Symbolic Policies Cause Economic Harm: Cognitive and Behavioural Responses to Nationalist Exclusion*

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August 12, 2026

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 a 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 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 marginalization, Behavioural economics, Social psychology, Religious minorities. *JEL Codes*: D91, J15, D83, Z13, J71, P16, Z12.

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1 Introduction

The past decade has witnessed a global revival of right-wing populism (Guriev and Papaioannou, 2022; Rodrik, 2018), a key feature of which has been its stress on ethnonationalism: promoting anti-pluralistic and anti-minority sentiments with a focus on race, ethnicity, and identity (Eichengreen, 2018; Mukand and Rodrik, 2018). As part of this political evolution, we observe the frequent use of exclusionary symbolic policies: expressive measures advanced by the state, whose primary purpose is not the redistribution of material resources, but the signalling of dominant values and identities. In Europe, examples of such policies include restrictions on religious attire in public spaces and place-based civic education initiatives targeting immigrant-concentrated neighbourhoods (Abdelgadir and Fouka, 2020; Foged et al., 2025). In the United States, examples include prohibitions on the use of "divisive concepts" in federal diversity and inclusion training programs, and bans on the teaching of Critical Race Theory in schools (Goldberg, 2023). In Israel, the adoption of the Nation-State Law enshrined Jewish national symbols and downgraded Arabic from its status as an official language (Barak-Corren et al., 2022). In India, the Hindu nationalist government has promoted a Hindu-centric vision of national identity by implementing symbolic policies, such as renaming cities and streets, replacing statues, and selectively rewriting historical narratives, which marginalise the country's sizeable Muslim population (Haas and Lindstam, 2024; Nussbaum, 2007; Jaffrelot, 2021).

Despite the widespread use of such exclusionary symbolic policies, the literature has paid little attention to their potentially damaging effects on ethnic minorities and other groups whose status and belonging may be diminished as a result. We argue that symbolic policies act as powerful signals that shape perceptions of national belonging—implicitly indicating who is considered a full member of the nation and who is not. These signals, we argue, can foster a deep sense of insecurity among marginalised groups, reinforcing social and economic disadvantage through cognitive and behavioural pathways. Specifically, we hypothesize that exclusionary symbolic policies—by undermining minorities' feelings of belonging—can increase cognitive load and harm economic decision-making.

Working memory is finite. When it fills with worry, preoccupation, or unresolved threat, the bandwidth left for economic reasoning shrinks and people make worse decisions (Sweller, 1988;

Kahneman, 2011). Under experimentally induced cognitive load, individuals become more risk averse, more impatient, and more prone to costly errors; higher decision complexity amplifies these distortions (Deck and Jahedi, 2015; Gabaix and Graeber, 2024). The effects run outside the lab. Workers in rural India who receive wages early raise output by 7 percent and make fewer unintentional mistakes in the days that follow, with the gain concentrated among the poorest workers (Kaur et al., 2025). The mechanism is not income but attention: money in hand frees cognitive resources for the task at hand. Dean et al. (2017) show that poverty taxes four distinct components of cognitive function, namely attention, inhibitory control, memory, and higher-order reasoning, each of which shapes economic behaviour directly.

Financial scarcity is not the only source of this drain. Exclusionary policies impose a different kind of load. When minorities are treated as outsiders, the psychological burden of defending one's identity and belonging consumes cognitive resources that would otherwise support careful decision-making (Tajfel and Turner, 1986).[1] This functions like the tunnelling effect in the scarcity literature (Mullainathan and Shafir, 2013): preoccupation with a threat crowds out attention to everything else, including choices that carry real economic stakes.[2] We show that identity threat operates through the same channel. Exposure to exclusionary nationalist policies heightens insecurity about one's social position; that preoccupation occupies cognitive resources and degrades economic decision-making without moving any material variable at all.

We conduct an experiment in India to examine the cognitive and economic consequences of exclusionary policies. India offers a compelling context in which to study the individual-level consequences of symbolic exclusionary politics. India, under the Bharatiya Janta Party (BJP), has witnessed a growing alignment between state policy and majoritarian ideology, particularly in relation to its large Muslim minority. Since 2014, a series of controversial policies have signalled a systematic exclusion of Muslims from full national belonging. This shift is mirrored online, where nationalist supporters have actively shaped public discourse and amplified polarising narratives (Fahad and Mustafa, 2025). These developments make India an urgent and revealing site for analysing the effects of ethnonationalist policies. India, however, is not unique. Similar dynamics have emerged globally, from anti-immigrant rhetoric in Europe and the U.S. to exclusionary na-

[1] Fukuyama (2018) argues that identity is intrinsically linked to individual self-worth and dignity.

[2] See Haushofer and Fehr (2014); Ridley et al. (2020); Schilbach et al. (2016) for the impacts of financial scarcity on cognition.

tionalisms 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 all referred to recent policies from the Indian government. We randomly varied whether participants saw tweets about policies that excluded minorities or those that were neutral. We measured participants' productivity in the tasks and mistakes in their economic decision-making. Participants were offered two payment contracts: a fixed contract guaranteeing modest earnings regardless of others' performance, and a tournament contract offering a larger reward to top performers and nothing otherwise. We categorized mistakes in economic decision-making as the likelihood of not choosing the money-maximising contract, with an aim to investigate how such mistakes varied after viewing exclusionary content.[4]

We show that exposure to symbolic exclusionary policies increases the likelihood that minorities choose the wrong contract. Treated participants are more likely to choose contracts that pay them less money than they could have earned with a different choice. This leads to lower earnings in the treatment group, despite no differences in performance on the main tasks. These outcomes appear to be driven by mechanisms consistent with our main hypotheses: treated participants reported significantly reduced feelings of belonging and recognition, and they performed worse on cognitive tasks. Taken together, the findings suggest that symbolic policies can be perceived as threats to minorities' sense of status and worth; such policies can increase cognitive load, thereby increasing decision mistakes and producing immediate and tangible material consequences. Far from being merely expressive, exclusionary policies can cause direct material harm.

We contribute to several strands of the literature, with our starting point in the emerging field of behavioural development, which examines the psychological effects of poverty (see reviews by Ridley et al. (2020); Schilbach et al. (2016); Haushofer and Fehr (2014)). This literature argues that poverty-related constraints lead to economic behaviours that perpetuate poverty. For example, Haushofer and Fehr (2014) highlight how poverty induces stress and negative emotional

[3]The study was conducted prior to Twitter's acquisition and subsequent rebranding as X.

[4]Note that we meant to hold high ethical standards and focused exclusively on real policies rather than hateful messages. Please refer to Appendix A for a more detailed discussion of the ethical aspects of the project.

states, which in turn lead to short-sighted and risk-averse decision-making by limiting attention and favouring habitual behaviours over goal-directed ones. Similarly, Mani et al. (2013) show that financial uncertainty reduces cognitive performance among the poor by consuming mental resources (see also Carvalho et al. (2016); Lichand and Mani (2020)). More generally, a growing body of research has demonstrated the negative effects of financial concerns on psychological well-being—such as happiness, sadness, worry, stress, or depression (Mullainathan and Shafir, 2013; Chemin et al., 2013; Haushofer and Shapiro, 2016, 2018; Green et al., 2016)—as well as on 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).

However, the existing literature has focused almost exclusively 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); 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 how identity threats, such as those posed by exclusionary policies, impose similar cognitive burdens as poverty, thereby affecting economic behaviour and decision-making.[5]

Second, our research directly contributes to a burgeoning literature on the political and social consequences of symbolic policies. This work includes studies on the effects of historical narratives (De Juan et al., 2025; Haas and Lindstam, 2024), commemorative status (Rahnama, 2025; Rozenas and Vlasenko, 2022), museum exhibitions (Balcells et al., 2022; Balcells and Voytas, 2025), and place names (Lindstam and Haas, 2026; Villamil and Balcells, 2021) and show that symbolic interventions can shape political attitudes, trigger backlash, and influence political behaviour. In particular, we add to a growing body of work showing that such policies can produce harmful effects for targeted groups even in the absence of direct material consequences, owing to their stigmatizing or exclusionary nature. For example, Abdelgadir and Fouka (2020) find that the 2004 French headscarf ban disrupted Muslim girls' educational trajectories by increasing perceptions of discrimination. Many girls, they show, viewed the law as "forcing 'a choice for Muslims: concede your faith or recede from French society'" (Abdelgadir and Fouka, 2020, 117). Similarly, Foged et al. (2025) examine

[5]Related work has shown that cognitive demands can shape behaviour in other high-stakes settings, such as policing (Dube et al., 2025). While their focus is on improving officer decision-making through training, the underlying insight that cognition under constraint affects professional performance, supports our core mechanism, albeit in a very different context.

Denmark's "ghetto laws," showing that although the policies did not impose material costs, the public labelling of neighbourhoods as "ghettos" was widely perceived as stigmatizing. This, in turn, triggered a cultural backlash among minority residents—reducing their assimilation efforts, the very goal the policy purported to promote.[6] In related work, Baudin et al. (2025) analyse a symbolic shift in France's citizenship law requiring second-generation immigrants to formally declare their willingness to become French citizens as a condition for naturalization. Although purely symbolic in nature, it was perceived as discriminatory and led to a measurable decline in national identification among those affected. Ferlenga (2025) shows that symbolic public displays in the form of Confederate monuments reduced the Black population share in the U.S. South through out-migration, and even today such symbols lower Black respondents' willingness to accept jobs in depicted locations, and Ankori-Karlinsky (2026) finds that Confederate monuments altered racial violence patterns by shaping shared beliefs about social order. Overall, these findings support our broader claim that symbolic policies transmit powerful identity signals with material consequences. Our paper contributes to this literature by identifying a novel channel through which symbolic policies can generate economic harm: by reducing cognitive bandwidth and negatively affecting the quality of decision-making among marginalized individuals.

Third, our insights also complement the findings from the discrimination and stereotype threat literature. While the discrimination literature provides valuable insights into how stereotypes, prejudice, and perceived discrimination shape outcomes for minority groups, our work diverges in crucial ways. Much of the recent research, such as Ang (2021), focuses on the direct psychological and emotional consequences of discrimination—such as trauma, emotional disturbance, or stereotype threat—and how these factors impact economic outcomes and well-being. For instance, research on stereotype threats (Steele and Aronson, 1995; Coffman, 2014) has documented how minorities internalize negative stereotypes, leading to reduced academic or economic performance in environments where their identity is salient. These self-fulfilling prophecies have been shown to affect educational achievement (Carlana, 2019), labour market outcomes (Glover et al., 2017), health care (Alsan and Wanamaker, 2018), and income mobility (Chetty et al., 2020). Relatedly, the literature

[6] Govind et al. (2024) document that the official designation of certain neighbourhoods as "ghettos" in Denmark led to negative income outcomes for residents through compositional responses and behavioural changes, consistent with stigma and identity effects that go beyond the direct policy instruments. Such mechanisms are analogous to the identity-linked performance differences we observe among Hindu subgroups in our sample.

also documents how making identity salient shapes expectations (of students) about fair evaluations — a channel distinct from stereotype threat (Hoff and Pandey, 2014).

Our work moves beyond this established focus on interpersonal discrimination and investigates a relatively unexplored mechanism: the cognitive load imposed by exclusionary policies, and subsequent impacts on decision-making. These policies challenge the national belonging of minority groups, independent of individual acts of explicit discrimination. We capture such feelings of insecure national belonging, in commonplace, naturally occurring settings, namely, exposure to news about government policies widely circulated in the media, while browsing a newspaper during a break or scrolling through a Twitter feed while commuting. This paper suggests that such cursory exposure consumes mental resources, leading to cognitive overload and mental fatigue, even in the absence of direct discrimination. Our work thus highlights how institutionalised identity-based exclusion operates as a cognitive constraint that, similar to the effects of poverty, reduces mental bandwidth and diminishes economic opportunities for minorities. Research on trauma exposure (Mueser and Taub, 2008; McInerney et al., 1992; Yehuda et al., 2004) underscores how emotional and cognitive stressors can impair performance and well-being. Our study builds on these insights by showing that exclusionary policies - both purely symbolic and those with additional material implications - act as persistent stressors, depleting cognitive resources in ways that differ from interpersonal discrimination. This distinction broadens the understanding of identity-related economic costs to include the effects of political exclusion.

Our mechanism also differs from stereotype threat. Stereotype threat operates through anxiety about confirming a negative group stereotype regarding *competence*, and canonically produces performance decrements on the stereotype-relevant task itself. In our setting, the trigger is not a cue about group ability but exposure to *state policies* that signal exclusion from national membership - a threat to belonging and recognition, not to perceived competence. Moreover, the identity dimension at stake differs: stereotype threat concerns a group's position on a vertical hierarchy of ability, whereas our mechanism concerns a horizontal dimension of *national belonging*. The exclusionary policies we study do not impugn Muslim participants' capability - they question whether Muslims are authentically Indian. Consistent with these distinctions, we observe no decline in task productivity; impairment instead spills over into an unrelated economic decision, a pattern consistent with a general reduction in cognitive bandwidth analogous to the effects of financial scarcity

(Mani et al., 2013). Our research thus offers a new perspective on how exclusionary policies, even when symbolic, can reinforce cycles of economic disadvantage for minority communities worldwide.

Emerging research highlights how the increased salience of ethnicity and stronger in-group bias can lead to poorer economic decision-making, such as worsening labour market and educational outcomes (Abdelgadir and Fouka, 2020), and exacerbate the risks of violent conflict by affecting intergroup relations (Blouin and Mukand, 2019). We extend this literature by investigating the causal effects of exclusionary policies that frame minorities as "inferior" or "second-class" citizens, focusing on their impact on cognitive and economic outcomes.

2 Conceptual Framework: Symbolic Policies and Cognitive Load

2.1 What are Symbolic Policies?

Research often distinguishes between the material and symbolic dimensions of public policy (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 – for example 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 behavior.

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).[7] The party's 2014 election manifesto stated that "India shall remain a natural home for persecuted Hindus, and they shall be welcome to seek refuge here", already framing the country in explicitly religious terms.[8] The BJP's period in office has consolidated this framing with the introduction of a range of policies related to religion and national identity. Many of these policies convey messages about who belongs to the Indian nation. Examples include the Citizenship Amendment Act (CAA) whose implementation would give Hindu's Indian citizenship by default while requiring Muslims to go through an onerous documentation process; restrictions on Muslim attire in some states; and renaming of public spaces and the construction of monuments honouring Hindu figures (Nussbaum, 2007; Jaffrelot, 2017; Roy, 2024). Alongside formal policies, a large online community of nationalist supporters has contributed to shaping public discourse. Studies indicate that India accounts for the majority of geolocated anti-Muslim content on Twitter, suggesting that the digital sphere can amplify exclusionary narratives (Butler, 2022).

We classify exclusionary policies in contemporary India into two broad categories, which we term *Symbolic Strong* and *Symbolic Weak* policies. This distinction builds on our definition of

[7] See Ahmed et al. (2025) for recent research on the potential reasons behind the rise of Hindu nationalism in India.

[8] See the final sentence on page 40 of the manifesto available at: https://www.bjp.org/images/pdf_2014/full_manifesto_english_07.04.2014.pdf.

symbolic policies as those that rely primarily on their meaning-making function rather than their immediate material impact. *Symbolic Strong* 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.

Symbolic Weak policies combine symbolic messaging with the potential for direct material consequences. These policies convey exclusionary messages similar to those classified as *Symbolic Strong*, 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.[9] 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.[10][11] 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 *Symbolic Strong* and *Symbolic Weak* policies operate primarily through their symbolic dimension in the short run, while differing in the extent to which they may translate into material consequences.

