# Onboarding, Matching, and Employee Turnover – A Field Experiment*

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**Abstract**

We evaluate how a low-cost onboarding practice at the beginning of the employment affects turnover among newly hired frontline employees. In partnership with a large German retail chain, we ran a field experiment across more than 900 stores and 2371 hires (August 2024 - July 2025). Treated stores implemented a structured welcome intervention: managers announced the newcomer in advance and hosted a brief social event with their colleagues on the first day, while control stores continued with business-as-usual onboarding. The intervention increases separation in the first month of employment by about 2 percentage points while cumulative separations over three and six months are largely unchanged, in line with the view that the intervention accelerates mutual learning about match quality. Both stayers and movers in the treatment group have higher working hours indicating that the treatment also fostered employee motivation.

**Keywords:** Field Experiment, Identity, Turnover, Employee Matching
**JEL classification:** C93, J24, J63, M54, M12

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# 1 Introduction

Employee onboarding encompasses the set of practices firms deploy at the very start of employment to welcome new hires, provide structure and guidance, and facilitate early social integration. These practices matter because the first weeks on the job are a period of intense uncertainty for both sides: employers are still forming beliefs about a worker's productivity, while workers are learning about the many non-monetary aspects of the job that are difficult to observe ex ante, from team dynamics to managerial style to day-to-day working conditions. Onboarding is often designed to smooth this adjustment process by fostering early connection, clarifying expectations, and supporting the accumulation of firm-specific human capital.

At the same time, onboarding can shape early turnover through two conceptually distinct channels. By strengthening attachment and generating firm-specific human capital more quickly, it may reduce separations that would otherwise occur before recruitment and training investments have time to pay off. Yet onboarding also increases interaction, feedback, and mutual observation, which can accelerate learning about match quality. Faster revelation of a poor fit can raise early separations—but such turnover need not be undesirable if it helps both sides avoid persisting in mismatches. Whether onboarding ultimately lowers or raises early turnover is therefore an empirical question, reflecting the balance between attachment-building and match-revealing forces.

We run a large-scale field experiment in collaboration with a German retail chain to test how a simple, low-cost onboarding practice at the beginning of the employment to enhance early social integration affects early turnover among front-line workers. We randomly assign 104 districts, comprising more than 900 supermarkets, either to continue with the status quo process or to a treatment group, implementing a structured *Welcome Intervention* for all new hires. We ask store managers in treated districts to announce the newcomer in advance, organize a short social gathering on the first day, and provide a small personalized gift in addition to implementing the existing training plan. The study covers 2,371 hires between August 2024 and July 2025.

The main finding is that the Welcome Intervention increases separations in the first month of employment. The first-month separation rate rises from 3.40% by 2 percentage points in treated stores. In contrast, separations over three and six months are statistically similar across groups. Both quits and dismissals increase in the first month. This pattern indicates that the intervention primarily shifts the timing of separations forward rather than reducing turnover overall.

To interpret these results, we develop a simple framework that combines identity-based views of onboarding with learning-based models of job matching. Research on identity predicts that social identities and relationships within the firm are key determinants of effort and retention, beyond wages and formal incentives. Theories on identity highlight that employees' identification with their organization or team can reduce the need for extrinsic incentives and enhance commitment (Akerlof and Kranton, 2005).[1] Empirical studies provide supporting evidence: organizational identification mediates the relationship between perceptions of fairness and turnover intention (Cho, Lee and Kim, 2014), influences engagement with organizational

[1] Similarly, research on social cohesion shows that the strength of interpersonal bonds within teams moderates the effectiveness of incentive schemes and can influence organizational outcomes (Delfgaauw et al., 2022).

values (Guadalupe, Kinias and Schloderer, 2020), and shapes labor supply decisions even in high-stakes contexts (Oh, 2023).[2] Field evidence further suggests that onboarding practices can be designed to activate newcomers' personal identities and generate persistent gains in retention and motivation (Cable, Gino and Staats, 2013). Together, these insights suggest that fostering identity and social integration at entry may reduce early employee turnover.

In contrast, job-matching models in labor economics emphasize that match quality is uncertain at hiring and is revealed through early signals. Because learning is fastest at the beginning of employment, policies that increase signal precision can raise early separations (Jovanovic, 1979). Our framework formalizes these two channels—an attachment channel that raises expected match quality and a match-revelation (learning) channel that increases early signal precision—and shows why turnover effects can be ambiguous. Importantly, both channels imply higher average match quality among retained workers, either through direct improvements or through selection.

Consistent with the match-revelation channel, the treatment increases both quits and dismissals in the first month, with little effect at longer horizons. Complementary survey evidence (with limited power) suggests that eventual leavers report lower organizational identification early in the employment relationship. But we also find that treated employees provide more overtime work hours irrespective of whether they separate or not indicating that the treatment also helped to foster employee motivation.

