What Do Employee Referral Programs Do? Measuring the Direct and Overall Effects of a Management Practice

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ABSTRACT

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A data appendix is available at http://www.nber.org/data-appendix/w25920
A randomized controlled trials registry entry is available at https://www.socialscienceregistry.org/trials/964

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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 control 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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Management practices are widely held to matter for firm performance (Ichniowski *et al.*, 1997; Ichniowski & Shaw, 1999; Bloom & Van Reenen, 2007; Syverson, 2011). In addition to the rich, growing body of work on the overall quality of a firm's management practices, scholars are increasingly interested in the impact of particular practices and how they matter.

An important area where management practices are understudied is firm hiring (Oyer & Schaefer, 2011), which is surprising given that hiring is believed critical for firm performance (Bloom & Van Reenen, 2011). One of the most common ways by which workers get hired is via employee referrals.[1] As surveyed by Topa (2019), while most work on referrals analyzes the perspective of job-seekers (e.g., Granovetter, 1974; Bayer *et al.*, 2008), a smaller stream of work analyzes referrals from the perspective of firms, showing that referral hires tend to be of higher quality than non-referrals, with lower turnover, lower recruiting costs, and sometimes higher productivity (Brown *et al.*, 2016; Burks *et al.*, 2015). Thus, it is unsurprising that many firms have employee referral programs (ERPs), a management practice where workers are explicitly encouraged to refer their social contacts for jobs, often using bonuses.[2]

Despite the prevalence of ERPs, that many firms use a management practice does not necessarily imply that it is valuable (Blader *et al.*, 2020; DellaVigna & Gentzkow, 2019). What do ERPs do and why? Beyond a possible role in hiring good workers, can ERPs provide firms with other benefits (or costs)? We address these questions using a 13-month randomized controlled 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. Our paper is the first RCT on ERPs 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 arm only provided information to encourage referrals. The other three arms additionally paid different referral bonuses of up to 40% of monthly salary after taxes if the referrer and the "referral" stayed at least 5 months. (As in past work, we use "referral" both for the recommendation process and for the person hired.)

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

[1] Roughly 25-40% of European jobs and about half of US jobs are found via networks (Topa, 2011). Referrals may matter for many features of labor markets, e.g., wage inequality (Montgomery, 1991).

[2] A wide range of firms use ERPs, including AT&T, Starbucks, UPS, Deutsche Bahn, IKEA, PWC, Walmart, Enterprise Rent-a-Car, Booking.com, and Google. 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 & Van Reenen, 2007) covering Canada, US, and UK, 23% of establishments have an ERP (see Appendix A.1 for details and discussion). 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.

behavior? That is, what is the impact of ERPs—both their existence and the bonus level—on 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 & Johannesson, 2008; Rebitzer & 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 workers and managers take a series of detailed surveys. In addition to being helpful for the RCT, the first two of these characteristics are common to many low-skilled jobs worldwide. The retail setting of the firm is also broadly representative of many jobs.

Section 3 shows that the direct effects of ERPs are directionally as expected. Higher bonuses lead to more referrals. However, while statistically significant, the magnitude of the impacts seems relatively small (though ERPs are still cost-effective based only on direct effects). Even under the largest bonus, only 5% of hires are referrals. Encouraging referrals without using a bonus leads to no referrals. We believe the seemingly low referral rate reflects (1) we are studying formal instead of informal referrals and (2) grocery jobs are perceived as unattractive, a point supported by surveys.[3] 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 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

[3]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.

stores where no RCT referrals are made. Nor are the effects related to managers behaving differently in treatment stores. Instead, our surveys 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). While direct benefits still exceed the costs of an ERP, only comparing referrals vs. non-referrals dramatically underestimates the total benefits of an ERP—this is a key takeaway of our paper. 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.

Encouraged by the effects of the RCT, the firm rolled out the ERP to all employees, including grocery workers in RCT control stores, as well as non-grocery jobs in logistics and food production, and also increased the referral bonus (Section 7). Once control grocery stores receive 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 reveal that grocery jobs are seen as unattractive, and that workers who care for their friends may hesitate to refer friends for these jobs. Non-grocery jobs are seen as more attractive.[4]

Our paper contributes to several literatures, most importantly, the one on management practices. Building on the robust empirical relation between management practices and outcomes, recent papers conduct RCTs on broad management practices (Bloom *et al.*, 2013) or particular practices like work from home (Bloom *et al.*, 2014). What is particularly noteworthy in our paper is an RCT of a common management practice *at-scale*.[5] Oyer & Schaefer (2011) argue that hiring is understudied in labor and personnel economics, and that RCTs are needed to address this gap.[6] To our knowledge, ours is the first, large-scale,

[4]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.

[5]Exceptions of at-scale, within-firm RCTs include Nagin *et al.* (2002), Blader *et al.* (2020), Gosnell *et al.* (2020), and Friebel *et al.* (2017, 2018), all on non-hiring topics.

