Team Building and Hidden Costs of Control[☆]

Gerhard Riener*, Simon Wiederhold[a]

Department of Economics, Mannheim, Germany

[a]*Ifo Center for the Economics of Education and Innovation, Munich, Germany*

Abstract

In a laboratory experiment, we investigate the interaction of two prominent firm strategies to increase worker effort: team building and control. We compare a team-building treatment where subjects initially play a coordination game to gain common experience (CE) with an autarky treatment where subjects individually perform a task (NCE). In both treatments, subjects then play two-player control games where agents provide costly effort and principals can control to secure a minimum effort. CE agents always outperform NCE agents. Conditional on control, however, CE agents' effort is crowded out more strongly, with the effect being most pronounced for agents who successfully coordinated in the team-building exercise. Differential reactions to control perceived as excessive is one explanation for our findings. **JEL code:** C92; M54; D03; J22

Keywords: Employee motivation; Principal-agent interaction; Team building; Experiment

1. Introduction

Employment relationships are typically characterized by incomplete contracts. Firms thus engage in various strategies to incentivize workers. Two prominent firm techniques in this regard are control devices designed to eliminate workers' most opportunistic actions (see, for example, Fehr and List, 2004) and team-building exercises across hierarchies to foster identification with the firm. Both techniques belong to the most widely applied management practices.[1] Previous evidence from field data points towards an interaction between control and team building at the workplace, suggesting that the nature of the worker-firm relationship affects how workers perceive certain coercive characteristics of employment contracts, such as minimum effort requirements or employee moni-

[☆]We are indebted to Agnes Bäker, Oliver Falck, Werner Güth, Bernd Irlenbusch, Oliver Kirchkamp, Philipp Lergetporer, Ljubica Nedelkoska, Hans-Theo Normann, Holger Rau, Jens Ruhose, Tobias Regner, Guido Schwerdt, Dirk Sliwka, Marie-Claire Villeval, Ludger Wößmann, Philipp Zahn, and conference participants at ESA 2011, EALE 2012, IMEBE 2012, SOLE 2012, and Verein für Socialpolitik 2012 for their valuable comments on earlier drafts of this paper. Financial support from the Deutsche Forschungsgemeinschaft (DFG) through RTG 1411 is gratefully acknowledged.

*The author is grateful to the financial support of the Duesseldorf Institute for Competition Economics

Email addresses: riener@dice.hhu.de (Gerhard Riener), wiederhold@ifo.de (Simon Wiederhold)

[1]This is indicated, for instance, by Staw and Epstein (2000) in their evaluation of popular management techniques. Buller and Bell Jr. (1986) provide an early exploration of the interaction of team building and goal-setting techniques.

toring.[2] However, these results are likely to suffer from severe confounds due to endogeneity and worker selection.[3] We thus conduct a controlled laboratory experiment to study whether the social nature of the worker-firm relationship affects the worker's reaction to control.

We design simple control games in a principal-agent setting without specific workplace framing. The agent provides transfers (effort), which are costly for her but beneficial to the principal. The latter can restrict the agent's choice set by imposing a minimum effort requirement that the agent is not allowed to fall short of (control). We investigate situations where the agent makes transfers out of endowed income and we elicit her transfer for each control level available for the principal to choose.[4] Additionally, we test the robustness of our findings across different environments by letting agents engage in a real-effort task, where we use the direct-response method to collect agents' transfer decisions.

Our treatment manipulation attempts to capture two essential features of team-building activities in the real world, namely, a joint task that facilitates gaining positive experience among team members and mutual judgment about this group experience (Sundstrom et al., 1990).[5] We propose a novel group induction task that exhibits these features. Subjects in the common experience (CE) treatment play a weakest-link game with pre-play communication and post-play judgment in the beginning of the experiment that is unrelated to the tasks to be performed in later stages. Coordination in the weakest-link game is relatively easy to achieve, ensuring a shared feeling of success in most of the teams. However, failure is still possible, so successfully coordinating pairs are justifiably pleased with their results. Our coordination game reflects that real-world team building typically is not directly related to the actual business of the firm, since its primary objective is to increase mutual support, communication, and the sharing of feelings between team members (Buller and Bell Jr., 1986; Salas et al., 1999). Thus, from the perspective of social identity theory, team building facilitates the emergence of social groups (for instance, Tajfel, 1978; Ashforth and Mael, 1989), a phenomenon increasingly explored in the experimental economics literature (Weber and Camerer,

[2] Akerlof and Kranton (2008) provide case-study evidence from the U.S. steel industry suggesting that "[w]hat matters is not more or less monitoring *per se*, but how employees think of themselves in relation to the firm" (Akerlof and Kranton, 2008, p. 212). Barkema (1995) documents for a sample of 116 executives of Dutch firms that higher monitoring is negatively correlated with working hours if managers are supervised by an in-house CEO, whereas the correlation is positive if monitoring is implemented impersonally by a parent company. Frey (1993) makes a theoretical argument that in environments where the principal and agent know one another personally, the agent is more likely to interpret being monitored as a signal of distrust than in distant principal-agent relationships. For an excellent overview, see Charness and Kuhn (2011).

[3] A number of factors—such as economic dependency on the job, organizational tenure, and (informal) organizational structure—simultaneously influence group attachment, reactions to control, and performance (Albert and Whetten, 1985; Ashforth and Mael, 1989; van Knippenberg and van Schie, 2000). Moreover, control-averse individuals are unlikely to apply for a position in a firm in which they expect a controlling leadership style, or they are more likely to resign from such job once its nature is revealed. Thus, in reality, work climate and employees' personal characteristics (for instance, degree of control aversion) are not mutually independent (Stanton, 2000; Antonakis and Atwater, 2002; Ploner et al., 2012).

[4] This resembles the experimental design by Falk and Kosfeld (2006). However, we extend the principal's action space to three control levels to examine within-subject differences in the agent's reaction to weak versus strong control.

[5] Geister et al. (2006) show that online feedback in virtual teams increases productivity.

2003; Eckel and Grossman, 2005; Charness, 2012).[6] Importantly, the team-building intervention in the CE treatment takes place *before* we establish the principal-agent relationship, that is, participants coordinate as equals first before entering a hierarchical relationship in the control games (for team-building across hierarchies, see Weber and Camerer, 2003). This design applies to several real-world situations that are relevant for organizations, for instance, within-firm promotions leading to a change in leadership or company acquisitions. We compare the CE treatment to a treatment where subjects complete a task in isolation in the beginning of the experiment (NCE treatment).

We find that our team-building intervention has important consequences for behavior. CE agents' transfers exceed those made by NCE agents for each control level. This result clearly suggests that team building has the potential to increase an agent's intrinsic motivation to act on behalf of the principal. In particular, CE agents generously reward not being controlled. However, when the principal imposes weak control, CE agents' effort is crowded out more strongly than NCE agents' effort, indicating higher hidden costs of control for CE agents.[7] When high effort is easy to enforce for the principal, treatment effects are in the same order of magnitude as in the case of weak control, but do not reach statistical significance. Consistent with treatment effects in transfers, we observe that agents' control beliefs also differ between treatments. CE agents are always more likely to expect no control and less likely to expect either weak or strong control than their NCE counterparts.

Exploring the channels through which the interaction of hidden costs of control and social distance between the principal and agents operates, we find that treatment effects are driven by those subjects who successfully coordinated in the team-building exercise. For these agents, we expect that team building rendered group identity salient. Agents who mis-coordinated in the team-building task do not react differently to control than do their NCE counterparts. Another mechanism that we can support with our data is that there are treatment differences in the reaction to principals not acting in accordance with agents' control beliefs. We find that CE agents reduce transfers if the level of control exceeds their subjective control beliefs; NCE agents do not retaliate against such felt excessive control. Since a rising level of control, ceteris paribus, increases the likelihood that control beliefs are violated, CE agents' inclination to punish unexpectedly harsh control behavior offers an explanation for our aggregate finding that CE agents respond more strongly to control than do their NCE counterparts.

We implemented two additional control treatments to rule out that participation in the team-building task *per se* or the mere existence of a feedback mechanism are responsible for the treatment effects. First, we did not provide information on the outcome of the team-building task and removed the outcome judgment before playing the control games. We can therefore test whether the possibility to provide and receive judgments exerts an additional effect on agent behavior. Second,

[6] Common experiences and interpersonal interactions are factors traditionally associated with group formation (for an early reference, see McDougall, 1920).

[7] When control is weak, the crowding out effect of control dominates the disciplining effect in both treatments as NCE agents and CE agents tend to reduce their transfers compared to the no control case.

we randomly rematched subjects after completing the team-building task (without feedback). This allows us to account for potential spill-over effects from the team-building exercise to the control games.

Our work is related to previous literature on the role of control devices at the workplace. Standard principal-agent theory suggests that policing workers will increase their performance, since they are merely self-interested and shirking is frequent (Alchian and Demsetz, 1972; Jensen and Meckling, 1976). This prediction, however, is challenged by a number of empirical studies showing that control can reduce worker effort by eroding intrinsic motivation (among others, Enzle and Anderson, 1993; Schnedler and Vadovic, 2011).[8] Probably the most important work on the crowding-out effects of control is the experimental paper by Falk and Kosfeld (2006), who show that control can incur costs that outweigh its benefits when dealing with reciprocal agents, as reciprocity is sensitive to control.[9] However, Falk and Kosfeld (2006) do not account for heterogeneous effects of control with regard to the nature of the principal-agent relationship, and thus neglect the possibility that social ties may increase or undermine motivation.

The relationship between team-building activities and worker performance has also been studied previously. Although the empirical evidence is somewhat ambiguous (Woodman and Sherwood, 1980; Buller and Bell Jr., 1986; Salas et al., 1999), it is often claimed that by engaging in team building "any group can transform itself ... into a high-performing team" (Shandler and Egan, 1996, p. x). Moreover, the literature on psychology in organizations recognizes identification with the firm as a powerful concept for explaining individual behavior, for instance, turnover intentions (van Dick et al., 2004).

Recent experimental findings support the existence of group-contingent social preferences, as participants typically exert more effort for members of the same group.[10] In fact, even random assignment to a *minimal group*, which is often just an arbitrary label, sometimes proves sufficient to induce people to display greater social concerns for the well-being of an in-group member (Chen and Li, 2009; Heap and Zizzo, 2009; Sutter, 2009; Chen and Chen, 2011). However, as Goette et al. (2012) suggested, this 'labeling' effect is rarely found in the real world. The authors provide evidence that the additional motives arising when group induction is not minimal are important determinants of individual behavior toward both in-group and out-group members (see also Pan and Houser, 2013).[11]

Our paper is closely related to the study by Dickinson and Villeval (2008), who investigate

[8] See van Dijk and van Winden (1997), Sliwka (2007), Ellingsen and Johannesson (2008), and von Siemens (2013) for theoretical investigations of the mechanisms underlying motivational crowding-out.

[9] Other papers that study the impact of extrinsic incentives on intrinsic motivation and voluntary cooperation in the laboratory include Fehr and Rockenbach (2003) and Bartling et al. (2012). Hagemann (2007), Schnedler and Vadovic (2011), and Ploner et al. (2012) replicate the original experiment by Falk and Kosfeld (2006).

[10] See Eckel and Grossman (2005), Charness et al. (2007), Chen and Li (2009), Heap and Zizzo (2009), and Chen and Chen (2011) for evidence from the laboratory, which is complemented by the field evidence provided in Goette et al. (2012) and Falk and Zehnder (2013).

[11] Charness et al. (2007) consider strategic tasks instead of pure allocation tasks, observing strong effects on individual behavior when group membership is made salient, but not for minimal groups.

whether the nature of the employment relationship affects agents' reactions to monitoring in a real-effort experiment. The authors find that tighter monitoring by the principal crowds out the agent's effort only if the principal and agent are socially close.[12] The authors, however, vary social distance by lifting anonymity. With this design, various confounding factors are conceivable, such as feelings of sympathy or antipathy evoked as the result of close, uncontrolled, and direct communication between subjects (Crawford, 1998; Dufwenberg and Muren, 2006; Goette et al., 2012). In contrast to Dickinson and Villeval (2008), control and effort decisions in our experiment are made anonymously. Keeping anonymity addresses concerns regarding non-anonymous interactions in economic experiments, and allows us to isolate the effect of a previous common experience on the agent's willingness to exert effort for the principal. Moreover, subjects in Dickinson and Villeval's experiment play monitoring games for several rounds, so feedback effects of monitoring on the interpersonal relationship are conceivable. The one-shot design of our experiment precludes such feedback effects.[13] In fact, we can rule out any strategic motives (for instance, reputation building) on the part of the principal to trust or on the part of the agent to exert effort (for a discussion, see Falk and Kosfeld, 2006).

