

Familiar Strangers:

Evidence from Referral-based Hiring Experiments in India

Kartik Srivastava^{*†}

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ABSTRACT. In developing-country labor markets, search frictions and network-based hiring sustain high churn and exclude poorly-connected workers, suggesting a potential for misallocation. I test whether redirecting referrals toward underrepresented minority workers in such segmented labor markets can raise both equity and productivity. In an Indian manufacturing firm, I experimentally increased the share of referral invitations allocated to minority lower caste incumbents. The policy raised their employment share by 15 pp (62%) and raised team output by 0.09 s.d. (5%). This improvement in production was driven by reduced monthly turnover which was lower by 4 pp (41%). Importantly, treatment did not induce declines in worker cohesion, contrary to common concerns about diverse and heterogeneous teams. A supplementary lab-in-field experiment pins down the mechanism: when lower caste workers enter as outsiders rather than via referrals, cohesion falls by 9% and output 22%, implying that recruitment mode – not entrant identity – drives short-run costs. At the firm, supervisors exposed to the policy continued to allocate referrals to lower caste workers after the intervention concluded, consistent with Bayesian learning which I rationalize with a model. Lower caste referral candidates saw large gains in job offers and employment, with no detectable displacement of upper caste candidates. Redirecting referrals can thus reassign jobs toward workers with low outside options, improving their labor market prospects as well as firm performance.

^{*}Harvard University; kartiksrivastava@g.harvard.edu; [Personal Website](#).

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1 Introduction

Search and matching frictions in developing country labor markets help sustain an equilibrium with large numbers of unemployed or underemployed jobseekers coexisting with a large number of firms struggling to recruit and retain workers (Breza and Kaur, 2025; Manpower Group, 2024). Given these frictions and a large degree of informality in these labor markets, the vast majority of jobs are matched through referrals – over 70% at the entry-level in particular (Chandrasekhar et al., 2020). Referrals have been shown to minimize search costs for both workers and firms, but a reliance on them concentrates benefits among those workers who have higher quality networks (Calvo-Armengol and Jackson, 2004; Beaman et al., 2018). They mechanically increase outside options for workers who are already prone to high turnover rates, thus making retention at the firms worse. Moreover, they trap workers with worse networks without access to good jobs.

This pattern is suggestive of segmentation in these labor markets by workers’ outside options. It is a common empirical regularity that job mobility correlates with demographic characteristics across contexts (Arrow and Borzekowski, 2004; ILO, 2024; Donovan et al., 2023). This indicates a misallocation problem: the reliance on networks in aggregate gatekeeps jobs away from those who likely have low reservation wages. In India, a primary vector of this type of segmentation is caste. In an original analysis of India’s nationally representative 2018–19 Periodic Labor Force Survey, I find a strikingly bimodal distribution of outside options. Lower castes make job-to-job transitions 25% less often than upper castes, are 46% more likely to be in rural areas, and 13% more likely to be unemployed, controlling for a range of other demographic characteristics like age, gender, and education. Redirecting referrals through lower-opportunity networks can shift jobs toward workers with low reservation wages—delivering equity and, via higher retention, productivity gains in a context where wages cannot adjust.¹

This paper studies if it may be possible to improve both equity and productivity if firms make better use of referrals from existing workers to hire workers from underrepresented minority groups. I study this in the context of caste in India through a large-scale field experiment in a manufacturing firm, with two supplementary lab-in-field experiments to elicit mechanisms for my results, and a Bayesian learning model to rationalize the trajectory of beliefs and output at the firm. The firm that the main experiment is set in is representative of typical manufacturing firms in developing country contexts: labor turnover is high, hiring happens predominantly through referrals, and the frontline workers would be classified as informal given the absence of formal contracts. In addition, referral

¹In much of the low-skill entry-level labor market in developing countries, wages are bound below by minimum wage laws, eliminating the firm’s ability to extract rents through wage cuts despite workers’ varying reservation wages. In this setting, all workers in the experiment receive the same minimum wage. Moreover, slack labor market conditions provide the firm with replacement workers easily, making efficiency wages unnecessary for retention improvements in their perception. This institutional context means that screening and retention gains must come through non-wage mechanisms like the selection margin, such as referral allocation.

opportunities, i.e. which of a team's incumbent workers are asked to bring a friend or relative to fill a vacancy, are disproportionately invited from upper caste incumbent workers. During the intervention period, referral allocations in treatment teams are made to incumbent lower caste workers, i.e. each time there is a vacancy, an incumbent lower caste worker is chosen randomly to bring a referral candidate to the firm.² I monitor personnel outcomes and output through administrative data from the firm on worker entry-exit and the units produced at the team level, respectively, at a high temporal frequency. In the lab setting, I replicate the careful measurement of output in a stylized task, and supplement it with a revealed measure of worker cohesion through a behavioral game.

Over the six months of the intervention, the share of lower caste workers in treatment teams increased by 15 percentage points – a 62% rise. The net effect of increased lower caste representation at the firm is theoretically ambiguous *ex ante*, depending on retention gains, any ability differences by caste (Neal and Johnson, 1996), the effects of diversity on workplace cohesion (Hjort, 2014; Ghosh, 2025), and any potential disutility from mixing with out-groups. In my setting, I find that the net effect on output is unambiguously positive. Over the six-month period, team-level output rose by 5% (0.09 standard deviations, *p*-value = 0.01). Importantly, the improvements in output continued after the experiment concluded, with the pooled effect over a 12-month period rising to 6% (0.11 standard deviations, *p*-value = 0.01).

I distill from the set of possible drivers for the output result to two that are particularly important in my setting: retention and workplace cohesion. Retention in treatment teams improved markedly. Monthly churn (defined as the sum of exits and entries) fell by 4 percentage points (41%) relative to control teams, driven primarily by fewer exits among newly hired lower caste workers. Treatment teams experienced 0.6 fewer exits and 0.7 fewer entries per month on average. Baseline survey data indicate that lower caste referrals were 65% more likely to be unemployed and disproportionately coming from rural areas compared to their upper caste counterparts, suggesting that their weaker outside options contributed to longer tenures at the firm. This translated to larger output gains in tasks requiring close coordination among teammates where the returns to retention and team stability are largest. In addition, I find no short-run output declines and no adverse effects on self-reported cohesion, which one might expect based on the literature on mixed teams, and was also a fear expressed by supervisors *ex-ante*.³ Both the retention and cohesion channels pull in the same direction to drive the positive treatment effects on output.

This intervention had lasting effects on firm behavior. Five months post-experiment, supervisors persisted in allocating referrals disproportionately to lower caste workers. Notably, even after the intervention ended, the share of referrals going to lower caste workers in treatment teams remained above their (now increased) share of the team's composition. In contrast, supervisors in

²Compliance with the assigned referral rules was high: 92% on average at the team-by-month level.

³I supplement these self-reported measures with revealed-preference data on cooperation in the lab-in-field extension.

control teams largely reverted to pre-intervention referral patterns. This suggests that supervisors in the treatment group updated their beliefs through exposure to the policy in the form of an enforced period of experimentation. This evolution of beliefs suggests learning, and that the driver of the initial wedge in allocation was not taste-based discrimination, since that would be immutable.

To interpret these findings, I develop a dynamic model of referral allocation in which supervisors begin with prior beliefs about the cohesion costs of cross-caste hires and may also exhibit taste-based discrimination. Calibrated with baseline survey data and incorporating Bayesian updating under different referral policies, the model shows that pessimistic priors can prevent supervisors from experimenting with lower caste referrals – even when doing so would improve retention and output eventually.⁴

Importantly, the change in the firm’s recruitment policy led to large improvements in the labor market prospects for lower caste entrants, without noticeable declines among their upper caste analogs. Before implementing the firm experiment, I built a network of potential referral candidates connected to the incumbent workers. This allows me to assess downstream effects on job seekers, comparing outcomes for both upper and lower caste referral candidates in treatment versus control teams. Among lower caste candidates linked to treatment teams, I find a 52% increase in the number of job offers received relative to their control counterparts. There is no corresponding effect for upper caste candidates. The improved job prospects for lower caste candidates translate into a 17% higher likelihood of securing paid employment and a 29% reduction in the duration of the most recent unemployment spell. Crucially, I find no evidence of displacement: upper caste candidates in treatment teams secure job opportunities at rates comparable to those in the control group. The absence of displacement can be reconciled by the fact that upper caste referral candidates are more likely to be inframarginal, i.e. in salaried employment prior to the intervention.⁵ These results are consistent with canonical models of occupational choice, where outside options and labor market trajectories are shaped by initial wealth and opportunity distributions (Banerjee and Newman, 1993).⁶

⁴This framework parallels the literature on strategic experimentation and multi-armed bandit problems (Gittins and Jones, 1979; Bolton and Harris, 1999; Aghion et al., 1991; Exley and Kessler, 2024; Li et al., 2025), where agents facing uncertainty avoid exploring potentially better options due to risk aversion or short-term incentives. In my setting, mandatory experimentation removes these frictions: by making exploration costless and enforced, the intervention allows treatment supervisors to update their beliefs and adopt more inclusive referral strategies that persist beyond the experimental period.

⁵More generally, job-to-job transitions in developing country contexts have been shown to be more likely to be associated with lateral rather than upward mobility. Donovan et al. (2023) show that workers who transition to higher-wage jobs in developing countries are 39 percentage points more likely to exit employment or revert to lower-wage work within a quarter, and developing economies exhibit such transition rates twice at a level that is twice as high as in richer countries. These trajectories are often segmented by demographics. In the Indian context, Abraham and Kesar (2025) show that only 6.7% of workers ever reach stable formal salaried jobs, while over 70% cycle among precarious informal trajectories, and that lower-caste workers are over-represented in the most volatile and lowest-paid paths.

⁶These estimates are, however, partial-equilibrium. They assume other firms and market wages remain unchanged. In a general-equilibrium setting, widespread adoption of the policy would gradually raise the outside options of lower-

Despite the gains to productivity we observe ex post, there are several reasons why supervisors at the firm may not expect this to have been profitable ex ante: lower castes could have had lower initial ability, diversity could have led to at least short run declines in productivity due to intermediate effects on cohesion, taste-based discrimination, fears that lower caste referrals may not convert to recruits because of migration costs, among others. More generally, supervisors may hold preferences against mixed teams that outweigh expected productivity gains, or they may be risk-averse, overweighting potential short-term disruptions even when long-term returns are positive. I find no support that they see lower castes as having less ability or not being able to join the firm at lower rates, but I find that supervisors do perceive lower caste referrals to trigger cohesion declines at baseline.⁷ Note also that a sufficient test for the absence of large cohesion costs (and any other costs associated with diversity such as lower initial ability among lower castes) would materialize in a short-run decline in output, which I can reject with a high degree of statistical confidence.

This points to a tension: cohesion declines do not materialize from lower caste referrals joining the firm, contrary to supervisor expectations and recent literature.⁸ What explains the lack of these declines? I answer this through two lab in field experiments. First, I discuss results from a vignette experiment that elicits worker preferences on recruitment modes with workers drawn from the same population as at the firm. Lower caste entrants hired as “strangers”, i.e. with no incumbent links, were expected to disrupt cohesion and reduce output. But framing the same entrant as an incumbent’s referral largely neutralized these concerns – mirroring what we find at the firm.⁹

Second, I conducted an incentivized lab-in-field experiment with nearly 500 participants, again drawn from a similar population as the firm’s workforce. Held across 12 workshops, this setting allows for tight control over task structure, recruitment conditions, and data collection – closely replicating the firm’s production environment. Each team began with three members (one lower caste, two upper caste) and completed two production rounds, separated by a recruitment phase that increased team size to four. I randomized teams into one of three recruitment modes: (1) referral by the lower caste incumbent; (2) referral by an upper caste incumbent; and (3) direct hiring of a

caste workers and could compress the retention premium that drives these results. I discuss in the conclusion why such wage adjustments are likely to be gradual in fragmented manufacturing labour markets, and why persistent differences in network access mean the mechanism should retain much of its force even after partial wage equalisation.

⁷On average, lower castes have lower levels of formal education or training, despite steady gains in recent decades (Hnatkovska et al., 2013). However, I focus on entry-level jobs, where prior experience or inherent ability play only a limited role. I find no difference in actual or perceived education in my sample across castes.

⁸Hjort (2014) finds short-term productivity declines in ethnically diverse teams in a Kenyan manufacturing firm. Ghosh (2025) shows similar effects when varying religious composition in an Indian plant, particularly for tasks requiring cooperation. In my study, treatment assignment is stratified by the coordination intensity of tasks to account for such variation.

⁹There are parallels to this pattern in other contexts where giving agents discretion improves cohesion. Endogenously chosen policies are more likely to foster cooperation and acceptance than those imposed from above (Dal Bó et al., 2010). Teams formed discretionarily through friend networks are more cohesive than those created exogenously (Anders and Pallais, 2025).

lower caste “stranger” without incumbent links.¹⁰

The results from the lab-in-field experiment echo those from the firm and shed light on why cohesion costs do not emerge. Both treatment conditions under which lower caste entrants join the team begin with similar output levels and experience the same nominal increase in diversity. Selection is held constant across arms by recruiting candidates similarly prior to allocating them to teams as either referrals or “strangers”, following [Pallais and Sands \(2016\)](#). Yet recruitment mode matters: in teams where the fourth worker is not hired through referrals, there is no significant gain in output. In contrast, when a similar worker is framed as a referral from an incumbent, team output rises by 22% ($p < 0.01$). In addition, unlike the firm experiment, the lab-in-field experiment allows for revealed-preference measurement of cohesion, using adaptations of behavioral cooperation games ([Schofield, 2014](#); [Breza et al., 2018](#)). These tasks capture how well randomly matched pairs of workers within teams cooperate. I find that cohesion is 9% higher in lower caste referral teams than in lower caste stranger teams (p-value = 0.02).¹¹

This paper contributes to a broad literature on labor market dynamics, social networks, and workplace diversity – particularly in contexts where segmentation and network access shape hiring. It builds on foundational models of job search and information frictions ([Jovanovic, 1979](#); [Montgomery, 1991](#)) and a growing body of work on the costs of high turnover in firms in developing economies ([Adhvaryu et al., 2023](#); [Boudreau, 2024](#); [Choudhury et al., 2023](#); [Donovan et al., 2023](#); [Halvorsen, 2021](#); [Shiferaw et al., 2020](#); [Skelton et al., 2019](#)). Closely related is the literature on social networks in recruitment. Studies in high-income settings typically use matched employer–employee data to compare outcomes for referred versus non-referred workers ([Brown et al., 2016](#); [Burks et al., 2015](#); [Dustmann et al., 2015](#); [Kugler, 2003](#)), while work in developing countries emphasizes referrals as a mechanism for mitigating adverse selection and moral hazard ([Heath, 2018](#); [Pallais and Sands, 2016](#)) or as a lens into referrer behavior and candidate selection ([Beaman and Magruder, 2012](#); [Beaman et al., 2018](#)). Another strand of literature is on the role of referrals and network connections more broadly in helping match workers who face access barriers to jobs, in particular in entry level roles ([Armona, 2025](#); [Egger et al., 2022](#); [Barwick et al., 2023](#); [Pallais, 2014](#)). My paper contributes to this literature by focusing on an upstream but understudied aspect: how referral opportunities are allocated among incumbent workers, and how this allocation affects team composition, retention, and productivity.

This paper also relates to a classic line of inquiry in labor economics on spatial and informational frictions, particularly the exclusion of disadvantaged groups from certain jobs or sectors ([Chetty](#)

¹⁰The first two mirror the treatment and control arms of the firm experiment, while the third mimics diversity-focused hiring policies that ignore existing team ties.

¹¹This gap appears both in incumbent-only pairings and in pairings involving the new hire. This suggests that the recruitment mode shapes *overall* team cohesion, not just the dyad between referrer and recruit. Disaggregating by caste pairing further reveals that the treatment effect is driven by stronger cooperation in cross-caste pairs; no significant effects are observed among same-caste pairs.

et al., 2014; Kain, 1968; Moretti, 2012; Hsieh et al., 2019; Bryan et al., 2014). My empirical findings demonstrate how limited network access can sustain misallocation.

Methodologically, my approach is distinctive in combining intra-firm variation in referral policies with evidence from a real production environment.¹² I embed the intervention within an existing firm and complement it with lab-in-field experiments (Bandiera et al., 2011; Hoffman and Stanton, 2024).

My findings also speak to literatures on workplace cohesion and the role of management in shaping productivity (Lazear, 2000; Mas and Moretti, 2009; Roethlisberger and Dickson, 1939; Bandiera et al., 2005; Bandiera et al., 2009; Fershtman and Gneezy, 2001; Prat, 2002; Hjort et al., 2025). Recent experimental evidence shows that conditions that leave individual output unchanged can still sharply depress team performance by raising coordination costs (Garg et al., 2025b). Another strand of research examines how diversity affects trust and output, often identifying short-run costs from coordination burdens or latent biases (Alesina and La Ferrara, 2005; Darova and Duchene, 2024; Ghosh, 2025; Hjort, 2014; Ottaviano and Peri, 2006). This paper demonstrates how a network-based recruitment strategy can reduce turnover while avoiding the cohesion losses that often accompany diversity interventions.

Beyond economics, the paper connects to sociology and social psychology on intergroup contact and individuation: people update beliefs about out-groups when exposure is repeated, structured, and individuating rather than purely categorical (Allport, 1954; Gaertner and Dovidio, 2000; Fiske and Neuberg, 1990; Brewer, 1999). Organizational network theory adds a mechanism for how such exposure can be made productive. Referrers can play a *tertius iungens* role – the “third who joins” – by actively forging ties between incumbents and entrants, transferring trust and work norms, and converting a perceived outsider into an ingroup-linked collaborator (Obstfeld, 2005; Burt, 2005; Coleman, 1988; Granovetter, 1973; Gaertner and Dovidio, 2000). I extend this insight to recruitment: when a hire arrives as an incumbent’s referral, the referrer brokers introductions, vouches for quality, and conveys expectations, lowering integration and coordination costs even if broader out-group attitudes remain unchanged. In this sense, referral-based hiring operates as a networked contact intervention – leveraging brokered ties to facilitate cooperation and stabilize teams – rather than a generic diversity shock.

The paper also has theoretical roots in models of networked labor markets, including those of Calvo-Armengol and Jackson (2004) and Bolte et al. (2024), who show how network structures shape employment allocations. My experiment tests these predictions in a real-world setting, linking referral networks to retention, cohesion, and productivity outcomes. Overall, my results suggest that it may be possible for firms to improve both efficiency and equity in low-cost ways, and have

¹²Prior research often relies on administrative data – where referral links are inferred as opposed to identified – or on stylized experiments where researchers act as employers. A potential limitation of these approaches is that they may not be able to fully capture the relational dynamics between firms and incumbents that shape referrals and workplace integration.

several policy implications, which are further contextualized and discussed in greater detail in the conclusion.

The rest of the paper proceeds as follows. In section 2, I provide some background on the context, and some descriptive motivating evidence through stylized facts. In sections 3 and 4, I present the experimental design and the empirical strategy respectively. Section 5 proposes a dynamic theoretical model rationalizing the firm’s referral allocation process. I discuss the results from the firm experiment and the lab-in-field extension in section 6. I conclude in section 7.

2 Background

2.1 Churn in developing country labor markets

Worker turnover (or labor market “churn”) is a pervasive feature of labor markets in low- and middle-income countries (LMICs). Relative to rich countries, employment relationships are shorter and separations more frequent. Job switching among wage workers is about three times higher in India and Ecuador than in the United States or Sweden, and churn in these settings tends to be lateral and not suggestive of progression on the job ladder (Donovan et al., 2023). Cross-country evidence emphasizes the central role of informality and unstable employment in shaping labor dynamics across developing regions, and much matching occurs through informal channels (World Bank, 2013).

High churn is costly for firms and workers alike. Frequent separations erode firm-specific human capital, raise recruitment and training costs, and disrupt team production and quality control, channels emphasized in the personnel economics literature (Acemoglu and Pischke, 1999; Bloom et al., 2016). Consistent with this, practices which improve retention and co-worker fit yield broad performance gains—e.g., referrals have been shown to raise profits by lowering turnover (Burks et al., 2015), managers with stronger people-management skills dramatically reduce attrition (Hoffman and Stanton, 2024), and recent reviews synthesize large productivity returns from better personnel practices and match quality (Hancock et al., 2013; Brown et al., 2016).

Specifically in my setting, recent evidence from Indian manufacturing underscores both the scale and the costs of churn. In large garment factories, only about half of workers are retained over a year, and quarterly quit rates are roughly 10%, magnitudes that imply substantial replacement and training costs for firms (Adhvaryu et al., 2023). Complementary work on a worker-voice technology highlights churn as a first-order margin in these plants and points to organizational practices that can reduce separations by improving problem-solving and match quality (Adhvaryu et al., 2022). Across this setting and other similar contexts, while churn has been established as a first-order cost for firms, the literature on interventions that limit it or its impact on output are scarce.

2.2 Referrals in developing country labor markets

In developing economies, informal mechanisms dominate recruitment processes, particularly for low-skilled and entry-level manufacturing jobs. Primary among them is the use of referrals from incumbent workers, which is particularly prevalent in settings where employers face significant informational asymmetries with respect to the reliability and productivity of potential workers. Referrals are especially common in manufacturing sectors where tasks are repetitive, entry-level positions tend to be abundant, and worker misconduct or mistakes can severely disrupt productivity. In such settings, referrals fulfill the dual role of improving the firms' screening and mitigating moral hazard. Over 70% of jobs in the manufacturing sector in India are estimated to be sourced through referrals, with similar proportions in other contexts in South Asia and Sub-Saharan Africa (Witte, 2021; Caria et al., 2023; Beaman et al., 2018).¹³

Employee referrals in these contexts are not merely a convenience; they serve as a vital informal institution to address key challenges faced by firms. For instance, in environments with weak legal enforcement and limited formal monitoring systems, firms often struggle to ensure worker discipline. Hiring through referrals leverages the social ties between referrers and referrals to create mechanisms of informal accountability. When a recruit underperforms or engages in misconduct, the referrer may face social or reputational repercussions, providing a natural incentive for referrers to recommend reliable candidates. This dynamic can reduce the need for costly monitoring systems and efficiency wages, making referrals particularly attractive to employers in resource-constrained settings. The reliance on referrals also reflects the socio-economic structure of many developing countries, where social networks play a central role in economic interactions (Afridi et al., 2020; Heath, 2018).

The proportion of jobs secured through referrals varies across regions. For instance, in the United States, approximately 13% of workers find employment via social contacts, while in the Philippines, this figure rises to 72% (Ioannides and Loury, 2004; Schmutte, 2015). The World Bank reports that the vast majority of jobs in developing countries are obtained either through social networks of friends and relatives or by workers directly approaching employers to inquire about openings (World Bank, 2013). Referral-based hiring is even stronger among job-seekers looking for entry-level opportunities (Barwick et al., 2023; Kramarz and Skans, 2014). The gains from this type of hiring are disproportionately meaningful for those in sparse networks with weak connections to the labor market (Egger et al., 2022).

Incumbent workers also value referrals, either by virtue of the status utility they confer or in a more general sense as a non-wage amenity. Friebe et al. (2023) show that even non-referred workers are more likely to stay at a firm when more referrals are hired, owing to the fact that they derive utility from being able to have a voice in choosing their coworkers through the prospect of

¹³Referrals are also commonly used in other (non-labor market) contexts in developing countries. Bryan et al. (2015) show that peer referrals improve the enforcement of loan repayments in the microcredit context.

future referrals. In my context, as well, I find that workers have a substantial willingness to pay for being able to allocate referrals: 74% are willing to trade off between 10 - 30 additional minutes of their regular lunch break in order to be able to allocate a referral.

Moreover, referrals not only serve the interests of firms but also offer substantial benefits to some workers by reducing the costs and frictions associated with job search. For many job seekers – particularly those with limited access to formal recruitment channels – social networks become the primary conduit to learning about employment opportunities. The frequency and quality of job leads that workers receive depend critically on these networks. In this system, it is plausible that individuals with higher quality networks would hear about higher quality job openings, affording them better outside options. In contrast, workers with weaker networks would be left with fewer prospects, perpetuating a cycle where only those already advantaged gain access to the most desirable positions. Thus, the very mechanism that allows firms to lower their marginal search costs – by leveraging referrals to target workers with dense connections – may simultaneously reinforce labor market disparities.

2.3 Caste and the implications of diversity

The vector of social stratification that best explains the saturation of networks and thus job mobility in India is caste. The Indian caste system is at least 3000 years old. The delineation of society into four hierarchical categories (“varnas”) - *Brahmins*, *Kshatriyas*, *Vaishyas*, and *Shudras* - is found in the book *Manusmriti*. The *Dalits* (previously considered “untouchable” by the traditional upper caste groups) exist outside of this hierarchy altogether, and are the lowest social group. Each *varna* in turn has hundreds of sub-groups known as “*jatis*”, such that *jatis* within varnas are also categorized hierarchically. In aggregate, there are estimated to be upwards of 4000 *jatis* in India ([Deshpande and Ramachandran, 2024](#)). These *jatis* tend to be characterized by strict rules of endogamy, helping sustain the institution of caste ([Moorjani et al., 2013](#)). The caste system also enforces norms around social interactions, such as restrictions on sharing food, residential segregation and, most importantly from the perspective of this paper, strict boundaries around occupational roles ([Oh, 2023](#); [Lowe, 2021](#)).

While the *jatis* form the basis of the caste system, modern statutory government classifications – General, Other Backward Classes (OBC), and Scheduled Castes (SC) – are significant in studying caste-based discrimination in India ([Munshi, 2019](#)). These classifications align with the aforementioned hierarchy. Broadly speaking, General castes rank above OBCs, while OBCs rank above SCs. Another group of importance is indigenous Indian communities, categorized as the Scheduled Tribes (ST). While there are noteworthy differences in the historical status of these groups, they are ranked in policy and inter-group interactions approximately at the same level as SCs. In this paper, I will pool together SCs and STs into a “lower caste” category, while the remaining are classified

into the “upper caste” category.¹⁴ In my setting, we can take the set of existing lower caste workers in teams as coming from some pre-determined selected population wherein through contact theory or training both workers and supervisors are not biased towards them in particular (Finseraas et al., 2019; Lowe, 2021; Mousa, 2020).

SCs (and STs) continue to face systemic discrimination in India despite constitutional protections. The caste system’s social hierarchy marginalizes SCs in several spheres, including access to education, employment, and social interactions. 24% of households across India still practice untouchability, a custom where physical contact with SCs is avoided to maintain “purity,” despite untouchability having been outlawed over 70 years ago. This form of exclusion restricts SCs’ access to shared community resources and perpetuates social isolation (Desai and Vanneman, 2011). Moreover, these caste-based differences are intertwined with spatial segregation. SCs often reside in separate hamlets within villages, and inter-caste interactions are minimal, leading to limited access to social capital and networks (Anderson, 2011).

Economic opportunities for SCs are often limited due to occupational stigmas and discrimination in hiring. While there has been some improvement in the economic standing of SCs over recent decades, owing in part to affirmative action programs in education and public sector employment, disparities remain significant (Hnatkovska et al., 2013; Asher et al., 2024). SCs are frequently relegated to the most physically demanding and least desirable jobs, reinforcing their historical economic subjugation. Approximately 50% of economic exchanges, such as food transfers and loans, occur within the same *jati*, highlighting the role of caste in structuring social and economic life (Munshi and Rosenzweig, 2015). Caste-based networks also dominate the dissemination of both information about job opportunities and provide pathways that drive migration even in the absence of direct job opportunities, through social insurance that supports periods of unemployment (Mohanty, 2024). This caste-based homophily results in SCs being underrepresented in higher-paying sectors. As a result, SCs often remain confined to low-paying, menial jobs with limited upward mobility, perpetuating economic disparities and social marginalization. Network-based job search has previously been used to explain persistent caste-based differences in occupational sorting and wage differences in urban areas (Banerjee and Knight, 1985).

Consistent with the literature on the contact theory, there is evidence that exposure to caste based out-group changes beliefs about these out-groups. Recent work stresses that it is broad exposure to a large number of out-group members that leads priors to be updated, as opposed to deep exposure to a smaller number (Chakraborty et al., 2024). In line with this, in my setting it is reasonable to expect that incumbent upper caste workers may still hold negative beliefs about lower

¹⁴This is a coarse classification, which necessarily introduces some errors of inclusion and exclusion. A granular version of this analysis would focus more squarely at the *jati* level. However, given the saturation of caste groups represented in my sample who happen to have migrated from a limited number of sending regions, this coarse classification is not a first order concern. Moreover, an analysis at the *jati* level would generally not be powered at conventional levels.

caste workers at large, as they are exposed to relatively few SC and ST members in their own teams.

A vast literature suggests that when firms are engaged in team-based production, diverse teams tend to be less productive in the short run owing to higher costs of coordination and cooperation among workers. This is shown in the context of religious diversity in India (Ghosh, 2025), where teams made of diverse Hindu and Muslim workers are less cohesive owing primarily to misplaced beliefs about the productivity and shirking of out-groups. There is similar evidence from ethnic groups in Kenya (Hjort, 2014), where there is horizontal and vertical discrimination within teams. In other contexts, there are more direct coordination costs from diversity, such as when workers who speak different languages are asked to coordinate on tasks, with output declines in mixed teams that ameliorate over time (Dale-Olsen and Finseraas, 2020; Anisfeld and Pandiloski, 2024). Álvarez Pereira et al. (2024) show that diversity is more beneficial in larger teams.

A related literature suggests that there is a risk of substantial backlash against affirmative action policies in the context of caste in India. In particular, Jodhka and Newman (2007) show that managers at firms express a distaste towards affirmative action policies, and use that as an explanation for the lack of access for lower caste workers in the formal private sector in India. In other contexts as well, affirmative action policies have been shown to harden identities and polarization, such as in the case of Brazil (Pérez Cervera, 2023). Xu (2021) shows in an audit study that putatively Black applicants who are shown to have received scholarships labeled as diversity-focused become less likely to receive call backs from prospective employers, while putatively White applicants face no such penalties. Kline et al. (2022) use large correspondence experiments to show discrimination patterns at scale in the US labor market.

2.4 Stylized facts under the status quo

In this section, I highlight descriptive evidence on occupational segregation, the role of networks in job search, and caste dynamics at the site(s) of the experiment, drawing primarily on the detailed baseline survey conducted at the firm where the primary experiment is conducted, and supplementary analysis from other data sources.

Fact 1: LCs are underrepresented in manufacturing work generally and have poorer job-to-job mobility. I use the 2014-15 round of the India Human Development Survey to analyze the representation of LCs across industry-occupation clusters. I find that controlling for education, district, age, and gender, LCs are more likely to be found in landless rural agricultural labor. Within urban areas, they are disproportionately more likely to be found in industries with low premia (Figure B.1). In the firm where the experiment is conducted, LCs make up 21% of the overall workforce at the firm, but the proportion is only 6% at the supervisor level, and none at the level of the firm's executive.

To speak to outside options and mobility more broadly, I use the Periodic Labour Force Survey

(PLFS, 2018–19 rotating panel). There is a peculiar equilibrium in the Indian labor market among the low-educated workforce: with a high share of job-switchers as well as a high share of workers in rural areas, unemployed, or generally in non-salaried work. This bimodality is driven almost entirely by caste: lower castes are much more likely to be found in low-value employment, in rural areas or just be unemployed. On the other hand, the high aggregate job-switching rate itself is driven entirely by upper castes (see Figures B.3 - B.5).¹⁵

Fact 2: Referrals play an outsized role in matching workers with entry-level jobs. Referrals are the primary mode of hiring in the firm where the experiment is conducted, with 92% of the helpers (i.e. the workers actually engaged in production) at the time of the baseline survey having been hired through referrals. The rate is approximately 81% when team leaders and upper level executives are also considered.¹⁶

Fact 3: Referral opportunities are concentrated among upper caste workers. In the baseline survey at the firm, I ask the universe of all workers at the firm the frequency of receiving referral opportunities. The probability of receiving a referral is more than thrice as large for UCs than LCs, while there is no detectable difference by the extent of education or experience at the firm (Figure B.6).¹⁷ Controlling for education and experience narrows the caste gap in referral access somewhat, though not meaningfully. Importantly, conditional on getting the opportunity to refer someone, a plurality of workers, refer their relatives to join the firm (48%), and a smaller share refer previous coworkers (22%) and other acquaintances (30%). These patterns hold irrespective of the referrer's caste.

Fact 4: There is substantial clustering in the firm's workforce, and worker networks are highly saturated by caste. As of the baseline, the firm's 724 workers are clustered into just 137 unique referrer-referral links. The average worker has 1.64 links across the firm's workforce (average degree centrality of the network), and 1.05 of these are made up of the same caste as the nodal worker. The density of the network is 0.0021, indicating relatively low levels of aggregated connectedness

¹⁵I link individuals across survey rounds and define a job-to-job transition as a change in job identifiers between subsequent rounds, and model two predicted probabilities using logit with state fixed effects and controls for age, sex, and schooling (restricted to low-education workers): (i) $\Pr(\text{switch job})$ and (ii) $\Pr(\text{rural unemployed or non-salaried})$, where the latter proxies low-quality outside options.

¹⁶This is not atypical: Afridi et al. (2021) show that 75% of the workers in their setting (garment manufacturing plants in India) find their jobs through referrals; Sapin et al. (2020) report an estimate of 72%; and Kumar and Srivastava (2025) find rates of 71%. Importantly, these estimates are all considerably higher than those from developed country settings. Schmutte (2015) reports a rate of 13% in the US context. These estimates are primarily from worker surveys across firms. In my setting, the share of workers matched through referrals is higher because it is concentrated within a specific factory. In general, recruitment policies are likely to vary much more across firms than within, which could explain the somewhat lower rate of referral dependence in a cross-firm worker survey.

¹⁷Education by itself does not vary meaningfully among the worker population, since the firm requires all its workers to have studied till at least the eighth grade. Only 11% of the workers at baseline report studied less than this requirement. Note that I am unable to identify instances of misreporting in the status of education, but anecdotally the firm itself also does maintain a rigorous standard of evidence that the workers must furnish.

across the graph. A network graph representing these referrer-referral links is in Figure B.7, and Figure B.8 presents a benchmark for what the network would look like if each referrer was equally likely to refer someone from either caste, while keeping the total number of UCs and LCs constant. This homophily in the workforce extends to the links of potential referral candidates, wherein most incumbent workers claim to share prospective referral links with others from their own caste (Figure B.9).

Fact 5: LCs have a higher retention rate at the firm, owing primarily to worse outside options.

At baseline, there is a high rate of churn among the workers at the firm: 11% on average per month. This is in line with the high rate of worker exits in manufacturing in developing countries in particular, and very similar to benchmarks found in the Indian context in prior work (Adhvaryu et al., 2022).¹⁸ I find that incumbent LCs have longer survival and retention rates at the firm (Kaplan-Meier survival χ^2 -stat = 34, p-value < 0.01, spending 30% longer at the firm on average; Figure B.10). These patterns are also reflected in the previous jobs reported in the cross-section of the firm's respondents at baseline. Prior to joining the firm, they were 20% (p-value = 0.04) less likely to be in fixed monthly employment, and 81% (p-value = 0.049) more likely to be in daily wage labor. Their aggregate earnings in their previous jobs were also lower (by 12%, p-value = 0.092).¹⁹

Fact 6: LC and UC incumbent workers have similar extensive margin referral preferences, but the characteristics of their potential referrals vary markedly.

As part of the baseline, I also collected detailed network information on potential referral candidates, asking respondents to list the names and some basic information of an (unconstrained) set of their contacts that they would bring to the firm if they ever received a referral opportunity. Both UC and LC respondents list the same number of contacts (2.02 on average), are highly likely to want to refer someone from their own caste group (86.5% on average), and are highly likely to want to refer someone from their close or extended family (83.1% on average).²⁰ However, LCs are likely to report having had 5 more conversations with their referral candidates in the previous 30 days than UCs (relative difference of 45%, p-value = 0.097), likely reflecting stronger links. Importantly, LC workers' referral candidates are substantially less likely to be in urban work (relative difference of 13%, p-value = 0.006), and are less than half as likely to already be in Delhi (p-value < 0.001). This underlines the fact that most LCs are picking contacts who are predominantly in rural areas, while UCs are generally considering those already in urban job-seeking cycles. Despite these differences, both UCs and LCs rate the

¹⁸This churn is costly for firms. In my setting, supervisors report spending about 15% of their time in status quo on training new workers. Meta-analysis work synthesizing evidence across contexts shows that worker turnover leads to substantial declines in productivity (Hancock et al., 2013).

¹⁹Following Caldwell and Danieli (2024), I conceptualize these caste gaps as differences in the feasibility set of jobs (owing to network constraints and mobility costs across space, as opposed to local commuting costs within a closed market).

²⁰These indicators are not statistically different for UC and LC respondents.

inherent ability of their referral candidates similarly (p-value = 0.202).

Fact 7: Supervisors worry that LC entrants would diminish team cohesion, but these views are not shared by the workers themselves. In baseline surveys with supervisors, I present them profiles of three hypothetical workers in random order, such that two of them have the same education, experience and state of origin, but differ in their names' caste labels (see Figure A.9). For each of the hypothetical profiles, supervisors are asked how they expect them to perform on the team on a range of attributes: their net productivity, how hardworking they would be, whether they would be easy to train, and whether they would get along with the team. The latter is meant to be a measure of the effect of the new entrant on the team's cohesion. I find that while supervisors do not believe that LCs would be differentially hardworking or easy to train, they would be less likely to get along with their team (by 5%, p-value = 0.014). This results in a net lower expectation of LCs' productivity (by 3%, p-value = 0.029). All of these measures were also elicited in the baseline survey with incumbent workers, which I use to generate the workers' beliefs about each caste group on productivity, hardwork, and cohesion. I do not find any statistically significant differences on any dimension, indicating that workers do not share their supervisors' apprehension that LCs would lower cohesion.

I also attempt to recover these insights from supervisors on their actual workers, since there may be other unintended biases in their consideration of hypothetical workers in the stylized exercise. On subjective ratings for their current workers across a range of indicators, I find precise nulls on the caste comparison, suggesting that they view all of their workers similarly across these dimensions. This is consistent with positive selection (i.e. LCs who are actually on the team have higher ability than hypothetical LC workers who will be drawn from the broader distribution), training (i.e. they believe the LCs are now trained and better than a draw from the distribution), and/or the contact hypothesis (i.e. their priors about their specific workers have improved as a result of extended exposure) (Lowe, 2021; Mousa, 2020). In a separate exercise as part of the baseline, I share with supervisors profiles of three randomly selected workers *currently on their team*, and ask them to rate potential (hypothetical) referral candidates from each of these workers on a range of dimensions. This exercise suggests that they believe their LC workers would refer others who are similarly qualified, but would be more likely to accept the job offer (6%, p-value = 0.018) and would be less likely to get along with the rest of their team (8%, p-value = 0.027). This underlines that supervisor beliefs are consistent with the literature from personnel and labor economics, wherein diverse teams must pay (at least a short run) cost in terms of lower cohesion.

Fact 8: No baseline adverse beliefs against lower caste candidates. At baseline, there is no clear correlation between reported cohesion and worker or team characteristics (such as worker caste, experience, and team size or caste composition). However, LC workers report having more friends in teams with below median LCs (92%, p-value = 0.059), indicating some selection and

differential group dynamics in teams with fewer LCs. Additionally, while supervisors exhibit beliefs consistent with an aversion to diversity, they do not exhibit systematic beliefs indicating lower inherent ability or productivity among lower caste hires. Through the same exercise on rating prospective referral candidates described in Fact 7 above (and detailed in Figure A.9), I find that supervisors do not rate hypothetical LC referral candidates any lower than UC referral candidates (p -value = 0.236). Overall, I do *not* see evidence consistent with standard versions of statistical or taste-based discrimination at baseline. However, I also do not statistically rule it out, and in particular the dynamic changes in tastes.

3 Data and experimental design

3.1 Data and measurement

The primary experiment is conducted in a footwear manufacturing plant just outside of Delhi, in a region that has a high density of footwear, textiles, and other similar manufacturing plants. This industrial cluster is about 30 kilometers from the city’s central business district, and primarily attracts migrant workers from neighboring states. These workers often reside in informal settlements in and around the industrial cluster, and tend to have previously worked in agriculture before migrating to the city for their current roles.

The firm employs between 700 and 900 workers on average, with the precise number varying month-on-month based on seasonality in demand. Production takes place in teams, with team size typically varying between 5 and 8 workers. As of the baseline survey, there are 132 teams at the firm. Each team is led by a supervisor, who is only nominally engaged in production himself, and is responsible for recruitment, training, troubleshooting production challenges, and other miscellaneous tasks.²¹ Each team works on a specific task (e.g. assembly, cutting, sole preparation). The full list of tasks and the number of teams performing each tasks is in Table A.2.

Production outcomes come from the company’s administrative data which reports weekly counts of units at the team level. Because totals vary by task (e.g., some teams produce soles while others stitch straps), I standardize weekly output using pre-experiment task-level means and standard deviations.

Separately, I use personnel data from the firm at the monthly level to recover retention, entries, exits, and referral allocations. I use this to build team \times month panels of team size, composition, and recruitment practices. Survey outcomes, elicited at baseline and endline with both workers and supervisors, cover outgroup beliefs, job satisfaction, and self-reported cohesion. The data inventory

²¹Frontline workers who are actively engaged in the production process are called “helpers”. Helpers tend to work on informal contracts, wherein they are paid a fixed monthly wage with no explicit financial incentives, and are able to leave the firm and occasionally re-join it. Supervisors are on permanent long-term contracts with the firm.

in Table A.1 documents the frequency, unit, timing, and source for each indicator, and I follow those definitions when constructing the analysis dataset.

