

Knowing Me, Knowing You:

A Field Experiment on Identity Uncertainty in India*

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Abstract

Many group identities that influence economic behavior are imperfectly observed. Individuals and institutions often conceal identity markers to limit discrimination. Yet concealment also creates uncertainty about group membership, hampering coordination in social interaction. To study this tradeoff, we paired high- and low-caste men for collaborative data-entry work in North India. We randomly assigned each mixed-caste pair to either be: (i) introduced by full names, making caste common knowledge; (ii) introduced by first names only, making caste disclosure a choice; or (iii) instructed not to disclose caste. Relative to full-name introductions, first-name introductions reduce the accuracy of beliefs about a partner's caste and confidence in those beliefs. They also weaken trust, willingness to interact, and perceived productivity. Explicit instructions not to disclose caste produce additional declines in some outcomes. Both conditions reduce participants' ability to express their "true selves," while greater confidence about a partner's caste is associated with stronger workplace ties, suggesting that legible identities help participants navigate social interactions. Concealment leaves a sizable caste disparity in higher-status role assignment intact. We conclude that where group identities are entrenched, reducing their legibility can undermine intergroup relations.

Keywords: Social identity; caste; team production; cooperation; field experiment.

JEL codes: C93, D83, J71, O12, Z13

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Research increasingly recognizes the importance of social identity for economic outcomes (Akerlof and Kranton, 2000; Alesina and La Ferrara, 2005; Horowitz, 2000; Hjort, 2014; Kline et al., 2022; Oh, 2023). An implicit assumption running through much of this work is that individuals' group identities are readily observable.¹ In practice, however, group membership is often hard to discern, whether because individuals actively conceal their identities, or because social group categories are inherently blurry and indeterminate (Southall, 1970). As such, people must rely on noisy cues—such as names, patterns of speech, dress, or skin tone—to make guesses about the group membership of strangers and acquaintances, with no guarantee of accuracy. For example, Moerman (1965, p. 1218) notes that “it is difficult to delimit Thai peoples by means of traits ... the green sarong which sometimes distinguishes the Khyn from their Lue neighbors may elsewhere characterize the Lue.” In his ethnography of Belfast, Northern Ireland, Burton (1978, p. 63) describes the fallible everyday practice of “telling” who is Catholic or Protestant, with one Catholic noting that it was sensible to “treat everyone as an Orangee [Protestant] until you knew different.”² In short, correctly assigning someone to an ethnic, racial, or religious group is fraught with challenges—particularly on first encounter, and sometimes long after. What are the consequences of the resulting identity uncertainty for intergroup relations? Do members of different groups get along better or worse when they are not sure about one another's social backgrounds?

Theory offers competing predictions. Under what we term the *discrimination hypothesis*, identity uncertainty improves cross-group interactions. An audience that cannot identify someone's group membership cannot discriminate against them on that basis, and will fall back instead on meritocratic, individual-specific evaluation criteria—judging people by “the content of their character.” This logic underlies masking interventions such as blind auditions and CV anonymization schemes (Goldin and Rouse, 2000; Behaghel et al., 2015), and sometimes motivates members of stigmatized groups to distort identity signals so as to “pass” as members of advantaged groups (Dahis et al., 2026; Hobbs, 2014). In contrast, under the *coordination hypothesis*, legible group identities help people navigate conversation and self-presentation. They provide shared expectations about appropriate topics, tone, and the disclosure of personal information, and about how one's behavior is likely to be interpreted. In Goffman's (1959) “dramaturgical” framework, people are strongly averse to awkwardness and loss of face in intimate social settings. Interactions are

¹In Akerlof and Kranton's (2000) model, “each person is endowed with a conception of her own categories and that of all other people” (718); that is, individuals can observe and categorize those they interact with. The paper acknowledges that people “with nondistinguishing physical features may be able to ‘pass’ as a member of another group” but suggests that identity choice “is often very limited” (p. 726).

²The full quotation from Burton (1978, p. 63) richly conveys the challenges involved in making inferences about group identification: “One Sunday I walked into the house I was living in and a group of men and boys were discussing Protestant characteristics. Gerard remarked it was easy to tell some ‘Prods’ but harder to do so with others, particularly the young. He knew because he had been wrong a couple of times at work and had ‘only just sorted it out.’ Two young lads said it was their voices but were laughed out of it. By contrast it was claimed if anything it was their looks. Raymond thought the whole discussion a joke and recalled the ‘buck eejit’ (a prize idiot) who after the 1969 riots proclaimed he would always now be able to tell an Orangeman. Kieran, ever reflective, said that it was sensible to ‘treat everyone as an Orangee until you knew different.’ Paul challenged one of the lads who was adamant he could tell with the question: ‘What about the wee girls, then, can you tell with them?’ There followed a confused admission that that was a little harder, though from another room a young girl shouted she could.”

akin to theatrical performances that require a shared script or “working consensus” of the situation to be successful. Knowing one another’s group identities is a critical input into that consensus, helping interlocutors calibrate tone and avoid offense. In urban Zambia, conspicuous ethnic markers enable members of different groups to immediately “fit their neighbours and acquaintances into categories which determine the mode of behaviour towards them” (Mitchell, 1956, 32). Consider—more prosaically—the conversational missteps that can arise from being unaware whether a new acquaintance at an academic conference is a tenured professor or a first-year PhD student. Where group identities structure social expectations, uncertainty may make people more guarded and less able to present themselves naturally.

We study the case of caste in Uttar Pradesh, India, a setting where social identity remains deeply embedded in economic, political, and everyday life (Munshi, 2019; Oh, 2023; Cassan et al., 2021; Shukla, 2025; Banerjee et al., 2013; Lowe, 2021; Anderson, 2011; Asher et al., 2026; Chandra, 2007). Strangers cannot straightforwardly pinpoint each other’s caste when first meeting, because the observable markers associated with caste are frequently ambiguous (Valmiki, 2008; Satyanarayanan and Lee, 2023). For our experiment, we recruited young men from General Castes (GCs, or “upper castes”) and Scheduled Castes (SCs, or “lower castes”) with caste-distinctive surnames, and randomly paired them for a day of paid collaborative data-entry work, approximating short-term spot labor arrangements common in the region. We randomly assigned cross-caste pairs to one of three information environments: *Revealed*—enumerators announced both partners’ full names, making caste common knowledge; *First Name Only*—enumerators introduced partners by first names only, permitting participants to choose whether to reveal caste or not; or *Hidden*—identical to *First Name Only*, other than the addition of instructions explicitly urging participants not to disclose caste-revealing information.³ At the conclusion of the work period, each participant privately completed measures of trust and social relations, along with beliefs about their partner’s caste. In a supplementary survey of men seeking work at labor stands in Lucknow (“labor-stand survey”), we show that uncertainty about colleagues’ caste is common during such employment spells, and surnames are primarily understood as caste signals. Thus, the information problem we study also arises in the surrounding labor market.

Our first set of results considers beliefs about caste identity. Our rich measurement of beliefs allows us to contribute new descriptive facts to a nascent literature on “passing” that relies on secondary data sources (Adukia et al. 2025; Mitrut et al. 2025; Dahis et al. 2026). As intended, caste identity is effectively common knowledge in the *Revealed* condition: 97% of participants correctly guess their partner’s caste category (from among five categories), and 97% believe that their partner will guess their own caste category correctly. Participants in *First Name Only*

³The treatments are designed to mirror realistic workplace information environments: from village labor markets in India, where workers grew up together, and thus tend to know each other’s caste (as in *Revealed*), to French public institutions, where policies of *laïcité* discourage displays of religious identity (as in *Hidden*).

are significantly less accurate and less confident when guessing the caste of their partners, with further losses of accuracy and confidence in *Hidden*. Caste category guesses are 14 percentage points less likely to be correct in *First Name Only*, and 22 percentage points less likely to be correct in *Hidden*. These effects should be considered a lower bound of the effective first stage on identity beliefs during the interaction, since they rely on data measured at the end of the workday, when partners have maximal information about each other. The results nevertheless show that a large fraction of participants correctly guess the caste of their partners even in the concealment conditions.

How do participants arrive at accurate guesses without knowledge of last names? First, caste often comes up explicitly in conversation: in *First Name Only*, 42% of participants report that their partner told them their caste. Second, many participants report making a guess based on how their partner spoke (e.g., how formal their Hindi was) or how they appeared (e.g., cleanliness). Combining ratings of clothing and speech with other potentially observable participant characteristics, we reach an out-of-sample predictive accuracy of 66%, shedding light on why even noisy signals of caste often yield accurate beliefs.

Surprisingly, low-caste and high-caste participants guess their partner's caste with similar accuracy in the *First Name Only* treatment, despite the expectation that marginalized groups have stronger incentives to keep their identity concealed. The evidence points to two explanations. First, identity disclosure is highly correlated across pairs. Most partnerships either have both members disclose (and learn) caste, or neither, rather than just one of the two. This correlation is a natural consequence of conversational norms, whereby one party's disclosure elicits reciprocation. We find descriptive support for such reciprocal norms in the labor-stand survey. Second, we do find the hypothesized asymmetry in *Hidden*: when instructed to hide caste identity, low-caste participants do so more successfully than high-caste participants. Thus, low-caste identities are concealed more effectively when the channel of reciprocation norms is shut down.

Our second set of results considers downstream impacts on social and economic relations. Withholding surnames weakens social relations even when participants remain free to disclose caste. Participants in the *First Name Only* and *Hidden* conditions report significantly weaker partner bonds compared to those in *Revealed*: identity fusion, measured on a 0–4 scale, declines by 0.29 and 0.47 points, respectively ($p < 0.01$ and $p < 0.001$), and the perceived likelihood of meeting one's partner again falls by 16 and 20 percentage points ($p < 0.01$ and $p < 0.001$). Incentivized trust game behaviors likewise deteriorate when last names are not revealed. Participants in the *First Name Only* and *Hidden* conditions send roughly 15% less to their partners ($p < 0.01$ for both comparisons), expect their partners to send 9 to 13% less ($p = 0.05$ and $p = 0.01$), and, in the *Hidden* condition, return 9% less of the final pool of money ($p = 0.01$). Finally, participants in the *Hidden* condition require roughly Rs. 37 more per day to choose to work again with their current cross-caste partner, rather than switch to an otherwise unknown same-caste worker,

relative to participants in the *Revealed* condition ($p < 0.1$). The negative effects on social relations are present for both lower- and upper-caste participants, indicating that the costs extend beyond any loss of status experienced by upper-castes.

Neither concealment condition affects an index of overall productivity. This does not imply that caste composition is irrelevant to performance: within the *Revealed* arm, mixed-caste teams produce less output than the small number of same-caste teams included in the experiment ($p = 0.11$).⁴ But, strikingly, concealment substantially lowers *perceptions* of both own and partner performance (by 0.35 to 0.52σ , all $p < 0.01$). Consequently, our treatments leave actual output mostly unchanged but distort perceptions of competence—a margin likely to matter in this setting for future job referrals and collaboration.

Why does withholding identity cues undermine social relations? Two pieces of evidence suggest that uncertainty over partner identity makes conversation and self-presentation harder to navigate. (i) *Loss of self*: Participants in *First Name Only* and *Hidden* report feeling substantially less able to show their “true selves” to their partners, with these effects most pronounced among participants who rank caste as more important for their personal identity at baseline. The concealment treatments also reduce perceptions that the partner was friendly. (ii) *Confidence*: Confidence in one’s guess about a partner’s caste identity descriptively explains much of the observed treatment effects. Once we control for guess confidence, the key *First Name Only* and *Hidden* coefficients shrink in magnitude and most become statistically insignificant. In addition, compared to participants who are close to certain of their partner’s caste, participants who report being fully (i.e., 100%) certain demonstrate discrete improvements in trust, identity fusion, and the likelihood of meeting their partner again. Where the costs of misreading identity might be substantial, *knowing* a counterpart’s identity appears to be qualitatively different from merely suspecting it. Corroborating evidence comes from our labor-stand survey, in which workers describe caste information as relevant to how they navigate interactions with a new partner, including friendship, sharing meals, and social distance. Consistent with this, a majority of labor-stand respondents report that they would prefer a workplace in which caste is revealed to one in which it is hidden, with this majority preference even true when restricting to lower-caste respondents.

We examine several alternative explanations for the adverse effects of masking identity. Against experimenter demand effects, we find that caste-related guesses about the study’s purpose are rare and unaffected by treatment. Against a channel of perceived similarity, we find that same-caste partnerships have *stronger* relations. Against the argument that surnames reveal more information than just caste, we find that surnames do not predict treatment effects after conditioning on caste, and within the concealment arms, the association between surname knowledge and better relations disappears after conditioning on caste beliefs and confidence.

⁴We included a small share of same-caste teams to obscure the study objective. Figure A15 shows that this strategy was successful.

If certainty about identity is valuable, why don't more participants in *First Name Only* reveal their identity? We argue that disclosure may involve uncertainty: individuals may not know whether discussing caste is socially appropriate, whether their partner would reciprocate, or how their identity would be received. We develop a simple model of strategic disclosure to illustrate how these considerations can sustain different social equilibria (Appendix Section A). Under one equilibrium, neither party voluntarily reveals identity; under another, disclosure is more common but selective. Consistent with this perspective, disclosure in our experimental data is more common when partners are similar in education or income, and participants tend to reveal reciprocally. The labor-stand survey provides complementary evidence: many workers report being reluctant to initiate disclosure but willing to reciprocate it, while others anticipate discomfort or adverse treatment. Thus, the benefits of identity certainty need not translate into spontaneous revelation when individuals are uncertain about the consequences of disclosure.

We also examine whether concealing caste reduces disparities in the allocation of a better-paid senior partner role. Despite similar ability by caste, the upper-caste worker takes the senior role 60% of the time in the *Revealed* condition ($p < 0.001$ for the null hypothesis of a 50:50 allocation). Neither concealment condition affects this caste disparity. The disparity may then be more of a consequence of caste differences in self-perception (e.g., the greater confidence of upper castes), than social perception.

Related literature Our paper contributes to a small but growing literature in economics on the social construction of group identity. Building on the identity-economics tradition, this work treats identities not simply as fixed traits, but as social categories that are endogenously concealed and inferred (Akerlof and Kranton, 2000; Rose, 2023). Recent work shows that the classification of group identity is shaped by observer perceptions and economic incentives: wealthier and literate individuals were more likely to be classified by census enumerators as White or Mulatto in the U.S. census (Adukia et al., 2025); more-educated Roma were more likely to “pass” as non-Roma in Romanian census data (Mitrut et al., 2025); and Indians manipulated caste identity to secure eligibility for land acquisition (Cassan, 2015). Most closely related, Dahis et al. (2026) show that Black men in the U.S. who passed as White earned more but experienced weaker family ties. We bring an experimental approach to this literature, enabling us to (i) estimate the first *causal* effects of passing versus identity revelation,⁵ and (ii) characterize the effectiveness of concealment using rich measures of beliefs—measures that are missing from the administrative data used in existing studies.⁶

⁵Or more precisely, the effects of opportunities to pass enabled by our *First Name Only* condition, and the effects of instructions to conceal identity using our *Hidden* condition.

⁶A small literature in political science likewise examines the visibility and identifiability of group identities, showing that ethnic markers are often less legible and fixed than commonly assumed (Robinson, 2024; Harris et al., 2024). We also contribute to experimental work on intergroup contact that shows that the effects of contact depend on the *type* of contact (Lowe, 2021; Ghosh, 2025; Greene et al., 2025; Chakraborty et al., 2024). For our core experimental comparisons we hold intergroup contact fixed while varying one aspect of the type of contact: knowledge of group identity. Psychologists hold polar opposite views on whether contact works better when group identities are

Second, we contribute to the experimental literature on discrimination (Bertrand and Duflo, 2017). Audit studies observe real interactions after identity is perceived (Fix and Struyk, 1993), but auditors may differ along many dimensions other than group identity. Correspondence and related online experiments solve this problem by randomizing identity cues, such as names, but usually observe selection or evaluation margins such as callbacks or purchases (Bertrand and Mullainathan, 2004; Kline et al., 2022; Kelley et al., 2023). Our experiment design combines features of both approaches, enabling us to identify the effects of perceived identity during in-person interactions. In doing so, we uncover new insights relevant for policies around blinding (Goldin and Rouse, 2000; Behaghel et al., 2015; Boring et al., 2025). While blinding may be a useful tool at the application stage, by reducing discrimination, it may backfire when applied to in-person interactions, given the social costs of failed coordination.⁷

Setting, experiment design, and data

Setting We ran the experiment in the districts of Barabanki and Ayodhya in Uttar Pradesh, India’s largest state, with a population of nearly 250 million people. Caste is a salient social cleavage in our study districts. It is often inferable from surnames, neighborhood of residence (given physical segregation within villages), and familiarity with local family histories and lineages; it might also be partially gauged through the noisier signals emitted by speech patterns, conversational content, appearance, and occupation.⁸ Many economic interactions involve people who do not know one another, including short-term work arrangements, hiring through informal networks, or market transactions in nearby towns. This makes the setting well-suited for studying the feasibility and consequences of identity uncertainty.

Caste bias in the study region Caste is a system of hereditary social stratification that permeates social, economic, and political life in Uttar Pradesh. Thousands of *jatis* are grouped into several broad administrative categories: General Castes, Other Backward Classes, Scheduled Castes, and Scheduled Tribes. These categories determine eligibility for affirmative action quotas in government jobs and higher education. Our study focuses on interactions

salient, or when interactions follow an approach of personalization (Brewer and Miller, 1984; Hewstone and Brown, 1986; Miller, 2002; Dovidio et al., 2003). We find evidence in favor of the former view.

⁷A related literature considers how individuals respond to the prospect of expected discrimination during an application process (Kang et al., 2016; Charness et al., 2020; Lepage et al., 2022; Ruebeck, 2023; Avery et al., 2024; Del Carpio and Fujiwara, 2026; Angeli et al., 2023). For example, Angeli et al. (2023) show that favela residents in Brazil perform worse at interview when they expect the interviewer to know their favela status. In addition, work on stereotype threat finds that the revelation of group identity can harm individual performance. Closest to us, Hoff and Pandey (2006) find that the public revelation of caste led to a decline in task performance—solving mazes—by low-caste subjects, when grouped with high-caste subjects. Our approach differs from these papers in that we study whether and how group identity becomes revealed to others (as opposed to just randomizing whether it is revealed or not), and we study implications for social interactions (rather than individual decision-making, like whether to apply for a job).

⁸Little quantitative evidence exists on the accuracy of caste guesses made via these noisier signals, though we note that in another Indian state, Odisha, Oh (2023, p. 2077) finds that 71% of workers struggle to form any guess about the caste of surveyors based on appearance alone.

between General Caste and Scheduled Caste participants, the latter being a marginalized group routinely subject to stigma and “untouchability.”

To avoid priming respondents, we deliberately opted not to measure caste-related bias in the experimental baseline survey. Instead, we use two sources of evidence to assess bias in the study population. First, as we show in the endline results, workers show greater trust of and interpersonal closeness with same-caste partners, and role allocation in tasks strongly favors GC participants. Second, we document strong caste bias in a scoping survey that we carried out in the same region. Appendix Figure A2 shows that both GC and SC caste groups strongly privilege their in-group during a hypothetical dictator game, over 70% of GC respondents reported that no one in their village would accept food from an SC individual, and almost all GC respondents reported that no one in their family would share a meal with an SC person. Notably, the share of GCs saying no one in their village would shake hands with SCs is considerably lower, at under 20%, indicating that untouchability manifests primarily around food, which triggers purity-pollution concerns, rather than everyday working interactions.⁹

Naming and the concealment of caste Social reform movements have repeatedly sought to reduce the salience of caste identity through changes to naming practices. In Sikhism, the collective surnames Singh (for men) and Kaur (for women) were adopted explicitly to help efface caste distinctions.¹⁰ Similar impulses have appeared in modern India. To subvert caste hierarchies, the Dravidian and Self-Respect movements in early twentieth-century Tamil Nadu—led by E. V. Ramasamy, known as Periyar—urged people to replace caste surnames with initials. Srinivas (1966) coined the term “Sanskritization” to capture efforts by lower-caste groups to emulate upper-caste practices, rituals, and surnames in pursuit of social acceptance and upward social mobility. More recently, urbanization and migration have made such strategic identity management increasingly feasible at the individual level, also: “In rural settlements, people can easily identify the caste identity of others ... But city life is characterized by a high degree of anonymity ... [which] provides the opportunity ... to transcend or hide [SCs’/Dalits’] ‘Untouchable-hood’ by changing surnames” (Parmar, 2020, 221).

Casual daily labor markets Casual daily labor markets are ubiquitous in developing countries and employ a large share of the world’s poor (Kaur, 2019; Cefala et al., 2024). In India, where youth unemployment is high, job seekers frequently gather at labor stands for temporary work, especially in construction. Many manufacturing firms also rely

⁹This fact helps alleviate ex ante concerns that participation would only attract more progressive upper-caste men willing to work with lower-caste partners.

¹⁰Sikh historical tradition holds that, at the founding of the Khalsa in 1699, Guru Gobind Singh initiated the *Panj Piare* (“Beloved Five”)—who hailed from both upper- and lower-caste backgrounds—into a shared “casteless fraternity” by having them drink from a common cup and abandon their Hindu caste names (Singh, 2004, 80). In practice, many Sikhs have retained additional family names indicative of clan or caste origins.

on daily or short-term labor to meet fluctuating manpower needs.

In October 2026, roughly one year after the experiment, we conducted a pre-registered survey of 377 men aged 18-40 years in multiple labor stands in Lucknow to examine how our setting relates to the surrounding labor market. Seventy-nine percent of our respondents reported spending a week or less on a typical job, and 95% reported spending two weeks or less (Appendix Figure A3, Panel B). The respondents also viewed the feasibility of concealing caste as dependent on the length of the encounter. Ninety-three percent of the respondents said hiding caste would be easy for a day, compared with just under 10% for a month (Panel A). In respondents' most recent jobs, uncertainty about colleagues' caste identity is common: the median respondent reports not knowing the caste for 40% of colleagues. These facts support the relevance of studying identity uncertainty in the context of short-term casual employment.

Importantly, even brief jobs may generate valuable social ties. In Chakraborty et al. (2024), we document continued contact and mutual help finding paid work between partners who worked together for as little as one day in West Bengal. Working together for longer strengthened these ties and increased subsequent job-search and financial assistance. Such relationships can therefore have economic value beyond the job itself in these settings.

Recruitment and baseline We recruited subjects through door-to-door campaigns in villages located near the study sites, using three eligibility criteria. First, respondents had to be men between the ages of 18 and 40. Second, they were required to have a last name drawn from a pre-specified list of distinctive caste surnames—names that a prior scoping survey had shown to be almost exclusively associated with a single caste category (see Appendix Section D.1).¹¹ We primarily recruited participants from SC or GC backgrounds, though we included a small number of OBCs for obfuscation purposes, as we explain below. Third, respondents completed short, timed exercises testing basic competencies in typing and document reading; only those who demonstrated at least a minimal proficiency in both skills could continue. Individuals who met these three criteria, who provided informed consent, and who then completed a short baseline survey, were invited to participate in the study. They were given a unique identification code, and assigned a date on which they would work.

Information conveyed by surnames. As mentioned, we recruited participants with caste-unambiguous surnames to ensure that the *Revealed* condition provided a strong caste signal. To do this, we conducted the scoping survey mentioned above, asking respondents which caste category they associated with each of the surnames used in our experiment. On average across surnames, 87% of respondents named the same caste category as in our study (Figure

¹¹This was, of course, not made salient to participants. The recruitment survey further confirmed that the respondent's reported caste category matched the caste typically linked to their surname.

A18). Furthermore, we show with our experimental participants below that the *Revealed* condition also reveals the specific caste (*jati*) of the partner, as intended. For example, the last name Singh reveals not only that the partner belongs to the General caste category, but also that the partner’s caste is Rajput, traditionally considered a warrior caste.

A key claim of our design is that treatment effects of surname revelation are due to the revelation of group identity (and subsequently any identity-correlated characteristics), rather than the revelation of information unrelated to group identity. To support this claim, we asked labor-stand survey respondents what each of the five most common surnames from the experiment (covering 89% of the core experimental sample) told them about a man, without reading out possible answers. Caste was by far the most common response, mentioned by 92–95% of respondents for each surname (Appendix Figure A3, Panel C), while the other information conveyed is consistent with caste-correlated characteristics. For example, roughly 50% of participants said that the two Brahmin surnames, Shukla and Mishra, conveyed information about religion, which is naturally caste-correlated, given that Brahmins are traditionally considered the priestly caste.

A related concern is whether surnames are also signals of geographic origin or family associations. To examine geographic concentration, we use the 2011 Socio Economic and Caste Census (SECC), a nationwide government enumeration of households and their socioeconomic characteristics.¹² These data show limited concentration of the surnames used in our experiment within individual villages. For the median participant, fewer than 5% of households sharing his surname live in the village where it is most common; the corresponding average across participants in the main sample is 3.8% (see Appendix Figure A20). Consistent with this, only 2–5% of labor-stand respondents said surnames convey where a man lives. In the results section, we provide further evidence that treatment effects are not driven by non-identity-related information conveyed by surnames.

Flow of the experiment Each iteration of the experiment lasted one full day and ran as follows:

1. *Arrival.* Participants ($4 \leq N \leq 32$) arrived at the study site and checked in with facilitators using their unique identification code. Upon arrival, participants were seated in widely-spaced chairs and instructed not to interact with one another, with enumerators monitoring compliance.
2. *Random assignment.* We randomly assigned individuals to pairs, and pairs to one of the three treatment conditions.

¹²We use publicly available rural household records from the 2011 SECC. We use data for the 570,181 male household heads in 2,062 villages in Lucknow and Ayodhya (Barabanki is missing from the SECC data). We define surnames as the last word of each name, with spelling variations harmonized across the SECC and experimental data.

