The Grass is *Not* Always Greener: The Effects of Local Labor Market Information on Search and Employment

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Abstract

We examine how information about local labor market conditions shapes search and employment outcomes for Indian job seekers. Participants were randomly shown information about either job postings, job applicants, or both from a popular job portal for their preferred city and occupation. Treated respondents are more likely to be employed seven months later. This overall effect varies considerably by baseline employment status. Among the initially *employed*, treatments reduce search off the portal and increase employment, with suggestive evidence of higher persistence in baseline jobs. In contrast, among the initially *unemployed*, treatments increase search activity but do not raise employment. Employment gains are concentrated among job seekers with less optimistic beliefs about labor market conditions. These findings suggest that limited information about labor market fundamentals may contribute to job churn in lower-income countries.

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Information frictions contribute to lower employment rates and higher worker churn in low-income countries (Carranza et al., 2022; Donovan et al., 2023). While a large literature has examined how such frictions affect unemployed individuals, we know much less about their effects on the search behavior of employed job seekers (Caria et al., 2024). Yet, limited information about labor market fundamentals may shape the work trajectories of not only the unemployed but also employed workers looking to switch jobs. The growth of online job portals in lower-income countries offers new opportunities to provide real-time market information—but how different types of job seekers interpret and act on such information remains an open question.

This paper studies the search and employment impacts of providing *employed* and *unemployed* job seekers with information about their local labor market conditions. We partner with QuikrJobs, a popular job portal operating in urban India, to generate this information using the portal's administrative data. We then randomize information provision to 6,155 job seekers recruited from the portal and track their behavior using surveys and administrative data. Our main result is that providing individuals with information on their local labor markets increases their likelihood of employment several months later, with effects concentrated among initially employed job seekers. Our ability to consider the responses of employed job seekers provides unique evidence on the behavior of this understudied population in lower-income countries.

In the experiment, participants are randomly assigned to a control group receiving general search advice or to one of several treatments showing average monthly labor market conditions in their preferred city and occupation on the portal. The *Demand* treatment provides information on the number of new job postings, along with their average salary and experience requirements. The *Supply* treatment provides the number of job applicants and either their educational distribution or the likelihood that they applied to multiple jobs. Finally, the *Tightness* treatment provides both demand- and supply-side information: the number of new postings, applicants, and total applications, with a subset additionally receiving data on the likelihood of employer contact.[1] As job finding probabilities depend on both job availability and competition among applicants (Pissarides, 2000), these variations were designed to examine how job seekers' decisions respond to different factors shaping their employment prospects. Information was shown to participants immediately after they completed a baseline survey and sent by email, with updates provided for two additional months.

Treated job seekers—those who received any of the three types of information treatments—are 4.8 percentage points (p.p.) more likely to be employed seven months later, an increase of 12% relative to the control group (p=0.027). This employment effect translates into an increase in average monthly earnings of Rs.1,320 (23.3%, p=0.012). Jobs are secured mainly through "non-portal" methods—i.e., other job portals excluding QuikrJobs, networks, etc. The *Tightness* group shows the largest increase in employment (6.2 p.p., p=0.012) and earnings (Rs.1,729, p=0.005), while the *Demand* and *Supply* groups show weaker positive

[1] All treatments also include general job search advice and show the same information for two similar occupations, motivated by evidence that job seekers may search too narrowly (e.g., Belot et al., 2018) or may become discouraged by information (e.g., Bandiera et al., 2025).

effects, though we cannot reject equality of coefficients across treatments.

To unpack the employment effect, we examine how job seekers adjusted their search behavior. Treated respondents are 4.6 p.p. (8.7%, p=0.026) more likely to report using the QuikrJobs portal, with the increase concentrated in the *Demand* and *Tightness* groups (6–7.4 p.p., p<0.05).[2] In contrast, there is no change in the likelihood of using non-portal methods. Although we cannot directly measure search efficacy, we find no significant effects on the likelihood of receiving or accepting job offers through non-portal methods.

How can employment increase through non-portal methods when search responses via these channels are muted? We reconcile this puzzle by examining heterogeneity by baseline employment status. Roughly one-third of the sample was employed at baseline, yet only 53% of these individuals in the control group remained employed seven months later. For these initially employed job seekers, treatments increase the likelihood of employment by 8.2 p.p. (15.5%, p=0.056), despite a significant decline in non-portal search.[3] Employment gains appear partly from increased persistence in baseline jobs (5.5 p.p., p=0.188), which were typically found through non-portal channels. While this average effect is imprecise, subsequent analysis shows that it masks meaningful heterogeneity by baseline beliefs. In contrast, among initially *unemployed* job seekers, we find no employment gains but observe a significant increase in portal usage. In short, initially employed respondents narrowed their search yet were more likely to be employed, while unemployed respondents searched more but did not secure jobs.

To investigate this divergent pattern, we examine how responses to the information treatments vary with job seekers' labor market expectations. At baseline, both employed and unemployed respondents have optimistic expectations about the labor market—as measured by job-finding beliefs and reservation wages—relative to actual conditions on the portal. We analyze heterogeneity in employment effects by splitting respondents above and below the sample median for each measure.[4] Effects are consistently larger in magnitude among treated job seekers with lower baseline expectations, though we are typically not able to reject equality with the high-belief group. For example, among initially employed respondents, those with below-median job-finding beliefs experience a 13 p.p. increase in employment (p=0.031), compared to a much smaller, insignificant effect for those above the median. These gains are consistent with reduced job churn, with treatments increasing persistence in baseline jobs by 10.2 p.p. (p=0.087). Among initially unemployed respondents, those with below-median job-finding beliefs see a 6.9 p.p. (p=0.06) increase in employment—accompanied by a higher willingness to accept offers—while effects for those above the median are near zero.

We also find limited evidence that the larger employment effects among workers with lower expectations are driven by differential belief updating about job finding on the portal

[2]While portal usage is higher, we find positive but statistically insignificant changes in the number or scope of applications using administrative data from the portal.

[3]We find similar reductions in non-portal search across all information treatments, but significant positive employment effects only for the *Tightness* group. The other treatments show positive but imprecise effects, and we cannot reject equality across treatments.

[4]Because employment effects are directionally similar across groups and we generally cannot reject equality of coefficients, we pool treatment groups for this analysis.

or differential search effort, though our measures only capture specific dimensions of these outcomes. Instead, we offer three possible interpretations consistent with larger gains for low-belief workers: baseline differences in worker characteristics, diminishing marginal utility over the value of the outside option, and asymmetric behavioral responses to bad news. While we cannot empirically distinguish between these interpretations, all three are consistent with low-belief job seekers reacting to poor labor market prospects by adjusting job retention if employed and offer acceptance if unemployed.

Separately, we also examine three alternative explanations for the contrasting average results between initially employed and unemployed job seekers—increased salience of the portal as a search method, negative selection among unemployed respondents, and differences in search efficacy—and find little empirical support for them.

Our paper makes three main contributions to the literature on search frictions in labor markets in lower-income countries. First, we provide new experimental evidence on how information frictions affect *employed* job seekers, an understudied population in the literature. Most existing studies examine *unemployed* individuals and focus on improving how workers signal skills (Abel et al., 2020; Bassi and Nansamba, 2022; Carranza et al., 2022) or reducing search costs and broadening exposure to job opportunities through fairs, matching, or transport subsidies (Beam, 2016; Abel et al., 2020; Bassi and Nansamba, 2022; Carranza et al., 2022; Abebe et al., 2021; Banerjee and Sequeira, 2023; Banerjee and Chiplunkar, 2024; Chakravorty et al., 2024; Kelley et al., 2024; Bandiera et al., 2025). While these interventions may indirectly convey information about labor market conditions, the information revealed is shaped by individuals' own search choices and the subset of firms they encounter. In contrast, we experimentally vary access to *market*-level information on local conditions for both employed and unemployed job seekers using a popular job portal.[5] Although standard search models emphasize the role of outside options in shaping on-the-job search (e.g., Pissarides, 1976), there is limited empirical evidence on this mechanism.[6] We show that aggregate information about local labor market conditions raises overall employment, with effects concentrated among employed job seekers and comparatively limited gains for the unemployed.

Second, we contribute to an open question in the literature on the relationship between beliefs and job search—specifically, whether correcting overly optimistic beliefs about job prospects reduces or increases search effort. This is an important policy question as over-optimism of unemployed job seekers has been consistently documented in many lower-income countries (Caria et al., 2024). Existing evidence is mixed: some studies show that learning negative information about job prospects can dampen search effort (Banerjee and Sequeira, 2023; Bandiera et al., 2025), while others find the opposite (Abebe et al., 2025). Our results highlight how both responses can emerge within the same labor market when

[5] While activity on the portal may not capture the entire labor market, we show that it is correlated with the broader labor market. Using portal activity also provides a uniquely detailed and timely window into features of labor market competition that are rarely observable in low-income settings. In doing so, our treatments are most similar in spirit to information-based active labor market policies studied in high-income countries (e.g. Altmann et al., 2018; Belot et al., 2018, 2022; Dhia et al., 2022).

[6] An exception is work by Jäger et al. (2024), which shows that low-wage workers in high-income countries underestimate their outside options and stay in their jobs.

outside options differ across workers. Among unemployed job seekers, negative information increased search activity without improving employment outcomes. In contrast, among employed job seekers, it reduced costly on-the-job search and raised employment in part by limiting job turnover.

Finally, we add to the literature on internet-based job search and its effectiveness in improving employment outcomes. Recent experiments that encourage greater use of digital job platforms in lower-income countries have generally found limited or no impacts on overall employment (Chakravorty et al., 2023; Afridi et al., 2022; Jones and Sen, 2022; Kelley et al., 2024).[7] One notable exception is Wheeler et al. (2022), who show that training workers to use LinkedIn in South Africa increased employment by 10%. Rather than encouraging greater portal use or improving platform-specific skills, our experiment provided real-time, market-level information about job competition on a portal to urban job seekers. This intervention influenced their overall search and employment decisions, both on and *off* the portal.[8] Our findings highlight the potential value of making such aggregate information publicly available, particularly in settings where reliable labor market data remain scarce (Nomura et al., 2017).

1 Context

1.1 Online job search and the QuikrJobs portal

Over the 2010s, India experienced a rapid increase in Internet adoption, going from less than 10% of the population using the internet in 2010 to nearly 50% in 2020 (World Bank, 2023). Adoption is estimated to be even higher in urban areas at 67% in 2020, with 90% of users accessing the internet daily (KANTAR, 2023). This widespread adoption has undoubtedly changed how individuals look for work. Indeed, as early as 2017, there were at least 22 job portals catering to the Indian market (Nomura et al., 2017). Naukri, which is reportedly the largest portal in the country, is estimated to have a database of 82 million job seekers and 5 million recruiters (Carranza and McKenzie, 2024).

We partner with a nationwide job portal, QuikrJobs, which features primarily entry-level, low-wage jobs in the retail and services sector. Employers and job seekers can use the platform for free or purchase premium features that prioritize their jobs or profiles. In 2019, QuikrJobs was active in over 1,000 cities, encompassing at least 8 million job-seekers and 2 million job postings with an average monthly salary of Rs.17,800 (US$252). Relative to its competitors, QuikrJobs features jobs that more closely reflect average urban incomes (Fernando et al., 2023). Although one of many online job portals in India, its national reach and focus on lower-wage employment make it a valuable setting for studying labor demand for and search behavior among young, urban workers.

[7] In high-income countries however, improvements in online job search technologies have been shown to facilitate faster re-employment and lower unemployment rates (Kuhn and Mansour, 2014; Bhuller et al., 2021).

[8] He et al. (2023) provide similar information—average applications per vacancy—on a Chinese job portal, but are unable to measure effects on off-platform search or employment, limiting direct comparison.

1.2 Job seekers in our sample

We recruited 6,155 study participants from the QuikrJobs portal. Appendix A.1 provides summary statistics on their demographics, employment, and job search behavior at baseline. Individuals in our sample are young, educated, equally likely to be male or female, and actively engaged in job search. The average age is 27 years (median 24 years) and a majority (63%) has at least a college degree. In terms of labor market experience, 68% have at least some paid work experience. At the time of the survey, 69% of job seekers had not worked for pay in the last 30 days; we refer to these job seekers as "initially unemployed." Meanwhile, 31% had worked for pay in the last 30 days ("initially employed").[9] Among those who worked in the previous month, 48% report earnings lower than Rs.15,000.[10]

Moving to measures of job search, nearly 85% of job seekers report spending at least one hour searching for work in the past 7 days. Respondents also use multiple search methods: in the past 30 days, 74% used the QuikrJobs portal and 78% used other non-portal channels, such as job websites, networks, newspapers, employment agencies, or a direct walk-in approach. Two-thirds of job seekers have been searching for a new job for at least a month and 19% for over 6 months. While unemployed job seekers have been searching for longer, measures of search effort are quite similar across initially employed and unemployed job seekers (Appendix A.2.1). However, as expected, reservation wages to accept new full-time jobs are lower for unemployed than employed respondents.

Finally, to understand selection into our sample, we compare the study sample to a sample of adults living in the same urban areas from the 2022–23 Periodic Labor Force Survey (Appendix A.3). The study sample is much younger and substantially more likely to have a college degree. In terms of work background, our sample is (unsurprisingly) much less likely to be employed. Among those who worked recently however, the share of individuals making less than Rs.15,000 in the previous month is similar across the two samples. Overall, these patterns are consistent with demographic divides in internet use documented around the world (Pew Research Center, 2016) as well as high unemployment rates among Indian college graduates (State of Working India, 2023).

1.3 Labor market conditions and job seekers' expectations

Our partnership with the QuikrJobs portal allows us to access and share information on real-time local labor market conditions. We now describe these conditions and assess how closely they align with job seekers' baseline expectations.

We measure job competition as the average monthly ratio of total applicants to new job

[9]Consistent with large well-documented gender gaps in employment in India, women are much less likely to be employed than men (Appendix A.2.1).

[10]While we lack detailed employment information at baseline, follow-up data from control respondents indicate that the majority (81%) are employed in private, for-profit firms, with the remainder in self-employment (15%) or other types (4%). Among those in for-profit firms, 92% are paid monthly, with a median income of Rs. 15,000. About 65% report access to at least one benefit—paid leave, pension, provident fund, or health insurance. The median establishment size is 30 employees. These patterns suggest that most control respondents are employed in lower-wage jobs within relatively formal, urban firms.

postings in respondents' preferred local markets. The median respondent faces a market with 30 applicants per new job posting; 90% of the sample is searching in markets with more than 13 applicants per new posting (Appendix A.4.1). Combined with evidence that firms on the portal often end up hiring from networks (Fernando et al., 2023), this competition implies a low chance of job finding from the portal. This is confirmed in our follow-up survey data: only 2.5% of employed respondents in the control group found their job through the portal. However, such low job-finding rates are typical for individual platforms, as job seekers routinely use multiple search channels and online applications have low match conversion rates (Marinescu and Skandalis, 2021).

Moreover, we also find that portal conditions align with broader labor market fundamentals. Among control respondents, those in markets with higher job competition on the portal are less likely to be employed and report fewer job offers overall in the follow-up survey, even after controlling for city, occupation, and sampling-batch fixed effects (Appendix A.4.7 and A.4.8). This suggests that variation in portal conditions captures meaningful differences in local employment opportunities.

We next compare these actual conditions with job seekers' baseline expectations. Each job seeker reported the percent chance of finding a new job through the QuikrJobs portal that they would be willing to accept within the next 30 days. The median response is 75%, with an interquartile range of 40–100% (Appendix A.4.2). When asked instead about the probability of finding a job through any method, the distribution of responses is nearly identical, suggesting that beliefs about the portal broadly mirror overall optimism in job-finding rates. Yet, only 40% of control respondents are working at the follow-up survey. We observe similar optimism if we instead consider wage expectations. The median salary respondents require to accept a new full-time job is Rs.25,000 (Appendix A.4.3). However, among those employed at baseline, 83% made less than Rs.25,000, and roughly 80% of postings in 2021 advertised salaries lower than Rs.25,000.[11] Baseline expectations are, thus, only weakly anchored to actual market conditions (Appendix A.4.5 and A.4.6).

Together, these patterns suggest that job seekers' expectations are too optimistic on average given the actual conditions they face—consistent with other evidence from similar contexts (e.g., Kelley et al., 2024; Abebe et al., 2025). Providing information about these conditions may therefore help job seekers better evaluate their employment prospects and make decisions accordingly.

2 Experimental Design

2.1 Sampling

From February 2021 to January 2022, we recruited job seekers on the portal to participate in the study. The recruitment occurred either through advertisements posted directly on the portal or through email invitations to job seekers who had recently applied for a job.

[11] We document a positive correlation between job-finding beliefs and reservation wages (Appendix A.4.4).

Eligibility was restricted to those searching for jobs in nine cities across India in one of five different job categories.[12] Interested individuals clicked on a link, which redirected them to an external survey screen inviting them to participate in the study by first completing a baseline survey. A total of 6,155 eligible individuals completed the baseline survey, where they specified their local labor market by selecting their desired city and occupation.[13]

2.2 Design

Randomization: Eligible job seekers were randomized at the individual level. This randomization was conducted in real-time by the survey software, such that as soon as an individual completed the baseline modules, they received information on the next screen as per their random assignment. We also emailed the same information to them within seven days of the baseline and sent updated versions via email for an additional two months.

Treatment construction: Information was compiled using administrative data shared by the portal. Information treatments were generated at the city×occupation level and were averaged across three months to reflect general trends and minimize the chance of reporting idiosyncratic behavior in a given month. Given the extended sampling period, the information was updated monthly to ensure respondents received timely information.[14]

Experimental groups: Individuals were randomly assigned to one of several experimental groups described below. The variations reflect the logic from standard search and matching models, where job-finding probabilities depend both on job availability (demand) and on competition from other applicants (supply).[15] Examples of the information provided to each group are shown in Appendix A.5.

Control (N=1,070): This group received only general job search advice and encouragement, without any information about local labor market conditions.

Demand (N=1,011): This group received information about the number and attributes of new job postings in their local labor market. Specifically, we shared the average number of new postings per month, average minimum salary, and the share of postings requiring no prior experience.

Supply (N=2,030): This group received information about the number and attributes of other applicants in their local labor market. We included information on attributes because competition depends not just on the number of applicants but also their quality. As quality of

[12]The cities are: Ahmedabad, Bhubaneswar, Chennai, Delhi, Hyderabad, Kolkata, Lucknow, Mumbai and Pune. The occupational categories include: Accountant, BPO/Telecaller, Data Entry, Sales and Receptionist. We selected these cities and job categories in consultation with the partner as they accounted for significant traffic on the portal and provided variation in geography and work.

[13]This constitutes less than 1% of job seekers active on the portal over the course of the experiment.

[14]For example, individuals sampled in early March 2021 received information from November 2020 to January 2021, while those sampled in early January 2022 received data from September to November 2021.

[15]In these models, this is formally captured by labor market tightness, $\theta = v/u$, where v denotes vacancies and u denotes job seekers. Higher θ implies better prospects for workers (Pissarides, 2000).

competing applicants may depend on their skills and search effort, we implemented two treatment variants. In the first version (n=1041), we shared information on the distribution of self-reported educational qualifications and the English skills of other applicants. In the second version (n=989), we shared information on the average number of applications in a month and percent of job seekers applying to multiple job postings. Both versions always included the average number of unique job applicants in a month in the local market.

Tightness (N=2,044): This group received information about new job postings *and* other applicants to capture the full picture of market conditions. There were also two variations in this group. In the first version (n=1,001), we included information about the average number of job postings, applications, and unique job seekers in a month. The second version (n=1,043) was similar to the first, but added information on the likelihood of employer contact from the portal, measured as the share of applicants whose contact details were viewed by employers.

Across the three type of treatments, we also presented the same information for two additional, similar occupations within the respondent's preferred city, and included the same general advice shown to the *Control* group.

A concern with this design may be that even within city$\times$occupation cells, job seekers face segmented markets by skills or experience, limiting the relevance of average conditions for their decisions. While this concern is valid, constructing more individualized measures of outside options would require rich data on worker characteristics and job flows that are rarely available in low-income settings. Our design instead asks whether coarse information about local market conditions can nevertheless help job seekers in their search process.

