Measuring the Indirect Effects of Adverse Employer Behavior on Worker Productivity – A Field Experiment

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Abstract

We conduct a field experiment to study how worker productivity is affected if employers act adversely towards their co-workers. Our employees work for two shifts in a call-center. In our main treatment, we lay off some workers before the second shift. Compared to two control treatments, we find that the layoff reduces the productivity of unaffected workers by 12%. We find suggestive evidence that this result is not driven by altered beliefs about the job or the management's competence, but caused by the workers' perception of unfair employer behavior. The latter interpretation is confirmed in a prediction experiment with professional HR managers. Our results suggest that the price for adverse employer behavior goes well beyond the potential tit-for-tat of directly affected workers.

JEL codes: Gift Exchange, Labor Markets, Productivity, Layoffs, Field Experiment
Keywords: C93, J50, J63

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1. Introduction

Management matters for the success and profitability of companies. Recent work has shown, for instance, that simple management practices, such as regular maintenance of machines, optimization of inventory, or recording types of quality problems, can improve the productivity of companies substantially (Bloom et al., 2013). While many such management practices relate to the structure of an organization, its workflow and the controlling thereof, the relationship between managers and workers is also of great importance. This relationship is not only characterized by the wage that is paid to workers, but also by the workers' perception of being treated fairly.

In fact, Akerlof's (1982) theory of gift exchange between employers and workers not only refers to a generous wage as an employer's gift to workers, but also to fair employer behavior towards workers as a group. Akerlof illustrates this second type of gift exchange in his "cash poster" example. A group of cash posters receive a fixed wage and do not have any career concerns. While some of them produce up to 46% more than requested, others produce just the required minimum output. The employer could potentially profit by raising the minimum output or by replacing less productive workers. However, Akerlof hypothesizes that such behavior would compromise the gift exchange relationship. Workers care for each other's well-being. If the employer harms a subset of workers, the others would punish the employer by reducing their performance, even if they are unaffected by the employer's actions.

In this paper, we test Akerlof's hypothesis on indirect gift exchange in a field experiment. Previous empirical work on gift exchange mainly examined the fair wage hypothesis by analyzing the effects of wage cuts or hikes on the productivity of directly affected workers. In contrast, we focus on the productivity of workers who observe that their colleagues are treated adversely by the employer, but who themselves are unaffected by the employer's behavior. Throughout the paper, we mean by "unaffected" that a worker's pay, working conditions, and prospects in the organization remain unchanged.

The distinction between the fair wage hypothesis and the indirect type of gift exchange is important for organizations. Consider, for example, an organization that wishes to reduce labor costs. In his seminal work, Bewley (1999) finds that firms prefer the layoff of some workers to a wage cut for all workers. He argues that wage cuts hurt the morale (and hence productivity) of workers who remain within the organization, in line with the fair wage hypothesis; in contrast, layoffs may only reduce the morale of workers who leave the organization. However, the indirect type of gift exchange implies that this may not be true; the

remaining workers may also penalize the employer after a layoff that is seen as unfair. With our experiment, we test whether adverse employer behavior against a subgroup of workers – as in the case of layoffs – is costly for the employer due to the unaffected workers' hostile reaction.

For research, the challenge is to confront workers with an adverse act by the employer towards co-workers, while keeping their own prospects in the organization constant. This is difficult to realize in a long-established organization, since such acts may change the workers' career expectations and thus effort incentives. For example, workers may fear that similar acts may take place in the future, so that switching jobs becomes an attractive option, which in turn may reduce their motivation to exert effort.

To avoid such confounds, we created our own short-lived organization with 195 employees. They worked for us in a call-center to perform a telephone survey in two separate shifts. Overall, our organization was very employee-friendly by paying a generous wage, offering flexible work times, a pleasant work atmosphere, and full discretion over how to perform the job. In one treatment, we implemented an adverse act that stands in stark contrast to this image, i.e., the random layoff of 20% of the workforce to cut costs. This can be executed while keeping the remaining workers' prospects (in the only remaining shift) constant. To rule out any career concerns, we made explicit, in several instances, that there would be no future employment possibilities in the organization. Moreover, we paid the wage upon arrival at each shift. Thus, workers knew at the beginning of the second shift that the layoff of their co-workers could not have any consequences for them.

We have three different treatments. In the "No-layoff" treatment, the staff remained unchanged for the second shift. In a second control treatment – called "Quasi-layoff" – we reduced our staff by 20% between shifts, but did not communicate the layoff to the remaining workers (we only informed them that fewer staff were present during the second shift). In the third treatment – called "Layoff" – we randomly dismissed 20% of the workers after the first shift and communicated the random layoff to the remaining workers as a measure to cut costs. We measure productivity by the number of calls that workers complete in each shift (other outcome variables are considered in robustness checks).

Our results show a clear effect of layoffs on the productivity of the remaining workers. Compared to the control treatments, productivity in our Layoff treatment drops by 12%. This effect is mainly due to reduced work effort at the beginning and at the end of the shift. We also find some evidence that the quality of the workers' output is reduced in the Layoff treatment. Importantly, there is no significant difference in performance between the two

control treatments. Thus, the reduction in staff size *per se* has no detectable effects on productivity.

To study which behavioral motives provoked this reaction to the layoffs, we employ follow-up and debriefing surveys. We find that workers were highly satisfied with most aspects of their job in the call-center. However, we also find evidence that, in the Layoff treatment, they were less satisfied with the "management's behavior towards their co-workers." This provides suggestive evidence that workers perceived the employer's behavior as unfair. We also can rule out a number of alternative explanations that are unrelated to the fairness aspect of the layoffs, such as peer-effects or altered beliefs about the importance of the job or the management's competence.

In order to gain further insight into what drove the productivity change, we recruited 43 professional HR managers (from medium-sized and large companies in Germany) to participate in an online experiment. In this experiment, we find evidence that the announcement of random layoffs was considered as unfair. We also asked the HR managers to predict the workers' productivity response in our field experiment. It turns out that they largely anticipated the detrimental effects of adverse behavior on the productivity of unaffected workers. Their predictions about the drop in performance in the Layoff treatment were remarkably accurate in the aggregate.[1]

We are well aware that in our experiment we can capture only short-run effects. The short-term nature of our organization is an essential property of the field experiment to keep the workers' expectations constant. The downside of this design feature is that our results do not allow us to study the long-term effects of adverse employer behavior. On the one hand, gift exchange interventions often wear off quickly (Gneezy and List 2006). On the other hand, there are several reasons why we could expect long-term effects of adverse employer behavior. First, we know from several studies on the effects of wage cuts that these can be of a long-term nature. Two recent papers, Coviello et al. (2018) and Krüger and Friebel (2018), find that changes in compensation can have long-lasting effects on the productivity of directly affected workers. Second, we think that adverse employer behavior in general is not only reflected in one-off events such as a layoff. Events of this kind could also include situations and conflicts that occur frequently, such as bullying, harassment, discrimination, or a lack of procedural and distributive fairness (Pfeffer 2007). In this case, the employees' reactions to adverse behavior may constantly be replenished so that they are of long-term nature. Finally,

[1] This result is very much in line with DellaVigna and Pope (2018a, 2018b), who find that, in the aggregate, experts can predict the effects of monetary and non-monetary incentives on workplace performance quite precisely.

we observe that many firms take significant measures that may alleviate the potentially negative effects of layoffs on morale. For example, they pay workers to leave the firm voluntarily, or they shift the blame by making a previous CEO responsible for the layoff.[2]

The rest of the paper is organized as follows. In the next section, we discuss how our paper contributes to the previous literature. In Section 3, we explain the setup of the field experiment, the follow-up survey among participants, and the debriefing. Section 4 presents the results. In Section 5, we examine different explanations for our results, using data from the follow-up and debriefing surveys, as well as from a prediction experiment with professional HR managers. Section 6 concludes. An extensive Online Appendix contains additional materials and robustness checks.

2. Related Literature

In this section, we first describe how our paper contributes to the literature on indirect reciprocity as well as to the survivor syndrome literature in management and psychology. Then we compare our results to those in previous field-studies on gift exchange and wage cuts.

