What Do Employee Referral Programs Do?*

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Abstract

In an RCT covering all grocery store jobs at a European grocery chain, 238 stores were randomized to give information to employees or pay them different bonuses for making referrals. Larger bonuses increase referrals and decrease referral quality, though the increase in referrals is fairly modest. Still, the employee referral programs (ERPs) are highly profitable. This reflects in part that referred workers are substantially more likely to stay than non-referrals, but primarily that non-referrals have higher retention in stores treated with ERPs relative to control stores. The firm rolled out an ERP across the entire firm and increased bonuses. In the post-RCT rollout, referral rates remain low for grocery jobs, but are high for non-grocery jobs, which are perceived as much more attractive. Our results (1) are consistent with a model where referrals are driven by both monetary incentives and altruism toward friends and (2) show that ERPs can have substantial benefits beyond the people who are referred, in our case, by making workers feel more respected.

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1 Introduction

Firms must find workers to fill their jobs, and workers must find firms that employ them. One of the most common ways by which workers get hired is via employee referrals.[1] A growing literature shows that referral hires tend to be of higher quality than non-referrals, with lower turnover, lower recruiting costs, and sometimes higher productivity (e.g., Brown et al., 2016; Burks et al., 2015; Castilla, 2005; Fernandez et al., 2000; Heath, 2018; Pallais and Sands, 2016). Thus, it may not come as a surprise that many firms have employee referral programs (ERPs), a management practice where workers are explicitly encouraged to refer their social contacts for jobs, often using bonuses.[2]

Whether an ERP would boost referrals or benefit a firm is theoretically and empirically an open question. On one hand, bonuses could be ineffective if the decision to refer is primarily driven by non-pecuniary motivations. Bonuses could even be counterproductive if they crowd out intrinsic motivations to refer (Deci et al., 1999; Benabou and Tirole, 2003) or lead to low-quality hires. Further, bonuses are not for free and may lead to less diverse hires if workers refer people like themselves. On the other hand, beyond generating referrals, ERPs could have broader organizational benefits. Workers may value being asked to be involved in the hiring process, in line with the view that many HR policies serve more than one purpose and affect whether workers feel respected (Ritter and Taylor, 1994; Ellingsen and Johannesson, 2008; Rebitzer and Taylor, 2011).

We examine what ERPs do using a 13-month randomized control trial (RCT) on over 10,000 workers in a large grocery chain in Eastern Europe. All 238 stores were randomly

[1] 25-40% of European jobs are found via referral (Pellizzari, 2010), as are about half of US jobs (Granovetter, 1974; Topa, 2011). Referrals may matter for many labor issues such as wage inequality (Montgomery, 1991; Calvo-Armengol and Jackson, 2004) and racial gaps in unemployment (Holzer, 1987; Zenou, 2015).

[2] The Society for Human Resource Management defines an ERP as "a recruiting strategy in which employers encourage current employees, through rewards, to refer qualified candidates for jobs in their organizations" (SHRM, 2016). CareerBuilder.com estimated that 69% of firms on its platform had a formal ERP (CareerBuilder, 2012). In the retail module of the World Management Survey (Bloom et al., 2014b) covering Canada, US, and UK, 23% of establishments have an ERP (see Appendix B.1). While we have no precise data about the use of bonuses in ERPs, observational studies of referrals in firms supports the view that bonuses are used widely (Brown et al., 2016; Burks et al., 2015; Fernandez et al., 2000).

assigned to Control (no ERP); an information intervention that encouraged referrals but did not include bonuses; or to one of three arms that additionally paid different bonuses of up to 40% of monthly salary after taxes if the referrer and referral stayed at least 5 months.

Beyond the large sample size it offers, the firm we study (described more in Section 2) is well-suited for an RCT on the impacts of ERPs for several reasons. First, because of high worker turnover, stores are constantly looking for new workers. Second, grocery cashier jobs have minimal qualifications, so everyone's friends could reasonably be hired. Finally, the retail setting of the firm is representative for millions of jobs worldwide.

To generate testable predictions, Section 3 presents a stylized model in which a worker decides whether to make a referral and whether to quit. Beyond bonuses or effort costs from making a referral, the worker has social preferences toward people she could refer. In addition, following Ellingsen and Johannesson (2008), the worker cares about the firm's belief about their altruism. An ERP makes the worker feel "respected" because it credibly signals that the firm believes the worker is altruistic, as a totally selfish worker may care little about the quality of whom they refer. The model yields predictions regarding the impact of having an ERP or referral bonus level on (P1) referral quantity, (P2) referral quality, (P3) overall attrition, and (P4) firm profits. It also predicts how perceived job quality affects referral quantity and the efficacy of referral bonuses (P5).

Sections 4-8 provide evidence for each of the five predictions. Higher referral bonuses indeed increase referrals (P1). However, while statistically significant, the magnitude of the impacts is economically small. Even under our largest referral bonus, referrals comprised only 5% of hires. Information that the firm welcomed referrals, without a bonus, had no impact on increasing referrals. While the number of referrals made is modest, consistent with P2, referral quality is high: referrals have 40% lower attrition than observably similar non-referrals, and are also (more noisily) less likely to absent. However, also consistent with P2, as referral bonuses increase, the relative retention benefit of referrals falls.

The paper's quantitatively most important, and potentially most surprising, result

stems from P3: having an ERP in a store leads to a roughly 15% reduction in worker attrition. Effects are similar in percentage terms among new workers and workers hired prior to the RCT. These effects cannot be mainly attributed to the incidence of referrals or to peer effects because attrition falls even in treatment stores where no RCT referrals are made. Nor are the effects related to managers behaving differently in treatment stores. Instead, surveys suggest that effects occur from workers feeling respected at being asked to be involved in hiring and/or valuing having a say in who they work with.

Consistent with P4, having an ERP is highly profitable, reducing labor costs by up to 2.7%. About 5% of the savings reflects that referrals have higher retention than non-referrals ("direct benefits"), while 95% of profit gains come from an ERP boosting the retention of non-referred workers ("indirect benefits"). Hence, only comparing referrals vs. non-referrals would dramatically underestimate the benefits of an ERP, which we believe to be a major take-away from our RCT. Direct benefits are non-monotonic in the bonus level, consistent with the model-predicted quantity-quality tradeoff.

Because the firm appreciated the effects of the RCT, it rolled out the ERP to all employees, including non-grocery jobs in logistics and food production. For non-grocery jobs, the ratio of referrals to total hires was almost three times as large as in the grocery jobs. In surveys among workers, managers, but also average citizens, a consistent picture emerges: grocery jobs are perceived as unattractive, implying that workers who care for their friends would not want to refer them for these jobs. However, production and logistics jobs are perceived as much more attractive.[3] These results are consistent with P5.

Our paper contributes to three main literatures. First, it substantially expands what is known empirically about referrals and ERPs, offering the first RCT on an ERP in a for-profit firm. Beyond assessing how ERPs affect referral-making, our RCT enables us to assess how having an ERP and the level of referral bonus affect worker outcomes and firm profits. While larger referral bonuses increase referrals (particularly for non-grocery jobs), we also

[3]The surveys also indicate (1) that job attractiveness explains why there are more referrals for grocery than non-grocery jobs and (2) that it is grocery jobs that are unattractive, not the firm itself.

show for the first time that they decrease the quality of referral hires, thereby illustrating a quantity-quality tradeoff. Prior work summarized by Hoffman (2017) compares average worker outcomes and/or average profit differences between referrals and non-referrals,[4] but lacks exogenous variation in ERPs. Thus, such work cannot assess in what regards ERPs are good or not. The exceptions are Beaman and Magruder (2012) and Beaman et al. (2018).[5]

In Beaman and Magruder (2012), subjects in India were recruited to do games in an economic experiment lab, and then asked to refer someone to participate. Subjects were offered fixed payments of varying amount for referrals or bonuses that depended on the performance of the referral.[6] Beaman and Magruder (2012) differ from us in that they focus on whether people have the ability to screen. In contrast, we focus on how ERPs affect a firm's quantity and quality of referrals, and, most importantly, we analyze whether ERPs have broader organizational consequences beyond the referrer and referral. Another difference between our settings is that we are studying referrals for existing jobs with a definite reputation, whereas lab experiment jobs may have less definite reputations.[7] Despite these differences, our results strongly support a key modeling decision of Beaman and Magruder (2012), namely, that referral-making reflects as if altruism of workers toward their friends.

Second, and beyond referrals, our results contribute to a small but influential literature on dual-purpose HR practices. As emphasized in Rebitzer and Taylor (2011), economists increasingly realize that HR practices can have multiple effects on workers, e.g., performance pay may both increase effort and attract better workers (Lazear, 2000). However, empirical

[4]To study why such differences occur, Pallais and Sands (2016) conduct an RCT in an online labor market. They find that referrals achieve better outcomes than non-referrals, and that this is driven by positive selection of referrals (as well as teamwork considerations).

[5]We discuss the relation of our paper to Beaman et al. (2018) at the end of the Introduction. Also, Kim and Fernandez (2017) show in a survey vignette experiment that people prefer referring jobs to strong ties.

[6]The only statistically significant impact on referrals came from performance-based referral bonuses.

[7]For Beaman and Magruder (2012), this likely has the virtue of abstracting away job reputational considerations, allowing tighter focus on their research question regarding people's ability to screen. Other differences between our settings is that Beaman and Magruder (2012) study one-time jobs, whereas we study indefinite jobs; Beaman and Magruder (2012) study performance on a cognitive ability test, whereas we study employee attrition, employee absence, firm profits, and other firm-level outcomes; and our data cover 18,000 workers (10,000 in RCT), compared to under 1,000 total participants in Beaman and Magruder (2012).

evidence on dual-purpose HR practices is relatively scarce.[8] We show that having an ERP both generates referrals (who yield benefits to the firm relative to non-referrals) and separately causes workers to stay longer due to feeling more respected. Our results are consistent with the theoretical insight of Ellingsen and Johannesson (2008) that workers care about being well-regarded by their employer. As far as we are aware, ours is the first academic paper to argue or show that ERPs can have broader organizational consequences beyond the referrer and referral.

Third, our paper helps advance theoretical understanding of why people make referrals. While quite simple and "partial equilibrium," our model of referral-making (based on superior information, altruism toward friends, and desire for respect) delivers five predictions, all of which are supported in the data. We believe this is valuable because most theories of referrals do not model the decisions of referrers, assuming that referrals occur exogenously.[9]

Beyond our main results, we also examine how ERPs relate to diversity. Practitioners often worry that ERPs may reduce diversity (Frank, 2018; Sharma, 2016). In our firm where women and youth are already over-represented, referrals are 2.8 years younger and 10pp more likely to be female. Having an ERP increases the share of female hires by 4pp, providing the first experimental evidence on how having an ERP affects diversity. These findings echo Beaman et al. (2018), who show that referrals disadvantage female survey enumerators in a Malawi NGO.[10] Our settings differ importantly in that Malawi enumerators are predominantly male, whereas our grocery store workers are predominantly female. Still, our results are broadly consistent with Beaman et al. (2018) in suggesting that ERPs may indeed lead to a less demographically representative workforce.[11]

[8] Noteworthy examples include Ritter and Taylor (1994) and Landers et al. (1996); see Rebitzer and Taylor (2011) for more.

[9] This is true across the main classes of models, including those based on learning (Simon and Warner, 1992; Dustmann et al., 2015), homophily (Montgomery, 1991), and moral hazard (Kugler, 2003; Castilla, 2005; Heath, 2018). Key exceptions are Saloner (1985), who studies competing referrers, and Ekinci (2016), who presents a career concerns model, where whom one refers provides information about one's ability.

[10] Beaman et al. (2018) cross-vary bonus structure (two fixed bonus levels or performance pay) with whether peopled were asked to refer a woman, man, or someone of either gender. In contrast, beyond randomizing the level of the bonus, we randomly vary whether or not an ERP exists.

[11] Smith (2005) argues that within-firm reputational concerns about making "bad referrals" make disad-

2 Study Background

The study firm. The firm is a large grocery retailer in an Eastern EU country.[12] The average store employs 24 workers, 19 of whom are cashiers, one is the store manager, and the rest are department managers or specialists (e.g., butchers, bakers). Stores have an average monthly sales of roughly €200,000. In grocery store jobs, the firm has roughly 5,000 cashiers, plus about 500 specialists. The firm also has 1,200 workers in non-grocery jobs: logistics (primarily truckers), production (workers at a central food production facility), and a small number of white-collar jobs.

Cashiers perform stocking and check-out functions. Most (97%) work full-time, and receive a monthly wage of €320-350 (depending on city vs. countryside location), plus a bonus tied to store performance (4% of wages, on average).[13] The cashier job has no formal requirements, so anyone's friend would presumably be qualified. Applicants are pre-screened via a centralized HR process. Those who pass the initial screen are sent to a store manager, who does interviews and makes hiring decisions. About 20% of non-referred applicants are hired. Cashier training is a two-week session after hire.

As is common for low-skill workers, attrition is high, at an annual rate of 80%.[14] Yet, turnover costs are non-negligible, with direct (administrative and training) costs around €250 per exit, plus additional costs for lost productivity (details in Appendix B.4). Indeed, in meetings with the authors, firm executives expressed strong interest in reducing attrition, and this helped motivated our study in the first place.

Why did the firm do the RCT? In October 2015, we met with the firm's top management and suggested implementing an ERP via an RCT.[15] Having an ERP was quite

vantaged Blacks hesitant to refer friends. Our paper is complementary by showing that referral-making may also be affected by reputational or altruistic concerns with respect to one's friends instead of the firm. Holzer (1987) and Fernandez and Fernandez-Mateo (2006) also study how referrals interact with race.

[12] We avoid naming the country to protect confidentiality, as the firm is one of the largest in the country.

[13] Butchers and specialists are paid €380-480 per month, plus a bonus similar to cashiers.

[14] E.g., among the call-center workers in Burks et al. (2015), roughly half exit in the first 90 days.

[15] Before running this paper's RCT, we worked with the firm on an RCT where the CEO communicated to store managers about the importance of bringing down attrition rates (Friebel et al., 2018). Controlling

natural for the firm to consider for several reasons. First, the firm had an ERP during the 2000s, though it was discontinued in 2008 when the firm's growth came to a halt. Second, some of the firm's competitors pay referral bonuses. Third, we argued that an ERP could help reduce turnover. The firm was willing to do an RCT in order to discover whether to have an ERP and in what form.[16] Thus, while we suggested the RCT and made suggestions on its design, the RCT was carried out by the firm.

Referral process. In the firm's definition, a employee referral occurs when someone is hired via the firm's formalized referral process. The process was designed to make it very easy for employees to suggest a new hire and to make the time costs for employees as low as possible. To make a referral, an existing employee called a dedicated contact in HR and answered a few brief questions (name of referral, relation to employee, how long know them, how often they meet). The phone number and referral process details were listed in the poster put up in the staff common room in each treatment store (Figure 1), with variations depending on the treatment arm. The referrer received a text message if the referrer was hired, and could always call HR again for updates.

RCT details. We refer to the five RCT arms as Control; information only or "R0"; or information plus bonus, with the arms called R50, R90, or R120. In the Control arm, nothing changed relative to before the RCT, and employees were not informed about the possibility to refer. However, HR was told to accept referrals from Control stores if any were called in.

In the four treatment arms (R0, R50, R90, R120), stores managers conducted information meetings with employees. During the meetings, all employees received a personalized letter explaining the ERP. Store managers read aloud the same letter during the meeting. Managers also hung posters in break rooms describing the ERP (Figure 1). The centralized HR office ensured that the meetings took place. Furthermore, HR communicated with the

for a store's treatment status in Friebel et al. (2018) does not affect any of our results.

[16]The firm's executives are generally interested in running RCTs, particularly in regard to operations. Several RCTs or "pilots" occurred during the ERP RCT (e.g., changing the order of items on the shelves).

store managers' bosses (i.e., the regional managers) who also monitored that managers were in compliance with the new ERP. Neither workers nor store managers were informed that an RCT was occurring.[17] Beyond the information provided, workers in R50, R90, and R120 received €15 after the referral was hired, and the remainder of €50, €90, or €120 (i.e., an additional €35, €75, €105) if the referrer and referral stayed 5 months, and this was clearly explained in the letter and posters. Workers hired after the RCT began were given letters explaining the ERP.

