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## On social identity, subjective expectations, and the costs of control

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# JENA ECONOMIC RESEARCH PAPERS



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## **On Social Identity, Subjective Expectations, and the Costs of Control**

by

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# On Social Identity, Subjective Expectations, and the Costs of Control\*

Gerhard Riener and Simon Wiederhold<sup>†</sup>

August 19, 2011

## Abstract

Controlling employees can have severe consequences in situations that are not fully contractible. However, the perception of control may be contingent on the nature of the relationship between principal and agent. We, therefore, propose a principal-agent model of control that takes into account social identity (in the sense of Akerlof and Kranton, 2000, 2005). From the model and previous literature, we conclude that a shared social identity between the principal and agent has

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both a cognitive, that is, belief-related, and a behavioral, that is, performance-related, dimension. We test these theoretical conjectures in a labor market experiment with perfect monitoring. Our findings confirm that social identity has important implications for the agent's decision-making. First, agents who are socially close to the principal (in-group) perform, on average, more on behalf of the principal than socially distant (no-group) agents. Second, social identity shapes the agent's subjective expectations of the acceptable level of control. In-group agents expect to experience less control than no-group agents. Third, an agent's reaction to the monitoring level she eventually faces also depends on social identity. If the experienced level of control is lower than the expected control level, that is, the agent faces a positive sensation, the increase in performance is less pronounced for in-group agents than for no-group agents. In the case of a negative sensation, however, in-group agents react stronger than no-group agents. Put differently, being socially distant from the principal amplifies the performance-enhancing effect of a positive control surprise and mitigates the detrimental performance effect of a negative surprise.

**JEL code:** C92, M54, D03

**Keywords:** Control; Identity; Employee motivation; Principal-agent theory; Lab experiment

# 1 Introduction

Control can have severe consequences in principal-agent relationships. Early empirical work by Barkema (1995) suggests this, as do experimental studies by Dickinson and Villeval (2008), Falk and Kosfeld (2006), Ploner et al. (2010), and, from a macroeconomic perspective, Porta et al. (1997). This work has largely ignored that the costs and benefits of control may depend on the nature of the relationship between the principal using the control devices and the controlled agent. Referring to case-study evidence from the U.S. steel industry, Akerlof and Kranton note 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 notion has also found experimental support in Dickinson and Villeval (2008). We develop their arguments further and propose that a shared group identity between the principal and agent influences the latter’s subjective expectations of the appropriate level of control. This “individually expected” level of control affects the agent’s perception of the principal’s actual control decision. We argue that the most influential factor in determining the agent’s performance is not the actually experienced level of control but its deviation from the subjective control belief. If the agent’s appraisal of the level of control imposed by the principal is a function of the social distance between principal and agent – we will show that it is indeed – a socially close agent will react differently to the principal’s control decision than a socially distant agent facing the same deviation from the expected level of control.

We propose a formal modeling framework that allows us to integrate social identity (in the sense of Akerlof and Kranton, 2000, 2005)<sup>1</sup> into a simple principal-agent model.

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<sup>1</sup>Following Ashforth and Mael (1989), we will use the notions of social identity and group identity

In the model, the agents decide how much effort to exert on behalf of the principal. The principal can eliminate the agent's most opportunistic choices by imposing a minimum effort requirement that the agent is not allowed to fall short of.<sup>2</sup> However, agents are free to exceed this minimum performance requirement, that is, to exert effort *voluntarily*. The primary variable that characterizes the principal-agent relationship in this model is social identity. Following the definition developed in social psychology, we refer to social identity as "that part of an individual's self-concept which derives from his knowledge of his membership of a social group (or groups) together with the value and emotional significance attached to that membership" (Tajfel, 1978, p. 63).<sup>3</sup> The more the individual perceives herself in terms of the characteristics she shares with other group members, the more likely she is to act on behalf of other members of the group (Ashforth and Mael, 1989; Turner et al., 1987; Van Knippenberg, 2000). For the purposes of our analysis, we distinguish between two general types of principal-agent relationships: in-group and no-group (Tajfel, 1970). In-group individuals identify at least partly with each other and behave in accordance with the social identity based on that group membership. No-group principal-agent relationships are characterized by the absence of a shared group identity; for example, one can think of both parties as being anonymous to each other. Notice that while our classification is categorical, the intensity of an in-group member's identification with the principal is a matter of

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interchangeably.

<sup>2</sup>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.

<sup>3</sup>This definition has been extensively applied to organizations (for instance, Abrams et al., 1998; Ashforth and Mael, 1989; Benkhoff, 1997; Hogg and Terry, 2000; Ouwerkerk et al., 1999). See also the literature on organizational identification, which is usually traced back to March and Simon (1958). Riketta (2005) provides a recent overview.

degree.<sup>4</sup>

Our modeling framework illustrates that the degree of social distance between principal and agent can have cognitive and behavioral implications for the agent. On the one hand, social identity may affect the level of both the agent's effort and control belief. We expect in-group agents to be more willing to exert effort on behalf of the principal than no-group agents. At the same time, we predict that in-group agents believe that they will face less control. On the other hand, there may be intergroup differences in the performance response to control *sensations*. We refer to sensation as the deviation of the agent's expected level of control from the actually experienced one. A positive sensation occurs if the agent's control belief exceeds the level of control faced. A negative sensation is defined accordingly. The model suggests that for both types of agents the willingness to exert effort on behalf of the principal increases in the sensation. However, we expect to find intergroup differences when taking into account the nature of the sensation. For positive sensations, we hypothesize that no-group agents will react with a higher level of reciprocation than in-group agents. For negative sensations, however, no-group agents will reciprocate less, that is, their performance will not decrease as much as for in-group agents.

We conduct a simple labor market experiment to test these behavioral predictions in a controlled environment. Our experimental design is inspired by Falk and Kosfeld (2006) but extends this and others' work in a number of important ways. First, we allow different levels of control in a within-subject design. The principal does not have a

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<sup>4</sup>For the ease of illustration, we abstract from formally modeling the intensity of an agent's identification with the principal. 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.

binary choice (control/no control) but can choose between three different control levels. We observe the agent's choices for each degree of control under the strategy method (Selten, 1967). Second, all subjects play a second stage where agents are remunerated for solving a real-effort task. Before solving the task, they must decide how much of their future remuneration they are willing to share with the principal. The principal can choose a minimum share that the agent must deliver. Instead of using the strategy method, agents receive information on the required minimum share before making their sharing decision and performing the task. The virtues of this procedure are twofold. First, we are able to assess the robustness of the findings from the strategy method in a situation that requires real effort. Second, and even more importantly, the real-effort game allows us to study the influence of an experienced sensation on the behavior of the subjects.

