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RESEARCH ARTICLE

Unethical employee behavior against coworkers following unkind management treatment: An experimental analysis

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We study unethical behavior toward unrelated coworkers as a response to managerial unkindness with two experiments. In our lab experiment, we do not find that subjects who experienced unkindness are more likely to cheat in a subsequent competition against another coworker who simultaneously experienced mistreatment. A subsequent survey experiment suggests that behavior in the lab can be explained by individuals' preferences for norm adherence, because unkind management behavior does not alter the perceived moral appropriateness of cheating. However, having no shared experience of managerial unkindness opens up some moral wiggle room for employees to misbehave at the costs of others.

1 | INTRODUCTION

Counterproductive work behavior hurts or is intended to hurt the organization or members of the organization, and its origin has been looked at from different theoretical perspectives (see Spector & Fox, 2002). One of these perspectives is the organizational justice approach. Skarlicki and Folger (1997) show that the response to perceived organizational unfairness goes beyond the classical employee satisfaction–performance relationship, focusing on retaliatory behaviors such as purposely damaged equipment. Therefore, organizational justice is decomposed into distributive, procedural, and interactional justice. The latter is a subset of procedural justice, and it is of special interest because it deals with the question how supervisors treat their employees—which is relevant for all organizational levels and day-to-day work. However, given the hierarchical system of organizations and the thereof resulting fear of losing the job, directly or indirectly reciprocal behaviors may not be a feasible option for unkindly treated employees, which raises the question whether these disgruntlements can disrupt workplace behavior in a less obvious way. Bennet and Robinson (2000) refer to counterproductive work behaviors as workplace deviance and argue that an employee will choose from a set of deviant behaviors, for example, the most feasibly or least costly,

depending on the situational context. If retaliation of the supervisor's behavior seems too costly but emotions caused by the supervisor's treatment motivate employees to engage in deviant behavior, they might choose an alternative target that is less powerful than the supervisor, even if this target is not responsible for the experienced mistreatment. Then, managerial unkindness may shape deviant behavior far beyond the known channel of direct reciprocity and impact decision making in less obvious and indirect ways. Especially at workplaces where cooperation and personal interactions are of utmost importance, a contagious spread of unethical behavior from one employee to the other would become an imminent threat for the firm. A better understanding of how these behaviors move across personnel may inform employers and policy makers about regulatory needs and the adequate extent of costly administrative measures (i.e., employee monitoring) in such workplace environments. Hence, this study seeks to answer the question whether perceived interactional injustice will be passed on to unrelated coworkers if there is no possibility to hurt the original perpetrator.

To test whether unkind management behavior triggers counterproductive work behavior that purposely makes coworkers worse off, we first conducted a laboratory experiment in which participants were assigned to working groups of three, with one group leader. For each group, the computer allocated two distinct real effort tasks (counting zeros vs. rating jokes), which differed regarding their perceived

[Correction added on 15 March 2021, after first online publication: Projekt Deal funding statement has been added.]

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pleasantness. The computer allocated the unpleasant counting task either to the group leader or to the two remaining group members. Afterward, the group leader was allowed to overrule the task allocation by switching, for example, from the counting to the more pleasant rating task. Because this particular reallocation is expected to trigger feelings of unkindness, we refer to it as the unkind treatment. After the first period, no further interactions between the group leader and the other two group members took place. The latter, however, received a new working task for which they could receive an additional monetary bonus if they performed better than the other group member. Here, subjects had the possibility to act unethical by manipulating the tournament. Our analysis reveals that more than half of the subjects (60%) chose to cheat. Comparing the treatment and control group, we do not observe a statistically significant difference, neither at the extensive nor at the intensive margin.

The tendency to engage in counterproductive work behavior toward coworkers, however, might heavily depend on the personal relationship between those involved. The social identity theory (see, e.g., Akerlof & Kranton, 2000; Tajfel & Turner, 1979) suggests that individuals who belong to the same group develop preferences for one another, which may alter decision making. Given that only knowing about identical painting preferences (i.e., Kandinsky vs. Klee; see Chen & Li, 2009) induces group identity, the shared experience of interactional injustice should establish an even stronger feeling of belonging to the same group, but the empirical evidence for group identity and unethical behavior is rather mixed. Kato and Shu (2016), for example, show that Chinese workers tend to engage more frequently in aggressive competitive behavior when they face coworkers with a distinct group identity (urban vs. rural migrant workers). Benistant and Villeval (2019), however, revisit this question in a lab experiment and do not find evidence for an influence of shared group identity on sabotage in tournaments. Meub et al. (2016) test whether a shared group identity has an impact on participants' truth telling. Lying increased their own payoff while reducing the payoff of another participant. The authors find more honest reports when the deprived party shared the same group identity (fellow student vs experimenter). Similar results were obtained by Hermann and Ostermaier (2018) and Soraperra et al. (2019).

Given that people care about the social appropriateness of their actions (Krupka & Weber, 2013) but a sin does not always seem to be seen as a sin when motive and opportunity exist to cross social norms (Liu et al., 2014), one might hypothesize that supervisors' unkindness is more likely to affect the social appropriateness of counterproductive work behavior in the absence of a shared experience. Therefore, we conducted an online survey experiment with new participants to evaluate the appropriateness of cheating in multiple hypothetical workplace scenarios closely related to our lab experiment. The scenarios build upon two employees in a fast food restaurant who compete for a monthly bonus. The winner is determined by the number of satisfied customers. For each customer who did not rate her service experience, the employee has the opportunity to leave a positive evaluation (i.e., the opportunity to cheat). Survey respondents were then asked to judge the moral appropriateness of cheating. Two scenarios

replicated the lab experiment, framing a situation in which both employees previously suffered (or not) from unkind supervisor treatment. In line with our findings from the lab, we do not observe any differences between the control and treatment group so that the perceived social norm not to hurt a fellow in-group member is a likely candidate to explain subjects' observed behavior. In two additional scenarios, we varied the competition in that the competitor for the bonus is not the former colleague. In case of having no common history, moderate levels of cheating are indeed assessed to be less morally inappropriate after suffering from interactional injustice.

Taken together, by conducting two experimental studies that rule out both imitation behavior and retaliation motives, we show that employees do not blindly engage in counterproductive work behavior toward their coworkers after experiencing interactional injustice. Nevertheless, supervisors should not interpret this finding as a justification for not thinking about interactional justice because our data also indicate that experienced unkindness opens some moral wiggle room for unethical employee behavior, which might especially spread in workplaces characterized by loose and anonymous employee structures.

