Job-Seekers Send Too Many Applications: Experimental Evidence and a Partial Solution

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Abstract

As job-seekers internalize neither the full benefits or costs of their application decisions, job openings do not necessarily obtain the socially efficient number of applications. Using a field experiment conducted in an online labor market, we find that some job openings receive far too many applications, but that a simple intervention can improve the situation. A treated group of job openings faced a soft cap on applicant counts and narrowed window on when they could received applicants after posting. Employer could easily opt out by literally clicking a single button. This tiny imposed cost on the demand side had large effects on the supply side, reducing the number of applicants to treated jobs by 11%—with even larger reductions in jobs where additional applicants were likely to be inframarginal. This reduction in applicant counts had no discernible effect on the probability a hire was made, or in the quality of the subsequent match. This kind of intervention is easy to implement by any online marketplace or job board and has attractive properties, saving job-seekers effort—on the order of 20%—while still allowing employers with high marginal returns to more applicants to get them.

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1 Introduction

A social planner wants the social value of a job application to equal the social cost of that application. As job-seekers internalize neither the full benefits nor the costs of their application decisions, there is no economic reason to think jobs obtain the socially efficient number of applications in a decentralized market. To the extent digitization of the search and matching process has dramatically lowered the cost of sending an additional application, we might suspect that there are frequently excess applications.

In this paper, we describe an experiment conducted in an online labor market that influenced the size of applicant pools faced by employers.[1] This was done by imposing a soft cap on the number of applicants that a job opening could receive, as well as limiting the duration of the window of time during which applications could be received: when a job opening received 50 applicants—or after 120 hours (5 days) had passed—no more applicants could apply unless the employer explicitly asked for more applicants. The intent of the intervention was to prevent job-seekers from applying to jobs where their application was likely to either be ignored or simply displace some other applicant, while still allowing employers with high marginal returns to more applicants to obtain them.

We find the treatment caused a substantial reduction in applicant counts—about 4 on average, or an 11% reduction. However, reductions were largest for jobs that otherwise would have received large number of applicants—the quantile treatment effect at the 95th percentile is a reduction of 20 applicants. Despite these reductions, the effect on hiring probability was a precise zero. About 41% of job openings were filled in both the treatment and the control group.[2] Furthermore, there is no evidence that better or worse matches were made in the treatment group, as measured by the feedback given by the employer at the end of the contract or in hours-worked. If anything, employer satisfaction rises slightly in the treatment.

The lack of effects on hiring or match quality is *ex ante* surprising, but if productivity differences among workers are reflected in wage bids, and workers within an applicant pool show little horizontal differentiation, then we

[1] We use the terms "employer," "worker" and "hire" to be consistent with the labor literature and not as a comment on the nature of the relationships created on the platform.

[2] This fill rate is actually quite similar to the fill rate reported by Indeed from 2015. `http://press.indeed.com/wp-content/uploads/2015/01/Time-to-fill-jobs-in-the-US.pdf`.

might expect firm indifference over applicants and even applicant pool size (conditional upon receiving at least one). This in a nutshell is the argument made in Romer (1992), except that he argues firms engaged in team production compress wages for morale reasons—a consideration not present in this marketplace. In this marketplace, firm indifference over applicants would lead to substantial crowd-out in hiring from marginal applicants. Indeed, in the treatment group, firms denied the right "tail" of 50+ applicants or late-arriving applicants simply hired from those before the cut-off, with apparently no ill effect.

There are explanations other than crowd-out which could explain the results, but we largely rule them out: there is no evidence that later applicants are adversely selected and thus irrelevant. There is also no evidence that the lack of an effect on hiring is purely due to employers in the treatment opting out of the cap, as the fraction of employers hiring applicants arriving after the cap fell dramatically in the treatment group.

Our claim is not that the applicant count does not matter—clearly going to 5 or 1 or even 0 would matter a great deal. Our claim instead is that for a substantial number of employers, the marginal benefit to more applicants seems to be less than the *de minimus* cost of pushing a single button. When search costs are already low, marginal applicants might simply not be worth very much, if anything. As it is, we estimate that only 7% of employers requested more applicants. Interestingly, it was relatively low-skilled job openings where the employer tended to ask for more applicants. This could be in part because of these job openings are more likely to hit the cap, and hence trigger the question about additional applicants. But these are also jobs where the employer might be building a team and plans to make multiple hires.

A natural question is what the treatment did to the experience of the job-seeker. It is important to note that although relatively few job openings were affected by the 50 applicant cap (about 10%), these job openings are disproportionately important to job-seekers, as they attracted 43% of applications. This difference simply reflects the fact that a randomly selected application is more likely to be sent to a job with a high applicant count.[3]

Although randomization was at the level of the job opening, we can also analyze the experiment at the level of the individual worker application. Workers were not aware of the employer's treatment assignment and workers

[3]This is a version of the friendship paradox (Feld, 1991).

typically apply to many jobs, so we frequently observe the same worker applying to both treated and control jobs. As such, although different workers might have different probabilities of applying to one kind of opening or another, we can focus in on the effect of the experiment using a within-worker analysis.

We find that a worker applying to a treated job opening had a 17% increase in their probability of being hired. This increase in application success might raise or lower overall application intensity in equilibrium (Shimer, 2004)—job applications have become more valuable to the worker, but fewer need to be sent to secure a job, on average. However, regardless of the effect on application intensity or the application cost, capping application numbers would still improve worker welfare relative to the *status quo* via a simple envelope theorem argument.

In this market, job-seekers could already observe and condition on the real-time number of applicants. This is not the case in most traditional labor markets—and this inability to condition is a source of "ball and urn" matching frictions, with jobs receiving too many or too few applicants by chance. Information about congestion can seemingly help (Gee, 2019; Bhole et al., 2021), but the congestion game itself is potentially wasteful if marginal returns as low.

To see this wastefulness with a simple example, suppose an application costs the job-seeker c and the social value of job post is $V(A)$, where A is the number of applicants. And suppose the job-seeker gets a fraction $\theta > 0$ of the job surplus if she is hired. If job-seekers think they are equally likely to be selected from among the applicants, they will apply until $\theta V(A)/A = c$, but a social planner would like $V'(A) = c$. Note that in the decentralized equilibrium, $\theta V(A) = Ac$. That is, the entire hired worker pay-off is consumed by application costs. If θ is sufficiently small or $V'(A)$ is sufficiently large, the platform might of course welcome more applications—echoing the economic intuition of Hosios (1990). But with the soft cap design of the experiment, the employer not lifting the cap is revealing that $V'(50) \approx 0$. And so while job-seekers might still want to play the congestion game and keep applying past 50, no social planner would want this game to continue.

