolic Policies affect Cognitive Load?

Exclusionary symbolic policies send signals about who is, and who is not, considered an authentic member of the nation. Repeated exposure to such signals may increase feelings of insecurity, exclusion, and uncertainty among members of targeted groups. We argue that these responses can have economic consequences by affecting the cognitive resources available for decision-making. Social identity theory explains why such signals bite. People derive part of their self-concept from group membership and seek secure standing for their groups (Tajfel and Turner, 1986). Policies that signal a group's exclusion from the national community may therefore threaten individuals' sense of recognition, belonging, and social standing. Integrated threat theory separates symbolic threats to a group's recognition and standing from realistic threats to material resources (Stephan and Stephan, 2000). Exclusionary symbolic policies act primarily through the former channel. They question whether minorities are authentic members of the nation, independent of any direct material consequence, and may thereby undermine the fundamental need to belong and to be

recognised as a valued member of society (Baumeister and Leary, 1995; Kruglanski et al., 2022).[15] Such threats may generate persistent concerns about status and belonging, leading individuals to devote attention and mental effort to questions unrelated to the task at hand.

A large literature shows that concerns that occupy attention and working memory can impair decision-making. When cognitive resources are diverted towards managing salient worries or unresolved concerns, fewer resources remain available for careful reasoning, increasing the likelihood of mistakes. Cognitive load affects economic decisions by increasing impatience, distorting choices, and reducing decision quality in a variety of settings (Deck and Jahedi, 2015; Gabaix and Graeber, 2024). Related work in the scarcity literature argues that financial concerns can similarly capture attention and reduce the cognitive resources available for other decisions (Schilbach et al., 2016; Dean et al., 2017).

Threats to identity and social standing can themselves increase cognitive load. Experimental evidence on social identity threat shows that activating a devalued identity reduces working memory capacity and impairs cognitive functioning beyond the focal task (Schmader and Johns, 2003; Schmader et al., 2008). Although the policies we study do not question competence directly, they may nevertheless generate concerns about recognition and belonging that place demands on limited cognitive resources. More generally, carrying an unrelated cognitive burden can distort subsequent choices and increase reliance on more impulsive decision-making (Shiv and Fedorikhin, 1999).

We argue that exclusionary symbolic policies operate through a similar mechanism. By directing attention towards concerns about identity, recognition, and belonging, exclusionary policies may compete for limited cognitive resources, divert attention from other tasks, and distort decision-making (Bordalo et al., 2016). In our framework, exclusionary policies increase feelings of insecurity and non-belonging. These concerns capture attention and reduce the cognitive resources available for economic decisions, increasing the likelihood of costly mistakes even when individual preferences and beliefs remain unchanged.

Our study is designed to test this mechanism. To measure distortions in decision-making, participants choose between two payment contracts that differ in their risk and reward structure. We define a decision mistake as choosing the contract that yields lower earnings than the available

[15] Significance-quest theory was developed primarily to explain radicalisation and violent extremism (Kruglanski et al., 2022). We draw only on its core motivational premise: the desire to matter and to be recognised as a person of worth.

alternative. Throughout the paper, we use the term "mistake" in a manner related to Nielsen and Rehbeck (2022), who define mistakes as choices that decision-makers would revise when confronted with inconsistencies between their preferences and choices. In our setting, participants complete a simple and familiar online work task in a short time frame, making it unlikely that they intentionally prefer lower earnings to higher earnings. We therefore interpret choices that leave money on the table as evidence of impaired decision quality.

# 3 Experiment Design

## 3.1 Overview of the experiment

We conducted an online field experiment with 1,801 gig workers across India, whom we hired to complete two data processing tasks. The first task involved tagging news-related tweets concerning government policies. This task served as the treatment: we randomised the content of the tweets to expose the treatment group to exclusionary symbolic policies. The second task required participants to transcribe text from social media screenshots, and served as our measure of productivity. Following the transcription task, participants were asked to choose a payment contract, which we use as our measure of economic decision-making. The two tasks differ in their cognitive demands: the productivity task is relatively low in cognitive complexity, while the contract choice task involves higher-order cognitive processing.

The experiment followed a structured eight-step sequence: (1) a baseline module to collect demographic information; (2) the tweet-tagging task, during which participants were exposed to the treatment manipulation; (3) a short survey capturing participants' feelings of national belonging and perceived social status; (4) a Stroop Test to assess cognitive interference; (5) a productivity task involving transcription; (6) a recapitulation of tweets, reminding participants of the tweets they had previously seen, reinforcing treatment salience immediately prior to the economic decision-making stage; (7) a payment choice task designed to measure economic decision making prowess; and (8) a final debriefing module. The sections below provide further detail on recruitment, treatment design, outcome measures, and our pre-analysis plan.

### 3.2 Recruitment and sampling

To study the cognitive and economic consequences of exclusionary policies, particularly among minority populations, we implemented an online experiment. This approach allowed us to target the kinds of gig economy workers typically engaged in data processing. The study was initially conceived as a fully online experiment. However, due to delays and uncertainty linked to the pandemic, we conducted a soft launch in-person under more controlled field conditions, followed by a larger, pre-registered online experiment. While our main results will focus on the online experiment, we will also report results from the in-person experiment.

For our main online field experiment, we recruited participants from eleven Indian states: Uttar Pradesh, Maharashtra, West Bengal, Andhra Pradesh, Telangana, Karnataka, Uttarakhand, Delhi, Gujarat, Assam, and Kerala. These states were selected based on two criteria: a substantial Muslim population and variation in governing party ideology.[16] Together, they represent approximately 700 million individuals (58% of India's total population), and 120.8 million Muslims (70.15% of India's Muslim population), according to the 2011 Census.

In the main experiment conducted online, participants were recruited via TGM Research.[17] Our sample was balanced, with approximately 50% Hindu and 50% Muslim participants. Participants received a flat participation fee as well as performance-based incentives for the experimental tasks described below. For a 30-minute survey, participants were compensated USD 2.50.[18]

### 3.3 Treatment variation

**Task 1: Treatment through Exposure to Exclusionary Policies.** We used the first task to generate treatment variation. In the first task, participants screened and evaluated a series of tweets about news articles discussing various government policies in India. Participants were asked to evaluate three aspects of each tweet: first, they were asked to categorise the topics of the tweets, choosing all applicable categories from a predefined list. Second, they were asked to rate the sentiment of the tweet on a sliding scale from -5 to 5. Finally, they were asked to count the

[16] In 2024, the states were governed by the following parties: Uttar Pradesh (BJP), Maharashtra (BJP-led Mahayuti coalition), West Bengal (AITC/TMC), Karnataka (INC), Telangana (INC), Gujarat (BJP), Assam (BJP), Uttarakhand (BJP), Delhi (AAP), and Kerala (LDF/CPI(M)). Andhra Pradesh changed government mid-year: YSRCP governed until June 2024, after which the TDP-led NDA coalition (TDP, BJP, and Jana Sena) took power following the June 2024 state assembly elections.

[17] `https://tgmresearch.com/`

[18] See `https://tgmpanel.in/en/payments-and-rewards.html` for details on how payments are executed in practice.

number of spelling mistakes in the main tweet, instructing them to ignore any comments or replies. These questions were meant to ensure that participants were engaged with the content, reinforcing the treatment exposure.

We randomly varied the policies discussed in the news articles embedded in the tweets. Participants were randomly assigned to one of three treatment arms. Below, we enumerate the contents of the tweets by policy type.[19]

1. **Treatment Arm 1: Strongly Symbolic Policies.** Participants saw tweets referencing policies that conveyed symbolic harm to minorities, creating a sense of exclusion without direct material consequences. These included:

   (a) Increasing the height of a statue of Shivaji, a Hindu warrior-king, symbolising majoritarian valorisation.

   (b) Renaming the city of Mughal Sarai, associated with India's Muslim heritage, to a Hindu name.

   (c) Renaming a Mughal Museum in Uttar Pradesh to honour a Hindu historical figure, reinforcing Hindu dominance in cultural representation.

   (d) Mandating the singing of India's national song, *Vande Mataram* ("Hail Mother" in English), in municipal schools, which carries Hindu religious connotations.

   (e) Removing references to Muslim rulers, such as the Mughals, from high school history textbooks, thus altering the historical narrative to exclude Muslim contributions.

2. **Treatment Arm 2: Weakly Symbolic Policies.** Participants saw tweets referencing exclusionary policies that, while also symbolic in nature, additionally carried the potential for material or legal consequences for Muslims. These included:

   (a) The Citizenship Amendment Act (CAA), which offers citizenship to non-Muslim groups unable to prove their legal status but excludes Muslims, creating a discriminatory legal framework.

[19] Appendix Figure S3 shows the complete set of tweets. Appendix Figure S4 provides an example of a neutral tweet (Panel (a)).

(b) The National Register of Citizens (NRC), which requires individuals to prove their citizenship status, placing the burden on minorities who may not have adequate documentation.

(c) Anti-Conversion Laws that restrict religious conversions, particularly those involving Muslims, often justified as necessary for social cohesion.

(d) Bans on girls wearing the hijab in schools, which impose restrictions on Muslim religious expression.[20]

(e) Prohibitions on cow slaughter, primarily impacting Muslim and other minority communities who traditionally engage in this practice, reinforcing religious restrictions.

3. **Control Group: Neutral Policies.** Participants in the control group saw tweets that reference politically neutral, general interest policies, with no reference to national exclusion. The tweets included:

(a) Advocacy for a one-term pension for former Members of the Legislative Assembly (MLAs), unrelated to any religious or ethnic implications.

(b) Announcements about developing a protocol for the micropropagation of mahogany trees for environmental conservation.

(c) Guidelines for pet owners to obtain licenses, targeting administrative improvements.

(d) Organising local plastic cleanup drives to promote environmental welfare.

(e) Information on regional bird species to raise awareness of biodiversity.

All news articles shown in our Tweets were based on real news stories. We asked ChatGPT to slightly rephrase them. We did this to ensure that all participants would come "fresh" to the treatment. We did a reverse image search to get images to accompany the news articles and we made up a news platform that seemed plausible "News Today". Treatment was assigned at the individual level by Qualtrics, after participants had given consent.

[20] While the French law on conspicuous religious symbols was introduced under the centre-left government of Jacques Chirac in 2004, it was framed as a universal secular measure rather than a policy targeting any single religious group. By contrast, the Karnataka ban on headscarves (*hijab*) in schools has been widely viewed as a measure specifically impacting Muslim minorities. Thus, the *hijab* ban policy arises from a different ideological and political motivation.

### 3.4 Outcomes and mechanisms

**Tweet Transcription Task:** Our measure of productivity comes from the second main task of the experiment, where participants were asked to transcribe text from screenshots of subtitled videos embedded in Tweets about animals. Specifically, participants were shown a series of video screenshots and asked to type out the subtitled text displayed within a circle in each image. We measured productivity by the accuracy and number of transcriptions completed. This task was incentivised: participants earned payment based on their performance, creating genuine stakes for effort. Participants were given a practice round before the timed task began: they were shown an example screenshot and asked to type out the sentence inside a highlighted yellow circle, paying attention to words, spaces, punctuation, and capitalisation. They were told they had 10 minutes to complete as many screenshots as possible and would be paid per word correctly transcribed, but the specific contract structure was revealed only after they completed the transcriptions. This ensured that effort during the task was not distorted by strategic responses to the payment scheme. Figure S5 shows an example transcription task along with the corresponding instructions.

**Contract choice:** We measure decision-making quality based on participants' choice between two payment contracts. One option was a piece-rate contract offering Rs. 10 per correct transcription. The alternative was a tournament-based contract, under which only the top third of performers received a higher payment of Rs. 50 per correct transcription, while the remaining two-thirds received nothing. After completing the transcription task but before learning their performance, participants were asked to select the contract under which they wished to be paid. This ordering ensures that contract choice reflects a decision made under uncertainty, rather than a response to known outcomes. We define a decision mistake based on realised performance. Participants whose performance placed them in the top third maximised earnings under the tournament contract, whereas participants in the bottom two-thirds earned more under the piece-rate contract. A participant is therefore coded as making a mistake if they choose the tournament contract but ultimately perform in the bottom two-thirds, or if they choose the piece-rate contract but ultimately perform in the top third. This design enables us to assess whether exposure to exclusionary messages affects participants' economic decision-making, specifically, whether they choose contracts that leave them

with less money than they could have earned with a different contract choice.[21]

**Feelings of Belonging:** We elicited participants' feelings of belonging through two sets of questions administered immediately after exposure to treatment. First, participants reported the extent to which each policy, if implemented, would affect (a) the daily finances of their household and (b) their status and worth in the eyes of other Indian citizens, using a five-point scale ranging from "not at all" to "a great deal." These questions allow us to assess whether participants correctly distinguished the two treatment arms— *Strongly Symbolic* versus *Weakly Symbolic* —as we intended. Second, participants rated their agreement with four statements on a five-point Likert scale ("strongly disagree" to "strongly agree"): "I feel nervous and stressed"; "I feel worried about facing discrimination in my day to day life"; "I feel sure about my place in society"; and "I feel as though others see me as a 'true Indian'." From these items we construct an insecurity index, reversing the positively-worded items so that higher values indicate greater insecurity. This index captures the core mechanism through which we expect treatment to operate: a shift in perceived social standing and national belonging.

**Cognitive load:** To measure cognitive load, we implemented two timed tasks administered between the treatment and the transcription task. The first was an Emotional Stroop Test (EST), in which participants saw a word displayed in a colour and had to select the ink colour as quickly as possible while ignoring the word's meaning. The task included both neutral words and emotionally charged words (e.g., terms related to religion or national identity). The second was a numerical Stroop task, in which participants saw a series of digits and had to select the number of digits displayed, suppressing the impulse to report the digits' face value. For both tasks, we measure response accuracy as our outcome.

**Additional questions:** Before participants chose a payment contract, we asked them to estimate their rank in the transcription task relative to all other participants. Specifically, we asked how many screenshots they believed they had correctly transcribed, and whether they believed their rank among other study participants fell in the top, middle, or bottom third. This allows us

[21] The contract choice design is inspired by the competition game standardly used in the literature to study gender differences in competition (Niederle and Vesterlund, 2007).

to test whether treatment effects on contract choice are driven by changes in beliefs about own performance—for example, if treated minorities became more pessimistic about their relative rank—or whether the contract choice pattern is better explained by decision-making errors arising from increased cognitive load. We also collected pre-treatment measures of political identity, religiosity, and intergroup attitudes, which serve as controls and as checks on randomisation balance.

### 3.5 Data cleaning and pre-registration

The results presented in the paper are based on data excluding participants who reported that they never speak English, participants who failed the attention check, and participants who selected a religion other than Hindu or Muslim. These exclusion restrictions were pre-registered.[22]

### 3.6 Hypotheses

Our argument is that exclusionary policies—even if only symbolic—increase the feeling of insecure belonging, thereby increasing cognitive load and causing participants to make more economic mistakes. In this section, we lay out our hypotheses as described in our pre-registration plan and describe how they will be tested. Importantly, we only pre-specified hypotheses for Muslim participants and left all analyses of Hindu participants as exploratory analysis. This is because while our theorised mechanisms make clear predictions about how exclusionary policies could undermine minorities' feelings of belonging, it is less clear what effects should be for members of the majority group who are not explicitly targeted by the policies and who may exhibit a more varied range of sympathies for the government's positions. Our pre-registered hypotheses are:

**Hypothesis 1** *Muslims in the treatment group will transcribe fewer tweets correctly than Muslims in the control group.*

**Hypothesis 2** *Muslims in the treatment group will make more mistakes in choosing the contract than Muslims in the control group.*

**Hypothesis 3** *Both the Strongly Symbolic and the Weakly Symbolic treatments will harm Muslims in the ways described in Hypotheses 1 and 2.*

[22]For the attention check we use the following: People are very busy these days. We are testing whether people read the survey questions. To show that you've read this much, please select both "very interested" and "extremely interested." Answer options: Extremely interested, Slightly interested, Very interested, Moderately interested.

### 3.7 Econometric Specification

To identify the causal effect of exposure to exclusionary policies on cognitive and economic outcomes, we estimate the following regression model:

$$y_i = \beta_0 + \beta_S \cdot \text{Symbolic}_i + \boldsymbol{\beta}_C \cdot \mathbf{X}_i + \gamma_{s(i)} + \epsilon_i \tag{1}$$

where $y_i$ represents the outcome variable for individual $i$, including cognitive test scores, contract choice (that is, whether participants selected the payoff-maximising contract), and productivity in a transcription task. $\text{Symbolic}_i$ is a binary indicator equal to 1 if individual $i$ was exposed to exclusionary policies that are either Strongly Symbolic or Weakly Symbolic. The coefficient $\beta_S$ captures the effect of exposure to tweets in either of the symbolic treatments, $\mathbf{X}_i$ is a vector of individual-level covariates, $\gamma_{s(i)}$ is a full set of fixed effects for the state $s$ in which individual $i$ resides, and $\epsilon_i$ is the error term, capturing unobserved factors affecting outcome, $y_i$, that are assumed to be uncorrelated with treatment assignment. We also estimate the following model, where we separately estimate the effects of Strongly Symbolic and Weakly Symbolic treatments:

$$y_i = \beta_0 + \beta_1 \cdot \text{StronglySymbolic}_i + \beta_2 \cdot \text{WeaklySymbolic}_i + \boldsymbol{\beta}_C \cdot \mathbf{X}_i + \gamma_{s(i)} + \epsilon_i \tag{2}$$

where $\text{StronglySymbolic}_i$ and $\text{WeaklySymbolic}_i$ are binary indicators equal to 1 if the individual was exposed to exclusionary policies belonging to the Strongly Symbolic and Weakly Symbolic treatments, respectively. The coefficient $\beta_1$ captures the effect of exposure to Strongly Symbolic tweets, while $\beta_2$ captures the effect of exposure to Weakly Symbolic tweets.

We estimate both models with and without controls. Specifications with controls include the covariate vector $\mathbf{X}_i$ and state fixed effects $\gamma_{s(i)}$. Because the sample was designed to achieve balance across gender and state, we always control for a female indicator and state fixed effects. We select the remaining covariates in $\mathbf{X}_i$ using three criteria: (i) baseline imbalance between the treatment and control groups, (ii) differences between Hindu and Muslim participants, and (iii) the Post-Double Selection (PDS) LASSO procedure of Belloni et al. (2014), following the recommendations of Cilliers et al. (2022).[23] The control set includes indicators for female, forward caste, and BJP

[23] In the PDS procedure, the female indicator and state fixed effects enter as always-included (unpenalised) regressors.

voting; age, years of education, survey week; indicators for the bottom and top income terciles; and four measures of identity strength (national, religious, state, and caste). We estimate all models by Ordinary Least Squares (OLS), separately for Muslim and Hindu participants.

# 4 Data

Appendix Table S1 reports baseline descriptive statistics for the full sample. The average participant is approximately 30 years old, with just under 40% identifying as female and exactly 50% as Muslim, in line with our recruitment strategy. The sample is highly educated, with an average of over 17 years of education, and exhibits a broad range of political affiliations and income levels. While Hindu and Muslim participants are broadly similar in terms of age, gender, caste composition, and education, there are stark differences in political preferences. Nearly 78% of Hindus report voting for the BJP in general elections, compared to only 23% of Muslims. Income distributions appear roughly comparable across both groups. Beyond this, the two groups are broadly similar. Muslims are, on average, 1.4 years younger, slightly less likely to belong to the General caste category, and have marginally lower educational attainment (0.4 years). These differences, while statistically significant, are small in magnitude. Gender composition and income distributions are comparable across the two groups. The sample covers respondents from 11 major Indian states, with an equal number of Hindus and Muslims in each state, in line with our recruitment strategy. Supplemental Appendix Table S2 compares the religious composition of our sample by state to the latest census figures from 2011. As intended, the sample over-represents Muslims relative to their population share.

We observe a few noteworthy imbalances in both the Hindu and the Muslim samples. Hindu participants in the Weakly Symbolic treatment group are less likely to be female and slightly less likely to feel close to the nation, feel close to their religion, or feel close to the state. Muslim participants in the Weakly Symbolic treatment group are significantly less likely to report voting for the BJP at both parliamentary and state levels and are marginally more likely to fall into the lowest income tercile (monthly income $\leq$ Rs 50,000), while being under-represented in the top tercile (income $>$ Rs 100,000). Although these differences are modest, we control for these variables in our

main regression specifications to account for any residual imbalance and to improve precision.[24] Notwithstanding the imbalances between Hindus and Muslims, it is important to note that our experiment does not aim to compare Hindus and Muslims at any point; we compare the treatment effect on outcomes within a religion.

# 5 Results

We present the main results for Muslim participants first before turning to the corresponding results for Hindu participants. The section proceeds in five steps. First, we examine the effects of the treatments on our two primary pre-registered outcomes: productivity in the transcription task and mistakes in selecting a payment contract. We also assess whether exposure to exclusionary policies affected participants' final earnings. Second, we investigate the proposed mechanisms. Specifically, we examine whether exclusionary policies threatened participants' social status and self-worth, reduced their sense of belonging, and increased cognitive burden. Third, we assess alternative and complementary mechanisms to determine whether the observed pattern of results is better explained by increased mistakes arising from reduced belonging than by changes in beliefs or preferences, such as risk taking or over- or under-confidence. Fourth, we present the results for Hindu participants and discuss how they inform the interpretation of the findings for Muslims. Finally, we assess the robustness of our findings by examining alternative treatment assignments and by comparing the main online sample with a smaller in-person sample collected during project preparation.

## 5.1 Main outcomes

### 5.1.1 Productivity

Our first outcome captures respondent productivity in the incentivised transcription task, measured as the number of correct transcriptions. We hypothesised that exposure to the exclusionary policy treatments would reduce productivity among Muslim participants. Figure 2.A reports the control and treatment means, where the treatment group pools observations across all exclusionary policies. Figure 2.B presents the estimated treatment effects, both without and with covariates. Across

[24] Note that there is no differential attrition across treatment groups. See Appendix Figure S1.

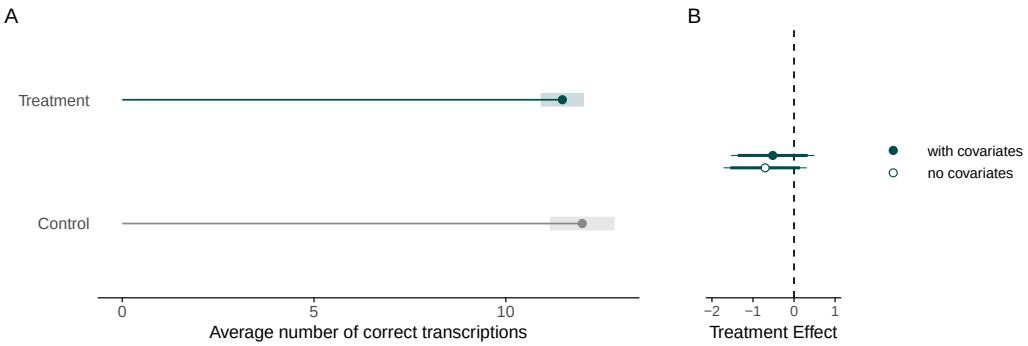



Figure 2: **Effects on Number of Correct Transcription (Muslim)**

*Notes*: Panel A shows group means for Muslim participants for any treatment (blue) and control (grey) groups, with 95% confidence intervals. Panel B presents the treatment difference plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table A4. For the list of control variables used in the regression, see Table A1.

specifications, the estimated effects are negative but not statistically significant. The results remain unchanged after controlling for a range of covariates to account for potential imbalance between treatment and control groups (Table A4).