Rationale for bonus structure. We suggested a 5-month tenure threshold because we learned from that over half of cashiers leave in the first 5 months while the attrition rate is significantly lower after that. Tenure thresholds are common in ERP bonuses (Brown et al., 2016; Burks et al., 2015; Fernandez et al., 2000). To choose bonus amounts, we surveyed non-grocery workers, who were not part of the RCT, for their opinions about the size of the referral bonus that should be paid. We asked those workers how much money would make them willing to make a referral for a hypothetical vacancy in their unit. We chose the bonus amounts for our treatment arms corresponding to the 25th (€50 per referral), 50th (€90), and 75th (€120) percentiles of the distribution of survey responses.[18] The head of HR suggested using a small immediate bonus of €15 upon the referral getting paid to increase interest.

All bonuses were paid in after-tax amounts, and were designed to be both reasonable and substantial relative to wages. In the R120 arm, the combined post-tax bonus of €120 is 40% of a cashier's monthly post-tax salary, which is comparable to or higher than referral bonuses used in other studies (see Appendix B.2 for details).

RCT timing. RCT materials (posters, letters, and instructions for the store man-

[17]Regional managers were informed at a training event with one of the authors about the nature of the RCT. We felt it was important to inform regional managers about the RCT to ensure that stores were fully compliant. However, regional managers were not involved in any operational or implementation aspects of the RCT, but rather solely monitored were store managers were complying.

[18]The non-grocery workers were told truthfully that we were surveying them as part of academic research; to avoid any announcement effects, no explicit reference to any pilot project in the firm or to our specific RCT was given.

agers) were sent to the treatment stores around November 20th, 2015, with the instruction to implement the ERP immediately. The central HR office and regional managers (to whom store managers reported) ensured compliance of the treatment store managers with the RCT procedures. We registered our RCT with the AEA RCT Registry on November 25, 2015 under ID AEARCTR-0000964. In the fall of 2016, about a year after the start of the RCT, we met with the firm's management to present the RCT results. After this meeting, it was decided to roll out an ERP to all jobs within the firm.

Safeguards to assure RCT validity. There are a couple immediate concerns for an RCT like ours. First, it is critical that employees in treated stores are aware of the ERPs. We address this using posters and letters to employees, and by having regional managers ensure that stores are in compliance. Also, we can test for awareness: in surveys carried out in fall 2016, 87% of employees in treatment stores reported being aware of the ERPs, indicating substantial awareness of the program, even though attrition is high.

Second, workers need to believe bonuses will be paid, as trust is often lower in many post-Communist countries (Aghion et al., 2010). We do not think this was a concern at all, given the group meetings, and the paper trail from the company letters and posters. Workers were told that they could call HR for any questions on the ERPs. Further, given that the country is in the EU and has high formal legal standards, the firm is legally bound to pay bonuses it tells workers it will pay, and workers are aware of this. In surveys among store managers in fall 2016, only 6% reported anything related to the lack of trust in the ERPs.

Data. The data are the firm's personnel and accounting data for Feb 2014-May 2017, complemented with multiple surveys that we conducted. The personnel data are for grocery store workers, cover over 18,000 workers, and contain standard personnel variables (e.g., hire dates, termination dates, and exit codes), as well as absences, earnings, bonuses, hours, and demographics. Importantly, our personnel data also include information from the firm's ERP, including who the referrer and the referral are, date of referral, and relationship of

referrer to referral. The main accounting variables are sales and shrinkage at the monthly and daily level by store. We assemble these datasets to create worker-month and store-month panels.[19]

The surveys cover store employees, store managers, and general population, and were asked before and during the RCT. We asked questions on workers' social connections, reactions to the ERPs, and social perceptions regarding grocery jobs and our firm. Further information on the surveys is discussed along the way, with details in Appendix B.3. Figure 2 summarizes our various datasets.

Randomization. The 238 stores were randomized into the five RCT arms.[20] Table 1 shows that the five store groups are well-balanced over observables. In Panel A, we regress a characteristic on a constant and dummies for the four treatment arms. Thus, the constant corresponds to the control group mean, and the coefficients correspond to differences between the different treatments group and the control group. We also show p-values for the F-statistic of joint significance of the four treatment dummies for each observable, and none are statistically significant. Panel B compares ERP stores (i.e., any of the treatments) vs. control stores, also finding no significant differences.

3 Model

We present a simple model to fix ideas on how ERPs affect employee outcomes, both directly in terms of affecting referrals and indirectly via creating respect. The goal is to make theoretical predictions about our RCT, not to provide an equilibrium analysis of ERPs.

[19] We observed referred applicants (hired and not hired), though 85 of 88 referred applicants are hired in the RCT, so referred hires and referred applicants are almost the same. Among non-referrals, we only observe hires, not applicants. We highlight two data limitations. First, our worker-month panel does not cover non-grocery workers, so our analyses of non-grocery workers are more limited, and use auxiliary data. Second, starting in Jan. 2017, our data do not have information on was referred, only on who made referrals.

[20] Randomization took place by computer. Allocations were re-drawn numerous times until store averages were reasonably similar across the treatment groups in store employees ("head count"), attrition, sales, and store square footage. We control for these variables linearly in our regressions, as suggested by Scott et al. (2002) and Bruhn and McKenzie (2009).

The model uses three key ideas. First, by using an ERP, the firm has access to more precise signals about a candidate's match quality (Simon and Warner, 1992; Brown et al., 2016; Dustmann et al., 2015). However, in contrast to these models, we assume that the information resides with an employee instead of the overall firm. Second, building on Beaman and Magruder (2012), the model includes social preferences of employees toward friends that they could potentially refer. Third, and potentially most important, our model incorporates workers caring about being respected (Ellingsen and Johannesson, 2008). Employees receive utility from their beliefs about the firm's beliefs about their pro-sociality, i.e., employees want the firm to think that they are pro-social.

Set-up. The firm employs an incumbent worker, I, and wants to hire an additional, new, worker. Following Ellingsen and Johannesson (2008), I can be of two different types $\Sigma \in \{0, \sigma\}$, where $0 < \sigma < 1$. Type Σ represents social preferences of I toward an individual, N, of their social network, who could be referred for the job opening. We are agnostic as to whether Σ reflects altruism or reputational considerations. For simplicity, we assume that $\Sigma = \sigma$ for sure, but assume that I initially believes the firm believes that $\Sigma = 0$ for sure.[21]

Incumbent I perfectly observes N's match quality, and chooses whether to refer them, $R = \{0, 1\}$.[22] The match reflects that a particular job suits some people better than others (e.g., some people are better than others at interacting with customers), and we assume $m \sim F(m)$. Making a referral requires a cost of effort $k > 0$. Furthermore, I has an outside option, $\varepsilon \sim G(\varepsilon)$, and decides whether to stay in the firm or leave it. Model timing is:

1. I believes that there is some chance that nature informs the the firm via a private signal that workers have $\Sigma = \sigma$.
2. I believes the firm decides whether to have an ERP. I does not know it is an RCT.
3. If there is an ERP, I has one network contact, N, and decides whether to refer them.
4. I decides whether to leave the firm.

[21] Appendix A.2 discusses this assumption and other model assumptions, including how they can be relaxed.
[22] Note that m can be observed by the firm only after the worker is hired.

Incumbent's Payoffs. I gets utility from three sources: (1) the ERP bonus, $b \equiv \widetilde{b} - k$, (2) N's utility, $U^N(\cdot)$, and (3) her belief, $\hat{\Sigma}$, about the firm's esteem for her. Letting $U^I(R=1)$ and $U^I(R=0)$ be utility from making or not making a referral, respectively, we have:

$$U^I(R = 1) = (1-\Sigma)b + \Sigma U^N(R=1) + B(\hat{\Sigma}) \tag{1}$$

$$U^I(R = 0) = \Sigma U^N(R=0) + B(\hat{\Sigma}) = B(\hat{\Sigma}), \tag{2}$$

Here, N's utility depends on job match, m, and job overall attractiveness, q, with $U^N(R=1) = m+q$. Match m represents all person-specific rewards from the job. Job attractiveness, q, is the same for all workers, and may depend not only on the wage but also on its non-pecuniary aspects, such as working conditions and reputation in society. In (2), we normalize N's utility if he is not referred to 0.

The third term, $B(\cdot)$, represents I's benefit from feeling esteemed or respected (Ellingsen and Johannesson, 2008), with $B(\hat{\Sigma}=0) = 0$ and $B(\hat{\Sigma}=\sigma) > 0$. The term, $\hat{\Sigma}$, is I's subjective belief of the firm's belief about Σ. As mentioned above, we assume that I's prior is $\hat{\Sigma} = 0$, i.e., I initially believes that the firm considers her to be opportunistic.

Firm Profits. The firm's payoff from a referral is $\pi = m - \widetilde{b}$. This is consistent with the fact that bad matches are expensive for the firm, as higher turnover means the firm has to spend a lot of money on training costs. Denote the share of referrals from the total number of employees as r. Then, the expected profit of the firm with an ERP is:

$$\pi = r\big(E[m|m > m^*] - \widetilde{b}\big) + (1-r)E[m] - cPr(Q),$$

where $E[m|m > m^*]$; $E[m]$ are the expected quality matches of the referred and non-referred workers correspondingly; c is the cost of attrition for an incumbent worker; and $Pr(Q)$ is the probability that the incumbent worker exits and is equal to $1 - G(\sigma)$.[23] In contrast, firm profits without an ERP are $E(m) - c(1 - G(0))$.

Our model yields five predictions. We provide intuition here and proofs in Appendix A.1. The model's assumptions are discussed more in Appendix A.2.

[23] The term, $Pr(Q)$, is a reduced form of having an incumbent with larger m than a potential new hire.

Prediction 1. *Higher referral bonuses will increase referrals.*

Prediction 2. *Referrals will be of higher quality than non-referrals. However, as referral bonuses increase, the quality of referrals decreases.*

Referrals are higher quality because I can observe N's match quality, and I prefers to make a referral when m is higher. There is no information on non-referrals so they are hired at random. As the bonus increases, I is willing to refer someone who is less suitable for the job, and average referral quality decreases.

Prediction 3. *Having an ERP increases retention. This should occur even in store where no referrals are made.*

Having an ERP makes I feel respected, as she believes that the firm would only choose to have an ERP if it believed that I had positive social preferences ($\Sigma = \sigma$). This makes I less likely to quit, and because it does not work through referrals, occurs even in stores where no referrals are made. Note that if $\Sigma = 0$, I would make referrals irrespective of m.

Prediction 4. *As long as the referral bonus is not too large, having an ERP increases firm profits. The relationship between referral bonuses and firm profits from hiring referrals (vs. hiring non-referrals) is ambiguous.*

Firm profits increase through two channels. First, having an ERP enables referrals, allowing the firm to exploit I's private information, and this improves profits as long as the b is not too large. Second, profits benefit from I staying longer. Turning to the relationship between bonus level and profits from referrals, on one hand, larger bonuses increase referrals, who are valuable relative to non-referrals. On the other hand, larger bonuses lower average referral quality, and also cost money.

Prediction 5. *More referrals will be made for attractive jobs than for less attractive jobs. Suppose that* $f'(m^*) < 0$, *which occurs if referrals are few. Then, the more attractive the job, the more responsive are referrals to bonuses.*

The first sentence reflects that I has social preferences toward potential referrals. For the third sentence, note that if a job has very low q and referrals are rarely made, then I is unlikely to be marginal, and increased bonuses may do little to push I to make a referral. However, for a higher quality job, I is more likely to be marginal.

P1-P4 are tested using the RCT. P5 is tested using surveys and the firm-wide ERP rollout.

4 Referral Bonuses and Referrals (Prediction 1 of Model)

Table 2 summarizes referral patterns across the five arms. There are 88 referred applicants and 85 referred hires. In 79 of 85 cases, referrals are hired in the same store as their referrers. Of the 6 exceptions, 3 are hired in the Control stores, where no information about an ERP was provided and no referrals are made. There are also no referrals made in information only ("R0") stores. The number of referrals made monotonically increases with the bonus. Still, in the highest bonus arm ("R120"), only 5% of hires are referred.

Figure 3 plots quarterly referrals made divided by the number of hires by treatment arm, showing a modest ratio throughout the RCT. After the RCT, when a single ERP is rolled out to the entire firm and the bonus increased, referrals increase, but the ratios are similar across the former treatment arms.

Table 3 shows RCT impacts of ERPs on whether a hire is referred, but using regressions with a number of store and individual controls listed in the table notes. Throughout the regressions, standard errors are clustered at the store level, as randomization occurs at the store level. Column 1 of Panel A regresses whether a hire is referred on dummies for the four treatment arms (an observation is a hire), where Control is the excluded category.[24] The results are similar with controls in column 2. Instead of using dummies for the four

[24]The coefficient on R0 is slightly negative, reflecting that there are 3 referrals hired at Control stores and 0 referrals hired at R0 stores. The 3 referrals hired at Control stores were referred by workers at different stores paying bonuses.

treatments, column 3 uses a dummy for having any of the four ERPs (excluded category is Control store). Having an ERP increases the chance an employee is referred by 2.5pp. This is highly statistically significant, but seems economically modest.

The 88 referrals occur in 34 stores. Referred workers are younger and are (weakly) more likely to be female, but we postpone age and gender analysis until Section 9.

5 The Quality of Referred Workers (P2)

As described in the RCT pre-registration, our main outcome variable is attrition, and our secondary outcome is absence. We focus on attrition for three reasons. First, like many firms, the firm regards high attrition as a critical business issue, causing it to spend large sums recruiting and training new hires, and high-turnover stores also have lower sales.[25] Second, worker retention is a standard measure of match quality (Jovanovic, 1979). Third, past work finds that some of the largest differences between referrals and non-referrals occur in attrition (Hoffman, 2017), so it seems natural to investigate attrition when analyzing ERPs. Absence is also an important outcome in grocery jobs (Glover et al., 2017) and other low-skill jobs. As for attrition, absenteeism is high and expensive for our firm, but we emphasize it less, first, because the firm regards attrition as the HR outcome of greatest interest, and second, because days absent in a month is highly skewed, allowing for less precision.[26]

Attrition. Panel (a) of Figure 4 shows that referred hires have higher survival than non-referred hires without any controls. Next, we add the controls that we will generally use for analyzing panel data, namely month-year of hire dummies, current month-year dummies, a 5th order polynomial in tenure, a dummy for being a cashier, demographic controls, and pre-RCT means of store-level characteristics (with the full list in the table notes). Table 4

[25] High attrition also impose serious costs and performance consequences on US firms, particularly in retail (Ton, 2014). As a policy issue, when attrition is high, firms may invest less in helping workers develop skills.

[26] Another common outcome in supermarkets is items scanned per minute (Mas and Moretti, 2009; Glover et al., 2017), but the firm's IT system does not allow us to measure worker-level items per minute. The firm's two main HR key performance indicators are attrition and absence.

estimates linear probability models of the attrition of workers hired during the RCT. In line with past work, Column 1 of Table 4 shows that, compared to non-referred workers, referred workers are 7.0 percentage points (hereafter, "pp") or 44% less likely to attrite each month.

Column 2 analyzes referral differences in turnover separately by during a worker's first five months of tenure and afterward. In months 1-5, referral attrition is lower by 9pp (or 50% relative to the attrition rate of non-referrals in the first 5 months), whereas it is lower by 3pp or 1/3 thereafter. Thus, while referral differences in attrition are strongest during the first five months, consistent with the structure of the referral bonus, referrals are still less likely to attrite after the 5-month milestone.[27]

Consistent with Prediction 2 of the model, column 3 of Table 4 shows that referral/non-referral attrition differences are smaller at higher levels of the referral bonus. For the R50 treatment group, the referral attrition difference is -11pp or about 70%. In contrast, for the R90 and R120 groups, the referral differences are about -6pp or a bit under 40%. These differences are statistically significant (p=0.03 for R50 vs. R90; p=0.05 for R50 vs. R120).

Table 4 classifies referrals according to the store where they work. However, results are robust to excluding the 6 referrals who get hired in different stores than their referrers.

Absences. Because the distribution of monthly absences is highly skewed, we perform negative binomial regressions.[28] Column 4 shows that referrals have 19% percent fewer absences per month, but this is not statistically significant. Column 5 shows that, during the first 5 months, referrals have significantly fewer (40%) absences than non-referrals, but after that, there is no difference. This could be due to referrals not wanting to be fired before 5 months to ensure that their friend gets the bonus. Referral absence differences do

[27] If referrals were staying longer than non-referrals solely to get a bonus, then referred attrition would be higher than non-referred attrition after 5 months, but this is not the case. Also, that referral differences are larger in the first 5 months than after does not mean that referrals are not useful for the firm. Even if referrals stayed longer solely to get a bonus, which does not seem to be the case in our setting, this could be the valuable to the firm. Appendix Figure D1 shows that referrals are less likely to attrite than non-referrals at most tenure levels.