We find in both effort sharing and the real effort that in-group agents expect lower levels of control than no-group agents and are on average more willing to exert effort voluntarily on behalf of the principal. In-group agents' efforts exceed the efforts selected by no-group agents for all control levels. We also observe that both types of agents increase their voluntary performance in the level of sensation. However, there are intergroup differences in the reactions to sensation. Having experienced a positive sensation, no-group agents act more favorably than in-group agents. The opposite is true for negative sensations; in-group agents reduce effort to a higher degree than no-group agents. Thus, in the polar cases of maximum and minimum sensation, the intergroup differences in the levels of effort selected nearly vanish. These findings imply that the stimulating impact of social identity on the agent's performance strongly depends on the

principal's control decision. Disappointing the agent's control expectations can prove costly, especially for in-group principals, and in extreme cases 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, social identity can easily backfire on the principal and actually reduce the agent's effort.

The previous literature on psychology in organizations recognized the importance of social identity as a powerful concept to explain individuals behavior, for example, turnover decisions (Dick et al., 2004a; Haslam, 2001; Haslam and Ellemers, 2006). Consequently, fostering an individual's identification with an organization has been stressed as one of the key means of improving staff retention (Davies, 2001) and increasing an individual's willingness to exhibit extra-role behavior (Dick et al., 2004b). However, using real-world data to estimate the behavioral consequences of social identity is problematic because it requires intimate knowledge of the nature of the principal-agent relationship. To establish social identity's causal effect on an agent's performance response to a principal's control decision, one would have to consider a myriad of variables: personal characteristics, economic dependency on the job, organizational tenure, recency of membership in the organization, satisfaction with the organization, informal organizational structure, and the existence of multiple foci of identification.<sup>5</sup> These factors

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<sup>5</sup>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 from her department, union, team or work group, etc. Indeed, apart from a few examples (for instance, Theory Z organization as described in Ouchi, 1981), a single organizational identity is unlikely to be encountered in most complex organizations (Ashforth and Mael, 1989). Because norms, values, goals, and performance standards might be different at the organizational and subunit levels, the presence of multiple foci of organizational identification renders the investigation of the performance effect of control conditional on the nature of the principal-agent relationship difficult.

influence both group identity and performance; hence, endogeneity resulting from omitted variables is a serious concern when these variables are not properly controlled for. Using a laboratory experiment to study how social identity based on group membership affects an agent's behavior ensures us that

- social identity is salient, that is, the individuals are aware of the group membership,
- the decision-relevant social identity is unique,
- group interests are strictly performance-related and do not focus on, for example, interpersonal relations, and
- performance is under the full volitional control of the agent, that is, independent of knowledge, skills, abilities, etc.<sup>6</sup>

A further virtue of the experimental approach is that we can clearly distinguish between “task performance” and “contextual performance.”<sup>7</sup> 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; Van Knippenberg, 2000). In our view, contextual performance is a more appropriate indicator of the effects that group identity has on effort because the agent

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<sup>6</sup>According to Van Knippenberg (2000), these criteria must be met to impose a causal relationship between social identity and performance.

<sup>7</sup>In this context, see also the distinction between “perfunctory” and “consummate” performance in Hart and Moore (2008). In perfunctory performance, a contract can be written, and its terms can be judicially enforced. Consummate performance concerns the spirit of the contract and is not legally enforceable.

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 exert effort voluntarily.

To the best of our knowledge, this is the first work that investigates the role of subjective control beliefs in determining agents' reactions to control. In particular, we show that social identity influences both the formation of beliefs over control and the reaction to the control level experienced. This complements a previous work carried out by Abeler et al. (Forthcoming) that analyzes the reaction to reference points in wages; however, it does so without principal-agent interaction. Our study hopefully motivates further research on the role of social identity and its interaction with subjective expectations within organizations. Particularly interesting in this respect would be an investigation of the long-run impact of control and how it corrodes previous positive experiences.

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 and highlighting directions for future research.

## 2 The Model

### 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 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  $\hat{m}(c)$ . Further, we assume that the agent's utility depends on the magnitude of the deviation between the expected degree of control and the degree of control eventually faced, that is, the level of sensation.

Following Akerlof and Kranton (2008), the agent's utility is composed of three components. First, the agent receives a constant wage,  $w$ , with her 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:  $f''(e) \geq 0$ . Third, and most importantly, the sensation term, denoted by  $g((\hat{m} - m), e)$ , defines the utility effect of a deviation of the agent's expected level of control from the actually experienced one. The utility function of the agent then reads:

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

This formulation of the agent's utility captures the relationship between overall

satisfaction with a task or job and social identity (Dick et al., 2004a). 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.<sup>8</sup> 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:

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

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

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<sup>8</sup>In the results section, we will define the empirical equivalent of sensation.

**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 the agent's willingness to exert effort increases in the level of sensation, that is, the larger the sensation, the larger 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 level 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 reciprocity 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. 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 Hypotheses

The agent maximizes (2) w.r.t.  $e$  s.t.  $e \geq m$ , where  $m$  is the control choice of the principal, which the agent takes as given.<sup>9</sup> 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, there may also be 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 the sensation.*

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<sup>9</sup>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). Moreover, the previous literature has 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 concerned with status than with money (Warwick, 1980), government managers perceive the relationship between pay and performance as being weaker (Buchanan, 1974, 1975; Ingraham, 1993; Perry et al., 1989; Rainey, 1983), and public employees strive less for pecuniary gains (Rainey, 1983) and more frequently doubt that good performance leads to promotions (Rainey, 1979; Rainey et al., 1995).

Conjectures 1 and 2 are the result of the agent's optimization calculus when abstracting from group identity. Therefore, both conjectures should only hold regarding 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  $c = 1$  refers to 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 implies that the level of effort exerted for the principal depends on social identity, that is,  $e = e(c)$ . It has been found that in-group members take on the group's perspective and are therefore more likely to expend effort on behalf of the group. Early laboratory studies find that simply assigning an individual to a group can be sufficient to induce in-group favoritism (Ashforth and Mael, 1989; Brewer, 1979). Evidence from the public sector suggests that the distance between principals and agents is expected to directly affect whether an agent shirks or works (Chaney and Saltzstein, 1998; Scholz, 1991). In a similar vein, experimental work carried out by Dickinson and Villeval (2008) shows a preferential treatment of subjects with lower social distance; however, their study is unable to distinguish identity from social pressure effects (for more on the effects of group identity, see Chen and Li, 2009). Therefore, we modify Conjecture 1 to incorporate the between-group differences in the inclination to exert effort voluntarily.

**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 on in-group favoritism also implies a “level effect” of social identity. Compared to their no-group counterparts, we expect in-group agents to supply, on average, more effort on behalf of the principal.

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

Second, the discussion of reference point effects in Hart and Moore (2008) implies that the individually expected level of control depends on social identity, that is,  $\hat{m} = \hat{m}(c)$ . Applying their theory of contracts as reference points to our setting, principal-agent pairs with a shared common (positive) 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 agents in an anonymous principal-agent relationships.

