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

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Abstract

We conduct a field experiment to study how workers' productivity is affected if employers act unfairly 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 percent. This result is not driven by altered beliefs about the job or the management's competence, but caused by 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 unfair employer behavior goes well beyond the potential tit-for-tat of directly affected workers.

JEL codes: Gift Exchange, Labor Markets, Fairness, 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 – including 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, in particular 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 a worker (as an incentive to work hard), but also by the worker's 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 the worker, but also to fair behavior of employers towards workers.

Akerlof's (1982) illustrative example is a group of cash posters. They receive a fixed wage and essentially do not have any career concerns. While some workers produce up to 46 percent 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. 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. A further consequence of the employer's behavior could be a change in identity within the organization (Akerlof and Kranton, 2000, 2005). By harming a subset of workers, the employer openly alienates himself from the whole group of workers, so that they see him as an outsider and act accordingly. Thus, even when workers are not directly affected by the employer's actions, they may reduce their performance when they see their colleagues being treated unfairly.

In this paper, we test this hypothesis. While a number of field experiments have examined the fair wage hypothesis by raising or cutting wages (Gneezy and List, 2006; Henning-Schmidt et al., 2010; Kube et al., 2012, 2013; Cohn et al., 2015; Gilchrist et al., 2015, DellaVigna et al., 2016; Esteves-Sorenson, 2018), there is so far no empirical work on the indirect effects of unfair employer behavior.[1] The question is what happens to a specific worker's productivity in a firm when the employer or a manager treats co-workers in a fair or

[1] Several studies document the adverse effects of conflicts between employers and employees on motivation and workplace productivity (e.g., Kleiner et al., 2002; Krueger and Mas, 2004; Mas, 2008; Montizaan et al., 2016). However, in these studies it is not clear whether workers react to the employer's behavior even if they are unaffected by his actions.

unfair way, while the specific worker is *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 are unchanged.[2] Such situations are common in daily business life. For instance, a worker may observe how a manager praises or criticizes a co-worker, or how a manager fires another worker. A priori, it is not clear what kind of consequences to expect in such situations. In case of observing someone being fired for unjustified reasons, a worker may not react at all to the employer's behavior, since he or she simply keeps the job under the same conditions. Alternatively, the worker may reduce productivity because of judging the employer's behavior as unfair, thus retaliating with less work effort. Yet, also the opposite reaction may occur when the worker who keeps the job increases productivity as a sign of gratitude for not having been fired. For HR management, the direction of this effect on workers matters a lot.

For research, the challenge is to confront workers with an unfair 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 any unfair behavior 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 conducted a field experiment (List and Rasul, 2011) in which we can cleanly measure the productivity response of unaffected workers when employers behave unfairly towards co-workers. To this end, 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 hourly wage, offering flexible work times, a pleasant work atmosphere, and full discretion over how to perform the job. The workers' task was to conduct interviews on refugee-integration and engagement in volunteering, which additionally creates the impression of a socially-oriented organization. This setting allowed us to implement in one treatment an unfair action that stands in stark contrast to this image, i.e., the random layoff of 20 percent of the workforce to cut costs. This unfair action can be executed while keeping the remaining workers' prospects (in the only remaining shift) constant. To rule out any career concerns, we made very explicit, in several instances, that there would be no future employment possibilities in the organization. Moreover, we paid the wage upon arrival to

[2] Of course, a worker still may feel affected when he or she could in principle have been the target of the employer's actions (Akerlof's gift exchange hypothesis does not rule out this possibility). We discuss this possibility in more depth below.

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. All of these measures are difficult or impossible to realize in ongoing organizations. The short-term nature of our organization is therefore an essential property and key asset of our field experiment.

To avoid confusion, our research question is not primarily about the consequences of layoffs for workplace productivity *per se* (which would require a long-term perspective). Our motivation for choosing layoffs as an intervention is to fabricate employer behavior that usually is seen as unfair (which is indeed the case in our setting) and that can conveniently be communicated to unaffected workers. Nevertheless, our field-experimental setting allows us to disentangle different behavioral motivations that could drive the "survivor syndrome," i.e., the layoff costs that are associated with a decrease in work morale and motivation of those who remain employed at the company.

In the experiment, we implemented three different treatments. In a standard control treatment, the staff remained unchanged for the second shift. In a second control treatment – called "Quasi-layoff" – we reduced our staff by 20 percent between shifts, but did not communicate the layoff to the remaining workers (we only informed them that fewer staff was present during the second shift). In the third treatment – called "Layoff" – we *randomly* dismissed 20 percent of the workers after the first shift and communicated the random layoff to the remaining workers as a measure to cut costs. As a follow-up questionnaire clearly revealed, this measure was seen as unfair both by the workers in the field experiment, but also by professional HR managers whom we recruited to gain more in-depth insights into what drives productivity changes in case of unfair employer behavior.