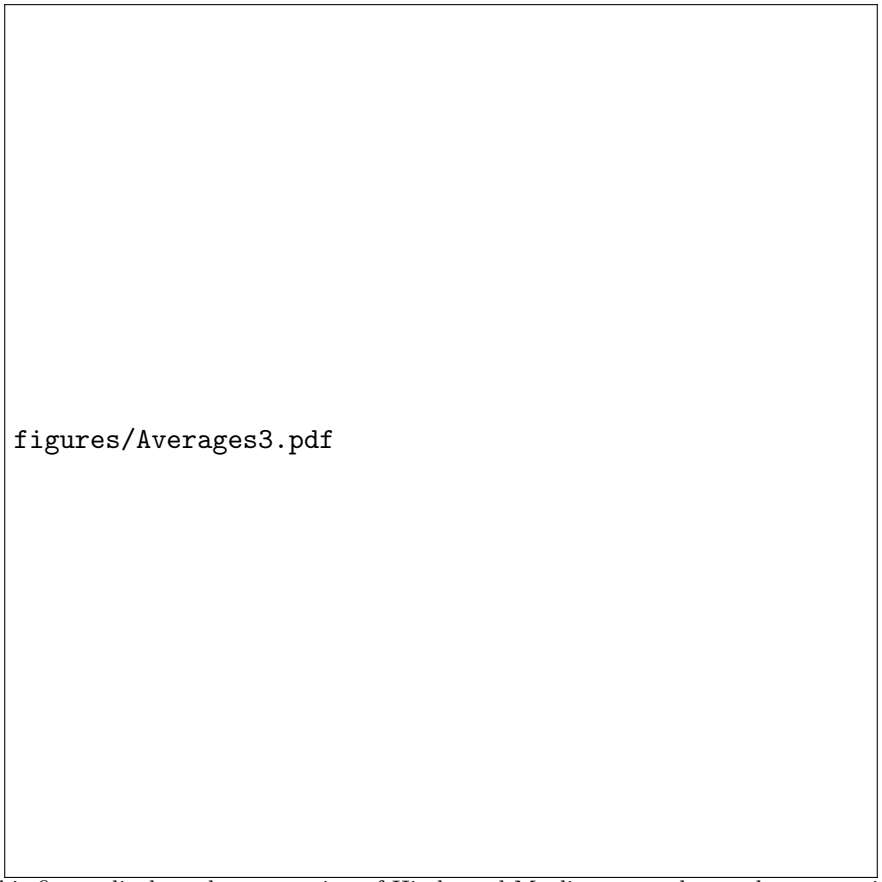
Figure 1 provides a snapshot of identity-related sentiment using representative data from Pew

[9]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 *Symbolic Strong*.

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

[11]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).

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. Representative data from Pew Research Center.

Research Center (N = 29,999).[12] The Figure shows responses to three questions on national belonging and perceived discrimination. Clear differences emerge 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.

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

2.3 How do Symbolic Policies affect Cognitive Load

Symbolically charged policies send signals about who is—and who is not—considered an authentic member of the nation. Exposure to exclusionary policies through news, social media, or other channels could heighten feelings of insecurity among excluded individuals regarding their place in the nation and society, and provoke stress and anxiety. These emotional responses create additional cognitive demands and may reduce the mental resources available for everyday decision-making.

A large body of evidence shows that cognitive load affects how people make decisions, usually for the worse. Cognitive load may increase risk-taking, increase impatience and impulsivity, or may lead to harmful economic decisions such as taking predatory payday loans and causing cycles of indebtedness. A related framework is most explicitly set out in an influential body of work called *scarcity theory.* The key idea described by scarcity theory is that scarcity (typically financial) forces attention towards negotiating tight budgets, leaving fewer mental resources for other decisions. While the evidence base for some elements of *scarcity theory* is still unsettled,[13] replications that show evidence of increased cognitive load and that measure choices in financial or consumer decision tasks [see PNAS review] tend to conclude that scarcity indeed impairs decision making. Combined with many studies that directly manipulate cognitive load, we conclude that existing evidence broadly supports the hypothesis that cognitive load decreases the quality of economic decisions.

An important contribution of our study is to show that when attention is forced towards negotiating and re-negotiating identity concerns, individuals could similarly experience increased cognitive load and make lower-quality economic decisions. As argued above, a crucial function of symbolic policies is to convey messages about meaning, often with a strong emotional resonance. Exclusionary symbolic policies tell members of minority groups that they do not belong to a collective identity. Members of minority groups who value the collective identity may experience a deeply unsettling emotional dissonance. Such a strong emotional response is likely to capture attention automatically and involuntarily. As Bordalo et al. (2016) argue, such attentional capture can "distract us from our goals and distort decisions."

Our study is designed to test this mechanism. In doing so, we make another smaller contribution to the decision-making literature by proposing a novel measure of distorted decisions. Acknowledging that increased feelings of insecurity could change preferences and beliefs as well as

[13] see Haushofer and Salicath; and Antonides etc for reviews

cognitive capacity, we measure distortions in decision-making by offering participants the choice between two contracts and observing whether they choose the contract that leaves them with less money. Throughout the paper, we refer to such a choice as a mistake somewhat in the sense of Kirby et al. who define a choice as a mistake if decision makers would revise their choice when confronted with axioms. This is arguably the case in our setting: participants all complete a relatively simple, standardised task in a short space of time in an online short-term work setting that is very familiar to all of them. In such circumstances, it is unlikely that participants would *want* to leave the study with less money than they could have earned with a different contract choice. We describe the experiment design and contract choice in more detail in section **??** and we return to a discussion of beliefs and preferences after presenting results in 5.3.

3 Experiment Design

3.1 Overview of the experiment

We conducted a 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.

In addition to the experimental tasks, participants completed brief survey modules both before and after the main activities, allowing us to collect demographic data and attitudinal measures.

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 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 field sample in the appendix.

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.[14] 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.[15] 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.[16]

[14] 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.

[15] https://tgmresearch.com/

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

3.3 Treatment variation

Task 1: Treatment through Exclusionary Messages. 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 categorize the topics of the tweets, choosing all applicable categories from a predefined list. The options available to participants include categories such as Religion, Entertainment, Food and/or travel, Sports, Politics, Animals and the environment, or None of the above. Second, they were asked to rate the sentiment of the tweet on a sliding scale from -5 to 5, where -5 represents "Extremely negative," 0 is "Neither positive nor negative," and 5 is "Extremely positive." Finally, they were asked to count the number of spelling mistakes in the main tweet, instructing them to ignore any comments or replies. Participants select their answer from a drop-down menu, which included options such as 0 (no mistakes), 1, 2, 3, or 4 or more. 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 randomized into one of three treatment arms defined as follows: (for the full set, see Figure E9 in the Appendix, See example in Appendix Figure E10, which gives the example of a neutral tweet in Panel (a). Here we enumerate the contents of all the tweets classified by policy type.

1. **Treatment Arm 1: Symbolic Strong 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: Symbolic Weak 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.

   (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.[17]

   (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 references politically neutral, general interest policies, with no reference to national exclusion. The tweets included:

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

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

[17] 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.

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

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

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

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. We made up a news platform that seemed plausible "News Today". We kept that number of re-tweets and likes constant, and we kept the identity and nature of commentators constant across all tweets. Treatment was assigned at the individual level by Qualtrics, after participants had given consent.

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. An example transcription task along with the corresponding instructions is shown in Figure 2.

- **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

Figure 2: **Transcription Task Example**

Notes: The top panel shows the instructions provided to participants for the transcription task, outlining the expectations for measuring productivity and accuracy. The bottom panel provides an example of the transcription task given to participants, which was used to assess productivity and cognitive performance.

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. 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.[18]

- **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—symbolic-strong versus symbolic-weak—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

[18]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).

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, ignoring the word's meaning. The task included both neutral words and emotionally charged words (e.g., terms related to religion or national identity). Response time on emotionally salient items, relative to neutral items, captures attentional interference driven by the emotional salience of the stimuli (see example: **ADD CITATION HERE**). The second was a numerical Stroop task, in which participants saw a series of digits and had to select the count of digits displayed, suppressing the impulse to report the face value of the digits themselves. For both tasks, we measure accuracy and response time. Together, these tasks assess the degree to which treatment content occupied cognitive bandwidth, consistent with the mechanism of cognitive load.

- **Additional questions**: Before participants chose a payment contract, we asked them to estimate their rank in the transcription task relative to all other participants. Participants were shown a scale from 1 (bottom) to 10 (top) and asked to place themselves on it. This allows us 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.[19]

[19]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, answer both "very interested" and "moderately interested." Answer options: Extremely interested, Very interested, Moderately interested, Slightly interested.

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.

- H1 Muslims in the treatment group(s) will transcribe fewer tweets correctly than Muslims in the control group.
- H2 Muslims in the treatment group(s) will make more mistakes in choosing the contract than Muslims in the control group.
- H3 Both the Symbolic Strong and the Symbolic Weak treatments will harm Muslims in the ways described in Hypotheses 1 and 2.

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:

$$y_i = \beta_0 + \beta_1 \cdot \text{SymbolicOnly}_i + \beta_2 \cdot \text{SymbolicPlus}_i + \boldsymbol{\beta_C} \cdot \mathbf{X}_i + \epsilon_i$$

where y_i represents the outcome variable for individual i, including cognitive test scores, contract choice (reflecting economic preferences), and productivity in a transcription task. SymbolicOnly_i is a binary indicator equal to 1 if individual i was exposed to exclusionary messages that are purely symbolic in nature (e.g., cultural erasure without direct legal or financial consequences). SymbolicPlus_i is a binary indicator equal to 1 if the individual was exposed to exclusionary messages with both symbolic and material implications (e.g., citizenship threats under the NRC-CAA

framework). The coefficient β_1 captures the effect of exposure to *Symbolic Only* exclusion, while β_2 captures the effect of *Symbolic Plus* exclusion.

In what follows, we present results with and without controls. All specifications with controls include state fixed effects and a female indicator. In addition, we include a set of individual covariates $\mathbf{X}_i$ selected based on a combination of three criteria: (i) variables that exhibit baseline imbalance across treatment and control groups, (ii) variables that differ significantly between Hindus and Muslims, and (iii) predictors selected via data-driven LASSO variable selection, using the Post-Double Selection (PDS) LASSO procedure of Belloni et al. (2014).[20] The resulting set of controls includes age, years of education, week of survey, indicators for being in the bottom and top terciles of income, caste category (General caste dummy), and a dummy for voting for BJP at either the parliamentary or state level. We also control for a set of identity-related attitudes: feeling close to nation, religion, state, and caste; and reported difficulty getting along with Muslims, Dalits, or BJP supporters. This approach ensures that covariates correlated with treatment assignment or outcomes are accounted for without overfitting or introducing post-treatment bias. ϵ_i is the error term, capturing unobserved factors affecting y_i that are assumed to be uncorrelated with treatment assignment.

This model enables us to isolate the impact of exposure to different types of exclusionary messages on individual outcomes. The coefficients β_1 and β_2 are of particular interest, as they estimate the average treatment effects of *Symbolic Only* and *Symbolic Plus* exclusionary policies, respectively. We estimate this model using Ordinary Least Squares (OLS), separately for Muslim and Hindu participants, to examine heterogeneous effects across groups.

4 Data

Appendix Table B4 reports baseline descriptive statistics for the full sample of the main experiment. 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.The sample covers respondents from 11 major Indian states, with an equal number

[20] This is in line with the recommendations of Cilliers et al. (2022).

of Hindus and Muslims in each state, in line with our recruitment strategy. Appendix Table B3 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.

In Appendix Table B7, we provide summary statistics separately for Hindu and Muslim respondents and compare mean differences across religions. While the two groups 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.

Appendix Table B12 compares the demographic and socioeconomic characteristics of Hindu and Muslim respondents. As expected, the most pronounced difference concerns political preferences: Muslims are substantially less likely to report voting for the BJP or its coalition partners. 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.

Appendix Tables B8 and B9 report baseline covariate balance across the control, Symbolic Plus, and Symbolic Only treatment arms, for the Hindu and the Muslim samples separately. As expected under random assignment, the treatment and control groups appear broadly comparable in terms of key demographic, economic, and political characteristics. Differences in means between treatment and control groups, reported in Columns (4) and (5), are generally small in magnitude and statistically indistinguishable from zero.

Nonetheless, we observe a few noteworthy imbalances in both the Hindu and the Muslim samples. Hindu participants in the Symbolic Plus 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 Symbolic Plus 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. Appendix Tables B10 and B11 replicate the covariate balance analysis pooling the two

treatment arms.

5 Results

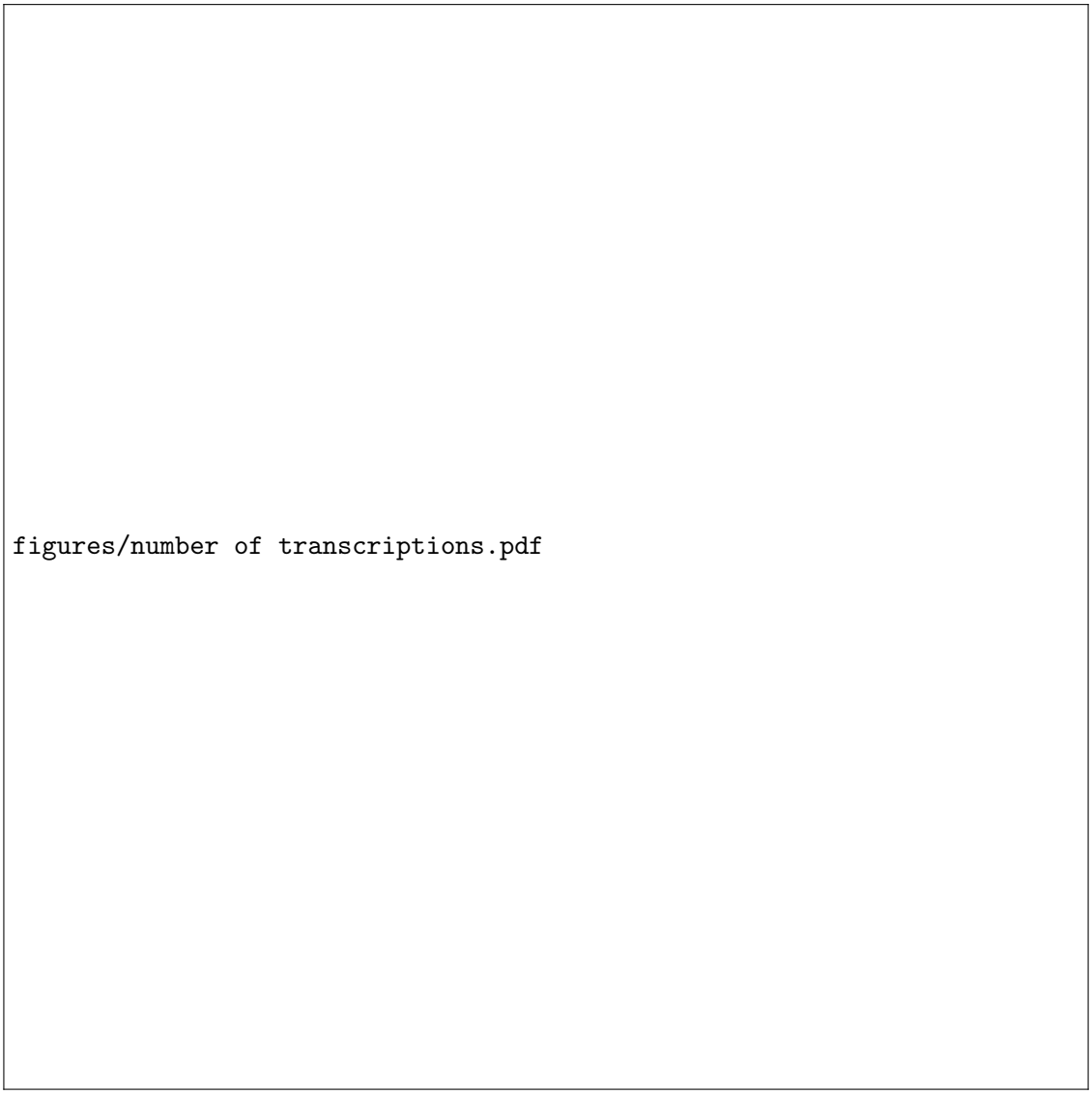
We present the results for Muslim participants in several steps. First, we examine whether the treatments affected our two primary pre-registered outcomes: productivity in the transcription task and the likelihood of making a mistake when selecting a payment contract. We also assess whether exposure to exclusionary policies affected participants' final earnings. Next, we investigate pre-registered mechanisms, examining whether participants perceived the policies as a threat to their social status and self-worth, whether these policies reduced their sense of belonging, and whether such effects, in turn, impaired cognitive bandwidth. Third, we explore alternative and complementary mechanisms by assessing whether the observed pattern of results is more consistent with an increased propensity for mistakes driven by reduced belonging than with changes in beliefs or preferences, such as risk-taking or over- or underconfidence. Fourth, we discuss results for Hindu participants, focusing on how they inform the interpretation of the main findings for Muslims. Finally, we assess robustness by examining alternative treatment assignments and by comparing our main results from the large online sample with those from a smaller field sample collected during project preparation.

