What Do Employee Referral Programs Do?*

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Abstract

Employee referral programs (ERPs) are randomly introduced in a grocery chain. Larger referral bonuses increase referrals and decrease referral quality, though the increase in referrals from having an ERP is modest. However, the overall effect of having an ERP is substantial, reducing attrition by roughly 15% and decreasing firm labor costs by up to almost 3%. This occurs, partly, because referrals stay longer than non-referrals, but, mainly, because all workers stay longer in treated than controls stores, even among stores where no referrals are made. The most-supported mechanism for these indirect effects is that workers value being involved in hiring.

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Hiring is of long interest to economists and is believed critical for firm performance (Ichniowski et al., 1997; Oyer and Schaefer, 2011; Bloom and Van Reenen, 2011). One of the most common ways by which workers get hired is via employee referrals.[1] As noted by Topa (2019), while most work on referrals analyzes the perspective of job-seekers, a growing literature analyzes referrals from the perspective of firms. This work shows that referral hires tend to be of higher quality than non-referrals, with lower turnover, lower recruiting costs, and sometimes higher productivity (e.g., Brown et al., 2016; Burks et al., 2015).[2] Thus, it may not come as a surprise that many firms have employee referral programs (ERPs), a management practice where workers are explicitly encouraged to refer their social contacts for jobs, often using bonuses.[3]

Despite the prevalence of ERPs, the fact that many firms engage in a management practice does not necessarily imply that it is valuable (Bloom et al., 2014; Blader et al., 2019; DellaVigna and Gentzkow, 2019).[4] What do ERPs do and why? We answer this question using a 13-month randomized control trial (RCT) on over 10,000 workers in a large European grocery chain, followed by the immediate rollout of an ERP to the entire firm. Ours is the first RCT on an ERP in a for-profit firm. To our knowledge, it is also the first, large-scale, within-the-firm RCT on any hiring procedure, a point we contextualize further below. All the firm's 238 stores were randomly assigned to Control (no ERP) or one of four ERP treatment arms inviting referrals. One treatment arm only provided information to encourage referrals, whereas the other three arms additionally paid different referral bonuses of up to 40% of monthly salary after taxes if the referrer and the person hired through a referral stayed at least 5 months. Following the literature, we will use the term "referral" both for the process through which a person is recommended to the firm, and for the person who is hired into the firm as a consequence of this process.

As can be asked for many management practices, there are two key conceptual questions regarding ERPs. First, what are the direct effects, i.e., effects on the targeted worker behavior? That is, what is the impact of ERPs—both their existence and the bonus level—on

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

[2] See Topa (2011, 2019), Hoffman (2017), and Rubineau and Fernandez (2015) for surveys.

[3] 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 (2012) estimated that 69% of firms on its platform had a formal ERP. In the retail module of the World Management Survey (Bloom and Van Reenen, 2007) covering Canada, US, and UK, 23% of establishments have an ERP (see Appendix A.1). ReferralPrograms.org (2017) reports that 82% of firms use cash bonuses in their ERP, 2% use donations, 7% use experiences, and 9% use no reward.

[4] E.g.,workplace wellness programs are popular, but RCTs find few benefits (Jones et al., 2019). Some practitioners express skepticism about ERPs, particularly regarding the use of larger bonuses (Bock, 2015).

generating employee referrals, as well as the quality of referral hires? This includes assessing whether referrals are of higher quality than non-referrals and whether this quality advantage varies with the bonus offered. Second, what are the overall effects of ERPs, i.e., what is the total impact of having an ERP on worker and firm outcomes? Overall effects are assessed among all workers, including incumbent workers and non-referred new hires. If having an ERP provides a positive signal to workers (e.g., that the firm respects its workers (Ellingsen and Johannesson, 2008; Rebitzer and Taylor, 2011) and trusts them not to make bad referrals) or a negative one (e.g., that the firm needs its workers' help in recruiting new workers), then overall effects may diverge sharply from direct effects.

By randomizing both the structure and existence of ERPs across stores, we designed the RCT to assess both direct and overall effects of ERPs. Beyond the large sample size it offers, the particular firm we study (described more in Section 2) is well-suited for the RCT. First, because of high worker turnover, grocery stores are constantly looking for new workers. Second, grocery cashier jobs have minimal qualifications, so everyone's friends could reasonably be hired. Third, the firm was willing to have its workers and managers take a series of detailed surveys. Finally, the retail setting of the firm is broadly representative of millions of jobs worldwide.

Section 3 shows that the direct effects of ERPs are directionally as expected. The higher the bonuses, the more referrals are made. However, while statistically significant, the magnitude of the impacts seems economically small. Even under the largest referral bonus, referrals comprise only 5% of hires. Encouraging referrals without paying a bonus leads to no referrals. We believe the seemingly low referral rate reflects (1) that we are studying formal instead of informal referrals and (2) that grocery jobs are perceived as unattractive, a point supported by surveys.[5] While the number of referrals is modest, referral quality is high: referrals have 40% lower attrition than observably similar non-referrals and are 19% less likely to be absent, though the absence difference is not statistically significant. However, as bonuses increase, the relative retention benefit of referrals falls.

Section 4 turns to overall effects, and provides the paper's quantitatively most important, and potentially most surprising, result: having an ERP in a store leads to a roughly 15% reduction in worker turnover. Effects persist throughout the RCT (i.e., for 13 months), and are similar in percentage terms among new workers and workers hired prior to the RCT. These effects cannot be mainly attributed to the incidence of referrals or to peer effects because turnover falls in treatment stores where no RCT referrals are made. Nor are the effects related to managers behaving differently in treatment stores. Instead, our surveys

[5]These surveys are discussed in Section 7 when comparing grocery and non-grocery jobs. Section 3 addresses informal vs. formal referrals, and discusses how low rates of formal referrals occur in other firms.

suggest that effects are due to workers feeling respected because the ERP invited them to be involved in hiring, and because workers value having a say in who they work with.

Section 5 shows that having an ERP is highly profitable, reducing labor costs by up to 2.8%. About 5% of the savings reflects that referrals have higher retention than non-referrals (i.e., direct benefits), while 95% of profit gains come from an ERP boosting the retention of non-referred workers (i.e., indirect benefits). Hence, only comparing referrals vs. non-referrals would dramatically underestimate the benefits of an ERP, which we believe to be a major take-away from our RCT. Direct benefits are non-monotonic in the bonus level, consistent with a quantity-quality tradeoff from referral bonuses.

Section 6 turns to heterogeneity analysis. Exploiting that our RCT was conducted across a large, national firm, we show that the overall impact of ERPs on attrition is larger in stores that were better performing before the RCT and in stores that are located in stronger local labor markets. The profit benefit of ERPs is much larger in these stores.

Because the firm appreciated the effects of the RCT, it rolled out the ERP to all employees, including grocery workers in RCT control stores, as well non-grocery jobs in logistics and food production (Section 7). Once control grocery stores are treated with an ERP, attrition rates between treatment and control stores converge, consistent with long-run stability of ERP impacts. Referrals for grocery jobs remain relatively modest. In contrast, for non-grocery jobs, the ratio of referrals to total hires is 3 times larger than for grocery jobs. Surveys with workers, managers, and the general public indicate that grocery jobs are perceived as unattractive, and that workers who care for their friends may hesitate to refer friends for these jobs. However, non-grocery jobs are perceived as much more attractive.[6]

Our paper contributes to several literatures, most importantly, to the one on management practices. Guided by robust empirical relations between management practices and outcomes (Ichniowski et al., 1997; Ichniowski and Shaw, 1999; Bloom et al., 2019), recent work has performed experimental evaluations of broad management practices (Bloom et al., 2013) or particular management practices like work from home (Bloom et al., 2014). What is particularly noteworthy in our paper is the RCT examination of a management practice *at-scale*.[7] Oyer and Schaefer (2011) argue that hiring has been understudied in labor and personnel economics, and that RCTs are needed to address this gap.[8] To our knowledge,

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

[7]Exceptions of at-scale, within-firm RCTs include Nagin et al. (2002), Blader et al. (2019), Gosnell et al. (2019), and Friebel et al. (2017, 2018), all on non-hiring topics.

[8]In the *Handbook of Labor Economics*, Oyer and Schaefer (2011) write that personnel economics "needs a series of carefully constructed hiring-related field experiments. Personnel Economics now has a very solid tradition of incentives-related field experiments, and we are eager to see this toolkit applied to hiring decisions."

ours is the first, large-scale, within-the-firm RCT on any hiring procedure in any context.[9] If only a small subset of stores were treated, as in many within-firm RCTs, it would have been quite hard for us to observe the indirect benefits of ERPs. Our finding that the benefit of ERPs is larger in higher-performing grocery stores also exploits the large scale of our RCT, and is consistent with ERPs being complementary to other HR management practices, an idea discussed frequently in theory (Milgrom and Roberts, 1990), but, beyond key exceptions like Ichniowski et al. (1997) and Blader et al. (2019), is often hard to examine empirically.[10]

Second, our results contribute to a small but influential literature on dual-purpose HR practices. As described in Rebitzer and Taylor (2011), HR practices can have multiple effects on workers, e.g., performance pay may both increase effort and attract better workers (Lazear, 2000). However, evidence on dual-purpose HR practices is relatively scarce.[11] We show that having an ERP generates referrals (who yield benefits to the firm relative to non-referrals) and separately causes workers to stay longer, arguably because they value being involved in hiring. Our results are consistent with the theoretical insight of Ellingsen and Johannesson (2007, 2008) that workers care about being well-regarded by their employer. As far as we know, ours is the first academic paper in any field to show that ERPs can have broader organizational consequences beyond the referrer and referral. Our finding that workers care about being respected broadly aligns with Blader et al. (2019), who find that respect helps explain the interactive effect of lean transportation and performance rankings.

Third, the paper substantially expands what is known empirically about referrals and ERPs. Beyond how ERPs affect referral-making, our RCT enables us to assess how having an ERP and the level of referral bonus affect worker outcomes and firm profits. While larger bonuses increase referrals, we show for the first time that they decrease the quality of referral hires, illustrating a quantity-quality tradeoff. As summarized by Topa (2011, 2019) and Hoffman (2017), prior work on referrals from the perspective of firms compares average worker outcomes between referrals and non-referrals, but lacks variation in ERPs (exogenous or otherwise) and thus cannot assess the firm consequences of ERPs—that is, our paper is the first to evaluate ERPs as a *management practice*.[12] Building on Topa's (2019) suggestion for research to analyze how referral differences vary across local labor markets, we exploit

[9] Development studies have randomized selection procedures in government (e.g., Ashraf et al., 2018) or NGOs (e.g., Del Carpio and Guadalupe, 2018), but not in a private firm. Audit studies examine hiring issues across firms (instead of randomizing a firm's hiring procedures). Appendix A.2 discusses further.

[10] We underscore that results are *consistent with* instead of *indicative of* complementarity, as we lack the detailed management surveys needed to measure non-ERP management practices.

[11] Ritter and Taylor (1994) and Landers et al. (1996) are noteworthy examples; Rebitzer and Taylor (2011) discuss the literature, including further examples.

[12] Papers randomize referral programs in non-firm contexts to study different questions from ours, such as whether people can screen for cognitive tests (Beaman and Magruder, 2012) or loan-paying (Bryan et al., 2015).

the wide geographic scale of our RCT to show that ERPs have larger overall effects in better local labor markets. Last, our RCT provides evidence on what motivates referrers, which is useful given that referrals occur exogenously in most models of referrals (see Ekinci (2016) for an exception).[13]

1 Conceptual Framework

How might ERPs with different referral bonuses affect outcomes and why? Since ERPs may be a dual-purpose HR practice (Rebitzer and Taylor, 2011), we discuss both in terms of direct effects (i.e., quantity and quality of referral hires) and overall effects. We then discuss how ERPs may affect firm profits, and how effects may vary by store characteristics and job quality. To cover a range of different theories, our discussion here is verbal. Appendix D provides a model with analytic insights into many of these issues.

Starting with direct effects, one would imagine that larger bonuses would increase referrals, though this is not obvious. If larger bonuses signaled to workers that making a referral is difficult, the effect could be zero or even negative (Benabou and Tirole, 2003). In terms of quality, one would expect following past work that referrals would be of higher quality than non-referrals.[14] For example, if incumbent workers are altruistic toward their friends (Bandiera et al., 2005, 2009), they may only be willing to refer a friend if the match quality between the friend and the job is above a threshold. As the referral bonus increases, incumbent workers will lower their match quality thresholds, becoming willing to refer less qualified friends because the financial reward is higher. Thus, increasing the bonus should decrease the quality of referrals. The Appendix D model shows that this is the case.

We now consider the overall effects of an ERP, including indirect effects beyond generating referrals, such as possible effects on incumbent retention. Through an ERP, a firm is asking its workers to become involved in recruiting rather than carrying out this process only through HR and line managers. An ERP does not delegate formal recruiting rights, but it gives some real authority to the workers, as envisaged by Aghion and Tirole (1997). Indeed, in our firm, 97% of referred applicants during the RCT were hired, and surveys indicate that workers understood that their referrals would be hired. In other settings, like large high-tech firms in the US, it is often promised to employees that referrals will receive serious consideration instead of being lumped in the mass resume file (Bock, 2015). Workers

[13] Ekinci (2016) presents a model of ERPs where potential referrers have career concerns.

[14] This may occur because more precise signals are observed on referrals relative to non-referrals (Simon and Warner, 1992; Dustmann et al., 2015; Brown et al., 2016); because good workers are friends with people like themselves (Montgomery, 1991); or because referrer-referral ties help solve moral hazard or team production problems (Fernandez et al., 2000; Kugler, 2003; Castilla, 2005; Pallais and Sands, 2016; Heath, 2018).

are thus not only given the opportunity to work with their friends; the delegation decision may also be valuable in terms of what it communicates to incumbent workers.

As noted by Benabou and Tirole (2003), decisions to delegate can communicate to workers that the firm believes workers to be of high ability and have good judgment. Workers may value this as a signal that the firm is likely to treat them better. Another possibility is that workers may intrinsically value the firm believing them to be altruistic. Ellingsen and Johannesson (2008) present a model of respect in the workplace where a worker's respect is her second-order beliefs about her social preferences, i.e., her belief about the firm's beliefs about whether she is altruistic. Having an ERP may be a credible way for a firm to communicate its esteem, e.g., the firm may only be willing to have an ERP when it believes workers to be altruistic (either toward their friends or toward the firm), as such workers will be more concerned than selfish workers in avoiding bad matches. Provided that workers value feeling esteemed, having an ERP should increase retention, even in stores where no referrals are made. Appendix D shows this formally in a signaling model.

We suspect that involvement and delegation may be particularly beneficial when taking place within the realm of hiring. Anecdotally, while professors sometimes skip various faculty meetings, nearly everyone comes to faculty hiring meetings, suggesting that faculty like being involved in hiring. This is consistent with business case study evidence that involving workers in hiring can have broader organizational benefits (DeLong and Vijayaraghavan, 2002).

It is not obvious though that the signaling benefit of an ERP would be positive. Having an ERP could communicate that the firm is having a hard time recruiting through non-referral channels, or that it expects to experience significant turnover in the future, for which it would need to do a lot of hiring. Such sentiments could make workers more likely to quit. Conditional on having an ERP, the signaling benefit of larger bonuses is also ambiguous.[15]

Turning to profits, ERPs will increase profits if the overall benefits (direct and indirect) exceed the cost of referral programs. This is an empirical question. It also unclear how the bonus level will affect profits, either overall or in terms of direct effects. For example, higher bonuses may boost referrals, but also cost money and may decrease referral quality.

In terms of heterogeneity, ERPs should have larger impacts in stores where possible benefits of ERPs are reinforced. For example, that the firm respect its workers (a possible indirect benefit of ERPs) may be more credible when the store is otherwise functioning well.

Last, how would we expect the prevalence of referrals and the impact of bonuses on referrals to vary based on the type of job? If people are altruistic toward their friends, we would expect that people would be more willing to refer their friends for better jobs, at least

[15]On one hand, larger referral bonuses expose the firm to greater risk of opportunism, so it could be a sign of greater trust. On the other hand, larger bonuses could indicate the firm expects future turnover problems.

comparing jobs within the realm of other lower-skill jobs.

2 Study Background

The study firm. The firm is one of three main grocery chains in an Eastern European Union (EU) country.[16] Prior to the RCT, the firm's management changed. The new management decided to pursue a strategy of increasing quality, partly triggered by the threat of entry from Lidl, a discount German chain. Reducing turnover was declared a high-priority goal to assure quality service and decrease excessive worker training costs.

As is common for low-skill workers, attrition is high, at an annual rate around 60% in the pre-RCT period.[17] Turnover costs are non-negligible, with direct (administrative and training) costs around €250 per exit, plus additional costs owing to lost productivity (details in Appendix A.10). In meetings with the authors, firm executives expressed strong interest in reducing attrition, and this helped motivate our study in the first place.

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 average monthly sales of roughly €200,000. In its retail activity, the firm has roughly 5,000 cashiers, plus about 500 specialists. The firm also has 1,200 workers in non-grocery-store jobs: logistics (primarily truckers), production (workers at a central food production facility), and a small number of white-collar jobs. Since we observe several years of personnel data, the number of employees observed is around 18,000.

Cashiers perform stocking and check-out functions. Most (95%) work full-time, and receive a monthly wage of roughly €350 (with minor variation depending on if location is urban or rural), plus a bonus tied to store performance (4% of wages, on average). The cashier job has no formal requirements, so anyone's friend would presumably be qualified. Applicants are pre-screened via a centralized HR process. Those who pass the initial screen are sent to a store manager, who does interviews and makes hiring decisions. About 20% of non-referred applicants are hired. New cashiers receive two days of formal training (where they are paid but do not work), followed by two weeks of on-the-job training. Cashiers were 88% of grocery worker hires during the RCT. Specialists are paid about €500 per month on average, plus a bonus similar to cashiers.

Why the firm did the RCT. In October 2015, we met with the firm's top management and suggested implementing an ERP via an RCT.[18] Having an ERP was quite natural

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

[17] As discussed below, turnover is particularly high for new hires: about half exit in the first 5 months. For comparison, about half of the call-center workers in Burks et al. (2015) exit in the first 90 days.

[18] Before running this paper's RCT, we worked with the firm on an RCT where (1) career incentives

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 investigate whether to have an ERP and in what form.[19] While we helped in designing the RCT (including the randomization of stores into treatments) and monitored the RCT's implementation through our contacts in the central HR office, the RCT was carried out by the firm.

Referral process. According to the firm's definition, an employee referral occurs when someone is hired via the firm's formalized referral process. The process was designed to make it very easy for employees to suggest a new hire and to make the time costs for employees as low as possible. To make a referral, an existing employee called a dedicated contact in HR and answered a few brief questions (name of referral, relation to employee, how long they have known 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 referral 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. Workers 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), store managers conducted information meetings with employees. During the meetings, all employees received a letter explaining the ERP, which store managers read aloud. Appendix E shows the letters. The meetings focused solely on the ERPs; managers did not tell workers that they were valued or that retention was important, nor did they discuss other worker concerns.

The centralized HR office ensured that the meetings took place. Also, HR communicated with the regional managers (to whom store managers report) who monitored that store managers were in compliance with the new ERP. Neither workers nor store managers were informed that an RCT was occurring.[20] Beyond the information provided, workers in R50,

were emphasized to workers, or (2) the CEO communicated to store managers about the importance of reducing turnover (Friebel et al., 2018). Section 4.1 compares the impact of these treatments to our results. Controlling for a store's treatment status in Friebel et al. (2018) does not affect any of our results.

