How do Digital Platforms affect Employment and Job Search? Evidence from India*

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August 16, 2023

Abstract

We use a randomized control trial to evaluate whether digital platforms improve employment outcomes among vocational training graduates in India. We uploaded a random subset of graduates to a digital platform, and assigned some to receive many text messages about job opportunities. We find evidence of voluntary unemployment: graduates respond to platform access by increasing their reservation wages, and by working significantly *less*. As good job offers fail to materialize on the platform, some graduates adjust their expectations downwards and resume working. These findings suggest that youth's beliefs about the effectiveness of matching interventions may reduce their potential impacts on employment.

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1 Introduction

Youth unemployment is a policy priority throughout the developing world. In India, the importance of solving the youth unemployment problem was buoyed by 2017-18 job numbers which revealed that youth joblessness in both urban and rural areas had spiked to approximately 18% (Slater, 2019). In the last decade, there has been a big push to identify solutions to this problem. On the one hand, governments have responded by investing in large scale labor market policies such as wage subsidy and skills training programs (McKenzie, 2017). On the other hand, researchers have explored whether job search assistance programs can improve match rates between job seekers and prospective employers (McKenzie, 2017). The impact of these interventions have been modest, suggesting that some youth may be voluntary unemployed (Groh et al., 2015; Banerjee and Chiplunkar, 2018). In particular, young job seekers may have unrealistic expectations about their job market prospects, turning down the jobs they have access to through these interventions to hold out for better opportunities that fail to materialize(Abebe et al., 2018). This points to the need for longer, more sustainable interventions, that can provide new employment opportunities to young job seekers *while* setting their expectations and improving their understanding of the labor market.

Our paper proposes to investigate the benefits of digital platforms – a technology that continuously advertises new job opportunities, and has the potential to provide job seekers with a better understanding of the labor market and the jobs they can feasibly get. We partner with JobShikari.com, a digital platform that sends SMS information on low-skilled jobs to candidates registered on their platform.[1] We enroll a randomly selected subset of vocational training graduates on the Job Shikari platform, and send them a brief text message indicating they will be registered with the platform. This is our first treatment group. For a second randomly-selected subset of new graduates, we provide access to the platform *and* grant them a priority ranking within Job Shikari's algorithm. We refer to this second sample as the priority treatment group. Job Shikari ultimately sent 1.6 SMS messages about job opportunities per person to the treatment group, and an additional 17.4 messages to the priority treatment group for a truly intensive information intervention. We can compare these two groups to control respondents who are not registered on Job Shikari in order to estimate 1) the causal impact of platform access on employment outcomes, and 2) how these employment responses change as respondents receive more information about job opportunities from the platform.

We find a strong, but unexpected response to being enrolled on the platform: new graduates are 9 percentage points less likely to be working 12 months after being notified they

[1] Job Shikari is no longer active. It was purchased by another digital platform after study completion.

would have access to Job Shikari. We also show that a steady stream of information for the priority treatment group results in these graduates “catching up” to the control group. Priority treatment graduates are only 4 percentage points less likely to be employed than control. This reversal in employment rates relative to the treatment group is statistically significant. These effects appear to be driven, at least in part, by changes in individuals' beliefs about what the platform can do for them: we see that reservation wages increase for the treatment group, and fall for priority treatment.

We also find that some youth “catch up” more quickly than others. We look at differences across four geographic zones in India (North, Delhi, South West, and East). We find that the reversal in employment rates is strongest among priority treatment graduates located in the South West and East. This sample was older, from lower castes and more likely to be married relative to our sample in the North or Delhi – features that may have increased the opportunity cost of holding out for a job on Job Shikari. They were also spatially mismatched relative to jobs advertised by the platform, which were largely in Delhi. While these positive priority treatment effects we observe for youth in the South West could be generated by new matches, several patterns in the data suggest that this group learned the platform was delivering jobs they were unwilling to migrate for, which prompted them to accept outside offers more quickly rather than hold out for better opportunities on Job Shikari.

These results provide clear evidence of voluntary unemployment among the young adults in our sample, as predicted by seminal models of job search (McCall, 1970; Jovanovic, 1979): perceptions of access to new sources of job opportunities should boost reservation wages, and reduce employment in the short run. These effects may be larger and more persistent if youth have inaccurate expectations about the effectiveness of the platform, and if job opportunities fail to materialize. Nevertheless, as these recent graduates receive additional information about job opportunities they should update their perceptions of the new arrival rate of jobs and adjust their beliefs about their employment prospects. These predictions are borne out in our data: youth are overly optimistic about their wage prospects at baseline, and when they are notified that they will be registered with a digital platform they increase their reservation wages, and reduce employment for at least 1 year. However, when we increase the amount of information that job seekers in our sample receive about jobs, it appears that a subset may have been able to overcome their biased beliefs. This is consistent with work by Bandiera et al. (2021) who show that job seekers revise their beliefs downwards in response to negative signals about their labor market prospects (when call back rates from employers are lower than they anticipated).

These results also suggest that the impact of digital platforms depends crucially on individuals' expectations of what these platforms can deliver. If platforms generate high expectations but the job offers are weak, we can expect to see some amount of voluntary unemployment as individuals hold out for better jobs. The magnitude, and stickiness, of this effect will depend on the extent to which individuals update their beliefs about the likelihood of finding a job. For several reasons, the digital platform we study may have been particularly well-suited to identifying this effect: job offers on the platform were relatively undesirable, and our sample of young, recent graduates may have been particularly susceptible to changing beliefs (and may have entered the study holding beliefs that were already biased by promotional statements from the training institutes they graduated from (Alfonsi, Namubiru, and Spaziani, 2023; Banerjee and Chiplunkar, 2018)). At the same time, we anticipate that this set of conditions may not be unusual. Youth are frequently the target of job matching interventions, particularly those that take place online. Moreover, private sector digital platforms may frequently have access to relatively undesirable opportunities: platforms commonly charge firms to make connections to job seekers, which is most valuable when matches are hard to find. For unskilled jobs, matches will be scarce when wages are low or working conditions are unappealing. If young graduates do not learn that platform jobs are negatively selected, they may stay unemployed for longer durations, as is the case in our study. This highlights the importance of educating youth about what they can expect from these new labor market interventions well ahead of time.

These findings speak to several literatures. First, there is a large literature evaluating the impacts of active labor market policies designed to reduce unemployment rates. Most closely connected to our own work are a series papers that aim to reduce search frictions through interventions that facilitate contact between job seekers and prospective employers (Abebe et al., 2018; Beam, 2016; Bassi and Nansamba, 2022; Groh et al., 2015); subsidize search costs (Abebe et al., 2021; Aeberhardt et al., 2019; Banerjee and Sequeira, 2020); and provide better information about applicants (Abebe et al., 2021; Abel, Burger, and Piraino, 2020; Banerjee and Chiplunkar, 2018). These researcher-led innovations often generate changes in the types of jobs acquired by at least some workers, but in general have had more muted impacts on employment rates (McKenzie, 2017).

Second, we contribute to a growing body of work on the role of digital platforms. Evidence from the US on the role of online job search has been somewhat mixed. In the early years of internet job search (1998-2000), Kuhn and Skuterud (2004) find that search durations were if anything longer for internet users once observable characteristics were controlled for. By contrast, Kuhn and Mansour (2014) find that by 2005-2008 online job search was associated with large reductions in unemployment durations. Wheeler et al. (2022) provide training to job seekers in South Africa on how to open Linkedin accounts and apply for jobs, which leads to a 7 percentage point increase in the probability of employment. Our work complements these two strands of research. Using a randomized control trial, we identify that platforms can have a negative impact on job acceptance off-the-platform. In our platform's case, this happened with youth who were relatively inexperienced with digital platforms and a platform with relatively thin job opportunities: this likely more closely mimics the early years of internet job search in the US and could reconcile the findings of Kuhn and Skuterud (2004) and Kuhn and Mansour (2014). Our study differs from Wheeler et al. (2022) by studying how vocational training graduates respond to these platforms when they are introduced to them organically (without training). While we come to the opposite conclusion from Wheeler et al. (2022), our findings may be reconciled if LinkedIn had access to better job opportunities, or if their program's training component was crucial for setting expectations about the platform's impact. Work by Belot, Kircher, and Muller (2019) also highlights the benefits of receiving additional assistance on these online platforms. They show that job seekers with longer unemployment spells who search more narrowly obtain more interviews when they are nudged to consider a larger breadth of jobs.

Finally, our results add to the literature documenting job seekers unrealistic beliefs about their labor market prospects. In this paper, access to the digital platform changed young graduates' beliefs about their outside options, which induced a change in reservation wages, and employment. The lack of positive matches created by the platform suggests this change in beliefs may not be rationalizable. These results link closely to work by (Jäger et al., 2022; Caldwell and Harmon, 2019) both of whom demonstrate that misperceptions about outside options influence worker bargaining, job transitions and mobility. Similarly, many of the papers studying labor market policies find that job seekers expectations about their job prospects are too high (Abebe et al., 2018; Banerjee and Chiplunkar, 2018; Banerjee and Sequeira, 2020; Bandiera et al., 2021). In Mozambique, Jones and Santos (2022) find that youth expectations over future wages respond to information about the labor market, though this effect only partially bridges the gap to actual likely wages. Our work expands on Jones and Santos (2022) by experimentally adding a new source of information on real job opportunities and tracking impacts through beliefs to employment outcomes.

The rest of the paper is organized as follows: Section 2 discusses the context; Section 3 presents a model of job search with digital platforms; Section 4 details the field experiment; Section 5 discusses our results; Section 6 concludes.

2 Context

2.1 Digital Platforms in India

The proliferation of the internet has made it an increasingly popular tool for millions of job seekers searching for work. At the forefront of this surge are digital platforms connecting prospective employees with potential employers. In India, there are over 10 digital platforms operating nationwide. Many of these platforms build online interfaces or mobile applications so that job seekers can browse job opportunities. Nevertheless, these same digital platforms actively promote free job alert services because they recognize that most job seekers are passive users of the platform and rely on notifications about jobs that match their specific skills/requirements. In low income countries SMS-based platforms are also common – built on the principle that SMS are much easier for low income job seekers than computers or smart phones.

In this paper we partner with Job Shikari, which at the time of study, was one of the only digital platforms advertising employment opportunities for job seekers without university degrees. Job Shikari catered to employers hiring for common service sector jobs including data entry operators, telecallers, and field executives – who perform a variety of administrative roles related to sales (Figure A1). Most jobs Job Shikari advertised required a high school education, and payed 10,000 rupees per month on average (141 USD) (Table A.1 Panel B). While Job Shikari had an online interface, it is not clear that job seekers would have found it. Rather, they relied on the text messages that Job Shikari sent about new job opportunities.[2]

We compare the jobs advertised by Job Shikari to the jobs held by a nationally representative set of respondents of the 2016 Periodic Labor Force Survey (PLFS). To ensure comparability with the eligibility criteria of the Job Shikari sample, we limit the PLFS data to workers aged 18-35 in urban locations. The jobs on Job Shikari offer wages that are very similar to those received by workers in the PLFS, but they are more likely to require passing grade 12 exams instead of completing grade 10 exams (Table A.2). Job Shikari consists exclusively of service sector jobs, while the PLFS includes a broader range of occupations. Yet, when we focus on job categories from the PFLS such as "Clerks, Service Workers/Shops and Market Sales Workers, and Elementary Occupations," we find that the job titles resemble the types of jobs we observe on Job Shikari (Figure A2).

[2]It is unclear how the results from our study will translate to job platforms that are dominated by active search using online interfaces because we cannot anticipate the extent to which job seekers digest the information they see online. Work by Kuhn and Mansour (2014) Kuhn and Skuterud (2004) suggests that job seekers can be relatively passive users of online platforms when they first start searching.

2.2 Vocational Training in India

Governments and private organizations have launched a variety of schemes to enhance skill development in informal settings, mainly in the form of vocational training programs. In India, the Ministry of Skill Development and Entrepreneurship (MSDE) launched The Skill India Campaign in 2015 to dramatically improve access to high quality skills training as quickly and efficiently as possible. As part of these efforts, they created the National Skill Development Corporation - a public-private partnership that supports various institutions in their efforts to bolster vocational training initiatives (National Skill Development Corporation, 2019). One of the NSDC's largest programs is the Pradhan Mantri Kaushal Vikas Yojana (PMKVY) scheme, which encourages youth to sign up for training programs by offering them monetary rewards upon successful completion of the program. According to the MSDE, 22.4 million youth had been enrolled in training programs across the country in 2021 (of Skill Development and Entrepreneurship, 2021).

While data on placement rates for vocational training graduates are hard to come by, a recent study by (Banerjee and Chiplunkar, 2018) sheds light on the topic. They find that 50% of trainees receive at least one interview upon graduating, 36% receive at least one offer and only 18% accept an offer. Moreover, only 9% are employed in the same job 3 months after graduating, 25% are employed in any job after 3 months, and 19% are employed in any job after 6 months. These findings also align with earlier studies that indicate unfavorable labor market outcomes for trainees, including a study conducted by the ILO (2004) in Andhra Pradesh, Maharashtra and Odisha that finds unemployment rates that range from 25-70%, and a study by the World Bank (2008) that documents unemployment rates of 60% among vocational training graduates across the country.[3] More broadly, these data suggest there may be a mismatch between the jobs that are available and the jobs that graduates want. Several studies suggest that vocational training graduates, and job-seekers more broadly, often have unrealistic expectations regarding the type of work and the wages they can secure (Groh et al., 2015; Abebe et al., 2018; Alfonsi, Namubiru, and Spaziani, 2023; Bandiera et al., 2021).

In this paper, we worked with the NSDC who provided contact information for recent PMKVY graduates from 168 training institutes specializing in 4 pre-selected trades (Telecom, Logistics, Sales and Security). These trades had the most employers and the highest rate of job offers on Job Shikari's platform. We randomly selected 30 graduates to call across 168 training centers. Approximately 50% of the calls did not result in a completed interview for various reasons, such as switched-off phones, outdated contact numbers, or respondents

[3] According to the latest estimates from the World Bank, India's unemployment rate stands at 7.3% while the youth unemployment rate has consistently remained three times higher at 23.2% since 2015.

declining to participate. We ultimately reached a sample of 2,662 graduates. Most of the sample of vocational training graduates we reached are male – only 11% of the respondents are female. They are relatively young (approximately 24), and only a third are married (Table A.1 Panel A). These vocational training programs typically cater to households from disadvantaged backgrounds, and over 65% of our sample comes from Scheduled Castes (SCs), Scheduled Tribes (STs), or Other Backward Class (OBCs).

Following Banerjee and Chiplunkar (2018), we compare our study sample to a nationally representative sample of the 68th Round of the National Sample Survey (NSS) conducted in 2011-2012. The NSS sample is constrained to individuals with at least high school level of education and between the age groups of 18-35 years of age to match the eligibility of the study sample. Table A.3 shows that our sample is similar in age and years of education, but has a higher concentration of unmarried males from SC/ST/OBCs.

We acknowledge that our results are influenced by the fact that we focus on vocational training graduates, whose characteristics may differ from the general population. However, the exact nature of these differences remains unclear. On the one hand, those who seek vocational training may have a lower marginal utility of current income, leading to higher reservation wages and inflated expectations regarding their labor market outcomes. On the other hand, it is possible that individuals who pursue vocational training may have lower beliefs about their job prospects, leading them to perceive the opportunity cost of training as relatively low. This may result in lower reservation wages and a more pessimistic outlook on their labor market prospects. Nonetheless, despite these considerations, we consider this group to be valuable for study as they represent a substantial population of approximately 74 million individuals globally, as estimated by the World Bank and ILO (2023).

3 Model

3.1 Status Quo

We consider dynamic searcher responses to the web platform through the lens of a finite-time version of the seminal search model from McCall (1970); Jovanovic (1979). Absent the platform, in each period $\{t\}_0^T$ workers draw a wage offer w_t from known distribution $F(w)$ with associated density $f(w)$, and $F(\underline{w}) = 0, F(\overline{w}) = 1$. Workers decide to accept that offer or wait until the next period and draw a new offer. We normalize the utility of unemployment to zero and assume zero job destruction, so that in each period t workers solve

$$V_t(w) = \max_{\{accept,reject\}} \{u(w_t) + \beta V_{t+1}(w_t), \beta E[V_{t+1}(w')]\} \tag{1}$$

where w_t is the wage offer they draw this period t, u is the utility they derive from this wage in period t, w' is the new wage draw they would get next period, V_{t+1} are the value functions associated with a particular wage, and β is the discount rate. The solution to this problem is a series of reservation wages which are declining in t, w_t^*, where workers accept any job offer $w_t > w_t^*$ which they keep until T and reject any other offer (See proof in Appendix B.1).

3.2 Digital Platform

Searchers on the platform draw a second wage offer w_t^p, which can be interpreted as the distribution of the best wage offer received from the platform in that period. The platform will be relevant to the job search problem if $q \equiv \mathbb{P}(w_t^p > w_t^*) > 0$. For simplicity we suppose $w^p \in \{\underline{\mathrm{w}}, \bar{w}\}$ so that $q = \mathbb{P}(w^p = \bar{w})$. To allow learning, we assume that searchers do not know q and instead form a belief $\hat{q}$. At baseline, searchers have uninformed priors so that $\hat{q} \sim U[0,1]$. Each period, searchers now receive an offer on and off the platform, they decide which (if any) of those offers to accept, and they update priors over $\hat{q}$ by Bayes' rule.[4] Finally, our framework does not allow on-the-job search on the platform, so that accepting a job offer on or off the platform ends the stream of platform offers. For our main results we need that on-the-job search is less efficient than off-the-job search. This would be true if adequately responding to information from the platform (going to the place of employment, submitting an application, etc.) is more challenging for the employed.