5.1 Main outcomes

5.1.1 Productivity

Our first outcome was designed to capture respondents' productivity in completing the incentivized transcription task. We measure productivity as the number of correct transcriptions. We hypothesized that exposure to the exclusionary policy treatments would reduce productivity among Muslim participants. Figure 3 presents the control and treatment averages, where the treatment includes data pooled across policies (left panel), and treatment effects pooled and disaggregated by policy type (right panel). The differences between the treatment and control groups are very small and not statistically significant. Although this finding runs counter to our first hypothesis, it is consistent with how participants ultimately performed the task. The transcription task proved to be relatively simple, generating little variation in outcomes (see Appendix Table D15). And as we

Figure 3: **Effects on Productivity**



Notes: This figure displays treatment effects on the number of correct transcriptions completed by respondents. The left panel shows group means for the pooled treatment (red) and control (gray) groups, with 95% confidence intervals. The right panel present coefficients from linear regression models. 90% and 95% confidence intervals are shown. The outcome ranges from 0 to 33.

show in Table D16, after controlling for the amount of English content participants view on the internet, years of education does not significantly predict the number of correct transcriptions. Our main hypothesis posits that treatment should reduce performance in cognitively demanding tasks. Ultimately, our transcription task turned out not to be cognitively demanding. In light of this, 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 4 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 the correct contract was 0.60 (95% CI [0.54, 0.66]), whereas in the pooled treatment group, this probability declined to 0.52 (95% CI [0.49, 0.56]). This difference of 8 percentage points is statistically significant.

5.1.3 Effects on Earnings

Treatment caused Muslims to earn less overall. We plot mean differences in Appendix Figure D1 and show the full payment distributions in Appendix Figure D2. There is a statistically significant difference in medians (Wilcoxon rank-sum test: W = 94,236.5, p = 0.048) and across the whole distribution (Kolmogorov–Smirnov test: D = 0.103, p = 0.032) although the mean difference is more suggestive (t-test: t-stat = 1.611, p = 0.108). The median participant in the control group earned 100 Rupees while the median participant in the treatment group earned 90 Rupees, a decline of 10% in earnings.

5.2 Main Mechanisms

According to our argument, 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 marginalized individuals and threaten their sense of worth and belonging. We expect that these emotional responses can lead to an increased cognitive burden among marginalized members of society, which could in turn lead to mistakes in economic decision making. In this way, even purely symbolic policies have the potential to harm economic outcomes among marginalized groups.

Figure 4: **Effects on Contract Mistakes**



Notes: This figure displays treatment effects on the probability of selecting the wrong payment contract. The left panel shows group means for the pooled treatment (red) and control (gray) groups, with 95% confidence intervals. The right and panel presents estimates of the treatment effect from linear regression models estimated by OLS with and without controls. 90% and 95% confidence intervals are shown in thin and thick lines. The outcome ranges from 0 to 1.

In this section, we examine whether the exclusionary policy treatments: (1) were perceived as a threat to minorities' sense of status and worth; (2) reduced feelings of belonging and triggered negative emotional responses; and (3) increased cognitive load.

5.2.1 Treatment Increases Perceived Threats to Belonging and Feelings of Insecurity

As a first step, we 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 categorized as material may also be viewed as threats to participants' financial well-being, we expect that both bundles of exclusionary policies – Symbolic Plus and Symbolic Only – would be perceived as threats to respondents' sense of social status and worth. We further hypothesize that this perception of status threat is a key mechanism through which exposure to exclusionary content impairs cognitive bandwidth.

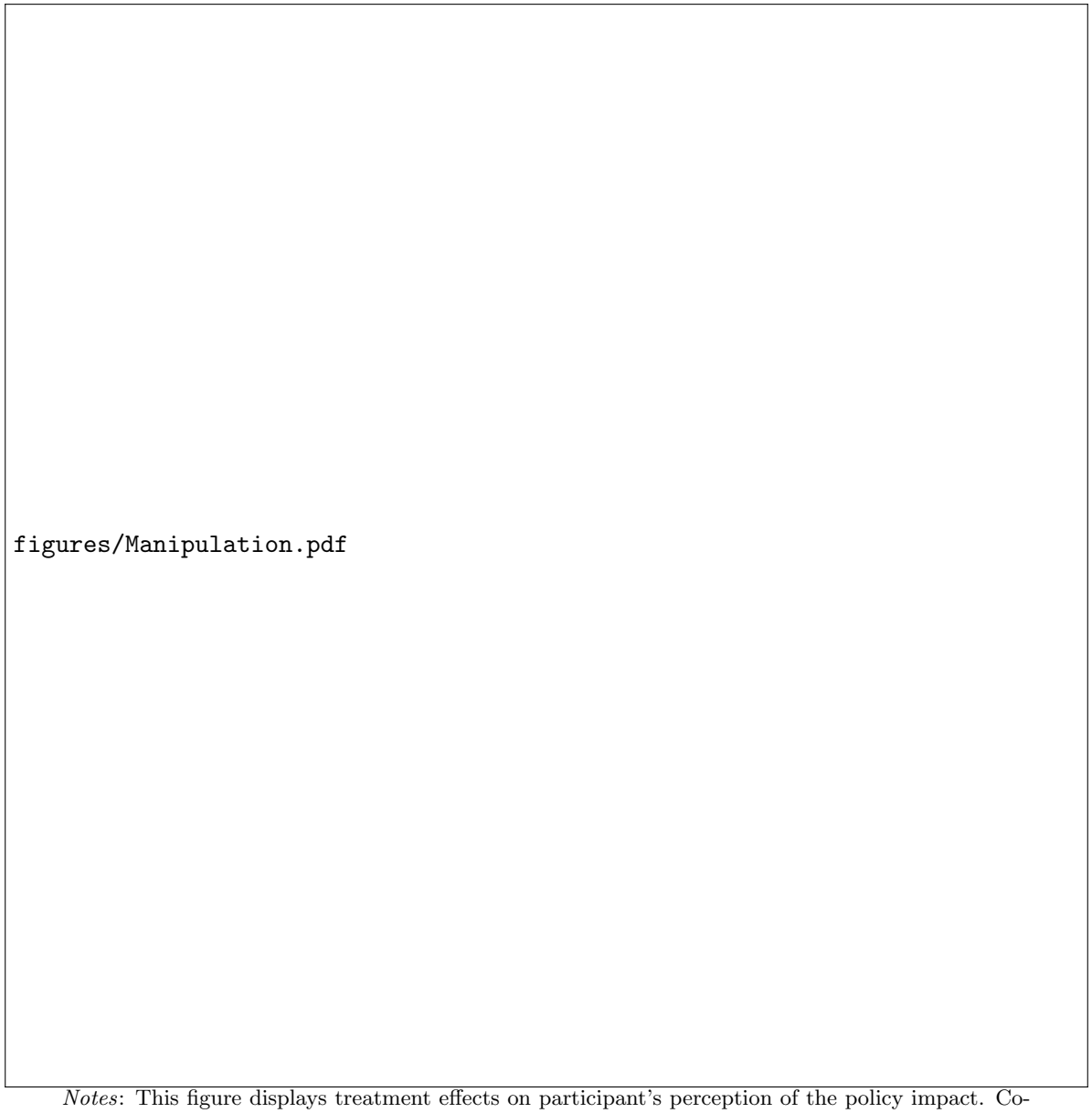
To assess this, participants answered two questions after viewing the five tweets. The first question focused on financial impact: "You just saw five tweets proposing policies. If any of the policies are implemented next week, how much will it affect the daily finances of you and your household?" The second 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?" Responses were recorded on a five-point scale ranging from "Not at all" to "A great deal."

Our results, shown in the right-hand panel of Figure 5, indicate that neither the symbolic plus nor the symbolic only policy treatments significantly affected respondents' perceived financial well-being. This finding supports our expectation that participants experience these policies primarily as symbolic rather than direct, material threats. However, both types of policies did significantly increase participants' perceptions that the policies may affect their social status and worth. These findings suggest that treatment effects are likely driven by concerns related to identity and belonging, rather than by perceived financial consequences.

Secondly, participants were asked to respond to a series of self-reported statements regarding their sense of belonging and emotional state. Specifically, they 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 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 additive index capturing overall feelings of insecure belonging.

Figure 6 displays the results. As expected, Muslim participants assigned to the exclusionary

Figure 5: **Perceptions of Policy Impact**



Notes: This figure displays treatment effects on participant's perception of the policy impact. Coefficients from linear regression models with 90% and 95% confidence intervals are shown. The outcomes range from 1 to 5.

treatment arms reported significantly greater insecurity about their recognition as equal members of the nation and their place in society. Treated Muslims also reported significantly more concern about discrimination and higher levels of stress and anxiety compared to their counterparts in the control group. Both the symbolic plus and symbolic only policy bundles produced similar effects. The average score on the insecurity index among treated Muslims was 2.71 (95% CI [2.65, 2.77]), compared to 2.32 (95% CI [2.23, 2.41]) in the control group.

Figure 6: **Feelings of Belonging**



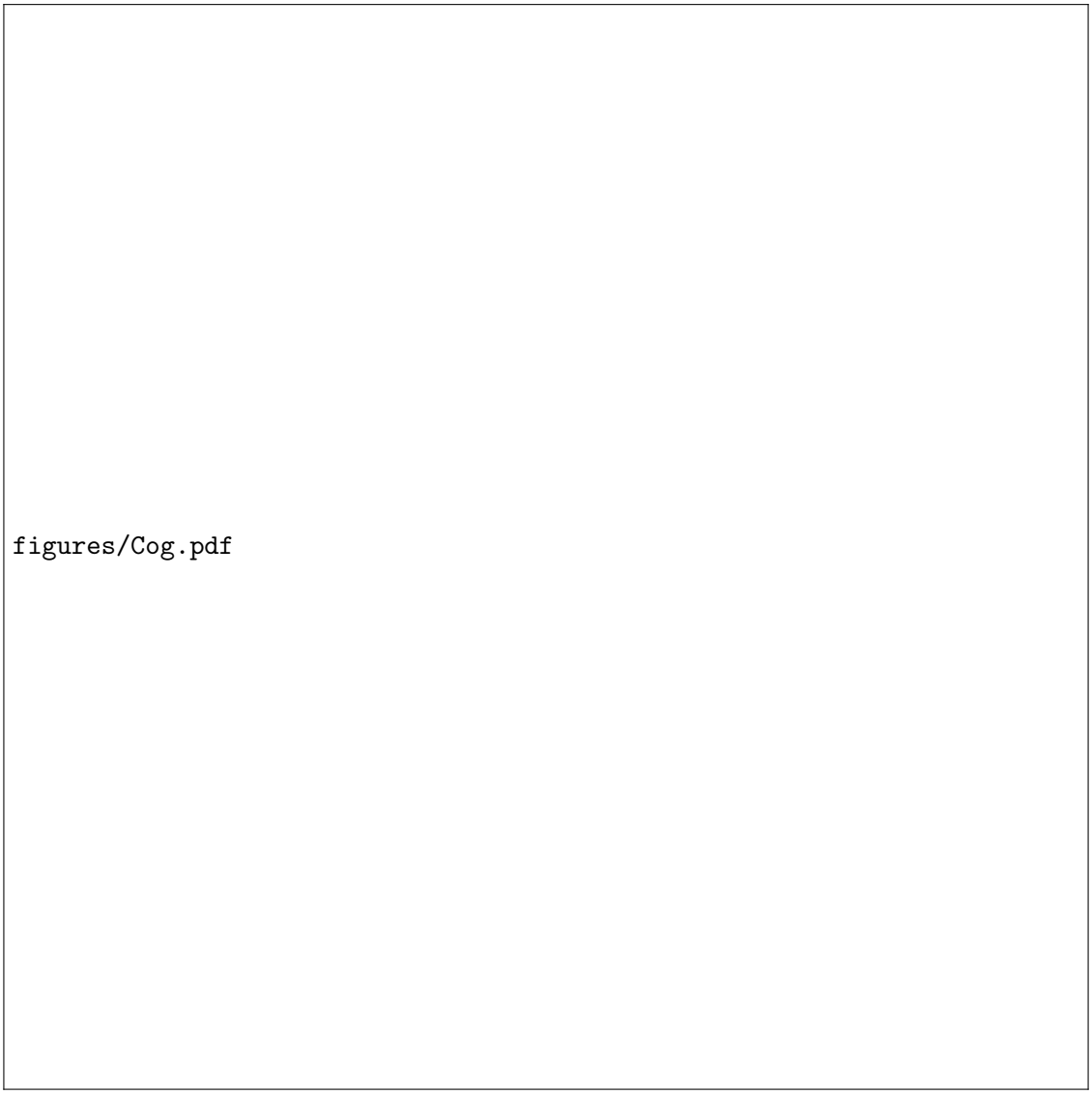
Notes: This figure displays treatment effects on feelings of belonging. Coefficients from linear regression models with 90% and 95% confidence intervals are shown. The outcomes range from 1 to 5.

5.2.2 Treatment Reduces Performance on a Cognitive Test

Finally, 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 7 supports this expectation. Treated Muslim respondents completed fewer tasks correctly, with an average of 16.5 correct answers (95% CI [16.0, 17.2]), compared to 17.5 (95% CI

Figure 7: **Treatment Effect on Cognitive Task**



Notes: This figure displays treatment effects on respondents' performance in a cognitive task. The left panel shows group means for the pooled treatment (red) and control (gray) groups, with 95% confidence intervals. The right and panel present coefficients from linear regression models. 90% and 95% confidence intervals are shown. The outcome ranges from 0 to 20.

[16.9, 18.7]) in the control group. This difference is statistically significant at the 5% level. As shown in the bottom panel, effect sizes are comparable across the two types of symbolic policies.

Figure 8: **Treatment Effects Symbolic only versus Symbolic Plus**



Notes:

5.2.3 Effects Go Through Symbolic Component

In Figure 8, we show that there are not significant differences between Symbolic Plus and Symbolic Mainly, expect where we hypothesised. This suggests our focus on symbolic content is meaningful.

5.3 Alternative mechanisms: beliefs, preferences or mistakes

The main hypothesis of our study is that exposure to exclusionary policies increases feelings of insecurity, which increases cognitive load and leads to decision mistakes. Of course, exposure to

exclusionary policies 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". In Appendix Figures **??** and **??**, 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 D6, 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 D7, we consider an alternative measure of risk-taking, this time by examining whether participants' choices aligned with their beliefs. Risk aversion would suggest that, all else equal, participants who believed they earned the bonus would be less likely to choose the bonus contract compared to participants who believed they earned the piece-rate. We show that participants who believed they earned the bonus are indeed less likely to choose that contract. However, treatment does increase this gap.

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 XX, column YY, 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 suggestive evidence that heightened insecurity is an important mechanisms driving the increase in mistakes.

5.4 Effects on Hindus

We find almost no effects for Hindus. They are not significantly more likely to choose the wrong contract; they do not feel threatened by the treatment and they do not believe that the policies discussed in the treatment will harm them. Interestingly, there is one stark effect: treated Hindus earn less. In contrast to Muslims, this is driven by the fact that some Hindus actually perform worse after treatment. We show this is Figure D8 where we plot the correct earnings, i.e. what participants would have earned if they chose the optimal contract for themselves. We see that this number is significantly lower for Hindus compared to Muslims. We use an Honest Causal Tree (CITE) to explore treatment heterogeneity in this effect (details in the Appendix). We find that the Hindus who reduce their performance are more likely to be close to the BJP, more likely to get along with Hindu versus Muslim neighbours and feel a strong attachment to their nation. This is consistent with an interpretation that a subset of Hindus feeling empowered by the experiment simply withdraw their effort from the study.

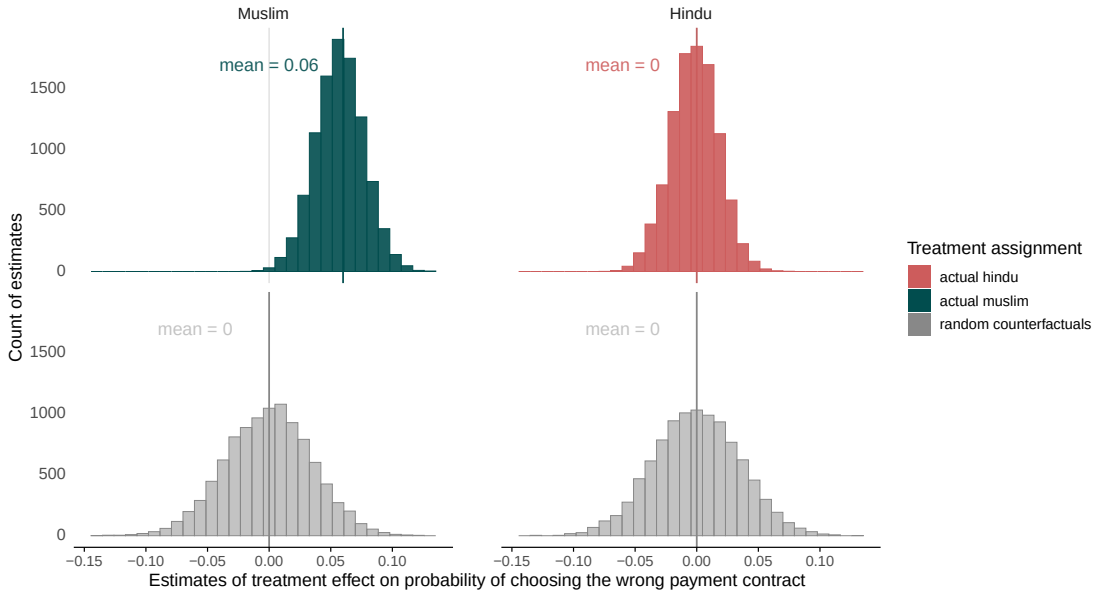
5.5 Robustness

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 generalize to a different setting. As part of the project's preparatory work, we conducted a separate experiment using a similar protocol with a fully field-based 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 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

Figure 9: **Re-randomising payment groups**



reassignment.