[6]In the *Handbook of Labor Economics*, Oyer & 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

within-the-firm RCT on any hiring procedure in any context.[7] 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 our RCT's large scale, and is consistent with ERPs being complementary to other HR management practices, an idea discussed frequently in theory (Milgrom & Roberts, 1990), but, beyond key exceptions like Ichniowski *et al.* (1997), Boning *et al.* (2007), and Blader *et al.* (2020), is often hard to examine empirically.[8]

Second, our results contribute to a small but influential literature on dual-purpose HR practices. As surveyed in Rebitzer & 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, with some noteworthy exceptions (Ritter & Taylor, 1994; Landers *et al.*, 1996), evidence on dual-purpose HR practices is relatively scarce. 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 & 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.

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 surveyed 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.[9] Building on Topa's 2019 suggestion for research to analyze how referral differences vary across local labor markets, we exploit the wide geographic scale of our RCT to show that ERPs have larger overall effects in better

decisions."

[7]Development studies have randomized selection procedures in government (e.g., Ashraf *et al.*, 2020) or NGOs (e.g., Del Carpio & 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.

[8]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.

[9]Papers randomize referral programs in non-inside-the-firm contexts to study different questions from ours, such as what type of customers should be targeted in customer referral programs (Kumar *et al.*, 2010). Appendix A.2 discusses further.

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, namely, a model of ERPs where potential referrers have career concerns.)

1 Conceptual Framework

How might ERPs with different referral bonuses affect outcomes and why? Since ERPs may be a dual-purpose HR practice (Rebitzer & 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 different 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 (Bénabou & Tirole, 2003). In terms of quality, one would expect following past work that referrals would be of higher quality than non-referrals.[10] For example, if incumbent workers are altruistic toward their friends (Bandiera *et al.*, 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 & 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 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.

[10]This may occur because more precise signals are observed on referrals relative to non-referrals (Simon & Warner, 1992; Dustmann *et al.*, 2015); because good workers are friends with people like themselves (Montgomery, 1991); or because referrer-referral ties reduce moral hazard (Castilla, 2005).

As noted by Bénabou & 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 & 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 & 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.[11]

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 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 functioning well. Also, workers may care more about respect in better labor markets, as their outside option may be better.

Last, how would we expect referral prevalence to vary by job type? If people are altruistic toward their friends, we would expect that people would be more willing to refer friends for better jobs, at least comparing jobs within the realm of other lower-skill jobs.

[11] 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.

2 Study Background

The study firm. The firm is one of three main grocery chains in an Eastern European Union (EU) country. We avoid naming the country to protect confidentiality, as the firm is one of the largest in the country. 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 jobs in many countries, attrition is high, at an annual rate around 60% in the pre-RCT period.[12] Turnover costs are non-negligible, with direct (administrative and training) costs around €250 per exit, plus additional costs due to lost productivity (details in Appendix A.11). In meetings with the authors, executives expressed strong interest in reducing attrition, and this helped motivate our study.

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.[13] Having an ERP was quite natural

[12] As discussed below, turnover is particularly high for new hires: about half exit in the first 5 months. Such turnover rates are not atypical for low-skill jobs, e.g., about half of the call-center workers in Burks *et al.* (2015) exit within 90 days. We use "attrition" and "turnover" synonymously for worker exit.

[13] Before running this paper's RCT, we worked with the firm on an RCT where (1) career incentives were emphasized to workers, or (2) the CEO communicated to store managers about the importance of

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.[14] 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, a referral occurs when someone is hired via the firm's formalized referral process. The process was designed so that making a referral would be as quick and easy 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 treatment stores (Figure 1), with variations depending on 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 central HR office ensured that 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 knew that an RCT was occurring.[15] Beyond the information provided, workers in R50, 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

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.

[14]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).

[15]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.

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). 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.[16] 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 and to increase the referral bonus to €30 at hire and €100 after 3 months (see Section 7 for rollout details).

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 workers it will pay, and workers are aware of this. In the surveys we carried out (explained

[16]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.

more later), we find no evidence of problems with procedural compliance in the RCT.

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.[17] 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.[18] We observe referred applicants (hired and not hired), though 85 of the 88 referred applicants during the RCT are hired, so referred hires and referred applicants are almost the same. Among non-referrals, we only observe hires, not applicants, though the firm informed us that roughly 20% of non-referred applicants are hired. 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.

Besides firm administrative data, we use surveys we carried out before, during, and after the RCT. In line with Shaw (2009) and Ichniowski & Shaw (2012), the surveys cover different types of respondents: store workers, store managers, and the country's general public. Topics include reactions to the ERPs; beliefs about mechanisms for the ERP effects; social perceptions of grocery jobs and our firm; and manager time use (Bandiera *et al.*, 2020). 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.[19] 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 compare ERP stores (i.e., any of the treatments) vs. control stores, and we find no significant differences.

[17]The firm's non-grocery workers are not part of the RCT and are not in our worker-month panel. Thus, our analyses of non-grocery workers are more limited and use auxiliary data.

[18]In the firmwide rollout (Jan-May 2017), we only have data on who made referrals, not who was referred.

[19]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 & 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.

Panel A of 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.[20] 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.

Panel B of Table 3 analyzes store-level referral hires during the RCT and yields similar conclusions. Having an ERP increases referral hires by 0.37 workers.

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 Topa (2011) for Europe. Obtaining under 10% of hires through ERPs is also common in other firms.[21] As noted by Topa (2019), the informal passing along of

[20] 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.