[28] In column 4, the estimated overdispersion parameter is $\alpha = 23.3 (s.e. = 0.96)$. This indicates a high degree of overdispersion and that negative binomial is more appropriate than poisson (Cameron and Trivedi, 2005).

not significantly vary by bonus size.

Adding store dummies. For analyzing referral/non-referral differences, we can add store fixed effects, which is useful given it is a non-randomized comparison.[29] Appendix Table D1 show that referral attrition differences are similar (and slightly larger) when store fixed effects are added. In contrast, the absence results lose significance and become noisier. Consistent with Burks et al. (2015), there are stark referral differences in attrition and more tentative ones in non-attrition performance variables like absence.

6 The Impact of ERPs on Worker Outcomes (P3)

6.1 Results

Attrition. Table 5 shows that ERPs reduce attrition of all workers, with similar percentage effects on new hires and incumbents.[30] Column 1 of Table 5 analyzes the impact of the randomly assigned ERP treatments on attrition during the RCT (as opposed to comparing referrals vs. non-referrals). Relative to workers in Control stores, workers in R0, R50, R90, and R120 stores have monthly attrition that is lower by 1.01pp, 0.45pp, 1.53pp, and 0.77pp, respectively, corresponding to attrition reductions of 15%, 7%, 23%, and 12%. These differences are statistically significant for R0, R90, and R120. Column 2 shows that having an ERP reduces attrition by 0.95pp or 14%. Given that referrals were only 2.5% of hires in ERP stores, it seems unlikely that these differences are entirely or primarily due to referred workers staying longer than non-referrals or people becoming more likely to stay as a result of making a referral. Comparing R0 vs. Control, recall that there are 0 referrals made and 0 referral hires in R0 stores. Thus, any reduction in attrition in R0 stores relative to Control

[29] In our main results on the impact of ERP, we cannot control for store fixed effects because our treatment is randomized at the store level, though we can control for store fixed effects if we exploit pre-RCT data.

[30] These results do not control for age because age is missing for workers who are hired before the start of the data and who do not attrite, and this missingness is highly correlated with attrition. However, if we restrict the analysis to workers hired after the start of the data period, whether or not we control for age has little impact on the main estimates.

stores cannot be due to workers being referred or making referrals.

Though our treatments are randomized, we may obtain additional power or control by exploiting the personnel data before the start of the RCT. Columns 3-4 report the results from a difference-in-difference regression of attrition on the RCT and treatment period dummies and their cross-product. Store dummies are included to control for persistent differences across stores in employee attrition and other characteristics, and current month-year dummies account for differences in attrition over time. Relative to column 1, results are slightly stronger in column 3, with significance for all 4 treatments. The column 4 coefficient of -1.21pp corresponds to a reduction of roughly 20%.[31]

To better understand the dynamics of the ERP effects, Figure 5 presents an event study where having an ERP is interacted with the quarter since the ERP RCT began. ERP impacts occur in the first quarter ("quarter 0" or Dec. 2015-Feb. 2015) of the RCT, though effects take a few months to fully realize. There is no pre-trend, supporting the validity of the RCT. After the RCT ends, and the ERP is rolled out to the Control stores, the attrition difference between treatment and Control stores vanishes. Panel (a) presents overall results. Panel (b) shows similar results restricting to stores where no referrals are made during the RCT.

Columns 5-6 of Table 5 show impacts of the ERPs on attrition of workers hired during the RCT, whereas columns 7-8 show impacts on incumbent workers, i.e., people already working at the firm at the start of the RCT. Panels (a) and (b) of Appendix Figure D2 show ERP impacts interacted with quarter of the RCT for new hires and incumbents.

Panels (c) and (d) of Appendix Figure D2 shows that the overall impact of attrition is driven by a decrease in voluntary attrition, whereas ERPs had no impact on (and insignificantly increased) involuntary attrition.

Attrition magnitudes. We estimate that having an ERP reduces overall attrition by about 15%, an economically sizable estimate. As a benchmark, Bloom et al. (2014a)

[31]The results are further robust to (and slightly stronger when) including store-specific time trends.

show that randomly assigning employees to work from home reduces attrition by half in Chinese call centers. Thus, compared to variables like TFP and output per hour, attrition may be easier to change. As another benchmark, in an earlier RCT, we found that a letter by the CEO to store managers asking them "to do what they can" to bring down turnover resulted in a 25% drop in turnover (Friebel et al., 2018). Hence, our setting may be one where turnover is relatively malleable.

Absences and store-level outcomes. Appendix Table D2 shows that ERPs have no impact on absence. Column 2 (baseline) estimates a coefficient of 0, whereas column 4 ("diff-in-diff") indicates that having an ERP reduces absence by 8%, but it is statistically insignificant.

Appendix Table D3 shows that having an ERP does not significantly affect store-level outcomes, including store-level hours, the shrinkage rate (i.e., share of inventory lost to theft, spoilage, and other reasons), sales per worker, or operational profit per worker (i.e., net sales minus cost of goods minus wages minus overheads). The coefficients on shrinkage, sales, and operational profit have a sign indicating benefit to the firm, but are not statistically significant.[32]

6.2 Mechanisms for ERP Impacts

The most natural reason an ERP would reduce turnover is by promoting referrals, as referrals are less likely to quit and referrers may be more likely to stay to get a bonus. Using mediation analysis and analyses restricting to stores with no referrals, Section 6.2.1 argues that promoting referrals explains only a modest share of the impact of an ERP. Section 6.2.2 next discusses additional mechanisms that, while plausible *ex ante*, are *ex post* inconsistent with the RCT results. Section 6.2.3 discusses mechanisms that are consistent with the RCT evidence, including our preferred mechanism of respect, and uses surveys to tease these apart.

[32]Operational profit is not a full measure of profit, so the statistically insignificant results here are not in any way inconsistent with our results on profits in Section 7. While the estimates Appendix Table D3 are reasonably precise, we appear to lack the statistical power to detect small changes in these variables.

6.2.1 Assessing Referrals as the Mechanism

Mediation analysis. How much of the effect of ERPs on attrition (a 15% reduction) came via effects related to referrals, i.e., getting more referrals or making referrers more likely to stay? Following Imai et al. (2010a,b), consider the following system:

$$M_{it} = \alpha_0 + \alpha_1 ERP_i + X_{it}\delta_2 + u_{it} \quad (3)$$

$$y_{it} = \beta_0 + \beta_1 ERP_i + \gamma M_{it} + X_{it}\delta_2 + v_{it} \quad (4)$$

Here, y_{it} is an outcome of person i in month t; M_{it} are the mediator variables, namely whether someone is referred or has made referrals; ERP_i is a dummy for experiencing an ERP; X_{it} are controls; and u_{it} and v_{it} are errors. A key goal in the mediation analysis is to estimate β_1 and γ. The mediator effect is $\alpha_1 * \gamma$, whereas the non-referral effect of ERP is β_1. Imai et al. (2010b) show that OLS produces consistent estimates under the assumption of Sequential Ignorability:

Assumption 1

$$y_{it}(e,m), M_{it}(e) \perp\!\!\!\perp ERP_i |\ X_{it} \quad (5)$$

$$y_{it}(e,m) \perp\!\!\!\perp M_{it}(e) |\ X_{it} \quad (6)$$

where $y_{it}(e,m)$ is the potential outcome for worker i in month t under treatment e and mediator m and $M_{it}(e)$ is the potential mediator under treatment e. Equation 5 of Assumption 1 will hold because of random assignment. While Equation 6 (i.e., that potential referral status is independent of potential duration conditional on observables) is much less obvious, we hypothesize that any bias is likely to have little effect on our qualitative conclusion that referrals are not a main driver of the ERP effect, as few RCT referrals were made, and the ERP coefficient is little affected by controlling for mediators.

Table 6 shows results. Columns 1-2 show the impact of ERP on being referred and referrals made to date using the full panel data. Columns 3-5 shows the impact of ERP as being referred and referrals made to date are gradually controlled for. The coefficient only falls from 0.95 to 0.86. The estimates imply that only 9% of the impact of ERP on

attrition is mediated via getting more referrals and having more referrals to date, whereas 91% remains unexplained.

Restrict to stores with no referrals. Appendix Table D4 shows that our main attrition results are similar when restricting attention to stores where no referrals are made during the RCT. If no referrals are made in a store, then there are no referrers, and only referrals that are made from other stores, making it very hard for referrals to drive the impact of ERP. This analysis reinforces the message of the mediation analysis, as well as the finding that R0 stores have roughly 15% lower attrition than Controls stores, even though the R0 treatment induced no referrals.

6.2.2 Unlikely Mechanisms for Non-referral Channel

Peer effects in attrition from referrals. It is unlikely that peer effects from referrals or referrers drive our results. First, there were relatively few referrals made. Second, and more importantly, the overall impact of having an ERP on attrition is similar to our baseline estimate even while restricting to stores where no referrals are ever made during the RCT.

ERPs help the firm make better hiring decisions. Having an ERP could help a firm improve its hiring process, perhaps by helping a store manager learn about what type of candidates to target or by freeing up additional time that would be spent on interviews. However, this also seems unlikely to explain our results. Beyond the fact that ERPs have large effects in stores where no referrals are made, ERPs also have similar percentage impacts on incumbents relative to their impacts on hires. This mechanism cannot explain why ERPs reduced incumbent attrition.

Other concurrent policies or managerial reactions in treatment stores. The firm informed us that it did not institute any concurrent policies in treatment stores. Despite this, could there have been any additional informal changes or changes in managerial effort? Recall that store managers were not aware there was an RCT. Furthermore, none of the ERPs

affected self-reported store manager time use, which we measured in a survey of managers in fall 2016.[33]

Control store frustration. Instead of workers in treatment stores being less likely to quit, perhaps workers in control stores became more likely to quit, if they happened to hear about the ERPs in other stores. There are a couple pieces of evidence against this interpretation. First, HR was told to accept referrals from treatment stores if employees called to make them, but they did not get any referrals from control store. Second, we instructed HR to record any complaints that it received from control stores about there not being an ERP, but there were no complaints made.

6.2.3 Possible Mechanisms for Non-referral Channel

The impact of ERP is strong in stores where no referrals are made; is relatively flat over time; affects hires and incumbents in similar percentage terms; is driven by quits, not fires; affects turnover, not absence; and treatment/control differences vanish once ERP is rolled out to control stores. What can explain this? We believe it should be a mechanism or mechanisms that increase the non-wage value of working at the firm, making employees less likely to quit but no more likely to exert effort to increase sales, reduce shrinkage, or not be absent. Such mechanisms may include:

1. *Employees feel respected after being asked to be involved in hiring or liked having some say about who they might work with.* Involving workers in hiring can be beneficial because it makes workers feel respected, perhaps because it gives workers some voice (Hirschman, 1970; Turco, 2016) or mild decision authority (Fehr et al., 2013; Bartling et al., 2014) regarding their co-workers.
2. *ERP is a positive signal about the firm being a better place to work.* Instead of being something to hiring or who a worker gets to work with, an ERP may increase a worker's

[33]This finding is in contrast with a significant effect on store manager time use of some of the treatments in Friebel et al. (2018), which explicitly targeted store managers.

perception of the overall quality of the firm, e.g., having a costly ERP may raise a worker's perception of how profitable the firm will be in the future.

3. *Workers think they may make referrals in the future.* Even if few workers took advantage of the ERP during the RCT, workers could imagine that there would be some chance that they would use the benefit in the future.
4. *ERPs increase informal referrals.* The ERPs could have increased "informal referrals," i.e., people who may have informally heard about the job from a friend, but where the friend was not willing to call HR to register the referral.

To shed further light on these explanations, we conducted phone surveys with 222 store managers and an in-store electronic kiosk survey with 113 store workers from the study firm. We explained that ERPs had reduced attrition at the firm separate from generating referrals, and asked them their opinion on which of the above four mechanisms (or a 5th option of a mechanism of their own choosing) was most likely to explain the result. We randomized the order in which the four mechanisms were presented. Table 7 shows that (1) was by far the most common explanation, chosen by 66% of managers and 50% of workers. There are some modest differences between workers and managers, e.g., workers think that (3) is a bit more common than managers, but the overall message from both populations is the same.

For option (1), one may wonder whether it may be parse further into whether (a) employees felt respected about being involved in hiring or (b) whether they liked having some say about who they might work with. For workers who selected (1) in the worker survey, they were asked whether (a) was the main reason, (b) was the main reason, or both were equally likely. On this, 15% said (a) was the main reason, 17% said (b) was the main reason, and 67% said both were equally likely.

For workers to feel respected from the ERP, one would imagine that they workers need to believe that referred candidates will be hired. This seems likely given that 97% of referred candidates during the RCT were hired, compared to roughly 20% for non-referred candidates. The surveys also asked respondents whether workers believed that referred candidates would

be hired, and respondents overwhelmingly answered yes (with details in the Appendix).

While researchers have not previously considered that ERPs may worker feel more respected, it is highly consistent with evidence from practitioners, who often argue that involving workers in hiring may increase their engagement.[34] One recruiting website argues that ERPs "help increase attachment to the organization and make employees feel as though they have a stake in the future of the business. Employees want to grow, so having a hand in the company's forward motion is exactly what they're looking for." Another recruiting website argues that ERPs make "current employees feel trusted and valued since they are participating in the companys future and growth."[35]

7 Referral Bonuses and Profits (P4)

We use the P2 and P3 results to calculate the profitability of the ERPs. Past work has calculated the profits of hiring a referral relative to a non-referral (Fernandez et al., 2000; Burks et al., 2015), but has yet to be able to calculate profit gains from an ERP. Since the ERPs reduced turnover, but did not affect absence, sales, or shrinkage, we focus exclusively on the attrition impacts. ERPs may also reduce a firm's recruiting costs (e.g., due to less time interviewing to find a candidate), but we set those aside, given we lack applicant data on non-referrals.

The attrition benefit of an ERP per worker-month is tc, where t is the impact of an ERP on turnover and c is the turnover cost. We also calculate the benefit of ERP in terms of specification populations, namely, referrals and all new hires. For population p, the benefit of lower turnover is $\theta_p t_p c$, where θ_p is the share of worker-weeks from group p in the treatment group and t_p is the attrition difference within population p. For all hires, we estimate t using column 2 of Table 5. We present results where c is based on direct, administrative costs

[34] For example, DeLong and Vijayaraghavan (2002) describe an investment bank that strongly involved the firm's bankers, from entry-level to senior-level, in hiring.

[35] The first quote is from: https://recruiterbox.com/blog/4-reasons-why-an-employee-referral-program-may-be-your-best-recruiting-tool and the second is from https://www.formstack.com/blog/2016/employee-referral-system-benefits.

(c =€250) or where c is based on the "full costs" of higher turnover (c =€1,150).

The cost of an ERP is the bonus paid to the referrer. The cost per referral is $b_0 + Pr(both) * b_1$, where b_0 is the bonus paid upon hire; $Pr(both)$ is the probability that referrer and referral stay 5 months after the referral; and b_1 is the bonus paid after 5 months. Appendix B.4 gives further detail on c and Appendix B.5 gives further detail on the profit calculation.

Results. Panel A of Table 8 reports the overall benefits from having an ERP versus not. Focusing first on c =€250, the benefit/savings from ERP is €2.37 per worker-month, far exceeding the cost per worker-month of €0.11. The overall net profit per worker-month is €2.26, or 0.6% of the labor costs. Only 6% of the turnover benefits accrue from ERPs yielding referrals, who have lower attrition. Rather, the vast majority of the benefit accrues from non-referral hires having lower attrition and from incumbents having lower attrition in ERP stores. Under c =€1,150, having an ERP become even more profitable, increasing profits by roughly €11 per month, or 2.7% of labor costs, a substantial benefit for a competitive industry like grocery retail.

If the ERP were evaluated entirely based on lower turnover from referrals, the benefits still outweigh the costs, though benefits and costs are relatively comparable under c =€250. The comparison becomes radically different once we account for the non-referral turnover benefits of an ERP.

Panel B repeats Panel A separately for the different ERPs. We use the more conservative c =€250. Focusing first on the turnover benefits from referrals hired during RCT, the benefits are non-monotonic, reflecting differences in the quality and prevalence of referrals between treatment arms. This broadly supports P3 of the model, i.e., that the relationship between b and profit benefits is ambiguous. Overall turnover savings are also non-monotonic in b, reflecting the non-monotonic relation in column 1 of Table 5. As for Panel A, an analyst focusing only on the referral benefits of ERPs would reach vastly different conclusions regarding the profitability of ERP (e.g., based solely on referral benefits, there is little profit

benefit from R120). The information-only ERP yields large profit gains despite producing 0 referrals.