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

[20]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. Regional managers were not involved in any operational or implementation aspects of the RCT, but rather solely monitored whether store managers were complying.

R90, and R120 received €15 after the referral was hired to provide an immediate reward. The remainder of €50, €90, or €120 (i.e., an additional €35, €75, €105) was paid if the referrer and referral stayed 5 months. This was clearly explained in the letter and posters, and workers hired after the RCT began were given letters explaining the ERP.

Rationale for bonus structure. We suggested a 5-month tenure threshold because a substantial share of cashiers leave in the first 5 months (about half in our pre-RCT data) while attrition is significantly lower after that. Tenure thresholds are very common in ERP bonuses (Brown et al., 2016; Burks et al., 2015; Fernandez et al., 2000; Topa, 2019). To choose bonus amounts, we surveyed non-grocery workers, who were not part of the RCT. We asked them how much money would make them willing to make a referral for a hypothetical vacancy in their unit. We suggested bonus amounts for the treatment arms corresponding to the 25th (€50 per referral), 50th (€90), and 75th (€120) percentiles of the distribution of survey responses.[21] All bonuses were paid in after-tax amounts (i.e., the firm already paid the worker's taxable share), and relative to wages were substantial. The combined post-tax bonus of €120 represents 40% of a cashier's monthly post-tax salary, which is comparable to or higher than referral bonuses examined in other studies (Appendix A.3 gives details).

RCT timing. Materials (posters, letters, and instructions for store managers) were sent to treatment stores around 11/20/2015, with instructions to implement the ERP immediately. Central HR and regional managers ensured compliance of treatment store managers with RCT procedures. We registered our RCT in the AEA Registry on 11/23/2015. In fall 2016, about a year after the RCT began, we met with top management to present the RCT results. After this, the firm decided to roll out an ERP to all firm jobs.

Safeguards to assure RCT validity. There are two immediate concerns for an RCT like ours. First, it is critical that employees in treated stores are aware of the ERPs. We address this using posters and letters to employees, and by having regional managers ensure that stores are in compliance. Also, 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, despite high employee turnover.

Second, workers need to trust that bonuses will be paid. While trust is low in many post-Communist countries, we do not think this was a concern at all for us, given the group meetings, and the paper trail from the company letters and posters. Workers were told that they could call HR about 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

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

workers it will pay, and workers are aware of this. We find no evidence of problems with procedural compliance in the surveys we carried out (explained more later).

Data. We assemble the firm's personnel and accounting data for Feb 2014-May 2017 to create worker-month and store-month panels. The personnel data are for grocery store workers, cover over 18k workers (7k active only in the pre-RCT period, and 11k active during the RCT or beyond), and contain standard personnel variables (e.g., hire and termination dates, exit codes), as well as absences, earnings, bonuses, hours, and demographics. The personnel data also include information from the firm's ERP, including who the referrer and referral are, date of referral, and relationship of referrer to referral. The main accounting variables are monthly sales, shrinkage (i.e., share of inventory lost to theft, spoilage, and other reasons), and operational profits (i.e., sales minus cost of goods minus wages minus shrinkage) by store.[22]

Besides firm administrative data, we use surveys we carried out before, during, and after the RCT. In line with Shaw (2009) and Ichniowski and Shaw (2012), the surveys covered different types of respondents: store workers, store managers, and the country's general population. Topics included reactions to the ERPs; beliefs about mechanisms; social perceptions of grocery jobs and our firm; and manager time use (Bandiera et al., 2019). Information on the surveys is discussed along the way, with details in Appendix A.4.

Randomization. The 238 stores were randomized into the five RCT arms.[23] Table 1 shows that the five store groups are well-balanced over observables. In each row of columns 1-6, we regress a pre-RCT observable 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 treatment groups 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. Columns 7-8 compares ERP stores (i.e., any of the treatments) vs. control stores, and finds no significant differences.

[22] We observe 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. Also, our worker-month panel does not cover non-grocery workers, so our analyses of non-grocery workers are more limited and use auxiliary data. Last, starting in Jan. 2017, our data do not have information on who was referred, only on who made referrals.

[23] Randomization took place on a coauthor's 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) for RCTs with multiply drawn randomization allocations. Our use of multiply drawn randomization allocations, coupled with significant correlations between many of the variables shown, contributes to the high p-values in Table 1, many of them close to 1.

3 Direct Effects: Quantity and Quality of Referrals

3.1 Impact of the ERPs on Generating Referrals

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 2 plots the share of referrals made per hire by quarter, showing a modest ratio during the RCT. After the RCT, when a single ERP is rolled out to the entire firm and the bonus increased, referrals increase, with similar referral rates across the former RCT arms.

Table 3 shows RCT impacts of ERPs on whether a hire is referred, but using regressions with various controls listed in the table notes. Standard errors are clustered by store, as ERPs are randomized by store. Column 1 of Panel A regresses whether a hire is referred on dummies for the four treatment arms (an observation is a hire), where Control is the excluded category.[24] The results are similar with controls in Column 2. Instead of using dummies for the four ERPs, Column 3 uses a dummy for having any of the four ERPs (excluded category is Control). Having an ERP increases the chance an employee is referred by 2.5pp. This is highly statistically significant, but seems economically modest.

How does one square the low rate of referrals in our RCT with the understanding that a large share of jobs are typically found via networks? As described by Topa (2019), a key distinction is between formal referrals through ERPs and informal referrals. As part of our *During RCT* survey in fall 2016, we surveyed 342 cashiers on how they found out about their jobs. For 154 workers hired during the RCT, 27% said they found out about the job through a friend or family member working at the firm, within the 25-40% of hires through informal networks reported by Pellizzari (2010) for Europe. Obtaining under 10% of hires through ERPs is also common in other firms.[25] As noted by Topa (2019), the informal passing along

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

[25] Little is known about the share of workers getting hired through ERPs since survey datasets usually measure informal referrals. Of firms listed on ReferralPrograms.org, a site primarily focused on the US tech industry (where ERPs are common (Bock, 2015)), the mean share of hires through ERPs is 33%, though a non-trivial share of firms (14%) get 10% or less of their hires from ERPs. For the four European firms listed on ReferralPrograms.org, the average share of hires from ERPs was 12%, the same percentage as in grocery jobs at our firm in the post-RCT rollout (see Section 7). Also, talking to another large grocery chain in the country where our study firm is located, that firm's share of hires from ERPs is less than 5% for grocery jobs. Thus, the fact that only a relatively modest share of grocery job hires at our firm comes from ERPs is

of job information from one person to another may differ qualitatively from the decision to formally refer someone to one's employer, e.g., there may be more important reputational considerations in the latter.[26]

The 88 referrals occur in 34 stores and are made by 75 referrers. As shown in column 1 of Table B1, referral hires are 2.7 years younger than non-referral hires, and are also 10pp more likely to be female, though the latter difference is not statistically significant. People tend to refer people like themselves demographically (Table B5), consistent with past work on referrals (Topa, 2019). Appendix A.5 provides additional facts on who makes referrals.

3.2 The Quality of Referred Workers

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.[27] Second, worker retention is a standard measure of match quality (Jovanovic, 1979). Third, past work finds that some of the largest differences between referrals and non-referrals are in attrition (Hoffman, 2017; Topa, 2019), so it is natural to study attrition when analyzing ERPs. Absenteeism is also an important outcome in low-skill jobs and is costly for our firm, but we emphasize it less, first, because the firm regards attrition as the HR outcome of greatest interest, and second, because the distribution of days absent per month is highly skewed, yielding less precision in estimation.[28]

Attrition. Panel (a) of Figure 3 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

consistent with data in other settings, particularly in the country we study and in Europe.

[26] Consistent with formal referrals differing qualitatively from informal referrals, in our data, there is no evidence that ERPs boost informal referrals. Also, in contrast to differences by formal referral status, there are no significant attrition differences between workers who are informally referred and those who are not. A different but related issue is whether workers have an incentive to report referrals formally. While there is no incentive to formally report referrals in R0, the incentive is substantial in the R50, R90, and R120 arms.

[27] See Appendix Table B8 for this correlation. High attrition also imposes serious costs on US retail firms (Ton, 2014). As a policy issue, when attrition is high, firms may invest less in workers skill development.

[28] Another common outcome in supermarkets is items scanned per minute (Mas and Moretti, 2009), but the firm's IT system does not allow us to measure worker-level items per minute. The firm's main HR key performance indicator is attrition and its secondary one is absence.

with past work, Column 1 of Table 4 shows that, compared to non-referred workers, referred workers are 7.0 pp or 44% less likely to leave each month.

Column 2 analyzes referral differences in turnover separately during a worker's first five months of tenure and also afterwards. 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.[29]

Consistent with the quantity-quality tradeoff in our conceptual framework, Column 3 of Table 4 shows that referral attrition differences are smaller at higher referral bonuses. For the R50 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=0.04 for R50 vs. R90; p=0.06 for R50 vs. R120). The survival curves in Panel (b) of Figure 3 show similar results.

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 use negative binomial regressions.[30] 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 (41%) 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.[31] Appendix Table B2 shows that referral attrition differences are similar (and slightly larger) when store fixed effects are added. Absence differences are statistically insignificant and noisy. Broadly consistent with Burks et al. (2015), there are stark referral differences in attrition, but we do not observe significant differences in our non-attrition performance variable of absence.

[29] If referrals were staying longer than non-referrals solely to get a bonus, then referred attrition would be higher than non-referred attrition after 5 months, but this is not the case. Also, that referral differences are larger in the first 5 months than after does not mean that referrals are not useful for the firm. Even if referrals stayed longer solely to get a bonus, which is not the case for us, this could still be valuable to the firm. Figure B1 shows that referrals are less likely to depart than non-referrals at most tenure levels.

[30] In Column 4, the estimated overdispersion parameter is $\alpha = 23.2 (s.e. = 0.95)$. This indicates sizable overdispersion and that negative binomial is more appropriate than Poisson (Cameron and Trivedi, 2005).

[31] In our main results on the overall impact of ERPs, we cannot control for store fixed effects because ERPs are randomized at the store level, though we can control for store fixed effects if we exploit pre-RCT data.

4 The Overall Impact of ERPs on Worker Outcomes

4.1 Results

Attrition. Table 5 shows that ERPs reduce attrition of all workers, with similar percentage effects on new hires and incumbents. Beyond showing conventional standard errors clustered by store, we also perform randomization inference for our main results (Young, 2019). The resulting p-values are similar to those from conventional clustering-by-store inference.

Column 1 of Table 5 analyzes the impact of the randomized 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 1.00pp, 0.47pp, 1.59pp, and 0.81pp, 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 0.97pp or 14%. Given that referrals are only 2.5% of hires in ERP stores, it seems unlikely that these differences are primarily due to referrals staying longer than non-referrals or people becoming more likely to stay as a result of making a referral. Comparing R0 vs. Control, recall 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 RCT. Columns 3-4 report the results from a diff-in-diff regression of attrition on interactions between treatment arm and whether the current month is during the RCT. Store dummies account for persistent differences across stores in employee attrition and other characteristics (including treatment arm during the RCT), and current month-year dummies account for differences in attrition over time. Relative to Column 1, results are slightly stronger in Column 3, with statistical significance for all 4 ERPs. The Column 4 coefficient of -1.19pp corresponds to a reduction of roughly 20%.[32]

To better understand the dynamics of the ERP effects, Figure 4 presents an event study where having an ERP is interacted with quarter of the year. ERP impacts occur in the first quarter of the RCT (i.e., Dec. 2015-Feb. 2015), though effects take a few months to fully realize. There is no pre-trend. The impact is durable throughout the RCT.[33] After the RCT ends, and an ERP is rolled out to 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. Instead of

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

[33]E.g., the ERP impact on attrition is -15% in the last quarter of the RCT. This is based on running Column 2 of Table 5 restricted to the last quarter of the RCT.

showing the difference between Control and ERP stores, panel (c) of Figure 4 shows separate regressions restricting to Control or ERP stores. Attrition is higher during summer, as for many retail jobs, and attrition has increased over time as the economy improved. Repeating panel (c) but restricting to stores with no RCT referrals, panel (d) shows similar results.

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 (c)-(d) of Figure 3 show that non-referred hires have higher survival in ERP than Control stores. Panels (a)-(b) of Figure B2 show ERP impacts interacted with quarter of the RCT for new hires and incumbents.

Panels (c)-(d) of Figure B2 show that the overall impact on attrition is driven by a decrease in voluntary attrition. ERPs had no significant impact on involuntary attrition.

Attrition magnitudes. Having an ERP reduces attrition by 15-20% durably for 13 months. As a benchmark, Friebel et al. (2018) study two treatments in an earlier RCT with the study firm. First, informing workers about career incentives (i.e., that managers are promoted from within) had no impact on turnover. Second, a letter from the CEO to store managers asking them "to do what they can" to reduce turnover led them to spend more time with employees and brought down turnover by 25% for several months before reverting back. The firm has also tried out various initiatives on their own to reduce turnover and most have been unsuccessful.[34] The RCT ERP is one of the most successful initiatives the firm has ever had in terms of reducing turnover. Besides being economically sizable, our RCT magnitude is plausible. As another benchmark, Bloom et al. (2014) show that randomly assigning employees to work from home reduces attrition by half in Chinese call centers. For lower-skill workers, attrition may be more malleable relative to output per hour and TFP.

Absences. Appendix Table B3 shows no significant impact of ERPs on absence. Column 2 (baseline) estimates a coefficient close to 0, whereas column 4 ("diff-in-diff") indicates that having an ERP reduces absence by 8%, but it is statistically insignificant.

Total hires and other store-level outcomes. Table B4 presents impacts of having an ERP on store-level outcomes using a store-month panel. Panel A uses only data from the RCT, whereas Panel B exploits the pre-RCT period to add store fixed effects, as in columns 3-4 of Table 5. As seen in column 1 of Table B4, total store hires decline by 0.13-0.22 hires per month. This decrease of 10-19% per month is consistent with the 15-20% drop in turnover from having an ERP. The impact is statistically significant at the 10% level in Panel B, but not in Panel A. Also, having an ERP does not have a statistically significant

[34] E.g., before we started working with the firm in 2015, the firm tried out increasing training for cashiers, introducing this gradually across stores. This failed to reduce turnover.

effect on stores' monthly shrinkage, sales per worker, operational profit per worker, or total hours worked. Still, the coefficients on hires, shrinkage, sales, and operational profit have a sign indicating benefit to the firm. ERPs are estimated to increase operational profits by 2 to 2.3% and to increase sales per worker by 2%. For these non-preregistered outcomes, we lack statistical power to detect small to moderate changes using only store-month data.[35]

4.2 Mechanisms for Overall ERP Impacts

The most natural reason an ERP would reduce turnover is by promoting referrals, as referrals are less likely to quit and referrers may be more likely to stay to get a bonus. However, Section 4.2.1 provides evidence that promoting referrals explains only a modest share of the impact of an ERP. Section 4.2.2 next discusses additional mechanisms that, while plausible *ex ante*, are *ex post* inconsistent with the RCT results. Section 4.2.3 discusses mechanisms that are consistent with the RCT evidence, including our preferred mechanism of workers valuing being involved in hiring, and uses surveys to tease these apart.

4.2.1 Assessing Referrals as the Mechanism

How much of the effect of ERPs on attrition (a 15% reduction) comes via effects related to referrals, i.e., getting more referrals or making referrers more likely to stay? The simplest evidence against referrals as the main mechanism comes by comparing R0 and Control stores. R0 stores have roughly 15% lower attrition than Controls stores, even though the R0 treatment induced no referrals.

A second way to address this question is mediation analysis. We repeat the analyses in columns 1-2 of Table 5, but additionally control for whether someone is referred and/or the number of referrals a person has made to date. The estimates imply that only 5% of the impact of having an ERP on attrition is mediated via having more referral hires and having workers made more referrals to date, whereas 95% of the impact remains unexplained. Also, relative to someone who has not made a referral, someone who has made a referral is no more likely to stay on average, though they are more likely to stay in the first 5 months after a referral. For brevity, we present mediation results in Appendix A.6.

Last, Appendix Table B6 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

[35]Thus, in analyzing impacts on profits in Section 5, we will combine the treatment effects on attrition with two different values on the cost of turnover, including one intended to account for lost sales following Blatter et al. (2012). We also note that operational profit in Table B4 is not a full measure of profit (e.g., it does not account for personnel costs at the central HR office).

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 the ERP.[36]

4.2.2 Unlikely Mechanisms for Non-referral Channel

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

ERPs help the firm improve hiring decisions. Perhaps having an ERP helps store managers learn about what type of candidates to target, or frees up additional time that would be spent on interviews? This also is 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 new hires. This mechanism cannot explain why ERPs reduced incumbent attrition.

Other concurrent policies or managerial reactions in treatment stores. Throughout the RCT, the firm did not differentiate any management practice by treatment status. Recall that store managers were not aware there was an RCT. Further, having an ERP did not affect firing or self-reported store manager time use (time use details in Appendix A.7).

Control store frustration. Instead of workers in treatment stores being less likely to quit, perhaps workers in control stores became more likely to quit, if they happened to hear about the ERPs in other stores, a particular form of a treatment spillover. There is evidence against this interpretation. First, HR was told to accept referrals from control stores if employees called to make them, but they did not get any referrals from control stores. Second, we instructed HR to record any complaints that it received from control stores about there not being an ERP, but there were no complaints made. Third, in all the surveys we conducted, both during and after the RCT, we never heard a worker mention anything about control store frustration.

4.2.3 Possible Mechanisms for Non-referral Channel

The impact of an 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 the ERP is rolled out to control stores. What explains this? It should be a mechanism or mechanisms

[36] Results are similar if we restrict to stores with no referral hires (instead of no referrals made). Panels (c) and (d) of Figure 3 show that non-referrals have better survival in ERP than Control stores.

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 not be absent. Such mechanisms may include:

1. *Employees feel respected after being asked to be involved in hiring or liked having some say about who they might work with.* Workers may value being involved in hiring, perhaps because it makes them feel respected (Ellingsen and Johannesson, 2007), or because it gives workers some voice (Hirschman, 1970; Turco, 2016) or some real authority (Fehr et al., 2013; Bartling et al., 2014) in hiring.
2. *The introduction of an ERP is a positive signal about the firm being a better place to work.* Instead of being simply about hiring or whom a worker gets to work with, an ERP may increase a worker's perception of the overall quality of the firm, e.g., having a costly ERP may raise a worker's expectation of the firm's future profitability.
3. *Workers think they may make referrals in the future.* Even if few workers took advantage of the ERP during the RCT, workers could imagine that there would be some chance that they would use the benefit in the future.
4. *ERPs increase informal referrals.* The ERPs could have increased "informal referrals," i.e., people who may have informally heard about the job from a friend, but where the friend was not willing to call HR to register the referral.[37]

To shed further light on these explanations, we conducted phone surveys with 222 store managers and an in-store electronic kiosk survey with 113 store workers from the study firm. We explained that ERPs had reduced attrition at the firm separate from generating referrals, and asked them their opinion on which of the above four mechanisms (or a 5th option of a mechanism of their own choosing) was most likely to explain the result. We randomized the order in which the four mechanisms were presented.