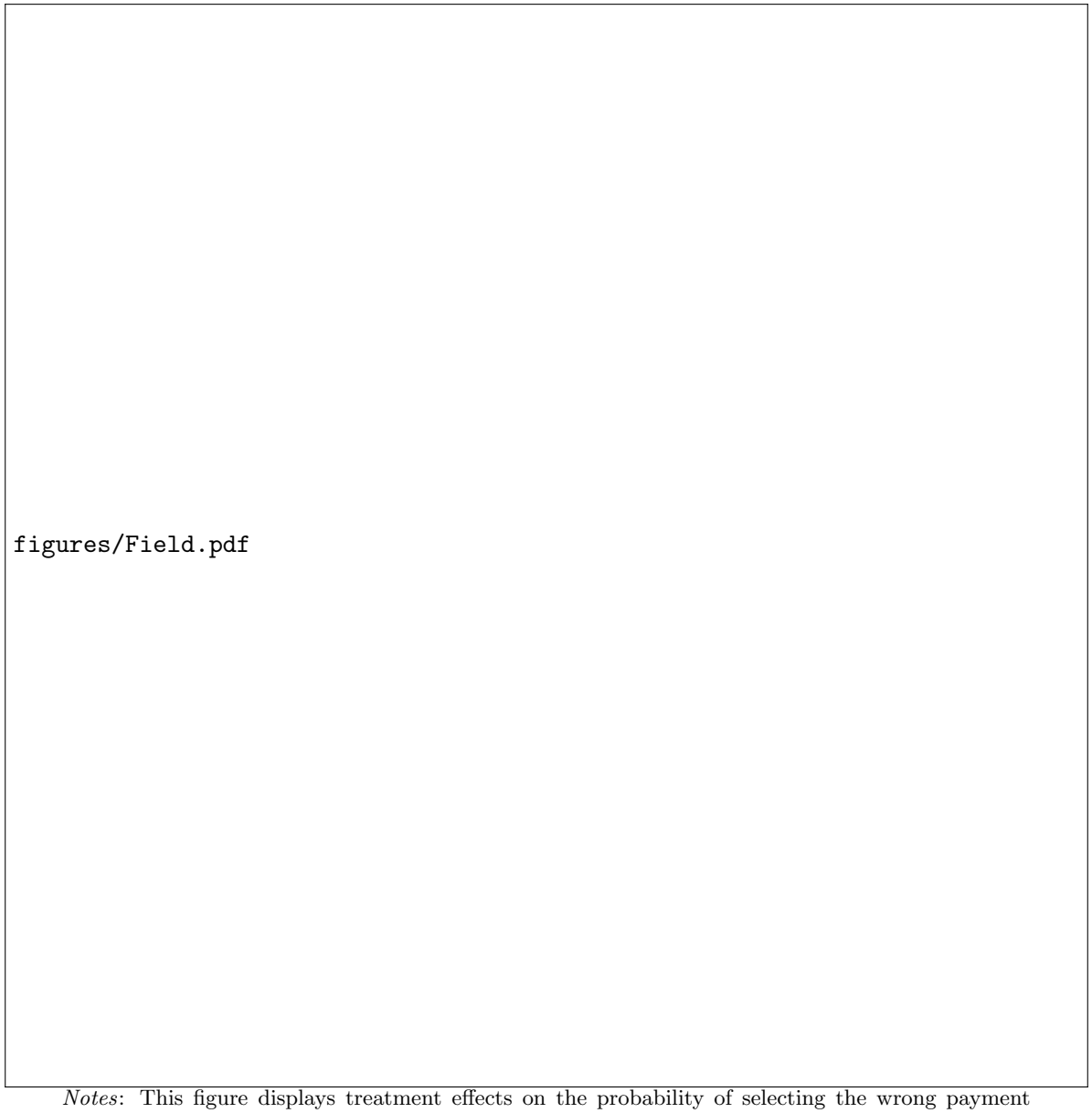
We conduct a parallel exercise in which treatment assignments are randomly reshuffled. This reshuffling generates a sample in which the true treatment effect is zero. We apply the same random group reassignment procedure to this benchmark sample and compare the resulting distribution to that obtained under the actual treatment assignments. As shown in the top panel of Figure 9, even under random group reassignment, the distribution of treatment effects lies almost entirely to the left 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 Field

We also conducted a complementary field experiment in West Bengal, involving approximately 1,000 participants. Due to implementation challenges we do not include these data in our main analysis.[21] 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 10, exposure to ex-

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

Figure 10: **Treatment Effects on Optimal Contract Decision (Field Sample)**



Notes: This figure displays treatment effects on the probability of selecting the wrong payment contract. The left panel shows group means for the pooled treatment (red) and control (gray) groups, with 95% confidence intervals. The right and panel present coefficients from linear regression models. 90% and 95% confidence intervals are shown. The outcome ranges from 0 to 1.

clusionary policies significantly reduced the likelihood of selecting the optimal payment contract, reinforcing the robustness and external validity of our core findings.

6 Discussion and conclusion

Our study provides evidence that exclusionary policies—even when symbolic—can increase cognitive load among exposed minorities, leading to economic mistakes. Participants exposed to these policies were more likely to make mistakes in choosing their payment contracts, resulting in lower earnings. Manipulation checks further revealed diminished feelings of national belonging, underscoring the psychological burden of exclusion.

Notably, symbolic only exclusionary policies, despite lacking immediate material impact, still disrupted cognitive processes and led to tangible economic losses. This highlights the broader risks of exclusionary rhetoric: it not only undermines emotional well-being but also imposes hidden economic costs.

While our study is based in India, its implications extend globally. In France, for example, hijab bans impose significant constraints on Muslim women's autonomy and sense of belonging (Abdelgadir and Fouka, 2020). Similar patterns are evident in Hungary, the United States, and Myanmar, where exclusionary laws and rhetoric marginalise ethnic or religious minorities. Across these contexts, such policies can erode cognitive capacity by forcing individuals to navigate hostile environments, with long-term implications for economic inequality and integration.

As ethnonationalist politics rise worldwide, our findings call attention to the often-overlooked cognitive and economic harm caused by symbolic exclusion. Beyond legal or physical impacts, exclusionary policies tax mental resources, potentially entrenching marginalisation through worsened economic decision making.

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Appendix

A 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.

B Experiment Design Details

B.1 Sample Selection – Online

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 B1). 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.

Table B1: Population Shares from Census 2011

State	**Population (millions)**	**Population share**	**Sample size**
UP	199.8	0.28	570
Maharashtra	112.4	0.16	321
West Bengal	91.3	0.13	260
Andhra Pradesh	49.6	0.07	141
Telengana	35.0	0.05	100
Karnataka	61.1	0.09	174
Uttarakhand	10.1	0.01	29
Delhi	16.8	0.02	48
Gujarat	60.4	0.09	172
Assam	31.2	0.04	89
Kerala	33.4	0.05	95
Total	**701.1**	**1.00**	**2000**

Table B2: State-wise Population and Sample Size

Table B3: Religious Composition of Survey Sample by State

	Sample					Census 2011	
State	Hindu	Muslim	Total	% Hindu	% Muslim	% Hindu	% Muslim
Andhra Pradesh	62	77	139	44.6%	55.4%	88.5%	9.6%
Assam	41	32	73	56.2%	43.8%	61.5%	34.2%
Delhi	21	29	50	42.0%	58.0%	81.7%	12.9%
Gujarat	75	57	132	56.8%	43.2%	88.6%	9.7%
Karnataka	85	108	193	44.0%	56.0%	84.0%	12.9%
Kerala	44	52	96	45.8%	54.2%	54.7%	26.6%
Maharashtra	146	179	325	44.9%	55.1%	79.8%	11.5%
Telangana	51	61	112	45.5%	54.5%	85.1%	12.7%
Uttar Pradesh	250	182	432	57.9%	42.1%	79.7%	19.3%
Uttarakhand	14	13	27	51.9%	48.1%	82.9%	13.9%
West Bengal	111	111	222	50.0%	50.0%	70.5%	27.0%
Total	900	901	1801	50.0%	50.0%	82.3%	17.7%

Notes: Columns (1)–(5) are data collected by us. Columns (6)–(7) provide data from Census 2011 for comparison purposes.

B.2 Summary Stats

Table B4: Summary statistics Full – Sample

Variable	Mean	Std. Dev.	Min.	Max.	N
Age	30.148	9.902	18	74	1801
Female	0.394	0.489	0	1	1801
Muslim	0.5	0.5	0	1	1801
Week	28.631	0.651	28	31	1801
General/Forward Caste	0.602	0.49	0	1	1801
SC/ST/OBC	0.36	0.48	0	1	1801
Years of Education	17.448	2.844	1	20	1801
Voted BJP (Parliament/State)	0.507	0.5	0	1	1801
Votes NDA (Parliament/State)	0.549	0.498	0	1	1801
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.414	0.493	0	1	1801
Income: Rs 50,001–100,000 (Middle tercile)	0.341	0.474	0	1	1801
Income: $>$ Rs 100,000 (Top tercile)	0.245	0.43	0	1	1801
Feels close to Nation	92.916	14.554	0	100	1801
Feels close to Religion	85.324	22.934	0	100	1801
Feels close to State	87.446	18.51	0	100	1801
Feels close to Caste	69.868	32.679	0	100	1801
Hard to get along with Hindus	44.887	36.779	0	100	1695
Hard to get along with Muslims	48.25	36.401	0	100	1705
Hard to get along with Dalits	45.52	31.524	0	100	1689
Hard to get along with Christians	44.839	31.339	0	100	1694
Hard to get along with BJP supporters	51.405	34.746	0	100	1703
Hard to get along with Congress supporters	47.943	32.411	0	100	1708

Notes: This table reports means, standard deviations, minimum, and maximum values for the full sample comprising Hindus and Muslims. The "Feels close to .." and "Hard to get along .." variables range from 0 (not at all) to 100 (very strong/very hard). The sample includes participants who met the criteria for effective online participation.

Table B5: Summary statistics - Hindu Sample

Variable	Mean	Std. Dev.	Min.	Max.	N
Age	30.841	10.574	18	73	900
Female	0.38	0.486	0	1	900
Week	28.416	0.493	28	29	900
General/Forward Caste	0.632	0.482	0	1	900
SC/ST/OBC	0.358	0.48	0	1	900
Years of Education	17.636	2.698	1	20	900
Voted BJP (Parliament/State)	0.783	0.412	0	1	900
Voted NDA (Parliament/State)	0.812	0.391	0	1	900
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.409	0.492	0	1	900
Income: Rs 50,001–100,000 (Middle tercile)	0.333	0.472	0	1	900
Income: $>$ Rs 100,000 (Top tercile)	0.258	0.438	0	1	900
Feels close to Nation	93.549	14.159	0	100	900
Feels close to Religion	81.813	25.108	0	100	900
Feels close to State	87.556	18.461	0	100	900
Feels close to Caste	66.88	32.346	0	100	900
Hard to get along with Hindus	44.683	39.434	0	100	851
Hard to get along with Muslims	51.976	31.816	0	100	865
Hard to get along with Dalits	46.669	30.509	0	100	846
Hard to get along with Christians	45.57	29.671	0	100	854
Hard to get along with BJP supporters	48.204	35.349	0	100	852
Hard to get along with Congress supporters	48.245	30.557	0	100	861

Notes: This table reports means, standard deviations, minimum, and maximum values for the Hindu sample. The "Feels close to .." and "Hard to get along .." variables range from 0 (not at all) to 100 (very strong/very hard). The sample includes participants who met the criteria for effective online participation.

Table B6: Summary statistics – Muslim Sample

Variable	Mean	Std. Dev.	Min.	Max.	N
Age	29.455	9.137	18	74	901
Female	0.408	0.492	0	1	901
Week	28.847	0.716	28	31	901
General/Forward Caste	0.573	0.495	0	1	901
SC/ST/OBC	0.363	0.481	0	1	901
Years of Education	17.26	2.972	1	20	901
Voted BJP (Parliament/State)	0.231	0.422	0	1	901
Voted NDA (Parliament/State)	0.286	0.452	0	1	901
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.42	0.494	0	1	901
Income: Rs 50,001–100,000 (Middle tercile)	0.349	0.477	0	1	901
Income: $>$ Rs 100,000 (Top tercile)	0.232	0.422	0	1	901
Feels close to Nation	92.283	14.919	0	100	901
Feels close to Religion	88.83	19.94	0	100	901
Feels close to State	87.336	18.567	0	100	901
Feels close to Caste	72.852	32.754	0	100	901
Hard to get along with Hindus	45.094	33.915	0	100	844
Hard to get along with Muslims	44.414	40.245	0	100	840
Hard to get along with Dalits	44.368	32.488	0	100	843
Hard to get along with Christians	44.095	32.95	0	100	840
Hard to get along with BJP supporters	54.61	33.85	0	100	851
Hard to get along with Congress supporters	47.635	34.209	0	100	847

Notes: This table reports means, standard deviations, minimum, and maximum values for the Muslim sample. The "Feels close to .." and "Hard to get along .." variables range from 0 (not at all) to 100 (very strong/very hard). The sample includes participants who met the criteria for effective online participation.