Our results show a very clear effect of layoffs that are perceived as unfair on the productivity of the remaining – and completely unaffected – workers. In our call-center, we have high-quality data on the number of calls, the time spent on the phone and the number of interviews completed. Compared to the control treatments, productivity in our Layoff treatment drops by a statistically and economically significant amount of 12 percent. We also find evidence that the quality of the workers' output decreases through the unfair layoff. Importantly, there is no significant difference in performance between the two control treatments (in both treatments, performance slightly decreases in the second shift). Thus, the reduction in staff size *per se* has no detectable effects on productivity. Further robustness checks reveal that our treatment differences are neither driven by peer-effects nor by altered beliefs about the importance of the job or the management's competence. Our robustness checks are not only based on a follow-up survey with workers in the field experiment, but also

on an additional experiment with 43 professional HR managers in medium-sized and large companies in Germany. In this experiment, we find strong 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 anticipate the detrimental effects of unfair behavior on the productivity of *unaffected* workers. Their predictions about the drop in performance in the Layoff treatment were remarkably accurate in the aggregate.[3] They do not anticipate the small drop in performance in the other two treatments. Hence, they even overestimate the magnitude of our main effect slightly.

In order to put our results into perspective, we compare the observed effect sizes with the effects that wage cuts or increases have had on the productivity of *affected* workers in previous studies (like Gneezy and List, 2006, or Kube et al., 2013). Our effect size is comparable with that in these studies, showing that unfair employer behavior can have similar effects on the performance of unaffected workers than direct wage cuts.

So far, only a few studies are available that investigate this type of gift exchange. Charness and Kuhn (2007) study in a laboratory experiment how worker productivity is affected by changes in their co-workers' wages, finding practically no externality of co-workers' wages. Cohn et al. (2014) conduct a field experiment in which workers perform a job in teams of two. In one treatment, they cut the fixed wage of one worker and leave the co-worker's wage unchanged. In their setting, the unaffected worker does not change his or her performance in response to the co-worker's wage cut. This may be due to the fact that the unaffected worker may not have perceived the co-worker's wage cut as unfair. Unfortunately, Cohn et al. (2014) do not have data on how the wage cut was perceived by the unaffected workers. In our field experiment, we can show that the layoff of co-workers is judged as unfair and that workers in the Layoff treatment reduce their productivity significantly, despite being unaffected by the layoffs. Our Quasi-layoff treatment shows that the reduction in staff itself is not considered as unfair and does not have any effect on productivity, a result that is similar to the one in Cohn et al. (2014).

A recent field experiment by Breza et al. (2018) examines productivity effects of pay inequality. They observe that pay inequality reduces output by a considerable amount when productivity is difficult to observe. In contrast, it has almost no effect when productivity is observable and pay inequality is well justified and understood by peers. The latter result indicates that workers have no problem with pay inequality per se, which is similar to the

[3] 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.

null-results of a pure staff reduction (as in our Quasi-layoff treatment). When output is more difficult to measure, relatively lower-paid workers reduce their output substantially in reaction to pay inequality. Yet, they are directly affected by being paid relatively less, so that this is different from our focus. Breza et al. (2018) observe that workers decrease their performance when they perceive pay inequality. 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. Obviously, this had direct effects on the higher-paid workers' happiness and work attitude, and it might have changed their beliefs about the employers' future behavior, making it impossible to measure the effect of employers' unfair behavior on *unaffected* workers. In our setting, the remaining workers did not have any contact with the unfairly treated (i.e., dismissed) co-workers in the second shift, as the laid-off workers were no longer present. This means that we provide a setting that avoids the potential confound of remaining workers being directly affected, and it tilts the odds against finding an effect of unfair employer behavior on the productivity of unaffected workers. This means that we can report causal evidence of a large drop in the productivity of unaffected workers in reaction to unfair employer behavior towards co-workers.

The rest of the paper is organized as follows. In Section 2, we explain the setup of the field experiment, the follow-up survey among participants, and the debriefing. Section 3 presents the results and their interpretation. In Section 4, we examine several alternative explanations for our results, using data from our follow-up survey, debriefing, and from a prediction experiment with professional HR managers. In Section 5, we discuss how our paper contributes to the previous literatures on field-experimental gift exchange and the survivor syndrome. Section 6 concludes and discusses implications for HR management. An extensive Online Appendix contains additional materials and robustness checks.

2. Experimental setup

2.1 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 conducts 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 were required to have an e-mail address, a mobile phone number, and they had to speak German fluently. They applied online by uploading their résumé (or by completing a short form), indicating possible working times, and consenting to the storage and processing of the generated data. We hired all 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. As wages for unskilled workers in German call-centers are usually rather low (Bosch and Weinkopf, 2008) and the hourly wage in our organization was one third above the German minimum wage, 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 offices). The other offices were rented-out by other organizations. Our workers had a 15-minutes time frame at 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 (a picture of a typical workplace is included in the Online Appendix). Each worker could have breaks whenever he/she wanted to (e.g., to go to the bathroom). There were no common breaks and no staffroom. 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

[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).

performance measures for each worker with respect to the total number of calls.[5] 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 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.

2.2 Experimental treatments in the call-center

We have 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 percent of the workers from the Layoff and Quasi-layoff treatments and asked them not to work for us during the second shift.[6] 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 and Quasi-Layoff treatment might have been concerned about the employer's reliability of actually paying the money after workers had learned about some co-workers' 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 following messages, shown in Figure 1, via e-mail (originally in German):

[5] 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.

