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The Effects of a Short-Term Corporate Social Impact Activity on Employee Turnover: Field Experimental Evidence

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Abstract. Short-term corporate social impact activities, wherein employees participate in firm-sponsored prosocial initiatives, are becoming increasingly common. However, it remains unclear whether short-term social impact activities affect employee behavior in a manner of relevance to the firm. Theoretically, arguments could be made in favor of or against the likelihood that such short-term activities would increase firm-benefitting employee behavior. We utilized data from a randomized field experiment implemented at a large Latin American bank to examine whether a short-term social impact activity can be beneficial for firms, focusing on one important outcome: turnover. Newly hired employees were randomly assigned to a short-term social impact activity as part of the new employee onboarding process or not. Notably, we find causal evidence that a day-long, short-term engagement reduced employee turnover almost a year later. We explore potential mechanisms behind this effect and find that employees' perceptions of organizational justice help to explain the effects of the intervention on turnover. We also explore heterogeneous treatment effects and find more substantial effects for male, rather than female, employees. This paper advances the literature on the implications of corporate social responsibility (CSR) practices by shedding light on the causal mechanisms through which a theoretically underexplored and practically relevant type of CSR activity can benefit organizations and their employees.

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1. Introduction

Organizations are increasingly implementing different types of socially responsible programs and policies (Carnahan et al. 2017, Shea and Hawn 2019, Flammer and Kacperczyk 2019, Rodell et al. 2020, Gupta et al. 2021). The continued proliferation and rapid evolution of various corporate social responsibility (CSR) practices have made research that rigorously evaluates the impact of distinct types of CSR practices more important than ever. Thus, to better understand whether, when, and through what mechanisms CSR influences organizations, scholars are increasingly breaking down the multidimensional construct of CSR into its different facets (Carnahan et al. 2017, Burbano et al. 2018, Singh et al. 2019) and examining the influence of these facets on specific firm stakeholders (Burbano 2016, Rodell et al. 2020, Odziemkowska and Henisz 2021). In this study, we focus on a group of critical internal stakeholders (i.e., employees) and examine whether and how they are affected by a short-term, firm-organized social impact activity.

CSR programs that enable employees to devote time and effort to a social impact activity organized and funded by their firm (Bode et al. 2015, Bode and Singh 2018, Rodell et al. 2016, Gatignon 2022) are a common component of companies' socially responsible efforts, and these programs have become almost ubiquitous over the past few decades (Grant 2012, Rodell 2013, Rodell et al. 2017). Although researchers have examined *long-term* employee volunteer programs (Bartel 2001, Pless et al. 2011, Grant 2012, Caligiuri et al. 2013, Bode et al. 2015, Gatignon-Turnau and Mignonac 2015, Hu et al. 2016, Jones 2016, Gatignon 2022), the effects of one-time, *short-term* initiatives remain underexplored (Cnaan et al. 2021), despite their increasing prevalence. These one-time events organized by firms involve employees contributing time toward a social cause (Cnaan et al. 2021), often alongside colleagues and coworkers. Examples of common activities include cleaning up beaches and roadsides, planting trees, or participating in community outreach activities (Hejjas et al. 2019).

Could short-term social impact activities such as these affect employee behavior in any meaningful way? Existing theory suggests that this is unclear. On one hand, the mechanisms underlying firm benefits from long-term corporate volunteering and pro bono programs suggest that short-term programs may be ineffective. Learning new skills (Burbano et al. 2018, Gatignon 2022) requires some continued exposure to the pro bono or corporate volunteering activity, for example. Furthermore, researchers have found that the persistence (Caligiuri et al. 2013) and intensity (Rodell 2013, Brockner et al. 2014) of employees' involvement in corporate volunteering programs are critical to influence employees' attitudinal and behavioral outcomes (Rodell et al. 2016). On the other hand, tenets from social psychology literature focusing on the effects of short-term interventions on individuals (Brockner and Sherman 2019) suggest that such a short-term initiative *could* have the potential to affect employee behavior. If a prosocial activity organized by a firm during the workday represents an event that is distinct from employees' routine day-to-day work activities (Morgeson et al. 2015), such an event—even a short one (Walton 2014, Morgeson et al. 2015, Brockner and Sherman 2019)—has the potential to result in internal sensemaking (Aguinis and Glavas 2019), which can influence individuals' perceptions of their environment and employer (Weick 1995, Hahn et al. 2014, Aguinis and Glavas 2019). If these perceptions are sufficiently altered, they could potentially result in firm-benefitting employee behavior.

Whether a short-term social impact activity involving employees could affect firm-benefitting employee behavior is thus an empirical question. To explore this question, we followed an abductive approach (Pillai et al. 2021) to analyze data from a randomized field experiment implemented at a large Latin American bank. New employees were randomly assigned whether to participate in a CSR intervention organized by the firm as part of the bank's new employee onboarding process or not. We tracked whether the employees left the firm over the bank's next turnover reporting cycle (approximately 10 months later), and we collected information on their perceptions of their employing company and job via a posttreatment survey.

We found that employees who were randomly assigned to participate in the intervention were approximately 50% less likely to leave the firm than those who were not (based on an intent-to-treat (ITT) analysis).¹ Employee turnover is an outcome well-established as critical to organizations (Glebbeeck and Bax 2004, Carnahan et al. 2012, Heavey et al. 2013, Li et al. 2022), as high employee turnover can disrupt several productivity-related outcomes (Shaw et al. 2005, Hausknecht et al. 2009, Argote et al. 2018), decrease financial performance (Heavey et al. 2013), and even threaten an organization's survival (Agarwal et al. 2009). Whereas existing studies have focused on the effects of voluntary, long-term CSR programs on turnover (Bode et al. 2015, Carnahan et al. 2017, Gatignon 2022), ours is the first, to

our knowledge, to suggest that a short, one-time intervention can have such effects. It is noteworthy that a one-time social impact activity could influence such a critical (long-term) employee behavioral outcome.

Given this finding, we next examine employees' post-treatment survey responses to explore the likely mechanisms at play. We find suggestive evidence that the intervention affected employees' perceptions of their firm, which in turn, mediated the effect of the intervention on turnover. Specifically, our findings suggest that perceptions of organizational justice, but not of other potential factors such as organizational identification or person-organization (P-O) fit, help explain the effects of the intervention on employee turnover.

We also explore heterogenous effects to examine which types of employees are most affected by such an intervention. Interestingly, we find suggestive evidence that the effects are greater for male employees than female employees, and we offer a post hoc explanation for why this might be the case. Specifically, given that women have been shown to be more likely to value (Burbano et al. 2022) and engage in prosocial and communal activities than men (Einolf 2011), our results are consistent with the argument that individuals for whom an intervention is more novel or nonroutine will be more likely to engage in "sensemaking" after participating in an activity, resulting in larger perceptual changes and in turn, stronger effects.

This paper is, to our knowledge, the first to examine the effect of a short-term corporate social impact activity on a critical employee behavioral outcome: turnover. We provide evidence of a mechanism underlying the effects on turnover—an increased perception of organizational justice—that is distinct from those shown to underlie effects of longer-term corporate social volunteering programs and pro bono work for employees (Burbano et al. 2018, Gatignon 2022). Notably, our paper is also the first to provide *causal* evidence that employee participation in a CSR initiative of any type influences employee perceptions and outcomes in a manner that is beneficial for organizations.² We thus contribute to an understanding of the microfoundations (Foss and Lindenberg 2013, Shea and Hawn 2019, Suddaby et al. 2020) of how CSR can lead to firm-benefitting employee behavior (Burbano 2016, Crilly et al. 2016, Bode and Singh 2018, Burbano et al. 2018, Burbano and Chiles 2022, Gatignon 2022).

2. Literature and Theory

Scholars are increasingly moving away from the broad debate on whether CSR positively influences firm financial performance (Margolis and Walsh 2001, Margolis et al. 2007) toward seeking to understand how CSR might benefit firms. Organizational stakeholders' perceptions of and behavioral responses to CSR are emerging as critical mechanisms behind the link between CSR

and organization-benefitting outcomes (Wagner et al. 2009, Bode et al. 2015, Farooq et al. 2017, Lungeanu and Weber 2021). To better understand how stakeholders respond to CSR, scholars have begun to examine the responses of specific groups of firm stakeholders, including investors (e.g., Chen and Gavius 2015), consumers (e.g., Servaes and Tamayo 2013, Shea and Hawn 2019), and suppliers (e.g., Fang and Cho 2020, Kraft et al. 2020), for example. In line with this approach, this paper focuses on one critical firm stakeholder: the employee (Burbano 2016, Flammer and Luo 2016, Flammer and Kacperczyk 2019, Crane 2020).

Researchers have also begun to break apart the multi-dimensional construct of CSR into its different facets to better understand how specific stakeholders react (Carnahan et al. 2017, Burbano et al. 2018, Singh et al. 2019, Portocarrero et al. 2023). This is particularly critical for examining the effect of CSR on employee responses, as employees have been shown to view their firm's CSR not as an aggregated whole but rather, as a group of different programs and practices (El Akremi et al. 2015). Although scholars have explored the relationship between various types of externally focused CSR initiatives, such as corporate philanthropy (Burbano 2016, 2021a) or environmental initiatives (Hejjas et al. 2019) and employee perceptions and behavior, internally focused CSR initiatives are likely to influence employee perceptions and behavior through different mechanisms and with different outcomes (Wood 2010, Morgeson et al. 2015, Farooq et al. 2017, Burbano et al. 2018, Portocarrero et al. 2023).

Scholars exploring the effects of employee participation in CSR have generally focused on long-term, ongoing corporate volunteering programs (Bartel 2001; Pless et al. 2011; Grant 2012; Caligiuri et al. 2013; Bode et al. 2015, 2022; Gatignon-Turnau and Mignonac 2015; Hu et al. 2016; Jones 2016; Gatignon 2022). Theoretically, it is important to distinguish short-term, single-occurrence activities and interventions from longer-term corporate volunteering programs. The mechanisms through which firms have been shown to benefit from longer-term corporate volunteering programs are unlikely to apply to short-term activities. The benefits of longer-term corporate volunteering and pro bono projects in the form of "stretch role" skills development (Bode and Singh 2018, Burbano et al. 2018, Bode et al. 2022) and institutional learning about operating in challenging environments (Gatignon 2022) require extended involvement in the service activity, for example. Furthermore, researchers have shown that in long-term volunteering programs, frequent (Caligiuri et al. 2013) and more intense (Rodell 2013, Brockner et al. 2014) volunteering leads to benefits for employees in the form of increased feelings of happiness (Paço and Nave 2013), self-integrity (Brockner et al. 2014), and engagement (Caligiuri et al. 2013). Less frequent, less intense volunteering may not have such effects on employees.

On the other hand, tenets of social psychology literature on the effects of short-term interventions on individuals more broadly suggest that CSR interventions could affect downstream employee outcomes, even longer-term outcomes (see Brockner and Sherman 2019). Short-term interventions can be effective in altering individual-level behavior *if* they succeed in changing the subjective meaning (i.e., perception) that individuals assign to themselves or their situations (Walton 2014, Walton and Wilson 2018). A nonroutine event at work could cause employees to engage in a sensemaking process that influences their perceptions of their work environment or employer (Morgeson et al. 2015). By influencing how people think about themselves, others, or their situations, short-term interventions can thus potentially have long-lasting downstream effects on attitudes and behaviors. For example, a short 10-day intervention consisting of writing and reflecting on interactions with consumers for a few minutes helped service providers change their perceptions of difficult clients and subsequently, increased customer satisfaction (Hulsheger et al. 2015). Likewise, an hour-long intervention that encouraged newly hired employees to focus on their authentic selves at work increased employee retention and boosted customer satisfaction ratings (Cable et al. 2013).