A puzzle presented by the results is how to reconcile (a) nearly complete crowd-out in hiring and (b) a fill rate far less than 100%. Substantial crowd-out implies that the employers who hired had many candidates who would have been acceptable. But with a fill rate of only about 41%, a substantial number of employers had no one in their pool they were willing to hire. If

all employers had the same hiring reservation value—and each applicant has some probability of exceeding this value—then the overall fill-rate implies the treatment should have reduced fill rates by about 26%—not the $\approx 0\%$ we find. The most likely explanation is that there is substantial employer heterogeneity in employer reservation utility. It seems likely that there are "tire kicker" employers testing the market but are not very serious, and serious employers strongly committed to hiring, but job-seekers cannot easily tell the difference. We present some suggestive evidence that this is the case.

This is the first experiment we are aware of where the number of applications to a job opening was experimentally reduced. The key contribution of the paper is to use this experimental variation to show that many job applicants are inframarginal in the decentralized labor market equilibrium. We also illustrate the crowd-out effect of other applicants in a particularly direct way, compared to the literature (Lalive et al., 2015). Our crowd-out results call into further question the equilibrium justification for job search assistance (Crépon et al., 2013; Marinescu, 2017).[4]

Paired with our contribution to the literature is a practical—albeit partial—solution that could be implemented by any computer-mediated labor matching marketplace. Market design interventions that save workers time or direct their applications to relatively under-subscribed openings could offer substantial welfare gains, even setting aside any employer benefits from more efficiently directed applications. The US non-institutional population *on average* spends about 15 hours a year on job search activities, which is about \$75B per year in time value at the median US wage.[5]

The rest of the paper is organized as follow. Section 2 describes the experimental context. Section 3 explains the design and discusses internal and external validity. Section 4 presents the experimental results. Section 5 discusses the implications of the results. Section 6 concludes.

[4]Though there is evidence that more targeted recruiting assistance can be helpful without much crowd-out (Horton, 2017, 2019) and that interventions that have job-seekers consider a wider range of options could be beneficial, as in Belot et al. (2019). The lack of an *increase* in hiring in the treatment is evidence against the "choice overload" hypothesis (Iyengar and Lepper, 2000), which itself has been called into question (Scheibehenne et al., 2010).

[5]Using data from 2013 and assuming 252 working days per year—see `https://www.bls.gov/tus/current/work.htm`.

2 Empirical context

Our setting is a large online labor market. In this market, employers post job openings to which workers can typically apply without restriction. The kinds of offered work include tasks that can be done remotely, including programming, graphic design, data entry, translation, writing and so on. Jobs can differ substantially in scope, with some formed matches lasting for years, while others might only last a day or two as a simple project is completed. See Horton et al. (2017) for roughly contemporaneous details on the distribution of kinds of work, contract structure, and patterns of trade in an online labor market.

Employers can solicit applications by recruiting workers, or workers can just apply to openings they find. The majority of applications on the platform come from workers finding job openings through various search tools and then submitting an application. Applying workers submit a wage bid (for hourly contracts) or a fixed amount (for fixed price jobs). When applying, the worker can observe the number of applicants that have already applied. Employers then screen applicants and potentially make a hire or make multiple hires—though hiring a single workers is by far the most common choice, conditional upon hiring anyone.

As we will see, applicants arrive very quickly. The reason for this speed is that workers have an incentive to apply as quickly as possible, all else equal, as they do not know exactly when the employer will start making a decision. We will demonstrate this speed later in the paper. Fast applications also seem to be the case in conventional markets when application behavior is observed (see van Ours and Ridder (1992)).

There is a burgeoning literature that uses online labor markets as a domain for research. Pallais (2013) shows via a field experiment that past on-platform worker experience is an excellent predictor of being hired for future job openings. Stanton and Thomas (2016) shows that agencies (which act as quasi-firms) help workers find jobs and break into the marketplace. Agrawal et al. (2013) investigate what factors matter to firms in making selections from an applicant pool and present some evidence of statistical discrimination, which can be ameliorated by better information. Horton (2017) explores the effects of making algorithmic recommendations to would-be employers. Barach and Horton (2020) reports the results of an experiment in which employers no longer had access to wage history when making hiring decisions.

Although our setting offers a rich, detailed look at hiring, there are limitations. A downside of our context is that it is one marketplace. However, when applications are observable in conventional markets, the success probability also appears to be quite low and is similar to what we observe (Skandalis and Marinescu, 2018). Although our context is unique, the basic economic problem—workers not internalizing the externalities of search intensity—is commonplace, and there is emerging evidence that the precise context matters less than we might imagine for generalization (DellaVigna and Pope, 2019). Furthermore, the job search that occurs on online job boards presently is quite similar to our setting, even if the resulting jobs are different (Marinescu and Wolthoff, 2020).

3 Design of the experiment

How the experiment worked was simple: once a job opening received 50 applicants or 120 hours (5 days) had elapsed since posting, the job was made "private" and no further would-be applicants could apply. The employer was notified of this change when it happened in the interface and via email. Employers could, at any time, revert the change from public to private by pushing a single button. Appendix A shows the interfaces where these notices were presented to employers and where they could in turn make choices.

Randomization was at the level of the employer. A total of 45,742 jobs openings posted by employers were assigned, covering job openings posted between 2013-11-04 and 2014-02-14. The software used by the platform to randomize employers to treatment cells has been used successfully in many experiments. There were 23,075 job posts in the treatment and 22,667 in the control. The p-value for a χ^2 test is 0.056, which is slightly concerning, but daily counts of allocated jobs show (which will show) has no obvious imbalance and a table of pre-randomization job attributes shows excellent balance, suggesting the low p-value from the χ^2 test is simply due to sampling variation. The experimental sample was itself randomly drawn from all job openings being posted on the platform. We do not report the exact fraction, but it was less than 1% of all job openings posted in the market, which reduces concerns about cross-group interference.

After being assigned to a cell, any subsequent job openings by that employer received the same treatment assignment. However, we only use the first job opening in our analysis, as subsequent job openings could have been

affected by the experience in the first opening.

