What Do Employee Referral Programs Do?*

Guido Friebel[†] Matthias Heinz[‡] Mitchell Hoffman[§] Nick Zubanov[¶]

July 2018

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.

JEL Classifications: J24, M51, J30, J63
Keywords: Referrals; employee referral programs; hiring; turnover; productivity

PRELIMINARY AND INCOMPLETE.

*We thank Iwan Barankay, Nick Bloom, Roberto Fernandez, Matt Gentzkow, Matt Jackson, Eddie Lazear, Liz Lyons, Stephane Saussier, Kathryn Shaw, John Van Reenen, and numerous seminar/conference participants for helpful comments. We thank Muhammad Azim for excellent research assistance. We are grateful to the World Management Survey (WMS) and especially Daniela Scur for sharing their data with us, which we use to report on the prevalence of referral programs. Financial support from the Michael Lee-Chin Family Institute and SSHRC is gratefully acknowledged. We also thank the Stanford Institute for Economic Policy Research for its hospitality.

[†]Goethe University of Frankfurt and IZA and CEPR
[‡]University of Cologne and CEPR
[§]University of Toronto Rotman School of Management and NBER
[¶]University of Konstanz and IZA

1 Introduction

Firms must find workers to fill their jobs, and workers must find firms that employ them. One of the most common way by which workers get hired is via employee referrals.[1] A growing literature examines whether firms benefit from hiring referrals (relative to non-referrals), finding that referrals are generally quite beneficial to firms, with lower turnover, lower hiring 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). Given these results, what should firms do (if anything) to increase referrals from their employees?

A common management practice to encourage referrals is an employee referral program ("ERP"), where worker are explicitly encouraged to refer social contacts for jobs, often using bonuses.[2] However, it is far from obvious that having an ERP would boost referrals or benefit a firm. Bonuses could be ineffective if the decision to refer is primarily driven by non-pecuniary motivations, and potentially counterproductive if bonuses crowd out intrinsic motivations to refer (Deci et al., 1999; Benabou and Tirole, 2003, 2006). Moreover, bonuses are not free and could encourage referrals of low-quality candidates. What do ERPs do? Are there important non-financial factors that affect whether people make referrals or affect the efficacy of ERPs? These questions are hard to answer, not only because we lack evidence, but also because little is known about why people make referrals. If groups like women (Beaman et al., 2018) or minorities (Holzer, 1987; Smith, 2005) are under-referred for jobs, for policy to address this, it is critical to better understand the factors affecting whether employees make referrals.

We examine these questions using a randomized control trial (RCT) on over 10,000 workers in a large grocery chain in Eastern Europe. As described in Section 2, stores were

[1] About 25-40% of European jobs are find 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 a retail module of the World Management Survey (WMS) (Bloom et al., 2014b) covering Canada, US, and UK, 23% of establishments have an ERP (see Appendix A.1). We do not know from CareerBuilder or WMS if ERPs in surveyed firms pay bonuses, but our impression is that most ERPs do, as seen in observational studies of referrals in firms (Brown et al., 2016; Burks et al., 2015; Fernandez et al., 2000).

randomly assigned to Control (no ERP); an information-only intervention that encouraged referrals using meetings, letters, and posters; or one of three arms that additionally paid bonuses of varying amounts to referrers. In these three arms, referrers received €15 post-tax upon a referral getting hired, plus €50, €90, or €120 post-tax if the referrer and referral stayed 5 months. These numbers were chosen based on surveys of other low-skilled workers in the company outside those studied in the RCT, so incentives were expected to be significant. We took great care to ensure that the ERPs were clear and salient to workers.

Though our conclusions are specific to the firm we study, similar contexts exist in other large firms employing low-skilled workers, and the firm suits our research question well for three reasons. First, the firm faces substantial worker turnover (over half of hires leave within 5 months), so stores are constantly looking for new workers. This ensures a large sample of hires, and helps rule out the concern that employees may not make referrals because stores are not hiring. Second, grocery cashier jobs have minimal qualifications, so everyone's friends could reasonably be hired, helping rule out the concern that employees may not make referrals because their friends are not qualified. Finally, the low monthly wages most store employees earn ($\approx$ €350) made it easier for the firm to try out bonuses that were large relative to wages.

To develop testable implications, we develop a simple model of the decision to refer in Section 3. Beyond bonuses or effort costs from making a referral, employees have "as if" social preferences toward people they know and could refer. These preferences could reflect reputational concerns, altruism, or enjoyment from working with a friend who is better matched with their job. The model yields five predictions regarding the impact of bonuses on referral quantity, referral quality, and firm profits, as well as on how perceived job quality affects referral quantity and the efficacy of referral bonuses.

Consistent with the model, Section 4 shows that higher referral bonuses increase referrals. However, while statistically significant, the magnitude of the impacts is economically small. Even under our largest referral bonus (about 45% of a worker's monthly post-tax earnings), referrals comprised only 5% of hires. Information that the firm welcomed referrals, without a

bonus, had no impact on increasing referrals.

Section 5 examines worker outcomes during the RCT. While the number of referrals made was small, referral quality is high: referrals have over 40% lower attrition than observably similar non-referrals, and are 20% less likely to be absent, though the absence difference is only statistically significant during the first 5 months of tenure. Consistent with the model, as referral bonuses increase, the relative retention benefit of referrals falls.

In our main result, having an ERP in a store leads to a roughly 15% reduction in worker attrition, with similar percentage reductions among workers hired during the RCT (including non-referred workers) and among incumbents working at the firm prior to the RCT. The effects we find cannot be attributed to the incidence of referrals or to potential 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, we believe the effects may occur from workers feeling greater voice or involvement in hiring.

Section 6 shows that having an ERP is highly profitable, reducing labor costs by at least 0.6-2.7% (depending on how turnover costs are calculated), which is a substantial reduction for a competitive, low-margin industry like grocery retail. This savings reflects in part that referrals have higher retention than non-referrals ("direct benefits"), but primarily that ERPs have the indirect benefit of boosting retention of non-referred workers ("indirect benefits"). 95% of profit gains are due to indirect benefits. Direct benefits are non-monotonic in the bonus level, consistent with the model-predicted quantity-quality tradeoff.

Section 7 presents data from the post-RCT ERP rollout, and from surveys of store managers, store workers, and everyday citizens. Given the benefits of the randomized ERPs, the firm rolled out an ERP to all jobs. While bonuses in the RCT were already sizable, the firm increased bonuses to €30 upon hire and €100 after 3 months, consistent with the RCT ERPs being valuable. This is useful as it helps rule out any lingering concerns about bonuses being too small. For grocery jobs, the ratio of referrals made to hires rose to 12%, which is an increase above the RCT, but still a modest one. In contrast, for non-grocery

jobs, the ratio was much higher at over one-third. In surveys, workers and managers believe that ERPs had little impact on referrals for grocery jobs because grocery jobs are perceived as unattractive.[3] Consistent with the model, the larger post-RCT referral bonus increased referrals more for non-cashier grocery jobs (e.g., butchers, bakers) than cashier jobs. Attrition differences between treatment and control stores vanish in the rollout. Continuing 18 months to the present day (July 2018), the firm continues to use the new ERP.

Section 8 discusses our results in regard to age and gender. Referrals are 2.8 years younger than non-referrals, and are 10pp more likely to be female. Having an ERP increases the share of female hires by 4pp. While young workers quit at much higher rates than older workers, young referrals have similar attrition to older non-referrals. Though women are more likely to be absent than men, female referrals have similar absence rates to male non-referrals. Section 9 concludes.