It is thus theoretically unclear whether a short-term corporate social impact activity could have any meaningful effect on employees or their firm-benefitting behavior. Moreover, there are significantly different levels of employee commitment, management requirements, and costs associated with ongoing versus one-time CSR initiatives (Hyde et al. 2014, Cnaan et al. 2021). As such, it is also practically relevant to consider whether a one-time activity can have firm-benefitting downstream effects on employees.

Empirically testing the relationship between a corporate social impact activity and employee behavioral outcomes presents several challenges. First, randomly assigning CSR-related activities or information to a group of employees is practically and logistically challenging because employees generally self-select into such programs (Burbano 2016, 2021a, b). As such, researchers have often been unable to establish a causal relationship between CSR activities and employee outcomes of interest more broadly (Rupp and Mallory 2015, Rodell et al. 2016). Consequently, much of the research examining the relationship between CSR constructs and employee perceptions and behavior has analyzed the influence of CSR in hypothetical situations (Evans and Davis 2011, Rupp et al. 2013) or relied on cross-sectional and longitudinal studies to evaluate the relationship between CSR activities and employee attitudes (Aguinis and Glavas 2012, Hofman and Newman 2014) and behaviors (Jones 2010, Rodell 2013, Carnahan et al. 2017, Bode and Singh 2018, Gatignon 2022). Studies

examining the *causal* relationship between CSR-related constructs and employee behavior have done so in the empirical context of online labor marketplaces with temporary workers (Tonin and Vlassopoulos 2015; Burbano 2016, 2021a, b; List and Momeni 2020; Burbano and Chiles 2022). Thus, an opportunity exists to push the boundaries of the empirical examination of these relationships by examining the causal effects of a CSR activity on employee behavior and attitudes in a corporate setting with full-time employees.

3. Empirical Setting and Field Experimental Design

We collaborated with a bank headquartered in a large city in Latin America to explore whether a short-term corporate social impact activity could influence an outcome of interest to the bank: employee turnover. Employee turnover is an outcome of great interest to organizational scholars (Glebbeek and Bax 2004, Carnahan et al. 2012, Heavey et al. 2013, Li et al. 2022) given its impact on productivity (Shaw et al. 2005, Hausknecht et al. 2009, Argote et al. 2018), financial performance (Heavey et al. 2013), and firm survival (Agarwal et al. 2009). The collaborating organization is a prominent player in both wholesale and retail banking (with more than 400 retail branches), serves a wide range of customers (from individuals and small-sized companies to large corporations), and offers a large set of products and services. The bank had nearly 20,000 employees and over 6 million customers at the time the intervention was implemented. It also has operations in various Latin American countries and in the United States. With respect to its corporate social performance and CSR reputation, it can be considered average relative to other leading Latin American banks.³ We discuss the generalizability of our context and implications for other contexts in Section 12.

3.1. The Experiment

The field experiment was integrated into the official onboarding process for new employees, all of whom had been at the company for less than three months.⁴ As part of the onboarding process, all new employees were required to participate in two collective, day-long onboarding activities. The first was a day-long cultural induction. The second was a day-long event at a local bank branch, during which new employees met current employees and learned about basic bank transactions and activities. These onboarding activities took place once or twice a month. All new employees were required to complete all onboarding activities within four months of joining the bank, and they were given up to three opportunities to do so.

Participation in a third onboarding activity, a day-long social impact activity, was randomly assigned to a subset of new employees. Those who were randomly

assigned to receive the CSR “treatment” were sent an email calendar invitation from human resources (HR) informing them of a final activity required to complete the bank’s onboarding process. Importantly, it did not specify the nature of the onboarding activity (as was also the case for the first two onboarding activities).⁵ The email invitation used language and formatting mirroring that of the first two onboarding initiatives.⁶ Those who joined participated in a firm-organized social impact activity for the day (engaging with and coaching low-income high school students at a local high school; details of the activity are provided in the next section). Employees in the measurement-only control condition spent that time at work as usual and were not informed of others’ participation in the activity.

The bank took several steps to increase the likelihood of employees’ satisfaction with the social impact activity and their perception of the activity as impactful on themselves and its beneficiaries (low-income high school students).⁷ The logistics and details of the initiative were designed by a consulting company that specializes in CSR and corporate volunteering in collaboration with a team including the authors and bank employees from the larger HR and CSR divisions. Moreover, the intervention was pilot tested eight months prior to the launch of the full experiment on 20 employees who had prior experience with corporate volunteering. Qualitative interviews with the pilot participants indicated high levels of satisfaction with the intervention as well as high perceptions of the impact on themselves and the beneficiaries.

3.1.1. The Social Impact Activity “Treatment.” The day-long activity began with a welcome session at 9:00 a.m. In a 90-minute session, employees were informed of the planned structure for the day and received instructions for what to do during the social impact activity.⁸ Employees were then transported by bus to a public, low-income local high school. After arriving at the high school, they were introduced (as a group) to the student beneficiaries, and everyone participated in a short ice-breaker activity.⁹ After this initial interaction, each employee was paired with a student beneficiary. All beneficiaries were seniors in the same public, low-income high school.

Once paired, employees had 90 minutes to speak with and coach their assigned student. The coaching session was described to employees as intended to help the low-income students envision an attainable future after graduating from high school (graduation would take place five months later). This is relevant because low-income students from public high schools in this context face an extremely uncertain future.¹⁰ At the end of the coaching session, employees and student beneficiaries had lunch together (60 minutes). Students then left the physical space where the activities had taken place, whereas

employees stayed to take part in a 90-minute debriefing session. After self-reflecting on their experience for 15 minutes (in response to a series of questions like “How did it go?” and “What surprised you?”), participants discussed their experiences in groups of about 25 employees.¹¹

3.2. Postexperiment Data Collection

Two weeks after the intervention, a survey was administered by the bank’s training and development team to evaluate all new employees’ experiences with the onboarding process (including those who did not participate in the intervention). The survey also asked questions about the new employees’ perceptions of their employing organization, which we use in our analyses to explore the mechanisms behind the effects we observe. As the survey was administered in Spanish, we followed the standard academic procedures of translating and back translating (Brislin 1970) to translate all items used in the survey from English to Spanish. Note that, although employees might have felt pressure to report positive perceptions in a survey administered by their employer, this would be consistent across conditions, such that social desirability bias in survey response would not prevent us from making valid comparisons across conditions.

The response rate to the survey was relatively high, at 81.44%. Importantly, there were no statistical differences in observables between those who did and did not complete the survey.¹² We examined whether there was differential attrition by condition, and we found no statistically significant differences in propensity to complete the survey. In total, 81.65% of the employees in the Treatment group and 80.95% in the Control group responded to the survey ($p = 0.905$).¹³ Additionally, we ran an ordinary least squares (OLS) regression predicting the likelihood of completing the survey, including as covariates the condition dummy variable and observable employee characteristics, and we found no significant effects (see Appendix A). Overall, these analyses suggest that the survey information is missing at random.

The bank’s HR department provided demographic information on the employees, as well as employee turnover data, at a natural data collection point (the end of their first quarter of the year in April 2019). This was 10 months after the implementation of the intervention.¹⁴ By this time, most study participants had been working at the bank for over one year (69%).

Importantly, we evaluate *actual* turnover rates rather than self-reported intentions to remain at the organization. Existing work has more commonly used self-reported intentions when exploring the link between CSR and (intended) turnover (e.g., Jones 2010). Very few studies have used objective measures of outcomes, such as actual turnover rates, to evaluate the impact of CSR activities (for notable exceptions, see Bode et al. 2015, Carnahan et al. 2017, Gatignon 2022). Given that we also

captured self-reported intentions to remain at the organization in the survey, we explored whether self-reported intention to stay effectively predicted whether the employee actually stayed at the firm, and we found that there was a relatively low correlation between actual turnover and self-reported intention to stay ($r = 0.192$).¹⁵ This highlights the importance of capturing the actual behavioral outcome rather than the self-reported outcome given that employees do not always actually do what they say they will do or sometimes, even what they think they will do (for a review of the relationship between stated intent to turnover and actual turnover, see Wong and Cheng 2020).

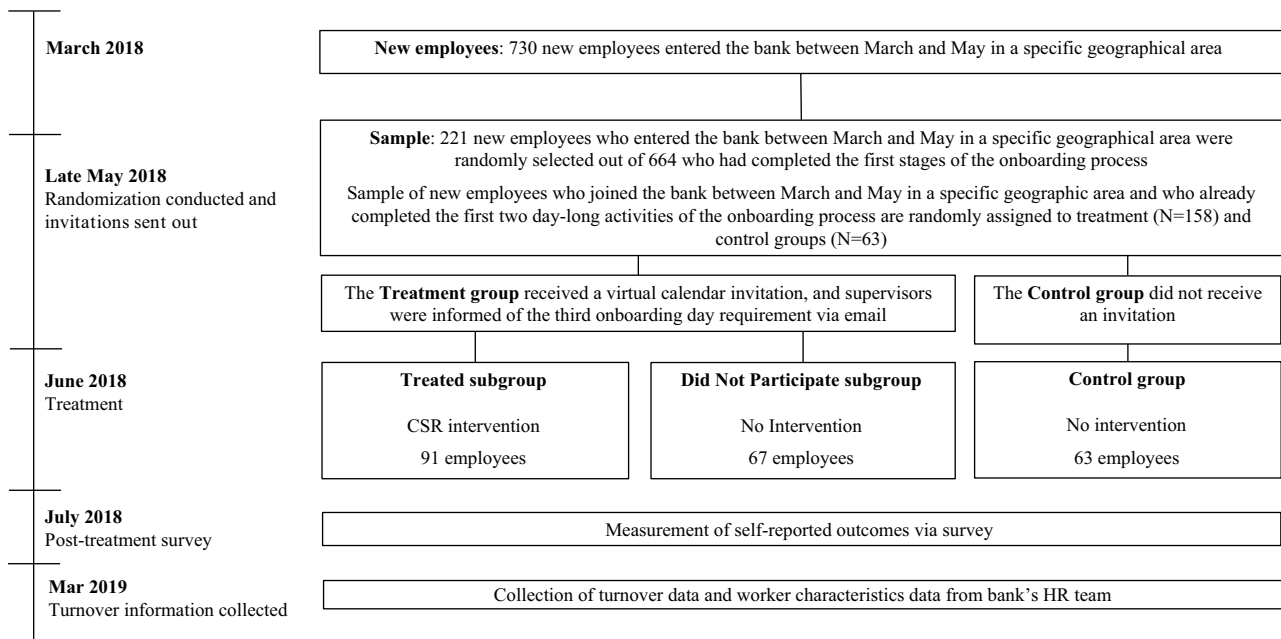
4. Sample

A randomly selected sample of employees who joined the bank in a large metropolitan area between March and May 2018 and who had completed the first two phases of the onboarding process were included in the experiment. In accordance with the collaborating bank’s wishes, one third of those who had completed the first two phases of the onboarding process (664) were included in the experimental sample: 221 new employees.¹⁶ The bank wanted to implement the social impact activity with close to 100 employees (because it had arranged for the high school coaching to be completed with up to 100 students at the local high school). Given prior data on the proportion of new employees who attend a given onboarding day, the bank organizers anticipated that not all employees randomly selected to participate would attend at the first opportunity.¹⁷ Based on the previous 12 months of onboarding participation data, an average of 59% of new employees participated in the onboarding activity at the first opportunity offered. Therefore, a participation rate of approximately 60% of employees who were invited to the third onboarding activity (the “intervention”) was expected. Given these projections and because the intervention was planned for up to 100 students (and thus, 100 employees), 158 employees (or 71% of all new employees in the study) were randomly selected to participate in the social impact activity, whereas 63 employees (29%) were randomly assigned not to participate. In line with expectations, 91 employees arrived for the third-day onboarding activity and thus, participated in the social impact activity (58% of those randomly selected). The various steps and corresponding sample sizes of the experiment are depicted in Figure 1.