2.1 Indirect Reciprocity

Indirect reciprocity implies that friendly or hostile acts of one party towards another are rewarded or punished by a third party. Nowak and Sigmund (1998) showed that, in an evolutionary interaction model, indirect reciprocity can emerge as the prevailing population behavior. In an organizational context, there are several behavioral motivations for indirect reciprocity. By harming a subset of workers, the employer may violate established work norms and openly alienate himself from the whole group of workers, so that they see him as an outsider and act accordingly (Akerlof and Kranton, 2000, 2005). Alternatively, the employer may reveal through adverse acts that his "type" does not prioritize worker welfare, so that workers are no longer willing to treat him generously (Ellingsen and Johannesson, 2008).

The empirical evidence on indirect reciprocal behavior primarily stems from laboratory experiments. Several papers examine behavior in the "helping game." In this game, a donor can help a receiver; helping is costly for the donor, but increases the receiver's

[2] Bartling and Fischbacher (2012) present experimental evidence from the laboratory that shifting the blame works and reduces the extent of negative reciprocity. Fuss (2009) shows that firms often reduce their wage costs by relying on early retirement or on the natural fluctuation of the workforce, or by adjusting the amount of overtime hours or temporary workers.

payoff, with the benefits exceeding the costs. When information about the receiver's past helping behavior is available, the chance of helping increases to the extent that the receiver has helped others in the past; see, e.g., Seinen and Schram (2006), Engelmann and Fischbacher (2009), and Ule et al. (2009). This holds even when donors do not have any strategic concerns, such as reputation-building (Engelmann and Fischbacher, 2009).

In a similar vein, a number of laboratory studies analyze the extent to which third (unaffected) parties are willing to enforce norms by punishing subjects whose actions violate these norms. The seminal paper by Fehr and Fischbacher (2004) studies behavior in a dictator game in which an unaffected party observes the dictator's decision and then can reduce, at its own costs, the dictator's payoff. Indeed, dictators who violate the 50-50 sharing norm get punished in 60% of the cases. Henrich et al. (2010) conduct experimental games with third-party punishment in 15 indigenous populations and find that the willingness to punish norm-violating behavior increases in community size. Jordan et al. (2016) demonstrate that third-party punishment is associated with feelings of anger about a player's behavior. Drzensky and Heinz (2016) study a setting in which the principal can benefit from laying off one agent (out of three). They find a large drop in the remaining agents' performance as a response to such a layoff.

The results from our field experiment are very much in line with those from the experimental literature. Our workers respond to the employer's adverse behavior towards their colleagues by reducing their performance, even though they are unaffected by his behavior. The advantage relative to the lab experiments is that we can observe indirect reciprocal behavior in an organizational context, where there is no guide for subjects to a specific behavioral response. Thus, indirect reciprocal behavior may constrain managerial decision-making and facilitate compliance to work norms, which may ultimately contribute to the organization's success. Moreover, the design of our study allows us to disentangle various alternative explanations for the workers' behavior in our setting. Thus, social concerns can dominate alternative behavioral motivations when the employer acts adversely towards a subgroup of workers.

So far, there have been only few field experiments on indirect reciprocity. Balafoutas and Nikiforakis (2012) and Balafoutas et al. (2016) examine norm enforcement in the field by studying how third parties react to norm violations of others (who pollute a public space or stand in the way of passengers on escalators). However, they find very little norm enforcement.

2.2 Survivor Syndrome

A large psychological literature studies the consequences of layoffs on those who remain employed at the company. In many instances, a drop in work morale and motivation is observed. This phenomenon is called "survivor syndrome"; see Datta et al. (2010), and Van Dierendonck and Jacobs (2012) for overviews. Surveys with workers who experienced layoffs in their organizations reveal self-reported lower work performance (Travaglione and Cross, 2006), lower commitment (Allen et al., 2001), increased willingness to switch jobs (Allen et al., 2001), and increased absenteeism (Travaglione and Cross, 2006). The advantage of our field experiment relative to these studies is twofold. First, our controlled experimental setting ensures that no factor besides the layoff message influences workplace performance. In contrast, in an ongoing organization that conducts layoffs, other human-resource policies may change as well (such as the workload, or the assignment of workers to units). The workers' reactions in the survey studies above are therefore not necessarily caused by indirect reciprocity. Second, we obtain objective performance data through the experiment, so that we can examine the behavioral consequences of the layoff for the employer.

2.3 Gift Exchange in the Field

A number of field experiments have examined Akerlof's fair wage hypothesis by raising or cutting fixed wages. Gneezy and List (2006) show that workplace performance increases after a surprise wage raise. However, this effect is not permanent and wears off after three hours of working. Kube et al. (2012) show that non-monetary aspects matter for the extent to which a gift is recognized as such. Two field experiments by Kube et al. (2013) and Cohn et al. (2014) examine the productivity effects of wage cuts. Both find strong negative effects of wage cuts, with an elasticity (percent change in output/percent change in total wage bill) of 0.60. Interestingly, we find a similar elasticity when we focus on the percent change in output of the remaining workers. Thus, our effect size with unaffected workers is comparable to what one finds when workers are directly affected by a wage cut.

The study by Cohn et al. (2014) is closely related to ours in that they also consider a setting in which there is scope for indirect reciprocal behavior. In their field experiment, workers perform a job in teams of two. In one treatment, they cut the fixed wage of one worker and leave the other worker's wage unchanged. However, while the workplace performance of the affected worker drops significantly, the unaffected worker does not change his or her performance in response to the co-worker's wage cut.[3] Thus, no indirect

[3] Relatedly, Breza et al. (2018) examine productivity effects of pay inequality. They observe that pay

reciprocal behavior is observed in their setting. Unfortunately, Cohn et al. (2014) do not have data on how the wage cut was perceived by the unaffected workers. In our field experiment, unaffected workers in the Layoff treatment reduce their productivity significantly. Moreover, we find suggestive evidence that they judge the layoff of their colleagues as unfair.

Finally, two recent papers use company data to analyze employees' reactions to wage cuts and find evidence for the significance of social preferences in the employer-employee relationship. Krueger and Friebel (2018) observe that an unexpected reduction in the slope of the bonus function at a consulting firm reduces output by 30%. Importantly, affected workers who experience this reduction maintain the output reduction until they drop out of the organization. Coviello et al. (2018) demonstrate in a sales call-center setting that a share of workers react to the wage reduction by "exerting voice", i.e., by displaying behavior that hurts both the organization and themselves. Again, our setting is different in that we analyze the reactions of workers who are not directly affected by the employer's adverse behavior.

3. Experimental setup

3.1 Field setting in the call-center

We used the following opportunity for our field experiment. A German university intended to conduct a nationwide survey on refugee integration and engagement in volunteering. The survey consisted of a number of short questions (a translation can be found in the Online Appendix). We essentially took over the management of this survey and established a private organization (the "Telefonstudio Saar") that would conduct the survey on behalf of the university. We rented out offices in a call-center for eight weeks and recruited workers in regional online and offline job markets. The job advertisement stated explicitly that the job consisted of conducting a telephone survey for a payment of 2 x 40 Euros, and that there would be no future employment opportunities in our organization (the Online Appendix contains the job posting). Applicants had to have an e-mail address and a mobile phone number. Moreover, they were required to speak German fluently. They applied online by uploading their résumé or by completing a short form. While applying, they indicated possible working times and agreed to the storage and processing of the generated data. We hired all

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inequality reduces output by a considerable amount when productivity is difficult to observe. While relatively higher-paid workers were not directly affected by the lower pay of their co-workers, Breza et al. (2018, p. 658) note that "if lower-paid workers were discontent and resentful, then working and eating lunch alongside them may have been socially awkward or unpleasant for their relatively higher-paid peers, dampening their desire to go to work." Thus, their situation is substantially different from ours, as the higher-paid workers kept contact with the disadvantaged, lower-paid workers.

applicants who indicated a sufficiently large range of potential working times and who met our requirements.[4]

The job comprised two work shifts of 3.5 hours each. The second shift was usually scheduled one week after the first. The fixed wage was 40 Euros per shift. This wage was around one third above the minimum wage in Germany. As wages for unskilled workers in German call-centers are usually rather low[5], we consider our wage as generous. In fact, in our follow-up questionnaire (described below), our workers indicated that they were very satisfied with their wage.