8 Job Quality Perceptions and Referrals (P5)

For our last prediction (P5), we lack randomized variation in perceptions of job quality. Instead, we exploit survey evidence from firm managers, firm employees, and our firm's host country's general population, and we also analyze the post-RCT firm-wide ERP rollout.

General public survey on occupational attractiveness. P5 says first that referrals are less likely for less attractive jobs. Panel (a) of Figure 6 presents ratings of the attractiveness of different occupations using surveys of the general population in the host country. Cashier jobs, comprising 90% of hires during our RCT, are the lowest-rated job of occupations asked, and our estimate of the mean is precise. In contrast, non-grocery jobs at the firm, namely, those in logistics and food production, are rated substantially higher.[36]

Employee and manager surveys. Table 10 shows that managers and workers strongly believe that the reason why the RCT only modestly increased referrals is because grocery jobs are regarded as undesirable. In the fall 2016 manager survey, we asked an open question about why ERPs had little impact on getting referrals. Undergraduate students in a lab later classified the reasons into several categories. As seen in column 1 of Panel A, the most common explanation, given by half of managers and four times more common than the next most common explanation, is that grocery store jobs are undesirable. In column 2, the share rises to over 2/3 if we exclude (1) the mechanical explanation of no open jobs in the store, (2) the response that the referral bonuses actually worked well, and (3) exclude instances where managers gave no reason. Panel B of Table 10 shows that similar findings apply to workers. We gave cashiers six most frequently mentioned reasons from the manager

[36] While panel (a) of Figure 6 accords with many aspects of occupational prestige in the US, there are some differences, e.g., doctors or teachers are not ranked very highly. This reflects historic reasons (the country is a post-communist society) as well as lower earnings in these professions relative to required qualifications.

survey and asked them to rank them.[37] 51% of workers listed "Many people perceive working conditions in supermarkets as not very attractive (e.g. low salary, high workload)" as the #1 reason why employees were not making referrals.

Beyond job quality, other reasons never featured prominently in the two surveys. First, reputational concerns vis-a-vis the firm, as opposed to vis-a-vis their friend, were not mentioned often. Only 12% of managers gave a response involving concern about people not wanting to make a referral to avoid embarrassment; likewise, only 16% of workers thought "Employees don't want to want to be responsible if their friend doesn't do a good job" was the main reason for the limited impacts observed. Second, if workers lack friends to refer or these friends already have good jobs, then an ERP cannot generate many hires. But the idea of workers not having friends looking for jobs was rarely mentioned.

Firm-wide ERP rollout and referral bonus increase. Because of the benefits from the RCT, the firm decided to roll an ERP across the firm. Under the new regime, employees received €30 when a referral was hired, plus an additional €100 if both parties stayed 3 months. This constitutes three changes, all of which make the bonus larger in expected value. First, twice as much money is being paid upon hire. Second, it reduces the months that the referrer and referral must stay for the referrer to get paid (3 instead of 5). Third, the second payment is higher (€100) is higher than in all but one of the RCT arms.[38]

The rollout serves several functions for us. First, it allows us to examine the impact of an ERP in grocery and non-grocery jobs. Second, it helps rule out concern that the RCT bonuses were too low in perceived expected value.[39] Third, we can test whether differences between treatment and control stores disappear once all stores are given the same treatment.

[37] These were the five most frequently mentioned reasons; to these, we added a sixth reason that wasn't mentioned, namely, that the size of the bonus could have been too small.

[38] There are three limitations that must be kept in mind in considering the new ERP. First, the firm was not willing to randomize this change. Second, we only were able to obtain data on who made referrals, not who was referred. Third, because our personnel data only cover grocery store workers (and not non-grocery store workers), we can only match the data on who made referrals to individual personnel records for grocery workers. Despite these limitations, the data on referrals made can still be very fruitfully used.

[39] Though the surveys indicated otherwise, perhaps workers were very present biased or had some other general concern regarding not getting the bonus fast enough. Having to wait 5 months to get a referral bonus is not a trivial length of time, particularly when almost half of referrals are leaving in the first 5 months.

Among grocery store workers, Table 9 shows that in the post-RCT period (January-May 2017), the ratio of referrals made to hires was 12%, which is an increase above the 5% ratio in R120 during the RCT. However, among non-grocery workers in production and logistics, the post-RCT ratio was about 35%. While are differences between grocery and non-grocery jobs other than attractiveness, these differences seem to reinforce our story.[40]

In sum, while front-loading and increasing the bonus further increased referrals for grocery jobs, the ratio of referrals made to hires only went to 12%; this suggests that front-loading plays a role, but is unlikely to be the primary cause of our RCT finding that bonuses modestly boosted referrals for grocery jobs. Second, the referral rate was substantially higher for non-grocery jobs, which is consistent with our earlier evidence that the bad reputation of grocery jobs plays a key role in limiting the efficacy of ERPs.

Asking the general public why there were more referrals for non-grocery than grocery jobs. To try to address some of these limitations, we did a second survey of randomly selected citizens from the country where the firm operates.[41] We asked them why they thought that few referrals were made for grocery jobs, whereas significant referrals were made for non-grocery jobs. As seen in column 3 of Panel A of Table 10, nearly 3/4 of respondents ascribed the difference in referral rates between grocery and non-grocery to grocery jobs being undesirable.

Alternative explanations beyond job quality perceptions for our evidence in support of P5. We discuss a few alternative explanations for P5 below, leaving the remainder to Appendix B.6.

Job or the Firm? We argued that our results are driven by grocery jobs being unattrac-

[40] For production and logistics jobs, the pay is higher than for cashier jobs, making the fixed €30 + €100 referral bonus a smaller share of pay. Another difference is that, unlike grocery jobs, not everyone's friends could work in logistics or food production. For logistics, most of the jobs are truck driver positions for which a license is required. For food production, the jobs require working at a central facility (unlike the grocery store jobs which are located around the country). Such restrictions should work against generating more referrals.

[41] By this point of time, the study firm CEO had left, and we (the authors) lost our ability to collect new surveys within the firm. Still, surveying the general public instead of firm employees provide a useful outside perspective.

tive, but could they be driven instead (or in addition) by our study firm being unattractive? Panel (b) of Figure 6 shows that our firm is well-regarded relative to other retail firms in the host country, suggesting that results are not driven by the firm being unattractive.

Reputational Cost toward the Firm? An alternative explanation for few referrals is that workers were concerned about their standing at the firm and did not wish to refer someone who could embarrass them (Smith, 2005). However, only 12% of managers surveyed gave a response involving concern about workers avoiding referrals to prevent embarrassment. Likewise, only 16% of workers surveyed thought "Employees don't want to want to be responsible if their friend doesn't do a good job" was the main reason for the limited impacts observed. Moreover, reputational cost toward the firm would not generate P4 or P5.

Were employees unaware of the ERPs? The firm took many steps to ensure that the ERPs would be well-understood and well-publicized to workers. This included the letters and posters described in Section 2, plus phone calls to ensure that store managers publicized the ERPs, plus guidance to regional managers to ensure that store managers were compliant. Also, in the fall 2016 survey, we asked workers if they were aware that the firm welcomed referrals, and 87% said yes in treatment stores. This indicates persistent awareness of the ERPs even though many workers attrited during the RCT. Further, in Panel B of Table 10, the explanation of employees not being aware of the ERP / not knowing how it worked shows quite limited support. A related issue would be if people forgot about the ERPs after a few months. In such a scenario, some referrals would be made after the ERPs were introduced, but effects would peter out over time. However, Figure 3 shows that this is not the case.

Job quality and responsiveness to bonus level. For non-grocery jobs, there was no ERP before the firm-wide rollout. Thus, it is somewhat challenging to use the non-grocery evidence to properly test the second part of P5, i.e., that referrals are more responsive to referral bonuses in good jobs than in bad jobs.[42] However, Table 9 shows that we can

[42] As far as we aware, formal referrals were not being made for non-grocery jobs before the firm's roll-out in January 2017. Thus, one can think of our evidence as tracing out a referral responsiveness curve, where initially there were 0 referrals at a bonus level of 0, and then either 35% or 12% at the bonus of €130.

provide evidence by separating grocery jobs into cashier jobs and non-cashier grocery jobs (e.g., butcher, baker, assistant manager), which are regarded as more attractive than cashier jobs. During the RCT, the ratio of referrals made by the group to hires was 5% for non-cashier grocery jobs compared to 2% for cashier jobs. Post-RCT, the ratio was 30% for non-cashier grocery jobs compared to 12% for cashier jobs. That is, ERPs have more of an impact on boosting referrals for better jobs, consistent with P5.

9 ERPs, Age, and Gender

While we focus on our pre-registered outcomes of attrition and absence, we here explore the common concern that ERPs may affect worker demography and make the workplace less diverse. Table 11 explores how referrals and ERPs relate to age (Panel A) and gender (Panel B). Column 1 shows that referrals are 3 years younger than non-referrals,[43] and are 10ppmore likely to be female, though this last difference narrowly misses statistical significance. Having an ERP increases the share of female hires by 4pp, though it does not affect age. Given that grocery workers at the firm are 85% female, making the workforce more female is making the workforce less representative of the general population, and thus decreases workforce diversity.[44]

Despite this, referrals yield high-value hires from disadvantaged workers. A common policy concern in many EU countries is youth unemployment, and in the country we study, the youth unemployment rate substantially exceeds that of older workers. In informal interviews with five store managers at our firm, all five expressed concern that young workers are more likely to quit than older workers. Column 4 of Panel A shows that this is, in fact, the case: among non-referrals, young workers (defined as under 25 at time of hire, following

[43] We are aware of limited work that finds this result. In an analysis of neighborhood effects, Bayer et al. (2008) observe that referral differences are greater for young workers. Other HR management practices interact with age, e.g., Friebel et al. (2017) find that young workers drive the impact of group bonuses.

[44] Likewise, although ERP does not affect age, referred workers are younger. Given that a high share of the firm's workforce is young, the referrals achieved are less demographically representative of the general population.

the OECD definition) are almost 50% (or 6pp per month) more likely to quit than older workers. However, referred young workers are no more likely to quit than non-referred older workers. If a firm is concerned about hiring a young worker and quickly losing them, our results indicate that referrals seem to neutralize this concern.[45]

Another common policy concern is that women have higher rates of absenteeism than men (Ichino and Moretti, 2009). Indeed, among non-referrals, women have 48% more absences than men (Column 5 of Panel B). However, referred female workers are no less likely to be absent than non-referred men. In fact, referral vs. non-referral differences in absenteeism are entirely concentrated among female referrals.

10 Concluding Remarks: External Validity

ERPs may affect firms in two ways: (1) Directly, i.e., by affecting referrals, or (2) Indirectly, i.e., via costs or benefits separate from generating referrals. We use the first RCT on ERPs in a for-profit firm and the post-RCT firmwide rollout to better understand these two pathways.

On (1), we find that larger bonuses increase referrals and that referrals are higher quality than non-referrals, though the share of referrals is modest. Larger bonuses decrease referral quality and the direct profit benefits of ERPs are non-monotonic in the bonus. Further, the level of referrals and the responsiveness of referrals to bonuses is higher for more attractive jobs. All these results are predicted by a model where referrers receive utility both from money and from the utility of their social contacts. These findings are consistent with, but substantially expand, the earlier findings of Beaman and Magruder (2012).

On (2), we show that ERPs can have substantial indirect benefits. Having an ERP reduces turnover by 15% and these effects are present even in stores where no referrals are made. The ERPs we randomized were highly profitable and 95% of the benefit came from

[45] One concern is that young workers might be working summers off from school, and would naturally attrite at the end of summer. However, column 4 is very similar when dropping summer months.

indirect benefits. Surveys suggest that indirect benefits arise from employees valuing being involved in the hiring process and having some say over who they would work with. These results help explain why ERPs are a common management practice. More generally, and beyond ERPs, our results help rationalize why firms seek employee participation in hiring (beyond the importance of using worker information for selection), and support the Ellingsen and Johannesson (2008) model of respect in the workplace. Beyond hiring, the notion that HR practices that involve workers may cause them to feel more respected may be relevant for many workforce practices, such as idea suggestion systems and 360 degree evaluation.

It is important to consider that the conclusions of a one-firm RCT may be context-specific, even when the sample size is very large. On (1), our results certainly do not imply in general that ERPs will only modestly increase referrals—in fact, the post-RCT ERP was very effective in motivating referrals for non-grocery jobs at our firm. In a high-skilled context, it could be that people are more responsive to bonuses, or potentially less responsive, e.g., if referral-making is instead driven by strong career motivations.

On (2), would having an ERP make workers feel respected outside of the low-skill jobs we study? We speculate the answer is Yes. Our experience is that professors sometimes skip various faculty meetings, but nearly everyone comes to faculty hiring meetings, suggesting that faculty like being involved in hiring. Of course, workers in high-skill jobs may feel more respected than workers in low-skill jobs, so it is possible that indirect benefits of ERPs would be lower in high-skill jobs.[46] On the other hand, if people are more willing to make referrals for better jobs, then there may be more opportunities to be involved in hiring for high-skill jobs than for low-skill jobs, possibly making respect benefits larger for high-skill jobs.

To go beyond casual observation and speculation on the external validly of (2), we conducted a survey of a representative sample of 400 US workers. We provided the following vignette (with bolding as in the original):

An employee is working at a firm where an ***employee referral program*** *is*

[46]Higher skill jobs also often have much lower hiring rates, even for referred workers.

> *introduced. Under the program, employees are asked to refer their friends for jobs, and they are paid a* ***bonus*** *if their friend is hired. In addition, under the referral program, the firm will provide* ***special consideration*** *in the hiring process to referred candidates. Do you think the firm having the employee referral program would make the employee feel more respected?*

In a representative survey of 436 US workers, 66% of workers said having an ERP would make the employee feel more respected, whereas only 11% said it would make the employee feel less respected, and 22% who said they were uncertain (see Appendix B.7 for details). Moreover, there is little relation between worker skill and workers' beliefs. This is consistent with the indirect benefits of an ERP being present in other contexts.

We look forward to future RCTs that can examine the ideas from our paper in other settings, both other low-skill settings and high-skilled settings. Further, there are other motivations to make or not make referrals beyond those discussed in our paper. For example, people may make referrals because it signals being a good corporate citizen, or may avoid making referrals if they think that the person referred will outshine them (Ekinci, 2016). We believe that it would be useful for future RCTs to explore such ideas as well.

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Figure 1: Referral Program Poster Used During RCT



Notes: This is a translated version of a poster used during the RCT, with the firm rename redacted.

