Search, Screening, and Information Provision: Personnel Decisions in an Online Labor Market

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Abstract

Marketplaces such as online labor markets are often in a position to provide agents with public certified information to facilitate trade. I examine how employers on oDesk.com, the world's largest online marketplace, use public information in hiring. By experimentally varying employers' access to applicants' past wage rates, I demonstrate that market provided cheap-to-observe signals of quality are used by employers as substitutes for costly search and screening. I show that when employers are searching for someone low skilled then the provision of coarse information from the market is sufficient and employers will not pay a cost to acquire more information. When employers are looking for someone high skilled they will pay fixed screening costs to acquire information beyond what is provided by the platform. If the coarse information is not provided by the marketplace, then even employers looking for unskilled labor will pay to acquire more information. This leads to more matches and hiring quality workers at a lower price. However, the cost savings from identifying and hiring these low cost, but high quality workers does not outweigh the upfront cost of information acquisition.

JEL J01, J30, M50, M51; Personnel Economics, Firm Employment Decisions, Information Acquisition

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1 Introduction

Identifying high quality workers is one of the most important problems a firm faces. According to a survey by Silicon Valley Bank, 90 percent of startups believe finding talent is their biggest challenge.[1] In fact, talent acquisition is so important to the strategy of firms like Facebook, that its CEO, Mark Zuckerberg, once remarked that, "Facebook has not once bought a company for the company itself. We buy companies to get excellent people."

But, how do firms determine which potential employees are indeed "excellent people" The answer has to do with what information about job applicants is available and how costly it is to acquire more information. This paper studies the effect of publicly provided information in a marketplace on search, screening, and match outcomes. In every marketplace in which agents search for a match, there is some choice to engage in costly private search. Private search does not occur in a vacuum. It occurs in the presence of public information, which is cheap to acquire. How much effort is exerted in costly private search must depend on the provision of public information, which exists in the marketplace.

It is generally assumed that more information publicly provided within a market, the better matches would be. This paper, however, will question these assumptions by pointing out that agents strategically adjust their use of costly search to the provision of cheap-to-observe information in the market. Thus, the equilibrium amount of information an employer has when hiring an applicant may not be monotonically decreasing in the cost of obtaining match quality information from the marketplace. This because the equilibrium level of information depends both on market provided information as well as information acquired via costly search and screening.

More specifically, my focus will be on the impact of worker wage rate history in online labor markets.[2] I chose to study these online markets for two reasons: first, their importance and prevalence is growing.[3] Second, the particularities of these markets have allowed me to run an experiment that would be impossible in a traditional market. Identifying the causal impact of publicly provided information in a marketplace is problematic because, in general, the provision of this information is an endogenous choice of the marketplace. In oDesk.com, the online labor market that was studied, I was able to completely randomize which employers observed or did not observe coarse platform provided information. Furthermore, I am able to utilize the market's system of messages and reviews to delve into precisely what sorts of costly information acquisition employers chose to engage in as well as how satisfied they were with their hiring choices after the fact.

Traditionally, firms have relied on resumes and costly interviews to ascertain if a worker is a good fit for the firm. Increasingly, firms are turning to "big data" and workplace analytics to cheaply generate insights into candidates' potential productivity.[4] However, technology that provides agents with cheap but noisy signals reduces incentives to acquire more precise information and consequently might lead to worse decisions. Online labor marketplaces such as oDesk.com are playing an increasing role in the hiring process. Online labor marketplaces, even more so than the traditional labor market, seek to har-

[1] http://www.svb.com/News/Company-News/Looking-For-a-Job--Try-a-Tech-Startup/

[2] The major players in this industry are oDesk.com, Elance.com, Freelancer.com, and Thumbtack.com. In December of 2013, oDesk and Elance merged to form oDesk-Elance.com, but maintained separate online platforms until May 2015 when the company changed its name to upwork.com and released one unified platform. My experiment was run post-merger, but solely on the oDesk.com platform.

[3] The online market for labor is already an economically important source of labor supply and demand, and it's estimated to grow substantially in the coming years (Agrawal et al. (2013a)). The Economist (2013) predicted that combined, online labor markets will be worth more than $5 billion by 2018.

[4] https://hbr.org/resources/pdfs/comm/workday/workday_report_oct.pdf

ness the power of big data and algorithms to foster efficient matching.[5] These marketplaces are in the position to provide large quantities of standardized and verified information to facilitate hiring. For example, oDesk.com does not allow workers to delete ratings or comments provided by employers after a job is completed. This information is distinct from what workers include in their resumes, and thus, valuable to employers. Despite the general view that more information is better when it comes to bilateral matching with asymmetric information, some research has hypothesized limitations to the social gains of the digitization of the labor market. Autor (2001b), proposes that information about workers' attributes can be usefully grouped into low and high "bandwidth" varieties.[6] Low bandwidth data are objectively verifiable information such as education, credentials, experience, and salaries – the kind of information that is easily provided for employers through online marketplaces. High bandwidth data are attributes such as quality, motivation, and "fit," which are typically hard to verify except through direct interactions such as interviews and repeated contact. Hence, the comparatively low cost of obtaining applicants low bandwidth information might reduce incentives of employers to ascertain applicants' high bandwidth information.

The experiment I designed, which is described in detail in section 3.2, altered the information set available to a randomly selected group of employers by hiding all the previous hourly wage rates an applicant earned on the platform. Past wage rates are particularly useful in the screening process, as they provide a snapshot estimate of the worker's relative marginal product. No matter the competitive market environment, a worker's marginal product must be at least his wage, or no employer would pay that wage. A worker's current or past employer may be better informed about a worker's ability than the overall marketplace, and past wages provide at least a glimpse of what others think a worker's marginal product might be. In conventional labor markets, employers do not directly have access to an applicant's past wages unless the applicant chooses to disclose it. In this paper, I consider an online marketplace where the status quo is, and always has been, for employers to know the complete work history, including wages and earnings, for all applicants to their job. I observe how altering the publicly available set of "low bandwidth" information by removing applicants' past wage rates changed the employers' screening strategy and the result this has on match formation, wage negotiation, and job outcomes.

This paper has five main findings. First I find that when employers are unable to observe applicant's past wage rates they increase their search by viewing more applicants and increase screening by interviewing more applicants more deeply. Employers strategically react to having less information by actively acquiring information through a more costly, but perhaps more precise, source by taking time to ask the applicant more questions and get more information about his quality.

Second, I find that when employers are unable to observe applicants' past wage rates employers are more likely to fill a job posting. This may seem surprising because standard economic theory predicts that removing information from an employer, causes the employer to be less able to differentiate between candidates based on quality. However, if there is a fixed cost associated with acquiring information, then a reduction in the exogenous information provision does not necessitate a reduction in the equilibrium level of information. This is because employers substitute for the reduction in market provided information with even more information from costly interviewing.

Third, I show that employers who are unable to observe past wage rates interview and hire cheaper

[5] Horton (2013) looks at the effects of algorithmically recruiting applicants to job openings for employers, and Agrawal et al. (2013b) shows that verified work experience disproportionally helps contractors from less developed countries.

[6] Rees (1966) makes a similar distinction between formal and informal information networks. Formal networks include the state employment services, private fee-charging employment agencies, newspaper advertisements, union hiring halls, and school or college placement bureaus. Informal sources include referrals from employees, other employers, miscellaneous sources, and walk-ins or hiring at the gate.

but not observably worse candidates, and are more satisfied with the work output. This selection effect on wages completely dominates a small positive negotiation effect for workers. Hired applicants earn a higher percentage of their original bid.

Fourth, I provide evidence that although employers hire cheaper workers and are *ex post* more satisfied with the work output when they can not observe past wage rates, employers are not making a mistake by using platform-provided information when available. Using a difference-in-difference approach I show that treated employers that increased their use of costly intensive search during the experimental period reduced their use of costly search once the experiment concluded and they once again could observe past wage rate histories. This finding indicates that when employers have access to coarse but cheap information, it may be optimal for them to use this information to reduce upfront hiring costs. However, employers need to be aware that this upfront cost savings comes at a price, as they will likely pay slightly more for an equally skilled worker.

Fifth, I demonstrate that not all employers are affected by the loss of the past wage history signal equally. The treatment disproportionally affects the search and screening behavior of employers that are looking to hire workers with a lower level of expertise.

The remainder of the paper proceeds as follows: Section 2 briefly reviews the relavent literature and describes this paper's contributions; section 3 describes empirical context; section 4 presents the empirical findings; section 5 discusses the results; and section 6 concludes.

2 Literature Review

One of the central problems in the personnel economics literature on hiring is that that firms and workers cannot costlessly observe all relevant aspects of potential trading partners. This means that search is a common feature of hiring.[7] Traditional economic literature models the labor market as employees searching for wages, which are posted by the hiring firm.[8] Another much smaller branch of literature has focused on the demand side of the market. Barron et al. (1985) and Barron et al. (1989) study employer search by relating the number of applicants or interviews per employment offer and the time spent on recruiting and screening per applicant or per interview to characteristics of the vacancy and the employer. They argue that search along the extensive margin and search along the intensive margin are substitutes. In traditional models of search, a firm (or employee) acquires information simultaneously on the existence of an additional candidate (or job) as well as the value of the match. In fact, hiring procedures in the firm generally consist of two sets of activities. One set involves recruitment of applicants, while the second set involves screening and selection from among these applicants.

In contrast to most of the literature on hiring that takes a search theoretic approach, this paper follows Van Ours and Ridder (1992), which shows that most vacancies are filled by a pool of applicants formed shortly after search. I separate the questions of locating an additional candidate from ascertaining the match quality of a candidate. Thus, the important economic question is, which of the applicants to the job should the firm choose, or should the firm choose none of the available applicants at all.[9] In doing so, I deviate from much of the current literature, and choose to describe the firm's hiring decision as an information acquisition model where a firm must choose how much information to acquire about

[7] See Devine and Kiefer (1991) for a detailed summary of the search literature on hiring.

[8] For example, Stigler (1961), Mortensen (1970) and McCall (1970); see Mortensen and Pissarides (1999) for a detailed review of costly applicant search.

[9] Additional support for focusing on selection comes from Van Ours and Ridder (1993) which finds that employers spend far more time on selection than on search.

applicants before choosing whether to hire or to not hire an applicant.[10]

The economic literature on endogenous information acquisition usually assumes that agents acquire information when the value of information exceeds its cost (Arrow (1996); Grossman and Stiglitz (1980)). The information acquisition literature has largely focused on situations where externalities such as the public nature of information, or behavioral considerations lead to either an over or under investment in information acquisition.[11]

In contrast to the existing literature, which highlights inefficient investments in information, I focus on how the endowment of information to agents alters incentives to acquire more costly information. Building on evidence that the equilibrium level of information acquisition is fundamentally dependent on the market institutions, this paper empirically demonstrates the effects of the market provision of information on employer hiring strategies, including costly search and screening.[12]

The literature on determinants of hiring focuses on how either firm and job attributes or market attributes affect recruiting and screening behavior. Holzer (1996) argues that employers choose among the various recruitment and screening methods on the basis of their relative costs and benefits.[13] Barron et al. (1989) shows that employers tend to screen more applicants for more important positions. My paper is closely related to Barron et al. (1997), which also allows the firm to endogenously acquire information about the match quality of the applicant. Barron found that both applicants and vacancy characteristics strongly influence firms' search both at the intensive and extensive margin. My paper, in contrast, focuses on the effect of market provided information on the firms' information acquisition decision, and the interaction with firm and applicant characteristics.[14]

This paper also contributes to a growing literature that details the impact of screening technology on the quality of hires. While information technology and big data signals seem to suggest "efficiency" to many practitioners and academics, I find evidence that they can get in the way of costly but more informative practices, with fixed costs, like interviewing. Strategies discussed in the literature include the use of job testing, labor market intermediaries, and employee referrals. My paper is similar to Hoffman et al. (2015) in that it investigates the effects of verifiable information on employer hiring. Hoffman found that in addition to reducing costs, verifiable signals such as job testing can solve agency problems in hiring. They also show that applicants hired using on the job testing have longer tenure than those hired through traditional means. My finding, that public verifiable information can reduce incentives to conduct costly search adds an interesting confounding factor to their results, as it makes clear the complete costs of using job testing to solve agency problems.[15]

[10]See figure A.1 which confirms that the applicant pool is generally set before employers begin to message applicants.

[11]See Kübler and Weizsäcker (2004); Kraemer et al. (2006); Hoffman (2014) as a recent sampling of experimental literature which focuses on behavioral reasons for miss-allocation in information.

[12]Endogenous information acquisition has been used to analyze auctions (e.g., Milgrom and Weber (1982)), voting (e.g., Martinelli (2006); Persico (2004)), and medical patient decision-making (e.g., Kőszegi (2003)), among many other applications.