The workers' job was to call phone numbers from a list generated by the Leibniz Institute for Social Sciences. For each phone number called, workers had to indicate on the list whether (i) the number did not exist (due to random generation), (ii) nobody answered the phone after ringing for 15 seconds, (iii) a telephone-answering device answered the call, (iv) the line was busy, (v) the interviewers were asked to call back later on, (vi) an interview was denied, or (vii) an interview was completed.

The workplace was a call-center that could be rented out to provide telephone services. We rented five single offices out of 15 (a picture of a typical workplace is included in the Online Appendix). The other offices were rented out by other organizations. Our workers had a 15-minutes time frame during which they were supposed to appear at the call-center. Upon arrival, we registered them and brought them to their single office. Three to five subjects worked for us simultaneously. Each worker could have breaks whenever he/she wanted to.[6] There were no common breaks and no staff rooms. The work environment made social interaction between our workers unlikely.

The call-center IT supplied us with precise connection data, so that we have exact performance measures for each worker with regard to the total number of calls.[7] On average, only one out of 50 dialed numbers led to an interview in the first shift. Hence, for a single worker the number of actually completed interviews was rather low and noisy. Thus, we use

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[4] Yet, we excluded applicants who were recommended by others whom we already had hired. This was done in order to avoid social ties between workers (we discuss this issue in more detail below).

[5] See Bosch and Weinkopf (2008). In 2016, the median annual gross wage for call-center workers in Germany was 24,146 Euros (see statista.com), which corresponds to an hourly net wage of around 10 Euros.

[6] Thus, our workers were able to shirk in different ways, e.g., by surfing the internet or using their smartphones. This design choice therefore impacts on implicit effort costs and productivity (Goerg et al., 2019). However, it is arguably a feature of many work environments.

[7] We requested that workers let the phone ring for 15 seconds (with the clock running on the phone's display). We rate an event as a call if (i) the number existed and (ii) the call time was at least 15 seconds or a client/answering device picked up the phone. In the Online Appendix, we show as a robustness check that our results remain unchanged when we vary the 15-second threshold.

the number of calls as our main outcome variable. It is the closest proxy to effort and has been used as performance measure in previous call-center studies (e.g., Friebel and Seabright, 2011, or Bloom et al., 2015). In the Online Appendix, we present our results for a number of alternative outcome variables.

3.2 Experimental treatments in the call-center

We conducted three treatments, which we call "No-layoff", "Quasi-layoff", and "Layoff." The first shift was identical in all treatments. After the first shift, we randomly selected 20% of the workers from the Layoff and Quasi-layoff treatments and asked them not to work for us during the second shift.[8] In the No-layoff treatment, all workers remained employed for the second shift. To keep workers' prospects in the organization constant across treatments, we paid them their wage in all treatments at the beginning of each shift. If we had paid the wage at the end of a shift, workers in the Layoff treatment might have been concerned about the employer's reliability of actually paying the money after learning about the layoffs. This could have affected their beliefs about prospects and hence their work effort. By paying in advance, we can rule out this potential confound when comparing workplace performance across treatments.

We communicated the layoffs to the remaining workers in a two-step procedure. The day before the second shift, we sent workers in the different treatments the messages shown in Figure 1 via e-mail (originally in German):

[8] Specifically, we called them two days before their second shift. We avoided the term "layoffs" at this stage. For ethical reasons, we paid these workers the promised 40 Euros for their participation in a survey a few weeks after the field experiment (we therefore did not deceive our workers). They were unaware of this when they received the layoff message. Moreover, we debriefed them about the field experiment and their role after the completion of all experiments. The remaining workers were not informed that laid-off workers would be paid the 40 Euros.

No-layoff treatment	Quasi-layoff and Layoff treatments
Dear Interviewer,	*Dear Interviewer,*
We are happy that so many of you supported our countrywide survey on refugee integration: more than 50 interviewers worked for us.	*We are happy that so many of you supported our countrywide survey on refugee integration: more than 50 interviewers worked for us.*
There will be no organizational changes tomorrow.	*There will be no organizational changes tomorrow, except that we will have 20% fewer staff.*
We would like to remind all interviewers about their second shift. Please arrive at the call-center on time. A colleague will guide you to your office.	*We would like to remind all interviewers about their second shift. Please arrive at the call-center on time. A colleague will guide you to your office.*
As we said earlier, this appointment is your last employment in our project. We can only offer you to work for us in two shifts of 3.5 hours each. We hope that you will understand.	*As we said earlier, this appointment is your last employment in our project. We can only offer you to work for us in two shifts of 3.5 hours each. We hope that you will understand.*

Figure 1: *Messages to workers before the second shift*

The message works as a simple reminder and (again) makes clear that there are no future employment opportunities in our organization. When workers arrived for their second shift, they received the messages shown in Figure 2 as a printed note on their desk:

No-layoff treatment	Quasi-layoff treatment	Layoff treatment
Please do not advertise this job to others any longer, we have enough workers!	*Please do not advertise this job to others any longer, we have enough workers!*	*Please do not advertise this job to others any longer, we have enough workers!*
	Due to organizational reasons, we already have 20% fewer staff.	*Due to organizational reasons, we already have 20% fewer staff.*
		The reason for this is that we decided to lay off some of your colleagues. This allows us to reduce costs. The selection of laid-off workers has been random.

Figure 2: *Messages to workers at the start of second shift*

The first sentence in all treatments contained a natural message for the workers. During the first shift, a number of them had recommended family members and friends to us

as suitable workers (we never hired any of them). The second sentence in the Layoff and Quasi-layoff treatment provides a partial justification for our request not to advertise the job to others, but without any reference to potential layoffs. The last part of the message for the Layoff treatment is central to our purpose. It informs workers about the layoffs, as well as why and how they took place. In particular, it mentions that layoffs were random, which made clear that they were not based on productivity in the first shift.[9] Moreover, the message communicates that the layoffs helped us to reduce costs. This was correct since we paid less to the call-center when we used fewer offices. This message was in clear contrast to the otherwise generous employer behavior. It made salient that the employer was concerned with labor costs and prioritized cost reductions over employee welfare. A significant literature shows that laying off workers is an extremely unpopular measure (e.g., Datta et al., 2010).

The three parts of the message sent a clear signal to our employees that we do not need more workers. Importantly, the layoff message did not appear in isolation from the other messages, but was part of a coherent explanation, which avoids potential suspicion about the true nature of the job (which we verify in our follow-up survey).

In our analysis, we will use the Quasi-layoff treatment as our main control treatment. It differs from the Layoff treatment only in the short layoff message at the beginning of the second shift. The No-layoff treatment additionally differs from the Layoff treatment by the message about the staff size reduction. By comparing the output in the No-layoff and Quasi-layoff treatment, we can check whether the staff size reduction per se has an effect on the workers' productivity.

To avoid deception in our experiment, the only promise we made was a payment of 2 x 40 Euros, which we kept for all complying workers. We never promised the participation in two shifts. During the recruiting, we only stated that two shifts per interviewer "are envisaged."

In preparation for the field experiment, we registered the project at the AEA RCT Registry (ID: AEARCTR-0001075). In our registration, we proposed the number of calls as our main outcome variable and the Quasi-layoff treatment as the baseline treatment. We applied for and received ethical approval from the Independent Review Board of Trier University (where two authors were affiliated when we started our study). Using phone data from Chadi et al. (2016), we calculated that we would need around 50 observations in each

[9] Random layoffs may be interpreted as a "fair" mechanism to reduce labor costs (see, e.g., Sebald, 2010, for a discussion of how random draws may change the attribution of responsibility). To study how impartial spectators would evaluate our layoffs, we conducted a vignette study with university students (in the spirit of Kahneman et al., 1986, or Charness and Levine, 2000). In this study, random layoffs were assessed as especially unfair; see the Online Appendix for details.

treatment to detect a 10-% treatment effect at the 5-% significance level with a probability of 80%.

3.3 Follow-up survey and debriefing

Follow-up survey. After the completion of the field experiment, we invited all workers to participate in a survey in which we asked them about their beliefs and perceptions during their job. Moreover, we evaluated the subjects' work satisfaction. We paid 15 Euros for their participation.[10] In total, 75.3% of the subjects who worked in both shifts participated in the survey.