Table B7: Summary Statistics by Religion

	Hindu				Muslim			
	mean	sd	min	max	mean	sd	min	max
Age	30.84	10.57	18	73	29.46	9.14	18	74
Female	0.38	0.49	0	1	0.41	0.49	0	1
Muslim	0.00	0.00	0	0	1.00	0.00	1	1
Week	28.42	0.49	28	29	28.85	0.72	28	31
General/Forward Caste	0.63	0.48	0	1	0.57	0.49	0	1
SC/ST/OBC	0.36	0.48	0	1	0.36	0.48	0	1
Years of Education	17.64	2.70	1	20	17.26	2.97	1	20
Voted BJP (Parliament/State)	0.78	0.41	0	1	0.23	0.42	0	1
Voted NDA (Parliament/State)	0.81	0.39	0	1	0.29	0.45	0	1
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.41	0.49	0	1	0.42	0.49	0	1
Income: Rs 50,001–100,000 (Middle tercile)	0.33	0.47	0	1	0.35	0.48	0	1
Income: $>$ Rs 100,000 (Top tercile)	0.26	0.44	0	1	0.23	0.42	0	1
Feels close to Nation	93.55	14.16	0	100	92.28	14.92	0	100
Feels close to Religion	81.81	25.11	0	100	88.83	19.94	0	100
Feels close to State	87.56	18.46	0	100	87.34	18.57	0	100
Feels close to Caste	66.88	32.35	0	100	72.85	32.75	0	100
Hard to get along with Hindus	44.68	39.43	0	100	45.09	33.91	0	100
Hard to get along with Muslims	51.98	31.82	0	100	44.41	40.24	0	100
Hard to get along with Dalits	46.67	30.51	0	100	44.37	32.49	0	100
Hard to get along with Christians	45.57	29.67	0	100	44.10	32.95	0	100
Hard to get along with BJP supporters	48.20	35.35	0	100	54.61	33.85	0	100
Hard to get along with Congress supporters	48.25	30.56	0	100	47.64	34.21	0	100
Observations	900				901			

B.3 Balance Tables

Table B8: **Balance Table - Hindu Sample**

Variable	(1) SPlus	(2) SOnly	(3) Control	(4) (SP - Ctrl)	(5) (SO - Ctrl)
Age	30.584	30.897	31.032	-0.447	-0.135
	(10.929)	(10.616)	(10.216)	(0.857)	(0.848)
Female	0.331	0.381	0.425	-0.094**	-0.043
	(0.471)	(0.487)	(0.495)	(0.039)	(0.040)
Week	28.405	28.409	28.431	-0.026	-0.022
	(0.492)	(0.492)	(0.496)	(0.040)	(0.040)
General/Forward Caste	0.608	0.619	0.668	-0.060	-0.049
	(0.489)	(0.487)	(0.472)	(0.039)	(0.039)
SC/ST/OBC	0.382	0.364	0.329	0.053	0.035
	(0.487)	(0.482)	(0.471)	(0.039)	(0.039)
Years of Education	17.632	17.639	17.636	-0.004	0.003
	(2.739)	(2.720)	(2.647)	(0.218)	(0.218)
Voted BJP (Parliament/State)	0.777	0.787	0.786	-0.009	0.001
	(0.417)	(0.410)	(0.411)	(0.034)	(0.033)
Voted NDA (Parliament/State)	0.811	0.811	0.815	-0.004	-0.004
	(0.392)	(0.392)	(0.389)	(0.032)	(0.032)
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.449	0.399	0.380	0.069*	0.018
	(0.498)	(0.490)	(0.486)	(0.040)	(0.040)
Income: Rs 50,001–100,000 (Middle tercile)	0.307	0.344	0.348	-0.041	-0.005
	(0.462)	(0.476)	(0.477)	(0.038)	(0.039)
Income: $>$ Rs 100,000 (Top tercile)	0.243	0.258	0.272	-0.028	-0.014
	(0.430)	(0.438)	(0.445)	(0.036)	(0.036)
Feels close to Nation	92.544	92.845	95.153	-2.609**	-2.308**
	(15.867)	(15.329)	(10.824)	(1.096)	(1.074)
Feels close to Religion	79.601	80.639	84.997	-5.395***	-4.358**
	(27.365)	(26.041)	(21.493)	(1.988)	(1.938)
Feels close to State	86.311	85.560	90.588	-4.277***	-5.028***
	(20.912)	(19.600)	(14.037)	(1.436)	(1.380)
Feels close to Caste	67.598	63.093	69.722	-2.124	-6.629**
	(32.998)	(33.082)	(30.756)	(2.584)	(2.598)
Hard to get along with Hindus	46.231	46.131	41.872	4.360	4.260
	(39.672)	(39.070)	(39.524)	(3.298)	(3.295)
Hard to get along with Muslims	53.215	50.085	52.582	0.633	-2.497
	(31.757)	(31.020)	(32.628)	(2.669)	(2.645)
Hard to get along with Dalits	49.047	46.627	44.512	4.535*	2.116
	(31.097)	(29.954)	(30.399)	(2.566)	(2.532)
Hard to get along with Christians	47.821	44.101	44.818	3.003	-0.717
	(31.465)	(28.227)	(29.213)	(2.526)	(2.401)
Hard to get along with BJP supporters	51.787	50.051	43.226	8.561***	6.825**
	(35.428)	(35.476)	(34.709)	(2.919)	(2.929)
Hard to get along with Congress supporters	49.761	46.964	47.987	1.775	-1.023
	(30.946)	(29.454)	(31.222)	(2.574)	(2.534)
Observations	296	291	313	609	604

Notes: Columns (1)–(3) report group means for the Symbolic Plus treatment, Symbolic Only treatment, and Control groups, with standard deviations in parentheses. Columns (4) and (5) report differences in means between the Symbolic Plus and Control groups, and the Symbolic Only and Control groups, respectively, with standard errors in parentheses. For a comparison of the combined treatment and control groups, please refer to Appendix Table **??**. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B9: **Balance Table - Muslim Sample**

Variable	(1) SPlus	(2) SOnly	(3) Control	(4) (SP - Ctrl)	(5) (SO - Ctrl)
Age	28.524	29.749	30.153	-1.629**	-0.404
	(9.602)	(9.048)	(8.644)	(0.755)	(0.729)
Female	0.411	0.421	0.391	0.020	0.030
	(0.493)	(0.495)	(0.489)	(0.040)	(0.040)
Week	28.838	28.830	28.875	-0.037	-0.046
	(0.716)	(0.709)	(0.724)	(0.059)	(0.059)
General/Forward Caste	0.560	0.588	0.569	-0.010	0.019
	(0.497)	(0.493)	(0.496)	(0.041)	(0.041)
SC/ST/OBC	0.379	0.341	0.370	0.009	-0.029
	(0.486)	(0.475)	(0.484)	(0.040)	(0.039)
Years of Education	17.117	17.222	17.459	-0.343	-0.237
	(2.935)	(3.009)	(2.969)	(0.243)	(0.246)
Voted BJP (Parliament/State)	0.181	0.232	0.285	-0.103***	-0.053
	(0.386)	(0.422)	(0.452)	(0.035)	(0.036)
Voted NDA (Parliament/State)	0.239	0.280	0.345	-0.106***	-0.065*
	(0.427)	(0.450)	(0.476)	(0.037)	(0.038)
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.443	0.447	0.363	0.080**	0.084**
	(0.498)	(0.498)	(0.482)	(0.040)	(0.040)
Income: Rs 50,001–100,000 (Middle tercile)	0.369	0.334	0.342	0.027	-0.007
	(0.483)	(0.473)	(0.475)	(0.040)	(0.039)
Income: $>$ Rs 100,000 (Top tercile)	0.188	0.219	0.295	-0.108***	-0.077**
	(0.391)	(0.414)	(0.457)	(0.035)	(0.036)
Feels close to Nation	92.275	92.286	92.288	-0.013	-0.002
	(14.900)	(15.005)	(14.899)	(1.228)	(1.231)
Feels close to Religion	87.974	89.244	89.313	-1.339	-0.069
	(20.705)	(19.533)	(19.565)	(1.663)	(1.609)
Feels close to State	85.955	87.151	89.061	-3.106**	-1.909
	(19.718)	(18.407)	(17.326)	(1.535)	(1.473)
Feels close to Caste	71.832	75.154	71.427	0.405	3.727
	(33.611)	(31.088)	(33.565)	(2.769)	(2.657)
Hard to get along with Hindus	45.874	43.020	46.555	-0.681	-3.534
	(34.096)	(33.216)	(34.498)	(2.926)	(2.866)
Hard to get along with Muslims	45.110	42.061	46.345	-1.235	-4.284
	(39.322)	(40.715)	(40.756)	(3.434)	(3.483)
Hard to get along with Dalits	45.569	42.348	45.340	0.230	-2.992
	(32.137)	(32.103)	(33.317)	(2.800)	(2.780)
Hard to get along with Christians	45.829	42.003	44.510	1.320	-2.506
	(32.701)	(32.615)	(33.581)	(2.828)	(2.817)
Hard to get along with BJP supporters	55.437	55.305	52.974	2.463	2.331
	(32.766)	(34.193)	(34.662)	(2.860)	(2.899)
Hard to get along with Congress supporters	46.772	46.466	49.883	-3.111	-3.417
	(33.925)	(34.294)	(34.444)	(2.909)	(2.914)
Observations	309	311	281	590	592

Notes: Columns (1)–(3) report group means for the Symbolic Plus treatment, Symbolic Only treatment, and Control groups, with standard deviations in parentheses. Columns (4) and (5) report differences in means between the Symbolic Plus and Control groups, and the Symbolic Only and Control groups, respectively, with standard errors in parentheses. For a comparison of the combined treatment and control groups, please refer to Appendix Table **??**. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B10: Balance Table (Any Treatment) - Hindu Sample

Variable	(1) Control group	(2) Treatment group	(3) Difference
Age	31.032	30.739	-0.293
	(10.216)	(10.767)	(0.740)
Female	0.425	0.356	-0.069**
	(0.495)	(0.479)	(0.034)
Week	28.431	28.407	-0.024
	(0.496)	(0.492)	(0.035)
General/Forward Caste	0.668	0.613	-0.054
	(0.472)	(0.487)	(0.034)
SC/ST/OBC	0.329	0.373	0.044
	(0.471)	(0.484)	(0.034)
Years of Education	17.636	17.635	-0.000
	(2.647)	(2.727)	(0.189)
Voted BJP (Parliament/State)	0.786	0.782	-0.004
	(0.411)	(0.413)	(0.029)
Voted NDA (Parliament/State)	0.815	0.811	-0.004
	(0.389)	(0.392)	(0.027)
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.380	0.424	0.044
	(0.486)	(0.495)	(0.034)
Income: Rs 50,001–100,000 (Middle tercile)	0.348	0.325	-0.023
	(0.477)	(0.469)	(0.033)
Income: $>$ Rs 100,000 (Top tercile)	0.272	0.250	-0.021
	(0.445)	(0.434)	(0.031)
Feels close to Nation	95.153	92.693	-2.460**
	(10.824)	(15.590)	(0.988)
Feels close to Religion	84.997	80.116	-4.881***
	(21.493)	(26.699)	(1.751)
Feels close to State	90.588	85.939	-4.649***
	(14.037)	(20.259)	(1.283)
Feels close to Caste	69.722	65.365	-4.357*
	(30.756)	(33.088)	(2.260)
Hard to get along with Hindus	41.872	46.182	4.310
	(39.524)	(39.340)	(2.836)
Hard to get along with Muslims	52.582	51.655	-0.926
	(32.628)	(31.403)	(2.276)
Hard to get along with Dalits	44.512	47.848	3.337
	(30.399)	(30.532)	(2.193)
Hard to get along with Christians	44.818	45.971	1.153
	(29.213)	(29.930)	(2.133)
Hard to get along with BJP supporters	43.226	50.924	7.698***
	(34.709)	(35.430)	(2.521)
Hard to get along with Congress supporters	47.987	48.383	0.396
	(31.222)	(30.225)	(2.189)
Observations	313	587	900

Notes: Each row reports means and standard deviations (in parentheses) for the control and treatment groups. Column (3) reports the difference in means and standard errors. For a comparison of the symbolic and material treatments separately to the control group, please refer to Table **??** Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: Balance Table (Any Treatment) - Muslim Sample

Variable	(1) Control group	(2) Treatment group	(3) Difference
Age	30.153	29.139	-1.014
	(8.644)	(9.341)	(0.657)
Female	0.391	0.416	0.025
	(0.489)	(0.493)	(0.035)
Week	28.875	28.834	-0.042
	(0.724)	(0.712)	(0.051)
General/Forward Caste	0.569	0.574	0.005
	(0.496)	(0.495)	(0.036)
SC/ST/OBC	0.370	0.360	-0.010
	(0.484)	(0.480)	(0.035)
Years of Education	17.459	17.169	-0.290
	(2.969)	(2.971)	(0.214)
Voted BJP (Parliament/State)	0.285	0.206	-0.078***
	(0.452)	(0.405)	(0.030)
Voted NDA (Parliament/State)	0.345	0.260	-0.086***
	(0.476)	(0.439)	(0.032)
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.363	0.445	0.082**
	(0.482)	(0.497)	(0.035)
Income: Rs 50,001–100,000 (Middle tercile)	0.342	0.352	0.010
	(0.475)	(0.478)	(0.034)
Income: $>$ Rs 100,000 (Top tercile)	0.295	0.203	-0.092***
	(0.457)	(0.403)	(0.030)
Feels close to Nation	92.288	92.281	-0.008
	(14.899)	(14.941)	(1.074)
Feels close to Religion	89.313	88.611	-0.702
	(19.565)	(20.119)	(1.435)
Feels close to State	89.061	86.555	-2.506*
	(17.326)	(19.066)	(1.333)
Feels close to Caste	71.427	73.498	2.071
	(33.565)	(32.387)	(2.356)
Hard to get along with Hindus	46.555	44.425	-2.130
	(34.498)	(33.653)	(2.516)
Hard to get along with Muslims	46.345	43.573	-2.772
	(40.756)	(40.025)	(3.020)
Hard to get along with Dalits	45.340	43.937	-1.403
	(33.317)	(32.133)	(2.426)
Hard to get along with Christians	44.510	43.906	-0.603
	(33.581)	(32.686)	(2.453)
Hard to get along with BJP supporters	52.974	55.370	2.396
	(34.662)	(33.469)	(2.493)
Hard to get along with Congress supporters	49.883	46.617	-3.265
	(34.444)	(34.082)	(2.537)
Observations	281	620	901

Notes: Each row reports means and standard deviations (in parentheses) for the control and treatment groups. Column (3) reports the difference in means and standard errors. For a comparison of the symbolic and material treatments separately to the control group, please refer to Table **??** Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B12: Balance Table (Hindus vs. Muslims)

Variable	(1) Hindu	(2) Muslim	(3) Muslim - Hindu
Age	30.841	29.455	-1.386***
	(10.574)	(9.137)	(0.466)
Female	0.380	0.408	0.028
	(0.486)	(0.492)	(0.023)
Week	28.416	28.847	0.431***
	(0.493)	(0.716)	(0.029)
General/Forward Caste	0.632	0.573	-0.060***
	(0.482)	(0.495)	(0.023)
SC/ST/OBC	0.358	0.363	0.005
	(0.480)	(0.481)	(0.023)
Years of Education	17.636	17.260	-0.376***
	(2.698)	(2.972)	(0.134)
Voted BJP (Parliament/State)	0.783	0.231	-0.552***
	(0.412)	(0.422)	(0.020)
Voted NDA (Parliament/State)	0.812	0.286	-0.526***
	(0.391)	(0.452)	(0.020)
Income: $\leq$ Rs 50,000 (Bottom tercile)	0.409	0.420	0.011
	(0.492)	(0.494)	(0.023)
Income: Rs 50,001–100,000 (Middle tercile)	0.333	0.349	0.015
	(0.472)	(0.477)	(0.022)
Income: $>$ Rs 100,000 (Top tercile)	0.258	0.232	-0.026
	(0.438)	(0.422)	(0.020)
Feels close to Nation	93.549	92.283	-1.266*
	(14.159)	(14.919)	(0.685)
Feels close to Religion	81.813	88.830	7.017***
	(25.108)	(19.940)	(1.068)
Feels close to State	87.556	87.336	-0.219
	(18.461)	(18.567)	(0.873)
Feels close to Caste	66.880	72.852	5.972***
	(32.346)	(32.754)	(1.534)
Hard to get along with Hindus	44.683	45.094	0.411
	(39.434)	(33.915)	(1.787)
Hard to get along with Muslims	51.976	44.414	-7.561***
	(31.816)	(40.245)	(1.754)
Hard to get along with Dalits	46.669	44.368	-2.301
	(30.509)	(32.488)	(1.534)
Hard to get along with Christians	45.570	44.095	-1.475
	(29.671)	(32.950)	(1.523)
Hard to get along with BJP supporters	48.204	54.610	6.406***
	(35.349)	(33.850)	(1.677)
Hard to get along with Congress supporters	48.245	47.635	-0.610
	(30.557)	(34.209)	(1.569)
Observations	900	901	1,801

C Correlations

Table C13: Cross-correlation table – Muslims

Variables	Age	Female	Week	GC	SC/ST/OBC	Education	VoteBJP	LIncome	MIncome	HIncome	Nation	Religion	State	Caste	HHindu	HMuslim	HDalit	HChristian	HBJP
Female	0.02																		
Week	-0.05	-0.14																	
GC	-0.01	-0.01	0.03																
SC/ST/OBC	-0.00	0.01	-0.01	-0.87															
Education	0.25	0.04	-0.07	-0.08	0.09														
VoteBJP	0.02	0.06	0.07	0.02	-0.03	0.07													
LIncome	-0.17	-0.04	0.02	0.03	-0.08	-0.29	-0.07												
MIncome	0.09	0.00	0.02	0.00	0.05	0.15	0.01	-0.62											
HIncome	0.10	0.04	-0.04	-0.04	0.03	0.17	0.07	-0.47	-0.40										
Nation	0.01	0.05	-0.06	-0.01	0.06	0.12	-0.02	0.00	-0.02	0.02									
Religion	0.01	0.06	-0.03	0.01	-0.02	-0.01	-0.09	0.07	-0.07	-0.01	0.32								
State	0.05	0.11	-0.07	-0.03	0.06	0.13	0.03	-0.02	-0.04	0.07	0.62	0.39							
Caste	0.10	0.14	-0.08	-0.03	0.02	0.03	0.06	0.02	-0.01	-0.01	0.23	0.47	0.39						
HHindu	0.05	0.07	-0.04	-0.06	0.07	-0.02	0.06	0.02	-0.01	-0.02	-0.05	0.08	-0.02	0.13					
HMuslim	0.01	0.00	0.03	-0.01	0.03	-0.01	0.09	-0.00	-0.01	0.01	-0.08	0.01	-0.05	0.12	0.61				
HDalit	0.03	0.09	-0.04	-0.03	0.03	-0.03	0.11	0.04	-0.04	0.01	-0.01	0.07	0.00	0.14	0.82	0.64			
HChristian	0.07	0.07	-0.02	-0.03	0.03	-0.01	0.11	-0.02	0.04	-0.02	-0.04	0.03	-0.04	0.11	0.78	0.62	0.78		
HBJP	0.06	0.07	-0.07	-0.05	0.06	0.03	0.01	-0.02	-0.02	0.05	-0.01	0.10	-0.00	0.07	0.48	0.14	0.42	0.36	
HCongress	0.02	0.02	0.04	-0.03	0.05	-0.00	0.08	0.00	-0.00	-0.00	-0.04	0.02	-0.02	0.11	0.62	0.75	0.67	0.64	0.20