[13]Holzer (1987) was the first to detail an employer search model in which firms choose hiring procedures as well as reservation productivity levels. Fallick (1992) details how more expensive recruitment methods must provide more applicants and or better ones.

[14]There also exists a more macro focused literature including Burdett and Cunningham (1998) which advocate that the analysis of employers' search would be greatly improved if the market conditions the firm faced at the time of search could be taken into account. Russo et al. (2000) details how tightness of the labor market affects employer recruitment behavior. The procyclicality of on-the-job search is mainly driven by the increase in the availability of better jobs in Pissarides (1994) and Schettkat and für Sozialforschung (1995). Russo et al. (2001), adds to this literature by analyzing changes in recruitment behavior at the individual firm level at different points of the business cycle.

[15]See Autor and Scarborough (2008) for other recent work focusing on the effects of job testing on worker performance. Work by Autor (2001a); Stanton and Thomas (2012); Horton (2013) focuses on labor market intermediaries. Recent empirical work which focus on the effects of employee referrals include Burks et al. (2013); Brown et al. (2012); Pallais and Sands (2013).

Finally, the paper seeks to tie hiring strategy directly to firm outcomes by measuring the causal impact of employer's use of public information on costly search and the effect of that costly search on employer satisfaction. Recent theoretical literature in management has highlighted the critical role of individuals in creating and sustaining competitive advantages (Abell et al. (2007); Teece (2007)).[16] Understanding firm-level recruitment and hiring strategies may help explain persistent firm-level conditional differences observed in profitability (Oyer and Schaefer (2011); Blasco and Pertold-Gebicka (2013)). My paper details the importance of costly search which enables an employer to assess applicants high bandwith information and hire cheap, high quality employees.

3 Empirical Context

During the past ten years, a number of online labor markets have emerged. In these markets, firms hire workers to perform tasks that can be done remotely, such as computer programming, graphic design, data entry, research, and writing. Online labor markets differ in their scope and focus, but common services provided by the platforms include maintaining job listings, hosting user profile pages, arbitrating disputes, certifying worker skills, and maintaining feedback systems. The experiment reported in this paper was conducted on oDesk, the largest of these online labor markets.

On oDesk, employers write job descriptions, self-categorize the nature of the work, and choose required skills for jobs posted to the oDesk website. Workers learn about vacancies via electronic searches or email notifications, and can submit applications to any publicly advertised job on the platform. After a worker submits an application, the employer can interview and hire the worker on the terms proposed by the worker or make a counteroffer, which the worker can accept or reject, and so on.

In 2014, employers spent $900 million on wages through oDesk. The 2013 wage bill was $285 million, representing 200% year-on-year growth from 2013. As of October 2014, more than 3.5 million employers and 9.7 million contractors have created profiles, though a considerably smaller fraction are active on the site. Approximately 2.8 million job openings were posted in 2014. See Agrawal et al. (2013a) for additional descriptive statistics on oDesk.

Based on dollars spent, the top skills in the marketplace are technical skills, such as web programming, mobile applications development (e.g., iPhone and Android), and web design. Based on hours worked, the top skills are web programming, data entry, search engine optimization, and web research. The top 5 countries for oDesk employers are: the United States, the United Kingdom, France, Germany, and Israel. Top top 5 countries for oDesk workers are: the United States, Philippines, Russia, Bangladesh, and the United Kingdom.

There has been some research which focuses on the oDesk marketplace. Pallais (2014) uses a field experiment to show hiring inexperienced workers generates information about their abilities. She further shows that because workers can not compensate firms for this information, inexperienced workers may be underemployed. Stanton and Thomas (2012) use oDesk data to show that agencies (which act as quasi-firms) help workers find jobs and break into the marketplace. Agrawal et al. (2013b) investigates which factors matter to employers in making selections from an applicant pool, and presents some evidence of statistical discrimination; the paper also supports the view of employers selecting from a more-or-less complete pool of applicants rather than serially screening.

[16] Also see Barney (1991); Hall (1993); Reed and DeFillippi (1990); Peteraf and Barney (2003); Coff and Kryscynski (2011).

3.1 Transacting on oDesk

The process for filling a job opening on oDesk is qualitatively similar to the process in conventional labor markets. First, a would-be employer on oDesk creates a job post and chooses whether to make it public or private. Public jobs can be seen by all workers on the platform, while private jobs only invited applicants can see. Employers choose a job title and describe the nature of the work. Additionally, employers choose a contractual form (hourly or fixed-price), specify what skills the project requires (both by listing skills and by choosing a category from a mutually exclusive list), and specify how much experience they want applicants to have. Employers also estimate how long the project is likely to last. Once the job posting is written, it is reviewed by oDesk and then posted to the marketplace.

Once posted to the marketplace, would-be job applicants can view all this information. Additionally, oDesk also presents verified attributes of the employer, such as their number of past jobs, average wage rate paid. Applicants can apply to any public job posting on the platform. When they apply, they include a bid, and a cover letter. After applying, the applicant immediately appears in the employer's "applicant tracking system" or ATS, with their name, picture, bid, self-reported skills, and a few pieces of oDesk-verified information, such as total hours-worked and their average feedback rating from previous projects (if any). Figure 1 shows the default view to an employer of the ATS prior to viewing the detailed information contained in the job application. By default, employers only view applicants for the job who are predicted to be a good fit using oDesk's proprietary machine-learning algorithms. To view a complete list of applicants the employer must click on the "Applicant" tab on the right side of the screen.

Employers can click on an applicant's limited listing to view their full profile, which has that applicant's disaggregated work history, with per-project details on feedback received, hours worked and wage rate earned on all past jobs. Figure 2 shows an employer's view of an applicant's disaggregated work history after expanding or viewing an application.

Although all job applications start with the worker applying to a job opening, not all of these applications are initiated by the worker. As in conventional labor markets, oDesk employers may choose to actively recruit candidates to apply for their jobs. Upon completing a job posting employers are shown a pool of 10 applicants who report having the skills requested for the posted job. Employers are given the option of inviting some or all of these applicants to apply to the job. Additionally, the employer can search on his own for some skill or attribute they are looking for in candidates. The search tools on oDesk will return lists of workers, and will contain information about that worker's past work history. If they choose, the employer can then "invite" a worker they are interested in. These recruiting invitations are not job offers, but rather invitations to apply to the employer's already-posted job opening.

Only 36% of employers choose to recruit applicants on jobs posted on oDesk. Of employers that choose to recruit, on average 3 out of 4 recruited applicants are recruited from the oDesk provided pool of applicants shown to all employers after posting a job. Of course, these recruited applicants are not required to apply to the job opening—about half do apply. These "recruited" applicants and organic applicants (applicants who are not recruited) both appear in the employer's ATS. Employers are free to evaluate candidates at any time after they post their job.

If the employer hires a candidate via oDesk, oDesk mediates the relationship. If the project is hourly, hours-worked are measured via custom tracking software that workers install on their computers. The tracking software, or "Work Diary," essentially serves as a digital punch clock.

The oDesk marketplace is not the only marketplace for online work (or IT work more generally). As such, one might worry that every job opening on oDesk is simultaneously posted on several other online labor market sites *and* in the traditional market. However, survey evidence suggests that online and offline hiring are only very weak substitutes and that posting of job openings on multiple platforms is relatively rare. For example, Horton (2010) found limited evidence of multiple postings when comparing jobs posted on oDesk and its largest (former) rival, Elance.

3.2 Description of the Experiment

In September 2014, I conducted an experiment on oDesk that altered the information set available to employers by hiding applicants' past wage rates. All employers, both new and experienced, operating on the oDesk platform were eligible for the experiment, and were randomized for the duration of the experiment into either a treatment or control group when they posted their first job during the experimental period. Once an employer was assigned to the treatment group, the treatment affected all aspects of the site. Thus, past wage information was completely hidden on worker's profiles both in search results and when viewing the detailed profile after clicking on an applicant in the ATS. Table 1 shows that randomization was effective and the experimental groups were well balanced.

Figure 3 shows the changes implemented both in search as well as in the employer's ATS for treated employers. When employers click an applicant's limited listing and view their full profile, which has that applicant's disaggregated work history, the information presented to the employers allocated to the treatment group differs from that presented to the set of control employers. Specifically, when viewing the per-project details both the number of hours worked on the job as well as the hourly wage rate earned on any job was removed. Employers allocated to the control group can view both the price, p, and applicants worked for as well as the quantity of work preformed, q. Employers allocated to the treatment group can only observe the $p * q$ or the total earnings on each job.

When reviewing past work history, which according to a February, 2015 client survey was the second most important piece of information in hiring after the bid amount, employers can only observe the title of the job, the total earnings on the job, and any feedback left (if available) by the past employer. Other than the lack of job specific past wage information, there were no other changes in the user interface or in information available between treated and control employers. Treated employers still know the number of past jobs the applicants worked, the total number of hours on the platform the applicants worked, as well as the applicants' total past earnings. Thus, it is possible for employers to calculate applicants' average past wages, but not applicants current wage rate.

3.3 Overview of the Data

Over the course of the experiment, 2948 employers were assigned to the control group and posted a total of 4661 job openings. 2975 employers where assigned to the treatment group and posted 4815 job openings. Table 2 presents summary statistics of the baseline hiring behavior on oDesk.com.[17] Beginning at the top of the hiring funnel and proceeding downwards, it is clear employers narrow down the set of applicants until they arrive at the candidate(s) they wish to hire. On average, 35 applicants apply to each job posting on oDesk. On average 1.3 of these applicants are invited to apply to the job by the employer leaving 33.6 applicants who apply to a job without being invited. I refer to these applicants as organic applicants.[18] On average, employers only view about 7 of the applications submitted to the job by organic applicants. It is here that employers become much more selective, choosing only to send messages to about 2 applicants on average. Employers specifically ask at least one question to about 60%

[17] To show baseline hiring behavior, I show average statistics for the control group of employers.

[18] For the analysis on screening, I look only at the messaging behavior of organic applicants. When an applicant is invited, oDesk automatically begins a message thread between the applicant and the employer. Thus, it becomes difficult to identify invited applicants the employer actually screens.

of the applicants they message, or about 1 applicant on average. Finally, in order to conduct "face-to-face" interviews, about half of applicants who are messaged are asked for a Skype ID by the employer. On average, this hiring process leads to about 40% of job openings posted on oDesk being filled.

4 Empirical Results

The first question I seek to answer using this experiment is, how do employers shift their hiring strategies to make use of publicly available low bandwith information such as applicants past hourly wage rate? Hiring can conveniently be separated into search and screening. Employers search to identify possible candidates, and then screen those candidates to identify his quality or fit for the job. Table 3 presents differences in mean search and screening measures across treatment groups.

To measure employer search, I track whether an employer "views" an application by clicking to expand the applicant's application within the applicant tracking system. Prior to "viewing" an application an employer only sees an applicant's bid, country, average feedback score, and total hours worked. Thus, "viewing" an application is a strong signal of interest in a candidate. Treated employers on average view another 0.4 applications from a baseline of 6.7 applications per opening. While this results seems small on a per job basis, there are over one hundred thousand jobs posted per month. On a per month basis, being unable to observe applicants past wages leads employers to considering an additional twenty thousand applicants, who previously were not even in the employers consideration set.

Employers on oDesk acquire information through a costly screening process. This process involves messaging an applicant using the platform provided messaging system. Employers message applicants, and are messaged back by applicants. All of the messages back and forth between one employer and one applicant are considered a message-thread. In these messages, employers ask applicants questions, and/or exchang Skype IDs in order to set up a "face-to-face" meeting with the applicant. The second panel of Table 3 looks at four measures of employer screening or information acquisition behavior: (i) the number of applicants "messaged" by the employer by formally contacting the applicant for further discussion; (ii) the number of applicants an employer "questions" as evidenced by use of a question mark in at least one message; (iii) the number of applicants and employer "questions" as evidenced by use of one or more question words: who, what, where, when, why, or how; and (iv) the number of applicants an employer "Skypes" by asking the applicant to exchange Skype IDs. Table 3 shows that employers that are unable to observe applicants' past wage rates increase the number of applicants they question as evidenced by both the number of applicants messaged that include a question mark, as well as the number of applicants messaged that include a question word. Employers ask questions to an additional 0.15 applicants per job opening. The raw differences in mean counts does not report any statistical difference in the number of applicants messaged or the number of applicants asked for Skype IDs.

Since the four measures of screening magnitude are all rough proxies for information acquisition behavior, I use a principal component analysis to create an aggregate measure of screening behavior. This measure explains 75% of the variation in the 4 measures. Figure 4 shows that the difference in the means between the first principal component is significantly different between employers who could and could not observe applicants' past wage history.

I collected the data from a randomized experiment; therefore the simple correlations reported here can be interpreted as causal relationships. However, to further allay concerns about omitted variables, and to potentially increase precision, in the next sections I estimate the effect of hiding applicants past wages on search and screening in a multivariate regression framework.