3 Data and Empirical Strategy

3.1 Data sources

Baseline survey: This survey is self-administered online via Surveymonkey for individuals interested in participating in the study. It contained questions on demographics, employment, preferred city and occupation, labor market beliefs (e.g. job-finding probabilities, reservation wages, and demand and supply conditions on the portal), and a short skills test focusing on basic English and Math skills. We also collected name and contact information to match individuals to the administrative data from the portal and for follow-up surveys.

Post-treatment survey: Once respondents finished the baseline modules, information treatments were delivered directly on screen as per random assignment. Following this, we asked three questions about the perceived value of the information and about changes in job-finding beliefs and strategies.[16]

[16]Unfortunately, in an effort to limit survey length, we did not elicit posterior beliefs about specific demand- or supply-side conditions immediately after the treatments.

Follow-up survey: Trained enumerators administered a follow-up survey over the phone about seven months, on average, after the baseline survey. This survey serves as our primary source of outcomes and covered information on job search and employment, labor market beliefs, and included willingness to pay exercises. To minimize attrition, a short survey was offered to those respondents who refused the full-length survey; this short survey only collected basic information on search and employment outcomes. A total of 3,430 individuals completed either the long or the short survey for an overall response rate of 56%.[17] We discuss attrition in more detail in section 4.3, but note here that response rates do not vary differentially by treatment status or by baseline employment status.

Administrative data: We observe data on portal search activity for up to six months after an individual is sampled into the experiment. Using contact information available from the baseline survey, we match 99.6% (N=6130) of baseline respondents to their job seeker profiles on the portal. For each matched user, we observe all applications; the details of vacancies associated with the applications, such as job category, posted salary, or experience requirements; employer engagement with applications; and, self-reported profile details of the job seeker, such as age, sex, etc.

3.2 Empirical approach

Given our experimental design, we estimate the intent-to-treat (ITT) impacts of information provision on employment and job search behavior.[18] We start by estimating the following pooled specification to study the impacts of receiving *any* local labor market information:

$$Y_i = \beta Treatment_i + \delta_s + \lambda_t + \gamma X_i + \epsilon_i \tag{1}$$

Y_i is the outcome of interest for job seeker i, measured post-treatment. $Treatment_i$ is a binary variable taking a value of 1 for those who received any local labor market information and 0 otherwise. We also include city×occupation fixed effects, δ_s, and sampling batch fixed effects, λ_t.[19] Given large differences in employment rates by gender in our context, we include a control for whether the respondent is female, X_i, but excluding it does not affect our main results (see Appendix B.13).[20]

We modify equation 1 to estimate effects by the type of information provided to job seekers. $Demand_i$, $Supply_i$, and $Tightness_i$ are mutually exclusive indicators for the corresponding treatment groups, as described in section 2.2.[21]

[17]We have 3,292 responses for the long version and 138 responses for the short version.

[18]While we did not register a pre-analysis plan, the AEA RCT registration specified these outcomes, which are the primary objects of interest in such studies.

[19]We group together respondents into a "sampling batch" based on the timing of our information treatment updates, mentioned in section 2.2.

[20]If a respondent's sex is missing and cannot be inferred from their name, we assign it a placeholder value and include a missingness indicator in the regression to account for the missing data.

[21]In the results section, we also present and discuss estimates that separately identify the two variations within the *Supply* and *Tightness* treatments.

$$Y_i = \beta_1 Demand_i + \beta_2 Supply_i + \beta_3 Tightness_i + \delta_s + \lambda_t + \gamma X_i + \epsilon_i \quad (2)$$

Finally, we examine heterogeneity by baseline characteristics by interacting the *Treatment* indicator in equation 1 with a binary variable $Covariate_i$, as shown below. For precision, we pool treatment groups in the main heterogeneity analysis but present and discuss disaggregated results where relevant.

$$Y_i = \alpha_1 Treatment_i + \alpha_2 Treament_i \times Covariate_i + \alpha_3 Covariate_i + \delta_s + \lambda_t + \gamma X_i + \epsilon_i \quad (3)$$

3.3 Balance

We report balance checks in Appendix B.1 using data from our baseline surveys and from portal activity prior to baseline. We compare covariates of job seekers assigned to any treatment group—i.e. pooled across the information treatments—with the control group and do not find any evidence of imbalance. We also test whether covariates jointly predict treatment status and do not find evidence for this (F-test p=0.589). Finally, for each covariate, we report whether coefficients from a regression model using separate treatment indicators—*Demand*, *Supply*, and *Tightness*—are jointly zero and can never reject the null hypothesis.

4 Main Results

4.1 Overall effects on employment and earnings

We estimate treatment effects on employment and earnings in Table 1 using data from the follow-up surveys. Panel A reports pooled effects of receiving any local labor market information, as per equation 1, while Panel B disaggregates effects by the type of information provided, as per equation 2.

Treated individuals are 4.8 p.p. more likely to be employed, a 12% increase relative to the control group (p=0.027; Panel A, column 1). Consistent with this increase, they report working 0.307 additional days in the past week, a 15% increase (p=0.017, column 2), where the unemployed respondents are coded as having worked zero days.

We then ask whether jobs were found through the portal (column 3) or other non-portal search methods (column 4).[22] Strikingly, treated respondents are more likely to report finding their current jobs through other *non-portal* methods (5.2 p.p., p=0.019, column 4); the effect is nearly identical to the overall employment increase. In comparison, there is a near-zero treatment effect on job finding through the portal itself (column 3). We explore possible explanations for this pattern in section 4.2.

[22]Non-portal methods include all other online and offline channels excluding the QuikrJobs portal (e.g. other online portals, direct walk-ins, newspapers, etc.). Respondents not currently employed are coded as zero for these outcomes.

Turning to earnings, we examine both the likelihood of positive earnings and the level of earnings in Indian rupees, coding the unemployed to have zero earnings (Panel A, columns 5–6).[23] Treated individuals are 4.3 p.p. (14%, p=0.044) more likely to report positive earnings, with a level increase of Rs.1,320 (23.3%, p=0.012). Appendix C.1 shows that the cumulative earnings distribution for the treatment group is indeed shifted to the right of the control group.[24]

Considering impacts by information type in Panel B, we find positive employment effects of 3.7–6.2 p.p. across all treatments—*Supply*, *Demand*, and *Tightness*. The *Tightness* treatment leads to the largest and most precisely estimated effects on employment (6.2 p.p., p=0.012) and earnings (Rs.1,729, p=0.005). The effects for *Demand* and *Supply* are smaller and less precisely estimated, although we cannot statistically reject equality of coefficients across treatment groups. Results remain similar when we further separate the variations within the *Supply* and *Tightness* groups (Appendix B.5).

Altogether, these findings demonstrate that providing job seekers with information about labor market fundamentals can increase employment and earnings.

4.2 What drives positive employment effects?

4.2.1 Overall effects on search behavior

We begin by examining how job seekers adjusted their overall search behavior. As our sample uses multiple search methods—the portal, other job websites, networks, direct walk-ins, etc.—Table 2 considers treatment effects on the usage of portal (columns 1–3) *and* non-portal methods (columns 4–6).

Treated job seekers are 4.9 p.p. more likely to report using the portal since baseline, an 8.7% increase relative to the control group (p=0.026; Panel A, column 1). Both *Demand* and *Tightness* treatments increase the likelihood of portal usage by similar magnitudes (6.7–7.1 p.p., p<0.05; Panel B). Meanwhile, the *Supply* treatment generates a smaller, statistically insignificant change; we can also reject equality of coefficients between *Supply* and the two other groups (p<0.05). In contrast, usage of non-portal methods is unchanged (column 4). These patterns suggest that information about demand-side conditions elicits stronger interest in the portal from job seekers. However, these changes in portal use cannot explain why employment gains arose primarily through non-portal methods (Table 1, column 4).

A possible explanation is that the treatments altered search behavior on the intensive margin—the number or types of applications submitted through non-portal channels—in ways that changed the arrival or quality of offers. Unfortunately, detailed application data across multiple search methods are difficult to obtain. While we have administrative data on

[23] Conditioning earnings on employment status at the follow-up survey would introduce selection concerns, which is especially relevant in our context given the observed employment effects.

[24] A back-of-the-envelope calculation suggests that this effect is largely driven by increased employment at the extensive margin: multiplying the treatment effect on employment by average earnings among the employed in the control group (Rs.16,425) accounts for about 60% of the treatment effect in Panel A, column 6.

portal applications, we must rely on self-reported applications from the follow-up surveys for other search methods, which are prone to recall issues.

To mitigate recall concerns, we focus instead on job offers, which are more salient to job seekers than applications.[25] We assess whether treated respondents were more likely to receive or accept offers through different search channels. Across both portal and non-portal channels, however, we find no evidence that the treatments affected the likelihood of receiving or accepting offers (columns 2–3 and 5–6 in Table 2). Appendix B.6 shows that results are similar even when further disaggregating the *Supply* and *Tightness* treatments. That is, even if treated respondents did alter the number or types of jobs they applied to in ways that we cannot fully capture, they were ultimately unable to obtain acceptable offers.[26]

These muted effects on search behavior cannot account for the average employment gains we documented earlier. Since our sample includes both *employed* and *unemployed* job seekers, we next examine whether baseline employment status shaped responses to the information treatments.

4.2.2 Heterogeneous effects by baseline employment status

At baseline, 31% of our sample is *employed* and searching. Information about local labor market conditions could affect these job seekers differently than their unemployed counterparts. In Tables 3 and 4, we therefore examine our search and employment outcomes *separately* for initially employed and unemployed job seekers, respectively.[27] As before, Panel A reports pooled treatment effects, while Panel B reports effects disaggregated by information type.

Among initially employed job seekers, treatments affect the choice of search methods (Table 3, columns 1–2). Treated job seekers are 8.1 p.p. (9.6%, p=0.014) less likely to use non-portal search methods.[28] Portal usage increases, but not significantly. These patterns are similar in magnitude and direction across the different information treatments (Panel B, columns 1–2). Consistent with reduced search effort, treated respondents also appear less likely to receive a job offer during the follow-up period (Appendix C.4, Panel A).

Turning to employment outcomes, we first note that in the control group, only 52.9% of initially employed job seekers are still employed at the follow-up survey. While this may appear low, it is consistent with evidence from other developing-country urban labor markets: for instance, in urban South Africa, only 43.1% of those employed at baseline remain

[25] In the follow-up surveys, respondents recalled offers more reliably than applications since baseline: only 1–5% could not remember or were unwilling to report offers, compared to 18–25% who could not recall the number of applications they submitted. This is likely because offers are relatively rare: control respondents received a median of just 1 job offer since baseline.

[26] Analysis using the portal's administrative data also shows limited changes to application behavior (Appendix C.2). Treated respondents submit 0.43 more applications in the 30 days after treatment (p=0.233; Panel A, column 1). Most of these additional applications were directed to non-preferred city–occupation cells, which saw an increase of 0.35 applications (p=0.137; Panel A, column 5).

[27] Appendix B.2 shows that we are balanced across treatment and control groups within subgroups of initially employed and unemployed job seekers.

[28] This decline spans different non-portal methods, such as other online portals and networks (see Appendix C.3, Panel A).

employed four months later (Carranza et al., 2022). Within this high-churn context, our information treatments significantly increase the likelihood of being employed at follow-up by 8.2 p.p.—a 15.5% increase relative to the control group (p=0.056; Panel A, column 3). This effect is strongest for the *Tightness* group, which shows a 10.4 p.p. increase in employment (p=0.031). The *Demand* and *Supply* groups also show positive, but statistically weaker, effects (6.4–7.6 p.p., p=0.16–0.19), and we cannot reject equality of coefficients between the treatment arms.

Moreover, treated respondents are significantly more likely to report a non-portal method as the source of their current employment (column 5). While this pattern could reflect either greater efficacy of non-portal search or persistence in baseline jobs that were typically found through non-portal channels, our evidence suggests that the latter explains most of the effect. Treated respondents are 5.5 p.p. more likely to remain in their baseline jobs (p=0.18), accounting for 67% of the overall employment increase.[29] Although this estimate is imprecise, we show in Section 5.2 that this average effect masks meaningful heterogeneity in treatment responses by baseline labor market expectations.

In contrast, among initially *unemployed* job seekers, we find significant increases in search activity but no significant effects on employment (Table 4). Treated job seekers are 4.6 p.p. more likely to have used the portal since baseline (p=0.094; Panel A, column 1). This effect is largest for the *Tightness* group (7.4 p.p., p=0.017; Panel B, column 1), with a similar, though less precise, effect for the *Demand* group (6 p.p.,p=0.101); we cannot reject equality between these groups. We also observe positive but insignificant increases in non-portal search methods (column 2).[30] Despite these search adjustments, employment effects are positive but not statistically significant (2.6 p.p., p=0.329; Panel A, column 3), with no differences across information treatments (Panel B). The fact that less than 1% of unemployed control respondents found jobs through the portal, despite 56% of them using the portal since baseline, suggests that returns to platform search are low, which may explain why the increase in search effect does not translate to average employment gains. However, as we show in Section 5.2, this muted average effect masks heterogeneity by baseline beliefs.

Together, these results show that our overall results in Tables 1 and 2 masked significant heterogeneity by baseline employment status. We discuss these results and the underlying mechanisms that may explain the varying responses of employed and unemployed job seekers in section 5.

4.3 Robustness to attrition in the follow-up survey

In the follow-up survey, we successfully obtained responses from 56% of job seekers about their employment status, with equal rates of completion among initially employed and unemployed respondents. A majority of non-responses (80%) reflected contact difficulties rather

[29] Among respondents employed at follow-up, we asked whether they were working at the same job on or before the baseline date.

[30] Disaggregating non-portal search methods, we find an increase in usage of other online methods (Appendix C.3, Panel B).

than refusals.[31] We present three pieces of evidence that attrition is not systematically related to treatment and thus is unlikely to threaten the internal validity of our estimates. First, completion rates do not differ by treatment status (Appendix B.3). This holds both for the full sample and within subgroups of initially employed and unemployed job seekers. Second, we do not find evidence that the baseline covariates of attriters vary systematically by treatment status and observed differences are usually quite small (Appendix B.4). Finally, we assess the robustness of our main employment result by explicitly accounting for attrition by reweighting observations according to the inverse probability of survey response, predicted by baseline characteristics and treatment status.[32] Appendix B.7 shows that our estimates remain similar after this adjustment.

5 Mechanisms

Our results reveal striking differences in how initially employed and unemployed job seekers responded to information about their preferred local labor markets on the portal. Employed respondents narrowed their search and were more likely to be employed, whereas unemployed respondents searched more, but did not experience employment gains. To unpack these results, section 5.1 outlines how the search problem varies by employment status and how these differences can shape responses to labor market information. Then, section 5.2 shows that employment effects vary systematically by baseline labor market perceptions and discusses several possible explanations for this pattern. Finally, section 5.3 discusses the plausibility of three alternative explanations for differential responses by baseline employment status—increased salience of the portal as a search method, negative selection among unemployed job seekers, and differences in search efficacy.

5.1 Framework: How Employment Status Shapes Job Search

Though both employed and unemployed job seekers are looking for new opportunities, they face distinct search problems due to differences in their outside options. We outline how these differences shape the trade-offs each group faces in engaging in continued search and how these trade-offs may be influenced by information about their local labor market conditions.

For employed job seekers, the search problem is to find a better job than the one they already have. They must weigh the value of their current employment against the net expected payoff of continued search, which depends on search costs, the effectiveness of search, and the perceived value of external opportunities. Information about labor market conditions can raise employment through two channels. First, information may allow employed workers to

[31] Since respondents could not know in advance that the calls concerned the survey, it is hard to imagine this type of non-response being systematically related to treatment status. The full set of reasons for non-completion is as follows: phone number not connected (80%); refusals (14%); incorrect phone number (3%), or other (3%).

[32] Specifically, we use a probit model to predict survey response using baseline characteristics shown in Appendix B.1, individual treatment indicators, and fixed effects for city×occupation and sampling batch. We then use the inverse of the predicted values as weights, increasing the importance of observations more likely to attrit.

re-optimize their search efforts—e.g. target job opportunities with higher chances of yielding an acceptable offer—and generate new and better matches. This could occur regardless of whether the information reveals favorable or unfavorable job prospects, as individuals can adjust their search behavior in either case. Second, if the information reveals poor job prospects, it increases the relative value of the current job by highlighting limited outside options. In this case, rather than risking unemployment, workers may rationally choose to remain in their jobs, adjust (costly) search effort, or both..[33] While information may raise employment under both scenarios, we expect greater job retention when labor market conditions are poor, and more job switching when conditions are favorable.

In contrast, for unemployed job seekers, the search problem is to find an acceptable job. They must decide how intensively to search, where to search, and whether to accept an offer or continue looking. Information about labor market conditions could affect these decisions in multiple ways. By providing a more accurate view of job prospects, it may prompt adjustments in search effort, targeting, or reservation wages, thereby increasing the likelihood of employment. The direction and magnitude of these adjustments will depend on whether the information conveys favorable or unfavorable conditions and on individual search costs. However, increased search effort or other adjustments may not lead to more acceptable offers if labor market conditions are poor or if there are other constraints, such as negative employer perceptions of unemployed job seekers, limited employment gains. Conversely, employment gains could arise even without a change in the offer arrival rate if poor conditions induce job seekers to become more willing to accept existing offers.

Alongside these considerations, baseline beliefs about labor market prospects may directly shape how individuals respond to information. Because respondents are over-optimistic on average (see section 1.3), the treatments largely delivered 'bad news' about external job opportunities. Examining heterogeneity by baseline beliefs therefore helps shed light on how job seekers interpreted the information treatments and clarifies the mechanisms behind the average employment effects.

5.2 Heterogeneity by baseline beliefs and job competition

We investigate heterogeneity in employment effects across two related dimensions on labor market conditions: baseline expectations and actual job competition on the portal.[34] As perceived and actual conditions are only weakly correlated (section 1.3), studying both lets us assess whether responses are more sensitive to beliefs or actual market conditions. We pool the treatment arms together for precision, as employment effects are directionally similar and we cannot reject equality of coefficients across information types; results by treatment arm are reported in the Appendix and discussed below.

[33] Workers appear to have some discretion: among control respondents who reported a previous job, 58% report quitting their jobs rather than being fired.

[34] While this heterogeneity was not pre-registered, it is theoretically motivated and the variables used are common in labor market settings. Following guidance from Banerjee et al. (2020), we view this analysis as exploratory but valuable for learning as much as possible from the experiment.

5.2.1 Labor market beliefs

Table 5 shows how employment effects vary by baseline labor market expectations using two measures: (i) prior about job-finding via the portal (Panel A); and (ii) reservation wages (Panel B).[35] To estimate heterogeneity, we interact the pooled treatment indicator with an indicator for above-median expectations, following specification 3.

Treated job seekers with below-median baseline beliefs are 8.1–9.1 p.p. more likely to be employed (p<0.01; column 1, Panels A and B).[36] Although the difference between the below- and above-median belief subgroups is only statistically significant for job-finding priors, employment effects for those with above-median beliefs are much smaller in magnitude and never significant across both measures (0.6–1.6 p.p.).[37] Appendix B.8.1 plots treatment effects across the belief distributions, showing that the size of the treatment effect decreases as the priors become more optimistic.

Splitting the sample by baseline employment status shows that beliefs shaped responses for both employed and unemployed respondents[38] Among employed job seekers, treated respondents with lower baseline expectations are 13–16.2 p.p. more likely to be employed (p<0.05; column 2). Higher employment is driven by greater job retention: these respondents are 10.2–11.7 p.p. more likely to remain in their baseline jobs, accounting for roughly 72–78% of the total employment effect (column 3).

Among unemployed job seekers, we also find evidence of higher employment for treated respondents with lower baseline beliefs. Those with lower job-finding priors experience a 6.9 p.p. (p=0.06) increase in employment, while those with lower reservation wages see a 5.2 p.p. (p=0.173) increase (column 4). While the interaction terms between treatment and the above-median belief measures are not statistically significant (columns 2–4), they are consistently large and negative, and consequently, the treatment effects for those with above-median baseline expectations are much smaller and never significant.

However, owing to smaller sample sizes in these interacted regressions, we are generally not able to reject equality in treatment effects across belief groups and therefore view this as suggestive evidence in helping to explain our average employment effects. Nevertheless, we discuss several possible interpretations of this heterogeneity in section 5.2.3. We now turn to how effects vary by *actual* labor market conditions.