During baseline surveys with the firm’s incumbent workers, I elicited a network of potential referral candidates that each worker was asked to report, listing names and some basic characteristics for those they would invite to the firm if they had the opportunity to invite referrals. It is with this sample that I conduct the follow-up with referral candidates. The main outcomes are collected via an endline phone survey and include earnings, job-search activity, realized employment, and the duration of recent unemployment spells.²² Outcome construction then follows the survey modules – for example, counts of job offers heard about in the past six months, indicators for receiving a factory job offer, current paid employment, and days unemployed.

The lab-in-field experiment generates two families of outcomes: (i) real-effort production and (ii) revealed equilibrium cooperation and cohesion. The production task, spread over two sessions, asks teams of workers to cut fabric into uniform strips and tie them into a single rope. Each worker is given an identical piece of fabric, with only the color differing across workers on a team, which they are asked to cut into strips of pre-specified dimensions. Workers in high-coordination teams must follow a predetermined color sequence (requiring continuous coordination) to construct their ropes. In low-coordination teams, the rope need not follow a pre-specified order of colors, allowing workers to work in parallel and combine their strips at the end of the production session. Output is measured at the end of each session by two independent enumerators; following a quality screen that enforces pre-specified strip dimensions (10 cm × 2 cm), I take the median of the two length readings (in “number of strips”) as the team outcome, with $\geq 90\%$ agreement between enumerators.

To capture equilibrium cooperation and cohesion, workers are paired up within teams to play a symbol-matching game. Workers independently attempt to identify overlaps across two unique symbol grids, and enumerators compute the true number of matches after collecting both sheets. I construct an individual-level revealed measure of cohesion as the absolute gap between reported and true matches, averaged across rounds. Smaller gaps indicate better coordination/understanding with teammates. This design preserves independent effort while isolating coordination quality across varying pairings, providing a lab-style analog to the firm’s survey-based self-reported cohesion outcomes used in the firm experiment.

²²I reach about 61% of listed referral candidates through this phone survey. Attrition is not differential by treatment, referral status, or caste. However, my ability to reach respondents does depend on task type since it is correlated with whether or not I have valid phone numbers for them, which in turn depends on whether incumbent workers at baseline were allowed to have their phones with them at the time of the baseline survey. In the robustness steps detailed in Section C, I discuss that the analysis of this survey is robust to the use of Lee Bounds based on Lee (2009) and Semenova (2025).

3.2 Design

This firm experiment is implemented at the team (and thus supervisor) level. For the duration of the experiment, which was announced to supervisors after the baseline surveys, I took away the discretionary powers of *all* supervisors to allocate referrals. In treatment teams, each time there was a vacancy in the team either due to an exit or increased labor demand, referral opportunities were allocated to a randomly chosen incumbent lower caste worker. In control teams, referral opportunities were allocated to a randomly chosen incumbent worker irrespective of their caste.²³ This design is represented in Figure A.2.²⁴

In the baseline, I attempted to survey the universe of all workers and supervisors at the firm, successfully managing to reach 94% and 92% respectively. The worker baseline surveys included a detailed network elicitation exercise, wherein respondents were asked to list the names and some basic demographic information for contacts that they would bring to the firm if they received a referral opportunity. Thus, prior to the allocation of teams to one of the two treatment groups, I was able to construct a network of potential referrals for the near-universe of incumbent workers, along with a cross-section of basic demographic and labor market indicators for these referral candidates, none of whom were at the firm as of the baseline.

The experiment lasted six months, wherein referrals were allocated based on each team's treatment status. Table 2 reports results on the compliance with randomization, with respect to the caste status of the recipients of the referral opportunity. Compliance with listed referral allocations was high on average, across both treatment and control teams.²⁵

A timeline of each implementation and survey component of the study is listed in Figure A.1. Following the intervention at the firm, I conducted a detailed endline survey with the universe of workers and supervisors, reaching 92% and 93% respectively. The primary outcomes for the firm experiment, however, come from administrative data. This includes weekly team-level data on the units produced or processed by each team. In addition, I use personnel data at the team-by-month level to generate measures of the teams' caste composition, tracking entry and exit by incumbent

²³Note that control teams in this experiment are different from the referral allocation process in status quo, where supervisors retain the ability to choose whom to invite referrals from. In practice, since referrals are allocated randomly within the control group, I am able to simulate ex-post counterfactual groups within the set of control teams that have a high or low incidental exposure to *likely* referrers. Likelihood of being asked to refer in the counterfactual here is derived from predictions from a MLE classifier using baseline referral allocation data. I find that there are no consistent differences in the referral candidate choices of high vs low likelihood referrals (Table B.19) and in the team-level treatment effects wherein treatment teams are compared separately to high vs low likelihood referral control teams (Table B.20). These results suggest that the control group here is a reasonable counterfactual group for this experiment.

²⁴The implementation of this referral allocation, across both treatment and control teams, was done in a centralized way by the firm's HR department. At the beginning of each month, I shared an ordered list of which incumbent worker each team's supervisor should call upon in the event of a vacancy that needs to be replaced. An example of this ordered list is in Figure A.3.

²⁵Referral allocations deviated from the randomized lists shared with supervisors in only 8% of team \times month observations.

status as well as caste group, and the links between referrers and referrals.

Following the conclusion of the experiment at the firm, I implemented a lab-in-field experiment (Figure A.4). This experiment was conducted in the form of stylized workshops where I set up mini “firms” with workers drawn from a population similar to the workers at the firm, recruited from eight industrial clusters across the city. Workshops were conducted over 12 spells, with each spell including two 45-minute production sessions and surveys before and after the production sessions. Each workshop spell starts with 30 workers, 10 of whom are lower caste and the remaining are upper caste. These workers are sorted into 10 teams, each with one lower caste and two upper caste workers. Each worker is asked to bring a potential referral candidate to the workshop, but this referral candidate is not involved in the first production session. Workers perform a standard task in teams. The task was designed to vary the extent of coordination across teams.²⁶ Each worker was given fabric of a certain color, and the team was asked to cut strips of this fabric of pre-determined dimensions, and then tie these strips together to make a rope as long as they could in the 45 minutes. In high-coordination teams, workers were asked to follow a particular order of colors, while the order was immaterial in low-coordination teams. Workers’ payoffs were linked to the overall performance of the team, i.e. they were given a fixed payoff of INR 600 (USD 7) for participating in the lab-in-field experiment, and INR 5 (USD 0.06) per strip for the rope they are able to construct as a team.

Each production session was followed by a round of surveys with workers, where I elicited measures of social cohesion. Following the first production session, teams were randomized into three groups, wherein the groups differed in the mode in which a fourth worker was added to the team. In the first group, “LC referrals”, the fourth worker is the referral of the incumbent lower caste worker. In the second group, “UC referrals”, the fourth worker is the referral of one of the two upper caste workers, chosen randomly. In both the first and second groups, I communicate an identical message to the incumbent workers, i.e. a fourth worker is being recruited by inviting a referral from one of the three incumbent workers. In the third group, “LC strangers”, the fourth worker is not connected to any of the incumbent workers, but is the referral of an incumbent lower caste worker from a *different* team. This mode of recruitment is communicated to the incumbent workers as a fourth worker being added to the team in order to balance the team’s caste composition. The first two groups are designed to replicate the firm experiment, wherein “LC referrals” and “UC referrals” map on to the treatment and control teams from the firm respectively. The “LC strangers” group is designed to replicate a non-referrals based diversity intervention, similar to common affirmative action programs and other policies that make the identity of the minority group salient, found both in the literature and in firms across the world. Allocation to these treatment conditions was stratified by the high vs. low-coordination nature of the task.

At the end of the second production session, workers are asked to rate cohesion and satisfaction.

²⁶A pictorial representation of the tasks is in Figures A.6 and A.7.

In addition, workers are organized in pairs and play a cooperation game in order to arrive at a revealed-preference measure of their equilibrium level of cohesion and cooperation, modeled on [Breza et al. \(2018\)](#). Each worker gets a sheet of 40 unique Greek symbols, and must work with his paired partner to match the correct number of symbols that overlap with his partner’s sheet. This symbol matching game is played twice by workers paired with another in their own team, generating natural variation in the identity of their partners over the two rounds.

The key comparison of interest in the lab-in-field experiment is “LC referrals” vs. “LC strangers”, and there were half as many teams allocated to the “UC referrals” arm as the other two in order to maximize power for the relevant comparison across the two LC arms. In addition, there are two key features of the lab-in-field experimental design worth highlighting. First, this design entirely switches off the effect of selection on productivity and cohesion, similar to [Pallais and Sands \(2016\)](#). All referral elicitation are made prior to the allocation of treatment, and the new workers in the “LC strangers” group are also referrals of similar incumbent LCs as in the “LC referrals” group. Second, the extent of nominal diversity is held constant across both types of teams: they go from 33% LC in the first production session to 50% LC in the second. With these two features in place, the only salient difference across the groups is the mode of recruitment and how this is communicated to the incumbent workers.

4 Empirical strategy

In analyzing data from the firm experiment, I follow a simple regression specification:

$$Y_{imt} = \alpha + \beta \cdot T_m + \gamma_s + \phi_t + \varepsilon_{imt} \quad (1)$$

where Y is an outcome for an individual i from team m in time period t , T denotes a binary for the treatment status, γ denotes fixed effects for the randomization strata s , and ϕ denotes fixed effects for the time period. For outcomes that are defined at the team level, the i subscript is omitted accordingly. Standard errors are always clustered at the team level.

In the lab-in-field experiment, I employ a similar regression specification as in (1) for all survey-based outcomes:

$$Y_{im} = \alpha + \beta \cdot T_m + Y_{im}^o + \kappa_t + \varepsilon_{im} \quad (2)$$

where indices are defined as in (1), T denotes a categorical variable for the three treatment groups, κ_t refers to fixed effects for the workshop spell, and Y_{im}^o refers to the value of the outcome collected in the screening or baseline survey prior to the production sessions, where available.

For analyzing the effects on team output in the lab-in-field extension, I employ the following

specification:

$$\Delta_m = \alpha + \beta \cdot T_m + \gamma_s + \kappa_t + \varepsilon_s \quad (3)$$

where $\Delta_m = \text{Output}_{m,2} - \text{Output}_{m,1}$ is the difference between the output at the team level between the first and second production sessions, T denotes a categorical variable with three values, one for each treatment condition, γ denotes fixed effects for the randomization strata, and κ_t refers to fixed effects for the workshop spell. Standard errors are always clustered at the team level. Note that I present the results on both the team-level output as well as the output per worker, where the latter is simply the former divided by three and four for the first and second production session respectively.

In the lab-in-field experiment, I also collected data on a continuous revealed measure of equilibrium cohesion and cooperation between workers on each team through a symbol-matching game. The effect on cooperation is analyzed with the following specification:

$$Y_{ipm} = \alpha + \beta \cdot T_m + \kappa_t + \varepsilon_{im} \quad (4)$$

where Y is the outcome for an individual i belonging to pair p in team m , T denotes a categorical variable for the three treatment groups, and κ_t refers to fixed effects for the work spell.

Lastly, I employ a similar specification for the endline follow-up survey with the referral candidates elicited at the baseline survey at the firm:

$$Y_{jmt} = \alpha + \beta \cdot T_m + \gamma_s + \phi_t + \varepsilon_{jmt} \quad (5)$$

where j refers to a candidate listed by an incumbent worker at endline as a potential referral candidate, T_m refers to the treatment status of team m , which is the team that the referrer of j at the firm is in, and the remaining variables are defined as in (1).

Across all the specifications listed above, the standard errors are always clustered at the team level, which is the unit of treatment assignment.

5 Theoretical framework

The firm's referral allocation problem is dynamic: supervisors decide each period how to allocate referral opportunities among incumbent workers, trading off possible short-run cohesion costs from changing team composition against longer-run gains from retention and stability. This section provides a unified overview that (i) introduces a common output–profit framework used throughout the theoretical framework, (ii) situates a one-period static benchmark, (iii) summarizes the dynamic Bayesian learning channel that the experiment activates, and (iv) links these pieces to estimate the firm's *perceived* profits over time, in order to help rationalize the initial pre-experimental status quo.

Full details and estimation are in Appendix G.

5.1 A common output–profit framework

Let a team in period t have state

$$S_t \equiv (n_{\ell,t}, n_{u,t}, \bar{q}_{\ell,t}, \bar{q}_{u,t}, \nu_{\kappa,t}, \sigma_{\kappa,t}^2),$$

where $n_{g,t}$ counts workers of group $g \in \{\ell, u\}$ (lower caste and upper caste), $\bar{q}_{g,t}$ is the average network quality (a proxy for the workers' outside option), and $(\nu_{\kappa,t}, \sigma_{\kappa,t}^2)$ are belief parameters about cohesion costs (mean and standard deviation). Let $L_t = n_{\ell,t} + n_{u,t}$, the LC share $s_t \equiv n_{\ell,t}/L_t$, and the referral share to LCs be $\rho_t \in [0, 1]$.

Production follows a Cobb–Douglas technology augmented by a cohesion term (Bartelsman et al., 2013; Bloom et al., 2016; Hamilton et al., 2003):

$$Y_t = A \cdot C(s_t, \rho_t) \cdot L_t^\alpha K_t^\beta, \quad C(s_t, \rho_t) \equiv 1 - \kappa_t |\rho_t - s_t|^\eta, \quad \eta > 0, \quad (6)$$

where κ_t is the (possibly time-varying) cohesion sensitivity and $C(\cdot) \in (0, 1]$ scales team efficiency as referral allocations deviate from the team's composition. Turnover creates training and reorganization costs; let Q_t denote quits and $\mu > 0$ convert quits into monetary costs. I write the reduced-form per-period profit as

$$\pi_t = Y_t - wL_t - \mu Q_t. \quad (7)$$

This decomposition nests the static benchmark and the dynamic model below; Appendix G gives the full specification used for estimation and the mapping to moments.

5.2 A one-period static benchmark

As a simplified reference point, consider a static one-period version of the firm's problem. This static benchmark serves primarily as a simplified reference to highlight the immediate trade-offs between cohesion and retention, absent the complicating dynamics of belief updating. A firm of fixed size L allocates a share $\rho \in [0, 1]$ of referral opportunities to LC incumbents; the remainder $1 - \rho$ goes to UC incumbents. Current diversity, exogenously determined, is $s \in [0, 1]$, equivalent to the pre-referral LC share of the team.

Production. Output obeys

$$Y = A C(s, \rho) L^\alpha, \quad C(s, \rho) = 1 - \kappa |\rho - s|^\eta, \quad \kappa > 0, \quad \eta > 1,$$

so cohesion (and hence productivity) is maximised when referrals mirror the team's exogenously

determined caste composition.

Training cost. Let the inverse turnover (retention) rates be γ_l and γ_u for LCs and UCs, and define the retention advantage $\delta \equiv \gamma_l - \gamma_u > 0$. Expected quits therefore cost the firm:

$$T(\rho) = \tau \left(\rho \frac{1}{\gamma_l} + (1 - \rho) \frac{1}{\gamma_u} \right),$$

with $\tau > 0$ converting exits into monetary terms.

Firm objective. Taking the uniform wage w as given, profit is

$$\pi(\rho; s, \delta) = AL^\alpha C(s, \rho) - T(\rho) - wL,$$

and the first-order condition balances the marginal cohesion loss against the marginal training gain.

Closed-form optimum. Solving the first order condition yields an interior solution whenever $\delta \neq 0$:

$$\rho^* = s + \frac{\delta}{|\delta|} \cdot \left[\frac{|\tau\delta|}{AL^\alpha \kappa \eta \gamma_u (\gamma_u + \delta)} \right]^{\frac{1}{\eta-1}}.$$

When $\delta = 0$ the optimum collapses to the boundary $\rho^* = s$.

There are several notable insights from this static model. First, the optimal referral share moves in step with the team's exogenously determined LC share. It is optimal for the firm to allocate more referrals to LCs in teams that have more LCs to begin with. Second, the share of referrals to LCs grows with a higher retention advantage to LCs (i.e. larger δ). As LCs' effect on the team's expected turnover increases owing to a greater difference between their and UCs' average retention rates, the team finds it more profitable to allocate more referrals to LCs. Third, the previous two relationships are mediated by the effect of LC hires on the team's cohesion. When there is a positive retention advantage from hiring LCs (i.e. $\delta > 0$), it becomes optimal for the firm to hire fewer LCs when the team's cohesion is more sensitive to diversity (i.e. larger κ). Fourth, the elasticity of the deviation from exogenous diversity is non-linear. The optimal share of LC referrals increases for small values of η , and then falls when cohesion is sharply misaligned.²⁷

This static benchmark indicates that even large retention gains through training-cost savings (δ) seldom justify allocating *all* referrals to LCs; the profit-maximising firm strikes a balance where the marginal cohesion cost equals the marginal training benefit. These insights and comparative statics are represented in Figures F.1 - F.4 of Appendix F.

²⁷The proofs for these comparative statics, and a longer discussion of the implications, are in Appendix F.

5.3 Dynamic Bayesian learning

The static model necessarily abstracts away from several inherently dynamic elements in this theoretical framework: the stochastic nature of the job arrival process that affects worker retention, the evolution of the stock of network quality that also affects worker retention, and the evolution of supervisor beliefs from observing the relationship between referral allocations, team cohesion, and output.

In the full dynamic Bayesian learning model, supervisors begin with Gaussian priors over the cohesion sensitivity,

$$\kappa \sim \mathcal{N}(\nu_{\kappa,0}, \sigma_{\kappa,0}^2),$$

and I carry these as the belief state $(\nu_{\kappa,t}, \sigma_{\kappa,t}^2)$ for each team.

Each period, I observe the following moments: (i) team output Y_t , (ii) the number of LC referrals realized N_t , (iii) team size and composition, allowing us to form the LC share s_t , (iv) the referral share ρ_t . Baseline output is taken from the pre-experiment period at the team level. I construct a per-referral signal for teams only in periods with at least one LC referral, using team-specific baselines and the observed number of LC referrals, and compute the noise variance from a first-stage regression of output on LC referrals. The measurement of the per-referral signal is:

$$\zeta_t \equiv \frac{Y_t - \bar{Y}_0}{N_t},$$

which is informative about κ because, holding other determinants of production fixed, a higher cohesion sensitivity reduces output more when referrals pull the team away from its inherent caste composition.

Under the production mapping in (6), this implies a Normal signal with precision increasing in N_t :

$$\zeta_t \mid \kappa \sim \mathcal{N}(\kappa, \sigma_\varepsilon^2/N_t).$$

To avoid reacting to one-off hires, I gate updates by cumulative exposure: beliefs are updated only after cumulative LC referrals reach a stock threshold $k^* \in (0, 1)$, i.e. $\sum_{\tau \leq t} N_\tau/L_\tau \geq k^*$.²⁸

Beliefs update via Normal-Normal conjugacy:

²⁸The model estimation below sets $k^* = 0.3$, but the insights are qualitatively simliar for a wide range of values across the initial distribution of the baseline LC share in the team.

$$\sigma_{\kappa,t+1}^2 = \left(\sigma_{\kappa,t}^{-2} + \frac{N_t}{\sigma_\varepsilon^2} \right)^{-1},$$

$$\nu_{\kappa,t+1} = \sigma_{\kappa,t+1}^2 \left(\sigma_{\kappa,t}^{-2} \nu_{\kappa,t} + \frac{N_t}{\sigma_\varepsilon^2} \zeta_t \right),$$

and otherwise beliefs remain anchored to the current value.

During the intervention, the treatment sets $\rho_t \approx 1$, mechanically raising N_t and making the signal more precise earlier. Control teams are much more unlikely to update their beliefs meaningfully due to weaker signals and the inability to cross the stock threshold. Hence posterior variances shrink faster and posterior means move more in treatment than control.

After the policy is lifted, supervisors again choose ρ_t discretionarily. I use the static choice rule from Section F, trading off cohesion and retention, to invert the observed discretionary post-experiment ρ_t into an implied belief κ_t for each team and month. This provides a level anchor for the belief path. I then reconcile the within-experiment Bayesian updates with this post-period anchor, ensuring a coherent trajectory over the full horizon.

In summary, in order to estimate the belief trajectories I initialize priors using baseline data, construct a per-referral signal and its precision, gate updates by cumulative exposure, apply Normal–Normal updates, and finally anchor the level of beliefs using revealed post-experiment referral choices. The resulting paths show pessimistic priors drifting toward lower perceived cohesion costs in treatment, with much slower movement in control. Mandatory LC referrals in treatment ensure that threshold is crossed quickly; control teams rarely accumulate enough exposure to update beliefs materially. Figure 3 documents the resulting divergence in inferred cohesion costs across arms.

When would beliefs converge without the experiment? The updating rule implies that, once the extensive margin exposure threshold is crossed, posterior precision accumulates linearly with LC-referral exposure. I define *exposure capital* after the threshold as $E_T \equiv \sum_{t>T_{\text{threshold}}} N_t$ and fix a tolerance $\varepsilon > 0$ for belief precision. A team's posterior s.d. after T periods is

$$\sigma_{\kappa,T}^2 = \left(\sigma_{\kappa,0}^{-2} + \frac{E_T}{\sigma_\varepsilon^2} \right)^{-1},$$

so achieving $\sigma_{\kappa,T} \leq \varepsilon$ is equivalent to accumulating

$$E_T \geq E^*(\varepsilon) \equiv \sigma_\varepsilon^2 \left(\varepsilon^{-2} - \sigma_{\kappa,0}^{-2} \right)_+.$$

If the status quo generates a stationary exposure rate $r \equiv \mathbb{E}[N_t]$, the expected time-to- ε is

$$\mathbb{E}[T(\varepsilon)] \approx T_{\text{threshold}} + \left\lceil E^*(\varepsilon)/r \right\rceil.$$

Two implications follow. First, if $r = 0$ (no LC referrals), beliefs never converge. Second, even with $r > 0$, small baseline exposure implies long horizons before precision is adequate—quantifying why mandated exposure can catalyze learning rather than merely *mechanically* increase LC hires. In the estimation I compute $E^*(\varepsilon)$ for each team using (i) the team-specific prior variance $\sigma_{\kappa,0}^2$, (ii) the noise scale σ_ε^2 from the first-stage output–referral regression, and (iii) a tolerance ε (reported in the calibration). I then use pre-experiment exposure rates to form $\widehat{T}(\varepsilon)$ and summarize the distribution across teams. I find that in the status quo, approximately 27% of the teams would never converge to the “correct” beliefs, which are defined as the 5th percentile of the eventual posterior beliefs in the treatment group. Among those that do converge, $\mathbb{E}[T(\varepsilon)]$ is 11.9 years.²⁹

5.4 Perceived profits and costly experimentation

To connect learning and worker flows to bottom-line firm performance, I simulate *perceived* weekly profits using the same panel objects as the reduced-form analysis. The simulator takes monthly entry flows from the data, constructs tenure-specific exit hazards for LCs and UCs, allows for a short delay before entrants become productive, and embeds a parsimonious belief path v_t that drifts from a pessimistic prior toward the true cohesion sensitivity once sufficient evidence accumulates.

Consistent with Sections 5.1 and F, production follows a Cobb–Douglas technology in effective labor, scaled by a belief-implied cohesion wedge. Let L_t denote the number of *productive* workers in the team in week t , where new hires may contribute only partially during an initial ramp-up period (Appendix G). Weekly output is

$$y_t = A L_t^\alpha \phi(v_t) + \varepsilon_t, \quad \varepsilon_t \sim \mathcal{N}(0, \sigma_\varepsilon^2). \quad (8)$$

Here $\phi(v_t) \in (0, 1]$ is the *belief-implied cohesion factor* obtained from the cohesion term in Section F:

$$\phi(v_t) \equiv \mathbb{E}[C(S_t, \rho_t; \kappa) \mid \kappa \sim \mathcal{N}(v_t, \sigma_{\kappa,t}^2)] \approx \frac{1}{1 + \phi_{\text{belief}} v_t}, \quad (9)$$

where $C(S_t, \rho_t; \kappa) \in (0, 1]$ is the cohesion term that penalizes mismatch between the team’s LC share s_t and the referral share ρ_t (see Section 5.1), and the final expression is the parsimonious approximation used in estimation.

Perceived profits are then

$$\pi_t = y_t - \mu \text{Churn}_t - f - \mathbf{1}\{t \geq 1\} c_0 e^{-t/\tau_{\text{impl}}} - \lambda |\rho_t - s_t|, \quad (10)$$

where:

- Churn_t is the number of separations plus the training/benching equivalent for new hires (scaled

²⁹This estimate uses a tolerance of $\varepsilon = 0.05$, which is about 15% of the baseline mean of the beliefs.

by cost per unit μ);

- f is a fixed operating cost;
- $c_0 e^{-t/\tau_{\text{impl}}}$ captures a fading implementation friction after the policy starts; and
- $\lambda|\rho_t - s_t|$ penalizes short-run reorganization when referral shares depart from the team’s current composition.

The parameter vector

$$\theta = (A, \alpha, \mu, f, \kappa_{\text{true}}, \kappa_{\text{prior}}, k^*, c_0, \tau_{\text{impl}}, \sigma_\varepsilon)$$

is estimated by SMM against three moment sets: (i) event-time paths of standardized weekly output, (ii) monthly entries and exits (churn), and (iii) monthly referral allocations and the resulting LC team shares.

Figure 4 reports the simulated path for perceived firm profits under the treatment policy, along with 95% confidence intervals arrived at through Monte Carlo simulations of the estimation process. The treatment path exhibits a short initial dip due to pessimistic beliefs, such that supervisors interpret an initial loss of about 9.3% relative to the status quo (95% confidence interval [5.2%, 19.9%]). The *perceived* profits trajectory then recovers as beliefs improve and LC cohorts mature into productive employment. The mean crossing time (*perceived* profits > baseline) is about 6.2 weeks, with a 5th–95th percentile interval of 4.1–14.2 weeks. The duration of the crossing time is non-trivial, suggesting that adoption requires meaningful learning over a consistent period of time with uninterrupted signals rather than a purely mechanical adjustment, and validating why the status quo persists in the absence of mandated experimentation.

Figures 3 and 4 summarize the mechanisms that rationalize both the pre-experiment status quo and the persistence we observe after the policy is lifted. Treatment acts as *directed exploration*: by mandating LC referrals, it generates the informative draws that move supervisors’ posteriors about cohesion costs, pushing beliefs down toward the true (lower) cost in Figure 3. Combined with the retention advantage of LC hires and the resulting stabilization of teams, this learning translates into *perceived* profits that dip briefly and then turn positive within the first two months in Figure 4.

Seen through the lens of the literature on multi-armed bandit problems, the supervisor each period chooses between “arms” that differ in both expected payoff and uncertainty—allocating referrals to UCs (an arm with familiar, myopically safer returns under pessimistic priors) versus to LCs (an arm whose payoff is initially uncertain because cohesion costs are unknown and expected to be high). In the status quo equilibrium, a pessimistic prior, implementation frictions, and churn costs tilt the firm against the LC arm, leading to under-exploration. The intervention forces the LC arm to be pulled often enough to clear a stock threshold for learning, shrink posterior variance, and lower the posterior mean cohesion cost. Once these conditions are met, the perceived value

of the LC arm rises above that of the UC arm, yielding both the profit path in Figure 4 and the post-experiment persistence in LC allocations.

Three implications follow from this logic. First, there is a larger post-experiment LC referral share where there was more LC exposure (i.e. stronger learning in treatment teams and in control teams more exposed to LC referrals incidentally). Second, there is a short transitional dip in *perceived* profits driven by pessimistic beliefs about cohesion costs, followed by recovery as beliefs improve and LC cohorts mature. Third, learning gains are larger where supervisors had more pessimistic beliefs, or in tasks with high coordination intensity where stability is most valuable. This is validated in the numerical estimation of belief trajectories, which suggests a positive elasticity of 0.23 in learning relative to initial pessimism. The formal state-space, belief updates, and profit mapping used to estimate these dynamics are in Appendix G.

6 Results

6.1 Team composition

The intervention significantly altered the composition of the recipients of referral opportunities, and the extent of workforce diversity in treatment teams. The probability of a referral opportunity going to an LC worker increased significantly from 24% to 92% in treatment teams. The latter indicates that over the course of the intervention, 92% of the referral opportunities in treatment teams were allocated to incumbent LC workers.³⁰ The number of LC hires through referrals in treatment teams increased by a corresponding 83%, rising from an average of 0.29 per month in control teams to 0.53 per month in treatment teams (Table 2). As a result, by the end of the intervention, the share of LC workers in treatment teams was higher by 15 percentage points relative to control teams, increasing from 25% to 40%.³¹ This is a non-trivial result: referral allocations move as a result of the intervention to incumbent LCs on the team, and they are successful in bringing their referral candidates to the firm.

Importantly, these changes in the composition are not associated with any significant differences in team size.³² These effects indicate that the intervention was largely implemented successfully, and substantially changed the workforce composition in treatment teams.

³⁰The remaining 8% corresponds to cases of imperfect compliance, which could be driven by administrative errors at the firm's HR department, an incumbent LC's refusal to accept a referral opportunity, and/or a supervisor overruling the prescribed referral allocation rule. I am unable to distinguish between these three due to constraints in the firm's personnel data.

³¹By the end of the intervention, 28 treatment teams were composed of majority LC workers, relative to 3 in control teams

³²There is a negative but statistically insignificant effect on the size of the team. This point estimate could be consistent with higher migration or joining costs for LC referral candidates, or the fact that treatment team supervisors do not replace all their vacancies owing to improvements in the team's productivity that are discussed below.

6.2 Output

How does the increased diversity affect team productivity and output? I find that the intervention produced significant gains in team output. Treatment teams exhibited a statistically significant 0.09σ increase in output over the duration of the experiment, translating to a 4.6 percentage point or about 5% improvement in performance relative to control teams. These gains are driven by teams working on high-coordination tasks. These teams showed a stronger effect of 0.10σ compared to 0.03σ in low-coordination tasks, the latter not statistically significant at conventional levels (Table 3). Importantly, the gains in output continue beyond the intervention. Pooled over the time period including the intervention and five months of the post-intervention period, the treatment effect on output rises to a statistically significant 0.11σ , again concentrated among the high coordination teams.

To identify the drivers for this effect on the output, I consider potentially competing forces that are significant inputs in this setting. The first is churn and team stability: if a team's workforce is more stable, and the firm is able to save on the training and transition costs triggered by repeat exits, we should expect a positive effect on output, *ceteris paribus*.

However, there may be costs induced by LC entry as well. These include higher transition and matching costs from LC entry, since they may take longer to migrate or adjust to the city, or the fact that LCs may have lower initial ability, or some disutility from tastes against diversity or LCs that get triggered by their entry. None of these explain the dynamics I observe and are not key considerations in this setting.³³ However, there is one notable cost that is a key consideration both in the literature and among the firm's supervisors: expected decline in worker cohesion and cooperation as a result of diversity. Recent literature suggests that mixed teams are associated with (at least) short run productivity costs owing to higher cohesion costs (Ghosh, 2025; Hjort, 2014), and as reported above in Section 2.4, the supervisors in this firm express this as a concern as well.

On net, it is not obvious *ex ante* given the competing effects of churn and cohesion which direction the treatment effects on productivity, if any, would materialize in. The fact that I see large positive effects on output could be driven by either of the two inputs. In the next subsection, I discuss results for each of these. In order to rationalize the mapping of the effects of either on output, I will focus on two additional considerations: how treatment effects vary dynamically, particularly in the short run after the implementation of the reform, and whether the effects are differential in teams

³³Several alternative cost stories appear to not matter significantly in this setting. First, I do not observe a short-run productivity dip when diversity rises via LC referrals, which suggests that LCs either do not have lower ability or their lower ability does not adversely affect output. Moreover, both workers and supervisors do not believe that LC entrants would have lower ability. Second, treatment teams continue to allocate referrals to LCs after the intervention ends, indicating belief updating rather than fixed tastes against LCs or preferences against diversity. Related experimental evidence shows that entrepreneurs in Ghana systematically underestimate workers' trustworthiness and update when they receive signals (Caria and Falco, 2024). Finally, churn falls in treatment teams, which is difficult to square with large, persistent transition costs. Taken together, these patterns make alternative explanations based on sizeable ongoing cohesion or matching costs unlikely to be first-order drivers.

that work on tasks that require a high or low degree of coordination among the workers.

6.3 Churn and cohesion

I first focus on the effects on churn. Consonant with the changes in the composition of treatment teams over the course of the intervention, I find a large treatment effect in improved retention in treatment teams, driven primarily by new hires. Treatment teams experienced a statistically significant 41% lower monthly churn, where churn is defined as the sum of exits and entries. Control teams lose 1.4 workers per month over the course of the experiment, and 0.8 of these are among new hires, i.e. workers hired any time after the baseline. Aggregate exit rates were 35% lower in treatment teams, a large and statistically significant difference. As noted above, this difference is most stark among new hires, where the treatment effect is 57% (Table 4).³⁴ This result is economically meaningful in this context. [Adhvaryu et al. \(2023\)](#), in the context of an Indian manufacturing firm, show that only about 50% of the firm's workers are retained through the period of a year. [Adhvaryu et al. \(2022\)](#) show, also in the Indian context, that quit rates are around 10% in their context within a quarter. The declines in churn I see in my setting thus represent large savings to the firm. In addition, the treatment effects I find are in line with benchmarks in the personnel economics literature. [Hoffman and Tadelis \(2021\)](#), in the context of a high-tech US firm, find that increasing a manager's people management skills from the 10th to 90th percentile reduces churn by about 60%.

To get a direct test of the effect of increased retention on output, I use the timing of the first exit faced by the team in the intervention period, which can reasonably be assumed to be quasi-random. In Figure B.16, which re-centers the dynamic treatment effect relative to the earliest post-experiment exits in event time, I show that improvements in team-level output intensified with more stability in teams. These results replicate broader findings from the literature on referrals across contexts, where referred workers have been shown to drive more productivity and profits for their firms than non-referred workers, driven primarily by lower turnover among referred workers ([Burks et al., 2015](#)). In this sense, LC referred workers seem to drive treatment effects on the intensive margin of referral allocation similar to the extensive margin effects of referrals as a recruitment tool themselves.

Importantly, in this setting, the value of retention shows up in output more strongly in teams where workers must coordinate. As control teams in high-coordination tasks keep losing workers as in the status quo, the dynamic treatment effect intensifies. This insight is consistent with a model of production where each time a worker exits a team with an established coordinated routine, even the workers who remain at the firm must learn to re-establish these routines with the new entrants. Turnover is disruptive in these settings, and gains from stability continue to accrue over time relative

³⁴As a consequence of this improved retention, workers in treatment teams had longer tenures on average, with a statistically significant increase of 1.4 months (24%) compared to the control mean. Naturally, this effect on tenure was pronounced among LC workers, who were newer in treatment teams than in control teams (Table B.9).

to control teams who keep losing workers at rates similar to the pre-experimental average.

Why are LC new hires more likely to be retained by the firm than UC new hires? I find that this is explained by the relatively large and systematic difference in outside options across the caste groups. In the baseline network elicitation, wherein I ask incumbent workers to list potential referral candidates, these differences in outside options are already apparent. I show descriptive comparisons between upper and lower caste incumbent workers' referral candidates on four key dimensions in Figure B.11. Through incumbent workers' responses about their prospective referral candidates, I show that lower caste incumbent workers believe that their referral candidates are 14% more likely to accept a hypothetical job offer, about half as likely to already be in Delhi, about 25% less likely to be employed in urban work anywhere, and about 65% more likely to be unemployed, relative to upper caste incumbent workers' referral candidates.

Next, I turn to the effects on team cohesion. At the endline survey among the firm's workers, I collected a range of self-reported measures of social cohesion, summarized in Table B.15. In columns 1-2, I report treatment effects on a self-reported rating of team cohesion, i.e. where respondents rate how cohesive and cooperative their team feels to them on a scale of 0 - 10. Self-reported team cohesion not lower in treatment teams – if anything, there is a small and weak positive treatment effect on the raw estimate, with improvements largely concentrated among LCs and slight negative but insignificant reports among UCs.³⁵ Next, I report treatment effects on the number of friends they report having at work. This is an unconditional response to questions where I ask them to report the number of coworkers they would spend time with outside work, and the number of coworkers they would spend time with at work (during lunch breaks, etc.). I do not find aggregate treatment effects on either dimension. However, LCs in treatment teams are statistically significantly more likely to report having friends at the firm that they spend time with outside (40%) and at the firm (26%). Relatedly, I find no treatment effects on job satisfaction, workers' expectation of leaving the firm, or job search activity (Table B.16).³⁶

In addition, I elicit beliefs about out-groups with a random subset of workers at endline. These questions are administered by sharing profiles of hypothetical workers. These profiles are presented in Figure A.9, where each respondent hears about three hypothetical workers, two of whom have identical education and experience but vary in their names and thus caste status.³⁷ Workers are then asked to rate how productive these hypothetical workers might be, and how well they expect the hypothetical workers to get along with the team. I find no significant treatment effects on either dimension (Table B.14). I take this as suggesting that there are no large biased or discriminatory

³⁵LCs have a systematically higher reported level of cohesion, and the weakly significant point estimate is largely driven by the composition effects of more LCs in treatment teams.

³⁶Note also that an indicator suggesting large declines in cohesion among UCs would be their exit from the firm. I find no evidence that there is a large degree of backlash among UCs in treatment teams that triggers differential exits among them or incumbents more broadly.

³⁷In this context, names are obvious signifiers of caste.

beliefs against out-groups.

Moreover, note that declines in cohesion, if present, would map on to output in a way that they would be visible in the short-run dynamic treatment effects on the output. This pattern would also be true for the other types of costs induced by diversity that I listed above: lower ability among LCs, higher matching costs for LC entrants, or any tastes against their entry. Figure 1 shows that there is no decline in output in the short run, which rejects at least that these costs cumulatively bind in my setting.³⁸

Overall, the firm experiment shows that directing referrals towards incumbent LC workers at the firm draws more LC hires in, improves retention in treatment teams, and manifests in higher output in these teams. Contrary to recent literature on diversity and the costs of cohesion in mixed teams, I find no noticeable negative effects on social cohesion in these teams. One potential concern with these results is that the measures of social cohesion from the surveys with workers are all self reported and may suffer from measurement error and social desirability bias. Owing to logistical constraints, I was unable to administer more detailed behavioral modules measuring cohesion. I address this gap in the lab-in-field experiment, results from which are discussed below.

6.4 Persistence and learning

Given the gains from this referral policy: increased output, increased retention, and no evidence of large declines in cohesion, a natural question one might ask is why the firm was not implementing this in status quo. One answer to this lies in the baseline surveys with supervisors, wherein I shared profiles of hypothetical workers of both castes, similar to the process outlined above and detailed in Figure A.9, and asked a range of questions eliciting supervisors' prior beliefs about these workers on a range of dimensions, including their perceived ability, how easy they would be to train, how long they are expected to last in the firm, and how well they might get along with the rest of the team.

I find that supervisors do not report that LC entrants would have lower ability, but they worry that LC entrants would not get along with what are predominantly UC teams. On average, supervisors report that LC entrants would be 8% less likely to get along with the team, which would adversely affect output (Figure B.12). Notably, these beliefs do not extend to their incumbent workers. I elicit

³⁸It is possible that there is an "optimal" level of diversity, beyond which cohesion declines are triggered and output is adversely affected. I test for this in two ways. First, I estimate quantile regressions on the dynamic output treatment effects, reported in Figure B.17. I do not find systematic evidence of heterogeneity in the treatment effects, suggesting a lack of underlying variation on this metric. Second, I pair treatment teams with control teams within randomization strata in a bootstrapped procedure that estimates pair-wise treatment effects over $N = 1000$ iterations. I report the distribution of the pooled treatment effects on output recovered from this exercise in Figure B.18. This suggests that negative treatment effects are low likelihood and greatly outweighed by the instances of large positive effects. Both these tests suggest that there is an absence of significant backlash or adverse effects on cohesion that trigger output declines in this setting.

from supervisors their beliefs about a randomly selected set of their incumbent workers, varied by caste, on their ability and how well they get along with the rest of the team. I find precise nulls in terms of caste differences: across all dimensions, supervisors believe their incumbent LC and UC workers are similar (Figure B.13).

In the theoretical framework outlined in Section 5, I use these initial beliefs of supervisors to model the pre-experimental status quo and why this equilibrium would persist in the absence of learning induced by experimentation. As the experiment drives mandated exposure to LC referrals, supervisors learn that cohesion declines are actually not triggered through this reform. I outline this learning process and the trajectory of supervisor beliefs in the model. The fundamental moment I use for this exercise is the post-experimental discretionary referral allocations, i.e. how treatment and control supervisors allocate referrals after the experiment ends and they are able to choose whom to invite referrals from.

I find that supervisors continued to allocate referrals to lower caste workers in treatment teams, even after the conclusion of the intervention. In Figure 2, I report the evolution of the share of referrals given to LC incumbents and the team's caste composition during and beyond the intervention. Referrals are almost always targeted at LCs in treatment teams during the intervention period as listed above, while they approximately track the share of LCs in control teams. This is because in the latter, the referral allocations are done at random without consideration of caste.³⁹ As the share of referrals to LCs in treatment teams remains close to 1, the share of LCs in the team increases steadily. After the intervention ends, and the supervisors regain the discretion to allocate referrals to incumbent workers, treatment team supervisors continue to allocate referral opportunities to LC incumbents. This is not entirely a mechanical effect driven by the increased share of LCs in these teams. The allocations in treatment teams are at higher rates than even the post-experimental the share of LCs. This is indicative evidence that supervisors learn about the true cohesion costs in this setting, and the benefits of retention from increased LC representation.⁴⁰ Meanwhile, in control teams, LC referrals drop to rates below the share of LC workers in these teams.⁴¹ In Table B.12, I

³⁹Note that the control group represents a recruitment regime that has more LC referrals than what we might expect in the status quo, thus suggesting that the results are an underestimate of what we might expect relative to the status quo. As outlined in Section 3, I can reject that the policy in the control group differs systematically from what we might expect in the steady state if referrals could be allocated discretionarily by control supervisors.