3. *Professionalism instructions and survey.* Each participant was individually called to complete a short “professionalism” survey, which presented a set of workplace rules and checked for comprehension of those rules. The rules were repeated until they were fully understood.
4. *Tasks in pairs.* Participants were escorted to private booths, where they worked in pairs on a sequence of five collaborative tasks lasting approximately four hours.
5. *Endline survey.* After completing the tasks, participants individually responded to an endline survey, led by an enumerator.
6. *Exit.* Immediately upon finishing the endline survey, participants were instructed to leave the study site. Enumerators ensured that participants did not interact across booths before departure.
7. *Incentive payments.* Participants were informed about their team earnings and bonus payments (see below), with base payments made on the same day. Bonus payments were disbursed within three days.

Randomization At the start of each workday, participants provided enumerators with their unique identification codes. We used these codes to retrieve the participant’s caste from a pre-loaded database, minimizing the salience of caste during the session itself. A research assistant entered the codes of attending participants into a randomization platform we built (an R Shiny app, see Appendix Figure A1), which used the preloaded baseline data to conduct that day’s randomization. The initial step of the randomization involved assigning participants to pairs:

- *Mixed GC-SC pairs (primary sample, $N = 532$ pairs).* Most participants were allocated to high-caste-with-low-caste (GC-SC) dyads.
- *Homogeneous GC-GC or SC-SC pair (obfuscation sample, $N = 49$ pairs).* For each daily session, one caste group (either GC or SC) was assigned to form that day’s homogeneous pair. On even-numbered days (e.g., July 2), two GC participants were randomly selected to constitute a homogeneous dyad, while on odd-numbered days, two SC participants were selected instead.¹³
- *Mixed GC-OBC or SC-OBC pair (obfuscation sample, $N = 18$ pairs).* If at least one OBC participant was present at a given session, exactly one was randomly selected and paired with a participant from the caste opposite to that day’s designated homogeneous group.¹⁴

¹³This dyad type and the next were introduced partway through the experiment to enhance obfuscation.

¹⁴This brings the total number of participants in the experiment to 1,198. Out of these, 1,144 completed both the work tasks and the endline survey. Some subjects left during the day because of personal or family obligations. We keep participants’ data for as long as they participated; that is, even if they did not complete all parts of the survey. All participants were compensated based on the parts of the study they completed.

We included homogeneous and OBC-containing dyads for obfuscation. If, following the experiment, participants discussed their assignments outside the experimental setting, the presence of these additional pair types helped ensure that the study design did not appear to be focused exclusively on GC-SC pairings. This reduced the likelihood that the intergroup relations angle of the experiment would be evident to future participants. Our main analysis concentrates on the GC-SC sample, which directly tests the impact of identity legibility. The smaller same-caste sample provides an additional comparison of team composition, which allows us to rule out alternative mechanisms.

After the randomization was implemented, any unmatched individuals were placed on a waitlist and asked to return on another day at their convenience. In practice, enumerators sought to invite participants in the appropriate proportions each day so as to limit the number of those waitlisted.

Treatment conditions We assigned all GC-SC pairs, with equal probability, to one of three conditions—*Revealed*, *First Name Only*, or *Hidden*. Since the purpose of the remaining homogeneous-caste and OBC-containing dyads was for participants to learn that non-GC-SC pairings were possible, we always assigned these dyads to *Revealed*. Our information treatments are as follows (see Table 1 for the exact wording):

- *Revealed* ($N = 176$ GC-SC pairs). Prior to the pairs of work partners being introduced to one another, participants privately completed the professionalism survey, which lists some neutral workplace guidelines (e.g., not communicating across booths, limiting phone use, coordinating short breaks). At the start of the task survey, with both partners present around a table in their assigned booth, an enumerator read aloud each participant's full (i.e., first and last) name twice, purportedly to confirm that the correct individuals were in the correct booth.¹⁵ Full names—preloaded by the enumerator—were then also displayed on the tablet screen after each task and were written on slips of paper placed on the desk. We asked participants to use these slips to record who served as the senior partner for each task. Given that we selected participants with caste-distinctive last names, the intent of the *Revealed* treatment was to ensure that caste identity is common knowledge among partners.
- *First Name Only* ($N = 179$ GC-SC pairs). Identical to *Revealed*, but with first names used throughout, rather than full names. Since first names are not caste-revealing in this context, this treatment left participants free to conceal or disclose caste to their partners.
- *Hidden* ($N = 177$ GC-SC pairs). Identical to *First Name Only*, except that the professionalism survey addi-

¹⁵During the informed consent process, all potential participants were explicitly informed that their first and last names might be shared with a work partner who “may come from a similar background to you, or from a very different background.” Participants were further instructed: “You should only participate in this study if you are comfortable with your first name, last name, and age being shared with your work partner, who is someone you do not know.”

tionally instructed participants not to disclose their last names or any other information that could reveal their caste. We justified the instruction on the grounds of respecting one another’s privacy. Participants answered one additional comprehension question concerning the extra instruction. Thus, this condition attempted to make caste identity less observable by discouraging the exchange of clear caste-revealing information during the interaction.¹⁶

Data entry tasks and piece-rate payments We randomly assigned pairs to booths to prevent any perceived association between treatment status and physical work location. We instructed pairs to perform a sequence of five paid collaborative tasks. Each task lasted up to 45 minutes, with 15-minute breaks in between, and the entire exercise took roughly four hours to complete. The tasks were designed to reflect a clear status hierarchy: one partner, dubbed the *senior partner*, operated the computer to type in the information, while the other, dubbed the *junior partner*, read aloud data from paper forms contained in labeled envelopes. Worker pairs chose which worker would be the senior partner for each task, with the exception of one randomly-chosen task, in which the senior partner role was assigned randomly. The tasks varied in content and difficulty; all, however, demanded close coordination under time pressure. To build familiarity with the technology and procedures, pairs started with the two simplest tasks, in this order:

1. *Ordered data entry.* Participants are given a long list of names, each accompanied by an exam grade. The same names appear on their computer screen in the same order, but without the grades. The task was to enter the correct grade for each name.
2. *Unordered data entry.* Participants again receive a list of names, this time accompanied by years of birth. Unlike the previous task, however, the names appear in a different order on the computer screen. Participants’ task was to locate each name in the paper list and enter the corresponding year of birth in a four-digit format (e.g., “1999”). Because completing the task required searching and cross-referencing across the two lists, it was more demanding.

Next, pairs carried out three more challenging tasks, in random order, allowing us to purge order effects in the analysis:

3. *Cross-validation.* Here, the envelope contains two separate paper lists of names with grades. The screen displays names from List 1 in the same order. Participants had to check whether the grade in List 1 matched

¹⁶Our treatments map onto a documented institutional response to caste-linked identification in Indian workplaces: suppressing surnames. For example, the Himachal Pradesh police adopted a policy of removing surnames from official communication, to explicitly reduce caste and regional divisions within the force (see *Indian Express*, “We don’t have surnames, we are the Himachal Pradesh Police”, Jun 24, 2011 and *Times of India*, “Himachal Pradesh police no-surname policy attracts Kerala”, Nov 6, 2011). Our *First Name Only* condition captures this kind of partial masking; surnames are withheld, but caste may still be inferred or disclosed during interaction. The *Hidden* condition goes a step further by discouraging disclosure of surnames and other caste-revealing information during interaction.

Table 1: Experimental manipulations, by treatment condition.

	Professionalism survey	Tasks survey			
		Enumerator checks	introductory	Tablet display, appeared after every task	Record sheet, sitting on desk throughout
Revealed	We wish to create a positive and productive work environment. To contribute to this environment, you must follow these factory guidelines ... You can take breaks when you need to, but do not make them too long. Ideally, coordinate short breaks together with your partner to maintain a balanced workflow and avoid interruptions for your partner. Do not communicate with workers in the other booths at all over the course of the work day. Please limit the use of your mobile phone while you are working.	Before you begin the work tasks, I must confirm that the correct partners are in the booth. Is Pramod Gautam in this booth? To repeat, is Pramod Gautam in this booth? [CONFIRM YES] Thank you. Is Anup Pandey in this booth? To repeat, is Anup Pandey in this booth? [CONFIRM YES]	Who do you select to be the senior partner for this task (enters information on the tablet)? Remember, the person chosen as senior partner will be paid 50 INR extra for this task. PARTICIPANTS SELECT: Pramod Gautam OR Anup Pandey	Number of times Pramod Gautam was senior partner [PARTICIPANTS WRITE NUMBER]; Number of times Anup Pandey was senior partner [ENTER NUMBER]	
First Name Only	« Same as Revealed »	Before you begin the work tasks, I must confirm that the correct partners are in the booth. Is Pramod in this booth? To repeat, is Pramod in this booth? [CONFIRM YES] Thank you. Is Anup in this booth? To repeat, is Anup in this booth? [CONFIRM YES]	Who do you select to be the senior partner for this task (enters information on the tablet)? Remember, the person chosen as senior partner will be paid 50 INR extra for this task. PARTICIPANTS SELECT: Pramod OR Anup	Number of times Pramod was senior partner [PARTICIPANTS WRITE NUMBER]; Number of times Anup was senior partner [ENTER NUMBER]	
Hidden	We wish to create a positive and productive work environment. To contribute to this environment, you must follow these factory guidelines ... You can take breaks when you need to, but do not make them too long. Ideally, coordinate short breaks together with your partner to maintain a balanced workflow and avoid interruptions for your partner. Do not communicate with workers in the other booths at all over the course of the work day. Do not tell your last name to your work partner, or provide information that could reveal your caste. It is important to respect each other's privacy. Please limit the use of your mobile phone while you are working.	« Same as First Name Only »	« Same as First Name Only »	« Same as First Name Only »	

Notes: This table presents relevant excerpts from the professionalism and tasks surveys, where the key experimental manipulations were delivered.

the grade in List 2, and then enter “0” if the name appeared in both lists but the grades differed, “1” if the name and grade matched across both lists, and “2” if the name from List 1 was missing from List 2.

4. *Receipts.* For this task, participants are given mock shop receipts showing purchased items and prices. On screen, they had to enter totals or check whether the items and prices matched across versions. The job mimics everyday record-keeping in workplace or market contexts, involving checks of discrepancies in numerical information.
5. *Ravens.* This task consisted of a Raven’s Progressive Matrices–style cognitive test, to measure abstract reasoning ability, rather than a data-entry exercise. Participants were shown visual patterns with one piece missing and had to choose the correct option to complete the pattern.

We compensated participants based on the joint accuracy of their performance across tasks. Team earnings ranged from 150 to 450 INR (roughly \$1.50 to \$4.50) per person, with compensation increasing in the number of correctly completed task items. In addition, the partner designated as the senior partner for a given task received a personal bonus of 50 INR. In total, a participant who served as senior for all five tasks could earn up to 250 INR in bonuses, on top of the team-based payment.

Outcomes We pre-specified five families of primary outcomes (AEA RCT Registry #15151), covering beliefs about group identity, social relations, trust, productivity and task allocation, and well-being at work. The paragraphs that follow denote the pre-specified primary outcomes. We also refer to some pre-specified secondary outcomes, explicitly flagging those outcomes as such. We detail deviations from the pre-registration in Appendix Section D.2.

Beliefs about group identity. At the end of the work session, we asked each worker to guess both the caste category (SC, ST, OBC, GC, or Muslim) and the specific jati (e.g., Brahmin, Chamar) of their partner. We then classify their answer as correct or not by matching their guess with the baseline data for their partner. Participants reported their level of confidence in the caste category guess on a 0 to 100% scale—with the answer reflecting the participant’s perceived probability that they guessed correctly—and we also asked participants whether they thought their partner would be able to correctly identify their own caste (a second-order belief). This allows us to characterize not only raw accuracy, but also workers’ meta-beliefs about their ability to conceal or reveal caste identity. To guard against strategic behavior, we did not incentivize guesses.¹⁷ For obfuscation, we also asked participants to make guesses about their partners’ marital status, age, asset ownership, and father’s occupation.

Social relations. We measured how participants valued future interaction with their assigned partner. First, in a series of incentivized willingness-to-pay (WTP) and willingness-to-accept (WTA) choices, participants decided whether they would forgo higher wages in order to work again with their current partner rather than a randomly selected co-worker of the same caste (signaled by full name). Participants also reported whether they expected to meet up with their partner in the future outside the study context. Finally, we used an overlapping circles (inclusion-of-other-in-self) measure to assess subjective identity fusion with the partner on a scale of 0 to 4.

Trust. Pairs played a standard incentivized trust game (Berg et al., 1995). In this game, Player A (the sender) received Rs. 50 and decided how much to send to their work partner. Transfers were tripled, and Player B (the receiver) then chose how much to return. Participants stated their intended transfer amounts if chosen as Player A,

¹⁷For example, if we had incentivized guesses, and new participants had learned from previous participants that this was the case, the new participants might have exerted more effort to learn the caste of their partner in the *First Name Only* and *Hidden* conditions, reducing the external validity of our measured beliefs about identity.

their expectations about how much their partner would transfer if assigned as Player A, and their conditional return amounts for each possible transfer if chosen as Player B (using the strategy method). These data allow us to capture multiple dimensions of trust and trustworthiness: willingness to place resources at risk, expectations of reciprocity, and actual reciprocity when endowed with power. We randomly assigned one partner to have their sender choice implemented, with the other having their return choice implemented.¹⁸

Productivity and task (mis)allocation. We measure productivity on the data-entry tasks as the number of fields correctly entered, a joint measure of quantity and quality. Because pairs were free to decide which worker would serve as the senior partner (i.e., operate the tablet) on all tasks but one, we also track the identity of the partner who assumed this higher-status role, task-wise. We complement these objective performance outcomes with a secondary one: perceptions of own and partner’s performance on a scale of 1 to 10.

Well-being at work. Finally, we measured workers’ affective experience during the session. Respondents rated the extent to which they felt happy, self-conscious, confident, relaxed, and anxious, each on a scale from 0 = not at all to 3 = very. As secondary outcomes, we also measured workers’ perceptions of the interpersonal aspects of the interaction, asking them to report the extent to which they were able to show their “true self” to their partner, as well as their partner’s friendliness, rudeness, bossiness, and the degree of tension experienced during the interaction, on scales ranging from 1 = not at all to 10 = very.

Estimation

We estimate our pre-specified intent-to-treat specification:

$$Y_i = \alpha + \beta_1 \text{First Name Only}_{p(i)} + \beta_2 \text{Hidden}_{p(i)} + \gamma X_i + \delta Y_{i0} + \lambda_s + \varepsilon_i, \quad (1)$$

where Y_i denotes the outcome of interest for individual i . Our analysis focuses on the individuals assigned to GC-SC pairs unless stated otherwise. The indicators $\text{First Name Only}_{p(i)}$ and $\text{Hidden}_{p(i)}$ denote assignment of pair p to the *First Name Only* or *Hidden* conditions, with the *Revealed* condition serving as the omitted reference category. X_i is a vector of individual-level controls (age and fixed effects for the participant’s level of education), Y_{i0} is the baseline measure of the outcome to improve precision (if available), and λ_s are randomization strata fixed effects (day-by-caste, where caste is GC or SC). We cluster standard errors at the pair level for outcomes measured at the individual level, and we estimate heteroskedastic-robust standard errors in specifications that include only one of the two caste

¹⁸Enumerators guided participants through a series of comprehension questions and worked examples to ensure that they understood the game’s setup.

categories.

Summary statistics Appendix Table A1 presents summary statistics for the experimental sample. The average participant is 23 years old. Additionally, 73% of the sample report having completed 12th grade. College completion is substantially lower at around 20%, partly because many individuals have not yet reached college completion age. The average monthly household income is Rs. 10,700 (roughly 120 USD), indicating that the participants are relatively low-income. Finally, in terms of identity rankings, participants most commonly identify as “Indian” first, followed by personal relationships (e.g., being a father, son, or brother), and then religion, caste, and state (Uttar Pradesh) identity, all of which receive similar rankings.

As expected, GC participants are, on average, more educated, come from wealthier families, and report having fewer siblings, consistent with lower fertility in this group (Appendix Table A2). GCs rate their Indian identity as more important than SCs do, and their caste identity as less important. At first glance, this might seem counterintuitive given the historical dominance and better socioeconomic outcomes of high castes. But recall that participants were asked to provide relative rankings of identities. One plausible explanation is that high castes may feel a stronger sense of pride in the nation, perceiving themselves as more central to the national identity and its historical narrative, whereas low castes may place greater emphasis on caste identity as a counter-response to marginalization.

Sample selection We approached 1,677 potential subjects in Barabanki and Ayodhya through door-to-door campaigns to arrive at our final experimental sample of 1,198 men. We do not find large or systematic differences between those who participated in the experiment and those who ultimately did not (Appendix Table A3). Participants are slightly younger and, consequently, somewhat less likely to have completed college. They also rank their religious identity marginally lower and personal identities (e.g., father, son, brother) marginally higher, while ranking their caste identity similarly. This likely reflects the fact that younger men had greater scheduling flexibility to participate. Crucially, however, we do not observe selection on the importance of caste identity. Participants are also positively selected on baseline productivity for both castes (Appendix Figure A4);¹⁹ for General caste workers in particular, this suggests that higher-ability men were not deterred from participating—whether by the nature of the task or by the prospect of contact with other castes.

Balance and attrition Appendix Table A4 reports baseline balance across treatment groups including reading and typing productivity, and shows that randomization was successful. We estimate specification 1 with participant characteristics as the dependent variable and report p -values for the full set of pairwise comparisons between treatment

¹⁹This is among men who met the selection criteria based on productivity; in practice very few men we approached did not.

groups. Across all comparisons, the estimated coefficients are small in magnitude and not statistically significant.

Out of the 1,198 experimental participants, 1,144 (95%) completed both the work tasks and the endline survey. Some subjects left during the day because of personal or family obligations. We keep participants' data for as long as they participated, that is, even if they did not complete all parts of the survey.

Results

Beliefs about group identity Our *Revealed* treatment condition works as intended: 97% of participants correctly guess their partner's caste category at the end of the workday, 88% correctly guess their partner's specific jati, participants report that they are 89% confident in their caste category guess on average, 53% report being completely (100%) confident in their caste-category guess, and 97% predict that their partner will guess their caste category accurately (see the omitted category means for columns 1 to 5, Panel A, Table 2). These numbers confirm that caste is approximately common knowledge in the *Revealed* condition.

Withholding surnames in *First Name Only* sharply reduces caste knowledge relative to *Revealed*; the additional restrictions in *Hidden* reduce knowledge further. Relative to the *Revealed* condition, correct caste category guesses fall by 14 and 22 percentage points (Table 2, Panel A, column 1), and correct *jati* guesses by 20 and 34 points (column 2). Participants are more uncertain about their guesses, with negative effects on confidence of 13 and 22 points (column 3), and even larger effects on the dummy variable for complete confidence (column 4). Meta-perceptions change, too: participants are less likely to believe that their partners can correctly guess their own caste category, and less likely to have accurate beliefs about whether the partner guesses correctly (columns 5 and 6). All pairwise differences are statistically significant at the 1% level, implying that *First Name Only* weakens common knowledge relative to *Revealed*, and *Hidden* reduces it still further. We further note that these estimates likely understate the effective first stage. By design, participants in *Revealed* learn their partner's caste immediately, generating four full hours of common knowledge. In the other conditions, by contrast, information likely filters through gradually over the course of the workday. Thus, although end-of-day guess rates are quite high, participants likely spend much of the interaction under conditions of uncertainty, with only incomplete and evolving information about their partner's identity.

When participants guess incorrectly, errors are not confined to adjacent caste categories. Among GC respondents (whose partner is SC), 54% of wrong guesses place the partner in the (intermediate) OBC category, but 41% classify the SC partner as GC—the same caste category as the respondent. Similarly, among SC respondents (whose partner is GC), 63% of wrong guesses place the partner in the OBC category, while 32% classify the GC partner as SC. In sum, while many mistakes reflect movement toward the middle, a large share also involve perceived “passing”

Table 2: Guessing partner's caste identity by treatment and caste group.

	Correctly guessed caste category (1)	Correctly guessed jati (2)	Confidence in caste category guess (3)	100% confidence in caste category guess (4)	Partner can guess my caste category (5)	Second-order belief accuracy (6)
A. Full sample						
First Name Only	-0.14*** (0.03)	-0.20*** (0.04)	-12.67*** (1.90)	-0.28*** (0.05)	-0.15*** (0.03)	-0.12*** (0.03)
Hidden	-0.22*** (0.03)	-0.34*** (0.03)	-22.06*** (1.86)	-0.41*** (0.05)	-0.35*** (0.04)	-0.23*** (0.03)
First Name Only vs. Hidden <i>p</i> -value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Revealed mean	0.97	0.88	89.10	0.53	0.97	0.94
<i>N</i>	1,013	1,013	1,013	1,013	1,013	1,013
B. SC sample						
First Name Only	-0.14*** (0.03)	-0.20*** (0.05)	-12.27*** (2.05)	-0.24*** (0.05)	-0.17*** (0.04)	-0.12*** (0.04)
Hidden	-0.16*** (0.03)	-0.28*** (0.04)	-22.31*** (2.08)	-0.37*** (0.05)	-0.34*** (0.04)	-0.24*** (0.04)
First Name Only vs. Hidden <i>p</i> -value	0.66	0.13	<0.01	<0.01	<0.01	0.01
Revealed mean	0.98	0.90	89.06	0.51	0.96	0.93
<i>N</i>	506	506	506	506	506	506
C. GC sample						
First Name Only	-0.13*** (0.03)	-0.20*** (0.04)	-13.31*** (2.09)	-0.31*** (0.05)	-0.15*** (0.03)	-0.12*** (0.03)
Hidden	-0.28*** (0.04)	-0.40*** (0.05)	-22.00*** (2.01)	-0.46*** (0.05)	-0.36*** (0.04)	-0.23*** (0.04)
First Name Only vs. Hidden <i>p</i> -value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Revealed mean	0.95	0.86	89.13	0.55	0.98	0.96
<i>N</i>	507	507	507	507	507	507
D. Difference in differences						
First Name Only × GC (vs. SC) <i>p</i> -value	0.81	0.92	0.52	0.03	0.51	0.90
Hidden × GC (vs. SC) <i>p</i> -value	<0.01	0.03	0.81	<0.01	0.64	0.89

Notes: Each column reports estimates from an OLS regression of the outcome on indicators for assignment to the *First Name Only* and *Hidden* treatments, with *Revealed* as the omitted category. Panel D reports difference-in-differences *p*-values from regressions that interact each treatment indicator with an indicator for GC participant (SC is the omitted group); the reported *p*-values correspond to the interaction terms. Outcomes are defined as follows: (1) equals 1 if the participant correctly guessed their partner's caste category; (2) equals 1 if the participant correctly guessed their partner's jati; (3) is confidence in the caste-category guess on a 0–100% scale; (4) equals 1 if the participant reported they were 100% confident in their caste-category guess; (5) equals 1 if the participant believes their partner can correctly guess their caste category; (6) equals 1 if the participant correctly predicted whether their partner would guess their caste category correctly. Standard errors are reported in parentheses. In Panels A and D, standard errors are clustered at the pair level. In Panels B and C (the SC and GC subsamples), standard errors are heteroskedasticity-robust. All specifications include randomization strata fixed effects and pre-specified controls for participants' education level and age. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

across the full caste-status divide. This passing is even more remarkable when put in the context of caste population shares in Uttar Pradesh: OBCs are roughly twice as common as either SCs or GCs; but we see frequent incorrect guesses of SC and GC despite their relatively smaller population shares.²⁰

Effects on caste knowledge remain strong when considering GCs and SCs separately (Table 2, Panels B and C). Surprisingly, the effects of *First Name Only* are similar for both high and low-caste participants: we do not see evidence that SCs are hiding their identity more often than GCs in this “choice” condition. As we elaborate below, one explanation is that disclosure tends to be correlated—typically when one partner discloses identity, so does the other.

With that said, we observe substantial heterogeneity by caste when comparing the *Hidden* condition to *First Name Only*. SC participants are able to guess their partner’s caste category with similar accuracy in these two conditions (Table 2, Panel B, column 1). In contrast, for GC participants, there is a large gap: their accuracy in *Hidden* is 15 percentage points lower than in *First Name Only* ($p < 0.01$), amounting to a nearly 30 percentage point loss in accuracy relative to the *Revealed* condition. This disparity suggests that SC individuals are more willing—or more effective—at concealing their identity when prompted to do so, perhaps because they are more practiced at concealing, or because high-caste individuals reveal their identity more readily (deliberately or inadvertently) through other social cues. A comparison of column 1 with column 5 speaks to SCs’ greater sophistication when hiding caste: while both GCs (Panel C) and SCs (Panel B) expect their partners to make worse caste guesses in *Hidden* than in *First Name Only* (column 5), only SCs are correct in this belief (column 1).

How caste is revealed Our treatments affect the strategies by which participants infer the caste of their partner, as demonstrated in Table 3. In *Revealed*, 74% report having used the partner’s last name to guess their caste, versus only 17% in *First Name Only* and 8% in *Hidden* (Panel A, column 2). In *First Name Only*, the most common explanation for how the participant guessed caste is that the partner told them their caste directly (42%). In these cases, participants rarely appear to misrepresent their caste: among those who reported that their partner had told them their caste, 97% correctly identified their partner’s caste at endline. In addition, GCs and SCs are similarly likely to tell their partner their caste, ruling out an equilibrium with unraveling, in which only GCs (as the members of the more advantaged social group) reveal, making the signal from SCs’ silence informative of their lower-caste status.

