Düsseldorf Institute
for Competition Economics

# DISCUSSION PAPER

No 66

# Team Building and Hidden Costs of Control

Gerhard Riener,
Simon Wiederhold

September 2012

HEINRICH HEINE
UNIVERSITÄT DÜSSELDORF

IMPRINT

DICE DISCUSSION PAPER

Published by
Heinrich-Heine-Universität Düsseldorf, Department of Economics, Düsseldorf Institute for Competition Economics (DICE), Universitätsstraße 1, 40225 Düsseldorf, Germany

Editor:

Prof. Dr. Hans-Theo Normann
Düsseldorf Institute for Competition Economics (DICE)
Phone: +49(0) 211-81-15125, e-mail: normann@dice.hhu.de

DICE DISCUSSION PAPER



ISSN 2190-9938 (online) – ISBN 978-3-86304-065-9

# Team Building and Hidden Costs of Control*

Gerhard Riener† and Simon Wiederhold‡

September 2012

**Abstract**

This paper investigates the interaction of intrinsic and extrinsic incentives. We propose a simple principal-agent model with control that incorporates the existence of social groups resulting from common experiences in the past. Our laboratory experiment shows that agents with previous common experiences with their principals (CE agents) perform better than agents without such experiences (NCE agents). However, as soon as actual control exceeds their expectation, CE agents decrease their performance substantially, which has no equivalent for NCE agents. This pronounced decrease in effort when control is perceived as excessive represents a novel channel through which hidden costs of control materialize. Our results have important implications for firms' strategies to motivate employees.

**JEL code:** C92; M54; D03; J22

**Keywords:** Employee motivation; Principal-agent theory; Experiments

*We are indebted to Agnes Bäker, Werner Güth, Bernd Irlenbusch, Oliver Kirchkamp, Ljubica Nedelkoska, Hans-Theo Normann, Holger Rau, Tobias Regner, Guido Schwerdt, Dirk Sliwka, Ludger Wößmann, Philipp Zahn and conference participants at ESA 2011, 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.

†Düsseldorf Institute for Competition Economics (DICE), Düsseldorf, Germany; riener@dice.hhu.de

‡Ifo Institute, Human Capital and Innovation, Munich, Germany; wiederhold@ifo.de.

# 1 Introduction

As employment relationships are typically characterized by incomplete contracts, firms pursue various strategies to provide incentives to workers to act on their behalf. On the one hand, firms commonly use control devices to eliminate workers' most opportunistic actions. 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, has been challenged by a number of empirical studies, which evidence that control can reduce worker effort by eroding intrinsic motivation, thus entailing significant 'hidden costs' (among others, Enzle and Anderson, 1993; Falk and Kosfeld, 2006; Schnedler and Vadovic, 2011).[1]

On the other hand, a frequently used organizational development intervention is team building, with the objective to foster worker identification with one another and with the firm (for instance, Buller and Bell Jr., 1986; Hardy and Crace, 1997). Although the empirical evidence regarding the impact of team-building activities on performance is somewhat scattered (Salas et al., 1999; Woodman and Sherwood, 1980), it is often claimed that by applying principles of team building, 'any group can transform itself [...] into a high-performing team' (Shandler and Egan, 1996, p. x). Team building across hierarchies may hold particularly high benefits for firms, because it has been shown that between groups—potentially induced by hierarchies—there are especially low levels of trust (Fershtman and Gneezy, 2001; Chen and Li, 2009; Falk and Zehnder, 2007).

In reality, firms have the choice to employ both control and team building for improving worker performance. This paper is the first to investigate how the two strategies interact. In particular, we argue that the behavioral reaction to control depends on the nature of the principal-agent relationship, developed through team experiences in the past.[2] George A. Akerlof and Rachel E. Kranton provide case-study results from the U.S. steel industry that are consistent with this view; they conclude 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). However, thus far, no study has attempted to rigorously examine the relationship between these two prominent firm intervention techniques.

We propose an analytical framework that incorporates the existence of interpersonal relations based on joint experiences into a simple principal-agent model. In the model,

[1] See van Dijk and van Winden (1997), Ellingsen and Johannesson (2008), von Siemens (2011), and Sliwka (2007) for theoretical investigations of the mechanisms underlying motivational crowding-out.

[2] A primary objective of team-building activities is to increase mutual supportiveness, communication, and sharing of feelings between team members (Buller and Bell Jr., 1986; Salas et al., 1999). Thus, team-building interventions facilitate the emergence of social groups in the sense of Ashforth and Mael (1989) and Tajfel (1978).

the agent decides how much effort to exert on behalf of the principal. The principal can restrict the agent's choice set by imposing a minimum effort requirement that the agent is not allowed to fall short of.[3] The principal can costlessly implement his chosen level of control. We distinguish between two general types of principal-agent relationships in the model: common-experience and no-common-experience relationships (henceforth, CE and NCE). CE individuals have been exposed to a team-building activity and, on that basis, developed trust in one another. NCE individuals, on the contrary, lack a common history of interpersonal relations; in fact, we think of them as being total strangers to each other.

The theoretical discussion delivers three main insights: First, feelings of belonging to a social group evoked by a team-building intervention influences both the agent's willingness to perform on behalf of the principal and his subjective expectations of the appropriate level of control. Second, the magnitude of hidden costs associated with the principal's control varies between agent types. Third, an important determinant of the differences in the response of agent effort to control is *sensation,* which we define as the deviation of the agent's subjective control belief from the level of control imposed by the principal.

We conduct a labor-market experiment to test these behavioral conjectures.[4] The experiment proceeds in three stages. In the first stage, subjects being exposed to the NCE treatment complete a task in isolation. In the CE treatment, group formation via team building takes place. We create a common experience between (pairs of) individuals by letting them play a coordination game with computer-mediated pre-play communication and post-play feedback. This novel approach to induce groups in the laboratory closely resembles team-building interventions in real firms. On the one hand, team-building activities in the field are a device to create group feelings among the participants (Salas et al., 1999). On the other hand, successful coordination is easy to achieve both in our group-induction stage and in firms' team-building measures. An additional feature of our coordination game is that it allows us to measure whether group formation can be expected to have been successful in developing a trust-based relationship between the participants. After this stage, each participant is randomly assigned a role as either principal or agent, and the subjects keep this role for the rest of the experiment.

In the second stage, the agent is provided a windfall endowment by the experimenter, part of which he can transfer to the principal. This transfer approximates effort. We use the strategy method (Selten, 1967) to elicit the agent's effort for each of the control levels

[3] As Falk and Kosfeld (2006), we argue that the minimum effort restriction implemented by the principal is the equivalent of employing control devices in the agent's work environment.

[4] Our experimental design builds on Falk and Kosfeld (2006) but extends it in a number of important ways.

available for the principal to choose.[5] In the third stage, the agent is remunerated for performing a real-effort task. Before the game begins, he must decide how much of his future remuneration he is willing to share with the principal. The agent receives information on the minimum share required by the principal before making the sharing decision and completing the task. Hence, the real-effort game permits us to study the influence of an experienced control sensation on the subjects' behavior, while we are able to control for a multitude of individual characteristics.

We find that a shared experience with the principal has belief- and performance-related consequences for the agent. CE agents expect lower levels of control than NCE agents and are, for each control level, more willing to exert effort. In addition, we observe that hidden costs of control exist for both types of agents. For NCE agents, the crowding-out effect of control dominates the disciplining effect only for low levels of control. For CE agents, however, the negative reaction to limited discretion is much more pronounced; only when high effort is easy to enforce, benefits of control prevail over hidden costs. This finding is caused by differences between agent types in the individual reaction to negative control sensations. We show that only CE agents retaliate if the level of control they face exceeds their control beliefs. Since a rising level of control, *ceteris paribus*, reduces the control sensation in the population, this inclination of CE agents to punish unexpectedly 'bad' control behavior by the principal explains the higher degree of control aversion of CE agents as compared to NCE agents.

To the best of our knowledge, this is the first work that investigates the role of subjective control beliefs in determining the agents' responses to control. Our results imply that CE agents are more quickly disappointed than their NCE counterparts by a principal who excessively uses control, as they attach a higher emotional significance to negative control sensations. In extreme cases, CE agents' willingness to sanction principals whose implementation of control exceeds the agents' control expectations can completely outweigh the benefits of a principal-agent experience previously shared. Consequently, intrinsic and extrinsic incentives appear as substitutes in our experiment.

Our research adds to field evidence reported by Barkema (1995). For a sample of 116 executives of Dutch firms, he documents that higher monitoring is negatively correlated with working hours if managers are supervised by an in-house CEO, while the correlation is positive if monitoring is implemented impersonally by a parent company. However, using real-world data to assess the behavioral consequences of monitoring as a function of social distance is problematic, because it requires intimate knowledge of the nature of the

[5] Having three observations for each individual enables us to measure hidden costs of control in a cleaner way than in previous analysis, because we can control for individual fixed effects.

principal-agent relationship. To establish a causal effect, one would have to consider a myriad of aspects, for instance, economic dependency on the job, organizational tenure, recency of membership in the organization, and (informal) organizational structure (Albert and Whetten, 1985; Ashforth and Mael, 1989; van Knippenberg and van Schie, 2000). These factors influence both group attachment and performance, and not being able to properly control for them entails the risk of seriously confounded results.

Besides omitted variables, selection is another reason why the interaction of different incentive devices at the workplace is difficult to investigate in the real world. In short, control averse individuals are unlikely to apply for a position in a firm in which they expect a controlling leadership style. Thus, in reality, work climate and the employees' personal characteristics (such as their reactions to a decrease in choice autonomy) are not mutually independent (Antonakis and Atwater, 2002; Stanton, 2000).[6] In our experiment, we can avoid these confounds as people are randomly assigned to treatments and specific workplace characteristics can be abstracted from.

There is an increasing body of experimental evidence showing that group membership created in the laboratory can increase participants' willingness to act on behalf of members of the same group (for an overview, see Goette, Huffman and Meier, 2011). In fact, even random assignment to a *minimal group*, which in often just an arbitrary label, has sometimes proved to be sufficient to induce people to display greater social concerns for the well-being of in-group members than for non-members (Chen and Li, 2009; Heap and Zizzo, 2009; Sutter, 2009). However, as suggested by Goette, Huffman and Meier (2011), this 'labeling' effect of groups does not well reflect real-world groups. Group formation in our experiment, which takes place by letting subjects cooperate in a short problem-solving game, is designed to capture the importance of common experiences for the emergence of groups.

Our paper is apparently related to the experimental evidence gathered by Falk and Kosfeld (2006), who show that control can yield costs that outweigh its benefits when dealing with reciprocal agents, as reciprocity is sensitive to control.[7] However, Falk and Kosfeld (2006) do not account for heterogeneous effects of control with respect to the nature of the principal-agent relationship, and thus neglect how the existence of social ties may increase or undermine motivation. Dickinson and Villeval (2008), testing the theoretical conjectures made by Frey (1993) in a real-effort experiment, find that tighter monitoring by the principal crowds out the agent's effort only if the principal and agent are socially close. The

[6] See Ploner, Schmelz and Ziegelmeyer (Forthcoming) for a related argument.

[7] Other papers that study the impact of extrinsic incentives on intrinsic motivation and voluntary cooperation in the laboratory are, for instance, Bartling, Fehr and Schmidt (2012) and Fehr and Rockenbach (2003). Ploner, Schmelz and Ziegelmeyer (Forthcoming) as well as Schnedler and Vadovic (2011) conduct replication studies of the original experiment by Falk and Kosfeld (2006).

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 the subjects (Dufwenberg and Muren, 2006; Goette, Huffman and Meier, 2011).

In contrast to Dickinson and Villeval (2008) and similar to Masella, Meier and Zahn (2012), control and effort decisions in our experiment are made anonymously. Subjects in the CE treatment only know that they have interacted with the other player before, but receive no other identifying information. Keeping anonymity allows us to isolate the effect of a previous common experience on the agent's willingness to exert effort for the principal. In addition, our one-shot design precludes feedback effects of control on group coherence. In Dickinson and Villeval (2008), subjects interact repeatedly and receive feedback on each other's decisions after each round. Another virtue of analyzing one-shot games is that neither reputation building nor payoff-driven reciprocity prevail. In fact, we can rule out any strategic motives of the principal to trust or the agent to exert effort. While Masella, Meier and Zahn (2012) focus on creating identity using the procedure introduced by Chen and Li (2009), we explicitly induce a positive experience between the principal and agent, as it is normally done in team-building exercises in real firms.

The remainder of this paper is organized as follows. In the next section, we present the modeling framework and derive predictions. Section 3 explains the experimental design, which is followed by a discussion of the results in Section 4. Section 5 concludes by providing the implications of our findings.

# 2 Theoretical Framework and Behavioral Hypotheses

## 2.1 Sensation-Dependent Preferences

In this section, we outline a modeling framework that we use to derive hypotheses on the relative importance of implicit incentives, explicit incentives and their interaction. In the model, we consider a situation in which the principal chooses a control level, $m$, that is observed by the agent before the latter decides how much effort to expend on behalf of the principal. We denote the level of control that the agent expects by $\hat{m}$. Further, we assume that the agent's utility depends on the magnitude of the deviation of the expected degree of control from the actually experienced one, that is, $\hat{m} - m$. We refer to this deviation as the level of sensation.

Let the agent's utility be composed of three components (Akerlof and Kranton, 2008). First, the agent receives a constant wage, $w$, with his utility increasing in the level of $w$.

Second, the agent exerts an effort of $e$, which causes a positive and non-decreasing marginal dis-utility. In other words, we assume that the costs of effort have the following form: $f'(e) > 0$ and $f''(e) \geqq 0$. Third, and most importantly, the term $g((\hat{m} - m), e)$ defines the utility effect of a sensation. The utility function of the agent then reads:

$$U_A(e; m) = \ln w - f(e) + g((\hat{m} - m), e) \tag{1}$$

This formulation of the agent's utility captures the general attitude toward the task the agent has to perform, which stems from sources such as pay satisfaction, $\ln w$, specific task characteristics, $f(e)$, and the (dis-)utility stemming from positive or negative control sensations (regarding the latter, see also Koszegi and Rabin, 2006).

**Sensation as a driver of behavior** Let us define $\Delta \equiv \hat{m} - m$ as the difference between the agent's individually expected and actually experienced degrees of control, which captures the level of sensation.[8] Assuming that the wage payment and effort dis-utility do not differ between agent types, we can abstract from $\ln w$ and $f(e)$ in what follows. The agent's utility is then determined by the sensation term only, and (1) reduces to:

$$U_A^{reduced}(e; m) = g(\Delta, e) \tag{2}$$

We further assume that the sensation term in (2) satisfies the following properties:

**Assumption 1.** $g(\Delta, e)$ *is continuous for all* $\Delta \in \mathbb{R}$ and $e \geq 0$, and twice differentiable for $e$ and for $\Delta \neq 0$.

**Assumption 2.** $g(\Delta, e)$ is strictly increasing in $\Delta$ and weakly increasing in $e$.

**Assumption 3.** $\frac{\partial^2 g}{\partial e^2} < 0$

**Assumption 4.** $\frac{\partial^2 g}{\partial \Delta \partial e} > 0$

The first part of Assumption 2 means that, *ceteris paribus*, the agent's utility increases in the level of sensation; the larger the difference between the expected and experienced control, the higher the agent's utility. The second part of Assumption 2 reflects the idea that, for a

[8] In the results section, we will define the empirical equivalent of sensation.

given level of sensation, reciprocity increases in effort. The intuition for this assumption can be seen from the following example. Suppose that there are two agents, Adam and Eve. Eve is willing to work hard and expects little control. Adam also expects a low control level, but he is not willing to work as hard as Eve. Let both Adam and Eve face high levels of control and experience the same kind of negative sensation. Our assumption is that this sensation will disappoint the hard-working Eve more (or at least not less) than the rather lazy Adam, so her utility decreases at least as much as Adam's does.

Assumption 3 establishes that the positive effort-dependence of $g(\bullet)$ decreases in the level of effort. If this was not the case, the optimization calculus for the agent would be trivial, as he would always choose the maximum possible effort.[9] Assumption 4 demonstrates how the effect that sensation has on utility changes in the level of effort. We assume that the higher the agent's willingness to work, the more pronounced the utility-enhancing effect of any sensation. Put differently, the more ready an agent is to expend effort, the more sensitive he is to the behavior of the principal and the stronger the utility effect of sensations.

## 2.2 Behavioral Predictions

The agent maximizes (2) w.r.t. $e$ s.t. $e \geqslant m$, where $m$ is the control choice of the principal, which the agent takes as given.[10] We solve the agent's optimization problem in the Appendix and derive the following conjectures concerning the behavior of the agent:

**Conjecture 1.** *Due to the existence of two equilibria, we may observe agents who do not exert any effort beyond the minimum requirement set by the principal, that is, $e = m$. However, we also expect to observe agents with a positive level of voluntarily expended effort, that is, $e > m$.*

**Conjecture 2.** *For any positive effort increment, $e > m$, effort increases in the level of sensation.*

Conjectures 1 and 2 are the result of the agent's optimization calculus when assuming that one can abstract from the existence of interpersonal relations between the principal

[9] Recall that we neglect direct costs of effort.