This paper makes several contributions to the literature on matching, identity, onboarding, and turnover in organizations. First, we provide large-scale experimental evidence on the causal effect of a scalable, inexpensive HR tool on early retention and match quality. We thus contribute to the recent field experimental literature on organizational practices targeting employee relationships, leadership quality, and social connectedness which can significantly affect turnover. For example, interventions that improve leader–subordinate interactions (Alan, Corekcioglu and Sutter, 2023; Wu, Liu and Xu, 2024), introduce social activities (Belot and Hakimov, 2025), or nudge middle managers to try to reduce turnover (Friebel, Heinz and Zubanov, 2022) reduce overall separation rates, while onboarding activities with a social impact component lower attrition among new hires (Portocarrero and Burbano, 2024). At the same time, firms adapt compensation systems and career structures to manage retention concerns (Labro and Omartian, 2025; Deller, 2023). In doing so, we fill an important gap in the turnover literature, which has largely focused on wages, incentives, and career prospects, by showing how a low-cost onboarding practice can systematically affect early-stage exits.

[2] In parallel, research on organizational socialization and onboarding argues that practices which facilitate newcomers' adjustment—through early social contact, information, and support—improve job satisfaction, commitment, and reduce withdrawal (Bauer and Erdogan, 2011).

Second, our paper also contributes to the literature on job matching (Jovanovic, 1979; Gibbons and Katz, 1991; Topel and Ward, 1992; Farber and Gibbons, 1996; Altonji and Pierret, 2001; Pinkston, 2009; Nagypál, 2007; Kahn, 2013). For one, we provide causal evidence for the typical claim in this literature that separations are driven by information acquisition on match quality by showing that an intervention that speeds up this information acquisition indeed increases early turnover. Moreover, we show that firm policies can actively shape this matching process through their onboarding processes actively fostering information acquisition.

The remainder of the paper is structured as follows: Section 2 conceptualizes the two potential effects of onboarding practices. Section 3 presents the design, followed by a description of the data in Section 4. We show and discuss results in Section 5. Section 6 concludes.

## 2 A Conceptual Framework

Consider the following simple model. An employee works for a firm for up to two periods $t = 1, 2$. Productivity and worker well-being on the job are match-specific. Following models of job matching such as Jovanovic (1979) we assume that the employee's net productivity $v$ at the firm and his personal utility $u$ from working are initially unknown and both employer and employee hold prior expectations that they are normally distributed with

$$\begin{aligned} v &\sim N\left(m_v, \sigma_v^2\right) \text{ and} \\ u &\sim N\left(m_u, \sigma_u^2\right). \end{aligned}$$

As the firm has hired the employee and the employee has decided to work for the firm, ex-ante expected productivity $m_v$ and utility $m_u$ are strictly positive. In period 1 both the firm and the employee receive signals $s_{v1}$ and $s_{u1}$ where

$$\begin{aligned} s_{v1} &= v + \varepsilon_v \text{ with } \varepsilon_v \sim N\left(0, \sigma_{\varepsilon_v}^2\right) \\ s_{u1} &= u + \varepsilon_u \text{ with } \varepsilon_u \sim N\left(0, \sigma_{\varepsilon_u}^2\right) \end{aligned}$$

so that both acquire information about the match quality when the worker starts to work.

The firm can now organize an onboarding process choosing $o \in \{0, 1\}$. The onboarding event can have two effects: It may increase productivity and utility by creating additional human capital and emotional attachment such that

$$\begin{aligned} m_v\left(o = 1\right) &\geq m_v\left(o = 0\right) \text{ and} \\ m_u\left(o = 1\right) &\geq m_u\left(o = 0\right). \end{aligned} \tag{1}$$

But by speeding up mutual information acquisition about match quality it can also affect the quality of the signal acquired in period 1 reducing noise such that

$$\begin{aligned} \sigma_{\varepsilon_v}^2\left(o = 1\right) &\leq \sigma_{\varepsilon_v}^2\left(o = 0\right) \text{ and} \\ \sigma_{\varepsilon_u}^2\left(o = 1\right) &\leq \sigma_{\varepsilon_u}^2\left(o = 0\right). \end{aligned} \tag{2}$$

Hence, onboarding can thus have two effects: It can improve utility and productivity and it can speed up learning about match quality.

After period 1 the employee can decide to quit $q \in \{0,1\}$ and the firm can decide to fire the employee $f \in \{0,1\}$ . Both the firm and the employee have an outside option normalized to 0 in period 2 such that the employee quits ($q = 1$) if $u < 0$ and the firm fires the employee ($f = 1$) if $v < 0$.