Table C14: Cross-correlation table – Hindus

Variables	Age	Female	Week	GC	SC/ST/OBC	Education	VoteBJP	LIncome	MIncome	HIncome	Nation	Religion	State	Caste	HHindu	HMuslim	HDalit	HChristian	HBJP
Female	0.08																		
Week	0.05	0.03																	
GC	0.18	0.08	0.08																
SC/ST/OBC	-0.18	-0.07	-0.08	-0.98															
Education	0.28	0.11	0.05	0.10	-0.10														
VoteBJP	0.01	0.02	0.06	0.04	-0.02	0.02													
LIncome	-0.15	-0.01	0.01	-0.09	0.09	-0.29	-0.07												
MIncome	0.01	-0.01	-0.05	0.03	-0.02	0.09	0.06	-0.59											
HIncome	0.15	0.03	0.04	0.08	-0.08	0.22	0.02	-0.49	-0.42										
Nation	0.08	0.05	0.01	-0.01	0.02	0.05	0.06	-0.02	-0.00	0.03									
Religion	0.11	0.04	0.07	0.07	-0.06	0.02	0.24	-0.04	0.00	0.04	0.33								
State	0.10	0.11	0.00	-0.05	0.05	0.13	0.07	-0.01	-0.02	0.03	0.53	0.41							
Caste	0.13	0.03	0.08	0.09	-0.09	0.05	0.14	-0.07	0.01	0.07	0.12	0.60	0.30						
HHindu	-0.08	-0.08	0.05	-0.05	0.05	-0.05	0.07	0.01	0.04	-0.06	-0.06	0.07	0.01	0.12					
HMuslim	-0.02	0.00	0.04	0.02	-0.01	-0.02	0.12	-0.04	0.08	-0.04	0.03	0.21	0.03	0.20	0.15				
HDalit	-0.02	0.00	0.09	0.00	0.00	0.01	0.09	-0.07	0.10	-0.03	-0.01	0.17	0.04	0.24	0.59	0.57			
HChristian	-0.01	0.03	0.07	-0.00	0.01	-0.00	0.08	-0.03	0.10	-0.08	-0.01	0.16	0.04	0.20	0.48	0.64	0.74		
HBJP	-0.05	-0.06	0.04	-0.02	0.02	-0.01	-0.00	0.01	0.04	-0.06	-0.06	0.05	-0.00	0.14	0.78	0.16	0.55	0.45	
HCongress	-0.02	0.03	0.06	-0.04	0.04	-0.02	0.10	-0.03	0.07	-0.04	0.03	0.18	0.06	0.20	0.34	0.58	0.61	0.61	0.34

D Additional results

Table D15: Summary of transcription outcome

	treatment	mean	min	q10	q25	q50	q75	q90	max
1	control	11.99	0.00	2.00	7.00	12.00	17.00	22.00	32.00
2	material	11.49	0.00	2.00	6.00	11.00	17.00	20.00	31.00
3	symbolic	11.46	0.00	1.00	6.00	11.00	16.00	22.00	33.00

Table D16: Cross-sectional associations between control variables and our main outcomes

	Dependent variable:	
	paste("n_transcriptions_correct ~", "english_websites +", control_vars_str)	paste("payment_choice_correct ~", "english_websites +", control_vars_str)
	(1)	(2)
english_websitesAlmost all the websites are in English	0.052	−0.026
	(0.517)	(0.037)
english_websitesAlmost none of the websites are in English	−12.943*	0.491
	(7.074)	(0.500)
english_websitesI do not visit websites on an average day	−1.701	0.465
	(4.129)	(0.292)
english_websitesSome websites are in English and some websites are in other languages	−0.791	−0.039
	(0.868)	(0.061)
female	−0.005	0.022
	(0.505)	(0.036)
stateAssam	−0.567	−0.165
	(1.513)	(0.107)
stateDelhi	−0.866	0.019
	(1.607)	(0.114)
stateGujarat	0.479	−0.077
	(1.280)	(0.090)
stateKarnataka	−1.258	−0.032
	(1.083)	(0.076)
stateKerala	−2.194*	0.042
	(1.332)	(0.094)
stateMaharashtra	0.794	−0.042
	(1.010)	(0.071)
stateTelangana	−1.778	0.017
	(1.254)	(0.089)
stateUttar Pradesh	−0.746	−0.074
	(0.987)	(0.070)
stateUttarakhand	−1.085	0.236
	(2.129)	(0.150)
stateWest Bengal	−0.811	−0.107
	(1.114)	(0.079)
age	−0.147***	0.009***
	(0.027)	(0.002)
votes_bjp	−1.327**	0.022
	(0.587)	(0.041)
income_low	0.063	−0.006
	(0.567)	(0.040)
income_high	−0.089	0.020
	(0.636)	(0.045)
sense_of_self_state	−0.030*	0.002
	(0.018)	(0.001)
sense_of_self_nation	0.028	0.001
	(0.020)	(0.001)
sense_of_self_caste	−0.009	−0.001
	(0.009)	(0.001)
sense_of_self_religion	−0.013	−0.0004
	(0.014)	(0.001)
harder_muslim	−0.027***	0.0005
	(0.008)	(0.001)
harder_BJP	−0.004	0.0001
	(0.008)	(0.001)
harder_dalit	0.013	0.00004
	(0.010)	(0.001)
week_of_year29	−0.176	0.098**
	(0.635)	(0.045)
week_of_year30	0.036	0.016
	(0.692)	(0.049)
week_of_year31	12.157*	−0.372
	(7.103)	(0.502)
general_forward_caste	−1.195**	−0.029
	(0.501)	(0.035)
years_of_education	0.137	−0.011*
	(0.086)	(0.006)
Constant	17.969***	0.359**
	(2.486)	(0.176)
Observations	901	901
R^2	0.090	0.053
Adjusted R^2	0.057	0.019
Residual Std. Error (df = 869)	6.980	0.493
F Statistic (df = 31; 869)	2.764***	1.576**
Note:		*p<0.1; **p<0.05; ***p<0.01

Figure D1: Mean earnings for Muslims

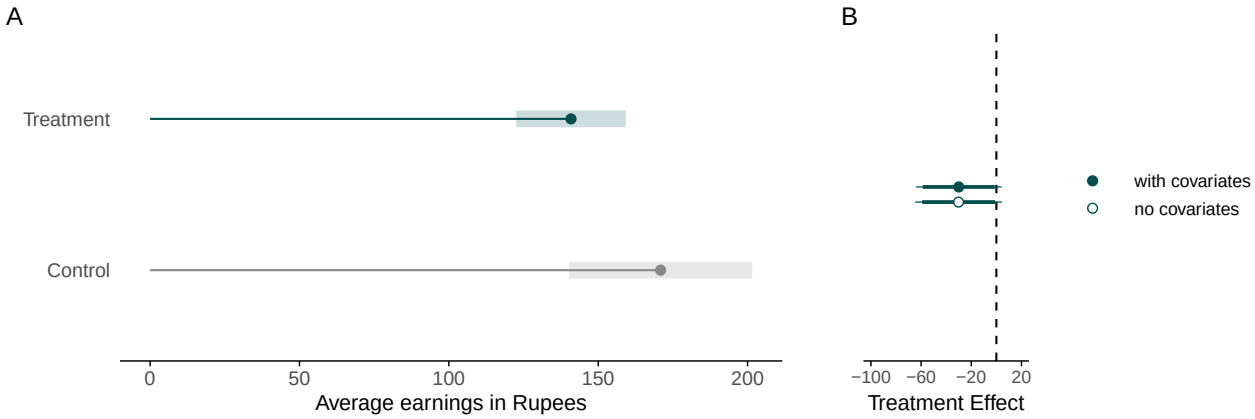


Figure D2: Distribution of earnings for Muslims

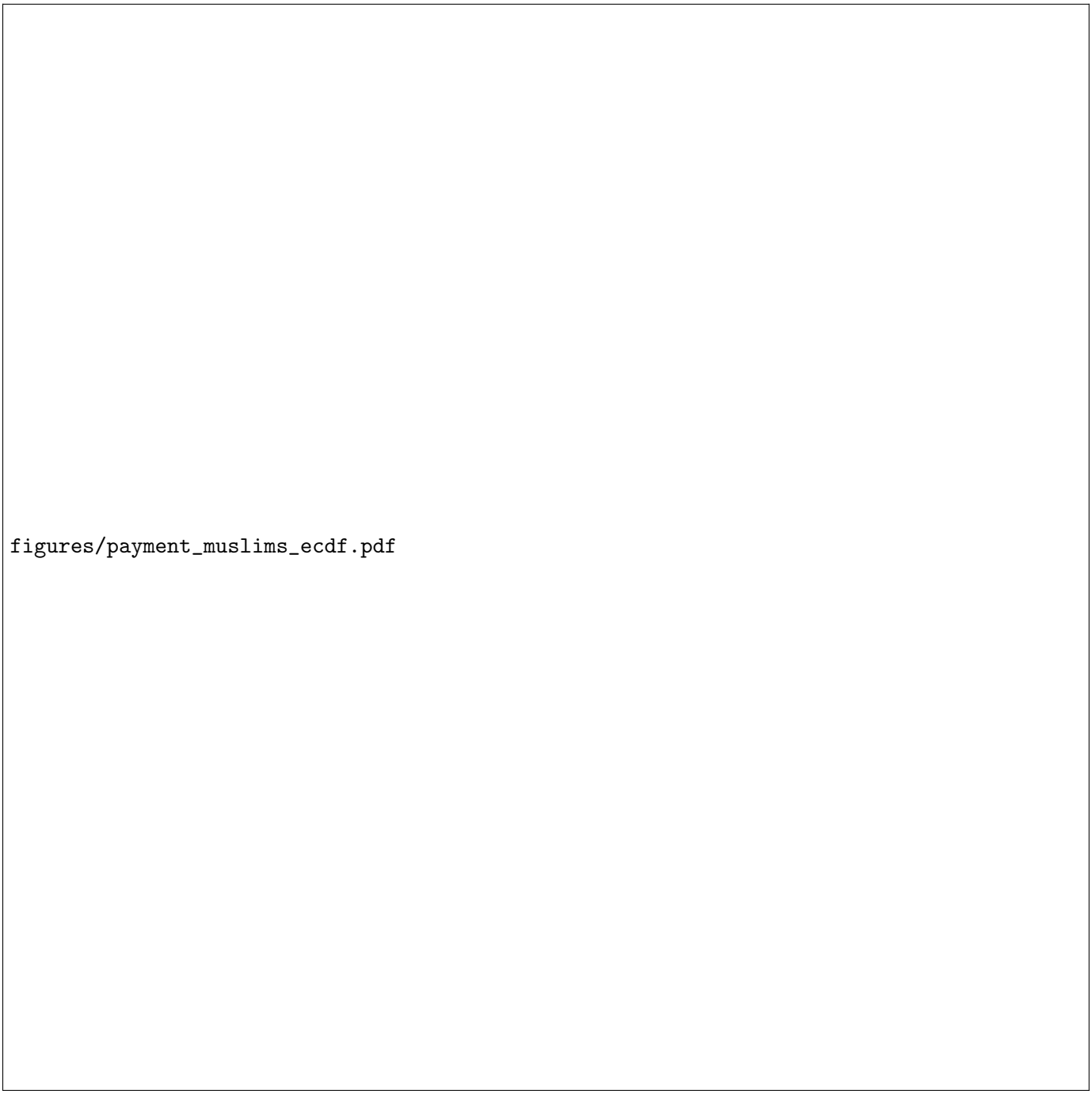


Figure D3: Distribution of earnings for Hindus



??