Figure 2: Datasets Used in the Paper

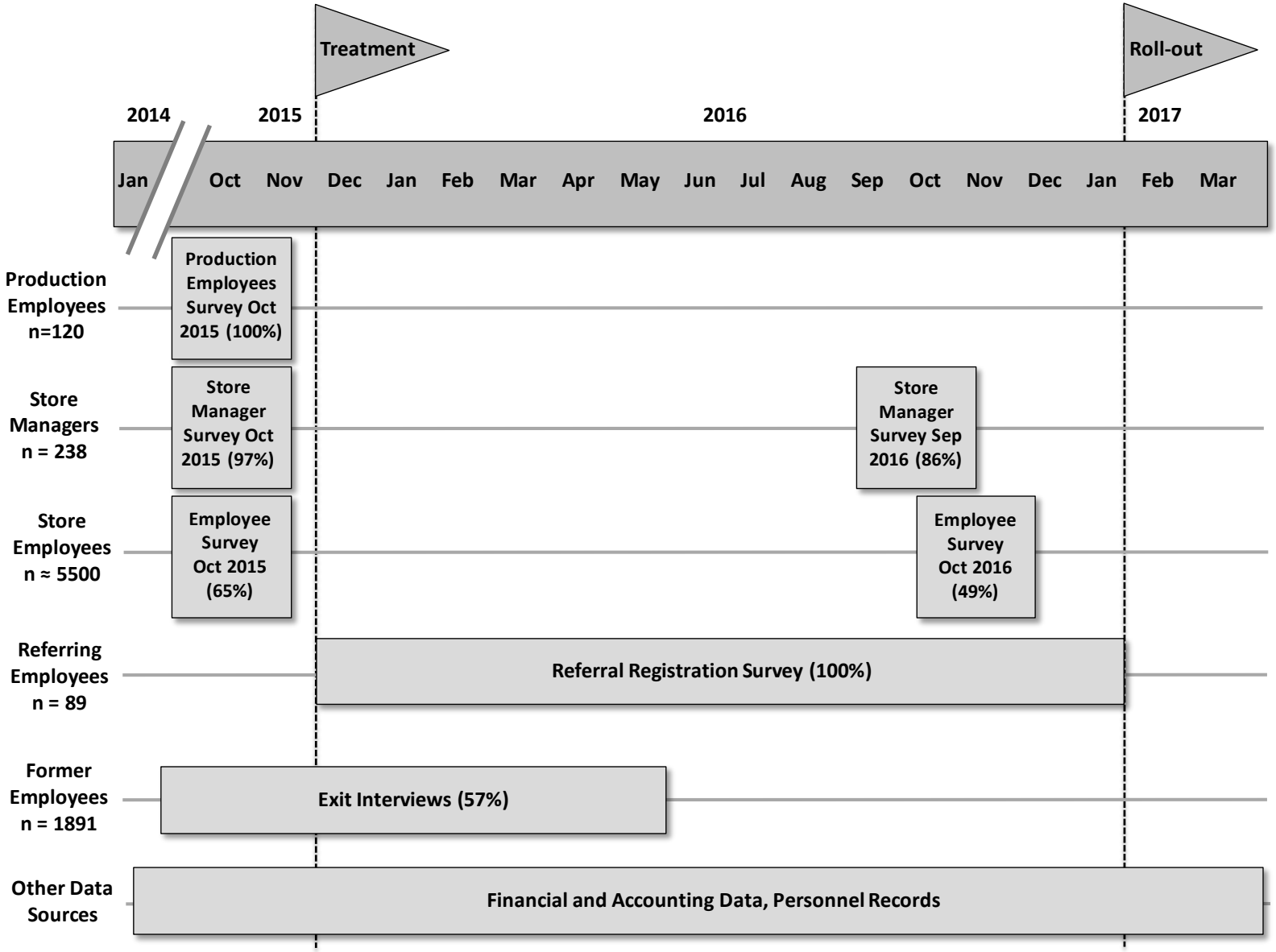
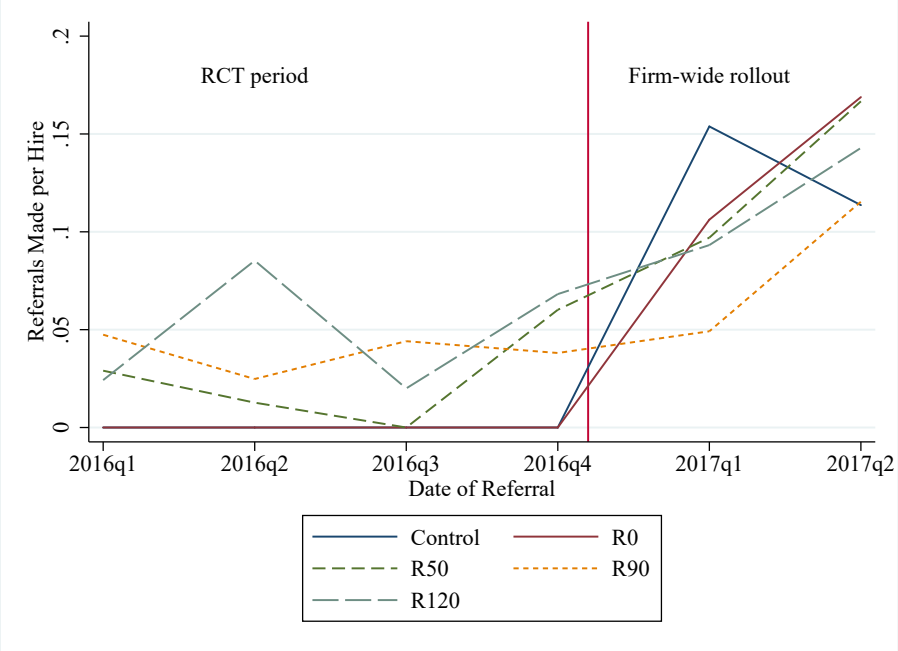
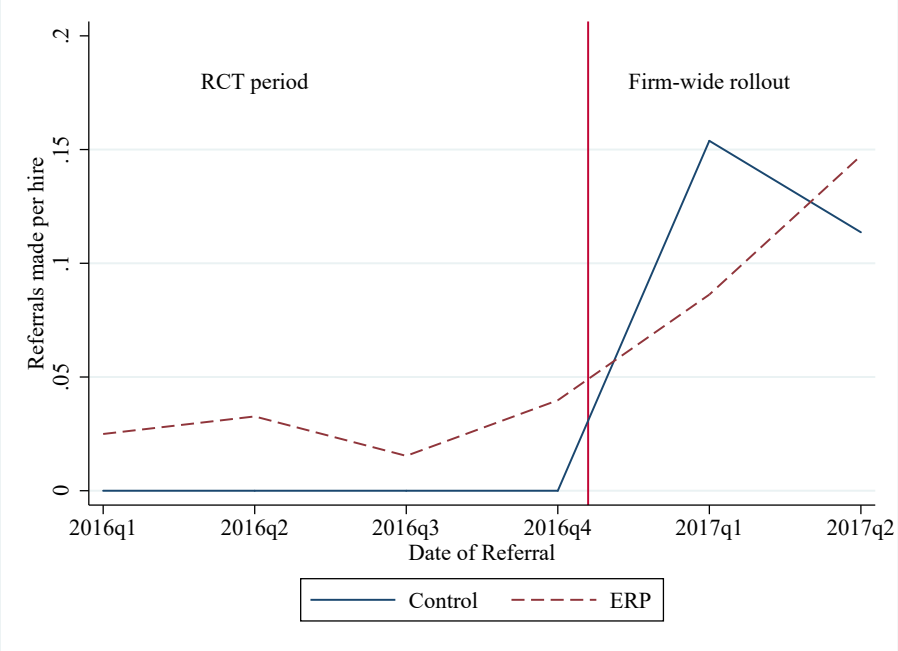


Figure 3: Referrals Made over Time in the RCT and Firm-wide Rollout



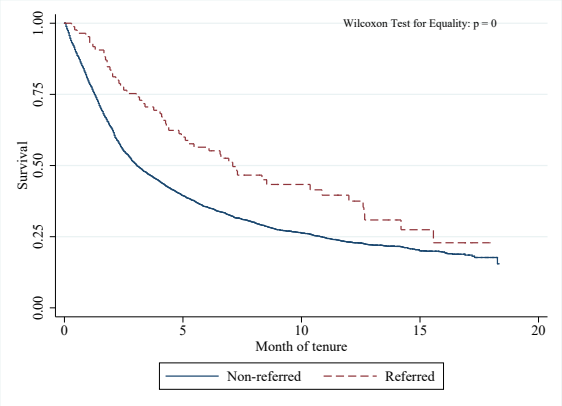
(a) Five Arms



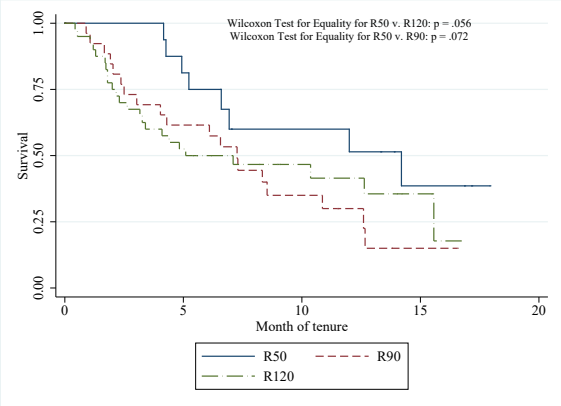
(b) ERP vs. Control

Notes: This figure shows referrals made divided by hires over time during the experiment across the 5 experimental arms. The vertical line is located in between 2016q4 and 2017q1, and separates the RCT period from the firm-wide rollout. Panel (a) shows the 5 arms and panel (b) shows control vs. ERP stores.

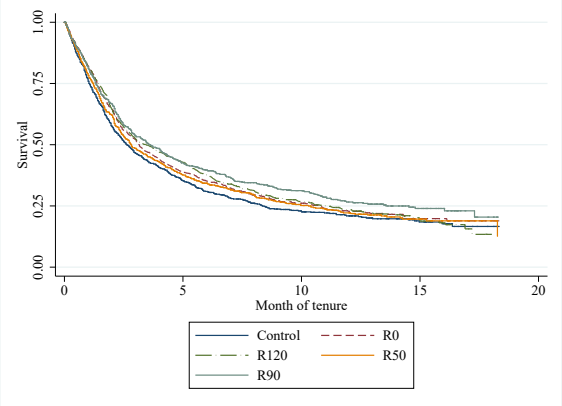
Figure 4: Survival Curve Comparisons



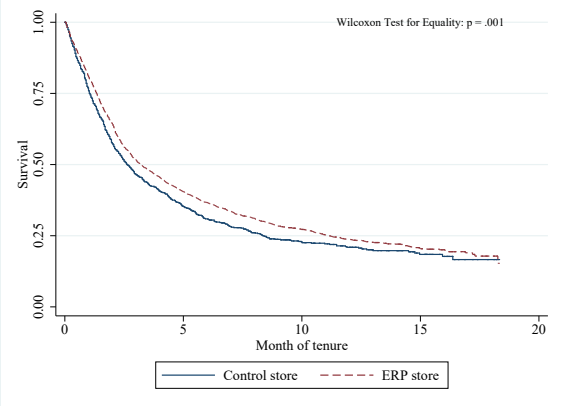
(a) Referrals vs. Non-referrals



(b) Referral Survival by Bonus Level



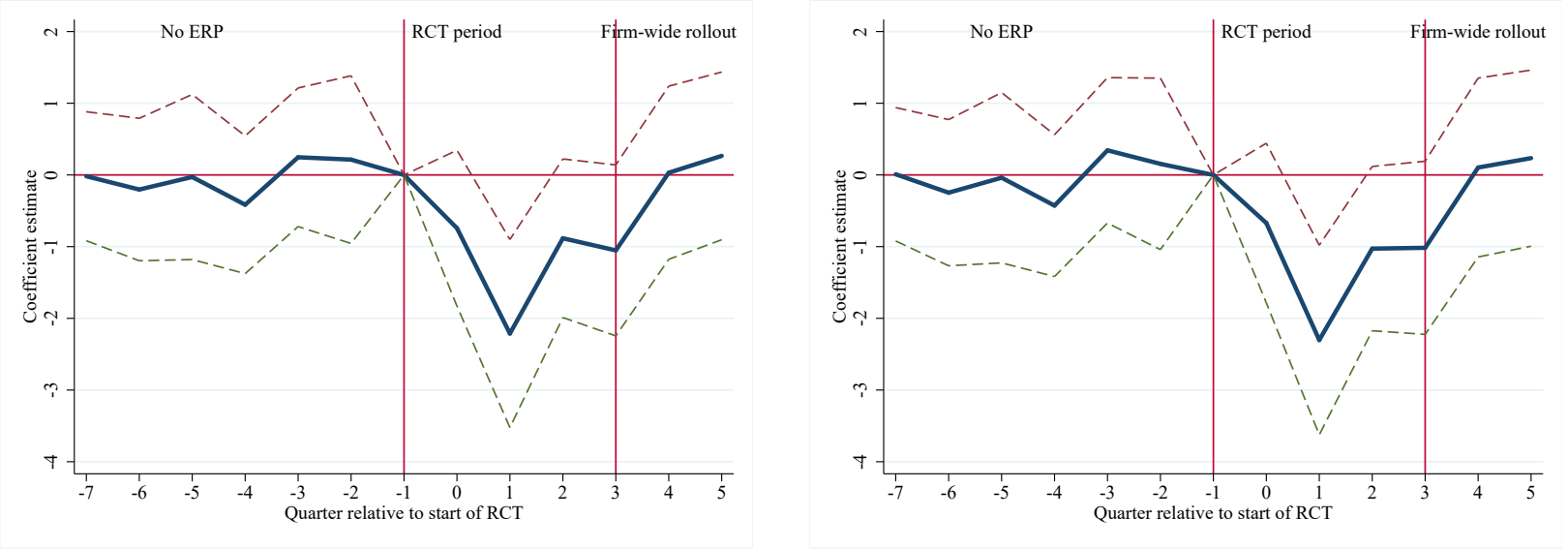
(c) Survival of Non-Referrals by Bonus Group



(d) Survival of Non-Referrals, by Ref Program Stores vs. Control

Notes: This figure presents different survival comparisons. Panel (a) compares referrals and non-referrals in terms of survival. Panel (b) analyzes the survival of referrals across the three positive bonus groups. Panel (c) analyzes the survival of non-referrals according to the five randomized treatments (Control, R0, R50, R90, R120). Panel (d) repeats panel (c) but splits according to whether there was a referral program, thereby grouping R0, R50, R90, and R120 together vs. Control. We restrict attention to workers hired during the RCT (December 2015-December 2016), but we follow them here through May 2017.

Figure 5: Event Studies: ERP Lowers Attrition during the RCT, and the Effect is Reversed Once ERP is Rolled out to Control Stores. ERP Effects are Similar in Stores with No Referrals During RCT.

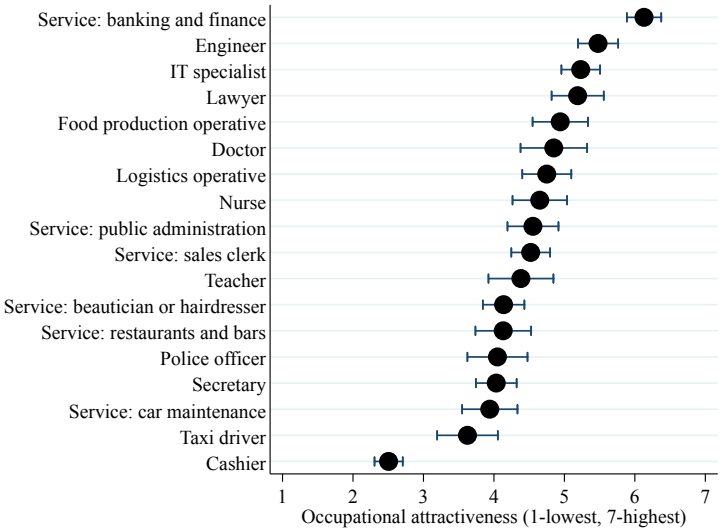


(a) Overall Results

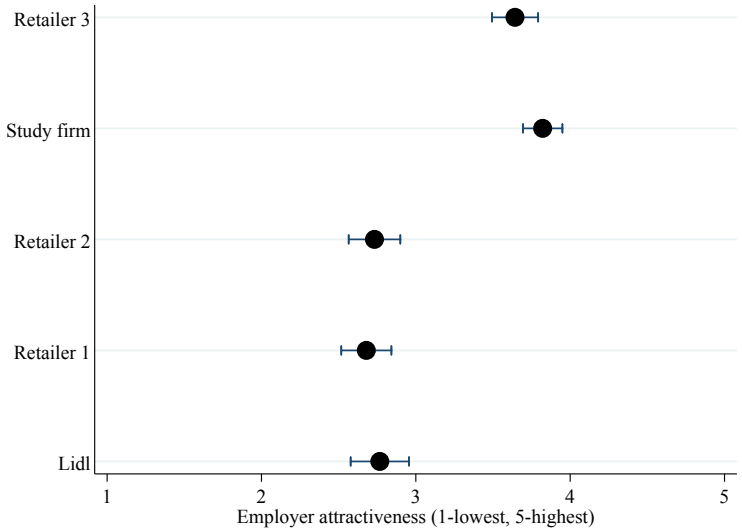
(b) Stores with No Referrals during RCT

Notes: The solid line denotes the coefficient estimate, with the dotted lines denoting the 90% confidence interval. Panel (a) analyzes the impact of the randomly assigned ERP treatments on attrition. The omitted category is the Control stores. The regression used in plotting the event study is similar to column 4 of Table 5. The difference is that we interact the ERP dummy with event time dummies indicating the current quarter relative to the start of the RCT. Quarter 0 refers to December 2015-February 2016. Quarter 3 covers 4 months, going from September 2016-December 2016, which we do since the RCT goes through December 2016. In addition, quarter 4 is January-March 2017 and quarter 5 is April-May 2017. The omitted category are the stores that were formerly treatment stores during the ERP. Panel (b) repeats Panel (a) while restricting attention to workers in stores where no referrals are ever made during the RCT.