⁴⁰There is growing evidence that firms (or teams within firms, in my setting) must experience a technology before they adopt it. Garg et al. (2025a) show that Bangladeshi garment factories only show a willingness to buy air purifiers after trial periods that let managers see productivity gains first hand; information, credit, and warranty treatments alone had no effect. An experiential barrier also appears in Atkin et al. (2017), where soccer-ball manufacturers in Pakistan rarely adopt a cost-saving cutting technology even when it is handed to them for free. In Bloom et al. (2012), Indian textile plants took up new practices only after consultants worked on-site, again pointing to hands-on learning as the driver of adoption. Kremer et al. (2013) argue that behavioral departures from expected-utility—especially small-stakes risk aversion consistent with loss aversion—help explain why Kenyan retail firms underinvest even in high-return opportunities, suggesting that experiential learning may be necessary to overcome perceived downside risk.

⁴¹I cannot rule out the absence of learning in control teams as a whole. Note that the incidence of LC referrals in control teams is also random, since the likelihood of an LC incumbent worker receiving a referral opportunity is equal

report point estimates indicating persistence in the pooled post-experimental period, indicating that in treatment teams, the share of referrals targeted to LCs is 49 percentage points (or 125%) larger than in control, and the probability that they allocate a greater share of referrals to LCs than their share of the team is 22 percentage points (or 63%) larger than in control.

Importantly, the persistence of higher LC referral shares after discretion is restored is hard to reconcile with time-invariant tastes or any other set of beliefs that is discriminatory about LCs or diversity policies. It is instead consistent with supervisors updating initially pessimistic beliefs about the cohesion declines triggered by diversity hiring once exposed to LC referrals. This learning is consistent with large updating in beliefs driven by changes in what is a core problem in this setting, i.e. the effect of churn on output, which remains intact owing to the absence of cohesion costs. Even small effects on churn could have outsized impacts on downstream firm output. Employers need only a modest degree of monopsony power to align hiring and training decisions with the strength of workers' outside options ([Manning, 2003](#)). Dynamic search models with counter-offers show that, once a firm can infer the arrival rate of competing offers, it invests only in employees whose quit risk is low ([Postel-Vinay and Robin, 2006](#)). When a statutory minimum wage pins pay to a floor – as in my setting – wage markdowns are off the table, yet some rents can be captured through ex ante screening: firms may seek to admit applicants whose observable traits signal weak outside options and, hence, longer expected tenure. This mechanism echoes the logic in [Acemoglu and Pischke \(1999\)](#), who argue that wage compression pushes employers to recoup training costs not through lower pay but through lower turnover.

Outside options, reservation wages, and quit propensities are usually hidden from employers at the point of hire. In my context, however, caste provides a visible proxy: it is strongly correlated with outside opportunities and immediately observable during recruitment ([Rose, 2023](#)). The experiment shows that supervisors could learn to exploit this signal, selecting lower-caste referrals to secure a retention advantage. This learning is evidenced in the continued allocations to LCs by treatment supervisors after the conclusion of the experiment, shown in figure 2 and discussed in the previous section.

6.5 The perceived cost of outsiders

The absence of cohesion declines at the firm and the eventual learning leads to an important open question: why, contrary to both the literature and supervisors' initial beliefs, do cohesion declines not get triggered in the firm experiment? I answer this question through two stylized experiments.

to the LC share of the population in expectation. While the aggregate LC share of referrals in control teams is lower in the post-intervention period, this masks considerable heterogeneity with respect to the incidence of exposure to LC referrals during the intervention. I show that there are qualitatively more LC referrals in the post-experiment period among control teams that randomly gave more referrals to LCs in the experimental period, though this difference is not always powered owing to a small sample size (Figure B.19).

The first of these is a vignettes experiment, where I elicit respondents' beliefs about the expected consequences of a range of different recruitment policies. The second is a lab-in-field workshop, where I employ workers to perform a stylized task, and test the true effects of a subset of these recruitment policies on both output and revealed measures of cohesion.

How workers perceive the fairness and suitability of recruitment policies is an important determinant in their productivity. In contexts with different social group identities that lead to heterogenous teams, and where workers must coordinate to work on production, their beliefs about cohesion and by extension, productivity, are a useful indicator of the relative costs and benefits of different recruitment policies. In order to aid in developing a descriptive sense of this comparison, I conducted a vignettes experiment with 152 manufacturing workers, drawn from a population similar to the one that participated in the lab-in-field extension. These surveys involved sharing with respondents the profile of a hypothetical team engaged in a coordinated production task, modeled on the task from the lab-in-field extension. The respondents were told about the initial caste composition (one LC and two UC workers⁴²), output at the individual and team level, and the initial self-reported cohesion. The information reported to the respondent is presented in Figure A.8. The respondent was then told that a fourth worker would be added to the firm, and they would be asked to estimate the output and cohesion in the subsequent production round. Each respondent was asked four of six potential recruitment policies in random order. The six recruitment policies presented to respondents are listed in the table below, with the text in italics listing the actual content that was communicated to the respondents.

Recruitment mode	Caste label visible	Communication to respondent
LC referral	No	<i>The new worker is hired through a referral given to [name of LC incumbent]</i>
LC stranger	No	<i>The new worker is unknown to existing workers, and his name is [name of new LC worker]</i>
UC referral	No	<i>The new worker is hired through a referral given to [name of a UC incumbent]</i>
UC stranger	No	<i>The new worker is unknown to existing workers, and his name is [name of new UC worker]</i>
LC referral	Yes	<i>The new worker is hired through a referral given to the lower caste worker: [name of LC incumbent]</i>
LC stranger	Yes	<i>The new worker, who is unknown to existing workers, is hired to balance the team by caste, and his name is [name of new LC worker]</i>

⁴²Caste identity was made salient through the workers' names, but not made explicit independently. This follows the same logic as the exercise to elicit priors about out-groups in the surveys with workers and supervisors at the firm.

The set of recruitment policies in consideration is designed to cover the combination of three aspects: the identity of the entrant (UC vs. LC), the mode of recruitment (stranger vs. referral), and whether the caste identity of the entrant is made salient in the recruitment. Note that I do not include a version where the latter varies by caste, since making caste salient for UC entrants would not be organic and would not have any parallels in commonly implemented recruitment policies. Another noteworthy aspect of this exercise is that while the entrants are identified by name in the case of outsiders, in the referral policies only the names of the *referrers* is made salient. The implicit understanding is that respondents are able to infer the identity of the entrant from the identity of the referrer, since homophily in networks and in referral allocations is the norm in this setting.

I summarize the results from this exercise in table 8, where I report both the means of the change in expected output as a result of each type of recruitment policy, as well as pairwise comparisons between policies with fixed effects for the respondent. In column (1), I report the average change in the expected output per worker between the first session of production (which was in the information given to the respondent in Figure A.8), and the second session of production (for which they were asked to estimate the output). The mean for both LC stranger policies (with and without the caste label) is negative, while the other policies are positive. Respondents indicate that output will be weakly lower if the entrant is LC and is hired as an outsider, while referral-based hiring has positive impacts on output across both LC and UC hires. These comparisons are presented more robustly through pairwise tests, wherein the comparisons include fixed effects for the individual respondents.

There are three noteworthy insights from the vignettes survey. First, respondents expect LC outsiders to have a worse impact on output relative to UC outsiders (0.8 units, p -value = 0.08), when neither policy makes caste salient. In fact, LC stranger hiring is expected to lower output relative to the comparisons with all the other recruitment policies. Second, respondents expect referrals to blunt the impact of hiring more LCs. In particular, LC referrals are expected to lead to a substantially higher output per worker than LC outsiders both when caste is not made salient for either policy (1.03 units, p -value = 0.02), and when caste is made salient for both (0.75 units, p -value < 0.01). Since this specification accounts for fixed effects at the individual level, this suggests that respondents perceive referral-based hiring to substantially improve output relative to the hiring of LC outsiders, where the salient difference between the policies is simply the mode of recruitment. Third, respondents perceive caste labels to have a negative impact on output, but not statistically significantly so.⁴³ An analogous comparison of the expected change in cohesion after recruitment is reported in Table B.25.

The vignettes experiment, however, is unincentivized and based on how workers expect recruitment policies to affect cohesion and output based on the mode of recruitment and the identity of the entrant. The lab-in-field experiment goes further. I test a subset of the policies listed in the

⁴³In a robustness test, I run the same specification but keeping only the first policy reviewed by each respondent (Tables B.26 and B.27). The results are directionally and qualitatively consistent, though I lose considerable power due to the vastly smaller sample size and the inability to use individual respondent fixed effects.

vignettes using the design detailed in Section 3, and allow two production sessions where workers produce output separated by a hiring event.

The treatment effects on output are summarized in Table 6. First, I show that LC stranger teams gain no statistically significant value in output from adding a fourth worker in the second session (column 3), and the corresponding output per worker (i.e. total units produced by the team divided by 3 in the first session and divided by 4 in the second session) decreases but not statistically significantly. The corresponding effects are large and positive for both referral teams at the team level, while the output per worker weakly improves though not statistically significantly. Importantly, the difference-in-difference estimate comparing the treatment groups, summarized in panel B, reports statistically significant differences in output as a result of the recruitment policy. The difference-in-difference estimate in terms of total units indicates that output is 23% higher in teams where LCs are hired through referrals, relative to teams where they are hired as outsiders, holding selection fixed. The analogous effect in terms of output per worker is 21%, both statistically significant at conventional levels.

Next, I present results from the revealed behavioral measures of cooperation between workers. At the end of the second session of work, each of the four workers was paired up with a randomly chosen other worker for a symbol-matching game, where each worker was given two sheets with a random set of symbols. Their task is to find the number of symbols that are in common across both sheets. An example illustrating this task is in Figure A.5. Each worker plays this game over two rounds, with two randomly selected workers from their own team, drawn independently each time. The outcome for each game is a share (between 0 and 1), indicating how close the players were in absolute terms to the correct number of matches between their two sheets. In Table B.1, I present treatment effects on this outcome. In column 1, I report that LC referral teams show a 5 percentage point (9%) higher cooperation than LC stranger teams (Table 5). This effect is particularly pronounced among teams working on a high coordination task, among whom cooperation is higher. Importantly, this net difference in cooperation is evident across the team members. In columns 1 - 3 in Table B.1, I report effects on the games involving pairs of only incumbent workers, i.e. those who were present in the first production session. In columns 4 - 6, I report effects on the sample of games wherein one of the two players was a new worker hired for the second production session. I find similar effects on both types of teams, indicating that the mode of recruitment has a level effect on cooperation across all types of workers, not only those who are involved in the recruitment directly, either as recruits themselves or those who refer them. I show treatment effects on teams disaggregated by caste composition in Table B.2. In columns 1 - 3, I report treatment effects on pairs of workers who both belong to the same caste group. Here, treatment effects are generally muted, except for teams working on tasks requiring a high degree of coordination. However, I report that there are large positive effects in cross-caste pairs reported in columns 4 - 6. Pooled across all teams, cooperation is higher by 7.8 percentage points in LC

referral teams relative to LC stranger teams when looking specifically at cross-caste pairs, with stronger effects of 8.8 percentage points among teams working on tasks that require a high degree of coordination.

As noted above, this design shuts down alternative explanations like selection, when comparing LC referral and LC strangers teams. These treatment effects then suggest that the mode of recruitment has a systematic effect on morale and cohesion across the team, and particularly in terms of the quality of cross-caste links that are forged in the process of working on the production task. Similar insights are evident in these workers' survey responses.

Together, the lab-in-field extension illuminates the key mechanism driving the results of the firm experiment. The absence of a detectable dip in output (and implicitly, cohesion) as a result of the increased diversity in treatment teams at the firm can be explained by the way in which the diversity is introduced, i.e. using the existing links between UC and LC incumbent workers to sand the edges of the diversity-element of this recruitment policy.

This finding also reflects a core insight from the intersection of a rich literature from organizational economics and social psychology, focusing on the role of intermediaries in lowering the integration cost of entrants by enabling the transfer of trust and norms (Obstfeld, 2005; Coleman, 1988; Gaertner and Dovidio, 2000; Bauer et al., 2007; Burt, 2005). This literature describes a *tertius iungens* ("the third who joins") orientation: a agent who actively forges ties between a newcomer and the team, brokering introductions, vouching for quality, and transmitting work norms. In my setting, referrers play this role. Referral-based hiring functions much like "buddy" or mentoring programs used worldwide, where a sponsor helps convert an outsider into a familiar insider, lowering integration costs and the likelihood of conflict. In this setting, such brokered ties help explain why diversity introduced via referrals does not depress cohesion, and we see the benefits of diversity without the apparent costs.

6.6 Worker-level effects

A key feature of the design of the firm experiment is the fact that I elicit an unconstrained list of referral candidates as part of the workers' baseline surveys. This allows me to focus on a cohort of workers who were not at the firm at baseline in the treatment group and in the control group, as in Figure A.2). There is a range of noteworthy questions for this population. First, does the treatment affect the aggregate job search activity for the connections of LC incumbent workers in treatment teams differently from the connections of LC incumbent workers in the random seeding teams? Second, does this displace the job arrival rate for the connections of UC incumbent workers in treatment teams relative to those in the random seeding teams? In order to answer these questions, I conducted a telephonic labor force participation and job search activity survey with the full set of referral candidates approximately 10 months after the baseline, and approximately 4 months after the culmination of the firm experiment.

There are two forms of attrition-related issues I faced in this survey. First, I have phone numbers for only 31% of the referral candidates. As part of the baseline elicitation, I collected a range of indicators for each of the referral candidates including their education, current employment status, and location. These indicators are available for the universe of referral candidates, and are analyzed and discussed in Fact 6 and Figure B.11. However, only a subset of incumbent workers are able to provide phone numbers for their candidates.⁴⁴ Second, conditional on having phone numbers, 61% of the referral candidates responded to the survey. This is consistent with typical response rates in phone surveys in similar contexts (Muralidharan et al., 2021; Brownstone and Srivastava, 2025). I present a comparison of the sample of incumbent workers who do and do not share phone numbers of referral candidates, and a comparison of referral candidates by a combination of the indicators reflecting whether they had a listed phone number and whether they responded to the survey in Table D.2. I do not find systematic differences between these populations.

Regardless of the suggestion that there is no systematic non-response bias, I follow Lee (2009) and Semenova (2025) in constructing bounds around these survey estimates (Table B.24). Exact Lee bounds are relatively uninformative in my setting, but generally reject the null hypothesis. However, given the clustering in the likelihood of being able to collect phone numbers for and tracking referral candidates, generalized Lee bounds are tight and strongly consistent with my reduced form point estimates. I implement either approach by trimming bounds within randomization-stratum \times caste cells and using cross-fitted propensity scores.

In Table B.21, I present results on referral candidates' job arrival rates. There is a reallocation in the treatment group on the number of job offers referral candidates had heard about in the preceding six months, decreasing among treated upper caste candidates (0.22 jobs, 16%) while increasing for treated lower caste candidates (0.27, 52%). While these effects are individually noisy, the difference in the estimates for each caste is statistically different from zero (p -value = 0.04). I find directionally similar effects on the probability of getting a job offer from a factory in the last six months, which decreases for upper castes (9%) while increasing for lower castes (26%), with the difference between caste groups significantly different from zero (p -value = 0.08).

In these surveys, I also elicited basic information on realized employment and recent spells of unemployment (Table 7). There is no significant treatment effect on the probability of paid employment among upper caste referral candidates, while the probability for lower caste referral candidates increases significantly (50%, $p < 0.1$). Likewise, there is no detectable effect on the duration of the most recent spell of unemployment for upper caste referral candidates, while there is a 29% decline for lower caste referral candidates ($p < 0.1$).

⁴⁴The primary determinant for whether I have a phone number for the referral candidate is whether the incumbent worker had their phone with them during the baseline survey, and could look up the candidates' phone numbers on their phone's contact book. This, in turn, is highly correlated with the workers' task. For some tasks, supervisors allow their workers to bring their phones to the line, which makes it more likely that they will have their phones during the baseline survey.

To aggregate the various worker-level outcomes, I construct a simple index of “labor market prospects,” borrowing from canonical models of the value of employment (McCall, 1970; Diamond, 1982; Mortensen and Pissarides, 1994), and more recent work on the conception of a sufficient statistic or index to capture an indicator of labor market welfare adapted to my setting (Caldwell and Danieli, 2024):

$$\text{Prospects} = (w_{\text{salaried}} - w_{\text{casual}}) \cdot p^{\text{salaried}} + \gamma \cdot q^{\text{offers}} - c \cdot d^{\text{unemployed}}.$$

Here p^{salaried} is the probability of being in paid work, q^{offers} is the number of job offers heard about in the last six months, and $d^{\text{unemployed}}$ is the duration (days) of the most recent unemployment spell. I calibrate the weights using endline information from control referral candidates and express the index in units of average monthly earnings: the salaried–casual earnings premium is $\approx 30\%$, the option value per job offer is $\gamma \approx 4\%$, and the daily loss from unemployment is $c \approx 1/30 \approx 3\%$.⁴⁵ Under this calibration, the treatment effect on prospects satisfies

$$\Delta \text{Prospects}_g = 0.30 \cdot \Delta p_g^{\text{salaried}} + 0.04 \cdot \Delta q_g^{\text{offers}} - 0.03 \cdot \Delta d_g^{\text{unemployed}},$$

so the observed increases for LC candidates in paid employment and job offers, together with shorter unemployment spells, map into sizable gains in prospects, while the small, statistically weak movements for UC candidates sum to effects close to zero. These conclusions are unchanged when I recompute the index using endpoints of the generalized Lee bound intervals for each component outcome (Table B.24): the LC gains remain positive and economically large, and UC effects remain negligible.

Finally, I assess efficiency in a Kaldor–Hicks sense by aggregating the group-specific changes in prospects under alternative planner weights w_g . With representative (population-share) weights, the weighted average $\sum_g w_g \Delta \text{Prospects}_g$ is positive; it remains positive under more egalitarian weights and grows under prioritarian weights that place greater emphasis on LC candidates (see Figure B.20). Combined with the fact that there is a non-negative effect on firm surplus (profits rise with outputs and training costs decrease), the intervention increases total surplus even if some UC candidates experience small, imprecise declines—i.e., an efficiency improvement in the Kaldor–Hicks sense.

Overall, these long-run follow-up estimates suggest substantial improvements in the job access and welfare outcomes for lower caste referral candidates, indicating an increased ability to sort into paid work. These gains come without any detectable costs to the displaced upper caste referral candidates. This is consistent with upper caste candidates having a better stock of network quality

⁴⁵ A shorter unemployment spell is not necessarily welfare-improving if optimal search would involve waiting longer (Alfonsi et al., 2025). In this setting, however, I find no evidence of returns to longer search: among control candidates, those with longer versus shorter spells end up in similar types of jobs at endline.

and being in the urban labor market already, validated in baseline indicators for these candidates elicited through their connections at the firm.

6.7 Additional robustness checks

The main findings are robust to a range of additional checks. First, I implement randomization inference procedures to account for the finite number of clusters and verify that the estimated treatment effects remain statistically significant under the sharp null of no effect. Second, I examine the potential influence of social desirability bias in self-reported cohesion measures using an abridged Marlowe-Crowne social desirability scale as detailed in [Dhar et al. \(2022\)](#). Social desirability does not seem to be driving responses in the endline surveys at the firm, and there is no difference across treatment groups on this specification. Lastly, across specifications and other tests such as the exclusion of outliers, the main results remain qualitatively unchanged. Detailed robustness tables and figures are reported in [Appendix C](#).

7 Conclusion

This paper examines the role of referral-based hiring in improving labor market access for under-represented groups while maintaining firm productivity. Using the case of caste-based labor market stratification in India, the findings provide evidence that structured interventions in referral allocation can meaningfully expand opportunities for historically marginalized workers without imposing significant costs on firms.

The study reveals that when referral opportunities are intentionally directed toward lower-caste (LC) workers, firms see substantial gains in workforce diversity and retention. This is particularly relevant in settings where hiring is overwhelmingly driven by social networks, reinforcing occupational segregation. By reallocating referral opportunities, firms not only increase representation but also benefit from reduced turnover, which in turn fosters more stable production teams. The improved retention of LC workers appears to be a key driver of productivity gains, suggesting that referrals serve not only as a mechanism for hiring but also as a means of sustaining a committed workforce.

A central concern in the literature on diversity and firm performance is the potential trade-off between inclusion and team cohesion. Prior research suggests that diverse teams may suffer initial disruptions in productivity due to coordination challenges. However, this study finds no evidence of short-term productivity declines in treatment teams. Instead, the intervention led to higher team output, particularly in tasks requiring a high degree of coordination. My findings challenge conventional wisdom by suggesting that diversity-related frictions can be mitigated when new hires are embedded within existing networks through referrals. The additional insights from a lab-in-field

extension reinforce this point: teams that integrated LC workers via referrals displayed significantly greater cooperation and social cohesion than teams where diversity was introduced through direct hiring without incumbent worker ties. These insights align with a heuristic-based understanding found among both workers and supervisors in this setting, reflecting an intuitive understanding of the kinds of costs that are found in the literature. When asked hypothetically about policies mixing workers by any demographic identity like religion, region, or caste, respondents expect cohesion and output declines, but that these costs disappear if links between incumbent workers and entrants are used in the recruitment process.

The study also sheds light on why firms and supervisors may not have previously implemented similar referral allocation strategies. The reluctance to shift away from status quo hiring practices may stem less from economic concerns about productivity and more from behavioral and institutional constraints. Supervisors tasked with allocating referrals may be guided by risk aversion and short-term performance targets, making them less willing to experiment with alternative hiring strategies even when such changes could be beneficial in the long run. In this sense, the study's findings suggest a misalignment between firm-level objectives—reducing turnover and increasing workforce stability—and individual decision-making at the managerial level. Addressing these constraints through incentive realignment or informational interventions could encourage firms to adopt referral policies that enhance diversity while improving workforce retention.

Importantly, there is nothing inherently special about caste as the operative characteristic in this context, except its role in mediating the quality of job search networks. What drives the results is not caste identity itself but the structure of outside options that caste happens to index. The same logic would apply to any group whose members face weaker external opportunities. As those options evolve – say, if the stock of LC network quality improves over time – the optimal referral policy could shift endogenously, even reversing to favor other groups. The principle this paper evaluates, therefore, is not whether referral quotas should be targeted by demographics, but whether they should be tuned with workers' effective outside options.

As a deliberate design choice, the treatment anchors the policy at an extreme – directing 100% of referral opportunities to LC incumbent workers in treatment teams. This maximal shift serves as an identification device for the learning mechanism and, importantly, I find no evidence of backlash or negative effects on output, retention, or cohesion. In steady state, however, the profit-maximizing policy is likely interior: once beliefs update and marginal gains diminish due to improvements in the stock of LC network quality, the optimal LC referral share would likely lie above the pre-experimental status quo but below 100%. The experiment therefore brackets the optimum and shows that the firm can move safely and profitably toward substantially greater LC representation.

Beyond the firm, standard general-equilibrium worries appear limited in this setting. Empirically, I find negligible and statistically insignificant adverse effects for untreated UC referral

candidates. Moreover, the production environment features uniform monthly wages with no explicit performance pay, so wage pass-through is mechanically muted; the relevant adjustment margin is hiring and selection rather than wages. These features suggest that reallocating referrals mainly changes who is hired and retained – not equilibrium wages – reducing the scope for negative spillovers onto non-treated workers or teams. If many teams (or firms) adopt similar referral rules, the micro efficiency gains documented here could scale up. Opening access for disadvantaged workers relaxes misallocation on the extensive margin – moving workers into higher-productivity salaried jobs – which theory and evidence link to aggregate growth (e.g., barriers that limit minorities’ and women’s access to high-return occupations account for sizable losses). Consistent with positive spillovers, evidence from migrant labor markets shows that when migrants’ employment expands, local firms and native workers can also gain, pointing to complementarities in labor demand rather than one-for-one displacement (Egger et al., 2022; Hsieh et al., 2019).

Taken together, these findings make a Kaldor-Hicks case for increased referrals for underrepresented minority groups: the sum of firm-level surplus and improved labor-market prospects for LC candidates outweighs any small, statistically indistinguishable losses among UCs.

The policy relevance is immediate in the Indian context. Public-sector jobs are particularly coveted given tenure and reservation policies, but this has been shown to distort the broader labor market, in addition to segmenting opportunity and leaving private labor markets to reproduce network-based exclusion. Recent syntheses also document the ongoing debate over expanding reservations beyond the public sector. The referral reform studied here offers a market-compatible alternative: it meaningfully raises representation and retention for LC workers without sacrificing productivity, providing a pathway to greater diversity in private firms that is efficiency-enhancing rather than distortionary.

Outside the Indian context as well, the results of this study offer important implications for labor market interventions aimed at increasing inclusion more generally. Many diversity-focused hiring policies rely on external mandates or quotas, which can sometimes generate resistance or unintended productivity losses. This paper provides evidence that referral-based hiring reforms offer an alternative pathway – one that leverages existing hiring mechanisms while fostering a more diverse and stable workforce. Firms operating in labor markets with strong social network effects may benefit from restructuring referral allocation in ways that broaden hiring pipelines without disrupting existing production dynamics.

Overall, these findings contribute to the broader literature on social networks in labor markets, workplace diversity, and firm efficiency. They highlight that referral-based hiring is not only a mechanism for filling vacancies but also a key determinant of workforce composition, retention, and long-term firm performance. In settings where turnover is high and recruitment frictions persist, structured referral interventions can serve as a low-cost strategy for firms to achieve both equity and efficiency goals.

Tables and figures

Table 1: Balance

Variable	(1) Control		(2) Treatment		T-test Difference (1)-(2)
	N	Mean/SE	N	Mean/SE	
Output	66	0.016 (0.125)	66	-0.016 (0.115)	0.033
Average team tenure	66	1.594 (0.116)	66	1.472 (0.076)	0.122
Share of workers with high school degrees	66	0.420 (0.023)	66	0.399 (0.023)	0.021
Share of workers with above median experience	66	0.494 (0.024)	66	0.499 (0.026)	-0.005
Self-reported team cohesion (out of 10)	66	8.811 (0.071)	66	8.757 (0.074)	0.053
Share of workers that matched through a referral	66	0.882 (0.019)	66	0.888 (0.019)	-0.006
Average number of connections at the firm before starting	66	1.809 (0.238)	66	1.361 (0.070)	0.448*
Share of workers that have referred anyone	66	-0.013 (0.296)	66	0.306 (0.028)	-0.319
F-test of joint significance (p-value)					0.598
F-test, number of observations					132

This table reports balance on a range of pre-intervention characteristics of the teams that are eventually randomized into treatment and control. Columns 1 and 2 report the number of observations and the mean and standard error at the team level for control and treatment teams respectively. Column 3 reports the magnitude of the difference between the two means, with statistical significance reported after controlling for randomization strata fixed effects, and clustering the standard errors at the team level. The p-value from the F-test of joint significance is reported in the bottom panel. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Compliance with referral allocations

	# LC referral hires	# LC referral hires / # referral hires
Treatment	0.240*** (0.058)	0.677*** (0.037)
Control mean	0.288	0.241
R ²	0.12	0.51
Observations	924	467
# teams	132	126
# months	7	6

This table reports treatment effects from the firm experiment on the number of lower caste referral hires at the team \times month level, and the number of lower caste referral hires as a proportion of all referral hires. The regressions use randomization strata fixed effects, and the standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Treatment effects on output (firm experiment)

	All teams		High coord teams		Low coord teams	
	RCT period	All	RCT period	All	RCT period	All
Treatment	0.088*** (0.032)	0.108** (0.042)	0.090** (0.036)	0.170*** (0.058)	0.030 (0.028)	-0.036 (0.030)
Control mean	-0.012	-0.021	0.084	0.043	-0.151	-0.114
High vs low p-val					0.084	0.045
R ²	0.82	0.75	0.88	0.79	0.77	0.73
Observations	3432	5940	2080	3600	1352	2340
# teams	132	132	80	80	52	52
# weeks	26	45	26	45	26	45

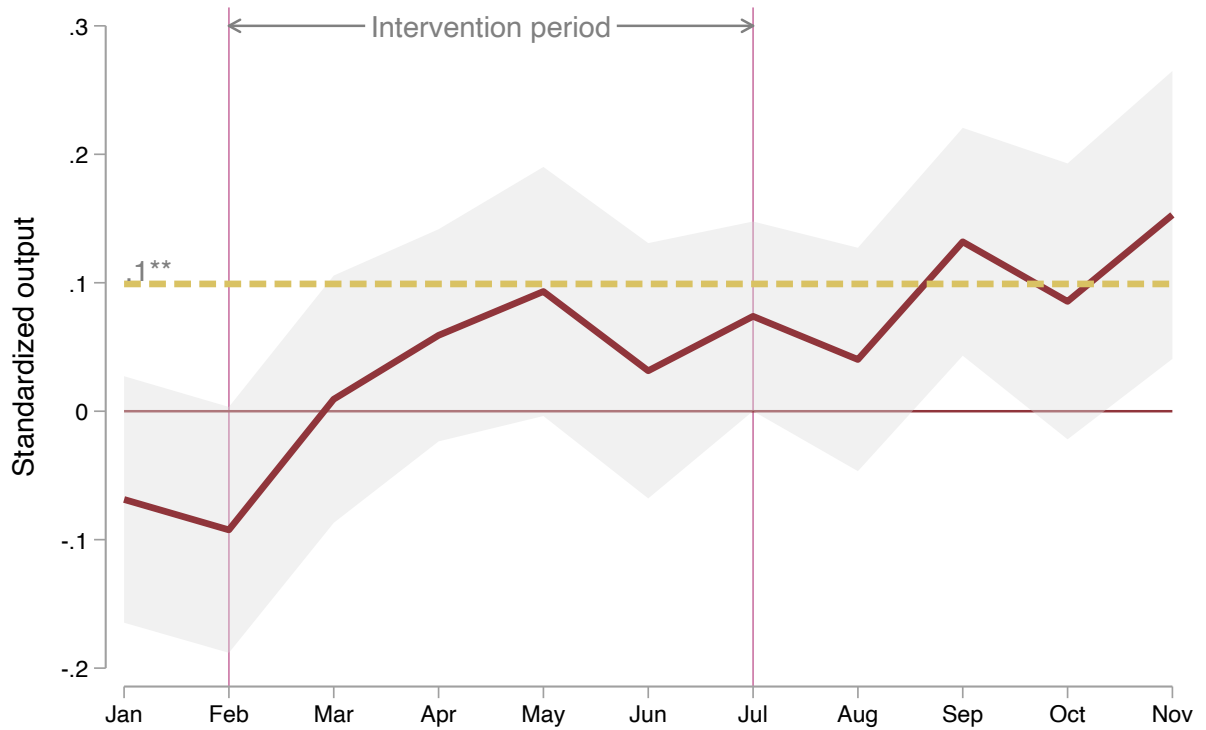
This table uses administrative production data at the team \times week level. The output is standardized at the team-type level, using pre-experiment data. Columns 1 and 2 report treatment effects on the standardized output for all teams over the entire period that the data is available for (February - November, 2024) and only the period when the intervention was active, respectively. Columns 3 and 4 report treatment effects only for teams working on high coordination tasks. Columns 5 and 6 report treatment effects only for teams working on low coordination tasks. Columns 5 and 6 also report in the panel below the main point estimates p-values from testing heterogeneous treatment effects by the extent of coordination. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata and week level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Treatment effects on churn (firm experiment)

	Exits			Entries		Churn
	Total	Incumbent	New hire	Total	LC	
Treatment	-0.522*** (0.074)	-0.074 (0.057)	-0.448*** (0.064)	-0.626*** (0.082)	0.333*** (0.047)	-1.147*** (0.139)
Control mean	1.472	0.684	0.788	1.281	0.190	2.753
R ²	0.26	0.49	0.21	0.24	0.14	0.16
Observations	924	924	924	924	924	924
# teams	132	132	132	132	132	132
# months	7	7	7	7	7	7

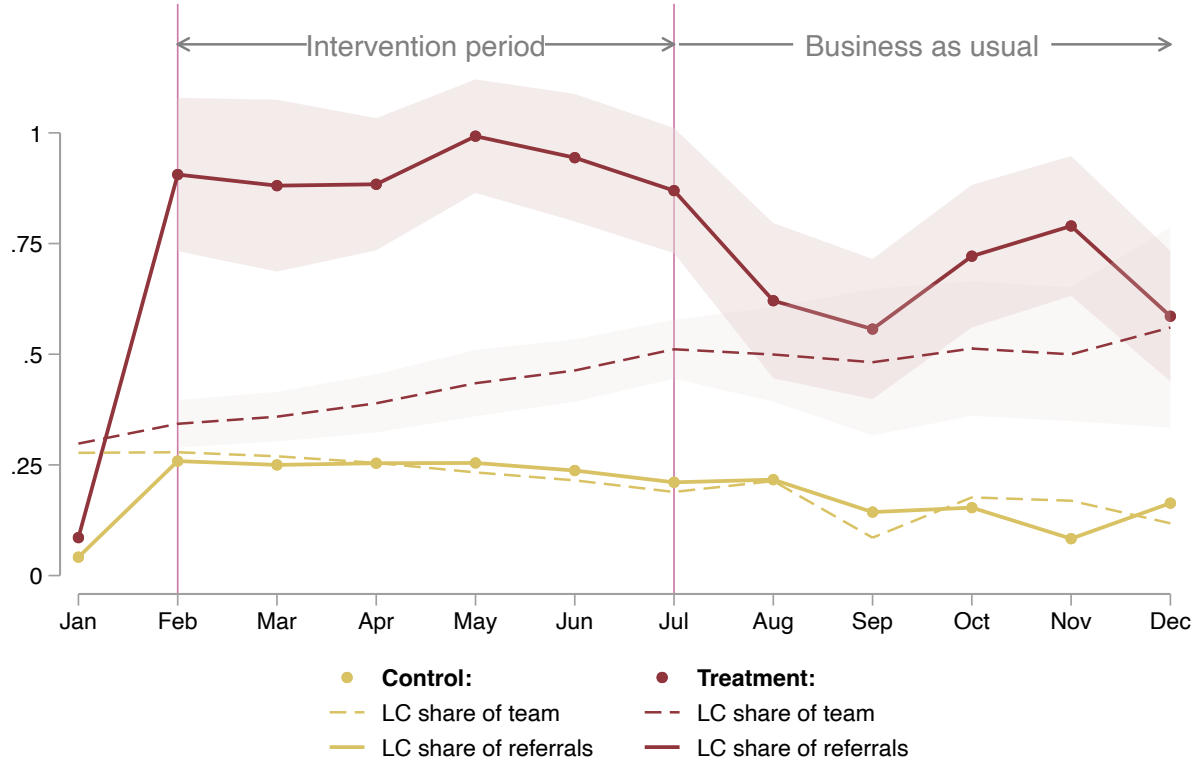
This table uses personnel data at the team \times month level to report treatment effects on worker retention outcomes from the experiment at the firm. In columns 1-4, I report the treatment effect on the number of total exits, incumbent exits, exits among new hires, and exits among lower castes respectively. In columns 5-6, I report treatment effects on the number of total entries and lower caste entries respectively. Column 7 reports treatment effects on overall churn, which is the sum of total exits and total entries. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Dynamic treatment effects on output (firm experiment)



This figure plots the evolution of the treatment effect on a standardized measure of team output at the monthly level, with 95% confidence intervals from standard errors clustered at the team level and fixed effects at the randomization strata and week level, the latter being the unit at which the data is reported. The dashed horizontal line indicates the pooled treatment effect over the entire period, from February - November 2024.

Figure 2: Dynamic referral allocations (firm experiment)



This figure plots the evolution of the team composition and referral allocation towards lower caste members over the intervention and post-intervention period. In the pre-intervention period (January 2024), the share of lower castes was approximately 26% and the share of referrals they received was approximately 8%. In the intervention period, treatment teams allocate nearly all referrals to their lower caste members (solid maroon line), which results in a gradual increase in the share of the team that is lower caste (dashed maroon line). In this period, control teams' share of referrals allocated to lower caste workers (solid yellow line) approximately tracks the share of workers that are lower caste (dashed yellow line), since referrals are allocated randomly. After the intervention period, the share of referrals that are allocated to lower caste workers in treatment teams remains above their share of lower caste workers, while reverts to the pre-experiment rate for control teams. The shaded areas in the treatment (maroon) lines reflect 95% confidence intervals from regressions on team \times month level data relative to the control (yellow) analog.