4 Results

As jobs were allocated to the experiment over time, we can begin by plotting daily means by experimental group. Figure 1 plots by-day times series for the treatment and control groups. In the top facet, the series is just the count of allocated job posts—as expected, numbers in treatment and control track closely.

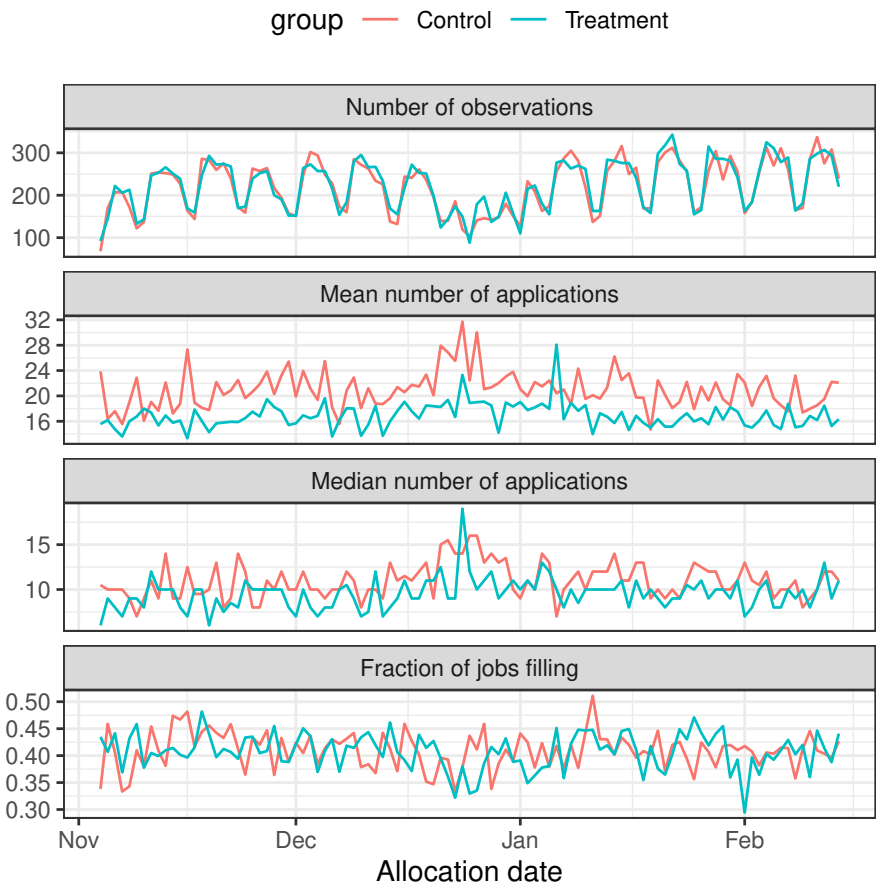
The second facet from the top is the mean number of applications. We can see the treatment is always substantially below the control. In the facet below, if we look at the *median* number of applications, the difference is smaller, yet still visually evident. This is suggestive that the intervention likely had effects that were not concentrated equally over all jobs, but rather were stronger for jobs that would otherwise receive many applicants. Previewing one of our main results, in the bottom facet for Figure 1, we can see no obvious evidence of a difference in the probability that a job was filled.

A table of summary statistics for our primary outcomes, by cell are in Appendix B.

4.1 Internal validity

Although the count of jobs by cell in Figure 1 suggests successful randomization, to better assess balance, in the top panel of Table 1, labeled "Pre-Treatment" we report means for several pre-experiment job characteristics, by group. In the first row, we have the vertical preference score, which is elicited from the employer when they post a job. It ranges from 1 (an employer interested in low cost and willing to work with relatively inexperienced workers) to 3 (an employer interested in the most experienced workers and willing to pay high wages). We also have the employer's estimate of how long they think the job will take (in weeks). Another pre-experiment outcome is whether the employer included an attachment in the job post (typically images or a more detailed specification). Below that, we have an indicator for whether the job opening was hourly. Across all pre-treatment measures, we obtain excellent balance, consistent with successful randomization.

Figure 1: Group-specific outcomes by allocation date, over time



Notes: This plot shows by-day times series for the two experimental groups. In the experiment, employers posting jobs were randomized to a treatment or a control. Employers in the treatment could not receive additional applicants once they received 50 applicants or 5 days had passed since posting. However, the employer could opt out of this cap by clicking a single button.

Table 1: Job opening attributes and outcomes for treatment group

	Means		Difference		
	Treatment	Control	Diff.	SE	t-stat
Pre-Treatment					
Vertical preference score	0.894	0.879	0.015	0.010	1.486
Duration weeks	8.435	8.465	−0.030	0.160	−0.187
Has attachment	0.109	0.108	0.001	0.003	0.463
Hourly job?	0.521	0.517	0.005	0.005	0.994
Post-Treatment					
Number of applications	16.849	20.821	−3.972	0.323	−12.315
Number of apps equal to 49	0.003	0.003	−0.000	0.001	−0.782
Number of apps equal to 50	0.018	0.003	0.016	0.001	16.566
Number of apps equal to 51	0.011	0.003	0.008	0.001	10.413
Number of apps equal to 52	0.008	0.003	0.005	0.001	7.032
Number of apps equal to 53	0.006	0.003	0.003	0.001	5.166
Number of apps equal to 54	0.005	0.003	0.001	0.001	2.453
Number of apps equal to 55	0.004	0.003	0.001	0.001	2.236
Number of apps equal to 56	0.003	0.003	0.000	0.001	0.589
Number of apps equal to 57	0.003	0.003	0.000	0.001	0.327
Number of apps greater than 57	0.035	0.077	−0.042	0.002	−19.545

Notes: Table of pre- and post-randomization job level attributes. There are 23,075 job openings in the treatment and 22,667 in the control. In the experiment, employers posting jobs were randomized to a treatment or a control. Employers in the treatment could not receive additional applicants once they received 50 applicants or 5 days had passed since posting. However, the employer could opt out of this cap by clicking a single button.

4.2 Effects on applicant pool composition

We visualize the effects of the intervention on applicant pools in Figure 2.