[35] Specifically, these measures are responses to the following questions: "What is the percent chance you will find a new job using QuikrJobs that you join in the next 30 days?" and "In the coming 3 months, what is the minimum monthly payment you would accept to work at a new full-time job?"

[36] Appendices B.10 and B.11 estimate these results separately by information type and show broadly similar patterns.

[37] These results are robust—and, if anything, show greater significance at conventional levels for the interaction terms for several results—after controlling for additional individual characteristics including age, college degree, paid labor market experience, and search intensity (see Appendix B.14.)

[38] Appendices B.8.2 and B.8.3 plot treatment effects across the belief distributions separately for initially employed and unemployed respondents.

5.2.2 Labor market competition

We sampled respondents from 45 local markets—defined as city×occupation cells—on the portal. Our measure of labor market competition exploits the variation across these markets by calculating the average monthly ratio of total applicants to new job postings.[39] The median respondent's preferred local market had roughly 30 applicants per new job posting.[40] We classify respondents' preferred markets as high-competition (*HC*) if the ratio of applicants to new postings is above or equal to 30, and low-competition (*LC*) otherwise.[41] We then interact the (pooled) treatment with this competition variable and present results in Table 6.

In the full sample, treated job seekers in *HC* markets are 6.2 p.p. more likely to be employed (p=0.045; column 1), with effects roughly half as large for those in *LC* markets, though we cannot reject equality between the two.[42] Among initially employed job seekers, treatment effects are concentrated in *HC* markets: employment rises by 13.7 p.p. ($p = 0.035$) in *HC* markets, compared to just 3.8 p.p. (p=0.508) in *LC* markets (column 2), though again this difference in not significantly different. This increase in *HC* markets is driven primarily by job retention (column 3), with the point estimate of 11 p.p. (p=0.073) accounting for roughly 80% of their employment effect. In contrast, among initially unemployed job seekers, employment effects are small and statistically indistinguishable across market types (column 4).[43]

Compared to the more consistent patterns by labor market beliefs we observed across the full sample and the subgroups by initial employment status, the heterogeneity by actual conditions is weaker. This may reflect that even *LC* markets still feature substantial competition—an average of 18 applicants per new posting—leaving little scope for respondents to experience truly favorable conditions. Alternatively, individuals' perceptions of job prospects may matter more than objective conditions in shaping responses to information.[44]

5.2.3 Understanding and interpreting the effect heterogeneity by labor market beliefs

Our evidence suggests that employment gains are larger among low-belief workers—those with below-median job-finding priors or reservation wages—across both initially employed and unemployed respondents. To shed light on why this might be the case, we start by examining whether the heterogeneity in employment effects is also visible in intermediate outcomes like belief-updating and search.

We asked respondents about their perceived likelihood of finding a job through the por-

[39] To ensure relevance to respondents' search decisions, we assign each individual the applicant-to-posting ratio from their sampling month. This measure is highly stable across the three months of information delivery, with correlation coefficients ranging from 0.91 to 0.99.

[40] We focused on new job postings rather than total postings to (i) avoid including inactive or stale listing and (ii) better capture the flow of jobs on the portal.

[41] We note however that there are *always* more applicants than vacancies, i.e., the ratio is greater than one.

[42] Appendix B.9 shows how this effect varies across the distribution of job competition.

[43] Appendix B.12 shows these results by information type and reaches similar conclusions.

[44] We also considered whether effects are concentrated among job seekers who are "correctly pessimistic"—that is, those with below-median beliefs searching in above-median competition markets. However, this requires interacting treatment with both belief and competition indicators, splitting the sample into eight subgroups, which leaves us underpowered to draw firm conclusions, particularly among initially employed respondents.

tal before and immediately after information delivery. On average, we find little evidence of belief-updating on this dimension (Appendix C.5.1).[45] There is also no indication of heterogeneous belief-updating for either employed or unemployed respondents (Appendix C.5.2), suggesting that differential employment responses are not driven by stronger belief updates among low-belief workers, at least on this measure.[46]

On the search side, changes in the search margin alone also cannot fully explain why employment gains are larger among low-belief workers. Among initially employed respondents, low-belief workers do not exhibit larger reductions in non-portal search than high-belief workers.[47] This suggests that the belief heterogeneity in employment effects appears to operate through the retention margin rather than through observable changes in search effort. Among initially unemployed respondents, low-belief workers marginally increase non-portal search (Appendix C.10, column 8). However, this does not translate into a significantly higher likelihood of receiving non-portal job offers (Appendix C.11, columns 1–2); instead, low-belief unemployed workers were significantly more likely to *accept* non-portal offers (columns 3–4), whereas high-belief workers showed no such effect, with the difference between the two significant at p=0.10–0.11 across our two belief measures.

Given that the belief heterogeneity in employment effects is more consistent with heterogeneous responses in retention and offer acceptance—rather than parallel shifts in search behavior or observed belief-updating—we outline three non-mutually exclusive explanations that can rationalize these findings.

First, heterogeneity by baseline beliefs may simply reflect underlying differences in worker characteristics rather than beliefs themselves. Appendix A.6 shows that workers with higher job-finding priors are more educated and use more search methods, while high reservation wage workers are older, more likely to be male, more educated, and less likely to report low earnings. While our results are robust to controlling for several of these additional baseline differences (Appendix B.14), we cannot rule out that unobserved characteristics correlated with baseline beliefs also contribute to the heterogeneous employment effects we document. For instance, if low-belief workers are systematically more risk-averse or face higher switching costs, they may prefer remaining in their current baseline jobs if already employed, while the unemployed may be more willing to accept available non-portal offers rather than risk remaining unemployed.

Second, it is possible that our treatments shifted beliefs about other (unmeasured) dimensions of the search problem, such as the expected value of the outside option.[48] If the

[45] The dependent variable is a change in beliefs (posterior–prior) about the likelihood of accepting a job via the portal.

[46] A lack of updating on this measure need not imply an absence of learning. First, we did not measure updated expectations about demand- or supply-side conditions on the portal, the items directly targeted by the information treatments. Second, since the elicited probability depends jointly on perceived market conditions and search effort, even if respondents updated negatively on market conditions, they might plan to adjust their search behavior in a way that offsets this revision, leaving their overall job-finding probability unchanged.

[47] Non-portal search declines similarly across belief groups when measured by reservation wages (Appendix C.10, column 4), and actually declines more among high-prior respondents (column 3).

[48] For example, information on posted salaries and experience requirements from our *Demand* treatment could change beliefs about external job quality.

treatments reduced the expected value of external opportunities, then assuming concavity over job payoffs can generate the observed pattern of heterogeneity through diminishing marginal utility. A uniform downward revision would lead to a larger utility loss for workers with low expectations than for workers with high expectations, who are on the flatter part of the utility curve. This larger utility loss for workers with low expectations could motivate greater job retention among the initially employed and a higher willingness to accept offers among the unemployed.

Finally, our results can also be understood through a behavioral lens in which workers may respond asymmetrically to information depending on their initial beliefs. When information points to poor prospects, job seekers with less optimistic priors may find it easier to internalize and act on that information, whereas those with more optimistic priors may be slower to incorporate it into their decision-making (Jones and Santos, 2022).

While we cannot empirically distinguish between these interpretations, all three are consistent with treated job seekers reacting to poor labor market prospects: employed respondents became more likely to remain in their current jobs, and unemployed respondents became more willing to accept jobs they might otherwise have declined.

5.3 Alternative explanations for divergent effects by baseline employment status

We now consider three additional explanations that could also explain our effect heterogeneity by baseline employment status. While we cannot conclusively rule out each explanation, the evidence suggests these factors are unlikely to be the primary drivers.

5.3.1 Increased salience of portal

Information treatments may have increased the salience of the QuikrJobs portal as a search method, relative to other channels. Because the portal featured high job competition and low job-finding rates, greater reliance on the portal may have (i) failed to improve employment chances for unemployed respondents and (ii) prevented initially employed respondents from finding new jobs, leading them to remain in their baseline jobs.

For initially unemployed respondents, however, we find the opposite pattern. Alongside greater portal use, the information treatments also increased usage of other online methods by 5.7 p.p. (Appendix C.3, column 1, p=0.04). Rather than concentrating search on the portal, the treatments appear to have expanded the set of search strategies, which is inconsistent with portal salience driving the absence of employment gains for unemployed respondents.

Among initially employed respondents, we observe some evidence of heterogeneity in employment effects by priors and actual job competition (Tables 5 and 6). If the treatments had merely increased the salience of the portal without conveying meaningful information, we would expect employment effects to be more uniform across these job seekers.

These findings are consistent with respondents reacting to the content of the information rather than simply shifting attention toward the portal.

5.3.2 Negative selection of unemployed job seekers

Another possibility is that initially unemployed job seekers on the portal are negatively selected on ability and are therefore less able to take advantage of the information treatments.

Baseline proxies of ability provide little support for this explanation. First, educational attainment is nearly identical across groups: 64% of both employed and unemployed respondents hold at least a college degree. Second, there is substantial overlap in performance on a 10-item skills test of basic English and math (Appendix A.2.2); if anything, unemployed job seekers score 0.43 points (or 0.18 standard deviations, p<0.01) higher than their employed counterparts (see Appendix A.2.1). Third, among respondents with prior paid work experience, unemployed and employed respondents report very similar baseline earnings (Appendix A.2.3).[49] These patterns suggest that the two groups are comparable on observable dimensions of ability and earnings potential.

Effect heterogeneity by baseline beliefs also provides suggestive evidence against this explanation. Among initially unemployed respondents, those with lower reservation wages or lower job-finding beliefs experience employment gains of 5.1–-6.9 p.p. (Table 5, column 4; p=0.06–0.173). While these estimates are imprecise, when coupled with the descriptive evidence, they support the view that at least some unemployed respondents are not systematically lower ability. Taken together, these patterns do not support the interpretation that negative selection among unemployed respondents is driving the observed heterogeneity in employment effects among the two types of job seekers.

5.3.3 Increased search efficacy

A final possibility is that employed job seekers were more effective than unemployed job seekers at adapting their search strategies to find new jobs in response to the information treatments. For instance, they may be able to leverage current workplace networks to identify acceptable opportunities—an option not available to unemployed respondents. If this were the case, we would expect treatment effects for employed respondents to come primarily from securing new jobs. However, nearly 70% of the employment effect reflects remaining in baseline jobs rather than transitions into new jobs (Table 3, column 6).

Consistent with the retention effect, we also do not find evidence that treatments changed the probability of obtaining job offers among initially employed respondents (see Appendix C.4, Panel A). In fact, they are 6–7 p.p. *less* likely to receive an offer overall (column 1, p=0.15) or from non-portal sources (column 7, p=0.096).[50] While offer outcomes alone cannot

[49]Unemployed respondents with paid work experience comprise 53% of unemployed respondents and 37% of the overall sample. They reported their typical monthly earnings in their last job, whereas employed respondents reported earnings in the previous month.

[50]We also do not find significant effects on the number of offers received or on the likelihood of accepting an offer across either portal or non-portal methods. Thought we do not restrict to active searchers in these regressions

definitively establish whether employed job seekers are more effective searchers, they lend further support to our finding that treatment effects for employed respondents largely reflect persistence in baseline jobs rather than success in obtaining new ones.

6 Discussion

6.1 Is reduced churn welfare improving?

The welfare consequences of reduced churn depend on whether it preserved more or less productive matches. We offer three pieces of suggestive evidence that it is unlikely that initially employed respondents were made worse off from lower mobility.

First, we examine retention effects across the baseline earnings distribution among initially employed respondents (see figure in Appendix C.9). In the control group, retention increases with earnings, consistent with higher-earning workers being in better jobs and therefore less likely to churn. Retention rates are 8-12 p.p. higher in the treatment group relative to control across the bottom three earnings bins, covering approximately 70% of the initially employed sample, and are slightly lower than control for the top bin. While sample sizes within each bin are too small to formally test whether effects differ across the distribution, this pattern suggests that the retention effect is not concentrated among the lowest-quality jobs.

Second, we compare job characteristics between treatment and control stayers at the time of the follow-up survey. If treatment induced workers to retain lower-quality jobs while control stayers only remained in relatively good jobs, we would expect to observe worse job quality among treatment stayers. We find no evidence of this pattern: treatment and control stayers report similar average earnings, number of benefits on the job, written contract rates, and working conditions (Appendix C.9).

Finally, Appendix C.6 shows that treatment effects on earnings conditional on employment are never significantly negative for initially employed (or unemployed) respondents, suggesting that workers were not pushed into worse-paying jobs, notwithstanding the selection concerns inherent in this comparison. While we cannot rule out differences on unobserved dimensions of job quality or speak to longer-run earnings trajectories, together these three pieces of evidence are consistent with the intervention reducing inefficient churn rather than simply preserving lower-quality matches.

6.2 Value of information

Information provided in the experiment was not readily available on the QuikrJobs portal. We offer two pieces of evidence to suggest that job seekers valued information about their local labor market conditions. First, respondents were asked whether they found the infor-

to avoid selection concerns, doing so yields similar conclusions.

mation useful on a three-point scale immediately after the information treatments were first delivered on screen. Treated respondents were 7.6% more likely to say that they found the information "very useful" (Appendix C.7). Second, we conducted hypothetical and incentivized willingness to pay (WTP) exercises for information with 1,961 respondents during the follow-up survey. A majority of respondents were willing to pay to access information about local labor market conditions on the portal (see details in Appendix C.8).

6.3 External validity

Our main results show that treatment effects are concentrated among initially employed job seekers, driven partly by increased job retention. To interpret the relevance of this finding more broadly, Appendix A.3 compares this subgroup to a sample of urban workers from the 2022–23 PLFS. As with the full study sample, this subgroup is younger and more educated than the average urban worker, but has a lower share of women, consistent with well-documented gender gaps in employment. Despite higher education levels in our study, the share of workers earning below Rs. 15,000 per month is comparable across both samples.

These patterns suggest our findings are most relevant for young, educated, urban job seekers who are employed in positions with lower pay relative to their expectations, but actively searching for better opportunities online. High job turnover and overoptimistic labor market beliefs among young urban workers have been documented in a range of developing contexts (Donovan et al., 2023; Caria et al., 2024), suggesting that information interventions of this kind could be beneficial elsewhere. As online job portals become increasingly prevalent across lower-income countries (Carranza and McKenzie, 2024), they offer a viable mechanism for delivering information about labor market fundamentals at scale.

7 Conclusion

Widespread Internet availability is changing how job seekers in lower-income countries look for work. Using a field experiment, we show that providing local labor market information to job seekers can increase employment rates, with effects concentrated among employed respondents with low baseline expectations. Encouraging large job portals to publicly and systematically share market-level information could thus support individuals' search efforts and suggests a specific role for government policy in job search assistance. While our data do not allow us to track earnings trajectories over a longer horizon, this remains an important question for future research. The answer could help clarify whether reduced churn can help workers climb job ladders in low-income settings.

An important limitation of our experiment is its partial equilibrium nature: only a small fraction (<1%) of job seekers on the portal received the information treatments. Existing evidence from Altmann et al. (2022) shows that providing information to a larger share of unemployed individuals in local labor markets reduces positive employment effects due to spillovers. It is unclear how these results would translate to settings without centralized search platforms that unemployed individuals must use to maintain benefit eligibility. If search is more decentralized, as is common in lower-income countries, information about conditions on widely used portals may still be valuable—particularly when such platforms attract substantial participation from both job seekers and employers and reflect broader market conditions. Moreover, our findings show that labor market information influences the behavior of employed job seekers as well—shaping decisions in ways that may prevent unemployment in the first place. Given recent evidence from the U.S. also pointing to the pervasiveness of on-the-job search (Faberman et al., 2022), future research could therefore seek to understand market-level impacts of information provision for both employed and unemployed individuals over longer time horizons.

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8 Tables

Table 1: Overall effects on employment and earnings

	Employment status		Employment source		Earnings last month	
	(1)	(2)	(3)	(4)	(5)	(6)
	Employed	# workdays in last 7 days	Portal	Non-portal	Positive	Amount (Rs)
Panel A: Pooled effects						
Treatment	0.048**	0.307**	0.000	0.052**	0.043**	1,320.148**
	(0.022)	(0.128)	(0.004)	(0.022)	(0.021)	(524.328)
Panel B: Effects by information type						
Demand (D)	0.037	0.263	-0.001	0.042	0.038	996.397
	(0.028)	(0.166)	(0.005)	(0.028)	(0.028)	(696.891)
Supply (S)	0.040	0.235	0.003	0.038	0.039	1,085.037*
	(0.025)	(0.144)	(0.005)	(0.025)	(0.024)	(600.723)
Tightness (T)	0.062**	0.403***	-0.001	0.070***	0.050**	1,729.112***
	(0.025)	(0.145)	(0.004)	(0.025)	(0.024)	(617.990)
p-value: T=D	0.314	0.344	0.849	0.270	0.628	0.276
p-value: T=S	0.287	0.168	0.277	0.126	0.575	0.245
p-value: D=S	0.896	0.850	0.470	0.875	0.979	0.891
N	3430	3312	3374	3374	3170	3170
Control Mean	0.401	2.047	0.009	0.380	0.308	5665.892

Notes: This table considers overall impacts on employment and earnings. Panel A shows impacts pooling across treatments, whereas Panel B reports impacts separately by information type and p-values from tests of equality of coefficients between treatment types. Outcomes are measured during the follow-up surveys. The dependent variables report: whether the respondent is employed at the time of the follow-up survey (column 1); the number of days worked out of the last 7 days (column 2); whether the source of current employment is the QuirkJobs portal (column 3) or another non-portal methods, including other online and offline search methods such as other job websites excluding QuikrJobs, networks, employment agencies and direct walk-ins (column 4); whether the respondent reported positive earnings in the last month (column 5); and the amount earned last month (column 6). Earnings are measured in Indian rupees and top 0.5% of values are winsorized. Variable in columns 2–6 are coded as zero for non-employed individuals at the follow-up survey. The number of observations is lower in column 2 because the variable was only collected in the long version of the follow-up survey, while it is lower in columns 3–6 due to item non-response. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female, and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 2: Overall effects on search activity

	Search method: Portal			Search method: Non-portal		
	(1) Used search method	(2) Received offer	(3) Accepted offer	(4) Used search method	(5) Received offer	(6) Accepted offer
Panel A: Pooled effects						
Treatment	0.049**	-0.017	-0.003	-0.000	0.002	0.022
	(0.022)	(0.018)	(0.007)	(0.019)	(0.022)	(0.020)
Panel B: Effects by information type						
Demand (D)	0.071**	-0.025	-0.009	0.010	-0.013	0.020
	(0.028)	(0.023)	(0.008)	(0.024)	(0.029)	(0.026)
Supply (S)	0.020	-0.013	0.003	-0.005	0.001	0.024
	(0.025)	(0.021)	(0.008)	(0.022)	(0.025)	(0.022)
Tightness (T)	0.067***	-0.016	-0.008	-0.002	0.010	0.020
	(0.025)	(0.021)	(0.008)	(0.022)	(0.025)	(0.022)
p-value: T=D	0.894	0.639	0.819	0.569	0.381	0.974
p-value: T=S	0.023	0.857	0.082	0.872	0.683	0.831
p-value: D=S	0.044	0.535	0.085	0.480	0.590	0.836
N	3430	3321	3285	3430	3429	3294
Control Mean	0.564	0.214	0.026	0.765	0.452	0.230

Notes: This table considers impacts on search activity via different search methods. Panel A shows impacts pooling across treatments, whereas Panel B reports impacts separately by information type and p-values from tests of equality of coefficients between treatment types. Outcomes are measured during the follow-up surveys. Columns 1–3 consider search outcomes using the QuikrJobs portal, whereas columns 4–6 consider outcomes from all non-portal methods. Non-portal methods include other online and offline search methods such as other job websites excluding QuikrJobs, networks, employment agencies, and direct walk-ins. The dependent variables consider whether the respondent: used the portal for job search since baseline (column 1); received or accepted a job offer via the portal (columns 2 and 3); used any non-portal search methods since baseline (column 4); and received or accepted a job offer from any non-portal search methods (columns 5 and 6). The control mean is the mean of the outcome variable for the control group. Observations for job offer variables are lower due to item non-response and because job offer acceptance information was only collected in the long version of the follow-up survey. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 3: Search and employment effects for initially employed job seekers