[21] 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

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.[22]

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 (Hoffman, 2017). 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 is attrition, and our secondary outcome is absence. We focus on attrition for three reasons. First, like many firms, our firm regards high attrition as a critical business issue, causing it to spend large sums recruiting and training new hires, and high-turnover stores also have lower sales.[23] Second, worker retention is a standard measure of match quality (e.g., Dustmann *et al.*, 2015). 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.[24]

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

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

[22]Consistent with formal referrals differing qualitatively from informal referrals, in our data, there is no evidence that ERPs boost informal referrals, and 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.

[23]Table B7 shows the negative correlation between attrition and sales. High attrition also imposes serious costs in US retail firms (Ton, 2014). Kuhn & Yu (2020) show that worker exits harm performance in Chinese retail stores. As a policy issue, when attrition is high, firms may invest less in worker skill development.

[24]Another common outcome in supermarkets is items scanned per minute (Mas & 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.

pre-RCT means of store-level characteristics (with the full list in the table notes). Table 4 estimates linear probability models of the attrition of workers hired during the RCT. In line with past work, column 1 of Table 4 shows that, compared to non-referred workers, referred workers are 7.0 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.[25]

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. As the distribution of monthly absences is highly skewed, we use negative binomial models.[26] Column 4 shows that referrals have 19% 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 their friend gets a bonus. Referral differences do not significantly vary by bonus size.

Adding store dummies. For analyzing referral/non-referral differences, we can add store fixed effects, which is useful given it is a non-randomized comparison.[27] Appendix Table 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

[25]If referrals were staying longer than non-referrals solely to get a bonus, then referred attrition would be higher than non-referred attrition after 5 months, but this is not the case. Also, that referral differences are larger in the first 5 months than after does not mean that referrals are not useful for the firm. Even if referrals 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.

[26]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.

[27]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.

do not observe significant differences in our non-attrition performance variable of absence.

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. Our statistical significance for attrition is also robust to accounting for multiple hypothesis testing, namely, that we have two key outcome variables (see Appendix A.6 for details).

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%. Results are further robust to (and slightly stronger when) including store-specific time trends.

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. There is no pre-trend. While ERP effects seem to take a few months to realize, with the largest estimated impact in the RCT's 2nd quarter (Mar. 2015-May 2015), we cannot statistically reject that effects are the

same throughout the RCT. The magnitudes of ERP effects remain economically important over the 13 months of RCT, with an ERP impact of -15% in the RCT's final quarter. 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 showing the difference between Control and ERP stores, panel (c) shows separate regressions restricting to Control or ERP stores. The attrition rate is higher during summer (as for many retail jobs) and increases over time as the country's overall economy improves. 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 ("quits"), with no significant impact of ERPs on involuntary attrition ("fires"). Focusing on quits further enhances event study statistical precision.

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. For example, before we started working with the firm in 2015, the firm tried out increasing training for cashiers, introducing this gradually across stores, but this failed to reduce turnover.

The RCT ERP is one of the most successful initiatives the firm has ever had in terms of reducing turnover. This is noteworthy, as the firm is mature and modern, and has had top executives with prior experience leading major grocery chains in Western Europe.

Besides being economically sizable, the magnitude of the ERP effects is still 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.

Absences. Appendix Table B3 shows no significant impact of ERPs on absence. Column 2 (baseline) estimates a coefficient close to 0. Column 4 ("diff-in-diff") indicates that ERPs reduce absence by 8%, but the coefficient 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% in hires is consistent with our 15-20% drop in turnover. The impact is statistically significant at the 10% level in Panel B, but not in Panel A.

Table B4 also shows that ERPs do not have a statistically significant 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 2.0-2.3%; increase sales per worker by 2%; and cut shrinkage by 1.7-2.5%. For these additional outcomes that we did not pre-register as main outcomes, we lack power to detect small to moderate changes using only store-month data.[28] We see no evidence that ERPs harmed store-level outcomes.

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 a 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. Workers in R0 stores have roughly 15% lower attrition than workers in Control 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

[28]Thus, in analyzing impacts on profits in Section 5, we will combine the treatment effects on attrition with two different values of 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).

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.7.

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 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.[29]

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 ERPs help store managers learn about what type of candidates to hire, or free up time spent on interviews? This is also unlikely to explain our results. Beyond the fact that ERPs have large effects in stores where no referrals are made, ERPs have similar percentage impacts on incumbents relative to their impacts on new hires. This mechanism cannot explain why ERPs reduce 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.8).

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.

[29]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.

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 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 & Johannesson, 2007), or because it gives workers some voice (Hirschman, 1970; Turco, 2016) or some real authority (Bartling *et al.*, 2014; Rasul & Rogger, 2018; Rasul *et al.*, 2020) 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 relatively few workers made referrals during the RCT, workers may think that they will do so 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 may not have been willing to call HR to register the referral.[30]

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 above four mechanisms were presented, with the option for the respondent to provide their own alternative mechanism always presented last, as is common in surveys.

Panel A of Table 6 shows that mechanism (1) is by far the most commonly chosen 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 believe in mechanism (3), but the overall message from both groups is the same.[31]

[30]Recall that ERPs fail to boost informal referrals in our *During RCT* worker survey (see footnote 22).

[31]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,

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% said (b), and 67% said both were equally likely. While (a) and (b) may be conceptually distinct, workers view them as closely related.[32] 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.9.

