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Abstract

This paper explores the role of social groups in explaining the reaction to control. We propose a simple model with a principal using control devices and a controlled agent, which incorporates the existence of social groups. Testing experimentally the conjectures derived from the model and related literature, we find that agents in social groups (i) perform more than other (no-group) agents; (ii) expect less control than no-group agents; (iii) decrease their performance substantially when actual control exceeds their expectation, while no-group agents do not react; (iv) do not reciprocate when facing less control than expected, while no-group agents do.

JEL Classification: C92, M54, D03.
Keywords: Identity, employee motivation, principal-agent theory, experiments.

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1 Introduction

Control is commonly used in employment relationships characterized by incomplete contracts to eliminate workers' most opportunistic actions. Standard principal-agent theory suggests that imposing tight control on workers will increase their performance, as workers are merely self-interested and shirking is frequent (Frey, 1993). This prediction has been challenged by a number of empirical studies, which typically evidence that control can reduce worker effort by eroding intrinsic motivation (Barkema, 1995; Enzle and Anderson, 1993; Falk and Kosfeld, 2006; Herzberg, 1986; Schnedler and Vadovic, 2007).[1] However, we argue that these hidden costs of control, but also the benefits of control, depend on the nature of the relationship between the principal and the agent. 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).

This paper investigates how belonging to a social group affects the behavioral reaction to control. We contribute to the previous literature by proposing an analytical framework that incorporates social identity (in the sense of Akerlof and Kranton, 2000, 2005) into a simple principal-agent model.[2] In the model, 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] We distinguish between two general types of principal-agent relationships to account for social identity in the model: in-group and no-group (Tajfel, 1970). Individuals in in-group relationships identify at least partly with each other and behave in accordance with the social identity based on that group membership. No-group individuals are strangers to each other.[4] The theoretical discussion delivers three main insights: First, social identity 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 effort exerted by the agent is primarily determined by the control *sensation,* which we define as

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

[2] The literature on social psychology suggests that the emergence of social identity is facilitated by interpersonal interactions, although they are not necessary for identification to occur (Ashforth and Mael, 1989; Tajfel, 1978). As will be elaborated below, group formation in our paper is *quasi-minimal*, meaning that group membership is induced by an interaction between individuals instead of being the result of simply assigning an arbitrary label. In the remainder of the paper, we will use the notions of social identity, group identity, and social group interchangeably.

[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] For the ease of illustration, we abstract from formally modeling the intensity of in-group members' identification with each other and instead apply a categorical classification. In the experiment, however, we are to some extent able to control for the degree of group identification through a question after the group formation stage.

the deviation of the agent's subjective control belief from the level of control imposed by the principal. Third, the agent's behavioral reaction to sensation is a function of social identity.

We conduct a labor market experiment to test these behavioral conjectures.[5] The experiment proceeds in three stages. In the first stage, group formation takes place. Focusing on social identity based on a shared experience, we create a trust-based relationship between subjects in the in-group treatment through a weakest-link game with computer-mediated pre-play communication. Subjects in the no-group treatment complete a task in isolation. In the second stage, we elicit the agent's effort for each of the three control levels the principal can choose between using the strategy method (Selten, 1967). In the third stage, agents are remunerated for performing a real-effort task. Before the game begins, they must decide how much of their future remuneration they are willing to share with the principal. Agents receive information on the minimum share required by the principal before making their sharing decision and completing the task. Thus, the real-effort game allows us to study the influence of an experienced control sensation on the behavior of the subjects.

We find that social identity has belief- and performance-related consequences. In-group agents expect lower levels of control than no-group agents and are, for each control level, more willing to exert effort beyond the required minimum (voluntary performance). Moreover, we observe that both types of agents increase their voluntary performance in the level of sensation. However, there are important intergroup differences when taking into account the direction of the sensation. For negative sensations, where the level of control the agent faces exceeds his control belief, in-group agents decrease voluntary sharing substantially. No-group agents, however, do not react to negative sensations. Having experienced a positive sensation, where the experienced level of control falls short of the agent's control belief, in-group agents act less favorably than their no-group counterparts. Thus, in the polar cases of maximum and minimum sensation, the intergroup differences in the levels of effort chosen nearly vanish.

Our results imply that the stimulating impact of social identity on the agent's performance depends on the latter's appraisal of the principal's control decision. If the agent feels that the principal does not act in accordance with the social identity based on their group membership, he appears to be inclined to punish this behavior. In extreme cases, this can completely outweigh the benefits of social identity. When we extrapolate our findings to situations in which the principal can choose even higher levels of control, identity salience can easily backfire on the principal, reducing an in-group agent's willingness to perform in the principal's interest below that of a no-group agent. To the best of our knowledge, this is the first work that investigates the role of subjective control beliefs in determining the agents' reactions to control. In particular, we show that social identity influences both the formation of beliefs over control and the response

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

to deviations of expected from actually experienced control.

Our research is inspired by an empirical study carried out by Barkema (1995). He was the first to document a correlation between social distance and the reaction to monitoring. Using survey data of 116 executives of Dutch firms, he finds that higher monitoring is associated with fewer working hours if managers are supervised by an in-house CEO. But if monitoring is implemented impersonally by a parent company, the correlation between monitoring and hours worked is positive. 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 principal-agent relationship. To establish a causal effect, one would have to consider a myriad of aspects, for example, personal characteristics, economic dependency on the job, organizational tenure, recency of membership in the organization, informal organizational structure, and the existence of multiple foci of identification.[6] These factors influence both group identity and performance; hence, endogeneity resulting from omitted variables is a serious concern when these variables are not properly controlled for. Therefore, we took an experimental approach to test our theoretical conjectures in a controlled environment.[7]

Closely related to our work is the experiment conducted by Falk and Kosfeld (2006), who show that control can yield costs that outweigh the benefits when dealing with reciprocal agents, as reciprocity is sensitive to control. However, Falk and Kosfeld (2006) do not account for social ties between the principal and the agent in their analysis, and thus neglect how these 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 primarily if the principal and the agent are socially close. The authors, however, vary social distance by lifting anonymity. With this design, various confounding factors are conceivable, such as feelings of sympathy or antipathy evoked as the result of close, uncontrolled, and direct communication between the subjects (Dufwenberg and

[6] Multiple group memberships are prevalent in real-world organizations (Albert and Whetten, 1985; Ashforth and Mael, 1989; Van Knippenberg and Van Schie, 2000). For instance, an employee's social identity may be derived not only from the firm as a whole but also, for instance, from his department, union, or team, with potentially different (and unobservable) norms, values, goals, and performance standards.