	Used method since baseline		Employment status	Employment source		
	(1)	(2)	(3)	(4)	(5)	(6)
	Portal	Non-portal	Employed	Portal	Non-portal	Employed at baseline job
Panel A: Pooled effects						
Treatment	0.042	-0.081**	0.082*	0.002	0.082*	0.055
	(0.043)	(0.033)	(0.043)	(0.008)	(0.043)	(0.042)
Panel B: Effects by information type						
Demand (D)	0.071	-0.086**	0.076	0.012	0.069	0.057
	(0.052)	(0.043)	(0.053)	(0.013)	(0.054)	(0.053)
Supply (S)	0.025	-0.084**	0.064	0.007	0.059	0.050
	(0.048)	(0.038)	(0.048)	(0.010)	(0.049)	(0.048)
Tightness (T)	0.041	-0.075**	0.104**	-0.008	0.114**	0.060
	(0.048)	(0.038)	(0.048)	(0.008)	(0.048)	(0.048)
p-value: T=D	0.491	0.790	0.547	0.087	0.332	0.940
p-value: T=S	0.691	0.793	0.314	0.083	0.166	0.797
p-value: D=S	0.306	0.971	0.790	0.720	0.826	0.888
N	1005	1005	1005	983	983	954
Control Mean	0.571	0.841	0.529	0.012	0.506	0.340

Notes: This table shows search and employment effects for the subsample of initially *employed* job seekers. Panel A shows impacts pooling across treatments, whereas Panel B reports impacts separately by information type and p-values from tests of equality of coefficients between treatment types. Outcomes are measured during the follow-up surveys. Employment at baseline is measured by whether an individual worked for pay in the last 30 days. The dependent variables are defined as follows: since baseline, whether the respondent used the QuikrJobs portal for job search (column 1) or used non-portal search methods (column 2); whether the respondent is employed at the time of the follow-up survey (column 3); whether the respondent's source of current employment is the portal (column 4) or a non-portal method (column 5); and, whether the respondent is employed at their baseline job (column 6). Columns 4–6 are coded as 0 for non-employed individuals at the follow-up survey. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Search and employment effects for initially unemployed job seekers

	Used search method since baseline		Employment status	Employment source	
	(1) Portal	(2) Non-portal	(3) Employed	(4) Portal	(5) Non-portal
			Panel A: Pooled effects		
Treatment	0.046*	0.027	0.026	-0.001	0.030
	(0.028)	(0.025)	(0.027)	(0.005)	(0.027)
			Panel B: Effects by information type		
Demand (D)	0.060	0.040	0.005	-0.007	0.018
	(0.036)	(0.031)	(0.035)	(0.005)	(0.035)
Supply (S)	0.013	0.023	0.025	0.001	0.022
	(0.031)	(0.028)	(0.030)	(0.005)	(0.030)
Tightness (T)	0.074**	0.025	0.038	0.001	0.044
	(0.031)	(0.028)	(0.030)	(0.006)	(0.030)
p-value: T=D	0.650	0.592	0.292	0.089	0.393
p-value: T=S	0.017	0.903	0.602	0.999	0.376
p-value: D=S	0.145	0.519	0.529	0.085	0.900
N	2226	2226	2226	2196	2196
Control Mean	0.559	0.731	0.355	0.008	0.337

Notes: This table shows search and employment outcomes for the subsample of initially *unemployed* job seekers. Panel A shows impacts pooling across treatments, whereas Panel B reports impacts separately by information type and p-values from tests of equality of coefficients between treatment types. Outcomes are measured during the follow-up surveys. Unemployment at baseline is defined as an individual not having worked for pay in the last 30 days. The dependent variables are defined as follows: since baseline, whether the respondent used the QuikrJobs portal for job search (column 1) or used non-portal search methods (column 2); whether the respondent is employed at the time of the follow-up survey (column 3); and, whether the respondent's source of current employment is the portal (column 4) or a non-portal method (column 5). Columns 4–5 are coded as 0 for non-employed individuals at the follow-up survey. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 5: Heterogeneous effects by labor market expectations

	Full sample	Initially employed only		Initially unemployed only
	(1)	(2)	(3)	(4)
	Employed	Employed	Employed at baseline job	Employed
Panel A: Job finding priors				
Treatment	0.091***	0.130**	0.102*	0.069*
	(0.030)	(0.060)	(0.059)	(0.036)
Treatment × High Prior	-0.085*	-0.091	-0.089	-0.087
	(0.044)	(0.085)	(0.083)	(0.054)
High Prior	0.083**	0.044	0.060	0.106**
	(0.040)	(0.078)	(0.075)	(0.049)
Estimate: Treat+Treat×High Prior	0.006	0.039	0.013	-0.018
p-value: Treat+Treat×High Prior=0	0.845	0.517	0.831	0.639
Panel B: Reservation wage (RW)				
Treatment	0.081***	0.162**	0.117*	0.051
	(0.031)	(0.066)	(0.061)	(0.037)
Treatment × High RW	-0.065	-0.139	-0.109	-0.049
	(0.044)	(0.086)	(0.084)	(0.054)
High RW	0.072*	0.146*	0.109	0.040
	(0.040)	(0.079)	(0.076)	(0.049)
Estimate: Treat+Treat×High RW	0.016	0.023	0.008	0.002
p-value: Treat+Treat×High RW=0	0.595	0.680	0.893	0.962
N	3430	1005	954	2226
Control Mean	0.401	0.529	0.340	0.355

Notes: This table considers heterogeneity in employment effects by baseline labor market expectations. Panel A focuses on heterogeneity by respondents' job finding prior at baseline, measured as the percent chance the respondent will find and accept a job via the QuikrJobs portal in the next 30 days. Panel B focuses on heterogeneity by baseline reservation wage, which is measured by asking respondents the minimum monthly salary they would require to accept a new full-time job in the next 3 months. "Treatment" is a pooled indicator for any of the different information treatments. "High Prior" and "High RW" and binary indicators for individuals whose labor market belief is above (or equal to) the sample median. "Treatment×High RW/Prior" is the interaction term. Column 1 considers the full sample; columns 2–3 restrict to initially employed job seekers; and column 4 restricts to initially unemployed job seekers. Columns 1, 2, and 4 report whether the respondent is employed at the follow-up survey. Column 3 asks whether initially employed respondents are employed at their baseline job. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Table 6: Heterogeneous employment effects by level of job competition

	Full sample	Initially employed only		Initially unemployed only
	(1)	(2)	(3)	(4)
	Employed	Employed	Employed at baseline job	Employed
Treatment	0.062**	0.137**	0.110*	0.024
	(0.031)	(0.065)	(0.062)	(0.038)
Treatment×LC	-0.028	-0.099	-0.104	0.004
	(0.044)	(0.086)	(0.084)	(0.054)
Low Competition (LC)	0.015	0.126	0.010	-0.021
	(0.049)	(0.101)	(0.097)	(0.058)
Estimate: Treat+Treat×LC	0.035	0.038	0.006	0.028
p-value: Treat+Treat×LC=0	0.259	0.508	0.920	0.461
N	3430	1005	954	2226
Control Mean	0.401	0.529	0.340	0.355

Notes: This table considers heterogeneity in employment effects by market-level competition for jobs on the portal. "Low Competition (LC)" is a binary indicator for whether the respondent's preferred local labor market (city×occupation) has a lower than sample median ratio of applicants per new job posting on the portal (median=30). "Treatment" is a pooled indicator across the different information treatments. "Treatment×LC" is the interaction term. Column 1 considers the full sample; columns 2–3 restrict to initially employed job seekers; and column 4 restricts to initially unemployed job seekers. Columns 1, 2, and 4 report whether the respondent is employed at the follow-up survey. Column 3 asks whether initially employed respondents are employed at their baseline job. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

ONLINE APPENDIX

The Grass is *Not* Always Greener: The Effects of Local Labor Market Information on Search and Employment

A. Nilesh Fernando and Niharika Singh

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Appendix A Sample and treatment details

Appendix A.1 Summary statistics

	Mean	N
Panel A: Demographics		
Female†	0.51	5963
Age†	27.26	5151
Has college or higher degree	0.63	6118
Skill test score (out of 10)	6.33	6155
Panel B: Employment background		
Has ever worked for pay	0.68	5800
Has worked for pay in last 30 days	0.31	5800
Earnings $<$ Rs. 15k last month	0.48	1695
Panel C: Job search behavior and beliefs		
Searching for at least 1 month	0.67	5771
Searching for more than 6 months	0.19	5771
Searched $>=$ 1 hour in last 7 days	0.85	5700
# search methods used in last 30 days (out of 6)	2.33	5612
Used QuikrJobs portal in last 30 days	0.74	5612
Applied to a job on QuikrJobs portal in last 30 days ‡	0.45	6130
Used non-portal methods in last 30 days	0.78	5612
Minimum salary to accept new full-time job in 3m	30321.36	6155
% chance of new job via portal in next 30 days	66.38	6155

Notes: This table shows summary statistics on respondents' demographics, employment, and job search behavior and beliefs. Data are from baseline surveys, unless denoted by † or ‡ (see below). For variables measured at baseline, there are fewer than 6,155 observations due to missing data from item non-response. "Skill test score" is the number of correct answers on a skills test with 10 English and Math questions. "Earnings < Rs. 15k last month" is only reported for those who worked for pay in the last 30 days. "# search methods used in last 30 days" include six distinct methods we asked about (portal, other job websites, networks, newspapers, direct walk-ins, and employment agencies). "Used non-portal methods in the past 30 days" refers to those who report using other job portals excluding QuikrJobs, networks, newspapers, employment agencies, or a direct walk-in approach. "% chance of accepting new job via portal in next 30 days" is a shortened description for a question that asked respondents about the percent chance of finding a new job via the portal that they would be willing to join in the next 30 days.
† These variables are compiled using follow-up surveys and the portal's administrative data. For the "Female" variable, we additionally used a name-based gender prediction algorithm and retained matches with at least 90% accuracy.
‡ This variable comes from the portal's administrative data.

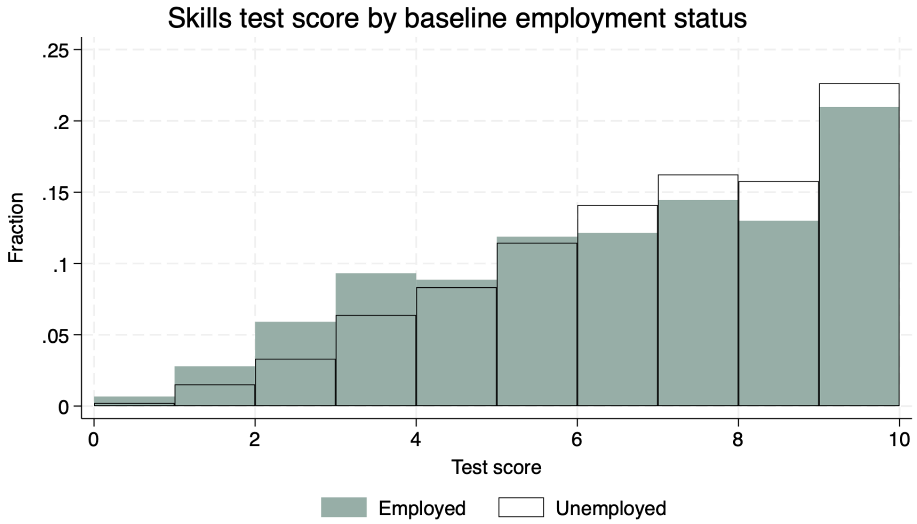
Appendix A.2 Descriptives by baseline employment status

Appendix A.2.1 Summary statistics

	Unemployed (1)	Employed (2)	Difference (1-2) (3)
Panel A: Demographics			
Female†	0.57	0.39	0.18***
Male†	0.43	0.61	-0.18***
Age†	27.00	28.11	-1.11***
Has college or higher degree	0.63	0.64	-0.01
Skill test score (out of 10)	6.54	6.11	0.43***
Panel B: Job Search Behavior and Beliefs			
Searching for at least 1 month	0.70	0.61	0.09***
Searching for more than 6 months	0.20	0.17	0.03***
Searched >= 1 hour in last 7 days	0.86	0.83	0.02**
# search methods used in last 30 days	2.35	2.36	-0.01
Used QuikrJobs portal in last 30 days	0.75	0.73	0.02
Applied on QuikrJobs portal in last 30 days‡	0.45	0.43	0.03*
Used non-portal methods in last 30 days	0.79	0.77	0.02*
Minimum salary to accept new full-time job in 3m	29755.80	31564.97	-1,809.17***
% chance of new job via portal in 30 days	67.05	66.59	0.46

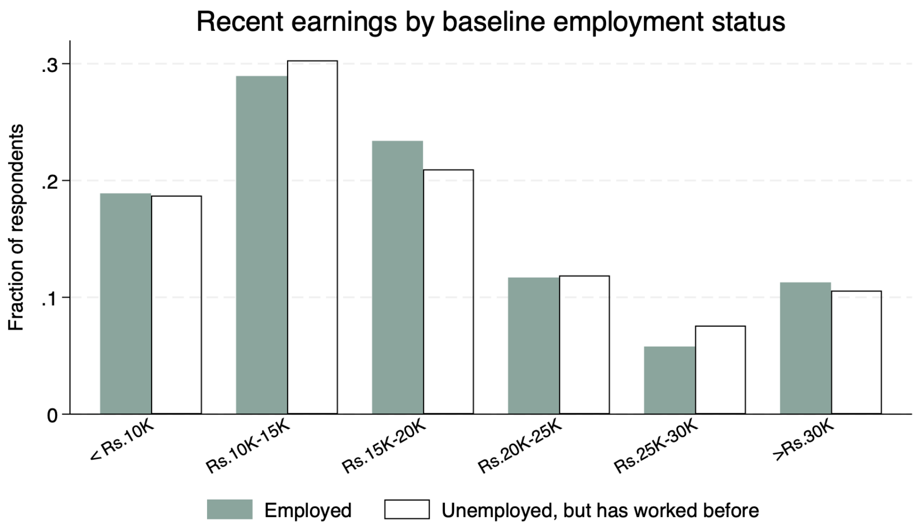
Notes: This table shows summary statistics separately for initially employed and unemployed job seekers at baseline. Sample is restricted to those that provided their employment status at baseline (5800 out of 6,155 respondents). Data are from baseline surveys, unless denoted by † or ‡ (see below). See Appendix A.1 for variable definitions. Columns 1–2 show means for unemployed and employed respondents, respectively. Column 3 shows the coefficient from a simple difference in means between the two groups for each covariate. * p<0.10, ** p<0.05, *** p<0.01
† These variables are compiled using follow-up surveys and the portal's administrative data. For the sex variables, we additionally used a name-based gender prediction algorithm and retained matches with at least 90% accuracy.
‡ This variable comes from the portal's administrative data.

Appendix A.2.2 Skills test score distribution



Note: This figure shows the distribution of skills test scores (out of 10) achieved by employed and unemployed respondents at baseline. The skills test was administered as part of the baseline survey and consisted of 8 questions covering basic English skills and 2 math problems.

Appendix A.2.3 Recent earnings distribution



Note: This figure shows the distribution of recent earnings at baseline for employed and unemployed respondents with prior paid work experience. Unemployed respondents with paid work experience are 37% of the overall sample and 53% of all unemployed respondents; their earnings reflect typical monthly earnings the last time they worked for pay. Earnings for employed respondents reflect the total earnings from the previous month.

Appendix A.3 Comparison of study sample to PLFS sample

The table below compares the experimental sample to adults aged 18-60 years living in the same urban areas as our sample using the 2022-23 Periodic Labor Force Survey (PLFS).

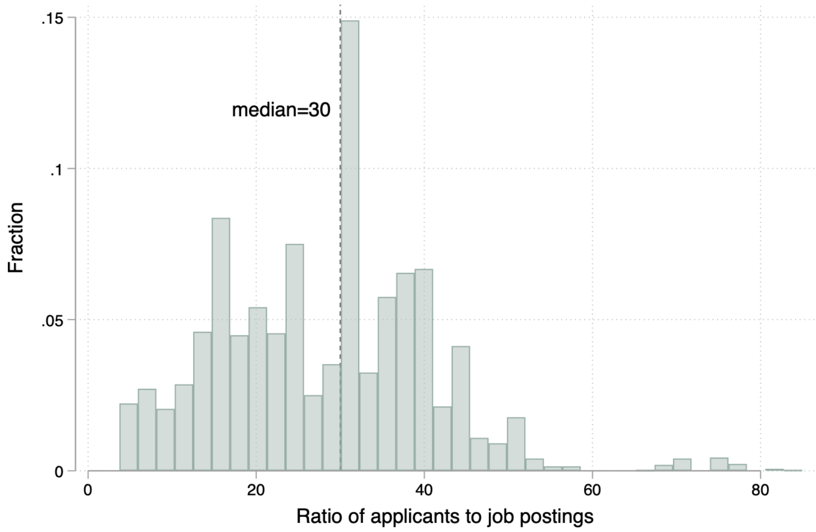
	Study Sample		
	All respondents	Employed respondents	PLFS Sample
	(1)	(2)	(3)
Female†	0.51	0.39	0.49
Age†	27.3	28.1	36.7
Has college degree	0.63	0.64	0.29
Has worked recently	0.31	1	0.51
Earnings < Rs. 15k last month	0.48	0.48	0.48

Notes: This table compares our study sample to a sample of adults from urban areas in the Periodic Labor Force Survey (PLFS) 2022-23. To construct the PLFS sample, we only use the first visit observations and restrict to individuals between the ages of 18-60 living in the same urban areas as our study sample. Employed respondents are those who reported working for pay in the last 30 days. "Has worked recently" is measured in the study sample as having worked anywhere for pay in the last 30 days; in the PLFS sample, we use the "Current Weekly Status," which measures employment on the basis of a 7 day reference period, and include all codes related to working in household enterprises or salaried/wage labor. "Earnings < Rs. 15k last month" is computed conditional on the work status definition used in "Has worked recently."

† In the study sample, these variables are compiled using follow-up surveys and the portal's administrative data. For the "Female" variable, we additionally used a name-based gender prediction algorithm and retained matches with at least 90% accuracy. The remaining variables are from the baseline survey.

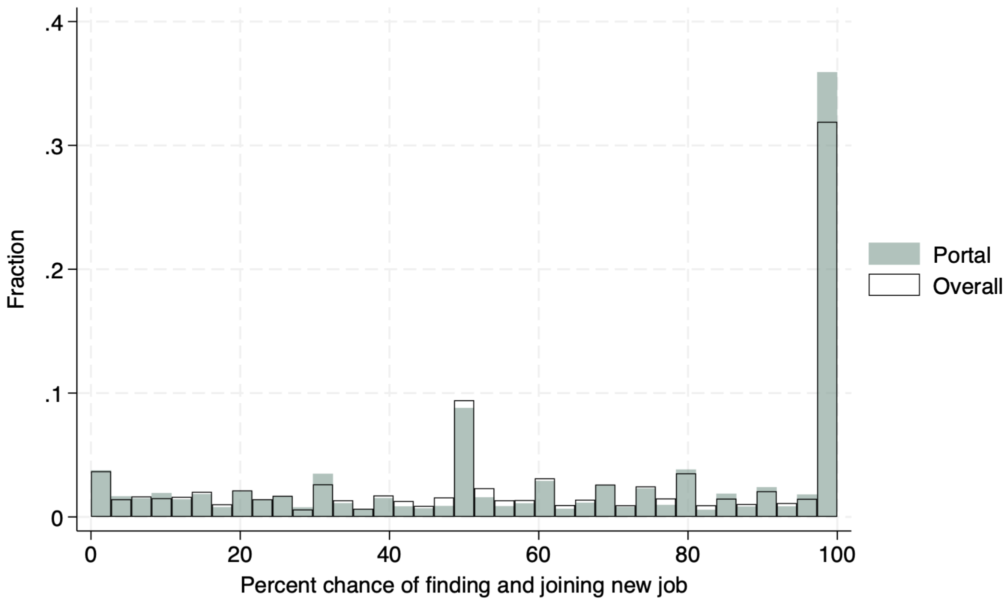
Appendix A.4 Actual and perceived labor market conditions

Appendix A.4.1 Job competition on the portal



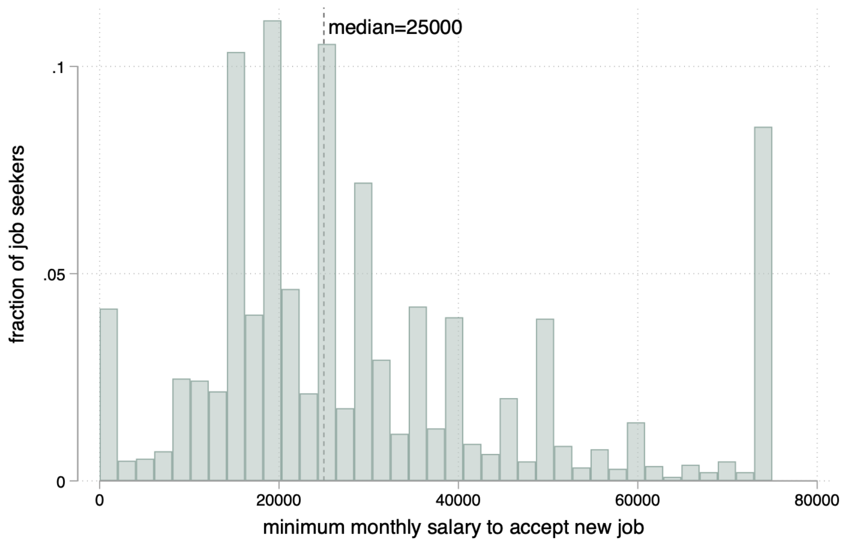
Note: This figure shows the distribution of job competition faced by the respondents in their preferred local labor market. Job competition is computed as the ratio of the number of unique applicants in a month to the number of new job postings in the same month for a given city-role combination.