5. Measures

5.1. Dependent Variable

Our dependent variable was *Turnover*. This was defined as a binary variable equal to one if the employee left the firm and zero if the employee remained at the firm at the point when the information was collected from the bank (10 months after the intervention).

Figure 1. Field Experiment Timeline

5.2. Independent Variable

We constructed *Treatment* and *Control* dummy variables to indicate the condition to which employees were randomly assigned. One represents that the employee was randomly assigned to the named group, whereas zero indicates that the employee was not.

5.3. Control Variables

We constructed variables based on several observable employee characteristics to ensure that our main results were robust to the inclusion of control variables for employee characteristics that could be correlated with preferences for or responses to CSR. Given that women have been shown to have greater preferences for social impact than men (Burbano et al. 2022), we included *Female*: a dummy variable that indicated whether employees were female (one) or male (zero). Younger individuals have been shown to (self-report that they) care more about social impact (Rosati et al. 2018). Thus, we included *Age*: a continuous variable that indicated the employee's age in years. Because compensation influences employee attitudes and other behaviors—including turnover and performance (Gupta and Shaw 2014)—and because prospective employees have been shown to be willing to accept lower wages to work with socially responsible firms (Burbano 2016), we included *Salary* as a control. *Salary* was a continuous variable that indicated the salary range of the employee (between 1 and 15). The bank did not share precise salary amounts, but they reported a salary range for each participant. These salary ranges were also highly correlated with the employee's rank in the bank. For example, 1 included an assistant with monthly earnings close to minimum wage

(U.S. \$270–\$300 per month), whereas 10 included an associate manager making somewhere between U.S. \$3,500 and \$4,500 per month. Given that most of the causal evidence for employee behavioral effects from CSR have been demonstrated in gig and short-term contract work settings (Burbano 2016, 2021a, b) and because managers typically employ CSR as a tool directed at long-term employees (Malos et al. 2018), we included the type of participants' contract as a control to roughly proxy for this difference in worker type. Specifically, *Undefined-term Contract* is a dummy variable that indicated whether the employee had a contract with an undefined term (one) or a fixed term (zero).

6. Sample Statistics and Randomization Balance

To assess the randomization balance in the study, we also utilized all employee-level demographic and work characteristics data that were provided by the bank (a broader set of variables than those included as control variables). Specifically, we included information on whether the employee was registered for the bank's health insurance plan (*Insurance*: 1 = registered, 0 = not registered) and whether they were *Single* (one) or not (zero). We also included five categorical variables. Chief Executive Officer (CEO) Report grouped the different teams or areas of employees into four categories based on their reporting path to the CEO: general management (including the office of the CEO and the board of directors), support (which includes all the support divisions at the bank, such as legal, corporate affairs, and human resources), banking (which includes the commercial and consumer banking teams), and risk

and financial planning (which includes the teams in charge of financial planning and risk assessment at the bank). *Rank* categorized employees into assistants, professional interns, analysts, managers (i.e., supervisors), and others (a category that includes all other employees, including lawyers and expert engineers). *Personnel Office* categorized employees into three groups depending on their human resource office. *Consumer Segment* indicated how the bank categorized employees as consumers. *Residence* indicated the area of the city in which employees lived.

Table 1 reports the means of all observable worker characteristics for the Treatment and Control groups. On average, employee participants were 25.32 years old, 43.44% of participants were women, and 90.5% were single. Moreover, 14.93% of employee participants held managerial positions, 27.15% were analysts, 23.08% were assistants, and 25.79% were paid professional interns. Analysis of variance (ANOVA) tests of the difference in means indicated that selection bias because of observables was minimal. Specifically, there were no statistically significant differences in observables

when comparing the two groups, suggesting that randomization was successful.

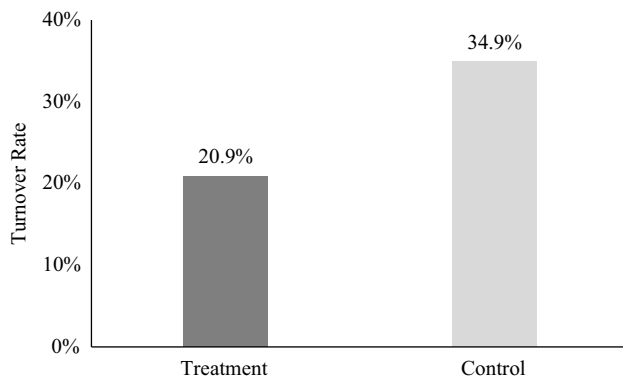
7. The Effect of the Short-Term Corporate Social Impact Activity on Employee Turnover

We present ITT effects in our analyses. In Figure 2, we illustrate turnover rates for the Treatment and Control groups of employees. Using OLS regression in Table 2, we report a statistically significant effect of the intervention on turnover ($\beta = 0.140, p = 0.029$). Employees randomly assigned to the Treatment group presented a notably lower turnover rate (20.9% or 33 of 158) than those in the Control group (34.9% or 22 of 63). This effect is robust to the inclusion of control variables in the OLS regression specification (see Model 2 in Table 2). A logistic regression analysis (see Models 3 and 4 in Table 2) indicated that employees randomly assigned to the social impact activity were 50.8% less likely to exit than those randomly assigned to the Control group ($p = 0.031$). The economic effects of these differences in turnover are consequential.

Table 1. Randomization Balance of Observable Characteristics

Variables	Treatment (N = 158)		Control (N = 63)		ANOVA	
	Mean	Standard deviation	Mean	Standard deviation	F statistic	p-value
<i>Undefined-term Contract</i>	0.506	0.502	0.524	0.503	0.05	0.816
<i>Salary</i>	6.044	3.545	6.143	3.564	0.03	0.852
<i>Female</i>	0.418	0.495	0.476	0.503	0.62	0.431
<i>Age</i>	25.070	4.437	25.968	6.370	1.42	0.235
<i>Insurance</i>	0.563	0.498	0.508	0.504	0.55	0.458
<i>Single</i>	0.911	0.285	0.889	0.317	0.26	0.609
<i>CEO Report</i>						
<i>General Management</i>	0.228	0.421	0.286	0.455	0.81	0.368
<i>Support</i>	0.418	0.495	0.333	0.475	1.34	0.248
<i>Banking</i>	0.241	0.429	0.270	0.447	0.21	0.651
<i>Risk and Financial Planning</i>	0.114	0.319	0.111	0.317	0.00	0.953
<i>Rank</i>						
<i>Assistant</i>	0.228	0.421	0.238	0.429	0.03	0.871
<i>Professional Intern</i>	0.259	0.440	0.254	0.439	0.01	0.933
<i>Analyst</i>	0.266	0.443	0.286	0.455	0.09	0.765
<i>Other</i>	0.095	0.294	0.079	0.272	0.13	0.717
<i>Manager</i>	0.152	0.360	0.143	0.353	0.03	0.866
<i>Personnel Office</i>						
1	0.627	0.485	0.587	0.496	0.29	0.590
2	0.108	0.311	0.111	0.317	0.01	0.940
3	0.266	0.443	0.302	0.463	0.29	0.593
<i>Consumer Segment</i>						
<i>Low Income</i>	0.139	0.347	0.127	0.336	0.06	0.811
<i>Young Consumer</i>	0.456	0.500	0.460	0.502	0.00	0.951
<i>High Potential Young Consumer</i>	0.361	0.482	0.349	0.481	0.03	0.872
<i>Top Consumer</i>	0.044	0.206	0.063	0.246	0.35	0.556
<i>Residence</i>						
<i>Modern</i>	0.519	0.501	0.476	0.503	0.33	0.568
<i>North</i>	0.184	0.388	0.206	0.408	0.15	0.698
<i>South</i>	0.114	0.319	0.127	0.336	0.07	0.787
<i>Center</i>	0.089	0.285	0.095	0.296	0.02	0.877
<i>East</i>	0.095	0.294	0.095	0.296	0.00	0.995

Note. p-value is the p-value from a one-way ANOVA test (probability of groups having different means) comparing the Treatment group vs. Control group.

Figure 2. Turnover Rate by Condition

8. Effects of the Intervention on Self-Reported Outcomes and Exploration of Potential Mechanisms

Given the strong and robust effects we observed on turnover, we next use data from the self-reported survey to explore potential mechanisms driving these effects. We explore perceptual factors that existing literature suggests could theoretically be affected by CSR and that, in turn, could lead to firm-benefitting employee behavior, such as increased likelihood to stay at an organization because of organizational identification, perception of organizational justice, perception of person-organization fit, and (reduced) work stress.

Organizational identification can be conceptualized as a type of social identification wherein individuals define themselves in terms of the organizations to which they

belong (Mael and Ashforth 1992). Employees' perceptions of the social responsibility of their employer are consistently and strongly related to their organizational identification (Zhao et al. 2022), such that we might expect a social impact activity to positively affect organizational identification. Organizational identification has, in turn, been linked to positive employee outcomes, such as turnover intentions, job performance, and absenteeism (for a meta-analytic review, see Ricketta 2005), such that we might expect a mediating effect on turnover.

Perception of organizational justice refers to employees' perceptions of how they and other employees are treated by the organization (Rupp 2011). According to organizational justice theory, employees' assessments of ethical treatment compel them "to respond not only to the treatment that they experience themselves but also to the observed third-party treatment of others" (Rupp and Mallory 2015, p. 223). Based on this aspect of the justice theory, researchers have theorized and found that employees' overall perceptions of workplace fairness are influenced by their perceptions of the level of social responsibility their firm demonstrates toward external stakeholders (e.g., Rupp et al. 2013, De Roeck et al. 2014). When employees participate in a social impact activity at work, their perceptions of their employer's sense of social responsibility are likely to be positively affected, which could have spillover effects on their perceptions of organizational justice. For example, the perception that one's employer cares for and behaves appropriately, fairly, and responsibly with the community and/or environment can lead employees to infer that the employer is also more likely to care for, respect, and treat them (and other employees) well (Burbano 2016). Perceptions of organizational justice have, in turn, been shown to lead to firm-benefitting employee outcomes like turnover intentions, performance improvement, and job satisfaction (for a meta-analytic review, see Cohen Charash and Spector 2002), such that we might observe a mediating effect on turnover.

Person-organization fit refers to "the compatibility between people and organizations that occurs when at least one entity provides what the other needs, they share similar fundamental characteristics, or both" (Kristof 1996, p. 45). Researchers have shown that CSR can influence individuals' perceptions of P-O fit (Jones et al. 2014). Moreover, P-O fit has been shown to relate to important individual-level organizational outcomes, including task performance, turnover, and organizational citizenship behaviors (for a meta-analytic review, see Hoffmann and Woehr 2006). Thus, we examine whether P-O fit might function as a mechanism linking the effects of the intervention on employee turnover.