2 | RELATED LITERATURE

2.1 | Generalized reciprocity

One way to conceptualize the behavioral pattern of passing on perceived (un)kindness to an unrelated third party is called generalized reciprocity (Herne et al., 2013; Strang et al., 2016).¹ In recent years, an increasing number of scholars employing experimental methods to establish causal relationships started to take an interest in the concept of both positive and negative generalized reciprocity across different settings. Regarding positive generalized reciprocity, Stanca (2009) shows that people have a tendency to pass on perceived prosocial behavior in a gift-exchange lab experiment. Mujcic and Leibbrandt (2018) replicated this prosocial tendency in a large field experiment: People who were granted way in a big parking lot had a higher probability to grant way to other cars in a subsequent traffic interaction. Nowak and Sigmund (2005) describe generalized positive reciprocity as an important evolutionary prerequisite for human development. In contrast, Gray et al. (2014) examine the tendency to pass on unkind experiences to innocent others. Across multiple allocation experiments, the authors find that participants who received unkind shares are more likely to choose unkind allocations themselves. Whereas Schnedler and Stephan (2020) do not observe that unkindly treated individuals share less of their endowment with an anonymous receiver in a subsequent dictator game, Strang et al. (2016) highlight the role of negative emotions triggered in unkindly treated individuals. Offering individuals the chance to write a letter to the source of unkindness helped them to regulate their emotional state and significantly increased shares allocated to third parties. More closely related to the workplace is the laboratory experiment by Zdaniuk and Bobocel (2013) who show that only being confronted with the face of a fair or unfair leader had a significant impact on the interactional

fairness of communicating a dismissal decision afterward to an unrelated other. Somehow related to the idea of generalized reciprocity, Houser et al. (2012) find that after receiving an unfair share in a dictator game, participants have a higher probability to lie when they are asked for private information in a subsequent unrelated die-roll game. Della Valle and Ploner (2017) replicate this finding and argue that this behavior aims at restoring one's own subjective fairness balance. This suggests, however, that generalized negative reciprocity is not driven by the intention to hurt someone, and, hence, perceived interactional injustice might not trigger counterproductive work behavior toward coworkers—especially because employees seem to care for the well-being of even unknown coworkers by punishing their employer with reduced performance after layoffs (Heinz et al., 2020).

2.2 | Trickle effects

Closely related to the concept of generalized reciprocity is the literature on the so-called trickle effects. In the context of organizational behavior, trickle effects describe how behavioral patterns, feelings, or emotions are passed down or across hierarchal layers, causing a contagious spread throughout entire companies (Masterson, 2001). Most studies examined how decisions originating at the top management level may ultimately interact with behavioral choices at lower hierarchical levels through the transmitting channel of mid-management personnel or intermediate supervisors (for a comprehensive review of such trickle effects in the workplace, see Wo et al., 2019). Multiple theoretical mechanisms have been proposed to conceptualize the decision-making process behind trickle effects in organizational behavior, with “social learning theory” (Bandura, 1986, 1977), “social exchange theory” (Blau, 1964) and “displaced aggression” (Marcus-Newhall et al., 2000; Tedeschi & Norman, 1985) being the most popular ones.

The concept of social learning theory builds on the idea that individuals imitate the observed behavior of their role models (e.g., supervisors) and reuse these experiences as a guide for own supervising practices in future interactions. The social exchange theory is based on normative beliefs. Focusing on the concept of reciprocity, social exchange theory predicts that individuals who experience kind treatment from their supervisors may feel obliged to pass on kindness to others. The concept, however, faces some limitations in conceptualizing trickle effects given the absence of normative beliefs that stress the moral obligation to displace negative experiences toward innocent others. Whereas both approaches describe a cognitive-based behavioral mechanism, displaced aggression conceptualizes a more affect-driven behavior moderated by negative emotions (Wo et al., 2015, 2019). In particular, displaced aggression describes the tendency in human behavior to rechannel experienced frustration, anger, or harmful behavior away from the originating source in fear of future consequences. Instead, victimized individuals may choose to unleash their negative emotions on unrelated others such as lower ranked subordinates, fellow coworkers, or even close family members (Hoobler & Hu, 2013; Restubog et al., 2011), triggering a contagious spread of unkind behavior in interpersonal exchange.

In a study based on survey data obtained from Chinese employees, Aryee et al. (2007) show that supervisors' perception of experienced interactional injustice trickles down to shape their own subordinates' perception of interactional injustice and in turn undermined subordinates' workplace behavior in regard to both organization- and individual-oriented organizational citizenship. Their findings are in line with the concept of displaced aggression, noting that supervisors who are treated unkindly by their direct superiors refrain from fighting back but rather rechannel their frustrations on to their own subordinates. Closely related to our research question is the study by Mawritz et al. (2012), which bases its empirical foundation on US survey data. The authors' analysis confirms the findings by Aryee et al. (2007), showing that abusive leadership at the top management level is positively associated with abusive supervisor behavior at the mid-management level. Moreover, their data reveal that subordinates who suffered from abuse are more likely to show deviant interpersonal behavior against other coworkers within their own work groups. The relationship between abusive supervision and interpersonal deviance, however, is moderated by the underlying work atmosphere, highlighting that personal ties among coworkers can serve as an effective buffer against displaced unkindness. Hence, their results emphasize the sensitivity of trickle effects with respect to social preferences and personal characteristics. By conducting two experimental studies building one on another, we want to derive causal insights on the assumed relationship and therefore to address a frequently mentioned limitation discussed in the abovementioned literature, whose empirical conclusions build almost exclusively on cross-sectional survey data (Wo et al., 2019). Furthermore, our controlled laboratory environment allows us to cleanly isolate behavioral spillovers from one individual to another while eliminating concerns that decision making is confounded by the individual desire to retaliate back the perpetrator through the interaction with third parties, a concern that cannot be neglected when relying on survey data.

3 | EXPERIMENTAL DESIGN

We conducted the laboratory experiment with a total of 249 student participants within nine sessions at the Magdeburg Experimental Laboratory of Economic Research (MaxLab) located at University of Magdeburg using “hroot” (Bock et al., 2014) for subject recruitment. The experimental design was executed in z-Tree (Fischbacher, 2007).² On average, participants earned 11.60 € (including a 5€ show-up fee) for a 1-h session. The experiment consisted of two stages and a follow-up questionnaire. Subjects were randomly allocated into groups of three with one subject as the group leader and two regular members.

3.1 | Stage 1

Each group had to work on two different real effort tasks, but the allocation of tasks to the subjects was done by the computer, which was common knowledge. Participants were paid a fixed wage of

200 taler (with 100 taler = 1 euro), and the working time was 10 min. Note that both the payment and the working time were identical for both tasks. We designed the tasks in such a way that they differed with regard to their perceived pleasantness (unpleasant vs. pleasant task), an idea similarly used by Gray et al. (2014) and Schnedler and Stephan (2020). To make the difference in pleasantness between the two tasks salient, all subjects received the instructions for both tasks and took part in 2-min trial rounds.

One task was the *counting task* used by Abeler et al. (2011), in which subjects had to count the number of zeros within 15*15 matrices filled with randomly ordered zeros and ones.³ It was crucial for our design that the counting tasks was perceived as being unpleasant. For this reason, we modified the version used by Abeler et al. (2011) and extended their original matrix size together with a reduction in font size. The task demands no specific skill set but a substantial level of mental focus. As mentioned above, we offered a fixed wage, but in order to ensure effort exertion, we asked for a minimum number of solved matrices necessary to receive the offered fixed wage. In the second task, the so-called rating task, subjects were asked to read and rate jokes on a slider from 0 (*not funny*) to 100 (*very funny*). Contrary to the counting task, we intended subjects to enjoy reading the jokes so that we did not ask for a minimum number of rated jokes and the fixed wage was guaranteed. Comparing the characteristics of the two tasks, we assumed that the overwhelming majority of people would prefer to read jokes instead of engaging in a tedious counting activity.

