The Value of Hiring through Referrals*

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Abstract

Employee referrals are a very common means by which firms hire new workers. Past work suggests that workers hired via referrals often perform better than non-referred workers, but we have little understanding why this may be. In this paper, we demonstrate this is because referrals allow firms to select workers better-suited for particular jobs. To test our model, we use novel and detailed productivity and survey data from nine large firms in three industries: call-centers, trucking, and high-tech. Referred workers are 10-30% less likely to quit and have substantially higher performance on rare "high-impact metrics" (e.g. creating patents and avoiding work accidents), despite having similar characteristics and similar performance on non-rare metrics. To identify the source of these behavioral differences, we develop four new statistical tests, all of which indicate that referrals benefit firms primarily by selecting workers with a better fit for the job, as opposed to referrals selecting workers with higher overall quality; to referrals enabling monitoring or coaching; or to it being more enjoyable to work with friends. We document that workers refer others like themselves, not only in characteristics but in behavior (e.g. unsafe workers refer other unsafe workers), suggesting that firms may gain by incentivizing referrals most from their highest quality workers. Referred workers achieve substantially higher profits per worker and the difference is driven by referrals from high productivity workers.

JEL Classifications: M51, J24, J30, O32, J63

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¶Evolv on Demand

1 Introduction

Hiring is a critical decision for firms, with substantial time and resources spent on finding the right people. Indeed, some economists have argued that hiring may be at least as important for firms as the design of incentives (e.g. Prendergast, 2010; Oyer and Schaefer, 2011). One of the most common means of hiring is employee referrals, where employees at a firm recommend candidates for jobs. In the U.S., the online job platform CareerBuilder estimates that 69% of firms have an employee referral program, and roughly 50% of workers report being referred to their job by a friend or family member (Topa, 2012).[1] The form of employee referral programs differs, from cases where referred workers receive special consideration in the application process, to ones in which existing employees receiving significant financial bonuses for their friend getting hired. Employee referrals are also extremely common outside the U.S., both in developed and developing countries, and a growing economics literature documents that referrals help workers find jobs and may increase their wages.[2] However, there is much less work on the implications of referrals for *firms*. Why do firms use employee referral programs?[3] Does referral-based hiring allow firms to get "better" workers and, if so, why?

This paper offers an answer to the largely unanswered question of why referred workers may perform better. We use a rich new dataset combining productivity and survey data from nine large firms in three industries, spanning hundreds of thousands of workers. We show that referred workers are substantially less likely to quit and perform better on rare events critical to firm performance; however, perhaps surprisingly, they have similar characteristics (e.g. cognitive and non-cognitive skills) and similar performance on non-rare performance metrics. Although there are competing explanations for why referred workers perform better, we provide evidence that referrals select workers who are better suited for the particular job. We show that this selection makes referred workers roughly 25% more profitable than non-referred workers; further, people tend to refer others like themselves, making referrals from high productivity workers especially valuable.

This paper disentangles "selection" and "treatment" impacts of referrals, and doing this has implications for economic theory, public policy, and management. Broadly, explanations for why referred workers may perform better are either about selection—the type of worker joining the firm–or treatment—effects of referrals after work has begun. In terms of selection, referrals may lead to workers who are either better overall or better suited for the particular job. In terms of treatment, referred workers could receive mentoring or coaching from referring workers, or find it more enjoyable to work with their friends. For economic theory, understanding what social networks actually do informs how they should be modeled.[4] For public policy, if referrals lead to better selection of workers

[1]See CareerBuilder "Referral Madness: How Employee Referral Programs Turn Good Employees into Great Recruiters and Grow Your Bottom Line." Galenianos (2011) reports that 37-53% firms advertise jobs through worker social networks. In a survey of firms on the last employee each firm hired, Holzer (1987) finds that 36% of firms filled their last opening using an employee referral.

[2]See Ioannides and Loury (2004) and Topa (2012) for reviews of the literature, as well as papers cited below.

[3]A simple reason firms use employee referral programs is to cut down on recruiting costs. Instead of paying to advertise jobs, firms can use employee referral programs to directly connect with candidates. While our paper mostly focuses on whether employee referrals lead to "better" workers, we return in Section 7 to referrals reducing recruiting costs in our analysis of the profit implications of referrals.

[4]For example, Montgomery (1991) offers a model of referrals and wage determination based on selection effects of

for jobs, policy efforts on “networking” may be best targeted to the unemployed, whereas if referrals benefit workers via treatment (e.g. coaching or monitoring), efforts may instead focus on mentoring and other support programs once work has begun. For managers, whether referral impacts operate through selection or treatment is important for the optimal design of employee referral programs.

We begin by developing a simple model of referral-based hiring. Firms receive more precise signals about job-match from referred applicants compared to non-referred applicants. We prove that this informational advantage leads to referred workers being better selected. In addition, after starting work, referred workers have a lower disutility of effort than non-referred workers since referring workers provide coaching to and monitor the work of referred workers.

We test these mechanisms using a rich new dataset ideally suited for studying why firms use referrals. Since individual-level productivity data is scarce, many empirical papers in personnel and organizational economics use data from one firm.[5] While powerful insights can still be drawn, there is often a question of whether the results generalize to other workers and firms in the economy. In contrast, our paper uses data from nine firms in three industries, deliberately chosen to span a broad range of worker types and skill groups.[6] Although there are differences across industries that we highlight along the way, our results are surprisingly consistent across firms and industries. This suggests that our findings may be relevant for many firms in the economy, although we certainly acknowledge that questions of generalizability may remain, even with nine firms. From a larger set of firms we considered for this project, we chose nine for our dataset where we had strong institutional knowledge and which shared a number of common data elements. For all firms, we have high-frequency individual productivity data across multiple dimensions, as well as demographics, cognitive skills, non-cognitive skills, and work friendships. As we show later, these data elements are critical for isolating how referrals may select “better-matched” new workers and for separating selection from treatment effects.

The first contribution of the paper is to compare referred and non-referred workers using hundreds of thousands of workers, millions of applicants, and a large number of outcomes, including worker productivity. Past work relating referral status to direct measures of worker productivity is very limited.[7] In addition, several of the outcomes and characteristics we study, namely innovation, work accidents, cognitive skills, non-cognitive skills, and experimental economic preferences, have never before been related to referral status (to our knowledge). Cognitive and non-cognitive skills are believed to be critical determinants of socioeconomic outcomes and overall workplace performance (Neal and Johnson, 1996; Heckman et al., 2006), yet referred and non-referred workers look similar on these, as well as on other characteristics and on non-rare performance metrics. However, referred

referrals, whereas Kugler (2003) offers one based on treatment effects of referrals.

[5]For an excellent recent example, see Lazear et al. (2012). There are also many studies using matched employee-employer datasets or detailed cross-firm surveys, but these datasets usually do not contain productivity at the individual worker level. We are one of the first papers in personnel and organizational economics (to our knowledge) to study individual productivity data from multiple firms.

[6]Call-center work is a relatively low-skill job, long-haul trucking is a moderate-skill job, and high-tech work requires advanced skills. Having a broad range of skills groups in the data is quite useful, as the value of referral-based hiring may be thought to vary based on the type of job.

[7]The only paper we are aware to relate referral status to direct measures of productivity in a firm is a sociology study, Castilla (2005), using a few hundred workers.

workers are substantially less likely to quit in all three industries, and perform better on rare "high impact" outcomes. Referred truckers have a 12% lower chance of having a preventable accident. Referred high-tech workers are significantly more innovative, being about 20% more likely to develop a patent than non-referred workers, even after controlling for a large number of observable job type and worker controls.

The second and primary contribution of the paper is provide direct evidence on selection vs. treatment impacts of referrals using four new tests, all of which indicate that referral impacts operate through selection. First, we show using event studies that referred employee performance does not suffer after the referring employee exits.[8] If referrals operate through treatment effects, e.g. mentoring or coaching, we might have expected there to be a negative impact. Our second strategy is a regression discontinuity design analyzing referred worker behavior shortly before and after referral bonus tenure thresholds (e.g. the referred worker needs to stay six months for the referring worker to receive a bonus). If referrals operate through treatments effects, one would think that the referred worker would be willing to postpone quitting by a day or week so that the referring worker can receive a bonus, but we see no evidence of this in the data. Third, we demonstrate that unlike referral status, the number of friends formed after the start of work does not significantly affect quitting, productivity, or high-impact outcomes. If referrals increase performance because referring workers serve as friends who provide mentoring or because it is more enjoyable to work with friends, having more friends should lead to higher performance; but this would not be true if referrals operate though selection. Fourth, referred workers score significantly higher on pre-job tests of job-specific skills and receive higher interview scores. Since these questions are asked before work has begun, they are unlikely to be due to improved monitoring or mentoring, or to people being more productive working with their friends.

The third contribution of the paper is to provide new evidence on *homophily* as a mechanism for selection and to quantify the implications of selection and homophily for the profitability of referred vs. non-referred workers. We show that there is homophily in referrals, that is, a tendency of people to refer others like themselves, not only in characteristics, but also in behaviors. Conditional on worker zip code, existing employees tend to refer others of a similar age, ethnicity, gender, marital status, and smoking status. In addition, referred workers tend to refer others with similar work behaviors. All else equal, more productive workers tend to refer other productive workers. Workers who have had trucking accidents end up referring other workers who go on to have trucking accidents. We compute that referred workers obtain 25% higher profits compared to non-referred workers. We show further that profits per worker vary widely depending on the identity of the person making the referral. Referrals from below-median productivity referring workers yields profits per worker below that of non-referred workers. In contrast, referrals from above-median productivity referring workers are extremely profitable.

The literature on referrals was pioneered by sociologists, particularly Granovetter (1973, 1974).

[8] Out of the four strategies, this is the only one for which we are aware of any parallel in the literature. Specifically, the sociology study, Fernandez et al. (2000), regress whether a worker quits on whether the referring worker *ever* quits. In contrast, our design exploits the timing of quitting, both in difference-in-difference and event study specifications, analyzing how referred worker behavior changes *after* the referring worker quits; see Section 5.

In more recent work by sociologists, Fernandez et al. (2000) and Castilla (2005) analyze differences between referred and non-referred workers at a call-center. There is also substantial work on employee referrals in the industrial psychology literature.[9]

In economics, our paper adds to a growing literature analyzing the importance of social networks for worker and firm performance (Bandiera et al., 2009, 2010; Bayer et al., 2008; Beaman, 2012; Hellerstein et al., 2011; Kugler, 2003; Laschever, 2009; Mortensen and Vishwanath, 1995; Pallais, 2013; Schmutte, 2012; Wahba and Zenou, 2005).[10] Beaman and Magruder (2012) conduct a randomized trial in India to analyze the impact of referral incentives; using a laboratory experiment where subjects were asked to refer new subjects and paid under different incentives based on those subjects' performance, the authors show that people have information about their friends' abilities. In a paper also using personnel data, Brown et al. (2012) study data from a U.S. corporation, comparing referred and non-workers on hiring, wage growth, turnover, and promotions. Our paper differs in that we focus on separating selection vs. treatment effects of referrals. Two papers that are related to selection vs. treatment effects of referrals are Heath (2011) and Dustmann et al. (2012). Heath (2011) analyzes survey data from Bangladeshi garment workers to argue that referrals help solve limited liability problems. Using ethnic hiring patterns with German administrative wage data, Dustmann et al. (2012) argue that referrals reduce information inefficiencies. By using direct measures of worker productivity, our paper provides sharp tests on selection vs. treatment. More broadly, our paper contributes to the economics literature on how human resource practices affect firm productivity;[11] more specifically, it contributes to the economics literature on hiring, an area which Oyer and Schaefer (2011) argue is significantly under-studied.

The paper proceeds as follows. Section 2 presents our model of hiring and worker behavior under referrals. Section 3 describes the data. Section 4 provides results on differences between referred and non-referred workers. Section 5 provides tests to separate treatment and selection impacts of referrals. Section 6 probes further into the nature of the selection in the data. Section 7 explores the implications of referrals and type of referring worker for firm profits. Section 8 concludes.

2 Conceptual Framework

We build a model of hiring incorporating both treatment and selection impacts of referrals. We do this because both selection and treatment effects have been recognized as potentially important, particularly outside the academic literature. Baron and Kreps (1999), a leading MBA textbook, describe both as part of the conventional wisdom of what referrals do. Baron and Kreps (1999, p. 342) describe how referrals may be a selection device, exploiting the superior information held by existing employees about candidate qualities. In addition, Baron and Kreps (1999, p. 342) describe

[9] For surveys, see Breaugh (2008) and Zottoli and Wanous (2000). A limitation of this literature is that the sample size is often small and usually does not use the tools of applied microeconomics.

[10] Calvo-Armengol and Jackson (2004) argue that social networks can have important effects on wage differentials and lifetime inequality. For a detailed treatment of the growing theoretical literature on social networks in economics in general, see Jackson (2008).

[11] The literature is too large to discuss here, but see e.g., Ichniowski et al. (1997) for a seminal paper and Bloom and Van Reenen (2011) for a recent survey.

how referring workers may provide support and coaching to colleagues they refer, that is, a treatment effect may be operative.

Our goal is to provide the simplest framework for understanding how referrals can have selection and treatment impacts on worker performance; we use this framework later in Section 5 to situate our different econometric tests of selection vs. treatment. In addition, we lay out a condition for positive selection of referred workers when the firm endogenously chooses a hiring policy that may set a lower standard for referred workers. The model is related to the referrals model in Dustmann et al. (2012) and Brown et al. (2012).

Consider a firm hiring from a population of potential new workers, each with an outside option of 0. Each worker have an underlying match quality m drawn from a distribution $N\left(\mu_0, \frac{1}{h_0}\right)$; that is, μ_0 is the mean and h_0 is the precision. When workers apply for the job, a normally distributed signal $\widehat{m}$ is observed about their match quality. For referred applicants, the signal is denoted by $\widehat{m}_R$ whereas for non-referred (or external) applicants, the signal is denoted by $\widehat{m}_E$. Specifically, we assume that $\widehat{m}_R \sim N\left(m, \frac{1}{h_R}\right)$ and $\widehat{m}_E \sim N\left(m, \frac{1}{h_E}\right)$, where $h_R > h_E$; that is, the firm observes more precise signals about referred than non-referred workers. We denote whether a worker was referred by the dummy variable $r \in \{0, 1\}$.

The sequence of events in the model is as follows:

1. The firm receives applications from referred workers and observes a signal $\widehat{m}_R$ for each one. The firm hires if the signal, $\widehat{m}_R$, exceeds a hiring threshold, m_R^*.

2. The firm receives applications from non-referred workers and observes a signal, $\widehat{m}_E$, for each one. The firm hires if the signal exceeds a hiring threshold, m_E^*.

3. The worker's true match m is realized. Production occurs with $y = m + \alpha e$. Workers have a cost of effort c_e that is drawn from a distribution function $F(c_e)$. Workers choose an effort level $e \in \{0, 1\}$ to maximize utility.

We make the following assumptions about worker utility and profits:

- The new worker's utility is: $u = w + (T(r, f) - c_e)e$. We assume that wages are determined by Nash Bargaining, with $w = \beta y = \beta(m + \alpha e)$. That is, workers bargain to receive a share β of output. The term $T(r, f)$ represents the treatment impact of referrals, for example, from monitoring or coaching. Denoting a worker's friends by f, we also allow $T(r, f)$ to depend on a person's number of friends. For now, we assume that $T(r, f) = k_r r$, but we will relax this assumption later in Section 5.3.

- Profits for the firm are production minus the wage bill: $\Pi = (1 - \beta) y = (1 - \beta)(m + \alpha e)$.

We solve backwards, beginning with the worker's effort decision. A worker will choose to exert effort if $k_r r + \beta\alpha - c_e > 0$, which occurs with probability $F(k_r r + \beta\alpha)$. The treatment benefit from referrals is thus parameterized by k_r.

We now move to the firm's decision about which applicants to hire. Firms will choose a hiring threshold m^* for referred and non-referred workers such that they earn 0 profits for signals at the threshold. Specifically, they will choose m^* so that $E(\pi|m^*) = 0$, leading to

$$\begin{aligned} 0 &= \frac{h_0}{h_0+h}\mu_0 + \frac{h}{h_0+h}m^* + \alpha F(\beta\alpha + k_r r) \\ m^* &= \frac{-h_0\mu_0}{h} - \frac{(h_0+h)\,\alpha F(\beta\alpha + k_r r)}{h} \end{aligned}$$

If fewer than half the workers in the population would not be well-suited for the job (i.e. $\mu_0 < 0$), then this equation establishes that referred applicants will face a lower hiring standard than non-referred applicants. That is, conditional on a signal of applicant quality, referred workers will be more likely to get hired.[12]

Because referred workers may face a lower hiring standard, it is not obvious that referred workers will have higher average match quality. However, solving further in Appendix A, we obtain that this is so:

Proposition 1 *Referred workers will have higher productivity than non-referred workers. Suppose that the effort cost reduction from being referred, k_r, is not excessively large. Then, observed productivity differences will reflect both selection and treatment effects. That is, referred workers will have higher average match quality and will exert more effort.*

To establish Proposition 1, we suppose that k_r is not excessively large. This assumption is made because when there are treatment effects from referrals, firms will further lower the hiring standard for referred applicants, anticipating that referred workers will do better *ex post* because of treatment effects. Assuming k_r is not excessively large rules out that the firm, anticipating very large treatment effects, would lower the hiring standard for referred workers so much so as to obtain negative selection.

Probably the most important implication of Proposition 1 is that simple performance comparisons will not be able to tell us to what extent selection and/or treatment effects are operative, since both selection and treatment will contribute to performance differences. Thus, we need to develop more fine-tuned empirical tests to isolate selection and treatment.

Discussion of Model Assumptions. In the model, there is only one dimension of worker performance, y. There is no quitting decision and no separation between different production activities (e.g. "standard" activities like writing code or producing miles vs. "high-impact" activities like developing new patents or avoiding trucking accidents). This is done solely to ease exposition and we may think of y as encapsulating all dimensions of performance, including retention, non-rare productivity measures, and high-impact productivity measures.

In addition, the model is static. However, it does not seem it would be conceptually difficult to make it dynamic, either with two periods (Brown et al., 2012) or an infinite number of periods (Dustmann et al., 2012). Our static model is well-suited for the empirical work, given that our focus is not on wage dynamics.

[12] Indeed, we observe that referred workers are substantially more likely to be hired; see Table C7.

Finally, we emphasize that our conceptual framework contains several specific assumptions (e.g. that signals are normally distributed). Though these assumptions seem reasonable to us, we caution against generalizing too broadly.

3 Data

For each of the 3 industries, we describe the production process, how productivity is measured, the survey data, and how referral status is measured. Some details about the firms cannot be given due to confidentiality restrictions. Due to space constraints, we provide variable definitions (e.g. cognitive and non-cognitive skills) in Appendix B. Table C1 summarizes the data available from the three industries.

Call-centers. The call-center data are from seven firms in the call-center industry, covering the period 2008-2012. The data have about 350,000 applicants and about 50,000 employees. Each of the seven firms provides service to large end-user companies, e.g. large credit card or cellular phone companies.[13] The workers are primarily from different parts of the U.S., with a small number from the Philippines, India, and Mexico. In these call-centers, the production process consists of in-bound and out-bound calls, with workers doing primarily customer service or sales work. Performance is measured using several industry-standard productivity measures: schedule adherence (or 'adherence'), average handle time (lower is better), the share of sales calls resulting in a successful sale, the share of time that a manager listens in and judges the service provided to be of high quality (quality assurance), and the share of time that the customer reports being satisfied with their experience. One very important outcome is turnover. In the call-center industry, turnover is very high and is costly for firms. In our data, roughly half of workers leave within the first 90 days. For the call-centers, a great deal of information is available from applicant job tests. The job tests numerous questions measuring cognitive ability and non-cognitive ability (namely, personality questions that can be mapped to the Big 5 characteristics), as well as asking the number of friends they have at the company and people they know working at the company. Referral status is measured via a self-report on the applicant's job test ("Were you referred to this job application by someone that already works for this company?")