Also related to our work are two recent papers that investigate the effect of group norms on the hidden costs of control in a principal-agent setting. Masella et al. (2014) create identity using the procedure introduced by Chen and Li (2009), while Leider and Kessler (2012b) establish a pro-social norm by allowing for non-binding agreements on behavior in the control game.[14] However, neither study captures essential features of real-world team building, in particular, its focus on creating a positive shared experience for participants so as to facilitate group formation and its independence from the firm's actual business.

The remainder of this paper is organized as follows. The next section explains the experimental design, which is followed by a discussion of the results in Section 3. Section 4 illustrates possible mechanisms explaining them. Section 5 concludes, providing the implications of our results.

2. Experimental Design

The experiment was comprised of three stages and four treatments. In all treatments, the second and third stage were identical. No subject participated in more than one treatment or session. The design was parsimonious, without work environment frames. Each participant was randomly assigned a treatment upon arrival at the lab. At the beginning of the experiment, subjects were informed that the experiment would comprise three stages, but received no further information

[12] The other condition that needs to be fulfilled to observe motivational crowding out is that the principal's payoff must directly depend on the agent's effort.

[13] While players interact in several games in our experiment, it is still of a one-shot nature. First, subjects in the CE treatment were not aware that they would continue playing with their partner beyond the initial coordination game. Second, agents did not learn of the principal's control decision, and principals received no feedback on agents' transfers, until the final stage of the experiment.

[14] Leider and Kessler (2012a) use a similar procedure to establish group norms in various two-person games.

about the stages.[15] At the end of the experiment, the participants completed a questionnaire on their socio-demographic characteristics.[16]

Treatment Manipulation Stage

No Common Experience (NCE). The NCE treatment constitutes our baseline treatment. Subjects in this treatment had to complete a repetitive task, borrowed from Gill and Prowse (2012), in isolation. The task was to move 48 sliders into the middle position within an allotted time of 2:15 minutes. Participants in this task received a flat fee of 80 ECUs, independent of their performance. This payoff choice was motivated by the average payoff of subjects participating in the pilot sessions of the common experience treatments. After performing the task, NCE subjects were randomly paired.

Common Experience Reshuffle (CER). Subjects in the CER treatment played a weakest-link game in groups of two in the first round of the experiment. The aim of this phase was to induce a feeling of belonging to a group as a consequence of the shared experience. Subjects could distribute 50 experimental currency units (ECUs, where 1 ECU was worth € 0.10) to a private or a group account. The returns to the group account were the smaller of the two contributions to that account, doubled by the experimenter. A subject's total payment was the sum of the private and group account. After explaining the game, a message on the computer screen asked the participant to discuss her strategy for the game with her partner via online chat.[17] The coordination game had an obvious focal point to facilitate the establishment of group feelings. We set the threshold for successful coordination relatively low, but it was still high enough to ensure that not all subjects managed to coordinate on an efficient outcome (see Section 4.1). Thus, pairs who successfully mastered the task had justifiable reason to be proud of their achievement.[18] The pairs from this stage were then reshuffled for the remaining experiment and the players were explicitly told that they will not interact with their partner from the weakest-link game again. This treatment ensures that subjects experienced a group feeling in the beginning of the experiment, but that this feeling was shared with a person

[15] The experiment's instructions can be found in the online appendix.

[16] We also ran two pilot sessions with a total of 36 participants. Results are not reported here.

[17] To ensure anonymity, we required the participants to limit their discussions to game-related topics. We checked the chat protocols to see whether personal information was exchanged during the two-player interaction. Nearly all subjects discussed only the game. Subjects in one pair in the CE treatment (see below), however, revealed their identity during the chat. Since dropping these subjects from the sample leaves all results virtually unchanged, we kept them in the sample.

[18] In a recent paper, Charness (2012) argues that the threshold for successfully mastering experimental team-building tasks should be set low enough to ensure large-scale success. In our opinion, however, the group feelings evoked by successful coordination depend to some extent on the difficulty of the joint task; if success in the group task is universal, the feeling of having reached a goal together may not materialize. Furthermore, we decided against a real-effort task in the common experience treatments to ensure that the allocation decision in the coordination game is under full volitional control.

with whom they did not continue playing with.[19]

Common Experience without Feedback (CEN). This treatment is similar to the CER treatment, with one important difference: subjects who interacted in the group-induction stage remained in fixed pairs for the rest of the experiment, and were also told so upon finishing the weakest-line game. Having both the CER treatment and the CEN treatment allows us to disentangle the effect of just having participated in the team-building exercise—which might have evoked feelings of sympathy or trust or even revealed something about the types of players in the population—from the specific relationship with the person a subject has experienced team building with.

Common Experience with Feedback (CE). This treatment is similar to CEN but adds another feature that is considered important in real-world team-building interventions: feedback (see, for example, Marks et al., 2001, who describe aspects of team formation and performance). After the weakest-link game the outcome of the game was disclosed and subjects were asked to let their partners know about how fair they judged the other's decision. Subjects could pick any natural number between 1 (very unfair) and 5 (very fair), but were not allowed to further explain their opinion. Both partners received this judgment before proceeding to the next stage. The feedback opportunity not only aligns our team-building exercise in the laboratory closer to real-world team building, it also provides a measure of the quality of the relationship established between the subjects. Obtaining such measure is important because subject behavior in later stages of the experiment may be affected by (self-perceived) group salience.[20]

In the presentation of results, we focus on treatment differences between NCE and CE. Section 4.1 provides an investigation of all four treatments, showing which features of the group-induction process can explain differences in behavior between NCE agents and CE agents.

Effort-Choice Stage

The effort-choice game was a modified version of the design developed by Falk and Kosfeld (2006). Before the game, subjects were assigned roles as either principal or agent. Each agent had an initial endowment of $E = 117$, which she could—partly or fully in integer amounts—transfer to the principal. This transfer approximated agent effort. The marginal monetary costs for the agent to expend one unit of effort were constant and set to 1. The principal had no initial endowment. However, she was given the option of restricting the agent's choice set by enforcing

[19] By design, the teams did not interact with one another in later stages of the experiment, so there was no competition for team members. Neither did we reveal the transfer or control choices made by other teams. Although the social identity literature demonstrates that salience of other groups and competition among groups reinforce awareness of group membership (Ashforth and Mael, 1989; Worchel et al., 1998), our goal was to investigate whether even a one-time interaction (in the initial coordination game) would be sufficient to detect behavioral differences in subsequent games. However, our group-induction method captures more than the pure labeling effect that results from simply assigning people to certain groups (minimal-group paradigm); it also accounts for the social ties aspect of groups that emerges from the experience of successfully coordinating in a joint task.

[20] In a companion paper (Riener and Wiederhold, 2013), we show that the specific reason for failure of group induction constitutes additional motives for the behavior of unsuccessful coordinators, which are absent for subjects who managed to coordinate successfully.

one of the following three minimum transfers: $E_{\min} \in \{0, 6, 21\}$. We extended the principal's action space compared to the original design by Falk and Kosfeld (2006), which allowed only either control or trust, to obtain within-subject information about the reaction to weak versus strong control. We tried to avoid the potential for experimenter demand effects by wording the principal's control options neutrally (Hagemann, 2007; Leider and Kessler, 2012b). Thus, in the instructions we described control as the principal 'requesting a minimum contribution' that the agent 'is not allowed to fall short of.'

The principal was paid twice the actual amount transferred by the agent with probability of 90%, and with a 10% probability the principal received twice the required minimum contribution. We introduced this element of uncertainty in the principal's payoff function to render it even easier for the agent to act in an opportunistic fashion (Charness and Dufwenberg, 2006), as she knows that the principal will not with complete certainty be able to attribute a low payoff to her selfish decision. Agents made conditional transfer decisions for each possible minimum effort level. The transfer that corresponded to the principal's actual control decision determined the agent's payoff. However, the level of control exercised by the principal was not revealed to the agent, nor did the principal receive feedback on the agent's choices.

Real-Effort Stage

After the effort-choice game, the experiment proceeded to its last stage. In this stage, agents had to add five two-digit numbers (Niederle and Vesterlund, 2007), and the remuneration depended on the number of correct answers (15 ECUs for each sum correctly solved). We deliberately chose the real-effort task to be different than the task NCE subjects performed in the first stage of the experiment so as to avoid the potential for experience with the task influencing behavior. Before the agent performed the task, she had to decide on the share of her future profits to be transferred to the principal.[21] Since the principal had no own endowment, the agent's transfer was again the only source of income for the principal in this game. The principal could choose her desired minimum share from the following possibilities: $E_{\min} \in \{0\%, 10\%, 20\%, 40\%\}$. We did not double transfers in the real-effort game.

Although Bruggen and Strobel (2007) do not find differences between effort-choice games and real-effort games in economic experiments, we believe that both types of games have a raison d'être in our experiment. Most previous studies investigating the hidden costs of control use the strategy method (for instance, Falk and Kosfeld, 2006, and its repititions). Multiple observations for each individual provide us with valuable information because the counterfactual can be observed and does not need to be inferred from the decisions of other subjects.[22]

[21] When confronted with the sharing decision, the agent already knew that the real-effort task would involve computing sums. Therefore, the agent's choice could have depended on her (perceived) numeracy skills. However, since number-adding skills should be randomly distributed across treatments, any non-zero correlation between numeracy skill and sharing is unlikely to bias our results on treatment effects in transfers.

[22] Leider and Kessler (2012b), in contrast, observe agents' transfers only for the control levels actually chosen by the principals.

However, one could argue that the only reason agents vary their transfer decisions across the different control levels is that the elicitation method induces this behavior.[23] The real-effort game, in which the agent's decisions are elicited with the direct-response method, is designed to give us more confidence that our findings are not simply an artifact of the elicitation method. However, since the real-effort game always takes place after the effort-choice game, we cannot rule out spill-over effects between both stages.[24] We intentionally changed two features of the game compared to the effort-choice stage (real effort instead of pure allocation decisions, direct-response method instead of strategy method) to alleviate concerns that spill-over effects compromise our findings.

Although we cannot know with certainty that the results from the real-effort task are independent of those from the effort-choice game, there are other reasons why the real-effort stage may lead to important insights. First, principal-agent relationships in real-world organizations almost always involve the agent expending real effort. Second, given the evidence on earned versus windfall money in dictator-like experiments, agents receiving a windfall endowment may be more inclined to act pro-socially than agents who have to work for their money (for an extreme example, see Cherry et al., 2002). Indeed, Thaler and Johnson (1990) and Arkes et al. (1994) show that unearned income leads to more risky behavior. Third, the potential role of control levels perceived as excessive in shaping the agent's behavior is unlikely to be properly revealed if the agent is not aware of the principal's actual control decision. In the real-effort game, agents receive feedback on principals' control choices *before* deciding on transfers, which allows us to investigate whether reactions to control perceived by the agent as excessive are a means by which hidden costs of control materialize (see Section 4.3).[25]

Belief Elicitation

We elicited subjects' control beliefs to study whether group induction affects expectations about control. Agents were asked to attach a likelihood to each possible control level before making their transfer choices in Stages 2 and 3. Likewise, principals had to state their beliefs regarding agents' control expectations (second-order beliefs). As in Falk and Kosfeld (2006) and Masella et al. (2014), we did not incentivize these answers.[26]

[23] For example, Gueth et al. (2001) report that playing a game sequentially leads to different results than when the exact same game is played using the strategy method. On the other hand, Brandts and Charness (2011) emphasize in a recent survey that, to date, there is no evidence that the strategy method induces behavior that is otherwise not present.

[24] We did not reverse the order of the tasks to avoid that the agent, after learning the principal's control choice in the real-effort game, updates her beliefs about the type of principal with whom she is paired. This would destroy the one-shot nature of our experiment and could introduce strategic motives influencing the principal's behavior (for instance, reputation building).