Debriefing. After the completion of the follow-up survey, we debriefed all workers, including those we had laid off. We informed them that the job at the call-center was part of an experiment and invited them to answer a few questions related to the field experiment. Specifically, we asked workers about the extent to which they expected negative consequences for themselves once they had learned about their co-workers' layoffs, about their perceptions of the management's competence, about their beliefs as to the importance of the job, as well as about their social network. To obtain precise data on the social network in our workforce, we asked workers whether they knew any co-workers from the call-center. For each correct name, we paid them 0.50 Euros. We also asked the remaining workers from the Layoff treatment about how they evaluated the layoff announcement. In total, 74.1% of the workers who were employed in both shifts responded to our questions in the debriefing. For more details on the questions from the surveys, see the Online Appendix.[11]

[Insert Table 1 about here]

Timeline. The timeline of the complete project is as follows. We started the recruitment of our subjects two weeks before the experiment. The field experiment took eight subsequent weeks. Six weeks after the end of the experiment, we started the follow-up survey, which took eight weeks. Six weeks after the end of the follow-up survey, we started the debriefing, which

[10] We also conducted an online experiment with our workers in which we elicited their degree of direct/indirect positive/negative reciprocity. The experimental game is a simplified and extended version of the moonlighting game (Abbink et al., 2000). At the point in time when they participated in the online experiment, workers had no reason to believe that the online experiment was related to their call-center job. We did not find any relationship between the experimental reciprocity measures and behavior in our field experiment. The detailed results can be obtained upon request from the authors.

[11] We find no evidence for significant selective participation in the follow-up and debriefing surveys (see the Online Appendix for details).

took nine weeks.

4. Results

4.1 Descriptive statistics and randomization checks

In total, we had 52 workers in the No-layoff treatment;[12] 69 in the Quasi-layoff treatment, of whom 13 were laid off; and 74 in the Layoff treatment, of whom 13 were laid off.[13] These numbers comprise only workers who participated in both shifts or who were laid off; 5 workers who were not laid off did not show up for the second shift. Thus, attrition within the field experiment is only 2.5%. In the following, we ignore the laid-off workers.[14] The selection into treatments was random.[15]

Table 1 summarizes, for each treatment, the surviving workers' performance in the first shift (Panel A) and their characteristics (Panel B). During the first shift, workers called on average 89.8 existing numbers (sd = 23.7). There are no economically or statistically significant differences in performance between treatments (Panel A, columns 5 and 6).

Workers were between 16 and 65 years old (mean = 27.7, sd = 10.4), 62.3% were female, and 91.7% were born in Germany. The sample was quite heterogeneous in terms of education and employment status. Half of the workers were university students, and a quarter consisted of unemployed individuals. Around 50% indicated that they regularly worked in short-term jobs similar to the one in the call-center. For almost all characteristics, we do not

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[12] This number includes seven workers for whom we lost the second-shift phone data due to IT problems. We dropped them from the sample. Our treatment groups are still balanced if we include their data. Moreover, if we include these workers in the dataset, our results for the self-reported performance remain unchanged; see the Online Appendix. Due to the reduced number of observations in the No-layoff treatment, comparisons between this and the other two treatments have to be taken with some care.

[13] We had to decide about the number of layoffs before we knew the number of workers who returned for the second shift. Since we anticipated a slightly higher dropout rate than was actually realized, we laid off (in the Layoff treatment) one or two fewer workers than we should have in order to meet the 20-% level exactly.

[14] In the Online Appendix, we show that there are no significant differences in the characteristics of laid-off workers and remaining workers with regard to their performance in the first shift.

[15] We had 8 x 4 days of data collection, with 4 days per week and three shifts per day (the exact weekdays vary between weeks due to public holidays). Two subsequent weeks (8 days) form a "block." We accepted only applicants who were available on at least two days (because of two shifts) in two different blocks, so that we could allocate them randomly to different treatments, independently of availability and time preferences. The allocation to treatments varied between blocks. In the first block, we only conducted the No-layoff treatment; in the three other blocks, we conducted all three treatments. Thus, in the No-layoff treatment, about 75% of workers were employed during the first two weeks, and the remaining 25% in the last six weeks. All workers in the Quasi-layoff and Layoff treatment were employed in the last six weeks (we chose this procedure to minimize potential spillover effects). In these weeks, we determined the treatment of a shift randomly (by coin-flips) so that the probability of the No-layoff treatment would be 1/6 and the probability of the Quasi-layoff and Layoff treatment 5/12 each.

find significant differences between treatments (Panel B, columns 5 and 6).[16] Thus, our worker sample is balanced and randomization worked well.

4.2 Main results

To study whether workers respond to the layoff announcement by exerting less effort, we first compare descriptively the difference in the average number of phone calls between shifts in our treatments. Figure 3 and Panel C (row 1) of Table 1 show the results. We find that the average number of calls decreases by 6.8 calls in the Quasi-layoff (7.6%), by 6.6 calls in the No-layoff (7.6%), and by 17.9 calls in the Layoff treatment (19.4%). Thus, the number of calls decreases in all treatments; however, the reduction is larger in the Layoff treatment compared to the other treatments. The difference in the change in the number of calls is significant between the Layoff and the Quasi-layoff treatment (t-test p-value = 0.031) and between the Layoff and No-layoff treatment (t-test p-value = 0.072).[17]

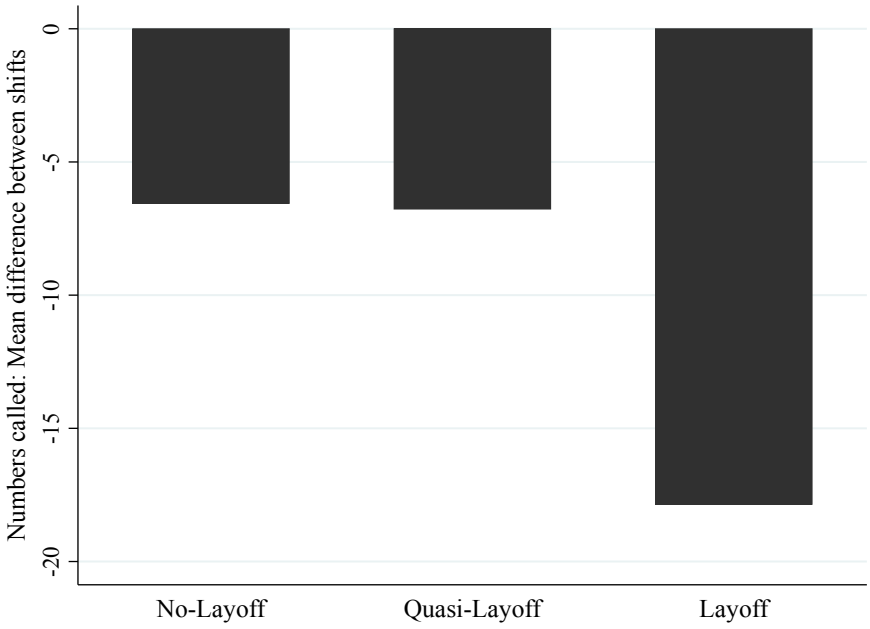

Figure 3: *Difference in the number of calls between the second and first shift, by treatment*

Next, we compare the performance reductions between treatments in several

[16] One exception is the variable "working day." The share of workers who worked between Monday and Wednesday is larger in the No-layoff than in the Layoff treatment. We therefore control in one specification of our regressions for the working day. We do not find any statistically significant effect of working days on performance.