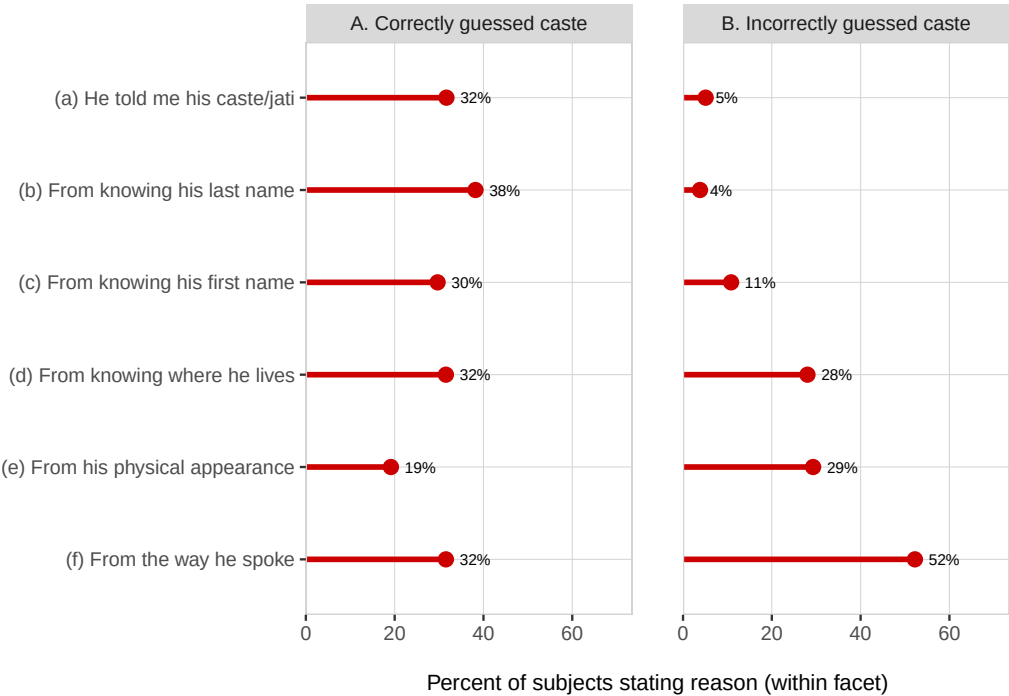
After names, the way the partner spoke (36%) and where the partner lives (35%) were the next most commonly cited reasons given for guesses (excepting the “other” category). In *Hidden*, we see evidence that participants

²⁰More specifically, the wrong guesses of SCs are roughly consistent with a Bayesian guesser with information only on population shares. The wrong guesses of GCs are consistent with a Bayesian guesser with information on population shares in addition to a signal that the partner might be GC.

followed our instructions, as far fewer report that their partner told them their caste directly (15%). The most common answers are that they guessed based on the way the partner spoke (49%) or based on where the partner lives (32%). The frequency of participants organically revealing their caste in *First Name Only* might suggest that the *Hidden* treatment is costly in its paternalism: it prevents a substantial fraction of participants from playing their “best response,” by revealing their caste. We explore this by estimating effects on downstream outcomes in the subsequent sub-sections.

Figure 1 breaks down the reported reasons for caste guesses according to the ultimate accuracy of those guesses. The overall finding is that accurate caste guesses rely disproportionately on direct or high-quality signals, whereas incorrect guesses lean more heavily on noisier, indirect ones. Among correct guesses (Panel A), participants most often mention knowing their partner’s last name (38%), and being told caste directly (32%). By contrast, incorrect guesses (Panel B) are more likely to be based on speech and physical appearance, which are more ambiguous proxies for caste. This suggests that when clear identifiers are unavailable, participants substitute toward weaker heuristics.

Figure 1: Reported reasons for caste guesses, by accuracy of guess.



Notes: The unit of analysis is the participant. The outcomes are binary indicators for whether respondents selected each stated reason in response to the question: “How were you able to guess your partner’s caste category or jati? Even if you are not completely sure about your guess, please tell us what made you guess the way you did. Select all that apply.” Within each panel, the figure shows the percentage of participants citing each stated reason, conditional on whether the participant correctly or incorrectly guessed their partner’s caste.

If participants mentioned physical appearance or speech cues as the basis for identifying their partner’s caste, enumerators asked what specific aspects of these cues informed their judgment. Most responses referred to what

Table 3: Reported cues used to infer partner's caste identity, by treatment and caste group.

	He told me (1)	Used last name (2)	Used first name (3)	Used speaking (4)	Used appearance (5)	Used where lives (6)	Used other (7)
A. Full sample							
First Name Only	0.17*** (0.05)	-0.57*** (0.04)	-0.17*** (0.05)	0.18*** (0.04)	0.13*** (0.03)	0.09** (0.05)	0.33*** (0.04)
Hidden	-0.10** (0.04)	-0.66*** (0.04)	-0.28*** (0.04)	0.29*** (0.04)	0.20*** (0.03)	0.07 (0.04)	0.48*** (0.04)
First Name Only vs. Hidden <i>p</i> -value	<0.01	<0.01	<0.01	0.01	0.08	0.60	<0.01
Revealed mean	0.25	0.74	0.42	0.19	0.10	0.26	0.45
<i>N</i>	1,013	1,013	1,013	1,013	1,013	1,013	1,013
B. SC sample							
First Name Only	0.14*** (0.05)	-0.58*** (0.05)	-0.15*** (0.05)	0.15*** (0.05)	0.14*** (0.04)	0.09* (0.05)	0.32*** (0.05)
Hidden	-0.12*** (0.05)	-0.66*** (0.04)	-0.27*** (0.05)	0.29*** (0.05)	0.22*** (0.05)	0.04 (0.05)	0.46*** (0.04)
First Name Only vs. Hidden <i>p</i> -value	<0.01	0.04	<0.01	0.01	0.12	0.28	<0.01
Revealed mean	0.27	0.76	0.41	0.19	0.13	0.27	0.46
<i>N</i>	506	506	506	506	506	506	506
C. GC sample							
First Name Only	0.20*** (0.05)	-0.56*** (0.05)	-0.18*** (0.05)	0.20*** (0.05)	0.13*** (0.04)	0.09* (0.05)	0.35*** (0.05)
Hidden	-0.08* (0.05)	-0.66*** (0.04)	-0.29*** (0.05)	0.29*** (0.05)	0.18*** (0.04)	0.11** (0.05)	0.50*** (0.04)
First Name Only vs. Hidden <i>p</i> -value	<0.01	<0.01	0.02	0.09	0.25	0.82	<0.01
Revealed mean	0.23	0.73	0.43	0.20	0.06	0.25	0.45
<i>N</i>	507	507	507	507	507	507	507
D. Difference in differences							
First Name Only × GC (vs. SC) <i>p</i> -value	0.14	0.56	0.37	0.36	0.77	0.93	0.62
Hidden × GC (vs. SC) <i>p</i> -value	0.25	0.91	0.50	0.94	0.52	0.13	0.34

Notes: Each column reports estimates from an OLS regression of the outcome on indicators for assignment to the *First Name Only* and *Hidden* treatments, with *Revealed* as the omitted category. Panel D reports difference-in-differences *p*-values from regressions that interact each treatment indicator with an indicator for GC participant (SC is the omitted group); the reported *p*-values correspond to the interaction terms. The outcomes are binary indicators for whether respondents selected each stated reason in response to the question: “How were you able to guess your partner’s caste category or jati? Even if you are not completely sure about your guess, please tell us what made you guess the way you did. Select all that apply”: (1) being told directly (“he told me his caste/jati”); (2) from knowing his last name; (3) from knowing his first name; (4) from the way he spoke; (5) from his physical appearance; (6) from knowing where he lives; and (7) other reasons. Standard errors are reported in parentheses. In Panels A and D, standard errors are clustered at the pair level. In Panels B and C (the SC and GC subsamples), standard errors are heteroskedasticity-robust. All specifications include randomization strata fixed effects and pre-specified controls for participants’ education level and age. **p* < 0.1; ***p* < 0.05; ****p* < 0.01.

their partner said or wore (Appendix Figure A5). Examples include: “He was talking about dairy work on the phone” (a Yadav [OBC]-associated occupation), “he said *Jai Bhim* on the phone” (an SC/Ambedkarite salutation), “he had a *tilak* and *kalawa*” (Brahmin [GC] religious markers), or “he spoke like an educated person.” Although these heuristics are less accurate than explicit identifiers, participants tend to rely on them, and they appear to provide some signal. This may help explain why caste identification rates remain high even in the concealed arms.

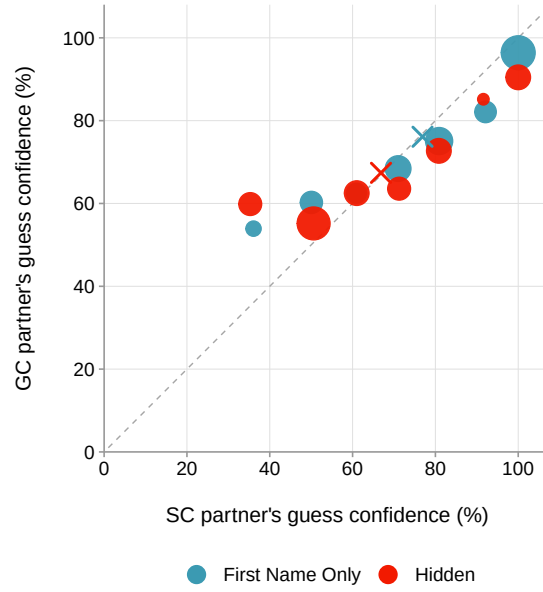
To better understand the high rates of correct caste identification in *First Name Only* and *Hidden*, we estimate a logit model predicting SC versus GC status using characteristics that could plausibly be observed, or quickly learned, in the workplace environment. These include demographics; family characteristics that are commonly discussed in Indian workplaces (such as marital status and household size); income; self-assessed ability; self-consciousness; and independent ratings of clothing, speech fluency, and whether the participant’s Hindi sounded *shudh* (“pure”) or not.²¹ The model achieves 66.3% out-of-sample accuracy in a 50/50 train–test split, helping explain the high accuracy of caste guesses even in the concealment conditions (Appendix Tables A7 and A8). Importantly, clothing quality and voice ratings add little predictive power over and above the other observables. The predictive signal appears to be carried not primarily by how participants look or sound, therefore, but other observable features.

Correlated identity disclosure Worker pairs tend to either reveal their caste identities together or not at all. Pooling across the *First-Name Only* and *Hidden* arms, 231 pairs had neither person tell the partner their caste identity, 86 pairs had both tell it, and only 24 pairs involved one-sided disclosure. Consistent with this process of correlated disclosure, subjects’ confidence in their caste guesses is strongly associated within pairs, for each concealment condition; that is, when the SC partner is confident in his caste guess, the GC partner tends to be confident in his guess also (Figure 2). Such reciprocity may partly reflect uncertainty about disclosure norms. When interacting with a stranger, individuals may not know whether their partner regards caste disclosure as appropriate, unwelcome, or risky. They may therefore hesitate to initiate disclosure, even when they would willingly reciprocate it. We see evidence for this in our labor-stand survey: Hindu respondents are 30 to 39 percentage points more likely to report that they would share their caste or ask the partner’s caste if the partner went first than if the partner had not already shared or asked.

Such reciprocity norms inherent in conversations may in turn help explain why caste knowledge is symmetric across SCs and GCs in *First Name Only*, where caste can be learned about through talking, but asymmetric in *Hidden*, where conversation about caste is proscribed (a rule that was largely followed).

²¹The latter ratings were provided by three Indian research assistants, based on a photograph taken on the work day and two short voice recordings: one from a common prompt administered to all participants, and one from a participant-specific response describing future plans.

Figure 2: Correlation between partners' confidence in guesses about each other's caste.



Notes: This figure plots, for each worker pair, each partner's confidence in their guess of the other's caste identity. The GC partner's confidence in their guess about their SC partner is given on the vertical axis, and the SC partner's confidence in their guess about their GC partner is given on the horizontal axis. Nearby values are grouped into bins, with point size proportional to the number of pairs in each bin. The crosses (x) denote arm means for the *First-Name Only* and *Hidden* treatments.

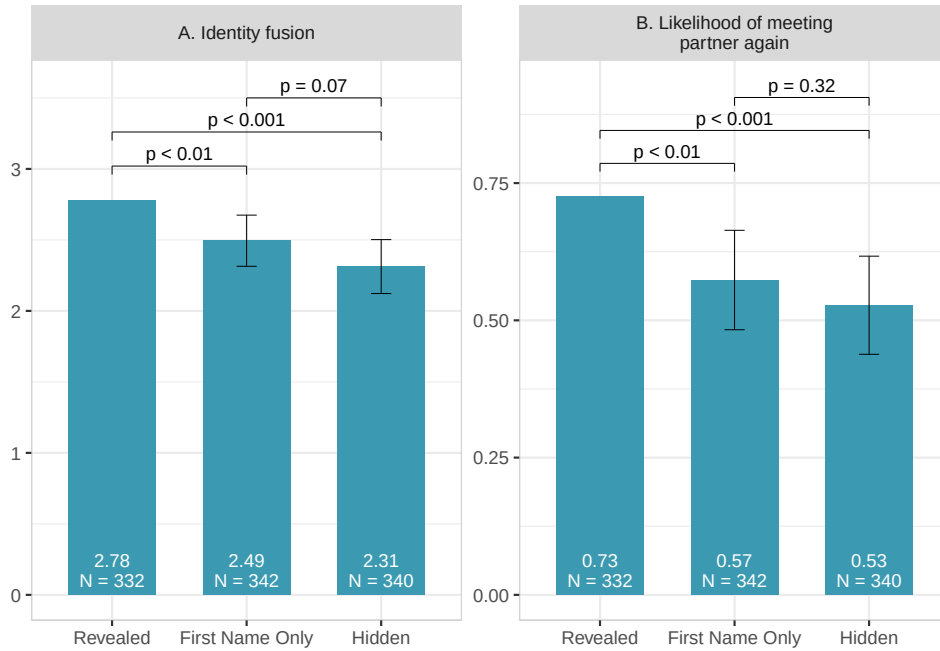
Social relations We now consider the first family of downstream outcomes. Recall the competing predictions of the discrimination and coordination hypotheses: the former predicts identity concealment to improve intergroup relations, while the latter predicts negative effects.

Withholding surnames worsens worker relations, even when workers remain free to disclose their identity (Figure 3). Compared to *Revealed*, participants in *First Name Only* are less likely to report feeling close with their partner, using the 0 to 4 scale of identity fusion (Panel A, $\hat{\beta} = -0.29$ or 0.32σ , $p < 0.01$), and 15 percentage points less likely to report expecting to meet up with their partner again ($p < 0.01$), off a high base of 73% (Panel B).²² These are large effects, especially given that the *First Name Only* treatment is subtle—merely the use of first names rather than full names—and *First Name Only* participants can in principle replicate the information content of the *Revealed* condition by simply telling their full name to their partner. Social relations worsen still further in the *Hidden* condition, consistent with the stronger effects of *Hidden* on caste knowledge, though these declines are smaller than the effects of moving from *Revealed* to *First Name Only* ($p = 0.07$ for identity fusion and $p = 0.32$ for expectation of future interaction).

For an incentivized measure of worker relations, we offered subjects the choice between working another day with their partner, versus a same-caste stranger who had participated previously in the research, and varied the wage

²²This high base cannot be attributed to pre-existing relationships: only 3% of participants in the sample reported encountering or having any knowledge of their work partner in the past.

Figure 3: Estimated treatment effects on social relations.



Notes: The unit of analysis is the participant. The outcomes are: (A) Which picture best describes your relationship with your work partner today? (0 = self and partner circles fully apart to 4 = self and partner circles fully overlapping); (B) Do you think you will meet up with your partner again in future? (0 = No, 1 = Yes). The *Revealed* bar in each panel shows the mean of the outcome variable for *Revealed* participants. The *First Name Only* and *Hidden* bars add the corresponding treatment effects, estimated from regressions that control linearly for participants' age, and include randomization-strata and education-bin fixed effects. The 95% confidence intervals are based on standard errors clustered at the pair level. Brackets report two-sided p -values for pairwise differences across arms.

offered across these two choices. We started with a wage of Rs. 400 ($\approx$ \$4) for both choices and then gradually increased the offered wage for the less-preferred choice, to ascertain their switching point. Table 4 presents Tobit results where the outcome variable is the wage premium required to switch from working with the partner to working with the stranger, with a larger wage premium reflecting stronger partner relations.²³

Subjects in the *Revealed* condition would prefer to work with their partner rather than with a same-caste stranger on average, requiring a Rs. 173 wage premium to switch (omitted mean, Table 4, column 1). The *Hidden* treatment harms partner relations, reducing the wage premium by 21%, or Rs. 37 ($p = 0.08$). This is driven primarily by GC participants (column 2, Rs. 43, $p = 0.08$) and less so by SCs (column 3, Rs. 30, $p = 0.18$). The effects for *First Name Only* are relatively small and not statistically significant, perhaps because subjects retain the possibility of learning about their partner's identity—much as they might with a stranger—leaving them more willing to continue working with their partner relative to the *Hidden* condition, where this opportunity is foreclosed.

²³If a subject reported indifference at some step, then this variable is equal to the midpoint between that wage and the next higher wage premium.

Table 4: Estimated treatment effects on the daily wage participants are willing to sacrifice to work again with partner rather than same-caste stranger.

	Full sample (1)	GC sample (2)	SC sample (3)
First Name Only	-4.07 (21.59)	-7.21 (25.16)	-0.56 (22.76)
Hidden	-36.88* (21.33)	-43.38* (24.40)	-30.48 (22.64)
First Name Only vs. Hidden p -value	0.12	0.14	0.19
Revealed mean	173	171	175
N	1,012	507	505

Notes: The unit of analysis is the participant. The outcome is the wage (in Rs.) that a participant would be willing to sacrifice to work with their partner again, rather than with a same-caste stranger. Tobit regressions control linearly for participants' age, and include randomization strata and education-bin fixed effects. Standard errors are clustered at the pair level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

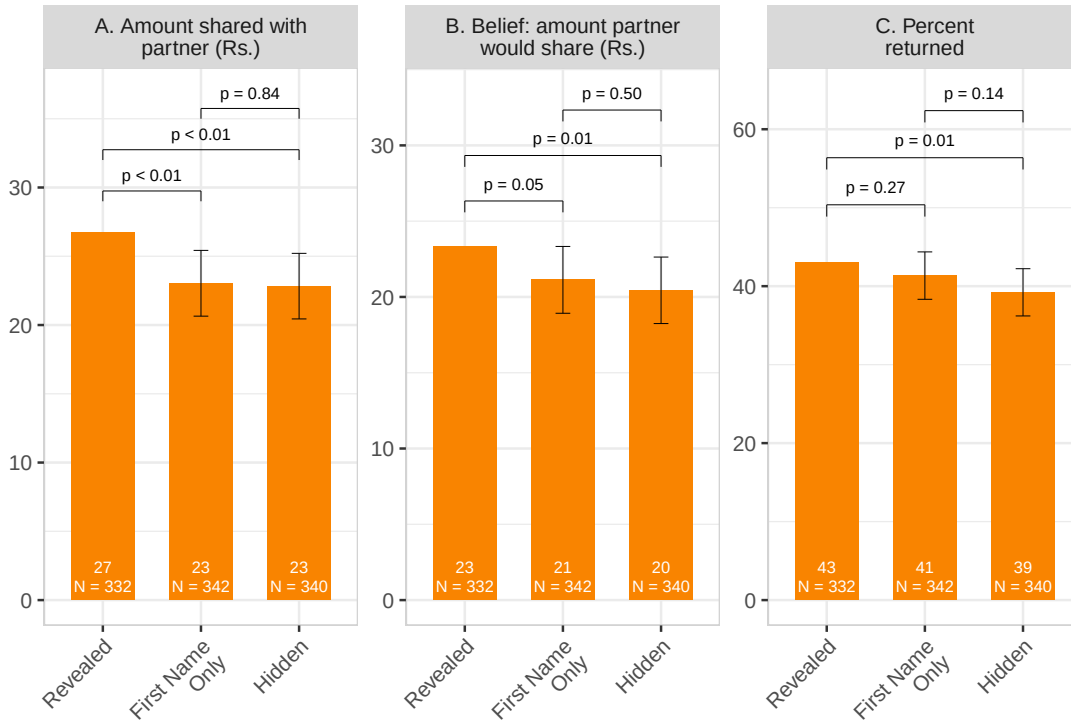
Trust Caste concealment also harms trust. Participants in the *Revealed* condition share Rs. 27 of their Rs. 50 endowment on average (Figure 4, Panel A).²⁴ Both the *First Name Only* and *Hidden* conditions reduce trust giving by roughly 15% (both $p < 0.01$). Meanwhile, the *First Name Only* and *Hidden* arms are statistically indistinguishable from one another ($p = 0.84$).

The similarity of the effects produced by the *First Name Only* and *Hidden* manipulations is telling. One concern is that directing participants to actively conceal their identity—as in *Hidden*—creates an artificially cagey setting, and that this is what dampens trust. But the *First Name Only* treatment involves no such instruction; rather, it withholds full caste-identifying information in a natural, unobtrusive way, yet the reduction in trust is near-identical. Thus, the decline in trust does not require explicit instructions to conceal caste.

Negative effects on Sender behavior could be driven by beliefs (expecting the partner to return less) or preferences (feeling less altruistic toward the partner). To unpack these channels, we turn to our trust game measures of beliefs and Receiver behavior. Participants in the *Revealed* condition expect their partner to share Rs. 23 of their endowment if selected as the Sender, compared to Rs. 21 and Rs. 20 in the *First Name Only* and *Hidden* conditions, respectively ($p = 0.05$ and $p = 0.01$ for the comparisons with *Revealed*). Once again, there is no significant difference between the *First Name Only* and *Hidden* treatment arms. The decline in transfers when identities are more opaque seems at least partly due to diminished expectations of reciprocity, therefore. Nevertheless, we also see a role for preferences: Panel C of Figure 4 examines treatment effects on actual reciprocity, measured as the percentage of the tripled amount that participants would return, averaged across all possible transfer levels. Participants

²⁴There is no statistically significant difference in the amounts sent by SC and GC participants.

Figure 4: Estimated effects on trust game outcomes.



Notes: The unit of analysis is the participant. The outcomes are: (A) the amount (Rs. 0–50) sent to the partner when the participant is Player A; (B) the participant’s belief about the amount (Rs. 0–50) the partner would send if selected as Player A; and (C) the percentage of the tripled amount the participant (as Player B) would return to their partner, averaged across all possible transfer levels. The *Revealed* bar in each panel shows the mean of the outcome variable for *Revealed* participants. The *First Name Only* and *Hidden* bars add the corresponding treatment effects, estimated from regressions that control linearly for participants’ age and include randomization-strata and education-bin fixed effects. The 95% confidence intervals are based on standard errors clustered at the pair level. Brackets report two-sided p -values for pairwise differences across arms.

in *Revealed* return 43% on average, compared to 41% in *First Name Only* and 39% in *Hidden*. The reduction in reciprocity is statistically significant for the *Revealed* versus *Hidden* comparison ($p = 0.01$) but not for the other two possible comparisons. For the case of reciprocal behavior, only the explicitly instructed hiding condition produces a detectable decline.

Taken as a whole, we see that treatments masking caste identity have measurable efficiency costs, cutting the additional surplus generated in the trust game by around 15%. Reciprocity also declines, with participants returning a smaller share of a smaller pie.

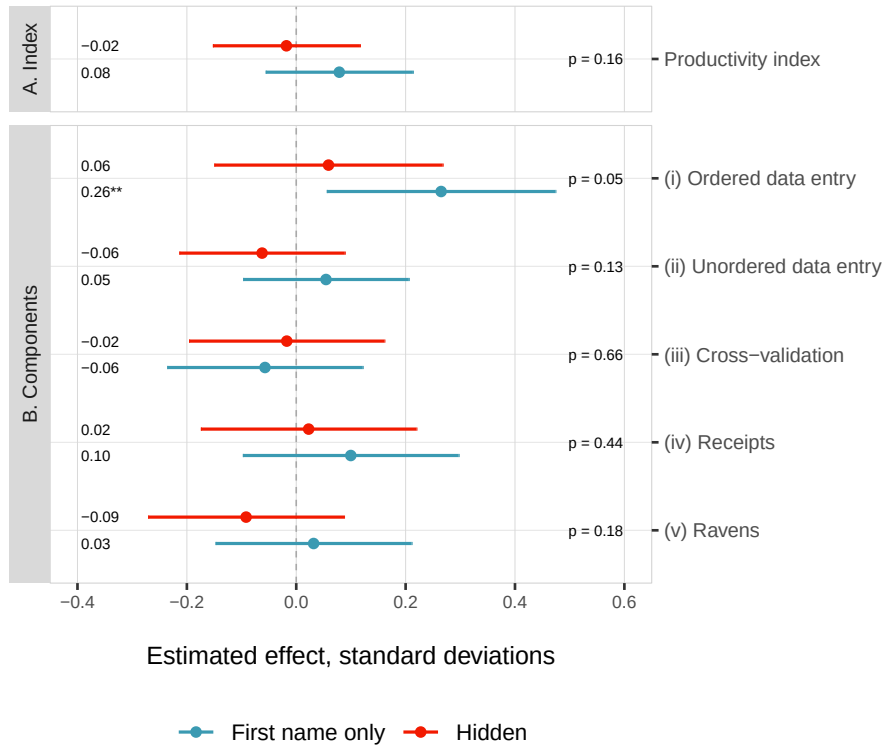
Heterogeneity by caste According to the pooled results presented so far, caste concealment harms relationships and trust. At first glance, this appears to support the coordination hypothesis, since identity uncertainty makes interactions harder rather than easier. But pooled effects could obscure heterogeneity. The discrimination hypothesis predicts that SC-reported relations should benefit from concealment because it shields them from discrimination; while if GCs have a preference for the hierarchy enabled by their caste status, they may be hurt more by concealment.

Appendix Table A5 finds that *First Name Only* and *Hidden* reduce identity fusion, willingness to meet the partner again, and trust for *both* GC and SC participants, firmly corroborating the coordination hypothesis. At the same time, the effects are more negative for GCs, with the difference statistically significant at at least the 10% level for two of the six tests. In addition, Table 4 showed previously that the negative effects on willingness to work together are also larger for GCs. These results suggest that while identity concealment undermines relations for both groups, the costs may be somewhat greater for the higher-status group in this context (GCs), consistent with a GC preference for hierarchy—albeit a limited one—alongside the more powerful coordination effects.

