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

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

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Abstract

Employee referral programs (ERPs) are randomly introduced in a grocery chain. On direct effects, larger referral bonuses increase referral quantity but decrease quality, though the increase in referrals from ERPs is modest. However, the overall effect of having an ERP is substantial, reducing attrition by 15% and significantly decreasing labor costs. This occurs, partly, because referrals stay longer than non-referrals, but, mainly, from indirect effects: non-referrals stay longer in treated than in control stores. The most-supported mechanism for these indirect effects is workers value being involved in hiring. Attrition impacts are larger in higher-performing stores and better local labor markets.

JEL Classifications: M51, M52, D23
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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. 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 45% 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. An important caveat in interpreting referrals having higher retention than non-referrals is that the referral bonuses incentivize workers to stay at least five months.

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, and are sizable for both new workers and workers hired prior to the RCT. These effects cannot be mainly attributed to the incidence of referrals or

[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.

to peer effects because turnover falls in treatment stores where no RCT referrals are made. Nor are the effects related to managers behaving differently in treatment stores. Instead, our surveys 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. Arguably the most plausible alternative, that workers stay because they hope to make referrals in the future (i.e., an option value story), is much less supported.

Section 5 shows that ERPs are highly profitable, reducing labor costs by up to 2.8%. About 5% of the savings reflects that referrals have higher retention than non-referrals (direct benefits), while 95% of profit gains come from ERPs boosting the retention of non-referred workers (indirect benefits). While direct benefits exceed the costs of an ERP, only comparing referrals vs. non-referrals dramatically underestimates the total benefits of an ERP.

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. Overall effects are also larger among male workers compared to female workers.

Encouraged by the effects of the RCT, especially the indirect ones, 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 increase from before, consistent with the increased bonus, but 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.

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 noteworthy in our paper is an RCT of a common management practice *at-scale*.[4] To our knowledge, ours is the first, large-scale, within-the-firm RCT on any hiring procedure in any context.[5] If only a

[4]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.

[5]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.

small subset of stores were treated, as in many within-firm RCTs, it would have been 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 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.[6]

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 (e.g., Brown *et al.*, 2016; Heath, 2018; Hensvik & Skans, 2016), 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.[7] 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 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. Ekinci (2016) is an exception, providing a model of ERPs where potential referrers have career concerns.

[6]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.

[7]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.

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.[8] 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.

As noted by Bénabou & Tirole (2003), decisions to delegate (e.g., by having an ERP) can communicate to workers that the firm believes workers to be of high ability and have good judgment. Workers may view such beliefs as a sign that the firm is likely to treat them well. Another possibility is that workers may intrinsically value the firm believing them to

[8] 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; Hensvik & Skans, 2016); or because referrer-referral ties reduce moral hazard (Castilla, 2005; Heath, 2018).

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.[9] Provided that workers value feeling esteemed, having an ERP should increase retention, and Appendix D shows this formally in a signaling model. This prediction can be made even for stores where no referrals are made, since the signalling benefit occurs due to the firm having an ERP, not due to workers making referrals.

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 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 having a bonus or of using larger bonuses is also ambiguous. On one hand, using a bonus, and especially a larger one, exposes the firm to greater risk of opportunism, so it could be a sign of greater trust. On the other hand, using a bonus is not necessary to an altruistic worker who is strongly internalizing the interests of their friend.

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 respects 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.

[9] This final point, that altruistic workers are more concerned in avoiding bad matches, may fail to hold if the people who benefit more from a job are ones who are less desirable for the firm.

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.[10] 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. One is the store manager, 19 are general store workers, whom we refer to as cashiers, 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 several functions, including stocking, cleaning, check-out, and answering customer questions. Most (95%) work full-time, and receive an average monthly wage of roughly €350 (with minor variation depending mainly on if location is urban or rural), plus a bonus tied to store performance (4% of wages, on average). This performance bonus is quite small compared to the referral bonus. 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 manage-

[10] 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.

ment and suggested implementing an ERP via an RCT.[11] Having an ERP was quite natural for the firm to consider for several reasons. First, the firm had an ERP during the 2000s, though it was discontinued in 2008 when the firm's growth came to a halt. Second, some of the firm's competitors pay referral bonuses. Third, we argued that an ERP could help reduce turnover. The firm was willing to do an RCT in order to investigate whether to have an ERP and in what form.[12] 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.[13] Beyond the information provided, workers in R50, R90, and R120

[11] 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 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.

[12] 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).

[13] 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,

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

Rationale for bonus structure. We suggested a 5-month tenure threshold because a substantial share of cashiers leave in the first 5 months (about half in our pre-RCT data) while attrition is significantly lower after that. Tenure thresholds are very common in ERP bonuses (Brown *et al.*, 2016; Burks *et al.*, 2015; Fernandez *et al.*, 2000). 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.[14] 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. Thus, there appears to be no compliance problem with the firm implementing the RCT.

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

but rather solely monitored whether store managers were complying.

[14] 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.

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

Data. We assemble the firm's personnel and accounting data for Feb 2014-May 2017 to create worker-month and store-month panels. The personnel data are for grocery store workers, cover over 18k workers (7k active only in the pre-RCT period, and 11k active during the RCT or beyond), and contain standard personnel variables (e.g., hire and termination dates, exit codes), as well as absences, earnings, bonuses, hours, and demographics.[15] 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.[16] 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 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.[17] 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

[15] 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.

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

[17] 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.

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.

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 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 over time, quarter by quarter, showing a modest ratio during the RCT. After the RCT, when a single ERP is rolled out to the entire firm with an increased bonus of €130 that is paid more quickly (€30 instead of €15 at hire, and the remainder of the bonus after only 3 months instead of 5 months), referrals increase, with similar referral rates across the former RCT arms.

Using regressions, Table 3 shows RCT impacts of ERPs on whether a hire is referred:

$$Referred_{is} \quad = \quad \alpha + \sum_k \beta^k R_s^k + X_{is}\delta + \epsilon_{is} \tag{1}$$

where $Referred_{is}$ is a dummy for whether worker i in store s was hired via referral, R_s^k are dummies for the different store-level treatments (i.e., whether store s has treatment k) where Control store is the excluded category, β^k are the coefficients of interest, X_{is} are control, and ϵ_{is} is an error. The controls are quarter-year of hire dummies, a dummy for being a cashier, demographic controls, and pre-RCT means of store-level characteristics (with the full list in the table notes). Standard errors are clustered by store, as ERPs are randomized by store. In addition, we perform randomization inference (Young, 2019), both for these results on direct effects and for our main results later on overall effects. The resulting p-values in square brackets are similar to those from conventional clustering-by-store inference.[18]

Column 1 of Table 3 regresses whether a hire is referred on dummies for the four treatment arms (an observation is a hire), showing that larger bonuses lead to more referrals.[19] The results are similar with controls in Column 2. Instead of using dummies for the four

[18]Our findings are also unchanged under a wild bootstrap (Cameron *et al.*, 2008). We cannot include store fixed effects in Table 3 because ERPs are randomized by store.

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

ERPs, Column 3 uses a dummy for having any of the four ERPs (the excluded category is again Control). Having an ERP increases the chance an employee is referred by 2.4pp. This is highly statistically significant, but seems economically modest.

Columns 4-6 of Table 3 analyze store-level referral hires during the RCT and yield 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 noted 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.[20] As noted by Topa (2019), the informal passing along of job information from person to person may differ qualitatively from deciding to formally refer someone to one's employer, with more important reputational considerations in the latter.

The 88 referrals occur in 34 stores and are made by 75 referrers. 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.[21] Attrition is the firm's primary HR key performance indicator (and its secondary one is absence). 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

[20] Little is known about the share of workers getting hired through ERPs since survey data usually measure informal referrals. As of August 2019, 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 was 33%, though a non-trivial share of firms (14%) got 10% or less of their hires from ERPs. For the four European firms on ReferralPrograms.org, the average share of hires from ERPs was 12%, the same percentage as in grocery jobs at our firm in the post-RCT rollout (see Section 7). Also, talking to another large grocery chain in the country where our study firm is located, that firm's share of hires from ERPs was 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.

[21] Panel B of Table B7 shows the negative correlation between attrition and sales. High attrition also imposes serious costs in US retail firms (Ton, 2014). Kuhn & Yu (2021) show that worker exits harm performance in Chinese retail stores.

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. Another common outcome in supermarkets is a worker's items scanned per minute (Mas & Moretti, 2009), but our firm does not regard it as a central performance variable, nor does the firm's IT system allow us to measure it.[22]

Attrition. Using workers hired during the RCT, Panel (a) of Figure 3 shows that referred hires have higher survival than non-referred hires.

To include control variables, in Table 4, we estimate Cox proportional hazard models:[23]

$$log(h_{it}) \quad = \quad \alpha_t + \beta \cdot Referred_i + X_{it}\delta \tag{2}$$

where h_{it} is the attrition hazard of worker i at month-of-tenure t, α_t is the log baseline hazard (i.e., tenure is controlled for nonparametrically), $Referred_i$ is a dummy for being referred, and X_{it} are control variables. Beyond the controls from Table 3 (controls for quarter-year of hire, job type, demographics, and pre-RCT store means), we additionally include current month-year dummies, and all of these together will be our standard controls throughout the paper.[24] We use data from the RCT. In column 1 of Table 4, the coefficient of -0.59 implies that, compared to non-referred workers, referred workers are 45% less likely to leave each month, as $e^{-.59} - 1 = -0.45$. One explanation for this is better matching and a second is that the referral bonus encourages referrals to stay at least 5 months.

Column 2 analyzes referral differences in turnover separately during a worker's first 5 months of tenure and also afterwards. In months 1-5, referral attrition is lower by 48% relative to non-referral attrition, whereas it is lower by 28% thereafter, though this latter difference is not statistically significant.[25] Still, both differences are economically sizable, particularly in comparison to past work.[26] While differences in attrition are strongest during the first 5 months—consistent with the structure of the referral bonus—referrals may also

[22] Unlike in Mas & Moretti (2009), in our firm, cashiers do a variety of tasks, so items scanned per minute is not conceptually synonymous with productivity. Still, not observing it is a limitation of our data.

[23] Our previous version of the paper used OLS to analyze attrition. Results are very similar with OLS.

[24] We include age in our analysis of direct effects of ERPs (i.e., in Tables 3-4) since age is only missing for a small share (0.6%) of workers hired during the RCT. Later, in our analysis of overall effects of ERPs, 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 (40% of RCT worker-months, 25% of workers), and mechanically, this missingness is highly correlated with attrition. Our main results are highly robust to different controls, including no controls.

[25] The post-5 month referral attrition difference is statistically significant if we estimate a linear probability model (using the same tenure controls for attrition that we use for absence) instead of a Cox model.

[26] Burks *et al.* (2015) estimate that referrals are 10-11% less likely to attrite than non-referrals for 8 firms with less skilled workers (i.e., call center and trucking firms). Thus, our referral attrition difference after 5 months is over twice as large as the overall referral difference in a large sample of broadly similar workers.

be less likely to attrite after 5 months. Also, we do not see evidence that referrals are more likely to attrite after 5 months, which would be expected if referrals were staying longer than non-referrals solely to get a bonus.[27] That referral differences are concentrated in the first 5 months does not mean that referrals are unuseful for the firm. Even if referrals stayed longer solely to get a bonus, which seems unlikely based on our results and based on prior evidence for matching (Brown *et al.*, 2016; Dustmann *et al.*, 2015; Simon & Warner, 1992), this could still be valuable to the firm, as the firm strongly wishes to reduce turnover.

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 85%. In contrast, for the R90 and R120 groups, the referral differences are 40% and 35%, respectively. These differences are statistically significant (p=0.02 for R50 vs. R90; p=0.01 for R50 vs. R120).

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

Absences. As the distribution of monthly absences is highly skewed, we analyze worker i's monthly absences, $Absences_{it}$, using negative binomial models of the below form:

$$Absences_{it} = G(f(t) + \beta \cdot Referred_i + X_{it}\delta) \tag{3}$$

where $G(\cdot)$ is the negative binomial function and $f(t)$ are controls for tenure. As in most European countries, it is easier to fire workers during a probationary period, and absences are thus more costly to the worker and less common (see Figure B2) during probation (Ichino & Riphahn, 2005). Probation at our firm lasts 3 months, as is typical for the country we study. Thus, we control for a probation period dummy, plus linear terms in tenure on both sides of 3 months, and these will be our non-Cox tenure controls throughout the paper.

Column 4 of Table 4 implies that referrals have 19% fewer absences per month (based on exponentiating the coefficient), but this is not statistically significant. Column 5 shows that, during the first 5 months, referrals have 36% fewer absences than non-referrals, which is sizable and marginally statistically significant, but after that, there is no evidence for a 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.[28] Appendix Table B2 shows that referral attrition differences are similar (and slightly larger) when store

[27] Appendix Figure B1 shows that referrals have lower attrition at most months of tenure.

[28] 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.

fixed effects are added. Absence differences are statistically insignificant and noisy. Broadly consistent with Burks *et al.* (2015), there are stark referral differences in attrition, but we do not observe significant differences in our non-attrition performance variable of absence.

4 The Overall Impact of ERPs on Worker Outcomes

4.1 Results

Attrition. Table 5 shows that ERPs reduce attrition of all workers, with substantial effects on both new hires and incumbents. Beyond showing robustness to randomization inference, that ERPs reduce attrition is also robust to accounting for a multiple hypothesis testing concern, namely, that we have two key outcome variables (see Appendix A.6 for details).

Column 1 of Table 5 shows the impact of the randomized ERP treatments on attrition during the RCT (as opposed to comparing referrals vs. non-referrals) using Cox models:

$$log(h_{ist}) \quad = \quad \alpha_t + \sum_k \beta^k R_s^k + X_{ist}\delta \tag{4}$$

where h_{ist} is the attrition hazard of worker i at store s at month-of-tenure t. As in equation (1), R_s^k are dummies for the store-level treatments. Relative to workers in Control stores, workers in R0, R50, R90, and R120 stores have monthly attrition that is lower by roughly 15%, 7%, 25%, and 13%.[29] These differences are statistically significant for R0, R90, and R120. Column 2 shows that having an ERP reduces attrition by about 15%. 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 of Table 5 report the results from a "diff-in-diff" Cox model where treatment arms are interacted with a dummy for whether the current month is during the RCT:

$$log(h_{ist\tau}) \quad = \quad \alpha_t + \gamma_s + \sum_k \beta^k R_s^k \cdot 1(\tau \text{ in RCT}) + X_{ist\tau}\delta \tag{5}$$

where τ indexes the current month-year and $1(\tau$ in RCT) is a dummy for τ being during the RCT. Store dummies, γ_s, account for persistent differences across stores in employee attrition

[29] For ease of exposition, as is common for Cox models (e.g., Chetty, 2008), we interpret the overall ERP treatment effect coefficients in log points as rough percentage effects. The approximation is close for overall effects, whereas we avoided doing this for Table 4, where the approximation is less valid.

and other characteristics (including treatment arm during the RCT). Relative to column 1, results are slightly stronger in column 3, with statistical significance for all 4 ERPs. The column 4 coefficient of -0.20 corresponds to a reduction of roughly 20%.

To better understand the dynamics of the ERP effects, Figure 4 presents an event study where having an ERP is interacted with quarterly dummies for time relative to the start of the RCT. Since standard event studies are based on linear models (Borusyak *et al.*, 2021), we momentarily turn away from Cox, and we estimate linear probability models:

$$y_{ist\tau} = f(t) + \gamma_s + \sum_q \beta^q ERP_s \cdot 1(\tau \text{ is q quarters from start of RCT}) + X_{ist\tau}\delta + \epsilon_{ist\tau} \quad (6)$$

where $y_{ist\tau}$ is a dummy for whether worker i at store s with tenure t attrites at time τ, $f(t)$ are controls for tenure (same as in equation (3)), β^q are the coefficients of interest, ERP_s is a dummy for whether store s has an ERP, and $\epsilon_{ist\tau}$ is an error.