[17] All tests in this paper are two-sided. For our performance measures, we report t-tests, since these variables are roughly normally distributed. For all other variables (which are not normally distributed), we either report Mann-Whitney tests (non-binary variables) or Chi square tests (binary variables). The cumulative distributions of the performance differences between treatments are displayed in the Online Appendix.

regression frameworks. First, we estimate the effect of the layoff announcement on the workers' performance in the difference-in-difference specification

$$y_i = \beta_0 + \beta_1 \textit{No-layoff}_i + \beta_2 \textit{Layoff}_i + \beta_3 t + \beta_4 \textit{No-layoff}_i \times t + \beta_5 \textit{Layoff}_i \times t + \varepsilon_{i,} \quad (1)$$

where y_i is the number of calls of subject *i*. The shift dummy *t* is 0 for the first and 1 for the second shift. The variables $\textit{Layoff}_i$ and $\textit{No-layoff}_i$ are dummy variables that are set to 1 for the corresponding treatment (the Quasi-layoff treatment serves as the baseline), and ε_i is the idiosyncratic error term. In our second specification, we additionally control for the subjects' age, gender, educational background (university degree, vocational training, etc.), and current employment status (regular job, student, unemployed). In the third specification, we additionally include dummy variables for each working day, a dummy capturing whether the working day was between Monday and Wednesday (versus between Thursday and Saturday), and dummy variables for the working time (morning, noon, afternoon). In the fourth specification, we omit the controls, estimating a generalized difference-in-difference model to assess potential regression to the mean:

$$y_{i1} = \beta_0 + \beta_1 \textit{No-layoff}_i + \beta_2 \textit{Layoff}_i + \beta_3 y_{i0} + \varepsilon_{i.} \quad (2)$$

The variable y_{i0} (y_{i1}) denotes worker *i*'s number of phone calls in the first (second) shift. In all specifications, standard errors are clustered both on the subject and time of day level (two-way clustering).[18]

[Insert Table 2 about here]

Table 2 shows the regression results for the four specifications. We observe three important patterns. First, as the negative coefficient for the shift *t* (which is significant in the first and second specification) indicates, workers seem to make fewer calls in the second shift compared to the first one in all treatments. Second, in all specifications, the coefficient for the No-layoff treatment is small and insignificant, showing that the reduction in performance is almost identical in the No-layoff and Quasi-layoff treatments (around 6%). Third, according to the estimates in the first, second and fourth specification, workers call around 11 fewer

[18] The working environment in our call-center makes social interaction between workers in the same shift unlikely (see Section 3.1). Indeed, we find no evidence for significant social interaction within shifts (see the Online Appendix). Hence, we do not cluster standard errors on the shift level in our regressions. When we cluster standard errors in our generalized diff-in-diff estimation on the shift level, our main results are unchanged (see the Online Appendix). Note that clustering standard errors in the first shift is not necessary to estimate our treatment effects as all workers were treated in the same way in the first shift.

numbers in the second shift in the Layoff treatment compared to the Quasi-layoff treatment. The baseline performance is around 90 phone calls in all treatments in the first shift. Thus, the coefficient shows that the layoff announcement reduces the number of calls by around 12%. In the third specification (which has to be taken with some care, as the specification includes more than 40 dummies), the size of the effect is even slightly larger.

A number of robustness checks confirm these results. Specifically, we use several alternative performance measures: the number of calls without the 15-second call time threshold or with a call time threshold of 30 seconds, the number of calls normalized by the number of interviews,[19] the gross working time, the total time spent on the phone, the total number of conducted interviews, and several self-reported performance measures. We also re-run our baseline regression and all regressions in which we use the alternative performance measures as outcomes, using the No-layoff treatment as the baseline treatment. The results are presented in the Online Appendix. In all regressions, the announcement of layoffs reduces performance by around 12 or more percent.

Main Result. *Workers respond to the announcement of layoffs by decreasing their performance by 12%. The mere announcement of the reduced staff size has no detectable effect on the workers' performance.*

We find two further noteworthy patterns when we compare worker performance across treatments. First, we observe that the drop in performance in the Layoff treatment is related to a reduction in what we call "gross working time." It is defined as the exact duration between the time when workers dial the first existing number and the time when they put down the phone for the last time. From the call-center IT we know at what time a worker called the first and the last existing number. We use this as proxy for the actual beginning and end of the working time. (We do not know the exact time when workers dialed the first or last number, since that could have been a non-existent phone number).

If we re-run our baseline regression using gross working time as a dependent variable, we find that it is 12% shorter in the Layoff treatment than in the other treatments; see the Online Appendix. It seems that workers in the Layoff treatment did not take longer breaks during the second shift. Instead, they called the first existing phone number around 8 minutes later and stopped calling numbers around 12 minutes earlier in the Layoff treatment compared

[19] Interviews took around five minutes. During this time, no other calls could be made. Therefore, the normalized number of calls is defined here as the number of calls divided by the total working time excluding interview time.

to the other treatments (we get this result from a difference-in-difference comparison between treatments).

Second, we find some evidence for differences in the likelihood with which workers were able to conduct and complete an interview on the phone. When a call is answered, workers have to persuade their conversation partner to participate in the survey. Hence, the completion of an interview can be interpreted as an indicator of a worker's quality of output. The probability of completing an interview conditional on a call being answered in the first shift is 17.6% (sd = 11.3) in the Layoff treatment, 17.9% in the Quasi-layoff treatment (sd = 9.4) and 16.6% (sd = 9.0) in the No-layoff treatment. The probabilities of completing an interview do not differ significantly between treatments in the first shift (t-test, all p-values > 0.620). In the second shift, the probabilities are 15.3% (sd = 9.4) in the Layoff treatment, 20.4% (sd = 12.8) in the Quasi-layoff treatment and 15.4% (sd = 9.3) in the No-layoff treatment. The changes in the probabilities of completing an interview between the first and second shift differ significantly between the Layoff and Quasi-layoff treatment (t-test p-value = 0.059); this difference is not significant between the Layoff and No-layoff treatment (t-test p-value = 0.640). When we re-run our baseline regression, using the probability of completing an interview as dependent variable, we find a statistically significant decrease in the Layoff compared to the Quasi-layoff treatment in most specifications (see the Online Appendix). To conclude, we find some evidence that after the layoff announcement workers exert less effort to persuade their interviewees. This indicates that the layoff announcement not only affects the gross working time, but also the performance while working.

5. Mechanisms

Our preferred explanation for our main result is that the workers in the Layoff treatment perceived the employer's behavior as unfair and therefore reduced their performance relative to the other treatments. To support this claim, we present three different types of evidence based on three different datasets: follow-up survey, debriefing survey, and an online prediction experiment. First, we provide direct evidence that is in line with our proposed mechanism. Second, we show that alternative behavioral motives are unlikely to explain our findings. Finally, we examine the results from a prediction experiment with HR managers. Their views on our setting are quite consistent with the proposed mechanism.

5.1 How did workers perceive their employer and the layoff announcement?

To study how workers perceived the employer's behavior, we asked them in the follow-up

survey how satisfied they were (i) with the working atmosphere in the call-center, (ii) the wage, (iii) the manager's behavior towards the worker him- or herself, and (iv) the manager's behavior towards the worker's colleagues. The scale ranged from 0 (not satisfied at all) to 10 (very satisfied).

As shown in Table 3, Panel A, the mean scores for all questions are rather high. This indicates that workers were quite satisfied with their job at the call-center. There are no significant differences in the scores between treatments, with two exceptions. Workers seemed to be more satisfied with the wage in the Layoff treatment than in the Quasi-layoff treatment; they are also less satisfied with the managers' behavior towards their colleagues (the effect is borderline significant). The Mann-Whitney test results have to be taken with some care, as these variables are highly right-censored.

[Insert Tables 3 and 4 about here]

When we regress the satisfaction scores in a Tobit regression on treatment dummies, we find no significant differences in the working atmosphere between treatments (Table 4, Panel A). However, workers in the Layoff treatment were more satisfied with the wage (Table 4, Panel B). One potential explanation for this could be that workers were more satisfied with their wage after observing that other workers had been laid off and had not received a second-shift wage at all. While the employer's behavior towards workers themselves was rated equally well in all treatments (see Table 4, Panel C), the management's behavior towards a worker's colleagues was perceived differently across treatments. As shown in Table 4, Panel D, the score for the managements' behavior towards co-workers is around 10% lower in the Layoff- compared to the Quasi-layoff treatment. This provides suggestive evidence that workers perceived the layoffs by the employer as unfair.[20]

Importantly, the results also suggest that the remaining workers themselves did not feel treated unfairly by the management (although they were just lucky enough not to be picked for being laid off). Moreover, the drop in satisfaction with the colleagues' treatment did not affect the workers' satisfaction with the atmosphere in the call-center.