[6] 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 the participation in a survey a few weeks after the field experiment (we therefore did not deceive our workers). They were unaware about 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 country-wide survey on refugee integration: more than 50 interviewers worked for us.	*We are happy that so many of you supported our country-wide 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 percent 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 following message, shown in Figure 2, conditional on the treatment, 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 treatments 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 for 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.[7] Moreover, the message communicates that the layoffs helped us to reduce costs, which was correct since we had to pay 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; the random determination of the layoffs indicated unfair behavior on the employer's side.[8] Indeed, a significant literature shows that laying off workers is an extremely unpopular measure (e.g., Datta et al., 2010; Drzensky and Heinz, 2016), and that they create significant costs for the laid-off workers (e.g., Black et al., 2015).

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).

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. 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 treatment to detect a 10-percent treatment effect at the 5-

[7] 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.

[8] Publicly listed firms that lay off large numbers of workers are often forced by regulation to inform the capital market at an early stage about the layoffs and the expected cost reductions (Lin and Yang, 2012). The focus on cost reductions in our layoff message resembles such an announcement.

percent significance level with a probability of 80 percent.

2.3 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 subjects' work satisfaction. We paid 15 Euros for their participation.[9] In total, 75.9 percent of the subjects who worked in both shifts participated in the survey.

Debriefing. After the completion of the online survey, we debriefed all workers (including those we had laid off), 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 when having learned about co-workers' layoffs, their perceptions of the management's competence, their beliefs about the importance of the job, and their social network. To get precise data on the social network in our workforce, we asked workers in an incentive-compatible way 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, 76.3 percent of the workers who were employed in both shifts responded to our questions in the debriefing. For more details on the follow-up survey and the debriefing, see the Online Appendix.

[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 took nine weeks.

[9] 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 Abbink et al.'s (2000) moonlighting game. 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. This suggests that our results are rather driven by identity (Akerlof and Kranton, 2000, 2005), than by reciprocity concerns. The detailed results can be obtained upon request from the authors.

3. Results

3.1 Descriptive statistics and randomization checks

In total, we had 52 workers in the No-layoff treatment;[10] 69 in the Quasi-layoff treatment, of whom 13 were laid off; and 74 in the Layoff treatment, of whom 13 were laid off.[11] 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 percent). In the following, we ignore the laid-off workers.[12] The selection into treatments was random.[13]

Table 1 summarizes for each treatment the surviving workers' characteristics and their performance in the first shift. Workers were between 16 and 65 years old (mean = 27.8, sd = 10.4), 62.1 percent were female, and 91.8 percent were born in Germany. The sample was quite heterogeneous in terms of education and employment status. Half of them were university students, and a quarter consisted of unemployed individuals. Around 50 percent indicated that they regularly worked in short-term jobs similar to the one in the call-center.

During the first shift, workers called on average 89.9 (existing) numbers (sd = 23.8). There are no economically or statistically significant differences in performance between treatments (see Panel A of Table 1). This is also true for personal characteristics (Panel B). Thus, our worker sample is balanced and randomization worked well.

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[10] This number includes seven workers for whom we lost the second-shift phone data (due to IT problems). All of them worked in the No-layoff treatment. 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 are unchanged; see the Online Appendix.

[11] We had to decide about the number of layoffs *before* we knew the number of workers who returned for the second shift. We had anticipated a slightly higher dropout rate than was actually realized, and hence laid off one or two fewer workers in the Layoff treatment than we should have to meet the 20-percent level.

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

[13] We had 8 x 4 days of data collection, with 4 days per week and three shifts per day. Two subsequent weeks (8 days) form a "block." We accepted only applicants that were available at least on two days (because of two shifts) in two different blocks, so that we could allocate them randomly to different treatments, independent 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 percent of workers were employed during the first two weeks, and the remaining 25 percent 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; see the discussion in Subsection 3.4). 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.

3.2 Main results

Our main research question is whether workers respond to the announcement of unfair layoffs by exerting less effort. Comparing the performance in the first and second shift, we find that the average number of calls decreases by around 8 percent in the No-layoff and Quasi-layoff treatments, and by 19 percent in the Layoff treatment (No-layoff versus Quasi-layoff treatment, t-test p-value = 0.972; Quasi-layoff versus Layoff treatment, t-test p-value = 0.031; No-layoff versus Layoff treatment, t-test p-value = 0.072; see also Table 1, Panel C).[14] The cumulative distributions (displayed in the Online Appendix) also show that performance drops significantly in all treatments. However, in the Layoff treatment, the reduction is much larger than in the No-layoff and Quasi-layoff treatment.

We also compare the performance reductions between treatments in several regression frameworks. First, we estimate the effect of the layoff announcement on the workers' performance in the following difference-in-difference specification:

$$y_i = \beta_0 + \beta_1 \, No\text{-}layoff_i + \beta_2 \, Layoff_i + \beta_3 \, t + \beta_4 \, No\text{-}layoff_i \times t + \beta_5 \, Layoff_i \times t + \varepsilon_i,$$

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 $Layoff_i$ and $No\text{-}layoff_i$ are dummies 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 subjects' age, gender, nationality, educational background (university degree, vocational training, etc.), and current employment status (regular job, student, unemployed). In the third specification, we additionally include dummies for each of the working days and dummies for the working time (morning, noon, afternoon; this specification has to be taken with some care, as the estimation includes more than 40 dummies). In the fourth specification, we omit the controls, and estimate a generalized difference-in-difference model to control for regression to the mean:

$$y_{i1} = \beta_0 + \beta_1 No\text{-}Layoff_i + \beta_2 Layoff_i + \beta_3 y_{i0} + \varepsilon_i.$$

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).[15] We use the Quasi-layoff treatment as our main control treatment since it

[14] All tests in this paper are two-sided. For our performance measures, we report t-tests, since data are roughly normally distributed. For all other variables, we report Mann-Whitney tests, since data are not normally distributed.

