

# When Bonuses Backfire: Evidence from the Workplace\*

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## Abstract

Monetary incentives are widely used to align employee actions with employer objectives. We conduct a field experiment in a retail chain to evaluate whether an attendance bonus reduces employee absenteeism. Apprentices in 232 stores were randomly assigned to a control group or one of two treatment groups in which a monetary or a time-off attendance bonus was introduced for one year. We find that neither variant of the attendance bonus led to a systematic reduction in absenteeism. On the contrary, the monetary attendance bonus increased absenteeism substantially, by around 45% on average, which corresponds to more than five additional days absent per employee and year. This effect is driven by the most recently hired apprentices. Survey results reveal that the monetary attendance bonus shifted the perception of absenteeism as acceptable behavior. The backfiring effect persists beyond the end of the experiment, indicating a lasting erosion of social norms.

Keywords: Compensation, monetary incentives, social norms, absenteeism, crowding-out, field experiment

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# 1 Introduction

Among scholars and practitioners alike, performance related rewards are widely regarded as a panacea for alleviating conflicts of interests between an employer and its employees.<sup>1</sup> Indeed, the standard principal-agent framework unambiguously prescribes that incentivizing an (influenceable) outcome will improve this outcome. The existing body of empirical evidence from firm-level field studies is mostly consistent with this theoretical argument and shows that performance related rewards generally serve their purpose (see, e. g., Lazear, 2000; Banker et al., 2000; Shearer, 2004; Bandiera, Barankay and Rasul, 2005; Hossain and List, 2012; Delfgaauw et al., 2013; Lourenço, 2016; Friebel et al., 2017; Manthei, Sliwka and Vogelsang, 2021).<sup>2</sup> In this paper, we present a firm-level field experiment and provide evidence that bonuses can also backfire in the workplace. We document the causal effect of a bonus that is diametrically opposed to its intended purpose. Specifically, we find that providing a monetary incentive to come to work on average led to a substantial increase in employee absenteeism.

The phenomenon that monetary incentives can backfire is by itself not entirely novel. Some economists and psychologists have already cast doubt on the paradigm that incentives always work.<sup>3</sup> Frey and Oberholzer-Gee (1997) and Kreps (1997) were among the first to discuss detrimental effects of monetary incentives from an economics perspective. More refined theoretical explanations of such “crowding-out” effects have followed (see, e. g., Bénabou and Tirole, 2003, 2006; Sliwka, 2007; Ellingsen and Johannesson, 2008). However, as Lazear (2018) notes, the existing empirical evidence of backfiring effects mostly comes from controlled laboratory experiments.<sup>4</sup> Gneezy and Rustichini (2000a) and Gneezy and Rustichini (2000b) provide two notable examples of field studies documenting backfiring effects of monetary incentives.<sup>5</sup> Gneezy and Rustichini (2000a) find that introducing a fine for parents who pick up their children late from a day-care center on average led to an increase in delays. Gneezy and Rustichini (2000b) show that paying a small commission for children collecting charitable donations from households reduced the total amount of donations collected relative to a control group that did not receive such a commission. However, it is important to note that these studies document backfiring effects in a social context. This means that the involved parties usually do not expect (financial) compensation for the desired action. It has therefore often been claimed that such backfiring effects are unlikely in the workplace, as financial compensation is an integral part of the contractual relationship between the involved parties. Along these lines, Prendergast (1999) concludes in his seminal article:

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<sup>1</sup>See, for example, Prendergast (1999); Bandiera, Barankay and Rasul (2011); List and Rasul (2011) and Lazear (2018) for reviews documenting the effectiveness of economic incentives in the workplace. According to Bloom and Van Reenen (2011), nearly one in two U.S. workers received some form of performance pay by the beginning of the century, with this proportion increasing over time. In a more recent survey among 200 large private companies, 94 percent of the respondents indicated that they use a short-term incentive program (WorldAtWork, 2016).

<sup>2</sup>For corresponding evidence from laboratory studies see, for example, Sprinkle and Williamson (2006), DellaVigna and Pope (2017), or Bandiera et al. (2021).

<sup>3</sup>See, for example, Gneezy, Meier and Rey-Biel (2011) and Deci, Koestner and Ryan (1999) for extensive reviews of the relevant literature from the fields of economics and psychology, respectively.

<sup>4</sup>See Fehr and Rockenbach (2003); Gneezy and Rustichini (2000b); Fehr and Falk (2002); Fehr and List (2004); Falk and Kosfeld (2006); Ariely, Bracha and Meier (2009); Christ (2013); Gill, Prowse and Vlassopoulos (2013) or Cardinaels and Yin (2015) for notable examples.

<sup>5</sup>On a related note, Cassar and Meier (2020) and List and Momeni (2020) report field experiments documenting that also prosocial incentives in the form of charitable donations can backfire.

Yet it is sometimes argued that [. . .] paying people on the margin to carry out some activity reduces their intrinsic enjoyment of the task. While this idea holds some intuitive appeal, it should be noted that there is little conclusive empirical evidence (particularly in workplace settings) of these influences. (Prendergast, 1999, p. 18)

In this paper, we focus on absenteeism, an employee's unplanned absence from work, as an economically relevant and universally observable measure of individual employee (mal)performance. An absent employee is inevitably unable to fulfill the obligations as stipulated in the employment contract. However, absenteeism is not illegitimate per se, since sickness, for example, may temporarily impair an employee's ability to work. In many countries, employment law accounts for this fact by mandating the provision of sick pay, that is a form of financial compensation for lost wages in the event of sickness.<sup>6</sup> As an immediate consequence, even an employee who is fit for work faces a material incentive to be absent and claim sickness.<sup>7</sup> For the employer, the economic consequences of absenteeism can be considerable.<sup>8</sup> While clearly, absence attributable to genuine sickness is legitimate and unavoidable, an employer has a strong interest in curbing shirking disguised as sickness. Crucially, however, an employer can rarely disclose whether an absent employee is genuinely sick or instead shirking. Absenteeism therefore provides a typical example of a moral hazard problem. We provide causal evidence that a conventional monetary incentive not only fails to overcome this moral hazard problem, but even exacerbates it.

We conducted a firm-level field experiment in collaboration with a German retail chain and implemented two variants of an attendance bonus among 346 apprentices in 232 stores over a period of one year.<sup>9</sup> The first treatment is a monetary attendance bonus that rewards the number of months with perfect attendance financially. Building on the work of [Lacetera and Macis \(2013\)](#) and [Vogelsang \(2021\)](#), who demonstrate the benefits of granting leisure time as an incentive, our second treatment is a time-off attendance bonus that provides a corresponding reward in the form of additional vacation days instead of money.

We find that neither variant of the attendance bonus led to a systematic reduction in absenteeism. On the contrary, the monetary attendance bonus increased absenteeism substantially, by around 45 percent on average, which corresponds to more than five additional days absent per employee and year. The time-off attendance bonus, on the other hand, had no systematic effect on absenteeism at all, that is, it proved neither harm- nor purposeful.

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<sup>6</sup>For a global overview of sick pay policies, see, for example, [Social Security Administration \(2018\)](#), [Social Security Administration \(2019b\)](#), [Social Security Administration \(2019a\)](#) and [Social Security Administration \(2020\)](#).

<sup>7</sup>In the spirit of standard labor supply models of work attendance, an employee is absent if, given the contractually stipulated working hours and wage, the increment utility from engaging in additional leisure exceeds the associated cost (see, e. g., [Allen, 1981](#)).

<sup>8</sup>Aside from the cost of sick pay, which in many states is at least partly borne by the employer, absenteeism can also result in lost revenue opportunities. Moreover, excessive absenteeism can, for example, adversely affect the work morale of those employees who frequently take over the work of their absent colleagues, which is, in turn, detrimental to performance. See, for example, [Goodman and Atkin \(1984\)](#) for an extensive discussion of the consequences of absenteeism on both employers and employees.

<sup>9</sup>As is common in the German labor market, the group of apprentices essentially includes all employees hired by the firm directly after school, excluding unskilled employees, employees with prior work experience or university graduates ([Acemoglu and Pischke, 1998](#)).

We explore the behavioral mechanisms underlying this backfiring effect and examine several theoretical explanations for detrimental effects of monetary incentives that have been proposed in the literature. Specifically, we consider whether the monetary attendance bonus reduced employees' perceived intrinsic costs of absenteeism (Bénabou and Tirole, 2003), signaled an unfavorable descriptive social norm (Sliwka, 2007), shifted their image concerns (Bénabou and Tirole, 2006), mitigated the expected material consequences of absenteeism (Gneezy and Rustichini, 2000a) or reduced the employees' esteem for the employer (Ellingsen and Johannesson, 2008). To examine these potential mechanisms empirically, we conducted a post-experimental survey that elicited employee perceptions along several dimensions. We then used an exploratory factor analysis to identify latent constructs among the survey variables related to these potential mechanisms and identified four factors that captured employees' perceptions about key aspects of the theoretical explanations: (i) intrinsic costs of absenteeism, (ii) image concerns and beliefs about descriptive social norms, (iii) expected material consequences of absenteeism and (iv) employee identification with the employer.

Our key finding is that the monetary attendance bonus reduced employees' perceived intrinsic costs of absenteeism significantly. As Bénabou and Tirole (2003) have shown, monetary incentives can indeed backfire in an otherwise standard principal-agent setting if the agent is uncertain about the personal costs of choosing an action that is desired by the principal. Providing an incentive for the agent to choose the desired action can then signal to the agent that the principal believes these costs are high, making the desired action appear less attractive for its own sake. As our survey data shows, compared to the control group, the apprentices for whom the monetary attendance bonus was introduced indeed reported feeling less guilty in case of being absent despite not being sick and also felt less obliged by their employment contract to always come to work. In other words, the monetary attendance bonus shifted employees' perceived costs of absenteeism, making this behavior appear more acceptable. In the terminology of Cialdini, Kallgren and Reno (1991), this reflects a change of the prevailing "injunctive social norm", that is, the perception of the relevant moral standard.<sup>10</sup> An important precondition for this signaling mechanism to work is that employees are ex-ante uncertain about their personal costs of absenteeism. Consequently, the backfiring effect should be particularly pronounced for the most recently hired employees. Compared to more experienced employees, they have generally acquired less information about the nature of the job along with the prevailing social norms, which in turn provides more scope for the signaling effect to alter their behavior. Indeed, we find that the backfiring effect is driven by the most recently hired employees.

We also investigate the effect of the attendance bonus on absenteeism after the end of the experiment and find that the detrimental effect of the monetary attendance bonus is persistent: Those apprentices for whom the monetary attendance bonus had been introduced on average still had substantially higher absenteeism than the control group, even after this bonus was removed. The monetary attendance bonus thus appears to have persistently shifted the apprentices' perception of absenteeism as acceptable behavior and thereby led to a lasting backfiring effect.

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<sup>10</sup>In contrast to descriptive social norms, which are determined by beliefs what *others do*, injunctive social norms reflect what *ought to be done*. Sliwka (2007) formalizes a related backfiring mechanism by which monetary incentives shift beliefs about descriptive social norms. However, we find little evidence that descriptive social norms, that is, beliefs about others' actions and feelings, are affected in our setting, but strong evidence of a shift of the injunctive social norm.

Our findings thus illustrate how incentives can shape social norms in the workplace. Newly hired employees, whose perceptions are yet malleable, appear to be particularly susceptible to such norm shifts, which have a lasting impact on their behavior. These observations suggest that the incentive structure chosen by a firm can shape the social norms of an entire workforce in the long run, when those employees whose norm perceptions were formed upon entry increasingly dominate the workforce. Our results therefore also contribute to the recent literature on the economics of corporate culture, which has stressed the importance of shared norms in guiding behavior in organizations (see, e. g., [Hermalin, 2012](#); [Guiso, Sapienza and Zingales, 2015](#); [Ashraf, Bandiera and Delfino, 2020](#); [Alan, Corekcioglu and Sutter, 2021](#)). Moreover, our results also complement experimental findings from the lab on the power of rules in shaping norms of behavior (see, e. g., [Galbiati, Schlag and van der Weele, 2013](#); [Danilov and Sliwka, 2017](#); [Lane, Nosenzo and Sonderegger, 2021](#)).

The existing empirical evidence on absenteeism, which is largely based on observational data, shows that employees tend to respond to macro-level policy changes affecting the cost of absenteeism, with higher costs typically being associated with lower absenteeism.<sup>11</sup> What crucially distinguishes our results from these previous findings, however, is that the attendance bonus is a management practice introduced by the employer, not a legal standard set by policymakers. In the latter case, no private information of the employer about employees' personal costs of (not) coming to work is revealed. However, it is precisely this type of signaling effect that plausibly explains the backfiring effect we observe. In the specific context of monetary rewards for attendance, an earlier field experiment by [Duflo, Hanna and Ryan \(2012\)](#) found that providing a monetary attendance bonus led to a considerable decrease in absenteeism among teachers in India. It should be noted, however, that in this particular setting, absenteeism was extremely pervasive before the introduction of the attendance bonus.<sup>12</sup> This suggests that absenteeism was already by and large perceived as acceptable behavior before, which leaves no scope for the attendance bonus to shift prevailing injunctive social norm as it does in our case.

The remainder of this paper is organized as follows: [Section 2](#) describes the experimental design and procedure. [Section 3](#) reports the main results. [Section 4](#) discusses the potential mechanisms underlying these results and reports further results. [Section 5](#) concludes.

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<sup>11</sup>In particular, cost changes arising from changes in the statutory sick pay compensation level (see, e. g., [Johansson and Palme, 2002, 2005](#); [Henrekson and Persson, 2004](#); [Puhani and Sonderhof, 2010](#); [Ziebarth, 2013](#); [Ziebarth and Karlsson, 2010, 2014](#)), the unemployment rate (see, e. g., [Johansson and Palme, 1996](#)) as well as employment protection regimes (see, e. g., [Ichino and Riphahn, 2005](#); [Riphahn, 2004](#)) have been considered.