Finally, we examine the effects on employees' perceptions of *stress at work*. Work stress can be defined as employees' experience of work situations in which demands are perceived to exceed the resources possessed to deal with

Table 2. Effects of a Short-Term Social Impact Activity on Turnover

	Turnover			
	Model 1	Model 2	Model 3	Model 4
<i>Treatment vs. Control</i>	0.140* (0.064)	0.144** (0.061)	0.492** (0.162)	0.432* (0.155)
<i>Undefined-term Contract</i>	—	0.145 (0.106)	—	0.383 (0.251)
<i>Salary</i>	—	0.025 (0.016)	—	0.876 (0.080)
<i>Female</i>	—	0.034 (0.055)	—	0.827 (0.283)
<i>Age</i>	—	0.004 (0.007)	—	1.022 (0.043)
<i>Constant</i>	0.349** (0.054)	0.496** (0.161)	0.537* (0.142)	1.130 (1.159)
<i>R²</i>	0.022	0.135	0.018	0.1256
<i>Number of employees</i>	221	221	221	221

Notes. Coefficients of OLS regressions are in Models 1 and 2. Odds ratios of logistic regression are in Models 3 and 4. Standard errors are in parentheses.

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

them (Lazarus 1966, 1999; Lazarus and Folkman 1984). Stress occurs in the context of the person-environment relationship (Lazarus 1966, 1999; Ganster and Rosen 2013), and the work environment is certainly one that can generate anxiety, pressure, and tension for individuals. A social impact activity could arguably alter employees' perceptions of the firm in a manner that reduces their perception of the degree of the (stressful) demands. First, by increasing perceptions of justice and fairness at work, a social impact activity could help mitigate employees' perceptions of the stressful demands being faced at work (Judge and Colquitt 2004). A sense of (stressful) demands at work is commonly experienced when employees are exposed to events that trigger perceptions of injustice at work (e.g., Elovainio et al. 2001, Judge and Colquitt 2004). Empirical studies have shown that increased perceptions of fairness can reduce employees' perceptions of the degree of stress in their work environment (e.g., Vermunt and Steensma 2001). Second, by strengthening employees' identification with the firm, the activity could aid employees in effectively dealing with stressful work demands. Researchers have regularly reported that organizational identification is strongly and inversely correlated with work stress (see Steffens et al. 2017). In turn, stress has been shown to relate to important outcomes in organizations, including job performance (Gilboa et al. 2008) and turnover intentions.

8.1. Measures

We construct proxies for these perceptual constructs from the posttreatment survey as follows.

8.1.1. Organizational Identification. Employees' sense of identification with the bank was measured using the five-item scale developed by Mael and Ashforth (1992). Agreement with statements was measured using a seven-point Likert scale, and the overall score was calculated by adding the scores of the five items: "When someone criticizes [name of the bank], it feels like a personal insult," "I am very interested in what others think about [name of the bank]," "When I talk about [name of the bank], I usually say we rather than they," "When someone praises [name of the bank], it feels like a personal compliment," and "[name of the bank]'s successes are my successes" ($\alpha = 0.86$).

8.1.2. Organizational Justice. We employed two items to measure employees' perceptions of how fairly and well the employing firm treats them and other employees; both items were adapted from the items of Burbano (2016) measuring justice perceptions regarding a CSR message. Agreement with the statements was measured on a seven-point Likert-type scale, and the overall score was calculated by adding the scores of the two items: "I believe that [name of the bank] treats its employees

fairly" and "I believe that [name of the organization] treats its employees well" ($\alpha = 0.92$).

8.1.3. Person-Organization Fit. We used a three-item measure of P-O fit adapted from Saks and Ashforth (2002). Employees responded to each question on a 100-point scale, where 1 = to a very little extent and 100 = to a very large extent. For ease of comparability with the other measures collected, we converted the score to a seven-point scale. The overall P-O fit score was calculated by adding the scores of three items: "To what extent are the values of the bank similar to your own values?," "To what extent does your personality match the personality or image of the organization?," and "To what extent is the organization a good match for you?" ($\alpha = 0.90$).

8.1.4. Work Stress. *Work stress* was measured using the Schaubroeck et al. (1989) three-item scale. Agreement with the statements was measured on a seven-point Likert-type scale, and an overall work stress score was calculated by adding the scores across all three items: "General aspects of [name of the bank] tend to cause me a great deal of stress and anxiety," "My job causes me a great deal of personal stress and anxiety," and "Relations with the people I work with cause me a great deal of stress and anxiety" ($\alpha = 0.89$).¹⁸

8.2. Preliminary Analysis

We first conducted exploratory factor analysis to assess the validity of the self-reported survey measures used. An unrotated factor analysis generated four factors with eigenvalues over 1.00, and the first factor explained 38.48% of the variance (see Appendix B). To make the factors interpretable, we reran the factor analysis with four factors using a Varimax rotation (see Appendix C). Results showed items from each scale loading onto different factors, providing evidence of the discriminant validity of the scales included in the study. Because we expected the measures to be correlated with each other, we also reran the factor analysis with four factors using an oblique Promax rotation and found results consistent with those from the orthogonal Varimax rotation (see Appendix D). These analyses suggest that items in each measure do indeed correspond to different constructs (i.e., that there is divergent validity). Bivariate correlations among variables for comparison groups, control variables, turnover, and self-reported survey variables are presented in Table 3.

8.3. The Main Effects of the Intervention on Employees' Self-Reported Perceptions

We first examined whether the intervention influenced self-reported employee perceptions. Table 4 reports the means and standard deviations of all self-reported outcome variables for the two main groups of employees in our analysis. To examine whether these comparisons are

Table 3. Correlation Matrix

Variables	1	2	3	4	5	6	7	8	9	10
1 Treatment vs. Control	—									
2 Undefined-term Contract	0.016 221	—								
3 Salary	0.013 221	0.855** 221	—							
4 Female	0.053 221	0.017 221	0.031 221	—						
5 Age	0.080 221	0.507** 221	0.572 221	0.006 221	—					
6 Turnover	0.147* 221	0.317** 221	0.320** 221	0.040 221	0.145* 221	—				
7 Organizational Identification	0.071 180	0.084 [†] 180	0.138 180	0.162* 180	0.195** 180	0.193** 180	0.86			
8 Organizational Justice	0.175* 178	0.035 178	0.063 178	0.048 178	0.018 178	0.29** 178	0.452** 178	0.918		
9 Person-Organization Fit	0.187* 178	0.088 178	0.087 178	0.022 178	0.127 [†] 178	0.227** 178	0.486** 178	0.451** 178	0.898	
10 Work Stress	0.107 180	0.077 180	0.080 [†] 180	0.084 180	0.059 180	0.142** 180	0.148* 180	0.390** 178	0.341** 178	0.89

Notes. The α coefficients are displayed on the diagonal. Sample sizes are displayed under each correlation. Treatment vs. Control = [Treatment (1), Control (0)]. Undefined-term Contract = [Undefined Term (1), Fixed Term (0)]. Female = [Female (1), Male (0)]. Turnover = [Left Company (1), Still at Company (0)].

* $p < 0.05$; ** $p < 0.01$; [†] $p < 0.10$.

robust to the inclusion of control variables, Table 5 reflects OLS regression analyses. We found a directionally positive but not statistically significant effect of the intervention on employees’ organizational identification without ($\beta = 0.814, p = 0.342$) and with controls ($\beta = 1.028, p = 0.220$). Examining the effects of the intervention on employees’ perceptions of organizational justice or fairness, we found a statistically significant effect both without ($\beta = 0.543, p = 0.020$) and with ($\beta = 0.546, p = 0.02$) control variables.

We also examined the effects of the intervention on P-O fit (see Table 5). We found a significant effect of the intervention on P-O fit ($\beta = 1.141, p = 0.013$), an effect that was robust to the inclusion of control variables ($\beta = 1.218, p = 0.008$). Finally, we examined the effects on employees’ perceptions of stress at work. We did not find a statistically significant effect of the intervention on employees’ work stress, although the coefficients were

in the expected direction and could have been underpowered because of our sample size ($\beta = 1.100, p = 0.151$ without controls; $\beta = 1.158, p = 0.134$ with controls).

8.4. Mediation Analyses

After finding that the intervention had a significant effect on employees’ perceptions of organizational justice and P-O fit, we explored whether either of these two employee perceptions were driving the effect of the intervention on turnover. We first ran simple mediation tests to examine whether each perception, independently, explained the effects of the intervention on turnover (Baron and Kenny 1986, VanderWeele and Vansteelandt 2009). Specifically, we conducted path analyses and decomposed the effects into direct, indirect, and total effects; a statistically significant indirect effect (IE) suggests that the effect of the intervention on

Table 4. Summary Statistics of Self-Reported Outcome Variables by Employee Group

Variables	Treatment		Control		ANOVA	
	Mean	Standard deviation	Mean	Standard deviation	F statistic	p-value
Turnover	0.209	0.408	0.349	0.481	4.81	0.029*
Organizational Identification	30.147	4.980	29.333	5.606	0.91	0.342
Organizational Justice	13.063	1.315	12.520	1.542	5.54	0.020*
Person-Organization Fit	19.037	2.562	17.896	3.076	6.35	0.013*
Work Stress	6.527	4.524	7.627	4.841	2.08	0.151

Note. p-values = one-way ANOVA p-values comparing the Treatment group vs. Control group.

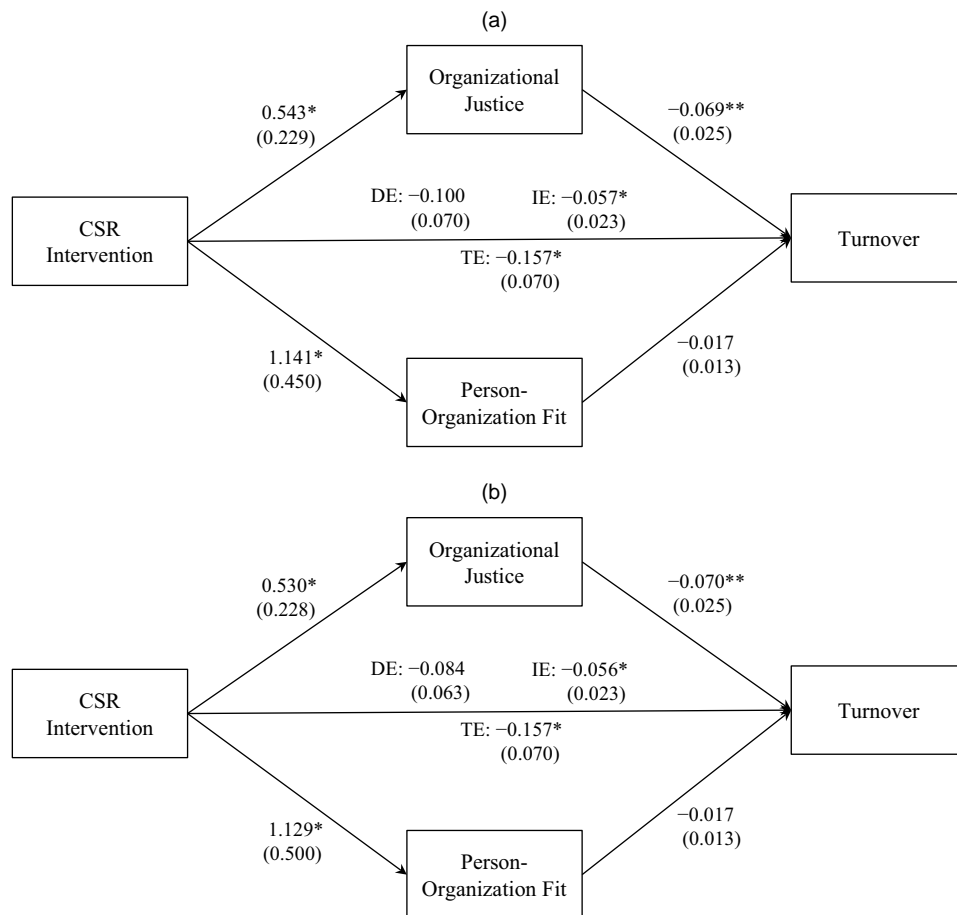
* $p < 0.05$.