[15] The working environment in our call-center makes social interaction between workers in the same shift unlikely (see Section 2.1). Indeed, we find no evidence for significant social interaction within shifts (see Online Appendix, Section A.9). 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

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).[16]

[Insert Table 2 about here]

Table 2 shows the regression results. We observe three important patterns. First, in all treatments workers make fewer calls in the second shift than in the first shift. Second, the reduction is almost identical between the No-layoff and Quasi-layoff treatment (around 6 percent). Third, the reduction in performance is much stronger in the Layoff treatment. We estimate that the layoff announcement reduces the number of calls by around 12 percent compared to the other treatments. The magnitude of these effects is roughly the same in all specifications.

A number of robustness checks confirm our 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,[17] the gross working time, the total time spent on the phone, the total number of conducted interviews, and several self-reported performance measures. The results are presented in the Online Appendix. In all regressions, the announcement of unfair layoffs reduces performance by around 12 or more percent.

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

There are 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

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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.

[16] In the Online Appendix, we present all regressions for all outcome variables with the No-layoff treatment as the baseline. The main quantitative results are the same as in Table 2.

[17] 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.

phone 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 number).

If we re-run our baseline regression using gross working time as a dependent variable, we find that it is 12 percent smaller 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 to the other treatments (we get this result from a difference-in-difference comparison between treatments).

Second, we find a treatment difference 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 17.4 percent (sd = 10.0) and statistically the same in all treatments. In the second shift, this probability differs between treatments. It is 15.3 percent (sd = 9.3) in the Layoff treatment and 20.4 percent (sd = 12.8) in the Quasi-layoff treatment (Layoff versus Quasi-layoff treatment, t-test p-value = 0.017).[18] 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 treatment compared to the Quasi-layoff treatment; see the Online Appendix. Thus, we find 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 during this working time.

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

To study how workers perceived the employer's behavior as a potential explanation of our main result, 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).

Overall, workers were quite satisfied with the job at the call-center. The mean score

[18] The difference in the probability of completing an interview between No-layoff and Layoff treatment is not statistically significant.

for the first question was 9.1 points (sd = 1.6), and for the second question 9.1 points (sd = 1.3). There are no significant differences between treatments for these questions (all t-test p-values > 0.120). Also, the manager's behavior towards workers themselves was rated equally well in all treatments (mean = 9.3, sd = 1.6, all t-test p-values > 0.617). However, the management's behavior towards a worker's colleagues was evaluated differently across treatments: On a descriptive level, the mean score for the fourth question is 9.3 points (sd = 1.4) in the No-layoff treatment, 9.3 points (sd = 1.3) in the Quasi-layoff treatment, but only 8.7 points (sd = 2.3) in the Layoff treatment (t-test (MW-test) for comparing Layoff and Quasi-layoff treatment: p-value = 0.096 (0.135); these tests have to be taken with some care, as the variables are highly right-censored). When we regress the satisfaction score for the management's behavior towards co-workers in a Tobit regression on treatment dummies, we find that the score is around 10 percent lower in the Layoff- compared to the Quasi-layoff treatment (see Table 3). The effect is statistically significant at the 5- or 10-percent level, depending on the specification. This shows that workers in the Layoff treatment were significantly less satisfied with the management's behavior towards their co-workers than in the other treatments.[19]

[Insert Table 3 about here]

These are important results. Note that one behavioral motivation for the drop in performance could have been that the remaining workers felt treated unfairly by the management since in principle they also could have been laid-off (they were just lucky enough not to be picked for being laid off). However, according to our follow-up survey results, this does not seem to be the case. Moreover, these results show that the drop in satisfaction about the colleagues' treatment did not spread out to the workers' satisfaction with the wage, atmosphere in the call-center, or management's behavior to the worker herself.

Next, we examine which contents of the layoff announcement were seen as anti-social in the Layoff treatment. To find out, we asked workers in this treatment the following 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)?"

[19] 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 3, we control for this variation; our main results are unchanged, indicating that it is no concern for our results that workers might not recall how satisfied they were with the job in the call-center after several weeks.

The items corresponded to (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 anti-social.

Finally, we asked the subjects from the Quasi-layoff treatment in the debriefing what exactly they thought when they read the message about the 20 percent staff size reduction. Only a minority indicated that they thought of layoffs: 70 percent either did not remember the message or thought the message was not important or thought of "nothing"; 12 percent thought that some of their co-workers decided to quit voluntarily; 18 percent 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).