Consider first the employee's decision whether to quit after period 1. He will update his beliefs about the utility on the job and using a standard result for jointly normal random variables we have that

$$\begin{aligned} E\left[u|\, s_{u1}\right] &= m_u + \frac{Cov\left[u, s_{u1}\right]}{V\left[s_{u1}\right]}\left(s_{u1} - E\left[s_{u1}\right]\right) \\ &= m_u + \frac{\sigma_u^2}{\sigma_u^2 + \sigma_{\varepsilon_u}^2}\left(s_{u1} - m_u\right) \end{aligned}$$

and thus quit if $E\left[u|\, s_{u1}\right] < 0$ which is equivalent to

$$s_{u1} < -\frac{\sigma_{\varepsilon_u}^2}{\sigma_u^2} m_u .$$

The quit probability is

$$P\left(q = 1\right) = \Pr\left(s_{u1} < -\frac{\sigma_{\varepsilon_u}^2}{\sigma_u^2} m_u\right).$$

Note that $s_{u1} \sim N\left(m_u, \sigma_u^2 + \sigma_{\varepsilon_u}^2\right)$ such that

$$\begin{aligned} P\left(q = 1\right) &= \Pr\left(s_{u1} < -\frac{\sigma_{\varepsilon_u}^2}{\sigma_u^2} m_u\right) = \Phi\left(\frac{-\frac{\sigma_{\varepsilon_u}^2}{\sigma_u^2} m_u - m_u}{\sqrt{\sigma_u^2 + \sigma_{\varepsilon_u}^2}}\right) \\ &= \Phi\left(-\sqrt{\sigma_u^2 + \sigma_{\varepsilon_u}^2}\,\frac{m_u}{\sigma_u^2}\right). \end{aligned}$$

where $\Phi()$ is the cdf of a standard normal distribution. Analogously we can derive the firing probability

$$P\left(f = 1\right) = \Phi\left(-\sqrt{\sigma_v^2 + \sigma_{\varepsilon_v}^2}\,\frac{m_v}{\sigma_v^2}\right).$$

Now consider the onboarding intervention. We first isolate the direct match-quality effect. First note that $P\left(q = 1\right)$ is strictly decreasing in $m_u$ and $P\left(f = 1\right)$ is strictly decreasing in $m_v$. We thus have:

**Proposition 1.** ***(Attachment Channel)*** *If onboarding increases only expected productivity and utility—i.e. if $m_v\left(o = 1\right) \geq m_v\left(o = 0\right)$ and $m_u\left(o = 1\right) \geq m_u\left(o = 0\right)$ while leaving signal noise unchanged, then onboarding strictly reduces both the probability that the employee quits, $P\left(q = 1\right)$, and the probability that the firm fires the employee, $P\left(f = 1\right)$.*

The intuition is straightforward: when the onboarding process creates more human capital (increase in $m_v$) or more employee attachment to the firm (increase in $m_u$) it makes it less attractive for the firm to fire the employee and more attractive for the employee to stay. Onboarding thus should lead to a reduction in early turnover from this perspective.

However, when the onboarding process conveys more information about match quality this picture may change. To see that note that both the firing and the quit probabilities $P\left(q=1\right)$ and $P\left(f=1\right)$ are strictly decreasing in the respective noise terms which implies the following:

**Proposition 2.** ***(Match-Revelation (Learning) Channel)*** *If onboarding reduces only the noise in the first period signal—i.e. if* $\sigma^2_{\varepsilon_v}\left(o=1\right) \leq \sigma^2_{\varepsilon_v}\left(o=0\right)$ *and* $\sigma^2_{\varepsilon_u}\left(o=1\right) \leq \sigma^2_{\varepsilon_u}\left(o=0\right)$ *while leaving expected match quality unchanged, then onboarding strictly increases both the probability that the employee quits,* $P\left(q=1\right)$*, and the probability that the firm fires the employee,* $P\left(f=1\right)$*.*

The intuition for this effect is the following: At hiring, both parties hold optimistic priors $m_v > 0$ and $m_u > 0$. With noisy signals, posterior beliefs remain close to these priors, making early separations unlikely. When onboarding improves signal precision, first-period signals receive more weight in posterior beliefs. This increases the probability that the updated assessment becomes sufficiently negative to trigger quitting or dismissal. In short, better information accelerates the identification of bad matches.

A better onboarding process thus can have an ambiguous effect on turnover as it can directly increase match quality by generating more human capital and attachment and thus reducing turnover. But it can also increase turnover when reducing uncertainty about match quality and thus facilitating to filter out bad matches early on.

In a next step we consider the expected second period utility of those employees who did not quit

$$E\left[u|\, s_{u1} \geq -\frac{\sigma^2_{\varepsilon_u}}{\sigma^2_u} m_u\right]$$

and analogously the productivity of those who were not fired

$$E\left[v|\, s_{v1} \geq -\frac{\sigma^2_{\varepsilon_v}}{\sigma^2_v} m_v\right].$$

We can show:

**Proposition 3.** *Onboarding increases the expected utility of the employees who did not quit and the expected productivity of those who are not fired.*