turnover is indeed explained by the mediator (Holland et al. 2017).¹⁹ Because some participants did not respond to the posttreatment survey, we implemented two versions of these simple mediation tests: maximum likelihood (ML) and maximum likelihood missing values (MLMV) estimations. When using ML, observations without complete information are excluded from the analysis. Thus, in the ML analyses, employees for whom turnover information was collected but who did not complete the posttreatment survey during which proxies for the mechanisms were gathered are excluded from the analysis. In the MLMV analyses, also known as full information maximum likelihood analyses, all observations are included. This method does not impute missing values. Rather, the MLMV estimation adjusts the likelihood function (i.e., case-wise likelihood function) so that each observation contributes all the information available in the data set (Enders and Bandalos 2001, Medeiros 2016). Coefficients of both estimators (ML and MLMV) should be read the same way as OLS coefficients. Table 6 reports the results of the simple mediation analyses of organizational justice and P-O fit on turnover independently. Model 1 in panel A represents that employees' perception of organizational justice fully mediates the effect of the intervention on turnover; we observe a

nonsignificant direct effect (DE; $\beta = 1.112, p = 0.108$) and a significant IE (IE: $\beta = 0.045, p = 0.046$). This result was robust to the use of MLMV (see Model 2 in panel A in Table 6). Model 1 in panel B reflects a marginal and partial mediation effect of P-O fit on the effect of the CSR treatment on turnover: a marginally significant direct effect (DE: $\beta = 1.121, p = 0.088$) and a marginally significant indirect effect (IE: $\beta = 0.036, p = 0.062$). This result was also robust to the use of MLMV (see Model 2 in panel B in Table 6).

Given that each variable presented at least a marginally significant effect when examined separately, we next included both organizational justice and P-O fit as concurrent mediators of the effects of treatment on turnover. Again, we used both ML and MLMV to estimate these multiple mediation models. As shown in Figure 3, the only variable that drove the effect of the intervention on turnover was employees' perception of organizational justice. P-O fit had a nonsignificant direct effect on turnover, suggesting that the effect uncovered in the simple mediation analysis could have been because of the shared variance between P-O fit and organizational justice. Taken together, these results provide strong evidence in support of a mediating effect of organizational justice in explaining the effect of the intervention on

Figure 3. Results of Multiple Mediation Models



Notes. (a) Maximum likelihood (n = 178). (b) Maximum likelihood missing values (n = 221). TE indicates coefficients of total effect of IV on DV. *p 0.05; **p 0.01.

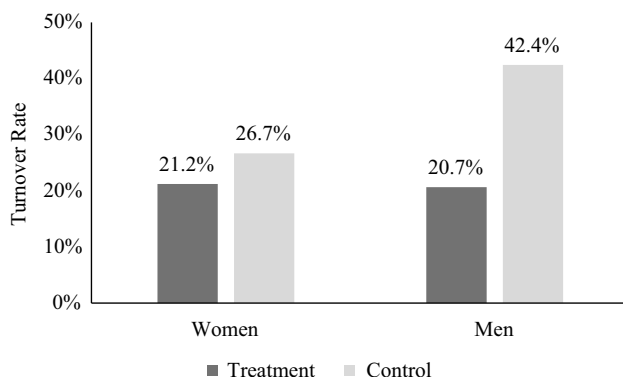
employee turnover. Our results suggest that a social impact activity acts as a separate event from the day-to-day routine that causes employees to engage in sensemaking and influences their perceptions of the organizational justice of their employer and that this change in perception is (at least partially) responsible for the effect on turnover.

9. Exploration of Heterogeneous Effects

We explored whether the treatment effect on turnover was driven by, or was more pronounced for, specific types or groups of employees. On one hand, women are more likely to value social impact (Kish-Gephart et al. 2010, Barbulescu and Bidwell 2013) and a sense of meaning at work derived from social impact (Burbano et al. 2022) than men. This might suggest that women should be more responsive to any type of CSR treatment. On the other hand, literature on wise interventions suggests that individuals for whom an intervention is more novel or nonroutine would be more likely to engage in “sensemaking” after participating in it, potentially resulting in larger perceptual changes and in turn, stronger effects. Given that women have been shown to be more likely to engage in prosocial and communal activities both inside and outside of work than men (Einolf 2011), an intervention in which employees are exposed to a social, communal activity could thus act as a greater “shock” or “event,” resulting in more sensemaking among men than women. This would, in turn, translate into the social impact activity having stronger effects on men than women.

We find suggestive evidence for the latter argument. In Figure 4, we present turnover rates by gender for both Treatment and Control groups. Although we do not find a statistically significant moderating effect of gender on turnover (see Table 7), possibly because of the relatively small sample size, it is notable that the turnover rate for male employees was 20.65% in the Treatment group compared with 42.42% in the Control group. By contrast, the turnover rate for female employees was 21.21% in the Treatment group and 26.67%, in the Control group. Table 7 reports OLS regression analyses that replicate the specifications in Tables 2 and 5 with the addition of an

Figure 4. Turnover Rate by Condition and Gender



interaction covariate between the treatment and the gender dummy variables. The results suggest that gender (at least marginally) moderates the effects of the intervention on several of the measured perceptual outcomes—organizational identification, organizational justice, P-O fit, and work stress. These results suggest that women’s perceptions about the organization were *less* affected by the intervention.

We further examined differences in responses to the intervention by gender by splitting our sample into male and female employees and replicating our treatment analyses (see Appendix E). Although these results should be interpreted with caution because of the relatively small sample sizes, the split sample analysis further suggests that the intervention was more effective in influencing male employees compared with their female counterparts. In fact, these results suggest that effect on the male subsample appears to be driving the average treatment effects of the intervention. Notably, male employees’ perceptions of stress as work were reduced by the intervention ($\beta = 2.556, p = 0.026$), whereas female employees’ perceptions of stress at work were not directionally negative nor statistically significant ($\beta = 0.680, p = 0.467$). Because men at a baseline tend to have lower levels of organizational identification, organizational justice, and perceptions of fit with their employers as well as experience more stress than women, a CSR intervention that affects these individual-level perceptions is more of a “shock” for men than women.

Another heterogeneous effect we explore is whether the effects are more pronounced for contract employees versus long-term employees.²⁰ Consistent with the logic that a social impact activity would have a greater effect on employees for whom it would serve as a greater “shock” or “event,” we surmised that, because CSR activities are more commonly offered to full-time, long-term employees and because contract workers have higher turnover rates, we might expect contract workers to be more affected by the intervention. We found no evidence that the type of contract moderated the effect of the intervention on turnover, self-reported intention to stay two weeks after the intervention, organizational identification, organizational justice, or work stress (see Table 8). We only found that type of contract moderated the effect of the intervention on participants’ perceptions of fit with the bank, such that contract workers in the Treatment group reported higher levels of P-O fit than those in the Control group.

10. Treatment on the (Instrumented) Treated Analysis

Our main analyses reported IIT comparisons. For our study, these are arguably the most conservative estimates given that some individuals who were randomly assigned to participate in the social impact activity did

Table 5. Effects of a Short-Term Social Impact Activity on Employee Perceptions

	<i>Org. Identification</i>		<i>Org. Justice</i>		<i>P-O Fit</i>		<i>Work Stress</i>	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
<i>Treatment vs. Control</i>	0.814 (0.854)	1.028 (0.835)	0.543** (0.230)	0.546* (0.232)	1.141* (0.453)	1.218** (0.455)	1.100 (0.763)	1.158 (0.769)
<i>Undefined-term Contract</i>	—	1.528 (1.535)	—	0.312 (0.432)	—	0.358 (0.844)	—	0.169 (1.413)
<i>Salary</i>	—	0.228 (0.237)	—	0.088 (0.068)	—	0.044 (0.134)	—	0.067 (0.218)
<i>Female</i>	—	1.674* (0.759)	—	0.141 (0.211)	—	0.174 (0.412)	—	0.787 (0.699)
<i>Age</i>	—	0.187 (0.097)	—	0.026 (0.029)	—	0.086 (0.057)	—	0.021 (0.090)
<i>Constant</i>	29.333** (0.723)	23.137** (2.256)	12.520** (0.195)	12.747** (0.667)	17.896** (0.384)	15.678** (1.304)	7.627** (0.646)	9.042** (2.077)
<i>R²</i>	0.005	0.076	0.031	0.045	0.035	0.057	0.012	0.026
<i>Number of employees</i>	180	180	178	178	178	178	180	180

Notes. Coefficients of OLS regressions are shown. Standard errors are in parentheses.

p* 0.05; *p* 0.01.

not actually do so. In Tables 9 and 10, we also report the results of a TO(I)T analysis, wherein random assignment to conditions was used as an instrumental variable for treatment to obtain an unbiased effect of treatment on the treated individuals.²¹ The TO(I)T analysis is particularly useful for evaluating the treatment effects of programs or interventions that have been partially implemented (Angrist et al. 1996) or that have allowed participants to “cross over from one condition to another in an uncontrolled way” (Shadish and Cook 2009, p. 612). Thus, the TO(I)T analysis speaks to the effectiveness of the intervention or treatment on those who participated in or received it (for an example of the use of TO(I)T analyses, see Kast et al. 2016). As expected, given that a portion of employees randomly assigned to the social impact activity did not participate in it, results from the TO(I)T are slightly

stronger (as indicated by regression coefficients and *R*² values) than the results presented based on the ITT analysis (see Tables 9 and 10).

11. Discussion of Potential Alternative Explanations

A potential issue in interpreting our effects is that they could be driven by other factors not specifically linked to the social responsibility-related aspect of employee participation in the intervention (and therefore, could be achieved by other non-CSR-related means). To assess the likelihood that the effects are linked to the social responsibility aspect of the intervention, we used a measure of employees’ perceptions of the social responsibility of the bank.²² As shown in Table 11, two weeks after the CSR treatment, employees in the Treatment group reported

Table 6. Results of Simple Mediation Analyses

	Turnover					
	Model 1			Model 2		
	DE	IE	TE	DE	IE	TE
	Panel A					
<i>Organizational Justice as Mediator</i>	0.112 (0.069)	0.045* (0.023)	0.157* (0.071)	0.096 (0.063)	0.044* (0.022)	0.140* (0.064)
<i>R²</i>		0.097			0.093	
<i>Number of employees</i>		178			221	
	Panel B					
<i>Person-Organization Fit as Mediator</i>	0.121 [†] (0.071)	0.036 [†] (0.019)	0.157* (0.071)	0.104 (0.064)	0.036 [†] (0.019)	0.140* (0.064)
<i>R²</i>		0.067			0.062	
<i>Number of employees</i>		178			221	

Notes. DE indicates coefficients of direct effect of IV (intent to treat) on DV (turnover). IE indicates coefficients of indirect effect of IV on DV through mediating variable. TE indicates coefficients of total effect of IV on DV. Model 1 used ML, and Model 2 MLMV.