[25] Emotions are likely to play a larger role in decision making when the agent's reward accrues only after successfully completing a task (Charness et al., 2004).

[26] Ivanov (2011) argues that a lump-sum payment for beliefs is preferable to more complicated elicitation procedures because it does not confuse subjects. Moreover, there is evidence that incentivization does not significantly improve the accuracy of the stated beliefs (e.g., Friedman and Massaro, 1997; Ortmann et al., 2000; Sonnemans and Offerman, 2001; Guarino et al., 2006; Offerman et al., 2009).

Implementation

The experiment was programmed in zTree (Fischbacher, 2007) and conducted at the computer laboratories of the Universities of Jena and Dusseldorf. Subjects were recruited via the ORSEE online recruitment system (Greiner, 2004). In total, 456 subjects (330 in Jena and 126 in Dusseldorf) participated in the experiment, primarily undergraduate students (see Table 1 for details). Each session lasted, on average, 45 minutes. After completing all three stages of the experiment, subjects were informed about their payment. The payoff-relevant stage was chosen at random. Then, subjects were asked to provide basic biographic information by filling out a questionnaire in return for an additional € 1. Furthermore, they received € 2.50 in Jena (€ 3.50 in Dusseldorf) for arriving on time for the experiment. The average total payoff was € 8.70 (€ 10.82), which is roughly equivalent to the hourly wage of a local research assistant. The maximum payoff was € 16.30 (€ 28.00).

Table 1: Implementation of Treatments

	No Common Experience	Common Experience			*Total*
		Reshuffle No Feedback	No Reshuffle No Feedback	No Reshuffle Feedback	
	NCE	CER	CEN	CE	
Jena	158	-	-	172	*330*
Dusseldorf	24	36	40	26	*126*
Total	*182*	*36*	*40*	*198*	*456*

Notes: This table shows the distribution of treatments and the number of observations. The experiments in Jena were conducted from December 2010 to January 2011, while the experiments in Dusseldorf were conducted in February 2014. NCE and CE treatments in Dusseldorf provide a check for comparability with the data obtained in Jena.

3. Results

The presentation of our main results is structured as follows. In Section 3.1, we report agents' transfers and beliefs in the effort-choice game. We then investigate whether hidden costs of control exist (Section 3.2) and, if so, whether these differ across treatments (Section 3.3). As the principals' control choices are likely driven by their beliefs about the agents' response to control, we briefly discuss them in Section 3.4. Since we consider the CE treatment to be closest to team building in the real world, the presentation of the results focuses on comparing the NCE treatment and CE treatment. Section 4 provides a discussion whether the specific method of group formation affects our results and sheds light on further mechanisms potentially explaining treatment effects in the hidden costs of control.

3.1. Transfers and Beliefs

Transfers

For both NCE agents and CE agents, we observe a non-negligible fraction of subjects whose transfers are at the minimum requirement imposed by the principal; this is the equilibrium strategy

with standard preferences. Judging by chi-square tests,[27] the proportion of agents with a zero voluntary transfer does not differ between NCE agents and CE agents for no control and weak control. For strong control, however, the proportion of agents who transfer the minimum amount is significantly larger for NCE agents than for CE agents: *No control*: 6.59% (NCE) vs. 10.19% (CE), $p = 0.384$; *Min 6*: 30.77% vs. 23.23%, $p = 0.242$; *Min 21*: 59.34% vs. 44.42%, $p = 0.020$. Not surprisingly, the proportion of subjects who choose to transfer only the minimum increases in control.

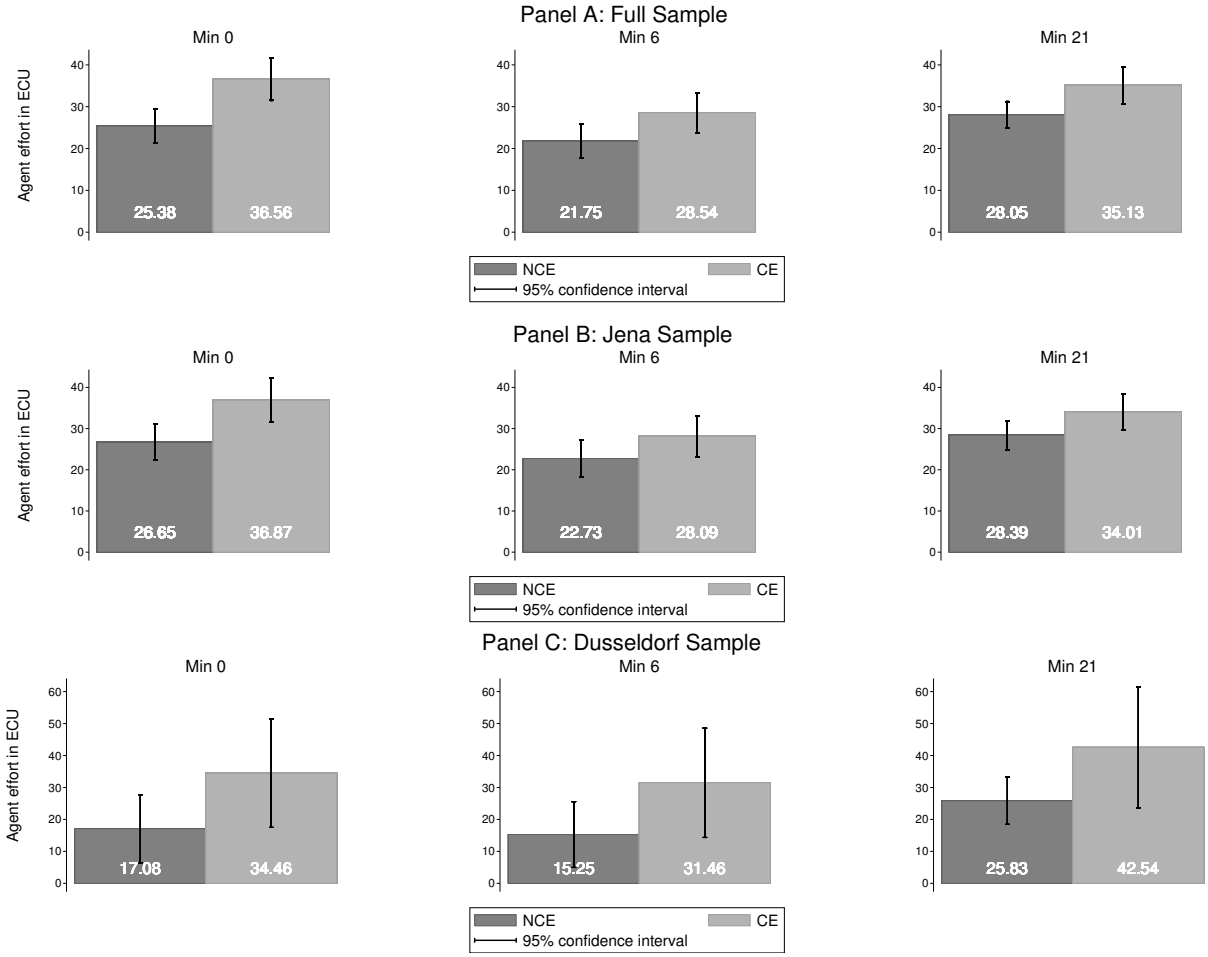
Our first result concerns treatment differences in agent transfers.

Result 1. *The team-building experience affects the level of effort provided by the agent.*

Support: Figure 1 plots the numbers of ECUs that agents transferred to their principals in each treatment as a function of the principal's control decision.[28] Some noticeable patterns emerge. First, we find that NCE agents' average transfers are significantly lower than CE agents' transfers for each control level (Panel A).[29] This result indicates that a joint team-building exercise by the principal and agent increases the agent's subsequent willingness to act in the principal's interest. Second, the order of magnitude of the treatment difference decreases in the level of control, suggesting that NCE agents may forgive control more easily than CE agents. This provides a first indication that the reaction to control varies by agent type, an aspect we examine more systematically below. Third, for both NCE agents and CE agents, exercising control appears to be counterproductive when the control device is weak, as both agent types tend to lower their transfers as compared to the no control case. Finally, only very few agents (at most two, depending on the control level) engage in an effort level of 39, which guarantees an equal split of the earnings between the principal and agent. We observe similar patterns for experimental participants in Jena (Panel B) and in Dusseldorf (Panel C), indicating that treatment differences in effort are persistent across environments.

[27] We report the results of two-sided tests throughout the paper.
[28] Table A.1 contains the average and median values of agents' transfers and beliefs.
[29] Performing Kolmogorov-Smirnov tests instead of Wilcoxon-Mann-Whitney rank-sum tests delivers similar results.

Figure 1: Agents' Transfers in the Main Treatments



Notes: This figure shows average agent effort for each level of control by treatment. In Panel A, we consider the full sample of NCE agents ($n = 91$) and CE agents ($n = 99$). There are significant treatment differences in transfers for all control levels (Wilcoxon-Mann-Whitney rank-sum test, *No control*: $p = 0.001$; *Min 6*: $p = 0.045$; *Min 21*: $p = 0.004$). In Panel B, we restrict the sample to the experiments conducted in Jena. This leaves us with 79 NCE agents and 86 CE agents. Treatment effects are significant for no control and strong control, while transfers for weak control are statistically indistinguishable between NCE agents and CE agents (*No control*: $p = 0.005$; *Min 6*: $p = 0.152$; *Min 21*: $p = 0.023$). Panel C reveals that transfers are significantly different for all control levels when considering the 12 NCE agents and 13 CE agents who participated in the experiments in Dusseldorf (*No control*: $p = 0.038$; *Min 6*: $p = 0.042$; *Min 21*: $p = 0.068$).

These results corroborate previous experimental findings regarding behavior in social groups (for instance, Tajfel, 1978; Ashforth and Mael, 1989; Fershtman and Gneezy, 2001), suggesting that the nature of the principal-agent relationship has a level effect on effort exerted on behalf of the principal. Early laboratory experiments evidence that simply assigning an individual to a group can be sufficient to induce in-group favoritism (Brewer, 1979; Ashforth and Mael, 1989), which is confirmed by more recent studies (among others, Chen and Li, 2009; Chen and Chen, 2011). The findings by Eckel and Grossman (2005), Charness (2012), Goette et al. (2012), and Pan and Houser (2013) indicate that the behavioral effects of identity induced using the minimal-group paradigm also carry over to environments where group membership is more salient. Furthermore, non-experimental

evidence from the public sector implies that the social distance between the principal and agent directly affects whether the agent shirks or works (Scholz, 1991; Chaney and Saltzstein, 1998).

Control Beliefs

CE agents expect no control (strong control) to be significantly more likely (less likely) to occur than agents without prior experience with their principals (see lower panel of Table A.1). On average, NCE agents expect, with a probability of 19.51%, that they will not face any control, while this likelihood is more than 13 percentage points higher for CE agents. The magnitude of the treatment difference in beliefs is almost identical, but with opposite sign, for strong control. The perceived likelihood of facing weak control does not differ between treatments. The average (median) value of the control level considered most likely is 15.74 (21) for NCE agents and 10.23 (6) for CE agents. This difference in modal control beliefs is significant at 1% (Wilcoxon-Mann-Whitney rank-sum test, $p < 0.001$). The result that group salience matters for agents' control beliefs finds additional support from Masella et al. (2014).[30]

It is important to note that, throughout the paper, a causal interpretation of treatment effects in beliefs is rendered difficult by the potential endogeneity of beliefs. However, there is no straightforward reason to expect that agents in either treatment have more incentive to be untruthful about their control beliefs. If there is indeed no systematic difference between treatments in the inclination to misreport beliefs, the fact that beliefs are likely to be measured with error will even bias treatment effects toward zero.

3.2. The Hidden Costs of Control

Our second result is that exercising control can backfire if the control device is weak.

