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The Impact of Relative Performance Information on Employees' Persistence in Voluntary Training

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Abstract. Firms increasingly rely on voluntary training programs to enhance employees' skills, yet such initiatives often suffer from high dropout rates. We investigate whether providing employees with information about their relative training performance affects their persistence in voluntary training. Social comparison theory suggests that relative performance information (RPI) addresses employees' inherent need to assess their ability relative to others and maintain a positive self-image by improving their standing. However, whether RPI on training performance is sufficient to increase persistence in voluntary training in the field is unclear. Besides particularly low barriers for dropping out, voluntary training is typically noncore to employees' primary tasks and often lacks a meaningful comparison group because participants come from a large, heterogeneous, and largely anonymous pool of employees. This calls into question whether the conditions required for social comparison mechanisms are sufficiently met for RPI to take effect. We study this question using a field experiment in a firm offering a voluntary online training program. During the training, employees either receive RPI about their training performance relative to others or only their own performance score. We show that RPI significantly increases employees' persistence in the training. Consistent with our proposed theoretical mechanism, this effect is particularly pronounced for more competitive individuals. We further find that the likelihood of completing the training is higher and people return to the training quicker if they receive RPI. Our study highlights a simple and cost-effective way to enhance the effectiveness of voluntary training.

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Keywords: relative performance information • social comparison theory • field experiment • training

1. Introduction

Developing human capital within organizations is a key success driver for many firms (Ployhart et al. 2014), and many firms invest substantial financial resources in employee training programs (Sitzmann and Weinhardt 2018). These programs may be directly related to daily tasks, but they also often address more general subjects like business concepts, self-management, information technology (IT), or health and safety. Because mandatory training can seem like an unwanted burden to many employees and undermine their openness to learning (Mathieu et al. 1992, Salamon et al. 2021), firms often leave it up to employees to both initiate and complete such training. Although this voluntary approach has clear benefits, it also leads to a common problem: many employees begin but do not complete the training. This issue is particularly pronounced in online and self-directed formats, where the barrier to dropping out is very low, but it also often arises in in-person training consisting of multiple sessions or modules where employees simply do not return to the next session or module. The resulting incomplete participation leads to both direct (e.g., licenses) and indirect costs (e.g., the missed opportunity to enhance skills; Sitzmann and Ely 2010) for organizations. Against this backdrop, we draw on social comparison theory and provide field evidence for how the provision of relative performance information (RPI) about employees' training performance during a voluntary training program influences their likelihood of continuing and completing the training rather than dropping out prematurely.

RPI—information about an individual's standing relative to peers engaged in the same task without financial implications—has been shown in laboratory

settings to be a powerful tool for guiding employee behavior (Hannan et al. 2008, 2013; Tafkov 2013; Loftus and Tanlu 2018; Knauer et al. 2021). Evidence from within organizations (Eyring and Narayanan 2018, Casas-Arce et al. 2023, Kohler et al. 2024) is sparser, with studies demonstrating that not all findings necessarily translate into a noisy field setting. Focusing on voluntary training, we investigate the effect of providing RPI in an environment that can be considered particularly unfavorable for RPI to influence behavior. There are at least two factors that provide tension for whether RPI about employees' training performance increases their persistence in voluntary training. First, RPI is most effective when individuals view the task as crucial and achieving high performance on that task as important (Tafkov 2013). By contrast, voluntary training in practice is often peripheral to employees' core responsibilities and low effort (or even dropping out) carries no negative consequences, likely limiting the extent to which they view it as important to outperform others in such a task (Baldwin and Magjuka 1991). Second, RPI works best when individuals are presented with a set of similar others to compare with (Tafkov 2013). The broad and heterogeneous participant pool typically found in company-wide training initiatives, however, may dilute the salience of such a comparison group with similar others. It is thus theoretically nontrivial and empirically unexplored whether the positive motivational effects reported in laboratory studies hold in a noisy business practice context such as voluntary training.

Drawing from social comparison theory, we argue that providing RPI on test scores throughout a voluntary training increases persistence in it because it speaks to the human need for assessing and improving one's abilities in relation to others (Smith 2000, Suls and Wheeler 2000, Besley and Ghatak 2008). The level of one's own ability often remains unclear to employees without such a social comparison (Ryan and Deci 2000, Hannan et al. 2008, Tafkov 2013). We argue that RPI provides employees with a better sense of their relative standing throughout the training and encourages them to voluntarily *return* to the next training module. This is because participating in another round of a training with another round of relative comparisons gives employees a less noisy measure of their ability (i.e., their initial relative assessment could be strongly affected by the good or bad luck of themselves and others). Moreover, engaging in another round of training and comparison provides employees with a recurring opportunity to defend or improve their performance relative to others. This helps them gain a positive self-image. We thus expect in our first hypothesis that the provision of RPI about employees' training performance throughout a voluntary training increases their persistence in it.[1]

Although the need for social comparison is inherent in almost all humans (Suls and Wheeler 2000), prior studies show that individuals differ in the extent to which they engage in and react to such comparisons. One character trait that likely matters in this context is the desire to outperform others (often referred to as competitiveness). Consistent with our theoretical reasoning that RPI increases persistence through social comparison processes, we predict in our second hypothesis that this effect is stronger for more competitive individuals.

To test our hypotheses, we conduct a field experiment with 79 employees participating in an online training on agile project management and OKR (Objectives and Key Results) systems at a medium-sized manufacturing company. Participation is voluntary, and there are no incentives attached to taking, completing, or performing well on this training. The training is fully self-directed (i.e., individuals can choose when and how fast to complete it) and divided into four consecutive modules, each concluding with a test. After completing the test, participants receive information about their personal test score on that module and can proceed to the next module. We randomly assign participants to one of two treatments, where we manipulate the performance information employees receive about their performance on each test module. In the treatment with RPI, employees receive their personal test score and the performance deciles of other employees that completed this module before them. Thus, they can directly compare their own training performance to the performance of others. In the control treatment (No RPI), employees receive only their personal test score, but no information about the other employees. In this condition it therefore remains unknown to participants how they performed relative to others.

Consistent with our first prediction, we find a significant positive effect of RPI on persistence in the training. In fact, our results show that when presented with RPI about their training performance, participants' odds of returning to the training and taking the subsequent training module's test are 4.35 times higher than those for participants who receive their own performance information only. Ultimately, 17.5% of employees in the control group complete the training by finishing all four modules successfully, while the completion rate in the RPI condition is 66.7%. This substantial difference underscores the positive impact of RPI on the likelihood of persisting in and eventually completing the training. Moreover, employees are on average around 24 days faster in completing the training with RPI than without it. Consistent with theory on social comparison processes, we also find support for our second prediction suggesting that the effect of RPI about the training performance is stronger for individuals with a stronger desire to outperform others. Our results remain robust

when controlling for potential confounding factors. Moreover, in a follow-up analysis we find no evidence that an individual's reaction to the provision of RPI depends on their absolute or relative training performance on the prior module.

Our work contributes to the literature and practice in several ways. First, we contribute to the accounting literature on employee skill and competence acquisition. Although a large stream of the management accounting literature examines how management control systems are used to motivate effort or improve decision making on employees' daily tasks (Bonner and Sprinkle 2002, Malmi and Brown 2008), only a few studies investigate how control choices foster employees' learning and the acquisition of new skills and competences (Kelly et al. 2021, Arnold et al. 2025). Kelly et al. (2021) show how a gamified approach that introduces a slot machine as a reward to training can have benefits on employees' training motivation. However, they also find that the effect is quite short-lived and deters employees who are more skeptical about gambling. Arnold et al. (2025) show in the laboratory how RPI on employees' main tasks can influence effort in training that provides useful skills for their immediate main task. We extend this stream of literature by demonstrating in the field how the provision of RPI on the *training itself* increases persistence in it. Providing RPI on the training seems to be a comparably simple option for firms aiming to reap the benefits from their investments in voluntary training programs. Moreover, this form of RPI can also be applied to training aimed at skills development for employees' *future* roles (e.g., leadership, general management skills, or personal development) and are thus not directly tied to current performance.

Second, we contribute to the literature on RPI in accounting, which has produced a solid body of laboratory evidence on the impact of RPI yet still lacks a comparable level of robust insights from the field. Some exceptions are Eyring and Narayanan (2018), Casas-Arce et al. (2023), and Kohler et al. (2024) who examine RPI effects in the field, but who also show that not all findings generated in the laboratory necessarily hold in more complex field settings. Therefore, we build on theory corroborated in laboratory studies to identify "causal relationships when participants face real-world stakes" (Lourenço 2019, p. 4). For social comparison mechanisms to thrive, individuals need to both personally *care about performance on the task* and have a set of *similar others to compare with* (Tafkov 2013). Both preconditions are particularly limited in voluntary training settings. Nevertheless, we find evidence that RPI generates positive effects even in a setting as unfavorable for RPI as voluntary training. In fact, speaking to employees' desire to get a better signal of their relative ability and to their constant drive upwards, RPI makes them actively seek out and return to another round of comparison. This provides evidence for the strong role of RPI in shaping employee behavior even in noisy real-world settings.