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

Finally, Conjecture 5 suggests that effort,  $e$ , and sensation,  $\Delta$ , are positively associated. This should hold for both types of agents. However, there may still be intergroup differences in the performance response to sensation. In the case of a negative sensation,  $\Delta < 0$ , we expect the decrease in the willingness to perform on behalf of the principal to be more pronounced for in-group than for no-group agents. This is because high control is likely to be seen as incongruent with the implied group identity. 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, a phenomenon that has been reported in the psychological literature by Koehler and Gershoff (2003).<sup>10</sup>

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<sup>10</sup>The existing literature is usually silent about the consequences of group members not acting in

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

If the sensation faced is positive,  $\Delta > 0$ , we expect in-group agents to reciprocate less than their no-group counterparts. Intuitively, no-group agents may be more pleasantly surprised by a positive sensation than in-group agents. Therefore, the former attach a greater value than the latter to this sensation, reacting with a relatively greater performance increase. However, because the previous literature does not provide clear guidance on the direction of intergroup differences for a positive surprise, we formulate Conjecture 7 as:

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

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; that is, 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 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.

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accordance with other group members' beliefs, norms, and values. There are, however, studies that focus on the potentially negative effects of identification on performance, for example, if informal group norms promote the minimization of work efforts (Van Knippenberg, 2000).

### 3 Experimental Design

To test the above conjectures, we conduct a simple labor market experiment. We implement a design that allows the principal to control the output of the agent by imposing a minimum effort restriction. Subjects are divided into groups of two, and the roles of principal and agent are randomly assigned. In the first step we induce group identity in the in-group treatment. In the no-group treatment, individuals perform a task in isolation. To test Conjectures 1, 3, 4, and 5, subjects play an effort choice game using the strategy vector method. Agents must state the efforts they are willing to exert for each level of control the principal can choose. This stage of the experiment is an extended version of the experimental design implemented by Falk and Kosfeld (2006). 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 test Conjectures 2, 6, and 7, subjects play a real-effort game after the effort choice game, where the agent learns about the principal's control decision before making his performance choice. We now turn to a detailed explanation of the stages.<sup>11</sup>

#### Group Formation Phase

In the group-formation phase, in-group subjects 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 agents' contributions to the public account, doubled by the experimenter. The agents' total payment was the sum of the public and group account. After an

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<sup>11</sup>We also ran two pilot sessions with a total of 36 participants whose results are not reported here.

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. 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. The coordination game had an obvious focal point, to ease the establishment of group feelings.<sup>12</sup> 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. After the game and the disclosure of the results, subjects had to give their partner feedback on how fair they found their partner's behavior. The subjects could pick any natural number between 1 (very unfair) and 5 (very fair). 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, 2011). The challenge here was to 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.<sup>13</sup>

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<sup>12</sup>Techniques of group induction have long been used in social psychology (for example, Turner, 1981) but only recently found their way into the experimental-economics literature (among others, Heap and Zizzo, 2009).

<sup>13</sup>80 ECU were the average payoff of in-group principals and agents, respectively, in the pilot sessions.

## Effort-choice phase

The effort-choice game was designed parsimoniously, without work environment frames. Each agent had an initial endowment of  $E = 1, 2, \dots, 117$ , 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; that is, they had to decide on efforts for all possible minimum effort levels without knowing the principal's actual decision. We refer to the difference between the agent's effort choice and the principal's control as voluntary sharing.

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 et al. (2010). 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 solve summations, and the remuneration depended on the number of correct answers. Before the agent started the task, he had to decide what share of his revenues to transfer to the principal. Because, again, the principal had no initial endowment, the agent's transfer was the only source of income for the principal at this stage. However, the principal could restrict the agent's choice set by imposing a minimum transfer, choosing his control level 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 this 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 et al., 2002). 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.

## Belief elicitation

To assess the role of deviations from expected control in shaping the agents' behavior, we elicited subjective control beliefs. The agents were asked to report their perception

of the likelihood of each possible control level available for the principal to choose. The principals had to reveal their beliefs regarding the agents' control beliefs (second-order beliefs). We did not incentivize these answers because there is evidence that eliciting expectations with or without incentivization for accuracy does not yield different results (Camerer and Hogarth, 1999; Grether, 1992).<sup>14</sup>

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.

## 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 et al. (2010). In particular, subjects were exposed to different work-place scenarios. For each of these situations, we ask 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

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<sup>14</sup>Gächter 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.

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 voluntary sharing (defined as an agent's contribution beyond the principal's minimum requirement) at all control levels. However, judging by Mann-Whitney tests<sup>15</sup>, the proportion of agents choosing to share exactly the minimum requirement imposed by the principal does not significantly differ between groups.<sup>16</sup> This challenges Conjecture 3, which predicts that in-group agents are more likely to voluntarily share their endowment with the principal than anonymous agents.<sup>17</sup>

Next, we turn to the intergroup differences in voluntary effort, depicted in Figure

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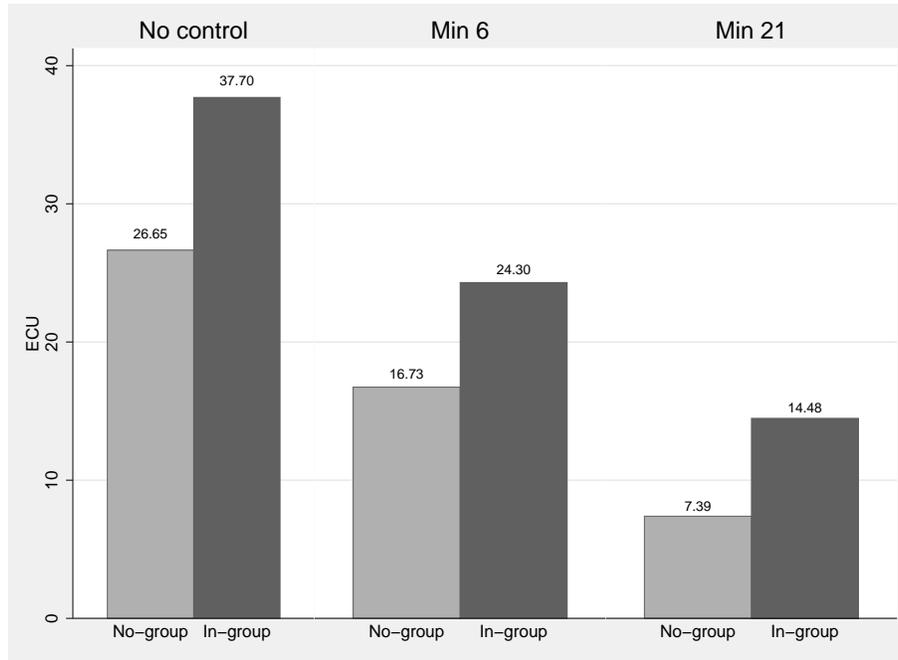
<sup>15</sup>If not otherwise stated, we report the results of two-sided Mann-Whitney tests throughout the paper.