Result 2. *There are hidden costs of control and these can be substantial enough to undermine the effectiveness of control. In particular, both NCE agents and CE agents respond to weak control by decreasing their transfers to the principal.*

Support: The cumulative distributions of transfers in Figure 2 provide evidence that there are hidden costs of control in both the NCE treatment and CE treatment. Using the exact Wilcoxon signed rank tests for paired observations, we can reject the null hypothesis that the modified distribution (see figure notes) in the no control condition is the same as the distribution for 6 control (NCE: $p < 0.001$, CE: $p < 0.001$). We can also reject the null hypothesis that the modified distribution in the no control condition is the same as the distribution for 21 control (NCE: $p = 0.004$, CE: $p < 0.001$). Interestingly, we do not observe differences in the distributions of transfers between *Min 6* and *Min 21* (NCE: $p = 0.659$, CE: $p = 0.470$). Moreover, in unreported least squares regressions,

[30] When assessing reactions to beliefs, a serious concern is the so-called *false consensus effect* (Ross, 1977; Charness and Dufwenberg, 2006). However, even if this effect was present, it is unlikely to affect our results regarding treatment differences because the false consensus effect should not differ between agent types. Moreover, there are a number of studies that control for the false consensus effect and still find evidence that beliefs cause actions (Frey and Meier, 2004; Reuben et al., 2009).

we find that agents in both treatments tend to choose a lower transfer if they face weak control than otherwise. When they are forced into high effort, agents do not increase their transfers above the level granted to trusting principals (see also Table 2 below).

The result that the crowding-out effect of control is especially pronounced when the control device is weak is in line with the experimental evidence gathered by Falk and Kosfeld (2006) and Leider and Kessler (2012b). However, those two studies both relied on between-subject differences as the principal was never permitted to vary the degree of control. With our design, which extends the principal's action space beyond a simple binary choice, we show that weak control is not suddenly perceived as kind just because strong control is an available choice. Thus while those principals who intrinsically dislike to implement control—but still feel tempted to secure a minimum profit—may perceive a little control as 'psychologically cheap,' the agent's assessment of this control as signaling distrust leads to substantial crowding out of agent effort.

3.3. Treatment Effects in the Hidden Costs of Control

Our third result, which is also the main result of the paper, summarizes our findings regarding a possible interaction between the nature of the principal-agent relationship and the hidden costs of control.

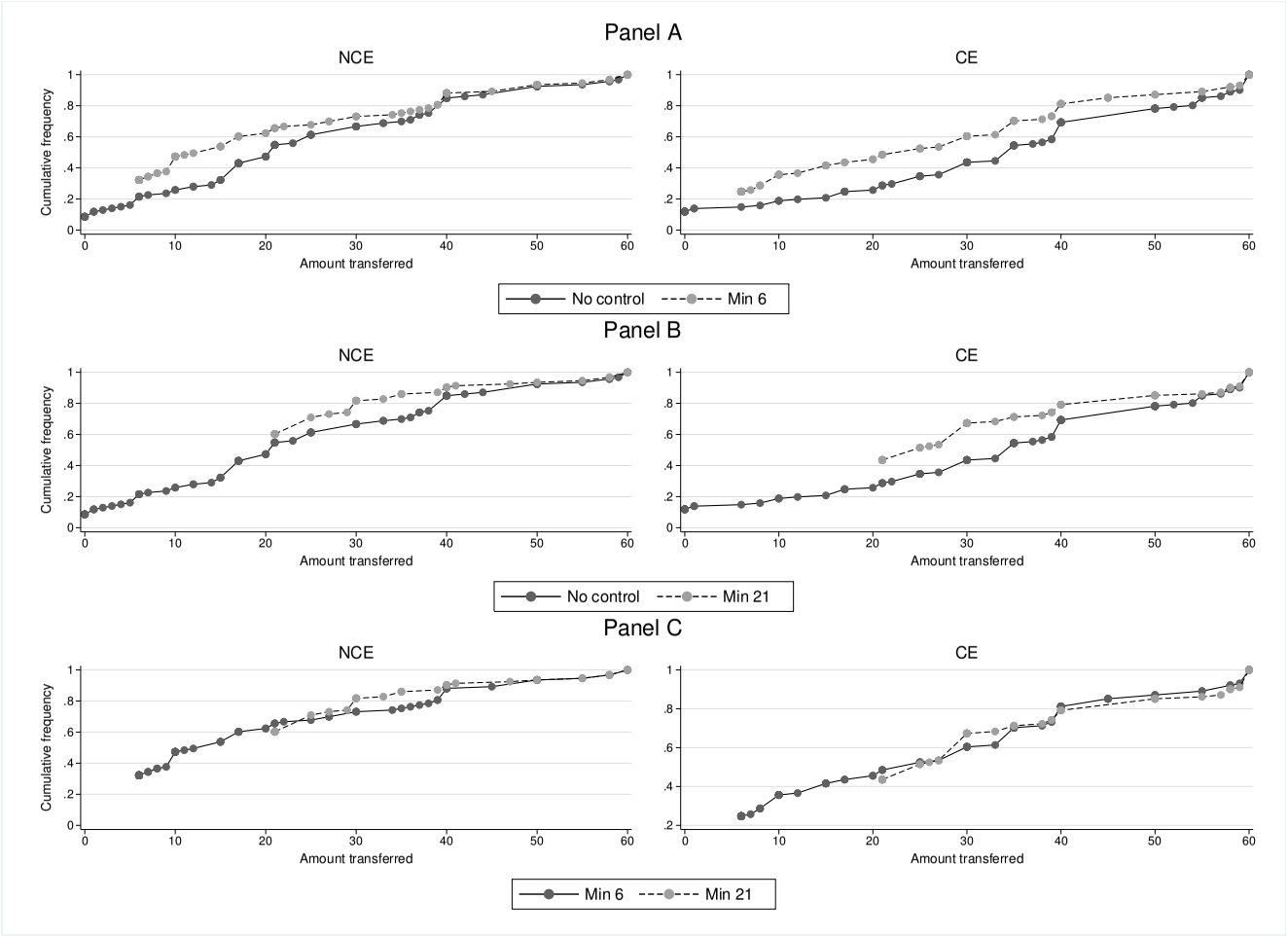
Result 3. *NCE agents and CE agents react differently to control.*

Support: Table 2 contains the results from least squares regressions of the agent's transfer choice (Columns (1) and (2)) or control belief (Columns (3) and (4)) on the principal's control choice (*Min 6* and *Min 21*), a treatment dummy (*CE agent*), and interaction terms between the control levels and the treatment dummy. Control variables encompass the agents' gender, age, experimental experience, and the number of semesters.[31] We further include agents' profits from the initial stage of the experiment to alleviate concerns that our results are driven by positive reciprocity to nice behavior by the principal in the team-building task. Since payoffs of NCE agents and CE agents in the first stage of the experiment can differ, a control for the initial payoffs also ensures that we do not erroneously attribute differences in the behavior of subjects that are actually caused by differences in initial payoffs to group induction.[32] All models include a dummy variable indicating whether the experiment was conducted in Jena or in Dusseldorf.[33] Standard errors are clustered at the levels of individuals and sessions, that is, we allow the error term to be correlated within individuals and within sessions.[34]

In Column (1), we observe that NCE agents opt for a positive transfer if the principal does not use her control option, as indicated by the positive and significant constant (to be interpreted as the

[31] Ploner et al. (2012) find that hidden costs of control are related to subjects' personal characteristics.
[32] We suppress detailed coefficients for brevity.
[33] Due to the inclusion of this laboratory dummy, the point estimates in Column (1) slightly differ from the numbers reported in Figure 1 and Table A.1, which are not conditional on control variables.
[34] We experimented with clustering standard errors at various levels. However, clustering at either the individual level or the session level yielded almost the same standard errors.

Figure 2: Distribution of Transfers in the Effort-Choice Game



Notes: This figure shows the cumulative distribution of transfers for NCE agents (LHS panels) and CE agents (RHS panels). We compare no and weak control (Panel A), no and strong control (Panel B), and weak and strong control (Panel C). To test whether the difference between the distributions is significant, we use the procedure employed by Falk and Kosfeld (2006). That is, when comparing the distribution of transfers in the no control condition with the distribution in the *Min 6* (*Min 21*) condition, we set any transfer strictly lower than 6 (21) equal to 6 (21). Similarly, when comparing the distributions in the *Min 6* and *Min 21* conditions, we set any transfer strictly lower than 21 equal to 21. Note that for each control level there were a few transfers above 60; these observations are summarized as transfer equal to 60.

NCE sample mean if the principal does not control). The coefficients on *Min 6* and *Min 21* show that NCE agents tend to choose a lower transfer if they face weak control than otherwise, and they do not increase their transfers above the level granted to trusting principals when control is strong.

The positive and significant coefficient on *CE agent* indicates that the initial team-building exercise causes agents to reciprocate trust, as CE agents' transfers are around 11 ECUs higher than those made by NCE agents in the absence of control. The interaction term *CE agent* × *Min 6* is negative and significant, which implies that—relative to the baseline of no control—CE agents reduce their transfers by 4 ECUs more than their NCE counterparts do when they face weak control. This result reinforces the above conclusion that exercising weak control backfires on the principal, and additionally shows that the nature of the relationship between the principal and agent alters the magnitude of the hidden costs of (weak) control. The coefficient on *CE agent* × *Min 21* is of similar magnitude as the interaction with weak control, but is less precisely estimated and fails to capture statistical significance. As we show below, the absence of treatment effects when the control device is strong is driven by different reactions to control depending on the outcome of the initial coordination game; while successful coordinators substantially decrease their transfers when facing strong control, unsuccessful coordinators react just like NCE agents (see Section 4.1). These results remain unchanged when adding controls for profits from the group-induction stage and individual heterogeneity (Column (2)).[35]

Consistent with the results for agent effort, we also find treatment effects in the beliefs about control. Relative to their perceived likelihood of facing no control, CE agents attach a 13 percentage points smaller likelihood to weak control and even a 27 percentage points smaller likelihood to strong control than their NCE counterparts (Column (3)). Results are qualitatively similar when we include additional controls (Column (4)).

[35] A closer inspection of our data revealed some outlying observations. For instance, there are two CE agents who—for each control level—transfer the full endowment of 117 ECUs to their principals. One CE agent always transfers 116 ECUs. Two of these subjects even rated her partner's decision in the group-induction task as 'very unfair.' It seems likely that these subjects did not understand the game. Median regressions, which are more robust in the presence of outliers than least squares regressions, suggest even stronger treatment effects than those reported here.

Table 2: Main Treatment Effects in the Effort-Choice Game

Dependent variable:				
	Agent transfer		Agent belief	
	(1)	(2)	(3)	(4)
Constant	25.581***	25.534***	19.505***	20.057***
	(2.502)	(2.618)	(2.212)	(2.244)
Min 6	–3.637***	–3.586***	3.264	2.954
	(1.299)	(1.358)	(2.544)	(2.642)
Min 21	2.670	2.414	38.220***	36.874***
	(1.693)	(1.752)	(4.671)	(4.688)
CE agent	11.170***	10.476***	13.495***	12.121***
	(3.366)	(3.392)	(4.301)	(4.266)
× Min 6	–4.383*	–4.414*	–13.415***	–11.701**
	(2.423)	(2.527)	(5.091)	(5.025)
× Min 21	–4.095	–3.645	–27.068***	–24.663***
	(3.119)	(3.221)	(8.298)	(8.307)
Profits initial round		X		X
Indiv. heterogeneity		X		X
Observations	570	546	570	546
Individuals	190	182	190	182
R-squared	0.06	0.08	0.26	0.26

* p<0.10, ** p<0.05, *** p<0.01

Notes: The table shows the results from least squares regressions. The dependent variable in Columns (1) and (2) is *Agent transfer*, denoting the number of ECUs transferred to the principal by the agent; in Columns (3) and (4), the dependent variable is *Agent belief*, i.e., the agent's perceived likelihood that she will face no, weak, and strong control, respectively. In Columns (1) and (2), coefficients on *Min 6* and *Min 21* indicate the change in agents' transfers when the principal chooses a control level of 6 or 21, relative no control; in Columns (3) and (4), coefficients indicate the change in the perceived likelihood from facing no control to a control level of 6 or 21. The baseline group are *NCE agents*, who performed a slider task in isolation in the first stage of the experiment. *CE agents* completed the team-building task with a partner in the first stage of the experiment. *Profits initial round* denotes agents' profits from the first stage of the experiment. *Indiv. heterogeneity* include gender, age, number of semesters, and experimental experience. Experimental experience is a binary variable that takes the value 1 if a subject participated in an experiment in the past; 0 otherwise. All specifications control for a dummy variable indicating the experimental laboratory (Dusseldorf or Jena). Control variables are de-meaned to facilitate interpretation of the coefficients. 8 agents (4 NCE and 4 CE) did not provide biographic information, so the number of observations decreases to 546, corresponding to 182 agents, when biographic controls are added. Heteroskedasticity-robust standard errors clustered at the levels of individuals and sessions are reported in parentheses.