**Proof:** We use a result that follows from property B.46 in Gourieroux and Monfort (1995) by which we have for two jointly normal random variables $X$ and $Z$ that $E[X|\, Z \geq 0] = E[X] + \frac{Cov[X,Z]}{\sqrt{V[Z]}} \frac{\phi\left(\frac{E[Z]}{\sqrt{V[Z]}}\right)}{\Phi\left(\frac{E[Z]}{\sqrt{V[Z]}}\right)}$ where $\phi()$ is the pdf of a standard normal distribution. Let $Z = s_{u1} + \frac{\sigma^2_{\varepsilon_u}}{\sigma^2_u} m_u$ such that $E[Z] = \left(\frac{\sigma^2_u + \sigma^2_{\varepsilon_u}}{\sigma^2_u}\right) m_u$. We thus have that

$$\begin{aligned} E\left[u|\, s_{u1} \geq -\frac{\sigma^2_{\varepsilon_u}}{\sigma^2_u} m_u\right] &= E\left[u|\, s_{u1} + \frac{\sigma^2_{\varepsilon_u}}{\sigma^2_u} m_u \geq 0\right] \\ &= m_u + \frac{\sigma^2_u}{\sqrt{\sigma^2_u + \sigma^2_{\varepsilon_u}}} \lambda\left(\sqrt{\sigma^2_u + \sigma^2_{\varepsilon_u}} \frac{m_u}{\sigma^2_u}\right) \end{aligned}$$

where $\lambda(x) = \frac{\phi(x)}{\Phi(x)}$ with $\lambda(x) > 0$ and $-1 < \lambda'(x) < 0$ (see Gourieroux and Monfort (1995, p. 284) for derivations of these properties). First note that this expression is strictly decreasing in $\sigma^2_{\varepsilon_u}$. Moreover, the first derivative of this expression with respect to $m_u$ is

$$1 + \lambda'\left(\sqrt{\sigma^2_u + \sigma^2_{\varepsilon_u}} \frac{m_u}{\sigma^2_u}\right) > 0$$

as $\lambda'(x) > -1$. We proceed analogously for $E\left[v|\, s_{v1} \geq -\frac{\sigma^2_{\varepsilon_v}}{\sigma^2_v} m_v\right]$ ∎

Here both effects go into the same direction: As the attachment channel increases productivity and utility due to human capital formation and increased identification. When on top of this the onboarding process also reduces noise and the first period signal becomes more informative, bad matches get more strongly negative signals and are more likely to quit or be fired while good matches continue to stay. This tighter selection makes the pool of stayers more positively selected on, so their average utility and productivity conditional on staying goes up, even though more people leave.

# 3 Experimental Design

## 3.1 The Background

We collaborate with a large retail chain that operates supermarkets throughout Germany. The firm is structured in several geographical regions. In the two regions in which the experiment is implemented, each region is divided into five to seven sales areas which are managed by sales managers. Each sales area consists of six to eleven districts which are managed by respective district managers. District managers supervise eight to twelve stores. The average store employs an average of eleven employees which are equivalent to seven full-time employees (July 25) and one store manager.

Store managers serve as the link to district management and are responsible for personnel planning and strategic decisions such as ordering goods for the store. Employees mainly work as cashiers and are responsible for restocking the shelves. New employees who start working in a store typically have a probation period of three to six months. Especially during this time of probation, turnover has been high with an average quit rate of about 30%. Based on a previous onboarding questionnaire, HR managers aimed to enhance the social dimension of the onboarding process starting from the first day. To causally study an intervention's success, they cooperated with us to implement a randomized controlled trial.

## 3.2 The Experiment

Within two regions of the retail company, we randomly assigned half of the districts to a treatment group, in which stores were asked to implement a new onboarding event for newly hired employees. In total, 104[3] districts (comprising more than 900 stores) have been randomly assigned to the treatment or control group using stratified randomization.

We stratified the districts with respect to the two regions and the previous turnover: quartiles of the mean turnover of the districts in the 14 months previous to the intervention are used. To improve balance at the store level, we additionally applied the min-max approach, as for instance applied by Ashraf, Bandiera and Jack (2014). Specifically, we generated 100 random assignments within strata and selected the draw that minimized the maximum absolute t-statistic between treatment and control stores across key store-level covariates: pre-intervention turnover, average headcount, store size, and average FTE.

In late June 2024, regional HR managers informed district managers about the upcoming project, starting August 1, with tailored communication depending on their treatment status. During the experimental period, district managers received bi-weekly reminders of new employees starting within four weeks, including treatment-dependent reminders about the project and the onboarding surveys.

### 3.2.1 Stores in the control districts

Stores in the control districts follow the pre-experimental onboarding process. This process relies on a standardized training plan that supervisors are expected to follow and complete with checkmarks. The plan, handed to employees at the start, covers both mandatory skill-based training and some social elements (e.g., first-day preparation and weekly feedback meetings). At the beginning of the intervention, district and store managers are reminded of the training plan.[4]

During the intervention period, newly hired employees in the control districts are surveyed as described below.

[3] Newly set up districts have been assigned to the control condition.

[4] Pre-intervention, completed plans were typically submitted to respective managers with all boxes checked regardless of what had been done. As a result, the training plan provided little reliable information about the actual processes, and onboarding practices varied widely across stores.

### 3.2.2 Stores in the treated districts

Stores in the treated districts introduced the *Welcome Intervention*, which combines social and symbolic elements to foster a sense of belonging and appreciation. In particular, store managers in the treated districts are asked to apply the following procedure. Before a new employee starts to work in the store, store managers announce the arrival of this new employee using a poster in the break room. On the new employee's first day, they organize a short social event: Employees on the morning shift are asked to stay 15 minutes longer, and employees on the afternoon shift are asked to come 15 minutes earlier (attendance is voluntary but will be counted as working time). During this period, the new employee has the opportunity to get to know the attending colleagues over a joint small meal/snack.