Figure D4: **Beliefs about performance**

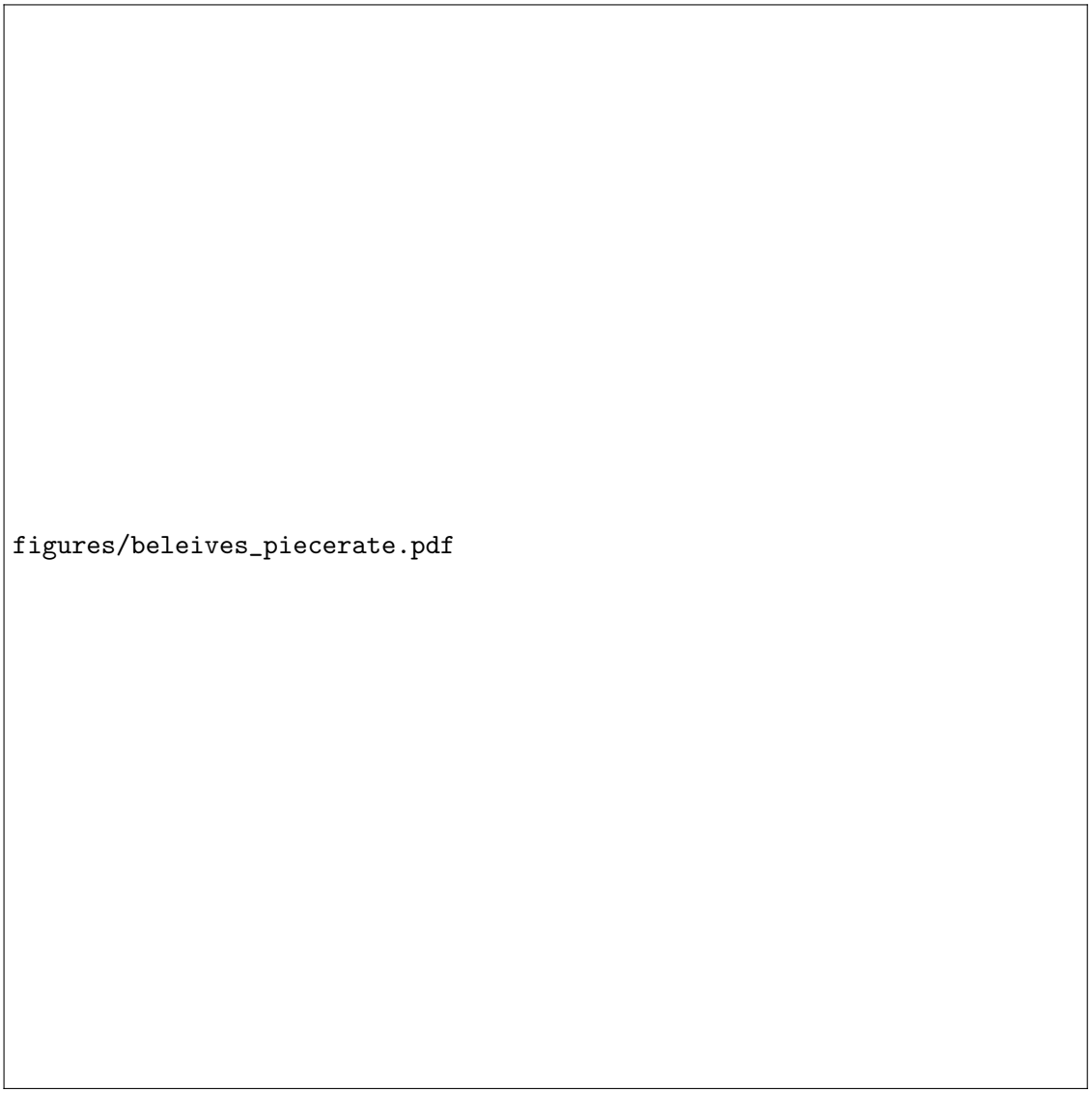


Figure D5: **Accuracy of beliefs**



Figure D6: **Types of mistakes**

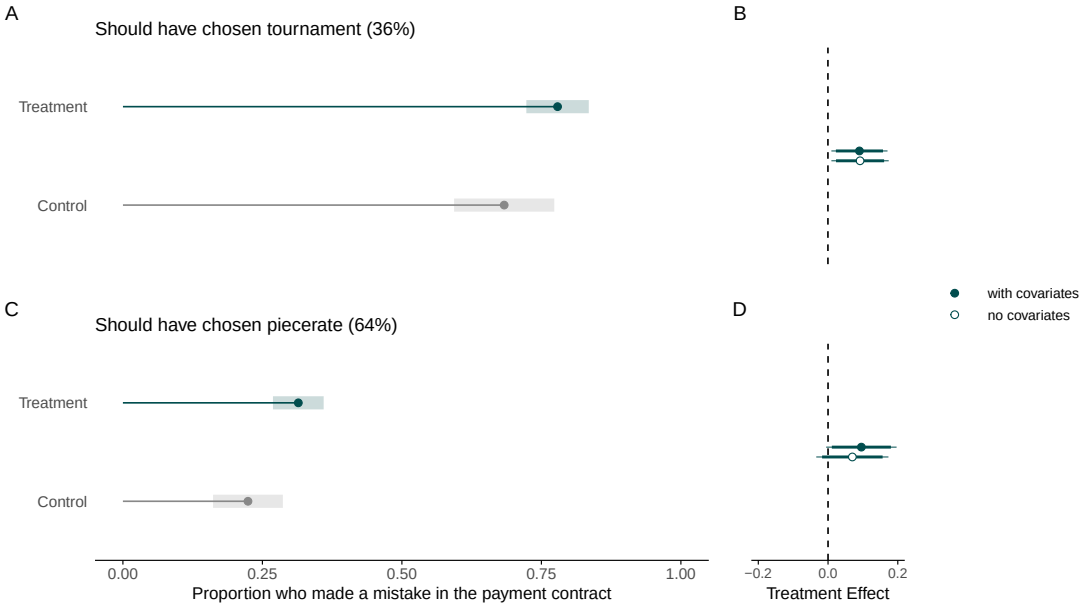
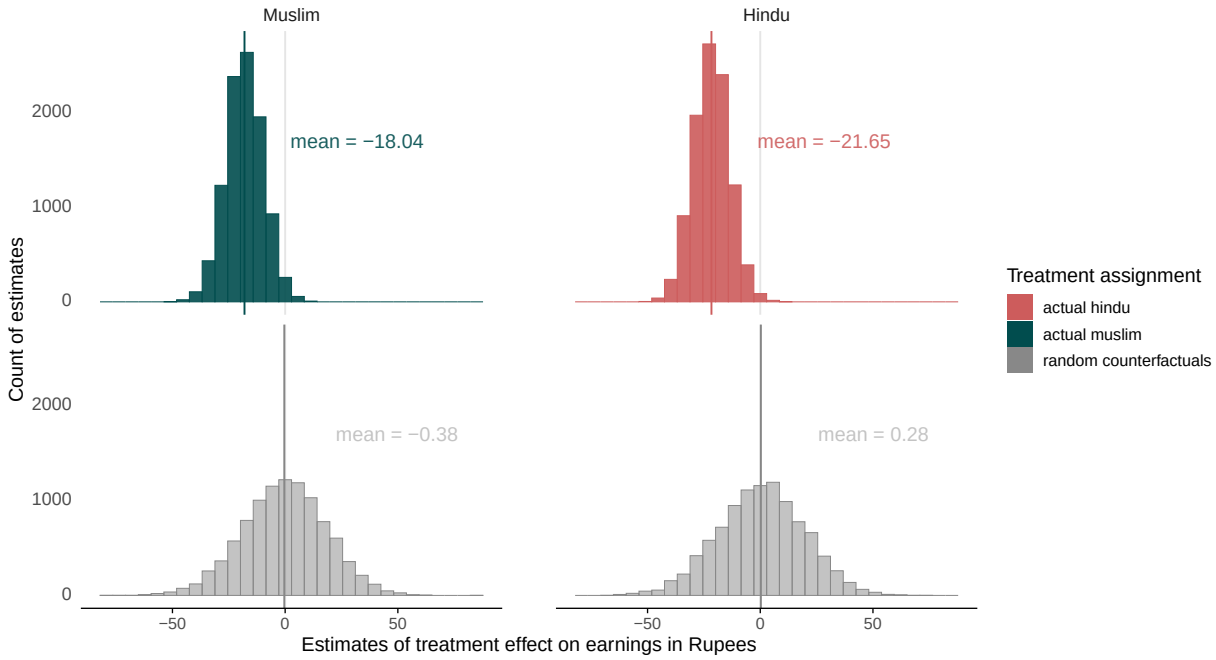
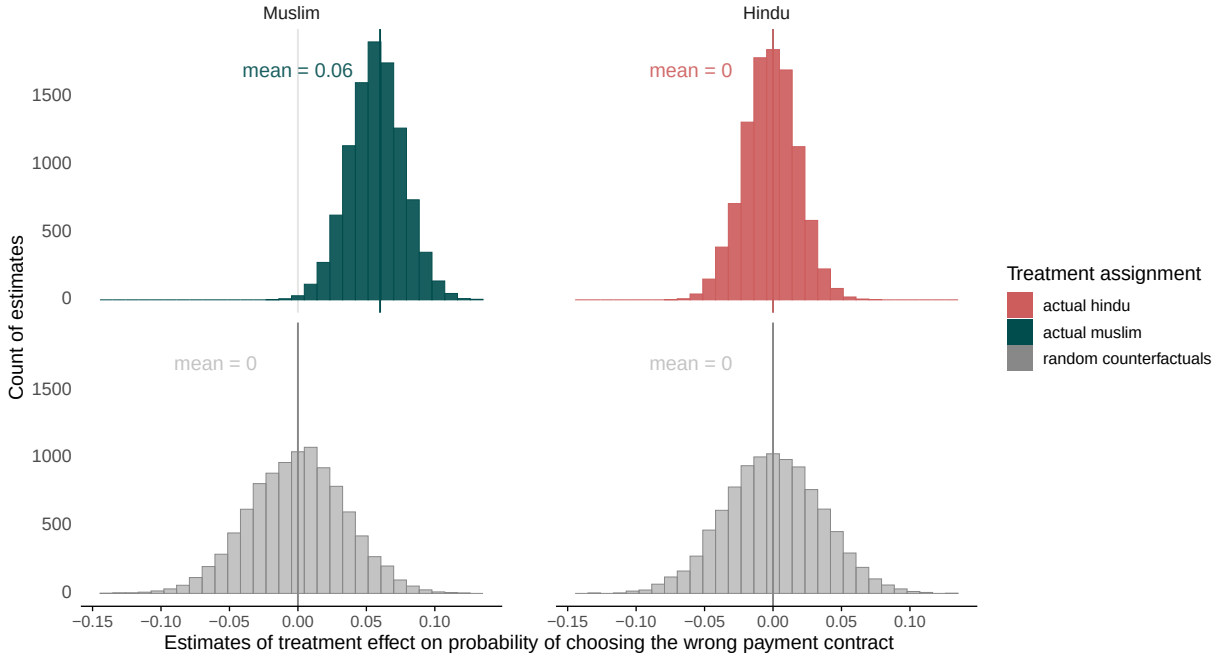
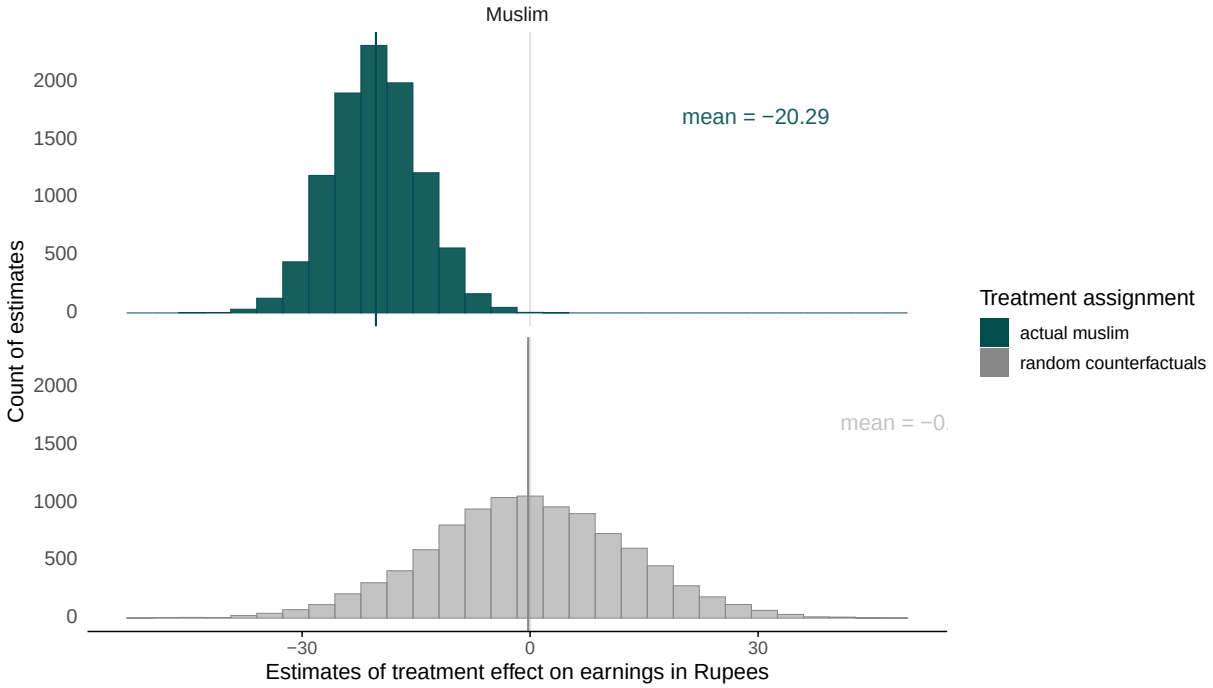


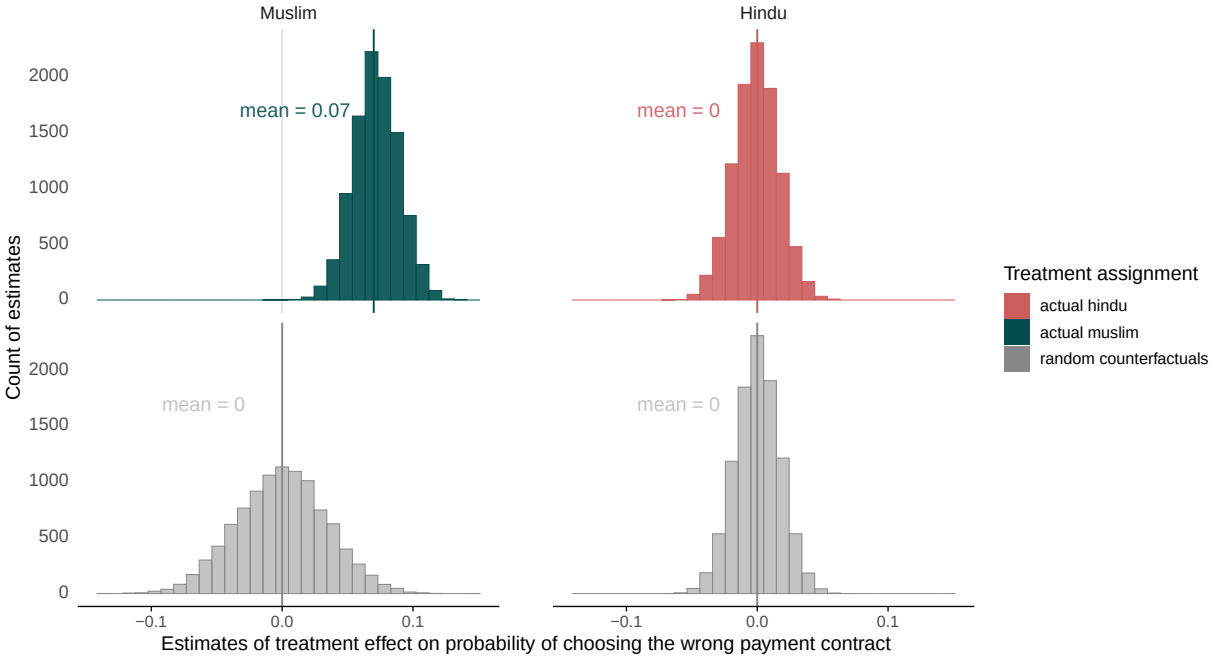
Figure D7: **Do choices align with beliefs?**




Muslim
Hindu
mean = −18.04
mean = −21.65
mean = −0.38
mean = 0.28
Count of estimates
2000
1000
0
−50
0
50
Estimates of treatment effect on earnings in Rupees
Treatment assignment
actual hindu
actual muslim
random counterfactuals


Muslim
Hindu
mean = 0.06
mean = 0
mean = 0
mean = 0
Count of estimates
1500
1000
500
0
−0.15
−0.10
−0.05
0.00
0.05
0.10
Estimates of treatment effect on probability of choosing the wrong payment contract
Treatment assignment
actual hindu
actual muslim
random counterfactuals


Muslim
mean = −20.29
mean = −0.
Count of estimates
2000
1500
1000
500
0
−30
0
30
Estimates of treatment effect on earnings in Rupees
Treatment assignment
actual muslim
random counterfactuals


Muslim
Hindu
mean = 0.07
mean = 0
mean = 0
mean = 0
Count of estimates
2000
1500
1000
500
0
−0.1
0.0
0.1
Estimates of treatment effect on probability of choosing the wrong payment contract
Treatment assignment
actual hindu
actual muslim
random counterfactuals