Figure 6: Attractiveness of Occupations and Grocery Chains in our Firm's Host Country



(a) Occupations



(b) Grocery Chains

Notes: Dots are mean attractiveness ratings, and whiskers are 95% confidence intervals for the means. The sample is people from the general public of the country where our retailer is based. In panel (a), in measuring occupational attractiveness, we use an approach similar to that in the General Social Survey (Smith and Son, 2014). Specifically, to reduce survey time and ensure maximize time, respondents are asked about 6 occupations from our overall list of 18 occupations. In the survey in panel (b), each respondent is asked about all 5 grocery retailers (our study firm, 3 other local retailers, and the German chain Lidl).

Table 1: Comparing Pre-Treatment Store Means across the Treatment Groups ($N = 238$ stores): Randomization Check

Panel A: Across All 5 Arms	Head count	Net sales	Shrink-age rate	In big city	Lidl store nearby	Attri-tion rate	Quit rate	Fire rate	Share female	Age	Unemp rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Constant	25.233***	209.776***	0.029***	0.370***	0.239***	4.975***	5.346***	0.841***	0.889***	32.456***	7.852***
	(2.707)	(23.341)	(0.001)	(0.072)	(0.064)	(0.415)	(0.508)	(0.221)	(0.012)	(0.642)	(0.329)
R0	0.586	1.080	0.001	0.110	0.053	0.286	0.325	0.163	-0.011	0.254	-0.263
	(3.565)	(30.012)	(0.002)	(0.102)	(0.092)	(0.537)	(0.746)	(0.294)	(0.017)	(0.820)	(0.449)
R50	-0.956	-16.132	-0.000	0.068	0.032	0.312	-0.081	-0.052	0.006	0.459	-0.444
	(3.783)	(31.075)	(0.002)	(0.102)	(0.091)	(0.565)	(0.695)	(0.263)	(0.017)	(0.858)	(0.479)
R90	0.769	-2.218	0.000	0.089	-0.052	0.291	0.510	0.060	-0.005	-0.500	0.058
	(3.947)	(32.161)	(0.001)	(0.102)	(0.085)	(0.578)	(0.710)	(0.283)	(0.018)	(0.899)	(0.487)
R120	0.094	0.713	-0.001	-0.057	-0.052	0.266	-0.104	0.189	0.005	0.804	0.056
	(3.912)	(33.816)	(0.001)	(0.099)	(0.085)	(0.580)	(0.677)	(0.283)	(0.017)	(0.821)	(0.484)
Observations	238	238	238	238	238	238	238	238	238	238	238
F-test	0.0648	0.123	0.411	0.970	0.599	0.102	0.311	0.374	0.342	0.719	0.428
p-val	0.992	0.974	0.800	0.425	0.664	0.982	0.870	0.827	0.849	0.580	0.788
Panel B: ERP (All 4 Treatment Arms) vs. Control Arm											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
ERP	0.123	-4.139	0.000	0.052	-0.005	0.289	0.162	0.090	-0.002	0.254	-0.148
	(3.000)	(25.558)	(0.001)	(0.080)	(0.070)	(0.454)	(0.561)	(0.236)	(0.013)	(0.696)	(0.369)
Observations	238	238	238	238	238	238	238	238	238	238	238
F-test	0.00169	0.0262	0.00926	0.428	0.00459	0.404	0.0837	0.145	0.0136	0.134	0.161
p-val	0.967	0.871	0.923	0.513	0.946	0.526	0.773	0.703	0.907	0.715	0.689

Notes: Panel A compares stores across the different conditions in terms of store-level characteristics. Each column is an OLS regression on dummies for the four treatment arms. The estimated constant corresponds to the mean in the control group. There are 46 stores in the control group, and 48 stores in each of the 4 treatment groups. The F-test and p-value correspond to the test for joint significance of the treatment dummies. A similar regression-based balance test can be found in DellaVigna et al. (2016). Panel B lumps all treatment stores together and compares versus Control stores. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 2: Summary of the Treatments and Referrals Made during RCT

	Control ($N = 46$)	R0 ($N = 48$)	R50 ($N = 48$)	R90 ($N = 48$)	R120 ($N = 48$)
Panel A: Summary of the Five RCT Arms					
Information to encourage referrals (posters, letter, meeting)	No	Yes	Yes	Yes	Yes
Bonus paid to referrer after referral is hired	0	0	€15	€15	€15
Bonus paid to referrer if both referrer & referral stay 5 months	0	0	€35	€75	€105
Panel B: Total Hires, Referrals Made, and Referrals Hired					
Number of Hires	763	747	750	708	842
Number of Referrals Made	0	0	18	28	42
Number of Referral Hires	3	0	16	26	40
Referrals as Share of Hires	.004	0	.021	.037	.048

Notes: This table compares means across treatment arms in the number of referrals made, as well as in the characteristics of referrals (as reported by the referrer). The first two columns of Panel B are blank because there were no referrals made in the Control or R0 conditions. During 2016, €1 was worth between about $1.04-$1.16 USD.

Table 3: Impact of ERPs on whether New Hires are Referred

DV = Hire is a Referral			
	(1)	(2)	(3)
R0	-0.004*	-0.000	
	(0.002)	(0.005)	
R50	0.017**	0.022**	
	(0.008)	(0.009)	
R90	0.033***	0.036***	
	(0.012)	(0.012)	
R120	0.044***	0.042***	
	(0.014)	(0.011)	
ERP			0.025***
			(0.006)
Observations	3,810	3,810	3,810
Controls	No	Yes	Yes

Notes: Standard errors clustered at the store level. An observation is a worker hired during the RCT. Controls are store-level controls (pre-RCT average monthly turnover rate, pre-RCT average monthly head count, pre-RCT average monthly sales, region dummies, square footage, whether the store is in a big town, and whether there is a Lidl store nearby), year-month of hire dummies, demographic controls (for age and gender), and a dummy for being a cashier. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Comparing Referrals vs. Non-referrals

Dep. var.:	Attrition (0-1) x 100			Monthly absences		
Method:	Linear Probability Model			Negative Binomial		
	(1)	(2)	(3)	(4)	(5)	(6)
Hire was referred	-6.98***			-0.19		
	(1.17)			(0.19)		
Referred X first 5m		-8.70***			-0.40*	
		(1.61)			(0.24)	
Referred X after 5m		-3.06*			0.21	
		(1.69)			(0.45)	
Referred X R50			-11.05***			0.08
			(1.62)			(0.36)
Referred X R90			-6.20***			-0.11
			(1.64)			(0.39)
Referred X R120			-6.08***			-0.39
			(1.99)			(0.24)
Observations	14,890	14,890	14,890	14,890	14,890	14,890
MDV if referred=0	15.93		15.93	1.361		1.361
Workers	3795	3795	3795	3795	3795	3795
MDV in first 5m if ref=0		17.73			1.151	
MDV after first 5m if ref=0		9.245			2.139	
F(R50 vs. R90)			0.03			0.72
F(R50 vs. R120)			0.05			0.28

Notes: Standard errors clustered at the store level. Columns 1-3 are linear probability models, where the dependent variable is whether an employee attrites in a month, with coefficients multiplied by 100 for readability. Columns 4-6 are negative binomial models, where the dependent variable is a worker's number of sick days in a month. An observation is a worker-month. The sample is grocery workers hired during the RCT. Controls are the same as in Table 3, plus current month-year dummies and a 5th order polynomial in tenure. Columns 2 and 5 also have a dummy for "after 5m of tenure." In column 3, the excluded category is non-referred, but we do not include a "Referred X R0" dummy because there were no referral hires in R0 stores. We also do not include "Referred X Control" because there are only 3 hires in Control stores—however, the other coefficients are similar if "Referred X Control" is included. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: The Impact of the ERPs on Attrition (Linear Probability Models)

Type of workers:	All	All	All	All	Hires	Hires	Inc	Inc
Sample period:	RCT	RCT	Pre &RCT	Pre &RCT	RCT	RCT	RCT	RCT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R0	-1.01**				-0.95		-1.01***	
	(0.39)				(1.14)		(0.34)	
R50	-0.45				-1.42		-0.35	
	(0.44)				(1.32)		(0.39)	
R90	-1.53***				-2.95**		-1.09***	
	(0.38)				(1.22)		(0.34)	
R120	-0.77*				-2.01		-0.46	
	(0.42)				(1.23)		(0.36)	
ERP		-0.95***				-1.82*		-0.74***
		(0.34)				(1.03)		(0.28)
R0 X RCT			-0.96**					
			(0.47)					
R50 X RCT			-0.98*					
			(0.50)					
R90 X RCT			-1.79***					
			(0.41)					
R120 X RCT			-1.09**					
			(0.45)					
ERP X RCT				-1.21***				
				(0.38)				
Store FE	No	No	Yes	Yes	No	No	No	No
MDV if ERP=0	6.677	6.677	5.420	5.420	17.26	17.26	4.353	4.353
Observations	74,206	74,206	204,517	204,517	14,890	14,890	55,960	55,960
Workers	10,013	10,013	16,942	16,942	3,795	3,795	5,881	5,881

Notes: Standard errors clustered at the store level. All columns are linear probability models, where the dependent variable is whether an employee attrites in a month, with coefficients multiplied by 100 for readability. An observation is a worker-month. Controls are the same as in column 1 of Table 3 except we do not use age controls because age is missing for workers who are hired before the start of the data and who do not attrite, and this missingness is highly correlated with attrition. Columns 3 and 4 add store fixed effects and exclude pre-RCT store-level controls. In terms of type of workers, "All" refers to all workers working at the firm from Feb 2014-Dec 2016, "Hires" refers to people hired during the RCT, and "Inc" refers to incumbents, i.e., individuals who were working with the firm at the time the RCT began. The RCT period is December 2015-December 2016. Data from the firm-wide ERP rollout (2017m1-2017m5) are not used here. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Mediation Analysis for Impact of ERPs on Attrition

Dep. Var.:	Referred (0-1)	Refs made to date	Attrition (0-1) x 100					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ERP	0.007***	0.016***	-0.95***	-0.90***	-0.86**			
	(0.002)	(0.003)	(0.34)	(0.34)	(0.34)			
Hire was referred				-6.37***	-6.34***		-6.35***	-6.33***
				(1.36)	(1.35)		(1.38)	(1.37)
Refs made to date					-2.36***			-2.36***
					(0.73)			(0.76)
R0						-1.01**	-1.00**	-1.00**
						(0.39)	(0.39)	(0.39)
R50						-0.45	-0.40	-0.37
						(0.44)	(0.44)	(0.44)
R90						-1.53***	-1.46***	-1.41***
						(0.38)	(0.38)	(0.38)
R120						-0.77*	-0.71*	-0.65
						(0.42)	(0.42)	(0.42)
Observations	74,206	74,206	74,206	74,206	74,206	74,206	74,206	74,206
MDV if ERP=0	4.8e-4	0	6.677	6.677	6.677	6.677	6.677	6.677
Workers	10,013	10,013	10,013	10,013	10,013	10,013	10,013	10,013

Notes: Standard errors clustered at the store level. All columns show OLS models. The controls are the same as in Table 5. The sample is workers at the firm during the RCT. "Refs made to date" means a person's running sum of referrals made to date during the RCT. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Why did ERPs Reduce Turnover Separate from Generating More Referrals?

Reason	Managers N=222	Workers N=113
"Employees felt more respected after being asked to be involved in the hiring process or liked having some say about who they might work with"	66%	50%
"Because FIRM NAME started the referral program, it made employees think that FIRM NAME was a better place to work."	23%	14%
"Employees didn't have a person to recommend, but they hoped to recommend a friend in the future."	13%	28%
"Employees referred their friends, but they did not tell FIRM NAME about it (and they did not get a bonus). The employees or their friends were more likely to stay at FIRM NAME."	3%	5%
"None of these reasons are important or likely. What is your explanation?"	10%	4%

Notes: This is from post-RCT surveys of workers and managers. Managers did the survey by phone and could select more than one option (hence, the options don't add up to 100%). Workers did the survey via an electronic kiosk at work and could only select one option. The manager response rate was 93%, with 222 of 238 managers responding.

Table 8: Profits from the ERPs

Panel A: Overall Profits from an ERP vs. Control		
Turnover cost number:	€250	€1,150
Justification:	Admin cost	"Full cost"
Total savings in turnover costs	2.37	10.89
Contribution to savings from:		
Referrals hired during RCT	0.13	0.58
Non-referral hires during RCT	0.79	3.62
Pre-RCT incumbents	1.45	6.69
Costs of the ERP	0.11	0.11
Profit per worker-month	2.26	10.78
Profit as share labor costs	0.6%	2.7%

Panel B: Profit by Particular ERP (turnover cost = €250)				
	R0	R50	R90	R120
Total savings in turnover costs	2.52	1.12	3.81	1.94
Contribution to savings from:				
Referrals hired during RCT	0	0.20	0.16	0.18
Non-referral hires during RCT	0.47	0.48	1.27	0.95
Pre-RCT incumbents	2.05	0.44	2.39	0.81
Costs of the ERP	0	0.06	0.13	0.24
Profit per worker-month	2.52	1.06	3.69	1.69

Notes: This table reports profit calculations using the method outlined in Section 7. Panel A reports the profit gains from having an ERP vs. Control, pooling all the ERP treatments together. Panel B reports the profit gains from having one of the particular ERPs compared to Control. All numbers are in euros per worker-month. The difference between the "administrative costs" and "full costs" of turnover is that the full costs also account for the fact that higher turnover tend to have lower profitability, as explained in Appendix B.4. See Appendix B.5 for further details on the profit calculation.

Table 9: Referrals Made as a Share of Total Hires by Job and by Period

	RCT	Post-RCT roll-out
All Grocery Jobs	2%	12%
Cashier	2%	9%
Grocery Non-cashier	5%	30%
Non-Grocery Jobs		35%

Notes: This table shows the number of referrals made as a share of total hires by job and period. For example, if there were a job-period where employees made 3 referrals and for which 10 new workers were hired, the number shown would be 30%. Post-RCT period is Jan-May 2017. During the post-RCT period, there are 1,133 hires in grocery jobs and about 500 hires in non-grocery jobs.

Table 10: Manager and employee surveys: why did the ERPs generate only a few referrals? General popular survey: why fewer referrals from cashiers than from logistics and food production workers?

Panel A: Managers & General Population

Reason	All managers (N=156)	All managers except those giving reasons 8, 9, 11 (N=118)	General population (N=68)
Undesirable job	48%	68%	74%
No friends to refer	10%	13%	
Didn't want to refer someone who could embarrass	12%	13%	
People were unaware of referral system	9%	10%	
No trust that firm will pay the money	6%	7%	
Referral process was burdensome	5%	5%	
Bonus too low; referral might not stay	4%	4%	
No open jobs in the store	6%		
Referral system worked in her store	11%		
Other reasons	11%	10%	3%
No reasons mentioned	8%		22%

Panel B: Employee Survey (N=342)	Rank 1	Rank 2	Rank 3	Rank 4	Rank 5
"Many people perceive working conditions in supermarkets as not very attractive (e.g. low salary, high workload)"	51%	29%	13%	5%	3%
"Employees' friends already have jobs"	23%	32%	30%	6%	10%
"Employees don't want to want to be responsible if their friend doesn't do a good job"	16%	23%	36%	17%	8%
"Employees were not informed by the company about the opportunity to refer a friend/did not know how the referral program worked"	4%	12%	14%	50%	19%
"The amount of money that employees could get for a bonus was too low"	7%	6%	6%	21%	59%

Notes: This Table is based on the surveys of store managers, store employees, and general population. *Store managers* in the referral program stores were presented with the findings of the RCT and asked for their opinion why the referral program had produced only a few referrals. Their answers, in free text, were classified into Reasons 1 to 11 by specially hired decoders. The randomly selected members of *general population* were contacted after the roll-out and asked "why were there fewer referrals from cashiers than from the logistics and food production workers?". Their answers were coded similarly to managers. Randomly selected *cashiers* were asked the same question as store managers, except that they had to choose from a fixed set of possible reasons.