Trucking. The trucking data are from a very large U.S. trucking firm, covering the period 2002-2009. To preserve the anonymity of the firm, we do not release the exact total number of applicants, employees, or employee-weeks in the sample. In the trucking firm, the production process consists of delivering loads between different locations. The drivers are paid almost exclusively by the mile (a piece rate), are non-union, and are away from home for long periods of time. The standard productivity measure in long-haul trucking is miles driven per week. There are substantial and persistent productivity differences across workers in miles per week, even though most drivers work the same number of hours (60 hours per week, which is the federal legal limit). A rare, but high-

[13]The firms contract with a firm called Evolv on Demand, which provides them with employee screening software and consulting services. We obtained the data from Evolv on Demand.

impact performance metric in trucking is accidents. In addition, turnover is also very high in trucking, with firms spending significant sums in trying to find ways to reduce quits. The baseline data include weekly productivity, accidents, quits, and a number of background characteristics, and are available for tens of thousands of workers.

In addition, very detailed survey data are available for a subset of 900 new drivers who received training starting in late 2005 and 2006. For these drivers, there is information on social networks at work, detailed demographics, cognitive ability, non-cognitive ability (personality), experimental preferences, who referred whom, and surveys about work satisfaction.[14]

Referral status for truckers is measured using both a survey question in the job application (how the worker found out about the job) and using administrative data from the firm's employee referral program. These two measures of referral status are highly correlated, suggesting that both are reliable (see Appendix B.1). Only for the trucking firm do we have matched data on who referred whom, and we only have the match for the period 2007-2009.

High-tech. We use data from a large high-tech firm covering the period 2003-2008. The data have about 1.18 million applicants and about 25,000 employees. Most workers in the sample are high-skill individuals with advanced education. At the high-tech firm, the production process depends on the job type. The largest share of the workers are engineers and computer programmers. In addition, some workers are in sales and customer support. Our standard productivity measures for these workers are subjective performance reviews (provided by the employee's supervisors) and detailed objective measures of employee behavior, including hours worked and the number of times one reviewed or debugged other people's code, built new code, or contributed to the firm's code library. In addition, we have data on worker innovation, a rare, but high-impact aspect of performance in many high-tech fields. Worker innovation is measured using patent applications and contributions to the firm's internal ideas board.[15] Turnover is very low at the high-tech firm, with workers staying years with the firm, instead of months or days as in the other industries. The high-tech workers are surveyed occasionally by the human resources department, giving us information on personality characteristics and work friendships. For the high-tech workers, referral status is measured using administrative data from the company's employee referral program (i.e. a current employee forwarded an applicant's resume to the HR department).

4 Referral Status and Worker Performance

In this section, we examine whether referred and non-referred workers differ in characteristics and behaviors. We first show that referred workers do not appear to have superior overall characteristics

[14] See Burks et al. (2008) and Appendix B for more on the collection of the data.

[15] At the high-tech firm, employees who create an invention file an Invention Disclosure Form. Attorneys from the firm then decide whether to file a patent application. Most of these patent applications are later approved as patents, but the process usually takes several years. For the data analysis in the paper, our variable of interest is patent applications per employee. This is advantageous in two respects. First, patent applications are observed right away (whereas actual patent award occurs usually multiple years later, though the two are highly correlated). Second, it allows us to compare referred and non-referred workers in terms of the ideas that the firm thought were most valuable to patent, instead of merely all the ideas that an inventor chose to disclose.

(such as higher schooling, cognitive ability, and non-cognitive ability), suggesting against referrals selecting "better overall" workers. Next, we document whether referred and non-referred workers differ in quitting, non-rare performance metrics, and rare high-impact performance metrics; we postpone the question of whether the differences are due to superior selection for the particular job or treatment effects until Section 5.

Characteristics. It is possible that referred workers could have overall better attributes than non-referred workers. However, Table 1 shows that referred and non-referred applicants and workers look mostly similar in terms of schooling, work experience, cognitive ability, non-cognitive ability, and experimental preferences.[16] It presents regressions of characteristics on referral status and controls:

$$y_i = \alpha + \beta * REF_i + X_i\lambda + \epsilon_i \tag{1}$$

where y_i is an individual characteristic of interest, REF_i is a dummy for being referred, X_i are demographic controls, and ϵ_i is an error. Throughout the paper, standard errors are clustered at the worker level (or for pre-work analysis, at the applicant level).[17] In Panel A, we see that referred call-center applicants and referred trucking workers actually have 0.09 and 0.20 *fewer* years of schooling, respectively, whereas referred high-tech workers have 0.05 more years of schooling. Referred and non-referred truckers look similar in terms of years of related experiences (experience with a large on-road vehicle), the number of jobs held in the last two years, and the longest duration of the previous job.

Panel B shows that referred and non-referred workers have similar levels of cognitive ability. Referred truckdrivers actually score 0.12 standard deviations lower on an IQ test and referred high-tech workers score 15 points higher on the SAT test (neither difference is statistically significant). Panel C shows that referred workers also do not have superior levels of non-cognitive ability, measured through the Big 5 personality characteristics. Referred call-center applicants have slightly *less* desirable non-cognitive skills than non-referred applicants, scoring 0.02 standard deviations lower on the overall Big 5 Index. Referred applicants are slightly less conscientious, less agreeable, and less open, though they are more extroverted. Similar patterns are observed for trucking workers, where referred workers score (a statistically insignificant) 0.03 standard deviations lower in the overall Big 5 Index. In addition, the patterns are also roughly similar for high-tech referred workers, who are less agreeable and more extraverted. On the overall Big 5 Index, referred high-tech workers score

[16]While we have rich characteristics data on *workers* in all three industries, we only have rich characteristics data on *applicants* for call-centers. For brevity, we present Table 1 focusing on the applicants from the call-center industry; in the call-center data, the referred vs. non-referred patterns among workers are similar to the ones among applicants.

[17]We do this because referral status, the main regressor of interest, varies at the individual level. We have explored different ways of clustering across the three industries and the results are generally very robust. For example, we get similar results if instead we cluster at the location level.

0.01 standard deviations higher (also insignificant).[18,19]

Panel D shows that referred workers also look similar in terms of experimental measures of preferences. While referred workers do not appear advantageously selected in terms of standard measures of human capital and ability (schooling and IQ), it could be the case that they differ in terms of hard to measure individual dimensions. For example, referred workers might be more likely to stay because they are more patient, because they have a greater risk tolerance for the weekly swings in truckdriver income, or because they are more altruistic. The data, however, do not support this. The one significant difference is that referred workers are actually somewhat less trusting than non-referred workers in a trust game.

It is important to note that, with the exception of SAT scores in high-tech, our measures of cognitive skills (Panel B), non-cognitive skills (Panel C), and experimental preferences (Panel D) were *not observed by firms* at the time of hire.[20] This is useful because according to our conceptual framework, if the firm has more precise information about an unobserved trait for referred applicants, then we should see higher levels of that trait among referred workers. That we do not see differences suggests that there is not more precise information on referred applicants about overall ability, leading us instead to examine other outcomes.[21]

Quitting. Despite similarities in observable characteristics, referred workers are substantially less likely to quit, and the effect is quite robust. Retention is a critical outcome for many firms, and is a commonly used measure of worker performance in personnel economics, particularly in lower-skill high-turnover settings (e.g. Autor and Scarborough, 2008). Figure 1 compares survival curves with no covariates, showing that for all three industries, referred survival exceeds non-referred survival. Table 2 adds covariates, estimating Cox Proportional Hazard models of the form:

$$log(h_{it\tau}) = \alpha_t + \beta_0 * REF_i + \gamma_\tau + X_i\lambda + \epsilon_{it\tau}$$

[18] There is a large and growing literature in economics on the importance of cognitive and non-cognitive skills for socioeconomic and workplace outcomes. See Borghans et al. (2008) for an excellent survey. In a recent example, Lindqvist and Vestman (2011) show that both cognitive and non-cognitive skills play important roles in explaining lifetime wage and employment patterns. In another paper looking at cognitive and non-cognitive skills as important measures of general worker ability, see Dal Bo et al. (2013), who show that randomly increasing wages for public sector workers increases the cognitive and non-cognitive skills of applicants.

[19] The reader may also ask whether cognitive and non-cognitive skills predict performance for the workers we study. Also using the subset of 900 workers from the trucking firm we analyze, Rustichini et al. (2012) show that cognitive and non-cognitive skills help predict worker strategies in experimental games, health behavior, worker retention, and worker accidents (they do not study referral status). Looking at our call-center and high-tech workers, we also find significant associations between cognitive and non-cognitive skills and performance, consistent with the large related literature in industrial psychology (Barrick and Mount, 1991). Results available on request. For our paper, *controlling* for cognitive and non-cognitive skills has little impact on the relationship between referral status and the various performance variables; this in unsurprising, given the very weak correlation between referral status and cognitive and non-cognitive skills.

[20] In the call-centers, data on cognitive and non-cognitive skills, as well as substantial other information about work-relevant skills and job fit, is collected by the job testing company, Evolv; only a final hiring recommendation is shared to the call-center firms. In trucking, the data was collected by the authors on workers during training. In high-tech, the data on non-cognitive skills were collected in a survey of existing workers.

[21] Besides regressions, we also show raw means of referred and non-referred workers in Tables C2-C5. While we see some differences between referred and non-referred workers in terms of demographics (depending on the industry), there are few differences in terms of work experience, cognitive ability, non-cognitive ability, and experimental references (as in the regression estimates). One difference is that referred workers report having slightly lower income had they continued at their past jobs.

where $h_{it\tau}$ is the quit hazard of worker i with tenure t at time τ; REF_i is a dummy for whether worker i is referred; γ_τ is a time fixed effect; X_i are individual covariates (including time of hire fixed effects); and $\epsilon_{it\tau}$ is an error. The main coefficient of interest is β_0 expressing the difference in quit rate between referred and non-referred drivers. Panel A shows that referred call-center workers are 13% less likely to quit. Panel B shows that referred truckers are about 12% less likely to quit. Given the coefficient on driver home state unemployment rate of -0.04, the reduction in quitting among referred workers is of the same magnitude impact as that from a 3 percentage point increase in the driver's home state unemployment rate. Among the high-tech workers, referred workers are around 30% less likely to quit. The coefficients are quite robust to adding various controls, including average productivity to date for truckers and average interview scores for high-tech workers.[22]

Although Table 2 and subsequent analysis focus on quits, which are much more common than fires in all three industries, we also find that referred workers are less likely to be fired.[23]

Non-rare Productivity. Table 3 shows that referred and non-referred workers tend to have similar productivity on non-rare metrics. We estimate regressions of the form:

$$y_{it\tau} = \alpha_t + \beta * REF_i + \gamma_\tau + X_i\Gamma + \epsilon_{it\tau} \tag{2}$$

where $y_{i,t}$ is worker i's productivity in his t^{th} period of tenure with the company in time period τ, α_t is a tenured fixed effect, γ_τ is a time fixed effect, X_i are controls (including cohort of hire fixed effects), and $\epsilon_{it\tau}$ is an error. To ease comparisons across performance measures and across industries, we normalize the y variables, so that all coefficients can be interpreted in terms of standard deviations. In the call-center industry and in trucking, there is no evidence that referred workers are more productive. In the call-center data, there are no statistically significant differences between referred and non-referred workers on 4 of 5 productivity measures, and on sales conversion, referred workers are slightly less productive, though the difference is quite small. Referred workers have a sales conversion rate that is -0.03 standard deviations below that of non-referred workers, and the standard error of 0.015 means we can rule out a difference of more than -0.05 standard deviations. In trucking, referred workers are 0.02 standard deviations less productive, but the coefficient becomes essentially 0 once zero mile weeks and other outliers are eliminated. In high-tech, referred workers have slightly higher subjective performance scores (0.04 standard deviations higher), but score no better on most objective performance measures. Overall for high-tech, referred workers may be slightly more productive, but the difference is not robust. As seen in the tenure-productivity curves

[22] Although the question falls outside the scope of our model, we also examined whether the impact of referral status on quitting varies with the business cycle or worker characteristics (such as driver experience level, driver tenure, and whether the driver has a training contract), focusing on data from trucking where we have the greatest statistical power. We found little evidence that the impact of referral status varies much with business cycle or worker characteristics. The impact of referral status was not significantly affected by the local unemployment rate, whether the driver was experienced or brand-new to trucking, or training contracts. In terms of tenure, referral effects actually became slightly stronger as tenure increased, but this was only marginally significant at the 10% level. Results available on request.

[23] In all three industries, we can distinguish quits and fires in the data. Referred workers are 3%, 13%, and 36% less likely to be fired in call-centers, trucking, and high-tech, respectively. The difference is highly statistically significant for trucking, but not quite statistically significant for call-centers and high-tech, reflecting that fires are much less common than quits.

in Figure 2, productivity between referred and non-referred workers is similar at most tenure levels.[24]

Rare High-impact Productivity Measures. Although referred workers have similar or only slightly higher productivity on most standard measures, they do appear to score significantly higher on "high-impact" measures. In the trucking industry, although productivity is measured in miles per week, an extremely important measure of performance is driver accidents, both from a business and policy point of view. Table 4 analyzes differences between referred and non-referred workers in accident risk. Using a linear probability model, we estimate that referred workers have a weekly accident probability that is about 0.13 percentages points below that of non-referred workers. Given a baseline accident probability of about 2% per week, this indicates that referred workers have roughly a 7 percent lower risk of having an accident each week. This finding is robust to including very rich controls.

An alternative explanation for why referred drivers have fewer accidents, that is unrelated to treatment or selection impacts of referrals, is that referred workers may be assigned different roles in a firm than non-referred workers. Although we have rich controls for the type of work that different drivers are doing, it might be possible that referred workers are receiving preferential treatment or work type assignment by the firm on some unobserved dimension. To address this, we take advantage of the fact that accidents are divided into "preventable," accidents the driver had control over, and "non-preventable," accidents the driver could not control. As seen in column 2, referred drivers are a highly significant 12.1% less likely to have preventable accidents, but only 3.7% less likely to have non-preventable accidents (this is not statistically significant).[25]

Table 5 shows that referred high-tech workers are also more innovative than non-referred workers. Panel A shows that referred workers are significantly more likely to file patent applications compared to non-referred workers. Patents are a standard measure of idea production in firms (e.g. Henderson and Cockburn, 1996), and though relatively rare in patents per worker, are believed to be an important driver of firm performance (Bloom and Van Reenen, 2002). Column 1 shows that referred workers develop about 0.18 more patents per worker, which falls to 0.14 once full demographic controls are applied. Given that the distribution of patents per employee is highly skewed, we estimate negative binomial models in columns 3 and 4, finding that referred workers produce about 24% more patents than non-referred workers.[26] Referred workers are also 1 percentage point more likely to

[24] One confound to estimating the relationship between referral status and productivity is differential attrition based on productivity. Among non-referred workers, the "bad apples" might get "weeded out" after some period of time, whereas both low-ability and high-ability referred workers may stick with the job. As a robustness check, we have re-done Table 3 restricting to workers at lower tenure levels. The results are very similar and available on request. Other approaches to address this concern would be to estimate a simultaneous equation model of productivity and retention with a common latent factor, or to estimate a fully structural dynamic model of productivity and retention (Hoffman and Burks, 2012), though doing either of these would require relatively strong assumptions.

[25] The classification of preventable vs. non-preventable accident is made by analysts at the trucking firm's insurance subsidiary and is based on Federal Motor Carrier Safety Administration guidelines. While the distinction is based on objective criteria, it is still imperfect, and safer drivers may also be likely to have fewer non-preventable accidents. Thus, if referred workers are safer, it is not surprising that they are also slightly less likely to have non-preventable accidents.

[26] The overdispersion parameter, α, is 2.77 (se=0.25) in column 3, and 2.66 (se=0.24) in column 4, indicating a highly significant degree of overdispersion, suggesting use of a negative binomial model instead of a poisson model (Cameron and Trivedi, 2005).

ever patent (only 6% of workers ever patent at the firm). To account for patent quality, we also look at citation-weighted patents in columns 7 and 8, merging in patent citation data from the NBER Patent Database. Patent citations are the most widely used measure of patent quality (e.g. Jaffe et al., 1993). Referred workers achieve 19% more citation-weighted patents than non-referred workers.[27]

Panel B shows that referred workers also are more innovative in terms of the number of ideas they contribute to the firm idea board. The high-tech firm has a structured online forum for proposing new business and technology ideas, to which all employees at the firm can contribute. Though many ideas are never implemented, several of the firm's most successful projects were proposed on the idea board. Since the number of ideas per person is highly right-skewed, we estimate negative binomial models, finding that referred workers produce 8-9% more ideas.[28] Similar significance levels (though smaller magnitudes) are achieved using OLS with log(1+ideas) as the dependent variable. One concern with interpreting this difference in ideas proposed as a true difference in useful ideas produced is that referred workers may have less of a "filter" and propose lower quality ideas than non-referred workers. However, Table C8 shows that the ratings assigned by fellow employees to the ideas of referred workers are no worse than the ideas of non-referred workers, helping rule out the differential filter explanation.

Discussion. How do the differences we find between referred and non-referred workers relate to those in other work? In terms of non-rare productivity, Castilla (2005) finds that referred workers have 3.5% more phone calls per hour than non-referred workers (0.7 more calls per hour, off a base of 20 calls per hour), using data from the call-center at one bank. Using a much larger sample, our 95% confidence interval for the relative performance advantage of referred workers is $[-0.149, 0.020]$, or in percentage terms, $[-1.80\%, 0.24\%]$. We can thus rule out very small performance differences, even ones 10 times smaller than those estimated by Castilla (2005).[29] In terms of quitting, Brown et al. (2012) and Castilla (2005) find that referred workers are 15% and 44% less likely to quit, respectively; thus, our finding that referred workers are 10-30% less likely to quit is in line with these estimates.[30]

Why is it that referred workers are less likely to quit and perform better on rare productivity metrics, but not on non-rare metrics? Taking our model seriously, it may be that referred workers perform better on some tasks, but not others based on whether there is an informational advantage to referral status on a given metric. The results suggest that referring workers may have a significant

[27] Following Trajtenberg (1990), we construct citation-weighted patents as one plus the number of citations for each patent. The results are robust to alternative specifications as well.

[28] The overdispersion parameter is 3.45 (se=0.10) in column 1 and 3.41 (se=0.10) in column 2, again indicating a highly significant degree of overdispersion.

[29] In our data, the average calls per hour is 8.25.

[30] Since most work in economics comparing referred and non-referred workers focuses on wages, for completeness, we examine this as well. In call-centers, our data do not contain wages for workers; however, we have been informed that there is very little wage variation among new workers, so it is unlikely there would be a difference between referred and non-referred workers. In trucking, since workers are paid by the mile, there is almost a 1:1 mapping between miles driven (non-rare productivity) and wages. Only in high-tech is there evidence of wage differences between referred and non-referred workers, as we show in Table C11. All else equal, referred employees earn 2% higher salaries than non-referred employees, which is in line with the estimates in Brown et al. (2012). Unlike in Brown et al. (2012), we do not see that the difference in salary decreases with driver tenure.

informational advantage in predicting who will want to quit (e.g. based on the referred worker's traits and family situation), whether they will be innovative at a company, and whether they will work safely at a company. In contrast, it may be equally difficult or easy to determine whether a call-center applicant will have excellent average handle time or whether a trucker applicant will attain high miles.