Our paper makes four contributions. First, to our knowledge, we provide the first field experimental evidence of the impact of ERPs on referral-making in a for-profit firm.[4] Despite relatively strong financial incentives and low time costs for making referrals, ERPs have little impact on increasing formal referrals in our initial setting of grocery jobs.

Second, we provide the first analysis of how ERPs affect worker outcomes and firm profits. Prior work summarized by Hoffman (2017) compares average worker outcomes and/or average profit differences between referrals and non-referrals, but lacks exogenous variation in ERPs. Such variation is important, first, as whether a worker is referred is not a policy choice for firms, and, second, our ERPs yields large benefits separate from increasing formal referrals. To our knowledge, we are the first academic paper in any field to consider that ERPs may have broader organizational impacts separate from their impact on referrers and referrals.[5] However, there is also a tradeoff from larger bonuses, as we show for the first time

[3] That is, workers want to avoid referring friends to jobs perceived as unattractive. The surveys also help rule out competing explanations and indicate that it is grocery jobs that are unattractive, not the firm itself.

[4] Beaman and Magruder (2012) and Beaman et al. (2018) randomly vary incentives to make referrals in a lab-experiment-in-the-field in India and for NGO survey enumerators in Malawi. Neither paper studies a for-profit firm (and they also differ in research question and other dimensions, as discussed more below).

[5] While we could not find prior academic work, that ERPs may have important indirect organizational benefits seems well-known to practitioners, as discussed later in Section 5.3.3. Somewhat related, Shinnar

that the relative advantage of referral hires decreases as referral bonuses increase.

Third, we provide novel evidence on how perceived job attractiveness affects referrals. We provide the first evidence, to our knowledge, that people are more likely to make referrals for jobs perceived as more attractive (namely, non-cashier instead of cashier jobs), and that referral bonuses are more effective for more attractive jobs.[6]

Fourth, we present a simple model where utility from making referrals is driven by both money and altruism, and show that all five of its predictions 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.[7]

Within work on employee referrals, there are a few RCTs, but they are focused primarily on different questions than ours. To study why referrals may achieve better outcomes than non-referrals, Pallais and Sands (2016) conduct an RCT in an online labor market. Deliberating hiring all applicants and using several treatments (e.g., whether referrer and referral are paired together), 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).

In Beaman and Magruder (2012), subjects in urban India were recruited to do games in an economic experiment laboratory, 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.[8] 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

et al. (2004) provide evidence that engaging in word-of-mouth communication can deepen organizational commitment.

[6]We caveat this contribution by noting that, unlike the other contributions, it is based on a policy change and surveys instead of randomized variation.

[7]This is true across the main classes of existing 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 provides a model of competing referrers, and Ekinci (2016), who presents a career concerns model, where a person's choice of whom to refer provides information about their underlying ability.

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

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.[9] Beaman et al. (2018) provide evidence that referrals disadvantage female survey enumerators in a Malawi NGO. They 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.[10]

In psychology, there is some work on ERPs, but it uses different methods (often lab experiments) and seems to focus on different questions.[11] In sociology, there is extensive work on referrals, but very little on ERPs. While not on ERPs, Smith (2005) argues that within-firm reputational concerns about making "bad referrals" make disadvantaged 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.

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.

[9] 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).

[10] Kim and Fernandez (2017) show in a survey vignette experiment that people prefer referring jobs to strong ties over weak ties. Abel et al. (2016) use RCTs in South Africa to show that randomly including a reference letter from a former employer increases the chance an applicant receives a call-back. This paper differs in its focus by studying past employer references as opposed to referrals from current employees.

[11] For example, Stockman et al. (2017) analyze whether students in a lab experiment may be less interested in a hypothetical organization if they know that referrals are being financially rewarded vs. not.

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

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 40% 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 A.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 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

[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 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).

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 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 €50, €90, or €120 if the referrer and referral stayed 5 months, and this was clearly explained in the letter and posters. Workers hired during the RCT 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

[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.

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. The bonus amounts were designed to be both reasonable and substantial. In the R120 arm, the combined bonus of €135 is 45% of a cashier's monthly take-home salary, which is comparable to or higher than referral bonuses used in other studies (see Appendix A.2 for details).

RCT timing. RCT materials (posters, letters, and instructions for the store managers) 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 the personalized letters and posters, and by having regional managers ensure that stores are in compliance. In addition, 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.

[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.

Second, workers need to believe the 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 A.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

[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 January 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).

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 referral bonuses affect referrals, the average quality of referral hires, and firm profits. We also investigate how job quality / attractiveness affects referrals and the efficacy of referral bonuses.

Set-up. We analyze the choice of an employee, E, whether to make a referral. Making a referral brings a monetary reward $\widetilde{b} \geq 0$ and requires a cost of effort c; so the net personal reward from making a referral is $b \equiv \widetilde{b} - c$. The employee also places some social weight $0 < \sigma < 1$ on the utility of the person they refer. We are agnostic as to whether this weight reflects reputational considerations or altruism. Letting $U^E(R = 1)$ and $U^E(R = 1)$ be utility from making or not making a referral, respectively, we have:

$$U^E(R = 1) = (1 - \sigma)b + \sigma U^W(R = 1) \tag{1}$$

$$U^E(R = 0) = \sigma U^W(R = 0) = 0 \tag{2}$$

In (2), we normalize E's outside option (i.e., their utility if they are not referred) to 0.

The utility the potential referral receives from the job is $U^W(R = 1) = m + q$, where m is the person's match with the job and q is the overall quality/attractiveness of the job. The match reflects that a particular job suits some people better than others (e.g., some people would enjoy being a cashier and interacting with customers, whereas others would not), and we assume $m \sim F(m)$. The attractiveness of the job, q, may depend not only on the wage but also on its non-pecuniary aspects, such as working conditions and reputation in society.

Analysis. Letting r be the share of employees who make a referral, we have:

$$\begin{aligned} r &= \Pr\left((1-\sigma)b + \sigma m + \sigma q > 0\right) \\ &= 1 - F(m^*) \end{aligned}$$

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

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

which is positive. Next, we analyze how bonuses affect the average quality of referral hires, $H^r \equiv E(m|m > m^*)$. Because $\frac{\partial m^*}{\partial b} < 0$, 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.

Next, 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. Also, to see how job quality affects the responsiveness of referrals to bonuses, note that:

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

Thus, if $f'(m^*) < 0$, then $sgn\left(\frac{\partial^2 r}{\partial b \partial q}\right) = -sgn\left(f'\right) = +$. This seems likely to hold if only a minority of workers make referrals.[21] Intuitively, if a job has very low q and referrals are rarely made, then E is unlikely to be marginal, and increased bonuses may do little to push E to make a referral. However, for a higher quality job, E is more likely to be marginal.

Last, we analyze firm profits from referral bonuses. On one hand, larger bonuses increase referrals, who may be valuable relative to non-referrals. On the other hand, larger bonuses lower average referral quality, and also cost money for the firm. Our model does not consider hiring of non-referred workers. Because the comparative static of profits with respect to referral bonuses only depends on the value of referrals relative to non-referrals, we normalize profits from non-referral hires to 0. Assuming a unit measure of potential referral hires, profits are $\pi = r(H^r - \widetilde{b})$ and we have:

$$\frac{\partial \pi}{\partial \widetilde{b}} = \frac{\partial r}{\partial \widetilde{b}}\left(H^r - \widetilde{b}\right) + r\left(\frac{\partial H^r}{\partial \widetilde{b}} - 1\right) \tag{3}$$

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

In (3), the first term is positive, and the second term is negative. We have the following predictions (P1-P5):

1. Higher referral bonuses will increase referrals.
2. As referral bonuses increase, the quality of referrals decreases.
3. The relationship between referral bonuses and firm profits from hiring referrals (vs. hiring non-referrals) is ambiguous.
4. More referrals will be made for attractive jobs than for less attractive jobs.
5. Suppose that $f'(m^*) < 0$, which occurs if referrals are few. The more attractive the job, the more responsive are referrals to bonuses.