[10] Note that the participation constraint always binds by assumption. Therefore, our model is highly applicable to employment in the public sector or large organizations with high 'sunk utility costs' (for instance, due to job security and the right to a pension). The previous literature has also demonstrated that public sector employees have certain personal characteristics that make working in the public domain preferable over working in the private sector. It has been found that, compared to private employees, public employees are more risk averse (Falk and Dohmen, 2010) and more concerned with status than with money (Rainey, 1983; Warwick, 1980). Moreover, employees in the public sector perceive the relationship between pay and performance as being weaker (Buchanan, 1974, 1975; Ingraham, 1993; Perry, Petrakis and Miller, 1989; Rainey, 1983).

and agent. Therefore, both conjectures refer to the effort responses to control or sensation within treatments. However, the model can easily be extended to incorporate heterogeneous principal-agent relationships. To formally separate individuals who developed trust in one another, for instance, by means of a previous team-building exercise, from those who are perfect strangers, we introduce the parameter $c \in \{0, 1\}$, where $c = 1$ identifies an CE principal-agent pair and $c = 0$ identifies an NCE pair.[11]

In particular, we think of CE and NCE agents as being different in several dimensions. First, social identity theory (Ashforth and Mael, 1989; Tajfel, 1978) implies that the level of effort exerted for the principal depends on the nature of the principal-agent relationship, that is, $e = e(c)$. Early laboratory experiments find that simply assigning an individual to a group can be sufficient to induce in-group favoritism (Ashforth and Mael, 1989; Brewer, 1979), which has been confirmed by more recent studies (for instance, Chen and Li, 2009; Goette, Huffman and Meier, 2011). Frank, Gilovich and Regan (1993) report that expecting their partners to be trustworthy impacts the participants' likelihood to cooperate in one-shot prisoner's dilemmas. Likewise, using observational data, Porta et al. (1997) document for a cross section of countries that trust matters for cooperation. Moreover, evidence from the public sector suggests that the social distance between the principal and agent is expected to directly affect whether an agent shirks or works (Chaney and Saltzstein, 1998; Scholz, 1991). Given these results, we modify Conjecture 1 to incorporate the intergroup differences in the inclination to exert effort voluntarily as follows:

**Conjecture 3.** *The proportion of CE agents exceeding the minimum performance level set by the principal is higher than the respective proportion of NCE agents.*

Moreover, the above literature also suggests that CE agents take on the principal's perspective and therefore supply more effort in the principal's interest than their NCE counterparts.

**Conjecture 4.** $e(c = 1) > e(c = 0)$

Second, various studies of decision-making in social contexts show that knowledge about other persons' personality features affect expectations regarding their behavior (for instance, Delgado, Frank and Phelps, 2005; Marchetti et al., 2011). This implies that the individually expected level of control depends is agent-type specific, that is, $\hat{m} = \hat{m}(c)$. The discussion

[11] Notice that we do not derive any conjectures on the principal's control choices from the model. Although one may suspect that the principal's control is contingent on the social ties with the agent—in the experiment, this is indeed sometimes observed—the focus of the paper is on the agent's behavior. However, many of the below arguments that militate in favor of group-contingent agent effort can as well be applied to the principal.

of reference point effects in Hart and Moore (2008) points out the direction of treatment differences in the control beliefs. Applying their theory of contracts as reference points to our setting, principal-agent pairs with a shared common experience conclude some kind of implicit contract, which affects expectations of the appropriate level of control. Because they expect their principals to be more cooperative, CE agents have lower control beliefs than NCE agents.

**Conjecture 5.** $\hat{m}(c = 1) < \hat{m}(c = 0)$

Finally, the model suggests that effort, $e$, and sensation, $\Delta$, are positively associated (see Conjecture 2). This result should hold for both types of agents. However, there may be treatment differences in the performance response to sensation if the direction of the sensation is accounted for. In the case of a negative sensation, $\Delta < 0$, we expect the decrease in the willingness to perform in the principal's interest to be more pronounced for CE than for NCE agents. Following Hart and Moore (2008), CE agents might interpret a negative sensation as the principal breaching the implicit contract. The agent, in turn, retaliates upon the principal for this norm violation, a phenomenon that has been reported in the psychological literature by Koehler and Gershoff (2003) and Sanfey (2009). As discussed by Goette, Huffman and Meier (2011), social ties between group members can be easily eliminated, and replaced with a desire for sanctioning, if a group member's behavior is seen as incoherent with the implied group identity. The stronger the social ties within the group, the more pronounced the negative emotional reaction to group members not acting in accordance with their fellows' beliefs or norms.

**Conjecture 6.** $\frac{de}{d\Delta}(c = 1) > \frac{de}{d\Delta}(c = 0)$ *for* $\Delta < 0$

The previous literature does not provide clear guidance on whether intergroup differences also exist for a positive sensation, $\Delta > 0$. Thus, we formulate Conjecture 7 in a neutral way:

**Conjecture 7.** $\frac{de}{d\Delta}(c = 1) \lesseqgtr \frac{de}{d\Delta}(c = 0)$ *for* $\Delta > 0$

It is important to note that our behavioral predictions differ from those discussed in Akerlof and Kranton (2005). They assume that strict supervision alters the nature of the principal-agent relationship, in the sense that a CE relationship suddenly becomes an NCE relationship. In our model, supervision or control does not affect the type of the relationship between the principal and agent. Rather, a negative control sensation, possibly interpreted as a sign of distrust, evokes even harsher negative feelings for the CE agent. This may be a more realistic view of CE and NCE relationships than the one put forward by Akerlof and Kranton (2005), since positive experiences will neither be completely eliminated by a negative experience nor will they return subjects to a state similar to having never shared a common experience with each other. Rather, we think that the weight the agent attaches to control sensations depends on the nature of the principal-agent relationship, as does the agent's behavioral reaction to them.

**On the Relationship between Sensation-Dependent Preferences and Hidden Costs of Control** Control entails costs in addition to the direct costs of its implementation ('hidden costs of control') if the agent exerts less effort when controlled than otherwise. Since a rising level of control, *ceteris paribus*, always reduces the control sensation in the population and, thus, increases aggregate disappointment, sensation-dependent preferences as described in the previous section do predict hidden costs of control. Furthermore, if Conjecture 6 holds and under the additional assumption that the differences in effort levels between CE and NCE agents do not increase in control, the model also predicts that hidden costs of control are higher for CE than for NCE agents.

# 3 Experimental Design

Subjects were divided into groups of two, and the role as either principal or agent was randomly assigned. This role was fixed throughout the whole experiment. No subject participated in more than one treatment or session. The design was parsimonious, without work environment frames. In the first step we established groups in the CE treatment. In the NCE treatment, individuals performed a task in isolation. Each participant was randomly assigned the treatment upon arrival at the lab.

Then, to test Conjectures 1, 3, 4, and 5, all subjects played an effort-choice game. Control was implemented by allowing the principal to impose a minimum effort restriction on the agent. The latter, without getting to know the principal's control decision, had to state the efforts they were willing to exert for each level of control the principal could choose. However, in light of the discussion in the previous section, sensation is likely to be salient primarily in situations where the principal's control decision is revealed to the agent. Therefore, to additionally test Conjectures 2, 6, and 7, subjects played a real-effort game after the effort-choice game, where the agent learned about the principal's control decision before making his performance choice. We now turn to a detailed explanation of the stages.[12]

[12] We also ran two pilot sessions with a total of 36 participants whose results are not reported here.

## Treatment Manipulation

In the group-formation phase, about half of the participants in the experiment played a weakest-link game. Subjects could distribute 50 experimental currency units (ECUs, where 1 ECU was worth 0.10 €) to a private or a public account. The returns to the group account were the smaller of the two contributions to the public account, doubled by the experimenter. A subject's total payment was the sum of the private and the group account. After an explanation of the game, a message on a screen asked each group of two to discuss their strategy for this game via an online chat.[13] The aim of this phase was to induce a feeling of belonging to the same group, as a consequence of the shared experience.[14] The coordination game had an obvious focal point, to ease the establishment of group feelings.[15] We refer to the principal-agents pairs that played the coordination game as CE subjects.

There was no competition among groups in the later stages of the experiment, nor did we reveal the control and effort choices made by the other principal-agent pairs to the subjects. Although the social identity literature has demonstrated that salience of other group(s) and competition among groups reinforce awareness of one's 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 is sufficient to detect behavioral differences. However, our design captures more than a pure labeling effect that results from simply assigning people to certain groups. Rather, we also account for the social ties aspect of groups that emerges from the shared common experience of the principal and agent and the knowledge of the other's behavior.[16] After the game and the disclosure of the results, the subjects had to give their partners feedback on how fair they found the other's behavior. The 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 feedback before the

[13] To ensure anonymity, the participants were asked to only chat about the game. We checked the chat protocols whether personal information was exchanged during the principal-agent interaction. It turned out that almost all subjects, indeed, limited themselves to chat about the game. A pair of subjects, however, revealed their identity during the chat. Since dropping these subjects from the sample leaves all results virtually unchanged, we decided to keep them in our preferred sample.

[14] One of the factors that traditionally are associated with group formation is interpersonal interaction (for an early reference, see McDougall, 1920).

[15] Techniques of group induction have long been used in social psychology (for instance, Turner, 1981) but only recently found their way into the experimental literature (among others, Chen and Li, 2009; Heap and Zizzo, 2009).

[16] As evidenced by Goette, Huffman and Meier (2011), the additional motives arising when group induction is not minimal are important determinants of individual behavior, especially with regard to the response to within-group norm violations.

next stage.[17]

Subjects in the NCE treatment were asked to perform a slider task (developed by Gill and Prowse, Forthcoming, slightly modified to fit our design needs.). The challenge here was to bring 48 sliders into the middle position within 2:15 minutes. Participants in this task received a flat fee of 80 ECUs, independent of their performance. This payoff choice for the NCE subjects was motivated by the average payoff of CE principal-agent pairs in the pilot sessions.

## Effort-Choice Phase

The effort-choice game is a modified version of the design of Falk and Kosfeld (2006). Before the game, subjects were informed about their assigned roles as principal or agent. Each agent had an initial endowment of $E = 1, 2, ..., 117$, where $e \in E$ represents the total effort exerted by the agent. The marginal monetary costs for the agent to expend 1 unit of effort were constant and set to 1. The principal had no initial endowment. The amount transferred to him by the agent was doubled by the experimenter so that the principal received $\pi_P = 2e$. The principal could restrict the agent's choice set by enforcing one of the following three minimum transfers: $E_{min} \in \{0, 6, 21\}$. We chose those control levels to investigate what a small (relative to the endowment) increase in control triggers in the agent. Agents played this game using the strategy vector method. They had to decide on efforts for all possible minimum effort levels without knowing the principal's actual decision.

## Real-Effort Phase

After the effort-choice game, the experiment proceeded without any feedback on the control or effort choices to the final round of the experiment, the real-effort game. Here, the agents had to add five two-digit numbers (Niederle and Vesterlund, 2007), and the remuneration depended on the number of correct answers. Before the agent started the task, he had to

[17] One concern of this type of group induction is that a subject might learn something about the type of the partner, in particular, whether he is kind or not. We designed the experiment in such a way that learning about the *real* type of the partner is very limited. 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 public account is not necessarily interpreted by the other player an act of kindness, but may just as well be regarded as selfish behavior (see von Siemens, 2011, for a related argument). Thus, the pure fact of successful coordination is not sufficient to make the players believe that their respective partner is kind. In this, however, our weakest-link game is very similar to team-building in reality. Here, successful coordination is typically also easy to achieve, because firms try to emphasize the positive aspects of their employees' personalities, leaving the negative aspects uncovered.

decide what share of his payoff to transfer to the principal.[18] Because, again, the principal had no initial endowment, the agent's transfer was the only source of income for the principal in this game. The principal could choose his desired level of control from the following possibilities: $E_{min} \in \{0\%, 10\%, 20\%, 40\%\}$. The agent was free to transfer a larger share to the principal than the latter requested as minimum, while the transfer was not doubled. The agent received 10 ECUs for each sum correctly solved.

Although Bruggen and Strobel (2007) find no difference in effort-choice and real-effort games in economic experiments, we think that both types of games have a *raison d'être* in our experiment. In the effort-choice game, we have three observations for each individual. Thus, we can control for individual fixed effects, which permits us to rule out that the magnitude of hidden costs of control are driven by subject heterogeneity.[19] Moreover, the effort-choice game allows for comparisons to previous studies on hidden costs of control, most notably Falk and Kosfeld (2006) and Ploner, Schmelz and Ziegelmeyer (Forthcoming). Finally, we want to investigate whether the effects of a common experience are salient in both cold and hot decision-making situations.

In the real-effort game, we are not able to account for subject fixed effects, since each agent takes only one transfer decision. However, using information from subjects on basic demographic characteristics (age, gender, field of study, experimental experience, etc.) allows us to control for individual heterogeneity to a considerable extent. Besides, the real-effort setting in our experiment is probably more informative than the effort-choice game for three reasons.

First, principal-agent relationships in real-world organizations almost always involve real effort for the agent to be exerted. Second, given the evidence on earned versus windfall money in dictator-like experiments, voluntary sharing is more costly for the agent in the real-effort game, as it involves own work (for an extreme example, see Cherry, Frykblom and Shogren, 2002). Thus, agents completing a real-effort task may be less inclined to share what they earn. Third, the strategy method forces the subjects to make their decisions cold. Therefore, the agent's effort reaction to, in particular, sensation might not be properly revealed. In the real-effort game, the agents received feedback on the principals' control choices *before* deciding on their transfer. This allows us to study disappointment as one particular mechanism leading to the existence of hidden costs related to the implementation

[18] At this stage, the agent already knew that the real-effort task would be to solve summations. The agent's sharing decision may thus depend on his (perceived) numeracy skills. However, under the assumption that groups do not systematically differ in their number-adding skills, this does not affect our results on treatment differences in voluntary sharing. Moreover, in the regressions, controls for the academic major capture that, for instance, natural scientists may have comparatively high math skills.

[19] Ploner, Schmelz and Ziegelmeyer (Forthcoming) provide evidence that the magnitude of hidden costs of control depends on subjects' personal characteristics.

of control.[20]

### Belief Elicitation

To assess the role of sensation in shaping the agents' behavior, we elicited subjective control beliefs. The agents had to report their perception of the likelihood of each possible control level available for the principal to choose before the agents' effort and the principals' control decisions. We also asked the principals to state their beliefs regarding the agents' control expectations (second-order beliefs). We did not incentivize these answers, because the participants had no strategic incentive to not truthfully state their beliefs. Moreover, there is evidence that eliciting beliefs with or without incentivization for accuracy does not yield different results (Camerer and Hogarth, 1999; Grether, 1992).

After having completed all of the tasks, the subjects were informed of their payment. The payoff-relevant stage was chosen at random. Then, subjects were asked to fill out a questionnaire in return for an additional €1. Furthermore, they received €2.50 for arriving on time for the experiment.

### Implementation

The experiment was programmed in zTree (Fischbacher, 2007) and conducted at the computer laboratory at the University of Jena. Subjects were recruited via the ORSEE online recruitment system (Greiner, 2004). In total, 330 subjects participated in the experiment, primarily undergraduate students at the University of Jena (see Table A.1 for details). One experimental session lasted an average of 45 minutes. The average payoff was €8.70, which is roughly equivalent to the hourly wage of a local research assistant. The maximum (minimum) payoff was €16.30 (€2.50).

# 4 Results

## 4.1 Randomization

A possible concern in experimental studies is whether the randomization process was successful. In spite of our random assignment of treatments by letting every participant in the experiment pick one ball from an urn, one may wonder whether we have indeed eliminated systematic differences between CE and NCE participants. To test for this, we compare both

[20] Emotions are likely to play a larger role in decision making when the reward for the agent accrues only after successfully completing a task (Charness, Frechette and Kagel, 2004).

subject types with respect to their gender, field of study, previous experimental experience, age, and semester. The results, displayed in Tables A.1 and A.2, indicate that the subjects do not significantly differ in their personal characteristics between groups. These results militate in favor of the internal validity of our experiment.

## 4.2 Treatment Manipulation

The mere fact that the CE agents and principals have been interacting in the initial stage of the experiment may not have always been sufficient to render group identity salient. However, Table 1 provides evidence that we were successful in inducing groups in the majority of cases. Recall that after the coordination game, both the principal and agent are asked how fair they evaluate the actions of their partner. In 126 out of 172 cases (73 percent), both players rated each other as either 'fair' or 'very fair.' For those individuals, we are confident that group induction has been successful. In the following, we drop the subjects for whom group induction is likely to not have worked properly from the sample. We do so for two reasons in particular. On the one hand, our hypotheses on the behavioral differences between the CE and NCE subjects derived in Section 2 rely on successful group induction. On the other hand, it can be expected that the impression to have been treated unfairly in the coordination game triggers certain behavior that, although interesting in its own right, is not in the focus of this paper.[21] However, our results continue to hold, yet become somewhat weaker, when we use the full sample of CE subjects. The detailed results are available from the authors upon request.