Finally, we add nuance to the RPI literature by examining individual-level heterogeneity in responses to RPI. Although most studies on RPI abstract away from personality, a small yet growing line of research investigates such moderators (Wang 2017, Slapničar et al. 2022, Erickson et al. 2025). Focusing on competitiveness—a core trait underlying social comparison—we show that the positive effect of RPI on persistence in voluntary training is stronger for more competitive individuals. As personality traits tend to correlate with certain functions or industries (Graziano et al. 1997, Ghosh and Waldman 2010), firms need to be aware that RPI may be a better fit in some contexts than in others. In contrast, we find no evidence that responses to RPI depend on individuals' prior performance on the training. This speaks to a broad applicability of RPI to boost persistence in voluntary training and alleviates concerns that the provision of RPI might discourage particularly high- or low-performing employees from further engaging with the training.

2. Theoretical Background and Hypothesis Development

Using management control systems, firms try to ensure that employees work toward the overall firm objectives (Merchant and Van der Stede 2017). Next to guiding employee behavior by holding them accountable for results, monitoring their actions, or creating a strong organizational culture, a central control mechanism lies in ensuring that employees possess the necessary skills to perform their tasks in the first place. This is often done through training (i.e., planned initiatives aimed at assisting employees in acquiring job-relevant knowledge and skills; Brown and Sitzmann 2011).

Prior research shows that training positively impacts employee skills and job-related behaviors and leads to increased organizational productivity, profits, and innovation (Noe et al. 2014). That said, motivating individuals to begin and continue with such training can be challenging. Recent field evidence demonstrates that firms struggle to attract, motivate, and retain the right employees in voluntary human capital development programs, which severely undermines their effectiveness (Sandvik et al. 2026). In addition, such issues appear to only further increase as training gets shifted to the digital realm. Self-directed online training sessions in 2023 outnumbered traditional classroom training sessions in midsize and large organizations based in the United States (Freifeld 2023). In contrast to instructor-led training, where employees dedicate a specific block of time to learning and attending the

training as a captive audience, such training allows employees to decide when and where to complete it. Although this provides much flexibility and freedom for the learner, it also requires learners' self-discipline to complete their training and balance it with other demands (Jones 2013). Anecdotal evidence and prior studies suggest that learning attrition rates are extremely high in this form of training. But also in regular in-person programs, individuals often drop out and do not show up for the next module or session (Sitzmann and Ely 2010, Sitzmann 2012, Sandvik et al. 2026). In other words, employees often initiate voluntary training but never finish it.

Some companies counteract this problem with monetary incentives, where employees must pay fines when they do not complete a training or receive bonuses when they complete it. That said, many firms deliberately do not want to put any financial or disciplinary sanction on the training engagement of employees but keep the administrative burden for the firm low and minimize participants' barriers to signing up (Sung and Choi 2018). Coupled with the insight that the mandatory nature of a training can undermine employees' openness to learning (Mathieu et al. 1992, Salamon et al. 2021), these firms instead look for low-cost alternatives that encourage individuals to pursue and engage in training voluntarily.

In this vein, two recent studies in the literature on management accounting and control have begun to examine how companies can foster employees' acquisition of new skills and competences through voluntary training. Kelly et al. (2021) find that providing employees incentives for participation in voluntary online training through earning tokens for a virtual slot machine increases their interest and performance in it. However, the effect disappears quite quickly once the novelty of the slot machine wears off. Perhaps even more importantly, introducing the slot machine even deters a subgroup of participants not interested in gambling from completing training modules. Arnold et al. (2025) show that providing individuals with RPI on performance in their core job tasks leads them to participate in training that improves their job-related skills as long as there is no tradeoff between the training and current work performance (e.g., if the training happens outside regular work hours). When there is a tradeoff between the training and current performance, they find that RPI on the core task decreases the likelihood of taking a training. Although potentially a powerful way to promote employees' voluntary skill development, introducing RPI on the core task is often quite costly to implement and—given its central role in incentives and promotion decisions—can lead to unintended consequences in an employee's core job (e.g., sabotage behavior; Hartmann and Schreck 2018). Moreover, it also requires that employees see a relatively immediate benefit from training in their job-related skills, and as such, may be less useful for more general training that develops skills needed for future tasks.

Complementing this recent literature, we suggest providing RPI *about the voluntary training performance* as an alternative, effective way to promote persistence in voluntary training that can overcome some of the shortcomings of these other solutions. Building on social comparison theory, we argue that RPI about training performance speaks to individuals' inherent desire to assess their competence relative to others and recurringly maintain a positive self-image. Given that social comparison is a universal human need, we expect it to be more sustainable and broadly appealing than the gamification approaches explored by Kelly et al. (2021). In addition, because we confine RPI to the training activity, it should be cheaper to implement, interfere less with employee behavior on the core job than job-related RPI, and offer the opportunity to foster persistence in training programs that are less useful for the current job but valuable for future tasks.

In the next sections, we discuss the literature on RPI, its roots in social comparison theory, and our hypotheses for how it contributes to employees' persistence in voluntary training activities.

2.1. RPI and Social Comparison Theory

With RPI, individuals receive information not only about their absolute performance on a certain task but also about their relative standing compared with others. When studying this control choice, the management accounting literature typically draws from social comparison theory (Festinger 1954). A core tenet of this theory is that individuals have an inherent need to compare themselves to others in order to evaluate their own opinions, beliefs, and—crucially for this study—abilities. When individuals seek to evaluate their ability, absolute and objective standards are often hard to come by. This creates uncertainty about one's own ability. In fact, even when other standards are present, there typically remains uncertainty about one's own ability (e.g., it is hard to evaluate whether passing a test with a score of 60% is a good or mediocre performance). To overcome this challenge, individuals begin comparing themselves to similar others (Wood 1996, Suls and Wheeler 2012). By doing so, they get a better sense of their own *relative* ability. In its long history, the literature on social comparison has particularly focused on two core aspects: who individuals choose to compare themselves to and what behavioral consequences social comparisons entail (Gerber et al. 2018).

Studies on RPI in management accounting typically address the latter aspect and zoom in on one particular behavioral outcome: The effort individuals invest in a task when presented with performance information of their peers. Studying the phenomenon in a variety of

environments—from college students (Eyring and Narayanan 2018) to emergency room physicians (Song et al. 2017) to warehouse workers in a wholesale company (Blanes i Vidal and Nossol 2011) or to employees of a retail chain (Kohler et al. 2024)—such studies frequently build on the notion that presenting individuals with RPI on their core jobs activates social comparison processes. Given that people strive to be slightly better than a similar peer (often referred to as the "unidirectional drive upwards"; Festinger 1954), they tend to respond to the provision of RPI by investing more effort to achieve results that help maintain a positive self-image (Diel et al. 2021). Put differently, the reason they are motivated is to attain an outcome that gives them a sense of pride or that avoids the feeling of shame about their own abilities.

The management accounting literature has gathered a solid body of evidence of the effort-inducing role of RPI, finding that the effect is particularly strong when individuals, for instance, work on effort-sensitive tasks, receive performance-based compensation, the RPI is public, there is a series of short-term competitions, there is an overarching long-term comparison, and the information individuals receive is more fine-grained (Smith 2000, Hannan et al. 2008, Tafkov 2013, Choi et al. 2016). Prior studies further find that its intensity also varies with other RPI-related design choices, such as the position of the reference point (Eyring and Narayanan 2018), the level of detail of the information (Kohler et al. 2024), and its frequency (Casas-Arce et al. 2017).

We build on this prior work on the motivational effects of RPI and shift the focus to employees' *persistence* in a task. In our setting, this means that we disentangle the effects of RPI on employees' effort and performance on a specific task (i.e., how well they perform on a given test) from quitting altogether (i.e., whether they choose to continue engaging with a task at all) as the most extreme form of low effort. Doing so allows us to test more directly whether providing employees with RPI on their training performance indeed helps tackle the problem of high dropout in voluntary training. In the following section, we build on social comparison theory to hypothesize why this should be the case.