After finishing the second trial period, all participants were informed about the task allocation within their group. We designed the program so that only two task allocation settings were realized.⁴ In Setting 1, the tedious counting task was assigned to the group leader, whereas the two remaining group members were allocated the rating task. In Setting 2, the allocation was reversed. Independent of the setting, the group leader learned that the allocated task would be her working task for Stage 2 as well. Before starting the 10-min working phase, we asked the group leader whether she wants to overrule the task allocation by exchanging her allocated tasks with the ones allocated to her group members. It was made clear to the group leader that there will be no further interactions between her and her group members in Stage 2. This ensured free decision making, avoiding false beliefs about future dependencies and potential retaliation by the group members. Note that we allowed only for a swap between Settings 1 and 2; the leader could not exchange only one task with a particular group member. If the leader decided to switch task allocations, the group members were informed about the intervention; otherwise, no information was shared. Because reallocating the group members' pleasant for the unpleasant task is expected to trigger feelings of unkindness, we refer to the task exchange in Setting 1 as being our unkind treatment. Consequently, affected group members in Setting 1 will represent the treatment group. The control group consists of members assigned to Setting 2 if no task exchange took place. In short, our analysis compares group members who both worked on the tedious counting task but differ in the way they received this unpleasant task, either by the neutral computer program or an (unkind) personal decision made by the group leader.

Afterward, the 10-min working period started. At the end of this first stage, we asked subjects to self-report their emotions using an extended⁵ version of the Positive and Negative Affect Schedule (PANAS) questionnaire (Watson et al., 1988) to assess potential differences in the emotional state of mind between the treatment and control group.

3.2 | Stage 2

In order to analyze generalized (instead of direct or indirect) negative reciprocity, it was clearly stressed to all subjects that the former group structure had been broken apart. The group members learned that their former group leader would now be working independently on an unrelated real effort task, without any further interactions or dependencies. As already mentioned, the group leader had to work on the previously allocated task once again, this time for 5 min and a fixed wage of 250 taler.

Given that the present study aims at investigating unethical behavior toward coworkers, we introduced a new real effort task. Similar to Dato and Nieken (2014) and Erkal et al. (2011), subjects were asked to encode words into numbers⁶ for 5 min (after a 2-min trial round) with a fixed wage of 250 taler. It was made clear that both former group members would be working on the encoding task, competing against each other for a bonus payment of another 250 taler. The winner of this tournament was determined by the number of points subjects had after finishing the task, with one point for each word that was correctly encoded. However, subjects had the possibility to modify the tournament outcome before its start in order to increase their probability of winning the monetary bonus. Similar to Charness et al. (2014), two modification options existed: Subjects could either increase their own point score (redemption) or decrease the point score of their opponent (sabotage).⁷ In contrast to Charness et al. (2014), in which redemption and sabotage were implemented in separate treatments, we allowed contestants to freely choose between both options. If subjects chose to increase their own score, they could buy up to 20 points at a cost of 10 taler for each point bought. If the sabotage option was chosen, subjects could reduce the point score of their opponent in the same size and at the same cost. Of course, subjects could refrain from any modification. Even though there is no payoff relevant difference between sabotage and redemption, we intentionally introduced both options simultaneously because different forms of unethical behavior are likely to bear different moral costs for individuals (Hermann & Mußhoff, 2019). Compared with cheating on one's own outcome, choosing to sabotage within the tournament clearly represents the intention to hurt the counterpart. By comparing modification choices within the treatment and control group, our design allows us to investigate whether this relative cost structure changes after experiencing the unkind treatment and subjects become more prone to hurt someone else directly. Following the decision on whether to cheat and if so, to what extent, the 5-min working period started. Figure 1 provides an overview of the main experimental design features. It is important to highlight that our experiment examines workplace behavior across two

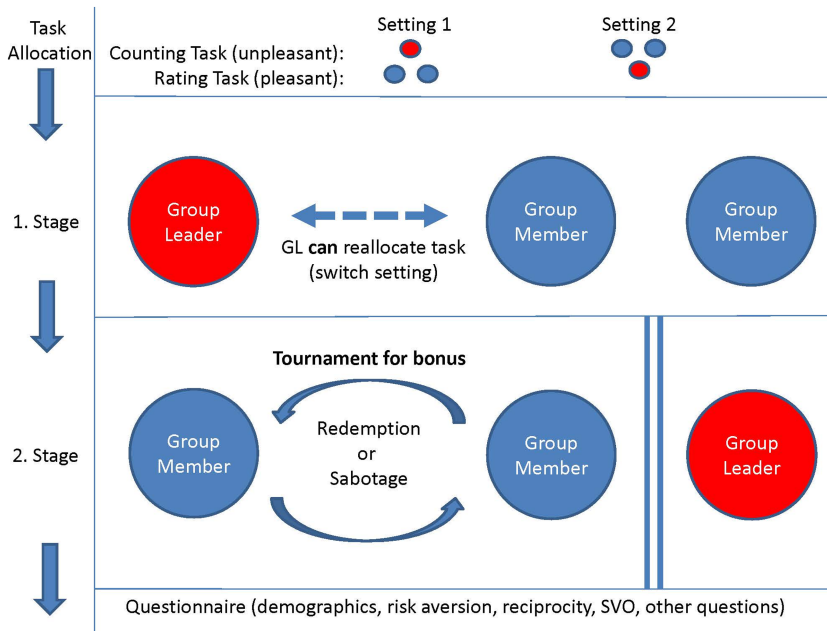


FIGURE 1 The stages of the experimental design [Colour figure can be viewed at wileyonlinelibrary.com]

distinct task dimensions. This ensures that modification choices in Stage 2 are unlikely to be driven by imitation or learning experiences, derived from engaging in the allocation task of Stage 1. Furthermore, by strictly separating group manager and group members after entering Stage 2, we guarantee that differences in modification choices between treatment and control group cannot be explained by retaliation motives against the manager.

After finishing the working period in Stage 2, we elicited subjects' beliefs regarding their opponent's modification behavior. If they correctly assessed their opponent's type of modification (or no modification at all) in a first step, subjects received a fixed payment of 150 taler. In a second step, they were asked about the extent of the opponent's modification.⁸ To elicit beliefs as accurately as possible, we employed an incentive structure similar to Gächter and Renner (2010) where the payoff decreases quickly with less accurate beliefs. For a correct guess, subjects could earn 500 taler.⁹ For all other guesses, subjects received 250 taler divided by the absolute estimation error. Finally, participants were asked to answer a short questionnaire that contained questions regarding the experiment (manipulation checks), some sociodemographic characteristics, risk attitudes, and reciprocal behavior using the reduced six-item version of the original scale developed by Perugini et al. (2003). Additionally, we elicited subject's social value orientation (SVO) (Murphy et al., 2011) and personality traits within a reduced form of the Big Five personality framework (Costa & McCrae, 1989).