Discussion. The model is very simple. It does not model non-referral hiring, or consider indirect effects of referral bonuses onto referrers or other workers. Indirect effects may be important for profits from ERPs, and we will investigate them, together with the predictions derived from the model in the next section. P1-P3 are tested using the RCT. P4 and P5 are tested using survey evidence and the firm-wide ERP rollout.

The biggest indirect effects from the RCT occur in employee turnover. While we do not provide a formal model for E's turnover decision, voluntary turnover can be regarded as comparing the value function of working at the firm with the outside option. The value function depends on usual earnings, ERP bonuses, and non-financial amenities of working at the firm, such as feeling that one has some say in the hiring process. We discuss such amenities further in discussing mechanisms for our ERP effects in Section 5.3.3.

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.[22] The results are similar with controls in column 2. Instead of using dummies for the four 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 8.

5 ERPs and Worker Outcomes (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.[23] Second, worker retention is a standard measure of match quality (Jovanovic, 1979). Third, past work

[22] 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.

[23] 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.

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.[24] Section 5.1 compares referrals vs. non-referrals. Section 5.2 analyzes the impact of ERPs. Section 5.3 discusses mechanisms for our results.

5.1 Referrals vs. Non-referrals

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 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.[25]

[24] 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.

[25] 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

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.[26] 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 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.[27] Appendix Table C1 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.

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 C1 shows that referrals are less likely to attrite than non-referrals at most tenure levels.

[26] 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).

[27] 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.

5.2 Impact of ERPs

Attrition. Table 5 shows that ERPs reduce attrition of all workers, with similar percentage effects on new hires and incumbents.[28] 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 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%.[29]

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

[28] 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.

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

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 C2 show ERP impacts interacted with quarter of the RCT for new hires and incumbents.

Panels (c) and (d) of Appendix Figure C2 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. Is this implausibly large? We believe not for two reasons. First, compared to variables like TFP and output per hour, attrition seems easier to change. For example, Bloom et al. (2014a) show that randomly assigning employees to work from home reduces attrition by half in Chinese call centers. Second, 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 large drops in turnover (Friebel et al., 2018). Hence, our setting may be one where turnover is relatively malleable.

Absences and store-level outcomes. Appendix Table C2 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 C3 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.[30]

5.3 Mechanisms for ERP Impacts

The most natural reason why 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 (Imai et al., 2010a,b) and analyses restricting to stores with no referrals, we show that promoting referrals likely explains only a modest share of the impact of ERP. We next discuss additional mechanisms that, while plausible *ex ante*, seem *ex post* inconsistent with the evidence. We then discuss mechanisms that are consistent with the evidence.

5.3.1 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) and Romero et al. (2018), consider the following system:

$$M_{it} = \alpha_0 + \alpha_1 ERP_i + X_{it}\delta_2 + u_{it} \quad (4)$$

$$y_{it} = \beta_0 + \beta_1 ERP_i + \gamma M_{it} + X_{it}\delta_2 + v_{it} \quad (5)$$

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:

[30] 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 6. While the estimates Appendix Table C3 are reasonably precise, we appear to lack the statistical power to detect small changes in these variables.

Assumption 1

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

$$y_{it}(e,m) \perp\!\!\!\perp M_{it}(e) |\ X_{it} \tag{7}$$

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 6 of Assumption 1 will hold because of random assignment. While Equation 7 (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 C4 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.

5.3.2 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.[31]

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. Identifying informal referrals is difficult, but we used store employee surveys to learn about them. Among other questions, we asked store employees how they had found their job; those who stated "via friends already working in [our study firm]" were labelled "informal referrals" . The share of the thus defined informal referrals—about a quarter in the workforce—is not boosted by any of the ERPs.

5.3.3 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 mechanisms can explain this? We believe it should be a mechanism or

[31] 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.

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. Three possible mechanisms consistent with this evidence are (1) workers value being more involved in hiring, (2) ERP is a positive signal about the firm being a better place to work, and (3) workers think they make referrals in the future.

To shed further light on these explanations, we conducted surveys with 8 randomly selected store and regional managers. We presented managers with explanations (1), (2), and (3) above, plus the explanation that workers made referrals, but did not inform the firm. As seen in Appendix Table C5, 7 of 8 managers selected (1) as the most likely explanation. We are currently conducting additional surveys.

Employees value being involved in hiring. Hiring is a central activity in many firms, and practitioners often argue that involving workers in hiring increases their engagement (DeLong and Vijayaraghavan, 2002).[32] Involving workers in hiring can be beneficial because it gives workers some voice (Turco, 2016) or mild decision authority (Fehr et al., 2013; Bartling et al., 2014) regarding their co-workers, or because it encourages workers to identify with the aims of the firm. In the context of ERPs, one recruiting website argued 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 having an ERP makes "current employees feel trusted and valued since they are participating in the companys future and growth."[33]

ERP is a signal from the firm. An ERP could be regarded by workers as a costly, positive signal from the firm, e.g., that the firm cares about them and is a desirable place to work (Dur, 2009). Indeed, the letters announcing the ERPs described the ERPs as stemming from a desire to have the "best and most attractive grocery stores" in the host country and to

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

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

become a "'market leader." This explanation received a couple votes in the manager survey, but much less than the explanation of greater involvement in hiring.

ERP as benefit to use in 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. Workers value the possibility of not only receiving a bonus, but also in being able to work with one's friend, as referrals as much more likely to be hired than non-referrals. However, this explanation received no support in the manager survey.

6 Referral bonuses and profits (P3)

We use the P2 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 ($c =$€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 A.4 gives further detail on c and Appendix A.5 gives further detail on the profit calculation.

Results. Panel A of Table 7 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.

7 Job quality perceptions and referrals (P4-P5)

To examine our last two prediction (P4 and 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. P4 says 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.[34]

Employee and Manager Surveys. Table 9 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 9 shows that similar findings apply to workers. We gave cashiers six most frequently mentioned reasons from the manager survey and asked them to rank them.[35] 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

[34]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.

[35]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.

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.[36]

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.[37] Third, we can test whether differences between treatment and control stores disappear once all stores are given the same treatment.

Among grocery store workers, Table 8 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.[38]

[36]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.

[37]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.

[38]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

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 frontloading 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.[39] 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 9, 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 P4. We discuss a few alternative explanations for P4 below, leaving the remainder to Appendix A.6.

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

jobs which are located around the country). Such restrictions should work against generating more referrals.

[39] 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.

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 9, 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.

Evidence on P5. 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 P5, i.e., that referrals are more responsive to referral bonuses in good jobs than in bad jobs.[40] However, Table 8 shows that we can 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.

[40] 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 level of 130 euros.

8 Referrals, Age, and Gender

While the paper focuses on the pre-registered outcomes of attrition and absence, Table 10 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,[41] and are 9ppmore likely to be female, though this last difference narrowly misses statistical significance. Having an ERP does not affect age, though it increases the share of female hires by 3pp (also narrowly missing significance).

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 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.[42]

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.

[41] 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.

[42] 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.