<sup>16</sup>The test statistics are as follows: *No control*:  $p = 0.238$ ; *Min 6*:  $p = 0.392$ ; *Min 21*:  $p = 0.102$ .

<sup>17</sup>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 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 confirmed for maximum control. The respective test statistics are *No control*:  $p = 0.880$ ; *Min 6*:  $p = 0.182$ ; *Min 21*:  $p = 0.017$ .

1.<sup>18</sup>

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



*Note:* This figure shows the average voluntary sharing for the three different control levels by group. We find 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*:  $p = 0.018$ ).

We 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 (as compared to being anonymous) 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 monitoring, the order of magnitude of the between-group

<sup>18</sup>The underlying table with test statistics can be found in Appendix B.

difference in voluntary sharing remains roughly constant.<sup>19</sup> These results are, on the aggregate level, in line with Dickinson and Villeval (2008), although they do not split their results by control level. Interestingly, 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;  $p = 0.006$ ). Moreover, out of the 23 in-group agents that shared unconditionally, ten chose to transfer at least half of the endowment (that is, sharing 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.<sup>20</sup> In addition, a variance ratio test shows that the variance is higher for subjects 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 (*Min 6*:  $p = 0.030$ ).

Comparing the cumulative distribution functions of voluntary sharing, we find a significant difference between the groups for each control level. This refines 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

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<sup>19</sup>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.

<sup>20</sup>This result is in line with previous findings by van Leeuwen and van Knippenberg (1999). They let the participants of their experiment, divided in groups, put sheets of paper in envelopes. The experimenters measured identification with the work group for each individual and elicited beliefs about other group members' task efforts. The effort of participants with a relatively high group identification was to a large degree independent of the effort they expected from the other group members. Low identifiers, however, increased their own effort substantially when they expected higher effort from their fellow group members.

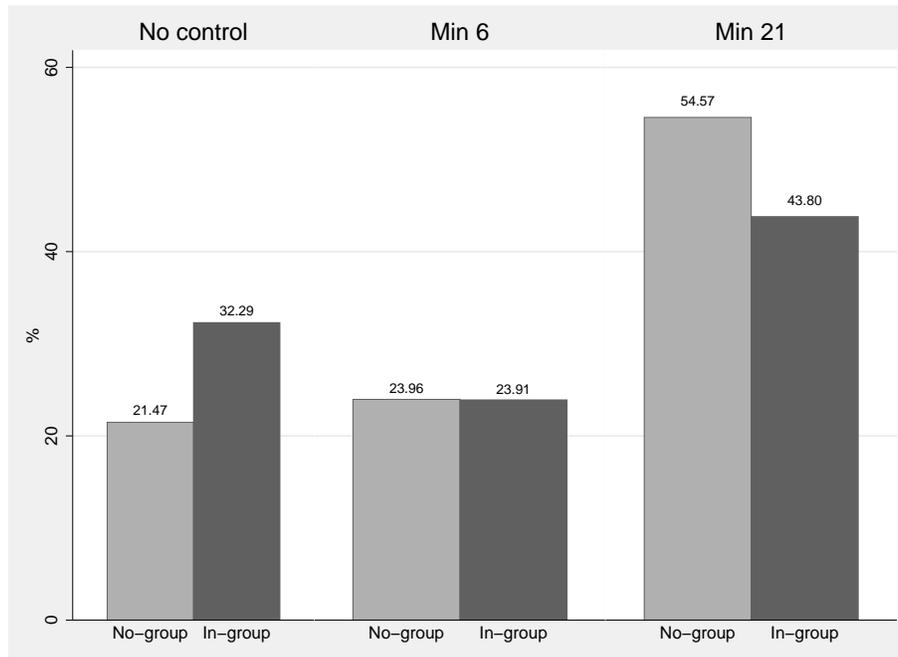
control, we can reject the equality of distributions at a significance level of 10 percent ( $p = 0.096$  and  $p = 0.056$ , respectively). Moreover, we find that voluntary sharing under lower control levels first-order stochastically dominates the sharing increment under higher control levels for both agent types.

**Beliefs** A glance at the agents' beliefs in Figure 2 reveals 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.<sup>21</sup> 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 control* and *Min 21*, we can reject the null that the distributions of both agent types are similar at the 5 percent level (Kolmogrov-Smirnov test,  $p = 0.026$  and  $p = 0.031$ , respectively). For the intermediate control level, *Min 6*, we do not observe intergroup differences in the control beliefs ( $p = 1.00$ ).

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<sup>21</sup>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 vs. in-group agents' perceived likelihood that their particular principal will choose no control, low control, and high control, respectively. We observe significant differences between groups in the *No control* and *Min 21* conditions (Mann-Whitney test,  $p = 0.016$  and  $p = 0.001$ ). Beliefs do not differ by group in the *Min 6* condition ( $p = 0.886$ ).

Thus far, we have compared the means and cumulative distribution functions 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.<sup>22</sup>

<sup>22</sup>We are aware that relying only on the modal belief as the agent's control reference point means that we do not take the "strength" of the mode into account. Recall that beliefs are elicited by asking

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 ( $p = 0.003$ ). The equality of the cumulative distribution functions of the modal control beliefs of the two types of agents can be rejected at the 5 percent 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 monitored differently than their no-group counterparts and are willing to share more with their principals.

Similar to Falk and Kosfeld (2006) and in contrast to Ploner et al. (2010), 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 ( $p = 0.001$ ). 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).

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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 monitoring 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 the results reported above unaffected.

## Principal

**Control** We find treatment effects in the principals' control decisions. In-group principals monitor their agents less often than no-group principals. As can be seen in the upper panel in Table 1, the proportion of principals deciding to monitor is significantly higher in the no-group than the in-group treatment (Fisher's exact test,  $p = 0.060$ ). 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).<sup>23</sup> 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 monitor in-group agents to a greater degree than no-group agents.

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<sup>23</sup>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 the total sample and for the subsample of principals with coordination satisfaction greater than 3.

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

<b>Principal</b>			
<b>Control decision</b>	No-group	In-group	<b>Total</b>
<i>No mon</i>	4	14	18
<i>Min 6</i>	24	21	45
<i>Min 21</i>	51	51	102
<b>Total</b>	79	86	165
<i>Fisher's exact test, p-value: 0.060</i>			
<b>S.O. belief</b>	No-group	In-group	<b>Diff.</b>
<i>No mon</i>	11.430 (10.431)	23.756 (22.087)	-12.325***
<i>Min 6</i>	32.506 (21.344)	28.035 (19.623)	4.471
<i>Min 21</i>	56.063 (25.106)	48.209 (24.245)	7.854**
Observations: 165 principal-agent pairs			

*Note:* This table reports the summary statistics of principals' 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. Standard deviations are given in parentheses. Beliefs are compared using a Mann-Whitney test: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ , while the distributions of the counts are tested with a two-sided Fisher's exact test.