Panel A of Table 6 shows that (1) was by far the most common explanation, chosen by 66% of managers and 50% of workers. There are modest differences between workers and managers, e.g., a larger share of workers supports (3), but the overall message from both groups is the same.[38]

Is it possible to parse further into whether (a) employees felt respected about being involved in hiring or (b) whether they liked having some say about who they might work with? We asked workers choosing (1) to specify whether (a) or (b) was the main reason or whether both were equally likely. As seen in Panel B of Table 6, 15% said (a), 17%

[37]Recall that ERPs fail to boost informal referrals in our *During RCT* worker survey (see footnote 26).

[38]For ERPs to credibly signal respect, workers must believe that candidates they refer will be hired. Indeed, 97% of referred candidates were hired compared to roughly 20% of non-referred candidates. Also, most survey respondents indicated that referred candidates would likely be hired, giving a mean of 6.1 on a scale between 1 (don't believe a referred friend would be hired) to 7 (are sure a referred friend will be hired).

said (b), and 67% said both were equally likely. While (a) and (b) may be conceptually distinct, workers view them as closely related.[39] We refer to (a) and (b) together as workers valuing being involved in hiring. While researchers have not previously considered that workers valuing being involved in hiring is a mechanism for the impact of ERPs, it is highly consistent with evidence from practitioners, as we discuss further in Appendix A.8.

Beyond surveying firm managers and workers about reasons for the indirect effects, a complementary approach to identifying mechanisms is to use a vignette (e.g., Kaur, 2019). In late 2018, we surveyed a representative sample of 548 US workers, what we call the *Vignette Survey of US Workers*. This allows us to study whether the mechanism we identify may hold in other contexts. We provided the following vignette (with bolding as in the original):

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

In the survey, 68% of workers said having an ERP would make the employee feel more respected, whereas only 11% said it would make the employee feel less respected, and 21% who said they were uncertain. Appendix A.9 provides details on the *Vignette Survey*.

Overall, our evidence indicates that most of the impact of ERPs does not come generating referrals. Rather, the explanation most supported by the survey evidence (intra-firm and US vignette) and intra-firm data patterns is that workers feel respected after being asked to be involved in hiring or value having some say about who they might work with.

5 The Impact of ERPs on Firm Profits

We use the results from Sections 3-4 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 significantly affect absence, sales, or shrinkage, we focus only on attrition impacts. ERPs may also reduce recruiting costs (e.g., due to less time interviewing candidates), but we set that 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 an ERP in

[39] This is unsurprising. Part of why someone may feel respected is that the firm is allowing them to help influence who they might work with.

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). Direct costs account for job advertising costs and the time spent by employees in hiring someone. Full costs additionally account for lost productivity costs, which are hard for us to precisely detect experimentally, but which we account for following Blatter et al. (2012). Appendix A.10 gives further detail on c.

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.11 gives further details 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 from an ERP is €2.44 per worker-month, far above the cost per worker-month of €0.10. The overall net profit per worker-month is €2.34, or 0.6% of the labor costs. Only 5% of the turnover benefits accrue from ERPs yielding referrals, who have lower attrition. Rather, most of the benefit accrues from non-referral hires having lower attrition and from incumbents having lower attrition in ERP stores. Under $c =$€1,150, ERPs becomes even more profitable, increasing profits by €11 per month, or 2.8% of labor costs, a substantial benefit in a competitive industry like grocery retail. If having an ERP is evaluated based on lower turnover from referrals, the benefits still outweigh the costs, though both are relatively comparable under $c =$€250. The comparison is 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 the RCT, the benefits are non-monotonic, reflecting differences in the quality and prevalence of referrals between treatment arms. This broadly supports P4, 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, focusing only on the referral benefits of ERPs yields vastly different conclusions regarding the profitability of an ERP (e.g., based solely on referral benefits, there is little profit benefit from R120). R0 yields large profit gains despite producing 0 referrals.

Panel C looks at profits under the post-RCT firmwide ERP rollout (discussed in further detail in Section 7). The share of turnover benefits from referral hires is 14%, which is higher than during the RCT, but most benefits are still not from referral hires.

6 Heterogeneity in ERP Impacts on Attrition

While we focus on the pre-registered overall impact of ERPs on attrition, we here examine heterogeneity based on two dimensions often discussed in the management practices literature. ERP impacts are larger in higher-performing stores and better local labor markets.

Store performance. A key question in experimentally evaluating any management practice is how do effects vary based on initial performance of the treated units? As seen in Panel A of Table 8, the direct effect of having an ERP on referrals does not significantly vary with store productivity, though results are in the direction of larger effects in higher-performing stores. In contrast, columns 1-3 of Panel B of Table 8 shows that the overall effect of ERPs on attrition is larger in stores that are more productive pre-RCT. As above, we use three standard measures of retail performance: shrinkage rate, Log(Sales per Worker), and Log(Operational Profits per Worker). We normalize each variable to ease interpretation.[40]

Column 1 shows that for a store at the mean level of shrinkage, the impact of having an ERP on attrition is -0.94pp per month or roughly -15%. However, at the 90th percentile of store shrinkage (i.e., stores with the worst shrinkage), the impact of ERPs on attrition is only -0.12pp or -2%, whereas at the 10th percentile, the ERP impact is -1.8pp or -27%.[41] Similar results are found measuring store performance using sales or operational profit. For a store at p10 of operational profit, the impact of ERPs on attrition is only -1%, whereas for a store at p90 of operational profit, the ERP impact is -25%. Table B11 shows similar findings in split samples based on above/below median pre-RCT performance, thus showing that our heterogeneity findings are not driven by outliers or our use of linear interaction terms. In contrast, ERP impacts do not vary based on stores' pre-RCT attrition rates.

An interpretation of these results is that the ability of ERPs to generate feelings of involvement and respect is higher in higher-performing stores. In lower-performing stores, the notion that the firm respects its workers may be less credible. The results are also consistent with complementarity between ERPs and the other management practices that drive performance in those stores.[42] Thus, our results are broadly consistent with work

[40] Table B8 shows that heterogeneity variables are correlated in expected directions. Still, all correlations, besides that between sales and operational profit, are below 0.6 in magnitude, indicating that our different heterogeneity analyses are not all exploiting the same comparisons across stores. To assess how direct benefits of ERPs vary by store performance, one also needs to know how referral/non-referral differences vary. Table B9 shows some evidence that referral/non-referral attrition differences are larger in higher-performing stores, at least with respect to shrinkage as the performance measure, though standard errors are reasonably large.

[41] At p10, the ERP impact is -0.96+1.28*0.66=-0.12pp. At p90, the ERP impact is -0.96-1.28*0.66=-1.8pp.

[42] Our results are consistent with instead of indicative of complementarity because we do not measure whatever practices may drive performance differences across stores. Non-management explanations for performance differences are possible but seem unlikely. Product selection is generally similar across stores, with the vast majority of RCT worker-months (over 90%) occurring at stores offering a full-service format.

showing HR management (HRM) practice complementarity (Ichniowski et al., 1997; Boning et al., 2007), as well as supportive of theories of management practice complementarity.

Local labor markets. Another key question in analyzing an HRM practice is how do effects vary by labor market quality? This is hard to answer in most RCTs because it requires observing effects across many labor markets. Beyond work on management practices, as noted by Topa (2019), examining heterogeneity in referral results by strength of the local labor market is important because theories predict that referrals do different things in tight and slack labor markets (Calvo-Armengol and Jackson, 2004). We exploit substantial cross-municipality variation in 2015 unemployment rates: across the 238 stores, which are in 78 municipalities, mean unemployment is 7.7, the SD is 2.3, the min is 4.8, and the max is 15.4. A municipality approximates a worker's local labor market in the country we study.

The direct effect of ERPs does not vary by labor market quality (column 5 of Panel A of Table 8). However, Panel B shows that the overall effect of an ERP on attrition is significantly larger in better local labor markets. If the municipal unemployment rate is at p90 (i.e., the local labor market is bad), our results imply that the impact of ERPs on attrition is -0.24pp per month or -4%. In contrast, if the unemployment rate is at p10, the ERP impact on attrition is -28%. In tight labor markets, workers have more options, so attrition may respond more strongly to HR practices that make workers feel more respected. Our results are broadly consistent with Burks et al. (2015), who show that differences between referrals and non-referrals tend to be larger in stronger local labor markets. The difference is we study the impact of ERPs, which are primarily driven by indirect effects, whereas Burks et al. (2015) examine referrals vs. non-referrals.[43]

Profits. Panels C-D of Table 8 show that our attrition heterogeneity estimates imply strong heterogeneity in profits. At stores in p90 of pre-RCT performance (based on shrinkage,

Product selection does not drive our results, as the Table 8 results are robust to restricting to full-service stores or to including interaction terms of ERP*(# of products offered) or ERP*(Share of products that are fresh goods), as seen in Table B10. Workplace technology is also similar across stores, and results are robust to controlling for an interaction of ERP with the number of store checkouts (total, manned, or self-checkout). Our performance heterogeneity is not just reflecting store size, as results are robust to controlling for ERP*(Head count) or ERP*(Store square meters). Competition from Lidl does not explain the results, as results are robust to including the interaction term ERP*(Dummy for Lidl store nearby). Demand shocks seem unlikely to account for us finding similar results on different performance measures, not only on sales and profits, but also on shrinkage, which is strongly affected by theft and thus presumably less affected by demand shocks. We are agnostic as to whether ERPs may be complementary with respect to management practices or the quality of store managers, as the two are often quite correlated (Bender et al., 2018). Bloom et al. (2019) document substantial intra-firm, cross-plant variation in management quality. Based on numerous visits of the authors to firm stores, operational management practices appear relatively similar across stores, suggesting that differences in management are likely to reflect differences in HRM practices.

[43]Our data also show that differences between referrals and non-referrals are larger in better local labor markets (see Table B9). However, that indirect benefits of ERPs increase in good local labor markets is far more consequential for profits compared to the direct benefits increasing in good local labor markets.

sales, and profits) or local labor market quality, ERPs decrease labor costs by around 5%. For stores at p10 of performance or labor market quality, the impact is close to 0.

Other heterogeneity. For brevity, Appendix A.12 discusses worker demographics. ERP effects on attrition are larger among men than women, but do not vary by age. In addition, having an ERP modestly increases the share of hires who are female.

7 Firmwide ERP Rollout

Because of the benefits shown in the RCT, the firm decided to roll out an ERP to the whole firm (excluding management), including grocery stores previously in the control group. Management was interested in reducing turnover further, and taking into account that referrals were increasing with bonus size in the RCT, they decided to make bonuses more attractive. Under the new firmwide ERP, employees receive €30 when a referral is hired, plus an additional €100 if both parties stay 3 months. Relative to the RCT bonuses, twice as much money is paid at hire; the duration that referrer and referral must stay is 2 months lower; and total payment (€130) is higher than in all RCT arms. As the ERP was extended to non-grocery workers, namely, food production and logistics workers, we examine how ERPs work in these jobs, which are perceived as more attractive (based on surveys discussed below).

Other than covering the whole firm and using a larger bonus, the new firmwide ERP is similar to the RCT ERPs. Referrals are made by calling HR. The new ERP was introduced using posters, letters, and meetings (see Figure E1). As before, the firm did not accompany the new ERP by emphasizing that workers were valued or that retention was important.[44]

Base results. Among grocery store workers, Table 10 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.[45] Interestingly, however, among non-grocery workers in production and logistics, the post-RCT ratio was 37%. For grocery workers, as noted earlier in Figure 4, the RCT treatment effect on attrition vanishes once an ERP is rolled out to control stores. This is corroborating evidence that ERPs affect attrition.

In sum, while front-loading and increasing the bonus increased referrals for grocery jobs, the ratio of referrals made to hires only increased to 12%; this suggests that front-loading plays a role, but is unlikely to be the main cause of our RCT finding that ERPs modestly boosted referrals for grocery jobs. Second, the referral rate was substantially higher

[44] There are two limitations to keep in mind for the new ERP. First, unlike for the RCT, we only have data on who made referrals, not who was referred. Second, because our personnel data do not cover non-grocery workers, we can only match data on who made referrals to individual personnel records for grocery workers.

[45] Panel (a) of Figure 2 shows that the ratio is similar across the 5 arms of the previously completed RCT.

for non-grocery jobs.

Responsiveness to bonus level across jobs. For non-grocery jobs, there was no ERP before the firmwide rollout. Thus, it is somewhat challenging to use the non-grocery evidence to examine whether referrals are more responsive to bonuses in good jobs than in bad jobs.[46] However, Table 10 shows that we can provide evidence by separating grocery jobs into cashier and non-cashier grocery jobs (e.g., butcher, baker, assistant manager), with non-cashier jobs seen as more attractive. During the RCT, the ratio of referrals made by the group to hires was 5% for non-cashier grocery jobs compared to 3% for cashier jobs. Post-RCT, the ratio was 17% for non-cashier grocery jobs, and 11% for cashier jobs.

7.1 Using Surveys to Understand the Rollout Results

As during the RCT, why are there relatively few referrals for grocery jobs during the rollout? Why are there more referrals for non-grocery jobs, i.e., those in logistics and food production? The answer supported by surveys is that non-grocery jobs are more attractive, and people prefer to refer friends to more attractive jobs. While there are differences between grocery and non-grocery jobs other than attractiveness, these differences seem to reinforce our story.[47] Being more willing to refer for better jobs is also consistent with the model in Appendix D.

Employee and manager surveys on why the RCT ERP generated few referrals. Table 9 shows that managers and workers 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 on why ERPs had little impact on getting referrals. Undergrads in a lab classified the reasons into 10 categories. 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 (Column 1 of Panel A). In Column 2, the share rises to 68% if we exclude the mechanical explanation of no open jobs, the response that ERPs worked well, and instances where managers gave no reason. Panel B of Table 9 shows that similar findings apply to workers. We gave cashiers the six most frequently mentioned reasons from the manager survey and asked them to rank them.[48] 51%

[46] As far as we know, formal referrals were not being made for non-grocery jobs before the firm's rollout 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 of €0, and 37% referrals made per hire at the bonus of €130.

[47] For production and logistics jobs, 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. Most logistics jobs are truckdriver positions requiring a license. Food production jobs require working at a central facility (unlike the grocery store jobs which are located around the country). Such restrictions should work against generating more referrals.

[48] 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.

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.

Other reasons received limited support. On reputational concerns vis-a-vis the firm (as opposed to vis-a-vis friends), 12% of managers gave a response about people not making referrals to avoid embarrassment. Likewise, only 16% of workers thought "Employees don't want to be responsible if their friend doesn't do a good job" was the main reason for the limited impacts observed.

General public surveys on occupational attractiveness and why there were more referrals for non-grocery than grocery jobs in the rollout. Given the above surveys results, we wanted to learn more about how cashier jobs are regarded compared to others. Thus, we did a survey of the general population in the host country. Panel (a) of Figure 5 shows ratings of different occupations' attractiveness. Cashier jobs, comprising 90% of grocery hires in the RCT, score the lowest. Non-grocery jobs at the firm, namely, those in logistics and food production, rate substantially higher.[49] Instead of grocery jobs being unattractive, could results be driven by our firm being unattractive? Panel (b) of Figure 5 indicates not, as our firm is well-regarded relative to other retail firms in the country.

In a second survey of the general public (*General Population Survey 2*), we also asked why there were more referrals for non-grocery than grocery jobs in the rollout. 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, 74% of respondents ascribed the difference in referral rates between grocery and non-grocery jobs to grocery jobs being undesirable. Appendix A.13 discusses alternative explanations.

Vignette evidence. As discussed in Appendix A.9 for brevity, we also find in our *Vignette Survey of US Workers* (see Section 4.2.3) that US workers describing their current job as more attractive are more willing to refer a qualified friend for a hypothetical opening.

8 Concluding Remarks and External Validity

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

[49] While Panel (a) of Figure 5 accords with many aspects of occupational prestige in the US, there are 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.

On (1), we find that larger bonuses increase referrals and that referrals are higher quality than non-referrals, though the share of referrals is modest, at least relative to statistics from studies of informal referrals. Larger bonuses decrease referral quality, and the direct profit benefits of ERPs are non-monotonic in the bonus. The level of referrals and the responsiveness of referrals to bonuses is higher for more attractive jobs. These results support models where workers are altruistic toward friends (e.g., Bandiera et al., 2005, 2009), and run contrary to popular claims that ERP bonus size does not affect referrals (Bock, 2015).

However, the most important finding of our paper concerns (2). Namely, the firm's ERPs have substantial indirect benefits. ERPs reduce turnover by 15-20% and these effects are present even in stores where no referrals are made. 95% of the profit gains of ERPs come from indirect benefits, and profit gains are larger in stores with better pre-RCT performance and in better local labor markets. Though we cannot fully rule out alternative explanations (e.g., workers hope to make referrals in the future), surveys suggest that indirect benefits arise from employees valuing being involved in the hiring process and having some say over who they would work with. These results may explain why ERPs are a common management practice. Beyond ERPs, our results help rationalize why firms seek employee participation in hiring (beyond the importance of using worker information for selection), and support the Ellingsen and Johannesson (2008) model of respect in the workplace. Beyond hiring, the notion that HR practices that involve workers may cause them to feel more respected may be relevant for many workforce practices, such as idea suggestion systems and 360° evaluation.

At our last discussion with management in May 2019, about 2.5 years after the RCT ended, the firm continued to use the rolled-out ERP. This longer-run persistence of a change in management practice echoes Bloom et al. (2020). In informal interviews in late 2018, both executives and store managers report high satisfaction with having an ERP. According to auxiliary records from the firm, referral rates remain sizable at roughly 30% for non-grocery jobs, and remain fairly low (but non-trivial) at roughly 10% for grocery cashier jobs.

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

On (2), would ERPs generate indirect effects in other contexts? As seen in Table 10, we observe substantial indirect effects for different jobs and ERPs, suggesting that indirect effects could be substantial for other low-skill jobs. While our analysis does not cover high-skill jobs, we speculate that ERPs may still generate indirect benefits from workers valuing

being involved in hiring. Of course, workers in high-skill jobs may feel more respected than workers in low-skill jobs, so it is possible that indirect benefits of ERPs would be lower in high-skill jobs.[50] On the other hand, if people are more willing to make referrals for better jobs, then there may be more opportunities to be involved in hiring for high-skill jobs than for low-skill jobs, possibly making respect benefits larger for high-skill jobs.

To go beyond casual observation and speculation on the external validly of (2), note that our *Vignette Survey of US Workers* (see Section 4.2.3) strongly supported that ERPs may also make US workers feel more respected. In that survey data, US workers with a bachelor's degree or higher are 10pp (s.e.=4.0pp) more likely than less-than-bachelor's workers to say that having an ERP would make an employee feel more respected. This is consistent with the possibility that indirect benefits of ERPs occur in contexts with higher-skill workers.

We look forward to future RCTs that can examine the ideas from our paper in other settings, both other low-skill settings and high-skill ones.