Productivity The harms to worker relations may be less concerning if accompanied by productivity and income gains. Do weaker social relations bring about a greater focus on work, as shown in Park (2019)? Figure 5 reports treatment effects on team productivity across the five tasks, and on an index measure summarizing those five outcomes. Neither the *First Name Only* nor *Hidden* condition produces a statistically significant change in overall productivity, and the difference between the two treatments is small (Panel A). Disaggregating into task components (Panel B) reveals only limited effects. The *First Name Only* condition increases productivity in the ordered data entry task, the simplest task, and the one performed first,²⁵ relative to both *Revealed* and *Hidden*. For all other tasks, however, the estimated treatment effects are small. The deterioration in social relations is therefore not accompanied by a statistically detectable increase in overall productivity.

²⁵One interpretation is that the “first stage” on beliefs about caste identity is largest at the start of the day, explaining why we only see a productivity effect on the first task. But multiple hypothesis testing concerns are also plausible: of 12 tests in Figure 5, only one is statistically significant.

Figure 5: Estimated treatment effects on team production.



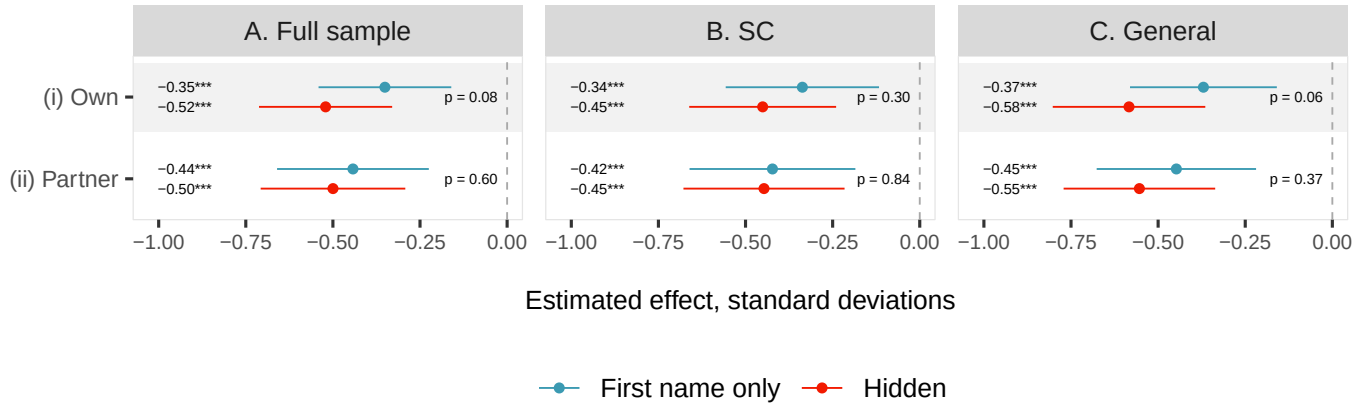
Notes: This figure plots treatment effects on team productivity. All outcomes are counts that measure the number of correctly inputted items for each task. Counts are standardized to z -scores using the mean and standard deviation of the variable in the *Revealed* condition. The index is the simple average of the five standardized component outcomes. All regressions include date and education fixed effects, the average age of the pair, as well as mean baseline levels of productivity for the pair in both reading and typing. Standard errors are clustered at the pair level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

While productivity is not affected by caste concealment, even with a limited sample of same-caste teams, we do see some evidence that team caste composition matters. Within the *Revealed* condition, same-caste teams have 0.16 standard deviations higher productivity than mixed-caste teams ($p = 0.11$, Appendix Figure A6).²⁶ This difference is consistent with other evidence from India that socially proximate teams perform better in collaborative tasks (Ghosh, 2025; Afridi et al., 2024).

Strikingly, the lack of effects on *actual* productivity (Figure 5) contrasts sharply with Figure 6, which shows large negative effects on *perceived* productivity. Participants in both the *First Name Only* and *Hidden* conditions rate both their own and their partner's productivity substantially lower, by roughly one-third to one-half of a standard deviation relative to *Revealed*, with especially large effects among GC participants. The cost of concealment thus also operates through misperception: when caste identity is uncertain, participants appear to interpret the same work interaction less favorably and infer lower competence from it. These less favorable assessments may have implications for future economic ties, including workers' willingness to collaborate again and their reservation wages for doing so.

²⁶The largest task-specific difference is in *receipts*, at 0.31 standard deviations ($p = 0.02$).

Figure 6: Estimated treatment effects on perceptions of own and partner productivity.



Notes: The figure reports estimated treatment effects on perceived productivity, measured separately for respondents' own performance and that of their partner. Outcomes are standardized to z-scores using the mean and standard deviation of the variable in the *Revealed* condition. All regressions control linearly for participants' age and include randomization-strata and education-bin fixed effects. Standard errors are clustered at the pair level. Horizontal bars denote 95% confidence intervals. Reported p -values correspond to tests of equality between the *First Name Only* and *Hidden* coefficients. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

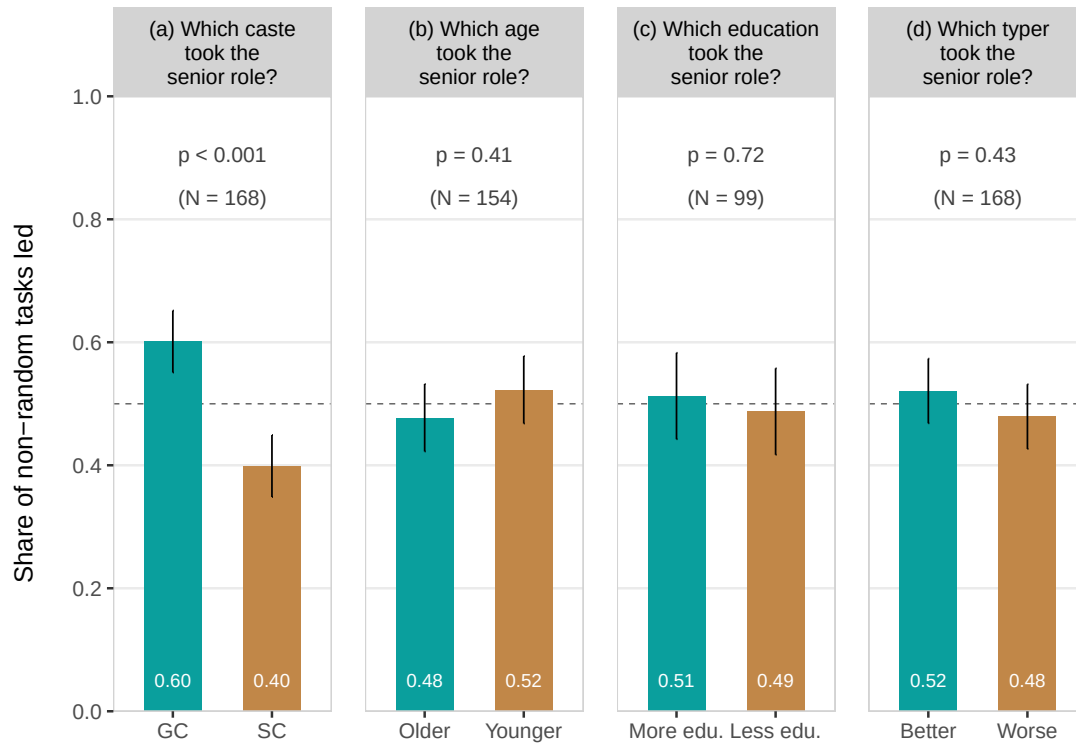
Role allocation The discrimination channel for identity-hiding makes a clear prediction: caste discrimination should be lower when caste cues are removed. We explore this idea by turning to data on ability and role assignment.

GCs and SCs perform similarly on baseline reading and tablet-typing tasks, which were designed to measure the skills required in the final workplace environment (Appendix Figure A9). The distributions for GC and SC participants substantially overlap, with GC participants having only modestly higher mean percentiles for both tasks (reading: 0.52 vs. 0.48; typing: 0.51 vs. 0.49). Despite similar ability by caste, GC participants take the senior role for 60% of tasks in *Revealed*, significantly above the 50% fair-split benchmark ($p < 0.001$, Figure 7, Panel (a)). This large caste disparity also holds when flexibly controlling for ability (Appendix Table A6). Caste identity, then, appears to be the key determinant of role assignment—we see no evidence of the senior role being taken by the older, more educated, or higher-ability partner (Figure 7, Panels (b)–(d)).

The removal of caste cues has no impact on the caste disparity (Table 5, column 1). The coefficients on both the *First Name Only* and *Hidden* conditions are small and statistically indistinguishable from zero. This suggests that caste discrimination—in which the GC partner pushes the SC partner into the junior role because of their lower-caste status—is not impacted by the removal of caste cues.²⁷ Similarly, the caste-concealment treatments do not give rise to meritocracy: they do not affect the probability of tasks being allocated to maximize total team productivity, which

²⁷Of course, given that the allocation of roles is determined by the worker pair jointly, a related mechanism would be if the SC partner pushes the GC partner into the senior role because of their higher-caste status. Such a mechanism could reflect the SC's internalization of the caste hierarchy, or the SC pre-empting a backlash due to their expectations of caste discrimination. We think of these mechanisms as also falling within the discrimination channel.

Figure 7: Determinants of senior partner role assignment in the *Revealed* condition.



Notes: This figure reports how senior partner role assignment in the *Revealed* condition varies by four partner characteristics. Each panel shows, for one within-pair comparison, the share of the four tasks in which partners selected the senior role that was led by each partner type. The two bars in a panel are complements. The sample is restricted to GC-SC pairs. The samples in the age and education panels are restricted to pairs that differ on the relevant characteristic. The dashed line marks equal assignment. Error bars show 95% confidence intervals. p -values test whether the “higher-status” partner’s share differs significantly from 0.5.

Table 5: Estimated treatment effects on senior partner role assignment.

	GC is senior (1)	Optimal typer is senior (2)
First Name Only	0.00 (0.03)	0.02 (0.04)
Hidden	-0.00 (0.03)	0.01 (0.04)
First Name Only vs. Hidden p -value	0.83	0.81
Revealed mean	0.60	0.48
N	2,028	2,028

Notes: Each column reports estimates from a linear regression. The unit of analysis is the pair-task. Tasks for which the senior role was randomly assigned (by the computer) are excluded. The outcome in column (1) is an indicator for whether the GC participant occupied the senior partner role. The outcome in column (2) is an indicator for whether the worker assigned to the senior (typing) role is the optimal typist. The optimal allocation assigns the senior role to the worker who maximizes total team productivity, defined as the assignment that yields the higher sum of within-sample percentile ranks in baseline reading and typing performance across the pair. All specifications include randomization-strata effects. Standard errors, clustered at the pair level, are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

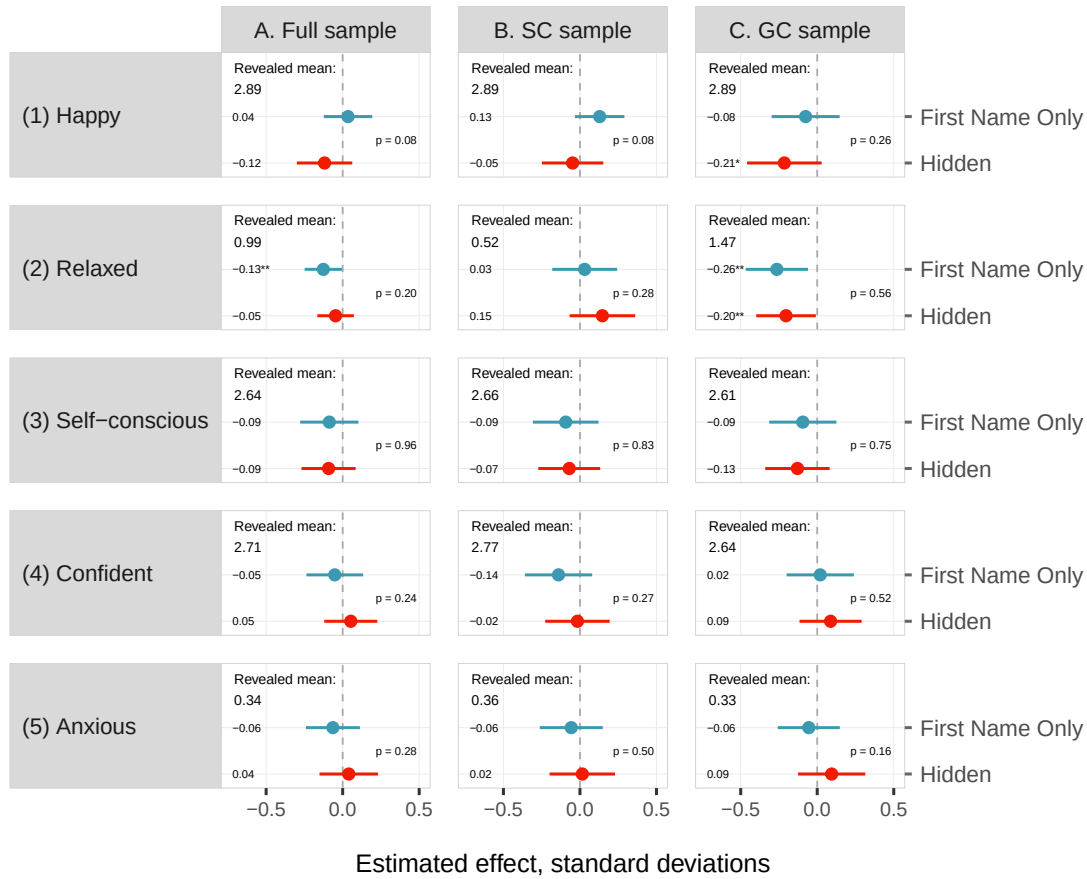
we define as the role assignment that yields the higher sum of within-sample percentile ranks in baseline reading and typing performance across the pair (column 2).

Our findings suggest that the caste disparity in task allocation is driven less by social perception (of caste identity), and more by self-perception. Indeed, GCs self-assess as more productive in the typing (i.e., senior) task despite similar measured ability, and they report being more relaxed during the interaction (Appendix Figure A8). These differences may help explain why they are more likely to be assigned the senior role. Finally, the caste disparity in role allocation appears to be accepted by participants: pooling all treatment groups, 92% of GCs and 92% of SCs report at endline that they were happy with how the roles were assigned.

Well-being at work Beyond worker relations and productivity, one way to capture the welfare effects of identity concealment is the well-being felt by the participants at the workplace. Treatment effects on self-reported well-being tend to be small and not statistically significant (Figure 8). Overall, concealing caste identity does not substantially affect participants' subjective experience of the interaction along these affective dimensions.

We note one exception: there is some evidence that identity hiding has a more positive effect on happiness and feeling relaxed for SCs than for GCs, consistent with priors—under the discrimination hypothesis—that the marginalized group has more to gain (and less to lose) from the removal of caste cues. For GCs, we see negative effects: both the *First Name Only* and *Hidden* conditions reduce feelings of being relaxed at work (both $p < 0.05$), while the *Hidden* condition also reduces happiness ($p < 0.1$). Among the set of affective dimensions for which we detect effects, therefore, the costs of concealment appear to be borne more by GCs.

Figure 8: Estimated treatment effects on well-being.



Notes: Each facet panel reports estimates from an OLS regression of an outcome on indicators for assignment to the *First Name Only* and *Hidden* treatments, with *Revealed* as the omitted category. Outcomes are based on responses to the question, “At work today, to what extent did you feel...[word indicated on each row]” (0 = Not at all, 1 = A little, 2 = Some, 3 = Very). In each facet, “Revealed mean” shows the average score on the original 0–3 point scale for the *Revealed* group. Treatment effects are standardized. In Panels A, standard errors are clustered at the pair level; in Panels B and C (the SC and General subsamples), standard errors are heteroskedasticity-robust. All specifications include randomization strata fixed effects and pre-specified controls for participants’ education level and age. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Mechanisms

Why does the removal of identity cues harm worker relations? We find evidence supportive of the coordination hypothesis described in the introduction. We argue that the identity uncertainty induced by our concealment treatments makes conversation and self-presentation harder to navigate, given that participants are unsure who the person sitting across from them “really is.” In contexts where misreading identity is socially consequential—because caste creates norms around hierarchy, ritual purity, and social distance—uncertainty may produce guardedness and self-monitoring. We now present evidence for this interpretation.

Authenticity and guardedness The loss of identity cues makes interactions feel less authentic and less friendly. In Figure 9, we analyze a set of pre-registered secondary outcomes capturing participants’ subjective experiences during the interaction itself. Participants in *First Name Only* and *Hidden* report feeling substantially less able to show their “true self” to their partner (“authenticity” from now), with declines of between 0.24 and 0.36 σ relative to *Revealed* (row (i), $p < 0.01$). Concomitantly, concealment also reduces perceptions that the partner was friendly by between 0.25 and 0.45 σ (with *Hidden* producing a measurably larger fall, row (ii)). Across the board, the treatment effects are similar for SCs and GCs, consistent with a failure of conversational coordination that hurts all parties. In contrast, we do not find effects of concealment on overtly adversarial behaviors, such as displays of rudeness, bossiness, or tension (rows (iii)-(v), Figure 9). Rather than producing openly hostile or discriminatory interactions, identity-hiding appears to make participants more guarded, superficial, and cautious—inhibiting their willingness to interact naturally and comfortably with one another.

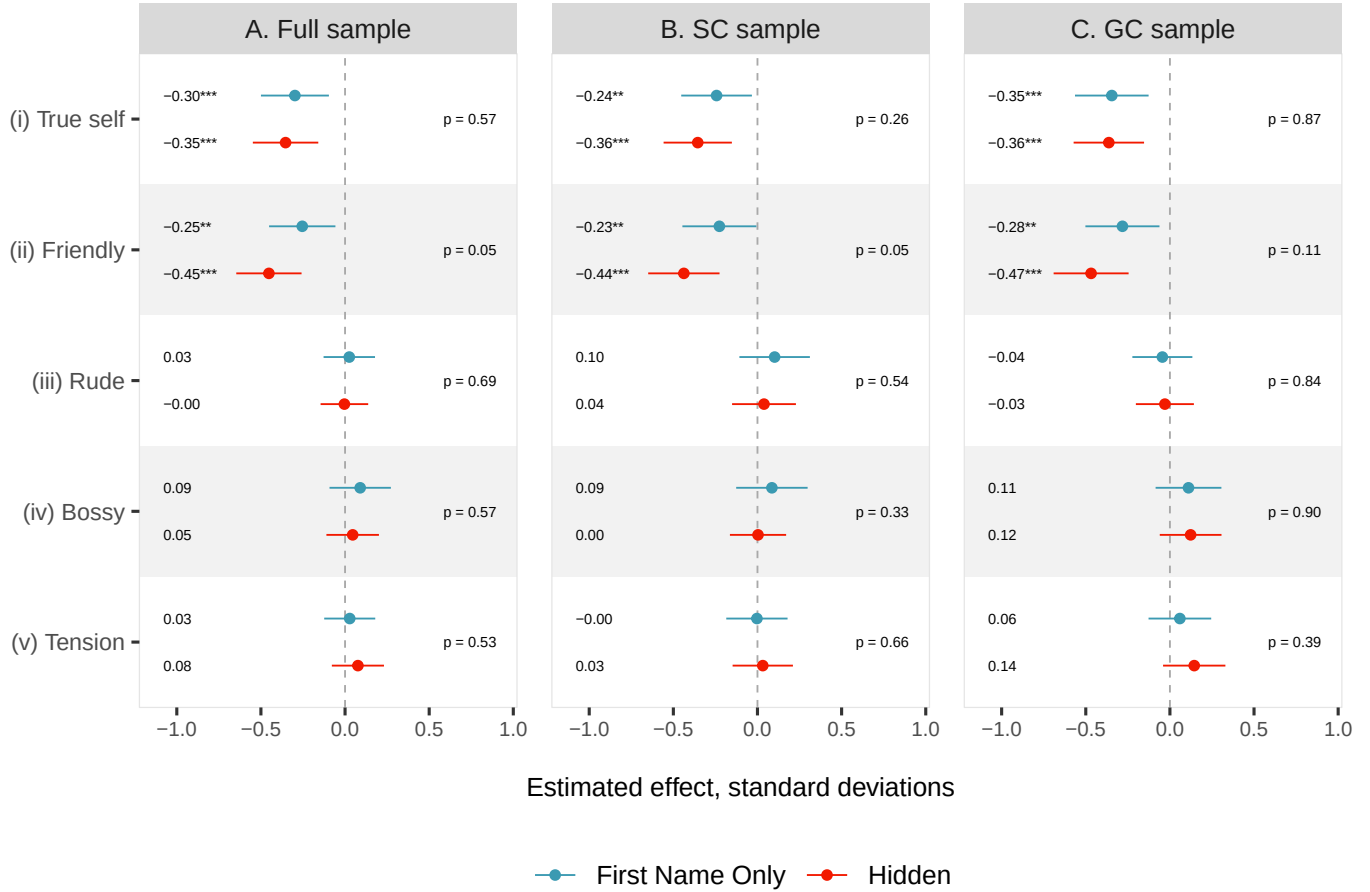
Our labor-stand surveys help explain why caste uncertainty might matter for these interactions. Among respondents who said they would ask a work partner’s caste, 22% said it would help them know how to interact, and 11% said it would help build a friendship (Appendix Figure A17). Twenty-six percent gave reasons involving restrictions on interactions such as sharing food with lower castes. These accounts clearly indicate that caste information shapes expectations about appropriate interaction, even if workers do not regard it as directly relevant for completing the job. They are consistent with our interpretation that uncertainty makes conversation and self-presentation harder to navigate, while also showing that some of the social conventions being navigated are discriminatory.

If caste helps structure how individuals interpret and manage social interactions, ambiguity about caste should be most disruptive for those who view caste as an important part of their personal identity. The results in Appendix Figure A10 chime with this interpretation. We estimate treatment effects of concealment, pooling *First Name Only* and *Hidden*, by the baseline importance participants attach to caste relative to other identities (being Indian, Hindu, from Uttar Pradesh, and personal relationships such as being a father, brother, or son). Participants who rank caste among their top two identities experience a negative effect on authenticity that is nearly twice as large as the effect among those who rank caste last.²⁸ We see a similar pattern for anxiety: because lower ranks indicate greater caste salience, the negative slope we observe (-0.087, $p < 0.1$) implies that anxiety rises more under concealment among participants for whom caste is more salient. These patterns suggest that concealment is especially disruptive for participants for whom caste is an important part of how they see themselves and navigate interactions with others.

Confidence We next probe the role of uncertainty directly, by investigating whether social relations are closer when participants are more confident about their partner’s caste identity. Appendix Table A10 reports treatment

²⁸We pool the first two ranks because there are few observations in the first category for caste.

Figure 9: Estimated treatment effects on subjective experiences during the interaction.



Notes: Each facet panel reports estimates from an OLS regression of an outcome on indicators for assignment to the *First Name Only* and *Hidden* treatments, with *Revealed* as the omitted category. Outcomes are self-reported measures of interaction quality: (i) ability to show one's true self (1 = not at all, 10 = completely); (ii) partner friendliness (1 = very unfriendly, 10 = very friendly); (iii) partner rudeness (1 = not at all rude, 10 = very rude); (iv) partner bossiness (1 = not at all bossy, 10 = very bossy); and (v) experienced tension (1 = no tension, 10 = a lot of tension). Treatment effects are standardized. In Panels A, standard errors are clustered at the pair level; in Panels B and C (the SC and GC subsamples), standard errors are heteroskedasticity-robust. All specifications include randomization strata fixed effects and pre-specified controls for participants' education level and age. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

effects on our core social outcomes before and after controlling for participants' confidence in their caste guess and their beliefs about the partner's caste. Panel A replicates the main experimental results: relative to *Revealed*, both *First Name Only* and *Hidden* substantially depress the amount of money sent in the trust game, identity fusion, willingness to meet again, and authenticity. Panel B adds controls for caste-guess confidence, rescaled from 0 to 1, along with interactions between the respondent's caste and their guess about their partner's caste. Two findings stand out. First, confidence is strongly associated with better outcomes. Moving from no confidence to full confidence in one's caste guess is associated with sending Rs. 19 more in the trust game, reporting 1.6 more units of identity fusion and a 50 pp higher likelihood of meeting up again, and scoring 5.9 points higher on feeling like one's true self (all $p < 0.01$). Second, the treatment effects largely disappear once we control for caste-guess confidence: all but one of the coefficients on *First Name Only* and *Hidden* become statistically indistinguishable from zero. Since confidence is measured post-treatment, this should not be read as causal mediation. But it is the regularity we would expect if concealment worsened social relations by generating uncertainty about a caste identity.²⁹

We explore the role of certainty non-parametrically in Appendix Figure A11. Restricting attention to participants in the concealment arms, we examine how social outcomes vary with confidence in the partner's caste identity, using seven bins: below 50%, 50-59%, 60-69%, ..., 90-99%, and 100%, and including our pre-registered controls, caste-guess controls, and a dummy for the *Hidden* treatment, making all comparisons within-treatment. As above, we stress that this exercise is correlational, and thus more exploratory than our comparisons across treatment groups.

The confidence-relations relationship is non-linear: outcomes improve discretely when participants report being 100% certain about their partner's caste. Participants who are fully certain show higher trust, greater identity fusion, and a higher perceived likelihood of meeting their partner again than those who are only partially certain—with especially discrete jumps at complete certainty for identity fusion and trust. This “certainty premium” hints that even a seed of doubt about group identity may be enough to keep participants guarded.³⁰ This would align with a model in which the social costs of misreading identity are large, making participants err on the side of caution, rather than proceeding as if their caste guess is correct.³¹ Future experiments could test for this idea causally: with a *Certain* treatment group, in which participants are told that their partner is an outgroup member, along with a *Near Certain* treatment group, in which participants are told that their partner was randomly selected from a pool of 19 outgroup members and one ingroup member.

²⁹Further, using the mechanism test in Kwon and Roth (2026), we fail to reject that caste-guess confidence—binned into seven categories (0–50%, followed by 10 percentage-point increments)—fully mediates the treatment effects.

