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The Hidden Costs of Downsizing

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The Hidden Costs of Downsizing

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Abstract:

We analyze whether a principal's *decision* to lay off an agent affects the performance of the surviving agents in a laboratory experiment. We find that agents reduce their performance by 37% as a response to the layoff decision. Heterogeneity in principals' decisions can largely be explained by different beliefs about how agents react to layoffs.

Keywords: Layoffs, survivor syndrome, behavioral economics

JEL Codes: J63, C91

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1. Introduction

Layoffs are an integral part of modern economies. The U.S. Bureau of Labor Statistics (2012) states that each year firms reported on average around 6,700 mass layoffs resulting in 1.2 million separated workers.² As layoffs impose costs on the displaced workers³, the regional economy and social insurances, it is no surprise that layoffs receive a lot of attention by practitioners, scholars and the general public (see e.g. Baron and Kreps, 1999; Baumol et al., 2003). The heated debate during the 2012 U.S. presidential campaign regarding Mitt Romney's previous involvement in mass layoffs is a case in point.⁴

From the firm's perspective, the benefits of layoffs, such as the reduction of labor costs or organizational slack, seem to be obvious. Firms considering layoffs have to weigh these benefits with potential costs. Some types of costs, such as severance payments, are ex ante more or less calculable. Other costs are hard to estimate in advance. In particular, the psychological literature suggests that there may be substantial costs associated with a decrease in the motivation of those who stay in firms after layoffs – a phenomenon called “survivor syndrome” (for an overview, see Datta et al. (2010) and van Dierendonck and Jacobs (2012)). Following downsizing, workers self-report lower effort, performance (Travaglione and Cross, 2006) and commitment (Allen et al., 2001), whereas turnover intentions (Allen et al., 2001) and absenteeism⁵ (Travaglione and Cross, 2006) increase.

There could be several explanations for survivor syndrome, e.g. changing job designs or the destruction of social networks. Our study is based on the idea that survivor syndrome results from the social interaction between principal and agents. We argue that agents dislike principals who downsize. Specifically, we investigate whether survivors reduce their performance as a response to a principal's *decision* to lay off. In our experiment, we find that survivors substantially decrease their performance if their principal decides to fire a co-worker. Heterogeneity in principals' decisions can largely be explained by different beliefs about how agents react to layoffs.

We choose the experimental approach as we focus on one potential channel for survivors' behavior, namely the principal's decision. By creating two identical layoff settings – either with or without a principal's decision – we study the impact of the decision in isolation.

In our experiment, three agents work for one principal whose payoff depends on the agents' performance in a real-effort task. Subsequently, we provide the principal with an incentive to fire one of the agents. After her decision the remaining agents work for the principal again. To investigate

² The U.S. Bureau of Labor Statistics defines a mass layoff event as an event where at least 50 workers separate for more than 30 days. The data refers to the years 2003 – 2011.

³ For example, displaced workers experience a significant increase in mortality (Browning and Heinesen, 2012; Eliason and Storrie, 2009; Sullivan and von Wachter, 2009) and divorce rates (Charles and Stephens, 2004). Rege et al. (2011) find a decrease in children's school performance; Huttunen et al. (2011), Jacobson et al. (1993) and Stevens (1997) a decrease in future earnings.

⁴ *The Economist* (3/2013, p.11/12) argues that “In last year's American presidential election Mitt Romney never recovered from early digs at this record of outsourcing and downsizing at companies owned by a private equity firm he helped found.”

⁵ Absenteeism is measured objectively.

whether the behavior of the survivors is driven by the principal's layoff decision or its implementation, we conduct a control treatment wherein it is randomly decided whether an agent is fired or not.⁶

Our results show that survivors reduce their performance as a response to the principal's decision to lay off a co-worker. The size of the effect is economically significant (37%). The decrease is strongest for survivors who characterize their principal as selfish, weaker if they characterize her more neutrally, and vanishes in the control treatment where the layoff occurs exogenously. The results provide causal evidence that survivors indeed decrease their performance as a response to the principal's voluntary decision. Based on "signaling of type" models (Ellingsen and Johannesson, 2008; Levine, 1998), we suggest that agents perceive a principal's decision to lay off as a signal that she does not expect them to perform well or cares more about her own income than the well-being of the agents. This signal might lead to a decrease in agents' performance. Agents' behavior in our experiment is strongly in line with field studies finding lower self-reported effort and self-reported performance after downsizing (see e.g. Travaglione and Cross, 2006). With the principal's signaling we provide one explanation for survivor syndrome.