4 | RESULTS

4.1 | Modification choices

Due to the fact that our treatment and control groups were realized endogenously through choices made by the group leaders, we ended

up with 60 regular group member subjects in the treatment group and 80 subjects in the control group. More than half of these subjects (60%) chose to cheat in the tournament to increase their winning chances. Comparing treatment and control, we do not observe a statistically significant difference, the share of subjects who decided to cheat is even slightly higher in the control than in the treatment group (61.25 vs. 58.33%, $\chi^2[2] = 0.21, p = 0.899$).^{10,11} Similarly, the average number of modification points bought does not differ between control and treatment group (3.59 vs. 3.47, $p = 0.737$).^{12,13} Figure 2 illustrates subjects' modification choices including the differentiation between redemption and sabotage. In line with our suggestion that sabotage might induce higher moral costs, we find that subjects chose the redemption option more often,¹⁴ but again, there is no difference between treated and untreated subjects.

Looking at the intensive margin (see Figure 3), a similar picture emerges: Conditioned on modifying the tournament via the redemption option, the control group bought on average 6.10 points, whereas

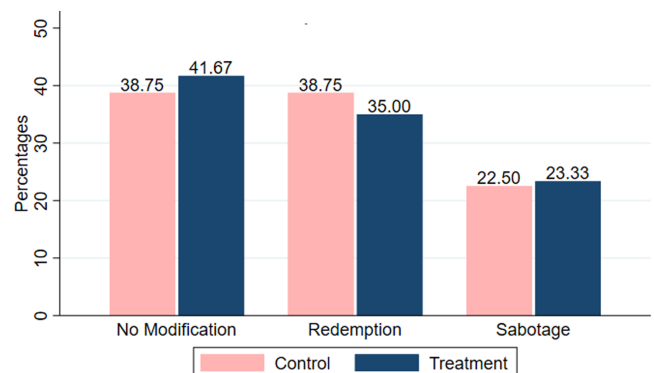
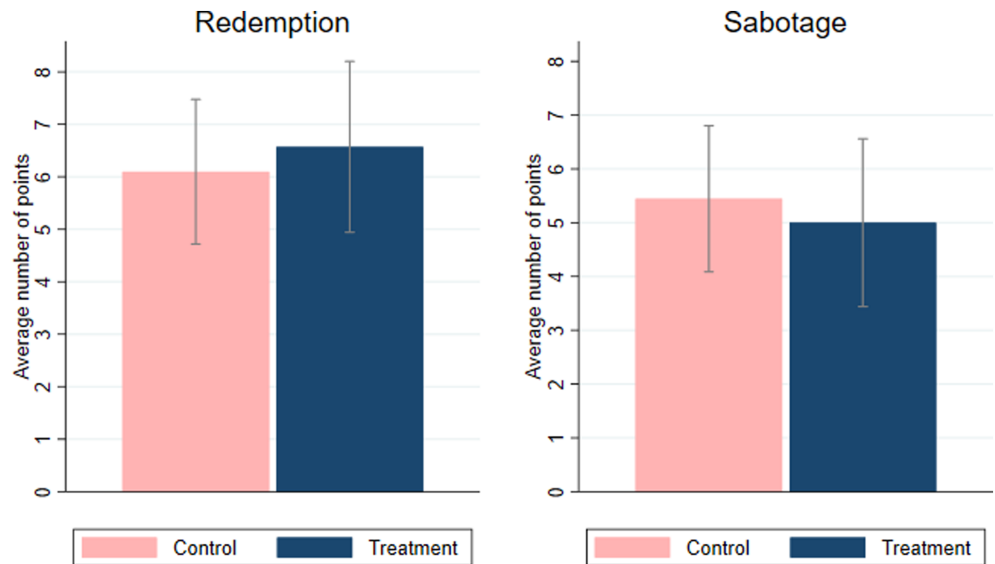


FIGURE 2 Modification choices (shares) [Colour figure can be viewed at wileyonlinelibrary.com]

FIGURE 3 Size of redemption and sabotage (intensive margin) [Colour figure can be viewed at wileyonlinelibrary.com]



the treatment group invested only slightly more with an average of 6.57 points ($p = 0.646$). This difference is even smaller in case of the sabotage option (on average 5.44 vs. 5.00 points, $p = 0.494$).

Next, we address potential heterogeneity concerns. To do so, we pooled both modification options for this analysis because the relative moral cost for the two modification types does not seem to have changed. Moreover, observation numbers within the subgroups become rather small if we refrain from pooling the two modification types. Table 1 summarizes the average modification size¹⁵ for different subgroups and differentiated according to control and treatment group. We observe a pattern that supports the suggestion that social preferences play a major role in the decision-making process with regard to unethical behavior (see, e.g., Grosch & Rau, 2020). First, based on the SVO questionnaire (Murphy et al., 2011), we classified subjects into prosocial and individualistic types. Subjects characterized by a higher tendency to act selfish (individualistic type) buy on average more modification points than more prosocial individuals ($p = 0.006$). Similarly, subjects splitted according to their negative reciprocal inclination, and the average number of modification points bought is higher for subjects with a stronger tendency to respond to unkind behavior ($p = 0.005$). Risk preferences ($p = 0.437$) and the fact

whether subjects passed the threshold in Stage 1 to earn the fixed wage ($p = 0.891$) are not related to subjects' cheating behavior. Remarkably, there is consistently no treatment effect across all subgroups.

4.2 | Manipulation checks

In order to rule out that this null effect is not specifically due to our experimental design, we have to check whether the group leader's behavior was perceived as unkind in first instance. Therefore, we have two pieces of evidence.

First, it is crucial that the two working tasks in Stage 1 were indeed perceived as being very distinct with regard to their degree of pleasantness. We therefore asked participants how entertaining both tasks have been on a scale from 1 to 7 (with 7 equal to *very entertaining*). Due to the fact that every subject experienced both tasks at least through the 2-min trial round, we feel confident to ask for the assessment of both tasks simultaneously. Subjects clearly assessed the rating task to be more entertaining than the counting task. For both the control (4.63 vs 2.13) and the treatment group (4.40 vs 2.30), this

TABLE 1 Average size of modification for different subgroups

		All	N	Control	Treatment	p-value
(1)	Prosocial type	2.78	86	2.74	2.83	0.527
	Individualistic type	4.74	54	5.00	4.42	0.922
(2)	Low negative reciprocal inclination	2.62	69	2.82	2.37	0.520
	High negative reciprocal inclination	4.42	71	4.32	4.57	0.839
(3)	Risk-loving	3.97	70	4.06	3.89	0.956
	Risk-averse	3.10	70	3.26	2.78	0.380
(4)	Threshold passed	3.31	74	3.28	3.35	0.752
	Threshold not passed	3.79	66	3.90	3.62	0.902

Note: The subgroups for risk preference and reciprocal inclination were determined through a median split.

difference is statistically highly significant ($p < 0.001$, Wilcoxon signed-rank test), whereas the assessment of the single task types does not differ across treatment and control group (rating task: $p = 0.425$; counting task: $p = 0.482$). Consequently, subjects should have preferred to work on the rating task. Hence, the prerequisite for triggering a feeling of unkindness through the group leader's reallocation decision is fulfilled.