³⁰We note also a parallel to the work on risk preferences of Callen et al. (2014). They find a preference for certainty in violation of expected utility theory.

³¹One useful analogy is that of being 99% confident in knowing the first name of a conversation partner. Given large social costs of misnaming an individual, the 1% doubt may well be enough to deter one from attempting to introduce the partner to others. In the case of imperfect knowledge of caste identity, 1% of doubt may be enough to avoid conversational topics and norms that would fit well with one's guess of the partner's caste.

Lastly, we also find that instances of endogenous identity resolution are associated with higher trust (Appendix Table A11). Participants who report having asked their partner's caste send more in the trust game and expect their partner to send more. These associations are stronger among GC participants, in line with higher-caste participants being more uncomfortable when partner identity remains uncertain. The evidence also suggests that identity resolution can reassure SC participants: they send more to GC partners when the partner has learned their caste. Overall, these correlations point in the same direction as the experimental results: interactions are more trusting when uncertainty about caste identity is resolved.

Alternative explanations

Other information conveyed by surnames Indian surnames may convey information beyond group identity, and such information might explain some treatment effects. Three pieces of evidence suggest that it is the revelation of group identity that matters. First, as we discussed above, labor-stand survey respondents report that our main experiment surnames reveal caste above all else, while other information revealed is plausibly caste-correlated—i.e., downstream from the revelation of group identity (Appendix Figure A3). Second, treatment effects do not vary by partner's surnames once we allow them to vary by partner's jati (Panel A, Appendix Figure A14). Pooling the two concealment conditions, we cannot reject equal effects across all surnames within a jati ($p = 0.43$). Third, participants who correctly guessed their partner's surname at the endline scored 0.36σ higher on an index of social relations ($SE = 0.11$; the index averages trust sent, identity fusion, and expecting to meet again). However, once we control for their belief about their partner's caste and their confidence in those beliefs, this association falls to -0.04σ ($SE = 0.11$) and loses statistical significance, while confidence remains strongly associated with the index (Panel B). Though this comparison is descriptive, it suggests that surname knowledge matters through what it reveals about group identity.

Traceability Knowledge of surnames might make partners more traceable, which might affect social relations. For example, the higher trust in *Revealed* could reflect a fear of being tracked down rather than genuine liking (though it is less intuitive that traceability would affect the private survey measures, like identity fusion). If operative, we would expect a weaker correlation between trust game giving and identity fusion in *Revealed*, where giving is more reflective of fear than liking, than in *First Name Only*. We do not find this. Trust game giving is similarly positively correlated with identity fusion in *Revealed* and *First Name Only* (0.27 and 0.23; $p < 0.001$ for both and $p = 0.48$ for the difference). A traceability channel for trust also seems unlikely given the cost and effort of tracking a partner down relative to the few rupees at stake.

For an additional test of the traceability hypothesis, we turn to the SECC data. This data shows that households sharing participants' surnames are spread across many villages (for 12 of the 14 most common surnames, no single village holds more than 5% of the households sharing that surname). In any case, if traceability did explain the benefits of revelation, we would expect larger benefits of revelation in cases where the partner's full name is shared by fewer people—it is easier to trace a past partner with an unusual name (e.g., Shreyansh Venkataraman) than one with a common name (e.g., Rahul Kumar). We measure partner traceability as the factor by which their surname shrinks the set of men with his name: log ratio of male household heads in Uttar Pradesh who share his given name to those who share his full name (from SECC data). The interaction between a pooled concealment treatment indicator and this measure is -0.02σ (SE = 0.06) and not statistically significant. And once we allow interactions with jati and surname as well, it rises to 0.26 (0.17) and 0.19 (0.18)—also not statistically significant and positive in sign, which is the opposite of what the traceability hypothesis predicts (Panel C).

Anonymity and information exchange One possibility, subtly different from our proposed mechanism, is that concealment harms social relations simply by reducing the set of topics that participants discuss. Since caste is correlated with many other characteristics in this setting, instructions to hide caste may also restrict discussion of education, family background, religion, marriage, or other identity-linked topics that people use to build rapport. Appendix Figure A12 shows evidence for this in the *Hidden* condition: participants are less likely to ask directly about caste and also ask less about several other identity-linked characteristics. These participants are also less likely to report having discussed a range of topics (Appendix Figure A13).

However, reduced conversational scope cannot fully explain the negative effects of concealment. In *First Name Only*, participants are *not* instructed to avoid caste-revealing information, and many use this discretion. Caste is asked more often than in *Revealed*, where surnames already reveal caste (row C, Appendix Figure A12). More generally, conversations in *First Name Only* are broad and look much closer to *Revealed* than to *Hidden* in terms of topics discussed (see Appendix Figure A13). Yet worker relations remain worse in *First Name Only* than in *Revealed*. The negative *First Name Only* effects therefore do not require the broad restrictions on conversation observed in *Hidden*, consistent with identity concealment changing how participants navigate an interaction even when the reported range of topics is similar. This pattern, however, is not consistent with a generic anonymity effect, in which prosociality increases simply in response to concrete information about a partner (Small and Loewenstein 2003; Andreoni and Rao 2011; Goette et al. 2012).

Experimenter demand and salience The *First Name Only* condition does not mention caste, limiting the possibility of demand effects, and yet produces negative effects that are similar to those in *Hidden*, where caste-related

disclosure is explicitly restricted. Furthermore, only 2.4% of subjects correctly guessed the study's purpose, with no effects of treatment (see Appendix Figure A15 and Appendix Table A12). Of course, the larger penalties sometimes observed in *Hidden* may reflect the added effects of a narrower conversational scope and greater caste salience, on top of identity uncertainty.

Restoration of caste hierarchy We also consider whether the benefits of revelation primarily reflect the restoration of upper-caste status advantages. This explanation would predict that the benefits of revelation are mainly concentrated among GCs, who benefit most from the existing hierarchical order. However, the negative effects of concealment are large for both GCs and SCs. Revelation also does not increase the rate at which GCs take the senior partner position. These findings suggest that the benefits of revelation extend beyond upper-caste status advantages, while remaining consistent with both groups finding interactions easier to navigate when identities are legible.

Beliefs about team composition Another possibility is that the treatment effects are driven by beliefs about team composition. In *Revealed*, participants *know* that they are in GC-SC pairs. In *First Name Only* and *Hidden*, they assign some probability of being paired with a same-caste partner, and some probability of being paired with an OBC partner, closer to them in the caste hierarchy than their actual partner. But this cannot explain the negative effects of concealment. If anything, believing that one's partner's caste is less distant should *improve* social relations. Consistent with this, Appendix Table A9 shows that homogeneous-caste teams included for obfuscation exhibit higher trust and greater identity fusion than GC-SC teams when identity is revealed, while as we showed earlier, homogeneous-caste teams are more productive (Appendix Figure A6). Finally, our exploration of identity uncertainty as a mediator of treatment effects in Appendix Table A10 included controls for beliefs about team composition.

Concealment to aid productivity Incomplete revelation in *First Name Only* could reflect a strategy to maximize productivity or affective benefits from avoiding caste discussion. We do not find evidence for this explanation. Although we find weak positive effects of *First Name Only* on productivity, perceived productivity declines. In the labor-stand survey, most respondents also expected the same output from SC-General pairs introduced by full names as from those introduced by first names only (Appendix Figure A7, Panel B). An anticipated productivity gain from concealment was therefore not the modal response. Finally, *First Name Only* participants are no happier than those in *Revealed* and report lower authenticity. Thus there is little evidence that concealment delivers productivity or emotional benefits.

Assuming a more socially distant group Concealment may worsen outcomes if participants respond to an unidentified partner by assuming that they belong to an even more socially distant group than the reality (Aigner and Cain, 1977; Bronchal, 2023). For example, a GC participant may respond more positively to an SC partner once revelation rules out the possibility that the partner is Muslim. The data do not support this explanation. Muslim guesses are rare in the concealment conditions ($< 1\%$), and as we showed earlier, wrong guesses are not systematically in the most pessimistic direction: both GCs and SCs often guess that their partner is OBC.

Selective revelation

The negative effects of *First Name Only* raise a natural question: why do participants not simply reveal caste and recreate the more positive outcomes observed in *Revealed*? We address this question using a simple conceptual framework, disclosure patterns within the experiment, and evidence from the labor-stand survey. Together, they point to the role of reciprocity, perceived social risks, and uncertainty about the benefits of revelation.

Conceptual framework Consider two workers paired for a joint task, each of whom can choose whether to reveal their caste identity. Mutual revelation can improve social interaction by making both identities common knowledge, thereby establishing common ground for conversation and helping participants anticipate how their behavior will be interpreted. However, these benefits need not be equally valuable to everyone. In particular, members of marginalized groups may anticipate that revelation could also expose them to stigma, exclusion, or hierarchical treatment.

Moreover, unilateral revelation can be socially costly. Revealing one’s identity without reciprocity does not establish mutual certainty about caste, and may create discomfort for both parties. A GC participant may hesitate to volunteer their caste because doing so could be perceived as asserting status, while an SC participant may fear revealing their identity without knowing how the other person will respond. Thus, even when mutual revelation would improve social interaction, neither participant may wish to disclose unilaterally.

We formalize these considerations in a simple Bayesian game (Appendix Section A). The model also assumes that some individuals in each caste group are committed non-disclosers, reflecting personal or social norms against voluntarily discussing caste.³² Other individuals choose strategically whether to reveal.

The game admits two classes of pure-strategy equilibria, which we interpret as distinct social norms governing disclosure. Under a *non-disclosure norm*, neither caste group reveals its identity, even when mutual revelation could improve interaction. Under a *selective-disclosure norm*, individuals reveal more frequently, but lower-caste

³²The presence of committed non-disclosers in both caste groups means that non-disclosure does not perfectly reveal identity in any equilibrium. A participant who does not reveal may belong to a particular group, but may also be following a broader norm against discussing caste.

participants do so only when their expected gains from revelation are sufficiently high.

The model therefore illustrates why the option to reveal need not eliminate identity uncertainty in *First Name Only*, as well as why voluntary revelation may be positively selected: individuals with more favorable expectations about cross-caste interaction have stronger incentives to disclose.

Empirical evidence on selection into revelation and the persistence of uncertainty The conceptual framework suggests that revelation depends on expectations about its consequences and may therefore be selective. It also highlights the role of disclosure norms, motivating our examination of whether revelation is reciprocal. We first study observed disclosure within the experiment. Later, we use the labor-stand survey to examine workers' willingness to initiate or reciprocate disclosure, and their expectations about how revelation would be received.

Disclosure within the experiment is strongly predicted by socio-economic similarity within the pair (Table 6). Participants paired with someone in the same income bracket are about 18 percentage points more likely to reveal caste, and those paired with someone at the same education level are about 25 percentage points more likely to do so. Relative to a mean disclosure rate of 42 percent, these effects are large, increasing revelation by roughly 40–60%. In contrast, own education, income, caste status, and age have little independent predictive power. This pattern suggests that disclosure is related to social distance between partners, consistent with workers being more willing to reveal when an interaction appears familiar or less risky.

The endogenous interaction measures point in the same direction (column 4). Partner disclosure is by far the strongest predictor of own revelation, suggesting that disclosure is often reciprocal rather than an individual decision made in isolation. Partner friendliness is also positively associated with disclosure, while partner bossiness and rudeness are negatively associated with disclosure, though the latter estimates are not statistically significant. These patterns are descriptive, but they suggest that participants resolve uncertainty when the interaction appears safer, more reciprocal, and more socially promising.

The labor-stand survey shows that many workers are reluctant to initiate identity disclosure. Overall, only about half of our respondents said they would tell a new work partner their caste unprompted, and a similar share said they would ask their partner's caste (Appendix Figure A16, Panel A). These numbers are higher among GC and OBC respondents than SC, as one might expect. However, willingness to discuss caste depends strongly on whether the partner shares or asks first. 75% of those unwilling to share on their own said they would share if the partner shared first, while 82% of those unwilling to ask would ask back if their partner asked them (Panel B). For many workers, reluctance to initiate therefore need not imply a wish to keep caste unknown.

The reasons respondents give (for not sharing or asking) suggest introducing caste can be uncomfortable, and its consequences differ across groups. While the most common reason was caste is irrelevant to work (about 51–57%),

Table 6: Who reveals their caste to their work partner?

	(1) Own chars.	(2) Pair similarity	(3) Own + similarity	(4) Own + sim. + endogenous [†]
<i>Own characteristics</i>				
General caste (=1)	0.002 (0.027)		−0.001 (0.026)	−0.013 (0.040)
Age	0.001 (0.008)		−0.004 (0.008)	−0.003 (0.007)
Married	0.005 (0.075)		0.022 (0.075)	−0.005 (0.061)
Completed 12th standard	0.094* (0.053)		0.031 (0.051)	0.028 (0.050)
Log HH income (bracket midpoint)	−0.051 (0.031)		−0.031 (0.032)	−0.004 (0.022)
<i>Pair similarity</i>				
Age gap (years)		0.000 (0.010)	0.002 (0.010)	0.001 (0.003)
Same income bracket as partner		0.182** (0.092)	0.169* (0.095)	0.041 (0.033)
Same education level as partner		0.245*** (0.082)	0.241*** (0.086)	0.065* (0.034)
<i>Endogenous controls (col. 4 only)[†]</i>				
Partner revealed their caste				0.690*** (0.086)
Partner friendliness (1–10)				0.042*** (0.016)
Partner rudeness (1–10)				−0.017 (0.027)
Partner bossiness (1–10)				−0.005 (0.030)
Tension with partner (1–10)				0.009 (0.033)
Mean dep. var.	0.424	0.424	0.424	0.424
Observations	342	342	342	342
Date FE	Yes	Yes	Yes	Yes

Notes: Sample is the *First Name Only* arm throughout. Outcome is an indicator equal to 1 if the respondent revealed their caste to their work partner, measured using the partner's report to avoid same-source bias. Columns 1–3 include only pre-determined baseline characteristics. Pair similarity variables equal 1 if respondent and partner share the same value: same income bracket; same education level (both completed 12th standard or both did not). Age gap is the absolute difference in years. Own income is the log of the household income bracket midpoint. All specifications absorb date fixed effects. Standard errors (in parentheses) are clustered at the pair-level.

[†] Col. 4 adds controls measured at endline that are potentially endogenous: whether the partner revealed their own caste (a simultaneous decision, measured using the subject's report) and interaction quality measures (partner friendliness, rudeness, bossiness, and tension).

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

a fair share of workers mentioned awkwardness (26–28%), avoiding discrimination or division (14–22%), as well as privacy concerns (7–19%) or worry about causing offence (13%) (Appendix Figure A17).³³ SC workers' greater reluctance is consistent with risks they face with disclosure: some respondents explicitly described asking about caste to avoid sharing food with or maintaining distance from lower-caste partners. Such responses give legitimacy to concerns about discriminatory treatment, even though fear is not the only reason workers hesitate. They also help interpret the experimental finding that disclosure is more common between socially similar partners, where workers feel more comfortable raising the subject.

Workers may also disclose less because they expect little benefit from revelation. Asked to compare SC–GC pairs introduced by full names versus first names only, 36% expected friendlier relations, 37% expected no difference, and 27% expected less friendly relations (Appendix Figure A7, Panel C). SC respondents were somewhat more pessimistic: 32% expected less friendly relations, compared with 25% of OBC and 23% of GC respondents. This difference is consistent with our experimental results: both groups gain on average from revelation, but the estimated gains are smaller for SC subjects (Appendix Table A5). Some workers (especially SC ones) may value knowing a partner's caste while also worrying about how the partner might react to theirs. Some workers may underestimate the average benefits of revelation; others may recognize these benefits but judge that they are not worth the risk of a particularly bad interaction. Whether they disclose will therefore depend on both their expectations and how costly an adverse response would be. Thus, the average gains in our study from revelation need not imply disclosure is always worthwhile.

Some concerns around caste disclosure, like awkwardness, suggest that participants might prefer caste to be revealed, but just not through the voluntary disclosure of the participants themselves (as in our *First Name Only* treatment group). We find evidence of this in our labor-stand survey. We asked workers whether they would prefer a workplace in which caste was revealed or one in which caste was hidden. 58% preferred the workplace in which caste was revealed, 29% were indifferent, and 13% preferred caste to be hidden. Even among SC respondents, the corresponding shares were 53%, 27%, and 20% (Appendix Figure A7, Panel A). These stated preferences line up with our core experimental findings. Group identity can be used for discrimination, but in our setting, the revelation of identity improves relations, and is preferred by participants, even those that belong to the marginalized group.

³³When respondents said they would not share or ask, enumerators wrote down their explanation and then coded every reason mentioned into a fixed list of options, which were not read out; a respondent could give more than one reason. When respondents said they would share or ask, only their own words were recorded. We translated these answers from Hindi and assigned each to a single theme, using Claude.

Conclusion

Debates over social integration tend to center on the relative virtues of embracing group identities—the so-called “politics of recognition” (Taylor, 1992)—versus suppressing them altogether, in favor of more universal, individualistic forms of belonging (Barry, 2002; Scott, 2007). This dichotomy overlooks a third, and arguably more realistic, scenario: that group identities persist, yet differ in how readily they can be discerned in the course of everyday interactions. In an experiment in North India, we find that the masking of identity reduces cross-caste trust, interpersonal closeness, feelings of authenticity, and friendliness, all while leaving a caste disparity in role allocations intact. Our findings thus underscore identity *legibility* as an underappreciated driver of intergroup cooperation. In contexts where identities and hierarchies are braided into social routines, individuals may come to rely on them as a source of convergent expectations that help them “make sense” of unfamiliar others (Schelling, 1960; Goffman, 1959). Under these circumstances, knowledge of group identity may grease the wheels of social interaction, whereas uncertainty may throw sand in the gears.

Our findings carry at least two practical implications. First, many economic interactions rely on weak ties—especially in the informal labor markets our study sought to emulate, and which remain pervasive throughout the developing world (Granovetter, 1973). Warm interpersonal interactions set the stage for exchanging useful information, securing referrals, and accessing future work. We find that despite little effect on actual productivity, concealment lowers perceived productivity and strains partner relations. It thus seems reasonable to speculate that identity-blinding schemes in the workplace may leave a lasting imprint beyond the immediate interaction. Second, our results indicate that top-down efforts to reduce identity salience may have weak or counterproductive impacts if they do not seek to transform underlying social structures. Concealment in our experiment fails to remove the traits and status differences through which hierarchy is reproduced, even as it disrupts the social coordination that helps people navigate them.

To be sure, our findings should not be taken to imply that identity revelation is universally desirable or advisable. In myriad settings—historical and contemporary—revelation would lead to domination and harm to minority groups. Disclosure in our experiment is selective, and the labor-stand survey describes several reasons why workers may hesitate to initiate it, including discomfort, privacy, and concerns about adverse treatment. Many subjects do not expect revelation to improve relations. The benefits of disclosure may thus co-exist with reasons to conceal or wait for another person to disclose first. A useful next step is to test whether changing expectations about the consequences of disclosure changes actual disclosure decisions and relations.

A final consideration concerns time horizons. Our experiment captures the short-run effects of identity concealment, which we argue apply to the types of jobs worked by our labor-stand respondents. Longer-term reductions in

identity legibility may have different consequences. Given time, norms of social interaction might evolve, allowing individuals to move through the social world without leaning on group identity cues. Thus, the long-run effects of identity concealment may be more positive. Historical efforts to abandon caste-distinctive names in South India point in this direction: Periyar's Self-Respect movement viewed caste titles and other markers not merely as expressions of identity, but as pillars of a hierarchical social order that it sought to challenge ([Venkatachalapathy, 2020](#); [Arni, 2024](#)).

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A Model

We develop a simple two-player Bayesian game of strategic identity disclosure, motivated by the *First Name Only* treatment, in which participants retain discretion over whether to reveal their caste. The model illustrates how the expected consequences of revelation and the costs of unilateral disclosure can sustain different patterns of identity revelation, even when mutual disclosure would facilitate social interaction.

Players and actions. Each player $i \in \{1, 2\}$ belongs to one of two caste groups, $c_i \in \{G, L\}$, representing General Caste and Scheduled Caste, respectively. Each player assigns probability $1/2$ to encountering a partner from either caste, and this prior is common knowledge.

In each caste group, a fraction $q > 0$ of individuals are committed non-disclosers. For these individuals, the action set is restricted to $\{H\}$, the action of withholding their caste identity. Whether an individual is a committed non-discloser is private information and independent of caste and payoff types. The remaining fraction $1 - q$ of each caste group consists of strategic players, who choose between two actions:

$$a_i \in \{R, H\},$$

where R denotes voluntarily revealing caste identity and H denotes withholding such revelation. Revelation includes deliberate communication of caste identity through either explicit statements or sufficiently informative cues. Withholding means that the participant does not voluntarily provide information that definitively identifies their caste. We assume that revelation is truthful and publicly observed within the pair, so that mutual revelation makes both players' caste identities common knowledge. As a tie-breaking rule, we also assume that strategic lower-caste players choose H whenever they are indifferent between revealing and hiding.³⁴

The model abstracts from other, incidental cues that may permit partial inference of caste. We return to the interpretation of the informational consequences of disclosure below.

Payoffs. Payoffs (of strategic players) depend on the disclosure decisions of both players and their caste identities. We assume $x > 0 > z$, and specify

$$u_i(a_i, a_j; c_i, c_j, y_i) = \begin{cases} 0, & \text{if } (a_i, a_j) = (H, H), \\ x, & \text{if } (a_i, a_j) = (R, R) \text{ and } c_i = c_j, \\ y_i, & \text{if } (a_i, a_j) = (R, R) \text{ and } c_i \neq c_j, \\ z, & \text{if } (a_i, a_j) \in \{(R, H), (H, R)\}. \end{cases}$$

Mutual revelation is the only outcome in which both players know each other's caste with certainty as common knowledge. We assume that the coordination benefits of identity revelation require this mutual certainty.

We normalize the payoff from mutual hiding to zero. Mutual revelation generates a payoff $x > 0$ in same-caste interactions. In cross-caste interactions, the payoff is y_i , which captures the individual's anticipated benefits of coordination as well as potential costs of discrimination or hierarchical treatment.

³⁴The tie-breaking rule rules out additional weak equilibria in which all strategic lower-caste types are exactly indifferent.

By contrast, when at least one player chooses H , mutual certainty is not achieved, even after updating beliefs about caste from equilibrium strategies. Unilateral revelation generates payoff $z < 0$ for both players, reflecting the potential discomfort of revealing identity without reciprocity.

Heterogeneity in the returns to revelation. For General-caste players, we assume

$$y_i = x$$

deterministically. This captures the assumption that General-caste participants do not expect to experience caste-based domination from mutual revelation of identities.

For lower-caste players,

$$y_i \sim F,$$

where F is continuous and strictly increasing on its support $[y, x]$. The parameter y_i captures heterogeneity in the anticipated quality of cross-caste interaction. Individuals may form expectations based on previous experiences or signals received during the interaction. In particular, a lower-caste player with $y_i < 0$ expects mutual revelation in a cross-caste match to generate a lower payoff than mutual concealment. By contrast, individuals with sufficiently high y_i anticipate gains from making caste identity mutually known.

Strategies and revelation rates. Since General-caste strategic players have no payoff-relevant type heterogeneity, their pure strategy is $s_G \in \{R, H\}$. A strategic lower-caste player's pure strategy is a measurable function

$$s_L : [y, x] \rightarrow \{R, H\}.$$

Let p_G and p_L denote the overall revelation probabilities among General-caste and lower-caste players, respectively, including committed non-disclosers.

Thus,

$$p_G = (1 - q) \mathbf{1}\{s_G = R\},$$

and

$$p_L = (1 - q) \int \mathbf{1}\{s_L(y) = R\} dF(y).$$

In particular,

$$0 \leq p_G, p_L \leq 1 - q.$$

Equilibrium intuition. Two opposing considerations shape revelation decisions.

First, unilateral revelation is costly. When a player expects their partner to conceal, revealing becomes unattractive. This creates the possibility of a no-disclosure equilibrium, even when mutual revelation could improve social interaction.

Second, mutual revelation can facilitate interaction by clarifying the social context. General-caste strategic players benefit from mutual revelation, while lower-caste strategic players differ in their anticipated returns from cross-caste disclosure. Thus, when General-caste strategic players reveal, lower-caste revelation may be selective.

The game can therefore support different disclosure norms: one characterized by universal non-disclosure, and another characterized by revelation among strategic General-caste players and selective revelation among strategic

lower-caste players.³⁵

We characterize these equilibrium classes formally. We assume $q < \frac{x}{x-2z}$, which ensures that the proportion of committed non-disclosers is not so large that strategic General-caste players lose the incentive to reveal when other strategic players do so.

Proposition 1 (Equilibrium characterization). *Suppose*

$$x > 0 > z, \quad \underline{y} < -x + 2z, \quad 0 < q < \frac{x}{x-2z},$$

and F is continuous and strictly increasing on $[\underline{y}, x]$. Assume that strategic lower-caste players choose H whenever indifferent. Then the following two classes of equilibria always exist, and every pure-strategy symmetric Bayesian Nash equilibrium belongs to one of these two classes:

(i) *No disclosure. All players, including strategic players, hide:*

$$s_G = H, \quad s_L(y) = H \quad \text{for all } y.$$

(ii) *Selective disclosure. All strategic General-caste players reveal, while strategic lower-caste players follow a cutoff strategy:*

$$s_G = R, \quad s_L(y) = \begin{cases} H & \text{if } y \leq y^*, \\ R & \text{if } y > y^*, \end{cases}$$

where

$$y^* = -\frac{p_L(x-2z) + 2qz}{1-q}$$

and

$$p_L = (1-q)[1 - F(y^*)].$$

Disclosure norms and identity uncertainty. The equilibrium classes can be interpreted as different social conventions governing the voluntary disclosure of caste. Under the *non-disclosure convention*, individuals do not voluntarily reveal identity, even when mutual revelation could improve interaction. Under the *selective-disclosure convention*, strategic General-caste individuals reveal, while strategic lower-caste individuals reveal only when their expected returns are sufficiently favorable.