Table 5: Treatment effect on team cohesion (lab-in-field)

	Score on symbol matching game		
	(1) All	(2) High coord	(3) Low coord
LC referral teams	0.055*** (0.021)	0.086*** (0.029)	0.025 (0.028)
UC referral teams	0.034 (0.021)	0.037 (0.029)	0.031 (0.031)
LC strangers mean	0.601	0.589	0.612
LC vs UC referrals p-value	0.366	0.149	0.863
R ²	0.06	0.12	0.05
Observations	960	480	480

This table reports treatment effects on pairwise tests from a symbol matching game to measure the extent of team cohesion. The outcome is defined as the one minus the absolute share of true matches between the two players' sheets that are not found. Thus, the outcome ranges between 0 and 1, wherein higher values indicate more cohesion. In column 1, I report the pooled effect on all teams. Columns 2 and 3 report the point estimates for high and low coordination teams respectively. All regressions use randomization strata fixed effects, and standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Treatment effect on output (lab-in-field)

All teams (N = 120)						High coordination teams (N = 75)						Low coordination teams (N = 45)								
Total units			Units per worker			Total units			Units per worker			Total units			Units per worker					
S1 (1)	S2 (2)	Δ (3)	S1 (4)	S2 (5)	Δ (6)	S1 (7)	S2 (8)	Δ (9)	S1 (10)	S2 (11)	Δ (12)	S1 (13)	S2 (14)	Δ (15)	S1 (16)	S2 (17)	Δ (18)			
<i>Panel A: Output by treatment group</i>																				
β_1 : LC stranger teams		54.49	63.38	9.26	18.16	15.84	-2.23	48.39	52.10	4.03	16.13	13.03	-3.01	64.50	82.17	17.67	21.50	20.54	-0.96	(3.12)
			(6.38)			(1.89)			(8.04)			(2.41)			(10.48)					
β_2 : LC referral teams		54.06	78.81	24.75***	18.02	19.70	1.68	48.73	74.97	26.23***	16.24	18.74	2.50	62.94	85.22	22.28***	20.98	21.31	0.32	(2.01)
			(4.07)			(1.29)			(5.42)			(1.68)			(6.15)					
β_3 : UC referral teams		49.88	75.00	25.13***	16.62	18.75	2.13	33.33	60.67	13.80	11.11	15.17	-0.84	47.11	91.11	44.00***	15.70	22.78	7.07	(3.81)
			(7.52)			(2.28)			(8.09)			(2.65)			(13.10)					
<i>Panel B: Difference-in-difference estimates</i>																				
$\Delta_1 - \Delta_2$		15.49**			3.91*			22.20**			5.51*			4.61			1.28			
		(7.54)			(2.28)			(9.64)			(2.65)			(13.10)			(3.81)			
$\Delta_1 - \Delta_3$		15.87			4.35			9.77			2.17			26.33			8.03			
		(10.43)			(3.11)			(12.63)			(3.86)			(17.41)			(5.07)			
$\Delta_2 - \Delta_3$		0.37			0.44			-12.43			-3.34			21.72			6.75			
		(7.82)			(2.43)			(9.56)			(3.03)			(16.61)			(4.97)			

This table reports effects on output by treatment group from the lab-in-field experiment. In panel A, I report means for teams of all three recruitment types. In columns 1 and 2, I report the average units of production for each treatment group in sessions 1 and 2 respectively, with fixed effects for the workshop spell. In column 3, I report the difference in the production between the two sessions. In columns 4 and 5, I report the analog effects on units per worker, i.e. team level output divided by 3 for session 1 and divided by 4 for session 2. Column 6 then, as in column 3, reports the difference in the per-worker output between the two sessions. The same pattern of outcomes is repeated in columns 7 - 18, for high and low coordination teams respectively. In panel B, I report the difference-in-difference estimates, comparing the single differences reported in columns 3, 6, 9, 12, 15, and 18 pairwise for each of the three combinations involved across the three treatment groups. The difference-in-difference estimates use workshop spell fixed effects (as well as coordination level fixed effects in column 3), with robust standard errors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Referral candidates' labor market outcomes

	Pr(in paid work)		Most recent unemployment spell (in days)	
	(1)	(2)	(3)	(4)
	Upper caste	Lower caste	Upper caste	Lower caste
Treatment	0.033 (0.040)	0.120* (0.062)	-1.555 (3.900)	-11.018* (6.603)
UC vs. LC p-value		0.238		0.218
Control mean	0.823	0.710	27.440	37.900
R ²	0.00	0.02	0.00	0.01
Observations	418	200	418	185

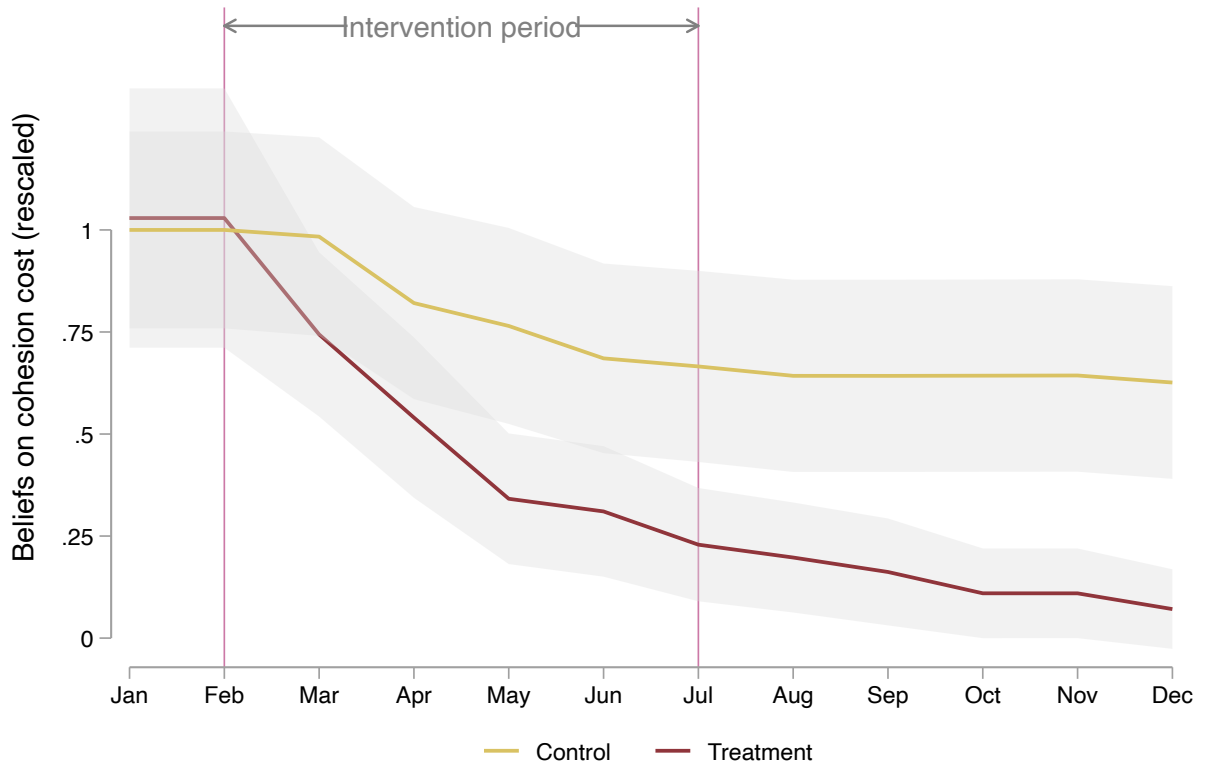
This table reports treatment effects on the labor market activity among referral candidates, i.e. connections of the incumbent workers, as reported in a phone-based follow-up survey. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Vignettes survey: changes in output

Recruitment type ↓→		Change in expected output per worker after recruitment					Obs	
		Mean	Pairwise test [row - column]					
			UC stranger	UC referral	LC stranger	LC referral		LC stranger
Caste label ↓→			No	No	No	No	Yes	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
UC stranger	No	0.16	-					93
UC referral	No	0.48	-0.09 (0.30)	-				107
LC stranger	No	-0.01	-0.80* (0.47)	-0.52 (0.35)	-			112
LC referral	No	0.42	0.16 (0.27)	0.04 (0.20)	1.03** (0.43)	-		88
LC stranger	Yes	-0.09	-0.16 (0.24)	-0.48* (0.25)	-0.12 (0.18)	-0.59** (0.28)	-	95
LC referral	Yes	0.44	0.09 (0.25)	0.21 (0.20)	0.36 (0.24)	-0.12 (0.18)	0.75*** (0.26)	105

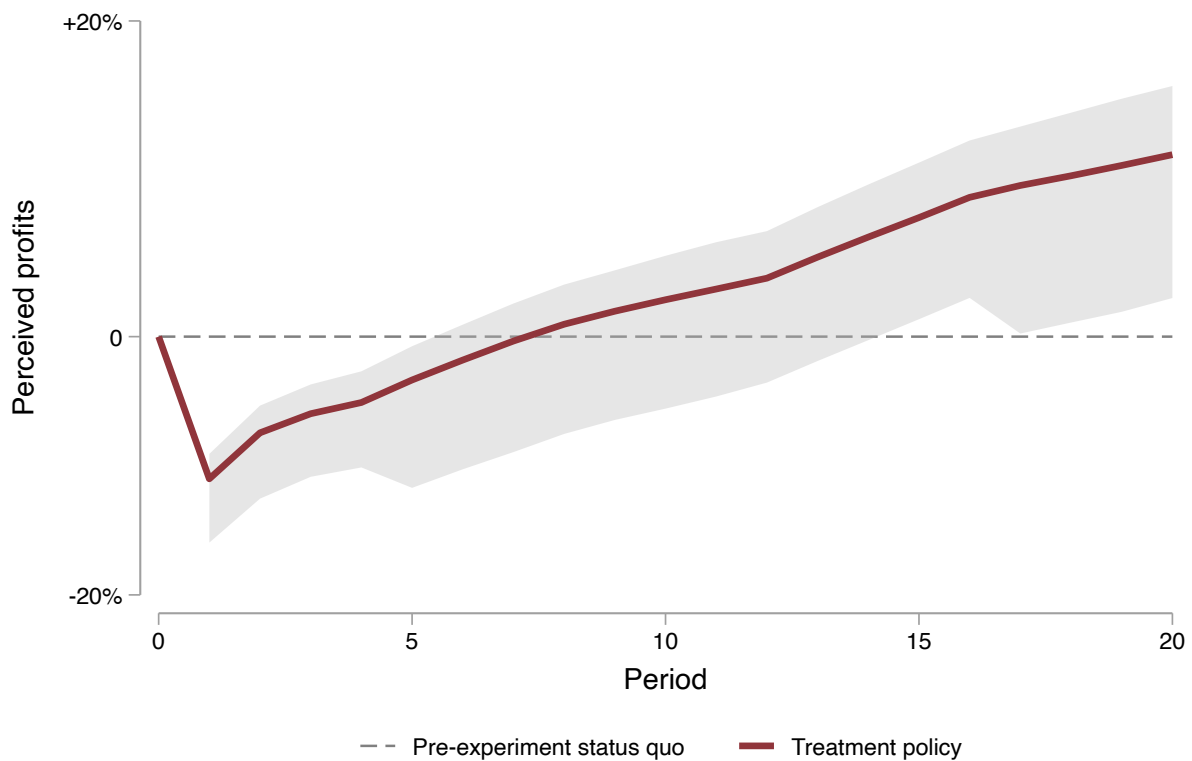
This table reports average reported responses for the change in expected output per worker as a hypothetical team grows from three to four through each reported mode of recruitment. The modes are listed in the labels of rows and columns, with each cell of the matrix in columns 2-6 reporting the comparison of the columns between the mode listed in the row and the mode listed in the column. Column 1 reports the mean change in output per worker for each listed mode of recruitment as reported in the row labels. Column 7 reports the number of respondents who faced a question on the recruitment type listed in the row labels. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 3: Evolution of beliefs on cohesion costs



This figure plots the trajectory of supervisor beliefs about κ , i.e. the cohesion cost in the Bayesian learning model described in section 5. The bands around treatment and control team trajectories indicate 95% confidence intervals. The initial beliefs are from the baseline survey with supervisors, and the trajectories are estimated from team-level panel data through the process described in section 5.

Figure 4: Evolution of perceived profits



This figure plots the simulated trajectory of perceived profits under the treatment policy relative to the pre-experiment benchmark, normalized to the mean of the control arm in the pre-intervention period. The solid red line shows the treatment path; the shaded region is the 95% Monte Carlo envelope from binomial exit and Gaussian output shocks. The initial dip reflects implementation frictions, pessimistic priors about cohesion, and churn while new hires are on the bench. Recovery occurs as supervisors update beliefs about cohesion costs, LC cohorts mature into productive workers, and team stability improves. Profits cross above baseline within 5–14 weeks (mean \approx 6 weeks).

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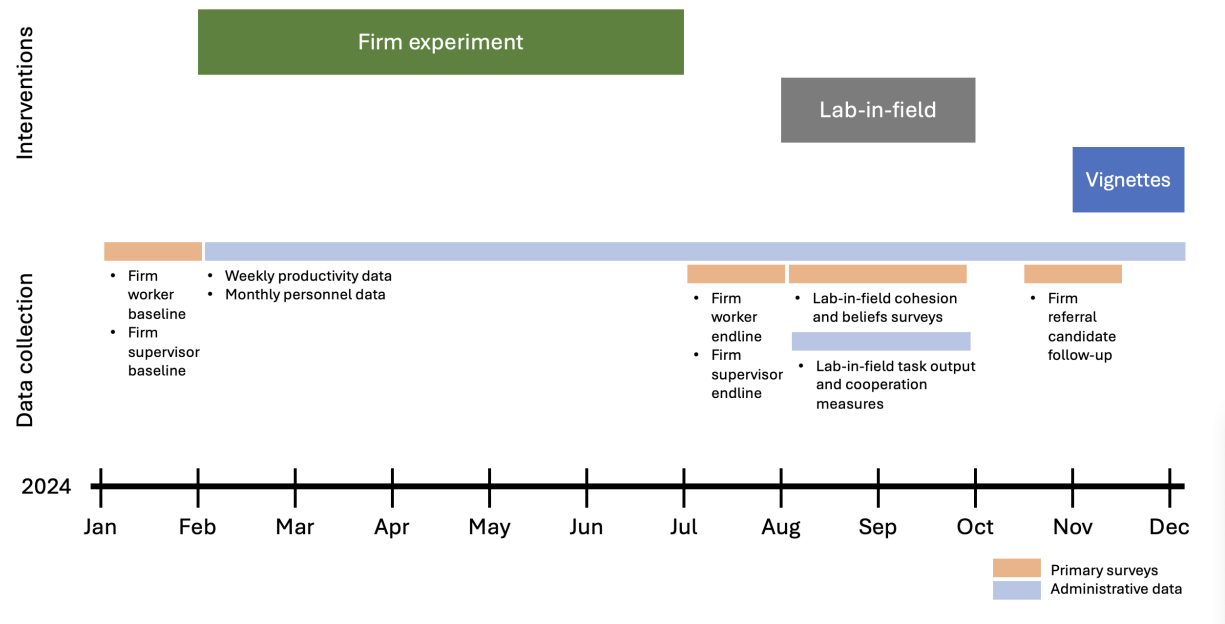
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A Study implementation details

Figure A.1: Study timeline



This figure plots the timeline for the study, noting both the different experiments as well as the rounds of data collection over the year of 2024.

Table A.1: Data sources

Indicators	Frequency	Unit	Time	Source
<i>Panel A: Firm experiment</i>				
Output	Weekly	Team	Jan – Dec	Admin data
Entries and exits	Monthly	Individual		
Referral allocations	Monthly	Individual		
Referral candidates elicitation	Once	Individual	Jan	Baseline
Outgroup beliefs	Twice	Individual	Jan & Jul	Baseline & endline
Job satisfaction	Twice	Individual		
Self-reported cohesion	Twice	Individual		
<i>Panel B: Follow-up of referral candidates</i>				
Earnings	Once	Individual	Oct	Referral-candidate end-line
Job search activity	Once	Individual		
<i>Panel C: Lab-in-field experiment</i>				
Output	Twice	Team	Aug	Enumerator counts
Symbol matching score	Twice	Individual		
Revealed measures of cohesion	Once	Individual		Endline
Outgroup beliefs	Twice	Individual		
<i>Panel D: Vignettes experiment</i>				
Effect on expected output	Once	Individual	Nov	Vignettes survey
Effect on expected cohesion	Once	Individual		

This table represents the frequency, unit, time period and source of the main indicators used for analysis across the firm experiment, the follow-up survey with referral candidates, the lab-in-field experiment, and the vignettes experiment in panels A, B, C, and D respectively.

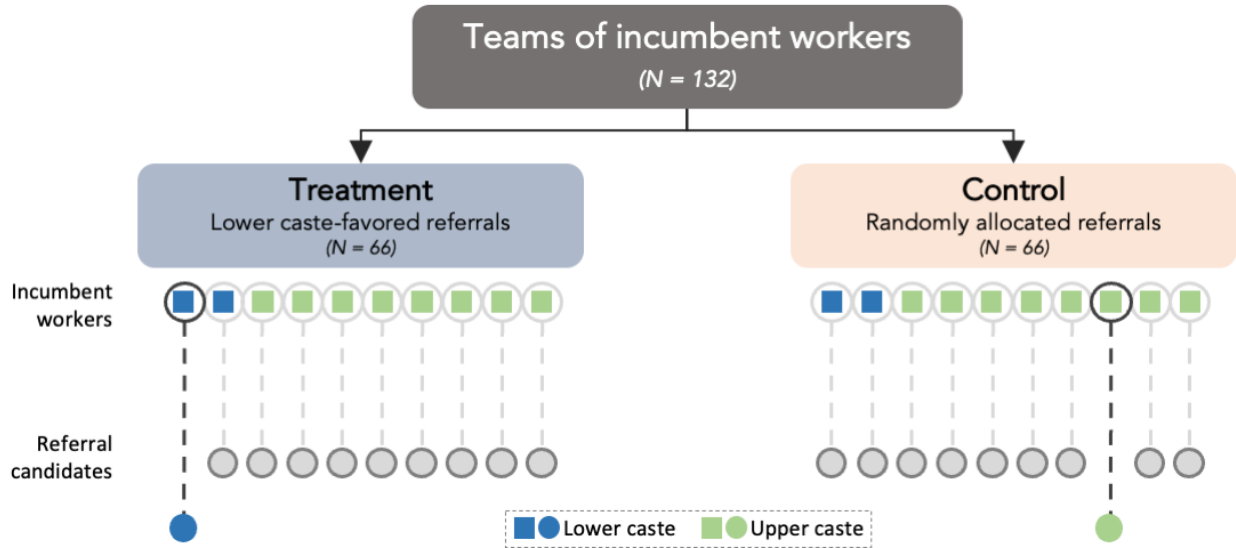
Firm experiment

Table A.2: Tasks at the firm

Task	Description	# of teams	Coordination level
Assembly	Preparing finished footwear pieces	27	High
Packing	Packing finished articles for shipping	22	High
Sole preparation	Layering the components that make up the sole of the footwear	20	High
Maintenance	Repairing machines across the factory	12	High
Cutting	Portioning fabric and other materials for subsequent steps	10	Low
Polymer	Preparing pellets for extruder and shaping into basic footwear structure	21	Low
Stitching	Stitching straps and logos onto footwear	21	Low
Total		132	

This table summarizes the tasks that teams at the firm work on, with a short description, the total number of teams engaged in each task, and the level of coordination required among workers to execute the task.

Figure A.2: Firm experiment design



This figure plots the design of the firm intervention. The square icons represent incumbent workers, with green squares representing UCs and blue squares representing LCs. At the baseline at the firm, each incumbent worker lists (an unconstrained number of) referral candidates, represented here by circles. In the treatment group, an LC incumbent worker (blue square) gets the opportunity to refer someone. This worker's referral candidate is T_1 (blue circle, to indicate homophily such that referrals and referrers belong to the same group, though that is not mandated in the referral elicitation). T_0 represents the candidates of the other incumbent workers who are not called upon to refer from their network. In the control group, the referral opportunity goes to a UC incumbent worker (green square), chosen at random. This worker's referral candidate is R_1 (green circle). R_0 represents the candidates of the other incumbent workers.

Figure A.3: Referral allocation lists

Line number	Department	Shift number	Order of asking	Employee ID
1	ASSEMBLY	A	1	12
1	ASSEMBLY	B	1	58
1	ASSEMBLY	B	2	00
1	ASSEMBLY	C	1	25
1	CUTTING	A	1	14
1	CUTTING	A	2	85
1	CUTTING	A	3	24
1	CUTTING	B	1	07
1	CUTTING	B	2	22
1	CUTTING	C	1	54
1	CUTTING	C	2	55
1	MAINTENANCE	A	1	74
1	MAINTENANCE	A	2	75
1	MAINTENANCE	C	1	46
1	MAINTENANCE	C	2	19
1	PACKING	A	1	49
1	PACKING	A	2	10
1	PACKING	B	1	41
1	PACKING	B	2	09
1	PACKING	B	3	20

This figure shows an example of the spreadsheets shared with the firm's HR department each month, listing the referral allocations they are asked to make in order. Note that the order is listed for both treatment and control teams alike, since both sets of supervisors are uniformly asked to defer to this list.

Lab-in-field experiment

Production task. Prior to commencing the production tasks in the lab-in-field workshops, each team member was assigned a random cloth color and instructed to cut strips only from that cloth. To prevent batching effects, participants were required to cut and tie strips sequentially rather than stockpiling them. In high-coordination teams, members were required to follow a predetermined color sequence when tying strips together, necessitating close coordination in both cutting and tying. In low-coordination teams, workers cut and tied strips for their own color independently

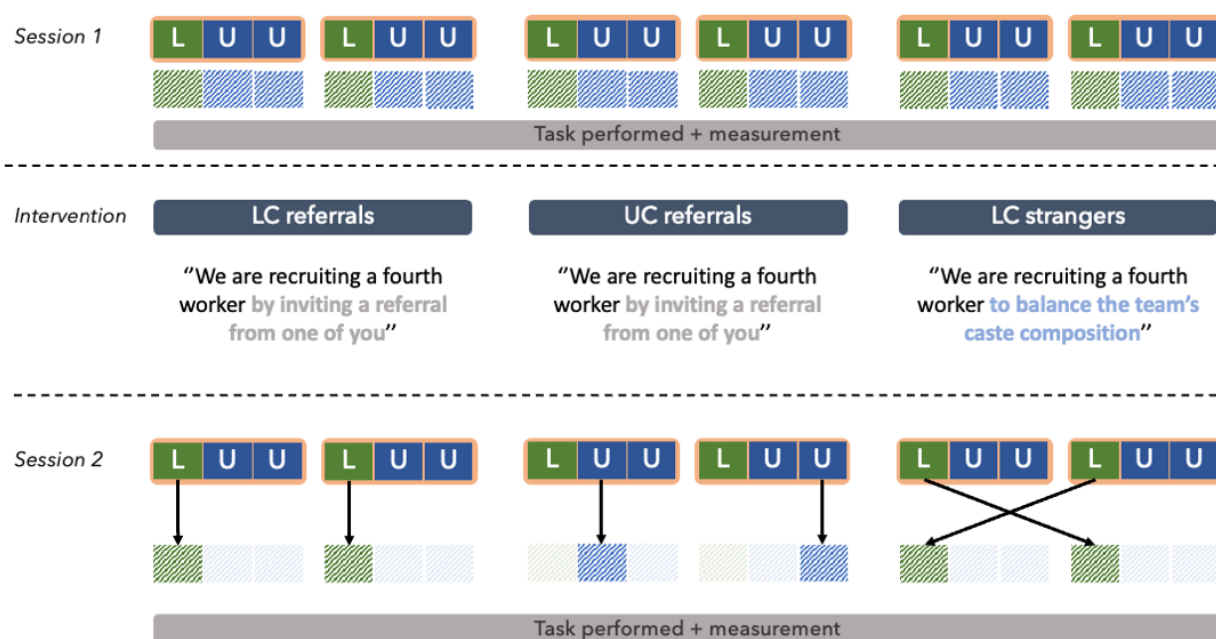
before combining them across teammates at the end.

At the end of each workshop session, two independent enumerators separately measured the length of each finished rope (in the units of the number of strips). Only ropes meeting the pre-specified dimensions (10 cm \times 2 cm per strip) were included in length calculations. The median of the two enumerators' measurements was used as the outcome, with over 90 percent agreement between the two measures. One enumerator also recorded the number of strips of each color to generate disaggregated output measures.

Symbol-matching game. For the symbol-matching game, each worker was randomly paired with a teammate and given five minutes to match as many symbols as possible across their two sheets. Each worker's sheet contained a unique, randomly assigned symbol grid from a set of 15 possible grids. Workers recorded the number of matches they believed to be correct, after which enumerators collected both sheets, noted the IDs of the worker and their partner, and computed the true number of matches. A second round followed with a new randomly assigned teammate.

"True" matches were estimated by comparing each combination of the two symbol grids. I then calculated the absolute difference between the true and reported matches, averaged at the worker level, and served as a revealed measure of cohesion. This design ensured independent effort while allowing measurement of coordination quality across varying teammate pairings.

Figure A.4: Lab-in-field experiment design



This figure shows the design of the lab-in-field experiment. Each of the workers recruited for this workshop are asked to identify referral candidates. In the first session of the lab-in-field exercise, randomly formed teams with one LC and two UCs each are asked to perform a task. After the production, each team is sorted into one of three recruitment policies, with the message given to incumbents detailed in the figure. In this recruitment process, each team increases from three to four workers. In the second session, these teams of four workers each work on another iteration of the same task. After both production sessions, the output is measured and all the workers are surveyed.

Figure A.5: Symbol matching game

Date: _____ Respondent ID: _____

Partner ID: _____

Κ	Ρ	Υ	φ	δ	Σ	Δ	σ
β	Θ	Ο	ά	π	ω	Ψ	μ

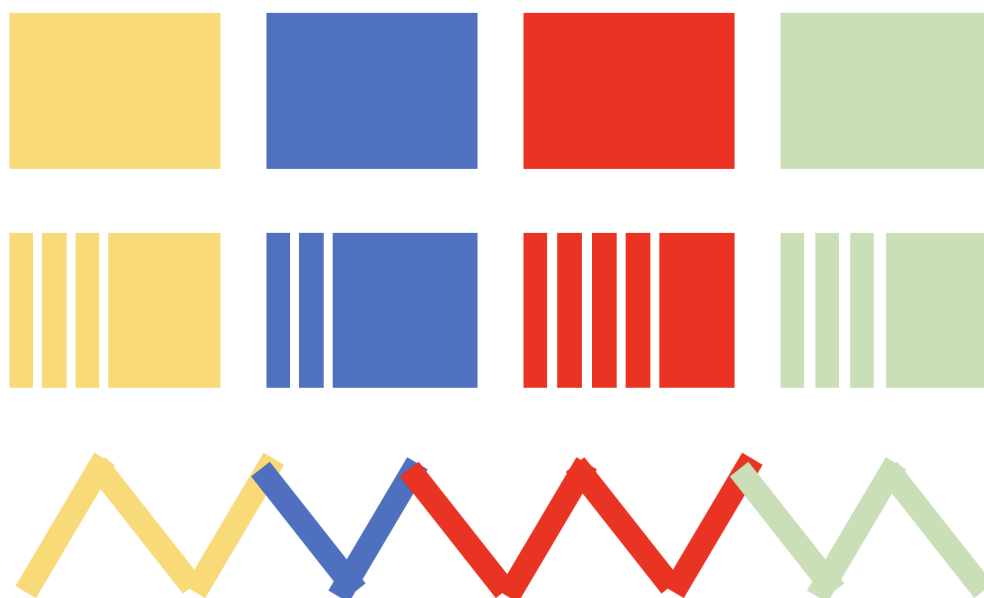
Date: _____ Respondent ID: _____

Partner ID: _____

ι	Α	Β	Ζ	ι	Ψ	η	β
ρ	Δ	Ζ	ώ	β	ι	α	φ
Ο	Υ	ό	φ	μ	Θ	Θ	Υ
η	ι	φ	Ω	μ	σ	Θ	β
ρ	Θ	ρ	Ε	Ε	ί	Μ	Ε

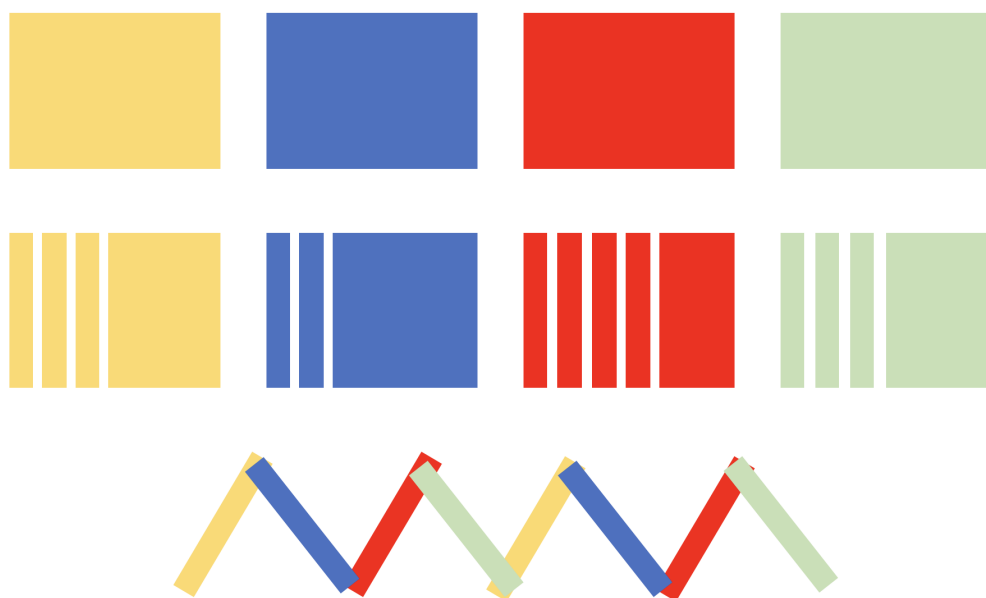
This figure shows an example of the sheets used by the workers in the symbol matching exercise used to get revealed measures of social cohesion.

Figure A.6: Illustrative example of the lab-in-field task: low coordination teams



This figure represents the task that the workers in the lab-in-field exercise worked on. Each worker was given a sheet of fabric of a predetermined size and color, represented in the first row of this figure. They were then asked to cut strips of predetermined dimensions, represented in the second row. Finally, the team was asked to tie together these strips, represented in the third row. The team was compensated for the length of this combined rope, made up of the strips tied together. In low-coordination teams, the task did not require for any particular order of colors to be followed in constructing the rope. The incentive and effort compatible thing for workers in these teams to do is to cut and tie as many strips as possible individually, and simply to tie their individual ropes together to make a cumulative rope that is as long as possible.

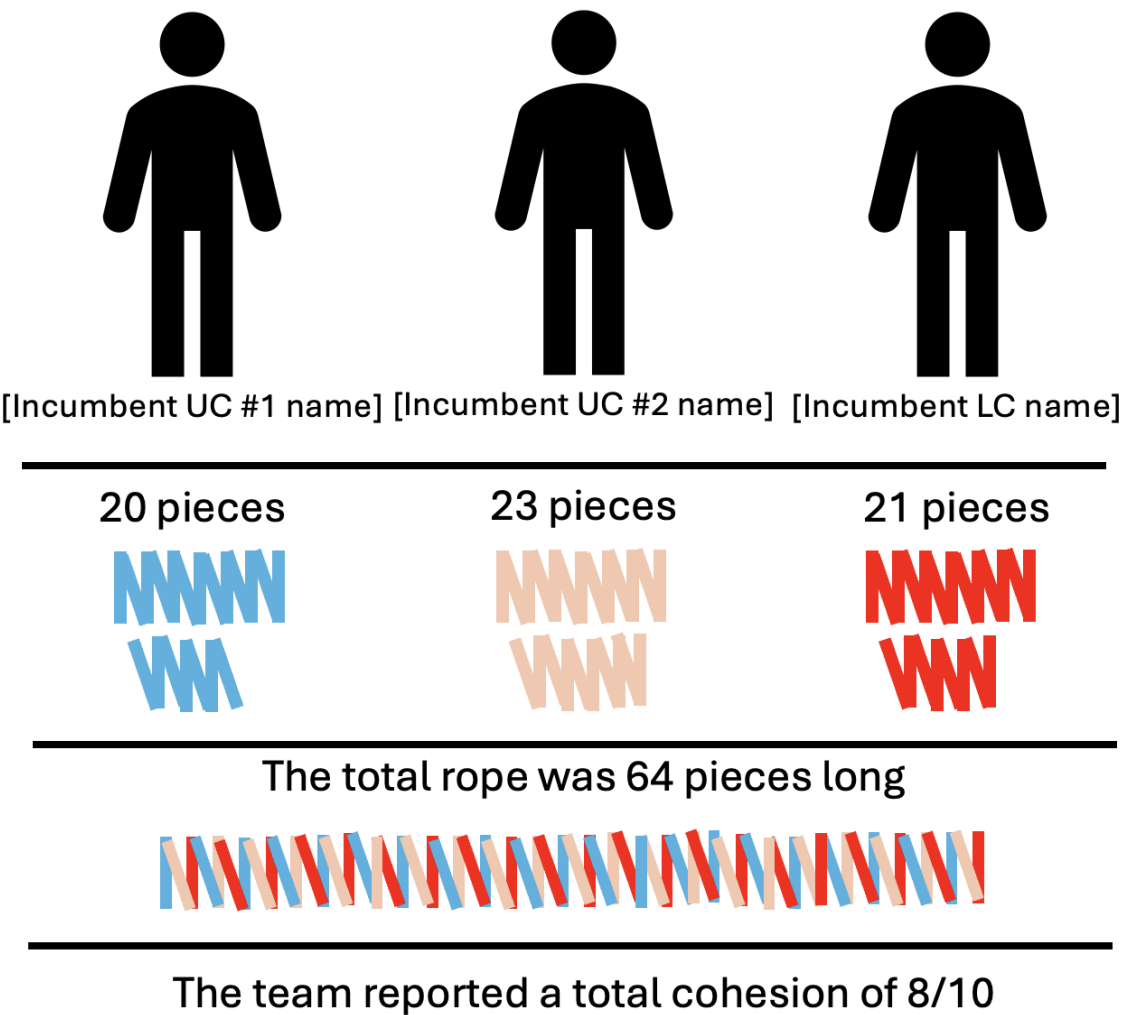
Figure A.7: Illustrative example of the lab-in-field task: high coordination teams



This figure represents the task in the lab-in-field similar to the previous figure, but for high-coordination teams. The key difference between these team types (and thus figures) is that high-coordination teams were asked to follow a pre-determined order of colors in constructing the rope from the strips. This necessitated workers to coordinate and time their effort together in order to construct a rope that is as long as possible.

Vignettes experiment

Figure A.8: Information provided in the vignettes survey



This figure was used in the vignette survey to make concrete for respondents the team for which a range of recruitment policies was then presented.

Figure A.9: Hypothetical profiles for belief elicitation

	Profile 1	Profile 2	Profile 3	
Name	LC name	UC name	LC name	UC name
State of origin	Bihar	Bihar	Bihar	Rajasthan
Education	5 years	5 years	5 years	10 years
Experience	0 years	0 years	0 years	5 years

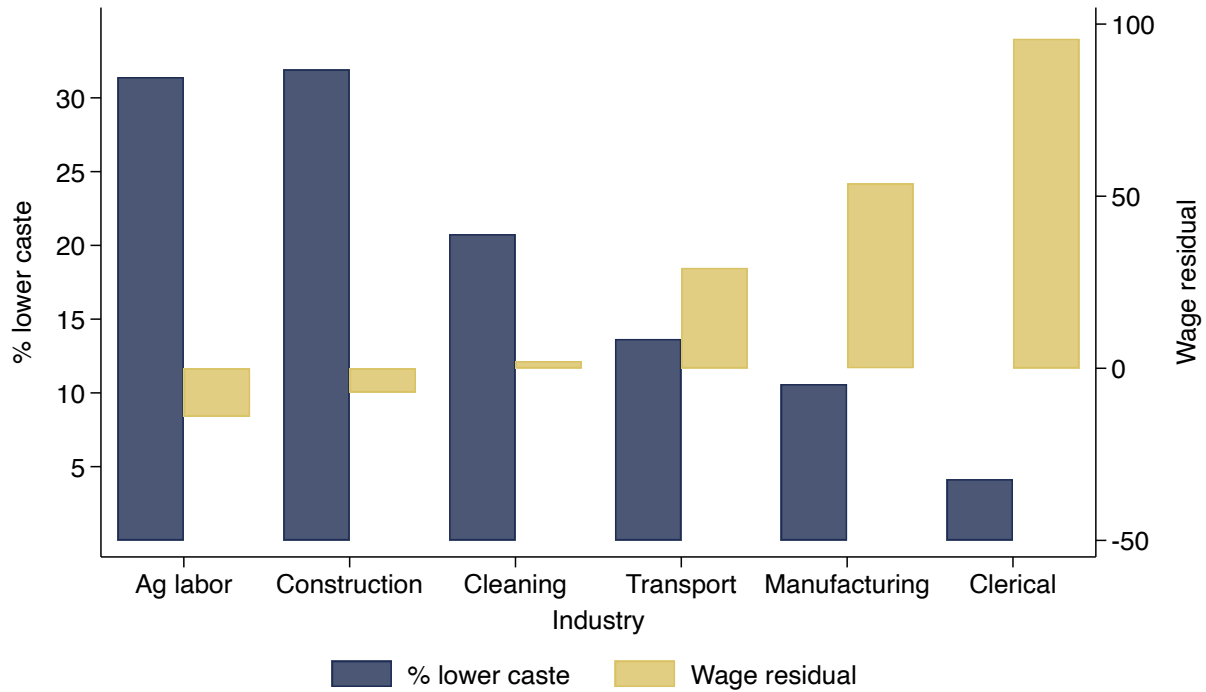
One element randomized

This figure represents the three profiles of hypothetical workers used in the baseline and endline surveys at the firm to elicit worker and supervisor beliefs about out-groups.

B Supplementary exhibits

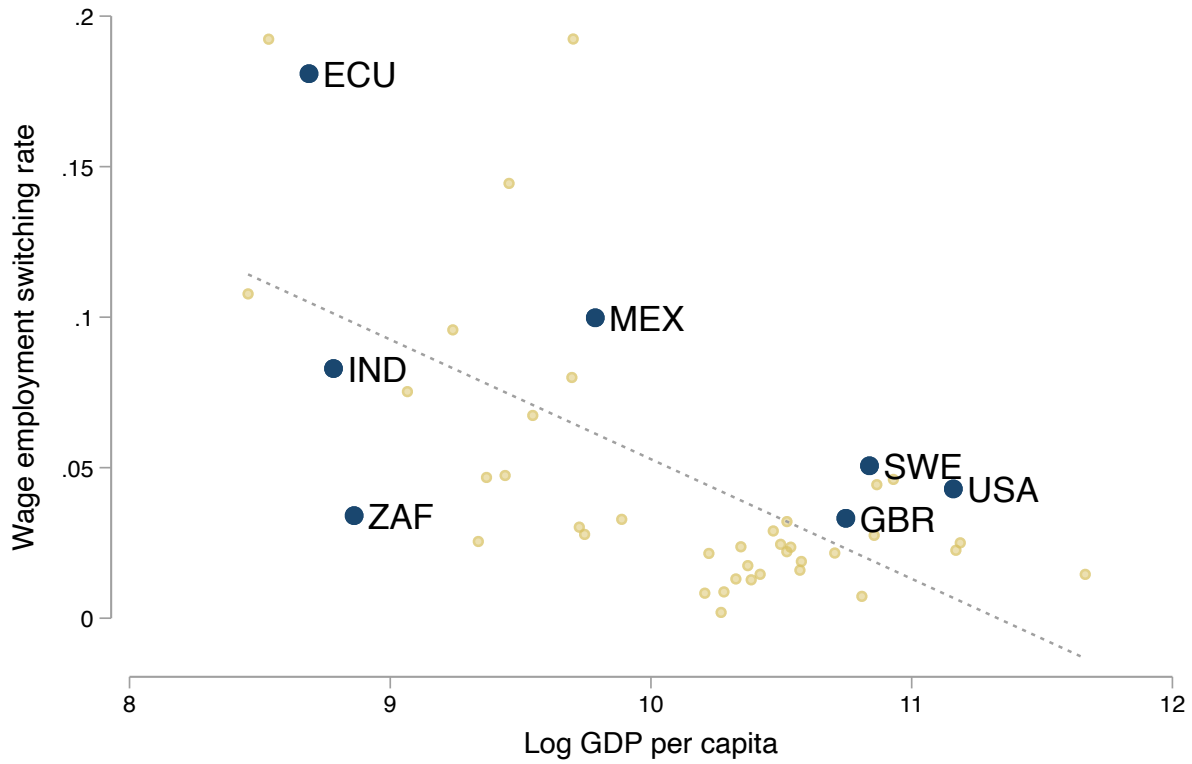
B.1 Original analysis of secondary data

Figure B.1: Descriptive comparison of worker caste composition and industry premia



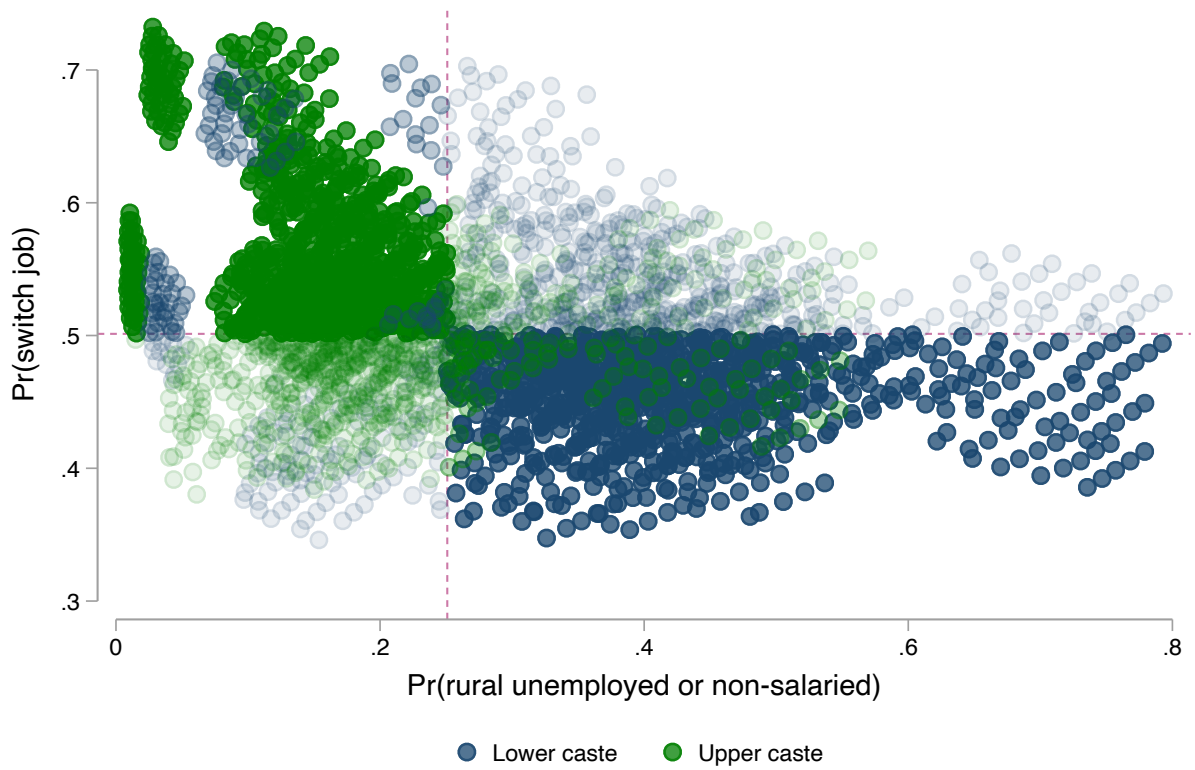
This figure represents original analysis of the nationally representative Indian Human Development Survey 2014-15 data. On the right axis, I plot the wage residual across the lowest key industry-occupation groups reported in the survey, controlling for age, education, gender, and district of birth. On the left axis, I plot the share of the workforce in the given industry-occupation group that is lower caste.

Figure B.2: Descriptive comparison of worker caste composition and industry premia



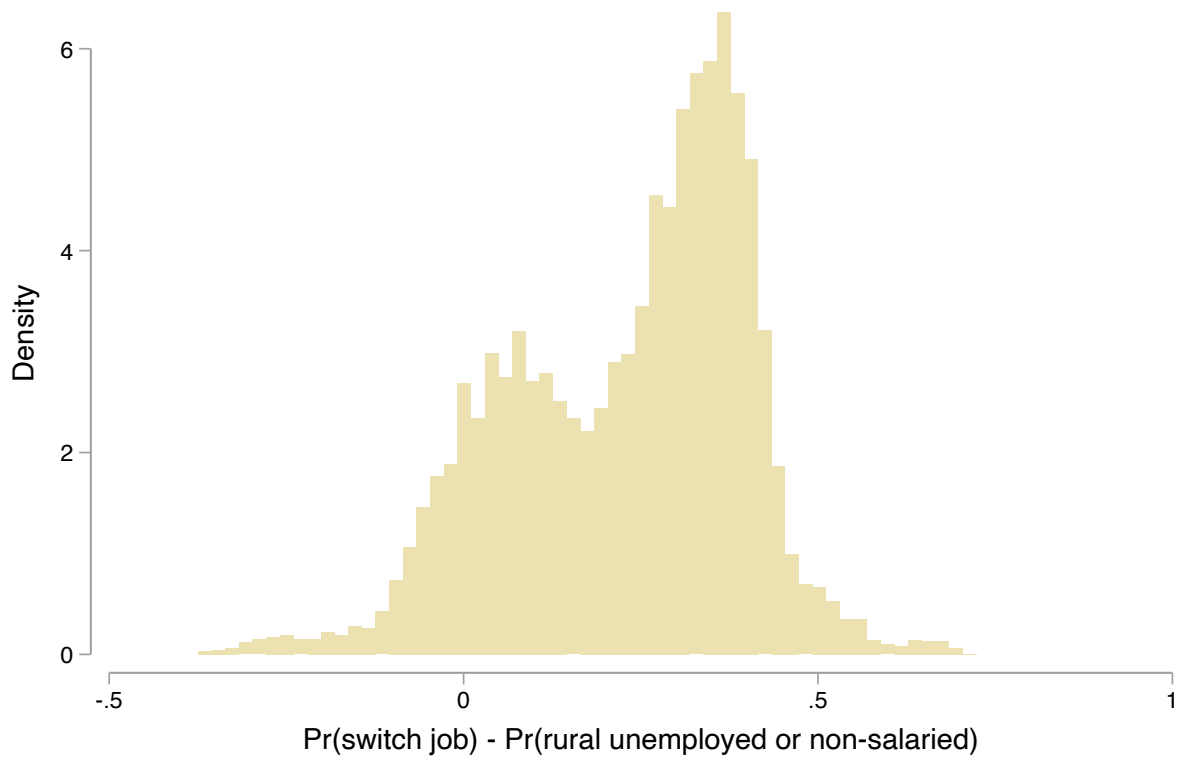
This figure represents original analysis of the nationally representative Indian Human Development Survey 2014-15 data. On the right axis, I plot the wage residual across the lowest key industry-occupation groups reported in the survey, controlling for age, education, gender, and district of birth. On the left axis, I plot the share of the workforce in the given industry-occupation group that is lower caste.