4.1 Employer Search

Due to the count nature of the outcome variable, I follow, Budish et al. (2015) and show estimates from a quasi-maximum likelihood Poisson model with heteroskedasticity-robust standard errors. The regressions in this section are derived from a version of the following model:

$$\text{VIEWED}_j = \beta_0 + \beta_1 \text{TRT}_j + \text{EMPLOYER COVARIATES}_j + \text{OPENING COVARIATES}_j + \epsilon_j \tag{1}$$

In table 4, the dependent variable is VIEWED_j which is the number of applicants viewed by the employer on job opening, j. TRT_j is an indicator for treatment assignment of the employer of job opening, j. Model (1) shows results with no covariates, Model (2) adds both employer and job opening covariates, which include: job subcategory dummies, the number of prior jobs filled, the employers past spending, the number of applications to the job, the number of recommended applicants to the job, and the average bid by applicants on the job, and if the employer specifically requested a particular skill for the job.

Treated employers view 1.07 times the number of applications from a baseline average of 7 applications per opening in the control group.[19] After observing an applicant's application and noticing that wage rate history information which was previously used in ascertaining that applicants quality is missing, employers view additional candidates.[20]

4.1.1 Characteristics of Searched Applicants

In addition to changing the number of applicants employers search for and screen, not being able to observe applicants' past wage history could alter the characteristics of the applicants employers choose to view, message, and hire. According to a 2015 client survey on oDesk.com, the top 3 features, in order of importance, used when making a decision over who to contact and eventually hire are: the applicant's hourly rate, the applicant's feedback rating (specifically does the applicant have "good enough" feedback), and the applicant's experience. Thus, I will analyze three main groups of applicant characteristics: wage characteristics, experience characteristics, and feedback characteristics.

Before treatment and control group employers choose to view an application, they can only observe the applicants' basic information. Thus, when deciding to view or not view an application, the information set is identical for both treatment and control employers. Therefore, I do not expect the treatment to have any effect on the characteristics of viewed applicants. It is useful to view table 5 as a placebo test, which confirms balance in the experiment.

4.2 Employer Screening

The differences in means presented above suggest that removing employers' ability to observe applicants' past wage rates increase screening. The addition of controls and structure by analyzing the data using quasi-maximum likelihood Poisson model with heteroskedasticity-robust standard errors confirms these results. The regressions in this section are derived from a version of the following model:

$$\text{Y}_j = \beta_0 + \beta_1 \text{TRT}_j + \text{EMPLOYER COVARIATES}_j + \text{OPENING COVARIATES}_j + \epsilon_j \tag{2}$$

[19] the coefficient on TRT from the poisson model is interpreted as a the difference in the logs of expected count of viewed application, thus, I interoperate the coefficient as being associated with viewing exp(.069)=1.07 times as many applicants.

[20] Table A.1 in Appendix B shows the results of log-linear regression models are similar to those of the quasi-poisson models.

In column (1) of Table 6, the dependent variable, Y_j, is the number of applicants messaged in job, j. Controlling for job opening characteristics as well as employer characteristics, employers in the treatment group message 1.08 times the number of applicants as control employers. The mean number of applicants messaged per job is 1.7. Thus, employers message an additional 0.14 applicants messaged per job, or over 21 thousand additional applicants per month.

In column (2), the dependent variable, Y_j, is the number of applicants messaged where Skype IDs are exchanged in job, j. Controlling for job opening characteristics as well as employer characteristics, employers in the treatment group ask 1.08 times the number of applicants for Skype IDs as control employers. This result is not significant at a 10% level, but is still consistant with the other results presented on employers screening behavior of applicants.

In column (3) the dependent variable, Y_j, is a count of the number of applicants messaged in job, j, where at least one message exchanged with the employer contained a question mark.[21] A question mark is taken as evidence of the employer asking the applicant a question, and thus, evidence of seeking additional information about that applicant. Controlling for the number of applicants messaged, treated employers use question marks in 1.15 times as many message-threads as control employers.

In column (4) the dependent variable, Y_j, is a count of the number of applicants messaged in job j, where at least one message contains at least one of the following question words: who, what, where, when, why, or how. This results confirms that employers are increasing the number of applicants questioned substantially per job. Here, a coefficient on the treatment indicator of .178 means that treated employers use at least one question word in 1.19 times the number of message-threads than control employers per job opening.

Taken all together, these four results in Table 6, as well as the results of the principle component analysis provide evidence that removing past wage information does not only cause employers to widen their choice set and contact more applicants, but to also attempt to get more information from the applicants they contact via message.[22]

4.2.1 Characteristics of Screened Applicants

Does altering the information publicly available to employers alter employers' preferences over characteristics of the applicants? I compare the observable characteristics of applicants who are messaged along three main dimensions: their wages, their experience, and their feedback.

The top panel of Table 7 indicates that treated employers choose to message applicants with bids that are on average 6% lower than the group of applicants messaged by control employers. These are applicants who clearly value their work product lower as evidenced by a lower profile wage rate and lower historical average accepted wages. There is some slight evidence that employers also seem to message applicants who have less work experience as indicated by a 10% reduction in the average dollar amount of previous hourly earnings, and a 15% reduction in fixed price earnings of the applicants messaged. However, this is not due to having worked fewer hours, but having been willing to previously work for a lower wage. These do not seem to be applicants who are newer to the platform or applicants with

[21] Using a count of total question marks over all messages sent in a job opening instead of a count of message-threads with question marks gives similar results. I report a count of message-threads to maintain consistency with the specification in column (1) and column (2). The alternate specifications are located in the appendix.

[22] In Appendix B, in Table A.4, I present the results of application level regressions these regression indicate that the odds of asking a question conditional on sending a message is higher for treated employers than control employers. However, not significantly so in all specifications. Interestingly, the results on odds of asking for a skype ID are a tightly measured zero indicating that employers have a tendency to ask either all or none of the applicants they message for skype IDs.

substantially worse work experience. Additionally, messaged applicants do not differ on their historical feedback scores. These results provide evidence that employers that are unable to use an applicants' past wages as a signal of quality seem to locate a subset of applicants who are substantially cheaper to an employer without being observably worse applicants as measured by market level signals.

4.3 Hiring Outcomes

I turn now to analyzing effects of employers substituting from cheap platform-provided information to information obtained through costly search and screening on the probability of a job being filled, the wage negotiation which occurs after a contract is offered, the wages paid on the job, and the employers' *ex post* job satisfaction.

4.3.1 Probability of Hiring

According to a 2013 oDesk client survey, the primary reason for not hiring a worker for an open job posting is that the employer "couldn't identify a contractor with the right skills." An employer will only fill a job when he can adequately identify an applicant who is is a proper match for the position at a wage the employer is willing to pay. The treatment unambiguously reduces the number of available signals the employer can use to identify if an applicant has the right skills. Thus, according to standard search theory I would presume that fewer matches would occur. However, as an employer's ability to ascertain quality is diminished, his incentives to acquire information increase. Employers that are unable to observe past wage information increase the number of applicants they view, the number of applicants they message, and the number of applicants from whom they acquire additional information. Thus, in equilibrium, the effect of hiding past wages may actually increase the precision of an employer's signal of an applicants' quality, and the probability of hiring an applicant for an open job posting. Table 8 shows that the treatment increases the probability of hiring.

The regressions in this section are derived from a version of the following linear model:

$$\mathrm{Y}_j = \beta_0 + \beta_1 \textsc{Trt}_j + \epsilon_j \tag{3}$$

The independent variable in Table 8 is $\textsc{AnyHired}_j$ is an indicator for whether the employer made a hire on job, j, and $\textsc{Trt}_j$ is an indicator for treatment assignment on job, j. In column (1) the sample is limited to only the first job posting by each employer after the start of the experiment. Further job postings were dropped from this specification to prevent against possible, but highly unlikely, employer selection into additional jobs posted to the platform. The coefficient on the treatment indicator is positive and highly significant, with the treatment increasing hiring by about 3 percentage points, from a baseline hire rate in the control group of only 40%. This is a percentage increase of 7%, which is extremely high and economically important to a platform that generates revenue by taking a percentage of contractor earnings. Column (2) includes both employer and job opening level covariates and shows that adding covariates has little effect on the outcome of the regression. Column (3) adds job postings that are not an employer's first during the course of the experiment. An indicator variable for the order of the job posting is included and standard errors are clustered at the employer level. Running the fill rate regression on the full sample only slightly reduces the coefficient from 0.029 to 0.026 and increases the precision of the estimates slightly. Adding covariates in column (4) increases precision slightly and has little effect on the magnitude of the coefficients. Removing a signal of an applicants' relative marginal value does not reduce the employer's ability to identify and hire quality applicants. In fact, the opposite is true, removing a cheap-to-observe signal of applicant quality increases the probability of a job being filled substantially.

4.3.2 Wage Negotiation and Wages

Employers who are unable to observe workers' past wage rate history message workers who bid nearly 10% lower than the workers messaged by control employers, but the overall effect on wages is ambiguous as the treatment may also affect bargaining. A survey by Hall and Krueger (2010) found that only about 30% of workers report that there was some bargaining in setting the wage for their current job. The bargaining rate is especially low for blue collar workers (5%) but much higher for knowledge workers (86%). On oDesk, at least 14% of jobs on the platform participate in some type of wage bargaining prior to signing a contract, as evidenced by agreeing to a contract wage that is not equal to the winning applicants' wage bid.[23]

Once an employer has chosen an applicant, hiding that applicant's past wage rate may alter negotiations over pay with the employer. Table 9 shows that there is a small but positive affect on wages, which comes from workers being offered contract on which they make a higher percentage of their bid. Column (1) reports an estimate of the regression:

$$\log(\textsc{wage to bid ratio}_{ja}) = \beta_0 + \beta_1 \textsc{Trt}_{ja} + \epsilon_{ja} \tag{4}$$

Where $\textsc{wage to bid ratio}_{ja}$ is calculated as the ratio of the wages paid to the winning applicant to the hourly bid submitted by the winning applicant in assignment, a, which came from job posting j. An assignment is the oDesk specific word for job once both parties have signed the contract. In this model, there is always only one assignment to each job posting, as I limit the analysis to only the first job assignment which is created from a job posting. I subset the data in this way, as negotiation effects on follow-up job assignments, which stem from an job posting in the experiment, cannot be directly attributable to hiding of the wage history of applicants for certain employers. A WAGE TO BID RATIO of 1 means the employer paid the employee exactly the employee's bid amount. WAGE TO BID RATIO below 1 indicates that the employer is paying the employee an amount less than the employee's bid. The coefficient on the treatment indicator is positive and highly significant, with the treatment increasing the wage to bid ratio by about 1.2%, from a baseline ratio of 0.973.

Table 10 demonstrates that despite the positive effect on negotiations, there is an overall negative effect of the treatment on wages. Employers who are unable to observe applicants' past wage rate history pay wages which are 10% lower. These results indicate the the treatment has two very interesting affects on wages. Firstly, there is a very large negative selection effect. Employers who are unable to observe applicants' past wage rates screen more and more deeply, and identify cheaper workers. There is also a positive information rent gained by the hired workers, as employers no longer know the applicants' past wage rate history and thus agree to pay the chosen applicant a higher percentage of his bid.

4.3.3 Feedback

Employers in the treatment group are hiring cheaper workers, but not workers who appear worse in measurable quality *ex ante*. Thus, employers that do not know past wage rate information tend to get their work completed cheaper relative to employers that make hiring decisions conditional on applicants' past wage rate histories. Although treatment employers do not choose applicants that are *ex ante*

[23] This is a lower bound estimate, as its possible that there wage negotiation, but the amount settled on was exactly the contractors bid. Getting data on wage negotiation and delving into this phenomenon is beyond the scope of this paper.

of lower quality than those chosen by control employers, the applicants true quality is still unknown. Thus, I turn my attention to determining if applicants hired through the fundamentally different search process caused by removing past wage rate information do a better job completing the job.

Specifically, conditional on an employer leaving feedback on the hired applicant, do treated employers leave different feedback than control employers? Table 11 shows there is no measurable difference in public feedback left between treatment and control employers, but that employers that could not view an applicants' past wage rates leave better private feedback on the first job they hired from a job opening. The regressions in this section are derived from a version of the following model:

$$\log(\text{FEEDBACK VARIABLE}_{ja}) = \beta_0 + \beta_1 \text{TRT}_{ja} + \text{CONTROLS}_{ja} + \epsilon_{ja} \mid \text{FEEDBACK LEFT}_{ja} \tag{5}$$

where the sample is again limited to only first job openings posted by each employer and only the first of the assignments spawned by that job opening. The reason the analysis is limited to only the first assignment spawned from a job opening, is that including follow-up assignments bias the results as employers are more likely to create a follow up assignment when the worker is of high quality. Controls for the employers's prior experience, the employer's prior experience with the hired worker, and category indicators are added to the both models.