**Table 1:** Group formation: Player's Satisfaction With the Partner in the CE Treatment

| | **Very unfair** | **Unfair** | **Neutral** | **Fair** | **Very fair** | ***Total*** |
|---|---|---|---|---|---|---|
| **Very unfair** | 0 | 0 | 1 | 0 | 7 | *8* |
| **Unfair** | 0 | 2 | 0 | 1 | 6 | *9* |
| **Neutral** | 1 | 0 | 4 | 1 | 4 | *10* |
| **Fair** | 0 | 1 | 1 | 2 | 7 | *11* |
| **Very fair** | 7 | 6 | 4 | 7 | 110 | *134* |
| ***Total*** | *8* | *9* | *10* | *11* | *134* | ***172*** |

*Note:* This table shows all combinations of a player's rating of the other player's allocation decision in the group formation stage (CE treatment). The polar cases of the player's decision options (that is, very unfair/very fair) were explained, while the other choices (that is, 2-4) only appeared as natural numbers. Each player received feedback on their partner's satisfaction rating.

[21] In a companion paper, we deliver a detailed assessment of the CE participants for whom group induction may have not worked. Interestingly, their behavior is statistically indistinguishable from the behavior of the NCE subjects, suggesting that it is not the common experience *per se* that drives future behavior.

The average payoff in the CE treatment (preferred sample) is 93.21 ECUs, while NCE players earn a flat fee of 80 ECUs for performing the slider task. In the regression analysis, we control for the players' profits in the weakest-link game, assuring us that the results are not driven by payoff differences in the initial stage of the experiment.

## 4.3 Effort-Choice Game

### Agent

**Performance** We begin our empirical analysis by examining the agents' effort decisions by group and control level. Our first result is that we find support for Conjecture 1. For both CE and NCE agents, we observe zero and positive voluntary sharing at all control levels. Judging by Chi-square tests[22], the proportion of agents choosing to share more than the minimum requirement imposed by the principal significantly differs between groups for maximum control (*No control*: $p = 0.687$; *Min 6*: $p = 0.189$; *Min 21*: $p = 0.041$). This is according to Conjecture 3, which predicts that CE agents are more likely to voluntarily share their endowment with the principal than NCE agents.

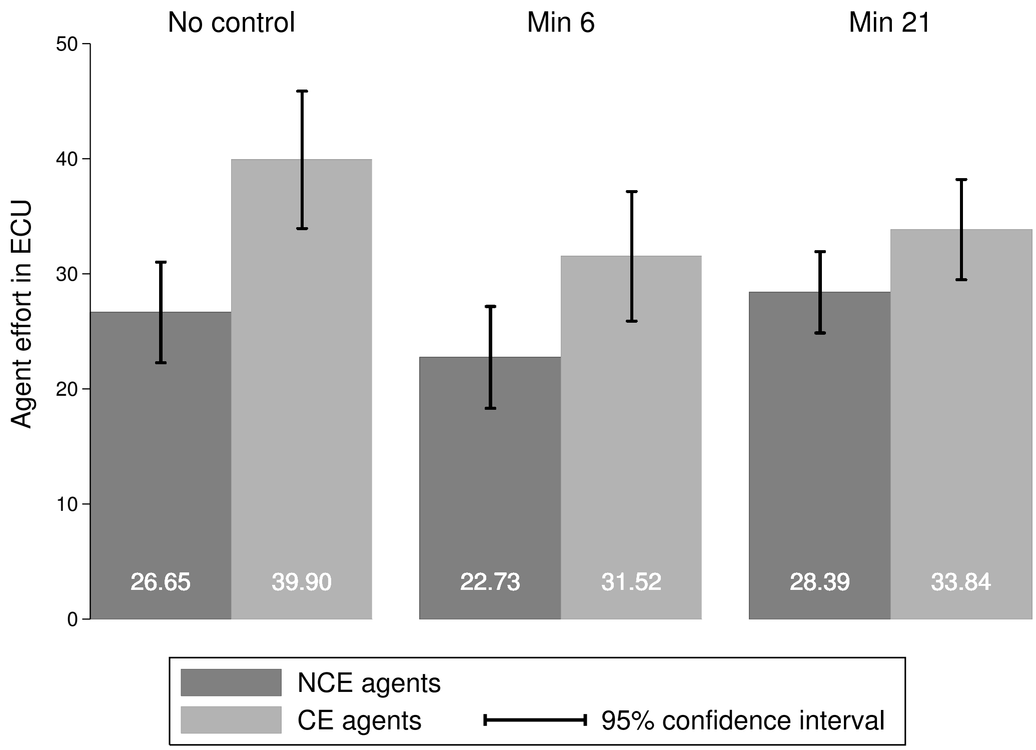
Figure 1 depicts treatment differences in total effort provision as a function of the principal's control decision.[23] Consistent with Conjecture 4, we find that NCE agents' average effort is significantly lower than CE agents' effort at all levels of control. This result indicates that a common experience of the principal and agent increases the agent's willingness to act in the principal's interest in the future. The order of magnitude of the treatment difference decreases in the level of control, suggesting that NCE agents may easier forgive high control than CE agents.[24] This implies that the hidden costs of control vary by agent type, which is an issue that we investigate more rigorously below. Before doing so, however, we elaborate on differences in the agents' expectations about the strength of the principals' control induced by our treatment variation.

[22] We report the results of two-sided tests throughout the paper.

[23] Table A.3 contains the average and the median values of both agent effort and beliefs.

[24] All treatment differences between CE and NCE agents reported in the paper remain qualitatively unchanged when comparing the agent types' cumulative distribution functions with the help of a Kolmogorov-Smirnov test.

**Figure 1:** Effort-Choice Game: Effort by Control Level and Agent Type

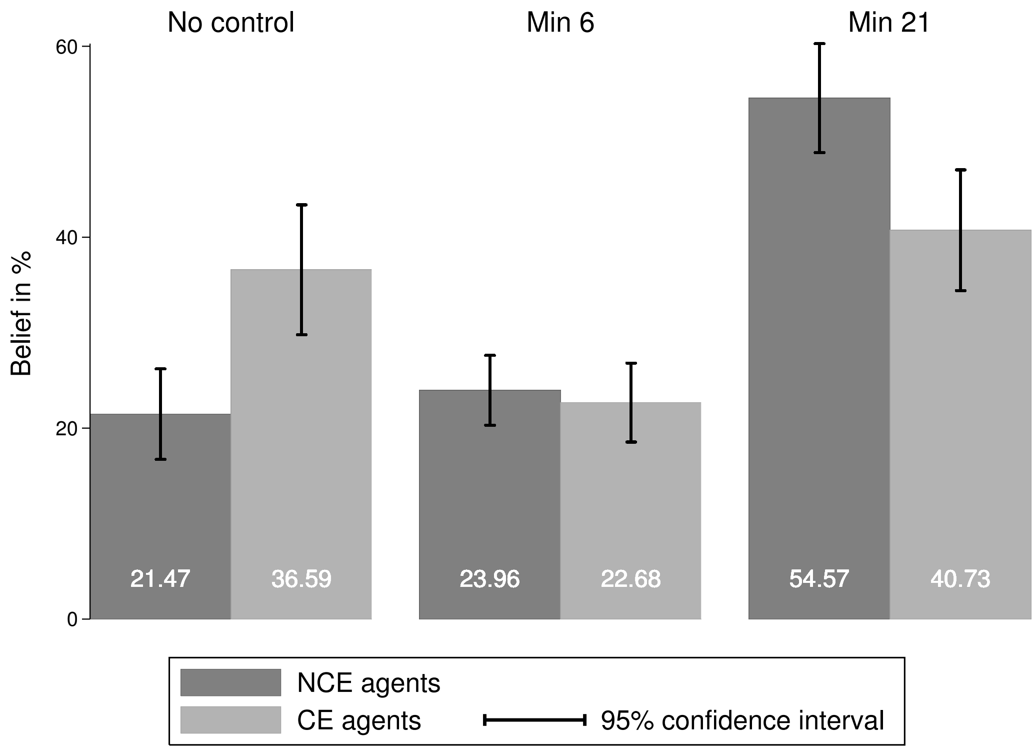



*Note:* This figure shows average agent effort for each of the principal's possible control levels by agent type. There are significant treatment differences in agent effort for all control levels (Mann-Whitney test, *No control*: $p < 0.001$; *Min 6*: $p = 0.014$; *Min 21*: $p = 0.009$). The sample consists of 63 CE and 79 NCE agents.

**Beliefs** Figure 2 shows that CE agents believe no control to be significantly more likely and maximum control to be significantly less so than agents without prior experience with the principals. On average, NCE agents expect with a probability of 21.5 percent that they will not face any control, while this number is more than 15 percentage points higher for CE agents. The treatment difference in beliefs is almost identical, but with opposite sign, for maximum control. The perceived likelihood of facing medium control does not differ between CE and NCE agents.[25]

[25] When assessing reactions to beliefs, a serious concern is the so-called *false consensus effect* (Charness and Dufwenberg, 2006; Ross, 1977). Even if this effect is present, however, it will not affect our results regarding treatment differences, as 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, Sapienza and Zingales, 2009).

**Figure 2:** Effort-Choice Game: Agents' Control Beliefs by Agent Type




*Note:* This figure illustrates NCE agents' *vis-à-vis* CE agents' perceived likelihood that their principal will choose no, medium, and high control, respectively. Average beliefs are presented. We observe significant differences between groups in the *No control* and *Min 21* conditions (Mann-Whitney test, $p$ = 0.001 and $p$ = 0.001). Beliefs do not differ by group in the *Min 6* condition ($p$ = 0.575). The sample includes 63 CE and 79 NCE agents.

Thus far, we have looked at the distributions of the agents' subjective control beliefs. To link the experiment to the theoretical model, however, we need to define the individually expected level of control, $\hat{m}$. We regard the control level that the agent believes is most likely to occur, that is, the mode of the control beliefs, as an appropriate representation of $\hat{m}$. In particular, we find it rather unlikely that subjects base their decisions on the expected values of their beliefs, as this would require cumbersome calculations.[26] According to Conjecture 5, NCE agents expect a higher degree of control than their CE counterparts. We find support

[26] We are aware that relying only on the modal belief as the agent's control reference point means that we neglect the 'strength' of the mode. Recall that beliefs are elicited by asking the agents to attach a likelihood to each possible control level. When using modal control beliefs, we treat an agent who thinks that he will face the maximum degree of control with probability 100 percent the same as an agent who believes that he will face the maximum level of control with probability 34 percent (the latter is the 'weakest' modal belief in our specification with three control levels). Therefore, we also used the *expected value* of control beliefs as an approximation of the agent's individually expected level of control. This alternative specification leaves all of our main results unaffected.

for this hypothesis in the data. The average (median) value of modal control beliefs is 8.84 (6) for CE agents and 15.08 (21) for NCE agents. This difference in modal control beliefs is significant at the 1 percent level (Mann-Whitney test, $p < 0.001$).

**Hidden costs of control** Similar to Falk and Kosfeld (2006), we find that hidden costs of control are often substantial enough to undermine the effectiveness of control. Moreover, whether or not hidden costs of control are sufficiently high to outweigh the benefits depends on both the control level and - as hypothesized in Section 2.2 - the agent type. To see that, consider the results of a linear fixed effects model in Table 2.[27] The dependent variable is agent effort, the explanatory variables are the principal's control choices.

From Column 1 it becomes apparent that, for the pooled sample of CE and NCE agents, agents tend to choose a lower performance if controlled than otherwise. The coefficient on *Min 6* suggests that, on average, an agent transfers approximately 5.9 ECUs less to a principal who chooses a control level of 6 than to a trusting principal. Even when he forces the agent to provide a high effort (*Min 21*), the principal receives almost 1.8 ECUs less than when he refrains from controlling. However, the difference in effort between no and maximum control is not significant.

When separately looking at NCE and CE agents (Columns 2 and 3, respectively), we observe that it clearly does not pay for the principal to choose a control level of 6. For both agent types effort is lowest at this medium level of control. However, only in the CE treatment it is not effective at all to implement control. When a CE principal imposes high control, he receives approximately 6.1 ECUs less than when trusting the agent completely. An NCE principal who decides for maximum control faces a slight, albeit insignificant, increase in profits. The treatment difference is highly significant ($p = 0.010$), suggesting that primarily CE agents seem to punish the principal's decision to choose strong control. This finding extends the previous literature on the hidden costs of control, which has so far not considered the interaction between control and group feelings based on common experiences. For CE principals trust is always better than control; for NCE principals (sufficiently high) control

[27] We also performed random effects regressions, in which we used information on the subjects' gender, age, experimental experience, number of semesters, and academic major to control for individual heterogeneity. These models yields results very similar to those reported below. Moreover, our results do not change in qualitative terms, although they become somewhat weaker, when considering a fixed effects negative binomial specification with jacknifed standard errors (see Table A.7 in Appendix A).

is an effective instrument to prevent selfish agents from acting in an opportunistic fashion.[28]

**Table 2:** Effort-Choice Game: Hidden Costs of Control

| Dependent variable: Agent effort | All agents (1) | NCE only (2) | CE only (3) |
|---|---|---|---|
| No control (= Baseline) | 32.528*** | 26.646*** | 39.905*** |
| | (0.928) | (0.978) | (1.666) |
| Min 6 | -5.894*** | -3.911** | -8.381*** |
| | (1.548) | (1.620) | (2.832) |
| Min 21 | -1.718 | 1.747 | -6.063** |
| | (1.497) | (1.804) | (2.420) |
| Observations | 426 | 237 | 189 |
| Groups | 142 | 79 | 63 |
| $R$-squared (within) | 0.060 | 0.067 | 0.100 |

*Note:* The table shows the results of fixed effects regressions of control on agent effort. Column 1 contains all agents, while Columns 2 and 3 separately show the results for NCE and CE agents, respectively. The coefficients on *Min 6* and *Min 21* indicate the change in the agent's effort provision when the principal chooses a control level of 6 or 21 compared to the baseline case of *No control*. Standard errors (in parentheses) are clustered by subject. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In sum, the results in the effort-choice game suggest that a shared experience in the coordination game has cognitive and behavioral implications. CE agents perceive the likelihood of being controlled differently than their NCE counterparts and are willing to share more with their principals. In particular, CE agents' performance is highest when the principal forbears controlling, a pattern that does not appear for NCE agents. This indicates that hidden costs of control are more pronounced in the CE treatment. However, both agent types respond to a medium level of control by decreasing their transfers to the principal, as medium control is a particularly strong signal of distrust.

**Disappointment** In Section 2.2, we argue that one important determinant of treatment differences in control aversion is that CE agents have a stronger feeling of disappointment

[28] Although, on average, CE agents seem to be control averse, there is also a non-negligible number of CE agents whose efforts are independent of the principal's control. The proportion of agents who transfer the same amount at all control levels (unconditional sharing) is approximately three times higher in the CE than in the NCE treatment (31.74 percent vs. 10.12 percent; Chi-square test: $p = 0.001$). When we drop the subjects who exert the same effort for each of the principal's control actions, we find stronger treatment effects than for the full sample. In particular, we can reject the hypothesis that the effect of control on effort is the same for CE and NCE agents at, respectively, the 10 percent (*Min 6*) and 1 percent (*Min 21*) significance level.

than NCE agents when the principal's control exceeds the agent's expectations. Although disappointment is likely to be more salient in the real-effort game, we now investigate whether we also find effects of negative control sensations on individual behavior when control is just hypothetical. In the context of the effort-choice game, we think of a negative sensation to occur when an agent who does not expect to face any control is actually controlled. Thus, in what follows, we focus on the subsample of agents who have a modal control belief of zero.

Table 3 shows the effect of a negative sensation on agent performance, using fixed effect regressions to control for individual heterogeneity. The dependent variable is voluntary effort, that is, agent effort beyond the principal's minimum requirement. This is a more appropriate outcome variable than total agent effort when analyzing the behavioral impact of a negative sensation, because it is not a function of the principal's control decision.[29] The results in the table evidence that CE agents who did not expect control reciprocate being controlled with a higher reduction in voluntary effort than their NCE counterparts. The treatment difference in the voluntary effort decrease when the principal controls as compared to the baseline of no control is approximately 9 ECUs, being significant at the 10 percent level for *Min 6* ($p = 0.099$) and insignificant for *Min 21* ($p = 0.130$).[30] These results provide a first indication that differences between agent types in the emotional importance attributed to excessive control provide a rationale for treatment differences in the magnitude of hidden costs of control. We will illuminate the meaning of disappointment for control aversion in more detail in the real-effort game, where sensations can be expected to be more salient.

[29] Using total agent effort as the dependent variable leaves all results on treatment differences in the response to a negative sensation unaffected.

[30] If we drop the one (eleven) NCE (CE) agent(s) whose effort provision is independent of the principal's control, the treatment differences in voluntary sharing become substantially stronger: *Min 6* (-20.7 ECUs; $p = 0.008$) and *Min 21* (-18.7 ECUs; $p = 0.014$).