Each store manager receives a budget of 25€ to buy food and drinks from the store. Additionally, during that get-together, new employees receive a personalized gift (box of chocolates or other gift with a value of 4€) with a handwritten post-it note welcoming them to the team. Store managers will also hand over the initial training plan. While the plan is identical to the plan used in the control group, store managers are also reminded of the importance of the social aspects in the training plan (e.g. regular feedback meetings).

### 3.2.3 The Survey

In both participating regions, we introduced a a new onboarding survey for all newly hired employees. This survey measures the perceived success of the onboarding process, as well as the extent to which new employees identify with the organization, their colleagues, and their supervisors. It also elicits commitment, communication, supervisor competence and relations, engagement, employer recommendation, personality traits, and reciprocity.

All new employees (both control and treatment group) in these two regions receive the onboarding survey within their first week of employment and again after three months to measure both short- and long-term effects of the new onboarding process. Employees whose contracts end within the first three months receive an exit survey on quit reasons, future employment, employer recommendation, identification with the organization, their colleagues, and their supervisors, commitment, engagement, and demographics.

After seven months, store managers were surveyed on onboarding processes, identification with staff and the organization, store communication, team cohesion, commitment (own and perceived staff commitment), engagement, and reciprocity. Participation rate in this survey was high, with more than 60% of store managers providing complete responses.

## 4 Data

Our final sample comprises all employees who started working at the organization between August 2024 and the end of July 2025. We exclude stores and new employees according to our preregistration: First, stores which are newly opened or permanently closed in the experimental period (August 2024 to end of January 2026) and their respective newly hired employees are

excluded from the final sample. Second, we remove stores and their new employees if they have changed their treatment status during the intervention period from August 2024 to July 2025. After these exclusions, our final sample consists of 2,371 employees who have started working at the organization.

Most hires are in sales roles (66.5%), followed by apprentices (13.6%), sales support (13.2%) and cleaning staff (5.9%). See Table A1 in the Appendix for an overview of employee characteristics and balance between those starting in treated and control stores.

The employees start in 730 different stores. These stores are balanced in terms of their characteristics such as store size, share of women, pre-experimental turnover, sick leave days and other store performance measures, see Table A2 in the Appendix.

## 5 Results

Recall that store managers in the treated districts were asked to organize the welcome events. Because the intervention was communicated top-down from district managers – who received bi-weekly reminders of us if they hired new employees – to store managers, implementation of the *Welcome Intervention* could not be directly enforced. We, thus, start our analysis by looking at the reduced form or intent-to-treat estimates results regressing turnover likelihoods on a treatment assignment dummy.

Table 1 shows the average treatment effect of treatment assignment on separation likelihood in the first month, the first three months, and the first six months of employment.[5]

Assignment to a treated store does not significantly affect separations over the first three or six months (Columns (2) and (3)). However, assignment to treatment *increased* the probability of separation during the first month of employment. The probability for a separation is about 2 percentage points higher for employees starting in a treated store compared to employees starting in a control store. This effect is large as it increases the unconditional separation rate in control stores, 3.40%, by more than half: 5.56% separate in a treated store in the first month of employment.[6]

To estimate the causal effect of the *Welcome Intervention* on the turnover of newly hired employees who are actually treated, we next examine compliance rates. We measure compliance using individual receipts from stores showing the items booked for the onboarding intervention of the new employee (i.e. the actual food consumed at the event).[7]

[5]According to collective bargaining agreements, employees can be dismissed during the probationary period, which is usually three months, with two weeks' notice, and within the first six months with 30 days' notice.

[6]We repeat this analysis with additional controls in Table A3 in the Appendix. The results remain very similar.

[7]While these receipts always prove implementation, a lack of a receipt sent can indicate either non-compliance or 'hidden' compliance, i.e. the intervention was implemented but store managers did not send the receipt. This proxy might hence underestimate treatment take-up. We will also use two further proxies for compliance: First, we have received monthly ledger entries listing all items booked in a given store for the intervention. While these lists contain all bookings for the intervention, they also list items mistakenly booked on this ledger. This proxy hence probably overestimates implementation rates. Second, new employees are asked to describe their first working day in the survey sent to them after one week of employment. For those who have answered the survey, we can infer from this answer whether they have actually received the *Welcome Intervention*. Due to selection into survey and open text form of the answer, this proxy is the noisiest measure of compliance.

Table 1: Average Treatment Effect

| | (1) <= 1 Month | (2) <= 3 Months | (3) <= 6 Months |
|---|---|---|---|
| Treatment assignment | 0.0207* | 0.00529 | 0.00976 |
| | (0.0107) | (0.0180) | (0.0216) |
| Observations | 2371 | 2371 | 2371 |
| R-squared | 0.0224 | 0.0280 | 0.0338 |
| Rand. Strata | x | x | x |
| Region x Month | x | x | x |

Note: The table reports OLS regressions. Clustered standard errors at the district level in parentheses. All specifications control for randomization strata, region interacted with the month of entry.* for p < 0.10, ** for p < 0.05 and *** for p < 0.01.