In column (1), FEEDBACK VARIABLE is the publicly viewable 1 to 5 star feedback rating that an employer can leave for a worker on assignment a from job opening j after the job is complete, TRT_{ja} is an indicator for treatment assignment of assignment a, which came from job posting j. The coefficient on the treatment indicator in column (1) is not significant, which indicates that employers that hire without knowing an applicant's past wage history do no leave better or worse feedback than employers that know this information.

There has been substantial research, including Nosko and Tadelis (2015) and Horton and Golden (2015) that details the limits of public reputation in platform markets. On oDesk, both employers and workers also have the option of leaving private feedback which is never displayed on the platform. They are told that this feedback will only be used internally by oDesk.

In Column (2), FEEDBACK VARIABLE is equal to a 0-10 rating, which is viewable only to oDesk. The coefficient on the treatment indicator is positive and significant. Employers that were not able to view applicants' past wages when hiring leave feedback scores that are 5% higher than the score left by employers that hired applicants with knowlage of their of their past wage rate history.

4.4 Are Employers making a Mistake?

The evidence presented in the previous sections demonstrates that when employers cannot observe past wage rate history they choose to acquire information about applicants' quality though more costly intensive search. This intensive search allows employers to better identify, at an upfront cost, high quality low cost employees, which can have long-term positive effects on firm outcomes. Thus, should employers ignore market provided low bandwidth information and rely on costly screening all the time?

To assess this, I take advantage of the fact that I observe the behavior of treated and control employers after the experiment was concluded, and look for persistent treatment effects. I plot the difference in mean levels of screening for treatment and control employers for each employer's first job posted during the experimental period, and each employers first job posted after the experimental period ended. The top panel of Figure 5 shows that employers search more than control employers during the experimental period. The bottom panel of Figure 5 shows that there is no difference in the screening behavior of employers who were treated during the experiment and employers who where not treated during the experiment once the experiment was completed.

I also examine the impact of the treatment on treated employers in the post period using difference-in-difference methodology. The models presented are quasi-maximum likelihood Poisson regression with heteroskedasticity-robust standard errors:

$$\text{Y}_j = \beta_0 + \beta_1 \text{Trt}_j + \text{Post Period}_j + \text{Trt x Post Period}_j + \text{Employer Covariates}_j + \text{Opening Covariates}_j + \epsilon_j \tag{6}$$

Where Y_j is one of the measures of costly intensive search used previously on job j. Table 12 shows that employers for which past wages were hidden reduce their use of costly search in their first job posted after the experiment ended. These results are consistent with a story that indicates that when employers have access to imprecise signals of worker quality they take advantage of these signals and use them instead of other more costly information acquisition strategies. However, it is extremely important to understand that these results are generated in a market for task-based labor where the long-run effects of hiring a slightly less skilled employee or an employee at a slightly higher cost are minimized relative to traditional labor markets due to lower costs of firing and shorter work durations.

4.5 Heterogenous Screening Effects

There is no one strategy that fits all when it comes to screening strategies. Generally, personnel economics assumes there are complementarities between certain firms and certain employees, such that firms should tailor their hiring to attract the employees that generate the most match specific productivity.[24] On oDesk, jobs range from data entry to complicated legal work, and the quality and experience of workers who compete to complete these jobs ranges from novice to expert. When posting a job opening on oDesk, the platform offers employers the opportunity to choose if they would like to see more beginner, intermediate, or expert workers. This expressed preference over contractor type gives insight into how important a high quality worker is to the employer on each job opening.

Figure 6 shows the differences in screening levels for treatment and control employers separated by the requested expertise level of the applicants. This split sample indicates that employers who are interested in hiring an expert level worker do not increase their level of costly search when the employer cannot observe applicants past wage rate history. This result is further confirmed by use of an interaction model that looks for differential treatment effects on intensive search by preferred contractor tier using using a quasi-maximum likelihood Poisson regression with heteroskedasticity-robust standard errors:

$$\text{Y}_j = \beta_0 + \beta_1 \text{Trt}_j + \text{Contractor Tier}_j + \text{Trt x Contractor Tier}_j + \text{Employer Covariates}_j + \text{Opening Covariates}_j + \epsilon \tag{7}$$

The results of Table 13 indicate that the treatment significantly increases the level of costly intensive screening for employers that are looking for beginner level workers. Employers that cannot observe past wage rate history and are looking to hire beginner applicants exchange Skype IDs with 1.25 times as many applicants as employers that can observe past wage history and are interested in hiring beginner applicants. The significan negative coefficient on the interaction term for "TRT x Expert" indicates that the treatment effects are significantly different for employers looking to hire experienced workers than for employers looking to hire beginner level workers. The effects on employers who are interested in hiring intermediate expertise workers are smaller than the effects on employers hiring for beginner jobs, but still significantly different from zero. Removing workers past wage rate history only alters employer screening behavior for those employers not interested in hiring expert level workers.

[24] The assumption of such a complementarity underlies the large literature on assortive matching in labor markets (see Rosen (1982) and Sattinger (1993)).

4.6 Robustness Check

If the employer were to have other high quality signals of applicant type available, such as having previously observed the applicant's productivity, I would expect the employer to rely on this information and, thus, hiding past wage information would have no effect. In addition to posting public jobs on oDesk, if an employer wishes to work with a specific applicant, he is able to create a private job posting. Only applicants expressly invited by the employer may apply to private job postings. In the sample, private job postings have a median number of applicants of 1. As such, the employers usually have a much deeper knowledge of the quality of the applicants to private job postings. Table 14 presents the results of running the main screening regression subsetted to include only private jobs. The coefficients on the TRT_j indicator are not significantly different than zero for all 4 measures of costly screening, demonstrating that there is no treatment effect on screening for private jobs.

5 Discussion

I find that when employers on oDesk are unable to observe workers past wage rates, they seek to acquire more information through costly search and screening. This increase in acquired information allows employers to fill more jobs and hire applicants who are cheaper but not worse on other observables. These employers also report being more satisfied with the work output. However, even after observing that acquiring information through costly search and screening leads to more and better matches, employers return to lower levels of search and screening behavior post experiment. I show that these results are primarily driven by employers that are looking to hire workers with lower level of experience.

This pattern of results is consistent with a particular story of information acquisition in markets where costly search and screening is available. Specifically, the results indicate that there is a fixed price to acquiring information through search and screening. If the employer is searching for someone low-skilled then the provision of coarse information from the market is good enough and the employer is not going to pay a fixed cost to acquire more information. If the employer is looking for someone high skilled he may be willing to pay the fixed cost to acquire more information about the applicants. If coarse information is not provided by the marketplace, then even when the employer is looking for someone unskilled he is willing to pay the fixed cost and acquire more information. This leads to the employer hiring a cheaper worker and being *ex post* more satisfied. But is the employer's increased satisfaction enough, that they were previously making a mistake by relying on platform provided information when looking for low skilled workers?

The evidence in this paper suggests that employers who increase their search and screening behavior when they are unable to observe past wage rates decrease their use of costly search and screening when the experiment is completed and they can once again observe workers' past wage rates. This result indicates that employers do not make a mistake by relying on platform provided information at least when hiring beginner level workers.

To get a better perspective on why employers that want to hire unskilled workers choose to rely on platform-provided information instead of costly screening although they know this will lead to hiring a more expensive worker, I conduct some back of the envelope calculations.

Figure 7, shows that employers hiring a beginner applicant expect to pay substantially less than those intermediate or advanced applicants. The average job that hires a beginner level contractor costs about \$500 compared to about \$1700 for hiring an expert level contractor. Based on the finding that employers who cannot observe applicants' past wage rates offer contracts to applicants that bid about 9.5% lower, I can estimate that costly screening saves an employer hiring a beginner applicant about \$50 on a job in wages. This is compared to about \$170 in wages when hiring for a job that is interested in expert level workers. While estimating the upfront cost of increased search and screening behavior is a bit more difficult, I do know from survey data contained in Figure 8 that 80% of employers spend fewer than 8 hours on search and screening. Most likely the costs of hiring are not linear. It seems fair to assume that a majority of hiring cost come not on posting or viewing, or messaging candidates on platform, but on organizing and screening the candidates off platform. Thus, it seems logical that employer prefers to pay an extra \$50 in wages and save an hour or two in time upfront costs, but might not be willing to pay an extra \$170 in wages to save a similar amount of time.

6 Conclusion

I designed and implemented an experiment that seeks to understand how employers on oDesk.com make use of publicly available, large quantities of standardized and verified information. This information is usually not available to employers in the traditional labor market. When employers do not have the ability to observe an applicant's past wage history, they substitute for this informational loss by exerting costly effort to acquire more information about candidates though interviewing. This strategic reaction to a reduced information environment actually leads employers to be more likely to fill an open job posting and to hire cheaper candidates. This treatment effect is limited to employers that are looking to hire low and intermediate level workers.

It is important to note the limitations of the analysis in this paper. One very important limitation of my analysis is that all firms in this market are hiring task-based labor. Thus, incentives to locate a top notch employee are lower than in the traditional labor market, since both the expected tenure of an employee and the firing costs are much lower than in the traditional labor market. Additionally, my experiment studies the effects of removing information, not adding information. Firm institutions are extremely important in hiring. For example, some firms recruit every year at a fixed set of colleges regardless of changes in academic rankings. Perhaps, basing hiring decisions on past wage history is as much a function of tradition as optimal information use. If this were the case, I would expect to observe a larger treatment effect when compared to adding past wage history information to a market that traditionally did not have this information.

My results are potentially relevant for understanding under which circumstances firms might seek to use online labor marketplaces. Online labor marketplaces can only reduce asymmetric information, when the of signals they provide are useful in matching. For example, if a firm is interested in hiring highly skilled labor, my findings suggest that the market provided signals are of little use. Instead, the firm must rely on costly screening by acquiring information about applicants. This might make an online labor platform a less attractive option for this type of labor, as one of the platform's main advantages, reduced hiring costs, cannot be fully harnessed. On oDesk, costly screening of applicants by asking questions or conducting Skype interviews is generally associated with more experienced employers. Less experienced employers may be relying too heavily on market-provided signals, which could reduce the quality of their matches and slow their access to the platform. By removing market provided signals like past wage rate information, the platform may be able to "push" first time users of the platform into relying less on the vague market provided signals and more heavily on costly techniques such as asking questions and conducting Skype interviews.

Finally, my results have more general implications about the relationship between coarse public information and costly private information acquisition. Firms need to consider the effects of providing their hiring agents with coarse data especially when long and short term incentives of agents and firms are not completely aligned. Hiring agents may use this data as a substitute for more costly but more precise information accession strategies. This increased use of coarse information might help to explain persistant discrimination towards underrepresented populations. These populations often appear to have lower coarse signals, but might be identified as diamonds in the rough if screened using more costly intensive interviewing.

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A More Details on Transacting on oDesk

Figure A.1 shows the timing of application, interviews and hiring decisions relative to the time the job was posted. Clearly most applications are submitted soon after the job is posted with the probability of an application being posted dropping steadily over time. Interviewing and hiring follow a bimodal distribution. Employers begin interviewing candidates shortly after the job is posted and then make job offers. Those who can not come to an agreement with their first choice applicant then interview some more and make a 2nd round of offers.

When an employer "views" and application by clicking on it, the application is expanded and the first thing shown below the bid information already observable on the unexpanded list of applicants is the applicant's cover letter and the text responses to any questions posed in the job posting. Figure A.2 shows an example of this information. Directly below this is the desegregated "work history" of the applicant which was shown above (in text) in figure 2.

B Search and Screening

In this section I explore a series of robustness checks and additional specifications related to the effects of hiding past wage rate information on search and screening behavior.

Table A.1 uses a log-linear model to rerun the results provided in Table 4. The quasi-poisson model is the preferred specification as only 37% of jobs skype any applicants, and this model better adjusts for the number of zeros which skew the distribution of the data.

Table A.2 uses the preferred quasi-poisson specification to show that the results on information acquisition are similar regardless of the dependent variable used as a rough proxy for the amount of intensive information acquisition the employer engaged in.