Table 11: Referrals, ERPs, and Demographics (Age and Gender)

Panel A: Age	(1)	(2)	(3)	(4)	(5)
Dep. Var.:	Age	Age	Age	Attrition (0-1) x 100	Monthly absences
Hire was referred	-2.76*			-6.78***	-0.21
	(1.48)			(1.35)	(0.32)
Young (less than 25)				6.62***	-0.14
				(0.65)	(0.09)
Young x referred				0.00	-0.04
				(1.97)	(0.50)
R0		0.79			
		(0.84)			
R50		-0.47			
		(0.76)			
R90		0.37			
		(0.72)			
R120		0.75			
		(0.80)			
ERP			0.37		
			(0.60)		
Observations	3,787	3,787	3,787	14,890	14,890
Mean dep. var.	31.35	31.35	31.35	15.71	1.362
Panel B: Gender					
Dep. Var.:	Female	Female	Female	Attrition (0-1) x 100	Monthly absences
Hire was referred	0.097			-12.39***	0.71**
	(0.075)			(1.88)	(0.30)
Female				-5.72***	0.44***
				(0.81)	(0.10)
Female X referred				6.83***	-1.28***
				(2.37)	(0.40)
R0		0.032			
		(0.029)			
R50		0.035			
		(0.025)			
R90		0.045			
		(0.030)			
R120		0.042			
		(0.033)			
ERP			0.039*		
			(0.022)		
Observations	3,809	3,809	3,809	14,890	14,890
Mean dep. var.	0.718	0.718	0.718	15.71	1.362

Notes: An observation is a worker in columns 1-3, and a worker-month in columns 4-5. Columns 1-4 present OLS regressions, whereas column 5 presents negative binomial regressions. Standard errors clustered at the store level. In Panel A here, the controls in columns 1-3 are the same as the controls in Table 3 except we do not use age controls. In columns 4-5 of Panel A here, the additional controls are the same as in Table 4. In Panel B here, the controls in columns 1-3 are the same as the controls in Table 3 except we do not control for gender. In columns 4-5 of Panel B here, the additional controls are the same as in Table 4, plus age controls. * significant at 10%; ** significant at 5%; *** significant at 1%

Online Appendix for "What Do Employee Referral Programs Do?"

Guido Friebel, Matthias Heinz, Mitchell Hoffman, and Nick Zubanov

This Online Appendix consists of several parts. Appendix A accompanies the model from Section 3. Appendix B provides additional discussion and results. For each subsection, we give the relevant section of the main paper that it accompanies. Appendix C is the Data Appendix. Appendix D contains additional figures and tables. Appendix E provides additional materials used by the firm in the ERPs.

Appendix A Theory Appendix

A.1 Solving the Model

Prediction 1. *Higher referral bonuses will increase referrals.*

Given the firm launches an ERP program with the bonus value $\widetilde{b}$, the employee utility functions will be as follows:

$$U^I(R = 1) = (1-\sigma)b + \sigma(m+q) + B(\sigma) \tag{7}$$
$$U^I(R = 0) = B(\sigma) = B(\sigma) \tag{8}$$

Thus, the probability, r, that the employee will refer their friend is equal to:

$$r = Pr(U^I(R=1) > U^I(R=0)) = Pr((1-\sigma)b + \sigma(m+q) > 0) = 1 - F(m^*),$$

where $m^* = -\frac{1-\sigma}{\sigma}b - q$. To analyze how bonuses affect the share of referral made, we have:

$$\frac{\partial r}{\partial b} = f(m^*) \cdot \frac{1-\sigma}{\sigma}$$

which is positive.

Prediction 2. *Referrals will be of higher quality than non-referrals. However, as referral bonuses increase, the quality of referrals decreases.*

The average match quality of a referred worker is equal to $H^r \equiv E[m|m > m^*]$, whereas the average match quality of a non-referred worker is $E[m]$. Thus, $H^r \geq E[m]$ for any m^* in support of $F(\cdot)$. Because $\frac{\partial m^*}{\partial b} = -\frac{1-\sigma}{\sigma} < 0$, we have $\frac{\partial H^r}{\partial b} < 0$. Intuitively, as b increases, E is willing to refer someone who is less suitable for the job, and average referral quality decreases.

Prediction 3. *Having an ERP increases retention. This should occur even in store where no referrals are made.*

We separately consider the retention of incumbent and new workers. As a result of having an ERP, the incumbent worker believes the firm believes that $\Sigma = \sigma$. Thus, they become more likely to stay. This occurs even in stores where no referrals are made because the mechanism involves respect, not referrals. Specifically, the probability of an incumbent worker staying is $G(B(\hat{\Sigma}))$, which is increasing in $\hat{\Sigma}$. Turning to the new worker, no referrals occur without an ERP, and an ERP generates positive referrals because m is continuous. Thus, since referrals are of higher than non-referrals (Proposition 2), having an ERP increases retention among the new worker. Since workers are either an incumbent or a new worker, overall retention increases.

Prediction 4. *As long as the referral bonus is not too large, having an ERP increases firm profits. The relationship between referral bonuses and firm profits from hiring referrals (vs. hiring non-referrals) is ambiguous.*

We begin with proving the second sentence first. In the Prediction 3, we have shown that an ERP increases retention, thus it has positive indirect effect on the firm's profit. The direct effect is positive, $H^r - \widetilde{b} > E[m]$, as long as the referral bonus, $\widetilde{b}$ is sufficiently small. To analyze how the size of the referral bonus affects profits from referrals we have:

$$\frac{\partial \pi}{\partial \widetilde{b}} = \frac{\partial r}{\partial \widetilde{b}} \left(H^r - \widetilde{b} - E[m] \right) + r \left(\frac{\partial H^r}{\partial \widetilde{b}} - 1 \right), \tag{9}$$

where the first term is positive (provided $\widetilde{b}$ is relatively small), and the second term is negative.

Now consider the overall impact of an ERP on firm profits. That is, compare $r\big(E[m|m > m^*] - \widetilde{b}\big) + (1-r)E[m] - c(1-G(\sigma))$ with $E(m) - c(1-G(0))$. Here, $c(1-G(\sigma)) < c(1-G(0))$ and $r\big(E[m|m > m^*] - \widetilde{b}\big) + (1-r)E[m] > E[m]$ provided that $\widetilde{b}$ is sufficiently small. Therefore, having an ERP increases firm profits.

Prediction 5. *More referrals will be made for attractive jobs than for less attractive jobs. Suppose that $f'(m^*) < 0$, which occurs if referrals are few. Then, the more attractive the job, the more responsive are referrals to bonuses.*

For the first part, to analyze the relevance of job attractiveness for the decision to refer, note that $\frac{\partial r}{\partial q} = f(m^*)$, which is positive because people value their friends and to refer them for better jobs. To see how job quality affects the responsiveness of referrals to bonuses, note that:

$$\frac{\partial^2 r}{\partial b \partial q} = -f'(m^*) \frac{1-\sigma}{\sigma} \tag{10}$$

Thus, if $f'(m^*) < 0$, then $sgn\big(\frac{\partial^2 r}{\partial b \partial q}\big) = -sgn(f') = +$. This seems likely to hold if only a minority of workers make referrals[1].

[1] E.g., if m has a normal (or log-normal) distribution, if the quality cutoff m^* is above the argmax of f, then $f' < 0$

A.2 Discussion of Model Assumptions

The model simplifies many aspects of reality. This subsection discusses our model assumptions.

The referral bonus is paid upon hire. In reality, the referral bonus is only paid partially upon hire, with most of the bonus paid only if the referrer and referral stay five months. If this encourages both parties to stay, this will only further accentuate the prediction that referrals stay longer, as well as that incumbent workers stay longer under ERPs. The model also is static, whereas reality is dynamic. Thus, m should be interpreted as outcomes over time at the firm instead of outcomes at one time. Thus, referral and non-referral hires also become incumbents capable of making referrals, so our predictions on the retention of incumbents actually cover the retention of all workers.

The incumbent has social preferences toward their friend, not toward the firm. We assume that the incumbent worker only has potential social preferences toward their friend, not toward the firm. If the worker had potential social preferences toward the firm, all predictions of the mode would be the same. The key feature of the model is that having an ERP involves delegating the hiring decision to the incumbent worker, and doing so is only valuable if the worker cares about the match quality of a referred worker. The incumbent worker may do so because they care about their friend (and the firm also happens to benefit from higher match quality) or because they directly care about the firm. In our model, the firm also has zero outside information outside of potential referrals.[2] Also, while we assume that the friend and firm equally benefit from match quality for simplicity, this assumption is not required.

The level of the referral bonus and respect. We assume that a worker's true social preferences can only take two values, and we do not analyze the worker updating their sense of respect in reflection of the particular value of $\widetilde{b}$. If worker social preferences can take many values, then choosing higher values of $\widetilde{b}$ could communicate that the firm has a particularly high belief about the value of altruism for a worker. On the other hand, outside our model, choosing a very high value of $\widetilde{b}$ could communicate other messages, such as that making referrals is an unpleasant task Benabou and Tirole (2003). Thus, because of these competing effects, we set this aspect aside. One can also examine empirically whether larger referral bonuses tend to have larger impacts on incumbent workers. Conditional on having a referral bonus, we do not observe a clear relation between the level of the bonus and incumbent retention effect.

Worker's perception of firm belief updating. The incumbent worker believes that the firm initially believes that the worker has $\Sigma = 0$ for sure. After seeing the ERP, the incumbent worker recognizes that the firm would not have the ERP unless the firm recognized that $\Sigma = \sigma$. Such belief updating is not consistent with Bayes' Rule, since a Bayesian will never update if they believe that the initial value of some event occurring is 0. This assumption is made entirely for simplicity of the model. One could alternatively

[2]Because of this, the decision to fully delegate hiring to incumbent workers via referrals is a prediction of the model, not an assumption.

assume that the worker believes that the firm believes that the worker has $\Sigma = \sigma$ with a 50% probability, and that seeing the ERP leads the worker to update to believe that the firm believes that $\Sigma = \sigma$ for sure.

Appendix B Additional Discussion and Results

B.1 World Management Survey on Retail (Accompanying Section 1)

The World Management Survey (WMS) of Bloom et al. (2014b) has traditionally focused on manufacturing firms. However, in 2009, the WMS surveyed 661 retail establishments, and information on ERP status is available for 537 of these establishments. As in other WMS surveys, interviews were conducted by phone using open-ended questions using safeguards to preserve firm confidentiality (see Bloom et al. (2014b) for further details). Of the 537 establishments, 352 are in Canada, 126 are in US, and 59 are in UK. Unlike the other WMS surveys, which do not ask about referral programs, the survey enumerators explicitly asked managers about whether the establishment had a referral program. In the data, the share of establishments having an ERP is 25% in Canada, 15% in the US, and 32% in UK. In many cases, respondents explicitly mentioned a bonus, but we currently do not have any data on whether the referral program reflected bonuses.

One reason why the WMS rate of referrals is lower than that in CareerBuilder is that the question is asked at the establishment level. A retail firm may decide that it has an employee referral program in general, but some local managers may choose not to apply it.

B.2 Additional Discussion on Referral Bonus Levels (Section 2)

We compare the referral bonuses in the RCT to those paid in other studies. The main other existing paper on experimental referral bonuses, Beaman and Magruder (2012), used referral bonuses of between 60 and 100 rupees, with some component based off of the performance of the referral. Beaman and Magruder (2012) report that 135 rupees is about $3, and that the average daily wage is about 110 rupees. Thus, they used referral bonuses of half to one day's worth of wage. These are lower than the nominal amounts paid and even a bit lower when converted into expected value terms taking into account that some people will quit.[3] In a very different context, Brown et al. (2016) report a modal referral bonus of $1,000 (median of $2,000), which is about 1% of annual salary at the firm (or about 12% of the monthly salary), which is similar to our bonuses in expected value, and lower than our bonuses when paid in nominal terms.[4] Our nominal bonuses are similar in percentage terms to the bonuses at the trucking firm in Burks et al. (2015), where drivers got $1,000 (or about one third of monthly salary) for referring an experienced driver, though there was also a 6-month tenure requirement.

[3] For example, our R90 treatment is €37.5 in expected value, which is about 11% of the monthly salary.

[4] The bonuses in Brown et al. (2016) also require people to stay 6 months.

B.3 Details of the Surveys Used in the Paper (Section 2)

We ran the following surveys:

1. *Pre-RCT Store Employee Survey*, *Pre-RCT Store Manager Survey*: In October-November 2015, just before the start of the RCT, we conducted paper survey of grocery workers, including store and regional managers whose responses were put in a separate file. The response rate was 61% for the employees and close to 100% for the managers. The groups of questions we asked concerned social connections within and beyond workplace, and their attitudes toward the firm, their managers or supervisors, and their job.

2. *During RCT Store Manager Survey*: In September-October 2016, we conducted phone surveys of store managers recording their time use and opinions as to the reasons our referrals program brought only a few referrals. The response rate was 92%.

3. *During RCT Store Employee Survey*: In October-December 2016, we conducted phone surveys of cashiers. For each store, we randomly called two cashiers, one with an above- and the other below-median tenure. All participated. We asked the same broad questions as in the manager survey about why referrals were few. In addition, we asked some (but not all) of the questions from the October-November 2015 employee survey.

4. *Post-RCT Store Manager Survey*: In summer 2018, we conducted phone surveys of store and regional managers regarding the mechanism for the observed indirect impact of ERPs on attrition.

In all surveys, subjects were told truthfully that we were conducting an international retail survey in partnership with a local university in the country. In addition, subjects were told truthfully that their employer would not see individual-level responses to the survey. Both phone surveys were conducted by native speakers recruited from a local university. In the initial paper survey, employees deposited responses into a box that was then picked up by an outside logistics company to ensure employees understood that the firm would have no access to individual survey responses.

Beyond these main surveys, we also regularly conducted exit interviews of employees when they left the firm, which were not about referrals. Finally, we also carried out a survey among 270 randomly picked citizens of the country to learn more about the reputation of the cashier job and different firms in the market *General Population Survey 1*, *General Population Survey 2*.

Notice that the above surveys, except that of general population, supported two other projects; therefore, we had to be parsimonious in the choice of questions relevant for this study.

B.4 Calculation of the costs of turnover (Section 7)

We consider the following costs of turnover:

1. Store-level recruiting and training costs (e.g., manager time)

2. Firm-level costs of employing HR personnel to deal with turnover
3. Lost profits from higher turnover

(1) and (2) can be estimated from administrative data, whereas (3) requires a regression analysis of the link between employee turnover and operational profits, subject to exogeneity assumptions. We base our calculations on the following numbers (using pre-RCT data): average number of quits of 1.2 per store per month, the average cashier salary of €345 per month, the average store manager salary of €940 per month, and the average operational profit (sales minus costs of sales minus wages minus overheads minus shrinkage) of €38,065 per store per month.

1. Store-level costs. We learned from several randomly selected store managers we interviewed in the Spring 2015 that it takes about one hour to interview each applicant, which given the hires per applicant rate of 0.4 means 2.5 hours per hire. It takes store manager two hours to instruct each new hire, half an hour to process the paperwork of each leaver, and another half an hour to rewrite the work schedule. Each newly hired worker undergoes a two-day on-the-job training during which he or she is paid but does not work. We assume the costs of training to be the store manager day's salary. A mentor (another cashier) spends two hours with each newly hired worker. Summing up, each quit takes 40 hours of cashier time and 18 hours of store manager time.

2. Firm-level costs for HR personnel. The head of HR informed us that there were 23 employees in the HR office whose job was to administer hiring and quits. In our calculations, we assume that their wages are 10% higher than the average wage of cashiers, giving the monthly wage budget of €8,730. On top of this figure comes the tax wedge of 38.8%, which is the Eurostat 2015 average tax wedge estimate for the eastern EU countries. Additionally, there are rental costs of the office space required to sit them, estimated at €10 per m^2 per month, which is in the range of office space rates in non-premium locations in the city where our study firm is based. Assuming every employee needs 5 m^2, these costs amount to €1,150 per month.

3. Lost operational profits. We observe a negative correlation between changes in the quite rate and changes in store operational profit. Regressing changes in log operational profit on changes in labor input and up to three lags of changes in the quit rate gives the coefficients -0.18, -0.14, -0.17 on the current, first and second lags of changes in the quit rate. (The coefficients on the deeper lags are small and insignificant.) For reasons of endogeneity (less productive stores may experience more quits), these results cannot be interpreted as causal evidence on the effect of quits; but a naïve estimate stemming from the regression coefficients would suggest a 4.9% decrease in operational profit (=0.1*(-0.18-0.14-0.17)) in response to a permanent increase in the quit rate by 10pp. If the present quit rate went from the pre-treatment average 0.06 down to zero, the operational profits would increase by 38,065*0.49*0.06≈ €1,120 per store per month on average, suggesting that each quit costs €930 in terms of lost profits.

Summing (1) and (2) amounts to €292 per store per month, or €244 per exit, which we round to €250. Summing (1), (2), and (3) yields €1,174 per quit, which we round to €1,150. This figure corresponds to about three months' worth of cashier gross salary, and is consistent with the estimates in Blatter et al. (2012) and Boushey and Glynn (2012).