**Second-order beliefs** The intergroup difference in the principals' second-order beliefs is consistent with our findings on the agents' actual beliefs. A significantly higher proportion of in-group principals think that the agent does not expect any control from him, 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, it is interesting that 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 per-

ceived no control as the agents' modal belief. Testing more rigorously for differences in the modal beliefs of in-group and no-group principals, we find them to be significantly different at the 10 percent level ( $p = 0.087$ ).

## 4.2 Real-effort game

In this section, we investigate the effect of social identity on the agents' decisions in a real-effort experiment. This adds realism to the analysis because agents completing a real-effort task may be less inclined to share what they earn. In addition, it can be argued that emotions play a larger role in decision making when real effort is involved (Charness et al., 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.<sup>24</sup> The analysis proceeds in two steps. First, we show summary statistics on the agents' voluntary efforts and the principals' control decisions. Then, in an econometric framework, we test for intergroup differences in the agents' reactions to sensation resulting from deviations of expected control from actual control. According to our behavioral hypotheses, negative sensations are emotionally more important to in-group agents than to anonymous agents. Therefore, the former are expected to decrease voluntary sharing more substantially than the latter when a negative sensation occurs. As discussed above, we do not have prior expectations on

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<sup>24</sup>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.

differences between groups for a positive sensation.

**Performance** Before discussing the performance results, it is important to emphasize again that, in contrast to the effort-choice game, the principal's control decision in the real effort game is revealed to the agent before he makes his sharing decision. Therefore, the agent does not hypothetically decide on a level of effort without knowing which control state will eventually occur. Rather, the agent conditions his sharing decision on the observed control level.

First, 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 we do observe 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 ( $p = 0.008$ ).<sup>25</sup> This result provides support for Conjecture 3. In Figure 3, we depict voluntary sharing by group and control level. We find striking similarities 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. However, the number of in-group and no-group observations differs for each control level<sup>26</sup>, so it may be misleading to analyze treatment effects by level of control. Across control levels, we observe that the average voluntary effort is two and a half times greater for in-group than for no-group agents ( $p = 0.002$ ).<sup>27</sup> When comparing the cumulative distribution

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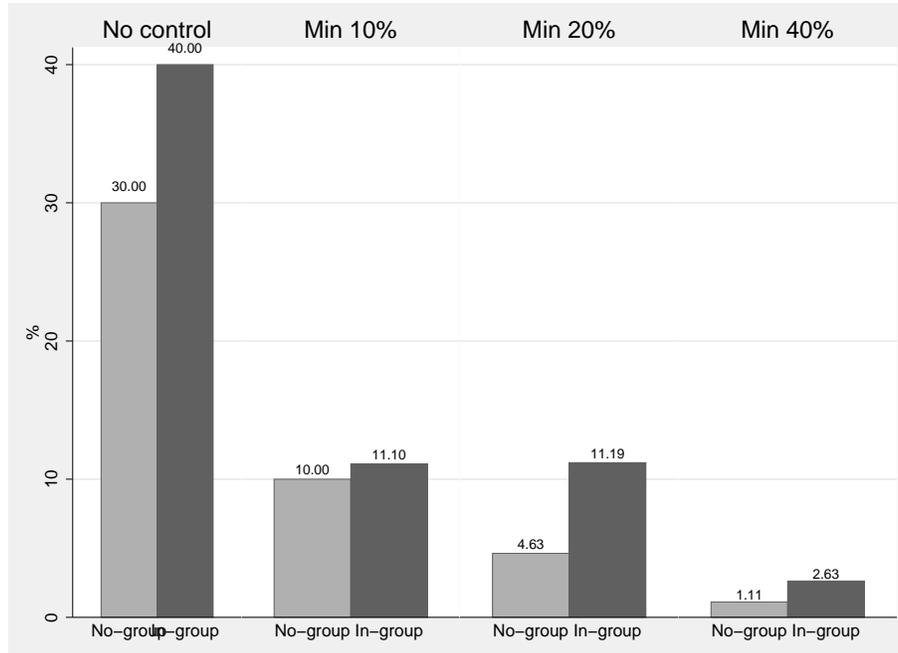
<sup>25</sup>We observe a similar test statistic when excluding the 14 principal-agent pairs that were not able to produce a satisfactory result in the coordination game.

<sup>26</sup>Table 5 in Appendix C provides further details.

<sup>27</sup>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

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 (Kolmogorov-Smirnov test,  $p = 0.009$ ).

Figure 3: Real-effort game: Voluntary sharing in percent



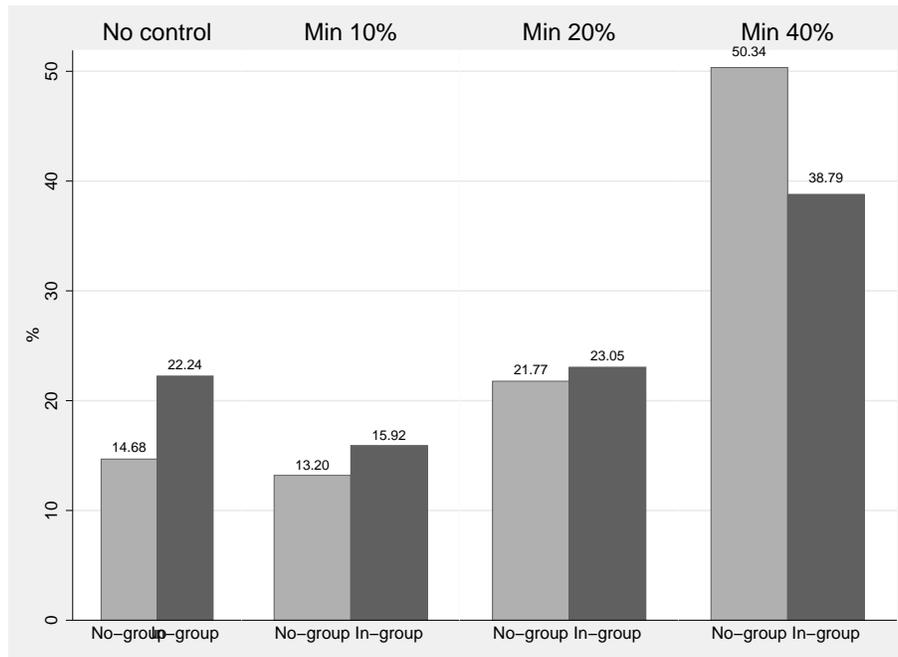
*Note:* This figure illustrates the agent's voluntary sharing in the real-effort game by level of control. Sharing here means the percentage of total earnings in the real-effort game that the agent chooses to transfer to the principal. The agent makes this decision *before* the real-effort game is played.