Panel (a) of Figure 4 shows overall results. While ERP effects seem to take a few months to realize, with the largest estimated impact in the RCT's 2nd quarter (Mar. 2016-May 2016), we cannot statistically reject that effects are the same throughout the RCT. The magnitudes of ERP effects (i.e., a drop of 1pp below the base of 6pp) remain economically important over the 13 months of the 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.

Figure 4 immediately suggests that the ERP impact on attrition is not driven by referrals. Specifically, panel (b) of Figure 4 shows similar results to panel (a) while restricting to stores where no referrals are made during the RCT. Instead of showing the difference between Control and ERP stores, panel (c) of Figure 4 shows separate regressions restricting to Control or ERP stores. Attrition is higher during summer, as for many retail jobs, and 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, thereby reinforcing the message of the comparison between panels (a) and (b).

The impact of ERPs on attrition is driven by a decrease in voluntary attrition ("quits"), as seen in Figure B3, which plots event studies separately by quits and "fires" (involuntary attrition). ERPs have no significant impact on fires. Relative to Figure 4, precision is even stronger when focusing on quits, which is unsurprising if ERPs only affect quits.

Returning to Cox models in Table 5, columns 5-8 show that attrition impacts are sizable among both new hires and incumbents, i.e., people already working at the firm at the start of the RCT. Using equation (4), we estimate that ERPs reduce new hire attrition by 11% (column 6) and incumbent attrition by 19% (column 8). That ERPs reduce incumbent attrition is important for thinking about mechanisms, as discussed below in Section 4.2.

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 ERP effects is plausible. As another benchmark, Bloom *et al.* (2014) show that randomly assigning workers to work from home reduces attrition by half in Chinese call centers. Cai & Wang (2021) show that allowing workers to evaluate their managers reduces attrition by over half in a Chinese car factory. Adhvaryu *et al.* (2021) show that assigning workers to participate in an anonymous survey about working conditions leads to a 20% drop in turnover in an Indian garment factory.

Bonus size and attrition impacts. On visual inspection, Table 5 shows no clear relation between the size of the referral bonus and the treatment effect on attrition. For example, consider incumbents (results in column 7), who are a key group to consider since ERPs cannot affect their selection into the firm. The largest treatment effect is from R90, where €90 are paid, but the second largest effect is from R0, where no bonus is paid.[30]

To analyze this more systematically, in panel (a) of Figure 5, we take models from Table 5 and add interactions of ERP with Log(1+Bonus Size):

$$log(h_{ist}) \quad = \quad \alpha_t + \beta_0 ERP_s + \beta_1 ERP_s \times Log(1 + \text{Referral Bonus Size}_s) + X_{ist}\delta \qquad (7)$$

Figure 5 presents the key interaction coefficients, β_1. We present results for all workers and incumbents. Besides using all stores, to isolate indirect effects, we also show results restricted to stores with no referrals made in the RCT. The interaction is statistically insignificant in all 4 cases and the "zeros" are reasonably precise. The 95% confidence intervals are all between -0.028 and 0.047, meaning that increasing the bonus by 1 log point (i.e., roughly doubling) would not decrease the ERP treatment effect (i.e., increase it in magnitude) by more than

[30] A joint F-test can marginally reject that all four treatments are equal (p = 0.075). However, if one restricts to stores with no RCT referrals, in an effort to isolate indirect effects, we fail to reject that the treatment effect is uniform across the treatments for incumbents ($p = 0.19$).

2.8%, or to increase it by more than 4.7%. Thus, we rule out that increasing the bonus by 1 log point would decrease the coefficient by more than 2.8%, e.g., would take an ERP effect of 15% and turn it to more than 18%. Similar null results are obtained using other functional forms, including ERP X Inverse Hyperbolic Sine of Bonus Size and ERP X Bonus Size.

Panel (b) of Figure 5 shows that there is no significant difference in having an ERP with no bonus vs. having an ERP with any bonus. We estimate models similar to the odd columns of Table 5 except that we lump R50, R90, and R120 together to create a dummy for having any bonus. Across the four specifications, we never reject that the attrition impact of an ERP with a bonus is different from the attrition impact of an ERP with no bonus.

Absences. Table B3 shows no significant impact of ERPs on absence. We see no effect for having an ERP overall or for the individual ERPs. Why do ERPs have no significant effect on absence despite reducing turnover? First, since the distribution of absences per month is highly skewed, this yields less precision in estimation. Based on the 95% confidence intervals in Table B3, we rule out that ERPs cause large reductions in absence, but we cannot rule out moderate ones, e.g., in column 2, we cannot rule out that ERPs reduce absence by less than 12%. Second, it could be that turnover is easier to affect in general than absence. Third, the feeling of respect and voice generated by ERPs (discussed in detail below) could lend itself to reducing turnover more than absence. It is hard to separate these possibilities. Finding effects on turnover but not absence is sometimes found in other management interventions (Cai & Wang, 2021), and is consistent with our preferred mechanism discussed below.

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, and the magnitudes are economically sizable despite being noisily estimated. 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.[31] We see no evidence that ERPs harm store-level outcomes.

[31] Thus, in analyzing impacts on profits in Section 5, we combine the treatment effects on attrition with two values of the cost of turnover, with one intended to account for lost sales following Blatter *et al.* (2012).

4.2 Mechanisms for Overall ERP Impacts

The simplest reason an ERP would cut 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 ERP impacts. Section 4.2.2 discusses additional mechanisms that, while plausible *ex ante*, are *ex post* inconsistent with the main RCT results (i.e., relatively few referrals, but sizable ERP effects on attrition) and basic institutional facts. Section 4.2.3 discusses mechanisms that are consistent with the main RCT results, including our preferred mechanism of workers valuing being involved in hiring, and uses surveys and additional data patterns 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.

Second, our main attrition results are similar when restricting attention to stores where no referrals are made during the RCT. Beyond the event studies in Figure 4, this is also seen in Cox models in Table B6, which repeats Table 5 restricted to stores with no RCT referrals made. 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 ERPs.[32]

A final way to address this question is mediation analysis, with details in Appendix A.7. We repeat the analyses in columns 1-2 of Table 5, but additionally control for whether someone is referred and/or the number of referrals a person has made to date. The estimates imply that only 5% of the impact of having an ERP on attrition is mediated via having more referral hires and having workers made more referrals to date. 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.

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

[32]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.

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 sizable impacts on incumbents (in addition to affecting 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 are 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. There is evidence against this interpretation. First, HR was told to accept referrals from control stores if employees called to make them, but they did not get any referrals from control stores. Second, we instructed HR to record any complaints that it received from control stores about there not being an ERP, but there were no complaints made. Third, in all the surveys we conducted, both during and after the RCT, we never heard a worker mention anything about control store frustration.

4.2.3 Possible Mechanisms for Non-referral Channel

The impact of an ERP is strong in stores where no referrals are made; is relatively flat over time; does not systematically vary with bonus size; is substantial for hires and incumbents; is driven by quits, not fires; 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. 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; Sockin, 2021; Dube *et al.*, 2021), or because it gives workers some voice (Hirschman, 1970; Turco, 2016) or real authority (Bartling *et al.*, 2014; Rasul & Rogger, 2018) 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.

Before turning to surveys, note that the "option value" story of (3) implies that ERP impacts on turnover should increase with referral bonus size: if workers stay because they hope to make a referral in the future, then workers should be more likely to stay when the bonus is higher. In contrast, signaling mechanisms do not predict a clear, positive relation between bonus size and treatment effects. Figure 5, discussed above, shows no such relation.

To shed further light on these explanations, we conducted surveys with store managers and workers from the study firm. Specifically, we did phone surveys with 222 store managers (or 93% of store managers) and an in-store electronic kiosk survey with 113 grocery store cashiers. These cashiers are broadly representative of cashiers at the firm.[33] 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.

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 to specify whether (a) or (b) was more likely to be the mechanism or whether both were equally likely. As seen in Panel B of Table 6, 15% said (a), 18% said (b), and 67% said both were equally likely. While (a) and (b) may be conceptually distinct, workers view them as closely related.[34] We refer to (a) and (b) together as workers valuing being involved in hiring. While researchers have not previously considered that

[33]The 113 cashiers represent only about 2% of cashiers at the firm, reflecting that only a small number of workers were asked to participate in the survey. However, as seen in Appendix Table B1, the characteristics of store workers who participate in our survey are similar to the overall population of grocery store cashiers. Based on our discussion with the firm, we believe that the firm asked workers to participate essentially at random, and this understanding is supported by the evidence in Table B1.

[34]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. Our results on (a) vs. (b) are similar when restricting to respondents choosing mechanism (1) as the most likely explanation.

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.

For ERPs to credibly signal respect in mechanism (1), 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. Furthermore, this very high likelihood for referrals to get hired was well-understood by workers. In our phone survey of managers, in a random subset of roughly half the managers, we asked managers on how workers thought that referred candidates would be treated. Most indicated that workers believed 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), with $N = 102$ managers. In our in-store kiosk survey of 113 workers, we asked the same question to workers, and received a quite similar mean of 5.8 out of 7.

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 from 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 calculates the profits of hiring a referral relative to a non-referral (Fernandez *et al.*, 2000;

Burks *et al.*, 2015), but has yet to be able to calculate profit gains from an ERP. Since the ERPs reduced turnover, but did not significantly affect absence, sales, or shrinkage, we focus only on attrition impacts. ERPs may also reduce recruiting costs (e.g., due to less time interviewing candidates), but we set that aside, given we lack applicant data on non-referrals.

The attrition benefit of an ERP per worker-month is tc, where t is the ERP's impact on turnover in terms of worker exits avoided and c is the turnover cost. Since Cox gives effects in percent terms instead of absolute levels, we estimate t using an OLS version of column 2 of Table 5 (see column 3 of Table A3). 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.

Profit 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 €2.49 per worker-month, far above the cost per worker-month of €0.10. Overall net profit per worker-month is €2.39 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 30% for $c =$€250 and an ROI of 498% 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 20.

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 13%, which is higher than during the RCT, but most benefits are still not from referral hires.

TFP. Given the decrease in labor costs of 0.6%-2.8%, plus an assumed output elasticity of labor for retail of 0.7 (Shin & Eksioglu, 2015), as well as our imprecisely-estimated increase in sales of 2%, our results imply an increase in TFP of roughly 2-4%. This effect is comparable to that in Englmaier *et al.* (2017), who find a TFP increase of 4% from better communication of an incentive system, and Friebel *et al.* (2017), who find a productivity increase of 3% from team incentives, both estimated in Europe. It is smaller than the effect of working from home (Bloom *et al.*, 2014) and extensive management consulting (Bloom *et al.*, 2013) using RCTs in China and India, which is unsurprising given that those are more extensive interventions.

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, as well as by gender and job type. ERP impacts are larger in higher-performing stores, in better local labor markets, and among male workers.

Store performance. A key question in experimentally evaluating any management practice is how do effects vary based on initial performance of the treated units? To measure pre-RCT store performance, we create a performance index from three standard variables of retail performance: shrinkage rate, Log(Sales per Worker), and Log(Operational Profits per Worker). To create the index, we normalize these variables, take the mean of the three, and then normalize again to ease interpretation.[35] As seen in column 1 of Panel A of Table 8, the direct effect of ERPs on referrals does not significantly vary with store performance.

However, the overall effect of ERPs on attrition is significantly larger in stores with higher performance pre-RCT. As seen in column 1 of Panel B of Table 8, for a store at the mean level of store performance, the impact of having an ERP on attrition is about

[35] Our approach to index creation follows past work such as Deming (2017). Panel B of Table B7 shows the correlation between the variables in the index, whereas Panel A shows the correlation of our main heterogeneity variables, including the index. To assess how direct benefits of ERPs vary by store performance, one also needs to know how referral differences vary. Table B8 shows that referral attrition differences may be larger in higher-performing stores, but the heterogeneity relationship is not statistically significant.

13%. However, for a store at the 90th percentile of performance, the ERP impact is 27%, whereas for a store at p10, the impact is essentially zero. This result is robust. Table B9 shows a similar finding while splitting the sample based on above/below median pre-RCT performance, thus showing that our heterogeneity conclusion is not driven by outliers or our use of linear interaction terms. Our findings are also robust to analyzing the three performance variables individually instead of using an index (Table B10). In contrast, ERP impacts do not vary based on stores' pre-RCT attrition rates.

An interpretation of these results is that the ability of ERPs to generate feelings of involvement and respect is higher in higher-performing stores. In lower-performing stores, the notion that the firm respects its workers may be less credible. The results are also consistent with complementarity between ERPs and the other management practices that drive performance in those stores.[36] Thus, our results are broadly consistent with work showing HR 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 HR 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 3 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 roughly -2%. 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

[36] 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.

et al. (2015) examine referrals vs. non-referrals.[37]

Heterogeneity in ERP impacts by worker characteristics. Columns 4 and 5 of Table 8 examine heterogeneity in ERP impacts by two key worker characteristics: gender and job type, namely, whether a worker is a cashier. Recall that cashiers are the entry-level generalists in the grocery stores, and the alternative is being a specialist (e.g., butcher, baker) or manager. Direct effects do not vary by these characteristics. On overall effects, column 4 shows that ERP impacts on attrition are larger for men than women, though impacts are statistically significant and sizable for both genders. For women, ERPs reduce attrition by 10%, whereas for men, ERPs reduce attrition by 30%. Recalling that most grocery store workers are female, one possibility is that when men are in gender non-congruent roles, they may be more sensitive to feelings of respect relative to women, broadly consistent with gender role congruity theory in psychology (Eagly, 1987; Eagly & Karau, 2002).[38]

To account for multiple hypothesis testing, Table 8 shows Westfall-Young adjusted p-values. These support that the treatment heterogeneity findings are statistically significant.

Profits. Panels C-D of Table 8 show that our patterns of attrition heterogeneity imply strong heterogeneity in profits. At stores in p90 of performance or labor market quality, ERPs decrease labor costs by up to 5%. For stores at p10 of either, the impact is close to 0.

7 Firmwide ERP Rollout

The firm's management was pleased with the effects of the ERPs, particularly the indirect benefits, and this sentiment was shared by central HR. 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. The rapid firmwide rollout speaks to the importance of the indirect effects: based on our interactions with management and central HR, it is unlikely that an ERP would have been rapidly rolled out at-scale, including for different jobs, had it not been for the indirect benefits of ERPs.

[37] 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.

[38] Other explanations are possible, but many are not supported in the data. Referrals are only 2-3% of RCT hires for both men and women, suggesting that gender difference explanations should focus on indirect effects. Estimating models from Figure 5 separately by gender, there is no evidence that ERP treatment effects on attrition increase with bonus size for either gender. This suggests that the Table 8 gender difference are unlikely to be driven by men being especially keen to make referrals in the future. Given our overall results on mechanisms, it is natural to consider potential gender differences in how ERPs affect respect.

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 9 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. As detailed in Appendix A.14, the 12% ratio is in line with what might be expected given the RCT relationship between bonus size and referrals, and given the increased bonus generosity post-RCT. 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 role, but is unlikely the main cause of our RCT finding that ERPs modestly boost referrals for grocery jobs. Grocery jobs can be separated into cashier and non-cashier grocery jobs (e.g., butcher, assistant manager), with non-cashier jobs seen as more attractive. Table 9 shows that the referrals ratio is higher for non-cashier jobs both during and after the RCT.

Attrition. For grocery workers, the RCT impact 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 (i.e., no pre-RCT data) to estimate how much attrition falls in control relative to treatment stores due to the rollout. As seen in the Cox diff-in-diff in Table B12, the rollout ERP reduces attrition by 0.22 log points. This mirrors the 0.20 log point RCT drop in column 4 of Table 5, corroborating that ERPs reduce attrition.

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.[39]

To learn more about how cashier jobs are regarded compared to others, we did a survey of the general population in the host country. Panel (a) of Figure 6 shows ratings of different occupations' attractiveness. Cashiers score the lowest. Non-grocery jobs at the firm, namely, those in logistics and food production, rate much higher. Instead of grocery jobs being unattractive, could results be driven by our firm being unattractive? Panel (b) of Figure 6 indicates not: our firm is well-regarded relative to other retail firms in the country.

Table A6 shows that firm managers, firm workers, and the country's general public 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 classified reasons into 10 categories. In the fall 2016 employee survey, we gave cashiers the six most popular reasons from the manager survey and asked them to rank them. Details are in Appendix A.15.