4. Examining alternative explanations

We propose that the perception of unfair behavior of the employer towards co-workers causes the productivity losses in the Layoff treatment. However, there exist a number of alternative behavioral motivations that could drive our results. In this section, we first use data from our follow-up survey and debriefing to provide evidence that alternative behavioral motives cannot explain our findings. Next, we consider the results from a prediction experiment with HR managers. They essentially corroborate our findings from the survey and debriefing. In the Online Appendix, we additionally show that spillover effects are unlikely to explain our main findings.

4.1 Evidence from follow-up survey and debriefing

Did workers expect future negative consequences after the announcement of the unfair layoffs? 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 were 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, 93.8 percent found our announcement credible in retrospect, and there were no significant differences between treatments (No-layoff 94.7 percent, Quasi-layoff 90.9 percent, Layoff 95.7 percent, t-test p-values > 0.353).

Moreover, no worker asked for further employment during our experiment.

Another concern could be that workers hoped for some recommendation from the management after the 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 unfair layoffs signal a lack of management competence? The announcement of unfair layoffs may have signaled to workers that the management was struggling with the organization of the project. This in turn may have lowered their work motivation. To evaluate this option, we requested 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 average ratings were quite high and consistent across treatments: 8.1 points (sd = 1.9) in the No-layoff treatment, 8.2 points (sd = 1.5) in the Quasi-layoff treatment, and 8.1 points (sd = 1.6) in the Layoff treatment (MW-test p-values > 0.617). Thus, the announcement of unfair layoffs 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 unfair layoffs signal a lack of task meaning? Another alternative explanation for our main result could be that the announcement of unfair layoffs lowered the workers' perception of the importance of the task for the employer. Several papers show that task meaning matters for work performance.[20] 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. We observe no statistically significant differences in the rating between treatments: The average rating is 7.9 points (sd = 1.7) in the No-layoff treatment, 7.3 points (sd = 1.9) in the Quasi-layoff treatment, and 7.2 points (sd = 2.1) in the Layoff treatment (MW-test p-values > 0.145). Thus, varying perceptions of the importance of the task cannot explain our main result either.[21]

[20] See Ariely et al. (2008), Grant (2008), Chandler and Kapelner (2013), Chadi et al. (2017), and Kosfeld et al. (2017).

[21] The difference between the ratings in the No-layoff and Layoff treatment is borderline insignificant. However, since there is no significant difference between the ratings in the Quasi-layoff and Layoff

Did the announcement of unfair layoffs signal that workers were too productive in the first shift? Another potential side-effect of the announcement of unfair layoffs 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 similar in all treatments and very low, indicating fairly strong disagreement with the statement: 2.1 points (sd = 2.4) in the No-layoff treatment, 2.0 points (sd = 1.8) in the Quasi-layoff treatment, and 2.2 points (sd = 2.2) in the Layoff-treatment (MW-test p-values > 0.616). 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 percent 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 harvest workers). This suggests that our workplace setup is not uncommon for many of them. 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 – 89.4 percent of our workers – report that they did not think that they were participating in an experiment. Importantly, this number does not vary significantly between treatments (No-layoff 92.1 percent, Quasi-layoff 86.4 percent, Layoff 89.4 percent, t-test p-values > 0.410). Thus, the layoff announcement did not alter our workers' perception of the credibility of the experimental setting. Moreover, we asked this question after we told subjects about the experiment. Thus, the remaining ten percent of workers may just fall prey to the hindsight bias.

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treatment, a lack of task meaning cannot explain our results.

4.2 Survey and experiment with professional HR managers

Human resources managers are primarily 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.

In order 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 online in a survey and an experiment.[22]

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 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. For that reason, we showed them the exact wording of the messages to workers before the second shift (as shown in Figure 2 in Section 2.2). We presented a neutral framing for the three treatments ("Group 1, 2, and 3") instead of 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 4 and Table 5 about here]

After HR managers had entered their predictions for the average performance in the second shift, we revealed the true averages (here shown in the bottom row of Table 4) and then presented them with a list of five potential reasons for the lower performance in the

[22] Heinz and Schumacher (2017) use the same subject pool. In their study, HR managers correctly predict experimental subjects' relative differences in social behavior based on résumé content.

Layoff treatment (see Table 5). First, we asked them which of those reasons they considered as the most likely reason for the significant drop in productivity in the Layoff treatment, and then we let them rate the likelihood with which each reason was responsible for the decline in productivity.

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

Table 4 shows the HR managers' predictions. For the No-layoff and the Quasi-layoff treatments they, on average, expected a slight drop, although their 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 ($p > 0.190$ in both cases). The managers' predictions for the Layoff treatment are remarkably different, as 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 ($p = 0.986$). Importantly, the managers' predictions for the Layoff treatment (labelled Group 3 in their wording) was significantly lower than their predictions for the other two treatments (two-sided t-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 that could occur from the announcement of unfair layoffs. Taking into account the drop in performance between the first and second shift of the No-layoff and Quasi-layoff treatment, the HR managers slightly overestimated the negative effect of the layoff message on productivity. The average predicted difference-in-difference effect (Layoff versus Quasi-layoff treatment) is -16.5 percent.

Figure 3 illustrates the distribution of the predicted difference-in-difference effect. The vertical line indicates the average difference-in-difference effects in our real data (-12 percent). We find that around 60 percent of the HR managers over- and 40 percent underestimated the drop in performance.