In this set up, workers that receive an offer $w^p = \bar{w}$ accept the offer and retain it until period T. Thus, the interesting dynamics (and the dynamics which likely applied to most of our sample, who did not receive high wage offers) are those who have not yet made a match on the platform. This means that after they are enrolled in the platform in period k, everyone who has not received an acceptable wage offer from the platform has the same history of wage offers in any period $t+k$: $w_k^p = w_{k+1}^p = ... = w_{k+t}^p = \underline{\mathrm{w}}$. We use this framework to derive the following model predictions, with proofs in the Appendix.

Proposition 1 *Access to the digital platform increases reservation wages for job searchers. The increase in reservation wages declines over time for searchers, and is smaller for older searchers.*

Access to the platform increases the expected future stream of high wage job offers, which increases the incentive to remain unemployed to receive those offers. Over time, the

[4]We focus on learning from the quality of offers rather than learning about offer rates because we do not see individuals learning along this second margin.

unemployed, who have by definition not yet received a high wage offer from the platform, update their priors $\hat{q}$ negatively.[5] In the appendix, we derive that after t periods of exposure to the platform the unemployed will form the posterior $E[\hat{q}] = \frac{1}{t+2}$ which clearly declines in t (See proof in Appendix B.2).

This increase in reservation wages has practical consequences for the graduates in our sample. As a result of reservation wages going up, employment may go up or down depending on whether the increase in the job arrival rate at higher wages outweighs the rate of declined jobs below the new reservation wage. This leads to an important corollary:

Corollary 1 *Suppose $\hat{q} > q = 0$ and $t < T$ for some job seekers. Then access to the digital platform reduces average employment rates.*

4 Experimental Design and Data Collection

4.1 Design

We ran our experiment from April 2015 to September 2016 with Job Shikari, a digital platform that charged companies a fee to send SMS messages to relevant job seekers. Once employers paid the fee associated with a fixed number of SMSs they waited for Job Shikari to run the platform's internal algorithm and send messages to relevant candidates. The algorithm prioritized job seekers working in the same trade and geographies as the jobs being advertised. The number of SMS that each recipient received was a function of the number of employers that contacted Job Shikari. In some cases that number was quite limited, particularly outside of Delhi (which we discuss in Section 5.2). All SMSs were constructed in the following way: *Data Entry Operator in Delhi, Salary 11500 Rupees, Please call +91*********. Interested job seekers contacted the phone number listed in the SMS to proceed with the next stages of the interview process (Job Shikari was no longer

[5]In the model, individuals perceive the platform as changing the probability they will receive a high quality job offer, which affects their reservation wage. They subsequently learn about the platform's effectiveness and further adjust their reservation wage. This approach is also consistent with other, less traditional reasons why platforms may boost perceived offer rates. For example, individuals may update beliefs about their "type" in response to the platform, and similarly update the ambient offer rate they expect (either on or off the platform). This would trigger the same dynamics on reservation wages and employment that we observe in our model. Similarly, individuals may apply to these jobs, get rejected, and update their expectations about their suitability for the jobs on offer. Alternatively, access to the platform may allow individuals to better motivate a lengthy unemployment spell to other household members if they believe that the job seeker's offer rate has been boosted. In the presence of these household bargaining dynamics, individuals will behave according to a weighted combination of their own optimized search problem and that of the household's. As households update their beliefs about the platforms' effectiveness over time they will renegotiate with individual job seekers, which will trigger the same dynamics on reservation wages and employment that we observe in our model.

involved, and did not track whether job seekers applied to the jobs). Employers waited to receive phone calls from these candidates.

Our sample consisted of a randomly selected group of graduates from 168 vocational training institutes working under NSDC's PMKVY program. We randomly assigned these graduates to a control group and one of two treatment arms, stratifying by location and their registered trade. Job Shikari did not contact, or send any SMSs, to job-seekers in the control group.[6] The platform contacted graduates in *both* treatment groups and sought their consent to upload their names, phone numbers, addresses, and trades of interest to the digital platform. No further information about why they were being uploaded to the platform was communicated to them. Once these graduates were uploaded to Job Shikari, they were sent another 'welcome SMS' introducing them to the platform. Graduates assigned to the first treatment group were eligible to receive text messages, but in practice they typically did not – likely because the number of SMSs the platform was sending was relatively small (constrained by the number of employers using the platform and the number of SMS they were prepared to pay for). Graduates assigned to the second treatment group received priority status (hence the designation 'priority treatment'): they appeared first on the list of job-seekers who matched a search query for a given job opportunity. Appearing first on these lists meant receiving many more SMSs. This priority status was never communicated to these individuals. Table 1 presents the number of SMSs that young graduates received by treatment status. Graduates in the treatment group received 1.6 text message on average (column 1), while those in the priority treatment group received an average of 19 text messages. In other words, 36 percent of job-seekers in the treatment group received at least one text message (column 1), while 65 percent of job-seekers in the priority treatment group received an SMS (column 2). Figure A8 shows the distribution of SMS messages received by graduates in the treatment and priority treatment groups, while Figure A9 represents a timeline of when SMSs were sent by Job Shikari.

[6] During their initial start-up phase, Job-Shikari adopted the strategy of obtaining lists of interested job-seekers by partnering with institutions like the National Skill Development Corporation, instead of promoting a formal platform that job-seekers could easily find and register on their own. While Job-Shikari had an online interface, it is uncertain whether job seekers would have come across it. As a result, Job-Shikari registered individuals from the lists they acquired in their system to receive text messages, providing them with the option to opt out if desired. On the other hand, individuals who were not on these lists had no convenient means of reaching out to Job-Shikari for registration. As a result, the control group did not have a practical opportunity to voluntarily participate. We purposely avoided asking graduates whether they had registered for the Job-Shikari platform to avoid creating a treatment effect through the provision of information.

4.2 Data

We conducted three rounds of phone surveys. We reached the full sample for baseline between April and July 2015 (2,662 graduates). Midline surveys took place approximately 9 months later on average, and we managed to reach 83% of respondents (2,230 graduates). Finally, we conducted the endline survey between June and September 2016, and successfully reached 71% of respondents (1,905 graduates). We allocated 30% of the sample to control, 40% to treatment, and 30% to priority treatment.[7] In addition to phone surveys, we also rely on a dataset shared by Job Shikari that has every text message that was ever sent to graduates (both in and outside of our sample) over the course of the study period. Table A.1 shows that we are balanced across most variables at baseline, though a few (six) variables demonstrate statistical imbalance. Reported reservation wage is the most concerning of these, where the difference between priority treatment and treatment is 950 rupees. We include individual fixed effects in our estimation strategy to account for any imbalance which is constant over time, and we will discuss additional checks we run for reservation wages in more detail in the results section.

Our survey instrument was relatively brief in an effort to increase participation and completion of our phone survey. Consistent with data availability, our approach to multiple testing is to focus analysis on a sparse set of outcomes which are both obvious and suggested by theory. Thus, our analysis focuses on 3 primary outcome variables: whether a respondent reports being currently employed at the time of survey ("employed");[8] the log earnings received by working respondents ("log wage"), and the log self-reported reservation wage. Reservation wages were solicited through an open-ended question ("What is the minimum salary that you would accept if you are looking for a job in your field/sector in your current location"). Both earnings and reservation wages are winsorized at the 99th percentile.[9]

[7]While these response rates are high for phone surveys, attrition may still pose concerns for analysis. Nevertheless, we do not see differential attrition between treatment groups – Table A.4. We also investigate whether attrition varies by baseline characteristics. While younger men with lower reservation wages are slightly less likely to respond in both control and treatment groups, overall we do not find evidence of differential attrition across treatment groups, either on average or as relates to baseline characteristics.

[8]This question was the feeder question into follow-up questions on job characteristics such as wages and working conditions. A minority (10%) of employed respondents report being self-employed. This number may well under-count casual and/or informal self-employment strategies that many use to earn income; from the perspective of our model we would consider this casual employment as similar to ongoing search

[9]Our trial was registered on the AEA RCT Registry (# AEARCTR-0001519 - where we conducted one fewer surveys than intended, and worked with a slightly smaller sample size than what was specified in the registry). However, we did not complete a pre-analysis plan (PAP). This prevents formal control of type one error through a multiple testing correction, as any efforts to do so would be influenced by the group of tests we report. Our approach to this has been to focus our primary analysis on only 3 outcomes, each obvious and easily derived from theory (employment, wages, and reservation wages). We also only report heterogeneity based on *ex ante* variables of stratification. Following the guidance of Banerjee et al. (2020), our readers may wish to interpret heterogeneity analysis as well as any reported analysis outside of our primary three

Feld, Nagy, and Osman (2022) find that winsorized, open-ended reservation wage solicitation perform reasonably well in terms of expected correlations with on-the-job amenities.[10]

Table A.1 Panel A provides information about the employment profile of our sample at baseline. Approximately 30% of the sample is employed, and 65% say they are actively looking for work at baseline. While the vast majority of graduates have access to the internet (approximately 76-80%), and many say they use the internet to find job opportunities (approximately 49%), fewer than 25% are formally registered with any digital platform at baseline.[11] While average salary offers on the platform were similar to the wages that employed individuals at baseline were working for, these wages lie far below what respondents think they should be paid. Indeed, graduates say they would not be willing to work for less than 12,000 rupees (172 USD) per month. These reservation wages are 20% higher than the average wages that employed individuals in our sample report earning, suggesting that respondens are overly optimistic about their wage prospects – a fact that is consistent with work by (Banerjee and Sequeira, 2020) and (Abebe et al., 2018) among others. Figure A7 further computes where graduates' baseline stated reservation wages lie in the distribution of actual wages that employed graduates from the same geo-zone and trade report at baseline. Most of the sample have reservation wages that are above the 50th percentile of wage offers, and the modal reported reservation wage is outside the winsorized support of observed wages.

While graduates are located across the country (North, South West, East and Delhi-NCR), the jobs that Job-Shikari advertised were almost exclusively located in Delhi-NCR (Figure A3). Job Shikari initially expected to have a broad national reach, but in reality, they identified a much higher concentration of jobs in Delhi. As a result, most individuals, regardless of their location, were receiving job opportunities from Delhi. Since graduates in the priority treatment group were assigned to receive a greater number of jobs through the portal, they ended up receiving more job offers from Delhi as well, regardless of their own location. This implies that some individuals were much closer to the jobs being advertised than others (Figure A4).[12]

Figure A5 also presents the average wage offers from Job Shikari relative to baseline wages. On average salary offers were comparable to the wages that employed individuals at

dependent variables as a secondary analysis.

[10]Though we should note that Feld et al ultimately prefer a different elicitation deriving from discrete choice experiments; we were unaware of this method or analysis when we implemented our program

[11]Registration with digital platforms at baseline is independent of the intervention. Only treatment and priority treatment were subsequently registered with Job Shikari.

[12]Delhi-National Capital Region (or Delhi-NCR) encompasses Delhi and several surrounding districts. North refers to Northern India excluding Delhi-NCR. South West and East comprise areas to the South West and East of Delhi-NCR, respectively.

baseline were working for. This does, however, mask some heterogeneity across geographic zones which we discuss later (Figure A6). We also show in Section 5.2 Job Shikari did not generate many new matches. To the extent that many labor market programs continue to have relatively muted impacts on employment (McKenzie, 2017), understanding how individuals respond to these opportunities remains an important policy question.

4.3 Estimation

We estimate the effects of our intervention by pooling the two follow up survey rounds and running the following regression using ordinary least squares:

$$y_{it} = \beta_0 + \beta_1 T_{it} + \beta_2 PT_{it} + \gamma_i + \delta_t + u_{it}$$

where y_{it} is an outcome of interest for graduate i in time period t. T_{it} is a dummy equal to 1 if the respondent was assigned to *either* of our treatments after enrollment to the portal (post-baseline), and 0 if the respondent was assigned to control. PT_{it} is an indicator equal to 1 for being in the priority treatment group after enrollment (post-baseline), and 0 if the respondent was assigned to the treatment group or the control group. This means T_{it} captures the impact of being enrolled to the portal, while PT_{it} captures the marginal impact of receiving more SMSs on the portal.[13] γ_i represents individual fixed effects; and δ_t represents a survey round fixed effect.[14] This specification emphasizes the difference between being registered on the platform (both treatment statuses) from the impact of receiving more information (only priority treatment). The coefficients of interest are β_1, which represents the average effect of being uploaded to the platform over time (relative to control); and β_2, which represents the average effect of receiving additional text messages over time as a result of being in the priority treatment group (relative to treatment). We cluster all regressions at the individual level (our unit of randomization).

5 Results

5.1 Employment and Job Search

Treatment graduates were notified they would be uploaded to a digital platform but only received 1.6 SMS on average. It follows that this treatment arm likely only shifted *expecta-*

[13] T_{it} and PT_{it} are equal 0 for all respondents at baseline.

[14] We pool midline and endline. We have also tested whether these effects differ between the midline and endline. These differences are not sufficiently statistically precise to yield helpful interpretations (Appendix Table A.6).

tions about the new arrival rate of jobs. We predict this will decrease employment rates as individuals anticipate and hold out for jobs that fail to materialize. This prediction is confirmed in our data (Table 2 Panel A). We see that treatment individuals' employment rate decreases by 9.2 percentage points in response to the notification that they were uploaded to Job Shikari (Column 1). Rather surprisingly, this translates into a 30 percent decrease in employment relative to the control group, and the effect persists for a full year. This result presents strong evidence of voluntary unemployment – these graduates prefer work less rather than accept the types of jobs they can access on the open labor market. While we may have expected to see these effect sizes fall by endline, long unemployment spells are relatively common in India (Naraparaju, 2017; Dhingra and Kondirolli, 2021; Biswas, 2022; Young, 2014), and it may take more than a year for the dis-employment effects we observe to fade (Appendix Table A.6). This effect also appears to be concentrated among individuals whose baseline observable would predict they earn high wages (Table A.5). Overall, the negative employment result suggests that the effectiveness of digital platforms will depend on individuals' expectations.

Nevertheless, we also establish that these effects on voluntary unemployment can be reversed. We predict that as graduates learn more about the types of jobs on the platform, and the feasibility of getting one of these jobs, they will re-adjust their expectations. This effect can be tested in the priority treatment group, which received 17.4 *more* text messages that the treatment group on average. The text messages they received revealed that jobs on offer were located heavily in Delhi, and were relatively low paying. We find that the disemployment effect is muted in the priority treatment group: graduates only experience a 4.8 percentage point (16%) decrease in employment relative to control.[15] This suggests that new labor market interventions may need to find ways to set individuals' expectations about their job prospects by providing more information about the broader labor market. Consistent with this hypothesis we find suggestive evidence that youth in the priority treatment group are 3 percentage points (p-value = 0.133) less likely to say job platforms are a helpful tool in their search for jobs (Table A.8).[16]

We also investigate whether accessing the platform has any impact on actual wages for the employed sample, though this is difficult to interpret because the selection of workers is

[15] We investigate the possibility of spillovers across alumni from the same training institute. Table A.7 finds no significant evidence of spillovers to the control group.

[16] Concerns about graduates misreporting their employment status to receive additional SMSs are mitigated by a number of facts. First, the research team did not affiliate itself with the digital platform, and its unlikely that respondents would have attributed the 19 SMS they received over 12 months to our team, particularly given that promotional messaging via SMS is not unusual in India. Second, we asked respondents whether they were interested in hearing about jobs, and the treatment group was no more likely to say 'yes'. Finally, misreporting does not explain the differences between treatment and priority treatment.

changing with treatment status. Table 2 presents the results on log wages (column 2). We do not see evidence of large effects on wages for the treatment group. The coefficient on wages for the priority treatment group is positive but insignificant. We might expect to see this positive result if more graduates (not just low ability types) are accepting jobs.

We hypothesize that these employment results are driven, at least in part, by graduates beliefs about what the platform can do for them. Table 2 Panel B presents the results on three different measures of respondents beliefs. While imprecisely estimated, we find that being uploaded to the digital platform leads to a small (3.1%) increase in reservation wages for the treatment group relative to control (p-value = 0.192), as graduates expect and hold out for better jobs. Conversely, we see that reservation wages decrease significantly in the priority treatment group, by approximately 5.1% as individuals realize the jobs on offer are not what they expected.[17] In the last three columns of Table 2 we ask respondents to estimate the probability they can get a job in their current location that pays 10,000, 16,000 and 20,000 rupees respectively. We see stronger negative effects (albeit imprecisely estimated) on the priority treatment group, which seems to suggest that individuals who receive more text messages have a more negative assessment of their probability of actually getting a job at these wage rates.[18]

Are these changes in reservation wages meaningful? To answer this question we explore how reservation wages respond to two other metrics of interest: being employed and searching for a job. Table 3 compares how reservation wages respond to treatment (column 1), relative to finding a job and/or actively searching for work (column 2). While the estimates in column (2) cannot be interpreted as causal, their magnitudes are informative. They suggest that being uploaded to the platform has a similar impact on reservation wages to becoming employed or searching for a job. In columns (3) and (4) we focus on a subsample of graduates who we expect to report reservation wages with less error – the 63% of graduates who actually

[17] The priority treatment group has higher reservation wages at baseline. In our analysis throughout, we address this through the use of individual fixed effects. Even with fixed effects, the possibility remains that mean reversion could explain these differences. To test this, we split our sample into respondents with baseline reservation wages above and below the mean. Table A.9 shows that individuals with low baseline reservation wages experience the strongest movements in employment and reservation wages. This indicates that the imbalance in reservation wages in priority treatment is biasing us against finding these effects, rather than towards them. Note we can calculate baseline reservation wages differently (as deviations from a group) but the results do not change meaningfully.