Interestingly, the heterogeneity in principals' decisions can largely be explained by different beliefs about how agents respond to their decision. Thus, given their expectations, principals mainly choose a profit-maximizing strategy.

Our results imply that firms laying off workers should be careful about the signal they send. In Chapter 5 we discuss how attempts to manage the signal could provide an explanation for several real life phenomena, such as higher CEO turnover after layoffs or implementation of costly social plans.

Related to our study, Cohn et al. (2011) investigate the impact of wage cuts on workers' performance in a field experiment. Similar to our experiment, workers perform a task for two periods for a fixed wage. In the second period, randomly chosen workers receive a wage cut – framed as a manager's decision. Cohn et al. (2011) find that affected workers decrease their performance. Interestingly, unaffected workers do not respond to a co-worker's wage cut. Even if one has to be careful when comparing results from different settings, the findings in Cohn et al.'s (2011) and in our experiment suggest that, for co-workers' behavioral response, wage cuts are fundamentally different from layoffs.

Our paper is also related to the experimental literature on third-party punishment. In the basic version of these experiments two parties play a social dilemma game, e.g. a trust- (Charness et al., 2008) or dictator-game (Fehr and Fischbacher, 2004). The main finding is that individuals whose material payoff is unaffected by the other players' actions nevertheless punish the perceived unfair behavior of others at own expenses. In our experiment, principals also exposed survivors to the risk of being fired. Thus, survivors do not act as a third party if they perceive the principal's decisions to fire a co-worker as an unkind act against themselves even if it was not directed towards them.

⁶ In their interview study, Charness and Levine (2000) argue that individuals' acceptance of layoffs depend on whether the reasons for layoffs are endogenous or exogenous.

To the best of our knowledge, we are the first to analyze how principals' *decisions* to fire an agent affect the behavior of survivors. The only studies we are aware of that investigate the causal impact of a co-worker's layoff on agents' performance are experiments by psychologists (Brockner et al., 1985; Brockner et al., 1986; Brockner et al., 1987). Here, groups of participants perform a real-effort task for two periods. Between the periods, the experimenters forced one individual to leave because of "room scheduling problems". The results are mixed: Brockner et al. (1985) and Brockner et al. (1986) find an increase whereas Brockner et al. (1987) find a decrease in second-period performance. In contrast to these studies we endogenize the decision to lay off in a principal-agent game.

2. Experimental Design

In our experiment, we implemented two treatments. Each treatment consists of two periods. The first period is the same in both treatments, while the second period differs.

First Period

After having randomly assigned all subjects either the role of a principal (located in room A) or that of an agent (located in room B), all subjects received the instructions for the first period of the experiment.⁷ They were informed that there will be a second part, for which they will receive the instructions later on. We also told them that the first part (i.e. period) of the experiment is independent from the second one, i.e. decisions of one party in the first period do not influence decisions of other parties in period two. All principals and agents received the same full information about the procedure for the first period of the experiment.

Each agent was informed that she and two other anonymous agents were matched with one principal. For the subsequent ten minutes, the agents performed a real-effort task, namely the "slider task", designed by Gill and Prowse (2012). In this task, agents face a computer screen that contains 48 sliders. Each slider can be pushed with the mouse⁸ on a small bar between 0 and 100. In the beginning, all sliders are located at 0. As in Gill and Prowse (2012), an agent's performance is measured by the number of sliders positioned exactly at 50 during the time span. Every two minutes a new set of sliders appeared on the screen. Agents were not forced to perform the slider task. In the instructions we informed them how to switch from the slider-screen to the internet browser.

All agents received a fixed wage of 8€ in the first part of the experiment, independent of their performance on the real-effort task. Thus, agents had no monetary incentives to work. The payoff to the principal was determined by her agents' performance. She received 0.03€ for each slider that one of her three agents positioned at 50 during the ten minutes.

⁷ In the instructions we applied a natural framing using expressions such as "employer" and "employee".

⁸ We covered the arrow keys of the keyboards with cardboard so that they could not be used.

Second Period

At the start of the second period, all principals and agents received new instructions. Subjects were informed that they were matched with the same principal/agents as in the previous period. In our main treatment, each principal had the choice to either lay off an agent and receive a fixed amount of 5€, or to keep all three agents.¹⁰ In reality, firms who decide for layoffs may try to increase profits by reducing costs. By using a fixed amount of money, we provided principals with an incentive to lay off.

If a principal decided against a layoff, we informed the three agents of their principal's decision, and the experimental procedure in the second period was precisely the same as in the first one. We conducted the real-effort task again for ten minutes. Each agent received a fixed wage of 8€ while the principal received 0.03€ for each slider that one of her three agents positioned at 50 during the ten minutes.