Table A4 in the Appendix reports first stage regressions of the individual receipts sent. Starting in a treated store significantly increases the probability of treatment take-up as measured by the individual receipts. To restrict ourselves to the most conservative yet accurate measure of compliance, we will run IV regressions with the individual receipts sent instrumented by treatment assignment.

Table 2: Local Average Treatment Effect

| | (1) <= 1 Month | (2) <= 3 Months | (3) <= 6 Months |
|---|---|---|---|
| Treatment Take-up | 0.0767** | 0.0196 | 0.0361 |
| | (0.0313) | (0.0616) | (0.0730) |
| Observations | 2371 | 2371 | 2371 |
| R-squared | 0.00191 | 0.0277 | 0.0329 |
| Rand. Strata | x | x | x |
| Region x Month | x | x | x |

Note: The table reports IV regressions. All specifications control for randomization strata, region interacted with the month of entry.* for p < 0.10, ** for p < 0.05 and *** for p < 0.01.

Table 2 reports the IV estimates. Mirroring the ITT results, we do not observe significant effects of the *Welcome Intervention* on separation probability during the first three or six months (see Columns (2) and (3) in Table 2). However, being actually treated significantly *increases* the probability of separation in the first month of employment (Column (1)). These results show that the *Welcome Intervention* does not reduce turnover in this setting but increases separation rates in the first month of employment. This pattern is consistent with the *match-revelation channel* – onboarding accelerates the resolution of uncertainty about match quality thus increasing turnover – outweighs any effect of the *attachment channel* which would have reduced turnover due to increased productivity and employee identification.

### 5.1 Quits and Dismissals

To better understand the mechanism, we distinguish between employee-initiated separations (quits) and employer-initiated separations (dismissals).

Table 3: Quits

| | (1)<br><= 1 Month<br>(Quit) | (2)<br><= 3 Month<br>(Quit) | (3)<br><= 6 Month<br>(Quit) |
|---|---|---|---|
| Treatment Take-up | 0.0606** | 0.0294 | 0.0336 |
| | (0.0304) | (0.0485) | (0.0568) |
| Observations | 2371 | 2371 | 2371 |
| R-squared | 0.00543 | 0.0241 | 0.0285 |
| Rand. Strata | x | x | x |
| Region x Month | x | x | x |

Note: The table reports IV regressions. All specifications control for randomization strata, region interacted with the month of entry.* for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.

Table 3 provides IV estimates for employee-initiated quits, and Table 4 those for employer-initiated dismissals. Both quit and dismissal rates increase significantly in the first month for treated employees. At longer horizons (three and six months), we do not observe statistically significant effects on quits or dismissals. Together, this pattern supports the interpretation that the intervention accelerates learning about match quality for employer and employees and shifts separations forward rather than increasing separations overall.

Table 4: Dismissals

| | (1)<br><= 1 Month<br>(Dismiss) | (2)<br><= 3 Month<br>(Dismiss) | (3)<br><= 6 Month<br>(Dismiss) |
|---|---|---|---|
| Treatment Take-up | 0.0160** | -0.00981 | 0.00255 |
| | (0.00721) | (0.0438) | (0.0601) |
| Observations | 2371 | 2371 | 2371 |
| R-squared | 0.00609 | 0.0253 | 0.0234 |
| Rand. Strata | x | x | x |
| Region x Month | x | x | x |

Note: The table reports IV regressions. All specifications control for randomization strata, region interacted with the month of entry.* for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.

To understand whether the intervention indeed mainly facilitates a better selection of high-performing workers as well as workers who better fit into the existing team instead of increasing productivity and worker well-being, we will study both survey and further admin data.

### 5.2 Identification with the Firm

Newly hired employees received an onboarding survey after one week and three months of employment. Participation rate is higher in the first survey with 616 employees having started the survey (25.98%) while only 407 employees provided answers in the second survey (17.17%).[8] This decrease in number of answers is to some extent mechanical as employees who are no longer employed after three months do not receive the second survey.

Our main measure of organizational identification is the Inclusion of Other in the Self (IOS) scale developed by Aron, Aron and Smollan (1992) and used and evaluated by for example Foulk et al. (2020) and Gächter, Starmer and Tufano (2015). The scale ranges from 0 to 100 with 100 indicating a full overlap of circles. The 'other' circle refers in our context to the organization itself.[9]

Table 5: Identification with the Firm

| | Survey 1 | Survey 2 | Survey 1 | Survey 1 |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatment Take-up | -9.813 | 9.430 | 7.368 | -29.52* |
| | (9.718) | (10.45) | (12.77) | (15.16) |
| Sample | All | All | Stayers | Separations |
| Observations | 590 | 401 | 331 | 259 |
| R-squared | 0.0798 | 0.116 | 0.133 | 0.120 |
| Rand. Strata | x | x | x | x |
| Region x Month | x | x | x | x |

Note: The table reports IV regressions. Robust standard errors in parentheses. All specifications control for randomization strata, region interacted with the month of entry.* for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.