9 Conclusion

Combining a large-scale RCT on referral programs with a subsequent policy change and rich surveys, our paper yields two main lessons:

First, referral-making is qualitatively well-described by a simple model where referrers receive utility both from money and from the utility of their social contacts. Our evidence for this conclusion is that referrals respond positively both to money and the perception of a job as attractive. The model also successfully rationalizes other data features, including that referral quality decreases in bonuses; that the direct profit benefits of referral programs are non-monotonic in the bonus; and that referral bonuses and job attractiveness are complementary (i.e., that referral bonuses appear to have larger impacts on referrals for more attractive jobs). These findings are highly consistent with, but substantially expand, the earlier findings of Beaman and Magruder (2012).

Second, ERPs can have substantial indirect benefits, i.e., substantial benefits beyond getting more referrals. The ERPs we randomized were highly profitable, and 95% of the benefit came from indirect benefits. Our survey evidence suggests that the indirect benefits may 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).

Despite the results of the RCT, our results certainly do not generally imply that ERPs are ineffective in increasing referrals—in fact, the 2017 ERP was very effective in motivating referrals for non-grocery jobs at our firm. Instead, our results are consistent with the view that the impacts and benefits of ERPs may be heterogeneous, being an example of a management practice that is more useful in some circumstances than others.[43]

[43] In this sense, our findings our consistent with recent work (e.g., Blader et al., 2016; Gibbons and Henderson, 2012) emphasizing "contingent management practices," i.e., management practices that are most useful in some circumstances instead of universally.

It is important to recognize that our results and lessons are specific to a particular context. While the firm we study is large and is a leading firm in the country where it operates, it is still only one firm and one featuring low-skill jobs. In a high-skilled context (e.g., where workers have graduate degrees), 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. In addition, in firms with high levels of modern management practices, it could be the case that there are smaller indirect benefits from ERPs, e.g., in contexts where workers already respected and feel like they have a say in hiring, an ERP might have smaller benefits to non-referrals. Low-skill settings like ours may also be ones where turnover is relatively malleable. Furthermore, lessons from a transition economy may not generalize to richer countries. Still, even if the magnitude of indirect benefits would be smaller in high-skilled contexts, one might imagine that they would be directionally similar. Moreover, low-skill jobs such as those in supermarkets represent a large and growing share of the economy (Goos et al., 2014).

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.

References

Abel, Martin, Rulof Burger, and Patrizio Pirainoz, "The Value of Reference Letters - Experimental Evidence from South Africa," 2016. Mimeo, Harvard University.

Aghion, Philippe, Yann Algan, Pierre Cahuc, and Andrei Shleifer, "Regulation and distrust," *Quarterly Journal of Economics*, 2010, *125* (3), 1015–1049.

Bartling, Björn, Ernst Fehr, and Holger Herz, "The intrinsic value of decision rights," *Econometrica*, 2014, *82* (6), 2005–2039.

Bayer, Patrick, Stephen L. Ross, and Giorgio Topa, "Place of Work and Place of Residence: Informal Hiring Networks and Labor Market Outcomes," *Journal of Political Economy*, 2008, *116* (6), 1150–1196.

Beaman, Lori and Jeremy Magruder, "Who gets the job referral? Evidence from a social networks experiment," *American Economic Review*, 2012, *102* (7), 3574–3593.

_ , Niall Keleher, and Jeremy Magruder, "Do Job Networks Disadvantage Women? Evidence from a Recruitment Experiment in Malawi," *Journal of Labor Economics*, 2018, *36* (1), 121–157.

Benabou, Roland and Jean Tirole, "Intrinsic and Extrinsic Motivation," *Review of Economic Studies*, 2003, *70* (3), 489–520.

_ and _ , "Incentives and Prosocial Behavior," *American Economic Review*, 2006, *96* (5), 1652–1678.

Blader, Steven, Claudine Gartenberg, and Andrea Prat, "The Contingent Effect of Management Practices," 2016. Mimeo, Columbia.

Bloom, Nicholas, James Liang, John Roberts, and Zhichun Jenny Ying, "Does working from home work? Evidence from a Chinese experiment," *Quarterly Journal of Economics*, 2014, *130* (1), 165–218.

_ , Renata Lemos, Raffaella Sadun, Daniela Scur, and John Van Reenen, "JEEA-FBBVA Lecture 2013: The New Empirical Economics of Management," *Journal of the European Economic Association*, 2014, *12* (4), 835–876.

Brown, Meta, Elizabeth Setren, and Giorgio Topa, "Do Informal Referrals Lead to Better Matches? Evidence from a Firms Employee Referral System," *Journal of Labor Economics*, 2016, *34* (1), 161–209.

Bruhn, Miriam and David McKenzie, "In Pursuit of Balance: Randomization in Practice in Development Field Experiments," *American Economic Journal: Applied Economics*, October 2009, *1* (4), 200–232.

Burks, Stephen V., Bo Cowgill, Mitchell Hoffman, and Michael Housman, "The Value of Hiring through Employee Referrals," *Quarterly Journal of Economics*, 2015, *130* (2), 805–839.

Calvo-Armengol, Antoni and Matthew O. Jackson, "The Effects of Social Networks on Employment and Inequality," *American Economic Review*, 2004, *94* (3), 426–454.

Cameron, Colin and Pravin Trivedi, *Microeconometrics: Methods and Applications*, Cambridge University Press, 2005.

CareerBuilder, "Referral Madness: How Employee Referral Programs Turn Good Employees Into Great Recruiters and Grow Your Bottom Line," CareerBuilder e-Book 2012.

Castilla, Emilio J., "Social Networks and Employee Performance in a Call Center," *American Journal of Sociology*, 2005, *110* (5), pp. 1243–1283.

Deci, Edward L, Richard Koestner, and Richard M Ryan, "A meta-analytic review of experiments examining the effects of extrinsic rewards on intrinsic motivation.," *Psychological bulletin*, 1999, *125* (6), 627.

DellaVigna, Stefano, John A List, Ulrike Malmendier, and Gautam Rao, "Estimating Social Preferences and Gift Exchange at Work," Technical Report, NBER Working Paper No. 22043 2016.

DeLong, Thomas J and Vineeta Vijayaraghavan, "S.G. Cowen: New Recruits," 2002, *Harvard Business School Case Study.*

Dur, Robert, "Gift Exchange in the Workplace: Money or Attention?," *Journal of the European Economic Association*, 2009, *7* (2-3), 550–560.

Dustmann, Christian, Albrecht Glitz, Uta Schönberg, and Herbert Brücker, "Referral-based Job Search Networks," *Review of Economic Studies*, 2015, *83* (2), 514–546.

Ekinci, Emre, "Employee Referrals as a Screening Device," *RAND Journal of Economics*, 2016, *47* (3), 688–708.

Fehr, Ernst, Holger Herz, and Tom Wilkening, "The lure of authority: Motivation and incentive effects of power," *American Economic Review*, 2013, *103* (4), 1325–59.

Fernandez, Roberto M., Emilio J. Castilla, and Paul Moore, "Social Capital at Work: Networks and Employment at a Phone Center," *American Journal of Sociology*, 2000, *105* (5), pp. 1288–1356.

Friebel, Guido, Matthias Heinz, and Nick Zubanov, "Middle managers, personnel turnover and sales: a long-term field experiment in a retail chain," 2018. Working paper, Frankfurt University.

_ , _ , Miriam Krueger, and Nikolay Zubanov, "Team Incentives and Performance: Evidence from a Retail Chain," *American Economic Review*, 2017, *107* (8), 2168–2203.

Gibbons, Robert and Rebecca Henderson, "What do Managers Do?: Exploring Persistent Performance Differences among Seemingly Similar Enterprises," 2012. Mimeo, Harvard Business School.