Appendix A.4.2 Job finding beliefs



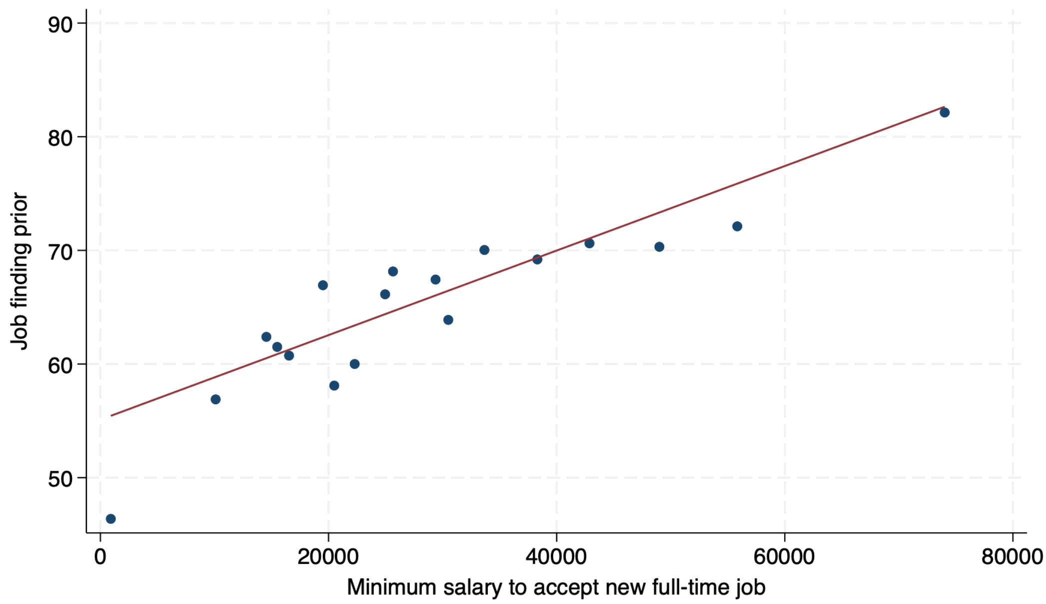
Note: This figure shows the distribution of respondents' job finding beliefs at baseline. Respondents were asked about the percent chance of finding a new job that they would be willing to join in the next 30 days through (i) the QuikrJobs portal and (ii) overall, that is, across all search methods.

Appendix A.4.3 Minimum monthly salary needed to accept new job



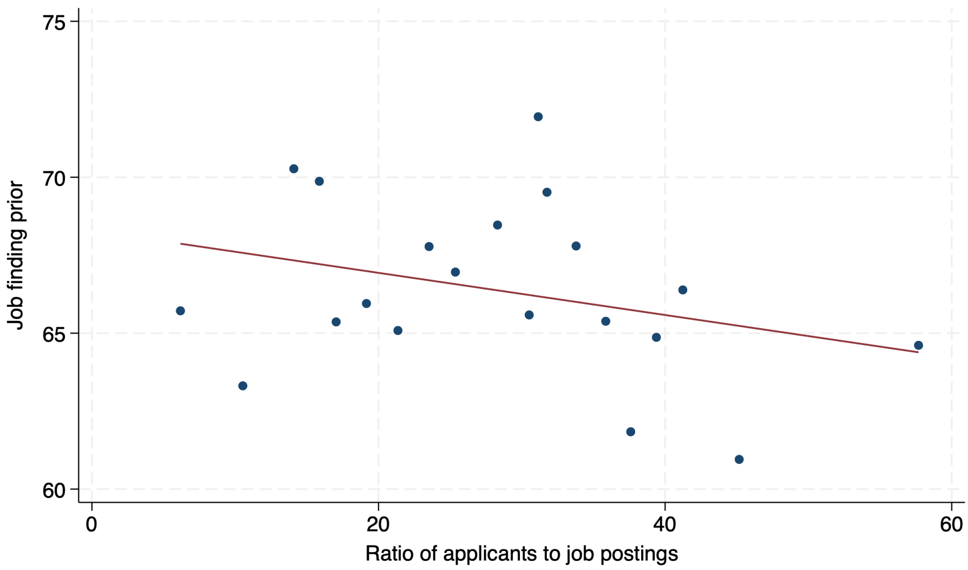
Note: This figure shows the distribution of reservation wages at baseline. Reservation wages are measured as the minimum monthly salary respondents required to accept a new full-time job in the next three months; this variable is top-coded at Rs.75,000 in the baseline survey.

Appendix A.4.4 Relationship between reservation wages and job finding beliefs



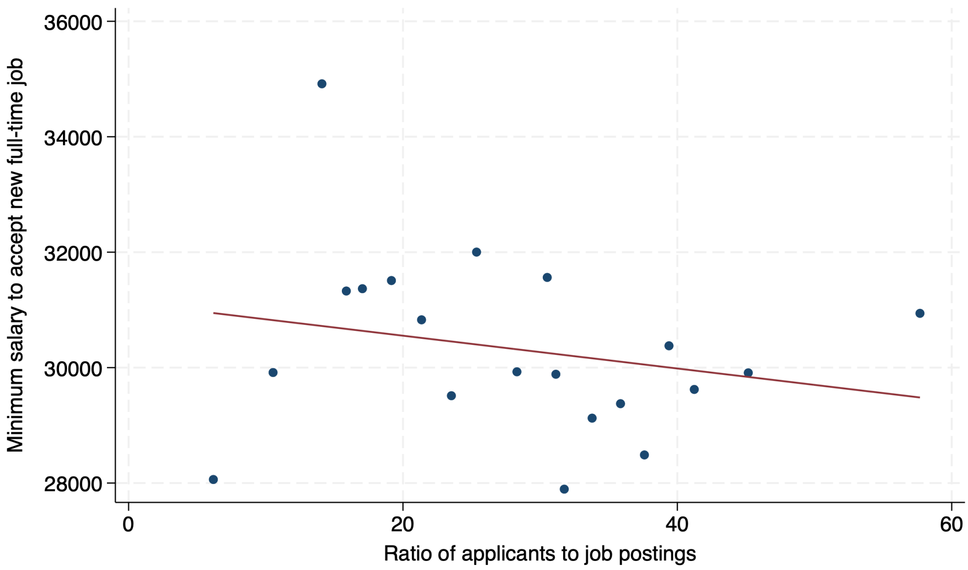
Note: This figure shows a binned scatterplot of respondents' reservation wages and job finding beliefs at baseline. Reservation wages are measured as the minimum monthly salary respondents required to accept a new full-time job in the next three months; this variable was top-coded at Rs.75,000 in the baseline survey. Job finding prior is defined as the percent chance of finding a new job via the QuikrJobs portal in the next 30 days that the respondent would be willing to join.

Appendix A.4.5 Job competition and job finding beliefs about the portal



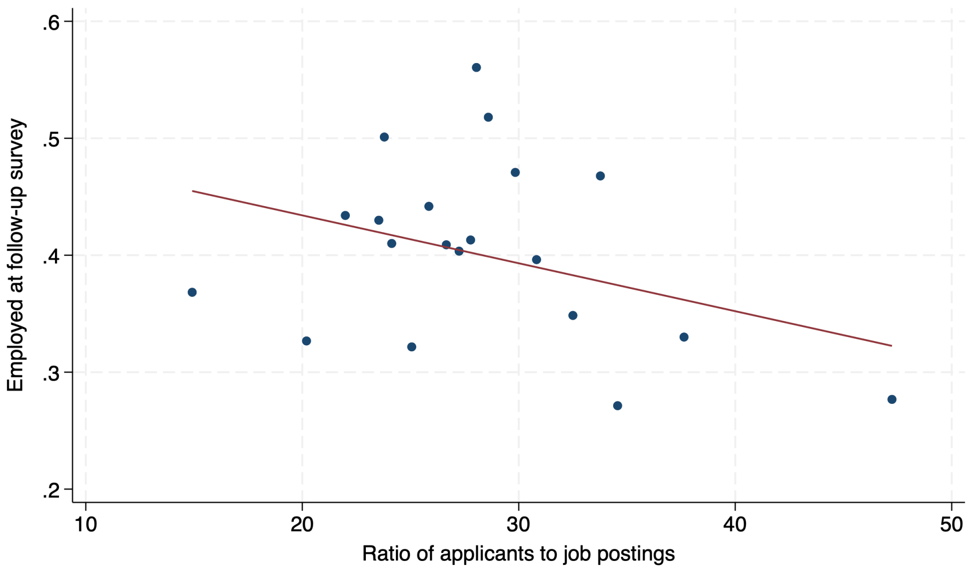
Note: This figure shows a binned scatterplot of the job competition on the portal and respondents' baseline job-finding belief via the portal. Beliefs data are from the baseline survey whereas the job competition measures come from the portal's administrative data for respondents' preferred local labor market.

Appendix A.4.6 Job competition and reservation wages



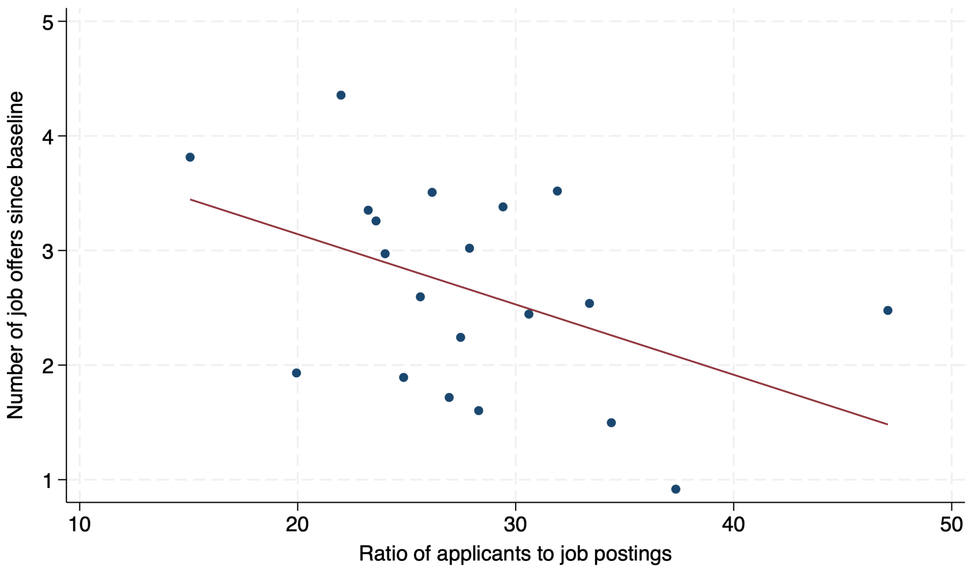
Note: This figure shows a binned scatterplot of the job competition on the portal and respondents' baseline reservation wages. Reservation wages are measured as the minimum monthly salary respondents required to accept a new full-time job in the next three months; this variable is top-coded at Rs.75,000 in the baseline survey. Beliefs data are from the baseline survey whereas the job competition measures come from the portal's administrative data for respondents' preferred local labor market.

Appendix A.4.7 Job competition and employment at follow-up survey



Note: This figure shows a binned scatterplot of the likelihood of employment in the follow-up survey and job competition on the portal. Controls for city, occupation, and sampling batch fixed effects are included. Sample is restricted to control respondents only.

Appendix A.4.8 Job competition and realized job offers



Note: This figure shows a binned scatterplot of the number of offers received since baseline reported in the follow-up survey and job competition on the portal. Controls for city, occupation, and sampling batch fixed effects are included. Offer variable is winsorized at top 0.5%. Sample is restricted to control respondents only.

Appendix A.5 Examples of information treatments

Control

Job Search Tips

Apply to newer job postings, as these employers are more likely to be hiring.

Keep applying to jobs instead of waiting to hear back! If interested in your application, employers usually contact you within 30 days.

It takes many applications to get an interview or a job. Keep applying!

Demand

Monthly Overview of New Jobs in Delhi for Sales

New Job Postings	177
Average Minimum Salary	₹13,750
% Postings Requiring No Experience	72%

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

On average, in a month:

- 177 new Sales jobs are posted in Delhi
- These jobs offer an average minimum salary of Rs. 13,750
- 72% of these jobs do not require or mention any minimum experience

What about other roles in your city?
Compare and consider applying.

Monthly Overview of Similar Roles in Delhi

	Marketing	Host/ Hostess
New Job Postings	102	111
Average Minimum Salary	₹14,890	₹20,243
% Postings Requiring No Experience	78%	99%

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

See more helpful tips below!

Job Search Tips

Apply to newer job postings, as these employers are more likely to be hiring.

Keep applying to jobs instead of waiting to hear back! If interested in your application, employers usually contact you within 30 days.

It takes many applications to get an interview or a job. Keep applying!

Supply 1

Monthly Overview of Jobseekers in Delhi for Sales

Education level	% of Jobseekers
10th pass or below	17%
12th pass/Diploma	41%
Bachelor's degree	36%
Master's or above	7%
Report English Language Skills	55%

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

On average, in a month:

- 1,968 users apply to Sales jobs in Delhi, and about 65% of them share their education details
- 43% of these users report having a Bachelor's degree or above
- 55% report English skills on their profile

What about other roles in your city?
Compare and consider applying.

Monthly Overview of Similar Roles in Delhi

	% of Jobseekers	
Education level	Marketing	Host/ Hostess
10th pass or below	13%	18%
12th pass/Diploma	36%	44%
Bachelor's degree	39%	32%
Master's or above	11%	6%
Report English Language Skills	57%	60%

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

See more helpful tips below!

Job Search Tips

Apply to newer job postings, as these employers are more likely to be hiring.

Keep applying to jobs instead of waiting to hear back! If interested in your application, employers usually contact you within 30 days.

It takes many applications to get an interview or a job. Keep applying!

Supply 2

Monthly Overview of Jobseekers in Delhi for Sales

Active Users	1,968
Total Applications	3,845
% users applying to more than 1 job	33%

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

On average, in a month:

- 3,845 applications are submitted by 1,968 users in Delhi for Sales jobs
- About 33% of these users apply to more than 1 Sales job

What about other roles in your city?
Compare and consider applying.

Monthly Overview of Similar Roles in Delhi

	Marketing	Host/ Hostess
Active Users	1,029	2,991
Total Applications	1,745	6,469
% users applying to more than 1 job	27%	40%

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

See more helpful tips below!

Job Search Tips

Apply to newer job postings, as these employers are more likely to be hiring.

Keep applying to jobs instead of waiting to hear back! If interested in your application, employers usually contact you within 30 days.

It takes many applications to get an interview or a job. Keep applying!

Tightness 1

Monthly Overview in Delhi for Sales

New Job Postings	177
Active Users	1,968
Total Applications	3,845

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

On average, in a month:

- 177 new Sales jobs are posted in Delhi
- 3,845 applications are submitted by 1,968 users in Delhi for Sales jobs

What about other roles in your city?
Compare and consider applying.

Monthly Overview of Similar Roles in Delhi

	Marketing	Host/ Hostess
New Job Postings	102	111
Active Users	1,029	2,991
Total Applications	1,745	6,469

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

See more helpful tips below!

Job Search Tips

Apply to newer job postings, as these employers are more likely to be hiring.

Keep applying to jobs instead of waiting to hear back! If interested in your application, employers usually contact you within 30 days.

It takes many applications to get an interview or a job. Keep applying!

Tightness 2

Monthly Overview in Delhi for Sales

New Job Postings	177
Active Users	1,968
Total Applications	3,845
% applications accessed by employers (within 10 days)	18%
% applications accessed by employers (within 30 days)	19%

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

On average, in a month:

- 177 new Sales jobs are posted in Delhi
- 3,845 applications are submitted by 1,968 users in Delhi for Sales jobs
- Employers access contact details of 19% of these applications within 30 days, meaning you have *upto* a 19% chance of being contacted. Do not wait, keep applying!

What about other roles in your city?
Compare and consider applying.

Monthly Overview of Similar Roles in Delhi

	Marketing	Host/ Hostess
New Job Postings	102	111
Active Users	1,029	2,991
Total Applications	1,745	6,469
% applications accessed by employers (within 10 days)	30%	46%
% applications accessed by employers (within 30 days)	32%	47%

The table above shows monthly averages using QuikrJobs data from Oct-Dec 2020

See more helpful tips below!

Job Search Tips

Apply to newer job postings, as these employers are more likely to be hiring.

Keep applying to jobs instead of waiting to hear back! If interested in your application, employers usually contact you within 30 days.

It takes many applications to get an interview or a job. Keep applying!

Appendix A.6 Descriptives by labor market beliefs

Appendix A.6.1 Summary statistics

	Job-finding prior			Reservation Wage		
	Low	High	Difference	Low	High	Difference
Panel A: Demographics						
Female†	0.51	0.52	-0.01	0.55	0.49	0.06***
Age†	27.28	27.25	0.02	25.78	28.53	-2.75***
Has college degree	0.61	0.65	-0.04***	0.59	0.66	-0.07***
Skill test score (out of 10)	6.07	6.59	-0.52***	6.19	6.46	-0.27***
Panel B: Employment background						
Has previously worked for pay	0.67	0.68	-0.01	0.66	0.69	-0.02*
Has worked for pay in last 30 days	0.31	0.31	0.00	0.29	0.33	-0.04***
Earnings $<$ Rs. 15k last month	0.47	0.48	-0.01	0.66	0.35	0.31***
Panel C: Job Search Behavior and Beliefs						
Searching for at least 1 month	0.67	0.67	0.00	0.67	0.67	0.01
Searching for more than 6 months	0.20	0.18	0.01	0.18	0.20	-0.01
Searched $>=$ 1 hour in last 7 days	0.83	0.86	-0.03***	0.86	0.84	0.02
# search methods used in last 30 days	2.22	2.43	-0.21***	2.33	2.33	0.00
Used QuikrJobs portal in last 30 days	0.73	0.75	-0.02	0.76	0.73	0.03**
Applied on QuikrJobs portal in last 30 days‡	0.45	0.44	0.02	0.47	0.43	0.03***
Used non-portal methods in last 30 days	0.76	0.80	-0.04***	0.77	0.78	-0.01

Notes: This table compares baseline characteristics of job seekers with below- and above-median labor market beliefs, separately for job-finding priors (columns 1–3) and reservation wages (columns 4–6). The median job-finding prior is 75% and the median reservation wage is Rs.25,000. Data are from baseline surveys, unless denoted by † or ‡ (see below). Columns 1 and 4 show means for job seekers with below-median beliefs, columns 2 and 5 show means for those with above-median beliefs, and columns 3 and 6 show the coefficient from a simple difference in means between the two groups for each covariate. * p<0.10, ** p<0.05, *** p<0.01
† These variables are compiled using follow-up surveys and the portal's administrative data. For the sex variables, we additionally used a name-based gender prediction algorithm and retained matches with at least 90% accuracy.
‡ This variable comes from the portal's administrative data.