**Table 3:** Effort-Choice Game: Disappointment

| Dependent variable: Agent voluntary effort | All agents (1) | NCE only (2) | CE only (3) |
|---|---|---|---|
| No control (= Baseline) | 47.075*** | 33.417*** | 52.929*** |
| | (2.144) | (2.159) | (2.885) |
| Min 6 | -21.625*** | -14.917*** | -24.500*** |
| | (3.581) | (2.950) | (4.913) |
| Min 21 | -33.700*** | -27.250*** | -36.464*** |
| | (3.106) | (4.675) | (3.924) |
| Observations | 120 | 36 | 84 |
| Groups | 40 | 12 | 28 |
| $R$-squared (within) | 0.637 | 0.684 | 0.643 |

*Note:* The table shows the effect of a negative sensation on voluntary effort provision (defined as the difference between the agent's total effort provision and the principal's control). To approximate a negative sensation in the effort-choice game, we only use observations on agents with a modal control belief of zero. Aside from the differences in outcome variable and sample, the results in Columns 1-3 are derived from regressions analogous to those underlying Table 2 (see there for further information concerning the variable definition and the econometric model). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

### Principal

**Control** We find treatment effects in the principals' control decisions. As can be seen in the upper panel in Table A.4, the proportion of principals deciding to control is significantly higher in the NCE than in the CE treatment (Fisher's exact test, $p = 0.031$). In particular, only 5 percent of the NCE principals decide to trust the agent completely, while this percentage is almost four times higher for CE agents (approximately 19 percent). This difference is significant at the 5 percent level (Fisher's exact test, $p = 0.014$).

**Second-order beliefs** The treatment differences in the principals' second-order beliefs are consistent with the agents' actual beliefs. As shown in the lower panel in Table A.4, CE principals find it more likely than NCE principals that their agents do not expect any control from them, while the opposite is true for the second-order beliefs regarding maximum control. For the intermediate level of control, we do not observe a significant treatment effect.

## 4.4 Real-Effort Game

In this section, we investigate how a common experience between the principal and agent affects effort and control decisions in the real-effort experiment. Most importantly, the real-effort setting allows us to examine the importance of control sensations as a determinant of treatment differences in the magnitude of hidden costs of control.

### Agent

**Sharing decisions** Regarding Conjecture 1, we observe both CE and NCE agents who decide to transfer only what the principals force them to share, while there is voluntary sharing as well.[31] Moreover, the intergroup difference in the proportion of agents deciding to exceed the minimum requirement set by the principal is significant at the 5 percent level (Chi-square test, $p = 0.011$). This result provides further support for Conjecture 3. Notice also that about 24 percent of the CE agents are willing to transfer half of their earnings to the respective principal, while only 4 percent of the NCE agents choose an equal split (Chi-square test, $p < 0.001$). No agent chooses to transfer more than 50 percent of his income.

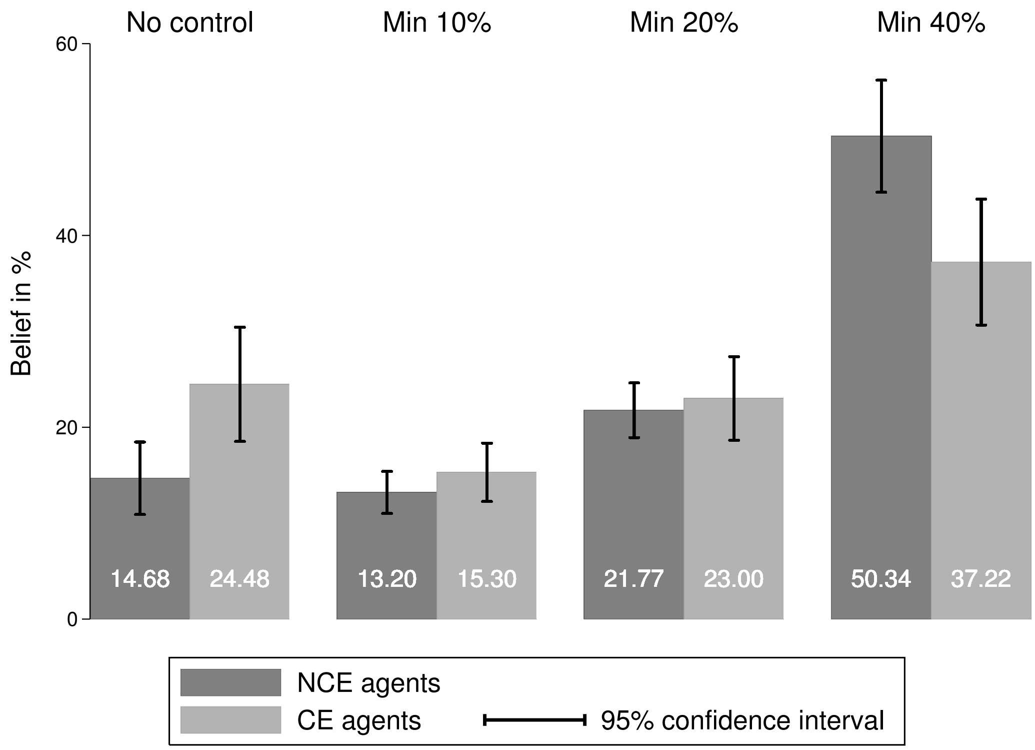
In the real-effort game, the number of CE and NCE observations differs for each control level, so it may be misleading to analyze treatment differences by the level of control. However, similar to our findings from the effort-choice game, at *any* control level, agents who had a shared experience with their principals in the coordination game transfer more than agents without such prior interactions.[32] Pooling across control levels, we find that the difference in total sharing between agent types is significant at the 1 percent level (38.48 vs. 33.77; Mann-Whitney test, $p = 0.003$). These results again show that the benefits of a previous common experience of the principal and agent materialize in a higher willingness of the agent to act on behalf of the principal (see Conjecture 4).

**Beliefs** Our findings regarding the agents' beliefs also match the previous results from the effort-choice game well. We observe significant treatment differences in the beliefs for no and maximum control, while the beliefs are statistically indistinguishable between groups for intermediate levels of control (Figure 3).

[31] In what follows, we only consider the agents' transfer decisions and ignore the actual performance in solving summations. We do so to ensure that effort is still fully under volitional control (van Knippenberg, 2000), just as in the effort choice game.

[32] These differences are not always significant, which is due to the low number of observations for control levels of 0 and 10, respectively. See Table A.5 for details.

**Figure 3:** Real-Effort Game: Agents' Control Beliefs by Agent Type




*Note*: In this figure, we depict the agents' beliefs regarding the various control levels that the principals may choose. Average beliefs are shown. CE agents expect their principals to trust them completely significantly more often than their NCE counterparts do (Mann-Whitney test, $p = 0.030$), and expect to face maximum control significantly less frequently ($p = 0.001$). There are no significant differences for intermediate levels of control (*Min 10%*: $p = 0.415$; *Min 20%*: $p = 0.939$). The results are based on observations of 63 CE and 79 NCE agents.

Next, we turn to the modal control beliefs, which are our experimental equivalent for the individually expected level of control, denoted by $\hat{m}$ in the model.[33] As in the effort-choice game, we again find that NCE agents have higher control expectations than their CE counterparts, supporting Conjecture 5. On average, NCE agents find a control level of 31.91 the most likely, which is significantly higher than CE agents' average modal control beliefs, 19.65 (Mann-Whitney test, $p < 0.001$). The respective medians are 40 for NCE and 20 for CE agents.

**Hidden costs of control** We now analyze hidden costs of control in the real-effort game. In contrast to the effort-choice game, where we observe three effort choices for each subject

[33] Twelve agents (six NCE and six CE agents) did not regard any of the four control levels as the most likely to occur. For these subjects we could not identify modal beliefs.

and can thus account for individual fixed effects, we now only have one effort observation by individual. Thus, the results in Table 4 are based on multivariate OLS regressions.[34] The outcome variable is the agent's transfer to the principal, expressed as a share of the agent's total earnings from solving equations. The main explanatory variables are the principal's individual control choices (Columns 1 and 3) or a dummy variable that takes the value of 1 when the principal decided for a high control level, that is, control of either 20 or 40 (Columns 2 and 4). It is important to notice that we do not perform the regressions separately for all agents and both groups individually, as we did in Table 2, because we now have to account for the treatment difference when the principal does not control. In the above fixed effects estimation this initial difference was dropped when we calculated differences between agent types in the response to control.

All models use information on the subjects' gender, age, previous experimental experience, number of semesters, and academic major to capture individual heterogeneity.[35] Moreover, we control for a number of additional confounding factors. On the one hand, to rule out that our results are just driven by positive reciprocity to nice behavior by the principal in the weakest-link game, we include the agents' profits from the initial stage of the experiment. On the other hand, we also account for the agent's previous effort choices, because the transfer in the real-effort game may be influenced by the decisions in the effort-choice game. We further include session dummies to control for session-specific effects (for instance, cohort size). Columns 3 and 4 contain the agents' modal beliefs in the real-effort game as an additional control variable. This is to check whether different patterns in control aversion for CE and NCE agents can be explained by treatment differences in the control beliefs.

Corroborating our results from the effort-choice game, we find that control entails hidden costs, which, at least for low control, should be taken seriously, because they are sufficiently high to outweigh the benefits. Moreover, the crowding-out effect of control varies substantially by agent type; in fact, it primarily occurs for CE agents. In Column 1, the positive and significant coefficient on *CE agent* indicates that CE agents transfer more of their earnings than NCE agents at the base level of no control. The coefficient on *Min 10* implies that NCE agents reduce their transfer to the principal by little more than 8 percentage points when facing a control level of 10. The transfer chosen by NCE agents for a control level of 20 is not statistically different from the transfer to a trusting principal. In the case of maximum control, the disciplining effect of control completely dominates the crowding-out

[34] We also conducted negative binomial regressions to test the robustness of the econometric model. The results (provided in Table A.8 in Appendix A) reinforce our OLS findings discussed below. In fact, most results get even stronger when negative binomial regressions are performed.

[35] Ploner, Schmelz and Ziegelmeyer (Forthcoming) find that especially the subjects' educational background influences the existence and magnitude of hidden costs of control.

effect for NCE agents. The coefficient on *Min 40* suggests that, relative to no control, NCE agents increase their transfer by almost 19 percentage points when facing maximum control.

The interaction terms show how the behavioral reaction to control differs between CE and NCE agents. First, observe that all interaction terms have a negative sign. This indicates that, once they are controlled, CE agents reduce their transfers by a larger amount than NCE agents. The treatment difference is significant for control levels of 20 and 40. However, CE agents who face a control level of 10 significantly lower their transfers relative to the no control case; the marginal effect is negative and significant at the 1 percent level ($p = 0.007$). If a CE agent is forced to transfer at least 20, the implementation of control is still detrimental to the principal, with the marginal effect being significant at the 5 percent level ($p = 0.040$). Strikingly, even maximum control is not effective in increasing the agent's transfer. The marginal effect for maximum control is positive, but small in magnitude and highly insignificant ($p = 0.873$).

Column 2 provides further evidence for treatment differences in the existence of hidden costs of control. The positive and highly significant coefficient on *High control* shows that ruling out transfer choices between zero and twenty pays off for the principal in the NCE treatment. On average, the transfer he receives when implementing strong control is approximately 19 percentage points higher than when he decides for no or low control. A different picture is again taking shape for CE agents. Their reaction to a control level of either 20 or 40 is less pronounced than what we observe for NCE agents, as indicated by the negative coefficient on *CE agent* × *High control*. The marginal effect is insignificant ($p = 0.118$), which suggests that when high control is imposed in the CE treatment, the motivation crowding-out effect and the disciplining effect break even. Holding constant the agents' control expectations, as we do in Columns 3 and 4, does not affect these findings.

**Table 4:** Real-Effort Game: Hidden Costs of Control

| Dependent variable: Agent sharing | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| CE agent | 19.436*** | 13.926** | 18.875*** | 12.030* |
| | (5.149) | (5.766) | (6.057) | (6.054) |
| Min 10 | -8.096** | | -8.791** | |
| | (3.832) | | (4.328) | |
| Min 20 | 4.777 | | 4.560 | |
| | (3.846) | | (4.054) | |
| Min 40 | 18.556*** | | 17.929*** | |
| | (3.586) | | (3.811) | |
| CE agent × Min 10 | -9.630 | | -9.538 | |
| | (7.712) | | (7.700) | |
| CE agent × Min 20 | -13.992** | | -12.879** | |
| | (5.593) | | (6.070) | |
| CE agent × Min 40 | -17.904*** | | -17.914*** | |
| | (5.479) | | (6.163) | |
| High control | | 18.599*** | | 18.190*** |
| | | (3.316) | | (3.438) |
| CE agent × High control | | -10.733* | | -9.598* |
| | | (5.744) | | (5.683) |
| Modal control beliefs | No | No | Yes | Yes |
| Individual heterogeneity | Yes | Yes | Yes | Yes |
| Further controls | Yes | Yes | Yes | Yes |
| Session dummies | Yes | Yes | Yes | Yes |
| Observations | 126 | 126 | 117 | 117 |
| $R$-squared (overall) | 0.979 | 0.958 | 0.978 | 0.960 |

*Note:* The table shows the results of OLS regressions of control on the agents' transfer to the principal in the real-effort game. In Columns 1 and 3, 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. In Columns 2 and 4, *High control* is a dummy variable that indicates whether or not the agent faced a control level of either 20 or 40. In Columns 1 and 2, we do not account for the agents' beliefs about the principal's control. In Columns 3 and 4, we additionally include the agents' modal control beliefs. *CE agent* is a binary variable, taking the value of 1 if the principal and agent played the coordination game at the beginning of the experiment and 0 otherwise. Individual heterogeneity is controlled for using biographic information on gender, age, experimental experience, number of semesters, and academic major. Experimental experience is a binary variable, which takes the value of 1 if a subject participated in an experiment in the past and 0 otherwise. The subject's academic major belongs to either of the following categories (see also Ploner, Schmelz and Ziegelmeyer see also Forthcoming: business administration and economics, other social sciences, humanities, engineering and natural sciences, as well as unknown/no student. The further controls comprise the agents' modal control beliefs, their profits from the first stage of the experiment, and their effort choices from the second stage. 16 out of 142 agents (eight NCE and CE agents, respectively) did not provide biographic information in the questionnaire at the end of the experiment. Thus, the results in Columns 1 and 2 are based on 126 agent observations. Nine of these agents (four NCE agents and five CE agents) did not regard a single control level as the most likely to be chosen by their principals, so the number of agent observations in Columns 3 and 4 reduces to 117. Heteroskedasticity-robust standard errors are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

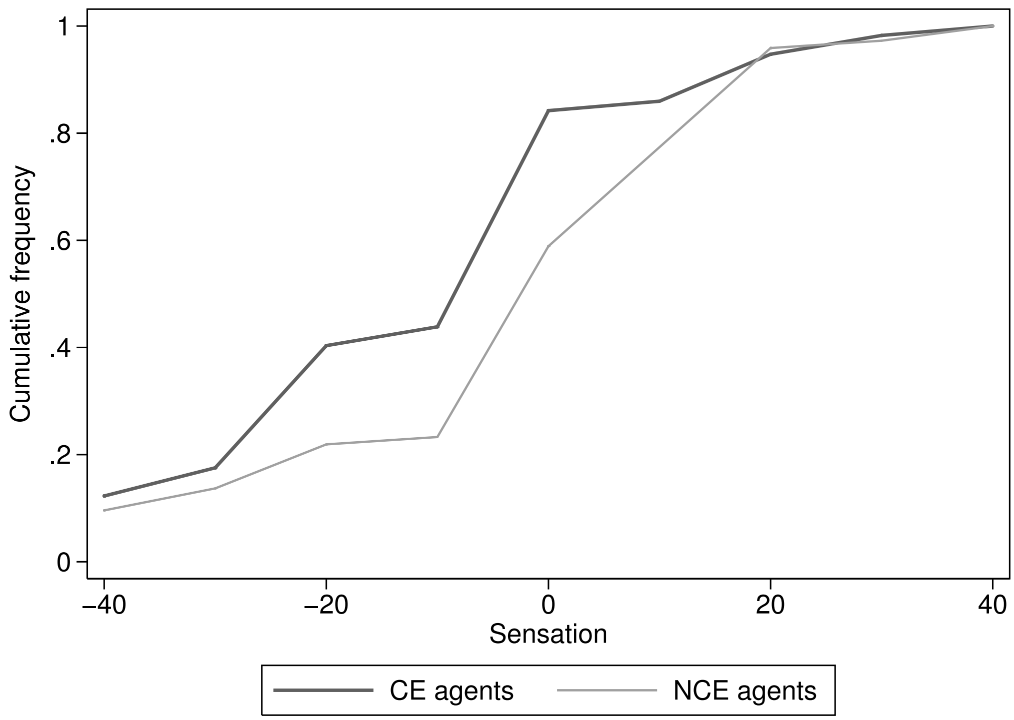
In sum, the evidence collected in the real-effort game shows that controlling CE agents, who otherwise are loyal to their principals, is often counterproductive because it can severely reduce the agent's intrinsic motivation to act on the principal's behalf. Unless control has high power, this crowding-out effect actually decreases the principal's payoff. For agents in anonymous and distant relationships to their principals, that is, NCE agents, control is a much more effective tool in disciplining the agent. Thus far, however, the underlying causes of hidden costs of control are still obscure. In the next step, we open this black box and investigate whether disappointment is a possible explanation for treatment differences in the degree of control aversion.