Glover, Dylan, Amanda Pallais, and William Pariente, "Discrimination as a Self-Fulfilling Prophecy: Evidence from French Grocery Stores," *Quarterly Journal of Economics*, 2017, *132* (3), 1219–1260.

Goos, Maarten, Alan Manning, and Anna Salomons, "Explaining job polarization: Routine-biased technological change and offshoring," *American Economic Review*, 2014, *104* (8), 2509–26.

Granovetter, Mark, *Getting a Job*, Cambridge, MA: Harvard University Press, 1974.

Heath, Rachel, "Why do Firms Hire using Referrals? Evidence from Bangladeshi Garment Factories," *Journal of Political Economy*, 2018, *Forthcoming.*

Hoffman, Mitchell, "The Value of Hiring through Employee Referrals in Developed Countries," *IZA World of Labor*, 2017, *369*, 1–8.

Holzer, Harry J., "Informal Job Search and Black Youth Unemployment," *American Economic Review*, 1987, *77* (3), pp. 446–452.

Ichino, Andrea and Enrico Moretti, "Biological Gender Differences, Absenteeism, and the Earnings Gap," *American Economic Journal: Applied Economics*, January 2009, *1* (1), 183–218.

Imai, Kosuke, Luke Keele, and Dustin Tingley, "A general approach to causal mediation analysis.," *Psychological Methods*, 2010, *15* (4), 309.

_ , _ , and Teppei Yamamoto, "Identification, inference and sensitivity analysis for causal mediation effects," *Statistical Science*, 2010, pp. 51–71.

Jovanovic, Boyan, "Job Matching and the Theory of Turnover," *Journal of Political Economy*, 1979, *87* (5), 972–90.

Kim, Minjae and Roberto M. Fernandez, "Strength Matters: Tie Strength as a Causal Driver of Networks' Information Benefits," *Social Science Research*, 2017, *65*, 268–281.

Kugler, Adriana D, "Employee Referrals and Efficiency Wages," *Labour Economics*, 2003, *10* (5), 531–556.

Mas, Alexandre and Enrico Moretti, "Peers at work," *American Economic Review*, 2009, *99* (1), 112–45.

Montgomery, James D, "Social Networks and Labor-Market Outcomes: Toward an Economic Analysis," *American Economic Review*, 1991, *81* (5), 1407–18.

Pallais, Amanda and Emily Glassberg Sands, "Why the Referential Treatment? Evidence from Field Experiments on Referrals," *Journal of Political Economy*, 2016, *124* (6), 1793–1828.

Pellizzari, Michele, "Do Friends and Relatives Really Help in Getting a Good Job?," *Industrial and Labor Relations Review*, 2010, *63* (3), 494–510.

Romero, Mauricio, Justin Sandefur, and Wayne Aaron Sandholtz, "Outsourcing Service Delivery in a Fragile State: Experimental Evidence from Liberia," 2018.

Saloner, Garth, "Old Boy Networks as Screening Mechanisms," *Journal of Labor Economics*, 1985, *3* (3), 255–267.

Scott, Neil W., Gladys C. McPherson, Craig R. Ramsay, and Marion K. Campbell, "The method of minimization for allocation to clinical trials: a review," *Controlled Clinical Trials*, 2002, *23* (6), 662 – 674.

Shinnar, Rachel S., Cheri A. Young, and Marta Meana, "The Motivations for and Outcomes of Employee Referrals," *Journal of Business and Psychology*, 2004, *19*, 271–283.

SHRM, "Designing and Managing Successful Employee Referral Programs," 2016.

Simon, Curtis J. and John T. Warner, "Matchmaker, Matchmaker: The Effect of Old Boy Networks on Job Match Quality, Earnings, and Tenure," *Journal of Labor Economics*, 1992, *10* (3), 306–30.

Smith, Sandra Susan, ""Don't put my name on it": Social Capital Activation and Job-Finding Assistance among the Black Urban Poor," *American Journal of Sociology*, 2005, *111* (1), 1–57.

Smith, Tom W. and Jaesok Son, "Measuring Occupational Prestige on the 2012 General Social Survey," 2014. GSS Methodological Report No. 122.

Stockman, Sara, Greet Van Hoye, and Marieke Carpentier, "The Dark Side of Employee Referral Bonus Programs: Potential Applicants Awareness of a Referral Bonus and Perceptions of Organisational Attractiveness," *Applied Psychology*, 2017, *66* (4), 599–627.

Ton, Zeynep, *The Good Jobs Strategy: How the Smartest Companies Invest in Employees to Lower Costs and Boost Profits*, Houghton Mifflin Harcourt, 2014.

Topa, Giorgio, "Labor Markets and Referrals," *Handbook of Social Economics*, 2011, *1*, 1193–1221.

Turco, Catherine J., *The conversational firm: Rethinking bureaucracy in the age of social media*, Columbia University Press, 2016.

Zenou, Yves, "A dynamic model of weak and strong ties in the labor market," *Journal of Labor Economics*, 2015, *33* (4), 891–932.