<sup>12</sup>[Duflo, Hanna and Ryan \(2012\)](#) report that more than one third of the teachers were absent in a baseline study. Also see, for example, [Chaudhury et al. \(2006\)](#) who discuss the general phenomenon of absenteeism among teachers in developing countries.

## 2 The Experiment

### 2.1 Background

We collaborate with a large retail chain, which operates supermarkets throughout Germany. The human resources manager responsible for a large region considered introducing a monetary attendance bonus to reduce absenteeism among the apprentices in the stores of this region. The idea originated from one of the retail chain's other regions where a comparable instrument for a different group of employees had previously been introduced, but not systematically evaluated. Before following the example of the other region, the human resources manager approached us for advice. We offered to systematically evaluate the effectiveness of an attendance bonus to reduce absenteeism. In addition, we proposed to vary the reward domain of the attendance bonus between money and time. For this purpose, the regional management let us implement a randomized controlled trial.

### 2.2 Environment

The experiment takes place among apprentices in the region's stores. The group of apprentices essentially includes all store employees hired by the firm directly after school, excluding unskilled employees. Besides working in the stores, apprentices receive training both on and off the job. The apprenticeship contract generally stipulates 37.5 working hours per week, with a regular working week including all weekdays from Monday to Saturday. The range of work tasks in the store includes, for example, customer service on site, the procurement and handling of goods and simple accounting. In a typical working week, the apprentices attend a vocational school on one or two days, with the time spent at school being counted as working time. The retail chain records absenteeism on school days, too. The apprentices receive a fixed wage and their annual vacation entitlement is generally 36 days. For the majority of apprentices, the apprenticeship begins in early fall and has a scheduled duration of three years.<sup>13</sup> After completing the apprenticeship, apprentices typically seek long-term employment with the retail chain. An average store employs around eight full-time employees and between one and two apprentices. Each store is managed by a store manager who is responsible for recording absenteeism. According to German employment law, an apprentice is generally entitled to sick pay by the employer for a period of up to six weeks.

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<sup>13</sup>The individual start of the apprenticeship varies between apprentices. The scheduled duration of the apprenticeship is 18, 24 or 36 months, depending on the specific program. The different programs also differ in terms of the specific work tasks. However, the daily working routine of the apprentices is similar across all programs.



## 2.3 Data Collection and Primary Outcomes

We obtain absence records, which contain information on each individual absence spell of each of the apprentices. In particular, an absence record contains the start and end date of an absence spell as well as the type of absence, which indicates whether it reflects unplanned absence, as in the case of sickness, or planned absence, as in the case of vacation.<sup>14</sup> We complement the absence records with further personnel data, including the start and end of the apprenticeship, school degree, age, gender and vocational school schedule of each apprentice. By combining the data, we obtain a comprehensive panel data set of individual absenteeism.

In addition, we collect our own survey data. Before the start of the experiment, we conducted a first survey on the general working conditions of apprentices. The more crucial second survey was designed and conducted after the end of the experiment to identify the mechanism underlying the effect of the attendance bonus.<sup>15</sup>

Our primary outcome is an apprentice's individual absence share, which is the ratio of an apprentice's aggregate number of days absent to the total number of this apprentice's regular work days within a given period.<sup>16</sup> The absence share may likewise be conceived as an estimate of an apprentice's probability of absence on any given regular work day within the underlying period.

## 2.4 Treatments

Apprentices are assigned to one of two treatment groups or the control group. In the two treatment groups, apprentices receive a bonus point for each month of perfect attendance, that is, each month without a single day of unplanned absence.<sup>17</sup> During the twelve month experimental period, the apprentices can thus receive a maximum of twelve bonus points. The treated apprentices receive quarterly feedback on their current bonus point score. The total number of bonus points is converted into actual rewards after the end of the experimental period. The two treatments, which we refer to as *Money* and *Time*, differ only with respect to the employed reward domain. [Figure 1](#) illustrates the conversion of bonus points into rewards.

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<sup>14</sup>In the remainder of this paper, we use the term absence to refer to unplanned absence only, which includes, in particular, sickness absence as well as any unexcused absence. If an absence spell lasts longer than three consecutive days, a medical certificate confirming the unfitness for work has to be submitted to the store manager. It should be noted, however, that apprentices commonly provide such a certificate, even if it is not required. For example, in the pre-experimental period, spells of absence without certificate account for only 15.14 percent of all absence spells for which no certificate was required. We therefore consider all absence spells equally, regardless of whether a certificate was submitted or not.

<sup>15</sup>In addition, the post-experimental survey contains a number of other, more general questions, for example on the apprentices' job satisfaction and working time organization. See [Figure 8](#) in the [Online Appendix](#) for the complete post-experimental survey as presented to the apprentices.

<sup>16</sup>A regular work day is any business day that does not fall within an apprentice's spell of planned absence, such as in the case of vacation. School days are also considered regular work days.

<sup>17</sup>Days of absence from vocational school are also considered.

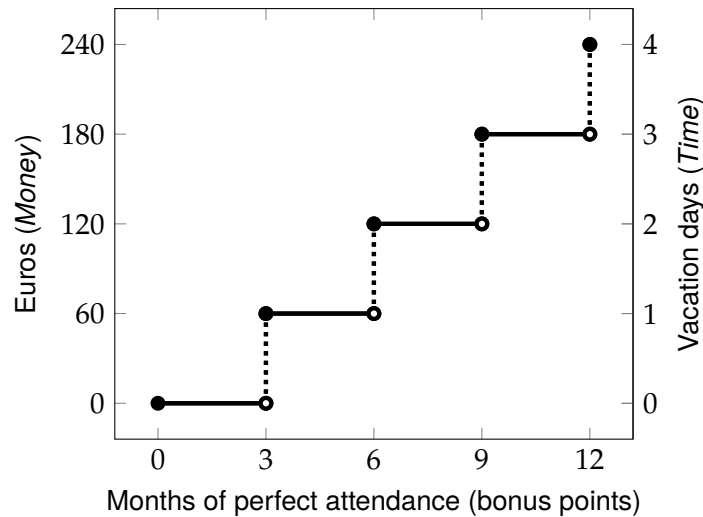


Figure 1: Conversion of bonus points into rewards

In the *Money* treatment, three bonus points correspond to a monetary bonus of 60 euros. Apprentices can thus receive a maximum monetary bonus of 240 euros. The amount is not subject to tax deductions and transferred to the apprentices' employee cards, which the apprentices use to shop from the retail chain's stores.

In the *Time* treatment, three bonus points correspond to a time-off bonus in the form of one additional vacation day. Apprentices can thus receive a maximum number of four additional vacation days. As with any regular vacation day, apprentices are asked to take these additional vacation days by the end of the calendar year in which they are granted.

Apprentices in the control group are not incentivized to come to work. That is, they neither receive a reward nor bonus points for their attendance. However, in the interest of fairness, they received a previously unannounced lump sum payment of 120 euros after the end of the experiment. This amount corresponds to half of the maximum reward in the *Money* treatment and it is also transferred to the apprentices' employee cards.

In calibrating the reward sizes, we relied on the expertise of the regional management and also took into account feasibility constraints. The result was that a maximum of four additional vacation days could be granted per apprentice in the *Time* treatment and that one additional vacation day has a monetary equivalent of 60 euros in the *Money* treatment. To validate our calibration, we elicited the apprentices' hypothetical willingness to pay for an additional vacation day in the post-experimental survey, which averaged 65.32 euros, suggesting our calibration is indeed plausible. The maximum reward in the *Money* treatment of 240 euros corresponds to more than a quarter of an apprentice's typical monthly salary.<sup>18</sup> Note also that we are investigating an attendance bonus of the kind that would be feasible in practice. While it is conceivable that a considerably higher reward size, for example, would have a different effect on absenteeism, such an attendance bonus would arguably not be profitable for the employer. We therefore consider reward sizes of the attendance bonus that an employer would typically also be willing to grant.

<sup>18</sup>According to the Federal Institute for Vocational Education and Training, the average collectively agreed salary for apprentices in the retail sector in Germany was 882 euros in 2018 (Beicht, 2019).



## 2.5 Experimental Sample and Treatment Assignment

With the exception of apprentices in their final year of training, all apprentices in the region's stores take part in the experiment.<sup>19</sup> There are two types of stores, which differ in terms of their ownership structure: type I and type II stores. It is important to note that the store type does not affect the general working conditions of apprentices. This distinction is yet relevant in that, for administrative reasons, the *Time* treatment could only be implemented in type I stores.<sup>20</sup> The original sample comprised 268 apprentices in 151 type I stores and 274 apprentices in 164 type II stores. We assigned treatments at the store level separately by store type using stratified randomization based on the apprentices' absenteeism in the pre-experimental period and the number of apprentices per store. Assigning treatments at the store level instead of the individual apprentice level implies that all apprentices in a given store receive the same treatment, which avoids potential spillover effects of the treatments among apprentices within the same store.

We calculated for each store the mean of the apprentices' mean monthly absence share in the period from August to November 2017 and obtained the quartiles by store type. We also divide stores into three groups, based on the number of apprentices per store. This results in a total number of twelve strata, within each of which treatments are randomly assigned, separately by store type. Overall, our analysis sample comprises 346 apprentices, of which 144, 53 and 149 were assigned to the *Money* treatment, the *Time* treatment and the control group, respectively.<sup>21</sup>

Table 1 provides a summary of the pre-experimental variables between treatment groups and the control group. In addition to the stratification variables, we further consider further variables contained in the personnel data. We assess balancing of these variables using the normalized difference between the sample means of the respective treatment group and the control group as recommended by Imbens and Wooldridge (2009). Following Imbens and Rubin (2015), variables may be considered balanced if their normalized difference does not exceed one quarter. Therefore, as Table 1 reveals, the pre-experimental variables may be considered balanced between treatment groups.<sup>22</sup>

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<sup>19</sup>Apprentices in their final year of training are excluded because their apprenticeship ends before the end of the experiment.

<sup>20</sup>Type I stores are fully owned by the retail chain, so the regional management could directly grant the monetary bonus as well as the additional vacation days. Type II stores are essentially franchising stores. While the regional management could bear the cost of the monetary bonus, it could not mandate store owners to grant their apprentices additional vacation days. At the request of the regional management, we therefore did not implement the *Time* treatment in type II stores.

<sup>21</sup>Originally, 234, 90 and 218 apprentices were assigned to the *Money* treatment, the *Time* treatment and the control group, respectively. The apprenticeship contracts of 142 of these 542 apprentices were terminated before the end of the experiment. The apprenticeship contract of another two apprentices became inactive during the experiment. As continuous employment during the experiment was a precondition for receiving the bonus, we excluded these apprentices from our analysis. We also excluded the 37 and 15 of the remaining apprentices who changed their store and apprenticeship program, respectively. A regression of an attrition indicator on the treatment dummies provides no evidence of systematic differences in attrition between the treatment groups or the control group.

<sup>22</sup>Of the 18 pairwise comparison *t*-tests of the means of the pre-experimental variables between the respective treatment group and the control group, only the age of the apprentices between the *Time* treatment group and the control group shows a weakly significant difference. The *p*-value is 0.08.

Table 1: Balancing of pre-experimental variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Money	Time	Control	All	$\tilde{\Delta}^{\text{Money}}$	$\tilde{\Delta}^{\text{Time}}$
<i>Absence share</i>	0.031 (0.048)	0.031 (0.045)	0.034 (0.061)	0.032 (0.053)	-0.048	-0.049
<i>Apprentices per store</i>	1.426 (0.698)	1.559 (0.786)	1.536 (0.791)	1.491 (0.750)	-0.148	0.029
<i>Second year</i>	0.354 (0.480)	0.340 (0.478)	0.295 (0.458)	0.327 (0.470)	0.126	0.095
<i>Tenure</i>	0.703 (0.470)	0.685 (0.476)	0.674 (0.468)	0.688 (0.469)	0.061	0.021
<i>Female</i>	0.451 (0.499)	0.453 (0.503)	0.443 (0.498)	0.448 (0.498)	0.017	0.020
<i>Age</i>	19.007 (3.051)	19.528 (4.286)	18.638 (2.817)	18.928 (3.182)	0.126	0.246
<i>School degree</i>	0.626 (0.735)	0.667 (0.766)	0.542 (0.701)	0.596 (0.725)	0.116	0.169
<i>School day share</i>	0.170 (0.138)	0.172 (0.126)	0.194 (0.180)	0.181 (0.156)	-0.150	-0.139
<i>In probation</i>	0.313 (0.465)	0.396 (0.494)	0.349 (0.478)	0.341 (0.475)	-0.077	0.097
Apprentices	144	53	149	346		
Stores	101	34	97	232		

*Note:* The table provides a summary of the pre-experimental variables between treatment groups and the control group. Columns (1) through (4) show sample means. Standard deviations are in parentheses. Columns (5) and (6) show the normalized difference of sample means between the respective treatment group and the control group, which is obtained as the difference in sample means between the respective treatment group and the control group, divided by the square root of the average of the two sample variances within the respective treatment group and the control group (Imbens and Rubin, 2015). *Absence share* is the mean monthly absence share per apprentice in the pre-experimental period, which is from August 1, 2017 to December 31, 2017. *Apprentices per store* indicates the number of apprentices in the same store. *Second year* is a binary indicator of whether an apprentice is in the second year of training at the start of the experiment on January 1, 2018. *Tenure* is an apprentice's tenure in years at the start of the experiment since the start of the apprenticeship. *Female* is a binary indicator of whether an apprentice is female. *Age* is an apprentice's age at the start of the experiment. *School degree* is a three-level indicator of an apprentice's school degree. It takes the value 0, 1 and 2 if an apprentice has a low, middle and high school degree, respectively. *School day share* is an apprentice's mean monthly school day share in the pre-experimental period. *In probation* is a binary indicator of whether an apprentice is in probation at the start of the experiment.