We also examine which contents of the layoff announcement were perceived as adverse in the Layoff treatment. To this end, we asked workers in this treatment the following

[20] We invited all subjects at the same time to participate in the follow-up survey. However, subjects worked in the call-center at varying times. Thus, there is considerable variation in the time between participation in the field experiment and participation in the survey. In specification 2 of Table 4, we control for this variation. Our main results are unchanged. Thus, a lack of memory cannot explain our results.

question in the debriefing: "To what extent did you perceive the following parts of the message as anti-social, on a scale between 1 (not anti-social at all) to 10 (very anti-social)?" The items were (i) layoffs per se, (ii) layoffs to reduce costs, (iii) cost savings per se, and (iv) the random selection of workers. Layoffs per se received an average rating of 6.5 points (sd = 2.9), the layoffs to reduce costs 5.8 points (sd = 2.8), the cost savings 5.2 points (sd = 2.6), and the random selection of workers 6.1 points (sd = 3.4). Thus, it seems that layoffs per se and the random selection of workers were perceived as particularly unfair.[21]

5.2 Can our results be explained by alternative mechanisms?

Next, we use data from our follow-up and debriefing surveys to show that several alternative motives are unlikely to explain our main results. In the Online Appendix, we additionally show this for potential spillover effects.

Did workers expect future negative consequences after the announcement of the layoff? One crucial design feature for the analysis of our research question is that the remaining workers' expectations about their prospects in the organization must be held constant across treatments. We therefore paid all workers their wage at the beginning of each work shift and emphasized repeatedly that there would be no future employment possibilities. In the debriefing, we asked workers about the extent to which they believed that there would be no future employment in our organization. Overall, 88.5% found our announcement credible in retrospect, and there were no significant differences between treatments (see Table 3, Panel B, row 1). Moreover, no worker asked for further employment during or after the experiment.

Another concern could be that workers hoped for some recommendation from the management after their employment, and that getting such a recommendation was less likely when the employer had laid off some workers. However, none of our workers asked for a certificate of employment or recommendation.

Did the announcement of the layoff signal a lack of management competence? The layoff announcement may have signaled to workers that the management was struggling with the

[21] We also asked the subjects from the Quasi-layoff treatment in the debriefing what exactly they thought when they read the message about the 20% staff size reduction. 70% either did not remember the message or thought the message was not important or thought of "nothing"; 12% thought that some of their co-workers decided to quit voluntarily; 18% thought that (i) there were layoffs, or (ii) fewer surveys than initially planned were needed, or (iii) workers had to quit because of bad performance (the distinction between the answers in (i) to (iii) is blurred in many cases). This indicates that only a minority thought of layoffs.

organization of the project. This in turn may have lowered their work motivation. To evaluate this explanation, we asked subjects to rate the management's competence on a scale from 1 (not competent at all) to 10 (highly competent). We made this request in the debriefing before informing workers that they had participated in an experiment. The mean ratings were quite high (8.1 points, sd = 1.7), and there were no significant differences between treatments (Table 3, Panel B, row 2). Thus, the layoff announcement had no significant impact on the workers' perception of the management's competence, ruling out that different perceptions might have driven the drop in performance in the Layoff treatment.

Did the announcement of the layoff signal a lack of task meaning? Another alternative explanation for our main result could be that the layoff announcement lowered the workers' perception of the importance of the task for the employer. To rule out that our results might have been driven by differing perceptions of task importance, we asked workers in the debriefing how important they believed the interviews to be for the employer, on a scale from 1 (not important at all) to 10 (very important). Again, we made this request before workers knew that they had participated in an experiment. The mean rating was 7.4 points (sd = 1.9); 76% of the workers indicated a score of 7 or higher. Thus, the majority of workers believed that the task was very important.[22] We observe no statistically significant differences in the rating between treatments (Table 3, Panel B, row 3). Thus, varying perceptions of the importance of the task cannot explain our main result either.

Did the announcement of the layoff signal that workers were too productive in the first shift? Another potential side effect of the layoff announcement could be that workers believe that they were "too productive" in the first shift and that the employer reduced the staff size as a consequence. They may then conclude that working less is justifiable. To counter such beliefs, we provided each worker with a large number of phone numbers to call, which were impossible to complete within a single shift. Moreover, we asked workers in the follow-up survey a number of questions referring to potential reasons why they performed better or worse in the second shift. In particular, we asked to what extent they agreed with the following statement: "I worked less in the second shift since my colleagues and I already performed very well in the first shift, so we could relax a bit." The scale was between 0 (do not agree) and 10 (fully agree). The average response to this statement was very low (2.1 points, sd = 2.1), indicating fairly strong disagreement with the statement. There were no

[22] We find no correlation between the workers' perception of the importance of the task and the number of phone calls in the first shift.

significant differences between treatments (Table 3, Panel B, row 4). The performance reduction in the Layoff treatment therefore cannot be explained by a change in the perceived necessity to work hard.

How credible was the experimental setting? Around 50% of our employees indicated in our follow-up survey that they are regularly employed in short-term jobs similar to the one in our call-center (e.g., as security guards at concerts or as harvest workers). This suggests that for many of them our workplace setup is not uncommon. However, our messages at the start of the second shift may have made workers suspicious about the true nature of their job. For example, they may have guessed that they participated in an experiment. In the debriefing, we asked workers whether they had any idea that they were part of an experiment when they worked in the call-center. The vast majority – 88.3% of our workers – reported that they did not think that they were participating in an experiment.[23] Importantly, this number does not vary significantly between the Layoff and the two other treatments (Table 3, Panel B, row 5). Thus, the layoff announcement did not alter our workers' perception of the credibility of the experimental setting. Moreover, we asked this question after we had told subjects about the experiment. Thus, the remaining 10% of workers may just have fallen prey to hindsight bias.

5.3 Do HR managers predict our main findings? How would they interpret them?

Human resources managers are concerned with optimizing employee performance. They are not only responsible for the design of employee benefits, recruitment and training, but also for organizational change, which may involve the announcement and implementation of layoffs. Thus, they are in an ideal position to predict the consequences of our layoff announcement, and to examine different potential explanations for these consequences.

To get access to professional HR managers, we collaborated with one of the largest associations of German HR managers ("Deutsche Gesellschaft für Personalführung e.V.") and with one of the largest HR magazines in Germany ("Personalwirtschaft"). Through both channels, HR managers were invited to participate in an online survey and experiment.[24]

In the instructions for the online experiment, we explained the design and procedures of our field experiment in detail. Subsequently, we informed the managers about the average

[23] Excluding workers in our baseline regression who reported that they were aware of participating in an experiment does not change our main results (see the Online Appendix).

[24] We describe all procedures and instructions of this online survey and experiment in the Online Appendix.

number of calls in the first shift. Given that there had been no treatment differences, we communicated an average of 90 calls across all treatments. Then we asked HR managers to predict the average number of calls in the second shift separately for the No-layoff, Quasi-layoff, and Layoff treatment. We showed them the exact wording of the messages to workers before the second shift (as shown in Figure 2). The three treatments were presented in neutral framing ("Group 1, 2, and 3"), so we did not mention the treatment names used in this paper.

To ensure that managers had an incentive to reveal their beliefs truthfully, we used the belief elicitation method from Gächter and Renner (2010). A manager earned 30 Euros for a correct guess; if he or she missed the true number of calls, the payoff was 20 Euros, divided by the absolute distance between his or her predicted and the true average number of phone calls. A manager's total earnings was the payoff from one randomly chosen prediction (out of the three predictions for the three treatments), plus a fixed participation fee of 30 Euros. On average, managers earned 32.50 Euros in total.

[Insert Table 5 and Table 6 about here]

After HR managers had entered their predictions for the average performance in the second shift, we revealed the true averages and then presented them with a list of five potential reasons for the lower performance in the Layoff treatment (see Table 6). We asked them first which of those reasons they considered as the most likely one for the significant productivity drop in the Layoff treatment, and then we let them rate the chance with which each reason was responsible for the decline in productivity.

We invited 110 HR managers, of whom 43 HR participated in the online experiment. They worked in 23 different two-digit industries (NACE codes);[25] 51% of the managers were female; 83% of them worked in organizations that employed more than 500 workers. They had, on average, 7.6 years (sd = 5.3) of professional experience in HR departments, and more than 70% of them had interviewed more than 50 job candidates (with an overall average number of interviews close to 300); 35% were involved several times in the implementation of layoffs, 21% were involved once, and 44% never.