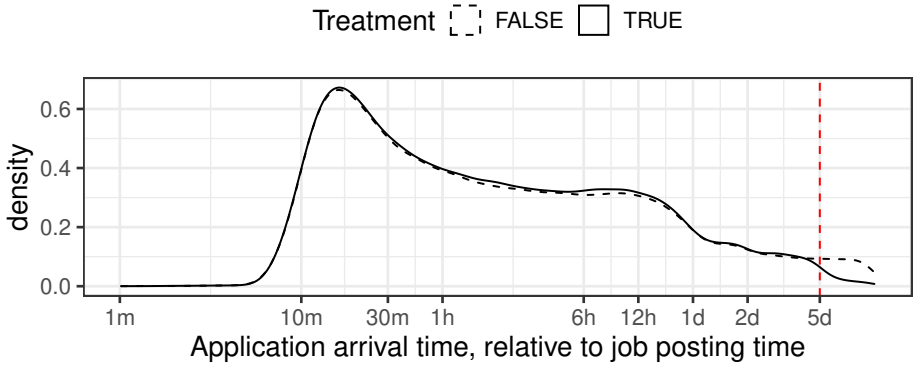
In Figure 2a we plot the (1) the arrival time of applicants to illustrate the effects of the 120 restriction. We observe the application arrival times—measured down to the millisecond—relative to when the job was posted. The sample is restricted to jobs that received 50 or fewer applicants and arrived within the first 10 days. We also remove a small fraction of applications that arrive in less than one minute, so as have a sensible distribution given our log scale. We can see that distributions are nearly identical up until 5 days, at which point the treated group shows a marked fall-off, consistent with how the treated intervention worked. We can also see how quickly applications typically arrive—both groups exhibit a spike around 20 minutes after posting. While applicant do continue to arrive later, by even one day after posting, quantities have fallen off considerably. The reason is not so much that search technology (though jobs are by default sorted chronologically), it is that job-seekers do not know when employers will make a decision, and so all else equal, they want to arrive earlier rather than later.

Figure 2b plots the kernel density estimates for the application counts for treatment and control. We restrict the domain to less than 200 applicants. As expected, there is a "jump" around 50 applicants in the treatment and no such jump for the control. Prior to 50, there is some slight visual evidence of fewer applicants, but this is better explored with a quantile regression.

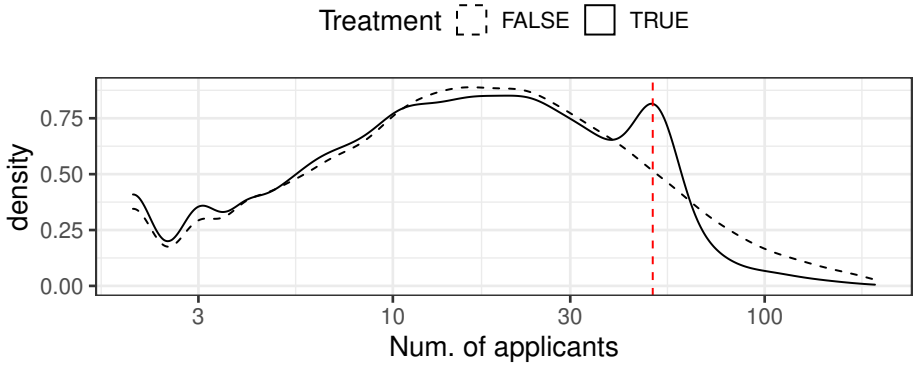
Despite the cut-off of 50 applicants, there is actually excess mass at numbers slightly greater than 50—a fact obscured by the density plot. We can see this excess pooling in the "Post-Treatment" panel of Table 1, we report the fraction of job openings receiving various numbers of applications. As expected, there is a much larger share in the treatment group receiving exactly 50 applicants. But we can also see a pooling at exactly 51 applicants. We see excess mass all the way up to 55 applications. What explains this "near 50" pooling is that applicants can and do withdraw their applications, which raised the effective cap. Despite this withdrawal, we include these applications in our sample. This means that even a treated employer hitting the soft cap could still obtain more than 50 applicants in total without requesting more applicants.

To better see where the effects were concentrated, we conduct a series of quantile regressions. Figure 2c reports estimates of quantile regression treatment effects on application counts, along with 95% CIs for each point

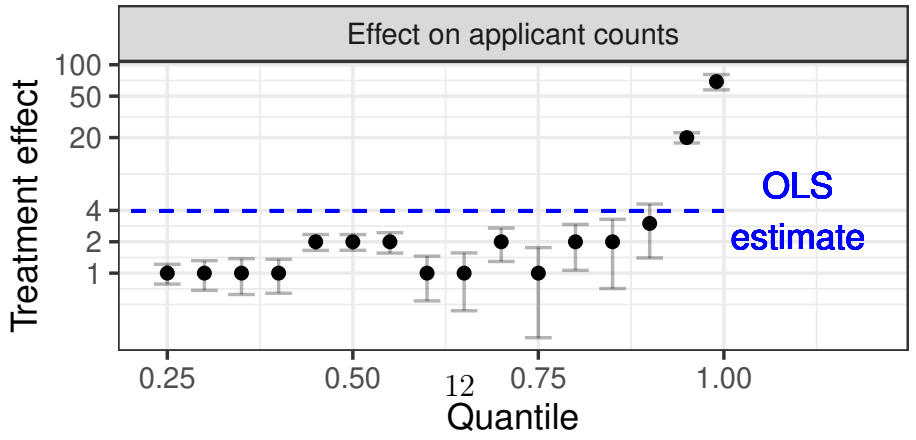
Figure 2: Evidence of the effect of the interventions of applicant pools



(a) Distribution of application arrival times, by treatment group, for jobs receiving fewer than 49 applicants



(b) Kernel density estimate of the distribution of applicants per job opening, by treatment and control



(c) Quantile treatment effects

Notes: In the experiment, employers posting jobs were randomized to a treatment or a control. Employers in the treatment could not receive additional applicants once they

estimate. The y-axis is log transformed. The x-axis is the associated percentile. Below about the 25th percentile, there is no evidence of an effect. Above that up until about the 90th percentile, the treatment lowered the number of applications by one or two applicants. Effects start to increase rapidly after the 90th percentile, such that the 99th percentile shows a reduction of more than 50 applicants. The main effect of the intervention was to reduce applicant counts dramatically for jobs that would otherwise receive large counts.

For comparison to these quantile effect estimates the OLS estimate of the treatment effect on the number of applicants is plotted as a horizontal dashed line. The mean effect is about a 4 applicant reduction. It is so relatively precise that a confidence internal is not plotted.