[18] We might expect people with more biased beliefs to respond more as they see lower wage offers coming in. While this is an interesting dimension of heterogeneity, most of the graduates in our sample report beliefs that are somewhat inconsistent with the reality of the labor market they are searching in. Figure A7 shows that many graduates in our sample report reservation wages that would be difficult to rationalize based on the actual job market. Those with the most biased beliefs are either misreporting their reservation wages or else do not use observed wage distributions to inform their reservation wages. In either case it is hard to predict whether we should anticipate seeing larger (they update more) or smaller (they are remain overly optimistic) impacts on reservation wages for this group.

report searching for work in our data. Reservation wages are poorly defined (both in the model and, likely, in survey responses) for individuals who are not searching, which means that accurate measurements of reservation wages are missing for those who are not presently searching. In order to avoid endogenous transitions in and out of search, we focus on the behavior of those engaged in search throughout our study period. The sample of always searchers is balanced (Table A.10), and exhibits similar employment and wage responses (Table A.11).[19] The impact on reservation wages is more pronounced for the always searchers: doubling from 3.1% to 7.7% in treatment, and from -5.1% to -8.0% for priority treatment. At the mean this suggests treatment status are changing reservation wages by approximately 500 rupees in the full sample, and 1000 rupees in the sample of searchers. Yet again the effects of treatment are similar in magnitude to becoming employed among this subsample.

While these reservation wage effects are large, the employment effects are more tightly estimated and larger in magnitude than what we would have expected from our estimated reservation wage effects. This could be because search behavior is not fully described by the simple search and matching model, or due to attenuation bias from the measurement error in reservation wages. We anticipate that measurement error in reservation wages is large which means that the two treatment effect estimates (employment and reservation wages) are not directly comparable. As a result, we emphasize the part of the story that is well captured by the neoclassical model even if other factors may also be at play.[20]

We can also estimate the effect of receiving an additional SMS on employment if we make the additional assumption that changes in employment opportunities change discretely after receiving the first SMS and then linearly with each offer received. We do so by instrumenting whether graduates receive any SMS, and the number of SMS they receive with indicators for being assigned to Treatment and Priority Treatment, respectively (where Treatment is a dummy equal to 1 if the individual was assigned to either of our treatments, as above). The results are presented in Table A.12. The results are broadly consistent with what we observe from the main specification in the paper. Receiving any SMS at all is associated with a 29 percentage point decrease in the probability of being employed (column 1). Each additional SMS received increases the probability of being employed by 0.8 percentage points. Job-seekers in the priority treatment group receive 17 text messages on average, such that their

[19]If treatment influenced transitions in and out of search, this may mean that this group is differently selected depending on which treatment group they belong to. We nonetheless think this is the most sensible group to focus on as accurate reservation wage data is missing for non-searchers and are somewhat reassured by the fact that search behavior is not associated with treatment (column 2 of Table A.14) and that the inclusion of individual fixed effects will remove any differential attributes of these always searchers which do not change over time.

[20]The fact that non-searchers report reservation wages makes clear that some respondents interpret this question differently than the model does.

probability of employment falls by only 15 percentage points relative to the control group. Similarly we find suggestive evidence that reservation wages increase by 10 percent (p-value = 0.15) for those who received any SMS, but start to fall significantly as individuals receive additional SMSs (column 3). We do not see evidence of large effects on wages (column 2).[21]

Finally, we look at whether individuals in the treatment groups spend more or less time searching and applying for jobs. We do not see any significant impacts on the extensive margin. Column 1 Table A.14 shows that the probability of engaging in any type of search does not change among treatment and priority treatment groups. This result is not altogether surprising, however, as individuals can be passive users of the platform as they wait for additional text messages. Nevertheless, we do see suggestive evidence that the platform has a large (albeit statistically insignificant) effect on the number of applications graduates submit across all platforms (Column 3). The point estimates are consistent with the treatment group applying less (3%) and the priority treatment group applying more (13%). These results become more pronounced when we look at the subset of always searchers: the treatment group applies to 15% fewer jobs while the priority treatment group applies to 13% more. While these results corroborate our evidence on voluntary unemployment, we still interpret these results with some caution. Our job-search measures captures applications across all platforms, and we are not able to measure job search intensity on different platforms: it could be that Job Shikari changes the amount of effort individuals invest in certain job search tools

[21] Relatedly, we can also investigate the impact of receiving a higher number of SMS offers and fewer *relevant* SMS messages on our three core outcomes (employment, wages and reservation wages). To determine the number of SMS messages, we calculate the total count of messages received by graduates until their survey participation. Assessing the attractiveness of SMS offers involves considering both the wage offered and the job location, which were the key factors conveyed in the SMS messages. To quantify the number of unattractive SMS offers, we multiply the number of SMS messages received by each graduate about a job that was not local, with a measure of how unappealing the wage offers were to individuals who received them in the same trade and geographic group. This measure, known as the "group offer rate," calculates the percentage of offers that were higher than the baseline wages for different wage bins, weighting by the number of graduates in that geographic/trade strata with baseline wages in that bin. We then take 1-the "group offer rate", such that higher values indicate a higher share of unattractive (low wage) offers. We present the results of this analysis in Table A.13, which are consistent with what we find above. When graduates receive a substantial number of SMS messages, they adjust their expectations regarding the platform's offerings and are more likely to become employed. Specifically, graduates who receive many SMS messages (as the priority treatment group did) are 4.7 percentage points more likely to secure employment compared to those who receive none. We observe a decrease in reservation wages by 4.7% (albeit statistically insignificant) for those receiving many SMS messages relative to those who received none (p-value = 0.134). We observe similar effects when focusing on the number of bad SMS messages received by graduates. Those who receive many bad SMS messages are 11.1 percentage points more likely to secure employment compared to those who receive none. Additionally, reservation wages decrease by 4.6% (albeit statistically insignificant) for those receiving many bad SMS messages. We do not see evidence of large effects on wages.

over others.[22]

5.2 Learning on the Platform

We find that sending additional SMS's to the priority treatment group results in higher reservation wages and employment rates relative to the treatment group. While our results on reservation wages suggest this effect is driven by changes in beliefs, it is also possible that match rates were higher among this group. We do not have direct evidence on matches generated by the platform, but we can use the experiment's geographic stratification to explore this further.[23] The geographic stratification is particularly informative as graduates in the four zones differ strongly in baseline characteristics, and also experienced treatment in very different ways. Table A.15 demonstrates that graduates in the South West zone are much older, more likely to be married, with less educated parents and less likely to be General Caste than those in Delhi NCR. Table A.15 also suggests that graduates in the East and North zones are disadvantaged in some characteristics relative to those in Delhi, though not always as significantly so as those in the South West.

The experience of being assigned to priority treatment was also very different by geography. Looking first at the quantity of jobs, Table 1 confirms that priority treatment group in Delhi NCR saw 57 text messages on average, while the priority treatment group in the North and East saw 20 and 14 job offers respectively. The priority treatment group in the South West received only 4 text messages on average. These differences were driven by the differential success that Job Shikari had in identifying employers in these different regions. The quality of jobs differs significantly by geographic zone as well. Figure A4 shows the location of the advertised jobs relative to graduates' primary residence in each part of the country. Individuals in Delhi NCR were seeing jobs exclusively in the city where they live. Individuals in the North were living in and around Delhi, which is where most of the SMSs were advertising work opportunities. Graduates in the South West and East were seeing jobs located in Delhi despite living over 500 kilometers away. Turning next to the distribution of wage offers, we can see that they are differentially attractive across geographic zones. The platform's wage offers were more appealing in the South West and the East, where the baseline wages were lower (Figure A6).

These differences affect the priority treatment group's probability of employment. Table 4

[22] Nevertheless, to the extent that it causes graduates to spend less time on *other* platforms, we would interpret this as evidence for voluntary unemployment because graduates do not need to invest effort in searching on Job Shikari beyond waiting for additional text messages.

[23] It was important that the survey not be associated with the Job Shikari treatment, and that enumerators not be aware of individuals' treatment status. This meant that we did not ask the treatment groups about whether they applied to the jobs they saw.

demonstrates that while treatment graduates in all four regions experience similar voluntary unemployment effects, the impact of being in the priority treatment group is only detected in the South West and the East. Individuals in the priority group in the South West experience a 8.9 percentage point increase in the probability of being employed relative to treatment. Similarly, priority treatment graduates in the East experience a 7.5 percentage point increase in the probability of being employed relative to treatment. The priority treatment effects in the North and Delhi NCR are approximately 0 and statistically insignificant. We conclude that the positive priority treatment effect in the whole sample is driven by those in the South West and the East: who are older and less well-off, who received information about jobs that were well matched on wage offers but poorly matched spatially.

These results may appear surprising at first glance because many more SMSs were sent to job-seekers in Delhi NCR and the North. Given the lower treatment priority intensity in the areas with the largest response, we validate whether these differential effects are plausibly driven by the intervention. More specifically, we include an indicator for whether graduates assigned to the priority treatment group received zero SMS in our standard specifications. If the priority treatment graduates did not receive any SMSs, we would expect them to look similar to the treatment group. Table A.16 column 1 confirms that the employment effects are concentrated among those in the priority treatment group who are receiving SMSs (the coefficient on priority treatment with zero SMS for the South West is the opposite sign (negative) and of similar magnitude to the coefficient on priority treatment for the South West). This supports the hypothesis that these graduates' differential employment behavior are likely to be a reaction to information received from the portal.

In principle, the positive priority treatment effects we observe for graduates far from Delhi could be generated by new matches, or by differences in how these individuals allow the platform to boost their reservation wages. If the positive priority treatment effects in the South West and East were attributable to newly generated matches, we would expect to see a migration response to rectify the spatial mismatch. Table 5 shows that on average, respondents do become more urbanized in response to priority treatment, providing evidence that individuals did respond to the information on job locations. To test whether this information led to new matches in the South West and East, we compare the heterogeneous results on employment by geographic zone to those on migration patterns, presented in Table 5. Focusing on the priority treatment group, we see that graduates in the North drive the urbanization results, as they are 12 percentage points more likely to be living in a city. Despite experiencing the largest bounce back in employment rates, graduates in the South West and East do not differentially move in response to priority treatment. We therefore conclude that the respondents in our sample did learn something meaningful about job location, namely that most job offers were located in urban Delhi. However the different patterns of employment and migration responses suggest that the platform itself did not lead to many new matches.[24]

Alternatively, the strong employment effects in the South West and East could be driven by graduates updating their beliefs about the likelihood of finding a job on the platform, and choosing to end voluntary unemployment spells more quickly. In particular, graduates in the South West and East may infer the platform is unlikely to deliver a job opportunity close to home, and respond by accepting employment opportunities closer to where they live. Graduates in the South West and East are also older, married, and poorer which means the opportunity costs of holding out for a good job on the platform could be higher; our model suggests this directly for older cohorts. This group may choose to end voluntary unemployment spells more quickly either due to the physical distance between regions, or to differences in the characteristics of our sample across regions. Somewhat strikingly, we find that the heterogeneity in priority treatment effects across geographical zones can be largely explained by differences in demographic characteristics (Table 6). In other words, we think this heterogeneous response is best explained by *older* job seekers updating their beliefs faster in response to text messages about jobs that are poorly matched.

To more formally test whether the priority treatment effects we observe are primarily a withdrawal from voluntary unemployment, we investigate whether the effects are largest for individuals who are unable to afford longer periods without work. Proposition 1 suggests that the option value of waiting for a platform job should be lower for older groups, so that they may revise their beliefs about the platform more quickly than their younger, single counterparts. Table A.17 shows that this is indeed the case.[25] While younger priority treatment graduates experience an insignificant 2 percentage point increase in their probability of employment, the older priority treatment graduates are 10.4 percentage points more likely to be employed. In fact, the negative employment effect disappears altogether for this older group. Figure 1 confirms this result: both younger and older graduates are less likely to be employed in response to treatment, but being in the priority treatment group helps only older graduates bounce back relative to treatment. Graduates in the oldest quintile are no less likely to be employed at endline than the control group.

[24]The idea that individuals could be learning something about the spatial distribution of jobs, and respond accordingly is consistent with other papers in this literature. Banerjee and Sequeira (2020) find evidence that job seekers have biased beliefs about the arrival rate of jobs in city centers. Similarly, Abebe et al. (2021) find that providing job seekers in Ethiopia with transport subsidies induces people to search more efficiently for work and find employment closer to the city center.

[25]Though outside the scope of our model, Table A.17 also presents the results for other groups that may be unable to afford longer periods without employment: married, highly educated, general caste and rural graduates.

6 Discussion

Matching youth to prospective employers has become a policy priority for many governments – global youth unemployment rates currently stand at 13% and continue to rise. While there have been numerous attempts to design interventions that will facilitate matches between job seekers and potential employers, they have had modest impacts on employment rates. The literature suggests this may be because youth are voluntary unemployed because of a "mismatch of expectations" (Banerjee and Duflo, 2019). Young graduates seem to hold out for better jobs rather than accepting jobs they can feasibly get.

In this paper we directly test this hypothesis by evaluating the impact of enrolling vocational training graduates onto a digital platform, and providing them with information about jobs. We find that graduates who were notified that they would be uploaded to a digital platform are 9 percentage points less likely to be employed for at least 1 year. We interpret this result as strong evidence for voluntary unemployment: graduates prefer to wait for good jobs than accept those they have access on and off the platform. The priority treatment group receives more information from the digital platform, and experiences a less strong disemployment effect. This result is driven by older graduates in the South West and East who choose not to relocate to Delhi, where most of the platform's jobs are located. Instead, they re-adjust their expectations and are more likely to accept a job off the platform.

In our context, we observe that access to a digital platform raises youth's expectations in ways that may not seem rational; and these expectations effects can only be overcome when individuals have sufficient information about the types of jobs that the platform has to offer. Some caution must be taken in extrapolating these results to the general performance of digital platforms and other online matching programs in labor markets. Several characteristics are specific to our context. First, the platform we worked with did not identify high quality jobs for the youth in our study. This means that our study emphasizes the costly behavioral response to *perceptions* of improved matching, without the associated benefit of an *actual* improvement in matching. While many other platforms will surely be able to make improvements in match quality, we note that this problem may be pervasive in low-skilled labor markets. Employers will have the greatest demand to circulate job information via a platform for the jobs with the most significant matching frictions. In contrast, when desired skills are not unusual, and job characteristics are desirable, it is reasonable to expect that employers can find a match without paying a platform.

Second, our sample are young, have recently graduated from vocational training institutes, and may have a shorter history of exposure to matching marketplaces than some populations. This may make them more susceptible to voluntary unemployment due to mismatched expectations than other populations. Several recent studies have documented mismatched expectations among youth in low income countries, perhaps particularly among vocational training institute graduates (e.g. Alfonsi, Namubiru, and Spaziani (2023); Banerjee and Chiplunkar (2018); Banerjee and Sequeira (2020); Jones and Santos (2022)). Vocational training is one of the most common policy response to youth unemployment, and so this sample seems appropriate for a study of this nature. At the same time, these recent graduates may be particularly ill-informed about the utility of digital platforms or their labor market prospects more broadly, generating a large gap between the perceived promise and the reality of the platform.

While our study takes place in India, similar patterns have been observed elsewhere. In Mozambique, (Jones and Santos, 2022) find that providing SMS information to university graduates about market wage rates leads to a quicker reduction in wage expectations, albeit one that still leaves a significant gap between expected and likely actual wages. This result recalls directly the gap between our standard treatment and priority treatment effects: by providing information on a range of job opportunities, treatment priority allowed respondents to estimate a more informed prior about their expected labor market characteristics, allowing some workers to resume working sooner. Similarly, Banerjee and Sequeira (2020) incentivize youth from Soweto, a township bordering Johannesburg, South Africa, to search in Johannesburg. After jobs fail to materialize, these youth reduce reservation wages and become more likely to work close to home. Once again, this results parallels both the mechanism in our model and the response we observe to priority treatment: the experience of searching in the city center informs youth about the arrival rate of jobs (which is low); they respond by accepting jobs closer to home after their expectations are realigned.[26] In all three cases, youth form unrealistically optimistic beliefs about what the labor market will deliver and/or how to search effectively; additional information and exposure de-biases some of this belief. Our paper extends these results to demonstrate that youth respond to a new source of job opportunities by withdrawing from employment as they wait for jobs to materialize.

Of course, none of these three studies generate more employment, or better matches for youth. A promising step forward is in Uganda, where Alfonsi, Namubiru, and Spaziani (2023) find that pairing vocational training graduates with alumni mentors leads to a quicker employment trajectory after graduation in part through sharing detailed information about labor market experiences (such as the need to start out at an unpaid internship). The Alfonsi, Namubiru, and Spaziani (2023) intervention differs from ours and those above in several ways: it is personal, tailored, and holistic. Workers can ask advice and learn more subtle interactions than simply receiving an SMS, or searching for work in a different location.

[26] Though the net effect on employment for treatment youth is zero in Banerjee and Sequeira (2020)

Together with our results and the results from Jones and Santos (2022) and Banerjee and Sequeira (2020), these studies suggest that matching interventions may be more effective when complementary information systems help calibrate the expectations of youth who use them.[27] Because private entities assisting in school-to-work transitions are incentivized to present positive analysis of their jobs and success rates, regulation (or public information provision) may be necessary to maximize the effectiveness of these technologies for young job seekers.

Ultimately, we contribute to a growing literature that documents that incorrect beliefs influence the labor supply of youth, by identifying that labor supply is sensitive to the presence of new matching interventions. It remains unclear when and why mis-calibrated expectations exist and persist for youth in the labor market, which is a compelling area for future work.