3.4. Principals' Control Choices and Beliefs

We also find treatment effects in principals' control decisions, as the proportion of principals deciding to control is significantly higher in NCE than in CE (Fisher's exact test, $p = 0.048$). Interestingly, only 4 out of 91 NCE principals decide to trust the agent completely, while 15 out of 99 CE principals do not impose any control; the difference is significant at 5% (Fisher's exact test, $p = 0.015$). Thus, CE principals control less frequently and less severely than their NCE counterparts.

Moreover, the treatment differences in principals' second-order beliefs are consistent with agents' actual beliefs. CE principals find it more likely than NCE principals that their agents expect them to trust (23.29% vs. 10.91%), while the opposite is true for second-order beliefs regarding strong control (49.72% vs. 58.01%). Judging by a Wilcoxon-Mann-Whitney rank-sum test, these

differences are significant at 5% or better. For medium control, second-order beliefs do not differ between treatments. Interestingly, there is only a very weak—and in fact negative—correlation (–0.06) between the agents' control beliefs and the principals' actual choices.

4. Mechanisms

Result 3 suggests that agents who previously interacted with their principals in the group-induction stage seem to be less forgiving of a principal who implements control. In this section we lay out various mechanisms that can potentially explain this result. We start by taking a closer look at the group-induction stage. We first consider the two additional control treatments in which we vary the group-induction process. We then investigate differences in treatment effects by quality of coordination in the group induction. Moreover, using the data from the direct-response method, we check whether the method to elicit agent effort affects our results. Finally, we explore disappointment about the principal's control choices as a further channel through which treatment effects in the hidden costs of control manifest.[36]

4.1. Group-Induction Process

Treatment Manipulation

The initial stage of the experiment differed in several dimensions between NCE and CE, for instance, in terms of cognitive load or environmental cues. If these differences determine behavior in future stages, the treatment effects in Table 2 would be hard to interpret. Moreover, it may also be that just the possibility to provide and receive personal feedback on the outcome of the group-induction stage (vis-à-vis mastering a task jointly with a partner) explains treatment effects. To alleviate these concerns, we conducted two additional control treatments (CER and CEN) whose results are shown in Table 3. In both treatments, agents performed the same team-building task as agents in the CE treatment, but did not send or receive an outcome judgment (nor was the outcome of the task disclosed).

In Columns (1) and (2), CER agents, who were matched with another partner after the team-building task, do not differ in their transfer decisions from NCE agents at any level of control.[37] Thus even though NCE agents initially performed a different task than CER agents that did not involve any interaction with a partner, both agent types make similar transfer choices subsequently. Agents in the CEN treatment, who remained in fixed pairs throughout the experiment, behave differently than CER (and NCE) agents, but strikingly similar to CE agents. Considering raw treatment effects in Column (1), the coefficient on *CEN agent* indicates that in the absence of control CEN agents

[36] Another potential mechanism explaining our results is that higher monetary payoffs gained by subjects who successfully coordinated in the first stage results in reciprocal behavior if the other player was seen as fair or cooperative in the first stage. However, there is no association between the agent's transfer in the effort-choice game and either her realized payoffs in the team-building task or the principal's contribution to the joint account in the team-building task, which is evidence against our findings being driven by a reciprocation motive. Results are available on request.

[37] To be able to directly compare the outcomes from the various common experience treatments, we use the CER treatment as baseline.

transfer 16 ECUs more to their principals than their CER counterparts do (CE agents: 15 ECUs). We also observe that the crowding-out effect induced by strong control is similar for CEN agents and CE agents. The coefficients on *CEN agent* $\times$ *Min 21* and *CE agent* $\times$ *Min 21* in Column (1) show that, compared to their CER counterparts, both CEN agents and CE agents decrease their transfers by 6 ECUs more when forced into strong control. These results suggest that treatment effects are neither caused by participation in the group-induction stage *per se* nor are they just an artifact of the feedback mechanisms implemented in the CE treatment.

Columns (3) and (4) further support these conclusions; CEN and CE agents have very similar control expectations, while the expectations of NCE agents are closer to those held by CER agents. However, since CER agents attach a very high likelihood to strong control, and at the same time almost never expect not to face any control, we even detect significant differences between NCE agents and CER agents.

Table 3: Comparison of Treatments

Dependent variable:				
	Agent transfer		Agent belief	
	(1)	(2)	(3)	(4)
Constant	22.158***	17.466***	5.556***	5.882***
	(5.308)	(5.907)	(1.485)	(1.489)
Min 6	–6.333***	–4.765**	10.833***	11.471***
	(2.075)	(2.424)	(2.186)	(2.121)
Min 21	4.333***	5.647***	72.500***	70.882***
	(0.255)	(1.008)	(6.635)	(6.587)
NCE agent	3.423	8.239	13.950***	14.175***
	(4.997)	(5.567)	(2.643)	(2.532)
× Min 6	2.696	1.178	–7.570**	–8.517**
	(2.376)	(2.724)	(3.288)	(3.400)
× Min 21	–1.663	–3.233	–34.280***	–34.009***
	(1.675)	(2.083)	(7.984)	(7.394)
CEN agent	16.333*	17.543**	20.944***	20.618***
	(8.401)	(8.442)	(6.512)	(6.338)
× Min 6	1.533	–0.035	–17.583***	–18.221***
	(3.518)	(4.285)	(6.103)	(6.607)
× Min 21	–6.183**	–7.497**	–45.250***	–43.632***
	(3.144)	(3.339)	(14.793)	(14.169)
CE agent	14.593***	18.780***	27.444***	26.297***
	(5.405)	(6.258)	(3.226)	(3.347)
× Min 6	–1.687	–3.235	–20.985***	–20.218***
	(3.060)	(3.449)	(4.267)	(3.877)
× Min 21	–5.758**	–6.879**	–61.348***	–58.672***
	(2.740)	(3.169)	(8.889)	(9.224)
Profits initial round		X		X
Indiv. heterogeneity		X		X
Observations	684	657	684	657
Individuals	228	219	228	219
R-squared	0.07	0.08	0.34	0.33

* p<0.10, ** p<0.05, *** p<0.01

Notes: The table shows the results from least squares regressions. The baseline group are *CER agents*. The CER treatment ($n = 18$) differs from CE insofar that subjects changed the partner after the initial team-building exercise and that they were neither allowed to provide nor to receive a rating of the other player's action during that exercise. Neither did we reveal the players' actions. The CEN treatment ($n = 20$) is similar to CER, the only difference being that *CEN agents* remained in fixed pairs throughout the experiment. In the even columns, specifications control for agents' profits from the first stage of the experiment and for individual heterogeneity (gender, age, number of semesters, experimental experience). All specifications control for a dummy variable indicating the experimental laboratory (Dusseldorf or Jena). Control variables are de-meaned. See Table 2 for a further description of the variables. 8 agents (4 NCE and 4 CE) did not provide biographic information. Hence the number of observations in the even columns decreases to 657, corresponding to 219 agents. Heteroskedasticity-robust standard errors clustered at the levels of individuals and sessions are reported in parentheses.

Quality of Coordination

The previous discussion shows that just participating in the initial team-building exercise is, by itself, not sufficient to explain the stronger negative reaction of CE agents to control. We now investigate whether the observed treatment effects are driven by those subjects who successfully

coordinated in the team-building exercise. Table 4 shows that 140 out of 198 players (71%) rated each other as either 'fair' or 'very fair.' For these individuals, we are confident that group formation was successful. We denote subjects with a mutual fairness rating of at least 'fair' as *CE successful*.[38] However, the non-negligible fraction of pairs in which at least one player rated her partner's decision with 'neutral' or less suggests that our team-building task did not always render group identity salient. We refer to subjects in pairs with at least one fairness rating below 'fair' as *CE unsuccessful*.[39]

Table 4: Player's Satisfaction with Partner in the CE Treatment

	Very unfair (=1)	**Unfair (=2)**	**Neutral (=3)**	**Fair (=4)**	**Very fair (=5)**	***Total***
Very unfair (=1)	**0**	0	1	1	7	*9*
Unfair (=2)		**2**	0	1	6	*9*
Neutral (=3)			**4**	2	8	*15*
Fair (=4)				**2**	10	*16*
Very fair (=5)					**118**	*149*
Total						***198***

Notes: This table shows all combinations of a player's rating of the other player's allocation decision in the team-building stage of the CE treatment. The polar cases of the player's decision options were described as 'very unfair' and 'very fair,' respectively, on the screen, while the remaining choices (i.e., 2–4) only appeared as natural numbers. For the sake of exposition, this table contains both the number and description associated with each satisfaction rating. The players received feedback on their partner's satisfaction rating before proceeding to the next stage. One-hundred-ninety-eight subjects (99 pairs of principals and agents) participated in the CE treatment. Since the table is symmetric, numbers below the diagonal are not shown to enhance clarity.

The results in Table 5 show—for CE agents who mastered the team-building task—significant treatment differences in the reactions to both weak and strong control. At the same time, these agents highly reward a trusting principal, as indicated by the positive coefficient on *CE agent* (Column (2)). For CE agents who did not coordinate successfully, and for whom it is unlikely that our pre-play manipulation induced a sense of group belonging, none of the treatment effects is significant (Column (3)). In fact, the interaction term *CE agent* $\times$ *Min 21* is even directionally positive, which explains why we could not detect significant treatment effects for strong control in the full sample (shown again in Column (1)). The results on agent beliefs in Columns (4)–(6) are consistent with the observed transfer pattern.

[38]The results shown below continue to hold when we define CE successful subjects as those sending and receiving the highest possible fairness rating of 'very fair.' This pertains to 60% of the cases.

[39]In Appendix B, we show that the mutual fairness judgment is a good indicator of the quality of coordination (that is, the mutual payoff) in the team-building task.

Table 5: Is Quality of Coordination Related to Behavior in the Effort-Choice Game?

Dependent variable:						
	Agent transfer			Agent belief		
	All	Successful coordinat.	Unsuccessful coordinat.	All	Successful coordinat.	Unsuccessful coordinat.
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	25.534***	25.706***	25.566***	20.057***	20.057***	20.057***
	(2.618)	(2.671)	(2.545)	(2.244)	(2.244)	(2.244)
Min 6	–3.586***	–3.586***	–3.586***	2.954	2.954	2.954
	(1.358)	(1.358)	(1.358)	(2.642)	(2.642)	(2.642)
Min 21	2.414	2.414	2.414	36.874***	36.874***	36.874***
	(1.752)	(1.752)	(1.752)	(4.688)	(4.688)	(4.688)
CE agent	10.476***	11.659***	3.964	12.121***	16.211***	2.335
	(3.392)	(3.361)	(7.821)	(4.266)	(4.795)	(5.238)
× Min 6	–4.414*	–5.593*	–1.592	–11.701**	–16.476***	–0.275
	(2.527)	(3.039)	(4.107)	(5.025)	(5.760)	(6.714)
× Min 21	–3.645	–8.638***	8.300	–24.663***	–32.157***	–6.731
	(3.221)	(2.683)	(6.988)	(8.307)	(9.236)	(9.957)
Profits initial round	X	X	X	X	X	X
Indiv. heterogeneity	X	X	X	X	X	X
Observations	546	462	345	546	462	345
Individuals	182	154	115	182	154	115
R-squared	0.08	0.08	0.09	0.26	0.28	0.35

* p<0.10, ** p<0.05, *** p<0.01

Notes: The table shows the results from least squares regressions. The baseline group are *NCE agents*, who performed a slider task in isolation in the first stage of the experiment. In Columns (1) and (4), we consider all CE agents (results are the same as in Columns (2) and (4) of Table 2). In Columns (2) and (5), we restrict the sample of CE agents to agents who successfully coordinated with their principals in the initial team-building task (*CE successful*). We deem coordination as successful if the mutual fairness rating within a principal-agent group was either 'fair' or 'very fair' ($n = 70$). In Columns (3) and (6), the sample of CE agents consists of agents who mis-coordinated in the team-building exercise, meaning that at least one player within a group rated her partner's fairness as less than 'fair' ($n = 29$) (*CE unsuccessful*). All specifications control for a dummy variable indicating the experimental laboratory (Dusseldorf or Jena), agents' profits from the first stage of the experiment, and individual heterogeneity (gender, age, number of semesters, experimental experience). See Table 2 for a further description of control variables. Control variables are de-meaned. 8 agents (4 NCE and 4 CE) did not provide biographic information. Heteroskedasticity-robust standard errors clustered at the levels of individuals and sessions are reported in parentheses.