[7] A further virtue of our lab experiment is that we can clearly distinguish between "task performance" and "contextual performance." The former refers to efforts that are part of the "usual" job requirements. The latter relates to one's other efforts in the environment in which task performance takes place, encompassing behaviors such as helping others and taking others' interests into account (Moorman, 1991; Moorman and Blakely, 1995). In our view, contextual performance is a more appropriate indicator of the effects that group identity has on effort because the agent is not required to engage in contextual efforts. Task performance, however, relates to performance on the job the agent was hired to do. Because the agent is likely to benefit from it in the form of bonus payments or promotion opportunities (Van Knippenberg, 2000), task performance is less contingent on the motivations that correspond to group membership. In the experiment, each principal-agent game is designed as a one-shot interaction, and all of the effort conducted on behalf of the principal is foregone by the agent. Consequently, the agent does not have any strategic incentive to voluntarily exert effort.

Muren, 2006; Goette, Huffman and Meier, 2011). In contrast to Dickinson and Villeval (2008), control and effort decisions in our experiment are made anonymously. Subjects in the in-group 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 group membership on the agent's willingness to exert effort for the principal.

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 our results in Section 4. Section 5 concludes by providing the implications of our findings.

2 The Modeling Framework

2.1 Sensation-Dependent Preferences

In this section, we outline a modeling framework that we use to derive hypotheses on the belief- and performance-related consequences of social identity. 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 agents 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:

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

This formulation of the agent's utility captures the relationship between overall satisfaction with a task or job and social identity (Dick et al., 2004). In fact, $U_A(e; m)$ can be regarded as 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 for 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 are independent of social identity, 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 Δ 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 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 were 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.

[8] In the results section, we will define the empirical equivalent of sensation.
[9] Recall that we neglect direct costs of effort.

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 Appendix A 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 abstracting from group identity. Therefore, both conjectures refer to the effort responses to control or sensation within groups. However, the model can easily be extended to incorporate intergroup differences as well. To allow us to formally distinguish between in-group and no-group agents, we introduce the parameter $c \in \{0, 1\}$, where $c = 1$ identifies an in-group relationship and $c = 0$ identifies a no-group relationship. We think of in-group and no-group 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 (e.g., 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

[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 example, 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, 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).

matters for cooperation. Moreover, evidence from the public sector suggests that the social distance between principals and agents 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 in-group agents exceeding the minimum performance level set by the principal is higher than the respective proportion of no-group agents.*

Moreover, the above literature also suggests that in-group agents take on the group's perspective and therefore supply more effort in the principal's interest than their no-group 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 (e.g., Delgado, Frank and Phelps, 2005; Marchetti et al., 2011). This implies that the individually expected level of control depends on social identity; thus, $\hat{m} = \hat{m}(c)$. The discussion of reference point effects in Hart and Moore (2008) points out the direction of between-group 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, in-group agents have lower control beliefs than no-group agents.

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

Finally, the model suggests that effort, e, and sensation, Δ, are positively associated (see Conjecture 2). This result should hold for both types of agents. However, there may be intergroup 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 in-group than for no-group agents. Following Hart and Moore (2008), in-group 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 incongruent 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* $\Delta < 0$

However, 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 as an in-group relationship suddenly becomes a no-group relationship. In our model, supervision or control does not affect the type of the relationship between the principal and the agent. Rather, a negative control sensation, possibly interpreted as a sign of distrust, evokes even harsher negative feelings for the in-group agent. This may be a more realistic view of in-group and no-group relationships, as 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.

3 Experimental Design

In the experiment, subjects were divided into groups of two, and the roles of principal and agent as well as the treatment were randomly assigned. The design was parsimonious, without work environment frames. In the first step we established groups in the in-group treatment. In the no-group treatment, individuals performed a task in isolation. 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 only occurs 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.[11]

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

Group-Formation Phase

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 (ECU, where 1 ECU was worth 0.10 €) to a private or public account. The returns to the group account were the smallest of the two contributions to the public account, doubled by the experimenter. A subject's total payment was the sum of the private and 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.[12] The aim of the this phase was to induce a feeling of belonging to the same group, as a consequence of the shared principal-agent experience.[13] The coordination game had an obvious focal point, to ease the establishment of group feelings.[14] We refer to the principal-agents pairs that played the coordination game as in-group.

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 *quasi-minimal* group induction, stemming only from 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 the agent and the knowledge of the other's behavior.[15] After the game and the disclosure of the results, subjects had to give their partner 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 next stage.

Subjects in the no-group 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

[12] 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. Because dropping these subjects from the sample leaves all results virtually unchanged, we decided to keep them in our preferred sample.

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

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

[15] 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.

bring 48 sliders into the middle position within 3 minutes. Participants in this task received a flat fee of 80 ECU, independent of their performance.[16]

Effort-Choice Phase

The effort-choice game was designed as a modified version of the experiment conducted by 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. We refer to voluntary sharing as the difference between the agent's effort choice and the principal's control.

Although we are not able to test all of the theoretical conjectures developed in Section 2, the effort-choice game still has a *raison d'être*. First and foremost, it allows for comparisons to previous studies on the costs of control, most notably Falk and Kosfeld (2006) and Ploner, Schmelz and Ziegelmeyer (Forthcoming). Second, the strategy vector method enables us to elicit differences in the agents' effort choices for a small (relative to a large) increase in the degree of control. Finally, we want to investigate whether social identity is salient in both cold and hot decision-making situations.

Real-Effort Phase

After the effort-choice game, the participants played a 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 decide what share of his payoff to transfer to the principal.[17] 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: $E_{min} \in$

[16] 80 ECU were the average payoff of in-group principals and agents, respectively, in the pilot sessions.

[17] 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, this is unlikely to affect our results on intergroup differences in voluntary sharing because, as a result of randomization, groups should not systematically differ in their number-adding skills.

$\{0\%, 10\%, 20\%, 40\%\}$. 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 ECU 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 the real-effort setting in our experiment is more informative than the effort-choice game. First, 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. Second, 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. Emotions are likely to play a larger role in decision making when real effort is involved (Charness, Frechette and Kagel, 2004), so we can examine the effect of control sensations on sharing decisions in this context. In general, we expect our setting in the real-effort game (reward for the agent only after successfully completing a task; feedback on the principal's control decision *before* the agent decides on sharing) to be closer to principal-agent relations in real-world organizations.[18]

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. We also asked the principals to state their beliefs regarding the agents' control expectations (second-order beliefs). We did not incentivize these answers because there is evidence that eliciting beliefs with or without incentivization for accuracy does not yield different results (Camerer and Hogarth, 1999; Grether, 1992).[19]

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.

[18] When making inferences on the agent's effort in response to control, we only look at his sharing 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.

[19] Gaechter and Renner (2010) carry out a public-good game and find that incentivized beliefs are more accurate there. However, incentivization has the (undesirable) side effect of increasing the subjects' willingness to cooperate.

Questionnaire

In the questionnaire, besides the standard demographic observations, we elicited subjects' attitudes toward control, employing the questions already used by Falk and Kosfeld (2006) and Ploner, Schmelz and Ziegelmeyer (Forthcoming). In particular, subjects were exposed to different work-place scenarios. For each of these situations, we asked subjects about their work motivation.