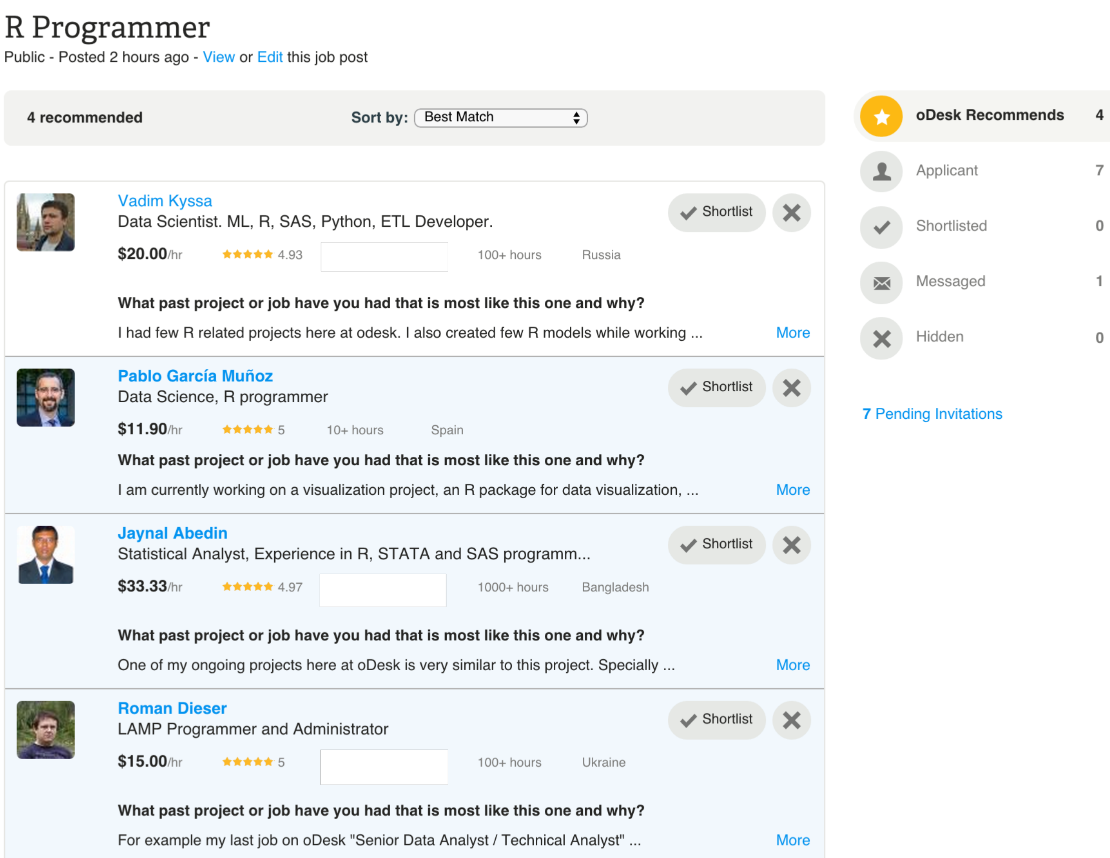
As a further robustness check, Table A.3 shows that running Table 6 using a log-linear specification instead of a quasi-poisson regression does not materially effect the direction of the results.

Table A.4 presents application level regressions, which indicate that the odds of asking a question conditional on sending a message is higher for treated employers than control employers. However, not significantly so in all specifications. Interestingly, the results on odds of asking for a skype ID are a tightly measured zero indicating that employers have a tendency to ask either all or none of the applicants they message for skype IDs.

C Job Outcomes

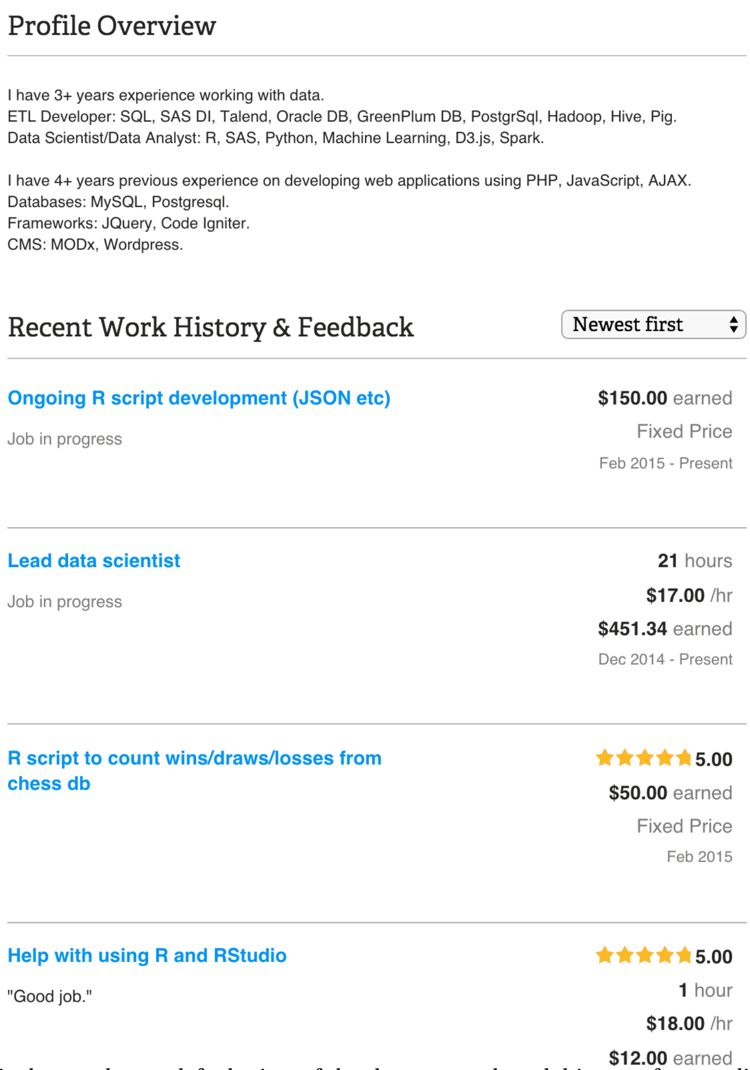
Figure A.3 plots the distribution of wage to bid ratios that are not equal to one for the first contract signed for each treatment and control employer after the start of the experiment. Some employees manage to negotiate wages which are higher than their initial bid amount. Interviews with workers on oDesk reveal, that for top level talent, it is not unheard of to use other offers to attempt to negotiate up the contract wage relative to the bid. Additionally, Figure A.3 allows us to conclude that the treatment effect does not appear to be driven by outliers such as bidding only one cent on a job opening.

Figure 1: Default View of the Applicant Tracking System (ATS)



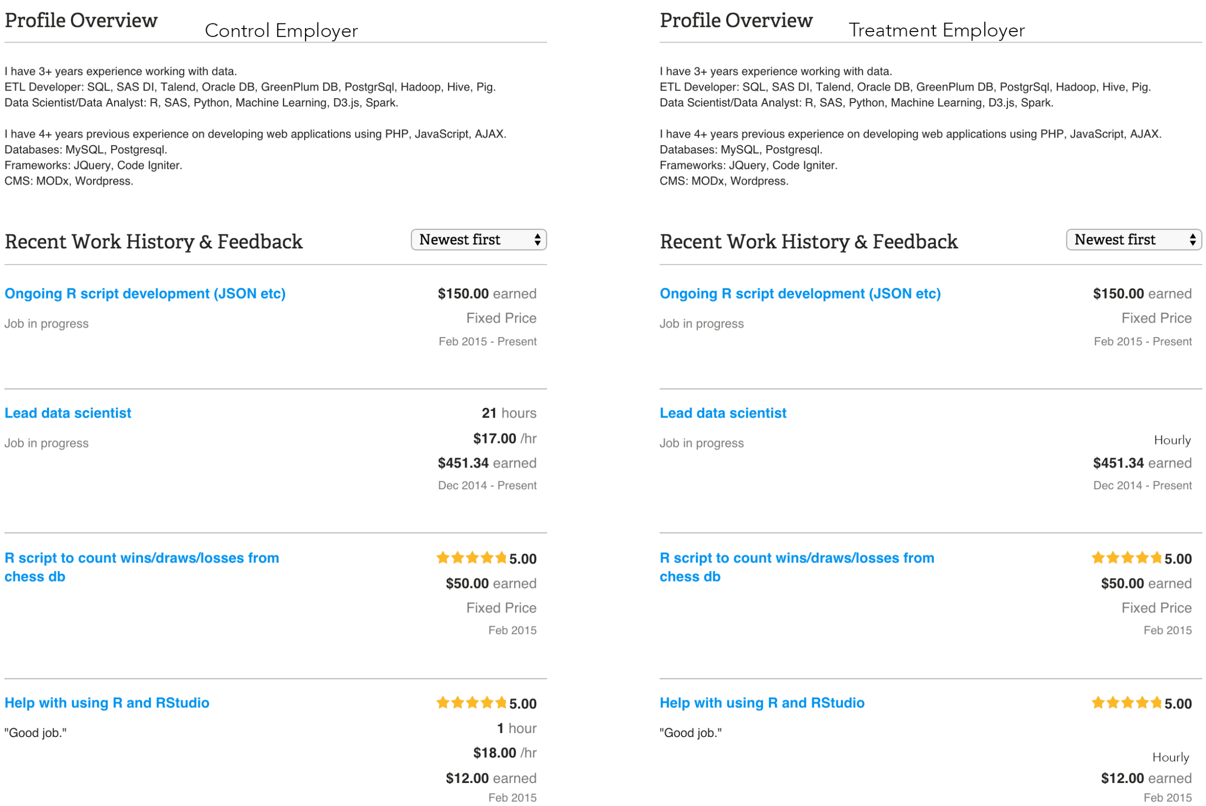
Note: This is the default listing of applications as observed by the employer after posing a job and having applicants apply. Only applicants recommended by oDesk's proprietary matching algorithm are displayed by default. Notice there are 7 total applications submitted at the time of the screenshot but only 4 are displayed by default. Employers can directly contact applicants from this page, directly hire applicants from this page, or they can click on a listing to expand it and view the applicants' complete application and work history. At the time of the experiment Job Success Score was not displayed. This feature was added later

Figure 2: Expanded View of Disaggregated Work History



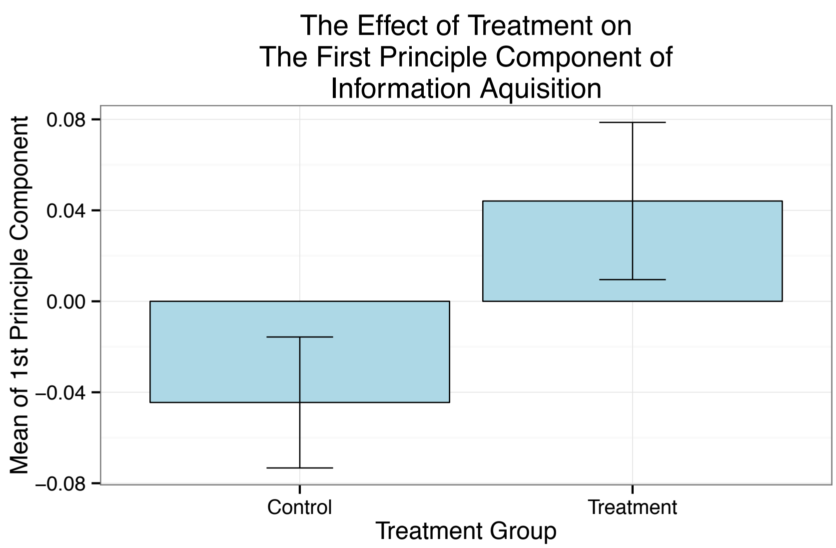
Note: This is the employers default view of the desegregated work history of an applicant which is viewable after expanding (viewing) an application. In the treatment group, the 21 hours and $17.00/hr would be hidden for the "lead data scientist" job. Only the $451.34 would be observable. The employer also sees an expanded profile (see A.2 in appendix **??**).

Figure 3: Changes to the UI for treated employers.



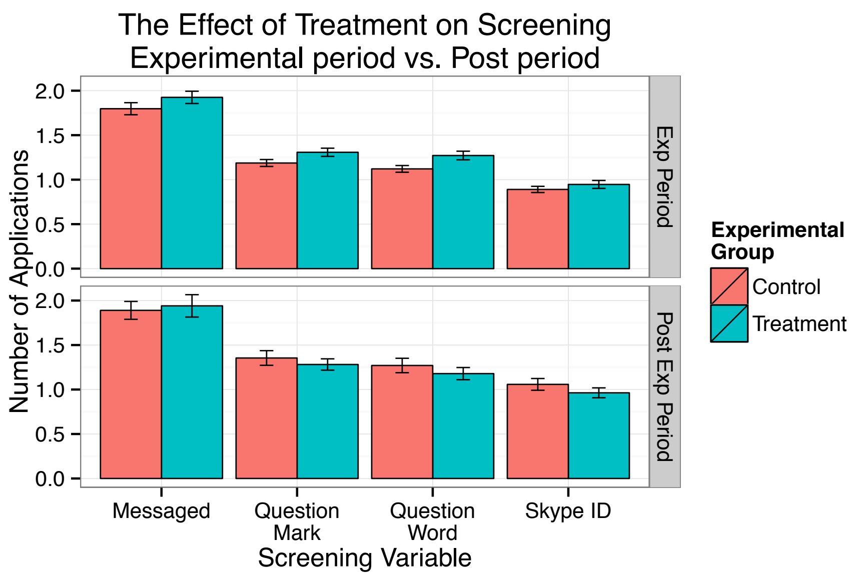
Note: This screen shot details the changes made to workers profile pages.