2.2. Hypothesis Development

At their core, training activities are supposed to increase individuals' abilities and competences. To track the learning progress, training often involves tests that get scored. These test scores are then communicated to employees and often serve as a proxy for the employee's learning success and their related ability. However, importantly, such test scores are often noisy and might not capture an employee's underlying ability adequately. For example, a high test score can be the result of high ability but also of extraordinary circumstances, such as luck, or an exceptionally easy test. Likewise, bad performance can be the result of bad luck or difficult tasks. Thus, a single test score does not provide clear guidance about an employee's ability and competence. This leaves employees with uncertainty about their ability and learning progress.

We suggest that providing RPI on employees' training performance can address this problem. Specifically, social comparison theory maintains that when faced with uncertainty about their ability, individuals turn to comparing their own performance with that of similar others (Festinger 1954). However, we suggest that RPI not only helps employees better assess their own ability relative to others in a one-shot situation but, more importantly, also encourages them to voluntarily *return* to the next voluntary training session and thereby increase their persistence in the training.

We base our reasoning on two arguments: First, performance information at a single point in time (e.g., on the first training assessment) is an inherently noisy measure. Although the relative comparison of one's own performance to others already provides a better ability signal than simply receiving one's own test score, comparisons in a single-shot situation could still be heavily influenced by idiosyncratic good or bad luck. Returning to the training and engaging in an additional relative comparison therefore provides individuals an opportunity to get a more accurate assessment of their competence (i.e., bad performers can check whether they were simply unlucky last time; good performers can confirm that their high ability was not just an artifact of good luck).

Second, returning to the training gives individuals an opportunity to maintain or improve their relative position. Prior literature documents that people tend to care about their performance relative to others—even in casual settings (e.g., leisurely sports activities or gaming; Frederick et al. 1994, Esteves et al. 2021). We expect RPI to foster a friendly competition that brings individuals back to the training and improves their relative standing. In pursuit of a positive self-image, returning to the training either helps successful individuals obtain a recurring confirmation of being a good (or even better) performer, or it gives lower-performing individuals the chance to rectify potential feelings of incompetence.[2]

We thus expect that RPI about employees' training performance speaks to their need for assessing and improving their ability relative to others by providing them a more accurate benchmark for their ability and instilling a friendly competition that motivates them to come back and improve. This leads to the following hypothesis.

Hypothesis 1. *Employees' voluntary training persistence is greater for those receiving RPI about training performance than for those who do not receive such RPI.*

To provide further insights into the underlying process for Hypothesis 1, we focus in Hypothesis 2 on the

potential variation of that effect depending on the individual's personality. Specifically, although the tendency to seek comparison with others is considered "a core element of human conduct" (Suls et al. 2002, p. 159), social comparison theory also maintains that individuals differ in the *extent* to which they choose to engage in social comparison and react to its outcomes (see Gerber et al. (2018) for a brief summary). This view is mirrored by recent studies on RPI that find that character traits like a high need for cognition or broader personality profiles like a set of "dark triad" traits influence behavior like effort provision or sabotage when an individual is presented with RPI (Wang 2017, Erickson et al. 2025).

One individual factor related to personality and often linked to social comparison is the extent to which a person desires to excel in comparison with others (i.e., competitiveness; Garcia et al. 2013, Newby and Klein 2014). We suggest that a person's tendency to be competitive should particularly influence the effect of RPI on their persistence with a voluntary training. Specifically, more competitive individuals likely place a higher value on both accurate performance information of their peers and the opportunity to improve their relative position over time. Thus, stronger competitiveness should strengthen the effect of RPI about employees' training performance on their persistence in voluntary training. We formulate the following hypothesis.

Hypothesis 2. *The effect of receiving RPI about training performance on voluntary training persistence increases with employee competitiveness.*

3. Field Setting and Experimental Design

We collect data from a medium-sized company (referred to as ElectroCo throughout this study) that specializes in developing and manufacturing decentralized automation technology. ElectroCo maintains a global workforce of more than 3,100 individuals. Approximately 1,700 of them work in Germany. The company maintains several production facilities, administrative functions and offices, and sales offices spread around the world in more than 46 countries.

ElectroCo's central human resources development department offers a large variety of voluntary training courses, all of which are available on an online learning platform, which has been in place for several years. These courses focus, among other things, on topics like product training or general management skills. Although primarily nonproduction workers use the platform to take this form of voluntary training, it is open to all employees of the company. Company policies support ongoing training during working hours, and there is no set development plan mandating specific training courses or qualifications that employees must attain. Instead, employees are free to choose the training courses they wish to attend based on their personal interests and career development goals, and they can do so without worrying about negative consequences for not completing such training. By providing voluntary training opportunities, the company aims to cater to individual employee needs and qualifications.

3.1. Agile Project Management and OKR Training Program

In our study, we analyze a voluntary training program focusing on agile project management and OKR that the company introduced in 2022/2023. Because it is offered on the company's learning platform, all employees are eligible to take this course. Like the other training courses provided on the learning platform, there are no consequences for completing the training, meaning that successful completion of the OKR training does not automatically qualify someone for promotions or bonuses, and no certificate is issued. In fact, ElectroCo neither assesses the learning outcome of individual employees nor communicates the results of the OKR training to the respective supervisors. Put differently, supervisors are neither aware of their employees' performance scores nor does the completion of the training influence their performance evaluations and/or promotion decisions. Thus, similar to other training courses offered by this company, both the barrier to signing up for the training *and* the accountability for completing it are low.

The OKR training program consists of four sequential modules. The first module covers the fundamentals of agile project management. The subsequent three modules focus on understanding and applying the OKR method. The online learning materials consist of written content, diagrams, and videos. The modules are structured in such a way that—depending on their previous knowledge—participants need between 30 and 60 minutes to go through and study the content. Each module ends with a test. The number of questions and maximum points achievable vary for each test module: The tests for the first and second modules have six questions each (with a maximum of 31 and 21 points, respectively). The third and fourth modules include eight questions each (with a maximum of 30 and 50 points, respectively). The tests comprise various question formats, including single-choice, multiple-choice, fill-in-the-blank, and matching questions. The score for each question can range from 1 to 12 points. Partially correct answers receive a partial point allocation.

To progress from one module to the next, employees must pass the test from the previous module first. If they do not reach the required passing threshold of 80%, they can repeat the test an unlimited number of times. Employees can even repeat the test after they have passed it to improve their score if desired. Because

of the technical nature of the learning platform, we could collect data from the module test, but no data from the learning module. Therefore, we cannot draw any conclusions about how long employees spend looking at the learning content but only focus on their behavior with respect to the tests.

After employees take the test for a specific module, the computer program checks the answers and generates a performance report. This performance report is approved by one of the coauthors of this paper (who is independent and not affiliated with the organization) before being e-mailed within two hours to the participating employee from a platform account via email.[3] The account sending the message was named in a manner to appear like an automatically generated message by the learning platform.

All employees are aware of this procedure and know that they receive the outcome of their test at the latest two hours after completing the test. In this email, participants receive their performance report informing them about their own performance (and in the RPI treatment also about their relative performance) and whether they are eligible to move to the next module. If they are eligible, employees can log in to the platform to continue learning.

3.2. Treatments

In a 1×2 design, we manipulate between participants the information employees receive about their relative training performance upon completion of each test module and randomly assign participants to either the treatment (RPI) or the control group (No RPI). Importantly, employees stay in the same group (RPI versus No RPI) throughout the entire experiment (i.e., all four modules).

In the RPI group, the email employees receive about their test contains information about the employee's own performance on that module (i.e., in percentage of the maximum points for that module test) and how their performance compares to the reference group. The reference group's performance is visualized in a chart using deciles (see appendix, RPI Screen). The employee's performance is then assigned to the respective decile. In case of a tie between multiple deciles, the employee is assigned to the upper decile. Although absolute values are not provided for the other deciles, the scale and length of the bars indicate the magnitude of the difference.[4]

The No RPI treatment is our control group. The email in this group contains only information about an employee's personal performance on that module test (i.e., in percentage of the maximum points for that module test). To ensure that no differences arise from the presentation of the personal performance feedback, they also receive a bar chart depicting their own performance—but only with one bar representing their (absolute) score (see appendix, No RPI Screen).[5]

3.3. Additional Measures

In Hypothesis 2, we predict that the positive effect of RPI about employees' training performance on persistence in voluntary training is stronger for employees who particularly strive to outperform others. To capture this specific character trait, we included a measure for a person's *competitiveness* in a survey that all participants had to complete before starting the training. More specifically, we use three items from Newby and Klein (2014) that capture the dimension of competitiveness most applicable for the theoretical process we intend to test ("dominance," also sometimes referred to as "interpersonal competitiveness"; Griffin-Pierson 1990). On a seven-point Likert scale ranging from "strongly disagree" (1) to "strongly agree" (7), we ask participants about the extent to which they agree with the following statements: "I view almost every situation as a way to prove that I am better at things than others," "I try to be the best person in the room at almost anything," and "For as long as I can remember, I have wanted to outperform others." The Cronbach's alpha for those three items is 0.733, with all factor scores above the commonly established thresholds. We average the scores for these three items to calculate *competitiveness*. This measure has a mean score of 4.03 overall and sufficient variation (standard deviation (SD) = 1.39, minimum = 1.00, median = 4.00, maximum = 6.67) to be able to test Hypothesis 2. Moreover, when comparing the means of the no RPI group (4.06) with the RPI group (4.00), we find no significant difference in the competitiveness across groups ($t = 0.21$, $p = 0.833$).