Figure B.3: Descriptive comparison of worker caste composition and industry premia



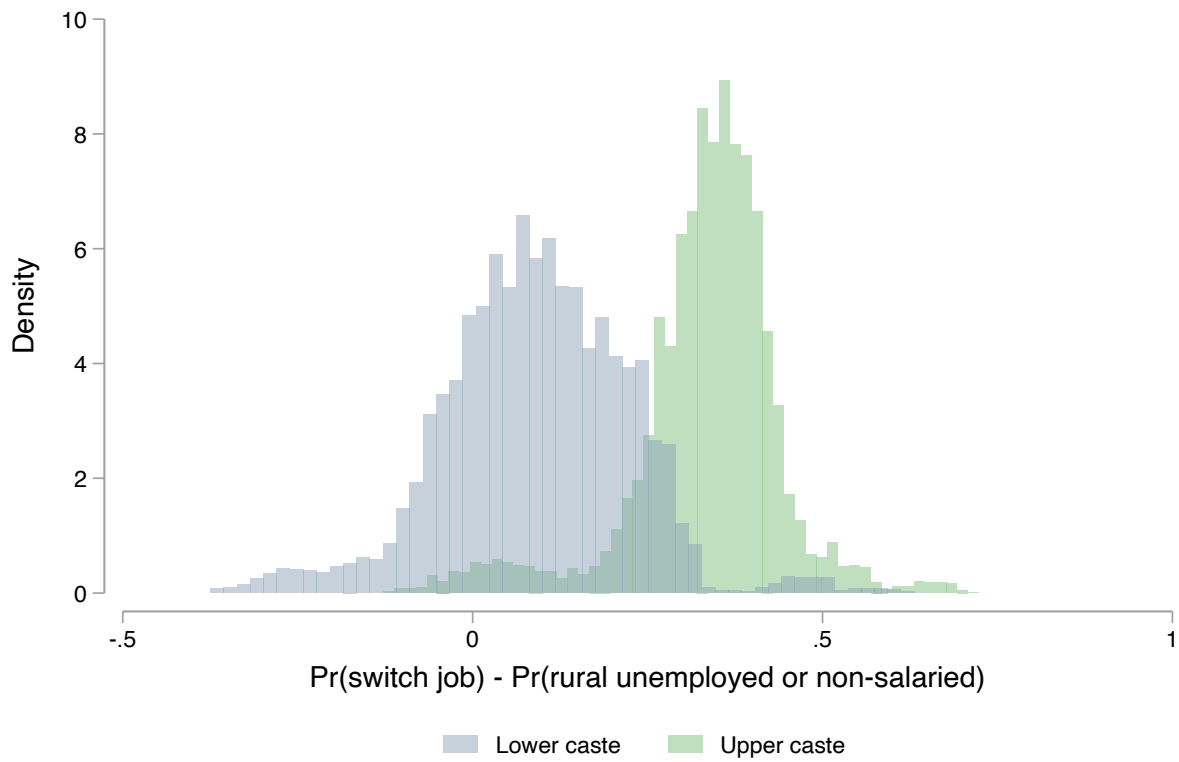
This figure represents original analysis of the nationally representative Indian Human Development Survey 2014-15 data. On the right axis, I plot the wage residual across the lowest key industry-occupation groups reported in the survey, controlling for age, education, gender, and district of birth. On the left axis, I plot the share of the workforce in the given industry-occupation group that is lower caste.

Figure B.4: Descriptive comparison of worker caste composition and industry premia



This figure represents original analysis of the nationally representative Indian Human Development Survey 2014-15 data. On the right axis, I plot the wage residual across the lowest key industry-occupation groups reported in the survey, controlling for age, education, gender, and district of birth. On the left axis, I plot the share of the workforce in the given industry-occupation group that is lower caste.

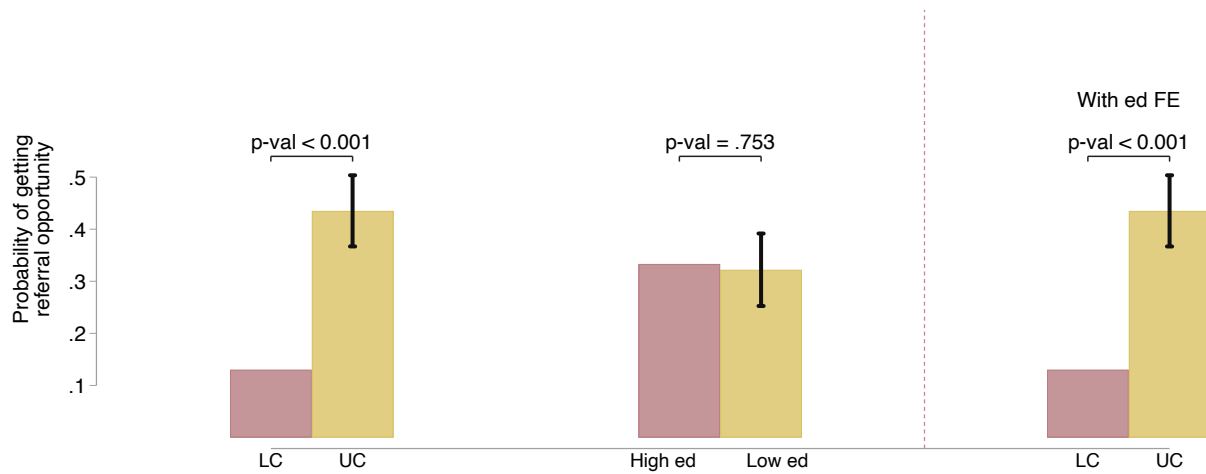
Figure B.5: Descriptive comparison of worker caste composition and industry premia



This figure represents original analysis of the nationally representative Indian Human Development Survey 2014-15 data. On the right axis, I plot the wage residual across the lowest key industry-occupation groups reported in the survey, controlling for age, education, gender, and district of birth. On the left axis, I plot the share of the workforce in the given industry-occupation group that is lower caste.

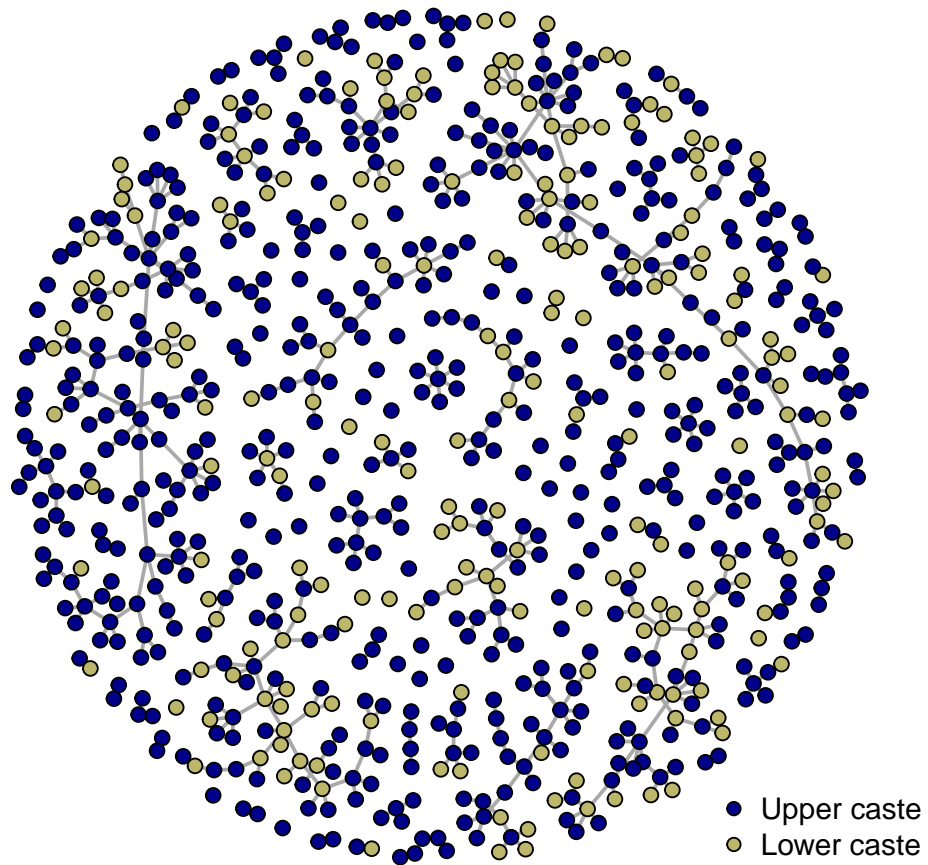
B.2 Baseline firm survey

Figure B.6: Targeting of referral opportunities at baseline



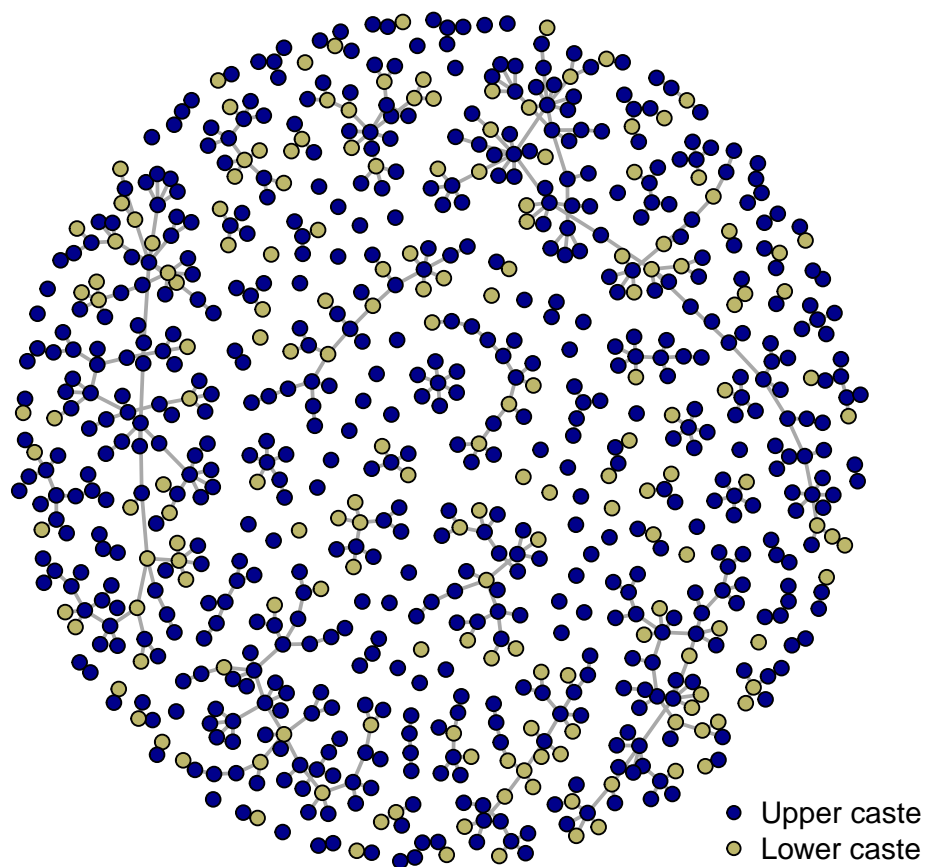
This figure uses data from the baseline survey at the firm to compare the referral allocation across UC and LC workers at the firm by the supervisors, with and without education FEs, which is used as a proxy for individual ability absent any other data.

Figure B.7: Incumbent worker network at baseline



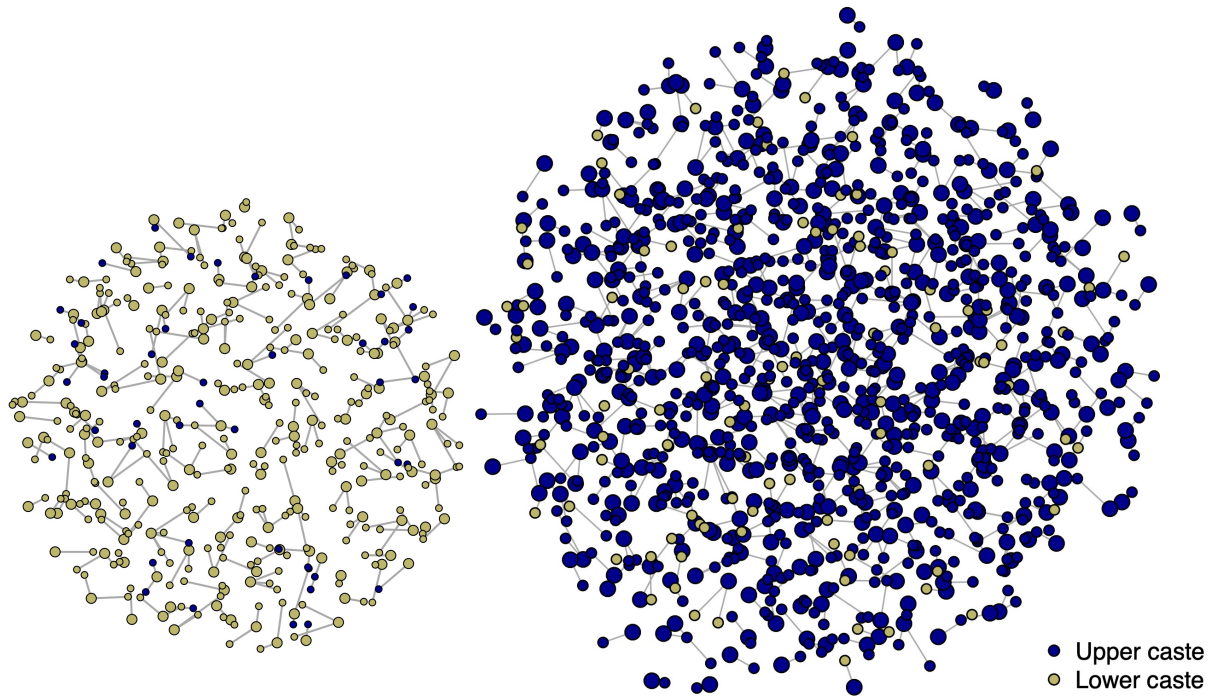
This figure represents a network graph by caste for incumbent workers at the firm at the time of the baseline, with links between referrers and referrals.

Figure B.8: Incumbent worker network at baseline (if random)



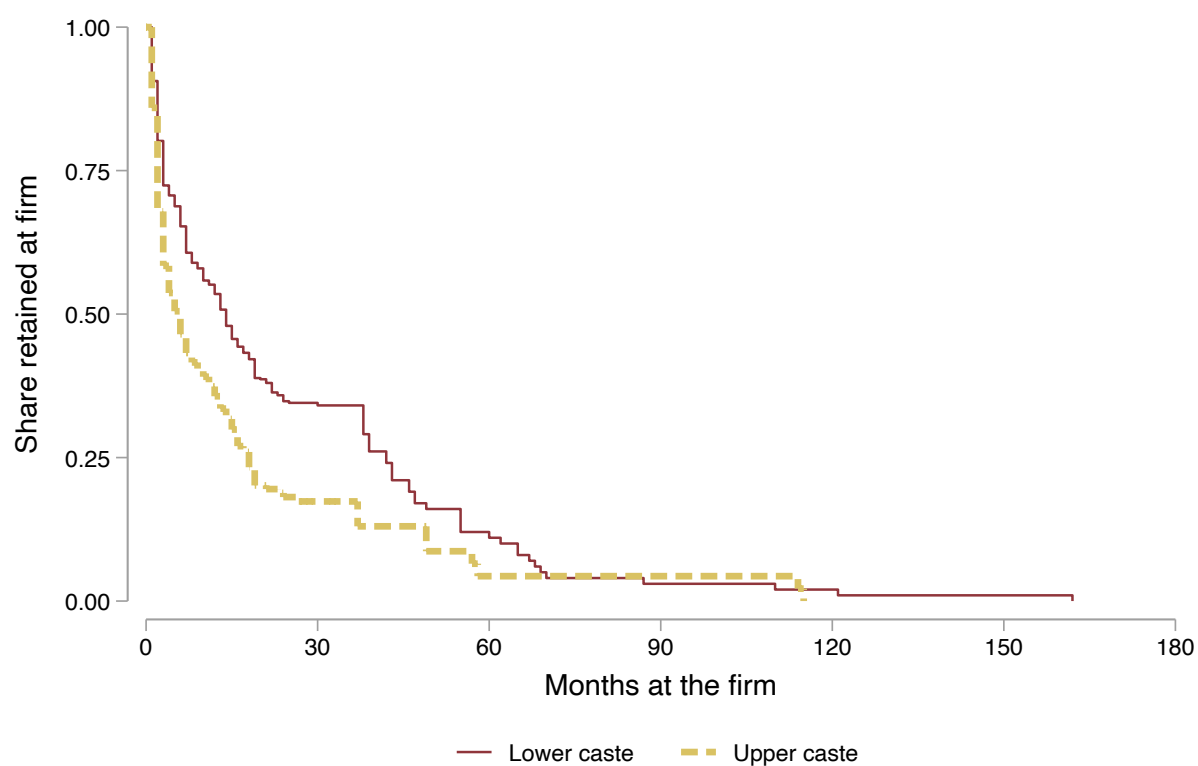
This figure represents a benchmark network graph relative to the previous slide, representing what links would look like if referrals were made proportionately by caste based on their share in the workforce.

Figure B.9: Network of referral elicitation



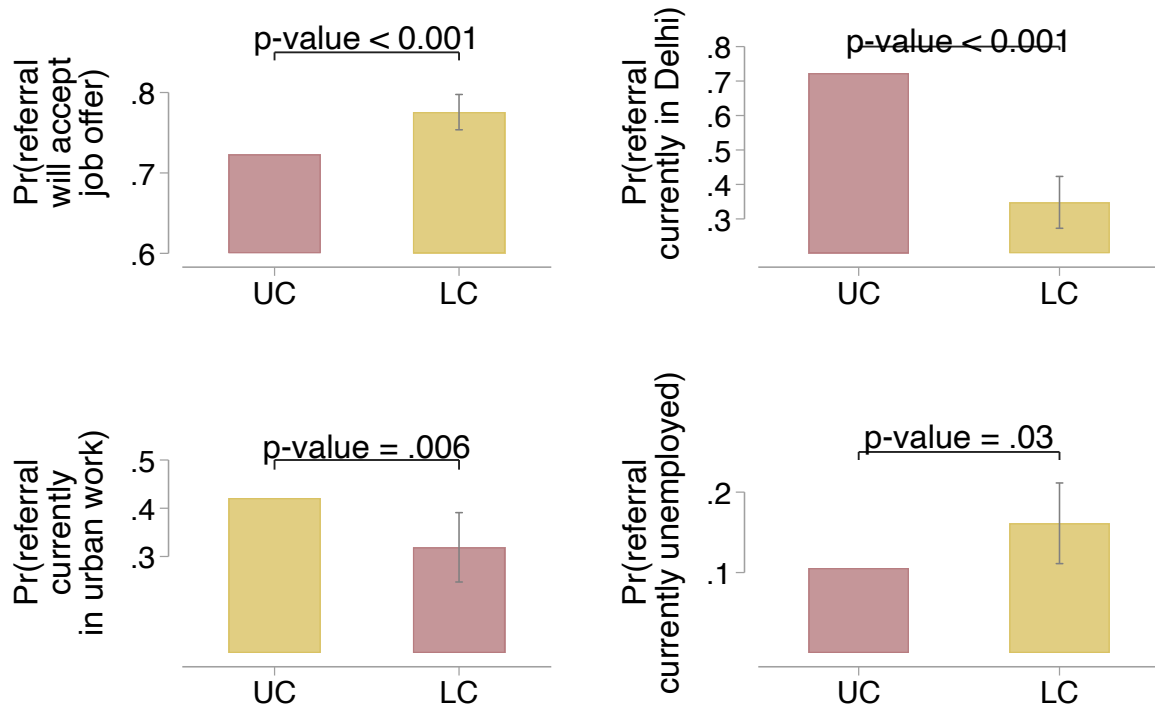
This figure represents a network graph focusing on the links between incumbent workers and their referral candidates, elicited at baseline. The graphs are represented separately for UC and LC incumbent workers, with each incumbent worker listing an average of 2.2 referral candidates.

Figure B.10: Baseline retention rates by caste



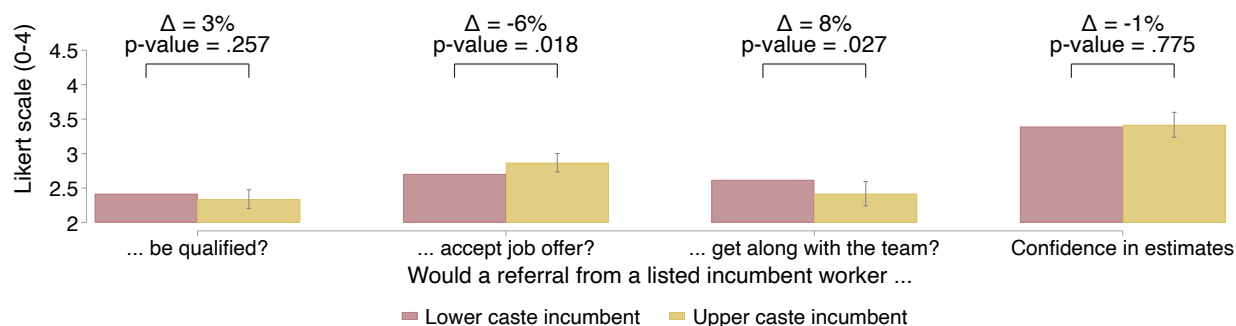
This figure shows estimates from a survival analysis among the set of incumbent workers at the firm as of the baseline, showing that lower caste workers have a higher expected retention rate.

Figure B.11: Descriptive comparison of referral candidates (firm experiment)



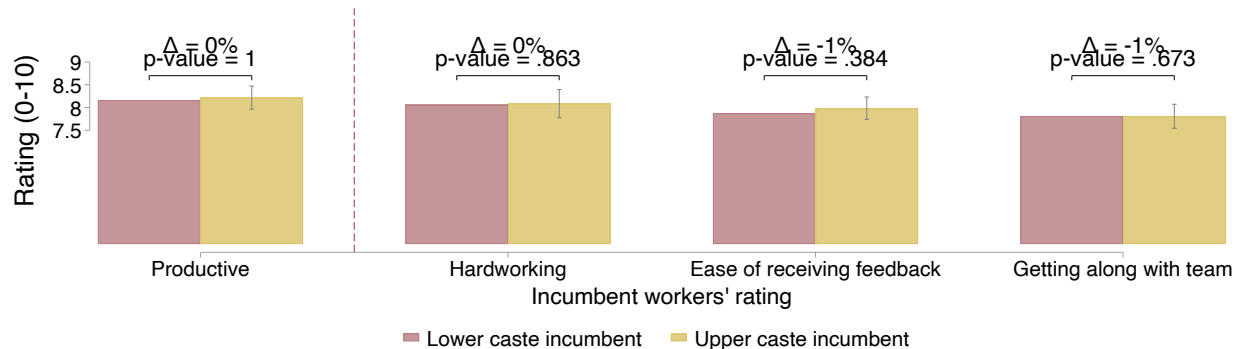
In this figure, I report simple descriptive comparisons of the referral candidates of lower caste and upper caste incumbent workers at the firm, as reported by the referrers (i.e. the incumbent workers) themselves during the baseline survey. The first graph reports the difference in lower and upper caste referral candidates' probability of accepting the job, as reported by the referrers. The second graph reports the difference in the probability of already being in Delhi as of the baseline survey, the site of the experiment, as reported by the referrers. The third graph reports the difference in the probability of the referral candidates being in urban work as of the baseline survey, as reported by the referrers. The fourth graph reports the difference in the probability of the referral candidates being unemployed as of the baseline survey, as reported by the referrers. The p-values for these comparisons are from simple bivariate regressions by the caste of the referrer with task fixed effects and robust standard errors.

Figure B.12: Supervisor beliefs about referrals by referrer caste at baseline



This figure summarizes supervisors' baseline ratings of whether a referral from a listed incumbent would (i) be qualified, (ii) accept a job offer, and (iii) get along with the team, plus the supervisor's confidence in those assessments. Ratings use a 0–4 Likert scale. Bars compare hypothetical prospective referrals from randomly selected lower caste vs. upper caste incumbents on the supervisor's team. Reported p-values come from bivariate regressions with supervisor fixed effects and robust standard errors.

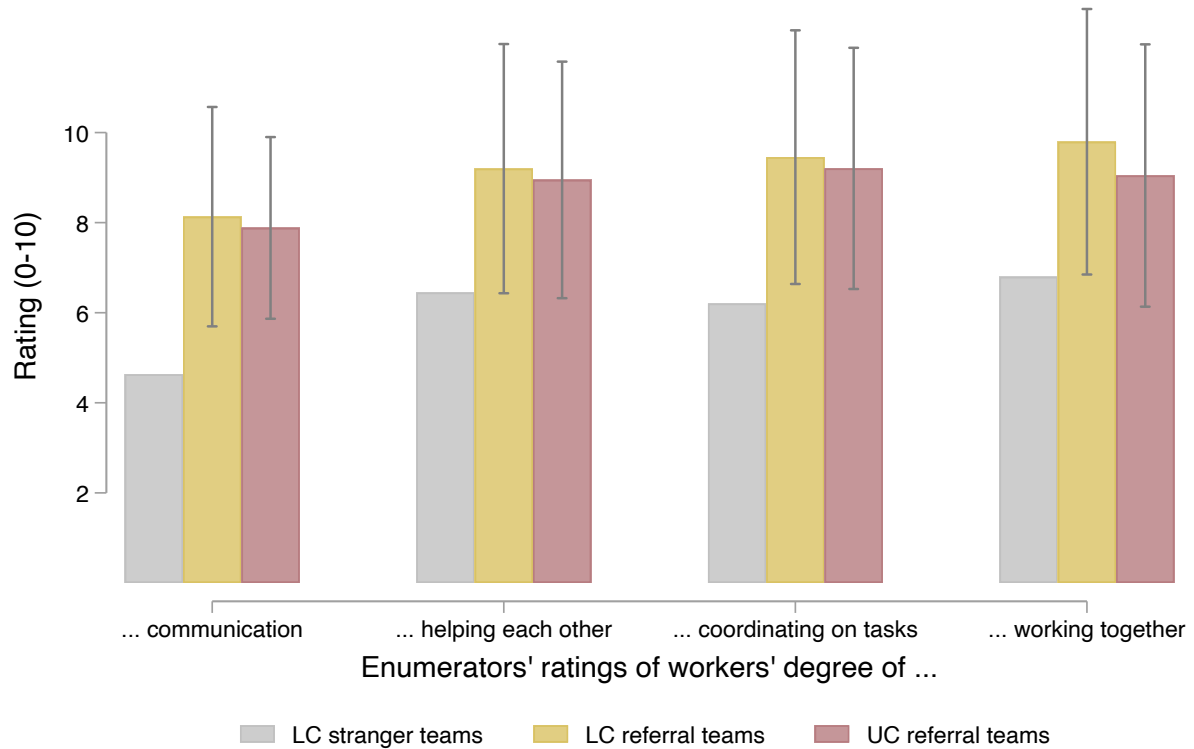
Figure B.13: Incumbent workers' ratings of teammates by caste at baseline



This figure plots incumbent workers' 0–10 ratings of teammates on productivity, hard-work, ease of receiving feedback, and getting along with the team, comparing randomly selected lower caste and upper caste incumbents on the supervisor's team. Reported p-values come from bivariate regressions with supervisor fixed effects and robust standard errors.

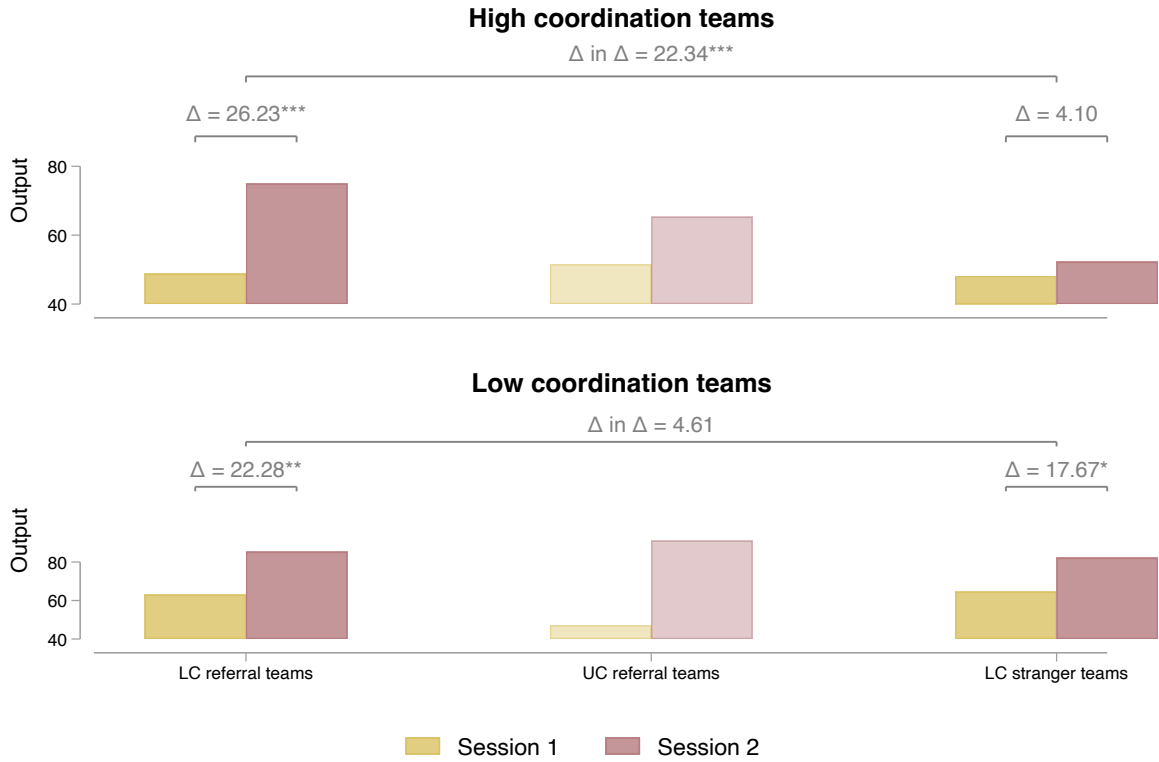
B.3 Lab-in-field survey

Figure B.14: Enumerators' observations of worker cohesion



This figure represents a bar graph plotting the extent of cohesion across the three types of teams in the lab-in-field exercise, as observed by the enumerators and rated on a scale of 0 - 10.

Figure B.15: Summary of results from lab-in-field experiment



This figure plots the treatment effect on raw output in the lab-in-field experiment. The yellow bar plots the output from the first session while the red bar plots the output from the second session, when each team had three and four workers respectively. This figure shows that in high coordination tasks, LC outsider teams gain no detectable value from session one to two, from adding an additional worker. On the other hand, the gains are large and detectable in the referral teams. These differences are denoted through difference in difference estimates marked as appropriate. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.1: Treatment effect on team cohesion by worker status (lab-in-field)

	Incumbent workers only			New worker involved		
	(1) All	(2) High coord	(3) Low coord	(4) All	(5) High coord	(6) Low coord
LC referral teams	0.051* (0.028)	0.095** (0.038)	0.007 (0.037)	0.060** (0.025)	0.076** (0.036)	0.044 (0.033)
UC referral teams	0.031 (0.030)	0.041 (0.044)	0.017 (0.039)	0.038 (0.026)	0.034 (0.032)	0.048 (0.041)
LC strangers mean	0.610	0.588	0.632	0.592	0.590	0.594
LC vs UC referrals p-value	0.514	0.258	0.805	0.462	0.238	0.924
R ²	0.08	0.19	0.07	0.13	0.17	0.18
Observations	480	240	240	480	240	240

This table reports treatment effects on pairwise tests from a symbol matching game to measure the extent of team cohesion, separately by the status of workers in the pairs. The outcome is defined as the one minus the absolute share of true matches between the two players' sheets that are not found. Thus, the outcome ranges between 0 and 1, wherein higher values indicate more cohesion. In columns 1-3, I report the effect on team cohesion considering only worker pairs where both workers were incumbent workers, i.e. were present in the first session of production prior to the recruitment. In columns 4-6, I consider only worker pairs where one of the two workers was a new worker recruited between the first and second session of production. All regressions use randomization strata fixed effects, and standard errors are clustered at the team level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Treatment effect on team cohesion by worker caste (lab-in-field)

	Same-caste pairs			Cross-caste pairs		
	(1) All	(2) High coord	(3) Low coord	(4) All	(5) High coord	(6) Low coord
LC referral teams	0.028 (0.026)	0.079** (0.035)	-0.025 (0.037)	0.078*** (0.028)	0.088** (0.037)	0.062 (0.040)
UC referral teams	-0.005 (0.035)	0.029 (0.044)	-0.052 (0.057)	0.069** (0.028)	0.052* (0.030)	0.096** (0.040)
LC strangers mean	0.628	0.596	0.663	0.573	0.581	0.565
LC vs UC referrals p-value	0.316	0.265	0.621	0.761	0.358	0.390
R ²	0.10	0.17	0.12	0.07	0.12	0.11
Observations	485	248	237	475	232	243

This table reports treatment effects on pairwise tests from a symbol matching game to measure the extent of team cohesion, separately by the caste group of the workers in the pairs. The outcome is defined as the one minus the absolute share of true matches between the two players' sheets that are not found. Thus, the outcome ranges between 0 and 1, wherein higher values indicate more cohesion. In columns 1-3, I report the effect on team cohesion considering only worker pairs where both workers belonged to the same caste. Note that this could include both upper caste-only pairs and lower caste-only pairs. In columns 4-6, I consider only worker pairs where both workers belonged to different caste groups. All regressions use randomization strata fixed effects, and standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Lab-in-field experiment: perceived fairness

	Fairness of recruitment (0-10)			Satisfaction with workshop (0-10)		
	(1) All	(2) High coord	(3) Low coord	(4) All	(5) High coord	(6) Low coord
LC referral teams	-0.552*** (0.149)	-0.436** (0.191)	-0.731*** (0.187)	-0.267** (0.129)	-0.311** (0.141)	-0.191 (0.191)
UC referral teams	-0.791*** (0.240)	-0.921*** (0.336)	-0.565** (0.279)	-0.061 (0.174)	-0.603*** (0.207)	0.864*** (0.191)
LC strangers mean	9.075	9.112	9.014	9.285	9.500	8.929
R ²	0.10	0.14	0.13	0.06	0.09	0.20
Observations	471	295	176	471	295	176

This table reports treatment effects on the perceived fairness of recruitment and satisfaction with the workshop, as reported in the endline survey at the lab-in-field experiment. Standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Lab-in-field experment: beliefs about out-groups

	Good at job			Hardworking			Good team member		
	(1) All	(2) High coord	(3) Low coord	(4) All	(5) High coord	(6) Low coord	(7) All	(8) High coord	(9) Low coord
LC referral teams	0.055 (0.152)	0.121 (0.178)	-0.097 (0.305)	-0.277 (0.180)	-0.211 (0.219)	-0.364 (0.304)	0.291* (0.174)	0.425* (0.216)	0.040 (0.216)
UC referral teams	-0.080 (0.162)	-0.001 (0.205)	-0.242 (0.274)	-0.269* (0.151)	-0.215 (0.215)	-0.282 (0.226)	0.093 (0.169)	0.090 (0.211)	0.113 (0.190)
LC strangers mean	0.125	0.070	0.220	0.140	0.081	0.240	-0.294	-0.372	-0.160
R ²	0.03	0.04	0.04	0.05	0.05	0.08	0.07	0.14	0.10
Observations	349	220	129	349	220	129	349	220	129

This table reports treatment effects on beliefs about hypothetical lower caste workers, as reported in the endline survey at the lab-in-field experiment. Standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Lab-in-field experment: Willingness to connect with teammates

	Advice			Socialize			Borrowing		
	(1) All	(2) H	(3) L	(4) All	(5) H	(6) L	(7) All	(8) H	(9) L
LC referral teams	-0.000*** (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
UC referral teams	0.000 (0.000)	-0.000*** (0.000)	0.000 (0.000)	0.000* (0.000)	0.000** (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
LC strangers mean	0.647	0.674	0.604	0.333	0.388	0.245	0.432	0.407	0.472
R ²	0.99	1.00	0.97	1.00	1.00	1.00	0.99	1.00	0.97
Observations	352	221	131	352	220	132	354	221	133

This table reports treatment effects on workers' future willingness to meet and socialize with their teammates, as reported in the endline survey at the lab-in-field experiment, along the three dimensions of social interactions listed: seeking advice, socializing, and borrowing money. Standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Lab-in-field experiment: Hypothetical referral allocation after lab-in-field

	Overall			Fairness			Productivity			Retention		
	(1) LC min	(2) Equal	(3) LC maj	(4) LC min	(5) Equal	(6) LC maj	(7) LC min	(8) Equal	(9) LC maj	(10) LC min	(11) Equal	(12) LC maj
LC referral teams	-0.007 (0.056)	-0.099 (0.065)	-0.153** (0.061)	0.027 (0.059)	-0.048 (0.056)	-0.128** (0.058)	-0.018 (0.070)	0.029 (0.069)	-0.105* (0.059)	-0.013 (0.065)	-0.010 (0.057)	-0.100* (0.059)
UC referral teams	-0.241*** (0.068)	-0.034 (0.069)	0.034 (0.079)	-0.255*** (0.070)	-0.001 (0.065)	0.075 (0.075)	-0.127* (0.076)	0.081 (0.076)	0.094 (0.070)	-0.069 (0.081)	0.065 (0.077)	-0.039 (0.079)
LC strangers mean	0.677	0.711	0.675	0.592	0.711	0.702	0.492	0.586	0.719	0.492	0.656	0.754
R ²	0.07	0.04	0.08	0.09	0.02	0.06	0.07	0.03	0.04	0.03	0.02	0.03
Observations	318	309	315	318	309	315	318	309	315	318	309	315

This table reports measures of how workers at the lab-in-field experiment would allocate future referral opportunities among a hypothetical set of workers. Standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: Lab-in-field: Compensating for lack of effort

	Worked hard to compensate for others		
	(1) All	(2) High coord	(3) Low coord
LC referral teams	0.006 (0.067)	0.163** (0.068)	-0.336** (0.123)
UC referral teams	-0.073 (0.084)	0.124 (0.092)	-0.516*** (0.104)
LC strangers mean	0.268	0.155	0.513
R ²	0.02	0.06	0.21
Observations	312	215	97

This table reports responses to a question where workers are asked if they worked harder to compensate for others on their team than they would have expected. Standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

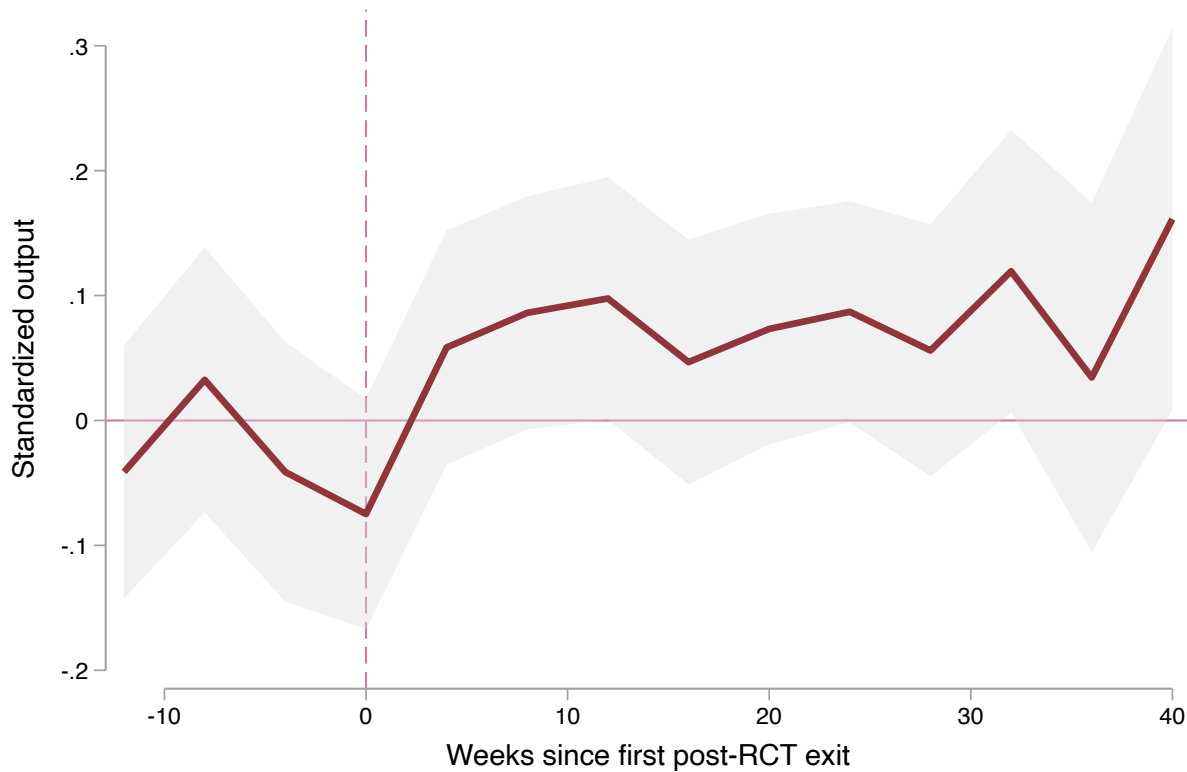
Table B.8: Lab-in-field: Reason for workshop

	Workshop conducted for research		
	(1) All	(2) High coord	(3) Low coord
LC referral teams	-0.045 (0.066)	0.093 (0.066)	-0.277** (0.126)
UC referral teams	0.119 (0.110)	-0.047 (0.109)	0.413*** (0.120)
LC strangers mean	0.344	0.267	0.471
R ²	0.03	0.07	0.28
Observations	470	294	176

This table reports responses to a question where workers are asked what they believe the purpose of the lab-in-field workshop was. Standard errors are clustered at the team level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.4 Treatment effects on firm administrative data

Figure B.16: Treatment effect on output with respect to retention (firm experiment)



This figure plots the evolution of treatment effects on team-level output at the firm as a result of the firm experiment, relative to the timing of the first exit at the firm in the experiment (or post-experiment) period. The estimates come from separate regressions for each “relative week” which is the calendar week relative to the week of the first experiment exit, with fixed effects at the randomization strata level and standard errors clustered at the team level.