**p* 0.05; [†]*p* 0.10.

Table 7. Moderation Analyses by Gender

	Turnover	Org. Identification	Org. Justice	P-O Fit	Work Stress
Treatment vs. Control	0.218* (0.087)	2.180 [†] (1.124)	0.908** (0.309)	2.010** (0.605)	2.556* (1.011)
Female	0.158 (0.108)	3.828** (1.429)	0.728 [†] (0.390)	1.543* (0.764)	3.123* (1.285)
Treatment vs. Control × Female	0.163 (0.129)	2.966 [†] (1.692)	0.802 [†] (0.462)	1.928* (0.904)	3.235* (1.521)
Constant	0.424** (0.075)	27.607** (0.960)	12.185** (0.265)	17.186** (0.518)	9.036** (0.863)
R ²	0.031	0.049	0.050	0.060	0.044
Number of employees	221	180	178	178	180

Notes. Coefficients of OLS regressions are shown. Standard errors are in parentheses.

* $p < 0.05$; ** $p < 0.01$; [†] $p < 0.10$.

higher levels of perceptions of CSR than employees in the Control group ($\beta = 0.851$, $p = 0.006$). This suggests that, as we expected, the intervention did indeed change employees' perceptions of the social responsibility of the employing organization.

Furthermore, to examine the relevance of this change in perception of the social responsibility of the organization on turnover, we reran the multiple mediation model described in Section 8.4 with perception of CSR as the independent variable—IV (rather than treatment versus control as the IV). As shown in Figure 5, perception of CSR had a direct effect on both potential mediators, and organizational justice was the only potential mediator with a significant path to turnover. Moreover, we found a significant mediation effect on turnover. Specifically, as illustrated in Figure 5(a), we found a nonsignificant direct effect of perception of CSR on turnover ($\beta = 0.017$, $p = 0.450$), a significant indirect effect on turnover ($\beta = 0.041$, $p = 0.007$), and a significant total effect ($\beta = 0.057$, $p = 0.001$). Results remained largely unchanged when using MLMV as an estimation method to incorporate all available information on turnover (see Figure 5(b)). These results provide further support for the notion that the social responsibility aspect of the intervention was indeed critical in driving effects on employee outcomes.²³

Another possible explanation could emerge from potential spillover effects, which would happen if the actions or behavior of employees in the Treatment condition influenced the outcomes of employees in the Control condition (Vazquez-Bare 2022). Specifically, we could observe spillover effects because we could not prevent communication between employees who did and did not receive an invitation to the third onboarding activity that served as our treatment. On one hand, one could argue that employees randomly assigned to the Control group learned that others were invited to a third onboarding activity and did not like that they were excluded, resulting in negative feelings toward the firm that could lead to adverse perceptual and behavioral effects. On the other hand, one could also argue for positive spillover effects. Because employees in the Control group found out about a community engagement activity implemented by the bank, their perception of the bank's social responsibility could have been favorably influenced. Although we cannot completely rule out spillover effects, we turned to our data to consider the merits of these alternative explanations. Here, it is important to note that we observed no statistical differences in employee outcomes between employees in the control condition and

Table 8. Moderation Analyses by Type of Contract

	Turnover	Org. Identification	Org. Justice	P-O Fit	Work Stress
Treatment vs. Control	0.200* (0.088)	1.339 (1.243)	0.820* (0.338)	2.255** (0.755)	2.253* (1.106)
Undefined-term Contract	0.352** (0.103)	1.574 (1.450)	0.480 (0.393)	2.449** (0.650)	2.278 [†] (1.291)
Treatment vs. Control × Undefined-term Contract	0.106 (0.122)	0.980 (1.713)	0.515 (0.463)	2.430** (0.890)	2.175 (1.524)
Constant	0.533** (0.074)	28.500** (1.055)	12.261** (0.288)	16.678** (0.555)	8.833** (0.939)
R ²	0.126	0.014	0.039	0.083	0.029
Number of employees	221	180	178	178	180

Notes. Coefficients of OLS regressions are shown. Standard errors are in parentheses.

* $p < 0.05$; ** $p < 0.01$; [†] $p < 0.10$.

Table 9. Results of Treatment on the (Instrumented) Treated Analysis: Models 1–8

	Turnover		Org. Identification		Org. Justice	
	Model 1	Model 2	Model 5	Model 6	Model 7	Model 8
<i>Treatment vs. Control</i>	0.244* (0.111)	0.248* (0.105)	1.313 (1.350)	1.658 (1.302)	0.868** (0.362)	0.866** (0.357)
<i>Undefined-term Contract</i>	—	0.128 (0.106)	—	1.725 (1.499)	—	0.454 (0.423)
<i>Salary</i>	—	0.025 (0.016)	—	0.239 (0.229)	—	0.103 (0.066)
<i>Female</i>	—	0.020 (0.055)	—	1.622* (0.734)	—	0.103 (0.204)
<i>Age</i>	—	0.004 (0.007)	—	0.188* (0.094)	—	0.032 (0.028)
<i>Constant</i>	0.349** (0.054)	0.475** (0.158)	29.333** (0.709)	23.174** (2.172)	12.520** (0.192)	12.880** (0.630)
<i>R²</i>	0.013	0.112	0.033	0.106	0.056	0.071
<i>Number of employees</i>	221	221	180	180	178	178

Notes. Coefficients of two-stage least squares are shown. Standard errors are in parentheses.

p* 0.05; *p* 0.01.

those who were invited to participate and did not (see the last column of Table F.1 in Appendix F). If a feeling of exclusion among Control group employees was driving our effects, we would expect them to exhibit more negative attitudes and behaviors toward the firm than the group of employees who were invited to participate but could not, but we saw no evidence of this or the opposite in our data.

Lastly, one could be concerned with the small number of employees randomly selected to be in the no-treatment control condition. A 34.9% turnover rate (22 of 63 employees) might seem high, and if these Control group employees

care not representative of the broader employee population, this could influence our results. To address this concern, we turn to the data from the employees who started at the company during the same three months as employee participants but were not randomly selected to be part of the study. The turnover rate of this group of 509 employees was 37.92% (193 employees left the organization). There were no statistical differences in turnover between Control group employees and those who were not randomly selected to be part of the study ($F = 0.21, p = 0.644$), suggesting that the turnover rate of the Control group employees is indeed representative.

Table 10. Results of Treatment on the (Instrumented) Treated Analysis: Models 9–12

	<i>P-O Fit</i>		<i>Work Stress</i>	
	Model 9	Model 10	Model 11	Model 12
<i>Treatment vs. Control</i>	1.825* (0.715)	1.931** (0.707)	1.774 (1.217)	1.868 (1.214)
<i>Undefined-term Contract</i>	—	0.043 (0.838)	—	0.052 (1.397)
<i>Salary</i>	—	0.010 (0.132)	—	0.079 (0.214)
<i>Female</i>	—	0.091 (0.404)	—	0.728 (0.684)
<i>Age</i>	—	0.074 (0.056)	—	0.022 (0.088)
<i>Constant</i>	17.896** (0.379)	15.973** (1.248)	7.627** (0.639)	9.000** (2.025)
<i>R²</i>	0.049	0.061	0.023	0.034
<i>Number of employees</i>	178	178	180	180

Notes. Coefficients of two-stage least squares are shown. Standard errors are in parentheses.

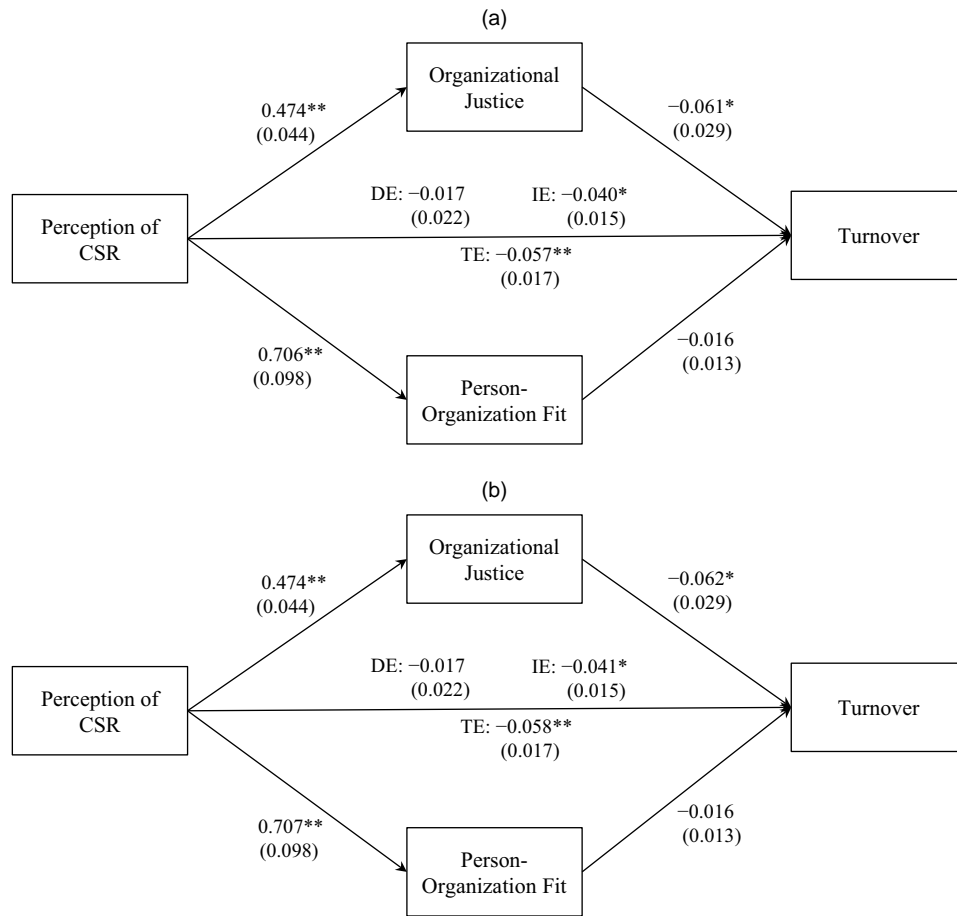
p* 0.05; *p* 0.01.

Table 11. Effects of the Intervention on Perception of CSR

	<i>Perception of CSR</i>	
	Model 1	Model 2
<i>Treatment vs. Control</i>	0.851** (0.304)	0.860** (0.304)
<i>Undefined-term Contract</i>	—	0.156 (0.565)
<i>Salary</i>	—	0.084 (0.089)
<i>Female</i>	—	0.070 (0.276)
<i>Age</i>	—	0.028 (0.038)
<i>Constant</i>	19.180** (0.257)	19.263** (0.872)
<i>R²</i>	0.043	0.070
<i>Number of employees</i>	178	178

Notes. Coefficients of OLS regressions are shown. Standard errors are in parentheses.

***p* 0.01.

Figure 5. Results of the Multiple Mediation Model with Perception of CSR as IV

Notes. (a) Maximum likelihood ($n = 178$). (b) Maximum likelihood missing values ($n = 221$). TE indicates coefficients of total effect of independent variable (IV) on dependent variable (DV).

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

These analyses help rule out potential alternate explanations, and we discuss avenues for future research to examine these potential alternative explanations directly in Section 12.3. Although we cannot completely rule out the possibility that non-CSR-related aspects of the intervention may have contributed in part to the effects we observed, we argue that this possibility does not diminish the importance of having causally identified that a one-time, short-term social impact activity (which is relatively inexpensive for a firm to implement) can have long-lasting, downstream behavioral effects on employees that are beneficial for firms.