Table 5 (row 1) shows the HR managers' predictions. For the No-layoff and the Quasi-layoff treatments, they expected, on average, a slight drop, although their average estimate is not significantly different from 90. Actual performance data (shown in the second row) were lower (in the low 80s), but predictions and real data were not significantly different (MW test

[25] For four managers, we do not know in which two-digit industry they worked.

p-values > 0.241 in both cases). Their predictions for the Layoff treatment are remarkably different. They expected a large downward shift of productivity in this treatment. Their guess was, on average, 74.5 calls, which was very close to the actual average of 74.6 calls (MW test p-value = 0.661). Importantly, the managers' predictions for the Layoff treatment was significantly lower than their predictions for the other two treatments (MW test p-values < 0.001 for both comparisons; the difference between No-layoff and Quasi-layoff is not significant). Hence, they were aware of the potentially large negative productivity shock caused by the layoff announcement. The average predicted difference-in-difference effect (Layoff versus Quasi-layoff treatment) is -16.5%.

As a next step, we wanted to know from the HR managers – after revealing to each participant the true performance in the second shift of each treatment – why the workers in the Layoff treatment had reduced their performance. Table 6 shows the managers' responses. Looking at the first column, we note that 76.7% of them indicated "the layoff of co-workers [was] considered as unfair behavior of the employer" as the most likely reason for lower productivity. All other options listed in Table 6 were much less often regarded as the most important reason for the productivity decline in the Layoff treatment.

The second column of Table 6 confirms the results from the first column. HR managers rated the unfair behavior of the employer by far as the most likely explanation for the decline in productivity in the Layoff treatment. The average rating of 8.6 is significantly larger than the average rating for any of the other four reasons (MW test p-values < 0.001 in all cases).

6. Conclusion

In this paper, we measured how an employer's adverse behavior towards co-workers changes the productivity of unaffected workers. Studying this question seems of utmost importance from the perspective of organizations, as it reveals a frequently overlooked price that employers have to pay when they adversely affect a subgroup of workers. While for methodological reasons we have looked at short-term effects, adverse employer behavior may also have longer-term effects (and it would be promising to look into that in the future).

In our experiment, the announcement of layoffs reduced the performance of unaffected workers by about 12%, compared to both a treatment without any layoffs and to a treatment where workers were informed about an equivalent reduction in staff size, without alluding to layoffs. This effect is mainly due to reduced work effort both at the beginning and at the end of the last shift of the layoff treatment. Moreover, we found indications that the quality of

workers' output also decreased after the layoff announcement.

Several follow-up and debriefing surveys allowed us to study the behavioral motivation for these findings. In the layoff treatment, workers were less satisfied with how management treated their co-workers than in the control treatments. Moreover, we can rule out that the drop in performance in the Layoff treatment was due to altered beliefs about the management's competence, task meaning, or the anticipation of negative consequences in the future. This suggests that the performance reduction is due to a form of indirect reciprocity and the workers' perception of unfair employer behavior. This interpretation is confirmed in a prediction experiment with professional HR managers. In the aggregate, they predicted the magnitude of the productivity drop very well. Importantly, they named the perception of layoffs as unfair measure as the most likely reason for the strong decline in workers' productivity. Thus, they had a sense for the potential consequences of an employer's adverse behavior on unaffected workers even in situations where the primary victims had already left the company and unaffected workers did not even have a chance to interact with them.

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Table 1: *Descriptive statistics, by treatment group*

	Layoff (n=61)	**Quasi-layoff** (n=56)	**No-layoff** (n=45)	**Layoff vs. Quasi-layoff** (p-value)	**Layoff vs. No-layoff** (p-value)
Panel A: Performance in first shift					
Number of calls (call time > 15 seconds)	92.5 (22.0)	88.9 (26.9)	87.4 (21.9)	0.437	0.245
Total call time (in minutes)	91.3 (24.2)	91.8 (25.2)	84.8 (23.1)	0.911	0.166
Gross working time (in minutes)	206.5 (9.7)	208.5 (10.1)	206.1 (9.5)	0.275	0.829
Amount of dialed numbers (self-reported; and non-existing)	243.2 (66.9)	238.7 (68.7)	230.5 (56.0)	0.720	0.308
Amount of dialed numbers (self-reported; only existing numbers)	104.9 (28.8)	106.1 (29.2)	105.1 (25.4)	0.826	0.971
Number of interviews (self-reported)	5.2 (2.6)	5.3 (2.0)	4.8 (2.5)	0.779	0.469
Number of interviews (self-reported; call-time > one min)	4.9 (2.5)	5.0 (2.2)	4.6 (2.2)	0.682	0.565
Completion of an interview (%)	17.6 (11.3)	18 (9.4)	16.6 (9.0)	0.859	0.620
Panel B: Worker characteristics and working times in first shift					
Age	26.3 (8.2)	30.0 (13.0)	26.9 (8.9)	0.296	0.954
Female (in %)	63.9	64.3	57.8	0.968	0.520
Nationality: German (%)	91.8	91.1	88.9	0.888	0.612
Student (%)	57.4	50.0	48.9	0.424	0.386
University degree (%)	14.8	14.3	6.7	0.943	0.194
Unemployed (%)	21.3	14.3	35.6	0.323	0.104
Vocational training (%)	24.6	21.4	17.8	0.685	0.400
Full- or part-time job (%)	39.3	42.9	28.9	0.700	0.264
Previous employm. similar to call-center job (%)	28.3	25.5	17.8	0.728	0.209
Regular employment on a short-term basis (%)	44.7	44.7	57.1	1.000	0.296
Working day: Monday - Wednesday (%)	70.5	62.5	40.0	0.360	0.002
Working day: Thursday - Saturday (%)	29.5	37.5	60.0	0.360	0.002
Time of the day: Morning (%)	24.6	35.7	37.8	0.189	0.144
Time of the day: Noon-afternoon (%)	36.1	32.1	31.1	0.655	0.594
Time of the day: Afternoon (%)	39.3	32.1	31.1	0.417	0.382
Panel C: Performance differences between second and first shift					
Number of calls (call time > 15 seconds)	-17.9 (29.2)	-6.8 (25.3)	-6.6 (34.5)	0.031	0.072
Total call time (in minutes)	-26.4 (28.7)	-14.8 (19.3)	-12.4 (28.8)	0.012	0.015
Gross working time (in minutes)	-31.4 (49.4)	-10.0 (28.7)	-11.7 (37.7)	0.005	0.028
Amount of dialed numbers (self-reported; and non-existing)	-38.0 (81.4)	-3.2 (56.5)	2.7 (70.6)	0.009	0.009
Amount of dialed numbers (self-reported; only existing numbers)	-18.8 (34.4)	-4.7 (32.4)	-1.3 (32.4)	0.025	0.010
Number of interviews (self-reported)	-1.3 (2.9)	-0.6 (2.3)	-0.4 (2.9)	0.161	0.113
Number of interviews (self-reported; call-time > one min)	-1.2 (2.7)	-0.7 (2.3)	-0.5 (2.9)	0.251	0.229
Completion of an interview (%)	-0.02 (0.13)	0.02 (0.14)	-0.01 (0.11)	0.059	0.640

Data sources: the IT data from the call-center (source 1); the number lists on which workers indicated the outcome from each phone call (source 2); the workers' résumés and a form that workers completed prior to

their first shift in the call-center (source 3); the follow-up survey (source 4). In Panel A, we use data from source 1 for the first three variables (and partly for the seventh variable) and data from source 2 for the remaining variables. In Panel B, we use data from source 3 for the first nine variables and data from source 4 for the last variable. Definition of variables: *Vocational training* means that a worker has completed a vocational training or is enrolled in a vocational training at the time when the field experiment took place. *Full-time or part-time job* means that a subject has a full-time or part-time job at the time of the field experiment (we did not count mini-jobs as full-time or part-time jobs). *Regular employment on a short-term basis* means that a subject's reply was "yes" to the question whether they had similar short-term jobs like the one in the call-center in Saarbrücken in the past two years (e.g., short term employment in construction or agriculture). Missing observations: Source 2: We dropped one observation in the Layoff treatment. The subject did not make notes about her phone calls on the list in the first shift. Source 3: We have no data for two subjects as they neither submitted their résumé nor participated in the survey. For 20 subjects, the employment data are missing, as they did not submit their résumés. No-layoff treatment: In two shifts, the IT of the call-center did not record the phone data. For our analysis, we drop the data from the subjects who worked in these two shifts. Our treatment groups are still balanced if we include the data from the omitted subjects. In the Online Appendix, we show that our main regression results are the same when we use the self-reported performance data as dependent variable and include the data from the omitted subjects. Statistical tests: In columns 5 and 6, we report p-values of either two-sided t-tests (Panel A and C; all performance measures are roughly normally distributed) or two-sided Mann-Whitney rank-sum tests (Panel B) for non-binary variables. For binary variables, we use a Chi-square test.