**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 for intermediate levels of control (Figure 4).<sup>28</sup> Regarding the cumulative distribution (feedback of 4 or 5) are on average three times as reciprocal as no-group agents ( $p = 0.002$ ).

<sup>28</sup>Excluding those in-group agents where group induction can be expected to have not worked properly, again, does not alter the results.

functions of control beliefs, however, the agents only differ significantly for maximum monitoring (Kolmogrov-Smirnov test,  $p < 0.000$ ).

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. 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 monitoring (*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. As in the effort choice game, we again find support for Conjecture 5. No-group agents have greater control expectations than their in-group counterparts. 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 ( $p < 0.001$ ). The respective medians

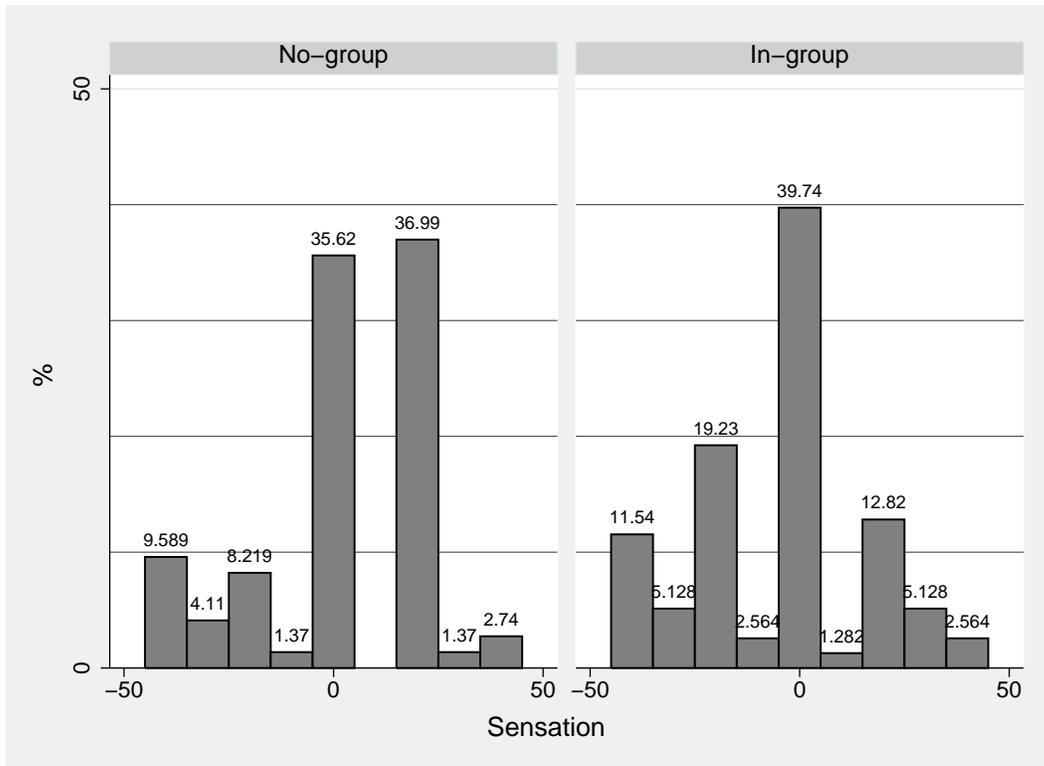
are 40 for no-group and 20 for in-group agents. The distributions of in-group and no-group agents' modal control beliefs are different at the 1 percent significance level (Kolmogorov-Smirnov test,  $p = 0.001$ ).<sup>29</sup>

**Reaction to sensation** We now turn to the analysis of the agents' reactions to *sensation*. In our study, we refer to sensation as the difference between the agent's modal belief regarding control and the actual level of control faced. A negative sensation occurs if the modal control belief is lower than actual control, which we interpret as some sort of disappointment felt by the agent. A positive sensation means that the agent experienced control lower than he expected.

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<sup>29</sup>When using the expected value of control beliefs instead of modal beliefs, we obtain qualitatively similar results.

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



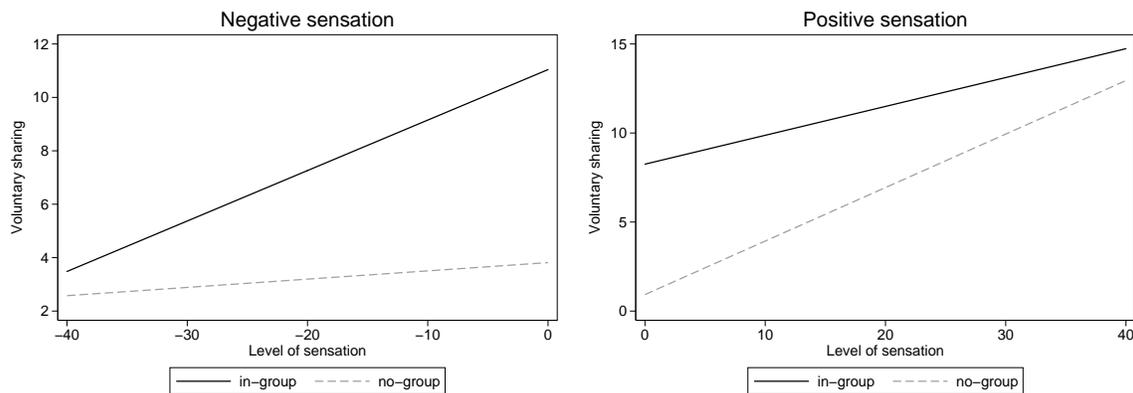
*Note:* This figure shows the histograms of the experienced sensation by group. The bars indicate the share of the agent population that faced a sensation of size  $i$ . The intergroup difference in sensation is significant at the 5 percent level (Mann-Whitney test,  $p = 0.023$ ).

Figure 5 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 face higher sensations on average. Interestingly, the average value of in-group agents' sensation is negative (-5), while no-group agents experience positive sensations on average (2.05). Concerning the respective cumulative distribution functions, we can reject similarity between the agent types at a significance level of 10

percent (Kolmogorov-Smirnov test,  $p = 0.058$ ).

Next, we investigate whether the reaction to sensation in voluntary sharing is different between groups. Before we come to the regression analysis, we show the agents' reactions to sensation graphically in Figure 6.

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 and negative sensations. Zero sensations are not included in calculating the slopes and are extrapolated.