Beyond surveying firm managers and workers about reasons for the indirect effects, a complementary approach to identifying mechanisms is to use a vignette. 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 used 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 likely make the employee feel more respected, whereas only 11% said it was unlikely to make the employee feel more respected, and 21% said they were uncertain. Appendix A.10 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.*,

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).

[32]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.

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 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 to hire someone. Full costs additionally account for lost productivity costs, which are hard to precisely detect experimentally, but which we account for following Blatter *et al.* (2012).

We also calculate benefits separately for referrals, non-referral new hires, and pre-RCT incumbents. For population p, the benefit of lower turnover is $\theta_p t_p c$, where θ_p is the share of ERP store worker-weeks represented by p, and t_p is the attrition difference within p. See Appendix A.11 for details on c and Appendix A.12 for details on calculating profits.

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.

Results. Panel A of Table 7 reports the overall benefits from having an ERP vs. not. Starting with $c =$€250, the benefit from an ERP is €2.44 per worker-month, far above the cost per worker-month of €0.10. Overall net profit per worker-month is €2.34 or 0.6% of labor costs. Only 5% of the turnover benefits come from ERPs yielding referrals, who have lower attrition. Most of the benefit comes from incumbents and non-referral hires having lower attrition in ERP stores. Under $c =$€1,150, ERPs become 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 ERPs are evaluated solely based on lower turnover from referrals (i.e., direct benefits alone), benefits outweigh costs, with a return on investment (ROI) of 32% for $c =$€250 and an ROI of 507% for $c =$€1,150, where ROI=100*(Benefit-Cost)/Cost. However, the comparison is vastly different once we account for ERPs' indirect effects on turnover. Accounting for direct and indirect effects, the ratio of ERP benefits to costs goes up by a factor of 19.

Panel B of Table 7 repeats Panel A separately for the different ERPs. We use the more conservative $c =$€250. Direct benefits (i.e., turnover benefits from referrals hired during the RCT) are non-monotonic in the bonus, reflecting differences in the quality and prevalence of referrals across treatment arms. Overall turnover savings are also non-monotonic in the bonus, reflecting the non-monotonic relation in column 1 of Table 5. Focusing only on direct

benefits yields misleading conclusions, e.g., on direct benefits alone, there is little profit benefit from R120. R0 yields large overall 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 show 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.[33]

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%.[34] 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, 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

[33]Table B7 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 B8 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 fairly large.

[34]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.

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.[35] Thus, our results are broadly consistent with work 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 & 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.[36]

To account for multiple hypothesis testing, Table 8 shows Westfall-Young adjusted p-values for the interaction terms. These support that the treatment heterogeneity on overall ERP effects for pre-RCT performance and labor marker quality is statistically significant.

Profits. Panels C-D of Table 8 show that our attrition heterogeneity estimates imply

[35]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 (e.g., product selection and technology are similar across stores). Appendix A.13 discusses further.

[36]In our data, differences between referrals and non-referrals are also larger in better labor markets (Table B8). However, this is far less consequential for profits compared to the indirect benefits of ERPs being larger in better labor markets.

strong heterogeneity in profits. At stores in p90 of pre-RCT performance (based on shrinkage, 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.14 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. As before, referrals are made by calling HR. The new ERP was rolled out firmwide in Jan. 2017, and, as in the RCT, was introduced using posters, letters, and meetings (Figure E1 shows posters from the rollout). As in the RCT, the firm did not accompany the new ERP by emphasizing that workers were valued or that retention was important. Unlike for the RCT, for the time period of the firmwide ERP rollout (i.e., Jan-May 2017), we only have data on who made each referral, not on who was referred.

Results on referral rates. Panel A of Table 10 summarizes the ratio of referrals made to hires across different jobs and time periods. Among grocery store workers, the ratio of referrals made to hires is 12% in the post-RCT period (Jan.-May 2017), which is an increase above the 5% ratio in the RCT's highest bonus arm, R120.[37] In contrast, among non-grocery workers in food production and logistics, the post-RCT ratio is 37%.

Thus, while front-loading and raising the bonus increases referrals for grocery jobs, the ratio of referrals made to hires only increases to 12%; this suggests that front-loading plays a

[37]The post-RCT ratio for grocery workers is similar across the 5 arms of the previously completed RCT (Panel (a) of Figure 2).

role, but is unlikely the main cause of our RCT finding that ERPs modestly boost referrals for grocery jobs. Also, the referral rate is substantially higher for non-grocery jobs.

Grocery jobs can be separated into cashier and non-cashier grocery jobs (e.g., butcher, baker, assistant manager), with non-cashier jobs seen as more attractive. Table 10 shows that the referrals ratio is higher for non-cashier jobs both during and after the RCT.

Results on attrition. For grocery workers, as noted above, the RCT treatment effect on attrition vanishes once an ERP is rolled out to control stores. While Figure 4 uses all our data, we can also use data only from the RCT and post-RCT periods (i.e., no pre-RCT data) to estimate how much attrition falls in control stores relative to treatment stores due to the rollout. As seen in the diff-in-diff in Table B9, the rollout ERP reduces monthly attrition by 1.36pp in control stores, which is similar (and opposite) to the 1.19pp RCT drop in treatment stores in column 4 of Table 5. This corroborates that ERPs reduce attrition. We cannot analyze the attrition of non-grocery workers because our main worker-month panel only covers grocery workers.