If a principal decided to lay off an agent, we randomly selected one agent by rolling a die in front of the agents. All three agents were informed about their principal's decision and whether she or one of her co-workers was fired. Afterwards, we conducted the real-effort task again for ten minutes. Only the two remaining agents (called "survivors" in the following) received the fixed wage of 8€. The principal obtained 0.03€ for each slider that the *two* survivors positioned at 50 in the following ten minutes plus a fixed amount of 5€. The dismissed agent had to stay in her place, but received no compensation in period two. Her second period performance had no monetary consequences, neither for her principal nor for herself.

To test whether it is really the principal's voluntary decision that affects agents' motivation, and not the fact that a co-worker is fired *per se*, or a general decline in performance in the second period, we conducted a control treatment. Here the experimental procedure was the same as in our main treatment, except that we randomly decided (by drawing lots) for each principal whether an agent was fired or not.¹¹ Our basic empirical strategy was to compare the first and second period performance *within* agents *across* our two treatments given firing or no firing.

In our experiment, the selection of *who* is fired was always random. We consciously decided against the use of performance criteria for two reasons.

First, in practice, there are a variety of constraints that restrict the flexibility of firms to fire employees based on their performance. In particular, in most countries a number of legal constraints protect particular groups of workers. For example, it is far more difficult (and more expensive because

⁹ Our goal was to calibrate the fixed amount in a way that both the decisions for and against firing were made by a substantial number of principals. For this purpose, we presented undergraduate students in a seminar the basic design of our experiment and ask them at which amount of money they would fire an agent. Most of the students stated to be indifferent at an amount of 5€

¹⁰ In the instructions, we used the German terms "Stellenabbau" (i.e. job reduction) and "Stellen streichen" (reduce jobs) which a media analysis (see Friebe and Heinz, 2012) revealed to be the most frequently used expressions to describe job shedding.

¹¹ For the purpose of comparability, we calibrated the number of firings in the control treatment to that of the main treatment.

of higher severance payments) to dismiss older or more tenured workers (see e.g. Blanchard and Tirole (2003) for the case of France). Also unions put pressure on firms to base their selection on social criteria or on union-membership (Baumol et al., 2003; Goerke and Pannenberg, 2011).¹² Even in the absence of legal restrictions, firms may be constrained because individual performance is hard to measure (e.g. in the case of teamwork). In the case of a plant closure, firms generally dismiss most or all of the workers in the affected establishments.¹³

Second, in our experimental design, a random selection of agents who are fired is the only method that does not provide the survivors with any information about the dismissed agents' characteristics.¹⁴ If the selection is based on criteria, it will be hard to distinguish whether survivors respond to the principal's decision or to that information. In particular, firing the least productive agents would provide the survivors a signal about their relative performance, which alone could influence the survivors' second period performance.

In both treatments, all subjects were fully informed about the complete procedure of the experiment. However, we did not inform principals about the first period performance of their three agents. To provide principals with a benchmark for the size of the fixed amount for firing relative to an agent's second period performance, we conducted two preliminary experimental sessions. Here, the first period was the same as in our main experiment. The second period was a simple repetition of the first one, without any reference to layoffs. In our main experiment, principals were informed about the minima, maxima and mean performance in these sessions while agents were only informed about their existence.

In the first period of the preliminary sessions the mean performance of the 36 participating agents amounted to 78.11 (standard deviation: 40.37). Thus, each agent generated on average 2.34€ for her principal. The three "least productive" agents generated 0€, 0.03€ and 0.09€, the three "most productive" agents generated 3.60€, 3.99€ and 4.89€. In period two, the mean performance was 79.56 (sd: 44.13), which converts to 2.39€ per agent. The three "least productive" agents generated each 0€ while the two "most productive" agents generated 3.66€ and the third one 4.89€. We find no significant differences between the first and second period performance (Wilcoxon matched-pair signed rank test, p-value: 0.450). Thus, we find no dominance of learning or exhaustion effects.

¹² Using data from the German Socio-Economic Panel, Goerke and Pannenberg (2011) find that in Western Germany union members are less likely to lose their jobs than non-members. The authors find no effect of union-membership on dismissals in cases of plant closure.

¹³ Empirical evidence on which workers separate in the case of downsizing is sparse. One exception is Dohmen and Pfann (2004) who study separation rates in one particular firm in a downsizing context. They find that low-performing blue-collar workers leave first. The effect does not, however, exist for white-collar workers. Note that the authors do not distinguish between voluntary turnover and dismissals.