[27] This interpretation is consistent with the positive effects of LinkedIn combined with a literacy training on South African youth (Wheeler et al., 2022)

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Tables

Table 1: SMS received

	(1) # SMS Received	(2) Any SMS Received	(3) # SMS Received	(4) Any SMS Received
Treatment	1.608***	0.361***		
	(0.112)	(0.016)		
Priority Treatment	17.401***	0.289***		
	(1.059)	(0.023)		
Treatment East			0.917***	0.331***
			(0.106)	(0.028)
Treatment DelhiNCR			7.028***	0.851***
			(0.581)	(0.033)
Treatment North			1.208***	0.363***
			(0.124)	(0.028)
Treatment SouthWest			0.392***	0.156***
			(0.076)	(0.024)
Priority Treatment			12.763***	0.231***
East			(1.400)	(0.042)
Priority Treatment			49.586***	0.151***
DelhiNCR			(4.876)	(0.033)
Priority Treatment			19.135***	0.492***
North			(1.251)	(0.035)
Priority Treatment			3.766***	0.124***
SouthWest			(0.922)	(0.042)
Respondent Fixed Effects	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes
Number of Individuals	2662	2662	2662	2662
Number of Observations	6797	6797	6797	6797

This table shows the number of SMS received. Column 1 is the *number* of SMS's that graduates received from Job Shikari, and Column 2 is an *indicator* for whether the respondent received any SMS from Job Shikari (column 2). Columns 3 and 4 consider the same two outcomes by geographic zones. Graduates in the control group did not receive any text messages. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 2: Employment and Beliefs

	Panel A: Employment	
	(1) Employed	(2) Log(Wage)
Treatment	-0.092***	-0.012
	(0.022)	(0.067)
Priority Treatment	0.048**	0.062
	(0.021)	(0.077)
Mean in Control	0.30	9.08
F-test T + PT	0.06	0.54
Respondent Fixed Effects	Yes	Yes
Survey Round Fixed Effects	Yes	Yes
Number of Individuals	2662	1257
Number of Observations	6866	2311

	Panel B: Beliefs			
	(1) Log(Reservation Wage)	(2) Prob10	(3) Prob16	(4) Prob20
Treatment	0.031	-0.010	-0.165	0.099
	(0.024)	(0.216)	(0.192)	(0.175)
Priority Treatment	-0.051**	-0.399*	-0.239	-0.252
	(0.025)	(0.207)	(0.187)	(0.178)
Mean in Control	9.3	4.9	3.0	2.0
F-test T + PT	0.44	0.07	0.05	0.42
Respondent Fixed Effects	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes
Number of Individuals	2622	2588	2570	2559
Number of Observations	6489	6337	6179	6097

Panel A illustrates the impact of the treatments on employment outcomes. The dependent variables are an indicator for whether the respondent is employed (column 1), and the logarithm of wages, winzorised at the 1% (column 2). Columns 1 includes all respondents in the sample while column 2 only include respondents who were employed at the time of survey. Panel B illustrates the impact of the treatments on beliefs. Column 1 presents the logarithm of respondents reservation wages (where we asked respondents "What is the minimum salary that you would accept if you are looking for a job in your field/sector and location"), winsorized at the 1%. Column 2-4 shows the probability that respondents think they will get a job that pays 10,000, 16,000 and 20,000 rupees respectively (where we ask "On a scale of 1-10, how likely is it that you will be offered a job that pays at least 10,000 in your location next month?"). Only 5% of job-seekers respond inconsistently with probabilities that weakly increase as the wage increases. *Treatment* is a dummy equal to 1 if the respondent was assigned to *either* of our treatments after enrollment to the platform (post-baseline), and 0 if the respondent was assigned to control. *Priority treatment* is an indicator equal to 1 for being in the priority treatment group after enrollment (post-baseline), and 0 if the respondent was assigned to the treatment group or the control group. We test whether the priority treatment group is significantly different from the control group by testing whether $T + PT = 0$. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 3: Reservation Wages

	All		Searchers	
	(1) Log(RW)	(2) Log(RW)	(3) Log(RW)	(4) Log(RW)
Treatment	0.031		0.077**	
	(0.024)		(0.037)	
Priority Treatment	-0.051**		-0.080**	
	(0.025)		(0.037)	
Employed		0.058***		0.071***
		(0.018)		(0.026)
Searching		-0.030*		
		(0.016)		
Mean in Control	9.29	9.29	9.26	9.26
F-test T + PT	0.44		0.94	
Respondent Fixed Effects	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes
Number of Individuals	2622	2621	1058	1058
Number of Observations	6489	6479	2551	2551

This table demonstrates that reservations wages are affected by the treatments, and by being employed or searching for a job. Column 1 and 3 in this table show the impact of treatment on log reservation wages winzorised at the 1%. Column 2 and 4 show the impact of being employed (=1 if employed) and searching (=1 if searching) for a job on log reservation wages winzorised at the 1%. In columns 1 and 2 we focus on the full sample, whereas in columns 3 and 4 we focus on the sample of respondents who are searching throughout the study period. *Treatment* is a dummy equal to 1 if the respondent was assigned to *either* of our treatments after enrollment to the platform (post-baseline), and 0 if the respondent was assigned to control. *Priority treatment* is an indicator equal to 1 for being in the priority treatment group after enrollment (post-baseline), and 0 if the respondent was assigned to the treatment group or the control group. In column 1 and 3 we test whether the priority treatment group is significantly different from the control group by testing whether $T + PT = 0$. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 4: Employment and Wages by Geographic Zone

	(1) Employed	(2) Log(Wage)
Treatment East	-0.051	0.193
	(0.037)	(0.243)
Treatment DelhiNCR	-0.112*	-0.215
	(0.059)	(0.139)
Treatment North	-0.087**	0.002
	(0.039)	(0.126)
Treatment SouthWest	-0.144***	-0.005
	(0.046)	(0.087)
Priority Treatment East	0.075**	0.128
	(0.033)	(0.147)
Priority Treatment DelhiNCR	-0.013	-0.007
	(0.055)	(0.069)
Priority Treatment North	0.016	0.173
	(0.039)	(0.209)
Priority Treatment SouthWest	0.089**	-0.021
	(0.044)	(0.091)
Mean in Control	0.30	9.08
Respondent Fixed Effects	Yes	Yes
Survey Round by Geo Fixed Effects	Yes	Yes
Number of Individuals	2662	1257
Number of Observations	6866	2311

This table shows the impact of the treatments on employment outcomes and wages - broken out by geographic zone. The dependent variables are an indicator for whether the respondent is employed (column 1), and the logarithm of wages winzorised at the 1% (column 2). Column 1 includes all respondents in the sample while column 2 only includes respondents who were employed at the time of survey. *Treatment* is a dummy equal to 1 if the respondent was assigned to *either* of our treatments after enrollment to the platform (post-baseline), and 0 if the respondent was assigned to control. *Priority treatment* is an indicator equal to 1 for being in the priority treatment group after enrollment (post-baseline), and 0 if the respondent was assigned to the treatment group or the control group. We include indicators for being in treatment and priority treatment for each geographic strata. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 5: Living in a City

	(1) In-City	(2) In-City
Treatment	-0.020	
	(0.024)	
Priority Treatment	0.060***	
	(0.022)	
Treatment East		0.002
		(0.044)
Treatment DelhiNCR		0.046
		(0.032)
Treatment North		-0.038
		(0.043)
Treatment SouthWest		-0.061
		(0.048)
Priority Treatment East		0.029
		(0.042)
Priority Treatment DelhiNCR		0.014
		(0.033)
Priority Treatment North		0.120***
		(0.040)
Priority Treatment SouthWest		0.042
		(0.048)
Mean in Control	0.51	0.51
F-test T + PT	0.11	
Respondent Fixed Effects	Yes	Yes
Survey Round Fixed Effects	Yes	Yes
Number of Individuals	2662	2662
Number of Observations	6889	6889

This table shows the impact of the treatments on the probability of living in a city. The dependent variable in both columns is an indicator for whether the respondent is currently living in a city. Column 1 estimates the impact of treatment and priority treatment for the pooled sample, while Column 2 estimates the impact of treatment and priority treatment for the sample broken out by geographic zone. *Treatment* is a dummy equal to 1 if the respondent was assigned to *either* of our treatments after enrollment to the platform (post-baseline), and 0 if the respondent was assigned to control. *Priority treatment* is an indicator equal to 1 for being in the priority treatment group after enrollment (post-baseline), and 0 if the respondent was assigned to the treatment group or the control group. In column 1 we test whether the priority treatment group is significantly different from the control group by testing whether $T + PT = 0$. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

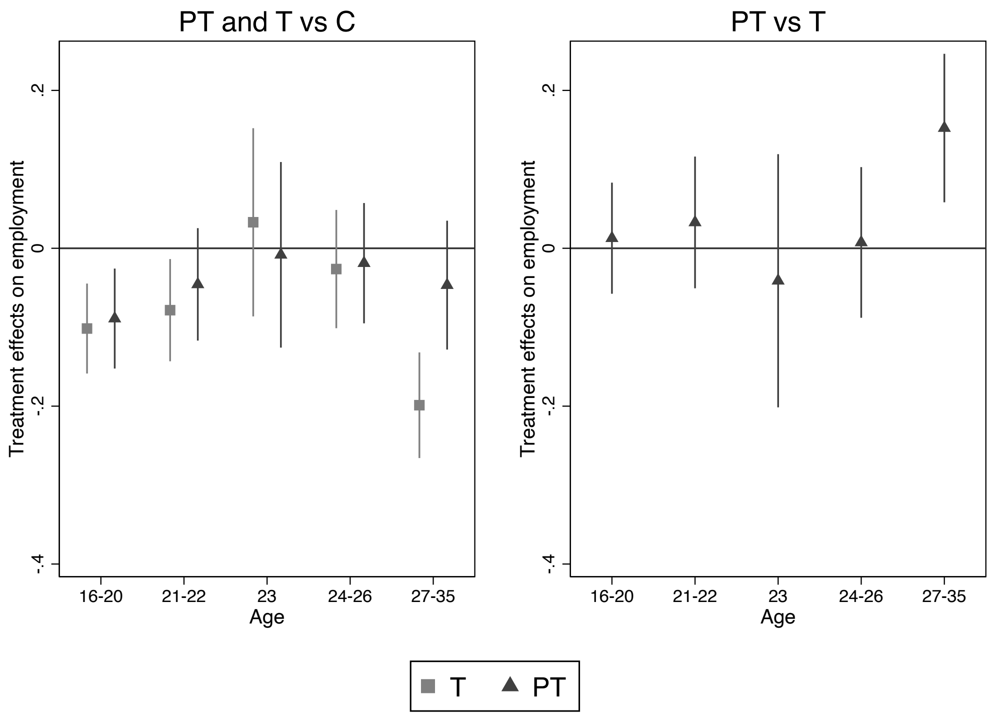
Table 6: Employment, Living in a city, and Search (controlling for Predicted Geographic Zone)

	Main			Predict		
	(1) Employed	(2) In-City	(3) Search	(4) Employed	(5) In-City	(6) Search
Treatment East	-0.051	0.002	-0.052	-0.284	-1.422***	-0.701*
	(0.037)	(0.044)	(0.044)	(0.323)	(0.231)	(0.377)
Treatment DelhiNCR	-0.112*	0.046	0.088	-0.329	-1.080***	-0.474
	(0.059)	(0.032)	(0.088)	(0.316)	(0.217)	(0.364)
Treatment North	-0.087**	-0.038	0.056	-0.319	-1.385***	-0.525
	(0.039)	(0.043)	(0.048)	(0.327)	(0.229)	(0.378)
Treatment SouthWest	-0.144***	-0.061	-0.033	-0.359	-1.405***	-0.589
	(0.046)	(0.048)	(0.052)	(0.327)	(0.225)	(0.377)
Priority Treatment East	0.075**	0.029	0.002	0.403	0.345	0.108
	(0.033)	(0.042)	(0.044)	(0.431)	(0.347)	(0.559)
Priority Treatment DelhiNCR	-0.013	0.014	0.052	0.295	0.381	0.191
	(0.055)	(0.033)	(0.081)	(0.419)	(0.331)	(0.544)
Priority Treatment North	0.016	0.120***	-0.037	0.352	0.497	0.070
	(0.039)	(0.040)	(0.048)	(0.435)	(0.343)	(0.556)
Priority Treatment SouthWest	0.089**	0.042	0.036	0.366	0.446	0.088
	(0.044)	(0.048)	(0.050)	(0.434)	(0.342)	(0.556)
Mean in Control	0.30	0.51	0.65	0.29	0.50	0.67
Respondent Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Individuals	2662	2662	2661	1837	1837	1837
Number of Observations	6866	6889	6828	5478	5479	5466

The first 3 columns regress three core outcomes (employment status, living in a city, and wether the respondent is searching for a job) on our indicators for treatment status interacted with geo-zone. The next three columns take these same regressions and control for a predicted geo-zone measure, which we constructed by regressing indicators for being in these zones on a set of demographic characteristics. Both the estimated treatment and priority treatment effects on all dependent variables are remarkably similar across geo-zones when we do so. This confirms that the heterogeneous treatment effects we observe in our geographic strata are at least partly attributable to the underlying characteristics that correlate with location and mediate search behavior. *Treatment* is a dummy equal to 1 if the respondent was assigned to *either* of our treatments after enrollment to the platform (post-baseline), and 0 if the respondent was assigned to control. *Priority treatment* is an indicator equal to 1 for being in the priority treatment group after enrollment (post-baseline), and 0 if the respondent was assigned to the treatment group or the control group. We include indicators for being in treatment and priority treatment for each geographic strata. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Figures

Figure 1: Effect of Treatments on Employment by Age Group



This figure plots our treatment effects on employment for progressively older samples. We bin ages into quintiles. In the left panel we display the estimates on treatment and priority treatment from running a specification of employment status on an indicator for being in treatment (not including priority treatment) and priority treatment, interacted with age quintiles. In the right panel we display the estimates on priority treatment from running our standard specification of employment status on any treatment and priority treatment interacted with age quintiles. For example, from the left panel we read that the impact of being in the treatment group (excluding priority treatment) is -0.10 relative to control for respondents who are 16-20 years old, and the impact of being in the priority treatment group is -0.089 relative to control for respondents ages 16-20. From the right panel we read that the impact of being in the priority treatment group relative to treatment is 0.01 for respondents ages 16-20.

Appendix for "How do Online digital platforms affect Employment and Job Search? Evidence from India"

Contents

A Appendix for Online Publication Only - Tables and Figures

A.1 Tables

A.1.1 Balance (Main)

Table A.1: Job and Graduates Characteristics

Panel A: Job-seeker Characteristics

	(1) Control	(2) Treatment	(3) Priority Treatment	(4) (1) vs. (2), p-value	(5) (1) vs. (3), p-value	(6) (2) vs. (3), p-value	(7) Joint F-test
=1 if male	0.86	0.89	0.88	0.04	0.20	0.53	0.13
Age	24.17	24.01	24.30	0.53	0.64	0.26	0.52
Education (Yrs)	14.17	14.22	14.29	0.63	0.28	0.50	0.55
=1 if completed 10th pass	0.98	0.99	0.98	0.57	1.00	0.57	0.80
=1 if completed 12th pass	0.94	0.94	0.95	0.87	0.38	0.28	0.52
=1 if completed more than 12th pass	0.73	0.75	0.77	0.52	0.11	0.29	0.27
=1 if married	0.27	0.28	0.26	0.46	0.57	0.18	0.40
=1 if Hindu	0.92	0.94	0.94	0.08	0.19	0.72	0.19
=1 if ST/SC caste	0.38	0.34	0.35	0.03	0.13	0.59	0.09
=1 if OBC caste	0.29	0.34	0.35	0.01	0.01	0.73	0.01
=1 if general caste	0.33	0.32	0.30	0.59	0.18	0.37	0.39
Father's education>0	0.80	0.83	0.81	0.14	0.63	0.34	0.31
Mother's education>0	0.55	0.58	0.52	0.26	0.34	0.03	0.10
=1 if live in village	0.49	0.48	0.48	0.65	0.98	0.66	0.87
=1 access to Internet	0.76	0.80	0.80	0.02	0.05	0.85	0.05
=1 access Internet for jobs	0.49	0.52	0.55	0.16	0.01	0.19	0.04
=1 if registered with a job portal	0.23	0.23	0.28	0.92	0.03	0.03	0.04
=1 family is helpful for search	0.65	0.64	0.64	0.49	0.78	0.69	0.78
=1 friends are helpful for search	0.60	0.61	0.62	0.71	0.34	0.52	0.63
=1 if currently employed	0.30	0.34	0.32	0.07	0.51	0.26	0.18
=1 if looking for job	0.65	0.66	0.65	0.69	0.95	0.74	0.91
Hours search (winz, 0.01)	5.79	6.50	5.58	0.29	0.79	0.18	0.35
Reservation wage winzorized (rupees)	12285.89	12215.82	13153.77	0.77	0.01	0.00	0.01
Current wage winzorized 99 (rupees)	11206.91	11187.65	10792.47	0.92	0.50	0.52	0.75
= 1 if Telecom	0.38	0.38	0.38	.	.	.	.
= 1 if Logistics	0.36	0.36	0.36	.	.	.	.
= 1 if SalesMarketing	0.18	0.18	0.18	.	.	.	.
= 1 if Security	0.08	0.09	0.08	.	.	.	.

Panel B: Job Characteristics

	Mean
Salary Offered	10011.21
Requires 10th pass	0.68
Requires 12th pass	0.27
Requires Diploma/Undergraduate	0.05

Panel A presents summary statistics for graduates, which are calculated using the baseline survey. Columns 1-3 show mean values of each variable for the control group and treatment groups at baseline. Columns 4-6 tests whether these characteristics differ significantly across groups (controlling for geographic and trade strata). Column 7 presents a joint F-test of treatment orthogonality. Panel B presents job characteristics, as they were advertised in the SMSs that Job Shikari sent to the treatment groups.