7.1 Using Surveys to Understand the Referral Rate 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.[38] 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

[38]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.

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.[39] 51% 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.[40] 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.15 discusses alternative explanations.

Vignette evidence. As discussed in Appendix A.10 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 hiring. ERPs may affect firms in two ways: (1) *Directly*, by affecting referrals in quantity or quality, or (2) *Indirectly*, via costs or

[39]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.

[40]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.

benefits separate from affecting referrals. We use the first RCT on ERPs in a for-profit firm and the post-RCT firmwide ERP rollout to better understand these two pathways.

On (1), we find that larger bonuses increase referrals and that referrals are higher quality than non-referrals, though the share of referrals is modest, at least relative to statistics from studies of informal referrals. Despite this, the direct profit benefits of ERPs are greater than ERP costs (with an ROI of at least 30% based on direct benefits alone), so ERPs are a valuable management practice in our setting based on direct benefits alone. Larger bonuses decrease referral quality, and the direct profit benefits of ERPs are non-monotonic in the bonus. There are more referrals for more attractive jobs. These results broadly support models where workers are altruistic toward friends (e.g., Bandiera *et al.*, 2009; Ashraf & Bandiera, 2018), 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 durably 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, thus swamping the direct benefits, and profit gains are larger in stores with better pre-RCT performance and in better local labor markets. 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. The indirect effects we observe broadly support the Ellingsen & Johannesson (2008) model of respect in the workplace. Beyond ERPs, our results help rationalize why firms seek employee participation in hiring (beyond the importance of using worker information for selection). 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.

We are aware that—as in many other RCTs—we cannot fully rule out alternative mechanisms for the indirect effects. Our main contribution, though, is to be the first paper to measure the overall effects of ERPs, as well as the first to separately measure the direct and indirect effects of ERPs. We believe that our findings help rationalize why ERPs are a common management practice. Specifically, not only can ERPs be profitable based on the referrals they generate, but they may be highly profitable and have first-order effects on total firm labor costs even when they generate relatively few referrals.

Our 13-month RCT is quite long relative to most existing studies on management practices[41] and impacts on attrition were as large in the last quarter of the RCT as in the

[41]E.g., the work-from-home RCT in Bloom *et al.* (2014) lasted for 9 months. In another noteworthy example, Jayaraman *et al.* (2016) study the impact of raising wages on productivity. Initial effects dissipate by month 2 and they refer to their 4 months of data as an extended period of time. Most firms are unwilling to carry out firm-wide RCTs for many years when the goal is to learn from the RCT to maximize profits.

first. Still, one may wonder whether indirect effects would have persisted several years into the future. We cannot experimentally answer this question because the firm rolled out ERPs to the whole firm after the successful RCT, but we can look instead at whether the use of the management practice persisted in the longer-run (Bloom *et al.*, 2020). 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. 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 sizable indirect effects in other contexts? As seen in Panel B of Table 10, we observe substantial indirect effects for different jobs and ERPs within our firm, suggesting that indirect effects could be substantial for other low-skill jobs. Our analysis does not cover high-skill jobs, but 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. Higher skill jobs also often have much lower hiring rates, even for referred workers. 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 external validity of the indirect effects, 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 agree 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.

Future RCTs can examine the ideas from our paper in other settings, both other low-skill settings and high-skill ones, and both for ERPs and other management practices.

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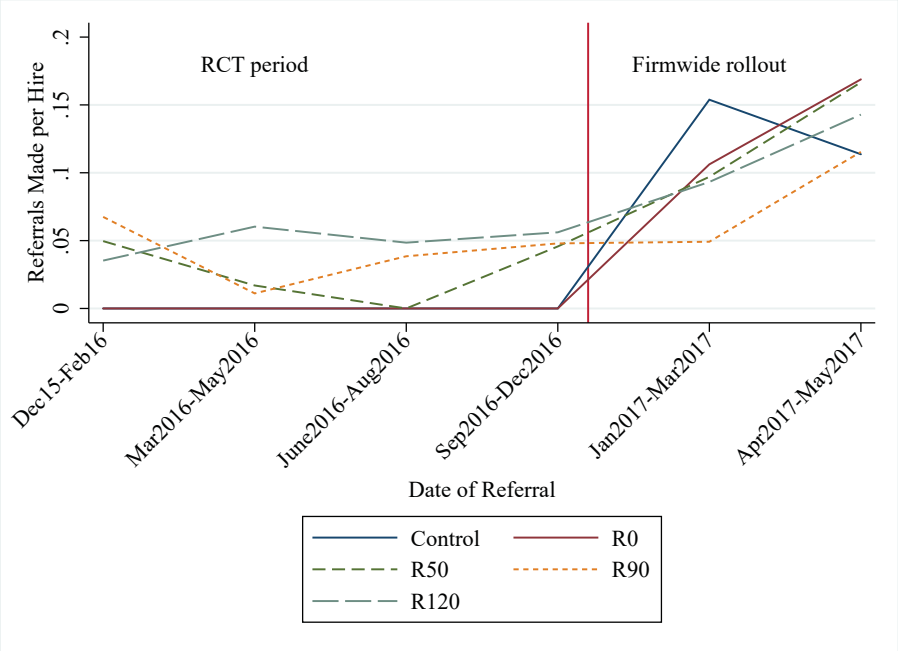
Figure 1: Referral Program Posters Used During RCT (translated and with firm identifiers redacted)