Appendix B Robustness

Appendix B.1 Randomization balance

	(1) Control Mean	(2) Treatment	(3) N	(4) Test: S=D=T=0
# non-missing item responses (out of 45)	40.504	0.024	6,155	0.497
		(0.083)		
Has at least college degree	0.614	0.018	6,118	0.639
		(0.016)		
Skill test score (out of 10)	6.311	0.026	6,155	0.760
		(0.078)		
Has ever worked for pay	0.655	0.020	5,800	0.241
		(0.016)		
Has worked for pay in last 30 days	0.301	0.001	5,800	0.112
		(0.016)		
Used portal for job search in last 30 days	0.758	-0.011	5,612	0.868
		(0.015)		
Applied on portal in last 30 days‡	0.461	-0.006	6,130	0.508
		(0.016)		
% chance of new job via portal	65.222	1.476	6,155	0.509
		(1.151)		
Minimum salary to accept new job	29,749.065	625.689	6,155	0.556
		(649.461)		
F-test p-value		0.589		

Notes: This table considers randomization balance. Column 1 shows the control group mean. Column 2 shows coefficients and standard errors from a regression comparing job seekers assigned to the (pooled) treatment group versus the control group. Column 3 shows the number of observations used in the regression for each variable; there are fewer observations for some variables due to item non-response. Column 4 reports p-values from a joint test conducted after regressing the covariate on separate indicators for information treatments—$Supply(S)$, $Demand(D)$, and $Tightness(T)$— and testing whether their regression coefficients are jointly zero. The last row shows the F-test p-value from a test that the listed covariates jointly predict being assigned to treatment; for this exercise, we code missing observations as -99 and add indicator variables tagging these observations in order to use the full sample for computing the test p-value. "# non-missing item responses (out of 45)" is a count of the number of questions with non-missing data from the baseline survey. Data are from baseline surveys, except if denoted by ‡, in which case the variable comes from the portal's administrative data. Regressions include fixed effects for city×occupation and sampling batch. * p<0.10, ** p<0.05, *** p<0.01

Appendix B.2 Randomization balance by baseline employment status

	Initially employed		Initially unemployed	
	(1) Control Mean	(2) Treatment	(3) Control Mean	(4) Treatment
# non-missing item responses (out of 45)	40.153	0.048	41.029	-0.047
		(0.135)		(0.091)
Has at least college degree	0.645	-0.015	0.607	0.027
		(0.030)		(0.020)
Skill test score (out of 10)	6.060	0.055	6.476	0.056
		(0.158)		(0.092)
Used portal for job search in last 30 days	0.729	0.010	0.776	-0.022
		(0.030)		(0.018)
Applied on portal in last 30 days‡	0.463	-0.030	0.471	-0.008
		(0.029)		(0.020)
% chance of new job via portal	63.924	2.784	66.268	1.151
		(2.157)		(1.421))
Minimum salary to accept new job	31,061.462	126.899	29,173.104	828.017
		(1284.904)		(789.465)
F-test p-value		0.705		0.627

Notes: This table considers randomization balance separately for employed and unemployed job seekers at baseline. Odd numbered columns show the control group mean, while even numbered columns show the treatment difference between the pooled treatment indicator versus the control group. We run regressions separately by subgroups and for each covariate. The last row shows the F-test p-value from a test that the listed covariates jointly predict being assigned to treatment for the subgroup; for this exercise, we code missing observations as -99 and add indicator variables tagging these observations in order to use the full sample for computing the test p-value. "# non-missing item responses (out of 45)" is a count of the number of questions with non-missing data from the baseline survey. Data are from baseline surveys, except if denoted by ‡, in which case the variable comes from the portal's administrative data. Regressions include fixed effects for city×occupation and sampling batch. * p<0.10, ** p<0.05, *** p<0.01

Appendix B.3 Follow-up survey response rates

	Completed			Completed long survey		
	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	Initially employed	Initially unemployed	Full sample	Initially employed	Initially unemployed
Panel A: Pooled effects						
Treatment	-0.002	-0.002	0.003	0.005	-0.001	0.011
	(0.016)	(0.031)	(0.020)	(0.016)	(0.031)	(0.020)
Panel B: Effects by information type						
Demand (D)	0.007	0.009	0.015	0.016	0.009	0.027
	(0.021)	(0.039)	(0.027)	(0.021)	(0.039)	(0.027)
Supply (S)	0.003	-0.004	0.004	0.006	-0.002	0.006
	(0.018)	(0.035)	(0.023)	(0.019)	(0.035)	(0.023)
Tightness (T)	-0.011	-0.007	-0.003	-0.002	-0.006	0.009
	(0.018)	(0.035)	(0.023)	(0.018)	(0.035)	(0.023)
p-value: T=D	0.351	0.632	0.445	0.347	0.656	0.454
p-value: T=S	0.363	0.914	0.734	0.612	0.887	0.874
p-value: D=S	0.847	0.703	0.621	0.599	0.750	0.380
N	6155	1793	4007	6155	1793	4007
Control Mean	0.557	0.565	0.548	0.528	0.538	0.518

Notes: This table considers follow-up survey completion rates by treatment status. Panel A shows impacts pooling across treatments, whereas Panel B reports impacts separately by information type and p-values from tests of equality of coefficients between treatment types. The dependent variables are as follows: whether the respondent completed either the long or the short version of the follow-up survey (columns 1–3) or completed the long version of the follow-up survey (column 4–6). Columns 1 and 4 consider the full sample. Columns 2 and 5 restrict the sample to initially employed respondents, and columns 3 and 6 to initially unemployed respondents. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch.
* p<0.10, ** p<0.05, *** p<0.01

Appendix B.4 Differences in attriter characteristics

	(1) Control Mean	(2) Treatment	(3) N	(4) Test: S=D=T=0
# non-missing item responses (out of 45)	40.542	-0.021	2,725	0.280
		(0.123)		
Has at least college degree	0.616	0.002	2,703	0.706
		(0.024)		
Skill test score (out of 10)	6.304	0.068	2,725	0.564
		(0.114)		
Has ever worked for pay	0.649	0.023	2,569	0.757
		(0.024)		
Has worked for pay in last 30 days	0.293	0.001	2,569	0.552
		(0.023)		
Used portal for job search in last 30 days	0.741	0.005	2,491	0.855
		(0.023)		
Applied on portal in last 30 days‡	0.456	-0.009	2,708	0.818
		(0.024)		
% chance of new job via portal	64.770	1.565	2,725	0.528
		(1.729)		
Minimum salary to accept new job	30,086.498	89.489	2,725	0.762
		(971.681)		
F-test p-value		0.991		

Notes: This table checks whether the baseline characteristics of attriters in the follow-up survey vary by treatment status. Data are from baseline surveys, except if denoted by ‡, in which case the variable comes from the portal's administrative data. Sample is restricted to those 2,725 individuals for whom we do not observe employment status at the follow-up survey. Column 1 shows the control group mean. Column 2 shows coefficients and standard errors from a regression comparing covariates for job seekers assigned in the (pooled) treatment group to the control group. Column 3 shows the number of observations used in the regression for each variable; there are fewer than 2,725 observations for some variables due to item non-response in the baseline survey. Column 4 reports p-values from a joint test conducted after regressing the covariate on separate indicators for information treatments—$Supply(S)$, $Demand(D)$, and $Tightness(T)$— and testing whether their coefficients are jointly zero. The last row shows the F-test p-value from a test that the listed covariates jointly predict being assigned to treatment for this group of attriters; for this exercise, we code missing observations as -99 and add indicator variables tagging these observations in order to use the maximum sample for computing the test p-value. "# non-missing item responses (out of 45)" is a count of the number of items with non-missing data from the baseline survey. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. * p<0.10, ** p<0.05, *** p<0.01

Appendix B.5 Effects on employment by individual treatments

	Employment status		Employment source		Earnings last month	
	(1)	(2)	(3)	(4)	(5)	(6)
	Employed	# workdays in last 7 days	Portal	Non-Portal	Positive	Amount (Rs)
Demand	0.037	0.263	-0.001	0.042	0.038	995.804
	(0.028)	(0.166)	(0.005)	(0.029)	(0.028)	(697.157)
Supply, Skills	0.028	0.232	-0.006	0.041	0.035	1,173.593
	(0.028)	(0.167)	(0.004)	(0.028)	(0.028)	(714.321)
Supply, Effort	0.052*	0.239	0.012*	0.035	0.043	995.391
	(0.029)	(0.169)	(0.007)	(0.029)	(0.028)	(705.380)
Tightness, Basic	0.065**	0.439**	-0.004	0.076***	0.058**	1,632.165**
	(0.029)	(0.171)	(0.004)	(0.029)	(0.029)	(724.415)
Tightness, Extended	0.061**	0.369**	0.001	0.065**	0.043	1,814.455**
	(0.028)	(0.167)	(0.005)	(0.029)	(0.028)	(749.654)
N	3430	3312	3374	3374	3170	3170
Control Mean	0.401	2.047	0.009	0.380	0.308	5665.892

Notes: This table considers overall impacts on employment and earnings, fully separating all treatment variations. Outcomes are measured during the follow-up surveys. The dependent variables report: whether the respondent is employed at the time of the follow-up survey (column 1); the number of days worked out of the last 7 days (column 2); whether the source of current employment is the QuirkJobs portal (column 3) or another non-portal method (column 4); whether the respondent reported positive earnings in the last month (column 5); and the amount earned last month (column 6). Earnings are measured in Indian rupees and top 0.5% of values are winsorized. Variable in columns 2–6 are coded as zero for individuals not employed at the time of the follow-up survey. The number of observations is lower in column 2 because the variable was only collected in the long version of the follow-up survey, while it is lower in columns 3–6 due to item non-response. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Appendix B.6 Effects on search by individual treatments

	Portal search			Non-portal search		
	(1) Searched	(2) Received offer	(3) Accepted offer	(4) Searched	(5) Received offer	(6) Accepted offer
Demand	0.071**	-0.025	-0.009	0.010	-0.013	0.020
	(0.028)	(0.023)	(0.008)	(0.024)	(0.029)	(0.026)
Supply, Skills	0.020	-0.035	-0.002	0.009	0.013	0.043*
	(0.029)	(0.023)	(0.009)	(0.025)	(0.029)	(0.026)
Supply, Effort	0.021	0.010	0.009	-0.018	-0.011	0.005
	(0.029)	(0.025)	(0.010)	(0.025)	(0.029)	(0.025)
Tightness, Basic	0.057*	-0.012	-0.012	0.010	0.043	0.026
	(0.029)	(0.024)	(0.008)	(0.025)	(0.030)	(0.026)
Tightness, Extended	0.077***	-0.019	-0.003	-0.013	-0.021	0.015
	(0.028)	(0.024)	(0.009)	(0.025)	(0.029)	(0.025)
N	3430	3321	3285	3430	3429	3294
Control Mean	0.564	0.214	0.026	0.765	0.452	0.230

Notes: This table consider impacts on job search, fully separating all treatment variations. Outcomes are measured during the follow-up surveys. Columns 1–3 consider search outcomes using the QuikrJobs portal, whereas columns 4–6 consider outcomes from all non-portal methods. Non-portal methods include other online and offline search methods such as other job websites excluding QuikrJobs, networks, employment agencies, and direct walk-ins. The dependent variables consider whether the respondent: used the portal for job search since baseline (column 1); received or accepted a job offer via the portal (columns 2 and 3); used any non-portal search methods since baseline (column 4); and received or accepted a job offer from any non-portal search methods (columns 5 and 6). The control mean is the mean of the outcome variable for the control group. Observations for job offer variables are lower due to item non-response and because job offer acceptance information was only collected in the long version of the follow-up survey. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

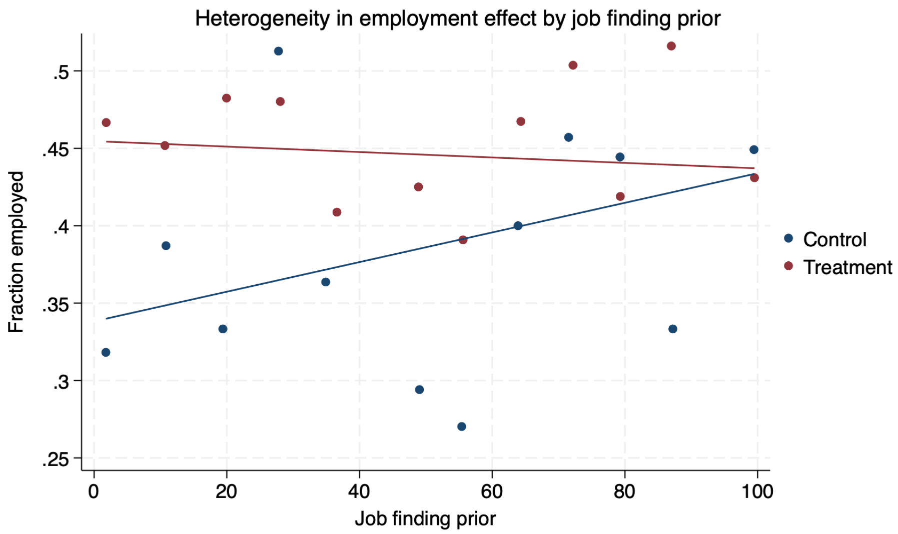
Appendix B.7 Robustness to attrition: Re-weighted estimates

	Full sample		Initially employed only	Initially unemployed only
	(1) Employed (Table 1, col 1)	(2) Non-portal job (Table 1, col 4)	(3) Employed (Table 3, col 3)	(4) Employed (Table 4, col 3)
Panel A: Pooled effects				
Treatment	0.046**	0.051**	0.086**	0.022
	(0.022)	(0.022)	(0.043)	(0.027)
Panel B: Effects by information type				
Demand (D)	0.028	0.034	0.077	-0.006
	(0.028)	(0.029)	(0.054)	(0.035)
Supply (S)	0.039	0.037	0.066	0.022
	(0.025)	(0.025)	(0.049)	(0.030)
Tightness (T)	0.063**	0.072***	0.110**	0.036
	(0.025)	(0.025)	(0.048)	(0.031)
p-value: T=D	0.164	0.134	0.477	0.172
p-value: T=S	0.234	0.100	0.274	0.597
p-value: D=S	0.683	0.889	0.816	0.350
N	3430	3374	1005	2226
Control Mean	0.401	0.380	0.529	0.355

Notes: This table reports our key employment results adjusted for attrition. Our adjustment involves first using a probit model to predict survey response using baseline variables shown in Appendix B.1, individual treatment indicators, and fixed effects for city×occupation and sampling batch of the respondent. We then use the inverse of the predicted values as weights for the regressions shown in this table. The column headers show the table and column numbers of the original results. Columns 1–2 reports effect on the full sample, whereas columns 3 and 4 report effects separating initially employed and unemployed respondents, respectively. Columns 1, 3 and 4 measured whether the respondent is employed at the time of the follow-up survey. Column 2 measures whether the source of current employment is a non-portal method and is coded as zero for non-employed individuals. The number of observations is lower in column 2 because the variable was only collected in the long version of the follow-up survey. The control mean is the mean of the outcome variable for the control group. Regressions include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Appendix B.8 Employment effect heterogeneity by perceived labor market conditions

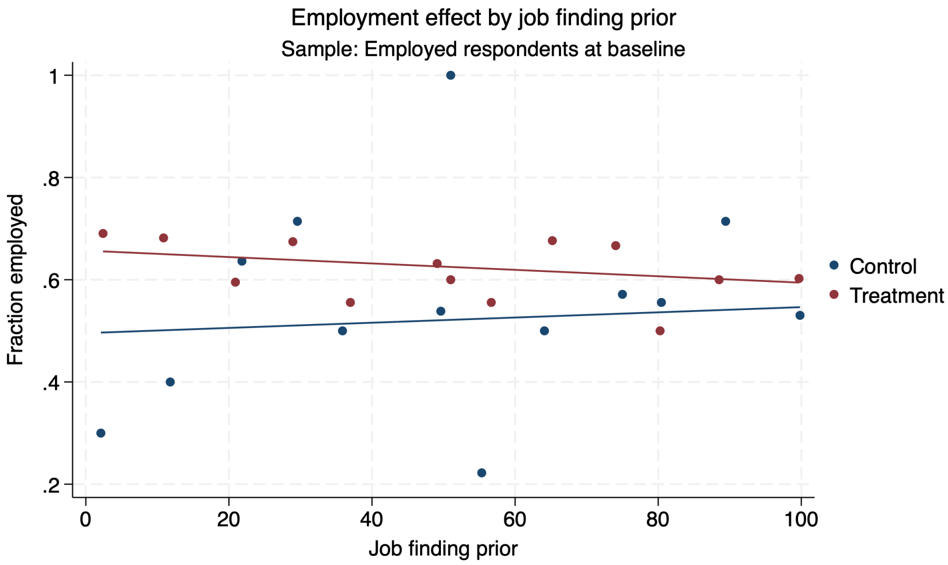
Appendix B.8.1 Full Sample



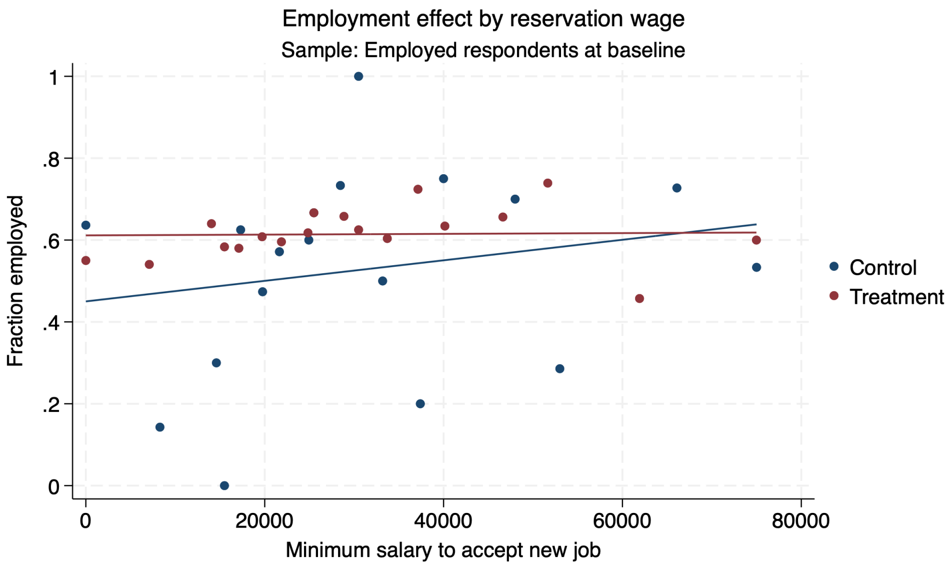
Notes: This figure shows a binned scatterplot of the treatment effect on employment in the follow-up survey by respondents' job-finding prior at baseline. The job finding prior measures the percent chance that the respondent will find and accept a new job using the QuikrJobs portal. Sample consists of all respondents—initially employed and unemployed—who completed the follow-up survey.

Notes: This figure shows a binned scatterplot of the treatment effect on employment in the follow-up survey by respondents' reservation wage at baseline. The reservation wage measures the minimum monthly salary required to accept a new full-time job. Sample consists of all respondents—initially employed and unemployed—who completed the follow-up survey.