¹⁴ The only alternative is to withhold the selection criterion. However, in this case subjects might ask the experimenter and/or form different beliefs about the selection procedure. This would result in a loss of control.

Taken together, principals in our main experiment knew that an agent is highly unlikely to generate more than 5€ in the second period – the amount which the principals receive for firing. Consequently, a principal should expect layoffs to pay – except she anticipates agents to respond to her decision.

Procedural Details

The experiment was conducted at the FLEX laboratory at Goethe University in Frankfurt. Subjects were undergraduate students of different disciplines of the university, recruited via ORSEE (Greiner, 2004). The experiment was programmed using z-Tree (Fischbacher, 2007) and an adapted version of the code from Gill and Prowse (2012). We asked several control questions in all treatments to ensure that all subjects understood the experimental design. In total 208 subjects participated in the experiment, a quarter as principals and three quarters as agents. 80 subjects participated in the main, 80 in the control treatment and 48 in the preliminary sessions. A session lasted approximately 75 minutes. On average principals earned 14.02€, agents 14.77€

3. Behavioral Predictions

The behavioral predictions depend on the assumptions about the subjects' preferences. We first consider the standard assumption of selfish individuals who maximize their own payoff. Second, we discuss predictions based on other-regarding preferences (for an overview of the literature, see Fehr and Schmidt (2006)).

The predictions under standard assumptions of selfish individuals are straightforward. As the agents receive a fixed wage they should not position any slider as long as they are not intrinsically motivated. Thus, independent of the decision for or against firing, we should not find any differences within and across treatments. As principles in our main treatment receive a fixed amount of 5€ for firing an agent, risk-neutral principals should always do so, unless they expect the productivity of the dismissed agents to be at least 5€. However, agents' behavior in the preliminary session already indicates that agents are motivated to position sliders, which can either be explained by intrinsic motivation or other-regarding preferences.

If agents dislike inequity (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), they should arrange sliders in both periods to increase the payoff to the principal, who receives no fixed endowment at the beginning of the experiment. However, as inequity theories only account for the allocation of goods, not for intentions, we should find no differences between our main and control treatment. Principals in our main treatment should decide against firing the more they are motivated by inequity aversion.

The distinction between endogenous and exogenous decisions becomes important if one assumes that an individual's behavior depends on their belief about their opponent's type. Ellingsen and Johannesson (2008) argue that individuals care about social esteem, and the utility of social

esteem depends on whether an audience is considered to be worth impressing. We follow the idea that agents in our experiment interpret their principal's decision as a signal of their principal's type. Particularly, agents might believe that the principal does not expect them to perform well or that she cares more about her own income than the well-being of the agents. Thus, we assume that a principal's decision to fire affects the agent's esteem for the principal in a negative way. According to these arguments, we should observe lower survivors' performance in the main compared to the control treatment. The opposite may hold if the principal decides against firing. For Levine's (1998) model, in which people's altruism or spite depends on their belief about their opponent, the behavioral predictions are the same.

If agents perceive their principal's decision against firing as a gift, reciprocity models (Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006; Rabin, 1993) predict that agents will respond by improving their performance. The predictions for survivors' behavior based on reciprocity models are complex. As survivors are not personally disadvantaged by firing, these models do not predict differences in survivors' performance within and across treatments. Principals, however, expose all agents *ex ante* to the risk of being dismissed, even if survivors keep their jobs. Following this argument, one could predict that survivors reciprocate this risk by lowering their performance levels in the main, but not in the control treatment.

Differences in survivors' performance between treatments can also be predicted based on "indirect" reciprocity, as suggested by evolutionary biologists. Nowak and Sigmund (2005) consider an interaction *a priori* between two individuals. They refer to indirect reciprocity, if other personally unaffected individuals in the population observe (or are informed about) that interaction and reward or punish (un)kind behavior. A principal's behavior depends on her belief about agents' concerns for reciprocity.

To sum up, regarding survivor behavior, inequality models do not predict differences between treatments, whereas signaling models and indirect reciprocity can rationalize negative responses in the main treatment, but suggest no effects in the control treatment.

4. Results

Before we come to our main results, two interesting facts about the real-effort task apply. First, 75% (i.e. 18 out of 24) of the agents who were indeed laid off in our two treatments provide a performance of zero or one in the second period. This indicates that the intrinsic motivation to position sliders seems to be quite low. Second, the variance of the performance of all agents is quite high. Aggregating the first period performance of all agents in our main and control treatments, average performance was 64.63, with a standard deviation of 34.76. 9.2% of the agents provided a performance below 5, 16.7% of the agents 'chose' (achieved) 100 or more. The high variance can be explained by agents' different productivities and/or their motivation to increase their principal's payoff.