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 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 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). A limitation of our data is that we do not observe items scanned per minute, so we cannot evaluate direct effects with respect to this variable.

[39]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.

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. An option value mechanism, where workers stay because they hope to make referrals in the future, is much less supported. Beyond ERPs, our results help rationalize why firms seek employee participation in hiring (beyond the importance of using worker information for selection).

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 and impacts on attrition were as large in the last quarter of the RCT as in the 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 firmwide after the successful RCT, reflecting the importance of the indirect effects. Instead, we can look 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 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 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, as in the model of Ekinci (2016).

On (2), would ERPs generate sizable indirect effects in other contexts? As seen in Panel B of Table 9, 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. While the difference is not significant, overall effects are larger for non-cashier grocery workers (e.g., managers, specialists) who are more skilled relative to cashiers (see also Panel B of 8). Still, 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 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. Differences between referrals and non-referrals have been found among broad ranges of workers, including higher-skills ones (Brown *et al.*, 2016; Burks *et al.*, 2015), and it would not be surprising to us if indirect effects of ERPs also occur in higher-skill settings.

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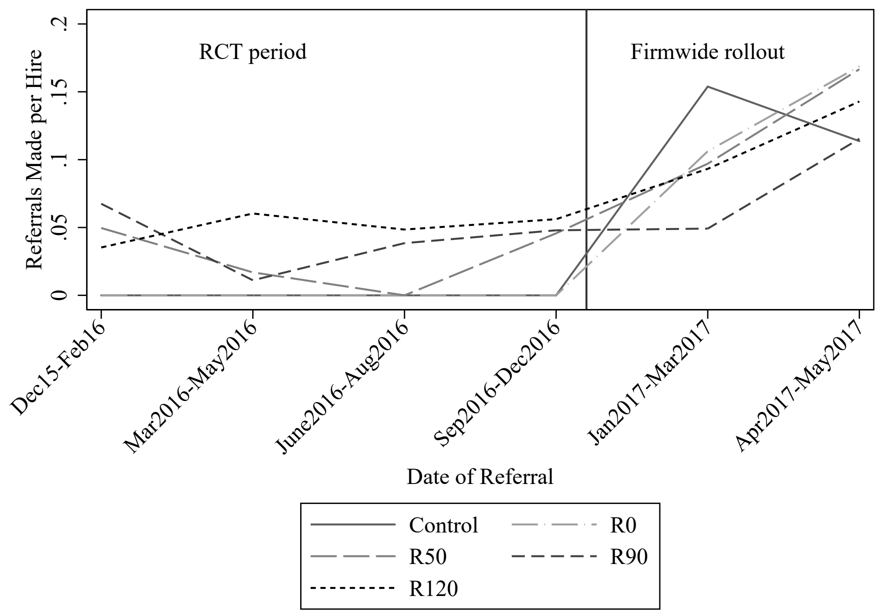
Figure 1: Referral Program Posters Used During RCT (translated and with firm identifiers redacted)

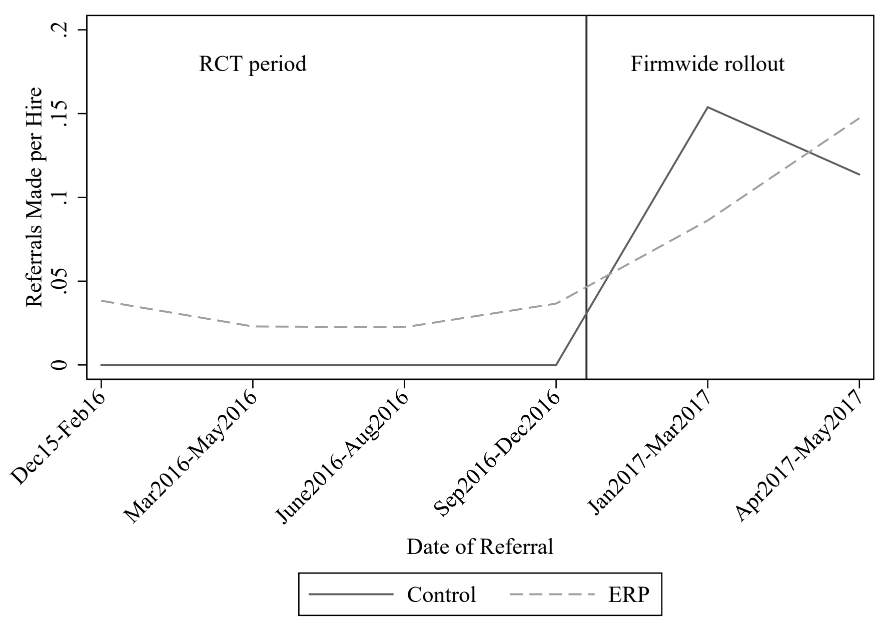
(a) Information Only Arm (R0)

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

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



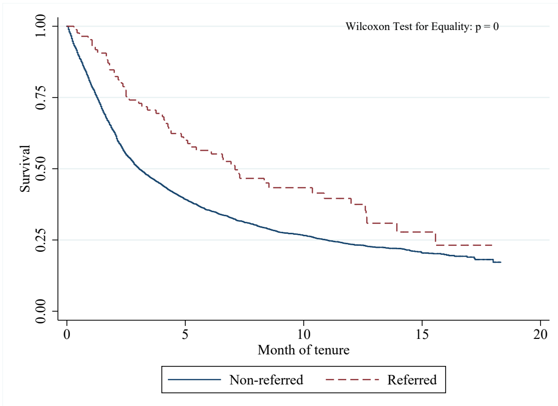
(a) Five Arms



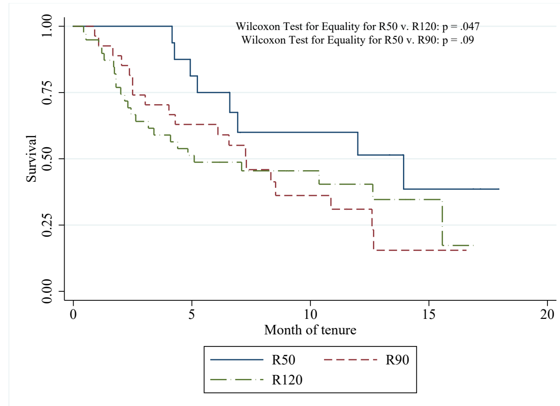
(b) ERP vs. Control

Notes: This figure shows referrals made divided by hires, and shows how this variable progresses over time 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.

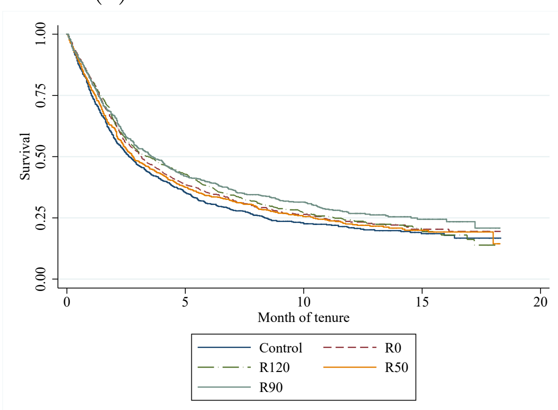
Figure 3: Survival Curve Comparisons



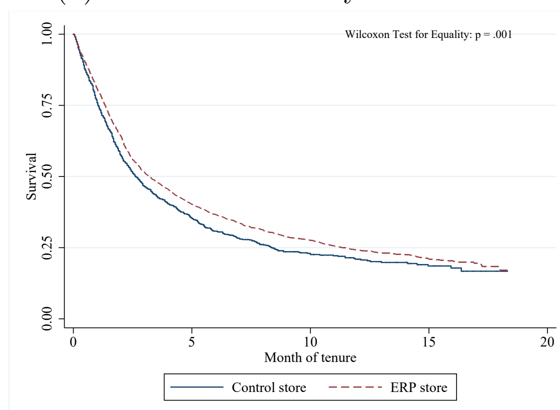
(a) Referrals vs. Non-referrals



(b) Referral Survival by Bonus Level



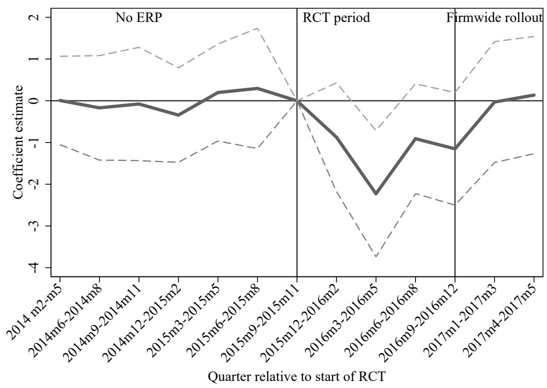
(c) Survival of Non-Referrals by Bonus Group



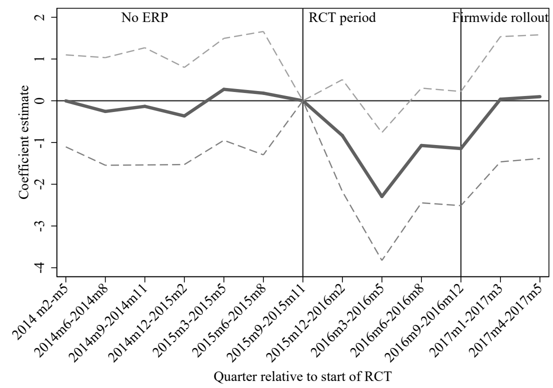
(d) Survival of Non-Referrals, by ERP Stores vs. Control

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 is 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 the end of our personnel data in May 2017. To show survival curves with granularity, spells are shown based on day of hire.

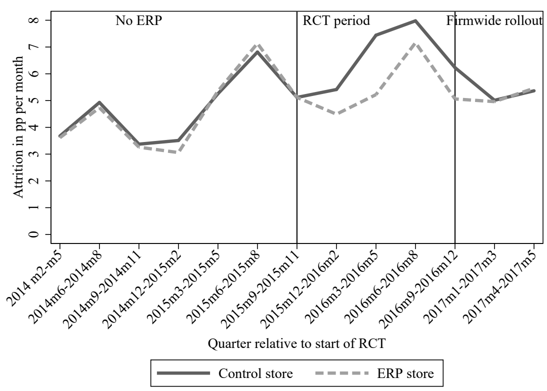
Figure 4: Event Studies: Having an ERP Lowers Attrition During the RCT, and the Effect is Reversed Once an ERP is Rolled out to Control Stores. ERP Effects are Similar in Stores with No Referrals During the RCT.



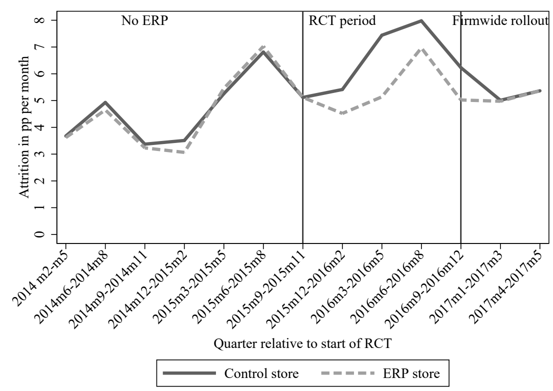
(a) ERP Impact in All Stores



(b) ERP Impact in Stores with No Referrals



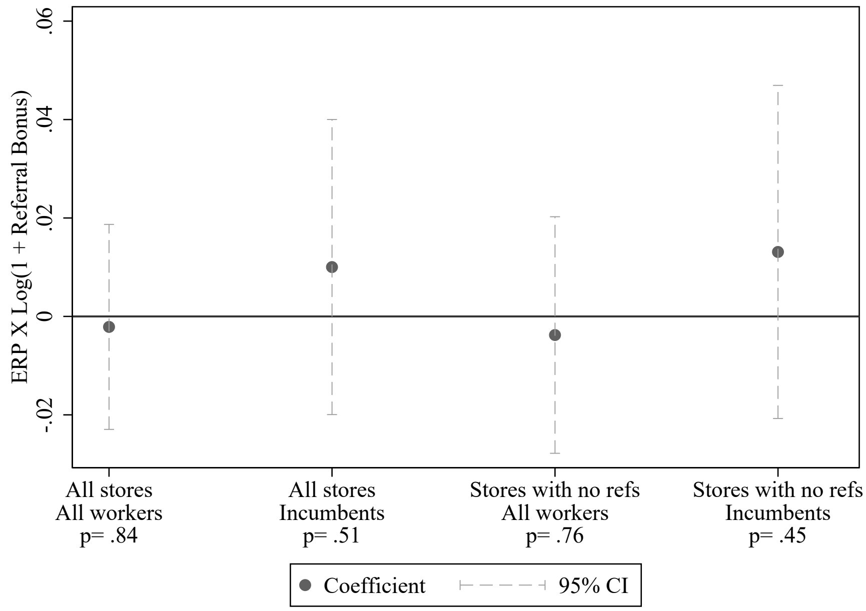
(c) ERP vs. Control in All Stores



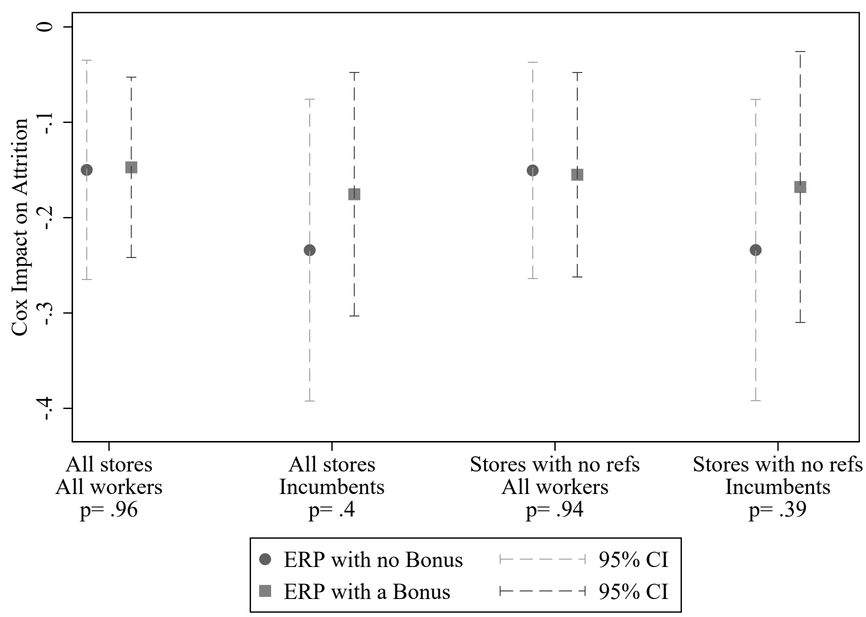
(d) ERP vs. Control, Stores with No Refs

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 equation used to plot the event study appears in equation (6). Controls are the same as in columns 3-4 of Table 5 except tenure controls here are a probation period dummy, plus linear terms in tenure on both sides of 3 months. 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 quarter-year dummies (where the quarter before the RCT, 2015m9-2015m11, is the omitted category), as well as store dummies, tenure controls (probation period dummy, plus linear terms in tenure on both sides of 3 months), 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 quarter-year 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 periods 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: The Impact of ERPs on Employee Attrition Does Not Significantly Vary with the Level of the Referral Bonus (Cox Models)