Second, we directly asked subjects of the treatment group about their fairness¹⁶ perceptions for the task allocations in Stage 1. The two questions read: "How fair do you think was the computer's task allocation in stage 1?" and "How fair do you think was the final task allocation?"—both again on a scale from 1 to 7 (with 7 equal to *very fair*). Even though we cannot completely eliminate this concern, we placed the second question on a successive screen in order to reduce the experimenter demand effect. Taking the obtained results therefore with some care, subjects claim the final task allocation to be more unfair (2.85 vs. 4.25 with $p < 0.001$, Wilcoxon signed-rank test), which supports the previous finding on the difference in the perceived pleasantness of the two tasks.

The third piece of evidence is obtained from the modified PANAS questionnaire that was implemented between Stages 1 and 2 to evaluate subjects' emotional state of mind. On a scale from 1 (*not at all*) to 5 (*extremely*), subjects had to state to which extent the 23 items fitted to their current mood. Comparing outcomes between control and treatment group, we find a (weakly) significant difference for the negative trait *hostile* ($p = 0.065$).¹⁷ Notably, when analyzing this item for the subgroups we have been looking at before, we find that there is no difference for individualistic types and subjects with a low reciprocal inclination ($p = 0.537$ and $p = 0.530$, respectively). The two groups, however, that are most likely to be sensitive to the unkind reallocation of tasks, the prosocial and highly reciprocal individuals, show significantly higher values in case of being treated (1.64 vs. 1.30 with $p = 0.069$ and 1.93 vs. 1.56 with $p = 0.065$, respectively). These numbers may serve as further evidence that the task reallocation has been perceived as unkind.

4.3 | Regression analysis

Given that the group leader's reallocation decision did not trigger unethical behavior toward coworkers, we conducted a detailed regression analysis to control for potential differences between control and treatment group and to investigate other important determinants that might explain subjects' cheating behavior. We look both at subjects' binary decision to engage in modification activities using probit and the actual modification size using tobit models. As before, we continue to pool both modification options for the regression analysis and look at unethical behavior in general. Results are summarized in Table 2. With the exception of one specification only, the estimated treatment effect is slightly negative, but statistically far from being significant. Hence, using different models and an extensive set of controls, the regression analysis confirms our results obtained from the preceding nonparametric testing.

Looking at further possible determinants of subjects' cheating behavior, it obviously does not matter whether subjects passed the threshold in the counting task and earned the fixed wage. This suggests that even though subjects are competing for a monetary bonus, cheating behavior is more likely to be driven by nonmonetary aspects. One of these aspects is the belief about the opponent's behavior. As observable in Specifications (2)–(4), the more modification points a subject expects from her opponent (0–20), the more likely she is to engage in cheating, too. The same holds for the number of modification points bought, as shown in Specifications (6)–(8). Given that the point estimate is below 1, however, we find that subjects do not perfectly adjust their own behavior toward their beliefs about their opponent's behavior. As already indicated in the context of the subgroup analysis, we find that individuals with a higher negative reciprocal inclination are about 20 percentage points more likely to cheat. However, this effect almost disappears for the average number of modification points using the tobit model. We also tested for an interaction effect between being highly reciprocal and modification beliefs (detailed results are available upon request), but both groups behave nearly identical. Notably, it does not only matter whether subjects expected their opponents to cheat in general, but they also adjusted the type of modification to their expectations: 69.62% of all subjects chose the modification option they also expected from their opponent. One might argue that subjects tried to justify their own behavior by stating that they expected the same behavior from their opponent, but due to the incentivized belief elicitation, this should be true, if at all, only for a minority of subjects.

The remaining control variables have the expected signs, but they are less robust across the different specifications. Additionally, controlling for some basic sociodemographic characteristics and the Big Five personality traits in Specifications (4) and (8) does not alter our main results.

4.4 | Group leader behavior

Even though the group leaders' behavior is not of our main interest, their decision making was crucial for obtaining the observations of interest. Hence, we shortly also look at group leaders' behavior to finalize the data analysis. Out of 83 groups, 41 groups faced the computer-generated task allocation of Setting 1 (i.e., the group leader received the unfavorable counting task). Eleven leaders refrained from exchanging tasks, but only one assessed the counting task to be more entertaining than the rating task. Given that it was made clear that no further interactions with the other two group members would take place in Stage 2, this behavior suggests altruistic preferences—nine of these group leaders are also classified as prosocial individuals based on the SVO questionnaire. In allocation Setting 2, only two out of 42 individuals actively decided to undertake the unpleasant task; both individuals were also classified as prosocial individuals.

Within the final questionnaire, we additionally asked all group leaders, "Please guess, how many group leaders would decide to exchange their counting for the rating tasks/their rating for the

TABLE 2 Main regression results

	Probit, dep. Var.: Modification choice (0/1)				Tobit, dep. Var.: Modification size (0–20)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	–0.029 (0.042)	–0.030 (0.043)	–0.009 (0.047)	0.007 (0.047)	–0.317 (0.884)	–0.357 (0.744)	–0.332 (0.837)	–0.168 (0.829)
Threshold passed		0.042 (0.085)	0.024 (0.084)	0.035 (0.085)		0.269 (0.935)	–0.083 (0.955)	–0.012 (1.060)
Rating task more fun		0.024* (0.014)	0.022 (0.014)	0.025** (0.013)		0.255 (0.218)	0.182 (0.219)	0.259 (0.204)
Mod. Belief (size)		0.024*** (0.008)	0.018** (0.008)	0.019** (0.009)		0.647*** (0.166)	0.581*** (0.161)	0.628*** (0.142)
Highly neg. Reciprocal			0.184** (0.086)	0.225* (0.101)			2.077 (1.449)	2.229* (1.318)
Individualistic type			0.111 (0.093)	0.072 (0.089)			1.750* (1.054)	1.082 (1.185)
Risk-loving			–0.097 (0.082)	–0.124* (0.069)			–0.328 (1.228)	–0.798 (1.233)
Constant					1.808*** (0.653)	–2.367* (1.268)	–3.261** (1.325)	–1.305 (4.794)
Observations	140	140	140	140	140	140	140	140
Pseudo-R ²	0.001	0.059	0.106	0.159	0.0001	0.052	0.063	0.085
Additional controls	NO	NO	NO	YES	NO	NO	NO	YES

Note: Additional controls include sociodemographic characteristics (gender, age, field of study, number of friends in current session) and the Big Five personality traits. For the probit specifications, the table reports average marginal effects. Standard errors clustered on the session level in parentheses.