In the following we present our results in two steps. First, we report the results for the agents in the main and in the control treatment. Here, using a within subjects design, we compare first and second period performance of agents. Next we analyze the behavior of the principals.

4.1. Agents' behavior

We start with the behavior of survivors in the main treatment. As depicted in the upper, left histogram of Figure 1, the 24 survivors provided a mean performance of 70.79 (sd: 35.61) in the first period, compared to 44.67 (sd: 53.73) in the second one. Thus, on average survivors decreased their performance by 37% from the first to the second period. The difference across performance levels is significant according to a Wilcoxon matched-pair signed rank test (p-value: 0.03). We conclude:

Result 1: Survivors significantly lower their performance in the second period compared to the first one if their principal decides to fire a co-worker.

One could wonder whether the effects are different for high- and low-performing individuals. To investigate this we conducted a median split based on first period performance. We find that the twelve low-performing agents decrease their performance by on average 44% (Wilcoxon matched-pair signed rank test, p-value < 0.03), the high-performing agents by a mean of 33% (Wilcoxon matched-pair signed rank test, p-value = 0.17).¹⁵

FIGURE 1 ABOUT HERE

Our predictions of a decrease in performance are based on the idea that agents perceive a principal's decision to lay off as an unkind act and/or as a signal of being matched with an unkind person. One might wonder whether agents differ in their perceptions of their principal's decision. To shed light on this, we asked agents in a questionnaire after the second period for an assessment of their principal.¹⁶ The answer options were "social", "self-serving", "rational"¹⁷, "others" and "I have no assessment". Interestingly, although survivors just observed one action by their principal (i.e. the decision to fire or not) almost all of them felt able to make a judgment about her. However, even if all survivors were faced with the same situation, assessments were quite heterogeneous. Twelve survivors assessed their principals as "rational", eight as "self-serving", two chose "others" and two had "no assessment".

Although it is not clear whether the assessments have a causal impact on performance, performance differences between the subgroups are remarkable. Survivors assessing their principals as

¹⁵ The performance of below-median agents is 41.50 (sd: 25.49) in period one and 23.25 (sd: 30.98) in period two. The respective numbers for high performers are 100.08 (sd: 11.42) and 66.08 (sd: 63.83).

¹⁶ We also asked principals and agents several psychological questions about their identification among each other. We find that layoffs are associated with a decrease in identification. The results are discussed in a separate paper (Drzensky et al., 2013).

¹⁷ The *Duden*, the most important German dictionary, describes the term "rational" as "reasonable, considered and make sense" which is less judgmental than "self-serving".

“self-serving” decreased their performance by 58%, whereas survivors who choose “rational” decreased their performance by only 10%.¹⁸

Is it really the voluntary decision that drives agents’ behavior? Alternatively it could be that agents respond to the layoff *per se*. To answer this question, we turn to the control treatment in which we randomly decided whether an agent is fired or not.

As presented in the lower histogram of Figure 1 (left side), the 24 survivors provide a mean performance of 52.63 (sd: 32.85) in the first period, compared to 50.33 (sd: 48.51) in the second one. According to a Wilcoxon matched-pair signed rank test, the performance is not significantly different across periods (p-value = 0.83). We conclude:

Result 2: Survivors provide the same performance in the first and in the second period if the decision to lay off a co-worker was exogenously made by the experimenter.

Note that the survivors’ average first period performance in the control treatment is lower than in the main treatment. This results from the high variance of the performance between individuals. One could argue that differentials in the first period performance may limit the comparability of our two treatments. However, as shown above, in the main treatment low-performers respond at least as strongly to the principal’s decision as high-performers. This strongly indicates that different first period performance levels between treatments do not distort the results.

How do agents react if no co-worker was fired? Results for the 24 agents in our main treatment are depicted in Figure 1 (upper histogram, right side). Agents whose principal decided against firing provide an average performance of 64.67 (sd: 37.46) in the first period. In the second period, they chose a mean performance of 77.50 (sd: 36.74), which is an average increase of 20%.¹⁹ The difference in performance is significant according to a Wilcoxon matched-pair signed rank test (p-value < 0.01).

In our control treatment agents provided a mean performance of 72.71 (sd: 34.17) in period one and 86.08 (sd: 42.10) in period two in the case of no firing – an increase of 18.5% (see Figure 1, lower histogram, right side). The difference in the performance distributions across periods is significant (Wilcoxon matched-pair signed rank test, p-value < 0.01). We conclude:

Result 3: In the case of no firing, agents increase their performance in both treatments.