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

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

Layoff treatment had reduced their performance. Table 5 shows the managers' responses. Looking at the first column, we note that 76.7 percent 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 5 were much less often regarded as the most important reason for the productivity decline in the Layoff treatment. The second-most frequently selected option – with 11.6 percent only – was that workers had reduced their efforts because they had thought that they had been too productive in the first shift. While this is a legitimate conjecture, we had not found evidence in the follow-up survey that workers felt that way (see Section 3.3).

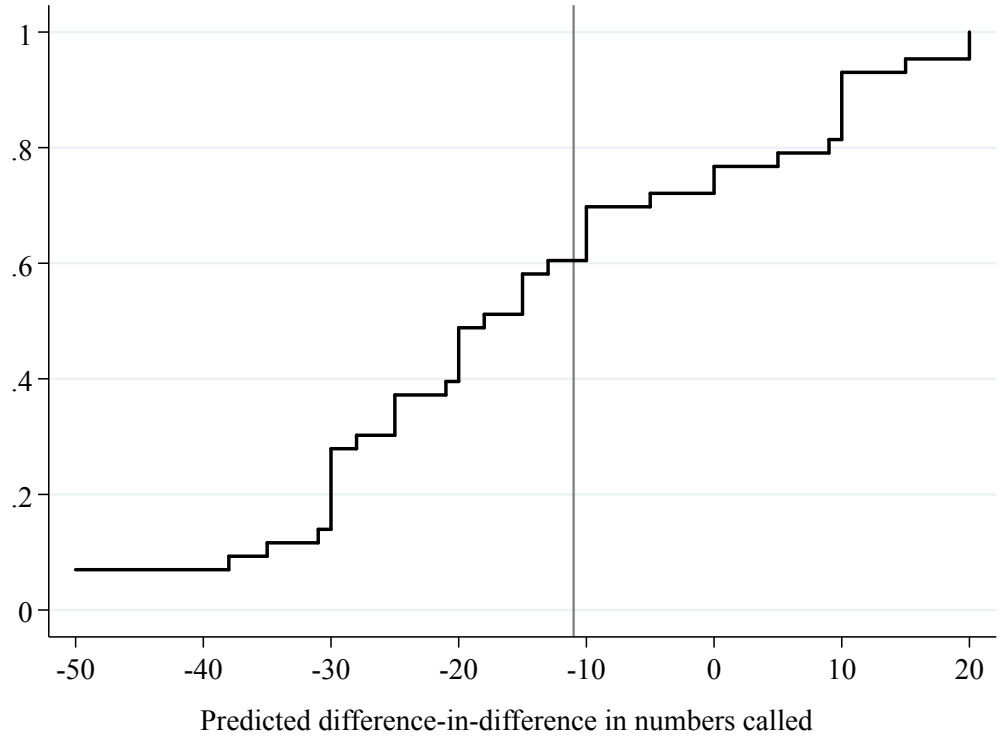

Figure 3: *Cumulative distribution of the predicted difference-in-difference in numbers called (Layoff vs. Quasi-layoff treatment). Predictions by HR managers.*

The second column of Table 5 confirms the insight from the first column. HR managers rated the unfair behavior of employers by far as the factor that is most likely the reason 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 (p-values < 0.001 in all cases). This confirms the importance that HR managers attach to unfair behavior of employers being the main driver of productivity shocks in our field experiment.

5. Relation to the literature

5.1 Gift exchange

As indicated in the introduction, there are a number of field-experimental studies that examine Akerlof's (1982) fair wage hypothesis by varying the fixed-wage. In contrast, we kept wages fixed, but varied the context of the organization (fair versus unfair employer behavior). In the following, we put the effect size in our experiment into perspective by comparing it to what is known from the previous gift exchange literature. This comparison has to be taken with some caution since the production functions potentially differ between the various experimental settings. Moreover, the impact of unfair employer behavior on workplace productivity may be smaller in a setting in which the organization has a low reputation for fairness.

[Insert Table 6 about here]

Table 6 provides an overview of how worker productivity has changed in various previous studies in reaction to a variation in their own fixed wage. To compare the effect sizes across the different studies, we calculate for each study the wage elasticity of performance, which is

$$\frac{\text{percent change in output}}{\text{percent change in total wage bill}}.$$

For our field experiment, we define this elasticity as

$$\frac{\text{percent change in output of remaining workers}}{\text{percent change in total wage bill}}.$$

From Table 6 we note that the wage elasticity of performance, if workers experience a fixed wage increase, ranges from 0.02 to 0.72 (the highest number stems from the first three hours of Gneezy and List's 2006 Study 2). Fixed wage cuts show an elasticity between 0.04 and 0.60 (the highest number is from Kube et al., 2013). In our case, we observe an elasticity of 0.60. Thus, our effect size is at least comparable to what one finds when wages are directly affected. Even when wages and job prospects are unchanged, workers react sharply to an employer's unfair behavior towards co-workers. Therefore, the effect of unfair employer behavior on the productivity of workers is substantial, and it should not be overlooked by employers.

5.2 Survivor Syndrome

We used layoffs as a means to implement unfair employer behavior in our organization. To some extent, our project is therefore related to the psychological literature on the survivor

syndrome. This phenomenon describes layoff costs that are associated with a decrease in work morale and motivation of those who remain employed at the company (for an overview, see Datta et al., 2010, and Van Dierendonck and Jacobs, 2012). Surveys with workers who experienced layoffs in their companies 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).