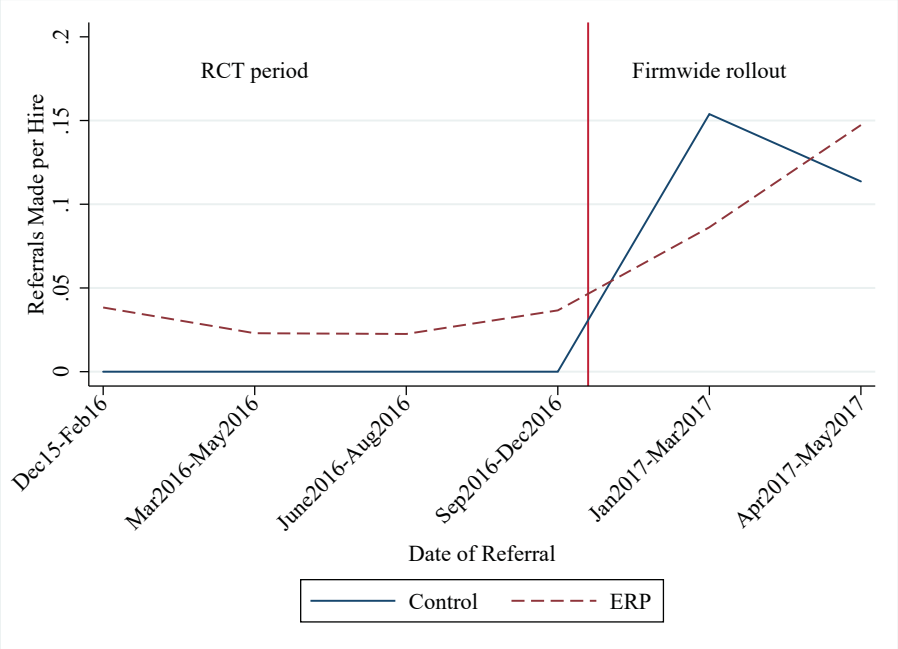
(a) Information Only Arm (R0)

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

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



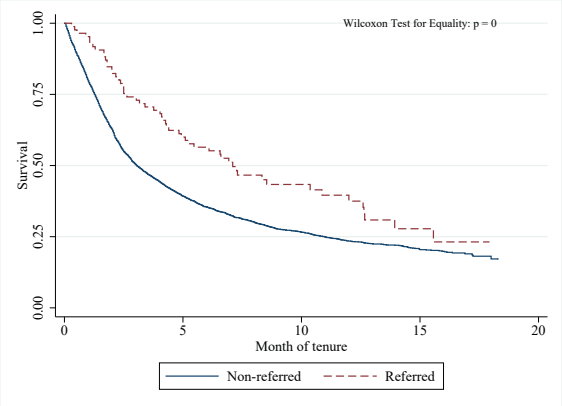
(a) Five Arms



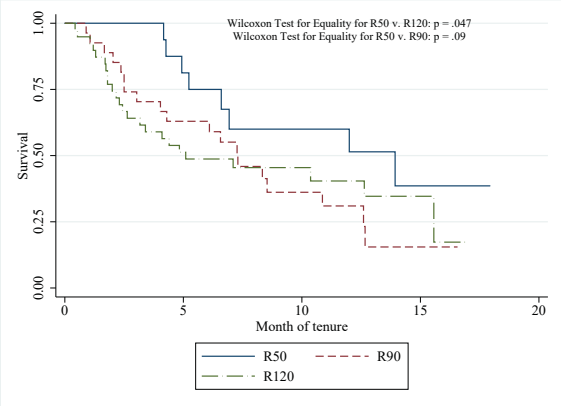
(b) ERP vs. Control

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.