Figure 4: First Principle Component of Screening



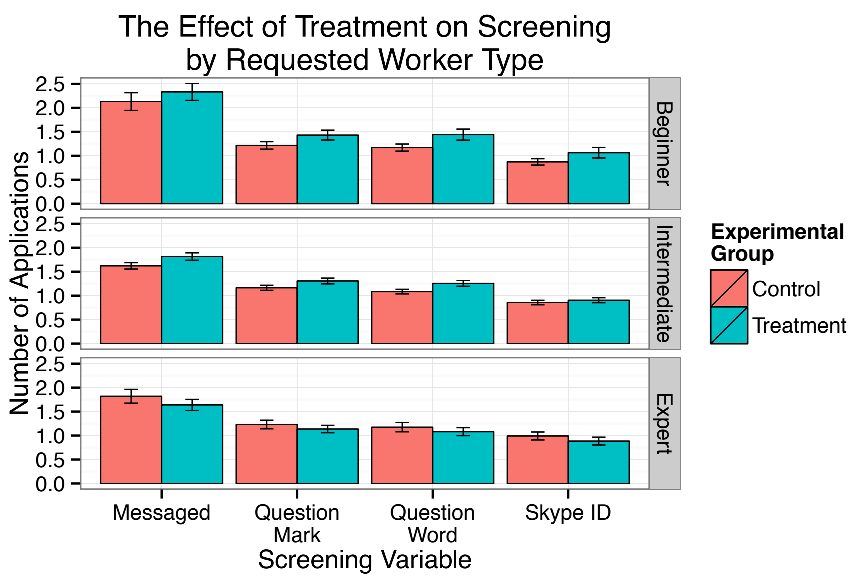
Note: The analysis was limited to the first job posting after the start of the experiment which were hourly job postings. The 4 inputs to the principle component analysis are: number of applicants messaged, number of applicants asked a question as indicated by use of a question mark, number of applicants asked a question as indicated by use of a question word, and the number of applicants asked for a skype id.

Figure 5: Screening by Experimental Period



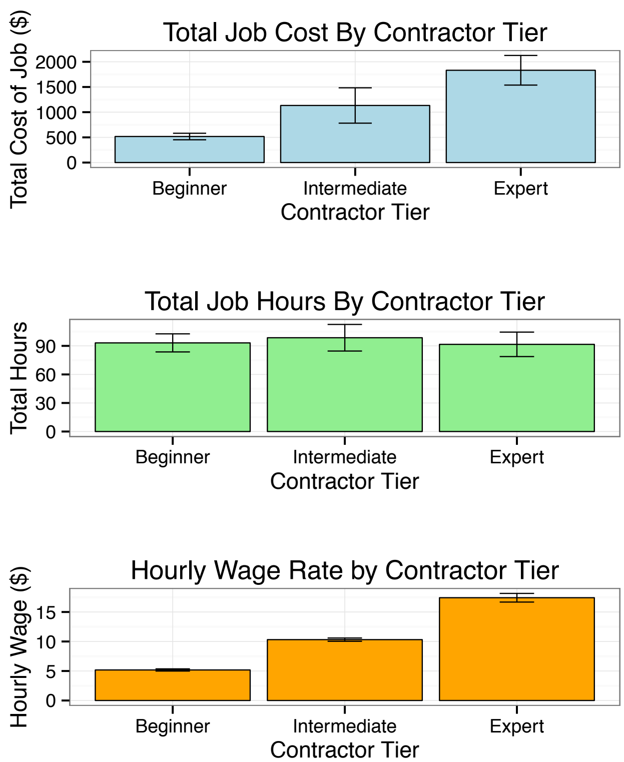
Note: This figure shows difference in raw counts and standard errors of screening outcomes between treatment and control employers separated by the jobs posted during the experimental period and directly after the experiment ended.

Figure 6: Screening by Requested Contractor Tier



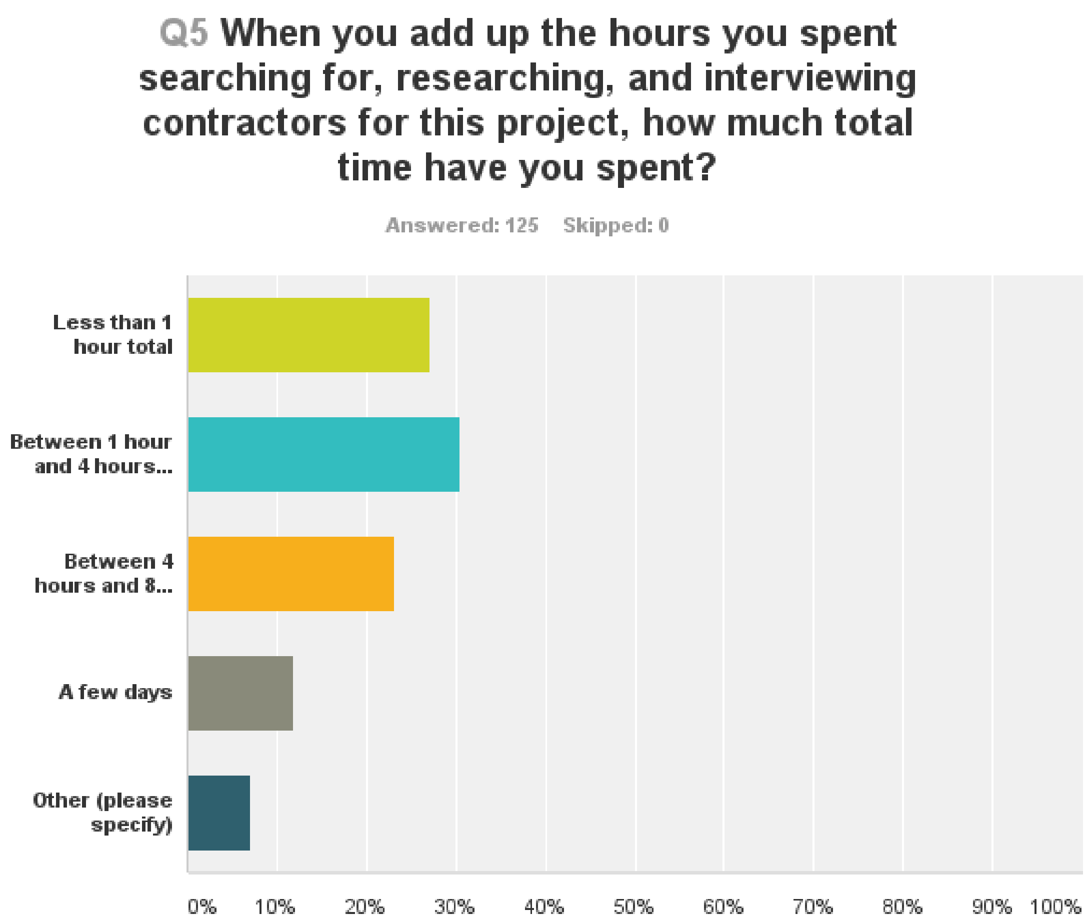
Note: This figure shows difference in raw counts and standard errors of screening outcomes between treatment and control employers separated by the skill level of requested applicants.

Figure 7: Job Characteristics by Contractor Tier



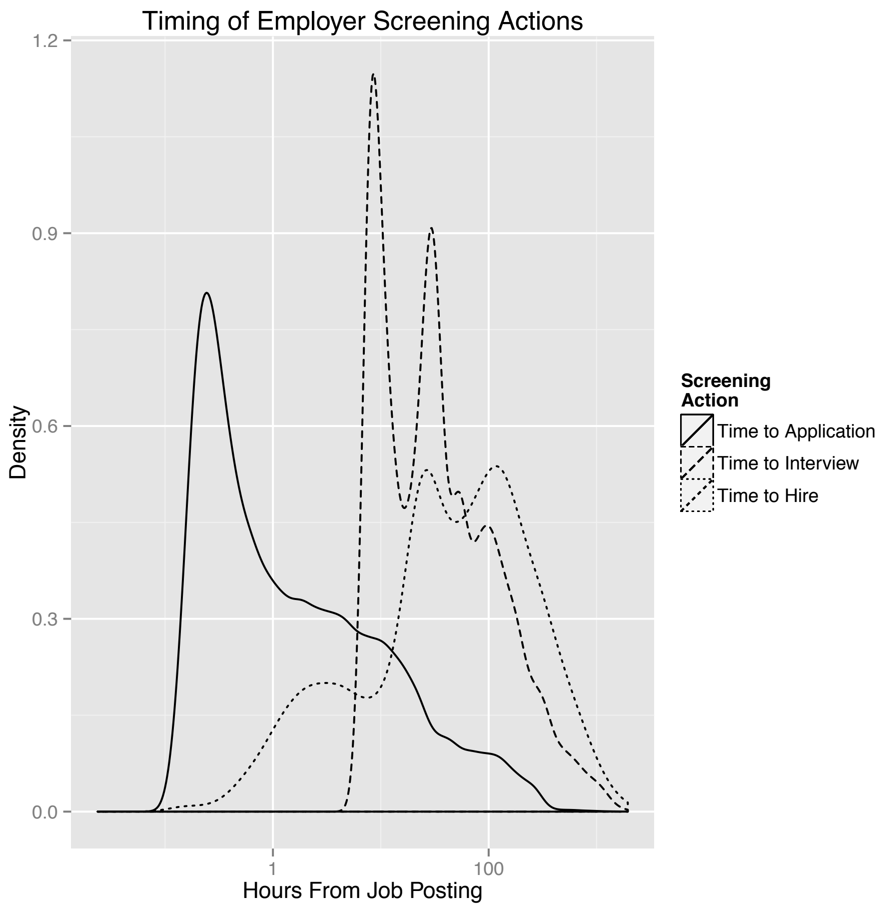
Note: This figure is limited to hourly job postings, that filled and billed more than $0.01.

Figure 8: Interviewing Survey



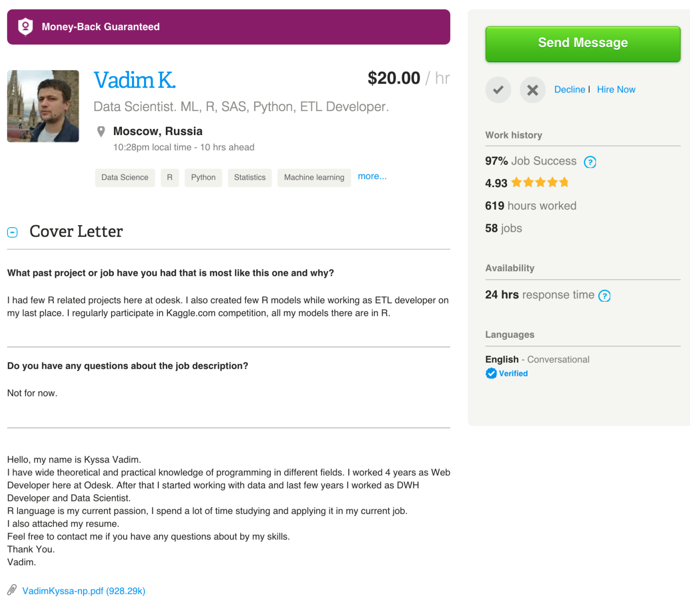
Note: This data came from a 2013 oDesk client survey