## 2.6 Procedural Details

Figure 2 provides an overview of the experimental procedure. The apprentices were invited to participate in the first survey on December 6, 2017.<sup>23</sup> All communication with apprentices is handled directly by the regional management, in close consultation with us. Apprentices are informed that the surveys are conducted by a university, which ensures their confidentiality. Otherwise, however, the involvement of a university is not disclosed. On December 28, 2017, apprentices were first informed about the attendance bonus.<sup>24</sup> All apprentices are informed that an attendance bonus will be temporarily introduced for randomly selected groups of apprentices in the region. Treated apprentices additionally received information about the timing of the project, the collection of bonus points and the conversion of these bonus points into rewards according to the respective treatment. Apprentices in the control group are informed that this project will only become relevant for them at a later point in time and that they will receive further information in due course.<sup>25</sup> If apprentices had any questions about the attendance bonus, they were encouraged to contact their training manager, who is their main contact for all organizational matters related to the apprenticeship and whom we informed about the experiment. We also provided the training manager with a guide that contained answers to potentially frequently asked questions, for example about the random assignment.<sup>26</sup> During the experiment, treated apprentices received quarterly feedback on the number of bonus points received in the preceding quarter and the current bonus point score.<sup>27</sup> On April 14, 2019, apprentices were sent their final feedback and were also informed about the amount of the respective reward to be received. Apprentices in the *Money* treatment group received their monetary bonus on their employee card by the of April 2019, apprentices in the *Time* treatment group were asked to take their additional vacation days by the end of 2019. On May 28, 2019, all apprentices were invited to participate in the second survey.<sup>28</sup> Finally, on August 28, 2019, apprentices in the control group were informed that they would receive a lump-sum payment of 120 euros, which was transferred to their employee cards by the end of August 2019.

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<sup>23</sup>Participation was voluntary and rewarded with a payment of 10 euros upon completion. Nearly one quarter (24.91 percent) of the apprentices completed the first survey.

<sup>24</sup>See Figure 7 in the Online Appendix for the first letters to apprentices about the attendance bonus.

<sup>25</sup>This information is intended to prevent the apprentices in the control group from altering their behavior because they feel disadvantaged should they learn, without the relevant background information, that other apprentices can receive a bonus while they themselves cannot. To substantiate the claim that the control group was not contaminated by the experiment, we also compare absenteeism in the control group with absenteeism among full-time employees not affected by the experiment and find no significant difference. See Table 7 in the Appendix.

<sup>26</sup>The apprentices only had minor queries and did not express any major complaints.

<sup>27</sup>The delay visible in Figure 2 between the end of each quarter and sending the feedback is due to the fact that we receive the quarterly absence records with a delay.

<sup>28</sup>Of the apprentices who were still employed at the time of the invitation, 29.19 percent completed the second survey. A regression of a survey participation indicator on the treatment indicators does not provide evidence of selective survey participation. See Figure 8 in the Online Appendix for the complete post-experimental survey.

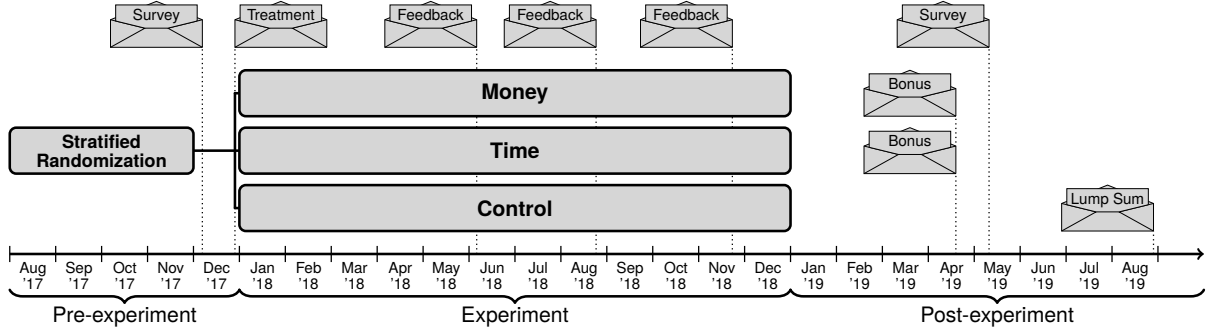


Figure 2: Experimental Procedure

## 2.7 Empirical Specification

In our main analysis, we consider for each apprentice the entirety of available observations during both the pre-experimental and the experiment period, which is from August 1 to December 31, 2017 and from January 1 to December 31, 2018, respectively.<sup>29</sup> We estimate the main treatment effects using variants of the following specification:

$$\text{Absence share}_{it} = \alpha_i + \lambda_t + \rho_1 \text{Money}_{it} + \rho_2 \text{Time}_{it} + \psi' \mathbf{Controls}_{it} + \epsilon_{it}, \quad (1)$$

where  $\text{Absence share}_{it}$  indicates the ratio of apprentice  $i$ 's aggregate number of days absent to the total number of apprentice  $i$ 's regular work days in period  $t$ . We consider a monthly and a yearly variant of Equation (1), where  $t$  corresponds to the current month and year, respectively. We denote by  $\alpha_i$  an apprentice-specific fixed effect, which captures any time-invariant unobserved heterogeneity associated with apprentice  $i$ . Accordingly,  $\lambda_t$  denotes a time-specific fixed effect, which captures any effect associated with period  $t$  that is common to all apprentices. The binary treatment indicators  $\text{Money}_{it}$  and  $\text{Time}_{it}$  are equal to unity only if an apprentice  $i$  belongs to the respective treatment group and period  $t$  falls within the experimental period, thus  $\rho_1$  and  $\rho_2$  represent the difference-in-differences estimators of the average  $\text{Money}$  and  $\text{Time}$  treatment effects, respectively. Equation (1) further includes  $\mathbf{Controls}_{it}$ , a column vector containing time-variant individual control variables. Specifically, we consider the share of vocational school days as well as the share of days in probation of apprentice  $i$  in period  $t$ . Finally,  $\epsilon_{it}$  denotes the idiosyncratic error term.

<sup>29</sup>Around half (50.58 percent) of the apprentices in the analysis sample start their apprenticeship after August 1, 2017. For these apprentices, we consider all observations from the start of their apprenticeship.

### 3 Main Results

Before discussing the estimation results, we present descriptive statistics of individual absenteeism. Figure 3 summarizes the mean monthly absence share per apprentice by period and group. In the pre-experimental period, the mean monthly absence share per apprentice is balanced between treatment groups and the control group. In an average month before the start of the experiment, an average apprentice was absent on around 3.23 percent of her regular work days or, in absolute terms, on 0.73 days. Figure 3 shows that the mean monthly absence share per apprentice increased overall in the experimental period compared to the pre-experimental period. In the control group, this increase is 21.86 percent, which is similar in magnitude to a corresponding increase of 25.71 percent in a group of 2339 full-time employees in the region's stores not participating in the experiment.<sup>30</sup> We therefore attribute this increase to a common trend in absenteeism.

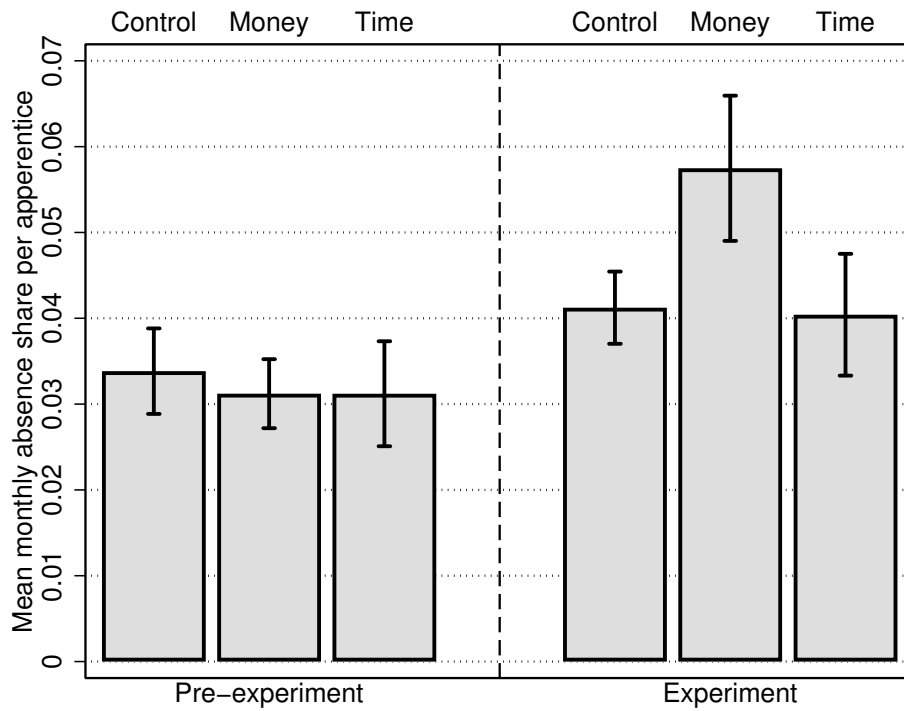
Most notably, the apprentices receiving the *Money* treatment on average exhibit a substantially stronger increase in absenteeism than the apprentices in the control group, which is diametrically opposed to the intended purpose of the attendance bonus. Specifically, in the *Money* treatment group, the mean monthly absence share per apprentice on average increased by 84.15 percent in the experimental period compared to the pre-experimental period. In contrast, the corresponding increase in absenteeism among apprentices receiving the *Time* treatment is 29.52 percent, which is not far beyond what we attribute to a common trend in absenteeism.

Table 2 presents the estimation results, which corroborate these findings. As Column (1) of Table 2 shows, the *Money* treatment significantly increased the monthly absence share by 0.02168 on average. Relative to the control group's mean monthly absence share in the experimental period, which is 0.04123, this corresponds to a 45.29 percent increase in absenteeism. Given the mean number of regular work days per month in the experimental period, which is 22.24, the *Money* treatment increased the expected number of days absent in an average month by 0.48. That is, the *Money* treatment on average caused apprentices being absent more than five additional days per year.<sup>31</sup> In contrast, the *Time* treatment does not appear to have systematically affected absenteeism at all. The magnitude of the average effect of the *Time* treatment is consistently close to zero. Thus, on average, the *Time* treatment proved neither harm- nor purposeful.

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<sup>30</sup>Table 7 in the Appendix shows that the change in absenteeism does not significantly differ between the control group and the group of full-time employees.

<sup>31</sup>We also investigate how the attendance bonus affected the extensive and intensive margin of absenteeism. The *Money* treatment on average caused apprentices to be absent longer within a month, but not necessarily more often. See Column (1) and Column (3) of Table 12 in the Appendix for the complete results.



Note: The figure shows sample means of the mean monthly absence share per apprentice in the respective period over all apprentices in the respective treatment group or control group. *Pre-experiment* indicates the pre-experimental period, which is from August 1, 2017 to December 31, 2017. *Experiment* indicates the experimental period, which is from January 1, 2018 to December 31, 2018. Error bars indicate standard errors of the mean.

Figure 3: Descriptive statistics of individual absenteeism

Table 2: Treatment effects on absenteeism

	Dependent variable:	
	Absence share <sub>it</sub>	
	(1) Monthly	(2) Yearly
<i>Money</i> <sub>it</sub>	0.02168** (0.01025)	0.02592** (0.01187)
<i>Time</i> <sub>it</sub>	0.00404 (0.00957)	0.00575 (0.01032)
Apprentices	346	346
Stores	232	232
Observations	5750	692

Note: The table shows estimates of the average treatment effects on absenteeism. The underlying specification is Equation (1). The dependent variable, Absence share<sub>it</sub>, is the absence share of employee *i* in period *t*, which reflects the ratio of employee *i*'s aggregate number of days absent to the total number of employee *i*'s regular work days in period *t*. *Money*<sub>it</sub> and *Time*<sub>it</sub> are binary treatment indicators of whether an apprentice *i* belongs to the respective treatment group and month *t* falls within the experimental period, which is from January 1, 2018 to December 31, 2018. Apprentice-specific and time-specific fixed effects as well as controls for the share of vocational school days and the share of days in probation of apprentice *i* in period *t* are included. Column (1) and (2) shows the results of the monthly and yearly variant, where period *t* reflects the current month and year, respectively. Standard errors clustered by store are in parentheses. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.



## 4 Discussion and Further Results

### 4.1 Potential Mechanisms

In a next step, we seek to understand why the attendance bonus fails to achieve its intended purpose. In particular, we aim to shed light on the mechanisms underlying the backfiring effect of the *Money* treatment. In psychology, there has long emerged a large strand of literature claiming that extrinsic rewards can undermine intrinsic motivation (see, e. g., [Deci, 1971](#); [Deci and Ryan, 1985](#); [Lepper, Greene and Nisbett, 1973](#)). More recently, several formal economic models providing explanations for such “crowding-out” effects have been proposed. In the following, we first argue conceptually how these explanations can be applied to our setting. We then report a post-experimental survey, which we have designed and conducted to examine the potential mechanisms empirically. We discuss three broader classes of potential mechanisms through which the attendance bonus may have increased absenteeism: by reducing the *psychological costs* of absenteeism, the expected *material consequences* or affecting the *employee’s identification* with the employer.

#### 4.1.1 Psychological Costs of Absenteeism

As the first broader class of potential mechanisms we consider the *psychological costs* of absenteeism. Under these costs we subsume the immaterial consequences of absenteeism that directly affect an employee’s utility when being absent from work. Based on the existing literature, we consider three distinct elements of these costs: *intrinsic costs*, *descriptive social norms* and *image concerns*.