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. 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 Effort-Choice Game

Performance We begin by examining the agents' effort decisions by group and control level. Our first result is that we find support for Conjecture 1. For both in-group and no-group agents, we observe zero and positive voluntary sharing at all control levels. Judging by Chi-square tests[20], 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*: $p = 0.336$; *Min 6*: $p = 0.200$; *Min 21*: $p = 0.022$). This is according to Conjecture 3, which predicts that in-group agents are more likely to voluntarily share their endowment with the principal than no-group agents.[21]

Next, we turn to the intergroup differences in voluntary effort, depicted in Figure 1.[22] We

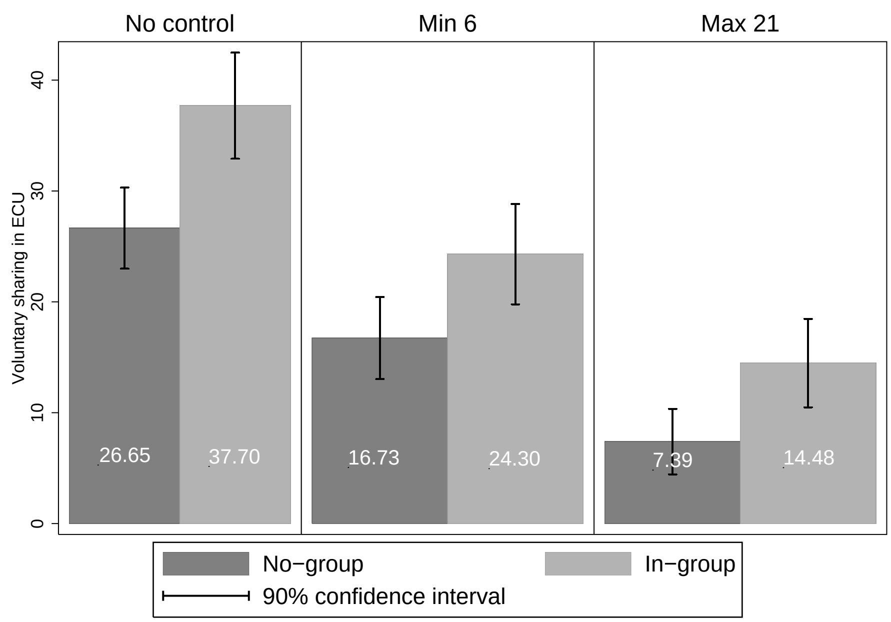
[20] We report the results of two-sided tests throughout the paper.

[21] However, the mere fact that the agents and principals had interacted before the effort-choice game may not have always been sufficient to render group identity salient. Recall that after the coordination game, both the principal and the agent are asked how fair they find the actions of their partner. We define group induction as successful if the agent's rating is either 4 or 5. Excluding the 14 in-group principal-agent pairs for whom group induction can be expected to have not worked properly, we find that Conjecture 3 is supported for both medium and maximum control. The respective Chi-square test statistics read: *No control*: $p = 0.984$; *Min 6*: $p = 0.088$; *Min 21*: $p = 0.003$.

[22] Table 4 in Appendix B contains the average and the median values of both the agent's voluntary performance and his beliefs.

find that voluntary sharing decreases in the degree of control. This holds for both in-group and no-group agents. Consistent with Conjecture 4, however, no-group agents' average voluntary sharing is significantly lower than in-group agents' sharing at all levels of control. This indicates that belonging to a social group entails behavioral consequences. The size and significance level of the treatment difference is largest when the principal trusts the agent completely. Comparing low and high control, the order of magnitude of the intergroup difference in voluntary sharing remains roughly constant. These findings are, in general, in line with Dickinson and Villeval (2008), although they do not split their results by control level.[23]

Figure 1: Effort-choice game: Average voluntary effort by control level and group



Note: This figure shows the average voluntary sharing (defined as an agent's contribution beyond the principal's minimum requirement) for the three different control levels by group. There are significant intergroup differences in voluntary sharing for all control levels (Mann-Whitney test, *No control*: $p = 0.004$; *Min 6*: $p = 0.063$; *Min 21*: $p = 0.018$).

Interestingly, the effort of in-group agents is often independent of the principal's control. The proportion of subjects that share the same amount at all control levels (unconditional sharing) is approximately two and a half times higher in the in-group than in the no-group treatment (26.74 percent vs. 10.12 percent; Chi-square test: $p = 0.006$). Moreover, out of the 23 in-group agents that shared unconditionally, ten chose to transfer at least half of the endowment (sharing

[23] For the subsample of those in-group principal-agent pairs for which group induction worked properly, we always find a more significant difference in voluntary effort between in-group and no-group agents than for the full sample.

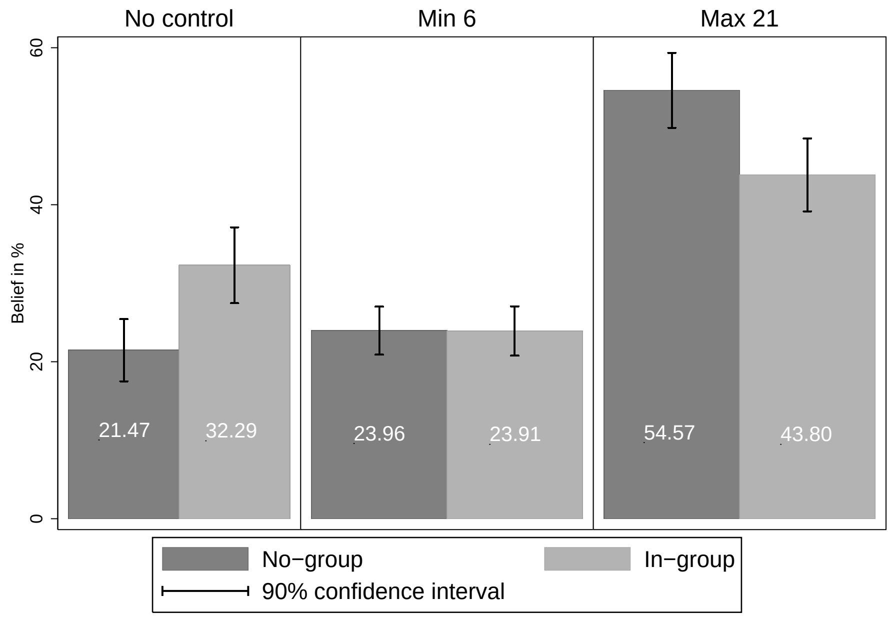
of 58 or more). Only one of the eight unconditionally sharing no-group agents transferred half of his endowment, while all of the others shared less. In addition, a variance ratio test shows that the variance of voluntarily expended effort is higher for agents in the in-group treatment for each level of control, significant at the 1 percent level for both no and maximum control (*No control:* $p = 0.006$; *Min 21*: $p = 0.002$) and significant at 5 percent for the intermediate level of control ($p = 0.030$).