The question we turn to know is how these applicant pool changes affected the probability the employer hired, and what kind of match they formed.

4.3 Effects on match formation probability

The main result on match formation can be seen visually in the bottom facet of Figure 1: despite this difference in applicant counts, there is no graphical evidence of a difference in the probability a job is filled, by cell. Confirming this, in Table 2 we report various regressions of hiring outcomes on the treatment indicator. In Column (1) of Table 2, the outcome is whether a hire was made. There was no discernible effect on whether the employer hired anyone—the treatment effect is close to zero and quite precise. Note that the baseline fill rate in the control is the intercept and is about 41%.

Although most employers that hire only hire one applicant if any, some employers do make multiple hires. And some make many hires, particularly if they are building a large team. It might be the case that employers with this plan would be more impeded by the treatment. To test for this possibility, in Column (2) of Table 2 we regress the total number of hires on the treatment indicator. It is negative, but not significant and fairly close to zero. There is no strong evidence the treatment reduced the total amount of hiring, presumably because those wishing to build large teams could easily avoid the cap in the treatment.

Although our results are suggestive of applicant crowd-out, it is not definitive—perhaps it was the soft nature of the cap that explains the lack of hiring effects. As an extreme case to make the logic clear, imagine if employers always received at most one acceptable applicant and that applicant

Table 2: Effects of the treatment on number of applications and whether the job opening filled

	Any hires?	Total hires	Any hires after 55?
	(1)	(2)	(3)
Treatment	-0.002	-0.016	-0.007***
	(0.005)	(0.010)	(0.001)
Intercept	0.411***	0.528***	0.020***
	(0.003)	(0.007)	(0.001)
N	45,742	45,742	45,742
R squared	0.00000	0.00006	0.00068

Notes: This table reports the effects of treatment assignment on whether a hire wade in Column (1), the total quantity of hires, in Column (2), and whether a hired applicant was greater than the 55th arrival. In the experiment, employers posting jobs were randomized to a treatment or a control. Employers in the treatment could not receive additional applicants once they received 50 applicants or 5 days had passed since posting. However, the employer could opt out of this cap by clicking a single button. Significance indicators: $p \leq 0.05 : *$, $.p \leq 0.01 : **$ and $p \leq .001 : ***$.

could arrive before or after the cap. With a hard cap, treated employers whose acceptable candidate would have arrived after the cap would simply not hire. With a soft cap, those treated employers would still be able to hire, but we would find no average difference between treatment and control in the probability that an applicant arriving after the cap was hired.

To assess this conjecture, in Column (3) of Table 2, the outcome is whether the employer hired anyone who applied after the 55th applicant. We can see that it is substantially reduced in the treatment—the treated group was 33% less likely to hire a 55+ applicant than the control. Another potential explanation for the lack of hiring effects could be that later applicants are adversely selected or perhaps arriving so late as to be effectively irrelevant. We will later show this more directly later in the paper, but there is no evidence this is the case.

4.4 Effects on match quality

Although we have no evidence that the treatment affected the *quantity* of matches, it is possible the treatment lowered the *quality* of matches. This conjecture does potentially require employers to make a mistake, given how low the costs were to simply pushing a button to get more applicants. However, that mistake could have been simply not noticing the button to avoid the cap or returning to hire long-after the cap was met and new applicants would not be forthcoming (as job-seekers avoid "stale" jobs).

To look for match quality effects, in Table 3, we regress several match outcomes on the treatment indicator. It is important to note that the sample is selected in the sense that these are only openings where a hire was ultimately made and we can make a post-hire measurement. Although we have no evidence of a change in fill rate because of the treatment, the precise jobs that were filled could be different—although we have no evidence to suggest that this is the case.

In Column (1), the outcome is the hired worker wage for hourly jobs. We see no evidence of a substantial change by treatment assignment—there is no evidence the employer was getting a worse deal, at least in terms of posted price. The outcome in Column (2) is the log total hours-worked per hired worker, conditional upon at least on hour (this is why the sample size is smaller). There is some slight evidence of an increase in hours-worked, which could suggest a better match (the employer wants to buy more hours) or a worse match (the hired worker takes more time to complete the task).

Table 3: Match outcomes, conditional upon a hire

	Log hired worker wage	Log hours-worked	Feedback on worker
	(1)	(2)	(3)
Treatment	-0.002	0.042	0.011
	(0.014)	(0.044)	(0.012)
Intercept	2.240***	2.987***	4.667***
	(0.010)	(0.031)	(0.009)
N	9,354	7,082	16,330
R squared	0.00000	0.00013	0.00005

Notes: The sample for these regressions are those job openings where a hire was made. In the experiment, employers posting jobs were randomized to a treatment or a control. Employers in the treatment could not receive additional applicants once they received 50 applicants or 5 days had passed since posting. However, the employer could opt out of this cap by clicking a single button. In Column (1), the sample consists of hourly job openings; in Column (2), job openings where at least 1 hour was billed. In Column (3) the sample is all job openings, including fixed price jobs. Significance indicators: $p \leq 0.05 : *$, $.p \leq 0.01 : **$ and $p \leq .001 : ***$.

In any event, there is no evidence of a substantial change in satisfaction: in Column (3) we report the average feedback that the employer left for the worker (on a 1 to 5 star scale). We see no significant difference in feedback by treatment assignment.[6] The sample is larger because fixed price jobs also generate feedback.

4.5 Which employers wanted more applicants?

We do not know exactly how many employers in the treated group opted to receive more applicants—it was not perfectly logged. However, we do know the final state of the job—public and private—so in the treatment, we can impute the fraction of jobs where the button was pushed to get more applicants.

In Figure 3a, we plot the imputed fraction of employers pushing the button to receive more applications. Some categories have a low volume and so the estimated fraction is imprecise. Generally, we see more button-pushing in relatively low-skilled categories. One likely explanation is that these are also the categories with relatively large application counts, and so the employer was more likely to hit the 50 cap. This point illustrates an advantage of the soft cap design rather than the platform trying to decide *a priori* which kinds of jobs have a higher marginal return to more applicants.