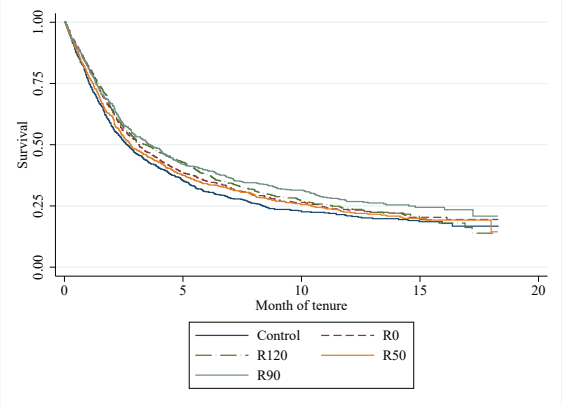
Figure 3: Survival Curve Comparisons



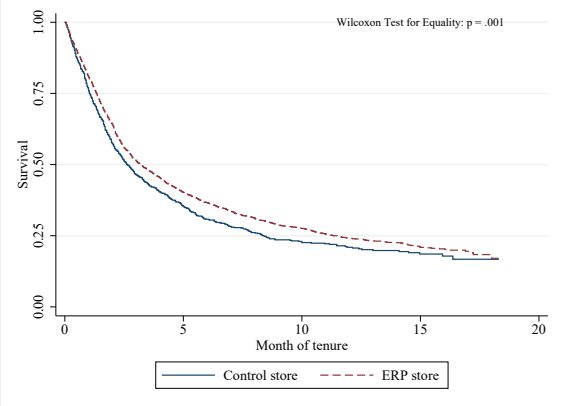
(a) Referrals vs. Non-referrals



(b) Referral Survival by Bonus Level



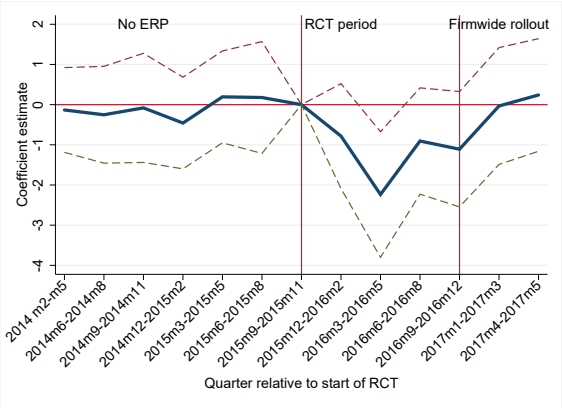
(c) Survival of Non-Referrals by Bonus Group



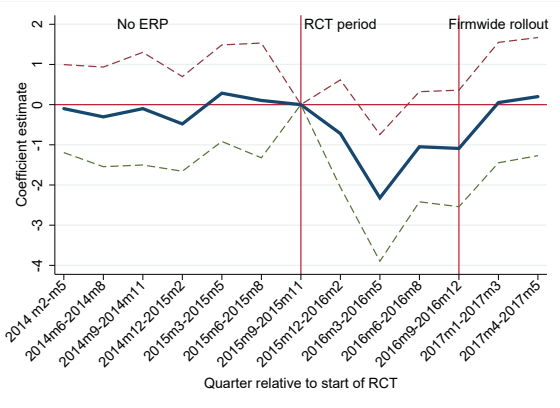
(d) Survival of Non-Referrals, by ERP Stores vs. Control

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.

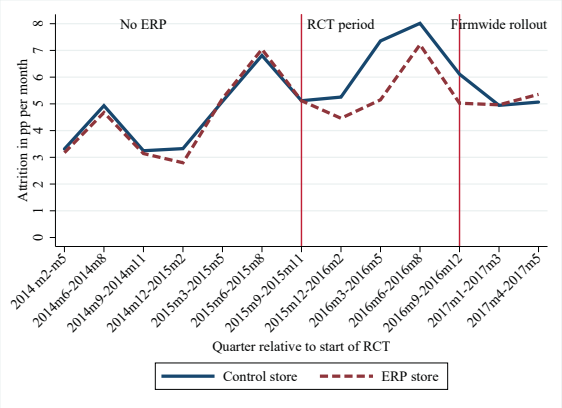
Figure 4: Event Studies: ERP Lowers Attrition during the RCT, and the Effect is Reversed Once ERP is Rolled out to Control Stores. ERP Effects are Similar in Stores with No Referrals During the RCT.