Comparing the cumulative distribution functions of voluntary sharing, we find a significant difference between groups at all control levels. This further strengthens the above results from the Mann-Whitney test. In the no control condition, we can reject the null hypothesis that the distributions of the two agent types are the same at the 1 percent significance level (Kolmogorov-Smirnov test, $p = 0.007$). In the cases of low and high control, we can reject the equality of distributions at a significance level of 10 percent *(Min 6*: $p = 0.096$; *Min 21*: $p = 0.056$). Moreover, we find that voluntary sharing under lower control levels first-order stochastically dominates the sharing increment under higher control levels for both agent types.

Beliefs Figure 2 shows that agents who had previously interacted with the principals believe no control to be significantly more likely and maximum control to be significantly less so than agents without prior experience with the principals.[24] The perceived likelihood of facing low control does not differ between the two treatments. A comparison of the cumulative distribution functions of the agents' control beliefs confirms these findings. For no and maximum control, we can reject the null that the distributions of both agent types are similar at the 5 percent level (Kolmogrov-Smirnov test, *No control*: $p = 0.026$; *Min 21*: $p = 0.031$). For the intermediate control level, we do not observe intergroup differences in the beliefs ($p = 1.00$).

[24] When assessing reactions to beliefs, a serious concern is the presence of the so-called *false consensus effect* (Ross, 1977). We will, therefore, not speculate on the magnitude of the effects but concentrate on the treatment differences, as the false consensus effect should not differ between treatments.

Figure 2: Effort-choice game: Average belief over control level by group



Note: This figure illustrates no-group agents' vis-à-vis in-group agents' perceived likelihood that their principal will choose no control, low control, 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, $p = 0.016$ and $p = 0.010$). Beliefs do not differ by group in the *Min 6* condition ($p = 0.886$).

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 unlikely that subjects base their decisions on the expected values of their beliefs, as this would require cumbersome calculations.[25] According to Conjecture 5, no-group members expect a higher degree of control than their in-group counterparts. We find support for this hypothesis in the data. The average (median) value of modal control beliefs is 10.59 (6) for in-group agents and 15.08 (21) for no-group agents. This difference in modal control beliefs is significant at the 1 percent level (Mann-Whitney test, $p = 0.003$). The equality of the cumulative distribution functions of the agent types' modal control beliefs can be rejected at the 5 percent

[25] 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.

level (Kolmogorov-Smirnov test, $p = 0.016$).

In summary, the results in the effort-choice game suggest that a shared experience in the coordination game has cognitive and behavioral implications. In-group agents perceive the likelihood of being controlled differently than their no-group counterparts and are willing to share more with their principals. Moreover, in about one quarter of all cases the effort of in-group agents was independent of the principal's control, while we observed such unconditional sharing only for every tenth no-group agent.[26]

Principal

Control We find intergroup differences in the principals' control decisions. As can be seen in the upper panel in Table 1, the proportion of principals deciding to control is significantly higher in the no-group than the in-group treatment (Fisher's exact test, $p = 0.060$).[27] In particular, only 5 percent of the no-group principals decide to trust the agent completely, while this percentage is more than three times higher for in-group agents (approximately 16 percent). This difference is significant at the 5 percent level (Fisher's exact test, $p = 0.025$). The occurrence of treatment effects in the principals' implementation of control is even more striking considering that the variance of in-group agents' sharing decisions is significantly higher than for no-group agents. This holds for all control levels, implying that sufficiently risk-averse principals should control in-group agents to a greater degree than no-group agents.

Second-order beliefs The principals' second-order beliefs are consistent with the agents' actual beliefs. As shown in the middle panel in Table 1, in-group principals find it more likely than no-group 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. Further, only two no-group principals think it is most likely that agents believe that they will not face any control, while 16 in-group principals perceived no control as the agents' modal belief (Fisher's exact test, $p = 0.001$). The frequency of principals who think that low or high control are the agents' modal

[26] Similar to Falk and Kosfeld (2006) and in contrast to Ploner, Schmelz and Ziegelmeyer (Forthcoming), we find that under certain conditions the hidden costs of control are substantial enough to undermine the effectiveness of control. Compared to no control, effort is lower when the principal imposes a control level of 6. This is the case for both in-group and no-group agents (Wilcoxon signed rank test, $p = 0.001$). In the case of maximum control, agents' efforts do not significantly differ from the no control case ($p = 0.168$ for in-group and $p = 0.306$ for no-group).

[27] However, in contrast to the our findings for the agents, it does not make a difference in the comparison of in-group and no-group principals' control choices whether group induction in the pre-game phase can be expected to have worked well. The *p-value* of Fisher's exact test of the treatment effects in the principals' control decisions is 0.060 for both the total sample and the subsample of principals with coordination satisfaction greater than 3.

belief does not significantly differ between treatments (*Min 6*: $p = 0.198$; *Min 21*: $p = 0.406$).

Finally, the lower panel in Table 1 compares the principals' second-order beliefs with the agents' control beliefs. Both no-group and in-group principals understate the agents' perceived probability not to face any control. However, judging by Mann-Whitney tests, this "mistake" seems to be more serious for no-group principals (no-group: $p = 0.007$; in-group: $p = 0.063$). No-group principals also fail to correctly anticipate the agents' beliefs regarding medium control ($p = 0.012$). The second-order beliefs of in-group principals, however, are statistically indistinguishable from the agents' actual beliefs ($p = 0.135$). For maximum control, both principal types have correct second-order beliefs (no-group: $p = 0.710$; in-group: $p = 0.167$). In summary, in-group principals are somewhat better able to anticipate the agents' actual beliefs than no-group principals. This is the consequence of the interaction in the initial coordination game, which apparently allowed the principal to gather information on the characteristics of their agent.

Table 1: Effort-choice game: Principals' actions and second-order beliefs by group

		Principal		
Control decision		**No-group**	**In-group**	***Total***
No control		4	14	18
Min 6		24	21	45
Min 21		51	51	102
Total		79	86	165
Fisher's exact test, *p*-value: 0.060				
S.O. belief		**No-group**	**In-group**	***Diff.***
No control	Average	11.43	23.76	-12.33***
	Median	10	20	
Min 6	Average	32.51	28.04	4.47
	Median	30	30	
Min 21	Average	56.06	48.21	7.85*
	Median	55	50	
Are S.O. beliefs "correct"?		**No-group**	**In-group**	
No control		no	no	
Min 6		no	yes	
Min 21		yes	yes	
Observations: 165 principals				

Note: This table reports the summary statistics of the principals' control decisions and beliefs in the effort-choice game, differentiated by group 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 no-group and in-group principals are shown: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Second-order beliefs are "correct" if the Mann-Whitney test does not reject the hypothesis that the agents' control beliefs and the principals' corresponding second-order beliefs are the same ($p > 0.1$). The distributions of the counts are tested for intergroup differences with Fisher's exact test.