5 Separating Treatment from Selection Effects of Referrals

We have shown that there are some significant behavioral differences between referred and non-referred workers, even though referred and non-referred workers look similar on most observable characteristics. We now turn to the main contribution of the paper, which has proven elusive for past literature: distinguishing selection effects of referrals from treatment effects.

We propose four new tests, and across all of them, the evidence is most consistent with selection benefits from referrals instead of treatment effects. The tests are based on quasi-experimental methods (event study or regression discontinuity) and/or unique data features (social network data and pre-job performance). We believe that each test adds to the literature's understanding of why firms use referrals. Each test on its own, however, is not foolproof, and there is usually some alternative explanation to consider (as is the case for most statistical tests). The strong advantage of us proposing four tests is that it strengthens our confidence in our finding; while one test finding evidence in favor of selection may leave open some questions, we believe that having four tests all pointing to the same conclusion is quite persuasive.

The first three tests attempt to isolate treatment effects and the fourth test attempts to isolate selection effects. Since referral benefits occur through treatment or selection, a test that fails to find evidence for one should be interpreted as evidence in favor of the other.[31]

In addition, even if a metric was uncorrelated with referral status in Section 4, it is still useful to try to separate selection and treatment effects, because either selection or treatment could potentially be negative.[32]

5.1 Impact of Referring Worker Exit

If referring employees monitor or coach referred workers, or if it is more enjoyable for referred workers to work with referring workers, one way to potentially isolate this is to look at what happens when the referring worker leaves. In terms of the model, the departure of the referring worker can be thought of as decreasing k_r (or setting k_r to 0). If treatment effects are operative, we would expect that decreasing k_r should decrease output in the model.

[31] That is, a test that fails to find evidence for treatment is supportive of selection effects and a test that fails to find evidence of selection is supportive of treatment effects.

[32] Referred workers might be negatively selected if referred applicants are nepotistically favored in hiring, even when unqualified. Treatment effects could be negative if referring workers teach "bad habits" or encourage shirking behavior. A zero overall effect could indicate zero selection and zero treatment; positive selection and negative treatment; or negative selection and positive treatment.

Table 6 shows that are no significant impacts of referring worker departure on quitting, productivity, or accidents, thus showing no evidence of treatment effects. We estimate equations of the form:

$$y_{it} = \alpha + \beta * REFGONE_{it} + \gamma_i + \delta X_r + \epsilon_{i,t} \tag{3}$$

where y_{it} is an outcome; $REFGONE_{it}$ is a dummy equal to 1 if the driver who referred i has left by time t; γ_i is an individual fixed effect (omitted when the outcome is quitting); and X_r are characteristics of the worker who referred i. If treatment effects were operative, we might expect quitting and accidents to increase, and productivity to decrease after referred worker departure. In Panel A, we estimate that referring worker departure is associated with an increase in quitting of 0.3 percentage points, a decrease in productivity of 33 miles per week, and a decrease in accidents of 0.1 percentage points, all insignificant. The estimates are moderately precise, though we cannot rule out decent-sized effects. For quitting, the 95% confidence interval on the referring worker having left is -0.09 percentage points to 0.73 percentage points, off a base of 2.4 percent chance of a quit per week. The are highly robust to adding demographics on both the referring and referred workers.[33]

If referring workers provide coaching or mentoring, we may imagine that the cost of an exit may be larger if the referred worker has recently started work compared to later on. However, Panel B of Table 6 fails to support this. The median tenure for the referred worker when the referring worker leaves is 27 weeks. Dividing referral exits into those when the referred worker has more than 27 weeks or less than or equal to 27 weeks, we find no evidence that the exits are more severe when the referred worker has low tenure. If anything, we see slightly more negative impacts, though the difference between the two is always statistically insignificant, and we cannot reject they are the same. After referring worker exit, the probability of quitting increases by 0.25 percentage points for an exit early in the referred worker's tenure, but 0.35 percentage points for an exit later in the referred worker's tenure. Eliminating 0 mile weeks, productivity *increases* by 66 miles following an exit early in the referred worker's tenure, but decreases by 33 miles for an exit later in the referred worker's tenure.

To examine whether the overall statistically zero effect in Table 6 masks trends before or after referring worker exit, we estimate an event study version of (3) and get similar findings as before, as seen in Figure C1. Instead of just looking before and after referring worker exit, we analyze referred worker behavior according to the *number of quarters* before or after referring worker exit:

$$y_{it} = \sum_{j=\underline{T}}^{\overline{T}} \theta_j RG_i^j + \gamma_i + \delta X_r + \epsilon_{i,t} \tag{4}$$

where RG_i^j is a dummy for being j quarters from when the worker who referred i has exited the firm.[34] Looking across quits, miles, and accidents, we see that the trend prior to referring worker

[33]An additional piece of evidence against a coaching or mentoring story is that the impact of referring worker departure is smaller for inexperienced than experienced workers. For newly trained inexperienced workers, the impact is 0.17 percentage points (se=0.30 percentage points), compared to 0.49 percentage points for experienced workers (se=0.30 percentage points).

[34]We normalize $\theta_{-1} = 0$. We assume that $\underline{T} = -5$ quarters and $\overline{T} = 4$ quarters. We "bin up" the end points

exit is relatively smooth. In addition, there are no clear impacts of referring worker exit, although we acknowledge that the quarter-by-quarter estimates are somewhat imprecise.

Threats to Identification and Caveats. The identifying assumption for estimating (3) is that the referring worker's departure is uncorrelated with the error for the referring worker. For example, if there was a common negative shock that affected both the referring and referred worker, it could increase quitting or decrease performance for both the referred and referring worker, independent of any treatment effect of the referring worker on the referred worker. This possibility of a common shock seems likely to *inflate* the magnitude of referring worker departure on referred worker performance, making it more likely to find supposed treatment effects.

We also add the caveat that the relatively solitary nature of production in trucking could make our conclusions less relevant for other industries. Just because the exit of a referring driver does not affect referred drivers does not imply that this would hold true for all industries. The trucking managers we spoke with emphasized that trucking is much less solitary than might be imagined, with drivers in frequent communication with one another using radio and hands-free cellphones. Drivers frequently share tips and give advice, so it is certainly quite possible that the referring driver exiting could have negative impacts of referred workers, if treatment effects were important. However, we find no evidence for them in this test.

5.2 Regression Discontinuity and Difference-in-Discontinuity Designs

If referrals operate through treatment effects, i.e. coaching or monitoring, referred worker behavior may be shaped by the incentives of the referring worker. In particular, if the referring worker receives a bonus provided that the referred worker stays for some period of time, then it seems likely that the referred worker would stay so that the referring worker can receive their bonus. In the call-center data, several locations require referred workers to stay 30 or 90 days before the referring worker receives a bonus. In the trucking data, for experienced driver referrals, referred workers must stay for at least 6 months before the referring worker can receive half their referral bonus. In contrast, for new drivers, the bonus is once the referred driver is hired. In terms of the model, we can think of there being an increase in k_r (the cost of "shirking" or quitting) before the tenure length requirement.

We exploit this contractual variation using a regression discontinuity (RD) design to isolate the impact of changes in k_r.[35] The most basic specification is the following:

$$S_{it} = \alpha + \beta * \mathbf{1}(t > t_0) + f(t) + \epsilon_{it} \tag{5}$$

where S_{it} is a dummy for staying (or not quitting) at tenure t; $\mathbf{1}(t > t_0)$ is a dummy for tenure being above threshold t_0 after which the referring worker receives a bonus; $f(t)$ is a flexible polynomial or spline in tenure; and ϵ_{it} is an error. The main coefficient of interest is β. If referred workers partially internalize the incentives of the person who referred them because treatment effects are operative,

by including dummies for the event time being less than or equal to $\underline{T}$ or greater than or equal to $\overline{T}$. We have also performed the analysis at a higher time frequency, both at the monthly and bi-monthly levels. The results are quite similar, but less precise.

[35] See e.g. Imbens and Lemieux (2008) and Lee and Lemieux (2010) for reviews on regression discontinuity designs.

we would expect β to be negative. That is, the referred worker should be less likely to stay after the referring worker becomes eligible for the bonus. We estimate this equation using OLS restricting the sample to referred workers where the referring worker receives a bonus after t_0; that is, we restrict to locations with 30 or 90 day rules in call-centers and experienced drivers in trucking.

Since only referred workers are eligible to make someone else receive the bonus, we can also implement a difference-in-discontinuity design, comparing impacts at the discontinuity between referred and non-referred workers:

$$S_{it} = \alpha + \beta_1 * \mathbf{1}(t > t_0) * Ref_i + \beta_2 * \mathbf{1}(t > t_0) + f_1(t) * REF_i + f_2(t) + \epsilon_{it} \tag{6}$$

The main coefficient of interest is β_1, representing the differential impact of crossing the t_0 threshold for referred vs. non-referred workers. The advantage of the difference-in-discontinuity design over the basic RD design is that it requires weaker assumptions. For example, it could be that, for some reason, crossing the t_0 threshold has a psychological impact on the worker,[36] thereby violating the RD assumption that the only thing changing at the discontinuity is the referral bonus. By using a difference-in-discontinuity design, we require only that the level of any confounding discontinuity be the same across referred and non-referred workers.[37]

Figure 3 provides visual evidence that there does not appear to be a change in retention after referred workers cross tenure thresholds when referring workers get bonuses, either for call-center workers (panels (a)-(b)) or truckers (panels (c)-(d)). If referred workers were postponing quitting so that referring workers could get bonuses, we would expect a decrease in retention after the bonus tenure thresholds. This decrease would occur for referred workers, but not for non-referred workers. For call-center workers, there does not seem to be much of a change after the bonus tenure threshold. For truckers, there is actually a very slight increase in retention after the bonus, but it is similar for referred and non-referred workers.

Table 7 shows there is no evidence that referred workers become less likely to stay after crossing the tenure threshold when the referring worker receives a bonus. As suggested by Imbens and Lemieux (2008), we show the results are quite robust to different bandwidth windows around t_0 and to different degrees in the polynomial. In Panel A for call-center workers, the estimates are slightly positive but insignificant for the RD regressions and very slightly negative and insignificant for the difference-in-RD regressions. In column 1 of the RD regressions, the coefficient of 0.0028 means that workers are 0.28 percentage points *more* likely to stay after the tenure threshold. The 95% confidence interval of $[-0.0008, 0064]$ means that we can rule out anything more (in magnitude) than a 0.08 percentage point decrease in quitting after the threshold. Panel B shows a pattern of slightly positive (often insignificant, but sometimes significant at the 10% level) impacts for truckers, both in the RD and difference-in-RD designs. These estimates are relatively precise, particularly the difference-in-RD estimates. Overall, the RD and difference-in-RD estimates show no evidence of

[36]The psychological impact could go in either direction. After completing six months, a trucker may feel he has "put in his time" and be less likely to stay. Alternatively, the trucker could feel proud of having completed six months and feel determined to stay.

[37]See Grembi et al. (2012) for this result and more on the difference-in-discontinuity design.

treatment effects of referrals.

Threats to Identification and Caveats. In the basic RD design, identification rests on there not being other changes at the discontinuity. We have multiple discontinuities and do not know of any institutional features that change at the discontinuities. Moreover, by using our difference-in-RD design, we can rule out psychological effects of crossing the discontinuity.[38]

One caveat is that the fact that we see no evidence of quitting postponement does not rule out treatment impact of referrals. It may be possible that referred workers receive mentoring or coaching from the person who referred them, but also feel no compunction about quitting early so that the referring worker does not get a bonus.[39]

5.3 Impact of Having More Friends at Work on Performance

Our next design exploits our rich data on worker friendships. In our model, the treatment effect of referrals operates through the social pressure function $T(r, f)$, which depends on a person's referral status and their number of friends at work. For most of the analysis, we have been assuming that $T(r, f) = k_r r$, that is, social pressure only depends on referral status. However, if treatment effects (e.g. mentoring and coaching) are important for explaining quitting or high-impact performance differences, one might imagine that many of the same effects could operate through the number of friends. For example, if referring workers are less likely to quit because social contacts provide coaching or because it is more enjoyable to work with more social contacts, we would expect both being referred and the number of friends to reduce quitting. We consider the below regression:

$$y_{i,t} = \alpha_t + \beta_1 * REF_i + \beta_2 * NFRIENDS_i + \gamma_t + X_i\Gamma + \epsilon_{i,t} \tag{7}$$

If referrals operate through treatment effects, and social ties are decreasing shirking and preventing workers from quitting, then we would likely expect β_2 to be positive.

Tables 8-11 show that the number of friends appears to be unrelated to most measures of performance across the 3 industries (with a few exceptions). Table 8 shows that people with more friends are no less likely not to quit. Among call-center and high-tech workers, workers with more friends are slightly less likely to quit, but the effect is insignificant. For truckers, those with more friends are slightly *more* likely to quit, but the effects are highly insignificant. The effects are robust to different measures of number of friends. For truckers, we look at log(1+self-reported friends), since some truckers report having a very large number of friends. For high-tech workers, we look at both self-reported friends and other-reported friends (the number of other people listing each worker as a friend), to address the issue that self-reported friendships may simply reflect differences across people

[38] The difference-in-RD design would be confounded if there was a psychological effect of crossing the referral threshold for referred, but not for non-referred workers, that was unrelated to the referring worker receiving a bonus. We believe that such an effect would be unlikely.

[39] A second caveat is that for most of the workers we study here, we do not have information on who referred whom, so we are unable to verify for all workers that the person who referred them is still with the company. Both in the call-centers and trucking, if the referring worker leaves, then he is not eligible to receive a bonus for the referring worker staying past t_0. However, for the trucking data when we can match referred to referring worker, in only 13% of the cases has the referring driver quit the company by the 6-month mark.

in what constitutes a friend. The estimates tell the same story.

Table 9 show that the number of friends does not predict productivity, except in high-tech where it appears to have a positive impact on some metrics.[40] The effects in high-tech are mostly modest. Each additional self-reported friend is associated with an increase of 0.005 standard deviations in terms of subjective performance reviews, though the effect is higher on other-reported friends. Table 10 shows that having more friends is not associated with having fewer trucking accidents, with the effects actually slightly positive (though not significant).[41] However, Table 11 shows that workers with more friends do appear somewhat more innovative, coming up with more patents.

Threats to Identification and Caveats. The number of friends a person has at work is not randomly assigned and may be correlated with unobservables, potentially biasing estimates of β_2 in (7). Most correlations would seem to bias the estimates upwards (e.g. people with unobservedly higher ability may have more friends), which would work against the finding of no treatment effects, but one can imagine stories that would induce a downward bias (e.g. people with unobservedly high ability focus on the job whereas low ability people spend work time socializing).

Even if friends do not predict performance outcomes, it could be that the type of treatment effect and social relationship provided by referring to referred workers might be stronger (or different) than that provided by friends to referred workers. Referring workers might feel an especially strong obligation to assist referred workers, an obligation not shared by non-referring friends. Still, the difference in magnitudes between the coefficients on referral status and number of friends seems telling. For example, in call-centers and high-tech, the coefficients on referral status are 3 and 15 times higher, respectively, than the coefficients on having an additional friend, as we saw in Table 8. In trucking, the friendship coefficients go in the opposite direction than those on quitting.

5.4 Direct Evidence on Better Job-Specific Match: Pre-Job Performance and Job-Fit Surveys

The three previous tests aimed to isolate treatment effects of referrals and generally failed to file them. Our fourth approach is different (though complementary) in that it aims to isolate selection effects, and finds strong evidence for them.

For many jobs, workers participate in pre-work tests that measure their performance or quality. In call-centers, applicants complete "job tests" that measure their likely performance as future employees. The high-tech workers are interviewed multiple times by committees of current workers. Given that work has not yet begun, as long as referring workers do not provide monitoring or coaching to affect performance, these tasks should isolate the selection benefits of referrals.[42]

[40] In call-centers, having more friends is actually associated with *lower* sales conversion, and in trucking, knowing more people before training is associated with fewer miles.

[41] Table 10 is based on the 900 truckers for whom we have social network data, so the sample size is much smaller than in Table 4; while the impact of referral on accidents is not statistically significant in Table 10, the magnitude of the effect is similar (and actually slightly larger), with referred workers 11.8% less likely to have an accident in a given week than non-referred workers (compared to 7% less likely in Table 4).

[42] While our conceptual framework made predictions about differences in worker performance instead of applicant performance, the main idea of the conceptual framework is still useful for thinking about applicant differences. For referred applicants, we might imagine that the signal $\widehat{m}_R$ is actually comprised of two signals, one observed by the

Table 12 shows that referred applicants have significantly higher pre-job performance, both in job tests (call-centers) and interview scores (high-tech). Panel A shows that referred call-center applicants score 0.05 standard deviations higher on their pre-job tests. Panel B shows that referred high-tech applicants score about 0.20 standard deviations higher. Once full controls are added, the coefficient on referrals falls to 0.16 standard deviations, and the effect is similar for both engineers and non-engineers.

While less direct than the evidence in the pre-work performance tests, we also find evidence of selection using surveys that asked workers about very specific aspects of job fit. For the trucking firm, we surveyed drivers about their satisfaction with various aspects of the job after several months of work. Table 13 shows that referred workers are 0.45 standard deviations less likely to feel bothered by an unexpectedly low paycheck (which can occur because drivers are paid by the mile and their productivity fluctuates), 0.3 standard deviations less likely to feel that the demands of the job interfered with their family life, and 0.3 standard deviations more likely to believe that they were home an acceptable number of times per month. While it is not impossible that mentoring or coaching by the referring worker could affect drivers' perceptions of these job features outside of initial selection, it seems much more likely that these factors reflect selection of particular types of people.

Threats to Identification and Caveats. An important confound to interpreting the results in Table 12 as evidence of selection would be if referring workers could coach referred applicants in preparation for job tests or job interviews. However, our discussions with managers indicate that possible coaching is unlikely to explain our results. For call-centers, managers we interviewed thought it was very unlikely that referring workers were preparing referred applicants for job tests. For high-tech workers, the interviews are unstructured. Interviewers are encouraged to evaluate the candidate from his or her own unique perspective, thereby making it difficult for a referring worker to prep an applicant.

For the high-tech applicants, another confound would be if referred applicants were more likely to face a more favorable group of interviewers (perhaps with the interviewers including the referring worker or other friends of the referred applicant), but there is little to support this concern. The high-tech firm uses most of its employees to conduct interviews (in our data, 67% of employees are observed conducting interviews) and interviewers are assigned to applicants in a quasi-random fashion. Applicants are highly unlikely to come across the referring worker or other friends in interviews. Using our data on the interviewers at every interview, as well as data on who each employee is friends with, we observe that only 3% of interviewers are later reported to be a friend of the applicant.[43]

referring worker and one observed by the firm. For non-referred applicants, there is only a signal observed by the firm. Assume that the firm signals are not observed until the end of the application process. Thus, if selection / superior information is important for referral effects, referred applicants will have superior pre-job performance because no referring worker signal is observed for non-referred applicants. See Autor and Scarborough (2008) for a related conceptual framework, where two signals are observed about some applicants, but only one signal about others. Autor and Scarborough (2008) is also about job testing, in their case, looking at whether introducing job testing disadvantages minority workers.

[43]Further, applicants with a greater number of people at an interview who later are declared as one of their friends

5.5 Qualitative Evidence and Discussion

In addition to our four new quantitative tests, we conducted interviews with multiple Human Resource managers in each of the three industries to gain further insights into why referred workers perform better. The interviews were conducted both before we had our results and afterward. Across the board, managers tended to believe that referred workers perform better, and when asked why, they emphasized selection. For example, at one of the call-center firms, the Vice President of Training, Customer Experience and Business Intelligence argued that selection was an important part of referral effects, arguing that referred workers had "a better expectation of what to expect on the job." Echoing this point, the Vice President of Recruiting at the trucking firm said of referred drivers that "The person knows the job they're going into" and that this is what enables them to attain higher retention. At the high-tech firm, a Senior Talent Specialist said "The person who is referring the person in knows about the environment and the culture. The person referring them can vouch for them, to do the job from a technical standpoint, and to fit in with the group, fix this or that. There is more of a back story with a candidate."[44]

From the analysis in Section 5, it is not obvious whether referrals lead to better match for a particular *firm or job* or instead better match for an *industry*. We believe, however, that our results are most consistent with match for the job than for a particular industry. If workers are quitting for other firms within the same industry, and if effects are driven by selection, then referred workers quitting less suggests that they are better matched for the particular job.