First, we regard an employee’s *intrinsic costs* of absenteeism. Consider an employee who has a preference to comply with the contract and to come to work unless being sick. If such an employee does not come to work despite not being sick, a loss of utility will result from this breach of contract, even if it remains undetected by the employer or a third party. [Frey and Oberholzer-Gee \(1997\)](#) proposed a theoretical framework that allowed monetary incentives to interfere with an agent’s cost of effort, but their reduced form approach does not model the underlying mechanism explicitly. [Bénabou and Tirole \(2003\)](#) formalized the idea that the provision of an incentive for accomplishing a task serves as a signal about the cost of the required effort. A key element of this theory is that agents are uncertain about their own preferences for a task. A specific incentive scheme chosen by the principal can then reveal information affecting agents’ beliefs about their own preferences.<sup>32</sup> In our context, an attendance bonus may signal to employees that the employer is concerned about absenteeism being widely regarded as acceptable behavior. This information can in turn affect employees’ belief about their intrinsic preference to comply with their contract. Put differently, the attendance bonus may shift employees’ perception of the “injunctive social norm”, that is their understanding of morally acceptable behavior ([Cialdini, Kallgren and Reno, 1991](#); [Krupka and Weber, 2013](#)). The attendance bonus can thus change employees’ perception in such a way that they regard absenteeism as more acceptable behavior, which relaxes the psychological costs associated with it.

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<sup>32</sup>[Bremzen et al. \(2015\)](#) provide experimental evidence for this theoretical proposition and show that rewards can convey negative information about the task.

Following [Sliwka \(2007\)](#), an employer's choice of an incentive scheme can serve as a signal about the *descriptive social norm*, that is the prevalent behavior among employees. The key idea of this model is that providing monetary incentives for a specific action reveals the employer's belief that most employees do not choose this action voluntarily. This, in turn, can reduce the psychological costs of non-compliance among other employees driven by conformity motives. In our setting, an employee may infer from the mere fact that an attendance bonus is introduced that absenteeism is prevalent among the other employees. Employees may justify absenteeism by the behavior of the majority, which makes it appear as more acceptable behavior, thus reducing the associated psychological costs. The main difference between these two related mechanisms is that the former implies that a monetary incentive shifts employees' perceptions about what they *ought to do*, while the latter implies that it shifts employees' beliefs about what others *do*.

In addition, an employee's *image concerns* can also contribute to the psychological costs of absenteeism. Consider, for example, an employee who is concerned about being perceived as reliable and motivated by the employer and who avoids being absent precisely because of these image concerns. As [Bénabou and Tirole \(2006\)](#) demonstrate, the provision of monetary incentives can impair such image motivation as rewards "create doubt about the true motive" ([Bénabou and Tirole, 2006](#), p. 1652) for which an action is taken.<sup>33</sup> Applied to our context, the reward may have undermined the reputational gains that employees achieve from fully complying with their contract, thereby mitigating the image costs of absenteeism.<sup>34</sup>

#### 4.1.2 Material Consequences of Absenteeism

Aside from the psychological costs, the attendance bonus may also affect employees' expectations of the *material consequences* of absenteeism. Of course, the attendance bonus inherently manipulates the material consequences of absenteeism. However, according to [Gneezy and Rustichini \(2000a\)](#), the introduction of an incentive scheme may reveal additional information about the contractual setting and thereby alter the original decision problem. Given that any employment contract is incomplete to the extent that it does not explicitly stipulate the consequences of all possible forms of misconduct, an employee can initially only vaguely assess them. [Gneezy and Rustichini \(2000a\)](#) argue that the introduction of a fine provides information about the consequences of the undesired behavior, while leaving the explicit terms of the contract unchanged. In our context, the attendance bonus may have led apprentices to believe that not receiving bonus points and the attendance bonus, respectively, is the most severe consequence of absenteeism. This certain and yet relatively mild consequence of absenteeism may have overshadowed the more severe expected consequences that initially made an employee abstain from absenteeism, such as, for example, the threat of dismissal. This change in the expected consequences of absenteeism, which implies a reduction of the overall associated costs, thus provides a further potential mechanism underlying the observed backfiring effect.

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<sup>33</sup>[Ariely, Bracha and Meier \(2009\)](#) provide experimental evidence that monetary rewards can indeed mitigate image concerns.

<sup>34</sup>Note that a key difference between [Bénabou and Tirole \(2003\)](#) or [Sliwka \(2007\)](#) on the one hand and [Bénabou and Tirole \(2006\)](#) on the other is the direction of signaling: According to the first two mechanisms, incentives are detrimental because they reveal unfavorable information to employees. According to the third mechanism, incentives undermine employees' scope to reveal favorable information to employers.

### 4.1.3 Employee Identification with Employer

Besides the psychological costs and the expected material consequences of absenteeism, which contribute directly to the overall costs of absenteeism, we also consider *employee identification* as another potential mechanism through which the attendance bonus may affect employees' decision to be absent. More specifically, we refer to employees' esteem for the employer. As formalized by Ellingsen and Johannesson (2008), the use of a control system may lead employees to think less of their employer, which in turn may reduce employees' desire to be esteemed by the employer. As a consequence, employees' willingness to comply with the employer's objectives for the mere sake of social esteem may be reduced. In our context, employees may perceive an attendance bonus as unkind or unfair, which may reduce employees' esteem for the employer. This, in turn, may reduce their motivation not to shirk.

## 4.2 Survey Results

After the end of the experiment and based on the above reasoning, we designed and conducted a survey to elicit apprentices' psychological costs of absenteeism, their perceived likelihood of different potential material consequences of absenteeism as well as their identification with the retail chain.<sup>35</sup> We then conducted an exploratory factor analysis on the mechanism-related survey variables to reduce dimensionality and to reveal potential latent constructs among these variables. Table 3 reports the results. Overall, four factors were extracted. The first of these factors, which we term *intrinsic costs*, comprises a variable capturing an apprentice's feeling of *guilt* in case of being absent despite not being sick as well as a variable capturing an apprentice's feeling of *obligation* to always come to work. The second factor, denoted as *image and belief*, comprises a variable capturing an apprentice's *image concerns* when being absent as well as a variable capturing an apprentice's belief about *others' guilt* in case of being absent despite not being sick, which reflects the descriptive social norm. The third factor comprises all five variables capturing an apprentice's perceived likelihood of different potential material consequences of absenteeism, which we therefore refer to as *material consequences*. The fourth factor, denoted as *employee identification*, comprises all six variables capturing an apprentice's identification with the retail chain.

We construct an index for each of these four factors by taking for each surveyed apprentice the mean of the relevant variables and consider the respective z-score, which is normalized to have a mean of 0 and a variance of 1. We then estimate the average treatment effects on each of the survey factor indices in order to investigate the extent to which the attendance bonus affected the perceptions along the different dimensions. Table 4 reports the results. The *intrinsic costs* index differs significantly and substantially between the *Money* treatment group and the control group. More precisely, among the surveyed apprentices in the *Money* treatment group, the *intrinsic costs* index is on average nearly half a standard deviation lower than among the surveyed apprentices in the control group. That is, compared to the control group, the apprentices receiving the *Money*

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<sup>35</sup>See Table 8 in the Appendix for the mechanism-related post-experimental survey items and Figure 8 in the Online Appendix for the complete post-experimental survey as presented to the apprentices. While we designed the survey items related to psychological costs and material consequences of absenteeism ourselves, we relied on an established standard scale for measuring employee identification, the "Affective Commitment Scale" (Allen and Meyer, 1990; Meyer, Allen and Smith, 1993).

Table 3: Exploratory factor analysis results

	Extracted factors:			
	(1) <i>Intrinsic costs</i>	(2) <i>Image and belief</i>	(3) <i>Material consequences</i>	(4) <i>Employee identification</i>
<i>Guilt</i>	0.827	-0.002	-0.007	0.140
<i>Obligation</i>	0.724	0.138	0.214	0.102
<i>Image concerns</i>	-0.041	0.873	0.140	-0.045
<i>Others' guilt</i>	0.357	0.656	-0.257	0.093
<i>Oral warning</i>	-0.011	-0.029	0.854	-0.023
<i>Written warning</i>	0.222	0.114	0.774	0.043
<i>No job offer</i>	-0.060	0.099	0.766	0.006
<i>Rejection</i>	0.037	-0.106	0.750	0.051
<i>Dismissal</i>	0.043	0.000	0.644	0.034
<i>Attached</i>	0.135	0.015	-0.030	0.798
<i>Belonging</i>	0.019	-0.029	0.115	0.797
<i>Part of family</i>	-0.028	0.019	-0.001	0.793
<i>Rest of career</i>	0.296	-0.022	0.038	0.777
<i>Meaning</i>	0.145	-0.003	-0.062	0.716
<i>Own problems</i>	-0.090	0.037	0.017	0.702
Observations	104	104	104	104

Note: The table shows varimax-rotated factor loadings obtained from an exploratory factor analysis on the mechanism-related post-experimental survey variables with principal-component factoring. The highlighted values indicate the variables included in the respective extracted factor. See Table 8 in the Appendix for the corresponding survey items.

Table 4: Treatment effects on survey factor indices

	Dependent variable:			
	(1) <i>Intrinsic costs</i> z-score <sub><i>i</i></sub>	(2) <i>Image and Belief</i> z-score <sub><i>i</i></sub>	(3) <i>Material consequences</i> z-score <sub><i>i</i></sub>	(4) <i>Employee identification</i> z-score <sub><i>i</i></sub>
<i>Money<sub><i>i</i></sub></i>	-0.45452** (0.22185)	-0.27572 (0.24006)	-0.00889 (0.22080)	0.11099 (0.20582)
<i>Time<sub><i>i</i></sub></i>	-0.09098 (0.27281)	-0.31311 (0.33037)	-0.30168 (0.47515)	0.57054** (0.28354)
Observations	104	104	104	104

Note: The table shows estimates of the average treatment effects on survey factor indices. The dependent variable is the respective survey factor index, which is constructed by taking for each surveyed apprentice the mean of the variables included in the respective extracted factor and normalizing it to have a mean of 0 and a variance of 1. See Table 3 for the variables included in the survey factors and Table 8 in the Appendix for the corresponding survey items. *Money<sub>*i*</sub>* and *Time<sub>*i*</sub>* are binary treatment indicators of whether an apprentice *i* belongs to the respective treatment group. Controls for the age, gender and assigned stratum of apprentice *i* are included. Standard errors clustered by store are in parentheses. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.

treatment feel less guilty when being absent despite not being sick and also feel less obliged by their contract to always come to work. The estimate of the *Money* treatment effect on the closely related *image and belief* index also exhibits a negative sign, but is not significantly different from zero.<sup>36</sup> The *material consequences* index, on the other hand, shows no such difference between the *Money* treatment group and the control group. Thus, there is no evidence that the *Money* treatment shifted apprentices' expected material consequences of absenteeism. We also do not find that any of the attendance bonus adversely affected apprentices' identification with the retail chain. Instead, among the surveyed apprentices in the *Time* treatment group, the *employee identification* index is even more than half a standard deviation higher on average than among the surveyed apprentices in the control group. We therefore find no evidence that the attendance bonus leads apprentices to feel detached from their employer.

Overall, our survey results are in particular consistent with the idea formalized by [Bénabou and Tirole \(2003\)](#): Incentives can shift beliefs about the personal costs of certain activities. In our context, the intrinsic costs of absenteeism, which are largely determined by the perception of the relevant injunctive social norm—the respective moral standard—appear to be a central element of these personal costs. The monetary attendance bonus has considerably reduced these intrinsic costs of absenteeism by shifting the apprentices' perception of absenteeism as acceptable behavior.

### 4.3 Presenteeism

Our survey results revealed that the *Money* treatment on average reduced apprentices' intrinsic costs associated with absenteeism. Along with the accompanying increase in absenteeism, this appears to be a clearly negative result from the employer's perspective. However, our findings also allow for a more positive interpretation: Employees may sometimes feel compelled to come to work despite being sick, a phenomenon commonly referred to as presenteeism.<sup>37</sup> If an attendance bonus leads employees to perceive absenteeism as more acceptable behavior, it should also reduce the perceived pressure to come to work despite being sick. Accordingly, given that the *Money* treatment reduced the intrinsic costs of absenteeism, we expect it to also reduce the presenteeism tendency. We elicit apprentices' presenteeism tendency in the post-experimental survey by letting them rate the statement "Sometimes I come to work despite being sick" on a six-point rating scale ranging from "completely disagree" to "completely agree". [Figure 4](#) shows the share of surveyed apprentices who completely agree to this statement, indicating a pronounced presenteeism tendency. The share of apprentices with a pronounced presenteeism tendency among the surveyed apprentices in the *Money* treatment group, which is 54.00 percent, contrasts with the corresponding share among the surveyed apprentices in the control group, which is 73.81 percent.<sup>38</sup> Thus, it appears that the *Money* treatment also made apprentices less likely to come to work despite being sick.

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<sup>36</sup>We additionally elicited beliefs about the descriptive social norm of absenteeism by asking the apprentices in the post-experimental survey to estimate the mean number of days absent per year in the year 2017, that is the year preceding the experiment. The mean estimate of the surveyed apprentices receiving the *Money* treatment, which is 14.46, is only slightly larger than the corresponding value of the surveyed apprentices in the control group, which is 13.76. The difference between these means is not significantly different from zero at any conventional level of confidence. Thus, also in this way, we find no evidence for a shift in the belief about the descriptive social norm of absenteeism.

<sup>37</sup>See, for example, [Johns \(2010\)](#) for a review.

<sup>38</sup>Regression results confirm that the *Money* treatment significantly and substantially reduces presenteeism tendency. See Column (1) of [Table 9](#) in the [Appendix](#) for the complete results.