4.2 Real-Effort Game

In this section, we investigate how belonging to a social group affects the agents' decisions in a real-effort experiment.

Performance Regarding Conjecture 1, we observe both in-group and no-group agents who decide to transfer only what the principals force them to share, while there is voluntary sharing as well. Moreover, the intergroup difference in the proportion of agents deciding to exceed the minimum requirement set by the principal is significant at the 1 percent level (Chi-square test, $p = 0.008$).[28] This result provides support for Conjecture 3.

In Figure 3, we depict voluntary sharing in the real-effort game by group.[29] Pooling across control levels, we find that the average voluntary effort is two and a half times greater for in-group than for no-group agents (Mann-Whitney test, $p = 0.002$).[30] Moreover, similar to our findings from the effort-choice game, at *any* level of control, agents who had a shared experience with their principals in the coordination game transfer more voluntarily than agents who had no prior interactions with their principals.[31] When comparing the cumulative distribution functions of agents' sharing behaviors, we can reject the null that the in-group and no-group agents' distributions are the same at the 1 percent level (Kolmogrov-Smirnov test, $p = 0.009$).

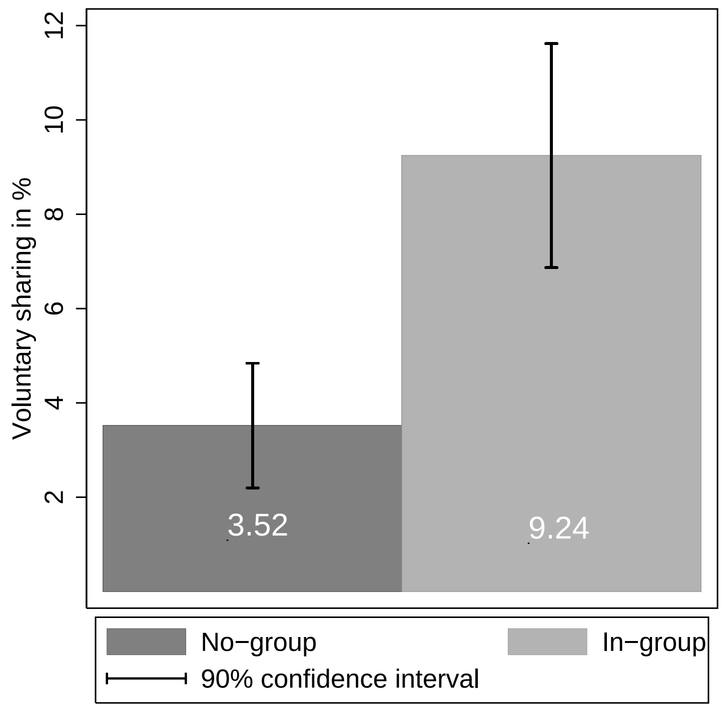
[28] We observe a similar test statistic when excluding the 14 principal-agent pairs that were not able to produce a satisfactory result in the coordination game.

[29] Table 5 in Appendix C provides the average and the median values of the agent's voluntary effort and his beliefs.

[30] In the subsample of principal-agent pairs for whom group induction worked properly we again find somewhat stronger results. In-group agents that evaluated their partner's behavior as fair or very fair (feedback of 4 or 5) share, on average, three times as much voluntarily as no-group agents (Mann-Whitney test, $p = 0.002$).

[31] However, as the number of in-group and no-group observations differs for each control level (see middle panel in Table 5), it may be misleading to analyze treatment effects by the level of control.

Figure 3: Real-effort game: Average voluntary sharing in percent

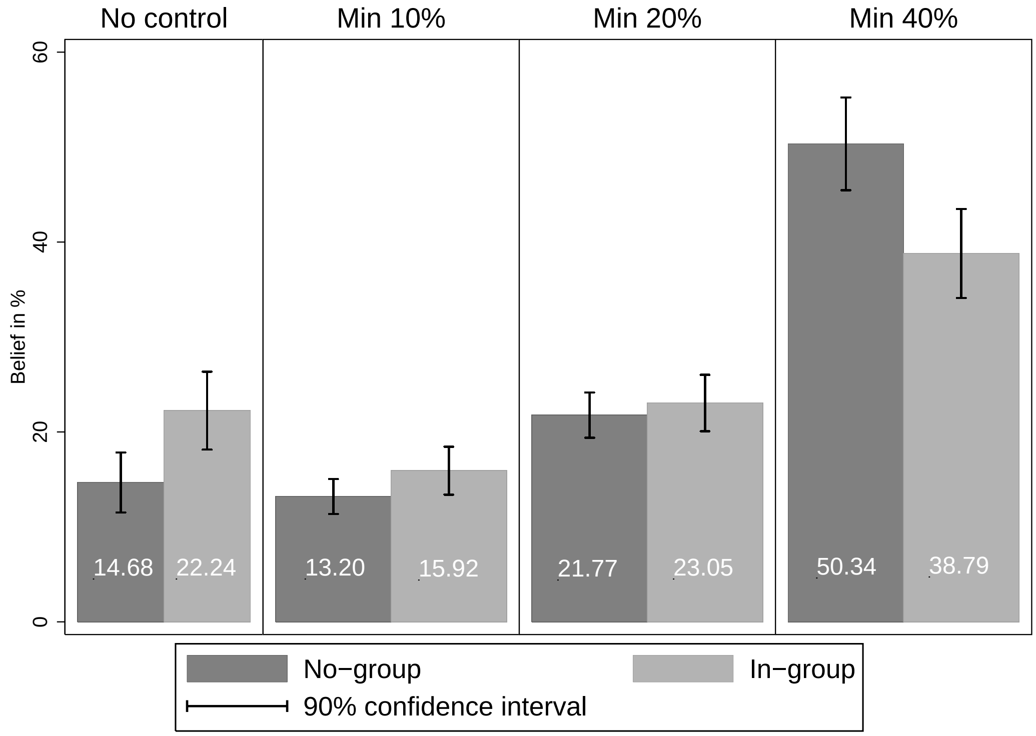


Note: This figure illustrates the agent's voluntary sharing in the real-effort game. Sharing here means the percentage of total earnings in the real-effort task that the agent chooses to transfer to the principal. The agent makes this decision *before* the real-effort game is played.

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 4).[32] Regarding the cumulative distribution functions of control beliefs, however, the agents only differ significantly for maximum control (Kolmogrov-Smirnov test, $p < 0.000$).

[32] Excluding those in-group agents for whom group induction can be expected to have not worked properly does not change the results.

Figure 4: Real-effort game: Average belief over control level by group



Note: In this figure, we depict the agents' beliefs regarding the various control levels that the principals may choose. Average beliefs are shown. In-group agents expect their principals to trust them completely significantly more often than their no-group counterparts do (Mann-Whitney test, $p = 0.076$), and expect to face maximum control significantly less frequently ($p = 0.002$). There are no significant differences for intermediate levels of control (*Min 10%*: $p = 0.416$; *Min 20%*: $p = 0.831$).