**Disappointment** In both the effort-choice game and real-effort game, we observe that hidden costs of control are more substantial in the CE treatment than in the NCE treatment. Complementing the preliminary evidence from the effort-choice game, we now provide further support for the idea that one essential determinant of the observed differences between agent types in the reaction to control is disappointment. We will show that only CE agents sanction principals whose implementation of control exceeds the agent's control expectations. This agent heterogeneity in the response to negative sensations is likely to explain our aggregate finding that CE agents exhibit a higher degree of control aversion than NCE agents (see Section 2.2).

To investigate treatment differences in the agent's reaction to disappointment, we use our concept of sensation, which we refer to as the deviation of the agent's modal control belief from the experienced level of control. If the level of control implemented by the principal is higher than what the agent expected, we think of him as being disappointed. In contrast, if the principal's control is below the agent's expectation, the latter may face a positive surprise. Figure 4 presents the cumulative distributions of sensation for the two agent types. It becomes apparent that CE agents are more (less) likely to face a negative (positive) sensation than their NCE counterparts. Accordingly, the average value of CE agents' sensations is negative (-7.72), while NCE agents experience positive sensations on average (2.05). The median of sensation is zero for both agent types.[36]

[36] All results pertaining to sensation continue to hold if we drop those agents who did not provide biographic information, which would leave us with a sample of 117 agents.

**Figure 4:** Real-Effort Game: Cumulative Distribution of Agent Sensation by Treatment




*Note:* This figure shows the cumulative distribution functions of the sensation experienced by CE and NCE agents, respectively. Sensation is the difference between expected and realized control. Underlying this figure are 130 agent observations, because twelve agents (six CE and NCE agents, respectively) did not regard a single control level as the most likely to be chosen by their principals. For these subjects a measure for sensation could not be constructed. We can reject similarity of agent types' CDFs at a significance level of 5 percent (Kolmogorov-Smirnov test, $p = 0.011$).

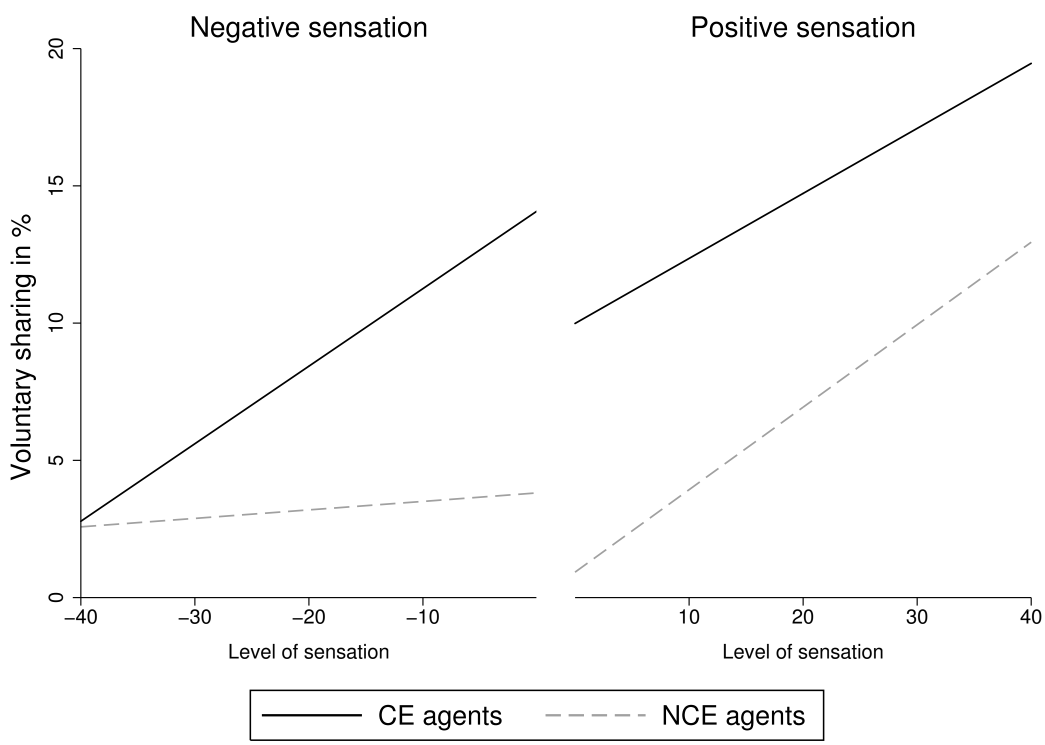
Next, we focus on the question whether the behavioral reaction to sensation is different between treatments.[37] Before we turn to the regression analysis, we show the agents' sharing responses to sensation graphically in Figure 5. Some results are noteworthy. First, in the absence of any sensation, there is almost a 12:1 difference in the inclination for voluntary sharing between agent types. On average, CE agents voluntarily share approximately 13.65 percent of their earnings if there is no sensation, while the voluntary sharing of NCE agents

[37] Just as in the effort-choice game, we look at voluntary sharing to shed light on this question. Since the construction of our sensation variable does not permit to account for the principal's control choice in the below regressions, we need a measure of effort that does not directly depend on the principal's control choice. Thus, we use voluntary sharing instead of total sharing as outcome. This change in the outcome variable does not affect our results on the differences in the reaction to sensation between agent types for two reasons: First, as we will show below, CE principals do not significantly differ from NCE principals in the magnitude of their control. Second, even if treatment differences with respect to control exist, they are likely to be captured by the sensation measure to a considerable extent.

is only slightly more than 1 percent in this case (Mann-Whitney test, $p < 0.001$). Second, support for Conjecture 2 comes from the observation that all sensation-response functions are positively sloped. Third, as suggested by Conjecture 4, CE agents share more voluntarily than their NCE counterparts at any level of sensation.

Moreover, the graph illustrates that Conjecture 6 is also supported by the data, and it refines Conjecture 7. Considering negative sensations plotted in the left panel in Figure 5, the drop in the sharing increment is more pronounced for CE agents than for NCE agents. Because the intergroup gap in voluntary sharing shrinks in the negative sensation, there is almost no difference between agent types at the minimum level of sensation, -40. For positive sensations, depicted in the right panel in Figure 5, however, there is no visible difference between CE and NCE agents in the reciprocal reaction to sensations.

**Figure 5:** Real-Effort Game: Agents' Reaction to Sensation by Treatment and Sensation Type (Linear Fit)




*Note:* This figure presents linear fitted graphs of the relationship between sensation and voluntary sharing. We use data for positive (right panel) and negative (left panel) sensations to construct the figure. Zero sensations are extrapolated. This figure is based on 130 agent observations. See Figure 4 for details of the sample.

To explore more rigorously how the agent's response to sensation differs by agent type we estimate multivariate OLS regressions.[38] The dependent variable is the agent's transfer to the principal beyond the latter's minimum requirement. As in Table 4, all regression models use demographic variables to control for individual heterogeneity. Moreover, the regressions contain the decisions from previous stages of the experiment as additional regressors to account for behavioral patterns in the exerted effort that are constant across games.

From Column 1, it becomes apparent that voluntary sharing increases in the sensation, providing support for Conjecture 2. The coefficient on *Sensation* is positive and significant at the 5 percent level. In Column 2, the positive and significant coefficient on *CE agent* indicates that, in the absence of any sensation, CE agents share more voluntarily than NCE agents. The interaction term *CE agent* × *Sensation* allows for treatment effects in the response to sensation. Due to the inclusion of the interaction term, *Sensation* in Column 2 refers only to NCE agents. Although the positive impact of sensation on voluntary sharing is somewhat weaker for NCE agents than for the total sample, it is still positive and significant. The insignificant interaction term suggests that CE and NCE agents increase voluntary sharing in sensation in a similar way.

Column 3 contains our main results. Here, we additionally consider the nature of the sensation. The positive and significant coefficient on *Positive sensation* shows that NCE agents voluntarily share the more the larger the (positive) difference between expected and experienced control is. The negative coefficient on *CE* × *Pos. sensation* indicates that CE agents reciprocate less than NCE agents when the control level faced falls short of the agent's expectation. However, the interaction term is not statistically significant.[39]

Facing a negative sensation, NCE agents do not react in terms of voluntary sharing; the coefficient on *Negative sensation* is insignificant. However, there is a significant interaction effect. The positive coefficient on *CE* × *Neg. sensation* shows that an CE agent who experiences a negative sensation decreases voluntary sharing by more than an NCE agent being confronted with a negative sensation of similar magnitude. This result provides support for Conjecture 6. In fact, it confirms our hypothesis that disappointment is an important determinant of differences in control aversion between agent types. Corroborating this conclusion, we find that (only) CE agents reciprocate negative sensations; the marginal effect is 0.26, being significant at the 5 percent level ($p = 0.040$). In sum, we find pronounced treatment

[38] We provide the results of negative binomial estimates in Table A.8 in Appendix A.

[39] We find a significant interaction term in the corresponding negative binomial estimations in Table A.9. A rationale for this result is provided by the theory of intention-based reciprocity (for instance, Charness and Rabin, 2002; Dufwenberg and Kirchsteiger, 2004; Rabin, 1993). As there is more uncertainty about the agents' propensities for acting selfish in the NCE treatment than in the CE treatment, imposing loose (or no) control is a particularly risky option for NCE principals. Hence, an NCE agent, being aware that the principal cannot know his type, may be more likely to consider a low level of control as a kind action, which he then reciprocates (von Siemens, 2011).

differences in the reactions to sensations, but these are only visible when accounting for the nature of the sensation.

**Table 5:** Real-Effort Game: Behavioral Effects of Control Sensations

| Dependent variable:<br>Agent voluntary sharing | (1) | (2) | (3) |
|---|---|---|---|
| Sensation | 0.109** | 0.100* | |
| | (0.050) | (0.055) | |
| CE agent | | 7.422** | 10.623*** |
| | | (2.906) | (3.529) |
| CE agent × Sensation | | 0.118 | |
| | | (0.104) | |
| Positive sensation | | | 0.288** |
| | | | (0.118) |
| CE agent × Pos. sensation | | | -0.166 |
| | | | (0.281) |
| Negative sensation | | | -0.037 |
| | | | (0.079) |
| CE agent × Neg. sensation | | | 0.292** |
| | | | (0.139) |
| Individual heterogeneity | Yes | Yes | Yes |
| Further controls | Yes | Yes | Yes |
| Session dummies | Yes | Yes | Yes |
| Observations | 117 | 117 | 117 |
| $R$-squared (overall) | 0.558 | 0.596 | 0.610 |

*Note:* This table reports the results of OLS regressions of sensation on voluntary sharing, that is, the agent's transfer beyond the principal's minimum requirement. *Sensation* is measured as the difference between expected and actual control, with expected control being approximated by the agent's modal control belief. *Positive sensation* indicates the level of the sensation if the sensation is strictly above zero. Otherwise, *Positive sensation* equals 0. *Negative sensation* is defined accordingly; that is, it exhibits non-zero (and negative) values if the sensation is strictly below zero. See Table 4 for details on the sample and construction of variables. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

### Principals

**Control** The principals' control decisions and second-order beliefs are shown in Table A.6. In contrast to the results of the effort-choice game, there are no treatment differences in the principal's choice of control (Fisher's exact test, $p = 0.126$).[40] It is also apparent from Table A.6 that only 8 percent (3 percent) of the CE (NCE) principals decide not to restrict the agent at all, with an insignificant difference between agent types (Fisher's exact test, $p = 0.171$). This result indicates that the majority of principals feels that no or low control are too risky choices.

A possible reason for this behavioral pattern is that the agents' profits from solving equations are uncertain, as performance in the real-effort task is not under the agents' full volitional control. Both CE and NCE principals, being unaware of the agents' math skills, may thus seek to minimize their risk of getting a low absolute transfer by imposing strong control. Moreover, it may also be that both types of principals expect a relatively low inclination of the agent to share voluntarily what he had to earn before. The absence of differences across treatments in the inclination to control, while at the same time CE agents expect to face less control than NCE agents do, explains our above finding that CE agents face a negative sensation more often than their NCE counterparts.

**Second-order beliefs** The lower panel in Table A.6 displays that, compared to NCE principals, CE principals expect their agents to believe that they are not controlled more frequently and maximally controlled less frequently. The treatment differences for these polar cases of control are significant at the 1 percent level. There are no treatment effects for the two intermediate control levels. These results are consistent with those from the effort-choice game. However, since CE and NCE principals do not differ in their actual control choices, the significant treatment differences in the second-order beliefs do not translate into behavioral differences between principal types.

# 5 Conclusions

The literature on psychology in organizations recognizes group coherence as a powerful concept to explain individual behavior, for instance, promotion decisions (Fajak and Haslam, 1998) and turnover intentions (van Dick et al., 2004; Haslam, 2001). In a series of papers, George A. Akerlof and Rachel E. Kranton introduced the concept of identity, and group membership based hereon, into mainstream economics (Akerlof and Kranton, 2000, 2002, 2005, 2008). Drawing on their utility-function approach, this paper develops a simple model in which an agent chooses to exert costly effort that has payment consequences for both the

[40] To enhance the comparability with the above results for the agents, we also dropped those principals being matched with an agent who did not give us demographic information or for whom we were not able to construct modal beliefs. The results are robust to this retrenchment of the sample.

principal and agent. The principal can restrict the agent's choice set by imposing a minimum allowable effort level. In this modeling framework, we explore whether an agent who previously interacted with the principal (CE relationship) reacts differently to the principal's use of control devices than an agent not having made a common experience with the principal in the past (NCE relationship).

We experimentally test the conjectures derived from the model and related literature in two types of games. In the effort-choice game, the agent's effort is a transfer out of a windfall endowment provided by the experimenter (see also Falk and Kosfeld, 2006; Ploner, Schmelz and Ziegelmeyer, Forthcoming). In the real-effort game, the agent performs a task and shares part of his work outcome with the principal. We manipulate the social distance between the principal and agent with the help of a team-building exercise that the CE subjects pass through in the beginning of the experiment.

The induction of group membership was evident in the behavioral choices, as CE agents supply more effort than NCE agents for all control levels available for the principal to choose. At the same time, CE agents expect to be controlled significantly less by their principals. The principals, however, do not always meet these expectations. In the effort-choice game, we observe that CE principals control less frequently and less severe than their NCE counterparts. But when the agent's transfer is not simply paid from a windfall endowment, both types of principals seem to expect little voluntary sharing and, thus, high benefits of (strong) control.

In general, our findings are consistent with earlier studies showing that membership in social groups tends to enhance pro-social behavior (among others, Chen and Li, 2009; Goette, Huffman and Meier, 2011; Heap and Zizzo, 2009; Sutter, 2009). Beyond that, however, we provide evidence that social group coherence yields important hidden costs. For CE agents, the crowding-out effect of control dominates the disciplining effect for a wider range of control levels than for NCE agents. A candidate mechanism explaining that hidden costs of control primarily occur for CE agents is the significant drop in the CE agent's performance when the degree of control implemented by the principal exceeds the agent's expectation, which has no equivalent for NCE agents. Compared to their NCE counterparts, CE agents seem to attach a higher emotional significance to the principal's control decision, so they are more disappointed when a negative control sensation occurs.

Interestingly, for both agent types, the crowding-out effect of control is especially severe when the degree of control implemented by the principal is low, which adds to the discussion raised by Ploner, Schmelz and Ziegelmeyer (Forthcoming) on the results of Falk and Kosfeld (2006). Since imposing a low minimum effort has little enforcement power, agents may interpret it as a signal that the principal expects not to even receive this low transfer. Such lack of trust in the agent's willingness to share voluntarily is severely punished.

Most previous studies that investigate work motivation or task performance in social groups yield evidence in support of a positive impact of groups on motivational and performance-related factors (for an overview, see van Knippenberg, 2000). Our results, however, uncover the dark side of groups. We find that CE agents, who are typically more generous toward their principals, develop the desire for sanctioning unexpectedly 'bad' behavior by the principal, as it is especially upsetting for the agent. NCE agents, in contrast, do not show the motive to retaliate if their control expectations are disappointed. In fact, although the anonymity of group members is maintained in our experiment, control appears to be interpreted as a sign of distrust, entailing substantial hidden costs for the principal. In situations with repeated principal-agent interactions, behavior that is regarded as incongruent with the implied group norms 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 the labor market, as they emphasize the virtues of a consistent leadership style. When a firm decides to impose extrinsic incentives through control devices or related instruments, the concurrent attempt to increase the employees' intrinsic motivation–for instance, via team-building exercises–is not advisable. In fact, both policies appear as strong substitutes in our study. It would be instructive to analyze more systematically the interactions between extrinsic and intrinsic incentives in a real-world environment, potentially through experimental manipulations of team-building exercises within firms.

# References

**Akerlof, George A., and Rachel E. Kranton.** 2000. "Economics and Identity." *Quarterly Journal of Economics*, 115: 715–753.

**Akerlof, George A., and Rachel E. Kranton.** 2002. "Identity and Schooling: Some Lessons for the Economics of Education." *Journal of Economic Literature*, XL: 1167–1201.

**Akerlof, George A., and Rachel E. Kranton.** 2005. "Identity and the Economics of Organizations." *Journal of Economic Perspectives*, 19: 9–32.

**Akerlof, George A., and Rachel E. Kranton.** 2008. "Identity, Supervision, and Work Groups." *American Economic Review Papers and Proceedings*, 98: 212–217.