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

counting task.” Even though the belief elicitation was not incentivized, the observed group leader behavior corresponds quite well to the predicted one. Leaders in Setting 1 predicted a 75.27% exchange probability (vs. 73.17% actual exchange rate), and a share of 17.74% (vs. 4.76%) was predicted in Setting 2. More importantly, we wanted to examine whether leaders have been aware of the impact that exchanging the tasks might have. Therefore, we asked, “Please put yourself into the position of your group member. Do you think your intervention in the task allocation was perceived as being fair or unfair?”—on a scale ranging from 1 (*very unfair*) to 7 (*very fair*). We find that group leaders who exchanged the unpleasant for the pleasant tasks stated an average of 2.67. This is very close to the perceived level of unfairness stated by the treated subjects (i.e., 2.85), and hence, leaders seemed to be aware of their behavioral impact and still intentionally engaged in unkind behavior for their own benefit.

Taken together, the group leaders' behavior and questionnaire responses support the evidence obtained from the regular group members that subjects indeed perceived the rating task to be more favorable and that the exchange of tasks in Setting 1 was an unkind action, which was beneficial for the group leader but adverse for the group members. Hence, the question arises whether generalized negative reciprocity simply does not play any role in the underlying context or whether our result is driven by the fact that both competing individuals were victims of the same unkind treatment. Hence, we invited a new group of university students to participate in an online survey experiment, which will be described in the next section.

5 | INCENTIVIZED ONLINE SURVEY EXPERIMENT

5.1 | Experimental design

In total, 206 survey participants were allocated to one out of four workplace scenarios, which are closely related to our lab experiment. We used hroot (Bock et al., 2014) for recruitment, and the survey was executed with SoSci Survey (Leiner, 2019). All scenarios build upon two fictional employees in a fast food restaurant who compete for a monthly bonus. The winner is determined by the number of satisfied customers. Given that both employees work their shifts separately and without any supervision, they could cheat at the cost of their absent coworker by leaving a positive evaluation for each customer who did not rate her service experience. The two *colleague* scenarios replicate the lab experiment, framing a situation in which both employees previously suffered (or not) from an unkind supervisor who has just left the company. Additionally, we conducted two *unknown coworker* scenarios in which we varied the competition in that the competitor for the bonus is not the former colleague but an unknown coworker who just joined the restaurant because the former colleague has also left.¹⁸ Given our primary interest in the *unknown coworker* setting, we oversampled these two scenarios so that we end up with 81 observations for the *colleague* (in treatment, $N = 42$, and in control,

$N = 39$) and with 125 observations for the *unknown coworker* (in treatment, $N = 64$, and in control, $N = 61$) scenarios.

After reading their randomly allocated scenario, survey respondents should judge the moral appropriateness of cheating. In order to prevent all participants from stating that cheating is morally completely inappropriate because they personally would never behave in such a way, we closely follow the experimental design proposed by Krupka and Weber (2013) and asked them about their beliefs on how the other participants judge the moral appropriateness of cheating. Given that the intensity of cheating might play a crucial role, participants had to judge different options the fictional employee had, such as no cheating at all, submitting a positive rating for every tenth, every fifth, every third, every other, or for every customer. For each possible option, the employee's behavior can be perceived as being *very morally inappropriate*, *somewhat morally inappropriate*, *somewhat morally appropriate*, and *very morally appropriate*. To incentivize participants to reveal their true beliefs, they received a payoff of 5 EUR in case of choosing the mode value for one randomly picked option and no payoff otherwise.

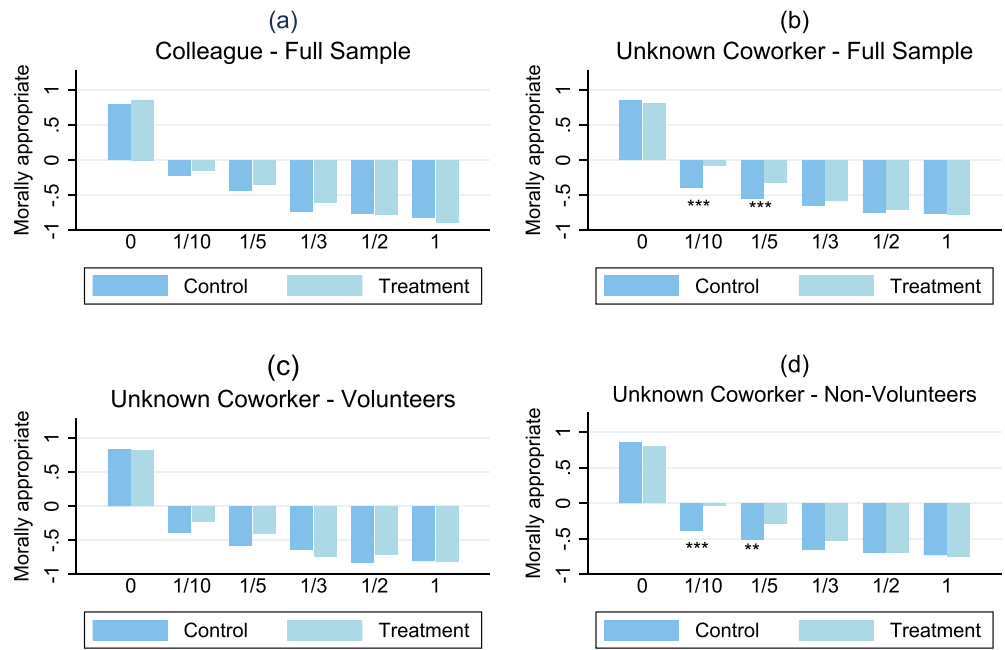
At the end of the survey, we collected sociodemographic information on age, gender, and the course of study. Additionally, we included a question regarding the participants' personal engagement in any kind of voluntary work, which will serve as a proxy for highly prosocial individuals (summary statistics can be found in Table A.4). To answer the survey completely, it took on average about 5 min.

5.2 | Results

In general, our results (see Figure 4) show that the decision to refrain from any cheating is always assessed to be highly moral (high positive moral score), whereas the opposite is true for all five cheating options (negative moral score). In line with our findings from the lab experiment, Figure 4a indicates no difference between the treatment and control scenario across any cheating intensity within the *colleague* setting. Hence, experienced unkindness does not seem to justify cheating at the cost of a colleague who suffered from the same experience.

Figure 4b shows the results for the *unknown coworker* setting. Whereas there is again no difference for high levels of cheating, the experienced unkindness seems to justify at least minor forms of cheating against an unknown coworker. In case of submitting a positive rating for every tenth possible customer, the average scores of -0.39 in the control and -0.08 in the treatment scenario already express a clear difference ($p = 0.002$). That difference becomes even more obvious when comparing shares of participants who judged this kind of cheating to be morally appropriate because this was true for only 18.03% of participants in the control scenario and this number more than doubles (40.63%) in the treatment scenario. For the option of submitting a positive rating for every fifth customer, the difference is still highly significant ($p = 0.008$) but less pronounced, the shares of participants who judge that behavior to be morally appropriate goes down to 11.48% in the control and to 18.75% in the treatment scenario.