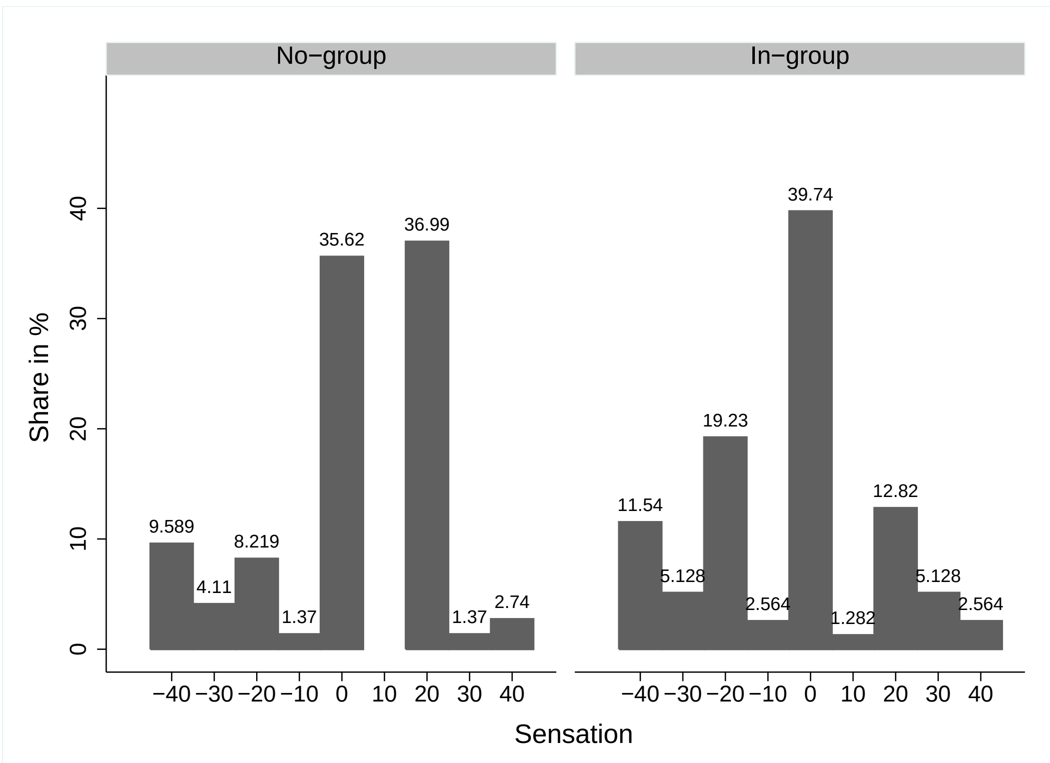
Next, we turn to the modal control beliefs, which are our experimental equivalent for the individually expected level of control, denoted by $\hat{m}$ in the model.[33] As in the effort-choice game, we again find that no-group agents have greater control expectations than their in-group counterparts, supporting Conjecture 5. On average, no-group agents find a control level of 31.91 the most likely, which is significantly higher than in-group agents' average modal control beliefs, 22.05 (Mann-Whitney test, $p < 0.001$). The respective medians are 40 for no-group and 20 for in-group agents. The distributions of in-group and no-group agents' modal control beliefs are different at the 1 percent significance level (Kolmogorov-Smirnov test, $p = 0.001$).

Sensation We now investigate the agent's reaction to sensation, which we refer to as the deviation of the agent's modal control belief from the experienced level of control. Figure 5

[33] 14 agents (six no-group and eight in-group 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. Thus, our below analysis of modal beliefs and sensation is only based on 151 observations.

presents the distributions of sensation for the two types of agents. It becomes apparent that the strong treatment differences in the individual control beliefs discussed above also translate into differences in sensation. In particular, we find that no-group agents, on average, face higher sensations. Interestingly, the average value of in-group agents' sensations is negative (-5), while no-group agents experience positive sensations on average (2.05). The intergroup difference of sensation is significant at the 5 percent level (Mann-Whitney test, $p = 0.023$). Concerning the respective cumulative distribution functions, we can reject similarity between agent types at a significance level of 10 percent (Kolmogorov-Smirnov test, $p = 0.058$).

Figure 5: Real-effort game: Histogram of sensation by group



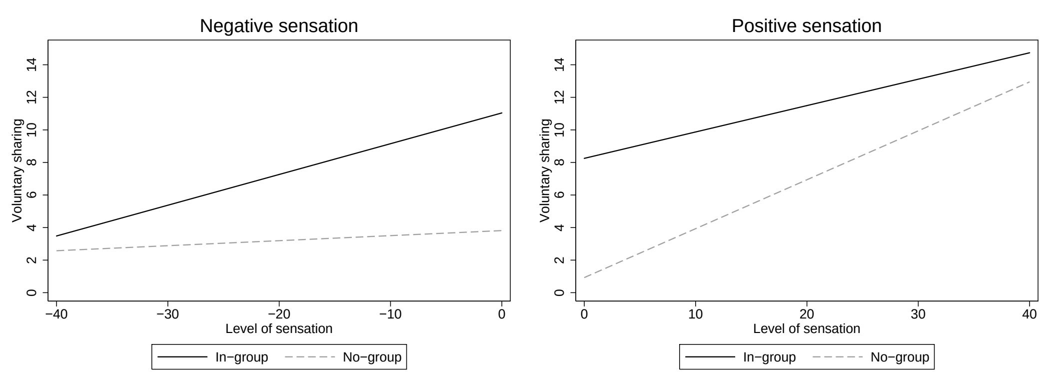
Note: This figure shows the histograms of the experienced sensation (difference between expected and realized control) by group. The bars indicate the proportion of the agent population that face a sensation of size i. Underlying this figure are 151 agent observations because 14 agents did not regard 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.

Next, we focus on the question whether the behavioral reaction to sensation is different between groups. Before we come to the regression analysis, we show the agents' sharing responses to sensation graphically in Figure 6. Some results are noteworthy. First, in the absence of any sensation, there is almost a 10:1 difference in the inclination for voluntary sharing between agent types. On average, in-group agents voluntarily share approximately 10.7 percent of their earnings if there is no sensation, while the voluntary sharing of no-group agents 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, in-group agents share more voluntarily than their no-group 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 (actual control exceeds the agent's expectation) plotted in the left panel in Figure 6, the drop in the sharing increment is more pronounced for in-group than for no-group 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. The picture is reversed for positive sensations (actual control is lower than the agent's expectation), depicted in the right panel in Figure 6. Here, the reciprocal reaction to sensations is stronger for no-group than for in-group agents. At the maximum level of sensation, 40, little difference in the level of voluntary sharing between groups remains.