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

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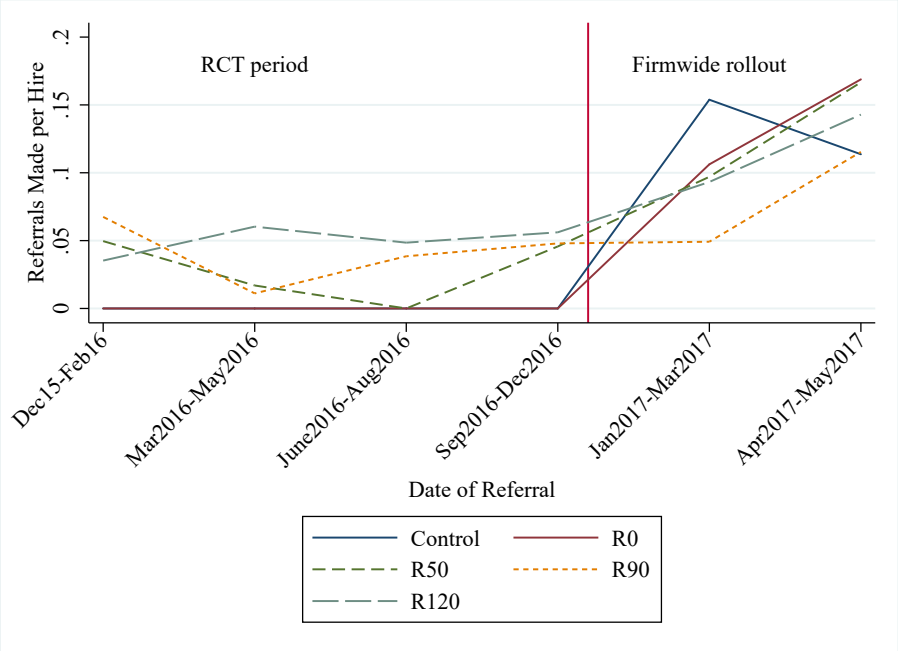
Figure 1: Referral Program Posters Used During RCT (translated and with firm identifiers redacted)

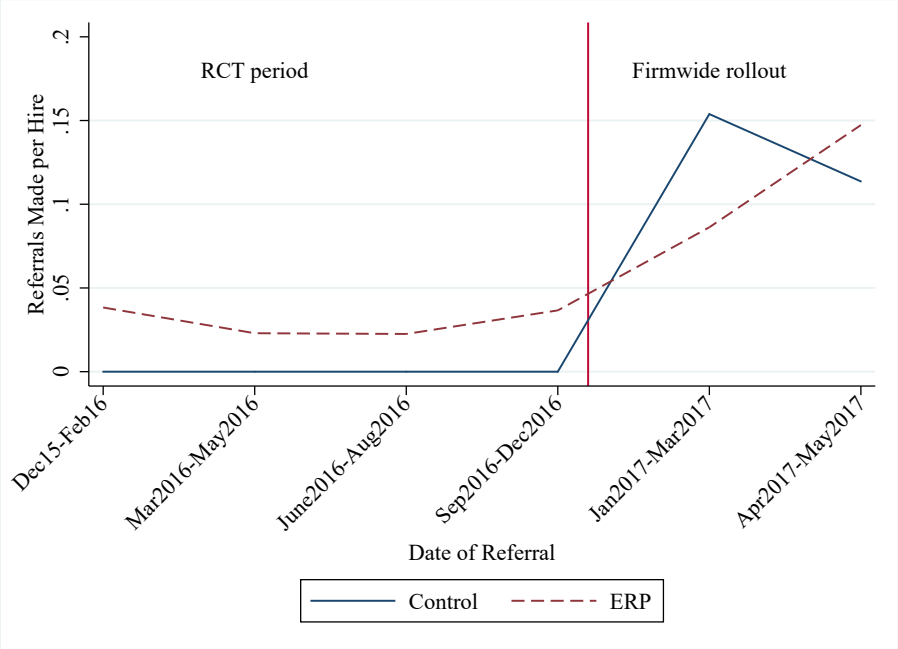
(a) Information Only Arm (R0)

(b) Information + Bonus Arms (R50, R90, R120)

Figure 2: Referrals Made over Time in the RCT and Firmwide Rollout



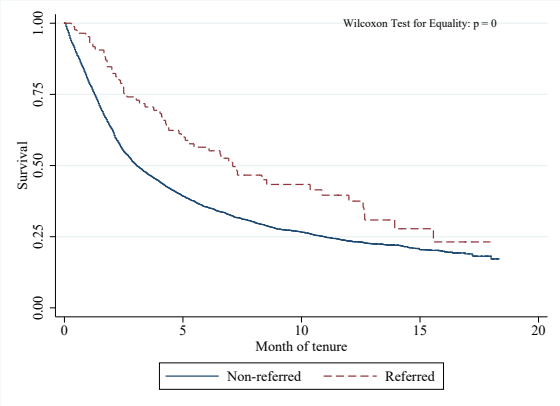
(a) Five Arms



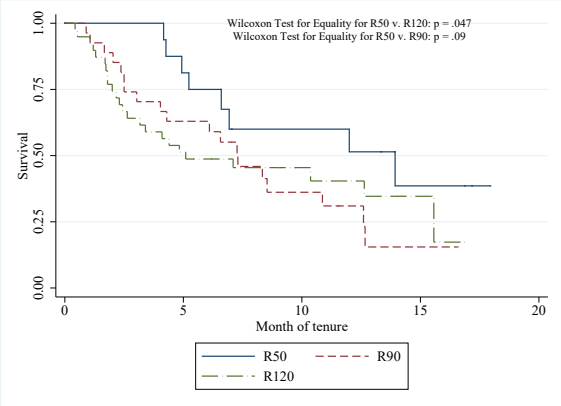
(b) ERP vs. Control

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 2016m9-2016m12 and 2017m1-2017m3, and separates the RCT period from the firmwide rollout. Panel (a) shows the 5 arms and panel (b) shows control vs. ERP stores. In panel (b), over the four quarters of the RCT, the number of referrals made is 24, 17, 21, and 26, whereas the ratio of referrals per hire is 3.8%, 2.3%, 2.3%, and 3.7%. The ratio is lower in June-August 2016 because there is more hiring then.

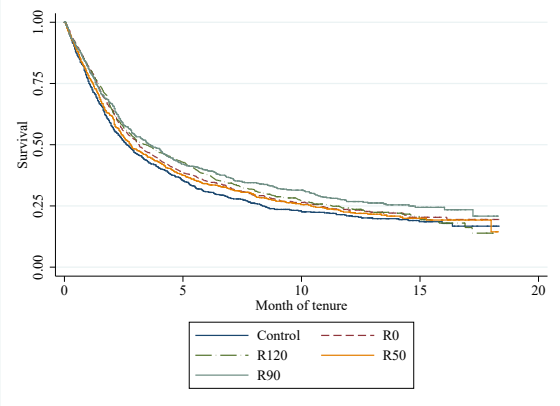
Figure 3: Survival Curve Comparisons



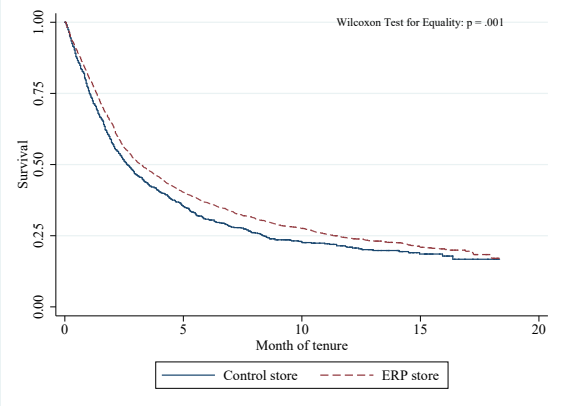
(a) Referrals vs. Non-referrals



(b) Referral Survival by Bonus Level



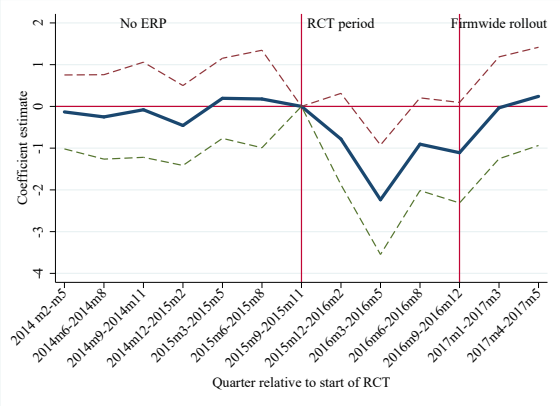
(c) Survival of Non-Referrals by Bonus Group



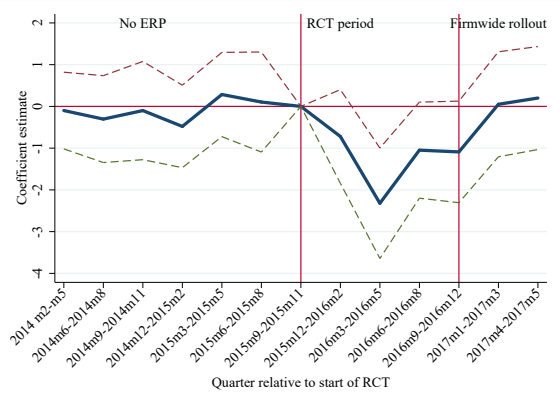
(d) Survival of Non-Referrals, by ERP Stores vs. Control

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 an ERP, 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.

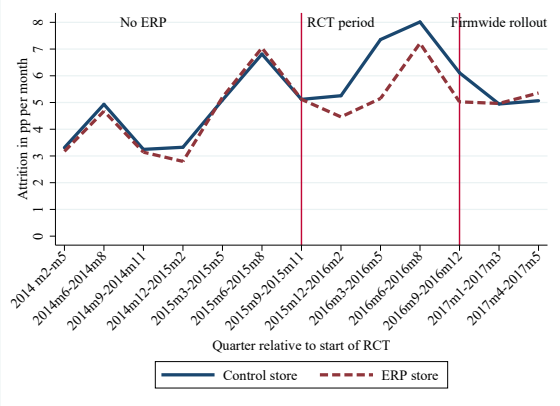
Figure 4: Event Studies: ERP Lowers Attrition during the RCT, and the Effect is Reversed Once ERP is Rolled out to Control Stores. ERP Effects are Similar in Stores with No Referrals During the RCT.



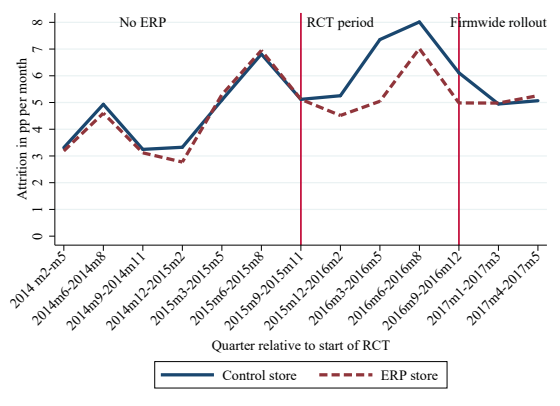
(a) ERP Impact in All Stores



(b) ERP Impact in Stores with No Referrals



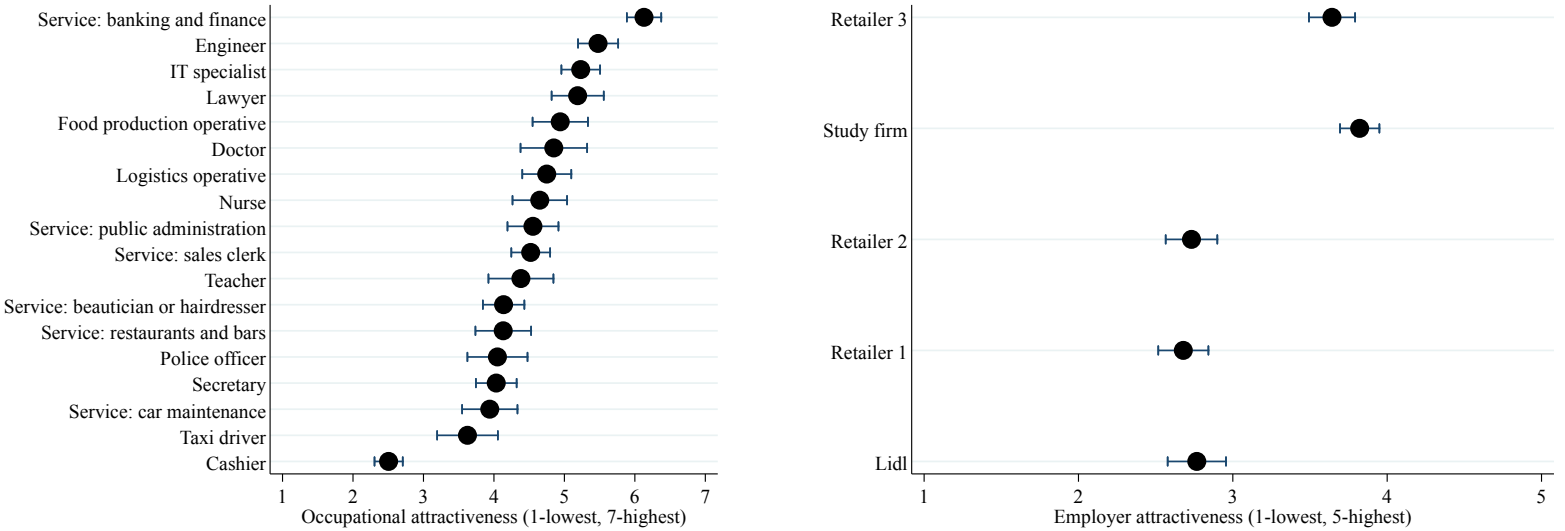
(c) ERP vs. Control in All Stores



(d) ERP vs. Control, Stores with No Refs

Notes: **Panel (a)** analyzes the impact of having a randomly assigned ERP (i.e., one of the four RCT ERPs) on attrition. The solid line denotes the coefficient estimates, with the dotted lines denoting the 90% confidence intervals. The regression used in plotting the event study is similar to column 4 of Table 5. The difference is that, instead of having an ERP*RCT dummy (i.e., an ERP store dummy times a dummy for the current month being during the RCT), we interact the ERP dummy with year-quarter dummies. The omitted quarter is the last quarter before the RCT, 2015m9-2015m11. **Panel (b)** repeats Panel (a) while restricting attention to workers in stores where no referrals are ever made during the RCT. **Panels (c) and (d)** perform regressions separately for ERP and Control stores. We regress attrition on year-quarter dummies (where the quarter before the RCT, 2015m9-2015m11, is the omitted category), as well as store dummies, a 5th order polynomial in tenure, age controls (with age in 6 bins, plus a dummy for age being missing), gender (including a dummy for gender being missing), and a dummy for being a cashier. All coefficients shown are normalized relative to Control store mean attrition in the quarter before the RCT (i.e., we show the year-quarter regression coefficients plus Control store mean attrition in 2015m9-2015m11). Panel (c) uses all stores. Panel (d) repeats panel (c) restricting attention to workers in stores where no referrals are ever made during the RCT. Note that some "quarters" are not three months, reflecting that the pre-RCT, RCT, and post-RCT period are not multiples of three months. The RCT begins toward the end of 2015m11 (on 11/20/2015), but results are robust to dropping 2015m11.

Figure 5: Attractiveness of Occupations and Grocery Chains in our Firm's Host Country



(a) Occupations

(b) Grocery Chains

Notes: Dots are mean attractiveness ratings, and whiskers are 95% confidence intervals for the means. The data are from our *General Population Survey 1* in early 2017. The sample is people from the general public of the country where our retailer is based. In panel (a), in measuring occupational attractiveness, we use an approach similar to that in the General Social Survey (Smith and Son, 2014). Specifically, to reduce survey time and ensure maximize time, respondents are asked about 6 occupations from our overall list of 18 occupations. Each respodent saw the cashier occupation plus 5 other occupations. In the survey in panel (b), each respondent is asked about all 5 grocery retailers (our study firm, 3 other local retailers, and the German chain Lidl).

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

	Comparing All 5 Arms						ERP vs. Control	
	Control (1)	R0 (2)	R50 (3)	R90 (4)	R120 (5)	p-val (6)	ERP (7)	p-val (8)
Outcome Variables								
Monthly hires	1.05***	0.13	0.12	0.16	0.34	0.77	0.19	0.22
	(0.12)	(0.18)	(0.20)	(0.20)	(0.27)		(0.15)	
Attrition rate	5.01***	0.29	0.32	0.30	0.27	0.98	0.29	0.52
	(0.42)	(0.54)	(0.57)	(0.58)	(0.59)		(0.46)	
Quit rate	5.40***	0.35	-0.17	0.55	-0.09	0.82	0.16	0.78
	(0.51)	(0.76)	(0.69)	(0.72)	(0.69)		(0.56)	
Fire rate	0.78***	0.15	0.05	0.03	0.19	0.92	0.10	0.59
	(0.18)	(0.25)	(0.23)	(0.23)	(0.24)		(0.19)	
Absences per worker	1.23***	0.10	-0.10	-0.08	0.10	0.25	0.00	0.96
	(0.08)	(0.12)	(0.13)	(0.11)	(0.11)		(0.09)	
Sales in 000's of €	209.78***	1.08	-16.13	-2.22	0.71	0.97	-4.14	0.87
	(23.34)	(30.01)	(31.07)	(32.16)	(33.82)		(25.56)	
Log(Sales per worker)	9.01***	-0.01	-0.02	-0.02	0.01	0.86	-0.01	0.67
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)		(0.02)	
Log(Operational profit per worker)	7.44***	0.00	-0.02	-0.02	-0.01	0.99	-0.01	0.68
	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)		(0.03)	
Log(Shrinkage ratio)	-3.58***	0.03	-0.03	0.00	-0.03	0.72	-0.01	0.83
	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)		(0.04)	
Non-outcome Variables								
Head count	25.11***	0.58	-0.98	0.75	0.08	0.99	0.11	0.97
	(2.70)	(3.55)	(3.76)	(3.93)	(3.89)		(2.99)	
In big city	0.37***	0.11	0.07	0.09	-0.06	0.42	0.05	0.51
	(0.07)	(0.10)	(0.10)	(0.10)	(0.10)		(0.08)	
Lidl store nearby	0.24***	0.05	0.03	-0.05	-0.05	0.66	0.00	0.95
	(0.06)	(0.09)	(0.09)	(0.09)	(0.09)		(0.07)	
2015 unemployment rate in a store's municipality	7.85***	-0.26	-0.44	0.06	0.06	0.79	-0.15	0.69
	(0.33)	(0.45)	(0.48)	(0.49)	(0.48)		(0.37)	
Store size (square meters)	648.55***	13.31	-35.44	4.29	-15.70	0.97	-8.38	0.90
	(58.78)	(75.23)	(79.57)	(82.38)	(79.43)		(64.10)	
Share of store workers who are cashiers	77.76***	1.87	2.26	1.02	1.54	0.62	1.68	0.16
	(1.08)	(1.52)	(1.53)	(1.55)	(1.47)		(1.20)	
Share female	88.93***	-1.14	0.60	-0.49	0.51	0.84	-0.13	0.92
	(1.18)	(1.70)	(1.66)	(1.75)	(1.72)		(1.33)	
Worker age	32.31***	0.12	0.36	-0.57	0.68	0.62	0.15	0.83
	(0.63)	(.81)	(0.85)	(0.88)	(0.81)		(0.69)	

Notes: This table compares pre-RCT store-level characteristics across the different treatment arms. Each row contains two store-level OLS regressions ($N = 238$). In columns 1-6, we regress characteristics on dummies for the four treatment arms. The estimated constant corresponds to the mean in the control group. The p-value in column 6 correspond to the test for joint significance of the treatment dummies. Columns 7-8 lump all treatment stores together and compares ERP versus Control stores. There are 46 stores in the control group, and 48 stores in each of the 4 treatment groups. "Head count" is the number of employees in a store. Note that the breaking of attrition into quits and fires is only available starting in 2015m4. The "quit rate" is the rate of voluntary attrition. The randomization was not stratified, but as noted in footnote 23, we drew randomization allocations numerous times, with an eye for detecting balance on several variables. The pre-RCT period is 2014m2-2015m10 (excluding 2015m11 since the RCT began midway through that month).* significant at 10%; ** significant at 5%; *** significant at 1%