12. Discussion

To our knowledge, our paper is the first to examine the causal effects of a short-term social impact activity on important employee perceptual and behavioral outcomes in a natural company setting. We found evidence that a one-time intervention can reduce employee turnover, a critical human capital outcome for firms. We show that a social impact activity can positively influence how

employees interpret their relationships with their employer, altering their perceptions of justice and fit within their organization. Moreover, we show that changes to employees' sense of organizational justice (but interestingly, not their identification or perceptions of fit) explain the effects of the intervention on turnover. We also found that the intervention had stronger effects on men than women. Notably, we found that a social impact activity can decrease male employees' perceptions of work-related stress, an important employee outcome from both an employee well-being perspective and from a strategic human capital management perspective (e.g., Sullivan and Bhagat 1992, Lee and Mitchell 1994, Kivimaki et al. 2002, Podsakoff et al. 2007, Goh et al. 2016, Hassard et al. 2018, Kembro et al. 2022) that has not been explored in the broader CSR literature to date.

12.1. Contributions

Our paper makes several important contributions. First, we contribute to the literature examining the implications of CSR from a human capital management perspective (e.g., Burbano 2016, Carnahan et al. 2017, Bode

and Singh 2018) by providing causal evidence that a one-time, short-term social impact activity can reduce employee turnover—an important behavioral employee outcome that is critical to the firm. Importantly, we also shed light on the individual-level mechanisms driving this effect, thus contributing to an understanding of the mechanisms through which CSR can induce firm-benefitting employee outcomes and be strategic for firms (Burbano 2016, Crilly et al. 2016, Bode and Singh 2018, Burbano and Chiles 2022, Gatignon 2022). In contrast to extant work examining employee participation in longer-term social impact activities (for a review, see Rodell et al. 2016), which suggests new skills development as a key to explaining improved employee outcomes (Bode and Singh 2018, Burbano et al. 2018, Gatignon 2022), we highlight the role that alterations to employees' perceptions of their employer play in explaining the effects of a short-term intervention. We show that organizational justice is the individual-level perception of the employer that best explains the effects of the social impact activity on turnover; organizational identification and perceptions of employees' fit with their employer (P-O fit) did not explain the effects. Our results suggest that a short-term CSR intervention can influence employees' perception that their employer cares for the community, leading them to infer that the employer is also more likely to treat them (and other employees) well and fairly; this, in turn, influences their staying behavior. Future work could explore whether employee *self-perceptions* or employees' perceptions of other individuals at work, such as supervisors or coworkers, are additional mechanisms that simultaneously contribute to downstream organization-benefitting employee behaviors and attitudes.

Second, our exploration of heterogeneous treatment effects informs the literature examining gender differences in responses to corporate social impact work and CSR more broadly (Abraham and Burbano 2022, Bode et al. 2022). We find suggestive evidence that the social impact activity had a stronger effect on men compared with women and that the main average turnover effect we observe is being driven by male, and not female, employees. This finding is consistent with literature on wise interventions, which suggests that individuals are more likely to be affected if an intervention is more novel or nonroutine for them. Given that women are more likely to value (Kish-Gephart et al. 2010, Barbulescu and Bidwell 2013, Burbano et al. 2022) and engage in prosocial and communal activities both inside and outside of work (Einolf 2011) than men, an intervention in which employees are exposed to a social, communal activity serves as a greater "shock" or "event" for men than women. Our findings suggest that by exposing men to social impact work, firms can change their perceptions of their employer in a manner that induces them to be more likely to stay at the firm and experience less stress. It is also possible that the implementation of an intervention such as

the one we examine could help diminish gendered stereotypes associated with social work that can have "negative career consequences [lower promotion rates] for those who [voluntarily] engage in this type of work" (Bode et al. 2022, p. 116), although we cannot test for this directly. Future research could explore whether a short-term social impact activity succeeds in reducing the negative unintended consequences of employees' voluntary participation in other corporate-sponsored social activities. Moreover, future work could also explore whether short-term social impact activities have differential effects on other indicators of employee well-being (beyond work stress), such as burnout, loneliness, or psychological well-being.

Third, we make a small contribution to the literature on the potential of short-term interventions to influence longer-term individual behavioral outcomes at work (Brockner and Sherman 2019, Lambert et al. 2022) by showing that altering employees' perceptions of their employer is a critical mechanism that explains the downstream effects of our intervention on employee outcomes. Thus far, the literature has focused on the role that changes to perceptions of the self and others play in explaining the downstream behavioral effects of short-term interventions (Cable et al. 2013, Hulsheger et al. 2015). We extend the notion of individuals' perceptions of others in organizational contexts to include employees' perceptions of their employing firm as critical to understanding how short-term interventions in the workplace can influence employee behavior.

Practically speaking, our paper provides causal evidence that a single, short-term social impact activity can have long-term, enduring effects on a critical employee outcome to organizations. Although the potential for social impact (i.e., impact on the community or the specific beneficiaries of the activity) of a short-term intervention is likely more limited than the impact of a longer-term volunteering program, the costs of implementing short-term interventions are relatively low compared with longer-term programs. Moreover, a short-term social impact activity does not require managers to determine how to sustain employee participation over time, which is one of the major challenges of long-term corporate volunteering programs (Grant 2012). Thus, the finding that a single short-term social impact activity incorporated as part of the employee onboarding process can have long-lasting downstream effects on employees in a manner that is beneficial to the firm is of relevance for firms seeking high benefit-to-cost ratios in their CSR activities.

12.2. Generalizability of Findings

It is important to discuss the scope conditions and generalizability of our results. Notably, the intervention was implemented during the onboarding or new employee induction process, a clear transition point (e.g., Cable and Judge 1996, Ashforth et al. 2007, Walton and Wilson 2018) during which employees' perceptions of their

employer are being formed. Single-occurrence interventions are arguably most effective during periods of transition for employees (Brockner and Sherman 2019). As such, we would not necessarily expect our results to generalize to nontransition periods. Rather, we expect that they would generalize to other periods of transition for employees: periods in which new norms are being imposed and during which employees' perceptions of their employer and work environment are being reassessed (Gonzalez et al. 2022). For example, this would likely apply to mergers and acquisitions, to periods of organizational restructuring, during a notable change in leadership, or during other such significant organizational changes.

It is also important to characterize the context in which the intervention was implemented to present a complete picture of the generalizability of our effects. The social impact activity was implemented in (1) the banking industry and (2) in an emerging economy in Latin America. We believe these to be strengths of our paper given that most research examining the effects of CSR on employees has focused on the consulting and/or legal services industries (e.g., Carnahan et al. 2017, Burbano et al. 2018, Bode et al. 2022), and there is very little examination of CSR in emerging Latin American countries (Aguinis et al. 2020). How might our results generalize to organizations outside the banking industry and in other country contexts? Here, our interpretation of the heterogeneous gender effects as evidence for the argument that effects should be greater when the intervention is non-routine, novel, or surprising helps shed light on how effects might compare in other contexts. The banking industry is not known to be a particularly socially responsible industry.²⁴ Thus, our results are arguably more likely generalize to other industries (or firms within an industry) that do not have reputations for being highly socially responsible (and may be even greater in industries or firms that employees expect to be socially irresponsible). Likewise, CSR in emerging economies is generally less formalized, less common, and less expected of companies (for a review of CSR in emerging economies, see Jamali and Karam 2018) than in developed economies. Given this, we expect that a social impact activity like the one we study in this paper would likely be more effective in other emerging country settings than in developed country settings. Future work could empirically test whether this is the case. Finally, it is important to note that the intervention was implemented in an organization with high average turnover rates. Treatment effects on turnover in organizations with very low average turnover rates would naturally be smaller.

12.3. Limitations and Opportunities for Future Work

Our study is not without limitations. First, we note that there was selection in our experiment, as was expected (given historic attendance rates at the bank's onboarding

events); not all employees who were randomly assigned to the intervention participated in it. We address this by presenting both ITT and TO(I)T analyses. Nonetheless, it is important to note that, in implementing a similar intervention, lower levels of employee compliance could reduce, and potentially even nullify, the effects. Second, another limitation is linked to the fact that the Control group did not receive any sort of intervention. As such, we cannot completely rule out the possibility that non-CSR-related mechanisms may help explain our results, although we provide evidence suggesting that this is highly unlikely (see Section 11). To address those possibilities empirically, future scholars could conduct field experiments with more than one treatment type: for example, comparing a CSR-related treatment with another type of treatment, such as a team-building exercise. Third, one more limitation is associated with the fact that we cannot completely rule out spillover effects in the field experiment—although we used our data to consider alternative explanations associated with possible violations of the stable unit treatment value assumption (SUTVA). Thus, future studies could try to further minimize, by design, the likelihood of SUTVA violations.

In addition, the effects of the social impact activity we evaluated can be considered peripheral to the core business of the bank. In other words, we evaluated an activity that, although designed to be part of the onboarding process, was not fully integrated into the bank's strategy, routines, or operations (Aguinis and Glavas 2013). Future research could explore whether short-term social impact activities that employees perceive to be peripheral versus those integrated with the core business are more effective. In addition, given that our intervention can be considered community oriented (i.e., the beneficiaries were high school students with underprivileged socioeconomic backgrounds) as opposed to environment oriented (Farooq et al. 2017), comparing the effectiveness of these different types of social impact treatments could also be an interesting avenue for future research.

13. Conclusions

Based on a field experiment implemented at a Latin American bank, we show that a one-time, short-term social impact activity implemented as part of the employee onboarding process decreased employee turnover—a critical human capital outcome for organizations. We provide suggestive evidence that the intervention improved employees' perceptions of organizational justice and that this mechanism helps explain the effect on turnover. We also provide suggestive evidence that these effects are greater for male employees than for female employees. The examination of effects based on a field experiment implemented in a real company context enabled us to establish causality in our findings and thus, to address a common challenge faced by researchers exploring the effects of employee participation

in CSR initiatives (including corporate volunteering) on employee outcomes more broadly. Whereas some have used online, gig-work labor marketplaces as settings to observe the causal effects on revealed employee behavior (Burbano 2016, 2021a, b; List and Momeni 2020; Burbano and Chiles 2022), ours is the first study to examine causal effects on employee behavior in a traditional employer-employee field (company) setting. Our findings and methodology thus reflect important contributions to the literature seeking to understand whether, and through which mechanisms, CSR-related activities can be beneficial to a firm and its employees.

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Appendix A. Survey Response

	Survey response
<i>Treatment vs. Control</i>	0.006 (0.060)
<i>Undefined-term Contract</i>	0.128 (0.115)
<i>Salary</i>	0.010 (0.020)
<i>Female</i>	0.026 (0.058)
<i>Age</i>	0.002 (0.007)
<i>Insurance</i>	0.050 (0.071)
<i>Single</i>	0.155 (0.109)
<i>Rank</i>	0.028 (0.034)
<i>CEO Report Support</i>	0.028 (0.086)
<i>Banking</i>	0.073 (0.095)
<i>Risk and Financial Planning</i>	0.045 (0.105)
<i>Personnel Office</i>	
1	0.123 (0.102)
2	0.034 (0.069)
<i>Consumer Segmentation</i>	
<i>Low Income</i>	0.078 (0.089)
<i>Young Consumer</i>	0.083 (0.141)
<i>High Potential Young Consumer</i>	0.123 (0.207)
<i>Residence</i>	
<i>Modern</i>	0.025 (0.078)
<i>North</i>	0.034 (0.091)
<i>South</i>	0.118

Appendix A. (Continued)

	Survey response
	(0.100)
<i>Center</i>	0.025 (0.101)
<i>Constant</i>	0.824** (0.251)
R^2	0.061
Number of employees	221

Notes. Coefficients of OLS regressions are shown. Standard errors are in parentheses.
 ** $p < 0.01$.