Our results differ from existing discussion and analysis of selection vs. treatment impacts of referrals. Heath (2011) argues that referrals help to reduce moral hazard and overcome limited liability constraints in Bangladeshi garment factories. Using household surveys, she shows that referring workers experience wage decreases when their referred worker's wage decreases. Since the workers we study are primarily from the U.S., it could be the case that referrals provide different benefits in wealthy countries and developing ones. Castilla (2005) shows that referred workers have higher productivity at a bank call-center. Based on qualitative interviews, Castilla (2005) suggests that productivity may be higher because referring workers serve as friends or "buddies" to referred workers. However, using explicit data on work friendships as well as on referrals, we find that the relationship between being referred and performance is very different from that of having more work friends and performance.

6 Mechanisms for Selection

Above, we have argued that the evidence is most consistent with referrals shaping worker performance through selection: referrals appear to select workers who are better matched for a particular job. In this section, we provide evidence on mechanisms for superior selection from referrals. We find

or one of their friends of friends do not receive higher interview scores. Regression results available on request.

[44]Some of the managers also believed that referred workers performed better because of treatment effects. For example, the Vice President of Recruiting at the trucking firm said of referred workers "When you go there, you already have an advocate in the family." However, there was substantially more discussion on selection benefits of referrals compared to discussion on treatment benefits.

evidence for homophily as one potentially important mechanism; in addition, we find that referred workers having worse outside options or different beliefs seem unlikely to be important mechanisms.

Table 14 shows evidence of homophily, the tendency of people to refer other people like themselves, in characteristics. We estimate the following equation:

$$x_{i_r} = \alpha + X_r\beta + \epsilon_{i_r} \tag{8}$$

where x_{i_r} is a characteristic of worker i who is referred by worker r; X_r is a vector of characteristics of the referring worker (including x); and ϵ_{i_r} is an error. For the X variables, we examine smoker status, race, age, gender, and marital status, and we control for the referring worker's 3-digit zip-code. We thus ask: Within a given 3-digit zip code, are African-Americans more likely to refer other African-Americans? Are older workers more likely to refer older workers? Looking along the diagonal of Table 14, we see that referred workers are more likely to refer those like themselves. A referring worker living in a given 3-digit zip code, if he is African-American, is more likely to nominate another African-American by 45 percentage points. All else equal, looking at a referring worker who is 10 years older, the worker he refers is likely to be 3 years older.

Next, we show in Table 15 that homophily extends to employee behavior: if an employee exhibits a particular behavior, all else equal, the people he refers are more likely to exhibit the same behavior. We consider regressions of the form:

$$y_{i_r t\tau} = \alpha + \gamma\overline{y}_r + X_r\beta_1 + X_i\beta_2 + \delta_t + \theta_\tau + \epsilon_{i_r t\tau} \tag{9}$$

where $y_{i_r t\tau}$ is a behavior of worker i referred by r at tenure t at time τ; γ is the main coefficient of interest; $\overline{y}_r$ is the average behavior of worker r; X_r is a vector of characteristics of r (including the referring worker's final tenure with the firm); X_i is a vector of characteristics of i; and δ_t and θ_τ are tenure and time fixed effects, respectively. Panel A shows that for a referring driver with average lifetime productivity 100 miles per week above the mean, the person they refer is 29 miles per week above the mean on average. Panel B shows that if the referring driver has an accident at some point, the person they refer is 14% more likely to have an accident.[45]

A confound to identifying behavioral homophily would be if there is some common shock that affected referring and referred workers (e.g. a local shock to the productivity of truckers in a given area). We assuage this concern, first, by adding rich geographic controls, including the 3-digit home residence zipcode of both the referring and referred driver, and by including trucking operating center fixed effects. In columns 3 and 4 of Panel A, we allow the operating center fixed effects to interact flexibly with the time fixed effects, allowing for location-specific time-varying shocks, but this has little impact on the estimates. In addition, the results in Panels A and B are robust to alternative measures of referring worker behavior; instead of using the referring worker's overall

[45] It would also be interesting to examine whether the workers referred by high-productivity and low-productivity referring workers differ in terms of hard-to-observe characteristics such as cognitive and non-cognitive skills. However, because the data on cognitive and non-cognitive skills are from workers hired in 2005 and 2006, whereas the data on who refers whom is from 2007-2009, we cannot do this comparison. See Appendix B.

average productivity, results are similar if we use the referring worker's productivity before the referral, meaning the results are not driven by a new shock that occurs affect the referred driver starts work.

To gain further insight, we ran quantile regressions in Table C12, showing that the impact of higher productivity referring workers is similar at the 10th, 50th, and 90th quantiles. This suggests that more productivity referring workers shift the entire distribution of productivity of the workers they refer.

Why do referrals from high-productivity workers seem to perform better than referrals from low-productivity ones? In our conceptual framework, greater information about referred applicants leads for them to be positively selected with greater productivity. Thus, one explanation is that conditional on a set of possible people to refer, high-productivity referring workers get more precise signals or otherwise choose to refer different types of people than low-productivity workers. An alternative explanation is that high-productivity and low-productivity referring workers tend to have different sorts of friends. However, without data on people's non-work friends, we are unable to distinguish between these two explanations.[46]

6.1 Alternative Mechanisms for Selection

We consider two alternative ways by which selection could operate and find limited support for them.

Worse Outside Option. One possibility is that referred workers are positively selected because they have lower outside options and are thus less likely to quit (Loury, 2006). As seen in Table C2, referred workers do indeed report slightly lower outside options. To examine the importance of differences in outside options for observed quitting differences, we consider quitting models both with and without controls for the outside option, as seen in Table C9. Controlling for the outside option has little impact on the referral dummy coefficient, reducing it by 7%, though the estimates are somewhat imprecise.

Differences in Productivity Beliefs. Another possibility is that referred and non-referred drivers could differ in their productivity beliefs. Using the same set of 900 truckers, Hoffman and Burks (2012) shows that workers who are more optimistic about future productivity, conditional on actual productivity to date, are less likely to quit. However, Table C10 shows that referred and non-referred workers have similar beliefs about their future productivity.

[46] We could extend our conceptual framework to allow for different productivity referring workers to have different types of friends on average, as in Montgomery (1991). We did this for a binary-match version of the conceptual framework and obtained similar predictions. A third explanation for why referrals from high-productivity workers perform better might be that high-productivity and low-productivity referring workers have different treatment effects. High-productivity workers might provide very beneficial coaching / mentoring, whereas low-productivity referring workers might provide unhelpful or even hurtful information. This seems unlikely for our context, given the findings in Section 5 of little evidence of treatment effects.

7 Implications for Profits

We now show that the behavioral differences between referred and non-referred workers translate into significant differences in profits from each kind of worker. More surprisingly, profits differ substantially depending on the ability of the referring worker. We focus the profits analysis on the trucking industry since the production process is relatively simple, making it relatively easy to calculate profits.

We measure firm profits using profits per worker, defined as the average discounted profit stream from a worker during his time with the company (once a worker exits, the profit stream stops). Profits per worker equals discounted revenue, minus costs from a worker having any accidents, minus the cost of training and recruiting a worker (which depends on whether the worker needs commercial driver's license training and whether they were referred), minus any referral bonuses paid, plus any training contract penalties collected when the worker quits:

$$\begin{aligned}\pi &= \text{Discounted revenue - Accident cost - Training/Recruiting cost - Referral bonus + Recovered damages} \\ &= \sum_{t=1}^{\infty} \delta^{t-1}\left(1-Q_t\right)\left[y_t\left(P-w_t-mc\right)-FC-c_A A_t\right]-TC\left(E,r\right)-500r-500\delta^{26}rE\left(1-Q_{26}\right)+\left(1-E\right)\sum_{t=1}^{\infty}\delta^{t-1}\theta k_t q_t\end{aligned} \quad (10)$$

where q_t is a dummy for quitting in week t; $Q_t = \sum_{s=1}^{t} q_s$ is whether a driver has quit in the first t weeks; y_t is a driver's weekly miles; P is the price the firm charges for one mile of shipment; w_t is wage per mile; mc is the non-wage marginal cost per mile (i.e. truck wear and gas costs); FC is fixed costs per week (i.e. support for the drivers and the opportunity cost of the truck); c_A is the cost of an accident; A is a dummy for having an accident; $TC(E, r)$ is the cost of training and recruiting a worker; E is a dummy for whether the worker is experienced; r is a dummy for whether the worker was referred; k_t is the quit penalty for a worker who quits in week t; and θ is the share of the training contract penalty collected by the firm. We assume values for P, mc, FC, c_A, TC, and θ based on consultation with the trucking firm.[47]

Table 16 shows profits per worker is significantly higher for referred workers, and varies strongly with the type of person making the referral. The average referred worker yields $2,201 in discounted profits whereas the average non-referred worker yields $1,756.[48] Note that bonuses paid to referred workers are taken out of the profits for referred workers, so the difference is the difference in discounted profits while accounting for referred workers receiving a bonus. In addition, we see that workers

[47] Specifically, we assume that $P = \$1.90$ per mile, $mc = \$1.20$ per mile, $FC = \$475$ per week, $c_A = \$1,000$, $TC = \$2,500 - \$500r - \$1,750e$, and $\theta = 0.3$. The relatively small assumed accident cost of $1,000 reflects the broad definition of an accident in the data. In addition, we assume that $\delta = 0.9957$, following Hoffman and Burks (2012). Hoffman and Burks (2012) estimate a structural model of quitting using a subset of the trucking firm data; they assume a weekly discount factor of 0.9957, corresponding to a "low" annual discount factor of 0.8, finding the model works best for discount factors in that range. If instead we assume a higher δ for our profits calculation here, the level of profits slightly increases, but our conclusions are identical. For example, if we assume $\delta = 0.9990$, corresponding to an annual discount factor of 0.95, referred average profits per worker is $2,838 compared to $2,285 for non-referred workers, a difference of 24%. Profits per worker from above-median referring workers is $4,493 compared to $1,412 for below-median referring workers.

[48] This difference in profits does not capture differences in the tendency of referred and non-referred workers to *make* referrals themselves. Table C6 shows that referred workers are 4 percentage points more likely to ever make a referral than non-referred workers. The difference we calculate may thus be an underestimate of the profit difference between referred and non-referred workers.

referred by above-median productivity drivers yield $4,190 in profits, whereas referrals from below-median productivity workers yield $1,063 in profits, which is below the profits from the average non-referred worker. In addition, there is a large difference in profits per worker based on whether the worker making the referral is ever observed to quit or not.

Interpretation. Although the profits formula, (10), includes several parts, so as to make the calculation as precise as possible, the main mechanisms behind the results are simple and seemingly general. Because referred workers are less likely to quit, there is less time spent "moving up" the productivity-tenure curve and more time achieving high-level productivity.

An implication of these results is that firms may gain by encouraging referrals from their most productive workers. In the firms we study here, all workers are eligible to receive bonuses for referring new employees who are hired and/or who stay for some period of time. Instead, firms may wish to give the largest incentives to their most productive workers. This could be achieved in different ways. First, firms could choose to award referral bonuses conditional on the referring worker having average productivity above some threshold. Alternatively, it might be optimal for workers only to become eligible for bonuses once they have achieved a certain tenure level. Finally, even without changing bonuses, firms could decide to award the greatest weight in their hiring process to those referred by the best workers.[49]

8 Conclusion

Why do so many firms appear to value using employee referrals to hire new workers? We present a conceptual framework illustrating how referrals can have both selection and treatment effects on worker behavior. We document that, compared to non-referred workers, referred workers are substantially less likely to quit, are more innovative, and have fewer accidents, even though they have similar characteristics (e.g. schooling, cognitive ability, non-cognitive ability, and experimental preferences) and score no higher on standard productivity measures. In the main contribution of the paper, we develop four new tests to separate selection and treatment impacts of referrals. All four tests provide evidence of selection instead of treatment. The behavioral differences we estimate translate into large differences in the profitability of referred and non-referred workers, and referrals from high-productivity referring workers are especially profitable.

Although it is difficult without a full general equilibrium model to analyze total welfare, our results suggest that referrals may have a significant positive impact on total welfare in the industries

[49]The reader may ask whether any of the firms we study are already incentivizing or giving preference to referrals from their best candidates. None of the nine firms gives different financial incentives for referrals based on any characteristics of the referring worker. In addition, none of the managers we spoke to said there was any advantage in the hiring process in being referred by some current employees versus others. In fact, at some firms, the hiring manager will not even *know* who the referring worker is, simply knowing that someone referred them. For example, the Vice-President of Driver Recruiting at the trucking firm said it was "very unlikely that the recruiter would know [the identity of] the referring driver. It is possible that they could figure it out but it would take a good bit of investigating." As to why firms may not be already optimally incorporating information about the referring worker into the design of their referral programs, it does not seem likely that incorporating this information would be logistically costly. Rather, it could simply reflect that optimal management practices, like other technologies, often face informational barriers to adoption, and only become adopted once managers become aware of them (Bloom et al., 2013).

we study, both by improving worker-firm matching, and because of the externalities associated with increased high-tech innovation and decreased trucking accidents.

Our findings have important implications for both policymakers and managers. For policymakers, there is frequently concern that a lack of social ties contributes to certain workers being disadvantaged.[50] Because referrals appear to benefit workers by matching them to better jobs as opposed to supporting them once work has begun, policy efforts on building social networks should focus on helping workers before they have jobs, as opposed to after work has begun.

For managers, our results on workers referring others like themselves suggest that firms may benefit by incentivizing referrals from their best workers. More speculatively, our results may be relevant for managers on the issue of "weak ties" vs. "strong ties." It has been argued that "weak ties" play an important role for workers in finding jobs (Granovetter, 1973, 1974); however, given our finding that referrals operate because of superior information about candidates, if "strong tie" relationships yield superior information about applicants compared to "weak tie" ones, then firms may wish to incentivize referrals more for applicants who are "strongly tied" to existing employees instead of "weakly tied."

We conclude by highlighting three limitations of our paper. First, our results focused on three specific industries, call-centers, trucking, and high-tech, and thus may not be generalizable to all firms in the economy. However, compared to most work in personnel economics that uses individual-level productivity data from a single firm in a single industry, the paper is a significant advance, and our findings are relatively consistent across industries. Further work is needed to confirm the results in other contexts. Second, our tests for selection vs. treatment impacts of referrals are not from a randomized experiment and rely on several identifying assumptions. However, we believe these assumptions are reasonable in our setting and that our quasi-experimental tests are a significant advance for the literature. Third, although we show that referred workers are valuable for firms and analyze why, our results *do not* necessarily imply that firms should expand employee referral programs. Our regression results address *average* differences between referred and non-referred workers, not the impact of making changes on the *margin*. To convincingly demonstrate that firms should expand employee referral programs, we would need exogenous variation in referral bonus size.[51] We intend to explore these issues in future work.

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[50] See e.g. "Employers Increasingly Relying on Internal Referrals in Hiring," New York Times, January 27, 2013.

[51] Such variation can also be used to estimate the impact of referrals on outcomes for workers; Abaluck et al. (2013) are working on an experiment along these lines.

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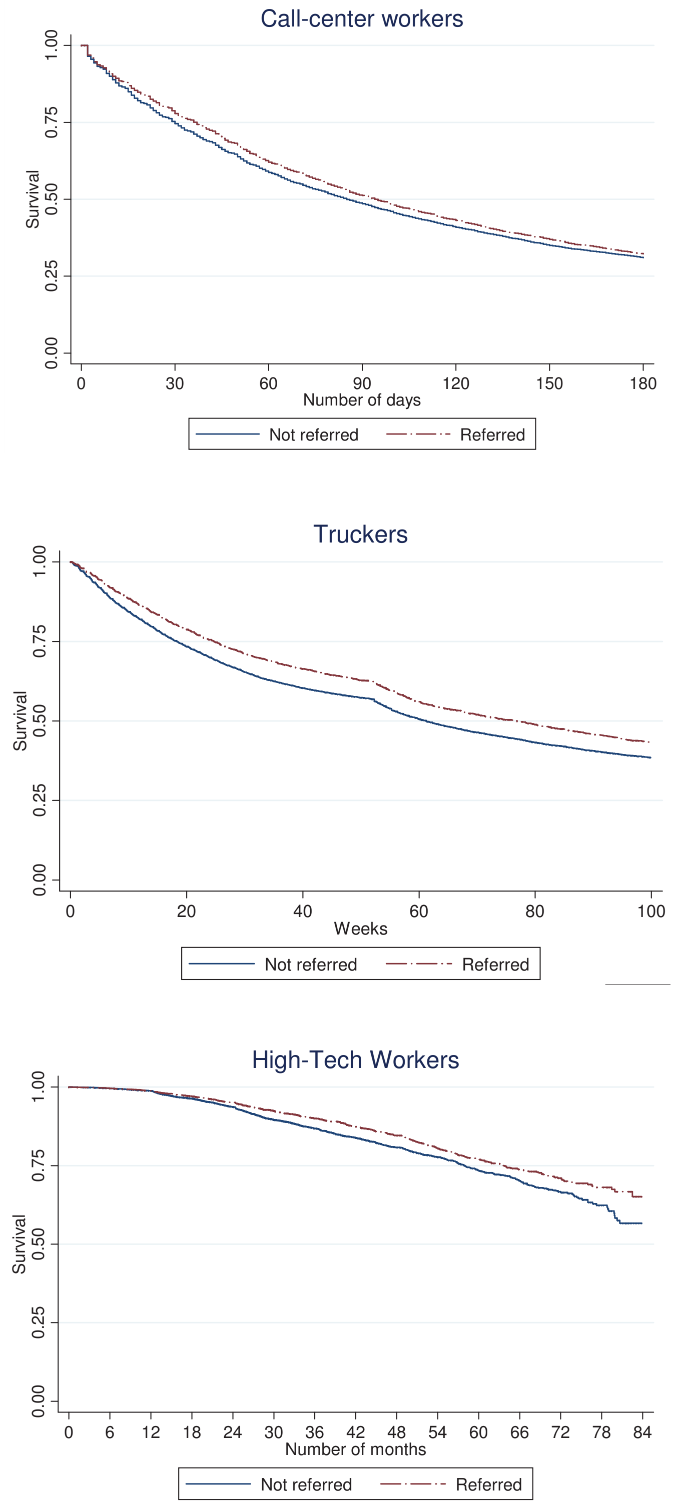
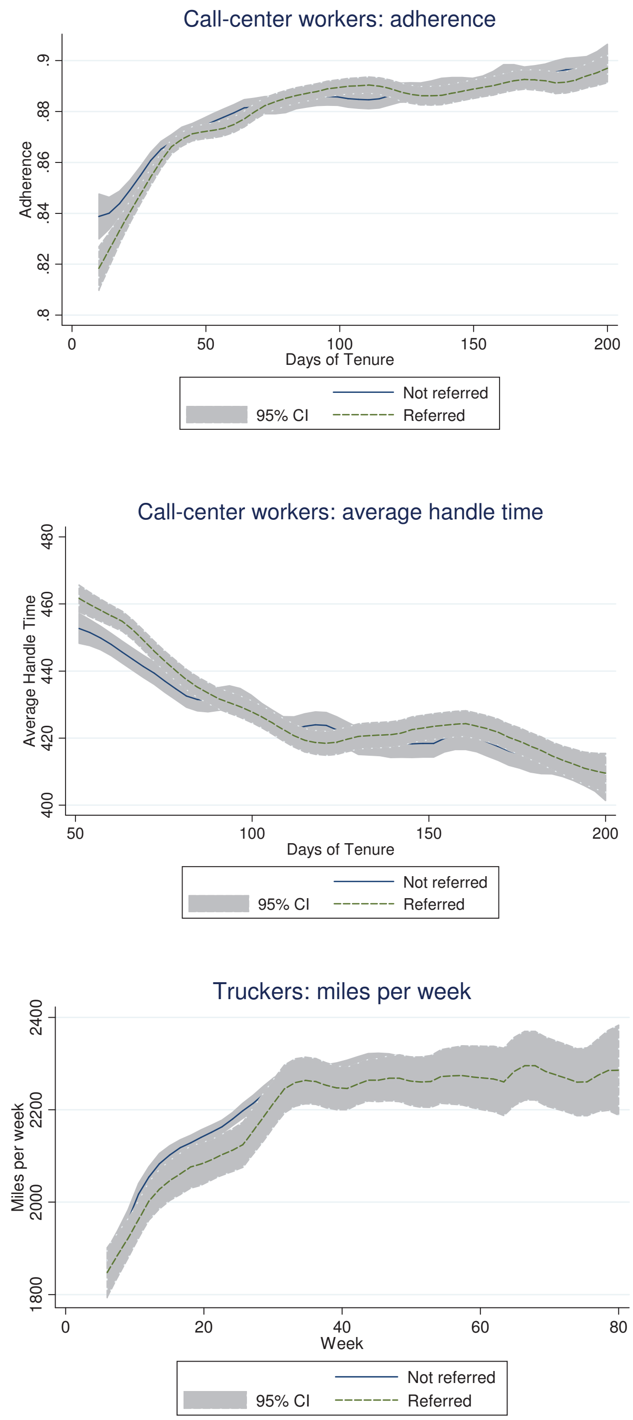
Figure 1: Referred Workers are Less Likely to Quit than Non-Referred Workers