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

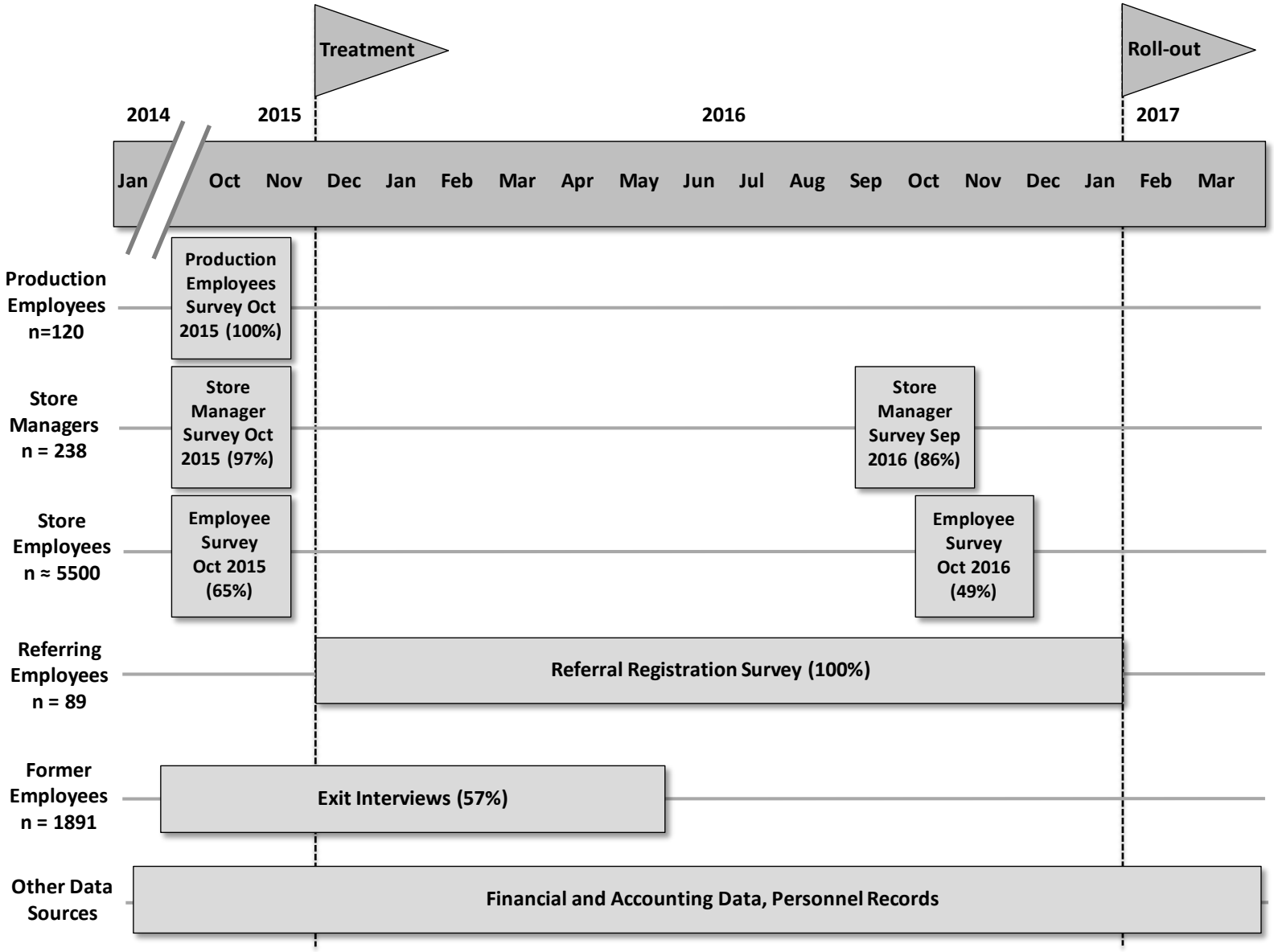
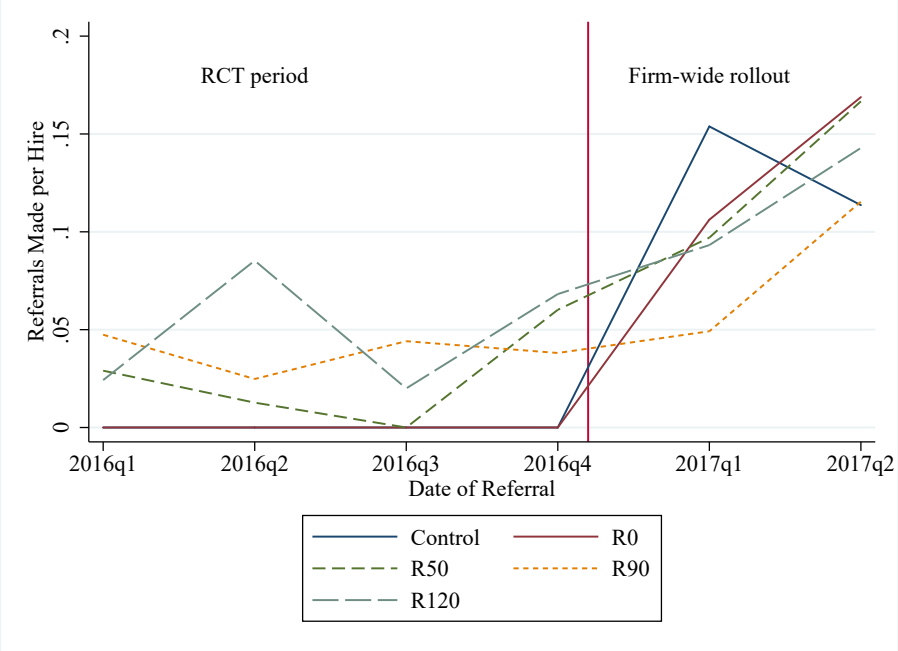
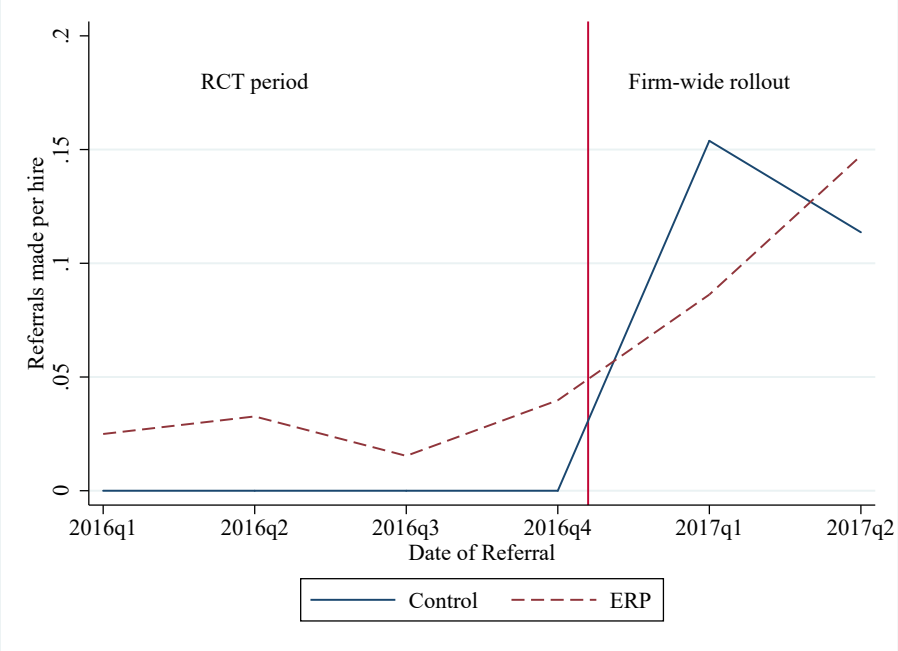


Figure 3: Referrals Made over Time in the RCT and Firm-wide Rollout



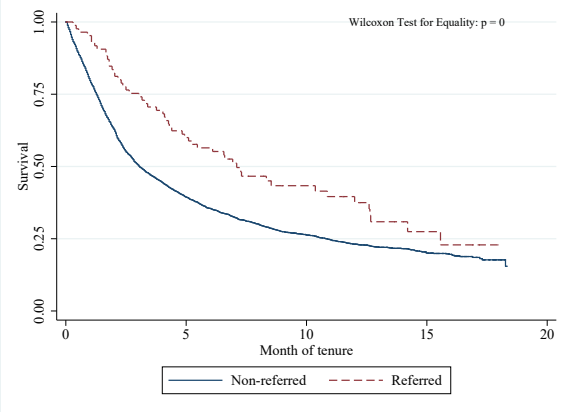
(a) Five Arms



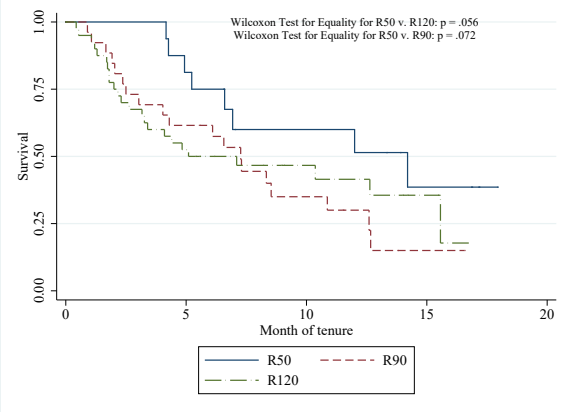
(b) ERP vs. Control

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.