Table 5 reports the results of having experienced the *Welcome Intervention* on the feeling of identification with the firm as measured in the first survey (Column (1)) and the second survey (Column (2)). Treated employees report slightly lower identification in wave 1 and slightly higher identification in wave 2, but these differences are not statistically significant. The positive effect in the second survey could reflect selection (better matches remain) or changes of identification over time. When restricting the wave-1 analysis to employees who remain employed, the coefficient is again positive: those who later stay employed also feel more identified from the beginning (n.s.). But those employees with a separation report significantly lower identification with the firm from the beginning. While statistical power for survey outcomes is limited, this pattern is consistent with the intervention operating primarily through faster sorting: workers who are a poorer match in terms of identification are more likely to separate.

[8] There is no significant treatment specific attrition in the survey. 25.96% of treated employees took part in survey 1, 17.44% in survey 2. 26.07% of the employees in control districts participated in survey 1, 18.57% in survey 2.

[9] Employees can move the "other" circle on the self circle to the position they want. After 5 months an external survey provider took over surveying employees. They could only implement the question using a slider. The slider was still continuous from 0 to 100, and had no default choice but clearly indicated 0%, 50% and 100% overlap.

### 5.3 Working Hours

Our framework predicts that productivity of the employees is affected through both channels, as onboarding can faciliate direct human capital acquisition but also due to the selection effect driven by early dismissals of bad matches leading to higher expected productivity of the retained employees. To test this we can assess the effect of the treatment on actual engagement and performance of the employees exhibited at work. One measure of productivity that we can access are the weekly overtime hours exerted by individual employees. This overtime work is voluntary and provides a measure of job engagement above contractual working hours. Table 6 shows that treated employees indeed provide significantly more weekly overtime hours starting already in the first month of employment, see Column (1). This effect of about 1 hour more overtime work per week is persistent as the effect remains of similar size for the first three as well as first six months of employment.

Table 6: Average weekly overtime hours

| | Average Overtime Work in | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Month 1 | Months 1–3 | Months 1–6 |
| Treatment | 1.150* | 1.414*** | 1.323*** |
| Take-up | (0.606) | (0.468) | (0.422) |
| Sample | All | All | All |
| Observations | 2225 | 2345 | 2346 |
| R-squared | 0.0617 | 0.0556 | 0.0628 |
| Rand. Strata | x | x | x |
| Region x Month | x | x | x |

Note: The table reports IV regressions. Robust standard errors in parentheses. All specifications control for randomization strata, region interacted with the month of entry.* for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.

To understand whether the positive effect of the intervention on overtime work is driven by stayers, we split our sample again to those who stay within the experimental period and those who eventually separate. In contrast to the theoretical prediction of our framework, both stayers *and movers* exert more overtime work in their first six months of employment (Table 7). This suggests that the treatment also helped to foster employee motivation.

Table 7: Average weekly overtime hours (Separations and Stayers)

| | Average Overtime Work in | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Month 1 | Months 1–6 | Month 1 | Months 1–6 |
| Treatment | 0.902 | 1.122** | 1.329 | 1.515** |
| Take-up | (0.796) | (0.524) | (0.913) | (0.616) |
| Sample | Stayers | Stayers | Separations | Separations |
| Observations | 1048 | 1089 | 1177 | 1257 |
| R-squared | 0.0934 | 0.110 | 0.0565 | 0.0580 |
| Rand. Strata | x | x | x | x |
| Region x Month | x | x | x | x |

Note: The table reports IV regressions. Robust standard errors in parentheses. All specifications control for randomization strata, region interacted with the month of entry.* for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.

## 6 Conclusion

Firms routinely invest in onboarding practices intended to build attachment and reduce early exits. At the same time, the first weeks of employment are a period of intensive learning for both sides: employers and workers update beliefs about match quality as they observe performance and experience working conditions. Onboarding may therefore reduce turnover by strengthening attachment, but it may also increase early turnover by accelerating the revelation of match quality. Whether onboarding ultimately reduces or increases early separations is an empirical question.

In a large-scale field-experiment, we randomly introduce a *Welcome Intervention* fostering information transmission between the newly hired employees and the team. The evidence shows that the treatment increases turnover in the first month of employment with little effect at longer horizons. The concentration of effects in the first month and the increases in both quits and dismissals suggest that the intervention accelerates the resolution of uncertainty about match quality rather than mechanically increasing attachment. Complementary survey evidence (with limited power) suggests that eventual leavers report lower organizational identification early in the employment relationship. But we also find that treated employees provide more overtime work hours irrespective of whether they separate or not indicating that the treatment also helped to foster employee motivation. Taken together, the intervention appears to shift separations forward and improve selection among retained workers.

These findings have two broader implications. First, onboarding should not be evaluated solely by whether it reduces short-run turnover: an increase in early separations can reflect faster sorting that improves the average quality of retained matches. Second, organizational practices can shape the matching process by changing the information environment at entry. For managers, this implies that onboarding design should be guided by the primary objective—retention through attachment versus early screening through faster match revelation—and evaluated using both retention timing and measures of match quality among stayers.