Although these findings do not support our first hypothesis, they are consistent with the nature of the transcription task. The task ultimately proved to be relatively undemanding. After controlling for participants' exposure to English-language content on the internet, years of education does not significantly predict transcription performance (Table A5). Our conceptual framework predicts that exposure to exclusionary policies should impair performance primarily on cognitively demanding tasks. If the transcription task did not impose substantial cognitive demands, the absence of treatment effects is perhaps unsurprising.

### 5.1.2 Contract mistakes

Our second outcome captures whether Muslim respondents in the treatment groups were more likely to make mistakes in their contract choice. Specifically, we assess whether they were more likely to select a payment contract that caused them to earn less money than they could have earned with a different choice in the experiment. We theorised that exposure to the exclusionary policy treatments would increase the likelihood of making a mistake.

Figure 3 shows that Muslim participants exposed to exclusionary policy treatments were significantly more likely to chose the wrong contract. In the control group, the probability of selecting

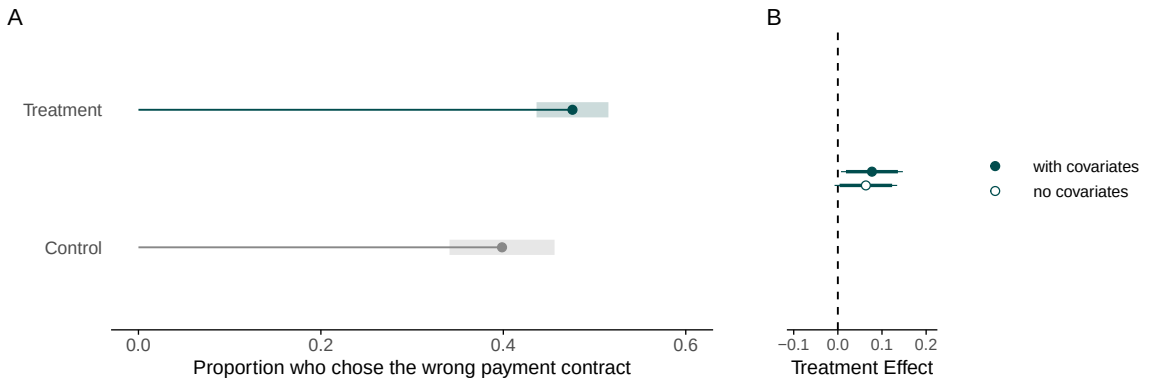



Figure 3: **Effects on Contract Mistakes (Muslim)**

*Notes*: Panel A shows group means for Muslim participants for any treatment (blue) and control (grey) groups, with 95% confidence intervals. Panel B presents the treatment difference plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table A4. For the list of control variables used in the regression, see Table A1.

the wrong contract was 0.40 (95% CI [0.34, 0.46]), whereas in the pooled treatment group, this probability increased to 0.48 (95% CI [0.44, 0.52]). This difference of 7.7 percentage points is statistically significant at the 5% level. Upon inclusion of the covariates, the treatment difference reduces marginally to 6.3 percentage points, as reported in Table A4.

### 5.1.3 Effects on Earnings

Treatment caused Muslims to earn less overall. We plot the full payment distributions in Appendix Figure A1. There is a statistically significant difference in medians (Wilcoxon rank-sum test: W = 94,236.5, p = 0.024) and across the whole distribution (Kolmogorov–Smirnov test: D = 0.103, p = 0.016) although the mean difference is more suggestive (t-test: p = 0.099). The median Muslim participant in the control group earned 100 Rupees while the same in the treatment group earned 90 Rupees, a decline of 10% in earnings.

### 5.1.4 Effects on Hindu Participants

We find little evidence that the treatments affect Hindu participants (see Figures A8 to A12). On average, treated Hindus do not exhibit lower productivity and are no more likely to choose the wrong contract. The one notable exception is earnings, which decline following treatment. Unlike for Muslims, this reduction appears to be driven by lower transcription performance rather than

poorer contract choices. Figure A12 illustrates this by plotting treatment effects on maximum possible earnings, that is, the earnings participants would have received had they selected the optimal contract. In contrast to Muslims, treated Hindus exhibit lower potential earnings, at least in the specification without controls.

To explore heterogeneity in this unexpected result, we estimate an Honest Causal Tree (Athey and Imbens, 2015) (see the Appendix for details). The analysis suggests that the decline in productivity is concentrated among Hindus who report greater affinity with the BJP, stronger attachment to the nation, and more favourable attitudes towards Hindu than Muslim neighbours. One possible interpretation is that these participants responded to the exclusionary policy treatments by reducing their engagement with the task. However, this analysis is exploratory, and these findings should be interpreted with caution. Understanding the mechanisms underlying the heterogeneous responses among Hindu participants would require a study explicitly designed and sufficiently powered to investigate treatment heterogeneity within the majority group.

## 5.2 Main Mechanisms

We hypothesise that symbolically charged policies send signals about who is considered an authentic member of the nation and who is not. Exposure to information about exclusionary policies—whether through the news, social media, or word of mouth—can heighten feelings of insecurity among marginalised individuals and threaten their sense of worth and belonging. We argue that these emotional responses increase cognitive burden, which in turn can lead to mistakes in economic decision-making. In this way, even purely symbolic policies have the potential to harm the economic outcomes of marginalised groups. In this section, we examine whether the exclusionary policy treatments (1) reduced feelings of belonging and triggered negative emotional responses, (2) increased cognitive burden, and (3) generated their effects through the symbolic component of the policies rather than through concerns about their material consequences.

### 5.2.1 Treatment Increases Perceived Threats to Belonging and Feelings of Insecurity

We first measured participants' sense of belonging and emotional state using four self-reported statements. Participants indicated their agreement with the following statements: "I feel as though others see me as a 'true Indian,'" "I feel sure about my place in society," "I feel worried about facing

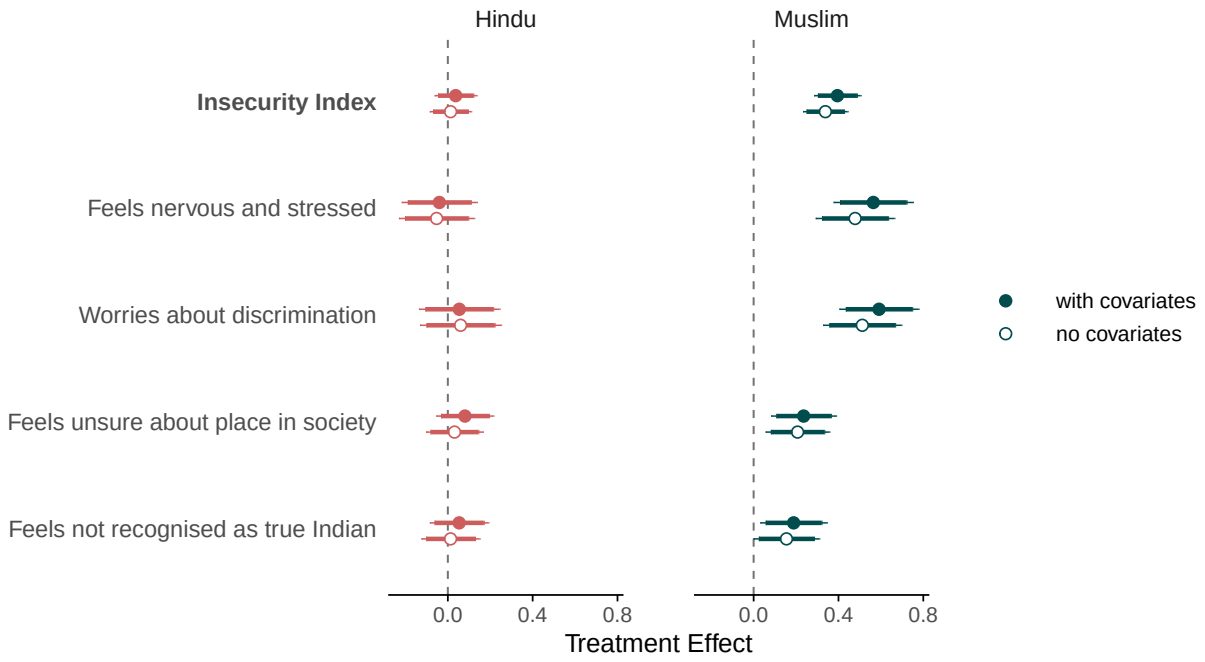



Figure 4: **Feelings of Belonging**

*Notes*: This figure displays treatment effects on feelings of insecurity. The treatment difference is plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression tables corresponding to this figure can be found in Table S5 (Muslim) and Table S10 (Hindu). For the list of control variables used in the regression, see Table A1.

discrimination in my day-to-day life," and "I feel nervous and stressed." Responses were recorded on a five-point Likert scale ranging from 1 (strongly disagree) to 5 (strongly agree). We recoded the items so that higher scores reflect greater insecurity or anxiety and constructed an average index capturing overall feelings of insecure belonging.

Figure 4 presents the results. As expected, Muslim participants assigned to the exclusionary treatment arms reported significantly greater insecurity about being recognised as equal members of the nation and about their place in society. They also expressed significantly greater concern about discrimination and higher levels of stress and anxiety than participants in the control group. The mean insecurity index among treated Muslims was 2.71 (95% CI [2.65, 2.77]), compared with 2.32 (95% CI [2.23, 2.41]) in the control group. We find no treatment effects among Hindu participants.

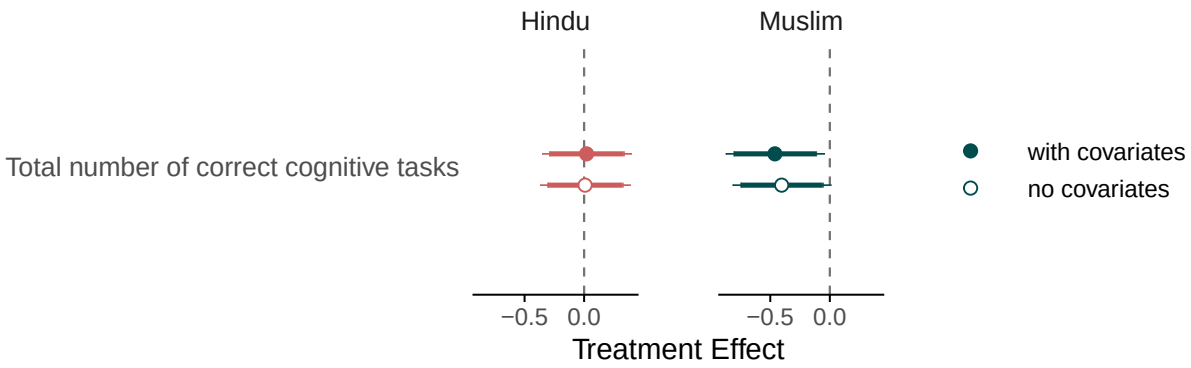



Figure 5: **Treatment Effect on Cognitive Task**

*Notes*: This figure presents the treatment difference for Hindu participants and Muslim participants. The treatment difference is plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression tables corresponding to this figure can be found in Table S5 (Muslim) and Table S10 (Hindu). For the list of control variables used in the regression, see Table A1.

### 5.2.2 Treatment Reduces Performance on a Cognitive Test

Next, we examine whether exposure to exclusionary policies impairs Muslim respondents' cognitive bandwidth. To assess this, participants were asked to complete a series of Stroop tasks (see Appendix for details), commonly used to measure cognitive control. Based on our theoretical framework, we expected that threats to belonging would increase cognitive load, leading Muslim respondents in the treatment group to perform worse on these tasks.

Figure 5 supports this expectation. Treated Muslim respondents completed fewer tasks correctly, with an average of 12.88 correct answers (95% CI [12.65, 13.11]), compared to 13.34 (95% CI [13.00, 13.68]) in the control group. This difference is statistically significant at the 5% level. In contrast, among Hindu respondents, the treatments had no effect on cognitive performance.

### 5.2.3 Effects Go Through Symbolic Component

A central question is whether the effects we document are driven by the symbolic component of the policies or by concerns about their potential direct material consequences. While both treatment bundles communicate exclusionary messages about minorities' place within the national community, the Weakly Symbolic policies additionally carry the potential for direct material harm. If symbolic exclusion is the primary mechanism, we should observe that both policy bundles affect perceptions of status and worth, but that the additional material component of the Weakly Symbolic policies

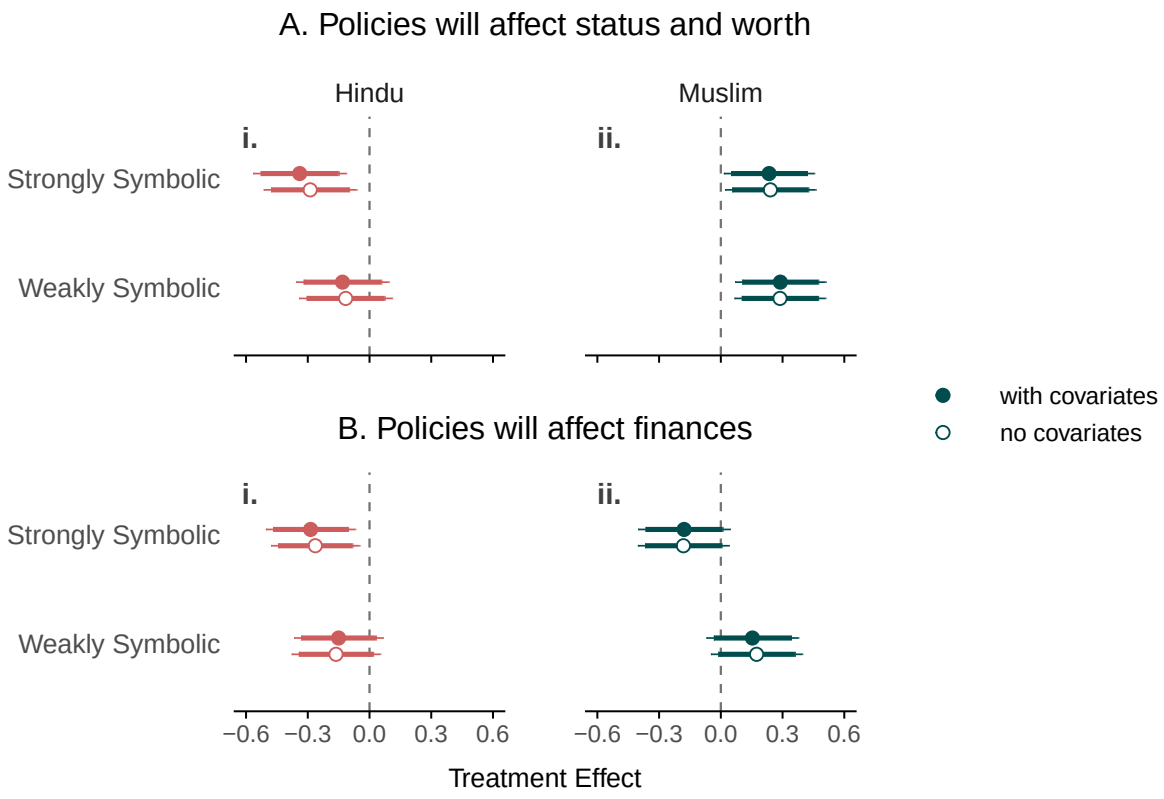



Figure 6: **Perceptions of Policy Impact**

*Notes*: This figure displays treatment effects on participant's perception of the policy impact. The treatment difference is plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S6. For the list of control variables used in the regression, see Table A1.

should not translate into larger downstream effects.

We first assess whether the policies described in the tweets were perceived by participants as posing a threat to their status and worth in the eyes of broader society. While policies categorised as Weakly Symbolic may also be viewed as threats to participants' financial well-being, we expect that both bundles of exclusionary policies – Strongly Symbolic and Weakly Symbolic – would be perceived as threats to respondents' sense of status and worth. To assess this, participants answered two questions after viewing the five tweets. The first question assessed perceived status: "You just saw five tweets proposing policies. If any of the policies are implemented next week, how much will it affect your status and worth in the eyes of other citizens?" The second question focused on financial impact: "You just saw five tweets proposing policies. If any of the policies are implemented next week, how much will it affect the daily finances of you and your household?" Responses were

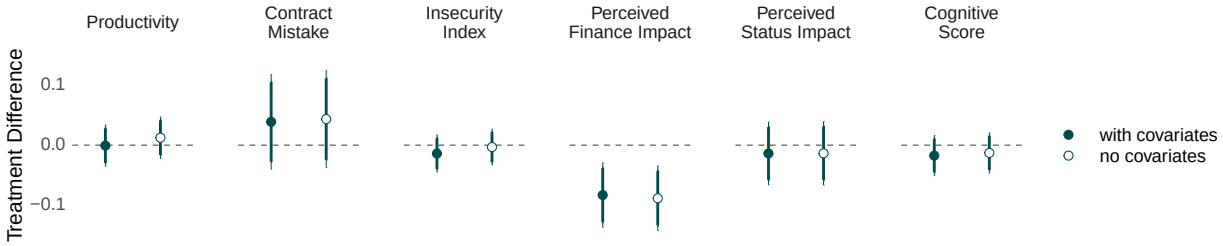



Figure 7: **Comparing Strongly Symbolic and Weakly Symbolic Treatments (Muslim)**

*Notes*: The figure displays the difference in treatment means comparing the Strongly Symbolic and Weakly Symbolic treatments for Muslim respondents. The treatment difference is plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. For the list of control variables used in the regression, see Table A1.

recorded on a five-point scale ranging from "Not at all" to "A great deal."

Figure 6 shows that both treatment bundles significantly increase the perceived impact of the policies on respondents' status and worth, indicating that participants interpret both sets of policies as threats to their social standing. At the same time, the Weakly Symbolic policies generate somewhat greater concern about financial consequences, consistent with our classification of these policies as carrying a potential material dimension. Thus, while the two treatment bundles differ in the extent to which they raise concerns about material consequences, both clearly operate through a symbolic channel affecting social status and belonging.

A crucial test is whether this additional material component produces larger downstream effects for the Weakly Symbolic policies. Figure 7 shows that it does not. We find no statistically significant differences between the Strongly Symbolic and Weakly Symbolic treatments on insecurity, cognitive performance, contract-choice mistakes, or earnings. We only observe a difference for perceived financial impact, where differences emerge as expected. If anticipated material consequences were driving our results, we would expect the Weakly Symbolic treatment to generate systematically larger effects. Instead, the two bundles produce remarkably similar outcomes. This suggests that the symbolic component of exclusionary policies is likely to be the primary driver of the cognitive and economic harms we document.

### 5.3 Alternative Mechanisms: Beliefs, Preferences or Mistakes

Our main argument is that exposure to exclusionary policies increases feelings of insecurity, which increases cognitive load and leads to decision mistakes. Of course, exposure to exclusionary poli-

cies might also change beliefs or risk preferences. While these may be important complementary mechanisms, in this section we discuss evidence showing that they are unlikely to be driving our results on decision mistakes.

First, we show that treatment did not significantly change beliefs. After participants had transcribed screenshots, we asked, "What do you believe your rank (in terms of the number screenshots you transcribed) is among other people who have taken part in this study?" They could reply "Top 1/3", "Middle 1/3" or "Bottom 1/3".[25] In Appendix Figures A4 and A5, we show that treatment did not significantly increase the probability that participants believed they were in the top 1/3, nor did treatment change the probability that beliefs were correct.

Next, we consider how treatment may have affected risk-taking. In Appendix Figure A6, we show that, on average, the higher rates of choosing the wrong contract after treatment, are not systematically due to participants taking too much or too little risk. We plot the proportions of mistakes separately for participants who should have chosen the piece-rate contract but choose the tournament one, and for participants who should have chosen the tournament contract but chose the piece-rate one. The results show that participants make both types of mistakes. In Figure A7, we consider an alternative measure of risk-taking, this time by examining whether participants' choices aligned with their beliefs. Risk aversion would lead some participants who believed the tournament was payoff-maximising to choose the safe piece-rate contract anyway, reducing the alignment between their beliefs and their choices. Consistent with this, alignment is imperfect. Importantly, however, treatment does not change the degree of alignment for either belief group, indicating that our results are not driven by treatment effects on risk-taking.

Thus, while treatment increases the probability of choosing the wrong contract, it does not appear to significantly shift beliefs or preferences. To further support our proposed mechanism of heightened insecurity, we examine correlations between contract choice mistakes and relevant mechanisms within the control group. In Table A6, we show that even after controlling for performance, beliefs and our standard set of pre-treatment covariates, participants' level of insecurity is still a significant predictor of the probability of choosing the wrong contract amongst control participants. Given that the treatment significantly increases feelings of insecurity, this provides further

[25] We elicited rank beliefs relative to all participants rather than the payoff-relevant group of twelve, which was revealed only at the payment stage. Since the groups are random draws from the same pool, both cut-offs correspond to roughly the top third of the distribution.

suggestive evidence that heightened insecurity is an important mechanisms driving the increase in mistakes.

### 5.4 Alternative Explanations: Negative Affect, Emotional Arousal and Material Fear

One concern is that the treatment tweets differ from the control tweets on dimensions other than symbolic exclusion. The tweets about the hijab ban, cow-slaughter prohibitions, textbook revisions, and city renaming may also be more negative, emotionally arousing, and politically salient than the neutral control tweets about trees or legislators' pensions. The effects we document could therefore reflect exposure to negative or arousing political content rather than symbolic exclusion. A related concern is that participants may interpret the policies as signals of future material harm, such as economic loss or discrimination. We examine these alternative explanations using participants' ratings of the content of the tweets (valence and topic) and their reports of how the tweets made them feel (worried, nervous, and threatened in their status and finances). We also exploit differences in the material consequences of the Strongly and Weakly Symbolic treatments.

First, the treatment shifts how both groups classify the content, and it does so almost identically. Relative to control, exposure to the treatment tweets raises the share of tweets identified as relating to religion by 43 percentage points among Hindus and 44 among Muslims (from about 1% under control in both groups); the difference in these treatment effects across groups is not significant ($p = 0.50$). Treatment raises the share identified as political to a similar extent in both groups (by 37 and 41 percentage points, respectively). Both groups therefore register the treatment content as concerning religion and politics to a comparable degree (Appendix Table S11), so differences in their responses do not reflect differences in how they read the content.