Table B.9: Average tenure

	Tenure (months)	
	All workers	Current workers
Treatment	1.383*** (0.300)	1.741*** (0.404)
Control mean	5.845	5.201
R ²	0.02	0.04
Observations	1365	796

This table reports the tenure at the worker level at the firm, as of the endline. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: Heterogeneous treatment effects on productivity

	Output					
Treatment	0.079 (0.058)	0.062 (0.056)	0.129* (0.068)	0.088** (0.040)	0.144** (0.059)	0.146** (0.064)
High baseline churn	0.064 (0.059)					
Treatment \times High baseline churn	0.010 (0.081)					
High baseline LC share		-0.227 (0.323)				
Treatment \times High baseline LC share		0.046 (0.079)				
High baseline cohesion			0.062 (0.051)			
Treatment \times High baseline cohesion			-0.083 (0.091)			
High baseline conflict risk				-0.068 (0.084)		
Treatment \times High baseline conflict risk				-0.050 (0.104)		
High baseline size					0.057 (0.051)	
Treatment \times High baseline size					-0.130 (0.086)	
High baseline cohesion cost beliefs						-0.025 (0.058)
Treatment \times High baseline cohesion cost beliefs						-0.104 (0.088)
Control mean	0.003	0.003	0.003	0.003	0.003	-0.049
R ²	0.75	0.75	0.75	0.75	0.75	0.78
Observations	5715	5715	5715	5715	5715	4590

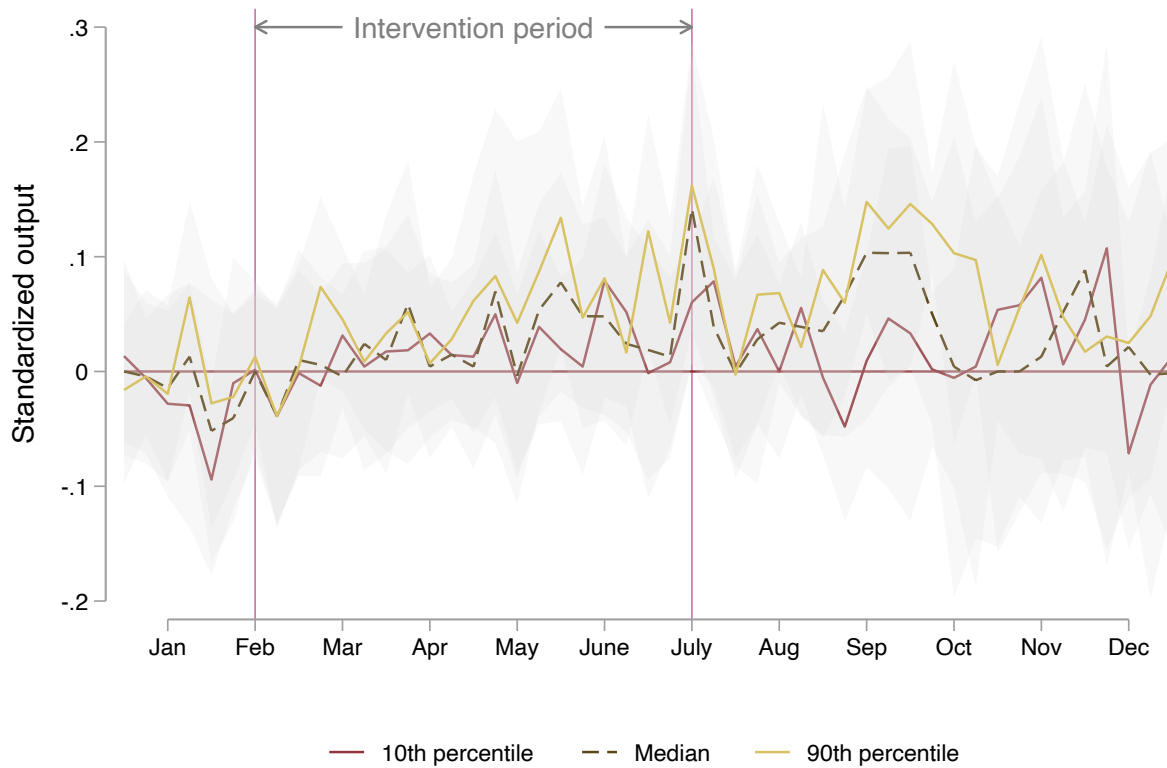
This table reports heterogeneous treatment effects for standardized productivity, with the basic specification wherein each variable that I study heterogeneity on defined as a binary for above or below median for each of the continuous variables. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata and week level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.11: Heterogeneous treatment effects on churn

	Churn					
Treatment	-0.938*** (0.166)	-0.950*** (0.159)	-0.974*** (0.175)	-1.186*** (0.127)	-1.087*** (0.161)	-1.222*** (0.195)
High baseline churn	0.267 (0.207)					
Treatment × High baseline churn	-0.425 (0.263)					
High baseline LC share		1.394 (1.102)				
Treatment × High baseline LC share		-0.408 (0.250)				
High baseline cohesion			0.092 (0.212)			
Treatment × High baseline cohesion			-0.378 (0.274)			
High baseline conflict risk				-0.855** (0.363)		
Treatment × High baseline conflict risk				0.918 (0.741)		
High baseline size					0.347 (0.220)	
Treatment × High baseline size					-0.170 (0.258)	
High baseline cohesion cost beliefs						-0.001 (0.243)
Treatment × High baseline cohesion cost beliefs						0.065 (0.292)
Control mean	2.785	2.785	2.785	2.785	2.785	2.798
R ²	0.16	0.16	0.16	0.16	0.16	0.16
Observations	889	889	889	889	889	714

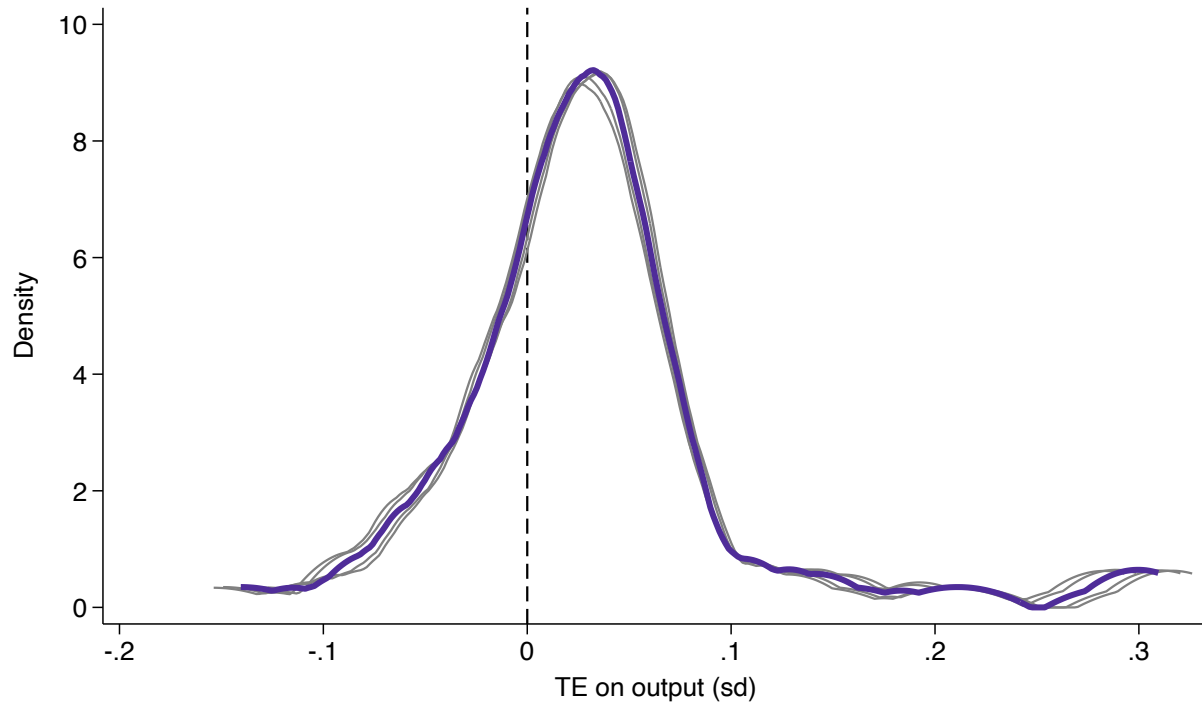
This table reports heterogeneous treatment effects for the sum of exits and entries in each team per month, with the basic specification wherein each variable that I study heterogeneity on defined as a binary for above or below median for each of the continuous variables. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata and month level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B.17: Quantile regressions on output



This figure shows event-time quantiles of standardized team output over the calendar year. Lines trace the 10th, 50th, and 90th percentiles; the shaded band marks the intervention period. Output is standardized by task using pre-period means and standard deviations. Quantiles are computed pooling teams within week. The pattern indicates broad-based gains rather than effects concentrated only in the upper tail, and the absence of negative effects among even the lower tail.

Figure B.18: Distribution of pooled output TEs



This figure plots kernel densities of estimated treatment effects on standardized team output across the main and robustness specifications. Each specification contributes one point estimate per sample; the vertical line marks zero. The distribution is centered to the right of zero, consistent with positive average effects on productivity. Output is standardized by task using pre-period moments.

Table B.12: Persistence in referral allocations

	Team LC share		Referral LC share		$\mathbb{I}\{\text{LC referrals} > \text{team LC share}\}$	
	During expt	Post expt	During expt	Post expt	During expt	Post expt
Treatment	0.154*** (0.026)	0.357*** (0.067)	0.583*** (0.034)	0.497*** (0.038)	0.522*** (0.041)	0.221*** (0.063)
Control mean	0.245	0.153	0.217	0.153	0.255	0.352
R ²	0.33	0.20	0.48	0.39	0.39	0.11
Observations	920	654	542	367	542	367
# teams	132	132	131	129	131	129
# months	7	5	7	5	7	5

This table reports indicators on referral allocations in teams during and post the experimental period. In columns 1 and 2, the outcome is the share of the team that is LC. In columns 3 and 4, the outcome variable is the share of the referrals given to LC incumbents. In columns 5 and 6, the outcome is a binary indicator for whether the referral allocations to LCs in a given month in a team was greater than its share of LCs. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata and month level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B.5 Treatment effects on endline survey at firm

Table B.13: Firm endline: team cohesion HTEs

	Team cohesion (0-10)		Friends outside		Friends at work		Bargaining power	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.130 (0.125)	-0.052 (0.145)	-0.037 (0.443)	-0.178 (0.536)	-0.236 (0.245)	-0.171 (0.296)	-0.044 (0.045)	-0.045 (0.056)
Lower caste	0.722*** (0.128)	0.501*** (0.191)	-0.799* (0.459)	-0.976* (0.539)	-0.850*** (0.313)	-0.580 (0.485)	-0.044 (0.064)	0.073 (0.104)
More diverse at baseline		0.123 (0.187)		-0.020 (0.569)		0.102 (0.453)		0.044 (0.062)
Treatment × Lower caste	0.144 (0.186)	0.273 (0.238)	1.357* (0.780)	2.123* (1.099)	0.973** (0.463)	1.279* (0.694)	0.060 (0.078)	-0.028 (0.118)
Treatment × More diverse at baseline		-0.205 (0.292)		0.344 (0.941)		-0.150 (0.585)		0.003 (0.099)
Lower caste × More diverse at baseline		0.345 (0.262)		0.288 (0.854)		-0.515 (0.633)		-0.205 (0.128)
Triple interaction		-0.146 (0.398)		-1.405 (1.589)		-0.486 (0.953)		0.145 (0.157)
Control mean	8.309	8.309	3.505	3.505	3.656	3.656	0.679	0.679
R ²	0.09	0.10	0.02	0.02	0.03	0.04	0.01	0.01
Observations	771	771	771	771	771	771	774	774

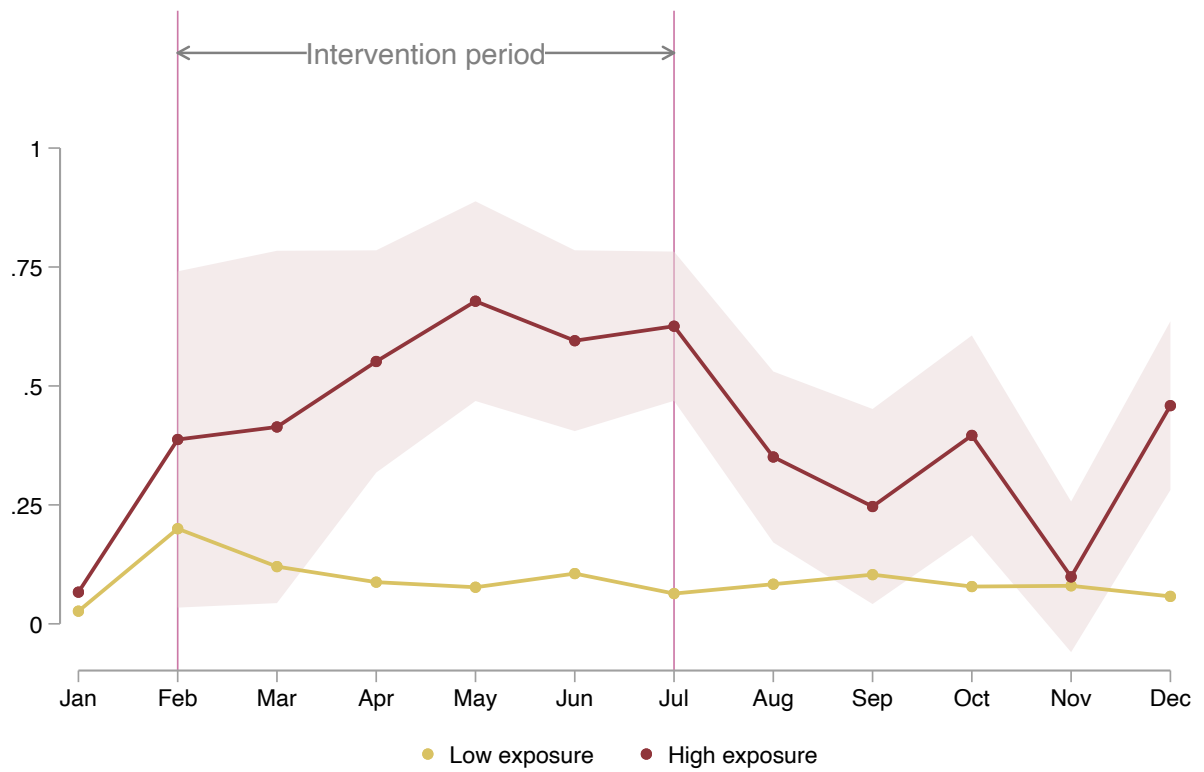
This table reports heterogeneous treatment effects on self reported cohesion, with respect to initial team diversity and the caste group of the respondents. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.14: Firm endline: beliefs

	Good at job		Hardworking		Good as team member	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.195 (0.175)	0.133 (0.234)	0.053 (0.191)	-0.154 (0.237)	0.227 (0.166)	0.444* (0.229)
Treatment \times Lower caste		0.219 (0.356)		0.579 (0.596)		-0.617 (0.376)
Control mean	-0.022	-0.022	-0.045	-0.045	-0.080	-0.080
R ²	0.09	0.09	0.07	0.08	0.10	0.11
Observations	208	208	204	204	205	205

This table reports treatment effects on beliefs about out-groups, as reported in the endline survey at the firm. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B.19: Referral allocation in control teams by LC exposure



This figure plots the evolution of referral allocations within the control group of teams, with each line plot depicting the subset of control teams with randomly induced above or below median exposure to lower caste referral candidates.

Table B.15: Treatment effect on cohesion (firm experiment)

	Team cohesion (0–10)				Friends outside				Friends at work				Bargaining power			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Raw	Composition-adjusted	UC	LC	Raw	Composition-adjusted	UC	LC	Raw	Composition-adjusted	UC	LC	Raw	Composition-adjusted	UC	LC
Treatment	0.161* (0.088)	-0.077 (0.086)	-0.130 (0.125)	0.014 (0.123)	0.412 (0.345)	0.458 (0.345)	-0.037 (0.443)	1.320** (0.609)	0.002 (0.200)	0.119 (0.189)	-0.236 (0.245)	0.738** (0.359)	-0.026 (0.034)	-0.022 (0.036)	-0.044 (0.045)	0.016 (0.062)
Control mean	8.309	.	8.156	8.885	3.505	.	3.644	2.974	3.656	.	3.827	3.000	0.679	.	0.689	0.641
Control LC share	0.210	0.210	0.210	0.210	0.207	0.207	0.207	0.207	0.207	0.207	0.207	0.207	0.209	0.209	0.209	0.209
Treatment LC share	0.511	0.511	0.511	0.511	0.511	0.511	0.511	0.511	0.511	0.511	0.511	0.511	0.512	0.512	0.512	0.512
UC vs. LC p-value	.	0.441	.	.	.	0.084	.	.	.	0.037	.	.	.	0.442	.	.
R ²	0.02	0.09	0.09	0.09	0.02	0.02	0.02	0.02	0.02	0.03	0.03	0.03	0.01	0.01	0.01	0.01
Observations	771	771	771	771	771	771	771	771	771	771	771	771	774	774	774	774

This table reports treatment effects on self-reported measures of team cohesion at the firm, as of the endline survey with workers. In columns 1-4, I report treatment effects on workers' ratings between 0 and 10 on how cohesive their team is. In columns 5-8, I report treatment effects on the number of friends they have at work with whom they would spend time outside of work. In columns 9-12, I report treatment effects on the total number of friends they have at work. In columns 13-16, I report treatment effects on whether workers believe that they have the ability to collectively bargain against the employer if a hypothetical conflict should arise. For each outcome, the first column reports treatment effects on the raw outcome. The second column reports the composition-adjusted treatment effect to account for level differences between LCs and UCs. The third and fourth columns report treatment effects on UCs and LCs respectively. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.16: Treatment effect on job satisfaction (firm experiment)

	Value referral		Job satisfaction rating (1 - 10)	
	(1)	(2)	(3)	(4)
Treatment	0.003 (0.035)	-0.035 (0.043)	0.101 (0.099)	0.093 (0.143)
Treatment × Lower caste		0.143* (0.077)		0.074 (0.257)
Control mean	0.738	0.738	8.298	8.298
R ²	0.01	0.02	0.01	0.01
Observations	774	774	773	773

This table reports treatment effects on how workers value referrals and how satisfied they are at the firm. In columns 1-2, I report treatment effects on an indicator that captures whether workers value the ability to make referrals more than they would like a hypothetical 15 minute extension to their lunch break. In columns 3-4, I report treatment effects on a self-reported rating for their job satisfaction, ranging between 0 and 10. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.17: Treatment effect on exit beliefs (firm experiment)

	Expect to leave firm		Ln(Expected earnings in 2 years)		# job offers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.009 (0.021)	-0.013 (0.029)	0.128 (0.081)	0.143 (0.118)	0.232 (0.408)	0.538 (0.421)
Treatment \times Lower caste		0.006 (0.050)		0.043 (0.190)		-0.603 (0.654)
Control mean	0.888	0.888	9.863	9.863	1.043	1.043
R ²	0.01	0.01	0.06	0.06	0.05	0.05
Observations	774	774	302	302	302	302

This table reports treatment effects on self-reported measures of their expectations around exiting the firm and their outside options. In columns 1-2, I report treatment effects on whether workers expect to leave the firm in the next five years. In columns 3-4, I report treatment effects on the logged expected earnings in two years. In columns 5-6, I report treatment effects on the number of job offers the workers heard in the previous month. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.18: Supervisors' time use

	Monitoring	Training	Recruitment	Admin work	Troubleshooting	Disciplinary	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.006 (0.410)	-0.853*** (0.179)	-0.159 (0.230)	0.356* (0.201)	-0.030 (0.752)	0.166 (0.114)	-0.420 (0.332)
Control mean	23.937	3.587	2.317	1.857	13.841	0.429	1.333
R ²	0.12	0.28	0.11	0.11	0.08	0.19	0.11
Observations	127	127	127	127	127	127	127

This table reports treatment effects on supervisors' self reported time use, as of the endline survey. Each column reports the number of hours in the previous week that supervisors spent on each of the listed tasks. All regressions include fixed effects at the randomization strata level, and robust standard errors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.19: Candidate characteristics by referrers' ex-ante likelihood of receiving referral opportunities

	Relationship	Labor market indicator			Education
	Family	In factory work	In urban work	Unemployed	Illiterate
Referrer likelihood	-0.352** (0.165)	0.016 (0.167)	0.064 (0.185)	-0.221* (0.129)	-0.000 (0.029)
Lower caste	-0.070 (0.053)	-0.107* (0.060)	-0.061 (0.066)	-0.017 (0.043)	-0.002 (0.010)
Mean for upper caste	0.811	0.347	0.420	0.104	0.010
R ²	0.01	0.01	0.01	0.01	0.00
Observations	1027	1027	1027	1027	1027

This table compares characteristics of potential referral candidates listed at baseline by incumbents predicted (via an MLE classifier fit to status-quo allocations) to be high- vs. low-likelihood referrers. Each column reports OLS estimates for an indicator outcome—family tie; in factory work; in urban work; unemployed; illiterate—on a continuous measure for the predicted probability that the referrer is a high-likelihood referrer, and a lower-caste referrer indicator. Task fixed effects included; robust standard errors clustered at the referrer level. The bottom row reports the mean among candidates listed by upper-caste incumbents. ** $p < 0.05$, *** $p < 0.01$

Table B.20: Heterogeneity in team-level treatment effects by ex-ante referral-likelihood environment

	Team composition		Retention		Productivity
	Size	LC share	Exits	Entries	(sd)
Treatment	-0.386 (0.489)	0.137*** (0.042)	-0.381*** (0.115)	-0.457*** (0.127)	0.065 (0.054)
High likelihood referral	-0.055 (0.466)	-0.046 (0.038)	0.145 (0.124)	0.175 (0.153)	-0.043 (0.055)
Interaction	0.581 (0.605)	0.028 (0.073)	-0.106 (0.189)	0.012 (0.171)	-0.032 (0.075)
Mean for control low likelihood referral teams	5.653	0.313	1.367	1.143	0.024
R ²	0.11	0.35	0.25	0.24	0.77
Observations	882	882	882	882	5670

Each column reports estimates from team-panel regressions of the listed outcome on (i) Treatment, (ii) High likelihood referral (indicator for teams above the median predicted probability that their incumbents receive referral opportunities under the status quo), and (iii) Treatment \times High. Outcomes include team size and LC share (monthly), exits and entries (monthly), and standardized team output (weekly). All models include randomization-strata and time fixed effects; standard errors are clustered at the team level. “Means” reports the pre-period mean for low-likelihood control teams. Treatment effects are similar across high- and low-likelihood environments, supporting the validity of the control counterfactual. ** $p < 0.05$, *** $p < 0.01$

B.6 Treatment effects on referral candidate follow-ups

Table B.21: Referral candidates' job arrival rate

	# job offers in last 6 months		Pr(any job offer from a factory)	
	(1)	(2)	(3)	(4)
	Upper caste	Lower caste	Upper caste	Lower caste
Treatment	-0.174 (0.157)	0.418** (0.202)	-0.048 (0.051)	0.110 (0.073)
UC vs. LC p-value		0.019		0.078
Control mean	1.349	0.520	0.502	0.420
Specification	Poisson	Poisson	OLS	OLS
R ²			0.00	0.01
Observations	418	200	418	200

This table reports treatment effects on the labor market activity among referral candidates, i.e. connections of the incumbent workers, as reported in a phone-based follow-up survey. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.22: Referral candidates' earnings

	Pr(earns anything)		Ln(1 + earnings)	
	(1)	(2)	(3)	(4)
	Upper caste	Lower caste	Upper caste	Lower caste
Treatment	-0.010 (0.027)	0.070* (0.041)	-0.037 (0.254)	0.288 (0.374)
UC vs. LC p-value		0.111		0.475
Control mean	0.919	0.870	8.447	8.021
R ²	0.00	0.01	0.00	0.00
Observations	418	200	418	200

This table reports treatment effects on the earnings and unemployment spells, i.e. connections of the incumbent workers, as reported in a phone-based follow-up survey. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.23: Referral candidates' migration activity

	Strongly willing to migrate		Max search distance (km)	
	(1)	(2)	(3)	(4)
	Upper caste	Lower caste	Upper caste	Lower caste
Treatment	0.005 (0.044)	-0.000 (0.056)	22.656 (136.315)	-21.590 (64.378)
UC vs. LC p-value		0.947		0.768
Control mean	0.732	0.820	791.124	731.140
R ²	0.00	-0.00	0.00	0.00
Observations	418	200	418	200

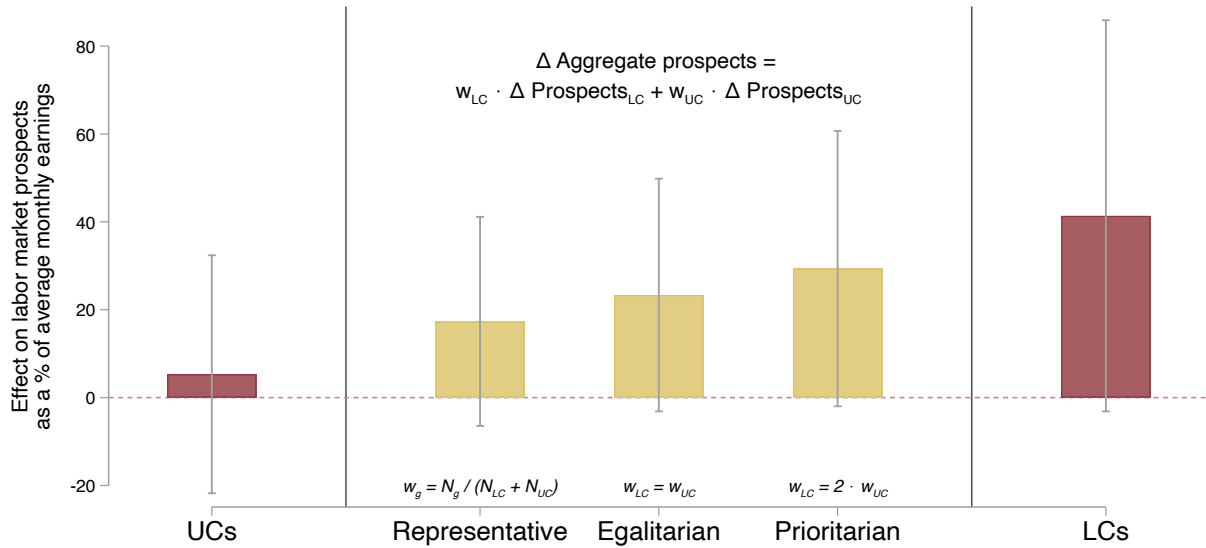
This table reports treatment effects on migration willingness and job search activity, i.e. connections of the incumbent workers, as reported in a phone-based follow-up survey. All standard errors are clustered at the team level, and the regressions include fixed effects at the randomization strata level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.24: Lee bounds

Outcome	Category	OLS	Exact Lee Bounds	Generalized Lee Bounds	
				Median	Bootstrapped p25–p75
Job offers in the last 6 months	UC	-0.22 (0.20)	[-0.71, -0.29]	[-0.42, -0.21]	[-0.72, -0.03]
Job offers in the last 6 months	LC	0.27* (0.14)	[0.40, 0.55]	[0.46, 0.47]	[0.26, 0.61]
Job offers from a factory	UC	-0.05 (0.05)	[-0.06, 0.01]	[-0.02, 0.04]	[-0.10, 0.08]
Job offers from a factory	LC	0.11 (0.07)	[0.04, 0.14]	[0.06, 0.10]	[-0.01, 0.17]
Pr(in paid work)	UC	0.03 (0.04)	[0.05, 0.12]	[0.06, 0.09]	[0.02, 0.11]
Pr(in paid work)	LC	0.12* (0.06)	[0.12, 0.22]	[0.21, 0.22]	[0.11, 0.26]
Most recent unemployment spell	UC	-1.56 (3.90)	[-9.27, 0.13]	[-3.76, 1.12]	[-7.01, 4.72]
Most recent unemployment spell	LC	-11.02* (6.60)	[-19.58, 6.26]	[-14.29, -13.18]	[-21.42, -3.85]

This table reports treatment effects on referral candidates' labor-market outcomes, separately for upper caste (UC) and lower caste (LC) candidates. The column indicated as OLS shows intent-to-treat coefficients from regressions of each outcome on treatment, with randomization-strata fixed effects; heteroskedasticity-robust standard errors are in parentheses. The column indicated as Exact Lee Bounds report sharp bounds that adjust for differential attrition using the [Lee \(2009\)](#) trimming procedure; trimming is performed within stratum \times caste category by the excess nonresponse in the arm with higher missingness. The columns indicated as Generalized Lee Bounds implement the generalized Lee-bounds procedure following [Semenova \(2025\)](#), using machine-learning-based estimation of selection propensities with cross-fitting. I report separately the median and the more conservative bootstrapped estimate for the bounds, with the latter defined as the 25th percentile for the lower bound and the 75th percentile of the upper bound over $N = 500$ bootstrapped iterations. Binary outcomes are in percentage points; durations are in days. Bounds shown in blue are intervals that do not include 0. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B.20: Aggregate change in labor market prospects



Bars show the treatment effect on the prospects index for UC and LC referral candidates, and the implied aggregate change under three planner-weight schemes: **Representative** ($w_g \propto N_g$), **Egalitarian** ($w_{LC} = w_{UC}$), and **Prioritarian** ($w_{LC} = 2 w_{UC}$) (weights normalized to sum to one). Prospects is defined as $\Delta \text{Prospects}_g = 0.30 \Delta p_g^{\text{salaried}} + 0.04 \Delta q_g^{\text{offers}} - 0.03 \Delta d_g^{\text{unemployed}}$, where p^{salaried} is salaried employment, q^{offers} counts job offers in the past six months, and $d^{\text{unemployed}}$ is days in the most recent spell; coefficients are calibrated from endline controls (salaried–casual premium $\approx 30\%$, option value per offer $\approx 4\%$, daily loss from unemployment $\approx 1/30$). Aggregate effects are computed as $w_{LC} \Delta \text{Prospects}_{LC} + w_{UC} \Delta \text{Prospects}_{UC}$. Effects are reported as a percent of average monthly earnings; vertical lines denote 95% confidence intervals (standard errors clustered at the referrer level).

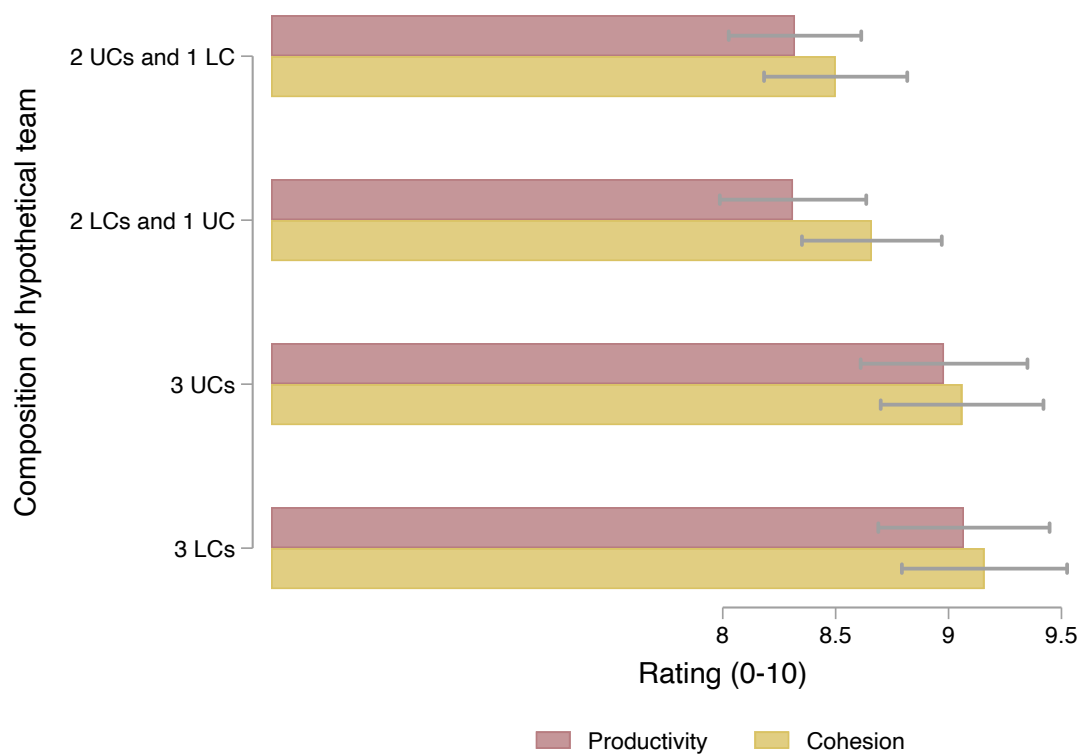
B.7 Vignettes survey

Table B.25: Vignettes survey: changes in cohesion

		Change in expected cohesion after recruitment					Obs	
Recruitment type ↓→	Caste label ↓→	Mean	Pairwise test [row - column]					
		UC stranger	UC referral	LC stranger	LC referral	LC stranger		
		(1)	No	No	No	No	Yes	(7)
UC stranger	No	0.76	-					93
UC referral	No	0.93	0.16** (0.06)	-				107
LC stranger	No	0.76	-0.02 (0.05)	-0.20*** (0.05)	-			112
LC referral	No	0.90	0.14** (0.06)	-0.04 (0.03)	0.14** (0.05)	-		88
LC stranger	Yes	0.61	-0.29*** (0.08)	-0.28*** (0.06)	-0.04 (0.04)	-0.23*** (0.07)	-	95
LC referral	Yes	0.82	0.11* (0.06)	-0.13** (0.05)	0.10 (0.06)	-0.02 (0.04)	0.18** (0.08)	105

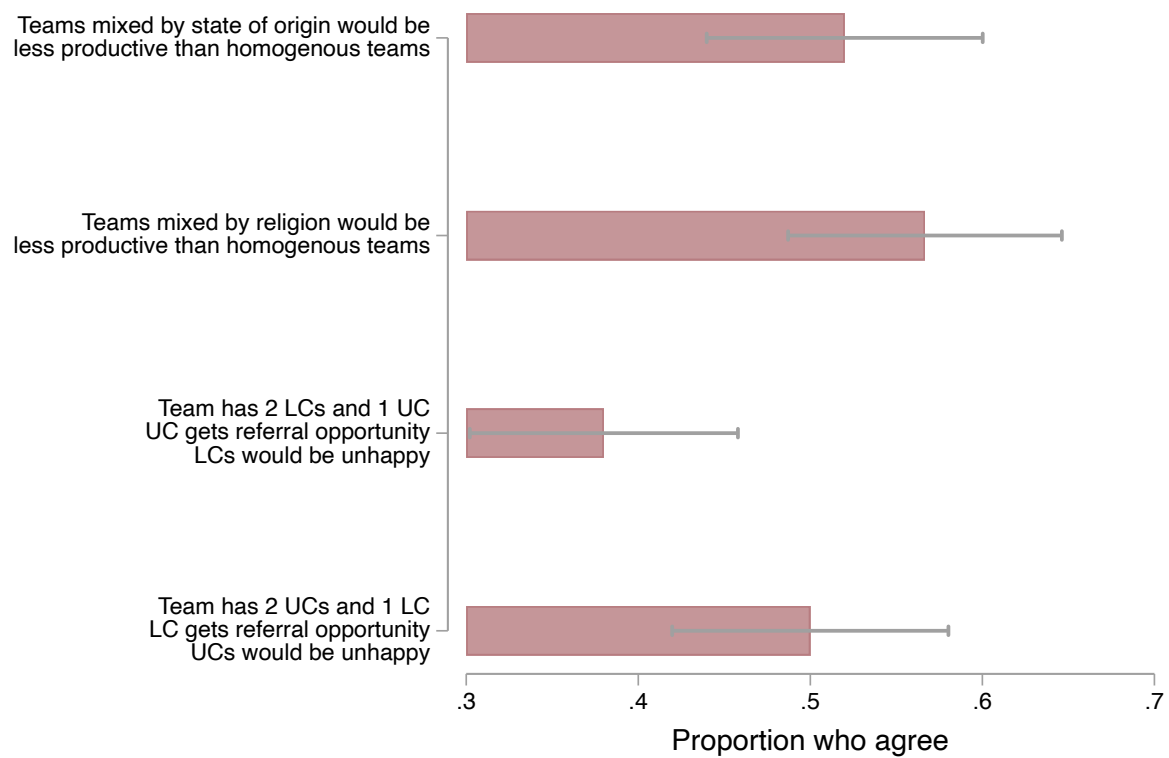
This table reports average reported responses for the change in expected team cohesion as a hypothetical team grows from three to four through each reported mode of recruitment. The modes are listed in the labels of rows and columns, with each cell of the matrix in columns 2-6 reporting the comparison of the columns between the mode listed in the row and the mode listed in the column. Column 1 reports the mean change in team cohesion for each listed mode of recruitment as reported in the row labels. Column 7 reports the number of respondents who faced a question on the recruitment type listed in the row labels. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure B.21: Vignettes survey: expectations of productivity and cohesion vs. team composition



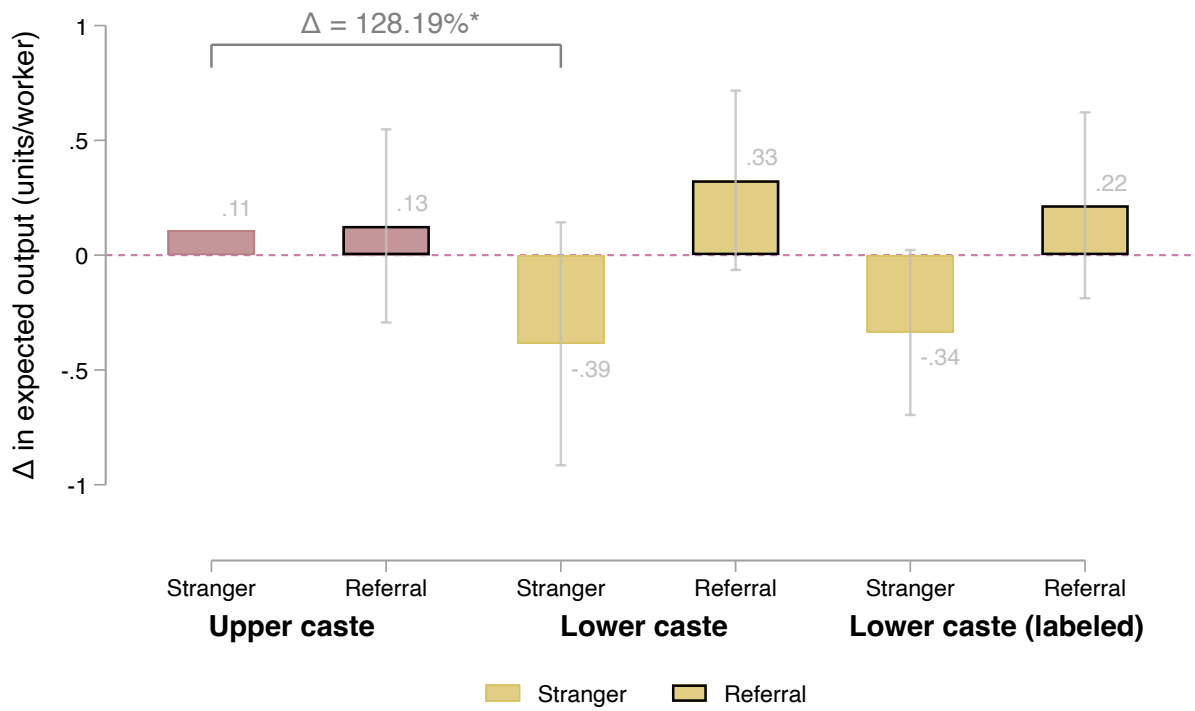
This figure summarizes responses from the vignettes survey on the expected productivity of hypothetical teams based on their composition.

Figure B.22: Vignettes survey: policy perceptions



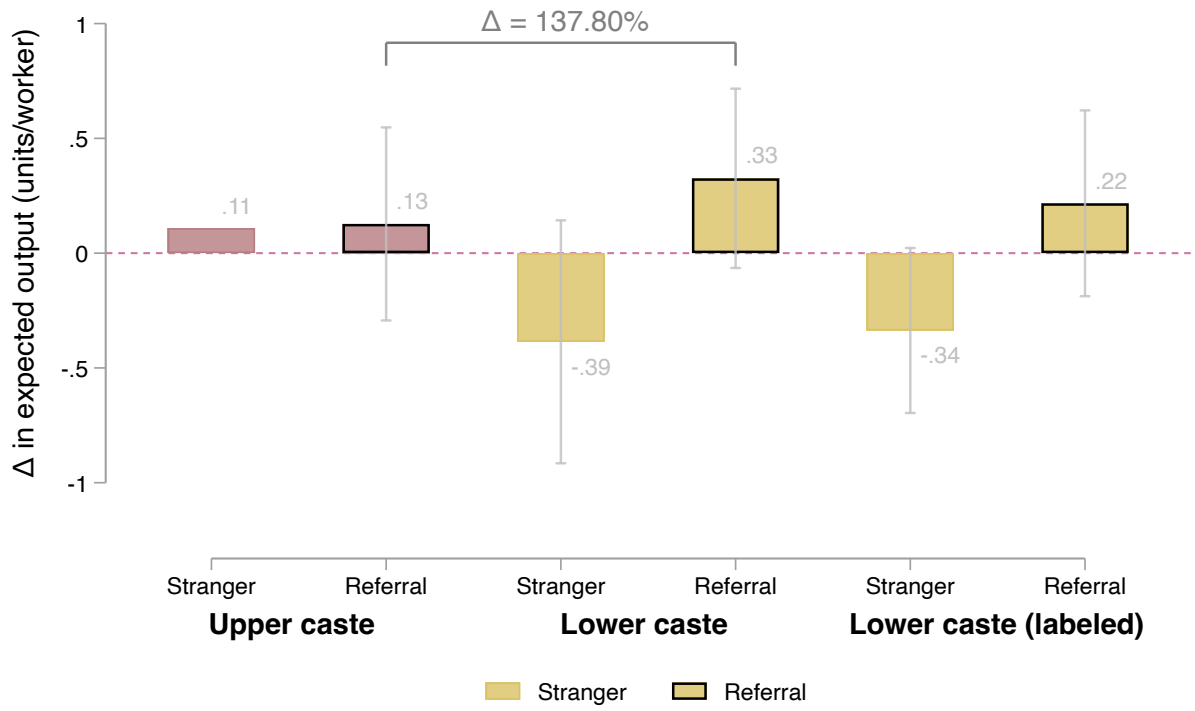
This figure summarizes responses from the vignettes survey on agreement rates to a range of statements concerning team diversity, cohesion and productivity.

Figure B.23: Vignettes survey



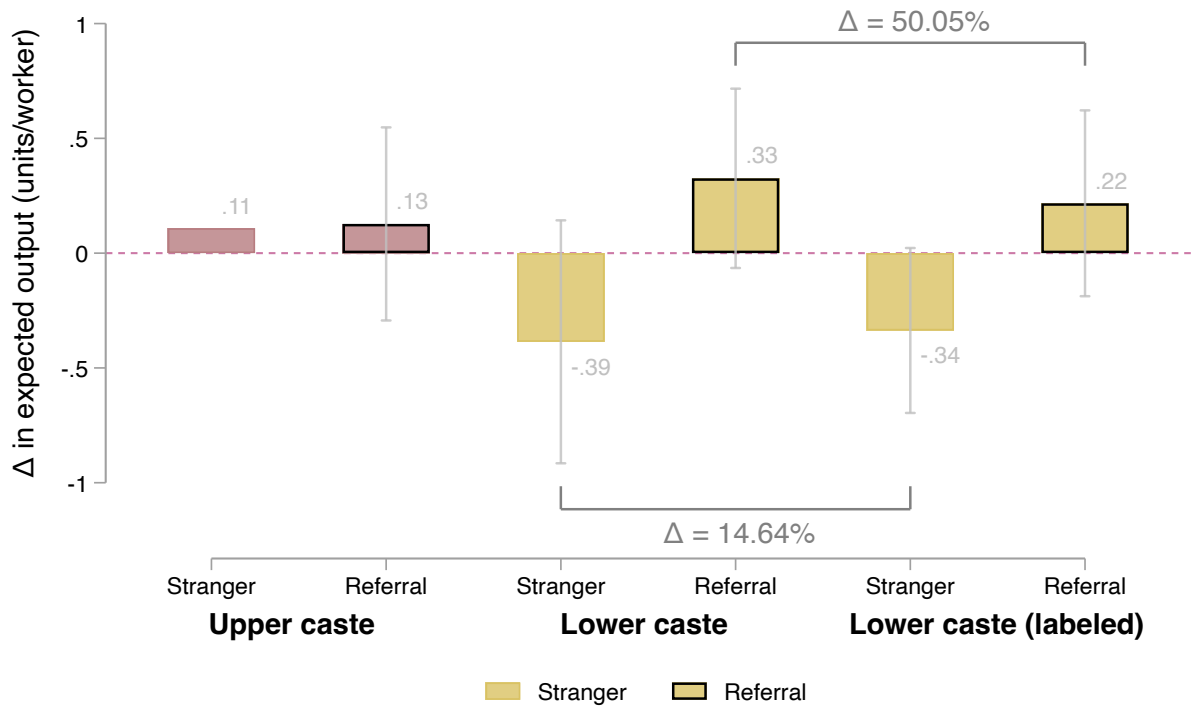
This figure plots treatment effects from the vignettes survey, focusing on pairwise comparisons. The point estimates correspond to the figures in Table 8.

Figure B.24: Vignettes survey



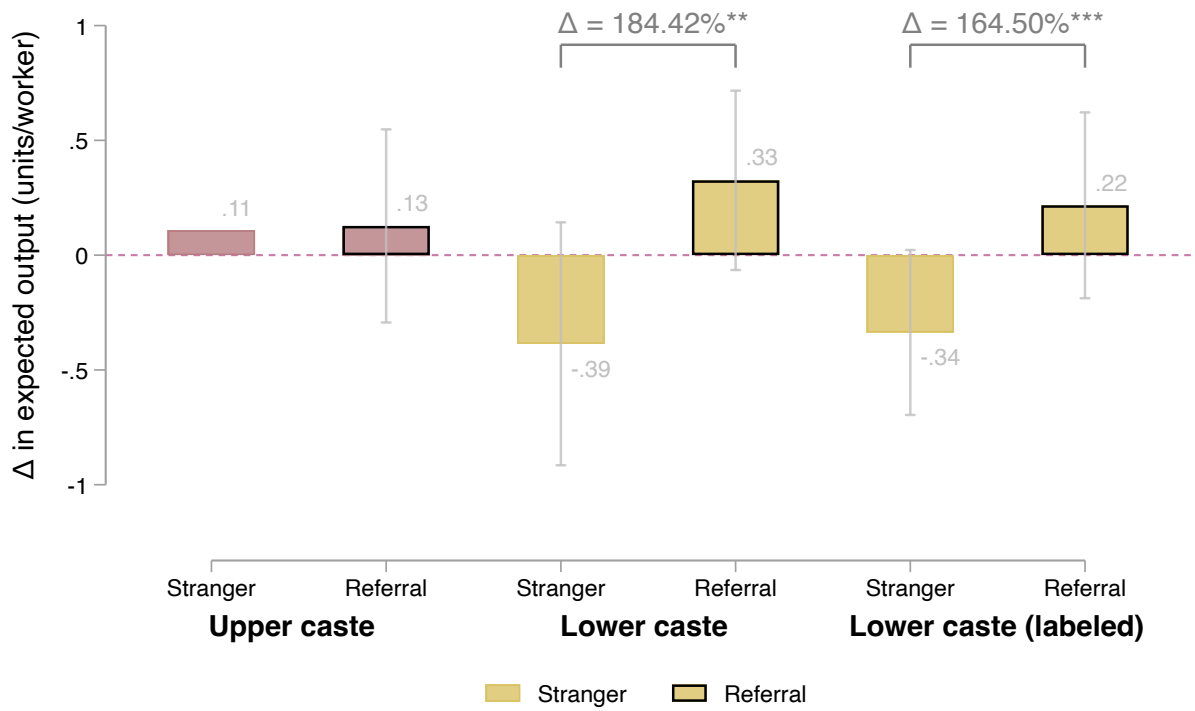
This figure plots treatment effects from the vignettes survey, focusing on pairwise comparisons. The point estimates correspond to the figures in Table 8.

Figure B.25: Vignettes survey



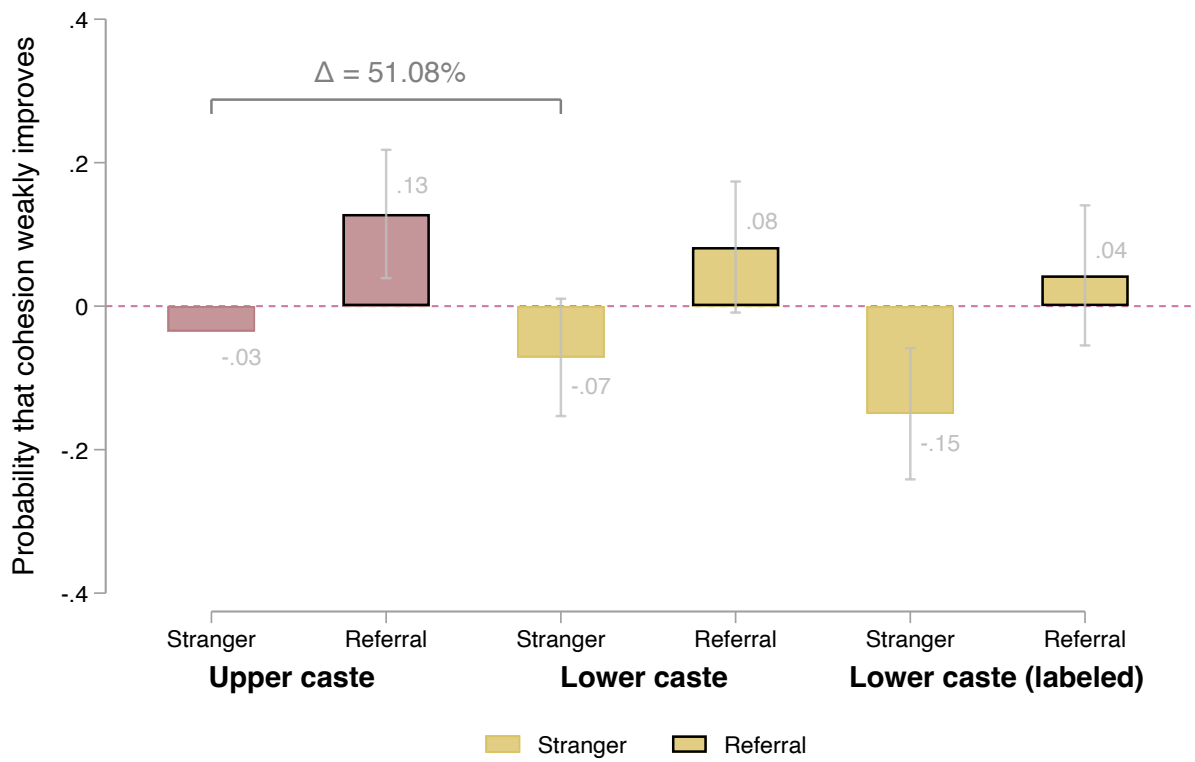
This figure plots treatment effects from the vignettes survey, focusing on pairwise comparisons. The point estimates correspond to the figures in Table 8.

Figure B.26: Vignettes survey



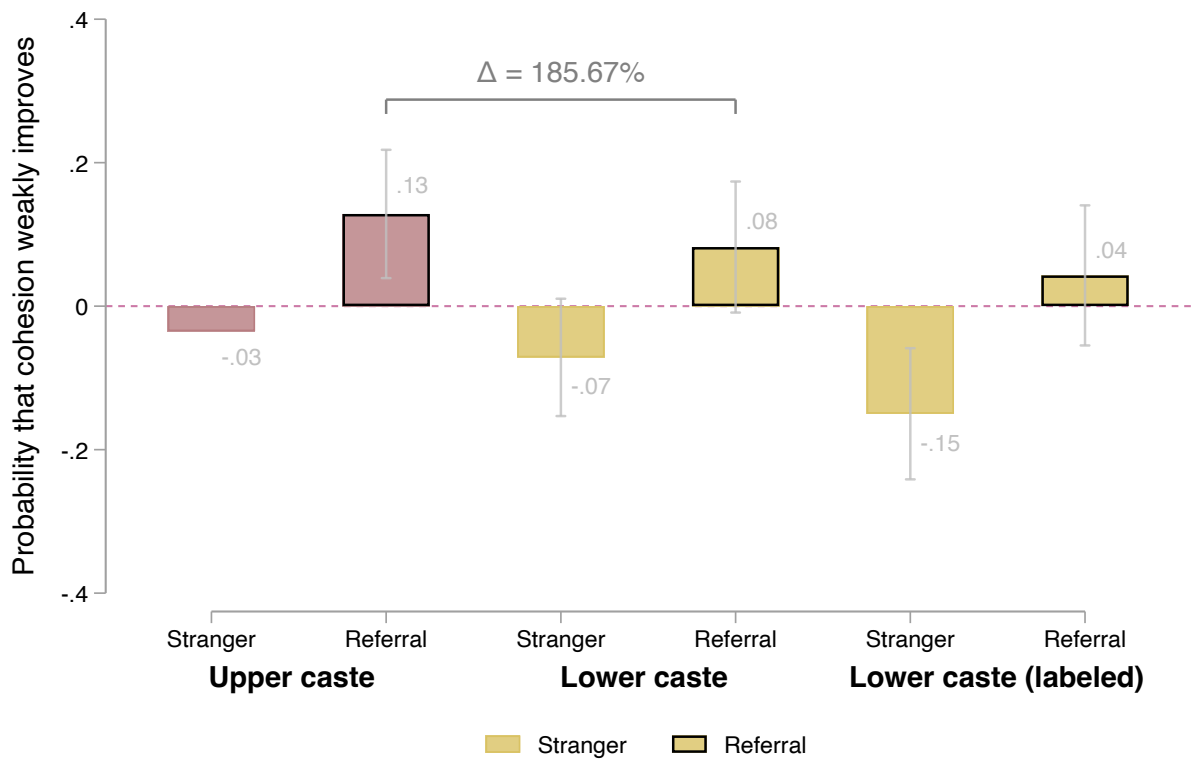
This figure plots treatment effects from the vignettes survey, focusing on pairwise comparisons. The point estimates correspond to the figures in Table 8.

Figure B.27: Vignettes survey



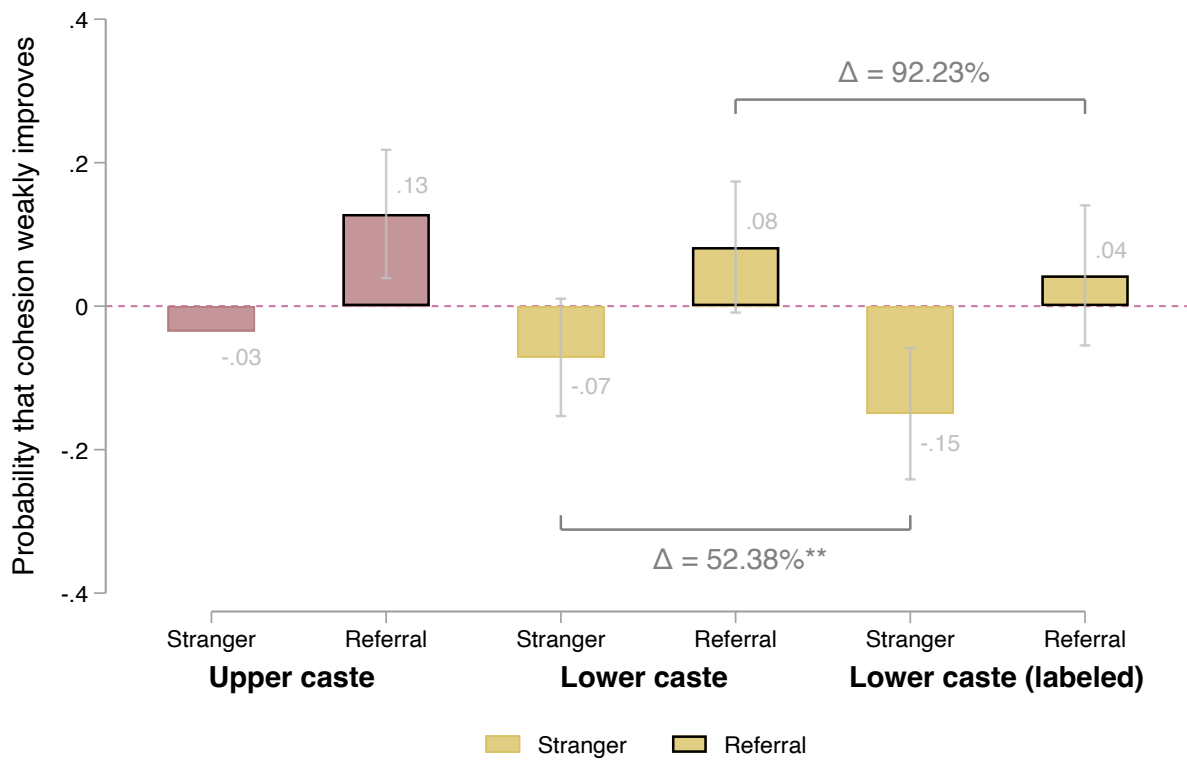
This figure plots treatment effects from the vignettes survey, focusing on pairwise comparisons. The point estimates correspond to the figures in Table B.25.

Figure B.28: Vignettes survey



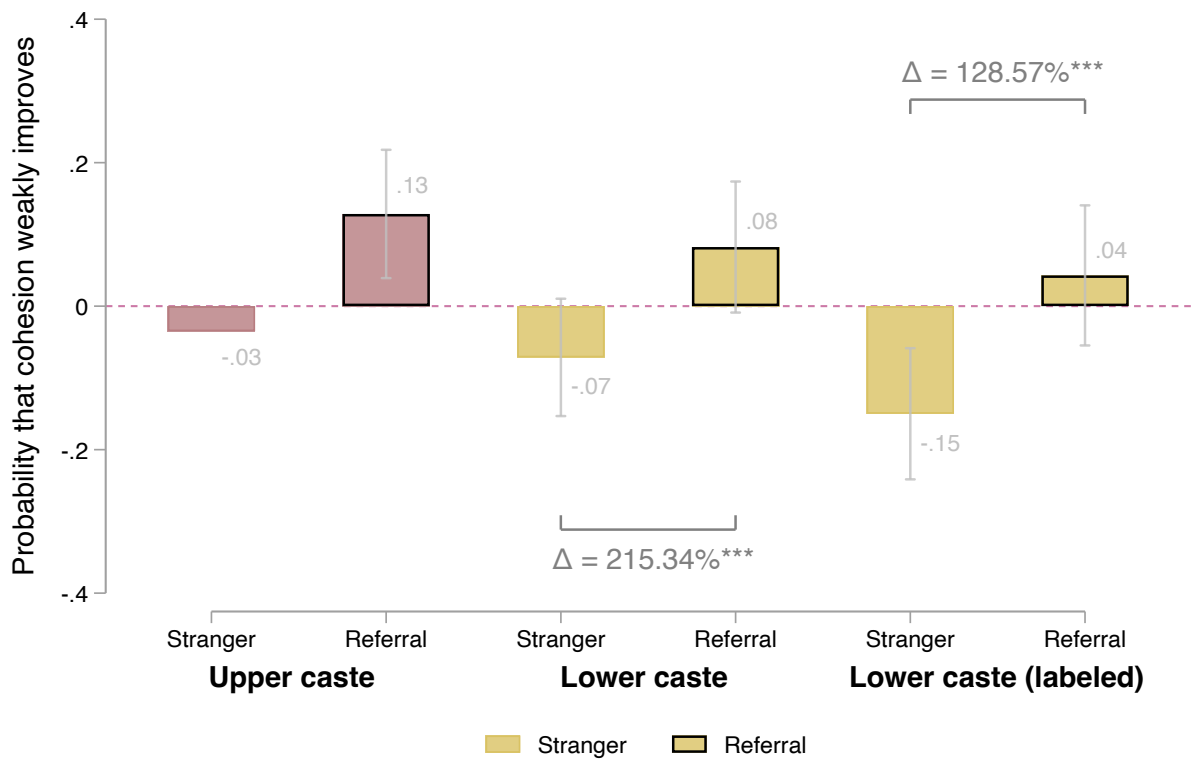
This figure plots treatment effects from the vignettes survey, focusing on pairwise comparisons. The point estimates correspond to the figures in Table B.25.

Figure B.29: Vignettes survey



This figure plots treatment effects from the vignettes survey, focusing on pairwise comparisons. The point estimates correspond to the figures in Table B.25.

Figure B.30: Vignettes survey



This figure plots treatment effects from the vignettes survey, focusing on pairwise comparisons. The point estimates correspond to the figures in Table B.25.

Table B.26: Effect on expected output (robustness)

		Change in expected output per worker after recruitment					Obs	
Recruitment type ↓→	Caste label ↓→	Mean	Pairwise test [row - column]					
			UC stranger	UC referral	LC stranger	LC referral	LC stranger	
		(1)	No (2)	No (3)	No (4)	No (5)	Yes (6)	
UC stranger	No	-0.32	-					38
UC referral	No	0.44	0.76* (0.45)	-				62
LC stranger	No	-0.31	0.00 (0.58)	-0.75 (0.51)	-			55
LC referral	No	0.58	0.89* (0.51)	0.14 (0.43)	0.89 (0.57)	-		45
LC stranger	Yes	0.17	0.48 (0.48)	-0.27 (0.39)	0.48 (0.54)	-0.41 (0.46)	-	57
LC referral	Yes	0.15	0.46 (0.46)	-0.29 (0.37)	0.46 (0.52)	-0.43 (0.44)	-0.02 (0.40)	43

This table reports average reported responses for the change in expected team cohesion as a hypothetical team grows from three to four through each reported mode of recruitment. The modes are listed in the labels of rows and columns, with each cell of the matrix in columns 2-6 reporting the comparison of the columns between the mode listed in the row and the mode listed in the column. Column 1 reports the mean change in team cohesion for each listed mode of recruitment as reported in the row labels. Column 7 reports the number of respondents who faced a question on the recruitment type listed in the row labels. This table only keeps the first policy that is offered to the respondent, as a robustness check to the main specification reported in Table 8. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.27: Effect on expected cohesion (robustness)

		Change in expected cohesion after recruitment						Obs
Recruitment type ↓→	Caste label ↓→	Mean	Pairwise test [row - column]					
			UC stranger	UC referral	LC stranger	LC referral	LC stranger	
		(1)	No (2)	No (3)	No (4)	No (5)	Yes (6)	(7)
UC stranger	No	0.74	-					38
UC referral	No	0.92	0.18** (0.08)	-				62
LC stranger	No	0.76	0.03 (0.09)	-0.16** (0.07)	-			55
LC referral	No	0.91	0.17** (0.08)	-0.01 (0.06)	0.15** (0.07)	-		45
LC stranger	Yes	0.72	-0.02 (0.09)	-0.20*** (0.07)	-0.04 (0.08)	-0.19** (0.07)	-	57
LC referral	Yes	0.86	0.12 (0.09)	-0.06 (0.06)	0.10 (0.08)	-0.05 (0.07)	0.14* (0.08)	43

This table reports average reported responses for the change in expected team cohesion as a hypothetical team grows from three to four through each reported mode of recruitment. The modes are listed in the labels of rows and columns, with each cell of the matrix in columns 2-6 reporting the comparison of the columns between the mode listed in the row and the mode listed in the column. Column 1 reports the mean change in team cohesion for each listed mode of recruitment as reported in the row labels. Column 7 reports the number of respondents who faced a question on the recruitment type listed in the row labels. This table only keeps the first policy that is offered to the respondent, as a robustness check to the main specification reported in Table B.25. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C Robustness tests

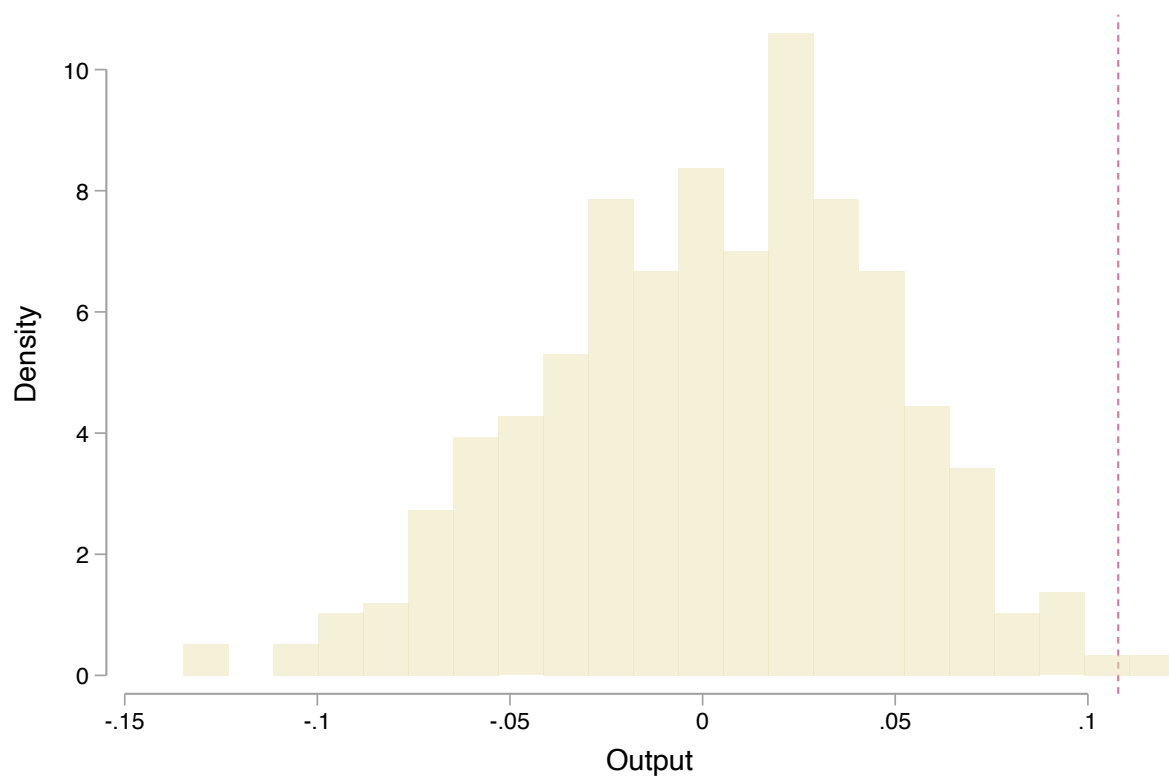
C.1 Randomization inference

Table C.1: Summary of randomization inference p-values

Variable	Standard p-value (1)	Randomization inference p-value (2)	Table in main text (3)
Output (pooled)	0.011	0.060	Table 3, Column 1
Output (high coordination)	0.004	0.076	Table 3, Column 3
Output (low coordination)	0.239	0.074	Table 3, Column 5
Team size	0.136	0.082	
Incumbent share	0.156	0.072	
LC share	0.000	0.066	
Total exits	0.000	0.054	Table 4, Column 1
Incumbent exits	0.198	0.060	Table 4, Column 2
New hire exits	0.000	0.076	Table 4, Column 3
LC exits	0.060	0.060	Table 4, Column 4
Total entries	0.000	0.074	Table 4, Column 5
LC entries	0.000	0.056	Table 4, Column 6
Total churn	0.000	0.066	Table 4, Column 7
Referrals to LCs	0.000	0.080	Table 2, Column 1
Referrals to LCs / Total referrals	0.000	0.072	Table 2, Column 2

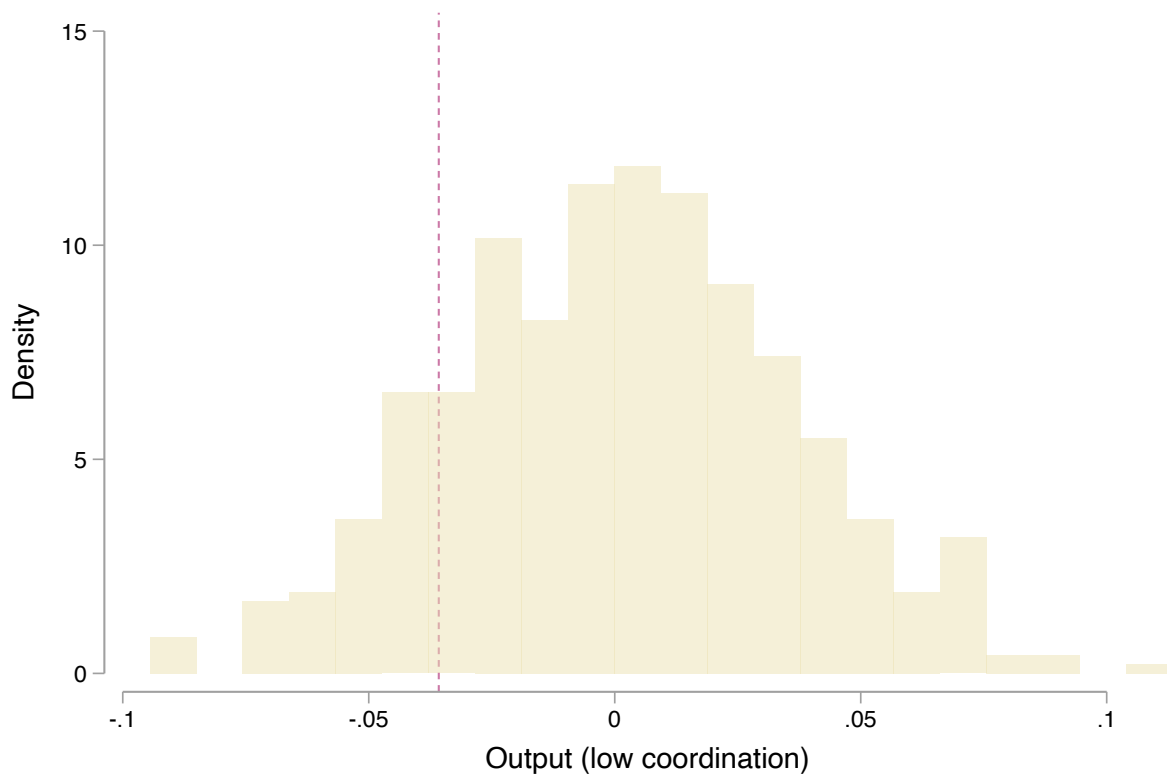
This table presents a summary of p-values from randomization inference from $N = 500$ simulations. For each given outcome variable, column 1 lists the standard p-value, column 2 lists the randomization inference p-value, with labels corresponding to tables in the main text in column 3.

Figure C.1: RI estimates of treatment effects on output



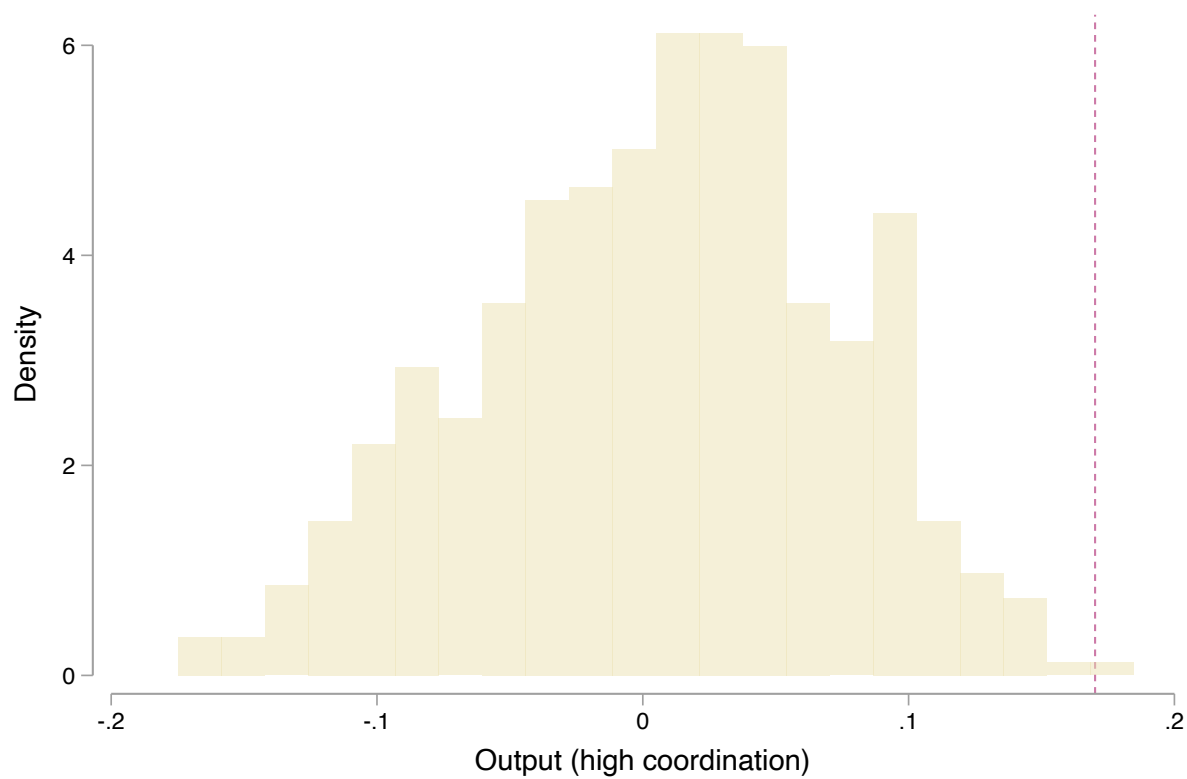
This figure plots the distribution of p-values from randomization inference simulations for the output listed in the figure caption, with the vertical dotted line denoting the standard p-value reported in the main text.

Figure C.2: RI estimates of treatment effects on output (low coordination teams)



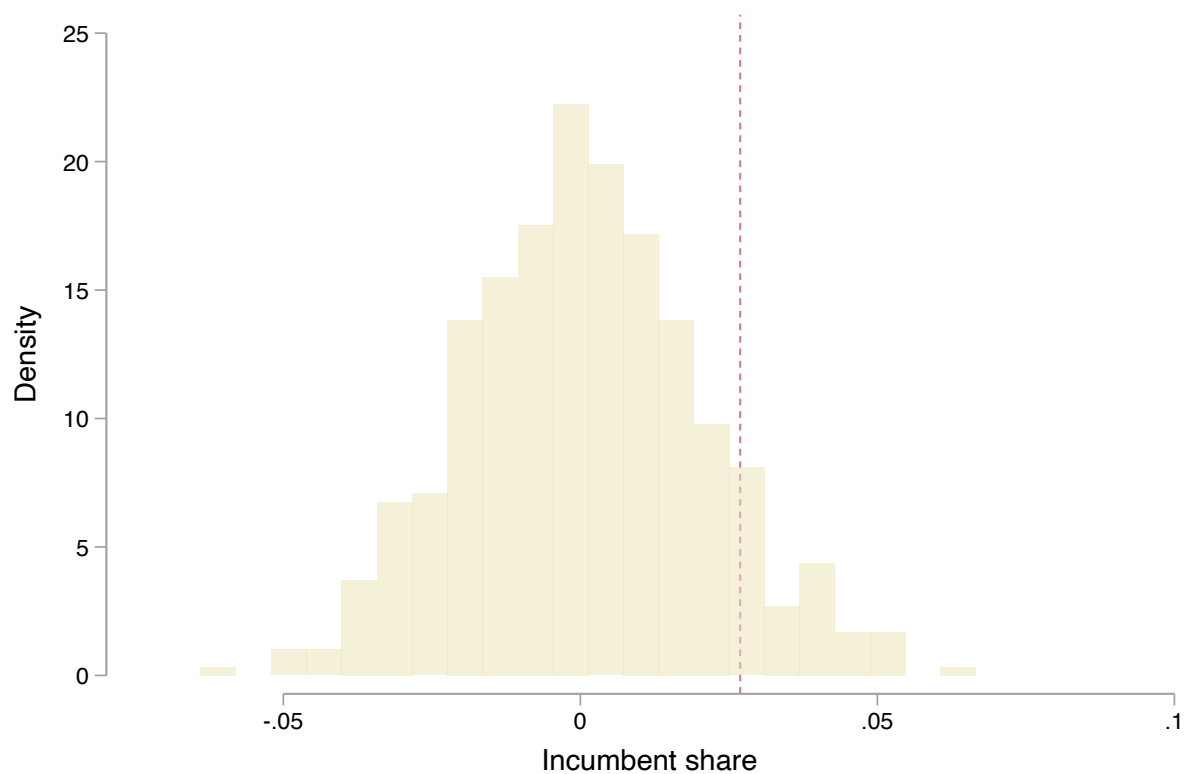
This figure plots the distribution of p-values from randomization inference simulations for the output listed in the figure caption, with the vertical dotted line denoting the standard p-value reported in the main text.

Figure C.3: RI estimates of treatment effects on output (high coordination teams)



This figure plots the distribution of p-values from randomization inference simulations for the output listed in the figure caption, with the vertical dotted line denoting the standard p-value reported in the main text.

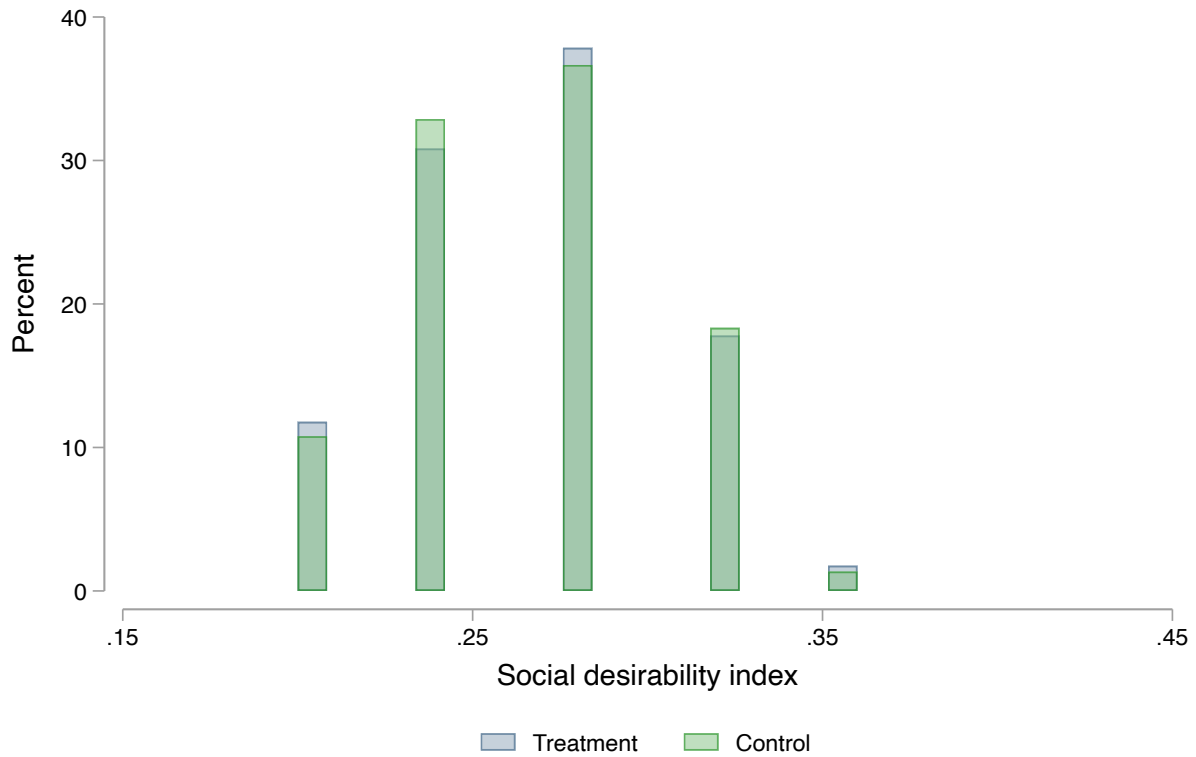
Figure C.4: RI estimates of treatment effects on team LC shares



This figure plots the distribution of p-values from randomization inference simulations for the output listed in the figure caption, with the vertical dotted line denoting the standard p-value reported in the main text.

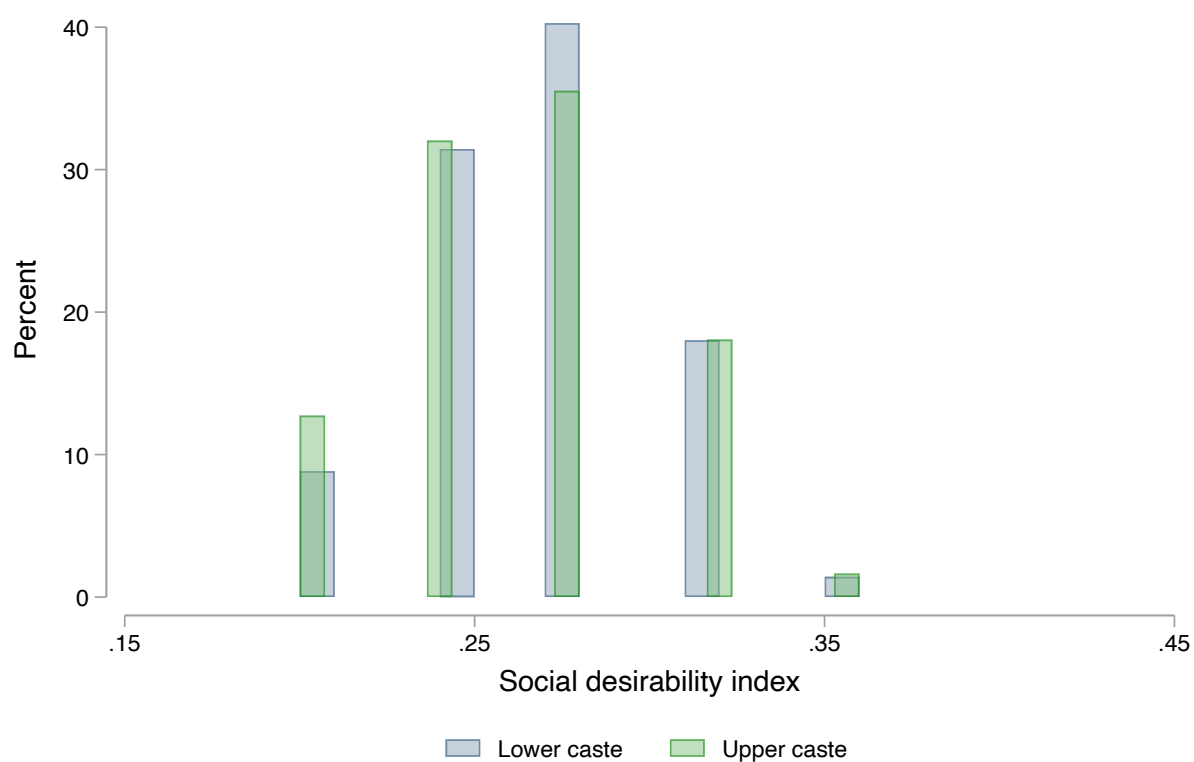
C.2 Social desirability bias

Figure C.5: Distribution of social desirability index by treatment



This figure plots the distribution of a social desirability index created from responses to questions from an adapted version of a Marlowe-Crowne Social Desirability Scale, reported separately for workers at the firm corresponding to treatment and control teams.

Figure C.6: Distribution of social desirability index by caste status



This figure plots the distribution of a social desirability index created from responses to questions from an adapted version of a Marlowe-Crowne Social Desirability Scale, reported separately for workers at the firm corresponding to lower caste and upper caste workers.

Table C.2: Heterogeneity by social desirability

	Team cohesion (0-10)		Friends outside		Bargaining power		Value of referral		Job satisfaction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	0.098 (0.092)	-0.246* (0.147)	0.209 (0.403)	0.031 (0.552)	-0.026 (0.039)	-0.028 (0.048)	0.005 (0.039)	-0.035 (0.048)	0.161 (0.116)	0.189 (0.169)
Above median social desirability (SD)	-0.208 (0.202)	-0.406* (0.218)	-1.354*** (0.370)	-1.512*** (0.434)	-0.116* (0.065)	-0.112 (0.072)	-0.061 (0.062)	-0.035 (0.075)	0.360** (0.165)	0.315* (0.183)
Treatment × Above median SD	0.291 (0.259)	0.549* (0.322)	1.166 (0.920)	-0.051 (0.733)	0.011 (0.089)	-0.046 (0.112)	-0.023 (0.080)	-0.010 (0.102)	-0.294 (0.248)	-0.499 (0.356)
Lower caste		0.581*** (0.156)		-0.896 (0.558)		-0.038 (0.065)		-0.096 (0.076)		-0.159 (0.213)
Treatment × Lower caste		0.301 (0.228)		0.890 (0.899)		0.027 (0.077)		0.136 (0.088)		0.043 (0.289)
Lower caste × Above median SD		0.663* (0.367)		0.778 (0.838)		-0.010 (0.134)		-0.090 (0.154)		0.207 (0.498)
Treatment × Lower caste × Above median SD		-0.696 (0.476)		2.122 (1.867)		0.121 (0.179)		0.010 (0.188)		0.307 (0.631)
Control mean	8.312	8.312	3.485	3.485	0.679	0.679	0.739	0.739	8.295	8.295
R ²	0.02	0.10	0.02	0.03	0.02	0.02	0.02	0.03	0.02	0.02
Observations	767	767	767	767	770	770	770	770	769	769

This table reports the primary outcomes from the endline survey at the firm, with the treatment indicator interacted with whether the respondent was above or below the median in the social desirability index, along with an interaction by caste status. Outcomes are as defined in the main text. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D Attrition and non-response

Table D.1: Sample comparison

Variable	Firm sample (F)		Vignettes sample (V)		LiF sample (L)		Pairwise test p-values			Observations
	Mean	SD	Mean	SD	Mean	SD	C vs. V	C vs. F	V vs. F	
Monthly income	11.978	4.803	0.000	0.000	12.150	9.002	.	0.000	.	
State of origin: Bihar	0.337	0.473	0.600	0.492	0.514	0.501	0.000	0.000	0.000	
State of origin: UP	0.539	0.499	0.300	0.460	0.311	0.464	0.000	0.000	0.000	
State of origin: Delhi	0.053	0.223	0.073	0.262	0.143	0.350	0.000	0.000	0.000	
Participated in cultivation last year	0.230	0.421	0.000	0.000	0.371	0.484	.	0.000	.	

This table presents a comparison of the sample of respondents across the three experiments: the firm workforce, the sample for the vignettes survey, and the sample for the lab in field experiment. In rows, I report means and standard deviations for indicators that are common across the three samples where available. I also present pairwise t-tests between each of the samples. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D.2: Attrition in referral candidate survey

Variable	(1)		(2)		T-test Difference (1)-(2)
	Not surveyed N/[Clusters]	Mean/SE	Surveyed N/[Clusters]	Mean/SE	
Same caste as referrer	573 [414]	0.607 (0.027)	454 [409]	0.656 (0.024)	-0.049
Likelihood of accepting job	573 [414]	6.981 (0.078)	454 [409]	7.870 (0.073)	-0.889***
In Delhi as of the baseline	472 [352]	0.612 (0.025)	406 [363]	0.621 (0.024)	-0.008
Unemployed as of baseline	573 [414]	0.401 (0.024)	454 [409]	0.414 (0.024)	-0.013
Expected quality on the job	573 [414]	6.806 (0.084)	454 [409]	6.830 (0.079)	-0.024
Close or extended family member	573 [414]	0.815 (0.020)	454 [409]	0.804 (0.020)	0.011

This table presents a comparison of the sample of referral candidates that I was able to track in the endline follow-up survey and those that did not respond. I represent the sample size, means and standard errors of the entire set of variables I observe for the entire universe of referral candidates. Column 3 represents the difference and statistical significance from a pairwise t-test. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

E Deviations from pre-analysis plan

This study was pre-registered at the AEA RCT Registry (AEARCTR-0013091) on March 2, 2024, with the final update to the pre-analysis plan recorded on August 24, 2024. The main experimental design elements, primary outcomes, and empirical specifications followed the pre-specified plan closely. Nonetheless, there were a small number of deviations and additions, documented here.

Analogous to primary outcomes from lab-in-field experiment. As planned, I measured the primary firm-level outcomes (team composition, productivity, retention, and cohesion) using administrative data and surveys. In the lab-in-field experiment, I added two outcome measures that were subsequently listed in the initial pre-analysis plan as an edited entry: (i) productivity effects, again disaggregated by high- and low-coordination tasks as in the firm experiment, and (ii) cooperation in a symbol-matching game, coded as the share of correct matches relative to the true number of matches. These were introduced to strengthen measurement of cohesion, which in the main firm experiment relied on self-reported survey data. Separately, I added two outcome measures from the vignette surveys that were not listed in the initial pre-analysis plan but are consistent with the conceptual framework: (i) respondents' reports of change in expected productivity, and (ii) respondents' reports of change in expected cohesion, before and after hiring workers through a range of different recruitment modes.

Heterogeneous treatment effects. The PAP pre-specified testing for heterogeneity by task type, team size, baseline lower caste share, churn rate, baseline cohesion, and supervisor beliefs. I primarily report heterogeneous treatment effects in the firm (and lab-in-field) experiment by coordination, which is defined at the task type. I report the other heterogeneities in appendix tables [B.10](#) and [B.11](#).

Specification refinements. The PAP proposed basic OLS specifications with randomization-strata fixed effects and team-level clustering. All core results follow this specification. For some lab-in-field and follow-up candidate outcomes, I also estimated difference-in-differences specifications or interacted treatment indicators with coordination intensity, in order to align more directly with the hypotheses motivating the extension. These refinements do not alter the interpretation of treatment effects. For the analysis of the referral candidate follow-up survey outcomes, I also employ a PDS lasso specification to select controls to improve precision.

Implementation notes. Minor deviations occurred in referral allocation due to (i) administrative errors in the HR department's implementation of the prescribed referral lists, (ii) occasional refusal of referral opportunities by selected incumbents, and (iii) rare supervisor overrides. These account for the small number of non-compliant referral opportunities reported in table [2](#). Separately, as noted in the edit to the original pre-analysis plan, the cross-randomization intervention where recruited workers would be shuffled across teams was not implemented due to a lack of feasibility.

F Static model

Consider a static model in which a firm allocates a share $\rho \in [0, 1]$ of referral opportunities to minority incumbent employees (LCs), with the remainder $1 - \rho$ going to majority incumbents (UCs). The firm has fixed team size L , and current team diversity is given by $S \in [0, 1]$, the share of team members who are LCs.

Production. Output depends on team cohesion and follows:

$$Y = A \cdot C(S, \rho) \cdot L^\alpha, \quad \text{where } A > 0, \alpha \in (0, 1)$$

$C(\cdot)$ denotes team cohesion, i.e. a measure of how well the team works together. Cohesion is highest when referrals align with the team's (exogenously determined) ethnic composition, and I model misalignment costs as:

$$C(S, \rho) = 1 - \kappa |\rho - S|^\eta, \quad \kappa > 0, \eta > 0$$

Training Cost. Turnover imposes training costs on teams, and I model them as a function of the exits. Let γ_l and γ_u be inverse turnover rates (i.e., a measure of retention) for LCs and UCs, respectively, and define the retention advantage:

$$\delta = \gamma_l - \gamma_u$$

For the purpose of the parameterization, I take γ_u as given and evaluate the response of the referral allocation ρ with respect to δ .

Expected turnover is a weighted average:

$$\bar{T}(\rho) = \rho \cdot \frac{1}{\gamma_l} + (1 - \rho) \cdot \frac{1}{\gamma_u}$$

$$T(\rho) = \tau \cdot \bar{T}(\rho) = \tau \left(\frac{\rho}{\gamma_l} + \frac{1 - \rho}{\gamma_u} \right)$$

This model is intentionally constrained to be static in order to aid interpretation. The expectations that the agent forms based on future turnover, based on retention probabilities, encapsulates a static version of this inherently dynamic process.

Firm Objective. The firm chooses ρ to maximize:

$$\pi(\rho; S, \delta) = AL^\alpha C(S, \rho) - T(\rho) - wL$$

Each worker at the firm gets paid a uniform wage w , and the size of the team remains fixed at L . The first-order condition (FOC) for profit maximization is:

$$\frac{d\pi}{d\rho} = AL^\alpha \cdot \frac{dC}{d\rho} - \frac{dT}{d\rho} = 0$$

$$\begin{aligned}\frac{dC}{d\rho} &= -\kappa\eta \cdot \text{sign}(\rho - S) \cdot |\rho - S|^{\eta-1} \\ \frac{dT}{d\rho} &= \tau \left(\frac{1}{\gamma_l} - \frac{1}{\gamma_u} \right) = -\tau \cdot \frac{\delta}{\gamma_l \gamma_u}\end{aligned}$$

A closed form solution for ρ^* can be arrived at as follows.

Case 1: $\rho^* > S$

$$\begin{aligned}\implies AL^\alpha \cdot \frac{dC}{d\rho} &= \frac{dT}{d\rho} \\ AL^\alpha \cdot \kappa\eta \cdot \gamma_l \gamma_u \cdot (\rho - S)^{\eta-1} &= \tau\delta \\ AL^\alpha \cdot \kappa\eta \cdot (\delta + \gamma_u)\gamma_u \cdot (\rho - S)^{\eta-1} &= \tau\delta \\ \implies \rho^* &= S + \left(\frac{\tau\delta}{AL^\alpha \cdot \kappa\eta \cdot (\delta + \gamma_u)\gamma_u} \right)^{\frac{1}{\eta-1}}\end{aligned}$$

Case 2: $\rho^* < S$

$$\begin{aligned}\implies AL^\alpha \cdot \frac{dC}{d\rho} &= \frac{dT}{d\rho} \\ -|\rho - S|^{\eta-1} &= \frac{\tau\delta}{AL^\alpha \cdot \kappa\eta \cdot \gamma_l \gamma_u}\end{aligned}$$

This case only has a solution when $\delta < 0$.

Combining the two cases together, we get the following general expression:

$$\rho^* = S + \text{sign}(\delta) \cdot \left(\frac{|\tau\delta|}{AL^\alpha \cdot \kappa\eta \cdot (\delta + \gamma_u)\gamma_u} \right)^{\frac{1}{\eta-1}}$$

Figure F.1 provides a visual representation of the marginal benefit and marginal cost of referral allocations with respect to the exogenous composition of the team. The model delivers an equilibrium where these curves intersect, wherein the firm cannot profitably allocate referrals differentially.

Comparative Statics

I analyze how the optimal referral share ρ^* responds to model primitives using the closed-form solution:

$$\rho^* = S + \text{sign}(\delta) \cdot \left(\frac{|\tau\delta|}{AL^\alpha \kappa \eta \gamma_U (\gamma_U + \delta)} \right)^{\frac{1}{\eta-1}}$$

When $\delta = 0$, i.e. there is no difference in the expected retention probabilities across LCs and UCs, the optimal share of referral allocation follows the (exogenously determined) share of LC workers on the team, which can be interpreted as the boundary solution $\rho^* = S$. In this section, I will focus on propositions that apply when $\delta \neq 0$, wherein the interior solution is only defined for $\rho^* > S$ if $\delta > 0$, and $\rho^* < S$ if $\delta < 0$.

Proposition 1: Effect of exogenous team diversity S Ceteris paribus, the optimal referral share ρ^* is strictly increasing in S :

$$\frac{d\rho^*}{dS} = 1 > 0$$

Proof. From the closed-form solution, ρ^* is linear in S with slope 1. □

In teams that are exogenously populated with a larger share of LCs, it is optimal for the firm to allocate a higher share of referrals to LCs. This is intuitive, since the team does not incur the cohesion cost of deviating from the inherent diversity of the team, while still capturing the retention benefits of increased representation of LCs. Figure F.2 provides a visual representation of this relationship, illustrating that the marginal benefit of referrals to LCs declines more slowly in teams that have more LCs.

Proposition 2: Effect of expected retention advantage δ The optimal referral share ρ^* is strictly increasing in δ for all values where $\delta \neq -\gamma_u$, i.e. $\gamma_l = 0$, and $\eta > 1$. That is,

$$\frac{d\rho^*}{d\delta} > 0 \quad \text{for all } \delta \in \mathbb{R}, \gamma_l \neq 0, \eta > 1$$

In particular, for $\delta > 0$, higher LC retention raises the share of referrals going to LCs ($\rho^* > S$) above their exogenous share of the team. For $\delta < 0$, lower LC retention lowers the referral share below their exogenous share of the team ($\rho^* < S$). $\rho^* \rightarrow S$ as $\delta \rightarrow 0$, and ρ^* is continuous in δ .

Proof. We begin with the generalized closed-form:

$$\begin{aligned}\rho^* &= S + \text{sign}(\delta) \cdot \left(\frac{|\tau\delta|}{AL^\alpha \kappa \eta \gamma_u (\gamma_u + \delta)} \right)^{\frac{1}{\eta-1}} \\ \Phi(\delta) &:= \left(\frac{|\tau\delta|}{AL^\alpha \kappa \eta \gamma_u (\gamma_u + \delta)} \right)^{\frac{1}{\eta-1}} \\ \frac{d\rho^*}{d\delta} &= \text{sign}(\delta) \cdot \frac{d\Phi}{d\delta} + \Phi(\delta) \cdot \frac{d}{d\delta} \text{sign}(\delta)\end{aligned}$$

Because $\text{sign}(\delta)$ is constant on either side of zero and discontinuous at zero, the derivative simplifies to:

$$\frac{d\rho^*}{d\delta} = \text{sign}(\delta) \cdot \frac{d\Phi}{d\delta} \quad \text{for } \delta \neq 0$$

Now differentiating $\Phi(\delta)$ for $\delta > 0$:

$$\frac{d\Phi}{d\delta} = \frac{1}{\eta-1} \cdot \left(\frac{\tau\delta}{AL^\alpha \kappa \eta \gamma_u (\gamma_u + \delta)} \right)^{\frac{2-\eta}{\eta-1}} \cdot \left(\frac{1}{(\gamma_u + \delta)^2} \right) > 0$$

A symmetric argument applies for $\delta < 0$ with the same magnitude but opposite direction.

Hence, for all $\delta \in \mathbb{R} \setminus \{-\gamma_u\}$ and $\eta > 1$, we have:

$$\frac{d\rho^*}{d\delta} = \frac{1}{\eta-1} \cdot \left(\frac{|\tau\delta|}{AL^\alpha \kappa \eta \gamma_u (\gamma_u + \delta)} \right)^{\frac{2-\eta}{\eta-1}} \cdot \frac{\gamma_u^2}{(\gamma_u + \delta)^2} > 0$$

□

When there are large gains from allocating referrals to LCs in (avoided) turnover and training costs, it is optimal for the firm to allocate a larger share of referrals to LCs. Theoretically, if this advantage reverses direction, it would be optimal for the firm to instead allocate a greater share to UCs. This relationship exists for cases where there are weakly increasing returns to cohesion from maintaining diversity, i.e. $\eta > 1$. Figure F.4 provides a visual representation of this relationship.

Proposition 3: Effect of cohesion sensitivity κ The optimal referral share ρ^* is decreasing in the cohesion sensitivity κ when there is a retention advantage for LCs, i.e. $\delta > 0$, and increasing in the cohesion sensitivity κ when there is a retention advantage for UCs, i.e. $\delta < 0$, when there are increasing returns in cohesion from maintaining pre-existing diversity, i.e. $\eta > 1$.

Proof. We begin with the generalized closed-form:

$$\begin{aligned}
\rho^* &= S + \text{sign}(\delta) \cdot \left(\frac{|\tau\delta|}{AL^\alpha \kappa \eta \gamma_u (\gamma_u + \delta)} \right)^{\frac{1}{\eta-1}} \\
&= S + \text{sign}(\delta) \cdot \nu \cdot \kappa^{\frac{1}{1-\eta}}, \text{ where } \nu \text{ is some constant } > 0 \\
\Rightarrow \frac{d\rho^*}{d\kappa} &= \text{sign}(\delta) \cdot \frac{\nu}{1-\eta} \cdot \kappa^{\frac{\eta}{1-\eta}}
\end{aligned}$$

For $1 - \eta < 0$, this implies that $\frac{d\rho^*}{d\kappa} < 0$ when $\text{sign}(\delta) > 0$, and $\frac{d\rho^*}{d\kappa} > 0$ when $\text{sign}(\delta) < 0$. □

κ mediates the curvature of the marginal benefit of referral allocations, resulting in a kinked design where the direction of the slope reverses symmetrically as the referral share deviates from the exogenous share of LCs on the team. As a result, if κ is low, i.e. the team is less sensitive to changes in composition, it is optimal for the firm to allocate more referrals to LCs. Figure F.3 provides a visual representation of this relationship.

Proposition 4: Effect of curvature parameter η The effect of η on ρ^* is non-monotonic, increasing for low values of η , and decreasing for large values of η , with an interior maximum.

Proof. Reframing the generalized closed-form expression:

$$\begin{aligned}
z(\eta) &= \frac{\tau\delta}{AL^\alpha \kappa \eta \gamma_U (\gamma_U + \delta)} > 0 \quad \text{so that} \quad \rho^*(\eta) = S + z(\eta)^{1/(\eta-1)} \\
\frac{d\rho^*}{d\eta} &= \frac{d}{d\eta} \left(z^{1/(\eta-1)} \right) = z^{1/(\eta-1)} \cdot \frac{d}{d\eta} \left(\frac{1}{\eta-1} \cdot \log z \right) \\
&= z^{1/(\eta-1)} \cdot \frac{1}{(\eta-1)^2} \cdot [1 - \eta - \log(z^\eta)] \\
&= \left(\frac{\tau\delta}{AL^\alpha \kappa \eta \gamma_U (\gamma_U + \delta)} \right)^{\frac{1}{\eta-1}} \cdot \frac{1 - \eta - \eta \log \left(\frac{\tau\delta}{AL^\alpha \kappa \eta \gamma_U (\gamma_U + \delta)} \right)}{\eta(\eta-1)^2}
\end{aligned}$$

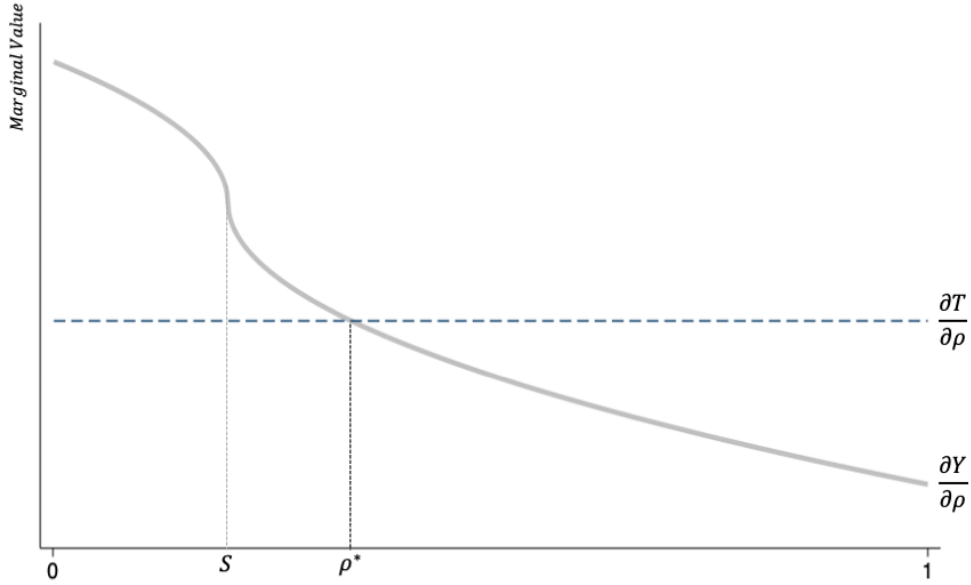
Note that,

$$1 - \eta - \eta \log(z),$$

can be positive or negative depending on η . When η is small, $\log(z)$ is small and the expression can be positive. When η grows large, $-\eta \log z$ dominates and the derivative turns negative.

Hence, $\rho^*(\eta)$ rises at first and then falls, resulting in a unique interior maximum.

Figure F.1: Static framework



This figure represents an example of the static model framework for $\eta = 1.5$ and $S = 0.2$. The downward sloping marginal benefit curve and the horizontal marginal cost curve. They intersect at the optimal referral allocation to LCs $= \rho^*$.

□

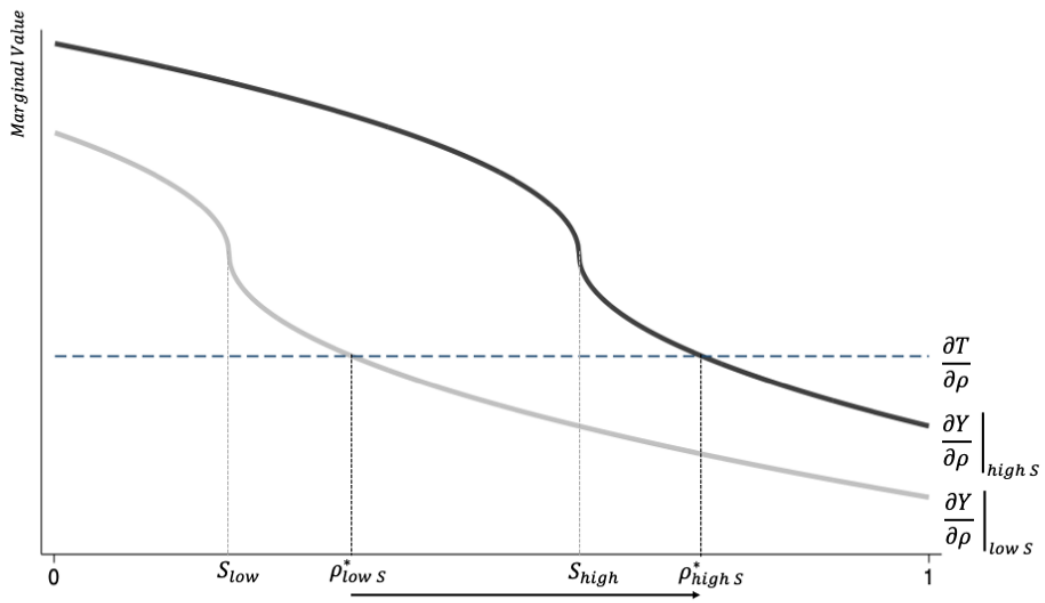
A high η implies a brittle, sensitive team culture where cohesion suffers sharply if referral shares diverge from the group's current composition. A low η suggests a more resilient or adaptable team culture where diversity imbalances are less costly in terms of cohesion.

Interpretation

The firm balances cohesion-driven productivity against retention-driven cost savings. The optimal referral strategy reflects both: referrals that are too imbalanced (relative to team composition) hurt cohesion, but referrals toward more stable workers (i.e., LCs when $\delta > 0$) reduce training costs. The equilibrium referral share ρ^* lies where marginal cohesion gain equals marginal training cost.

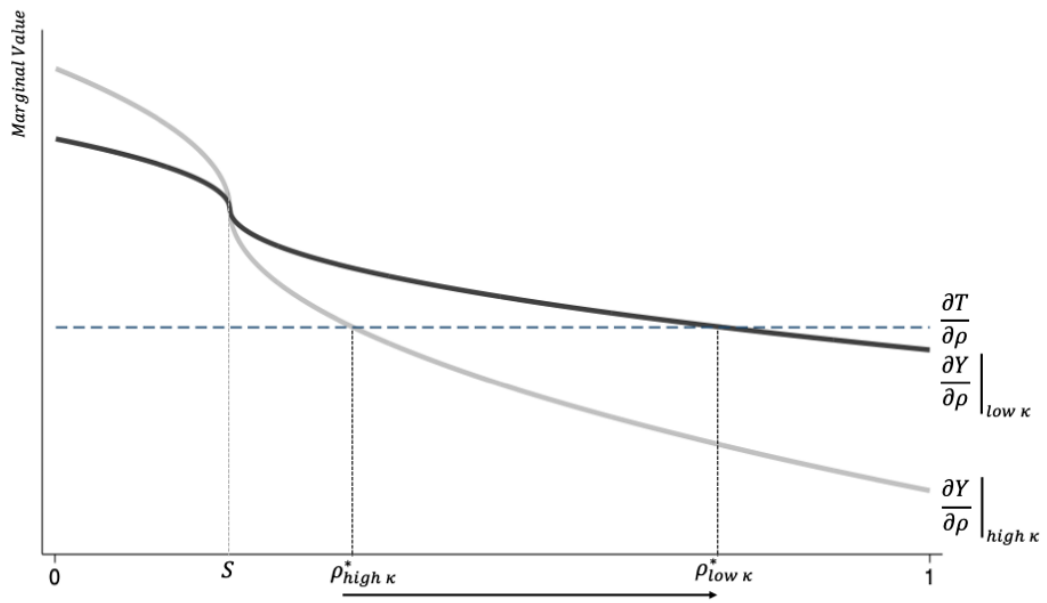
Even for a wide range of “ideal” parameter values (i.e. large δ and small κ), it is not clearly optimal to allocate all referrals to LCs, as in the experiment at the firm. This is the case for a very restricted parameter space.

Figure F.2: Referral allocation with respect to exogenous diversity



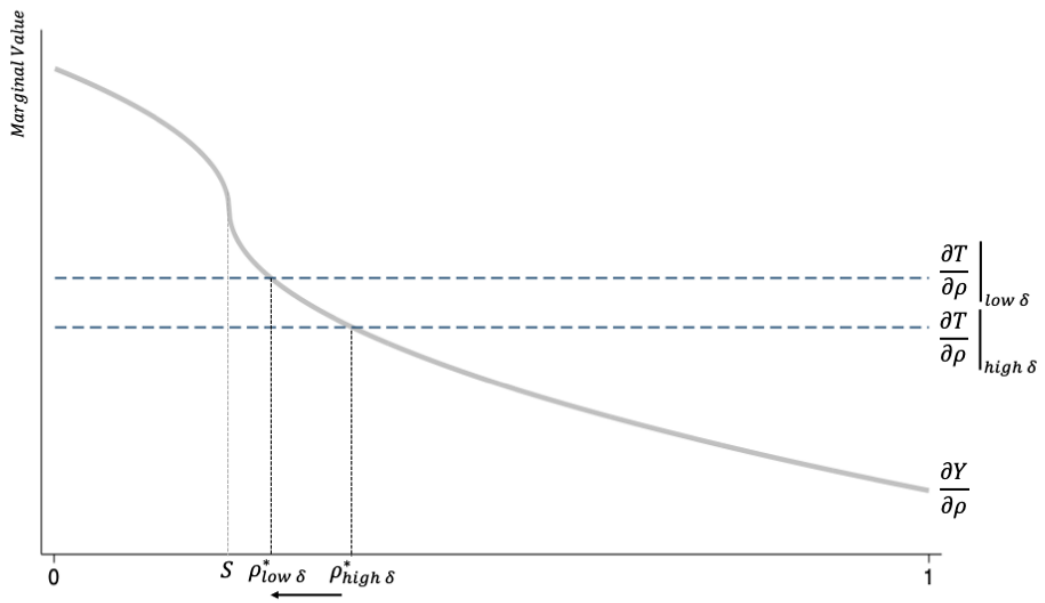
This figure represents an example of the static model framework for $\eta = 1.5$ and $S_{low} = 0.2$ and $S_{high} = 0.6$. The optimal share of LC referrals increases as the exogenous diversity of the team increases.

Figure F.3: Referral allocation with respect to cohesion sensitivity



This figure represents an example of the static model framework for $\eta = 1.5$ and $\kappa_{low} = 0.1$ and $\kappa_{high} = 0.4$. The optimal share of LC referrals increases as the sensitivity to cohesion costs decreases.

Figure F.4: Referral allocation with respect to retention advantage



This figure represents an example of the static model framework for $\eta = 1.5$ and $\delta_{low} = 0.1$ and $\delta_{high} = 0.4$. The optimal share of LC referrals decreases as the retention advantage of hiring LCs decreases.

G Bayesian learning and perceived profits

This appendix consolidates the formal dynamic model, the belief-updating machinery, and the perceived-profits simulator used for estimation.

G.1 Environment, state and flows

Time is discrete in weeks $t \in \mathbb{Z}$, with $t = 1$ the first treatment week and $t \leq 0$ pre-experiment. Let L_t^ℓ and L_t^u denote the stock of *productive* LC and UC workers. New hires enter a k_{delay} -week bench before becoming productive. Define the LC share among productive workers $s_t = L_t^\ell / (L_t^\ell + L_t^u)$ (with $0/0 := 0$). Monthly entry flows are taken from the data and distributed evenly over weeks.

The referral rule is $r_t \in [0, 1]$:

$$r_t^C = s_t \quad (\text{control}); \quad r_t^T = 1 \quad (\text{treatment}).$$

Weekly LC and UC entries are $E_t^\ell = r_t E_t$ and $E_t^u = (1 - r_t) E_t$. Incumbent exits follow tenure-specific hazards; let $h_{g,\tau}^{\text{mo}}$ be monthly hazards by $g \in \{\ell, u\}$ and $\tau \in \{0, 1\}$ (new vs. incumbent), mapped to weekly via

$$h_{g,\tau}^{\text{wk}} = 1 - (1 - h_{g,\tau}^{\text{mo}})^{1/4}.$$

Let $X_{g,t}^{\text{inc}} \sim \text{Binomial}(L_t^g, h_{g,1}^{\text{wk}})$ and $X_{g,t}^{\text{new}} \sim \text{Binomial}(E_t^g, h_{g,0}^{\text{wk}})$ denote exits among incumbents and first-week (bench) exits. Productive stocks update as

$$L_{t+1}^g = L_t^g + \text{Mature}_t^g - X_{g,t}^{\text{inc}} - X_{g,t}^{\text{new}}, \quad (11)$$

where Mature_t^g is the cohort leaving the bench after k_{delay} weeks. Weekly churn is

$$C_t = E_t + X_{\ell,t}^{\text{inc}} + X_{u,t}^{\text{inc}} + X_{\ell,t}^{\text{new}} + X_{u,t}^{\text{new}}. \quad (12)$$

G.2 Production and cohesion

In the high-frequency simulator (weekly), I parameterize effective labor as a convex aggregation of LC and UC stocks,

$$L_t^{\text{eff}} = \omega L_t^\ell + (1 - \omega) L_t^u, \quad \omega \in [0, 1],$$

and use a parsimonious belief multiplier to capture the cohesion penalty:

$$y_t = A (L_t^{\text{eff}})^\alpha \cdot \phi(v_t) + \varepsilon_t, \quad \phi(v_t) \equiv \frac{1}{1 + \phi_{\text{belief}} v_t} \in (0, 1], \quad \varepsilon_t \sim \mathcal{N}(0, \sigma_\varepsilon^2). \quad (13)$$

This nests the formulation $Y_t = A C(S_t, \rho_t) L_t^\alpha K_t^\beta$ shown in Section 5 by holding K_t fixed at the weekly horizon and mapping $C(\cdot)$ to $\phi(v_t)$.

G.3 Beliefs about cohesion costs

Structural Bayesian updating.. Supervisors enter with Gaussian priors

$$\kappa \sim \mathcal{N}(v_{\kappa,t}, \sigma_{\kappa,t}^2), \quad (14)$$

and observe a per-referral cohesion signal $s_t = (Y_t - \bar{Y}_0)/N_t$ when $N_t > 0$ LC hires occur. Under (6), $s_t \mid \kappa \sim \mathcal{N}(\kappa, \sigma_\varepsilon^2/N_t)$. Once a minimal stock of LC referrals $\sum_{\tau \leq t} N_\tau \geq k^*$ is accumulated, beliefs update via Normal–Normal conjugacy:

$$\sigma_{\kappa,t+1}^2 = \left(\sigma_{\kappa,t}^{-2} + N_t / \sigma_\varepsilon^2 \right)^{-1}, \quad (15)$$

$$v_{\kappa,t+1} = \sigma_{\kappa,t+1}^2 \left(\sigma_{\kappa,t}^{-2} v_{\kappa,t} + N_t s_t / \sigma_\varepsilon^2 \right), \quad (16)$$

and otherwise remain at $(v_{\kappa,t}, \sigma_{\kappa,t}^2)$.

Reduced-form belief path for weekly profits.. For estimation and to align timing with the reduced-form moments, I use a smooth parametric path that approximates the Bayesian learning dynamics:

$$v_t = \kappa_{\text{true}} + (\kappa_{\text{prior}} - \kappa_{\text{true}}) \exp\left[-\eta (t - t_{\text{learn}})_+\right], \quad (x)_+ \equiv \max\{x, 0\}, \quad (17)$$

with speed $\eta > 0$ and delay $t_{\text{learn}} \geq 0$. A stock threshold k^* can accelerate the drift once enough LC referrals have accumulated. The mapping from v_t to output is via $\phi(v_t)$ in (13).

Convergence time under gated Normal–Normal learning.. Let priors be $\kappa \sim \mathcal{N}(v_{\kappa,0}, \sigma_{\kappa,0}^2)$ and, in periods with $N_t > 0$, observe the per-referral signal $s_t = (Y_t - \bar{Y}_0)/N_t$ with $s_t \mid \kappa \sim \mathcal{N}(\kappa, \sigma_\varepsilon^2/N_t)$. Learning occurs when teams cross a threshold: updates occur only once the exposure stock satisfies $\sum_{\tau \leq t} N_\tau / L_\tau \geq k^*$, after which Normal–Normal conjugacy implies

$$\sigma_{\kappa,t+1}^{-2} = \sigma_{\kappa,t}^{-2} + \frac{N_t}{\sigma_\varepsilon^2}, \quad v_{\kappa,t+1} = \sigma_{\kappa,t+1}^2 \left(\sigma_{\kappa,t}^{-2} v_{\kappa,t} + \frac{N_t}{\sigma_\varepsilon^2} s_t \right).$$

Under the linearized measurement used in estimation, where $z_t \approx \kappa_t + v_t$ and $\text{Var}(v_t) = \sigma_\varepsilon^2/(N_t S_t^2)$, simply replace N_t by $N_t^{\text{eff}} \equiv N_t S_t^2$. For credible-interval targeting, one can set $\varepsilon = \text{CI}^\alpha / (2z_{1-\alpha/2})$, where CI^α is the desired half-width at level α . The decomposition $T(\varepsilon) = T_{\text{threshold}} + E^*(\varepsilon)/r$ clarifies that slow learning arises both from rare exposure (small r) and from failing to clear the stock gate quickly (large $T_{\text{threshold}}$).

For each team i I compute $E_i^*(\varepsilon) = \sigma_{\varepsilon,i}^2(\varepsilon^{-2} - \sigma_{\kappa,0,i}^{-2})$, estimate r_i from pre-experiment LC referrals, approximate $T_{\text{threshold},i}$ from the pace at which $\sum_{\tau \leq t} N_{i\tau}/L_{i\tau}$ reaches k^* , and report $\widehat{T}_i(\varepsilon) = T_{\text{threshold},i} + E_i^*(\varepsilon)/r_i$. I summarize $\{\widehat{T}_i(\varepsilon)\}_i$ through three indicators. Median = 11.9 years, p90 = 36.7 years, and the share with $r_i = 0$ where no updating happens is 26.8%.

G.4 Profits and frictions

Perceived weekly profits are

$$\pi_t = y_t - \mu C_t - f - \mathbf{1}\{t \geq 1\} c_0 e^{-t/\tau_{\text{impl}}} - \lambda |\rho_t - s_t|, \quad (18)$$

where μ weighs turnover/training costs, f is a fixed operating cost, c_0 and τ_{impl} capture a fading implementation friction specific to the treatment arm, and $\lambda |\rho_t - s_t|$ penalizes short-run reorganization when the referral rule departs from the status quo composition. This profit mapping is the modified weekly version of the common overarching framework discussed in the main paper in equation (7).

G.5 Estimation by simulated method of moments

Let $\theta = (A, \alpha, \mu, f, \kappa_{\text{true}}, \kappa_{\text{prior}}, \phi_{\text{belief}}, \eta, t_{\text{learn}}, k^*, c_0, \tau_{\text{impl}}, k_{\text{delay}}, \omega, \sigma_{\varepsilon})$. Given θ , the simulator (i) takes monthly entries from the data and weekly hazards $h_{g,\tau}^{\text{wk}}$, (ii) updates stocks and beliefs week by week using (11)–(17), and (iii) produces arm-by-time series for output, turnover and referrals. I form simulated moments to mirror:

- (M1) Event-time means of standardized weekly output (treatment and control).
- (M2) Arm-by-month means of entries, exits and churn.
- (M3) Arm-by-month shares of LC referrals and the implied LC composition.

Parameters minimize $Q(\theta) = [m_{\text{data}} - m^b(\theta)]^\top W [m_{\text{data}} - m^b(\theta)]$ with a diagonal weight matrix W equal to the inverse of the estimated variance of each moment (team-level variance / number of teams). Uncertainty bands in Figure 4 are Monte Carlo envelopes over binomial exit and Gaussian output shocks; the mean crossing time $T^* = \min\{t \geq 1 : \bar{d}_t > 0\}$ is about 6.2 weeks with a 10–90 percentile interval [5, 14].

Equation (17) produces the belief paths underlying Figure 3, consistent with the Normal–Normal learning logic in (15)–(16) when treatment forces early exposure to LC referrals. Combining (13) and (18) with observed entry flows and hazards yields the perceived profit trajectories in Figure 4: a short initial dip followed by recovery as beliefs improve and LC cohorts mature into productive work.