The existence of different equilibrium classes suggests an additional possibility: individuals meeting strangers may be uncertain about which disclosure convention their partners follow. Such uncertainty could further discourage revelation and make silence difficult to interpret. This possibility goes beyond the baseline game, which assumes common knowledge of equilibrium strategies, and is not required for the formal results in Proposition 1.

The framework is deliberately parsimonious. It models the incentives for voluntary disclosure, rather than the sequential process of conversation or all the informational cues through which caste may be inferred in practice. In particular, reciprocal disclosure during conversation is an empirical phenomenon that the framework motivates but does not formally derive.

³⁵Even if strategic players from only one caste choose H , observing H does not identify caste with certainty, since a positive fraction $q > 0$ of each caste group consists of committed non-disclosers. Thus, neither mutual non-disclosure (H, H) nor unilateral revelation (R, H) or (H, R) can unravel to reproduce the mutual certainty achieved under (R, R) , even when equilibrium strategies are common knowledge.

A.1 Proofs

Proof of Proposition 1. Let p_G and p_L denote the overall revelation probabilities in the two caste groups.

Since each player assigns probability $1/2$ to encountering either caste, the expected payoff from revealing for a strategic lower-caste player of type y is

$$U_L(R | y) = \frac{1}{2}p_Lx + \frac{1}{2}p_Gy + \left(1 - \frac{p_G + p_L}{2}\right)z.$$

The payoff from hiding is

$$U_L(H | y) = \frac{p_G + p_L}{2}z.$$

Thus, the gain from revealing is

$$\Delta_L(y) \equiv U_L(R | y) - U_L(H | y) = \frac{1}{2}p_Gy + \frac{1}{2}p_Lx + z(1 - p_G - p_L).$$

Similarly, for a strategic General-caste player, the payoff gain from revealing is

$$\Delta_G = \frac{1}{2}(p_G + p_L)x + z(1 - p_G - p_L).$$

We consider the two possible pure strategies of strategic General-caste players.

Case 1: Strategic General-caste players hide.

In this case,

$$p_G = 0.$$

The lower-caste payoff difference becomes

$$\Delta_L(y) = \frac{1}{2}p_Lx + (1 - p_L)z,$$

which is independent of y .

If this expression is nonpositive, all strategic lower-caste players hide by the tie-breaking assumption. This implies $p_L = 0$.

At $p_G = p_L = 0$, we have

$$\Delta_L(y) = \Delta_G = z < 0.$$

Thus, universal non-disclosure is an equilibrium.

If instead $\Delta_L(y) > 0$, all strategic lower-caste players reveal, implying $p_L = 1 - q$. But then

$$\Delta_G = \frac{1}{2}(1 - q)x + qz > 0,$$

where the inequality follows from

$$q < \frac{x}{x - 2z}.$$

Strategic General-caste players would therefore deviate to revealing, contradicting $p_G = 0$.

Consequently, the only equilibrium with strategic General-caste players hiding is universal non-disclosure.

Case 2: Strategic General-caste players reveal.

In this case,

$$p_G = 1 - q.$$

For a strategic General-caste player,

$$\begin{aligned}\Delta_G &= \frac{1}{2}(1 - q + p_L)x + z(q - p_L) \\ &= \frac{1}{2}(1 - q)x + qz + p_L\left(\frac{x}{2} - z\right).\end{aligned}$$

Since $x > 0 > z$ and $q < x/(x - 2z)$, we have

$$\Delta_G > 0$$

for every $p_L \in [0, 1 - q]$. Thus, revealing is a strict best response for strategic General-caste players.

For a strategic lower-caste player,

$$\begin{aligned}\Delta_L(y) &= \frac{1}{2}(1 - q)y + \frac{1}{2}p_Lx + z(q - p_L) \\ &= \frac{1}{2}[(1 - q)y + p_L(x - 2z) + 2qz].\end{aligned}$$

Since $q < 1$, this expression is strictly increasing in y . Consequently, strategic lower-caste players follow a cutoff rule:

$$s_L(y) = R \iff y > y^*,$$

where

$$y^* = -\frac{p_L(x - 2z) + 2qz}{1 - q}.$$

Because only the fraction $1 - q$ of lower-caste players can voluntarily reveal, consistency requires

$$p_L = (1 - q)[1 - F(y^*)].$$

Substituting the cutoff gives

$$p_L = (1 - q)\left[1 - F\left(-\frac{p_L(x - 2z) + 2qz}{1 - q}\right)\right].$$

To establish existence, define

$$T(p) = (1 - q)\left[1 - F\left(-\frac{p(x - 2z) + 2qz}{1 - q}\right)\right].$$

The function T is continuous on $[0, 1 - q]$.

At $p = 0$, the implied cutoff is

$$y^*(0) = -\frac{2qz}{1 - q} < x,$$

where the inequality follows from $q < x/(x - 2z)$. Hence

$$T(0) > 0.$$

At $p = 1 - q$, the implied cutoff is

$$\begin{aligned} y^*(1 - q) &= -(x - 2z) - \frac{2qz}{1 - q} \\ &> -x + 2z \\ &> \underline{y}, \end{aligned}$$

where the first strict inequality follows from $z < 0$ and $q > 0$.

Therefore,

$$T(1 - q) < 1 - q.$$

By continuity and the Intermediate Value Theorem, there exists

$$p_L^* \in (0, 1 - q)$$

such that

$$p_L^* = T(p_L^*).$$

Every such fixed point generates a selective-disclosure equilibrium. The maintained assumptions do not guarantee uniqueness, so multiple cutoff equilibria may exist.

Finally, since the strategic General-caste strategy must be either R or H in a pure-strategy equilibrium, the two cases exhaust the possibilities. $\square$

B Supplementary Tables

Table A1: Baseline summary statistics for the experimental sample.

	N	Mean	St. Dev.	Min.	Pctl. 25	Median	Pctl. 75	Max.
(a) Age	1198	23.30	4.34	18.00	20.00	22.00	26.00	40.00
(b) Number of siblings	1198	2.86	1.14	0.00	2.00	3.00	4.00	8.00
(c) Household size	1198	5.91	1.71	1.00	5.00	6.00	7.00	20.00
(d) Married	1198	0.27	0.45	0.00	0.00	0.00	1.00	1.00
(e) Number of children (if married)	329	1.38	0.95	0.00	1.00	1.00	2.00	9.00
(f) Household income (midpoint, INR)	1198	10659	8185	2500	2500	7500	17500	55000
(g) Completed 12th standard	1198	0.73	0.44	0.00	0.00	1.00	1.00	1.00
(h) Completed college	1198	0.20	0.40	0.00	0.00	0.00	0.00	1.00
(i) Identity ranking: Caste	1198	3.70	0.99	1.00	3.00	4.00	4.00	5.00
(j) Identity ranking: Religion	1198	3.69	1.11	1.00	3.00	4.00	5.00	5.00
(k) Identity ranking: Indian	1198	1.31	0.83	1.00	1.00	1.00	1.00	5.00
(l) Identity ranking: From U.P.	1198	3.33	1.36	1.00	2.00	3.00	5.00	5.00
(m) Identity ranking Father/Son/Brother	1198	2.97	1.16	1.00	2.00	3.00	4.00	5.00

Notes: This table presents summary statistics for variables collected at baseline. Household income (row (f)) is coded as the midpoint of the income bin, or the minimum value for the top-coded bin. Identity rankings (rows (i)–(m)) are measured on a 1 (most) to 5 (least) ranking scale. All statistics are based on the experimental sample.

Table A2: Baseline differences in means (GEN vs. SC)

	General Caste	Scheduled Caste	Diff. (GEN–SC)	N
(a) Age	23.07 (4.24)	23.59 (4.46)	–0.52* (0.27)	1,064
(b) Number of siblings	2.69 (1.10)	3.11 (1.16)	–0.42*** (0.07)	1,064
(c) Household size	5.95 (1.88)	5.93 (1.62)	0.03 (0.11)	1,064
(d) Married	0.24 (0.43)	0.31 (0.46)	–0.07** (0.03)	1,064
(e) Number of children	1.46 (0.77)	1.29 (1.08)	0.16 (0.11)	296
(f) Household income (midpoint, INR)	12,307.33 (9,325.06)	9,064.85 (6,437.23)	3,242.48*** (491.27)	1,064
(g) Completed 12th standard	0.77 (0.42)	0.67 (0.47)	0.10*** (0.03)	1,064
(h) Completed college	0.24 (0.43)	0.17 (0.38)	0.07*** (0.02)	1,064
(i) Identity ranking: Caste	3.88 (0.97)	3.51 (0.96)	0.37*** (0.06)	1,064
(j) Identity ranking: Religion	3.92 (1.04)	3.46 (1.10)	0.46*** (0.07)	1,064
(k) Identity ranking: Indian	1.19 (0.66)	1.45 (0.98)	–0.26*** (0.05)	1,064
(l) Identity ranking: From U.P.	2.91 (1.27)	3.75 (1.33)	–0.83*** (0.08)	1,064
(m) Identity ranking: Father/Son/Brother	3.10 (1.00)	2.83 (1.29)	0.27*** (0.07)	1,064

Notes: This table reports baseline differences in means between General and Scheduled caste respondents. Columns (1) and (2) report group means with standard deviations in parentheses. Column (3) reports the difference in means (GEN minus SC); standard errors are shown in parentheses below. Household income is coded as the midpoint of the income bin (minimum for the top-coded bin). Identity rankings are measured on a scale from 1 (most important) to 5 (least important).

Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Sample selection

Sample Variable	Baseline only (1)		Experimental (2)		Diff (2–1)		N	
	Mean	SE	Mean	SE	Mean	SE	1	2
(a) Age	24.21	0.24	23.30	0.13	−0.919***	0.252	448	1198
(b) Number of siblings	2.99	0.06	2.86	0.03	−0.130**	0.065	447	1198
(c) Household size	5.90	0.09	5.91	0.05	0.012	0.098	432	1198
(d) Married	0.29	0.02	0.27	0.01	−0.018	0.024	479	1198
(e) Number of children	1.42	0.08	1.38	0.05	−0.041	0.094	140	329
(f) HH income (INR '000)	10.21	0.35	10.66	0.24	0.451	0.448	431	1198
(g) Completed 12th standard	0.74	0.02	0.73	0.01	−0.012	0.025	431	1198
(h) Completed college	0.25	0.02	0.20	0.01	−0.044*	0.023	431	1198
(i) Identity ranking: Caste	3.70	0.05	3.70	0.03	−0.001	0.056	421	1198
(j) Identity ranking: Religion	3.46	0.05	3.69	0.03	0.224***	0.063	421	1198
(k) Identity ranking: Indian	1.24	0.04	1.31	0.02	0.068	0.046	421	1198
(l) Identity ranking: From U.P.	3.42	0.07	3.33	0.04	−0.088	0.077	421	1198
(m) Identity ranking: Father/Son/Brother	3.18	0.06	2.97	0.03	−0.203***	0.066	421	1198

Notes: Means and standard errors (SE) by group. The first group represents the sample of individuals who participated in the baseline survey, but did not take part in the final experiment, whereas the second group represents the latter. Household income (row (f)) is coded as the midpoint of the income bin, or the minimum value for the top-coded bin and is shown in thousands of INR. Identity rankings (rows (i)–(m)) are measured on a 1 (most) to 5 (least) ranking scale. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Randomization checks

	Control (First Name Only)		Revealed vs Control		Hidden vs Control		Revealed = Hidden
	Mean	SD	β	p -value	β	p -value	p -value
(a) Age	23.53	4.73	−0.19	0.52	−0.40	0.21	0.52
(b) Number of siblings	2.94	1.13	−0.03	0.72	−0.06	0.40	0.63
(c) Household size	5.92	1.79	0.08	0.52	0.00	0.99	0.52
(d) Married	0.30	0.46	−0.03	0.36	−0.04	0.17	0.64
(e) Household income (midpoint, INR)	11,034	8,586	−457	0.40	−556	0.31	0.85
(f) Completed 12th standard	0.72	0.45	0.02	0.55	−0.02	0.50	0.23
(g) Completed college	0.21	0.41	0.00	0.89	−0.03	0.39	0.48
(h) Reading productivity (correct/second)	0.177	0.070	−0.003	0.47	0.002	0.63	0.25
(i) Typing productivity (correct/second)	0.155	0.052	−0.003	0.34	−0.004	0.25	0.83
(j) Identity ranking: Caste	3.69	0.97	0.08	0.29	−0.04	0.55	0.10
(k) Identity ranking: Religion	3.70	1.11	0.02	0.81	−0.04	0.65	0.48
(l) Identity ranking: Indian	1.34	0.87	−0.06	0.27	−0.02	0.76	0.45
(m) Identity ranking: From U.P.	3.34	1.41	−0.03	0.72	−0.01	0.95	0.76
(n) Identity ranking: Father/Son/Brother	2.94	1.14	0.00	0.97	0.10	0.22	0.23
(o) Well-being score (% of maximum)	45.90	12.45	−0.47	0.56	−1.22	0.13	0.34
(p) Completed endline survey	0.96	0.21	−0.01	0.56	0.01	0.66	0.33

Notes: Each row is a separate regression of the baseline characteristic on indicators for the *Revealed* and *Hidden* arms, with randomization-strata (date $\times$ caste) fixed effects and standard errors clustered at the work-booth $\times$ date level. The sample is all 1,064 participants randomized to the three main arms. The balance results remain the same if we also include the obfuscation sample. The omitted group is First Name Only. Household income (row (e)) is the midpoint of the reported bin, or the minimum for the top bin. Reading and typing productivity (rows (h)–(i)) are correct answers per second on the baseline reading and typing tasks. Identity rankings (rows (j)–(n)) run from 1 (most important) to 5 (least). The well-being score (row (o)) averages five items, with self-conscious and anxious reverse-coded, scaled 0–100. Row (p) is whether the participant completed the endline survey.

Table A5: Heterogeneity by caste: GEN vs SC

	(1) Identity fusion	(2) Meet up again	(3) Trust sent
<i>General caste (GEN)</i>			
First name only	−0.365*** (0.104)	−0.204*** (0.051)	−4.020*** (1.311)
Hidden	−0.544*** (0.107)	−0.213*** (0.051)	−4.633*** (1.353)
<i>Mean dep. var. (Revealed)</i>	2.777	0.741	27.349
<i>Scheduled Caste (SC)</i>			
First name only	−0.199** (0.099)	−0.101* (0.052)	−3.140** (1.375)
Hidden	−0.386*** (0.103)	−0.186*** (0.051)	−2.928** (1.331)
<i>Mean dep. var. (Revealed)</i>	2.783	0.711	26.145
<i>p</i> : GEN = SC, First Name Only	0.089	0.040	0.489
<i>p</i> : GEN = SC, Hidden	0.105	0.618	0.182
<i>N</i>	1,014	1,014	1,014

Notes: Omitted treatment: Revealed (GEN and SC). Standard errors are clustered at the pair-level. Each column comes from a single regression interacting treatment status with caste identity; the two coefficients shown under each caste panel heading are the interactions of that caste dummy with the *First Name Only* and *Hidden* treatment indicators respectively. All specifications control for age, and absorb randomization strata and education bin fixed effects via `reghdfe`. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: General-caste senior-role allocation, accounting for baseline ability

	(1)	(2)	(3)	(4)	(5)
Predicted share General senior at equal ability	0.599*** (0.026)	0.599*** (0.026)	0.599*** (0.025)	0.689*** (0.038)	0.599*** (0.025)
R^2	0.000	0.006	0.042	0.107	0.076
Observations	167	167	167	167	167
H_0 : share = 0.50	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$	$p < 0.001$
F -test: ability controls = 0	—	$p = 0.621$	$p = 0.132$	$p = 0.011$	$p = 0.121$
Ability gap controls	None	Linear	Linear	Quadratic	Quintiles
Ability level controls	None	None	Linear	Linear	None

Notes: The dependent variable is the share of tasks in which the General-caste participant takes the senior role. The table reports the predicted share of tasks in which the General-caste participant is senior when General and SC participants have equal baseline ability. Ability is measured using baseline reading and typing scores. Ability gap controls are functions of the General-minus-SC baseline ability gap; ability level controls account for the overall level of baseline ability in the pair. Standard errors are reported in parentheses. The F -test reports the joint significance of the included ability controls. *** $p < 0.01$.

Table A7: Logit model: Predicting caste from observable characteristics

	P(SC) (1)
<i>Demographics</i>	
Age	0.008* (0.005)
Married	0.095** (0.044)
Number of siblings	0.112*** (0.015)
Household size	-0.030*** (0.011)
<i>Education and income</i>	
Completed 12th grade	-0.101*** (0.033)
College degree	-0.043 (0.039)
Monthly household income (Rs.)	-0.000*** (0.000)
<i>Self-assessment and affect</i>	
Self-assessed: better at reading	0.131*** (0.035)
Self-conscious yesterday (1–4)	0.080*** (0.018)
<i>Appearance and speech cues</i>	
Clothing quality (PC1)	-0.014 (0.011)
Fluency/pronunciation (PC1)	0.003 (0.011)
Shudh Hindi rating (PC1)	-0.010 (0.014)
Observations	1,099
McFadden R^2	0.120

Notes: The outcome is an indicator equal to 1 for Scheduled Caste and 0 for General Caste. The sample excludes OBC respondents. The table reports average marginal effects from a logit model. All predictors are measured at baseline and are observable through natural workplace interaction. Clothing quality, fluency/pronunciation, and Shudh Hindi rating are each the first principal component of ratings by three independent raters. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Out-of-sample classification accuracy (50/50 Train–Test Split)

	Predicted SC	Predicted General	Total
True SC	205	98	303
True General	102	189	291
Total	307	287	594
Overall accuracy: 66.3% (chance baseline: 50.0%)			

Notes: The logit model is estimated on a random 50% training sample (seed 12345) and evaluated on the held-out 50% test sample ($N = 594$). The predictor set is the PC1 specification from Table A7: baseline demographics, income, self-assessed ability, and self-consciousness, plus the first principal component of clothing quality, fluency/pronunciation, and Shudh Hindi ratings. A participant is predicted SC when the estimated $\Pr(\text{SC}) \geq 0.5$.

Table A9: Social relations: Homogeneous vs heterogeneous revealed teams

	(1) Trust sent	(2) Identity fusion	(3) Meet up again
Same-caste pair (=1 if HH or LL, =0 if HL)	3.157* (1.913)	0.266* (0.138)	0.001 (0.072)
Mean dep. var. (HL)	26.778	2.781	0.723
Observations	425	425	425

Notes: Standard errors clustered at the pair-level in parentheses. Sample: HH-Revealed, LL-Revealed, and HL-Revealed arms. Homogeneous = 1 if same-caste pair (HH or LL), 0 if cross-caste (HL). Mean dep. var. is for the heterogeneous (HL) group. All specifications control for age and absorb randomization strata and education bin fixed effects. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A10: Caste confidence mediates treatment effects

	(1) Trust sent	(2) Identity fusion	(3) Meet up again	(4) True self
<i>Panel A: Treatment effects only</i>				
First name only	-3.711*** (1.221)	-0.286*** (0.092)	-0.152*** (0.046)	-0.713*** (0.246)
Hidden	-3.920*** (1.217)	-0.468*** (0.097)	-0.198*** (0.045)	-0.846*** (0.237)
<i>Mean (Revealed)</i>	26.747	2.780	0.726	7.503
<i>First name only vs. Hidden p-value</i>	0.844	0.066	0.322	0.571
<i>N</i>	1,014	1,014	1,014	1,014
<i>Panel B: Controlling for caste confidence and beliefs</i>				
First name only	-1.558 (1.138)	-0.091 (0.089)	-0.068 (0.047)	0.005 (0.207)
Hidden	-0.009 (1.348)	-0.126 (0.101)	-0.067 (0.048)	0.426* (0.218)
<i>Caste certainty</i>				
Caste confidence (0–1)	19.002*** (2.620)	1.574*** (0.212)	0.505*** (0.093)	5.933*** (0.488)
<i>Guessed caste × own caste</i>				
Guessed GEN × own GEN	0.128 (1.569)	-0.022 (0.201)	-0.146* (0.083)	0.097 (0.401)
Guessed GEN × own SC	-3.526** (1.566)	-0.077 (0.146)	0.190*** (0.064)	-0.424 (0.300)
<i>Mean (Revealed)</i>	26.747	2.780	0.726	7.503
<i>First name only vs. Hidden p-value</i>	0.167	0.716	0.970	0.051
<i>N</i>	1,014	1,014	1,014	1,014

Notes: Omitted treatment: Revealed. All specifications control for age and absorb randomization strata and education bin fixed effects. Panel B adds caste confidence (rescaled 0–1) and guessed partner caste interacted with own caste. SEs clustered at the pair-level. Guessed SC × own GEN and Guessed SC × own SC are omitted in Panel B, because of collinearity (strata fixed effects include caste). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Asking about identity is associated with greater trust

	General Caste		Scheduled Caste	
	(1)	(2)	(3)	(4)
	Trust: Sent	Trust: Belief	Trust: Sent	Trust: Belief
Partner asked your caste?	1.004 (2.011)	-1.147 (2.057)	4.014** (2.011)	-0.078 (1.857)
Asked partner their caste?	4.057** (2.052)	4.463** (2.105)	1.253 (1.925)	2.174 (1.779)
Mean Dep. Var.	25.03	22.5	24.3	21.5
Observations	564	564	563	563
R-squared	0.128	0.124	0.149	0.133

Notes: Robust standard errors reported in parentheses. Columns (1)–(2) restrict the sample to GC participants and columns (3)–(4) to SC participants. $e_did_p_ask_caste = 1$ if the partner asked about caste; $e_did_ask_p_caste = 1$ if the respondent asked the partner's caste. All specifications include date fixed effects and pre-specified controls for baseline education bin and age. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

Table A12: Effect of Revealing/Hiding on Study Purpose Mentioning Caste

	(1)
First Name Only	0.013 (0.011)
Hidden	0.015 (0.011)
Revealed mean	0.014
Observations	1064
R-squared	0.146
Strata FE	Yes
Education FE	Yes
Clusters	532

Notes: Main sample (mixed high/low caste pairs, three arms). Outcome equals 1 if the participant's open-ended description of the study purpose refers to caste or social relations (coded by Claude). Omitted category: Revealed. Includes a control for age, as well as randomization strata and education fixed effects. Standard errors clustered by booth $\times$ date.

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

C Supplementary Figures

Figure A1: The randomizer

Across India, Chakraborty et al. Randomizer: Participant ID Entry (updated: sept 05, 2025)

Participant ID 1 :

Participant ID 2 :

Participant ID 3 :

Participant ID 4 :

Participant ID 5 :

Participant ID 6 :

Participant ID 7 :

Participant ID 8 :

Participant ID 9 :

Instructions:

1. Enter up to 32 participant IDs in the input fields on the left.

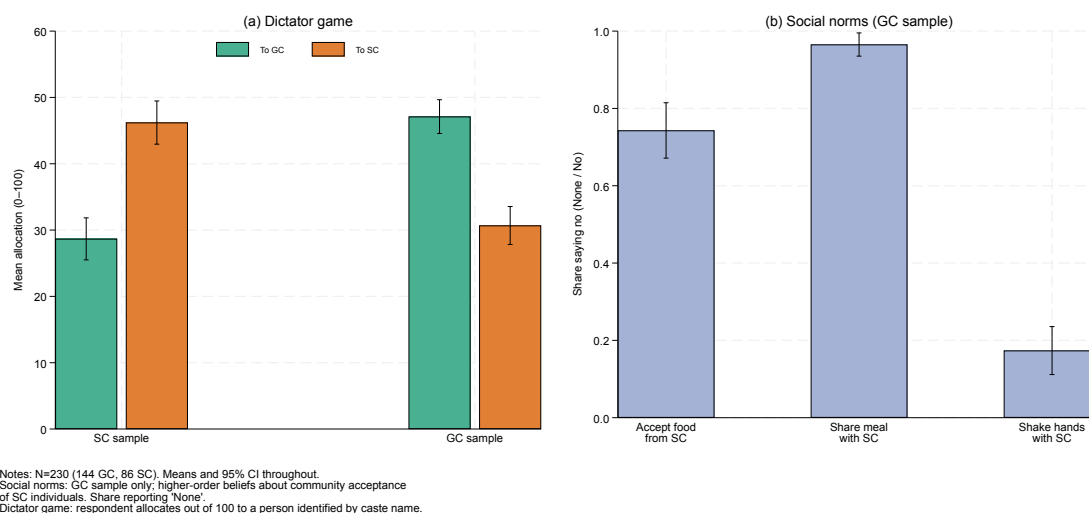
2. Click 'Submit' to generate the CSV file.

Notes:

- The Participant ID must be an EXACT match to the 6-character IDs in our data (e.g., WEDGIE).
- All characters must be in UPPERCASE.