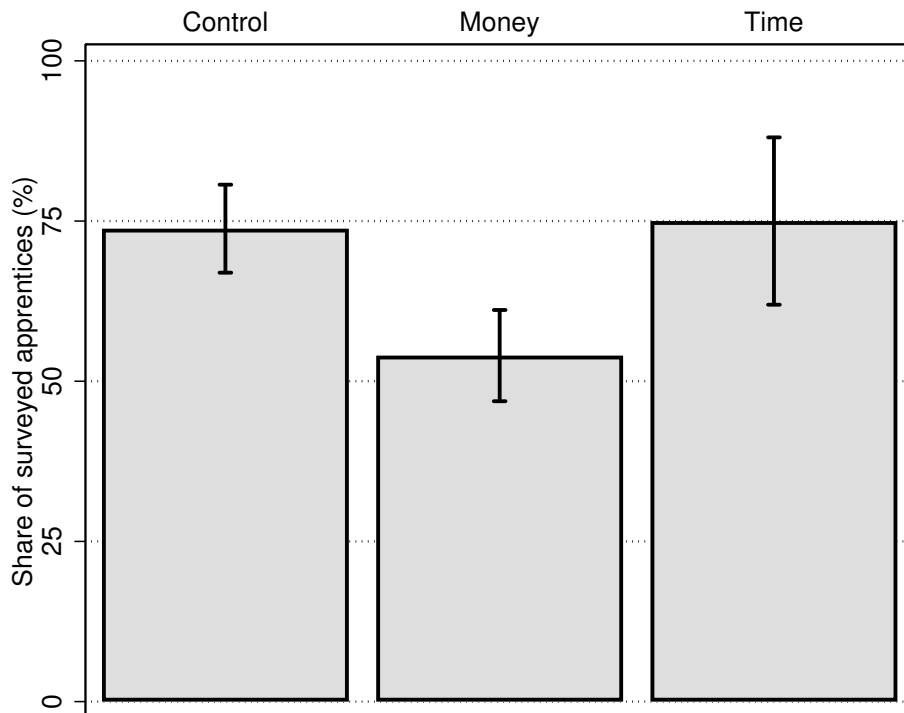


Figure 4: Share of surveyed apprentices with pronounced presenteeism tendency

Note: The figure shows the percentage of surveyed apprentices in the respective treatment group or control group who “completely agree” to the statement “Sometimes I come to work despite being sick.”. Agreement was elicited in the post-experimental survey and measured on a six-point rating scale ranging from “completely disagree” to “completely agree”. Error bars indicate standard errors of the mean.

#### 4.4 The Role of Tenure

Following [Bénabou and Tirole \(2003\)](#), we argued that the attendance bonus backfired as it reduced the intrinsic costs associated with absenteeism. The core idea of their model is that the employer’s use of an incentive can signal relevant information to employees pertaining to their personal costs of choosing an action desired by the employer. An important precondition for this signaling mechanism to work is that an employees are uncertain about these personal costs. Otherwise, there would be no scope for such a signaling effect and the (standard) incentive effect of the reward would always prevail.

This reasoning suggests that the backfiring effect should be more pronounced for more recently hired apprentices. The rationale is as follows: The more recently hired apprentices are less familiar with the working environment, they should thus be more uncertain about norms of behavior than more senior apprentices. In contrast, more senior apprentices have already learned more about their work tasks, the cost of the required effort for accomplishing these tasks and also their intrinsic costs of absenteeism. The information gain associated with the signaling effect of introducing the attendance bonus should therefore be greater for more recently hired apprentices. To test the hypothesis that the backfiring effect of the *Money* treatment is particularly pronounced for more recently hired apprentices, we take advantage of the fact that there are two distinct cohorts of apprentices, which inherently differ in terms of apprentices’ tenure at the start of the experiment: First and second year apprentices. [Table 5](#) presents the results of estimating



Table 5: Treatment effects on absenteeism by cohort

	Dependent variable:	
	Absence share <sub>it</sub>	
	(1) Monthly	(2) Yearly
$Money_{it}$	0.03966*** (0.01326)	0.04372*** (0.01507)
$Money_{it} \times \text{Second year}_i$	-0.05256** (0.02052)	-0.05589*** (0.02145)
$Time_{it}$	0.00975 (0.01038)	0.01233 (0.01179)
$Time_{it} \times \text{Second year}_i$	-0.01970 (0.02234)	-0.02419 (0.02307)
Apprentices	346	346
Stores	232	232
Observations	5750	692

*Note:* The table shows estimates of the average treatment effects on absenteeism by cohort. The underlying specification is a variant of Equation (1). The dependent variable, Absence share<sub>it</sub>, is the absence share of apprentice  $i$  in period  $t$ , which reflects the ratio of apprentice  $i$ 's aggregate number of days absent to the total number of apprentice  $i$ 's regular work days in period  $t$ .  $Money_{it}$  and  $Time_{it}$  are binary treatment indicators of whether an apprentice  $i$  belongs to the respective treatment group and period  $t$  falls within the experimental period, which is from January 1, 2018 to December 31, 2018.  $\text{Second year}_i$  is a binary second year cohort indicator of whether apprentice  $i$  is in the second year of training at the start of the experiment on January 1, 2018. Apprentice-specific and time-specific fixed effects as well as controls for the share of vocational school days and the share of days in probation of apprentice  $i$  in period  $t$  are included. The treatment indicators and the time-specific fixed effects are interacted with the second year cohort indicator. Column (1) and (2) shows the results of the monthly and yearly variant, where period  $t$  reflects the current month and year, respectively. Standard errors clustered by store are in parentheses. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.

heterogeneous treatment effects on absenteeism by cohort. It turns out that the estimate of the  $Money$  treatment effect for the cohort of first year apprentices is indeed nearly twice as large as the estimate of the overall  $Money$  treatment effect. The estimated effect of the interaction of the  $Money$  treatment indicator and the second year cohort indicator is negative, significantly different from zero and exceeds the magnitude of the  $Money$  treatment effect for the cohort of first year apprentices.<sup>39</sup> The estimate of the composite  $Money$  treatment effect for the cohort of second year apprentices thus even exhibits a negative sign.<sup>40</sup>

To explore this heterogeneity further, we subdivide each cohort by the apprentices' respective cohort median tenure and obtain four groups.<sup>41</sup> Figure 5 illustrates the estimates of the composite average  $Money$  treatment effects for these four groups. It shows that the magnitude of these estimates tends to decrease in the apprentices' tenure at the start of the experiment. The overall

<sup>39</sup>Columns (2) and (4) of Table 10 in the Appendix show that these effects remain qualitatively robust even when the absence share is subject to 99% winsorizing.

<sup>40</sup>However, the estimate of the composite  $Money$  treatment effect for the cohort of second year apprentices is not significantly different from zero at any conventional level of confidence. Thus, there is no evidence of a standard incentive effect for this cohort either. However, considering the extensive and intensive margin of absenteeism, we find that the  $Money$  treatment leads second year apprentices to be absent for shorter periods and less frequently within a month on average. See Column (2) and (4) of Table 12 in the Appendix for the complete results.

<sup>41</sup>We consider the apprentices' tenure in years at the start of the experiment.

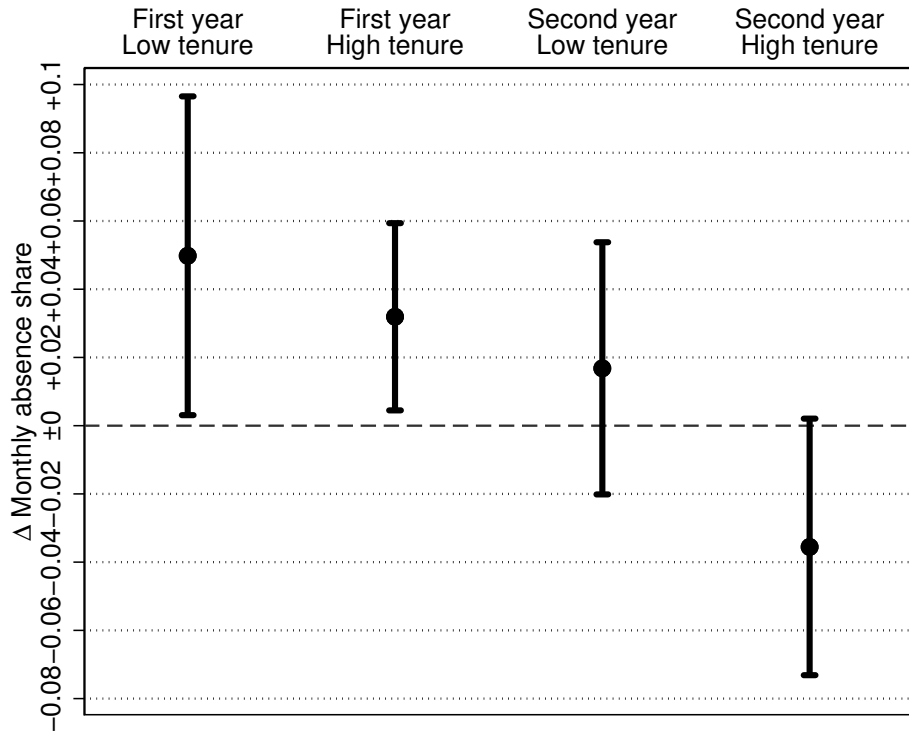


Figure 5: Composite *Money* treatment effect on absenteeism by cohort and tenure

Note: The figure shows estimates of the composite average *Money* treatment effects for each subgroup defined by cohort and tenure. The underlying specification is a variant of Equation (1). The dependent variable is  $Absence\ share_{it}$ , the absence share of apprentice  $i$  in month  $t$ , which reflects the ratio of apprentice  $i$ 's aggregate number of days absent to the total number of apprentice  $i$ 's regular work days in month  $t$ . The binary treatment indicators  $Money_{it}$  and  $Time_{it}$  indicate whether an apprentice  $i$  belongs to the respective treatment group and month  $t$  falls within the experimental period, which is from January 1, 2018 to December 31, 2018. A four-level cohort and tenure indicator captures for each apprentice  $i$  the year of training as well as a binary classification of tenure within each cohort at the start of the experiment on January 1, 2018. Specifically, *First year* and *Second year* indicates the first and second year of training, respectively. *Low tenure* and *High tenure* indicates that the tenure is weakly below and strictly above the respective cohort median, respectively. Apprentice-specific and time-specific fixed effects as well as controls for the share of vocational school days and the share of days in probation of apprentice  $i$  in month  $t$  are included. The treatment indicators and the time-specific fixed effects are interacted with the cohort and tenure indicator. The figure shows the composite average *Money* treatment effect for each subgroup, that is the average *Money* treatment effect for the reference group (*First year, Low tenure*), plus the respective interaction effect. Standard errors are clustered by store. Error bars indicate 95 % confidence intervals.

backfiring effect of the *Money* treatment appears to be driven by the cohort of the first year apprentices, notably so by the more inexperienced half of it. Conversely, for the most senior group of the apprentices, second year apprentices with above median tenure, the *Money* treatment on average actually led to a reduction in absenteeism. Overall, we document a pronounced heterogeneity of the average *Money* treatment effect with respect to the apprentices' tenure. This is consistent with the reasoning elaborated above: A more recently hired apprentice is typically more uncertain about the personal costs of absenteeism, which provides more scope for the type of signaling effect as described by Bénabou and Tirole (2003).<sup>42</sup>

<sup>42</sup>We also estimated the average treatment effects on the *intrinsic costs* index by cohort and find that the negative effect of the *Money* treatment is indeed more pronounced for the cohort of first year apprentices. See Table 11 in the Appendix for the complete results.

## 4.5 Strategic Behavior

The incentive scheme of the attendance bonus is designed such that apprentices have a new opportunity to receive a bonus point every month. The rationale for this design choice is that a larger number of days absent early in a year, for example, due a longer period of sickness, should not unduly reduce incentives to come to work later in the year. In principle, it is conceivable that the increase in absenteeism caused by the *Money* treatment is due to the apprentices strategically accumulating days of absence within those months in which they have already missed their bonus point, while otherwise behaving in accordance with the incentive scheme. Such an explanation may seemingly reconcile the apparent backfiring effect of the *Money* treatment on absenteeism with a purely standard incentive effect. It is noteworthy, however, that such behavior could not be explained by standard economic reasoning alone: The material incentive to be absent on any given day within a month in which no more bonus point can be received is never stronger among apprentices in the *Money* treatment group than among apprentices in the control group, who receive no bonus points anyway. In other words, while the marginal returns to absenteeism fall back to the level of the control group once it is clear that no more bonus point can be received in a given month, they never fall below this level. It is nevertheless worth examining how the treatments affected whether an apprentice was not absent in a given month and thus received a bonus point. If the apprentices in the *Money* treatment group, despite having more days absent overall compared to the apprentices in the control group, strategically accumulated them within only a few months, a higher overall absence share may even be consistent with a larger total number of bonus points.

Table 6 presents the results of estimating the treatment effects on receiving bonus points. The underlying specification is a variant of Equation (1), where the dependent variable is a binary indicator of whether an apprentice received a bonus point in a given month, or would have received one according to the incentive scheme.<sup>43</sup> The coefficients thus reflect the average marginal effects of the treatments on the probability of receiving a bonus point in a given month. Column (1) of Table 6 shows that the estimates of the *Money* and *Time* treatment effects are not significantly different from zero and exhibit a negative sign. Column (2) of Table 6 further shows that for the cohort of first year apprentices, who drive the overall backfiring effect, the estimate of the *Money* treatment effect on the probability of receiving a bonus point is even significantly negative and also large in magnitude.<sup>44</sup> Thus, the first year apprentices in the *Money* treatment group not only had more days absent compared to the control group, but also received significantly fewer bonus points on average.