When discussing the competitive orientation of individuals, the literature often juxtaposes motives related to outperforming others with the related, but conceptually different, motive of trying to avoid failure (e.g., approach versus avoidance motivation: Elliot and Church (1997) and Elliot (2006); ahead-seeking versus behind-averse behaviors: Roels and Su (2014)). Although—given the voluntary, inconsequential, and private nature of the task—we do not expect this alternative competitiveness trait to moderate our theorized process, we address potential concerns that it picks up variation from *competitiveness* by controlling for *avoidance* in all analyses that include *competitiveness*.[6] We capture *avoidance* by averaging participants' scores for the following three questions from Gilbert et al. (2007): "I worry about failure because it means you cannot keep up and compete with other people in your life," "If I do not strive to achieve, I will be seen as inferior to other people," and "I struggle to achieve things so that other people will not look down on me."

3.4. Implementation

ElectroCo's learning platform is characterized by the fact that employees decide which training to enroll in, when to begin, and at what pace to complete it. Similar to the other training courses offered by ElectroCo, employees were invited to participate in the training used for our field experiment via email through the company's human resources development department. The invitation to the training course was initially sent out to all employees in Germany in July 2022, with a reminder in September 2022. In March 2023, the training was then rolled out worldwide, meaning that the remaining employees also received an email invitation to participate. The training invitation contained information about why the company was providing this training, the training structure, and the expected maximum overall duration of four hours. The only difference from other training invitations was that we communicated that this particular training was accompanied by a research study and hence required a separate registration and the completion of a short questionnaire before starting with module 1. This questionnaire included our measures for *competitiveness* and *avoidance*, as well as information on the participants' demographics and motivation to engage in the training. Besides that, no further framing or explanation of the study's aim or content was provided.

Although employees could register either immediately after they received the invitation email or later, we made it clear to those signing up early that the first training module would only be accessible later, after which it would be available for exactly one year (i.e., from September 29, 2022, to October 2023). Therefore, once the experiment began, all employees who had previously registered received an email with the link to the training. This link was valid until October 2023. Employees who signed up after September 29, 2022, received this email shortly after they signed up, but it was also only valid until October 2023.[7] This procedure was consistent with other training courses offered on the learning platform, which are always available for one year. This is the one-year period in which we ran our experiment and implemented our manipulation.

Beyond the brief note that the training would be accompanied by a scientific project (and the fact that they had to fill in a short questionnaire before getting access to module 1), participants in our study were unaware of the experiment itself and our treatments, thus limiting the issue of treatment-specific selection bias (Floyd and List 2016). Given the online and self-directed nature of the training that allows for no direct human interaction, as well as the fact that the training was open to all 3,100 employees worldwide and participants were dispersed over many different departments, it is also unlikely that employees would be able to share their treatment condition with other participants.

3.5. Pilot Phase

As this particular training was newly established and our manipulation involves RPI of peers, we had to find a way to generate the performance information of a reference group. To do so, we used the first 23 employees who registered for the training in July and August 2022 to generate the data for the reference group.[8] These individuals could begin immediately with the training and did not have to wait for access to module 1 until September 29.[9]

While working on the training, this group received information on their performance like the control group (i.e., only their own performance). We then used the first attempt of the employees in the pilot group for each module to generate the peer information in form of deciles for the actual experiment. Choosing the final or best score of this group as a reference for future participants would have falsely created the impression that employees in the reference group had all passed the test on their first attempt and/or had a much higher score than they actually had in their first attempt.

A limitation of this approach is that the 23 employees who were part of the pilot phase are not randomly selected but likely highly motivated to participate and complete the training. This prevents us from including these employees in the control group and, additionally, potentially biases the RPI upwards. That said, having a highly motivated reference group creates a particularly suitable testing ground for investigating mechanisms based on social comparison theory, because it increases the likelihood that even the strongest performers will have a high reference level to aspire to and a means to satisfy their "unilateral drive upward" (Festinger 1954). Moreover, research suggests that individuals tend to respond best to targets that are challenging yet achievable (Locke and Latham 2015, Eyring and Narayanan 2018). Although having a highly motivated group as a benchmark potentially contributes to a higher challenge induced through RPI, using real data from participants' colleagues ensures that the performance level presented to them via RPI is actually also achievable. Nevertheless, we perform additional analyses where we control for ability and past performance. Our results are robust to these factors and alleviate potential concerns that a comparably more motivated reference group might bias our estimates in either direction

4. Results

In total, 156 employees registered for the OKR training, which is slightly above the typical number of employees beginning other training courses the company offers via the learning platform. The first 23 employees who registered were assigned to the pilot phase used to create the RPI information. Of the remaining 133

registered employees, 79 began the module 1 test of the training. The 79 employees who started the module 1 test therefore comprise our sample for the experiment. The average age of these employees was 40.8 years, with a mean tenure of 6.6 years in the company, and 21.5% were female. The low number of female employees is consistent with the overall low number of women working at ElectroCo.

We randomly assigned 40 employees to the No RPI treatment and 39 to the RPI treatment.[10] The training consisted of four different modules. In our main analysis, we analyze the data on the level of the individual choice to proceed with the next module. All 79 participants had to begin the first module in order to be included in our study, and they received the manipulation only after they completed the first module at least once. Thus, we focus on modules 2–4, where it was up to employees to decide whether to keep engaging with the training and take the next module. That said, to be eligible to proceed with the training and take the next module, employees had to pass the preceding module. For example, if an employee did not pass the first module and/or stopped after the first module, they were not eligible to complete the tests for module 3 or 4. In our main analysis, we therefore exclude instances where employees did not pass the previous module (either because they failed or did not even take the test) and focus on those instances where employees were eligible to take a specific module test and thus had an actual choice to proceed with the training or quit.

This results in 147 observations. Our main variable of interest is *ModuleStarted*, which takes the value of one if an employee began the respective module test and zero if not. The results in panel B of Table 1 show that in 81.6% of these observations, participants decided to begin the module. In the No RPI treatment it was in 66% of the cases, in the RPI group it was 90.4%. Moreover, only 17.5% (7 of 40) of the employees who began the training in the No RPI group completed all four modules, whereas in the RPI group it was 66.7% (26 of 39 employees). These findings provide first evidence consistent with our hypothesis that RPI increases persistence in voluntary training. Panel C shows the point at which employees dropped out of the training.

4.1. Hypothesis 1

We predict in Hypothesis 1 that providing RPI about employees' training performance throughout voluntary training increases their persistence in it. We test this prediction with a logistic regression, where *ModuleStarted* is the dependent variable and *RPI* the main independent variable (one for *RPI*, zero for no *RPI*). Moreover, as we have multiple observations per individual in our data, we cluster the standard errors on an individual level and include indicator variables for modules 3 and 4 (module 2 is the baseline) to control for module specific effects (e.g., differences in the content or difficulty levels). Consistent with Hypothesis 1, the results in column 1 of Table 2 show that participants in the *RPI* treatment are significantly more likely to return to the training and take the next module (1.471, $p < 0.01$). In fact, the odds of starting the next module when a participant is eligible to start it are 4.35 times higher in the RPI group than in the No RPI group.[11]

However, in testing for random assignment, we find that employees in the RPI group scored significantly higher in their first attempt in module 1 than those in the No RPI group (88.1% versus 75.6%, $t = 2.73$, $p < 0.01$). As our manipulation only shows up after they completed module 1's test the first time, performance differences in this test score cannot be explained by our treatment. To account for this in our analysis, we run the same logistic regression as before but include a control variable for the participant's first test score on module 1 (*FirstScore*). The results in column 2 of Table 2 show that, although there is a marginal effect of the *FirstScore* on the likelihood of beginning the next module ($p = 0.11$), the treatment effect of *RPI* still is significant (1.361, $p < 0.01$). Thus, even after controlling for *FirstScore*, employees receiving RPI throughout the training are significantly more likely to return to it and begin the next module. This provides strong support for our first hypothesis.