FIGURE 4 Mean moral appropriateness rating for cheating [Colour figure can be viewed at wileyonlinelibrary.com]



Note: Participants' answers were converted into numerical scores with "very morally inappropriate" corresponding to -1, "somewhat morally inappropriate" to -1/3, "somewhat morally appropriate" to +1/3 and "very morally appropriate" to +1. Significance levels are denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Given that moral perceptions might differ between individualistic and prosocial individuals, we split the sample into two subgroups (see Figure 4c,d). For highly prosocial survey respondents, as identified by regular volunteering activities, we do not find any treatment effect. For non-volunteers, however, the pattern remains nearly unchanged compared with the pooled sample, with significant differences between treatment and control for the two lowest cheating intensities. This finding is also in line with the suggestion that individuals prone to a specific type of deviant workplace behavior, such as organizational deviance, are likely to be different from individuals who are prone to interpersonal deviance (Bennet & Robinson, 2000).

6 | CONCLUSION

By conducting a laboratory and an online survey experiment, we investigated whether individuals are (more) likely to engage in counterproductive work behavior toward coworkers after experiencing unkind supervisor treatment if the supervisor is no feasible target for retaliation. Our laboratory experiment reveals that workers who suffered from interactional managerial injustice do not show a higher probability in cheating activities at the cost of their coworkers. In our survey experiment, we implemented a setting comparable with our lab experiment to investigate the perceived moral appropriateness of cheating. In the case of having a joint experience of unkind group leader behavior, there is no difference between control and treatment group. Given previous evidence showing that shared (traumatic) experiences can bond and shift preferences between involved parties quite intensively (see, e.g., Whitehouse et al., 2017; Jong et al., 2015), we

additionally analyze whether the common experience of victimization might drive individuals' inclination for unethical behavior against others. When introducing a previously unknown and unencumbered coworker into the hypothetical workplace scenarios, we indeed find that cheating is perceived to be less morally inappropriate after experiencing unkind supervisor behavior.

The latter finding is in line with Chang et al. (2019) who show that norm perceptions are sensitive across differently framed situations and may depend on individuals' social identity. Evidence stressing the importance of personal relationships among workers has been derived by Yang et al. (2013) who show that sharing a strong collective identity (e.g., within work groups) can moderate the positive relationship between experienced unfairness and counterproductive behavior against others. In regard to trickle effects of unethical behavior within organizations, Mawritz et al. (2012) support this narrative. Their results indicate that a sound and functioning work atmosphere among coworkers can buffer or even reverse the tendency to pass on frustration in the aftermath of unkind supervision. The data derived from our survey experiment clearly confirm this suggestion and further enrich our understanding of the complex relationship between other-regarding preferences and the emergence of unethical behavior in the workplace. In particular, our experimental approach allows us to contribute to the literature by offering causal insights on how personal ties among workforce members can moderate the tendency to displace frustrations against innocent others while controlling for potential confounding factors such as indirect retaliation motives. In regard to the literature on generalized negative reciprocity, our results support the suggestion by Della Valle and Ploner (2017) who find that unethical choices in the aftermath of own unfair treatment seem to

aim at restoring one's own subjective fairness balance rather than the explicit wish to hurt innocent others.

One might argue that the null result in the laboratory experiment might be explained by the fact that behavior in contests is simply driven by the intrinsic wish to win the contest prize (Benistant & Villeval, 2019). Nevertheless, findings presented by Flory et al. (2016) and Harbring and Irlenbusch (2005) indicate that people take social preferences into account when they decide to cheat in a tournament. Even though the survey experiment is not directly comparable with the lab results, the difference between the colleague and unknown coworker settings supports this suggestion. Given that many workplace structures rely on tournament environments to increase work effort (Bull et al., 1987; Harbring et al., 2007; Lazear & Rosen, 1981) and that competitive markets are known to be an ideal breeding ground for unethical behavior (Cartwright & Menezes, 2014; Charness et al., 2014; Dato & Nieken, 2019, 2014; Harbring & Irlenbusch, 2011), the investigation of this specific setting seems to be relevant. Nevertheless, our study definitely raises a number of questions for further research.

Besides calling for further studies that investigate nontournament settings, it is important to point out that our analysis examines interactional injustice in a single one-shot scenario. Whereas individuals might be able to deal quite well with managerial unkindness, for example, in case of having only a bad day, more severe or repeated mistreatment may increase one's personal frustration level and trigger an outburst of accumulated anger against third parties, following the notion of displaced aggression. Due to ethical concerns, however, it is unreasonable to push participants over their emotional edge, and, hence, the experimental approach is not suited for investigating more severe forms of managerial unkindness. Blind and uncontrolled outbursts of accumulated frustrations, however, are probably also less likely than deliberately engaging in less obvious and more subtle forms of counterproductive behavior. In that regard, our survey experiment presents empirical evidence showing that experienced injustice on the job can indeed open up some moral wiggle room in which affected workers can justify moderate levels of cheating—which in turn could negatively affect perceived group identity and, hence, increase the danger of more serious forms of counterproductive work behaviors.

It is important to note that in contrast to our laboratory experiment, we do not observe actual behavior in the survey study. However, Krupka and Weber (2013) and Chang et al. (2019) show that differences in social norm assessments, elicited through the incentivized approach used here, serve as a powerful predictor for actual decision making. Nevertheless, one might question whether subjects undergo the same emotional distress from reading a hypothetical workplace scenario than from actually experiencing it. Hence, it is an undisputed fact that more research on this topic is needed. Especially quasi-experimental environments could solve ethical issues while allowing scholars to derive deeper causal insights on the impact of perceived interactional unfairness on counterproductive work behavior toward coworkers.

Taken together, our study shows that even in a competitive workplace setting in which immoral behavior is incentivized, people seem

to refrain from immediately lashing out against other group members. The moral appropriateness of deviant workplace behavior, however, seems to be dependent on the situational context. The risk of a contagious spread of unethical behavior might be especially prevalent in workplaces characterized by loose and anonymous employee structures such as warehouses with high employee turnover (Min, 2007). Besides pointing to the relevance of interactional justice, our paper delivers an additional argument in favor of creating a functional working atmosphere beyond the traditional narrative of pushing labor productivity. Especially in contrast to costly monitoring measures that are known to bear the risk of triggering adverse employee reactions (Falk & Kosfeld, 2006), investments enhancing the personal relationship among employees (such as office remodeling or joint employee activities) appear to be a powerful instrument to contain or at least buffer the spread of unkind behavior in the aftermath of (perceived) interactional injustice.

Undoubtedly, the displacement of personal frustrations and anger can also be observed outside the workplace. Card and Dahl (2011), for example, find a strong increase in domestic violence in areas in which the local football team unexpectedly lost. If displaced aggression is even more prevalent among individuals that are less closely connected, such trickle effects may help to explain broader societal upheavals such as the rise of xenophobia. Individuals may have a tendency to displace their own experienced frustrations on to minority groups and refrain from displaying the same degree of acrimony against otherwise similar fellow countrymen. Of course, the question of whether trickle effects indeed contribute to the rise of xenophobia is beyond the scope of this paper. However, we believe that our findings call for more extensive efforts to bring (apparently) dissimilar or unknown people closer together—whether on the job or in daily life.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the corresponding author upon request and will be uploaded to a data repository after the research is published.