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<sup>43</sup>More precisely, this indicator reflects whether an apprentice was not absent in a given month, which, according to the incentive scheme, results in the apprentice receiving a bonus point. However, the incentive scheme is only effective for treated apprentices and only during the experimental period. The indicator therefore reflects whether an apprentice would have received a bonus point under the incentive scheme.

<sup>44</sup>Specifically, the *Money* treatment on average reduced a first year apprentice's probability of receiving a bonus point by 8.22 percentage points. Relative to the probability of being eligible for a bonus point in a given month in the experimental period among first year apprentices in the control group, which is 78.10 percent, this corresponds to a decrease in the probability of receiving a bonus point of 10.52 percent.

Table 6: Treatment effects on receiving bonus points (by cohort)

	Dependent variable:	
	Bonus point <sub>it</sub>	
	(1)	(2)
<i>Money</i> <sub>it</sub>	-0.02635 (0.02532)	-0.08218*** (0.03080)
<i>Money</i> <sub>it</sub> × Second year <sub>i</sub>		0.16308*** (0.05424)
<i>Time</i> <sub>it</sub>	-0.00815 (0.03104)	-0.06234* (0.03560)
<i>Time</i> <sub>it</sub> × Second year <sub>i</sub>		0.15982** (0.06554)
Apprentices	346	346
Stores	232	232
Observations	5750	5750

*Note:* The table shows estimates of the average treatment effects on receiving bonus points (by cohort). The underlying specifications are variants of Equation (1). The dependent variable, *Bonus point*<sub>it</sub>, is a binary indicator of whether an apprentice *i* was not absent in month *t* and thus would have received a bonus point under the incentive scheme. *Money*<sub>it</sub> and *Time*<sub>it</sub> are binary treatment indicators of whether an apprentice *i* belongs to the respective treatment group and month *t* falls within the experimental period, which is from January 1, 2018 to December 31, 2018. Second year<sub>i</sub> is a binary second year cohort indicator of whether apprentice *i* is in the second year of training at the start of the experiment on January 1, 2018. Apprentice-specific and time-specific fixed effects as well as controls for the share of vocational school days and the share of days in probation of apprentice *i* in month *t* are included. Column (2) shows the results of a variant in which the treatment indicators and the time-specific fixed effects are interacted with the second year cohort indicator. Standard errors clustered by store are in parentheses. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.

The significantly positive estimate of effect of the interaction of the *Money* treatment indicator and the second year cohort indicator shows heterogeneity of the *Money* treatment effect in analogy to the heterogeneous effect of the *Money* treatment on absenteeism. In fact, the estimate of the composite average *Money* treatment effect on the probability of receiving a bonus point is positive and weakly significant for the cohort of second year apprentices.<sup>45</sup> However, as shown above, these apprentices do not exhibit a pronounced backfiring effect of the *Money* treatment on absenteeism in the first place. Conversely, we find no evidence of a standard incentive effect of the *Money* treatment on the probability of receiving a bonus point for first year apprentices. A merely strategic accumulation of days absent within certain months in conjunction with otherwise incentive scheme-compliant behavior can thus not explain the backfiring effect of the *Money* treatment on absenteeism.

<sup>45</sup>The estimate (standard error) of the average composite *Money* treatment effect for the cohort of second year apprentices is 0.08090 (0.04386). The corresponding *p*-value is 0.066.

#### 4.6 Persistence of the Backfiring Effect

Next, we examine whether and to what extent the backfiring effect of the *Money* treatment and the evident treatment effect heterogeneity are persistent. The finding that apprentices exhibit systematic differences in the elicited intrinsic costs of absenteeism, despite the fact that the survey was only conducted after the end of the experiment, already indicates that the *Money* treatment had a lasting effect on apprentices' perceptions. However, the question remains as to whether it also affected absenteeism persistently. To this end, we investigate how the *Money* treatment effect on absenteeism evolves over time. In addition to the four quarters of the 2018, the experimental period, we also consider the first two quarters of 2019, which we refer to as the post-experimental period.<sup>46</sup>

Figure 6 presents the results. While the attendance bonus was in fact no longer in place in the post-experimental period, the estimate of the *Money* treatment effect for the cohort of first year apprentices remains sizeable and is similar in magnitude to the effect during the experiment.<sup>47</sup> Accordingly, for the cohort of second year apprentices for whom the *Money* treatment already induced no systematic increase in absenteeism in the experimental period, no persistent detrimental effect becomes apparent. Overall, the *Money* treatment substantially and persistently increased absenteeism among the more recently hired apprentices. The monetary attendance bonus apparently undermined the injunctive social norms of behavior for more recently hired employees, who had not yet formed stable beliefs about these norms. This detrimental effect persistently continued to shape their behavior even after the end of the experiment.

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<sup>46</sup>After the end of the second quarter of 2019, most second year apprentices completed their apprenticeship.

<sup>47</sup>This finding is related to evidence by Robinson et al. (2021) who study the role of symbolic awards and find that issuing a certificate for perfect attendance on average decreased subsequent attendance among U.S. school students.

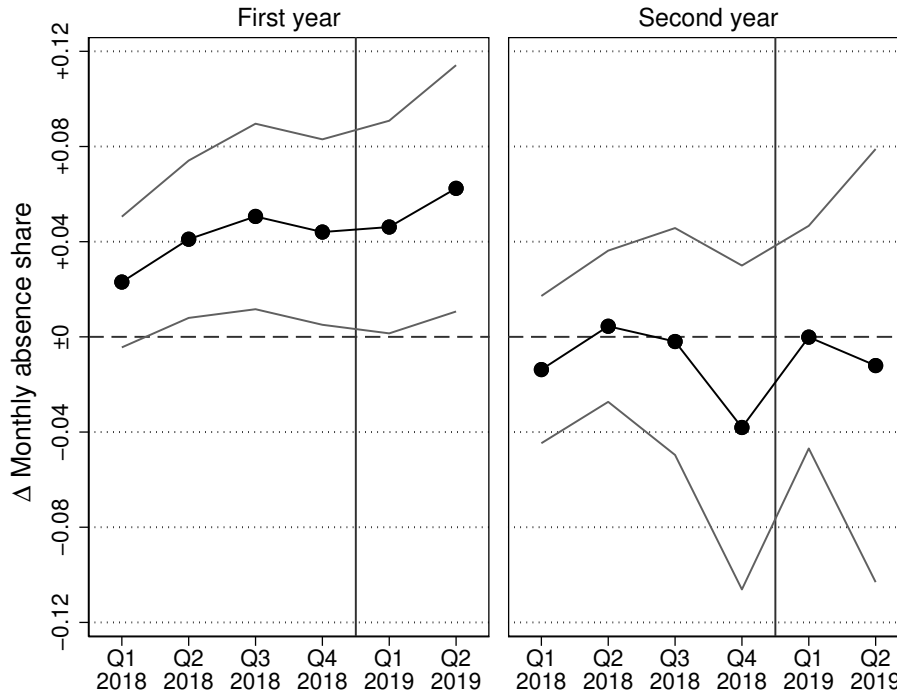


Figure 6: Evolution of the *Money* treatment effect on absenteeism over time by cohort

Note: The figure shows estimates of the composite average *Money* treatment effect on absenteeism in each quarter, separated by cohort. The underlying specification is a variant of Equation (1). The dependent variable is  $Absence_{it}$ , the absence share of apprentice  $i$  in month  $t$ , which reflects the ratio of apprentice  $i$ 's aggregate number of days absent to the total number of apprentice  $i$ 's regular work days in month  $t$ . The binary treatment indicators  $Money_{it}$  and  $Time_{it}$  indicate whether an apprentice  $i$  belongs to the respective treatment group and month  $t$  falls within the (post-)experimental period, which is from January 1, 2018 to June 30, 2019. A six-level quarter indicator captures the quarters since the start of the experiment on January 1, 2018. For example, *Q1 2018* indicates the first quarter of the experimental period. Apprentice-specific and time-specific fixed effects as well as controls for the share of vocational school days and the share of days in probation of apprentice  $i$  in month  $t$  are included. The treatment indicators are interacted with the quarter indicator. The figure shows the composite average *Money* treatment effects in each quarter since the start of the experiment, that is the average *Money* treatment effect in the first quarter of the experimental period (*Q1 2018*), plus the respective interaction effect. The specification is estimated separately for each cohort. *First year* and *Second year* indicates the first and second year of training, respectively. Standard errors are clustered by store. Ribbons indicate 95% confidence intervals.

#### 4.7 Differential Backfiring Effects Between Treatments

While we document a statistically significant and sizeable backfiring effect of the *Money* treatment, our results do not provide evidence of a corresponding effect of the *Time* treatment. The estimates of the time *Time* treatment effect are consistently close to zero. Moreover, the estimates of the *Money* and *Time* treatment effect are significantly different from each other for the cohort of first year apprentices, who drive the backfiring effect overall.<sup>48</sup> This suggests that the *Time* treatment, while likewise not leading to a systematic reduction in absenteeism, is less prone to entail a backfiring effect.

<sup>48</sup>The  $p$ -value of a Wald test of the equality of the coefficients is 0.0279.



Our survey results also reveal no systematic effect of the *Time* treatment on the intrinsic costs of absenteeism. That is, in contrast to the *Money* treatment, the *Time* treatment does not appear to have systematically shifted apprentices' perception of absenteeism as acceptable behavior. In contrast, [Table 4](#) shows a significantly positive effect of the *Time* treatment on apprentices' identification with the retail chain. More precisely, among the surveyed apprentices who receive the *Time* treatment, the *employee identification* z-score is on average more than half a standard deviation higher than among surveyed apprentices in the control group. Similarly, we find that reported job satisfaction is significantly higher among surveyed apprentices receiving the *Time* treatment than among surveyed apprentices in the control group.<sup>49</sup> This suggests that the *Time* treatment is by and large positively received by the apprentices.

The finding that the time-off attendance bonus is less prone to entail a backfiring effect compared to the monetary variant is consistent with some of the findings in the related literature. For example, [Lacetera, Macis and Slonim \(2013\)](#) provide an overview of the effects of different economic incentives on the willingness to donate blood. They conclude that the adverse effects of economic incentives on prosocial behavior tend to be mitigated when the type of the incentive evokes a less clear economic connotation. [Lacetera and Macis \(2010\)](#) find in a randomized hypothetical survey experiment that rewarding blood donations with cash would lead a substantial fraction of donors to stop donating altogether, while granting a voucher of equivalent value would not. Moreover, [Lacetera and Macis \(2013\)](#) show that an Italian law granting blood donors a paid day off work is even associated with a sizeable increase in donations. In a more workplace-related context, two notable studies find differential negative effects of removing different types of economic incentives on employees' subsequent performance. [Bareket-Bojmel, Hochman and Ariely \(2017\)](#) find in a firm-level field experiment that removing a cash bonus is associated with a slightly stronger productivity decline than removing a bonus in the form of a meal voucher.<sup>50</sup> Similarly, [Vogelsang \(2017\)](#) provides evidence from a laboratory experiment that removing performance pay leads to a less pronounced drop in performance in a real-effort task when the reward domain is time instead of money.

## 5 Concluding Remarks

Monetary incentives are a key tool for aligning potentially conflicting interests of employers and employees by motivating employees to act in the interest of the employer. Although the effectiveness of monetary incentives to enhance performance is well documented in the literature, some studies have already cast doubt on whether this relationship holds universally. Specifically, there is evidence—mostly from laboratory experiments or settings not directly related to the workplace social contexts—that monetary incentives sometimes fail to serve their intended purpose. They may even backfire, that is, achieve the exact opposite of the intended effect. However, there exists little evidence of such backfiring effects from the workplace, where monetary incentives are commonplace.

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<sup>49</sup>See Column (2) of [Table 9](#) in the [Appendix](#) for the complete results. We elicit the apprentices' general job satisfaction in the post-experimental on a six-point rating scale ranging from *completely dissatisfied* to *completely satisfied*.

<sup>50</sup>This difference is, however, not statistically significant.

In our firm-level field experiment, we investigated the effectiveness of two variants of an attendance bonus on employee absenteeism. Besides a monetary bonus, we also considered a time-off bonus in the form of additional vacation days. We find that neither of the two variants of the attendance bonus systematically reduced absenteeism. On the contrary, the monetary attendance bonus even led to a substantial increase in absenteeism, by around five additional days absent per year. The time-off bonus, on the other hand, although well received, had no systematic effect on absenteeism at all. Results from a post-experimental survey reveal that the monetary attendance bonus reduced apprentices' intrinsic costs associated with absenteeism. More precisely, we find that those apprentices for whom the monetary attendance bonus was introduced on average feel less guilty in case of being absent despite not being sick and also feel less obliged by their contract to always come to work. Thus, the monetary attendance bonus leads to absenteeism being perceived as more acceptable behavior.

We find that the backfiring effect is driven by the most recently hired employees, whose perceptions of social norms are arguably less consolidated, in contrast to more senior employees. This finding seemingly implies that the introduction of an experienced workforce would not be associated with such unintended detrimental effects. However, this conclusion is flawed in that it neglects that the introduction of such an incentive may shape the perceptions of social norms of all new hires persistently. Indeed, we find that the backfiring effect of the monetary attendance bonus for the most recently hired employees persists even after the attendance bonus is removed. Over time, the norms of the entire workforce may erode as there are more and more employees whose perceptions of social norms were persistently altered upon entry. Our results illustrate how incentives can shape social norms, and do so in a lasting way.<sup>51</sup>

A key question that inevitably arises is what our results imply for the provision of monetary incentives for other forms of behavior in the workplace. At its core is the question of how to reconcile our results with the mostly positive effects of monetary incentives found in previous firm-level field experiments. In our case, the backfiring effect of the monetary attendance bonus is likely so pronounced because there apparently existed a strong norm against absenteeism ex-ante. The signaling effect of providing monetary incentives for behavior that was previously widely considered *normal* then undermines these norms. Managers are therefore well advised to carefully examine the prevailing social norms before introducing monetary incentives.