¹⁸ Survivors who assessed their principal as “self-serving” provided an average performance of 84.00 (sd: 27.91) in the first, compared to 35.13 (sd: 48.83) in the second period. Agents who stated that she is “rational” chose a mean performance of 67.42 (sd: 34.97) in period one and 60.58 (sd: 62.04) in period two.

¹⁹ There is almost no variation in agents’ assessment of the principals in the questionnaire. 19 out of the 24 agents assess their principal as “social”.

While, in the case of no firing, agents' performance increase in the main treatment is (at first glance) in line with signaling of type and reciprocity models, it is quite surprising that agents also increase their performance to the same extent in the control treatment. We can only speculate about the reasons for agents' behavior in case of no firing.

As we found no performance differences between periods in the preliminary experimental sessions, learning effects in the real-effort task cannot explain the results. One explanation for the performance increase in the control treatment could be that agents are motivated to equalize payoffs between principals. By chance, some principals "lose" one agent, but receive the fixed amount of 5€. As none of the agents in our experiment has generated more than 5€, principals' expected payoffs might be lower in the case of no-firing. If agents in the no-firing cases are motivated to equalize principals' payoffs, they should increase their performance.

To sum up, we find that agents decrease their performance as a response to a principal's layoff decision, which is in line with signaling of type and indirect reciprocity models. However, as we find no treatment effects in the case of no firing, our results do not provide evidence that agents reward a principal's decision against firing by increasing their performance.²⁰

4.2. Principals' behavior

Do principals expect agents to respond to their decision? Interestingly, principals behaved quite heterogeneously: 40% decided to lay off an agent, 60% decided against it.²¹ Can different expectations explain the heterogeneity in principals' decisions?

To answer these questions, we elicited principals' beliefs about agents' performance. Immediately after principals in the main treatment made their decisions, we asked them first to estimate the average performance of *all survivors* in the second period of the respective experimental session. Second we also asked them to estimate the mean second period performance of all agents whose principals did not fire an agent. Both questions were incentivized, i.e. we paid the principals 2€ if their estimation met the $\pm 10\%$ interval of the true value.²² The fact that we asked (and incentivized) principals for their estimation of agents' performance given *both* possible decisions, allows us to reconstruct their monetary trade-off.

TABLE 1 ABOUT HERE

²⁰ The fact that we find treatment differences in the negative domain but not in the positive one, relates to previous studies comparing positive and negative reciprocity. For example Offerman (2002) finds that second-movers in his experiment had a stronger reaction to an intentional hurtful choice by a first mover than to an intentional helpful choice. Kube et al. (forthcoming) find in a field experiment that workers reduce their output in response to wage cuts, while an equivalent wage increase does not result in productivity gains.

²¹ One may ask whether firing paid in our experiment. Although both options were close together, principals earned on average slightly more by firing an agent (7.68€ compared to 6.98€ in the case of no layoff). Note, that this result cannot be generalized as it depends on the value of the fixed amount.

²² Note that the principals were not aware of the opportunity to earn the additional amount of money when they decided whether to shed a job or not. Agents were never informed about this.

Table 1 provides the results. As shown in Row 1, the twelve principals who decided to lay off agents expected on average a payoff of 2.13€ for each survivor (Column 1), compared to 2.29€ per agent in the case of no firing (Column 2). The difference in expected performance between periods is not significant (Wilcoxon matched-pair signed rank test, p-value = 0.34). Thus, principals who fire expect agents not to respond to the principal's decision. On average, they expect a mean payoff of $(2.13\text{€} \times 2) + 5\text{€} = 9.26\text{€}$ in the case of firing, compared to $(2.29\text{€} \times 3) = 6.87\text{€}$ in the case of no firing.²³ Given their beliefs, principals who decide to lay off agents thus maximize their payoff.²⁴ We conclude:

Result 4: Principals who decide to fire an agent do not expect that the agent's performance depends on the principal's decision.

Interestingly, as shown in Table 1, Row 2, we find that the eight principals who decided against firing expect an average payoff of 1.35€ for each survivor, compared to 2.56€ per agent in the case of no firing. The difference in expected performance is significant according to a Wilcoxon matched-pair signed rank test (p-value < 0.06). Thus, those principals expected the agents' performance to be 90% higher in the case of a decision against firing. This translates to a payoff of $(1.35\text{€} \times 2) + 5\text{€} = 7.70\text{€}$ in the case of firing, in comparison to $(2.56\text{€} \times 3) = 7.68\text{€}$ in the case of no firing.²⁵ Interestingly, those expectations are very precise, as in our experiment normalized performance²⁶ in the case of not firing is also 90% higher than the survivors' performance. We conclude:

Result 5: Principals who do not fire an agent have quite accurate expectations of how their decision affects their agents' performance.