Second, both groups rate the treatment content as more negative, but only Muslims report greater worry, nervousness, and status threat. Relative to control, both groups rate the treatment tweets as more negative in content, though the increase is far larger among Muslims (+3.02) than among Hindus (+0.92) on the $-5$ to $+5$ valence scale. Yet this shared negativity translates into greater worry, nervousness, and perceived status threat only among Muslims (Figures 4 and 6, and Appendix Table S5). Among Hindus, by contrast, the same content produces no change in worry or nervousness and, if anything, a reduction in perceived status threat ($-0.23$, $p = 0.02$; Figure

6 and Appendix Table S10). Thus, although both groups perceive the treatment content as more negative, only the group the policies target experiences greater insecurity and anxiety.

These comparisons do not fully separate the effects of exclusion from those of negative content. The treatment tweets are rated as more negative than the control tweets, so the control condition is not valence-matched. Within the Muslim sample alone, we therefore cannot distinguish negativity caused by exclusion from the effects of negativity itself. However, the comparison with Hindu participants weighs against a generic negativity explanation: the same treatment content is perceived as more negative by Hindus but does not increase their worry, nervousness, or perceived status threat.

Third, the results also weigh against anticipated material harm as the main explanation. If Muslims respond to the treatments because they anticipate concrete economic or other material consequences, we should expect stronger effects from the Weakly Symbolic treatment, which contains a material component, than from the Strongly Symbolic treatment, which is purely symbolic. Instead, the two treatments produce similar effects on insecurity, cognition, contract-choice mistakes, and earnings, and the Strongly Symbolic treatment drives the increase in contract-choice mistakes (Figure 7 and Table A4). Moreover, among Muslims, the Strongly Symbolic tweets generate less financial worry ($-0.33$, $p = 0.002$) and less nervousness ($-0.23$, $p = 0.03$) than the Weakly Symbolic tweets. Participants exposed to the treatments also report no change in the expected financial consequences of the policies, despite reporting greater threats to their status and self-worth. Taken together, these results suggest that anticipated material harm is unlikely to explain the effects we observe.

### 5.5 Robustness

We assess the robustness of our main result along two dimensions. First, because the correctness of the piece-rate or the tournament decision depended on the participant's group assignment, we verify that our findings are not driven by group assignments. Second, we examine whether the results generalise to a different setting. As part of the project's preparatory work, we conducted a separate experiment using a similar protocol with a fully in-person sample in West Bengal. Due to concerns with the recruitment procedure (detailed and documented in the pre-registration), we do not present these findings as main results. Nevertheless, we show that in this separate sample with

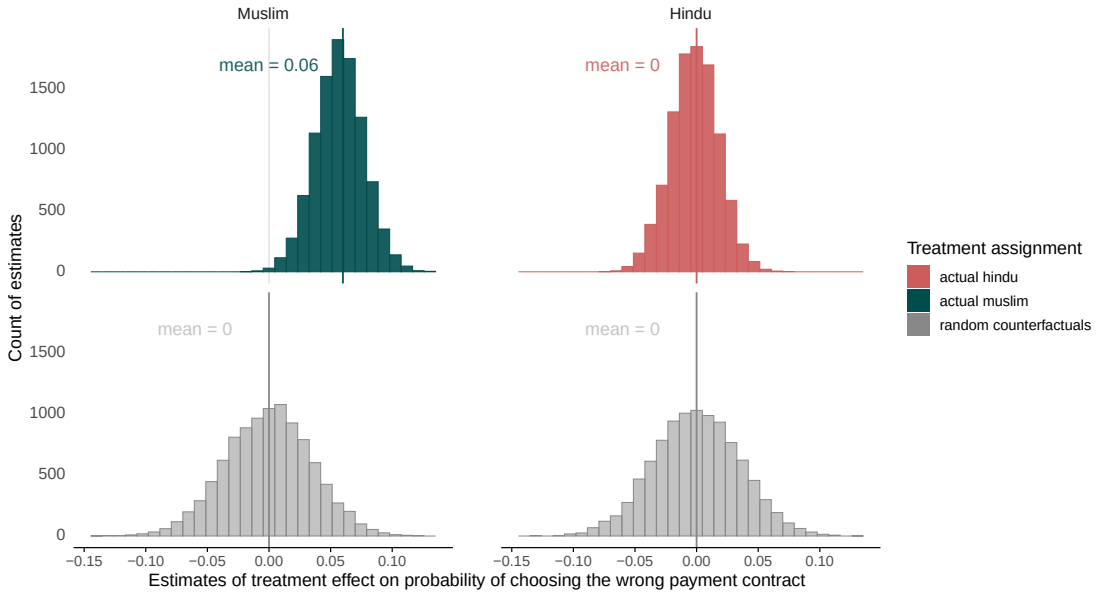



Figure 8: **Re-randomising payment groups**

*Note*: This figure displays the distribution of estimated treatment effects on the probability of choosing the wrong payment contract among Muslim and Hindu respondents under random reassignment of participants to groups. The groups are resampled to only include 50% Muslim participants and 50% Hindu participants as in the experiment. In each iteration, we randomly form new groups of equal size to the experiment (12 participants per group), determine whether each participant's contract choice would be correct within the newly formed groups, and estimate the corresponding treatment effect; we repeat this procedure 10,000 times. The top panel uses the actual treatment assignments, while the bottom panel uses randomly reshuffled treatment assignments, for which the true treatment effect is zero.

lower education and different internet habits, results on treatment effects and mechanisms are the same as in our primary, pre-registered online sample.

### 5.5.1 Random re-assignments to groups

We randomly generate new groups of equal size as in the actual experiment (12 participants per group). For each reassignment, we determine whether each participant's contract choice would be correct within the newly formed groups and estimate the corresponding treatment effect. We repeat this procedure 10,000 times to obtain a distribution of treatment effects under random group reassignment.

We conduct a parallel exercise in which treatment assignments are randomly reshuffled. This reshuffling generates a sample in which the true treatment effect is zero. We apply the same random group reassignment procedure to this benchmark sample and compare the resulting distribution to that obtained under the actual treatment assignments. As shown in the top panel of Figure 8,

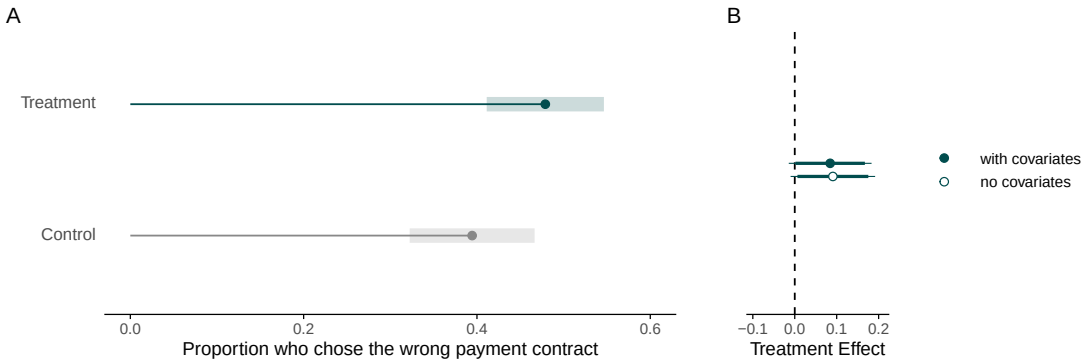



Figure 9: **Treatment Effects on Optimal Contract Decision (Muslim participants, Field Sample)**

*Notes*: Panel A shows group means for Muslim participants for any treatment (blue) and control (grey) groups, with 95% confidence intervals. Panel B presents the treatment difference plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table A8. For the list of control variables used in the regression, see Table A1.

even under random group reassignment, the distribution of treatment effects for Muslims lies almost entirely to the right of zero, and its mean is close to our baseline estimate. As expected, conducting the same procedure with reshuffled treatment assignments leads to a more diffuse distribution centred at zero.

### 5.5.2 Evidence from the in-person experiment

We also conducted a complementary in-person experiment in West Bengal, involving approximately 1,000 participants. The details of the in-person experiment can be found in Appendix SB. Due to implementation challenges we do not include these data in our main analysis.[26] However, in a restricted sample excluding individuals with insufficient language proficiency (see caveats in Appendix), we find that the treatment effects on optimal contract choice closely mirror those observed in our online experiment. As shown in Figure 9, exposure to exclusionary policies significantly increased the likelihood of making a payment choice mistake, reinforcing the robustness and external validity of our core findings. The corresponding regression estimates are reported in Appendix Table A8.

[26]For instance, we discovered limited English comprehension among many respondents after the experiment.

# 6 Discussion

Our study provides evidence that exclusionary policies, even when symbolic, can increase cognitive load among exposed minorities, leading to economic mistakes. Muslim participants in India exposed to these policies are more likely to make mistakes in choosing their payment contracts, resulting in lower earnings. No such effect is found among the Hindu participants. Further, the Muslim participants in the treatment group reveal diminished feelings of national belonging, underscoring the psychological burden of exclusion. Notably, Strongly Symbolic exclusionary policies, despite lacking immediate material impact, still disrupt cognitive processes and lead to tangible economic losses. This highlights the broader risks of exclusionary narratives: it not only undermines emotional well-being but also imposes hidden economic costs. Additionally, in identifying this channel, we show that symbolic politics can widen the economic gap between groups without redistributing a single rupee, simply by occupying the mental resources on which good decisions depend.

A few issues in our study merit a deeper discussion. First, while we cleanly identify that exclusion operates through a cognitive channel, our evidence on the precise mediating role of that channel, and of insecurity in particular, in explaining suboptimal economic choices remains suggestive rather than causal. The treatment raises insecurity and increases mistakes among the Muslims participants, conditional on performance, beliefs, and covariates, but we stop short of a formal causal mediation analysis. Relatedly, our data cannot fully discriminate among cognitive mechanisms. The pattern we document, unchanged beliefs, unchanged accuracy of self-assessment, yet noisier choice, is consistent with the depletion of working memory that the scarcity literature emphasises, but also with explanations related to computational-anxiety in which threatened individuals overestimate the volatility and unpredictability of their environment, raising decision noise and degrading the mapping from beliefs to choices (Browning et al., 2015; Bishop and Gagne, 2018). Both accounts locate the damage in cognition rather than in preferences, and both carry the same welfare implications, but distinguishing between them would require a design that manipulates cognitive load directly. Second, no policy is ever perfectly symbolic: to a targeted minority, a renamed city or a revised textbook may also serve as a harbinger of more tangible harms to come, so part of the effect we measure could reflect anticipated future persecution rather than symbolic exclusion in the present. Our manipulation checks bound this concern, treated participants report no change

in the expected financial impact of the policies, while reporting significant threats to their status and worth, but they cannot eliminate an expectations channel entirely.

Third, an important question in the context of our paper is whether the results are externally valid and generalisable. The threat to external validity arises from three main sources. The first is that ours is an online field experiment, and whether our results hold in more general settings outside the ambit of an online decision environment is an open question. Further, due to the nature of our experiment, we measure short-run responses to discrete exposures in a controlled task environment. This buys clean identification but leaves open how the effect behaves over time, whether repeated, lifelong exposure compounds into persistent disadvantage or whether individuals habituate and the marginal effect decays. Here, it is worth noting that our setting is not an unfamiliar one for the participants. These days, people spend an increasing share of their time on social media, and the design of our experiment mirrors an environment that a member of a minority group (and participants in general) would encounter routinely: scrolling through a platform like Twitter, where news of exclusionary policies is regularly on display. Thus, while our study does not identify long-run effects, it locates the mechanism in precisely the kind of setting in which an individual will repeatedly find themselves. Finally, the stakes in our experiment are low. Though real and incentivised, they are modest, and whether the same cognitive tax distorts the higher-stakes choices that shape economic lives remains an open question.

Stepping back, our results connect literatures that have largely developed in isolation. A political-economy tradition has shown that symbolic policies, renamed cities, contested monuments, revised curricula, citizenship laws, shape beliefs, identification, migration, and political behaviour, yet has tended to treat their consequences as operating downstream, through what people come to think or do. A behavioural literature on scarcity has shown that financial and time constraints capture attention and degrade decision-making, yet has focused almost exclusively on material deprivation, leaving identity-related concerns aside. We bridge the two by showing that exclusionary symbolic politics operate through the same cognitive load mechanism long associated with poverty, and that they do so immediately, without requiring any meaningful updating of political beliefs. Our findings also speak to a third, older tradition. Integrated threat theory has long predicted that symbolic threats to a group's identity and standing affect its members independently of any realistic threat to material resources (Stephan and Stephan, 2000; Riek et al., 2006); our two

treatment arms constitute, to our knowledge, the first field-experimental test of this prediction in the domain of economic decision quality, and the similarity of effects across the two arms is precisely what the theory anticipates.[27] This placement reframes how the costs of exclusionary politics should be interpreted. The effect of such policies includes not only the legal and physical harms that are visible and easy to count, but also a diffuse, recurring stream of worse decisions that is far harder to see. That distinction is not academic. As ethnonationalist movements gain ground across democracies, through nation-state laws, citizenship registries, the erection or removal of contested monuments, and restrictions on minority religious dress, the same cognitive tax we document in India may be levied wherever a state signals that some of its citizens belong less fully than others (Abdelgadir and Fouka, 2020). If so, the economic costs of such politics are systematically underestimated, and they fall hardest on those already at the margins, deepening the already existing social faultlines.

# 7 Conclusion

In this paper we ask whether the symbolic acts of exclusionary politics, the renamings, contested monuments, and citizenship rules that signal a minority's diminished standing without redistributing resources, carry economic consequences for the people they target. Our field experiment with Muslim and Hindu gig workers in India shows that they do. Muslims exposed to social media content about such policies were more likely to choose the wrong payment contract and earned less as a result, even though their productivity or their beliefs about their own performance were unchanged.

Our findings open several avenues for future work. The most direct extension concerns the mechanism: a design that experimentally manipulates cognitive load alongside exclusionary exposure, or that pairs exposure with physiological or behavioural measures of distress, could pin down the mediating pathway that our data can only offer suggestive evidence of. A second priority concerns dynamics. To answer whether minorities get habituated to exposure to exclusionary policies in the long run will require longitudinal designs that track individuals across repeated exposures.

[27] Two clarifications. ITT concerns an in-group's perception of threat from an out-group, not specifically a majority's view of a minority; its dominant application to majority prejudice towards immigrants is empirical, not definitional. And its canonical outcome is hostility towards the threatening out-group, not, as we study here, degraded economic decision quality among the targeted group itself.

At the very least, combining experimental estimates with data on how often minorities actually encounter exclusionary content would permit a fuller understanding of the aggregate cognitive cost of symbolic exclusionary policies. Third, future work should move from modest, task-scale decisions to the high-stakes choices, over schooling, savings, migration, and occupation, where a recurring cognitive tax could translate into large and durable gaps in well-being. Fourth, our exploratory results for the majority group hint that exclusionary politics may affect majorities and minorities through distinct channels, withdrawal of effort among some Hindus who appear sympathetic to the policies, versus impaired decision-making among targeted Muslims, a heterogeneity that merits a study designed specifically to examine it. Fifth, and a natural companion question is whether *inclusive* symbolic policies, signals of recognition and equal standing, can restore the bandwidth that exclusionary ones consume, a possibility with direct implications for policy.

More broadly, our findings place identity alongside material scarcity as a source of cognitive burden with real economic consequences. As ethnonationalist movements advance across democracies, the costs of their symbolic politics are likely to be underestimated, because the harm they do is diffuse, recurring, and largely invisible, and it falls hardest on those already at the margins. Symbolic exclusion is not costless. It is paid, quietly and in all likelihood continuously, in the worsened decisions of those a state marks as outsiders.

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# Main Appendix

# A Additional Results

Table A1: Description of the Covariates

| Variable | Definition | Min | Max | Mean | Median | SD |
|---|---|---|---|---|---|---|
| Female | Respondent identifies as female (binary) | 0.00 | 1.00 | 0.41 | 0.00 | 0.49 |
| Age | Age in years | 18.00 | 74.00 | 29.46 | 27.00 | 9.14 |
| Voted BJP | Voted for BJP in last general election (binary) | 0.00 | 1.00 | 0.23 | 0.00 | 0.42 |
| Income: bottom tercile | Monthly household income $\leq$ Rs 50,000 (binary) | 0.00 | 1.00 | 0.42 | 0.00 | 0.49 |
| Income: middle tercile | Monthly household income Rs 50,001–100,000 (binary) | 0.00 | 1.00 | 0.35 | 0.00 | 0.48 |
| Income: top tercile | Monthly household income $>$ Rs 100,000 (binary) | 0.00 | 1.00 | 0.23 | 0.00 | 0.42 |
| State identity strength | Strength of state identity (0–100 scale) | 0.00 | 100.00 | 87.34 | 98.00 | 18.57 |
| National identity strength | Strength of national identity (0–100 scale) | 0.00 | 100.00 | 92.28 | 100.00 | 14.92 |
| Caste identity strength | Strength of caste identity (0–100 scale) | 0.00 | 100.00 | 72.85 | 87.00 | 32.75 |
| Religious identity strength | Strength of religious identity (0–100 scale) | 0.00 | 100.00 | 88.83 | 100.00 | 19.94 |
| Forward caste | Member of a general/forward caste (binary) | 0.00 | 1.00 | 0.57 | 1.00 | 0.49 |
| Years of education | Years of formal education completed | 1.00 | 20.00 | 17.26 | 18.00 | 2.97 |

*Notes*: This table describes the covariates used as control variables in the treatment-effect regressions reported throughout the paper. Unless otherwise noted, specifications estimated "with controls" include this full set, along with state fixed effects.

## A.1 Additional Results on Main Outcomes

Table A2: Balance Table (Muslim)

| Variable | Control | Treatment | Difference | P-value |
|---|---|---|---|---|
| Female | 0.391 | 0.416 | 0.025 | 0.532 |
| | [0.489] | [0.493] | | |
| Age | 30.153 | 29.139 | -1.014 | 0.123 |
| | [8.644] | [9.341] | | |
| Forward caste | 0.569 | 0.574 | 0.005 | 0.950 |
| | [0.496] | [0.495] | | |
| Years of education | 17.459 | 17.169 | -0.290 | 0.175 |
| | [2.969] | [2.971] | | |
| Income: $\leq$ Rs 50,000 (Bottom tercile) | 0.363 | 0.445 | 0.082 | 0.025** |
| | [0.482] | [0.497] | | |
| Income: Rs 50,001–100,000 (Middle tercile) | 0.342 | 0.352 | 0.010 | 0.829 |
| | [0.475] | [0.478] | | |
| Income: $>$ Rs 100,000 (Top tercile) | 0.295 | 0.203 | -0.092 | 0.003*** |
| | [0.457] | [0.403] | | |
| Voted BJP | 0.285 | 0.206 | -0.078 | 0.013** |
| | [0.452] | [0.405] | | |
| State identity strength | 89.060 | 86.555 | -2.506 | 0.061* |
| | [17.326] | [19.066] | | |
| National identity strength | 92.288 | 92.281 | -0.008 | 0.994 |
| | [14.899] | [14.941] | | |
| Caste identity strength | 71.427 | 73.498 | 2.071 | 0.379 |
| | [33.565] | [32.387] | | |
| Religious identity strength | 89.313 | 88.611 | -0.702 | 0.625 |
| | [19.565] | [20.119] | | |
| F-test p-value (joint) | | | | 0.039** |
| Observations | 281 | 620 | | |

*Notes*: This table reports means, with standard deviations in square brackets, of pre-treatment characteristics for Muslim participants, separately for the control group and the pooled treatment group. The *Difference* column reports the treatment-minus-control difference in means, with stars indicating statistical significance. The row *F-test p-value (joint)* reports the $p$-value from a joint test of equality of all listed characteristics across the two groups. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Summary of Main Outcomes (Muslim)

| Variable | Min | Q25 | Median | Q75 | Max | Mean | SD |
|---|---|---|---|---|---|---|---|
| Number of correct transcriptions | 0.00 | 6.00 | 12.00 | 17.00 | 33.00 | 11.64 | 7.19 |
| Payment choice correct | 0.00 | 0.00 | 1.00 | 1.00 | 1.00 | 0.55 | 0.50 |
| Payment | 0.00 | 10.00 | 90.00 | 160.00 | 1650.00 | 150.26 | 242.13 |
| Insecurity index | 1.00 | 2.00 | 2.75 | 3.00 | 5.00 | 2.59 | 0.80 |
| Policy affects finances | 1.00 | 1.00 | 2.00 | 4.00 | 5.00 | 2.48 | 1.39 |
| Policy affects status | 1.00 | 2.00 | 3.00 | 4.00 | 5.00 | 2.92 | 1.36 |
| Cognitive score correct total | 1.00 | 14.00 | 14.33 | 14.33 | 14.33 | 13.02 | 2.93 |

*Notes*: This table reports summary statistics for the main outcome variables among Muslim participants. For each variable, the table reports the minimum, the 25th percentile (Q25), the median, the 75th percentile (Q75), the maximum, the mean, and the standard deviation. *Payment choice correct* is an indicator equal to 1 if the participant selected the earnings-maximising contract. *Payment* is measured in Indian Rupees. The *insecurity index* and the *policy affects finances* and *policy affects status* items are measured on a five-point scale. *Cognitive score* is the participant's performance on the cognitive task.