One should be aware that assignment to CE successful and CE unsuccessful is not random and may be subject to selection effects. For instance, a low fairness rating may simply indicate weak social preferences, while subjects with a high fairness rating may be (conditionally) cooperative. However, learning about the social preferences of the partner is very limited in the case of successful coordination. Since it is strictly payoff-maximizing to coordinate on the focal point in the weakest-link game, a subject's decision to transfer the full endowment to the group account may not necessarily be interpreted by the other player as an act of kindness or cooperativeness, but may just as well be regarded as selfish behavior (see von Siemens, 2013, for a related argument). We therefore just interpret the above results as being suggestive of the role of successful coordination in the group-induction task for treatment effects in the reaction to control.

4.2. Elicitation of Effort: Direct-Response Method

To gain more confidence that our results are not just an artifact of the elicitation method, we now present the findings from the real-effort stage, where we elicited agents' transfer decisions using the direct-response method. However, since the real-effort task always took place after the effort-choice game, we cannot rule out the existence of spill-over effects between both experiments. As explained in Section 2, we tried to alleviate this concern by altering the decision frame.

Table 6 reports the results of least squares regressions of the agent's transfer and control belief, respectively, on the level of control exercised by the principal.[40] Transfers are expressed as the percentage of profits from solving equations that the agent is willing to share with the principal.[41] Since we have few observations for small control, an analysis attempting to identify treatment effects separately for each control level is likely to be underpowered. For this reason, we use the level of control as a continuous variable in Columns (1)–(3). Column (4) contains dummy variables for each possible control level to investigate differential responses to weak vs. strong control despite few observations per control cell. In addition to biographic variables and profits from the initial stage of the experiment, the regressions also include agent transfers from the effort-choice game to account for behavioral patterns that are constant across games.

When control is included as a continuous variable, we observe that the fraction of earnings that NCE agents transfer to their principals increases in the level of control; the coefficient on *Control* in Columns (1)–(3) is positive and significant. However, the results in Column (4) again indicate that control entails hidden costs that, at least for weak control, are sufficiently high to outweigh the benefits. The negative and significant coefficient on *10%* implies that NCE agents reduce their transfers to the principal by 12 ECUs when the control level increases from 0% to 10%. Only when a high effort (40%) can be enforced, the disciplining effect of control dominates the crowding-out effect, leading to an increase in agents' transfers. But since almost 60% of the NCE principals exercised the maximum control level of 40%, a continuous measure of control has a significantly positive coefficient.

Agents who passed through the group-induction stage have a stronger inclination to reciprocate no control than NCE agents; the coefficient on *CE agent* is positive across specifications, being significant when control is measured as a continuous variable (Columns (1)–(3)). The interaction term *CE agent* $\times$ *Control* is negative and significant, indicating that, conditional on being controlled, CE agents reduce their transfers by a larger amount than do NCE agents or react less positively to high levels of control. These treatment effects are directionally similar when dummies for each control level are included, but fail to capture statistical significance due to the small number of

[40] Table A.2 provides summary statistics of agents' transfers and beliefs in the real-effort game. We observe that our findings from the effort-choice game also carry over to the real-effort context: CE agents transfer more to their principals than do NCE agents at *any* control level and expect to be controlled less often than their NCE counterparts.

[41] We ignore agents' actual performance in solving sums so as to ensure that effort is still fully under volitional control (van Knippenberg, 2000), just as in the effort-choice game. Moreover, we cannot rule out that the decision on the share of the agent's income to be transferred to the principal alters the marginal incentives for performing the real-effort task.

observations in some control cells (Column 4).[42]

Using agent beliefs as outcome in Columns (5)–(8), we observe robust treatment effects across specifications: CE agents consistently attach a higher likelihood to facing no control and a lower likelihood to facing control (in particular, maximum control) than NCE agents do.[43] We cautiously interpret these findings as showing that treatment differences in the reaction to control are robust across different methods of eliciting agent effort.

4.3. Disappointment

A final mechanism potentially driving treatment differences in the magnitude of hidden costs of control is that CE agents attribute a higher emotional importance to control levels that exceed their expectations. To investigate this mechanism, we introduce the concept of sensation in our setup. We define 'sensation' as the deviation of the agent's modal control belief from the experienced level of control. Since the control level is salient only in the real-effort game, we study the effects of control sensations in the real-effort environment. If the level of control implemented by the principal exceeds the agent's expectation, we think of her as being disappointed by the principal's control decision. In contrast, if the principal's control is below the agent's expectation, the agent may be happily surprised.

In Table 7, we estimate least squares regressions to explore how the agent's response to sensation differs by treatment and by the nature of the sensation. The dependent variable is the agent's transfer to the principal over and above the minimum requirement (*Agent voluntary transfer*).[44]

NCE agents do not appear to react to negative sensations; the coefficient on *Negative sensation* is small and insignificant once controls are added. However, there is a negative and significant interaction effect, which suggests that CE agents who experience a negative control sensation decrease their voluntary transfers to a greater degree than do corresponding NCE agents. In fact, CE agents reciprocate negative sensations; for instance, when including controls for demographic characteristics and decisions from previous stages of the experiment in Column (3), the main effect is negative (equal to -0.20) and significant at 5% ($p = 0.025$). This substantiates our claim that disappointment is a relevant determinant of differences between agent types in the response to control.

Positive sensation carries a significantly positive coefficient, meaning that the larger the (positive) difference between expected and experienced control, the more NCE agents voluntarily transfer to their principals. The coefficient on *CE agent* $\times$ *Positive sensation* is insignificant, which shows that CE agents reciprocate positive sensations in a similar manner as do NCE agents. Thus treat-

[42] Specifically, the baseline of no control—to which all other coefficients are compared—is just based on two observations (NCE) and six observations (CE), respectively. See upper panel in Table A-2.

[43] Note that the problem of having too few observations per control cell does not exist when considering agent beliefs, since agents attach a likelihood to *all* control levels available for the principal to choose. Accordingly, treating control as a continuous variable or as a factor variable leads to similar results.

[44] We use voluntary transfers instead of total transfers as outcome because the construction of our sensation variable does not permit accounting for the principal's control choice in the regression analysis. We thus need a measure that does not directly depend on the principal's control decision.

Table 6: Main Treatment Effects in the Real-Effort Game

Dependent variable:								
	Agent transfer				Agent belief			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	12.588***	13.136***	13.168***	30.238***	–3.495	–4.827	–4.761	9.845
	(3.815)	(3.531)	(3.557)	(6.278)	(4.945)	(5.745)	(5.209)	(7.976)
Control	0.692***	0.692***	0.696***		1.211***	1.215***	1.185***	
	(0.105)	(0.097)	(0.099)		(0.192)	(0.199)	(0.185)	
CE agent	11.549***	9.866***	9.251**	7.442	17.311**	21.247***	21.531***	21.805**
	(3.830)	(3.345)	(3.389)	(6.555)	(6.468)	(7.501)	(7.272)	(9.971)
× Control	–0.256**	–0.238**	–0.222**		–0.596**	–0.636**	–0.649**	
	(0.102)	(0.096)	(0.099)		(0.253)	(0.249)	(0.242)	
Min 10%				–12.216**				8.780
				(5.445)				(9.192)
Min 20%				–5.050				5.583
				(6.482)				(8.395)
Min 40%				11.235*				33.528***
				(6.176)				(8.355)
CE agent × Min 10%				–6.209				–23.383
				(5.670)				(13.768)
CE agent × Min 20%				–2.393				–13.164
				(6.661)				(8.915)
CE agent × Min 40%				–7.040				–26.112**
				(6.339)				(11.003)
Profits initial round		X	X	X		X	X	X
Transfer effort-choice game		X	X	X		X	X	X
Indiv. heterogeneity			X	X			X	X
Individuals	190	190	182	182	190	190	182	182
R-squared	0.44	0.51	0.53	0.67	0.20	0.25	0.28	0.30

* p<0.10, ** p<0.05, *** p<0.01

Notes: The table contains the results of least squares regressions. In Columns (1)–(3), the dependent variable is *Agent transfer*, i.e., how much of the remuneration from solving equations the agent transfers to the principal; in Columns (4)–(6), the dependent variable is *Agent belief*, i.e., the likelihood the agent attaches to control levels of 0%, 10%, 20%, and 40%, respectively. *Control* is a continuous measure of the principal's control choice. The coefficients on *Min 10%*, *Min 20%*, and *Min 40%* indicate the change in the agent's transfer when the principal chooses a control level of 10%, 20%, and 40%, respectively, relative to the baseline case of no control. Depending on the specification, we account for agents' decisions from previous stages of the experiment and individual heterogeneity. All specifications control for a dummy variable indicating the experimental laboratory (Dusseldorf or Jena). All control variables are de-meaned. See Table 2 for details on the control variables. 8 agents (4 NCE and 4 CE) did not provide biographic information; thus the number of observations in Columns (3), (4), (7), and (8) decreases from 190 to 182. Heteroskedasticity-robust standard errors clustered at the session level are shown in parentheses.

ment effects in hidden costs of control cannot be explained by positive control sensations.[45]

Table 7: Effect of Control Sensations

Dependent Variable: Agent voluntary transfer			
	(1)	(2)	(3)
Constant	–0.175	0.782	1.241
	(0.976)	(1.068)	(1.031)
CE agent	9.107***	8.138***	7.790***
	(2.381)	(2.087)	(2.105)
Negative sensation	0.084*	0.045	0.037
	(0.044)	(0.054)	(0.060)
× CE agent	–0.180*	–0.228**	–0.233**
	(0.097)	(0.096)	(0.100)
Positive sensation	0.332***	0.337***	0.331***
	(0.112)	(0.102)	(0.103)
× CE agent	–0.255	–0.231	–0.219
	(0.171)	(0.164)	(0.164)
Profits initial round		X	X
Transfer effort-choice game		X	X
Indiv. heterogeneity			X
Individuals	175	175	167
R-squared	0.13	0.26	0.28

* p<0.10, ** p<0.05, *** p<0.01

Notes: The table shows the results of least squares regressions. The dependent variable is *agent voluntary transfer*, i.e., how much of the remuneration from solving equations the agent transfers to the principal on top of the minimum level required by the principal. *Sensation* is measured as the difference between expected and actual control, with expected control being approximated by the agent's modal control belief. *Negative sensation* indicates the absolute level of the sensation if the sensation is strictly below zero; otherwise, the variable equals 0. *Positive sensation* is defined similarly; that is, it exhibits non-zero (and positive) values if the sensation is strictly above zero. In Columns (2) and (3), we control for agents' previous decisions using profits/transfers from Stages 1 and 2 of the experiment, respectively. In Column (3), we additionally control for individual heterogeneity. All specifications control for a dummy variable indicating the experimental laboratory (Dusseldorf or Jena). All control variables are de-meaned. See Table 2 for a description of the control variables. In Columns (1) and (2), the total number of observations is 175 because 15 agents (7 NCE and 8 CE) did not have a modal belief. In Column (3), the number of observations is 167 because 8 agents (4 NCE and 4 CE) did not provide biographic information. Heteroskedasticity-robust standard errors clustered at the session level are shown in parentheses.

The above evidence is suggestive that treatment differences in hidden costs of control materialize through disappointment. A behavioral explanation for this result could be that subjects who cooperated in the group-induction task at the beginning of the experiment have some affinity or esteem for one another.[46] If the principal controls excessively (as perceived by the agent) in a later stage of the experiment, she is interpreted as holding the agent in low esteem. However, this hurts

[45] A note of caution against interpreting these results causally is again warranted due to the potential endogeneity of control beliefs.