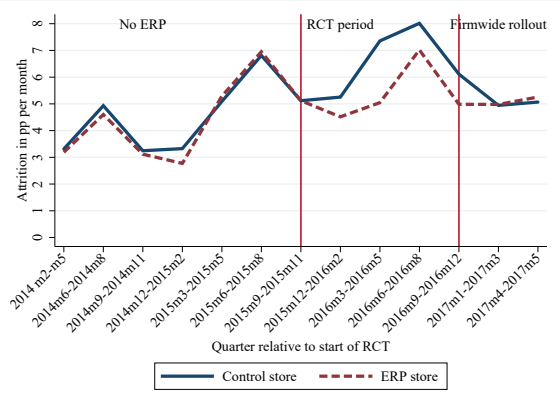
(a) ERP Impact in All Stores



(b) ERP Impact in Stores with No Referrals



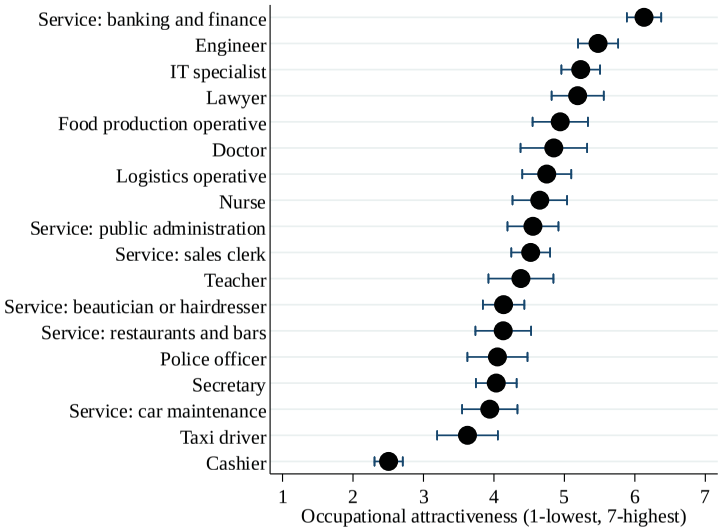
(c) ERP vs. Control in All Stores



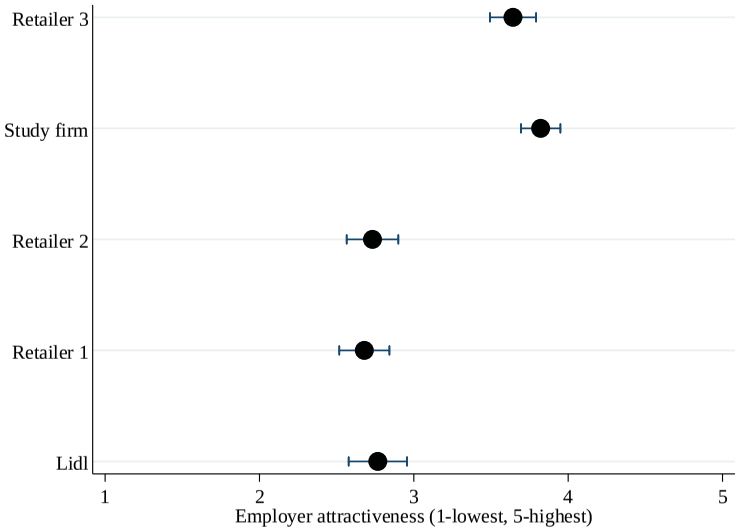
(d) ERP vs. Control, Stores with No Refs

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 95% 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. We divide the 13 months of the RCT into Dec. 2015-Feb. 2016, March 2016-May 2016, June 2016-Aug. 2016, and Sept. 2016-Dec. 2016, but results are similar if have 4 months for the first quarter instead of the last quarter. The RCT actually 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 (see GSS Methodological Report 122 by Tom Smith and Jaesok Son). Specifically, to reduce survey time and ensure maximize time, respondents are asked about 6 occupations from our overall list of 18 occupations. Each respondent 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 19, 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). The two treatments from Friebel *et al.* (2018) are also balanced across the treatments here, with p-values of 0.87 (column 6) and 0.61 (column 8) for one treatment and 0.77 (column 6) and 0.82 (column 8) for the other treatment. * 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: The Impact of ERPs on Whether New Hires are Referred (Worker-level) and on a Store's Total Number of Referrals Hired During the RCT (Store-level)

	(1)	(2)	(3)
Panel A: The Impact of ERPs on Whether New Hires are Referred			
DV = Hire is a Referral, Observation is a Hire			
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
Panel B: The Impact of ERPs on the Total Referrals Hired During the RCT			
DV = Total Refs Hired, Observation is a Store			
R0	-0.065*	-0.079	
	(0.037)	(0.107)	
R50	0.268**	0.340**	
	(0.126)	(0.163)	
R90	0.497**	0.506**	
	(0.220)	(0.223)	
R120	0.747**	0.715***	
	(0.300)	(0.258)	
ERP			0.371***
			(0.133)
Observations	238	238	238
Controls	No	Yes	Yes

Notes: Standard errors clustered at the store level. The implied p-values here are also similar when made using randomization inference following Young (2019). In Panel A, an observation is a grocery worker hired during the RCT and the dependent variable is whether the worker was referred. 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. In Panel B, an observation is a store and the dependent variable is the number of referrals made. The controls are the store-level controls from Panel A. **The Panel B findings are similar if the dependent variable is total referrals *made* (instead of hired) during the RCT or if the dependent variable is the share of RCT hires who are referred.** * 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 Panel A of 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 by store are in parentheses. "Rand-t" randomization inference p-values following Young (2019) are in square 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 Panel A 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 19. 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.11. See Appendix A.12 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)
Westfall-Young p-vals	{0.92}	{0.64}	{0.27}	{0.68}	{0.22}
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)
Westfall-Young p-vals	{0.06}	{0.06}	{0.07}	{0.75}	{0.02}
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%

Main 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.

Additional notes on inference: To account for multiple hypothesis testing in analyzing treatment effect heterogeneity, the curly brackets display family-wise error rate (FWER) adjusted p-values based on the Westfall & Young (1993) free step-down procedure (5,000 replications) and while accounting for clustering by store by using a clustered bootstrap. In both Panels A and B, the family of tested hypotheses is the set of five tests about whether the coefficient on ERP X Characteristic equals 0 for the 5 displayed characteristics. That is, each family has 5 hypotheses. For multiple hypothesis testing adjustments applied to our baseline overall attrition results in Table 5, please see Appendix A.6. Stars are based on the conventional clustered-by-store standard errors in parentheses, with * 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 Time Period: Referrals Made as a Share of Hires (Panel A) and the Share of Turnover Savings from Referral Hires (Panel B)

	RCT	Post-RCT Rollout
Panel A: Referrals 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 (i.e., the Share of Benefits that are Direct Benefits)		
All Grocery Jobs	5%	14%
Cashier	4%	13%
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%. Italics are used to indicate that grocery jobs are separated into cashier and grocery non-cashier jobs (e.g., butcher, baker, assistant manager). The time period for the RCT is December 2015-December 2016. The post-RCT period is January 2017-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 share, whereas the indirect benefit share is equal to 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. In both panels, the entry for non-grocery jobs during the RCT is missing because the RCT was restricted to grocery jobs.