A.1.2 External Validity

Table A.2: Comparing Jobs on Job Shikari to PFLS Sample

	PFLS Sample	PFLS Sub-Sample	Baseline Sample
	(1)	(2)	(3)
Salary	10852.43	8554.80	10011.21
Requires less 10th pass	0.54	0.62	
Requires 10th pass	0.18	0.18	0.68
Requires 12th pass	0.28	0.20	0.27
Trade == Data Entry Operator			0.45
Trade == Driver			0.01
Trade == Field Executive			0.28
Trade == Others			0.00
Trade == Receptionist			0.02
Trade == Retail/Cashier			0.01
Trade == Security Supervisor			0.01
Trade == Telecaller			0.20
Trade == Security Guard			0.03
Trade == Legislators, Senior Officials, and Managers	0.11	0.00	
Trade == Professionals	0.10	0.00	
Trade == Associate Professionals	0.09	0.00	
Trade == Clerks	0.05	0.14	
Trade == Service Workers and Shop and Market Sales Workers	0.20	0.50	
Trade == Skilled Agricultural and Fishery Workers	0.03	0.00	
Trade == Craft and Related Trades Workers	0.18	0.00	
Trade == Plant and Machine Operators and Assemblers	0.10	0.00	
Trade == Elementary Occupations	0.14	0.36	

We compare the set of jobs on Job Shikari to a nationally representative sample jobs held by respondents of the 2016 Periodic Labor Force Survey (PLFS). In Column 1, we restrict the PFLS to cover workers between the age groups of 18-35 years in urban jobs to match the eligibility of the Job-Shikari sample. In Column 2, we apply a further restriction to the PFLS sample by focusing on trades that match the Job-Shikari sample more closely (Clerks, Service Workers/Shops and Market Sales Workers, and Elementary Occupations. In Column 3, we present summary statistics for the set of jobs in our sample.

Table A.3: Comparing Baseline Sample of Respondents to NSS Sample

	NSS Sample	Baseline Sample
	(1)	(2)
Male	0.55	0.88
Age	25.37	24.15
Married	0.50	0.27
Educ (year)	12.29	14.22
HH (size)	5.50	
Hindu	0.75	0.93
Caste (General)	0.39	0.32
Caste (OBC)	0.38	0.33
Caste (SC)	0.11	0.35

This table compares our study sample of vocational training graduates to a nationally representative sample of the 68th Round of the National Sample Survey (NSS) conducted in 2011-2012. The NSS sample is constrained to individuals with at least high school level of education and between the age groups of 18-35 years of age to match the eligibility of the study sample. Column 1 presents summary statistics for the NSS sample, while column 2 presents summary statistics for our baseline sample.

A.1.3 Attrition

Table A.4: Attrition

	(1) Answered Midline	(2) Answered Midline	(3) Answered Endline	(4) Answered Endline
Treatment	0.017		-0.017	
	(0.017)		(0.021)	
Priority Treatment	0.024		0.018	
	(0.017)		(0.021)	
=1 if male		0.028**		0.000
		(0.012)		(0.002)
Age		-0.002**		0.000
		(0.001)		(0.000)
Education (Yrs)		-0.002		-0.000
		(0.002)		(0.000)
=1 if married		0.005		-0.002
		(0.010)		(0.002)
=1 if Hindu		0.020		-0.001
		(0.015)		(0.002)
=1 if ST/SC caste		-0.001		0.001
		(0.008)		(0.001)
=1 if live in village		-0.002		-0.001
		(0.008)		(0.001)
=1 if currently employed		0.012		-0.002
		(0.009)		(0.001)
=1 if looking for job		0.011		-0.001
		(0.008)		(0.001)
=1 access to Internet		-0.000		-0.000
		(0.011)		(0.002)
=1 access Internet for jobs		0.003		-0.001
		(0.009)		(0.002)
=1 if registered with a job portal		0.004		0.002
		(0.009)		(0.002)
Log(RW)		-0.015*		0.001
		(0.008)		(0.001)
Number of Individuals	2662	1696	2662	1696

We investigate differential attrition at midline (column 1) and endline (column 3) by regressing an indicator for responding to our survey on an indicator for being in the treatment and priority treatment groups. We also investigate how attrition varies by baseline characteristics at midline (column 2) and endline (column 4). Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.4 Employment by Baseline Expected Wage

Table A.5: Employment by Baseline Expected Wage

	(1) Employed (Low Wage)	(2) Employed (High Wage)	(3) Employed (All)
Treatment	-0.071**	-0.108***	-0.071**
	(0.031)	(0.030)	(0.031)
Priority Treatment	0.039	0.055*	0.039
	(0.029)	(0.029)	(0.029)
Treatment X High Exp. Wage			-0.037
			(0.043)
Priority Treatment X High Exp. Wage			0.016
			(0.041)
Mean in Control	0.19	0.36	0.29
Respondent Fixed Effects	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes
Number of Individuals	961	1347	2308
Number of Observations	2867	3645	6512

This table shows how the impact of treatment and priority treatment on employment vary based on baseline expected wages. We use baseline observables (living in a village, gender, age, marital status, religion, caste) to predict baseline wages. We then define high (low) predicted baseline expected wages as wages above (below) the median. Column 1 restricts the sample to graduates with below median predicted baseline wages. Column 2 restricts the sample to graduates with above median predicted baseline wages. Columns 3 considers the full sample and interacts treatment and priority treatment with an indicator for above median predicted wages at baseline. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.5 Outcomes at Midline and Endline

Table A.6: Treatment Effects by Survey Round

	(1) Employed	(2) Log(RW)	(3) In-City	(4) Search	(5) Log(Hours)	(6) Log(Apps)
Treatment X Midline	-0.087***	0.035	-0.010	0.004	-0.097	-0.125
	(0.023)	(0.026)	(0.028)	(0.030)	(0.151)	(0.094)
Treatment X Endline	-0.098***	0.027	-0.033	0.007	0.109	0.089
	(0.027)	(0.028)	(0.028)	(0.032)	(0.156)	(0.104)
Priority Treatment X Midline	0.054**	-0.043	0.047*	-0.010	0.070	0.113
	(0.022)	(0.029)	(0.027)	(0.030)	(0.140)	(0.097)
Priority Treatment X Endline	0.041	-0.061**	0.076***	0.019	-0.170	0.158
	(0.026)	(0.030)	(0.027)	(0.031)	(0.148)	(0.106)
Mean in Control	0.30	9.29	0.51	0.65	2.06	1.32
Treat_Mid = Treat_End	0.66	0.77	0.44	0.91	0.13	0.02
PTreat_Mid = PTreat_End	0.58	0.57	0.35	0.36	0.08	0.62
Respondent Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Individuals	2662	2622	2662	2661	1743	1815
Number of Observations	6866	6489	6889	6828	2986	3333

This table presents the impact of treatment and priority treatment at midline and endline. The dependent variables are an indicator for being employed (column 1), log reservation wages (column 2), whether the respondent lives in a city (column 3), whether the respondent is looking for work (column 4), the number of hours spent searching in the past week across all platforms (column 5), and the number of applications made in the last 3 months across all platforms (column 6). We test whether the coefficients on treatment and priority treatment are statistically different at midline and endline. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.6 Spillovers

Table A.7: Spillovers to control group

	(1) Employed	(2) Log(RW)
High Share of Sample in Treatment (per NSDC)	0.029	0.021
	(0.037)	(0.036)
High Share of Sample in Priority Treated (per NSDC)	-0.019	0.012
	(0.035)	(0.036)
Respondent Fixed Effects	Yes	Yes
Survey Round Fixed Effects	Yes	Yes
Number of Individuals	799	780
Number of Observations	2056	1929

This table investigates the presence of spillovers. We focus on the control group exclusively. The dependent variables are an indicator for being employed (column 1), and log reservation wages (column 2). Our independent variables "High Share of Sample in Treatment/Priority Treated" are calculated by taking the share of the sample within the NSDC institute that is assigned to treatment and priority treatment, and creating an indicator if this share is above the median. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.7 Assessment of Job Portals

Table A.8: Assessment of Job Portals

	(1) Job Portals are helpful
Treatment	0.022
	(0.017)
Priority Treatment	-0.026
	(0.017)
Mean in Control	0.77
F-test T + PT	0.82
Survey Round Fixed Effects	Yes
Geo Round Fixed Effects	Yes
Trade Round Fixed Effects	Yes
Number of Individuals	2217
Number of Observations	3769

We ask respondents whether websites which have information about jobs can be/or have been helpful in searching for employment. We only collected this variable at midline and endline, which means we cannot include individual fixed effects in this specification. We include our strata fixed effects (geography and trade), as well as survey round fixed effects. We test whether the priority treatment group is significantly different from the control group by testing whether $T + PT = 0$. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.8 Employment and Beliefs by Baseline Reservation Wage

Table A.9: Employment and Beliefs by Baseline Reservation Wage

	Employed			Reservation Wage		
	(1) (Low RW)	(2) (High RW)	(3) Full	(4) (Low RW)	(5) (High RW)	(6) Full
Treatment	-0.107***	-0.058*	-0.107***	0.045	-0.007	0.045
	(0.029)	(0.035)	(0.029)	(0.028)	(0.036)	(0.028)
Priority Treatment	0.063**	0.033	0.063**	-0.053*	-0.009	-0.053*
	(0.027)	(0.036)	(0.027)	(0.031)	(0.034)	(0.031)
Treatment X High Res Wage			0.049			-0.052
			(0.045)			(0.046)
Priority Treatment X High Res Wage			-0.029			0.043
			(0.045)			(0.047)
Mean in Control	0.23	0.44	0.30	9.01	9.82	9.28
Respondent Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Individuals	1399	731	2130	1376	715	2091
Number of Observations	3965	2053	6018	3862	1985	5847

This table shows how the impact of treatment and priority treatment vary based on baseline reservation wages. The dependent variable in columns 1, 2 and 3 is an indicator for being employed, while the dependent variable in columns 4, 5 and 6 is log reservation wages. Column 1 and 4 restricts the sample to graduates with below median reservation wages at baseline. Column 2 and 5 restricts the sample to graduates with above median reservation wages. Columns 3 and 6 consider the full sample and interact treatment and priority treatment with an indicator for above median reservation wages at baseline. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.9 Balance (Always Searchers)

Table A.10: Graduates' characteristics (for Always-Searchers)

	Panel A: Job-seeker Characteristics						
	(1) Control	(2) Treatment	(3) Priority Treatment	(4) (1) vs. (2), p-value	(5) (1) vs. (3), p-value	(6) (2) vs. (3), p-value	(7) Joint F-test
=1 if male	0.90	0.93	0.89	0.12	0.90	0.09	0.16
Age	23.74	23.49	23.73	0.41	0.91	0.48	0.66
Education (Yrs)	14.21	14.32	14.40	0.56	0.34	0.66	0.64
=1 if completed 10th pass	0.99	1.00	0.99	0.49	0.99	0.47	0.70
=1 if completed 12th pass	0.95	0.95	0.95	0.94	0.91	0.97	0.99
=1 if completed more than 12th pass	0.76	0.76	0.80	0.86	0.27	0.31	0.48
=1 if married	0.26	0.27	0.25	0.71	0.74	0.47	0.77
=1 if Hindu	0.92	0.96	0.97	0.09	0.04	0.59	0.09
=1 if ST/SC caste	0.40	0.40	0.36	0.95	0.27	0.21	0.40
=1 if OBC caste	0.30	0.35	0.36	0.16	0.17	0.95	0.29
=1 if general caste	0.30	0.25	0.29	0.11	0.76	0.21	0.23
Father's education>0	0.80	0.77	0.82	0.46	0.59	0.19	0.41
Mother's education>0	0.56	0.55	0.57	0.79	0.95	0.74	0.94
=1 if live in village	0.49	0.52	0.52	0.32	0.28	0.88	0.50
=1 access to Internet	0.78	0.83	0.82	0.15	0.34	0.67	0.34
=1 access Internet for jobs	0.57	0.61	0.65	0.38	0.12	0.42	0.29
=1 if registered with a job portal	0.27	0.25	0.32	0.53	0.17	0.03	0.10
=1 family is helpful for search	0.68	0.65	0.65	0.45	0.44	0.93	0.68
=1 friends are helpful for search	0.63	0.63	0.62	0.91	0.74	0.80	0.94
=1 if currently employed	0.28	0.30	0.27	0.62	0.70	0.36	0.65
=1 if looking for job	1.00	1.00	1.00	.	.	.	.
Hours search (winz, 0.01)	9.92	10.65	8.77	0.61	0.45	0.19	0.42
Reservation wage winzorized (rupees)	11707.80	11216.59	12113.83	0.28	0.31	0.03	0.09
Current wage winzorized 99 (rupees)	11018.91	9691.51	8448.70	0.31	0.04	0.19	0.11
= 1 if Telecom	0.32	0.35	0.34	.	.	.	.
= 1 if Logistics	0.39	0.40	0.42	.	.	.	.
= 1 if SalesMarketing	0.18	0.17	0.13	.	.	.	.
= 1 if Security	0.11	0.08	0.10	.	.	.	.

This table presents summary statistics for the sample of job seekers who are searching throughout the study period, which are calculated using the baseline survey. Columns 1-3 show mean values of each variable for the control group and treatment groups at baseline. Columns 4-6 tests whether these characteristics differ significantly across groups (controlling for geographic and trade strata). Column 7 presents a joint F-test of treatment orthogonality.

A.1.10 Outcomes for Always Searchers

Table A.11: Employment and Beliefs (Always Searchers)

	Panel A: Employment	
	(1)	(2)
	Employed	Log(Wage)
Treatment	-0.048	0.007
	(0.036)	(0.079)
Priority Treatment	0.049	0.122
	(0.033)	(0.171)
Mean in Control	0.28	9.04
F-test T + PT	0.96	0.46
Respondent Fixed Effects	Yes	Yes
Survey Round Fixed Effects	Yes	Yes
Number of Individuals	1070	472
Number of Observations	2665	795

	Panel B: Beliefs			
	(1)	(2)	(3)	(4)
	Log(Reservation Wage)	Prob10	Prob16	Prob20
Treatment	0.077**	0.128	-0.061	0.114
	(0.037)	(0.333)	(0.288)	(0.272)
Priority Treatment	-0.080**	-0.376	-0.421	-0.353
	(0.037)	(0.333)	(0.288)	(0.265)
Mean in Control	9.3	4.7	2.9	2.0
F-test T + PT	0.94	0.50	0.12	0.40
Respondent Fixed Effects	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes
Number of Individuals	1058	1031	1017	1015
Number of Observations	2551	2471	2408	2386

Panel A illustrates the impact of the treatments on employment outcomes for graduates who are always searching. The dependent variables are an indicator for whether the respondent is employed (column 1), and the logarithm of wages winzorised at the 1% (column 2). Columns 1 includes all respondents in the sample while column 2 only include respondents who were employed at the time of survey. Panel B illustrates the impact of the treatments on beliefs for graduates who are always searching. Column 1 presents the logarithm of respondents reservation wages (where we asked respondents “What is the minimum salary that you would accept if you are looking for a job in your field/sector and location”). Column 2-4 shows the probability that respondents think they will get a job that pays 10,000, 16,000 and 20,000 rupees respectively (where we ask “On a scale of 1-10, how likely is it that you will be offered a job that pays at least 10,000 in your location next month?”). Only 5% of job-seekers respond inconsistently with probabilities that weakly increase as the wage increases. We test whether the priority treatment group is significantly different from the control group by testing whether $T + PT = 0$. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.11 Intensive Margin Effects

Table A.12: Employment and Beliefs (IV specification)

	(1) Employed	(2) Log(Wage)	(3) Log(RW)
=1 if received SMS	-0.290***	-0.058	0.107
	(0.069)	(0.228)	(0.075)
Number of SMSes sent	0.008***	0.005	-0.005**
(panel)	(0.002)	(0.008)	(0.002)
Mean in Control	0.30	9.08	9.29
Respondent Fixed Effects	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes
Number of Individuals	2294	639	2197
Number of Observations	6468	1686	6054

This table estimates the impact of information received by the portal, instrumenting whether graduates receive any SMS, and number of SMS they receive, with indicators for being assigned to Treatment and Priority Treatment, respectively (where Treatment is a dummy equal to 1 if the job seeker was assigned to either of our treatments). Column 1 focuses on the impact on employment, and column 2 focuses on reservation wages. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A.13: Employment and Beliefs (by number of SMS received)

	(1) Emp	(2) Log(Wage)	(3) Log(RW)	(4) Emp	(5) Log(Wage)	(6) Log(RW)
Treatment	-0.088***	0.002	0.021	-0.082***	-0.008	0.009
	(0.022)	(0.069)	(0.025)	(0.020)	(0.065)	(0.022)
Number of SMS (few)	0.022	0.055	-0.002			
	(0.023)	(0.068)	(0.026)			
Number of SMS (many)	0.047*	-0.017	-0.047			
	(0.025)	(0.068)	(0.031)			
Number of Bad SMS				0.043	0.138*	0.019
(few)				(0.028)	(0.083)	(0.031)
Number of Bad SMS				0.111**	0.243	-0.046
(many)				(0.044)	(0.197)	(0.074)
Mean in Control	0.30	9.08	9.29	0.30	9.08	9.29
Respondent Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Individuals	2662	1254	2622	2662	1254	2622
Number of Observations	6836	2301	6479	6836	2301	6479