4.2. Hypothesis 2

In Hypothesis 2, we predict that the positive effect of receiving RPI about training performance on voluntary training persistence increases with employee competitiveness. To test it, we again estimate a logistic regression with clustered standard errors on an individual level and include module 3 and 4 indicator variables. The dependent variable is *ModuleStarted*, whereas the independent variables comprise *RPI*, *competitiveness*, *avoidance*, and the respective interaction between *RPI* and both *competitiveness* and *avoidance*. For ease of interpretation, we center both continuous variables (i.e., *competitiveness* and *avoidance*) at their mean. Hence, the coefficient for *RPI* represents the impact of *RPI* on *ModuleStarted* at mean levels of *competitiveness* and *avoidance* (Aiken and West 1991).[12]

The results in column 3 of Table 2 show that for average levels of *competitiveness* and *avoidance*, *RPI* has a positive and significant simple effect (1.646, $p = 0.01$, odds ratio 5.19). Moreover, consistent with Hypothesis 2, the effect of *RPI* is stronger for participants scoring high on *competitiveness* (0.885, $p = 0.05$).[13] Thus, the more utility an employee gains from outperforming others, the more likely the employee engages in an additional module of training once provided with RPI. If there is no RPI, we find no effect of competitiveness (-0.281, $p = 0.33$). As expected, our control variable capturing *avoidance* shows neither a significant simple

Table 1. Sample Selection and Descriptive Statistics

Panel A: Sample selection	
Total enrollment	156
Employees who were part of the pilot group	(23)
Employees who did not access the course (no-shows and early dropouts)	(54)
Final sample	$n = 79$

Panel B: Descriptive statistics

Sample selection steps	No RPI	RPI	Total	Δ p-value
N	40 (50.6%)	39 (49.4%)	79 (100%)	
ModuleStarted	0.660 (0.478)	0.904 (0.296)	0.816 (0.389)	<0.001
Finished	0.175 (0.385)	0.667 (0.478)	0.418 (0.496)	<0.001
Age	40.550 (10.276)	40.974 (9.472)	40.759 (9.826)	0.849
Tenure	6.250 (5.843)	6.923 (6.339)	6.582 (6.063)	0.625
Gender	0.875 (0.335)	0.692 (0.468)	0.785 (0.414)	0.049
FirstScore	75.645 (24.944)	88.172 (14.184)	81.829 (21.186)	0.008

Panel C: Dropouts at the module level in the control group and treatment group

	No RPI			RPI		
Observations	Started	Not started	Passed	Started	Not started	Passed
Module 1	40	0	30	39	0	38
Module 2	19	11	14	31	7	30
Module 3	9	5	9	28	2	26
Module 4	7	2	7	26	0	26

Notes. Panel A shows the sample selection. Panel B shows the descriptive statistics (mean and standard deviation) for the participants. Except for *ModuleStarted*, these variables are calculated on an individual level. *N* displays the number of employees. *ModuleStarted* includes only the 147 decision points, where employees could decide on whether to begin the module or not. This excludes module 1 (where all participants had to begin to be included in our sample), as well as when participants did not pass the previous module. Panel C shows per module how many participants began, did not begin even though they were eligible to begin, and passed. *Finished* is an indicator variable, with a value of one (zero) to denote successful (unsuccessful) completion of the fourth module. *Age* is measured in years. *Tenure* captures the number of years the employee has worked at the company. *Gender* is an indicator variable, with a value of one (zero) to denote male (female) employees. *FirstScore* represents the initial percentage score in module 1 before employees received feedback on their performance (mean-centered for all subsequent analyses).

effect (0.0212, $p = 0.95$) nor a significant interaction effect (0.118, $p = 0.81$), and excluding *avoidance* altogether does not materially affect the hypothesis tests.[14]

Thus, these results are consistent with our theory that individuals who seek to best others react more strongly to RPI about training performance than those who score low on this personality trait. This provides support for Hypothesis 2 and, by extension, our theoretical reasoning for Hypothesis 1.

4.3. Additional Outcome Variables: Training Finished and Time to Complete

The focus of our study lies in the effect of RPI on employees' persistence in voluntary training. However, when RPI increases the persistence in such training, a natural follow-up question is whether RPI also increases the likelihood of completing them. Therefore, we examine the likelihood of finishing the training successfully and completing all four modules. To do so, we create the indicator variable *Finished* on an employee level, which takes the value of one if an employee completed all four modules successfully and zero if they dropped out along the way.

We run a logistic regression with *Finished* as the dependent variable. RPI is the main independent variable and, like in our main models, we control for *FirstScore*. As we have only one observation per individual in this analysis, we do not cluster standard errors or include module indicators. The results of column 1 in Table 3 show a positive and significant effect of RPI (2.013, $p < 0.01$). The odds of completing the training are 7.5 higher in the RPI treatment than in the No RPI treatment. Thus, the higher persistence induced by RPI also results in higher completion rates of the voluntary training.

In column 2, we run the same logistic regression and include *competitiveness* and *avoidance*, as well as their respective interactions with RPI. The results show that RPI still has a positive effect on the likelihood to complete the training (2.144, $p < 0.01$). However, the interaction between *RPI* and *competitiveness* does not reach statistical significance (0.387, $p = 0.42$). Thus, although the effect of *RPI* on persistence in voluntary training is particularly strong for employees scoring high on *competitiveness*, we do not find significant evidence for such an effect on the likelihood of completing the training.

Table 2. Hypotheses Test

	(1) *ModuleStarted*	(2) *ModuleStarted*	(3) *ModuleStarted*
RPI	1.471***	1.361***	1.646***
	(<0.01)	(<0.01)	(0.01)
FirstScore		0.016	0.011
		(0.11)	(0.14)
Competitiveness			−0.281
			(0.33)
RPI × *Competitiveness*			0.885**
			(0.05)
Avoidance			0.021
			(0.95)
RPI × *Avoidance*			0.118
			(0.81)
Module 3	0.502	0.488	0.402
	(0.34)	(0.36)	(0.45)
Module 4	1.594**	1.544*	1.477*
	(0.04)	(0.05)	(0.07)
Constant	0.324	0.340	0.403
	(0.32)	(0.32)	(0.28)
Observations	147	147	147
(Pseudo) R^2	0.131	0.144	0.191

Notes. This table displays the result of logit regressions (p-values in parentheses); 147 observations represent those instances where employees could decide whether to engage in the module. Instances where an employee did not pass the previous module or dropped out before are excluded from this analysis. Standard errors are clustered on an individual level (68 clusters). *ModuleStarted* is an indicator variable, taking on a value of one (zero) to denote if a participant began a specific module or not. *RPI* is an indicator variable equal to one (zero) for the RPI (no RPI) condition. *FirstScore* represents the initial score in module 1 before employees received feedback on their performance (mean-centered). *Competitiveness* is the average of a three-item construct and captures an employee's desire to outperform others (mean-centered). *Avoidance* is the average of a three-item construct and captures an employee's desire to avoid failures (mean-centered). Modules 3 and 4 are indicator variables for the respective modules.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table 3. Additional Outcomes

	(1) *Finished*	(2) *Finished*	(3) *Time*	(4) *Time*
RPI	2.013***	2.144***	−578.127*	−833.674***
	(<0.01)	(<0.01)	(0.06)	(0.00)
FirstScore	0.033	0.031	0.220	21.842*
	(0.10)	(0.15)	(0.99)	(0.09)
Competitiveness		−0.255		364.575**
		(0.47)		(0.04)
RPI × *Competitiveness*		0.387		−427.479*
		(0.42)		(0.05)
Avoidance		0.0005		258.147
		(1.00)		(0.21)
RPI × *Avoidance*		0.189		−215.676
		(0.73)		(0.33)
Constant	−1.521***	−1.563***	920.776***	1,004.556***
	(<0.01)	(<0.01)	(0.00)	(<0.01)
Observations	79	79	33	33
(Pseudo) R^2	0.226	0.243	0.123	0.457

Notes. Columns 1 and 2 present the results of logit regressions (p-values in parentheses) and columns 3 and 4 from an OLS regression. *Finished* is an indicator variable equal to one (zero) for those employees who successfully completed the training (or not). *Time* is the aggregated time between the modules (i.e., the time between passing module 1 (2) (3) until starting module 2 (3) (4)) measured in hours. *RPI* is an indicator variable equal to one (zero) for the RPI (no RPI) condition. *FirstScore* represents the initial score in module 1 before employees received feedback on their performance (mean-centered). *Competitiveness* is the average of a three-item construct and captures an employee's desire to outperform others (mean-centered). *Avoidance* is the average of a three-item construct and captures an employee's desire to avoid failures (mean-centered).
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

That said, the small sample size might have contributed to this insignificant finding.