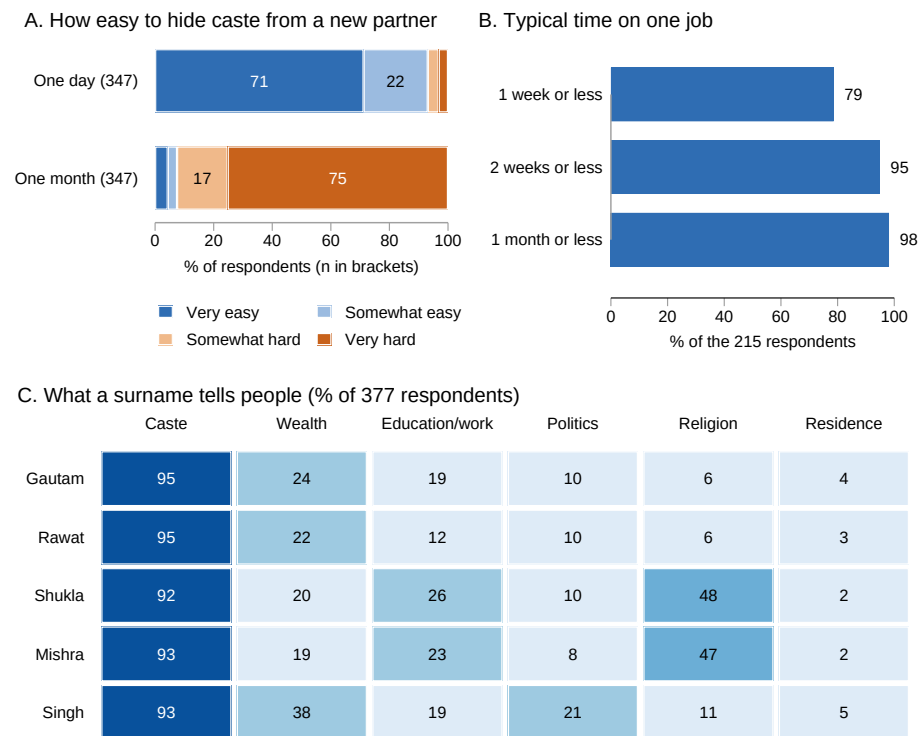
Notes: This figure shows a screenshot of the randomizer app we built to assign subjects to pairs and pairs to treatments each day. Subjects were given unique IDs at the end of the baseline survey, which they brought to the work site on the day they were invited. The app was linked to our baseline database, so entering a subject’s ID pulled the relevant caste information and allowed the app to assign subjects appropriately. Using baseline-linked IDs ensured that neither names nor caste needed to be recorded on the day of the experiment, limiting the salience of caste during randomization.

Figure A2: Caste bias in study region



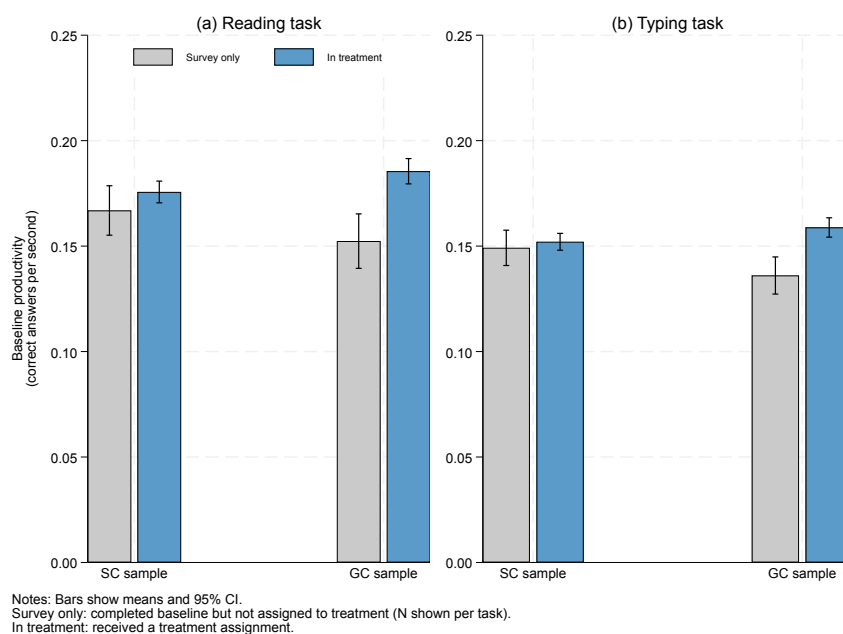
Notes: Panel (a) shows mean allocations from a dictator game in which respondents distribute 100 units to a hypothetical recipient identified by caste name (General Caste or Scheduled Caste). Bars show means by respondent caste (GC or SC), separately for giving to a GC recipient (green) and an SC recipient (orange). Panel (b) is restricted to GC respondents and displays three measures of social acceptance norms toward SC individuals: the share reporting that “none” in their village would accept food from an SC person, the share who say “none” in their family would share a meal with an SC person, and finally the share reporting that “none” in their village would shake hands with an SC person. The food and handshake items are second-order beliefs about community norms; the meal-sharing item is a self-report about their family. We asked all questions in a scoping survey conducted in the study region. Error bars are 95% confidence intervals. N = 230 (144 GC, 86 SC).

Figure A3: Hiding caste at work, job length, and what surnames convey (labor-stand survey)



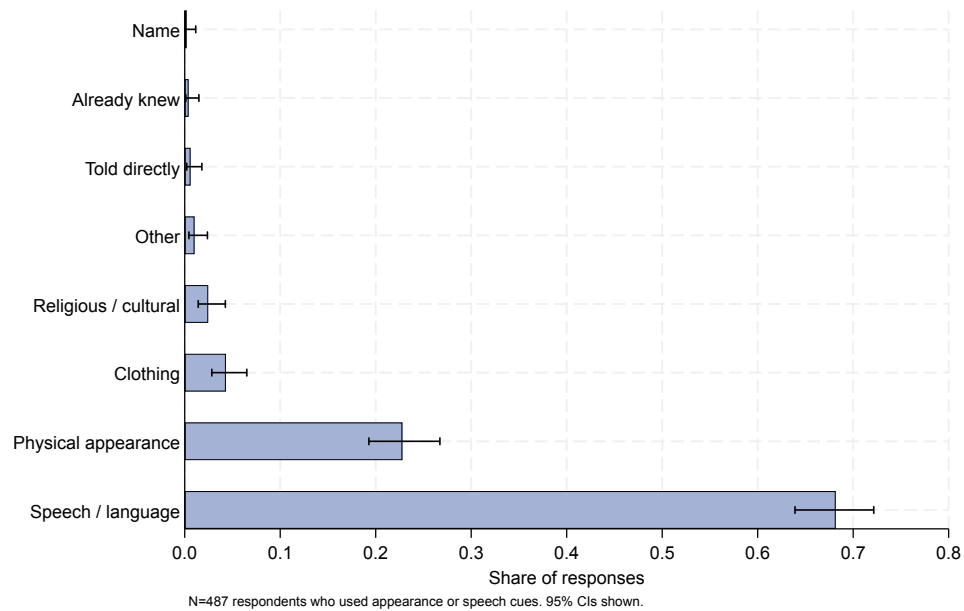
Notes: Supplementary survey of men aged 18–40 at labour stands in Lucknow (October 2026). Panel A: Hindu respondents; how easy it would be to hide their caste from a new partner on a one-day or a one-month job outside their home village. Panel B: typical time spent on one job. Panel C: share of all respondents mentioning each type of information when told a man's surname; more than one option was allowed. Sample sizes are shown in each panel.

Figure A4: Sample selection on baseline productivity



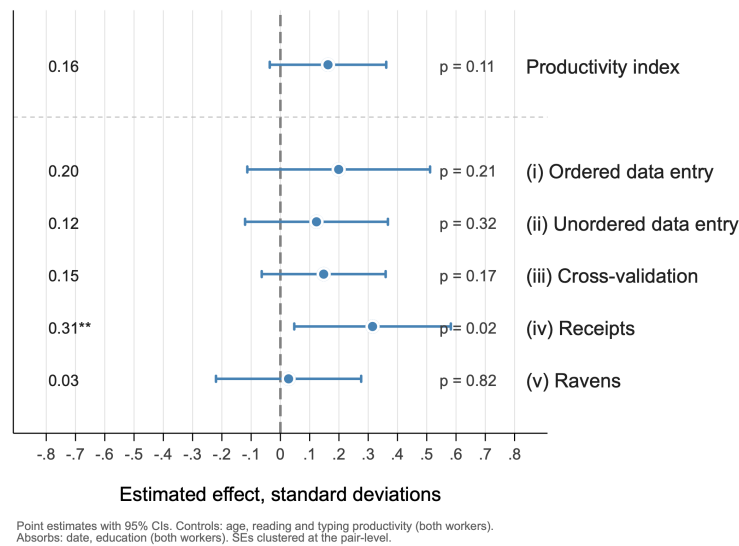
Notes: Bars show mean baseline productivity (correct answers per second) with 95% confidence intervals, separately by caste and participation status. "Survey only" denotes individuals who completed the recruitment survey but were not assigned to a treatment; "In treatment" denotes those who received a treatment assignment. Both GC and SC participants are positively selected on productivity relative to non-participants, and the pattern holds across both the reading and typing tasks.

Figure A5: Open ended responses on how subjects guessed their partner's identity



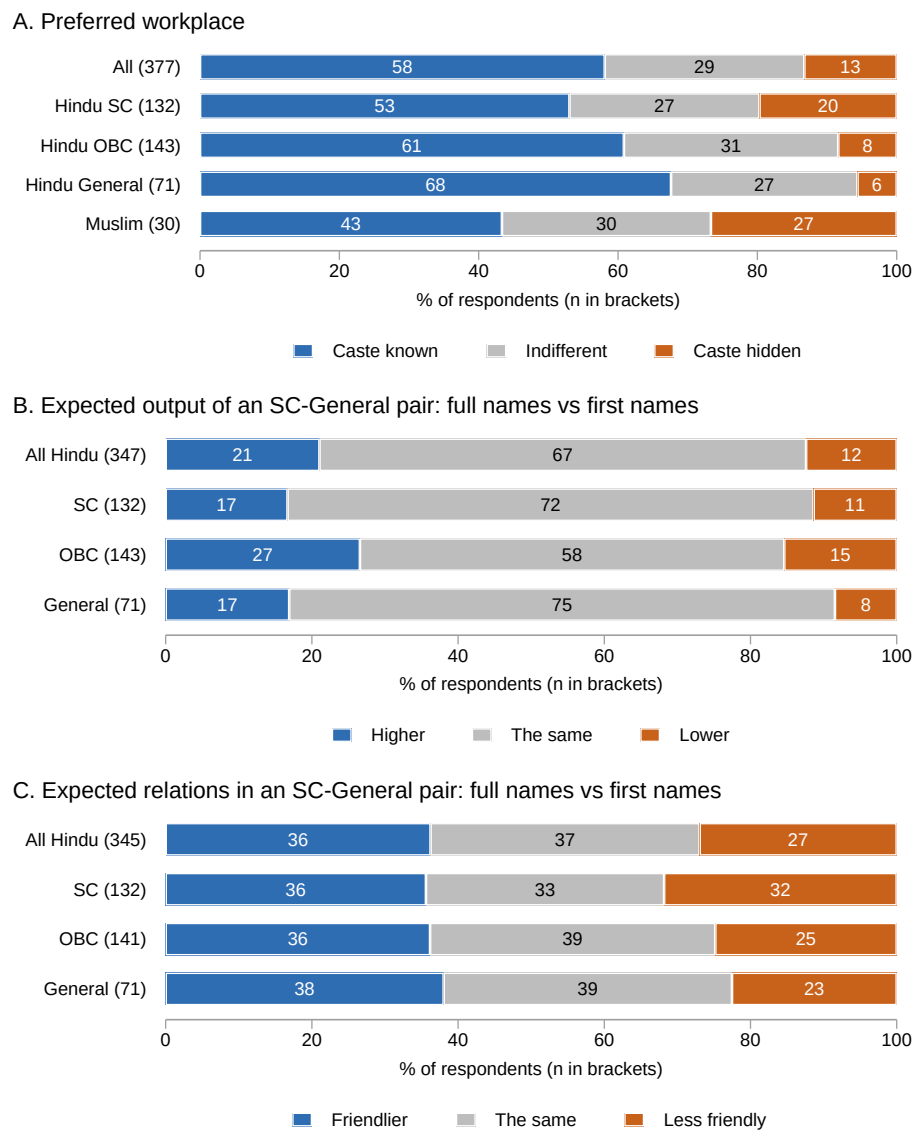
Notes: This figure summarizes qualitative responses from participants who reported that they could infer their partner's identity from appearance or speech cues. Each open-ended response is classified into one of the following categories based on the specific cue mentioned by the participant.

Figure A6: Productivity of homogeneous versus heterogeneous teams



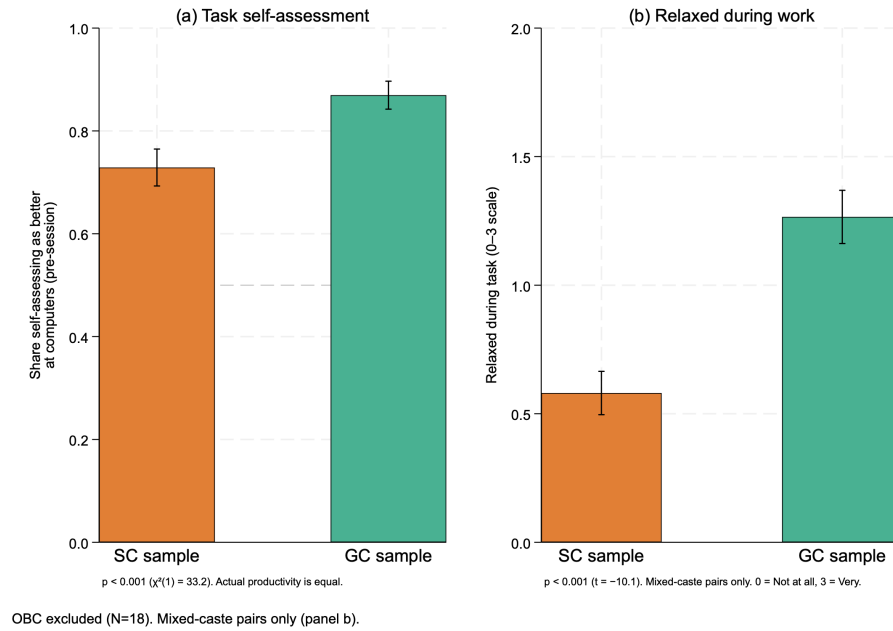
Notes: This figure compares the productivity of homogeneous versus heterogeneous teams in the *Revealed* condition. All regressions control for age and baseline productivity levels of each partner in each task (reading and typing), as well as fixed effects for education-level bin and date. Standard errors are clustered at the pair level.

Figure A7: Workplace preferences and expected effects of revealing caste (labor-stand survey)



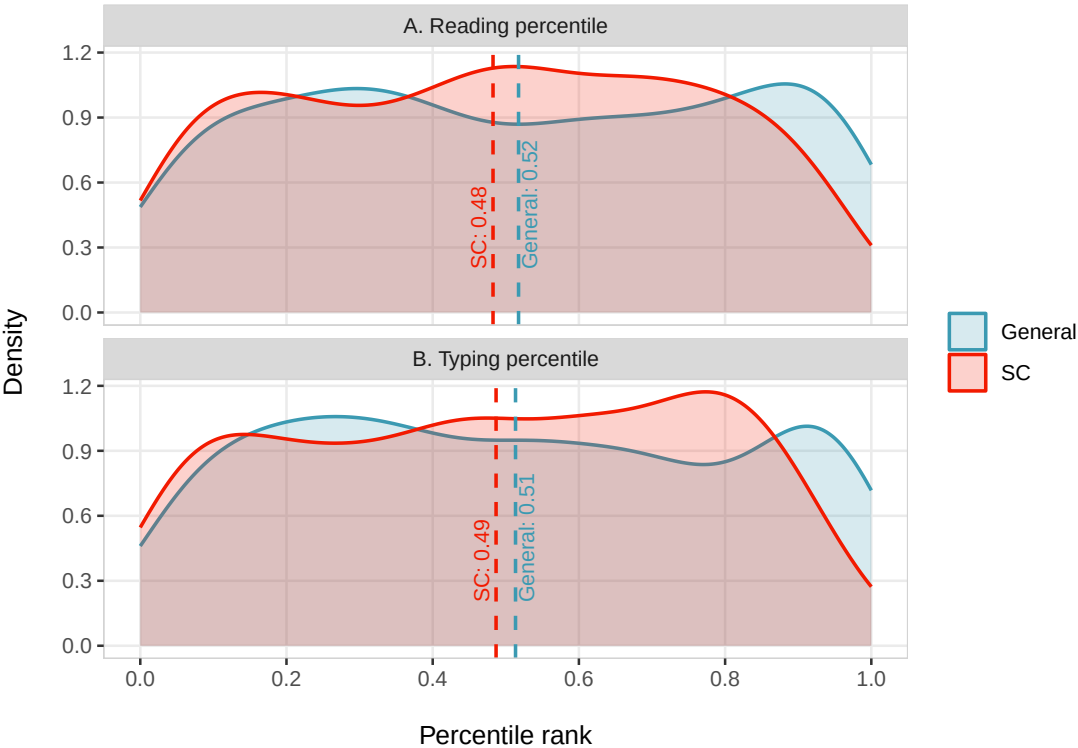
Notes: Follow-up survey of men aged 18–40 at labor stands in Lucknow. Panel A: all respondents; preference between a workplace where the employer keeps workers’ last names and backgrounds hidden, so caste can be hidden, and one where every worker’s caste is known. Panels B–C: Hindu respondents; an SC and a General-caste man work together, introduced either with full names, revealing caste, or with first names, free to share their caste. Respondents predicted the pair’s output (B) and relations (C) with full names relative to first names; “don’t know” answers are excluded. Sample sizes are in brackets.

Figure A8: Differences between General and Scheduled Castes in traits



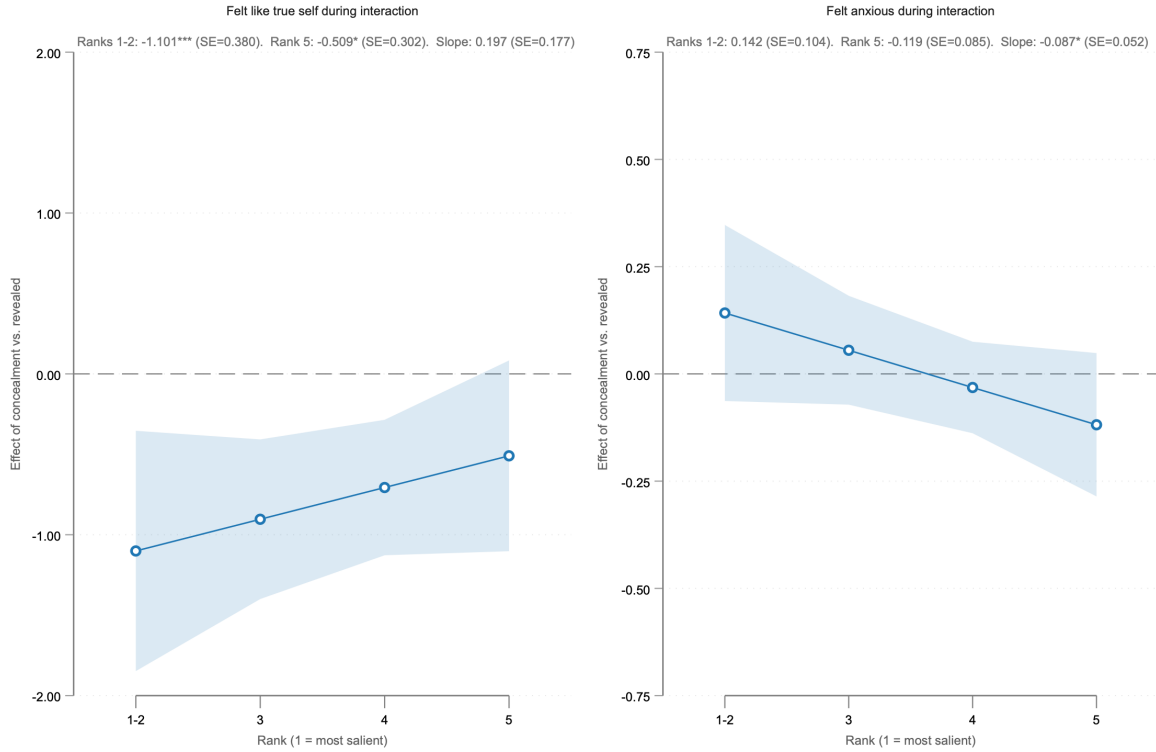
Notes: The figure compares SC and GC participants on traits measured before or during the work session. Panel (a) reports the share of participants who self-assessed as better at the computerized task than the paper task before the session. Actual ability is similar across caste groups. Panel (b) reports how relaxed participants appeared during the task, measured on a 0–3 scale, for mixed-caste pairs only. Bars show group means with 95 percent confidence intervals. Reported p-values are from tests of equality of means across caste groups. OBC participants are excluded.

Figure A9: Reading and typing ability is similar for General and Scheduled Castes



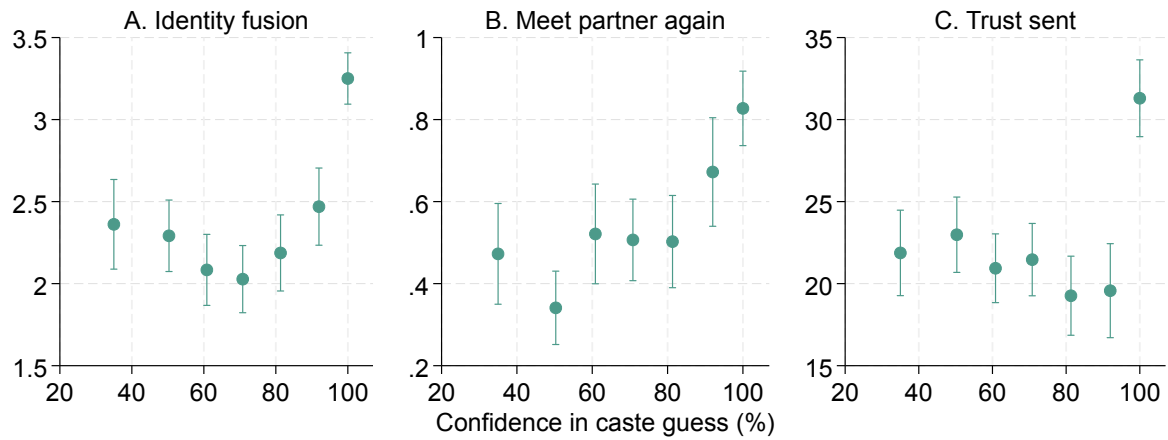
Notes: The figure shows kernel density estimates of reading and typing percentile ranks for General and SC participants using data from the baseline productivity tasks. Percentiles are constructed within task, with higher values indicating greater productivity. Vertical dashed lines indicate group means.

Figure A10: Concealment hurts more if caste identity is salient



Notes: Each panel plots the marginal effect of concealment (first name only or hidden) relative to the revealed condition on the outcome indicated in the title, at each level of own caste identity salience. Identity salience is measured by participants' pre-experiment ranking of five identities (caste, religion, Indian, from U.P., and father/brother/son), where rank 1 denotes the most salient identity; ranks 1–2 are pooled due to small cell sizes. Marginal effects are derived from an OLS regression of the outcome on an indicator for concealment, the salience rank (entered continuously), and their interaction, with controls for age, education bin, partner's salience rank, and demeaned randomization strata fixed effects. Standard errors are clustered at the pair-level. Shaded bands denote 95% confidence intervals. The subtitle reports the marginal effect at ranks 1–2 and rank 5, and the estimated interaction slope (change in concealment effect per unit increase in rank). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

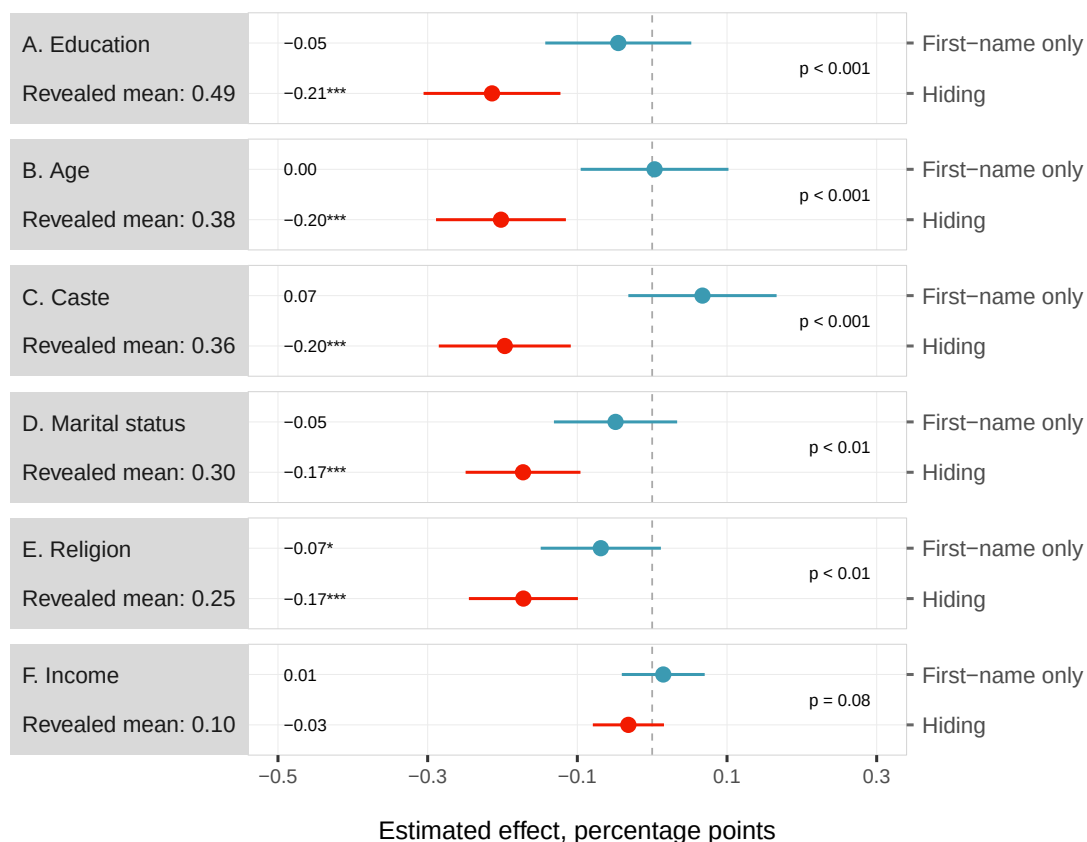
Figure A11: Is there a certainty premium in intergroup relations?