This table presents the impact of receiving a higher number of SMS offers, and fewer relevant SMS messages, on job-seekers. Column (1), (2) and (3) present the impact of receiving a higher number of SMS offers on the probability of employment, wages and reservation wages, respectively. Column (3), (4) and (5) present the impact of receiving a higher number of *bad* SMS offers on the probability of employment, wages, and reservation wages, respectively. The number of SMS is the total count of messages received by job-seekers until their survey participation. The number of bad SMS is measured as the number of NON-LOCAL SMS X (1-group offer rate), where the group offer rate calculates the percentage of offers that were higher than the baseline wages for different wage bins, weighting by the number of job-seekers in that geographic/trade strata with baseline wages in that bin. Number of SMS (few) and Number of SMS (many) are indicators for having more between 0-5 and more than 5 SMS respectively (the omitted group is an indicator for having 0 SMS). Number of Bad SMS (few) and Number of Bad SMS (many) are indicators for having between 0 and 5 bad SMS, and more than 5 bad SMS, respectively (the omitted group is an indicator for having 0 bad SMS). Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.12 Job Search

Table A.14: Job Search

	All			Searchers	
	(1)	(2)	(3)	(4)	(5)
	Search	Log(Hours)	Log(Apps)	Log(Hours)	Log(Apps)
Treatment	0.005	-0.003	-0.037	0.015	-0.153
	(0.027)	(0.138)	(0.088)	(0.151)	(0.112)
Priority Treatment	0.003	-0.042	0.131	0.003	0.133
	(0.026)	(0.127)	(0.090)	(0.139)	(0.116)
Mean in Control	0.65	2.06	1.32	2.06	1.31
F-test T + PT	0.78	0.74	0.33	0.90	0.87
Respondent Fixed Effects	Yes	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of Individuals	2661	1743	1815	932	893
Number of Observations	6828	2986	3333	1908	1768

This table shows how treatment affects different measures of job search. The dependent variables are an indicator for whether the respondent is actively searching for employment (column 1), the log number of hours spent searching in the past week across all platforms (winzorised at the 1%)- where people who are not searching are assigned a value of 0 hours (column 2/4), the log number of job applications submitted in the last 3 months across all platforms (winzorised at the 1%) (column 3/5). Columns 1, 2 and 3 include all respondents in the sample at baseline, while columns 4 and 5 include all respondents who report consistently searching for work throughout the study. We test whether the priority treatment group is significantly different from the control group by testing whether $T + PT = 0$. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.13 Balance (by Geo-Zones)

Table A.15: Graduates' Characteristics (by Geographic Zone)

	(1) DelhiNCR	(2) North	(3) SouthWest	(4) East	(5) (1) vs. (2), p-value	(6) (1) vs. (3), p-value	(7) (1) vs. (4), p-value
=1 if male	0.81	0.89	0.87	0.90	0.00	0.01	0.00
Age	23.33	22.97	26.95	23.69	0.30	0.00	0.32
Education (Yrs)	14.78	14.28	14.54	13.70	0.00	0.10	0.00
=1 if completed 10th pass	1.00	0.99	0.99	0.97	0.23	0.40	0.00
=1 if completed 12th pass	0.98	0.95	0.95	0.90	0.02	0.06	0.00
=1 if completed more than 12th pass	0.81	0.76	0.76	0.70	0.08	0.10	0.00
=1 if married	0.17	0.21	0.41	0.28	0.15	0.00	0.00
=1 if Hindu	0.86	0.91	0.95	0.97	0.01	0.00	0.00
=1 if ST/SC caste	0.16	0.27	0.27	0.59	0.00	0.00	0.00
=1 if OBC caste	0.20	0.37	0.45	0.24	0.00	0.00	0.12
=1 if general caste	0.64	0.36	0.27	0.17	0.00	0.00	0.00
Father's education>0	0.89	0.80	0.82	0.79	0.00	0.03	0.00
Mother's education>0	0.77	0.53	0.61	0.46	0.00	0.00	0.00
=1 if live in village	0.06	0.46	0.46	0.69	0.00	0.00	0.00
=1 access to Internet	0.96	0.83	0.79	0.66	0.00	0.00	0.00
=1 access Internet for jobs	0.61	0.54	0.55	0.43	0.03	0.08	0.00
=1 if registered with a job portal	0.49	0.25	0.22	0.17	0.00	0.00	0.00
=1 family is helpful for search	0.62	0.66	0.61	0.66	0.17	0.85	0.19
=1 friends are helpful for search	0.54	0.63	0.62	0.61	0.01	0.02	0.05
=1 if currently employed	0.45	0.32	0.48	0.16	0.00	0.38	0.00
=1 if looking for job	0.54	0.64	0.65	0.72	0.00	0.00	0.00
Hours search (winz, 0.01)	3.20	6.17	5.99	7.09	0.00	0.00	0.00
Reservation wage winzorized (rupees)	17321.81	12278.82	12055.50	11019.78	0.00	0.00	0.00
Current wage winzorized 99 (rupees)	16591.01	11752.19	8337.65	9202.25	0.00	0.00	0.00

This table presents summary statistics for graduates across geographic zones, which are calculated using the baseline survey. Columns 1-3 show mean values of each variable across geographic zones. Columns 4-6 tests whether these characteristics differ significantly across geography.

A.1.14 Controlling for zero SMS Received

Table A.16: Employment, Wages and Reservation Wages by Geographic Zone (Controlling for zero SMS)

	(1) Employed	(2) Log(Wage)	(3) Log(RW)	(4) In-City
Treatment East	-0.051	0.191	0.073	0.002
	(0.037)	(0.243)	(0.050)	(0.044)
Treatment DelhiNCR	-0.112*	-0.215	0.033	0.046
	(0.059)	(0.139)	(0.057)	(0.032)
Treatment North	-0.086**	0.001	0.033	-0.038
	(0.039)	(0.126)	(0.037)	(0.043)
Treatment SouthWest	-0.144***	-0.005	-0.023	-0.061
	(0.046)	(0.087)	(0.050)	(0.049)
Priority Treatment East	0.084**	0.268	-0.042	0.032
	(0.039)	(0.193)	(0.058)	(0.049)
Priority Treatment DelhiNCR	-0.013	-0.007	-0.128*	0.014
	(0.055)	(0.069)	(0.077)	(0.033)
Priority Treatment North	0.011	0.042	-0.051	0.113***
	(0.040)	(0.159)	(0.038)	(0.039)
Priority Treatment SouthWest	0.183***	0.053	-0.078	0.089
	(0.060)	(0.124)	(0.072)	(0.068)
Priority Treatment East, 0 SMS	-0.019	-0.267*	-0.046	-0.007
	(0.049)	(0.154)	(0.056)	(0.061)
Priority Treatment DelhiNCR, 0 SMS	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)
Priority Treatment North, 0 SMS	0.038	0.796*	0.084	0.049
	(0.068)	(0.451)	(0.081)	(0.089)
Priority Treatment SouthWest, 0 SMS	-0.132**	-0.109	0.087	-0.066
	(0.060)	(0.131)	(0.075)	(0.073)
Respondent Fixed Effects	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes
Geo-Specifc Time Trend	Yes	Yes	Yes	Yes
Number of Observations	6866	2311	6489	6889

This table validates whether the differential effects we see across geographic zones are plausibly driven by the intervention. We regress our outcomes of interest on indicators for being in treatment and priority treatment by geographic zone, as well as an indicator for whether the treatment priority job-seeker received 0 SMS ("TP0"). Column 1 focuses on employment, column 2 considers the log of wages, column 3 displays the impact on log reservation wages, and column 4 on whether the graduate lives in a city. If the treatment priority group did not receive any SMSs, we would expect them to look similar to the treatment group. As such, adding the estimates of being in the priority treatment group and being in the priority group treatment but receiving no SMS, should yield a coefficient close to zero. Note that for Delhi NCR, there are no job seekers with zero SMS. Standard errors are clustered at the respondent level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.1.15 Employment by Respondent Characteristic

Table A.17: Employment by Respondent Characteristic

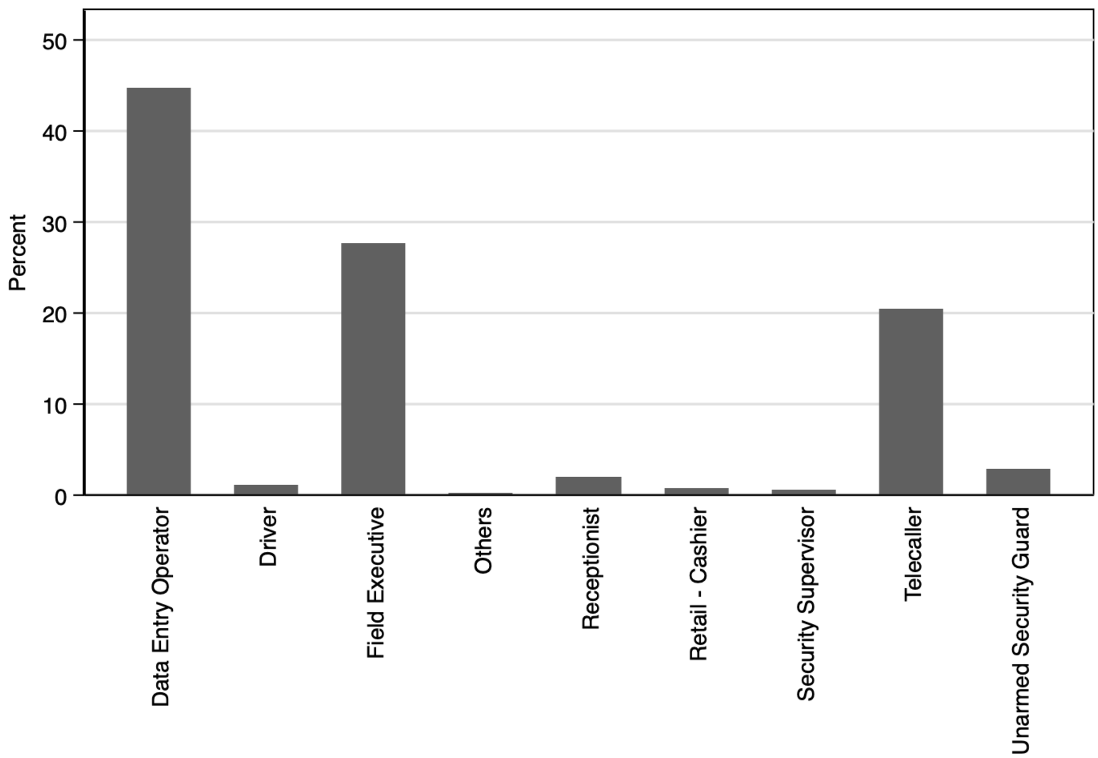
	(1) Emp	(2) Emp	(3) Emp	(4) Emp	(5) Emp
Treat	-0.075***	-0.075***	-0.108**	-0.106***	-0.075***
	(0.026)	(0.026)	(0.043)	(0.032)	(0.026)
Priority Treat	0.020	0.030	0.040	0.028	0.058**
	(0.025)	(0.024)	(0.043)	(0.030)	(0.024)
Treat X Old	-0.052				
	(0.047)				
Priority Treat X Old	0.084*				
	(0.044)				
Treat X Mar		-0.059			
		(0.048)			
Priority Treat X Mar		0.065			
		(0.047)			
Treat X Ed			0.023		
			(0.050)		
Priority Treat X Ed			0.008		
			(0.049)		
Treat X Vil				0.027	
				(0.043)	
Priority Treat X Vil				0.040	
				(0.041)	
Treat X Gen					-0.051
					(0.047)
Priority Treat X Gen					-0.052
					(0.047)
Mean in Control	0.29	0.29	0.29	0.29	0.29
Respondent Fixed Effects	Yes	Yes	Yes	Yes	Yes
Survey Round Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of Individuals	2307	2305	2297	2308	2271
Number of Observations	6510	6504	6480	6512	6407

This table shows how the impact of treatment and priority varies with different graduates' characteristics. The dependent variable across all columns is an indicator for being employed. The specification in column 1 interacts treatment and priority treatment with an indicator for being above the median age in our sample ("Old"). The specification in column 2 interacts treatment and priority treatment with an indicator being married ("Mar"). The specification in column 3 interacts treatment and priority treatment with an indicator for being above the median education in our sample ("Ed"). The specification in column 4 interacts treatment and priority treatment with an indicator being in a village ("Vil"). Finally the specification in column 5 interacts treatment and priority treatment with an indicator for being general caste ("Gen"). Standard errors are clustered at the respondent level. Older job seekers react more strongly to priority treatment. None of the estimates on columns 2-5 are statistically significant, though it appears that married job seekers (who may have dependents) are more likely to respond to priority treatment status. Similarly, lower caste job seekers and those based in villages respond more strongly to priority treatment. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

A.2 Figures

A.2.1 Job Types that Job Shikari sent via SMS

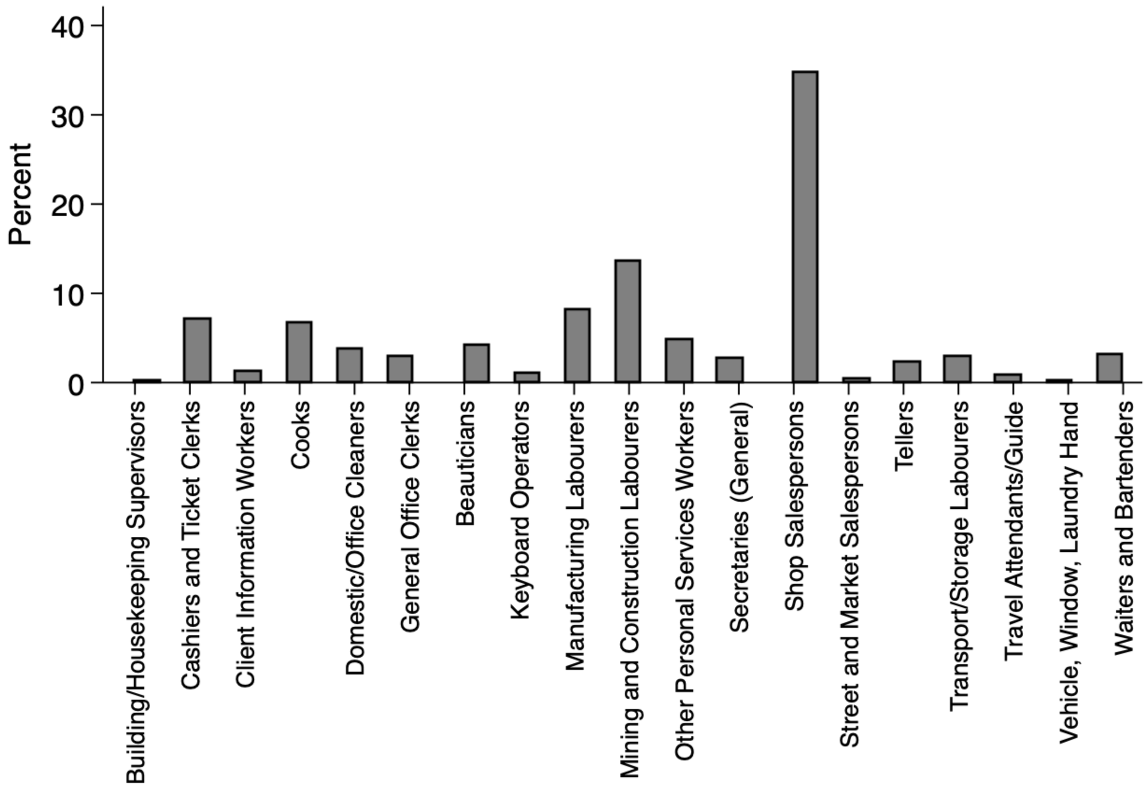
Figure A1: Job Types that Job Shikari sent via SMS



This figure represents the trades that were advertised in the SMSs that Job Shikari sent to our sample from August 2015 to September 2016.

A.2.2 External Validity

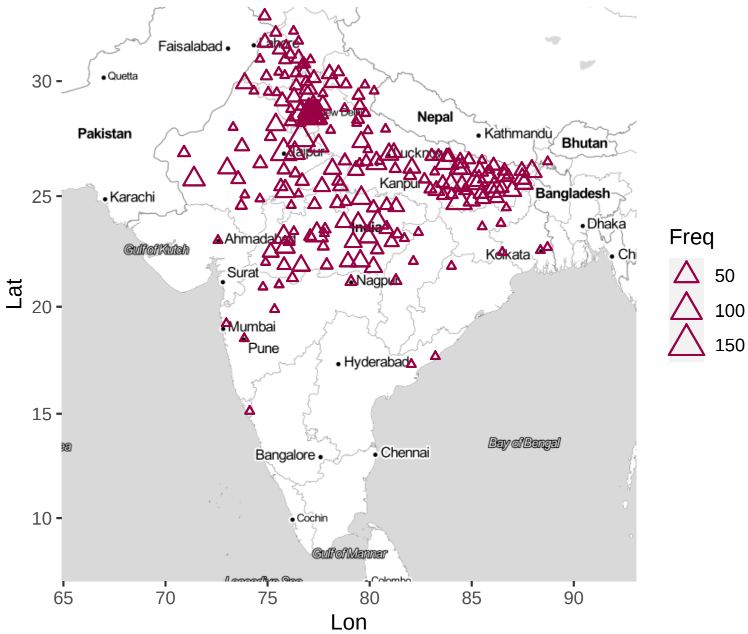
Figure A2: Occupations in the PFLS Sample



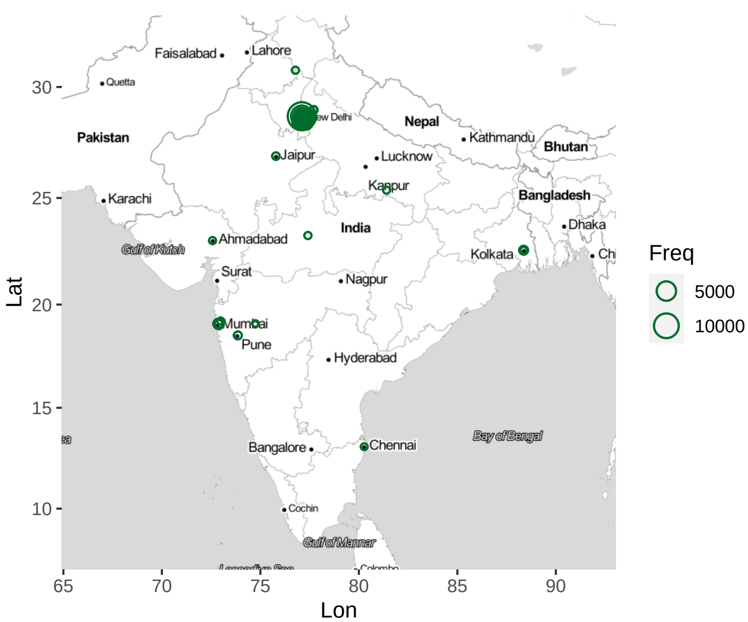
This figure represents the set of occupations that are held by respondents of the 2016 Periodic Labor Force Survey (PLFS), focusing on Clerks, Service Workers/Shops and Market Sales Workers, and Elementary Occupations exclusively.