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# A Appendix

Table A1: Balance Table (Individual Level)

| Variable | (1) All | (2) Control | (3) Treated | (4) Difference (3)-(2) |
|---|---|---|---|---|
| Female | 0.614 | 0.594 | 0.632 | 0.038 |
| | (0.487) | (0.491) | (0.482) | (0.030) |
| Sales Staff | 0.665 | 0.665 | 0.664 | -0.001 |
| | (0.472) | (0.472) | (0.472) | (0.034) |
| Apprentices | 0.136 | 0.139 | 0.133 | -0.006 |
| | (0.343) | (0.346) | (0.339) | (0.017) |
| Sales Support | 0.132 | 0.128 | 0.136 | 0.007 |
| | (0.339) | (0.335) | (0.343) | (0.024) |
| Cleaning Staff | 0.059 | 0.057 | 0.061 | 0.005 |
| | (0.236) | (0.232) | (0.240) | (0.020) |
| Other | 0.008 | 0.010 | 0.006 | -0.005 |
| | (0.089) | (0.102) | (0.075) | (0.006) |
| Month hired | 5.267 | 5.117 | 5.408 | 0.291 |
| | (3.676) | (3.651) | (3.695) | (0.209) |
| Region 1 | 0.571 | 0.567 | 0.575 | 0.008 |
| | (0.495) | (0.496) | (0.495) | (0.108) |
| Working Hours (% of Full-time Position) | 63.692 (23.312) | 63.683 (23.363) | 63.700 (23.274) | 0.017 (1.632) |
| Observations | 2,371 | 1,148 | 1,223 | 2,371 |

Note: Values in parentheses indicate standard deviations (Columns (1)-(3)) and standard errors (Column (4)). Standard errors are clustered at the district level. * for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.

Table A2: Balance Table (Store Level)

| Variable | (1) All | (2) Control | (3) Treated | (4) Difference (3)-(2) |
|---|---|---|---|---|
| Region 1 | 0.537 | 0.538 | 0.536 | -0.002 |
| | (0.499) | (0.499) | (0.499) | (0.102) |
| Store size | 771.423 | 771.154 | 771.696 | 0.543 |
| | (140.417) | (135.838) | (145.111) | (13.237) |
| Share Women | 0.790 | 0.784 | 0.796 | 0.012 |
| | (0.147) | (0.143) | (0.151) | (0.021) |
| FTE | 0.684 | 0.688 | 0.681 | -0.007 |
| (pre 12 months) | (0.065) | (0.063) | (0.067) | (0.010) |
| Sick days | -0.000 | -0.035 | 0.036 | 0.071 |
| (pre 12 months) | (1.000) | (1.024) | (0.975) | (0.078) |
| Monthly Turnover | 2.252 | 2.272 | 2.233 | -0.039 |
| (pre 12 months) | (1.645) | (1.586) | (1.704) | (0.148) |
| Revenue | 0.000 | -0.012 | 0.012 | 0.023 |
| (pre 12 months, standardized) | (1.000) | (0.965) | (1.036) | (0.120) |
| Average receipt | -0.000 | 0.027 | -0.028 | -0.055 |
| (pre 12 months, standardized) | (1.000) | (1.012) | (0.988) | (0.127) |
| Revenue/productive working hour | 0.000 | 0.050 | -0.051 | -0.100 |
| (pre 12 months, standardized) | (1.000) | (0.997) | (1.002) | (0.102) |
| Observations | 730 | 368 | 362 | 730 |

Note: Values in parentheses indicate standard deviations (Columns (1)-(3)) and standard errors (Column (4)). Standard errors are clustered at the district level. * for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.

Table A3: Average Treatment Effect

| | (1) <= 1 Month | (2) <= 3 Months | (3) <= 6 Months |
|---|---|---|---|
| Treatment assignment | 0.0187* | -0.0000404 | 0.0105 |
| | (0.0111) | (0.0176) | (0.0199) |
| Female | -0.00404 | -0.0174 | -0.0936*** |
| | (0.00876) | (0.0210) | (0.0195) |
| Observations | 2365 | 2365 | 2365 |
| R-squared | 0.0339 | 0.0434 | 0.0599 |
| Rand. Strata | x | x | x |
| Region x Month | x | x | x |
| Controls | x | x | x |

Note: The table reports OLS regressions. Clustered standard errors at the district level in parentheses. All specifications control for randomization strata, region interacted with the month of entry. We further add these controls: gender, female share in store, job type, contractual working hours (share of full-time position), store size, opening date, and pre-experimental FTE, sick leave, turnover, and performance of the store. * for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.

Table A4: First stage regression

| | (1)<br>Treatment Take-up |
|---|---|
| Treatment assignment | 0.270*** |
| | (0.0126) |
| Observations | 2371 |
| R-squared | 0.228 |
| Rand. Strata | x |
| Region x Month | x |

Note: The table reports OLS regressions serving as the first stage of our IV regressions. All specifications control for randomization strata, region interacted with the month of entry.* for $p < 0.10$, ** for $p < 0.05$ and *** for $p < 0.01$.