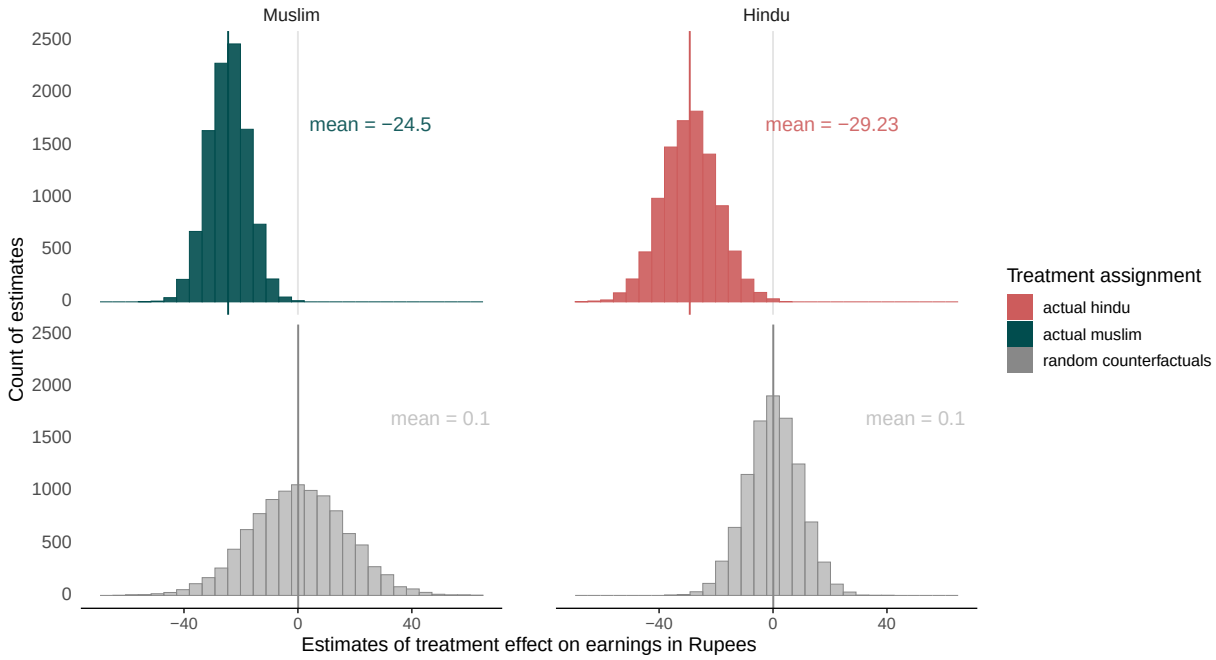
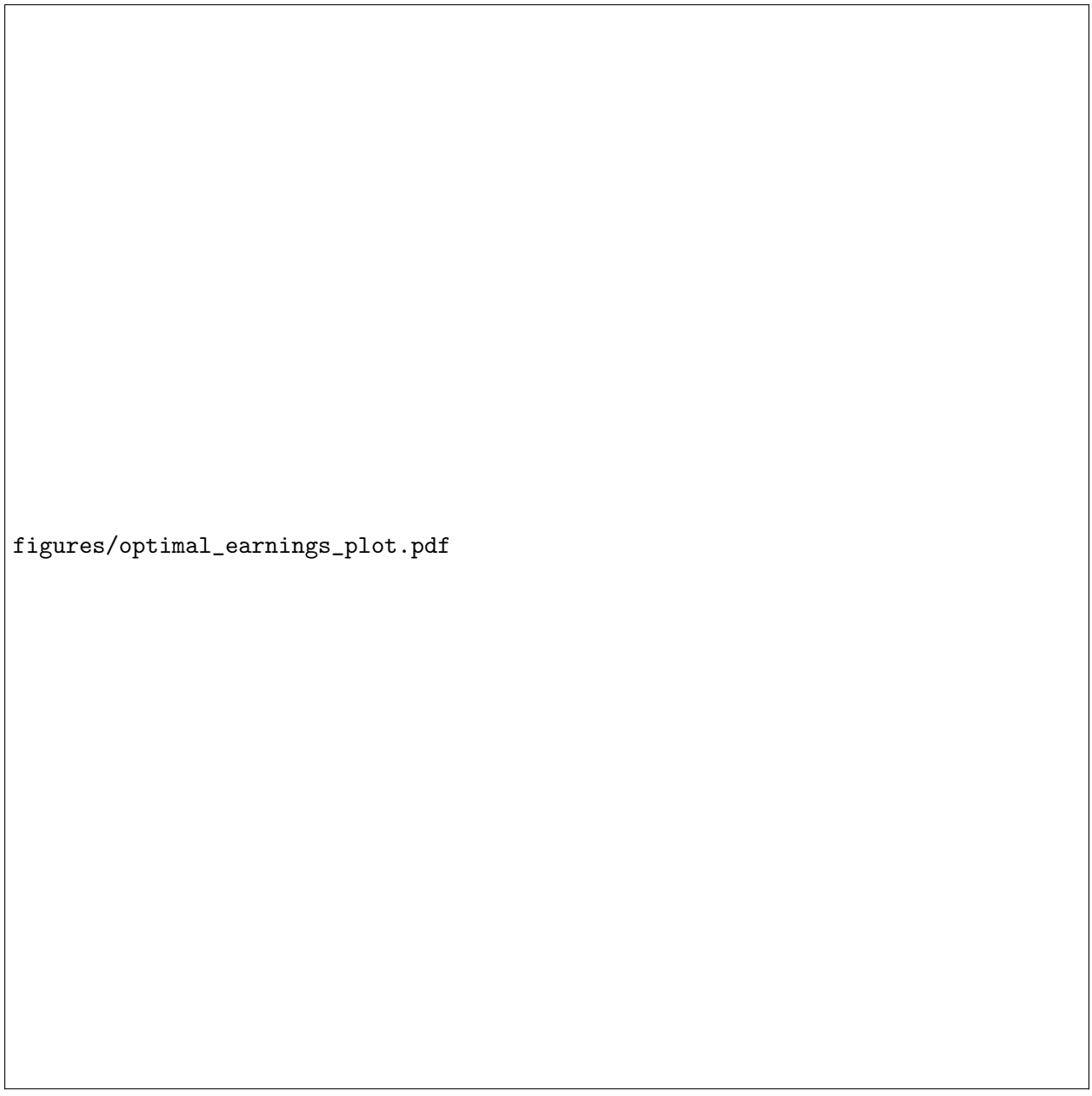
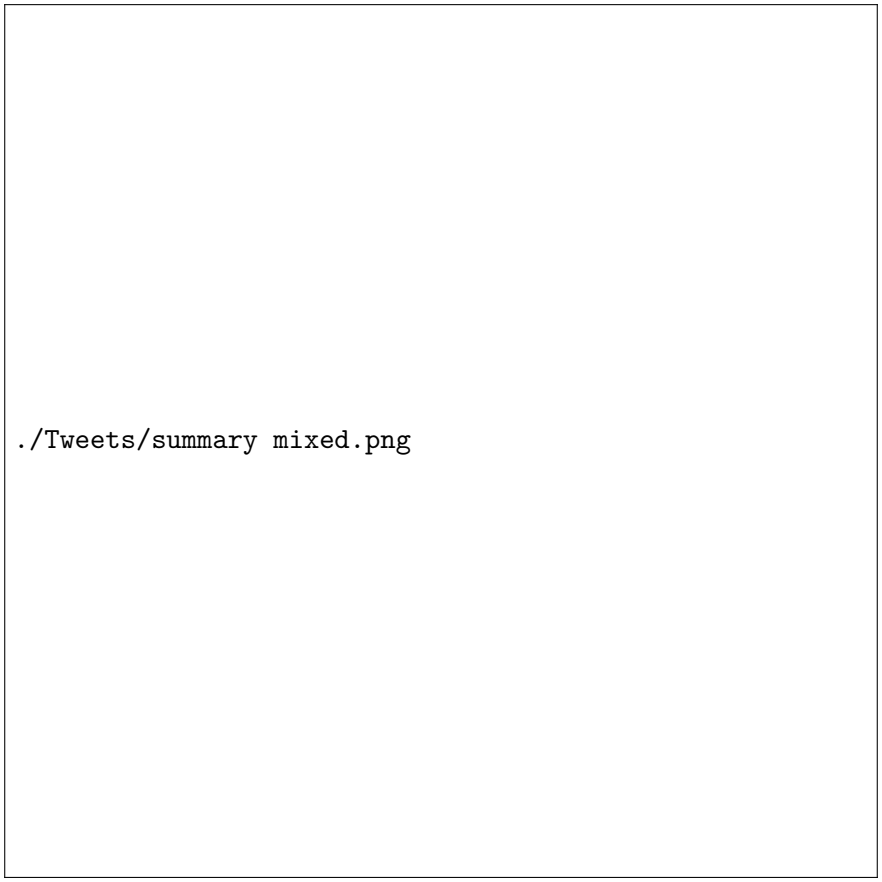

Muslim
Hindu
mean = −24.5
mean = −29.23
mean = 0.1
mean = 0.1
Count of estimates
2500
2000
1500
1000
500
0
−40
0
40
Estimates of treatment effect on earnings in Rupees
Treatment assignment
actual hindu
actual muslim
random counterfactuals

Figure D8: **Optimal Payments**

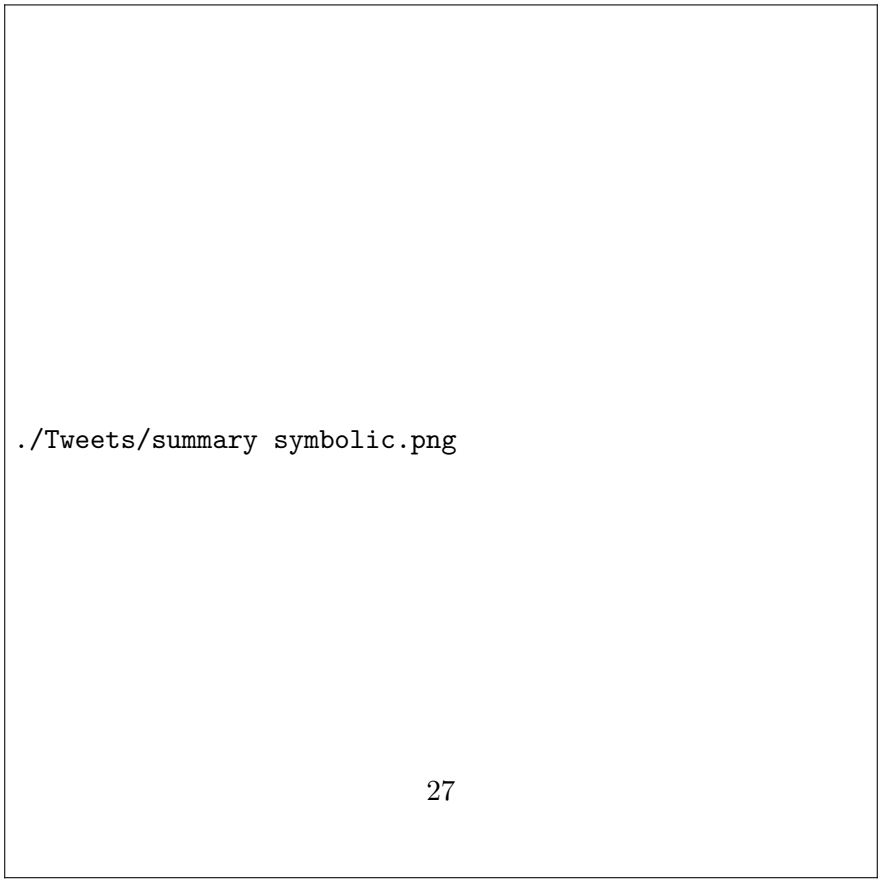


E Figures

Figure E9: Summary of Tweets by Treatment Arms



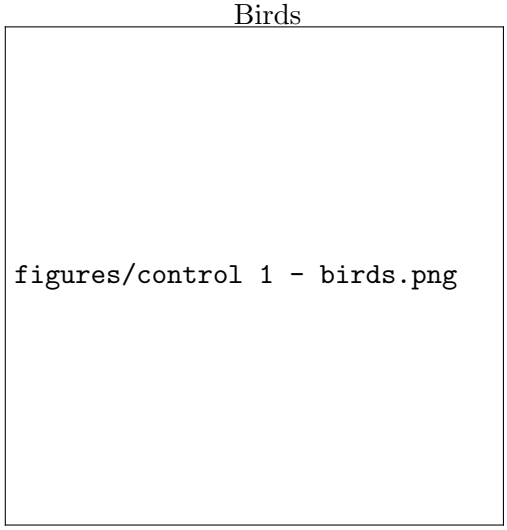
(a) Symbolic Plus Tweets



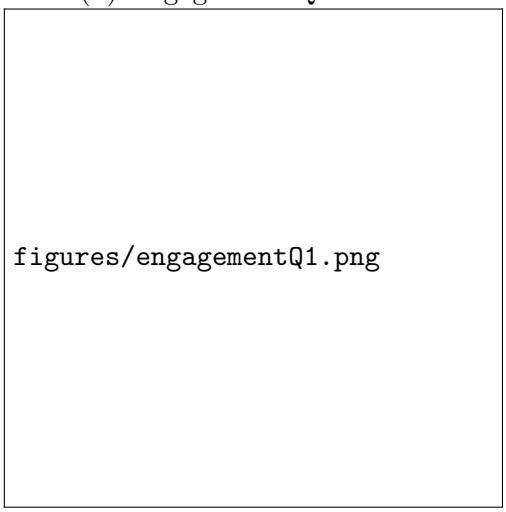
(b) Symbolic Only Tweets

Figure E10: Example Tweet and Engagement Questions

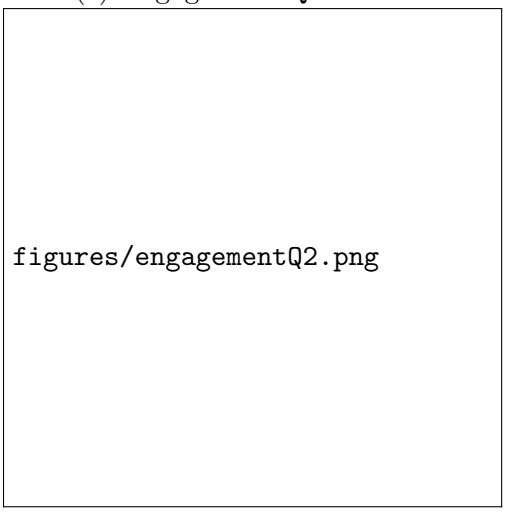
(a) Example Control Tweet: Regional Birds



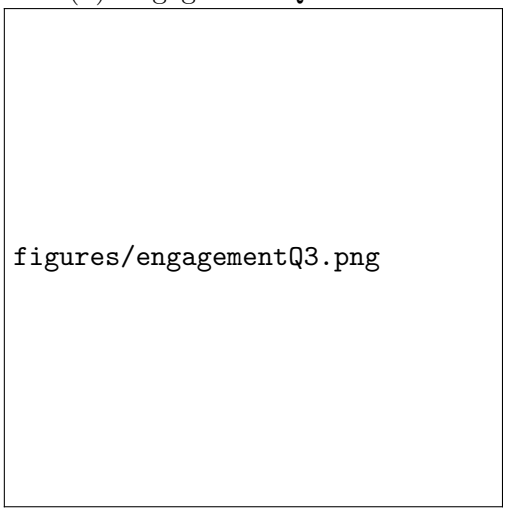
(b) Engagement Question I



(c) Engagement Question II

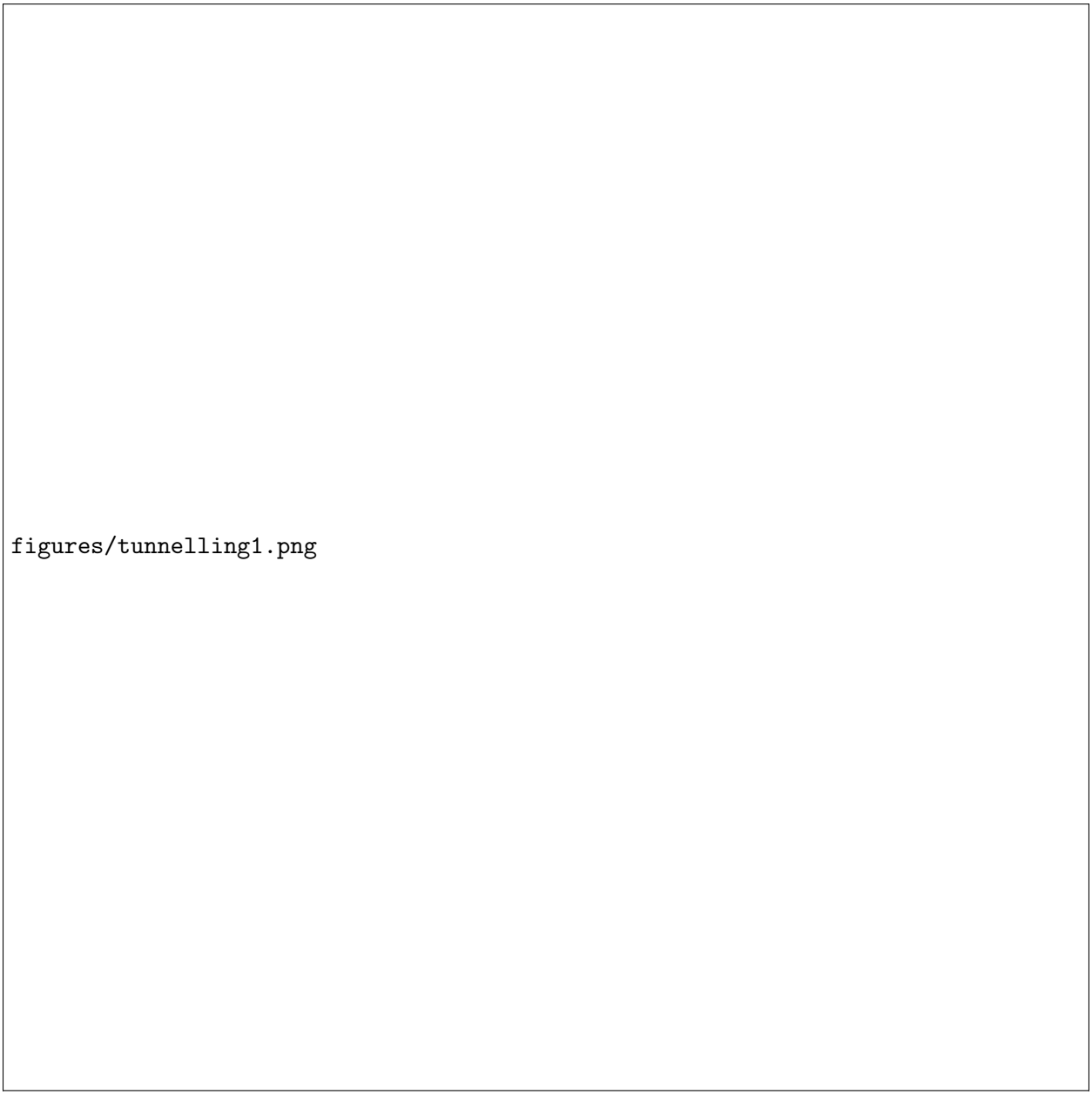


(d) Engagement Question III



Notes: Panel (a) shows a sample control tweet featuring a neutral topic on regional birds. 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 E11: Emotional Stroop Test



(a) Example (non-emotional) Stroop Test