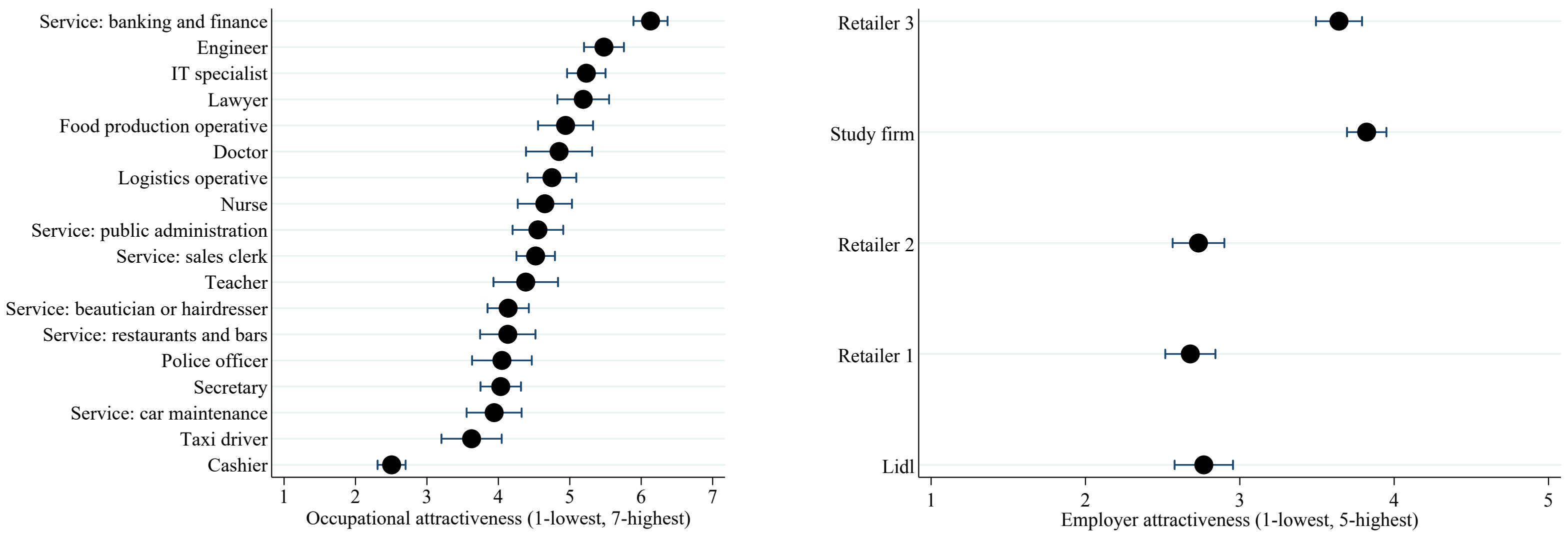
(a) The Interaction Term, ERP X Log(1+Bonus), is Indistinguishable From Zero



(b) No Significant Difference Between the No Bonus and Positive Bonus ERPs

Notes: Using Cox models, this figure shows how the impact of ERPs on attrition varies with the level of the referral bonus. **Panel (a)** estimates models from equation (7). Controls are the same as in Table 5. The first and second comparisons are based on columns 1 and 7 of Table 5, respectively. The third and fourth comparisons are similar to the first and second, but restrict to stores where there were no referrals made during the RCT; they are based on columns 1 and 7 of Table B6, respectively. The bonus size is 0 for control stores and R0 stores. Each p-value corresponds to the test that the interaction term equals 0. **Panel (b)** estimates models similar to those in Table 5 except we lump R50, R90, and R120 together to create a single dummy for an ERP treatment paying a positive bonus. Thus, we have two key regressors: ERP with no Bonus (i.e., the R0 dummy) and ERP with a Bonus (i.e., the sum of the dummies, R50+R90+R120). Each p-value corresponds to the test that the two key coefficients are equal.

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



(a) Occupations (b) Grocery Chains

Main notes: Dots are mean attractiveness ratings, and whiskers are 95% confidence intervals for the means. The data are from our *General Population Surveys 1 and 2* in early 2017 and mid 2018. 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, respondents are asked about 6 occupations from our overall list of 18 occupations. Each respondent sees the cashier occupation plus 5 other occupations. In the survey data in panel (b), each respondent is asked about all 5 grocery retailers (our study firm, 3 other local retailers, and the German chain Lidl).

Additional notes on Panel A: While Panel (a) of Figure 6 accords with many aspects of occupational prestige in the US, there are differences: for example, doctors and 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.

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, All at the Monthly Level								
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 (x100)	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 (x100)	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 (x100)	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	7.44***	0.00	-0.02	-0.02	-0.01	0.99	-0.01	0.68
per worker)	(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	7.85***	-0.26	-0.44	0.06	0.06	0.79	-0.15	0.69
a store's municipality	(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	77.76***	1.87	2.26	1.02	1.54	0.62	1.68	0.16
who are cashiers	(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 corresponds to the test for joint significance of the treatment dummies. Columns 7-8 lump all treatment stores together and compare 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. The attrition rate, quit rate, and fire rate are the average monthly rates, multiplied by 100 for ease of presentation. The division of attrition into quits and fires is only available starting in 2015m4, whereas the attrition data go back to 2014m2. The randomization was not stratified, but as noted in footnote 17, 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: Panel A summarizes the treatments. Panel B compares means across treatment arms in the number of referrals made and hired. 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)

Dependent variable:	Hire is a Referral (0 or 1)			Total Referrals Hired in RCT		
What is an observation:	A Hire			A Store		
	(1)	(2)	(3)	(4)	(5)	(6)
R0	-0.004*	-0.001		-0.065*	-0.079	
	(0.002)	(0.005)		(0.037)	(0.107)	
	[0.112]	[0.894]		[0.062]	[0.481]	
R50	0.017**	0.022**		0.268**	0.340**	
	(0.008)	(0.009)		(0.126)	(0.163)	
	[0.028]	[0.011]		[0.028]	[0.035]	
R90	0.034**	0.037***		0.497**	0.506**	
	(0.014)	(0.013)		(0.220)	(0.223)	
	[0.017]	[0.008]		[0.015]	[0.017]	
R120	0.042***	0.040***		0.747**	0.715***	
	(0.014)	(0.010)		(0.300)	(0.258)	
	[0.001]	[0.000]		[0.002]	[0.001]	
ERP			0.024***			0.371***
			(0.006)			(0.133)
			[0.000]			[0.007]
Observations	3,811	3,811	3,811	238	238	238
Controls	No	Yes	Yes	No	Yes	Yes

Notes: This table presents OLS regressions, with standard errors clustered at the store level in parentheses. "Rand-t" randomization inference p-values following Young (2019) are in square brackets (1,000 replications). In columns 1-3, 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), quarter-year 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 columns 4-6, an observation is a store and the dependent variable is the number of referrals made. The controls are the store-level controls from columns 1-3. Stars are based on the clustered standard errors in parentheses, with * significant at 10%; ** significant at 5%; *** significant at 1%

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

Dep. var.:	Attrition			Monthly absences		
Method:	Cox Prop. Hazard Model			Negative Binomial		
	(1)	(2)	(3)	(4)	(5)	(6)
Hire was referred	-0.59***			-0.22		
	(0.15)			(0.19)		
Referred X first 5m		-0.66***			-0.45*	
		(0.18)			(0.25)	
Referred X after 5m		-0.33			0.22	
		(0.25)			(0.38)	
Referred X R50			-1.87***			0.14
			(0.55)			(0.38)
Referred X R90			-0.52***			-0.25
			(0.18)			(0.37)
Referred X R120			-0.43**			-0.39*
			(0.21)			(0.20)
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	3796	3796	3796	3796	3796	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)			0.02			0.46
F(R50 vs. R120)			0.01			0.22

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 Cox proportional hazard models, where the failure event is whether an employee attrites in a given month, and with coefficients shown (instead of odds ratios). 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 we include current month-year dummies and controls for worker tenure. In columns 1-3, tenure is controlled for nonparametrically via the Cox model. In columns 4-6, tenure controls are a probation period dummy, plus linear terms in tenure on both sides of 3 months. Column 5 also has a dummy for "after 5m of tenure." In columns 3 and 6, the excluded category is non-referred, but we do not include a "Referred X R0" dummy because there are 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. In columns 1 and 3, the mean dependent variable for non-referrals is 15.91, indicating that average worker attrition is roughly 16% per month in our sample of new workers. In column 4, the estimated overdispersion parameter is $\alpha = 23.8$ (s.e.=0.97). This indicates sizable overdispersion and that negative binomial is more appropriate than Poisson. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: The Impact of the ERPs on Attrition (Cox Prop. Hazard 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	-0.15***		-0.16**		-0.06		-0.24***	
	(0.06)		(0.08)		(0.07)		(0.08)	
	[0.012]		[0.057]		[0.399]		[0.003]	
R50	-0.07		-0.17**		-0.07		-0.09	
	(0.06)		(0.08)		(0.08)		(0.09)	
	[0.280]		[0.041]		[0.360]		[0.320]	
R90	-0.25***		-0.30***		-0.19***		-0.30***	
	(0.06)		(0.07)		(0.07)		(0.08)	
	[0.000]		[0.000]		[0.007]		[0.001]	
R120	-0.13**		-0.17**		-0.12		-0.14*	
	(0.06)		(0.07)		(0.07)		(0.08)	
	[0.033]		[0.014]		[0.103]		[0.073]	
ERP		-0.15***		-0.20***		-0.11*		-0.19***
		(0.05)		(0.06)		(0.06)		(0.06)
		[0.002]		[0.003]		[0.067]		[0.002]
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: An observation is a worker-month. All columns are Cox models, where the failure event is whether an employee attrites in a given month. Coefficients are reported, with standard errors clustered by store in parentheses. "Rand-t" randomization inference p-values following Young (2019) are in square brackets (1,000 replications). Controls are the same as in column 1 of Table 4 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 (age is thus missing for 40% of worker-months during the RCT), and this missingness is highly correlated with attrition. Also, to ensure convergence of the likelihood, for workers hired in 2003-2012, we lump quarter-year of hire dummies into year of hire dummies, and we include one dummy for workers hired in 2002 or before. 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 2015m12-2016m12. 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 17. 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?

Sample: Individuals in the Survey:	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%	13%
“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 Regarding 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”		18%
“Both were equally likely”		67%

Notes: These data are 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%). The manager response rate was 93%, with 222 of 238 managers responding. The percentages reported in this table are based on the 207 managers who provided an answer to the key survey question on mechanisms. (For the 15 managers who did not provide an answer to this question, e.g., because they did not wish to try to explain worker behavior, we can still make use of their responses on other questions.) Workers did the survey via an electronic kiosk at work and could only select one option. 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. Panel B is based on asking respondents to parse further into the first reason. Specifically, workers are asked, which of the following reasons is more likely to be the main reason for the lower turnover rate. The answers were “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” 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	2.49	11.44
Contribution to savings from:		
Referrals hired during RCT	0.12	0.57
Non-referral hires during RCT	0.77	3.53
Pre-RCT incumbents	1.59	7.33
Costs of the ERP	0.10	0.10
Profit per worker-month	2.39	11.34
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	2.52	1.26	3.94	2.12
Contribution to savings from:				
Referrals hired during RCT	0	.19	.15	.18
Non-referral hires during RCT	.45	.44	1.26	.94
Pre-RCT incumbents	2.08	.63	2.53	1.01
Costs of the ERP	0	.05	.11	.22
Profit per worker-month	2.52	1.21	3.83	1.90

Panel C: Profits from Rollout ERP vs. Control		
Turnover cost number:	€250	€1,150
Total savings in turnover costs	2.73	12.54
Contribution to savings from referrals hired during rollout	0.37	1.68
% of savings from referrals hired during the rollout	13%	13%
Costs of the rollout ERP	0.54	0.54
Profit per worker-month	2.18	12.00

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, Local Unemployment, and Worker Characteristics: Direct and Overall Effects of Having an ERP

Characteristic: (first 3 are normalized)	Store performance index	Attri-tion rate	Unemploy-ment rate	Male	Cashier
	(1)	(2)	(3)	(4)	(5)
Panel A: Direct Effects. OLS Models, DV = Hire is a Referral (x100).					
ERP	2.32***	2.39***	2.39***	2.67***	1.62
	(0.60)	(0.61)	(0.61)	(0.70)	(1.62)
ERP X Characteristic	0.50	0.31	-0.94	-0.89	0.90
	(0.47)	(0.49)	(0.60)	(1.12)	(1.52)
Westfall-Young p-vals	{0.51}	{0.53}	{0.33}	{0.68}	{0.68}
Panel B: Overall Effects. Cox Models, DV = Worker Attrites in a Month.					
ERP	-0.13***	-0.15***	-0.15***	-0.10*	-0.26**
	(0.04)	(0.05)	(0.04)	(0.05)	(0.13)
ERP X Characteristic	-0.10**	0.03	0.10**	-0.20**	0.13
	(0.05)	(0.03)	(0.04)	(0.10)	(0.13)
Westfall-Young p-vals w/ linear probability model	{0.07}	{0.84}	{0.07}	{0.04}	{0.93}
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 Store Characteristics					
10th Percentile Stores	1.1%	0.6%	1.1%	Not applicable	
90th Percentile Stores	0.1%	0.6%	0.2%	Not applicable	
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	4.9%	2.9%	5.2%	Not applicable	
90th Percentile Stores	0.3%	2.6%	0.9%	Not applicable	

Main notes: Standard errors clustered by store are in parentheses. The store performance index is defined in Section 6.

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. An observation is a new hire during the RCT. N=3,811 except for column 4 where N=3,810.

Panel B: Each column is similar to column 2 of Table 5, with the difference that we add two regressors: ERP X Char and Char. An observation is a worker-month in the RCT among grocery workers. N=74,188 except for column 4 where N=74,174.

Panels C-D: We present the profit gains from having an ERP as a share of firm labor costs, similar to Table 7. The difference is we 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 assumes a turnover cost of €1,150 (i.e., the full cost of turnover).

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 using a clustered bootstrap. In both Panels A and B, there are two families of tested hypotheses, one for store-level characteristics (in columns 1-3) and one for worker-level characteristics (in columns 4-5). The family of tested hypotheses is the set of 3 or 2 tests about whether the coefficient on ERP X Characteristic equals 0 for the 3 or 2 displayed characteristics. For the Cox proportional hazard models in Panel B, the Westfall-Young p-values are based on linear probability models with the same controls as the Cox models except tenure is controlled for as in Figure 4 instead of nonparametrically. We use a linear probability model here because Cox is not supported by the 'wyoung.ado' Stata package (Jones *et al.*, 2019) that we use for Westfall-Young. As discussed in footnote 23, attrition results are very similar with Cox and OLS. 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: 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%	13%
Cashier	4%	12%
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.

Web Appendix, "What Do Employee Referral Programs Do? Measuring the Direct and Overall Effects of a Management Practice", by Friebel, Heinz, Hoffman, and Zubanov

Appendix A provides additional discussion and results. For each subsection, we give the relevant section of the main paper that it accompanies. Appendix B contains additional figures and tables. Appendix C is the Data Appendix. Appendix D presents a model accompanying Section 1. Appendix E provides materials used by the firm in the ERPs.

Appendix A Additional Discussion and Results

A.1 World Management Survey (Accompanying the Introduction)

The WMS (Bloom *et al.*, 2014) has traditionally focused on manufacturing. However, in 2009, the WMS surveyed 661 retail establishments. Data on ERP status is available for 537 establishments. Like other WMS surveys, phone interviews were conducted using open-ended questions (Bloom *et al.* (2014) give details). Of the 537 establishments, 352 are in Canada, 126 are in US, and 59 are in UK. Unlike other WMS surveys, which do not ask about ERPs, enumerators explicitly asked managers about whether the establishment had an ERP. The share of establishments with an ERP is 25% in Canada, 15% in the US, and 32% in UK. In many cases, respondents mentioned a bonus, but we currently do not have data on whether the ERP used bonuses. One reason why the WMS rate of referrals is lower than that in CareerBuilder is that the question is asked at the establishment level. A retail firm may decide to have an ERP, but some local managers may choose not to apply it.

A.2 RCTs on Hiring Procedures and Referral Programs (Intro)

RCTs related to firm hiring procedures. As far as we are aware, ours is the first, large-scale, within-the-firm RCT on any hiring procedure. As mentioned in footnote 5 in the main text, development economics RCTs have randomized selection procedures in government (see Ashraf *et al.* (2020) for a prominent example) or NGOs (e.g., Deserranno, 2019), but not in a private firm. Thus, this work cannot examine impacts on profits, and the signaling role of hiring procedures like ERPs may differ when chosen by a profit-maximizing firm compared to when chosen by a government or NGO. Beyond audit studies, there are also RCTs in online labor platforms which change features of worker-firm matching, but these analyze the impact of platform features (i.e., features of the entire market) as opposed to randomizing an individual firm's hiring procedures, the type of research called for by Oyer & Schaefer (2011). Also, studies use RCTs to vary some feature of an organization (e.g., pay structure) and see how that affects the quality or quantity of applicants. Such studies examine who gets hired, but do not study the impact of a hiring procedure.