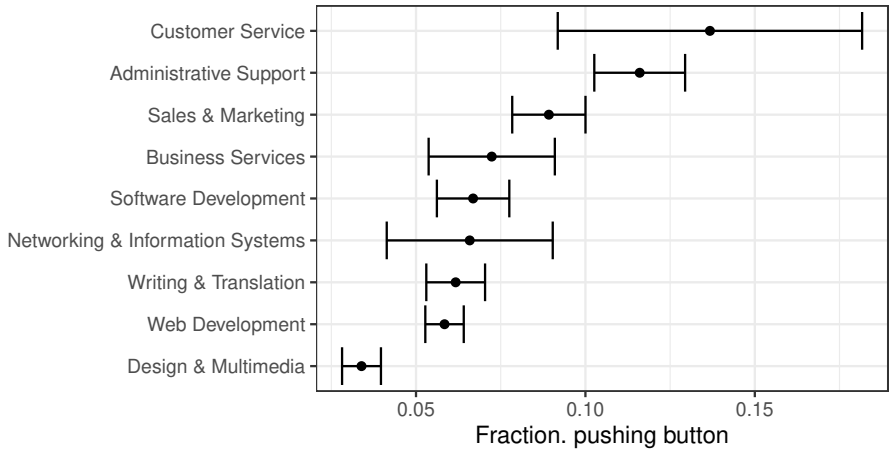
Figure 3b shows the fraction of jobs with more than 55 or fewer than 55 applicants, by category and treatment status. The y-axis is on a log scale, as the fraction of openings exceeding the cap is quite small in some categories, and some categories are quite small. We can see that, for example, "Design & Multimedia" has large reductions in the 55+ group, as this category tends to attract many applicants and few would-be employers pushed the button. "Customer Service" shows little evidence of a decline in the 55+ group, but we can also see that this a small category, with less than 1% of jobs posted.

4.6 Effects on job-seekers

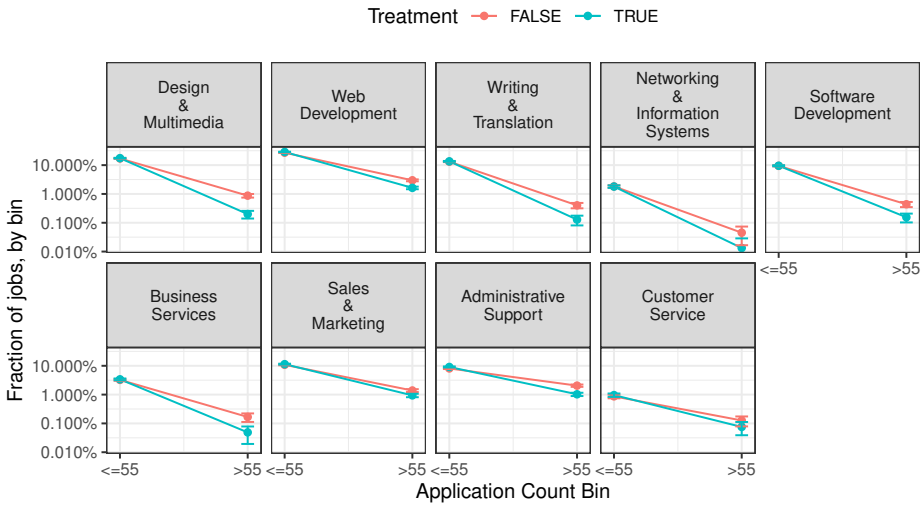
With smaller applicant pools but the same amount of hiring, we might expect that job-seekers applying to treated job openings enjoyed a higher probability of being hired. Although randomization was at the level of the job opening,

[6] Numerical feedback is prone to inflation and strategic misreporting, but as (Filippas et al., 2018), star feedback is still highly correlated with measures of reviewer satisfaction.

Figure 3: The kinds of jobs that required more applications



(a) Implied fraction of employers pushing the button



(b) Percentage of jobs with 55+ applicants, by category and group

we observe individual worker applications. Workers did not know the treatment status of the job openings when deciding whether or not to apply. We observe 129,520 distinct job-seekers collectively sending 738,861 applications to job openings assigned to the experiment. The mean number of applications per worker is 5.7, while the median is 2.

We might worry that the kinds of workers that apply to job openings with many applicants (which are disproportionately found in the control) are different from those applying to jobs in the treatment. However, as workers send many applications, we can include a worker-specific fixed effect to perform a within-worker analysis, obviating concerns about selection. We estimate a regression of the form

$$y_{ij} = \beta \cdot \textsc{Trt}_j + \textsc{AppCount}_j + \gamma_i + \epsilon \tag{1}$$

where $\textsc{Trt}_j$ is the treatment assignment of the applied-to job opening, γ_i is a worker-specific fixed effect and $\textsc{AppCount}_j$ is a fixed effect for the total applicant count for that opening. As workers can and do send different numbers of applications, we weight these regressions by the inverse of the total number of applications sent by the worker. This has the effect of treating all workers equally.

In Column (1) of Table 4, we report an estimate of Equation 1 where the outcome is an indicator for whether the worker was hired. As expected, workers are more likely to be hired when applying to a treated job opening. Given the baseline hiring probability, this coefficient implies about a 17% increase in hiring probability on a per-application basis. Note that the baseline probability of being hired is about 3%—which is nearly identical to that rate found by Skandalis and Marinescu (2018). Although the sign of the effect is mechanically determined (given results from Table 2), the magnitude cannot be read off directly from the reduced applicant pool size.

As another confirmation that the treatment affected the worker's application experience, in Column (2), the outcome is the rank in the applicant pool (i.e., first applicant is 1, second applicant is 2, etc.). Unsurprisingly, this falls as well, as "late" applications are missing from treatment jobs.

Job-seekers do not just apply to a job opening—they also choose a wage bid. Workers applying to a job opening that already has many applicants—as is more common in the control—might bid down. In Column (4), we see some evidence that workers bid up when facing a smaller pool—the treatment raises wage bids by about 1.2%. This is suggestive that aside from simply

Table 4: Association between treatment status of applied-to job opening and application outcomes

	Hired	Rank	Log wage bid
	(1)	(2)	(3)
Treatment	0.003***	-13.712***	0.012***
	(0.001)	(0.186)	(0.002)
DV Mean	0.02	33.23	1.89
Worker FE	Y	Y	Y
Worker Cluster SE	Y	Y	Y
N	738,861	738,861	466,592
R squared	0.54924	0.80494	0.95144

Notes: This table reports regressions of applicant-level outcomes on the treatment status of the applied-to job opening. The sample consists of all applications sent to assigned job openings. In the experiment, employers posting jobs were randomized to a treatment or a control. Employers in the treatment could not receive additional applicants once they received 50 applicants or 5 days had passed since posting. However, the employer could opt out of this cap by clicking a single button. Regressions are weighted by the inverse total number of applications sent by each worker. Each regression includes a worker-specific fixed effect. Standard errors are clustered at the level of the individual job applicant. Significance indicators: $p \leq 0.05 : *$, $.p \leq 0.01 : **$ and $p \leq .001 : ***$.

saving job-seekers time, the treatment could also transfer some surplus from employers to workers by reducing *in situ* competition. However, recall that there was no evidence of a substantial change in hired worker wage at the job level, so this bidding effect is not likely of first order importance, at least at the soft cap level considered.