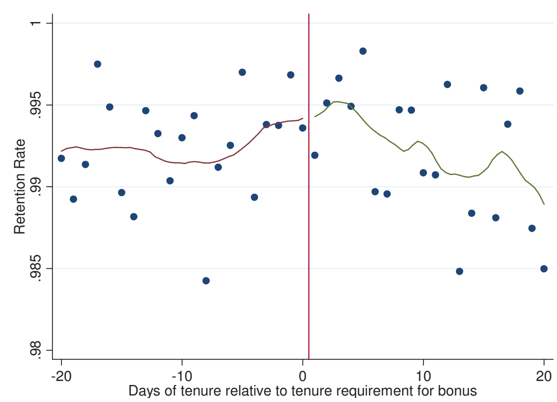
Notes: These graphs plot the share of workers surviving at any given tenure level, comparing referred and non-referred workers. The top graph is from seven call-center firms, the middle graph is from a trucking firm, and the bottom graph is from a high-tech firm. Across all three graphs, a Wilcoxon test for equality test for equality of No Referral vs. Referrals yields $p < 0.01$. The call-center and high-tech firm graphs include all exits, whereas the trucking firm graph only analyzes exits from quits.

Figure 2: Referred and Non-referred Workers have Similar Non-rare Productivity Growth

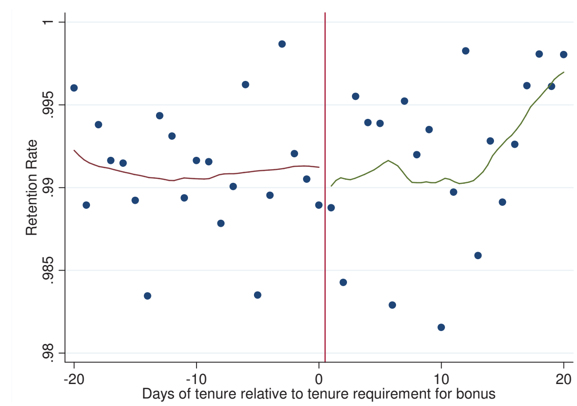


Notes: These graphs compare referred and non-referred workers on non-rare productivity as a function of tenure. The top two graphs are from workers in the call center industry and the bottom graph is from workers in the trucking industry. Adherence refers to schedule adherence and is measured on a scale from 0 to 100 percent; a higher number is indicative of higher productivity. Average handle time is a measure of the average time spent per call; a lower number is indicative of higher productivity. For miles per week in trucking, a higher number is indicative of higher productivity. The figures are plotted using a local polynomial regression with an Epanechnikov kernel.

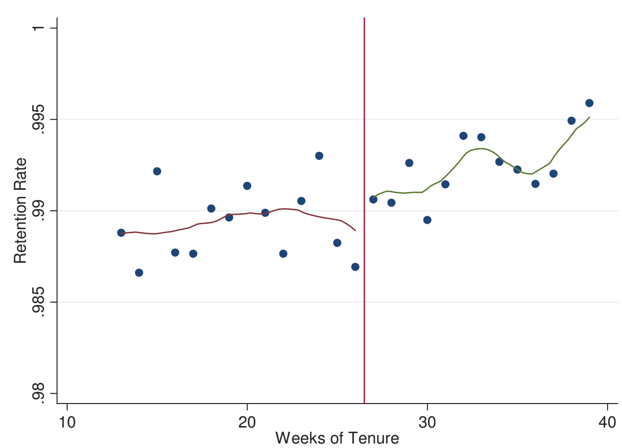
Figure 3: Separating Treatment from Selection, Section 5.2: Do Workers Postpone Quitting so Referring Workers Can Receive Bonuses? Regression Discontinuity Evidence



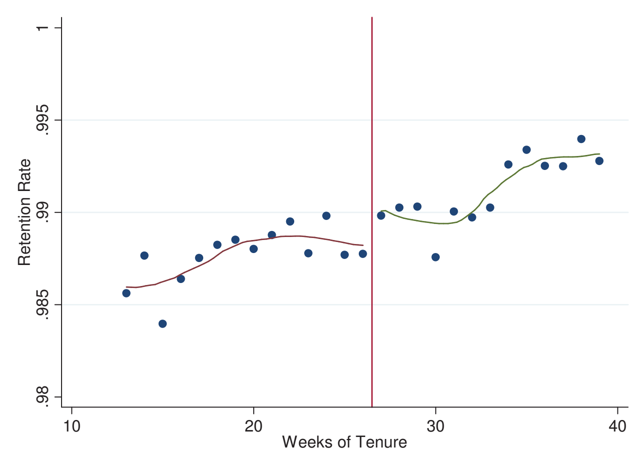
(a) Referred Workers, Call-centers



(b) Non-referred Workers, Call-centers



(c) Referred Workers, Trucking



(d) Non-referred Workers, Trucking

Notes: These graphs analyze whether workers are willing to postpone quitting so that the person who referred them can receive a bonus. If this were the case, retention should decrease after the bonus tenure requirement, with workers postponing their quitting until after the required tenure length. We see no evidence for this in these pictures. The points plotted are the average share of drivers not quitting in a week. For the call-center workers, the bandwidth is plus or minus 20 days. For the truckers, the bandwidth is plus or minus 13 weeks. The lines are plotted using a local polynomial regression.

Table 1: Do Referred Workers Have More Desirable Characteristics? Schooling & Work Experience, Cognitive Ability, Non-cognitive Ability, and Experimental preferences

Panel A: Schooling & Work Experience Dep var:	(1) Schooling in years	(2) Schooling in years	(3) Schooling in years	(4) Related experience in years	(5) Number of jobs in last 2 years	(6) Maximum number of years at a previous job
Sample:	Call-center applicants	Trucking workers	High-tech workers	Trucking workers	Trucking workers	Trucking workers
Referral	-0.0858***	-0.2093	0.0540**	-0.0020	0.1186	-0.1555
	(0.0124)	(0.1340)	(0.0270)	(0.1168)	(0.1006)	(0.5848)
Observations	49,565	894	10,890	894	893	517
R-squared	0.1012	0.0467	0.1483	0.0796	0.0577	0.401
Mean dep var	12.61	12.85	16.97	1.170	1.601	8.044

Panel B: Cognitive Ability Dep var:	(1) Intelligence (normalized)	(2) IQ (normalized)	(3) SAT total score
Sample:	Call-center applicants	Trucking workers	High-tech workers
Referral	-0.024***	-0.120	14.943
	(0.004)	(0.099)	(10.032)
Observations	314,549	849	899
R-squared	0.0093	0.078	0.180
Mean dep var	0	0	1401

Panel C: Personality Dep var:	(1) Big 5 index (normalized)	(2) Conscientiousness (normalized)	(3) Neuroticism (normalized)	(4) Agreeableness (normalized)	(5) Extraversion (normalized)	(6) Openness (normalized)
Call-centers, applicants:						
Referral	-0.018***	-0.126***	-0.009**	-0.032***	0.104***	-0.056***
	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	350,377	357,511	355,939	357,424	357,424	357,424
R-squared	0.010	0.015	0.003	0.007	0.014	0.022
Trucking, workers:						
Referral	-0.033	-0.044	0.055	-0.169*	0.102	NA
	(0.049)	(0.092)	(0.091)	(0.099)	(0.092)	
Observations	895	895	895	895	895	
R-squared	0.029	0.019	0.015	0.061	0.029	
High-tech, workers:						
Referral	0.009	0.054	-0.067	-0.151***	0.079	-0.000
	(0.026)	(0.049)	(0.049)	(0.048)	(0.048)	(0.049)
Observations	1,853	1,853	1,855	1,855	1,854	1,854
R-squared	0.034	0.037	0.043	0.031	0.031	0.033

Panel D: Experimental Preferences Dep var:	(1) CRRA risk aversion	(2) Patient options chosen	(3) Beta in HD model	(4) Delta in HD model	(5) Trust	(6) Altruism V1	(7) Altruism V2
Sample:	Trucking workers	Trucking workers	Trucking workers	Trucking workers	Trucking workers	Trucking workers	Trucking workers
Referral	-0.021	-0.025	-0.015	-0.006	-0.536**	-0.018	-0.251
	(0.157)	(0.157)	(0.018)	(0.012)	(0.226)	(0.185)	(0.187)
Observations	894	894	894	872	894	894	894
R-squared	0.021	0.053	0.019	0.006	0.035	0.044	0.016
Mean dep var	0.503	0.597	0.841	0.974	3.367	1.659	3.641

Notes: This table presents regressions of various outcomes on whether a worker was referred and controls. An observation is a worker or applicant, with robust standard errors in parentheses. For an explanation of how the different variables are measured and defined, see Appendix B. For the call-centers, the regressions include race, age, and gender controls. For trucking, the regressions include race, age, gender, marital status, and work type controls. For high-tech, the regressions include race dummies, gender dummies, month of hire dummies, job category dummies, and job rank dummies. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 2: Referrals and Quitting

Panel A: Call-center workers	(1)
Referral	-0.13***
	(0.03)
Observations	20,040

Panel B: Truckers	(1)	(2)	(3)	(4)	(5)
Referral	-0.115***	-0.115***	-0.116***	-0.119***	-0.114***
	(0.022)	(0.022)	(0.022)	(0.025)	(0.025)
State unemployment rate		-0.042***	-0.045***	-0.033***	-0.035***
		(0.010)	(0.010)	(0.011)	(0.011)
Avg miles to date			-0.038***	-0.034***	-0.032***
			(0.002)	(0.003)	(0.003)
Demog Controls	No	No	No	No	Yes
Sample	All	All	All	Has demogs	Has demogs
Observations	0.94M	0.94M	0.94M	0.85M	0.85M
Clusters	N	N	N	0.80N	0.80N

Panel C: High-tech Workers	(1)	(2)
Referral	-0.30***	-0.33***
	(0.06)	(0.06)
Demog Controls	No	Yes
Observations	16,223	16,223

Notes: This table examines whether a worker's referral status predicts quitting. All specifications are Cox proportional hazard models with standard errors clustered by worker in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

In Panel A, an observation is a worker. The controls are cohort (month of hire) dummies, location dummies, and client dummies. We restrict to workers who are with the company for 200 days or less.

In Panel B, an observation is a worker week. Standard errors clustered by training facility-week of hire in parentheses. Controls include cohort (year of hire) dummies, year dummies, and driver training contracts. Demographic controls include gender, race dummies, marital status, and age bin dummies for the different age groups: 25-30, 30-35, 35-40, 40-45, 45-50, 50-55, 55-60, and 60-80. Controls for training contracts are included in all regressions. The exact sample size is withheld to protect firm confidentiality, $M >> 100,000$, $N >> 10,000$.

In Panel C, an observation is a worker. All regressions include month of hire fixed effects. Demographic controls are interview scores, race dummies, gender dummies, whether the worker is an engineer, and education dummies.

Table 3: Referrals and Non-rare Productivity Measures (Normalized)

Panel A: Call-center Workers	(1)	(2)	(3)	(4)	(5)
Dep var:	Adherence share	Average handle time	Sales conversion rate	Quality assurance	Customer satisfaction
Referral	-0.021	-0.000	-0.027*	7.8e-5	0.003
	(0.013)	(0.010)	(0.015)	(9.5e-5)	(0.003)
Observations	152,683	749,848	134,386	31,908	603,860
Clusters	3136	12,497	3,192	2,864	11,859
R-squared	0.119	0.559	0.655	0.178	0.033

Panel B: Truckers						
Sample:	All Weeks		Exclude 0 Mile Weeks		Trim 1/99 %	
	(1)	(2)	(3)	(4)	(5)	(6)
Referral	-0.024**	-0.019*	0.001	0.005	0.001	0.005
	(0.010)	(0.011)	(0.010)	(0.011)	(0.010)	(0.011)
Demog Controls	No	Yes	No	Yes	No	Yes
Observations	$0.99M$	$0.91M$	$0.86M$	$0.79M$	$0.84M$	$0.78M$
Clusters	$0.99N$	$0.80N$	$0.85N$	$0.71N$	$0.85N$	$0.71N$
R-squared	0.180	0.165	0.080	0.086	0.079	0.085

Panel C: High-tech Workers	(1) Subjective performance	(2) Subjective performance	(3) Hours worked	(4) Code reviews	(5) Bug actions	(6) Builds	(7) P4calls	(8) Wiki edits	(9) Views
Referral	0.038***	0.040***	-0.086***	0.006	0.025	-0.001	-0.001	0.064***	0.036**
	(0.011)	(0.012)	(0.014)	(0.012)	(0.015)	(0.009)	(0.009)	(0.016)	(0.014)
Demogs	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	104,255	104,255	250,980	289,689	289,689	289,689	289,689	289,689	289,689
R-squared	0.08	0.08	0.04	0.12	0.03	0.11	0.01	0.02	0.05
Clusters	16546	16546	11066	11123	11123	11123	11123	11123	11123

Notes: This table examines whether a worker's referral status predicts productivity. All specifications are OLS regressions with standard errors clustered by worker in parentheses. Each productivity measure has been normalized so that the coefficients are all in standard deviation units. The non-rare productivity metrics are described further in Appendix B. * significant at 10%; ** significant at 5%; *** significant at 1%

In Panel A, productivity is measured using one of 5 different normalized measures. The workers are from 7 call-center firms, spread across many different locations. Each column is an OLS regression including day of tenure dummies, location dummies, client dummies, and the number of times that each outcome was measured to compute the dependent variable. An observation is a worker-day.

In Panel B, productivity is measured in terms of miles and then normalized. The specifications include time fixed effects (for each month), cohort fixed effects (by year of hire), tenure fixed effects (by week), work type controls, and the annual state unemployment rate. An observation is a driver-week. "Trim 1/99%" refers to trimming the lowest 1% and highest 1% of the miles observations (ignoring all 0 mile weeks). Demographic controls include gender, race dummies, marital status, and age bin dummies for the different age groups: 25-30, 30-35, 35-40, 40-45, 45-50, 50-55, 55-60, and 60-80. Controls for training contracts are included in all regressions. The exact sample size is withheld to protect firm confidentiality, $M >> 100,000$, $N >> 10,000$.

In Panel C, productivity is measured in terms of a subjective performance rating or objective performance measure (all normalized). Demographic controls are interview scores, race dummies, gender dummies, whether the worker is an engineer, and education dummies.

Table 4: Referrals and Trucking Accidents

	(1)	(2)	(3)
Dependent var (0-1):	Accident	Preventable accident	Non-preventable accident (placebo test)
Referral	-0.00133***	-0.00104***	-0.00026
	(0.00031)	(0.00019)	(0.00017)
Mean dep var	.01899	.00862	.00708
% reduction for referred drivers	7.0%	12.1%	3.7%
Observations	M	M	M
R-squared	0.00587	0.00440	0.00183

Notes: This table examines whether a worker's referral status predicts employee trucking accidents. All specifications are linear probability models with time fixed effects (for each year), cohort fixed effects (by year of hire), tenure fixed effects (by week), work type controls, and seasonality controls. Standard errors clustered at the driver level in parentheses. An observation is a driver-week. The exact sample size is withheld to protect firm confidentiality, $M >> 100,000$, $N >> 10,000$. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Referrals and Innovation, High-tech Workers

Panel A: Patents	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep var:	Num patents	Num patents	Num patents	Num patents	Ever patent	Ever patent	Citation-weighted patents	Citation-weighted patents
Referral	0.0184*	0.0143	0.2447***	0.2374***	0.0091**	0.0074**	0.1901*	0.1956**
	(0.0097)	(0.0100)	(0.0846)	(0.0836)	(0.0035)	(0.0035)	(0.0997)	(0.0974)
Method	OLS	OLS	Negative binomial	Negative binomial	OLS	OLS	Negative binomial	Negative binomial
Demog Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	17,168	17,168	17,168	17,168	17,168	17,168	17,168	17,168
R-squared	0.1343	0.1428			0.1996	0.2077		
Mean dep var	0.108	0.108	0.108	0.108	0.0566	0.0566	0.1661	0.1661

Panel B: Ideas on Idea Board	(1)	(2)	(3)	(4)
Dep var:	Num ideas	Num ideas	Log(1+Ideas)	Log(1+Ideas)
Referral	0.082*	0.089*	0.030**	0.029*
	(0.050)	(0.049)	(0.015)	(0.015)
Method	Negative binomial	Negative binomial	OLS	OLS
Demog Controls	No	Yes	No	Yes
Observations	11,123	11,123	11,123	11,123
R-squared			0.068	0.073

Notes: Robust standard errors in parentheses. An observation is a worker. Demographic controls are interview scores, race dummies, gender dummies, and education dummies. All regressions also include month of hire dummies, job category dummies, and job rank dummies. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Separating Treatment from Selection, Section 5.1: The Impact of Referring Worker Exit on Referred Worker Behavior for Truckers

Panel A: Baseline Results	(1)	(2)	(3)	(4)
Dep var:	Quit (0-1)	Miles	Miles if miles> 0	Accident (0-1)
After referring driver has left	0.0031	-33.13	14.65	-0.0011
	(0.0021)	(67.23)	(45.80)	(0.0072)
Observations	41,670	40,927	36,648	41,670
R-squared	0.0945	0.3484	0.2900	0.0447
Mean dep var	0.0238	1684	1880	0.0202

Panel B: Early vs. Late Referring Worker Exits	(1)	(2)	(3)	(4)
Dep var:	Quit (0-1)	Miles	Miles if miles> 0	Accident (0-1)
After referring driver has left * exit soon after referred starts	0.0025	-2.33	67.57	0.0022
	(0.0034)	(90.14)	(57.08)	(0.0113)
After referring driver has left * exit far after referred starts	0.0035	-69.86	-32.99	-0.0051
	(0.0026)	(98.87)	(65.87)	(0.0082)
Observations	41,670	40,927	36,648	41,670
R-squared	0.0945	0.3484	0.2901	0.0447
Mean dep var	0.0238	1684	1880	0.0202