Table 2: *Baseline regression: number of calls*

	Number of calls			Numbers called s_2
Specification	(1)	(2)	(3)	(4)
Constant	88.929***	56.118***	-25.501	18.564**
	(3.595)	(20.407)	(32.648)	(9.093)
t	-6.768**	-6.768*	-10.235	
	(3.407)	(3.502)	(7.493)	
No-layoff	-1.506	-2.378	17.202	-0.217
	(4.854)	(5.236)	(11.746)	(6.014)
No-layoff × t	0.212	0.212	-3.602	
	(6.184)	(6.358)	(13.624)	
Layoff	3.530	2.105	6.790	-10.079**
	(4.568)	(4.574)	(8.476)	(5.024)
Layoff × t	-11.085**	-11.085**	-18.056**	
	(5.370)	(5.428)	(7.847)	
Numbers called s_1				0.715***
				(0.094)
R^2	0.044	0.108	0.246	0.266
Sample size	324	324	324	162
Subject characteristics	No	Yes	Yes	No
Point in time	No	No	Yes	No

OLS regressions. In specifications (1)-(3), the dependent variable is the number of calls. In specification (4), the dependent variable is the number of calls in the second shift. We only count phone calls if (i) the number is from the list of phone numbers that workers received, and (ii) the phone rang for at least 15 seconds (in case neither a telephone-answering device nor a client picked up the phone while it was ringing). In a rare number of cases (less than 2% of all phone calls), workers received an incoming call from a client who had been called in the past. We treat these phone calls as regular phone calls. If we exclude these phone calls, our main results are unchanged. The shift dummy *t* is set to zero (one) for the first (second) shift. *No-layoff* and *Layoff* are treatment dummies set to one for the corresponding treatment (and zero otherwise). Controls: *Subject characteristics* are dummies for *Female*, *Student*, *University degree*, *Regular job*, *Vocational training* and *Unemployed.* Moreover, we include *Age* and *Age squared*. *Point in time* are dummies for the *Working day* (a dummy that is set to one for the day on which a worker worked in the call-center) and *Time of day* (three dummies – one for the morning (8 am to 12 am), the afternoon (12 am to 4 pm), and the evening (4 pm to 8 pm)). In specification 3, we also include a dummy that captures whether a subject worked between Monday and Wednesday. Standard errors are clustered on both subject level and time of the day. One subject worked between 10 am and 1.30 pm in one shift; we treat the standard errors of this subject as if she had worked in the morning. * p<0.1, ** p<0.05, *** p<0.01.

Table 3: *Results from the follow-up and debriefing survey*

	Layoff	**Quasi-layoff**	**No-layoff**	**Layoff vs. Quasi-layoff** (p-value)	**Layoff vs. No-layoff** (p-value)
Panel A: Satisfaction with different aspects of the job in the call-center					
Working atmosphere	9.0 (1.5)	9.1 (1.5)	9.2 (1.8)	0.912	0.425
Wage	9.3 (1.2)	8.9 (1.3)	9.0 (1.6)	0.059	0.815
Manager's behavior towards the worker him-/herself	9.2 (1.1)	9.5 (1.1)	9.0 (2.0)	0.561	0.413
Manager's behavior towards the worker's colleagues	8.7 (2.2)	9.3 (1.3)	9.3 (1.4)	0.135	0.261
Panel B: Beliefs and perceptions about the job in the call center					
Future employment opportunities in the organization (%)	95.7 (20.6)	90.9 (29.1)	96.7 (18.3)	0.367	0.824
Workers' perception of management competence	8.1 (1.6)	8.3 (1.5)	8.0 (1.9)	0.672	0.810
Workers' perception of task importance	7.2 (2.1)	7.3 (1.9)	7.6 (1.7)	0.811	0.509
Workers' belief that they were "too productive" in the first shift	2.2 (2.2)	2.0 (1.8)	2.1 (2.4)	0.616	0.678
Workers' belief about the credibility of the experimental setting (in %)	89.1 (31.4)	86.4 (34.7)	90.0 (30.5)	0.690	0.904

Panel A shows the workers' responses to the question 1 in the follow-up survey. Panel B shows the workers' responses to question 2 from the follow-up survey and to the questions 1, 2, 5 and 6 in the debriefing survey. The exact wording of the questions can be found in the Online Appendix, Section A.14 and A.15. Workers who did not participate in the follow-up survey (debriefing) and the seven workers for whom we have no performance data are excluded. In columns 5 and 6, we report p-values of two-sided Mann-Whitney rank-sum tests for non-binary variables. For binary variables, we use a Chi-square test.

Table 4: *Workers' satisfaction with employer*

	Panel A: Working atmosphere		**Panel B: Wage**		**Panel C: Behavior towards workers**		**Panel D: Behavior towards co-workers**	
Specification	(1a)	(2a)	(1b)	(2b)	(1c)	(2c)	(1d)	(2d)
Constant	10.820***	12.298**	9.667***	12.479***	12.719***	13.810**	11.857***	12.165***
	(0.622)	(4.617)	(0.436)	(4.357)	(0.872)	(5.436)	(0.669)	(3.662)
No-layoff	0.529	2.490	0.697	0.926	-1.614	-3.324	-0.147	1.229
	(0.861)	(1.810)	(0.677)	(1.260)	(1.007)	(2.781)	(0.890)	(1.852)
Layoff	-0.057	0.218	0.994**	0.720	-0.786	-0.237	-1.464*	-1.744*
	(0.813)	(0.946)	(0.480)	(0.827)	(0.837)	(1.389)	(0.798)	(0.884)
R^2	0.001	0.087	0.009	0.097	0.007	0.104	0.010	0.131
Sample size	122	122	122	122	122	122	122	122
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Tobit regressions. The reported coefficients are marginal effects. The dependent variables are the workers' responses to the questions "How satisfied were you with the working atmosphere in the call-center?" (Panel A), "How satisfied were you with the salary?" (Panel B), "How satisfied were you with the management's behavior towards you?" (Panel C) and "How satisfied were you with the management's behavior towards your co-workers?" (Panel D) in the follow-up survey. Workers who did not participate in the follow-up survey are excluded. All independent variables are defined in the same way as in our baseline regression. Additional controls: (i) a dummy that is set to one if a worker was employed in other short-term employment relationships (zero otherwise); we asked this question in the follow-up survey; (ii) number of days between working in the second shift in the call-center and participating in the follow-up survey. Standard errors are clustered on the day-shift level. * p<0.1, ** p<0.05, *** p<0.01.

Table 5: *HR managers' predicted and actual mean number of phone calls in the second shift*

	Layoff	**Quasi-layoff**	**No-layoff**
Predictions	74.5 (20.3)	89.4 (14.6)	87.5 (9.9)
Real Data	74.6 (31.6)	82.2 (34.3)	80.9 (34.7)

Table 6: *Opinion of HR managers: reasons for lower productivity in Layoff treatment*

	Most likely reason (percent)	**Mean likelihood: each reason (scale: 0 (unlikely) - 10 (very likely))**
"The workers expected future negative consequences for themselves."	7.0%	3.3 (3.3)
"The workers perceived the survey as unimportant."	2.3%	3.1 (2.6)
"The layoff of co-workers was considered as unfair behavior of the employer."	76.7%	8.6 (2.2)
"The organizers of the phone survey were considered as being incompetent."	2.3%	4.7 (2.9)
"The workers thought that they were too productive in the first shift."	11.6%	4.6 (2.8)