5. Discussion and Conclusion

While adverse effects of downsizing on the survivors' performance are suggested in the management literature, we provide one channel which could explain the effects: the *decision* of the principal.

The creating of two identical layoff settings – either with or without a principal's decision – allows us to study the impact of the decision in isolation. Our results show that survivors reduce their performance by 37% if their principal lays off a co-worker, whereas the agents' performance remains

²³ Focusing on the individual level, ten out of the twelve principals expected that the average payoff would be higher by firing compared to not doing so.

²⁴ This result relates to Falk and Kosfeld (2006). The authors find in an experimental game that principals rather control agents if they do not expect agents to respond negatively to their decision.

²⁵ On the individual level, four principals expected to earn more and four to earn less by not firing.

²⁶ After an adjustment of the difference in period one performance, we divided the average second period performance of agents in the case of no firing by the survivors' average performance in period

two: $\frac{\left(\frac{70.79}{64.67}\right) \times 77.5}{44.67} = 1.9$.

constant in the case of an exogenous decision to fire. This is in line with signaling of type (such as Ellingsen and Johannesson, 2008; Levine, 1998) and indirect reciprocity models.

The agents' performance decrease in our experiment is consistent with field studies finding lower self-reported effort and self-reported performance after downsizing (see e.g. Travaglione and Cross, 2006). Responses to the principal's downsizing decision could also provide an explanation for lower commitment, higher turnover intentions (Allen et al., 2001) and increased absenteeism (Travaglione and Cross, 2006).

Our results imply that firms deciding in favor of layoffs should be careful how the decision is perceived by their workers. There might be several ways in which firms could limit the signaling character of the layoff decision – and thus, probably negative responses of survivors. In firms laying off workers one can observe a number of policies that are puzzling at first glance. In the following we discuss signaling concerns as potential explanations for several real life phenomena.

First, instead of firing workers, firms often use natural fluctuations to reduce the level of staffing (see Fuss, 2009). The existence of such policies is quite surprising as firms can more rapidly adjust their labor force via layoffs. As survivors respond negatively to an endogenous layoff, firms may be motivated to prevent the signal of laying off workers.

A second fact discussed by Cascio (1993) is that firms laying off workers often claim layoffs to be the “last resort”. In other words, those firms declare that they have no choice – as in our control treatment. One explanation for this communication strategy could be that firms try to prevent sending a negative signal.²⁷ The question remains whether employees really believe management's declaration. One way to verify declarations might be to “open the books” to works councils, as suggested by Freeman and Lazear (1995).

Third, Billger and Hallock (2005) show that CEO turnover is higher after downsizing. A rationale for this could be that firms try to limit the negative impact of the layoff decision by separating from the management with the layoff history. Even if the firm/shareholders were finally responsible for the decision, the new management can blame the predecessors. For the same reason, Bartling and Fischbacher (2012) argue that firms hire interim managers during restructurings.²⁸

Fourth, the negative signal of the layoff decision can also be attenuated by the provision of positive signals. Focusing on survivors' attitudes, Baron and Kreps (1999) recommend that the layoff “process should be kind to the leavers” (p. 433), e.g. by providing outplacement services. Similarly, firms deciding to shed jobs often make financial offers for voluntary leaves. These offers are quite expensive and, because of their better outside options, Jeon and Laffont (1999) suggest that it is the more able workers who separate. Interestingly, even the capital market seems to reward voluntary

²⁷ Firms may also blame exogenous forces, like governmental regulation or macroeconomic shocks for layoffs (see American Management Association, 1991). As firms still decide whether and how to shed jobs, it is not clear whether such arguments really attenuate the negative signal of the layoff decision.

²⁸ Bartling and Fischbacher (2012) focus in a lab experiment on the delegation of decision rights. They argue that restructuring firms often hire interim managers to “shift the blame”.