Moreover, we examine how long individuals wait between modules. If employees engage in social comparisons and want to get a sense of their ability and outperform others, we expect that those with RPI are faster in returning to the training and engaging in the next module. Thus, for all participants who completed the training, we calculate the aggregate *Time* between passing the previous module-test and the start of the next module (i.e., the time they passed the test of module 1 (2) (3) and the first time they began the test of module 2 (3) (4)).[15] The average aggregated *Time* they leave between the individual modules in the no RPI module is 921.5 hours (with a median of 266.4 hours) and in the RPI group is 344.5 hours (with a median of 143.2 hours). This translates to a difference of 24 days (5.1 days). The results of an ordinary least squares (OLS) regression presented in column 3 of Table 3 show that the RPI group leaves significantly less time between the modules compared with the No RPI group (−578.1, $p = 0.06$). Moreover, the results in column 4 show that this effect is particularly pronounced for those scoring high on *competitiveness* (−427.5, $p = 0.05$). This is consistent with our reasoning that competitive individuals are even more eager to learn their relative ability and outperform others.

4.4. Follow-Up Analysis: Past Performance and Ability Differences

In our theory development we suggest that RPI increases persistence in voluntary training as employees want to get a better signal of their relative ability, improve their relative position, or recurringly experience the feeling of success. That said, given the low cost of dropping out from voluntary training, employees with low past training performance might also be tempted to avoid any further comparison by not getting back to the training. Similarly, employees with high training performance might embrace their success without risking to fall behind in case they work on a subsequent module. In this section, we therefore investigate whether the effect of RPI on persistence differs depending on employees' past performance and ability.

First, we test whether the effect of RPI on the likelihood of returning to the training differs depending on past performance. We include the absolute performance achieved in the previous module ($Performance_{t\text{-}1}$, mean-centered for ease of interpretation), as well as the interaction of *RPI* and $Performance_{t\text{-}1}$ in the main regressions used for testing Hypothesis 1 and Hypothesis 2. The results presented in columns 1 and 2 of Table 4 show that neither the simple effect of $Performance_{t\text{-}1}$, nor the interaction between *RPI* and $Performance_{t\text{-}1}$ is significant. This suggests that the absolute past performance does not affect employees' reaction to RPI with regard to their persistence. Importantly, however, the simple effect of *RPI* remains significant in both specifications, and the interaction between *RPI* and *competitiveness* remains marginally significant ($p = 0.105$) in column 2.

In columns 3 and 4, we replace the absolute past performance variable with the relative performance employees achieved in the previous module. Specifically, we create a variable called $PerformanceDecile_{t\text{-}1}$, which captures the decile in which an employee's performance fell in the previous module based on the benchmark scores from the pilot study (e.g., employees ranking in the top decile compared with the reference group receive the value of 10, whereas employees in the bottom decile receive the value of 1). We mean-center the variable for ease of interpretation and include it, as well as its interaction with *RPI*, in our main regression equations. The results (depicted in columns 3 and 4 of Table 4) show that the relative performance decile in the previous module does not significantly influence the effect of RPI on returning to the training or our conclusions for Hypothesis 1 and Hypothesis 2. Alternatively, we generate two indicator variables that capture whether an employee placed in the top three or bottom three deciles as compared with the peer group from the pilot study (i.e., top 30 or bottom 30% performers) in the prior round. Like before, we include these indicator variables in our main regressions for Hypothesis 1 and Hypothesis 2 and additionally interact them with the RPI treatment variable. The results in columns 5 and 6 show no significant simple effect of the $Top30Performance_{t\text{-}1}$ and $Bottom30Performance_{t\text{-}1}$ indicator variables, and we neither find a significant interaction with the treatments. This suggests that there is no difference in the response to RPI depending on whether employees were ranked in the top 30 or bottom 30% compared with the midrange. Results remain consistent when we use different cutoff values (i.e., 20% or 10%).

In untabulated analyses, we take a closer look at *FirstScore* instead of the past performance. Because this represents the score employees achieved at their very first try of the first module (i.e., before they have been exposed to the manipulation), this is a reasonable signal of employees' motivation and ability for the training. We run the same test as for Hypothesis 1, but instead of simply controlling for the (mean-centered) *FirstScore*, we also interact *RPI* with *FirstScore*. We find no significant interaction between *RPI* and *FirstScore*, whereas the hypothesized effects of Hypothesis 1 and Hypothesis 2 remain robust. When we put employees in deciles of their *FirstScore* and replace the absolute *FirstScore* with this relative measure, we again find no significant effect of the *FirstScoreDecile* or the interaction of *RPI* × *FirstScoreDecile*. Lastly, we create indicator variables for the top 30 or bottom 30% of *FirstScore* ($Top30FirstScore_{t\text{-}1}$ and $Bottom30Firstscore_{t\text{-}1}$) and include these indicator variables and their interaction with *RPI* in the regression instead of the absolute *FirstScore*. Again, neither of the indicator variables nor their interaction with *RPI* reaches statistical significance, whereas the treatment effect of *RPI* (and its interaction with *Competitiveness*) remain significant. The results remain consistent when we use different cutoff values (i.e., 20% or 10%) for those indicators.

These results suggest that RPI about employees' training performance increases the likelihood of returning to the training independent of prior performance or ex-ante ability. This speaks to a broad applicability of RPI to boost persistence in voluntary training and alleviates concerns that the provision of RPI might deter certain subgroups. Specifically, the RPI literature suggest that very high or low performing individuals might react differently to the provision of RPI compared with individuals with average performance (Festinger 1954, Eyring and Narayanan 2018, Diel et al. 2021), because they are missing meaningful comparison groups (e.g., they are too far ahead of or behind of others). We do not find evidence for such unintended side effects. RPI increases persistence uniformly across performance levels in our setting. We acknowledge, however, that our design of RPI potentially contributed to that finding. We deliberately decided to provide individuals with decile information of the peer group instead of providing only the mean or 75th percentile information to ensure that even very high or low performing individuals still have meaningful comparison groups that speak to their constant drive upwards and sense of comparing their relative ability (Festinger 1954).

4.5. Additional Analysis: Performance Effects

Although the primary focus of our study is on how RPI about employees' training performance influences their likelihood of returning to voluntary training, we eventually also examine the performance that employees achieved on the module tests. We do so in two ways. First, we test whether the provision of RPI influences employees' likelihood of passing a module in the voluntary training. As pointed out above, there was an absolute performance threshold of 80% that employees

Table 4. *FirstScore* and Past Performance

	(1) *ModuleStarted*	(2) *ModuleStarted*	(3) *ModuleStarted*	(4) *ModuleStarted*	(5) *ModuleStarted*	(6) *ModuleStarted*
RPI	1.224**	1.483**	1.578***	1.769**	1.302*	1.516**
	(0.02)	(0.03)	(0.00)	(0.01)	(0.06)	(0.04)
Firstscore	0.011	0.008	0.011	0.008	0.0131	0.0114
	(0.20)	(0.27)	(0.21)	(0.28)	(0.15)	(0.22)
$Performance_{t-1}$	0.0196	0.0153				
	(0.68)	(0.75)				
$RPI \times Performance_{t-1}$	0.103	0.0898				
	(0.16)	(0.21)				
$PerformanceDecile_{t-1}$			0.020	0.018		
			(0.84)	(0.86)		
$RPI \times PerformanceDecile_{t-1}$			0.266	0.222		
			(0.15)	(0.23)		
$Top30Performance_{t-1}$					0.196	0.071
					(0.79)	(0.92)
$RPI \times Top30Performance_{t-1}$					1.195	1.095
					(0.37)	(0.42)
$Bottom30Performance_{t-1}$					0.335	0.281
					(0.73)	(0.77)
$RPI \times Bottom30Performance_{t-1}$					−0.703	−0.556
					(0.56)	(0.65)
Competitiveness		−0.271		−0.267		−0.275
		(0.34)		(0.35)		(0.35)
RPI × *Competitiveness*		0.763		0.748*		0.805*
		(0.11)		(0.10)		(0.08)
Avoidance		0.0102		−0.003		0.010
		(0.97)		(0.99)		(0.97)
RPI × *Avoidance*		0.164		0.171		0.130
		(0.74)		(0.73)		(0.78)
Module 3	0.452	0.394	0.263	0.223	0.498	0.411
	(0.40)	(0.46)	(0.66)	(0.71)	(0.41)	(0.50)
Module 4	1.738**	1.620*	1.502*	1.417*	1.634*	1.511*
	(0.04)	(0.05)	(0.06)	(0.08)	(0.06)	(0.08)
Constant	0.262	0.341	0.403	0.465	0.185	0.310
	(0.47)	(0.40)	(0.25)	(0.23)	(0.76)	(0.62)
Observations	147	147	147	147	147	147
Pseudo R^2	0.178	0.212	0.173	0.208	0.166	0.204