[46] Ellingsen and Johannesson (2008) use the concept of esteem in a related context. They argue that individuals always strive for esteem, which influences the incentive structure in principal-agent interactions. However, we attempt to induce esteem exogenously through the team-building activity, which is different from the approach taken by Ellingsen and Johannesson.

the agent more in the CE treatment because the relative drop in esteem is larger than in the NCE treatment due to inflated expectations from Stage 1. CE agents, in turn, retaliate against their principals for the perceived lack of trust. Such a phenomenon of retaliation is reported in experimental economics by Irlenbusch and Sliwka (2005) and Dickinson and Villeval (2008) and in the psychological literature by Koehler and Gershoff (2003) and Sanfey (2009). Moreover, as discussed by Goette et al. (2012), social ties between group members can be easily dissolved and replaced with the desire for punishment if a group member's behavior is seen as incoherent with the implied group identity.

5. Conclusions

Firms typically engage in several personnel management strategies when attempting to increase worker performance. Since firms use these strategies simultaneously and in various intensities, identifying their separate effects and, especially, their interaction poses an identification problem that is hard to overcome with observational data. We therefore conducted a laboratory experiment to investigate the impact of two very popular management practices on individual behavior: team building and control. While previous research deals with team building—and group identity based thereon—and control separately, almost nothing is known about how these strategies interact.

We find that agents who initially engaged in a team-building task (CE treatment) exert higher effort than agents who did not (NCE treatment) for all control levels from which the principal can choose. This finding is consistent with earlier studies showing that membership in social groups tends to enhance pro-social behavior. However, conditional on being controlled, CE agents reduce their transfers more strongly than do NCE agents, so the effectiveness of control as an incentive scheme is lower when team building is simultaneously applied.

Our results further suggest that the interaction of social distance with hidden costs of control can be attributed to a 'disappointment effect.' Compared to their NCE counterparts, CE agents seem to attach higher emotional significance to the principal's control decision, so they appear to be more disappointed when the principal's control exceeds their expectations. When such negative control sensation occurs, CE agents considerably reduce their transfers, a phenomenon not observed for NCE agents. In addition, the quality of interaction in the team-building task affects treatment differences: if CE agents did not have a positive experience with their principal in the initial task, they behave just like NCE agents.

Most previous studies that investigate work motivation or task performance in social groups find evidence in support of a positive impact of groups on motivational and performance-related factors (for an overview, see van Knippenberg, 2000). Our results uncover another facet of groups. We find that group identity facilitates the sanctioning of group members not acting in accordance with

their fellows' beliefs or norms.[47] In situations with repeated principal-agent interactions, behavior that is regarded as incongruent with the implied group identity may also corrode previous positive experiences, just as unexpectedly kind behavior may strengthen social ties. This is a promising avenue for future research.

Our findings also have important implications for personnel economics because they emphasize the virtues of a consistent leadership style. When a firm decides to impose extrinsic incentives through control devices or related instruments, a concurrent attempt to increase intrinsic motivation—for instance, via team-building exercises—is not advisable. In fact, the strategies appear to be substitutable in our study. The results also suggest that firms engaged in team-building activities should set the threshold for success relatively low to ensure a high success rate. However, too low a boundary may wipe out the positive effect of team building on intrinsic motivation, as participants realize that they do not learn anything about their fellows. It would thus be instructive to analyze more systematically the interactions between intrinsic and extrinsic incentives in a real-world environment, perhaps through experimental manipulation of team-building exercises within firms. Moreover, our one-shot design clearly comes at the cost of decreased external validity because in many real settings, especially at work, coordination and monitoring activities are based on repeated interactions. Extending our setup to repeated interactions as in Dickinson and Villeval (2008) would thus be an interesting avenue for further research.

[47] Note that our results are not in line with the individual behavior conjectured in Akerlof and Kranton (2005). They assume that strict supervision alters the nature of the principal-agent relationship, in the sense that excessive control will suddenly return CE agents to a state that is no different from an NCE relationship. Our findings imply that supervision or control does not necessarily erode previous experiences of the principal and agent; rather, experiencing control seems to evoke even harsher negative feelings in the CE agent, leading to a particularly strong decrease in transfers.

References

Akerlof, G.A., Kranton, R.E., 2005. Identity and the economics of organizations. Journal of Economic Perspectives 19, 9–32.

Akerlof, G.A., Kranton, R.E., 2008. Identity, supervision, and work groups. American Economic Review Papers and Proceedings 98, 212–217.

Albert, S., Whetten, D.A., 1985. Organizational identity, in: Cummings, L.L., Staw, B.M. (Eds.), Research in organizational behavior. Greenwich, CT: JAI Press., pp. 263–295.

Alchian, A.A., Demsetz, H., 1972. Production, information costs, and economic organization. American Economic Review 62, 777–795.

Antonakis, J., Atwater, L., 2002. Leader distance: A review and a proposed theory. Leadership Quarterly 13, 673–704.

Arkes, H.R., Joyner, C.A., Pezzo, M., Nash, J.G., Siegel-Jacobs, K., Stone, E., 1994. The psychology of windfall gains. Organizational Behavior and Human Decision Processes 59, 331–347.

Ashforth, B.E., Mael, F., 1989. Social identity theory and the organization. The Academy of Management Review 14, 20–39.

Barkema, H.G., 1995. Do top managers work harder when they are monitored? Kyklos 48, 19–42.

Bartling, B., Fehr, E., Schmidt, K.M., 2012. Screening, competition, and job design: Economic origins of good jobs. American Economic Review 102, 834–864.

Brandts, J., Charness, G., 2011. The strategy versus the direct-response method: A first survey of experimental comparisons. Experimental Economics 14, 375–398.

Brewer, M.B., 1979. In-group bias in the minimal intergroup situation: A cognitive-motivational analysis. Psychological Bulletin 86, 307–324.

Bruggen, A., Strobel, M., 2007. Real effort versus chosen effort in experiments. Economics Letters 96, 232–236.

Buller, P.F., Bell Jr., C.H., 1986. Effects of team building and goal setting on productivity: A field experiment. Academy of Management Journal 29, 305–328.

Chaney, C.K., Saltzstein, G.H., 1998. Democratic control and bureaucratic responsiveness: The police and domestic violence. American Journal of Political Science 42, 745–768.

Charness, G., 2012. Efficiency, team building, and spillover in a public-goods game. Departmental Working Papers, Department of Economics, UCSB, UC Santa Barbara.

Charness, G., Dufwenberg, M., 2006. Promises and partnership. Econometrica 74, 1579–1601.

Charness, G., Frechette, G.R., Kagel, J.H., 2004. How robust is laboratory gift exchange? Experimental Economics 7, 189–205.

Charness, G., Kuhn, P.J., 2011. Lab labor: what can labor economists learn from the lab?, in: Ashenfelter, O., Card, D. (Eds.), Handbook of Labor Economics. Elsevier. volume 4a, pp. 229–330.

Charness, G., Rigotti, L., Rustichini, A., 2007. Individual behavior and group membership. American Economic Review 97, 1340–1352.

Chen, R., Chen, Y., 2011. The potential of social identity for equilibrium selection. American Economic Review 101, 2562–2589.

Chen, Y., Li, S.X., 2009. Group identity and social preferences. American Economic Review 99, 431–457.

Cherry, T.L., Frykblom, P., Shogren, J.F., 2002. Hardnose the dictator. American Economic Review 92, 1218–1221.

Crawford, V., 1998. A survey of experiments on communication via cheap talk. Journal of Economic Theory 78, 286–298.

van Dick, R., Christ, O., Stellmacher, J., Wagner, U., Ahlswede, O., Grubba, C., Hauptmeier, M., Hoehfeld, C., Moltzen, K., Tissington, P.A., 2004. Should i stay or should i go? explaining turnover intentions with organizational identification and job satisfaction. British Journal of Management 15, 1467–8551.

Dickinson, D., Villeval, M.C., 2008. Does monitoring decrease work effort? the complementarity between agency and crowding-out theories. Games and Economic Behavior 63, 56–76.

van Dijk, F., van Winden, F., 1997. Dynamics of social ties and local public good provision. Journal of Public Economics 94, 323–341.

Dufwenberg, M., Muren, A., 2006. Generosity, anonymity, gender. Journal of Economic Behavior & Organization 61, 42–49.

Eckel, C., Grossman, P., 2005. Managing diversity by creating team identity. Journal of Economic Behavior and Organization 58, 371–392.

Ellingsen, T., Johannesson, M., 2008. Pride and prejudice: The human side of incentive theory. American Economic Review 98, 990–1008.

Enzle, M.E., Anderson, S.C., 1993. Surveillant intentions and intrinsic motivation. Journal of Personality and Social Psychology 64, 257–266.

Falk, A., Kosfeld, M., 2006. The hidden costs of control. American Economic Review 96, 1611–1630.

Falk, A., Zehnder, C., 2013. A city-wide experiment on trust discrimination. Journal of Public Economics 100, 15–27.

Fehr, E., List, J.A., 2004. The hidden costs and returns of incentives – trust and trustworthiness among ceos. Journal of the European Economic Association 2, 743–771.

Fehr, E., Rockenbach, B., 2003. Detrimental effects of sanctions on human altruism. Nature 422, 137–140.

Fershtman, C., Gneezy, U., 2001. Discrimination in a segmented society: An experimental approach. The Quarterly Journal of Economics 116, 351–377.

Fischbacher, U., 2007. z-Tree: zurich toolbox for ready-made economic experiments. Experimental Economics 10, 171–178.

Frey, B.S., 1993. Does monitoring increase work effort? the rivalry with trust and loyalty. Economic Inquiry 31, 663–670.

Frey, B.S., Meier, S., 2004. Social comparisons and pro-social behavior: Testing 'conditional cooperation' in a field experiment. American Economic Review 94, 1717–1722.

Friedman, D., Massaro, D., 1997. Understanding variability in binary and continuous choice. Psychological Bulletin Review 5, 370–389.

Geister, S., Konradt, U., Hertel, G., 2006. Effects of process feedback on motivation, satisfaction, and performance in virtual teams. Small Group Research 37, 459–489.

Gill, D., Prowse, V.L., 2012. A structural analysis of disappointment aversion in a real effort competition. American Economic Review 102, 469–503.

Goette, L., Huffman, D., Meier, S., 2012. The impact of social ties on group interactions: Evidence from minimal groups and randomly assigned real groups. American Economic Journal: Microeconomics 4, 101–115.

Greiner, B., 2004. An online recruitment system for economic experiments. GWDG Bericht 63, 79–93.

Guarino, A., Huck, S., Jeitschko, T., 2006. Averting economic collapse and the solipsism bias. Games and Economic Behavior 57, 264–285.

Gueth, W., Huck, S., Mueller, W., 2001. The relevance of equal splits in ultimatum games. Games and Economic Behavior 37, 161–169.

Hagemann, P., 2007. What's in a frame? comment on: The hidden costs of control. Mimeo.

Heap, S.P.H., Zizzo, D.J., 2009. The value of groups. American Economic Review 99, 295–323.

Irlenbusch, B., Sliwka, D., 2005. Incentives, Decision Frames and Crowding Out: An Experimental Investigation. Technical Report 1758. IZA.

Ivanov, A., 2011. Attitudes to ambiguity in one-shot normal-form games: An experimental study. Games and Economic Behavior 71, 366–394.

Jensen, M.C., Meckling, W.H., 1976. Theory of the firm: Managerial behavior, agency costs and ownership structure. Journal of Financial Economics 3, 305–360.

van Knippenberg, D., 2000. Work motivation and performance: A social identity perspective. Applied Psychology 49, 357–371.

van Knippenberg, D., van Schie, E.C.M., 2000. Foci and correlates of organizational identification. Journal of Occupational and Organizational Psychology 73, 137–147.

Koehler, J.J., Gershoff, A.D., 2003. Betrayal aversion: When agents of protection become agents of harm. Organizational Behavior and Human Decision Processes 90, 244–261.

Leider, S., Kessler, J., 2012a. Norms and contracting. Management Science 58, 62–77.

Leider, S., Kessler, J.B., 2012b. Finding the cost of control. Mimeo.

Marks, M.A., Mathieu, J.E., Zaccaro, S.J., 2001. A temporally based framework and taxonomy of team processes. Academy of Management Review 26, 356–376.

Masella, P., Meier, S., Zahn, P., 2014. Incentives and group identity. Games and Economic Behavior 86.

McDougall, W., 1920. The Group Mind. Cambridge: Cambridge University Press.