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

	Control ($N = 46$)	R0 ($N = 48$)	R50 ($N = 48$)	R90 ($N = 48$)	R120 ($N = 48$)
Panel A: Summary of the Five RCT Arms					
Information to encourage referrals (posters, letter, meeting)	No	Yes	Yes	Yes	Yes
Bonus paid to referrer after referral is hired	0	0	€15	€15	€15
Bonus paid to referrer if both referrer & referral stay 5 months	0	0	€35	€75	€105
Panel B: Total Hires, Referrals Made, and Referrals Hired					
Number of Hires	763	748	750	709	841
Number of Referrals Made	0	0	18	28	42
Number of Referral Hires	3	0	16	27	39
Referrals as Share of Hires	.004	0	.021	.038	.046

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	0.034**	0.037***	
	(0.014)	(0.013)	
R120	0.042***	0.041***	
	(0.014)	(0.011)	
ERP			0.025***
			(0.006)
Observations	3,811	3,811	3,811
Controls	No	Yes	Yes

Notes: Standard errors clustered at the store level. An observation is a grocery 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, square footage, region dummies, whether the store is in a big town, and whether there is a Lidl store nearby), year-month of hire dummies, age controls (with age in 6 bins, plus a dummy for age being missing), gender (including a dummy for gender being missing), and a dummy for being a cashier. The implied p-values here are also similar when made using randomization inference following Young (2019). * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Comparing Referrals vs. Non-referrals during the RCT

Dep. var.:	Attrition (0-1) x 100			Monthly absences		
Method:	Linear Probability Model			Negative Binomial		
	(1)	(2)	(3)	(4)	(5)	(6)
Hire was referred	-6.96***			-0.19		
	(1.17)			(0.20)		
Referred X first 5m		-8.76***			-0.41*	
		(1.62)			(0.24)	
Referred X after 5m		-2.86*			0.22	
		(1.67)			(0.45)	
Referred X R50			-11.00***			0.07
			(1.66)			(0.37)
Referred X R90			-6.15***			-0.08
			(1.61)			(0.39)
Referred X R120			-6.09***			-0.41*
			(2.00)			(0.24)
Observations	14,879	14,879	14,879	14,879	14,879	14,879
Mean DV if referred=0	15.91		15.91	1.362		1.362
Workers	3,796	3,796	3,796	3,796	3,796	3796
Mean DV in first 5m if ref=0		17.75			1.152	
Mean DV after first 5m if ref=0		9.100			2.143	
F(R50 vs. R90)			0.04			0.77
F(R50 vs. R120)			0.06			0.28

Notes: An observation is a worker-month during the RCT (December 2015-December 2016). The sample is grocery workers hired during the RCT. Standard errors clustered at the store level are in parentheses. 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. That is, the DV equals 0 until the worker attrites, where s/he has a DV=1 and exits the sample. Columns 4-6 are negative binomial models, where the dependent variable is a worker's number of sick days in a month. 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
Coefficients shown:	Treatment dummies		Treatment X RCT period dummies		Treatment dummies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R0	-1.00**		-0.92*		-0.91		-1.05***	
	(0.40)		(0.47)		(1.15)		(0.34)	
	[0.015]		[0.071]		[0.425]		[0.001]	
R50	-0.47		-0.98*		-1.29		-0.38	
	(0.45)		(0.50)		(1.32)		(0.39)	
	[0.301]		[0.060]		[0.336]		[0.323]	
R90	-1.59***		-1.75***		-2.90**		-1.25***	
	(0.38)		(0.41)		(1.22)		(0.34)	
	[0.000]		[0.000]		[0.016]		[0.001]	
R120	-0.81*		-1.11**		-1.95		-0.52	
	(0.42)		(0.45)		(1.23)		(0.36)	
	[0.066]		[0.016]		[0.110]		[0.141]	
ERP		-0.97***		-1.19***		-1.76*		-0.81***
		(0.34)		(0.39)		(1.04)		(0.28)
		[0.007]		[0.008]		[0.087]		[0.003]
Store FE	No	No	Yes	Yes	No	No	No	No
Mean DV if ERP=0	6.677	6.677	5.434	5.434	17.24	17.24	4.362	4.362
Observations	74,188	74,188	203,798	203,798	14,879	14,879	55,953	55,953
Workers	10,003	10,003	16,942	16,942	3,796	3,796	5,870	5,870

Notes: Standard errors clustered at the store level are in parentheses. "Rand-t" randomization inference p-values following Young (2019) are in brackets (1,000 replications). All columns are linear probability models, where the DV 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 grocery 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 working with the firm at the time the RCT began. The RCT period is December 2015-December 2016. Data from the firmwide ERP rollout (2017m1-2017m5) are not used here; this is why the number of workers in columns 3 and 4 is roughly 17k instead of 18k. In the randomization inference, we do not account for the fact that allocations were re-drawn multiple times in the actual RCT randomization, as described in footnote 23. Stars are based on the clustered standard errors in parentheses, with * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Why did ERPs Reduce Turnover Separate from Generating More Referrals?

Reason	Managers N=222	Workers N=113
Panel A: Why did ERPs Reduce Turnover Separate from Generating More Referrals?		
"Employees felt more respected after being asked to be involved in the hiring process or liked having some say about who they might work with"	66%	50%
"Because FIRM NAME started the referral program, it made employees think that FIRM NAME was a better place to work."	23%	14%
"Employees didn't have a person to recommend, but they hoped to recommend a friend in the future."	13%	28%
"Employees referred their friends, but they did not tell FIRM NAME about it (and they did not get a bonus). The employees or their friends were more likely to stay at FIRM NAME."	3%	5%
"None of these reasons are important or likely. What is your explanation?"	10%	4%
Panel B: Parsing Further for Workers Selecting the First Reason Listed Above		
"Employees felt more respected after being asked to be involved in the hiring process"		15%
"Employees liked having some say about who they might work with"		17%
"Both were equally likely"		67%

Notes: This is from the *Post-RCT Surveys of Grocery Store Managers and Workers*. Managers did the survey by phone and could select more than one option (hence, the options don't add up to 100%). Workers did the survey via an electronic kiosk at work and could only select one option. The manager response rate was 93%, with 222 of 238 managers responding. For Panel A, we randomized the order in which the four reasons were presented to respondents, with "None of these reasons are important or likely" always presented last. For Panel B, as noted in the main text, for workers selecting "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", we asked them to parse further about whether "Employees felt more respected after being asked to be involved in the hiring process" or Employees liked having some say about who they might work with" was the main reason or whether both were equally likely.

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	2.44	11.21
Contribution to savings from:		
Referrals hired during RCT	0.13	0.58
Non-referral hires during RCT	0.75	3.47
Pre-RCT incumbents	1.56	7.16
% of savings from referrals hired during the RCT	5%	5%
Costs of the ERP	0.10	0.10
Profit per worker-month	2.34	11.12
Profit as share labor costs	0.6%	2.8%

Panel B: Profit by Particular ERP (turnover cost = €250)	R0	R50	R90	R120
Total savings in turnover costs	2.50	1.18	3.97	2.02
Contribution to savings from:				
Referrals hired during RCT	0	0.20	0.15	0.18
Non-referral hires during RCT	0.44	0.42	1.25	0.92
Pre-RCT incumbents	2.05	0.56	2.57	0.92
Costs of the ERP	0	0.05	0.11	0.22
Profit per worker-month	2.50	1.13	3.86	1.80

Panel C: Profits from Rollout ERP vs. Control		
Turnover cost number:	€250	€1,150
Total savings in turnover costs	2.68	12.31
Contribution to savings from referrals hired during rollout	0.37	1.68
% of savings from referrals hired during the rollout	14%	14%
Costs of the rollout ERP	0.54	0.54
Profit per worker-month	2.13	11.76

Notes: This table reports profit calculations using the method outlined in Section 5. Panel A reports the profit gains from having an ERP vs. Control, pooling all the RCT ERP treatments together. Panel B reports the profit gains from having one of the particular ERPs compared to Control. Panel C reports the profit gains from having the ERP used in the firmwide rollout (with €30 on hire and €100 after 3 months) vs. Control. All numbers are in euros per worker-month. Labor costs are assumed to be €400 per worker-month. The difference between the "administrative costs" and "full costs" of turnover is that the administrative costs are only the direct costs to hire and train a replacement and do not account for lost productivity, as explained in Appendix A.10. See Appendix A.11 for further details on the profit calculation.

Table 8: Heterogeneity by Pre-RCT Store Performance and Local Unemployment: Direct and Overall Effects of Having an ERP

Characteristic: (all normalized)	Log shrinkage rate Higher is worse.	Log sales per worker	Log operational profit per worker	Attri-tion rate	Unemploy -ment rate
	(1)	(2)	(3)	(4)	(5)
Panel A: Direct Effects. DV = Hire is a Referral (x100).					
ERP	2.46***	2.43***	2.30***	2.44***	2.44***
	(0.63)	(0.59)	(0.59)	(0.62)	(0.62)
ERP X Characteristic	-0.05	0.45	0.67	0.30	-0.94
	(0.54)	(0.53)	(0.47)	(0.49)	(0.59)
Panel B: Overall Effects. DV = Worker Attrites in a Month (x100).					
ERP	-0.94***	-0.90***	-0.88***	-0.96***	-1.07***
	(0.33)	(0.33)	(0.33)	(0.35)	(0.33)
ERP X Characteristic	0.66**	-0.57*	-0.64*	0.06	0.65**
	(0.31)	(0.30)	(0.35)	(0.24)	(0.25)
Panel C: Reduction in Labor Costs from Having an ERP, Assuming Turnover Cost of €250, i.e., "Admin Costs." Calculated for Stores at p10 and p90 of Various Characteristics					
10th Percentile Stores	1.1%	0.1%	0%	0.6%	1.2%
90th Percentile Stores	0%	1.0%	1.0%	0.5%	0.1%
Panel D: Reduction in Labor Costs from Having an ERP, Assuming Turnover Cost of €1,150, i.e., "Full Costs." Calculated for Stores at p10 and p90 of Various Characteristics					
10th Percentile Stores	5.1%	0.5%	0.2%	3.0%	5.4%
90th Percentile Stores	0.3%	4.6%	4.9%	2.5%	0.7%

Notes: Standard errors clustered at the store level are in parentheses. In Panel A, each column is similar to column 3 of Table 4, with the difference being that we add two regressors: ERP X Characteristic and Characteristic. In Panel A, an observation is a new hire during the RCT. In Panel B, each column is similar to column 2 of Table 5, with the difference being that we add two regressors: ERP X Characteristic and Characteristic. In Panel B, an observation is a worker-month during the RCT among all grocery workers. Panels C-D present the profit gains from having an ERP as a share of firm labor costs, similar to Table 7. The difference is that we use the estimates in Panel B of this table to calculate the profit gains as a share of labor costs for stores at the 10th percentile of a characteristic and at the 90th percentile of a characteristic. Panel C does the calculation assuming a turnover cost of €250 (i.e., the administrative cost of turnover), whereas Panel D does the calculation assuming a turnover cost of €1,150 (i.e., the full cost of turnover). Shrinkage is the share of inventory lost to theft, spoilage, and other reasons, so higher shrinkage means worse performance. * significant at 10%; ** significant at 5%; *** significant at 1%

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 *During RCT Survey of Grocery Store Managers and Employees* in fall 2016, as well as a post-RCT survey of the general population of the country where the study firm operates (*General Population Survey 2*). In the *During RCT Survey*, store managers in treatment stores were presented with the findings of the RCT and asked for their opinion why the ERPs had produced only a few referrals. Their answers, in free text, were classified into Reasons 1 to 11 by undergraduate coders. In the *General Population Survey 2*, randomly selected members of the general population were contacted after the ERP rollout and asked "why were there fewer referrals from cashiers than from the logistics and food production workers?". Their answers were coded similarly to managers. In the *During RCT Survey*, 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: Heterogeneity by Job and by Period: Referrals Made and Share of Turnover of Savings from Referral Hires

	RCT	Post-RCT Rollout
Panel A: Refs Made as a Share of Total Hires		
All Grocery Jobs	3%	12%
Cashier	3%	11%
Grocery Non-cashier	5%	17%
Non-Grocery Jobs		37%
Panel B: Share of Turnover Benefits from Referral Hires		
All Grocery Jobs	5%	14%
Cashier	4%	3%
Grocery Non-cashier	12%	20%
Non-Grocery Jobs		35%

Notes: Panel A 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%. The post-RCT period is Jan-May 2017, and is the period during which the firm rolled out a new ERP to all the firm at once (paying €30 at hire and €100 after 3 months). During the post-RCT period, there are 1,079 hires in grocery jobs and about 500 hires in non-grocery jobs. Panel B shows the share of turnover benefits from referral hires. The percentage shown is the direct benefit, whereas the indirect benefit is given by 100% minus the percentage shown. For example, for grocery non-cashier jobs in the post-RCT ERP rollout, 20% of the turnover benefits are direct and 80% are indirect.

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

Guido Friebel, Matthias Heinz, Mitchell Hoffman, 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 contains additional figures and tables. Appendix C is the Data Appendix. Appendix D presents a formal model accompanying Section 1. Appendix E provides additional materials used by the firm in the ERPs.

Appendix A Additional Discussion and Results

A.1 World Management Survey (Accompanying the Introduction)

The WMS (Bloom et al., 2014) has traditionally focused on manufacturing firms. However, in 2009, the WMS surveyed 661 retail establishments. 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 (Bloom et al. (2014) give details). Of the 537 establishments, 352 are in Canada, 126 are in US, and 59 are in UK. Unlike other WMS surveys, which do not ask about ERPs, the survey enumerators explicitly asked managers about whether the establishment had an ERP. 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 ERP used bonuses. One reason why the WMS rate of referrals is lower than that in CareerBuilder is that the question is asked at the establishment level. A retail firm may decide to have an ERP, but some local managers may choose not to apply it.

A.2 RCTs Related to Firm Hiring Procedures (Introduction)

As far as we are aware, ours is the first, large-scale, within-the-firm RCT on any hiring procedure. As mentioned in footnote 9 in the main text, studies in development economics have randomized selection procedures in government (see Ashraf et al. (2018) for a prominent example) or NGOs (e.g., Del Carpio and Guadalupe, 2018; Deserranno, 2019), but not in a private firm. Thus, this work cannot examine impacts on profits, and the signaling role of hiring procedures like ERPs may differ when chosen by a profit-maximizing firm compared to when chosen by a government or NGO. Beyond audit studies, there are also RCTs in online labor platforms which change features of worker-firm matching, but these analyze the impact of platform features (i.e., features of the entire labor market) as opposed to randomizing an individual firm's hiring procedures, the type of research called for by Oyer and Schaefer (2011). In addition, there are many studies that use natural experiments or RCTs to vary some feature of an organization (e.g., the compensation structure) and see how that affects the quality or quantity of applicants. While such studies examine who gets hired, they are not studying the impact of a hiring procedure.

A.3 Additional Discussion on Referral Bonus Levels (Section 2)

We compare our RCT bonuses to those paid in other studies. In our RCT, workers could earn up to 40% of monthly salary for making a referral, and we also paid well in expected value terms taking into account that referrer and referral had to stay 5 months post-referral.[1] In their study in a financial firm, 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 12% of the monthly salary), which is similar to our bonuses in expected value, and lower than the maximum value of our bonuses.[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 1/3 of monthly salary) for referring an experienced driver, though there was also a 6-month tenure requirement. In a lab-in-the-field study, Beaman and Magruder (2012), used ERP bonuses of 60-100 rupees, or less than one day's wage, with some component based off of the performance of the referral.[3]

A.4 Details of the Surveys Used in the Paper (Section 2)

As seen in Figure A1 below, we analyze the following surveys conducted at the study firm:

1. *Pre-RCT Survey of Non-grocery Employees*: In Oct.-Nov. 2015, we surveyed 120 food production workers at the firm about how much money would make them willing to a employee referral for a hypothetical vacancy. These responses were used to choose the bonus levels for the RCT, as noted in Section 2. The response rate was 100%.
2. *During RCT Survey of Grocery Store Managers and Employees*: In September-October 2016, we conducted phone surveys of store managers recording their time use and their opinions regarding why the RCT ERPs were generating only a modest number of referrals. The response rate was 92%. 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. These surveys are analyzed in the main text in Section 7, with results in Table 9.[4]
3. *Post-RCT Survey of Grocery Store Managers and Employees*: In summer and fall 2018, we conducted phone surveys of store and regional managers regarding the mechanism for the observed indirect impact of ERPs on attrition. In fall 2018, we conducted similar surveys for employees, but via in-store electronic kiosk.[5] These surveys are analyzed in the main text in Section 4.2.3, with results in Table 6.

[1] The referral bonus was paid for 45% of referral hires. Thus, the expected value of value of the bonuses was €37.5, €55.5 and €69 in R50, R90, and R120, respectively. These are equivalent to 13-23% of monthly salary, i.e., up to a week of monthly salary.

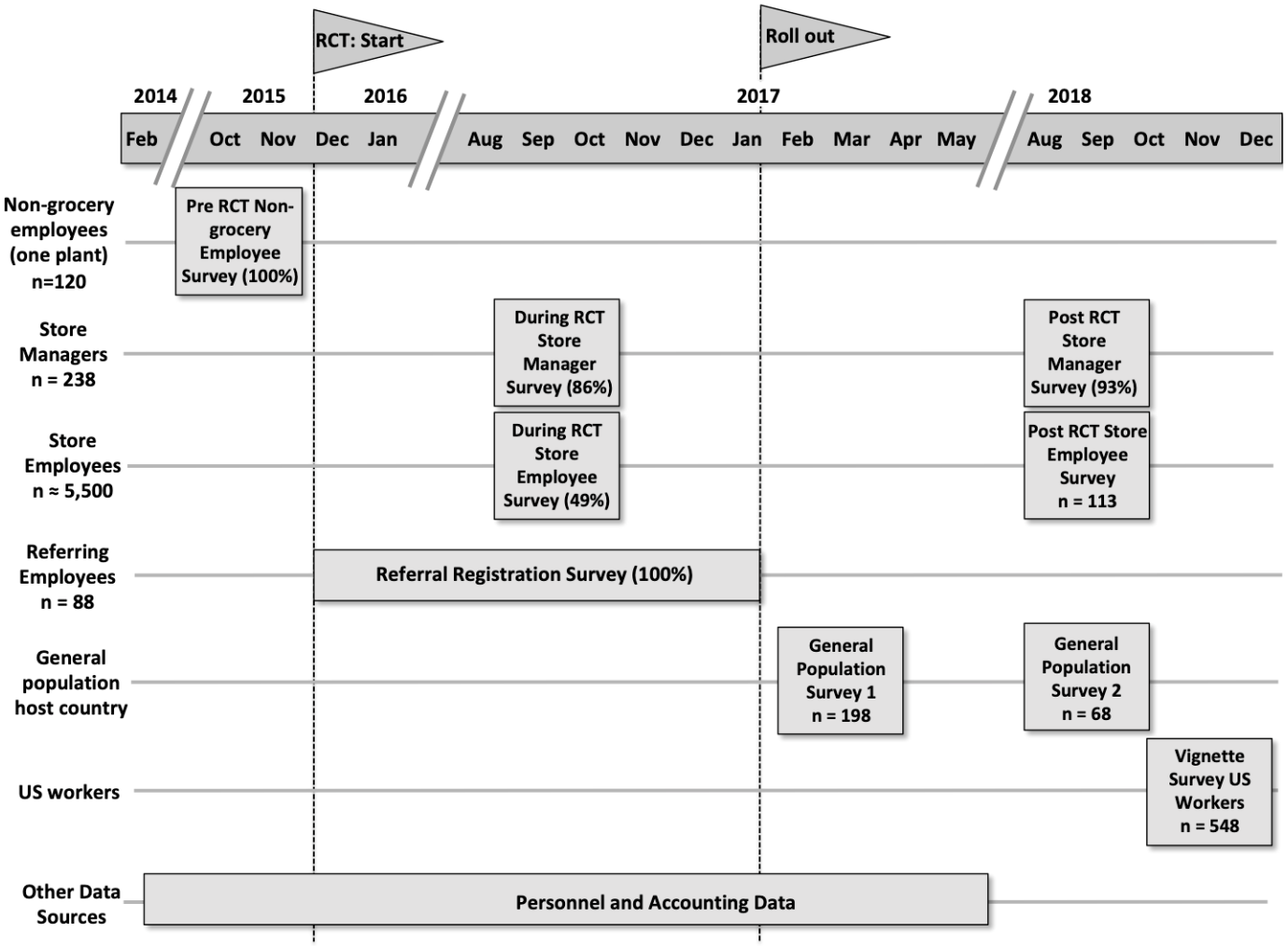
[2] The bonuses in Brown et al. (2016) also require people to stay 6 months.