**Albert, Stuart, and David A. Whetten.** 1985. "Organizational identity." In *Research in organizational behavior.* , ed. L. L. Cummings and Barry M. Staw, 263–295. Greenwich, CT: JAI Press.

**Alchian, Armen A., and Harold Demsetz.** 1972. "Production, information costs, and economic organization." *American Economic Review*, 62: 777–795.

**Antonakis, John, and Leanne Atwater.** 2002. "Leader Distance: A Review and a Proposed Theory." *Leadership Quarterly*, 13: 673–704.

**Ashforth, Blake E., and Fred Mael.** 1989. "Social Identity Theory and the Organization." *The Academy of Management Review*, 14: 20–39.

**Barkema, Harry G.** 1995. "Do Top Managers Work Harder When They Are Monitored?" *Kyklos*, 48: 19–42.

**Bartling, Björn, Ernst Fehr, and Klaus M. Schmidt.** 2012. "Screening, Competition, and Job Design: Economic Origins of Good Jobs." *American Economic Review*, 102: 834–864.

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

**Bruggen, Alexander, and Martin Strobel.** 2007. "Real effort versus chosen effort in experiments." *Economics Letters*, 96: 232–236.

**Buchanan, Bruce.** 1974. "Government Managers, Business Executives, and Organizational Commitment." *Public Administration Review*, 34: 339–347.

**Buchanan, Bruce.** 1975. “Red-Tape and the Service Ethic: Some Unexpected Differences Between Public and Private Managers.” *Administration & Society*, 6: 423–444.

**Buller, Paul F., and Cecil H. Bell Jr.** 1986. “Effects of Team Building and Goal Setting on Productivity: A Field Experiment.” *Academy of Management Journal*, 29: 305–328.

**Camerer, Colin F., and Robin M. Hogarth.** 1999. “The effects of financial incentives in experiments: a review and capital labor production framework.” *Journal of Risk and Uncertainty*, 19: 7–42.

**Chaney, Carole K., and Grace H. Saltzstein.** 1998. “Democratic control and bureaucratic responsiveness: The police and domestic violence.” *American Journal of Political Science*, 42: 745–768.

**Charness, Gary, and Martin Dufwenberg.** 2006. “Promises and partnership.” *Econometrica*, 74: 1579–1601.

**Charness, Gary, and Matthew Rabin.** 2002. “Understanding social preferences with simple tests.” *Quarterly Journal of Economics*, 117: 817–869.

**Charness, Gary, Guillame R. Frechette, and John H. Kagel.** 2004. “How Robust is Laboratory Gift Exchange?” *Experimental Economics*, 7: 189–205.

**Chen, Yan, and Sherry Xin Li.** 2009. “Group Identity and Social Preferences.” *American Economic Review*, 99: 431–457.

**Cherry, Todd L., Peter Frykblom, and Jason F. Shogren.** 2002. “Hardnose the Dictator.” *American Economic Review*, 92: 1218–1221.

**Delgado, Mauricio R., Robert H. Frank, and Elisabeth A. Phelps.** 2005. “Perceptions of moral character modulate the neural systems of reward during the trust game.” *Nature Neuroscience*, 8: 1611–1618.

**Dickinson, David, and Marie-Claire Villeval.** 2008. “Does monitoring decrease work effort?: The complementarity between agency and crowding-out theories.” *Games and Economic Behavior*, 63: 56–76.

**Dufwenberg, Martin, and Astri Muren.** 2006. “Generosity, anonymity, gender.” *Journal of Economic Behavior & Organization*, 61: 42–49.

**Dufwenberg, Martin, and Georg Kirchsteiger.** 2004. “A Theory of Sequential Reciprocity.” *Games and Economic Behavior*, 47: 268–298.

**Ellingsen, Tore, and Magnus Johannesson.** 2008. "Pride and prejudice: The human side of incentive theory." *American Economic Review*, 98(3): 990–1008.

**Enzle, Michael E., and Sharon C. Anderson.** 1993. "Surveillant Intentions and Intrinsic Motivation." *Journal of Personality and Social Psychology*, 64: 257–266.

**Fajak, Amanda, and S. Alexander Haslam.** 1998. "Gender solidarity in hierarchical organizations." *British Journal of Social Psychology*, 37: 73–94.

**Falk, Armin, and Christian Zehnder.** 2007. "Discrimination and in-group favoritism in a citywide trust experiment." *IZA Discussion Paper No. 2765, Zurich IEER Working Paper No. 318.*

**Falk, Armin, and Michael Kosfeld.** 2006. "The Hidden Cost of Control." *American Economic Review*, 96: 1611–1630.

**Falk, Armin, and Thomas Dohmen.** 2010. "You get what you pay for: Incentives and selection in the education system." *Economic Journal*, 102: F256–F271.

**Fehr, Ernst, and Bettina Rockenbach.** 2003. "Detrimental effects of sanctions on human altruism." *Nature*, 422: 137–140.

**Fershtman, Chaim, and Uri Gneezy.** 2001. "Discrimination in a segmented society: An experimental approach." *The Quarterly Journal of Economics*, 116(1): 351–377.

**Fischbacher, Urs.** 2007. "z-Tree: Zurich toolbox for ready-made economic experiments." *Experimental Economics*, 10(2): 171–178.

**Frank, Robert H., Thomas Gilovich, and Dennis T. Regan.** 1993. "The evolution of one-shot cooperation: An experiment." *Ethology and Sociobiology*, 14: 247–256.

**Frey, Bruno S.** 1993. "Does monitoring increase work effort? The rivalry with trust and loyalty." *Economic Inquiry*, 31: 663–670.

**Frey, Bruno S., and Stephan Meier.** 2004. "Social Comparisons and Pro-social Behavior: Testing 'Conditional Cooperation' in a Field Experiment." *American Economic Review*, 94: 1717–1722.

**Gill, David, and Victoria L. Prowse.** Forthcoming. "A Structural Analysis of Disappointment Aversion in a Real Effort Competition." *American Economic Review.*

**Goette, Lorenz, David Huffman, and Stephan Meier.** 2011. "The Impact of Social Ties on Group Interactions: Evidence from Minimal Groups and Randomly Assigned Real Groups." *American Economic Journal: Microeconomics.*

**Greiner, Ben.** 2004. "An Online Recruitment System for Economic Experiments." *GWDG Bericht*, 63: 79–93.

**Grether, David M.** 1992. "Testing Bayes rule and the representative heuristic: some experimental results." *Journal of Economic Behavior and Organization*, 17: 623–38.

**Hardy, Charles J., and R. Kelly Crace.** 1997. "Foundations of Team Building: Introduction to the Team Building primer." *Journal of Applied Sport Psychology*, 9: 1–10.

**Hart, Oliver, and John Moore.** 2008. "Contracts as Reference Points." *Quarterly Journal of Economics*, 123: 1–48.

**Haslam, S. Alexander.** 2001. *Psychology in Organizations: The Social Identity Approach.* Sage Publications.

**Heap, Shaun P. Hargreaves, and Daniel J. Zizzo.** 2009. "The value of groups." *American Economic Review*, 99: 295–323.

**Ingraham, Patricia W.** 1993. "Of Pigs in Pokes and Policy Diffusion: Another Look at Pay-for-Performance." *Public Administration Review*, 53: 348–356.

**Jensen, Michael C., and William H. Meckling.** 1976. "Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure." *Journal of Financial Economics*, 3: 305–360.

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

**Koszegi, Botond, and Matthew Rabin.** 2006. "A Model of Reference-Dependent Preferences." *Quarterly Journal of Economics*, 121: 1133–1165.

**Marchetti, Antonella, Ilaria Castelli, Katia M. Harlé, and Alan G. Sanfey.** 2011. "Expectations and outcome: The role of Proposer features in the Ultimatum Game." *Journal of Economic Psychology*, 32: 446–449.

**Masella, Paolo, Stephan Meier, and Philipp Zahn.** 2012. "Incentives and Group Identity."

**McDougall, William.** 1920. *The Group Mind.* Cambridge: Cambridge University Press.

**Niederle, Muriel, and Lise Vesterlund.** 2007. "Do women shy away from competition? Do men compete too much?" *Quarterly Journal of Economics*, 122: 1067–1101.

**Perry, James L., Beth Ann Petrakis, and Theodore K. Miller.** 1989. "Federal Merit Pay, Round II: An Analysis of the Performance Management and Recognition System." *Public Administration Review*, 49: 29–37.

**Ploner, Matteo, Katrin Schmelz, and Anthony Ziegelmeyer.** Forthcoming. "Hidden Costs of Control: Three Repetitions and an Extension." *Experimental Economics.*

**Porta, Rafael La, Florencio Lopez de Silane, Andrei Shleifer, and Robert W. Vishny.** 1997. "Trust in large organizations." *American Economic Review*, 87: 333–338.

**Rabin, Matthew.** 1993. "Incorporating Fairness Into Game Theory and Economics." *American Economic Review*, 83: 1281–1302.

**Rainey, Hal G.** 1983. "Public Agencies and Private Firms: Incentive Structures, Goals, and Individual Roles." *Administration & Society*, 15: 207–242.

**Reuben, Ernesto, Paola Sapienza, and Luigi Zingales.** 2009. "Is mistrust self-fulfilling?" *Economics Letters*, 104: 89–91.

**Ross, Lee.** 1977. "The "False Consensus Effect": An Egocentric Bias in Social Perception and Attribution Processes." *Journal of Experimental Social Psychology*, 13: 279–301.

**Salas, Eduardo, Drew Rozell, Brian Mullen, and James E. Driskell.** 1999. "The Effect of Team Building on Performance: An Integration." *Small Group Research*, 30: 309–329.

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

**Schnedler, Wendelin, and Radovan Vadovic.** 2011. "Legitimacy of Control." *Journal of Economics & Management Strategy*, 20: 985–1009.

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

**Selten, Reinhard.** 1967. "Die Strategiemethode zur Erforschung des eingeschraenkt rationalen Verhaltens im Rahmen eines Oligopolexperimentes." In *Beitraege zur experimentellen Wirtschaftsforschung.* , ed. Heinz. Sauermann, 136–168. Tuebingen: J.C.B. Mohr.

**Shandler, Michael, and Michael Egan.** 1996. *Vroom!: Turbo-charged team building.* New York: AMACOM.

**Sliwka, Dirk.** 2007. "Trust as a Signal of a Social Norm and the Hidden Costs of Incentive Schemes." *American Economic Review*, 97: 999–1012.

**Stanton, Jeffrey M.** 2000. "Reactions to Employee Performance Monitoring: Framework, Review, and Research Directions." *Human Performance*, 13: 85–113.

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

**Tajfel, Henri,** ed. 1978. *Differentiation between social groups. Studies in the social psychology of intergroup relations.* Academic Press.

**Turner, John C.** 1981. "The experimental social psychology of intergroup behavior." In *Intergroup behaviour.* , ed. John C. Turner and Howard Giles, 66–101. Chicago: University of Chicago Press.

**van Dick, Rolf, Oliver Christ, Jost Stellmacher, Ulrich Wagner, Oliver Ahlswede, Cornelia Grubba, Martin Hauptmeier, Corinna Hoehfeld, Kai Moltzen, and Patrick A. Tissington.** 2004. "Should I Stay or Should I Go? Explaining Turnover Intentions with Organizational Identification and Job Satisfaction." *British Journal of Management*, 15: 1467–8551.

**van Dijk, Frans, and Frans van Winden.** 1997. "Dynamics of Social Ties and Local Public Good Provision." *Journal of Public Economics*, 94: 323–341.

**van Knippenberg, Daan.** 2000. "Work Motivation and Performance: A Social Identity Perspective." *Applied Psychology*, 49: 357–371.

**van Knippenberg, Daan, and Els C. M. van Schie.** 2000. "Foci and correlates of organizational identification." *Journal of Occupational and Organizational Psychology*, 73: 137–147.

**von Siemens, Ferdinand.** 2011. "Intention-Based Reciprocity and the Hidden Costs of Control." *CESifo Working Paper Series No. 3553.*

**Warwick, Donald P.** 1980. *A theory of public bureaucracy.* Harvard University Press.

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

**Worchel, Stephen, Hank Rothgerber, Eric Anthony Day, Darren Hart, and John Butemeyer.** 1998. "Social identity and individual productivity within groups." *British Journal of Social Psychology*, 37: 389–413.

# A Appendix

## The Optimization Program

The agent maximizes equation (2) w.r.t. $e$ s.t. $e \geqslant m$, where $m$ is the control choice of the principal, taken as given by the agent.

$$\max\, U_A\,(e; m) = g\,(\Delta, e) \tag{A.1}$$

$$\text{subject to } e \quad \geq \quad m$$

The Lagrangian to this problem is:

$$\mathcal{L}_A\,(\cdot) = g\,(\Delta, e) + \lambda\,(m - e) \tag{A.2}$$

The Kuhn-Tucker conditions read:

$$\frac{\partial \mathcal{L}_A}{\partial e} = g'(\Delta, e) - \lambda \leq 0 \quad (e \geq 0) \tag{A.3}$$

$$\frac{\partial \mathcal{L}_A}{\partial \lambda} = m - e \geq 0 \quad (\lambda \geq 0) \tag{A.4}$$

$$e^*\,[g'(\Delta, e) - \lambda] = 0 \tag{A.5}$$

$$\lambda\,(m - e) = 0 \tag{A.6}$$

$$e, \lambda \geq 0 \tag{A.7}$$

Rearranging leads to the following conditions:

- $e = 0 \vee g'(\Delta, e) - \lambda = 0$
- $\lambda = 0 \vee \lambda\,(m - e) = 0$

$I.$) If $\lambda = 0$ (the constraint is non-binding)

$a$) $e = 0$: no solution since Assumption 3 holds;

$b$) $g'(\Delta, e) = 0$: possible solution.

$II$.) If $\lambda \neq 0$, it follows that $e = m$: possible solution.

One solution to the agent's maximization problem is to not exert any effort beyond the minimum requirement set by the principal, that is, a binding constraint can be optimal (solution $II$). In the case of a positive effort increment, solution $I.b$ determines how the agent responds (in terms of effort) to changes in the experienced control sensation. We use the implicit function theorem to derive an expression for the derivative $de/d\Delta$ without imposing the functional form of the implicit function $g'(\Delta, e)$. We obtain:

$$\frac{de}{d\Delta} = -\frac{\partial^2 g}{\partial \Delta \partial e} / \frac{\partial^2 g}{\partial e^2} \tag{A.8}$$

From Assumptions 3 and 4, it follows that $de/d\Delta$ is positive.

## Randomization

**Table A.1:** Randomization Table: Categorical Variables

| | Treatment | | | |
|---|---|---|---|---|
| | NCE | | CE | |
| | Count | Std. Dev. | Count | Std. Dev. |
| **Gender** | | | | |
| Male (n=150) | 69 | (4.7) | 81 | (5.5) |
| Female (n=180) | 89 | (4.3) | 91 | (5.6) |
| Pearson chi2(1): | 0.389 | | | |
| *p*-value: | 0.533 | | | |
| **Faculty** | | | | |
| Biology Pharmaceutics (n=28) | 11 | (3.9) | 17 | (5.4) |
| Chemistry (n=7) | 5 | (5.1) | 2 | 2.8) |
| Applied Sciences Jena (n=33) | 21 | (4.5) | 12 | (5.1) |
| Mathematics and Informatics (n=8) | 4 | (5.4) | 4 | (8.6) |
| Humanities (n=80) | 42 | (4.2) | 38 | (5.7) |
| No Student (n=12) | 5 | (4.8) | 7 | (6.3) |
| Medicine (n=9) | 5 | (5.2) | 4 | (5.9) |
| Philosophy (n=40) | 16 | (4.5) | 24 | (5.3) |
| Physics and Astronomy (n=7) | 4 | (4.2) | 3 | (6.1) |
| Law (n=22) | 12 | (4.4) | 10 | (6.4) |
| Theology (n=2) | 0 | (0.0) | 2 | (4.9) |
| Economics (n=46) | 17 | (4.2) | 29 | (5.4) |
| Not specified (n=36) | 16 | (4.6) | 20 | (5.8) |
| Pearson chi2(11): | 12.400 | | | |
| *p*-value: | 0.334 | | | |
| **Experience in Economic Experiments** | | | | |
| Yes (n=213) | 106 | (4.6) | 107 | (5.4) |
| No (n=81) | 36 | (4.3) | 45 | (6.0) |
| Not specified (n=36) | 16 | (4.6) | 20 | (5.8) |
| Pearson chi2(1): | 0.665 | | | |
| *p*-value: | 0.415 | | | |
| **Total** (n=330) | 158 | (4.5) | 172 | (5.1) |

*Note:* This table presents the randomization of the treatment regarding the categorical variables, that is, gender, subject of study, and previous experience in economic experiments. We perform Pearson's chi-squared tests for the equality of distributions of the participants' characteristics in the treatment group and the control group, respectively.

**Table A.2:** Randomization Table: Continuous Variables

| | NCE | CE | Difference | *p*-value |
|---|---|---|---|---|
| Age | 22.408 | 22.257 | 0.152 | 0.688 |
| | (3.151) | (3.314) | | |
| | [142] | [152] | | |
| Semester | 3.569 | 3.230 | 0.349 | 0.355 |
| | (3.336) | (2.985) | | |
| | [137] | [145] | | |

*Note:* This table presents the randomization of the treatment regarding the age and the semester of the experimental subjects. Standard deviations are shown in parentheses, the number of observations is listed in brackets. The group-specific means are tested on equality using a two-sample t-test. The number of observations varies between the variables, because of differences in the subjects' response behaviors.