RCTs on referral programs. As noted in the main text, papers randomize referral programs in non-inside-the-firm contexts to study different questions from ours. For example, on customer referrals, beyond Kumar *et al.* (2010), work studies how different bonuses (Ahrens *et al.*, 2013) or access to premium services (Belo & Li, 2018) motivate e-referrals for online platforms. Beaman & Magruder (2012), Bryan *et al.* (2015), and Fafchamps *et al.* (2020) study whether people can screen for cognitive tests, loan-paying, and agricultural training returns, respectively. Goldberg *et al.* (2019) examine low-cost peer referrals for tuberculosis.

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

We compare our RCT bonuses to those paid in other studies. In our RCT, workers could earn up to 40% of monthly salary for making a referral, and we also paid well in expected value terms taking into account that referrer and referral had to stay 5 months post-referral.[1] In their study in a financial firm, Brown *et al.* (2016) report a modal referral bonus of $1,000 (median of $2,000), which is about 1% of annual salary at the firm (or 12% of the monthly salary), which is similar to our bonuses in expected value, and lower than the maximum value of our bonuses.[2] Our nominal bonuses are similar in percentage terms to the bonuses at the trucking firm in Burks *et al.* (2015), where drivers got $1,000 (or about 1/3 of monthly salary) for referring an experienced driver, though there was also a 6-month tenure requirement.

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

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

1. *Pre-RCT Survey of Non-grocery Employees*: In Oct.-Nov. 2015, we surveyed 120 food production workers at the firm about how much money would make them willing to make an employee referral for a hypothetical vacancy in their unit. These responses were used to choose the bonus levels for the RCT, as noted in Section 2. The response rate was 100%.
2. *During RCT Survey of Grocery Store Managers and Employees*: In September-October 2016, we conducted phone surveys of store managers recording their time use and their opinions regarding why the RCT ERPs were generating only a modest number of referrals. The response rate was 92%. In October-December 2016, we conducted phone surveys of cashiers. For each store, we randomly called two cashiers, one with an above- and the other below-median tenure. All participated. We asked the same broad questions as in the manager survey about why referrals were few. These surveys are analyzed in the main text in Section 7, with results in Table A6.[3]
3. *Post-RCT Survey of Grocery Store Managers and Employees*: In summer and fall 2018, we conducted phone surveys of store managers regarding the mechanism for the observed indirect impact of ERPs on attrition. In fall 2018, we conducted similar surveys for cashiers, but via in-store electronic kiosk.[4] These surveys are analyzed in the main text in Section 4.2.3, with results in Table 6.

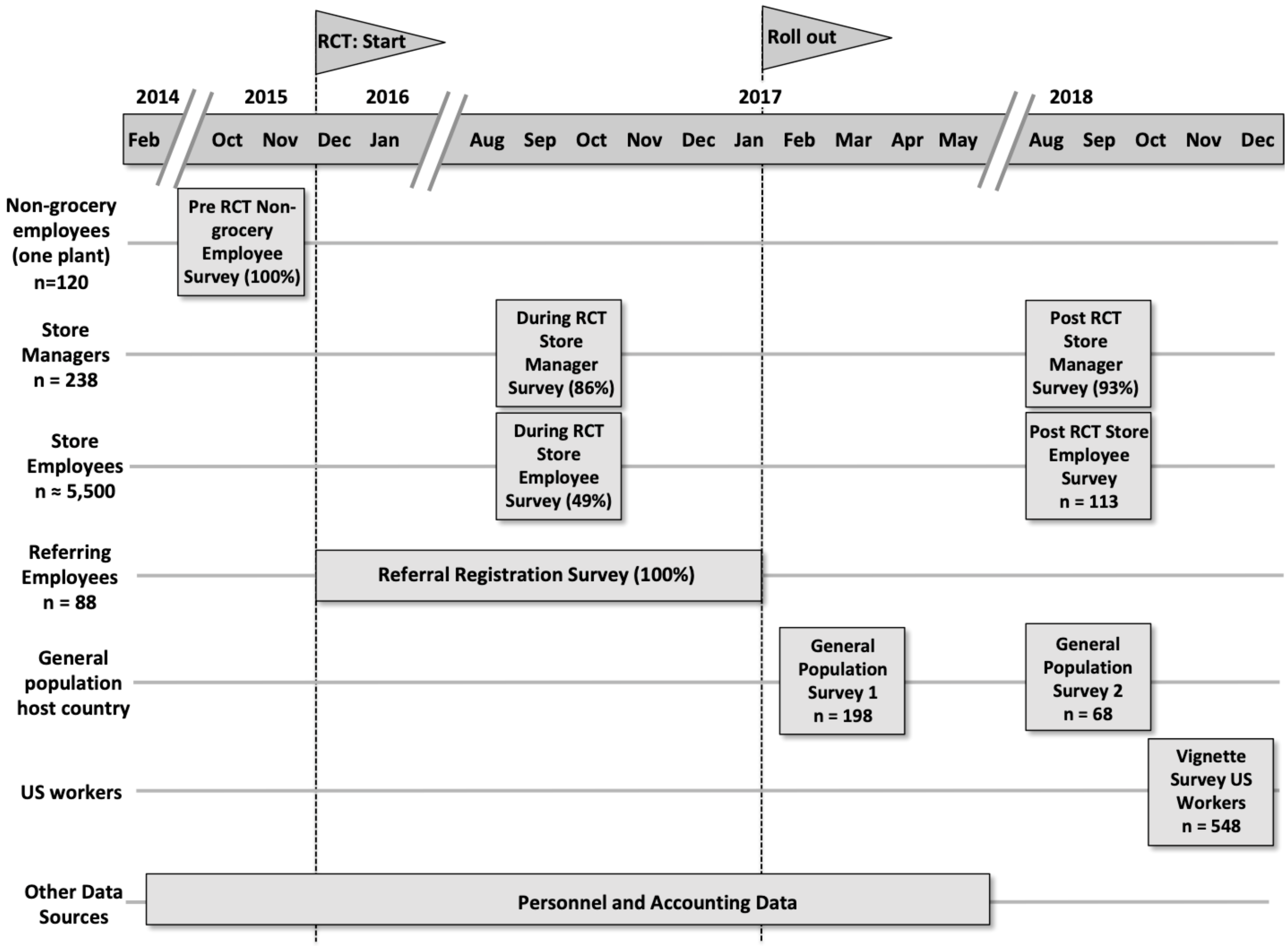
[1] The referral bonus was paid for 45% of referral hires. Thus, the expected value of bonuses was €37.5, €55.5 and €69 in R50, R90, and R120, equivalent to 13-23% of monthly salary, i.e., up to a week of salary.

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

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

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

Figure A1: Datasets Used in the Paper



Notes: The *Pre-RCT survey of non-grocery employees* is discussed in Section 2 in describing how we selected the level of the referral bonuses for the RCT. The *During RCT surveys of managers and employees* are analyzed in Section 7, with results in Table A6 (Panel A covers managers, whereas Panel B covers employees). These help gain insight on why the RCT ERPs generated only a modest number of referrals. The *Post-RCT surveys of managers and employees* are analyzed in Section 4.2.3, with results in Table 6. These help gain insight on the mechanism for the indirect effects of ERPs on employee attrition. The *Post-RCT survey of employees* was conducted by the firm using store kiosks, so we do not know how many workers saw the survey on the kiosks (or know the share of workers who agreed to participate conditional on seeing the surveys). The *Referral Registration Survey* provides information on who referred whom and is used throughout the paper. The very brief survey is conducted by the firm as part of the referral process. Starting in January 2017, we only have information on who made referrals, not on who is referred. *General Population Survey 1* is analyzed in Section 7, with results in Figure 6. *General Population Survey 2* is analyzed in Section 7, with results in Figure 6 and Panel A of Table A6. The *Vignette Survey of US Workers* is discussed in Sections 4.2.3, 7, and 8. It provides further evidence regarding the mechanism for ERP impacts on attrition, and also allows us to examine results across both lower-skill and higher-skill workers. In addition, it provides further evidence supporting the first part of Prediction 5, i.e., that workers should be more willing to make referrals for more attractive jobs. Appendix A.10 provides details on the *Vignette Survey*.

In all the firm surveys, subjects were told truthfully that we were conducting an international retail survey in partnership with a local university. In addition, subjects were told truthfully that their employer would not see individual-level responses to the survey. Phone surveys were conducted by native speakers.[5]

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

- *General Population Survey 1*: Conducted in early 2017, this survey collected opinions regarding the attractiveness of different occupations and retail firms. This survey is analyzed in the main text in Section 7, with results in Figure 6.
- *General Population Survey 2*: Conducted in August-September of 2018, this survey continued to collect more data on the attractiveness of different occupations and retail firms. In addition, it explained to subjects that a grocery store firm had instituted an ERP, and that few referrals had been made for grocery jobs, whereas many referrals were made for non-grocery jobs. Subjects were then asked why they thought this was. This survey is analyzed in the main text in Section 7, with results in Figure 6 and Panel A of Table A6.

Finally, we also ran a *Vignette Survey of US Workers* described below in Section A.10.

A.5 Who Makes Referrals? (Section 3.1)

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

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

A.6 Multiple Hypothesis Testing (Sections 4 and 6)

As discussed in Section 3.2, we pre-registered two main outcome variables: (1) attrition (primary outcome) and (2) absence (secondary outcome). As we examine both outcomes

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

simultaneously, we account for multiple hypothesis testing by calculating family-wise error rate adjusted p-values based on the Westfall & Young (1993) free step-down procedure. For comparison, we also show Bonferroni-corrected adjusted p-values, as well as conventional clustered by store p-values. As seen in Table A1, with the exception of restricting the analysis to new hires during the RCT, the adjusted p-values indicate a statistically significant effect of having an ERP on attrition. In each column, the family of hypotheses has two hypotheses, one for attrition and one for absence.

Table A1: Accounting for Multiple Hypothesis Testing in Table 5.
Dep. Var. = Attrition, OLS Models, With Coefficients Multiplied by 100

Type of workers:	All	All	Hires	Inc
Sample period:	RCT	Pre &RCT	RCT	RCT
Analogous Column from Table 5:	(2)	(4)	(6)	(8)
ERP	-0.99***	-1.29***	-1.77*	-0.81***
	(0.32)	(0.39)	(1.00)	(0.27)
Conventional clustered p-vals	{0.002}	{0.001}	{0.077}	{0.003}
Westfall-Young p-vals	{0.012}	{0.005}	{0.200}	{0.012}
Bonferroni p-vals	{0.004}	{0.002}	{0.153}	{0.006}

Notes: This table shows family-wise error rate adjusted p-values based on the Westfall & Young (1993) free step-down procedure (5,000 replications) for our analysis of how having an ERP affects attrition. We implement OLS versions of the Cox models in the even columns of Table 5, replacing the non-parametric Cox tenure controls with the parametric tenure controls described after equation (3). We use a linear probability model here because Cox is not supported by the 'wyoung.ado' Stata package (Jones *et al.*, 2019) that we use for Westfall-Young. In each column, the family of hypotheses includes one for attrition and one for absence. The Westfall-Young p-vals account for clustering by store by using a clustered bootstrap. For brevity of presentation, we do not show the absence results here, as there is no statistically significant impact of having an ERP on absence either under conventional clustered by store inference (Table B3) or using Westfall-Young p-values. Stars are based on the conventional clustered-by-store standard errors in parentheses, with * significant at 10%; ** significant at 5%; *** significant at 1%

A.7 Mediation Analysis (Section 4.2.1)

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

$$M_{it} = \alpha_0 + \alpha_1 ERP_i + X_{it}\delta_2 + u_{it} \quad (8)$$

$$y_{it} = \beta_0 + \beta_1 ERP_i + \gamma M_{it} + X_{it}\delta_2 + v_{it} \quad (9)$$

Here, y_{it} is an outcome of person i in month t (namely, whether i exits the firm during t); M_{it} are the mediator variables, namely whether someone is referred or someone's referrals made to date; ERP_i is a dummy for having an ERP in one's store; X_{it} are controls; and u_{it} and v_{it} are errors. A key goal in the mediation analysis is to estimate β_1 and γ. The mediator effect is $\alpha_1 * \gamma$, whereas the non-referral effect of the ERP is β_1. Imai *et al.* (2010b) show that OLS produces consistent estimates under Assumption 1 below.

Assumption 1

$$y_{it}(e', m), M_{it}(e) \perp\!\!\!\perp ERP_i | \ X_{it} \tag{10}$$
$$y_{it}(e', m) \perp\!\!\!\perp M_{it}(e) | \ X_{it} \tag{11}$$
for any treatment $e, e' \in \{0, 1\}$, for any mediator m, and for any controls X

where $y_{it}(e', m)$ is the potential outcome for worker i in month t under treatment e'; and mediator m and $M_{it}(e)$ is the potential mediator under treatment e. Equation (10) of Assumption 1 will hold because of random assignment. Equation (11), i.e., that potential referral status is independent of potential duration conditional on observables, is much less obvious.[6] For example, a person who is likely to be referred under an ERP may have other positive unobservables relative to someone unlikely to be referred. Given past research suggesting that referrals are positively selected (Brown *et al.*, 2016; Burks *et al.*, 2015), we hypothesize that any bias would be toward biasing upward the estimate of γ. That is, any bias would seem to work against our conclusion that referrals are not a main driver of the ERP effect, making our qualitative conclusion even stronger.

Table A3 shows results. Columns 1-2 show the impact of having an ERP on being referred and referrals made to date using the full panel data. Columns 3-5 shows the impact of having an ERP as the mediators are gradually controlled for. The coefficient only falls in magnitude from -0.99 to -0.95. The estimates imply that only 5% of the impact of having an ERP on attrition is mediated via getting more referrals and having more referrals to date, whereas 95% remains unexplained. Column 5 shows that having made referrals so far to date does not significantly predict whether a person will attrite. Column 6 separate referrals made to date into those made in the last 5 months vs. those not made in the last 5 months. For each referral made in the last 5 months, a person is 1.9pp less likely to attrite, consistent with referrers staying a bit longer to get a bonus.

A.8 Manager Time Use (Section 4.2.2)

During the RCT in fall 2016, store managers were asked about the share of time during the preceding few months that they spent on four time use categories: goods/products, customers, administration, and human resources. We have time use data for store managers in 199 of the 238 stores. To assess whether manager time use is affected by having an ERP, we regress normalized time use for each category on a dummy for having an ERP. As seen in Table A2, there is no impact of an ERP on time use.

[6]Equation (11) would hold if the mediator were directly randomized (Imai *et al.*, 2010b), but one cannot force someone to be a referral hire or to make referrals. We experimented with estimating Equation (9) while instrumenting the mediator (either whether someone is referred or makes referrals) using the level of the referral bonus. Doing so had little impact on β_1 compared to OLS, but produced a large standard error for γ (despite having a strong first stage). Because of this imprecision with IV, we stick with OLS.

Table A2: No Impact of Having an ERP on Normalized Manager Time Use (N=199)

Time spent on:	Goods		Customers		Administration		HR	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ERP	-0.10	-0.11	-0.18	-0.13	0.19	0.20	0.02	-0.00
	(0.17)	(0.17)	(0.19)	(0.20)	(0.16)	(0.17)	(0.18)	(0.18)
Store controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors in parentheses. An observation is a store manager. The dependent variable is normalized time use on each of the 4 categories. Controls are the store-level controls listed in Table 3. HR is based on two separate questions added together (one on managing people and one on dealing with turnover), but we also see a null effect of ERPs if both questions are analyzed separately. In a smaller, unrelated phone survey with 129 store managers in Jan. 2016, there is also no effect of ERPs on whether managers report having recently increased effort to reduce turnover.