4.7 Should we have expected match quality effects?

With fewer applicants to choose from, we would *a priori* expect worse matches. If we imagine each applicant offers some idiosyncratic surplus, fewer choices means a lower expected pay-off. However, there are economic reasons why we might expect these marginal losses in match quality to be small. In a nutshell, if vertical differentiation gets priced into worker wage bids and horizontal differentiation is not too important (as we might expect *within* a self-selecting group of job-seekers responding to a particular job posting), price competition would tend to make employers indifferent over applicants in the pool, as in Romer (1992). This in turn would tend to make applicant pool size practically irrelevant and explain why employers would be so willing to substitute.

It is useful to step back from the labor context to highlight the importance of pricing and vertical differentiation, which is perhaps easier to think about with a simple good. Suppose you are at a grocery store and you are selecting an apple to buy from the store's display. If there is a lot of variation in quality—some look great, some are bruised or have soft spots—but all are offered at the same price, having more apples offered would help you form a better "match." You are more likely to get a very good apple if you have more to choose from. And if good apples are very rare, then the marginal benefit declines more slowly. This is the logic of why we might expect worse matches with fewer choices.

But now instead suppose each apple was priced differently and independently to account for its own vertical attributes. So long as each apple views itself as not having much market power, having more apples does not help very much. You, the consumer, are going to get the quality-adjusted market rate apple surplus, not matter what you choose. Any additional surplus you get is going to reflect perhaps horizontal preferences or pricing error—both of which would likely be small for a good like apples.

This "individually priced apple" notion has strong support in our data. In Table 5, we report regressions of an indicator for an application leading to a

hire (x 1,000) on the log wage bid from the applying worker and an opening-specific fixed effect. The sample consists of applications only to hourly job openings in the control group. In Column (1), the sign on the wage goes the "wrong" way—higher bidding workers are *more* likely to get hired. But of course, this bid is a choice by the applying job-seeker, and they are not bidding randomly. They know if they are a "good apple" or not and can tailor their bid accordingly.

In Column (2), we add a worker-specific fixed effect, exploiting the matched structure of our data, as in Abowd et al. (1999). With the inclusion of worker and job fixed effects, we are estimating the effect of the wage bid on hire probability by observing the same job-seeker varying his or her wage bid across jobs, and hopefully accounting for any job-specific, common factors that might cause the workers to bid up or down. Our specification is now

$$y_{ij} = \beta \cdot \log w_{ij} + \gamma_i + \delta_j + \epsilon,$$

where γ_i is the worker fixed effect and δ_j is a job opening fixed effect.

As expected, the larger the wage bid, the less likely the worker is hired. The effect is enormous relative to the non-fixed effect regression. We see the same coefficient on the wage bid in the regression reported in Column (5), where we regress on rank and wage bid simultaneously. The small coefficient on rank reiterates the point made earlier how rank effects cannot be very important.

What these Table 5 regressions suggest is that workers generally bid cognizant of their perceived productivity. As such, rather than interpreting the wage as a direct bid into the auction for the job, the wage should be thought of as a component of the total bid, which also includes efficiency and quality, as in a scoring auction. There are presumably still some latent differences that employers care about that the worker cannot price in—if everything that mattered was priced in, epsilon changes in wage bid would lead to a worker being hired for sure or never hired. And so there is still some "room" for match quality to change with the number of options available, but it is less than one might expect if the endogeneity of wage bidding was not appreciated.

Table 5: Association between worker application wage bidding and hiring in the control group

	Hires (1/0) x 1000		
	(1)	(2)	(3)
Log wage bid	2.018***	-11.019***	-11.018***
	(0.247)	(1.117)	(1.117)
Applicant arrival rank			-0.002
			(0.008)
Intercept	9.492***		
	(0.526)		
N	262,463	262,463	262,463
DV Mean	13	13	13
Worker FE	N	Y	Y
Job Opening FE	N	Y	Y
Worker Cluster SE	Y	Y	Y
R squared	0.00025	0.75011	0.75011

Notes: The table reports regressions of application-level outcomes—namely whether the applicant was hired. In the experiment, employers posting jobs were randomized to a treatment or a control. Employers in the treatment could not receive additional applicants once they received 50 applicants or 5 days had passed since posting. However, the employer could opt out of this cap by clicking a single button. The regressions are weighted by the inverse of the total number of applications sent by the worker. The sample consists of all applications to all job openings assiged to the control group in the experiment. Standard errors are clusterd at the worker level. Significance indicators: $p \leq 0.05 : *$, $.p \leq 0.01 : **$ and $p \leq .001 : ***$.

5 Discussion

The key finding of the experiment was that reducing application counts had no discernible effect on whether the employer hired, or the subsequent match. The natural question that arises is whether the lack of effects on hiring is surprising, and what this implies about the hiring process. The results are surprising. Consider the simplest model of batch hiring—a firm is selecting from a complete pool of applicants and chooses the best one, subject to the candidate exceeding some threshold. Suppose that search is random and that the firm receives an exogenous number of applicants a, who are all drawn IID from some distribution of worker productivity, which the worker bids, plus some noise. Suppose, further, all firms have some reservation surplus for hiring—they are willing to hire if an applicant is above that value, but otherwise not. Let $\underline{u}$ be the probability that a randomly selected applicant exceeds that hiring reservation surplus.