A.2.3 Location of Graduates and Jobs across India

Figure A3: Location of Graduates and Jobs across India



(a) Graduates



(b) Jobs

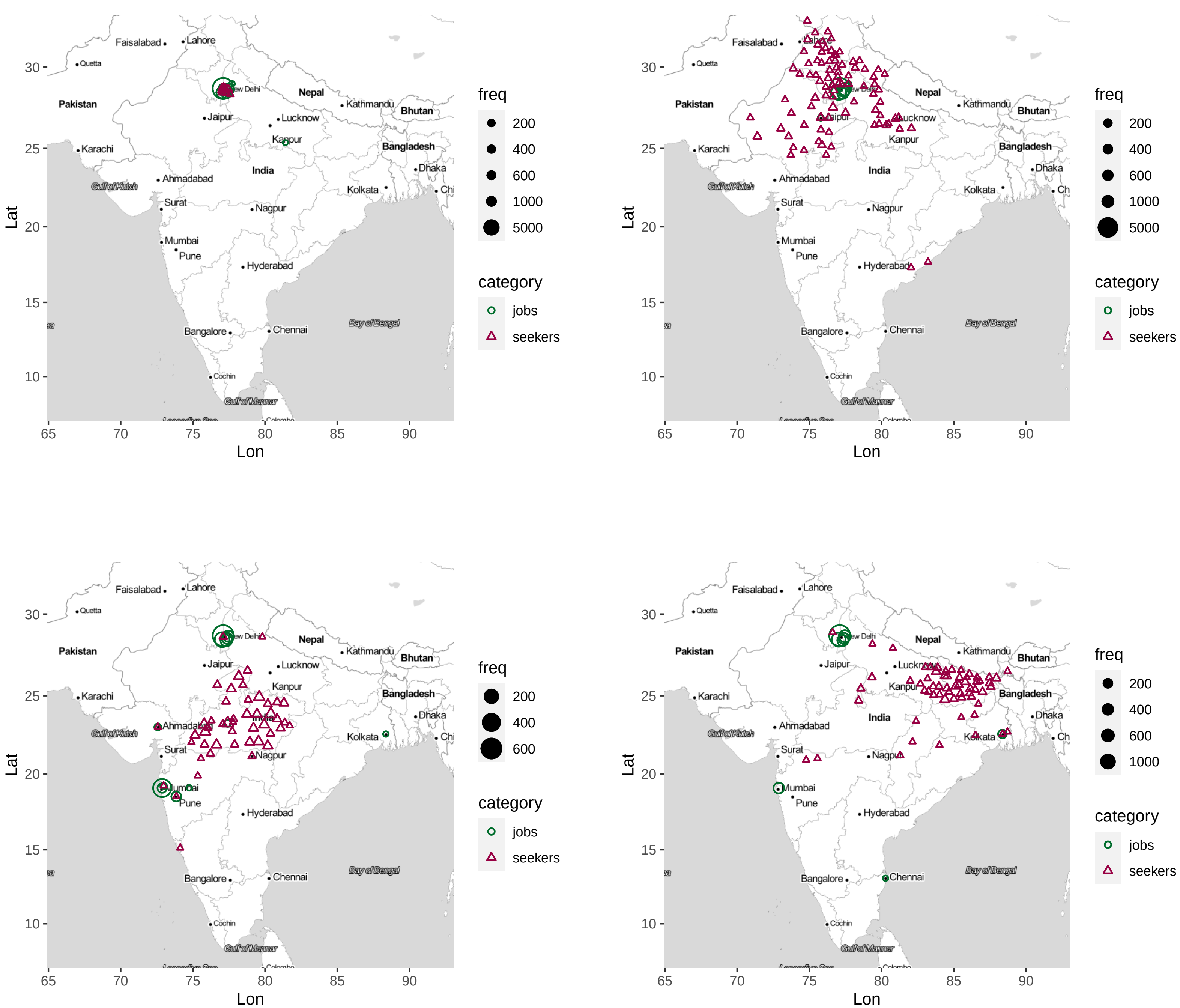
This figure presents the location of graduates (Panel A) at baseline, and the location of the jobs that Job Shikari sent to our sample from August 2015 to September 2016 (Panel B).

A.2.4 Locations of Graduates and Job offers by Geographic Zone

Figure A4: Locations of Graduates and Job offers by Geographic Zone

(a) DelhiNCR

(b) North



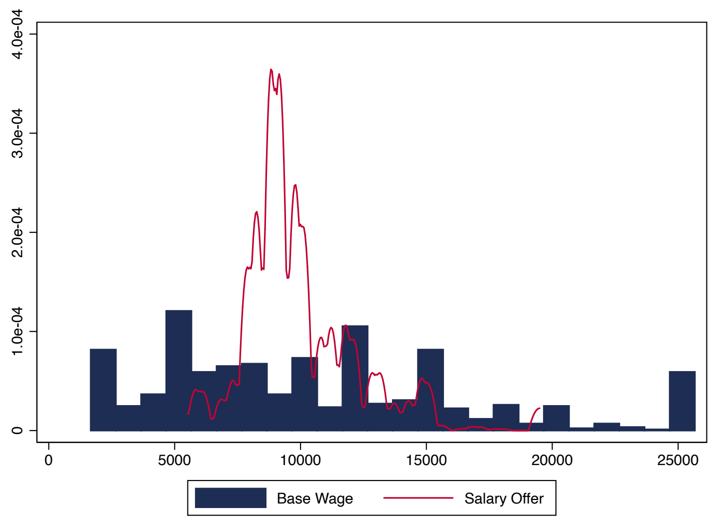
(c) South West

(d) East

This figure presents the location of graduates at baseline broken out by geographic zone at baseline, and the location of the jobs that Job Shikari sent to our sample in those respective geographic zones from August 2015 to September 2016. Panel A focuses on Delhi NCR, Panel B focuses on the North, Panel C focuses on the South West, and Panel D focuses on the East.

A.2.5 Distribution of Baseline Wages and Salary Offers

Figure A5: Distribution of Baseline Wages and Salary Offers

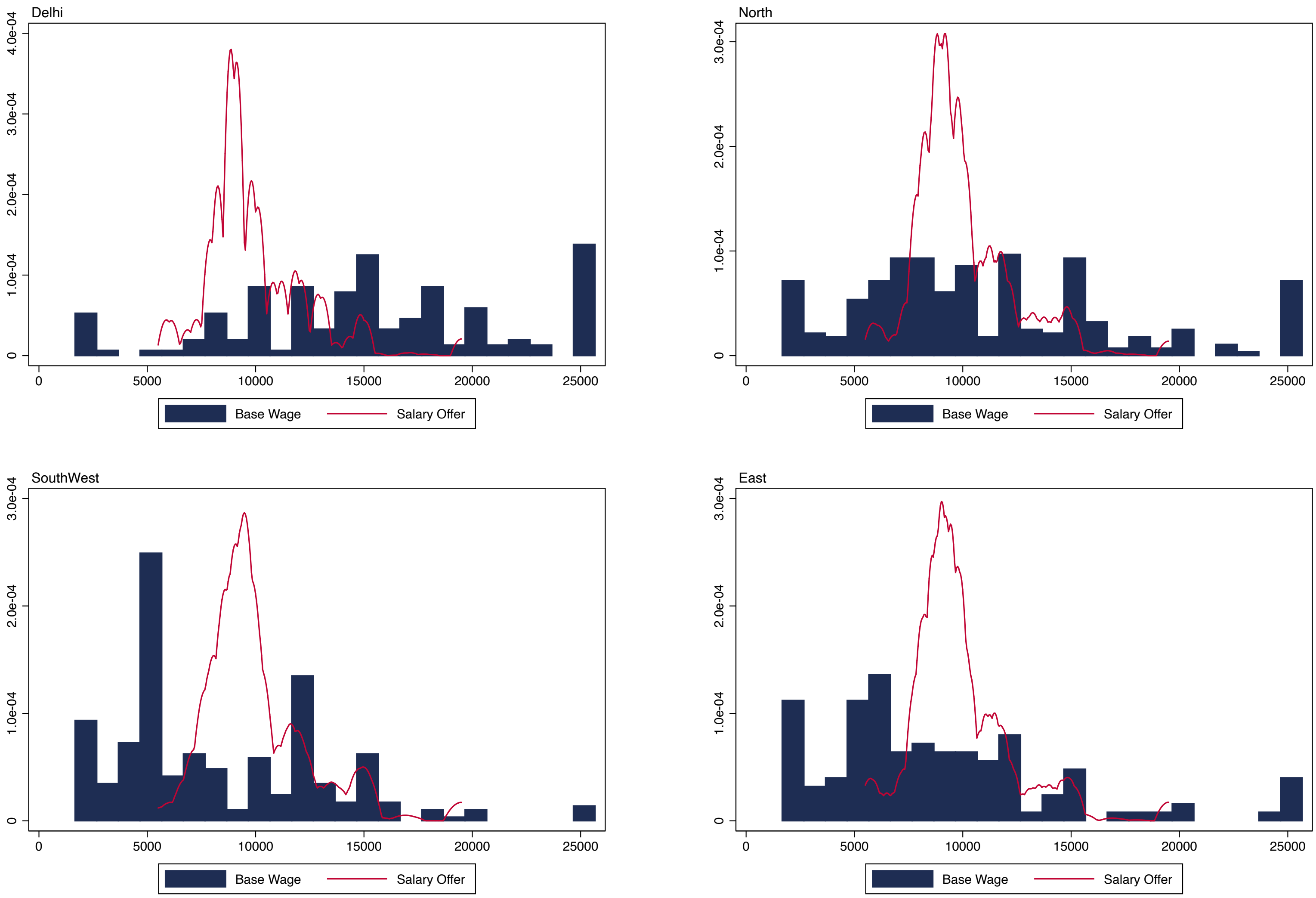


This histogram reflects the distribution of wages that employed graduates in our sample report at baseline. The k-density plot displays the set of salary offers that were advertised in the SMSs that Job Shikari sent to our sample from August 2015 to September 2016.

A.2.6 Distribution of Baseline Wages and Salary Offers by Geographic Zone

Figure A6: Distribution of Baseline Wages and Salary Offers by Geographic Zone

(a) DelhiNCR (b) North



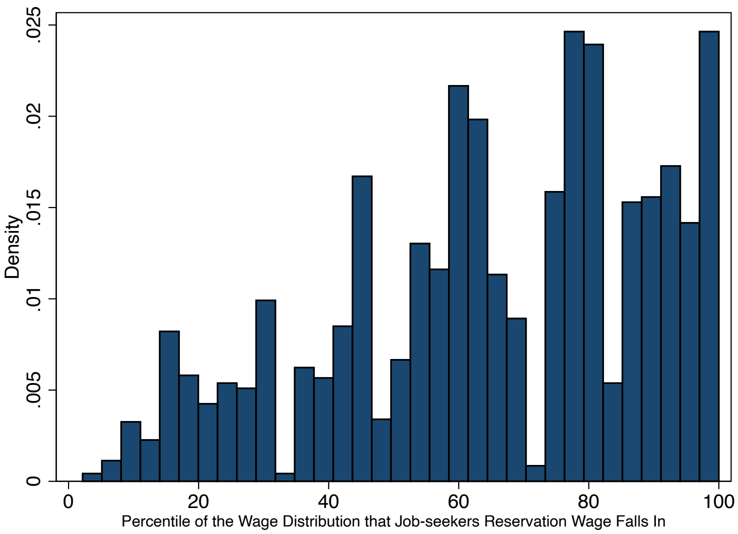
(c) South West (d) East

This histogram reflects the distribution of wages that employed graduates in our sample report at baseline, where each panel focuses on a different geographic zone (Delhi NCR in panel A, North in panel B, South West in panel C and East in panel D). The k-density plot displays the set of salary offers that were advertised in the SMSs that Job Shikari sent to our sample in that geographic zone from August 2015 to September 2016.

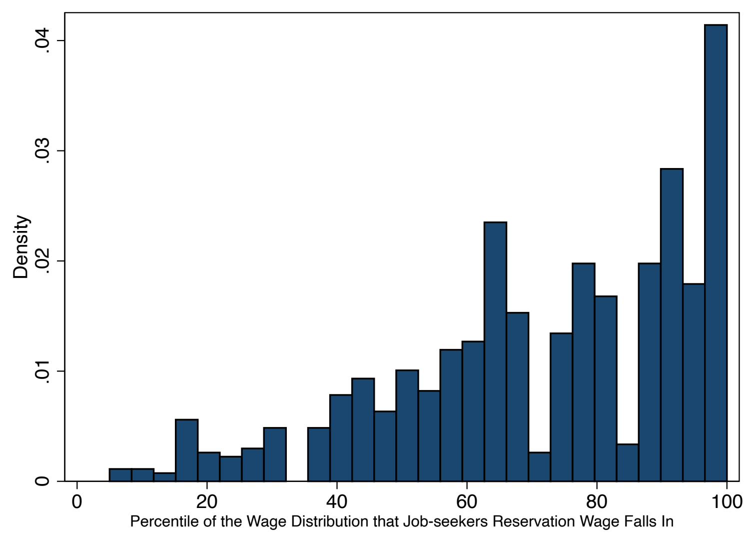
A.2.7 Reported Reservation Wages are High

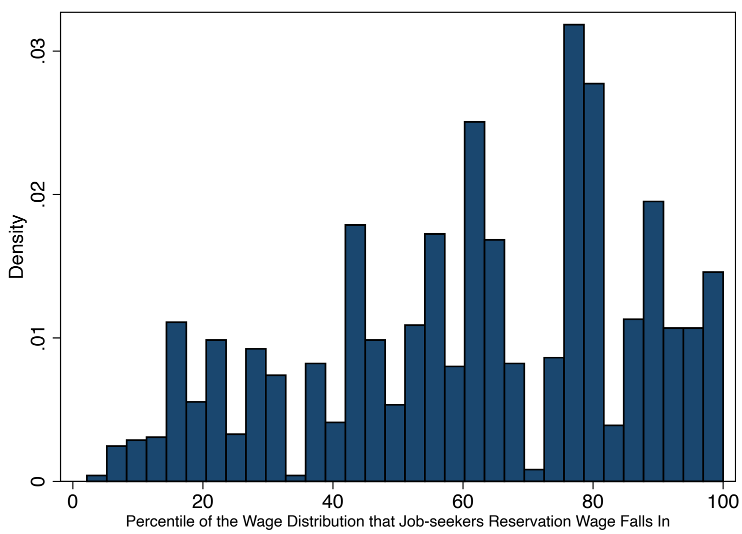
Figure A7: Reported Reservation Wages are High

(a) All



(b) Employed



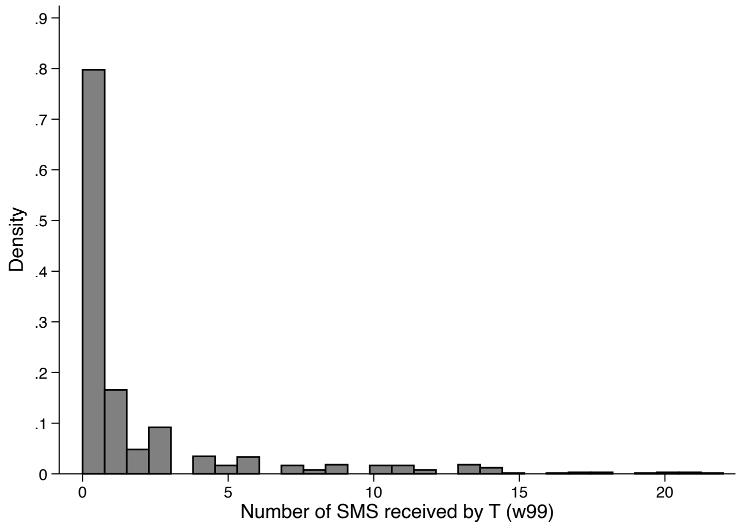


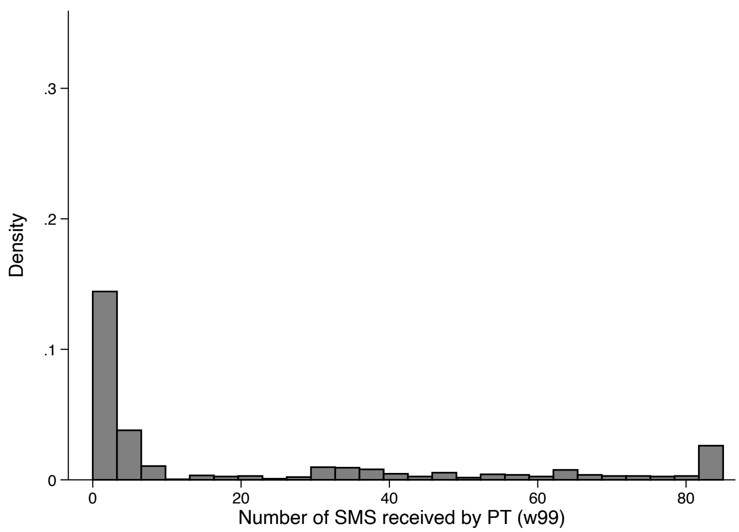
(c) Unemployed

This figure computes where graduates baseline stated reservation wages lie in the distribution of actual wages that employed graduates from the same geo-zone and trade report at baseline. This x-axis reflects which percentile of the wage distribution at baseline a graduates' baseline stated reservation wages falls. The y-axis is a density function, representing the likelihood that graduates reservation wages fall in that percentile of the wage distribution. Panel a) presents this distribution for the full sample, panel b) presents the distribution for the set of employed graduates at baseline, and panel c) presents the distribution for the set of unemployed graduates at baseline.