Notes: The figure plots adjusted mean predictions from OLS regressions of each outcome on confidence bins, controlling for dummy for *Hidden*, dummy for guessing partner is General-caste fully interacted with whether participant is General-caste, age, education, and randomization strata (General-caste dummy interacted with work date). The sample is restricted to the *First Name Only* and *Hidden* arms. Standard errors are clustered at the pair-level. Confidence in the caste guess is grouped into seven bins: below 50%, 50–59%, 60–69%, 70–79%, 80–89%, 90–99%, and 100%. Each point is plotted at the mean confidence within the bin; capped lines are 95% confidence intervals.

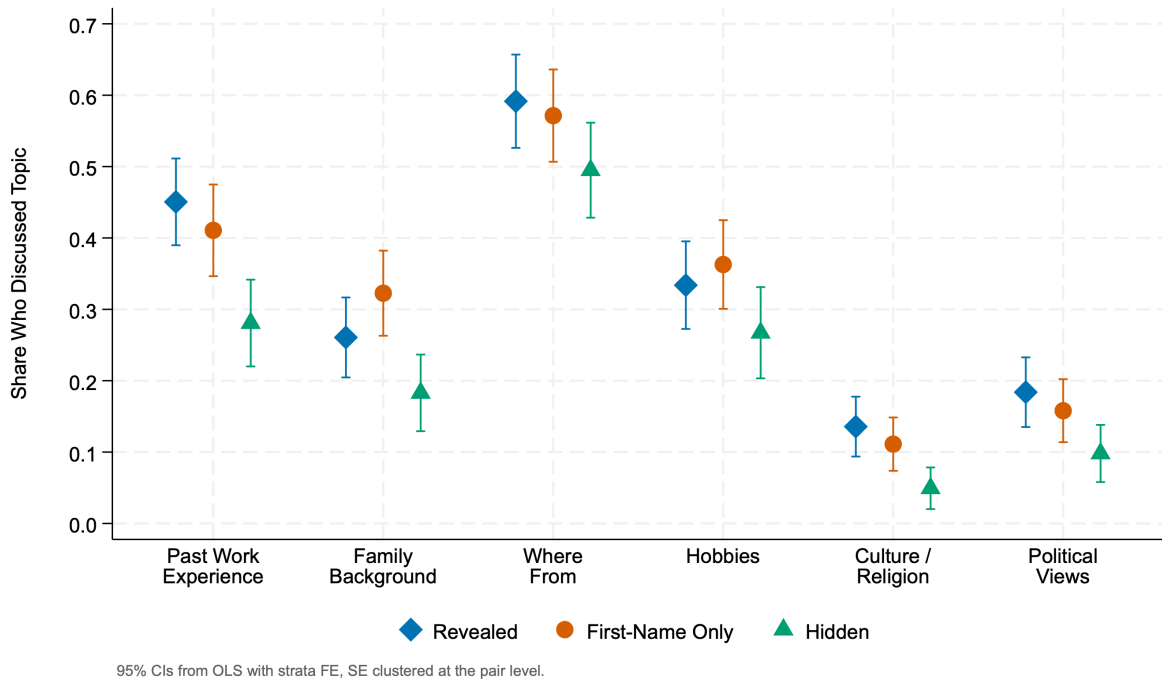
Figure A12: *Hidden* reduces questions about identity-linked topics.

Partner asked about:



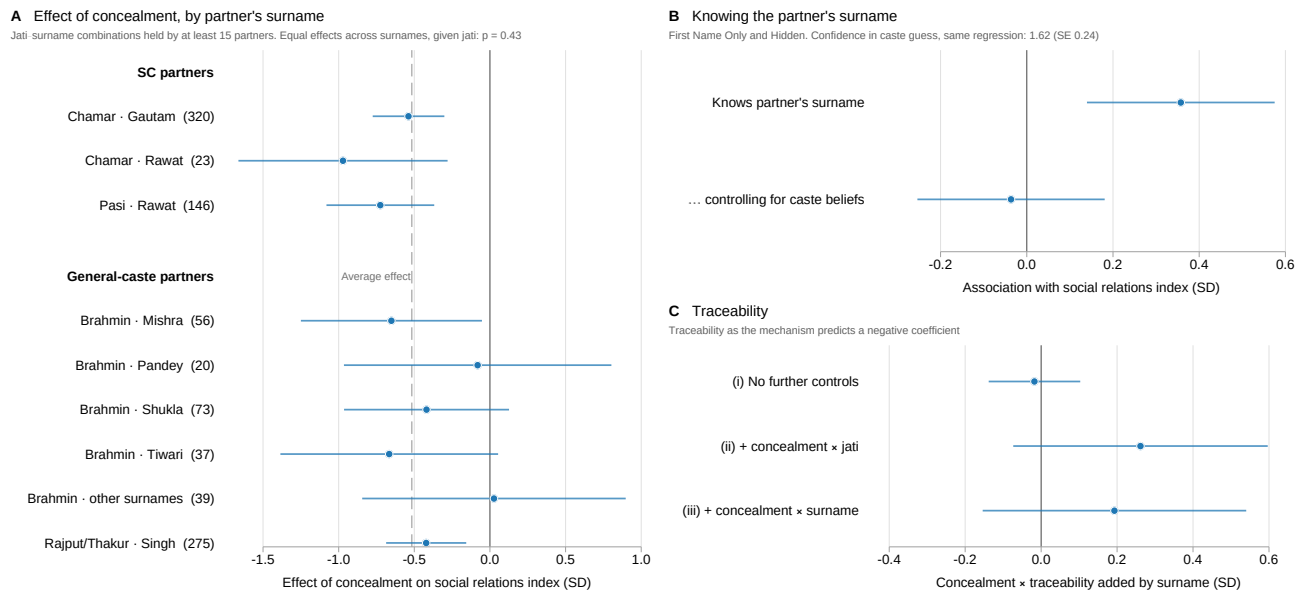
Notes: The unit of analysis is the participant. The outcomes measure whether the participant reports that their partner attempted to discover specific personal characteristics during the work session. In particular, participants were asked whether their partner tried to discover their age, education level, family income, caste (e.g., by asking their last name), marital status, or religion (0 = No, 1 = Yes). Each point reports the estimated treatment effect relative to the *Revealed* condition. The *Revealed mean* is the raw mean of the outcome for participants in the *Revealed* condition. The *First Name Only* and *Hidden* estimates are coefficients from regressions that control linearly for participants' age and include randomization-strata and education-bin fixed effects. Horizontal bars show 95% confidence intervals based on standard errors clustered at the pair level. Reported p-values test the null hypothesis that the *First Name Only* and *Hidden* treatment effects are equal. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure A13: Topics discussed with partner by treatment



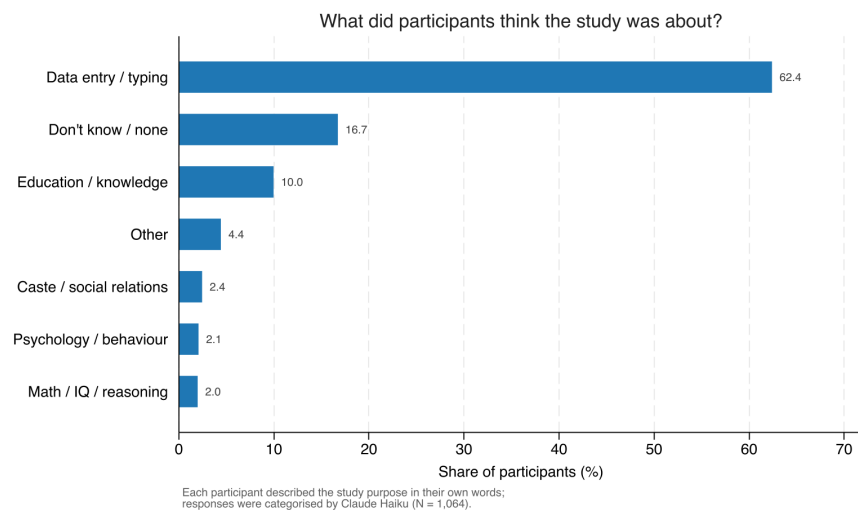
Notes: Each point is the share of participants who reported discussing that topic with their partner. Estimated via OLS with no constant, including demeaned strata fixed effects; 95% confidence intervals shown. Standard errors clustered at the pair-level. All regressions control linearly for participants' age and include randomization-strata and education-bin fixed effects. Sample pools SC and GEN participants restricted to the main analysis sample.

Figure A14: Surnames matter through caste rather than other information



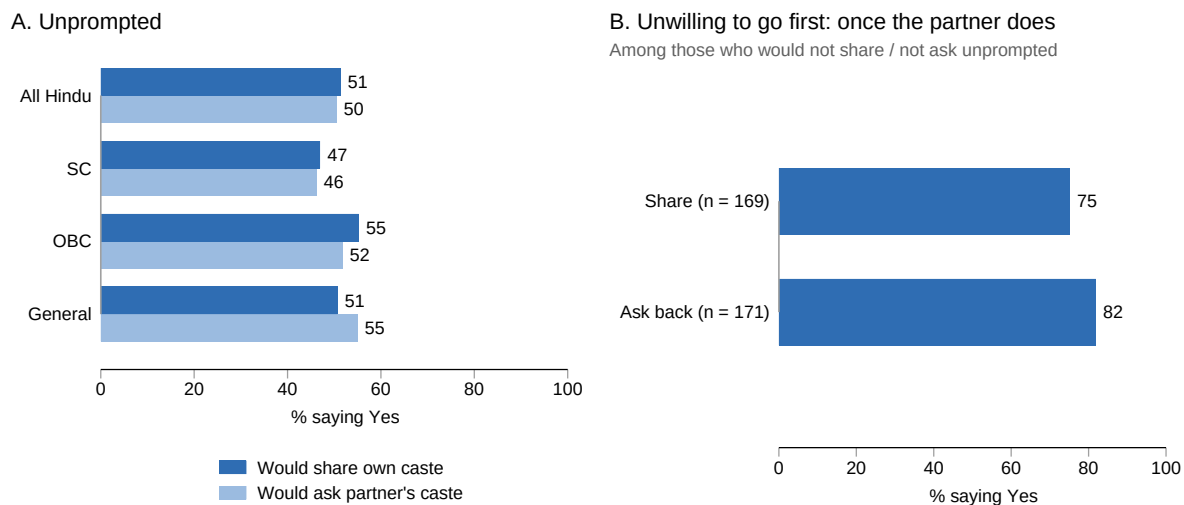
Notes: Outcome is the social relations index (Revealed-arm SDs); controls and clustering as in the main specification; 95% confidence intervals. Panel A: effect of concealment by the partner's jati and surname, for combinations held by at least 15 partners (number in parentheses); rarer surnames are pooled. The dashed line is the average effect. Panel B: concealment arms only; "knows surname" means the respondent stated it correctly at endline; the second row adds controls for caste beliefs and confidence. Panel C reports the interaction between a pooled concealment dummy and a standardized traceability measure: the log ratio of the number of male household heads sharing a partner's given name to the number sharing his full name. Counts use records across U.P. partners whose full names have no match are represented by a separate indicator and its interaction with concealment. Specifications (ii) and (iii) in addition allow concealment effects to vary by partner's jati and surname, respectively.

Figure A15: The purpose of the study, according to endline respondents.



Notes: This figure summarizes endline subjects' reports of the purpose of the study. Subjects were asked at endline, "What do you think was the purpose of this research study? What was it trying to understand?" Subjects' qualitative responses were recorded by enumerators and categorized using Claude.

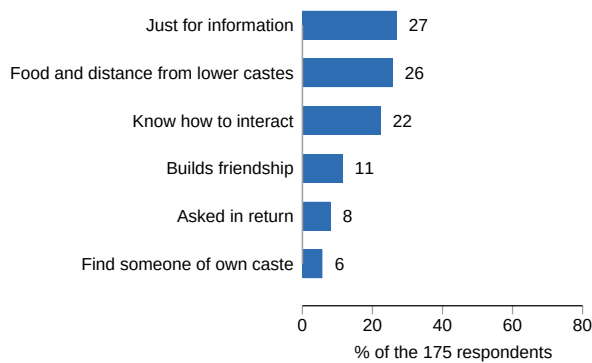
Figure A16: Disclosing caste to a new work partner, and reciprocity (labor-stand survey)



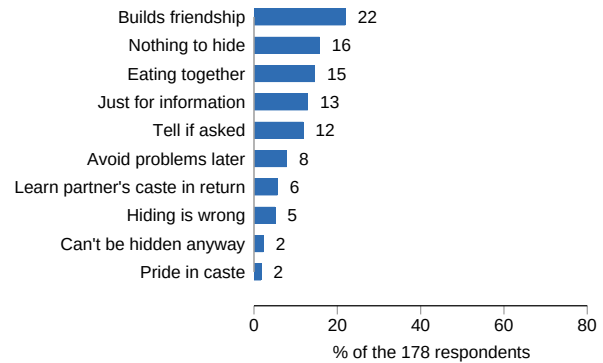
Notes: Follow-up survey of men aged 18–40 at labor stands in Lucknow; Hindu respondents. Panel A: share who would tell a new partner on a temporary job their own caste, and who would ask the partner's caste. Panel B: among those who would not share (ask) unprompted, the share who would share their caste if the partner shared his first (would ask the partner's caste if the partner asked theirs). Sample sizes are in the panel labels.

Figure A17: Reasons for discussing or avoiding caste with a new work partner (labor-stand survey)

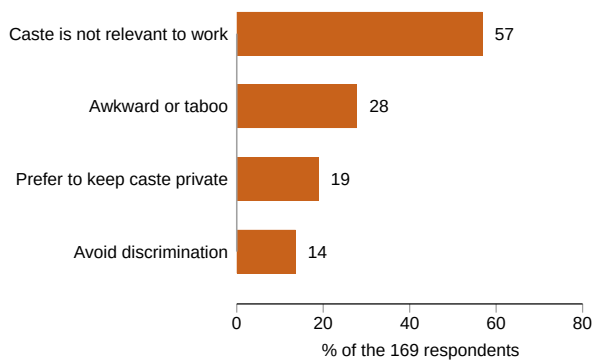
A. Would ask: reasons (n = 175)



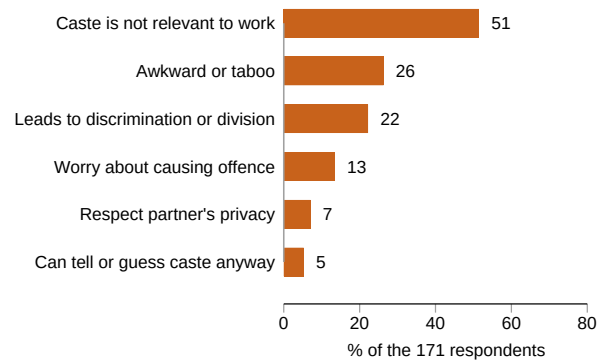
B. Would share: reasons (n = 178)



C. Would not share: reasons (n = 169)



D. Would not ask: reasons (n = 171)



Notes: Follow-up survey of men aged 18–40 at labor stands in Lucknow; Hindu respondents. Panels A–B: respondents who would ask their partner's caste (A) or share their own (B), giving the reason in their own words; answers were translated and grouped into themes. Panels C–D: respondents who would not share (C) or not ask (D); reasons as coded by the enumerator, several allowed. Each panel's denominator is shown in its title.

D Supplementary text

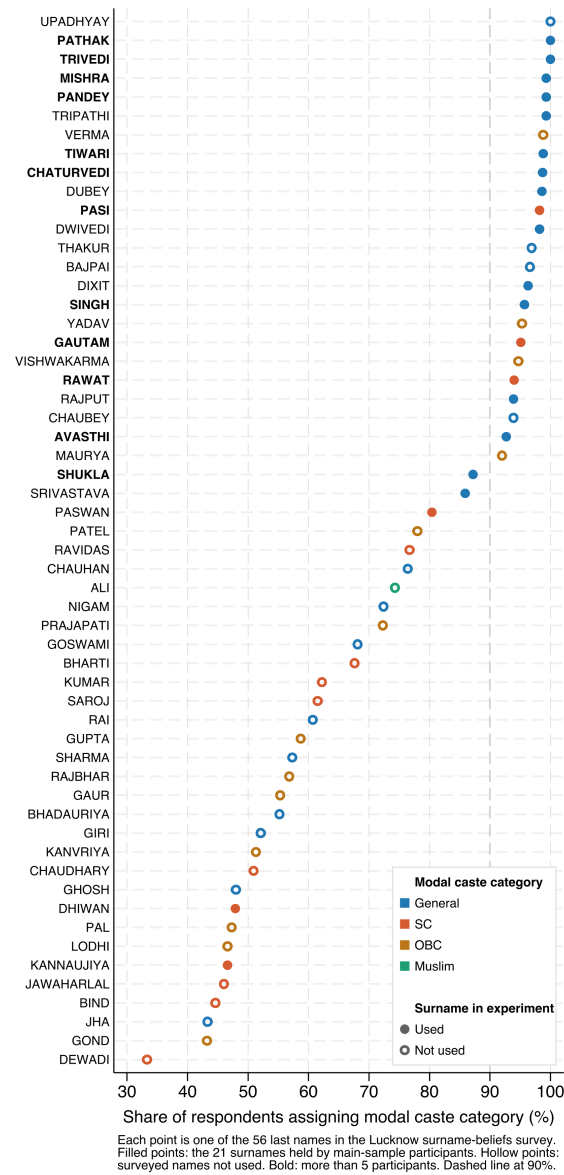
D.1 Selecting caste salient last names for the experiment

We conducted a supplementary beliefs survey to assess the caste informativeness of surnames with 460 residents of Lucknow: 172 General-caste, 194 OBC and 94 SC respondents. Each respondent was asked about 18 or 19 randomly drawn surnames from a list of 56 surnames. Participants reported the caste category associated with that surname and their confidence in their judgment. Figure A18 distinguishes the surnames used in the experiment from other names included in the survey. The surnames most frequently used in the experiment (at least 5 participants with that last name, marked in bold) have modal-category shared above 87%.

The experimental surnames also have high distinctiveness in the SECC data. Within the *Revealed* arm, the tenth percentile of partner-surname distinctiveness is 94.5% (Figure A19). Thus we have limited variation for studying how effects change as names become less informative. Our findings must therefore be read as the consequences of withholding salient identity cues among workers whose surnames are caste-informative.

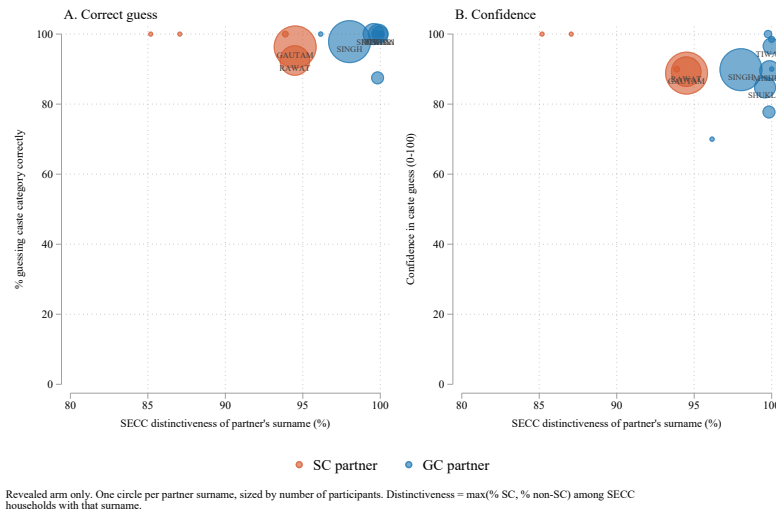
Figure A20 shows that surnames in our experiment reveal very little about a participant's familial or residential associations, again using SECC data. For the median participant, the most common village for his surname holds under 5% of the households sharing it; across participants in the main sample, the average is 3.8%. This assuages concerns that our concealment treatments may be contaminated by signals beyond caste that last names provide.

Figure A18: Caste informativeness of last names



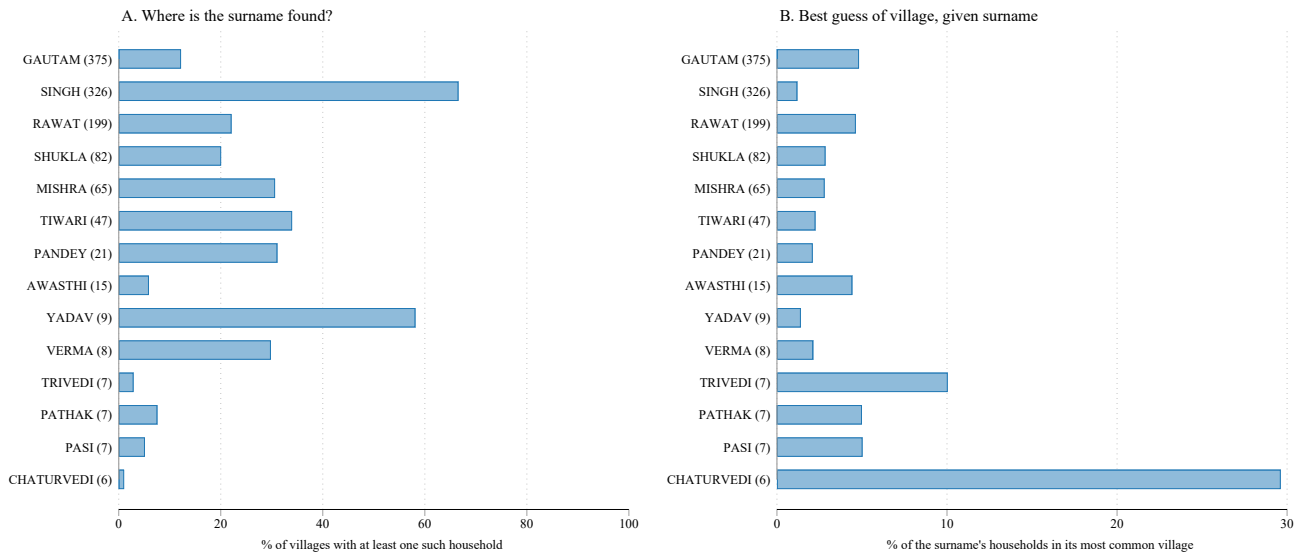
Notes: Each point is one of 56 last names in a survey of residents of Lucknow, who were asked the caste category of each name. The horizontal axis is the share of respondents choosing the most common (modal) category; color shows that category. Filled points are the 21 surnames held by participants in the main experiment; hollow points are surveyed names not used in the experiment. Bold labels mark surnames held by more than five participants in the experiment.

Figure A19: Caste legibility of partners' surnames in the Revealed arm



Notes: Revealed arm only. Each circle is a partner surname, sized by the number of participants. Distinctiveness is the larger of the SC and non-SC shares among SECC 2011 households with that surname. Panel A: share guessing the partner's caste category correctly. Panel B: confidence in the guess (0–100).

Figure A20: Surnames reveal little about a man's village



Notes: The figure uses the Socio-Economic and Caste Census (SECC) 2011 list of rural households in Lucknow and Ayodhya (Faizabad) districts, restricted to male household heads. A village is a unique combination of district, tehsil, gram panchayat and village (2,062 villages). A head's surname is the last word of his name, with transliteration variants harmonised using the same spelling rules we apply to participants' names. The figure covers the 14 surnames held by at least five participants; the number of participants with each surname is in brackets. Panel A shows the share of villages with at least one household whose head has the surname. Panel B shows the share of the surname's households that live in the single village where the surname is most common. This is the probability of guessing a man's village correctly from his surname alone, when the guess is the surname's most common village. As a benchmark, the largest village holds 0.38% of all households, which is the success rate of a guess that ignores the surname.

D.2 Deviations from pre-analysis plan

We followed the pre-registration in both outcome construction and empirical analysis. The deviations are minor and listed below.

- **Meal-sharing outcome.** The pre-registration included a meal-sharing measure as part of the social ties outcomes. Due to an implementation error, enumerators did not distribute the registration slips after the first two days of the study. We therefore cannot include this outcome in the analysis.
- **Well-being outcomes.** Ideally, we would have controlled for baseline well-being when estimating treatment effects on well-being measured at endline. However, we discovered an implementation error: the baseline well-being questions were not translated into Hindi. We therefore do not use the baseline well-being measure, and omit the baseline outcome control in these regressions.

D.3 Ethical considerations

The study protocols were separately reviewed and approved by four Institutional Review Boards, at the University of California San Diego, University of California Davis, Duke University and University of British Columbia.

D.3.1 Informed consent and experimental design

We obtained informed consent from all adult participants before participation. The study was conducted in a supervised workplace setting, and participants were adequately compensated for both attendance and performance. Because the object of interest is how identity visibility shapes interaction, participants were not told in advance that caste was the focus of the study. This was necessary to limit experimenter demand effects and strategic behavior that would have altered the interactions we aimed to study. The intervention itself was deliberately light-touch, operating through small differences in how partners were introduced and, in one condition, whether they were instructed not to reveal caste-linked information.

D.3.2 Risks to participants

The main ethical concern is that revealing or concealing identity may affect participants' comfort, trust, or sense of authenticity during the interaction. We reduced these risks by conducting the study in a controlled environment and avoiding any explicit discussion of caste by the research team during the work session (with the exception of the instructions to not disclose caste in the *Hidden* condition). Participants were not asked to disclose sensitive information beyond what arose naturally in the course of interaction. The structured setting likely reduced the scope for more extreme forms of discriminatory behavior. Consistent with this, we do not find evidence for large well-being differences across the treatment arms.

D.3.3 Interpretation of results

Our findings should not be interpreted as a general normative claim that identity concealment is harmful or that disclosure is always beneficial. The experiment identifies the effects of identity legibility on a specific margin: social and workplace interactions in a formal setting. In other contexts, limiting the visibility of identity may protect individuals from more severe forms of discrimination, exclusion, or violence, which our design is not intended to measure.

More broadly, our results should not be read as placing the burden of better social relations on members of marginalized groups. If reduced identity uncertainty improves trust and collaboration, the relevant implication is not that disadvantaged individuals ought to reveal more, but that institutions should be made safer and more inclusive so that identity does not carry such acute social risk in the first place.