Table A4: Main Treatment Effects (Muslim)

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Payment choice mistake | | | | Number of correct transcriptions | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Any treatment | 0.077** | 0.063* | | | −0.519 | −0.705 | | |
| | (0.036) | (0.036) | | | (0.517) | (0.516) | | |
| Weakly symbolic | | | 0.058 | 0.044 | | | −0.507 | −0.906 |
| | | | (0.041) | (0.041) | | | (0.593) | (0.591) |
| Strongly symbolic | | | 0.097** | 0.083** | | | −0.530 | −0.507 |
| | | | (0.041) | (0.041) | | | (0.592) | (0.590) |
| Constant | 0.399*** | 0.562*** | 0.399*** | 0.571*** | 11.993*** | 16.861*** | 11.993*** | 16.953*** |
| | (0.030) | (0.170) | (0.030) | (0.171) | (0.429) | (2.430) | (0.429) | (2.434) |
| Adjusted $R^2$ | 0.0041 | 0.0229 | 0.0040 | 0.0229 | 0.0000 | 0.0465 | -0.0011 | 0.0460 |
| Observations | 901 | 901 | 901 | 901 | 901 | 901 | 901 | 901 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| $H_0$: Weakly = Strongly, p-value | | | 0.330 | 0.326 | | | 0.969 | 0.489 |

*Notes*: This table reports OLS estimates of treatment effects on the two primary outcomes for Muslim participants: the probability of making a payment-choice mistake and the number of correct transcriptions. Columns (1), (2), (5), and (6) report the effect of being assigned to any treatment, while the remaining columns report separate effects for the Weakly Symbolic and Strongly Symbolic treatments. Even-numbered columns include the full set of pre-treatment controls. Standard errors are reported in parentheses. The row "$H_0$: Weakly = Strongly" reports the $p$-value from a test of the equality of the Weakly Symbolic and Strongly Symbolic coefficients. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5: Correlates of Main Outcomes (Muslim)

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Payment choice mistake | | Correct transcriptions | |
| | (1) | (2) | (3) | (4) |
| Female | −0.069 | −0.070 | −1.111 | −1.065 |
| | (0.063) | (0.064) | (0.930) | (0.952) |
| Age | −0.009** | −0.010*** | −0.148*** | −0.147*** |
| | (0.004) | (0.004) | (0.054) | (0.055) |
| Voted BJP | −0.035 | −0.029 | −1.210 | −1.138 |
| | (0.068) | (0.071) | (1.002) | (1.053) |
| Income: bottom tercile | −0.023 | −0.027 | −0.935 | −0.835 |
| | (0.074) | (0.075) | (1.089) | (1.122) |
| Income: top tercile | −0.046 | −0.062 | 0.141 | 0.161 |
| | (0.075) | (0.075) | (1.097) | (1.124) |
| National identity strength | 0.002 | 0.002 | 0.053 | 0.048 |
| | (0.003) | (0.003) | (0.037) | (0.039) |
| Caste identity strength | −0.0003 | −0.0004 | −0.022 | −0.022 |
| | (0.001) | (0.001) | (0.015) | (0.016) |
| Religious identity strength | 0.002 | 0.002 | 0.017 | 0.015 |
| | (0.002) | (0.002) | (0.028) | (0.029) |
| Forward caste | −0.003 | −0.007 | −1.006 | −0.897 |
| | (0.063) | (0.064) | (0.921) | (0.954) |
| Years of education | 0.025** | 0.025** | 0.130 | 0.172 |
| | (0.011) | (0.011) | (0.162) | (0.169) |
| Conversation: all English | | −0.031 | | −0.290 |
| | | (0.095) | | (1.417) |
| Conversation: almost all English | | 0.091 | | 0.081 |
| | | (0.074) | | (1.097) |
| Conversation: almost none English | | 0.177 | | 2.847 |
| | | (0.151) | | (2.257) |
| Websites: all English | | 0.012 | | 0.740 |
| | | (0.111) | | (1.652) |
| Websites: almost all English | | 0.115 | | 0.771 |
| | | (0.115) | | (1.717) |
| Constant | 0.088 | −0.082 | 16.727*** | 15.306*** |
| | (0.313) | (0.324) | (4.595) | (4.835) |
| Adjusted $R^2$ | 0.0405 | 0.0468 | 0.0504 | 0.0337 |
| Observations | 281 | 281 | 277 | 277 |
| English variables | No | No | Yes | Yes |
| State FE | Yes | Yes | Yes | Yes |
| Week FE | Yes | Yes | Yes | Yes |

*Notes*: This table reports OLS regressions of the two main outcomes, the probability of making a payment-choice mistake (columns 1–2) and the number of correct transcriptions (columns 3–4), on pre-treatment characteristics among Muslim participants in the control group. Columns (2) and (4) additionally control for measures of English-language exposure, namely the share of conversations and of websites conducted or read in English. All columns include state and survey-week fixed effects. Standard errors are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

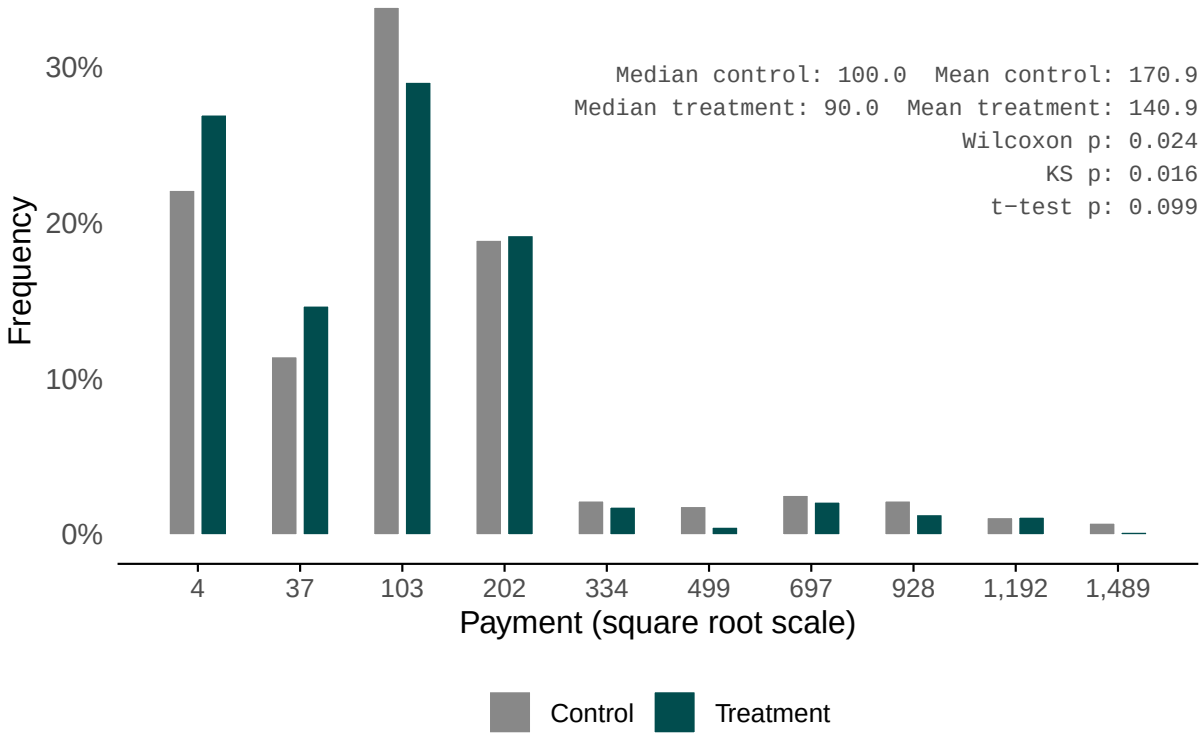



Figure A1: Distribution of earnings (Muslim)

*Note*: This figure displays the distribution of earnings for Muslim respondents in any treatment (blue) and control (grey) groups. The x-axis is plotted on a square-root scale. Earnings are measured in Indian Rupees. The treatment and control distributions differ significantly in their medians (Wilcoxon rank-sum test: $W = 94{,}236.5$, $p = 0.024$) and across the whole distribution (Kolmogorov–Smirnov test: $D = 0.103$, $p = 0.016$).

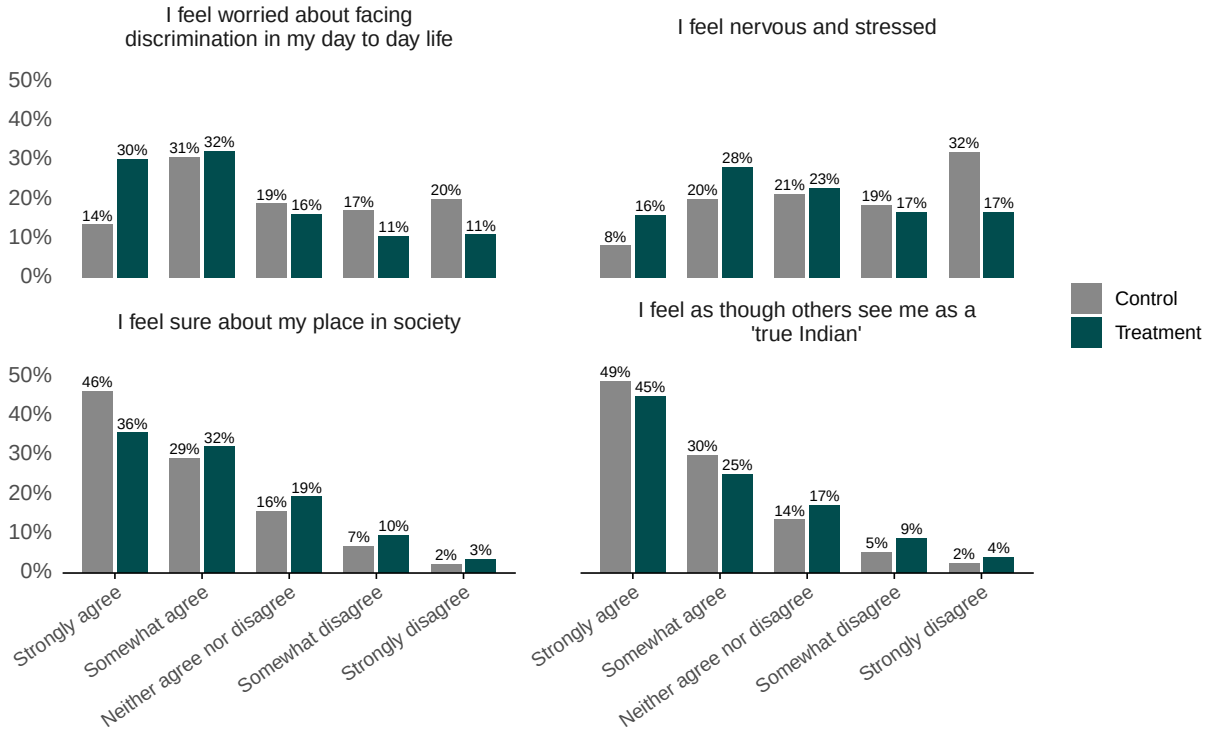



Figure A2: How Insecure Do Participants Feel? (Muslim)

*Note*: This figure displays the distribution of Muslim respondents' answers, by treatment arm, to four statements measuring feelings of insecure belonging: "I feel as though others see me as a 'true Indian'," "I feel sure about my place in society," "I feel worried about facing discrimination in my day-to-day life," and "I feel nervous and stressed." Responses were recorded on a five-point Likert scale ranging from 1 (strongly disagree) to 5 (strongly agree). The regression table corresponding to this figure can be found in Table S5.

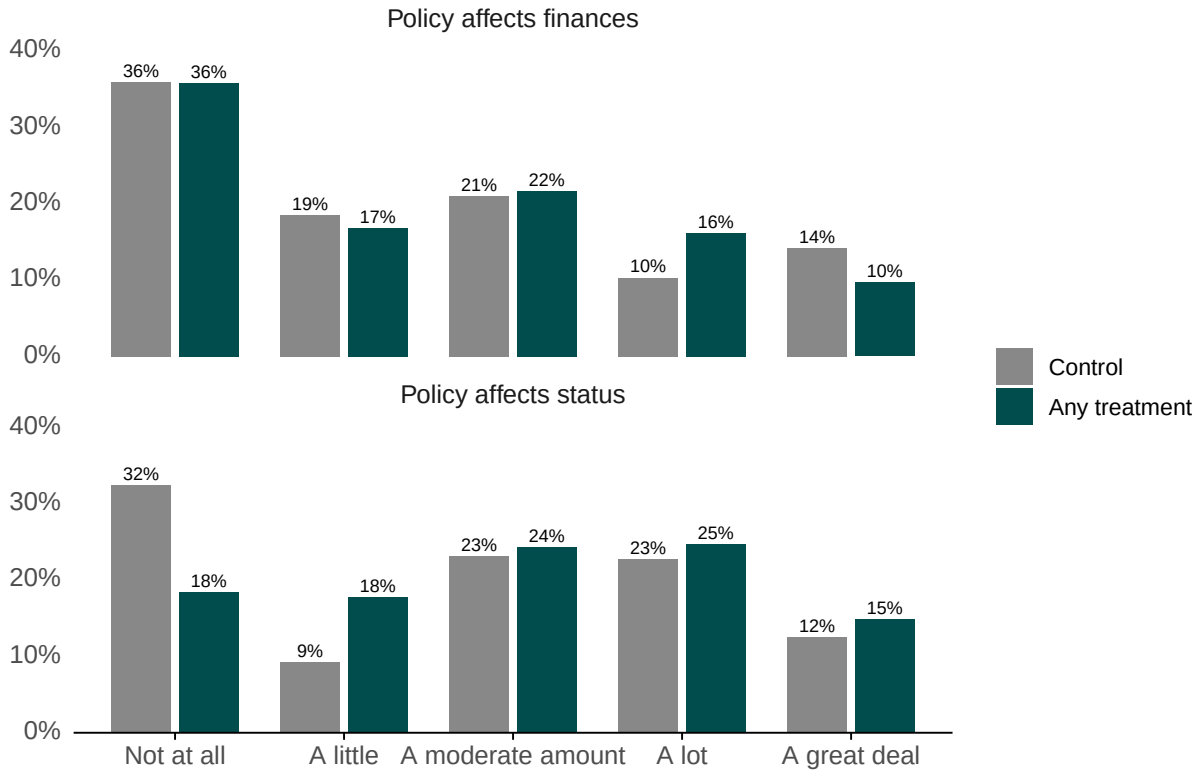



Figure A3: How Would the Treatment Policies Affect Status and Finances? (Muslim)

*Note*: This figure displays the distribution of Muslim respondents' answers, by treatment arm, to two questions asking how much the policies described in the tweets would affect (a) the daily finances of their household and (b) their status and worth in the eyes of other citizens. Responses were recorded on a five-point scale ranging from "Not at all" to "A great deal." The regression table corresponding to this figure can be found in Table S6.

## A.2 Additional Results on Alternative Mechanisms

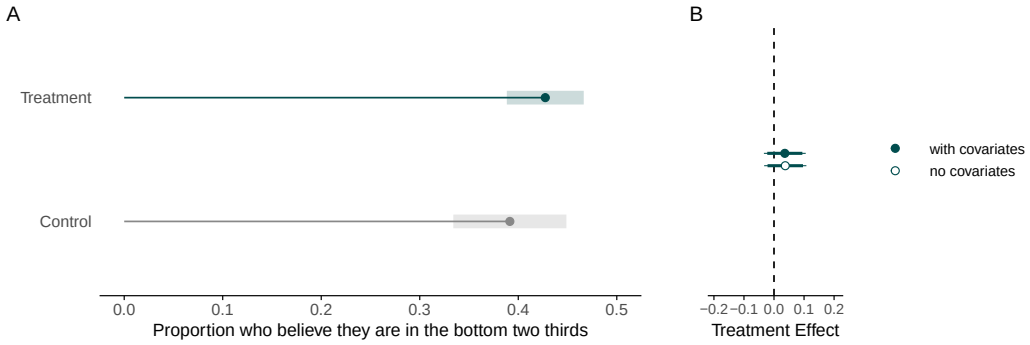



Figure A4: Beliefs about performance (Muslim)

*Note*: This figure displays treatment effects on Muslim respondents' beliefs about their own performance. After completing the transcription task, participants were asked what they believed their rank was, in terms of the number of screenshots transcribed, among other study participants ("Top 1/3," "Middle 1/3," or "Bottom 1/3"). Coefficients from linear regression models are shown. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S7. For the list of control variables used in the regression, see Table A1.

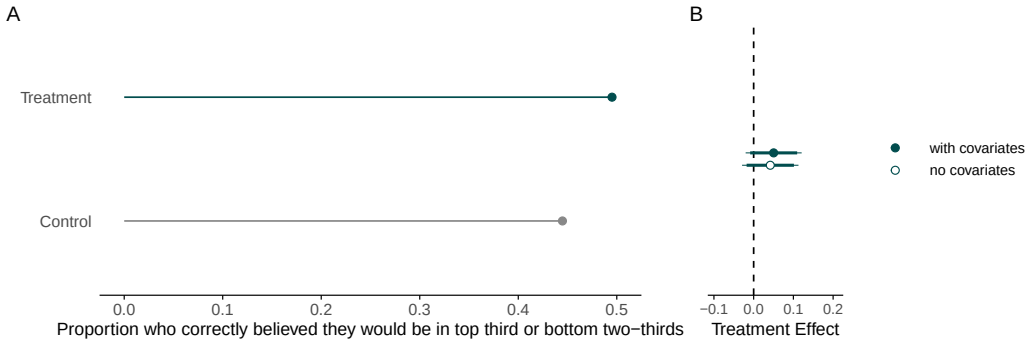



Figure A5: Accuracy of beliefs (Muslim)

*Note*: This figure displays treatment effects on the accuracy of Muslim respondents' beliefs about their own performance, measured as whether a participant's belief about whether they would be in the top third or bottom two thirds matched their actual position among study participants. Coefficients from linear regression models are shown. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S7. For the list of control variables used in the regression, see Table A1.

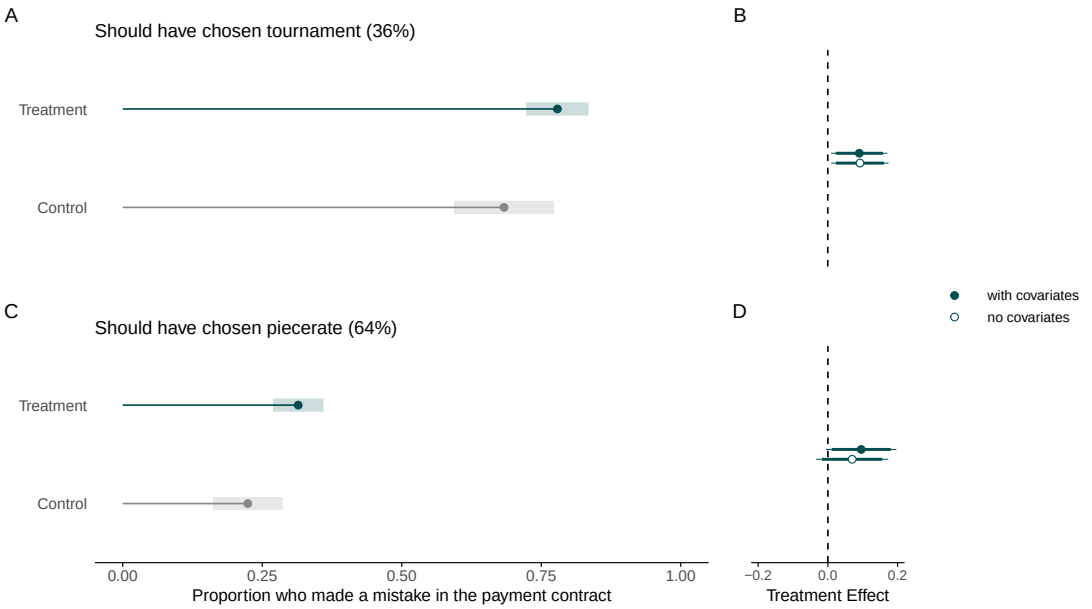



Figure A6: Types of mistakes (Muslim)

*Note*: This figure displays the proportion of Muslim respondents making a contract-choice mistake, by treatment status. Panels A and B cover participants for whom the tournament contract was optimal (who err by choosing the piece-rate); Panels C and D cover participants for whom the piece-rate contract was optimal (who err by choosing the tournament). Panels A and C report the proportion making a mistake in the treatment and control groups; Panels B and D report the corresponding treatment effects, with and without covariates. In Panels A and C, 95% confidence intervals are shown. In Panels B and D, 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S8. For the list of control variables used in the regression, see Table A1.

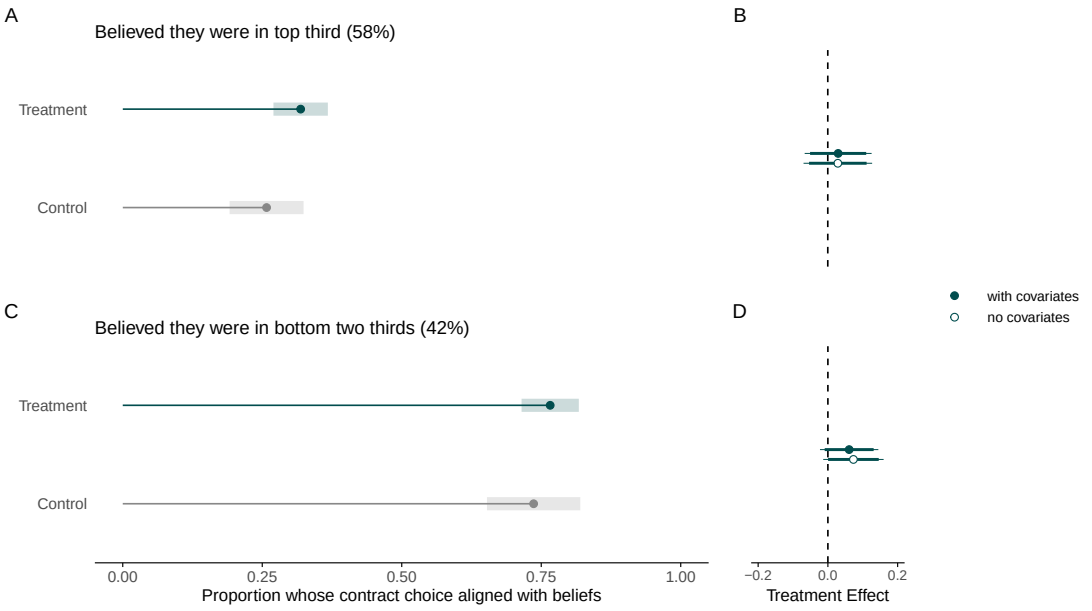



Figure A7: Do choices align with beliefs? (Muslim)

*Note*: This figure examines whether Muslim respondents' contract choices aligned with their beliefs about their own performance. It displays the proportion of respondents whose contract choice aligned with their beliefs, by treatment status. In Panel A the outcome is the proportion who chose the tournament, and in Panel C the outcome is the proportion who chose the piece-rate. Panels A and B cover respondents who believed they were in the top third, for whom the tournament contract was payoff-maximising (aligned means choosing the tournament); Panels C and D cover respondents who believed they were in the bottom two thirds, for whom the piece-rate contract was payoff-maximising (aligned means choosing the piece-rate). Panels A and C report the proportion of aligned choices in the treatment and control groups; Panels B and D report the corresponding treatment effects, with and without covariates. In Panels A and C, 95% confidence intervals are shown. In Panels B and D, 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S8. For the list of control variables used in the regression, see Table A1.