Note that our estimates do not include the costs of uniform and placing job adverts, nor the costs turnover has on the firm's reputation and talent pool, and are therefore somewhat conservative.

B.5 Profit Calculation Details (Section 7)

Absences. We do not account for absence in profit calculation, as there is no impact of ERP on absence. In addition, the overall absence difference between referrals and non-referrals is not statistically significant. If one incorporates absence into benefit of hiring a referral, then referrals will have 0.38 fewer absences per month. This will slightly accentuate the referral gains, but will have no impact on the conclusions.

Profit gain from incumbents. For the profit calculation, profit gain from incumbents is defined as total savings in turnover costs minus the contribution to turnover savings from referral minus the contribution to turnover savings from non-referral RCT hires.

Pr(both). To calculate $Pr(both)$, or the probability that both referrer and referral stay 5 months, we simply count up the number of instances where both parties divided by the number of referrals. Our data extend 5 months after the RCT, so we are able to see 5 months later for all referrals made during the RCT. We use a single number for $Pr(both)$ as opposed to lettering it vary by referral bonus group.

B.6 Further Discussion on the Reasons for Few Referrals Other than Job Attractiveness (Section 8)

Did workers not have friends looking for jobs? If employees do not have friends to refer, then an ERP may have little impact on referrals. However, we believe that this explanation is unlikely to explain our results for three reasons. First, during 2016, the unemployment rate was roughly 8% (and much higher for youth who make up a sizable share of the firm's workforce), so there was a significant share of people who were unemployed. Second, in both the manager and worker surveys (*During RCT Store Manager Survey*, *During RCT Store Employee Survey*) listed in Table 10, not having friends to refer received much less support than grocery jobs being undesirable as an explanation for the result. For example, while 48% of managers mentioned grocery jobs being undesirable as an explanation, only 10% mentioned employees not having friends to refer. Third, the firm has operations throughout the country where it is located, in both urban and rural areas. Even if someone had moved or had social contacts living elsewhere in the country, those social contacts could have found a job at a local facility.

Was the referral process difficult? Store employees could have perceived it as burdensome to call the HR department to register a referral. We do not think this is a strong explanation because the process was designed to be very brief (just a few questions about how someone knows their referral). The store have relatively low opportunity cost of time, given that they are willing to work for just over €2 per hour. Given the possibility of

earning €135 in one treatment arm, it seems unlikely that a short phone call would be of sufficient cost to dissuade someone from making a referral.[5]

Was the expected value of the bonus too low? Given the five-month tenure requirement for a referral to be paid, would this make the expected value of the bonus too low? In our data, the chance that both referral and referrer stay for five months after the referral is hired is about 45%. This means that the bonus is worth $15 + .45 * 50$=€37.5 in the R50 treatment, €55.5 in R90, and €69 in R120. Relative to a post-tax monthly wage of roughly €300 for cashiers, this still is a sizable bonus (about 13-23% of monthly salary). Though our judgement of what is a "sizable bonus" may be subjective, the literature on incentives shows strong effects of bonuses of this magnitude.

After the RCT, the new referral bonus paid €30 at hire, plus €100 after 3 months. Since about 60% of referrals during the RCT would have lead to payments, the expected value of the new bonus was €90. This is even larger than R120, and also provides the money sooner.

B.7 Survey of US Workers

The survey of US workers was carried out using an online survey. The survey responses were:

- It is very unlikely to make the worker feel more respected.
- It is unlikely to make the worker feel more respected.
- It is somewhat unlikely to make the worker feel more respected.
- It is uncertain whether it will make the worker feel more respected.
- It is somewhat likely to make the worker feel more respected.
- It is unlikely to make the worker feel more respected.
- It is very likely to make the worker feel more respected.

Appendix C Data Appendix

Referrals Data. Beyond the 88 referrals reported in Section 4, there is also one additional referral made that we cannot match to other records. Store managers were not eligible to participate in the ERP, as they general authority over hiring decisions. Our analyses on overall attrition impacts of ERPs include store managers, but results are similar if store managers are excluded.

Age. Although we found a significant difference between referrals and non-referrals in age, we do not control for age in our main regressions. Age information (specifically, date

[5]Of course, if people are highly present-biased (e.g., Madrian and Shea, 2001), this could help explain why they are not willing to make referrals. We cannot rule this out, but it seems unlikely in our case.

of birth) is only available at the time of hire or attrition (and is not available in monthly payroll records), causing age to be missing for workers joining the firm before 2014 who never attrite. This causes age to be missing in a fashion that is correlated with attrition (i.e., people with missing age are more likely to never attrite). However, results are robust if we control for age while restrict attention to individuals hired during our data (who thus consistently have age data).

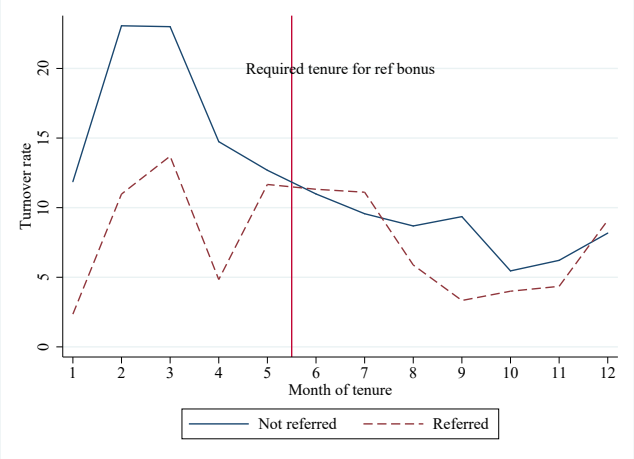
Gender. For workers where gender is missing, we impute missing gender based on name by using gender-specific endings that exist in the language of the country where our firm is based.

Race. The personnel data from the firm do no contain variables for race or nationality. However, racial/ethnic heterogeneity is very limited in the country we study, so we do not think that this would bias our results in any way.

Attrition Codes. In our dataset, employees receive up to 4 attrition codes, which are assigned by the store manager. We classify someone as being terminated for cause if any of the 4 codes indicate a termination for cause. Exit codes are missing for many workers exiting before 2015m4. In contrast, starting in 2015m4 and after, exit codes are missing for less than 4% of terminations. Thus, we restrict our analysis of quits and fires to 2015m4 and after.

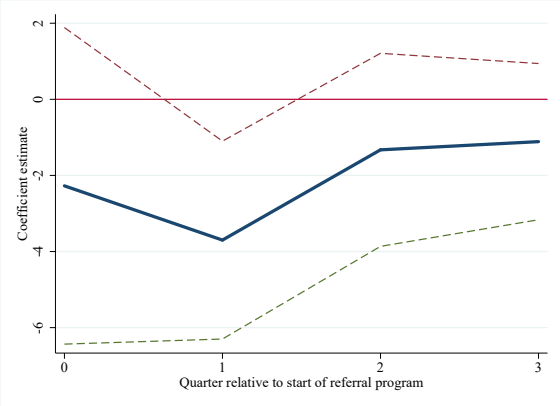
Appendix D Additional Figures and Tables

Figure D1: Attrition Hazard for Referrals vs. Non-referrals

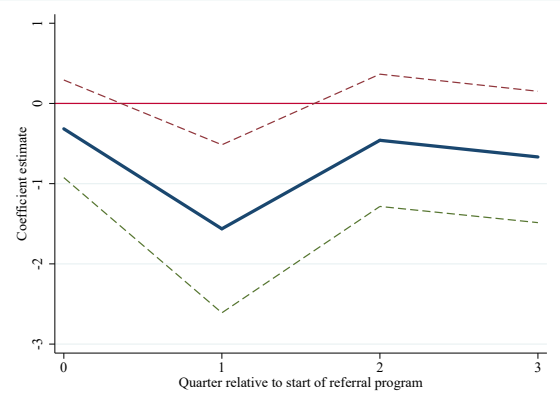


Notes: This figure shows the monthly quiz hazard as a function of worker tenure comparing referred vs. non-referred workers. The sample is workers hired during the RCT. The referral and referrer must stay 5 months after the referral is hired in order for the referrer to be paid. The vertical tenure threshold line is drawn in between x=5 and x=6 because both referral and referrer must stay at least 5 months.

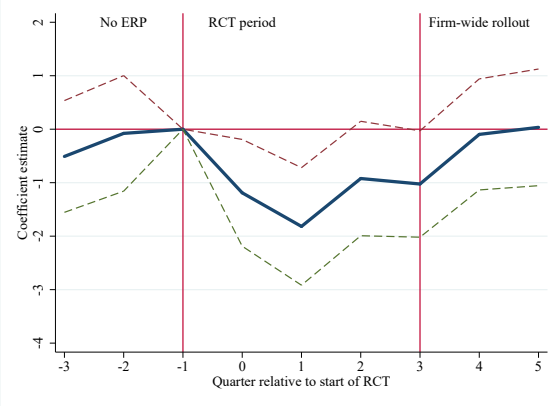
Figure D2: Event Studies on Impact of ERPs: Additional Subsamples and Outcomes



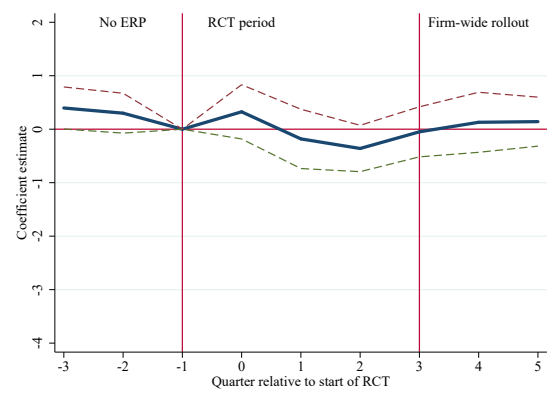
(a) Sample is New Hires During RCT



(b) Sample is Incumbents During RCT



(c) Outcome is Voluntary Exits ("Quits")



(d) Outcome is Involuntary Exits ("Fires")

Notes: These figures are similar to the main event study in panel (a) of Figure 5. The difference is they analyze different samples or look at different individual outcomes (other than overall attrition). Panel (a) analyzes workers hired during the RCT, whereas panel (b) analyzes workers who were incumbents at the firm when the RCT began (i.e., they had been hired in the past). For both panel (a) and panel (b), it is not possible for the event study to go before the RCT because RCT hires and RCT incumbents do not attrite prior to the start of the RCT. Panel (c) analyzes voluntary attrition as the outcome variable, whereas panel (d) analyzes involuntary attrition. In panels (c) and (d), there are only 3 quarters graphed before the RCT because information on exit codes only begins in 2015m4.

Table D1: Robustness Check on Comparing Referrals vs. Non-referrals: Further Controls

Dep. var.:	Attrition (0-1) x 100			Monthly absences		
Method:	Linear Probability Model			Negative Binomial		
	(1)	(2)	(3)	(4)	(5)	(6)
Hire was referred	-7.54***			-0.11		
	(1.23)			(0.29)		
Referred X first 5m		-9.11***			-0.35	
		(1.58)			(0.34)	
Referred X after 5m		-3.86**			0.36	
		(1.96)			(0.63)	
Referred X R50			-11.87***			0.54
			(1.92)			(0.58)
Referred X R90			-6.55***			-0.18
			(2.21)			(0.69)
Referred X R120			-7.17***			-0.32
			(1.85)			(0.36)
Observations	14,890	14,890	14,890	14,890	14,890	14,890
Workers	3795	3795	3795	3795	3795	3795
MDV if referred=0	15.93		15.93	1.361		1.361
MDV in first 5m if ref=0		17.73			1.151	
MDV after first 5m if ref=0		9.245			2.139	
F(R50 vs. R90)			0.07			0.44
F(R50 vs. R120)			0.08			0.20

Notes: This table is similar to Table 4. The difference is we additionally control for store fixed effects. Because we control for store fixed effects, we no longer control for pre-RCT means of store-level variables. * significant at 10%; ** significant at 5%; *** significant at 1%

Table D2: The Impact of the ERPs on Monthly Absence

Type of workers:	All	All	All	All	Hires	Hires	Inc	Inc
Sample period:	RCT	RCT	Pre &RCT	Pre &RCT	RCT	RCT	RCT	RCT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R0	-0.04				-0.02		-0.06	
	(0.09)				(0.17)		(0.11)	
R50	-0.03				-0.19		-0.00	
	(0.11)				(0.17)		(0.12)	
R90	0.03				-0.06		0.05	
	(0.10)				(0.17)		(0.11)	
R120	0.04				0.00		0.02	
	(0.10)				(0.16)		(0.11)	
ERP		0.00				-0.06		0.00
		(0.09)				(0.14)		(0.09)
R0 X RCT			-0.19*					
			(0.10)					
R50 X RCT			-0.02					
			(0.11)					
R90 X RCT			0.06					
			(0.10)					
R120 X RCT			-0.17					
			(0.11)					
ERP X RCT				-0.08				
				(0.08)				
Store FE	No	No	Yes	Yes	No	No	No	No
Observations	74,206	74,206	204,517	204,517	14,890	14,890	55,960	55,960
MDV if ERP=0	1.451	1.451	1.284	1.284	1.328	1.328	1.492	1.492
Workers	10013	10013	16942	16942	3795	3795	5881	5881

Notes: This table is similar to Table 5 except the outcome is monthly absences and the specifications are negative binomial instead of OLS. * significant at 10%; ** significant at 5%; *** significant at 1%

Table D3: Impact of having an ERP on Store-level Outcomes

Dep. var.:	Log hours	Log shrinkage rate	Log sales per worker	Log operational profits per worker
	(1)	(2)	(3)	(4)
ERP	-0.013	-0.026	0.019	0.020
	(0.016)	(0.024)	(0.015)	(0.021)
Observations	3,017	2,993	2,993	2,989
MDV if ERP=0	7.886	-3.793	9.109	7.530

Notes: Standard errors clustered at the store level. An observation is a store-month. All columns include standard pre-RCT store means, plus the pre-RCT store-level mean of the dependent variable if it is not included among our standard controls. The shrinkage rate is shrinkage divided by net sales. Operational profits per worker are store-level net sales minus cost of goods minus wages minus overheads * significant at 10%; ** significant at 5%; *** significant at 1%

Table D4: The Impact of the ERPs on Attrition: Restrict to Stores with No Referrals Made during the RCT

Type of workers:	All	All	All	All	Hires	Hires	Inc	Inc
Sample period:	RCT	RCT	Pre &RCT	Pre &RCT	RCT	RCT	RCT	RCT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R0	-1.02***				-1.13		-1.02***	
	(0.39)				(1.12)		(0.34)	
R50	-0.53				-1.21		-0.45	
	(0.50)				(1.54)		(0.43)	
R90	-1.55***				-3.16**		-1.11***	
	(0.42)				(1.39)		(0.37)	
R120	-0.98**				-3.36**		-0.46	
	(0.48)				(1.39)		(0.41)	
ERP		-1.01***				-2.00*		-0.79***
		(0.35)				(1.05)		(0.29)
R0 X RCT			-0.96**					
			(0.47)					
R50 X RCT			-1.15**					
			(0.56)					
R90 X RCT			-1.82***					
			(0.43)					
R120 X RCT			-1.10**					
			(0.48)					
ERP X RCT				-1.23***				
				(0.39)				
Store FE	No	No	Yes	Yes	No	No	No	No
Observations	59,692	59,692	165,433	165,433	11,547	11,547	45,494	45,494
MDV if ERP=0	6.677	6.677	5.420	5.420	17.26	17.26	4.353	4.353
Workers	8041	8041	13725	13725	2963	2963	4808	4808

Notes: This table is similar to Table 5 except we restrict attention to workers in stores where no referrals are ever made during the RCT. * significant at 10%; ** significant at 5%; *** significant at 1%

Appendix E Documents Used in the Experiment

Table D5: Demographic Homophily

	(1) Age	(2) Female
Age of referrer	0.39***	
	(0.14)	
Referrer is female		0.38***
		(0.14)
Observations	61	84
Mean dep. var.	28.00	0.774

Notes: There are fewer observations in column 1 because referrers are missing age if they were hired before the start of the data and do not attrite during the data. We control for month of hire and whether someone is a cashier.

Figure E3: Poster Used during 2017 Referral Program Roll-out: Food Production



Notes: This is a translated version of the poster used for food production workers.

Figure E4: Poster Used during 2017 Referral Program Roll-out: Grocery Workers



Notes: This is a translated version of the poster used for grocery workers.

Figure E5: Poster Used during 2017 Referral Program Roll-out: Logistics Workers



Notes: This is a translated version of the poster used for logistics workers.

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