Although we had no theoretical predicition on the direction of the treatment effect for positive sensations, a possible explanation for our findings in this regard is intention-based reciprocity (Charness and Rabin, 2002; Falk and Fischbacher, 2006). As there is more uncertainty about the agents' propensities for acting selfish in the no-group treatment than in the in-group treatment[34], imposing loose control is a risky option particularly for the no-group principal. Hence, a no-group 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).

Figure 6: Agents' reactions to sensations: 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 151 agent observations. See Figure 5 for details.

The effect of sensation on voluntary sharing is more rigorously investigated in Table 2. Be-

[34] The intergroup differences in the principals' abilities to correctly anticipate the agents' control beliefs, displayed in Tables 1 and 3, are consistent with this argument.

cause our dependent variable, voluntary sharing (*Voluntary*), can only take nonnegative integer values and displays signs of over-dispersion (see table for formal tests), we perform negative binomial regressions.[35]

Table 2: Negative binomial estimates of the effect of sensation on voluntary sharing by groups in the real-effort task

	(1)	(2)	(3)
Sensation	0.014***	0.018*	
	(0.005)	(0.010)	
In-group		1.112***	2.319***
		(0.275)	(0.683)
In-group × sensation		-0.001	
		(0.011)	
Pos. sensation			0.073***
			(0.024)
In-group × pos. sensation			-0.070**
			(0.028)
Neg. sensation			-0.029
			(0.024)
In-group × neg. sensation			0.057**
			(0.028)
Observations	151	151	151
Wald chi-squared	128.95	164.62	203.79
Prob > chi-squared	0.000	0.000	0.000
alpha	1.707***	1.617***	1.537***
	(0.160)	(0.174)	(0.179)
χ^2 for alpha=0	1725.632***	1470.375***	1348.030***

Note: This table reports the results of negative binomial regressions of sensation on voluntary sharing by group. *Sensation* is measured as the difference between expected and actual control, with expected control being approximated by the agent's modal control belief. Because 14 subjects did not have modal control beliefs, the regressions include 14 fewer observations than in the full sample. *In-group* is a binary variable, taking the value of 1 if the principal and the agent played the coordination game at the beginning of the experiment and 0 otherwise. *Pos. sensation* indicates the level of the sensation if the sensation is strictly above zero. Otherwise, *Pos. sensation* equals 0. *Neg. sensation* is defined accordingly; that is, it exhibits non-zero (and negative) values if the sensation is strictly below zero. Likelihood-ratio test for *alpha* (estimated without robust standard errors) show over-dispersion for all specifications, which justifies the choice of the negative binomial model. Heteroskedasticity-robust standard errors, which are clustered by session, are presented in parentheses.
* z<0.10, ** z<0.05, *** z<0.01.

[35] Our results remain qualitatively the same under a range of alternative specifications. For instance, we estimated the models with a full set of session dummies as additional regressors. We also ran probit and OLS regressions.

From Column 1, it becomes apparent that voluntary sharing increases in the sensation, providing support for Conjecture 2. In Column 2, the positive and highly significant coefficient on *In-group* indicates that in the absence of any sensation, in-group agents share more voluntarily than no-group agents. The interaction term *In-group* × *sensation* allows for treatment effects in the response to sensation. Due the inclusion of the interaction term, *Sensation* in Column 2 refers only to no-group agents. Although the positive impact of sensation on voluntary sharing is somewhat weaker for no-group agents than for the total sample, it is still positive and significant. The insignificant interaction term suggests that in-group and no-group agents increase voluntary sharing in sensation equally.

In Column 3, we additionally consider the nature of the sensation. The positive and significant coefficient on *Pos. sensation* shows that no-group agents voluntarily share more the larger the (positive) difference between expected and experienced control. The negative coefficient on the interaction term *In-group* × *pos. sensation* indicates that the effect on sharing of a positive sensation is less pronounced for in-group than for no-group agents, which refines Conjecture 7. Facing a negative sensation, no-group agents do not seem to react in terms of voluntary sharing; the coefficient on *Neg. sensation* is insignificant. However, there is a significant interaction effect. The positive coefficient on *In-group* × *neg. sensation* shows that an in-group agent who experiences a negative sensation decreases voluntary sharing by more than the respective no-group agent. This result provides support for Conjecture 6. Hence, there are significant treatment differences in the reactions to sensations, but these are only visible when accounting for the nature of the sensation.[36]

Questionnaire Results from the questionnaire provide further evidence that the agent's relationship to the principal, developed in the initial coordination game, is driving our results on intergroup differences. The attitudes toward control generally do not significantly differ between the two types of agents.

Principals

Control The principals' control decisions and second-order beliefs are shown in Table 3. Consistent with the results of the effort-choice game, there are treatment differences in the principal's choice of control (Fisher's exact test, $p = 0.094$).[37] Compared to their no-group counterparts, in-group principals are more often inclined to choose no or low control. However, it is also

[36] All coefficients are of the same sign and of a comparable magnitude as those presented above when we exclude in-group agents who did not provide a sufficiently positive feedback (rating above 3) on their partners in the initial coordination game.

[37] If those principals are excluded from the sample who were not satisfied with their partners in the initial coordination game (rating below 4), the principal's control choice does not differ between groups ($p = 0.164$).

apparent from Table 3 that only 8 percent (3 percent) of the in-group (no-group) principals decide not to restrict the agent at all, with an insignificant difference between groups (Fisher's exact test, $p = 0.171$). This result indicates that trusting the agent completely is too risky of a choice for the majority of principals. It is nevertheless striking that, in relative terms, in-group principals select no or low control three times as often as no-group principals (6.3 percent vs. 19.76 percent).

Second-order beliefs The middle panel in Table 3 displays that in-group principals expect their agents to believe that they are not controlled more frequently and maximally controlled less frequently than no-group principals. There are no treatment effects for the two intermediate control levels. We also observe intergroup differences in the modal second-order beliefs. For instance, only one no-group principal thinks that the agent finds no control most likely, while eight in-group principals have this belief (Fisher's exact test, $p = 0.034$). This is the same 1:8 ratio that we observed in the effort-choice game. The frequency of principals with a modal second-order control belief of 10 ($p = 0.037$) or 20 ($p = 0.097$) also differs between groups ($p = 0.395$ for maximum control).

The results on the principals' abilities to correctly anticipate the agents' control beliefs, shown in the lower panel in Table 3, are also along the lines of those found in the effort-choice game. Again, both types of principals tend to understate the agents' beliefs not to face any control (Mann-Whitney test: no-group: $p = 0.001$; in-group: $p = 0.083$). Moreover, in-group principals are slightly better in guessing the agents' control beliefs than their no-group counterparts. Thus, it appears that the initial coordination game revealed information on the agents' control expectations, thereby influencing beliefs about beliefs.