Appendix B.8.2 Initially Employed Sample

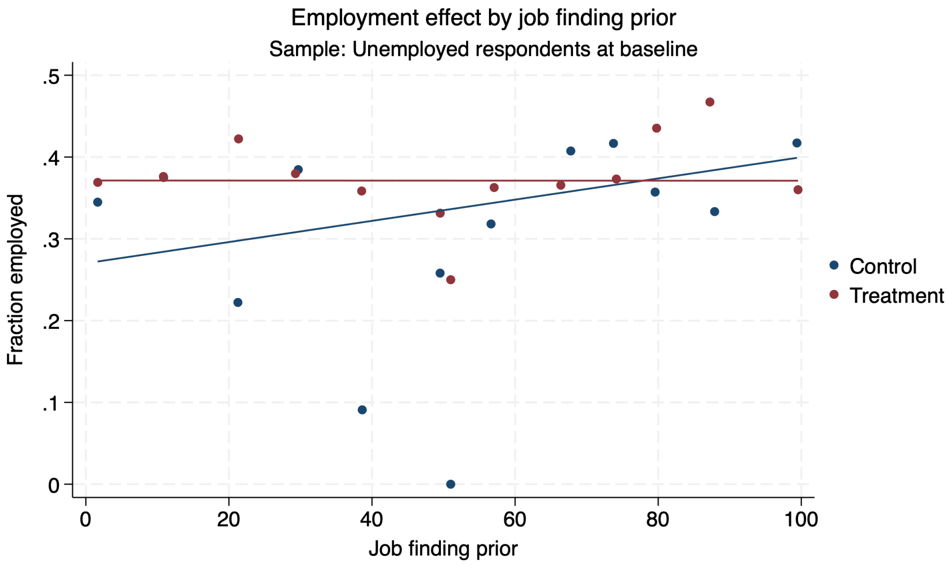


Notes: This figure shows a binned scatterplot of the treatment effect on employment in the follow-up survey by respondents' job-finding prior at baseline. The job finding prior measures the percent chance that the respondent will find and accept a new job using the QuikrJobs portal. Sample is restricted to initially employed respondents who completed the follow-up survey.

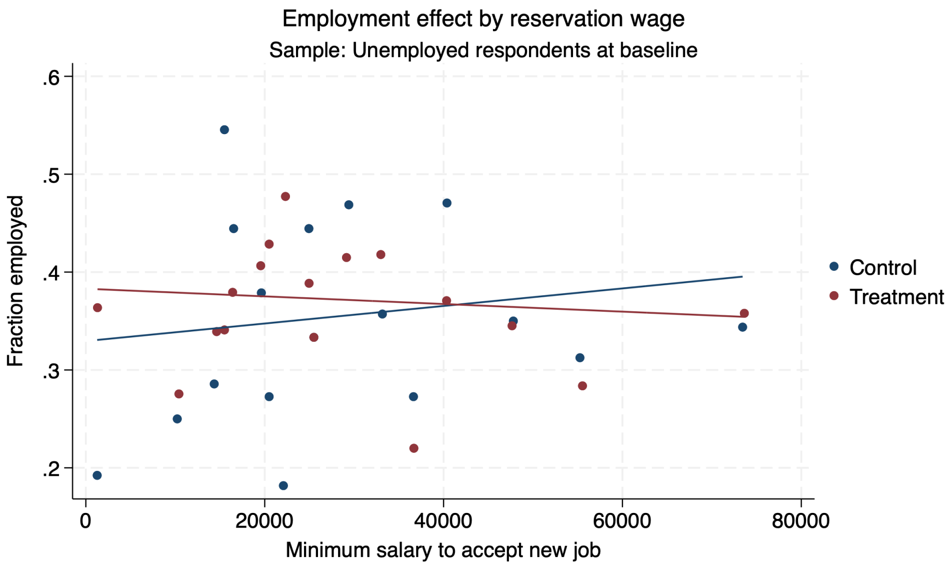


Notes: This figure shows a binned scatterplot of the treatment effect on employment in the follow-up survey by respondents' reservation wage at baseline. The reservation wage measures the minimum monthly salary required to accept a new full-time job. Sample is restricted to initially employed respondents who completed the follow-up survey.

Appendix B.8.3 Initially Unemployed Sample

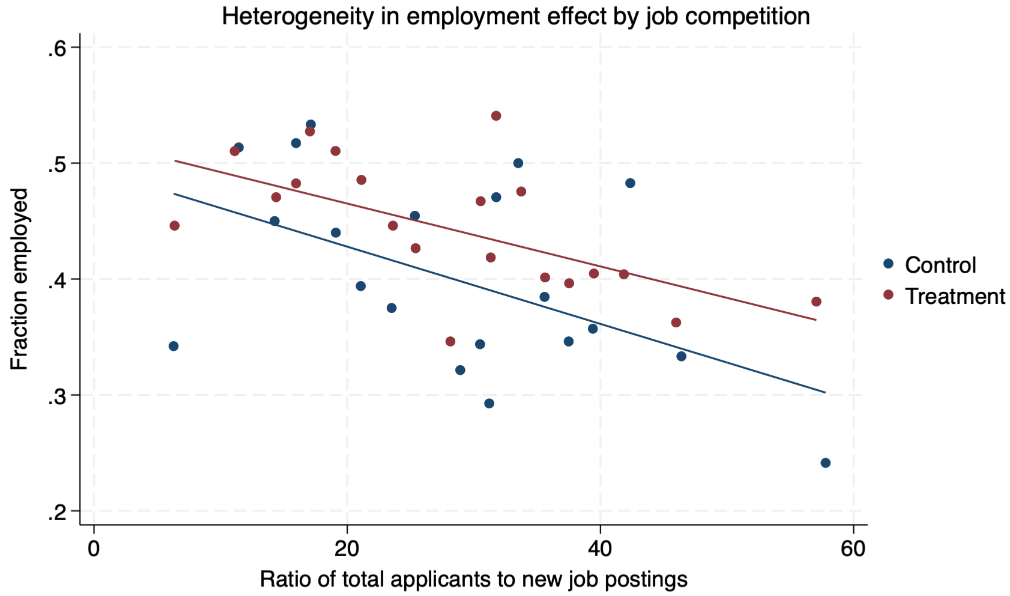


Notes: This figure shows a binned scatterplot of the treatment effect on employment in the follow-up survey by respondents' job-finding prior at baseline. The job finding prior measures the percent chance that the respondent will find and accept a new job using the QuikrJobs portal. Sample is restricted to initially unemployed respondents who completed the follow-up survey.



Notes: This figure shows a binned scatterplot of the treatment effect on employment in the follow-up survey by respondents' reservation wage at baseline. The reservation wage measures the minimum monthly salary required to accept a new full-time job. Sample is restricted to initially unemployed respondents who completed the follow-up survey.

Appendix B.9 Employment effect by actual labor market conditions



Notes: This figure shows a binned scatterplot of the treatment effect on employment in the follow-up survey by job competition in respondents' preferred local labor market. Job competition is measured as the average monthly ratio of total applicants to new job postings. Sample consists of all respondents—initially employed and unemployed—who completed the follow-up survey.

Appendix B.10 Effect heterogeneity by job finding prior and information type

	Full sample	Initially employed only		Initially unemployed only
	(1)	(2)	(3)	(4)
	Employed	Employed	Employed at baseline job	Employed
Demand (D)	0.089**	0.131*	0.117	0.039
	(0.040)	(0.076)	(0.077)	(0.049)
Demand × High Prior	-0.103*	-0.105	-0.118	-0.071
	(0.057)	(0.106)	(0.107)	(0.070)
Supply (S)	0.078**	0.120*	0.101	0.062
	(0.034)	(0.068)	(0.068)	(0.042)
Supply × High Prior	-0.075	-0.108	-0.099	-0.078
	(0.049)	(0.097)	(0.095)	(0.061)
Tightness (T)	0.104***	0.141**	0.095	0.089**
	(0.034)	(0.068)	(0.068)	(0.042)
Tightness × High Prior	-0.083*	-0.070	-0.066	-0.103*
	(0.050)	(0.095)	(0.095)	(0.061)
High Prior	0.083**	0.045	0.061	0.106**
	(0.040)	(0.078)	(0.075)	(0.049)
p-value: D+D×High Prior=0	0.725	0.736	0.993	0.518
p-value: S+S×High Prior=0	0.939	0.855	0.972	0.707
p-value: T+T×High Prior=0	0.560	0.286	0.666	0.751
N	3430	1005	954	2226
Control Mean	0.401	0.529	0.340	0.355

Notes: This table replicates Table 5, Panel A using separate treatment indicators—*Demand*, *Supply* and *Tightness*—instead of the pooled treatment indicator. Treatment indicators are interacted with a binary variable for individuals whose job-finding priors at baseline are above (or equal to) the sample median. This prior measured as the percent chance the respondent will find and accept a job via the QuikrJobs portal in the next 30 days. Column 1 considers the full sample; columns 2–3 restrict to initially employed job seekers; and column 4 restricts to initially unemployed job seekers. Columns 1, 2, and 4 report whether the respondent is employed at the follow-up survey. Column 3 asks whether initially employed respondents are employed at their baseline job. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Appendix B.11 Effect heterogeneity by reservation wage and information type

	Full sample	Initially employed only		Initially unemployed only
	(1)	(2)	(3)	(4)
	Employed	Employed	Employed at baseline job	Employed
Demand (D)	0.074*	0.157*	0.143*	0.032
	(0.041)	(0.081)	(0.079)	(0.050)
Demand × High RW	-0.071	-0.139	-0.154	-0.052
	(0.057)	(0.108)	(0.108)	(0.070)
Supply (S)	0.075**	0.129*	0.095	0.052
	(0.035)	(0.076)	(0.070)	(0.043)
Supply × High RW	-0.068	-0.113	-0.082	-0.054
	(0.049)	(0.099)	(0.095)	(0.061)
Tightness (T)	0.092***	0.197***	0.121*	0.059
	(0.035)	(0.073)	(0.070)	(0.043)
Tightness × High RW	-0.058	-0.162*	-0.108	-0.042
	(0.049)	(0.096)	(0.095)	(0.061)
High RW	0.071*	0.144*	0.109	0.040
	(0.040)	(0.079)	(0.076)	(0.049)
p-value: D+D×High RW=0	0.950	0.802	0.880	0.676
p-value: S+S×High RW=0	0.834	0.808	0.839	0.967
p-value: T+T×High RW=0	0.336	0.580	0.850	0.701
N	3430	1005	954	2226
Control Mean	0.401	0.529	0.340	0.355

Notes: This table replicates Table 5, Panel B using separate treatment indicators—*Demand*, *Supply* and *Tightness*—instead of the pooled treatment indicator. Treatment indicators are interacted with a binary variable for individuals whose baseline reservation wages are above (or equal to) the sample median. Reservation wages are measured as the minimum monthly salary required to accept a new job in the next three months. Column 1 considers the full sample; columns 2–3 restrict to initially employed job seekers; and column 4 restricts to initially unemployed job seekers. Columns 1, 2, and 4 report whether the respondent is employed at the follow-up survey. Column 3 asks whether initially employed respondents are employed at their baseline job. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Appendix B.12 Effect heterogeneity by job competition and information type

	Full sample	Initially employed only		Initially unemployed only
	(1)	(2)	(3)	(4)
	Employed	Employed	Employed at baseline job	Employed
Demand (D)	0.071*	0.122	0.123	0.030
	(0.041)	(0.081)	(0.080)	(0.049)
Demand × LC	-0.068	-0.084	-0.125	-0.052
	(0.057)	(0.107)	(0.108)	(0.070)
Supply (S)	0.044	0.127*	0.091	0.008
	(0.035)	(0.074)	(0.070)	(0.042)
Supply × LC	-0.007	-0.114	-0.080	0.035
	(0.049)	(0.098)	(0.096)	(0.060)
Tightness (T)	0.077**	0.153**	0.121*	0.038
	(0.035)	(0.072)	(0.070)	(0.043)
Tightness × LC	-0.028	-0.090	-0.115	-0.001
	(0.049)	(0.096)	(0.096)	(0.061)
Low Competition (LC)	0.014	0.124	0.008	-0.020
	(0.049)	(0.101)	(0.097)	(0.058)
p-value: D+D×LC=0	0.937	0.588	0.974	0.664
p-value: S+S×LC=0	0.289	0.837	0.863	0.325
p-value: T+T×LC=0	0.161	0.329	0.931	0.387
N	3430	1005	954	2226
Control Mean	0.401	0.529	0.340	0.355

Notes: This table replicates Table 6 using separate treatment indicators—*Demand*, *Supply* and *Tightness*—instead of the pooled treatment indicator. "Low Competition (LC)" is a binary indicator for whether the respondent's preferred local labor market (city×occupation) has a lower than sample median ratio of applicants per new job posting on the portal (median=30). Column 1 considers the full sample; columns 2–3 restrict to initially employed job seekers; and column 4 restricts to initially unemployed job seekers. Columns 1, 2, and 4 report whether the respondent is employed at the follow-up survey. Column 3 asks whether initially employed respondents are employed at their baseline job. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix B.13 Employment effects without controlling for sex of respondent

	Full sample		Initially employed only	Initially unemployed only
	(1) Employed (Table 1, col 1)	(2) Non-portal job (Table 1, col 4)	(3) Employed (Table 3, col 3)	(4) Employed (Table 4, col 3)
	Panel A: Pooled effects			
Treatment	0.040*	0.044**	0.078*	0.019
	(0.022)	(0.022)	(0.042)	(0.027)
	Panel B: Effects by information type			
Demand (D)	0.035	0.039	0.078	0.001
	(0.029)	(0.029)	(0.053)	(0.035)
Supply (S)	0.030	0.028	0.057	0.017
	(0.025)	(0.025)	(0.048)	(0.031)
Tightness (T)	0.054**	0.063**	0.101**	0.031
	(0.025)	(0.025)	(0.048)	(0.031)
p-value: T=D	0.441	0.355	0.610	0.345
p-value: T=S	0.242	0.104	0.268	0.598
p-value: D=S	0.848	0.676	0.657	0.608
N	3430	3374	1005	2226
Control Mean	0.401	0.380	0.529	0.355

Notes: This table replicates our key employment results without controlling for sex of the respondent. The column headers show the table and column numbers of the original results. Panel A shows impacts pooling across treatments, whereas Panel B reports impacts separately by information type and p-values from tests of equality of coefficients between treatment types. Outcomes are measured during the follow-up surveys. Columns 1–2 reports effect on the full sample, whereas columns 3 and 4 report effects separating initially employed and unemployed respondents, respectively. Columns 1, 3 and 4 measured whether the respondent is employed at the time of the follow-up survey. Column 2 measures whether the source of current employment is a non-portal method and is coded as zero for non-employed individuals. The number of observations is lower in column 2 because the variable was only collected in the long version of the follow-up survey. The control mean is the mean of the outcome variable for the control group. Regressions include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

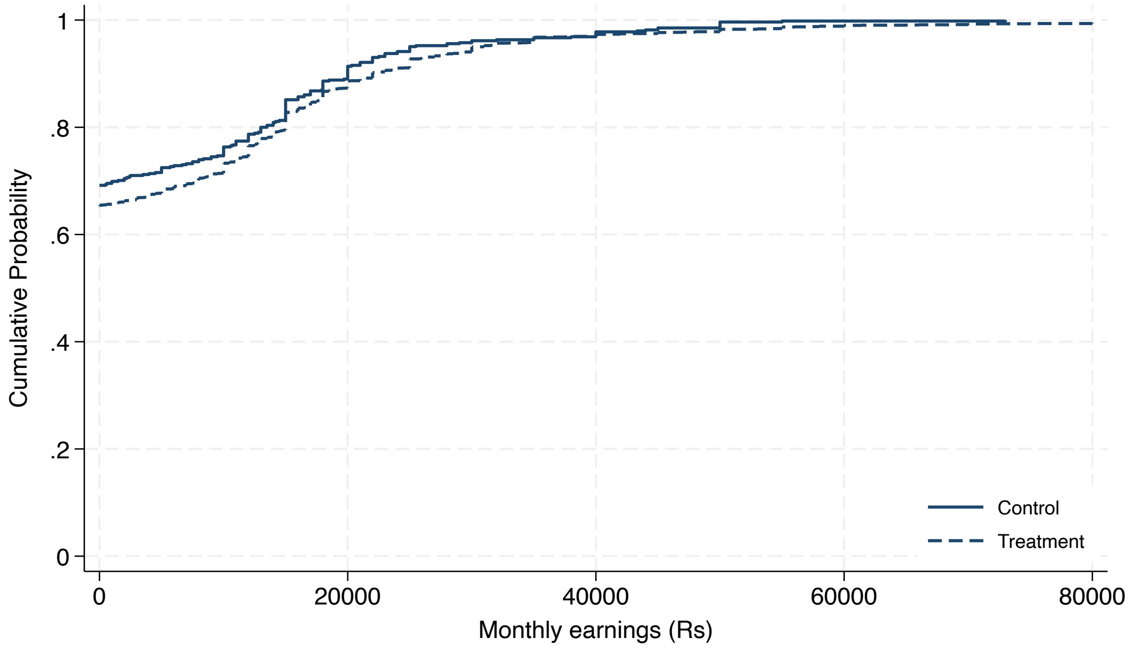
Appendix B.14 Heterogeneous effects by labor market beliefs, with additional individual controls

	Full sample	Initially employed only		Initially unemployed only
	(1)	(2)	(3)	(4)
	Employed	Employed	Employed at baseline job	Employed
Panel A: Job finding priors				
Treatment	0.085***	0.124**	0.103*	0.065*
	(0.030)	(0.058)	(0.056)	(0.036)
Treatment × High Prior	-0.090**	-0.083	-0.088	-0.098*
	(0.043)	(0.083)	(0.080)	(0.053)
High Prior	0.079**	0.038	0.061	0.109**
	(0.039)	(0.076)	(0.072)	(0.049)
Estimate: Treat+Treat×High Prior	-0.005	0.041	0.015	-0.033
p-value: Treat+Treat×High Prior=0	0.872	0.488	0.801	0.388
Panel B: Reservation wage (RW)				
Treatment	0.078**	0.159**	0.118**	0.048
	(0.030)	(0.064)	(0.058)	(0.037)
Treatment × High RW	-0.073*	-0.137	-0.107	-0.062
	(0.043)	(0.085)	(0.081)	(0.054)
High RW	0.064*	0.118	0.081	0.043
	(0.039)	(0.077)	(0.073)	(0.049)
Estimate: Treat+Treat×High RW	0.005	0.022	0.010	-0.014
p-value: Treat+Treat×High RW=0	0.871	0.691	0.854	0.723
N	3430	1005	954	2226
Control Mean	0.401	0.529	0.340	0.355

Notes: This table replicates Table 5, and adds baseline controls for age, having a college degree, any paid labor market experience, and search intensity (searching for at least 1 hour in the last week). Missing values for control variables are replaced with a placeholder value of -99 and missingness indicators are included. Panel A focuses on heterogeneity by respondents' job finding prior at baseline, measured as the percent chance the respondent will find and accept a job via the QuikrJobs portal in the next 30 days. Panel B focuses on heterogeneity by baseline reservation wage, which is measured by asking respondents the minimum monthly salary they would require to accept a new full-time job in the next 3 months. "Treatment" is a pooled indicator for any of the different information treatments. "High Prior" and "High RW" and binary indicators for individuals whose labor market belief is above (or equal to) the sample median. "Treatment×High RW/Prior" is the interaction term. Column 1 considers the full sample; columns 2–3 restrict to initially employed job seekers; and column 4 restricts to initially unemployed job seekers. Columns 1, 2, and 4 report whether the respondent is employed at the follow-up survey. Column 3 asks whether initially employed respondents are employed at their baseline job. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Appendix C Additional results

Appendix C.1 Earnings distribution by treatment status



Notes: This graph shows the cumulative distribution of reported earnings in the previous month by treatment status, as recorded in the follow-up surveys. Those who are not employed at the follow-up survey are coded to have zero earnings. The solid line is the distribution for the control group, while the dashed line is the distribution of the treatment group. Earnings are measured in Indian Rupees and the top 0.5% of values are winsorized.