Notes. This table displays the result of logit regressions (p-values in parentheses); 147 observations represent those instances where employees could decide whether to engage in the module. Instances where an employee did not pass the previous module or dropped out before are excluded from this analysis. Standard errors are clustered on an individual level (68 clusters). *ModuleStarted* is an indicator variable, taking on a value of one (zero) to denote if a participant began a specific module or not. *RPI* is an indicator variable equal to one (zero) for the RPI (no RPI) condition. *FirstScore* represents the initial score in module 1 before employees received feedback on their performance (mean-centered). $Performance_{t-1}$ is the maximum score employees achieved on the previous module (mean-centered). $PerformanceDecile_{t-1}$ captures an employee's decile placement on the prior module's test ranging from 1 (bottom decile) to 10 (top decile) as compared with the peer group from the pilot study (mean-centered). $Top30Performance_{t-1}$ and $Bottom30Performance_{t-1}$ are indicator variables that denote whether an employee was placed in the top 30 or bottom 30% of participants on the prior module's test compared with the peer group from the pilot study. *Competitiveness* is the average of a three-item construct and captures an employee's desire to outperform others (mean-centered). *Avoidance* is the average of a three-item construct and captures an employee's desire to avoid failures (mean-centered). Modules 3 and 4 are indicator variables for the respective modules.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

needed to pass before being eligible to proceed to the next module. Based on this threshold, we generate the variable *Passed*, which takes the value one (zero) if the employee passed (did not pass) a particular module. However, as not everyone who passed the previous module also started the next module test, we include only those observations in our analysis where employees began working on a module test.[16] Regressing *Passed* on *RPI*, *FirstScore*, and the module indicator variables, and clustering the standard errors on an employee level, we find that RPI indeed has a positive and significant effect on the likelihood to pass a module ($p < 0.01$; see column 1 of Table 5). Second, we generate the variable $Performance_t$ on each module based on the highest score employees achieved on a module and regardless of how many attempts were taken. Using the same regression specification as above, we again find that *RPI* shows a significant and positive effect ($p = 0.09$) on participants' performance, even after controlling for *FirstScore*. This is consistent with prior studies examining the effects of RPI on task performance.

Table 5. Performance

	Model 1 *Passed*	Model 2 *Performance*
RPI	1.586***	3.556*
	(<0.01)	(0.09)
FirstScore	0.023	0.190
	(0.13)	(0.11)
Module 2	−0.207	−4.144
	(0.73)	(0.11)
Module 3	0.371	1.357
	(0.64)	(0.51)
Module 4		−4.853***
		(0.00)
Constant	1.410***	86.20***
	(<0.01)	(<0.01)
Observations	166	199
Pseudo R^2	0.150	0.130

Notes. This table displays the result of linear regressions (p-values in parentheses). Model 1 automatically drops observations from module 4, because there is no variation in *Passed* in that module (i.e., all 33 participants who began module 4 passed it). In model 2, all results from all employees who finished a particular module are included. Standard errors are clustered on an individual level. *RPI* is an indicator variable equal to one (zero) for the RPI (no RPI) condition. *FirstScore* represents the initial score in module 1 before employees received feedback on their performance (mean-centered). *Passed* is an indicator variable, taking the value of one (zero) if a participant passed a specific module or not. *Performance* is the highest score of an employee's individual module tests.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

5. Conclusion

We explore the impact of providing employees with RPI about their voluntary training performance on their persistence in voluntary training. In line with our hypotheses, we find a positive effect of providing employees with such RPI on their likelihood of returning and continuing with their next training module. Eventually, this mechanism also leads to substantially higher completion rates in our field experiment. Consistent with social comparison theory as the guiding theoretical lens for our study, we also find that our results are significantly stronger for individuals with a more competitive personality. The effectiveness of RPI also holds irrespective of an employee's relative performance in the prior round, alleviating concerns this intervention might deter particularly high- or low-performing individuals.

This has important implications for organizations who struggle with training their employees in the best way. Although training is an important control choice that firms use to ensure that employees possess the skills and competences to work toward the firm's goals, they often want to provide low barriers to entry into training programs and keep them voluntary. Dropout is a significant issue in such settings. Complementing recent studies in management accounting that investigate ways to encourage training participation (Kelly et al. 2021, Arnold et al. 2025), we show that through the provision of RPI on employees' training performance, firms have a relatively straightforward option to encourage persistence in voluntary training. We do so by conducting a field experiment, which allows us to identify causal relationships directly in the real-life context we are interested in (Floyd and List 2016).

We also show that providing RPI about employees' training performance to encourage persistence in voluntary training works particularly well for more competitive individuals. Doing so, we both provide evidence for our proposed underlying mechanism (social comparison) and, more broadly, add to the literature that studies conditions in which RPI has particularly strong effects. This also has practical implications for firms considering providing such RPI on training. Prior research shows that competitiveness is a character trait found more frequently in certain functions or industries than others, leading some contexts to be better fit for providing employees with RPI. Anni et al. (2025), for instance, find that individuals low on agreeableness—a basic personality trait typically inversely associated with competitiveness (Graziano et al. 1997)—tend to self-select into functions like sales or marketing, making this a group of employees that might be particularly responsive to receiving RPI on their training performance. Similarly, if certain industries tend to attract competitive individuals (e.g., by emphasizing competitive "up-or-out" contracts such as in consulting, law firms, or academia; Ghosh and Waldman 2010), they might present a better fit for RPI interventions than others. Overall, firms should carefully consider not just the type of performance information they present but also its fit with character traits of the target group that receives such information.

Finally, although we explicitly focus on employee persistence in voluntary training, there are plenty of activities in an organizational context that are critical for the success of a firm yet voluntary for the individual, in the sense that they are neither explicitly part of their job description nor explicitly incentivized (e.g., extrarole behaviors like submitting improvement ideas or contributing to knowledge databases; Van Dyne et al. 1995, Podsakoff et al. 2000). As such, our field evidence on RPI as a simple tool to encourage continuous engagement in voluntary training also speaks to a broader literature on activities that are similar in nature.

In addition to the mere provision of RPI, prior studies suggest that the way RPI is implemented can vary substantially across organizations, which can have consequences for its effectiveness in shaping employee behavior. Balancing conceptual clarity in our research design with the institutional demands of our field setting, we naturally had to make several choices on the implementation of RPI in our study that warrant further discussion and offer opportunities for future

research. We highlight three major choices here and discuss their implications for the generalizability of our results. First, RPI in our setting is fully private, and no one else ever learns about the performance of a single other individual. Prior research has shown that when RPI is public it not only affects employee self-image, but can also trigger an additional motivational mechanism in form of social status (Tafkov 2013, Casas-Arce et al. 2023). Given the private nature of RPI in our study, this mechanism is not applicable, likely rendering our effects a more conservative estimate for many implementations in practice.

Second, in line with the company's policy for training, all participants in our study were presented with an absolute threshold for passing a module. This means that part of the ambiguity about a participant's own ability was likely somewhat alleviated by knowing whether they missed or passed this minimum score. Consequently, the effect of RPI we isolate in our study is incremental to the impact such an absolute threshold may have. Absent such an absolute threshold (or any other type of feedback that puts an employee's performance into context), we expect the impact of RPI to be even stronger. In fact, Eyring et al. (2021) demonstrate that providing RPI alone might show even stronger motivational effects than the combined impact of absolute and relative feedback, with the former distracting from the salience of the latter.

Third, in line with the literature that suggests goals need to be challenging yet achievable to unlock their full motivational potential (Locke and Latham 2015, Eyring and Narayanan 2018), we chose to disaggregate relative performance into deciles and provide performance information for each. This ensured employees had a better grasp on their relative position and more likely had an achievable higher decile rank to aspire to (or lower decile rank to avoid). This allows us to create a powerful setting for testing the effect of RPI in the field and minimize the risk that our RPI manipulation entails some unintended consequences that harm the organization. We note, however, that RPI implementation varies in practice, with designs ranging from providing the mean or median performance score to a full ranking and/or every participant's performance (Tran and Zeckhauser 2012, Kramer et al. 2016, Hannan et al. 2019). Future research can test to what extent the granularity of RPI matters for how likely employees return to their voluntary training sessions, as well as how these other design choices impact the effectiveness of RPI in voluntary training.

Besides being mindful about the different ways in which RPI can be implemented, we also encourage future studies to provide more direct evidence for the underlying mechanisms tested in our study. Although we designed the experiment in a way that rules out many alternative explanations and analyzed the interaction between our manipulation and our participants' competitiveness as an indirect test for our reasoning based on social comparison theory, we could not send out a postexperimental questionnaire to gather additional direct evidence. Finally, we also lack direct evidence on how participants engaged with the training content. Although we have access to the most crucial data for testing our hypotheses (i.e., test completion, test scores, and time between tests), we do not have information on when, how often, and for how long the participants accessed the training modules themselves. Future research focusing on RPI's impact on the learning experience per se could investigate the patterns of *how* individuals learn when provided with RPI.