Figure A.1: PDF of Timing of Screening Behavior



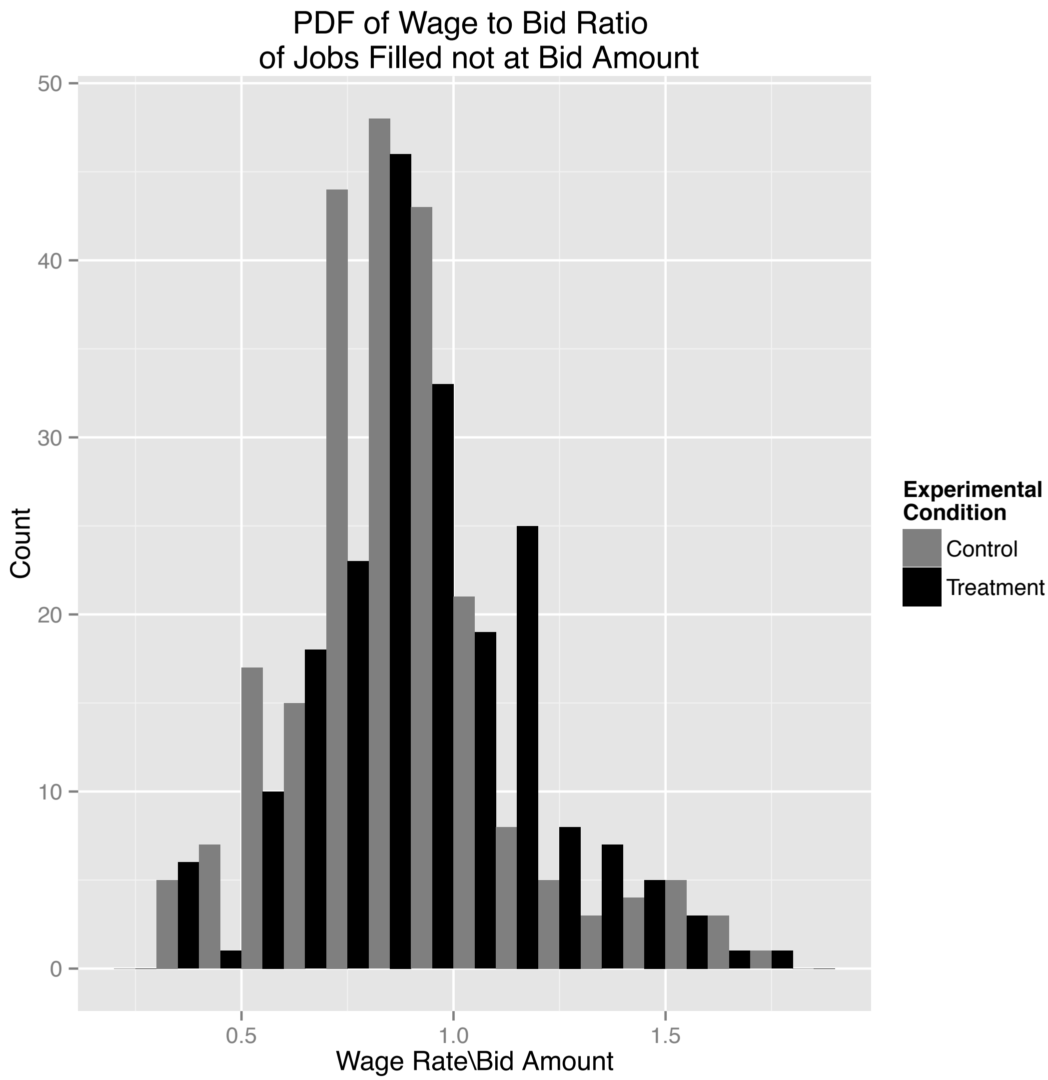
Note: This figure is limited to only non-invited applicants to public job postings.

Figure A.2: Expanded View of ATS Application



Note: At the time of the experiment Job Success Score was not displayed. Upon expanding an application, the employer also sees an expanded profile See A.2 in appendix **??**.

Figure A.3: Distribution of the Non-equal Wage to Bid Ratio by Treatment Group



Note: This table plots the distributions on Wage-to-Bid ratio for treatment and control jobs. Wage-to-bid ratio is calculated as the ratio of the wages paid to the winning applicant to the hourly bid submitted by the winning applicant in that job. The top and bottom .5% of wage to bid ratios were dropped. All bid to wage ratios equal to one were dropped.

Table 1: Balance Table of Employer, Job Posting, and Applicant Characteristics

	Control mean: $\bar{X}_{CTL}$	Treatment mean: $\bar{X}_{TRT}$	Difference In Means	p-value	
Employer Attributes					
Prior Job Postings	23.584	24.172	0.588 (1.312)	0.654	
Prior Billed Jobs	10.767	11.415	0.648 (0.633)	0.306	
Prior Spend by Employers	5880.736	6293.257	412.521 (483.769)	0.394	
Num Prior Contractors	10.966	11.888	0.921 (0.806)	0.253	
Avg Feedback Score of Employer	4.811	4.785	-0.026 (0.016)	0.097	†
Num of Reviews of Employer	8.155	8.923	0.767 (0.721)	0.287	
Job Posting Attributes					
Number non-invited Applicants	33.618	33.474	-0.144 (1.088)	0.894	
Avg Best Match Score	0.355	0.358	0.003 (0.004)	0.396	
Avg Bid	12.768	12.605	-0.163 (0.241)	0.498	
Prefered Experiance in Hours	33.816	34.233	0.416 (3.397)	0.902	
Estimated Job Duration in Weeks	17.208	16.909	-0.299 (0.548)	0.585	
Applicant Attributes					
Tenure in Days	670.913	664.847	-6.066 (5.684)	0.286	
Hours Worked to Date	808.262	785.033	-23.229 (19.039)	0.222	
Num Past Jobs Worked	17.342	17.469	0.127 (0.357)	0.721	
Past Hourly Earnings	6500.266	6213.784	-286.481 (199.042)	0.150	
Past Fixed Wage Earnings	1120.839	1092.754	-28.085 (36.861)	0.446	
Num Prior Employers	13.726	13.792	0.066 (0.263)	0.802	
Min Feedback Rating	3.185	3.187	0.002 (0.013)	0.871	
Avg Feedback Rating	4.594	4.589	-0.005 (0.004)	0.215	
Max Feedback Rating	4.936	4.930	-0.005 (0.002)	0.027	*
Wage Bid	10.500	10.337	-0.164 (0.261)	0.531	
Profile Wage	10.609	10.408	-0.201 (0.235)	0.393	
Min Hr. Wage (6 months)	7.499	7.208	-0.291 (0.183)	0.112	
Avg Hr. Wage (6 months)	9.072	8.801	-0.271 (0.210)	0.198	
Max Hr. Wage (6 months)	11.243	11.048	-0.194 (0.253)	0.442	

Notes: This table reports means and standard errors across experimental groups of employer, job posting, and applicant characteristics. The Top Panel reports characteristics of employers allocated to treatment and control. The middle panel reports characteristics of job postings by treatment and control groups for the first job posted after allocation to the experiment for each employer. The bottom panel reports characteristics of employers at the time they were allocated to treatment or control groups. The bottom and top 1% by average historical wage were dropped. Reported p-values are the for two-sided t-tests of the null hypothesis of no difference in means across groups. In the bottom panel, standard errors are clustered at the employer level. Significance indicators: $p \leq 0.10$:† ,$p \leq 0.05 : *$, $p \leq 0.01 : **$ and $p \leq .001 : ***$.

Table 2: Baseline Screening Behavior

Statistic	N	Mean	St. Dev.	Min	Median	Max
Number of Applicants	2,948	35.105	43.296	0	22	639
Number of Organic Applicants	2,948	33.691	43.036	0	20.5	639
Number of Applications Viewed	2,948	7.321	9.257	0	5	122
Number of Organic Applicants Messaged	2,948	1.797	3.684	0	1	91
Number of Organic Applicants Questioned	2,948	1.121	2.050	0	0	36
Number of Organic Applicants Skyped	2,948	0.890	1.890	0	0	23
Number of Hires	2,948	0.580	1.039	0	0	26
Pct of Jobs Filled	2,948	0.403	0.491	0	0	1

Notes: This table provides baseline (control) statistics of hiring on oDesk. The statistics reported are for first job openings of employers assigned to the control group. Organic Applicants are applicants who were not invited to apply to the job.

Table 3: Search and Screening Behavior

	Control mean: $\bar{X}_{CTL}$	Treatment mean: $\bar{X}_{TRT}$	Difference In Means	p-value	
Measures of Search					
Num. Viewed Applications	6.671	7.122	0.451 (0.242)	0.062	†
Measures of Screening					
Num. Messaged Applicants	1.797	1.925	0.128 (0.097)	0.188	
Num. Skyped Applicants	0.890	0.946	0.056 (0.057)	0.322	
Num. Questioned Applicants (Q Word)	1.121	1.271	0.150 (0.062)	0.015	*
Num. Questioned Applicants (Q Mark)	1.187	1.308	0.120 (0.060)	0.046	*

Notes: This table reports means and standard errors across experimental groups for the number of applicants searched, and intensely screened by treatment and control group. The top panel reports 1 measure of search behavior, and the bottom panel reports 4 measures of screening behavior. Reported p-values are the for two-sided t-tests of the null hypothesis of no difference in means across groups. Significance indicators: $p \leq 0.10 :\dagger$,$p \leq 0.05 : *$, $p \leq 0.01 : **$ and $p \leq .001 : ***$.

Table 4: Effect of Hiding Past Wages on Search

	Dependent variable:	
	Num Viewed	
	Poisson	Poisson
	(1)	(2)
Hidden Wages Treatment Group (TRT)	0.065^{*}	0.069^{**}
	(0.035)	(0.034)
Constant	1.898^{***}	1.773^{***}
	(0.025)	(0.134)
Opening Level Covariates	No	Yes
Observations	5,922	5,855

Notes: The sample is restricted to hourly first job posts by an employer. Models (2) include covariates including: category indicators, prior jobs billed by the employer, employers prior spendings on the platform, the number of applications to the job openings, the number of recommended applications to the job opening, and the average applicants bid, and an indicator if requested skills. Significance indicators: $p \leq 0.10 : *, .p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table 5: Characteristics of Viewed Applicants

	Control mean: $\bar{X}_{CTL}$	Treatment mean: $\bar{X}_{TRT}$	Difference In Means	p-value
Viewed Applications				
Bid Amount	12.585	12.345	-0.240 (0.344)	0.485
Profile Wage Rate	12.564	12.338	-0.226 (0.315)	0.474
Avg 6 Month Wage	10.624	10.228	-0.396 (0.284)	0.163
Min 6 Month Wage	8.660	8.271	-0.388 (0.246)	0.114
Max 6 Month Wage	13.241	13.031	-0.210 (0.369)	0.569
Previous Hours Worked	899.176	932.298	33.122 (31.199)	0.288
Prior Billed Openings	22.725	22.916	0.192 (0.663)	0.772
Previous Hourly Earnings	8016.728	8344.449	327.721 (336.873)	0.331
Previous FP Earnings	1615.918	1565.433	-50.485 (73.387)	0.492
Avg Feedback	4.661	4.660	-0.000 (0.006)	0.969
Min Feedback	3.234	3.241	0.007 (0.021)	0.747
Max Feedback	4.960	4.959	-0.001 (0.003)	0.831

Notes: This table reports outcome means and standard errors across experimental groups of applicants who are viewed by employers. The treatment group are employers who are unable to observe past wage history information of the applicants. The unit of randomization was employer. The standard error for the difference in means is in parentheses next to the estimate. Reported p-values are the for two-sided t-tests of the null hypothesis of no difference in means across groups. Standard errors are clustered at the employer level. Significance indicators: $p \leq 0.10 :\dagger$,$p \leq 0.05 : *$, $p \leq 0.01 : **$ and $p \leq .001 : ***$.

Table 6: Effect of Hiding Past Wages on Information Acquisition

	Dependent variable:			
	Number of Apps Messaged	Number of Apps Skyped	Number of Apps with "?"	Number of Apps with Question Words
	Poisson	Poisson	Poisson	Poisson
	(1)	(2)	(3)	(4)
Hidden Wages Treatment Group (TRT)	0.085*	0.078	0.114**	0.145***
	(0.051)	(0.059)	(0.048)	(0.051)
Constant	0.607***	−0.627**	0.010	0.028
	(0.176)	(0.260)	(0.208)	(0.224)
Opening Level Covariates	Yes	Yes	Yes	Yes
Observations	5,855	5,855	5,855	5,855

Notes: This table shows the relationship between measures of information acquisition and the treatment. The level of observation is the job posting. Estimates are from quasi-maximum likelihood Poisson models. Heteroskedasticity-robust standard errors are reported. The sample is restricted to hourly first job posts by an employer. The DV in Model (1) is a count of the number of applicantion which where messaged. The DV in Model (2) is a count of the number of applications that included the word "skype" in a message with the employer. The DV in Model (3) is a count of the number of applications that exchanged messages including a question mark with the employer. The DV in Model (4) is a count of the number of applications that included at least one of the following question words: Who, What, Where, When, Why, or How. All models include covariates: category indicators, prior jobs billed by the employer, employers prior spending on the platform, the number of applications to the job openings, the number of recommended applications to the job opening, the average applicants bid, and if the employer requested a specific skill. Significance indicators: $p \leq 0.10 : *$, $.p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table 7: Characteristics of Messaged Applicants

	Control mean: $\bar{X}_{CTL}$	Treatment mean: $\bar{X}_{TRT}$	Difference In Means	p-value	
Messaged Applicants					
Bid Amount	13.964	13.119	-0.845 (0.512)	0.099	†
Profile Wage Rate	13.490	12.741	-0.748 (0.443)	0.091	†
Avg 6 Month Wage	11.697	10.874	-0.823 (0.405)	0.042	*
Min 6 Month Wage	9.375	8.677	-0.698 (0.341)	0.041	*
Max 6 Month Wage	14.912	13.957	-0.955 (0.549)	0.082	†
Previous Hours Worked	1189.007	1167.145	-21.861 (56.595)	0.699	
Prior Billed Openings	30.631	29.023	-1.607 (1.254)	0.200	
Previous Hourly Earnings	11607.165	10348.016	-1259.150 (627.746)	0.045	*
Previous FP Earnings	2347.145	1985.281	-361.863 (136.299)	0.008	**
Avg Feedback	4.713	4.708	-0.004 (0.008)	0.620	
Min Feedback	3.200	3.227	0.027 (0.035)	0.441	
Max Feedback	4.978	4.973	-0.005 (0.004)	0.181	

Notes: This table reports outcome means and standard errors across experimental groups of applicants who are messaged. The treatment group are employers who are unable to observe past wage history information of the applicants. The unit of randomization was employer. The standard error for the difference in means is in parentheses next to the estimate. Reported p-values are the for two-sided t-tests of the null hypothesis of no difference in means across groups. Standard errors are clustered at the employer level. Significance indicators: $p \leq 0.10$:† ,$p \leq 0.05 : *$, $p \leq 0.01 : **$ and $p \leq .001 : ***$.