## Effort-Choice Game

**Table A.3:** Effort-Choice Game: Summary Statistics of Effort and Beliefs by Agent Type

| | | **Agent type** | | |
|---|---|---|---|---|
| **Performance** | | **NCE** | **CE** | ***Diff.*** |
| *if No control* | Average | 26.65 | 39.90 | -13.26*** |
| | Median | 23 | 39 | |
| *if Min 6* | Average | 22.73 | 31.52 | -8.79** |
| | Median | 17 | 30 | |
| *if Min 21* | Average | 28.39 | 33.84 | -5.45*** |
| | Median | 21 | 30 | |
| **Belief** | | **NCE** | **CE** | ***Diff.*** |
| *No control* | Average | 21.47 | 36.59 | -15.12*** |
| | Median | 20 | 35 | |
| *Min 6* | Average | 23.96 | 22.68 | 1.28 |
| | Median | 20 | 20 | |
| *Min 21* | Average | 54.57 | 40.73 | 13.84*** |
| | Median | 50 | 40 | |
| Observations: 142 agents | | | | |

*Note:* This table reports summary statistics of the agents' effort decisions and beliefs by treatment. Voluntary sharing is the effort chosen by the agent beyond the minimum requirement imposed by the principal. The beliefs are the 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). Effort levels and beliefs are compared using a Mann-Whitney test. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The results are based on 63 CE and 79 NCE agents.

**Table A.4:** Effort-Choice Game: Summary Statistics of Control Choices and Second-Order Beliefs by Principal Type

| | | Principal type | | |
|---|---|---|---|---|
| **Control decision** | | **NCE** | **CE** | ***Total*** |
| *No control* | | 4 | 12 | 16 |
| *Min 6* | | 24 | 15 | 39 |
| *Min 21* | | 51 | 36 | 87 |
| ***Total*** | | 79 | 63 | 142 |
| *Fisher's exact test*, *p*-value: 0.031 | | | | |
| **S.O. belief** | | **NCE** | **CE** | ***Diff.*** |
| *No control* | Average | 11.43 | 25.75 | -14.32*** |
| | Median | 10 | 20 | |
| *Min 6* | Average | 32.51 | 26.92 | 5.59 |
| | Median | 30 | 30 | |
| *Min 21* | Average | 56.06 | 47.33 | 8.73* |
| | Median | 55 | 50 | |
| Observations: 142 principals | | | | |

*Note:* This table reports the summary statistics of the principals' control decisions and beliefs in the effort-choice game, differentiated by treatment. Control decisions are given as counts. The beliefs of the principal are the second-order beliefs over the control beliefs of the agents. Hence, the means of the second-order beliefs add up to 100 (disregarding rounding errors). Beliefs are compared using a Mann-Whitney test, while the differences in the averages between NCE and CE principals are shown: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The results are based on our preferred sample of 63 CE and 79 NCE principal observations.

## Real-Effort Game

**Table A.5:** Real-Effort Game: Summary Statistics of Sharing Decisions and Beliefs by Agent Type

| | | Agent type | | |
|---|---|---|---|---|
| **Sharing decision** | | **NCE** | **CE** | ***Diff.*** |
| *No control* | Average | 30 | 41.67 | |
| | Median | 30 | 45 | -11.67 |
| | Observations | 2 | 6 | |
| *Min 10%* | Average | 20 | 25.83 | |
| | Median | 20 | 22.50 | -5.83 |
| | Observations | 3 | 6 | |
| *Min 20%* | Average | 24.63 | 33.67 | |
| | Median | 20 | 30 | -9.03*** |
| | Observations | 30 | 18 | |
| *Min 40%* | Average | 41.11 | 42.82 | |
| | Median | 40 | 40 | -1.70* |
| | Observations | 44 | 33 | |
| *Pooled* | Average | 33.77 | 38.48 | |
| | Median | 40 | 40 | -4.71*** |
| | Observations | 79 | 63 | |
| **Belief** | | **NCE** | **CE** | ***Diff.*** |
| *No control* | Average | 14.68 | 24.48 | -9.80** |
| | Median | 10 | 20 | |
| *Min 10%* | Average | 13.20 | 15.30 | -2.10 |
| | Median | 10 | 10 | |
| *Min 20%* | Average | 21.77 | 23.00 | -1.23 |
| | Median | 20 | 20 | |
| *Min 40%* | Average | 50.34 | 37.22 | 13.12*** |
| | Median | 50 | 30 | |
| Observations: 142 agents | | | | |

*Note:* This table reports summary statistics of the agents' transfer decisions and beliefs in the real-effort game by agent type. Notice that the agent makes his sharing decision *before* he starts to solve the real-effort task. Effort levels and beliefs are compared using a Mann-Whitney test. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. See Table A.3 for further details.

**Table A.6:** Real-Effort Game: Summary Statistics of Control Choices and Second-Order Beliefs by Principal Type

| | | Principal type | | |
|---|---|---|---|---|
| **Control decision** | | **NCE** | **CE** | ***Total*** |
| *No control* | | 2 | 6 | 8 |
| *Min 10%* | | 3 | 6 | 9 |
| *Min 20%* | | 30 | 18 | 48 |
| *Min 40%* | | 44 | 33 | 77 |
| ***Total*** | | 79 | 63 | 142 |
| *Fisher's exact test*, $p$-value: 0.126 | | | | |
| **S.O. Belief** | | **NCE** | **CE** | ***Diff.*** |
| *No control* | Average | 5.67 | 17.90 | -12.23*** |
| | Median | 5 | 10 | |
| *Min 10%* | Average | 15.42 | 17.75 | -2.33 |
| | Median | 10 | 15 | |
| *Min 20%* | Average | 27.20 | 24.14 | 3.06 |
| | Median | 25 | 25 | |
| *Min 40%* | Average | 51.71 | 40.21 | 11.50*** |
| | Median | 50 | 40 | |
| Observations: 142 principals | | | | |

*Note:* This table reports the summary statistics of the principals' decisions and beliefs in the real-effort game by treatment. See Table A.4 for further details. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Robustness: Negative Binomial Estimates

**Table A.7:** Effort-Choice Game: Hidden Costs of Control – Negative Binomial Regressions

| Dependent variable: Agent effort | All agents (1) | NCE only (2) | CE only (3) |
|---|---|---|---|
| No control (= Baseline) | 1.574*** | 1.539*** | 1.677*** |
| | (0.220) | (0.248) | (0.404) |
| Min 6 | -0.188*** | -0.149* | -0.238** |
| | (0.065) | (0.082) | (0.103) |
| Min 21 | 0.124* | 0.243** | -0.021 |
| | (0.070) | (0.095) | (0.097) |
| Observations | 426 | 237 | 189 |
| Groups | 142 | 79 | 63 |
| $F$ | 20.50 | 15.96 | 5.55 |
| Prob > $F$ | 0.000 | 0.000 | 0.006 |

*Note:* The table shows results based on fixed effects negative binomial estimations of control on agent effort. See Table 4 for details on the sample and construction of variables. Standard errors are computed via the Jackknife replication method, where the number of replications equals the the number of clusters or groups. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.8:** Real-Effort Game: Hidden Costs of Control – Negative Binomial Regressions

| Dependent variable: Agent sharing | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| CE agent | 0.661*** | 0.525*** | 0.650*** | 0.462** |
| | (0.110) | (0.173) | (0.129) | (0.180) |
| Min 10 | 0.300*** | | -0.317*** | |
| | (0.102) | | (0.118) | |
| Min 20 | .204** | | 0.198* | |
| | (0.100) | | (0.103) | |
| Min 40 | 0.632*** | | 0.610*** | |
| | (0.089) | | (0.093) | |
| CE agent × Min 10 | -0.257 | | -0.268 | |
| | (0.225) | | (0.217) | |
| CE agent × Min 20 | -0.459*** | | -0.427*** | |
| | (0.124) | | (0.132) | |
| CE agent × Min 40 | -0.620*** | | -0.623*** | |
| | (0.119) | | (0.134) | |
| High control | | 0.678*** | | 0.650*** |
| | | (0.093) | | (0.098) |
| CE agent × High control | | -0.433** | | -0.382** |
| | | (0.168) | | (0.167) |
| Modal control beliefs | No | No | Yes | Yes |
| Individual heterogeneity | Yes | Yes | Yes | Yes |
| Further controls | Yes | Yes | Yes | Yes |
| Session dummies | Yes | Yes | Yes | Yes |
| Observations | 126 | 126 | 117 | 117 |
| Wald *chi2* | 59451.66 | 30168.52 | 54426.86 | 29447.86 |
| Prob > *chi2* | 0.000 | 0.000 | 0.000 | 0.000 |

*Note:* The table provides estimates based on regressions analogous to those underlying Table 4, but using a negative binomial model instead of OLS. See Table 4 for details on the sample and construction of variables. The Wald test statistics are obtained from regressions without robust standard errors. All other results presented in the table, however, are based on estimations with robust standard errors. * $z < 0.10$, ** $z < 0.05$, *** $z < 0.01$.

**Table A.9:** Real-Effort Game: The Impact of Sensation on Voluntary Sharing – Negative Binomial Regressions

| Dependent variable: Agent voluntary sharing | (1) | (2) | (3) |
|---|---|---|---|
| Sensation | 0.018 | 0.031** | |
| | (0.012) | (0.014) | |
| CE agent | | 1.546*** | 3.039*** |
| | | (0.569) | (0.675) |
| CE agent × sensation | | -0.012 | |
| | | (0.022) | |
| Pos. sensation | | | 0.104*** |
| | | | (0.023) |
| CE agent × pos. sensation | | | -0.118*** |
| | | | (0.041) |
| Neg. sensation | | | -0.042* |
| | | | (0.025) |
| CE agent × neg. sensation | | | 0.082** |
| | | | (0.037) |
| Individual heterogeneity | Yes | Yes | Yes |
| Further controls | Yes | Yes | Yes |
| Session dummies | Yes | Yes | Yes |
| Observations | 117 | 117 | 117 |
| Wald *chi2* | 1198.31 | 1442.13 | 998.62 |
| Prob > *chi2* | 0.000 | 0.000 | 0.000 |

*Note:* The results in this table are based on regressions analogous to those underlying Table 5, while we use a negative binomial model instead of OLS. See Table 5 for details on the sample and construction of variables. Notice that the model in Column 3 did not converge when including the full set of controls. Thus, amongst the control variables capturing the subjects' educational background, we only keep whether or not a participant is an economist. Robust standard errors are shown in parentheses. * $z < 0.10$, ** $z < 0.05$, *** $z < 0.01$.

# B Experimental Instructions

## B.1 Written (English Translation)

Welcome to this experiment and thank you for your participation!

In this experiment – financed by the German Research Foundation (DFG) – you can earn money, depending on your own performance and decisions as well as the decisions of other participants. Therefore, it is important that you read these instructions carefully.

If you have questions at any time, please press the ‘pause’ key on your keyboard. We will come to you and answer your question. Please pose your question quietly. All participants in the experiment receive the same printed instructions. You will obtain more information on your screen as the experiment progresses. Please read this thoroughly and carefully. The information on the screen is only intended for the respective participant. Please do not look at the screens of other participants and do not talk to each other. If you offend against these rules, we are unfortunately required to expel you from the experiment. Please switch off your mobiles now.

**General Schedule** This experiment takes about 45 minutes. The experiment comprises three stages and a final questionnaire. You will receive detailed instructions for every stage during the experiment on your screen. Please read these instructions carefully.

**Questionnaire** A short questionnaire will follow after the experiment. Having filled out this questionnaire, please remain seated until we call you separately for payment.

**Further Schedule** After you have read the instructions carefully, please wait for the other participants and then start with the computer program on your screen. After the completion of the questionnaire after the last part of the experiment you will be called individually to receive your payment. Please note that for this experiment the use of electronic devices (e.g. pocket calculators) is not allowed. Please turn off your cell phones now.

**Good luck!**

## B.2 On-Screen (English Translation)

**Intro** [*The headlines (in bold) are not shown to the participants*]

Welcome to the experiment!

Thank you for your willingness to participate in this experiment. Please read all descriptions during the experiment carefully.

This experiment consists of three stages. At the end of the experiment, one of these stages is chosen randomly to determine your payment. Payment is made after the experiment. All payouts are rounded to the nearest 10 cents.

During the experiment, all calculations are shown in ECU. One ECU corresponds to 0.10.

For showing-up to this experiment in time, you will, in addition to your payoffs earned during the experiment, receive 2.50.

All information you provide in this experiment will be treated confidentially.

**Pre-Game: Group Formation Intro**

Part One

You will be asked to make a decision with one of of the other participants in this experiment – your partner. Below we will explain the decision situation.

Both you and your partner have an endowment of 50 ECUs for this task. You can distribute this money to a private account, to a group account, or to both accounts.

Your total payoff depends on your choice how to distribute the money and on the choice of your partner. You will receive the sum of the private account and twice the minimum deposit in the group account. The following formula illustrates your payment:

Total payment = money on the private account + 2 times the minimum of your contribution to the group account and your partner's contribution to that account

Before you will have to make your decision you now have the opportunity to talk with your partner about this game and your strategies how to play the game.

**Pre-Game: Group Formation Chat** Now, please talk to your partner about the game! Also discuss how you will play the game best! After the time has expired, you will be passed on automatically.

**Pre-Game: Group Formation Game** Please divide the 50 ECUs between the group account and the private account.

Group account

Private account

[PARTICIPANT MAKES DECISION]

**Pre-Game: Group Formation Feedback** Results

Group account

Your partner's contribution

Your contribution
Private Account
Your contribution
Your payoff
Please tell your partner now how you assess his decision
Very unfair (1) (2) (3) (4) (5) Very fair
[PARTICIPANT MAKES DECISION]

**Pre-Game: Group Formation Feedback Back** Your partner regards you on a scale of 1-5 (where 1 is very unfair and 5 is very fair) as: [Chosen number of partner]

In the course of the experiment you will again play together with your partner. To recognize your partner, all decisions of your partner will be marked in blue!

**Pre-Game: No Group Formation Intro**

Part One
In this stage, you are asked to solve as many tasks as possible in 2:15 minutes. The task is to bring the slider shown on the screen in the middle position (to the value 50). Once you have positioned the slider for the first time, you will see the position (shown as a number). You can move the slider as often as you want. The number of sliders you already positioned correctly will appear at the top of the screen.

You get 80 ECUs for this task, no matter how many sliders you move in the correct position.

**Pre-Game: No Group Formation Game** [PARTICIPANT PERFORMS SLIDER TASK]

[When finished] Please wait until everyone is finished with the task.

**Effort-Choice Game: Intro**

**Principal [NCE/CE]** [*below, the first part in the square brackets is only shown to NCE participants, the second part is only shown to CE participants*]

Part Two
Players: You play this stage together with [another player/your partner form the previous stage].

Endowment: You do not have any own endowment in this stage. Your [fellow player/partner] has an endowment of 117 ECUs. Your [fellow player/partner] has the opportunity to share with you something from his endowment of 117 ECUs. You have the opportunity to require a minimum contribution from your [fellow player/partner]. In his sharing decision, your [fellow player/partner] is not allowed to fall short of this minimum. However, your [fellow player/partner] is free to give you more than the required minimum contribution.

The possible minimum contributions are:

No minimum contribution / 6 / 21

Own payoff: Your [fellow player's/partner's] transfer, which is doubled by the experimenter. You get paid that amount with a probability of 90%. With a probability of 10% you will receive the minimum contribution that you required (again, doubled by the experimenter); that is, you cannot know whether your [fellow player/partner] provided only the minimum requirement or more.

Payoff of your [fellow player/partner]: Your [fellow player/partner] gets the difference between his endowment and the amount transferred to you.

**Agent [NCE/CE]**

Part Two

Players: You play this stage together with [another player/your partner form the previous stage].

Endowment: In the following stage of the experiment, you have an endowment of 117 ECUs. Your [fellow player/partner] does not have an own endowment. You have the opportunity to transfer to your [fellow player/partner] some part of your endowment. Your [fellow player/partner] may request a minimum contribution from you. If he decides to do so, you are not allowed to fall short of this minimum requirement in your decision how much to transfer to him.

The possible minimum contributions you may face are:

No minimum contribution / 6 / 21

Own payoff: You will get paid the difference between your endowment and the amount you decide to transfer to your [fellow player/partner].

Payoff of your [fellow player/partner]: The amount that you have decided to transfer, which is doubled by the experimenter. Your [fellow player/partner] gets this amount with a probability of 90%. With a probability of 10% he receives the minimum contribution that he required for himself (again, doubled by the experimenter).

**Effort-Choice Game: Decision Principal** Below you can decide how much your [fellow player/partner] at least has to a transfer to you.

Please indicate here how high the minimum requirement should be.

- No minimum
- At least 6 (so you get at least 12)
- At least 21 (so you get at least 42)

[PRINCIPAL MAKES DECISION]

**Effort-Choice Game: Expectations Agent**

**Expectations** Please indicate now for how likely you consider the following minimum requirements of your [fellow player/partner]. Please provide all information in percent (that is, your numbers must add up to 100).