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<sup>51</sup>In this light, another reading of our results is that field experiments with an experienced workforce may tend to underestimate the norm-shaping impact of incentives, which may only unfold over a longer time horizon as more employees with more malleable perceptions enter the workforce.

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## Appendix

Table 7: Potential contamination of the control group

	Dependent variable:	
	Absence share <sub>it</sub>	
	(1) Monthly	(2) Yearly
<i>Control</i> <sub>it</sub>	-0.00280 (0.00658)	-0.00371 (0.00666)
Employees	2488	2488
Stores	500	500
Observations	42103	4976

*Note:* The table shows estimates of the average contamination effects on absenteeism. The underlying specification is a variant of Equation (1). The dependent variable, Absence share<sub>it</sub>, is the absence share of employee *i* in period *t*, which reflects the ratio of employee *i*'s aggregate number of days absent to the total number of employee *i*'s regular work days in period *t*. *Control*<sub>it</sub> is a binary indicator of whether employee *i* is an apprentice who belongs to the control group taking part in the experiment and period *t* falls within the experimental period, which is from January 1, 2018 to December 31, 2018. The reference group consists of 2339 full-time employees working in the region's stores during the pre-experimental and experimental period. Employee-specific and time-specific fixed effects are included. Column (1) and (2) shows the results of the monthly and yearly variant, where period *t* reflects the current month and year, respectively. Standard errors clustered by store are in parentheses. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.

Table 8: Mechanism-related post-experimental survey variables for factor analysis

Variable	Survey item
<b>PSYCHOLOGICAL COSTS OF ABSENTEEISM</b>	
<i>Others' guilt</i>	"Most apprentices would have a guilty conscience if they were absent despite not being sick."
<i>Guilt</i>	"I would have a guilty conscience if I was absent despite not being sick."
<i>Obligation</i>	"I feel obliged by my contract to always come to work."
<i>Image concerns</i>	"When I am absent, I sometimes worry that my store manager thinks I am shirking."
<b>MATERIAL CONSEQUENCES OF ABSENTEEISM</b>	
<i>Rejection</i>	"... experience rejection by colleagues."
<i>Oral warning</i>	"... receive an oral warning by my store manager."
<i>Written warning</i>	"... receive a written warning."
<i>No job offer</i>	"... not receive a job offer after completing my apprenticeship."
<i>Dismissal</i>	"... be dismissed from the apprenticeship."
<b>EMPLOYEE IDENTIFICATION WITH EMPLOYER</b>	
<i>Career</i>	"I would be happy to spend the rest of my career with RETAIL CHAIN."
<i>Meaning</i>	"RETAIL CHAIN has a great deal of personal meaning to me."
<i>Own problems</i>	"I feel as if this RETAIL CHAIN's problems are my own."
<i>Belonging</i>	"I do not feel a strong sense of belonging to RETAIL CHAIN."
<i>Attached</i>	"I do not feel emotionally attached to RETAIL CHAIN."
<i>Part of family</i>	"I do not feel like 'part of the family' at RETAIL CHAIN."

*Note:* The table provides an overview of the survey items corresponding to the mechanism-related post-experimental survey variables included in the factor analysis. For the survey items related to *psychological costs of absenteeism* and *employee identification with the employer*, apprentices are asked to rate each statement on a six-point rating scale ranging from "completely disagree" to "completely agree". For the survey items related to *material consequences of absenteeism*, apprentices are asked to rate the likelihood of each stated consequence of excessive absenteeism on a six-point rating scale ranging from "very unlikely" to "very likely". For the analysis, survey variables were coded such that the value 0 and 5 corresponds to the scale minimum and maximum, respectively. Survey items related to *employee identification with employer* are based on the established "Affective Commitment Scale" (Allen and Meyer, 1990; Meyer, Allen and Smith, 1993). The variables *Belonging*, *Attached* and *Part of family* were subsequently reverse coded for the analysis as prescribed. See Figure 8d, e and g in the Online Appendix for the complete questions as presented to the apprentices in the post-experimental survey.

Table 9: Treatment effects on presenteeism tendency and job satisfaction

	Dependent variable:	
	(1) <i>Presenteeism tendency z-score<sub>i</sub></i>	(2) <i>Job satisfaction z-score<sub>i</sub></i>
<i>Money<sub>i</sub></i>	-0.50720** (0.20193)	-0.04168 (0.22307)
<i>Time<sub>i</sub></i>	-0.51014 (0.44016)	0.58181** (0.29044)
Observations	104	104

*Note:* The table shows estimates of the average treatment effects on presenteeism tendency and job satisfaction. The dependent variable *Presenteeism tendency z-score<sub>i</sub>* and *Job satisfaction z-score<sub>i</sub>* is the respective survey variable, normalized to have a mean of 0 and a variance of 1. Both survey variables were elicited in the post-experimental survey. The survey variable *Presenteeism tendency* was measured on a six-point rating scale ranging from “completely disagree” to “completely agree”. The corresponding survey item is “Sometimes I come to work despite being sick.”. The survey variable *Job satisfaction* was measured on a six-point rating scale ranging from “completely dissatisfied” to “completely satisfied”. The corresponding survey item is “How satisfied were you with your work overall [in 2018]?”. *Money<sub>i</sub>* and *Time<sub>i</sub>* are binary treatment indicators of whether an apprentice *i* belongs to the respective treatment group. Controls for the age, gender and assigned stratum of apprentice *i* are included. Standard errors clustered by store are in parentheses. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.

Table 10: Treatment effects on winsorized absenteeism (by cohort)

	Dependent variable:			
	Absence share <sub>it</sub> (winsorized)			
	Monthly		Yearly	
	(1)	(2)	(3)	(4)
<i>Money</i> <sub>it</sub>	0.01818** (0.00910)	0.03313*** (0.01158)	0.01624* (0.00866)	0.02984*** (0.01015)
<i>Money</i> <sub>it</sub> × Second year <sub>i</sub>		-0.04374** (0.01885)		-0.04296** (0.01865)
<i>Time</i> <sub>it</sub>	0.00112 (0.00875)	0.00668 (0.00916)	0.00145 (0.00925)	0.00668 (0.01019)
<i>Time</i> <sub>it</sub> × Second year <sub>i</sub>		-0.01868 (0.02130)		-0.01936 (0.02226)
Apprentices	346	346	346	346
Stores	232	232	232	232
Observations	5750	5750	692	692

*Note:* The table shows estimates of the average treatment effects on winsorized absenteeism (by cohort). The underlying specifications are variants of Equation (1). The dependent variable, Absence share<sub>it</sub> (winsorized), is the absence share of apprentice *i* in period *t*, which reflects the ratio of apprentice *i*'s aggregate number of days absent to the total number of apprentice *i*'s regular work days in period *t*, subject to 99% winsorizing. That is, the absence share of apprentice *i* in period *t* is set to the 99th percentile of the absence shares of all apprentices in period *t*, if it exceeds this value. *Money*<sub>it</sub> and *Time*<sub>it</sub> are binary treatment indicators of whether an apprentice *i* belongs to the respective treatment group and period *t* falls within the experimental period, which is from January 1, 2018 to December 31, 2018. Second year<sub>i</sub> is a binary second year cohort indicator of whether apprentice *i* is in the second year of training at the start of the experiment on January 1, 2018. Apprentice-specific and time-specific fixed effects as well as controls for the share of vocational school days and the share of days in probation of apprentice *i* in period *t* are included. Columns (2) and (4) show the results of a variant in which the treatment indicators and the time-specific fixed effects are interacted with the second year cohort indicator. Columns (1) through (2) and (3) through (4) show the results of the monthly and yearly variant, where period *t* reflects the current month and year, respectively. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.

Table 11: Treatment effects on *intrinsic costs* by cohort

	Dependent variable:
	(1)
	<i>Intrinsic costs</i>
	z-score
$Money_i$	-0.50208** (0.22669)
$Time_i$	-0.28739 (0.28233)
$Second_i$	-0.29276 (0.35382)
$Money_i \times Second\ year_i$	0.22604 (0.54059)
$Time_i \times Second\ year_i$	1.18424** (0.48270)
Observations	104

*Note:* The table shows estimates of the average treatment effects on intrinsic costs by cohort. The dependent variable *Intrinsic costs* z-score<sub>*i*</sub> is the *intrinsic costs* index, which is constructed by taking for each surveyed apprentice the mean of the variables included in the extracted factor and normalizing it to have a mean of 0 and a variance of 1. See Table 3 for the variables included in the survey factors and Table 8 in the Appendix for the corresponding survey items.  $Money_i$  and  $Time_i$  are binary treatment indicators of whether an apprentice *i* belongs to the respective treatment group.  $Second\ year_i$  is a binary second year cohort indicator of whether apprentice *i* is in the second year of training at the start of the experiment on January 1, 2018. The treatment indicators are interacted with the second year cohort indicator. Controls for the age, gender and assigned stratum of apprentice *i* are included. Standard errors clustered by store are in parentheses. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.



Table 12: Treatment effects on extensive and intensive margin of absenteeism (by cohort)

	Dependent variable:			
	Extensive Margin: Monthly number of absence spells <sub>it</sub>		Intensive Margin: Mean monthly absence spell length <sub>it</sub>	
	(1)	(2)	(3)	(4)
$Money_{it}$	-0.00555 (0.02589)	0.03865 (0.03368)	0.49951** (0.23893)	0.90185*** (0.30841)
$Money_{it} \times \text{Second year}_i$		-0.12435** (0.05470)		-1.19017** (0.46991)
$Time_{it}$	0.00286 (0.03958)	0.02868 (0.04427)	0.05777 (0.19756)	0.25676 (0.21291)
$Time_{it} \times \text{Second year}_i$		-0.07495 (0.08926)		-0.65071 (0.44275)
Apprentices	346	346	346	346
Stores	232	232	232	232
Observations	5750	5750	5750	5750

*Note:* The table shows estimates of the average treatment effects on the extensive and intensive margin of absenteeism (by cohort). The underlying specifications are variants of Equation (1). The dependent variable Monthly number of absence spells<sub>it</sub> reflects the number of absence spells that apprentice  $i$  commenced in month  $t$ . The dependent variable Mean monthly absence spell length<sub>it</sub> reflects the mean number of days that an absence spell of apprentice  $i$  lasts within month  $t$  and it takes the value 0 if apprentice  $i$  was not absent in month  $t$ .  $Money_{it}$  and  $Time_{it}$  are binary treatment indicators of whether an apprentice  $i$  belongs to the respective treatment group and month  $t$  falls within the experimental period, which is from January 1, 2018 to December 31, 2018.  $\text{Second year}_i$  is a binary second year cohort indicator of whether apprentice  $i$  is in the second year of training at the start of the experiment on January 1, 2018. Apprentice-specific and time-specific fixed effects as well as controls for the share of vocational school days and the share of days in probation of apprentice  $i$  in month  $t$  are included. Columns (2) and (4) show the results of a variant in which the treatment indicators and the time-specific fixed effects are interacted with the second year cohort indicator. \*, \*\*, \*\*\* indicate significance at the 10%, 5% and 1% level, respectively.

## Online Appendix

*<intended for online publication>*

# Retail Chain Letterhead

Retail chain  
Regional management address

Retail chain , Regional management address

Apprentice name  
Apprentice address

28.12.2017

Dear Apprentice name,

in the coming time, the project *Attendance Bonus Apprentices* will be implemented in this region.

From January 2018 to December 2018, you will therefore receive one point for each month without any day of absence. The number of points collected will be converted into euros at the end of the year and transferred to your employee card in February 2019.<sup>1</sup>

Points	Reward
3-5	60€
6-8	120€
9-11	180€
12	240€

You will receive written feedback on your current score once a quarter. Please note that payoff is contingent upon the ongoing apprenticeship on the payoff date on January 31, 2019.

Some of the other apprentices receive the bonus for a different period or in a different form. The apprentices were randomly assigned for this purpose.

If you have any questions, please do not hesitate to contact your training manager or the experts from the competence center.

Sincerely,

*Signature*

Human Resources Manager

<sup>1</sup>The following are counted as days of absence: unexcused absence, sickness with certificate, sickness without certificate, cure or rehab, reintegration and stress tests. Days of absence from vocational school will also be considered.

(a) Money treatment

Figure 7: First letter to apprentices about attendance bonus

# Retail Chain Letterhead

Retail chain  
Regional management address

Retail chain , Regional management address

Apprentice name  
Apprentice address

28.12.2017

Dear Apprentice name,

in the coming time, the project *Attendance Bonus Apprentices* will be implemented in this region.

From January 2018 to December 2018, you will therefore receive one point for each month without any day of absence. The number of points collected will be converted into additional vacation days at the end of the year. The vacation days will then be at your disposal from February 2019.<sup>1</sup>

Points	Reward
3-5	1 vacation day
6-8	2 vacation days
9-11	3 vacation days
12	4 vacation days

You will receive written feedback on your current score once a quarter. Please note that payoff is contingent upon the ongoing apprenticeship on the payoff date on January 31, 2019.

Some of the other apprentices receive the bonus for a different period or in a different form. The apprentices were randomly assigned for this purpose.

If you have any questions, please do not hesitate to contact your training manager or the experts from the competence center.

Sincerely,

*Signature*

Human Resources Manager

<sup>1</sup>The following are counted as days of absence: unexcused absence, sickness with certificate, sickness without certificate, cure or rehab, reintegration and stress tests. Days of absence from vocational school will also be considered.

(b) *Time* treatment

Figure 7: First letter to apprentices about attendance bonus

# Retail Chain Letterhead

Retail chain  
Regional management address

Retail chain , Regional management address

Apprentice name  
Apprentice address

28.12.2017

Dear Apprentice name,

in the coming time, the project *Attendance Bonus Apprentices* will be implemented in this region. The project is carried out in different forms and at different times. The apprentices were randomly assigned for this purpose.