Table 3: Real-effort game: Principals' actions and second-order beliefs by group

		Principal		
Control decision		**No-group**	**In-group**	***Total***
No control		2	7	9
Min 10%		3	10	13
Min 20%		30	26	56
Min 40%		44	43	87
Total		79	86	165
Fisher's exact test, p-value: 0.094				
S.O. Belief		**No-group**	**In-group**	***Diff.***
No control	Average	5.67	15.91	-10.24***
	Median	5	10	
Min 10%	Average	15.42	18.70	-3.28
	Median	10	15	
Min 20%	Average	27.20	24.78	2.42
	Median	25	25	
Min 40%	Average	51.71	40.62	11.09***
	Median	50	40	
Are S.O. beliefs "correct"?		**No-group**	**In-group**	
No control		no	no	
Min 10%		yes	yes	
Min 20%		no	yes	
Min 40%		yes	yes	
Observations: 165 principals				

Note: This table reports the summary statistics of the principals' decisions and beliefs in the real-effort game, differentiated by group treatment. See Table 1 for further details.

5 Conclusions

The previous literature on psychology in organizations recognized identity as a powerful concept to explain individual behavior, for example, promotion decisions (Fajak and Haslam, 1998) and turnover intentions (Dick et al., 2004; Haslam, 2001). In this paper, we explore the role of social groups in explaining the behavioral reaction to control in a principal-agent setting. Incorporating concepts from identity economics (Akerlof and Kranton, 2000, 2005) into a simple analytical framework, we expect an agent who identifies himself with the principal (in-group) to react differently to control than an agent without any social ties with the principal (no-group). In particular, we hypothesize that social identity shapes the agent's behavior when a control *sensation* occurs, where the level of control implemented by the principal deviates from the agent's expectation.

We experimentally test the validity of these hypotheses, manipulating the social distance between the principal and the agent with the help of a coordination game that is played in the beginning of the experiment. The induction of group membership was evident in the behavioral choices, as in-group agents supply more effort than no-group agents for all control levels available for the principal to choose. At the same time, in-group agents expect to be controlled significantly less by their principals. The principals meet these expectations; we observe that in-group principals control less frequently than their no-group counterparts.

However, if the principal's control exceeds the agent's expectation, an in-group agent's performance decreases sharply, while the performance of a no-group agent remains unaffected. A candidate mechanism explaining the pronounced reaction of in-group agents to negative sensations is the higher emotional significance they attach to the principal's control decision. Thus, in-group agents are more disappointed than no-group agents if a negative sensation occurs. Moreover, the intergroup effort gap also shrinks in the positive sensation. No-group agents are more inclined than in-group agents to increase performance when facing a lower level of control than the one that was expected. Intuitively, a positive sensation may be interpreted by the agent as a signal of kindness or trustworthiness, which is more risky to send if the principal and the agent have never been interacted before. Therefore, a principal revealing himself as a trusting actor who implements only little control (as compared to the agent's expectation) may surprise no-group agents more pleasantly than in-group agents. This greater value no-group agents attach to positive sensations translates into greater willingness to exert effort in the principal's interest.

We contribute to the research on the relationship between social identity and work motivation or task performance. Virtually all previous studies yield evidence in support of a positive impact of identity on motivational and performance-related factors (for an overview, see Van Knippenberg, 2000). Little is known about the possible detrimental consequences of identity. Our results imply that identity determines whether agents punish their principals for control levels they perceive as inappropriately high. In principal-agent relationships that do not involve the feeling of belongingness to a group, agents lack the motive to retaliate if their control expectations are disappointed. In the presence of a shared group identity between the principal and the agent, however, unexpectedly "bad" behavior by the principal generates the desire for sanctioning, as it is especially upsetting for the agent. A closer connection between the principal and the agent, even when group membership is anonymous, increases the likelihood that control is interpreted as a sign of distrust and, thus, entails substantial hidden costs. In the long run with repeated principal-agent interactions, behavior that is regarded as incongruent with the implied social identity may also corrode previous positive experiences, just as unexpectedly kind behavior may strengthen social ties. This is a promising avenue for future research.

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A 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.

$$\max U_A(e; m) = g(\Delta, e) \tag{3}$$

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

The Lagrangian to this problem is:

$$\mathcal{L}_A(\cdot) = g(\Delta, e) + \lambda(m - e) \tag{4}$$

The Kuhn-Tucker conditions read:

$$\frac{\partial \mathcal{L}_A}{\partial e} = g'(\Delta, e) - \lambda \leq 0 \quad (e \geq 0) \tag{5}$$

$$\frac{\partial \mathcal{L}_A}{\partial \lambda} = m - e \geq 0 \quad (\lambda \geq 0) \tag{6}$$

$$e^* \left[g'(\Delta, e) - \lambda \right] = 0 \tag{7}$$

$$\lambda(m - e) = 0 \tag{8}$$

$$e, \lambda \geq 0 \tag{9}$$

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 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:

$$\frac{de}{d\Delta} = -\frac{\partial^2 g}{\partial\Delta\partial e} / \frac{\partial^2 g}{\partial e^2} \tag{10}$$

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

B Effort-Choice Game

Table 4: Effort-choice game: Summary statistics of the agents' voluntary sharing decisions and beliefs by group

		Agent		
Voluntary sharing		**No-group**	**In-group**	***Diff.***
if No mon	Average	26.65	37.70	-11.05***
	Median	23	36.5	
if Min 6	Average	16.73	24.30	-7.57*
	Median	11	24	
if Min 21	Average	7.39	14.48	-7.09**
	Median	0	4	
Belief		**No-group**	**In-group**	***Diff.***
No mon	Average	21.47	32.29	-10.82**
	Median	20	25	
Min 6	Average	23.96	23.91	0.05
	Median	20	20	
Min 21	Average	54.57	43.80	10.77***
	Median	50	40	
Observations: 165 agents				

Note: This table reports summary statistics of the agents' effort decisions and beliefs, differentiated by group 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, while the differences in the respective averages between no-group and in-group agents are displayed: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Real-Effort Game

Table 5: Real-effort game: Summary statistics of agents' voluntary sharing decisions and beliefs by group

		Agent		
Voluntary sharing		**No-group**	**In-group**	***Diff.***
	Average	3.52	9.24	-5.72***
	Median	0	5	
Observations				***Total***
No control		2	7	9
Min 10%		3	10	13
Min 20%		30	26	56
Min 40%		44	43	87
Belief		**No-group**	**In-group**	***Diff.***
No mon	Average	14.68	22.24	-7.56*
	Median	10	10	
Min 10%	Average	13.20	15.92	-2.72
	Median	10	10	
Min 20%	Average	21.77	23.05	-1.28
	Median	20	20	
Min 40%	Average	50.34	38.79	11.55***
	Median	50	32.5	
Observations: 165 agents				

Note: This table reports summary statistics of the agents' performance decisions and beliefs in the real-effort game, differentiated by group treatment. Voluntary sharing is the share of an agent's earnings in the real-effort task that he is willing to transfer to the principal beyond the latter's required minimum transfer. Notice that the agent makes his performance decision *before* he starts to solve the real-effort task. See Table 4 for further details.

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