Table A6: Insecurity is Associated With Mistakes

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Payment choice mistake | | | |
| | (1) | (2) | (3) | (4) |
| Insecurity index | 0.141*** | 0.132*** | 0.125*** | 0.128*** |
| | (0.037) | (0.040) | (0.040) | (0.038) |
| Believes piecerate | | | 0.105* | 0.151** |
| | | | (0.062) | (0.059) |
| Number of correct transcriptions | | | | 0.021*** |
| | | | | (0.004) |
| Constant | 0.073 | −0.434 | −0.533 | −0.947*** |
| | (0.090) | (0.346) | (0.350) | (0.343) |
| Adjusted $R^2$ | 0.0462 | 0.0754 | 0.0822 | 0.1667 |
| Observations | 281 | 281 | 281 | 281 |
| Controls | No | Yes | Yes | Yes |
| State FE | No | Yes | Yes | Yes |
| Week FE | No | Yes | Yes | Yes |

*Notes*: This table reports OLS estimates of the relationship between the insecurity index and the probability of making a payment-choice mistake, estimated within the control group. *Believes piecerate* is an indicator equal to 1 if the participant believed the piece-rate contract would yield higher earnings. *Number of correct transcriptions* controls for task performance. Columns (2)–(4) include the full set of pre-treatment controls together with state and survey-week fixed effects. Standard errors are reported in parentheses. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

## A.3 Additional Hindu Results

Table A7: Balance Table (Hindu)

| Variable | Control | Treatment | Difference | P-value |
|---|---|---|---|---|
| Female | 0.425<br>[0.495] | 0.356<br>[0.479] | -0.069 | 0.051* |
| Age | 31.032<br>[10.216] | 30.739<br>[10.767] | -0.293 | 0.693 |
| Forward caste | 0.668<br>[0.472] | 0.613<br>[0.487] | -0.054 | 0.123 |
| Years of education | 17.636<br>[2.647] | 17.635<br>[2.727] | -0.000 | 0.999 |
| Income: $\leq$ Rs 50,000 (Bottom tercile) | 0.380<br>[0.486] | 0.424<br>[0.495] | 0.044 | 0.227 |
| Income: Rs 50,001–100,000 (Middle tercile) | 0.348<br>[0.477] | 0.325<br>[0.469] | -0.023 | 0.536 |
| Income: $>$ Rs 100,000 (Top tercile) | 0.272<br>[0.445] | 0.250<br>[0.434] | -0.021 | 0.542 |
| Voted BJP | 0.786<br>[0.411] | 0.782<br>[0.413] | -0.004 | 0.957 |
| State identity strength | 90.588<br>[14.037] | 85.939<br>[20.259] | -4.649 | 0.000*** |
| National identity strength | 95.153<br>[10.824] | 92.693<br>[15.590] | -2.460 | 0.013** |
| Caste identity strength | 69.722<br>[30.756] | 65.365<br>[33.088] | -4.357 | 0.054* |
| Religious identity strength | 84.997<br>[21.493] | 80.116<br>[26.699] | -4.881 | 0.005*** |
| F-test p-value (joint) | | | | 0.151 |
| Observations | 313 | 587 | | |

*Notes*: This table reports means, with standard deviations in square brackets, of pre-treatment characteristics for Hindu participants, separately for the control group and the pooled treatment group. The *Difference* column reports the treatment-minus-control difference in means, with stars indicating statistical significance. The row *F-test p-value (joint)* reports the $p$-value from a joint test of equality of all listed characteristics across the two groups. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

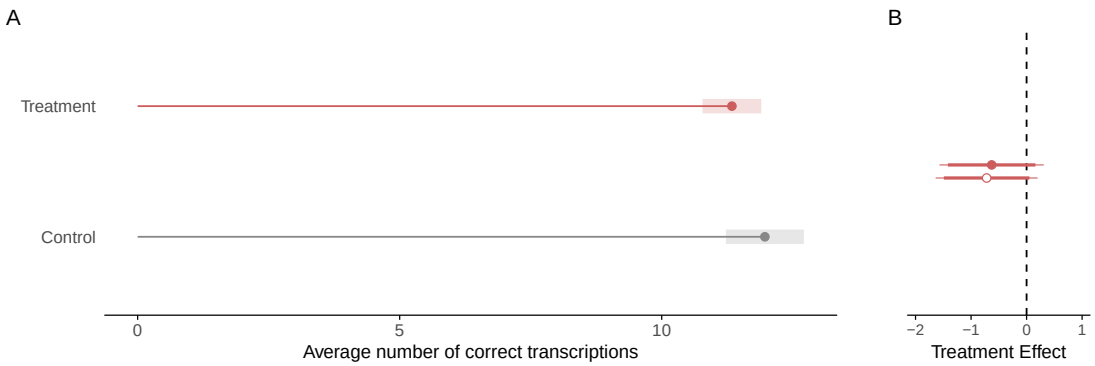



Figure A8: Treatment Effects on Number of Correct Transcriptions (Hindu)

*Notes*: Panel A shows group means for Hindu participants for any treatment (teal) and control (grey) groups, with 95% confidence intervals. Panel B presents the treatment difference plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S9. For the list of control variables used in the regression, see Table A1.

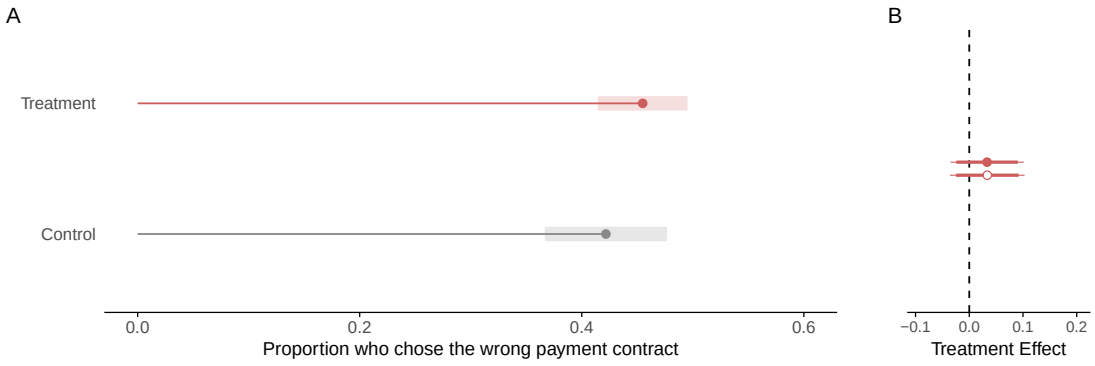



Figure A9: Treatment Effects on Payment Choice Mistake (Hindu)

*Notes*: Panel A shows group means for Hindu participants for any treatment (teal) and control (grey) groups, with 95% confidence intervals. Panel B presents the treatment difference plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S9. For the list of control variables used in the regression, see Table A1.

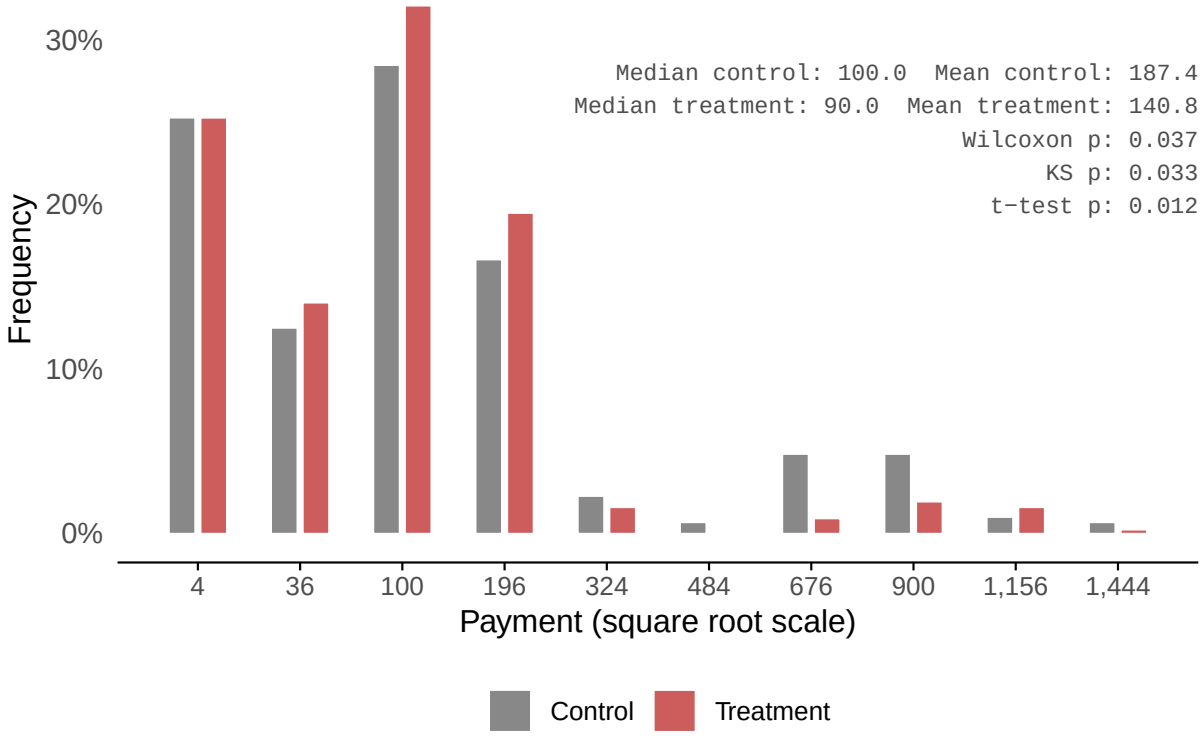



Figure A10: Distribution of earnings (Hindu)

*Note*: This figure displays the distribution of earnings for Hindu participants in any treatment (teal) and control (grey) groups. Earnings are measured in Indian Rupees.

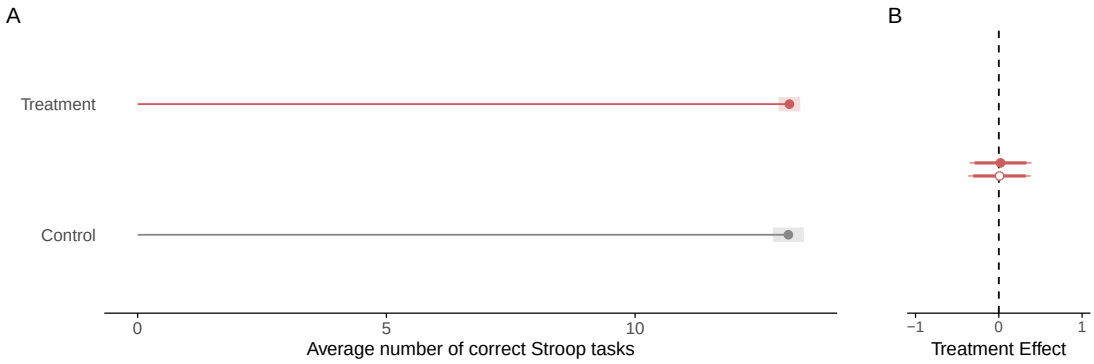



Figure A11: Treatment Effects on Number of Correct Cognitive Tasks (Hindu)

*Notes*: Panel A shows group means for Hindu participants for any treatment (teal) and control (grey) groups, with 95% confidence intervals. Panel B presents the treatment difference plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S10. For the list of control variables used in the regression, see Table A1.

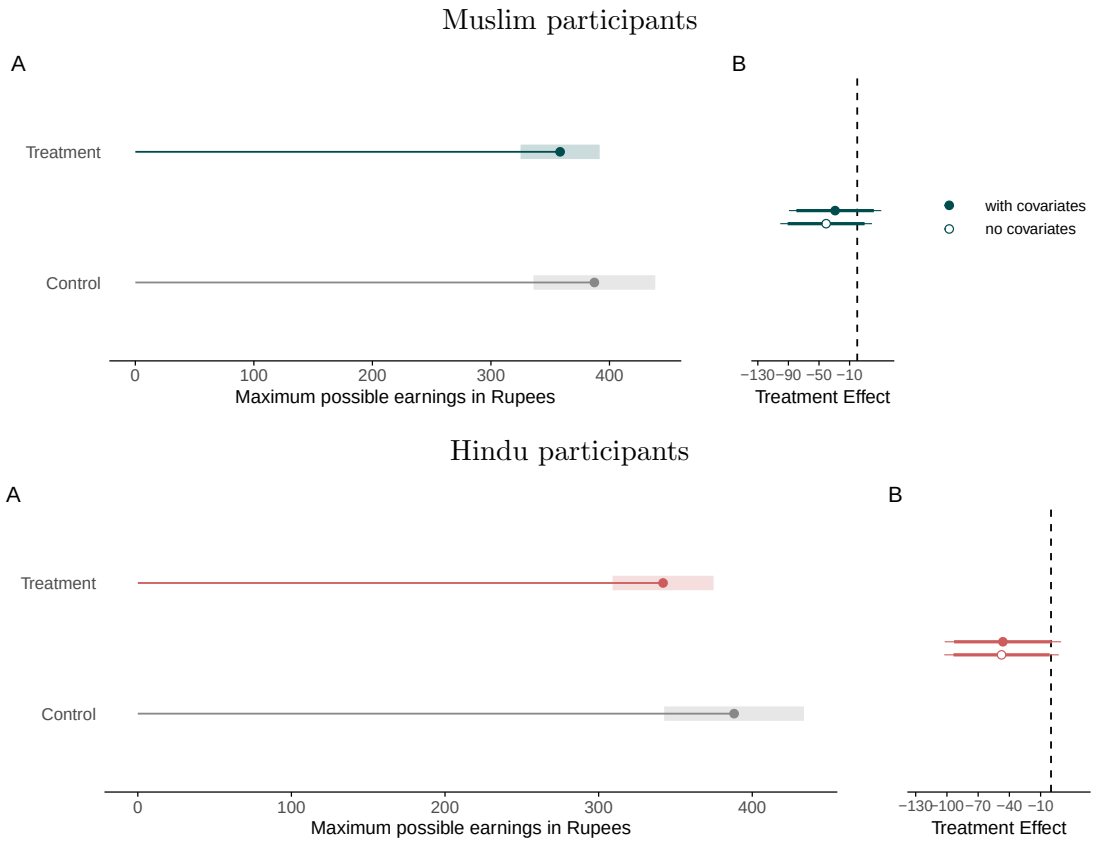



Figure A12: Treatment Effects on Maximum Possible Earnings

*Notes*: This figure displays treatment effects on the maximum possible earnings—what participants would have earned had they chosen the contract that was optimal for them—for Muslim participants (top) and Hindu participants (bottom). In each plot, Panel A shows group means for any treatment and control groups, with 95% confidence intervals, and Panel B presents the treatment difference plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. Earnings are measured in Indian Rupees. For the list of control variables used in the regression, see Table A1.

## A.4 Results From the In-Person Pilot

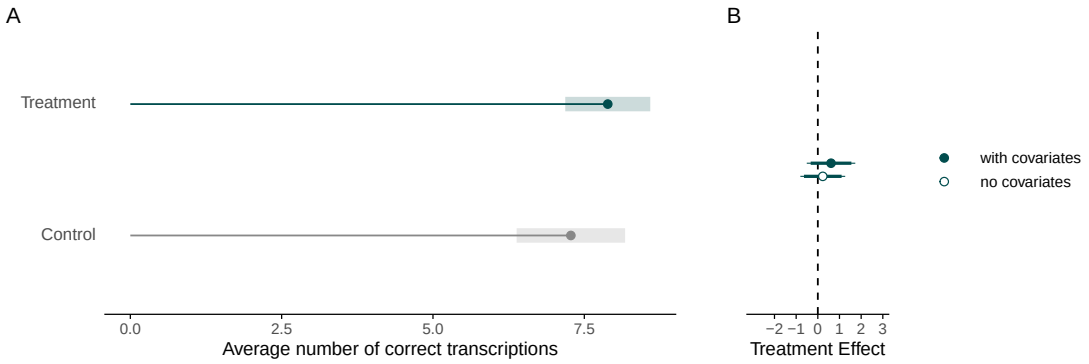



Figure A13: Treatment Effects on Number of Correct Transcriptions (Muslim participants, Field Sample)

*Notes*: Panel A shows group means for Muslim participants for any treatment (teal) and control (grey) groups, with 95% confidence intervals. Panel B presents the treatment difference plotted as coefficients from linear regression models. 90% and 95% confidence intervals are shown in thick and thin lines, respectively. The regression table corresponding to this figure can be found in Table S12. For the list of control variables used in the regression, see Table A1.

Table A8: Treatment Effects on Payment Choice Mistake (Muslim participants, Field Sample)

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Payment choice mistake | | | |
| | (1) | (2) | (3) | (4) |
| Any treatment | 0.084* | 0.091* | | |
| | (0.050) | (0.051) | | |
| Weakly symbolic | | | 0.067 | 0.070 |
| | | | (0.059) | (0.061) |
| Strongly symbolic | | | 0.106* | 0.115* |
| | | | (0.063) | (0.063) |
| Constant | 0.394*** | −0.263 | 0.394*** | −0.272 |
| | (0.037) | (0.315) | (0.037) | (0.315) |
| Adjusted $R^2$ | 0.0046 | 0.0166 | 0.0029 | 0.0150 |
| Observations | 393 | 393 | 393 | 393 |
| Controls | No | Yes | No | Yes |
| $H_0$: Weakly = Strongly, p-value | | | 0.574 | 0.519 |

*Notes*: This table reports OLS estimates of treatment effects on the probability of making a payment-choice mistake for Muslim participants in the in-person field experiment in West Bengal, restricted to participants with sufficient language proficiency (see Appendix SB). Columns (1) and (2) report the effect of being assigned to any treatment, while columns (3) and (4) report separate effects for the Weakly Symbolic and Strongly Symbolic treatments. Columns (2) and (4) include the full set of pre-treatment controls. Standard errors are reported in parentheses. The row "$H_0$: Weakly = Strongly" reports the $p$-value from a test of the equality of the Weakly Symbolic and Strongly Symbolic coefficients. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

# B Ethics Appendix

The primary ethical concern is that our treatment may negatively affect minority individuals, particularly in terms of cognition and productivity. Is it ethical to knowingly expose individuals to harm through our treatments and potentially leave them in a "harmed state"? We address and mitigate these concerns in the following ways:

1. **Factual policies**: The tweets we use are based on factual policies that can naturally be encountered in daily life. To ensure ethical integrity, we take care that our tweets and news articles are not gratuitously harmful. While many individuals are regularly exposed to harmful material online, our content does not include anything hateful. Instead, we focus on factual policies. Moreover, exposure to these policies may raise awareness, potentially helping participants cope with them in the future.

2. **Informed consent**: All participants were informed in advance that some of the material they encounter may be disturbing or potentially harmful, although we will not provide any false information. In addition, we explain that social media sometimes contains harmful content and ensure that participants consent to this exposure before proceeding.

3. **Debriefing**: All participants were debriefed at the end of the experimental sessions. This debriefing allowed participants to reflect on the experience and seek further information, and we offered to provide relevant resources if needed.

4. **Feedback from participants**: After conducting test and pilot sessions, we asked participants whether they would be willing to take part in similar sessions in the future. The overwhelming majority responded positively, indicating their willingness to participate again.

# Supplemental Appendix

# SA Further Details on Experiment Design

## SA.1 Recruitment and Sample Selection

The online sample was selected using the following criteria:

1. 30-minute online survey
2. Sample size: 1000 Hindus and 1000 Muslims
3. Balanced across gender: 50% women and 50% men
4. Representative sample at the state level from the following states: Maharashtra, Karnataka, Uttar Pradesh, New Delhi, West Bengal, Uttarakhand, Andhra Pradesh, Telengana, Gujarat, Kerala, and Assam, in the proportions following the census figures (see Table S2). The total sample from each state should follow these numbers, but we require a 50:50 share of Hindus and Muslims as mentioned above.
5. Participants need to understand English well enough to answer our surveys.

We additionally collected GPS locations of the cities of the survey participants in addition. We also paid additional variable monetary incentives based on performance at the end of the survey. The approximate average amount we gave participants was Rs. 300 (around €3.4) per person. This amount is in addition to any incentives they receive for completing the survey.

## SA.2 Experiment stimuli

Figure S1: Construction of the Analytical Sample

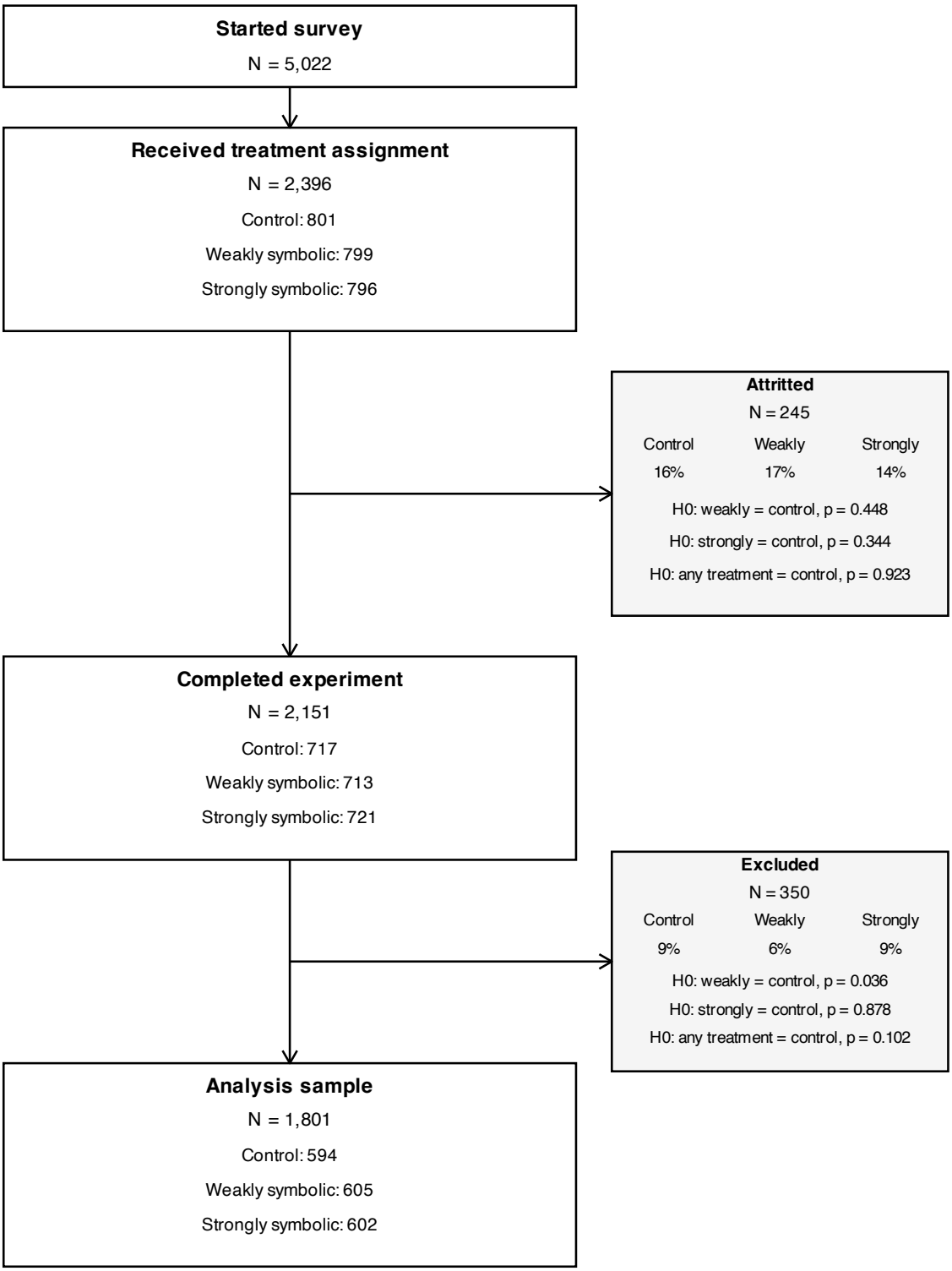





Figure S2




(a) Tweets used in the Weakly Symbolic Treatment




(b) Tweets used in the Strongly Symbolic Treatment




(c) Tweets in the Control Treatment

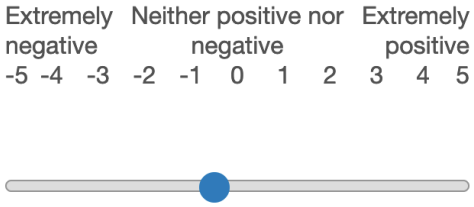
Figure S3: Summary of Tweets by Treatment Arms

*Notes*: Panel (a) presents a summary of the tweets used in the *Weakly Symbolic* treatment, which highlight policies that are symbolic in nature but may also carry tangible or legal consequences for some individuals. Panel (b) displays the tweets used in the *Strongly Symbolic* treatment, which reflect policies that signal social exclusion without direct material impact. Panel (c) shows the tweets used in the *Control* treatment, which cover neutral topics unrelated to exclusion or identity, and serve as a baseline comparison.