Notes: All specifications are OLS regressions including month-by-month dummies, cohort (year of hire) dummies, and week of tenure dummies. Standard errors clustered at the worker level in parentheses. An observation is a worker-week. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Separating Treatment from Selection, Section 5.2: Do Referred Workers Postpone Quitting so Referring Workers Can Receive Bonuses? Regression Discontinuity and Difference-in-RD Evidence

Panel A: Call-center	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	0.0028	0.0021	0.0022	0.0030	0.0008	0.0029	0.0028	0.0035
$\mathbf{1}(\text{tenure}> t_0)$	(0.0018)	(0.0030)	(0.0031)	(0.0031)	(0.0014)	(0.0022)	(0.0022)	(0.0023)
Sample restriction	Ref	Ref	Ref	Ref	Ref	Ref	Ref	Ref
Polynomial deg	2	2	3	4	2	2	3	4
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Bandwidth	± 10day	± 10day	± 10day	± 10day	± 20day	± 20day	± 20day	± 20day
Observations	10,534	10,534	10,534	10,534	20,945	20,945	20,945	20,945
R-squared	0.0004	0.0061	0.0062	0.0063	0.0001	0.0131	0.0131	0.0132
Diff-in-RD Estimate	-0.0004	-0.0003	-0.0003	-0.0003	-0.0009	-0.0008	-0.0008	-0.0008
$\mathbf{1}(\text{tenure}> t_0)$*Ref	(0.0023)	(0.0023)	(0.0023)	(0.0023)	(0.0017)	(0.0016)	(0.0016)	(0.0016)
Sample restriction	None	None	None	None	None	None	None	None
Polynomial deg	2	2	3	4	2	2	3	4
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Bandwidth	± 10day	± 10day	± 10day	± 10day	± 20day	± 20day	± 20day	± 20day
Observations	23,731	23,731	23,731	23,731	47,050	47,050	47,050	47,050
R-squared	0.0003	0.0041	0.0041	0.0042	0.0001	0.0075	0.0076	0.0076
Panel B: Truckers	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	0.0045	0.0044	0.0069*	0.0074*	0.0039*	0.0040*	0.0053*	0.0054*
$\mathbf{1}(\text{tenure}> t_0)$	(0.0029)	(0.0029)	(0.0040)	(0.0040)	(0.0021)	(0.0021)	(0.0028)	(0.0028)
Sample restriction	Exp&Ref	Exp&Ref	Exp&Ref	Exp&Ref	Exp&Ref	Exp&Ref	Exp&Ref	Exp&Ref
Polynomial deg	2	2	3	4	2	2	3	4
Demog controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Bandwidth	± 10 wk	± 10 wk	± 10 wk	± 10 wk	± 20 wk	± 20 wk	± 20 wk	± 20 wk
Observations	0.022M	0.022M	0.022M	0.022M	0.045M	0.045M	0.045M	0.045M
R-squared	0.0091	0.0099	0.0099	0.0100	0.0091	0.0097	0.0097	0.0097
Diff-in-RD Estimate	0.0028	0.0028	0.0025	0.0022	0.0013	0.0013	0.0004	0.0006
$\mathbf{1}(\text{tenure}> t_0)$*Ref	(0.0018)	(0.0018)	(0.0024)	(0.0025)	(0.0013)	(0.0013)	(0.0017)	(0.0017)
Sample	Exp	Exp	Exp	Exp	Exp	Exp	Exp	Exp
Polynomial deg	2	2	3	4	2	2	3	4
Demog controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Bandwidth	± 10 wk	± 10 wk	± 10 wk	± 10 wk	± 20 wk	± 20 wk	± 20 wk	± 20 wk
Observations	0.096M	0.096M	0.096M	0.096M	0.195M	0.195M	0.195M	0.195M
R-squared	0.0111	0.0115	0.0115	0.0115	0.0098	0.0101	0.0101	0.0101

Notes: This table examines whether referred workers postpone quitting so that their friends can receive tenure-specific referral bonuses. All specifications are regressions with standard errors clustered by worker in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

In Panel A, an observation is a worker-day. Controls include location dummies, client dummies, month of hire dummies, and job type dummies. t_0 varies by call-center location, and is either 30 days or 90 days. For the RD design for call-center workers, the sample is restricted to workers who are referred, i.e. "Ref." For the difference-in-RD design, there is no sample restriction.

In Panel B, an observation is a worker-week. All specifications include time fixed effects (for each year), cohort fixed effects (by year of hire), and work type dummies. t_0 is 6 months (26 weeks). For the RD design for truckers, the sample is restricted to workers who are referred and experienced (as only referred experienced drivers are eligible for the referring worker to receive a bonus after 6 months), i.e. "Exp&Ref." For the difference-in-RD design, the sample is restricted to experienced workers, i.e. "Exp." The exact sample size is withheld to protect firm confidentiality, $M >> 100,000$, $N >> 10,000$.

Table 8: Separating Treatment from Selection, Section 5.3: The Impact of Number of Friends on Quitting

Panel A: Call-center workers	(1)
Referral	-0.07**
	(0.03)
Friends at company	-0.02
	(0.02)
People know at company	-0.03
	(0.02)
Observations	20,040

Panel B: Truckers	(1)	(2)	(3)	(4)	(5)	(6)
Referral		-0.181		-0.179		-0.176
		(0.143)		(0.142)		(0.142)
Friends made during training	0.002	0.006				
	(0.010)	(0.010)				
Annual income if continued at past jobs		0.008**		0.008**		0.008**
		(0.003)		(0.003)		(0.003)
Log(1+Friends made during training)			0.013	0.040		
			(0.076)	(0.076)		
Trainees known before training					-0.003	-0.009
					(0.115)	(0.121)
Controls	No	Yes	No	Yes	No	Yes
Observations	38,460	38,381	38,460	38,381	38,460	38,381

Panel C: High-tech Workers	(1)	(2)
Referral	-0.320***	-0.319***
	(0.061)	(0.061)
Friends at company (self listed)	-0.021	
	(0.018)	
Friends at company (colleague listed)		-0.018
		(0.030)
Demog Controls	Yes	Yes
Observations	16,223	16,223

Notes: This table examines whether the number of friends a worker has predicts quitting. All specifications are Cox proportional hazard models with standard errors clustered by worker in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

In Panel A, an observation is a worker. The controls are cohort (month of hire) dummies, location dummies, and client dummies. We restrict to workers who are with the company for 200 days or less.

In Panel B, an observation is a worker week. All drivers in the sample were hired at one of the training schools in late 2005 or 2006. The specifications include tenure fixed effects (by week). Controls include age, gender, race, education, work type, self-reported income if the driver had not gone through training, and education level. The magnitudes of the estimated referral coefficients are similar to those in Panel B of Table 2, but are less precise because the sample size is much smaller.

In Panel C, an observation is a worker. All regressions include month of hire fixed effects. Demographic controls are interview scores, race dummies, gender dummies, whether the worker is an engineer, and education dummies.

Table 9: Separating Treatment from Selection, Section 5.3: The Impact of Number of Friends on Non-rare Productivity Measures (Normalized)

Panel A: Call-center Workers Dep var:	(1) Adherence share	(2) Average handle time	(3) Sales conversion rate	(4) Quality assurance	(5) Customer satisfaction
Referral	-0.0001	-0.0040	-0.0441**	0.0001	0.0023
	(0.0163)	(0.0122)	(0.0173)	(0.0001)	(0.0048)
Number of friends at company	0.0038	0.0007	0.0077	-0.0000	-0.0034
	(0.0055)	(0.0051)	(0.0050)	(0.0000)	(0.0032)
Number of people you know at company	-0.0175**	0.0015	0.0078	0.0000	0.0028
	(0.0070)	(0.0053)	(0.0071)	(0.0000)	(0.0026)
Observations	152,683	726,982	134,386	31,908	526,355
Clusters	3,136	11,586	3,192	2,864	9,232
R-squared	0.1196	0.5499	0.6557	0.1782	0.0334

Panel B: Truckers Dep var:	(1) Miles	(2) Miles	(3) Miles	(4) Miles	(5) Miles	(6) Miles
Referral		0.010		0.012		0.005
		(0.049)		(0.049)		(0.049)
Friends made during training	-0.001	0.001				
	(0.006)	(0.003)				
Log(1+Friends made during training)			-0.052	-0.016		
			(0.034)	(0.027)		
Trainees known before training					-0.018	-0.095**
					(0.056)	(0.042)
Controls	No	Yes	No	Yes	No	Yes
Observations	29,880	29,880	29,880	29,880	29,880	29,880
R-squared	0.016	0.120	0.017	0.120	0.016	0.121

Panel C: High-tech Workers Dep var:	(1) Subj Perf	(2) Subj Perf	(3) Hours worked	(4) Code rev	(5) Bug actions	(6) Builds	(7) P4calls	(8) Wiki edits	(9) Views
Referral	0.036***	0.030**	-0.084***	0.001	0.022	-0.002	-0.001	0.061***	0.034**
	(0.012)	(0.012)	(0.014)	(0.012)	(0.015)	(0.009)	(0.009)	(0.016)	(0.014)
Friends at company (self listed)	0.005***		-0.000	0.007***	0.007***	0.002**	0.002	0.003	0.002
	(0.002)		(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Friends at company (colleague listed)		0.037***							
		(0.003)							
Demog Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	104,255	104,255	250,980	289,689	289,689	289,689	289,689	289,689	289,689
Clusters	16,546	16,546	11,066	11,123	11,123	11,123	11,123	11,123	11,123
R-squared	0.081	0.086	0.037	0.121	0.031	0.115	0.006	0.022	0.050

Notes: This table examines whether the number of friends a worker has predicts non-rare productivity. All specifications are OLS regressions with standard errors clustered by worker in parentheses. Each productivity measure has been normalized so that the coefficients are all in standard deviation units. The non-rare productivity metrics are described further in Appendix B. * significant at 10%; ** significant at 5%; *** significant at 1%

In Panel A, productivity is measured using one of 5 different normalized measures. The workers are from 7 call-center firms, spread across many different locations. Each column is an OLS regression including day of tenure dummies, location dummies, client dummies, and the number of times that each outcome was measured to compute the dependent variable. An observation is a worker-day.

In Panel B, productivity is measured in terms of miles and then normalized. All drivers in the sample were hired at one of the training schools in late 2005 or 2006. The specifications include tenure fixed effects (by week). Controls include age, gender, race, education, work type, self-reported income if the driver had not gone through training, and education level. We normalize restricting to the sample with mile weeks greater than 0.

In Panel C, productivity is measured in terms of a subjective performance rating or objective performance measure (all normalized). Demographic controls are interview scores, race dummies, gender dummies, whether the worker is an engineer, and education dummies.

Table 10: Separating Treatment from Selection, Section 5.3: The Impact of Number of Friends on Trucking Accidents

	(1)	(2)	(3)	(4)
Referral	-0.00292	-0.00310	-0.00297	-0.00299
	(0.00240)	(0.00241)	(0.00242)	(0.00241)
Friends made during training		0.00032		
		(0.00021)		
Log(1+Friends made during training)			0.00193	
			(0.00139)	
Trainees known before training				-0.00168
				(0.00208)
Mean dep var	0.0247	0.0247	0.0247	0.0247
Observations	42,309	42,309	42,309	42,309
R-squared	0.00927	0.00936	0.00934	0.00929

Notes: This table examines whether the number of friends an employee has predicts employee trucking accidents. All specifications are linear probability models with demographic, education, and work type controls. An observation is a driver-week. Standard errors clustered at the driver level in parentheses. All drivers in the sample were hired at one of the training schools in late 2005 or 2006, as this is the sample of truckdrivers for which we have friendship data. The sample is thus smaller than that in Table 4, so the estimates here are less precise. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Separating Treatment from Selection, Section 5.3: The Impact of Number of Friends on Innovation

Panel A: Patents								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep var:	Num patents	Num patents	Num patents	Num patents	Ever patent	Ever patent	Citation-weighted patents	Citation-weighted patents
Referral	0.0151	0.0119	0.2356***	0.2266***	0.0078**	0.0066*	0.1897*	0.1877*
	(0.0097)	(0.0100)	(0.0849)	(0.0836)	(0.0035)	(0.0035)	(0.0980)	(0.0966)
Friends	0.0108***	0.0108***	0.0138	0.0156*	0.0017	0.0017	0.0171	0.0140
	(0.0037)	(0.0037)	(0.0091)	(0.0092)	(0.0011)	(0.0011)	(0.0110)	(0.0110)
Method	OLS	OLS	Negative binomial	Negative binomial	OLS	OLS	Negative binomial	Negative binomial
Demog Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	17,168	17,168	17,168	17,168	17,168	17,168	17,168	17,168
R-squared	0.1405	0.1465			0.2048	0.2098		
Mean dep var	0.108	0.108	0.108	0.108	0.0566	0.0566	0.166	0.166

Panel B: Ideas on Idea Board				
	(1)	(2)	(3)	(4)
	Num ideas	Num ideas	Log(1+Ideas)	Log(1+Ideas)
Referral	0.0711	0.0801	0.0269	0.0261
	(0.0494)	(0.0491)	(0.0153)	(0.0153)
Friends	0.0239	0.0245	0.0086	0.0082
	(0.0146)	(0.0146)	(0.0046)	(0.0046)
Method	Negative binomial	Negative binomial	OLS	OLS
Demog Controls	No	Yes	No	Yes
Observations	11,123	11,123	11,123	11,123
R^2			0.069	0.075

Notes: This table examines whether the number of friends a worker has predicts innovation, both in terms of patents and ideas on the firm's internal idea board. Robust standard errors in parentheses. An observation is a worker. Demographic controls are interview scores, race dummies, gender dummies, and education dummies. All regressions also include month of hire dummies, job category dummies, and job rank dummies. For Panel A, "Friends" is measured using self-listed friends, whereas in Panel B, "Friends" is measured using colleague-listed friends. Results are qualitatively similar with either definition. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 12: Separating Selection and Treatment, Section 5.4: Do Referred Applicants Perform Better in Pre-Job Assessments (Job Tests or Interviews)?

Panel A: Call-center Workers	(1) Job Test Score (normalized)
Referral	0.0493***
	(0.0042)
Observations	273,918
R-squared	0.0214

Panel B: High-tech Workers	(1) Interview score (normalized)	(2) Interview score (normalized)	(3) Interview score (normalized)	(4) Interview score (normalized)
Referral	0.214***	0.163***	0.156***	0.168***
	(0.004)	(0.004)	(0.006)	(0.005)
Observations	641,043	641,043	274,995	366,048
Clusters	239,110	239,110	57,883	181,227
Controls	No	Yes	Yes	Yes
Sample	Full	Full	Eng Only	Non-Eng
R-squared	0.009	0.156	0.067	0.210

Notes: This table compares referred and non-referred workers in terms of scores on job tests and in terms of average interview scores. OLS regressions with standard errors clustered by applicant in parentheses. The dependent variable is either standardized job test score or standardized interview score, and the coefficients can be read in terms of standard deviations. * significant at 10%; ** significant at 5%; *** significant at 1%

In Panel A, an observation is a worker. The controls included are cohort (month of hire) dummies and dummies for the type of job applied to.

In Panel B, an observation is an applicant-interview. Getting hired at the firm generally requires multiple interviews, so there are multiple interviews per applicant. Controls include cohort (year of hire) dummies, year dummies, and driver training contracts. Demographic controls include job type, interview type, and quarter of hire.

Table 13: Separating Treatment from Selection, Section 5.4: Using Surveys to Test Whether Referred Workers are Better Matched on Specific Job Dimensions

	(1) Feel bothered when receive unexpectedly low paycheck (normalized)	(2) Demands of the job interfered with family life (normalized)	(3) Acceptable number of times at home per month (normalized)
Referral	-0.457**	-0.299*	0.310*
	(0.197)	(0.176)	(0.173)
Observations	223	215	226
R-squared	0.07	0.05	0.04

Notes: This table examines whether a worker's referral status predicts survey measures of job match, and in turn, how much of the increased retention from being referred can be explained by these variables. The model are OLS regressions. Robust standard errors in parentheses. All drivers are from the same training school and were hired in late 2005 or 2006. The survey questions are asked on a 5-point scale from Strongly Disagree (-2) to Strongly Agree (+2), and then normalized in-sample. Controls for age, gender, race, and marital status in all regressions. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 14: Truck Driver Referrals: Homophily in Characteristics

	(1) Smoker	(2) Black	(3) Hispanic	(4) Female	(5) Married	(6) Age
Referring Drv is Smoker	0.20***	-0.05	0.02	0.10**	0.07	0.43
	(0.07)	(0.04)	(0.03)	(0.04)	(0.07)	(1.36)
Referring Drv is Black	-0.14*	0.43***	-0.02	0.03	-0.24***	-1.40
	(0.07)	(0.07)	(0.03)	(0.05)	(0.08)	(1.67)
Referring Drv is Hispanic	-0.11	-0.15**	0.46***	-0.01	-0.01	-0.58
	(0.11)	(0.06)	(0.12)	(0.09)	(0.13)	(2.79)
Referring Drv is Female	0.12	0.01	-0.02	0.12	-0.11	4.51**
	(0.11)	(0.08)	(0.05)	(0.08)	(0.11)	(2.17)
Referring Drv is Married	-0.06	-0.04	-0.03	0.04	0.11*	0.98
	(0.05)	(0.04)	(0.02)	(0.04)	(0.06)	(1.27)
Age of Referring Drv	-5.4e-4	-3.0e-3*	-7.3e-4	6.6e-4	6.2e-4	0.27***
	(2.0e-3)	(1.5e-3)	(8.6e-4)	(1.5e-3)	(2.5e-3)	(0.08)
Observations	972	972	972	972	972	965
R-squared	0.56	0.65	0.68	0.60	0.50	0.54
Mean dep var	0.289	0.157	0.0565	0.124	0.430	40.32

Notes: This table presents OLS regressions of referred worker characteristics on the characteristics of referring drivers. An observation is a referred worker and the sample is restricted to matched referred workers hired in 2007-2009. Robust standard errors in parentheses. All specifications include 3-digit zip code dummies. The top panel analyzes all drivers whereas the lower panel eliminates workers who work in team pairs. Within a 3-digit zip code, drivers are more likely to refer people with similar characteristics to themselves. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 15: Truck Driver Referrals: Homophily in Behaviors

Panel A: Productivity	(1)	(2)	(3)	(4)
Sample	All	Miles>0	All	Miles>0
Avg miles per week (productivity)	0.290***	0.302***	0.279***	0.292***
of referring driver	(0.032)	(0.031)	(0.031)	(0.030)
Time-Location Controls	Month, Op Center	Month, Op Center	Month-Op Center	Month-Op Center
Observations	40,817	36,543	40,817	36,543
R-squared	0.237	0.170	0.293	0.217
Mean dep var	1684	1881	1684	1881

Panel B: Accidents	(1)	(2)
Referring driver ever had an accident	0.0030*	0.0029
	(0.0018)	(0.0018)
Mean dep var	0.0202	0.0202
Coef(ref driv accident) / mean dep var:	14.9%	14.4%
Tenure controls	Cubic	Week of tenure dummies
Observations	41,530	41,530
R-squared	0.0067	0.0116

Notes: This table presents OLS regressions of referred worker behavior on the behavior of referring drivers. An observation is a referred worker-week and the sample is restricted to matched referred workers hired in 2007-2009. Standard errors clustered by worker in parentheses. All specifications include demographic controls, cohort (year of hire) dummies, and work type controls for the *referred* driver. They also include demographic controls, work type controls, and length of tenure for the *referring* driver. * significant at 10%; ** significant at 5%; *** significant at 1%

In Panel A, the dependent variable is worker productivity in miles per week. As indicated in the table, columns 1 and 2 include month-by-month controls and operating center controls. Columns 3 and 4 include month by operating center controls. Week of tenure dummies for the referred worker are included in all columns.