[3] The authors report that 135 rupees is about $3, and that the average daily wage is about 110 rupees.

[4] This survey also had to cover questions on unrelated topics. Thus, we had to be parsimonious in choosing questions relevant for this study.

[5] The reason why these surveys were not conducted until summer and fall of 2018 is because our study firm CEO had earlier left the firm, causing us to lose the ability to conduct surveys. However, in summer 2018, we were able to re-engage with top executives at the firm in order to carry out these post-RCT surveys.

Figure A1: Datasets Used in the Paper



Notes: The *Pre-RCT survey of non-grocery employees* is discussed in Section 2 in describing how we selected the level of the referral bonuses for the RCT. The *During RCT surveys of managers and employees* are analyzed in Section 7, with results in Table 9 (Panel A covers managers, whereas Panel B covers employees). These help gain insight on why the RCT ERPs generated only a modest number of referrals. The *Post-RCT surveys of managers and employees* are analyzed in Section 4.2.3, with results in Table 6. These help gain insight on the mechanism for the indirect effects of ERPs on employee attrition. The *Post-RCT survey of employees* was conducted by the firm using store kiosks, so we do not know how many workers saw the survey on the kiosks (or know the share of workers who agreed to participate conditional on seeing the surveys). The *Referral Registration Survey* provides information on who referred whom and is used throughout the paper. The very brief survey is conducted by the firm as part of the referral process. Starting in January 2017, we only have information on who made referrals, not on who is referred. *General Population Survey 1* is analyzed in Section 7, with results in Figure 5. *General Population Survey 2* is analyzed in Section 7, with results in Panel A of Table 9. The *Vignette Survey of US Workers* is discussed in Sections 4.2.3, 7, and 8. It provides further evidence regarding the mechanism for ERP impacts on attrition, and also allows us to examine results across both lower-skill and higher-skill workers. In addition, it provides further evidence supporting the first part of Prediction 5, i.e., that workers should be more willing to make referrals for more attractive jobs. Appendix A.9 provides details on the *Vignette Survey*.

In all the firm surveys, subjects were told truthfully that we were conducting an international retail survey in partnership with a local university. In addition, subjects were told truthfully that their employer would not see individual-level responses to the survey. Phone surveys were conducted by native speakers recruited from a local university.[6]

In addition to these within-firm surveys, we also did phone surveys of randomly picked members of the general public of the country where the study firm operates:

- *General Population Survey 1*: Conducted in early 2017, this survey collected opinions regarding the attractiveness of different occupations and retail firms. This survey is analyzed in the main text in Section 7, with results in Figure 5.
- *General Population Survey 2*: Conducted in August-September of 2018, this survey explained to subjects that a grocery store firm had instituted an ERP, and that few referrals had been made for grocery jobs, whereas many referrals were made for non-grocery jobs. Subjects were then asked why they thought this was. This survey is analyzed in the main text in Section 7, with results in Panel A of Table 9.

Finally, we also ran a *Vignette Survey of US Workers* described below in Section A.9.

A.5 Who Makes Referrals? (Section 3.1)

Who makes referrals? Since the 88 RCT referrals are made by 75 referrers, most referrers made one referral during the RCT. In the ERP rollout, there are 314 referrals made by 268 referrers, of whom 193 are grocery workers. Broadly consistent with Burks et al. (2015), referrals are more common from workers with lower absence rates. In terms of links between referrer and referrals, the most common one is family member (about 1/3 of referrals in the RCT), followed by friend and acquaintance (about 20% each).

What stores do referrals come from? In basic summary statistics, stores where workers make referrals have higher employees and sales than stores with no RCT referrals. However, stores with referrals also hire more workers in general. At the individual level, store characteristics do not much predict whether a hire is a referral. Table 8 shows that ERP impacts on whether hires are referred are larger in stores with higher profits and lower local unemployment, but these differences are not statistically significant.

A.6 Mediation Analysis (Section 4.2.1)

Following Imai et al. (2010a,b), consider the following system:

$$M_{it} = \alpha_0 + \alpha_1 ERP_i + X_{it}\delta_2 + u_{it} \quad (1)$$

$$y_{it} = \beta_0 + \beta_1 ERP_i + \gamma M_{it} + X_{it}\delta_2 + v_{it} \quad (2)$$

[6] We also did pre-RCT pen-and-paper surveys with about 3k grocery workers and 230 store managers. We asked questions on social connections in and outside the workplace, and on attitudes about one's job, managers, and the firm. These surveys helped us design the RCT, but are not used in analysis. In the pre-RCT worker survey, the rate of informal referrals is 26%, similar to the 27% rate in the *During RCT* survey (see Section 3.1)—this is further evidence that the ERPs did not substantially boost informal referrals.

Here, y_{it} is an outcome of person i in month t (namely, whether i exits the firm during t); M_{it} are the mediator variables, namely whether someone is referred or someone's referrals made to date; ERP_i is a dummy for having an ERP in one's store; 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 the ERP is β_1. Imai et al. (2010b) show that OLS produces consistent estimates under Assumption 1 below.

Assumption 1

$$y_{it}(e', m), M_{it}(e) \perp\!\!\!\perp ERP_i |\ X_{it} \tag{3}$$

$$y_{it}(e', m) \perp\!\!\!\perp M_{it}(e) |\ X_{it} \tag{4}$$

for any treatment $e, e' \in \{0, 1\}$, for any mediator m, and for any controls X

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 (3) of Assumption 1 will hold because of random assignment. Equation (4), i.e., that potential referral status is independent of potential duration conditional on observables, is much less obvious.[7] For example, a person who is likely to be referred under an ERP may have other positive unobservables relative to someone unlikely to be referred. Given past research suggesting that referrals are positively selected (Brown et al., 2016; Burks et al., 2015; Pallais and Sands, 2016), we hypothesize that any bias would be toward biasing upward the estimate of γ. That is, any bias would seem to work against our conclusion that referrals are not a main driver of the ERP effect, making our qualitative conclusion even stronger.

Table B7 shows results. Columns 1-2 show the impact of having an ERP on being referred and referrals made to date using the full panel data. Columns 3-5 shows the impact of having an ERP as the mediators are gradually controlled for. The coefficient only falls in magnitude from -0.97 to -0.92. The estimates imply that only 5% of the impact of having an ERP on attrition is mediated via getting more referrals and having more referrals to date, whereas 95% remains unexplained. Column 5 shows that having made referrals so far to date does not significantly predict whether a person will attrite. Column 6 separate referrals made to date into those made in the last 5 months vs. those not made in the last 5 months. For each referral made in the last 5 months, a person is 1.7pp less likely to attrite, consistent with referrers staying a bit longer to get a bonus.

A.7 Manager Time Use (Section 4.2.2)

During the RCT in fall 2016, store managers were asked about the share of time during the preceding few months that they spent on five time use categories: goods/products, customers, administration, human resources, and employee turnover. We have time use data for store managers in 199 of the 238 stores. To assess whether manager time use was

[7] Equation (4) would hold if the mediator were directly randomized (Imai et al., 2010b), but one cannot force someone to be a referral hire or to make referrals. We experimented with estimating Equation (2) while instrumenting the mediator (either whether someone is referred or makes referrals) using the level of the referral bonus. Doing so had little impact on β_1 compared to OLS, but produced a large standard error for γ (despite having a strong first stage). Because of this imprecision with IV, we stick with OLS.

affected by having an ERP, we regressed normalized time use for each category on a dummy for having an ERP. As seen in Table A1, there is no impact of an ERP on time use.

Table A1: No Impact of Having an ERP on Normalized Manager Time Use (N=199)

Time spent on:	Goods		Customers		Administration		HR		Turnover	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ERP	-0.10	-0.10	-0.18	-0.13	0.19	0.20	0.08	0.08	-0.08	-0.12
	(0.17)	(0.16)	(0.19)	(0.20)	(0.16)	(0.17)	(0.16)	(0.17)	(0.19)	(0.18)
Store controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. An observation is a store manager. The dependent variable is normalized time use on each of the 5 categories. Store controls are pre-RCT head count, attrition, sales, store square footage, region dummies, in big city dummy, and dummy for having a Lidl store nearby. * significant at 10%; ** significant at 5%; *** significant at 1%

A.8 Discussion from Practitioners and Sociology (Section 4.2.2)

Our mechanism results, i.e., that having an ERP attrition appears to be due to workers valuing being involved in hiring, is highly consistent with two main points raised by business practitioners. First, practitioners point out specifically that ERPs make workers feel more involved in the hiring process. One recruiting website argues that ERPs "help increase attachment to the organization and make employees feel as though they have a stake in the future of the business. Employees want to grow, so having a hand in the company's forward motion is exactly what they're looking for." Another recruiting website argues that ERPs make "current employees feel trusted and valued since they are participating in the company's future and growth."[8]

Second, separate from ERPs, practitioners point out that involving workers in hiring can be beneficial to firms by increasing feelings of involvement. For example, DeLong and Vijayaraghavan (2002) describe an investment bank that seems to benefit by strongly involving the firm's bankers, from entry-level to senior-level, in hiring.

Turning to sociology, Fernandez and Weinberg (1997) is a study showing that referrals receive special consideration at different stages of the hiring process. In their Discussion section, the authors briefly consider that the desire to involve lower-level employees could be one reason why referrals receive special consideration in hiring (page 899).

A.9 Vignette Survey of US Workers (Sections 4.2.3, 7, and 8)

The *Vignette Survey of US Workers* was carried out by the online survey company Pureprofile on our behalf. Participants came from a pool of regular survey takers who have an account with Pureprofile. On average, active members of their pool take around five surveys per month. Most of the surveys are run by commercial companies, but researchers also use online surveys increasingly. The invitation form for the survey was generic and did not mention ERPs. We used respondents between age 18-65.

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

Table A2 below compares characteristics of survey participants to the 2018 March CPS. Compared to the CPS, participants in the survey are older, whiter, and more educated, but our survey still contains a broad mix of workers of different skills.

Table A2: Comparing *Vignette Survey* Participants to the CPS

	Vignette Survey	CPS
Female	.51	.52
Age	47.08	41.28
Black	.08	.12
Hispanic	.08	.2
Asian	.05	.07
Bachelor's or more	.54	.32

Notes: For the CPS, we restrict attention to individuals with age between 18 and 65.

ERPs and respect. As noted with the vignette's full text in Section 4.2.3, the main question in our vignette survey was "Do you think the firm having the employee referral program would make the employee feel more respected?" The survey responses were:

- It is very unlikely to make the worker feel more respected (2.6%).
- It is unlikely to make the worker feel more respected (4.0%).
- It is somewhat unlikely to make the worker feel more respected (4.7%).
- It is uncertain whether it will make the worker feel more respected (20.6%).
- It is somewhat likely to make the worker feel more respected (21.2%).
- It is unlikely to make the worker feel more respected (26.1%).
- It is very likely to make the worker feel more respected (20.8%).

Section 8 reports a comparison of workers with a bachelor's degree or higher versus workers with less than a bachelor's in terms of whether they believe that having an ERP would make an employee feel more respected, defined as a dummy for one of categories 5-7 above. We regress whether an employee would feel more respected on a dummy for having a bachelor's or higher with robust standard errors. Of course, the purpose of this regression is not to establish a causal relation between education and one's answer to the survey.[9] Rather, our point is simply that among higher-skilled workers, the belief that having an ERP affects respect is even a bit stronger than among lower-skilled workers.

Job quality and referrals. In addition to the above, we asked the below vignette:

> *Think of your **current main job**. Assume your employer has an **open job** in your department. One of your friends or relatives would probably match the requirements of the job. On a scale from (1) very unlikely to (7) very likely, would you try to **refer your relative/friend** to your employer?*

We combined answers to this question with questions where we asked *How attractive is your current job?* and *How attractive is your current employer?* on a scale from 1-7. As

[9] Indeed, if one controls for gender, race, and 6 age categories, the coefficient on bachelor's degree or higher falls to 5pp (s.e.=4pp).

seen in Table A3 below, a 1σ increase in job attractiveness increases the chance that someone would be willing to make a referral (defined as response 5-7 to the above vignette) by about 20pp. A 1σ increase in firm attractiveness increases referral willingness by 7-8pp. These results support that people are more willing to make referrals for better jobs.

Table A3: People who Rate their Job or Employer as More Attractive Report Being More Willing to Make a Referral (N=333 workers). From *Vignette Survey of US Workers.*

Dep. Var.:	Would refer (0 or 1)		Normed willingness to refer	
	(1)	(2)	(3)	(4)
Job attractiveness (normalized)	0.211***	0.197***	0.386***	0.341***
	(0.036)	(0.040)	(0.072)	(0.078)
Employer attractiveness (normalized)	0.074**	0.083**	0.302***	0.337***
	(0.037)	(0.041)	(0.072)	(0.079)
Demographic controls	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. Controls cover gender, race, 6 age categories, and 4 education categories. In columns 1 and 2, the DV is 1 if someone chose 5-7 and is 0 if someone chose 1-4 on a scale from (1) very unlikely to (7) very likely. In columns 3 and 4, the DV is the normalized value of the 1-7 score. The question was not asked to people who were unemployed or self-employed.

A.10 Calculation of the Costs of Turnover (Section 5)

We base our calculations on the following numbers: an average cashier salary of €350 per month, an average store manager salary of €900 per month, and overall average grocery store worker salary of €400 per month.

Direct costs (administration and training). Based on conversations with several store managers, we assume it takes about 18 hours of worker time and 20 hours of store manager time to hire a new worker. For store managers, this is based on time spent on interviewing candidates, processing the paperwork of each leaver, re-writing work schedules, communicating with staff regarding turnover events, and training the new workers. For workers, we focus on cashiers who are by far the largest group of grocery worker hires. Each newly hired worker undergoes a two-day (=16 cashier hours) formal training. After this, a mentor (another cashier) also spends two hours with each newly hired worker. Summing up, the cost of this time is about €150.

In addition, the head of HR informed us that there were 23 employees in the HR office whose job is to perform administrative tasks related to hiring and turnover. Inclusive of their monthly salaries, as well as the rent and utility cost of housing their offices, we assume these workers have a total monthly cost of €10,000 per month. This entails about €35 per turnover event. Finally, the firm needs to pay job advertising costs and uniform costs for new workers, which we assume add about €65 per turnover event.

Combining all direct costs together yields roughly €250 per turnover event.

Total costs. Beyond administrative costs, turnover also often has consequences in terms of productivity (Blatter et al., 2012; Boushey and Glynn, 2012). Turnover events can be disruptive to incumbent workers' productivity and new workers often require time to get

up to full speed. Blatter et al. (2012) study total hiring costs (inclusive of direct costs and lost productivity) for different types of firms and jobs using rich data from Switzerland. For large firms like ours (i.e., for firms with 100+ employees), Blatter et al. (2012) estimate average hiring costs to be 17 weeks of salary. For the job of cashier, the average hiring cost (i.e., across firms of different sizes) is 10 weeks, and they find that hiring costs increase in skill. Because Blatter et al. (2012) do not report hiring costs specifically for cashiers in large firms, we assume an intermediate value. To be conservative, we weight skill as more important than firm size, and assume a hiring cost of 12.5 weeks, which translates into a monetary value of €1,150.[10,11] Since Blatter et al. (2012) do not include costs that turnover may have on the firm's reputation or talent pool, our estimates may be somewhat conservative regarding long-run cost.

Recall from Section 4.1 that having an ERP led to a 2% increase in sales and a 2-2.3% increase in store-level operational profits, though the coefficients were statistically insignificant. While the coefficients were somewhat imprecise, these results are very broadly consistent with broader benefits of reducing turnover beyond direct costs.

Our full turnover cost is also consistent with a recent study by Kuhn and Yu (2019) on a retail firm in China. Kuhn and Yu (2019) exploit having daily sales data, coupled with a strongly enforced two-week notification period before attrition events, to estimate the cost of turnover events using an event study approach. They estimate that turnover events cost the firm 63 days worth of worker wages. Since their workers work 6 days per week, this is equivalent to a turnover cost of 10.5 weeks of pay. This is similar to our assumed total hiring cost of 12.5 weeks.

A.11 Profit Calculation Details (Section 5)

Absences. We do not account for absence in the 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.

Profit gain from incumbents. We calculate the turnover benefits from pre-RCT incumbents using the residual in total savings in turnover costs after the savings from referral and non-referral hires is taken out. That is:

$$\begin{aligned} \text{Savings from pre-RCT incumbents} &= \text{Total savings in turnover costs} \\ &- \text{Savings from referrals hired in RCT} \\ &- \text{Savings from non-referral hires in RCT} \end{aligned}$$

[10] For cashiers, Blatter et al. (2012) estimate that direct recruitment costs comprise 21% of total hiring cost for cashiers. This is very close to the value of €250 that we use.

[11] Rather than making an assumption based on Blatter et al. (2012), an alternative approach we have experimented with is to relate store-level turnover to store-level profits using our data. For example, there is a negative relation between turnover and profit in a regression of store-level monthly profit per worker on store-level monthly turnover, store fixed effects, and time fixed effects. And adding the costs from such an approach with the direct costs, we can arrive at a similar level of profit to the assumed value of €1,150. In addition, Appendix Table B8 shows cross-sectionally in pre-RCT data that higher-attrition stores also have higher shrinkage, lower sales per worker, and lower operational profit per worker.

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

A.11.1 Firmwide ERP Rollout and Different Jobs

In order to calculate profits under the firmwide ERP rollout starting in January 2017, we need to make some additional assumptions beyond those made in the RCT. This is for two reasons. First, the firm rolled out the new ERP (€30 upon hire, €100 after 3 months) to the entire firm at once and did not randomize. Second, as discussed in the main text in footnote 22, during the rollout (i.e., starting January 2017), we only observe data on who makes referrals, not on who is referred.

Contribution to turnover savings from referrals hired during the rollout. These savings are given by $\theta_r^R t_r^R c$, where θ_r^R is the share of observations from referrals in the rollout and t_r^R is the attrition benefit of referrals relative to non-referrals in the rollout. The superscript "R" is for rollout, whereas the subscript "r" is for referral.