For you, the project will be relevant at a later point in time. You will be informed in due course.

If you have any questions, please do not hesitate to contact your training manager or the experts from the competence center.

Sincerely,

*Signature*

Human Resources Manager

(c) Control group

Figure 7: First letter to apprentices about attendance bonus

Thank you for your willingness to participate in this short survey “Job Satisfaction and Absenteeism among Apprentices 2019”.

We would like to ask you briefly about your work at RETAIL CHAIN in the past period. The survey is conducted by UNIVERSITY and is therefore **absolutely anonymous**. Apart from UNIVERSITY, no one will gain access to the completed surveys. RETAIL CHAIN will later only receive average values averaged over at least 20 apprentices.

The login credentials for the online survey are used to ensure correct store allocation. UNIVERSITY may link the data to other key figures. However, it is not possible for RETAIL CHAIN to draw conclusions about persons, activities or key figures at any time.

(a) Screen 1

If you think about your work at RETAIL CHAIN in 2018:

**How satisfied were you with the following aspects?**

Please tick one value on the scale for each question:

If you were completely **satisfied**, the value **1**.

If you were completely **dissatisfied**, the value **6**.

If you were **partly satisfied/partly dissatisfied**, a value in between.

**How satisfied were you ...**

		1	2	3	4	5	6	
1. ... with your work overall?	completely satisfied	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely dissatisfied
2. ... with your compensation?	completely satisfied	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely dissatisfied
3. ... with your working hours?	completely satisfied	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely dissatisfied
4. ... with your workload?	completely satisfied	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely dissatisfied
5. ... with the fair treatment by the company?	completely satisfied	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely dissatisfied
6. ... with your health condition?	completely satisfied	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely dissatisfied

(b) Screen 2

In the following, your personal assessment is requested.

Please estimate the average number of days absent (in days per year) of an apprentice at RETAIL CHAIN ...

- 1. ... in 2018  Day(s)
- 2. ... in 2017  Day(s)

Please estimate the average number of these days on which an apprentice at RETAIL CHAIN is absent even though he or she is **not** actually sick.

- 3. ... in 2018  Day(s)
- 4. ... in 2017  Day(s)

(c) Screen 3

Figure 8: Post-experimental survey (see note on page 51)

Please rate how likely it is that the following consequences will occur if an apprentice is absent too many days.

Please tick one value on the scale for each statement:

If it is very **likely**, the value **1**.

If it is very **unlikely**, the value **6**.

If it is **partly likely/partly unlikely**, a value in between.

**How likely is it that, as a result of too many days absent, you will ...**

1 2 3 4 5 6

- |  |             |                       |                       |                       |                       |                       |                       |               |
|--|-------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|---------------|
| 1. ... experience rejection by colleagues.                         | very likely | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | very unlikely |
| 2. ... receive an oral warning by my store manager.                | very likely | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | very unlikely |
| 3. ... receive a written warning.                                  | very likely | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | very unlikely |
| 4. ... not receive a job offer after completing my apprenticeship. | very likely | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | very unlikely |
| 5. ... be dismissed from the apprenticeship.                       | very likely | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | very unlikely |

(d) Screen 4

**How do you rate the following statements?**

Please tick one value on the scale for each statement:

If you completely **agree**, the value **1**.

If you completely **disagree**, the value **6**.

If you **partly agree/partly disagree**, a value in between.

1 2 3 4 5 6

- |  |                  |                       |                       |                       |                       |                       |                       |                     |
|--|------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|---------------------|
| 1. Most apprentices would have a guilty conscience if they were absent despite not being sick. | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 2. I would have a guilty conscience if I was absent despite not being sick.                    | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 3. I feel obliged by my contract to always come to work.                                       | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 4. Sometimes I come to work despite being sick.  | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 5. When I am absent, I sometimes worry that my store manager thinks I am shirking.             | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |

(e) Screen 5

Figure 8: Post-experimental survey (see note on page 51)



Please think again about the year 2018. Now think of an **ordinary month**. Please indicate your answers to the following questions in **working days per month**.

- |   |                             |
|---|-----------------------------|
| 1. On how many days did you have to work at inconvenient hours?     | <input type="text"/> Day(s) |
| 2. On how many days would you rather have stayed at home?           | <input type="text"/> Day(s) |
| 3. On how many days did you not work because you were sick?         | <input type="text"/> Day(s) |
| 4. On how many days did you not work even though you were not sick? | <input type="text"/> Day(s) |
| 5. On how many days did you work even though you were sick?         | <input type="text"/> Day(s) |

(f) Screen 6

**How do you rate the following statements?**

Please tick one value on the scale for each statement:

If you completely **agree**, the value **1**.

If you completely **disagree**, the value **6**.

If you **partly agree/partly disagree**, a value in between.

- |   |                  | 1                     | 2                     | 3                     | 4                     | 5                     | 6                     |                     |
|---|------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|---------------------|
| 1. I would be happy to spend the rest of my career with RETAIL CHAIN. | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 2. RETAIL CHAIN has a great deal of personal meaning to me.           | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 3. I feel as if this RETAIL CHAIN's problems are my own.              | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 4. I do not feel a strong sense of belonging to RETAIL CHAIN.         | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 5. I do not feel emotionally attached to RETAIL CHAIN.                | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |
| 6. I do not feel like "part of the family" at RETAIL CHAIN.           | completely agree | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | <input type="radio"/> | completely disagree |

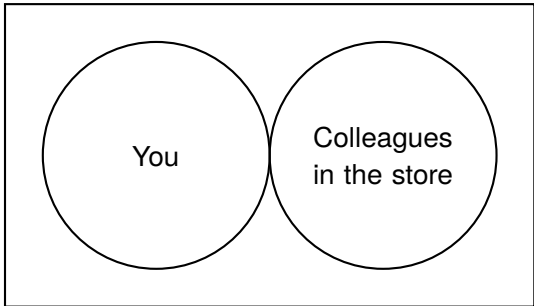
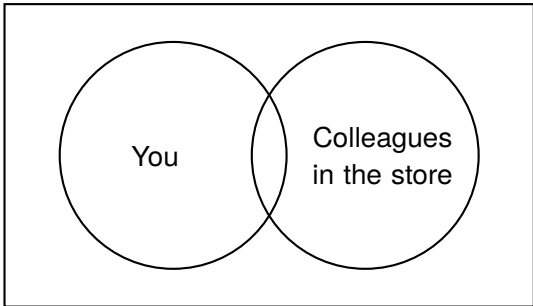
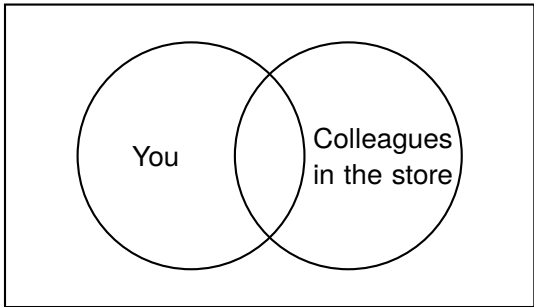
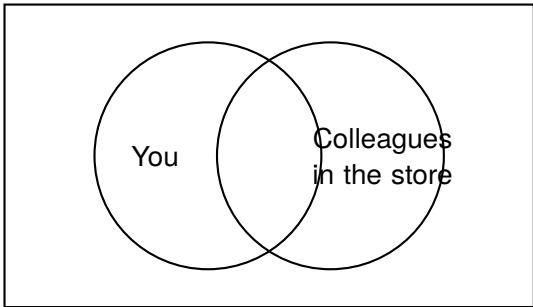
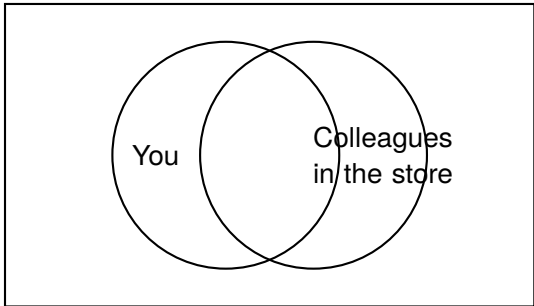
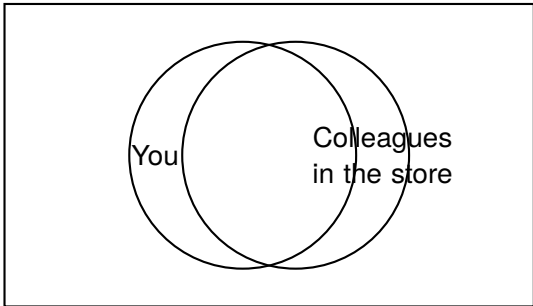
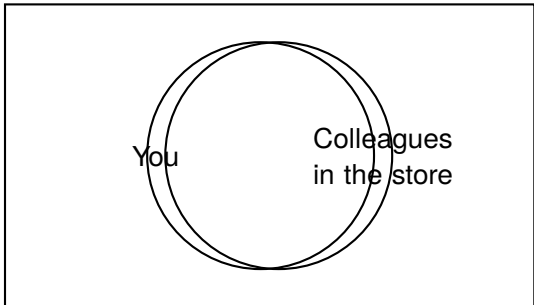
(g) Screen 7

Figure 8: Post-experimental survey (see note on page 51)

Please think again about the year 2018.

If you think about the relationship between you and your colleagues in the store:

**Which of the following figures best represents this relationship?**

<input type="radio"/>		<input type="radio"/>	
<input type="radio"/>		<input type="radio"/>	
<input type="radio"/>		<input type="radio"/>	
<input type="radio"/>			

(h) Screen 8

Figure 8: Post-experimental survey (see note on page 51)

Please enter an amount each to complete the sentences below.

*If there is no corresponding amount for you, please enter "0". Recall that the following statements are purely hypothetical. In no case will you actually have to make or receive a payment.*

**Imagine the following situation: It is the middle of the year and you have already used half of your annual vacation.**

"For a monetary amount of at least  euros, I would give up one of my vacation days."

"For a monetary amount of at most  euros, I would purchase an additional vacation day."

(i) Screen 9

Now we would like to know something about you as a person.

We remind you once again that the survey is conducted by UNIVERSITY and is therefore **absolutely anonymous**. Apart from UNIVERSITY, no one will gain access to the completed surveys. RETAIL CHAIN will later only receive average values averaged over at least 20 apprentices.

**How do you rate the following statements?**

Please tick one value on the scale for each statement:

If you completely **agree**, the value **1**.

If you completely **disagree**, the value **6**.

If you **partly agree/partly disagree**, a value in between.

	1	2	3	4	5	6	
1. If someone does me a favor, I am willing to return it.	completely agree	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely disagree
2. If someone harms me on purpose, I will try to pay that person back in kind, even if it means a cost to me.	completely agree	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely disagree
3. I give up something today so that I can afford more tomorrow.	completely agree	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely disagree
4. I tend to put things off until later, even when it would be better to do them right away.	completely agree	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely disagree
5. I am rather reserved.	completely agree	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely disagree
6. I trust others easily, I believe in the good in people.	completely agree	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely disagree
7. I am comfortable and prone to laziness.	completely agree	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely disagree
	:						

(j) Screen 10

Figure 8: Post-experimental survey (see note on page 51)

		1	2	3	4	5	6		
8.	I am relaxed and do not let stress disturb me.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
9.	I have little artistic interest.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
10.	I am outgoing, I am sociable.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
11.	I tend to criticize others.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
12.	I complete tasks thoroughly.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
13.	I get nervous and insecure easily.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
14.	I have an active imagination, I am creative.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
15.	I am efficient and work fast.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
16.	I make plans and carry them out.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
17.	I am reliable and conscientious.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
18.	I do not give up until the task is done.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
19.	I am easily distracted, do not stay on task.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
20.	I can be a little careless.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>
21.	I tend to be messy.	completely <b>agree</b>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	completely <b>disagree</b>

(k) Screen 10 (continued)

Please recall the project "Attendance Bonus Apprentices" at RETAIL CHAIN in 2018. To which group did you belong?

I had the opportunity to receive a bonus in the form of money.

I had the opportunity to receive a bonus in the form of vacation days.

I was not yet assigned to a group and will receive a comparable bonus later.

(l) Screen 11

Figure 8: Post-experimental survey (see note on page 51)

1. How often did you talk to other apprentices about the project "Attendance Bonus" in 2018?  
 Times

2. Did your store manager know you were participating in the project "Attendance Bonus"?  
 Yes  
 No

3. Did your store manager know you were receiving a bonus in the form of money?  
 Yes  
 No

4. Would you rather have had the opportunity to receive a bonus in the form of vacation days?  
 Yes  
 No

3. Did your store manager know you were receiving a bonus in the form of vacation days?  
 Yes  
 No

4. Would you rather have had the opportunity to receive a bonus in the form of money?  
 Yes  
 No

(m) Screen 12

5. In your opinion, to what extent has the project "Attendance Bonus" influenced the behavior of the participating apprentices?

(n) Screen 13

6. What did you think about the project "Attendance Bonus" in general?

(o) Screen 14

Figure 8: Post-experimental survey

*Note:* The figure shows the screen content of the computerized post-experimental survey. It was implemented using *SoSci Survey* and made available to apprentices via a local survey server of the university. The apprentices were sent a letter with the URL including individual login credentials. The questions presented in [Figure 8b, d, e, g and j](#) use six-point rating scales ranging from 1 (maximum) to 6 (minimum), which correspond to the typical German school grading system. We use this representation because apprentices are familiar with it. For the analysis, however, we code the responses to correspond to a six-point rating scale ranging from 0 (minimum) to 5 (maximum). See [Table 8](#) in the [Appendix](#) for further information on how the variables collected in the post-experimental survey are used in the analysis.