Table 8: Effect of Hiding Past Wages on Job Fill Rate

	Dependent variable:			
	I(Job Filled)			
	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
Hidden Wages Treatment Group (TRT)	0.029^{**}	0.027^{**}	0.026^{**}	0.023^{**}
	(0.013)	(0.013)	(0.012)	(0.011)
Constant	0.403^{***}	0.392^{***}	0.404^{***}	0.384^{***}
	(0.009)	(0.061)	(0.009)	(0.055)
Job Order Dummy	No	No	Yes	Yes
Opening Level Covariates	No	Yes	No	Yes
Employer Level Covariates	No	Yes	No	Yes
Observations	5,922	5,855	9,476	8,973

Notes: The results are limited to hourly job posts. Columns (1) and (2) are limited to first job postings by employers. Columns (3) and (4) use the full sample with standard errors clustered at the employer level. Columns (2) and (4) add covariates including: category indicators, prior jobs billed by the employer, employers prior spendings on the platform, the number of applications to the job openings, the number of recommended applications to the job opening, and the average applicants bid. Significance indicators: $p \leq 0.10 : *, .p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table 9: Effect of Treatment on Hired Worker Wage Negotiation

	Dependent variable:
	log(Wage to Bid Ratio)
Hidden Wages Treatment Group (TRT)	0.012^{*}
	(0.006)
Constant	-0.027^{***}
	(0.005)
Observations	1,500

Notes: The sample is restricted to assignments originating from an hourly first job post by an employer that hire exactly 1 applicant. The top and bottom .5% of ratios were dropped Significance indicators: $p \leq 0.10 : *$, $.p \leq 0.05 : **$ and $p \leq .01 : * * *$.

Table 10: Effect of Hiding Past Wages on Hourly Wage Paid

	Dependent variable:
	log(Wage Rate)
Hidden Wages Treatment Group (TRT)	-0.102^{**}
	(0.046)
Constant	2.165^{***}
	(0.033)
Observations	1,514

Notes: The sample is restricted to hourly job posts by an employer. The sample is further restricted to first job posts by an employer which hired exactly 1 applicant. Heteroskedasticity-robust standard errors are reported. Significance indicators: $p \leq 0.10 : *, .p \leq 0.05 : **$ and $p \leq .01 : * * *$.

Table 11: Effect of Treatment on Job Feedback Score

	Dependent variable:	
	log(Public Feedback)	log(Private Feedback)
	(1)	(2)
Hidden Wages Treatment Group (TRT)	0.012	0.056^{*}
	(0.018)	(0.032)
Constant	1.427^{***}	2.180^{***}
	(0.058)	(0.067)
Employer Level Covariates	Yes	Yes
Observations	1,042	1,212

Notes: The sample is restricted to assignments originating from an hourly first job post by an employer. The sample includes only public job openings. The sample is further limited to include only the first job post spawned from each assignment. Covariates included are category indicators, the total value of the job, the total number of hours of the job, and the number of prior jobs the employer and worker completed. Significance indicators: $p \leq 0.10 : *$, $.p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table 12: Effect of treatment on Intensive Search
By Experimental Period

	Dependent variable:		
	Number of Apps Skyped	Number of Apps with "?"	Number of Apps with Question Words
	(1)	(2)	(3)
Hidden Wages			
Treatment Group (TRT)	0.079	0.112^{**}	0.142^{***}
	(0.060)	(0.048)	(0.051)
Post Treatment Period	0.232^{***}	0.169^{**}	0.175^{**}
	(0.077)	(0.074)	(0.077)
TRT x Post Period	-0.202^{*}	-0.176^{*}	-0.247^{**}
	(0.109)	(0.100)	(0.109)
Constant	-0.782^{***}	0.015	0.036
	(0.243)	(0.180)	(0.196)
Opening Level Covariates	Yes	Yes	Yes
Observations	7,920	7,920	7,920

Notes: Estimates are from quasi-maximum likelihood Poisson models. Heteroskedasticity-robust standard errors are reported. All models include covariates: category indicators, prior jobs billed by the employer, employers prior spending on the platform, the number of applications to the job openings, the number of recommended applications to the job opening, the average applicants bid, and a skill requested indicator Significance indicators: $p \leq 0.10 : *$, $.p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table 13: Effect of Hiding Past Wages on Information Acquisition
By Requested Contractor Tier

	Dependent variable:			
	Number of Apps Messaged	Number of Apps Skyped	Number of Apps with "?"	Number of Apps with Question Words
	Poisson	Poisson	Poisson	Poisson
	(1)	(2)	(3)	(4)
Hidden Wages Treatment Group (TRT)	0.109	0.225^{*}	0.184^{*}	0.219^{**}
	(0.112)	(0.121)	(0.097)	(0.101)
Intermediate Contractor	-0.186^{**}	−0.019	−0.046	−0.091
	(0.086)	(0.096)	(0.077)	(0.077)
Expert Contractor	−0.032	0.130	0.025	0.009
	(0.108)	(0.114)	(0.096)	(0.104)
TRT x Intermediate	0.025	−0.153	−0.051	−0.049
	(0.127)	(0.144)	(0.117)	(0.120)
TRT x Expert	−0.190	-0.344^{**}	-0.248^{*}	-0.278^{*}
	(0.153)	(0.169)	(0.136)	(0.149)
Constant	0.647^{***}	-0.646^{**}	0.034	0.085
	(0.207)	(0.273)	(0.226)	(0.239)
Opening Level Covariates	Yes	Yes	Yes	Yes
Observations	5,754	5,754	5,754	5,754

Notes: This table shows the relationship between measures of information acquisition and the treatment by requested contractor tier. The level of observation is the job posting, and the baseline group is beginner level contractors. Estimates are from quasi-maximum likelihood Poisson models. Heteroskedasticity-robust standard errors are reported. The sample is restricted to hourly first job posts by an employer. The DV in Model (1) is a count of the number of applications messaged. The DV in Model (2) is a count of the number of applications that included the word "skype" in a message with the employer. The DV in Model (3) is a count of the number of applications that exchanged messages including a question mark with the employer. All models include covariates: category indicators, prior jobs billed by the employer, employers prior spending on the platform, the number of applications to the job openings, the number of recommended applications to the job opening, the average applicants bid and an indicator for requested skills. Significance indicators: $p \leq 0.10 : *$, $.p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table 14: Effect of Hiding Past Wages on Information Acquisition on Private Job Postings

	Dependent variable:			
	Number of Apps Messaged	Number of Apps Skyped	Number of Apps with "?"	Number of Apps with Question Words
	Poisson	Poisson	Poisson	Poisson
	(1)	(2)	(3)	(4)
Hidden Wages Treatment Group (TRT)	−0.186	−0.227	−0.224	−0.177
	(0.241)	(0.259)	(0.216)	(0.249)
Constant	−0.750	-1.897^{**}	−0.427	−0.623
	(0.521)	(0.783)	(0.741)	(0.776)
Opening Level Covariates	Yes	Yes	Yes	Yes
Observations	2,392	2,392	2,392	2,392

Notes: This table shows the relationship between measures of information acquisition and the treatment. The level of observation is the job posting. Estimates are from quasi-maximum likelihood Poisson models. Heteroskedasticity-robust standard errors are reported. The sample is restricted to hourly first job posts by an employer.The DV in Model (1) is a count of applicants messaged. The DV in Model (2) is a count of the number of applications that included the word "skype" in a message with the employer. The DV in Model (3) is a count of the number of applications that exchanged messages including a question mark with the employer. The DV in Model (4) is a count of the number of applications that included at least one of the following question words: Who, What, Where, When, Why, or How. All models include covariates: category indicators, prior jobs billed by the employer, employers prior spending on the platform, the number of applications to the job openings, the number of recommended applications to the job opening, the number of applications that exchanged messaged with the employer and the average applicants bid. Significance indicators: $p \leq 0.10 : *$, $.p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table A.1: Effect of Hiding Past Wages on Search
Log Linear Models

	Dependent variable:
	Num Viewed
	OLS
Hidden Wages Treatment Group (TRT)	0.049^{*}
	(0.026)
Constant	1.430^{***}
	(0.120)
Opening Level Covariates	Yes
Observations	5,855

Notes: The sample is restricted to hourly first job posts by an employer. All models include covariates including: category indicators, prior jobs billed by the employer, employers prior spendings on the platform, the number of applications to the job openings, the number of recommended applications to the job opening, and the average applicants bid. Significance indicators: $p \leq 0.10 : *$, $.p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table A.2: Effect of Hiding Past Wages on Information Acquisition
Alternate DVs

	Dependent variable:		
	Total Number of Words	Total Number of Question Marks	Total Number of Question Words
	Poisson	Poisson	Poisson
	(1)	(2)	(3)
Hidden Wages			
Treatment Group (TRT)	0.064	0.080	0.082
	(0.066)	(0.060)	(0.059)
Constant	5.847***	0.923***	0.966***
	(0.285)	(0.255)	(0.260)
Opening Level Covariates	Yes	Yes	Yes
Observations	5,855	5,855	5,855

Notes: Estimates are from quasi-maximum likelihood Poisson models. Heteroskedasticity-robust standard errors are reported. The sample is restricted to hourly first job posts by an employer. All models include covariates: category indicators, prior jobs billed by the employer, employers prior spending on the platform, the number of applications to the job openings, the number of recommended applications to the job opening. Significance indicators: $p \leq 0.10: *$, $.p \leq 0.05: **$ and $p \leq .01: ***$.

Table A.3: Effect of Hiding Past Wages on Information Acquisition
Log Linear Models

	Dependent variable:			
	log1p(Apps Messaged)	log1p(Apps Skyped)	log1p(Apps with "?")	log1p(Question Words)
	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
Hidden Wages				
Treatment Group (TRT)	0.016	0.012	0.024	0.040^{***}
	(0.020)	(0.025)	(0.015)	(0.015)
Constant	1.255^{***}	0.181	0.926^{***}	0.824^{***}
	(0.096)	(0.144)	(0.073)	(0.075)
Opening Level Covariates	Yes	Yes	Yes	Yes
Observations	3,285	2,195	2,799	2,678

Notes: This table shows the relationship between measures of information acquisition and the treatment. The level of observation is the job posting. Estimates are from quasi-maximum likelihood Poisson models. Heteroskedasticity-robust standard errors are reported. The sample is restricted to hourly first job posts by an employer. The DV in Model (1) is a count of the number of applications that included the word "skype" in a message with the employer. The DV in Model (2) is a count of the number of applications that exchanged messages including a question mark with the employer. The DV in Model (3) is a count of the number of applications that included at least one of the following question words: Who, What, Where, When, Why, or How. All models include covariates: category indicators, prior jobs billed by the employer, employers prior spending on the platform, the number of applications to the job openings, the number of recommended applications to the job opening, the number of applications that exchanged messaged with the employer and the average applicants bid. Significance indicators: $p \leq 0.10 : *$, $.p \leq 0.05 : **$ and $p \leq .01 : ***$.

Table A.4: Effect of Hiding Past Wages on Information Acquisition

	Dependent variable:			
	I(Applicant Messaged)	I(Applicant Skyped)	I(Applicant Qword)	I(Applicant Qmark)
	(1)	(2)	(3)	(4)
Hidden Wages				
Treatment Group (TRT)	0.088	−0.004	0.182^{**}	0.096
	(0.055)	(0.080)	(0.086)	(0.095)
Constant	-2.020^{***}	-0.783^{**}	0.772^{**}	0.717^{*}
	(0.214)	(0.360)	(0.378)	(0.415)
Opening Level Covariates	Yes	Yes	Yes	Yes
Observations	186,635	10,541	10,541	10,541

Notes: This table shows the relationship between measures of information acquisition and the treatment. The level of observation is the application to a job posting. Estimates are from logit models. Heteroskedasticity-robust standard errors clustered at the employer level are reported. The sample is restricted to hourly first job posts by an employer. all models include opening level covariates including a category indicator, employer past earnings, employer past jobs, number of applications, number of recommended applications, and requested skills. Significance indicators: $p \le 0.10 : *$, $.p \le 0.05 : **$ and $p \le .01 : ***$.