Because we do not observe who is referred in the rollout, we take θ_r^R to be the share of observations from referrals in the RCT times the ratio of referrals made per hire in the rollout relative to the RCT.

Since we do not have experimental variation in the rollout ERP, we make an assumption about t_r^R using rough extrapolation of the RCT results. In the RCT, the difference between referral and non-referral attrition decreases as the size of the bonus increases in Table 4. Given that R120 has a referral/non-referral attrition difference of roughly 6pp per month, for a higher bonus of €30 + €100, we assume $t_r^R = 5$pp per month.

Total savings in turnover costs from rollout. During the RCT, the overall impact of the ERP on employee turnover did not systematically vary with the level of the referral bonus, as can be seen in the odd columns of Table 5. Thus, for the profit calculations, we assume that total turnover savings from the rollout ERP is the same as total turnover savings from the RCT ERPs, plus the incremental benefit of turnover savings from referrals hired during the rollout relative to during the RCT.[12]

Different jobs. To calculate overall turnover benefits of the RCT ERP separately by job, we perform our main turnover regression separately by job. The overall turnover benefits during the rollout is assumed the same as during the RCT, plus incremental benefits from referral hires. The turnover savings from referrals are scaled using referrals per hire. The attrition difference between referrals and non-referrals is given by the data during the RCT and is assumed to be 5pp per month in the rollout.

[12] That is, $t^R c = tc + \left(\theta_r^R t_r^R c - \theta_r t_r c\right)$, where t^R is the impact of the rollout ERP on turnover relative to no ERP; t is the impact of the RCT ERP on turnover; θ_r is the share of observations from referrals in the RCT; and t_r is the difference in attrition between referrals and non-referrals during the RCT.

A.12 Further Discussion on Referrals and Diversity (Section 6)

This Appendix discusses demographic issues related to ERPs. This analysis is not pre-registered. We also note that grocery workers are mostly female and disproportionately young, so conclusions drawn here may not necessarily hold in other contexts.

Heterogeneity in ERP impacts by worker demographics. Table A4 characterizes heterogeneity in ERP impacts by worker demographics. Column 3 shows that overall ERP impacts on attrition do not significantly vary between younger and older workers. However, column 4 shows that ERP impacts on attrition are larger for men than women, though impacts on attrition are statistically significant and robust for both.[13] Part of why ERPs have larger impacts for men is that men have higher attrition than women at the firm (11.5pp vs. 5.4pp per month during the RCT), though the impact of ERPs as a share of mean attrition is still larger for men. Another possibility is that when men are in gender non-congruent roles (i.e., most grocery workers are female), they may be more sensitive to feelings of respect relative to women.

Table A4: Treatment Heterogeneity by Demographics: Direct and Overall Effects

Dep. var.:	Hire is a Ref (0-1)		Attrition (0-1)	
	(1)	(2)	(3)	(4)
ERP	2.74***	2.56***	-0.53*	-0.79***
	(0.72)	(0.61)	(0.31)	(0.29)
ERP X Male	-0.97		-3.37***	
	(1.13)		(1.23)	
ERP X Young		-0.21		-0.21
		(0.91)		(0.79)

Notes: The table presents analyses similar to those in Panels A-B of Table 8 except we analyze heterogeneity here with respect to demographic variables. Standard errors clustered at the store level are in parentheses. Coefficients multiplied by 100 for readability. * significant at 10%; ** significant at 5%; *** significant at 1%

ERP impacts on diversity of hires. Beyond examining heterogeneity in ERP effects by age/gender, we can also consider diversity of hires as an outcome.[14] For this analysis, we turn instead to Appendix Table B1. Having an ERP does not affect the age of hires, but it increases the share of female hires by 4pp, marginally significant at the 10% significance level. Part of the impact reflects that ERPs increase referrals and referrals are 10pp more likely to be female than non-referrals, but much of this effect is unexplained by the simple homophily channel. Note that the tendency of ERPs to boost the share of hires who are female does not drive any of our results on the impact of ERPs on attrition because we control for a worker's gender throughout the paper's analysis.

Referrals and demographic gaps. In our data, referrals yield high-value hires from workers in traditionally disadvantaged groups. A common concern in EU countries is

[13] E.g., if we estimate column 4 in Table 5 separately by gender, we obtain an ERP X RCT coefficient for women of -0.82(se=0.37) and a coefficient for men of -3.32(se=1.07).

[14] Past work studies demographic implications of referrals (Fernandez and Fernandez-Mateo, 2006; Beaman et al., 2018). However, our paper gives the first RCT evidence on how having an ERP affects diversity.

youth unemployment. In the country we study, youth unemployment far exceeds the rate for older workers. In informal interviews with five store managers at our firm, all expressed concern that young workers are more likely to quit than older workers. Column 4 of Panel A of Table B1 shows this is true: among non-referrals, young workers (i.e., < 25 at time of hire, following the OECD definition) are almost 50% (6.7 pp per month) more likely to quit than older workers. However, referred youth are no more likely to quit than non-referred older workers. If a firm worries that young workers will quit shortly after hire, our results suggest that referrals may neutralize this concern.[15] Another common policy concern is that women have higher rates of absenteeism than men (Ichino and Moretti, 2009). Among non-referrals, women have 46% more absences than men (Column 5 of Panel B of Table B1). However, referred women are no less likely to be absent than non-referred men. Also, the referral advantage in absenteeism occurs only in women.

A.13 Further Discussion on the Reasons for Few Referrals Other than Job Attractiveness (Section 7)

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 2 shows that this is not the case.[16]

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 the *During RCT Surveys of Store Managers and Employees* 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 moved or had contacts living

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

[16] Over the four quarters of the RCT in Figure 2, the number of referrals made is 24, 17, 21, and 26, whereas the ratio of referrals per hire is 3.8%, 2.3%, 2.3%, and 3.7%. The ratio is lower in June-August 2016 because there is more hiring then.

elsewhere in the country, those contacts could have found a job at a local facility.

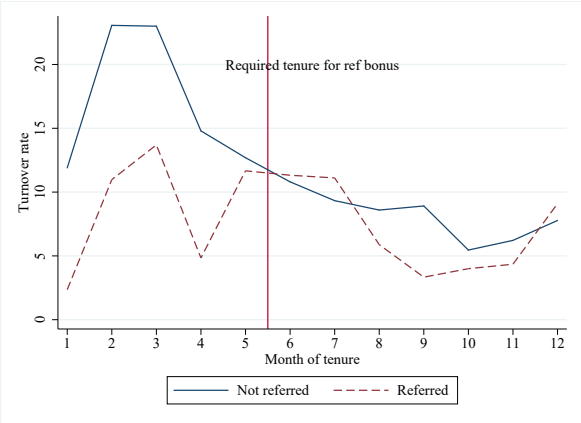
Was the referral process difficult? Store employees could have perceived it as burdensome to call the HR department to register a referral. We do not think this is a strong explanation because the process was designed to be very brief (just a few questions about how someone knows their referral). The 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.[17]

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

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

Appendix B Additional Figures and Tables

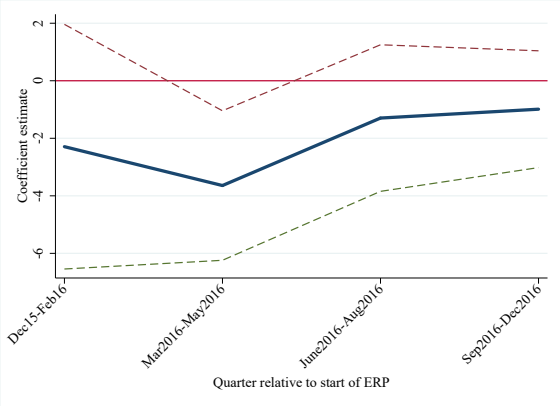
Figure B1: Attrition Hazard for Referrals vs. Non-referrals



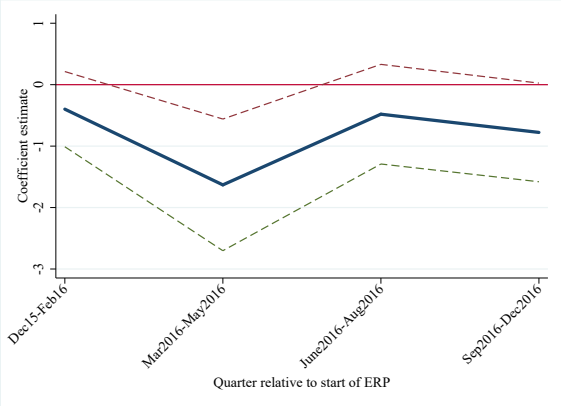
Notes: This figure shows the monthly quiz hazard as a function of worker tenure comparing referred vs. non-referred workers. The sample is grocery workers hired during the RCT. The referral and referrer must stay 5 months after the referral is hired in order for the referrer to be paid. The vertical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

[17] 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.

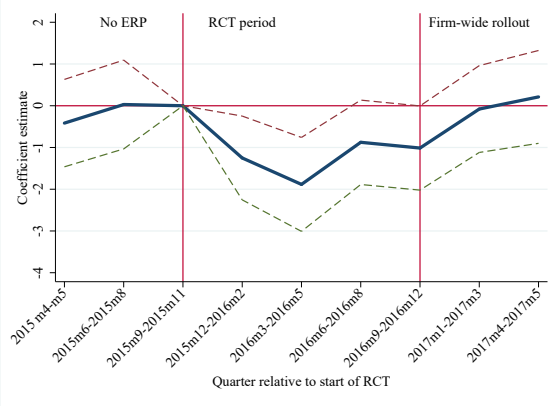
Figure B2: Event Studies on Impact of ERPs: Additional Subsamples and Outcomes



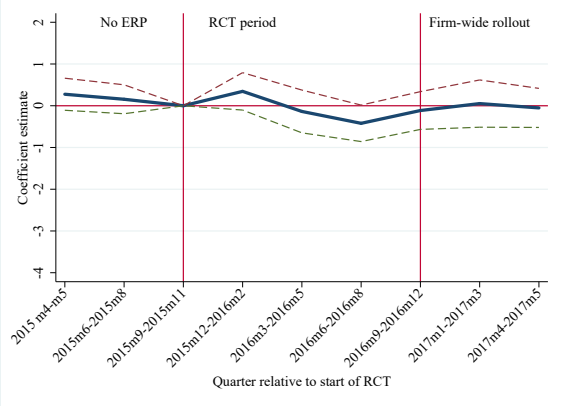
(a) Sample is New Hires During RCT



(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 4. The difference is they analyze different samples or look at different individual outcomes (other than overall attrition). Panel (a) here analyzes grocery workers hired during the RCT, whereas panel (b) here analyzes grocery 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 (b) here, 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 B1: Referrals, ERPs, and Demographics (Age and Gender)

Panel A: Age					
Dep. Var.:	Age of New Hires			Attrition (0-1) x 100	Monthly absences
	(1)	(2)	(3)	(4)	(5)
Hire was referred	-2.68*			-6.80***	-0.20
	(1.48)			(1.33)	(0.32)
Young X referred				0.09	-0.05
				(1.96)	(0.50)
Young (age<25)				6.67***	-0.13
				(0.65)	(0.09)
R0		0.68			
		(0.84)			
R50		-0.49			
		(0.77)			
R90		0.30			
		(0.73)			
R120		0.76			
		(0.81)			
ERP			0.32		
			(0.60)		
Observations	3,787	3,787	3,787	14,879	14,879
Mean dep. var.	31.36	31.36	31.36	15.69	1.363
Panel B: Gender					
Dep. Var.:	New Hire is Female (0-1)			Attrition (0-1) x 100	Monthly absences
	(1)	(2)	(3)	(4)	(5)
Hire was referred	0.093			-12.36***	0.73**
	(0.077)			(1.90)	(0.31)
Female X referred				6.81***	-1.31***
				(2.39)	(0.41)
Female				-5.59***	0.46***
				(0.82)	(0.11)
R0		0.037			
		(0.029)			
R50		0.036			
		(0.026)			
R90		0.049			
		(0.030)			
R120		0.041			
		(0.032)			
ERP			0.040*		
			(0.022)		
Observations	3,810	3,810	3,810	14,879	14,879
Mean dep. var.	0.718	0.718	0.718	15.69	1.363

Notes: In columns 1-3, an observation is a grocery worker hired during the RCT. In columns 4-5, an observation is a grocery-worker month during the RCT. Columns 1-4 show OLS regressions and column 5 shows negative binomial regressions. Standard errors clustered by store are in parentheses. 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%

Table B2: Robustness Check on Comparing Referrals vs. Non-referrals: Adding Store Fixed Effects

Dep. var.:	Attrition (0-1) x 100			Monthly absences		
Method:	Linear Probability Model			Negative Binomial		
	(1)	(2)	(3)	(4)	(5)	(6)
Hire was referred	-7.55***			-0.11		
	(1.24)			(0.29)		
Referred X first 5m		-9.17***			-0.37	
		(1.59)			(0.34)	
Referred X after 5m		-3.73*			0.38	
		(1.95)			(0.64)	
Referred X R50			-11.97***			0.51
			(1.96)			(0.58)
Referred X R90			-6.51***			-0.13
			(2.19)			(0.70)
Referred X R120			-7.18***			-0.34
			(1.89)			(0.35)
Observations	14,879	14,879	14,879	14,879	14,879	14,879
MDV if referred=0	15.91		15.91	1.362		1.362
Workers	3796		3796	3796		3796
MDV in first 5m if ref=0		17.75			1.152	
MDV after first 5m if ref=0		9.100			2.143	
F(R50 vs. R90)			0.07			0.49
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 B3: 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
Coefficients shown:	Treatment dummies		Treatment X RCT period dummies		Treatment dummies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R0	-0.04		-0.19*		-0.01		-0.03	
	(0.10)		(0.10)		(0.16)		(0.11)	
	[0.697]		[0.081]		[0.961]		[0.880]	
R50	-0.04		-0.03		-0.17		0.02	
	(0.11)		(0.11)		(0.17)		(0.13)	
	[0.686]		[0.827]		[0.408]		[0.911]	
R90	0.04		0.05		-0.05		0.07	
	(0.11)		(0.10)		(0.17)		(0.11)	
	[0.700]		[0.628]		[0.772]		[0.628]	
R120	0.07		-0.15		0.01		0.07	
	(0.11)		(0.11)		(0.16)		(0.12)	
	[0.533]		[0.190]		[0.976]		[0.614]	
ERP		0.01		-0.08		-0.05		0.04
		(0.09)		(0.08)		(0.14)		(0.09)
		[0.856]		[0.378]		[0.763]		[0.781]
Store FE	No	No	Yes	Yes	No	No	No	No
Observations	74,188	74,188	203,798	203,798	14,879	14,879	55,953	55,953
MDV if ERP=0	1.452	1.452	1.288	1.288	1.329	1.329	1.492	1.492
Workers	10,003	10,003	16,942	16,942	3,796	3,796	5,870	5,870

Notes: This table is similar to Table 5 except the outcome is monthly absences and the specifications are negative binomial instead of OLS. To ensure the likelihood converges, for the year-month of hire dummies, month-years of hire at or before 2002m1 are lumped together. Because the maximum likelihood procedure is slower than OLS, we perform randomization inference using 100 replications instead of 1,000. * significant at 10%; ** significant at 5%; *** significant at 1%

Table B4: Impact of having an ERP on Store-level Outcomes

Dep. var.:	Monthly hires	Log shrinkage rate	Log sales per worker	Log operational profits per worker	Log hours
	(1)	(2)	(3)	(4)	(5)
Panel A: Impact of Having an ERP During RCT					
ERP	-0.128	-0.025	0.020	0.020	-0.012
	(0.112)	(0.024)	(0.015)	(0.021)	(0.015)
Observations	3,016	2,993	2,993	2,989	3,017
Mean DV if ERP=0	1.285	-3.793	9.109	7.530	7.886
Panel B: Diff-in-diff Impact Using Pre-RCT and RCT Periods					
ERP X RCT	-0.222*	-0.017	0.020	0.023	-0.020
	(0.125)	(0.026)	(0.017)	(0.021)	(0.018)
Observations	8,223	5,603	8,182	5,594	5,633
Mean DV if ERP=0	1.144	-3.704	9.048	7.488	7.879

Notes: Standard errors clustered by store are in parentheses. An observation is a store-month. In Panel A, we control for the controls listed in footnote 23, plus region dummies, year-month dummies, and pre-RCT store-level mean of the dependent variable. In Panel B, we control for store dummies and year-month dummies. The shrinkage rate is the share of inventory lost to theft, spoilage, and other reasons, so higher shrinkage is worse. Operational profits per worker are store-level sales minus cost of goods minus wages minus shrinkage. * significant at 10%; ** significant at 5%; *** significant at 1%

Table B5: Demographic Homophily Between Referrers and Referrals

	(1) Age	(2) Female
Age of referrer	0.45***	
	(0.12)	
Referrer is female		0.36**
		(0.14)
Observations	60	84
Mean dep. var.	27.71	0.774

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

Table B6: 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
Coefficients shown:	Treatment dummies		Treatment	Treatment X RCT period dummies	Treatment dummies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R0	-1.01**		-0.93**		-1.07		-1.05***	
	(0.39)		(0.47)		(1.13)		(0.34)	
R50	-0.54		-1.16**		-1.08		-0.47	
	(0.51)		(0.55)		(1.54)		(0.44)	
R90	-1.62***		-1.79***		-3.13**		-1.29***	
	(0.42)		(0.43)		(1.39)		(0.37)	
R120	-1.00**		-1.10**		-3.29**		-0.49	
	(0.47)		(0.48)		(1.40)		(0.41)	
ERP		-1.02***		-1.21***		-1.93*		-0.85***
		(0.35)		(0.39)		(1.05)		(0.29)
Store FE	No	No	Yes	Yes	No	No	No	No
Observations	59,677	59,677	164,860	164,860	11,536	11,536	45,490	45,490
MDV if ERP=0	6.677	6.677	5.434	5.434	17.24	17.24	4.362	4.362
Workers	8034	8034	13725	13725	2964	2964	4800	4800

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 B7: 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)	(9)
ERP	0.007***	0.009***	-0.97***	-0.93***	-0.92***	-0.92***			
	(0.002)	(0.002)	(0.34)	(0.34)	(0.34)	(0.34)			
Hire was referred				-6.36***	-6.35***	-6.33***		-6.33***	-6.33***
				(1.34)	(1.33)	(1.33)		(1.35)	(1.35)
Refs made to date					-0.54				-0.51
					(0.85)				(0.87)
Refs made in last 5m						-1.74*			
						(0.92)			
Refs made not in last 5m						1.76			
						(1.71)			
R0							-1.00**	-1.00**	-1.00**
							(0.40)	(0.40)	(0.40)
R50							-0.47	-0.42	-0.42
							(0.45)	(0.45)	(0.45)
R90							-1.59***	-1.52***	-1.51***
							(0.38)	(0.39)	(0.39)
R120							-0.81*	-0.74*	-0.74*
							(0.42)	(0.42)	(0.42)
Observations	74,188	74,188	74,188	74,188	74,188	74,188	74,188	74,188	74,188
MDV if ERP=0	4.8e-4	0	6.677	6.677	6.677	6.677	6.677	6.677	6.677
Workers	10,003	10,003	10,003	10,003	10,003	10,003	10,003	10,003	10,003

Notes: Standard errors clustered by store are in parentheses. 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. "Refs made in last 5m" is a person's running sum of referrals during the last 5 months. "Refs made not in last 5m" is a person's running of sum of referrals made during the RCT while excluding referrals made during the previous 5 months. * significant at 10%; ** significant at 5%; *** significant at 1%

Table B8: Correlation Coefficients for the Different Dimensions of Heterogeneity

	Log shrinkage rate	Log sales per worker	Log operational profit per worker	Attrition rate	Unemployment rate
	(1)	(2)	(3)	(4)	(5)
Log shrinkage rate	1.00				
Log sales per worker	-0.45	1.00			
Log profits per worker	-0.55	0.86	1.00		
Attrition rate	0.37	-0.26	-0.11	1.00	
Unemployment rate	-0.07	0.03	-0.07	-0.30	1.00

Notes: Correlation coefficients are reported. Correlations are calculated using our worker-month panel during the RCT period. The five characteristics are store-level means calculated during the pre-RCT period.