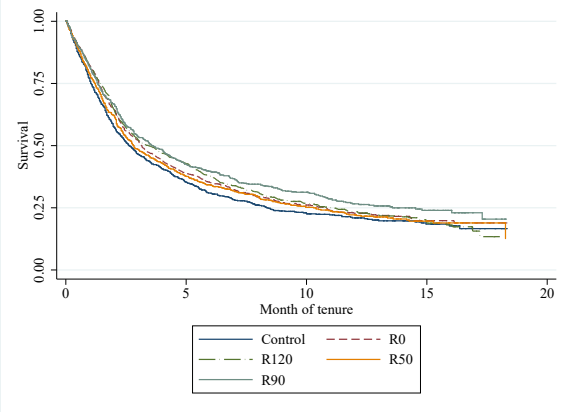
Figure 4: Survival Curve Comparisons



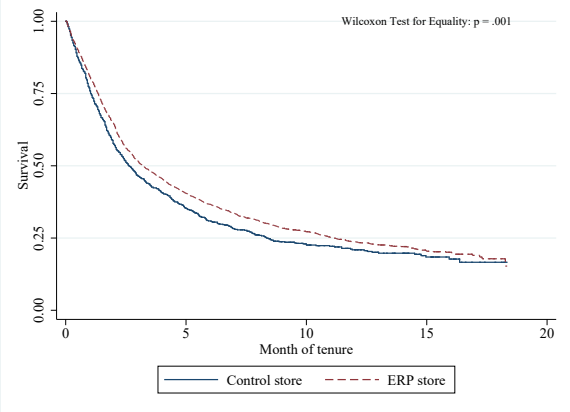
(a) Referrals vs. Non-referrals



(b) Referral Survival by Bonus Level



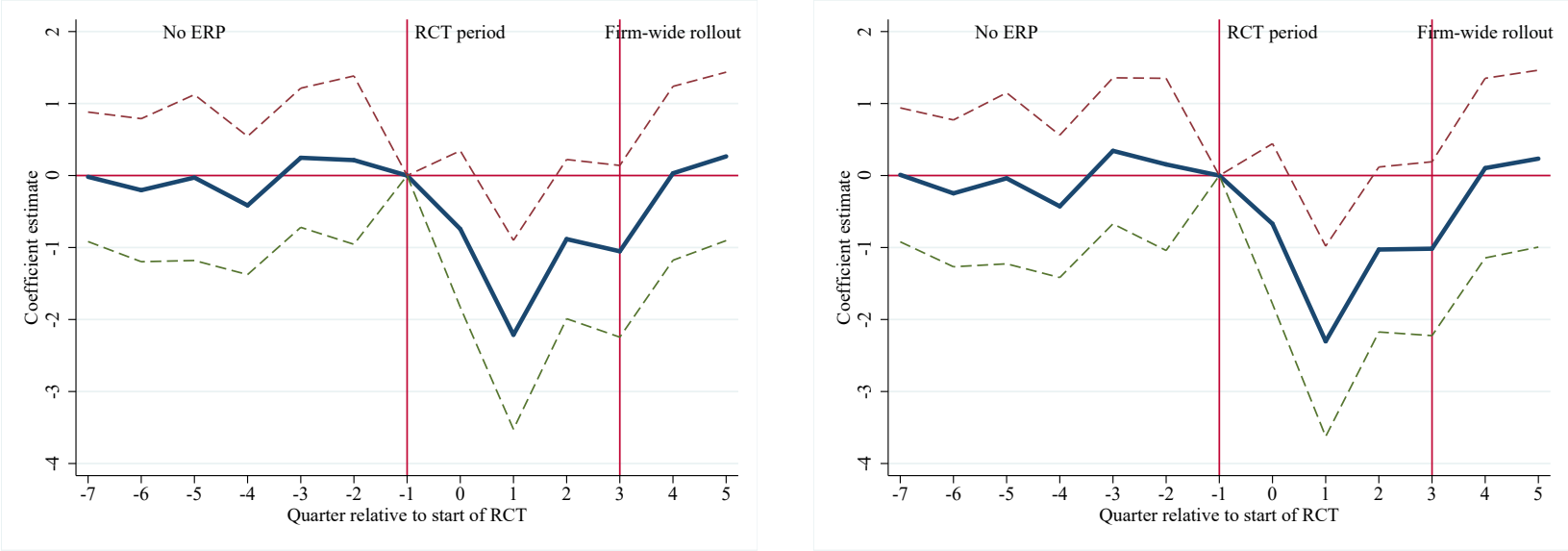
(c) Survival of Non-Referrals by Bonus Group



(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.

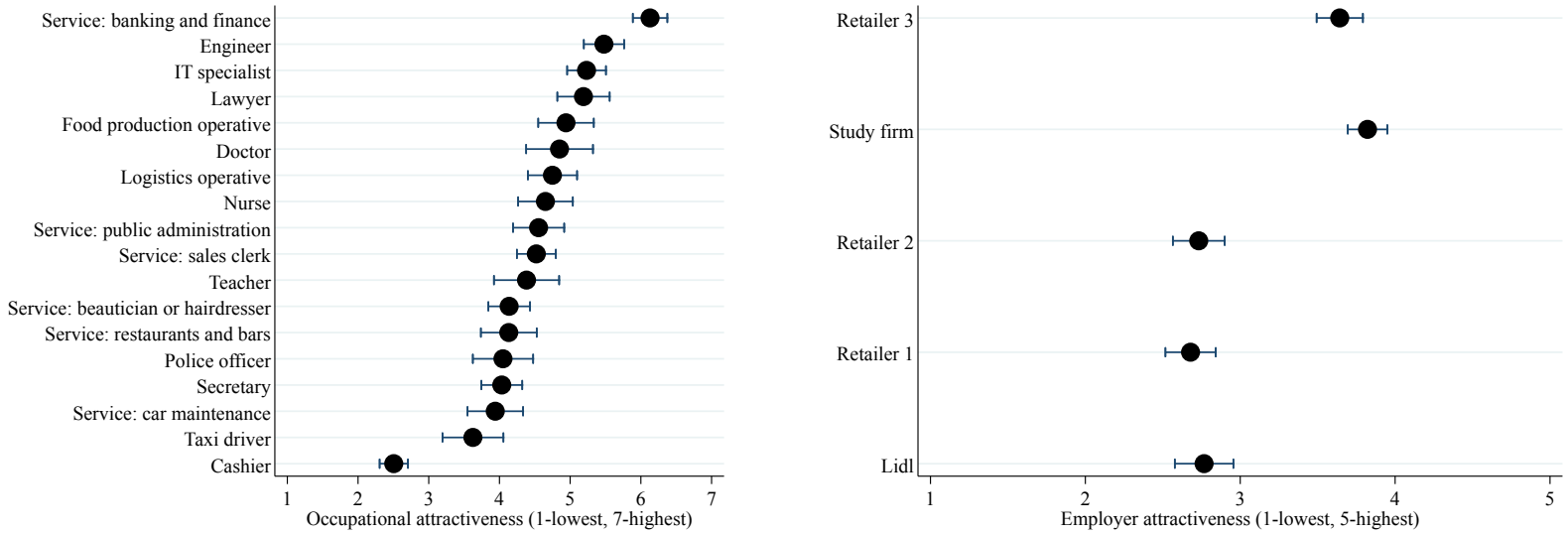


(a) Overall Results

(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: In measuring occupational attractiveness, we followed the General Social Survey method (Smith and Son, 2014). Different survey respondents were asked to rate occupations from different six-item subsets of the 18-item occupations list we designed. This was done to meet the respondents' time and attention constraints. Dots are mean attractiveness ratings, and whiskers are 95% confidence intervals for the means.

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	€50	€90	€120
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: 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 6. 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 A.4. See Appendix A.5 for further details on the profit calculation.

Table 8: 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 9: 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 10: 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	0.481	0.481	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	0.478	0.478	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 provides additional discussion and results. For each subsection, we give the relevant section of the main paper that it accompanies. Appendix B is the Data Appendix. Appendix C contains additional figures and tables. Appendix D provides additional materials used by the firm in the ERPs.