There exist a few laboratory experiments on the survivor syndrome. In these studies, subject groups perform a real-effort task for two periods. Between periods, experimenters force one group member to leave the group because of "room scheduling problems." These studies find mixed results: Brockner et al. (1985) and Brockner et al. (1986) find an increase whereas Brockner et al. (1987) find a decrease in second-period performance. Drzensky and Heinz (2016) study the decision of principals to lay off agents endogenously. They find a large drop in subjects' performance as a response to a principal's layoff decision. Around half of the subjects whose co-worker was fired respond to the layoff and decrease their performance substantially, while the other half of the subjects do not react at all.

The advantage of our study relative to the survey literature is that we have objective performance data and that our experimental setting ensures that no factor besides the layoff message influences the workplace performance (in a real-world organization that conducts layoffs, other human resource policies may change as well). The advantage relative to the lab experiments is that our workers are unaware that they are part of an experiment so that we avoid experimenter demand effects. Moreover, workers in our setup were employed in a real employment relationship, and our follow-up studies allow us to disentangle various alternative explanations of the survivor syndrome in our setting.

6. Conclusion

In this paper, we measured how an employer's unfair 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 for unfair behavior. It is known that unfair wage cuts affect the productivity of *affected* workers in a negative way (see, e.g., Kube et al, 2013). Yet, it is much less clear whether and to what extent unfair behavior backfires on employers, in the sense that it also changes the behavior of *unaffected* workers. Measuring this latter aspect has been a major challenge because it is so difficult to test in the field. In a long-lived organization, any type of unfair behavior of employers towards co-workers will almost necessarily also change

the unaffected workers' career prospects and beliefs about the employer's future behavior. As a consequence, any behavioral change of unaffected workers may be due to the expectation of getting affected in the future. The impact on unaffected workers that we are looking for can therefore only be cleanly measured if it is possible to credibly rule out future interaction. Our approach of founding a short-lived organization – by running a call-center where 195 workers had to conduct telephone interviews in two shifts – satisfies this condition to identify the effect of unfair employer behavior on unaffected workers.

Of course, one might criticize the absence of a longer-term perspective in our short-lived organization as an unrealistic feature in labor markets, thereby questioning the external validity of our setting. Yet, data from Germany show that this presumed limitation is much less severe at closer inspection. In 2016, 45 percent of new contracts were issued as fixed term contracts (http://www.zeit.de/wirtschaft/2017-09/arbeitsvertraege-befristet-neue-mitarbeiter-unternehmen), often with a maturity of less than a year. While certainly a significant share of these fixed term contracts may be converted into permanent ones, these figures indicate that also in reality many work contracts approach their end very frequently, and it is exactly in this situation when unfair behavior of employers towards co-workers might influence unaffected workers with fixed term contracts to a large degree. Hence, we believe that taking into account these effects is very important for the management of companies.

In our experiment, the announcement of unfair layoffs triggered a reduction in the performance of unaffected workers of about 12 percent, compared to both a treatment without any layoffs and an additional control treatment where workers were informed about an equivalent reduction in staff size, without alluding to layoffs, to the attempt to save costs, or to the random determination of workers to be fired. The latter Quasi-layoff treatment shows that it is not the reduction in staff that causes the negative productivity shocks. In our Layoff treatment, we observed that gross working time and the quality of workers' output decreased and that workers were less satisfied with how management treated their co-workers, as a follow-up survey revealed.

Through the design of the workplace and the results from the follow-up survey we can rule out that the significant 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. Rather, the unfair nature of the employer's behavior towards co-workers was essential to understanding the negative productivity shock.

A prediction experiment and a survey among 43 professional HR managers confirmed that the random firing of co-workers to save costs was seen as unfair and as the most likely

reason for the strong decline in the productivity of unaffected workers. The HR managers predicted the magnitude of the productivity decline in the aggregate very well, showing that they had a sense for the potential consequences of an employer's unfair 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. This may explain why layoffs – which are often seen as unfair – are usually circumvented by HR managers by relying on the natural fluctuation of the workforce to reduce the number of workers or by shifting the blame, for instance by blaming a previous CEO for the unfair action (i.e., the layoff).[24]

[24] 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 the natural fluctuation of the workforce and by adjusting the amount of overtime hours or temporary workers rather than firing permanent workers.