Niederle, M., Vesterlund, L., 2007. Do women shy away from competition? do men compete too much? Quarterly Journal of Economics 122, 1067–1101.

Offerman, T., Sonnemans, J., Kuilen, G.V.D., Wakker, P.P., 2009. A truth serum for non-bayesians: Correcting proper scoring rules for risk attitudes. Review of Economic Studies 76, 1461–1489.

Ortmann, A., Fitzgerald, J., Boeing, C., 2000. Trust, reciprocity, and social history: A re-examination. Experimental Economics 3, 81–100.

Pan, X.S., Houser, D., 2013. Cooperation during cultural group formation promotes trust towards member of out-groups. Proceedings of the Royal Society B 280. DOI: 10.1098/rspb.2013.0606.

Ploner, M., Schmelz, K., Ziegelmeyer, A., 2012. Hidden costs of control: Three repetitions and an extension. Experimental Economics 15, 323–340.

Reuben, E., Sapienza, P., Zingales, L., 2009. Is mistrust self-fulfilling? Economics Letters 104, 89–91.

Riener, G., Wiederhold, S., 2013. Heterogeneous treatment effects in groups. Economics Letters 120, 408–412.

Ross, L., 1977. The "false consensus effect": An egocentric bias in social perception and attribution processes. Journal of Experimental Social Psychology 13, 279–301.

Salas, E., Rozell, D., Mullen, B., Driskell, J.E., 1999. The effect of team building on performance: An integration. Small Group Research 30, 309–329.

Sanfey, A.G., 2009. Expectations and social decision-making: biasing effects of prior knowledge on ultimatum responses. Mind & Society 8, 93–107.

Schnedler, W., Vadovic, R., 2011. Legitimacy of control. Journal of Economics & Management Strategy 20, 985–1009.

Scholz, J.T., 1991. Cooperative regulatory enforcement and the politics of administrative effectiveness. American Political Science Review 85, 115–136.

Shandler, M., Egan, M., 1996. Vroom! Turbo-charged team building. AMACOM, New York.

von Siemens, F., 2013. Intention-based reciprocity and the hidden costs of control. Journal of Economic Behavior and Organization 92, 55–65.

Sliwka, D., 2007. Trust as a signal of a social norm and the hidden costs of incentive schemes. American Economic Review 97, 999–1012.

Sonnemans, J., Offerman, T., 2001. Is the quadratic scoring rule really incentive compatible? Working Paper. University of Amsterdam.

Stanton, J.M., 2000. Reactions to employee performance monitoring: Framework, review, and research directions. Human Performance 13, 85–113.

Staw, B.M., Epstein, L.D., 2000. What bandwagons bring: Effects of popular management techniques on corporate performance, reputation, and ceo pay. Administrative Science Quarterly 45, 523–556.

Sundstrom, E., De Meuse, K.P., Futrell, D., 1990. Work teams: Applications and effectiveness. American Psychologist 45, 120.

Sutter, M., 2009. Individual behavior and group membership: Comment. American Economic Review 99, 2247–2257.

Tajfel, H. (Ed.), 1978. Differentiation between social groups. Studies in the social psychology of intergroup relations. Academic Press, Oxford.

Thaler, R., Johnson, E., 1990. Gambling with the house money and trying to break even: The effects of prior outcomes on risky choice. Management Science 36, 643–660.

Weber, R.A., Camerer, C.F., 2003. Cultural conflict and merger failure: An experimental approach. Management Science 49, 400–415.

Woodman, R.W., Sherwood, J.J., 1980. The role of team development in organizational effectiveness: A critical review. Psychological Bulletin 88, 166–186.

Worchel, S., Rothgerber, H., Day, E.A., Hart, D., Butemeyer, J., 1998. Social identity and individual productivity within groups. British Journal of Social Psychology 37, 389–413.

Appendix A. Additional Descriptives

Table A.1: Main Treatments: Summary Statistics of Agents' Transfers and Beliefs in the Effort-Choice Game

		Agents								
		Panel A Full Sample			**Panel B Jena Sample**			**Panel C Dusseldorf Sample**		
Transfer		*NCE*	*CE*	*Diff.*	*NCE*	*CE*	*Diff.*	*NCE*	*CE*	*Diff.*
if Min 0	Average	25.38	36.56	-11.18***	26.65	36.87	-10.22***	17.08	34.46	-17.38**
	Median	21	35		23	35		15	33	
if Min 6	Average	21.75	28.53	-6.78**	22.73	28.09	-5.36	15.25	31.46	-16.21**
	Median	15	25		17	25		9.50	27	
if Min 21	Average	28.05	35.13	-7.08***	28.39	34.01	-5.62**	25.83	42.54	-16.71*
	Median	21	25		21	25		21	30	
Belief		*NCE*	*CE*	*Diff.*	*NCE*	*CE*	*Diff.*	*NCE*	*CE*	*Diff.*
Min 0	Average	19.51	33.00	-13.49***	21.47	31.77	-10.30**	6.58	41.15	-34.57***
	Median	10	30		20	25		0.50	40	
Min 6	Average	22.77	22.85	-0.08	23.96	23.44	0.52	14.92	18.92	-4.00
	Median	20	20		20	20		12.50	20	
Min 21	Average	57.73	44.15	13.58***	54.57	44.79	9.78**	78.50	39.92	38.58***
	Median	60	40		50	40		80	40	
Observations		91	99		79	86		12	13	

Notes: This table reports summary statistics of agents' transfers and beliefs in the effort-choice game by treatment. Beliefs are the agents' first-order beliefs regarding the probability of facing the respective control level. Thus the means of the beliefs add up to 100 for each agent type (disregarding rounding errors). Transfers and beliefs are compared using Wilcoxon-Mann-Whitney rank-sum tests, while the *Diff.* column shows differences in the averages between NCE agents and CE agents. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Main Treatments: Summary Statistics of Agents' Sharing Decisions and Beliefs in the Real-Effort Game

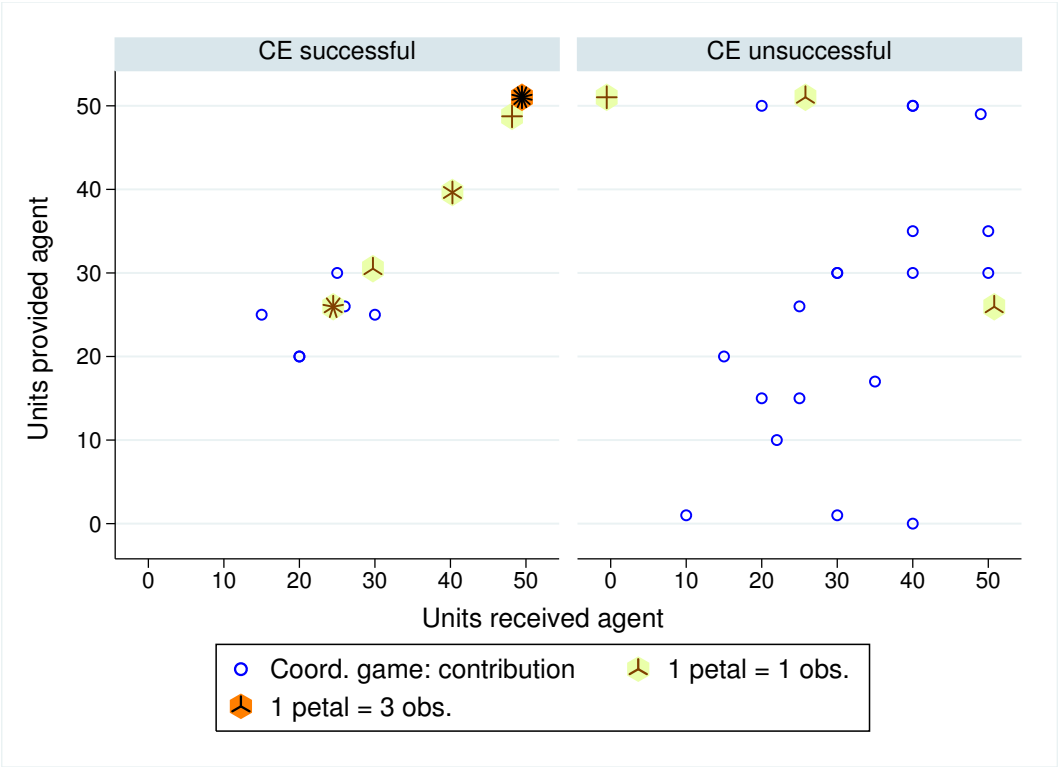
		Agent Sample								
		Panel A: Full Sample			**Panel B Jena Sample**			**Panel C Dusseldorf Sample**		
Transfer		*NCE*	*CE*	*Diff.*	*NCE*	*CE*	*Diff.*	*NCE*	*CE*	*Diff.*
Min 0	Average	30.00	38.33		30.00	38.33				
	Median	30	35	-8.33	30	35	-8.33			
	Observations	2	6		2	6				
Min 10%	Average	20.00	22.73		20.00	20.73				
	Median	20	20	-2.73	20	17	-0.73			
	Observations	3	15		3	11				
Min 20%	Average	24.24	31.27		24.63	30.13				
	Median	20	25	-7.03***	20	25	-5.50**			
	Observations	33	26		30	24				
Min 40%	Average	40.92	42.38		41.11	42.62				
	Median	40	40	-1.46**	40	40	-1.51			
	Observations	53	52		44	45				
Pooled	Average	33.95	36.24		33.77	36.03		35.08	37.62	
	Median	40	40	-2.29**	40	40	-2.26*	40	40	-2.54
	Observations	91	99		79	86		12	13	
Belief		*NCE*	*CE*	*Diff.*	*NCE*	*CE*	*Diff.*	*NCE*	*CE*	*Diff.*
Min 0	Average	14.18	24.07	-9.89***	14.68	22.59	-7.91*	10.83	33.85	-23.02**
	Median	10	20		10	10		2.50	30	
Min 10%	Average	12.73	15.70	-2.97	13.20	16.33	-3.13	9.58	11.53	-1.95
	Median	10	10		10	10		5	10	
Min 20%	Average	20.22	22.04	-1.82	21.77	23.16	-1.39	10.00	14.61	-4.61
	Median	20	20		20	20		10	10	
Min 40%	Average	52.88	38.19	14.69***	50.34	37.92	12.42***	69.58	40.00	29.58*
	Median	50	30		50	30		77.50	25	

Notes: This table reports summary statistics of agents' transfers and beliefs in the real-effort game by agent type. The agent makes the decision as to how much of her remuneration from solving equations she will transfer to the principal *before* the agent starts the task. For the Dusseldorf sample, we only report aggregate statistics for the chosen transfer due to the small number of observations per control cell. Beliefs are the agents' first-order beliefs regarding the probability of facing the respective control level. Thus the means of the beliefs add up to 100 for each agent type (disregarding rounding errors). Transfers and beliefs are compared using Wilcoxon-Mann-Whitney rank-sum tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B. Coordination Success

Figure B.1 suggests that judgment of the partner's action depends more on successful coordination than on the payoff gained. Even when CE successful subjects do not reach the profit-maximizing outcome of 100, the degree of coordination is still high. In 64 out of 70 cases (91%), a CE successful player's contribution to the group account corresponded to the amount the fellow player sent. On the other hand, CE unsuccessful players reach coordination in only 3 out of 29 cases (10%). Thus, due to the weakest-link design of our game, in almost 90% of the cases some units transferred to the group account were wasted in the CE unsuccessful sample.

Figure B.1: Quality of Coordination in the CE Treatment



Notes: This is a sunflower graph that illustrates, for each group of two, the players' contributions to the group account in the team-building task in the CE treatment. Transfers are shown separately for CE successful subjects (left panel) and CE unsuccessful subjects (right panel). CE successful means that the mutual fairness rating within a principal-agent group was either 'fair' or 'very fair'; the CE unsuccessful sample consists of subjects who mis-coordinated in the team-building exercise, meaning that at least one player within a group rated her partner's fairness as less than 'fair'. In the graph, observations are depicted by blue circles at their exact location if the respective combination of transfers is observed only once. Observations in bins with higher densities are represented by light or dark sunflowers. Each petal of a light sunflower represents one observation. Each petal of a dark sunflower represents three observations. No units are wasted when both players transfer the same amount to the group account; that is, when the observations lie on a 45-degree line from the origin (not shown).