Some results are noteworthy. First, support for Conjecture 2 comes from the observation that all sensation-response functions in Figure 6 are positively sloped. Second, in-group agents share more voluntarily than their no-group counterparts at *any* level of sensation. This supports Conjecture 4. Finally, the graph gives some indication that Conjecture 6 is also supported by the data. Consider the negative sensations plotted in the left panel in Figure 6. As expected, the drop in the sharing increment for negative sensations is more pronounced for in-group than for no-group agents. Because the between-group 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, depicted in the right panel in Figure 6. Here, the slope is steeper for no-group than for in-group agents, which suggests that the marginal increases in voluntary sharing for no-group agents exceed those for in-group agents when experiencing positive sensations. At the maximum level of sensation, 40, little sharing difference between groups remains. Thus, the reciprocal reaction to positive sensations (in terms of voluntary sharing) is more pronounced for no-group than for in-group agents. In the absence of any sensation, however, voluntary sharing by in-group agents is higher than by no-group agents. In fact, there is almost a 10:1 difference in the “baseline” reciprocity between in-group and no-group agents. On average, in-group agents voluntarily share approximately 10.7 percent of their earnings if they are not surprised, while the voluntary sharing of no-group agents is only slightly more than 1 percent ( $p = 0.001$ ).

The effect of sensation on voluntary sharing is more rigorously investigated in Table 2. Because our dependent variable, voluntary sharing (*Voluntary*), can only take non-negative integer values and displays signs of over-dispersion<sup>30</sup>, we perform a negative binomial regression. Standard errors in the regressions are robust to heteroskedasticity and clustered by session.<sup>31</sup>

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<sup>30</sup>We report more formal tests of over-dispersion in Table 2.

<sup>31</sup>Our results remain qualitatively the same under a range of alternative specifications. First, as another means of adjusting for heteroskedasticity, we also estimated weighted negative binomial models, using the share of observations for sensation of level  $i$  in the total number of observations as a weight. Moreover, we estimated the models with a full set of session dummies as additional regressors and the standard errors are robust to arbitrary heteroskedasticity. Finally, we also conducted probit and OLS estimations.

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

	Dependent: Voluntary sharing		
	(1)	(2)	(3)
Sensation	0.014*** (0.005)	0.018* (0.010)	
Ingroup		1.112*** (0.275)	2.319*** (0.683)
Ingroup $\times$ sensation		-0.001 (0.011)	
Pos. sensation			0.073*** (0.024)
Ingroup $\times$ pos. sensation			-0.070** (0.028)
Neg. sensation			-0.029 (0.024)
Ingroup $\times$ 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*** (0.160)	1.617*** (0.174)	1.537*** (0.179)
$\chi^2$ for alpha=0	1725.632***	1470.375***	1348.030***

*Note:* This table reports the results of a negative binomial regression of the effects of sensation on voluntary sharing by group. We refer to sensation as the deviation of expected modal control from actual control. Because 14 subjects had flat prior beliefs over control levels, these regressions include 14 fewer observations than in the full sample. The negative binomial model was chosen to account for the over-dispersion of the data. As can be seen from the table, the results from a likelihood-ratio test for *alpha* (estimated without robust standard errors) indicate over-dispersion for all specifications.

Heteroskedasticity-robust standard errors, which are clustered by session, are presented in parentheses.

\*  $z < 0.10$ , \*\*  $z < 0.05$ , \*\*\*  $z < 0.01$ .

The positive and highly significant coefficient on *Sensation* in Column 1 indicates support for Conjecture 2; voluntary sharing increases in sensation, that is, the difference between individually expected and actually experienced monitoring. In Column 2, we investigate whether no-group and in-group agents differ in their reactions to sensation. However, we do not yet distinguish between the two types of sensation (positive or negative). *In-group* 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. The coefficient on *In-group* is positive and significant at the 1 percent level, which indicates that in the absence of any sensation, in-group agents share more voluntarily than no-group agents. The interaction term  $In\text{-}group \times sensation$  allows for treatment effects in the response to sensation. Due to the inclusion of the interaction term, *Sensation* in Column 2 refers to no-group agents. Although the positive impact of sensation on voluntary sharing is somewhat weaker for no-group agents compared to the total sample, it is still positive and significant. The interaction term is not significant; hence, in-group and no-group agents increase voluntary sharing in sensation equally. However, before we conclude that both agent types respond to sensation in the same way, it behooves us to consider arguments and evidence in support of a subtler hypothesis: that behavioral differences between the agent types depend on the nature of the sensation. We test for this in Column 3. The variable *Pos. sensation* measures the difference between the agent's modal control belief and the actually experienced level of control *if* this difference 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 agent's modal control belief is strictly lower than the experienced level of control. Treatment

effects are captured by the interaction terms *In-group*  $\times$  *pos. sensation* and *In-group*  $\times$  *neg. sensation*, respectively. We first restrict our attention to positive surprises. The positive and significant coefficient on *Pos. sensation* shows that no-group agents voluntarily share more the larger the (positive) difference between the expected level of control and experienced control. The negative coefficient on *In-group*  $\times$  *pos. sensation* indicates that the effect on sharing of a positive sensation is less pronounced for in-group than for no-group agents. This result 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*  $\times$  *neg. sensation* indicates 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 sensation, but these are only visible when accounting for the nature of the sensation.<sup>32</sup>

**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.

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<sup>32</sup>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 on their partners in the initial coordination game, that is, a rating below 4.

## 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,  $p = 0.094$ ). Compared to their no-group counterparts, in-group principals are more often inclined to choose no or low control. However, it is also 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, which 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** Turning to the second-order beliefs, a picture similar to the effort-choice game emerges. In-group principals expect their partners to believe that they are not monitored more frequently and maximally monitored less frequently than no-group principals ( $p = 0.002$  and  $p = 0.007$ , respectively). There are no treatment differences 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. This is the same 1:8 ratio that we observed in the effort choice game. However, we find no significant difference between the two types of principals in the modal second-order beliefs ( $p = 0.181$ ).<sup>33</sup>

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<sup>33</sup>The results do not change if those principals are excluded from the sample who were not satisfied with their partners in the initial coordination game (rating below 4).

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

<b>Principal</b>			
<b>Control decision</b>	No-group	In-group	<b>Total</b>
<i>No mon</i>	2	7	9
<i>Min 10%</i>	3	10	13
<i>Min 20%</i>	30	26	56
<i>Min 40%</i>	44	43	87
<b>Total</b>	79	86	165
<i>Fisher's exact test, p-value: 0.094</i>			
<b>S.O. Belief</b>	No-group	In-group	<b>Diff.</b>
<i>No mon</i>	5.671 (6.271)	15.907 (20.191)	-10.236***
<i>Min 10%</i>	15.418 (13.127)	18.698 (14.683)	-3.280
<i>Min 20%</i>	27.203 (15.011)	24.779 (12.635)	2.423
<i>Min 40%</i>	51.709 (26.115)	40.616 (22.386)	11.093***
Observations: 165 principal-agent pairs			

*Note:* This table reports the summary statistics of principals' decisions and beliefs in the real-effort 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. The median belief is 30 for in-group and 40 for no-group principals. Standard deviations are given in parentheses. Beliefs are compared using a Mann-Whitney test: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ , while the distributions of the counts are tested with a two-sided Fisher's exact test.