ENDNOTES

- ¹ Krebs (1982) also refers to this pattern as “displaced reciprocity.”
- ² We are grateful for the provision of the z-Tree codes used in Gill and Prowse (2019); Benndorf et al. (2019); Murphy et al. (2011); and Abeler et al. (2011), which were publicly available.
- ³ Screenshots and instructions (translated from German) are provided in the supporting information Appendix I.
- ⁴ To exclude deception concerns, we avoided the term randomization and told participants only that the computer automatically determined the distribution of tasks.
- ⁵ We added the affects angry, happy, and irritated to the original questionnaire because these emotions are likely to be affected by perceived unkind behavior.
- ⁶ For each word, a two-digit number was assigned to a respective letter of the alphabet, displayed in an encryption table at the bottom of the

screen. Each word and each specific encryption table was randomly created by the computer program (Benndorf et al., 2019). The real effort task was chosen as it requires no specific skill set.

⁷ We used a neutral wording for both sabotage and redemption in order to avoid any behavioral adjustments. See supporting information Appendix II for exact wording.

⁸ This step was omitted if they believed that there was no modification at all, and the second belief elicitation was not mentioned while selecting the first one in order to avoid hedging.

⁹ As in Gächter and Renner (2010), we allowed for an error tolerance of $+/-1$ point.

¹⁰ Table A.1 provides summary statistics for subjects' sociodemographic characteristics and personality traits. We observe a statistically significant difference as regards subjects' gender between control and treatment group. This difference, however, cannot explain the null result because we neither observe a treatment effect for females nor for males (both $p > 0.8$).

¹¹ To compare: 68.18% of the subjects who were allocated the rating task ($N = 22$) and allowed to keep this task decided to modify their tournament outcome, whereas all subjects who were allocated the counting task and received the rating task from the group leader ($N = 4$) refrained from buying modification points.

¹² If not stated otherwise, the p -values are obtained from a two-sided Wilcoxon rank-sum test.

¹³ To assess the significance of this null result, we calculated the statistical power given our sample size for three different hypothetical effect sizes (equal to a Cohen's d of 0.2, 0.5, and 0.8). We used the software G*Power (Faul et al., 2009) and set the p -value equal to 5%. Considering a low, a medium, and a large treatment effect, the statistical power would be equal to 20%, 81%, and 99%, respectively.

¹⁴ A two-sided binomial test reveals a significant difference between sabotage choices (39.39%) and the 50% threshold ($p = 0.038$), indicating nonrandom modification choices.

¹⁵ Results separated into extensive and intensive margin can be found in Table A.2.

¹⁶ Due to the fact that there is no fitting translation for "unkindness" in German, we asked for participant's assessment of perceived unfairness in the questionnaire. We claim that especially from a victim's perspective, unkindness, and unfairness can be seen as sufficiently close.

¹⁷ For the full list of the PANAS trait comparisons, please see Table A.3.

¹⁸ Please note that we never used the term "colleague" or "unknown coworker" to avoid framing effects. For all details, see supporting information Appendix III.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of this article.

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APPENDIX A.

TABLE A.1 Subjects' sociodemographics and personality traits

	Treatment	Control	p-value
Age	23.55	23.55	0.529
Female	0.433	0.700	0.024
First degree	0.467	0.400	0.432
Friends	0.483	0.613	0.594
Risk	4.900	4.088	0.037
Individualistic type	0.400	0.375	0.764
Reciprocal inclination (neg.)	2.994	2.950	0.893
Reciprocal inclination (pos.)	5.944	5.867	0.757
Openness	4.850	5.013	0.265
Conscientiousness	4.911	4.854	0.852
Extraversion	4.989	4.700	0.221
Agreeableness	5.322	5.329	0.790
Neuroticism	4.022	4.238	0.253
N	60	80	

Note: p-Values are obtained from Pearson's χ^2 or two-sided Wilcoxon rank-sum tests, respectively.

TABLE A.2 Modification choices within subgroups (extensive and intensive margin)

	Extensive margin				Intensive margin			
	Control	Treat	p	N	Control	Treat	p	N
(1) Prosocial type	0.56	0.47	0.421	86	4.89	6.00	0.791	45
	Individualistic type	0.70	0.75	0.684	54	7.14	5.89	0.488
(2) Low neg. Reciprocal inclination	0.51	0.43	0.512	69	5.50	5.46	0.924	33
	High neg. Reciprocal inclination	0.71	0.73	0.810	71	6.10	6.23	0.984
(3) Risk-loving	0.58	0.59	0.873	70	7.05	6.55	0.850	41
	Risk-averse	0.64	0.56	0.555	70	5.10	4.92	0.442
(4) Threshold passed	0.63	0.56	0.563	74	5.24	6.00	0.677	44
	Threshold not passed	0.60	0.62	0.901	66	6.50	5.88	0.626

Note: p-Values are obtained from Pearson's χ^2 or two-sided Wilcoxon rank-sum test, respectively.

TABLE A.3 PANAS results

	Control	Treatment	p-value
Proud	2.48	2.72	0.224
Happy	2.54	2.58	0.729
Irritable	2.45	2.40	0.705
Enthusiastic	1.78	1.95	0.405
Ashamed	1.30	1.32	0.731
Angry	2.10	2.08	0.805
Alert	2.93	2.97	0.788
Nervous	1.61	1.68	0.733
Determined	2.85	2.92	0.788
Attentive	3.31	3.32	0.979
Jittery	1.73	1.63	0.789
Afraid	1.14	1.15	0.933
Distressed	1.59	1.45	0.458
Interested	2.58	2.55	0.877
Irritated	1.85	1.87	0.693
Excited	1.84	1.90	0.927
Strong	2.21	2.23	0.846
Envious	1.74	1.77	0.735
Guilty	1.19	1.10	0.236
Scared	1.26	1.25	0.724
Hostile	1.40	1.57	0.065
Inspired	2.09	2.07	0.935

Note: p-Values are obtained from two-sided Wilcoxon rank-sum tests.

TABLE A.4 Survey respondents' characteristics by treatment

	Treatment	Control	p-value
<i>Colleague setting</i>			
Age	24.81	24.64	0.970
Male	0.524	0.436	0.429
Prosocial type	0.405	0.513	0.329
Reciprocal inclination (neg.)	2.690	2.906	0.304
Reciprocal inclination (pos.)	5.984	5.966	0.909
N	42	39	
<i>Unknown coworker setting</i>			
Age	24.76	23.89	0.436
Male	0.540	0.492	0.594
Prosocial type	0.281	0.393	0.184
Reciprocal inclination (neg.)	2.786	2.644	0.619
Reciprocal inclination (pos.)	6.104	6.060	0.580
N	64	61	

Note: p-Values are obtained from Pearson's χ^2 or two-sided Wilcoxon rank-sum tests, respectively.