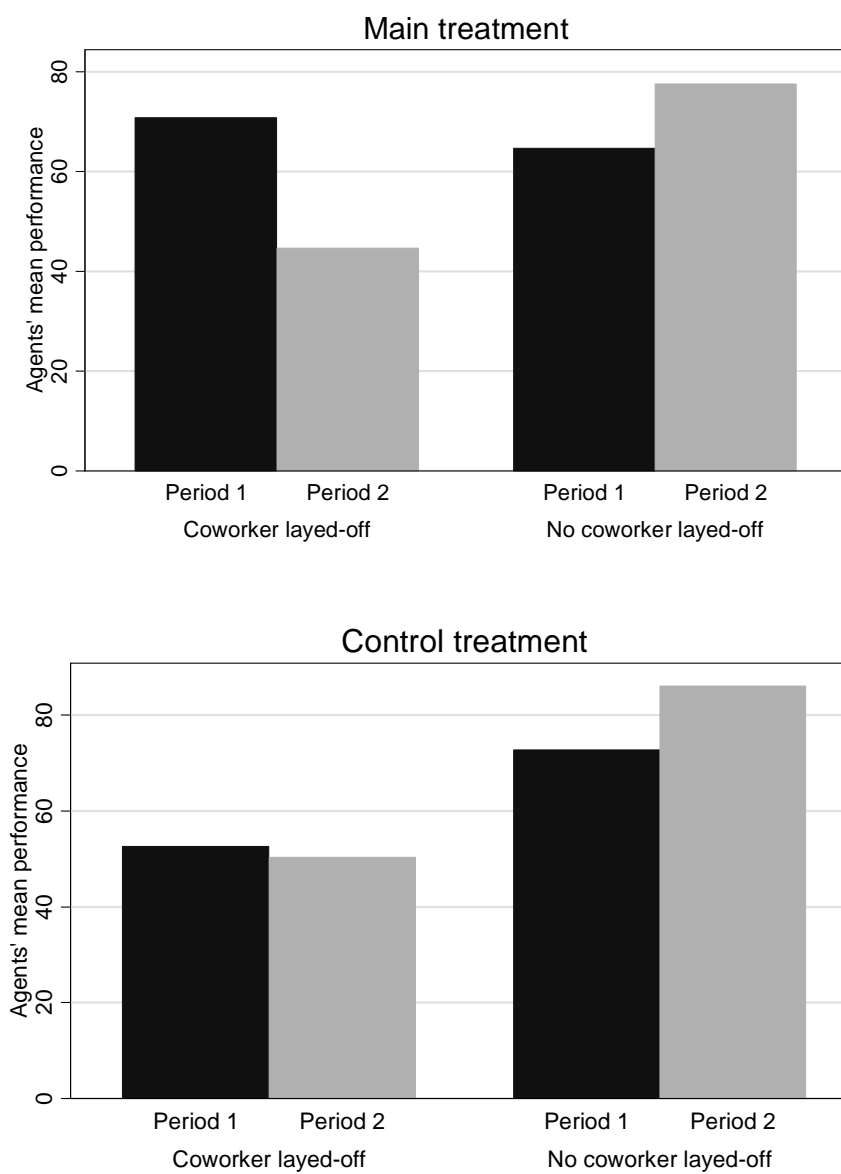
leaves. Nixon et al. (2004) find that stock returns are less negative if firms accompany large downsizings with financial offers for voluntary leaves.

The other main result of our experiment is that we find heterogeneity in principals' decisions. Those who decide against firing have quite accurate beliefs about how agents respond to principals' decisions. In contrast, principals deciding to lay off typically do not anticipate these effects. Heterogeneous layoff policies also occur in practice. Some firms, such as *Lincoln Electric* or *Nucor*, never lay off while others do. There are several explanations for why firms have different layoff policies, e.g. hold-up problems or benefits of downsizing may differ across firms.

Our results suggest that different beliefs about survivors' reactions could also provide an explanation for heterogeneous layoff policies in the field. However, one might wonder why downsizing firms do not update their beliefs about survivors' behavioral response. Survivors' behavior in our main treatment could provide an explanation. Even if all survivors were faced with the same situation, we found indications that survivors interpret the layoff decision in different ways. Thus, depending on workforce characteristics, different beliefs about survivors' behavior could be accurate in the field.

Figures and Tables

Figure 1: Performance of agents, by treatments and decision



The upper histogram presents the results of the main, the lower histogram of the control treatment. The black bars show the agents' average performance in the first, the gray bars in the second period.

Table 1: Principals' expectations in the main treatment

	Principals' mean second-period pay off (in €) generated by agents in the case of	
	lay off	no lay off
Expectations: Principal who decides to lay off (n=12)	2.13 (0.90)	2.29 (0.93)
Expectations: Principal who decides against lay off (n=8)	1.35 (1.07)	2.56 (1.23)
Real data (n=24)	1.34 (1.61)	2.32 (1.10)

Columns 1 provides the average payoff generated by each agent in period two in the case of firing. Column 2 shows the same in the case of no firing. Rows 1 and 2 provides principals' expectations, given their decision. Row 3 depicts the payoff that was generated by subjects in our main treatment.

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