Appendix B. Results of Exploratory Factor Analysis

Factor	Eigenvalue	Difference	Proportion of variance	Cumulative variance
1	4.045	2.434	0.385	0.385
2	1.611	0.075	0.153	0.538
3	1.686	0.092	0.160	0.698
4	1.778	1.103	0.169	0.868
5	0.675	0.281	0.064	0.932
6	0.394	0.203	0.038	0.969
7	0.191	0.058	0.018	0.987
8	0.133	—	0.013	1.000

Note. Bolded numbers represent factors with and Eigenvalue over 1.0.

Appendix C. Factor Loadings of Exploratory Factor Analysis—Varimax Rotation

	1	2	3	4
Organizational identification				
When someone criticizes [company name], it feels like a personal insult.	0.671	0.116	0.097	0.066
I am very interested in what others think about [company name].	0.645	0.208	0.104	0.174
When I talk about [company name], I usually say “we” rather than “they”.	0.624	0.262	0.168	0.291
[Company name]’s successes are my successes.	0.770	0.280	0.032	0.279
When someone praises [company name], it feels like a personal complement.	0.802	0.249	0.048	0.166
Perceptions of organizational justice				
I believe that [company name] treats its employees well.	0.203	0.224	0.203	0.863
I believe that [company name] treats its employees fairly.	0.199	0.178	0.207	0.850
Person-organization fit				
To what extent are the values of the organization similar to your own values?	0.147	0.661	0.182	0.240
To what extent does your personality match the personality or image of the organization?	0.239	0.861	0.115	0.163
To what extent is the organization a good match for you.	0.214	0.904	0.187	0.172
Work stress				
General aspects of [company name] tend to cause me a great deal of stress and anxiety.	0.059	0.152	0.855	0.163
My job causes me a great deal of personal stress and anxiety.	0.045	0.149	0.927	0.150
Relations with the people I work with cause me a great deal of stress and anxiety.	0.077	0.112	0.715	0.181
Variance	2.707	2.399	2.320	1.887
Proportion of variance	0.291	0.258	0.249	0.203

Note. Bolded numbers represent item loading over .5.

Appendix D. Factor Loadings of Exploratory Factor Analysis—Promax Rotation

	1	2	3	4
Organizational identification				
When someone criticizes [company name], it feels like a personal insult.	0.750	0.076	0.114	0.108
I am very interested in what others think about [company name].	0.679	0.019	0.006	0.074
When I talk about [company name], I usually say “we” rather than “they”.	0.621	0.057	0.130	0.107
[Company name]’s successes are my successes.	0.786	0.069	0.118	0.105
When someone praises [company name], it feels like a personal complement.	0.852	0.048	0.018	0.090

Appendix D. (Continued)

	1	2	3	4
Perceptions of organizational justice				
I believe that [company name] treats its employees well.	0.027	0.011	0.920	0.004
I believe that [company name] treats its employees fairly.	0.035	0.040	0.913	0.011
Person-organization fit				
To what extent are the values of the organization similar to your own values?	0.014	0.673	0.106	0.045
To what extent does your personality match the personality or image of the organization?	0.063	0.910	0.031	0.038
To what extent is the organization a good match for you.	0.025	0.956	0.037	0.032
Work stress				
General aspects of [company name] tend to cause me a great deal of stress and anxiety.	0.022	0.006	0.006	0.882
My job causes me a great deal of personal stress and anxiety.	0.010	0.001	0.032	0.964
Relations with the people I work with cause me a great deal of stress and anxiety.	0.135	0.014	0.081	0.723
Variance	3.989	3.812	3.444	3.040
Proportion of variance	0.428	0.409	0.370	0.326

Note. Bolded numbers represent item loading over .5.

Appendix E. Split Sample Analyses by Gender

Panel A						
	Turnover		Org. Identification		Org. Justice	
	Women	Men	Women	Men	Women	Men
Treatment vs. Control	0.055 (0.093)	0.218* (0.088)	0.787 (0.993)	2.180 [†] (1.274)	0.106 (0.319)	0.908* (0.324)
Constant	0.267** (0.077)	0.424** (0.075)	31.435** (0.832)	27.607** (1.087)	12.913** (0.266)	12.185** (0.278)
R ²	0.004	0.047	0.008	0.028	0.002	0.073
Number of employees	96	125	77	103	76	102

Panel B				
	P-O Fit		Work stress	
	Women	Men	Women	Men
Treatment vs. Control	0.082 (0.641)	2.010** (0.625)	0.680 (0.930)	2.556* (1.128)
Constant	18.730** (0.535)	17.186** (0.536)	5.913** (0.779)	9.036** (0.963)
R ²	0.000	0.094	0.007	0.048
Number of employees	76	102	77	103

Notes. Coefficients of OLS regressions are shown. Standard errors are in parentheses.

*p 0.05; **p 0.01.

Appendix F. Comparisons Across Subgroups of Employees

Variables	ANOVA									
	1. Treated		2. Did not participate		3. Control		Treated vs. Control		Did Not Participate vs. Control	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation	F	p-values	F	p-values
Turnover	0.165	0.373	0.269	0.447	0.349	0.481	7.17	0.008*	0.98	0.324
Organizational Identification	31.04	3.98	28.69	6.05	29.33	5.61	4.13	0.044*	0.30	0.585
Organizational Justice	13.29	1.22	12.69	1.39	12.52	1.54	9.88	0.002**	0.32	0.574
Person-Organization Fit	19.45	1.97	18.35	3.24	17.90	3.08	12.30	0.001**	0.52	0.474
Work Stress	6.04	3.95	7.33	5.28	7.63	4.84	4.23	0.042*	0.09	0.767

*p 0.05; **p 0.01.

Endnotes

¹ Because not all employees randomly assigned to the intervention participated in it (i.e., selection), we also evaluated the effects of intervention on the employees who were actually exposed to it, finding stronger effects in the treatment on the instrumented treated (TO(I)T) analysis.

² Because employees tend to self-select into these initiatives (Grant 2012) and most studies have been correlational (Rodell et al. 2016), prior work has not yet assessed whether there is a causal effect of participation in CSR programs on employee outcomes (Bode and Singh 2018, Gatignon 2022).

³ For example, like most leading Latin American banks, it has adhered to the latest Equator Principles, which provide financial institutions with a framework to identify, assess, and manage environmental and social risks associated with financing projects. Moreover, the bank supports the principles of the United Nations' Global Compact, including the prevention of violations of human rights, protecting the environment, and promoting the fight against corruption.

⁴ Institutional review board approval was obtained.

⁵ Translated from Spanish to English, the content of the email calendar invitation reads as follows: "Remember to schedule a full day; this is important for your induction process. Do not worry, your supervisor has already been informed of your participation in this induction activity. If you have any questions, please do not hesitate to write us at [the bank's induction email address]." Importantly, participants' supervisors were simply informed that a third day-long onboarding activity was required for all new employees, and they were informed which employees were asked to participate in the first opportunity.

⁶ For each of the regular onboarding activities, employees were given up to three opportunities to comply. The participation rate after the third opportunity to comply with the onboarding activities was approximately 90% over the prior 12 months.

⁷ Researchers have noted that if employees are not satisfied with the initiative and do not perceive it as high in impact and meaningfulness, it is not reasonable to expect positive downstream behaviors to emerge from their participation (Caligiuri et al. 2013, Paço and Nave 2013).

⁸ Employees participated in a short lecture (30 minutes) on the theory behind a successful coaching session with students, were shown two videos (10 minutes) addressing how to effectively interact with the beneficiary (i.e., how to build a trusting environment), were given specific verbal guidelines for the interaction with the beneficiary (30 minutes), and were given time to ask questions regarding the activity (20 minutes).

⁹ The icebreaker activity used was a "rock paper scissors" championship.

¹⁰ Students have few opportunities to make a decent living without obtaining a university degree. It is hard for these students to pay for private university, and the few public universities that offer high-quality education are very competitive; potential students often apply several times before getting in, a process that can take years.

¹¹ Treated employees responded to a short survey to capture their satisfaction with the activity (88% response rate), which indicated high levels of overall satisfaction with the intervention, as well as different aspects of it: impact on them, impact on beneficiaries, organization, duration, and training. Moreover, an open-ended question asked, "What was the most valuable thing that you got from this experience?" Most treated employees described making an impact on the life of other individuals as the most valuable aspect of the activity.

¹² $p = 0.10$ for means comparisons of control variables and all other demographic variables used to assess the balance of the randomization process (see Section 6).

¹³ Two employees randomly assigned to the Treatment group partially responded to the survey (did not answer all questions in the survey). Although we included these partial responses in our main analysis, our results are robust to replicating our analysis without these two observations.

¹⁴ Although we would have liked to gather additional information at the end of the second quarter of 2019 or even later, a management change precluded us from doing so.

¹⁵ We used a three-item measure of intention to stay (Gellatly et al. 2006) from participants' survey responses. Agreement with the statements was measured on a seven-point Likert-type scale, and the overall score was calculated by adding the three items "I am not planning to search for a new job in another organization during the next 12 months," "I rarely think of quitting my job at [name of the bank]," and "If I have my own way, I will be working for this organization one year from now" ($\alpha = 0.72$).

¹⁶ In total, 730 new employees joined the bank; 664 of these had completed the first phases of the onboarding process, and 221 of these (one third of the 664) were randomly selected to be included in the experiment.

¹⁷ Work deadlines, for example, are a common reason that employees opt out of the first opportunity for a required onboarding activity.

¹⁸ All three items in the "Role Tension" dimension of this scale were included.

¹⁹ We used Stata 17's structural equation modeling tool to conduct these analyses.

²⁰ We also examined whether age and salary moderated the effect of the intervention. We found no statistically significant results, which suggest that neither age nor salary influenced the downstream effects of the intervention on employees.

²¹ Given that random assignment to conditions can only be related to outcomes through its effects on the receipt of treatment, it can be used as an instrument for receiving or participating in the treatment, thereby providing an unbiased estimate of the effects of treatment on the treated (Angrist et al. 1996).

²² Perception of CSR is a measure of how socially responsible the employee perceives its employing firm to be. It was measured using a three-item scale from Wagner et al. (2009): "[Name of bank] is concerned with improving the well-being of stakeholders and society at large," "[Name of bank] is a socially responsible company (it undertakes social and environmental initiatives on a voluntary basis)," and "[Name of bank] follows high ethical standards" ($\alpha = 0.91$). Agreement with statements was measured using a seven-point Likert-type scale, and the overall score was calculated by adding the scores of the three items.

²³ Note that the bank was limited by statistical power concerns inherent in adding another condition arm without being able to include more employees in the sample. Because we cannot completely rule out the possibility of non-CSR-related aspects of the intervention driving results, future field experimental research should consider implementing interventions with multiple treatment arms to tease this out directly (for example, randomly assigning a social impact intervention, a non-CSR activity such as a team-building intervention, and no intervention).

²⁴ It has been argued that the "business culture in the banking industry favors dishonest behavior and thus has contributed to the loss of the industry's reputation" (Cohn et al. 2014, p. 88).

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