Table A3: Mediation Analysis for Impact of ERPs on Attrition

Dep. Var.:	Referred (0-1)	Refs made to date	Attrition (0-1) x 100						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ERP	0.007***	0.009***	-0.99***	-0.95***	-0.95***	-0.95***			
	(0.001)	(0.002)	(0.32)	(0.32)	(0.32)	(0.32)			
Hire was referred				-6.21***	-6.21***	-6.18***		-6.18***	-6.18***
				(1.40)	(1.39)	(1.40)		(1.42)	(1.42)
Refs made in last 5m in RCT						-1.88**			
						(0.93)			
Refs made to date not in last 5m in RCT						1.69			
						(1.77)			
Refs made to date during RCT					-0.65				-0.62
					(0.90)				(0.91)
R0							-1.01***	-1.01***	-1.01***
							(0.38)	(0.38)	(0.38)
R50							-0.50	-0.46	-0.45
							(0.43)	(0.43)	(0.43)
R90							-1.58***	-1.51***	-1.51***
							(0.36)	(0.37)	(0.37)
R120							-0.85**	-0.79*	-0.78*
							(0.41)	(0.40)	(0.40)
Observations	74,188	74,188	74,188	74,188	74,188	74,188	74,188	74,188	74,188
Mean DV if ERP=0	4.8e-4	0	6.677	6.677	6.677	6.677	6.677	6.677	6.677
Workers	10,003	10,003	10,003	10,003	10,003	10,003	10,003	10,003	10,003

Notes: Standard errors clustered by store are in parentheses. All columns show OLS models. The controls are the same as in Table 5 except that we use the tenure controls employed in columns 4-6 of Table 4. The sample is workers at the firm during the RCT. "Refs made to date" means a person's running sum of referrals made to date during the RCT. "Refs made in last 5m" is a person's running sum of referrals during the last 5 months. "Refs made not in last 5m" is a person's running of sum of referrals made during the RCT while excluding referrals made during the previous 5 months. * significant at 10%; ** significant at 5%; *** significant at 1%

A.9 Discussion from Practitioners and Sociology (Section 4.2.2)

Our data show that ERPs reduce attrition separate from generating referrals, and our data and surveys suggest this is due to workers valuing being involved in hiring. This mechanism is highly consistent with two key points raised by business practitioners. First, practitioners state specifically that ERPs make workers feel more involved in the hiring process. One recruiting website argues that ERPs "help increase attachment to the organization and make employees feel as though they have a stake in the future of the business. Employees want to grow, so having a hand in the company's forward motion is exactly what they're looking for." Another recruiting website argues that ERPs make "current employees feel trusted and valued since they are participating in the company's future and growth."[7]

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

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

A.10 Vignette Survey of US Workers (Sections 4.2.3, 7, and 8)

Vignettes have a long tradition in economics (Kahneman *et al.*, 1986). Kaur (2019) is a recent example using a vignette to identify mechanisms.

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

Table A4: Comparing *Vignette Survey* Participants to the CPS

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

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

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

Table A4 compares characteristics of survey participants to the 2018 March CPS. Compared to the CPS, participants in the survey are older, whiter, and more educated, but our survey still contains a broad mix of workers of different skills.

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

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

Section 8 reports a comparison of workers with a bachelor's degree or higher versus workers with less than a bachelor's in terms of whether they believe that having an ERP would make an employee feel more respected, defined as a dummy for one of categories 5-7 above (i.e., somewhat likely, likely, or very likely). We regress whether an employee would feel more respected on a dummy for having a bachelor's or higher with robust standard errors. Of course, the purpose of this regression is not to establish a causal relation between education and survey answers.[8] Rather, this shows that believing ERPs increase workers' feeling of being respected may be even more prevalent among higher-skilled than lower-skilled workers.

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

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

We combined answers to this question with questions where we asked *How attractive is your current job?* and *How attractive is your current employer?* on a scale from 1-7. As seen in Table A5 below, a 1σ increase in job attractiveness increases the chance that someone would be willing to make a referral (defined as response 5-7 to the above vignette) by about 20pp. A 1σ increase in firm attractiveness increases referral willingness by 7-8pp. These results support that people are more willing to make referrals for better jobs.

A.11 Calculation of the Costs of Turnover (Section 5)

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

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

Table A5: People who Rate their Job or Employer as More Attractive Report Being More Willing to Make a Referral (N=333 workers). From *Vignette Survey of US Workers.*

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

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

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

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

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

Total costs. Beyond administrative costs, turnover also often has consequences in terms of productivity (Blatter *et al.*, 2012; Boushey & Glynn, 2012). Turnover events can be disruptive to incumbent workers' productivity and new workers often require time to get up to full speed. Blatter *et al.* (2012) study total hiring costs (inclusive of direct costs and lost productivity) for different types of firms and jobs using rich data from Switzerland. For large firms like ours (i.e., for firms with 100+ employees), Blatter *et al.* (2012) estimate average hiring costs to be 17 weeks of salary. For the job of cashier, the average hiring cost (i.e., across firms of different sizes) is 10 weeks, and they find that hiring costs increase in skill. Because Blatter *et al.* (2012) do not report hiring costs specifically for cashiers in large firms, we assume an intermediate value. To be conservative, we weight skill as more important than firm size, and assume a hiring cost of 12.5 weeks, which translates into a monetary value of €1,150.[9,10] Since Blatter *et al.* (2012) do not include costs that turnover may have on the firm's reputation or talent pool, our estimates may be somewhat conservative regarding long-run cost.

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

Our full turnover cost is also consistent with a recent study by Kuhn & Yu (2021) on a retail firm in China. Kuhn & Yu (2021) exploit having daily sales data, coupled with a strongly enforced two-week notification period before attrition events, to estimate the cost of turnover events using an event study approach. They estimate that turnover events cost the firm 63 days worth of worker wages. Since their workers work 6 days per week, this is equivalent to a turnover cost of 10.5 weeks of pay. This is similar to our assumed total hiring cost of 12.5 weeks.

A.12 Profit Calculation Details (Section 5)

Absences. We do not account for absence in the profit calculation, as there is no impact of ERPs on absence. In addition, the overall absence difference between referrals and non-referrals is not statistically significant.

Savings from referrals hired in RCT. We use the formula $\theta_p t_p c$. The attrition difference between referrals and non-referrals, t_p, is given by column 1 of Table 4, whereas θ_p is the share of RCT worker-weeks in ERP stores from referrals.

Savings from non-referral hires in RCT. Using $\theta_p t_p c$, here, t_p is the impact of having an ERP on attrition for non-referral hires and is estimated by running column 2 of Table 5 but restricting to non-referral hires. θ_p is the share of RCT worker-weeks in ERP stores from non-referral hires.

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

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

Pr(both). To calculate $Pr(both)$, i.e., the probability that both the referrer and referral stay 5 months, we count up the number of instances where both parties stayed five

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

[10] Rather than making an assumption based on Blatter *et al.* (2012), an alternative approach is to estimate the relationship between store-level turnover and store-level profits. This is the approach pursued by Friebel *et al.* (2018), who estimate a total turnover cost of €1,470. Our results are qualitatively similar and strengthened if we use this total cost level. Table B7 shows cross-sectionally in pre-RCT data that higher-attrition stores also have higher shrinkage, lower sales per worker, and lower operational profit per worker.

months divided by the total number of referrals. Our data extend 5 months after the RCT, so we are able to see 5 months of data post-referral for all referrals made during the RCT. We use a single number for $Pr(both)$ as opposed to letting it vary by referral bonus group.

A.12.1 Firmwide ERP Rollout and Different Jobs

In order to calculate profits under the firmwide ERP rollout starting in January 2017, we need to make some additional assumptions beyond those made in the RCT. This is for two reasons. First, the firm rolled out the new ERP (€30 upon hire, €100 after 3 months) to the entire firm at once and did not randomize. Second, as discussed in the main text in footnote 16, during the rollout (i.e., starting January 2017), we only observe data on who makes referrals, not on who is referred.

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

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

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

Total savings in turnover costs from rollout. During the RCT, the overall impact of the ERP on employee turnover did not systematically vary with the level of the referral bonus, as can be seen in the odd columns of Table 5. Thus, for the profit calculations, we assume that total turnover savings from the rollout ERP is the same as total turnover savings from the RCT ERPs, plus the incremental benefit of turnover savings from referrals hired during the rollout relative to during the RCT.[11]

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

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

A.13 Alternative Explanations for Larger Impact of ERPs in Higher-Performing Stores (Section 6)

Product selection is generally similar across stores, with the vast majority of RCT worker-months (over 90%) occurring at stores offering a full-service format. Product selection does not drive our finding, as the result in column 1 of Panel B of Table 8 is robust to restricting to full-service stores or to including interaction terms of ERP*(# of products offered) or ERP*(Share of products that are fresh goods), as seen in Table B11. Workplace technology is also similar across stores, and results are robust to controlling for an interaction of ERP with the number of store checkouts (total, manned, or self-checkout). Our performance heterogeneity is not just reflecting store size, as results are robust to controlling for ERP*(Head count) or ERP*(Store square meters). Competition from Lidl does not explain the results, as results are robust to including the interaction term ERP*(Dummy for Lidl store nearby). Demand shocks seem unlikely to account for us finding similar results on different performance measures, not only on sales and profits, but also on shrinkage, which is strongly affected by theft and thus presumably less affected by demand shocks. We are agnostic as to whether ERPs may be complementary with respect to management practices or the quality of store managers, as the two are often quite correlated Bender *et al.* (2018). Bloom *et al.* (2019) document substantial intra-firm, cross-plant variation in management quality. Based on numerous visits of the authors to firm stores, operational management practices appear relatively similar across stores, suggesting that differences in management are likely to reflect differences in HRM practices.

A.14 Predicting the Rate of Referrals in the Rollout (Section 7)

The RCT bonuses feature €15 right away and the remainder after the referrer and referral stay 5 months. This is paid out about 45% of the time in our data. The post-RCT rollout bonus features €30 paid at hire, plus an additional €100 after 3 months, and this is paid out 59% of the time.

Panel (a) of Figure B4 plots the log share of referrals to hires (y-axis) against the log expected value of the referral bonus (x-axis), assuming no discounting. The line of best fit is drawn using the 3 positive referral bonuses in the RCT, and then we see how well we can predict out-of-sample for the larger bonus in the rollout. As seen in panel (a) of Figure B4, a log-log graph does a decent job predicting the referral rate in the rollout, though the model prediction is slightly too low.

However, one difference between the RCT and rollout ERPs is that more money is paid more quickly in the rollout ERP. This is an issue, not only because of the uncertainty about getting paid, but also because some people tend to strongly prefer money now to money later for many reasons (e.g., credit constraints, present bias, etc.), perhaps especially lower-skill workers like the ones we study. One natural way to incorporate this into the perceived benefit from making a referral is to make use of $\beta - \delta$ discounting (Laibson, 1997), where β is the term of immediate present bias and δ is the standard exponential discount factor. In the context of lower-skill workers making labor supply decisions, Fang & Silverman (2009) structurally estimate that $\beta \approx 0.35$. Thus, we repeat panel (a) but assume that the value of the referral bonus is discounted with $\beta = 0.35$.[12] As seen in panel (b) of Figure B4, we get a very good fit once we allow for $\beta - \delta$ discounting. That is, if we estimate the line of best using the 3 positive referral bonuses in the RCT, the line of best fit provides an excellent prediction of referrals per hire during the rollout.

Overall, this exercise suggests that the level of referrals during the rollout is very much in line with what might be expected given the relationship between bonus size and referrals during the RCT. Of course, we have not estimated a structural model of referral behavior, so Figure B4 needs to be taken with a standard appropriate caution on non-structural out-of-sample prediction.

A.15 Using Surveys to Understand Why the Relatively Small Number of Referrals for Grocery Store Jobs (Section 7)

In the fall 2016 manager survey, we asked an open question on why ERPs had little impact on getting referrals. Undergrads in a lab in Germany classified the reasons into 10 categories. The most common explanation, given by half of managers, is that grocery store jobs are undesirable, as seen in column 1 of Panel A of Table A6 below. In column 2, the share rises to 68% if we exclude the mechanical explanation of no open jobs, the response that ERPs worked well, and instances where managers gave no reason.

Panel B of Table A6 shows that similar findings apply to workers. In the fall 2016 employee survey, we gave cashiers the six most frequently mentioned reasons from the manager survey and asked them to rank them.[13] 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.

As seen in Table A6, 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.

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 A6, 74% of respondents ascribed the difference in referral rates between grocery and non-grocery jobs to grocery jobs being undesirable.

[12] E.g., the value of the rollout bonus is 30 + β*Pr(Both Stay)*100 = 30 + .35*.59*100. For simplicity, we assume $\delta = 1$. Fang & Silverman (2009) structurally estimate an annual $\delta \approx 0.9$. We get a very similar picture if we assume $\delta = 0.9$, which is to be expected given the tenure requirement for the second part of the bonus is only 3 months (post-RCT) or 5 months (during the RCT).

[13] 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.

Table A6: Manager and Employee Surveys: Why Did the ERPs Generate Only a Few Referrals? General Population Survey: Why Fewer Referrals from Cashiers than from Logistics and Food Production Workers?

Panel A: Managers & General Population

Reason	All managers (N=202 managers in survey)	All managers except those giving reasons 8, 9, 11	General population (N=68 people in the survey)
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 workers in survey)	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%	31%	29%	6%	11%
"Employees don't want to want to be responsible if their friend doesn't do a good job"	16%	23%	36%	17%	9%
"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%	20%
"The amount of money that employees could get for a bonus was too low"	7%	6%	6%	21%	60%

Main 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*).

Panel A notes: 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. There are 202 managers in the survey, but responses here are based on 156 managers since managers in Control stores were not asked about ERPs. 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.

Panel B Notes: 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. There are 342 workers in the survey, but responses here are based on 274 workers since workers in Control stores were not asked about ERPs.

A.15.1 Reasons Other than Job Attractiveness for the Relatively Small Number of Referrals Made for Grocery Store Jobs

Were employees unaware of the ERPs? The firm took many steps to ensure that the ERPs would be well-understood and well-publicized to workers. This included the letters and posters described in Section 2, plus phone calls to ensure that store managers publicized the ERPs, plus guidance to regional managers to ensure that store managers were compliant. Also, in the fall 2016 survey, we asked workers if they were aware that the firm welcomed referrals, and 87% said yes in treatment stores. This indicates persistent awareness of the ERPs even though many workers attrited during the RCT. Further, in Panel B of Table A6, the explanation of employees not being aware of the ERP / not knowing how it worked shows quite limited support. A related issue would be if people forgot about the ERPs after a few months. In such a scenario, some referrals would be made after the ERPs were introduced, but effects would peter out over time. However, Figure 2 shows that this is not the case.[14]

Did workers not have friends looking for jobs? If employees do not have friends to refer, then an ERP may have little impact on referrals. However, we believe that this explanation is unlikely to explain our results for three reasons. First, during 2016, the unemployment rate was roughly 8% (and much higher for youth who make up a sizable share of the firm's workforce), so there was a significant share of people who were unemployed. Second, in the *During RCT Surveys of Store Managers and Employees* listed in Table A6, not having friends to refer received much less support than grocery jobs being undesirable as an explanation for the result. For example, while 48% of managers mentioned grocery jobs being undesirable as an explanation, only 10% mentioned employees not having friends to refer. Third, the firm has operations throughout the country where it is located, in both urban and rural areas. Even if someone moved or had contacts living elsewhere in the country, those contacts could have found a job at a local facility.

Was the referral process difficult? Store employees could have perceived it as burdensome to call the HR department to register a referral. We do not think this is a strong explanation because the process was designed to be very brief (just a few questions about how someone knows their referral). Store employees likely have a relatively low opportunity cost of time, given that they are willing to work for just over €2 per hour. Given the possibility of earning €135 in one treatment arm, it seems unlikely that a short phone call would be of sufficient cost to dissuade someone from making a referral.[15]

Was the expected value of the bonus too low? Given the 5-month tenure requirement, would this make the expected value of the bonus too low? In our data, the chance that both the referral and referrer stay for five months after the referral is hired is about 45%. This means that the expected bonus is roughly equal $15 + .45 * 50 =$€37.5 in the R50 treatment, €55.5 in R90, and €69 in R120. Relative to a post-tax monthly wage

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

[15]Of course, if people are highly present-biased, this could help explain why they are not willing to make referrals. We cannot rule this out, but it seems unlikely in our case.

of roughly €300 for cashiers, this still is a sizable bonus (about 13-23% of monthly salary). Though our judgment of what is a "sizable bonus" is subjective, the literature on incentives shows strong effects of bonuses of this magnitude (Bandiera *et al.*, 2011). After the RCT, the rollout ERP paid €30 at hire, plus €100 after 3 months. Thus, the expected value of the new bonus was about €90. This is larger than R120 and provides money sooner.