(a) Example Control Tweet: Mahogany Micropropagation




(b) Engagement Question I




(c) Engagement Question II




(d) Engagement Question III

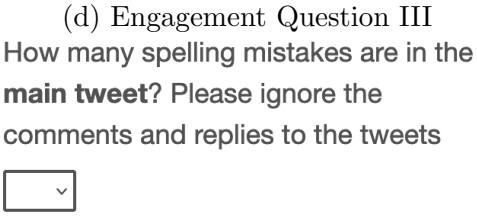


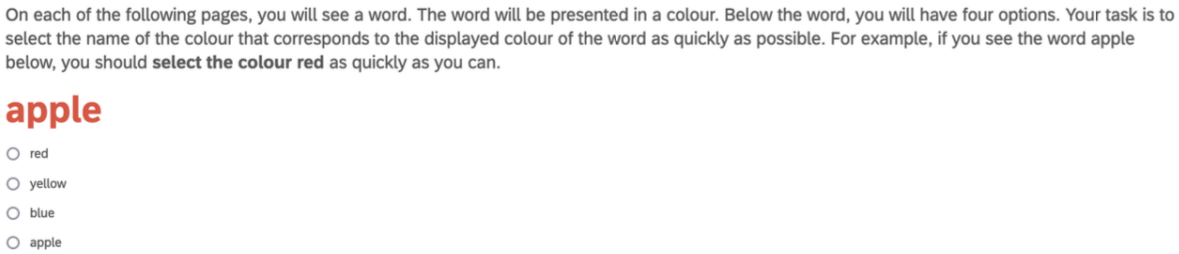

Figure S4: Example Tweet and Engagement Questions

*Notes*: Panel (a) shows a sample control tweet featuring a neutral topic on mahogany micropropagation. Panels (b), (c), and (d) display engagement questions used to measure participant engagement with the tweet content. The source, "News Today," is a fictional news agency, with headlines inspired by real articles but modified to be similar to typical social media content. This approach ensured tweets in the study resembled those encountered in daily online environments.

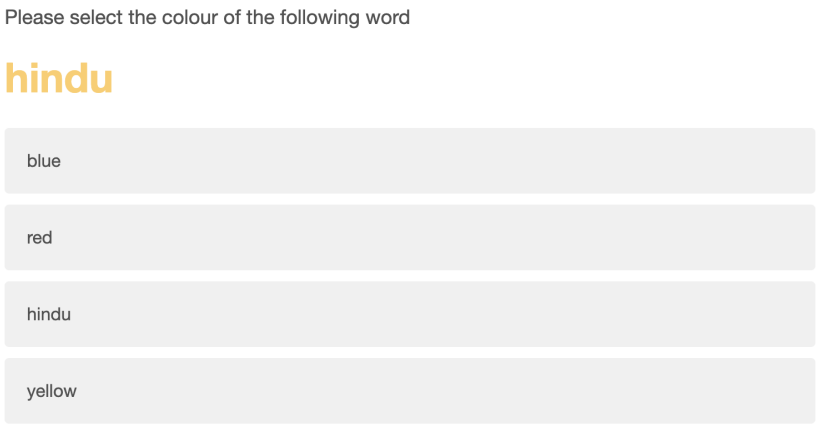



Figure S5: Transcription Task Example

*Notes*: This figure shows an example of the screenshots participants were asked to transcribe in the productivity task. Participants typed out the subtitled text displayed inside the highlighted yellow circle, attending to words, spaces, punctuation, and capitalisation. After a short practice round, participants were given 10 minutes to transcribe as many screenshots as possible and were paid per word correctly transcribed. The structure of the payment contract was revealed only after the transcription task was completed, ensuring that effort during the task was not distorted by strategic responses to the payment scheme.




(a) Example (non-emotional) Stroop Test




(b) Example (emotional) Stroop Test

Figure S6: Emotional Stroop Test

*Notes*: Panel (a) illustrates example (non-emotional) Stroop Test. Panel (b) illustrates example (emotional) Stroop Test.

## SB The Field Experiment in West Bengal

The experiment reported in the main text was conducted online. As noted in Section 3, however, the project began with a smaller field study run in person during the preparatory phase. The study was originally designed as a fully online experiment targeting gig-economy workers, but pandemic-related delays and uncertainty led us first to pilot the design as a "soft launch" under more led, in-person field conditions before scaling up to the larger, pre-registered online sample. This appendix documents that field experiment. Throughout, we treat its results as exploratory and the online study as confirmatory, for the reasons set out below.

**Location and coverage.** For logistical reasons, in-person recruitment and data collection were conducted exclusively in the state of West Bengal. Recruitment spanned 46 locations across 12 districts: North 24 Parganas, South 24 Parganas, Birbhum, East Burdwan, West Burdwan, Hooghly, Jalpaiguri, Kolkata, Nadia, North Dinajpur, South Dinajpur, and West Midnapore. West Bengal is home to 24.6 million Muslims, or 14.29% of India's total Muslim population. The twelve districts we cover together account for 19.0 million Muslims—77.24% of West Bengal's Muslim population—and 20.81% of the state's total population.

**Implementation.** In-person recruitment and field implementation were carried out by DAI Research, while the online sample was recruited by TGM Research. Data collection took place between 8 January and 25 February 2024 across 105 supervised sessions, each lasting roughly one to one-and-a-half hours. Participants were engaged for approximately half a day and received a fixed participation fee together with performance-based incentives, averaging about Rs. 300 (€3.4) per person. Sessions were conducted in controlled settings that mirrored the online protocol.

**Sample.** We recruited 1,504 participants, balanced by religion (754 Hindu and 750 Muslim) and by gender (roughly half women), consistent with our target of a 50:50 Hindu–Muslim split obtained through religion-stratified recruitment. After applying our exclusion restrictions—most importantly the removal of participants with insufficient English proficiency (discussed below), along with those who failed the attention check or reported a religion other than Hindu or Muslim—the analysis sample comprises approximately 1,000 participants. Relative to the online sample, field participants

had lower levels of education and different patterns of internet use.

**Design.** The field experiment followed the same protocol as the online study. Participants completed a tweet-tagging task that delivered the treatment, followed by a transcription task that measured productivity and a choice between a piece-rate and a tournament contract that measured economic decision-making. As online, there were three arms—the policy bundles we label Strongly Symbolic and Weakly Symbolic in the main text correspond, respectively, to the "symbolic" and "material" (mixed) arms of the field study—and a neutral control. Assignment was randomised at the individual level within Qualtrics, stratified by religion. The pre-registered field design assigned half of participants to the control arm and split the remainder evenly across the two treatment arms (a planned 375 Strongly Symbolic, 375 Weakly Symbolic, and 750 control), in contrast to the equal three-way split used online. Cognitive load in the field was measured using Raven's Progressive Matrices and a numerical Stroop task; the online study instead paired the numerical Stroop with an emotional Stroop task.

**Pre-registration and exploratory status.** Both the field and online experiments were pre-registered with the AEA RCT Registry, with the intervention window running from December 2023 to July 2024. The two studies differ in one consequential respect. We did not pre-register English-language proficiency as an exclusion criterion for the field experiment, and only discovered during fieldwork that a substantial share of in-person participants had limited English comprehension. Because the tasks and survey instruments were administered in English, this materially affected data quality. We therefore (i) pre-registered the subsequent online study with a larger sample and an explicit English-proficiency exclusion, alongside standard attention checks, and (ii) treat the field results as exploratory and the online results as confirmatory. The exclusion of low-proficiency participants is thus a deviation from the field pre-analysis plan, whereas the online analysis follows its pre-analysis plan without deviation.

**Results.** Despite these differences in setting, education, and internet use, the field results closely track the online findings. As shown in Figure 9, treated Muslim participants in the field were significantly more likely to choose the wrong payment contract, while treatment effects on the transcription task were again limited (Figure A13). This pattern mirrors the online results and

reinforces the robustness and external validity of our core findings.

# SC Supplemental Results

Table S1: Muslims and Hindus in the Analysis Sample

| Variable | Muslim | Hindu | Difference | P-value |
|---|---|---|---|---|
| Female | 0.408<br>[0.492] | 0.380<br>[0.486] | 0.028 | 0.217 |
| Age | 29.455<br>[9.137] | 30.841<br>[10.574] | -1.386 | 0.003*** |
| Voted BJP | 0.231<br>[0.422] | 0.783<br>[0.412] | -0.552 | 0.000*** |
| Income: $\leq$ Rs 50,000 (Bottom tercile) | 0.420<br>[0.494] | 0.409<br>[0.492] | 0.011 | 0.647 |
| Income: Rs 50,001–100,000 (Middle tercile) | 0.349<br>[0.477] | 0.333<br>[0.472] | 0.015 | 0.497 |
| Income: $>$ Rs 100,000 (Top tercile) | 0.232<br>[0.422] | 0.258<br>[0.438] | -0.026 | 0.203 |
| State identity strength | 87.336<br>[18.567] | 87.556<br>[18.461] | -0.219 | 0.802 |
| National identity strength | 92.283<br>[14.919] | 93.549<br>[14.159] | -1.266 | 0.065* |
| Caste identity strength | 72.852<br>[32.754] | 66.880<br>[32.346] | 5.972 | 0.000*** |
| Religious identity strength | 88.830<br>[19.940] | 81.813<br>[25.108] | 7.017 | 0.000*** |
| Forward caste | 0.573<br>[0.495] | 0.632<br>[0.482] | -0.060 | 0.010*** |
| Years of education | 17.260<br>[2.972] | 17.636<br>[2.698] | -0.376 | 0.005*** |
| Observations | 901 | 900 | | |

*Notes*: This table reports means, with standard deviations in square brackets, of pre-treatment characteristics for Muslim and Hindu participants in the analysis sample (901 Muslim participants and 900 Hindu participants). The *Difference* column reports the Muslim mean minus the Hindu mean, with stars indicating whether the difference is statistically significant, based on a $t$-test for continuous variables and Fisher's exact test for proportions. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S2: Religious Composition of Survey Sample by State

| State | Population (m) | Pop. share (%) | Muslim share (%) | Hindu share (%) | Sample Muslim N | Sample Hindu N | Sample total | Sample share (%) |
|---|---|---|---|---|---|---|---|---|
| Andhra Pradesh | 84.6 | 7.0 | 9.6 | 88.5 | 77 | 62 | 139 | 7.7 |
| Assam | 31.2 | 2.6 | 34.2 | 61.5 | 32 | 41 | 73 | 4.1 |
| Delhi | 16.8 | 1.4 | 12.9 | 81.7 | 29 | 21 | 50 | 2.8 |
| Gujarat | 60.4 | 5.0 | 9.7 | 88.6 | 57 | 75 | 132 | 7.3 |
| Karnataka | 61.1 | 5.0 | 12.9 | 84.0 | 108 | 85 | 193 | 10.7 |
| Kerala | 33.4 | 2.8 | 26.6 | 54.7 | 52 | 44 | 96 | 5.3 |
| Maharashtra | 112.4 | 9.3 | 11.5 | 79.8 | 179 | 146 | 325 | 18.0 |
| Telangana | – | – | – | – | 61 | 51 | 112 | 6.2 |
| Uttar Pradesh | 199.8 | 16.5 | 19.3 | 79.7 | 182 | 250 | 432 | 24.0 |
| Uttarakhand | 10.1 | 0.8 | 13.9 | 83.0 | 13 | 14 | 27 | 1.5 |
| West Bengal | 91.3 | 7.5 | 27.0 | 70.5 | 111 | 111 | 222 | 12.3 |
| Total | 701.1 | 57.9 | 17.3 | 78.8 | 901 | 900 | 1801 | 100.0 |

Table S3: Balance Table by Treatment Arm (Muslim)

| Variable | Control | Strongly symbolic | Weakly symbolic | Diff: strongly | P: strongly | Diff: weakly | P: weakly |
|---|---|---|---|---|---|---|---|
| Female | 0.391 | 0.421 | 0.411 | 0.030 | 0.514 | 0.020 | 0.689 |
| | [0.489] | [0.495] | [0.493] | | | | |
| Age | 30.153 | 29.749 | 28.524 | -0.404 | 0.580 | -1.629 | 0.031** |
| | [8.644] | [9.048] | [9.602] | | | | |
| Forward caste | 0.569 | 0.588 | 0.560 | 0.019 | 0.700 | -0.010 | 0.881 |
| | [0.496] | [0.493] | [0.497] | | | | |
| Years of education | 17.459 | 17.222 | 17.117 | -0.237 | 0.336 | -0.343 | 0.160 |
| | [2.969] | [3.009] | [2.935] | | | | |
| Income: $\leq$ Rs 50,000 (Bottom tercile) | 0.363 | 0.447 | 0.443 | 0.084 | 0.046** | 0.080 | 0.057* |
| | [0.482] | [0.498] | [0.498] | | | | |
| Income: Rs 50,001–100,000 (Middle tercile) | 0.342 | 0.334 | 0.369 | -0.007 | 0.921 | 0.027 | 0.545 |
| | [0.475] | [0.473] | [0.483] | | | | |
| Income: $>$ Rs 100,000 (Top tercile) | 0.295 | 0.219 | 0.188 | -0.077 | 0.041** | -0.108 | 0.003*** |
| | [0.457] | [0.414] | [0.391] | | | | |
| Voted BJP | 0.285 | 0.232 | 0.181 | -0.053 | 0.166 | -0.103 | 0.004*** |
| | [0.452] | [0.422] | [0.386] | | | | |
| State identity strength | 89.060 | 87.151 | 85.955 | -1.909 | 0.196 | -3.106 | 0.043** |
| | [17.326] | [18.407] | [19.718] | | | | |
| National identity strength | 92.288 | 92.286 | 92.275 | -0.002 | 0.999 | -0.013 | 0.991 |
| | [14.899] | [15.005] | [14.900] | | | | |
| Caste identity strength | 71.427 | 75.154 | 71.832 | 3.727 | 0.161 | 0.405 | 0.884 |
| | [33.565] | [31.088] | [33.611] | | | | |
| Religious identity strength | 89.313 | 89.244 | 87.974 | -0.069 | 0.966 | -1.339 | 0.421 |
| | [19.565] | [19.533] | [20.705] | | | | |
| F-test p-value (joint) | | | | | 0.074* | | 0.029** |
| Observations | 281 | 311 | 309 | | | | |

*Notes*: This table reports means, with standard deviations in square brackets, of pre-treatment characteristics for Muslim participants, separately for the control group and the Strongly Symbolic and Weakly Symbolic treatment arms. The *Diff: strongly* and *Diff: weakly* columns report each arm's difference in means from the control group, with stars indicating statistical significance. The row *F-test p-value (joint)* reports the $p$-value from a joint test of equality of all listed characteristics between each arm and the control group. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S4: Balance Table by Treatment Arm (Hindu)

| Variable | Control | Strongly symbolic | Weakly symbolic | Diff: strongly | P: strongly | Diff: weakly | P: weakly |
|---|---|---|---|---|---|---|---|
| Female | 0.425 | 0.381 | 0.331 | -0.043 | 0.315 | -0.094 | 0.021** |
| | [0.495] | [0.487] | [0.471] | | | | |
| Age | 31.032 | 30.897 | 30.584 | -0.135 | 0.873 | -0.447 | 0.602 |
| | [10.216] | [10.616] | [10.929] | | | | |
| Forward caste | 0.668 | 0.619 | 0.608 | -0.049 | 0.240 | -0.060 | 0.148 |
| | [0.472] | [0.487] | [0.489] | | | | |
| Years of education | 17.636 | 17.639 | 17.632 | 0.003 | 0.988 | -0.004 | 0.985 |
| | [2.647] | [2.720] | [2.739] | | | | |
| Income: $\leq$ Rs 50,000 (Bottom tercile) | 0.380 | 0.399 | 0.449 | 0.018 | 0.703 | 0.069 | 0.099* |
| | [0.486] | [0.490] | [0.498] | | | | |
| Income: Rs 50,001–100,000 (Middle tercile) | 0.348 | 0.344 | 0.307 | -0.005 | 0.974 | -0.041 | 0.324 |
| | [0.477] | [0.476] | [0.462] | | | | |
| Income: > Rs 100,000 (Top tercile) | 0.272 | 0.258 | 0.243 | -0.014 | 0.770 | -0.028 | 0.480 |
| | [0.445] | [0.438] | [0.430] | | | | |
| Voted BJP | 0.786 | 0.787 | 0.777 | 0.001 | 1.000 | -0.009 | 0.867 |
| | [0.411] | [0.410] | [0.417] | | | | |
| State identity strength | 90.588 | 85.560 | 86.311 | -5.028 | 0.000*** | -4.277 | 0.003*** |
| | [14.037] | [19.600] | [20.912] | | | | |
| National identity strength | 95.153 | 92.845 | 92.544 | -2.308 | 0.032** | -2.609 | 0.018** |
| | [10.824] | [15.329] | [15.867] | | | | |
| Caste identity strength | 69.722 | 63.093 | 67.598 | -6.629 | 0.011** | -2.124 | 0.411 |
| | [30.756] | [33.082] | [32.998] | | | | |
| Religious identity strength | 84.997 | 80.639 | 79.601 | -4.358 | 0.025** | -5.395 | 0.007*** |
| | [21.493] | [26.041] | [27.365] | | | | |
| F-test p-value (joint) | | | | | 0.323 | | 0.155 |
| Observations | 313 | 291 | 296 | | | | |

*Notes*: This table reports means, with standard deviations in square brackets, of pre-treatment characteristics for Hindu participants, separately for the control group and the Strongly Symbolic and Weakly Symbolic treatment arms. The *Diff: strongly* and *Diff: weakly* columns report each arm's difference in means from the control group, with stars indicating statistical significance. The row *F-test p-value (joint)* reports the $p$-value from a joint test of equality of all listed characteristics between each arm and the control group. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

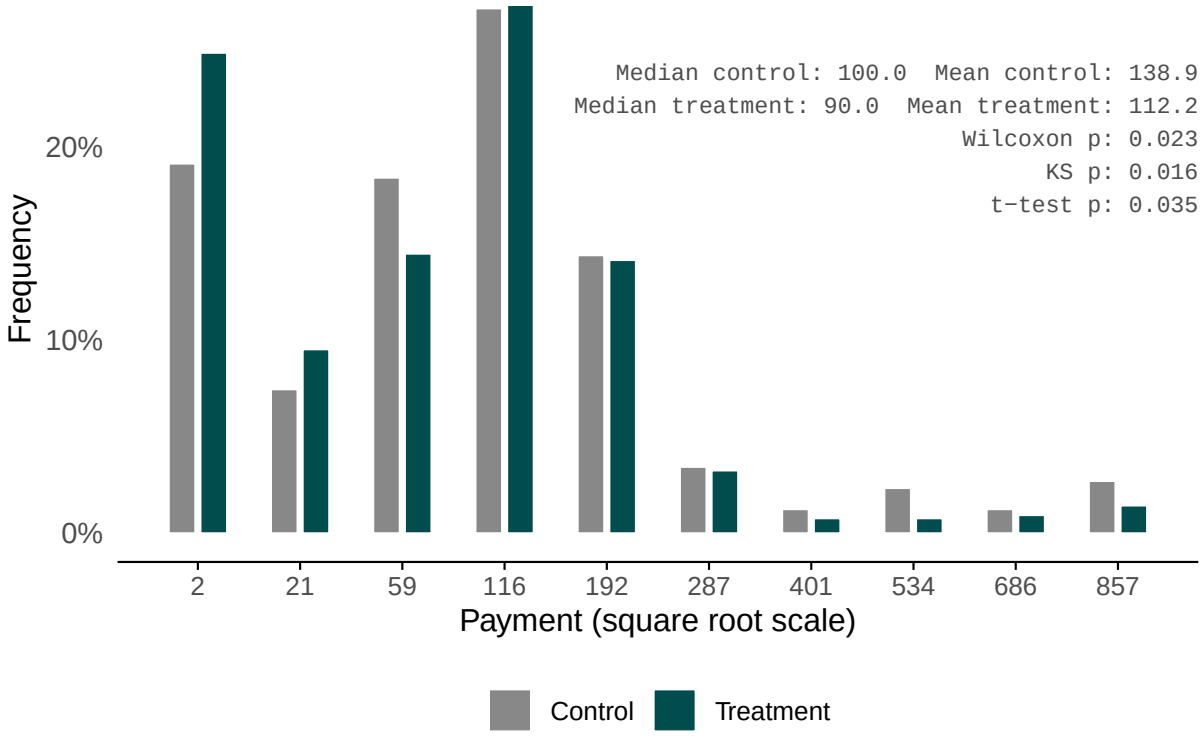



Figure S1: Distribution of earnings for Muslim participants without outliers

*Note*: This figure displays the distribution of earnings for Muslim respondents in any treatment (blue) and control (grey) groups, excluding outliers, i.e. 24 participants (3% of the sample) who earned more than 3.5 standard deviations more than the mean. Earnings are measured in Indian Rupees.