How likely do you think it is that your [fellow player/partner]

- Leaves the decision to you
- At least requires 6 from you
- At least requires 21 from you

[AGENT MAKES DECISION]

**Effort-Choice Game: Second-Order Expectations Principal**

**Expectations** Please think about with which probability your [fellow player/partner] expects you to impose each possible minimum contribution. Please provide all information in percent (that is, your numbers must add up to 100).

How likely do you think it is that your [fellow player/partner] thinks that ...

- You leave the decision to him.
- You require at least 6
- You require at least 21

[PRINCIPAL MAKES DECISION]

**Effort-Choice Game: Decision Agent (strategy method)** The table shown below lists all possible minimum requirements your [fellow player/partner] can impose. Your [fellow player/partner] has already made his decision, which you do not know as of yet. Please decide for all possible minimum requirement of your [fellow player/partner] how much you are willing to transfer to him. Your final payment is determined by the transfer decision you have made for your [fellow player's/partner's] actual minimum restriction. Please note that all possible transfers must not exceed 117 ECUs. If you have questions, please press the *Pause* button.

| Your [fellow player/partner] ... | Your transfer |
|---|---|
| leaves the decision to you | |
| requires you to contribute at least 6. | |
| requires you to contribute at least 21 | |

[AGENT MAKES DECISION]

**Real-Effort Game: Intro**

**Principal [NCE/CE]**

Part Three

Task: In this stage, your [fellow player/partner from the previous stages] is required to solve tasks within 5 minutes. The task is to add 5 two-digit numbers.

Your [fellow player/partner] receives 15 ECUs per correctly solved equation. You have the opportunity to require a minimum percentage of his final income from your [fellow player/partner], which he is not allowed to fall short of. You do not have the possibility to solve these tasks. Thus, the only payoff that you receive in this stage is the money transferred to you.

You can now indicate how much you require from your [fellow player/partner]. Your [fellow player/partner] then has the opportunity to determine what percentage of his income he will actually transfer you. However, he has to transfer to you at least the minimum you specified.

Please indicate what percentage of your [fellow player's/partner's] income he has to cede to you:

Nothing \ 10% of his income \ 20% of his income \ 40% of his income

[PRINCIPAL MAKES DECISION]

**Agent [NCE/CE]**

Part Three

Task: In this stage you are required to add two-digit numbers. You have a total of 5 minutes to do so.

You earn 15 ECUs per correctly solved equation.

Players: You play the game again with a [fellow player/your partner from the previous stages]. This player does not have the opportunity to earn anything by solving equations. You can give your [fellow player/partner] a portion of your income. Please note that your [fellow player/partner] can request a minimum percentage of your income, which you are not allowed to fall short of.

The possible minimum requirements are:

Nothing \ 10% of his income \ 20% of his income \ 40% of his income

Before you begin the task and make your decision on how much of your income to transfer, you will get informed about the amount you at least have to transfer.

## Real-Effort Game: Expectations Agent

**Expectations** Please indicate now for how likely you consider the following minimum requirements of your [fellow player/partner]. Please provide all information in percent (that is, your numbers must add up to 100).

How likely do you think it is that your [fellow player/partner]

- Leaves the decision to you
- Requires at least 10% from you
- Requires at least 20% from you
- Requires at least 40% from you

[AGENT MAKES DECISION]

## Real-Effort Game: Second-Order Expectations Principal

**Expectations** Please think about with which probability your [fellow player/partner] expects you to impose each possible minimum contribution. Please provide all information in percent (that is, your numbers must add up to 100).

How likely do you think it is that your [fellow player/partner]thinks that ...

- You leave the decision to him.
- You require at least 10% from him
- You require at least 20% from him
- You require at least 40% from him

[PRINCIPAL MAKES DECISION]

**Real Effort-sharing agent** Your [fellow player/partner] forces you to transfer to him at least <x>% of your income.

Please indicate what percentage of your income from this stage you want to transfer to your [fellow player/partner].

[AGENT MAKES DECISION]

**Real-Effort Game: Perform**

[AGENT ADDS NUMBERS]

**Real-Effort Game: End** You have solved <x> equations correctly.

**Experiment: End** The experiment is now complete. We will now show you which of the three stages of the experiment is chosen to determine your payoff.

# PREVIOUS DISCUSSION PAPERS

66 Riener, Gerhard and Wiederhold, Simon, Team Building and Hidden Costs of Control, September 2012.

65 Fonseca, Miguel A. and Normann, Hans-Theo, Explicit vs. Tacit Collusion – The Impact of Communication in Oligopoly Experiments, August 2012.
Forthcoming in: European Economic Review.

64 Jovanovic, Dragan and Wey, Christian, An Equilibrium Analysis of Efficiency Gains from Mergers, July 2012.

63 Dewenter, Ralf, Jaschinski, Thomas and Kuchinke, Björn A., Hospital Market Concentration and Discrimination of Patients, July 2012.

62 Von Schlippenbach, Vanessa and Teichmann, Isabel, The Strategic Use of Private Quality Standards in Food Supply Chains, May 2012.
Forthcoming in: American Journal of Agricultural Economics.

61 Sapi, Geza, Bargaining, Vertical Mergers and Entry, July 2012.

60 Jentzsch, Nicola, Sapi, Geza and Suleymanova, Irina, Targeted Pricing and Customer Data Sharing Among Rivals, July 2012.

59 Lambarraa, Fatima and Riener, Gerhard, On the Norms of Charitable Giving in Islam: A Field Experiment, June 2012.

58 Duso, Tomaso, Gugler, Klaus and Szücs, Florian, An Empirical Assessment of the 2004 EU Merger Policy Reform, June 2012.

57 Dewenter, Ralf and Heimeshoff, Ulrich, More Ads, More Revs? Is there a Media Bias in the Likelihood to be Reviewed?, June 2012.

56 Böckers, Veit, Heimeshoff, Ulrich and Müller Andrea, Pull-Forward Effects in the German Car Scrappage Scheme: A Time Series Approach, June 2012.

55 Kellner, Christian and Riener, Gerhard, The Effect of Ambiguity Aversion on Reward Scheme Choice, June 2012.

54 De Silva, Dakshina G., Kosmopoulou, Georgia, Pagel, Beatrice and Peeters, Ronald, The Impact of Timing on Bidding Behavior in Procurement Auctions of Contracts with Private Costs, June 2012.
Forthcoming in Review of Industrial Organization.

53 Benndorf, Volker and Rau, Holger A., Competition in the Workplace: An Experimental Investigation, May 2012.

52 Haucap, Justus and Klein, Gordon J., How Regulation Affects Network and Service Quality in Related Markets, May 2012.
Published in: Economics Letters 117 (2012), pp. 521-524.

51 Dewenter, Ralf and Heimeshoff, Ulrich, Less Pain at the Pump? The Effects of Regulatory Interventions in Retail Gasoline Markets, May 2012.

50 Böckers, Veit and Heimeshoff, Ulrich, The Extent of European Power Markets, April 2012.

49 Barth, Anne-Kathrin and Heimeshoff, Ulrich, How Large is the Magnitude of Fixed-Mobile Call Substitution? - Empirical Evidence from 16 European Countries, April 2012.

48 Herr, Annika and Suppliet, Moritz, Pharmaceutical Prices under Regulation: Tiered Co-payments and Reference Pricing in Germany, April 2012.

47 Haucap, Justus and Müller, Hans Christian, The Effects of Gasoline Price Regulations: Experimental Evidence, April 2012.

46 Stühmeier, Torben, Roaming and Investments in the Mobile Internet Market, March 2012.
Forthcoming in: Telecommunications Policy.

45 Graf, Julia, The Effects of Rebate Contracts on the Health Care System, March 2012.

44 Pagel, Beatrice and Wey, Christian, Unionization Structures in International Oligopoly, February 2012.

43 Gu, Yiquan and Wenzel, Tobias, Price-Dependent Demand in Spatial Models, January 2012.
Published in: B. E. Journal of Economic Analysis & Policy,12 (2012), Article 6.

42 Barth, Anne-Kathrin and Heimeshoff, Ulrich, Does the Growth of Mobile Markets Cause the Demise of Fixed Networks? – Evidence from the European Union, January 2012.

41 Stühmeier, Torben and Wenzel, Tobias, Regulating Advertising in the Presence of Public Service Broadcasting, January 2012.
Published in: Review of Network Economics, 11, 2 (2012), Article 1.

40 Müller, Hans Christian, Forecast Errors in Undisclosed Management Sales Forecasts: The Disappearance of the Overoptimism Bias, December 2011.

39 Gu, Yiquan and Wenzel, Tobias, Transparency, Entry, and Productivity, November 2011.
Published in: Economics Letters, 115 (2012), pp. 7-10.

38 Christin, Clémence, Entry Deterrence Through Cooperative R&D Over-Investment, November 2011.
Forthcoming in: Louvain Economic Review.

37 Haucap, Justus, Herr, Annika and Frank, Björn, In Vino Veritas: Theory and Evidence on Social Drinking, November 2011.

36 Barth, Anne-Kathrin and Graf, Julia, Irrationality Rings! – Experimental Evidence on Mobile Tariff Choices, November 2011.

35 Jeitschko, Thomas D. and Normann, Hans-Theo, Signaling in Deterministic and Stochastic Settings, November 2011.
Forthcoming in: Journal of Economic Behavior and Organization.

34 Christin, Cémence, Nicolai, Jean-Philippe and Pouyet, Jerome, The Role of Abatement Technologies for Allocating Free Allowances, October 2011.

33 Keser, Claudia, Suleymanova, Irina and Wey, Christian, Technology Adoption in Markets with Network Effects: Theory and Experimental Evidence, October 2011.
Forthcoming in: Information Economics and Policy.

32 Catik, A. Nazif and Karaçuka, Mehmet, The Bank Lending Channel in Turkey: Has it Changed after the Low Inflation Regime?, September 2011.
Published in: Applied Economics Letters, 19 (2012), pp. 1237-1242.

31 Hauck, Achim, Neyer, Ulrike and Vieten, Thomas, Reestablishing Stability and Avoiding a Credit Crunch: Comparing Different Bad Bank Schemes, August 2011.

30 Suleymanova, Irina and Wey, Christian, Bertrand Competition in Markets with Network Effects and Switching Costs, August 2011.
Published in: B. E. Journal of Economic Analysis & Policy, 11 (2011), Article 56.

29 Stühmeier, Torben, Access Regulation with Asymmetric Termination Costs, July 2011.
Forthcoming in: Journal of Regulatory Economics.

28 Dewenter, Ralf, Haucap, Justus and Wenzel, Tobias, On File Sharing with Indirect Network Effects Between Concert Ticket Sales and Music Recordings, July 2011.
Forthcoming in: Journal of Media Economics.

27 Von Schlippenbach, Vanessa and Wey, Christian, One-Stop Shopping Behavior, Buyer Power, and Upstream Merger Incentives, June 2011.

26 Balsmeier, Benjamin, Buchwald, Achim and Peters, Heiko, Outside Board Memberships of CEOs: Expertise or Entrenchment?, June 2011.

25 Clougherty, Joseph A. and Duso, Tomaso, Using Rival Effects to Identify Synergies and Improve Merger Typologies, June 2011.
Published in: Strategic Organization, 9 (2011), pp. 310-335.

24 Heinz, Matthias, Juranek, Steffen and Rau, Holger A., Do Women Behave More Reciprocally than Men? Gender Differences in Real Effort Dictator Games, June 2011.
Published in: Journal of Economic Behavior and Organization, 83 (2012), pp. 105-110.

23 Sapi, Geza and Suleymanova, Irina, Technology Licensing by Advertising Supported Media Platforms: An Application to Internet Search Engines, June 2011.
Published in: B. E. Journal of Economic Analysis & Policy, 11 (2011), Article 37.

22 Buccirossi, Paolo, Ciari, Lorenzo, Duso, Tomaso, Spagnolo Giancarlo and Vitale, Cristiana, Competition Policy and Productivity Growth: An Empirical Assessment, May 2011.
Forthcoming in: The Review of Economics and Statistics.

21 Karaçuka, Mehmet and Catik, A. Nazif, A Spatial Approach to Measure Productivity Spillovers of Foreign Affiliated Firms in Turkish Manufacturing Industries, May 2011.
Published in: The Journal of Developing Areas, 46 (2012), pp. 65-83.

20 Catik, A. Nazif and Karaçuka, Mehmet, A Comparative Analysis of Alternative Univariate Time Series Models in Forecasting Turkish Inflation, May 2011.
Published in: Journal of Business Economics and Management, 13 (2012), pp. 275-293.

19 Normann, Hans-Theo and Wallace, Brian, The Impact of the Termination Rule on Cooperation in a Prisoner's Dilemma Experiment, May 2011.
Published in: International Journal of Game Theory, 41 (2012), pp. 707-718.

18 Baake, Pio and von Schlippenbach, Vanessa, Distortions in Vertical Relations, April 2011.
Published in: Journal of Economics, 103 (2011), pp. 149-169.

17 Haucap, Justus and Schwalbe, Ulrich, Economic Principles of State Aid Control, April 2011.
Forthcoming in: F. Montag & F. J. Säcker (eds.), European State Aid Law: Article by Article Commentary, Beck: München 2012.

16 Haucap, Justus and Heimeshoff, Ulrich, Consumer Behavior towards On-net/Off-net Price Differentiation, January 2011.
Published in: Telecommunication Policy, 35 (2011), pp. 325-332.

15 Duso, Tomaso, Gugler, Klaus and Yurtoglu, Burcin B., How Effective is European Merger Control? January 2011.
Published in: European Economic Review, 55 (2011), pp. 980-1006.

14 Haigner, Stefan D., Jenewein, Stefan, Müller, Hans Christian and Wakolbinger, Florian, The First shall be Last: Serial Position Effects in the Case Contestants evaluate Each Other, December 2010.
Published in: Economics Bulletin, 30 (2010), pp. 3170-3176.

13 Suleymanova, Irina and Wey, Christian, On the Role of Consumer Expectations in Markets with Network Effects, November 2010.
Published in: Journal of Economics, 105 (2012), pp. 101-127.

12 Haucap, Justus, Heimeshoff, Ulrich and Karaçuka, Mehmet, Competition in the Turkish Mobile Telecommunications Market: Price Elasticities and Network Substitution, November 2010.
Published in: Telecommunications Policy, 35 (2011), pp. 202-210.

11 Dewenter, Ralf, Haucap, Justus and Wenzel, Tobias, Semi-Collusion in Media Markets, November 2010.
Published in: International Review of Law and Economics, 31 (2011), pp. 92-98.

10 Dewenter, Ralf and Kruse, Jörn, Calling Party Pays or Receiving Party Pays? The Diffusion of Mobile Telephony with Endogenous Regulation, October 2010.
Published in: Information Economics and Policy, 23 (2011), pp. 107-117.

09 Hauck, Achim and Neyer, Ulrike, The Euro Area Interbank Market and the Liquidity Management of the Eurosystem in the Financial Crisis, September 2010.

08 Haucap, Justus, Heimeshoff, Ulrich and Luis Manuel Schultz, Legal and Illegal Cartels in Germany between 1958 and 2004, September 2010.
Published in: H. J. Ramser & M. Stadler (eds.), Marktmacht. Wirtschaftswissenschaftliches Seminar Ottobeuren, Volume 39, Mohr Siebeck: Tübingen 2010, pp. 71-94.

07 Herr, Annika, Quality and Welfare in a Mixed Duopoly with Regulated Prices: The Case of a Public and a Private Hospital, September 2010.
Published in: German Economic Review, 12 (2011), pp. 422-437.

06 Blanco, Mariana, Engelmann, Dirk and Normann, Hans-Theo, A Within-Subject Analysis of Other-Regarding Preferences, September 2010.
Published in: Games and Economic Behavior, 72 (2011), pp. 321-338.

05 Normann, Hans-Theo, Vertical Mergers, Foreclosure and Raising Rivals' Costs – Experimental Evidence, September 2010.
Published in: The Journal of Industrial Economics, 59 (2011), pp. 506-527.

04 Gu, Yiquan and Wenzel, Tobias, Transparency, Price-Dependent Demand and Product Variety, September 2010.
Published in: Economics Letters, 110 (2011), pp. 216-219.

03 Wenzel, Tobias, Deregulation of Shopping Hours: The Impact on Independent Retailers and Chain Stores, September 2010.
Published in: Scandinavian Journal of Economics, 113 (2011), pp. 145-166.

02 Stühmeier, Torben and Wenzel, Tobias, Getting Beer During Commercials: Adverse Effects of Ad-Avoidance, September 2010.
Published in: Information Economics and Policy, 23 (2011), pp. 98-106.

01 Inderst, Roman and Wey, Christian, Countervailing Power and Dynamic Efficiency, September 2010.
Published in: Journal of the European Economic Association, 9 (2011), pp. 702-720.

**Heinrich-Heine-University of Düsseldorf**

**Düsseldorf Institute for
Competition Economics (DICE)**

Universitätsstraße 1 _ 40225 Düsseldorf
www.dice.hhu.de

ISSN 2190-9938 (online)
ISBN 978-3-86304-065-9