Appendix A Additional Discussion and Results

A.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.

A.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.[1] 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.[2] 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

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

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

monthly salary) for referring an experienced driver, though there was also a 6-month tenure requirement.

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

We ran the following surveys:

1. 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. 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. 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.

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.

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.

A.4 Calculation of the costs of turnover (Section 6)

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.

A.5 Profit Calculation Details (Section 6)

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.

A.6 Further Discussion on the Reasons for Few Referrals Other than Job Attractiveness (Section 7)

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 listed in Table 9, 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.[3]

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

[3]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.

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.

Appendix B 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.

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 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).

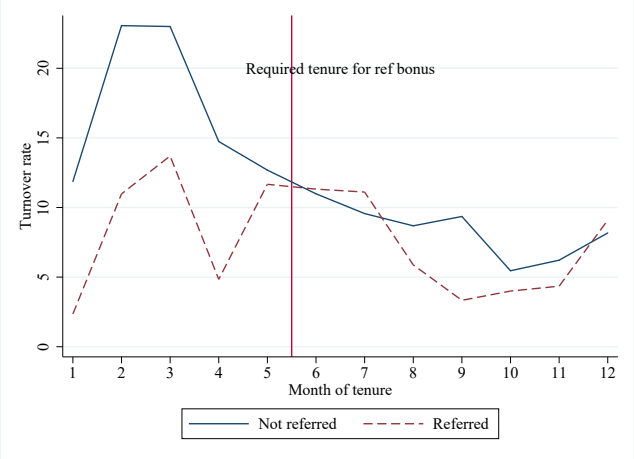
Gender. For workers where gender is missing, we impute missing gender based on name by using gender-specific endings that exist in the language of the country where our firm is based.

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.

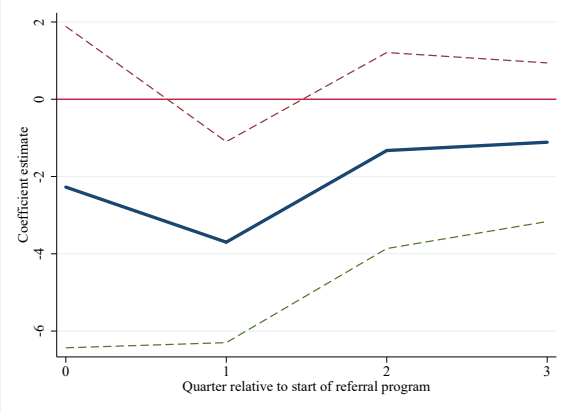
Appendix C Additional Figures and Tables

Figure C1: Attrition Hazard for Referrals vs. Non-referrals

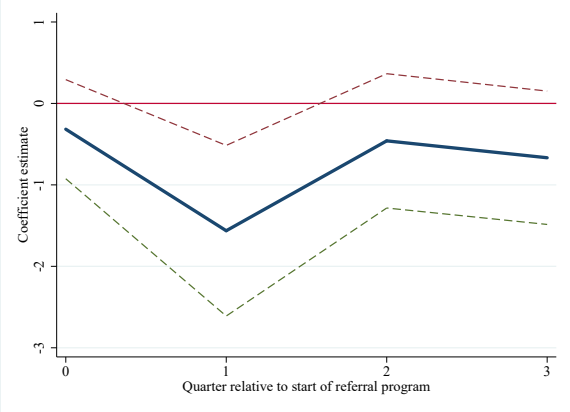


Notes: This figure shows the monthly quiz hazard as a function of worker tenure comparing referred vs. non-referred workers. The sample is workers hired during the RCT. The referral and referrer must stay 5 months after the referral is hired in order for the referrer to be paid. The vertical tenure threshold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

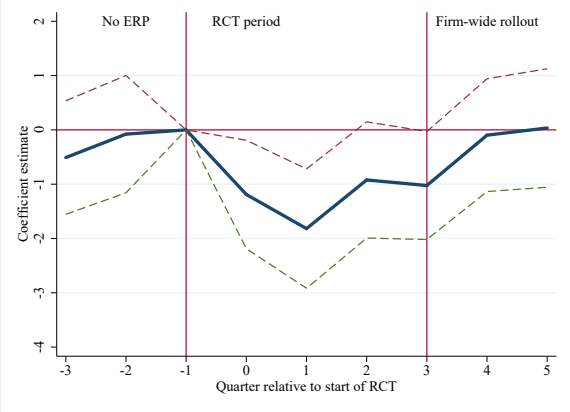
Figure C2: Event Studies on Impact of ERPs: Additional Subsamples and Outcomes



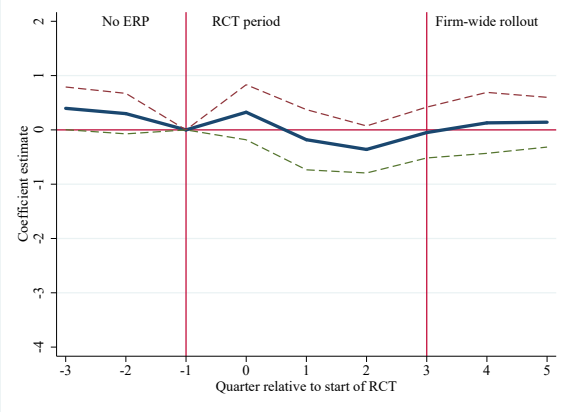
(a) Sample is New Hires During RCT



(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 C1: 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 C2: 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 C3: 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 C4: 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%

Table C5: Manager Survey: Why did ERPs Reduce Turnover Separate from Generating More Referrals? (N=8)

Reason	Share of managers selecting that option
"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"	87.5%
"Because FIRM NAME started the referral program, it made employees think that FIRM NAME was a better place to work."	25%
"Employees didn't have a person to recommend, but they hoped to recommend a friend in the future."	0%
"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."	0%
"None of these reasons are important or likely. What is your explanation?"	12.5%

Notes: Managers could select more than one option.

Appendix D Documents Used in the Experiment

Figure D3: 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 D4: Poster Used during 2017 Referral Program Roll-out: Grocery Workers



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

Figure D5: Poster Used during 2017 Referral Program Roll-out: Logistics Workers



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

Appendix References

Beaman, Lori and Jeremy Magruder, "Who gets the job referral? Evidence from a social networks experiment," *American Economic Review*, 2012, *102* (7), 3574–3593.

Blatter, Marc, Samuel Muehlemann, and Samuel Schenker, "The costs of hiring skilled workers," *European Economic Review*, 2012, *56* (1), 20–35.

Bloom, Nicholas, Renata Lemos, Raffaella Sadun, Daniela Scur, and John Van Reenen, "JEEA-FBBVA Lecture 2013: The New Empirical Economics of Management," *Journal of the European Economic Association*, 2014, *12* (4), 835–876.

Boushey, Heather and Sarah Jane Glynn, "There are significant business costs to replacing employees," *Center for American Progress*, 2012.

Brown, Meta, Elizabeth Setren, and Giorgio Topa, "Do Informal Referrals Lead to Better Matches? Evidence from a Firms Employee Referral System," *Journal of Labor Economics*, 2016, *34* (1), 161–209.

Burks, Stephen V., Bo Cowgill, Mitchell Hoffman, and Michael Housman, "The Value of Hiring through Employee Referrals," *Quarterly Journal of Economics*, 2015, *130* (2), 805–839.

Madrian, Brigitte C. and Dennis F. Shea, "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior," *Quarterly Journal of Economics*, 2001, *116* (4), 1149–1187.