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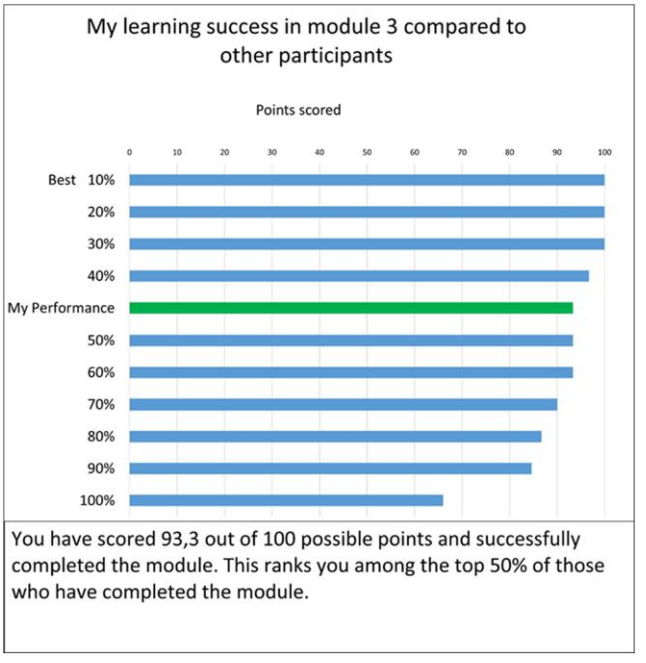
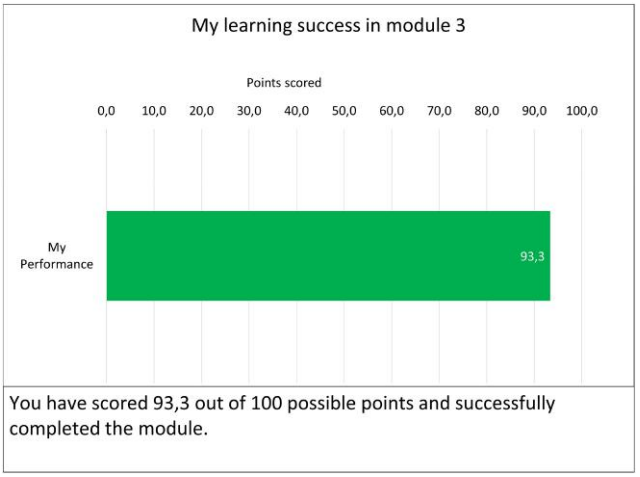
Appendix

RPI Screen

Display of Relative Performance Information



No RPI Screen
Display of Personal Performance Information



Endnotes

[1] Although companies might provide other reference points such as passing hurdles or subjective feedback, there still remains substantial uncertainty about one's own ability. For example, even if someone passes a test with a score of 60%, this person cannot fully evaluate whether the performance was just sufficient, very high, or actually mediocre. In such cases, RPI puts one's own performance in a broader perspective, suggesting that RPI provides incremental value even in presence of absolute performance information. In fact, the field site of our study also uses an absolute passing hurdle for their tests, and we focus on the *incremental informational value* of RPI specifically and its effect on voluntary training persistence.

[2] Note that, in the absence of RPI, individuals can still assess their performance improvement relative to their prior self (i.e., compare their absolute scores from one round to the next). However, this neither accounts for differences in the difficulty between assessments nor for average learning effects of participants between assessments (e.g., participants get used to this form of learning, develop learning strategies), rendering it a much less useful method for assessing ability improvements.

[3] This manual email procedure was necessary because there was no other technical solution where the reports could be sent automatically in that environment. Moreover, the delay in receiving a report ensured that employees did not simply rush through all four modules at once and had to *return* to the platform to begin the next module. This creates a stronger setting for testing our theory.

[4] Note that the peer information is completely anonymous and does not allow any conclusions to be drawn about the individual performance data of the peers; that is, RPI is private.

[5] Following previous studies, RPI is presented in the form of a figure (Eyring and Narayanan 2018, Schreck 2020). Furthermore, the information is explained in a text below the figure.

[6] Although the correlation between competitiveness and avoidance is positive and significant ($r = 0.469$, $p < 0.01$), they are conceptually different constructs, and we treat them as such. This is consistent with studies suggesting that striving for success is different from avoiding failure (Roels and Su 2014).

[7] Consequently, employees have varying amounts of time to complete the training depending on their registration date. Employees were notified of the remaining duration upon registration. Furthermore, they were informed about the remaining time two months before the training expired through a message on the learning platform and via e-mail.

[8] Employees who registered by August 17 were included in the pilot group.

[9] At the time the experiment began and the first participant from the treatment group required RPI, the RPI provided to the treatment group was calculated from the participants in the pilot phase who had completed the relevant module at that time. (Module 1: 23 employees (September 27); Module 2: 21 employees (October 6); Module 3: 21 employees (October 18); Module 4: 19 employees (October 18)). RPI remains the same for all employees in the RPI Group.

[10] Testing for randomized assignment shows no significant difference between the RPI and No RPI group in terms of age ($p = 0.849$), company tenure ($p = 0.625$), or department ($\chi^2(7) = 7.136$, $p = 0.415$). However, significantly more female participants were in the RPI group ($p = 0.049$). In untabulated analyses, we include gender as additional control variable. Yet none of our conclusions change with including gender as control. All results of interest remain significant, and gender has no significant effect in any of the analyses. Thus, for brevity we do not include it in our main analysis. The second variable for which we found significant differences was the score in the first attempt of the first module, before employees even see the manipulation. As we discuss in the text, we include the *FirstScore* as a control variable in all our analyses.

[11] We derive this ratio by exponentiating the regression coefficient for *RPI* from our logistic regression model (in this case: $e^{(1.471)} = 4.35$). The odds ratio is defined as the ratio of the odds of the outcome at one level of an independent variable x relative to the odds of the outcome at another level of x (Osborne 2014). Given the dichotomous nature of *RPI* in our setting, it thus compares the odds of continuing with the next module in the RPI group with the odds of continuing in the non-RPI group.

[12] Failing to mean-center the continuous variables would lead the coefficient for *RPI* to capture its impact on *ModuleStarted* when *competitiveness* and *avoidance* take the value of zero, which is not defined on our scale ranging from one to seven.

[13] In untabulated robustness tests, we run the main analysis of column 2 separately for those scoring above or below the midpoint of four on the competitiveness scale, leaving out the 17 observations from the eight participants that score exactly at the midpoint of four. Consistent with our theory, results show that RPI has a positive effect on *ModuleStarted* in the high-competitiveness subsample (2.166, $p = 0.01$), but RPI has no effect on *ModuleStarted* for those below the midpoint (1.062, $p = 0.14$). The pattern is similar if we repeat the analysis for those at or above the 75th percentile on the competitiveness scale (2.580, $p = 0.03$) and at or below the 25th percentile (-0.163, $p = 0.89$).

[14] Our competitiveness measure is based on an established construct (Newby and Klein 2014) to ensure it reflects the character trait of interest. However, as it is a measured construct, a potential concern might be confounding variables. For instance, more competitive employees might come disproportionately from certain departments and see the skills learned in the training or how useful these skills are differently. First, we test whether *competitiveness* varies across departments and find no significant association ($p = 0.726$). With regard to the value and importance employees attach to the training (which we captured in the pre-experimental survey), we indeed find a weak but significantly positive correlation of our competitiveness measure with both the importance of the skills ($r = 0.187$, $p = 0.099$) and the usefulness ($r = 0.234$, $p = 0.038$). Importantly, however, when we include those two variables and their interactions with the RPI treatment in our main regression used for testing Hypothesis 2, we find no significant effect of either of the two variables alone or their respective interaction coefficients with

RPI (all $p > 0.266$). Our hypothesized coefficients of *RPI* (coefficient 1.921, $p = 0.032$) and *RPI* × *Competitiveness* (coefficient 0.976, $p = 0.058$) remain significant. These results further strengthen confidence that it is competitiveness that increases the effectiveness of RPI.

[15] The analysis here includes only those 33 who completed the entire training. We also perform an untabulated analysis on a module level, where we include observations from those who dropped out as well. The results are consistent with those presented here.

[16] Note that for this analysis, we also include performance scores for module 1. This is because employees had the opportunity to retake the test after receiving the results from their first attempt (including the manipulation). Additionally, we control for the first test score (*FirstScore*) to account for those instances where participants took the first module only once and had not been exposed to the manipulation yet.

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