Appendix C.2 Overall effects on portal applications

	# applications		# distinct markets for applications		# applications in non-preferred markets	
	(1) 30 days	(2) 180 days	(3) 30 days	(4) 180 days	(5) 30 days	(6) 180 days
	Panel A: Pooled effects					
Treatment	0.428	0.371	0.024	0.011	0.347	0.210
	(0.359)	(0.619)	(0.103)	(0.132)	(0.233)	(0.420)
	Panel B: Effects by information type					
Demand (D)	0.352	0.364	0.086	0.103	0.348	0.259
	(0.458)	(0.799)	(0.133)	(0.185)	(0.302)	(0.544)
Supply (S)	0.563	0.610	0.039	0.022	0.451*	0.315
	(0.413)	(0.707)	(0.122)	(0.152)	(0.270)	(0.474)
Tightness (T)	0.332	0.137	-0.022	-0.046	0.243	0.083
	(0.405)	(0.692)	(0.113)	(0.146)	(0.264)	(0.471)
p-value: T=D	0.962	0.745	0.352	0.371	0.699	0.711
p-value: T=S	0.517	0.422	0.543	0.593	0.372	0.557
p-value: D=S	0.611	0.731	0.700	0.633	0.706	0.907
N	6130	6130	6130	6130	6130	6130
Control Mean	5.465	9.550	1.483	2.200	2.794	5.548

Notes: This table considers impacts on the the number and scope of applications on the QuikrJobs portal. The dependent variables are as follows: the number of applications submitted on the portal within 30 and 180 days of the baseline (columns 1–2); the count of unique city × occupation cells, i.e. local markets, where job seekers submitted job applications within 30 and 180 days of the baseline (columns 3–4); and the number of applications submitted outside of the respondent's preferred labor market within 30 and 180 days of the baseline (columns 5–6). The top 1% of values are winsorized across all columns. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation cells and sampling batch. Robust standard errors are in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Appendix C.3 Effects on usage of non-portal search methods

	Used method since baseline		
	(1) Other online methods	(2) Networks	(3) Other offline methods
	Panel A: Initially employed job seekers		
Treatment	-0.076*	-0.060	-0.029
	(0.039)	(0.040)	(0.039)
N	1005	1005	1005
Control Mean	0.706	0.329	0.288
	Panel B: Initially unemployed job seekers		
Treatment	0.057**	-0.009	-0.006
	(0.028)	(0.025)	(0.024)
N	2226	2226	2226
Control Mean	0.572	0.279	0.245

Notes: This table shows impacts on the usage of different non-portal search methods for initially employed (Panel A) and initially unemployed job seekers (Panel B). Outcomes come from follow-up surveys. The dependent variables report whether the respondent used: other online methods excluding the QuikrJobs portal, such as other job portals, social media, or company websites (column 1); social networks, which include referrals from family, friends, or co-workers (column 2); and other offline methods, such as newspapers, employment agencies, direct walk-ins, etc. The control mean is the mean of the dependent variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city$\times$occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix C.4 Likelihood of offers by baseline employment status

	All methods			Search method: Portal			Search method: Non-portal		
	(1) Any offer	(2) N offers	(3) Accepted offer	(4) Any offer	(5) N offers	(6) Accepted offer	(7) Any offer	(8) N offers	(9) Accepted offer
Panel A: Initially employed job seekers									
Treatment	-0.060	-0.121	-0.002	0.008	-0.128	0.007	-0.071*	0.066	-0.015
	(0.042)	(0.603)	(0.041)	(0.035)	(0.299)	(0.008)	(0.042)	(0.379)	(0.040)
N	1005	966	966	973	923	963	1005	965	966
Control Mean	0.624	2.969	0.302	0.192	0.891	0.012	0.553	1.951	0.302
Panel B: Initially unemployed job seekers									
Treatment	0.020	0.215	0.015	-0.020	-0.009	-0.015	0.033	0.222	0.030
	(0.028)	(0.348)	(0.024)	(0.023)	(0.195)	(0.010)	(0.028)	(0.216)	(0.023)
N	2226	2137	2137	2158	2041	2131	2225	2135	2136
Control Mean	0.507	2.442	0.231	0.216	0.966	0.033	0.403	1.442	0.204

Notes: This table shows impacts on search efficacy via different search methods for initially employed job seekers (Panel A) and initially unemployed job seekers (Panel B). Outcomes are measured during the follow-up surveys. Columns 1–3 consider efficacy across all methods, whereas columns 4–6 consider search via the QuikrJobs portal and columns 7–9 consider other non-portal search methods. The dependent variables are: whether the respondent received any offer (columns 1, 4, and 7); the number of offers received (columns 2, 5, and 8); and whether the respondent accepted any offer (columns 3, 6, and 9). The top 0.5% of values are winsorized for the number of offers. The control mean is the mean of the outcome variable for the control group. There are fewer observations for the number of job offers and offer acceptances as these variables were only collected in the long version of the follow-up survey. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix C.5 Treatment effects on job finding beliefs

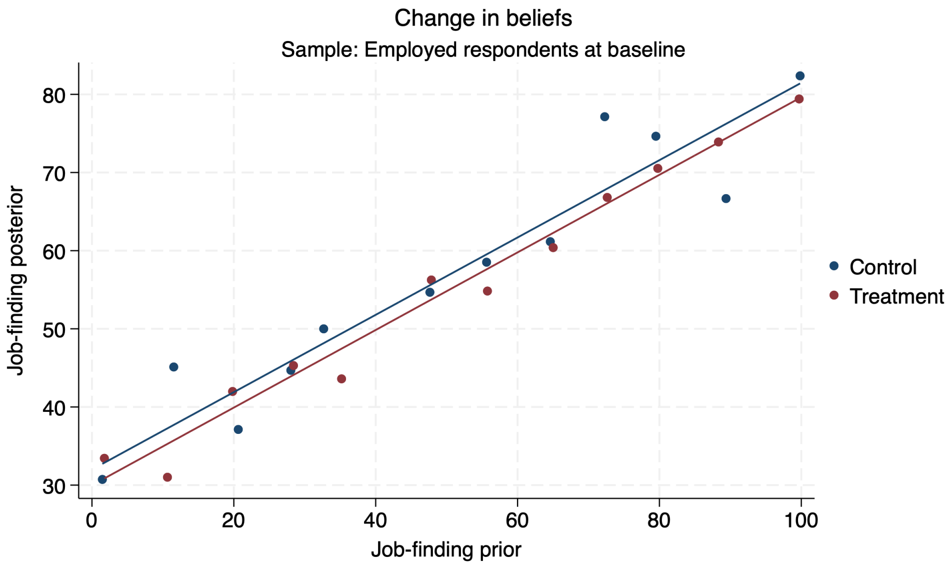
Appendix C.5.1 Average effects

	Change in job finding beliefs (posterior-prior)		
	(1)	(2)	(3)
	Full sample	Initially employed	Initially unemployed
Panel A: Pooled effects			
Treatment	-1.489	-2.571	-0.792
	(1.066)	(2.006)	(1.317)
Panel B: Effects by information type			
Demand (D)	-1.999	-4.544*	-0.331
	(1.402)	(2.600)	(1.742)
Supply (S)	-0.964	-2.556	0.039
	(1.198)	(2.292)	(1.478)
Tightness (T)	-1.760	-1.432	-1.866
	(1.191)	(2.222)	(1.479)
p-value: T=D	0.846	0.166	0.319
p-value: T=S	0.420	0.546	0.122
p-value: D=S	0.402	0.397	0.809
N	5738	1662	3760
Control Mean	0.933	0.917	0.944

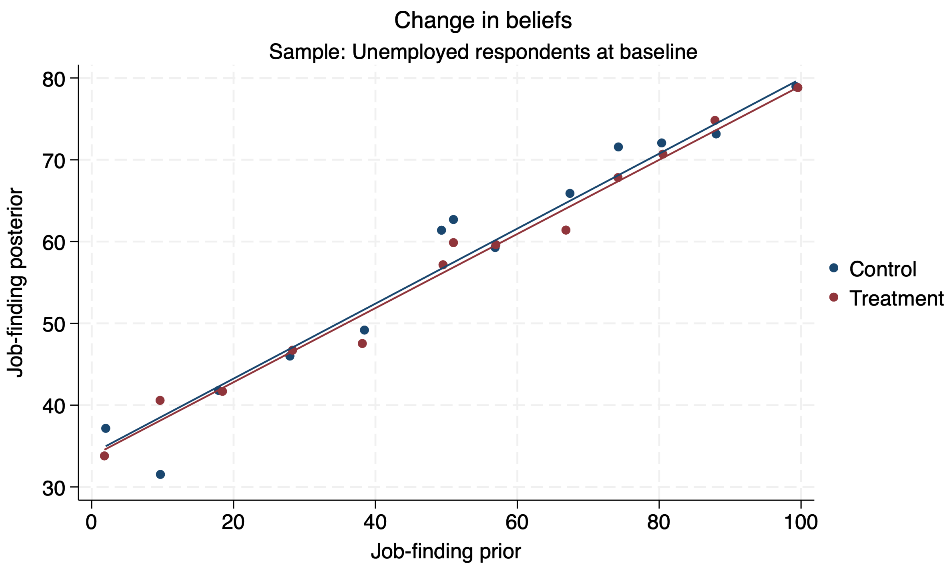
Notes: This table considers treatment effects on job finding beliefs. Panel A shows impacts pooling across treatments, whereas Panel B reports impacts separately by information type and p-values from tests of equality of coefficients between treatment types. The dependent variable is the change in job finding beliefs via the portal in the next 30 days, i.e. posterior-prior. We asked respondents for the percent chance that they will find and accept a new job using the QuikrJobs portal in the next 30 days twice: once as part of the baseline modules *before* treatment and again immediately *after* the information treatments were shown as part of a post-treatment survey module. Column 1 show results for the full sample, whereas columns 2 and 3 show results for subsamples of initially employed and unemployed job seekers, respectively. The number of observations in less than 6,155 in column 1 due to missing item response. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city$\times$occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix C.5.2 Belief-updating across prior belief distribution

Panel (a): Employed respondents



Panel (b): Unemployed respondents



Note: This figure shows a binned scatterplot of the treatment effect on job-finding beliefs. We measured beliefs about job-finding on the QuikrJobs portal before and immediately after information delivery. Panel (a) is restricted to all initially employed respondents, whereas panel (b) is restricted to all initially unemployed respondents.

Appendix C.6 Effects on earnings by baseline employment status

	Initially employed only		Initially unemployed only	
	(1)	(2)	(3)	(4)
	Earnings	Earnings | employed	Earnings	Earnings | employed
		Panel A: Pooled effects		
Treatment	1,754.144	-406.120	1,068.999*	1,832.462
	(1,336.754)	(2,043.360)	(548.511)	(1,282.643)
		Panel B: Effects by information type		
Demand (D)	1,522.314	-1,029.291	420.238	992.076
	(1,719.672)	(2,642.354)	(680.709)	(1,611.027)
Supply (S)	1,462.900	-331.820	981.860	1,714.323
	(1,528.156)	(2,277.158)	(642.942)	(1,541.798)
Tightness (T)	2,195.246	-140.614	1,474.349**	2,304.768
	(1,559.778)	(2,367.243)	(663.067)	(1,498.364)
p-value: T=D	0.676	0.705	0.117	0.393
p-value: T=S	0.588	0.924	0.432	0.689
p-value: D=S	0.970	0.768	0.389	0.639
N	908	506	2078	672
Control Mean	9107.247	19515.529	4370.854	14275.450

Notes: This table considers treatment effects on earnings by baseline employment status. Panel A shows impacts pooling across treatments, whereas Panel B reports impacts separately by information type and p-values from tests of equality of coefficients between treatment types. Columns 1–2 restrict the sample to initially employed respondents whereas Columns 3–4 to initially unemployed respondents. Columns 1 and 3 show unconditional earnings, coding those who are non-employed at the follow-up survey to have zero earnings. Columns 2 and 4 show earnings, conditional on being employed in the follow-up survey. Earnings are measured in Indian rupees and top 0.5% of values are winsorized. The number of observations is lower than the subsamples of initially employed and unemployed respondents due to item non-response for earnings. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix C.7 Effects on usefulness of information

	Information was:		
	(1)	(2)	(3)
	Not useful	Somewhat useful	Very useful
Treatment	0.007	-0.043**	0.036**
	(0.008)	(0.017)	(0.017)
N	5738	5738	5738
Control Mean	0.061	0.479	0.460

Notes: This table considers effects on self-reported usefulness of information. This outcome is collected as part of the post-treatment survey module immediately after treatment delivery. Respondents were asked: "Did you find the information you just saw useful?" and the dependent variables are indicator variables for responses to this question: information was not useful (column 1), somewhat useful (column 2), or very useful (column 3). The number of observations is less than 6,155 because of item non-response. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Appendix C.8 Willingness to pay exercises

Hypothetical WTP

Survey enumerators asked 787 respondents the following question during the follow-up survey phone calls:

> "Imagine you could go to a website that give you information about jobs in your city or nearby cities on QuikrJobs. It would show how many new jobs are posted monthly. It can also tell you how many other people are looking for these jobs and what their profiles are like. This may help you understand competition for jobs. We are thinking of putting such information out using the QuikrJobs data. Would you be willing to pay Rs.$\{20, 40, 60, 80, 100\}$ to access this service for 3 months? Please answer truthfully as you may have a chance to access this service in the future at this price."

In our discussions with the partner, a dedicated webpage was the format under consideration for such information provision. We wanted to mimic what would have been feasible for the partner in asking this question. 50% of respondents were willing to pay to access the website. The share by randomized price is given below.

Amount (Rs.)	Take-up Percent (%)
20	57
40	55
60	50
80	39
100	48

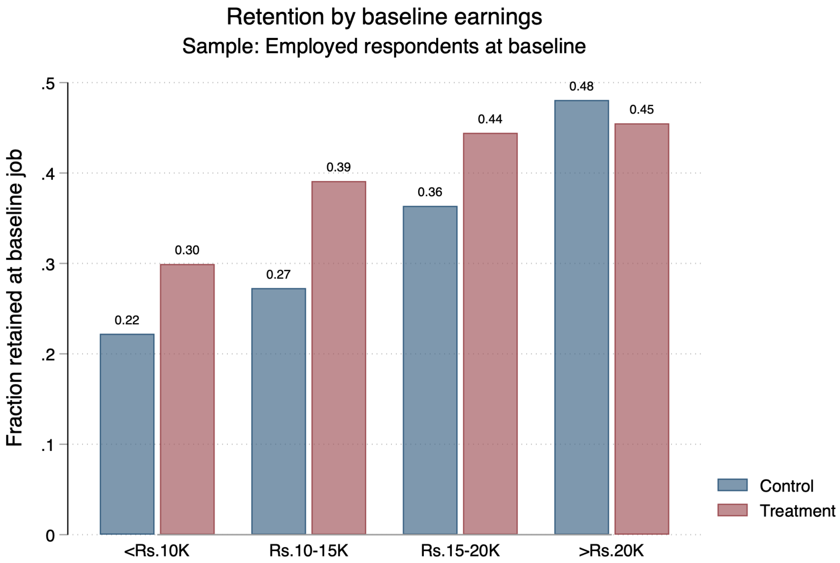
Incentivized WTP

Since the website in the hypothetical WTP exercise did not actually exist, we also implemented an incentivized WTP exercise during the follow-up phone survey with a different subset of respondents (N=1,174). Among this set, 67% were willing to pay to access information. The script followed by enumerators was as follows:

> "As I mentioned in the beginning, you will receive Rs.[AMOUNT] amount if you complete the survey. We have a new offer for you! Would you be willing to spend that amount – that is, Rs.[AMOUNT] to continue receiving the same information that you received earlier for the following 3 months? That is, instead of receiving the cash, you can spend it on the information. Please answer truthfully as we will begin sending you updated information next month onwards if you are interested."

Respondents received at least Rs.30 for completing the follow-up survey, but overall compensation could vary because of differences in incentivized earnings from the baseline survey. We surprised respondents at the end of the survey by providing the information for free and giving them their full compensation.

Appendix C.9 Descriptive evidence on quality of retained jobs



Notes: This figure shows the fraction of initially employed respondents that remained at their baseline job by baseline earnings bin, separately for treatment and control groups. Sample is restricted to initially employed respondents with follow-up survey data. Baseline earnings were collected in range bins. Bins are defined as: less than Rs.10,000 (20% of sample), Rs.10,000–15,000 (28%), Rs.15,000–20,000 (23%), and above Rs.20,000 (29%). The top bin combines several bins above Rs.20,000 due to small sizes.

Comparison of job characteristics between treatment and control stayers

	Treatment		Control		
	Mean	N	Mean	N	Difference
Monthly earnings (Rs.)	20008.30	289	20496.13	47	-487.83
Number of job benefits (out of 4)	1.72	309	2.00	55	-0.28
Written contract	0.17	304	0.15	54	0.02
Respected at work (1-4 scale)	3.35	300	3.42	53	-0.06
Good friends at work (1-4 scale)	3.39	299	3.21	53	0.19
Learning opportunities at work (1-4 scale)	2.83	300	2.81	53	0.02
Job satisfaction (1-5 scale)	3.21	298	3.08	52	0.13

Notes: This table compares job characteristics at follow-up between treatment and control group stayers—initially employed respondents who remained at their baseline job at the time of the follow-up survey. Monthly earnings are winsorized at the top 0.5%. Number of job benefits counts the following: paid leave, pension, provident fund, and health insurance. Non-monetary job quality measures are reported on a 1–4 scale, except job satisfaction which is reported on a 1–5 scale. * p<0.10, ** p<0.05, *** p<0.01

Appendix C.10 Search by labor market beliefs

	Initially Employed Sample				Initially Unemployed Sample			
	(1) Portal	(2) Portal	(3) Non-portal	(4) Non-portal	(5) Portal	(6) Portal	(7) Non-portal	(8) Non-portal
Treatment	0.089	0.065	0.021	-0.065	0.051	0.010	0.042	0.057*
	(0.061)	(0.065)	(0.051)	(0.050)	(0.039)	(0.039)	(0.035)	(0.034)
Treatment × High Prior	-0.092		-0.196***		-0.011		-0.032	
	(0.084)		(0.063)		(0.055)		(0.049)	
Treatment × High RW		-0.040		-0.029		0.069		-0.054
		(0.087)		(0.067)		(0.055)		(0.049)
High Prior	0.090		0.148***		0.030		0.053	
	(0.077)		(0.056)		(0.050)		(0.045)	
High RW		0.047		0.002		-0.039		-0.004
		(0.079)		(0.059)		(0.050)		(0.045)
Estimate: Treat+Treat×High Belief	-0.003	0.025	-0.175	-0.094	0.040	0.080	0.010	0.003
p-value: Treat+Treat×High Belief=0	0.964	0.662	0.000	0.035	0.303	0.040	0.772	0.923
N	1005	1005	1005	1005	2226	2226	2226	2226
Control Mean	0.571	0.571	0.841	0.841	0.559	0.559	0.731	0.731

Notes: This table reports treatment effects on whether respondents engaged in any portal and non-portal search since baseline. Columns 1–4 focus on initially employed respondents, whereas columns 5–8 on initially unemployed respondents. Odd-numbered columns interact treatment with an indicator for above-median job-finding priors, while even-numbered columns interact treatment with an indicator for above-median reservation wages. 'High Prior' and 'High RW' are binary indicators for individuals whose labor market belief is above or equal to the sample median. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Appendix C.11 Non-portal offers among initially unemployed respondents

	Any non-portal offers since baseline		Accepted non-portal offer since baseline	
	(1)	(2)	(3)	(4)
Treatment	0.041	0.050	0.067**	0.071**
	(0.039)	(0.040)	(0.031)	(0.033)
Treatment × High Prior	-0.019		-0.077*	
	(0.056)		(0.047)	
Treatment × High RW		-0.031		-0.076
		(0.056)		(0.047)
High Prior	0.057		0.099**	
	(0.051)		(0.042)	
High RW		-0.000		0.020
		(0.051)		(0.043)
Estimate: Treat+Treat×High Belief	0.022	0.019	-0.010	-0.005
p-values: Treat+Treat×High Belief=0	0.578	0.633	0.776	0.883
N	2225	2225	2136	2136
Control Mean	0.403	0.403	0.204	0.204

Notes: This table reports treatment effects on offer receipt and acceptance from non-portal sources among initially unemployed respondents. Columns 1 and 3 interact treatment with an indicator for above-median job-finding priors. Columns 2 and 4 interact treatment with an indicator for above-median reservation wages. 'High Prior' and 'High RW' are binary indicators for individuals whose labor market belief is above or equal to the sample median. The control mean is the mean of the outcome variable for the control group. Regressions control for whether the respondent is female and include fixed effects for city×occupation and sampling batch. Robust standard errors are in parentheses. * p<0.10, ** p<0.05, *** p<0.01