Table S5: Insecurity treatment effects (Muslim participants)

| | *Dependent variable:* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Insecurity index | | Does not feel true Indian | | Feels unsure | | Worries about discrimination | | Feels nervous and stressed | | Cognitive score | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Any treatment | 0.395*** | 0.338*** | 0.189** | 0.155* | 0.236*** | 0.207*** | 0.591*** | 0.512*** | 0.564*** | 0.478*** | −0.461** | −0.404* |
| | (0.056) | (0.054) | (0.080) | (0.079) | (0.078) | (0.077) | (0.095) | (0.094) | (0.095) | (0.095) | (0.210) | (0.209) |
| Constant | 2.317*** | 3.426*** | 1.829*** | 3.580*** | 1.893*** | 3.651*** | 3.007*** | 3.303*** | 2.537*** | 3.167*** | 13.338*** | 10.789*** |
| | (0.046) | (0.253) | (0.066) | (0.372) | (0.065) | (0.361) | (0.079) | (0.443) | (0.079) | (0.446) | (0.174) | (0.987) |
| Adjusted $R^2$ | 0.0516 | 0.1637 | 0.0050 | 0.0673 | 0.0090 | 0.0833 | 0.0401 | 0.0999 | 0.0365 | 0.0903 | 0.0042 | 0.0516 |
| Observations | 901 | 901 | 901 | 901 | 901 | 901 | 901 | 901 | 901 | 901 | 901 | 901 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |

*Notes*: This table reports OLS estimates of treatment effects for Muslim participants on the insecurity index (columns 1–2), its four components (columns 3–10), and the cognitive-task score (columns 11–12). Each column reports the effect of being assigned to any treatment; even-numbered columns include the full set of pre-treatment controls. Standard errors are reported in parentheses. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S6: Policy treatment effects

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Policies affect status | | Policies affect finances | |
| | (1) | (2) | (3) | (4) |
| Weakly symbolic | 0.289** | 0.287** | 0.154 | 0.174 |
| | (0.112) | (0.113) | (0.114) | (0.113) |
| Strongly symbolic | 0.234** | 0.241** | −0.179 | −0.182 |
| | (0.112) | (0.112) | (0.114) | (0.113) |
| Constant | 2.737*** | 2.509*** | 2.484*** | 2.163*** |
| | (0.081) | (0.464) | (0.082) | (0.465) |
| Adjusted $R^2$ | 0.0060 | 0.0385 | 0.0077 | 0.0624 |
| Observations | 901 | 901 | 901 | 901 |
| Controls | No | Yes | No | Yes |
| $H_0$: Weakly = Strongly, p-value | 0.616 | 0.669 | 0.003 | 0.001 |

*Notes*: This table reports OLS estimates of treatment effects on Muslim participants' perceptions of how the policies shown in the tweets would affect their status and worth in the eyes of other citizens (columns 1–2) and the daily finances of their household (columns 3–4). All columns report separate effects for the Weakly Symbolic and Strongly Symbolic treatments. Even-numbered columns include the full set of pre-treatment controls. Standard errors are reported in parentheses. The row "$H_0$: Weakly = Strongly" reports the $p$-value from a test of the equality of the Weakly Symbolic and Strongly Symbolic coefficients. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S7: Beliefs treatment effects

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Believes piecerate is optimal | | | | Correct beliefs about payment rank | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Any treatment | 0.036 | 0.037 | | | 0.050 | 0.042 | | |
| | (0.035) | (0.036) | | | (0.036) | (0.036) | | |
| Weakly symbolic | | | 0.036 | 0.029 | | | 0.050 | 0.036 |
| | | | (0.041) | (0.041) | | | (0.041) | (0.041) |
| Strongly symbolic | | | 0.036 | 0.046 | | | 0.050 | 0.047 |
| | | | (0.041) | (0.041) | | | (0.041) | (0.041) |
| Constant | 0.391*** | 0.846*** | 0.391*** | 0.850*** | 0.445*** | 1.149*** | 0.445*** | 1.152*** |
| | (0.029) | (0.169) | (0.029) | (0.169) | (0.030) | (0.170) | (0.030) | (0.171) |
| Adjusted $R^2$ | 0.0000 | 0.0214 | -0.0011 | 0.0205 | 0.0011 | 0.0294 | -0.0000 | 0.0284 |
| Observations | 901 | 901 | 901 | 901 | 901 | 901 | 901 | 901 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| $H_0$: Weakly = Strongly, p-value | | | 0.991 | 0.662 | | | 0.999 | 0.781 |

*Notes*: This table reports OLS estimates of treatment effects on Muslim participants' beliefs about their own performance: an indicator equal to one if the participant believes the piece-rate contract is the earnings-maximising choice (columns 1–4) and an indicator equal to one if the participant's belief about their payment rank is correct (columns 5–8). Columns (1), (2), (5), and (6) report the effect of being assigned to any treatment, while the remaining columns report separate effects for the Weakly Symbolic and Strongly Symbolic treatments. Even-numbered columns include the full set of pre-treatment controls. Standard errors are reported in parentheses. The row "$H_0$: Weakly = Strongly" reports the $p$-value from a test of the equality of the Weakly Symbolic and Strongly Symbolic coefficients. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S8: Further beliefs treatment effects

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Payment choice mistake (subsample by optimal payment) | | | | Choice aligns with beliefs (subsample by prior beliefs) | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Any treatment | 0.096* | 0.070 | 0.090** | 0.092** | 0.030 | 0.029 | 0.061 | 0.073* |
| | (0.051) | (0.053) | (0.041) | (0.042) | (0.049) | (0.050) | (0.043) | (0.044) |
| Constant | 0.682*** | 0.796*** | 0.224*** | 0.091 | 0.736*** | 0.866*** | 0.257*** | 0.195 |
| | (0.042) | (0.262) | (0.034) | (0.194) | (0.041) | (0.238) | (0.035) | (0.215) |
| Adjusted $R^2$ | 0.0076 | 0.0259 | 0.0066 | 0.0347 | -0.0017 | 0.0284 | 0.0020 | -0.0145 |
| Observations | 323 | 323 | 578 | 578 | 375 | 375 | 526 | 526 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Subsample | Piecerate not optimal | | Piecerate optimal | | Believes piecerate | | Does not believe piecerate | |

*Notes*: This table reports OLS estimates of treatment effects for Muslim participants on two outcomes, each estimated separately within subsamples. Columns (1)–(4) report effects on the probability of making a payment-choice mistake, for participants for whom the piece-rate contract was not optimal, that is, the tournament contract was optimal (columns 1–2), and for those for whom the piece-rate contract was optimal (columns 3–4). Columns (5)–(8) report effects on whether the participant's contract choice aligns with their stated beliefs, for those who believed the piece-rate contract would yield higher earnings (columns 5–6) and for those who did not (columns 7–8). Each coefficient is the effect of being assigned to any treatment. Even-numbered columns include the full set of pre-treatment controls. Standard errors are reported in parentheses. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S9: Treatment Effects on Main Outcomes (Hindu)

| | *Dependent variable:* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Payment choice mistake | | | | Number of correct transcriptions | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Any treatment | 0.033 | 0.033 | | | −0.629 | −0.719 | | |
| | (0.035) | (0.035) | | | (0.478) | (0.468) | | |
| Weakly symbolic | | | 0.055 | 0.053 | | | −0.728 | −0.759 |
| | | | (0.040) | (0.041) | | | (0.555) | (0.543) |
| Strongly symbolic | | | 0.011 | 0.014 | | | −0.528 | −0.680 |
| | | | (0.040) | (0.041) | | | (0.557) | (0.543) |
| Constant | 0.422*** | 0.362** | 0.422*** | 0.363** | 11.968*** | 15.230*** | 11.968*** | 15.227*** |
| | (0.028) | (0.174) | (0.028) | (0.174) | (0.386) | (2.302) | (0.387) | (2.304) |
| Adjusted $R^2$ | -0.0001 | -0.0006 | 0.0000 | -0.0007 | 0.0008 | 0.0741 | -0.0002 | 0.0731 |
| Observations | 900 | 900 | 900 | 900 | 900 | 900 | 900 | 900 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| $H_0$: Weakly = Strongly, p-value | | | 0.291 | 0.338 | | | 0.723 | 0.885 |

*Notes*: This table reports OLS estimates of treatment effects on the two main outcomes for Hindu participants: the probability of making a payment-choice mistake and the number of correct transcriptions. Columns (1), (2), (5), and (6) report the effect of being assigned to any treatment, while the remaining columns report separate effects for the Weakly Symbolic and Strongly Symbolic treatments. Even-numbered columns include the full set of pre-treatment controls. Standard errors are reported in parentheses. The row "$H_0$: Weakly = Strongly" reports the $p$-value from a test of the equality of the Weakly Symbolic and Strongly Symbolic coefficients. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S10: Treatment Effects on Insecurity and Cognitive Performance (Hindu)

| | *Dependent variable:* | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Insecurity index | | Does not feel true Indian | | Feels unsure | | Worries about discrimination | | Feels nervous and stressed | | Cognitive score | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Any treatment | 0.037 | 0.013 | 0.054 | 0.013 | 0.081 | 0.031 | 0.054 | 0.060 | −0.040 | −0.053 | 0.021 | 0.008 |
| | (0.050) | (0.050) | (0.070) | (0.070) | (0.069) | (0.068) | (0.097) | (0.098) | (0.090) | (0.091) | (0.190) | (0.192) |
| Constant | 2.225*** | 3.181*** | 1.728*** | 2.431*** | 1.780*** | 3.202*** | 2.840*** | 3.321*** | 2.553*** | 3.769*** | 13.080*** | 13.557*** |
| | (0.041) | (0.245) | (0.057) | (0.344) | (0.055) | (0.336) | (0.078) | (0.480) | (0.073) | (0.445) | (0.154) | (0.943) |
| Adjusted $R^2$ | -0.0005 | 0.0560 | -0.0005 | 0.0423 | 0.0004 | 0.0404 | -0.0008 | 0.0214 | -0.0009 | 0.0260 | -0.0011 | 0.0133 |
| Observations | 900 | 900 | 900 | 900 | 900 | 900 | 900 | 900 | 900 | 900 | 900 | 900 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |

*Notes*: This table reports OLS estimates of treatment effects for Hindu participants on the insecurity index, its four components, and the cognitive-task score. Odd-numbered columns report the effect of being assigned to any treatment without controls, while even-numbered columns add the full set of pre-treatment controls. Standard errors are reported in parentheses. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S11: Treatment Effects on How Participants Classify the Content

| | Hindu | | Muslim | | Difference |
|---|---|---|---|---|---|
| Content is about... | Control | Treatment $\Delta$ | Control | Treatment $\Delta$ | in $\Delta$ |
| Religion | 0.01 | 0.428*** | 0.01 | 0.439*** | 0.011 |
| | | (0.011) | | (0.011) | (0.016) |
| Politics | 0.21 | 0.372*** | 0.23 | 0.407*** | 0.034** |
| | | (0.012) | | (0.012) | (0.017) |

*Notes*: This table reports how exposure to the treatment tweets changes the way participants classify the content they saw, estimated separately by religion in the online effective sample (Hindu $n = 900$, Muslim $n = 901$). Each participant tagged the topics of the tweets they were shown; the dependent variable is the share of a participant's tweets tagged as relating to religion or to politics. The *Control* columns report mean tag shares in the control group; the *Treatment* $\Delta$ columns report the difference between the pooled treatment group and control, with heteroskedasticity-robust (HC1) standard errors in parentheses. The *Difference in* $\Delta$ column reports the difference in the treatment effect between Muslim and Hindu participants (the interaction of treatment and religion) with its standard error. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S12: Treatment Effects on Number of Correct Transcriptions (Muslim participants, Field Sample)

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Number of correct transcriptions | | | |
| | (1) | (2) | (3) | (4) |
| Any treatment | 0.610 | 0.229 | | |
| | (0.570) | (0.527) | | |
| Weakly symbolic | | | 0.338 | −0.073 |
| | | | (0.669) | (0.622) |
| Strongly symbolic | | | 0.941 | 0.576 |
| | | | (0.711) | (0.651) |
| Constant | 7.278*** | 8.124** | 7.278*** | 7.988** |
| | (0.419) | (3.233) | (0.420) | (3.237) |
| Adjusted $R^2$ | 0.0004 | 0.1905 | -0.0006 | 0.1901 |
| Observations | 393 | 393 | 393 | 393 |
| Controls | No | Yes | No | Yes |
| $H_0$: Weakly = Strongly, p-value | | | 0.437 | 0.364 |

*Notes*: This table reports OLS estimates of treatment effects on the number of correct transcriptions for Muslim participants in the in-person field experiment in West Bengal, restricted to participants with sufficient language proficiency (see Appendix SB). Columns (1) and (2) report the effect of being assigned to any treatment, while columns (3) and (4) report separate effects for the Weakly Symbolic and Strongly Symbolic treatments. Columns (2) and (4) include the full set of pre-treatment controls. Standard errors are reported in parentheses. The row "$H_0$: Weakly = Strongly" reports the $p$-value from a test of the equality of the Weakly Symbolic and Strongly Symbolic coefficients. For the list of control variables used in the regression, see Table A1. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

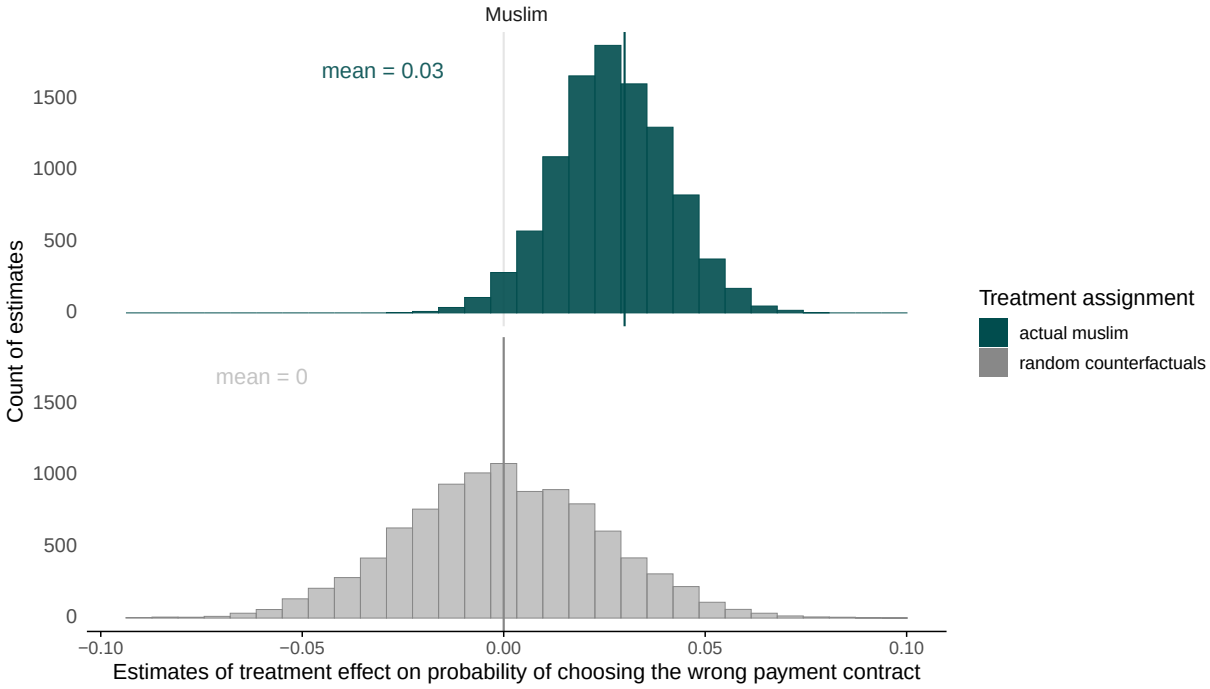



Figure S2: Random Resampling Muslim participants Only

*Note*: This figure repeats the random group-reassignment exercise of Figure 8, but forms the resampled groups from Muslim participants only. In each of 10,000 iterations, participants are randomly reassigned to groups of equal size to the experiment, each participant's contract choice is re-evaluated for correctness within the newly formed groups, and the corresponding treatment effect is estimated. The figure plots the resulting distribution of estimated treatment effects on the probability of choosing the wrong payment contract.

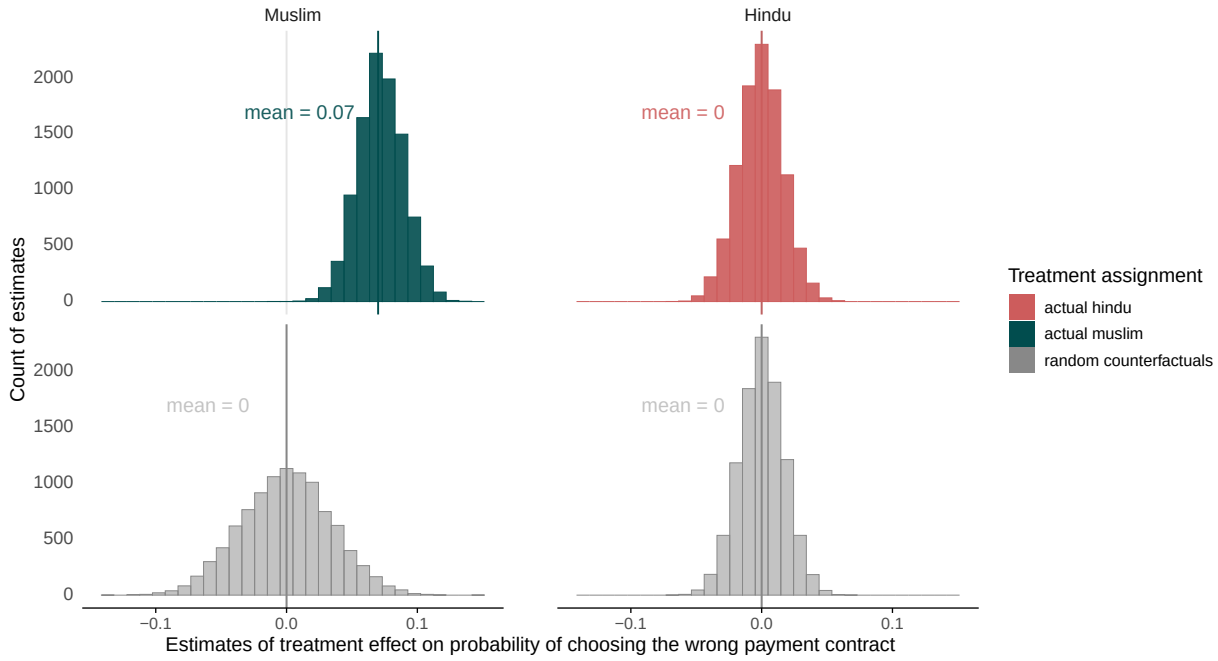



Figure S3: Random Resampling Population Shares

*Note*: This figure repeats the random group-reassignment exercise of Figure 8, but forms the resampled groups using the religious composition of the broader population rather than the balanced 50:50 Muslim–Hindu split of the experimental sample. In each of 10,000 iterations, participants are randomly reassigned to groups, each participant's contract choice is re-evaluated for correctness within the newly formed groups, and the corresponding treatment effect is estimated. The figure plots the resulting distribution of estimated treatment effects on the probability of choosing the wrong payment contract.

# SD Experimental Instructions

## Consent

# Welcome to our project!

We are building a database of Twitter posts, which we will use as part of a future project. In general, we are interested in understanding perceptions of different Twitter posts. We will ask you to examine different posts; complete some timed tasks; and to answer some questions about yourself and your opinions.

We estimate that these evaluations and tasks should take you approximately 30 minutes to complete. On average, participants who complete this study will be compensated with Rs. 450. The minimum that you will earn if you complete the study is Rs 300. The maximum will depend on your performance in the tasks and your choices.

We will strictly protect the confidentiality of research records and identities. Participants will be assigned a random number that will be used to track data produced by the study. Results of the study used in any presentation or publication will be anonymized.

There will be no deception in this study: everything we tell you is accurate to the best of our knowledge and we will provide confirmation where possible. We take this very seriously.

There are no direct benefits to participation beyond the contribution of advancing scientific understanding and the payments you get for participation. We do not anticipate any risks to participation beyond those that one might encounter in everyday life. However, if you have any questions about the study, or if you find the material upsetting, we encourage you to contact the study's principal investigator whose details are provided below. You may also contact the principal investigator if you wish to see the study results, though this information will only be available following the conclusion of the research period.

You have the right to withdraw from this study at any point and for any reason.

Contact details of the Principal Investigator

Dr Joseph Gomes
University of Louvain
joseph.gomes@uclouvain.be

Please acknowledge your consent to participate in this study by checking the “I consent, begin the tasks” option below and clicking the arrow button to begin.

*By clicking the button below, you acknowledge that your participation in the study is voluntary, you are 18 years of age or older, and that you are aware that you may choose to terminate your participation in the study at any time and for any reason.*

Do you consent to be part of this project?

- ○ I consent, begin the tasks
- ○ I do not consent

**Initial demographics**

# Demographic information

What is your age in years?

What is your gender?

- ○ Woman
- ○ Man
- ○ Other

What is your religion?

- Hindu
- Muslim
- Christian
- Sikh
- Buddhist/Neo-Buddhist
- Jain
- Parsi/Zoroastrian
- Jewish
- No Religion
- Other

What is your caste category?

- General/Forward Caste
- Other Backward Caste
- Scheduled Caste
- Scheduled Tribe
- Other
- Don't know
- Don't want to answer

Which state are you currently living in?

What is the highest level of education you have completed?

What is the pre-tax monthly income of your household? Select the amount that comes closest to your household's average pre-tax income over the last year.

On an average day, how many websites that you visit are in English?

- All the websites are in English
- Almost all the websites are in English
- Some websites are in English and some websites are in other languages
- Almost none of the websites are in English
- None of the websites are in English
- I do not visit websites on an average day

On an average day, how many of your in-person, face-to-face conversations are in English?

- All my conversations are in English
- Almost all my conversations are in English
- Some conversations are in English and some conversations are in other languages
- Almost none of my conversation are in English
- None of my conversations are in English

People are very busy these days. We are testing whether people read the survey questions. To show that you've read this much, please select both "very interested" and "extremely interested".