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Table 1: Randomization Check for Worker Characteristics

	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 (25.2)	91.8 (24.2)	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; existing and non-existing)	243.2 (66.9)	238.7 (68.1)	230.4 (57.1)	0.718	0.283
Amount of dialed numbers (self-reported; only existing numbers)	104.9 (28.8)	106.2 (29.0)	104.8 (26.7)	0.807	0.984
Number of interviews (self-reported)	5.1 (2.6)	5.3 (2.0)	4.8 (2.5)	0.727	0.510
Number of interviews (self-reported; call-time > one min)	4.8 (2.6)	5.0 (2.2)	4.6 (2.2)	0.626	0.618
Completion of an interview (in %)	17.6 (11.2)	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)	29.9 (13.0)	27.0 (8.9)	0.296	0.855
Female (in %)	63.9	64.3	56.8	0.968	0.451
Nationality: German (in %)	91.8	91.1	90.1	0.888	0.872
Student (in %)	57.4	50.0	50.0	0.424	0.454
University degree (in %)	14.8	14.3	6.8	0.943	0.207
Unemployed (in %)	21.3	14.3	34.1	0.323	0.144
Vocational training (in %)	24.6	21.4	18.2	0.685	0.433
Full- or part-time job (in %)	39.3	42.9	29.5	0.700	0.300
Previous employment similar to call-center job (in %)	30.4	27.5	23.5	0.741	0.483
Regular employment on a short-term basis (in %)	55.3	55.3	42.8	1.000	0.296
Working day: Monday - Wednesday (in %)	70.5	62.5	40.9	0.360	0.002
Working day: Thursday - Saturday (in%)	29.5	37.5	59.1	0.360	0.002
Time of the day: Morning (in %)	24.6	35.7	38.6	0.189	0.123
Time of the day: Noon-afternoon (in %)	36.1	32.1	29.5	0.655	0.484
Time of the day: Afternoon (in %)	39.3	32.1	31.8	0.417	0.428
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; existing and non-existing)	-38.0 (81.4)	-3.2 (56.5)	4.0 (77.6)	0.009	0.006
Amount of dialed numbers (self-reported; only existing numbers)	-18.8 (34.4)	-4.7 (32.4)	2.6 (50.5)	0.025	0.009
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)	-1.3 (2.8)	-0.7 (2.3)	-0.5 (2.9)	0.192	0.181
Completion of an interview (in %)	-0.02 (0.13)	0.02 (0.14)	-0.01 (0.12)	0.059	0.458

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- or part-time job at the time of the field experiment (we did not count mini jobs as full- 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 Saarbruecken 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 ranksum 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***	-30.140	18.564**
	(3.595)	(20.407)	(31.946)	(9.093)
t	-6.768**	-6.768*	-10.134	
	(3.407)	(3.502)	(7.486)	
No-layoff	-1.506	-2.378	17.314	-0.217
	(4.854)	(5.236)	(11.726)	(6.014)
No-layoff × t	0.212	0.212	-4.602	
	(6.184)	(6.358)	(13.450)	
Layoff	3.530	2.105	6.324	-10.079**
	(4.568)	(4.574)	(8.592)	(5.024)
Layoff × t	-11.852**	-11.085**	-18.702**	
	(5.370)	(5.428)	(7.941)	
Numbers called s_1				0.719***
				(0.099)
R^2	0.044	0.108	0.251	0.270
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 percent 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*, *German*, *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; zero otherwise) and *Time of day* (three dummies – one for the morning (8 am to 12 am), the noon (12 am to 4 pm), and evening (4 pm to 8 pm)). 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 has worked in the morning. * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Workers' Satisfaction with Employer

	Panel A: Working athmosphere		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***	12.150**	9.667***	11.737***	12.719***	13.295**	11.857***	10.704**
	(0.622)	(5.007)	(0.436)	(4.617)	(0.872)	(5.789)	(0.669)	(4.106)
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 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. 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) day and time of when the subject worked in the call-center (second shift); (iii) number of days between working in the second shift in the call-center and participating in the follow-up survey. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 4: 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 5: 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 (2.2)
"The workers perceived the survey as unimportant."	2.3%	3.1 (1.5)
"The layoff of co-workers was considered as unfair behavior of the employer."	76.7%	8.6 (1.2)
"The organizers of the phone survey were considered as being incompetent."	2.3%	4.7 (1.9)
"The workers thought that they were too productive in the first shift."	11.6%	4.6 (1.8)

Table 6: Effects of gift exchange on workplace performance

Study	Intervention	Task	Output Change	Elasticity
Gneezy and List (2006)[1]	Pay raise USD 12 – USD 20	Library Book Coding	27%	0.41
Gneezy and List (2006)[2]	Pay raise USD 10 – USD 20	Fundraising	72%	0.72
Kube et al. (2012)	Pay raise EUR 36 – EUR 43	Library Book Coding	5%	0.26
Kube et al. (2013)	Pay cut EUR 15 – EUR 10	Library Book Coding	-20%	0.60
Cohn et al. (2014)	Pay cut EUR 12 – EUR 9	Selling Promotion Cards	-15%	0.60
Cohn et al. (2015)	Pay raise CHF 22 – CHF 27	Newspaper Distribution	3%	0.13
Gilchrist et al. (2016)	Pay raise USD 3 – USD 4	Online Data Entry	18%	0.54
Esteves-Sorenson (2018)	Pay raise USD 12 – USD 20	Data Entry	2%	0.03[3]
DellaVigna et al. (2016)	Pay raise USD 7 – USD 14	Folding Envelopes	2%	0.02[3]
DellaVigna et al. (2016)	Pay cut USD 7 – USD 3	Folding Envelopes	-2%	0.04[3]
This Paper	**Layoffs**	**Phone Survey**	**-12%**	**0.60**[4]

[1] Study 1, first 90 minutes; [2] Study 2, first 3 hours; [3] effect is not statistically significant; [4] this number is the percent change in output of the remaining workers over the percent change in total wages.