In Panel B, the dependent variable is whether the worker had an accident in a given week. We show results with a cubic in tenure in column 1 and with week of tenure dummies in column 2. Both columns include month-by-month controls and operating center controls.

Table 16: Profits from Non-Referred and Referred Workers, and by Type of Referral

Worker Type	Profits per Worker
Referred (overall)	\$2,201
Non-referred (overall)	\$1,756
Referred (matched sample)	\$2,503
Referred, referring worker w/ above median productivity	\$4,190
Referred, referring worker w/ below median productivity	\$1,063
Referred, referring worker does not quit in data	\$2,756
Referred, referring worker eventually quits in data	\$1,798

Notes: The calculation here is an accounting exercise based on the data from the trucking firm. Profits are calculated assuming a Fixed Cost of \$475 per week, a price of \$1.90 per mile, a non-wage marginal cost of \$1.20 per mile, and a marginal cost of training of \$2,500, and a collection rate of 30% on new workers with training contracts. A weekly discount factor of 0.9957 is assumed, corresponding to an annual discount factor of 0.8.

Web Appendix: For Online Publication Only

A Proof of Proposition 1

Proposition 1. *Referred workers will have higher productivity than non-referred workers. Suppose that the effort cost reduction from being referred, k_r, is not excessively large. Then, observed productivity differences will reflect both selection and treatment effects. That is, referred workers will have higher average match quality and will exert more effort.*

Proof. The treatment effect from referrals is obvious. A selection effect from referrals means that $E(m|\widehat{m}_R > m_R^*) - E(m|\widehat{m}_E > m_E^*) > 0$. Before computing this difference, we start by examining $E(m|\widehat{m} > m^*)$ in general; the expression we derive can be applied for both referred and non-referred workers.

$$\begin{aligned} E(m|\widehat{m} > m^*) &= \int E(m|\widehat{m}, \widehat{m} > m^*) f(\widehat{m}|\widehat{m} > m^*) d\widehat{m} \\ &= \int \left[\frac{h_0}{h_0+h}\mu_0 + \frac{h}{h_0+h}\widehat{m}\right] f(\widehat{m}|\widehat{m} > m^*) d\widehat{m} \\ &= \frac{h_0}{h_0+h}\mu_0 + \frac{h}{h_0+h} E(\widehat{m}|\widehat{m} > m^*) \end{aligned}$$

To solve for $E(\widehat{m}|\widehat{m} > m^*)$, we use the fact that $\widehat{m}$ has a normal distribution: $\widehat{m} \sim N\left(\mu_0, \frac{1}{h_0} + \frac{1}{h}\right) = N\left(\mu_0, \frac{h_0+h}{h_0 h}\right)$.

$$\begin{aligned} E(\widehat{m}|\widehat{m} > m^*) &= E\left(\mu_0 + \sqrt{\frac{h_0+h}{h_0 h}} Z | \mu_0 + \sqrt{\frac{h_0+h}{h_0 h}} Z > m^*\right) \\ &= \mu_0 + \sqrt{\frac{h_0+h}{h_0 h}} E\left(Z | Z > \frac{m^* - \mu_0}{\sqrt{\frac{h_0+h}{h_0 h}}}\right) \\ &= \mu_0 + \sqrt{\frac{h_0+h}{h_0 h}} H\left(\frac{m^* - \mu_0}{\sqrt{\frac{h_0+h}{h_0 h}}}\right) \end{aligned}$$

where $H(x) \equiv \phi(x) / (1 - \Phi(x))$ is the hazard function of a standard normal and Z is a standard normal random variable. Plugging in, this yields the following expression:

$$\begin{aligned} E(\widehat{m}|\widehat{m} > m^*) &= \frac{h_0}{h_0+h}\mu_0 + \frac{h}{h_0+h}\left[\mu_0 + \sqrt{\frac{h_0+h}{h_0 h}} H\left(\frac{m^* - \mu_0}{\sqrt{\frac{h_0+h}{h_0 h}}}\right)\right] \\ &= \mu_0 + \frac{h}{h_0+h}\sqrt{\frac{h_0+h}{h_0 h}} H\left(\frac{m^* - \mu_0}{\sqrt{\frac{h_0+h}{h_0 h}}}\right) \\ &= \mu_0 + \sqrt{\frac{h}{h_0(h_0+h)}} H\left(\frac{m^* - \mu_0}{\sqrt{\frac{h_0+h}{h_0 h}}}\right) \qquad (11) \end{aligned}$$

Before proving the general result, it is worthwhile to start with the case of no treatment effects. We would like to prove that referred workers will be unambiguously positive selected for any $h_R > h_E$.

Lemma 2 *Suppose that there is no effort in the model, i.e. $\alpha = 0$. Then referred workers will perform better than non-referred workers, with referred workers positively selected.*

Proof. Our goal is to show that $\frac{dE(m|\widehat{m}>m^*)}{dh} > 0$. We plug into (11) for m^*, using $h_0\mu_0 + hm^* = 0$, or $m^* = -\frac{h_0}{h}\mu_0$:

$$\begin{aligned}
\mu_0 + \sqrt{\frac{h}{h_0\left(h_0+h\right)}}H\left(\frac{m^*-\mu_0}{\sqrt{\frac{h_0+h}{h_0 h}}}\right) &= \mu_0 + \sqrt{\frac{h}{h_0\left(h_0+h\right)}}H\left(\frac{-\frac{h_0}{h}\mu_0-\mu_0}{\sqrt{\frac{h_0+h}{h_0 h}}}\right) \\
&= \mu_0 + \sqrt{\frac{h}{h_0\left(h_0+h\right)}}H\left(\frac{-\mu_0\left(1+\frac{h_0}{h}\right)}{\sqrt{\frac{h_0+h}{h_0 h}}}\right) \\
&= \mu_0 + \sqrt{\frac{h}{h_0\left(h_0+h\right)}}H\left(-\mu_0\sqrt{\frac{h_0\left(h_0+h\right)}{h}}\right) \\
&= \mu_0 + \theta H\left(-\frac{\mu_0}{\theta}\right)
\end{aligned}$$

where we define $\theta \equiv \sqrt{\frac{h}{h_0(h_0+h)}}$. Because θ is a monotonically increase function of h, it suffices to show that $\frac{dE(m|\widehat{m}>m^*)}{d\theta} > 0$. Taking the derivative, we have:

$$\begin{aligned}
\frac{dE\left(m|\widehat{m}>m^*\right)}{d\theta} &= \frac{d}{d\theta}\left[\theta H\left(-\frac{\mu_0}{\theta}\right)\right] \\
&= H\left(-\frac{\mu_0}{\theta}\right) + \frac{\mu_0}{\theta}H'\left(-\frac{\mu_0}{\theta}\right) \\
&= H\left(x\right) - xH'\left(x\right) \\
&= H(x)\left(x^2 - xH\left(x\right)+1\right)
\end{aligned}$$

where we define $x \equiv -\frac{\mu_0}{\theta}$. We note that $sgn\left(H(x)(x^2 - xH\left(x\right)+1)\right) = sgn\left(x^2 - xH\left(x\right)+1\right)$, and that $x^2 - xH\left(x\right) + 1 > 0$ for all x. ■

Once the model has an effort component ($\alpha > 0$), firms will account for the treatment effect from referrals ($k_r > 0$) in setting the referred applicant hiring standard. For example, if k_r is very large, then the firm may be willing to hire almost all referred applicants, despite being selective with respect to non-referred applicants, thereby making it so referred workers are not positively selected. Given a particular k_r, referred workers will still be positively selected if the precision of referred applicant signals is sufficiently high relative to the precision of non-referred applicant signals.

Returning to the main proof, we now allow for treatment effects, $\alpha > 0$ and $k_r > 0$. We use the expectation in (11) to compute the difference between referred and non-referred workers in average match quality. We define $\theta_R \equiv \sqrt{\frac{h_R}{h_0(h_0+h_R)}}$, $\theta_E \equiv \sqrt{\frac{h_E}{h_0(h_0+h_E)}}$, $F_R \equiv F(k_r + \beta\alpha)$, and $F_E \equiv F(\beta\alpha)$.

$$
\begin{aligned}
& E\left(m \mid \widehat{m}_R > m_R^*\right) - E\left(m \mid \widehat{m}_E > m_E^*\right) \\
= \;& \theta_R H\left(\frac{m_R^* - \mu_0}{\sqrt{\frac{h_0 + h_R}{h_0 h_R}}}\right) - \theta_E H\left(\frac{m_E^* - \mu_0}{\sqrt{\frac{h_0 + h_E}{h_0 h_E}}}\right) \\
= \;& \theta_R H\left(\left(m_R^* - \mu_0\right)\sqrt{\frac{h_0 h_R}{h_0 + h_R}}\right) - \theta_E H\left(\left(m_E^* - \mu_0\right)\sqrt{\frac{h_0 h_E}{h_0 + h_E}}\right) \\
= \;& \theta_R H\left(-\left(\frac{h_0 + h_R}{h_R}\right)\left(\mu_0 + \alpha F_R\right)\sqrt{\frac{h_0 h_R}{h_0 + h_R}}\right) - \theta_E H\left(-\left(\frac{h_0 + h_E}{h_E}\right)\left(\mu_0 + \alpha F_E\right)\sqrt{\frac{h_0 h_E}{h_0 + h_E}}\right) \\
= \;& \theta_R H\left(-\frac{\left(\mu_0 + \alpha F_R\right)}{\theta_R}\right) - \theta_E H\left(-\frac{\left(\mu_0 + \alpha F_E\right)}{\theta_E}\right)
\end{aligned}
$$

For a given θ_R and θ_E (that is, for a given h_R and h_E), for any α, we can choose k_r small enough so that the terms αF_R and αF_E are sufficiently small, and the total difference is positive.

To see that there is a positive productivity difference between referred and non-referred even without assuming that k_r is not excessively large, note first that if $\alpha > 0$ and $k_r = 0$, that referred workers will have higher average match quality. Denote the hiring standards in this case by $(m_{R,0}^*, m_{E,0}^*)$. If k_r is increased, $m_{R,0}^*$ is still a feasible hiring standard for referred workers, and the optimum hiring standard must yield as least as high average productivity. $\blacksquare$

B Data Appendix

B.1 Reliability of Referral Measurement

Are our measures of referral status reliable? As indicated above, referral status is measured at the call-center firm by asking applicants, at the trucking firm by asking applicants and through the firm's administrative referral program, and at the high-tech company via the firm's administrative referral program. For the trucking data, self-reported referrals exceed administratively reported referrals by 24%. Of workers listed as referred in the firm's administrative program, nearly all report being referred, whereas of workers who are not listed in the firm's administrative program, some do report being referred. This difference reflects three factors: (1) Lying by applicants about their referral status, (2) Referring workers informing referred workers about the job, but who do not submit the worker's name so that they would become eligible to receive a referral bonus, and (3) Imperfect matching across datasets. Our results from the trucking firm are similar whether we use the self-reported referral definition or whether we use the administrative definition, suggesting that differences in referral status definition are unlikely to account for differences in regression results across industries.

B.2 Call-center Firms

Evolv on Demand, the job testing firm that tracks the data from the call-center firms, keeps common demographic variables (i.e., race, gender, and age) separate from retention and productivity data. This is to ensure the non-job relevant demographics data do not influence hiring and personnel management decisions. Thus, our data do not allow us to control for gender, race, and age in retention and productivity regressions. However, controlling for these variables has little impact on estimates for both trucking and high-tech workers, suggesting that omitting them would have relatively little impact on estimates for call-center workers. We do control for race, gender, and age in characteristics comparisons in Table 1.

Schooling. Years of schooling, given in one of several educational categories. We use 12 years for a high-school graduate; 14 years of schooling for an Associate Degree or Technical Diploma; 16 years for a Bachelor's Degree; 18 years for a Master's Degree; and 20 years for a Doctorate.

Intelligence. We measure intelligence using questions from applicant job tests at the seven call-center firms. The questions were developed by industrial/organizational psychologists at the job testing firm, Evolv on Demand, following appropriate validation processes. Because the job tests are proprietary and active, we cannot publish here the exact wording of the intelligence questions.

Personality. As for intelligence, we measure the Big 5 personality characteristics using questions from applicant job tests at the seven call-center firms. The questions were developed by industrial/organizational psychologists at the job testing firm, Evolv on Demand, following appropriate validation processes. The industrial/organizational psychologists also designed the mapping from job test questions into the Big 5 characteristics. As for intelligence, because the job tests are proprietary and active, we cannot publish the exact wording of the personality questions.

Big 5 Index. An equally-weighted average of the z-scores from the Big 5 personality characteristics, reversing neuroticism, as in Dal Bo et al. (2013).

Adherence share. Share of the time that a worker is at his or her seat working of the total amount of time they are schedule to work. A higher number indicates higher productivity. We use the normalized version for regressions (normalized according to our data).

Average handle time. Average number of seconds spent by a worker on a call. A lower number indicates higher productivity. We use the normalized version for regressions (normalized according to our data).

Sales conversion rate. Share of the time that a sales call results in a sale. This number is only available for call-center workers engaged in sales. A higher number indicates higher productivity. We use the normalized version for regressions (normalized according to our data).

Quality assurance. Managerial rating of whether a worker was providing quality service, measured on a 0-1 scale. A higher number indicates higher productivity. We use the normalized version for regressions (normalized according to our data).

Customer satisfaction. Rating from post-call customer surveys of whether customers were satisfied with the service provided, measured on a 0-1 scale. A higher number indicates higher productivity. We use the normalized version for regressions (normalized according to our data).

Friends at company, People know at company. Applicants were asked the questions "How many friends do you have that work at this company?" and "How many people do you know that work at this company?" The response options for both questions were "0," "1-2," "3-4," or "5 or more." We convert "1-2" to 1.5, "3-4" to 3.5, and "5 or more" to 5 for both questions.

Job test score. An applicant's overall score on the job test. We normalize the score in-sample.

B.3 Trucking Firm

For 900 of the workers, there is extensive information about worker background, cognitive ability, non-cognitive ability, and experimental preferences. These workers were hired at one of the trucking firm's training schools in late 2005 and 2006. Most of the survey data was collected during the workers' commercial driver's license training.

Information on who referred a given worker is only available for October 1, 2007 through December 31, 2009.

Schooling. Years of schooling, given in one of several educational categories. We use 12 years for a high-school graduate; 14 years of schooling for an Associate Degree or Technical Diploma; 16 years for a Bachelor's Degree; 18 years for a Master's Degree; and 20 years for a Doctorate.

IQ. IQ is measured using Score on Ravens Progressive Matrices test. The test consists of 5 sections with 12 questions each, producing a score out of 60. We normalize the test scores in sample.

Big 5 Personality. We measure the Big 5 personality characteristics with the same measures as in Rustichini et al. (2012) using the Multidimensional Personality Questionnaire (MPQ), which consists of 154 multiple-choice questions. For all the traits, we normalize the scores in sample. Rustichini et al. (2012) note that it is difficult to use the MPQ to create a measure of Openness that is separate from cognitive ability or intelligence. Thus, following Rustichini et al. (2012), we do not define a separate measure of Openness, and focus only on 4 of the Big 5 characteristics.

Big 5 Index. An equally-weighted average of the z-scores from the 4 of the Big 5 personality characteristics (excluding Openness), reversing neuroticism, similar to as in Dal Bo et al. (2013).

CRRA risk aversion. (CRRA) risk aversion is measured using a task similar to that in (Holt and Laury, 2002). CRRA uses choices between (A) Getting 2, 3, 4, 5, 6, or 7 dollars for sure vs. (B) A lottery with a 50% chance of getting 10 dollars and a 50% chance of getting 2 dollars.

Patient options chosen. Subjects completed a time preference experiment where they chose between getting $80 at a later date or receiving between $45 and $75 today, making a total of 28 choices. The later date varied was 1 day, several days, 1 week, or 4 weeks. Patient options chosen is the share of the 28 choices where the worker chose the patient option.

Beta in HD model. The time preference experiment can be used to estimate a model of hyperbolic discounting (HD) with beta-delta preferences (Laibson, 1997). A time period corresponds to 1 day, so β is the amount of present bias between today tomorrow.

Delta in HD model. The δ implied by the present-bias experiment (Laibson, 1997).

Trust. Subjects played a Sequential Prisoner's Dilemma. In the game, Player 1 could send $0 or $5 to Player 2. Player 2 can respond by sending $0, $1, $2, $3, $4, or $5 back. Any funds sent by Player 1 or 2 are doubled by the researcher. Subjects played both roles using the Strategy Method. The variable trust is the average number of dollars sent by Player 1.

Altruism V1. The number of dollars Player 2 would send back in the Sequential Prisoner's Dilemma if Player 1 sent him or her $0.

Altruism V2. The number of dollars Player 2 would send back in the Sequential Prisoner's Dilemma if Player 1 sent him or her $5.

Miles. The number of miles driven by a driver each week. When asked the reason for significant cross-driver differences in average miles per week, managers at the trucking firm emphasized several factors, including speed, skill at avoiding traffic, route planning, skill at not getting lost, and coordinating with people to unload the truck. For example, drivers who arrive late to a location may have to wait long periods of time for their truck to be unloaded, which can be highly detrimental to weekly miles. Trucking loads are assigned primarily by proximity and are not assigned based on driver ability. See Hubbard (2003) and Hoffman and Burks (2012) for more on measuring productivity in trucking.

Accident. Whether the driver has a trucking accident in any given week. The company's definition of an accident is quite broad and includes serious as well as relatively minor accidents. Our use of a weekly dummy variable for having an accident is sensible, as we observe very few weeks where drivers have multiple accidents.

Preventable accident. An accident that a driver had control over and thus is at least partially at fault. Determined to be preventable (instead of non-preventable) by analysts at the firm's insurance subsidiary, based on guidelines from the Federal Motor Carrier Safety Administration.

Non-preventable accident. An accident that the driver could not control, based on guidelines from the Federal Motor Carrier Safety Administration.

Annual income if continued at past jobs. Drivers were asked: "Which range best describes the annual earnings you would normally have expected from your usual jobs (regular and part-time together), if you had not started driver training with [Firm A], and your usual jobs had continued without interruption?" The answers were $10,000 increments from $0-$10,000, $10,000 to $20,000, and so on, up to more than $70,000.

B.4 High-tech Firm

There are about 25,000 workers in the high-tech dataset. However, most data is missing for about 8,000 of these workers, leaving about 17,000 workers for most of the analysis.

Schooling. Years of schooling, given in one of several educational categories. We use 12 years for a high-school graduate; 14 years of schooling for an Associate Degree or Technical Diploma; 16 years for a Bachelor's Degree; 18 years for a Master's Degree; and 20 years for a Doctorate.

SAT Total. Combined score from Math and Writing sections of the SAT. Each is out of 800 points, so the total score is out of 1600 points. We focus on the Math and Writing section scores because data on Verbal section scores are missing for almost all respondents. The SAT data are obtained from a 2006 survey of existing employees administered by the firm.

Big 5 Personality. The Big 5 Personality characteristics were measured using the Big Five Inventory Test (John et al., 1991). The data are obtained from a 2006 survey of existing employees administered by the firm. For all the 5 traits, we normalize the scores in sample.

Subjective performance. An employee's quarterly performance review score, provided by the employee's manager. It is given on a scale from 0-5. We normalize the scores in sample.

Hours worked. The data include timestamps of the most frequent activities that employees engage in, which we use to construct the average number of hours worked per day over the course of a month. Employees log an average of around 5-7 hours per day. Since employees work longer hours than this, our hours measure only captures some of the activities that employees engage in. We normalize the number of hours in-sample.