With this collection of a applicants, the probability that the firm would hire anyone is

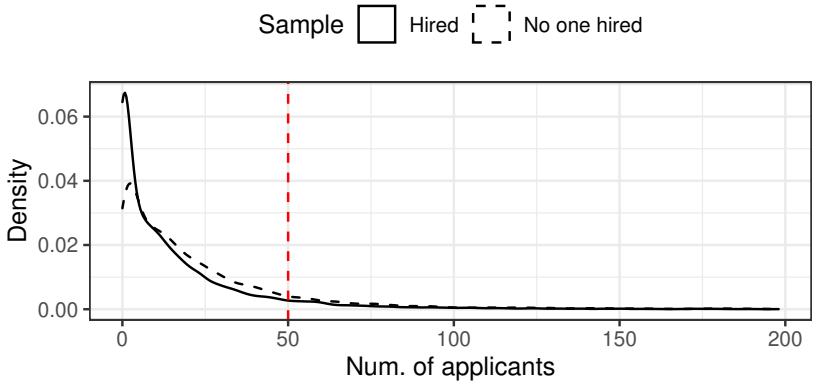
$$\begin{aligned} Pr(\textsc{Hired}|a) &= 1 - (1 - \underline{u})^a \\ &\approx 1 - \exp(-\underline{u}a). \end{aligned}$$

Using the control group, the maximum likelihood estimate of $\underline{u}$ is 0.0292, with a standard error 0.0003. If we then use that fitted value of $\underline{u}$ and apply it in the treatment group, the predicted percentage reduction in the fill rate from the treatment is 26%. This is, of course, highly counterfactual to the experiment which found no reduction in hiring. This prediction is much larger and is well out of the confidence interval in Table 2.

With the size of applicant pools that we observe in the control group and the overall fill rate near 50%, each application must have a low hire probability. But if each applicant has a low hire probability, all else equal, there should be very little crowd-out—it is rare for the firm to get one above-the-bar applicant, nevermind two. With little crowd-out, reducing the applicant pool by some percentage amount should have about the same percentage effect. That we observe no reduction in hiring suggests something is wrong with this simple characterization of the hiring problem.

One potential rationalization for substantial crowd-out but a far-from-100% fill rate is employer heterogeneity. Some employers are easy to please, get many applicants and hire, but would also still hire with a much smaller

Figure 4: Distribution of applications to control job openings, by whether the opening was filled



Notes: This figure plots the kernel density estimate for the log number of applications in the control group, by whether or not the job opening led to a hire.

pool; other employers are hard to please, get many applicants, do not hire and obviously would not hire with a much smaller pool either. However, it does not appear that job-seekers know how to condition on this "pickiness"—in Figure 4, we plot the distribution of applications to control openings, by whether or not the job filled. There is no visual evidence that job-seekers try to "avoid" bad jobs, at least in application counts above 49. This is further evidence that more guidance to job-seekers around the likely effect of their applications could be welcome.

6 Conclusion

This paper shows that many applications in a decentralized labor market are likely inframarginal. The value of the marginal proposal for at least some employers was less than the cost of pushing a button. This creates a market design opportunity. Although our results come from a particular marketplace, the issues are commonplace in matching markets and could potentially be addressed by a market-designing platform.

Aside from the pure cost savings, presumably some prevented applications could be re-directed to other jobs where they have a higher value. Our

context does not allow us to answer the question of whether any of the "saved" application were re-directed to jobs where they would have a higher value. This would be an interesting question for future research.

Another interesting question is what would be the optimal number of applications per job, and whether market design changes could help bring that about. Although we have focused on crowd-out and job-seekers not internalizing costs, job-seekers also do not internalize the full benefits of the applications in creating matches—externalities abound in equilibrium search models (Diamond, 1982; Mortensen and Pissarides, 1994; Hosios, 1990). Beyond crowd-out and crowd-in, there are also congestion concerns that would be interesting to explore empirically (Roth and Xing, 1994; Albrecht et al., 2006).

Our paper illustrates that even small interface and policy changes by platforms can lead to large economic changes. As more of economic life becomes computer-mediated, the opportunity for platforms to control or influence the number of counter-parties a seller interacts with has grown (Varian, 2010; Fradkin, 2015; Halaburda et al., 2017; Li and Netessine, 2019). And although the effects of the Internet on labor markets was initially thought to be limited (Kuhn and Skuterud, 2004; Kroft and Pope, 2014), with the continuing maturation and expansion of the Internet, this characterization might already be changing (Kuhn and Mansour, 2014).

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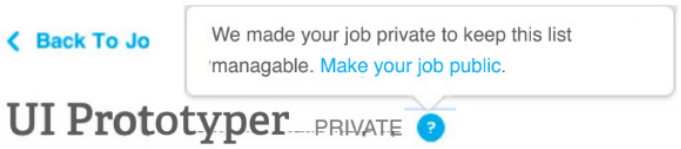
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A Interfaces

Figure 5a shows the on-app notice that employers received that their job was made private. The employer could request "re-opening" the job with a single button click both on the notice. Both treated and control workers could make their job private at any time—the interface is in Figure 5b. They could also

Figure 5: Notices to employers about their job opening



(a) Notice to treated employers that their job was made private



(b) Button to make a public job private



(c) Button to make a private job public

Notes: These are screenshots of the interfaces shown to employers.

make a private job public at any time—the interface is in Figure 5c. Neither workers nor employers knew about the treatment *ex ante* and workers never learned about the treatment.

B Additional analyses

In addition to our regression approaches, many of the main results can be observed simply by comparing means of outcomes. Table 6 reports summary statistics for the job opening sample, by treatment assignment.

Table 6: Summary statistics for the experimental sample of job openings

	N	Min	25th	Mean	Median	75th	Max	StDev
Number of apps								
Control	22,667	0.00	2.00	20.82	11.00	13.00	1,536.00	37.31
Treatment	23,075	0.00	1.00	16.85	9.00	11.00	3,194.00	31.37
Any hires								
Control	22,667	0.00	0.00	0.41	0.00	0.00	1.00	0.49
Treatment	23,075	0.00	0.00	0.41	0.00	0.00	1.00	0.49
Total hires								
Control	22,667	0.00	0.00	0.53	0.00	0.00	75.00	1.22
Treatment	23,075	0.00	0.00	0.51	0.00	0.00	33.00	0.92
Average wage bid								
Control	10,277	0.01	6.15	12.27	10.30	11.11	96.89	8.64
Treatment	10,459	0.01	6.06	12.17	10.22	11.06	83.33	8.58
Average wage hired								
Control	4,660	0.01	5.77	11.74	9.93	10.64	96.89	8.42
Treatment	4,694	0.01	5.83	11.72	10.00	10.75	80.00	8.19

Notes: Opening level outcomes by treatment and control group.