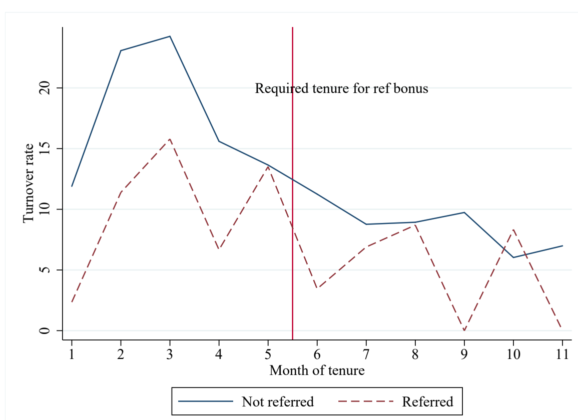
A.16 Responsiveness of Referrals to the Bonus Level Across Jobs (Section 7)

In Appendix D, our model predicts that, under a reasonable assumption, there should be greater responsiveness of referrals to bonuses for better jobs. The results of the firmwide rollout are broadly consistent with this prediction.

For non-grocery jobs, there was no ERP before the rollout. Thus, it is challenging to use the non-grocery evidence to examine whether referrals are more responsive to bonuses in good jobs than in bad jobs. Still, as far as we know, formal referrals were not made for non-grocery jobs before the rollout in Jan. 2017. Thus, one can think of our evidence as tracing out a referral responsiveness curve, where initially there were 0% referrals at a bonus of €0, and 37% referrals made per hire at the bonus of €130. Also, Table 9 shows that we can provide evidence on the prediction by separating grocery jobs into cashier and non-cashier grocery jobs (e.g., butcher, baker, assistant manager), with non-cashier jobs seen as more attractive. During the RCT, the ratio of referrals made by the group to hires was 5% for non-cashier grocery jobs compared to 3% for cashier jobs. Post-RCT, the ratio was 17% for non-cashier grocery jobs, and 11% for cashier jobs.

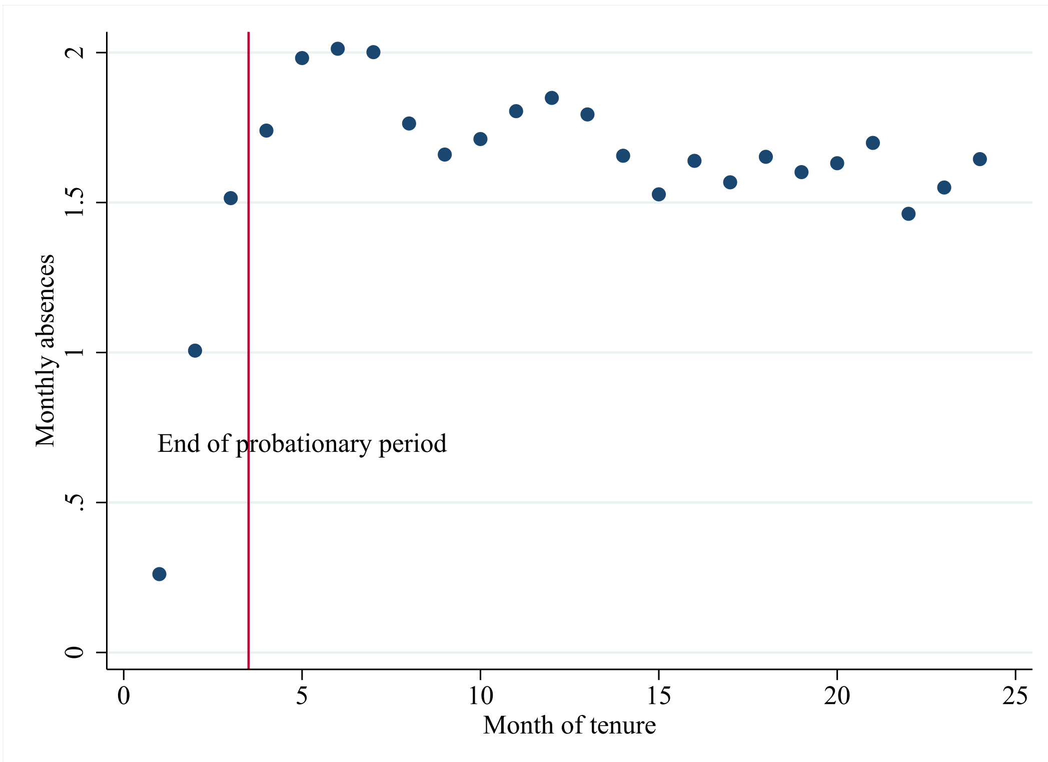
Appendix B Additional Figures and Tables

Figure B1: Monthly Attrition Hazard for Referrals vs. Non-referrals During the RCT



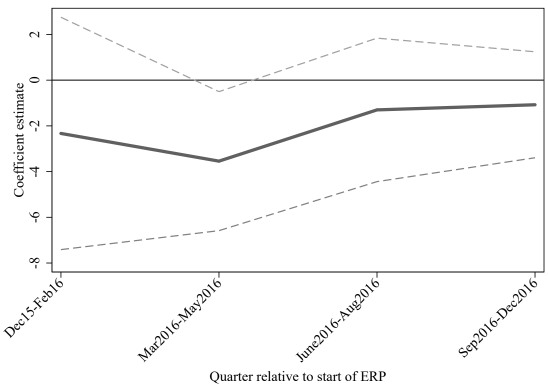
Notes: This figure shows the monthly attrition hazard as a function of worker tenure comparing referred vs. non-referred workers. The sample is the same as in Table 4 (i.e., months during the RCT worked by newly hired grocery workers). The referral and referrer must stay 5 months after the referral is hired in order for the referrer to be paid. The vertical tenure threshold line is drawn in between x=5 and x=6 because both referral and referrer must stay at least 5 months.

Figure B2: Relationship Between Month of Tenure and Monthly Absences for Full Sample of Workers

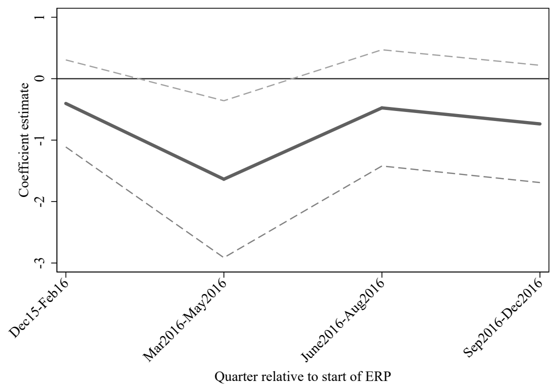


Notes: This figure shows average worker monthly absences by tenure. Each dot represents the average number of absences in a month of tenure. We use all the workers in our data, and restrict the picture to the first 24 months of tenure.

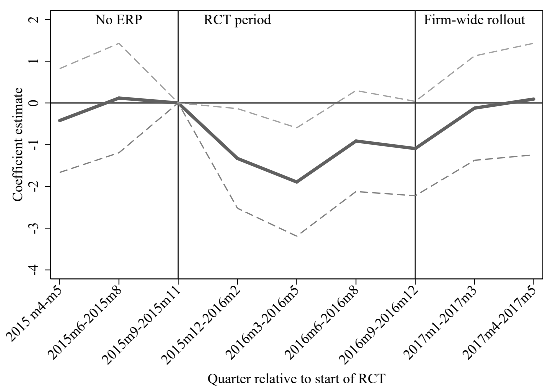
Figure B3: Event Studies on Impact of ERPs: Additional Subsamples and Outcomes. Solid Lines are Coefficients, Dotted Lines Show 95% Confidence Intervals



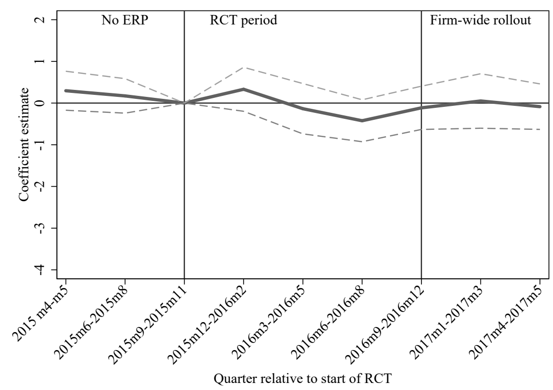
(a) Sample is New Hires During RCT



(b) Sample is Incumbents During RCT



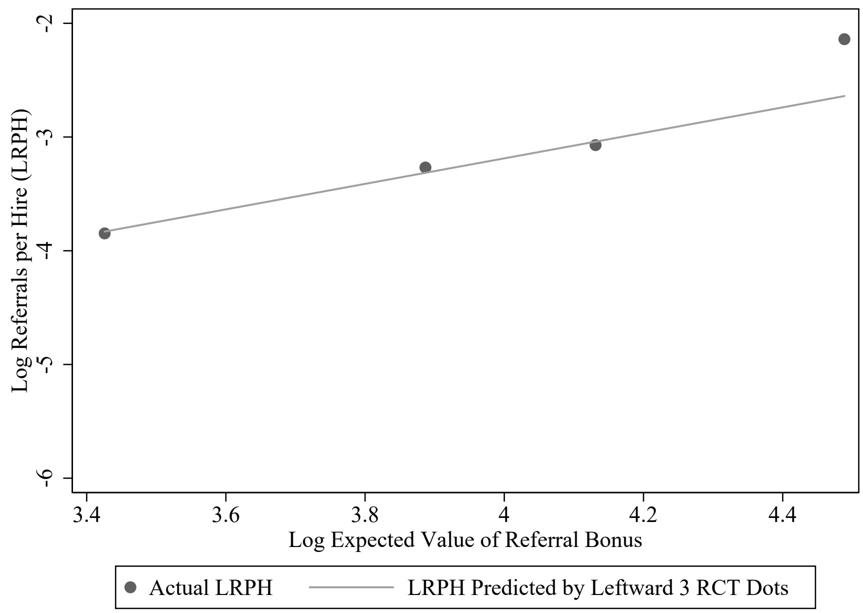
(c) Outcome is Voluntary Exits (“Quits”)



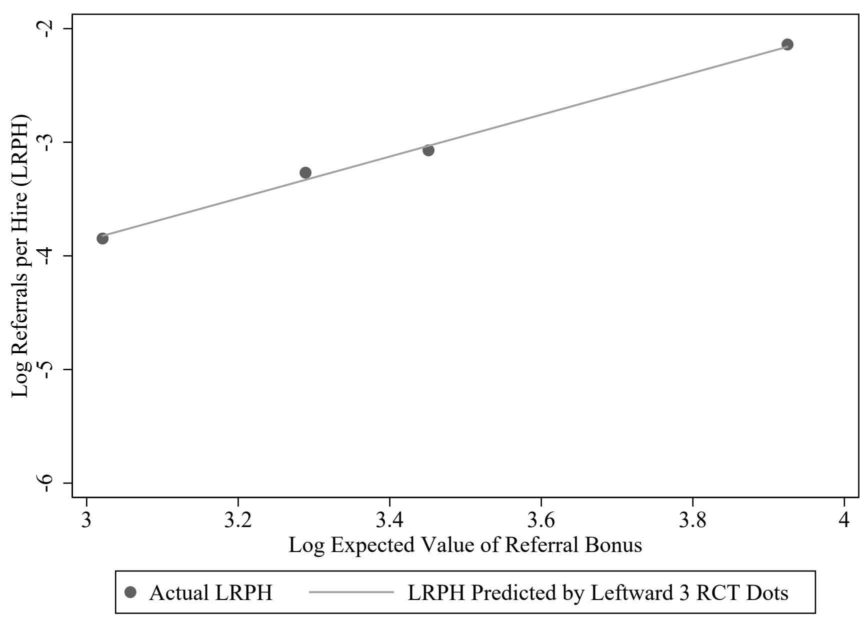
(d) Outcome is Involuntary Exits (“Fires”)

Notes: These figures are similar to the main event study in panel (a) of Figure 4. The difference is they analyze different samples or look at different individual outcomes (other than overall attrition). Panel (a) here analyzes grocery workers hired during the RCT, whereas panel (b) here analyzes grocery workers who were incumbents at the firm when the RCT began (i.e., they had been hired in the past). For both panel (a) and (b) here, it is not possible for the event study to go before the RCT because RCT hires and RCT incumbents do not attrite prior to the start of the RCT. Panel (c) analyzes voluntary attrition as the outcome variable, whereas panel (d) analyzes involuntary attrition. In panels (c) and (d), there are only 3 quarters graphed before the RCT because information on exit codes only begins in 2015m4.

Figure B4: Using the Relationship Between Bonus Size and Referrals During the RCT to Predict the Rate in the Rollout



(a) No Present Bias



(b) Present bias of $\beta = 0.35$

Notes: This figure shows the relationship between expected utility from getting the bonus and the rate of referrals per hire, using a log-log plot. In panel (a), we assume no present bias and no discounting. In panel (b) we assume a present bias term of $\beta = 0.35$ following Fang & Silverman (2009).

Table B1: Workers in the Post-RCT Survey of Workers Look Similar to the Overall Population of Cashiers at the Firm

	Cashiers in survey	All firm cashiers in May 2017
Female	.92	.91
Tenure below 3m	.11	.13
Tenure of 3-6m	.09	.1
Tenure of 7-12m	.15	.1
Tenure of 13-24m	.14	.12
Tenure more than 24m	.51	.55

Notes: This table compares workers in the post-RCT survey of workers discussed in Section 4.2.3 to the overall population of cashiers at the firm. Since the survey is performed in fall 2018, i.e., after our personnel data ends, we use the last month of personnel data. As can be seen, the two populations are very similar in terms of gender and tenure. The five ranges of tenure are the possible answers in the worker survey. Exact tenure is not asked about to avoid any perception that a worker's response could be tied to a particular worker.