## 5 Conclusions

In this paper, we study the effect of control on performance in a principal-agent framework. Including concepts from identity economics (Akerlof and Kranton, 2000, 2005, 2008) in a simple principal-agent model, we propose that the agent's control belief is

contingent on the social distance to the principal. We expect an agent who identifies himself with the principal, that is, the former partly defines himself in terms of salient group membership (*in-group*), to have a different control reference point than an agent who is unknown to the principal (*no-group*). We formalize how subjects respond (in terms of effort) to deviations from the individually expected control level. Based on this modeling framework, we derive hypotheses about the differences between agent types in their reactions to a principal's control. We hypothesize that the between-group differences in the agents' behaviors depend on the nature of the control sensation, with the latter being defined as the difference between the expected and actually experienced control levels. We expect in-group agents, on average, to be more willing to exert effort on behalf of the principal than no-group agents. However, this sharing discrepancy is smaller the more negative the control sensation faced by the agent. Intuitively, no-group agents are less disappointed by negative sensations than their in-group counterparts.

Using a modified version of the principal-agent game developed by Falk and Kosfeld (2006), we experimentally test the validity of these hypotheses. Unlike the experiment in Falk and Kosfeld (2006), the principal can choose between three different minimum performance requirements, and we observe the agent's choices for these three control levels using the strategy method. Furthermore, we conduct a real-effort experiment, in which we reveal the principal's control decision before the agent decides how much effort to exert for the principal. We manipulate the social distance between the principal and agent with the help of a coordination game with an obvious focal point that played prior to the control game.

We find that in-group agents are less frequently monitored and, on average, supply

more effort than no-group agents. However, as hypothesized, the behavioral reaction to control depends heavily on the relationship between principal and agent. First, there are pronounced intergroup differences in the individually expected level of control. That is, as compared to their no-group counterparts, in-group agents expect to be monitored significantly less by their principals. Second, in-group agents who experience a positive sensation reciprocate less than no-group agents. This is, to the best of our knowledge, a new result in the literature on control. Third, if the principal's control exceeds the agent's expectation, in-group agents' performances decrease more sharply than that of no-group agents.

We contribute to the research on the relationship between social identity and work motivation and task performance. Previous studies yield converging evidence in support of a positive impact of identity on motivational and performance-related factors (for an extensive review, see Van Knippenberg, 2000). Little is known about the possible detrimental consequences of identity. In this paper, we propose a channel through which a shared identity between the principal and agent may negatively affect the latter's performance. If the agent feels that the principal does not act in accordance with the social identity based on their group membership, he may be inclined to punish this behavior. In our experiment, we find that identity generally stimulates the agent's willingness to conduct effort on behalf of the principal. However, we also observe that agents, especially in-group agents, retaliate for control levels they perceive as inappropriately high.

Group memberships (even multiple ones) are central to organizational life. Therefore our research also has implications for organizations. These can be effectively illustrated

using the US public procurement system as an example. As a central element of a far-reaching government reform in the beginning of the 1990s (Gore, 1993), public purchasing functions became more decentralized and less rule-bound (Kelman, 2002, 2006; McCue and Pitzer, 2000). It has been argued that a closer connection between the purchasing agent and end-user leads to higher service benefits being enjoyed by the latter, which more than offsets potential cost savings realized through volume purchases (McCue and Gianakis, 2001). Although we are not aware of any study investigating whether the above government reform was successful in achieving its objectives, our results suggest that purchasing officers in this changed procurement environment may indeed be more dedicated to “pleasing the customer.” Conversely, if end-users are not sufficiently aware of their social distance from government buyers and what is expected in terms of appropriate behavior, a closer connection between the agent and principal may well backfire on the principal and actually reduce the agent’s effort. How behavior that is regarded as incongruent with the implied social identity affects principal-agent relationships in the long run, possibly corroding group identity, 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.  $e \geq m$ , where  $m$  is the control choice of the principal, taken as given by the agent.

$$\max U_A(e; m) = g(\Delta, e) \quad (3)$$

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

The Lagrangian to this problem is:

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

The Kuhn Tucker conditions read:

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

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

$$e^* [g'(\Delta, e) - \lambda] = 0 \quad (7)$$

$$\lambda(m - e) = 0 \quad (8)$$

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

This 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  $e = m$ . Depending on the shape of  $g(\Delta, e)$ , a binding constraint can be optimal.

One solution to the agent's maximization problem is to not exert effort beyond the minimum requirement set by the principal (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 Assumption 4 and Assumption 3, it follows that  $de/d\Delta$  is positive.

## B Effort choice

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

Agent			
<b>Voluntary sharing</b>	No-group	In-group	<b>Diff.</b>
<i>if No mon</i>	26.646 (2.197)	37.698 (2.879)	-11.052***
<i>if Min 6</i>	16.734 (2.223)	24.302 (2.720)	-7.568*
<i>if Min 21</i>	7.392 (1.776)	14.477 (2.400)	-7.084**
<b>Belief</b>	No-group	In-group	<b>Diff.</b>
<i>No mon</i>	21.468 (2.380)	32.291 (2.908)	-10.822***
<i>Min 6</i>	23.962 (1.838)	23.907 (1.886)	0.055
<i>Min 21</i>	54.570 (2.868)	43.802 (2.794)	10.767***

Observations: 165 principal-agent pairs

*Note:* This table reports summary statistics of the agents' decisions and beliefs, differentiated by group treatment. Agents' voluntary sharing decisions and all subjects' beliefs are presented as averages, where standard deviations are given in parentheses. The agents' beliefs are the first-order beliefs regarding the probability of facing the respective control level. Thus, the beliefs add up to 100 for each agent type. Effort levels and beliefs are compared using a two-sided Mann-Whitney test: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## C Real effort

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

Agent			
<b>Voluntary sharing</b>	No-group	In-group	<b>Diff.</b>
	3.519 (7.056)	9.244 (13.243)	-5.725***
<b>Belief</b>	No-group	In-group	<b>Diff.</b>
<i>No mon</i>	14.684 (16.852)	22.244 (22.894)	-7.561*
<i>Min 10%</i>	13.203 (9.781)	15.919 (14.076)	-2.716
<i>Min 20%</i>	21.772 (12.728)	23.047 (16.557)	-1.274
<i>Min 40%</i>	50.342 (26.074)	38.791 (26.137)	11.551***
Observations: 165 principal-agent pairs			

*Note:* This table reports summary statistics of the agents' voluntary sharing decisions and beliefs in the real-effort game, differentiated by group treatment. The average value of voluntary sharing is presented. Standard deviations are given in parentheses. The agents' beliefs are the first-order beliefs regarding the probability of facing the respective control level. Thus, the beliefs add up to 100 for each agent type. Effort levels and beliefs are compared using a two-sided Mann-Whitney test: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .