Social Networks and Hiring: Experimental Evidence from Pakistan

*

Maryiam Haroon†
and Christina Brown‡

September 2026

Abstract

Network-based hiring dominates labor markets in developing countries, yet whether this reflects efficient screening or market failures remains unclear. We investigate two mechanisms: information asymmetries favor workers with known productivity, or connections generate match-specific returns through productivity complementarities, enhanced trust, or reduced monitoring costs. Through a randomized controlled trial with 87 construction managers and 1,500 workers in Pakistan, we provide objective productivity information, introduce performance-based pay, and randomly remove managers from worksites. We find networks operate through search friction reduction rather than productivity selection—connected workers exhibit no productivity advantage yet secure employment 30% faster. Providing productivity information generates substantial wage gains ($13.95 monthly income, $0.40 daily wages), but exclusively for connected workers who simultaneously reduce hours worked. Unconnected workers experience null effects despite identical information, revealing that productivity signals complement rather than substitute for social capital. Performance incentives fail to shift hiring patterns or productivity. Exploiting variation in manager presence, we find connected workers face hiring advantages only when their manager is present, yet productivity remains unchanged—indicating networks provide non-transferable private information about reliability rather than productivity complementarities.

JEL: J24, J23, D85, C93, O15

Keywords: Social Networks, Hiring Practices, Low-Wage Labor Markets, Worker Productivity, Short Term Hiring, Information asymmetry, Field Experiment

*We gratefully acknowledge generous funding from NSF, STEG, PEDL, IGC, and UChicago. We received IRB approval from UChicago and Lahore School of Economics. AEA Registry-0016460. Our team provided excellent research assistance. All remaining errors are our own.

†Corresponding Author. University of Chicago. Email: maryiamharoon@uchicago.edu

‡University of Chicago. Email: christinabrown@uchicago.edu

1 Introduction

Why do employers rely so heavily on hiring from their social networks? In a perfectly competitive labor market, firms would hire exclusively on the basis of worker productivity. In practice, however, information frictions, search costs, and limited contractability make such hiring infeasible. Network-based hiring is not marginal—it is the dominant recruitment mode in many labor markets. In low-income countries such as Bangladesh and the Philippines, 60–70 percent of workers obtain jobs through referrals, and informal hiring remains common even in advanced economies (Caria et al. 2020a; Witte 2021; Beaman and Magruder 2012; Delcuvellerie et al. 2019). Yet it remains unclear whether referrals primarily alleviate market frictions or reflect selection of more productive workers. Moreover, within-network hiring may disadvantage marginalized groups lacking extensive connections (Witte 2021; Beaman et al. 2018).

We distinguish between two primary mechanisms underlying managerial preference for connected workers. Information asymmetries may lead managers to favor workers about whom they have superior productivity information. Alternatively, connections may yield match-specific returns through relationship-specific productivity gains, social preferences for hiring in-group members, or reduced monitoring costs through enhanced trust. To distinguish between these mechanisms, we investigate two fundamental questions. First, can targeted interventions—specifically, information about worker productivity, performance-based managerial incentives or varying on site supervision —induce managers to increase hiring of high-productivity unconnected workers? Second, what explains changes in hiring patterns: do workers strategically reallocate effort between productive activities and social capital investment in response to information, incentive and monitoring regimes?

We address these questions through a randomized controlled trial with 87 managers and 1,500 workers in Lahore, Pakistan's construction sector. The short-term, daily contracting structure provides an ideal setting to experimentally manipulate hiring environment features. Our design includes three treatments: (i) *information* treatment: managers receive objective, standardized productivity metrics, eliminating informational asymmetries between connected and unconnected workers; (ii) *incentive* treatment: managerial bonus payments linked to team productivity to test whether aligning incentives reduces network-based hiring, and (iii) *monitoring* treatment: decoupling hiring authority from supervisory oversight to isolate direct management's role. We measure treatment effects on managerial hiring decisions and worker outcomes using high-frequency observational data combined with surveys. Worker outcomes span three dimensions: daily productivity (measured through task completion rates in standardized brick, tile, and block work), income (daily wages and days worked per month), and job search effort (days spent searching for work). This strategy tests whether treatment-induced changes affect not only on-the-job productivity but also broader labor market outcomes.

Casual labor dominates employment in developing economies, particularly in South Asia where it accounts for over 30 percent of total employment (Nayar et al. 2011). Pakistan exemplifies this pattern: with the world's 9th largest labor force, a substantial fraction operates under predominantly casual arrangements featuring short-term contracts (typically 1-5 days)[1] and exhibits strong poverty gradients, with 47 percent of casual laborers living below the poverty line—a pattern similar to India.[2] These markets exhibit high search and matching costs, informational frictions, and severe employment volatility.[3] In our sample, laborers spend an average of 9 days per month searching for work and work only 17 days per month. Workers find jobs primarily through social connections or by attending local "labor stands," spot markets where low-skilled laborers wait each morning for employers seeking to hire. Network connections dramatically reduce job search duration: connected workers find employment in 12.59 days compared to 18.02 days for those without connections—a 30% reduction. Understanding these mechanisms has direct policy relevance: if information matters, policies could facilitate credible signaling; if social preferences drive hiring, programs could support marginalized employers.

We develop a model in which managers maximize expected utility by choosing between connected and unconnected workers. Managerial utility depends on output and private utility from employing a connected worker. The model nests three mechanisms generating preferential hiring: (i) *selection*—connected workers have higher ability that networks help identify; (ii) *information*—managers hold more precise productivity beliefs about connected workers; (iii) *match-specific returns*—connections generate relationship-specific gains through enhanced productivity, trust-based reductions in monitoring costs or social preferences. We test these predictions through experimental variation isolating each mechanism.

Our exploratory analysis reveals network-based hiring does not operate through productivity selection. Connected workers are neither more productive nor earn higher wages-associations between connectedness and productivity are statistically insignificant, as are wage differentials. However, connected workers allocate time systematically differently: they spend 9.380 fewer minutes socializing with peers ($p < 0.01$) and reduce job search effort by 1.395 days ($p < 0.05$). These patterns indicate that in informal spot labor markets, social networks primarily reduce search frictions rather than screen for ability.

Our first main result demonstrates information provision generates economically meaningful treatment effects exclusively for connected workers. While average treatment effects are modest (0.312 SD in daily wages, $p < 0.10$), connected workers experience substantial

[1] These jobs are mostly concentrated in construction, agriculture, and manual services.

[2] Pakistan Bureau of Statistics, Labour Force Survey 2020-21.

[3] Casual laborers search 0.9-1.4 months for work annually even by 2009-10, representing 9-12 percent of the year without income in India.

gains of \$13.95 in monthly income ($p < 0.05$) and \$0.40 in daily wages ($p < 0.05$).[4]

Our second result shows performance-based incentives yield precisely estimated null effects across all outcomes. Managers exhibit no differential hiring by connectedness at baseline, and incentives generate no detectable shifts. Worker productivity remains unchanged, with statistically insignificant evidence of negative interaction effects for connected workers (−0.19 SD). These results indicate simple compensation schemes are insufficient to realign hiring decisions with productivity in contexts with relational norms.

Our third result evaluates workers' strategic responses. Under information provision, connected workers reduce working hours by 0.603 hours per day ($p < 0.05$) while increasing wages, extract higher returns with lower effort when productivity becomes observable. Unconnected workers demonstrate no measurable behavioral adjustments, reflecting inability to convert productivity signals into improved outcomes absent complementary social capital. Under performance incentives, connected workers exhibit greater non-working time at baseline but sharply reduce it when incentives are introduced, though these effort reallocation do not translate into significant productivity gains. The contrast reveals workers respond strategically when interventions credibly shift labor market opportunities.

Finally, exploiting observational and experimental variation in manager presence, we find connected workers exhibit a hiring advantage only when their manager is present to exercise hiring discretion. This interaction pattern reveals that social networks provide managers with non-transferable private information about worker reliability that reduces hiring uncertainty but cannot be credibly transmitted to other personnel. However, experimental evidence from the monitoring treatment yields precisely estimated null effects, suggesting productivity complementarities, if present, are economically small.

This study makes four contributions. First, we isolate which channels explain employer reliance on hiring within social networks and the magnitude of each channel. While previous work has shown fellow employees have better information about network members,[5]

Second, we provide evidence on how different hiring conditions incentive schemes, information access, and monitoring—affect both productivity and hiring outcomes in short-term labor markets. While existing work examines these factors in isolation (Abebe et al. 2020; Beaman and Dillon 2018; Witte 2021) we explore them together, shedding light on trade-offs managers face and the extent to which reducing information frictions or altering

[4]Monetary values are expressed in constant 2021 international dollars using the private-consumption PPP conversion factor for Pakistan (WDI, PA.NUS.PRVT.PP; 42.00 PKR per international dollar in 2021); nominal rupee values from later survey waves are deflated to 2021 by the Pakistan CPI before conversion (deflators: 2024 = 1.766; 2025 = 1.845, provisional).

[5]Beaman and Magruder (2012); Burks et al. (2015); Dustmann et al. (2012); ?); Kramarz and Skans (2014); Witte (2021), there is less systematic evidence on other mechanisms: homophily, productivity selection, and match-specific complementarities. Our experimental design provides exogenous variation in information availability, managerial incentives, and monitoring structure, allowing us to separately identify these channels.

incentives influences managerial responses.

Third, our findings establish that social networks operate through search friction reduction rather than productivity selection; that productivity signals complement rather than substitute for social capital; and that simple incentive schemes cannot overcome relational dynamics in informal markets. One possible mechanism is that employers face multidimensional uncertainty: beyond productivity, managers value trustworthiness and reliability—attributes difficult to observe directly and credibly signaled only through connections. In informal labor markets with limited formal contracting and signals, networks help reduce uncertainty in ways productivity measurement alone cannot capture.

Fourth, we make a methodological contribution to the personnel economics literature by extending experimental productivity measurement to construction labor markets. The credible measurement of on-the-job productivity remains a central methodological challenge in labor economics. Recent research has developed innovative measurement approaches across multiple contexts- online labor platforms (Pallais 2014; **?**), and formal sector firms (Bandiera et al. 2007; Mas and Moretti 2009; DellaVigna and Linos 2022; Bloom et al. 2013). We build on these foundations by developing measurement tools for manual construction labor in informal markets—a context presenting distinct challenges but representing predominant employment for low-income workers in developing economies.[6] This enables us to address fundamental questions requiring direct productivity observation: What information do employers possess about worker productivity absent formal credentials? How effectively can workers signal quality when standard mechanisms lack credibility?

The remaining paper is organized as follows. Section 2 presents our theoretical framework. Section 3 details our experimental design, treatments, and data. Section 4 provides main results. Section 5 concludes.

2 A Theoretical Framework: Why Managers Hire Connected Workers?

Employers often rely on their social networks when making hiring decisions. Previous research has hypothesized several explanations for why employers may prefer to overhire workers from their social networks (for review, see Schlachter and Pieper (2019); Witte (2021); Heath (2018a); Matsuda and Nomura (2017); Dhillon et al. (2021); Beaman (2012)). We develop a theoretical framework to systematically examine the relevance of two potential mechanisms within a stylized static hiring model.

[6]Construction employs approximately 7 percent of the global workforce (Mito 2019).

2.1 Model Setup

Consider a one-period model in which an employer chooses to hire a worker from a pool of two candidates: a connected worker ($c = 1$), who has a social connections to the employer and an unconnected worker ($c = 0$), with no such connections. The employer chooses the worker that maximizes expected utility:

$$U(c) = E\left[u(y(\theta(c), c))\right] + \alpha(c) - e(c)$$

where $U(c)$[7] is the employer's expected utility from their employee's output. Worker's output, $y(\theta(c), c)$, directly depends on worker ability, $\theta(c)$, and connectedness, c. We assume employers are risk-averse over output with concave utility $u(\cdot)$. The term $\alpha(c)$ captures the private utility from hiring connected workers (e.g., social preferences, reciprocity, trust, uncertainty, information, or favors), while $e(c)$ captures employer effort costs of monitoring and managing them.

We consider that the employment decision as a one-sided where employers make an offer to an employee at an externally set wage and employees accept all offers. This assumption is appropriate given the context and is similar to many low-wage settings.[8] We assume the employer chooses the worker type that delivers higher utility:

$$\text{Hire connected worker if } U(1) > U(0).$$

The difference in expected utility between connected and unconnected hires can be expressed as:

$$\Delta U = U(1) - U(0) = E\left[u'(\cdot)\right]\left[y'(\cdot)\frac{\partial \theta}{\partial c} + \frac{\partial y}{\partial c}\right] + \alpha'(c) - e'(c) \tag{1}$$

where $c \in \{0, 1\}$ denotes connectedness. Although c is a binary indicator, we employ derivative notation $\frac{\partial \theta}{\partial c}$ and $\frac{\partial y}{\partial c}$ as a heuristic representation of the discrete differences $\theta(1) - \theta(0)$ and $y(1) - y(0)$, respectively. This formulation assumes that the expected marginal utility $E[u'(\cdot)]$ is approximately constant with respect to the components in brackets, allowing it to be factored outside the expectation operator.[9]

[7] We could write a more general version of the employer's utility function where employers select which employee to hire across connected and unconnected employees. However, since the variation we focus is across worker connectedness to the employer, we reduce the choice problem to just be between two types. All results hold for the more general choice problem.

[8] In South Asian low-wage labor markets, such as construction and agriculture, wages are generally standardized within cities (Kaur 2019; Breza et al. 2019a). We think of this as similar to minimum wage workers in other contexts, where wages are generally set outside the firm. Additionally, there is a large supply of unemployed laborers available at any time to hire. Laborers physically visit at established intersections ("labor stands"), making supply visible, and it is uncommon for laborers to reject a job that offers a wage of a least the market rate.

[9] Formally, this holds exactly if $u(\cdot)$ is linear in the relevant arguments, or approximately under small changes in output and match quality.

In this theoretical framework, employers' preference for hiring socially connected workers stems from three broad sources: selection, information asymmetry, and match-specific returns. The utility from hiring a connected worker can be decomposed into productivity-related factors (selection on productivity) and relationship-based factors (better information, social preferences, trust, and reduced uncertainty regarding match-specific returns). These mechanisms generate testable predictions through five distinct channels linking social connectedness to hiring decisions and labor market outcomes.

2.2 Model Predictions: Selection

Selection Effect: ($\frac{\partial \theta}{\partial c} > 0$) Connected workers may have higher average productivity due to education, experience, or skills. Employers may hire them based on perceived productivity rather than connectedness (Castilla 2005; Montgomery 1991; Topa 2001). Assortative matching may further reinforce this effect (Calvo-Armengol and Jackson 2004; Ioannides and Loury 2004). We present evidence on this using descriptive data.

2.3 Model Predictions: Information Asymmetry

Asymmetric information: ($\frac{\partial \sigma_\theta^2}{\partial c} < 0$) Employers have better informational advantages about a connected worker's ability, (θ), for those in their social network (Beaman and Magruder 2012; Heath 2018b). This could be due to directly observing correlates of ability or lower verification costs. We test for information asymmetry by alleviating information frictions.

2.4 Model Predictions: Match Specific Returns

Match-specific returns from hiring socially connected workers operate through three distinct mechanisms which are productivity gains, social preferences, and reduced uncertainty or trust.

Match-Specific Productivity: ($\frac{\partial y}{\partial c} > 0$) The productivity of connected employers and employees is higher than unconnected matches, conditional on the ability of the employees. Enhanced communication and improved working dynamics create match-specific complementarities that increase output and productivity, particularly in team-based production (Beaman and Magruder 2012; Pallais and Sands 2014). We test for match specific productivity gains through varying managers presence.

Social Preferences: ($\alpha'(c) > 0$) Employers derive private utility from hiring connected workers. Hiring connected workers may generate intrinsic benefits (altruism) or extrinsic ones (reciprocal favors, side payments, informal exchanges) (Foster and Rosenzweig 2001; Bloch et al. 2008). These private benefits increase the attractiveness of connected hires independent of their productivity. To test whether social preferences drive this effect, we

vary hiring conditions by introducing performance incentives that counteract the tendency to favor connected workers.

Risk and trust: ($e'(c) < 0$) Social connections reduce monitoring and enforcement costs. Supervising and working with socially connected workers may be less costly due to shared norms, familiarity, lower risk and mutual trust. These cost reductions are particularly salient in labor-intensive, short-duration markets such as construction and agriculture, where monitoring costs are substantial (Varian 1990; Heath 2018a; Bandiera et al. 2009; Jackson 2017; Hanna and Linden 2014). To test for risk and trust, we randomly assign some managers to be absent from the site.

3 Isolating Hiring Mechanisms: An Experimental Approach

3.1 From Theory to Empirics

This study investigates how network connectedness of employee and employer structures differentially affects productivity and hiring outcomes (y), as discussed in Section 4. We generate random variation in both employer and employee structures through three treatment conditions, enabling us to test the predictions derived from the theoretical model. This section also describes the collected data and outlines the construction of our key measures, including productivity (y) and connectedness (c)

3.2 Experiment: Information, Incentive, and Monitoring Treatments

We designed a field experiment to test the two hypothesized mechanisms underlying social network-based hiring: connectedness reduces information frictions; and connectedness generates match specific returns. The randomized controlled trial was conducted in partnership with construction firms in Lahore, Pakistan, where workers are typically employed on short-term contracts. Our experimental design isolates each mechanism by randomizing key aspects of employer-employee interactions across three treatment arms—information, incentives, and monitoring—allowing us to assess their relative contributions to employers' hiring decisions.

- **Information Treatment** For workers with above-average productivity at baseline, we randomly selected a subset and provided their names and contact information to managers, informing managers that these were high-performing laborers.[10] Each manager received a list of 2–6 above-average laborers drawn from three sources: (i)

[10]Workers were randomly selected within subsample defined at the task-site and time-frame level.

their own teams, (ii) laborers hired via labor stands, and (iii) workers from other construction sites.[11] Each treated worker appeared on at least two manager's list, resulting in 290 treated and 150 control laborers. The information set additionally included average site and task-level productivity benchmarks. The treatment was randomized at the manager-laborer dyad level, with implementation commencing on May 2, 2020.[12]

- **Incentive Treatment** Managers were offered performance-based incentives tied to the average productivity of their teams on a given day. For each manager, we identified active work assignments eligible for the incentive treatment.[13] Managers were informed of each job's treatment status prior to worker allocation each day.[14] Incentive targets were calibrated using historical productivity data at the task and site level within the relevant time frame to ensure they were ambitious yet attainable.[15] Table 1 presents the incentive structure and associated targets.

Table 1: Incentive Treatment: Performance Targets and Corresponding Incentive Levels

Performance Group	Target Percentile	Incentive (% of Manager Salary)
Average	0–75th	0
Above-average	76–90th	5
Significantly above-average	91–100th	10

Notes: Targets are defined based on historical productivity percentiles at the task and site levels. Incentive amounts were calibrated using the current average monthly manager salary in the sample and verified with information provided by site owners.

The treatment was randomized at the manager-laborer dyad and job-day level with equal probability of assignment to treatment or control. Payments were contingent on meeting predefined productivity thresholds, conditional on passing quality checks.[16] The incentive treatment commenced in February 2021 and was implemented for 367 treated and 383 control jobs.[17] A detailed breakdown of treatment allocation is provided in Appendix A3, Table A4.

- **Monitoring Treatment** We vary whether managers perform a monitoring role for the team they hire on a given day by temporarily removing managers from the work-

[11] To expand the pool of unconnected workers while preserving site dynamics, laborers already employed by other managers on the same site were excluded.

[12] The RCT is ongoing, and information treatments continue. See Table A4 for the distribution of treatments.

[13] Field teams maintained a continuous presence on-site, minimizing risks of misreporting by managers.

[14] Notification occurred before labor assignments to prevent selection bias.

[15] Percentile-based targets were explained to managers, including a breakdown of component benchmarks.

[16] No incentives were awarded if work failed to meet quality standards (e.g., chipped or misaligned tiles, uneven surfaces, or excessive waste), as verified by an independent quality supervisor.

[17] The treatment is ongoing.

site. Manager presence was varied by inviting managers to attend off-site sessions on randomly assigned days. Managers reported their weekly availability in advance, and treatment was randomized at the manager-day level conditional on availability. The treatment commenced on March 22, 2022, covering 63 manager-days[18] We supplement the experimental variation with observational analysis of naturally occurring variation in manager presence due to limited data on this treatment arm.

Treatments were assigned to laborers and managers engaged in brick, tile, and block work tasks. The information treatment was followed by the incentive treatment and then monitoring treatment was implemented. Due to site-specific workflow constraints, some managers and laborers were exposed only to the earlier treatments. We present the results separately for each treatment to examine how effects differ for workers connected versus unconnected to their hiring managers.[19]

Construction firm owners, site supervisors, and managers were briefed on the RCT design and data collection protocols. After obtaining approval, managers attended group sessions covering study objectives, output measurement, and productivity score calculations. Information about high-productivity workers was communicated in individual meetings, as were details on incentive structures and targets. On treatment days, incentive designs and targets were reiterated in daily briefings. Managers also attended individual sessions explaining productivity measurement tools, during which feedback on observation instruments was collected.

3.3 Timeline and Data

The study uses data collected from January 2020 to June 2025,[20] covering at least one complete construction cycle per site. The same employers and employees were observed before and during the RCT. Figure A6 illustrates the timeline of data collection and treatment implementation. The dataset comprises daily observations of workers, workers surveys, and manager surveys, collected both at baseline and during the RCT. These data allow us to examine the mechanisms through which social networks influence hiring decisions by tracking workers' search behavior, effort, productivity, and time allocation across treatment conditions.

Worker Daily Observations We conducted full-day observations of workers engaged in brick, tile, and block work at partner construction sites using an employee observation tool. The tool records detailed, task-level data on an hourly basis, capturing both output and time allocation. Output measures include the number of bricks, tiles, and blocks

[18] See Table A4 for the distribution of treatment allocation. The intervention is ongoing.

[19] As data collection continues, we will present pooled results across all three treatments.

[20] Data collection and treatment implementation are ongoing; analyses will be updated as additional data become available.

handled; paste bowls prepared; materials discarded (with reported reasons); tile and bowl dimensions; and total area completed. The tool also records methods and equipment used for material transport, including equipment size. Time allocation measures include work time and time spent on breaks, interpersonal assistance, interruptions, and interactions with site managers. Additionally, the tool captures supervisory visits, communications with managers, and subjective performance ratings. Field assistants supplemented these records with narrative notes to contextualize daily work patterns. Laborer observations were conducted from May 2021 to June 2025, covering both baseline and RCT phases. At baseline, each laborer was observed for at least three full workdays.

Worker Survey A baseline survey collected employee's information on demographics, contractual arrangements, payment mechanisms, job search behavior, income, work history, social networks, and workplace monitoring practices. The survey also measured laborers' social ties with other workers, supervisors, and hiring managers. Surveys were administered in person in the local language.[21] During the RCT, shortened phone surveys tracked employment outcomes in two rounds (2024 and 2025), measuring job search effort, days worked in the past week, income, and job offers received.

Manager Survey A survey of 87 managers collected data on their beliefs about worker productivity, labor search processes, social networks, and demographic characteristics. The survey was conducted prior to monitoring treatment implementation.

Measurement of Workers Productivity A key challenge in the recent literature lies in accurately measuring laborer productivity. Existing studies measure laborer productivity using stylized tasks in controlled or artificial settings. We add novelty by conducting comprehensive in-person observations of workers during routine construction activities. Construction work varies widely in quality and quantity, so we piloted work processes extensively to identify tasks that are relatively standardized, exhibit minimal quality variation, and are easily quantifiable. We focused on brick, tile, and block work (including associated plaster), decomposing these tasks into discrete steps to capture both the quantity performed and the efficiency with which tasks were completed.[22]

We designed an observation tool to quantify workers' daily productivity with hourly activity data.[23] This approach yields a high-frequency dataset that closely captures

[21] Instruments were pilot-tested and refined through iterative field team debriefings.

[22] Construction work is organized by distinct roles and stages, complicating direct productivity comparisons across task types. Many tasks also exhibit unobservable quality variation. For instance, a worker passing bricks quickly may damage materials, a quality difference not evident when measuring only the number of bricks laid by day's end.

[23] Each enumerator observed one worker per day. The tool was developed through consultations with architects, construction firms, managers, and workers. Enumerators received one week of training and were required to achieve 90% accuracy during a one-week pilot before beginning data collection. Field

workers' daily productivity. Our productivity measure incorporates both the rate at which laborers complete their tasks and total time spent at work each day, controlling for day, enumerator and equipment level productivity shocks. Specifically, productivity is measured as the rate of bricks, tiles, or blocks placed per minute, multiplied by total minutes worked (end time minus start time minus break and idle time). The observation tool also records time spent on ancillary tasks such as preparing paste or transporting materials. This rich information allows us to convert time spent on these secondary tasks into a comparable productivity unit. Baseline productivity was calculated from at least three observation days per laborer. We also collected managers' subjective productivity assessments of workers on a 10-point Likert scale.

Figure A8 presents the conditional relationship between productivity in period t and its lagged. The data exhibit strong positive autocorrelation in worker productivity, with the persistence evident throughout the productivity distribution rather than concentrated at particular quantiles. Workers in the upper tail of the lagged productivity distribution demonstrate a marked propensity to remain in the upper tail in the subsequent period, while those in the lower tail similarly persist. This pattern suggests that a substantial component of observed productivity variation reflects permanence or highly persistent worker-specific heterogeneity rather than transitory shocks. Figure A22 presents binscatter plots examining the relationship between our productivity measure and its components. Productivity is strongly and positively associated with brick per minute and plaster per minute residuals, indicating that workers who perform these tasks at higher rates are substantially considered more productive, even after accounting for day, equipment, and enumerator fixed effects. Most variance in productivity stems from differences in output rather than time inputs. A Shapley decomposition (Allen and Arkolakis 2014; Audoly et al. 2025) confirms this: brick per minute and plaster per minute residuals account for 68.6% and 31.0% of total R-squared, respectively, while time-based variables contribute negligibly (Table A30). This indicates that, once fixed effects are netted out, productivity differences are almost entirely driven by variation in task-specific efficiency.[24]

3.4 Data Descriptives: Workers and Managers

Workers The study is conducted in collaboration with construction firms in Pakistan. Table A2 reports summary statistics for our sample of 1,500 laborers,[25] alongside a representative sample of short-term wage workers employed in construction (or similar) work in Punjab from the 2021 Labor Force Survey. Laborers in our sample are relatively young,

supervisors monitored data collection daily, reviewed forms for completeness and accuracy, and held regular debriefings. We chose in-person observation over video recording due to equipment costs, site damage risks, and weather exposure.

[24] Without controlling for fixed effects, brick per minute and plaster per minute still remain the primary determinants of explained variance.

[25] This draft includes data from rounds 2 to 19.

with a mean age of 26.5 years; approximately half are married, and 36 percent have completed at least secondary education (compared to 28 percent nationally). Only 15 percent report Lahore as their hometown, indicating that most have migrated for work. Their average daily income is 3.32 USD. On average, laborers have worked with their current manager for about one year (384 days) at this or other sites and have approximately seven years of total labor experience (2,691 days) with any managers. Formal training is rare, with only 21 percent having received any, and there is substantial variation in training duration. Laborers worked 17.8 days in the past month and spent 16 days searching for their current job, highlighting frictions in job matching. Personal networks dominate job search, with 91 percent of workers finding work through connections in general and only 9 percent through labor stands (Figure A11b). Additional measures—including commute time and cost, task allocation, and payment mechanisms—are also summarized in Table A2.

Table A3 reports baseline observations for 4,862 worker-days. On average, laborers passed six bricks and prepared and passed 0.65 bowls per minute. Productivity exhibits substantial variation even for these relatively simple tasks: the productivity score has a mean of 0.0002 (SD = 0.68) and ranges from 4.37 to 7.05 (the productivity distribution is presented in Figure A7).

Managers To assess the role of social networks under different treatment conditions, we first examine managers overseeing brick, tile, and block work, who are responsible for hiring, supervising, and monitoring laborers. Table A1 presents their characteristics and managerial practices. Managers are, on average, 38.3 years old; 92% are married, 59% have completed secondary education, and 36% are from Lahore. They report 3.09 years (1,114 days) at their current site and 13.49 years (4,858 days) of total experience, managing an average of 2.2 sites concurrently. The majority (97 percent) reported using their social networks to search for laborers, in general, reflecting the importance of social connectedness in employability and worker search. In contrast, they rely only 9% of them times to find laborers through labor stands, and 3% used other means (Figure A11a). On average, managers spent two days in the past month searching for laborers. Monitoring intensity varied: 60% monitored hourly, 15% continuously, 18% twice daily, and 7% at the end of the day.

Connectedness, Wages, Search Effort, Time Allocation, Productivity Our descriptive evidence show that there is no productivity differential associated with network connectedness—a one standard deviation increase in connectedness corresponds to merely a 0.008 standard deviation increase in productivity ($p > 0.10$), suggesting that network-based hiring does not operate through superior ability matching (see Table A10). The point estimate implies that moving from the 25th to the 75th percentile of the connected-

ness distribution would yield less than a 0.01 standard deviation change in productivity—an economically negligible effect. This conclusion is reinforced by the absence of wage premiums: connectedness yields no statistically significant effects on either daily wages (0.020 SD, SE=0.015) or monthly income(0.820 SD, SE=0.93).

The primary channel through which networks operate manifests in time allocation patterns. Connected workers exhibit significantly longer work hours, spending an additional 6.033 minutes per day on productive tasks ($p < 0.01$), which aggregates to approximately 2.0 hours per month or 24 hours annually. Concurrently, they reduce peer socialization by 9.380 minutes daily ($p < 0.01$), suggesting a reallocation of time from social activities to work. Job search activity decreases by 1.395 minutes per day ($p < 0.05$), reflecting enhanced employment security and reduced need to find alternative employment. Notably, time spent socializing with managers remains negligible and statistically indistinguishable from zero (0.061 minutes, $p > 0.10$), indicating that maintaining network advantages does not require ongoing relationship cultivation with supervisors. Demographically, connected workers demonstrate higher educational attainment (typically secondary level or above) and less likely to be married, potentially reflecting greater geographic mobility or different life-cycle stages that facilitate network formation and maintenance.

Beliefs about Workers Productivity Figures A9 and A10 present binned scatter plots of workers' actual productivity against managers' subjective beliefs (rated 1–10), separately for connected and unconnected workers before and after the treatments. The descriptive evidence shows that the relationship between beliefs and productivity is essentially flat for both groups prior to the intervention, indicating limited predictive power. Considerable heterogeneity exists across all manager rating levels, particularly among connected workers with low-to-moderate ratings (1-3). Unconnected workers show relatively tighter clustering. Prior to treatment, managers' beliefs capture only partial information about true productivity, with accuracy weakest at lower rating levels and modestly stronger at higher levels. After the information treatment, there is a strongly positive correlation between manager rating and worker productivity among connected workers, implying that managerial ratings begin to align more closely with actual productivity among connected workers. By contrast, the relationship for unconnected workers remains weak or even slightly negative. This pattern suggests that managers are more likely to update their beliefs about worker ability for connected workers only after the treatment.

3.5 Intervention Fidelity

In this section, we address potential concerns related to the implementation of the experiment and provide supporting evidence. First, we show that baseline covariates are

balanced across the information, incentive, and monitoring treatments (see Appendix Section A4). Second, we examine sample attrition (Appendix Section A5). Finally, we present evidence on managers' understanding of the treatments (Table A5).

We report covariates balance across the three treatment arms in Appendix Section A4. Overall, covariates are balanced at the laborer level for the information treatment (Table A7). A small number of covariates—daily wage and number of days worked in the past month—are significant at the 10% level, consistent with what would be expected by chance. Because jobs cannot be observed prior to treatment assignment, we do not have pre-treatment laborer- job level data for the incentive and monitoring treatment. Instead, we assess balance across treatment and control groups along location and day of treatment allocation. Figure A12 presents these results for the incentive treatment, while Figure A15 shows the monitoring treatment. Overall, we find balance across days and locations for both treatments. The incentive treatment shows a minor location imbalance at the 10% significance level, and the monitoring treatment exhibits marginally higher assignment on Wednesdays and Mondays (5% and 10% levels, respectively). These small imbalances are consistent with random variation, and we control for days and locations in all specifications to ensure robustness.

Laborers typically work on short-term contracts, often lasting only few days, which generates high turnover in the sample. Follow-up surveys were conducted by phone, and we pool data from the two follow-up rounds to examine attrition by treatment status (Table A8). We find no evidence that attrition is systematically related to the information treatment. In addition, attrited workers do not differ significantly in baseline covariates or in their interactions with treatment status.[26] Attrition is negligible for the incentive treatment (0.66%).[27] Attrition also does not appear to be systematically related to the monitoring treatment (Table A9). However, attrition rates vary across days of the week—for instance, attrition is higher on Mondays, Tuesdays and Thursdays for treatment groups. To address this, we also estimate results using the Lee bounds approach to account for differential attrition across treatment groups.

Managers demonstrated understanding of treatment arms. We assessed their comprehension of information treatment and find that 80% reported the shared information to be useful, and 37% had contacted and offered work to workers for whom the information was provided (results presented in Table A5).[28]

[26]The time lag between baseline survey and phone survey are presented in figures A23c, A23d, A23a, and A23b.

[27]We are still processing additional data, which will add approximately 85 worker-days observations to the analysis. Preliminary checks indicate that attrition in this sample is not systematically related to the incentive treatment.

[28]We report only statistics for which sample sizes are sufficient.

4 Results

We investigate two mechanisms underlying employers' reliance on social networks in hiring: information asymmetries and match-specific returns. First, we test whether information asymmetries drive network hiring by providing managers with objective productivity signals in the information treatment and examining whether this increases hiring of high-performing unconnected workers. Second, we use the incentive and monitoring treatments to test for match-specific returns. The incentive treatment introduces performance-based pay to test whether financial rewards can overcome social preferences or private utility from hiring connected workers, examining if compensation attenuates employers' non-pecuniary preferences. The monitoring treatment randomly removes managers from worksites to identify match-specific productivity effects, examining whether productivity advantages for connected workers depend on their manager's presence. We further investigate whether hiring preferences reflect manager-specific private information by analyzing whether connected workers' employment advantages persist when their hiring manager is absent.

4.1 Information Asymmetry: Who Gains from Productivity Information

To test the information asymmetry channel, we implement an information treatment that provides managers with productivity signals about workers. We evaluate the effect of this intervention on workers' search effort, employment, and earnings. Our primary analysis examines heterogeneous treatment effects by connectedness, assessing whether providing productivity information increases employment opportunities for less-connected workers.

Specification We estimate heterogeneous treatment effects at the laborer level using the following specification:

$$Y_{ijt} = \beta_0 + \beta_1 Connected_{ij} + \beta_2 Info_{ij} + \beta_3 \left(Info_{ij} \times Connected_{ij}\right) + \varepsilon_{ijt} \tag{2}$$

where Y_{ijt} denotes the outcome for laborer j at followup ($t = 1$). Outcomes include search effort (an indicator for whether the worker searched for work during period t (i.e., today or yesterday), employment measures (days worked, hours worked, and a binary indicator for employment on the survey day), and income (self-reported daily and monthly wages). Our coefficient of interest, β_3, captures the differential effect of the information treatment for connected relative to unconnected workers. The specification includes site and enumerator fixed effects, and includes controls for baseline outcomes. Individual (la-

borer) fixed effects are not separately identified in this design: the information treatment is assigned once per laborer and the estimation sample contains a single post-treatment observation per laborer, so laborer fixed effects would mechanically absorb the treatment indicator. The ANCOVA specification with baseline-outcome controls is therefore our preferred estimator (McKenzie 2012). Finally, we control for laborer-level characteristics selected via the post-double-selection Lasso procedure (Belloni et al. 2014). Standard errors clustered at the laborer level, the unit of randomization.

Results Table A11 presents the results of the information treatment on search effort, employment, and income, including fixed effects. Heterogeneous treatment effects by connectedness are reported in Table A12 with fixed effects and in Table A13 with fixed effects and additional controls selected using post-double-selection LASSO. Tables A14 and A15 present Lee bounds estimates for connected and unconnected workers, respectively, to account for differential attrition across treatment groups.

Search Effort Several months following the information treatment, treated laborers report lower search effort on the survey day (0.009 SD) and a day prior (0.011), though these effects are substantively small and not statistically significant. A similar pattern emerges among treated workers with higher connectedness, but point estimates remain indistinguishable from zero. Lee bounds in Tables A14 and A15 confirm search effort remained near zero for both connected and unconnected workers across all specifications.

Employment The information treatment influenced employment patterns differentially by connectedness. While the overall treatment effect on days worked is negligible (−0.096 days), connected workers experienced a 0.256 day reduction (not statistically significant). Connected workers reported fewer hours per day in the previous month (−0.603 hours, significant at 5%). The likelihood of being employed on the survey day declined by 0.093 for connected treated workers, though this effect is not statistically significant.

Income The information treatment generates wage gains concentrated entirely among connected workers. Treated workers overall report $9.904 higher monthly income and $0.316 higher daily wages, though these effects are not statistically significant. However, the interaction between connectedness and the information treatment reveals that these gains accrue exclusively to connected workers: the interaction term indicates an additional $14.533 increase in monthly income and $0.429 increase in daily wages (both significant at 5%) for connected workers. Lee bounds in Table A14 confirm these wage and income gains are robust for connected workers, with the lower bound at 16.873 USD (SE = 10.978) and upper bound at 28.057 USD for monthly income, and bounds of 0.529 USD to 0.855 USD for daily wages. In contrast, unconnected workers experienced no

measurable income benefits from the treatment.

The information treatment reveals that productivity signals and social networks operate as complements rather than substitutes. Once productivity information became available, connected workers captured substantial wage premia more closely aligned with their productivity while reducing work hours, they leveraged the information to improve match efficiency and extract rents. These benefits concentrated exclusively among connected workers, indicating that productivity signals are most effectively translated into labor market gains when workers possess the social capital to leverage such information. Descriptive evidence suggests managers updated beliefs about worker productivity only for connected workers post-treatment, with these updated beliefs more closely aligned with objective productivity measures. This pattern suggests that connectedness enables longer-term relationships between employers and employees: once managers identify high-productivity connected workers, they invest in the relationship through higher wages, anticipating continued access to reliable, high-quality labor. These findings suggest information interventions enhance the value of existing network connections rather than equalizing opportunities between connected and unconnected workers. Our results contrast with worker-directed information interventions that typically produce modest effects on search and employment (Dammert et al. 2015; Abebe et al. 2025; Knaus et al. 2017), and align instead with evidence that credible signals reducing employer uncertainty influence wages (Pallais 2014; Bassi and Nansamba 2021; Hou 2024; Harris 2015).

4.2 Match-Specific Returns

4.2.1 Can Performance Incentives Overcome Social Preferences from Network Hiring?

In this section, we examine whether incentives can attenuate managers' tendency to hire socially connected workers. Leveraging the incentive treatment arm of our experiment, we test whether managerial incentives alter hiring decisions for incentivized jobs.

Specification We test whether productivity-based incentives reduces managers' propensity to hire highly connected workers. Specifically, we estimate the following specification:

$$Y_{ijlt} = \beta_0 + \beta_1 Connected_{ij} + \beta_2 Incentive_{ilt} + \beta_3 \left(Incentive_{ilt} \times Connected_{ij}\right) + \varepsilon_{ijlt} \tag{3}$$

where Y_{ijlt} is an indicator equal to one if laborer j is hired by manager i for location l on day t under the incentive treatment. We also examine the effect of incentive treat-

ment on productivity, effort and managers supervisory behavior. Connected_{ij} measures the social connectedness between worker j and manager i, and Incentive_{ilt} is an indicator for assignment to the incentive treatment. The coefficient on the interaction term $\text{Incentive}_{ilt} \times \text{Connected}_{ij}$ captures the differential effect of incentives on hiring probabilities by worker connectedness. All specifications include site, enumerator, location, and day fixed effects, with standard errors clustered at the manager-location-day-laborer level. As a robustness check, we re-estimate the productivity specification with laborer fixed effects, which are identified because incentive assignment varies across days within laborer; the time-invariant connectedness level is absorbed while the treatment effect and its interaction with connectedness remain identified (Appendix Table **??**). Standard errors are clustered at the manager–location–day–laborer level, corresponding to the unit of randomization. We report both conventional and randomization-inference p-values.

Results We present results on how performance-based incentives affect managers' hiring decisions and workers' productivity and behavior. Table A16 presents results examining whether managers are more likely to hire unconnected workers for incentivized positions. Table A17 presents findings testing whether higher-productivity workers are systematically assigned to incentivized jobs, using baseline productivity measures as the key explanatory variable. Table A18 estimates the effect of incentives on worker productivity, including heterogeneous effects by social connectedness to managers. Tables A20 and A21 examine how workers adjust their working and non-working time in response to incentives, shedding light on the mechanisms underlying productivity changes. We report results from fixed effects, and post-double-selection (PDS) Lasso specifications. We also check the robustness of our results using a bootstrap method.

Hiring We find no evidence that incentives alter hiring patterns based on social connectedness (Table A16). Across all specifications, the coefficients on the incentive treatment, connectedness, and their interaction are essentially zero with very small standard errors, indicating precisely estimated null effects. The baseline hiring probability is 0.1%, reflecting the large pool of potential workers relative to available positions. The results suggest that managers do not systematically favor connected workers in hiring decisions, and performance incentives do not meaningfully shift this behavior. Furthermore, we find no evidence that managers systematically assign higher-productivity workers to incentivized positions. These findings suggest that neither worker ability nor social ties drive managerial allocation decisions in response to productivity-based incentives.

Productivity Tables A18 and A19 examines how monetary incentives and social connectedness with managers influence worker performance, measured by comprehensive measure of productivity and total output (number of bricks and tiles passed). Across

specifications, the incentive treatment alone has no significant effect on either outcome, suggesting that monetary rewards do not, on average, increase worker output. Connectedness, by contrast, is positively and weakly associated with productivity—workers who are more socially tied to their managers perform better under baseline conditions—though this relationship does not extend to total output, where the coefficient is small and negative. The interaction between connectedness and incentives is negative in the productivity regressions but positive in the output regressions, though both coefficients are statistically insignificant. This pattern suggests that social ties may enhance intrinsic motivation and productivity absent financial rewards, while formal incentives may partially attenuate these relational motivations in the short term, consistent with motivation crowding theory. However, the positive interaction in output models hints at potential complementarity over longer time horizons, where incentives and relationships together may sustain cumulative effort. Results are consistent across both fixed effects and post-double-selection LASSO specifications, indicating robustness to model selection.

Time Allocation Tables A20 and A21 report the effects of incentives and social connectedness on worker time allocation, distinguishing between working time (minutes actively engaged in task) and non-working time (minutes spent idle or away from task). Across both fixed effects and post-double-selection LASSO specifications, the incentive treatment alone has no statistically significant impact on either working or non-working time, suggesting that monetary incentives do not, on average, alter how workers allocate time during the workday. By contrast, connectedness with the manager shows a negative association with working time (−10.4 minutes, $p < 0.10$) and a positive, though imprecise, association with non-working time (+8.7 minutes), indicating that more connected workers spend less time actively working and somewhat more time idle under baseline conditions—potentially reflecting greater comfort, trust, or discretion within the workplace relationship. The interaction terms between connectedness and the incentive treatment reveal an opposite pattern: connected workers increase working time (+6.5 minutes, not significant) and reduce non-working time (−10.1 minutes, $p < 0.10$) when incentives are introduced. This suggests that financial incentives realign effort allocation for connected workers, increasing time on task. Overall, these results indicate that while connected workers may exercise greater flexibility in effort allocation absent explicit rewards, the introduction of incentives activates latent effort capacity, resulting in reduced idle time. This pattern is consistent with the hypothesis that socially connected workers respond more strongly when monetary rewards are layered onto existing relational ties.

Monitoring Table A22 examines managerial monitoring behavior, measured by the number of visits managers make during the workday, as a function of incentives and manager–worker connectedness. Across both specifications, no coefficient attains statis-

tical significance. The null results indicate that monitoring intensity remains invariant to both treatment assignment and relationship characteristics. The stability of managerial oversight across conditions implies that observed performance responses to incentives or connectedness operate through worker effort rather than changes in supervision, pointing to worker-side mechanisms as the primary channel through which these factors influence productivity.

Overall, our findings indicate that productivity-based incentives show limited effects on both worker effort, productivity, managerial hiring and supervision. Whereas some studies find that performance pay can substantially increase output (Lazear 2000; **?**), our evidence aligns more closely with research documenting the limits of incentives in relational or complex work environments (**?**). The lack of differential hiring patterns by connectedness under incentive schemes is consistent with findings on the durability of social ties in hiring decisions (Fisman et al. 2017), and workers' minimal time allocation responses suggest that the incentives offered were not sufficient to alter behavior (**?**). These results suggest that simple bonus schemes may be insufficient to overcome managerial preferences for connected workers or substantially modify effort, pointing to the importance of context-specific incentive design.

4.2.2 Are Socially Connected Worker-Manager Pairs More Productive?

We identify match-specific productivity effects by exploiting non-experimental and random variation in on-site managerial supervision. The monitoring treatment decouples hiring authority from day-to-day oversight, generating exogenous variation in whether workers are supervised by managers to whom they are socially connected versus unconnected managers. This institutional feature allows us to test whether social connections between workers and their direct supervisors generate productivity complementarities distinct from general ability differences.

Specification We estimate the differential effect of connected supervision on worker output using the following specification:

$$Y_{ijt} = \beta_0 + \beta_1 Connected_{ij} + \beta_2 Monitor_{it} + \beta_3 \left(Monitor_{it} \times Connected_{ij}\right) + \varepsilon_{ijt} \tag{4}$$

where Y_{ijt} denotes the productivity of worker j hired by manager i on day t, $Monitor_{it}$ is an indicator for the absence of manager on day t, and $Connected_{ij}$ measures the social connectedness between worker j and manager i. The coefficient on the interaction term $Monitor_{it} \times Connected_{ij}$ identifies the differential productivity effect of connected workers

under managerial supervision, isolating match-specific complementarities from general productivity differences. We include site, enumerator, and day fixed effects to account for systematic variation across these dimensions, and control for baseline worker ability measured prior to randomization. Standard errors are clustered at the manager-day level, corresponding to the unit of treatment assignment.

Results We assess match-specific productivity complementarities using two empirical strategies that exploit different sources of variation in managerial supervision. First, we analyze observational variation in managers' on-site presence during standard operations. While manager attendance is endogenous—determined by managers' optimization over competing time demands—this variation provides initial evidence on whether worker productivity varies with their hiring manager's presence and whether such effects differ by worker-manager connectedness. Results are presented in Table A23. These estimates should be interpreted as descriptive associations rather than causal effects, as unobserved factors correlated with both manager presence and worker productivity may confound inference.

Second, we exploit experimental variation generated by the monitoring treatment, in which managers were randomly removed from the worksites. This approach provides a clean identification strategy under weaker assumptions. The experimental sample is substantially smaller than the observational sample, limiting statistical power. We estimate treatment effects using both intent-to-treat specifications with fixed effects (Table A24) and two-stage least squares to account for potential non-compliance (Table A25). Comparing observational and experimental estimates allows us to assess whether endogeneity concerns substantially bias the non-experimental associations.

Productivity The coefficient on manager presence is negative and statistically significant ($\beta = -0.169$, $p < 0.01$), indicating that higher managerial presence is associated with lower worker productivity. Connectedness exhibits no direct effect on productivity. The interaction term between manager presence and connectedness is positive but not statistically significant ($\beta = 0.053$, SE = 0.050), suggesting that while social ties may partially attenuate the negative effect of managerial presence, this moderation is not statistically distinguishable from zero (Table A23). The point estimate implies that for connected worker-manager pairs, manager presence is less negatively associated with productivity (total effect of -0.116) than for non-connected pairs (total effect of -0.169). When monitoring is conducted by supervisors rather than hiring managers, the pattern changes markedly: the main effect of supervisor presence becomes smaller and statistically insignificant ($\beta = -0.026$, SE = 0.040), and the interaction with connectedness turns negative though remains insignificant ($\beta = -0.029$, SE = 0.043), suggesting that the relationship between monitoring and productivity may depend critically on the identity of

the monitor.

Tables A26 and ?? reports experimental estimates from the monitoring treatment. The coefficients are identical across specifications, reflecting perfect compliance. Neither the main effect of manager removal ($\beta = 0.014$, SE = 0.147-0.213) nor the interaction with connectedness ($\beta = -0.010$, SE = 0.323-0.469) approaches statistical significance. The experimental results provide no evidence of match-specific productivity complementarities, though the small sample limits the precision of inference.

The experimental point estimates are directionally consistent with the observational findings. The substantially reduced sample size in the experimental analysis limits our ability to detect modest effects. These results align with research documenting weak monitoring effects absent complementary management systems (Bandiera et al. 2007; Bloom et al. 2013) and with Breza (2017), who find that social connectedness does not significantly moderate supervision effectiveness in similar settings. The contrast with studies finding substantial monitoring impacts (Callen et al. 2020; McKenzie 2012) likely reflects contextual heterogeneity in oversight efficacy.

4.2.3 Do Managers Exhibit Higher Trust or Reduced Uncertainty for Connected Workers?

Lastly, we test whether greater trust or reduced uncertainty drives network-based hiring by exploiting variation in managers' on-site presence. If managers perceive lower risk when hiring connected workers, this should manifest when managers exercise hiring discretion. We estimate whether connectedness more strongly predicts hiring when managers are physically present:

$$Y_{ijt} = \beta_0 + \beta_1 Connected_{ij} + \beta_2 Monitor_{it} + \beta_3 \left(Monitor_{it} \times Connected_{ij}\right) + \varepsilon_{ijt} \tag{5}$$

Where Monitor_{it} serves as a dummy variable indicating the monitoring treatment status for manager i at time t, and $\text{Monitor}_{ilt} \times \text{Connected}_{ij}$ represents the interaction term capturing how monitoring differentially affects the hiring of connected versus unconnected workers. The model includes fixed effects and controls for employees' baseline productivity to account for pre-existing differences in worker quality. The main outcome variable, Y_{ijlt}, measures the hiring of laborer j by manager i on day t during the monitoring treatment period. To address potential correlation in hiring decisions made by the same manager on the same day, standard errors are clustered at the manager-day level, which corresponds to the unit of randomization in the experimental design. The analysis reports conventional standard errors with asymptotic p-values.

4.2.4 Results

We examine whether managers exhibit reduced uncertainty toward connected workers by exploiting observational variation in manager presence. Table A27 exploits observational variation in on-site presence to test whether connectedness more strongly predicts hiring when managers are present. We complement this with experimental evidence from random manager removal (Tables A28–A29).

Hiring Table A27 analyzes worker-day observations to test whether connectedness more strongly predicts hiring when managers are physically present. The interaction between manager presence and connectedness is positive and significant ($p < 0.01$). This pattern indicates that connected workers' hiring advantage emerges specifically when their manager is present on-site to exercise hiring discretion. When managers are absent, the hiring gap between connected and unconnected workers disappears. These results align with findings from the RCT sample as well though not statistically significant.

The economic mechanism reflects information asymmetry and non-transferable knowledge. Managers accumulate private information about connected workers through social networks—knowledge about reliability, work habits, and trustworthiness that productivity measures cannot capture. This information reduces behavioral uncertainty in hiring decisions but cannot be credibly transmitted to substitute decision-makers. When the connected manager is absent, other personnel lack this information advantage and hire based on observable characteristics, eliminating the differential. Conditioning on productivity and fixed effects, social ties function as a private information channel that reduces hiring risk only when the informed manager exercises choice.

These findings align with theoretical models emphasizing that networks reduce informational frictions in labor markets (Montgomery 1991; Calvó-Armengol and Jackson 2004). Our results demonstrate that network-based information advantages are manager-specific and non-transferable, consistent with Pallais (2014)'s finding that employer learning about worker quality remains firm-specific. The pattern contrasts with explanations based on statistical discrimination (Altonji and Shakotko 1987) or pure homophily (McPherson et al. 2001), as the hiring advantage operates specifically through the connected manager's presence. The evidence supports models where social ties provide credible signals about unobservable worker characteristics (Dustmann et al. 2016). The dependence on manager presence relates to **?**'s finding that social connections between managers and workers affect personnel decisions through information channels rather than favoritism. Our contribution is demonstrating that this information remains personal to the manager and non-codifiable, explaining why networks persist in labor markets even when objective performance data exists.

5 Conclusion

We present experimental evidence from a randomized controlled trial involving 87 construction managers and 1,500 workers in Lahore, Pakistan designed to identify the mechanisms underlying network-based hiring in short-term labor markets. Through three targeted interventions—providing objective productivity information, implementing performance-based managerial incentives, and randomly varying on-site supervision—we distinguish between information asymmetries about worker productivity and match-specific returns through productivity complementarities or social preferences.

Our findings reveal a striking pattern. At baseline, connected workers exhibit no productivity advantage over unconnected workers, yet managers overwhelmingly rely on network-based hiring. Providing productivity information does not shift hiring toward high-productivity unconnected workers. Instead, connected workers capture higher wages while reducing hours worked. Performance-based managerial incentives similarly produce null effects on hiring composition. Moreover, the connected worker hiring premium disappears entirely when managers are absent from worksites, yet worker productivity remains statistically indistinguishable across manager presence conditions.

These results demonstrate that network hiring reflects relationship-specific reputational information that complements rather than substitutes for productivity information. Managers do respond to productivity data when provided—connected workers who are revealed to be high-productivity receive higher wages. In addition, managers also update their beliefs about connected workers. However, this information does not reduce the hiring advantage of connected workers or increase hiring of high-productivity unconnected workers. This pattern suggests networks convey information about worker reliability—specifically, attendance and responsiveness to short-notice hiring—that is valuable when productivity is unknown. However, once productivity information becomes public they further value it. Prior research has emphasized either information asymmetries about productivity (Rees 1966; Montgomery 1991; Calvó-Armengol 2004) or match-specific productivity gains (Akerlof 1982; Bandiera et al. 2009). Our experimental design reveals that networks transmit reputational capital about non-contractible behaviors which is further valued when productivity information becomes available.

The implications for labor market inequality are profound. Employment disparities reflect differential access to relationship-specific reputational capital rather than productivity differences or discrimination (Munshi 2003; Chandrasekhar et al. 2020). Workers without existing connections face fundamental barriers: they cannot build reputational capital without employment, yet cannot access employment without reputational capital. Our findings also explain why informational interventions often fail to reduce network reliance. Productivity information alone cannot substitute for relationship-specific knowledge about reliability. The commitment problems inherent in short-term labor

markets—workers' inability to commit to availability, managers' inability to enforce attendance—make relationship-specific information valuable precisely because it cannot be credibly communicated through formal mechanisms. Policies aimed at reducing hiring disparities must address underlying contracting frictions rather than simply improving information transmission.

Our findings point to several directions for future research. What interventions might enable unconnected workers to build reputational capital? Digital reputation platforms, temporary wage subsidies, or performance bond contracts could help overcome these barriers. How do network dynamics evolve as labor markets formalize, and do similar mechanisms operate in high-income countries? More broadly, our results highlight the challenge of designing labor market institutions in developing economies. The prevalence of network hiring reflects the absence of institutions that enable workers to credibly signal reliability, suggesting that reducing employment inequality requires institutional reforms that address commitment problems rather than information frictions alone.

References

Stefano Caria, Simon Franklin, and Marc Witte. Searching with friends. Discussion Paper 13857, IZA, 2020a. URL https://www.iza.org/publications/dp/13857/searching-with-friends.

Marc Witte. Why do workers make job referrals? experimental evidence from ethiopia. Technical report, 2021. Retrieved Jan 17, 2021, from https://www.dropbox.com/s/8zs4h4u8imsh9c5/JobReferrals_2021_Witte2.pdf?dl=0.

Lori Beaman and Jeremy Magruder. Who gets the job referral? evidence from a social networks experiment. *The American Economic Review*, 102(7):3574–3593, 2012. ISSN 00028282. URL http://www.jstor.org/stable/41724646.

C. Delcuvellerie, N. A. Ali, K. Vyborny, E. Field, and R. Garlick. Women want to work and employers want to hire women – where is the disconnect? *DevPak Blog*, 2019. URL https://devpakblog.com/2019/10/16/women-want-to-work-and-employers-want-to-hire-women-where-is-the-disconnect/. Retrieved January 17, 2022.

Lori Beaman, Niall Keleher, and Jeremy Magruder. Do job networks disadvantage women? evidence from a recruitment experiment in malawi. *Labor Economics*, 36 (1):121–157, 2018. doi: 10.1086/693869.

Reema Nayar et al. *More and better jobs in South Asia*. World Bank Publications, 2011.

Stephen V. Burks, Bo Cowgill, Mitchell Hoffman, and Michael Housman. The value of hiring through employee referrals *. *The Quarterly Journal of Economics*, 130(2):805–839, 02 2015. ISSN 0033-5533. doi: 10.1093/qje/qjv010. URL https://doi.org/10.1093/qje/qjv010.

Christian Dustmann, Tommaso Frattini, and Ian P. Preston. The effect of immigration along the distribution of wages. *The Review of Economic Studies*, 80(1):145–173, 04 2012. doi: 10.1093/restud/rds019. URL https://doi.org/10.1093/restud/rds019.

Francis Kramarz and Oskar Nordström Skans. When strong ties are strong: Networks and youth labour market entry. *The Review of Economic Studies*, 81(3):1164–1200, 2014. doi: 10.1093/restud/rdt049.

Girum Abebe, A Stefano Caria, Marcel Fafchamps, Paolo Falco, Simon Franklin, and Simon Quinn. Anonymity or distance? job search and labour market exclusion in a growing african city. *The Review of Economic Studies*, 88(3):1279–1310, 12 2020. ISSN 0034-6527. doi: 10.1093/restud/rdaa057. URL https://doi.org/10.1093/restud/rdaa057.

Lori Beaman and Andrew Dillon. Diffusion of agricultural information within social networks: Evidence on gender inequalities from mali. *Journal of Development Economics*, 133:147–161, 2018. ISSN 0304-3878. doi: https://doi.org/10.1016/j.jdeveco.2018.01.009. URL https://www.sciencedirect.com/science/article/pii/S0304387818300117.

Amanda Pallais. Inefficient hiring in entry-level labor markets. *American Economic Review*, 104(11):3565–99, November 2014. doi: 10.1257/aer.104.11.3565. URL https://www.aeaweb.org/articles?id=10.1257/aer.104.11.3565.

Oriana Bandiera, Iwan Barankay, and Imran Rasul. Social connections and incentives in the workplace: Evidence from personnel data. *Econometrica*, 75(4):1047–1094, 2007.

Alexandre Mas and Enrico Moretti. Peers at work. *American Economic Review*, 99(1): 112–145, 2009.

Stefano DellaVigna and Elizabeth Linos. Rcts to scale: Comprehensive evidence from two nudge units. *Econometrica*, 90(1):81–116, 2022.

Nicholas Bloom, Benn Eifert, Aprajit Mahajan, David McKenzie, and John Roberts. Does management matter? evidence from india. *The Quarterly Journal of Economics*, 128(1):1–51, 2013.

T Mito. Developing the construction industry for employment-intensive infrastructure investments. *International Labour Office, Geneva, Switzerland. Available online: https://www. ilo. org/wcmsp5/groups/public/—ed_ emp/—emp_ policy/—invest/documents/publication/wcms_ 734235. pdf (accessed on April 2023)*, 2019.

S D Schlachter and J R Pieper. Employee referral hiring in organizations: An integrative conceptual review, model, and agenda for future research. *Journal of Applied Psychology*, 104(11):1325–1346, 2019. doi: 10.1037/apl0000412.

Rachel Heath. Why do firms hire using referrals? evidence from bangladeshi garment factories. *Journal of Political Economy*, 126(4):1691–1746, 2018a. doi: 10.1086/697903. URL https://doi.org/10.1086/697903.

Norihiko Matsuda and Shinsaku Nomura. Fast, easy and cheap job matching: Social networks in bangladesh. Technical Report 8107, World Bank Policy Research Working Paper, 2017. Retrieved Jan 17, 2022 from https://openknowledge.worldbank.org/bitstream/handle/10986/27307/WPS8107.pdf?sequence=5.

Amrita Dhillon, Vegard Iversen, and Gaute Torsvik. Employee referral, social proximity, and worker discipline: Theory and suggestive evidence from india. *Economic*

Development and Cultural Change, 69(3):1003–1030, 2021. doi: 10.1086/704512. URL https://doi.org/10.1086/704512.

Lori A Beaman. Social networks and the dynamics of labour market outcomes: Evidence from refugees resettled in the us. *The Review of Economic Studies*, 79(1):128–161, 2012.

Supreet Kaur. Nominal wage rigidity in village labor markets. *American Economic Review*, 109(10):3585–3616, October 2019. doi: 10.1257/aer.20141625. URL https://www.aeaweb.org/articles?id=10.1257/aer.20141625.

Emily Breza, Supreet Kaur, and Nandita Krishnaswamy. Social norms as a determinant of aggregate labor supply. Working Paper 25880, National Bureau of Economic Research, May 2019a. URL http://www.nber.org/papers/w25880.

Emilio J Castilla. Social networks and employee performance in a call center. *American Journal of Sociology*, 110(5):1243–1283, 2005.

James D. Montgomery. Social networks and labor-market outcomes: Toward an economic analysis. *American Economic Review*, 81(5):1408–1418, 1991.

Giorgio Topa. Social interactions, local spillovers and unemployment. *The Review of Economic Studies*, 68(2):261–295, 2001.

Antoni Calvo-Armengol and Matthew O Jackson. Peer effects in the workplace. *The Review of Economic Studies*, 71(3):673–703, 2004.

Yannis M Ioannides and Linda Datcher Loury. Social networks in labor markets. *American Economic Review*, 94(1):155–159, 2004.

Rachel Heath. Why do firms use referrals? *Journal of Labor Economics*, 36(2):393–437, 2018b.

Amanda Pallais and Emily G Sands. Evaluating a peer referral incentive program among new hires. *American Economic Review*, 104(4):1482–1513, 2014.

Andrew D Foster and Mark R Rosenzweig. The two dimensions of social networks: Consumption and investment in village economies. *Review of Economic Studies*, 68(4): 1055–1081, 2001.

Francis Bloch, Garance Genicot, and Debraj Ray. Endogenous formation of links to share risk in village economies. *Review of Economic Studies*, 75(1):113–143, 2008.

Hal R. Varian. *Intermediate Microeconomics: A Modern Approach*. W.W. Norton & Company, New York, 2nd edition, 1990.

Oriana Bandiera, Iwan Barankay, and Imran Rasul. Social networks and incentives in the workplace: Evidence from personnel data. *Journal of Labor Economics*, 27(2):233–277, 2009.

Matthew O Jackson. The economic consequences of social network structure. *Journal of Economic Literature*, 55(1):49–95, 2017.

Rema Hanna and Leigh Linden. Rewarding schooling success and perceived returns to education: Evidence from india. *Journal of Labor Economics*, 32(4):i1–i38, 2014.

Treb Allen and Costas Arkolakis. Trade and the topography of the spatial economy. *The Quarterly Journal of Economics*, 129(3):1085–1140, 2014.

Richard Audoly, Rory McGee, Sergio Ocampo-Diaz, and Gonzalo Paz-Pardo. A practitioner's note on the shapley-owen-shorrocks decomposition. *FRB of New York Staff Report*, (1163), 2025.

David McKenzie. Beyond baseline and follow-up: The case for more t in experiments. *Journal of Development Economics*, 99(2):210–221, 2012.

Alexandre Belloni, Victor Chernozhukov, and Christian Hansen. High-dimensional methods and inference on structural and treatment effects. *Journal of Economic Perspectives*, 28(2):29–50, 2014.

A. C. Dammert, V. Galdo, and J. Galdo. Integrating mobile phone technologies into labor-market intermediation: A multi-treatment experimental design. *IZA Discussion Paper No. 9012*, 2015. URL https://ftp.iza.org/dp9012.pdf.

G. Abebe, O. Bandiera, V. Bassi, L. Beaman, E. Galasso, and M. Macis. Welfare impacts of factory jobs: Experimental evidence from ethiopia. *World Bank Economic Review*, 39(1):124–152, 2025. doi: 10.1093/wber/lhz022. URL https://academic.oup.com/wber/article/39/1/124/7639510.

M. Knaus, J. Kluve, and H. Schmitz. Heterogeneous employment effects of job search programs: A machine learning approach. *IZA Discussion Paper No. 10961*, 2017. URL https://www.iza.org/publications/dp/10961/heterogeneous-employment-effects-of-job-search-programmes-a-machine-learning-approa

V. Bassi and F. Nansamba. The impact of disclosing soft skills certificates at recruitment: Experimental evidence from uganda. *Journal of Development Economics*, 148:102551, 2021. doi: 10.1016/j.jdeveco.2020.102551. URL https://www.povertyactionlab.org/evaluation/impact-disclosing-soft-skills-certificates-recruitment-labor-market-outcomes-uganda

Y. Hou. Information frictions and the labor market for public school teachers. *Journal of Labor Economics*, 42(1):1–35, 2024. doi: 10.1086/708707. URL https://www.journals.uchicago.edu/doi/10.1086/708707.

B. Harris. Information is power: Fostering labor market competition through transparent wages. *Brookings Institution*, 2015. URL https://www.brookings.edu/articles/information-is-power-fostering-labor-market-competition-through-transparent-wages/.

Edward P. Lazear. Performance pay and productivity. *American Economic Review*, 90 (5):1346–1361, 2000. doi: 10.1257/aer.90.5.1346.

Raymond Fisman, Nikolaj A. Harmon, Emir Kamenica, and Ingo Munk. Labor market discrimination in hiring through referrals. *Journal of Political Economy*, 125(2):351–393, 2017. doi: 10.1086/690946.

Emily Breza. Peer effects and loan repayment: Evidence from the krishna default crisis. Working Paper, Harvard University, 2017.

Michael Callen, Saad Gulzar, Ali Hasanain, and Yasir Khan. Data and policy decisions: Experimental evidence from pakistan. *The Quarterly Journal of Economics*, 135(2): 619–676, 2020.

Antoni Calvó-Armengol and Matthew O Jackson. The effects of social networks on employment and inequality. *American Economic Review*, 94(3):426–454, 2004.

Joseph G Altonji and Robert A Shakotko. Do wages rise with job seniority? *Review of Economic Studies*, 54(3):437–459, 1987.

Miller McPherson, Lynn Smith-Lovin, and James M Cook. Birds of a feather: Homophily in social networks. *Annual Review of Sociology*, 27(1):415–444, 2001.

Christian Dustmann, Albrecht Glitz, and Uta Schönberg. Referral-based job search networks. *Review of Economic Studies*, 83(2):514–546, 2016.

Albert Rees. Information networks in labor markets. *American Economic Review*, 56 (1/2):559–566, 1966.

Antoni Calvó-Armengol. Job contact networks. *Journal of Economic Theory*, 115(1):191–206, 2004. ISSN 0022-0531. doi: https://doi.org/10.1016/S0022-0531(03)00250-3. URL https://www.sciencedirect.com/science/article/pii/S0022053103002503.

George A. Akerlof. Labor contracts as partial gift exchange. *Quarterly Journal of Economics*, 97(4):543–569, 1982.

Kaivan Munshi. Networks in the modern economy: Mexican migrants in the us labor market. *Quarterly Journal of Economics*, 118(2):549–599, 2003.

Arun G. Chandrasekhar, Melanie Morten, and Alessandra Peter. Network-based hiring: Local benefits; global costs. Working Paper 26806, National Bureau of Economic Research, 2020.

Abhijit Banerjee, Arun G Chandrasekhar, Esther Duflo, and Matthew O Jackson. Diffusion of microfinance. *Science*, 341(6144):1236498, 2013.

Abhijit Banerjee, Arun G Chandrasekhar, Esther Duflo, and Matthew O Jackson. Gossip: Identifying central individuals in a social network. *Econometrica*, 84(2):485–550, 2016.

Emily Breza, Arun G Chandrasekhar, and Tyler H McCormick. Seeing the forest for the trees? an investigation of network estimation using aggregated relational data. *Quantitative Economics*, 10(1):47–83, 2019b.

Vivi Alatas, Abhijit Banerjee, Rema Hanna, Benjamin A Olken, Julia Tobias, and Matthew Wai-Poi. Targeting the poor: Evidence from a field experiment in indonesia. *American Economic Review*, 102(4):1206–40, 2012.

Arun G Chandrasekhar and Randall Lewis. Network formation with partial sampling: Theory and evidence. *RAND Journal of Economics*, 47(3):698–720, 2016.

Abhijit Banerjee, Arun G Chandrasekhar, Esther Duflo, and Matthew O Jackson. Diffusion of rival goods through social networks. Unpublished manuscript, 2012.

Stefano Caria, Simon Franklin, and Melissa Witte. Network-based job search: Experimental evidence. Uses a three-step approach to collect job-seeker network data—identifies only pairs of job-seeking partners, with no network centrality measure, 2020b.

Norihiko Matsuda and Shinsaku Nomura. The temptation of social networks under job search frictions. *The World Bank Economic Review*, 39(1):1–25, 2025. doi: 10.1093/wber/lhae010. URL https://academic.oup.com/wber/article/39/1/1/7628497.

Melissa Witte. How social networks influence labor market outcomes. Studies worker–worker networks using complete social network data; survey instrument not available online, 2018.

Federico Cingano and Alfonso Rosolia. People i know: Job search and social networks. *Labour Economics*, 19(5):582–595, 2012.

Marcus Eliason, Lena Hensvik, Francis Kramarz, and Oskar Nordström Skans. Workers, firms, and unemployment. *Working Paper*, 2019. Using Swedish register data to examine workers-to-workers and workers-to-firms connections.

Khalid Nadvi. Collective efficiency and collective failure: The response of the sialkot surgical instrument cluster to global quality pressures. *World Development*, 27(9): 1605–1626, 1999.

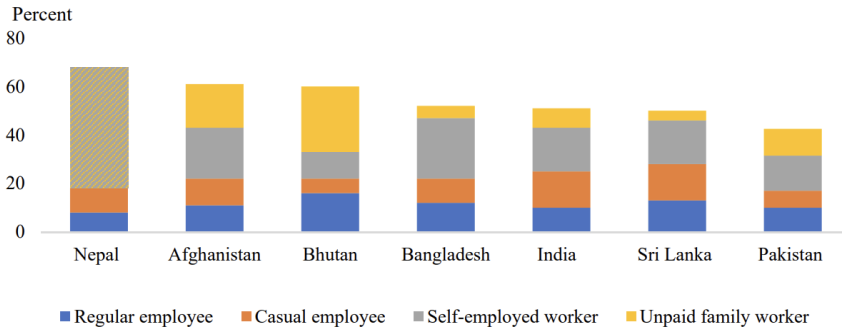
Lakshmi Iyer, Gaurav Khanna, and Ashutosh Varshney. Caste and the structure of networks in india. *Econometrica*, 90(3):1129–1160, 2022.

Ali Cheema, Asim Ijaz Khwaja, and Ahmad Qadir. Kinship networks and economic outcomes: Evidence from pakistan. *Journal of Development Economics*, 160:102997, 2023.

Appendix

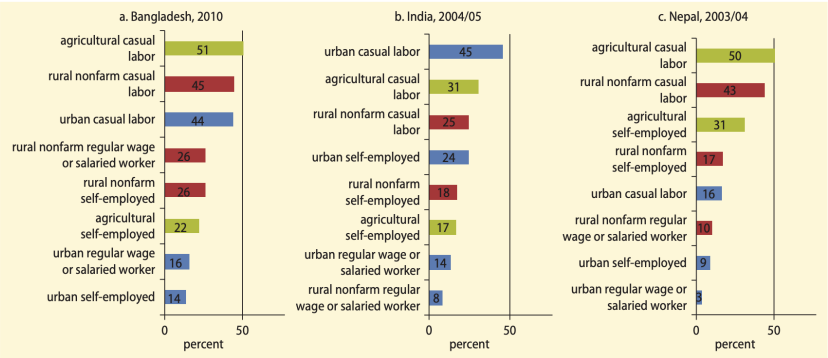
A1 Figures

A1.1 Employment, Work Search and Poverty Rates in South Asia



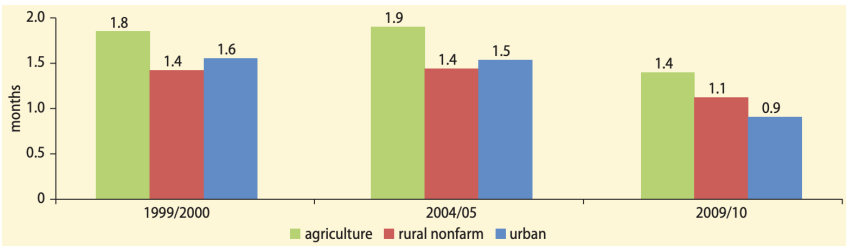
Source: World Bank More and Better Jobs in South Asia Report

Figure A1: Employment Rates by type of job



Source: World Bank More and Better Jobs in South Asia Report

Figure A2: Percentage of workers in households below the poverty line, by employment status



Source: World Bank More and Better Jobs in South Asia Report

Figure A3: Average Number of Months Without Work, casual laborers in India

A1.2 Structure of Construction Work

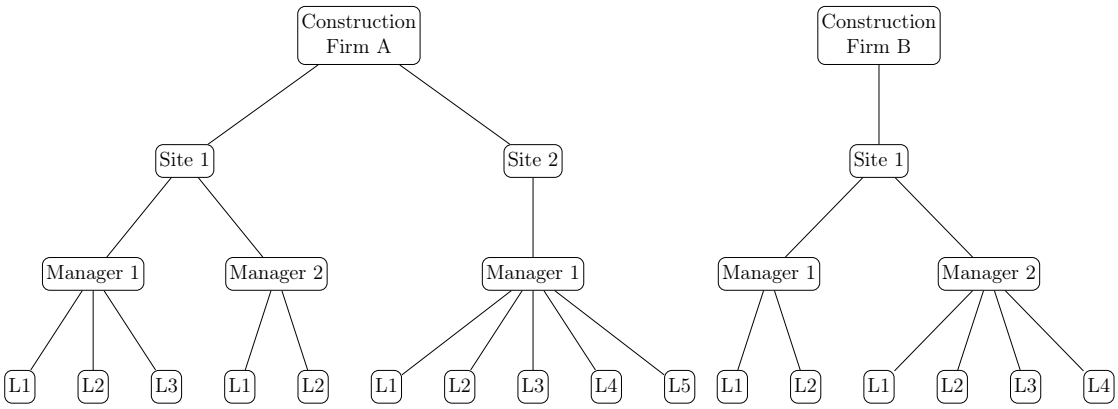


Figure A4: Structure of Construction Work

Note: The flowchart illustrates the organizational hierarchy typically observed in the construction sector in Pakistan. Each construction firm may operate through multiple construction sites, with the number of sites varying according to the firm's scale and scope of work. These sites are managed by one or more managers, depending on the size and complexity of the project. In most cases, construction sites have task-specific managers—each responsible for overseeing a particular aspect of the work (e.g., brick, tile, block, masonry, electrical, plumbing). These managers, in turn, hire and supervise their respective teams of laborers.

A1.3 Construction Sites

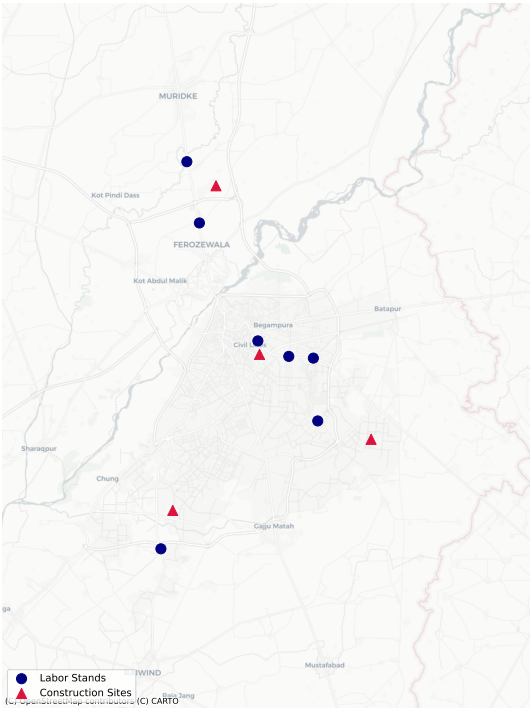


Figure A5: Intervention Location

Note: The map displays the city of Lahore and its surrounding areas, highlighting construction sites that are part of the randomized controlled trial in red, and the nearby labor stands in blue.

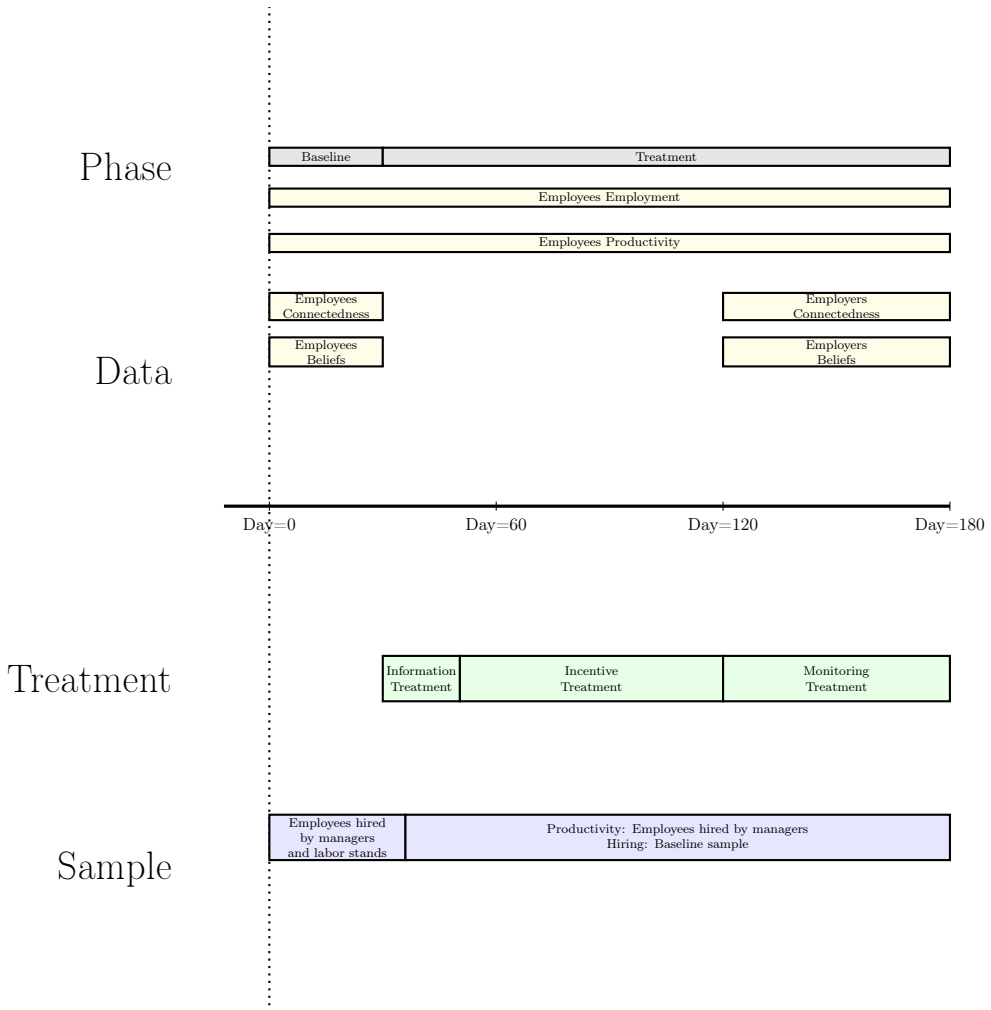


Figure A6: Timeline of the RCT Roll-out, and data collection on employees and employers.

A2 Data Descriptive for Managers and Laborers

Table A1: Manager's Descriptive Statistics

	Mean (1)	St. Dev. (2)
Demographic Characteristics		
Age	38.41	10.67
Marital status - Married	0.92	0.28
Education (completed secondary level)	0.59	0.50
Hometown is Lahore	0.34	0.48
Monthly wage (Dollar)	484.00	565.82
Work Experience		
... on the current site (Days)	1092.74	1759.32
... overall (Days)	4852.35	2713.83
Number of sites working at the same time	2.15	2.33
Team Composition		
Team size	30.56	39.30
Duration a worker usually stay on team (Days)	23.85	158.03
Effort to Find Laborers		
Search time in the last month (Days)	2.07	3.74
Work Search for Laborers		
Found through labor stand	0.09	0.29
Found through social network	0.96	0.19
Found through other means	0.04	0.19
Labor Stand Used to Find Laborers		
Ever used	0.40	0.49
In last 6 months	0.53	0.51
Currently Uses	0.32	0.47
Monitoring Frequency		
Throughout day	0.15	0.36
Every hour	0.60	0.49
Twice a day	0.19	0.39
At day end	0.06	0.24

Notes: The sample comprises data collected for 85 managers as of June 2025.

Table A2: Laborer's Descriptive Statistics

	Mean (1)	St Dev (2)	Mean (3)	St Dev (4)
	Baseline		LFS (Punjab)	
Demographic Characteristics				
Age	26.47	9.42	32.65	12.29
Marital Status - Married	0.50	0.50	0.67	0.47
Education (completed secondary level)	0.36	0.48	0.28	0.45
Hometown is Lahore	0.15	0.35	0.03	0.18
Family size	6.93	3.60	6.33	2.79
Daily wage (Dollar)	3.32	0.77		
Monthly wage (Dollar)	59.33	37.74	71.04	32.13
Work Experience				
Total (Days)	2690.69	2767.63		
Work Status - Full Time	0.99	0.12	0.86	0.35
Training				
Received any prior	0.21	0.41	0.20	0.40
Duration of training (Days)	496.26	771.81		
Commute to Work				
Daily Cost (Rupees)	100.41	126.43		
Time Taken (Minutes)	28.65	19.87		
Work Search for Current Job				
Found through labor stand	0.09	0.29		
Found through social network	0.91	0.29		
Labor Stand to Find Work				
Ever used	0.32	0.47		
Used in past 6 months	0.69	0.46		
Used in past 1 month	0.57	0.50		
Effort to Find Work				
Search time for current job (Days)	16.38	73.77		
Days Last Month				
Days Worked	17.77	10.33		
Days wanted to work but had no work	8.56	7.90		
Task Assignment				
By mason	0.03	0.16		
By supervisor	0.02	0.15		
By contractor	0.63	0.48		

Note: The sample comprises data collected from 1,500 laborers surveyed at the baseline.

Table A3: Laborer's Descriptive Statistics: Observation Data

	N	Mean	SD	Min	Max
Working time (Min)	4862	231.723	76.507	1	470
Number of plaster bowls passed and prepared per minute	3240	0.659	0.918	0.008	28.333
Number of bricks passed per minute	3835	6.090	6.532	0.12	138.5
Total time spent on brick and plaster	4862	121.761	80.703	0	420
Productivity score	4254	0.0002	0.689	-4.379	7.046
Productivity rate	4254	-0.091	167.407	-910.982	2225.216
Observations	4862				

Notes: This table summarizes key laborer-level statistics from baseline data collection through daily laborer observation. The observations are at the laborer day level. Productivity score and rate measures are based on the comprehensive method that incorporates both brick and plaster tasks.

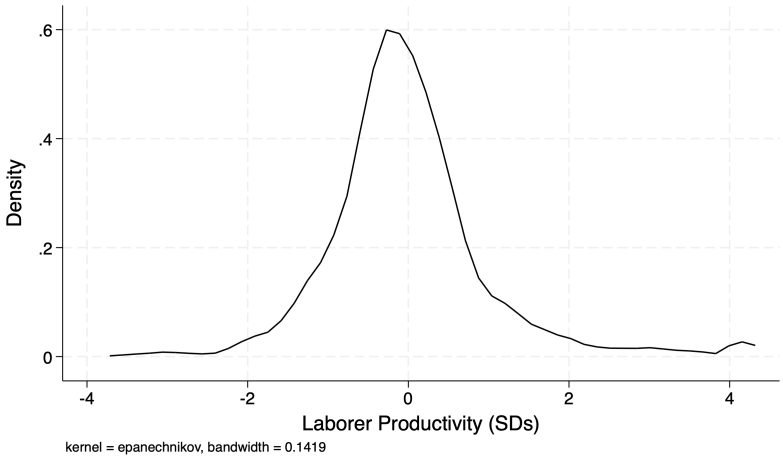


Figure A7: Productivity Distribution

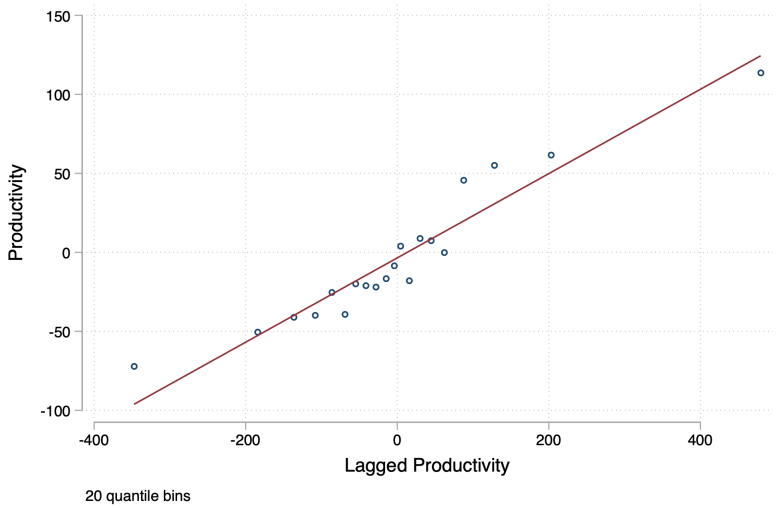


Figure A8: Productivity and Lagged Productivity

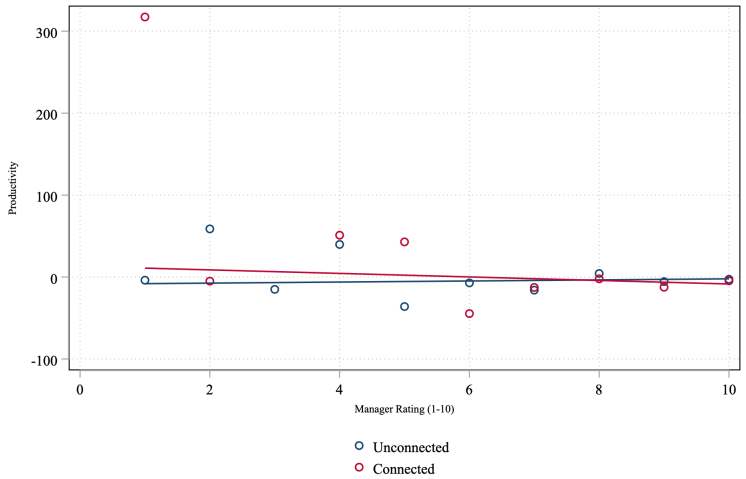


Figure A9: Worker Productivity and Rating – Pre–Treatment

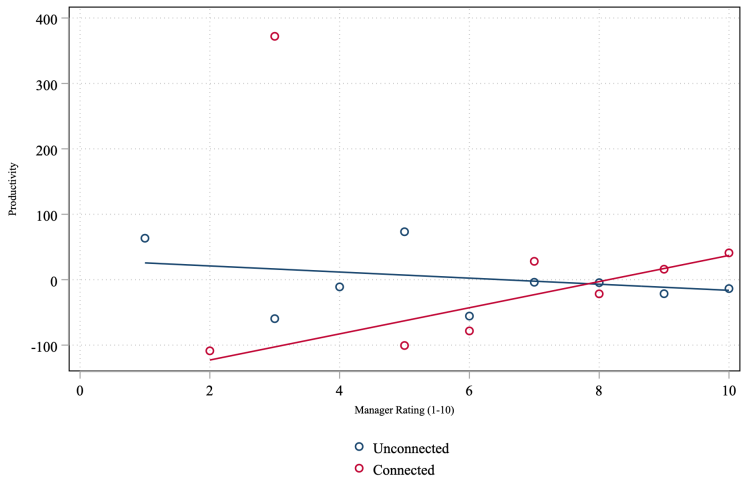
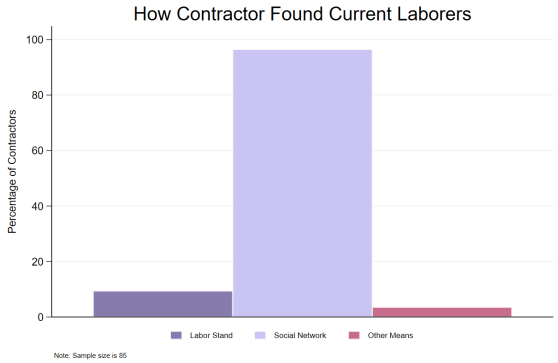
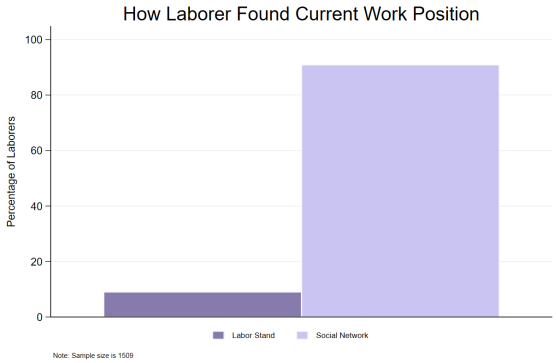


Figure A10: Worker Productivity and Rating – Post–Treatment



(a) Managers Finding Laborers

Note: This figure shows the sources used by laborers to find work.



(b) Workers Finding Work

Note: This figure shows the sources used by managers to find workers.

Figure A11: Search Channels in the Labor Market

A3 Data Descriptive from RCT Phase

Table A4: Distribution of Clusters by Treatment and Control

	Treatment		Control		Total
	N	Percentage	N	Percentage	N
Information (Laborers)	294	66	151	34	445
Incentive (Laborer-Manager-Location Days)	367	49	383	51	750
Monitoring (Laborer-Manager Days)	38	75	13	25	51

Notes: This table reports the number and percentage of clusters assigned to each group by treatment status.

Table A6: Intervention Fidelity

	Mean	St. Dev.
	(1)	(2)
Find information provided useful	0.80	0.40
Contacted laborers	0.37	0.49
Offered work to laborer	0.34	0.48
Will contact laborer in the future	0.83	0.38

Notes: The sample includes 41 managers who, as part of the information treatment, were asked whether they found the above-average laborer information useful, had contacted or hired any of them, or planned to do so in the future.

A4 Balance Tables

This section presents the balance between treatment and control groups for the information, incentive, and monitoring treatment arms. Table A7 shows the balance for the information treatment, while Figures A12 and A15 display the balance for the incentive and monitoring treatments, respectively.

Table A7: Balance for Information Treatment

	Control Mean (1)	Treatment Mean (2)	Diff (T - C) (3)	p-value (4)
Age	26.16	27.15	-0.99	0.332
Completed at least secondary school	0.32	0.39	-0.07	0.168
Marital status - Married	0.53	0.47	0.06	0.273
Hometown is Lahore	0.17	0.18	-0.01	0.896
Family size	6.74	7.18	-0.44	0.236
Daily wage (Dollar)	3.23	3.35	-0.11*	0.068
Work experience (Days)	2820.11	2781.66	38.45	0.901
Received any training	0.13	0.09	0.04	0.246
Time commute (Minutes)	31.51	28.54	2.97	0.273
Travel cost (Rupees)	73.87	78.07	-4.21	0.725
Number of days worked (Last month)	18.81	16.99	1.82*	0.085
Search Time (Days)	11.14	12.41	-1.27	0.766
Laborer's productivity before Treatment	33.01	36.80	-3.79	0.778
Connectedness to the team	0.06	0.05	0.01	0.620
Connectedness to the manager	0.13	0.12	0.01	0.525
Observations	445			

Note: This table reports balance across treatment and control groups for the information treatment using laborer baseline data. It presents the control and treatment means, the difference between the two means, and the p-value indicating the statistical significance of the difference. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

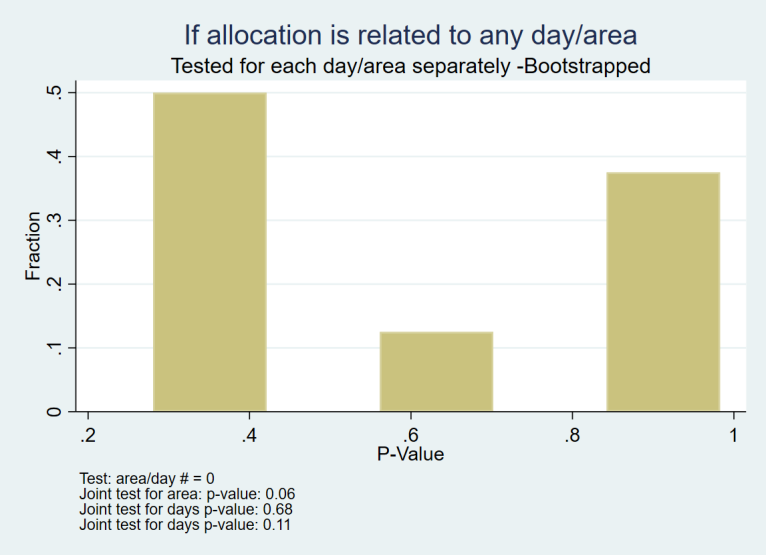


Figure A12: Balance Tests for Incentive Treatment Across Days and Locations

Notes: The figure reports balance across treatment and control groups for the incentive treatment. It shows the p-values from tests of equality between treatment and control groups for days and locations.

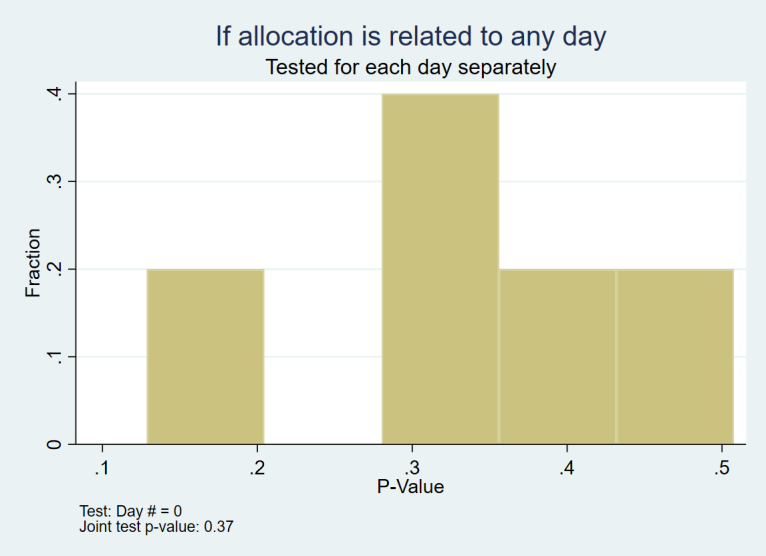


Figure A13: Balance Tests for Incentive Treatment Across Days of the Week

Notes: The figure reports balance across treatment and control groups for the incentive treatment. It shows the p-values from tests of equality between treatment and control groups for each day of the week.

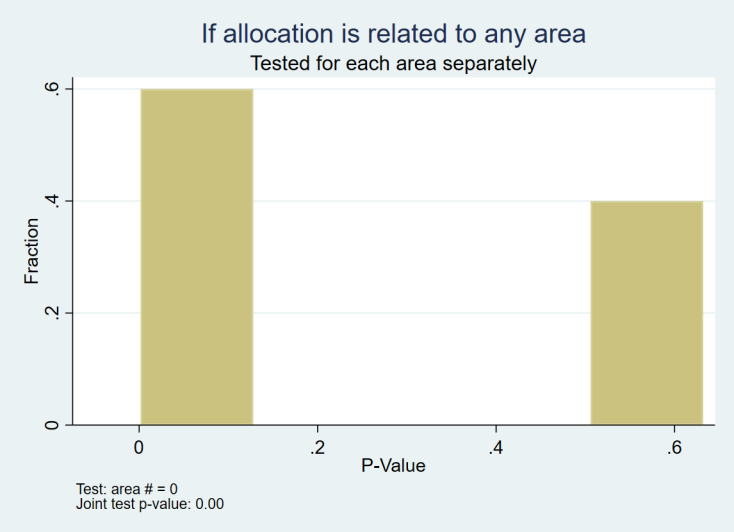


Figure A14: Balance for Incentive Treatment Across Locations

Notes: The figure reports balance across treatment and control groups for the incentive treatment. It shows the p-values from tests of equality between treatment and control groups for locations.

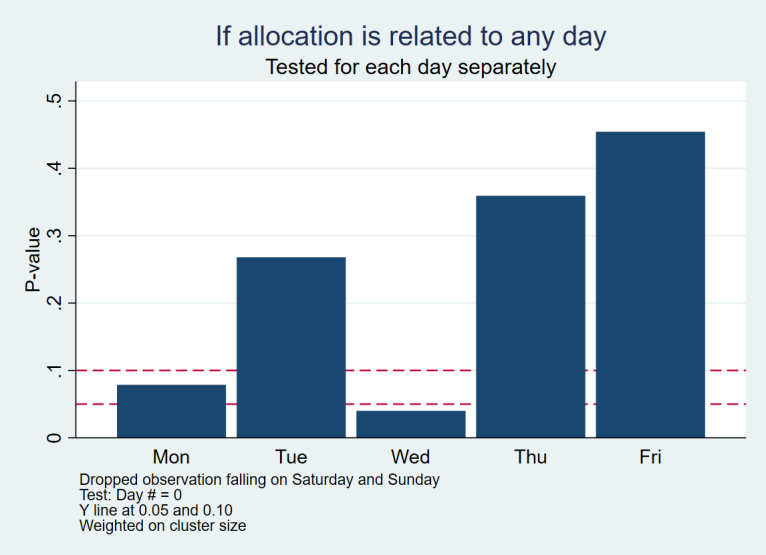


Figure A15: Balance for Monitoring Treatment

Note: This graph shows balance for treatment assignment for the monitoring treatment using laborer manager day level data. Data for Saturday and Sunday were dropped. It presents the control and treatment means, the difference between the two means, and the p-value indicating the statistical significance of the difference. The dotted lines indicate significance at 5% and 10% levels.

A5 Attrition Tables

This section presents attrition for the information and monitoring treatments. Table A8 reports attrition for the information treatment, and Table A9 for the monitoring treatment. We present attrition in relation to treatment, covariates and its interaction.

Table A8: Attrition for Information Treatment

	Coef. (1)	SE (2)	p-value (3)
Age	0.002	0.012	0.833
Completed at least secondary school	-0.105	0.141	0.454
Marital status - Married	0.069	0.138	0.616
Hometown is Lahore	-0.132	0.184	0.475
Family size	-0.018	0.021	0.412
Daily wage (Dollar)	-0.099	0.118	0.405
Work experience (Days)	-0.000	0.000	0.315
Received any training	0.033	0.193	0.863
Time commute (Minutes)	0.005	0.003	0.128
Travel cost (Rupees)	0.000	0.001	0.678
Number of days worked (Last month)	0.005	0.007	0.504
Search Time (Days)	0.002	0.004	0.591
Laborer's productivity before Treatment	0.000	0.000	0.574
Connectedness to the team	0.301	0.682	0.660
Connectedness to the manager	0.410	0.536	0.445
Information treatment	0.188	0.591	0.750
Age × Information treatment	-0.000	0.013	0.973
Completed at least secondary school × Information treatment	0.111	0.169	0.514
Marital status - Married × Information treatment	0.169	0.174	0.332
Hometown is Lahore × Information treatment	0.234	0.226	0.301
Family size × Information treatment	0.000	0.027	0.995
Daily wage (Dollar) × Information treatment	-0.024	0.141	0.863
Work experience (Days) × Information treatment	0.000	0.000	0.701
Received any training × Information treatment	-0.120	0.245	0.624
Time commute (Minutes) × Information treatment	-0.007	0.004	0.117
Travel cost (Rupees) × Information treatment	0.000	0.001	0.834
Number of days worked (Last month) × Information treatment	0.001	0.009	0.924
Search Time (Days) × Information treatment	-0.003	0.004	0.542
Laborer's productivity before Treatment × Information treatment	-0.001	0.001	0.213
Connectedness to the team × Information treatment	-0.988	0.907	0.278
Connectedness to the manager × Information treatment	-0.067	0.693	0.923
Constant	0.439	0.501	0.383

Note: Dependent variable is an indicator for attrition. Reported are coefficients and robust standard errors from regressions of information treatment assignment, covariates, and their interactions. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

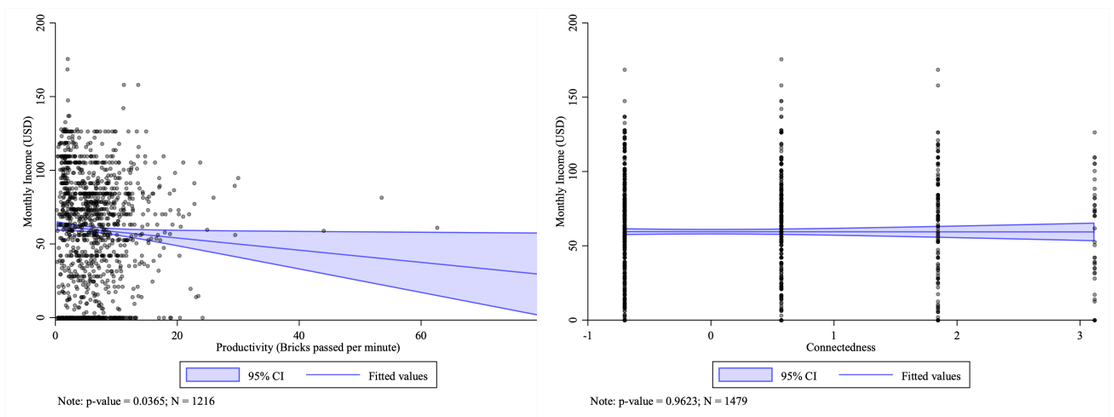
Table A9: Attrition for Monitoring Treatment

	Coef.	SE	p-value
Monitoring Treatment	-0.000	0.232	1.000
Monday	-0.588***	0.170	0.001
Tuesday	-0.615***	0.212	0.006
Wednesday	-0.400	0.325	0.224
Thursday	-0.455**	0.212	0.037
Friday	-0.182	0.187	0.336
Observations	70		

Note: Dependent variable is an indicator for attrition. Reported are coefficients and robust standard errors from regressions on monitoring treatment assignment, days, and their interaction. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A6 Results

A6.1 Selection of Workers: Productivity and Connectedness



(a) Income and Productivity (b) Income and Connectedness

Notes: Connectedness measures a laborer's social ties with their manager. Productivity is the number of bricks passed per minute at baseline.

Figure A16: Relationships Income, Productivity and Connectedness.

Table A10: Relationship between Productivity, Income and Search Effort and Connectedness including Fixed Effects and Controls

	Productivity	Daily Wage ($)	Monthly Income ($)	Working (Mins)	Socializing (Mins)	Socializing with Manager (Mins)	Job Search	Days Worked (Last Month)
Connectedness	0.008	0.020	0.820	6.033***	-9.380***	0.061	-1.395**	0.048
	(0.027)	(0.015)	(0.993)	(1.902)	(1.855)	(0.087)	(0.618)	(0.277)
Mean	-0.000	3.353	59.470	201.626	198.932	0.909	12.905	17.618
Observations	1302	1302	1302	1302	1302	1302	1301	1302
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Connectedness measures a laborer's social ties with their manager. Productivity is the average daily output of a laborer at baseline. Daily wage is self-reported in U.S. dollars. Time spent working captures total minutes performing work-related tasks. Time spent socializing (team) measures minutes interacting with all coworkers on site, while time spent socializing (manager) measures minutes interacting specifically with managers. Current job search duration is the number of days spent searching for the current job. All regressions control for baseline laborer productivity (except column 1). Standard errors, clustered at the laborer level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Fixed effects include site and enumerator; additional laborer-level controls are selected via PDS Lasso.

A6.2 Information Asymmetry

Table A11: Effect of Information Treatment

	Monthly Income ($)	Daily Wage ($)	Days Worked (Last Month)	Days Absent (Last Month)	Hours Worked (Per Day Last Month)	Worked (Today)	Searched Work (Today)	Worked (Yesterday)	Searched Work (Yesterday)
Information Treatment	7.397	0.272	-0.499	-0.700	0.098	-0.051	-0.004	-0.085	0.013
	(6.246)	(0.177)	(1.182)	(1.161)	(0.232)	(0.064)	(0.017)	(0.061)	(0.015)
Mean	84.511	3.795	19.728	9.554	8.726	0.598	0.022	0.674	0.011
N	255	255	282	282	255	282	282	282	282
R^2	0.006	0.034	0.002	0.002	0.005	0.009	0.009	0.013	0.018
RI P-value	0.263	0.171	0.660	0.536	0.704	0.421	0.686	0.154	0.616
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Monthly income and daily wages are self-reported and expressed in U.S. dollars. Days worked last month indicates the number of days a worker was employed in the previous month, while days absent measures the number of days a worker was absent during the same period. Hours worked per day last month captures the average number of hours worked per day in the previous month. Laborer worked today and laborer worked yesterday are indicators for whether the worker was employed on the respective days, and laborer searched work today and laborer searched work yesterday indicate whether the worker actively searched for employment on those days. Standard errors are reported in parentheses and are clustered at the laborer level. Fixed effects include site and and enumerator fixed effects. All regressions include controls for baseline outcomes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Heterogeneous Information Treatment Effects by Connectedness

	Monthly Income ($)	Daily Wage ($)	Days Worked (Last Month)	Days Absent (Last Month)	Hours Worked (Per Day Last Month)	Worked (Today)	Searched Work (Today)	Worked (Yesterday)	Searched Work (Yesterday)
Information Treatment	9.285	0.312	-0.428	-0.804	0.055	-0.053	-0.004	-0.084	0.013
	(6.333)	(0.180)*	(1.191)	(1.169)	(0.230)	(0.064)	(0.018)	(0.061)	(0.015)
Connectedness	-1.222	0.035	-0.588	0.049	0.422	0.068	0.017	0.036	-0.005
	(4.552)	(0.132)	(1.105)	(1.036)	(0.217)*	(0.047)	(0.024)	(0.043)	(0.008)
Connectedness x Information Treatment	13.952	0.397	-0.157	0.190	-0.512	-0.119	-0.015	-0.086	0.003
	(7.626)*	(0.222)*	(1.383)	(1.313)	(0.260)**	(0.061)*	(0.025)	(0.058)	(0.012)
Mean	84.511	3.795	19.728	9.554	8.726	0.598	0.022	0.674	0.011
N	252	252	279	279	252	279	279	279	279
R^2	0.046	0.084	0.007	0.003	0.023	0.022	0.013	0.022	0.018
RI P-Value	0.173	0.108	0.710	0.488	0.848	0.404	0.686	0.159	0.600
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Connectedness measures a worker's social connection with their manager. Monthly and daily wages are self-reported and expressed in U.S. dollars. Days worked last month indicates the number of days a worker was employed in the previous month, while days absent measures the number of days a worker was absent during the same period. Hours worked per day last month captures the average number of hours a worker worked per day in the previous month. Laborer worked today and laborer worked yesterday are indicators for whether the worker was employed on the respective days. Laborer searched work today and laborer searched work yesterday indicate whether the worker actively searched for employment on the respective days. Standard errors are reported in parentheses and are clustered at the laborer level. All regressions include controls for baseline outcomes. Fixed effects include site and enumerator fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Heterogeneous Information Treatment Effects by Connectedness

	Monthly Income ($)	Daily Wage ($)	Days Worked (Last Month)	Days Absent (Last Month)	Hours Worked (Per Day Last Month)	Worked (Today)	Searched Work (Today)	Worked (Yesterday)	Searched Work (Yesterday)
Information Treatment	9.904	0.316	-0.096	-1.126	0.074	-0.050	-0.009	-0.074	0.011
	(6.436)	(0.184)*	(1.146)	(1.123)	(0.223)	(0.062)	(0.017)	(0.060)	(0.013)
Connectedness	-16.302	-0.285	-3.587	0.014	3.191	0.146	0.064	0.247	-0.029
	(26.129)	(0.781)	(5.784)	(5.415)	(1.250)**	(0.277)	(0.108)	(0.264)	(0.045)
Connectedness x Information Treatment	14.533	0.429	-0.256	0.405	-0.603	-0.093	-0.010	-0.085	0.004
	(7.309)**	(0.215)**	(1.304)	(1.229)	(0.251)**	(0.061)	(0.022)	(0.057)	(0.011)
Mean	84.511	3.795	19.728	9.554	8.726	0.598	0.022	0.674	0.011
N	252	252	279	279	252	279	279	279	279

Notes: Connectedness measures a worker's social connection with their manager. Monthly and daily wages are self-reported and expressed in U.S. dollars. Days worked last month indicates the number of days a worker was employed in the previous month, while days absent measures the number of days a worker was absent during the same period. Hours worked per day last month captures the average number of hours a worker worked per day in the previous month. Laborer worked today and laborer worked yesterday are indicators for whether the worker was employed on the respective days. Laborer searched work today and laborer searched work yesterday indicate whether the worker actively searched for employment on the respective days. Standard errors are reported in parentheses and are clustered at the laborer level. Fixed effects include site and enumerator fixed effects. We add controls at the laborer level selected using PDS Lasso. All regressions include controls for baseline outcomes. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Information Treatment Effects Using Lee Bound for Connected Workers

	Monthly Income ($)	Daily Wage ($)	Days Worked (Last Month)	Days Absent (Last Month)	Hours Worked (Per Day Last Month)	Worked (Today)	Searched Work (Today)	Worked (Yesterday)	Searched Work (Yesterday)
lower	16.873	0.529	-3.031	-3.797	-0.526	-0.281	-0.030	-0.208	0.000
	(10.978)	(0.376)	(2.836)	(2.405)	(0.441)	(0.145)*	(0.030)	(0.134)	(0.012)
upper	28.057	0.855	1.403	0.673	-0.111	-0.131	0.006	-0.058	0.036
	(13.368)**	(0.378)**	(2.639)	(2.580)	(0.461)	(0.137)	(0.039)	(0.143)	(0.028)
N	157	157	157	157	157	157	157	157	157

Notes: Monthly and daily wages are self-reported and expressed in U.S. dollars. Days worked last month indicates the number of days a worker was employed in the previous month, while days absent measures the number of days a worker was absent during the same period. Hours worked per day last month captures the average number of hours a worker worked per day in the previous month. Laborer worked today and laborer worked yesterday are indicators for whether the worker was employed on the respective days. Laborer searched work today and laborer searched work yesterday indicate whether the worker actively searched for employment on the respective days. Standard errors are reported in parentheses and are clustered at the laborer level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Information Treatment Effects Using Lee Bound for Unconnected Workers

	Monthly Income ($)	Daily Wage ($)	Days Worked (Last Month)	Days Absent (Last Month)	Hours Worked (Per Day Last Month)	Worked (Today)	Searched Work (Today)	Worked (Yesterday)	Searched Work (Yesterday)
lower	-7.731	-0.175	-0.836	-2.030	-0.098	0.031	-0.017	-0.056	-0.017
	(8.747)	(0.237)	(1.587)	(1.639)	(0.315)	(0.078)	(0.015)	(0.081)	(0.017)
upper	3.655	0.200	1.120	-0.074	0.368	0.096	-0.000	0.009	0.008
	(7.821)	(0.210)	(1.696)	(1.514)	(0.278)	(0.077)	(0.020)	(0.086)	(0.024)
N	260	260	260	260	260	260	260	260	260

Notes: Monthly and daily wages are self-reported and expressed in U.S. dollars. Days worked last month indicates the number of days a worker was employed in the previous month, while days absent measures the number of days a worker was absent during the same period. Hours worked per day last month captures the average number of hours a worker worked per day in the previous month. Laborer worked today and laborer worked yesterday are indicators for whether the worker was employed on the respective days. Laborer searched work today and laborer searched work yesterday indicate whether the worker actively searched for employment on the respective days. Standard errors are reported in parentheses and are clustered at the laborer level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A6.3 Social Preferences

Table A16: Heterogeneous Incentive Treatment Effects by Connectedness on Hired

	Hired	Hired
Incentive Treatment	-0.000	-0.000
	(0.000)	(0.000)
Connectedness	0.000	-0.000
	(0.000)	(0.000)
Connectedness x Incentive Treatment	-0.000	-0.000
	(0.000)	(0.000)
Mean	0.001	0.001
Observations	1142656	1142656
R-squared	0.000	
Type	Fixed Effects	PDS Lasso
RI P-Value	1.0	

Notes: Hired is a dummy variable if a worker was selected for hiring on a given day. Incentive treatment is a dummy for treatment. Standard errors are reported in parentheses and are clustered at the laborer manager day location level. Fixed effects include site and and manager fixed effects. Standard errors are reported in parentheses and are clustered at the laborer level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Worker's Choice During Incentive Treatment

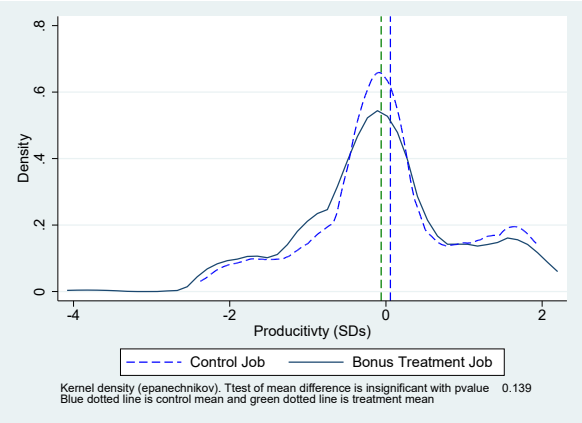
	Incentive Treatment	Incentive Treatment
Productivity (Baseline)	-0.017	-0.017
	(0.041)	(0.040)
Mean	0.057	0.057
Observations	632	633
R-squared	0.187	
Type	Fixed Effects	PDS Lasso

Notes: Productivity is the average output of a laborer measured at baseline. Incentive treatment is a dummy for treatment. Standard errors are reported in parentheses and are clustered at the laborer manager day location level. Fixed effects include site and and manager fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A18: Heterogeneous Incentive Treatment Effects by Connectedness on Productivity

	Productivity	Productivity
Incentive Treatment	0.001	0.001
	(0.082)	(0.080)
Connectedness	0.219*	0.219*
	(0.121)	(0.119)
Connectedness x Incentive Treatment	-0.165	-0.165
	(0.106)	(0.104)
Mean	0.033	0.033
Observations	590	591
R-squared	0.189	
Type	Fixed Effects	PDS Lasso
RI P-Value	0.813	

Notes: Connectedness measures a worker's social connection with their manager. Productivity is the daily output of a laborer on a given day. Standard errors are reported in parentheses and are clustered at the laborer manager day location level. Fixed effects include site and and manager fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



Notes: The figure reports balance across treatment and control groups for the incentive treatment. It shows the p-values from tests of equality between treatment and control groups for locations.

Figure A17: Distribution of baseline labor productivity by job assignment

Table A19: Heterogeneous Incentive Treatment Effects by Connectedness on Productivity

	Number of bricks/tiles passed	Number of bricks/tiles passed
Incentive Treatment	0.023	0.023
	(0.053)	(0.052)
Connectedness	-0.035	-0.035
	(0.057)	(0.056)
Connectedness x Incentive Treatment	0.061	0.061
	(0.062)	(0.061)
Mean	0.011	0.011
Observations	631	632
R-squared	0.498	
Type	Fixed Effects	PDS Lasso
RI P-Value	0.428	

Notes: Connectedness measures a worker's social connection with their manager. Productivity is the daily output of a laborer on a given day. Standard errors are reported in parentheses and are clustered at the laborer manager day location level. Fixed effects include site and and manager fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A20: Heterogeneous Incentive Treatment Effects by Connectedness on Effort

	Working Time (Mins)	Working Time (Mins)
Incentive Treatment	4.859	4.859
	(7.074)	(6.938)
Connectedness	-10.433*	-10.433*
	(6.026)	(5.910)
Connectedness x Incentive Treatment	6.499	6.499
	(5.724)	(5.614)
Mean	247.744	247.744
Observations	631	632
R-squared	0.474	
Type	Fixed Effects	PDS Lasso
RI P-Value	0.409	

Notes: Working time is the amount of time a worker spends on tasks in a given day. Incentive treatment is a dummy for treatment status. Standard errors are reported in parentheses and are clustered at the laborer manager day location level. Fixed effects include site and and manager fixed effects. Standard errors are reported in parentheses and are clustered at the laborer level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A21: Heterogeneous Incentive Treatment Effects by Connectedness on Effort

	Non Working Time (Mins)	Non Working Time (Mins)
Incentive Treatment	3.126	3.126
	(6.672)	(6.544)
Connectedness	8.719	8.719
	(6.263)	(6.143)
Connectedness x Incentive Treatment	-10.088*	-10.088*
	(5.938)	(5.824)
Mean	204.264	204.264
Observations	631	632
R-squared	0.475	
Type	Fixed Effects	PDS Lasso
RI P-Value	0.791	

Notes: Non-working time is the amount of time a worker spends idle or socializing on a given day. Incentive treatment is a dummy for treatment. Standard errors are reported in parentheses and are clustered at the laborer manager day location level. Fixed effects include site and and manager fixed effects. Standard errors are reported in parentheses and are clustered at the laborer level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A22: Heterogeneous Incentive Treatment Effects by Connectedness on Manager's supervision

	Visits	Visits
Incentive Treatment	-0.025	-0.025
	(0.028)	(0.028)
Connectedness	-0.016	-0.016
	(0.042)	(0.041)
Connectedness x Incentive Treatment	-0.029	-0.029
	(0.042)	(0.041)
Mean	0.962	0.962
Observations	623	624
R-squared	0.381	
Type	Fixed Effects	PDS Lasso
RI P-Value	0.409	

Notes: Visits are the number of times the managers visits the laborer assigned to the treatment condition. Incentive treatment is a dummy for treatment. Standard errors are reported in parentheses and are clustered at the laborer manager day location level. Fixed effects include site and and manager fixed effects. Standard errors are reported in parentheses and are clustered at the laborer level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A6.4 Match-Specific Productivity Gain

Table A23: Heterogeneous Manager's Presence Effects By Connectedness (For Non Experimental Data)

	Productivity
Monitoring (Manager)	-0.169***
	(0.053)
Connectedness	-0.028
	(0.037)
Monitoring (Manager) x Connectedness	0.053
	(0.050)
Mean	0.000
Observations	1559
Fixed Effects	Yes

Notes: Connectedness measures a worker's connection with their manager. Productivity is measured at the individual laborer–day level. Monitoring is a dummy variable indicating whether a manager or supervisor was physically present on site. All specifications include manager and day fixed effects. Standard errors are reported in parentheses and are clustered at the laborer level. The figure uses non-RCT data to estimate correlations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A24: Heterogeneous Supervisor's Presence Effects By Connectedness (For Non Experimental Data)

	Productivity
Monitoring (Supervisor)	-0.026
	(0.040)
Connectedness	0.046
	(0.042)
Monitoring (Supervisor) x Connectedness	-0.029
	(0.043)
Mean	0.000
Observations	5106
Fixed Effects	Yes

Notes: Connectedness measures a worker's connection with their manager. Productivity is measured at the individual laborer–day level. Monitoring is a dummy variable indicating whether a manager or supervisor was physically present on site. All specifications include manager and day fixed effects. Standard errors are reported in parentheses and are clustered at the laborer level. The figure uses non-RCT data to estimate correlations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A25: Heterogeneous Manager or Supervisor's Presence Effects By Connectedness (For Non Experimental Data)

	Productivity
Monitoring (Manager and supervisor)	0.034
	(0.038)
Connectedness	0.023
	(0.031)
Monitoring (Manager and supervisor) x Connectedness	-0.004
	(0.032)
Mean	0.000
Observations	5113
Fixed Effects	Yes

Notes: Connectedness measures a worker's connection with their manager. Productivity is measured at the individual laborer–day level. Monitoring is a dummy variable indicating whether a manager or supervisor was physically present on site. All specifications include manager and day fixed effects. Standard errors are reported in parentheses and are clustered at the laborer level. The figure uses non-RCT data to estimate correlations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

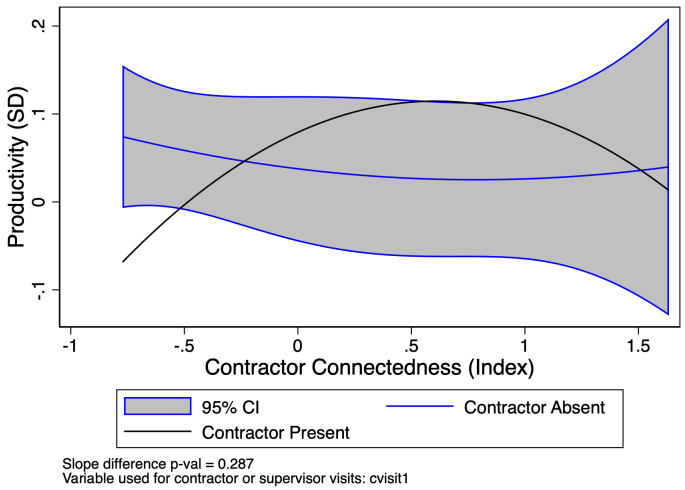


Figure A18: Heterogeneous Manager's Presence Effects By Connectedness (For Non Experimental Data)

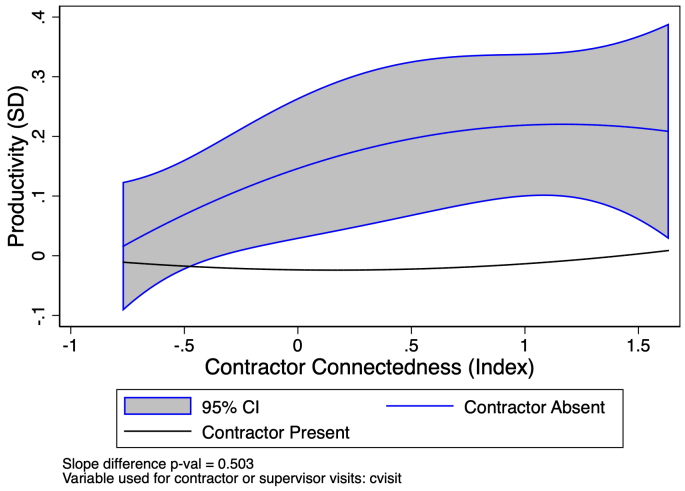


Figure A19: Heterogeneous Supervisor's Presence Effects By Connectedness (For Non Experimental Data)

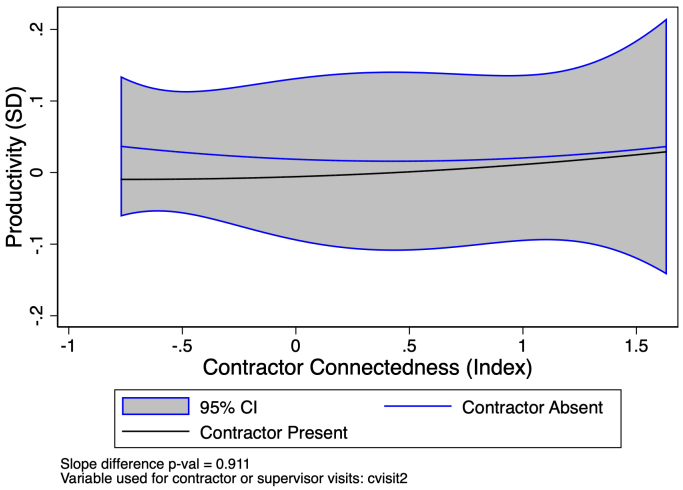


Figure A20: Heterogeneous Manager and Supervisor's Presence Effects By Connectedness (For Non Experimental Data)

Table A26: Heterogeneous Manager's Presence Effects by Connectedness (For Experimental Data)

	(1) ProductivIty
Monitoring Treatment	0.014
	(0.213)
Laborer's Connectedness to Manager_comp	-0.134
	(0.465)
Connectedness*Monitoring Treatment	-0.010
	(0.469)
Mean	0.425
N	22
R^2	0.419
Fixed Effects	Yes
Weighted	Yes
RI P-Value	0.909

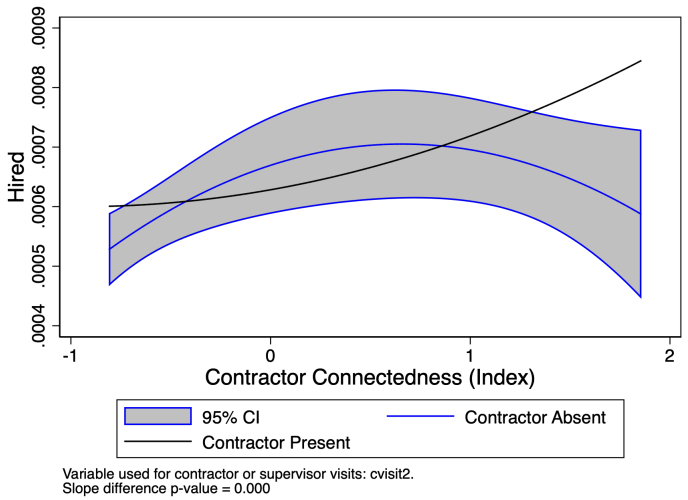
Notes: Connectedness measures a worker's connection with their manager. Productivity is measured at the individual worker–day level. Monitoring treatment is a dummy variable equal to one if a manager was removed from the site on the day of treatment. All specifications include managers fixed effects. Standard errors are reported in parentheses and are clustered at the manager–day level. Regressions control for days on which treatment was imbalanced. The table uses RCT data. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A6.5 Risk and Uncertainty

Table A27: Heterogeneous Manager's Presence Effects By Connectedness (For Non Experimental Data)

	Hired
Monitoring	0.00002582
	(0.00002979)
Connectedness	-0.00003931
	(0.00003029)
Monitoring x Connectedness	0.00011470***
	(0.00003215)
Mean	0.001
Observations	9022376
Fixed Effects	Yes

Notes: Connectedness measures a worker's connection with their manager. Hired is a dummy variable for the worker hired on a given day. Monitoring is a dummy variable indicating whether a manager was physically present on site. All specifications include manager and day fixed effects. Standard errors are reported in parentheses and are clustered at the laborer level. The figure uses non-RCT data to estimate correlations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



Notes: The figure illustrates how laborer hiring varies with workers' connectedness under different manager presence conditions. Manager presence is also a dummy variable, with the black line representing periods when the manager is on site and the blue line representing periods when the manager is absent. The graph is based on non-RCT observational data.

Table A28: Heterogeneous Monitoring Treatment Effect by Connectedness including Fixed Effects (For Experimental Data)

	(1) Hired
Monitoring Treatment	0.00011758
	(0.00028150)
Connectedness	-0.0.00012374***
	(0.00003653)
Connectedness*Monitoring Treatment	-0.00002264
	(0.00005592)
Mean	0.00024582
N	38998
R^2	0.00006055
Fixed Effects	No
Weighted	Yes
RI P-Value	0.78600000

Notes: Connectedness measures a worker's connection with their manager. Hired is a dummy variable if a worker was hired by a manager on a given day. Monitoring treatment is a dummy variable equal to one if a manager was removed from the site on the day of treatment. All specifications include enumerator and site fixed effects. Standard errors are reported in parentheses and are clustered at the manager–day level. Regressions control for days on which treatment was imbalanced. The table uses RCT data. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A29: Heterogeneous Monitoring Treatment Effect By Connectedness (For Non Experimental Data)

	(1) Hired	(2) Monitoring Treat Implement	(3) Hired
Monitoring Treatment	0.00011758	0.90964242	
	(0.00028150)	(0.06812182)***	
Connectedness	0.00012374	0.00000000	-0.00012374
	(0.00003653)***	(0.00000000)	(0.00003483)***
Connectedness*Monitoring Treatment	-0.00002264	0.02654778	-0.00002608
	(0.00005592)	(0.03991063)	(0.00005696)
Monitoring Treat Implement			0.00012926
			(0.00029385)
N	38998	38998	38998
R^2	0.00006055	0.63067695	0.00007133
Weighted	Yes	Yes	Yes
IV	No	No	Yes (Treatment Allocated)
Fixed Effects	No	No	No

Notes: Connectedness measures a worker's connection with their manager. Hired is a dummy variable if a worker was hired by a manager on a given day. The monitoring treatment is instrumented using random assignment in the first stage, where allocation to treatment serves as the instrument and the outcome is take-up of treatment (manager removal). All specifications include enumerator and site fixed effects. Standard errors are reported in parentheses and are clustered at the manager–day level. Regressions control for days on which treatment assignment was imbalanced. The table uses RCT data. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A7 Laborer Productivity Score

A7.1 Simplified Measure

We construct a simplified measure of laborer productivity by computing a standardized z-score based on output per unit of time. For brick work, output is measured as the number of bricks passed, and productivity is defined as the ratio of bricks passed to the time spent passing them for each worker on a given day[29].
Specifically, we first compute the rate of brick passing per minute as:

$$bricks\ per\ minjt = \frac{number\ of\ bricks\ passed jt}{total\ time\ spent\ on\ bricks_{jt}} \tag{6}$$

Next, we standardize this rate using the sample mean (μ) and standard deviation (σ) to obtain a z-score:

$$brick\text{-}zscore_{jt} = \frac{bricks\ per\ minjt - \mu}{\sigma} \tag{7}$$

This z-score serves as our simplified daily productivity score for each worker. We multiply the z-score with working time to account for full day of work.

$$Simplified\ Rate_{jt} = brick\text{-}zscore_{jt} \times working\ time_{jt} \tag{8}$$

A similar approach is applied to compute productivity scores for block, and tile work, adjusting for the relevant task-specific outputs and time inputs.

A7.2 Comprehensive Measure

In addition to the simplified productivity measure, we construct a comprehensive productivity index that leverages the granular time-use data and the nature of tasks performed by each laborer. We begin by calculating the total effective working time (in minutes) for each laborer on a given day, defined as the total minutes worked excluding breaks and idle periods:

$$working\ time_{jt} = total\ time_{jt} - break\ time_{jt} - idle\ time_{jt} \tag{9}$$

Next, we compute task-specific productivity z-scores. For brick work ($bricks\ per\ min_{jt}$), the rate is defined as the number of bricks passed divided by the total time spent passing them. For plastering, we use the number of bowls of plaster prepared and passed per minute.[30]

$$bricks\ per\ min_{jt} = \frac{number\ of\ bricks\ passed_{jt}}{time\ spent\ on\ bricks_{jt}} \tag{10}$$

$$plaster\ per\ min_{jt} = \frac{number\ of\ bowls\ of\ plaster\ prepared\ and\ passed_{jt}}{time\ spent\ on\ plaster_{jt}} \tag{11}$$

To account for systematic variation across sites, days, enumerators, equipments, and other factors, we residualize these task-specific rates[31]. Specifically, we estimate the following regressions:

[29] In case bricks are dumped or placed, they are included in the brick passed measurement.
[30] Brick and plaster work are typically performed in tandem.
[31] Equipment includes tools such as trolleys, lifts, shovels, hammers, or wrenches

$$bricks\ per\ min_{jt} = \alpha + \gamma_s + \delta_t + \theta_e + \mu_q + + \varepsilon_j \tag{12}$$

$$plaster\ per\ min_{jt} = \alpha + \gamma_s + \delta_t + \theta_e + \mu_q + \varepsilon_j \tag{13}$$

where γ_s denotes site fixed effect, δ_t day fixed effect, θ_e enumerator fixed effect, μ_q equipment fixed effect, and ε_i is the residual for laborer j. We then standardize the predicted residuals to obtain z-scores for each task:

$$brick\text{-}zscore_{jt} = \frac{\widehat{bricks_per_min}_{jt} - \mu_{\text{brick}}}{\sigma_{\text{brick}}} \tag{14}$$

$$plaster\text{-}zscore_{jt} = \frac{\widehat{plaster_per_min}_{jt} - \mu_{\text{plaster}}}{\sigma_{\text{plaster}}} \tag{15}$$

These task-specific z-scores are then aggregated into a composite productivity score, weighted by the time spent on each activity:

$$Prod_Score_{jt} = \frac{brick\text{-}zscore_{jt} \times time\ spent\ on\ bricks_{jt} + plaster\text{-}zscore_{jt} \times time\ spent\ on\ plaster_{jt}}{total\ time\ on\ bricks\ and\ plaster_{jt}}$$

Finally, we scale the composite score by the total effective working time to obtain the rate for comprehensive measure of laborer productivity:

$$Rate_{jt} = Prod_Score_{jt} \times working\ time_{jt}$$

This comprehensive productivity measure can be readily extended to other tasks such as block, tile, and additional construction activities.

Table A30: Shapley Values

	Shapley Value	Percentage
Working Time	0.00130	0.2
Brick Per Min (Residuals)	0.49492	68.6
Brick Time	0.00059	0.1
Plaster Per Min (Residuals)	0.22405	31.0
Plaster Time	0.00112	0.2

Dependent Variable = Productivity
Total R-squared = 0.722
N = 3,484

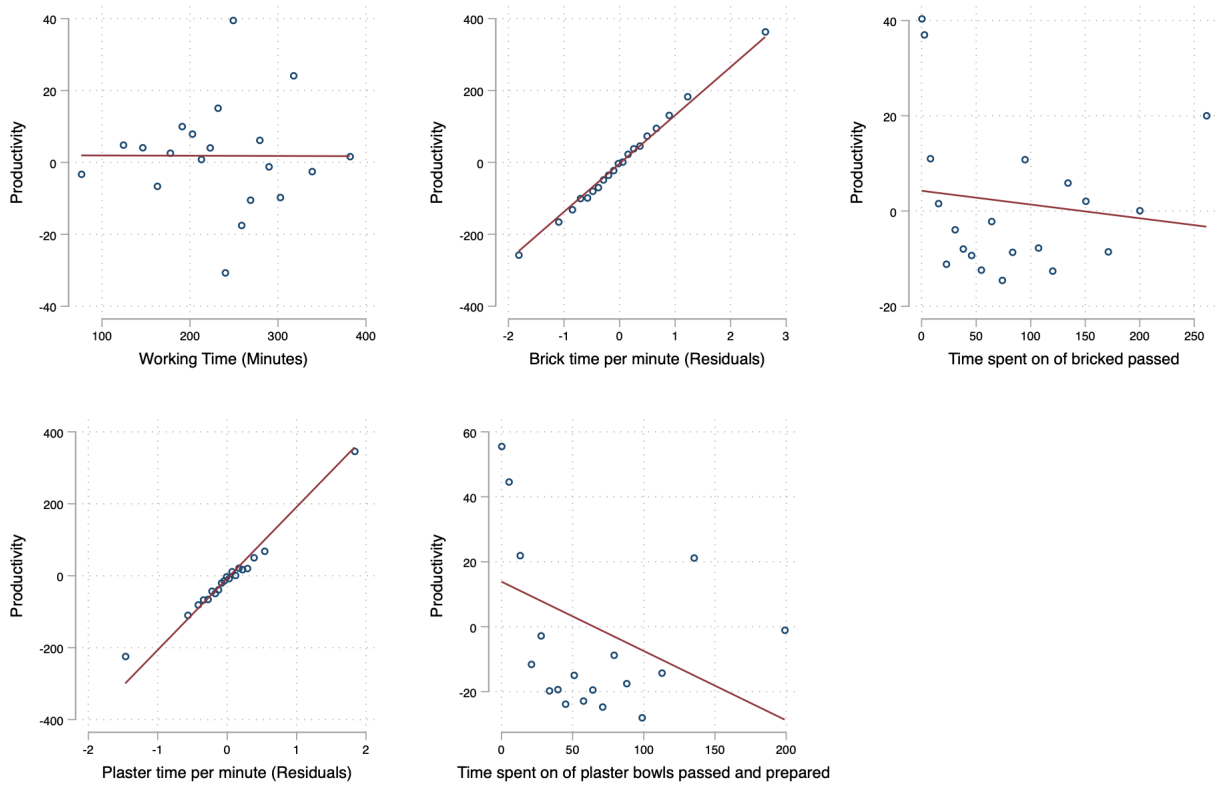
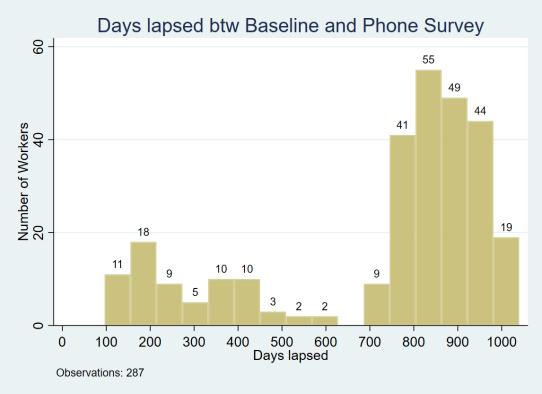


Figure A22: Productivity Measure and Constituent Components

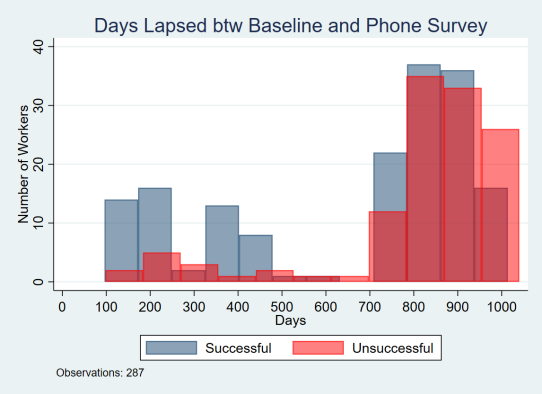
A8 Social Networks

Building on the extensive theoretical and empirical literature on social network measurement, our approach combines elements of both the gossip and ARD frameworks, tailored to the context of job search and hiring, where information is rivalrous and individuals may behave strategically. Unlike Banerjee et al. (2013, 2016), which model the diffusion of non-rival information, we explicitly allow for the possibility that workers selectively share or withhold job information depending on incentives and competition. Similarly, while Breza et al. (2019b) propose a cost-effective ARD method to approximate network structure without a full census, we adapt their approach to focus on work-related ties most predictive of hiring opportunities rather than demographic or household traits. In contrast to much of the labor market literature, which typically infers networks from administrative data, our design directly elicits socially relevant and strategically salient connections. Related work has largely applied these methods outside labor markets. For instance, Alatas et al. (2012) study information aggregation in Indonesian villages in the context of community-based targeting, Chandrasekhar and Lewis (2016) show that partial sampling generates non-classical measurement error and propose analytical corrections, and Banerjee et al. (2012) provide suggestive evidence of strategic behavior in the diffusion of rival goods. The existing literature therefore either models non-rival information flows or requires a full census for network recovery, whereas our approach adapts these tools to construct a measure of worker centrality that is firmly grounded in the labor market and predictive of hiring outcomes.
We collect novel network data to measure laborers' connectedness with both managers and co-workers along multiple dimensions, drawing on earlier work highlighting the role of networks in labor markets (Beaman 2012; Caria et al. 2020b; Matsuda and Nomura 2025; Witte 2018; Cingano and Rosolia 2012;

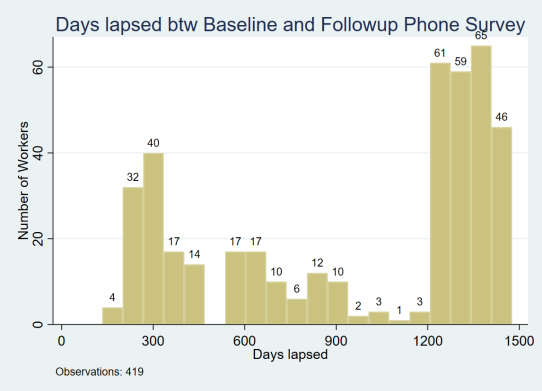
Eliason et al. 2019; Nadvi 1999). To capture these networks, we construct a continuous measure of social connectedness that is not limited to family or kinship ties, past employment links, or socio-economic groups. A laborer's connection to a manager is defined through interactions on the construction site, including (i) visiting relationships, (ii) seeking advice on jobs, health, or finances, (iii) receiving medical or material support in emergencies, and (iv) socializing. his allows us to capture direct links between a laborer and a manager. We also extend the measure to capture a laborer's interactions with other workers and team members on site, thereby incorporating indirect connections through shared ties within the work team. In addition to work-based interactions, we incorporate hometown and caste as key predictors of social network formation. These traits, while largely exogenous to our model, are well-established bases of social interaction and trust in South Asia and other low- and middle-income countries, and they strongly influence network structure and economic outcomes (Munshi 2003; Iyer et al. 2022; Cheema et al. 2023). By including these dimensions, our measure of social connectedness captures both strategic, work-related ties and deeper, identity-based social cleavages that shape labor market access in contexts such as Pakistan and India.



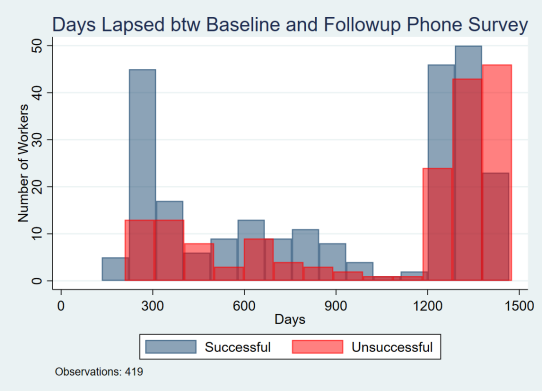
(a) Distribution of days between Baseline and first follow-up survey



(b) Distribution of days between Baseline and first follow-up survey by call outcome



(c) Distribution of days between Baseline and second follow-up survey



(d) Distribution of days between Baseline and second follow-up survey by call outcome

Figure A23: Histograms showing the time elapsed between Baseline and follow-up surveys, overall and by call outcome.