Table B9: Heterogeneity in Referral Differences by Different Dimensions of Heterogeneity. DV = Worker Attrites in a Month (x100)

Characteristic:	Log shrinkage rate	Log sales per worker	Log operational profit per worker	Attrition rate	Unemployment rate
	(1)	(2)	(3)	(4)	(5)
Hire was referred	-6.88***	-6.90***	-6.90***	-6.99***	-6.93***
	(1.16)	(1.16)	(1.18)	(1.17)	(1.17)
Referred X Char	2.43***	-0.67	-0.80	1.23*	0.87**
	(0.84)	(1.01)	(1.15)	(0.65)	(0.43)

Notes: Standard errors clustered at the store level are in parentheses. Each column is similar to column 1 of Table 4, with the difference being that we add two regressors: ERP X Characteristic and Characteristic. An observation is a worker-month during the RCT among people hired during the RCT. * significant at 10%; ** significant at 5%; *** significant at 1%

Appendix C Data Appendix

Referrals data. Beyond the 88 referrals reported in Section 3.1, there is also one additional referral made that we cannot match to other records. Store managers were not eligible to participate in the ERP, as they have general authority over hiring decisions. Our analysis on the overall attrition impacts of ERPs includes store managers, but results are similar if store managers are excluded.

Age. Although we find a significant difference between referral and non-referral hires in age, we do not control for age in our main regressions. 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

Table B10: Robustness for Panel B of Table 8

Characteristic: (all normalized)	Log shrinkage rate Higher is worse.	Log sales per worker	Log operational profit per worker	Attri-tion rate	Unemploy-ment rate
	(1)	(2)	(3)	(4)	(5)
Panel A: Restrict to Full-Service Grocery Stores.					
ERP X Characteristic	0.60*	-0.74**	-0.83*	-0.20	0.72**
	(0.33)	(0.35)	(0.47)	(0.27)	(0.28)
Panel B: Add ERP*(Pre-RCT Mean # of Products Offered).					
ERP X Characteristic	0.62**	-0.56*	-0.59	0.02	0.64**
	(0.31)	(0.30)	(0.36)	(0.26)	(0.25)
Panel C: Add ERP*(Pre-RCT Share Products that are Fresh Goods).					
ERP X Characteristic	0.63**	-0.61**	-0.77**	0.10	0.60**
	(0.31)	(0.28)	(0.32)	(0.23)	(0.24)
Panel D: Add ERP*(Pre-RCT Mean # of Total Checkouts).					
ERP X Characteristic	0.66**	-0.58*	-0.62*	0.03	0.64**
	(0.33)	(0.30)	(0.36)	(0.25)	(0.25)
Panel E: Add ERP*(Pre-RCT Mean Head Count).					
ERP X Characteristic	0.63**	-0.55*	-0.60*	0.03	0.62**
	(0.32)	(0.30)	(0.36)	(0.25)	(0.25)
Panel F: Add ERP*Store Square Meters.					
ERP X Characteristic	0.61**	-0.55*	-0.57	0.02	0.63**
	(0.30)	(0.30)	(0.36)	(0.24)	(0.25)
Panel G: Add ERP*(Dummy for Lidl Store Nearby).					
ERP X Characteristic	0.65**	-0.56*	-0.63*	0.07	0.67***
	(0.30)	(0.29)	(0.35)	(0.24)	(0.25)

Notes: This table is a robustness check to Panel B of Table 8. It shows how the key interaction term coefficients change as either the sample is restricted (as in Panel A) or where we also include regressors for an additional characteristic and the interaction of ERP times that characteristic (Panels B-G). In Panels B, C, D, and F, the additional characteristic is normalized. Panel D is robust to looking separately at manned checkouts or self-checkouts. * significant at 10%; ** significant at 5%; *** significant at 1%

Table B11: Overall ERP Impacts on Attrition: Split by Above/Below Median Store Performance and Unemployment

Characteristic: (all binarized)	Shrinkage rate Higher is worse.	Sales per worker	Operational profit per worker	Attri-tion rate	Unemploy-ment rate
	(1)	(2)	(3)	(4)	(5)
Below Median	-1.55**	-0.75	-0.27	-1.39***	-1.39***
	(0.60)	(0.54)	(0.51)	(0.43)	(0.46)
Above Median	-0.32	-1.22**	-1.53***	-0.95*	-0.22
	(0.43)	(0.49)	(0.43)	(0.54)	(0.44)

Notes: Each entry is similar to column 2 in Table 5, with the difference that we are splitting by above or below median of the store performance and unemployment variables. * significant at 10%; ** significant at 5%; *** significant at 1%

attention to individuals hired during our data (who thus consistently have age data). Age is missing for 24 workers hired during the RCT.

Gender. When worker gender is missing, we impute it based on name by using gender-specific endings that exist in the language of the country where our firm is based. After imputation, gender is missing for one grocery worker hired during the RCT.

Race. The firm's personnel data do no contain variables for race or nationality, as racial/ethnic heterogeneity is very limited in the country we study.

Attrition codes. Employees receive up to 4 attrition codes, which are assigned by the store manager. We classify someone as "fired" 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.

Multiple spells. Some workers in our data have multiple spells, where they return to working at the firm after a break in the record. In our population of workers, it is not uncommon to take breaks in employment. When a worker has multiple spells, we only count the final attrition event, and not the earlier ones. In addition, if a worker has a date of hire which is more recent than the current date, we assign the date of hire to that worker's earliest date on record. Our results are similar if we instead consider only the most recent spell.[18] For referrals made in the RCT, we impose that referral spells be counted so as to not exclude referrals, for reasons of statistical precision. That is, for a small number of people who are hired as a referral despite having an earlier spell, we count those as separate spells. Results are similar if we do not do this.

Appendix D Theory Appendix

D.1 Formal Model

We present a simple model to fix ideas on how ERPs affect employee outcomes, both directly in terms of affecting referrals and indirectly via creating respect. The model takes up three ideas. First, an ERP provides the firm with more precise signals about a candidate's match quality (Simon and Warner, 1992; Brown et al., 2016; Dustmann et al., 2015). In contrast to these models, we assume that the information resides with an employee instead of the overall firm. Second, workers have social preferences toward friends they may refer (Bandiera et al., 2009; Beaman and Magruder, 2012). Third, and potentially most important, our model incorporates workers caring about being respected (Ellingsen and Johannesson, 2008). More precisely, employees who are pro-social want the firm to think that they are pro-social.[19]

[18] That is, the results are similar if we do not assign hire dates to the earliest date on record, and instead merely drop observations which have negative tenure.

[19] We assume the worker cares about being regarded as pro-social because (1) it is realistic for our setting; (2) referral models naturally contain altruism so it is simple to include the worker caring about this; and (3) doing so follows Ellingsen and Johannesson (2008). The model's logic would still hold if the worker cared about the firm thinking it had another trait about which the firm credibly signals a positive belief by having an ERP (e.g., the firm would not want to have an ERP for a worker with bad judgment).

Set-up. The firm employs an incumbent worker, I, and wants to hire an additional worker. Following Ellingsen and Johannesson (2008), I can be of two different types $\Sigma \in \{0, \sigma\}$, where $0 < \sigma < 1$. Type Σ represents the social preferences of I toward an individual, N, of their social network, who could be referred for the job opening. In our model, Σ reflects altruism, but it could also represent reputational considerations. For simplicity, we assume that $\Sigma = \sigma$ for sure, but assume that I initially believes the firm to believe that $\Sigma = 0$. This simplifying assumption is discussed in Appendix D.3.

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

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

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

$$U^I(R = 1) = (1 - \Sigma)b + \Sigma U^N(R = 1) + B(\hat{\Sigma}) \tag{5}$$
$$U^I(R = 0) = \Sigma U^N(R = 0) + B(\hat{\Sigma}) = B(\hat{\Sigma}), \tag{6}$$

Here, N's utility depends on the job match, m, and job overall attractiveness, q, with $U^N(R = 1) = m + q$. Match m represents all person-specific rewards from the job. Job attractiveness, q, is the same for all workers, and may depend not only on the wage but also on its non-pecuniary aspects, such as working conditions and reputation in society. In (6), we normalize N's utility if he is not referred to 0.

The third term, $B(\cdot)$, represents I's benefit from feeling esteemed or respected (Ellingsen and Johannesson, 2008). The term, $\hat{\Sigma}$, is I's belief of the firm's belief about Σ. We assume that $B(\hat{\Sigma}) = \hat{\Sigma}$ for $\Sigma = \sigma$ and $B(\hat{\Sigma}) = 0$ for $\Sigma = 0$, i.e., I's utility increases in firm beliefs if she is altruistic, but she doesn't care what the firm thinks if she is selfish. We assume that I's prior is $\hat{\Sigma} = 0$, i.e., I initially believes that the firm considers her to be selfish.

Firm Profits. The firm's payoff from a referral is $\pi = m - \widetilde{b}$. Bad matches are expensive for the firm, because the firm has to spend resources on training costs. With the share of referrals in the total number of employees denoted by r, the expected profit of the firm with an ERP is:

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

where $E[m|m > m^*]$, and $E[m]$ are the expected quality matches of the referred and

non-referred workers, respectively; c is the cost of attrition for an incumbent worker; and $Pr(Q)$ is the probability that the incumbent worker exits and is equal to $1 - G(\sigma)$.[20] In contrast, firm profits without an ERP are $E(m) - c(1 - G(0))$.

Our model yields five predictions. We provide intuition here and proofs in Appendix D.2. The model's simplifying assumptions are discussed more in Appendix D.3.[21]

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

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

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

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

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

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

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

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

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

Predictions 1-4 are tested using the RCT. Prediction 5 is tested using surveys and the firmwide ERP rollout.

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

[21] The short-cuts discussed are: (i) because of the static game, the bonus is paid upon hire and not after five months; (ii) social preferences only relate to a potential referral (and not intrinsically to the firm); (iii) the worker can only have two types; (iv) the worker's belief updating is non-Bayesian.

D.2 Solving the Model

We first show that there exists a separating equilibrium where the worker believes the firm will choose to have an ERP if the firm received a private signal that I is altruistic, but that the firm will not have an ERP if it does not receive such a signal. In contrast, there is no separating equilibrium in the opposite direction, i.e., where the firm would have an ERP if and only if it did not receive such a signal. We then derive the five predictions within the context of the separating equilibrium.

Let $t \in \{0, 1\}$ denote whether the firm receives a private signal that the worker is altruistic, and let $ERP \in \{0, 1\}$ denote whether the firm has an ERP. Further, let m^* denote the threshold match quality where I makes a referral if $m > m^*$; likewise, let ε_0^* be the threshold value where I will quit the firm if her ε is higher and when no ERP is used, and let ε_1^* be the threshold value under an ERP.

We show it is a Perfect Bayesian Equilibrium where the worker believes the firm chooses $ERP = t$; where $m^* = -\frac{1-\sigma}{\sigma}b - q$; and where $\varepsilon_0^* = 0$ and $\varepsilon_1^* = \sigma$.

To show this, we first derive the optimal behavior of I given the firm's strategy. If there is no ERP, the firm believes that the worker is selfish, so I's utility if she stays at the firm is $B(\hat{\Sigma}) = B(0) = 0$ compared to ε at the outside option, so $\varepsilon_0^* = 0$. In contrast, if there is an ERP, $\hat{\Sigma} = \sigma$, so $\varepsilon_1^* = \sigma$. Under an ERP, I chooses where to make a referral, which occurs when $(1-\sigma)b + \sigma(m+q) > 0$, yielding $m^* = -\frac{1-\sigma}{\sigma}b - q$.

Now, we check that the firm's strategy is optimal given the worker's strategy. If $t = 1$, the firm's profits from having an ERP are $r\big(E[m|m > m^*] - \widetilde{b}\big) + (1-r)E[m] - c(1 - G(\sigma))$, which is larger than $E(m) - c(1 - G(0))$, provided that the referral bonus $\widetilde{b}$ is not too large. In contrast, if $t = 0$, the firm thinks there is no retention benefit of having an ERP, as the firm thinks the worker is selifsh in the absence of a good signal, and selfish workers don't care about the firm's esteem. Specifically, the firm's profits from having an ERP are $E(m) - r\widetilde{b} - c(1 - G(0))$, which are lower than the profits without an ERP of $E(m) - c(1 - G(0))$.

It is also easily seen that there cannot be a separating equilibrium where the worker believes the firm chooses $ERP = 1 - t$. When $t = 0$, the firm believes there is no retention benefit to having an ERP, because selfish workers don't care about being esteemed. The firm has that $\pi(ERP = 1) = E(m) - r\widetilde{b} - c(1 - G(0))$, which is less than $\pi(ERP = 0) = E(m) - c(1 - G(0))$. We now turn to showing the five predictions.

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

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

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

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

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

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

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

which is positive.

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

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

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

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

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

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

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

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

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

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

To analyze the relevance of job attractiveness for the decision to refer, note that $\frac{\partial r}{\partial q} = f(m^*)$, which is positive because people value their friends and to refer them for better jobs. To see how job quality affects the responsiveness of referrals to bonuses, note that $\frac{\partial^2 r}{\partial b \partial q} = -f'(m^*)\frac{1-\sigma}{\sigma}$. Thus, if $f'(m^*) < 0$, then $sgn\left(\frac{\partial^2 r}{\partial b \partial q}\right) = -sgn(f') = +$. This seems likely to hold if only a minority of workers make referrals.[22]

D.3 Discussion of Model Assumptions

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

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

The incumbent has social preferences toward their friend, not toward the firm. We assume that the incumbent worker only has potential social preferences toward their friend, not toward the firm. If the worker had potential social preferences toward the firm, all predictions of the mode would be the same. The key feature of the model is that having an ERP involves delegating the hiring decision to the incumbent worker, and doing so is only valuable if the worker cares about the match quality of a referred worker. The incumbent worker may do so because they care about their friend (and the firm also happens to benefit from higher match quality) or because they directly care about the firm. In our model, the firm also has zero outside information outside of potential referrals.[23] Also, while we assume that the friend and firm equally benefit from match quality for simplicity, this assumption is not required.

The level of the referral bonus and respect. We assume that a worker's true social preferences can only take two values, and we do not analyze the worker updating their sense of respect in reflection of the particular value of $\widetilde{b}$. If worker social preferences can take many values, then choosing higher values of $\widetilde{b}$ could communicate that the firm has a particularly high belief about the value of altruism for a worker. On the other hand, outside our model, choosing a very high value of $\widetilde{b}$ could communicate other messages, such as that making referrals is an unpleasant task (Benabou and Tirole, 2003). Thus, because

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

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

of these competing effects, we set this aspect aside. One can also examine empirically whether larger referral bonuses tend to have larger impacts on incumbent workers. Conditional on having a referral bonus, we do not observe a clear relation between the level of the bonus and incumbent retention effect.

Worker's perception of firm belief updating. The incumbent worker believes that the firm initially believes that the worker has $\Sigma = 0$ for sure. After seeing the ERP, the incumbent worker recognizes that the firm would not have the ERP unless the firm recognized that $\Sigma = \sigma$. Such belief updating is not consistent with Bayes' Rule, since a Bayesian will never update if they believe that the initial value of some event occurring is 0. This assumption is made entirely for simplicity of the model. One could alternatively assume that the worker believes that the firm believes that the worker has $\Sigma = \sigma$ with a 50% probability, and that seeing the ERP leads the worker to update to believe that the firm believes that $\Sigma = \sigma$ for sure.

Appendix E Documents Used in the RCT and in the Firmwide ERP Rollout

We first present the letters given to workers in the RCT. These are followed by Figure E1, which shows the posters that were used in the 2017 firmwide ERP rollout.

[FIRM logo]

Dear Employee,

Over the last couple of years, FIRM has dedicated a lot of its attention and resources to ensuring the quality of its products and services, as well as investing in the development and renovation of its stores. We believe that we are on the right path to becoming one of the best and most appealing grocery stores in COUNTRY!

In order to become a market leader, we continuously seek out the best employees, who can become permanent members of our large team. Right now, we also invite you to join our recruitment process and to recommend a friend, a relative, or an acquaintance for a job at one of our FIRM stores.

How can I recommend my friend, relative, or acquaintance?

1. Find a candidate who, in your opinion, would fit a vacant position in your or any other stores in which we are looking for employees (information on new positions available will be provided by your store manager).
2. Call and register* your recommended candidate.
   *register by calling us at XXX (YYY, regional human resources manager)
3. Send your recommended candidate to a store where positions are available.

We believe that together with your help we can find professional employees and create a friendly work environment for every one of you!

Best wishes,
[FIRM logo]

Notes: This is a translated and redacted version of the letter employees received in the R0 group during the RCT.

[FIRM logo]

Dear Employee,

Over the last couple of years, FIRM has dedicated a lot of its attention and resources to ensuring the quality of its products and services, as well as investing in the development and renovation of its stores. We believe that we are on the right path to becoming one of the best and most appealing grocery stores in COUNTRY!

In order to become a market leader, we continuously seek out the best employees, who can become permanent members of our large team. Right now, we also invite you to join our recruitment process and to recommend a friend, a relative, or an acquaintance for a job at one of our FIRM stores. If they get hired, the person who recommended them (you) will receive a **bonus!**

How can I recommend my friend, relative, or acquaintance?

1. Find a candidate who, in your opinion, would be suitable for a vacant position in your or any other stores in which we are looking for employees (information on new positions available will be provided by your store manager).
2. Call and register* your recommended candidate.
   *register by calling us at XXX (YYY, regional human resources manager)
3. Send your recommended candidate to a store where positions are available.
4. **If your recommended candidate:**
   - Fits the requirements of a position
   - Is hired and stays in employment for at least 5 months

We will award you a bonus €ABC! (after tax)

IMPORTANT!

- The bonus is awarded after taxes are deducted. A part of this bonus – **€**15 – you will receive after your candidate gets hired (included in your next month's salary), while the rest of this bonus will be given 5 months after you and your recommended employee have worked through that period (5 months) at our company.
- Please be aware that the whole bonus will be paid out only if your recommended candidate is hired and only after they have completed 5 months of employment at our company.
- The bonus and its payouts will be organized directly by the Human Resources department; therefore, it is very important to call and register your candidate before you send them to a store.

We believe that together with your help we can find professional employees and create a friendly work environment for every one of you!

Best wishes,
[FIRM logo]

Notes: This is a translated and redacted version of the letter employees received in the R50, R90, and R120 groups during the RCT. The amount ABC was 50, 90, or 120 euros depending on treatment.

Figure E1: Posters Used during the 2017 Firmwide ERP Rollout

Notes: This is a translated and version of the posters during the 2017 firmwide ERP rollout (with identifying firm information redacted). From left to right, the posters are for grocery store workers, logistics workers, and food production workers, respectively. Except for the different pictures, the posters are the same.

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