Table B2: Robustness Check on Table 4: Adding Store Fixed Effects

Dep. var.:	Attrition			Monthly absences		
Method:	Cox Prop. Hazard Model			Negative Binomial		
	(1)	(2)	(3)	(4)	(5)	(6)
Hire was referred	-0.65***			-0.15		
	(0.14)			(0.25)		
Referred X first 5m		-0.72***			-0.41	
		(0.17)			(0.31)	
Referred X after 5m		-0.38			0.36	
		(0.27)			(0.50)	
Referred X R50			-1.91***			0.40
			(0.57)			(0.59)
Referred X R90			-0.60***			-0.26
			(0.20)			(0.60)
Referred X R120			-0.50**			-0.28
			(0.20)			(0.26)
F(R50 vs. R90)			0.03			0.45
F(R50 vs. R120)			0.02			0.28

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

Table B3: The Impact of the ERPs on Monthly Absence

Type of workers:	All	All	All	All	Hires	Hires	Inc	Inc
Sample period:	RCT	RCT	Pre &RCT	Pre &RCT	RCT	RCT	RCT	RCT
Coefficients shown:	Treatment dummies		Treatment X RCT period dummies		Treatment dummies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R0	0.02		-0.17		-0.07		0.04	
	(0.09)		(0.11)		(0.16)		(0.10)	
	[0.797]		[0.101]		[0.733]		[0.702]	
R50	0.02		-0.02		-0.21		0.07	
	(0.11)		(0.11)		(0.17)		(0.12)	
	[0.795]		[0.847]		[0.209]		[0.594]	
R90	0.05		0.02		-0.09		0.10	
	(0.10)		(0.10)		(0.17)		(0.10)	
	[0.571]		[0.885]		[0.603]		[0.331]	
R120	0.07		-0.16		-0.08		0.10	
	(0.10)		(0.10)		(0.16)		(0.11)	
	[0.424]		[0.131]		[0.699]		[0.356]	
ERP		0.04		-0.08		-0.11		0.08
		(0.08)		(0.08)		(0.14)		(0.08)
		[0.509]		[0.359]		[0.565]		[0.454]
Store FE	No	No	Yes	Yes	No	No	No	No
Observations	74,188	74,188	203,798	203,798	14,879	14,879	55,953	55,953
Mean DV if ERP=0	1.452	1.452	1.288	1.288	1.329	1.329	1.492	1.492
Workers	10,003	10,003	16,942	16,942	3,796	3,796	5,870	5,870

Notes: This table is similar to Table 5 except the outcome is monthly absences and the specifications are negative binomial instead of Cox. Standard errors clustered by store in parentheses. "Rand-t" randomization inference p-values following Young (2019) are in square brackets. For speed, we perform randomization inference using 100 replications instead of 1,000. Stars are based on the clustered standard errors in parentheses, with * significant at 10%; ** significant at 5%; *** significant at 1%

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

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

Notes: Standard errors clustered by store are in parentheses. An observation is a store-month. In Panel A, we control for the controls listed in footnote 17, plus region dummies, month-year dummies, and the pre-RCT store-level mean of the dependent variable. In Panel B, we control for store dummies and month-year dummies. The shrinkage rate is the share of inventory lost to theft, spoilage, and other reasons, so higher shrinkage is worse. Operational profits per worker are store-level sales minus cost of goods minus wages minus shrinkage. Operational profit is not a full measure of profit (e.g., it does not account for personnel costs at the central HR office). Therefore, our analysis of operational profits per worker here is conceptually distinct from the profits analysis in Section 5, where profit impacts are driven by reductions in different types of labor costs. * significant at 10%; ** significant at 5%; *** significant at 1%

Table B5: Demographic Homophily Between Referrers and Referrals

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

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

Table B6: The Impact of the ERPs on Attrition: Restrict to Stores with No Referrals Made during the RCT

Type of workers:	All	All	All	All	Hires	Hires	Inc	Inc
Sample period:	RCT	RCT	Pre &RCT	Pre &RCT	RCT	RCT	RCT	RCT
Coefficients shown:	Treatment dummies		Treatment X RCT period dummies		Treatment dummies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R0	-0.15***		-0.15**		-0.07		-0.24***	
	(0.06)		(0.08)		(0.06)		(0.08)	
R50	-0.07		-0.19**		-0.06		-0.10	
	(0.07)		(0.09)		(0.09)		(0.10)	
R90	-0.26***		-0.31***		-0.21**		-0.31***	
	(0.07)		(0.07)		(0.09)		(0.09)	
R120	-0.16**		-0.18**		-0.21**		-0.12	
	(0.07)		(0.07)		(0.09)		(0.09)	
ERP		-0.15***		-0.20***		-0.12**		-0.19***
		(0.05)		(0.06)		(0.06)		(0.07)
Store FE	No	No	Yes	Yes	No	No	No	No
Observations	59,677	59,677	164,860	164,860	11,536	11,536	45,490	45,490
Mean DV if ERP=0	6.677	6.677	5.434	5.434	17.24	17.24	4.362	4.362
Workers	8034	8034	13725	13725	2964	2964	4800	4800

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

Table B7: Correlation Coefficients for the Different Dimensions of Heterogeneity

Panel A: Five Main Dimensions of Heterogeneity (i.e., the ones in Table 8)

	Store performance index	Attrition rate	Unemployment rate	Male	Cashier
	(1)	(2)	(3)	(4)	(5)
Store performance	1.00				
Attrition rate	-0.29	1.00			
Unemployment rate	0.01	-0.30	1.00		
Male	-0.01	0.07	-0.09	1.00	
Cashier	0.02	0.03	-0.01	-0.11	1.00

Panel B: Looking Separately at the Variables in Store Performance Index

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

Notes: Correlation coefficients are reported. The store-level characteristics are store-level means calculated during the pre-RCT period. Correlations are calculated using our worker-month panel during the RCT period (i.e., the correlations are weighted by a store's worker-months during the RCT). The unemployment rate is the 2015 municipal unemployment rate.

Table B8: Comparing Referrals vs. Non-referrals in Attrition: Heterogeneity Analysis (Cox Models)

Cox models	Store performance index (1)	Attrition rate (2)	Unemployment rate (3)	Male (4)	Cashier (5)
Hire was referred	-0.59***	-0.59***	-0.59***	-0.58***	-0.58***
	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)
Referred X Char	-0.10	0.08*	0.11*	-0.20**	-0.07
	(0.06)	(0.04)	(0.06)	(0.08)	(0.06)

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

Table B9: Robustness Check for Store-level Heterogeneity (Columns 1-3 of Panel B of Table 5): Split by Above/Below Median Store Performance and Unemployment

Characteristic: (all binarized)	Store performance index (1)	Attrition rate (2)	Unemployment rate (3)
Below Median	-0.09	-0.22***	-0.18***
	(0.07)	(0.06)	(0.06)
Above Median	-0.20***	-0.13**	-0.05
	(0.06)	(0.06)	(0.08)

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

Table B10: Robustness Check for Column 1 of Table 8: Heterogeneity Separated by Three Different Dimensions of Pre-RCT Store Performance Instead of by the Index

Characteristic: (all normalized)	Log shrinkage rate Higher is worse. (1)	Log sales per worker (2)	Log operational profit per worker (3)
Panel A: Direct Effects. DV = Hire is a Referral (x100).			
ERP	2.41***	2.38***	2.25***
	(0.62)	(0.58)	(0.58)
ERP X Characteristic	-0.09	0.44	0.69
	(0.53)	(0.52)	(0.47)
Panel B: Overall Effects. Cox, DV = Worker Attrites.			
ERP	-0.15***	-0.14***	-0.13***
	(0.04)	(0.05)	(0.05)
ERP X Characteristic	0.08**	-0.08*	-0.08*
	(0.04)	(0.04)	(0.05)

Notes: This table is similar to Panels A and B of Table 8, but looks separately at the 3 dimensions of the store performance index. Standard errors clustered at the store level are in parentheses. Shrinkage is the share of inventory lost to theft, spoilage, and other reasons, so higher shrinkage means worse performance. * significant at 10%; ** significant at 5%; *** significant at 1%

Table B11: Examining Alternative Explanations for Larger Attrition Reductions in Higher-Performing Stores (i.e., Column 1 of Panel B of Table 8): Cox Models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ERP X Store performance index	-0.12**	-0.10**	-0.11***	-0.10**	-0.10**	-0.10**	-0.10**
	(0.06)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)
Restrict to Full-Service Grocery Stores	X						
Variable to add:							
ERP*(Pre-RCT Mean # of Products Offered)		X					
ERP*(Pre-RCT Share Products that are Fresh Goods)			X				
ERP*(Pre-RCT Mean # of Total Checkouts)				X			
ERP*(Pre-RCT Mean Head Count)					X		
ERP*(Store Square Meters)						X	
ERP*(Dummy for Lidl Store Nearby)							X

Notes: This table accompanies the discussion in Appendix A.13 and is a robustness check to column 1 of Panel B of Table 8. It shows how the key interaction term coefficient changes as either the sample is restricted (column 1) or where we also include regressors for an additional characteristic and the interaction of ERP times that characteristic (columns 2-7). In columns 2, 3, 4, and 6, the additional characteristic is normalized. Panel 4 is robust to looking separately at manned checkouts or self-checkouts. * significant at 10%; ** significant at 5%; *** significant at 1%

Table B12: Estimating the Impact of ERPs on Attrition Using the Firmwide ERP Rollout in a Difference-in-Differences Design. Cox Model of Worker Attrition

	(1)
Control store X Post-RCT rollout	-0.22**
	(0.09)
Mean DV in control stores	6.136
Observations	100,257
Workers	11,193

Notes: This table is similar to column 4 of Table 5 except the data are from the RCT and post-RCT periods (Dec 2015-May 2017). The key regressor is being in a control store (i.e., a store assigned to no ERP for the RCT) interacted with a dummy for the time being in the post-RCT rollout period (i.e., Jan 2017-May 2017). We control for store fixed effects, current month-year fixed effects, and the other controls also included in column 4 of Table 5. The coefficient on the key regressor represents the change in attrition in control stores as a result of the rollout (relative to the change in attrition in treatment stores). "Mean DV in control stores" means the mean attrition in worker-months at stores that were assigned to no ERP during the RCT. * significant at 10%; ** significant at 5%; *** significant at 1%

Appendix C Data Appendix

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

Age. As discussed in footnote 24 in the main text, we do not control for worker age in our analysis of the overall effects of ERPs, as age is missing for workers joining the firm before the start of the data and who never attrite. Age is missing for 25% of all workers and 40% of all worker-months during the RCT. Our results are robust if we control for age while restricting attention to workers hired during our data period (who thus consistently have age data).

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

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

Multiple spells. Some workers in our data have multiple spells, where they return to working at the firm after a break in the record. In our population of workers, it is not uncommon to take breaks in employment. When a worker has multiple spells, we only count the final attrition event, and not the earlier ones. In addition, if a worker has a date of hire which is more recent than the current date, we assign the date of hire to that worker's earliest date on record. Our results are similar if we instead consider only the most recent spell.[16] For referrals made in the RCT, we impose that referral spells be counted so as to not exclude referrals, for reasons of statistical precision. That is, for a small number of people who are hired as a referral despite having an earlier spell, we count those as separate spells. Results are similar if we do not do this. In Cox analysis, event time starts anew following a person's attrition event.

Compensation data. While we have data on worker compensation, we do not analyze it. The reason is that pay is relatively deterministic, being primarily determined by a person's position and tenure.

Tenure/Time/Cohort effects. In the Cox model, tenure is controlled for nonparametrically. To avoid potential collineraity, we use cohort effects at the quarter-year of hire instead of month-year of hire.

Stratification in Cox models. For the binary heterogeneity variables (i.e., gender and job type) in Table 8, Cox results are very similar if we stratify the baseline hazard based on each of those variables in the relevant regression.

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

Appendix D Theory Appendix

D.1 Formal Model

We present a simple model to fix ideas on how ERPs affect employee outcomes, both directly in terms of affecting referrals and indirectly via creating respect. The model takes up three ideas. First, an ERP provides the firm with more precise signals about a candidate's match quality (Simon & Warner, 1992; Brown *et al.*, 2016; Dustmann *et al.*, 2015). In contrast to these models, we assume that the information resides with an employee instead of the overall firm. Second, workers have social preferences toward friends they may refer (Bandiera *et al.*, 2005, 2008, 2009; Beaman & Magruder, 2012; Rubineau & Fernandez, 2015; Ashraf & Bandiera, 2018). Third, and potentially most important, our model incorporates workers caring about being respected (Ellingsen & Johannesson, 2008). More precisely, employees who are pro-social want the firm to think that they are pro-social.[17]

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

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

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

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

$$U^I(R = 1) = (1 - \Sigma)b + \Sigma U^N(R = 1) + B(\hat{\Sigma}) \tag{12}$$
$$U^I(R = 0) = \Sigma U^N(R = 0) + B(\hat{\Sigma}) = B(\hat{\Sigma}), \tag{13}$$

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

Here, N's utility depends on the job match, m, and job overall attractiveness, q, with $U^N(R=1) = m + q$. Match m represents all person-specific rewards from the job. Job attractiveness, q, is the same for all workers, and may depend not only on the wage but also on its non-pecuniary aspects, such as working conditions and reputation in society. In (13), we normalize N's utility if he is not referred to 0.

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

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

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

where $E[m|m > m^*]$, and $E[m]$ are the expected quality matches of the referred and non-referred workers, respectively; c is the cost of attrition for an incumbent worker; and $Pr(Q)$ is the probability that the incumbent worker exits and is equal to $1 - G(\sigma)$.[18] In contrast, firm profits without an ERP are $E(m) - c(1 - G(0))$.

Our model yields five predictions. We provide intuition here and proofs in Appendix D.2. The model's simplifying assumptions are discussed more in Appendix D.3.[19]

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

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

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

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

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

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

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

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

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

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

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

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

D.2 Solving the Model

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

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

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

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

Now, we check that the firm's strategy is optimal given the worker's strategy. If $t = 1$, the firm's profits from having an ERP are
$r\big(E[m|m > m^*] - \widetilde{b}\big) + (1-r)E[m] - c(1 - G(\sigma))$, which is larger than $E(m) - c(1 - G(0))$, provided that the referral bonus $\widetilde{b}$ is not too large. In contrast, if $t = 0$, the firm thinks

there is no retention benefit of having an ERP, as the firm thinks the worker is selfish in the absence of a good signal, and selfish workers don't care about the firm's esteem. Specifically, the firm's profits from having an ERP are $E(m) - r\widetilde{b} - c(1 - G(0))$, which are lower than the profits without an ERP of $E(m) - c(1 - G(0))$.

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

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

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

$$U^I(R = 1) = (1 - \sigma)b + \sigma(m + q) + B(\sigma) \tag{14}$$

$$U^I(R = 0) = B(\sigma) = B(\sigma) \tag{15}$$

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

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

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

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

which is positive.

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

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

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

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

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

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

$$\frac{\partial \pi}{\partial \widetilde{b}} = \frac{\partial r}{\partial \widetilde{b}} \left(H^r - \widetilde{b} - E[m] \right) + r \left(\frac{\partial H^r}{\partial \widetilde{b}} - 1 \right), \tag{16}$$

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

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

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

To analyze the relevance of job attractiveness for the decision to refer, note that $\frac{\partial r}{\partial q} = f(m^*)$, which is positive because people value their friends and value referring them for better jobs. To see how job quality affects the responsiveness of referrals to bonuses, note that $\frac{\partial^2 r}{\partial b \partial q} = -f'(m^*)\frac{1-\sigma}{\sigma}$. Thus, if $f'(m^*) < 0$, then $sgn\big(\frac{\partial^2 r}{\partial b \partial q}\big) = -sgn(f') = +$. This seems likely to hold if only a minority of workers make referrals.[20]

D.3 Discussion of Model Assumptions

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

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

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

The incumbent has social preferences toward her friend, not toward the firm. We assume that the incumbent worker only has potential social preferences toward her friend, not toward the firm. If the worker had potential social preferences toward the firm, all predictions of the model would be the same. The key feature of the model is that having an ERP involves delegating the hiring decision to the incumbent worker, and doing so is only valuable if the worker cares about the match quality of a referred worker. The incumbent worker may do so because she cares about her friend (and the firm also happens to benefit from higher match quality) or because she directly cares about the firm. In our model, the firm also has zero outside information outside of potential referrals.[21] Also, while we assume that the friend and firm equally benefit from match quality for simplicity, this assumption is not required.

The level of the referral bonus and respect. We assume that a worker's true social preferences can only take two values, and we do not analyze the worker updating their sense of respect in response to the particular value of $\widetilde{b}$. If worker social preferences can take many values, then choosing higher values of $\widetilde{b}$ could communicate that the firm has a particularly high belief about the value of altruism for a worker. On the other hand, outside our model, choosing a very high value of $\widetilde{b}$ could communicate other messages, such as that making referrals is an unpleasant task (Bénabou & Tirole, 2003). Thus, because of these competing effects, we set this aspect aside. One can also examine empirically whether larger referral bonuses tend to have larger impacts on incumbent workers. Conditional on having a referral bonus, we do not observe a clear relation between the level of the bonus and incumbent retention effect, as seen in Figure 5 in the main text.

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

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

We first present the letters given to workers in the RCT. These are followed by Figure E1, which shows the posters that were used in the 2017 firmwide ERP rollout. The only information redacted in these documents is firm identifiers (i.e., firm name or logo); the name of the country where the firm operates; and employee names and contact information.

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

[FIRM logo]

Dear Employee,

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

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

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

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

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

Best wishes,
[FIRM logo]

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

[FIRM logo]

Dear Employee,

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

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

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

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

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

IMPORTANT!

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

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

Best wishes,
[FIRM logo]

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

Figure E1: Posters Used during the 2017 Firmwide ERP Rollout

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

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