- [ ] Extremely interested
- [ ] Slightly interested
- [ ] Very interested
- [ ] Moderately interested

**Pre-treatment attitudes**

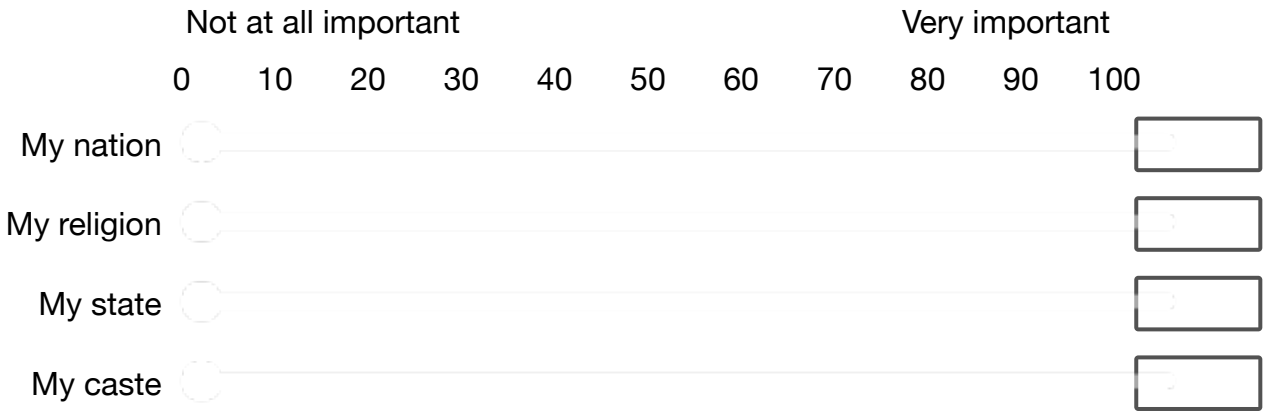
If **parliamentary elections** were held tomorrow, which party's candidate would you vote for?

- Bharatiya Janata Party (BJP)
- Indian National Congress (INC)
- All India Trinamool Congress (AITC)
- Left Front (CPI(M), CPI, RSP and others)
- Samajwadi Party (SP)
- Bahujan Samajbadi Party (BSP)
- Apna Dal (Soneylal) (ADAL)
- Apna Dal (AD)
- Shiv Sena (SHS)
- Nationalist Congress Party (NCP)
- Shiv Sena (Uddhav Balasaheb Thackrey) (SHSUBT)
- Nationalist Congress Party – Sharadchandra Pawar (NCPSP)
- Communist Party of India (Marxist) (CPM)
- Telugu Desam (TDP)
- Telangana Rashtra Samithi (TRS)
- Yuvajana Sramika Rythu Congress Party (YSRCP)
- Janata Party (JnP)
- All India Majlis E Ittehadul Muslimeen (AIMIM)
- Janata Dal (Secular) (JD-S)
- Independent (IND)
- All India United Democratic Front (AIUDF)
- Asom Gana Parishad (AGP)
- United People's Party, Liberal (UPPL)
- Indian Union Muslim League (IUML)
- Revolutionary Socialist Party (RSP)
- Kerala Congress (KEC)
- I would vote for another political party, not one of the ones above
- I would not vote for any political party

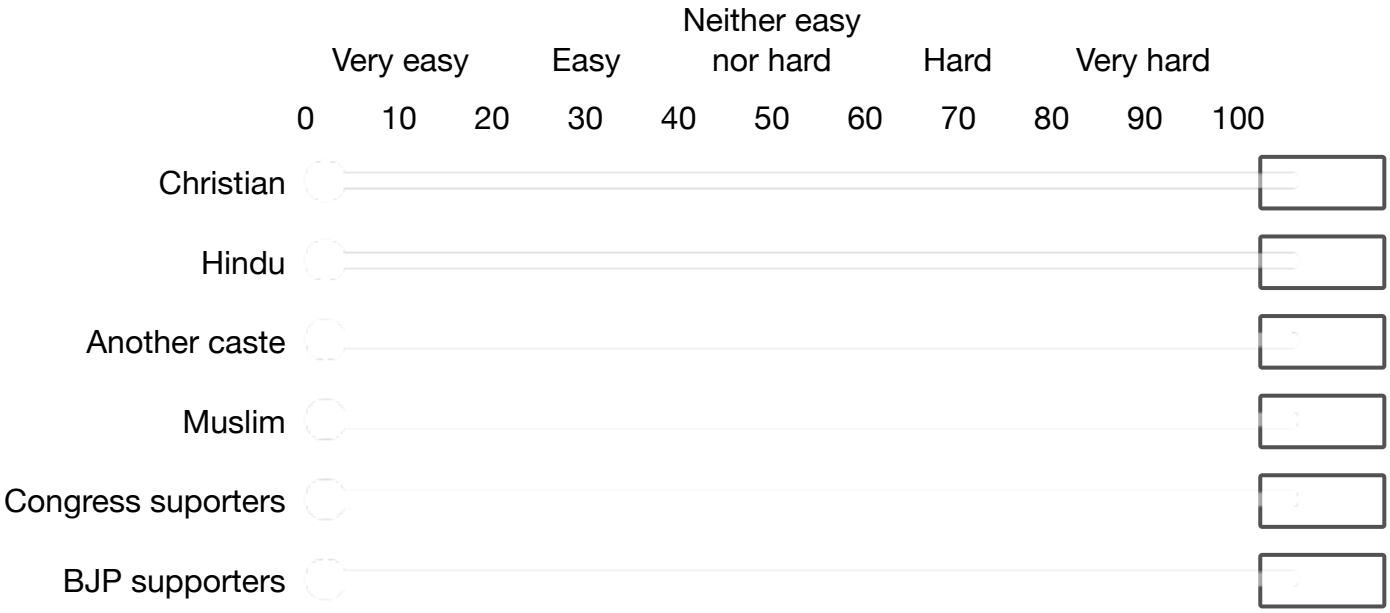
If **state assembly** elections were held tomorrow, which party's candidate would you vote for?

- Bharatiya Janata Party (BJP)
- Indian National Congress (INC)
- All India Trinamool Congress (AITC)
- Left Front (CPI(M), CPI, RSP and others)
- Samajwadi Party (SP)
- Bahujan Samajbadi Party (BSP)
- AKALI DAL - SANT FATEH SINGH GROUP (ADS)
- Suheldev Bharatiya Samaj Party (SBSP)
- Independent (IND)
- Shiv Sena (SHS)
- Nationalist Congress Party (NCP)
- Maharashtra Navnirman Sena (MNS)
- Communist Party of India (Marxist) (CPM)
- All India Forward Bloc (AIFB)
- Revolutionary Socialist Party (RSP)
- Telugu Desam (TDP)
- Telangana Rashtra Samithi (TRS)
- Yuvajana Sramika Rythu Congress Party (YSRCP)
- Praja Rajyam Party (PRAP)
- All India Majlis E Ittehadul Muslimeen (AIMIM)
- Janata Dal (Secular) (JD-S)
- Karnataka Jantha Paksha (KJP)
- Aam Aadmi Party (AAAP)
- Rashtriya Janata Dal (RJD)
- Bhartiya Tribal Party (BTB)
- All India United Democratic Front (AIUDF)
- United People's Party, Liberal (UPPL)
- Indian Union Muslim League (IUML)
- I would vote for another political party, not one of the ones above
- I would not vote for any political party

We all belong to many different social groups. It is normal for some of these groups to be more important to us than others when we think about ourselves and who we are. For each of the groups listed below, please use the slider to record how important the group is for your sense of self (how close you feel to the group) where 0 means "not at all important" and 100 means "very important".




If a new family moved to your neighbourhood, would it be easy or hard for you to get along with them if they were:




**Tweets-instructions**

# Section 1: Evaluating Tweets

In the next pages, you will see 7 Tweets. Please study each Tweet closely and answer

the questions that follow it.

**Tweets-general control-engagement**

1}

Please rate the sentiment of this tweet. To what extent is this a positive or negative news story?

| Extremely negative | | | | Neither positive nor negative | | | | Extremely positive | | |
|---|---|---|---|---|---|---|---|---|---|---|
| -5 | -4 | -3 | -2 | -1 | 0 | 1 | 2 | 3 | 4 | 5 |

How many spelling mistakes are there in the **main tweet**?

Which categories does this tweet belong to? Please check all that apply.

- [ ] Food and/or travel
- [ ] Entertainment
- [ ] Sports
- [ ] Politics
- [ ] Religion
- [ ] Animals and the environment
- [ ] None of the above

**Tweets-control-engagement**

1}

Please rate the sentiment of this tweet. To what extent is this a positive or negative news story?

| Extremely negative | | | | Neither positive nor negative | | | | Extremely positive | | |
|---|---|---|---|---|---|---|---|---|---|---|
| -5 | -4 | -3 | -2 | -1 | 0 | 1 | 2 | 3 | 4 | 5 |

How many spelling mistakes are there in the **main tweet**?

Which categories does this tweet belong to? Please check all that apply.

- [ ] Entertainment
- [ ] Religion
- [ ] Food and/or travel
- [ ] Politics
- [ ] Sports
- [ ] Animals and the environment
- [ ] None of the above

**Tweets-mixed-engagement**

1}

Please rate the sentiment of this tweet. To what extent is this a positive or negative news story?

| Extremely negative | | | | Neither positive nor negative | | | | Extremely positive | | |
|---|---|---|---|---|---|---|---|---|---|---|
| -5 | -4 | -3 | -2 | -1 | 0 | 1 | 2 | 3 | 4 | 5 |

How many spelling mistakes are there in the **main tweet**?

Which categories does this tweet belong to? Please check all that apply.

- [ ] Sports
- [ ] Religion
- [ ] Food and/or travel
- [ ] Entertainment
- [ ] Animals and the environment
- [ ] Politics
- [ ] None of the above

**Tweets-symbolic-engagement**

1}

Please rate the sentiment of this tweet. To what extent is this a positive or negative news story?

| Extremely negative | | | | Neither positive nor negative | | | | Extremely positive | | |
|---|---|---|---|---|---|---|---|---|---|---|
| -5 | -4 | -3 | -2 | -1 | 0 | 1 | 2 | 3 | 4 | 5 |

How many spelling mistakes are there in the **main tweet**?

Which categories does this tweet belong to? Please check all that apply.

- [ ] Politics
- [ ] Animals and the environment
- [ ] Religion
- [ ] Sports
- [ ] Entertainment
- [ ] Food and/or travel
- [ ] None of the above

**Tweets feelings and affect**

The last few Tweets you saw are about a series of proposed policies

- ${e://Field/policy1}
- ${e://Field/policy2}
- ${e://Field/policy3}
- ${e://Field/policy4}
- ${e://Field/policy5}

summary_picture_url}

If any of the policies are implemented next week, how much will it affect the following:

| | Not at all | A little | A moderate amount | A lot | A great deal |
|---|---|---|---|---|---|
| the daily finances of you and your household | ○ | ○ | ○ | ○ | ○ |
| your status and worth in the eyes of other Indian citizens | ○ | ○ | ○ | ○ | ○ |

To what extent do you agree or disagree with the following statements:

| | Strongly disagree | Somewhat disagree | Neither agree nor disagree | Somewhat agree | Strongly agree |
|---|---|---|---|---|---|
| I feel sure about my place in society | ○ | ○ | ○ | ○ | ○ |
| I feel worried about facing discrimination in my day to day life | ○ | ○ | ○ | ○ | ○ |
| I feel nervous and stressed | ○ | ○ | ○ | ○ | ○ |
| I feel as though others see me as a "true Indian" | ○ | ○ | ○ | ○ | ○ |

**Transition to tasks**

# Thank you. You will now complete a series of timed tasks. Please work as quickly and accurately as possible.

**EST instructions**

# Timed Task 1

On each of the following pages, you will see a word. The word will be presented in a colour. Below the word, you will have four options. Your task is to select the name of the colour that corresponds to the displayed colour of the word as quickly as possible. For example, if you see the word apple below, you should select the colour blue as quickly as you can.

# Apple

- ○ apple
- ○ blue
- ○ yellow
- ○ red

**EST neutral**

Please select the colour of the following word

# ${lm://Field/1}

- red
- blue
- yellow
- ${lm://Field/1}

**Est emotional**

Please select the colour of the following word

# ${lm://Field/1}

- yellow
- ${lm://Field/1}
- red
- blue

**Transition EST to Stroop**

Thank you. You will now complete the next timed task.

**Stroop task instructions**

# Timed Task 2

In the following task, you will see a series of digits. Your goal is to select the number of digits you see as quickly as possible. For example, in the question below, the correct answer is '3'

How many digits do you see below? Please select a number between 1 and 9 as quickly as possible.

**5 5 5**

- 1
- 2
- 3
- 4
- 5
- 6
- 7
- 8
- 9

**Stroop task**

How many digits do you see below? Please select a number between 1 and 9 as quickly as possible.

## ${lm://Field/1}

1

2

3

4

5

6

7

8

9

**Transition timed tasks to transcribing**

# Thank you. You will now complete another task involving Tweets. This time you will have the opportunity to earn money based

on your performance.

**Transcription instructions**

# Timed Task 3

In this task, you will be asked to type out **the subtitled text within the circle in the video screenshots**. You will be shown several video screenshots. Please complete as many as you can within **10 minutes**. You will be paid **for each** word correctly transcribed from a **screenshot.** You will receive more details about the payment after completing the task.

Please pay your full attention and attempt to complete as many transcriptions as possible.

Here is an example. Please see the text inside the **yellow circle.** You need to type out the sentence inside that yellow circle correctly, paying attention to all words, spaces, punctuation and capitalization. For practice, please type this sentence in the space provided below (you will not be paid for this practice). Click next and your 10 minutes will begin.



**Transcribing tweets**

1}

**Transcription beliefs**

Thank you for completing the transcriptions. Please tell us how you evaluate your own performance in the task.

How many screenshots do you believe you correctly transcribed in the transcription task?

What do you believe your rank (in terms of the number screenshots you transcribed) is among other people who have taken part in this study?

- Top 1/3rd
- Middle 1/3rd
- Bottom 1/3rd

**Transition to payment**

# Thank you. The following screens will give you instructions to claim your payment.

**Treatment reminder**

Thank you once again for contributing to our research project. As a brief reminder of the work you have done, you saw at least five tweets related to the following policies

- ${e://Field/policy1}
- ${e://Field/policy2}
- ${e://Field/policy3}
- ${e://Field/policy4}
- ${e://Field/policy5}

summary_picture_url}

You completed several timed tasks, including an incentivised transcription task; and you answered a number of survey questions.

Everyone who completed these activities is entitled to a common flat payment. In addition, you will be compensated for whatever you earned in the transcription task.

On the next page, we will describe how you can claim your payment.

**Payment choice**

**Thank you!** You have come to the payment section of the transcription task. Now please pay attention to decide how you want to be paid for correctly transcribed screenshots. For the purpose of the payments, we have grouped you with 11 other similar participants who have also taken part in this study. Therefore you are in a group of 12.

There are **two alternative ways** in which you can be paid. You can decide for yourself which alternative you prefer.

- **Piece-rate method:** in this payment method, you will be paid at the rate of **Rs. 10** for each **correctly transcribed screenshot**, irrespective of how many you got

correct relative to other participants.

- **Winner-take-all method:** in this payment method, you will be paid at the rate of **Rs. 50** for each c**orrectly transcribed screenshot**, but you will be paid only if you are among the top **4** in the group. If you are not among them, you will earn **Rs 0**.

On the next page, we will describe each method in more detail.

**Piece-rate payment method**

In this payment method, you will be paid at the rate of **Rs. 10** for each correctly transcribed screenshot, irrespective of how many such transcriptions you typed correctly relative to others.

Here is an example:

- Suppose you correctly transcribe 3 words in 10 minutes. You will earn **3 * Rs. 10 = Rs. 30** for this task.

- Suppose you correctly transcribe 10 words in 10 minutes. You will earn **10 * Rs. 10= Rs. 100** for this task.

Note: how much others score has **no bearing** on what you earn in the piece rate method.

To see if you have understood, suppose you choose the **piece rate** and you **correctly transcribe 5 words** in 10 minutes and **your rank is 11 out of 12 (i.e second from the bottom)**? How much will you earn?

- 0 * 5 = Rs. 0
- 10 * 5 = Rs. 50
- 20 * 5 = Rs. 100
- 40 * 5 = Rs. 200

That was incorrect. Please try again. Suppose you choose the **piece rate** and you **correctly transcribe 5 words** in 10 minutes and **your rank is 11 out of 12 (i.e second from the bottom)**? How much will you earn?

- 0 * 5 = Rs. 0
- 10 * 5 = Rs. 50
- 20 * 5 = Rs. 100
- 40 * 5 = Rs. 200

To see if you have understood, suppose you choose the **piece rate** and you **correctly transcribe 15 words in 10 minutes** and **your rank is 1st**? How much will you earn?

- 0 * 15 = Rs. 0
- 10 * 15 = Rs. 150
- 50 * 15 = Rs. 750

That was incorrect. Please try again. Suppose you choose the **piece rate** and you c**orrectly transcribe 15 words in 10 minutes** and **your rank is 1st**? How much will you earn?

- 0 * 15 = Rs. 0
- 10 * 15 = Rs. 150
- 50 * 15 = Rs. 750

**Winner-take-all payment method**

In this payment method, you will be paid at the rate of **Rs. 50** for each correctly transcribed screenshot. But you will be paid only if you are among the top **4 participants** in your group of 12.

Here is an example:

Suppose you correctly transcribe 3 words.

- You will earn a large amount i.e., **3 * Rs. 50 = Rs. 150** if you are among the top 4 out of 12.
- You will earn 0 if you are not among the top 4 out of 12.

Suppose you correctly transcribe 10 screenshots.

- You will earn a large amount i.e., 1**0 * Rs 50 = Rs. 500** if you are among the top 4 out of 12.
- You will earn 0 if you are not among the top 4 out of 12.

To check your understanding: Suppose you choose the **winner-take-all** payment method. You **correctly transcribe 5 words** and **your rank is 2 out of 12 (i.e. the second-best performer)**. How much will you earn?

- 0 * 5 = Rs. 0
- 10 * 5 = Rs. 50
- 20 * 5 = Rs. 100
- 50 * 5 = Rs. 250

To check your understanding: Suppose you choose the **winner-take-all** payment method. You **correctly transcribe 5 words** and **your rank is 2 out of 12 (i.e. the second-best performer)**. How much will you earn?

- 0 * 5 = Rs. 0
- 10 * 5 = Rs. 50
- 20 * 5 = Rs. 100
- 50 * 5 = Rs. 250

Now again imagine you are in a **group with 12 people**. You choose the **winner-take-all** payment method. You **correctly transcribe 8 words** and **your rank is 6th**. How much will you earn?

- 0 * 8 = Rs. 0
- 10 * 8 = Rs. 80
- 20 * 8 = Rs. 160
- 50 * 8 = Rs. 400

That was incorrect. Please try again. Imagine again you are in a **group with 12 people**. You choose the **winner-take-all** payment method. You **correctly transcribe 8 words** and **your rank is 6th**. How much will you earn?

- 0 * 8 = Rs. 0
- 10 * 8 = Rs. 80
- 20 * 8 = Rs. 160
- 50 * 8 = Rs. 400

Now, please choose your preferred payment method for the tweet transcription task. The available options are listed below.

When making your choice, consider the rank you believe you achieved based on your score.

My choice of payment is:

- ○ Piece-rate method (Rs. 10 for every correctly typed screenshot)
- ○ Winner-take-all method (Rs. 50 for every correctly typed screenshot if you are among the top 4 participants in your group; Rs. 0 otherwise)

**Debriefing**

# Debriefing

Thank you very much for taking the time to participate. The main tasks are over and we would now like to tell you a bit more about the social media posts that you were asked to evaluate, before ending with a few, short closing questions.

We randomly assigned you to view social media posts about different types of policies. The tweets themselves were constructed by us, but they are all inspired by real tweets. We did not use any fake information and they represent policies that are actually in place in different parts of the country.

It is critical to remember that some of these policies are not universally accepted and may be controversial in nature provoking a lot of discussion and debate throughout the country.

The study you participated in was designed with the goal of learning more about how exposure to these different policies impacts individual cognition and behaviours.

It is our hope and expectation that participants did not find the study's content upsetting, and that there will not be any long-term adverse consequences from exposure to these tweets in our study. We chose to use real policies in part because it strengthened our expectation that the study would not expose participants to any risks beyond those they experience in their everyday lives.

However, if you have any questions about the study, or if you found the material upsetting, we encourage you to contact the study's principal investigator, Joseph F. Gomes, at joseph.gomes@uclouvain.be.

Thank you once again for taking part in this project!

# Closing questions

Would you be happy to participate in a similar study in the future?

- Yes
- No

Do you use any of the following social media platforms?

| | No | Yes |
|---|---|---|
| Twitter (X) | | |
| Facebook | | |
| Instagram | | |
| Whatsapp | | |
| Snapchat | | |
| Other | | |

Do you associate the outlet News Today with any of the following parties?

| | No | Yes | I do not know enough about the link between News Today and this party |
|---|---|---|---|
| Bharatiya Janata Party (BJP) | ○ | ○ | ○ |
| Indian National Congress (INC) | ○ | ○ | ○ |
| All India Trinamool Congress (AITC) | ○ | ○ | ○ |
| Left Front (CPI(M), CPI, RSP and others) | ○ | ○ | ○ |
| Samajwadi Party (SP) | ○ | ○ | ○ |
| Bahujan Samajbadi Party (BSP) | ○ | ○ | ○ |
| AKALI DAL - SANT FATEH SINGH GROUP (ADS) | ○ | ○ | ○ |
| Suheldev Bharatiya Samaj Party (SBSP) | ○ | ○ | ○ |
| Independent (IND) | ○ | ○ | ○ |
| Shiv Sena (SHS) | ○ | ○ | ○ |
| Nationalist Congress Party (NCP) | ○ | ○ | ○ |
| Maharashtra Navnirman Sena (MNS) | ○ | ○ | ○ |
| Communist Party of India (Marxist) (CPM) | ○ | ○ | ○ |
| All India Forward Bloc (AIFB) | ○ | ○ | ○ |
| Revolutionary Socialist Party (RSP) | ○ | ○ | ○ |
| Telugu Desam (TDP) | ○ | ○ | ○ |
| Telangana Rashtra Samithi (TRS) | ○ | ○ | ○ |
| Yuvaiana Sramika | | | |

| | | | |
|---|---|---|---|
| Rythu Congress Party (YSRCP) | ○ | ○ | ○ |
| Praja Rajyam Party (PRAP) | ○ | ○ | ○ |
| All India Majlis E Ittehadul Muslimeen (AIMIM) | ○ | ○ | ○ |
| Janata Dal (Secular) (JD-S) | ○ | ○ | ○ |
| Karnataka Jantha Paksha (KJP) | ○ | ○ | ○ |
| Aam Aadmi Party (AAAP) | ○ | ○ | ○ |
| Rashtriya Janata Dal (RJD) | ○ | ○ | ○ |
| Bhartiya Tribal Party (BTB) | ○ | ○ | ○ |
| All India United Democratic Front (AIUDF) | ○ | ○ | ○ |
| United People's Party, Liberal (UPPL) | ○ | ○ | ○ |
| Indian Union Muslim League (IUML) | ○ | ○ | ○ |
| I would vote for another political party, not one of the ones above | ○ | ○ | ○ |
| I would not vote for any political party | ○ | ○ | ○ |

What do you think this study is about?

In the box below, please let us know if you have any further thoughts about the study:

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