The following measures correspond to objective tasks performed at work, such as writing and reviewing computer code. The activity measures are best thought of as capturing a combination of effort and output, as opposed to output alone. For all the following measures, we winsorize high outliers, limiting activity measures for an employee*months to the 99th percentile of the distribution of all employee months with a positive amount of activity,

Code reviews. Before new code becomes part of the firm's canonical code base, it must be reviewed by one or more peers. These reviews are fairly regular; the average engineer participates in just over one per workday. We count the number of code reviews an engineer participates in as an author and the number as a reviewer. The former is a proxy for the amount of new code written, while the latter is a measure of ones helpfulness as a reviewer and the extent to which one has been assigned responsibility for maintaining an important part of the firm's codebase.

Bugs database actions (bug actions). Bugs are tracked by a database. Employees make entries as they identify, diagnose, and fix bugs. Software engineers are most likely to be involved in diagnosing and fixing bugs, while non-engineers often identify them.

Builds. Software is "built" (essentially a compilation) primarily for testing purposes, and also to use the software in an internal or external environment. While performing a build is clearly work-related activity, a software engineer who performs a lot of builds for every unit of code they complete may be working less efficiently than one who performs fewer builds.

Perforce calls (P4calls) . Engineers make calls to the Perforce system, a third-party software program that maintains the firms codebase and facilitates engineers interaction, for a variety of purposes, including when they check out code for editing or viewing, when they submit code for review. As with builds, an efficient engineer may accomplish a given task using fewer P4 calls than a less efficient engineer.

Wiki page edits and views. The firm's code is documented in an extensive internal wiki. When significant changes are made to code, the engineer responsible often updates the wiki. Providing documentation of changes is viewed as good citizenship.

Number of Friends. Based on a 2006 survey of existing employees conducted by the firm. Employees listed other employees they said were their friends. From this, we construct both self-reported friends and other-reported friends (based on whether others report the employee as a friend). Note that only for the high-tech firm do we have data on who is friends with whom (for the call-center and trucking firms, we only have data on the number of each employee's friends).

Patents. Number of patent applications associated with an employee. Employees who create an invention file an Invention Disclosure Form. Attorneys from the firm then decide whether to file a patent application. Most of these patent applications are later approved as patents, but the process usually takes several years. Throughout the paper, we measure "patents" using patents applied for. Patents with multiple employee co-authors from the high-tech firm are counted toward each employee.

Citation-weighted patents. Following Trajtenberg (1990), we construct citation-weighted patents as one plus the number of citations for each patent. To limit the influence of outliers, we trim employees with citation counts above the 90th percentile in-sample.

B.5 Matching High-tech Inventors to Patent Citations

Our goal was to match inventors at the high-tech firm to patent citations of their patents. Because the data from the high-tech firm do not contain patent numbers, we matched employee names from the company database to the U.S. Patent Inventor database (Lai et al., 2010), which lists the first and last name of each inventor on each patent (and occasionally middle name or initial). For each inventor, we attempted a variety of matches with each name in the employee names – starting with the most difficult match (full name), and proceeding until the easiest match (last name only), in the following order:

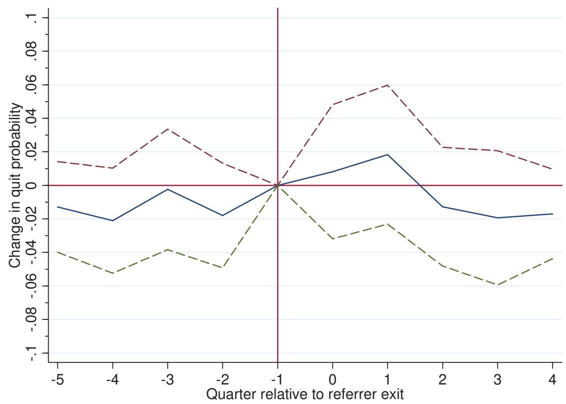
1. The inventor's full name
2. The inventor's first name and last name (middle name or initial excluded).

3. The inventor's first name, middle initial and last name.
4. The inventor's first name and last name.
5. The inventor's first initial and last name.
6. The inventor's last name.

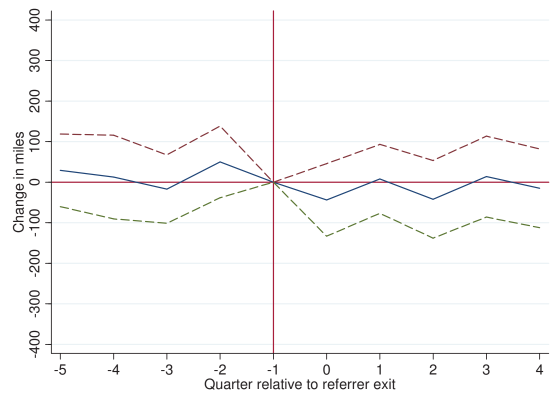
If any of these attempts returned exactly one unique employee, we linked the patent inventor to the unique employee and discontinued further matches attempts for that employee. If an inventor was matched to zero or multiple employees, we discarded the inventor and invention (thereby ensuring that each inventor is linked to either zero or one employee record). This matching technique connects 90% of the firm's inventors listed in the U.S. Patent Inventor database. Many of the remaining ten percent are non-employees who had collaborated with the firm on technological projects (usually from academia).

C Additional Figures and Tables

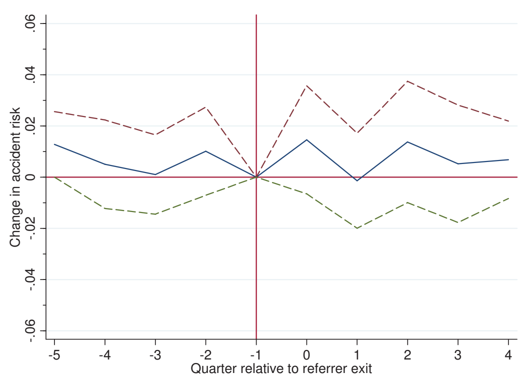
Figure C1: Separating Selection and Treatment, Section 5.1: Event Studies on Referring Worker Exit (with 95% Confidence Intervals)



(a) Quits



(b) Miles



(c) Accidents

Notes: These graphs present event studies on whether referred workers are affected by the exit of referring workers. The solid line denotes the coefficient estimate, with the dotted lines denoting the 95% confidence interval. Controls include cohort (year of hire) dummies, week of tenure dummies, and time fixed effects (at the monthly level). Individual fixed effects are included in the miles regression and the accidents regression. Standard errors clustered by worker.

Table C1: Summary of Data Elements

	Call-centers	Trucking	High-tech
Referral status	W,A	W,A	W,A
Productivity	W	W	W
Social network	W	W	W
Job test or interview scores	W,A		W,A
Demographics	A	W,A	W,A
Cognitive ability	W,A	W	W
Personality	W,A	W	W
Experimental games		W	
Who referred whom		W	

Notes: This table summarizes the data elements from the three industries. "W" means a data element is available for workers. "A" means a data element is available for applicants.

Table C2: Comparing Referred and Non-Referred Workers on Demographics

Panel A: Call-center Applicants	No Referral	Referral		Obs
Female	0.66	0.58	*	59,356
Black	0.32	0.31	*	58,688
Hispanic	0.26	0.27	*	58,688
More than high school education	0.23	0.16	*	49,804
Share in each	0.72	0.28		362,009

Panel B: Trucking Workers	No Referral	Referral		Obs
African-American	0.15	0.14	*	N
Hispanic	0.05	0.05		N
Female	0.07	0.13	*	N
Married	0.37	0.38		N
Age when start	38.8	38.7		$0.80N$
Online application	0.72	0.55	*	N
Smoker	0.34	0.37	*	N
Share in each	0.80	0.20		N
Years of schooling	12.97	12.78		895
High school dropout	0.04	0.03		895
High school graduate	0.36	0.46	*	895
Some college	0.36	0.31		895
Technical school	0.14	0.13		895
College degree or more	0.11	0.07		895
Credit score	588	584		784
Regular jobs in last 2 years	1.56	1.69		894
Annual income if continued at past jobs	31.05	27.90	*	895
Maximum years at a previous job	7.91	7.97		518
Months holding reg jobs in last 2 years	17.73	18.37		895
Parent worked in trucking	0.12	0.15		895
Trainees known before training	0.15	0.19		698
Friends made since training	5.45	6.33		698
Exper (yrs) w/large onroad vehicle	1.12	1.09		895

Panel C: High-tech Workers	No Referral	Referral		Obs
White	0.57	0.66	*	25,282
Asian	0.36	0.30	*	25,282
Non-White/Asian	0.07	0.04	*	25,282
Female	0.33	0.23	*	25,282
Share in each	0.67	0.33		25,282

Notes: This table compares referred and non-referred workers in term of various demographic characteristics. * significant at 5%

In Panel A, demographic variables are missing for many of the call-center applicants.

In Panel B, N is the number of drivers in the full trucker sample. The exact N is withheld to protect firm confidentiality, $N >> 10,000$.

Table C3: Comparing Referred and Non-Referred Workers on Cognitive Ability

Panel A: Call-center Applicants	No Referral	Referral		Obs
Intelligence (std deviations)	0.004	-0.01	*	314,549

Panel B: Trucking Workers	No Referral	Referral	Obs
IQ (std deviations)	-0.00	0.01	850

Panel C: High-tech Workers	No Referral	Referral	Obs
SAT Math Score	724	726	910
SAT Writing Score	671	683	902

Notes: This table compares referred and non-referred workers in cognitive ability. Intelligence for call-center applicants is measured from questions on the job tests. IQ is measured using the Ravens Progressive Matrices test. The SAT scores are out of 800. See Section B for variable definitions. * significant at 5%

Table C4: Comparing Referred and Non-Referred Workers on Non-cognitive Ability (Normalized Measures)

Panel A: Call-Center Applicants	No Referral	Referral		Obs
Conscientiousness	0.04	-0.10	*	357,976
Neuroticism	0.00	-0.01	*	356,401
Agreeableness	0.01	-0.03	*	357,889
Extraversion	-0.03	0.08	*	357,889
Openness	0.01	-0.03	*	357,889

Panel B: Trucking Workers	No Referral	Referral	Obs
Conscientiousness	0.00	-0.04	895
Neuroticism	-0.03	0.03	895
Agreeableness	0.05	-0.07	895
Extraversion	-0.05	0.06	895

Panel C: High-tech Workers	No Referral	Referral		Obs
Conscientiousness	0.00	0.00		1,851
Neuroticism	0.04	-0.05		1,853
Agreeableness	0.06	-0.07	*	1,853
Extraversion	-0.03	0.03		1,852
Openness	0.03	-0.03		1,852

Notes: This table compares referred and non-referred workers in non-cognitive ability, measured using the Big 5 Personality Characteristics. See Section B for variable definitions. * significant at 5%.

Table C5: Comparing Referred and Non-Referred Workers on Experimental Preferences

Panel A: Trucking Workers	No Referral	Referral		Obs
Risk aversion (CRRA), Version 1	0.48	0.46		895
Risk aversion (CRRA), Version 2	0.11	0.07		895
Patient option chosen (share)	0.60	0.58		895
Beta from hyperbolic discounting model, TUnit=Day	0.84	0.83		895
Delta from hyperbolic discounting model, TUnit=Day	0.98	0.97		873
Trust (P1 sending, SPD Game)	3.41	2.90	*	895
Altruism V1 (P2 return if P1 sends $0, SPD Game)	1.52	1.55		895
Altruism V2 (P2 return if P1 sends $5, SPD Game)	3.74	3.46		895

Notes: This table compares referred and non-referred workers across several experimental economic games. The "SPD Game" is a Sequential Prisoner's Dilemma. See Appendix B for an explanation of the different experiments. * significant at 5%

Table C6: Who Makes Referrals? Evidence from the High-tech Firm

	(1) Number of referrals	(2) Ever refer
Referred	0.0798***	0.0421***
	(0.0119)	(0.0058)
Friends (self-reported)	0.0170***	0.0052***
	(0.0053)	(0.0017)
Mean interview score	0.0274***	0.0120***
	(0.0079)	(0.0031)
Asian	-0.0941**	-0.0287
	(0.0443)	(0.0176)
Hispanic	-0.0744	0.0544
	(0.1582)	(0.0651)
Female	0.0661*	0.0270*
	(0.0357)	(0.0140)
Masters degree	0.0143	0.0072
	(0.0169)	(0.0076)
Doctoral degree	0.0952***	0.0434***
	(0.0335)	(0.0126)
Observations	17,168	17,168
R-squared	0.1676	0.1598
Mean dep var	0.264	0.167

Notes: This table presents OLS regressions on the number of referrals made by existing employees at the high-tech firm. Robust standard errors in parentheses. In column (1), we look at the number of referrals made per person, whereas in column (2), we examine whether an employee has ever made an employee referral (0-1). * significant at 10%; ** significant at 5%; *** significant at 1%

Table C7: Referred Applicants are More Likely to be Hired and Less Likely to have Problematic Pasts

Panel A: Call-center	(1)
Referral	0.0706***
	(0.0013)
Observations	363,394
R-squared	0.0071
Mean dep var for ref=0	0.0706

Panel B: Trucking	(1) Hired	(2) Felony	(3) DWI	(4) Work history prob	(5) App false	(6) Not fit for firm
Referral	0.1186***	-0.0038***	-0.0003**	-0.0015***	0.0002	-0.0017**
	(0.0019)	(0.0004)	(0.0002)	(0.0004)	(0.0002)	(0.0007)
Observations	A	A	A	A	A	A
R-squared	0.0088	0.0001	0.0000	0.0000	0.0000	0.0000
Mean dep var for ref=0	0.171	0.0101	0.00144	0.00871	0.000870	0.0206

Panel C: High-tech Dep var:	(1) Made offer	(2) Made offer	(3) Hired	(4) Hired
Referred	0.01753***	0.01660***	0.01373***	0.01301***
	(0.00055)	(0.00055)	(0.00049)	(0.00049)
Controls	No	Yes	No	Yes
Observations	1,175,016	1,175,016	1,175,016	1,175,016
R-squared	0.00349	0.00658	0.00286	0.00528
Mean dep var for ref=0	.0039	.0039	.0028	.0028

Notes: This table presents OLS regressions of whether a worker was hired on referral status. In addition, it also presents regressions of whether a work was disqualified for a particular reason on referral status. Robust standard errors in parentheses. An observation is an applicant to the firm. In Panel B, the exact number of applicants, A, is withheld to protect firm confidentiality, $A >> 100,000$. In Panel C, controls are gender and race dummies. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C8: Do Referred Workers Propose More Ideas by Proposing More Low Quality Ideas? Ruling out the "Junk Ideas" Explanation for Referred/Non-referred Innovation Differences at the High-tech Firm

Dep Var: "Idea Rating" on Firm Idea Board	(1)	(2)	(3)
Referral	0.001	0.001	-0.005
	(0.005)	(0.005)	(0.008)
Observations	254,292	254,292	80,041
R^2	0.276	0.277	0.392
Demog Controls	No	Yes	Yes
Outliers Removed	No	No	Yes
Clusters	20,991	20,991	12,196

Notes: OLS regressions with standard errors clustered by worker in parentheses. All regressions control for the submitter's job category, rank, cohort, location and tenure – as well as the categorization of the idea, the idea's creation date, the person rating the idea and the number of characters used to describe it. The demographic controls refer to the submitter's ethnicity, gender, educational background, and mean interview score. In column 3, we try to remove outlier ideas. Some ideas submitted were more akin to "rants" than new business or product suggestions. These rants typically had a high number of idea raters. Thus, in column 3, ideas with fewer than four raters or greater than 20 raters were moved. Corresponding to the 25th and 75th percentiles of the number of raters, this is a very conservative elimination of rants. Results are also similar cutting at the 10th and 90th percentiles.

Table C9: Alternative Explanation for Quitting Differences: Is the Quitting Difference Between Referred and Non-Referred Workers Due to Differences in the Outside Option?

	(1)	(2)
Referral	-0.242*	-0.224
	(0.137)	(0.137)
Annual income if continued at past jobs		0.008**
		(0.003)
Observations	38,381	38,381

Notes: This table examines whether a worker's referral status predicts their moral hazard behavior at work. All specifications are Cox Proportional Hazard models with demographic controls, education controls, and work type controls. Standard errors clustered at the driver level in parentheses. Demographic controls include gender, race dummies, marital status, and age bin dummies for the different age groups: 25-30, 30-35, 35-40, 40-45, 45-50, 50-55, 55-60, and 60-80. Education controls are dummies for high school graduate, some college, and college. Work type controls are dummies for different work configurations and for receiving any salary or activity-based pay. All drivers are from the same training school and were hired in late 2005 or 2006. * significant at 10%; ** significant at 5%; *** significant at 1%

Table C10: Alternative Explanation for Quitting Differences: Do Referred Workers have Different Beliefs?

	(1)	(2)
Dep Var:	Miles expectation	Miles overprediction
Referral	34.30	0.40
	(58.63)	(52.68)
Avg miles to date	0.55***	
	(0.07)	
Observations	8,713	8,628
R-squared	0.31	0.12
Mean dep var	2323	330.0

Notes: This table examines whether a worker's referral status predicts their moral hazard behavior at work. All specifications are OLS regressions with demographic controls, education controls, and work type controls. Standard errors clustered at the driver level in parentheses. Demographic controls include gender, race dummies, marital status, and age bin dummies for the different age groups: 25-30, 30-35, 35-40, 40-45, 45-50, 50-55, 55-60, and 60-80. Education controls are dummies for high school graduate, some college, and college. Work type controls are dummies for different work configurations and for receiving any salary or activity-based pay. All drivers are from the same training school and were hired in late 2005 or 2006. Referral is a 0-1 variable indicating a worker found the job through a referral (versus other ways). * significant at 10%; ** significant at 5%; *** significant at 1%

Table C11: Do Referred Workers Earn High Salaries? Evidence from High-tech

	(1)	(2)	(3)	(4)
Dep var:	Log(Salary)	Log(Salary)	Log(Salary)	Log(Salary)
Referral	0.0187**	0.0200**	0.0160	0.0164
	(0.0065)	(0.0065)	(0.0087)	(0.0087)
Referral x Tenure			0.0025	0.0034
			(0.0049)	(0.0048)
Observations	245,322	245,322	245,322	245,322
R^2	0.542	0.549	0.542	0.549
Demog Controls	No	Yes	No	Yes
Clusters	10,655	10,655	10,655	10,655

Notes: This table examines whether referred workers earn a higher salary. An observation is an employee-month. Standard errors clustered by employee in parentheses. All regressions include controls for tenure, exchange rate and fixed effects for job type, job rank, cohorts and month-of-year. Demographic controls include mean interview score and fixed effects for ethnicity, gender and educational background.

Table C12: Truck Driver Referrals: Homophily in Miles per Week, Quantile Regressions

	(1)	(2)	(3)
Dep var:	Miles>0	Miles>0	Miles>0
Quantile:	Q10	Q50	Q90
Avg miles per week (productivity)	0.262***	0.324***	0.303***
of referring driver	(0.016)	(0.013)	(0.015)
Observations	36,543	36,543	36,543
Mean dep var	1881	1881	1881

Notes: This table presents quantile regressions of referred worker productivity on the productivity of referring drivers. The regressions are the same as in column 2 of Table 15, but are specified as a quantile regression instead of OLS. An observation is a referred worker-week and the sample is restricted to matched referred workers hired in 2007-2009. Standard errors in parentheses. The dependent variable is non-rare worker productivity in miles per week. All specifications include demographic controls, cohort (year of hire) dummies, week of tenure dummies, month-by-month controls, operating center controls, and work type controls for the *referred* driver. They also include demographic controls, work type controls, and length of tenure for the *referring* driver. * significant at 10%; ** significant at 5%; *** significant at 1%

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