A.2.8 Distribution of SMS received by Respondents

Figure A8: Distribution of SMS received by Respondents

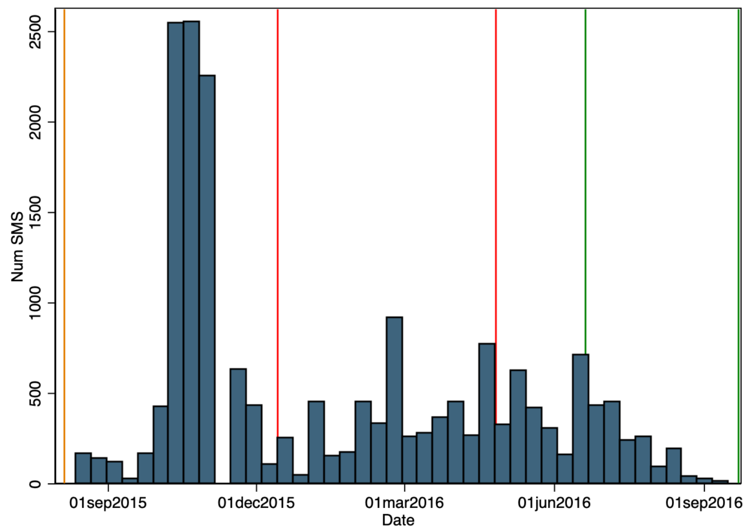




This figure presents the number of SMS that graduates received in the treatment (Panel A) and priority treatment (Panel B) groups. We winsorize the number of SMS at the 99th percentile.

A.2.9 Timeline of SMS sent by Job Shikari

Figure A9: Timeline of SMS sent by Job Shikari



This figure represents a timeline of when SMSs were sent by Job Shikari to the graduates in our sample. The orange line represents the end of baseline, the red lines represent the start and end dates for mildine (since we could not midline everyone on the exact same date), and the green lines represent the start and end dates for endline (since we could not endline everyone on the exact same date).

B Appendix for Online Publication Only - Model

B.1 Status Quo

In period T the worker armed with wage offer w solves:

$$V_T(w) = \max_{accept,reject} [u(w_T), 0]$$

The worker will accept the wage offer this period if:

$$u(w_T) > 0 \tag{1}$$

The worker will accept any wage offer since wage offers are always greater than zero.

In period T-1, the worker solves:

$$V_{T-1}(w) = \max_{accept,reject} [u(w) + \beta V_T(w), \beta E[V_T(w')]]$$

The worker will accept the wage offer this period if

$$\begin{aligned}
u(w_{T-1}) + \beta u(w_{T-1}) &> \beta E[u(w')] \\
(1+\beta)u(w_{T-1}) &> \beta E[u(w')] \\
u(w_{T-1}) &> \frac{\beta}{1+\beta} E[u(w')] \\
u(w_{T-1}) &> \frac{\beta}{1+\beta} \int u(w') f(u(w')) dw' \qquad (2)
\end{aligned}$$

Based on the utility function, the discount rate, and the density of to wage offers $f(\cdot)$, this implicity defines the reservation wage $\bar{w}_{T-1}$; clearly some wages will not be acceptable at time $T-1$ that were acceptable at time t.

Next, we demonstrate that reservation wages are declining in t. To see this, note that if the decision vector $(h_{T-k}(w), ..., h_T(w))$ is $(accept, accept, ..., accept)$ in time periods $(T-k, T-k+1, .., T)$ for a worker who receives offer w in any period $T-k$, then $V_{T-k}(w) = \frac{1-\beta^{k+1}}{1-\beta} u(w)$. Therefore, to demonstrate that reservation wages are declining in t we demonstrate that if w_{T-k} is acceptable in period $T-k$, it is acceptable forever, which means we can write $V_{T-k}(w_{T-k}) = \frac{1-\beta^{k+1}}{1-\beta} u(w_{T-k})$.

We prove this by induction on V.

First, consider period $T-1$, and an offer w_{T-1} where $h_{T-1}(w_{T-1}) = accept$ (the wage offer is accepted in period T-1). We know this same wage offer will be accepted in time T since all offers are accepted at time T. Therefore we can write

$$V_{T-1}(w_{T-1}) = u(w_{T-1}) + \beta u(w_{T-1}) = \frac{1-\beta^2}{1-\beta}u(w_{T-1}) \tag{3}$$

Next, consider period $T-k$, and assume $V_{T-k+1}(w_{T-k+1}) = \frac{1-\beta^k}{1-\beta}u(w_{T-k+1})\ \forall\ w_{T-k+1} > w^*_{T-k+1}$, where w^*_{T-k+1} is the reservation wage at time $T-k+1$. That is, that any acceptable wage offer at time $T-k+1$ would be accepted at all future wage offers. Then consider the decision to accept a wage offer w_{T-k+1} at time $T-k+1$:

$$V_{T-k+1}(w) = \max_{accept,reject}\left[u(w) + \beta V_{T-k+2}(w), \beta E\left[V_{T-k+2}(w')\right]\right]$$

Where by the definition of the reservation wage:

$$V_{T-k+1}(w) = \beta E[V_{T-k+2}(w')] = \frac{1-\beta^k}{1-\beta}u(w^*_{T-k+1}) \tag{4}$$

In **period T-k**, consider $w < w^*_{T-k+1}$, so that $h_{T-k+1}(w) = reject$. Then $h_{T-k}(w) = reject$ if:

$$\begin{aligned}
u(w) + \beta V_{T-k+1} &< \beta E[V_{T-K+1}(w')] \\
u(w) + \beta^2 E[V_{T-k+2}(w')] &< \beta E[V_{T-k+1}(w')] \qquad \text{by (4)} \\
\underbrace{u(w) + \beta\frac{1-\beta^k}{1-\beta}u(w^*_{T-k+1})}_{A} &< \underbrace{\beta E[V_{T-k+1}(w')]}_{B}
\end{aligned}$$

Since $w < w^*_{T-k+1}$

$$\begin{aligned}
\underbrace{u(w) + \beta\frac{1-\beta^k}{1-\beta}u(w^*_{T-k+1})}_{A} &< u(w^*_{T-k+1}) + \beta\frac{1-\beta^k}{1-\beta}u(w^*_{T-k+1}) \\
&< u(w^*_{T-k+1})\frac{1-\beta^{k+1}}{1-\beta} \\
&< u(w^*_{T-k+1})\frac{1-\beta^k}{1-\beta} + u(w^*_{T-k+1})\beta^k \\
&< \beta E[V_{T-k+2}(w')] + \beta^k u(w^*_{T-k+1}) \qquad \text{by (4)}
\end{aligned}$$

Thus, the searcher in time $T-k$ will reject all wage offers $w < w^*_{T-k+1}$ if

$$\beta E[V_{T-k+2}(w')] + \beta^k u(w^*_{T-k+1}) < \underbrace{\beta E[V_{T-k+1}(w')]}_{B}$$

$$E[V_{T-k+1}(w')] - E[V_{T-k+2}(w')] > \beta^{k-1} u(w^*_{T-k+1})$$

To evaluate this expression note two things.

First, the searcher could play strategy $h_{T-k+2}(w), h_{T-k+3}(w), ..., h_T(w)$ in periods $T-k+1, T-k+2, ..., T-1$ and $h_T(w) = accept$ in period T. If the searcher did that, because the wage offer distribution is stable she would receive in expectation $E[V_{T-k+2}(w')]$ in periods $T-k, ..., T-1$ plus an additional utility payment $\beta^{k-1} E_{t-k+1}[u(w_T)|h_{T-k+1}(w), h_{T-k+2}(w), ..., h_T(w), h_T(w)]$ in period T. Since $V_{T-k+1}(w)$ is an optimum, we know that

$$E[V_{T-k+1}(w)] > E[V_{T-k+2}(w)] + \beta^{k-1} E_{t-k+1}[u(w_T)|h_{T-k+1}(w), h_{T-k+2}(w), ..., h_T(w), h_T(w)]$$

Second, define ω_t as the received wage in time t, i.e. $\omega_t = 0$ if the wage offer is rejected at time t and $\omega_t = w$ for an accepted wage offer. Note that the time $t-k+1$ expectation of the $t-k+1+\tau$ expected received wage is increasing in τ.

$$E_{T-k+1}[u(\omega_{T-k+1+\tau})|h_{T-k+1}(w), h_{T-k+2}(w), ..., h_T(w)] = (1 - F(w^*_{T-k+1}))E[u(w)|w > w^*_{T-k+1}] + \sum_{d=1}^{\tau} \prod_{a=1}^{d} F(w^*_{T-k+a})(1 - F(w^*_{T-k+1+d}))E[u(w)|w > w^*_{T-k+1+d}]$$

Since τ only adds positive numbers to this expectation, it is clearly increasing in τ. Thus, from the perspective of time $t-k$, the searcher expects to receive higher wages in more distant future periods τ: intuitively, each period further in the future allows more chances at a high wage accepted offer. So your expected value of a new wage draw, which is just a weighted sum of expected wages in future periods, will have the property where the expected wage in each future period is higher the more distant that period is. This means that the expected wage in period T is going to be larger than the average of the expected wages. Now, we also know that the expected value of a new wage draw is equal to the weighted sum of utility at the reservation wage in T-k+1 (the average of the expected wages), by definition of the reservation wage:

$$\beta E[V_{T-k+2}(w')] = \beta \sum_{\tau=0}^{T-k+2} \beta^\tau E[u(\omega_{T-k+2+\tau})|h_{T-k+2}, ..., h_T] = \frac{1-\beta^{T-k+1}}{1-\beta} u(w^*_{T-k+1})$$

Thus, we can be guaranteed that the average expected wage from following the $T-k+2,...,T$ strategies is w^*_{T-k+1} and that $E_{T-k+1}[u(\omega_T)|h_{T-k+2,...,h_T,h_T}] > u(w^*_{T-k+1})$. Thus

$$E[V_{T-k+1}(w')] - E[V_{T-k+2}(w')] > \beta^{k-1}u(w^*_{T-k+1})$$

And the searcher would reject any offer at time $T-k$ that she would reject at $T-k+1$. In turn, this means that

$$V_{T-k}(w) = \frac{1-\beta^{k+1}}{1-\beta}u(w^*_{T-k})$$

which implies that any wage accepted at $T-k$ is always accepted at later time periods and reservation wages are declining in t.

B.2 Digital Platform

B.2.1 Job seeker beliefs

Each period the job seeker updates their prior about the probability of receiving a better wage offer from the platform. They follow Bayes Rule:

$$f(q|x) = \frac{p(x|q)f(q)}{\int p(x|q)f(q)dq}$$

Where the digital platform can successfully produce a good wage draw with *unknown probability* q, and we hypothesize that q is anywhere in the range [0,1]. The value of q is random and we suppose that q follows a *continuous prior density function* f(q) = 1. We have simplified the problem to having a discrete likelihood because the platform only has two outcomes $x = \bar{w}$ and $x = \underline{w}$ such that $p(x = \bar{w}|q) = q$ and $p(x = \underline{w}|q) = 1 - q$. In this case, anyone who receives an offer of $\bar{w}$ from the platform will accept it; so the only interesting history is that for a seeker who has only received offers for $\underline{w}$ from the platform. Suppose in time **period 1**, the job seeker gets 1 SMS and it's a bad offer. We can compute the posterior pdf for q after seeing one bad draw:

$$\begin{aligned}
Hypothesis &= q \\
Prior &= f(q)dq = 1 \cdot dq \\
Likelihood &= p(x = \underline{w}|q) = 1 - q \\
Bayes\ Numerator &= p(x = \underline{w}|q) * f(q) = 1 - q \\
Total\ Probability &= p(x = \underline{w}) = \int_0^1 p(x = \bar{w}|q)f(q)dq = \int_0^1 (1-q)dq
\end{aligned}$$

In time **period t**, where the job seeker gets bad offers each period, we can compute the posterior pdf for q after seeing t bad draws:

$$\begin{aligned}
f(q|x = \underline{w}) &= \frac{p(x = \underline{w}|q) * f(q)}{\int_0^1 p(x = \bar{w}|q)f(q)dq} \\
&= (t+1)(1-q)^t
\end{aligned}$$

We are interested in the expected value of q given these t bad draws:

$$\begin{aligned}
E[q|x = \underline{w}] &= \int_0^1 qf(q|x = \underline{w})dq \\
&= \int_0^1 q \cdot (t+1)(1-q)^t dq \\
&= (t+1) \int_0^1 q \cdot (1-q)^t dq \\
&= (t+1) \left[\frac{1}{t+1} q(1-q)^{t+1} \mid_0^1 - \int \frac{1}{t+1}(1-q)^{t+1} \right] \quad \text{Int. by parts} \\
&= q(1-q)^{t+1} \mid_0^1 - \int (1-q)^{t+1} \\
&= -\frac{1}{t+2}(1-q)^{t+2} \mid_0^1 \\
&= \frac{1}{t+2}
\end{aligned}$$

Which shows that the job seekers' posterior q declines over time, they are less likely to think the platform can provide a higher wage offer.

B.2.2 Value function

A job seeker who has not yet received an offer of $\bar{w}$ from the platform and has a wage offer in hand of w "off-the-platform" must decide whether or not to accept the off-the-platform wage each period. They can accept, and get w this period and the continuation value of this wage offer in the future. They can reject, at which point they will receive a new wage draw on and off the platform. They expect the platform to provide a high wage offer with probability $\frac{1}{t+2}$. If they see this wage draw $\bar{w}$, they will accept it because it dominates all other non-platform wage offers. With probability $\frac{t+1}{t+2}$ they believe the platform will yield a bad wage offer $\underline{w}$, which they won't accept and they will be left with the continuation value of some wage offer w'.

We define a new value function for a seeker with access to the platform, who has a history of $\underline{w}$, $\underline{w}$, ... $\underline{w}$ in the t periods that they have received offers from the platform and a current wage offer off of the platform of w as $W_t(w)$.

$$W_t(w) = \max_{accept,reject} u(w) + \beta W_{t+1}(w), \beta \frac{t+1}{t+2} \int W_{t+1}(w') f(w') dw' + \frac{1}{t+2} W_{t+1}(\bar{w})$$

Since a person will accept and retain any job offer of $\bar{w}$, we know that $W_{t+1}(\bar{w}) = \frac{(1-\beta^{T-t})}{(1-\beta)} u(\bar{w})$. Moreover, given that the continuation value of rejecting a particular job offer is declining in t, we know that a person who accepts a job at wage w will retain it. Therefore:

$$W_t(w) = \max_{accept,reject} u(w) + \beta \frac{1-\beta^{T-t}}{1-\beta} u(w), \beta [\frac{t+1}{t+2} \int W_{t+1}(w') f(w') d(w') + \frac{1}{t+2} \frac{1-\beta^{T-t}}{1-\beta} u(\bar{w})] \tag{5}$$

Since $F(\bar{w}) = 1$, we know that the payoff associated with rejecting a wage offer of w is higher with access to the platform than it would be without (because you now have the chance to get this better offer). This gives the result in Proposition 1 and Corollary 1 that access to the platform increases reservation wages and that unemployment increases in the event that $\hat{q} > q = 0$ (job seekers believe the digital platform will deliver a high wage draw, but the true probability is zero, so they don't get a job).

Note that the difference between W and V is the option value of continued search allowed by the platform

$$W^O = \beta \frac{1}{t+2} [\frac{1-\beta^{T-t}}{1-\beta} u(\bar{w})] \tag{6}$$

whereas if the searcher chooses according to value function V, then we know that with a similar probability $1/(t+2)$

$$V^O = \frac{1}{t+2}\beta E[V_{t+1}(w')] = \frac{1}{t+2}\frac{1-\beta^{T-t}}{1-\beta}u(w^*_{T-t}) \tag{7}$$

Taking the derivative with respect to t, we have

$$\frac{dW^0 - V^0}{dt} = [u(\bar{w}) - u(w^*_{T-t})][ln(\beta)\frac{1}{t+2}(\frac{\beta^{T-t}}{1-\beta}) - \frac{1}{(t+2)^2}\frac{1-\beta^{T-t}}{1-\beta}] - \frac{1}{t+2}\frac{1-\beta^{T-t}}{1-\beta}u'(w^*_{T-t})\frac{dw^*_{T-t}}{dt} < 0 \tag{8}$$

since $ln(\beta) < 0$, $\bar{w} > w^*_{T-t}$, and $dw^*_{T-t}/dt > 0$. This suggest that over time, the gap between W and$V$ shrinks, consistent with the fact that $W_T(w) = V_T(w)$: in the final period the two value functions are identical.

Finally, note that W also shrinks faster towards V for older searchers

$$\frac{dW^O - V^O}{dT} = [u(\bar{w}) - u(w^*_{T-t})][-ln(\beta)\frac{1}{t+2}\beta^{T-t}1-\beta] + \frac{1}{t+2}\frac{1-\beta^{T-t}}{1-\beta}u'(w^*_{T-t})\frac{dw^*_{T-t}}{dT} > 0 \tag{9}$$